# Essays on Power in Labor Markets

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"Essays on Power in Labor Markets"

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# Essays on Power in Labor Markets

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## Abstract

Workers' pay and working conditions are determined not just by the productivity of their labor, but also by power and institutions. Factors affecting pay and working conditions may include the availability of good outside job options for workers; the presence of formal and informal labor market institutions which govern the distribution of rents between workers and other stakeholders; and the scale and scope of labor market regulation. In this dissertation, I examine three aspects of power and institutions in labor markets.

In Chapter 1, "Employer Concentration and Outside Options" (co-written with Gregor Schubert and Bledi Taska), we ask the question "To what extent does employer concentration matter for workers' wages across the US, and for whom does it matter most?". Employer concentration – a scarcity of employers that a given worker can work for in their local labor market – can reduce the degree of effective competition for workers, increasing employers' wage-setting power (relative to a more competitive labor market). Using US-wide data over 2013-2016, we estimate the effect of employer concentration on wages, making two primary new contributions. First, we develop an instrument for employer concentration, based on differential local exposure to national firm-level trends. We use the instrument to estimate the effect of plausibly exogenous variation in employer concentration on wages across the large majority of U.S. occupations and metropolitan areas. Sec-

ond, we adopt a flexible "probabilistic" approach to labor market definition, identifying relevant job options outside a worker's own occupation using new occupational mobility data constructed from 16 million resumes. We use this data to develop an index of the value of these outside-occupation job options and estimate the effect of this index of outside-occupation option value on wages.

We find that moving from the median to the 95th percentile of employer concentration reduces wages by 3%. But we also reveal substantial heterogeneity: the effect of employer concentration is at least four times higher for occupations with low outward mobility (like registered nurses or security guards) than for those with high outward mobility. While the majority of U.S. workers are not in highly concentrated labor markets, these estimates suggest that a material subset of workers do experience meaningful negative wage effects from employer labor market power. Our findings imply that labor market regulatory agencies and antitrust authorities should take employer concentration seriously. Policy responses may be most effective if targeted toward the local occupational labor markets characterized by both high employer concentration and few outside-occupation job options for workers.

In Chapter 2, "The Declining Worker Power Hypothesis" (co-written with Lawrence H. Summers), we argue that the decline of workers' power to share in the rents generated by firms over the last forty years in the US has substantial explanatory power for major macroeconomic trends: specifically, the decline in the labor share of income, the rise in measures of profitability like Tobin's Q, firm valuations, average profitability, and firm-level markups, and the decline in average unemployment even while inflation stayed low and stable (which can be interpreted as a decline in the Non-Accelerating Inflation Rate of Unemployment, or NAIRU). Roughly quantifying the degree of worker rent-sharing using estimates of union, industry, and firm size wage premia, we show that the decline in labor rents corresponds closely in magnitude to the decline in the aggregate labor share over 1982-2016, that industries and states with bigger declines in labor rents saw bigger declines in their labor shares and average unemployment rates, and that industries with bigger declines in labor rents saw bigger increases in measures of Tobin's Q and average profitability. Together, our results suggest that the decline of worker power should be considered a major contributor to the defining macroeconomic trends of the last four decades in the US economy.

In Chapter 3, "Incentives To Comply With the Minimum Wage in the US and UK" (partially co-written with Lindsay Judge), we investigate the compliance incentives created by the minimum wage penalty and enforcement regimes in the US and UK. Using data on penalties levied on firms for violations of the Fair Labor Standards Act's minimum wage and overtime provisions in the US, and data on penalties levied on firms for violations of the National Minimum Wage in the UK, we estimate the average cost firms may expect to pay if they are caught violating the minimum wage. From a cost-benefit analysis standpoint, our estimates suggest that most firms must expect to face a probability of detection of at least 47-88% in the US, and at least 44-56% in the UK, to have an incentive to comply with the minimum wage. The level of government resources available for inspection, and the cost and difficulty for employees of bringing court action against non-paying employers, makes it likely that the true probability of detection is substantially lower than this for many firms. That is, the penalty and enforcement regimes in both the US and UK seem unlikely to give many firms an incentive to comply with the minimum wage. These limited incentives to comply with the minimum wage may help explain the substantial prevalence of minimum wage underpayment in both

countries.

Overall, the three chapters indicate the importance of labor market power and labor market institutions in the determination of wages – particularly for low- and middle-income workers. If society wishes to raise pay for low- and middle-income workers and to reduce income inequality, the essays in this dissertation suggest a number of possible policy responses: responding to employer concentration in a targeted manner through antitrust, wage floors, and other labor market regulations, increasing workers' power to share in the rents produced by their firms by strengthening unions and promoting a role for workers in corporate governance – and ensuring that the enforcement regimes for these worker protections provide sufficient incentives for employers to comply.

# Contents

To Mum, Dad, Alyssa, and Simon.

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# 1

# Employer Concentration and Outside Options

*Co-authored with Gregor Schubert and Bledi Taska.*

## 1.1 Introduction

In recent years, concerns about employer concentration have increased. Employer concentration has been posited as a possible explanation for inequality, low pay, and stagnant pay growth. Antitrust authorities have been called on to consider employer concentration in the review of merger and acquisition cases. Concerns have been raised that employer concentration facilitates restrictions on competition like no-poaching agreements. And, since employer concentration can be a source of monopsony power,[1] concerns around high employer concentration have bolstered calls to raise minimum wages and strengthen collective bargaining.[2]

To assess whether – or in which cases – policy should respond to employer concentration, we need to understand the nature and effects of employer concentration in the US. In this paper, we seek to answer the question: *To what extent does employer concentration matter for US workers' wages, and for whom does it matter the most?* We estimate the effects of employer concentration on average hourly wages across over 100,000 US SOC 6-digit occupation-by-metropolitan-area labor markets over 2013–2016, following Azar et al. (2020a) in measuring employer concentration with a Herfindahl-Hirschman Index (HHI) constructed from Burning Glass Technologies' online job postings database. Our empirical strategy addresses two common empirical issues: endogeneity and market definition.

The first empirical issue is endogeneity. While recent research has documented a negative relationship between local employer concentration and wages, the extent to which this is causal – and the magnitude of any such causal effect – is unclear: employer concentration may be correlated with other local economic conditions which also affect wages, complicating the estimation of any underlying wage-concentration relationship.

To respond to this issue, we propose a new identification approach for the effects of employer concentration on wages, drawing on shift-share and granular IV methodology (Borusyak et al., 2018; Gabaix and Koijen, 2020). Specifically, we instrument for employer concentration within a particular local occupation with the predicted change in employer concentration, predicting each local employer's hiring with its national hiring in that occupation (excluding the local area in ques-

[1] Other possible sources of monopsony power include search frictions, switch costs, and worker and job heterogeneity (Manning, 2003; Robinson, 1933).

[2] Authors making the arguments in this paragraph include, variously, Bahn (2018); Krueger and Posner (2018); Marinescu and Posner (2020); Marinescu and Hovenkamp (2019); Naidu et al. (2018); Shambaugh et al. (2018).

tion). This enables us to construct shocks to local employer concentration that are plausibly orthogonal to local productivity, with the key identifying assumption being that each large firm's decision to increase or decrease its hiring nationwide is exogenous with respect to the local economic conditions in any specific local occupation.

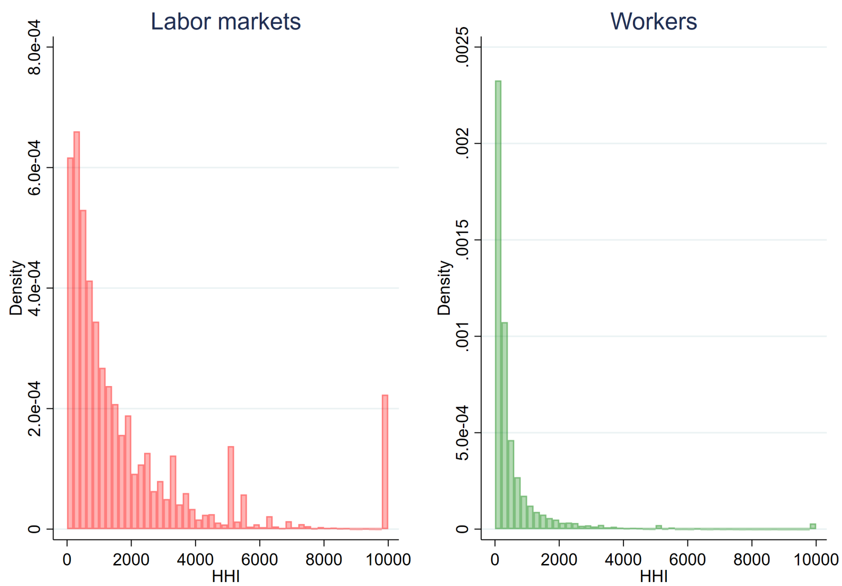
The second empirical issue is market definition. Assessing the effect of local employer concentration on wages, and pinpointing the workers who are most affected by it, requires a good definition of the relevant local labor market for workers. Using new, highly-granular occupational mobility data constructed from 16 million US workers' resumes (obtained by Burning Glass Technologies),[3] we show that occupational mobility is high and highly heterogeneous across occupations. This suggests that regressing wages on within-occupation employer concentration – as much recent research does – without considering the availability of these outside occupation job options (1) may obscure heterogeneity, as some occupations are a better approximation of workers' true labor market than others, and (2) may lead to biased estimates, as workers who are in high-concentration labor markets (*within* their local occupation) also tend to have poor local job options *outside* their occupation.

To respond to this issue, we introduce two new factors into our baseline regressions of wages on within-occupation employer concentration. First, we allow the estimated coefficient on within-occupation employer concentration to vary by occupations' outward mobility. This enables us to estimate different effects of employer concentration on wages for low-mobility vs. high-mobility occupations (for whom the SOC 6-digit occupation is less likely to be a good approximation to their true labor market). Second, we develop a measure of the value of workers' outside job options in other occupations – an "outside-occupation option index" – and estimate its effect on wages in our baseline regression alongside the effect of within-occupation employer concentration. Our outside-occupation option index is the weighted average of local wages in all occupations except

[3] The large sample size – an order of magnitude more than other data sources – enables us to estimate occupational transitions reliably between a large share of US occupations. We are making this new occupational mobility dataset publicly available.

the worker's own, with each weight the product of: (i) occupational mobility flows to each outside occupation and (ii) the local relative employment share in each outside occupation.[4] We use a shift-share IV approach to identify effects of changes in this outside-occupation option index on wages, instrumenting for local occupational wages with the leave-one-out national mean wage in outside option occupations.

**Figure 1.1:** Histogram of employer HHI across occ-city labor markets and across workers, 2016




Note: HHI is measured using Burning Glass Technologies vacancy data, at the level of a SOC 6-digit occupation by metro area labor market. Our data covers occupation-metro area labor markets which include 110m of the 140m workers in the U.S. labor market in 2016. Left panel shows the distribution of HHIs across occ-city labor markets in 2016. Right panel shows the distribution of HHIs across workers in 2016 (i.e. the distribution of HHIs across occ-city labor markets, weighted by employment in each of these labor markets).

How much does employer concentration matter for wages? Our baseline results suggest that while most workers are not in highly concentrated labor markets (see Figure 1.1), moving from the median to the 95th percentile HHI (as faced by workers) results in 2.6 log points lower wages.[5] This

[4]We show that this index can be derived from our conceptual framework in which workers' wage depends on the expected value of their outside option.

[5]Our instrumental variable estimates are about 50% larger than our OLS estimates, suggesting that some

average masks substantial heterogeneity: within-occupation employer concentration matters substantially more for workers who are less able to find comparably good jobs in other occupations. For occupations in the bottom quartile of occupational mobility, like registered nurses and security guards, moving from the median to 95th percentile HHI is associated with between 4 and 8 log points lower wages; for occupations in the highest quartile of occupational mobility, like counter attendants or bank tellers, our point estimate is zero and the confidence interval rules out any decrease in wages greater than 1.8 log points. As expected, we find that regressions of wages on employer HHI suffer from omitted variable bias if the availability of outside-occupation options is not included in the analysis, with an upward bias in the coefficient size of about 30-40%. A back-of-the-envelope calculation, using our coefficient estimates, suggests that over 10% of the 110 million workers covered by our data experience wage suppression of 2% or more as a result of employer concentration. Many of the most-affected workers are healthcare workers, reflecting both high healthcare employment concentration and low occupational mobility.

We also find a positive and significant effect of an increase in the value of outside-occupation options, holding constant within-occupation employer concentration: for the median occupation, moving from the 25th to the 75th percentile value of outside-occupation options across cities is associated with 3.7 log points higher wages. These magnitudes are meaningful relative to the degree of geographic wage dispersion across cities: for the median occupation, moving from the 25th to the 75th percentile city by average wage is associated with a 21 log points higher wage. Overall, our paper demonstrates that the availability of job options outside workers' own firm – which is affected both by within-occupation employer concentration and the quality of outside-occupation job options – matters substantially for workers' own wages.

Related literature: We build on a growing body of work demonstrating an empirical rela-

combination of omitted variables or measurement error bias the coefficient towards zero in simple regressions of wages on employer concentration.

tionship between wages and employer concentration, which began in recent years with Azar et al. (2020b), Azar et al. (2020a), Benmelech et al. (2018), and Rinz (2018),[6] as well as a growing theoretical literature demonstrating an effect of employer concentration on wages (Azkarate-Askasua and Zerecero, 2020; Berger et al., 2019; Jarosch et al., 2019). We make two contributions to this literature: we use a new instrument to estimate plausibly causal effects of employer concentration on wages, and we show that simple wage-concentration regressions which do not do consider outside-occupation options are likely biased and obscure important heterogeneity.

Second, in estimating the effect of outside-occupation options on wages, we add to a literature on outside options in the labor market, including Beaudry et al. (2012), who show local spillovers from changes in industrial employment in the US; Caldwell and Danieli (2018), who find wage effects of workers' outside options in Germany, estimated from the diversity of jobs held by similar workers; Macaluso (2019), who shows that the skill mix of local employment affects laid-off workers' outcomes in the US; and Alfaro-Urena et al. (2020), who estimate the outside option value of jobs at multinational corporations in Costa Rica. Third, in using occupational transitions to identify outside options we build on papers which use worker flows to identify the scope of workers' labor markets (Manning and Petrongolo, 2017; Nimczik, 2018), and to study skill similarity across occupations and industries (Arnold, 2020; Neffke et al., 2017; Shaw, 1987).

Finally, we contribute to a broader literature on imperfect competition in labor markets, including both the literature on labor market monopsony and the elasticity of the labor supply curve to the firm (e.g. Azar et al., 2019a; Azkarate-Askasua and Zerecero, 2020; Berger et al., 2019; Boal and Ransom, 1997; Manning, 2003), and the large search-and-matching literature which features outside options in the worker-firm wage bargain (e.g. Burdett and Mortensen, 1980; Cahuc et al.,

[6] As well as Lipsius (2018), Hershbein et al. (2019), Gibbons et al. (2019), and Qiu and Sojourner (2019) in the US, Abel et al. (2018) in the UK, Marinescu et al. (2019) in France, Martins (2018) in Portugal, and Dodini et al. (2020) in Norway.

2006).[7]

## 1.2 Conceptual framework

There are a number of models of the labor market in which employer concentration matters for wages. First, employer concentration can generate upward-sloping labor supply curves to individual firms, leading to wage markdowns (e.g. Berger et al., 2019).[8] Second, the presence of a small number of firms facilitates collusion to suppress wages. Third, in a bargaining model of the labor market, employer concentration reduces the number of feasible outside options for workers, as the average worker in a given labor market has few distinct firms as alternative possible employers: this reduces workers' relative bargaining position and therefore the wage (Jarosch et al., 2019).[9] In this paper, the conceptual framework guiding our empirical analysis is the third: *employer concentration worsens workers' outside options*. While the main focus of our paper is empirical, we outline below a stylized framework where we formalize this intuition (developed further in Appendix A.1).

Wage bargaining. At the start of each period, each employed worker Nash-bargains with her employer i. The outcome is wage $w_i$, equal to the value of the worker's outside option if she leaves

[7] Additional work on monopsony includes the empirical estimates of the elasticity of labor supply to the firm in Webber (2015) and Sokolova and Sorensen (2020), and empirical analyses of specific industries, firms, or worker classes in Hirsch and Schumacher (2005), Staiger et al. (2010), Ransom and Sims (2010), Ashenfelter et al. (2013), Matsudaira (2014), Naidu et al. (2016), Bassier et al. (2019), Goolsbee and Syverson (2019), and Dube et al. (2020).

[8] One aspect of monopsony power as initially discussed by Robinson (1933).

[9] Several recent papers specifically show that an HHI of employer concentration is a relevant statistic for firms' labor market power. Berger et al. (2019) show in a general equilibrium oligopsony model that firms with higher market share have lower labor supply elasticities, and that the the wage-bill HHI is a relevant statistic for assessing the welfare effects of firms' labor market power. Azkarate-Askasua and Zerecero (2020) also show that firms' employment shares affect the elasticity of labor supply to the firm, in a model which also features worker bargaining power. Arnold (2020) and Naidu and Posner (2020) show that an employer HHI is related to the size of the wage markdown under Cournot competition. Jarosch et al. (2019) show that in a search model with bargaining, the presence of large employers worsens workers' outside option and so reduces wages, with the effect determined by a concentration index closely related to an HHI.

her job, $oo_i$, plus share $\beta$ of the match surplus (where $p_i$ is the product of the match):[10]

$$w_i = \beta p_i + (1 - \beta) oo_i \tag{1.1}$$

Job search. Once bargaining with incumbent workers has concluded, firms post vacancies to fill empty positions – vacated either by labor force exit, or by bargaining breakdown with an existing worker. Each posted vacancy offers a wage equal to the wage the firm has bargained with its incumbent workers. Job seekers – workers whose previous wage bargain broke down, or who newly entered the labor force – are each paired randomly with a vacancy within their labor market. (For now, consider a situation where all workers and all jobs are perfectly substitutable.) Random matching means that the chance of any given worker receiving a job offer from a particular firm j is equal to firm j's share of vacancies in the labor market, $\sigma_j$ (in the spirit of Burdett and Mortensen (1980) and Jarosch et al. (2019)). If a worker does not receive any job offers, she moves to unemployment for the period and receives b.

Outside option value. The outside option for an employed worker is to leave her current job and become a job seeker. Her outside option value is therefore a weighted average of the wages paid by each local firm (weighted by the probability of being matched with each feasible firm), and the unemployment benefit (weighted by the probability of receiving no job offers):[11]

$$oo_i = \sum_j \sigma_j \cdot w_j + \sigma_i \cdot b. \tag{1.2}$$

[10] We assume all workers at the same firm i have the same outside option.

[11] Note that this implies the wage at firm i will depend *negatively* on its share of the labor market $\sigma_i$. Why? In a labor market with atomistic firms, every job seeker would be matched with a feasible employer each period. But in a labor market with some large employers, the chance that a worker is re-matched with the firm she just quit is non-zero, and we assume this firm does not re-hire her. This means that the probability that a worker at firm i receives a job offer from *any other* firm if she leaves her job is proportional to the share of all jobs which are outside her firm, $(1 - \sigma_i)$. This intuition can be translated to a less stylized setting with on-the-job search, where workers can only receive outside job offers from firms which are *not* their own firm.

Average wage. To a second order approximation, we can therefore express the average wage in the labor market ($\bar{w} = \sum_i \sigma_i w_i$) as a function of the sum of the squares of employer shares ($HHI = \sum_i \sigma_i^2$):

$$\bar{w} = \psi \cdot \bar{p} + (1-\psi) \cdot b - \beta(1-\beta) \sum_i \sigma_i^2 \hat{p}_i \tag{1.3}$$

where coefficient $\psi = 1 - (1-\beta)HHI$ is a function of employer concentration HHI and worker bargaining power $\beta$, $\bar{p} = \sum_i \sigma_i p_i$ is average productivity across firms, and $\hat{p}_j = p_j - \bar{p}$ is the difference between firm j's productivity and the market average. This expression illustrates that (1) the wage declines as employer concentration increases, since workers are less likely to receive job offers from other firms, and (2) there is an interaction between employer concentration and worker bargaining power, since the outside option matters less if workers have more bargaining power over the match surplus.[12]

Outside-occupation job options. The framework above assumes a clearly-delineated labor market, with all workers and jobs equally substitutable. This is typically the approach taken in theoretical and empirical work on employer concentration and wages (and the direct analog of the market definition approach in antitrust in product markets).[13] This is not, however, how most labor markets work in practice. We therefore now adapt our earlier framework, defining the baseline labor market as a worker's local occupation and incorporating workers' option to switch occupation. The value of the outside option is still the weighted average of the wage in each other local firm, but the weight is now a product of two factors: (i) the probability that a worker from occupation o will receive *any* job offer from a firm in occupation p, which we denote $Prob(o \to p)$, and (ii) the va-

[12] The third term depends on the joint distribution of employment shares and productivity. If their correlation is sufficiently small the wage is simply a concentration- and bargaining power-weighted average of productivity p and unemployment benefit b.

[13] Labor markets have typically been defined as a single occupation or industry within a given local area (commuting zone, metropolitan area, or county), and debate has focused on how narrow an occupational or industrial definition to draw (e.g. Azar et al., 2020a,b). Jarosch et al. (2019) and Dodini et al. (2020) define local labor markets more flexibly as clusters of firms inferred using worker flows or common skill requirements (respectively), but still use a binary concept of the labor market.

cancy share of firm j in occupation p, $\sigma_{j,p}$. The outside option for workers in firm i and occupation o is therefore:[14]

$$oo_{i,o} = \underbrace{\mathrm{Prob}(o \to o) \cdot \sum_{j \neq i} \sigma_{j,o} \cdot w_{j,o}}_{\text{own-occ options}} + \underbrace{\sum_{p \neq o}^{N^{occs}} \mathrm{Prob}(o \to p) \sum_{l}^{N_p} \sigma_{l,p} \cdot w_{l,p}}_{\text{outside-occ options}} + \underbrace{\mathrm{Prob}(o \to o)\sigma_{i,o} \cdot b}_{\text{unemployment}}$$

Define $oo^{occs} = \sum_{p \neq o}^{N^{occs}} \mathrm{Prob}(o \to p) \sum_{l}^{N_p} \sigma_{l,p} \cdot w_{l,p}$, the value of outside-occupation job options. We can then express the average wage in occupation o, to a second order approximation, as:

$$\bar{w}_o = \tilde{\psi}_o \left(\alpha \bar{p}_o + (1-\alpha) oo^{occs}\right) + (1 - \tilde{\psi}_o) b - \tilde{p}_o \tag{1.4}$$

where $\tilde{\psi}_o = 1 - (1-\beta)\mathrm{Prob}(o \to o)\mathrm{HHI}_o$, $\alpha = \frac{\beta}{1 - \mathrm{Prob}(o \to o)(1-\beta)}$, and $\tilde{p}_o = \beta(1-\beta)\mathrm{Prob}(o \to o) \sum_i \sigma_i^2 \hat{p}_{i,o}$. This expression suggests that the average wage in occupation o is, roughly, a weighted average of average productivity in occupation o ($\bar{p}_o$), the value of jobs outside occupation o ($oo_o^{occs}$), and unemployment benefits (b), where the weights depend on employer concentration within workers' occupation, the likelihood a worker will remain in her own occupation, and worker bargaining power.[15] As before, higher within-occupation employer concentration reduces the wage. There is also an interaction with $\mathrm{Prob}(o \to o)$: the less likely it is that a worker can find a job in a different occupation, the more employer concentration in her own occupation matters.

Implications for empirical analysis. Typically, regressions of wages on employer concentration at the level of a local labor market take the binary market definition approach: they do not take into account workers' (differential) ability to switch occupation. The discussion above illustrates two problems that this could cause when estimating the effect of concentration on wages.

[14] This assumes that employment decisions are taken at the firm-by-occupation level.

[15] Ignoring the final term $\tilde{p}_o$, which is once again small if the average productivity of individual firms is not strongly correlated with their employment shares

First, one should expect the effect of within-occupation employer concentration on wages to be different for low-mobility vs. high-mobility occupations. Second, if the degree of employer concentration within a local occupation (HHI) is correlated with the quality of outside options *outside* the local occupation ($oo^{occs}$), then estimation of the effect of concentration on the wage may be biased without controlling for outside-occupation options.

In our empirical analysis, we take both of these into account: we regress local average occupational wages on *both* within-occupation employer concentration *and* a measure of the value of outside-occupation options, allowing the coefficients to vary according to an occupation's degree of mobility.

## 1.3 Measuring employer concentration

In our empirical analysis, we will jointly estimate the effect of both within-occupation employer concentration and outside-occupation job options on wages. We follow Azar et al. (2020a) and Hershbein et al. (2019) in using Burning Glass Technologies' ("BGT") database of online vacancy postings to measure employer concentration.[16] We calculate the Herfindahl-Hirschman Index (HHI) of each employer's share of vacancy postings within individual SOC 6-digit occupations and metropolitan areas, in each year 2013–2016:

$$HHI_{o,k,t} = \sum_{i=1}^{N} \left( \frac{v_{i,o,k,t}}{\sum_{i=1}^{N} v_{i,o,k,t}} \right)^2 \tag{1.5}$$

where $v_{i,o,k,t}$ denotes the number of vacancy postings from employer i in occupation o and metropolitan area k in year t. The BGT vacancy posting data covers the near-universe of online job postings,

[16] Why use vacancies rather than employment data? First, we are not able to obtain firm-level employment data within local occupations. Second, vacancies may be a better reflection of workers' feasible outside options than employment. In equilibrium, one would expect vacancy and employment HHIs to be highly correlated. Indeed, Marinescu et al. (2019) show in France that an HHI of employment flows (reflecting filled vacancies) is highly correlated with employment HHIs.

drawn from over 40,000 distinct sources including company websites and online job boards, with no more than 5% of vacancies from any one source (Hazell and Taska, 2019).[17]

Representativeness. Since the Burning Glass Technologies vacancy data covers the near-universe of online job postings, it is relatively representative of the vacancies which are advertised online. There are however two reasons for concern. First, not all vacancies are posted online. Azar et al. (2020a) estimate that in 2016, the BGT vacancy database captured around 85% of all job vacancies both online and offline (as measured from the Help Wanted Online database), but this is likely substantially lower for certain occupations where a large share of jobs are advertised offline or informally.[18] Second, in occupations where firms tend to hire many workers for each posted vacancy, our estimates of employer concentration will be biased to the degree that larger firms may hire a higher number of people per vacancy posting.[19]

To understand the degree to which each of these might be an issue, we calculate a measure of 'represented-ness' of each occupation in the BGT data: the occupation's share of vacancy postings in the BGT database relative to the occupation's share of total employment (as per BLS OES). By this metric, occupations which are particularly underrepresented include low-wage food service jobs, cleaners, home health aides, laborers, and cashiers. In our estimates of the effect of employer concentration on wages, we carry out a number of sensitivity checks to account for underrepresentation of

[17] Each vacancy posting contains the job title, company name, location, date, and job description. Using proprietary parsing technology, BGT imputes a SOC 6-digit occupation code. More details on the process by which BGT obtains, parses, and deduplicates this data can be found in Carnevale et al. (2014). To identify jobs at the same employer, we largely group jobs by employer name. We discuss the data and our process for identifying employers in detail in Appendix A.2.

[18] If the missing vacancies disproportionately come from small firms or households, which seems likely, we will overestimate employer concentration for underrepresented occupations.

[19] If there is a big difference in the ratio of hires per job posting between large and small firms, we will underestimate concentration in labor markets with skewed employer size distributions, relative to those with more symmetric employer size distributions. Our measures of employer concentration may therefore be less reliable for occupations for which there are many large employers who hire a lot of workers for undifferentiated job roles.

certain occupations.[20] For further discussion of the BGT vacancy data, see Appendix A.2.

### 1.4 Identifying outside-occupation options

To jointly estimate the effect of *both* within-occupation employer concentration *and* outside-occupation job options on wages, we also need a measure of outside-occupation options. We use data on workers' mobility patterns between occupations to identify these.[21]

BGT resume data. Since there is no existing US occupational mobility data with high enough granularity to study transitions between SOC 6-digit occupations, we construct a new data set of occupational transitions using 16 million unique US resumes, which enable us to observe longitudinal snapshots of workers' job histories over 2002–2018.[22] This resume data was collected by labor market analytics company Burning Glass Technologies ("BGT"), who sourced the resumes from a variety of BGT partners, including recruitment and staffing agencies, workforce agencies, and job boards.

Transition share. We use this data to construct our measure of occupational transitions $\pi_{o \to p}$, which approximates the probability of a worker moving from occupation o to occupation p

[20] We also control for occupation-by-year fixed effects, which should assuage concerns about the relative representativeness of the data for different occupations.

[21] We see occupational mobility patterns as a transparent, non-parametric way to capture the value of a different occupation as an outside option, since they capture a combination of both feasibility and desirability. In Appendix A.5 we compare our approach to approaches based on task or skill similarity.

[22] The CPS has at least an order of magnitude fewer occupational transition observations than our data over the same time period. This matters: with 705,600 possible transition pairs between the SOC 6-digit occupations, data sets with even a few million observations are not big enough to capture many transition paths.

conditional on leaving her job:[23]

$$\pi_{o \to p} = \frac{\text{\# in occ o in year t observed in occ p in year t} + 1}{\text{\# in occ o in year t observed in a new job in year t} + 1}$$
$$\approx \text{Prob(move from occ o to occ p|leave job)} \tag{1.6}$$

Leave share. We also construct the 'occupation leave share', approximating the share of people who leave their occupation when they leave their job:[24]

$$\text{leave share}_o = \frac{\text{\# in occ o in year t \& no longer in occ o in year t} + 1}{\text{\# in occ o in year t \& in a new job in year t} + 1}$$
$$\approx \text{Prob(leave occ o|leave job)} \tag{1.7}$$

We calculate these as averages across the whole US and across all years in our data, to capture the underlying degree of occupational similarity rather than transitory fluctuations from year to year.[25]

Representativeness. The BGT resume data set is largely representative of the U.S. labor force in its distribution by gender and location. However, it over-represents younger workers and white-collar occupations. Since we use this data set to estimate occupational transitions paths from one occupation to another, the over-representation by occupation is not a substantial concern as long as we still have sufficient data for most occupations to have some degree of representativeness

[23] Specifically, $\pi_{o \to p}$ is the share of people observed in occupation o at some point in year t who are also observed in occupation p at some point in year $t + 1$, as a fraction of all those in occupation o in t who are observed in a new job at some point in $t + 1$. We exclude jobs lasting 6 months or less. Our measure includes people with jobs in two different occupations at the same time – implicitly assuming that this indicates viability as an outside option.

[24] Specifically, this measure captures the share of people observed in occupation o in year t who are *no longer* observed in occupation o at any point in year $t + 1$, as a share of those observed in occupation o in year t who are observed in some new job in year $t + 1$.

[25] We estimate transition probabilities $\pi_{o \to p}$ and leave shares for a large proportion of the possible pairs of SOC 6-digit occupations. We exclude the occupations for which we have fewer than 500 observations in the BGT data (roughly the bottom 10% of occupations), resulting in 786 origin SOC 6-digit occupations in our data.

*within* each occupation. The over-representation of younger workers, however, might be a concern if younger workers tend to be more mobile or to have different occupational mobility patterns than older workers. We therefore adjust for the over-representation by age by re-weighting our observed occupational transitions to match the distribution of employment by age within each U.S. occupation, provided by the BLS for 2012-2017. We discuss the BGT resume data in more detail in Appendix A.2.

Five facts about occupational mobility. Is it sensible to use occupational transitions to infer the scope of workers' labor markets? Are job options outside the narrow occupation even an important part of most workers' labor markets? Using the BGT data, we document five stylized facts about occupational mobility which suggest the answer to both questions is yes.

1. Occupational mobility is high, suggesting that the SOC 6-digit occupation fails to capture many workers' true labor markets: the average probability of a worker leaving her 6-digit occupation when she leaves her job - the "occupation leave share" defined above - is 23%.
2. Mobility is heterogeneous across occupations, suggesting that the SOC 6-digit occupation is a better approximation of the labor market for some occupations than others: a quarter of occupations have a leave share lower than 19%, and a quarter higher than 28% (Table 1.1, Figure 1.2).[26]
3. Aggregating up the SOC classification hierarchy - which groups ostensibly similar occupations - still fails to capture most occupational transitions, suggesting that this cannot solve the market definition problem.[27]

[26] Almost all of the occupations with low leave shares are highly specialized, including various medical, legal and educational occupations (see Appendix Table A.4). In contrast, many high lave share occupations require more general skills, including restaurant hosts/hostesses, cashiers, tellers, counter attendants, and food preparation workers.

[27] For the median occupation, 87% of moves to a different 6-digit occupation are also to a different 2-digit occupation, but with substantial variation (see Table 1.1). For example, only 39% of systems software developers leave their 2-digit occupation group when they move across 6-digit occupations, compared to 95% of flight attendants. Note that management roles are often considered a separate 2-digit occupational group from non-management roles in the same field. Excluding transitions to and from management, at the median 67% of SOC 6-digit occupational transitions cross SOC 2-digit boundaries.

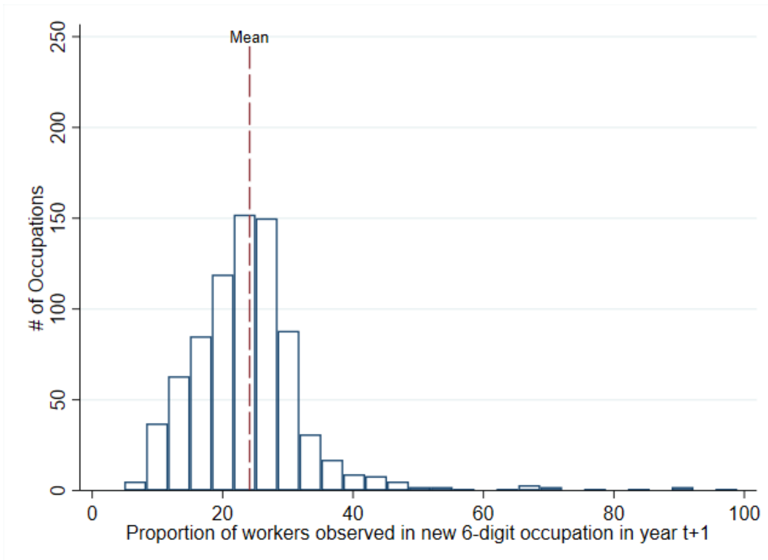
4. The occupational transition matrix is sparse, suggesting that workers' relevant labor markets are mostly comprised of only a few occupations, and is highly asymmetric, suggesting that the relevance of occupations as outside options is not symmetric across occupation pairs (unlike in many task- and skill-based measures of occupational similarity).[28]

5. Empirical occupation transitions reflect similarities between occupations in terms of their task requirements, wages, amenities, and leadership responsibilities, suggesting that occupational transitions do indeed reflect the underlying feasibility of an occupation as an outside option (see Figure 1.3).[29]

[28] See Appendix Figure A.7 and Appendix Table A.5). The asymmetry partly reflects the fact workers in an occupation with specialized skills may be able to move to occupations which require generalist skills (e.g. retail salespersons) but the reverse flow is less feasible.

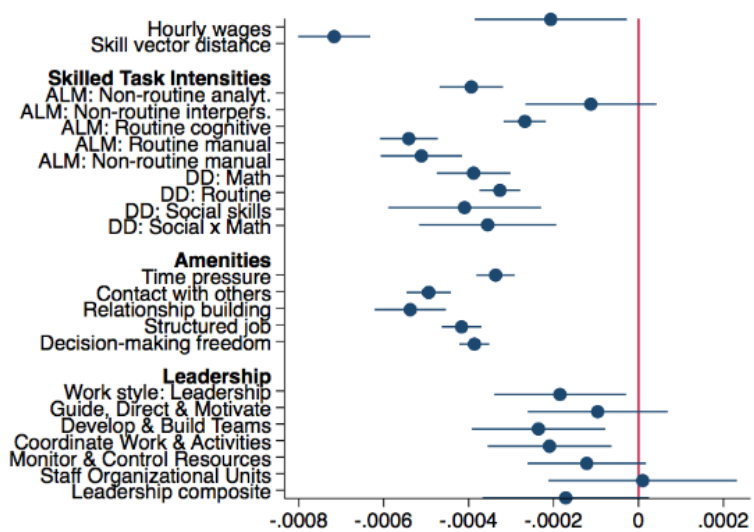
[29] To show this, we regress our measure of occupational transitions on a number of different occupational characteristics derived from the O*Net database: the vector difference in the importance scores for all "Skill" task content items (see Macaluso (2019)); task composites capturing the distinction between cognitive vs. manual, routine vs. non-routine task contents, and social skills, based on Autor et al. (2003) and Deming (2017); characteristics that proxy for flexibility on the job (Goldin, 2014), such as time pressure and the need for establishing and maintaining interpersonal relationships; and characteristics measuring leadership responsibilities. In every pairwise regression of occupational mobility on the absolute difference in characteristics (controlling for the difference in wages), the coefficients are significantly negative or statistically insignificant, as shown in Figure 1.3. Similarly, Macaluso (2019) finds that mobility between U.S. SOC 2-digit occupations is highly correlated with task similarity. See Appendix A.6 for more details on our analysis.

**Figure 1.2:** Outward occupational mobility from SOC 6-digit occupations




Note: "Occupation leave share" by occupation, for 786 occupations. Dashed line indicates sample mean.

**Figure 1.3:** Occupational transitions and occupational characteristic similarity




Note: Coefficients and 95% confidence intervals from regressions of occupation transition shares $\pi_{o \to p}$ on pairwise differences in occupational characteristics. See Appendix A.6 for more details.

**Table 1.1:** Summary statistics: BGT occupational mobility data

| Percentile (occ.) | 1 | 5 | 10 | 25 | 50 | 75 | 90 | 95 | 99 |
|---|---|---|---|---|---|---|---|---|---|
| *Panel A: Number of obs. in the BGT occ. mobility data in '000s, by occ. (2002-2015)* | | | | | | | | | |
| Observations | 0.6 | 1.1 | 1.6 | 4.9 | 20.8 | 112.3 | 466.8 | 853.9 | 3,471.9 |
| *Panel B: Share leaving job and occupation, by occ. (2002-2015)* | | | | | | | | | |
| Share in diff. job | 0.30 | 0.35 | 0.37 | 0.40 | 0.45 | 0.52 | 0.61 | 0.66 | 0.74 |
| Share leaving 6d. occ. | 0.047 | 0.062 | 0.074 | 0.90 | 0.10 | 0.12 | 0.14 | 0.18 | 0.29 |
| Leave share | 0.09 | 0.11 | 0.14 | 0.19 | 0.24 | 0.28 | 0.33 | 0.38 | 0.69 |
| *Panel C: Share of occupational transitions which cross SOC 2d boundary (2002-2015)* | | | | | | | | | |
| All occ. transitions | 0.55 | 0.65 | 0.70 | 0.79 | 0.87 | 0.93 | 0.97 | 0.98 | 1.00 |
| Excl. management | 0.40 | 0.48 | 0.51 | 0.59 | 0.67 | 0.75 | 0.80 | 0.83 | 0.87 |

Note: We exclude occupations with $<$500 observations in the BGT resume data. In Panel A, an observation is a person-year unit that is also observed in the data the following year. Panel B shows the share of workers observed in a new job or new occupation from one year to the next, and the "leave share", defined in section 1.4 as the share leaving their occupation conditional on leaving their job. Panel C shows the share – by origin occupation – of all SOC 6-digit occupational transitions which also span SOC 2-digit boundaries. The percentiles refer to percentiles across occupations, such that (for example) the median occupation in our data has 20,800 observations (Panel A).

Measuring outside-occupation options. The facts above suggest that we can use occupational transitions to infer workers' outside option occupations. Informed by our conceptual framework, we therefore define a measure of the outside option value of jobs in other occupations - the *outside-occupation option index* $oo^{occs}$ – as a weighted average of the wage in each alternative occupation, weighted by a measure of the likelihood that the worker will move to a job in each of those alternative occupations if she leaves her current job: $oo^{occs}_{o,k,t} = \sum_{p \neq o}^{N_{occs}} \text{Prob}(o \to p)_{o,k,t} \cdot w_{p,k,t}$, where subscripts refers to the city (k), current occupation (o), possible destination occupations (p), and year (t). To proxy for $\text{Prob}(o \to p)$, we use the product of two variables: (1) the national average empirical occupation transition share $\pi_{o \to p}$, and (2) the relative employment share of occupation p in city k compared to the national average, $\frac{s_{p,k}}{s_p}$.[30] Our empirical outside-occupation option index is therefore:

$$oo^{occs}_{o,k,t} = \sum_{p \neq o}^{N_{occs}} \pi_{o \rightarrow p} \cdot \frac{s_{p,k,t}}{s_{p,t}} \cdot \bar{w}_{p,k,t} \tag{1.8}$$

We construct this outside-occupation option index for each year 1999-2016 for as many SOC 6-digit occupations and cities as our data allows (using the BLS Occupational Employment Statistics (OES) to obtain relative employment shares $\frac{s_{p,k,t}}{s_{p,t}}$ and average wages $\bar{w}_{p,k,t}$). Summary statistics of our index are shown in Table 1.2.[31]

[30] The national occupation transition share proxies for the likelihood that, nationwide, the average worker's best job option outside her occupation would be in each other occupation p; the local relative employment share adjusts this for the local availability of jobs in each occupation p.

[31] Note: we use "cities" to refer to the CBSAs (metropolitan and micropolitan statistical areas) and NECTAs (New England city and town areas) for which data is available in the BLS OES. Of the possible 786,335 occupation-city cells, wage data in the BLS OES only exists for approximately 115,000 each year. The missing occupations and cities are primarily the smaller ones. To create a consistent panel of occupations over time we crosswalk SOC classifications over time: see Appendix A.4.

Table 1.2: Summary statistics: main data set

| Percentile (occ.-city) | 1 | 5 | 10 | 25 | 50 | 75 | 90 | 95 | 99 |
|---|---|---|---|---|---|---|---|---|---|
| *Panel A: Employer concentration HHI (2016)* | | | | | | | | | |
| HHI | 35 | 97 | 167 | 390 | 965 | 2,222 | 5,000 | 7,222 | 10,000 |
| HHI (emp-wt) | 10 | 22 | 33 | 81 | 221 | 536 | 1,357 | 2,256 | 5,556 |
| *Panel B: Outside-occupation option index* $oo^{occs}$ *(2016)* | | | | | | | | | |
| $oo^{occs}$ (2016) | 1.4 | 2.1 | 2.6 | 3.5 | 4.8 | 6.6 | 8.8 | 10.6 | 16.1 |
| $\frac{oo^{occs}}{wage}$ | 0.03 | 0.06 | 0.09 | 0.15 | 0.23 | 0.34 | 0.45 | 0.53 | 0.74 |
| $\frac{oo^{occs}}{wage}$, emp-wt | 0.06 | 0.11 | 0.14 | 0.23 | 0.34 | 0.45 | 0.55 | 0.63 | 0.77 |
| *Panel C: Occupation-city wages and employment (2016)* | | | | | | | | | |
| Employment | 30 | 40 | 50 | 90 | 220 | 670 | 1,980 | 3,920 | 14,410 |
| Mean hourly wage | 9.05 | 10.50 | 11.94 | 15.40 | 21.42 | 31.08 | 44.07 | 53.68 | 90.50 |
| Wage, emp-wt | 8.97 | 9.94 | 10.99 | 13.39 | 18.33 | 30.28 | 45.10 | 56.42 | 80.50 |
| *Panel D: national hourly wage distribution (2016) from BLS OES* | | | | | | | | | |
| Hourly wage | – | – | 9.27 | 11.60 | 17.81 | 28.92 | 45.45 | – | – |

Note: Panels A, B, and C show summary statistics for our main data set in 2016, calculated over all occupation-city-year cells for which we have wage data, a vacancy HHI, and an outside-occupation option index. This comprises 103,300 occupation-by-city labor markets and 109,366,900 workers (according to the BLS OES data). Panel D shows the national 10th, 25th, 50th, 75th, and 90th percentile of the hourly wage distribution according to the full BLS OES data set, for comparison.

## 1.5 Empirical Approach

Our analysis thus far suggests four testable predictions: (1) higher employer concentration reduces wages, (2) better outside-occupation options increase wages, (3) the wage-HHI relationship is stronger for occupations with limited outward mobility, and (4) the estimated wage-HHI relationship may be biased if within-occupation HHI is correlated with outside-occupation job options. In this section, we lay out our approach to evaluate these predictions.

Regression specification. Our baseline specification regresses the log of the average hourly wage in a SOC 6-digit occupation, metropolitan area ("city"), and year, on the log of the HHI of employer concentration ($HHI_{o,k,t}$), the log of our outside-occupation option index ($oo^{occs}_{o,k,t}$), and a set of occupation-by-year and city-by-year fixed effects ($\alpha_{o,t}$, $\alpha_{k,t}$):

$$\ln \bar{w}_{o,k,t} = \alpha + \alpha_{o,t} + \alpha_{k,t} + \gamma_1 \ln HHI_{o,k,t} + \gamma_2 \ln oo^{occs}_{o,k,t} + \xi_{o,k,t} \tag{1.9}$$

We also run this regression allowing the coefficients $\gamma_1$ and $\gamma_2$ on the HHI and outside-occupation option index to vary according to the occupation's degree of outward mobility in our BGT resume data (the "occupation leave share"), interacting $HHI_{o,k,t}$ and $oo^{occs}_{o,k,t}$ with an indicator variable for the applicable quartile of outward mobility of occupation o.

We run these regressions across the largest possible subset of U.S. occupation-city-year cells for which we can obtain all our data: Our full data set for our baseline regressions over 2013–2016 comprises 212,417 occupation-city-year observations.[32] We use BLS OES data for average hourly wages by occupation, city and year for the dependent variable $\bar{w}_{o,k,t}$. As discussed above, the HHI is constructed from BGT vacancy posting data and the outside-occupation option index is constructed from wage and employment data from BLS OES, and occupational transition shares from BGT resume data.[33]

[32]This includes 387 cities and 715 occupations, with 94,809 occupation-city labor markets appearing in at least one year from 2013–2016. We have data on the wage, HHI, and outside-occupation option index (*but not* the instruments) for a larger set of occupation-city-year labor markets. We calculate summary statistics and counterfactuals on this larger set.

[33]Our estimates of the effect of employer concentration on wages could inform analyses based either on bargaining models or models where employer concentration affects the wage via the elasticity of labor supply to the firm. However, our outside-occupation option index is informed specifically by a bargaining model. (Note also: a bargaining framework has different implications from a monopsonistic framework for employment).

Identification: employer concentration. Endogeneity issues may bias the estimated coefficients on the HHI. The direction of the bias is ambiguous: an increase in employer concentration could reflect the expansion of a highly productive large firm, which would result in higher employer concentration (expected to reduce wages) but also higher average productivity (expected to increase wages). Or, an increase in employer concentration could reflect a lack of local dynamism, with few new firms, which may lead to higher employer concentration alongside falling productivity.[34]

We therefore instrument for local labor market concentration, creating an instrumental variable which leverages differential local occupation-level exposure to national firm-level hiring, in a strategy which builds on both the "granular" instrumental variable approach (GIV) of Gabaix and Koijen (2020) (which uses plausibly exogenous idiosyncratic firm-level variation to instrument for changes in market-level aggregates), and on the shift-share 'Bartik' approach. Our strategy is based on the facts that (a) increases in local employer concentration are often driven by individual large firms growing, (b) these firms usually operate across many labor markets, (c) local labor markets are differentially exposed to different large firms, and (d) the employment growth of these large firms nationally is likely orthogonal to economic conditions in a specific local occupation.

Specifically, we note that the growth in local employer concentration in occupation o is a function of the growth in local occupational employment for each employer j, $g_{j,o,k,t}$ (leaving aside firm entry): $\Delta HHI_{o,k,t} = \sum_j \sigma^2_{j,o,k,t} - \sum_j \sigma^2_{j,o,k,t-1} = \sum_j \sigma^2_{j,o,k,t-1}\left(\frac{(1+g_{j,o,k,t})^2}{(1+g_{o,k,t})^2} - 1\right)$. The increase in local occupational employer concentration is a function both of initial concentration and of the growth rates of firm-level vacancies $g_{j,o,k,t}$ relative to overall vacancy growth in the labor market $g_{o,k,t}$.

[34]Concerns like these are raised in many of the critiques of the empirical literature which finds a negative correlation between local employer concentration and wages, including Berry et al. (2019) and Rose (2019). Rose (2019) argues that empirical strategies attempting to identify a causal effect of employer concentration on wages must isolate the effect of employer concentration from changes in labor demand; our identification strategy attempts to do this. Hsieh and Rossi-Hansberg (2019) show that over recent decades large national firms have expanded into more local labor markets, reducing local employer concentration and possibly increasing productivity.

We use the leave-one-out firm-level national vacancy growth in occupation o (which we denote $\tilde{g}_{j,o,t}$) to instrument for local firm-level vacancy growth in occupation o ($g_{j,o,k,t}$), constructing our instrument for the HHI, $Z^{HHI}_{o,k,t}$, as:

$$Z^{HHI}_{o,k,t} = \log\left(\sum_j \sigma^2_{j,o,k,t-1}\left(\frac{(1+\tilde{g}_{j,o,t})^2}{(1+\tilde{g}_{o,k,t})^2} - 1\right)\right) \tag{1.10}$$

where $\tilde{g}_{o,k,t} = \sum_j \sigma_{j,o,k,t-1}\tilde{g}_{j,o,t}$ is the predicted local growth rate in vacancies, as predicted from the national (leave-one-out) growth of hiring in occupation o by each local firm j.[35] The key assumptions for this instrument to be valid are that the firm's national leave-one-out vacancy growth is (i) correlated with its local vacancy growth, but (ii) uncorrelated with the determinants of occupation-specific productivity growth in any given local labor market k. Through the lens of shift-share instruments (Borusyak et al., 2018), our instrument features plausibly exogenous 'shocks' (a function of firms' national hiring growth), and possibly endogenous exposure 'shares' (the last-period local occupational vacancy shares of each of those firms).[36]

One concern with this instrumental variable is that differential local exposure to national firms' growth may differentially affect total labor demand, not just employer concentration. In our model, the effect of a large firm's growth on local labor market concentration is quadratic, whereas the effect of a large firm's growth on local labor demand or productivity is linear. Thus, we control for (1) the growth rate of local vacancies in the occupation-city labor market ($g_{o,k,t} = \sum_j \sigma_{j,o,k,t-1} g_{j,o,k,t}$), and (2) the predicted growth rate of local vacancies based on large firms' national growth (i.e. the

[35]Note that by taking the log of the instrument, we implicitly exclude observations where the predicted change in HHI based on national firm-level growth is negative. Note also that we are instrumenting for the local level of the HHI with an instrument derived from an expression for the change in the HHI.

[36]For intuition, a hypothetical example: assume that in Bloomington IL, State Farm has a large employment share of insurance sales agents, while in Amarillo TX employment is dispersed across several insurance companies. In years where State Farm grows substantially faster than other insurance companies nationwide, under most assumptions about how that growth is allocated geographically, employer concentration of insurance sales agents will grow by more in Bloomington IL than in Amarillo TX.

direct linear analog to our concentration index: $\tilde{g}_{o,k,t} = \sum_j \sigma_{j,o,k,t-1}\tilde{g}_{j,o,t}$ as defined above). With these controls, we should be estimating the effect of a change in local labor market concentration due to changes in large firms' employment, holding constant any direct linear effect on local labor demand or productivity.[37]

A second concern is bias due to the fact that our exposure 'shares' do not sum to one. As such, following Borusyak et al. (2018) we introduce an "exposure control": the sum of the squared vacancy shares of local firms j which have *any* growth in other metro areas $\sum_j \sigma^2_{j,o,k,t-1} \cdot 1[\tilde{g}_{j,o,t} \neq 0]$.[38] We further discuss identification conditions in Appendix A.7.

Third, our instrument is unlikely to be strong for small changes in employer concentration in initially unconcentrated labor markets – if each firm has only a trivial share of local employment, even substantial hiring growth will not much change local employer concentration. We therefore apply our estimates of the effect of employer concentration on wages only to local labor markets with above-median employer concentration.

Notwithstanding these caveats, we see our approach as a substantial new contribution with regard to the problem of estimating the effect of employer concentration on wages. Some recent empirical work instruments for changes in employer concentration in a given local occupation with changes in (the inverse of) the number of employers in the same occupation in other local areas (e.g. Azar et al. (2020a,b); Gibbons et al. (2019); Marinescu et al. (2019); Qiu and Sojourner (2019); Rinz (2018)). This circumvents some endogeneity concerns, but a concern remains that national

[37] Controlling for national trend exposure directly to prevent it from confounding a nonlinear IV is similar to the "double Bartik" approach in Chodorow-Reich and Wieland (forthcoming). While the assumption that these linear terms capture demand effects is relatively strong, note that their inclusion does not affect our baseline coefficient estimates (Appendix Tables A.9 and A.11). The most plausible demand-related threat to our identification is that the growth of a large national firm, locally, pushes up wages by more in areas with inelastic labor supply to the local occupation than in areas with elastic labor supply to the local occupation, and that the elasticity of labor supply to the local occupation is correlated with the initial local employment share of the large national firm in that occupation.

[38] This controls for the fact that different local occupations may have different initial shares of employment accounted for by large national firms.

occupation trends in concentration may be correlated with unobservable national trends in occupational productivity, demand, or supply, which could confound estimated wage effects.[39] Our strategy allows us to use occupation-year fixed effects to control for national occupation-level factors which affect wages. Other recent empirical work uses M&A activity to generate plausibly exogenous variation in local labor market concentration, including Arnold (2020) for all industries, and Prager and Schmitt (2019) for hospital mergers. This avoids endogeneity concerns about the cause of the change in concentration, but reflects one specific source of concentration (M&A activity accounts for less than 2% of changes in local employer concentration (Arnold, 2020)) and cannot fully isolate the effects of employer concentration from other local economic effects of the M&A activity. Our approach allows us to examine the effects of various sources of variation across broad swathes of the US labor market, and to control at least somewhat for effects on local labor demand.[40] Ultimately, we believe that this set of complementary identification approaches – based off different variation, and with different strengths – can together provide a useful picture of the effects of employer concentration on wages.

Identification: outside-occupation options. Endogeneity issues may also bias the coefficients on our outside-occupation option index: a positive local demand shock for an occupation similar to a worker's own may come at the same time a positive local demand shock for her own occupation (driven, for example, by a common product market shock or a regulatory change). In addition, there is a reverse causality problem: if occupation p and occupation o are good outside options for each other, then a wage increase in o will increase wages in p and vice versa. To identify causal effects, we need exogenous shocks to the wages in workers' outside-occupation options which do not affect, and are not affected by, the local wages in their own occupation.

We use a 'Bartik' shift-share approach, instrumenting for local wages in each outside option oc-

[39]The authors control for variables like labor market tightness to address this.

[40]Dodini et al. (2020) adopt an additional different strategy, demonstrating that workers laid-off in mass layoffs see larger wage losses in more concentrated labor markets in Norway.

cupation p in city k with the leave-one-out national mean wage for occupation p excluding its wage in city k ($\bar{w}_{p,\not{k},t}$). We also instrument for the local relative employment share in each occupation using the initial employment share in that occupation in 1999, the first year for which we have data ($\frac{s_{p,k,1999}}{s_{p,1999}}$).[41] Our instrument for the $oo^{occs}$ index is:

$$Z^{oo}_{o,k,t} = \sum_{p}^{N_{occs}} \left( \pi_{o \to p} \cdot \frac{s_{p,k,1999}}{s_{p,1999}} \cdot \bar{w}_{p,\not{k},t} \right) \tag{1.11}$$

The identifying variation within a given occupation across different cities comes from differences in each city's initial occupational employment composition. Identifying variation over time within the same occupation-city cell comes from national (leave-one-out) changes over time in wages of local outside-option occupations.[42] For our instrument to be valid, the national leave-one-out mean wage $\bar{w}_{p,\not{k},t}$ in outside option occupation p must be correlated with the local wage of occupation p in location k (relevance condition), but must not affect the local wage in initial occupation o through a direct channel other than increasing the quality of local outside-occupation options (*conditional* on controlling for occupation-year and city-year fixed effects).[43] We discuss conditions for identification further in Appendix A.7, following the approach to shift-share IVs of Borusyak et al. (2018).

[41] Or the first year the occupation-city is in the data, if it is not present in 1999.

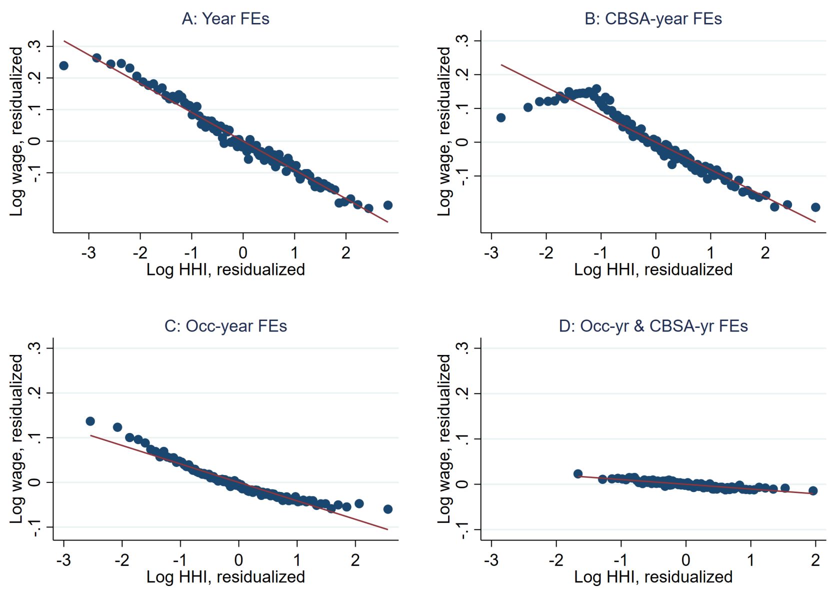
[42] That is, in a year when there is a national wage shock to one of occupation o's outside option occupations p, cities which had a higher proportion of their jobs in occupation p in 1999 should see bigger increases in the wage of occupation o (because they were more exposed to the shock to their outside options). This instrumental variable strategy is closely related to that of Beaudry et al. (2012), who use national industry wage premia to substitute for city-level industry wages when estimating spillover effects of cities' industrial composition.

[43] The inclusion of these fixed effects means that differences in city-level trends or national productivity of different occupations do not represent an issue for our identification strategy. An additional concern may be that groups of local occupations that share similar labor markets experience similar location-specific industry shocks. We show that our results are robust to controlling for common exposure to industry shocks (see Appendix Table A.9).

## 1.6 Results: employer concentration

There is a robust negative correlation between log vacancy HHIs and log wages at the occupation-city level (Figure 1.4). In a regression with occupation-year and city-year fixed effects, the OLS relationship is strongly statistically significant, with a coefficient of -0.010 (Table 1.3, column *a*).[44]

**Figure 1.4:** Correlations between wage and HHI




Note: Figure shows binned scatter plots of log wages and log within-occupation HHI index for occupation-CBSA cells over 2013–2016, residualized on different combinations of fixed effects. Slopes for the line of best fit on each graph are: A: -0.09, B: -0.08, C: -0.04; D: -0.01.

When instrumenting for the HHI, the coefficient magnitude increases by around 40% relative to the OLS specification (Table 1.3, column *c*). This suggests some combination of omitted variable bias or measurement error biasing the coefficient toward zero in simple OLS regressions of wages on

[44] Similarly Hershbein et al. (2019) find a coefficient of -0.014, regressing wages on vacancy concentration in local SOC 6-digit occupations over 2010–2017. Other papers' estimates are not directly comparable because of different labor market definitions or wage measures.

HHI.[45] We also introduce a control for our outside-occupation option index (Table 1.3, columns *b* and *d* for OLS and 2SLS respectively). The coefficient on the instrumented outside-occupation option index is positive and highly statistically significant, confirming that outside-occupation job options matter for wages. After introducing the outside-occupation option index the coefficient on the HHI falls by about a third in the OLS regressions and about a fifth in the IV regressions, consistent with omitted variable bias. This is because the vacancy HHI is negatively correlated with workers' outside-occupation options: workers with worse options *within* their occupation also have worse options *outside* their occupation (as illustrated in Appendix Figure A.13).

How big is the average effect of employer concentration on wages? Our baseline coefficient estimates in Table 1.3, column *d* – instrumenting for both employer concentration and outside-occupation job options – suggest that going from the HHI faced by the median worker to the HHI faced by the worker at the 95th percentile (from an HHI of 221 to 2,256) would be associated with a 2.6 log points lower hourly wage.[46] This is at the low end of the estimates reviewed in Marinescu and Hovenkamp (2019).[47] As noted, we caution against applying these coefficient estimates to labor markets with very low initial levels of employer concentration, since our instrument is weak in these cases.[48]

Heterogeneity by occupational mobility. Re-running our baseline regression, but allowing the coefficients on the HHI and outside-occupation option index to vary for occupations with different degrees of outward occupational mobility, we find that the average effect of within-

[45] The first stage is shown in Table A.7, column (a).

[46] Calculated as $(\ln(2256) - \ln(221)) \cdot -0.011 = -0.026$.

[47] Reviewing existing evidence, they suggest that a 10% increase in employer concentration (at the SOC 6-digit occupation by commuting zone level) leads to a 0.3% to 1.3% decrease in wages. Our point estimate in Table 1.3, column *d* suggests a 10% increase in concentration (at the SOC 6-digit occupation by MSA level) leads to a 0.11% decrease in wages on average.

[48] Figure A.15 illustrates that the correlation between our (log) HHI instrument and the (log) HHI for occupation-city labor markets in 2016 somewhat breaks down for occupation-city cells with a very low value of the HHI or of our HHI instrument. Similarly, regressing wages only on occupation-city cells with very low HHIs results in a much weaker instrument (with, for example, a Kleibergen-Paap F-statistic of 14 for occupation-city cells with HHIs less than 50) and no significant relationship between wages and HHI.

**Table 1.3:** Regression of wage on HHI and $oo^{occs}$, full sample

| *Dependent variable:* | Log wage | | | |
|---|---|---|---|---|
| | (a) OLS | (b) OLS | (c) 2SLS IV | (d) 2SLS IV |
| Log HHI | -0.010 | -0.007 | -0.014 | -0.011 |
| | (0.001) | (0.001) | (0.003) | (0.003) |
| Log outside-occ. options | | 0.106 | | 0.095 |
| | | (0.007) | | (0.009) |
| Vacancy growth | | | -0.001 | -0.001 |
| | | | (0.001) | (0.001) |
| Predicted vacancy growth | | | -0.011 | -0.013 |
| | | | (0.010) | (0.010) |
| Exposure control | | | 0.008 | 0.004 |
| | | | (0.008) | (0.007) |
| Observations | 184,411 | 184,411 | 184,411 | 184,411 |
| F-Stat | | | 705 | 379 |

Note: Heteroskedasticity-robust standard errors clustered at the city level shown in parentheses. Units of observation are 6 digit SOC by city by year, for all observations with available data over 2013–2016 inclusive. All regressions feature occupation-by-year and MSA-by-year fixed effects. Columns (a) and (b) show OLS regressions and columns (c) and (d) show 2SLS IV regressions (where both the log HHI and log outside-occ. option index are instrumented). The reported F-stat for the 2SLS IV regressions is the Kleibergen-Paap Wald F statistic. The vacancy growth and predicted vacancy growth variables are rescaled by dividing by 10 (so that 1% vacancy growth in a local area corresponds to a value of 0.001, rather than 0.01), such that the coefficient estimates can be seen in the table for most specifications. See text for detailed explanation of instruments and controls.

**Table 1.4:** Regression of wage on HHI and $oo^{occs}$, by quartile of occupation leave share

| *Dependent variable:* | Log wage | | | |
|---|---|---|---|---|
| | (a) OLS | (b) OLS | (c) 2SLS IV | (d) 2SLS IV |
| Log HHI X Q1 occ mobility | -0.015 | -0.014 | -0.024 | -0.025 |
| | (0.002) | (0.002) | (0.004) | (0.005) |
| Log HHI X Q2 occ mobility | -0.013 | -0.007 | -0.016 | -0.010 |
| | (0.001) | (0.001) | (0.003) | (0.003) |
| Log HHI X Q3 occ mobility | -0.011 | -0.005 | -0.013 | -0.007 |
| | (0.001) | (0.001) | (0.003) | (0.004) |
| Log HHI X Q4 occ mobility | -0.002 | 0.002 | -0.005 | 0.000 |
| | (0.001) | (0.001) | (0.003) | (0.004) |
| Log outside-occ. options X Q1 occ mobility | | 0.083 | | 0.060 |
| | | (0.009) | | (0.011) |
| Log outside-occ. options X Q2 occ mobility | | 0.109 | | 0.100 |
| | | (0.007) | | (0.009) |
| Log outside-occ. options X Q3 occ mobility | | 0.115 | | 0.107 |
| | | (0.007) | | (0.010) |
| Log outside-occ. options X Q4 occ mobility | | 0.114 | | 0.109 |
| | | (0.007) | | (0.010) |
| Vacancy growth | | | -0.001 | -0.001 |
| | | | (0.001) | (0.001) |
| Predicted vacancy growth | | | -0.012 | -0.016 |
| | | | (0.010) | (0.010) |
| Exposure control | | | 0.008 | 0.004 |
| | | | (0.008) | (0.008) |
| Observations | 184,411 | 184,411 | 184,411 | 184,411 |
| F-stat | | | 164 | 87 |

Note: Heteroskedasticity-robust standard errors clustered at the city level shown in parentheses. Independent variables labelled "X Qi outward mobility" show the coefficient on an interaction term between the HHI or outside-occupation option index (respectively) with an indicator variable which takes the value 1 if the occupation in question is in the ith quartile of outward occupational mobility (where "Q1" represents the least outwardly mobile occupations, and so on). See text for detailed explanation of variables. See notes to Table 1.3 for further details on regression specification.

occupation employer concentration on wages conceals substantial heterogeneity (Table 1.4, Figure 1.5).[49] For the quartile of occupations with the lowest outward mobility, as proxied by our occupation "leave share", our coefficient estimate suggests that going from the median to the 95th percentile HHI faced by workers would be associated with 5.8 log points lower wages.[50] For the quartile of occupations with the highest outward mobility, the point estimate is zero, and the confidence interval suggests that an equivalent increase in the HHI would be associated with at most a 1.8 log point lower wage.[51]

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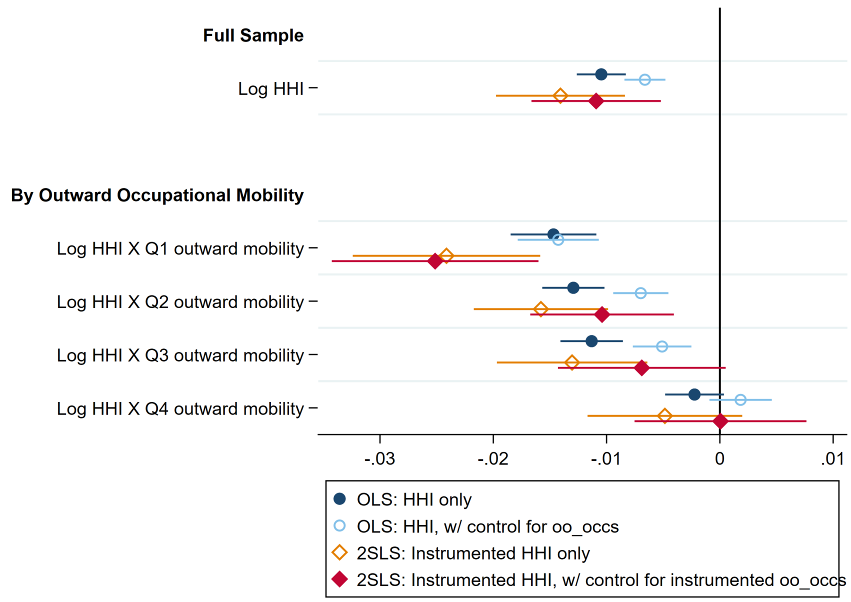
[49] In Appendix Tables A.14 and A.15, we explore whether there is evidence for heterogeneity of the effect of concentration on wages for different quartiles of the wage distribution, or for different occupation groups, but do not find patterns which are clearly statistically significantly different across these groups. We also do not find conclusive evidence of non-linearity in the concentration-wage relationship when we estimate the effect separately by quartile of initial employer HHI (Appendix Table A.13).

[50] Calculated as $(\ln(2256) - \ln(221)) \cdot -0.025 = -0.058$.

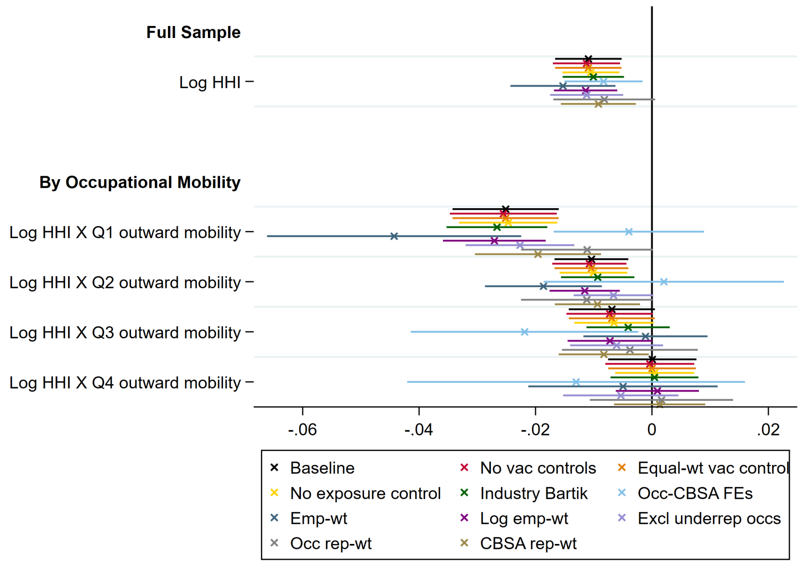
[51] Calculated as: $(\ln(2256) - \ln(221)) \cdot 1.96 \cdot -0.004 = -0.018$. The pattern of our results are consistent with Prager and Schmitt (2019), who find that hospital mergers which induce large increases in concentration reduce nursing and pharmacy workers' wages substantially, somewhat suppress wages of non-medical hospital professionals, and have no detectable effect on wages for the remainder of hospital workers (in maintenance and repairs, operations, housekeeping, catering, and medical records). They interpret these differentials as reflecting the degree to which workers have industry-specific skills. Our estimates would similarly suggest that nursing and pharmacy workers would experience substantially higher wage effects of employer concentration than maintenance, housekeeping, and catering workers, since the former tend to have lower occupational mobility.

**Figure 1.5:** Coefficients on wage-HHI regressions




Note: Coefficients on log HHI and 95% confidence intervals from our baseline regressions of occupation-CBSA wages on employer HHI. Navy and light blue represent OLS regression coefficient of wages on HHI, with and without control for outside-occupation job options respectively. Orange and red represent 2SLS regression coefficient of wages on instrumented HHI with and without control for (instrumented) outside-occupation job options respectively. Top panel presents coefficients for the full sample (as in Table 1.3); bottom panels present the coefficients estimated separately by quartile of outward occupational mobility (as in Table 1.4). Regressions use annual data for occupation-by-CBSA labor markets over 2013-2016, and include occupation-year and CBSA-year fixed effects as well as controls described in the text. Standard errors clustered at CBSA level.

**Figure 1.6:** Coefficients on wage-HHI regressions: robustness checks




Note: Coefficients on log HHI and 95% confidence intervals from our baseline 2SLS IV regressions of occupation-CBSA wages on instrumented employer HHI, across various robustness checks (as in Appendix Tables A.9-A.12).

Robustness checks. We explore a number of additional variations on our baseline analyses (Figure 1.6, Appendix Tables A.9-A.12). First, we show that our coefficient estimates are similar if we remove the controls for vacancy growth (Tables A.9 and A.11 column *a*), if we remove the exposure control (column *c*), or if we follow Gabaix and Koijen (2020) in adding an additional control for the equal-weighted vacancy growth of local firms ($g^{e}_{o,k,t} = \frac{1}{N}\sum_{j}^{N} g_{j,o,k,t}$) to reflect common local occupation-specific shocks (column *b*). Second, we control for an industry Bartik shock to control for common exposure to national industry trends (column *d*), and similarly find little change in our estimates. Third, we re-run our baseline regressions with fixed effects for occupation-city and year, rather than occupation-year and city-year (column *e*). Here, identifying variation comes from year-to-year changes in employer concentration within the same occupation-city labor market. In the full sample, we see a statistically significant negative point estimate of the same order of magnitude as our baseline estimates, but estimates by quartile of outward occupational mobility are noisy (which may not be surprising given the short time period). Fourth, we show that our coefficient estimates are similar if we weight each occupation-city cell by employment or log employment (Appendix Tables A.10 and A.12, columns *a* and *b*), assuaging concern that our results may have been driven by small occupations or cities. Fifth, to address concerns about representativeness in our BGT vacancy data, we re-run our baseline regression estimates *excluding* any occupations which are substantially underestimated in the vacancy data (column *c*),[52] finding coefficient estimates almost identical to those in our baseline regressions, and also weight each occupation or metropolitan area respectively by its represented-ness in the BGT data (columns *d* and *e*).

## 1.7 Results: outside-occupation options

We can also use our baseline regressions to ask: how big are the effects of outside-occupation options on wages? Our baseline coefficient estimate (Table 1.3 column *d*) suggests that a 10 log point higher outside-occupation option index is associated with roughly 1 log point higher wages in workers' own occupation. This implies that moving from the 25th to the 75th percentile value of outside-occupation options across cities for the median occupation leads to 3.7 log points higher wages.[53] This is quite large in the context of the geographic variation of wages: for the median occupation, the interquartile range of average wages across cities in 2016 was 21 log points.[54] In addition, find-

[52] We exclude occupations with a 'represented-ness' less than 0.5 in the BGT vacancy data, corresponding to about one third of occupations.

[53] We calculate this by estimating the interquartile range of the outside-occupation option index for each occupation across cities in 2016, taking the median across occupations, and applying our coefficient estimate of 0.95. With the same exercise for employer concentration, we find that for the median occupation, moving from the 25th to the 75th percentile HHI across cities would result in a wage increase of 1.4 log points.

[54] For a specific example where outside-occupation options might be relevant, consider Baltimore, MD, and Houston, TX. They are a similar size with a similar average hourly wage, but statisticians in Baltimore earned 26% more than statisticians in Houston in 2016. Applying our baseline coefficient estimate suggests that around 20% of this difference may be attributable to differential availability of outside-occupation job options.

ing a large, significant, and positive effect of shocks to outside-occupation options on wages reinforces our conclusions that workers' true labor markets are broader than their narrow 6-digit SOC occupations, and that our "probabilistic" method of identifying relevant outside options can capture workers' true labor markets relatively well.[55]

Robustness checks. There may be concerns our coefficients are biased by exposure to correlated industry shocks which affect both a workers' own occupation and her outside option occupations.[56] To control for this possibility we construct a shift-share "industry Bartik" shock and include it in our baseline regressions.[57] The industry Bartik shock has a significantly positive effect on wages, but our coefficients on the outside-occupation option index are only slightly attenuated (Appendix Tables A.9 and A.11, column *d*).

In addition, while our HHI data only covers 2013–2016, we can calculate our outside-occupation option index from 1999 onwards. Over this longer period, we find large, positive, and significant effects of outside-occupation options on wages, even with both occupation-by-city and occupation-by-year fixed effects (Appendix Table A.16). We also find large effects if we calculate the outside-occupation option index using occupational mobility at the SOC 2-digit or 3-digit level instead of 6-digit level (Appendix Table A.17); if we split our analysis into three time periods (Appendix Table A.18); if we weight by employment (Appendix Table A.19), or if we control for local occupational employment (Appendix Table A.20).

[55] While we do not consider the effects of outside-city options on wages in this paper, our methodology could easily be extended to do so. The wage effect of local employer concentration and outside-occupation options is limited by workers' option to move.

[56] For example, if (1) the finance industry and the tech industry are disproportionately likely to employ both accountants and data scientists, (2) San Francisco has a large share of tech employment while New York has a large share of finance employment, and (3) being a data scientist is a good outside option occupation for an accountant, then in years where tech is booming nationwide, this will impact SF more than NY. Accountants in SF will see wages rising by more than accountants in NY but this may be driven simply because more accountants in SF already work in tech.

[57] The shock is constructed such that the exposure of occupation o in city k to each industry ι is defined as the employment share of industry ι in occupation o nationwide, multiplied by the employment share of industry ι in city k. See Appendix A.7.

## 1.8 Discussion and Implications

What might our results suggest about the aggregate effects of employer concentration? We use our coefficient estimates in a rough back-of-the-envelope quantification of the "wage effect" of employer concentration in each above-median HHI labor market in 2016, relative to a scenario where their HHI is reduced to 200 (roughly the median in our data in 2016),[58] as follows:

$$\text{wage effect}_{o,k,t} = \big(\log(\text{HHI})_{o,k,t} - \log(200)\big) \cdot \gamma_1^q \tag{1.12}$$

where $\gamma_1^q$ denotes the estimated coefficient on the log(HHI) in our baseline regression specification in Table 1.4 column *d*, for the appropriate quartile q of outward occupational mobility. Note that this exercise considers the effect of changes in employer concentration *holding all else constant*, including local productivity. It can illustrate the degree to which wages may be marked down from local occupational productivity as a result of employer concentration, but cannot necessarily illustrate what would happen if a specific policy or business decision were to change local employer concentration (as it might also change local productivity). It also rests on the assumption that we can apply our estimated coefficients linearly.

Roughly 49 million of the 110 million workers in our data set are in occupation-city labor markets with an HHI greater than 200. Of these, our counterfactual wage exercise suggests that roughly 3 million workers have wages which are 5% lower or more as a result of above-median employer concentration, and a further 9 million have wages which are 2%-5% lower. This suggests that perhaps 10% of the US labor market sees wage suppression of 2% or more as a result of employer concen-

[58] An HHI of 200 might correspond to roughly 50 equal-sized employers, or two large employers each with 10% of workers and an atomistic 'fringe' of firms employing the rest. This level of concentration is not typically thought to be a concern in product markets. Note: There may be monopsony power even in unconcentrated labor markets arising from employer heterogeneity or search frictions (Naidu and Posner, 2020).

tration.[59] We show in Table 1.5 the average estimated wage effect for different combinations of employer concentration and outward occupational mobility. Our estimates suggest that employer concentration slightly widens inequality: the share of workers affected by employer concentration is smaller than average in the highest quartile of the wage distribution and in high-wage cities (Appendix Figures A.18 and A.19). Note that our estimates focus only on wages, but employer concentration may also affect non-wage benefits and workplace amenities.[60]

Which occupations are most affected by employer concentration? In Table 1.6, we list the twenty-five occupations with the largest number of workers who see an estimated wage effect of 2% or greater in their local occupational labor market (excluding occupations which are substantially under-represented in the BGT vacancy data). A large share of the most-affected occupations are in healthcare, including more than one million registered nurses, licensed practical and vocational nurses, and nursing assistants, and around 200,000 pharmacists and pharmacy technicians.[61] According to our estimates, large numbers of security guards and hairdressers, hairstylists, and cosmetologists are also affected by employer concentration, as large shares of their local labor markets are comprised of employment by a few large companies or chains (although, note that both occupations are somewhat underrepresented in our vacancy data).[62]

[59] This may be an overestimate since the figure include some occupations which are underrepresented in the BGT vacancy posting data. Excluding all occupations with a 'represented-ness' of less than 0.5 in the BGT vacancy data, there remain 7.1 million workers (6.5% of workers in our data) whose wages are suppressed by at least 2% as a result of employer concentration. On the other hand, our data only covers 110 million of the 140 million nonfarm employees in 2016, and those not represented in our data are disproportionately in non-metropolitan areas or small occupations. One might expect these workers to face greater wage suppression from employer concentration than the average in our data.

[60] Qiu and Sojourner (2019) and Marinescu et al. (2020) find negative relationships between employer concentration and the receipt of employment-based health insurance, and labor rights violations respectively.

[61] This is in keeping with recent work that has found large effects of hospital mergers on wages of nursing and pharmacy workers (Prager and Schmitt, 2019), and a low elasticity of the labor supply of registered nurses to individual hospitals (Staiger et al., 2010).

[62] Note also that we consider employer concentration at the level of a salon chain, many of which are franchised: one might argue that it is better to consider employer concentration at the level of individual franchised salons, though non-compete and no-poaching agreements may make this distinction moot in practice.

**Table 1.5:** Counterfactual wage effects of setting HHI to 200 (& number affected)

| | 2,500< HHI <10,000 | 1,500< HHI <2,500 | 500< HHI <1,500 | 200< HHI <500 | 0< HHI <200 |
|---|---|---|---|---|---|
| Lowest mobility | 7.8% | 5.8% | 3.6% | 1.1% | 0 |
| | (1.2m) | (1.5m) | (5.2m) | (5.2m) | (8.0m) |
| Q2 mobility | 3.1% | 2.3% | 1.4% | 0.4% | 0 |
| | (1.3m) | (1.2m) | (5.2m) | (7.6m) | (18.3m) |
| Q3 mobility | 2.2% | 1.6% | 1.0% | 0.3% | 0 |
| | (1.1m) | (0.9m) | (4.3m) | (9.4m) | (19.0m) |
| Q4 mobility | 0 | 0 | 0 | 0 | 0 |
| | (1.4m) | (1.1m) | (4.6m) | (7.2m) | (5.6m) |

Note: This table shows the estimated wage impact (and number of people affected in parentheses) of lowering the HHI to 200 in all occupation-city cells where it was greater than 200 in 2016. 200 is roughly the median HHI as experienced by workers. The estimated wage impact is calculated as the difference between the actual log HHI and the log of 200, multiplied by the estimated coefficient in our wage-HHI regressions (with the coefficient used corresponding to the appropriate quartile of occupational outward mobility, as estimated in Table 1.3 column (d)). The impact number in each cell in the table is the average impact across all workers in that cell: so, for example, for the 1.2 million workers in our data who are in occupations in the lowest quartile of outward mobility, and who are in occupation-city labor markets with an HHI greater than 2500, the average estimated wage impact of employer concentration on their wage is 7.8%. Note (1) this exercise implicitly holds productivity constant, and (2) our data set covers around 110 million workers in total, from the BLS OES occupation-by-metropolitan area employment and wage data.

**Table 1.6:** Twenty-five occupations with most people affected by employer concentration (based on a predicted occupation-CBSA wage effect of 2% or greater)

| Occupation | National employment | Share of occupation w/ estimated effect 2% or greater | Number in occupation w/ estimated effect 2% or greater |
|---|---|---|---|
| Security guards | 1,103,120 | .88 | 969,230 |
| Registered nurses | 2,857,180 | .19 | 545,520 |
| Nursing assistants | 1,443,150 | .34 | 488,040 |
| Hairdressers, hairstylists, and cosmetologists | 352,380 | .88 | 311,500 |
| Nonfarm animal caretakers | 187,360 | .86 | 161,540 |
| Fitness trainers and aerobics instructors | 257,410 | .62 | 158,310 |
| Childcare workers | 569,370 | .25 | 144,290 |
| Licensed practical and licensed vocational nurses | 702,400 | .19 | 131,840 |
| Radiologic technologists | 200,650 | .64 | 128,600 |
| Pharmacists | 305,510 | .38 | 117,360 |
| Emergency medical technicians and paramedics | 244,960 | .46 | 112,290 |
| Medical and clinical laboratory technologists | 166,730 | .55 | 92,510 |
| Phlebotomists | 120,970 | .75 | 90,780 |
| Pharmacy technicians | 398,390 | .21 | 84,360 |
| Medical assistants | 623,560 | .13 | 81,300 |
| Respiratory therapists | 126,770 | .62 | 78,360 |
| Management analysts | 637,690 | .11 | 68,230 |
| Lawyers | 619,530 | .11 | 68,200 |
| Librarians | 129,350 | .52 | 66,730 |
| Dentists, general | 105,620 | .62 | 65,730 |
| Software developers, applications | 794,000 | .082 | 64,990 |
| Bakers | 180,450 | .36 | 64,300 |
| First-line supervisors of personal service workers | 190,420 | .32 | 61,720 |
| Real estate sales agents | 151,840 | .4 | 60,190 |
| Massage therapists | 95,830 | .61 | 58,820 |

Note: This table lists the twenty-five occupations with the largest number of workers who experience an estimated wage impact of 2% or more as a result of employer concentration (see Table 1.5 for description of how this effect is calculated). We exclude "teachers and instructors, all other" since the classification of this group in the BGT data relative to OES may not be fully comparable, and since many teachers are public sector employees. We also exclude occupations that are very under-represented in the BGT vacancy data relative to overall employment (with a cutoff with represented-ness$<$0.5, or around the 33rd percentile). In Appendix Table A.23 we list the degree of represented-ness of each of these occupations in the BGT vacancy data.

Our back-of-the-envelope exercise suggests that while employer concentration suppresses wages for several million workers, the majority of American workers likely do not experience significant wage suppression as a result of employer concentration. Thus, policymakers should focus attention on the subset of workers who face both concentrated labor markets within their occupation and limited opportunities for occupational mobility.

Implications: antitrust. One area where this can be done is antitrust.[63] Marinescu and Hovenkamp (2019) and Naidu et al. (2018) argue that antitrust authorities should use measures of employer concentration as a preliminary screen for anticompetitive effects of mergers in labor markets (as they already do in product markets). Our analysis suggests that this screen should involve two variables: the HHI in a local 6-digit SOC occupation, and the degree of outward mobility from that occupation.[64]

However, it is important to note that our findings *do not* tell us that *all* increases in employer concentration reduce wages. In some cases, if higher employer concentration comes alongside higher productivity, workers' wages may be higher in the high-concentration high-productivity scenario than a low-concentration lower-productivity scenario, so seeking to reduce employer concentra-

[63] Several scholars have called for antitrust authorities to pay attention to employer concentration (Hemphill and Rose, 2017; Hovenkamp, 2018; Krueger and Posner, 2018; Marinescu and Hovenkamp, 2019; Naidu et al., 2018; Steinbaum and Stucke, 2020). Historically antitrust authorities paid little attention to employer concentration (though monopsony is referred to in the 1992 DoJ-FTC Horizontal Merger Guidelines (Phillips, 2019)), but this has changed in recent years: the topic has featured in FTC and DoJ hearings, the FTC is expanding its retrospective merger review to scrutinize labor market power, and the FTC raised concerns about wage suppression for nurses in a September 2020 public comment on a proposed hospital merger in Hendrick TX.

[64] The screen should also evaluate whether employer concentration in outside-occupation options will be affected by the merger – a concern in occupations whose outside options are predominantly in the same industry, like healthcare. Our proposal differs slightly from Marinescu and Hovenkamp (2019), who argue that antitrust authorities should screen for anti-competitive effects of mergers based only on the HHI in a local SOC 6-digit occupation. Our finding that the wage suppressive effects of employer concentration are at least four times higher for low outward mobility occupations suggests that screening based only on local within-occupation HHI without considering outward occupational mobility will lead to some mergers being scrutinized which may have little effect on wages, while others which may have serious anti-competitive effects may go unnoticed.

tion may not be the best response: close scrutiny of individual cases, and industry- and occupation-specific studies, are necessary to understand whether antitrust action would be appropriate in any specific circumstance.[65] In addition, while increased antitrust scrutiny of labor markets is important, it is unlikely to affect the majority of workers impacted by employer concentration (Naidu and Posner, 2020), since most changes in employer concentration are not caused by mergers and acquisitions and many concentrated labor markets do not feature illegal anti-competitive practices.

Implications: policy to raise wages. In many cases, rather than seeking to reduce employer concentration it may be more appropriate to recognize the fact that employer concentration may give large firms scope to pay a wage which is marked down relative to productivity – and to design labor market policies to counteract this. One such way to do this might be equipping workers with countervailing power by bolstering support for collective bargaining.[66] An alternative might be strengthening minimum wages or benefits standards in local labor markets characterized by high employer concentration.[67]

Implications: promoting mobility. Our results suggest that employer concentration within a local occupation matters less if workers can find similarly good jobs outside their occupa-

[65] As emphasized by Hovenkamp (2018), Berger et al. (2019), and Arnold (2020). Naidu et al. (2018) argue that antitrust authorities should permit mergers where the incremental increase in workers' wages because of increased productivity would *outweigh* any incremental decrease in workers' wages induced by the increase in employer concentration.

[66] In our conceptual framework outlined in section 1.2, higher worker bargaining power $\beta$ reduces the weight placed on the outside option in the wage bargain and therefore reduces the importance of employer concentration in wage determination. There is some suggestive empirical evidence that labor markets with higher unionization rates see smaller effects of employer concentration on wages. When we re-run our baseline regression of the effect of employer concentration on wages with an interaction with states' right-to-work status – a proxy for the ease of forming a union – we find larger effects of both employer concentration and outside-occupation options on wages in right-to-work states (Appendix Table A.21). Prager and Schmitt (2019) find larger effects of hospital mergers on nursing wages when nursing unionization rates are lower and in right-to-work states, and Benmelech et al. (2018) find a stronger relationship between employer concentration and wages in U.S. manufacturing firms where unionization rates are lower.

[67] Indeed, higher minimum wages, would be expected to have less of a negative effect on employment in labor markets where employers have monopsony power. Azar et al. (2019b) find that US labor markets with higher employer concentration see smaller employment effects of minimum wage increases.

tion – and, most likely, if workers can easily move geographically. This suggests that policies which make it easier to switch occupation and/or to work in different geographic areas may – by increasing workers' outside options – reduce the degree to which employer concentration can suppress wages.[68] These could include reducing any disproportionate barriers to acquiring training, licensing, or certification in occupations, increasing reciprocal recognition of state-specific licenses and certifications, and increasing affordable housing supply in high-cost cities. In addition, restrictions on worker mobility *within* an occupation (like non-compete clauses) could exacerbate the effects of employer concentration on wages.[69]

Incidence of employer concentration effects. Our estimates suggest that increases in employer concentration reduce local wages, but cannot tell us whether the incidence of these wage reductions falls on firms in the form of higher profits, or consumers in the form of lower prices (and the balance likely depends on the nature of product market competition). Similarly, Kahn and Tracy (2019) argue that the ultimate incidence of local labor market concentration falls to a large extent on local landowners as lower local wages reduce local rents and house prices. Understanding the ultimate incidence of these effects is important to determine the appropriate policy response.

## 1.9 Conclusion

Our findings point to a middle ground between two prominent views about the effects of employer concentration in the US labor market. On the one hand, employer concentration is *not* a niche issue confined to a few factory towns: we find large, negative, and significant effects of employer concentration on wages when estimated using nuanced market definitions and plausibly exogenous

[68] Indeed, the decline in occupational and geographic mobility in the U.S., which may partly reflect an increase in costs of mobility, could be acting to increase the effects of employer concentration (Molloy et al., 2011; Xu, 2018).

[69] See Johnson and Kleiner (2020) on the effect of state licensing standards on mobility, Ganong and Shoag (2017) on the effect of housing costs on mobility, and Johnson et al. (2020); Starr et al. (2019) on the effect of non-competes.

variation across the majority of the US labor market, and our back-of-the-envelope calculations suggest that perhaps 10% of the U.S. private sector workforce experiences non-trivial wage effects of employer concentration. On the other hand, most workers are not in highly concentrated labor markets, and the effects of employer concentration therefore do not seem big enough to have a substantial effect on the aggregate wage level or degree of income inequality in the U.S. economy (though other sources of monopsony power may still be important).[70] The fact that employer concentration affects wages for several million American workers suggests that increased policy attention to this issue is appropriate, in terms of antitrust, policies to raise wages, and policies to increase worker mobility. For these policy decisions, our work underscores that the definition of the labor market is vitally important.

[70] Similarly, Rinz (2018), Berger et al. (2019), and Lipsius (2018) show that employer concentration has fallen over recent decades in most local industries, casting doubt on the argument that changing employer concentration can explain median pay stagnation or rising income inequality. It is possible, however, that the decline in countervailing worker power has exposed firms' latent monopsony power, meaning that employer concentration (and other sources of monopsony power) have greater wage effects than in the past (Erickson and Mitchell, 2007; Naidu et al., 2018; Stansbury and Summers, 2020).

# 2

# The Declining Worker Power Hypothesis

*Co-authored with Lawrence H. Summers.*

## 2.1 Introduction

Since the early 1980s in the US, the share of income going to labor has fallen, measures of corporate valuations like Tobin's Q have risen, average profitability has risen even as interest rates have declined, and measured markups have risen. Over the same time period, average unemployment has fallen very substantially, even as inflation has stayed low with no sign of accelerating – suggesting a decline in the NAIRU.

We argue that the decline in worker power has been the major structural change responsible for these economic phenomena. A decline in worker power, leading to a redistribution of rents from labor to capital, would predict a fall in the labor income share, an increase in Q, corporate profitability, and measured markups, and a fall in the NAIRU. In this paper, we estimate the magnitude of the decline in worker rent-sharing in the U.S. over recent decades, show that it is large enough to be able to explain the entire decline in the aggregate labor share and a substantial fraction of the decline in the NAIRU, and show that at the state and industry level, declines in worker power are consistent with changes in labor shares, unemployment, and measures of corporate profitability. Our focus on the decline in worker power as one of the major structural trends in the U.S. economy is in line with a long history of progressive institutionalist work in economics, sociology, and political science, exemplified by Freeman and Medoff (1984), Levy and Temin (2007), Bivens, Mishel, and Schmitt (2018), Kristal (2010), Rosenfeld (2014), and Ahlquist (2017). As an explanation for these recent macro trends, we believe that the evidence for the declining worker power hypothesis is at least as compelling as – and likely more compelling than – the other commonly-posited explanations: specifically, technological change, globalization, and rising monopoly or monopsony power.[1] While it is possible that globalization or technological change caused the decline in the labor share, it is difficult to reconcile each of these purely competitive explanations with the rise in Q, average profitability, and measured markups over recent decades (which suggest an increase in economic rents

[1] For recent papers arguing that different aspects of globalization or technological change can explain the decline in the U.S. labor income share, see for example Elsby, Hobijn, and Sahin (2013), Karabarbounis and Neiman (2014), Abdih and Danninger (2017), Autor and Salomons (2018), Acemoglu and Restrepo (2018), and Autor, Dorn, Katz, Patterson, and Van Reenen (2020). For papers arguing that rising monopoly power can explain the decline in the labor share and/or rising corporate valuations and markups, see Barkai (forthcoming), Gutiérrez and Philippon (2017), (2019), Eggertsson. Robbins and Wold (2019), Farhi and Gourio (2018), De Loecker, Eeckhout, and Unger (2020). For arguments that rising monopsony power could play a role in these trends, see CEA (2016), Furman and Krueger (2016), Glover and Short (2018), Benmelech, Bergman, and Kim (2018), Philippon (2020). For work on the role of the decline in worker power in the declining U.S. labor share, see Elsby, Hobijn, and Sahin (2013) and Abdih and Danninger (2017), who both find some role for the decline in unionization, but argue that it is not the dominant factor, and Kristal (2010) and Jaumotte and Osorio Buitron (2015), who argue that differential declines in worker power across countries can explain differential patterns of change in the labor share and income inequality.

accruing to capital owners). Alternatively, while it is possible that rising monopoly or monopsony power caused the decline in the labor share, and these would also be natural explanations for the rise in Q, average profitability, and measured markups, it is more difficult to reconcile rising monopoly or monopsony power with the decline in the NAIRU.

What do we mean by declining worker power? We consider the American economy to be characterized by three types of power, to varying degrees: monopoly power, monopsony power, and worker power. Firms' monopoly power – arising from explicit barriers to entry, or from innate features of particular product markets, such as heterogeneous production technologies or short-run fixed costs – generates pure profits or rents. Firms' monopsony power in the labor market – arising from labor market concentration and/or labor market frictions – result in an upward sloping labor supply curve to the firm, enabling the wage to be marked down to some degree below the marginal revenue product. Worker power – arising from unionization or the threat of union organizing, firms being run partly in the interests of workers as stakeholders, and/or from efficiency wage effects – enables workers to increase their pay above the level that would prevail in the absence of such bargaining power.[2] This power gives workers an ability to receive a share of the rents generated by companies operating in imperfectly competitive product markets, and can act as countervailing power to firm monopsony power.

In this framework, therefore, a decline in worker power results in a *redistribution of product market rents from labor to capital owners*.

What caused this decline in worker power? The decline in worker power in the U.S. economy over recent decades was a result of three broad shifts. First, institutional changes: the policy environment has become less supportive of worker power by reducing the incidence of unionism and the credibility of the "threat effect" of unionism or other organized labor, and the real value of the

[2] We use worker power as synonymous with bargaining power, rent-sharing power, and insider-outsider power.

minimum wage has fallen. Second, changes within firms: the increase in shareholder power and shareholder activism has led to pressures on companies to cut labor costs, resulting in wage reductions within firms and the "fissuring" of the workplace as companies increasingly outsource and subcontract labor.[3] And third, changes in economic conditions: increased competition for labor from technology or from low-wage countries has increased the elasticity of demand for US labor, or, in the parlance of bargaining theory, has improved employers' outside option. In this paper, we emphasize the relative importance of the first two factors. While globalization and technological change surely did play some role in the decline in worker power, the cross-sector and cross-country evidence suggests that they are unlikely to have been the most important factors: within the US, unionization has declined at similar rates across both tradable and non-tradable industries, and the decline in the US unionization rate has been much more pronounced than in many other countries (all exposed, to some extent, to similar international trends in technology and globalization).

We start our analysis in Section 2.2 by examining the empirical evidence of a decline in worker power. Most notable is the decline of the private sector union membership rate, from over one third at its peak in the 1950s to 6% today. In addition, the private sector union wage premium has declined somewhat since the early 1980s, suggesting that unionized workers are less able to share in the rents created by firms than they were in the past.

A different type of evidence of the importance of labor power comes from the fact that even without unions, workers may receive wage premia in other settings. Workers in larger firms, and in certain industries (like manufacturing, mining, telecommunications, and utilities), receive substantially higher wages relative to observably equivalent workers in smaller firms or in other industries, and evidence suggests that these large firm and industry wage differentials to a large extent reflect rents. But workers' ability to receive rents in large firms, or in high-rent industries, appears to have declined. Using the CPS, we show that since the 1980s there has been a decline of about one third

[3] For a detailed exposition of this trend, see Weil (2014).

in the large firm wage premium, and a decline of about one third in the dispersion of industry wage premia.

A further source of evidence that worker power has been attenuated is the apparent decline in the relationship between workers' pay and the profitability, revenues, and/or product market power of their firm or industry. In a classically competitive labor market, workers' pay is determined by the marginal product of labor within their labor market, and there should be no correlation between a worker's pay and their firm's or industry's performance. In practice however, there is a positive relationship (suggesting a degree of rent-sharing). We show that the strength of this relationship has diminished over time: in manufacturing industries, the degree to which increases in revenue productivity translate into higher pay has declined since the 1960s,[4] and we find suggestive evidence of a broad-based weakening in the relationship between industrial concentration and pay across sectors.

So, a large body of evidence points to a decline in worker power. But how big is this decline, in macroeconomic terms? In Section 2.3, we use our estimates of the union wage premium, large firm wage premium, and industry wage premia to quantify the magnitude of the decline in total rents going to labor over 1982-2016. We demonstrate that labor rents are an important macroeconomic phenomenon, and that they have declined substantially: from 12% of net value added in the non-financial corporate business sector in the early 1980s to 6% in the 2010s. (This is likely an under-estimate, since we cannot quantify explicitly the decline in labor rents caused by the rise of activist shareholders). This decline in labor rents is largely due to changes that have taken place within in-

[4]Note that this is a different issue than in Stansbury and Summers (2019). In Stansbury and Summers (2019), we investigate the degree to which there is a relationship between changes in productivity and changes in compensation at the level of the whole economy. We find a close to one-for-one relationship between changes in productivity and pay at the level of the whole economy over the postwar period, which has not attenuated since the 1970s/80s. This finding could be consistent either with competitive or with imperfectly competitive labor markets, and is not inconsistent with our finding that the relationship between productivity and pay at the industry level has weakened (which indicates a decline in the degree of rent-sharing within different industries).

dustries, rather than changes that have taken place across industries as employment has shifted from manufacturing to services.

The decline in labor rents could have been driven by two things: either a destruction of rents available to be shared (as product market competition increased, perhaps as a result of globalization), or a redistribution of rents from labor to capital. Industry-level evidence tends to suggest that the decline in labor rents was largely a result of the latter: the majority of industries which saw substantial declines in rents to labor also saw substantial increases in profits to capital over 1987-2016,[5] and in manufacturing – the sector with the biggest decline in the labor share – the manufacturing industries with the greatest exposure to low-wage import competition were not the industries with the biggest declines in labor rents.

In Section 2.4, we demonstrate that the trends in factor shares, corporate profitability, Q, and measured markups that have sometimes been attributed to rising monopoly power can be equally or more convincingly explained by our hypothesis of declining worker power. We begin by replicating the recent decomposition exercise of Farhi and Gourio (2018). Farhi and Gourio suggest that trends in factor shares, the profitability of capital, the investment-capital ratio, the risk-free rate, and other macroeconomic variables, can be explained by an increase in average markups – alongside rising risk premia and increased unmeasured intangibles. In this framework, they estimate that average markups in the U.S. rose from 7% to 15% over the 1980s to the 2000s. While their analysis makes it clear that there are changes that cannot be explained by a perfectly competitive model, we note that there is essentially no way in their framework to distinguish between the rise in markups they posit (indicating a rise in monopoly and/or monopsony power), and a fall in worker power. Modifying their decomposition, we show that our hypothesis of declining worker power – holding markups constant – can explain the macro facts in the model equally well.

[5] Our industry-level analysis spans 1987-2016, the longest period with data for consistent NAICS industries.

Next, we take our measure of the magnitude of lost labor rents (calculated in Section 2.3 from union wage premia, large firm wage premia, and industry wage premia) to the aggregate data on the nonfinancial corporate sector. We show that our estimate of the decline in labor rents – at roughly 6% of nonfinancial corporate sector value added since the 1980s – is big enough to (over-)explain the entire decline in the net labor share. At the state level, our measure of the decline in the labor rent share is predictive of changes in the labor share over 1984-2016.

We then compare trends in labor rents, labor shares, profitability, and measures of Q for 51 industries (at roughly the NAICS 3-digit level). We show that industries with larger declines in labor rents over 1987-2016 had much larger declines in their labor shares and increases in their average profitability. In horserace regressions, industry-level labor rents have substantially more power to explain changes in labor shares, profitability, and Q, than measures of product market concentration (which have been used as indicators of a rise in monopoly power).

In Section 2.5, we argue that the decline in worker power would be consistent with another highly salient aspect of the macro experience of recent decades: the substantial decline in both average unemployment and average inflation. The unemployment rate was below 5%, the level previously thought to have been the NAIRU, for nearly half of the twenty-three years from 1997 to 2020, and was below 4% from May 2018 until February 2020, at levels not reached since the 1960s. At the same time, inflation has been low and has shown little sign of accelerating. These facts suggest that there has been a quite substantial decline in the NAIRU, and/or a flattening of the Phillips Curve.

Almost all models of declining worker power predict a fall in the NAIRU, as the decline in the cost of labor increases firms' hiring, and/or as "wait unemployment" falls. In keeping with these predictions, we show that states and industries with bigger falls in worker power over the last four decades saw bigger falls in their unemployment rate. Extrapolation from our analysis of state-level unemployment rates suggests that the aggregate change in worker power could be big enough to explain a large fraction of the decline in the NAIRU. (We further verify this conclusion with informal calculations in the Appendix, drawing on various models of the relationship between worker power and the NAIRU). We note, on the other hand, that an increase in monopoly power offers no explanation for the decline in the NAIRU. If anything it has usually been thought to act in the other direction: in the presence of downward nominal wage rigidity, rising monopoly power would tend to predict rising prices (as firms transition to a new equilibrium of higher markups and higher prices) alongside a rise in unemployment (as the rise in monopoly power leads to a restriction in output). Increasing monopsony power would tend to be associated with less, rather than more, hiring and so does not provide a natural explanation for a declining NAIRU. And globalization and technological change, while possibly disinflationary, would tend to increase average unemployment by increasing disruption and structural change in the economy, making their implications for the NAIRU ambiguous.

In Section 2.6, we address possible objections to the declining worker power hypothesis. First, we show that the apparent weakness of investment relative to fundamentals – which has been a major motivator of the monopoly power argument – can be reconciled with our hypothesis. Second, we show that recent research emphasizing the importance of between-firm reallocation in explaining changes in factor shares is consistent with the declining worker power hypothesis. Third, we note that our measure of labor rents does not incorporate any increase in rents which may have accrued to the highest earners – such as executives, or top earners in finance – and should be thought of as a measure of the decline in the rents accruing to the *majority* of workers.[6] Fourth, we argue that the rise in occupational licensing has likely not played a major role in the trend in aggregate labor rents over recent decades. Finally, we note that the decline in the labor share has been much more pronounced in the U.S. than other industrialized economies similarly exposed to globalization and technological change, and that the decline in the labor share has been most pronounced in U.S. manu-

[6] This is because our measure of labor rents is estimated in the CPS, which is top-coded for high earners and has higher non-response rates for these groups.

facturing, which (given increasing globalization) is not an industry where a large rise in monopoly power seems likely to have occurred. We also note that there is little evidence of any large increase in import-adjusted sales concentration in manufacturing, or in local-level sales concentration in services, and that local labor market concentration has declined over time. Together, these suggest to us that globalization, technological change, or rising monopoly or monopsony power alone lack the ability to explain recent economic developments in a unified way.

While the focus of this paper is on the distribution of rents between labor and capital, we note that the decline of labor rents has also likely increased inequality in labor incomes: the declines in unionization and the real value of the minimum wage, and the fissuring of the workplace, affected middle- and low-income workers more than high-income workers, and some of the lost labor rents for the majority of workers may have been redistributed to high-earning executives (as well as capital owners). Consistent with these hypotheses, we show that the decline in labor rents was larger for non-college-educated workers than for college-educated workers, and estimate, in a back-of-the-envelope exercise, that the decline in labor rents could account for a large fraction of the increase in the income share of the top 1% over recent decades.

Overall, we conclude that the decline in worker power is one of the most important structural changes to have taken place in the U.S. economy in recent decades. Our emphasis on the decline of worker power is justified both by the strength of the direct evidence, and by its ability to provide a unified explanation for a variety of macroeconomic phenomena: changes in labor and capital incomes, profitability, and the NAIRU.

This raises important challenges for policy. If a major feature of the U.S. economy were a rise in monopoly or monopsony power, reducing restrictiveness and increasing competition in markets could improve both efficiency and equity. But if, as we argue, the major explanation of the decline in the labor share and rise in corporate profitability is a decline in worker power, then measures to restrict monopoly or monopsony power alone – or indeed, to restrict globalization or technological change – may do little to reverse this trend. More profoundly, if markets are innately characterized by some degree of imperfect competition and rents, then completely eliminating all sources of market power may not be feasible. Instead, if increases in the labor share are to be achieved, institutional changes that enhance workers' countervailing power – such as strengthening labor unions or promoting corporate governance arrangements that increase worker power – may be necessary (but would need to be carefully considered in light of the possible risks of increasing unemployment).

## 2.2 Evidence of declining rent-sharing in US labor markets

Why do firms share rents with workers? There are three groups of reasons. First, workers may be able to lay claim to rents directly, either as a result of explicit bargaining power through unions, implicit bargaining power through the threat of union organizing (Freeman and Medoff 1984), or another ability to wield power within the firm. Second, some firms may be run partly in the interests of workers as stakeholders, rather than solely in the interests of shareholders. Third, it may be in firms' interests to share rents with workers for efficiency wage reasons – where workers are paid an above-market wage to incentivize effort (e.g. Yellen 1984) – or to maintain morale (perhaps as a result of fairness norms, as in Akerlof and Yellen (1986)). Efficiency wages may also play a role in reducing the cost to firms of paying above-market wages: if worker productivity increases when wages rise, then some of the extra cost of sharing rents with workers is offset by productivity benefits (Bulow and Summers 1986, Summers 1988).[7]

Evidence from a wide range of sources has demonstrated the existence of rent-sharing in the U.S. labor market. Unionized workers, workers at large firms, workers in specific industries, and workers at certain firms receive substantial wage premia relative to observably equivalent workers. Sim-

[7] The rents received by workers may be 'true' rents or pure profits generated by a firm's monopolistic power in the product market – or, they may be 'quasi' rents generated by sunk investments (Grout 1984, Caballero and Hammour 2005), or by the cost of recruiting new workers either in a frictional labor market or in a setting where job-specific training is required (e.g. Mortensen and Pissarides 1999, Manning 2003).

ilar wage premia also exist for workers who switch jobs, suggesting they do not reflect unobserved worker characteristics. These wage premia tend to be positively correlated with indicators of rents at the firm and industry level, including profits and concentration, and inversely correlated with quit rates (both of which are suggestive of rent-sharing). In addition, there is evidence of sizeable passthrough of industry- or firm-level shocks to productivity and profits into workers' compensation. And there is a large body of work documenting persistent wage losses for displaced workers, which partly reflect lost rents.[8]

Over recent decades however, a number of forces have likely reduced labor rents in the U.S., particularly for lower-wage workers. Most obvious have been the decline in unionization and union bargaining power, and the erosion of the real value of the minimum wage. In addition, the increase in shareholder activism and the rise of the shareholder value maximization doctrine increased the power of shareholders relative to managers and workers, likely increasing pressure on firms to cut labor costs and, in particular, to redistribute rents from workers to shareholders.[9] The increased 'fissuring' of the workplace, with outsourcing of non-core business functions, may be an outgrowth of this phenomenon (Weil 2014). In this section, we present a range of empirical evidence of this decline in rent-sharing.

[8] We briefly review evidence on union, industry, and firm size wage premia later in this section. For evidence on firm-specific wage premia, see Groshen (1991), and Davis and Haltiwanger (1991), and the large AKM literature starting with Abowd, Kramarz, and Margolis (1999). Estimates from the AKM literature suggest that firm effects and the covariance of worker and firm effects can explain 17-20% of the variance of wages (Abowd, Lengermann, and McKinney 2003, Abowd, McKinney, and Zhao 2018, Song et al 2019), and that around one-third of this reflects rents (Sorkin 2018). For evidence on wage losses for displaced workers, see (e.g) Jacobson, Lalonde, and Sullivan (1993), Davis and Von Wachter (2011), and Lachowska, Mas, and Woodbury (2018), among others.

[9] See, for example, Shleifer and Summers (1988), who argue that a primary effect of hostile takeovers is the redistribution of value to shareholders from other stakeholders. Some evidence consistent with this mechanism can be found in Davis, Haltiwanger, Handley, Lipsius, Lerner, and Miranda (2019), who find that wage premia in target firms were largely erased after private equity buyouts.

### 2.2.1 Declining unionization rates

Unions are the clearest-cut example of workers having rent-sharing power. Unionized workers receive significantly higher wages than observationally equivalent nonunion workers, with most estimates of the private sector union wage premium between 15% and 25% (Rosenfeld 2014).[10] But the ability of workers to share in rents through unions has declined substantially in recent decades. Private sector union membership gradually declined from a peak of around one third in the 1950s to 24% in 1973, and then declined more rapidly, reaching 6% in 2019 (Figure 2.1, Rosenfeld 2014, Hirsch and Macpherson 2019).[11] In addition, estimates of the union wage premium suggest that it has declined since the early 1980s.[12]

Note that the impact of unions on workers' ability to receive rents likely extended beyond the workers who were unionized receiving wage premia. In industries where pattern bargaining was common, non-unionized firms would match the wage increases in union contracts (with the most famous example being the 1950 "Treaty of Detroit"). Even without pattern bargaining, the "threat effect" of unionization of workers in nonunion firms likely incentivized firms to offer better wages and benefits than they otherwise would have (e.g. Leicht 1989, Farber 2005, Denice and Rosenfeld 2018).[13] And union bargaining power may have more generally supported norms of equity in pay

[10] Empirical evidence is consistent this wage premium representing a redistribution of rents from capital to labor. For example, Abowd (1987) finds substantial evidence to support a dollar-for-dollar tradeoff between workers and shareholders in union contract settlement data. Lee and Mas (2012) show that new unionization reduces firms' equity value. If this represents a redistribution of rents from capital to labor, the magnitude of the average effect they find would be consistent with a 10% union wage premium.

[11] The measured decline in the unionization rate may be an underestimate: as the unionization rate approaches zero, misclassification bias tends to produce inflated estimates (Card 1996, Western and Rosenfeld 2011).

[12] We estimate the union log wage premium for private sector workers in the CPS-ORG, regressing the log hourly wage on a dummy variable for union membership or coverage and controls for education, demographics, geography, occupation, and industry (More details in Appendix B.1). Our estimate falls from 21 log points in 1982 to 15 by 2019. These are both within the historical range over the 20th-21st century as estimated by Farber et al (2018).

[13] Unions may also raise wages for non-union workers in frictional labor markets as employers raise wages to retain the ability to hire easily (Manning 2003). On the other hand, unions may have negative spillovers

structures (Western and Rosenfeld 2011).

The decline in unionization rates and union bargaining power was driven by a combination of institutional factors, which weakened labor law and its enforcement, and economic factors, which increased the elasticity of demand for labor and so weakened workers' ability to bargain for higher wages. Institutional factors included the breakdown of pattern bargaining in the 1980s, the expansion of the number of right-to-work states, and decreasing political support for and enforcement of labor laws.[14] Economic factors that reduced worker bargaining power included increased import competition for manufactured goods and deregulation of transportation and telecoms, both of which reduced firms' abilities to compete while paying high wages (Peoples 1998, Levy and Temin 2007, Rosenfeld 2014). Note, however, that these economic factors are unlikely to have been the main drivers in the decline in U.S. unionization: the proportional decline in the unionization rate from the mid-1980s to the mid-2000s was almost identical across a range of sectors which had very different exposures to globalization, technological change, and deregulation over the period in question (manufacturing, mining, transportation and utilities, retail trade, construction, and wholesale trade), and the rate of unionization has declined much more quickly in the U.S. than in most other industrialized economies, despite similar trends in globalization and technology (Schmitt and Mitukiewicz 2012, Denice and Rosenfeld 2018).[15]

on the wages of nonunion workers if the union raises wages but restricts employment in the union sector (e.g. Oswald 1982). Overall, though, evidence suggests a positive correlation between unionization rates and non-union wages, suggesting that union spillovers are on net positive (Farber 2005, Leicht 1989, Neumark and Wachter 1995, Denice and Rosenfeld 2018, Fortin, Lemieux, and Lloyd 2019).

[14]See, for example, Levy and Temin (2007) and Rosenfeld (2014). Workers' ability to organize was reduced both by a direct weakening of labor law and labor law enforcement, and by an increased corporate use of union avoidance tactics (Bronfenbrenner 2009, McNicholas et al 2019). The 'fissuring' of the employment relationship has also decreased workers' ability to organize: workers employed as independent contractors, or employees in franchises, often have their terms of employment to some extent dictated by the end employer or franchisor (respectively), but lack the legal ability to collectively bargain with that end employer (see e.g. Paul 2016, Steinbaum 2019).

[15]See Appendix B.3 for unionization rates by industry. Note also that, while Acemoglu, Aghion and Violante (2001) argue that the decline of unionization was endogenous, driven by skill-biased technological change, Farber et al (2018) find that the pattern of decline of U.S. union membership is unlikely to be

### 2.2.2 Declining large firm wage premium

A large body of literature shows that large firms pay workers higher wages than their otherwise equivalent counterparts at smaller firms.[16] While this firm size effect could be driven by a number of different causes – workers with higher unobserved productivity, compensating differentials, a greater propensity to pay efficiency wages, a decision to pay higher wages to fill vacancies faster – several studies have found that even when attempting to account for these possibilities a large unexplained firm size premium often remains (e.g. Brown and Medoff 1989). This implies that some substantial portion of the large firm wage premium reflects rents to labor.[17] Over recent decades, however, the large firm wage premium has fallen (Hollister 2004, Even and Macpherson 2014, Cobb and Lin 2017, Song et al 2019). Estimating the large firm wage effect for observably equivalent private sector workers over 1990-2019 from the CPS-ASEC, we find a substantial decline in wage premia for workers at firms with 500 or more employees, relative to workers at small firms (Figure 2.2),[18] likely indicating a decline in rent-sharing. (To interpret it as something other than a decline in rent-sharing, there must have been either a substantial reduction in compensating differentials as small firms became relatively worse to work at, or a reduction in the sorting of highly productive workers into large firms.) Note that if large firms' monopoly power had systematically increased over recent decades without any change in worker rent-sharing power, the large firm wage premium would have been expected to increase rather than decrease.

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consistent with this.

[16] See e.g. Brown and Medoff (1989), Bulow and Summers (1986), Davis and Haltiwanger (1996).

[17] And is consistent with large firms being more likely to have product market power – and so, rents.

[18] We run log wage regressions on dummies for firm size and various demographic, occupation, and location controls. We obtain estimated for the firm size wage effects for workers at firms of 1000+, 500-999, and 100-499 workers, relative to firms with <100 workers. We regress on 5-year pooled samples as the sample size is too small for precise annual estimates. See Appendix B.1 for more details.

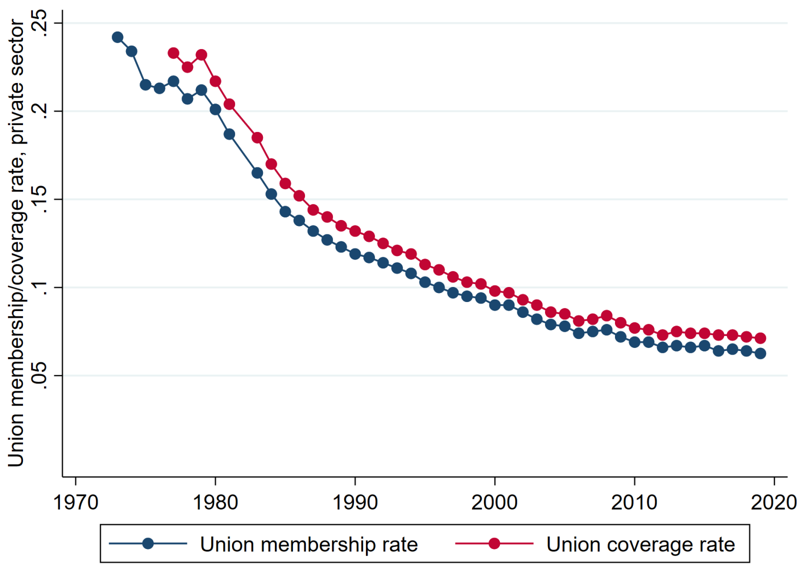


**Figure 2.1:** Union membership and coverage rates, private sector
Note: Union membership and coverage rate are from UnionStats.com, calculated by Hirsch and Macpherson.

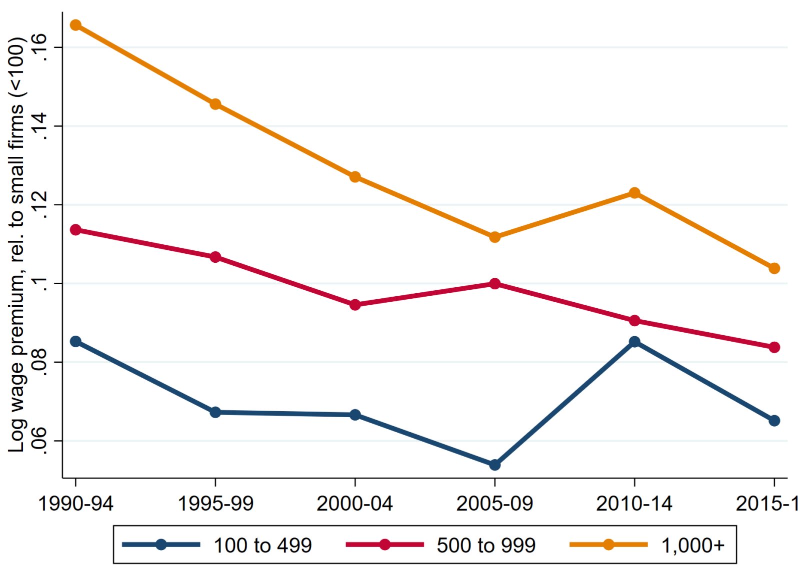


**Figure 2.2:** Large firm wage effect, private sector
Notes: Large firm wage premium is estimated for firms with 100-499, 500-999, and 1,000+ employees in the CPS ASEC for five-year periods over 1990-2019, controlling for education, demographics, geography, occupation, industry, and union status. More details on estimation procedures are in the text and in Appendix B.1.

### 2.2.3 Declining variance of industry wage differentials

A large body of work on the inter-industry wage structure, over several decades, has found substantial and persistent dispersion of wages across industries for observably similar workers. Evidence suggests that industry wage differentials to a large extent reflect rent-sharing with workers: the wage differentials persist even when accounting for worker productivity differences and compensating differentials, and are correlated with industry-level profitability, concentration, and capital-labor ratios (see for example Dickens and Katz 1987, Krueger and Summers 1988, Katz and Summers 1989, Gibbons and Katz 1992, Abowd et al 2012).[19]

Using the CPS-ORG, we estimate industry wage differentials for private sector workers in each year over 1984-2019. We regress log wages on a set of industry dummies at different levels of aggregation (18 sectors, 77 industries, or 250 detailed industries) alongside controls for education, demographics, geography, and occupation, and union membership/coverage.[20] This gives us a set of estimated wage fixed effects for each industry. If rent-sharing with labor has declined in recent decades, we would expect the variance of industry wage premia to have declined. As Figure 2.3 shows, this is the case, at all levels of industry aggregation.[21]

[19]Further evidence that these premia indicated the presence of rents included the fact that wage premia for workers in different occupations in the same industry were highly correlated, and that industries with higher wage premia tended to have lower quit rates and higher ratios of applicants to job openings (shown in the previously mentioned studies, as well as Slichter (1950), Ulman (1965) and Holzer, Katz, and Krueger (1991)). More recently, Abowd et al (2012) found that industry wage differentials were strongly correlated with firm effects in an AKM decomposition, strengthening the case that they are to some extent a function of rents.

[20]Sectors correspond to NAICS sectors, industries to BEA industry codes (roughly NAICS 3-digit), and detailed industries to SIC industries. More details on estimation are in Appendix B.1. Note that the CPS-ORG data is top-coded, so we will not observe changes in firm size or industry wage premia for very high earners.

[21]Kim and Sakamoto (2008) also find evidence of a decline in inter-industry wage dispersion using the CPS-ORG, albeit with a different methodology. Note that our result does not conflict with the result of Haltiwanger and Spletzer (2020), who find that the dispersion of average log earnings across industries has risen over 1997-2013: this pattern also exists in our raw CPS data, but is reversed once occupation and individual characteristics are controlled for. In addition, much of the decline in industry wage differentials we identify in the CPS occurs before 1997.

As with the decline in firm size wage effects, the decline in the variance of industry wage effects could be a result of falling rent-sharing, but could equally be a result of changing compensating differentials or sorting by unobserved worker productivity. We have no a priori reason to believe that there has been a substantial change in compensating differentials in the necessary direction (as it would imply that high-wage industries used to have much worse amenities, but have improved over time). We can test the sorting explanation by estimating industry fixed effects using the longitudinal component of the CPS, which enables us to control for worker-level unobserved productivity. The proportional decline in the variance of industry fixed effects estimated longitudinally is as large as for the cross-sectional estimates, suggesting that the decline we observe is not driven primarily by a change in the degree of sorting of highly productive workers into high-wage industries.[22][23]

### 2.2.4 Decreased passthrough of productivity and profit shocks

A different source of evidence that worker power has been attenuated is the apparent decline in the relationship between workers' pay and the profitability, revenues, and/or product market power of their firm or industry. A perfectly competitive labor market would imply no relationship between firm- or industry-level performance and workers' pay, but in practice there is substantial evidence that firms and industries with higher productivity or profitability do pay more to observably equivalent workers (as reviewed in Card et al 2018).[24]

[22] More details on the longitudinal estimates of are available in Appendix B.1. Note also that even to the extent that industry fixed effects do represent rents, a decline in the dispersion of industry fixed effects could be a result of a decline in the dispersion of industry-level rents, holding constant the degree of rent-sharing. This does not appear to be the case: the cross-industry dispersion of various measures of profitability has not fallen over the period. Another possibility is that the fall in the employment-weighted standard deviation of industry fixed effects simply represents a reallocation of workers from high-rent to low-rent industries. This also does not appear to be driving the result: the non-employment-weighted standard deviation of industry fixed effects has fallen by roughly the same amount. For more details, see Appendix B.1.

[23] A further indication that our measure of industry labor rents is picking up rents: we find that industries with higher wage premia have substantially and significantly lower quit rates (as found also by Holzer, Katz and Krueger 1991).

[24] See also Appendix B.4 for a review of some of this evidence.

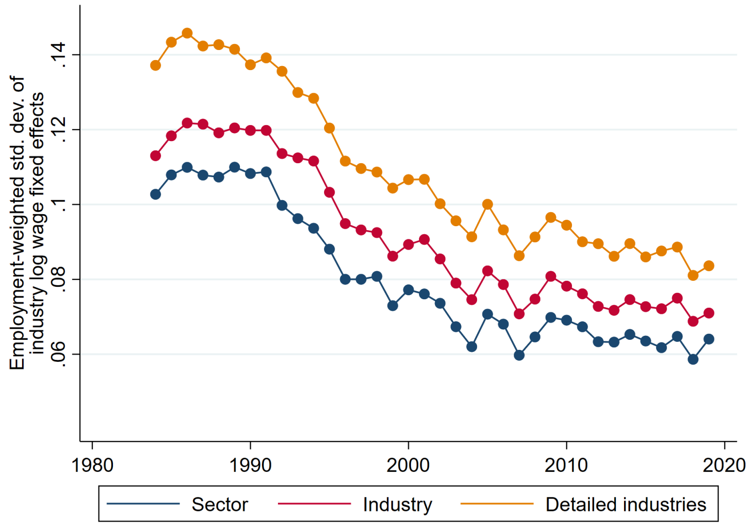


**Figure 2.3:** Standard deviation of industry wage effects
Note: Industry fixed effects are calculated as the fixed effect on industry dummies in annual log wage regressions from the CPS-ORG over 1984-2019, with demographic, location, and occupation controls. "Sector" refers to 18 aggregated NAICS sectors, "Industry" to 77 industries (roughly NAICS 3-digit level), and "Detailed industries" to 250 SIC industries.

There is some evidence to suggest, however, that this relationship has weakened over time. Using the NBER CES Manufacturing data, which covers 473 NAICS 6-digit manufacturing industries over 1958-2011, we regress the annual change in log value added per worker on the annual change in log compensation per worker.[25] We find evidence of rent-sharing over the period: in years with 10 log points higher value added per worker, average pay in a given industry was 2.5 log points higher. But the strength of that relationship fell by about half from the 1960s-70s to the present (Figure 2.4). In similar work, Bell, Bukowski, and Machin (2019) find a declining relationship between profits per worker and compensation per worker in U.S. manufacturing industries, also using the NBER CES data. Benmelech, Bergman, and Kim (2019) report a decline in the relationship between out-

[25] Following Stansbury and Summers (2019) we use a 3-year moving average of each variable in the regression. Our results are robust to the choice of moving average length. Note that NAICS 6-digit manufacturing industries are very narrowly defined: for example, NAICS 337110 "Wood kitchen cabinet and countertop manufacturing".

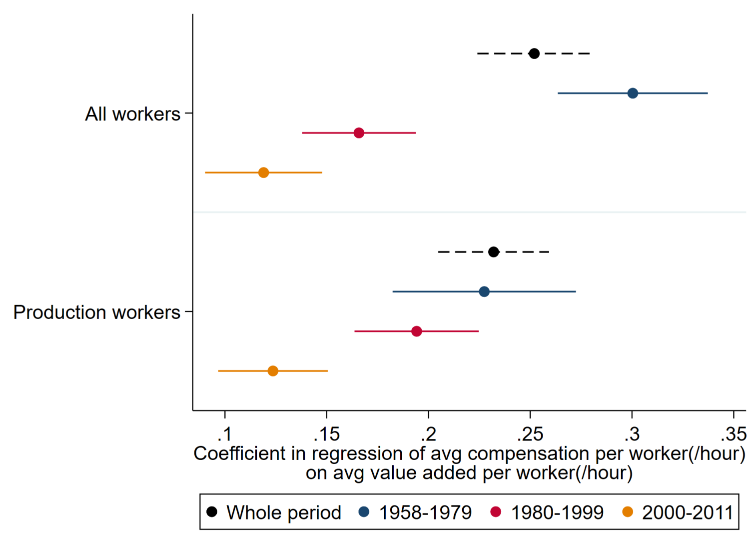


**Figure 2.4:** NAICS 6-digit industry-level regression of average compensation per worker on average value added per worker, manufacturing

Note: Coefficients in NBER CES Manufacturing industry regressions (NAICS 6-digit): 1958-2011. "All workers" regresses change in log compensation per employee on change in log value added per employee, 3-year moving averages (following Stansbury and Summers 2019). "Production workers" regresses change in log average hourly production worker pay on change in log value added per production worker hour, 3-year moving averages. Regressions have industry and year fixed effects. Standard errors clustered by industry. Dots represents point estimates from separate regressions, lines represent 95% confidence intervals.

put per hour and compensation per hour at the plant level in U.S. manufacturing over 1978-2007. Together, this evidence is strongly suggestive of a decline in rent-sharing in U.S. manufacturing: workers in firms and industries with higher revenue productivity and higher profits appear to share in this less than they used to. We also examine evidence on the relationship between product market concentration and wages. At the very aggregated sector level, we find a positive relationship between average product market concentration and the sector wage premium, but the strength of that relationship declined over 1982-2012. In regressions of product market concentration on wage premia at the industry level, the general trend also suggests a weakening of the relationship between average concentration and the wage premium, but the change over time is not statistically significant.[26]

### 2.2.5 Increased use of domestic outsourcing and subcontracting

A final indicator that rent-sharing has declined is the increase in the use of outsourcing and subcontracting of business functions, franchising, the growth in independent contracting and the gig economy, and the decline in internal labor markets, often referred to jointly as the "fissuring" of the workplace (see Weil 2014, Bernhardt, Batt, Houseman, and Applebaum 2016, and Bidwell, Briscoe, Fernandez-Mateo, and Sterling 2013). If workers' ability to share in firm-level rents depends on them being employed within the firm, then one would expect that this fissuring would lead to wage decreases, particularly for workers working (indirectly) for high-rent firms.[27] There is increasing

[26]See Appendix B.3 for details of these analyses. Note that if product market concentration became a noisier measure of monopoly power over time, we might expect to see a weakening relationship between concentration and wage premia even if the underlying relationship between monopoly power and wage premia remained constant.

[27]Factors driving the fissuring of the workplace may have been an increase in shareholder pressure to cut labor costs, increased ability to coordinate and monitor the performance of contracted out workers, increased focus on firm "core competencies", declining union presence, and an erosion of antitrust standards prohibiting non-price vertical restraints (see Weil 2014, Bernhardt et al 2016, Bidwell et al 2013, Steinbaum 2019). Factors which make rent-sharing more likely if workers are employed within the boundaries of the firm include the degree to which rent-sharing is determined by unionization or the threat of unionization, and/or the degree to which rent-sharing depends on a sense of pay equity or internal labor markets within the firm.

evidence that outsourced workers receive wage penalties, and that this is related to a loss of rents.[28] While the scale of fissuring is difficult to measure with existing data (Bernhardt et al 2016), evidence suggests that it is widespread. Weil (2019) estimates that – as a rough lower bound – 19 percent of private sector workers were in industries where fissured arrangements predominate. Looking at specific occupations, the share of workers in security, cleaning, and logistics occupations who work in business services industries rose from less than 10% in 1970 to 35%, 25%, and 20% respectively in 2015 (Dorn, Schmieder, and Spletzer 2018).

### 2.3 Estimating the magnitude of the decline in labor rents

The evidence in Section 2.2 paints a picture of declining rent-sharing with labor – but was it big enough to explain the macro trends we have seen? We use a back-of-the-envelope approach to estimate the total quantity of labor rents in the U.S. nonfinancial corporate sector for each year from 1982-2016, as follows:

$$\text{Total labor rents} = \text{Union rents } + \text{Industry rents } + \text{Firm size rents}$$

where "union rents" refers to rents arising from union wage premia for unionized workers, "industry rents" refers to rents arising from industry wage premia, and "firm size rents" refers to rents arising from large firm wage premia. We calculate union rents, industry rents, and firm size rents from

[28] Dube and Kaplan (2010) find that outsourced janitors and guards lose wage premia, consistent with a loss of firm-specific rents; Dorn, Schmieder, and Spletzer (2018) find evidence of a loss of wage premia for outsourced workers in food, cleaning, security, and logistics occupations; Mishel (2018) links the decline in the manufacturing wage premium to the increase in the use of staffing agencies; and Wilmers (2018) finds that workers at supplier firms which become dependent on a dominant buyer lose wages, consistent with a loss of rents. Evidence from Handwerker (2018) and Song et al (2019) is also consistent with the fissuring of the workplace leading to a loss of rents: Handwerker finds that wages are lower in firms with more concentrated occupational employment, and this concentration has increased over time, and Song et al (2019) find an increase in the sorting of highly-paid workers into high-paying firms (and vice versa).

our estimates of union, industry, and firm size wage premia as outlined below.[29] Note that our estimate is of the total quantity of labor rents for the majority of workers, excluding the very highest earners, since top-coding and non-response in the CPS mean we cannot estimate union, industry, or firm size rents for these earners.

Union rents: For each year t, we estimate the share of total compensation in the non-financial corporate sector which was union rents, using estimates of the union log wage premium $\mathrm{uwp}_t$, the union coverage rate in each year $\mathrm{ucr}_t$, and compensation in the non-financial corporate sector,[30] as follows:

$$\text{Union rents}_t = \text{compensation}_t \left(1 - \frac{1}{1 + \mathrm{ucr}_t \cdot (e^{\mathrm{uwp}_t} - 1)}\right)$$

Industry rents: For each industry j and year t, we estimate the share of total compensation in that industry which was industry rents. We start with our estimated industry fixed effects from log wage regressions, at the level of 19 NAICS sectors for 1987-2016 and 9 SIC sectors for 1982-1986. To calculate the industry wage premia from the estimated fixed effects, we first rescale the estimated industry fixed effects relative to the lowest-fixed-effect large industry, which is Retail Trade. (This calculation assumes that there are zero labor rents on average for workers in Retail Trade.) We then treat half of the deviation of the industry fixed effect from the Retail Trade fixed effect as an industry wage premium ("rents"). We only consider half of the industry wage differentials to represent rents because, even though we have controlled for as many person-level characteristics as we can, there may still be worker sorting into industries on unobserved productivity differences, and because part of the estimated inter-industry wage differentials may reflect compensating differentials. While we choose to simply cut industry wage effects in half for transparency, we have reason to believe this is

[29] Full details of the calculation are in Appendix B.2. We focus on the nonfinancial corporate sector for our baseline estimates, and present estimates of labor rents for the full corporate sector in Appendix B.2.

[30] We estimate the union log wage premium from the CPS-ORG for 1984-2019, and use estimates from Blanchflower and Bryson (2004) for years 1982 and 1983. We estimate the union coverage rate for workers in private industries excluding finance, insurance, and real estate for 1984-2019 from the CPS-ORG and extend these back to 1982 using data on the private sector union coverage rate from unionstats.com.

reasonable: first, our estimates of industry wage premia from the longitudinal component of the CPS, controlling for person fixed effects, are very highly correlated with our cross-sectional estimates and are exactly half as big on average; and second, we benchmark our estimates against estimates of industry wage premia and the degree of rent-sharing from two papers using AKM estimation on US data.[31] This approach gives us industry wage premium $\mathrm{iwp}_{j,t}$, and allows us to calculate industry rents as:

$$\text{Industry rents}_t = \sum_{j}^{\text{industries}} \text{compensation}_{j,t} \left(1 - \frac{1}{e^{\mathrm{iwp}_{j,t}}}\right)$$

where *compensation* refers to our estimate of total nonfinancial corporate sector compensation for each industry. [32]

Firm size rents: For each firm size class s and year t we estimate the share of total nonfinancial corporate compensation which was firm size rents, using our firm size wage fixed effect estimates from the CPS for 1990-2016. As with the industry wage fixed effects, we halve the firm size (log) wage fixed effects to get our estimate of the firm size premium $\mathrm{fsp}_{s,t}$, to account for possible compensating differentials and/or unobserved productivity differences. The firm size premium is estimated for firms of 500+ workers or 100-499 workers, relative to firms with 1-99 workers. We impute firm size rents for the years 1982-1989 using data on compensation share by firm size class and estimated firm size log wage premia from Levine et al (2002). [33] This gives us the following expression for firm

[31] More details on our longitudinal fixed effect estimates and benchmarking procedure are in Appendix B.1. For our benchmarking procedure, we take estimates for the average firm fixed effect across different U.S. sector over 1990-2001 from Abowd et al (2012), and apply Sorkin's (2018) estimate that one third of firm fixed effects on average represent rents. This gives us a rough estimate of the average log wage premium due to rents in each sector, over 1990-2001.

[32] This is calculated as: compensation in industry j $\cdot$ $\frac{\text{total compensation in nonfinancial corporate sector}}{\text{total compensation in private industries}}$. We make this adjustment because we want to estimate only the labor rents going to workers in the nonfinancial corporate sector, but we do not have data on compensation by industry broken down by corporate vs. noncorporate sector.

[33] Full details on the imputation procedure are available in Appendix B.2.

size rents:

$$\text{Firm size rents}_t = \sum_{s}^{\text{firm size classes}} \text{compensation}_{s,t} \left(1 - \frac{1}{e^{fsp_{s,t}}}\right)$$

where *compensation* refers to our estimate of nonfinancial corporate sector compensation by firm size class.[34]

Using this method, we think it likely that we will *underestimate* the true decline of labor rents over recent decades. First, because our estimates are based on union, industry, and firm size wage premia calculated relative to a baseline sector (non-unionized firms for union rents, Retail Trade for industry rents, and firms of under 100 employees for firm size rents), our calculation of total labor rents will miss any decline in rent-sharing which has occurred commonly across industries, firm size classes, and/or union status. This could include a generalized increase in shareholder activism and more "ruthless" corporate management practices, a generalized increase in the use of domestic outsourcing, or a generalized decrease in the threat effect of unions. Second, in each calculation we assume that there are no rents in the baseline sector: workers receive the wage that would prevail in the absence of worker power. Our calculation will therefore miss any decline in rent-sharing which is specific to these baseline sectors – with the most obvious candidate being a decline in rents arising from the erosion in the real value of the minimum wage. Third, our estimates of labor rents are based on union, industry, and firm size *earnings* premia. Total rents, however, are estimated as a share of *compensation*. The union and large firm premia for non-wage benefits are likely greater than for wages, making our calculation of total union and firm size rents an underestimate.[35]

There are, on the other hand, some factors which could make our estimate of the decline in labor rents an overestimate. First, while we cut our estimated industry wage fixed effects and firm size

[34] This is estimated as total compensation in the nonfinancial corporate sector, multiplied by the payroll share of each firm size class (from the Census Bureau SUSB data).

[35] Mishel, Bivens, Gould, and Shierholz (2012) show that the union premium is greater for non-wage benefits than for wages. Hollister (2004) finds that large firms are more likely to provide health and pension benefits, controlling on observables, but this differential has fallen over time, exacerbating the fall in the large firm wage premium.

fixed effects in half to account for unobserved productivity or compensating differentials, it is possible that they remain overestimates of the degree of rents (though our benchmarking exercise should assuage this concern). Second, we assume that there are zero rents in the baseline sectors (non-unionized firms, Retail Trade, and firms of under 100 employees) – but in some models, worker power in one sector lowers pay in other sectors (by restricting employment in the high worker power sector, leading workers to spill over into the low worker power sector, reducing wages). If this is the case, we would overestimate total labor rents.[36] On net, we think these concerns are outweighed by the factors pushing our estimate to be an underestimate.

### 2.3.1 Labor rents in the nonfinancial corporate sector, 1982-2016

Our measure of labor rents, as a share of net value added in the nonfinancial corporate business sector, declined from around 12% in the early 1980s to around 6% in the 2010s (Figure 2.5, Table 2.1). Union rents fell by 2.1 pp as the unionization rate and union wage premia fell. Industry rents fell by 2.4 pp as industry wage premia fell and employment fell in high-rent industries. Firm size rents fell by 1.2 pp as firm size premia fell.

A set of simple counterfactuals illustrates that the decline in total labor rents is primarily due to changes in the ability of workers to lay claim to rents within any given industry, rather than changes in sectoral composition of the economy. First, if unionization within each sector had not fallen (and union wage premia had not fallen), but the sectoral composition of compensation had changed as it did over 1987-2016, union rents would have fallen from 2.4% to 1.9% over 1987-2016 (rather than falling from 2.4% to 0.9%).[37] On the other hand, if the sectoral composition of compensation had

[36] A further concern might be that we estimate union and industry wage effects in the CPS-ORG without controlling for firm size (which is not available in the CPS-ORG). As a robustness check, we estimate union, firm size, and industry wage premia all together in the CPS ASEC over 1990-2019. The estimated falls in the size of the union wage premium and industry wage premia are very close to those estimated from the CPS-ORG data.

[37] We carry out our counterfactual over 1987-2016 rather than 1982-2016 because it means we are able to

not changed, but unionization rates within each sector, and union wage premia, had fallen to the levels they were at in 2016, union rents would have fallen by essentially the same amount that they fell in reality: from 2.4% to 0.9% over 1987 to 2016.[38] For industry rents, if industry wage premia had not declined but the sectoral composition of compensation had still changed over 1987-2016, the industry rent share would have only declined by around one tenth of a percentage point.[39] If industry wage premia had fallen but the sectoral composition of compensation had stayed the same, the industry rent share of net value added would have fallen from 5.2% in 1987 to 3.4% in 2016 rather than from 5.2% to 2.6%. Finally, for firm size rents, the share of workers in large firms has actually grown over the period, both in aggregate and within almost every sector, such that the decline in firm size rents reflects exclusively the decline in the firm size premium rather than compositional shifts.

Note that our analysis of the role of "union rents" only considers the *direct* effect of the decline in unionization: the loss of wage premia for unionized workers. To the extent that union power also increased the compensation of non-union workers in certain industries or large firms through "threat effects", our estimates of the decline in industry or firm size rents could also be capturing effects of the decline of unions.[40]

While our analysis in this paper is primarily focused on shifts in income between labor and capital, rather than inequality in labor incomes, we note that the decline in labor rents appears to have

use consistently defined NAICS industries. This is the period over which the majority of the fall in labor rents happened.

[38] This is because by 2016 the unionization rate in manufacturing had fallen to almost the level that it was in services. So, shifting the sectoral composition from services back to manufacturing in 2016 would have made little difference to aggregate unionization.

[39] This is due to two offsetting forces. The decline of the share of total compensation in manufacturing – which has a high average wage premium – exerted downward pressure on the industry rent share, but this was offset by increases in the compensation share of professional, scientific, and technical services, and health care and social assistance, which had high and medium-sized wage premia in the late 1980s (respectively).

[40] Supporting this, there is a very strong relationship between the decline in industry, firm size, and union rents at the state and industry level. See Appendix B.3 for details.

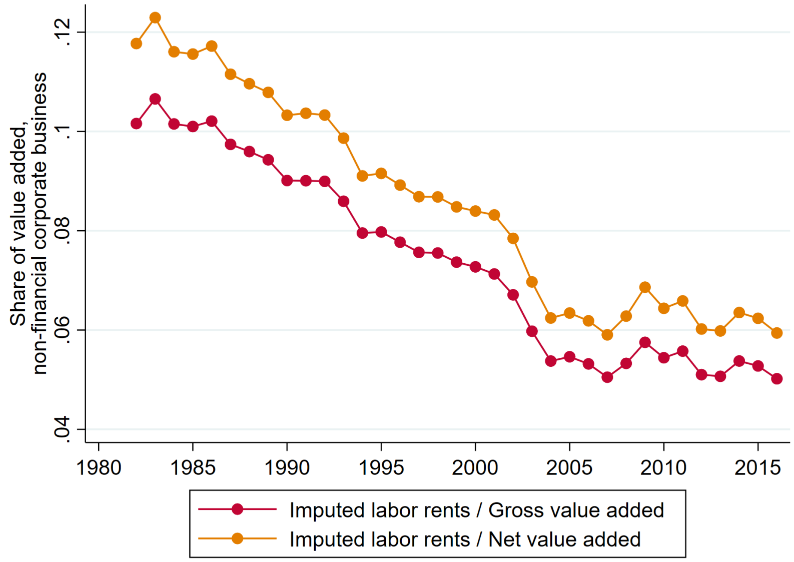


**Figure 2.5:** Estimated labor rents as share of value added, nonfinancial corporate sector
Note: Labor rents imputed from estimated union, firm size, and industry wage premia as described in Section 2.3. Data on compensation and value added in nonfinancial corporate business sector from BEA NIPA.

disproportionately affected workers with less formal education. Over 1984-2016, labor rents as a share of compensation fell by 8 percentage points for workers with no college or some college education, and by 4.5 percentage points for workers with a four year college education or more. This differential was driven by significantly larger declines in unionization rates and firm size rents for non-college workers.[41] There is a large body of work documenting the effect of the decline in unionization on the rise in income inequality in the United States. See, for example, DiNardo, Fortin, and Lemieux (1996), Card (1996), Rosenfeld (2014), Farber and others (2018), Fortin, Lemieux, and Lloyd (2018).

[41] See Appendix B.2 and B.3 for the detail underlying these calculations. We start in 1984 as we cannot estimate union membership and wage premia by education group before 1984.

**Table 2.1:** Estimated labor rents as share of value added, nonfinancial corporate sector

| | 1982 | 1986 | 1996 | 2006 | 2016 |
|---|---|---|---|---|---|
| Shares of net value added, nonfinancial corporate sector | | | | | |
| Total labor rent share | 11.7% | 11.7% | 8.9% | 6.2% | 5.9% |
| Union rent share | 3.0% | 2.6% | 1.7% | 1.2% | 0.9% |
| Firm size rent share | 3.7% | 3.5% | 2.9% | 2.3% | 2.5% |
| Industry rent share | 5.0% | 5.6% | 4.2% | 2.6% | 2.6% |
| Shares of gross value added, nonfinancial corporate sector | | | | | |
| Total labor rent share | 10.1% | 10.2% | 7.8% | 5.3% | 5.0% |
| Union rent share | 2.6% | 2.3% | 1.5% | 1.1% | 0.7% |
| Firm size rent share | 3.2% | 3.0% | 2.6% | 2.0% | 2.1% |
| Industry rent share | 4.3% | 4.9% | 3.7% | 2.2% | 2.2% |

### 2.3.2 Were labor rents redistributed or destroyed?

One natural explanation for the steep decline of labor rents is that it represents greater market pressures on particular industries, coming from technology, globalization, or some other extrinsic forces. If this were the case, one would expect (1) that returns to capital would fall alongside rents to labor, and (2) that the total rents in the industry – profits, plus labor rents – would be falling. It is striking, however, that for the industries in which the majority of the decline in labor rents took place, this was not the case – suggesting there was a very important element of redistribution of rents from labor to capital.

In 29 industries – which employed around 30% of the private sector workforce in 2018 – returns to capital rose even while rents to labor fell over 1987-2016. Together, these industries were responsible for 73% of the decline in labor rents over the period. Of these industries, those responsible for the largest shares of the total decline in labor rents were: several manufacturing industries, wholesale trade, telecoms, utilities, and trucking. In the majority of these industries – 21 industries, employing around 24% of the private workforce in 2018 – returns to capital rose by more than rents to labor fell over 1987-2016, implying that the total underlying profits generated by these industries rose, even as rents to labor fell. These industries were responsible for 38% of the total decline in labor rents over 1987-2016.[42]

We also take a closer look at manufacturing industries. The manufacturing sector can account for the majority of the decline in the labor share since the 1980s. It is a sector which saw particularly large declines in unionization and in our estimates of industry wage premia. And it is the sector that has been the most exposed to global competition over recent decades. This raises the question: were labor rents destroyed most in the manufacturing industries which were most exposed to global competition? Using changes in import penetration from low-wage countries as our measure of exposure to global competition, we investigate this for 18 manufacturing industries over 1989-2007.[43] Contrary to the predictions of the globalization thesis, labor rents declined the most in the industries with the smallest increases in low-wage import penetration over the period (Figure 2.6). This evidence, while not dispositive, casts further doubt on the argument that the decline in labor rents in manufacturing since the late 1980s was primarily a result of globalization. Overall, these results suggest that a large share of the decline in labor rents was a result of a redistribution of rents from labor to capital, rather than a destruction of rents as a result of increased competition or market pressure. This informs our approach in the rest of the paper.

[42] See Appendix B.2 for details of these calculations. We study 51 industries at roughly the NAICS 3-digit level, over 1987-2016 (since consistent industry-level data through 2016 is not available before 1987.)

[43] We use low-wage import penetration data from Bernard, Jensen, and Schott (2006), updated by Peter Schott in 2011. Low-wage import penetration is calculated as the share of domestic sales within each industry represented by imports from low-wage countries, defined as countries with GDP per capita less than 5% of the U.S. level. We study 1989-2007 as this is the period for which we have consistently-defined data on low-wage import penetration. (See Appendix B.2 for more details). Our sample period covers the period after the accession of China into the WTO, as well as the large increases in global trade in the 1990s. However, our sample period does not cover the effects of globalization in the 1970s and early-to-mid 1980s. Competition from low-wage countries would have been relevant for only a few industries during this period: in 1989, imports from low-wage countries only made up more than 1% of the US market in three manufacturing industries: apparel, textiles, and miscellaneous durable goods (Bernard and others (2006)).. On the other hand, competition from high-wage countries may have destroyed rents in other manufacturing industries earlier in the postwar period, and this is not captured in our sample. Borjas and Ramey (1995), for example, argue that increased foreign competition in durable goods manufacturing over 1976-1990 destroyed rents in that sector, reducing the wage premia paid to workers.

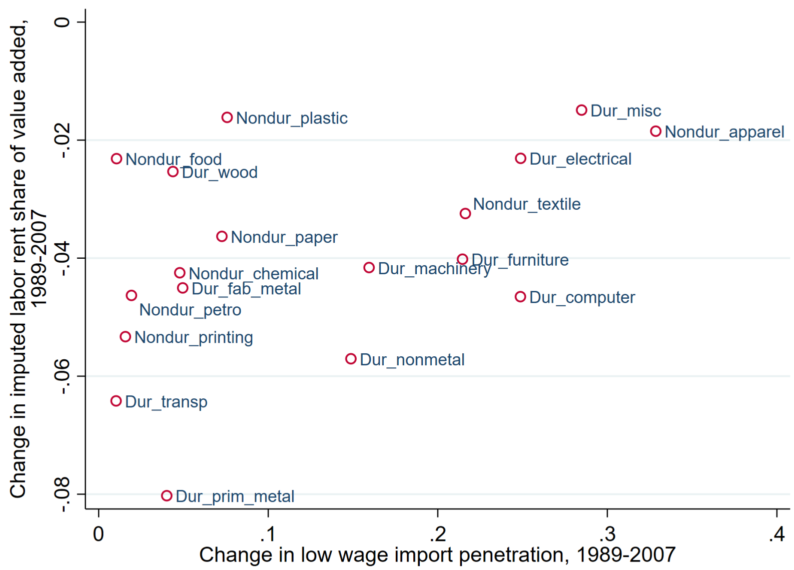


**Figure 2.6:** Low-wage import penetration and labor rents in manufacturing, 1989-2007
Note: Manufacturing industries at BEA industry code level. Imputed labor rent share of value added calculated from estimated industry and union wage premia (from CPS-ORG), and compensation and value added by industry from BEA Industry Accounts. Low-wage import penetration by industry from Bernard, Jensen, and Schott (2006).

## 2.4 Factor shares, profits, and measured markups

The labor share of income has declined since the 1980s, with a corresponding rise in the capital share (Elsby, Hobijn, and Sahin 2013, Karabarbounis and Neiman 2014). The Tobin's Q of publicly listed corporations – the ratio of their stock market value to the replacement cost of their capital stock – has risen from around 1 in 1970 to 1.75 by 2015, alongside an increase in the value of financial assets relative to the value of productive capital. (Eggertsson et al 2018). The average profitability of capital has risen, even as the risk-free rate has declined. And, by a range of measures, several authors have found that markups have risen (e.g. De Loecker et al 2020, Eggertsson et al 2018, Covarrubias et al 2019).[44]

A number of explanations have been proposed for the decline in the labor share of income. Many of these have centered on certain aspects of globalization or technological change – such as the increase in offshoring, the declining price of capital goods, or rising automation – as the major cause of the decline in the labor share. These include Elsby, Hobijn, and Sahin (2013), Abdih and Danninger (2017), Acemoglu and Restrepo (2018), and Autor et al (2020) (focusing on the U.S.); and Karabarbounis and Neiman (2014), Dao, Das, Koczan, and Liang (2017), and Autor and Salomons (2018) (taking a cross-country perspective).

More recently, a growing body of research argues that these trends can be explained by a rise in the market power of corporations. Rising monopoly power in product markets would lead firms to increase their markups, reducing the labor share of income and increasing corporate profitability. This would in turn increase Tobin's Q and the value of financial assets relative to physical capital. Different aspects of this argument have been made by (in alphabetical order) Barkai (2017), Brun and Gonzalez (2017), Covarrubias, Gutiérrez, and Philippon (2019), De Loecker and Eeckhout

[44] The magnitude of the rise in measured markups depends on the method used. See Traina (2018), Karabarbounis and Neiman (2018), Edmond, Midrigan, and Xu (2018), and Baqaee and Farhi (2020). All measures that we are aware of show some increase in markups over recent decades.

(2019), De Loecker, Eeckhout, and Unger (2020), Eggertsson, Robbins, and Wold (2019), Farhi and Gourio (2018), Gonzalez and Trivin (2017), Grullon, Larkin, and Michaely (2019), Gutiérrez and Philippon (2017, 2019), Hall (2018), Philippon (2020). Some authors have also argued that these trends could be rationalized by a rise in companies' monopsony power in labor markets (e.g. CEA 2016, Furman and Krueger 2016, Glover and Short 2018, Benmelech, Bergman, and Kim 2019, Philippon 2020).

It is difficult to rationalize the trends in corporate valuations, corporate profitability, and measured markups in a model of perfect competition. In this sense, we agree with the monopoly/monopsony power arguments that the explanation of these macro trends must involve some degree of rents created by imperfect competition (in contrast to explanation based solely on technological change or globalization).

Our preferred explanation for these macro trends, however, focuses on a redistribution of existing rents rather than a creation of new rents. That is, the decline in the labor share, and the rise in corporate valuations, profitability, and measured markups, could have been caused by a decline in worker power.

To see this, consider an economy characterized by three types of power, to varying degrees: monopoly power, monopsony power, and worker power.

Firms have monopoly power in the product market, created by a combination of monopolistic competition and restrictions to entry. They set their price at a markup above marginal cost, and make some 'pure profits' or rents which are not fully competed away by new entrants. These rents may arise as a result of explicit barriers to entry, regulatory or otherwise. But they may also arise from heterogeneous production technologies, with new entrants unable to perfectly replicate incumbents' products or production techniques. And in the short run, there may be rents because of the presence of fixed costs due to previously installed capital, and prices in excess of variable costs.[45]

[45] Note that in the latter two cases the existence of rents does not necessarily signal a market imperfection

Firms may also have monopsony power in the labor market, by which we mean the wage-setting power firms derive from an upward-sloping labor supply curve. This can arise either from employers' size in their local labor market ("conventional monopsony"), and/or from labor market search frictions, switching costs, or different worker preferences for different employers ("dynamic monopsony"). In a monopsonistically competitive labor market, the wage a firm pays is a markdown from the marginal revenue product of labor at the firm.[46]

Finally, there is also worker power. By worker power, we mean workers' ability to increase their pay above the level that would prevail in the absence of such bargaining power. In this framework, worker power not only acts as countervailing power to firm monopsony power, but also gives workers an ability to receive a share of the rents generated by companies operating in imperfectly competitive product markets. We use the term worker power as synonymous with worker bargaining power, worker rent-sharing power, and insider-outsider power of the kind that was used in earlier work to explain increases in unemployment.[47]

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which can be corrected through antitrust or competition policy. In this framework the presence of rents is therefore, to some extent, an innate feature of the structure of particular product markets.

[46] Our definition of monopsony power follows the modern monopsony literature. In the presence of monopsony, the size of the wage markdown is an inverse function of the elasticity of labor supply to the firm. The perfectly competitive case occurs where the elasticity of labor supply to the firm is infinite. Labor market concentration and search frictions both therefore create monopsony power because they both generate upward-sloping labor supply curves to the firm – but their welfare and policy implications can be different, as highlighted by Manning (2003).

[47] Note that monopsony power and worker power are distinct concepts in our framework. The term "monopsony power" is sometimes used to refer to a broader conception of employer power than we use here: for example, in some bargaining models, firm monopsony power might be considered the exact inverse of worker power (the wage is partly determined by the firm's and worker's relative bargaining power over the match surplus). We distinguish between monopsony power and worker power for two reasons. First, in our framework, worker power is not necessarily simply the inverse of employer wage-setting power: worker power enables workers to claim a share of the rents produced within the firm, potentially raising their wage above the marginal product in their labor market. This can occur even in a world of no labor market concentration or search frictions, where labor supply to the firm is completely elastic. Second, the source of the change in wage-setting power matters for diagnosis and policy solutions: a decline in worker power caused by a decline in unionization implies a different policy solution as compared to a rise in employer power caused by an increase in labor market frictions or concentration. The two concepts of worker power and monopsony power are, however, linked in the sense that worker power operates as countervailing power to firm monopsony power. As worker power declines, firms' ability to exercise their monopsony power rises without the underly-

In this framework, if workers' ability to receive some of the rents generated by their firms has fallen over time, we would expect to see a decline in the labor share – as rents going to workers fall and rents going to shareholders rise (holding constant the total quantity of rents generated). We would also expect to see a divergence between the average profitability of capital and the risk-free rate, as profits to shareholders rise, and a rise in Tobin's Q and the ratio of financial wealth to physical capital, as the rise in profits to shareholders increases the net present value of the claim shareholders have over corporate profits (even as the asset value of firms does not change). Indeed, Greenwald, Lettau, and Ludvigson (2019) find that a reallocation of income from labor to shareholders can account for a large share of the rise in equity valuations from 1989 to the present.[48]

In addition, while a fall in worker rent-sharing power should not have any implication for firms' underlying markups (which are determined by their product market power), it does have implications for measured markups. This is because measures of aggregate markups used in recent literature depend on firms' costs, including firms' labor costs – even if the labor costs partly represent rents accruing to labor as well as the true marginal cost of production.[49] This implies that markups, as

ing elasticity of labor supply to the firm having changed (as described in, for example, Erickson and Mitchell 2007).

[48] Specifically, they find that a series of "factor share shocks" have reallocated rewards to shareholders and away from labor compensation, accounting for 43% of the increase in equity valuations since 1989. They do not take a stance on the cause of these factor share shocks, but note that they could be due to changes in industrial concentration, worker bargaining power, offshoring and outsourcing, or technological change.

[49] The production function approach used by De Loecker, Eeckhout, and Unger (2019) estimates markups as a function of the (estimated) elasticity of output with respect to variable inputs, and the ratio of sales to variable costs – which include some labor costs. The rise in measured markups in the U.S. is mostly due to an increasing ratio of sales to variable costs, which could be a result of falling labor costs as labor rents fell. The user cost approach of Gutiérrez and Philippon (2017) estimates markups as the ratio of sales to costs, which are calculated as operating expenses plus an imputed cost of capital. Operating expenses include labor costs. Again, this means that changes in measured markups could be due to changes in labor costs as a result of falling labor rents. (See Appendix B.6 for more details on this). It would in theory be feasible to take these approaches and apply them only to non-labor costs to estimate markups, but there is no publicly available data of sufficiently good quality to do this across the entire set of industries. We note that Anderson, Rebelo, and Wong (2019) estimate markups across the U.S. in retail trade, using the markup of the price of each good sold over its replacement cost (i.e. not including labor costs), and find no secular increase in markups over 1979-2014.

they have been measured in recent papers, cannot be used to distinguish between a story of rising product market power and a story of falling worker power: a rise in measured markups could reflect a fall in worker rent-sharing power just as much as it could reflect a rise in true markups and firms' monopoly power.

### 2.4.1 Accounting decomposition, based on Farhi and Gourio (2018)

This implies that rising monopoly power, rising monopsony power, and falling worker power could each in theory account for the changes in factor shares, profits, and markups. But is the magnitude of the decline in labor rents consistent with these trends? To calibrate the plausibility of the declining labor rents explanation, we build on the accounting decomposition in Farhi and Gourio (2018). Farhi and Gourio extend the neoclassical growth model to account for six major recent macroeconomic trends, including the decline in the labor share, increases in valuation ratios, and moderate increases in profitability alongside a declining risk-free rate. Using this model, they identify a role for rising monopoly power in explaining these macro trends (alongside roles for unmeasured intangibles and rising risk premia). They estimate that average economy-wide markups rose from 8% to 15% over 1984-2016.

Their model, however, assumes competitive labor markets with no rent-sharing. We replicate their accounting decomposition, with one alteration: we hold the degree of monopoly power (markups) fixed, and instead introduce a rent-sharing parameter to allow workers to share in monopoly profits. We incorporate this in the simplest way possible: the monopolistic representative firm maximizes profits as before, but then shares the rents or 'pure profits', with share $\pi_L$ going to labor. This reduced-form approach is similar to that adopted in much of the literature on rent-sharing (as reviewed in Card et al (2018)). It can be micro-founded with a strongly efficient bargaining model where workers, seeking to maximize total pay to labor, and shareholders, seeking to maximize their profits, jointly bargain over the firm's production decisions (MacDonald and Solow 1981).

Farhi and Gourio carry out their decomposition targeting nine empirical moments for the U.S. private sector over 1984-2016: gross profitability, the gross capital share, the investment-capital ratio, the risk-free rate, the price-dividend ratio, population growth, TFP growth, the growth rate of investment prices, and the employment-population ratio. They estimate nine parameters: the discount factor, the probability of a disaster, the depreciation rate of capital, the Cobb-Douglas parameter in the aggregate production function, population growth rate, TFP growth, the growth rate of investment-specific productivity, labor supply, and the markup. We target the same nine moments and estimate eight of the same nine parameters – but, instead of estimating the markup, we estimate the rent-sharing parameter with labor, holding the markup fixed at the level that Farhi and Gourio estimate for the period 2001-2016 (1.15). Identification is nearly recursive in the Farhi/Gourio decomposition, with many parameters estimated tightly by their near-equivalent moments. Identification in our approach is therefore nearly identical to that in Farhi and Gourio: it has different implications for only two of the nine empirical moments – the gross capital share $\frac{\Pi}{Y}$ and gross profitability $\frac{\Pi}{K}$ (equivalent in the Farhi/Gourio model to the marginal product of capital). The equations below show the difference between the two approaches: in the Farhi/Gourio model, the rent-sharing parameter $\pi_L$ is implicitly set to be constant at zero, and the markup $\mu$ is allowed to vary. In contrast in our model, the markup $\mu$ is set to be constant at 1.15, and $\pi_L$ is allowed to vary.

$$\text{Capital share } \frac{\Pi}{Y} = \frac{\alpha + (1 - \pi_L)(\mu - 1)}{\mu}$$

$$\text{Profitability of capital } \frac{\Pi}{K} = \frac{\alpha + (1 - \pi_L)(\mu - 1)}{\alpha}\left(r^* + \delta + g_Q\right)$$

By construction of the recursive identification process in the decomposition, our model returns exactly the same parameter estimates as Farhi/Gourio for 6 of the 9 parameters estimated. Table 2.2 below shows only the parameter estimates which differ between the Farhi/Gourio model ("FG")

**Table 2.2:** Estimated parameters and changes over time

| Parameter | | Model | First sample (1984-2000) | Second sample (2001-2016) | Difference |
|---|---|---|---|---|---|
| Markup | $\mu$ | FG | 1.079 | 1.146 | 0.067 |
| | | SS | Fixed: 1.15 | Fixed: 1.15 | – |
| Rent-sharing with labor | $\pi_L$ | FG | Fixed: 0 | Fixed: 0 | – |
| | | SS | 0.441 | 0.022 | -0.419 |
| Cobb-Douglas parameter | $\alpha$ | FG | 0.244 | 0.243 | -0.001 |
| | | SS | 0.26 | 0.244 | -0.016 |
| TFP growth | $g_Z$ | FG | 1.298 | 1.012 | -0.286 |
| | | SS | 1.233 | 1.01 | -0.223 |

Note: only parameters where our estimates differ from Farhi and Gourio's estimates are shown. In the "SS" estimation, markup $\mu$ is held constant at 1.15. In the "FG" estimation, rent-sharing parameter $\pi_L$ is implicitly held constant at 0. The "FG" estimates in this table correspond to the baseline parameter estimates in Table 2 of Farhi and Gourio (2018).

and our model ("SS"). To fit the data best, Farhi/Gourio estimate a rise in the average economy-wide markup from 1.08 to 1.15 over the period. When we hold the markup constant at 1.15, but allow the rent-sharing parameter to vary, we estimate instead that the rent-sharing parameter fell from 0.44 to 0.02 over the period.[50] Our model also has slightly different implications for the Cobb-Douglas parameter $\alpha$ and TFP growth $g_Z$: our model suggests a somewhat smaller slowdown in TFP growth over the period, and a slight fall in the Cobb-Douglas parameter $\alpha$ (implying a small degree of labor-complementing technological change).

What does the estimated fall in the rent-sharing parameter imply for total labor rents? The rise in markups estimated by Farhi and Gourio, from 1.08 to 1.15, imply a rise in the "pure profit" share of output from 7.3% in the 1980s-90s to 12.8% in the 2000s-10s. Since we hold the markup at 1.15 throughout the 1980s-2010s in our estimation, the pure profit share of our economy is 12.8% throughout 1982-2016. The estimated fall in the rent-sharing parameter therefore implies that the share of gross private sector output which was labor rents fell by 5.3 percentage points, from 5.6%

[50] A rent-sharing parameter of 0.44 is quite plausible when compared to the range of estimate from studies of rent-sharing. See Appendix B.4 for details.

to 0.3%, over the period. This is quite similar to our estimate of the decline of labor rents in Section 2.3: we estimated that labor rents fell by 5.1 percentage points of gross value added in the nonfinancial corporate sector over 1982-2016 (corresponding to a fall of 4.1 percentage points of gross business sector value added). There is no necessary reason why these two estimates should line up so closely: the estimate of the fall in labor rents from the Farhi/Gourio model comes from the best fit of 9 parameters to 9 macro moments in each of the two periods, while our estimate of the fall in labor rents comes from our estimated union, industry, and firm size wage premia using CPS data. (Note that, to match Farhi and Gourio's results, we set up our calibration such that labor rents must equal zero in the second period. Therefore, the percentage point change in the share of output represented by labor rents is a more appropriate comparator than the levels.)

We see this accounting exercise as suggesting that, (1) the degree of the fall in rent-sharing with labor which is required to be consistent with a number of key macro moments over 1982-2016 is both relatively consistent with our empirical estimates of the actual fall in rent-sharing with labor, and relatively consistent with estimates of rent-sharing elasticities from the micro literature; and (2) despite the differential implications for investment of a rise in monopoly power vs. a fall in rent-sharing, when incorporated into a full general equilibrium model it is possible to reconcile a fall in labor rent-sharing (in an efficient-bargain type framework) with the data on capital and investment, without implausible implications for other macro variables.

### 2.4.2 Aggregate and state-level evidence: factor shares

Next, we compare our estimates of the decline in the labor rent share of value added with aggregate changes in factor shares. The net labor share in the nonfinancial corporate sector (compensation over net value added) fell by 4.4 percentage points over 1982-2016.[51] Our measure of the labor rent

[51] Following Bridgman (2018) and others, for our main results at the aggregate and industry levels we use the labor share of value added net of depreciation, as the depreciation rate has risen over the period.

share of net value added in the nonfinancial corporate sector fell by almost 6 percentage points over the same period. This suggests that the decline in imputed labor rents as estimated from industry, union, and firm size wage premia can more than fully explain the decline in the net labor share over the period (as shown in Figure 2.7): that is, the entirety of the shift in the functional income distribution in the nonfinancial corporate sector could be explained by a redistribution of rents from labor to capital.

The other side of the coin of the fall in the labor share is the rise in the capital share. Since our measure of labor rents can be interpreted as a measure of the firm's profits which go to labor, with the rest of the firm's profits going to capital, we can define the "Total profit share" of value added as the share of value added accounted for by capital income plus labor rents. While the capital share of net value added has risen over 1982-2016, our imputed measure of the total profit share has stayed roughly constant or even fallen slightly (Figure 2.8) – consistent with the interpretation that the total profitability of firms (and their monopoly power) has not risen over the period, but that these profits instead partly been redistributed from labor to capital.

We observe a similar pattern with state-level data. Estimating state-level labor rent shares in the same way as we estimate the aggregate labor rent share, we show that states with bigger declines in their imputed labor rent share also saw bigger declines in their labor share over 1984-2016 (Figure 2.9).[52] This strong relationship persists in regressions at the annual level, with year and state fixed effects, as shown in Table 2.3 (both for the labor rent share, and for the union rent share component of it).[53]

[52] The coefficient in a regression of the change in the state labor share over 1984-88 to 2012-16 on the change in the labor rent share over the same period is 0.76, with a p-value of 0.002 and an R-squared of 0.19. We calculate the labor share as state-level compensation over GDP, and calculate labor rents as a share of state GDP, using data from the BEA Regional Economic Accounts. We start in 1984 because it is the first year for which we can estimate state-level unionization and union wage premia. More details are in Appendix B.2.

[53] Similarly, Hazell (2019) finds that right-to-work laws (which reduce union power) reduce state-level labor shares.

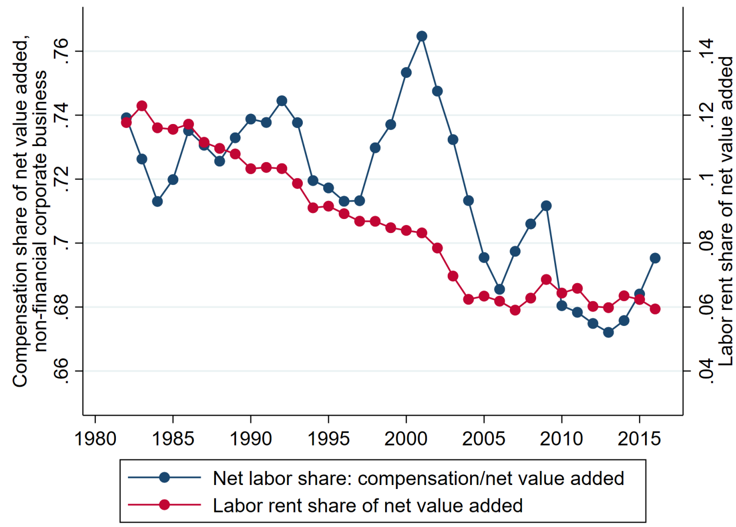



**Figure 2.7:** Net labor share and imputed labor rent share, nonfinancial corporate
Note: Net labor share in the nonfinancial corporate sector is calculated as compensation over net value added, using BEA NIPA data. Imputed labor rent share calculated as described in Section 2.3.

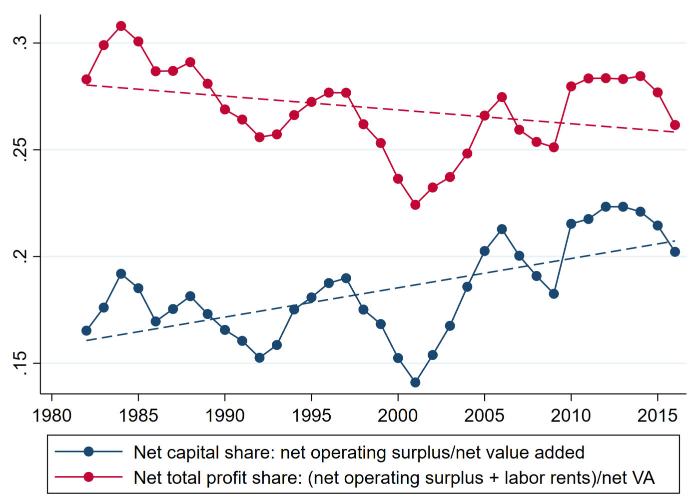



**Figure 2.8:** Net capital share & imputed profit share, nonfinancial corporate
Notes: Net capital share in the nonfinancial corporate sector is calculated as net operating surplus over net value added. Our measure of the net total profit share is calculated as the net operating surplus plus our measure of imputed labor rents (explained in Section 2.3), divided by net value added. Dashed lines are lines of best fit.

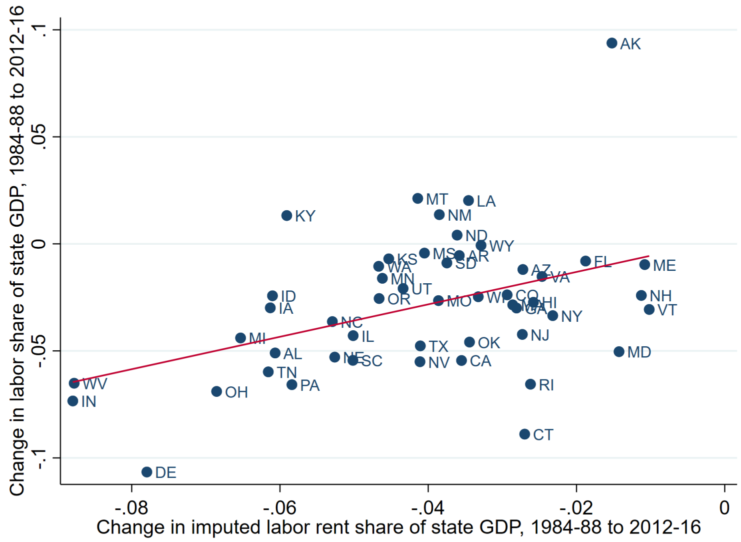



**Figure 2.9:** Changes in state-level labor share and labor rent share, 1984-88 to 2012-16
Notes: Imputed labor rent share of state GDP calculated from estimated union, firm size, and industry wage premia, state-level unionization rates (estimated in CPS), and compensation by industry (from BEA Regional Economic Accounts). Labor share of state GDP defined as state-level compensation over GDP.

### 2.4.3 Industry-level evidence

Next, we estimate labor rents at the level of 51 industries over 1987-2016.[54] We analyze the relationship between industry-level changes in labor rents and changes in the labor share, profitability, and Q. Since a number of recent papers have highlighted the link between industrial concentration and changes in labor shares and profitability, we also incorporate product market concentration, using measures of industry level top 20 import-adjusted sales concentration calculated from Compustat

[54]Our industry definitions are very close to the BEA industry codes (roughly NAICS 3-digit). (See Appendix B.7 for more details on industry definitions). For consistency with the previous section, we do not analyze industries in finance, insurance, and real estate. We also follow Covarrubias, Gutiérrez, and Philippon (2019) in omitting the industry management of companies and enterprises. Our calculation of industry rents and union rents follows the procedure for the aggregate level rents calculation closely, with the exception that it is comprised only of union rents and industry rents (and not firm size rents), as we do not have data on compensation shares by firm size class and industry (See Appendix B.2 for more details). Note that for industry rents, the wage premium is estimated relative to the lowest-wage large industry, which is Food Services and Drinking Places.

**Table 2.3:** State-level regressions of labor share on measures of labor power

| | | | | |
|---|---|---|---|---|
| *Panel A: Regression of labor share on imputed labor rent share, 1984-2016* | | | | |
| Imputed labor rent share of | 0.94** | 1.09** | 0.69** | 0.52** |
| state GDP | -0.14 | -0.28 | -0.06 | -0.13 |
| Fixed effects | None | Year | State | Year, State |
| Observations | 1,650 | 1,650 | 1,650 | 1,650 |
| *Panel B: Regression of labor share on imputed union rent share, 1984-2016* | | | | |
| Imputed union rent share of | 1.76** | 1.46* | 1.98** | 1.04* |
| state GDP | -0.48 | -0.68 | -0.24 | -0.4 |
| Fixed effects | None | Year | State | Year, State |
| Observations | 1,650 | 1,650 | 1,650 | 1,650 |

Robust standard errors, clustered at state level, in parentheses. + p<0.10, * p<0.05, ** p<0.01.

and Census data by Covarrubias, Gutiérrez, and Philippon (2019).[55][56]

Our analysis shows that, over 1987-2016, industries with larger falls in their imputed labor rent share also saw substantially larger falls in their labor share (Figure 2.10).[57] There is a negative, though somewhat weaker, relationship between changes in the labor share and average top

[55] We are grateful to Germán Gutiérrez and Thomas Philippon for sharing with us the measures of concentration they constructed for Covarrubias, Gutiérrez, and Philippon (2019). Covarrubias et al (2019) construct top 4, 8, 20, and 50 import-adjusted sales concentration ratios for each of the 53 BEA industries. They use two data sources: Compustat data on publicly-listed companies, re-weighted to reflect the composition of the underlying economy, and Census data on all firms. The Compustat concentration ratios are available annually for our whole sample period (1987-2016). The Census concentration ratios are available for the years 1997, 2002, 2007, and 2012. They adjust for imports by multiplying the domestic sales concentration ratio by the share of U.S. produced goods in total domestic sales in that industry. More details on the construction of these variables are available in Covarrubias, Gutiérrez, and Philippon (2019). Note that the Compustat measure only covers publicly-traded firms, and trends in publicly-traded firms have not always been representative of aggregate trends within individual industries (see e.g. Davis, Haltiwanger, Jarmin, and Miranda 2006).

[56] Concentration is an imperfect measure of firms' market power (see e.g. Berry, Gaynor and Scott Morton 2019 and Syverson 2019). We use concentration in this paper because recent literature has noted the rise in concentration, alongside rising markups and falling labor shares, and has often interpreted this as rising monopoly power.

[57] A similar relationship exists for changes in the industry-level unionization rate. See Appendix B.3.


**Figure 2.10:** Change in labor share and imputed labor rent share, by industry

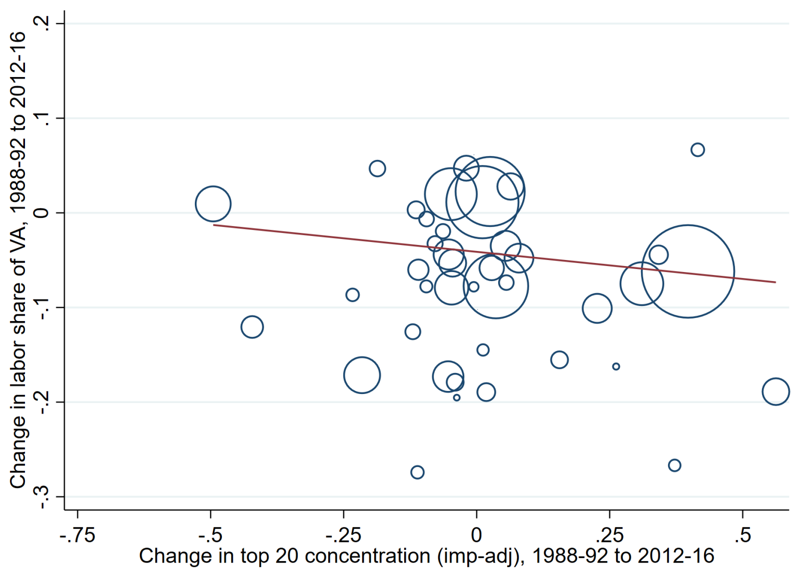



**Figure 2.11:** Change in labor share and top 20 sales concentration (imp-adj), by industry

Notes to Figs 2.10-2.11: Each bubble is an industry (at BEA industry code level). Bubble size represents industry average employment over 2012-2016. The red line is an employment-weighted line of best fit. Concentration data calculated from Compustat by Covarrubias et al (2019). Imputed labor rent share is our calculation.

20 import-adjusted sales concentration (Figure 2.11).

We regress the gross and net labor share on the imputed labor rent share of industry value added, and on product market concentration, at the annual level over 1987-2016, including different combinations of year and industry fixed effects (Table 2.4, panels A and C). Coefficients on the labor rent share are large, positive, and highly significant, and coefficients on concentration are negative and mostly significant.

What is the explanatory power of the decline in labor rents relative to the rise in concentration? Over 1997-2012 (the period for which we have the more accurate Census-based concentration data, and in which Covarrubias et al (2019) argue concentration has led to rising monopoly power) the average industry saw a fall in its labor share of 5.2 percentage points. Using the coefficient from the specification with industry and year fixed effects, the average industry's fall in their labor rent share over 1997-2012 was associated with 4.3 percentage points fall in the labor share. The average industry-level increase in import-adjusted top-20 sales concentration was associated with a 0.5 percentage point fall in the labor share. This suggests that declining labor rents can explain the majority of the average fall in labor shares at the industry level, whereas the average increase in concentration can explain only around 10%.[58]

Next, we analyze our measures of labor power alongside three measures of profitability at the industry level over 1987-2016: the gross profit rate (defined as gross operating surplus over fixed assets), as well as two measures of Tobin's Q calculated from firm-level Compustat data by Covarrubias et al (2019): the weighted average Tobin's Q across publicly-listed firms within an industry ("aggregate Q"), and the median firm Q.[59] Figures 2.12 and 2.13 illustrate that over the whole pe-

[58]The relative explanatory power of the worker power measures vs. concentration measures is similar if we use other measures of concentration (top 4, 8, or 50 sales ratios, and using measures from Census vs. Compustat). The comparison of coefficient magnitudes is even starker over 1987-2016: the average fall in the labor rent share was associated with 10.1pp fall in the labor share, while the average increase in import-adjusted top 20 sales concentration over this period was associated with 0.1pp fall in the labor share. The average industry's fall in the labor share over this period was 5.2 percentage points.

[59]Results are very similar when we use the simple average Q across firms, rather than the weighted average.

riod, falling labor rent shares were associated with rising gross profitability, while rising concentration was associated with rising profitability. In horse-race regressions of profitability measures on our measures of imputed labor rents and industrial concentration (Table 2.4, Panels B and D), coefficients on the imputed labor rent share are almost all negative and, for the 1987-2016 regressions, mostly statistically significant.[60] Coefficients on the concentration measures on the other hand are mostly not significant, and often negative (the opposite sign than would be predicted if rising monopoly power was causing higher profitability). The coefficient from the regression over 1987-2016 with industry and year fixed effects suggests that the average increase in top-20 import-adjusted sales concentration over 1987-2016 was associated with 0.003 points increase in the median firm Q at the industry level, while the average fall in the labor rent share was associated with 0.06 points increase in median Q (although neither are significantly different from zero with industry and year fixed effects). The median industry saw an increase in its median firm Q of 0.34 over the period – suggesting again that the decline in worker power has more explanatory power than the rise in concentration for changes in industry-level profitability.

## 2.5 Unemployment, inflation, and the Phillips Curve

Recent decades in the U.S. have seen a substantial decline in the trend unemployment rate, without inflationary pressure. The unemployment rate was below 5%, the level previously thought to have been the NAIRU, for nearly half of the twenty-three years from 1997 to 2020, and was below 4% from May 2018 until February 2020, at levels not reached since the 1960s. At the same time, inflation has been low and has shown little sign of accelerating. These facts suggest that there has been a fall in the NAIRU (Crump et al 2019, Tuzemen 2019, Blanchard et al 2015). In this section of the paper, we argue that falling worker power could account for these broad features of the unemploy-

[60] This is consistent with Salinger (1984), who argued that in the 1980s, Q was low in industries with high monopoly power because unionized workers received the monopoly rents.

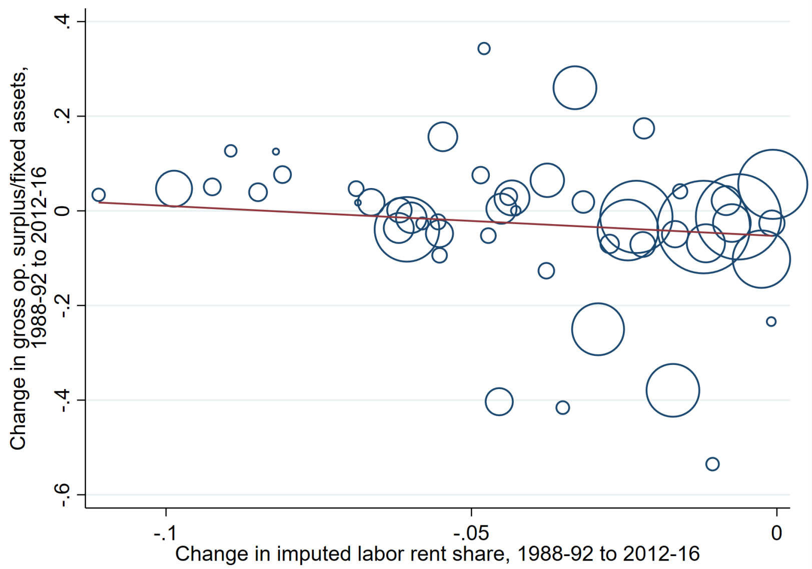



**Figure 2.12:** Change in gross profitability and imputed labor rent share, by industry

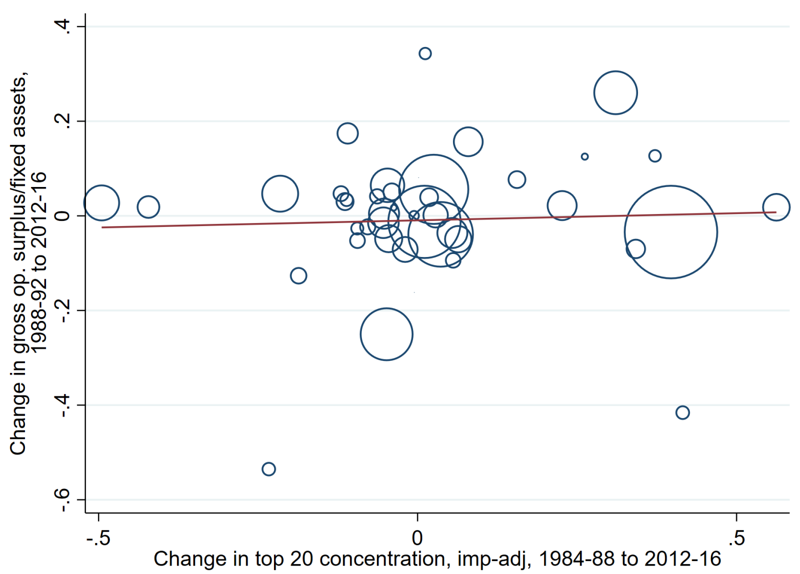



**Figure 2.13:** Change in gross profitability and top 20 sales concentration (imp-adj), by industry
Notes to Figs 12-13: Each bubble is an industry (at BEA industry code level). Bubble size represents industry average employment over 2012-2016. The red line is an employment-weighted line of best fit. Concentration data calculated from Compustat by Covarrubias et al (2019). Imputed labor rent share is our calculation.

**Table 2.4:** Industry-Level Regressions – Labor shares, profitability, and investment-to-profits

*Panel A: Regressions of labor shares and investment-profit on labor rent share and Compustat concentration. N = 1,189 (41 industries, 1987-2016)*

| *Dependent variable:* | Labor share of gross value added | | | | Labor share of net value added | | | | Investment to profit ratio | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Imputed labor rent share of | 2.24** | 2.44** | 1.81** | 2.22** | 2.40** | 2.62** | 2.56** | 3.65** | 3.12** | 3.78* | 2.26** | 5.74** |
| gross value added[a] | -0.5 | -0.64 | -0.16 | -0.26 | -0.39 | -0.47 | -0.32 | -0.44 | -1.12 | -1.47 | -0.68 | -1.3 |
| Avg top 20 sales concentration, | -0.23** | -0.23** | -0.05 | -0.05 | -0.19* | -0.21* | -0.06 | -0.07 | 0.25 | 0.24 | -0.15 | -0.16 |
| imp-adj (Compustat) | -0.06 | -0.07 | -0.05 | -0.05 | -0.08 | -0.08 | -0.09 | -0.07 | -0.19 | -0.2 | -0.22 | -0.2 |
| Fixed effects | None | Year | Ind | Yr, Ind | None | Year | Ind | Yr, Ind | None | Year | Ind | Yr, Ind |

*Panel B: Regressions of profitability on labor rent share and Compustat concentration. N=1,189 (41 industries, 1987-2016)*

| *Dependent variable:* | Gross profit rate | | | | Aggregate Q | | | | Median Q | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Imputed labor rent share of | -0.8 | -1.03 | -0.60* | -1.93** | -3.66** | -3.16* | -3.17** | 1.58 | -3.22** | -2.46* | -4.43** | -1.24 |
| gross value added | -0.55 | -0.71 | -0.28 | -0.55 | -1.14 | -1.33 | -0.93 | -1.51 | -0.99 | -1.16 | -0.86 | -1.36 |
| Avg top 20 sales concentration, | -0.11 | -0.1 | 0.02 | 0.04 | 0.02 | 0.01 | -0.31 | -0.31 | 0.19 | 0.18 | 0.15 | 0.16 |
| imp-adj (Compustat) | -0.1 | -0.1 | -0.13 | -0.12 | -0.12 | -0.13 | -0.32 | -0.3 | -0.13 | -0.13 | -0.2 | -0.2 |
| Fixed effects | None | Year | Ind | Yr, Ind | None | Year | Ind | Yr, Ind | None | Year | Ind | Yr, Ind |

*Panel C: Regressions of labor shares and investment-profit on labor rent share and Census concentration. N = 174 (45 ind. for 1997, 2002, '07, '12)*

| *Dependent variable:* | Labor share of gross value added | | | | Labor share of net value added | | | | Investment to profit ratio | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Imputed labor rent share of | 1.88** | 1.95** | 2.18** | 2.67** | 2.45** | 2.56** | 3.14** | 3.76** | 4.32* | 4.52* | 5.29+ | 7.24+ |
| gross value added[a] | -0.65 | -0.72 | -0.37 | -0.42 | -0.49 | -0.54 | -0.43 | -0.48 | -1.76 | -1.91 | -2.89 | -4.29 |
| Avg top 20 sales concentration, | -0.51** | -0.52** | -0.24* | -0.28* | -0.45** | -0.46** | -0.40* | -0.45** | 0.49 | 0.48 | -0.85 | -0.88 |
| imp-adj (Census) | -0.09 | -0.09 | -0.11 | -0.11 | -0.12 | -0.12 | -0.18 | -0.16 | -0.42 | -0.42 | -0.68 | -0.78 |
| Fixed effects | None | Year | Ind | Yr, Ind | None | Year | Ind | Yr, Ind | None | Year | Ind | Yr, Ind |

*Panel D: Regressions of profitability measures on labor rent share and Census concentration. N = 174 (47 ind. for 1997, 2002, '07, '12)*

| *Dependent variable:* | Gross profit rate | | | | Aggregate Q | | | | Median Q | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Imputed labor rent share of | -0.75 | -0.84 | -1.35* | -2.84** | -1.79 | -2.15 | 0.52 | -2.22 | -0.79 | -0.22 | -3.25* | -1.56 |
| gross value added | -0.81 | -0.85 | -0.58 | -0.55 | -1.52 | -1.62 | -2.2 | -3.06 | -1.16 | -1.14 | -1.48 | -1.95 |
| Avg top 20 sales concentration, | -0.45 | -0.44 | 0.35 | 0.45 | -0.47* | -0.43+ | -1.49+ | -0.89 | -0.09 | -0.1 | -0.91+ | -0.39 |
| imp-adj (Census) | -0.33 | -0.33 | -0.29 | -0.31 | -0.22 | -0.22 | -0.86 | -0.71 | -0.16 | -0.16 | -0.47 | -0.42 |
| Fixed effects | None | Year | Ind | Yr, Ind | None | Year | Ind | Yr, Ind | None | Year | Ind | Yr, Ind |

Robust standard errors, clustered at industry level, in parentheses. + p<0.10, * p<0.05, ** p<0.01.

a: Imputed labor rent share of gross value added is used for gross labor share and investment-profit regressions. Imputed labor rent share of net value added is used for net labor share regressions. Note also: Investment-profits are 98% winsorized. Regressions are for 41/45 industries because we do not have concentration data for all 51 non-financial industries.

ment and inflation experience.

On a theoretical level, the fall in the NAIRU could be explained by a fall in worker power. Almost all models of worker insider power or rent-sharing power would predict that as worker bargaining power falls, the NAIRU would also fall. The mechanisms – and their welfare implications – vary according to the model. First, a fall in worker bargaining power may reduce the marginal cost to a firm of increasing its employment, reducing unemployment (see e.g. Mortensen and Pissarides 1999, Figura and Ratner 2015). Blanchard and Giavazzi (2003) model the implications of worker power and monopoly power jointly: in their model falling worker power leads to lower unemployment as the incentive for firms to hire rises, while rising monopoly power leads to higher unemployment as firms reduce their output.[61] Second, this effect may be reinforced or magnified by a reduction in the distinction between insiders and outsiders in wage-setting (see e.g. Blanchard and Summers 1986, Calmfors and Driffil 1988, Gali 2020). Third, a reduction in the availability of high wage jobs at, for example, unionized firms may reduce the incentives for "wait unemployment", where unemployed workers search for longer to try to get a high wage job, or "rest unemployment", where unemployed workers in high-rent sectors with temporary downturns wait for jobs to return (e.g. Hall 1975, Bulow and Summers 1986, Alvarez and Veracierto 1999, Alvarez and Shimer 2011).[62] Past empirical evidence suggested that areas and industries with higher rates of unionization have tended to have higher unemployment rates, and unionized firms have tended to see lower employment growth.[63]

[61] More specifically, their model predicts that in the short run (with no entry of firms), falling worker power reduces the labor share with no effect on unemployment, but in the long run (where all firms pay entry costs and there are no positive rents), falling worker power reduces unemployment with no effect on the labor share. If the world is always somewhere between the pure short run and pure long run – there is some entry, but there are still some positive rents – then falling worker power in their model would predict a falling labor share and falling unemployment.

[62] On the other hand, in very frictional labor markets where a low elasticity of labor supply to the firm enables a large wage markdown, aggregate unemployment could fall as worker bargaining power rises (Manning 2003).

[63] See e.g. Freeman and Medoff (1984), Summers (1986), Montgomery (1989), Blanchflower, Millward, and Oswald (1991), Leonard (1992).

More recently, Erickson and Mitchell (2007), Figura and Ratner (2015) and Krueger (2018) have argued that the fall in labor power would lower the NAIRU, and Leduc and Wilson (2017) and Ratner and Sim (2020) have argued that a fall in worker bargaining power could have caused the flattening of the Phillips Curve.[64]

It is less clear how to reconcile trends in the NAIRU with rising globalization, technological change, or monopoly power: the other main explanations for the trends in the labor share and corporate profitability we examine in this paper. While increased globalization and technological change may have led to disinflationary pressure in the U.S. economy, their effect on the NAIRU would be ambiguous: disinflationary pressure as a result of lower input costs may reduce the NAIRU, but the job displacement associated with both of these phenomena may increase it.[65] And it is not possible to explain the substantial fall in the NAIRU as a result of an increase in aggregate monopoly power. While theoretical models differ on whether rising monopoly power should increase unemployment or leave it constant, there is no a priori reason to believe that an increase in monopoly power would reduce unemployment; and at the same time, an increase in monopoly power may be a source of inflationary pressure.[66] Neither of these appear obviously compatible with the trends of falling unemployment and low and stable inflation that have characterized the last

[64] A number of other drivers have been posited for the fall in the NAIRU, including the changing demographic composition of the workforce (Shimer 1998, Tuzemen 2018), changes in productivity growth (Ball and Mankiw 2002), improvements in job matching (Katz and Krueger 1999), and, most recently, the decline in job destruction and reallocation intensity and the aging of workers and firms (Crump et al 2019).

[65] See, for example, Kohn (2005).

[66] In some models of monopoly power, the employment rate is reduced with no effect on the unemployment rate. In other models, rising monopoly power leads to rising unemployment. Manning (1990), for example, shows that rising monopoly power combined with increasing returns to scale can lead to higher unemployment. Blanchard and Giavazzi (2003), Geroski, Gregg and Van Reenen (1996), and Ebell and Haefke (2009) show that monopoly power plus some non-zero worker bargaining power can lead to higher unemployment. In terms of inflation: Higher markups would likely imply a higher price level (in the presence of some downward nominal wage rigidity), and therefore an increase in the inflation rate during the transition from one steady state to a new, higher-markup steady state (see, for example, Phelps (1968)). An increase in markups, acting as a cost-push shock, would tend to imply a higher level of inflation for a given degree of labor market slack.

three to four decades (as noted by Van Reenen 2018, Basu 2019 and Syverson 2019).[67]

### 2.5.1 State-level evidence

The theory discussed above suggests that falling worker power could explain the aggregate decline in unemployment seen in the U.S. in recent decades. State-level trends in unemployment and labor rents are consistent with this. Figure 2.14 shows that states with bigger falls in their imputed labor rent share over 1984-2016 also had bigger falls in their state unemployment rate.[68] Regressing the state unemployment rate on the state imputed labor rent share at the annual level, with various combinations of industry and year fixed effects, we find a consistently large, positive, and significant relationship between the two variables: higher state labor rent shares are associated with higher unemployment, with the coefficient in the specification with year and state fixed effects suggesting that a 1 percentage point lower labor rent share of GDP is associated with 0.15 percentage points lower unemployment (as shown in Table 2.5).[69]

### 2.5.2 Industry-level evidence

Industry-level patterns in unemployment and labor rents are also consistent with the hypothesis that declining worker power has lowered the NAIRU. As we found at the state level, industries which saw larger declines in their imputed labor rent share saw larger declines in their industry-level unemployment rate (Figure 2.15).[70] Regressions of the annual industry-level unemployment rate

[67] Note also that increasing monopsony power would tend to be associated with less hiring and increased labor market frictions, and so also does not provide a natural explanation for a declining NAIRU.

[68] The coefficient on the line of best fit is 0.36, and the p value is 0.01. The R-squared is 13%.

[69] Disaggregating the unemployment rate by age and gender, the large, statistically significant relationship between state-level labor rents and unemployment rates holds for workers aged 25-54, and 16-24, for both men and women, but not for workers aged 55 to 65. The estimated coefficients are particularly large for all workers aged 16 to 24 and for women aged 25-54, consistent with Bertola, Blau, and Kahn's (2007) cross-national findings.

[70] We measure industry unemployment in the CPS, defining it as the unemployment rate amongst all workers who reported having worked in a given industry in their current job (if employed) or most recent job (if

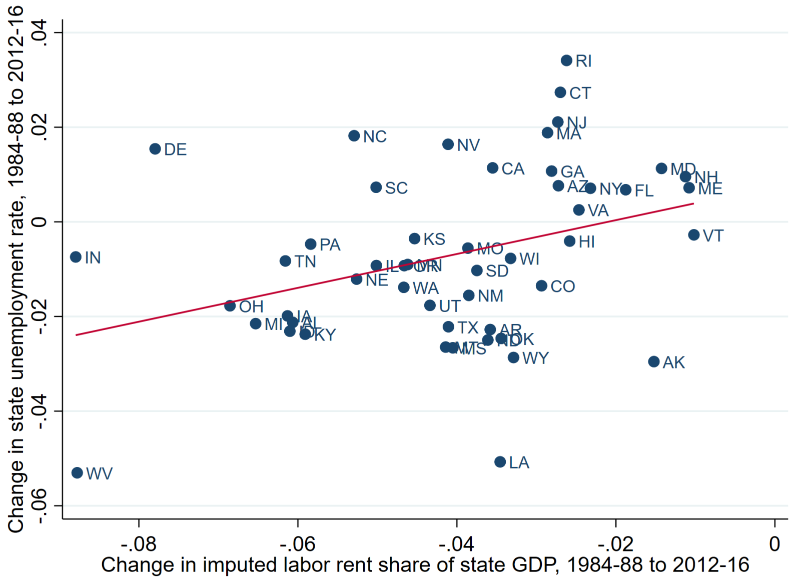



**Figure 2.14:** State-level changes in unemployment and labor rents, 1984-88 to 2012-16
Notes: The red line is a line of best fit. State unemployment rate is calculated from CPS.

**Table 2.5:** State-level regressions of unemployment on measures of labor power

| | | | | |
|---|---|---|---|---|
| *Panel A: Regression of unemployment on imputed labor rent share, 1984-2016* | | | | |
| Imputed labor rent share of state GDP | 0.14* <br> -0.06 | 0.22* <br> -0.09 | 0.08+ <br> -0.04 | 0.15* <br> -0.06 |
| Fixed effects | None | Year | State | Year, State |
| Observations | 1,650 | 1,650 | 1,650 | 1,650 |
| *Panel B: Regression of unemployment on imputed union rent share, 1984-2016* | | | | |
| Imputed union rent share of state GDP | 0.56** <br> -0.18 | 0.60* <br> -0.24 | 0.50** <br> -0.16 | 0.54* <br> -0.24 |
| Fixed effects | None | Year | State | Year, State |
| Observations | 1,650 | 1,650 | 1,650 | 1,650 |

Robust standard errors, clustered at state level, in parentheses. + p<0.10, * p<0.05, ** p<0.01.

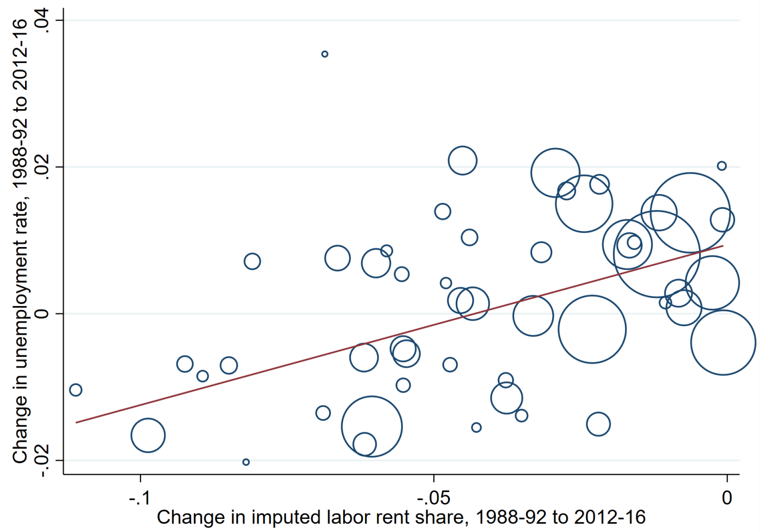



**Figure 2.15:** Change in unemployment and imputed labor rent share, by industry
Notes: Bubble size represents industry average employment 2012-2016. Red line is an employment-weighted line of best fit.

on the imputed labor rent share and imputed union rent share, with industry and year fixed effects, have positive and significant coefficients (Table 2.6), with the magnitude in the specification with industry and year fixed effects suggesting that a 1 percentage point lower imputed labor rent share is associated with a 0.1 percentage point decline in industry unemployment.[71]

### 2.5.3 Unemployment for college and non-college workers

In Section 2.3, we decomposed the decline in labor rents for workers with and without a college degree (BA+) over 1984-2016, and showed that while both groups saw a decline in their labor rents, the decline was substantially larger for non-college workers. If declining labor rents leads to a lower

unemployed).

[71] Supplementing this analysis, we also show in Appendix B.3 that there is a significant relationship between industry-level unemployment and unionization rates, and between industry-level labor market tightness, labor rent shares, and unionization rates. Note that, in contrast, regressions of the annual industry-level unemployment rate on measures of industrial concentration show no significant relationship, and the coefficients are positive.

**Table 2.6:** Industry-level regressions of unemployment on measures of labor power

| | | | | |
|---|---|---|---|---|
| *Panel A: Regression of unemployment on imputed labor rent share, 1987-2016* | | | | |
| Imputed labor rent share of | -0.16** | -0.16** | -0.03 | 0.10** |
| gross value added | -0.05 | -0.05 | -0.03 | -0.03 |
| Fixed effects | None | Year | Ind | Year, Ind |
| Observations | 1,530 | 1,530 | 1,530 | 1,530 |
| *Panel B: Regression of unemployment on imputed union rent share, 1987-2016* | | | | |
| Imputed union rent share of | -0.27* | -0.23+ | -0.21* | 0.20** |
| gross value added | -0.1 | -0.12 | -0.08 | -0.06 |
| Fixed effects | None | Year | Ind | Year, Ind |
| Observations | 1,530 | 1,530 | 1,530 | 1,530 |

Robust standard errors, clustered at state level, in parentheses. + $p<0.10$, * $p<0.05$, ** $p<0.01$. 51 industries.

NAIRU, one might expect to see larger declines in average unemployment for non-college workers than for college educated workers over the same period. This has been the case: the unemployment rate of workers without a four-year college degree has fallen substantially relative to the unemployment rate of workers with a bachelors' degree, as shown in Figure 2.16.

### 2.5.4 Quantitative implications for the NAIRU

Can we say anything about whether the magnitude of the decline in worker power is big enough to account for the decline in the NAIRU? One recent study on this topic is Figura and Ratner (2015), who study the decline in worker power as proxied for by the decline in the labor share of income. They show that industries and states with bigger falls in their labor share over 2001-2014 saw bigger increases in their vacancy/unemployment ratio (labor market tightness). They argue that this is consistent with a decline in worker bargaining power increasing the incentive for firms to create jobs, and that the decline in the labor share of income could have led to a two-thirds of a percentage

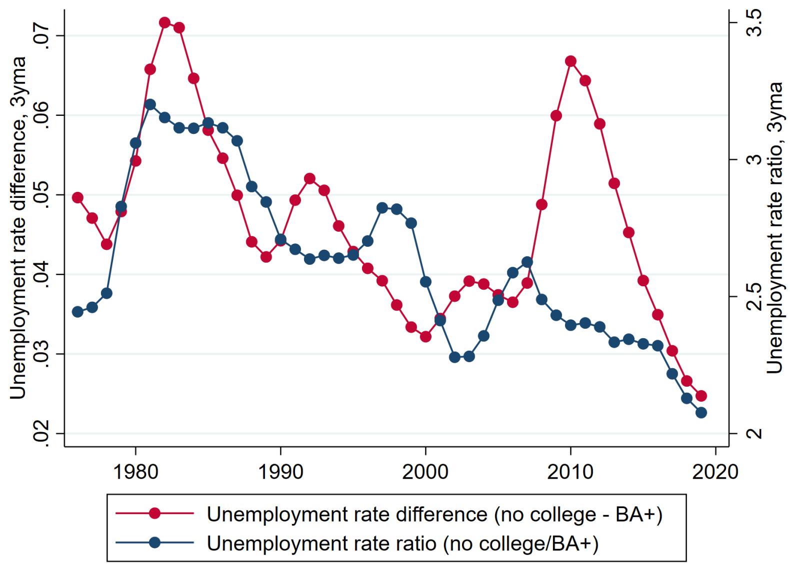



**Figure 2.16:** Unemployment, non-college relative to college workers, 1976-2019, 3-year moving avg.
Note: BA+ and non-college unemployment rates calculated from CPS. Unemployment rate difference is defined as non-college unemployment rate minus BA+unemployment rate. Unemployment rate ratio is defined as non-college unemployment rate divded by BA+ unemployment rate. “Non-college” refers to people with no or some college education; “BA+” refers to people with a bachelors’ degree or more. Points are 3-year moving averages.

point fall in the NAIRU.[72] We can similarly use our state-level and industry-level estimates to back out a naïve extrapolation of the aggregate relationship between worker power and unemployment. Applying the coefficients from the state-level regressions in Table 2.5 to our estimate of the fall in labor rents in the nonfinancial corporate sector over 1982 to 2016 (a fall of 5.1 percentage points) would have predicted a three-quarters of a percentage point fall in the NAIRU.We have reason to believe that both the Figura and Ratner (2015) estimate and our estimate of the effect of the decline of worker power on the NAIRU may be underestimates of the true effect, since they are based on state/industry-level variation which may miss some aggregate effect, and since the imperfection of the labor share (in the case of Figura and Ratner) or the imputed labor rent share (in our case) as proxies for the decline in worker power is likely to cause attenuation bias.[73]

## 2.6 Possible objections and further considerations

### 2.6.1 Investment

Investment has been falling over recent decades relative to measures of corporate profitability such as operating surplus and Tobin's Q, as well as relative to GDP and fixed assets (Gutiérrez and Philippon 2017, Alexander and Eberly 2018, Crouzet and Eberly 2019). These trends have been a major motivator of the monopoly power argument (see e.g. Gutiérrez and Philippon 2017, Eggertsson et al 2018). One might argue these trends in investment are hard to reconcile with our argument that there has been a macroeconomically important decline in worker power: some models predict that a

[72] More formally, they argue that the negative relationship they find between the labor share and the V/U ratio is consistent with a counter-clockwise rotation in the Job Creation curve in a standard DMP search model. After estimating the slope of the Beveridge curve, they can then estimate the degree to which a decline in worker bargaining power may affect equilibrium unemployment.

[73] While a full model-based investigation of the degree to which the decline in worker power may have affected the NAIRU is beyond the scope of this paper, we carry out four back-of-the-envelope exercises in Appendix B.5. These illustrate that, in simple models with plausible parameter values, it is possible for the decline in worker power that we have seen to generate very large changes in the NAIRU.

decline in worker power, reducing the marginal cost of production, would lead to an increase in investment.[74] To what extent are the facts on investment compatible with our argument of declining worker power?

First, we note that it is not clear that investment, properly measured, *has* declined substantially relative to value added or fixed assets. The relative price of investment goods has declined, meaning that while there has been a decline in net investment relative to net value added in nominal terms, there has been no decline in net real fixed investment relative to net real value added in the nonfinancial corporate sector (as shown in Figure 2.17).[75] And Crouzet and Eberly (2019, 2020) show that a rise in intangible investment could account for the majority of the apparent decline in investment relative to fixed assets. Second, we note that the theoretical predictions of declining worker power for investment are actually ambiguous. It is possible that a decline in worker power leads to less investment: by reducing the marginal cost of labor to firms, declining worker power may lead to the substitution of labor for capital (or at least, less substitution of capital for labor), reducing investment relative to a scenario where worker power had not declined.

Third, the fall in investment relative to measures of corporate profits can be explained by our declining worker power hypothesis. In efficient bargain models of worker rent-sharing, the degree of worker power does not affect the firm's investment decision. The firm optimally maximizes profits, then distributes the rents between labor and capital. To understand if investment has fallen relative to the underlying profitability of firms, we must therefore measure both profits to capital and

[74] As argued by Eggertsson et al (2018), for example.

[75] Net investment to net value added is calculated using data on gross nonresidential investment and the consumption of nonresidential fixed capital by nonfinancial corporate business, from the Fed Z1 accounts, and gross value added in the nonfinancial corporate business sector from BEA NIPA. For the ratio of real net investment to real net value added, investment is deflated by the implicit price deflator for nonresidential fixed private sector domestic investment from the BEA, and value added is deflated by the implicit price deflator for nonfinancial corporate business, from the BLS.

profits to labor. Defining the ratio of investment to total profits as follows:

$$\frac{\text{Investment}}{\text{Total profits}} = \frac{\text{Investment}}{\text{Net operating surplus } + \text{ imputed labor rents}}$$

we show in Figure 2.18 that while net investment over net operating surplus (profits to capital) has fallen substantially over the last thirty years in the nonfinancial corporate sector, average net investment over our measure of net total profits has only declined very slightly. That is, even nominal investment has not weakened much relative to our measure of firms' total profitability.[76] The relationship between labor power and investment-to-profits also holds at the industry level: industries with larger declines in their imputed labor rent share saw larger declines in the ratio of investment to operating surplus, even in annual regressions when controlling for a variety of industry and year fixed effects (Table 2.4).[77]

### 2.6.2 Firm-level dynamics: labor shares, and markups

Our analysis in this paper is primarily at the industry and aggregate level. Recent research has emphasized the role of firm-level dynamics in trends in labor shares, markups, and wages. First, several papers find a large role for between-firm reallocation in the decline of the labor share and rise in measured markups. Second, research with matched employer-employee data suggests that the dispersion of average earnings at the firm level has risen. Can we reconcile our results with this firm-level evidence on labor shares, markups, and wages?

Labor share and markups: Autor et al (2020) find that two-thirds of the decline in the aggre-

[76] Crouzet and Eberly (2020) attribute a share of the growing weakness of investment relative to Q to product market rents. Our explanation could be compatible with this: instead of the product market rents arising from increased monopoly power, they may have been rents that were previously paid to labor so did not show up in Q.

[77] In contrast, coefficients on average top 20 sales concentration are noisy (see Table 2.4), and there is no apparent relationship between the change over 1988-2016 in the average top 20 sales concentration ratio and the investment-to- gross operating surplus ratio (see Appendix B.3).

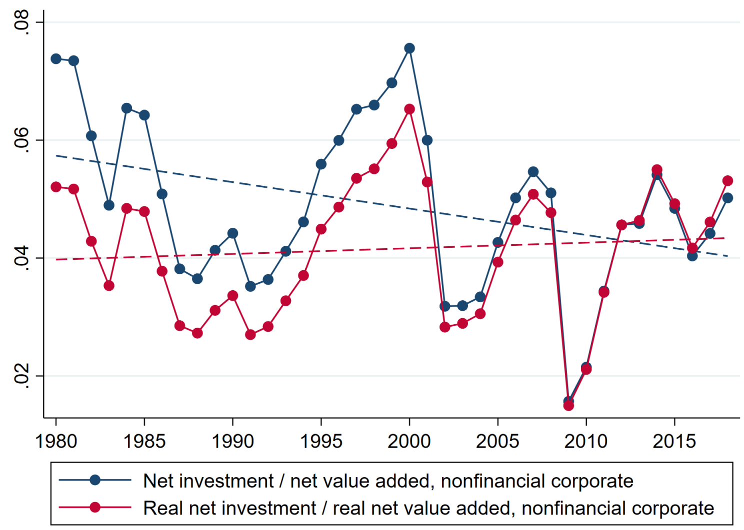



**Figure 2.17:** Real and nominal net investment over net value added, nonfinancial corporate sector

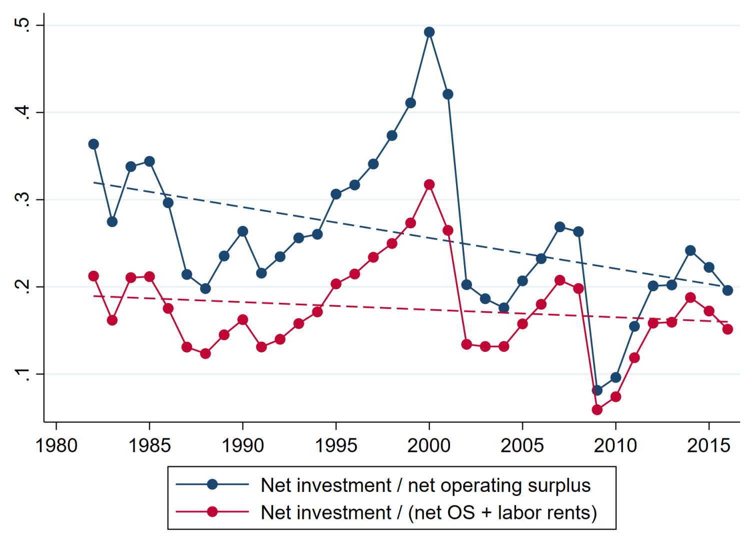



**Figure 2.18:** Ratio of net investment to profits to capital, and to imputed total profits, nonfinancial corporate

Notes to Figs 2.17 and 2.18: Net investment is measured as gross fixed investment in nonresidential structures, equipment, and intellectual property products, minus consumption of fixed capital (both from Fed Z1 accounts). For Figure 2.17, the deflator for investment is implicit price deflator for nonresidential fixed private sector domestic investment from BEA, and deflator for value added is implicit price deflator for nonfinancial corporate business from BEA.

gate labor share can be explained by between-firm reallocation, with one-third explained by within-firm falls in the labor share. The median firm saw no decline in their labor share, while firms with initially low labor shares saw their labor shares fall still further. Kehrig and Vincent (2020) find similar dynamics in manufacturing, showing that the decline in the labor share is driven by establishments which are growing in size and at the same time see falling labor shares. De Loecker, Eeckhout and Unger (2020) find that the rise in the aggregate measured markup results largely from a reallocation of activity to high-markup firms, the median markup did not change, and markups for already high markup firms increased.

It is clear that our proposed mechanism – a fall in labor rent-sharing power – could explain within-firm declines in labor shares and increases in measured markups. It is also possible to reconcile our proposed mechanism with the portion of the decline in the labor share (or rise in measured markups) that results from the reallocation of economic activity across firms. First, it could simply be the case that firms which experienced bigger falls in worker power also grew faster for some exogenous reason. Second, it is possible that this faster growth itself is at least partly a result of falling worker power. To see this, note that (1) if workers receive a competitive wage plus some portion of a firm's rents, then unit labor costs are higher at high-rent firms than at low-rent firms, but (2) unless workers' share of rents in high-rent firms is higher than the aggregate labor share, high-rent firms will still have lower labor shares than low-rent firms. Therefore, as workers' rent-sharing power declines, unit labor costs fall disproportionately more at high-rent, low-labor-share firms than at low-rent, high-labor-share firms. This improves the competitive advantage of high-rent firms, creating an incentive for them to expand. This would lead to a reallocation of economic activity from high labor share to low labor share firms.

Firm wage effects: There has been an increase in the dispersion of average wages at the firm level over recent decades, which has led to suggestions that this could indicate a divergence in firm-level rents (see e.g. Barth et al 2016). This might be seen as supporting the hypothesis of rising monopoly power, rather than declining worker power. In fact, the evidence is more consistent with declining worker power. Song et al (2019) use matched employer-employee data to decompose the variance of U.S. wages into firm effects, worker effects, and the covariance of the two, following Abowd, Kramarz, and Margolis (1999) ("AKM"). The firm effects indicate the firm-specific pay premium, holding worker "quality" constant, and can be interpreted as some combination of rent-sharing and compensating differentials (Sorkin 2018, Card et al 2018). Song et al (2019) show that the increase in the variance of firm-level average wages over 1980-2013 was entirely due to an increase in the *sorting* of high-wage workers into high-wage firms, and not an increase in the dispersion of the firm *premia* paid to equivalent workers (Song et al 2019). In fact, they find a small decline in the variance of firm effects over the period. These trends are consistent with a decline in rent-sharing: the decline in the variance of firm fixed effects could reflect declining wage premia in formerly high-wage firms, and the increase in the sorting of high-wage workers into high-wage firms (and vice versa) could reflect the fissuring of the workplace. On the other hand, if an increase in monopoly power had caused total rents to increase, holding constant the initial degree of rent-sharing with workers, one would have expected firm effects to become more dispersed rather than less (if rents increased more for already high-rent firms).

Note that the decline in the variance of firm fixed effects estimated by Song et al (2019) has been substantially smaller than the decline we estimate in the variance of industry fixed effects. There are two ways to reconcile these. First, note that a large decline in the variance of industry wage premia, but a small decline in the variance of firm wage premia, would be consistent with an aggregate decline in labor rents as a result of the fissuring of the workplace, as an increasing share of workers work at firms with low rents (and fewer at firms with high rents).[78] Second, evidence from La-

[78] Following the suggestion of Christina Patterson in her remarks at the Spring 2020 BPEA meetings, we note that the relationship between firm and industry effects can be written as $\gamma_{\text{ind}} = \sum_j \frac{E_j}{E_{\text{ind}}} \gamma_j$, where $\gamma_{\text{ind}}$ and $\gamma_j$ denote industry and firm wage effects, respectively.

chowska et al (2020), who carry out an AKM decomposition in Washington state over 2002-2014, suggests that the underlying secular decline in the variance of firm fixed effects over recent decades may have been larger than that estimated by Song et al (2019). Specifically, the decline in the variance of firm fixed effects for hourly wages over 1980-2013 may have been larger than that estimated by Song et al for annual earnings. Also, since the variance of firm fixed effects appears to be cyclical in Lachowska et al (2020), and the endpoint of the Song et al calculation – 2007-2013 – was a period with a historically weak labor market, one might have expected the variance of firm fixed effects in this period to be cyclically high.[79]

### 2.6.3 Labor rents to the highly-paid: executive compensation and finance

There has been roughly a doubling of the share of national income accruing to executives, managers, and supervisors in non-financial firms since 1979 (Bakija, Cole, and Heim 2012). High-earning financial sector workers have also seen large rises in their compensation. Could these reflect rising labor rents?[80]

First, note that we estimate labor rents from the CPS, where the earnings data is top-coded, and non-response is high for people in the top tail of the income distribution. This means that estimate of the decline in labor rents should be considered to be the decline in rents for the majority of workers, but not including the highest-paid – and so, not including top executives, managers, or many

[79]Song et al (2019) find that over 1980-87 to 2007-13 there was a decline of about 3.5% in the variance of firm fixed effects. While they use different data sets, Lachowska et al (2020) and Song et al (2019) find very similar declines in the variance of firm fixed effects for annual earnings over the period they study in common (2002-2014), suggesting the two studies may be comparable. Lachowska et al find a much larger decline in the variance of firm fixed effects four hourly wages than annual earnings over this period, and they find large countercyclicality in the variance of firm fixed effects, with their estimates suggest that the variance of firm fixed effects will have been particularly high during 2007-2013 period (the endpoint of the comparison in Song et al (2019)).

[80]See Bivens and Mishel (2013) for evidence on the existence of rents in executive pay, and Phillipon and Reshef (2006) for evidence on rents in financial sector compensation.

financial sector workers.[81]

It is, therefore, plausible that some of the lost labor rents we measure were redistributed to top management and executives, rather than to shareholders. Indeed, this could be consistent with our evidence, since we estimate that the decline in the labor rent share of value added in the nonfinancial corporate sector (for the majority of workers) was greater than the actual decline in the labor share (which includes the executive compensation). Note, though, that the majority of the increase in executive compensation over this period accrued to executives and managers who receive self-employment, S-corporation, or partnership income (Bakija, Cole, and Heim 2012; Smith, Yagan, Zidar, and Zwick 2019).[82] Since it is ambiguous whether income from these sources should be considered capital or labor income, it is unclear whether to consider the rising income of executives and managers of S-corporations and partnerships as a redistribution of rents from workers' labor income to managers' labor income, or simply from labor to capital.[83]

It is also plausible that some of the lost labor rents we measure were redistributed to high-paid financial sector workers. When estimating labor rents for entire corporate sector (including finance), we find a very similar decline in labor rents as we do for the nonfinancial sector – meaning that the

[81] Specifically: our baseline estimate of labor rents, for the nonfinancial corporate sector, will omit high-paid executives and managers because of CPS top-coding/non-response, and will omit all financial sector workers by construction. Our estimate of labor rents for the entire corporate sector will include many financial sector workers, but will omit increases in pay for the highest-paid financial sector workers because of CPS top-coding. See Appendix B.2 for more details.

[82] Bakija et al (2012) estimate that the increase in the income share of top 1% managers, executives, and supervisors who work for closely-held businesses was around 2.2 percentage points over 1979 to 2005, while the increase in the income share of top 1% salaried managers, executives, and supervisors was only around 0.4 percentage points.

[83] Note also that Smith et al (2019) argue that the decline in the labor share has been overstated because of the increase in income accruing to the top 1% of earners which comes from pass-through enterprises. This income is booked as capital income but some may more appropriately be considered labor income. While the degree of the decline in the aggregate labor income share may be ambiguous as a result of the difficulties of imputing passthrough income to labor or capital (and imputing self-employment income), what is not ambiguous is that the share of total income going to the vast majority of workers has declined since the 1980s. For example, Piketty, Saez, and Zucman (2018) estimate that for the bottom 99% of people, for example, the share of total national income accounted for by labor compensation declined from 69% in 1978 to 59% in 2014.

inclusion of the majority of financial sector workers does not affect our conclusions. However, since the CPS earnings data is top-coded, our calculation will miss any increase in rents accruing to very highly-paid professionals in finance. In our CPS-ORG data, the share of workers in Finance, Insurance, and Real Estate who had top-coded earnings rose from 2% in 2000 to 9% by 2019. It is possible that these workers saw their labor rents increase over the period where the majority of workers saw labor rents decrease – but note that, since this is a relatively small group of workers, even rather drastic increases in rents for the top 5-10% of financial sector workers would not have made a major difference to the overall trend in labor rents for the entire corporate sector.

### 2.6.4 Occupational licensing

While unionization, industry wage premia, and firm size wage premia have fallen over recent decades, the extent of occupational licensing has risen. Have we overestimated the decline in labor rents by failing to consider occupational licensing? We believe that accounting for the rise of occupational licensing would not substantially change our results. First, note that for many professions in which occupational licensing has increased in recent years, occupational licenses are less likely to transfer rents worker to capital owners than they are to transfer rents from unlicensed workers to licensed workers, or from consumers to workers (for example, hairdressers, manicurists, and cosmetologists, real estate agents, or self-employed workers in the building trades). Recent work by Kleiner and Soltas (2019) estimates that 70% of the welfare loss of "marginal" occupational licensing is borne by workers. Even if we were to assume that all rents accruing to workers as a result of occupational licensing were obtained at the expense of capital, a back-of-the-envelope calculation suggests that the rise of occupational licensing could only have resulted in an increase in labor rents of 0.2-0.7 percentage points of value added: the share of the U.S. labor force required to have an occupational licence is estimated to have risen by around 7-12% from the 1980s until 2008 (Kleiner and Krueger 2013, Council of Economic Advisers 2015), and the wage premium for licensed workers in the U.S.

appears to be in the range of 4-8% (Gittleman, Klee, and Kleiner 2017; Bryson and Kleiner 2019).

### 2.6.5 Further evidence on labor shares and concentration

In this section, we address a number of empirical trends which point to weaknesses in the arguments that globalization, technological change, and/or monopoly and monopsony power were the predominant drivers of the falling labor share and rising corporate profits. First, we note that while technological change and globalization are ubiquitous, the extent of increases in inequality – both between capital and labor incomes, and within labor incomes – differ substantially across countries (see e.g. Gutiérrez and Piton 2019). This would tend to suggest a substantial role for country-specific factors in explaining the decline in the labor share – as argued by Philippon (2020) among others – pointing up the monopoly power or worker power explanations as candidates. A large proportion of the decline in the U.S. labor share can be accounted for by the manufacturing sector. The centrality of the manufacturing sector in the decline in the U.S. labor share would tend to favor the declining worker power hypothesis over the rising monopoly power hypothesis: given the increases in international trade driven by the opening of low-wage economies to international markets, and reductions in transport costs and trade barriers, it seems unlikely that U.S. manufacturing has seen a substantial increase in product market power over recent decades. In contrast, the manufacturing sector saw large declines in unionization over recent decades and can account for a large share of our estimated decline in labor rents.

Our hypothesis, which emphasizes the relative power of labor and capital, can therefore fit the combination of cross-country and cross-industry facts better than hypotheses based on globalization, technological change, or monopoly power (given far more empowered shareholders and weaker unions in the U.S. than in the rest of the industrial world). In keeping with this, cross-country evidence from Kristal (2010) and Jaumotte and Osorio Buitron (2015) suggests that countries with bigger declines in unionization saw bigger declines in their labor shares and bigger increases in income inequality.[84]

Second, while monopoly power and monopsony power are without doubt present in certain parts of the U.S. economy[85] – and our baseline framework in fact assumes the existence of both types of power – we also note that the direct evidence of a large aggregate increase in either monopoly power or monopsony power is unclear.

The large rise in industry-level sales concentration over recent decades has frequently been invoked as a likely driver of rising monopoly power (Grullon, Larkin, and Michaely 2019; Gutiérrez and Philippon 2017, 2019). Yet industrial organization economists point up a number of reasons to be skeptical that this increase in concentration reflects a large increase in aggregate monopoly power (see, for example, Shapiro (2018), Berry, Gaynor, and Scott Morton (2019), Basu (2019), Syverson (2019)). First, it is not clear whether this large aggregate increase is still present when defining markets appropriately; import-adjusted measures of sales concentration in manufacturing have fallen or risen only marginally since the 1980s (Covarrubias et al 2019), and in many service industries, where the relevant market is often smaller than the entire U.S. market, local-level sales concentration is actually falling, possibly even reflecting increased local-level competition as large firms spread their business into new markets (Rossi-Hansberg et al 2019). Second, industries may become more concentrated as efficient firms compete and win market share: several authors have documented a relationship between rising product market concentration and rising productivity in certain sectors

[84] Bental and Demougin (2010) also argue that cross-country trends in the labor share may have been driven by an erosion of worker bargaining power, but as a result of improved monitoring technologies. Earlier work studying cross-country trends in labor shares includes Bentolila and Saint-Paul (2003).

[85] In terms of monopoly power: Covarrubias, Gutiérrez, and Philippon (2019) and Philippon (2020) for example document that since 2000, rising concentration has been associated with slower turnover of lead firms and rising prices, particularly in Telecoms, Airlines, and Banking, and present case studies of several products where prices are substantially higher in the U.S. than Europe. In terms of monopsony power: Berger et al (2019) estimate welfare losses of 5% of lifetime income arising from employers' power in the labor market (as indexed by workers' elasticity of labor supply); and Schubert et al (2021), and Arnold (2020) find sizeable negative effects on wages for workers in highly concentrated labor markets. See Sokolova and Sorensen (2020) for a review of the empirical evidence on the elasticity of labor supply to the firm.

(Peltzman 2018, Autor et al 2020, Ganapati 2020, Crouzet and Eberly 2019). Third, even where concentration ratios have increased in well-defined markets, they are usually below levels which typically raise profit concerns (Shapiro 2018).[86] In addition, we note that the substantial decline in the large firm wage premium is the opposite of what one would expect to see if large firms were gaining more monopoly power.

Similarly, there is less direct evidence of a rise in labor market monopsony power – in terms of an increasingly inelastic labor supply curve to firms – than there is of a fall in worker power. It does not seem plausible that monopsony power has increased as a result of an increase in labor market concentration (Bivens et al 2018): local labor market concentration is low for most workers, particularly when considering the availability of jobs in other occupations or industries (Schubert, Stansbury, and Taska 2021), and has actually fallen, not risen, for most workers over recent decades (Rinz 2018). Berger, Herkenhoff, and Mongey (2019) estimate that the fall in local labor market concentration since the 1970s was large enough to predict a 3 percentage point increase in the labor share. And while the proliferation of non-compete clauses and occupational licensing requirements may have increased switching costs for some workers, the rise of the internet should at the same time have substantially reduced the costs of job search for workers and employers, so the net change in the degree of labor market frictions is unclear.[87] One piece of evidence which might indicate a rise in

[86] In recent years some authors have also argued that the rise in common ownership across firms, as documented by Azar, Schmalz, and Tecu (2018) among others, has led to reduced competition and increased monopoly power (Azar and Vives 2019). More research would be valuable in this regard; the theoretical links between common ownership concentration and monopolistic behavior by firms remain debated, and there does not yet appear to be a clear empirical consensus on the relationship between common ownership and industry-level outcomes like investment, prices, markups, and production (Schmalz 2018; Backus, Conlon, and Sinkinson 2019).

[87] On non-competes and no-poaching agreements, see Kleiner and Krueger (2013), Krueger and Ashenfelter (2016), Furman and Krueger (2016), Starr et al (2019). On the internet and job search, see Stevenson (2009), Kuhn and Mansour (2014), and Bhuller, Kostol, and Vigtel (2019). There has been a decline in the job-switching rate over time: this may either suggest an increase in the costs of job switching, consistent with higher monopsony power, or a decrease in the dispersion of job-specific rents, reducing workers' incentive to switch jobs (Molloy et al 2011).

monopsony power is Webber (2018), who estimates a decline in the firm-level elasticity of quits to the wage over 2003-2011: more research would be valuable to identify whether this reflects a long-term trend or reflects the slow labor market recovery after the Great Recession.

## 2.7 Concluding remarks

The evidence in this paper suggests that the American economy has become more ruthless, as declining unionization, increasingly demanding and empowered shareholders, decreasing real minimum wages, reduced worker protections, and the increases in outsourcing domestically and abroad have disempowered workers – with profound consequences for the labor market and the broader economy. We argue that the reduction in workers' ability to lay claim to rents within firms could explain the entirety of the change in the distribution of income between labor and capital in the United States in recent decades, and could also explain the rise in corporate valuations, profitability, and measured markups, as well as some of the decline in the NAIRU. We believe the declining worker power hypothesis has been substantially underemphasized as a cause of these macroeconomic trends, relative to other proposed causes: globalization, technological change, and rising monopoly or monopsony power.

An important set of issues which we do not explore in detail relate to inequality in labor income. It seems plausible that the same kinds of situations which encourage rent-sharing also encourage the compression of compensation relative to productivity: unions, generous benefit structures, formalized wage-setting processes and so forth. Consistent with this, we find that the decline in labor rents has been greater for workers without college degrees than for those with college degrees.[88] It is also plausible that the decline in the rent-sharing power of the majority of workers could explain some

[88] There is a large body of work consistent with this. Several authors document an important role for declining unionization in the rise in wage inequality (including DiNardo et al (1996), Card (1996), Farber et al (2018)); others document a role for the rise in outsourcing and the 'fissuring' of the workplace (including Weil 2014).

of the increase the income share of the top 1%. Over 1979 to 2014, the income share of the top 1% is estimated to have risen by between 4.9 and 9 percentage points (Auten and Splinter 2019, Piketty, Saez, and Zucman 2018). If we assume that all of the decline in labor rents we estimate in this paper represented redistribution from the bottom 99% to the top 1% (whether as labor or capital income), it could explain between 41% and 76% of the entire increase in the top 1% income share over the last forty years. If we assume instead that labor rents were redistributed as capital income across the entire income distribution, but in proportion to the actual distribution of capital income arising from firm ownership, then our estimated decline in labor rents could still account for 24%-45% of the increase in the income share of the top 1%.[89]

In future research it would be valuable to more explicitly consider alternative bargaining models and their implications for wages and employment, and for total output and investment. A further promising avenue is distinguishing between the degree of product market monopoly power vs. labor market power in the U.S. economy by estimating markups on different types of inputs. With sufficiently detailed data on input costs, markups could be estimated on non-labor inputs and on labor inputs separately. Markups over labor and non-labor inputs following the same path would be consistent with a rise in monopoly power; markups over non-labor inputs staying constant while markups over labor rise would be more consistent with a fall in worker power or a rise in monopsony power.[90]

A fair question about the labor rents hypothesis regards what it says about the secular stagnation hypothesis that one of us has put forward (Summers 2013). We believe that the shift towards more corporate income, that occurs as labor rents decline, operates to raise saving and reduce demand. The impact on investment of reduced labor power seems to us ambiguous, with lower labor costs on the one hand encouraging expanded output and on the other encouraging more labor-intensive

[89] For details of our calculations, see Appendix B.3.

[90] Though, finding differential trends in markups on labor inputs vs. non-labor inputs would not be conclusive evidence, because this could also be driven by technological change (Baqaee and Farhi 2020).

production. So, decreases in labor power may operate to promote the reductions in demand and rising gap between private saving and investment that are defining features of secular stagnation.

Finally, it is worth highlighting that our hypothesis is perhaps more deeply threatening to existing thinking than the other prominent hypotheses for the causes of the decline in the labor share. The globalization or technological change perspectives would imply that any adverse distributional consequences have come alongside greater efficiency, which would have made Pareto-improving redistribution possible (at least in principle). The monopsony and monopoly perspectives suggest that the rise in inequality has come alongside the economy becoming less efficient, which allows economists to be in the congenial place of arguing for policies that simultaneously perfect markets, increase efficiency and promote fairness. In contrast, the declining worker power perspective would imply that the increased inequality we have seen over recent decades may not have come alongside greater efficiency. And the policy implication if these trends are to be reversed – doing more to preserve rent-sharing – interferes with pure markets and may not enhance efficiency on at least some measures.[91]

More profoundly, if declines in worker power have been major causes of increases in inequality and lack of progress in labor incomes, if policymakers wish to reverse these trends, and if these problems cannot be addressed by making markets more competitive, it raises questions about capitalist institutions. In particular, it raises issues about the effects of corporate governance arrangements which promote the interests of shareholders only, versus a broader set of stakeholders – a constantly simmering debate that has gained new prominence with the Business Roundtable's embrace of stakeholder capitalism. And it suggests that institutions which share rents with workers are likely to be necessary as a form of countervailing power (of the sort initially proposed by Galbraith (1952)).

[91] The degree to which labor market rent-sharing institutions promote or reduce aggregate efficiency depends on the underlying degree of competition in the labor market, the availability of rents in the product market, and the nature of the rent-sharing institutions, as discussed by Manning (2003) and others.

# 3

# Incentives to Comply with the Minimum Wage in the US and UK

*Note: Most of the UK portion of this analysis (section 3) draws directly from my joint work with Lindsay Judge, published as Resolution Foundation Briefing Note "Under the Wage Floor", January 2020.*

## 3.1 Introduction

The minimum wage is a fundamental protection for workers in both the US and UK labor markets, providing a wage floor below which workers' pay cannot fall. A large body of research has focused on attempting to quantify the effect of a higher minimum wage on pay, employment, income inequality, and other economic outcomes (see, for example, Dube 2019, Manning 2021, Clemens 2021). But the minimum wage is only effective to the degree that it is actually complied with. And in both the US and the UK, there is evidence of substantial underpayment of the minimum wage (Bernhardt et al 2013, Weil 2014b, Eastern Research Group 2014, Cooper and Kroeger 2017, Low Pay Commission 2019).

In this paper, I ask the question "What incentive do firms have to comply with the minimum wage in the US and the UK"? Since the federal minimum wage is set by the FLSA, which also sets federal overtime laws, in the US I also investigate firms' incentives to comply with overtime laws.

How can one quantify a firm's incentive to comply with the minimum wage (or other wage protections, like overtime laws)? A long tradition in economics applies a cost-benefit framework to decisions to comply with the law (Becker 1968). This framework suggests that a profit-maximizing company will comply with a law if the expected costs of non-compliance, if a violation is detected, exceed the expected extra profits which the company can make it if does not comply:

$$\text{probability of detection} \cdot \text{expected cost if detected} > \text{profits from non-compliance}$$

Ashenfelter and Smith (1979) applied this cost-benefit framework to firms' incentives to comply with the Fair Labor Standards Act in the US, with Grenier (1982), Chang and Ehrlich (1985), Lott and Roberts (1995), Weil (2005), Hallett (2018) and others applying this framework to the minimum wage subsequently.

I apply this framework to estimate firms' implied incentives to comply with the federal minimum wage and overtime law in the US (as per the Fair Labor Standards Act 1938), and with the national minimum wage in the UK (as per the National Minimum Wage Act 1998). While the cost to a firm of being found to have underpaid the minimum wage or overtime could include both the penalties imposed by the legal system and any reputational costs arising from investors', customers', or workers' dissatisfaction with the violation, in this essay I focus only on the explicit penalties imposed by the legal system. While reputational costs also matter for firms' compliance decision with labor and employment regulation (Ji and Weil 2015, Johnson 2020), I do not analyze reputational costs in this paper. This is because there is strong reason to believe that, for at least some companies, reputational costs are insignificant, meaning that one cannot rely only on reputational costs to ensure compliance with minimum wage and overtime laws: if so, workers at companies which do not face reputational costs may suffer from underpayment, and companies which do face reputational costs may be at a competitive disadvantage if competing in product markets against others which do not.

In the US, while minimum wage and overtime violations can in theory incur large penalties under the Fair Labor Standards Act ("FLSA"), in practice the available data suggests that most firms pay relatively little. All (detected) violators of federal minimum wage and overtime laws must repay back wages to workers who were underpaid. Violators may also be required to pay up to an equal amount in liquidated damages, and while this often occurs in court actions, the available evidence suggests this occurs only in a minority of investigations by the Department of Labor ("DOL"). And while the DOL may require willful and/or repeat violators to pay civil monetary penalties in addition to back wages and liquidated damages, analysis of the DOL's compliance and enforcement database – which contains all concluded FLSA wage and hour investigations over 2005-2020 – shows that only 11% of detected FLSA violations are considered repeat and/or willful, that nearly half of these are not required to pay any civil monetary penalty, and that typical penalties (when levied) are relatively small: for repeat and/or willful violators, the median penalty applied is around 30 cents per dollar of back wages owed. Finally, while the FLSA provides for criminal prosecution in the case of serious violations, it is rare: data from the Bureau of Justice Statistics shows that there were 10 criminal convictions for violation of the FLSA's minimum wage or overtime provisions during 2005-2016 (a period during which the DOL identified nearly 3,000 willful FLSA minimum wage and/or overtime underpayments).

What does this mean for compliance incentives? The magnitude of the penalties actually levied on violating firms suggests that typical firms would have to face extremely high probabilities of detection to have a financial incentive to comply with federal minimum wage and overtime law. The typical first-time violator detected by the DOL would have to expect an 88% or greater probability of detection to have an incentive to comply; if liquidated damages are expected to be levied, a typical violator would have to expect a probability of detection of 47%; and even for the most egregious violators, who might expect to have to pay liquidated damages and a very high civil monetary penalty, first-time violators must expect a probability of detection of one in three to have an incentive to comply with the law. Higher penalties are levied on repeat violators, but even then, the typical repeat violator detected by the DOL would have to expect a probability of detection of more than 78% to have an incentive to comply (or 44% if liquidated damages are expected to be levied). For many firms, the actual probability of detection is likely substantially lower than this. Indeed, given limited resources for investigation and inspection at the federal level, for some firms the probability of receiving a DOL inspection in any given year may be as low as 2% (Ji and Weil 2015, Galvin 2016).

And while for some firms, the threat of a worker complaint leading either to a DOL investigation or a collective action lawsuit will be enough to incentivize compliance, in many cases worker complaints are unlikely: workers may be unaware their pay represents a minimum wage or overtime violation, unable to spare the time or resources to file a complaint or bring a suit, or unwilling to complain as a result of fear of retaliation, involvement with the legal system, or job loss. This is particularly likely to be true for the most vulnerable workers.

In the UK, while penalties levied on violating firms caught by HM Revenue and Customs ("HMRC") have increased substantially in recent years, the total cost of a minimum wage violation for a typical non-compliant firm remains relatively low. A typical firm caught violating the minimum wage by HMRC pays arrears (the value of the wages owed) as a penalty worth 100% of the arrears owed (or a penalty worth 200% of arrears owed if the penalty is not paid promptly). However, around 40% of minimum wage violations identified by HMRC are typically offered the option to "self-correct", meaning that the firm pays arrears owed to workers but pays no penalty on these violations. Minimum wage violations identified through the employment tribunal are in theory eligible for substantial penalties if there are aggravating circumstances, but in practice these penalties are almost never levied – and many workers fail even to recoup the back wages they are owed. And while it is in theory possible for firms or individuals to be criminally prosecuted and subject to an unlimited fine for severe minimum wage violations, there were only 14 prosecutions between 2007 and 2018, (a period during which HMRC identified 7,486 cases where a minimum wage violation was identified and a civil penalty levied), and the average fine across these prosecutions was £ 2,695.

What does this mean for compliance incentives? Under the HMRC penalty regime, most firms would have to expect at least a 50% chance of being caught (by HMRC), in order to have an incentive to pay their workers the minimum wage. While HMRC has substantial inspection resources, meaning that for many firms the probability of detection may well be 50% or higher (particularly for large firms), a back-of-the-envelope exercise using estimates of non-compliance by firm size suggests that for the typical firm violating the minimum wage, the probability of detection in a given year is between 3% and 13%. And while HMRC is not the only enforcement channel – workers can also take a minimum wage complaint to an employment tribunal or county court – the fact that firms rarely have to pay more than the arrears owed in employment tribunals or county courts means that firms would have to expect a near-certain probability of detection for this channel to provide a substantial deterrent to minimum wage non-compliance.

Overall, the analysis in this paper therefore suggests that for many firms in both the US and the UK compliance with the minimum wage essentially rests on firms' reputational concerns or managerial goodwill. Viewed from this perspective, it is perhaps unsurprising that non-compliance with the minimum wage appears to be common in both countries. In the US, for example, Galvin (2016) estimates that 16.9% of low-wage workers across the US experienced a minimum wage violation in 2013, losing on average 23% of their earnings. In the UK, the Low Pay Commission estimates that more than 22% of individuals covered by minimum wage rates were underpaid in a given month (Low Pay Commission 2019), and that over a third of these were underpaid by more than 62p per hour. If the minimum wage is to be an effective tool for ending low pay – and if it is to do so while also ensuring a level playing field for businesses who wish to pay their workers in accordance with the law – compliance and enforcement should be a central focus for policymakers.

## 3.2 US: Incentives to comply with the Fair Labor Standards Act

The Fair Labor Standards Act ("FLSA") in the US provides the framework for federal minimum wage and overtime law. Under the FLSA, it is illegal to pay covered, non-exempt workers less than the federal minimum wage of $7.25 an hour. Similarly, FLSA overtime rules require that covered workers are paid at least one-and-a-half times their regular pay for any hours they work in excess of 40 hours per week. (In this paper, I focus only on federal level minimum wage and overtime law and its enforcement. While state-level protections and enforcement are also vital, incentives to comply at the federal level are important both to ensure that workers throughout the country are protected regardless of their state, and to ensure that companies are not incentivized to shift production locations to avoid having to comply with the law.)

There are two primary mechanisms to enforce the FLSA. Triggered by an employee complaint or a targeted enforcement action, the Department of Labor can investigate employers and mandate the payment of back pay in the case of minimum wage or overtime violations. Alternatively, employees can take an employer to court, either individually or as part of a collective action (which can cover all employees "similarly situated" as long as they provide written consent to opt in to the proceedings). The FLSA provides scope for sizeable penalties for violations of minimum wage and overtime law.

Whenever a company is found to have underpaid the minimum wage or overtime, it must pay its employee back pay (to make up the difference between what the employee was actually paid and what she should have been paid). The employer may also be required to pay the employee up to an additional equal amount in liquidated damages (and, if the case has proceeded to court, may be required to pay legal fees). After a Department of Labor ("DOL") investigation, repeat or willful violators of the FLSA may also incur civil monetary penalties of up to $2,014 for each violation. The DOL can use the hot goods provision can be used to embargo goods which have been manufactured in violation of the FLSA. And willful violators of the minimum wage or overtime pay provisions in the FLSA can be criminally prosecuted, leading to a fine of up to $10,000, and – if a repeat violator – possible imprisonment for up to six months.

### 3.2.1 Penalties for FLSA violations

What penalties are levied on firms violating the FLSA's wage and hours provisions in practice? All firms found to have underpaid minimum wages or overtime are required to pay back wages to the employees in question. The degree to which other additional costs are levied varies. I analyze each in more detail below.

#### Civil monetary penalties

All repeat and/or willful violators of the FLSA may be required to pay a civil monetary penalty. These penalties can be studied using the Department of Labor's WHISARD database, which contains data on "all concluded Wage and Hour Division actions since FY 2005". The WHISARD database contains information on 148,043 cases since FY 2005 featuring at least one violation of FLSA minimum wage or overtime provisions, where back wages were found to be owed, in the fifty US states and the District of Columbia. For each case, the data breaks out the back wages the employer agreed to pay for minimum wage and overtime violations, the number of employees due back wages, and the civil monetary penalties assessed under the FLSA. It also flags whether an employer was found to be a repeat and/or willful violator. A number of important facts can be documented from the WHISARD data set which can inform calculations of firms' incentive to comply with the FLSA.

First, the vast majority of minimum wage or overtime violations detected by the DOL were considered first-time and non-willful, meaning that no civil monetary penalty could be assessed. Of the 148,043 cases since 2005 where the DOL has detected minimum wage or overtime violations with back wages owed, 91% were first-time violations. 2% of the first-time violations were found to be willful, and 9% of the repeat violations were found to be willful. This breakdown is similar whether the cases involved minimum wage underpayment, overtime underpayment, or both, as shown in Figure 3.1. The maximum amount that first-time, non-willful violators may have paid as a result of a minimum wage violation was the value of back wages owed plus an equal amount in liquidated damages.

Second, even of repeat and/or willful violations where a civil monetary penalty may in principle be levied, 41% of violators were required to pay no civil monetary penalty. For these firms, the maximum total penalty they might have faced is back wages plus an equal amount in liquidated damages. The share of repeat and/or willful violators paying no civil monetary penalty has remained roughly stable since 2005, though recent years saw an uptick in the share of repeat violators paying no civil monetary penalty (Figure 3.2).

Third, in the cases where a civil monetary penalty is assessed, the amounts are often quite small relative to the total value of minimum wage or overtime underpayment. Table 3.1 illustrates the distribution of civil monetary penalties, per dollar of back wages (i.e., per dollar of wages the firm initially saved by violating the FLSA). The median first-time, willful violator of the FLSA was required to pay a penalty worth 14 cents for each dollar of wages they owed to their employees. The median repeat, non-willful offender was required to pay a penalty of 3 cents for each dollar of wages

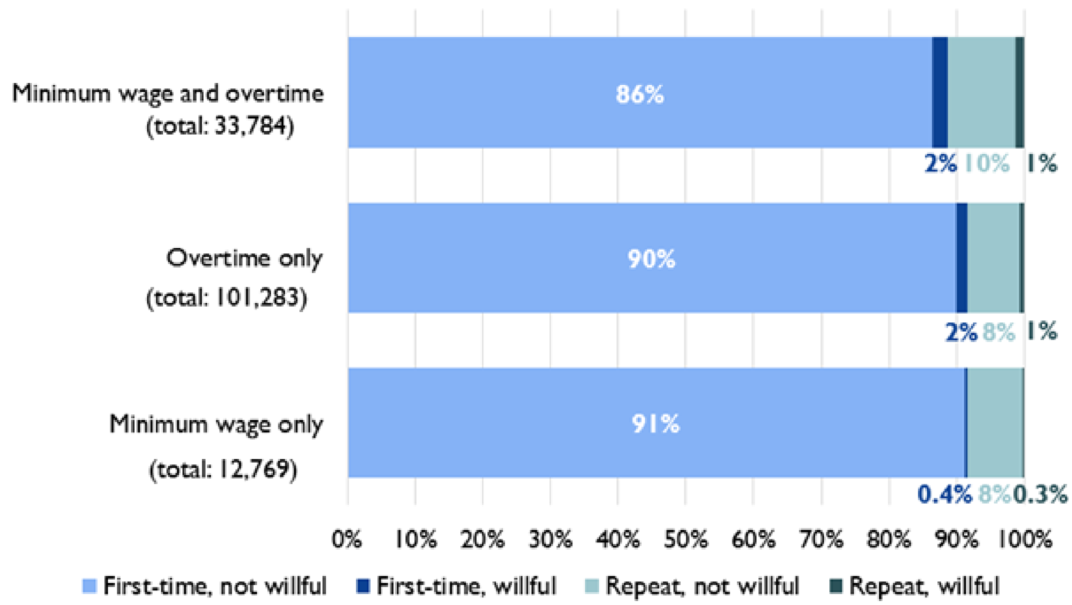



**Figure 3.1:** Most violations are not eligible for civil monetary penalties
Source: Department of Labor WHISARD database. Notes: This figure shows the share of FLSA minimum wage and overtime violations deemed repeat and/or willful, of the concluded WHD actions FY 2005 to January 2021.

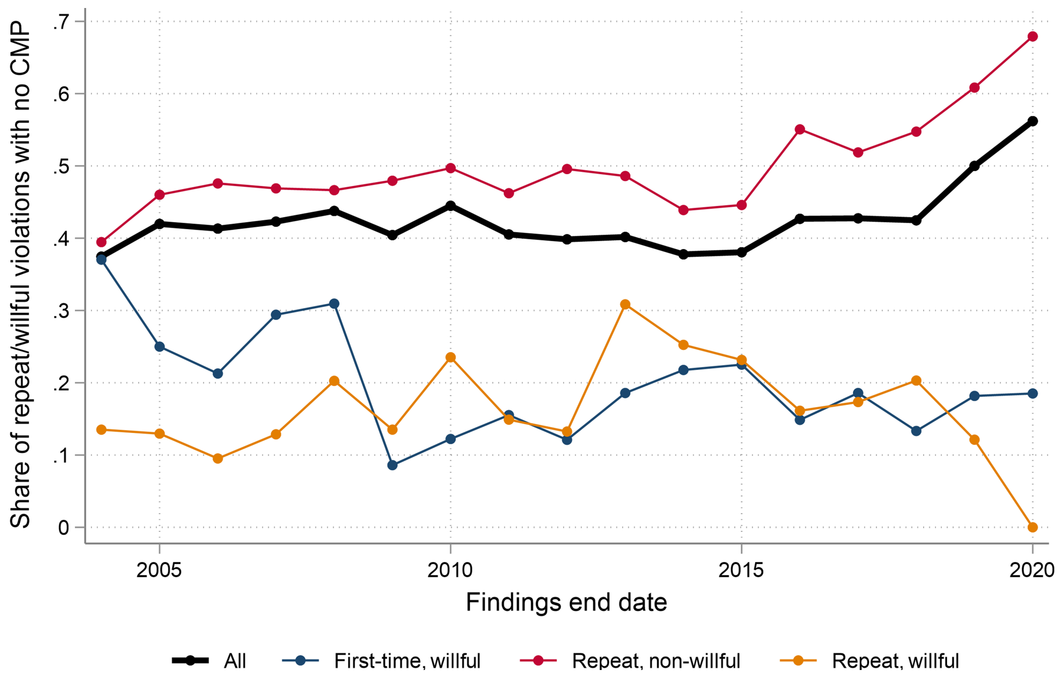



**Figure 3.2:** 40% of repeat/willful violations are not required to pay a civil monetary penalty
Source: Department of Labor WHISARD database. Notes: This figure shows the share of repeat and/or willful FLSA minimum wage and overtime violations which were not assessed any civil monetary penalty, of the concluded WHD actions FY 2005 to January 2021. "Findings end date" refers to the latest date in which the DOL found violations.

owed. For the violations which were perhaps the most egregious – repeat and willful – the median offender had to pay a penalty of 29 cents per dollar of wages owed, and 21% of repeat and willful violators had to pay a penalty worth more than $1 per dollar of wages owed. There are some very high penalties at the tails of the penalty distribution: the 99th percentile penalty for repeat, willful violators was more than ten dollars per dollar of back wages.

Overall, this analysis illustrates that of 148,043 DOL cases since 2005 where back wages were found to be owed, any civil monetary penalty was levied in only 6.3% of cases, and a penalty worth more than $1 per dollar of wages saved was levied in only 1.4% of cases. Even these figures are likely overestimates of the actual civil monetary penalties firms pay, as the civil monetary penalty figure reported in the DOL WHISARD database represents the civil monetary penalty assessed by the DOL, not the civil monetary penalty payment received. Weil (2014b) finds that firms often pay substantially lower civil monetary penalties than the value initially assessed by the WHD, reporting that over 1998-2008, the civil monetary penalties ultimately deemed receivable were only 61% of the initial amount assessed on average.

### Liquidated damages

In principle, all firms committing minimum wage violations – whether detected by the DOL or through a court action – may be required to pay liquidated damages equal to the amount owed in back wages. While assessing liquidated damages for wage and hours violations appears to be the norm in court actions against employers, in Department of Labor investigations, it appears that this happens more rarely. For violations in the 1990s and 2000s, liquidated damages were almost never assessed by the Department of Labor (Weil 2018). For example, Weil (2010) estimates that for cases concluded between 2003 to 2008, "less than one half of one percent of cases had liquidated damages computed by investigators and zero cases had liquidated damages assessed", and Bobo (2011) writes that "I had never heard of workers getting liquidated damages when they filed complaints with the

**Table 3.1:** Large penalties are only levied for a minority of repeat and/or willful violations

| Category | No. of cases | Mean CMP | % with $0 CMP | CMP per dollar of back wages owed, by percentile | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | P1 | P5 | P10 | P25 | P50 | P75 | P90 | P95 | P99 |
| First-time, willful | 2,504 | $0.36 | 18% | 0 | 0 | 0 | $0.05 | $0.14 | $0.34 | $0.75 | $1.25 | $3.66 |
| Repeat, non-willful | 12,242 | $0.49 | 48% | 0 | 0 | 0 | 0 | $0.03 | $0.43 | $1.31 | $2.30 | $6.04 |
| Repeat, willful | 1,248 | $0.91 | 19% | 0 | 0 | 0 | $0.09 | $0.29 | $0.82 | $2.10 | $3.86 | $10.45 |

Source: Department of Labor WHISARD database.

Notes: This table shows the distribution of civil monetary penalties ("CMP"s) assessed, per dollar of back wages assessed, for all FLSA minimum wage and overtime violations in the DOL WHISARD database. This database contains data on all concluded Wage and Hour Division actions starting from FY 2005 and ending in January 2021.

Department of Labor". In more recent years the DOL has increased its use of liquidated damages (Weil 2018). The extent to which this increase occurred is unclear from publicly available information (as the DOL WHISARD database does not include data on liquidated damages).

### Hot goods provision

Under section 15(a) of the FLSA, often called the "hot goods" provision, the DOL is able to embargo goods which have been manufactured in violation of the act. This provision can create substantial incentives for compliance in the industries in which it is applied: it increases the probability of detection by incentivizing companies higher in the supply chain to monitor their subcontractors to avoid production delays, and it increases the cost of detection as the costs of goods embargos can be many multiples of DOL fines (Weil 2005). Prior to the Obama administration, the hot goods provision was primarily used in the garment industry. According to Weil (2018), under his tenure as Administrator of the DOL Wage and Hour Division the DOL substantially increased their usage of this provision in the garment industry, as well as using it in agriculture. For firms in these industries, the increased use of the "hot goods" provision means that the expected cost of violations is likely to be larger – perhaps much larger – than the penalties laid out above. However, while the DOL has the ability to use the hot goods authority beyond garments and agriculture (Koltookian 2014, Weil 2014b), the extent to which this occurs is unclear.

### Criminal prosecution

Finally, while the FLSA enables willful violators to be criminally prosecuted, only 10 convictions have occurred for FLSA minimum wage or overtime violations between fiscal years 2005 and 2016 (as shown in Table 3.2 below). Over the same time period, the Department of Labor assessed roughly 113,417 FLSA minimum wage and overtime violations where back wages were owed, of which roughly 2,925 were found to be willful and therefore possible candidates for criminal prosecution

**Table 3.2:** Criminal convictions for FLSA minimum wage or overtime violations are rare

| | Number of defendants convicted under FLSA minimum wage or overtime provisions | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Fiscal year | 2005 | 2006 | 2007 | 2008 | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 |
| Minimum wage | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 3 | 0 | 0 |
| Overtime | 1 | 1 | 2 | 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 |

Source: Bureau of Justice Statistics, obtained on request.
Notes: Table shows number of defendants convicted under U.S. Code Title 29 Chapter 8 section 206 (minimum wage) and U.S. Code Title 29 Chapter 8 section 207 (overtime) in fiscal years 2005-2016 inclusive.

at least in principle. This suggests that, even conditional on the violation being detected and being deemed to be willful by the DOL, firms face less than a 0.4% chance of a criminal conviction for a willful minimum wage or overtime violation.

### 3.2.2 Calculating the probability of detection required to incentivize compliance

With this data about the range of penalties firms face under different circumstances, we can use the cost-benefit approach outlined in the introduction to ask: *what is the minimum probability of detection which is required to incentivize compliance with FLSA minimum wage and overtime laws?*

First, as Chang and Ehrlich (1985) showed, note that a firm has an incentive to comply with the FLSA if:

$$\text{Probability of violation being detected} \cdot \text{Cost of violation per dollar of unpaid wages} > 1$$

Intuitively, this is because each dollar of underpayment translates roughly into an additional dollar of profit for the firm (the right hand side of the expression), while the expected cost of each dollar of underpayment is the product of the probability of detection and the cost per dollar of unpaid wages (the left hand side).

This in turn suggests that the minimum probability of detection required to incentivize compli-

**Table 3.3:** Scenarios to calculate probability of detection required to incentivize compliance

| Scenario | Back wages? | Liquidated damages? | Likelihood of being deemed "willful"? | Civil monetary penalty? |
|---|---|---|---|---|
| 1 | Yes | No | Average likelihood | Median penalty, if willful |
| 2 | Yes | No | Certain | Median penalty |
| 3 | Yes | Yes | Certain | Median penalty |
| 4 | Yes | Yes | Certain | 95th percentile |

ance is the reciprocal of the expected penalty per dollar of unpaid wages:

$$\text{Minimum probability required to incentivize compliance} = \frac{1}{\text{expected penalty per dollar of unpaid wages}}$$

Note that this formula will overestimate the benefit of non-compliance to the extent that efficiency wage effects are important: to the extent that paying higher wages reduces (costly) turnover or increases employee productivity, paying a wage \$1 higher will lead to less than \$1 in foregone profit.

On the other hand, this formula will underestimate slightly the benefit of non-compliance because of the time value of money: typically, there is a long lag between the initial underpayment and the payment of back wages (since it takes time for the worker to notice the violation, complain to the DOL, and for the DOL to investigate). Since the firm does not have to pay interest on back wages, even in cases where the worker is repaid the back wages, the firm has essentially received an interest-free loan from the worker. To estimate the minimum probability required to incentivize compliance, we need estimates of the expected penalty per dollar of back wages unpaid. I use the DOL data on actual penalties levied to evaluate this quantity separately for first-time and repeat violators – since repeat violators are eligible to receive a higher penalty – examining four scenarios where a firm has underpaid \$1,000. Each scenario involves different combinations of liquidated damages and civil monetary penalties. The scenarios are outlined in Table 3.3.

Based on the publicly available data from DOL WHISARD, it appears that the most likely scenario for a firm caught violating the FLSA by the DOL is either scenario 1 or scenario 2 – the firm pays back wages, does not pay liquidated damages, and pays the median civil monetary penalty if its violation is deemed willful. Since liquidated damages are only levied in a minority of DOL cases, scenario 3 – which involves liquidated damages and the median civil monetary penalty – is likely less common and so less salient for firms. Scenario 4 – which involves liquidated damages and the 95th percentile civil monetary penalty – should be seen as a very extreme case, applying to the most egregious violators only. For firms taken to court for FLSA wage and hour violations in an individual or collective action, the outcome is likely to be somewhere between Scenario 1 and Scenario 3, as back wages are always awarded and liquidated damages often also awarded.

### First-time violators

Figure 3.3 illustrates expected penalties for a first-time violator of the FLSA for each of the four scenarios. It also shows the minimum probability of detection required to incentivize that firm to comply. In Scenario 1 – for the typical first-time violator, facing the average likelihood of being deemed "willful" (about 2%), and paying the median penalty if so – the expected penalty for a $1,000 wage violation is $3, meaning that the firm would have to expect detection with a 99.7% probability to have an incentive to comply with the law. In Scenario 2 – where the firm expects its violation will certainly be deemed willful, and it will have to pay the median civil monetary penalty for willful first-time violations – the expected total cost of a $1,000 wage violation is $1,140, meaning the firm would have to expect detection with an 88% probability or more to have an incentive to comply with the law. Scenario 3 adds liquidated damages to Scenario 2. In this case, the total expected cost of a $1,000 violation rises to $2,140: making the required probability of detection to incentivize compliance 47%. Finally, Scenario 4 assumes the firm will have to pay liquidated damages and a very high civil monetary penalty – the 95th percentile penalty levied on first-time, willful violators. In this case, the total cost of a $1,000 wage or hour violation rises to $3,250, implying a required probability of detection of one third to incentivize compliance.

### Repeat violators

Figure 3.3 illustrates expected penalties and the minimum probability of detection required to incentivize compliance for a repeat violator of FLSA for each of the four scenarios. In Scenario 1 – for the typical repeat violator, facing the average likelihood of being deemed "willful" and paying the median civil monetary penalty for non-willful or willful repeat violators respectively – the probability of detection required to incentivize compliance would be 95%. In Scenario 2 – if the firm is certain to be deemed willful, and pays the median willful repeat civil monetary penalty – the probability of detection required to incentivize compliance falls to 78% (because the civil monetary penalty for willful repeat violators is substantially higher than the median civil monetary penalty for non-willful repeat violators). If liquidated damages are also levied, the required probability of detection falls to 44%. Finally, for repeat violators at the upper tail of the penalty distribution – paying liquidated damages and a civil monetary penalty at the 95th percentile for willful repeat violators – the firm would have to pay a substantial penalty of $3,860 alongside back wages of $1,000 and liquidated damages of $1,000, implying that the minimum probability of detection required to incentive compliance is 17%.

Furthermore, this analysis may represent an overestimate of the costs firms face when they are found to have violated the minimum wage or overtime laws. This is because I assume that the variable "amount of back wages agreed to" in the DOL data set reflects the actual back wages the firm failed to pay. In practice, firms may pay less in back wages less than the actual value of their initial underpayment. First, there is a two year statute of limitations for non-willful violations, and a three year statute of limitations for willful violations, meaning that for multi-year violations, the back wages paid will often be less than the full amount of income workers should have received. Sec-


**Figure 3.3:** Without liquidated damages, typical first-time violators must expect detection with near-certainty to have an incentive to comply

Source: Author's calculations; Department of Labor WHISARD database.

Notes: This figure shows the estimated cost to a firm of detection by the DOL, if it has underpaid the minimum wage or overtime by $1,000, and is a first-time violator. Each calculation includes the firm paying $1,000 in back wages, and calculates the expected civil monetary penalty based on different assumptions about the firm's likelihood of being deemed willful and the civil monetary penalty if deemed willful. Likelihoods of being deemed willful, and civil monetary penalties, are estimated from the DOL WHISARD database

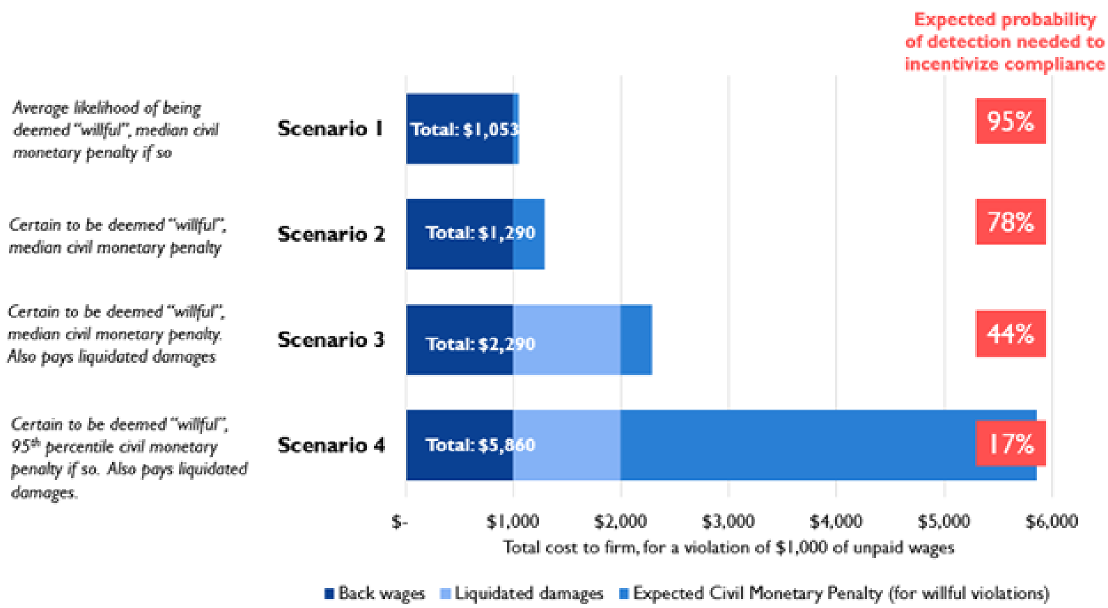



**Figure 3.4:** Typical repeat violators of FLSA minimum wage or overtime provisions must expect detection with a probability of 44%-95% to incentivize compliance

Source: Author's calculations; Department of Labor WHISARD database.

Notes: This figure shows the estimated cost to a firm of detection by the DOL, if it has underpaid the minimum wage or overtime by $1,000 and is a repeat violator. Each calculation includes the firm paying $1,000 in back wages, and calculates the expected civil monetary penalty based on different assumptions about the firm's likelihood of being deemed willful and the civil monetary penalty if deemed willful. Likelihoods of being deemed willful, and civil monetary penalties, are estimated from the DOL WHISARD database.

ond, firms may have kept bad records, which means that the DOL may not be able to prove the full amount that the firm owes workers (Bobo 2011). Third, employers often reach a settlement where they are only required to pay back some portion of the wages underpaid (Hallett 2018, Cooper and Kroeger 2017). Moreover, as discussed on page 8, there is reason to believe that firms do not always pay the full amount of civil monetary penalties assessed. Understanding whether firms' incentives to comply with the minimum wage are greater or less than our estimates here requires balancing these factors against the possibility of efficiency wage effects on productivity and turnover (as discussed earlier in this section): the presence of efficiency wage effects would cause us to overestimate the minimum probability of detection required to incentivize compliance (i.e. firms have a greater incentive to comply than we estimate), while the fact that our data likely overestimates the actual costs firms pay when found noncompliant would cause us to underestimate the minimum probability of detection required to incentivize compliance (i.e. firms have less incentive to comply than we estimate).

### 3.2.3 What is the probability of detection firms face?

How do the required probabilities of detection estimated in the previous section compare to the actual probabilities of detections firms face? It is by definition impossible to obtain good data on the probability of detection faced by firms violating the minimum wage or overtime laws, because it is impossible to estimate accurately the number of firms who are non-compliant. However, it is possible to get some idea of whether most firms' probability of detection is in the same order of magnitude as the ranges necessary for them to have an incentive to comply, for each of the three main enforcement channels: a DOL-initiated investigation, a worker complaint to the DOL leading to an investigation, or a worker bringing individual or collective action against the firm.

## DOL targeted investigations

The DOL Wage and Hour Division's statutes cover an estimated 7.3 million establishments and 135 million workers (Weil 2018). Hamaji et al (2019) estimate that for each federal wage and hour investigator in 2018, there were 175,000 workers covered by the FLSA (compared to a ratio of 1:69,000 in 1978). As this would suggest, the probability that any given establishment will be inspected by the DOL is correspondingly low. Galvin (2016) estimates that the probability that any given employer was investigated by the Wage and Hours Division of the DOL in 2012 was 0.5 percent, and that even in the most heavily-targeted industries – retail, fast food, and janitorial services – the probability of any given employer receiving an inspection in a given year did not reach 1 percent. Ji and Weil (2015) use the WHD inspection record over 2001 to 2005 to infer that, in fast food, the annual probability of any given establishment being inspected was less than 2 percent.

To maximize the efficacy of its scarce resources, the DOL has been increasingly focused in recent years on targeting its investigations towards the sectors and firms which are most likely to offend. This has likely substantially increased effectiveness, as evidenced by the increase in the share of directed investigations which find violations (Weil 2018). In addition, the number of DOL WHD investigators was increased by 40% from 2009 to 2015 (Perez 2015), suggesting that inspection probabilities may have increased by a similar amount over that period. Unless this increase in targeting and resources was able to increase the probabilities of detection by more than an order of magnitude, however, it still seems likely that for many establishments, the probability of a minimum wage violation being detected by a DOL investigation is substantially lower than that required to incentivize compliance.

### DOL complaint-initiated investigations

Another channel by which a firm can be caught violating federal minimum wage or overtime law is through worker complaints to the DOL. Given the scarcity of inspection resources and the large number of covered workers, this is a major channel on which enforcement relies in practice (Lott and Roberts 1995, Weil and Pyles 2006, Alexander and Prasad 2014, Clemens and Strain 2020). As of 2017, 50% of WHD investigations were complaint-led and 50% proactive (Weil 2018). However, there are many circumstances in which workers who are being underpaid will not complain to the DOL.

First, many workers are unaware that the pay practices of their employer violate the law (Bobo 2011, Alexander and Prasad 2014). Workers may simply not know the minimum wage or overtime law. Or, employers may violate the law in ways which are hard to detect for workers. These include requiring unpaid apprenticeships or training before hiring someone as a paid worker; making illegal deductions from paychecks, including deducting money from wages for workers' mistakes (even though this is illegal if it brings the wage below the minimum); failing to give breaks or failing to pay for breaks which were not taken; requiring workers to stay before or after shifts without pay; failing to pay workers for driving time between jobs; misclassifying workers as exempt from overtime requirements when they should be covered; paying piece rates (for the job or by the day) which do not amount to the federal hourly minimum wage or do not reach workers' statutory overtime pay; making workers pay to work (for example, for the right to earn tips); or misclassifying workers as independent contractors.

Second, workers may suspect that they have been underpaid, but lack any record of their hours to prove it (see e.g. Dombrowski et al 2017). Or the 'fissured' workplace may muddy workers' status as employees, making workers unable to determine their rights (Weil 2018). Third, even for those who are aware that they are being underpaid, many may be reluctant to complain. They may be scared of employer retaliation if they make a complaint, or of losing their job if the firm is penalized by the DOL (Weil and Pyles 2005, Bernhardt et al 2009, Alexander and Prasad 2014). They may also be reluctant to complain to the federal authorities if they have other reasons to avoid the authorities, such as if they or someone in their family are undocumented (see e.g. Fine (2006), Milkman et al (2010), Fussell (2011), Grittner and Johnson (2021)). Or, in high-turnover industries, they may simply move on and find another job, if pursuing the old employer for the back wages is too time-consuming or complicated (Bobo 2011). Finally, as Yaniv (1994) and Clemens and Strain (2020) note, at some levels of the minimum wage, a firm and worker may collude to avoid paying the minimum wage, meaning that the worker will not want to complain to the DOL.

These factors make it unsurprising that complaints are scarce and that, when they do come, they are often not from the most vulnerable workers. Weil and Pyles (2006) estimated for example that for every 130 overtime violations, only one complaint is received, and that the industries with the highest rate of FLSA complaints to the DOL are not the industries with the highest rates of violations. In addition, even if a worker does complain, that the scarcity of WHD resources means that not all worker complaints are pursued (Weil 2018).

Finally, since complaints have a positive externality – the individual worker's benefit from complaining is likely lower than the overall social benefit, since other workers are likely also affected by the same violations – even in a world where workers were fully informed about their rights under minimum wage and overtime law, and not afraid of retaliation, complaints would be under-provided relative to an efficient scenario (Weil and Pyles 2005).

### Court actions

The final FLSA enforcement channel is a worker pursuing the employer through the courts, either individually or as part of a collective action. The use of collective actions for FLSA wage and hour claims has grown rapidly: over 2013-2019 an average of 7,900 cases were filed in federal court each year under the FLSA, compared to less than 2,000 in 2000 (Seyfarth Shaw 2020). This means that collective actions have rapidly become a meaningful complement to DOL enforcement action: the presence of collective actions appears to roughly double the probability of detection firms face. This has occurred despite the fact that the opt-in nature of collective actions under the FLSA makes it substantially more difficult – relative to other class actions – for a large group of workers to be assembled to bring a case of meaningful value against an employer (Ruckelshaus 2008, Becker and Strauss 2008). However, as with individual complaints, collective action is not a realistic avenue for many workers – given lack of information or awareness about the law, lack of time and resources to pursue a complaint, or fear of retaliation or job loss. Costs may also be an issue. When an employee brings an FLSA claim to court, she is often responsible for the costs of litigation upfront, and may remain liable for attorneys' fees if unsuccessful (Ruan 2012). Free legal services organizations have only limited capacity to take on minimum wage and overtime cases (and federally funded legal services organizations are prohibited from serving undocumented workers). And while some plaintiff-side lawyers will take cases on a no-win no-fee basis, this is often difficult in minimum wage and overtime cases for individuals or small groups of low-wage workers because of low damage amounts (Becker and Strauss 2008, Lee 2014). Moreover, the increased use of mandatory arbitration clauses and collective action waivers means far fewer workers are able to exercise the right to bring a claim against an employer for underpayment (Ruan 2012, Sternlight 2015, Colvin and Gough 2015, Estlund 2018). Colvin (2018) estimates that 56% of non-union private sector employees are subject to mandatory arbitration clauses and 23% are subject to class action waivers.

### State-level enforcement

In the analysis in this paper, I focus on the incentives created by the federal enforcement system. It is important to note that many states have minimum wage and overtime requirements which exceed the federal standard, and many states have stronger enforcement apparatus and larger penalties. Several states for example require treble damages in the case of minimum wage violations, and the District of Columbia in 2013 became the first jurisdiction to approve quadruple damages for unpaid wage claims (Hallett 2018); many states have a stronger record in criminal prosecutions of minimum wage violations; and many states are more likely to levy civil monetary penalties. On the other hand, many states do not provide substantially higher penalties or greater enforcement resources over and above the federal system. These varying patterns of state-level legal coverage and enforcement activity mean that the federal penalty and enforcement system is the main protection for a large share of US workers. In this brief, I focus on the federal level not because state-level protections are unimportant, but because the federal level provides the baseline for workplace protections to avoid race-to-the-bottom dynamics playing out between states, and because for many millions of workers the protection at the federal level is the most relevant.

### 3.2.4 Evidence on non-compliance with minimum wage and overtime laws

The analysis above suggests that many firms have little incentive to comply with minimum wage and overtime laws. It is therefore unsurprising that there is evidence of widespread non-compliance: indeed, as noted by Nicole Hallett (2018): "Given this reality, the wage theft crisis is less surprising than the fact that any employer decides to comply with the law at all". A survey of front-line workers in low-wage industries in Chicago, Los Angeles and New York found that 68% of these workers experienced at least one pay-related violation of federal or state law in any given week, at an average cost of 15% of the affected workers' wages (Bernhardt et al 2009). Estimates using data from the Current Population Survey suggest variously that 2.4 million workers in the US' 10 most populous states are underpaid by an average of 25% of their weekly wages as a result of federal or state-level minimum wage violations (Cooper and Kroeger 2017); that 560,000 workers in New York and California experienced a minimum wage violation in any given week in 2011, with losses amounting to 37%-49% of worker income (Eastern Research Group 2014); and that 16.9% of low-wage workers across the US experienced a minimum wage violation in 2013, losing on average 23% of their earnings (Galvin 2016).

Some of the best evidence on the prevalence of non-compliance in specific high-risk sectors comes from random inspections. Weil (2005) finds that, in a random inspection of apparel contractors in 2000, more than half were not in compliance with the minimum wage provisions of the FLSA and a typical contractor owed about $3,700 in back wages. Weil (2014b) reports that DOL investigations of the top 20 fast food outlets over 2001-2005 found 40% of outlets in violation of the FLSA minimum wage or overtime provisions. Weil (2018) reports that in random DOL WHD inspections of the garment industry in 2015 and 2016, 85% of workplaces had violations. These estimates suggest that the total income lost to minimum wage and overtime underpayments may be very large. Cooper and Kroeger (2017) estimate that minimum wage underpayments alone, in the US' 10 most populous states, total $8 billion per year, and the Eastern Research Group's (2014) estimates imply that minimum wage underpayments in California and New York amount to around $1.7 billion per year. Recoveries by the enforcement system are a fraction of the true estimated volume of underpayment. The DOL recovered an average of around $280 million in unpaid wages each year for the last five years, and McNicholas et al (2017) estimate that the entire enforcement system – the DOL, state enforcement agencies, and class action settlements – recovered an average of $1 billion per year over 2015-16 in underpayments resulting from minimum wage and overtime violations, off-the-clock and meal break violations, illegal deductions, and employee misclassification. Finally, it should be emphasized that wage and hours violations disproportionately hit the most vulnerable workers: violation rates are significantly higher for nonwhites and noncitizens (Galvin 2016), and undocumented workers suffer violations at high rates but rarely receive relief (Bobo 2011).

## 3.3 UK: Incentives to comply with the National Minimum Wage

*NOTE: This section draws directly from my joint work with Lindsay Judge, published as Resolution Foundation Briefing Note "Under the Wage Floor", January 2020. The substantive analysis in this section should therefore be considered to be co-authored with Lindsay Judge.*

Introduced in 1999, the National Minimum Wage (and more recently, National Living Wage) sets a minimum floor for hourly pay in the UK labor market. As of April 2021, the age-based minimum hourly wages are £8.91 for anyone aged 23 and over, £8.36 for 21 to 22 year olds, £6.56 for 18 to 20 year olds, £4.62 for those under 18. In addition, there is a separate apprentice minimum wage of £4.30 for any apprentices over 19 in the first year of an apprenticeship, and all apprentices under 19 (all referred to as the "minimum wage" going forward).

There are two formal channels through which failure to pay the minimum wage can be enforced. First, minimum wage violations may be detected through an investigation by HM Revenue & Customs ("HMRC"), which may be triggered by an employee complaint or as part of a targeted enforcement action (based on predicted risk of underpayment or on intelligence). Second, minimum wage violations may be detected or as a result of a worker-initiated action: in these cases, workers must initially approach the Advisory, Conciliation, and Arbitration Service's (ACAS') early conciliation system and then, if conciliation fails or if the worker opts out of conciliation with their employer, proceeding to an employment tribunal (or more rarely a county court) (ACAS 2018). The potential penalties a violating firm may pay depend on the enforcement channel. During an HMRC investigation, one of two routes may be followed. First, in cases where an HMRC investigation is instigated in response to a worker complaint, and "where the potential arrears owed are low and the number of workers is small", the firm may be allowed to "self-correct" (BEIS 2018). This means that firm simply pays the arrears (back wages) owed to the worker, and are not liable to pay any penalties or be criminally prosecuted. Alternatively, the firm may be issued a Notice of Underpayment, in which case alongside the arrears owed, the firm is required to pay a penalty of 200% of the arrears owed up to a maximum of £20,000 per worker, which is reduced by half if paid within 14 days. Finally, HMRC can refer serious minimum wage violations – for the "small minority of employers that are persistently non-compliant and/or refuse to cooperate with a NMW Officer" – for criminal prosecution, which can lead to a potentially unlimited fine. Individual officers of the company can be criminally liable if the offence was committed with their consent or connivance, or if it was attributable to their neglect. Finally, a director may be disqualified for breach of their duties in the case of a minimum wage violation. In an ACAS conciliated settlement, the firm will at most have to repay arrears owed, but will not be subject to further financial penalties. In an employment tribunal, alongside having to repay arrears owed, the firm may have to pay compensation to the worker for any financial losses incurred as a result of the minimum wage underpayment (for example, if the worker had to incur overdraft fees as a result of being underpaid). If there are "aggravating features", the firm may also be required to pay a penalty of 50% of the wages owed up to a maximum of £20,000 (although the penalty is reduced by half if paid within 21 days). In rare cases, employment tribunal judges may make a cost order if one party has exhibited "vexatious, abusive, disruptive, or otherwise unreasonable" conduct, has brought a case with no reasonable prospect of success, or has breached an order of practice (BEIS 2019). In a county court, the firm may be required to pay compensation alongside arrears, and may have to pay the worker's legal costs, but there is no provision for the court to levy any further penalty. The different enforcement channels and possible penalties are visualized in Appendix Figure C.8.

### 3.3.1 Penalties for National Minimum Wage violations

The limited data available suggests that the penalties firms incur in practice for minimum wage violations rarely reach the upper limits allowed by the law.

## HMRC investigations

Unlike in the US, where there is publicly-available case-by-case data from the Department of Labor on the outcomes of individual investigations, HMRC only makes available aggregate data on the volume of arrears corrected, penalties levied, and number of workers who received arrears in each year. The value of total arrears collected by HMRC, has increased substantially in recent years. The value of penalties collected has also increased substantially, largely because of uplifts in 2014 and 2016 in the size of the penalty firms are required to pay. In the two most recent years for which data is available – 2017/18 and 2018/19 – HMRC identified £40 million of unpaid wages and levied penalties to the value of £31.1 million against firms (Table 3.4).

All firms found to have underpaid the minimum wage in an HMRC investigation are obliged to repay workers the wages owed, and evidence suggests this typically happens promptly. Violating firms then fall into one of three categories when determining whether any penalty is owed in addition to arrears: (1) some firms are allowed to self-correct, in which case they pay no penalty beyond the arrears owed (an option since 2015), (2) some firms pay the penalty within 14 days of receiving the Notice of Underpayment, in which case the penalty is worth 100 per cent of arrears, or (3) some firms pay the penalty more than 14 days after receiving the Notice of Underpayment, in which case the penalty is worth 200 per cent of arrears. In 2017/18 and 2018/19, of the 2,572 firms found by HMRC to have underpaid the minimum wage, 27% of firms were able to self-correct (corresponding to roughly 40% of total arrears owed), 52% were levied a penalty worth 200% of arrears but paid a penalty worth only 100% of arrears because of the prompt payment discount, and 13% paid a penalty worth 200% of arrears. (The remainder of firms paid some penalty, but not the full 200% amount, likely because the underpayments occurred before the penalty uplift in 2016). As a result, over 2017/18 and 2018/19, the average penalty imposed on all firms found by HMRC to have violated the minimum wage was £0.78 for each £1 of arrears owed, and the average penalty for

**Table 3.4:** Arrears recovered and penalties levied by HMRC have risen substantially over the last ten years

| | Total arrears identified | Arrears self-corrected (incurring zero penalty) | Arrears identified in investigation (incurring a penalty) | Penalties levied | Average penalty as % of arrears, for non-self-correcting firms | Average penalty as % of arrears, across all violating firms |
|---|---|---|---|---|---|---|
| 2018/19 | £24.4m | £10m | £14.4m | £17m | 118% | 70% |
| 2017/18 | £15.6m | £5.9m | £9.7m | £14.1m | 145% | 90% |
| 2016/17 | £10.9m | £6.0m | £4.9m | £3.9m | 80% | 36% |
| 2015/16 | £10.3m | £4.6m | £5.7m | £1.78m | 31% | 17% |
| 2014/15 | £3.3m | N/A | £3.3m | £0.93m | 28% | 28% |
| 2013/14 | £4.7m | N/A | £4.7m | £0.82m | 17% | 17% |
| 2012/13 | £4.0m | N/A | £4.0m | £0.78m | 20% | 20% |
| 2011/12 | £3.6m | N/A | £3.6m | £0.77m | 21% | 21% |
| 2010/11 | £3.8m | N/A | £3.8m | £0.52m | 14% | 14% |
| 2009/10 | £4.4m | N/A | £4.4m | £0.11m | 2.50% | 2.50% |

Source: Author's analysis of Government Evidence on Minimum Wage Non-Compliance for each year in question.

Note: Figures for total arrears identified in 2018/19 include £6.1m of arrears voluntarily paid by employers as part of the Social Care Compliance Scheme, instituted to allow employers to review and self-correct minimum wage arrears owed after legal issues regarding minimum wage eligibility in the social care sector were clarified.

firms not offered (or not exercising) the option to self-correct was equivalent to £1.29 for each £1 of arrears owed (Table 3.4).

### Employment Tribunals

There is no aggregate data on awards issued by employment tribunals in minimum wage cases, meaning it is difficult to estimate with certainty the degree to which firms bear costs in excess of the arrears owed after a judgment finding a minimum wage violation. The Ministry of Justice, however, provides an online database which provides some information on each employment tribunal action taken since February 2017. I manually analyzed this database in August 2019, extracting all cases where minimum wage arrears were listed and where the jurisdiction was one or more of National Minimum Wage, unlawful deduction of wages, or breach of contract, or where the judgment featured the words "minimum wage". This led to a sample of 141 cases.

This database shows that in the vast majority of minimum wage cases, tribunals awarded arrears only (and no compensation, cost awards, or penalties for "aggravating features"). Of the 141 cases in the online database where a firm was found to have underpaid the minimum wage and where information about the arrears or award was provided, only one featured a financial penalty for 'aggravating features', five involved a cost award, and seven featured compensation in relation to the minimum wage offence. The rarity of penalties being levied for "aggravating features" appears to occur more broadly across all employment tribunal cases: during the three and a half year period from the start of 2016 (when tribunals were first given the power to impose financial penalties for "aggravating features") and September 2019, financial penalties for any aggravated breach of employment law were levied in only 28 of more than 55,000 employment tribunal cases which found in favor of the worker (and the average penalty per firm was substantially smaller than the maximum, at £3,137, with firms eligible for a 50% discount on the penalty value if they pay the penalty promptly).

In addition, evidence suggests that despite being required in principle to pay arrears owed to workers, many employers found to have underpaid workers in employment tribunal judgments fail to do so. According to a 2013 study by the Department for Business, Innovation and Skills, only 32% of the 882 interviewees who had brought successful employment tribunal claims for unpaid wages (which includes minimum wage claims, as well as other cases of unpaid wages) received their payment in full without pursuing further enforcement, and 44% received no payment at all (BIS 2013).

A similar analysis of data on minimum wage claims in the county courts is not possible because records are not publicly available in a centralized database. However, the outcomes are likely similar to those in the employment tribunal: on the small claims track (for claims worth less than £10,000), the court may not typically order costs, fees or expenses.

### Criminal prosecution and individual liability

Grossly non-compliant firms can in principle be criminally prosecuted and subject to an unlimited fine if convicted, and individual officers of a company can also be liable if the minimum wage offence was committed with their consent or connivance, or if it was attributable to their neglect (see e.g. HMRC 2016). Between 1999 and 2018, however, there were just 14 prosecutions of firms for minimum wage violations (Table 3.5), and no prosecutions of individual officers under Section 32 of the Minimum Wage Act. During the same period, HMRC identified 7,486 cases where a minimum wage violation was identified and a civil penalty levied. That is, 0.2% of cases where HMRC identified minimum wage violations (and did not allow self-correction) resulted in successful criminal prosecutions. While the maximum fine in a criminal case is in theory unlimited (although was capped at £5,000 before March 2015), the average fine across these 14 cases was £2,694.50 and the average total costs levied on firms (including fines, paying compensation to workers, and workers' costs) in each case was £5,286.54.

Individual company directors can also be disqualified if found to have connived or consented

**Table 3.5:** Criminal prosecutions for National Minimum Wage Act violations are rare

| | Number of prosecutions for National Minimum Wage Act violations | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Fiscal year | 2007 | 2008 | 2009/ | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | 2017 |
| Number of prosecutions | 2 | 5 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 4 | 1 |
| Average fine (£) | 1,750 | 1,410 | 2,250 | - | - | 1,000 | - | - | - | 6,500 | 2,977 |
| Average cost order (£) | 500 | 190 | 100 | - | - | 1,000 | - | - | - | 965 | 633 |
| Average compensation (£) | 0 | 2,215 | 0 | - | - | 0 | - | - | - | 4,238 | 0 |

Source: Author's analysis of data in BEIS (2018), Annex C.
Notes: "Fiscal year" denotes starting year of Fiscal year (i.e. 2007 means FY 2007/2008). Table shows number of prosecutions under the National Minimum Wage Act in fiscal years 2007/8 to 2017/18. There were no prosecutions before 2007.

with underpayment, but the publicly available data suggests this had happened in only four cases as of 2019 (see Appendix Table C.1).

### 3.3.2 Calculating the probability of detection required to incentivize compliance

As in my analysis of the US in section 2, I can now use the framework outlined in the introduction to ask: what is the minimum probability of detection which is required to incentivize compliance with the UK's National Minimum Wage? I answer this question for five different scenarios for a firm found to have violated the minimum wage (illustrated in Figure 3.5).

The first scenario involves the typical worker-initiated action, whether through ACAS conciliation, the employment tribunal, or (much more rarely) the county court. Since the available evidence analyzed in section 3.1 suggests that employers almost never have to pay more than the amount of arrears owed in these cases (and often less than this), the expected cost of a £1,000 minimum wage violation is roughly £1,000 if the firm only expects to be detected via employee action. Therefore, the firm would have to expect detection with certainty to have an incentive to comply with the minimum wage purely as a result of this enforcement channel.

Scenarios 2-5 involve an HMRC investigation.

Scenario 2 assumes the firm will expect ex ante to face the average outcome of violating firms subject to an HMRC investigation in 2017/18 and 2018/19, with some probability of being offered the option of self-correction: in this case, a penalty equal to 78% of arrears owed is assumed to be levied, making the expected cost of a £1,000 minimum wage violation £1,780 in total. In this case, the expected probability of detection required to incentivize compliance is 56%.

Scenario 3 assumes that the firm will not be offered the option to self-correct, but will be able to pay the penalty levied promptly, meaning the penalty levied is equal to 100% of arrears owed, the expected cost of a £1,000 minimum wage violation is £2,000 in total, and the expected probability of detection required to incentivize compliance is 50%.

Scenario 4 assumes that the firm can expect the average outcome of all firms not offered self-correction by HMRC – that is, the firm faces the average probability that it will not be able to pay the penalty within 14 days of the Notice of Underpayment – meaning that the expected penalty is 129% of arrears owed and the expected probability of detection required to incentivize compliance is therefore 44%.

Finally, Scenario 5 assumes that the firm will have to pay the maximum penalty of 200% of arrears with certainty, because it knows for some reason that it will not be able to benefit from the prompt payment discount (i.e. it will be unable to pay the penalty within 14 days of receiving the Notice of Underpayment). In this case, the expected cost of detection after a £1,000 minimum wage underpayment is £3,000, making the expected probability of detection required to incentivize compliance 33%.

### 3.3.3 What is the probability of detection firms face?

Estimating the actual probability of detection most firms face is, as for the US analysis in section 2.3, difficult. The available evidence suggests, however, that it is likely smaller than the sizeable probabilities of 41%-53% required to incentivize compliance for firms facing typical outcomes under the

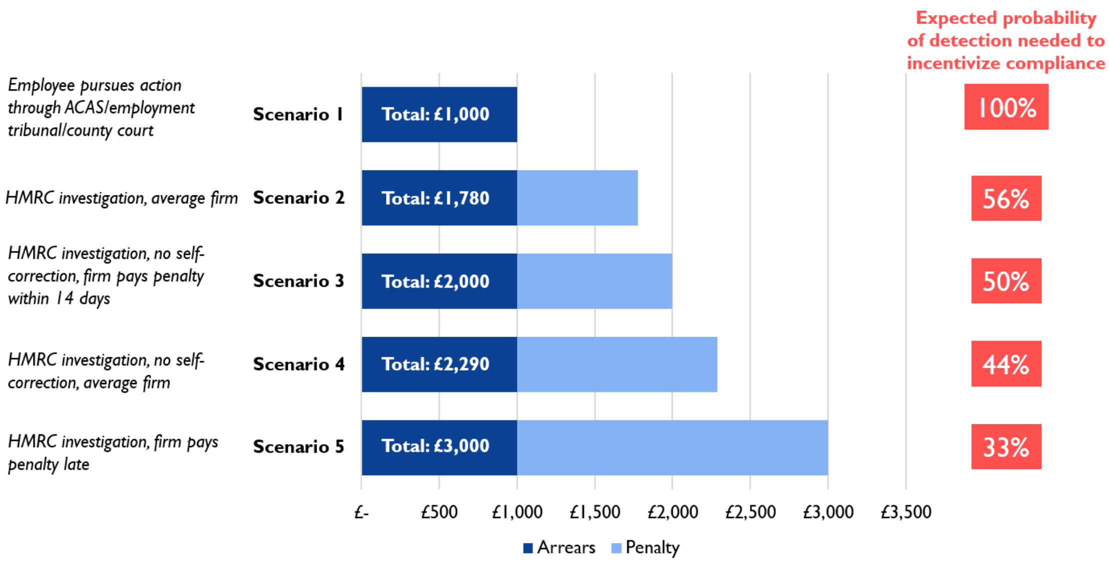



**Figure 3.5:** Typical violators must expect a probability of detection of around 1 in 2 to have an incentive to comply with the minimum wage

Source: Author's calculations; BEIS (2018); Ministry of Justice Online Employment Tribunal Database

Notes: This figure shows the estimated cost to a firm of detection of minimum wage underpayment through ACAS/employment tribunal/county courts (Scenario 1) or by HMRC (Scenarios 2-5), if it has underpaid the minimum wage by £1,000. Each calculation includes the firm paying £1,000 in arrears owed, and adds the expected penalty based on different assumptions about the firm's likelihood of being offered the option to self-correct and the likelihood of being able to pay the penalty within 14 days (and so receiving a 50% discount). For the "average firm", average penalties are estimated from data on total penalties levied and total arrears recovered by HMRC in 2017/18 and 2018/19, from BEIS (2018) and BEIS (2019).

HMRC penalty regime.

### HMRC investigation

HMRC investigates minimum wage violations through two channels: first, by following up on worker complaints, and second through targeted enforcement activity focused on specific sectors or firms that are considered more likely to be underpaying the minimum wage. Funding for HMRC's minimum wage enforcement activities has substantially risen in recent years, from £8 million per year in 2013/14 to £26.3 million by 2018/19, and the number of HMRC compliance officers nearly tripled over the same period from 170 to 412 (BIS 2014a, Metcalf 2019, BEIS 2020). In turn, the value of arrears recovered for workers increased more than fivefold over 2013/14-2018/19 (Table 3.4). As of April 2019, the minimum wage rates covered around two million workers, and this was set to rise to 2.7 million workers by 2020 as the minimum wage rates were increased to two-thirds of median earnings. This means that as of April 2019 there were around 4,850 minimum wage workers for each minimum wage compliance inspector.

What is the probability of detection firms face if they violate the minimum wage? The combination of a relatively well-resourced compliance department and targeted inspections may suggest a relatively high probability of detection for some firms. The UK's Low Pay Commission publishes estimates (calculated using the Annual Survey of Hours and Earnings) of the number of workers who were underpaid the minimum wage as of April 2018, and the size class of the firms these workers were employed at. Using these estimates, it is possible to calculate rough lower and upper bounds of the number of firms which were underpaying the minimum wage, by assuming that for each firm size category, the total number of workers underpaid were employed in the minimum or maximum number of firms possible. This exercise suggests that in April 2018, between 10,960 and 50,140 firms may have underpaid the minimum wage. HMRC identified 1,456 firms underpaying the minimum wage in 2018/19. This suggests that a typical violating firm's probability of detection was no greater than 13%, and may have been as low as 3%.

On the other hand, with the same type of calculation, the probability that an instance of a worker being underpaid is detected seems substantially higher: the Low Pay Commission estimates that in April 2018 there were 439,000 individuals paid below the minimum wage, and in 2018/19 HMRC identified minimum wage arrears for 221,581 workers (BEIS 2020), suggesting that perhaps up to half of the underpayment of workers was detected by HMRC (although the Low Pay Commission only estimates the number of workers underpaid in a given month, suggesting that more workers may be underpaid over the course of the whole year). Reconciling this estimate with the estimate of the share of underpaying firms who are detected suggests that large firms may face very high probabilities of detection while small firms may face very low chances of detection. This is plausible both because in a firm with more workers, the likelihood that a worker complains may be larger, and because large firms are more likely to be a focus of HMRC targeted enforcement activity. Note, however, that these estimates should be considered to have a relatively large error bound attached, since the Low Pay Commission's estimates on the prevalence of minimum wage underpayment come from survey data which is likely both to contain instances of apparent minimum wage underpayment which were lawful and to miss instances of unlawful minimum wage underpayment (a particular concern since the survey is administered to firms).

### Worker-led enforcement action

All worker-led enforcement actions (except worker complaints which trigger an HMRC investigation) must start with a call to ACAS before proceeding to an employment tribunal or county court. According to ACAS records, 10,310 calls were received relating to the minimum wage in 2017/18, and 4,430 of these calls specifically referred to non-payment of the minimum wage (of which 1,980 were referred to HMRC) (BEIS 2018). With the Low Pay Commission's estimates indicating that 439,000 workers were underpaid the minimum wage in April 2018, this suggests a probability of around 0.1%-0.3% that a worker who is being underpaid will commence an enforcement action with a call to ACAS. While many of these cases may be settled in conciliation or investigated by HMRC, it is worth noting that very few proceed to the employment tribunal phase: in 2018-19, 350 cases were taken to employment tribunal in relation to minimum wage underpayment.

It is perhaps unsurprising that worker-initiated enforcement actions are relatively infrequent. As in the US, it is likely that many workers are unaware that they are being underpaid. This is because there are various ways that workers can be underpaid the minimum wage, many of which are hard to detect (or alternatively easy to conceal).

First, firms may pay an effective hourly rate below the minimum wage: examples include requiring salaried workers to work longer than their contracted hours, setting piece rates such that workers are unable to work sufficiently quickly to ensure their average hourly rate is at or above the minimum wage, failing to pay workers for the time they are required to travel in the course of their job (particularly common in the social care sector), failing to pay workers for time spent at work before clocking in or after clocking out (including time spend queuing to clock in, or to go through check-in or leaving procedures), or requiring workers to do trial shifts or attend training without pay.

Second, even if the actual gross rate that workers are paid is lawful, firms could unlawfully deduct items which bring the actual pay received below the minimum wage (including unlawful deductions for accommodation, required workplace clothing or items, or missed targets or damage to goods) or could unlawfully require workers to purchase items for work (such as uniforms) which bring their hourly pay net of these purchases below the minimum wage.

Third, firms may pay a lower minimum wage than that for which the worker is eligible, for example by misclassifying workers as apprentices in order to pay the apprentice minimum wage, paying the apprentice minimum wage to adult workers in their second year of the apprenticeship (who are legally entitled to the full minimum wage), or failing to increase the pay of workers who age into a higher minimum wage band. Fourth, firms may misclassify workers as self-employed, meaning that workers may not believe they are entitled to the minimum wage. Finally, workers may simply be unaware of their right to the minimum wage, or what the minimum wage is, particularly if workers are foreign and do not speak English as a first language.

Examples of many of the above violations are detailed for the restaurant sector in Kik et al (2019a), for warehousing in Kik et al (2019b) and Goodley and Ashby (2015), for hotels in López-Andreu et al (2019), for the social care sector in Rubery et al (2011), Hussein 2011, Pennycook (2013), HMRC (2013), and Gardiner (2015), for apprentices in Ritchie et al (2017) and across the labor market in Ipsos Mori (2012), BIS (2014b), Citizens Advice (2015), and Clark and Herman (2017).

Even if workers believe they are being underpaid, they may not know how to prove this. While employers are legally required to provide payslips to all workers, Cominetti and Judge (2019) find that almost 10% of workers report that their employer does not provide a payslip (and that this is higher for immigrant and agency workers), and Clark and Herman (2017) find that 21% of workers in domestic households do not receive a payslip. Even where payslips are provided, they may not list hours worked or may not list clearly what pay has been received, making it difficult for workers to confirm the hourly rate they received (Kik et al 2019b).

Many workers who know they are being underpaid may be unwilling to bring an enforcement action. Workers may be afraid of retribution by their employer, including losing their job (López-Andreu et al 2019, Kik et al 2019a, Kik et al 2019b, Clark and Herman 2017). Workers who are in violation of the law in other capacity – for example, if they do not have legal work permission in the UK – may also be unwilling to bring an enforcement action (Ipsos Mori 2012).

Finally, challenging underpayment through the employment tribunal or court system as an individual requires knowledge, energy, and often financial resources. While workers today do not pay to make a claim to an employment tribunal, between 2013 and 2017 there was a fee of £390 to take a case. It costs around 3-6% of the total value of the claim to issue in the county courts and, while these costs are likely to be recovered if the employee wins the claim, the upfront cost and risk of non-recovery may be too high to bear for many minimum wage workers.

Beyond the direct costs, it can be difficult to navigate the legal system without a lawyer, particularly for workers who may have limited formal education or may not speak English fluently. According to Ministry of Justice tribunal statistics, 74% of claimants at employment tribunals were represented by lawyers in 2017/18 and a further 3% by trade union representatives. There is limited free or low-cost access to lawyers for workers seeking to bring minimum wage claims. Legal aid is not available to pay for legal advice or representation in employment matters (with the exception of discrimination cases) (Pyper 2017). Legal representation can be covered by workers' trade unions, but only around 10% of workers in the lowest hourly pay decile are trade union members (Tomlinson 2019). And while legal representation is sometimes also covered by an individuals' home contents insurance policy, 50% of households in the bottom half of the income distribution do not have home contents insurance (and, given the cost of home contents insurance, this seems very likely to be higher amongst those on the lowest incomes who are most likely to be affected by minimum wage underpayment). While there are several free legal advice charities, including Law Centres and LawWorks, as well as individuals and charities which offer pro bono legal representation in tribunals and courts, these are not able to provide advice and representation to all those requiring it: the Free Representation Unit for example notes that "there is enormous unmet demand for representation in social security and employment tribunals, and we typically only have the resources to represent around one third of the cases referred to us" (Free Representation Unit 2018). Pursuing cases in the employment tribunal also takes time: according to Ministry of Justice tribunal statistics, the median age of case at clearance for single employment claims was 24 weeks in 2019, and there was a 20% increase in caseload outstanding from 2018 to 2019.

The above analysis suggests that the system creates little incentive for workers to bring an employment tribunal or county court claim for minimum wage underpayment unless the amount the worker stands to gain is large (and interviews with legal professionals and underpaid workers suggest similarly – see, for example, Kik et al (2019) and Clark and Herman (2017)). Supporting this, employment tribunal cases feature substantially higher arrears per worker than minimum wage violations detected by HMRC: the mean value of arrears per worker in employment tribunal minimum wage cases over February 2017-August 2019 was £4,198, and nearly two thirds of these employment tribunal minimum wage cases involved arrears per worker of £1,000 or more, compared to only one quarter of HMRC cases (Appendix Figure C.6). Moreover, of the 141 successful minimum wage cases in the Ministry of Justice's online employment tribunal database, 120 cases listed an additional jurisdiction beyond the minimum wage, unlawful deductions, or breach of contract (Appendix Figure C.7), suggesting that it may only be worth it financially to bring an employment tribunal case where multiple different employment violations have been committed (especially in the case of violations which potentially entitle the workers to substantial compensation, like unfair dismissal).

Finally, as this analysis may suggest, the types of workers who are most likely to be underpaid the minimum wage (younger people or those working in smaller firms, for example) are also some of the least likely to challenge underpayment on their own behalf (Cominetti and Judge 2019).

### 3.3.4 Evidence on non-compliance with the National Minimum Wage

Given the sizeable disparity between the probability of detection firms would need to expect to incentivize compliance, and our best estimates of the actual probabilities of detection many firms face, it is perhaps unsurprising that, as in the US, there is substantial evidence of minimum wage underpayment. For example, using data from the Annual Survey of Hours and Earnings (ASHE), the Low Pay Commission (2019) estimates that more than 22% of individuals covered by the minimum wage rates were underpaid (defining "covered" as earning up to 5p per hour more than the applicable minimum wage rate). The Department for Business, Energy, and Industrial Strategy estimates a similar fraction (BEIS 2017b). This underpayment is often quite serious: BEIS (2017b) estimates that roughly half of underpaid workers are underpaid by more than 50p per hour, and the Low Pay

Commission estimates that over a third of underpaid workers are underpaid by more than 62p per hour. Note, moreover, that since ASHE is a survey of employers, it is highly likely that the most serious intentional underpayment is not captured in this data, and the prevalence of underpayment in the informal economy is also likely substantially underestimated (BEIS 2016, Loy Pay Commission 2019). Estimates of minimum wage underpayment from the Labour Force Survey – a survey of workers – are substantially higher and show a greater increase over time than estimates of minimum wage underpayment from the ASHE (LeRoux et al 2013).

Who is most subject to underpayment? According to the Low Pay Commission's (2019) estimates, the youngest and oldest minimum wage workers are more likely to be underpaid than other age groups, the largest number of underpaid workers are in hospitality, retail and cleaning, and maintenance, and childcare is the occupation with the highest rate of minimum wage underpayment. Bewley et al (2014) find that minimum wage non-compliance is more likely where there is use of non-standard work practices including shiftworking, compressed hours, or agency working.

## 3.4 Conclusion and implications

The evidence in this paper suggests that for many firms in the US and the UK, the current combination of penalty levels and detection probabilities are insufficient to create an incentive to comply with the minimum wage. Incentives to comply can be increased by some combination of increasing average penalties and increasing the probability of detection, as illustrated in Figure 3.6. The two are related: as Weil (2014b) argues, to create an effective deterrent, the expected penalty must increase exponentially as the probability of detection declines. This suggests that both must be an important part of the toolkit.

Increasing the probability of detection is certainly important. Increased staff and resources for proactive inspections by the Department of Labor in the US and by HMRC in the UK would sub-

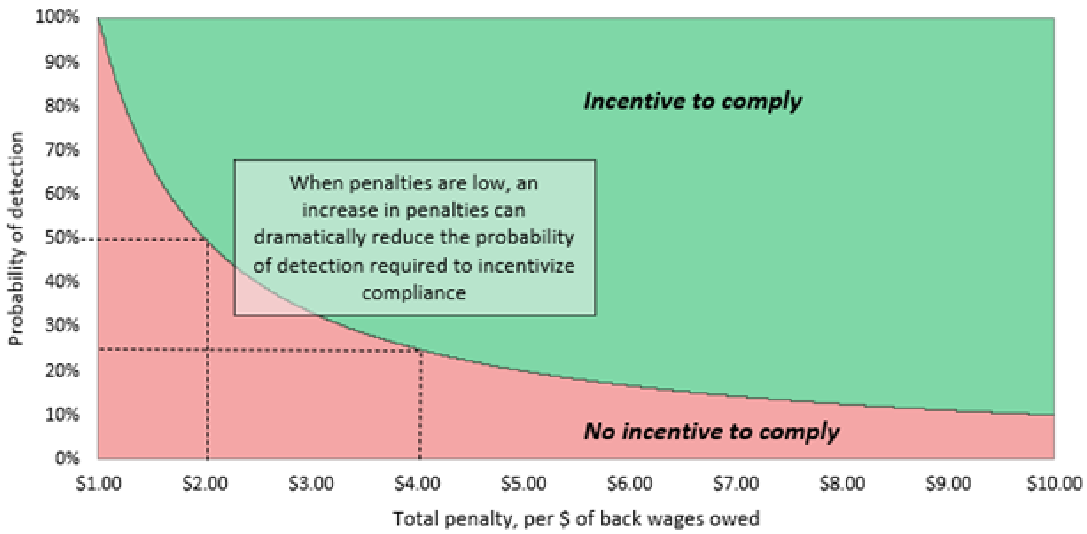



**Figure 3.6:** Meaningful compliance incentives require both high penalties and non-trivial detection probabilities
Source: Author's calculations.

stantially increase firms' incentive to comply with the law, as would a more strategic deterrence-based enforcement approach (Levine, Toffel, and Johnson 2012, Weil 2014b, Weil 2018, Metcalf 2018). Increased cooperation with worker advocacy organizations like worker centers, as well as increased resources for individuals to pursue legal action, could similarly increase the probability of detection (Fine and Gordon 2010, Weil 2014b, Fine 2017). Given the non-linear relationship between penalties and probabilities of detection required to create an incentive to comply, if probabilities of detection are too low, penalties would need to be extremely – perhaps punitively – high to create an effective deterrence regime (as illustrated in Figure 6). Such high penalties may not be desirable from the perspective of fairness.

However, given the current penalty regime in both the US and the UK, it is questionable whether the probability of detection could ever feasibly rise to the levels needed to generate compliance incentives for all (or almost all) firms. The scale of inspection resources required for all firms operating in low-wage labor markets to believe that they face a 40% or 50% chance of minimum wage violations being detected – let alone a 78% or 88% chance, as the typical penalties levied by the Department of Labor in the US would require – would likely be unfeasible, particularly since many firms are small, violations are often hard for employees to detect, and/or employees are unwilling or unable to report them. This suggests that substantially higher expected penalties – alongside higher detection probabilities – need to be a central part of an effective minimum wage enforcement regime. Indeed, evidence suggests that firms do respond to the incentives a stronger penalty regime creates: Galvin (2016) finds that in the US, states with higher state-level penalties for minimum wage violations see lower rates of non-compliance, driven particularly by states with treble damages and states with higher civil or criminal penalties.

How can the expected penalty be increased? In any penalty regime, a balance needs to be struck between creating a deterrent, but also ensuring fairness and proportionality. In the US, this might include making sure that liquidated damages are always levied, substantially increasing the civil monetary penalties for willful and repeat violations, extending the statute of limitations for willful and/or repeat violations, levying interest on back wages, extending the use of the "hot goods" provision across as many industries as possible, and substantially increasing the use of criminal prosecutions. In the UK, this might include substantially reducing the share of minimum wage violations which are eligible for self-correction (paying no penalty), removing or reducing the prompt payment discount for penalties levied by HMRC, increasing the scope for penalties in cases of egregious, intentional, or negligent minimum wage violation, automatically levying penalties in employment tribunal cases where minimum wage underpayment is found, and increasing the use of criminal prosecutions and director disqualifications for the most serious offenses.

When considering fairness and proportionality, it is illustrative to note that in both the US and the UK the penalties firms face for underpaying workers – wage theft – are far smaller than the penalties individuals face for the criminal offense of theft. For example, in the US shoplifting goods worth $2,500 or more can lead to felony charges and imprisonment in every state (Traub 2017).

There were over 66,000 FLSA minimum wage or overtime violations detected by the DOL over 2005-2016 which resulted in $2,500 or more being underpaid to employees. The total value of back wages in these cases was $1.9 billion. Over the same period there were 10 criminal convictions under FLSA minimum wage or overtime provisions. In the UK, for theft of property worth between £500 and £10,000 where the offender has 'medium culpability' (meaning that they had a significant role in the offence, that it was somewhat planned, and/or that it involved a breach of responsibility) the range of recommended sentences is between a low-level community order and 36 weeks' custody (according to Sentencing Council guidelines). Two-thirds of the firms sanctioned by HMRC for minimum wage underpayment in 2017 and 2018 owed arrears of more than £500 and almost half owed individual workers more than this amount (Appendix Figure C.6).

Ensuring firms have a strong incentive to comply with the minimum wage will only become more important in the context of proposals to raise minimum wages substantially in both the US and the UK. The higher the minimum wage, the larger the number of workers covered and the greater the financial incentive for firms to avoid compliance. In the context of the existing penalty regime, there is a substantial risk that large increases in minimum wages will fail to translate into large increases in take-home pay for many workers, unless penalties and enforcement are systematically strengthened.

Finally, while outside the scope of this paper, it should be emphasized that effective minimum wage enforcement must deal with the increasingly 'fissured' workplace, where more and more workers are employed not directly by the company they perform work for but by subcontracting firms or staffing agencies (especially in the cleaning, food service, and security sector), or by franchisees of an overarching brand management company (especially in hotels and fast food) (Weil 2014a). There is substantial evidence that these employment structures increase noncompliance with labor and employment laws, as well as making detection of this noncompliance more difficult (see e.g. Weil (2010) for the US and Bewley et al (2014) for the UK). In addition, a growing number of low-wage workers are misclassified as independent contractors. Since firms are not required to pay independent contractors minimum wage or overtime, this misclassification can often result in minimum wage violations that workers themselves may not be aware of.

While the creation of 'fissured' employment structures is partly a response to changing economic and technological conditions, in many cases the incentive for firms to create these 'fissured' employment structures appears to be an increased ability to subvert labor and employment law – or at least to reduce employment costs by turning a blind eye to non-compliance (Ruckelshaus 2008, Zatz 2008, Weil 2010, Weil 2014a, Weil 2014b). In addition, even where the 'fissuring' of the workplace does not lead to explicitly illegal behavior, it is an increasing concern that these structures – in particular, the classification of workers as independent contractors –violates the spirit and intent of laws designed to provide workers with the right to minimum wages, overtime pay, and other employment rights: in many cases, the span of control the firm exerts over workers is not matched to the span of their legal responsibility for these workers (Weil and Goldman 2020, Paul 2019, Paul 2020).

Both the DOL in the US and HMRC and the Director of Labour Market Enforcement in the UK have taken several steps to address noncompliance in the context of the fissured workplace (Weil 2018, Metcalf 2018). In the US, for example, the DOL has enacted a proactive strategic enforcement approach focused explicitly on industry structure (see e.g. Weil 2010, Weil 2014b. Weil 2018). There are a number of proposals as to how to make further progress. While a detailed examination of them is outside the scope of this paper, addressing this issue could include an expansion of the definition of a joint employer, third-party liability for labor and employment violations, a clampdown on the misclassification of employees as independent contractors, a more expansive definition of the employment relationship, explicit extensions of employment protections to workers regardless of their legal status as employees, and the use of antitrust to align firms' span of control with their responsibilities under labor and employment law (see, e.g. Zatz (2008), Rogers (2010), Taylor (2017), Trades Union Congress (2018a, 2018b), Metcalf (2018), Paul (2019), Block and Sachs

(2020), Goldman and Weil (2020)).

A

# Appendix to Chapter 1

## A.1 Conceptual framework: more detail

This section expands on the conceptual framework presented briefly in section 1.2 in the main body of the paper.

## Conceptual framework: effect of concentration on wages

Timing. Each period has two phases: the hiring phase and the production phase. During the hiring phase, workers exit or enter the labor market, employed workers bargain with their employer, and new workers are hired. During the production phase, employed workers produce at the firms they were hired at, receiving the wage determined in the hiring phase, and unemployed workers receive unemployment benefit b. At the start of the next period (in the next hiring phase), employed workers may leave their firms and/or renegotiate their wage with their current employer, and unemployed workers may search for a job. More details of each step in the process follow. Note that in this initial simple framework, we consider a clearly-defined labor market where all workers and all jobs are perfect substitutes. We relax this assumption later.

Firms. There are N firms in the labor market. Each firm j can employ up to $n_j$ workers and has a constant returns to scale production technology with labor productivity $p_j$ over a period. Firm size and productivity are both exogenously determined from the perspective of this model, and do not change over the period we are considering. In addition, no new firms can enter.[1]

Labor market exit and entry. At the start of each period, during the hiring phase, a fraction $\xi$ of workers from each firm 'die' – that is, they leave their jobs *and* the labor market, for exogenous reasons (for example, family reasons, relocation, retirement, ill health, or death). These

[1] We choose this extremely simple, static set-up to illustrate as cleanly as possible the impact of employer concentration on wages. With the possibility for firm entry, and for firm growth – with some entry and/or adjustment costs – the general intuition of a negative impact of employer concentration on outside options and therefore on wages would persist, but the exact impact would depend on the degree to which new firms can enter, the speed at which incumbent firms can grow, and the distribution of production technologies across new and incumbent firms, and small and large firms.

workers are replaced by an equal number of workers who are 'born' – i.e., they are new to the local labor market (perhaps they have moved, or newly entered or re-entered the labor force), who enter as job seekers.

Wage bargaining. Each worker who is currently employed at the start of the period Nash-bargains with her employer i over the wage.[2] The outcome is a wage $w_i$, equal to the value of the worker's outside option if bargaining breaks down and she leaves her job, $oo_i$, plus a share $\beta$ – reflecting worker bargaining power – of the match surplus:

$$w_i = \beta(p_i - oo_i) + oo_i = \beta p_i + (1-\beta)oo_i \tag{A.1}$$

If wage bargaining breaks down, the worker leaves her job and becomes a job seeker. We assume that all workers at a given firm have the same set of outside options in expectation, such that all workers at any given firm will in equilibrium receive the same wage.

Vacancies. After labor market exit and wage bargaining have happened, firms post vacancies to fill the positions which have been vacated by either worker exit or the breakdown of wage bargaining. There is no cost to post vacancies to fill existing positions, but firms cannot post vacancies for new positions (i.e. there is no firm growth).[3] A firm can only post one vacancy for each position they

[2] The Nash bargaining outcome can be derived as the outcome of a bargaining problem where the firm and worker both wish to maximize their joint surplus from the match, where the surplus generated is the difference between the product of the match $p_i$ and the worker's outside option $oo_i$. The specific bargaining problem which generates the Nash outcome in our framework is one where the wage satisifes $w_i = \text{argmax}_w (w_i - oo_i)^{\beta}(p_i - w_i)^{(1-\beta)}$, as shown in Jaeger et al. (forthcoming) or Manning (2011). (This is a particularly simple formula which arises in part from the assumption that the firm's outside option is zero).

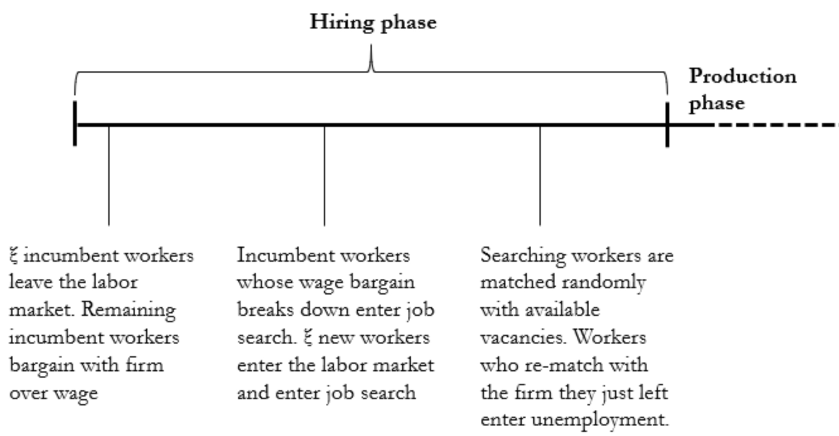
[3] This assumption is not necessary for the qualitative direction of our conclusions but keeps things simple.

wish to fill. Vacancies are posted as take-it-or-leave-it wage offers, with the posted wage equal to the wage the firm is paying to its other workers (which was decided on in the wage bargaining process described above). This constraint – that similar workers are paid similar amounts – is often observed in practice in firms. It may be motivated by fairness concerns or explicit internal pay hierarchies or bargaining agreements.[4]

Job seekers. Job seekers are comprised of workers who have newly entered the labor market, workers who have left their previous job because their match broke down at the start of the period, and workers who were unemployed in the previous period. Job search can only take place at the start of the period, during the hiring phase. There is no on-the-job-search: only workers who have left their current employer may search for a new job. Each job seeker applies to all local employers $j \in N$, but search and matching frictions mean that each job seeker will receive exactly one job offer during each hiring phase. Specifically, a ball-urn matching procedure randomly matches pairs of job seekers and firms (in a procedure similar to the framework developed by Jarosch et al. (2019)). For a given job seeker i, define the probability of receiving an offer from each firm j as $\alpha_{ij}$. The probability of receiving no offers is therefore $1 - \sum_j \alpha_{ij}$. If job seekers do not accept the job offer they receive, or if they receive no job offers, they remain unemployed for the period, receiving unemployment

[4]There is a large literature on the role of internal vs. external factors in determining the wages of new hires as compared to incumbent workers. For some examples: Bewley (1999), in interviews with firms in New England, assembles a range of evidence that conceptions of internal fairness and equity are extremely important in wage setting. Galuscak et al. (2012) use firm-level data in 15 EU countries and find strong evidence that firms do not wish to differentiate between the wages of newly hired workers and similarly qualified incumbents even if external labor market conditions change. Our assumption that new hires receive a 'take-it-or-leave-it' wage offer conforms to the survey evidence of Hall and Krueger (2012), who find in a survey of 1,300 US workers that two thirds of workers considered their offers to be 'take-it-or-leave-it' and did not bargain over the wage.

benefit b. We assume b is strictly lower than the wages offered by any feasible employer, such that a job seeker who receives an offer always accepts it. In our model this condition will always be satisfied as long as the productivity of all jobs is greater than the unemployment benefit: this is because the wage offered to each worker by firm j is equivalent to the wage bargained by the existing workers at firm j, which itself is strictly greater than unemployment benefit b. [5] The timing of the wage bargain and job search process is illustrated in the figure below.




Outside option value for employed workers. The wage at each firm is determined by the bargain with employed workers, which in turn depends partly on the value of the outside option for these employed workers. What is this outside option value? The outside option for an employed worker bargaining with her employer is to leave her current job and become a job seeker. She does

[5] The analysis above assumes that workers care only about money. If we consider instead utility (which may include a value of leisure or work), the productivity of all jobs must be greater than the money-equivalent utility value of unemployment (including the unemployment benefit and any utility or disutility of unemployment relative to work) for all workers.

not know with certainty what her outcome will be as she will be matched with at most one feasible job if she leaves her current job. Her expected wage if she leaves her job is therefore a weighted average of the wages paid by each firm j, $w_j$, weighted by the probability of being matched with each firm j, $\alpha_j$, as well as the unemployment benefit b multiplied by the probability of receiving no job offers $1 - \sum_{j \neq i}^{N} \alpha_j$:

$$oo_i = \sum_{j \neq i}^{N} \alpha_j \cdot w_j + \left(1 - \sum_{j \neq i}^{N} \alpha_j\right) \cdot b \tag{A.2}$$

We assume that each firm-worker match has weakly positive surplus, such that the bargained wage is always weakly greater than the outside option value. This means that, in equilibrium, no bargaining session will break down.[6]

Equilibrium wage. The bargained wage for workers at firm i (which is also paid to new hires at firm i) satisfies:

$$\begin{aligned} w_i &= \beta p_i + (1-\beta) oo_i \\ &= \beta p_i + (1-\beta)\left(\sum_{j} \alpha_j \cdot w_j + \left(1 - \sum^{j} \alpha_j\right) \cdot b\right) \end{aligned} \tag{A.3}$$

What do the probabilities of being matched with each feasible firm, $\alpha_j$, correspond to? Since each job seeking worker is randomly matched with one vacancy, and there are equal numbers of vacancies and job searchers, the probability of the offer a job seeker receives being from a particular firm j is proportional to the share of vacancies posted by that firm j, as a share of all vacancies in the labor

[6]Note that we assume complete information about the outside option for both the worker and the firm.

market, $\sigma_j$. This is in the same spirit as Burdett and Mortensen (1980) and Jarosch et al. (2019) who use the employment share to proxy for the likelihood of getting a job offer from a given firm.

In a labor market with infinitesimally small firms, this procedure would lead to every job seeker receiving a match from a feasible employer each period. However, in a labor market with some large employers, a worker who left firm j at the start of the period has some non-zero chance of being re-matched with firm j in the job search process (in fact, her chance of being re-matched with her former employer is $\sigma_j$). Knowing this, during the wage bargain process a large employer can threaten *not* to re-employ their worker if that worker leaves the firm but is re-matched with them in the job search process. We assume that this threat is credible and is exercised: if bargaining breaks down between a given worker and her employer j, and if the random job search process re-matches that worker to her former employer j, employer j will refuse to re-hire her (as in Jarosch et al., 2019) and she will instead have to move to unemployment for the period.[7] These assumptions imply the following wage equation for a worker at firm i:

$$w_i = \beta p_i + (1-\beta)\left(\sum_{j\neq i} \sigma_j \cdot w_j + \sigma_i \cdot b\right) \tag{A.4}$$

Note that the outcome of the wage bargain reached by workers at firm i depends on the outcome of

[7] In a single-period game, this strategy would not be time-consistent for the employer, as they are unable to fill the vacancy for the period and so lose the potential for positive surplus from re-hiring the worker. However, in a multiple period game with repeated interactions between firms and workers, and/or firm reputations, it can be in the firm's interest to avoid re-hiring a worker who has quit the firm in order to maintain the firm's reputation in future bargaining rounds with this or other workers - even if this comes at the cost of an unfilled vacancy in the current period. A less stylized setting producing a similar outcome could be a setting with on-the-job search, where workers can receive job offers from firms other than their own, and when they do so, they can use these as outside options to bargain for a higher wage. In this setting, workers at the largest firms will receive the fewest outside offers.

the wage bargain reached by workers at all other local firms j, but the wage bargained by workers at firms j depends on the wage bargained by workers at firm i. To solve this "reflection problem", we iteratively substitute for $w_j$. The wage expression becomes

$$\begin{aligned} w_i = \beta p_i + \beta(1-\beta)\sum_{j\neq i}\sigma_j p_j + \beta(1-\beta)^2\sum_{j\neq i}\sigma_j\sum_{k\neq j}\sigma_k p_k + \dots \\ + (1-\beta)\sigma_i b + (1-\beta)^2\sum_{j\neq i}\sigma_j^2 b + (1-\beta)^3\sum_{j\neq i}\sigma_j\sum_{k\neq j}\sigma_k^2 b + \dots \end{aligned} \tag{A.5}$$

expanded out to the third order, where the ellipses (...) signify further expansions to higher orders.

Since we are interested in the *average* wage in the labor market, we then take the average wage across all firms in the labor market, $\bar{w} = \sum_i \sigma_i w_i$. This gives us the expression:

$$\begin{aligned} \bar{w} = \beta\left(\sum_i \sigma_i p_i + (1-\beta)\sum_i \sigma_i\sum_{j\neq i}\sigma_j p_j + (1-\beta)^2\sum_i\sigma_i\sum_{j\neq i}\sigma_j\sum_{k\neq j}\sigma_k p_k + \dots\right) \\ + (1-\beta)b\left(\sum_i \sigma_i^2 + (1-\beta)\sum_i\sigma_i\sum_{j\neq i}\sigma_j^2 + (1-\beta)^2\sum_i\sigma_i\sum_{j\neq i}\sigma_j\sum_{k\neq j}\sigma_k^2 + \dots\right) \end{aligned} \tag{A.6}$$

To simplify this expression, define $\bar{p} = \sum_i \sigma_i p_i$ as the average productivity across firms, and denote $\tilde{p}_j = p_j - \bar{p}$ as the difference between firm j's productivity and the market average. In addition, define the rth order concentration index $\Omega_r$ as the concentration index with r "steps" as in the expression

$$\Omega_r = \underbrace{\sum_i \sigma_i \sum_{j\neq i}\sigma_j\sum_{k\neq j}\sigma_k \dots \sum_{m\neq n}\sigma_m\sum_{p\neq m}\sigma_p^2}_{\text{with r summation terms or "steps" in the expression}} \tag{A.7}$$

such that the first order concentration index $\Omega_1$ is the sum of the squared employer shares (the HHI: $\Omega_1 = \sum_i \sigma_i^2$), the second order concentration index is $\Omega_2 = \sum_i \sigma_i \sum_{j \neq i} \sigma_j^2$, and so on. Also define $\Omega_0 = 0$. We can then rewrite the average wage equation (A.6) as

$$\begin{aligned}\bar{w} = \beta\bar{p}\left(1 + \sum_{n=1}^{\infty}(1-\beta)^n\left(1 - \sum_{r=1}^{n}\Omega_r\right)\right) + (1-\beta)b\left(\sum_{n=1}^{\infty}(1-\beta)^{n-1}\Omega_n\right) \\ - \beta\left((1-\beta)\sum_i \sigma_i^2\tilde{p}_i + (1-\beta)^2\sum_i \sigma_i \sum_{j\neq i}\sigma_j^2\tilde{p}_j + (1-\beta)^3\sum_i \sigma_i \sum_{j\neq i}\sigma_j \sum_{k\neq j}\sigma_k^2\tilde{p}_k + \ldots\right)\end{aligned} \tag{A.8}$$

That is, the average wage in a given labor market is a function of three terms: the average productivity in the labor market $\bar{p}$, multiplied by a function of worker bargaining power and employer concentration; the employment benefit b, multiplied by a function of worker bargaining power and employer concentration; and a third term which reflects the interaction between employer share $\sigma_j$ and employer relative productivity $\tilde{p}_j$.

The first and second terms of our wage expression illustrate that the wage declines as average employer concentration increases: as different aspects of average employer concentration increase, $\Omega_r$ increases, meaning that less weight in the wage equation falls on average productivity ($\bar{p}$) and more weight falls on the unemployment benefit (b). This in turn is because large employers can credibly threaten not to re-hire workers who quit, reducing these workers' bargaining power by making their outside option worse (i.e. making it more likely that they will enter unemployment if they quit their firm). So, higher employer concentration suppresses wages by worsening workers' outside option in the wage bargain.

Note also that the relationship between employer shares $\sigma_j$ and firm productivity $p_j$ factors into the wage in both the first and third terms. Since average productivity $\bar{p}$ is determined by the productivity of each employer, and the share of that employer in the labor market, the first term illustrates that average productivity will be higher and therefore the wage will be higher if the high productivity firms are also the large firms. However, the third term mitigates this effect somewhat: it reflects the fact that, if it is the largest firms which are the most productive, the passthrough of average productivity to average wages via the outside option channel will be lower than if it is the smallest firms which are the most productive, because the largest firms are in fewer workers' outside option set.

To obtain the average wage expression we cite in the main body of the paper, we take a second order approximation of A.8 in the employer shares $\sigma_j$ (i.e. removing all terms with $\sigma_j^n$ where $n > 2$). This reduces the expression to become a function of the squares of the employer shares – and, therefore, the commonly-used Herfindahl Hirschmann Index or HHI:

$$\bar{w} = (1 - (1 - \beta)HHI)\,\bar{p} + (1 - \beta)HHIb - \beta(1 - \beta)\sum_i \sigma_i^2 \tilde{p}_i \tag{A.9}$$

That is, this expression suggests that higher employer concentration increases the weighting on the unemployment benefit b, and decreases the weighting on the productivity of labor p, relative to a world with no employer concentration. If all firms have the same productivity $p_i = \bar{p} \forall i$, or if the correlation of market shares and firm productivity is small, the last term is close to zero and the wage is simply a concentration- and bargaining power-weighted average of productivity p and unemployment benefit b.[8]

Note, then, that one might mean two different things when one asks "what is the effect of employer concentration on wages?". First, one might be asking "what is the effect of this labor market having become more concentrated, relative to the past?". On the one hand, rising concentration exerts downward pressure on wages by worsening worker outside options. On the other hand, rising concentration may well exert upward pressure on wages by increasing average productivity:

On the other hand, one might be asking "what is the effect of the employer concentration in this labor market, in terms of suppressing workers' wages below their productivity?". In that case, the answer to this question takes the degree of productivity in the labor market *as given* and looks only at the effect of employer concentration in reducing wages below that level of productivity by worsening outside options. In a sense, this is trying to *isolate* the effect of employer concentration on outside options from its potential effect on productivity. This *latter* question is the one we are focusing on in this paper.

[8]Note: in our framework, for simplicity the *only* way the worker can end up unemployed is if they do not get matched with a firm, and the only way they do not get matched with a firm is if their 'match' in the random matching process is their current employer, who refuses to re-hire them. This means that in an unconcentrated labor market ($HHI = 0$) in this model, there would be no unemployment and the average wage would equal the average product $\bar{w} = \bar{p}$. On the other hand in a labor market with only one firm ($HHI = 1$) the wage would be $\beta p + (1 - \beta)b$, the Nash bargaining formula when the only outside option for workers is unemployment. In our framework, therefore, the only reason there is a markdown of the wage from the marginal product is because there is employer concentration, which results in some probability of becoming unemployed if bargaining breaks down with thew worker's own firm. This simplification is for clarity of exposition only; one could extend this framework to incorporate steady state unemployment even in an unconcentrated labor market, but where higher employer concentration increases the probability of unemployment if a match breaks down.

### Incorporating outside-occupation options

The conceptual framework explained above, however, assumes that all jobs and workers are perfectly substitutable in a clearly delineated labor market. As discussed in this paper, this is rarely the case in practice. Workers can switch between jobs in different occupations and locations, but differentially so for different options. In this paper, we focus on occupations within a given metro area. One can easily extend our framework to incorporate also the option to move metro area.[9]

Ideally, we would be able to delineate which firms in outside occupations are in a worker's feasible labor market for any given worker, and estimate the probability that a worker would receive a job in that outside occupation. In practice, we cannot. To work with commonly-available data definitions and publicly-available data, we must instead work with the occupational definitions from the SOC 6-digit classification scheme. We therefore extend our framework above by defining the primary labor market for workers as their local occupation o and incorporating an outside option term reflecting the value of moving to jobs in other occupations. Refer back to our equation (A.4) for the

[9] While the outside option to move location certainly matters, the data suggests that for most workers this will be less important than the outside option to get a job in another occupation in workers' current city. Occupational mobility is substantially higher than geographic mobility: only around 3% of U.S. workers move between metropolitan areas each year (according to IRS county-to-county migration data), and over 80% of job applications by workers are sent to jobs within their metropolitan area Marinescu and Rathelot (2018). In addition, geographic mobility has declined over time (Molloy et al., 2011), and the proliferation of state-level occupational licensing has made geographic mobility more difficult for many workers (Johnson and Kleiner, 2020). There are, however, a subset of more mobile workers – like highly educated professionals – for whom the failure to consider job options outside their own city may be more problematic.

bargained wage in firm i,

$$w_i = \beta p_i + (1-\beta)\left(\sum_{j\neq i}\sigma_j \cdot w_j + \sigma_i \cdot b\right)$$

but now edit this equation to reflect that some feasible jobs are in workers' own occupation o whereas some are in other occupations p (Where employer share $\sigma_{j,o}$ now refers to firm j's share of vacancies *within* occupation o):

$$w_{i,o} = \beta p_{i,o} + (1-\beta)\Bigg(\underbrace{\text{Prob}(o \to o)\cdot \sum_{j\neq i}\sigma_{j,o}\cdot w_{j,o}}_{\text{own occupation options } oo^{own}} + \underbrace{\sum_{p\neq o}^{N^{occs}}\text{Prob}(o \to p)\sum_{l}^{N_p}\sigma_{l,p}\cdot w_{l,p}}_{\text{outside occupation options } oo^{occs}}$$
$$+ \underbrace{\text{Prob}(o \to o)\sigma_{i,o}\cdot b}_{\text{unemployment}}\Bigg) \tag{A.10}$$

This expression states that the bargained wage for workers in firm i in occupation o is a function of their productivity at firm i $p_{i,o}$, the outside option value of moving to other firms j in their own occupation o, the outside option value of moving to other firms l in other occupations p, and the outside option value of moving to unemployment and receiving benefit b. Note that the expression for the probabilities of a worker from firm i in occupation o matching with firm l in occupation p if she becomes a job seeker is now the product of firm l's vacancy share in occupation p, $\sigma_{l,p}$, as well as the probability of the worker getting her job offer from some firm in occupation p conditional on having left her job in occupation o, $\text{Prob}(o \to p)$.[10]

[10]Note also that this expression assumes implicitly that each firm only employs workers of one occupation (or, alternatively, that the worker's own firm i can only refuse to re-employ that worker if she is re-matched

To take this expression to the data, we note that if employers' vacancy shares are relatively similar to their current employment shares, $\sum_{l}^{N_p} \sigma_{l,p} \cdot w_{l,p}$ can be approximated simply the average wage in local occupation p. This eliminates the need for us to consider the reflection problem that wages in occupation o affect wages in occupation p and vice versa - instead, we can use data on the average wage in each local occupation p to control directly for the effect of wages in occupation p on occupation o (as we do in our empirical implementation). This gives us the wage expression

$$w_{i,o} = \beta p_{i,o} + (1-\beta)\left(\mathrm{Prob}(o \to o)\sum_{j \neq i} \sigma_{j,o} w_{j,o} + \sum_{p \neq o}^{N^{occs}} \mathrm{Prob}(o \to p)\bar{w}_p + \mathrm{Prob}(o \to o)\sigma_i b\right) \tag{A.11}$$

We define $(\mathrm{Prob}(o \to o)\sum_{j \neq i} \sigma_{j,o} w_{j,o}$ as our outside-occupation option index $oo_o^{occs}$. As in the simple framework, we iteratively substitute for wages in other local firms j in the same occupation o, rearrange, take the average wage in the local occupation, and write the resulting wage expression in terms of our higher order concentration indices $\Omega_{r,o}$ (where subscript o denotes that this is the concentration index for employers in local occupation o). This gives us an expression for the average

with a job in her initial occupation o, but not if she is re-matched with firm i with a job in a new occupation p.

wage in occupation o:

$$
\begin{aligned}
\bar{w}_o = {} & (\beta\bar{p}_o + (1-\beta)oo_o^{occs}) \left(1 + \sum_{n=1}^{\infty}(1-\beta)^n \text{Prob}(o \to o)^n \left(1 - \sum_{r=1}^{n} \Omega_{r,o}\right)\right) \\
& + b\left(\sum_{n=1}^{\infty}(1-\beta)^n \text{Prob}(o \to o)^n \Omega_{n,o}\right) \\
& - \beta\left((1-\beta)\text{Prob}(o \to o)\sum_i \sigma_i^2 \tilde{p}_{i,o} + (1-\beta)^2 \text{Prob}(o \to o)^2 \sum_i \sigma_i \sum_{j \neq i} \sigma_j^2 \tilde{p}_{j,o} + \dots\right)
\end{aligned}
\tag{A.12}
$$

Once again taking a second order approximation in employer shares – that is, only considering concentration index $\Omega_{1,o} = \sum_i \sigma_{i,o}^2 = \text{HHI}_o$ – we can simplify the expression for the average wage in occupation o to become

$$
\begin{aligned}
\bar{w}_o = {} & (1 - (1-\beta)\text{Prob}(o \to o)\text{HHI}_o)(\alpha\bar{p}_o + (1-\alpha)oo^{occs}) + (1-\beta)\text{Prob}(o \to o)\text{HHI} \cdot b \\
& - \beta(1-\beta)\text{Prob}(o \to o)\sum_i \sigma_i^2 \tilde{p}_{i,o}
\end{aligned}
\tag{A.13}
$$

where $\alpha = \frac{\beta}{1-\text{Prob}(o \to o)(1-\beta)}$.

Ignoring the final term, which is very small if the average productivity of individual firms is not strongly correlated with their vacancy shares, the average wage in occupation o is a weighted average of the average productivity in occupation o, $\bar{p}_o$, the value of jobs outside occupation o, $oo_o^{occs}$, and unemployment benefit b. The weights are a function of worker bargaining power $\beta$, the probability that workers are matched with another firm in their own occupation if they leave their job

$\text{Prob}(o \to o)$, and employer concentration $\text{HHI}_o$.

As before, employer concentration within workers' own occupation increases the relative likelihood of workers ending up unemployed if they quit their job, increasing the weighting on b the unemployment benefit in the wage bargain and reducing the weighting on other jobs (productivity $\bar{p}_o$ and the value of moving to outside options outside workers' own occupation $oo^{occs}$). In addition, though, there is now an interaction with $\text{Prob}(o \to o)$, the likelihood of the worker staying in her occupation if she leaves her job. The more likely she is to stay in her own occupation if she leaves her job – i.e., the less likely she is to be able to find a job in a different occupation – the more employer concentration in her own occupation matters for her wage. Finally, as before, note that there is an interaction with worker bargaining power $\beta$. The more bargaining power a worker has over the match surplus, the less the outside option matters in the wage bargain and therefore the less employer concentration matters for the wage.[11]

## A.2 Burning Glass Technologies Vacancy Posting Data

This section contains further information about the vacancy posting data set from Burning Glass Technologies ("BGT"), which we use to construct our employer concentration index (as discussed briefly in Section 1.4). (We also use a different data set from BGT – the resume data set – to construct our measures of occupational mobility. We discuss the BGT resume data set in more detail in Appendix section A.3.)

[11] Note that if there is no possibility of finding a job in another occupation (i.e. $\text{Prob}(o \to o) = 1$, and $oo_o^{occs} = 0$), this expression becomes identical to the expression for the average wage in the first part of this section, equation (A.9).

Burning Glass Technologies is an analytics software company that provides real-time data on job growth, skills in demand, and labor market trends. They frequently collaborate with academic researchers by providing data. The BGT vacancy data on online job postings has been used in several other academic papers, including Azar et al. (2020a) and Hazell and Taska (2019).

### Vacancy posting data overview

Burning Glass Technologies constructs its vacancy database by collecting online job postings from about 40,000 websites, capturing the near-universe of online US job vacancies. They only measure *new* vacancy postings, so do not re-capture a given vacancy if it is left open for a long time.[12] They use proprietary algorithms to de-duplicate vacancies (for example if the same vacancy is posted on different websites).

We use BGT's vacancy data for the years 2013–2016. Over this four year period, we have data on 74 million vacancies which had been assigned a SOC 6-digit occupation and metropolitan area by BGT. Of these, about one third or 24.8 million had no information about the employer, while 49.2 contained employer names (with a total of 1.02 million different employers).

### Defining the employer and calculating the HHI

A key aspect for our purposes is how an "employer" is defined in the data. BGT's algorithm attempts to group together name variants for employers into a standard set, counting for example "Lowe's" or "Lowes" as the same employer. However, there may be some instances where employ-

[12] To capture vacancies which firms keep online to hire workers continually for a given job, BGT consider a vacancy to be "new" if the identical vacancy is still online after 60 days (Carnevale et al., 2014).

ers which are in reality the same have not been detected by the algorithm due to large differences in spelling, punctuation, or naming conventions. We therefore carry out an additional layer of grouping by removing punctuation, spacing, and capitalization, and adjusting for common spelling differences or acronyms. We also used the Agency for Healthcare Research Quality's "Compendium of US Health Systems" database for 2016 to link hospitals to the health systems which own them where possible, treating a health system as a single employer rather than a specific hospital. This match was not always perfect: there are several cases where we have not necessarily succeeded in matching all hospitals to their owner, because of the presence of multiple hospitals in our database with the same name. We also manually scanned several thousand of the largest employers in the database to group together different employer names which were evidently part of the same ultimate employer.

This means that we for the most part treat vacancies as offered by the same employer if the *name* listed by the employer on the vacancy is sufficiently similar, or if there is a well-known or easily-identifiable relationship between a parent and subsidiary company with different names (such as "Alphabet" and "Google", or two hospitals which are part of the same health system).

We do not capture relationships where one company owns another company but the names are not similar enough to identify this easily: this means that in some cases we will understate employer concentration by attributing vacancies to different employers. On the other hand, our employer categorization means that individual establishments of an employer – or even franchises of a brand – will be treated as the same employer, which may overstate employer concentration if pay decisions are made at the level of the establishment or franchise rather than the overall firm or brand group. It is not entirely conceptually clear whether employer concentration should be measured at the level of the establishment or the firm. On the one hand, individual establishments often have independent hiring policies; but on the other hand, multi-establishment firms often have common internal pay scales meaning they effectively operate as one employer across establishments. Similarly, it is not entirely conceptually clear whether franchises of the same brand should be considered as separate employers. One the one hand, they are independent businesses; on the other hand, franchisees' human resources policies are often at least partly dictated by the franchisor (Weil, 2014a), and there have been a number of prominent cases where franchisors have required franchisees not to 'poach' each others' employees (with (Krueger and Ashenfelter, 2018) estimating that over half of major franchisors have no-poaching agreements in their franchise contract). We view the question of the appropriate *level* at which to calculate employer concentration – taking into account ownership structures across firms, as well as establishment structures within firms – as a fruitful avenue for further research.

How do we treat the one third of vacancies which do not include an employer name? When we calculate our HHI statistics for each occupation-metropolitan area-year cell we assume that each vacancy listing by an employer with no name information in the database is a *separate employer* (as do Azar et al. (2020a)). This will lead us to mechanically underestimate the HHI, as it is likely that at least some of these different vacancy postings where no name information is available come from the same employer in practice (Azar et al. (2020a) note that the vacancy postings without employer name information are often due to staffing companies not disclosing on whose behalf they are posting a given job).

### Summary statistics

Here, we provide summary statistics for the 49.2 million vacancies which contain employer names. As one might expect given the skewed distribution of employment, the large majority of these vacancies are accounted for by a small group of large employers: 841 employers each posted more than 10,000 vacancies online over 2013–2016, and these 841 employers are responsible for a total of 32.4 million vacancies. While many of the small employers in our data are only present in the data for a subset of the four years 2013–2016, a subset of large employers are present for all four years (as shown in Table A.1): as a result more than 75% of all vacancies in our database are listed by employers which are present in all four years of the sample. If employers hire a lot in any one year, they also tend to hire a lot in other years: the correlation of local occupation-specific vacancies by employer from one year to the next is 0.77.

### Vacancy postings, job vacancies, and employment

A natural question is how our data on vacancy postings relates to total job vacancies and to total employment. In theory, when calculating an HHI of employer concentration, one would either like to use data on the share of job vacancies or the share of employment accounted for by each employer. Instead, we have the share of job *postings* accounted for by each employer at the level of each SOC 6-digit occupation, metropolitan area, and year.

BGT estimates that its vacancy data covers the near-universe of online job postings. The Bureau of Labor Statistics' JOLTS database (Job Openings and Labor Turnover Survey) collects data on job *openings*, where each opening represents a specific position that the firm is actively recruiting to fill. The conceptual difference between a job posting and a job opening is that one job posting (a job advertisement) could be used to fill multiple job openings, if the firm needs to hire several people for a job with the same title, job description, and location at the same time. This may be a particular concern when measuring employer concentration, as a large employer may hire more workers per job posting than a small employer, and so we would systematically underestimate concentration in labor markets with a highly skewed distribution of employer size, relative to labor markets with more symmetric distributions of employer size. For example, when hiring for warehouse laborers, a large warehousing company like Amazon might hire several workers under a job ad for a "Warehouse Associate".[13] On the other hand, for occupations where there is a high degree of granularity of individual job titles and job requirements within an occupation, we may be more likely to observe a one-to-one mapping between job *postings* and job *openings*. One might expect, therefore, that our measures of employer concentration will be less reliable for occupations for which there are many large employers who hire a lot of workers who are not required to be much differentiated in their job tasks, job titles, and qualifications or skills. If an occupation has a particularly low ratio of job postings to job openings, one would expect it to be underrepresented in our data relative to its employment in the general workforce: As discussed later in the 'representativeness" section, our data appears to be underrepresentative particularly for certain large low-wage occupations like laborers, cashiers, and food serving and preparation workers, for whom this might be a particularly common

[13] In the extreme case, where each firm only posts one vacancy per occupation that it is hiring for, our measure of the HHI will actually be a measure of 1/N where N is the number of firms hiring for that occupation in that local area.

phenomenon. Ideally, we would be able to calculate employer concentration at the level of true job openings/vacancies, or employment, rather than vacancy postings, but we are not aware of a data set that enables us to observe firm-level local occupational employment or vacancies in the US.

### Representativeness

To what extent is the online job *posting* data representative of all job *openings*? Carnevale et al. (2014) estimated as of 2014 that between 60 to 70 percent of all job openings could be found in the BGT online vacancy posting data. They do this by comparing the number of new job postings (as measured by BGT) to the number of active job openings as measured by the JOLTS database (inflating the BGT job postings number by the new jobs to active jobs ratio in the Help Wanted Online database to take account of the fact that BGT only captures new postings while JOLTS captures all active job postings). Azar et al. (2020a), using the same methodology, estimate that the share of job openings online as captured by BGT is roughly 85% of total job openings as measured by the JOLTS database in 2016, and the jobs that are not online are usually offered by small businesses and union hiring halls.

The BGT vacancy data has been used in several other academic papers in recent years, which have carried out detailed analyses of its representativeness. We provide a brief summary of the representativeness of the BGT vacancy data here and refer the interested reader to Carnevale et al. (2014), Hershbein and Kahn (2018), and Azar et al. (2020a) for more details. Note in particular that Azar et al. (2020a) use the BGT vacancy data for the same purposes as we do: to calculate employer HHI concentration indices at the level of local SOC 6-digit occupations.

Hershbein and Kahn (2018) compare the distribution of BGT vacancies across major industry groups to the distribution of job vacancies in the Bureau of Labor Statistics' JOLTS database. While BGT is overrepresented in health care and social assistance, finance and insurance, and education, and underrepresented in accommodation and food services, public administration/government, and construction, the differences are mostly small in magnitude. Hershbein and Kahn (2018) also compare the distribution of BGT vacancies by occupation to both the stock and flow of employment in the United States, showing that BGT vacancy data has a much larger than average representation of computer and mathematical occupations, management, healthcare, and business and financial operations, and lower representation in transportation, food preparation and serving, production, and construction. This degree of representativeness does not change much over time in the BGT sample.

To analyze representativeness by occupation systematically, we calculate a measure we call 'represented-ness': the share of all vacancies in our data represented by each SOC 6-digit occupation, divided by the share of all employment in the BLS occupational employment statistics which is represented by each SOC 6-digit occupation. Note that our 'represented-ness' measure captures three dimensions: one is the degree to which the BGT vacancy *posting* data is representative of the totality of vacancy postings in the US, one is the degree to which vacancy *postings* are representative of true vacancies (job openings), and one is the degree to which individual occupations have high or low turnover (and as a result, a high or low ratio of vacancies to employment). We are interested primarily in the first two of these three, and would ideally compare the representativeness of our BGT vacancy data to a data set of the universe of online *and* offline vacancies by occupation, but this is not available.

We show a scatter plot of the share of vacancies each occupation accounts for in our data, relative to the share of employment that occupation accounts for in the BLS OES, in Appendix Figure A.1.

Of the largest occupations in the data, sales occupations are relatively equally represented in BGT data as compared to the BLS OES; registered nurses, truck drivers, and computer and software occupations are overrepresented, while laborers, cashiers, waiters, janitors, personal care aides, and food preparation and serving workers are substantially underrepresented in the BGT vacancy data. This pattern of underrepresentativeness may not be surprising. These underrepresented occupations are all occupations which tend to have a higher share of their employment accounted for by self-employment, households, or small employers, who may be more likely to advertise through local advertisement channels (posted, for example, on physical job boards, or hired through local agents) or through networks, referrals, or word-of-mouth. In addition, some of these underrepresented occupations may be more likely to have a high ratio of job openings to job postings (a high number of workers hired per job posting).

Similarly, zooming in on the next tier of occupations by size, we see overrepresentation of financial, information, management, and healthcare occupations, relatively even representation of sales, delivery, and mechanical occupations, and underrepresentation of workers in occupations with a large share of self-employment (construction, plumbing, landscaping), employment by individual households (maids and housekeeping cleaners, home health aides), or employment where firms may run single job ads for many workers, or which may advertise informally (dishwashers, cooks, food preparation workers, receptionists).

For our purposes, we have two potential representativeness concerns. One concern might be that the representativeness of our data is correlated in some way with factors which would affect both employer concentration and the wage. This concern is only relevant for the *estimated effect of concentration in our regressions* if our database systematically underrepresents low-wage occupation-city labor markets even when controlling for occupation and fixed effects: that is, that within a given occupation, the lower-wage cities are underrepresented and within a given city, the lower-wage occupations are underrepresented. For our normative conclusions in terms of estimating the *aggregate number* of workers who are affected by employer concentration, and creating a ranking of which occupations are more or less affected, underrepresentativeness of the data is more of a concern: if some occupations are underrepresented in the BGT resume data, they may appear more concentrated when in fact, it is simply the case that online vacancy postings reflect fewer of the true vacancies available in the labor market for that occupation. As such, we take care when drawing these conclusions not to isolate specific occupations which appear to be severely underrepresented in our data.

## A.3 Burning Glass Technologies Resume Data

The Burning Glass Technologies resume data set is a new proprietary data set of 16 million unique US resumes spanning years over 2002–2018. Resumes are sourced by BGT from a variety of BGT partners, including recruitment and staffing agencies, workforce agencies, and job boards. Using the raw resumes, BGT populates a database which contains observations for each individual, denoting their education, jobs, and years in which they worked in each job. BGT's proprietary occupation parser assigns SOC 6-digit occupation codes to each job title listed on each resume. With this data set, we are able to observe 16 million unique workers' job histories and education up until the point where they submit their resume, effectively making it a longitudinal data set (spanning different segments of the 2002–2018 period for different workers). In this paper, we use the resume data to construct occupational transition matrices between SOC 6-digit occupations at a highly granular level. We describe the data set and our methods further below.

### Construction of occupation transition matrices

Before calculating occupation transition matrices, we apply a number of filters to the raw BGT data:

- Reduce the number of mis-parsed job or resume observations in our data set: eliminate all jobs listed as having lasted more than 70 years, and eliminate any resumes submitted by workers whose imputed age is less than 16 or greater than 100.[14]
- Eliminate all jobs held before 2001.
- Eliminate all resumes with non-US addresses.
- Eliminate any jobs which are listed as having lasted less than 6 months, to ensure that we are only capturing actual jobs rather than short-term internships, workshops etc.

The final number of resumes that contain at least two sequential years of job data under these restrictions is 15.8 million.

From each of these resumes, we extract a separate observation for each job a worker was observed in, in each year they were observed in that job. (We define a 'job' as a unique job title-employer-

[14] See the next subsection for more details on how we impute ages to the resumes.

occupation combination, meaning that a worker can in theory switch job but remain at the same employer and/or in the same occupation.) For each job, we retain information on the SOC 6-digit occupation code. This gives us a data set of 80.2 million worker-job-occupation-year observations, where each worker might be observed in multiple jobs in the same year (either if jobs were held concurrently or the worker switched from one job to another within a given year).

To identify occupational transitions from year to year, we match all sequential pairs of worker-job-occupation-year observations. For instance, if a worker had a job as a Purchasing Manager in the period 2003-2005, and a job as a Compliance Officer in 2005-2007, we would record sequential occupation patterns of the form shown in the table below.

Illustrative example of sequential job holding data.

| Year: | 2004 | 2005 | 2006 |
|---|---|---|---|
| *Occ. in year t* | *Occ. in year t+1* | | |
| Purchasing Mgr. (11-3061) | 11-3061<br>13-1040 | | |
| Compliance Off. (13-1040) | | 13-1040 | 13-1040 |

This matching of sequential job-year coincidence pairs results in 178.5 million observations (including year-to-year pairs where workers are observed in the same occupation in both years). We use these sequential job-year coincidence pairs to construct our measures of occupational mobility as follows. For each pair of (different) occupations o to p, we count the total number of sequential job-year coincidence pairs where the worker is observed in occupation o at any point in year t and is observed in occupation p at any point in year $t + 1$. We then divide this by the total number of workers in occupation o in year t who are still observed in the sample in the following year $t + 1$.

Since our data is not fully representative on age within occupations, we compute these occupation transition shares separately for different age categories (24 and under, 25 to 34, 35 to 44, 45 to 54, and 55 and over).[15] We then aggregate them, reweighting by the average proportion of employment in each of these age categories in that occupation in the US labor force over 2012–2017 (from the BLS Occupational Employment Statistics). Our aggregate occupational mobility matrix has therefore been reweighted to correspond to the empirical within-occupation age distribution in the labor force, reducing the potential for bias arising from the skewed age distribution of our sample.

### Summary statistics

Below, we describe the characteristics of the BGT resume data and how it compares to other data sets. All statistics refer to the final set of 15.8 million filtered resumes, or 178.5 million observations of sequential job-year coincidence pairs ('observations') from these resumes, unless otherwise noted.

Job number and duration: The median number of jobs on a resume is 4, and more than 95% of resumes list 10 or fewer jobs (note that a change of job under our definition could include a change of job title or occupation under the same employer). The median length job was 2 years,

[15] Where we impute age based on the year in which the worker finished either college or high school, as described in the next section.

with the 25th percentile just under 1 year and the 75th percentile 4 years. The median span of years we observe on a resume (from date started first job to date ended last job) is 12 years. Table A.2 shows more information on the distribution of job incidences and job durations on our resumes.

Gender: BGT imputes gender to the resumes using a probabilistic algorithm based on the names of those submitting the resumes. Of our observations, 88% are on resumes where BGT was able to impute a gender probabilistically. According to this imputation, precisely 50% of our observations are imputed to be more likely to be male, and 50% are imputed to be more likely to be female. This suggests that relative to the employed labor force, women are very slightly over-represented in our data. According to the BLS, 46.9% of employed people were women in 2018.

Education: 141.3 million of our observations are on resumes containing some information about education. The breakdown of education in our data for these data points is as follows: the highest educational level is postgraduate for 25%, bachelor's degree for 48%, some college for 19%, high school for 8% and below high school for less than 1%. This substantially overrepresents bachelor's degree-holders and post-college qualifications: only 40% of the labor force in 2017 had a bachelor's degree or higher according to the BLS, compared to 73% in this sample (full comparisons to the labor force are shown in Figure A.2). It is, however, to be expected that the sample of the resumes which *provide* educational information are biased towards those with tertiary qualifications, because it is uncommon to put high school on a resume. Imputing high school only education for all resumes which are missing educational information substantially reduces the overrepresentation of those with a BA and higher: by this metric, only 58% of the BGT sample have a bachelor's degree or higher. This remains an overrepresentation, but this is to be expected: a sample drawn from online resume submissions is likely to draw a more highly-educated population than the national labor force average both because many jobs requiring little formal education also do not require online applications, and because we expect online applications to be used more heavily by younger workers, who on average have more formal education. As long as we have enough data to compute mobility patterns for each occupation, and workers of different education levels *within* occupations do not have substantially different mobility patterns, this should not be a reason for too much concern.

Age: We impute individuals' birth year from their educational information and from the date they started their first job which was longer than 6 months (to exclude internships and temporary jobs). Specifically, we calculate the imputed birth year as the year when a worker started their first job, minus the number of years the worker's maximum educational qualification requires, minus 6 years. High school is assumed to require 12 years, BA 16 years, etc. For those who do not list any educational qualification on their resume, we impute that they have high school only, i.e. 12 years of education. Since we effectively observe these individuals longitudinally - over the entire period covered in their resume - we impute their age for each year covered in their resume.

As a representativeness check, we compared the imputed age of the people corresponding to our 2002-2018 sample of sequential job observations in the BGT sample to the age distribution of the labor force in 2018, as computed by the BLS. The BGT data of job observations substantially overrepresents workers between 25 and 40 and underrepresents the other groups, particularly workers over 55. 55% of observations in the BGT sample would have been for workers 25-40 in 2017, compared to 33% of the US labor force - see Figure A.3 for the full distribution. One would expect a sample drawn from online resume submissions to overweight younger workers for three reasons: (1)

because younger workers may be more familiar with and likely to use online application systems, (2) because older workers are less likely to switch jobs than younger workers, and (3) because the method for job search for more experienced (older) workers is more likely to be through direct recruitment or networks rather than online applications. Moreover, by the nature of a longitudinal work history sample, young observations will be overweighted, as older workers will include work experiences when they are young on their resumes, whereas younger workers, of course, will never be able to include work experiences when they are old on their current resumes. Therefore, even if the distribution of resumes was not skewed in its age distribution, the sample of job observations would still skew younger.

As noted above, we directly address this issue by computing occupational mobility only after reweighting observations to adjust the relative prevalence of different ages in our sample relative to the labor force. For instance, this means that we overweight our observations for 45-49 year olds, as this age category is underrepresented in our sample relative to the labor force.

Occupation: The BGT automatic resume parser imputes the 6-digit SOC occupation for each job in the dataset, based on the job title. Of 178.5 million useable observations in the data set, 169.6 million could be coded into non-military 6-digit SOC occupations by the BGT parser. 833 of the 840 6-digit SOC occupations are present, some with few observations and some with very many. Ranking occupations by the number of observations, the 10th percentile is 1,226 observations, 25th percentile is 4,173, the median is 20,526, 75th percentile is 117,538, and the 90th percentile is 495,699. We observe 216 occupations with more than 100,000 observations, 83 occupations with more than 500,000 observations, and 19 occupations with more than 2 million observations.[16]

Figure A.4 compares the prevalence of occupations at the 2-digit SOC level in our BGT data to the share of employment in that occupation group in the labor force according to the BLS in 2017. As the figure shows, at a 2-digit SOC level, management occupations, business and finance, and computer-related occupations are substantially overweight in the BGT data relative to the labor force overall, while manual occupations, healthcare and education are substantially underrepresented.

Location: Since not all workers list the location where they work at their current job, we assign workers a location based on the address they list at the top of their resume (if any address is provided). 115.4 million of our observations come from resumes that list an address in the 50 US states or District of Columbia. The broad patterns of the demographic distribution of populations across the US is reflected in our data. By Census region, the Northeast, Midwest, South, and West regions represent 24%, 22%, 38%, and 16% of our BGT sample, while they constitute 18%, 22%, 37%, and 24% of the BLS labor force: that is, our sample is very close to representative for the Midwest and South regions, somewhat overweights the Northeast, and underweights workers from the West region. Zooming in on US states (Figure A.5), we see that New Jersey, Maryland and Delaware, for instance, are 1.5-2x as prevalent in our data as they are in the overall US labor force (probably partly

[16] The occupations with more than 2 million observations are: General and Operations Managers; Sales Managers; Managers, All Other; Human Resources Specialists; Management Analysts; Software Developers, Applications; Computer User Support Specialists; Computer Occupations, All Other; First-Line Supervisors of Retail Sales Workers; Retail Salespersons; Sales Representatives, Wholesale and Manufacturing, Except Technical and Scientific Products; First-Line Supervisors of Office and Administrative Support Workers; Customer Service Representatives; Secretaries and Administrative Assistants, Except Legal, Medical, and Executive; Office Clerks, General; Heavy and Tractor-Trailer Truck Drivers; Financial Managers; Food Service Managers; Medical and Health Services Managers.

because our identification of location is based on residence and the BLS OES data is based on workplace), while Nebraska, Montana, South Dakota, Alaska, Idaho and Wyoming are less than half as prevalent in our data as they are in the overall US labor force.

### Advantages over other datasets

As a large, nationally-representative sample with information about labor market history over the past year, the Current Population Survey is often used to study annual occupational mobility. Kambourov and Manovskii (2013) argue however that the CPS should be used with caution to study occupational mobility. First, the coding is often characterized by substantial measurement error. This is particularly a concern for measuring mobility from one year to the next, as independent coding is often used when there are changes in employers, changes in duties, or proxy responses, and this raises the likelihood of an occupational switch being incorrectly identified when in fact the occupation remained the same.

Due to its structure, the CPS is also only able to identify occupational mobility at an annual or shorter frequency. The PSID is another data source frequently used to study occupational mobility. As a truly longitudinal dataset it is able to capture truly annual mobility (or mobility over longer horizons), but its small sample size means that it is unable to provide a more granular picture of mobility between different pairs of occupations.

The BGT dataset allows us to circumvent some of these concerns. Its key advantage is its sample size: with 16 million resumes (after our parsing) covering over 80 million job-year observations, we are able to observe a very large number of job transitions and therefore also to observe a very large number of transitions between different pairs of occupations. Our sample of job-year observations is more than an order of magnitude larger than that which would be available from the CPS when pooling over the same time period we use (2002–2018). And individuals listing their own jobs means that there is less of a risk of independent coding falsely identifying an occupational switch when none occurred.[17]

### Caveats and concerns

The BGT resume data set does, however, have other features which should be noted as caveats to the analysis.

1/ Sample selection: There are three areas of concern over sample selection: first, our data is likely to over-sample people who are more mobile between jobs, as the data is collected only when people apply for jobs; second, our data is likely to over-sample the types of people who are likely to apply for jobs online rather than through other means; and third, our data is likely to over-sample the types of people who apply for the types of jobs which are listed through online applications.

2/ Individuals choose what to put on their resume: We only observe whatever individuals have chosen to put on their resume. To the extent that people try to present the best possible picture of their education and employment history, and even sometimes lie, we may not observe certain jobs or education histories, and we may be more likely to observe "good" jobs and education histories than "bad" ones. The implication of this concern for our measure of job opportunities

[17] In addition, the length of many work histories in the data allows for inferring a broader range of latent occupational similarities by seeing the same individual work across different occupations, even when the jobs are decades apart (although we do not take advantage of this feature of the data in this paper).

depends on the exact nature of this distortion. If workers generally inflate the level of occupation that they worked at, this would not necessarily distort our estimates of job transitions systematically, unless transition probabilities across occupations vary systematically with the social status / level of otherwise similar jobs. At the same time, if workers choose to highlight the consistency of their experiences by describing their jobs as more similar than they truly were, we may underestimate the ability of workers to transition across occupations. Conversely, if workers exaggerate the breadth of their experience, the occupational range of transitions would be overestimated. In any case, this issue is only likely to be significant, if these types of distortions exist for many observed workers, do not cancel out, and differ systematically between workers in different occupations.

We are only aware of a very limited number of studies directly trying to estimate the incidence of misrepresentations on resumes. For instance, Sloane (1991) surveys HR executives in banking and finds that 51 responding executives were jointly aware of a total of 17 instances of meaningfully falsified job titles, which seems small given the presumably large number of resumes that these executives would have processed during their careers. All but one of the respondents estimated the incidence of falsification of *any* part of the resume to be below 20%, with most opting for lower estimates. Note that this study was done before online search made verification of basic resume information much faster and more affordable. More recently, Nosnik et al. (2010) found that 7% of the publications listed by a sample of urology residency applicants on their resumes could not be verified.

While such low rates of misrepresentation seem unlikely to introduce systematic bias into our data, it is also important to keep in mind that we are trying to estimate the *plausibility* in a bargaining setting of other jobs constituting relevant outside options. If the skills of a job that they haven't actually held are plausibly consistent with *other* jobs on their resume in the eyes of jobseekers - and ultimately of employers - then this still constitutes evidence that these jobs are perceived as pertaining to the same labor market.

3/ Parsing error: Given the size of the dataset, BGT relies on an algorithmic parser to extract data on job titles, firms, occupations, education and time periods in different jobs and in education. Since there are not always standard procedures for listing job titles, education, dates etc. on resumes, some parsing error is likely to exist in the data. (For example, the database states that 25,000 resumes list the end date of the most recent job as 1900. We exclude these from the data, but there may be other parsing errors we are unable to detect).

4/ Possible duplicates: The resume data is collected from online job applications. If a worker over the course of her career has submitted multiple online job applications, it is possible that her resume appears twice in the raw database. BGT deduplicates the resume data based on matching name and address on the resume, but it is possible that there are people who have changed address between job applications. In these cases, we may observe the career history of the same person more than once in the data. Preliminary checks suggest that this is unlikely to be a major issue.

### Comparability with CPS occupational mobility

The average occupation "leave share" in our BGT resume data is 23%. This is roughly the probability that a worker leaves their SOC 6-digit occupation when they leave their job. This is constructed from the average share of workers leaving their occupation (11%) and the average share of workers leaving their job (46%) in any given year.

To what extent is our measure similar to measures of occupational mobility constructed from the CPS? Our measure is not strictly comparable to the concept of annual occupational mobility estimated from the CPS by Kambourov and Manovskii (2008) and Xu (2018) for two reasons. First, the occupation categorization is different: we use SOC 6-digit occupations (of which there are a total of 840 in US data) and the CPS uses Census occupation codes, which are slightly broader. Second, because of the nature of our resume data, we cannot measure annual occupational mobility (share of workers whose main job was in occupation o on date d in year t whose main job was no longer in occupation o on date d in year $t + 1$). Instead, our measure of the average share of workers leaving their occupation in any given year (11%) reflects the total number of workers who are observed in occupation o in year t who are *not* observed in occupation p at any point in year $t + 1$. This makes it a slightly more conservative measure of occupational mobility than the annual occupational mobility concept commonly constructed from the CPS.

With these caveats in mind: our measure of occupational mobility – the share of workers leaving their occupation being 11% from one year to the next – is somewhat lower than the occupational mobility estimate from Kambourov and Manovskii (2008), who find occupational mobility of 0.20 at the Census 3-digit level in the CPS for the late 1990s. Our measure is, however, in a similar range to Xu (2018) who finds occupational mobility of 0.08 in 2014.

The fact that our measure is relatively low compared to Kambourov and Manovskii (2008) is interesting, since sample selection bias might be expected to *overstate* occupational mobility in our data set if the people applying for jobs (whose resumes we observe) are more mobile than average.

Our "occupation leave share" represents not *unconditional* annual occupational mobility but rather the degree of outward occupational mobility *conditional* on leaving the worker's initial job. We find that 46% of workers in our data are observed in some new job from one year to the next. This is consistent with the average length of a job in our data being 2 years. Note that according to the definition of a job we have chosen to work with, leaving your job does not necessarily entail leaving your firm: moving occupation or job title at the same firm would entail leaving your job. The CPS reports that median employee tenure at their firm in 2018 was 4.2 years, so an average job duration of 2 years in our data is consistent with workers working on average 2 consecutive jobs at the same employer.

## A.4 OES Occupational Code Crosswalk

In our analysis of the effect of outside-occupation options on wages, we run some regressions over a longer period of 1999–2016. To construct our data set of wages and employment at the occupation-CBSA level over this period, we need to create a crosswalk for OES occupational codes from SOC 2000 to SOC 2010.

We start from the crosswalk provided by the BLS for matching occupation codes. The crosswalk is based on an exact match if a SOC 2000 code corresponds to exactly one SOC 2010 code.

When SOC 2000 codes map into multiple SOC 2010 codes, or vice versa, we create a probabilistic mapping. This mapping is based on relative employment shares between the target occupation codes as of 2009 and 2012, obtained at a national level from the BLS.

When one SOC 2000 code splits into multiple SOC 2010 codes, its employees are split based on the relative employment shares in the resulting SOC 2010 codes as of 2012.

When there are multiple SOC 2000 codes mapping into multiple SOC 2010 codes, the number of employees in 2009 and 2012 are counted for the whole cluster of ambiguous assignments. Then, unique assignments within the cluster are made based on the ratio of total 2012 to 2009 employees in the cluster. The remaining employees are apportioned based on their relative share in the remainder. For 2010 and 2011 numbers, the OES combines data collected under both the old and new classification system, and grouped them under either SOC 2010 codes or hybrid identifiers.[18] Where this combination did not result in ambiguity with regard to the meaning of the SOC 2010 code used, this difference in collection methods was ignored and the content of the OES 2010 code transferred one-to-one into the applicable SOC 2010.[19]

Where the OES 2010 code is more aggregated than the SOC 2010 code, it was split based on 2012 employment shares in the target codes.[20]

Using these occupational crosswalks, we can stack the OES occupational employment and wage data by city provided by the BLS, creating an unbalanced panel of 2.5 million occupation-by-city-by-year data points of employment and mean hourly and annual wages for the years 1999-2016. Out of these data, the 2005-2016 panel that was originally coded by the BLS using the new CBSA definition has about 1.8 million data points.

[18]Detailed breakdown of the affected codes available at: `https://www.bls.gov/oes2010_and_2011_oes_classification.xls`

[19]This was the case for the following OES 2010 codes: 11-9013, 15-1799, 51-9151

[20]This was the case for the following OES 2010 codes: 13-1078, 15-1150, 15-1179, 21-1798, 25-2041, 25-3999, 29-1111, 29-1128, 29-2037, 29-2799, 31-1012, 31-9799, 39-4831, 41-9799, 43-9799, 47-4799, 49-9799, 51-9399.

## A.5 Alternative approaches to estimating occupational similarity

In Section 1.4 of this paper, we define workers' baseline labor market as a SOC 6-digit occupation within a metropolitan area.[21]

We then use occupational transitions to identify workers' outside options. There are two other possible methods of estimating occupational similarity to infer which jobs are good options for workers' outside their occupation: skill-and task-based similarity measures, and demographic- and qualification-based similarity measures. Why do we use occupational mobility?

To answer this question, we ask: What makes jobs in a given occupation a good outside option? Good outside option jobs should be both *feasible* in the sense that the worker can relatively easily become as productive as an average worker in that job, and should be at least somewhat *desirable* to work in (relative to the worker's current job). We show that occupational mobility measures capture the underlying feasibility of a job transition, in the sense that they represent moves that people actually made. This means that they can capture many dimensions of feasibility of a transition – including task, skill, and amenity similarity, but also including other constraints that prevent moves in practice but may not be observed in task or skill data (e.g. regulation, occupational licensing barriers, etc.). Since occupational transitions also reflect moves people have (mostly) chosen to make, they also incorporate the desirability of moves between different occupations.

[21] We choose local SOC 6-digit occupations as our baseline labor market, rather than industries, since research on human capital specificity suggests that occupations are a more accurate approximation of the set of jobs open to workers (Kambourov and Manovskii, 2009; Sullivan, 2010). We choose a metropolitan area as an approximation of the jobs that are available to workers without having to move. A Commuting Zone would be a better geographic measure than a metropolitan area, but unfortunately the BLS data does not include wages by SOC 6-digit occupation at the Commuting Zone level.

Skill- and task-based occupational similarity measures define two occupations as more similar, the more similar the skills and tasks are that they require. For example, Macaluso (2019) measures occupational skill similarity using the vector difference of occupational skill content, and Gathmann and Schönberg (2010) use the angular separation of occupations' task content vectors. A skill- or task-based measure of the similarity between two occupations does indeed capture many dimensions of the feasibility of an occupational transition. However, it has a number of weaknesses relative to a transition-based measure.

First, a skill- or task-based similarity measure cannot capture non-skill-related aspects which affect the feasibility of moving from one occupation to another occupation, such as occupational licensing or certification barriers between two occupations which may have similar skill requirements. Second, a skill- or task-based similarity measure cannot capture the desirability of moving from one occupation to another: it may be that two occupations are very similar in terms of the skills and tasks that they require, but the amenities may differ (for example, long or unpredictable hours being required may make an occupation less desirable for parents of young children) – so that the kind of people that work in one occupation may not want to work in the other.

Third, skill- or task-based similarity measures are (usually) symmetric between occupation pairs, whereas transitions data can capture the asymmetry of the value of different occupations as outside options for each other: occupation p may be a relevant outside option for occupation o but not the other way around, perhaps because of generalist/specialist skill differentials, differences in job hierarchy or status, or specific requirements for experience, training or certification. Fourth, skill- or task-based similarity measures require both the ability to *measure* the underlying skill and task requirements for each occupation with some accuracy *and* substantial assumptions as to how skill and task data should be combined to create a similarity measure. Skill- and task-based similarity measures can be highly sensitive to these assumptions. In contrast, a transition-based measure has the advantage of being non-parametric. This allows us to capture the equilibrium job choice policy function without having to impose a particular model of how workers and firms choose to offer and accept jobs, or about equilibrium play (Bajari et al., 2007).

Demographic- and qualification-based occupational similarity measures define two occupations as more similar, the more similar are their workers based on their observable demographic and educational characteristics. (This is a simplified version of the approach used by Caldwell and Danieli (2018), who probabilistically identify workers' outside options using the distribution of other similar workers across jobs and locations). This type of measure can capture occupational similarity in terms of the skills required, based on workers' inherent characteristics and education/training, and in terms of preferences determined by these factors. It also has the advantage of requiring substantially fewer assumptions than a skill- and task-based measure, since it uses workers' actual labor market choices to reveal their outside options. Since it does not consider career paths, however, a demographic- and qualification-based occupational similarity measure cannot capture the role of occupation-specific experience and learning, or obstacles to occupational transitions, in determining future employment options. In that sense, a demographic- and qualification-based measure of occupational similarity can be thought of as a static approach to defining a 'revealed' labor market, whereas a transition-based measure can be thought of as a dynamic approach. In addition, as with skill- and task-based approaches, this approach in practice requires assumptions on which observables are relevant for job choices and parametric assumptions on the functional form of the choice function.

Our transitions-based measure does have a major potential drawback relative to a skill- or task-based measure: off-equilibrium outside options are not observed if bargaining is efficient. It may be the case that another occupation is very feasible but slightly less desirable, which makes it a relevant outside option for a worker but one that is rarely exercised in equilibrium. However, if the number of workers and firms is large enough to observe rare transitions, worker preferences are continuous, and idiosyncratic shocks have enough variance to induce many workers to change occupations, these off-equilibrium options will on average still be revealed by the transition data - and we believe these conditions hold for job transitions.

More specifically, there are three conditions under which the above concern about off-equilibrium options in the 'revealed labor market' approach based on observed occupational transitions is not significant. First, there is a continuous distribution of worker heterogeneity with regard to preferences over different firms, and so any given worker's closest outside options (off-equilibrium option) are revealed by the actual equilibrium paths of similar workers (similar to the way that choice probabilities map to expected value functions in discrete choice models with i.i.d. preference shocks (McFadden, 1974)). Second, there has to be a sufficient number of similar workers and firms to observe these transitions. Third, that the only *relevant* off-equilibrium outside options for workers in the wage bargaining process are those which are quite similar to their existing job or skill set in expected match quality (i.e. that cashier jobs are not relevant outside options for engineers), such that the variance of worker preferences beyond the expected match quality is large enough to manifest in different job matches for all relevant outside options. If these conditions are satisfied, the expected relevant off-equilibrium options for workers in a given occupation can be inferred by the equilibrium choices of other workers in the same occupation.

## A.6 Determinants of occupational mobility

In section 1.4 we showed that empirical occupational transitions reflect underlying similarity in occupations' task and skill requirements and in their amenities. We explain this analysis in more detail here.

### Occupation characteristics: measures

Task requirements. To measure occupational similarity in terms of tasks required, we use two different approaches from prior literature.

First, we use the vector difference between the importance scores for "Skill" task content items provided by the O*Net database of occupational characteristics, as proposed by Macaluso (2019). In our measure, as in Macaluso (2019), dissimilarity is measured as the average difference in importance scores (scaled to lie between zero and ten) across the full set of 35 tasks. For a similar notion of task distance, see (Gathmann and Schönberg, 2010).

Our measure of average task distance $\bar{D}_{op}$ between occupations o and p is defined as:

$$\bar{D}_{op} = \frac{1}{35} \sum_{k=1}^{35} |S_{k,occ\ p} - S_{k,occ\ o}|,$$

where $S_{k,occ\ p}$ is the standardized skill k measure for occupation p.

Second, we use composite task measures from recent literature relating occupational task content to important economic outcomes. We consider six task composites (denoted "ALM") first introduced in Autor et al. (2003) and updated to the most recent O*Net version in Acemoglu and Autor (2011). These composites mainly capture the distinction between cognitive vs. manual and routine vs. non-routine task contents. We also consider a categorization by Deming (2017) (denoted "DD"), which recasts the occupational task composites and also introduces a composite capturing social skill-related task intensity.[22]

Job amenities. We measure similarity in the "temporal flexibility" of different occupations using the 5 O*Net occupation characteristics that Goldin (2014) identifies as proxies for the ability to have flexibility on the job: time pressure, contact with others, establishing and maintaining interpersonal relationships, structured vs. unstructured work, and the freedom to make decisions.[23] These amenities are particularly important because, as Goldin (2014) notes, "certain occupations impose heavy penalties on employees who want fewer hours and more flexible employment" (p. 1106), which in turn may contribute to gender gaps in earnings. Note that higher scores in each of these domains imply more rigid time demands as a result of business needs and make it less likely that workers are able to step away from their job whenever they need to.

[22] We update the task composites from Deming (2017) by using the latest source for task contents on O*Net, and computing the composites at the level of SOC 2010 occupational codes.

[23] The five characteristics correspond the following O*Net survey items: IV.C.3.d.1 - How often does this job require the worker to meet strict deadlines?; IV.C.1.a.4 - How much does this job require the worker to be in contact with others (face-to-face, by telephone, or otherwise) in order to perform it?; IV.A.4.a.4 - Developing constructive and cooperative working relationships with others; IV.C.3.b.8 - To what extent is this job structured for the worker, rather than allowing the worker to determine tasks, priorities, and goals?; IV.C.3.a.4 - Indicate the amount of freedom the worker has to make decisions without supervision.

Leadership responsibility. Another reason for observing occupational transitions may be career advancement (which is often reflected in a change of occupation). To study whether this appears in our data, we identify occupational characteristics measuring leadership responsibilities from the O*Net database, and create a new "leadership" composite measure defined at the level of each SOC 6-digit occupation. The measure incorporates the six characteristics most associated with leadership positions in the O*Net data, alongside the O*Net work style category for leadership. Since this is a new composite measure of an important occupational characteristic, we outline it in more detail here.

We used the following algorithm to determine which characteristics measure leadership responsibilities: On the O*Net website, we looked at the work activity characteristics that describe "Interacting with Others". For each of them, we considered the list of top 20 occupations with the highest level of that characteristic and counted how many of them are managerial positions, as evidenced by the words "supervisor", "manager", "director", or equivalents, in the occupation title. We selected all the characteristics for which the share of managerial positions among the top 20 occupations was greater than half, as these characteristics seem to be associated with "leadership" in some sense; we also added the O*Net work style category for leadership. The final list of characteristics contains the following O*Net items: I.C.2.b. - Leadership work style: job requires a willingness to lead, take charge, and offer opinions and direction; IV.A.4.a.2. - Communicating with Supervisors, Peers, or Subordinates; IV.A.4.b.1. - Coordinating the Work and Activities of Others; IV.A.4.b.2. - Developing and Building Teams; IV.A.4.b.4. - Guiding, Directing, and Motivating Subordinates; IV.A.4.c.3. - Monitoring and Controlling Resources; IV.A.4.c.2. - Staffing Organizational Units

(We were reassured to note that for 6 of these 7 characteristics, "Chief Executives" are among the Top 20 occupations in terms of importance of this measure.). We use the mean score across these 7 characteristics as our "leadership" composite. All variables are converted into standardized Z-scores before including them in regressions, so coefficients represent the effect of a one standard deviation difference in the characteristic on the outcome variable.

### Occupational similarity and mobility

To evaluate whether workers are more likely to move to occupations that have similar characteristics to their current occupation, we estimate the following regression:

$$\pi_{o\to p} = \alpha_o + \beta^{abs}|X_{occ\ p} - X_{occ\ o}| + \gamma|\Delta w_{o\to p}| + \varepsilon_{op}. \tag{A.14}$$

where $\pi_{o\to p}$ is the share of job changers in the origin occupation o that move into target occupation p, $|X_{occ\ p} - X_{occ\ o}|$ is the absolute difference between the target and the origin occupation in each of the occupational characteristics $X_o$ defined above, and $\alpha_o$ are origin occupation fixed effects to control for differences in outward mobility across occupations. We control for absolute wage differences between the occupations in all regressions except for those estimating the effect of wages or amenity differences on occupational mobility,[24] but note that the results are qualitatively similar without the wage controls.

We would expect the coefficient on the absolute difference in characteristics to be negative: the

[24] Amenities are most likely to be priced into wages (Goldin, 2014) and controlling for the latter would therefore be inappropriate.

greater the difference between two occupations, the less likely we should be to observe the worker moving from one into the other. Our results bear this out: in every regression of pairwise occupational mobility on the absolute difference in characteristics, the coefficients are significantly negative or statistically insignificant, as shown in Figure 1.3.[25]

The previous results impose symmetry on the likelihood of occupational transitions – but between many pairs of occupations, the probability of moving in one direction is likely to be different than the probability of moving in the other direction. To study whether differences in characteristics also predict the direction of occupational flows, we estimate a similar regression equation to that shown in equation (A.14), but now using the *relative* (target minus origin) difference in occupational characteristics as the independent variable:

$$\pi_{o\to p} = \alpha_o + \beta^{\text{rel}}(X_{\text{occ }p} - X_{\text{occ }o}) + \gamma\Delta w_{o\to p} + \varepsilon_{op}. \tag{A.15}$$

Again, we include origin occupation fixed effects and now control for relative wage differences between the occupations in all regressions except for the amenity differences and the wage regression. The $\beta^{\text{rel}}$ coefficients obtained from estimating equation (A.15) for the different measures are shown in Figure A.11. (Note that this analysis involves directed relationships between occupations, so if the same share of moves in each direction is observed for an given occupation pair, the estimated

[25] Our findings build on Macaluso (2019), who showed that greater skill distance between SOC 2-digit occupations is associated with lower occupational flows between these occupations: we demonstrate this relationship at the SOC 6-digit level with a larger variety of task and skill measures, and show that differences between occupations in temporal flexibility and leadership responsibilities also appear to determine workers' likelihood of moving between them.

effect of differences between them would be zero.)

A number of our predictions are borne out in the data: we find (1) that workers are more likely to move towards jobs with higher wages; (2) that workers transition on average *towards* jobs that require more leadership responsibility - as would be expected from moves up the career ladder; (3) that occupational transitions have on average been *towards* occupations that have higher analytical content and require more social skills, and out of occupations with more routine task requirements;[26] and (4) that workers have on average been moving into occupations that require more contact and working relationships with others (and so have less time flexibility).

While occupational transitions therefore do reflect similarity in tasks, temporal flexibility, and leadership requirements, we note that there is substantial variation in occupational transitions which is not captured by these other occupational similarity measures. Appendix Table A.3 shows the adjusted R-squared statistics from regressions of $\pi_{o \to p}$ on our measures of skill distance, wage difference, amenity difference (temporal flexibility), leadership difference, and a composite skill measure. In all of these cases, while the correlation is strong and positive, the explanatory power is low.

The failure of skill similarity measures to explain many occupational transitions can be illustrated by a few cases from our data. First, consider some occupation pairs that are very similar on a skill distance metric (in the lowest distance decile), but where our data shows almost no (less than 0.01%) chance of moving from one to the other when switching jobs, in either direction: Surveyors vs. Medical & clinical laboratory technologists; Carpenters vs. Dental assistants; Travel agents vs. Po-

[26] These patterns could be in line both with career progression for individual workers, and/or with the aggregate decline of routine occupations over the same time period documented by, for example, Acemoglu and Autor (2011), and the increasing demand for social skills documented by Deming (2017).

lice, fire & ambulance dispatchers. In all of these occupational pairs it is intuitively clear why they may look similar in terms of an abstract description of the tasks involved, but in practice this skill distance does not make them relevant outside options for one another because of differences in other job characteristics or requirements. Second, consider another pair of occupations which are very similar on the skill distance metric (again, in the lowest distance decile): Pediatricians vs. Management analysts. When pediatricians change jobs, 8.7% of them become management analysts, but less than 0.01% of management analysts switching jobs become pediatricians. The skill distance metric misses the fact that one of these occupations requires extensive training and licensing which means that, in practice, the occupational move is only possible in one direction.

## A.7 IV analysis details

### Identification assumptions for concentration instrument

This section provides more formal details on the assumptions required for the IV identification of the effects of labor market concentration on wages. Our instrument can be interpreted as a type of granular IV following Gabaix and Koijen (2020), where market-level trends are instrumented for using idiosyncratic firm-level shocks (for details on the granular IV identification approach see Gabaix and Koijen (2020)). Or, it can be seen through the lens of the Bartik or shift-share IV approach, following Borusyak et al. (2018), with exogenous 'shocks' in the form of differential national hiring patterns for large firms, and initial squared employer shares of each firm in a given local labor market determining the exposure to those shocks.

We can rewrite the concentration instrument as

$$Z^{HHI}_{o,k,t} = \sum_j \sigma^2_{j,o,k,t-1}\left(\frac{(1+\tilde{g}_{j,o,t})^2}{(1+\tilde{g}_{o,k,t})^2} - 1\right)$$
$$= \sum_j \sigma^2_{j,o,k,t-1}\tilde{G}_{j,o,k,t}$$

where $\tilde{G}_{j,o,k,t} = \frac{(1+\tilde{g}_{j,o,t})^2}{(1+\tilde{g}_{o,k,t})^2} - 1$ is the predicted firm-level excess local vacancy growth relative to the average predicted local occupation vacancy growth - the time-varying shock - and $\sigma^2_{j,o,k,t-1}$ is the exposure of the local concentration index to that shock.[27]

As noted in the main text, we add three controls to our baseline specification. To control for any effects on local labor demand of differential exposure to large national firms' hiring, we control for (1) the growth rate of local vacancies in the occupation-city labor market ($g_{o,k,t} = \sum_j \sigma_{j,o,k,t-1} g_{j,o,k,t}$), and (2) the predicted growth rate of local vacancies based on large firms' national growth ( $\tilde{g}_{o,k,t} = \sum_j \sigma_{j,o,k,t-1}\tilde{g}_{j,o,t}$). To control for differential initial exposure to non-local firms, we introduce our "exposure control" $e_{o,k,t} = \sum_j \sigma^2_{j,o,k,t-1} \cdot 1[\tilde{g}_{j,o,t} \neq 0]$.

In our fixed effects IV estimation of equation (1.9), the exclusion restriction for the instrument on the HHI concentration index is then equivalent to

$$\mathrm{Cov}[Z^{HHI}_{o,k,t}, \xi_{o,k,t} | \Gamma_{kt}, \Gamma_{ot}, g_{o,k,t}, \tilde{g}_{o,k,t}, e_{o,k,t}] = \mathbb{E}\left[\sum_{t=1}^{T}\sum_{o}^{N^{occs}} \sigma^2_{j,o,k,t-1}\tilde{G}^{\perp}_{j,o,k,t}\xi_{o,k,t}\right] \to 0$$

Here, $\tilde{G}^{\perp}_{j,o,k,t}$ represents $\tilde{G}_{j,o,k,t}$ after it has been residualized with regard to city-k-by-year-t fixed ef-

[27] For simplicity of exposition, we assume here that employer concentration and outside-occupation options are not correlated – but the logic of this argument does not depend on this assumption.

fects $\Gamma_{kt}$ and occupation-o-by-year-t fixed effects $\Gamma_{ot}$, as well as our three control variables $g_{o,k,t}$, $\tilde{g}_{o,k,t}$, $e_{o,k,t}$.

This orthogonality condition holds under two assumptions. First, we require that the national firm-level growth shocks are quasi-randomly assigned conditional on local exposure to structural wage shocks $\xi_{o,k,t}$, the fixed effects $\Gamma_{kt}$ and $\Gamma_{ot}$, actual and predicted average local vacancy growth $g_{o,k,t}$ and $\tilde{g}_{o,k,t}$, and initial exposure to non-local firms $e_{o,k,t}$. That is,

$$\mathbb{E}[\sum_j \sigma^2_{j,o,k,t-1}\tilde{G}_{j,o,k,t}|\xi_{o,k,t}, \Gamma_{kt}, \Gamma_{ot}, g_{o,k,t}, \tilde{g}_{o,k,t}, e_{o,k,t}] = \tau_1\Gamma_{kt}+\tau_2\Gamma_{ot}+\tau_3 g_{o,k,t}+\tau_4\tilde{g}_{o,k,t}+\tau_5 e_{o,k,t} \quad \forall o \in N^{occs}$$

for some constant parameters $\tau_1$, $\tau_2$, $\tau_3$, $\tau_4$, and $\tau_5$. That is, once we account for the control variables, expected local squared exposure to excess national firm-level growth needs to be random in expectation.[28]

Second, there needs to be a large number of independent firm-level shocks, that is,

$$\mathbb{E}[(\tilde{G}_{j,o,k,t} - E[\tilde{G}_{j,o,k,t}])(\tilde{G}_{j,o,k,t} - E[\tilde{G}_{j,o,k,t}])|\phi_{pt}, \phi_{jt}, \Gamma_{kt}, \Gamma_{ot}] = 0$$

for all $p, j \in N^{occs}$ if $p \neq j$.

The first assumption requires that the local size-squared-weighted exposure to national firm-level employment shocks does not affect the local wage in occupation o through a direct channel

[28]In a robustness check, we also include a control for average vacancy growth across firms within a local occupation, with each firm weighted equally, $\frac{1}{N}\sum_j g_{j,o,k,t}$. This is suggested by Gabaix and Koijen (2020) as an appropriate control for local demand effects in a granular IV setting, as it controls for the increase in vacancies experienced commonly across all firms in the local labor market. The identification assumptions in the specification with this control would require that local squared exposure to excess national firm-level growth is random in expectation conditional on this proxy for local labor demand (alongside the other controls and fixed effects already discussed).

other than increasing the local labor market concentration $HHI_{o,k,t}$, conditional on the control variables. Note that this allows for different local occupations to have different average expected average growth rates based on national firm growth. It only requires that whether this growth is driven by the *national* growth of locally large firms vs. small firms varies across local occupations in a way that is uncorrelated with local wage residuals.

To be concrete, note the hypothetical example from the main text, which considered insurance sales agents in Bloomington, Illinois and in Amarillo, Texas. In each city, there are several insurance companies who employ insurance sales agents. Assume that in Bloomington, State Farm has a large share of local insurance sales agent employment, while in Amarillo employment is more dispersed amongst a number of insurance companies. In years where State Farm grows substantially faster than other major insurance companies nationwide, under most combinations of the distribution of that growth across cities and the initial distribution of employer shares in each city, employer concentration of insurance sales agents will grow by more in Bloomington IL than in Amarillo TX. Moreover, our granular IV identification approach controls for local growth rates of overall insurance sales agent employment in both cities. Thus, it allows for each city to be exposed differently to overall trends in the demand for insurance sales agents. The identification only requires that once we account for overall city exposure to insurance sales agent demand, whether that demand was driven by the city's major employer or smaller employers is not correlated with local idiosyncratic wage shocks for insurance sales agents.

How does the first-stage assumption work? The first stage of our regression holds if, when firm j grows nationally, local occupation-city labor markets with a higher share of vacancies accounted for by firm j in year t − 1 see a larger increase in employer concentration. A sufficient condition for this to be the case under *most* initial employer share distributions is if firm j's new vacancies are allocated evenly across occupation-city labor markets, such that each occupation-city labor market sees the same growth rate in its firm j vacancies as the national average.[29] However, this condition is not necessary: in fact, the first stage can be valid even if the growth rate of firm j's new vacancies in low-initial-employment-share occupation-city labor markets is higher than in high-initial-employment-share labor markets, as long as this relationship is not too strong. In our data, for a given employer, there is a negative relationship between the initial vacancy share in an occupation-city labor market and the next year's vacancy growth rate, but this relationship is not sufficiently strong to invalidate our first stage (for each one percentage point increase in the initial vacancy share, there is roughly a 1.4 percentage point lower vacancy growth rate from one year to the next). Empirically, our first stage holds for occupation-city labor markets with HHIs above all but very low levels, as shown in the main text (and in Appendix Tables A.7 and A.13).

[29]Note that this is not the case – i.e. the first stage might not hold – for *all* possible combinations of the distribution of employment growth and initial employer shares. For example, consider a world in which there is a labor market for where Employer X has 80% of the market in one city, and the rest of the market is comprised of atomistic firms; and Employer X has 65% of the market in another city, with the rest of the market comprised of atomistic firms. If Employer X grows by 10% in both locations in a given year, and the other firms do not grow at all, employer concentration will actually increase by more in the latter than the former market. This circumstance, however, only occurs when comparing two labor markets which both have extremely high levels of employer concentration already, and so is not relevant for the vast majority of the labor markets in our data (only 6% of which have HHIs of greater than 6,800 – and only 2% of workers in our data face HHIs of greater than 5,000). In practice our first stage regressions are positive and strongly significant even when segmenting our data to analyze only cells with high HHIs of 2,500 of more, meaning this concern is not hugely relevant in practice for our empirical analysis.

### Identification assumptions for outside-occupation option index instrument

This section provides more formal details on the assumptions required for identification of the outside-occupation options effect on wages using the instrumental variables strategy based on national leave-one out mean wages.

As described in Section 1.5, our instrument for the $oo^{occs}$ index, $Z^{oo}$, is the weighted average of national leave-one out mean wages in occupation p, $\bar{w}_{p,\not{k},t}$, where the weights are the product of the year 1999 relative employment share in each of those occupations in the worker's own city, $\frac{s_{p,k,1999}}{s_{p,1999}}$,[30] and the national occupation transition shares from the worker's occupation o to each of the other occupations, $\pi_{o\rightarrow p}$:

$$Z^{oo}_{o,k,t} = \sum_{p}^{N_{occs}} \left( \pi_{o\rightarrow p} \cdot \frac{s_{p,k,1999}}{s_{p,1999}} \cdot \bar{w}_{p,\not{k},t} \right) \tag{A.16}$$

To make the assumptions transparent under which this wage instrument identifies the coefficient on our outside-occupation option index in equation (1.9), we again follow the framework presented in Borusyak et al. (2018).[31] Note that we can write the instrument as

$$Z^{oo}_{o,k,t} = \sum_{p=1}^{N^{occs}} s_{okp} \bar{w}_{p,\not{k},t}$$

[30] Or the first year in the data, if there is no data for the occupation-city cell in 1999.

[31] For simplicity, assume that the outside-occupation option index and the concentration index are not correlated - but the intuition for the identification does not depend on that.

where $s_{okp} = \pi_{o \to p} \cdot \frac{s_{p,k,1999}}{s_{p,1999}}$ is a measure of predicted local exposure to the shock. In our fixed effects IV estimation of equation (1.9), the exclusion restriction for the instrument for outside-occupation options is then equivalent to

$$\mathrm{Cov}[Z^{oo}_{o,k,t}, \xi_{o,k,t} | \Gamma_{kt}, \Gamma_{ot}, g_{o,k,t}, \tilde{g}_{o,k,t}, e_{o,k,t}] = \sum_{t=1}^{T} \sum_{p=1}^{N^{occs}} \bar{s}_{okp} w^{\perp}_{p,\cancel{k},t} \varphi^{oo}_{pt} \to 0$$

where $\bar{s}_{okp} = \mathbb{E}[s_{okp}]$ is the average exposure to occupation p, and $\varphi^{oo}_{pt} \equiv \mathbb{E}[s_{okp}\xi_{o,k,t}]/\mathbb{E}[s_{okp}]$ is an exposure-weighted expectation of the structural wage residuals. Moreover, $w^{\perp}_{p,\cancel{k},t}$ represents $\bar{w}_{p,\cancel{k},t}$ after it has been residualized with regard to city-k-by-year-t fixed effects $\Gamma_{kt}$ and occupation-o-by-year-t fixed effects $\Gamma_{ot}$, as well as the concentration index and control variables.

Borusyak et al. (2018) show that this orthogonality condition holds under two assumptions. First, we require that the national occupation-level shocks are quasi-randomly assigned conditional on local exposure to structural wage shocks $\varphi_{pt}$, the fixed effects $\Gamma_{kt}$ and $\Gamma_{ot}$, and the control variables. That is,

$$\mathbb{E}[\bar{w}_{p,\cancel{k},t} | \varphi^{oo}_{pt}, \Gamma_{kt}, \Gamma_{ot}, g_{o,k,t}, \tilde{g}_{o,k,t}, e_{o,k,t}] = \tau_1 \Gamma_{kt} + \tau_2 \Gamma_{ot} + \tau 3 g_{o,k,t} + \tau 4 \tilde{g}_{o,k,t} + \tau 5 e_{o,k,t} \quad \forall p \in N^{occs}$$

for some constant parameters $\tau_1$ through $\tau_5$. Second, there needs to be a large number of independent occupational shocks, that is,

$$\mathbb{E}[(\bar{w}_{p,\cancel{k},t} - \mu)(\bar{w}_{j,\cancel{k},t} - \mu) | \varphi_{pt}, \varphi^{oo}_{jt}, \Gamma_{kt}, \Gamma_{ot}, g_{o,k,t}, \tilde{g}_{o,k,t}, e_{o,k,t}] = 0$$

for all $p, j \in N^{occs}$ if $p \neq j$, and also $\sum_{p=1}^{N^{occs}} \bar{s}_p^2 \to 0$.

The first assumption requires that the national leave-one-out mean wage $\bar{w}_{p,\not{k},t}$ in outside option occupation p is correlated with the local wage of occupation p in location k (relevance condition), but does not affect the local wage in initial occupation o through a direct channel other than increasing the quality of local outside options $Z^{oo}_{o,k,t}$. However, this lack of a direct effect only needs to hold *conditional* on controlling for fixed effects that include the national wage trend in occupation o itself and wage trends that are common to all occupations in city k.[32] The inclusion of these fixed effects increases our confidence that the assumptions for instrument validity hold.

### Industry Bartik shock

One possible concern with the identification assumptions required for our outside-occupation index – which may not entirely be picked up by our occupation-year, or city-year fixed effects – is that industry-level wage trends may differentially impact local occupations based on their city's direct exposure to those industries, rather than only based on indirect exposure through outside occupation job options. As discussed in the text, an example of this could be the following. Imagine that the finance industry and the tech industry employ both accountants and data scientists to a disproportionate degree relative to other occupations, and that San Francisco has a large share of employment in tech while New York has a large share of employment in finance. Imagine further that being a data scientist is a good outside option occupation for an accountant. In years where the tech indus-

[32] As an example, note that national-level correlation in the wages of a pair of occupations (e.g. Compliance Officers and Financial Analysts), perhaps due to common industry shocks, does *not* invalidate this identification strategy, because we are holding national wage trends constant for each occupation and are identifying outside option effects from the differences between cities *within* occupations.

try is booming nationwide, this will impact San Francisco more than New York. Accountants in San Francisco will see wages rising by more than accountants in New York – partly driven by the increase in the outside option value of becoming a data scientist, but partly simply because more accountants in SF already work in the tech industry, as compared to accountants in NY, and so they will see their wages rise by more.

To control for this possible omitted variable bias, we incorporate an industry Bartik shock in a robustness check for our baseline regressions. We construct the industry Bartik shock for each occupation-city-year cell. The industry Bartik shock for occupation o in city k in year t is defined as

$$\sum_{\iota}^{\text{industries}} \frac{\frac{\text{emp}_{\iota,o,t-1}}{\text{emp}_{o,t-1}} \cdot \frac{\text{emp}_{\iota,k,t-1}}{\text{emp}_{k,t-1}} \cdot \bar{w}_{\iota,t}}{\frac{\text{emp}_{\iota,o,t-1}}{\text{emp}_{o,t-1}} \cdot \frac{\text{emp}_{\iota,k,t-1}}{\text{emp}_{k,t-1}}}$$

where $\iota$ denotes each NAICS 4-digit industry. The shock to each industry is the national industry wage $\bar{w}_{\iota,t}$, and the exposure of each local occupation to that shock is determined by (1) the national share of employment in that occupation which is in industry $\iota$, $\frac{\text{emp}_{\iota,o,t-1}}{\text{emp}_{o,t-1}}$ and (2) the share of employment in that city which is industry $\iota$, $\frac{\text{emp}_{\iota,k,t-1}}{\text{emp}_{k,t-1}}$. The exposure measures are lagged by one year to avoid the possibility of endogenous responses of employment to the industry-level shock in question. The Bartik instrument relies on the assumption that national industry-level wage shocks are uncorrelated with local occupation-level wage trends, except to the extent that the former causes the latter. We use data on employment by NAICS 4-digit industry and SOC 6-digit occupation from the Bureau of Labor Statistics Occupational Employment Statistics to construct the employment shares of each occupation by industry, and we construct data on industry employment shares by metropolitan statistical area ("city") from Eckert et al. (2020)'s county-by-industry employment data, which was constructed from the County Business Patterns database. We report our baseline regression results, controlling for this industry Bartik shock, in Panel A of Table A.9.

In other unreported specifications, we construct the industry Bartik shock with, variously, national wage growth in each industry $\iota$, national employment growth in each industry $\iota$, or the national wage bill growth in each industry $\iota$. In all these specifications, our coefficients on the outside-occupation option index and HHI index (instrumented) remain very similar to those in the baseline specification.

## A.8 Stata commands

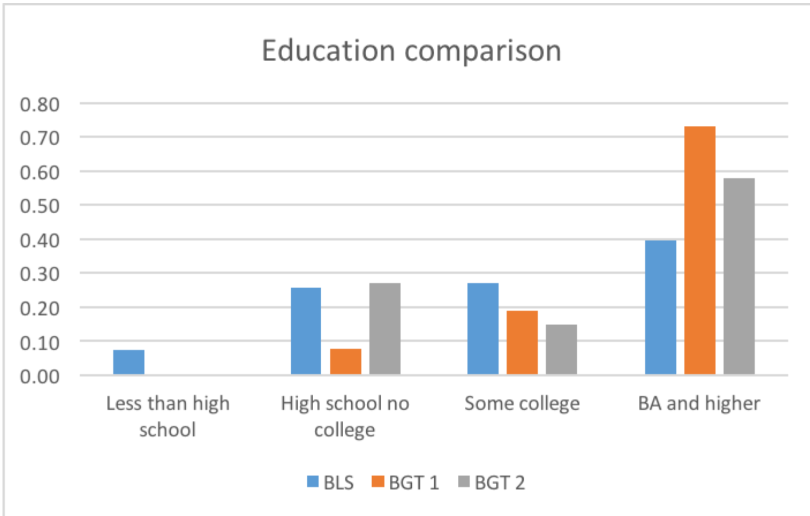
In our estimation, we used a number of user-written Stata commands: *reg2hdfe* (Guimaraes and Portugal, 2010), *reghdfe* (Correia, 2016), *ivreg2hdfe* (Bahar, 2014), *binscatter* (Stepner, 2013), *binscatter2* (Droste, 2019), and *coefplot* (Jann, 2013).

## A.9 Appendix Figures

**Figure A.1:** BGT Vacancy Data: representedness of occupations, relative to BLS OES

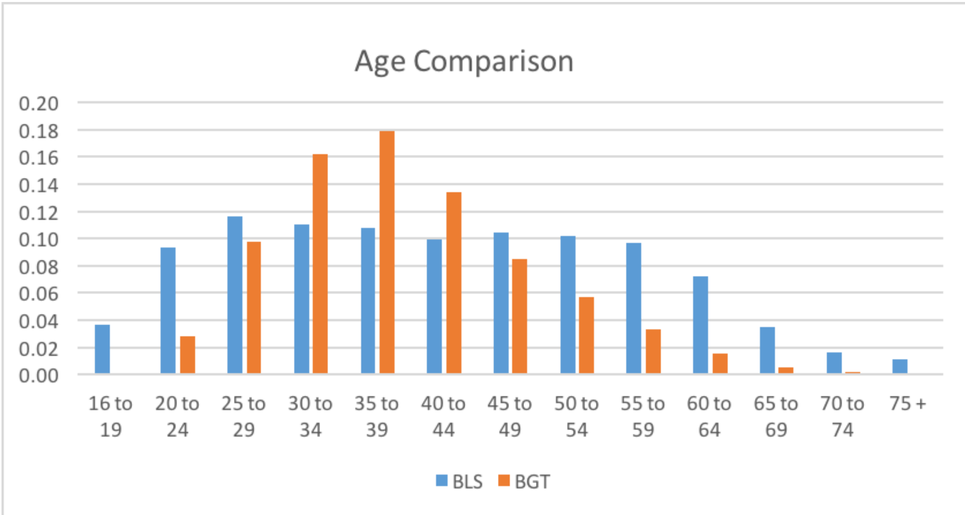

Note: Comparison of distribution of share of each SOC 6-digit occupation in the BGT vacancy data, relative to its share in the BLS occupational employment statistics, with occupations comprising greater than 1% share of either data set labeled. Red line is the 45 degree line. The vacancy data is discussed in detail in Appendix Section A.2.

**Figure A.2:** BGT Resume Data: education relative to 2018 labor force



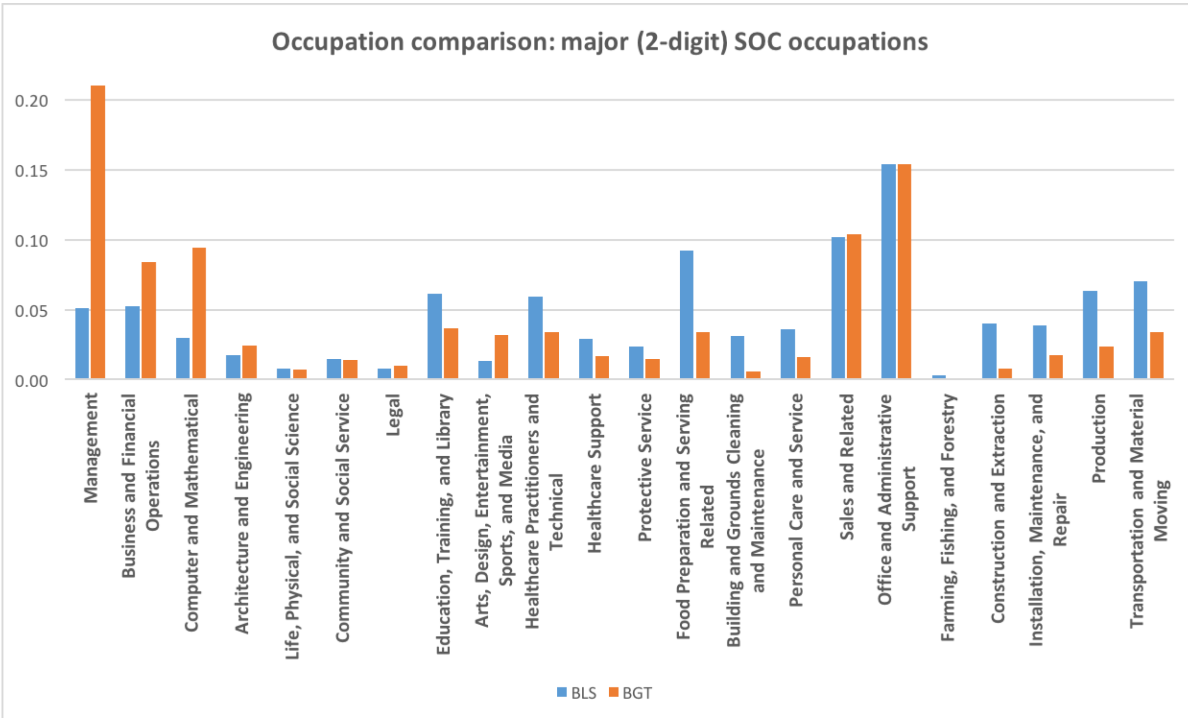

Note: Comparison of distribution of highest educational attainment in the labor force, according to BLS data, to distribution in BGT resume data. Two versions are shown: BGT 1 excludes all resumes missing educational information, while BGT 2 assumes all resumes missing educational information have high school education but no college. The resume data is discussed in detail in Appendix Section A.3.

**Figure A.3:** BGT Resume Data: age distribution relative to 2018 labor force



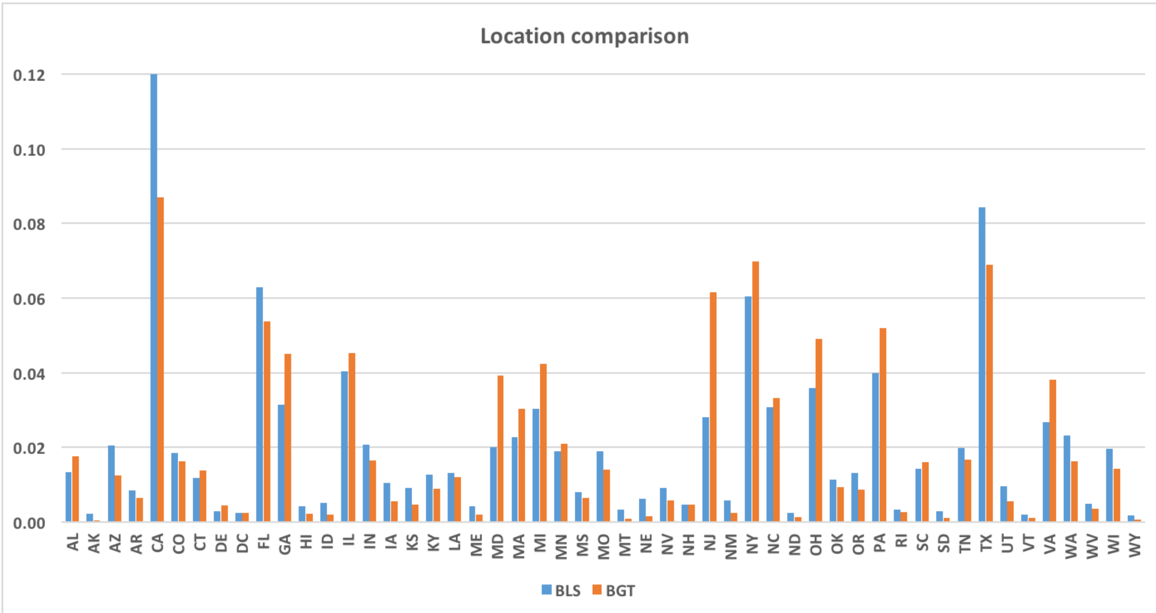

Note: Comparison of distribution of age in the labor force, according to 2018 BLS data, to distribution of imputed worker ages in BGT resume data. The resume data is discussed in detail in Appendix Section A.3.

**Figure A.4:** BGT Resume Data: occupations relative to 2017 labor force



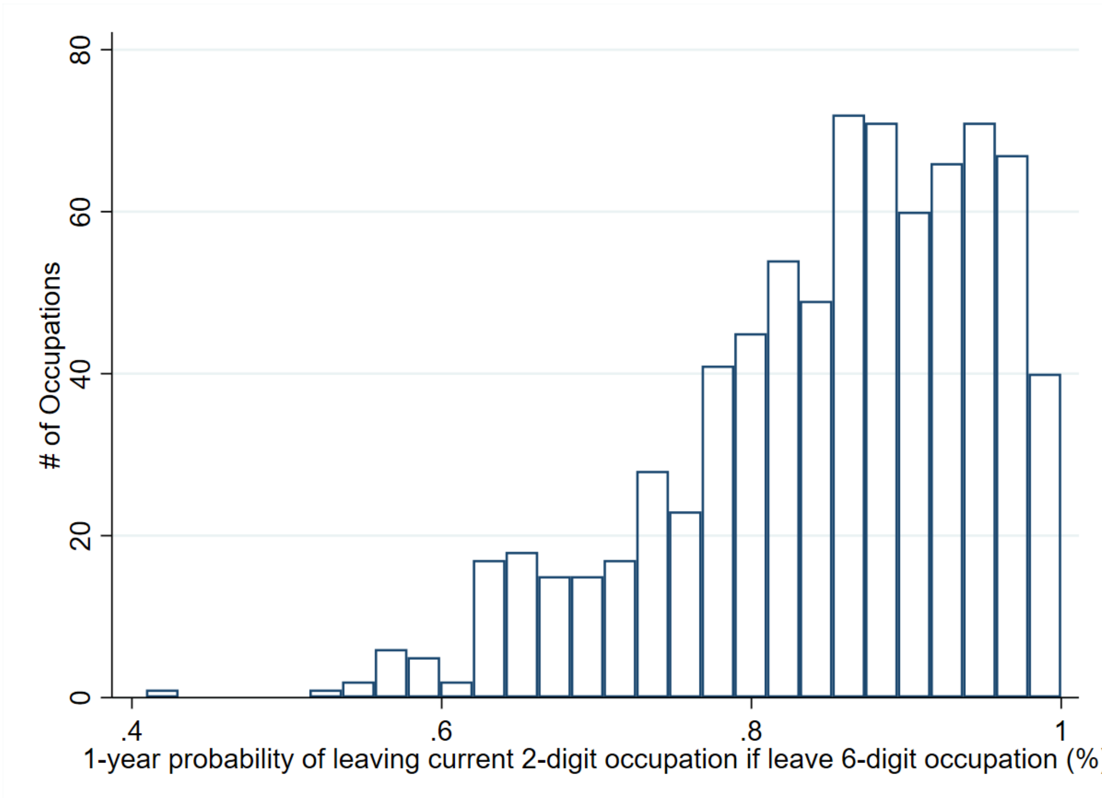

Note: Comparison of distribution of 2-digit SOC occupations in the labor force, according to 2017 BLS data, to distribution of occupations in BGT resume data. The resume data is discussed in detail in Appendix Section A.3.

**Figure A.5:** BGT Resume Data: locations relative to 2017 labor force



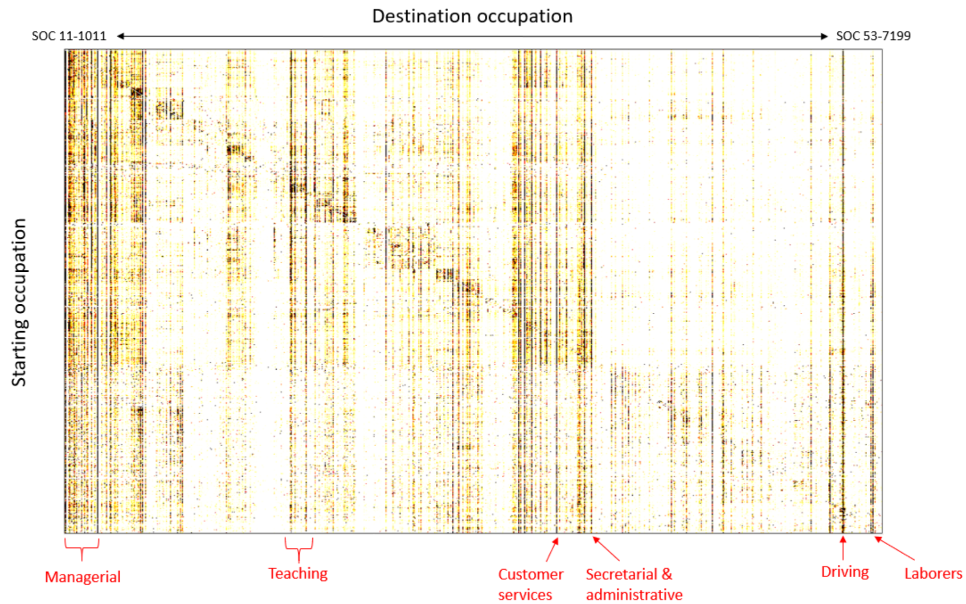

Note: Comparison of distribution of employment by U.S. state, according to 2017 BLS data, to distribution of resume addresses in BGT resume data. Graph shows share of total in each state. The resume data is discussed in detail in Appendix Section A.3.

**Figure A.6:** Occupational mobility: SOC 6-digit moves that are also 2-digit moves



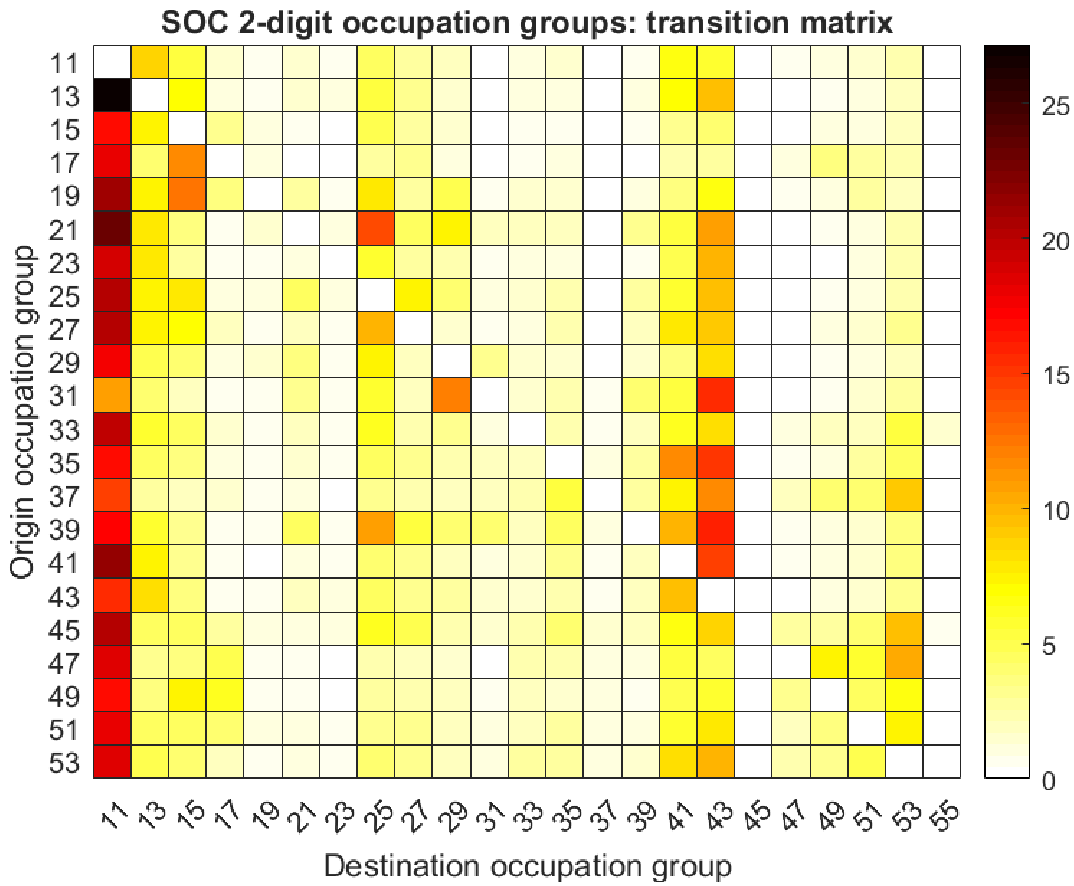

Note: Distribution of the proportion of workers moving 6-digit SOC occupation who *also* move 2-digit SOC occupation, by occupation, calculated from BGT resume data for 2002-2015 period. Histogram shows 786 occupations. The resume data is discussed in detail in Appendix Section A.3.

**Figure A.7:** 6-digit SOC occupational transition matrix



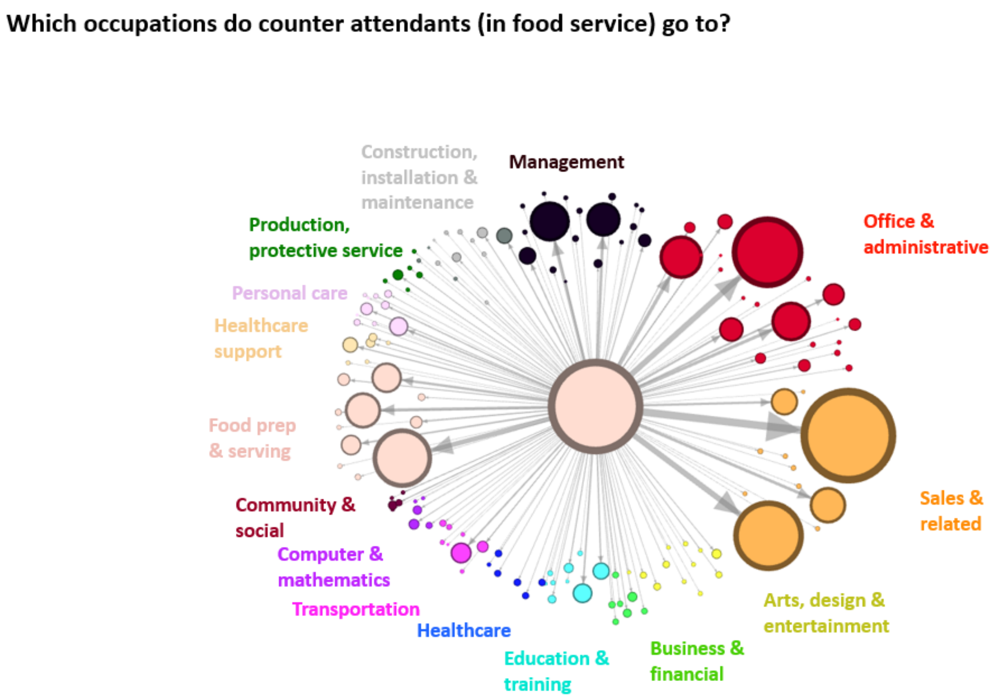

Note: Occupational transition matrix showing transition probability between 6-digit SOC occupations conditional on leaving the initial job. Occupations are sorted in SOC numerical order. Cells colored black have a transition probability of 1% or greater conditional on leaving the initial job. Transitions to own occupation are excluded. Data computed from BGT resume data set for 2002-2015. The annotation points out certain common destination occupations, which show up as darker vertical lines on the heatmap. The presence of a darker line along the diagonal suggests that workers commonly transition to occupations which are close to their own according to the numerical order of SOC codes. The resume data is discussed in detail in Appendix Section A.3.

**Figure A.8:** 2-digit SOC occupational transition matrix



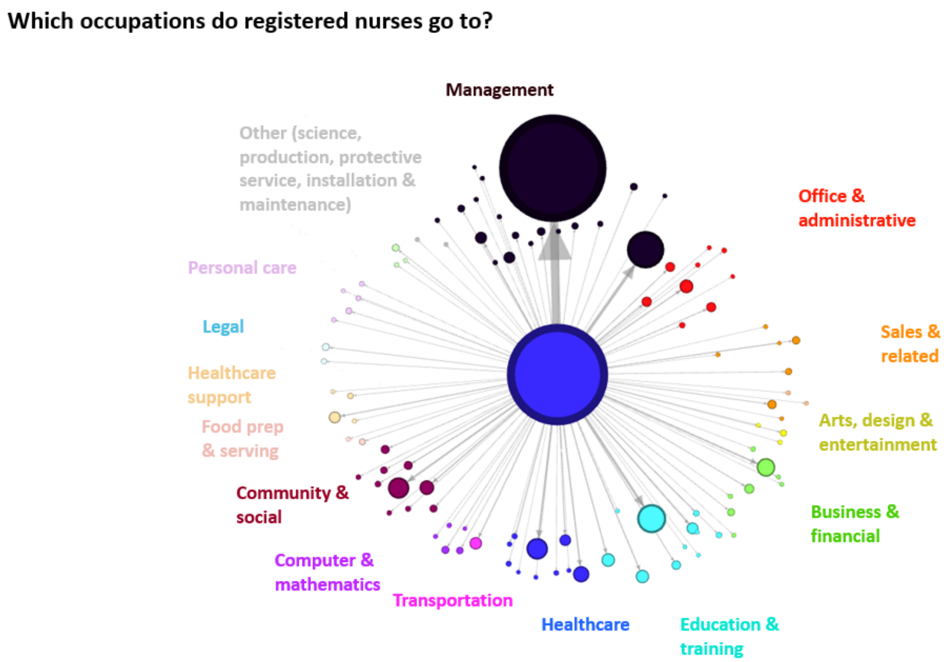

Note: Occupational transition matrix showing transition probability between 2-digit SOC occupation groups conditional on leaving the initial job. Cells colored black have a transition probability of 25% or greater conditional on leaving the initial job. Job transitions within an occupation group are excluded. Data computed from BGT resume data set for 2002-2015. The resume data is discussed in detail in Appendix Section A.3.

**Figure A.9:** Examples of probabilistic labor markets: counter attendants



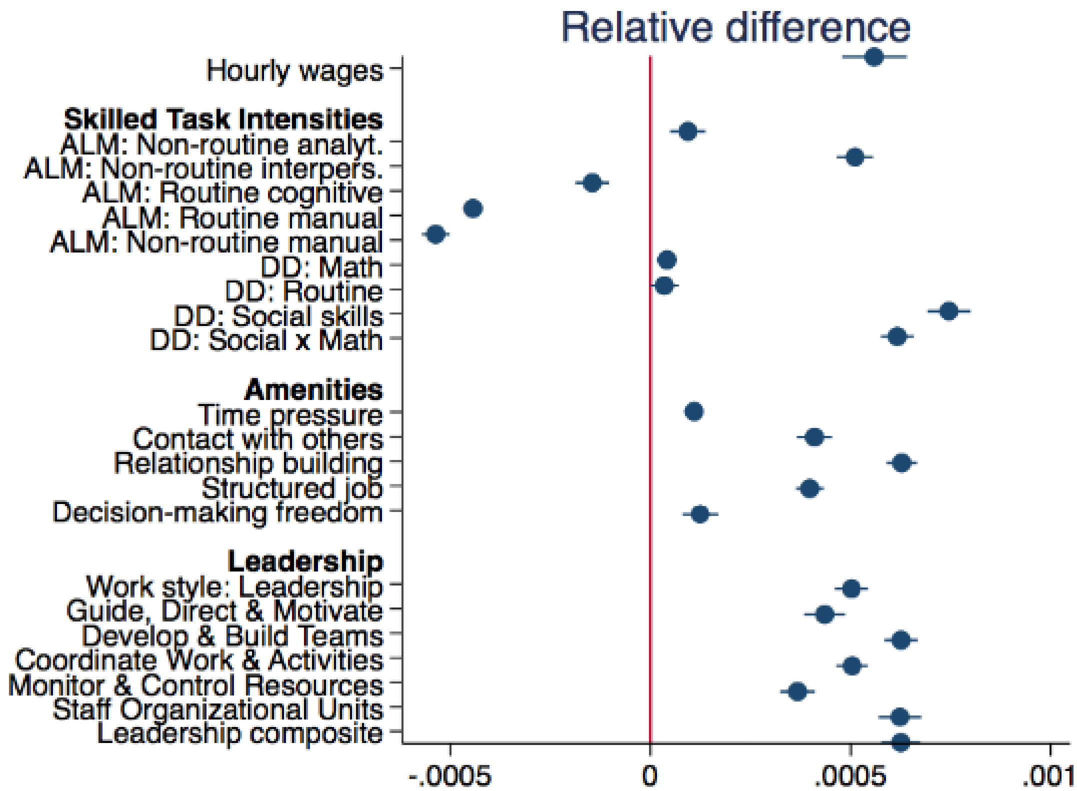

Note: Example visualization of occupational transitions for counter attendants in the food industry. Each bubble is a SOC 6-digit occupation, and the colors represent SOC 2-digit occupational groups. The size of each bubble is proportional to the share of counter attendants in the BGT data who switch occupation, who are observed in each destination occupation in the following year. The resume data is discussed in detail in Appendix Section A.3.

**Figure A.10:** Examples of probabilistic labor markets: registered nurses



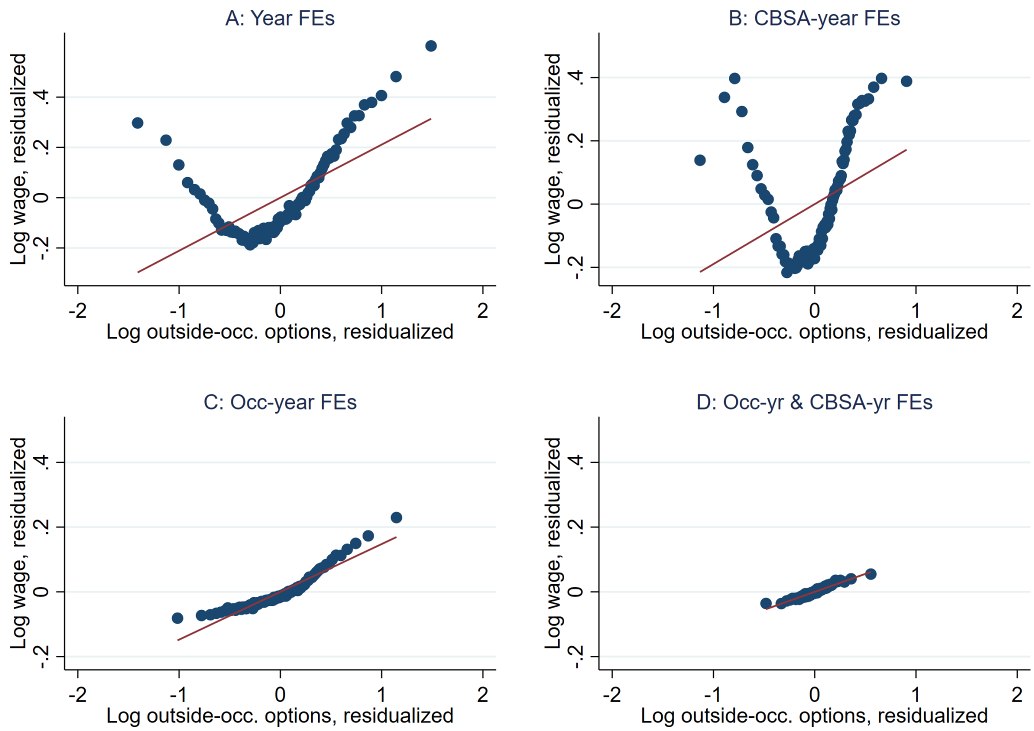

Note: Example visualization of occupational transitions for registered nurses. Each bubble is a SOC 6-digit occupation, and the colors represent SOC 2-digit occupational groups. The size of each bubble is proportional to the share of registered nurses in the BGT data who switch occupation, who are observed in each destination occupation in the following year. The resume data is discussed in detail in Appendix Section A.3.

**Figure A.11:** Determinants of occupational mobility



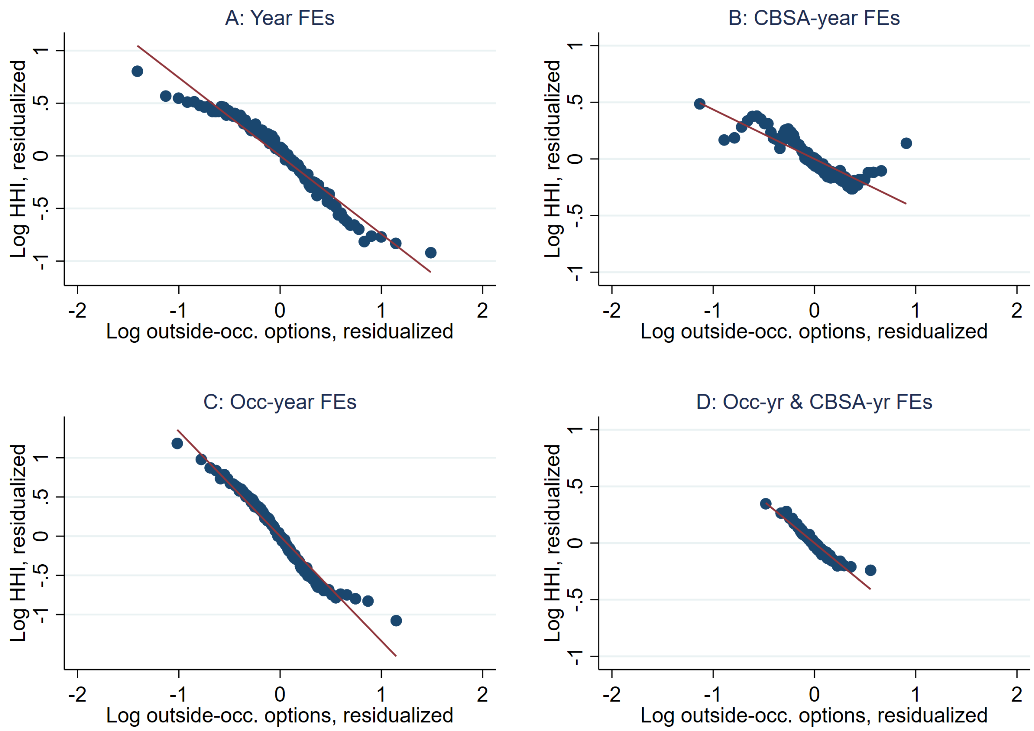

Note: This plot shows the coefficients and 95% confidence intervals from regressions of occupation transition shares $\pi_{o \to p}$, calculated from Burning Glass Technologies Resume data, on relative differences in occupational characteristics: $\pi_{o \to p} = \alpha_o + \beta f(X_{occ\ o \to p}) + \gamma f(\Delta w_{o \to p}) + \varepsilon_{op}$ where the function $f(\cdot)$ represents the difference in characteristic between starting occupation $o$ and destination $p$, and $\alpha_o$ is occupation $o$ fixed effect. Regressions also include absolute avg. hourly wage differences (except for the amenities regressions). Standard errors are clustered at the origin occupation level. This is the analog of Figure 1.3, which shows coefficients on the regression of occupation transition shares on the absolute difference in characteristics between the pairs of occupations. These analyses are discussed in more detail in Appendix Section A.6.

**Figure A.12:** Correlations between wage and outside-occupation option index



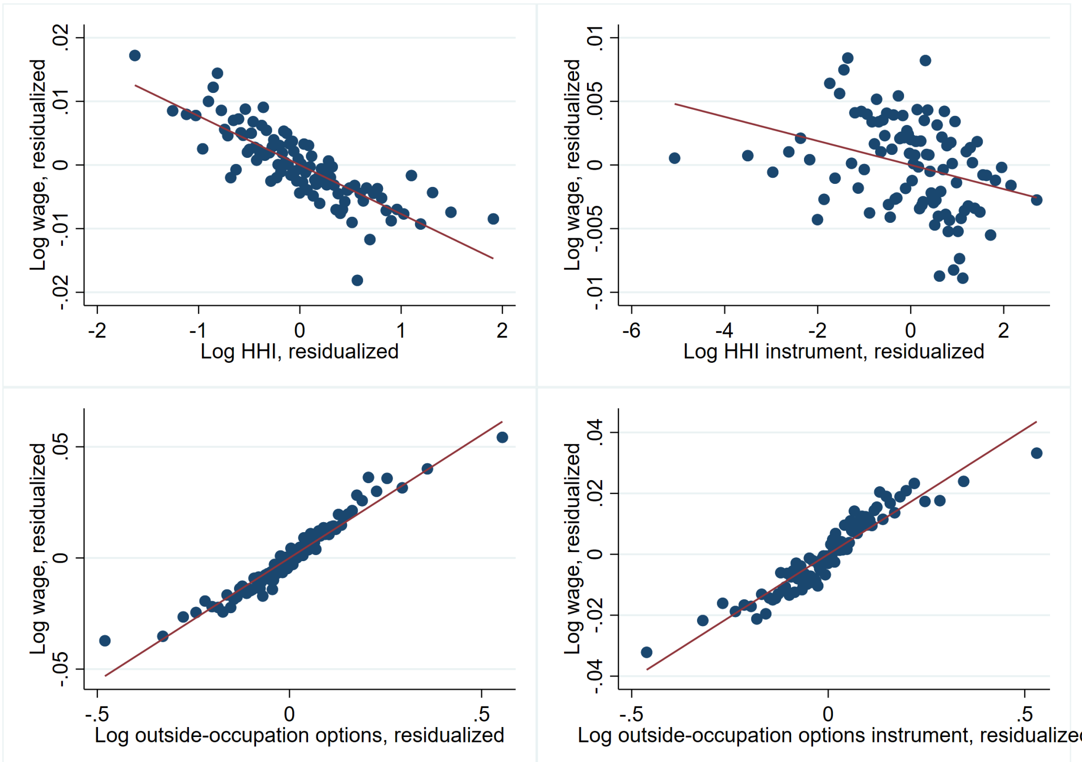

Note: Figure shows binned scatter plots of the correlation between average log wages and log outside-occupation option index for occupation-CBSA cells over 2013–2016, residualized on different combinations of fixed effects (as described by the panel titles). Regression coefficients for the line of best fit on each graph are: A: 0.16, B: 0.12, C: 0.15; D: 0.11. The non-linear shape of the figures without occupation fixed effects (panels A and B) is explained by healthcare occupations which tend to have both low outward mobility and high pay.

**Figure A.13:** Correlations between HHI and outside-occupation option index



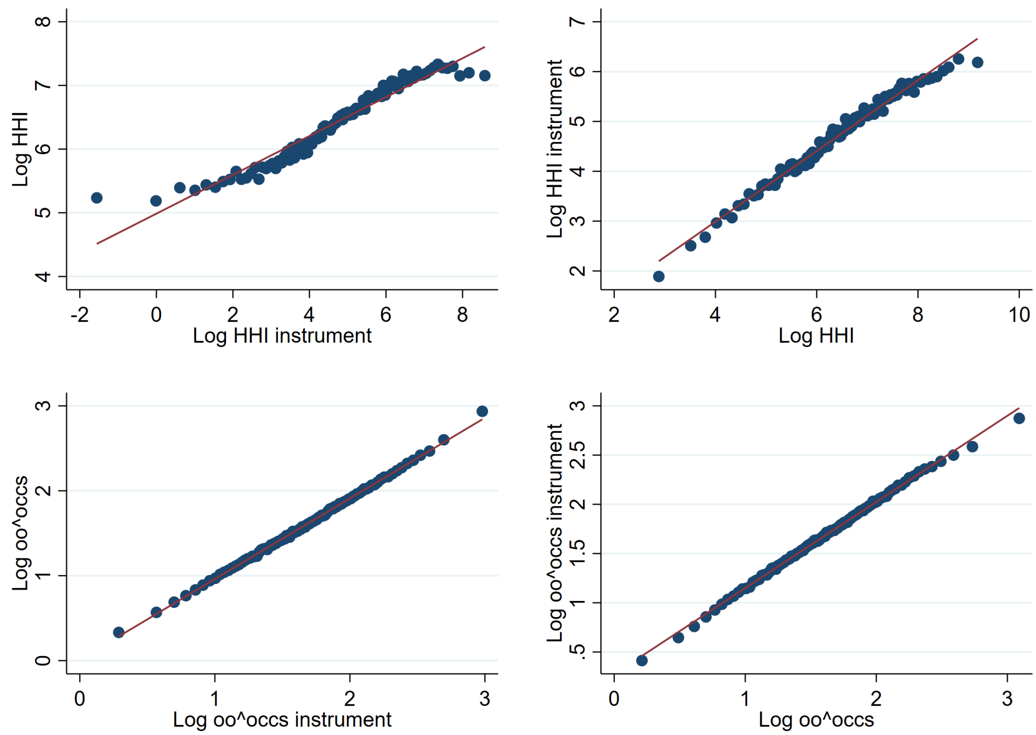

Note: Figure shows binned scatter plots of the correlation between average log HHI and log outside-occupation option index for occupation-CBSA cells over 2013–2016, residualized on different combinations of fixed effects (as described by the panel titles). Regression coefficients for the line of best fit on each graph are: A: -0.74, B: -0.44, C: -1.34; D: -0.74.

**Figure A.14:** Visualization of baseline regression results



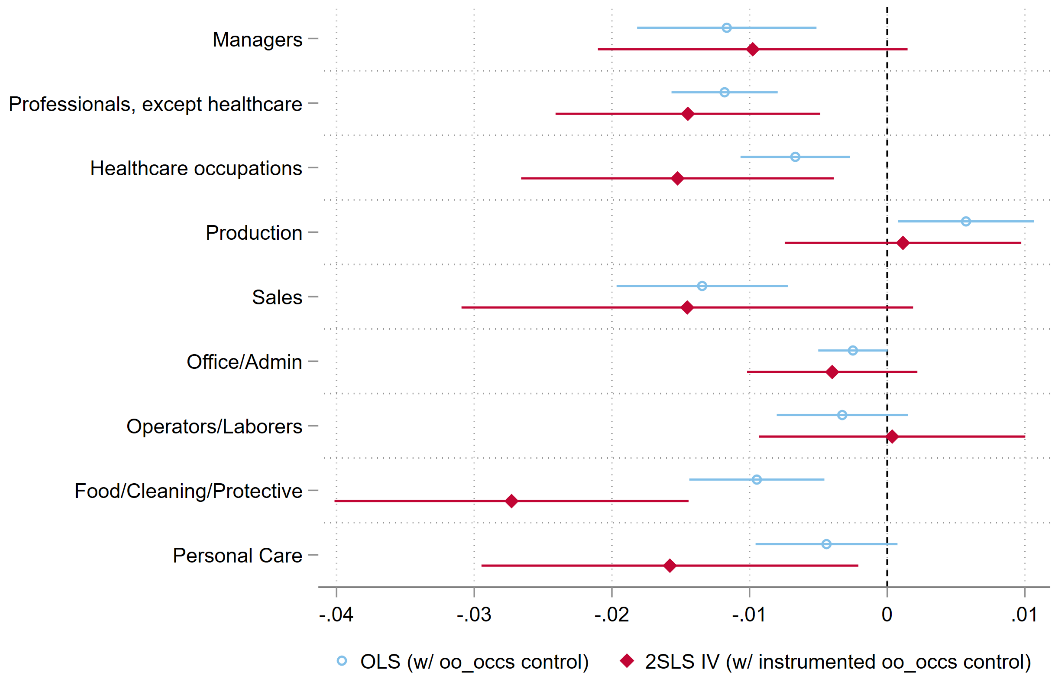

Note: Binned scatter plots of the log wage on log HHI and log outside-occupation options, and instrumented, including full controls as in baseline regression specification (i.e. the left panels correspond to coefficient estimates in Table 1.3 column (b), and the right panels correspond to the reduced form equivalent of the 2SLS IV coefficient estimates in Table 1.3 column (d)).

**Figure A.15:** Visualization of first-stage regressions



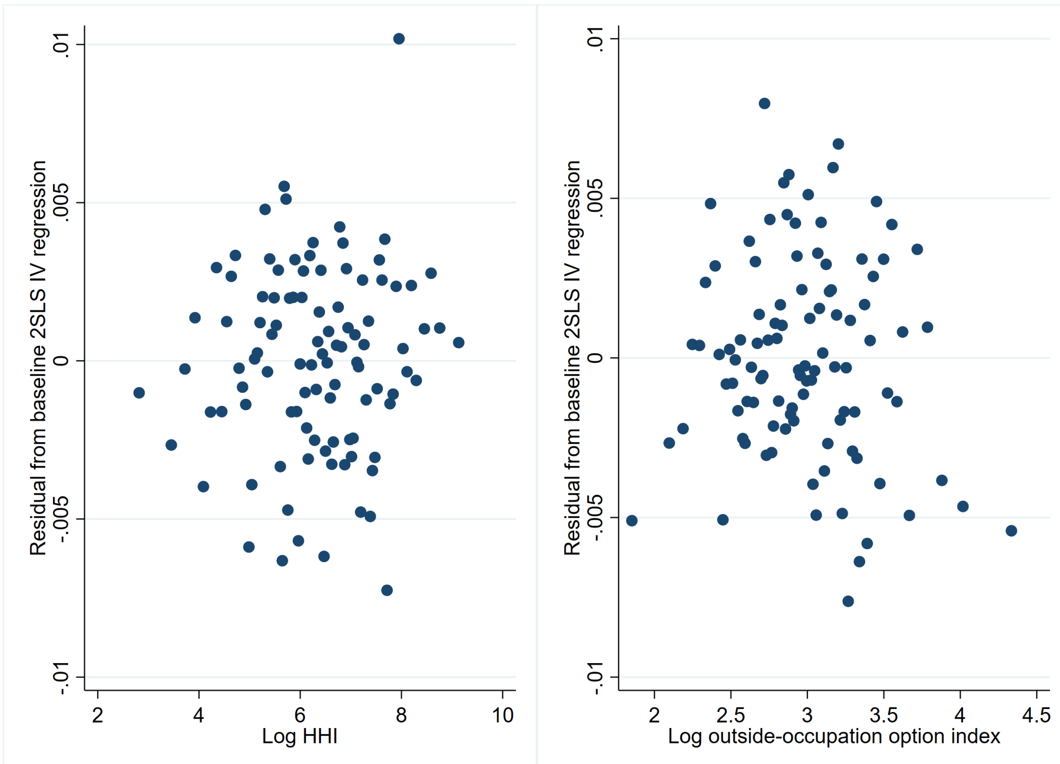

Note: Binned scatter plots of the correlation between the HHI instrument and raw variable (top panel) and outside-occupation option index instrument and raw variable (bottom panel) for occupation-CBSA cells in 2016.

**Figure A.16:** Coefficients on wage-HHI regressions: by occupation group



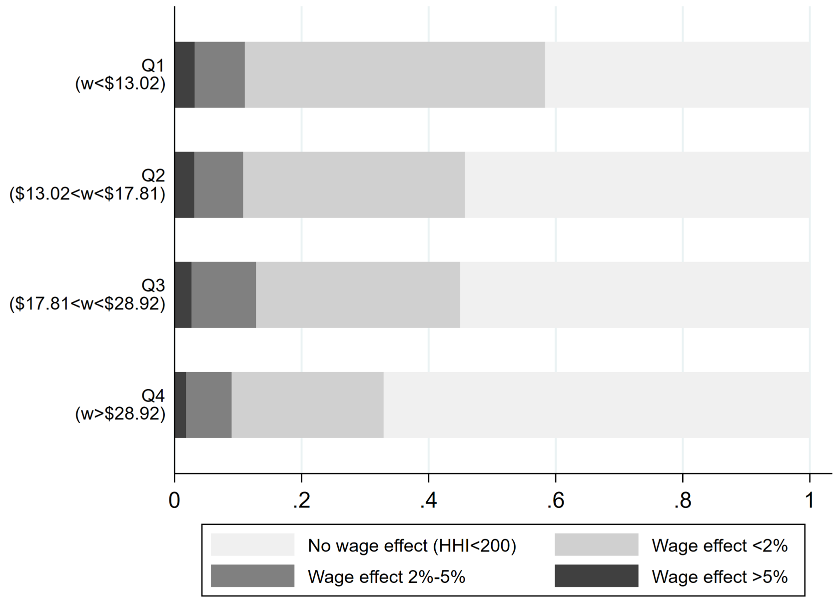

Note: Coefficients on HHI and 95% confidence intervals from regressions of occupation-CBSA wages on instrumented local employer HHI, controlling for (instrumented) outside-occupation job options, with coefficient on HHI allowed to vary by occupation group. Regressions span 2013-2016 and include occupation-year and CBSA-year fixed effects, as well as the other controls included in our baseline 2SLS IV regressions (described in Section 1.5). Standard errors are clustered at the CBSA level. Occupation groups are listed in descending order of average wage. Occupation groups map from SOC 2-digit occupations as defined in Appendix Table A.6. Coefficient estimates correspond to those in Appendix Table A.15 column (a).

**Figure A.17:** Baseline regressions: residual plots




Binned scatter plots of residuals from baseline 2SLS IV regression of wage on log HHI and log outside-occupation option index, against the log HHI and log outside-occupation option index respectively. (The baseline regression results are reported in column (d) of Table 1.3).

**Figure A.18:** Average estimated effect of employer concentration relative to HHI=200, by hourly wage




Note: This figure shows what share of workers may have experienced different degrees of wage suppression as a result of employer concentration, across the US hourly wage distribution, relative to a counterfactual where HHI was 200 (holding all else constant). We estimate the wage effect of employer concentration as described in section 1.8.

**Figure A.19:** Average estimated effect of employer concentration relative to HHI=200, by metro area


Note: This figure shows the average estimated wage effect of concentration in each metro area relative to a counterfactual HHI of 200 (holding all else constant), plotted against the average hourly wage in that metro area in 2016 (left) and the employment in that metro area in 2016 (right) according to BLS OES data. Bubble size in the left hand graph represents metro area employment in 2016. We estimate the wage effect of employer concentration as described in section 1.8: we use our coefficient estimates for the effect of the HHI on the wage, by quartile of outward occupational mobility, to calculate a counterfactual wage for each occupation-city labor market if the HHI had been 200.

## A.10 Appendix Tables

**Table A.1:** Summary statistics in BGT vacancy data

| | p5 | p10 | p25 | p50 | p75 | p90 | p95 |
|---|---|---|---|---|---|---|---|
| Total vacancies posted by employer (by employer) | 1 | 1 | 1 | 2 | 5 | 19 | 58 |
| No. of years employer present (by employer) | 1 | 1 | 1 | 1 | 2 | 3 | 4 |
| No. of years employer present (vacancy-weight) | 2 | 3 | 4 | 4 | 4 | 4 | 4 |
| Occ. share relative to BLS OES (by occ.) | 0.11 | 0.18 | 0.37 | 0.81 | 1.80 | 4.28 | 7.22 |
| Occ. share relative to BLS OES (emp.-weight) | 0.16 | 0.21 | 0.34 | 0.62 | 1.14 | 2.10 | 2.83 |
| Metro area share relative to BLS OES (by MSA) | 0.60 | 0.67 | 0.76 | 0.90 | 1.12 | 1.35 | 1.54 |
| Metro area relative to BLS OES (emp.-weight) | 0.60 | 0.63 | 0.75 | 0.90 | 1.03 | 1.17 | 1.34 |

Note: This table shows some summary statistics from the BGT vacancy data for 2013–2016 inclusive. 'No. of years employer present' refers to the number of years in which a given employer posted a vacancy, with a maximum of 4. The vacancy-weighted version of this statistic weights each observation by the number of vacancies an employer posted. 'Occ. (or metro area) share relative to BLS OES' refers to the share of each SOC 6-digit occupation (/metro area) in our vacancy data, relative to the share of that SOC 6-digit occupation (metro area) in the BLS OES data for the entire country (calculated for each year 2013-2016 then averaged across the four years). The employment-weighted version of this statistic weights each occupation-metro area cell by employment in that cell in 2016.

**Table A.2:** Distribution of number of jobs on resume and duration of jobs in BGT resume data set.

| *Percentile* | 10th | 25th | 50th | 75th | 90th |
|---|---|---|---|---|---|
| *# Jobs on resume* | 2 | 3 | 4 | 6 | 9 |
| *Job duration (months)* | 4 | 12 | 24 | 48 | 98 |

Note: This table shows some summary statistics from the BGT resume data: the distribution of the number of jobs in each resume (across all 16 million resumes in our data set), and the distribution of average job duration in months (across all the jobs reported in our data set).

**Table A.3:** Adjusted R-squared from regressions of occupational transitions on occupational similarity (based on tasks, skills, amenities, or wages)

| *Dependent variable:* | $\pi_{o \to p}$ | |
|---|---|---|
| *Included characteristic* | No FE | Incl. origin SOC FE |
| Skill distance | 0.011 | 0.025 |
| Wages | 0.003 | 0.021 |
| Job amenities | 0.021 | 0.039 |
| Leadership | 0.017 | 0.033 |
| Skill composites | 0.035 | 0.058 |

Note: Table shows adjusted R-squared from regressions of the form $\pi_{o \to p} = \varkappa + \alpha_o + \beta \Delta X_{occ\ p-o} + \varepsilon_{op}$. Here, $\pi_{o \to p}$ is the share of job changers in the origin occupation $o$ that move into target occupation $p$, and $\alpha_o$ are origin occupation fixed effects (included only in the second column). All regressions contain a constant. The variable $\Delta X_{occ\ p-o}$ represents the group of included characteristic differences noted in the table, which are included in relative target-minus-origin form and as absolute distances, with the exception of skill distance. All regressions are weighted by the average 2002-2015 national employment in the origin SOC. Note that the underlying occupational transition matrix is sparse, with many cells that show zero transitions, which is why the linear regression fit yields a relatively small R-squared. These analyses are discussed in more detail in Appendix Section A.6.

**Table A.4:** Twenty large occupations with lowest leave shares and highest leave shares

| Initial occupation | Leave share | Employment (2017) | Obs. (BGT) | Modal new occupation |
|---|---|---|---|---|
| Dental hygienists | .062 | 211,600 | 17,458 | Dental assistants |
| Nurse practitioners | .088 | 166,280 | 57,830 | Registered nurses |
| Pharmacists | .09 | 309,330 | 121,887 | Medical and health services managers |
| Firefighters | .098 | 319,860 | 60,039 | Emergency medical technicians and paramedics |
| Self-enrichment education teachers | .1 | 238,710 | 169,369 | Teachers and instructors, all other |
| Physical therapists | .11 | 225,420 | 44,314 | Medical and health services managers |
| Postsecondary teachers, all other | .11 | 189,270 | 825,879 | Managers, all other |
| Graphic designers | .12 | 217,170 | 439,953 | Art directors |
| Emergency medical technicians and paramedics | .12 | 251,860 | 111,180 | Managers, all other |
| Fitness trainers and aerobics instructors | .13 | 280,080 | 281,903 | Managers, all other |
| Licensed practical and licensed vocational nurses | .13 | 702,700 | 254,787 | Registered nurses |
| Lawyers | .13 | 628,370 | 667,960 | General and operations managers |
| Registered nurses | .13 | 2,906,840 | 1,427,102 | Medical and health services managers |
| Health specialties teachers, postsecondary | .13 | 194,610 | 41,963 | Medical and health services managers |
| Physicians and surgeons, all other | .14 | 355,460 | 59,630 | Medical and health services managers |
| Heavy and tractor-trailer truck drivers | .14 | 1,748,140 | 2,174,486 | Managers, all other |
| Radiologic technologists | .14 | 201,200 | 80,347 | Magnetic resonance imaging technologists |
| Hairdressers, hairstylists, and cosmetologists | .14 | 351,910 | 107,167 | Managers, all other |
| Coaches and scouts | .14 | 235,400 | 533,082 | Managers, all other |
| Chief executives | .15 | 210,160 | 1,425,400 | General and operations managers |
| ... | | | | |
| Installation, maintenance, and repair workers, all other | .29 | 153,850 | 60,742 | Maintenance and repair workers, general |
| Parts salespersons | .29 | 252,770 | 34,038 | First-line supervisors of retail sales workers |
| Billing and posting clerks | .29 | 476,010 | 274,963 | Bookkeeping, accounting, and auditing clerks |
| Data entry keyers | .29 | 180,100 | 288,523 | Customer service representatives |
| Cashiers | .29 | 3,564,920 | 1,753,947 | Customer service representatives |
| Insurance claims and policy processing clerks | .3 | 277,130 | 235,763 | Claims adjusters, examiners, and investigators |
| Stock clerks and order fillers | .3 | 2,046,040 | 597,137 | Laborers and freight, stock, and material movers, hand |
| Packers and packagers, hand | .3 | 700,560 | 101,025 | Laborers and freight, stock, and material movers, hand |
| Cooks, institution and cafeteria | .3 | 404,120 | 5,174 | Cooks, restaurant |
| Helpers–production workers | .31 | 402,140 | 112,759 | Production workers, all other |
| Sales rep., wholesale & mfg., tech. & scient. products | .31 | 327,190 | 198,337 | Sales rep., wholesale & mfg., exc. techn. & scient. products |
| Hosts and hostesses, restaurant, lounge, and coffee shop | .31 | 414,540 | 159,098 | Waiters and waitresses |
| Shipping, receiving, and traffic clerks | .31 | 671,780 | 318,080 | Laborers and freight, stock, and material movers, hand |
| Loan interviewers and clerks | .32 | 227,430 | 234,933 | Loan officers |
| Counter attendants, cafeteria, food concession, and coffee shop | .32 | 476,940 | 118,131 | Retail salespersons |
| Bill and account collectors | .32 | 271,700 | 310,951 | Customer service representatives |
| Tellers | .32 | 491,150 | 468,829 | Customer service representatives |
| Machine setters, operators, and tenders† | .32 | 154,860 | 6,805 | Production workers, all other |
| Telemarketers | .36 | 189,670 | 47,409 | Customer service representatives |
| Food servers, nonrestaurant | .45 | 264,630 | 13,199 | Waiters and waitresses |

Note: This table shows the twenty large occupations with the lowest and the highest occupation leave shares - defined as share of workers observed in one occupation in one year but not in the following year, divided by the share that leave their job over that period (see Section 1.4) - in the BGT data over 2002-2015, as well as total national employment in that occupation in 2017 from the OES, the number of occupation-year observations in the BGT data ('obs.') and the most popular occupation that workers who leave the initial occupation move to ('modal new occupation'). Large occupations are defined as those with national employment over 150,000 in 2017 (roughly the 75th percentile of occupations when ranked by nationwide employment). † Full occupation title is "Molding, coremaking, and casting machine setters, operators, and tenders, metal and plastic."

**Table A.5:** Forty thickest occupational transition paths for large occupations

| Initial occupation | New occupation | Transition share | Employment (2017) | Obs. (BGT data) |
|---|---|---|---|---|
| Licensed practical and licensed vocational nurses | Registered nurses | .3 | 702,700 | 254,787 |
| Nurse practitioners | Registered nurses | .23 | 166,280 | 57,830 |
| Construction managers | Managers, all other | .19 | 263,480 | 917,349 |
| Sales rep., wholesale & mfg., tech. & scient. products | Sales rep., wholesale & mfg., exc. tech. & scient. products | .19 | 327,190 | 198,337 |
| Physicians and surgeons, all other | Medical and health services managers | .19 | 355,460 | 59,630 |
| Software developers, systems software | Software developers, applications | .19 | 394,590 | 53,322 |
| Legal secretaries | Paralegals and legal assistants | .18 | 185,870 | 132,543 |
| Accountants and auditors | Financial managers | .18 | 1,241,000 | 1,459,175 |
| Registered nurses | Medical and health services managers | .16 | 2,906,840 | 1,427,102 |
| Cost estimators | Managers, all other | .16 | 210,900 | 124,646 |
| Human resources specialists | Human resources managers | .16 | 553,950 | 2,035,604 |
| Physical therapists | Medical and health services managers | .16 | 225,420 | 44,314 |
| Architectural and engineering managers | Managers, all other | .15 | 179,990 | 749,670 |
| Computer programmers | Software developers, applications | .15 | 247,690 | 533,764 |
| Software developers, applications | Computer occupations, all other | .15 | 849,230 | 2,110,229 |
| Computer network architects | Computer occupations, all other | .15 | 157,830 | 407,591 |
| Cooks, short order | Cooks, restaurant | .15 | 174,230 | 39,906 |
| Cooks, institution and cafeteria | Cooks, restaurant | .14 | 404,120 | 5,174 |
| First-line supervisors of construction trades and extraction workers | Construction managers | .14 | 556,300 | 186,747 |
| Computer systems analysts | Computer occupations, all other | .14 | 581,960 | 1,152,614 |
| Sales rep., wholesale & mfg., exc. tech. & scient. products | Sales managers | .13 | 1,391,400 | 4,377,654 |
| Light truck or delivery services drivers | Heavy and tractor-trailer truck drivers | .13 | 877,670 | 226,349 |
| Computer occupations, all other | Managers, all other | .13 | 315,830 | 3,515,188 |
| Health specialties teachers, postsecondary | Medical and health services managers | .13 | 194,610 | 41,963 |
| Meat, poultry, and fish cutters and trimmers | Heavy and tractor-trailer truck drivers | .13 | 153,280 | 2,383 |
| Sales rep., wholesale & mfg., tech. & scient. products | Sales managers | .13 | 327,190 | 198,337 |
| Operating engineers and other construction equipment operators | Heavy and tractor-trailer truck drivers | .13 | 365,300 | 55,317 |
| Sales managers | Sales rep., wholesale & mfg., exc. tech. & scient. products | .13 | 371,410 | 3,471,904 |
| Health specialties teachers, postsecondary | Registered nurses | .13 | 194,610 | 41,963 |
| Industrial engineers | Engineers, all other | .13 | 265,520 | 171,358 |
| Network and computer systems administrators | Computer occupations, all other | .13 | 375,040 | 1,103,700 |
| Industrial production managers | Managers, all other | .12 | 171,520 | 750,609 |
| Computer network support specialists | Computer user support specialists | .12 | 186,230 | 237,766 |
| Software developers, systems software | Computer occupations, all other | .12 | 394,590 | 53,322 |
| Financial analysts | Financial managers | .12 | 294,110 | 664,903 |
| Legal secretaries | Secretaries and admin. assistants, except legal, medical, & exec. | .12 | 185,870 | 132,543 |
| Mechanical engineers | Architectural and engineering managers | .12 | 291,290 | 408,178 |
| Food batchmakers | Industrial production managers | .12 | 151,950 | 12,729 |
| Licensed practical and licensed vocational nurses | Medical and health services managers | .11 | 702,700 | 254,787 |
| Food batchmakers | Heavy and tractor-trailer truck drivers | .11 | 151,950 | 12,729 |

Note: This table shows the 'thickest' occupational transition paths from large occupations (defined as those with national employment greater than 150,000 in 2017). The transition share from occupation o to occupation p is defined as the share of all occupation leavers from the initial occupation o who move into that particular new occupation p (as in Section 1.4). Only occupations with at least 500 observations in the BGT data and 2017 OES employment data are shown.

**Table A.6:** Assignment of SOC 2-digit occupation groups into 10 larger occupation categories

| Occupation category | 2-digit SOC occupation group | SOC code |
|---|---|---|
| Managers | Management occupations | 11-0000 |
| Managers | Business and financial operations occupations | 13-0000 |
| Professionals, except healthcare | Computer and mathematical operations | 15-0000 |
| Professionals, except healthcare | Architecture and engineering occupations | 17-0000 |
| Professionals, except healthcare | Life, physical, and social science occupations | 19-0000 |
| Professionals, except healthcare | Community and social service occupations | 21-0000 |
| Professionals, except healthcare | Legal occupations | 23-0000 |
| Professionals, except healthcare | Education, training, and library occupations | 25-0000 |
| Professionals, except healthcare | Arts, design, entertainment, sports, and media occupations | 27-0000 |
| Healthcare occupations | Healthcare practitioners and technical support occupations | 29-0000 |
| Healthcare occupations | Healthcare support occupations | 31-0000 |
| Food, Cleaning, and Protective Service | Protective service occupations | 33-0000 |
| Food, Cleaning, and Protective Service | Food preparation and serving related occupations | 35-0000 |
| Food, Cleaning, and Protective Service | Building and grounds cleaning and maintenance occupations | 37-0000 |
| Personal Care | Personal care and service occupations | 39-0000 |
| Sales | Sales and related occupations | 41-0000 |
| Office/Administrative | Office and administrative support occupations | 43-0000 |
| Operators / Laborers | Farming, fishing, and forestry occupations | 45-0000 |
| Operators / Laborers | Transportation and material moving occupations | 53-0000 |
| Production | Construction and extraction occupations | 47-0000 |
| Production | Installation, maintenance, and repair occupations | 49-0000 |
| Production | Production occupations | 51-0000 |

Note: Table shows our allocation of 2-digit SOC occupational groups into 9 larger occupation categories for the purpose of analyzing whether the relationship between wages and HHI differs across occupation groups. These occupation categories are drawn primarily from Acemoglu and Autor (2011).

**Table A.7:** First-stage regressions: HHI instrument

| *Dependent variable: log vacancy HHI (segmented by quartile of occ mobility in cols (b)-(e))* | | | | | |
|---|---|---|---|---|---|
| | Full sample | By quartile of occ mobility | | | |
| | | Q1 | Q2 | Q3 | Q4 |
| | (a) | (b) | (c) | (d) | (e) |
| Log vacancy HHI instrument | 0.081 | | | | |
| | (0.003) | | | | |
| Log outside-occ. options instrument | -0.712 | | | | |
| | (0.044) | | | | |
| Log HHI instrument X Q1 occ mobility | | 0.078 | | | |
| | | (0.004) | | | |
| Log outside-occ options instrument X Q1 occ mobility | | -0.566 | | | |
| | | (0.049) | | | |
| Log HHI instrument X Q2 occ mobility | | | 0.084 | | |
| | | | (0.003) | | |
| Log outside-occ options instrument X Q2 occ mobility | | | -0.785 | | |
| | | | (0.056) | | |
| Log HHI instrument X Q3 occ mobility | | | | 0.074 | |
| | | | | (0.003) | |
| Log outside-occ options instrument X Q3 occ mobility | | | | -0.742 | |
| | | | | (0.052) | |
| Log HHI instrument X Q4 occ mobility | | | | | 0.085 |
| | | | | | (0.004) |
| Log outside-occ options instrument X Q4 occ mobility | | | | | -0.818 |
| | | | | | (0.050) |
| Vacancy growth | -0.061 | -0.461 | -0.023 | -0.097 | -0.588 |
| | (0.044) | (0.163) | (0.012) | (0.062) | (0.064) |
| Predicted vacancy growth | -0.038 | -0.473 | -0.023 | -0.099 | 0.085 |
| | (0.100) | (0.373) | (0.219) | (0.237) | (0.083) |
| Exposure control | 1.424 | 1.578 | 1.335 | 1.464 | 1.432 |
| | (0.037) | (0.054) | (0.059) | (0.059) | (0.049) |
| Observations | 184,411 | 46,300 | 46,189 | 46,080 | 45,840 |

Note: In column (a) we run a first-stage regression for our HHI instrument. In columns (b) through (e) we run separate first-stage regressions for our HHI instrument, segmenting our data into four quartiles by outward occupational mobility (the occupation "leave share" as defined in Section 1.4). Heteroskedasticity-robust standard errors clustered at the city level shown in parentheses. Units of observation are 6 digit SOC by city by year, for all observations with available data over 2013–2016 inclusive. All regressions have occupation-year and CBSA-year fixed effects.

**Table A.8:** First-stage regressions: outside-occ. options instrument

| *Dependent variable: log outside-occ. options (segmented by quartile of occ mobility in cols (b)-(e))* | Full sample | By quartile of occ mobility | | | |
|---|---|---|---|---|---|
| | | Q1 | Q2 | Q3 | Q4 |
| | (a) | (b) | (c) | (d) | (e) |
| Log outside-occ. options instrument | 0.783 | | | | |
| | (0.018) | | | | |
| Log HHI instrument | -0.000 | | | | |
| | (0.000) | | | | |
| Log outside-occ options instrument X Q1 outward mobility | | 0.689 | | | |
| | | (0.018) | | | |
| Log HHI instrument X Q1 outward mobility | | -0.001 | | | |
| | | (0.000) | | | |
| Log outside-occ options instrument X Q2 outward mobility | | | 0.819 | | |
| | | | (0.019) | | |
| Log HHI instrument X Q2 outward mobility | | | 0.000 | | |
| | | | (0.000) | | |
| Log outside-occ options instrument X Q3 outward mobility | | | | 0.816 | |
| | | | | (0.021) | |
| Log HHI instrument X Q3 outward mobility | | | | -0.000 | |
| | | | | (0.000) | |
| Log outside-occ options instrument X Q4 outward mobility | | | | | 0.810 |
| | | | | | (0.024) |
| Log HHI instrument X Q4 outward mobility | | | | | -0.000 |
| | | | | | (0.000) |
| Vacancy growth | 0.001 | -0.007 | 0.003 | -0.004 | 0.004 |
| | (0.001) | (0.005) | (0.000) | (0.001) | (0.005) |
| Predicted vacancy growth | 0.018 | -0.010 | -0.021 | 0.036 | 0.034 |
| | (0.017) | (0.041) | (0.023) | (0.032) | (0.022) |
| Exposure control | 0.001 | 0.010 | 0.008 | -0.006 | -0.004 |
| | (0.005) | (0.010) | (0.008) | (0.009) | (0.005) |
| Observations | 184,411 | 46,300 | 46,189 | 46,080 | 45,840 |

Note: In column (a) we run a first-stage regression for our outside-occupation option index instrument. In columns (b) through (e) we run separate first-stage regressions for our outside-occupation option index instrument, segmenting our data into four quartiles by outward occupational mobility (the occupation "leave share" as defined in Section 1.4). Heteroskedasticity-robust standard errors clustered at the city level shown in parentheses. Units of observation are 6 digit SOC by city by year, for all observations with available data over 2013–2016 inclusive. All regressions have occupation-year and CBSA-year fixed effects.

**Table A.9:** Regression of wage on HHI and outside-occupation options: robustness

| *Dependent variable:* | Log wage | | | | |
|---|---|---|---|---|---|
| | (a)<br>No vac<br>controls | (b)<br>Equal-wt<br>vac | (c)<br>No exposure<br>control | (d)<br>Industry<br>Bartik | (e)<br>Occ-CBSA<br>FEs |
| Log HHI,<br>instrumented | -0.011<br>(0.003) | -0.011<br>(0.003) | -0.011<br>(0.002) | -0.010<br>(0.003) | -0.008<br>(0.003) |
| Log outside-occ. options,<br>instrumented | 0.095<br>(0.009) | 0.095<br>(0.009) | 0.095<br>(0.009) | 0.085<br>(0.009) | -0.002<br>(0.007) |
| Exposure control | 0.004<br>(0.007) | 0.004<br>(0.007) | | 0.003<br>(0.008) | -0.001<br>(0.004) |
| Vacancy growth | | -0.001<br>(0.001) | -0.001<br>(0.001) | -0.007<br>(0.004) | -0.003<br>(0.003) |
| Predicted vacancy growth | | -0.013<br>(0.010) | -0.012<br>(0.010) | 0.033<br>(0.030) | -0.008<br>(0.008) |
| Equal-weighted vacancy<br>growth | | -0.000<br>(0.000) | | | |
| Industry Bartik | | | | 0.146<br>(0.014) | |
| Observations | 184,411 | 184,411 | 184,411 | 169,341 | 158,393 |
| F-Stat | 386 | 373 | 491 | 620 | 221 |
| Fixed effects | Occ-year<br>CBSA-year | Occ-year<br>CBSA-year | Occ-year<br>CBSA-year | Occ-year<br>CBSA-year | Occ-CBSA<br>Year |

Note: These represent robustness checks for our baseline regression specification (reported in column (d) of Table 1.3). Each column is a different robustness check. All columns are 2SLS IV regressions estimated using our HHI and outside-occupation option instruments. Column (a) excludes our controls for local actual and predicted vacancy growth. Column (b) includes an additional control for equal-weighted vacancy growth of local firms in the relevant occupation. Column (c) excludes our HHI exposure control. Column (d) includes an industry Bartik shock to control for correlated industry shocks across occupation-metro area cells. All these specifications feature occupation-year and city-year fixed effects. Column (e) runs our baseline regression specification, but with occupation-city and year fixed effects. Other regression info: Heteroskedasticity-robust standard errors clustered at the city level shown in parentheses. Units of observation are 6 digit SOC by city by year, for all observations with available data over 2013–2016 inclusive. F-Stat is Kleibergen-Paap Wald F statistic.

**Table A.10:** Regression of wage on HHI and outside-occupation options: robustness (2)

| *Dependent variable:* | Log wage | | | | |
|---|---|---|---|---|---|
| | (a) Emp weight | (b) Log emp weight | (c) Drop low rep. occs | (d) Occ rep. weight | (e) CBSA rep. weight |
| Log HHI | -0.015 | -0.011 | -0.011 | -0.008 | -0.009 |
| | (0.005) | (0.003) | (0.003) | (0.004) | (0.003) |
| Log outside-occ. options | 0.118 | 0.103 | 0.093 | 0.108 | 0.094 |
| | (0.027) | (0.009) | (0.009) | (0.011) | (0.009) |
| Vacancy growth | -0.000 | -0.001 | -0.001 | -0.001 | -0.001 |
| | (0.001) | (0.001) | (0.000) | (0.001) | (0.000) |
| Predicted vacancy growth | -0.018 | -0.012 | -0.018 | 0.020 | -0.020 |
| | (0.007) | (0.008) | (0.011) | (0.032) | (0.009) |
| Exposure control | 0.003 | 0.007 | 0.008 | 0.018 | 0.003 |
| | (0.017) | (0.008) | (0.009) | (0.013) | (0.008) |
| Observations | 184,411 | 184,258 | 137,567 | 184,411 | 184,411 |
| F-Stat | 96 | 370 | 411 | 369 | 221 |

Notes: These represent robustness checks for our baseline regression specification (reported in column (d) of Table 1.3). Each column is a different robustness check. All columns are 2SLS IV regressions estimated using our HHI and outside-occupation option instruments. Columns (a) and (b) weight the regressions by employment and log employment of the occupation-MSA cell, respectively, with employment weights calculated as the average employment in that occupation-by-MSA labor market over 2013–2016 according to BLS OES. Column (c) drops all occupations with average represented-ness in the BGT vacancy data of 0.5 or less. Columns (d) and (e) weight the regressions by average represented-ness of the occupations, and MSAs, in the BGT vacancy data (respectively). Represented-ness by occupation (/CBSA) in the BGT vacancy data is calculated as the share of all vacancies accounted for by a given occupation (/CBSA) in the BGT vacancy data in a given year, divided by the share of employment accounted for by a given occupation (CBSA) in the BLS OES in that same year, averaged over 2013–2016. About one third of occupations in our data have occupation represented-ness of 0.5 or less in the BGT data. Other regression info: Heteroskedasticity-robust standard errors clustered at the city level shown in parentheses. Units of observation are 6 digit SOC by city by year, for all observations with available data over 2013–2016 inclusive. All specifications feature occupation-year and city-year fixed effects. F-Stat is Kleibergen-Paap Wald F statistic.

**Table A.11:** Regression of wage on HHI and outside-occupation options, by quartile of outward mobility: robustness checks

| *Dependent variable:* | Log wage | | | | |
|---|---|---|---|---|---|
| | (a) No vac controls | (b) Equal-wt vac | (c) No exposure control | (d) Industry Bartik | (e) Occ-CBSA FEs |
| Log HHI X Q1 outward mobility | -0.026 | -0.025 | -0.025 | -0.027 | -0.004 |
| | (0.005) | (0.005) | (0.004) | (0.004) | (0.007) |
| Log HHI X Q2 outward mobility | -0.011 | -0.010 | -0.010 | -0.009 | 0.002 |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.010) |
| Log HHI X Q3 outward mobility | -0.007 | -0.007 | -0.007 | -0.004 | -0.022 |
| | (0.004) | (0.004) | (0.003) | (0.004) | (0.010) |
| Log HHI X Q4 outward mobility | -0.000 | 0.000 | 0.000 | 0.000 | -0.013 |
| | (0.004) | (0.004) | (0.003) | (0.004) | (0.015) |
| Log outside-occ. options X Q1 outward mobility | 0.060 | 0.060 | 0.060 | 0.051 | 0.003 |
| | (0.011) | (0.011) | (0.011) | (0.012) | (0.012) |
| Log outside-occ. options X Q2 outward mobility | 0.100 | 0.100 | 0.100 | 0.090 | -0.020 |
| | (0.009) | (0.009) | (0.009) | (0.010) | (0.017) |
| Log outside-occ. options X Q3 outward mobility | 0.106 | 0.107 | 0.107 | 0.100 | 0.004 |
| | (0.010) | (0.010) | (0.010) | (0.010) | (0.016) |
| Log outside-occ. options X Q4 outward mobility | 0.108 | 0.108 | 0.109 | 0.094 | 0.013 |
| | (0.010) | (0.010) | (0.010) | (0.011) | (0.024) |
| Exposure control | 0.004 | 0.004 | | 0.003 | -0.002 |
| | (0.008) | (0.008) | | (0.008) | (0.004) |
| Vacancy growth | | -0.001 | -0.001 | -0.007 | -0.003 |
| | | (0.001) | (0.001) | (0.004) | (0.003) |
| Predicted vacancy growth | | -0.015 | -0.015 | 0.028 | -0.009 |
| | | (0.010) | (0.010) | (0.030) | (0.009) |
| Equal-weighted vacancy growth | | -0.000 | | | |
| | | (0.000) | | | |
| Industry Bartik | | | | 0.143 | |
| | | | | (0.014) | |
| Observations | 184,411 | 184,411 | 184,411 | 169,341 | 158,393 |
| Fixed effects | Occ-year CBSA-year | Occ-year CBSA-year | Occ-year CBSA-year | Occ-year CBSA-year | Occ-CBSA Year |

Note: This table repeats the robustness checks in Table A.9, but allowing the coefficient on the HHI and outside-occupation option index to vary by quartile of outward occupational mobility (the occupation "leave share" defined as in Section 1.4). Heteroskedasticity-robust standard errors clustered at the city level shown in parentheses. Units of observation are 6 digit SOC by city by year, for all observations with available data over 2013–2016 inclusive.

**Table A.12:** Regression of wage on HHI and outside-occupation options, by quartile of outward mobility: robustness checks (2)

| *Dependent variable:* | Log wage | | | | |
|---|---|---|---|---|---|
| | (a) Emp weight | (b) Log emp weight | (c) Drop low rep. occs | (d) Occ rep. weight | (e) CBSA rep. weight |
| Log HHI X Q1 outward mobility | -0.044 | -0.027 | -0.023 | -0.011 | -0.020 |
| | (0.011) | (0.004) | (0.005) | (0.006) | (0.006) |
| Log HHI X Q2 outward mobility | -0.019 | -0.012 | -0.007 | -0.011 | -0.009 |
| | (0.005) | (0.003) | (0.003) | (0.006) | (0.004) |
| Log HHI X Q3 outward mobility | -0.001 | -0.007 | -0.006 | -0.004 | -0.008 |
| | (0.005) | (0.004) | (0.004) | (0.006) | (0.004) |
| Log HHI X Q4 outward mobility | -0.005 | 0.001 | -0.005 | 0.002 | 0.001 |
| | (0.008) | (0.004) | (0.005) | (0.006) | (0.004) |
| Log outside-occ. options X Q1 outward mobility | 0.093 | 0.067 | 0.059 | 0.067 | 0.069 |
| | (0.025) | (0.012) | (0.011) | (0.013) | (0.012) |
| Log outside-occ. options X Q2 outward mobility | 0.135 | 0.108 | 0.105 | 0.118 | 0.098 |
| | (0.024) | (0.010) | (0.009) | (0.013) | (0.010) |
| Log outside-occ. options X Q3 outward mobility | 0.125 | 0.115 | 0.105 | 0.116 | 0.099 |
| | (0.032) | (0.010) | (0.010) | (0.012) | (0.010) |
| Log outside-occ. options X Q4 outward mobility | 0.101 | 0.117 | 0.102 | 0.116 | 0.105 |
| | (0.043) | (0.010) | (0.011) | (0.014) | (0.009) |
| Vacancy growth | 0.000 | -0.001 | -0.001 | -0.001 | -0.001 |
| | (0.001) | (0.001) | (0.000) | (0.001) | (0.000) |
| Predicted vacancy growth | -0.020 | -0.015 | -0.020 | 0.016 | -0.023 |
| | (0.008) | (0.008) | (0.011) | (0.031) | (0.009) |
| Exposure control | 0.025 | 0.006 | 0.009 | 0.014 | 0.002 |
| | (0.021) | (0.008) | (0.009) | (0.013) | (0.009) |
| Observations | 184,411 | 184,258 | 137,567 | 184,411 | 184,411 |

Notes: This table repeats the robustness checks in Table A.10, but allowing the coefficient on the HHI and outside-occupation option index to vary by quartile of outward occupational mobility (the occupation "leave share" defined as in Section 1.4). Heteroskedasticity-robust standard errors clustered at the city level shown in parentheses. Units of observation are 6 digit SOC by city by year, for all observations with available data over 2013–2016 inclusive. All specifications feature occupation-year and city-year fixed effects.

**Table A.13:** Regression of wage on HHI and outside options: heterogeneity by quartile of HHI

| *Specification:* | OLS | 2SLS IV | First stage for HHI | | | |
|---|---|---|---|---|---|---|
| *Dependent variable:* | Log wage | | Log HHI (by quartile of HHI) | | | |
| | | | Q1 | Q2 | Q3 | Q4 |
| Log HHI (instrument) | -0.008 | -0.015 | 0.048 | | | |
| X Q1 HHI | (0.002) | (0.005) | (0.003) | | | |
| Log outside-occ. options (instrument) | 0.114 | 0.102 | -0.608 | | | |
| X Q1 HHI | (0.008) | (0.010) | (0.057) | | | |
| Log HHI (instrument) | -0.004 | -0.010 | | 0.021 | | |
| X Q2 HHI | (0.001) | (0.006) | | (0.001) | | |
| Log outside-occ. options (instrument) | 0.099 | 0.087 | | -0.137 | | |
| X Q2 HHI | (0.007) | (0.009) | | (0.016) | | |
| Log HHI (instrument) | -0.004 | -0.010 | | | 0.024 | |
| X Q3 HHI | (0.001) | (0.005) | | | (0.002) | |
| Log outside-occ. options (instrument) | 0.099 | 0.088 | | | -0.082 | |
| X Q3 HHI | (0.007) | (0.008) | | | (0.014) | |
| Log HHI (instrument) | -0.005 | -0.011 | | | | 0.031 |
| X Q4 HHI | (0.001) | (0.005) | | | | (0.003) |
| Log outside-occ. options (instrument) | 0.104 | 0.091 | | | | -0.177 |
| X Q4 HHI | (0.007) | (0.009) | | | | (0.021) |
| Vacancy growth | | -0.001 | -0.891 | -0.013 | -0.551 | -0.086 |
| | | (0.001) | (0.190) | (0.008) | (0.050) | (0.056) |
| Predicted vacancy growth | | -0.013 | 0.455 | -0.176 | -0.086 | 0.213 |
| | | (0.010) | (0.698) | (0.217) | (0.106) | (0.104) |
| Exposure control | | -0.000 | 10.547 | 1.725 | 0.829 | 0.785 |
| | | (0.007) | (0.838) | (0.096) | (0.039) | (0.022) |
| Observations | 184,411 | 184,411 | 45,827 | 45,884 | 45,916 | 45,897 |

Notes: Column (a) repeats our baseline OLS regression and Column (b) repeats our baseline 2SLS IV regression (corresponding to Table 1.3 columns (b) and (d) respectively), but allow the coefficients on the HHI and outside-occupation option index to vary based on the average HHI of the occupation-city cell in question. Specifically, occupation-city labor markets are split into four quartiles based on their average HHI over the four year period in question 2013–2016. Quartile boundaries for the average HHI in the occupation-city labor market over 2013–2016 are 276 (p25), 622 (p50), and 1,340 (p75). The remaining columns (c) through (f) show the first stage HHI regressions for the HHI instrument, separately for each HHI quartile. Other info: Heteroskedasticity-robust standard errors clustered at the city level shown in parentheses. Units of observation are 6 digit SOC by city by year, for all observations with available data over 2013–2016 inclusive. Regressions have occupation-year and city-year fixed effects.

**Table A.14:** Regression of wage on HHI and outside options: heterogeneity by occupational wage quartile

| *Specification:* | OLS | 2SLS IV | First stage for HHI | | | |
|---|---|---|---|---|---|---|
| *Dependent variable:* | Log wage | | Log HHI (by occ wage quartile) | | | |
| | | | Q1 | Q2 | Q3 | Q4 |
| Log HHI (instrument) | -0.003 | -0.009 | 0.082 | | | |
| X Q1 occ. wage | (0.002) | (0.004) | (0.004) | | | |
| Log outside-occ. options (instrument) | 0.081 | 0.047 | -0.357 | | | |
| X Q1 occ. wage | (0.010) | (0.015) | (0.068) | | | |
| Log HHI (instrument) | -0.002 | -0.004 | | 0.097 | | |
| X Q2 occ. wage | (0.001) | (0.003) | | (0.004) | | |
| Log outside-occ. options (instrument) | 0.109 | 0.091 | | -0.459 | | |
| X Q2 occ. wage | (0.008) | (0.012) | | (0.048) | | |
| Log HHI (instrument) | -0.004 | -0.011 | | | 0.074 | |
| X Q3 occ. wage | (0.001) | (0.004) | | | (0.003) | |
| Log outside-occ. options (instrument) | 0.113 | 0.083 | | | -0.647 | |
| X Q3 occ. wage | (0.007) | (0.010) | | | (0.041) | |
| Log HHI (instrument) | -0.012 | -0.014 | | | | 0.071 |
| X Q4 occ. wage | (0.002) | (0.004) | | | | (0.003) |
| Log outside-occ. options (instrument) | 0.095 | 0.089 | | | | -0.648 |
| X Q4 occ. wage | (0.008) | (0.010) | | | | (0.048) |
| Vacancy growth | | -0.001 | -0.350 | -0.560 | -0.032 | -0.511 |
| | | (0.001) | (0.077) | (0.063) | (0.018) | (0.196) |
| Predicted vacancy growth | | -0.013 | 0.175 | 0.124 | -0.392 | -0.258 |
| | | (0.010) | (0.054) | (0.125) | (0.208) | (0.233) |
| Exposure control | | 0.000 | 1.588 | 1.515 | 1.158 | 1.768 |
| | | (0.007) | (0.070) | (0.045) | (0.046) | (0.069) |
| Observations | 184,411 | 184,411 | 19,428 | 40,181 | 64,770 | 60,031 |

Notes: Column (a) repeats our baseline OLS regression and column (b) repeats our baseline 2SLS IV regression (Table 1.3 columns (b) and (d) respectively), but allow the coefficients on the HHI and outside-occupation option index to vary based on the average wage of the occupation in question. Specifically, each occupation's average wage is calculated in our BLS OES data sample as of 2016, and occupations are split by employment-weighted quartiles of this distribution (such that all observations from one occupation across cities and years are always in the same quartile). The cutoff for the 25th percentile is $13.01, the median is $18.49 and the 75th percentile is $31.74. The remaining columns (c) through (f) show the first stage HHI regressions separately for each occupation wage quartile. Other info: Heteroskedasticity-robust standard errors clustered at the city level shown in parentheses. Units of observation are 6 digit SOC by city by year, for all observations with available data over 2013–2016 inclusive. Regressions have occupation-year and city-year fixed effects.

**Table A.15:** Regression of wage on HHI and outside options: heterogeneity by occupation group

| | OLS | 2SLS IV | First stage (by occupation group) | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Log HHI (instrument) | -0.012 | -0.010 | 0.057 | | | | | | | | |
| X Managers | (0.003) | (0.006) | (0.003) | | | | | | | | |
| Log outside-occ. options (instrument) | 0.098 | 0.107 | -0.642 | | | | | | | | |
| X Managers | (0.010) | (0.013) | (0.067) | | | | | | | | |
| Log HHI (instrument) | -0.012 | -0.014 | | 0.071 | | | | | | | |
| X Professionals, excl managers | (0.002) | (0.005) | | (0.004) | | | | | | | |
| Log outside-occ. options (instrument) | 0.087 | 0.077 | | -0.849 | | | | | | | |
| X Professionals, excl managers | (0.007) | (0.010) | | (0.063) | | | | | | | |
| Log HHI (instrument) | -0.007 | -0.015 | | | 0.058 | | | | | | |
| X Healthcare | (0.002) | (0.006) | | | (0.003) | | | | | | |
| Log outside-occ. options (instrument) | 0.024 | -0.008 | | | -0.207 | | | | | | |
| X Healthcare | (0.008) | (0.012) | | | (0.052) | | | | | | |
| Log HHI (instrument) | 0.006 | 0.001 | | | | 0.085 | | | | | |
| X Production | (0.003) | (0.004) | | | | (0.004) | | | | | |
| Log outside-occ. options (instrument) | 0.103 | 0.081 | | | | -0.817 | | | | | |
| X Production | (0.010) | (0.013) | | | | (0.058) | | | | | |
| Log HHI (instrument) | -0.013 | -0.015 | | | | | 0.074 | | | | |
| X Sales | (0.003) | (0.008) | | | | | (0.006) | | | | |
| Log outside-occ. options (instrument) | 0.119 | 0.128 | | | | | -1.069 | | | | |
| X Sales | (0.010) | (0.016) | | | | | (0.105) | | | | |
| Log HHI (instrument) | -0.002 | -0.004 | | | | | | 0.088 | | | |
| X Office/Admin | (0.001) | (0.003) | | | | | | (0.005) | | | |
| Log outside-occ. options (instrument) | 0.093 | 0.087 | | | | | | -0.730 | | | |
| X Office/Admin | (0.007) | (0.011) | | | | | | (0.085) | | | |
| Log HHI (instrument) | -0.003 | 0.000 | | | | | | | 0.101 | | |
| X Operators/Laborers | (0.002) | (0.005) | | | | | | | (0.007) | | |
| Log outside-occ. options (instrument) | 0.063 | 0.068 | | | | | | | -0.162 | | |
| X Operators/Laborers | (0.010) | (0.014) | | | | | | | (0.138) | | |
| Log HHI (instrument) | -0.009 | -0.027 | | | | | | | | 0.068 | |
| X Food/Cleaning/Protective Service | (0.002) | (0.007) | | | | | | | | (0.004) | |
| Log outside-occ. options (instrument) | 0.069 | 0.022 | | | | | | | | -0.009 | |
| X Food/Cleaning/Protective Service | (0.009) | (0.015) | | | | | | | | (0.065) | |
| Log HHI (instrument) | -0.004 | -0.016 | | | | | | | | | 0.091 |
| X Personal Care | (0.003) | (0.008) | | | | | | | | | (0.010) |
| Log outside-occ. options (instrument) | 0.074 | 0.054 | | | | | | | | | -0.564 |
| X Personal Care | (0.010) | (0.015) | | | | | | | | | (0.109) |
| Observations | 184,411 | 184,411 | 27,955 | 25,917 | 28,326 | 26,802 | 11,924 | 20,977 | 9,181 | 18,373 | 6,611 |

Notes: Column (a) repeats our baseline OLS regression and column (b) repeats our baseline 2SLS IV regression (Table 1.3 columns (b) and (d) respectively), but allow the coefficients on the HHI and outside-occupation option index to vary for different aggregated occupation groups. The remaining columns show the first stage HHI regressions separately for each occupation group. Occupation groups map from SOC 2-digit occupations as defined in Appendix Table A.6. Other info: Heteroskedasticity-robust standard errors clustered at the city level shown in parentheses. Units of observation are 6 digit SOC by city by year, for all observations with available data over 2013–2016 inclusive. Regressions have occupation-year and city-year fixed effects. Vacancy growth, predicted vacancy growth, and exposure controls are included but coefficients not presented for space reasons.

**Table A.16:** Regression of wage on outside-occupation options: 1999–2016

| *Dependent variable:* | Log wage | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Panel A: OLS regressions** | | | | |
| $oo^{occs}$ | 0.140 | 0.082 | 0.095 | 0.044 |
| | (0.010) | (0.004) | (0.005) | (0.006) |
| *Observations* | *1,944,370* | *1,944,370* | *1,944,370* | *1,944,370* |
| **Panel B: 2SLS IV regressions** | | | | |
| $oo^{occs}$, instrumented | 0.122 | 0.070 | 0.076 | 0.030 |
| | (0.010) | (0.005) | (0.006) | (0.006) |
| *Observations* | *1,944,370* | *1,944,230* | *1,944,370* | *1,944,114* |
| Fixed effects | Year | Occ-Year, City | Occ-Year, City-Year | Occ-Year, Occ-City |

Notes: This table repeats our baseline regressions with the outside-occupation option index only, over a longer period (1999-2016). Heteroskedasticity-robust standard errors clustered at the city level shown in parentheses. Units of observation are 6 digit SOC by city by year, for all observations with available data over 1999–2016 inclusive.

**Table A.17:** Regressions of wage on outside-occupation option index: aggregated occupation codes, with different combinations of fixed effects

| *Dependent variable:* | Log wage | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Panel A: Minor SOC Group (3-digit) regressions:** | | | | |
| OLS: $oo^{occs}$ | 0.184 | 0.091 | 0.106 | 0.071 |
| | (0.013) | (0.007) | (0.011) | (0.009) |
| IV: $oo^{occs}$, instrumented | 0.143 | 0.113 | 0.105 | 0.125 |
| | (0.018) | (0.012) | (0.015) | (0.014) |
| *Observations* | *486,487* | *486,481* | *486,487* | *485,815* |
| **Panel B: Major SOC Group (2-digit) regressions:** | | | | |
| OLS: $oo^{occs}$ | -0.063 | 0.081 | 0.002 | 0.080 |
| | (0.030) | (0.009) | (0.024) | (0.009) |
| IV: $oo^{occs}$, instrumented | -0.182 | 0.079 | 0.060 | 0.327 |
| | (0.038) | (0.028) | (0.029) | (0.113) |
| *Observations* | *137,650* | *137,650* | *137,650* | *137,609* |
| Fixed effects | Year | Occ-Year<br>City | City-Year<br>Occ-Year | Occ-Year<br>Occ-City |

Notes: This table reports 2SLS IV regressions of the wage on outside-occupation option index with outside options defined at the level of 3-digit or 2-digit occupations (rather than SOC 6-digit). Heteroskedasticity-robust standard errors clustered at the city level shown in parentheses. Units of observation are 2-digit or 3-digit SOC by city by year, for all observations with available data over 1999–2016 inclusive. As noted in the paper, 'cities' refers to CBSAs (metropolitan and micropolitan statistical areas) or NECTAs (New England city and town areas). Each cell reports the coefficient for the variable of interest in one specification, with included fixed effects held constant within each column.

**Table A.18:** Regressions of wage on outside-occupation option index, sample split into three periods, with different combinations of fixed effects

| *Dependent variable:* | Log wage | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Panel A: 1999–2006:** | | | | |
| OLS: $oo^{occs}$ | 0.132 | 0.071 | 0.085 | 0.029 |
| | (0.011) | (0.004) | (0.005) | (0.005) |
| IV: $oo^{occs}$, instrumented | 0.100 | 0.053 | 0.060 | 0.018 |
| | (0.011) | (0.005) | (0.006) | (0.004) |
| *Observations* | *788,519* | *788,463* | *788,519* | *772,025* |
| **Panel B: 2007–2011:** | | | | |
| OLS: $oo^{occs}$ | 0.141 | 0.091 | 0.097 | 0.035 |
| | (0.010) | (0.005) | (0.006) | (0.005) |
| IV: $oo^{occs}$, instrumented | 0.130 | 0.080 | 0.084 | 0.013 |
| | (0.010) | (0.006) | (0.007) | (0.005) |
| *Observations* | *579,283* | *579,242* | *579,283* | *565,394* |
| **Panel C: 2012–2016:** | | | | |
| OLS: $oo^{occs}$ | 0.149 | 0.096 | 0.105 | 0.026 |
| | (0.010) | (0.005) | (0.006) | (0.008) |
| IV: $oo^{occs}$, instrumented | 0.145 | 0.090 | 0.094 | 0.011 |
| | (0.012) | (0.007) | (0.007) | (0.009) |
| *Observations* | *576,568* | *576,525* | *576,568* | *562,592* |
| Fixed effects | Year | Occ-Year City | City-Year Occ-Year | Occ-Year Occ-City |

Notes: This table shows 2SLS IV regressions of wages on outside-occupation options, splitting our sample over three periods. Heteroskedasticity-robust standard errors clustered at the city level shown in parentheses. Units of observation are 6-digit SOC by city by year, for all observations with available data over 2002–2016 inclusive (split into three five-year periods). As noted in the paper, 'cities' refers to CBSAs (metropolitan and micropolitan statistical areas) or NECTAs (New England city and town areas). Each cell reports the coefficient for the variable of interest (outside-occupation option index) in one regression specification, with included fixed effects held constant within each column.

**Table A.19:** Regressions of wage on outside-occ. option index, employment-weighted, with different combinations of fixed effects

| *Dependent variable:* | Log wage | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Panel A: OLS** | | | | |
| $oo^{occs}$ | 0.382 | 0.094 | 0.132 | 0.027 |
| | (0.026) | (0.010) | (0.009) | (0.008) |
| **Panel B: 2SLS** | | | | |
| $oo^{occs}$, instrumented | 0.396 | 0.096 | 0.106 | 0.026 |
| | (0.025) | (0.013) | (0.015) | (0.010) |
| *First stage:* | | | | |
| Coeff. on $oo^{occs,inst}_{o,k,t}$ | 1.106 | 0.886 | 0.874 | 0.923 |
| | (0.052) | (0.020) | (0.020) | (0.081) |
| 1st-stage F-Stat. | 451 | 1929 | 1943 | 129 |
| Fixed effects | Year | Occ-Year<br>City | City-Year<br>Occ-Year | Occ-Year<br>Occ-City |
| *Observations* | *1,944,370* | *1,944,230* | *1,944,230* | *1,931,901* |

Note: This shows regressions of wages on outside-occupation options, with weighted by the average employment of their occupation-city over the sample period. Heteroskedasticity-robust standard errors clustered at the city level shown in parentheses. Units of observation are 6 digit SOC by city by year, for all observations with available data over 1999–2016 inclusive. The instrumented outside-occupation option index uses the national leave-one-out mean wage in outside option occupations to instrument for the local (city-level) wage, and the initial local employment share in outside option occupations to instrument for the current local employment share. Each cell reports the coefficient for the variable of interest (outside-occupation option index) in one regression specification, with included fixed effects held constant within each column.

**Table A.20:** Regressions of wages on outside-occupation options, incorporating employment share

| *Dependent var.:* | Employment share | Log wage | |
|---|---|---|---|
| $oo^{occs}$ , instrumented | -0.226 | 0.030 | 0.026 |
| | (0.027) | (0.006) | (0.006) |
| Empl. share | | | -0.015 |
| | | | (0.001) |
| Fixed effects | Occ-City, Occ-Year | Occ-City, Occ-Year | Occ-City, Occ-Year |
| *Observations* | *1,931,901* | *1,931,901* | *1,931,901* |

Notes: Heteroskedasticity-robust standard errors clustered at the city level shown in parentheses. Units of observation are 6 digit SOC by city by year, for all observations with available data over 1999–2016.

**Table A.21:** Regression of wage on HHI and $oo^{occs}$: Right-to-work and non right-to-work states

| *Dependent variable:* | Log wage | | | |
|---|---|---|---|---|
| | (a) OLS | (b) OLS | (c) 2SLS IV | (d) 2SLS IV |
| Log HHI X Non right-to-work | -0.007 | -0.004 | -0.011 | -0.008 |
| | (0.001) | (0.001) | (0.003) | (0.003) |
| Log HHI X right-to-work | -0.014 | -0.010 | -0.018 | -0.015 |
| | (0.001) | (0.001) | (0.003) | (0.003) |
| Log outside-occ options X Non right-to-work | | 0.099 | | 0.086 |
| | | (0.008) | | (0.009) |
| Log outside-occ options X right-to-work | | 0.114 | | 0.104 |
| | | (0.008) | | (0.009) |
| Vacancy growth | | | -0.001 | -0.001 |
| | | | (0.001) | (0.001) |
| Predicted vacancy growth | | | -0.012 | -0.013 |
| | | | (0.010) | (0.010) |
| Exposure control | | | 0.011 | 0.007 |
| | | | (0.008) | (0.007) |
| Observations | 184,411 | 184,411 | 184,411 | 184,411 |
| F-Stat | | | 377 | 201 |

Notes: This table shows our baseline regression specifications as in Table 1.3 but allowing the coefficients on the HHI and outside-occupation options to differ in right-to-work states and non right-to-work states. States are classified as right-to-work or non-right-to-work according to data from the National Conference of State Legislatures. States which passed right-to-work laws in 2015 or 2016 (Wisconsin and West Virginia) are coded as non-right-to-work in our sample, under the assumption that it would take some time for the passage of right-to-work statutes to affect labor market behavior. Other info: Heteroskedasticity-robust standard errors clustered at the city level shown in parentheses. Units of observation are 6 digit SOC occupation by city by year, for all observations with available data over 2013–2016 inclusive. All specifications include occupation-by-year and city-by-year fixed effects.

**Table A.22:** Counterfactual wage effects of setting HHI to 200,
excluding occupations with a represented-ness<0.5 in our BGT vacancy data

| | 2,500< HHI <10,000 | 1,500< HHI <2,500 | 500< HHI <1,500 | 200< HHI <500 | 0< HHI <200 |
|---|---|---|---|---|---|
| Lowest mobility | 7.6% (0.7m) | 5.7% (0.9m) | 3.6% (3.6m) | 1.1% (4.1m) | 0 (7.3m) |
| Q2 mobility | 3.0% (0.5m) | 2.3% (0.6m) | 1.4% (2.8m) | 0.5% (3.7m) | 0 (10.2m) |
| Q3 mobility | 2.1% (0.4m) | 1.6% (0.4m) | 1.0% (2.5m) | 0.3% (7.0m) | 0 (13.8m) |
| Q4 mobility | 0 (0.3m) | 0 (0.4m) | 0 (1.7m) | 0 (1.5m) | 0 (1.8m) |

Notes: This table repeats the analysis in Table 1.5, but dropping any occupations with an average represented-ness in our BGT vacancy data of less than 0.5 (roughly the bottom third of occupations). This is because our concentration data on these occupations might be a substantial overestimate of the true degree of employer concentration, if our data is disproportionately sampled from large employers for these occupations.

**Table A.23:** Twenty-five occupations with most people affected by employer concentration (based on a predicted occupation-CBSA wage effect of 2% or greater)

| Occupation | Represented-ness in occupation in BGT vacancy data |
|---|---|
| Security guards | .63 |
| Registered nurses | 2.1 |
| Nursing assistants | .75 |
| Hairdressers, hairstylists, and cosmetologists | .71 |
| Nonfarm animal caretakers | .69 |
| Fitness trainers and aerobics instructors | .61 |
| Childcare workers | 1.3 |
| Licensed practical and licensed vocational nurses | 1.4 |
| Radiologic technologists | .69 |
| Pharmacists | 1.2 |
| Emergency medical technicians and paramedics | .59 |
| Medical and clinical laboratory technologists | .98 |
| Phlebotomists | 1.4 |
| Pharmacy technicians | .91 |
| Medical assistants | .8 |
| Respiratory therapists | .99 |
| Management analysts | 1.7 |
| Lawyers | .83 |
| Librarians | .5 |
| Dentists, general | 1.1 |
| Software developers, applications | 4.6 |
| Bakers | 1.1 |
| First-line supervisors of personal service workers | .68 |
| Real estate sales agents | 1.9 |
| Massage therapists | .83 |

Notes: This table lists the degree of represented-ness of each of these twenty-five occupations in the BGT vacancy data. Represented-ness is defined as the occupation's share of vacancy postings in the BGT database relative to the occupation's share of total employment (as per BLS OES). The twenty-five occupations in this table correspond to the occupations with the highest number of people affected by employer concentration, as listed in Table 1.6. The more underrepresented an occupation is in the BGT vacancy data, the more likely we are overestimating the degree of employer concentration in these occupations and therefore overestimating the effect of concentration. On the other hand, in better-represented occupations we might be more confident that we are accurately prioritizing these occupations.

# B

# Appendix to Chapter 2

## B.1 Estimation of wage premia

### B.1.1 Estimating the union wage premium in the CPS-ORG

Following Hirsch and Macpherson (2019), we estimate the union wage premium using the CPS-ORG over 1984-2019. All the CPS data sets used in this paper are downloaded from IPUMS (Flood et al 2020). We restrict the sample to private sector workers, and we also drop workers for whom wages were imputed in the CPS (following Bollinger and Hirsch (2006). We do this because the wage imputation procedure in the CPS does not take union status into account, which biases estimates of the union wage premium downwards). Our key variable of interest - union status - is a dummy variable which takes the value 1 if the worker was either a member of a union or covered by a collective bargaining agreement. We construct our dependent variable - hourly wage - as either the hourly wage reported by the worker, or the weekly earnings divided by usual hours worked at the respondent's main job (if hourly wage was not reported). We then regress the log hourly wage on union status and various control variables: age, age squared, male dummy, 6 race categories, Hispanic dummy, age # male, age squared # male, married dummy, married # male, state, dummy for central city, 6 education categories, education # age, education # age squared, education # male, dummy for full-time workers, dummies for 383 occupation categories, and for 250 industry categories (where # denotes an interaction). We run our regressions separately for each year, estimating a separate union wage premium for each year. Our estimates are shown in Figure B.1 alongside estimates from Hirsch and Schumacher (2004), Blanchflower and Bryson (2004), and Hirsch and Macpherson (2019).

### B.1.2 Estimating firm size premia in the CPS ASEC

Following Song et al (2019) (and others), we estimate the firm size wage premium using the CPS-ASEC sample over 1990-2019 (the years for which the CPS-ASEC collected respondents' employer size). We restrict the sample to private sector workers. Our key independent variable is the size of workers' employer in the last year (variable FIRMSIZE in the IPUMS database, which indicates "the

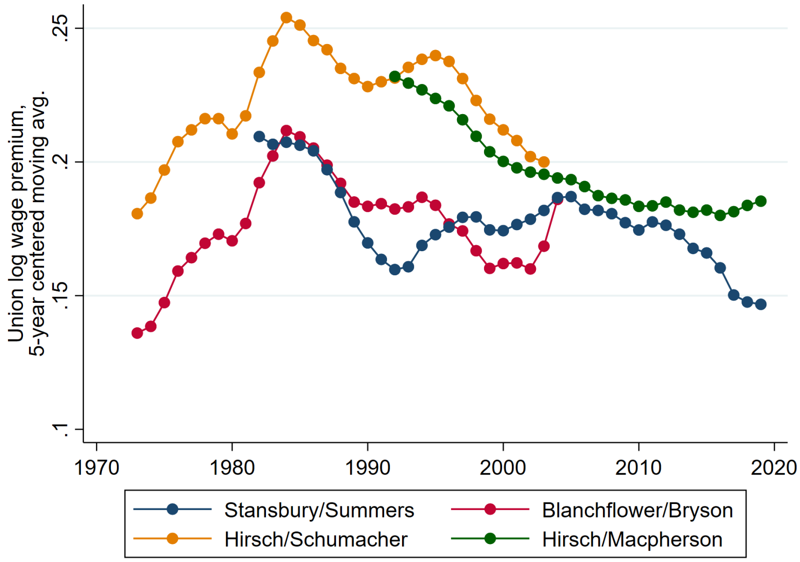



**Figure B.1:** Estimated union log wage premium, private sector

total number of persons who worked for the respondent's employer during the preceding calendar year, counting all locations where the employer operated"). We use four size classes: fewer than 100 employees, 100 to 499 employees, 500 to 999 employees, or 1000+ employees. The ASEC only collects workers' annual earnings, weeks worked last year, and usual hours worked per week last year, so we construct our dependent variable - hourly wage - from these variables (introducing measurement error if workers misremember or misreport any of these variables).

We then regress the log hourly wage on separate categorical variables for the different firm size classes, and a large set of control variables (the same controls as in the union wage premium regression, but also including union status): age, age squared, male dummy, 6 race categories, Hispanic dummy, age # male, age squared # male, married dummy, married # male, state, dummy for central city, 6 education categories, education # age, education # age squared, education # male, dummy for full-time workers, 383 occupation categories, 250 industry categories, and a dummy for union membership or coverage (where # denotes an interaction). The union membership/coverage variable is only available for one quarter of the ASEC sample each year, which makes our estimates of the firm size premium relatively noisy if we run them separately for each year (as the sample size is relatively small). So, we run our regressions over pooled 5-year periods: 1990-1994, 1995-1999, 2000-2004, 2005-2009, 2010-2014, 2015-2019.

### B.1.3 Estimating industry wage premia in the CPS-ORG

Following Katz and Summers (1989), we estimate industry wage premia from the CPS-ORG over 1982-2019. We restrict the sample to private sector workers, which gives us between 120,000 and 150,000 observations per year (for a total of 5.3 million observations over 1982-2019). Our independent variable of interest is the industry the worker is employed in. We estimate industry wage premia in separate regressions at three different levels of industry aggregation: 18 sectors (aggregated from the CPS IPUMS ind1990 industry variable to sectors at the NAICS level, excluding "Management of Companies and Enterprises"), 56 industries corresponding to BEA industry codes (roughly, NAICS 3-digit industries), and 228 detailed industries, which correspond to SIC industries (at the level of the ind1990 codes in CPS-IPUMS). More details on how we allocated workers in each ind1990 code to each NAICS sector and BEA industry are in the final section of this Appendix. Our dependent variable is the log hourly wage. As in the union wage premium regressions, this is either the hourly wage reported by the worker, or the weekly earnings divided by usual hours worked at the respondent's main job (if hourly wage was not reported).[1] We then regress the log hourly wage on separate categorical variables for each industry, and a large set of control variables (the same as in the firm size regression): age, age squared, male dummy, 6 race categories, Hispanic dummy, age # male, age squared # male, married dummy, married # male, state, dummy for central city, 6 education categories, education # age, education # age squared, education # male, dummy for full-time workers, 383 occupation categories, and a dummy for union membership or coverage (where # denotes an interaction). We run the regressions separately for each year over 1984-2019, and separately for each level of industry aggregation. Note that our baseline regressions do not control for firm size, since it is only available in the CPS ASEC from 1990 onwards. However, we replicate almost identical industry wage premia estimates, controlling for firm size, in the CPS ASEC from 1990 onwards.

### B.1.4 Estimating industry wage premia in the CPS-ORG: longitudinal estimates

Our baseline estimates of industry wage premia are estimated cross-sectionally. We also estimate industry wage premia in the CPS-ORG longitudinally, as a robustness check. Restricting our sample only to the individuals who can be matched from one year to the next year (using the cpsidp variable available at CPS IPUMS), we have between 15,000 and 45,000 unique observations in each year. The industry fixed effects in a longitudinal regression, however, are estimated only from people

[1] Note that the CPS top-codes earnings data for high-earning individuals. The (weighted) share of respondents with top-coded earnings in the private sector varies as the top-coding threshold changes three times over our sample period: the lowest share is 1% in 2000 and the highest share is to just under 5% by 2019. Excluding CPS respondents with top-coded incomes makes no perceptible difference to our estimated sector wage premia or industry wage rents.

who move jobs from one sector to another during the 12-month period between our two observations: this means that the industry fixed effects are estimated from a sample of only 2,600-10,000 observations per year (with a median number of industry switchers of 7,803 per year). The small sample size implies that, even when estimating the industry fixed effects only for our sample of 9 large SIC sectors (or 18 NAICS sectors), estimates of the industry fixed effects are rather noisy. Measurement error of industry coding, which is well-documented in the CPS (see e.g. Kambourov and Manovskii 2008), may then lead to concerns of relatively serious attenuation bias. Nonetheless, we find that there is a strong and highly statistically significant relationship between the log wage fixed effects we estimate from the large cross-sectional samples, and the log wage fixed effects we estimate from the smaller longitudinal sample of industry movers. Averaging the wage effects over 5-year periods within sector, a regression of the cross-sectional wage effects on the longitudinal wage effects, weighted by industry compensation, gives a coefficient of exactly 2 (with a standard error of 0.1 and R-squared of 74%) - supporting our practice of halving the raw cross-sectional fixed effects to estimate the true industry wage premia.

Figure B.2 shows estimated log wage fixed effects for each NAICS sector, relative to Retail Trade, where each point on the plot represents the average log wage fixed effect for a NAICS sector over a five year period (1982-1984, 1985-1989, 1990-1994, 1995-1999, etc.). There is an extremely close relationship between the estimated fixed effects from the cross-sectional data vs. from the longitudinal data. In addition, the decline in the standard deviation of the longitudinal industry log wage fixed effects is proportionally as large or larger than the decline in the standard deviation of the cross-sectional industry log wage fixed effects, as shown in Figure B.3.


**Figure B.2:** Correlation between cross-sectional and longitudinal industry wage fixed effects

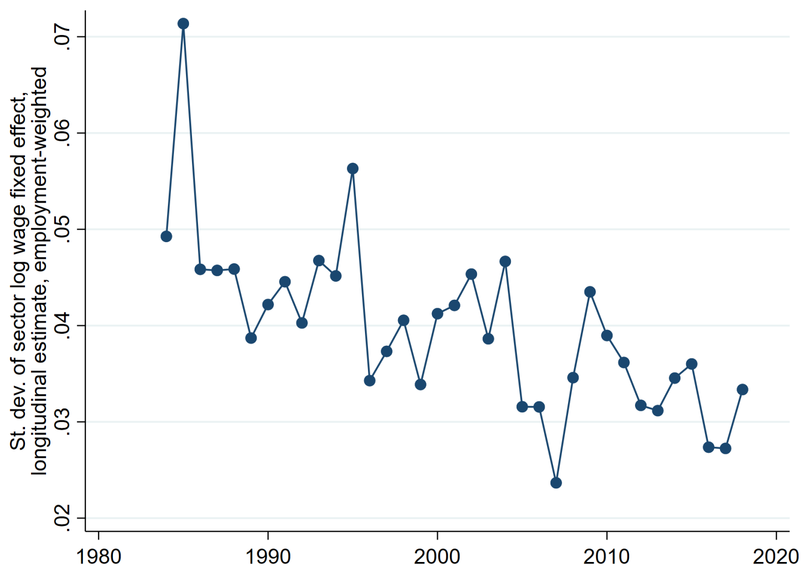



**Figure B.3:** Employment-weighted standard deviation of industry log wage fixed effects, estimated longitudinally for NAICS sectors

### B.1.5 Benchmarking our estimates of industry wage premia against the literature

Our estimates of industry wage premia involve (1) estimating industry log wage fixed effects cross-sectionally in the CPS across sectors, controlling for a large number of person- and job-level covariates; (2) rescaling these fixed effects relative to Retail Trade (which is set to have a wage premium of zero); and (3) cutting these estimates in half. There may be concerns, however, that our procedure of cutting the coefficients in half does too little - or too much - to account for unobserved productivity or for compensating differentials. Either unobserved productivity or compensating differentials could generate variation in industry fixed effects without rents being the underlying cause.

These concerns should be somewhat assuaged by the analysis above, which shows that estimates of sector wage premia from sector movers in the longitudinal component of the CPS - controlling for person-level fixed effects - are very highly correlated with our estimates from the cross-sectional CPS and are exactly half as big, on average. This gives strong support to our practice of using the cross-sectional effects and cutting them in half. (We use the cross-sectional estimated effects, rather than the longitudinal ones, as the sample size is much larger so they are much less noisy).

As an alternative check on our methodology of cutting the fixed effects in half to obtain wage premia, we also benchmark our estimates against estimates from Abowd et al (2012) and Sorkin (2018), two papers which use employer-employee matched administrative data in the U.S. to study, respectively, the role of firm fixed effects in industry wage differences, and the role of rents in firm fixed effects. Abowd et al (2012) use an AKM decomposition to estimate firm and worker effects in different industries and provide data on the average firm fixed effect within each SIC 1987 industry for the period 1990-2001. Sorkin (2018) decomposes the degree to which the estimated firm fixed effects in an AKM model are due to rents versus compensating differentials and finds that around 1/3 of firm fixed effects are due to rents while 2/3 are due to compensating differentials. We use these two papers to generate approximate estimates of industry wage premia which are due to rents, for each of the 9 SIC sectors. First, we take the Abowd et al (2012) estimates of the average firm effect across SIC industries, and aggregate these up to the level of 9 SIC sectors using a simple average. We then rescale these sector-level average firm effects relative to Retail Trade, setting the average firm effect for Retail Trade to be zero. We finally divide these estimates by three, reflecting Sorkin's (2018) finding that only 1/3 of estimated firm fixed effects reflect rents. We compare our estimates of industry wage premia due to rents, approximated using Abowd et al (2012) and Sorkin (2018), with our baseline estimates of industry wage premia estimated in the CPS over the same time period of 1990-2001. The comparison can be seen in Figure B.4. For all sectors except Mining, there is a strikingly close relationship between the two estimates. That is, our estimates of industry wage premia from the CPS over 1990-2001 line up well with our back-of-the-envelope estimate of industry rents from Abowd et al (2012) and Sorkin (2018) - estimated using results from papers which explicitly remove the effects of unobserved productivity (through worker fixed effects) and compensating differentials (through the Sorkin (2018) procedure). This can give us some degree of confidence that our estimates of industry wage premia do primarily reflect rents.

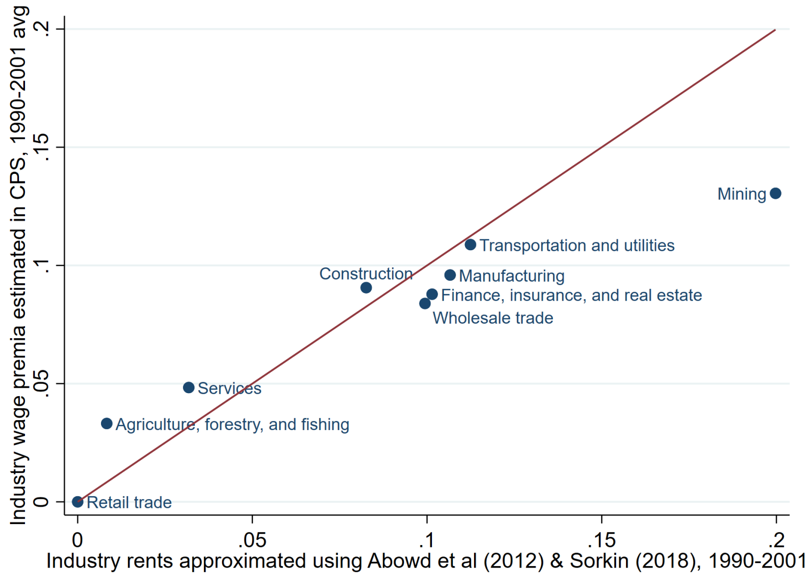



**Figure B.4:** Correlation between industry wage premia as estimated from CPS, and industry rents approximated from AKM models

### B.1.6 Decline in the variance of industry wage premia: alternative weighting

Our estimation of the decline in the variance of industry wage premia may be sensitive to weighting choices we make, both in the estimation of the industry fixed effects in the wage regressions, and in the weighting across industries when constructing the standard deviation of the fixed effects. In our baseline estimates, we weight each person equally in the estimation of the industry wage fixed effects, and we weight each industry by its employment when calculating the standard deviation of the fixed effects. Here, we present three figures to show that the weighting decisions do not have a substantial impact on the estimated outcomes.

In Figure B.5, we show the equal-weighted and employment-weighted standard deviations of

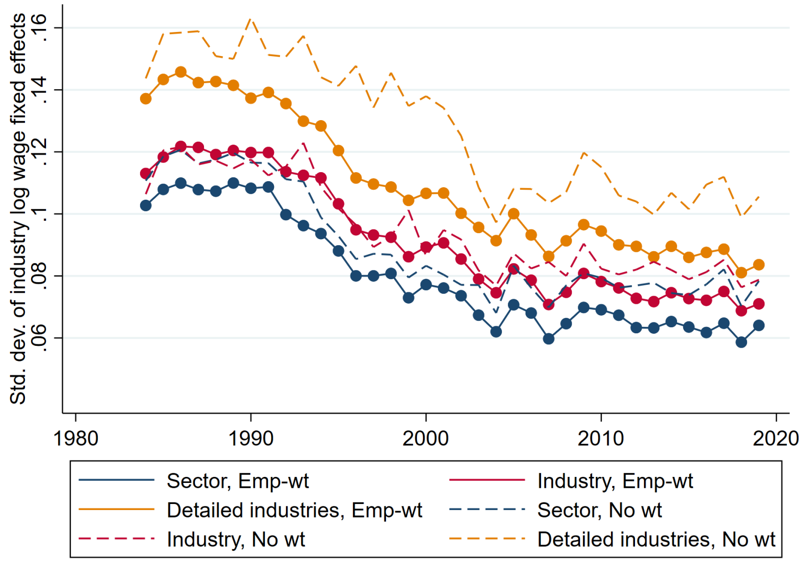



**Figure B.5:** Decline in standard deviation of industry log wage effects: equal-weighted and employment-weighted

the industry log wage effects estimated with equal weights across people. The similar trend in both equal-weighted and employment-weighted standard deviations is another way of illustrating the fact that the majority of the decline in the variance of industry wage premia occurred within industries. In Figure B.6, we show the equal-weighted and employment-weighted standard deviations of the industry log wage effects, estimated with log wage weights across people in the initial regressions using the CPS data. In Figure B.7, we show the equal-weighted and employment-weighted standard deviations of the industry log wage effects, but estimated with wage weights across people in the initial regressions. While the wage-weighted estimates are noisier than the equal-weighted estimates, the pattern is very similar across all three figures. The industry wage premium estimates with the different weighting schemes are also very similar, as shown in Figure B.8.

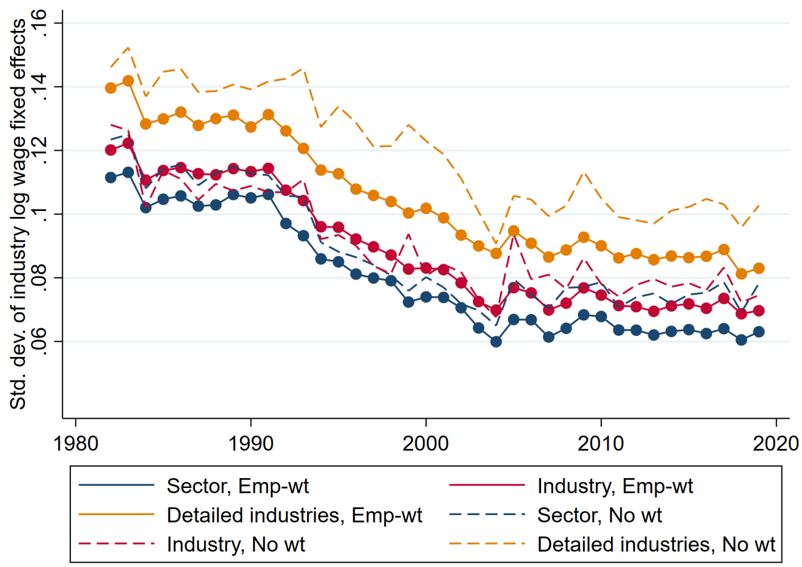



**Figure B.6:** Decline in standard deviation of industry log wage effects, with log-wage weighting in underlying regressions

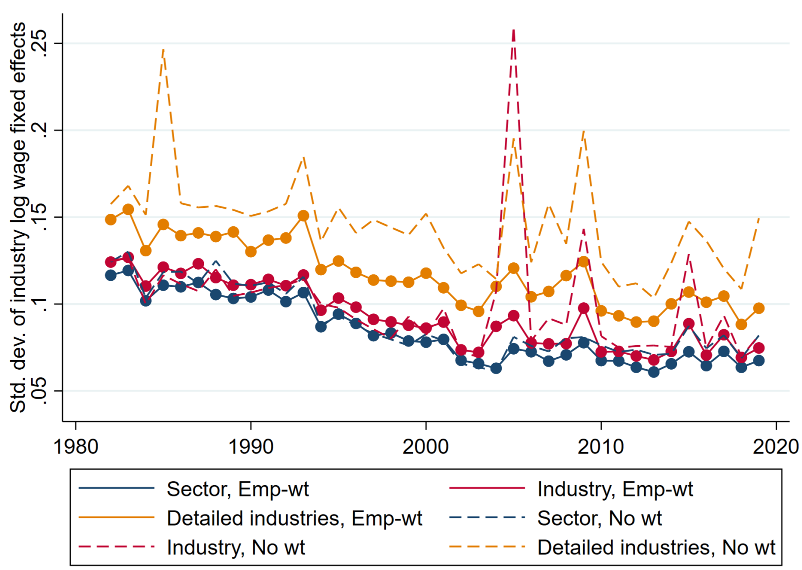



**Figure B.7:** Decline in standard deviation of industry log wage effects, with wage weighting in underlying regressions

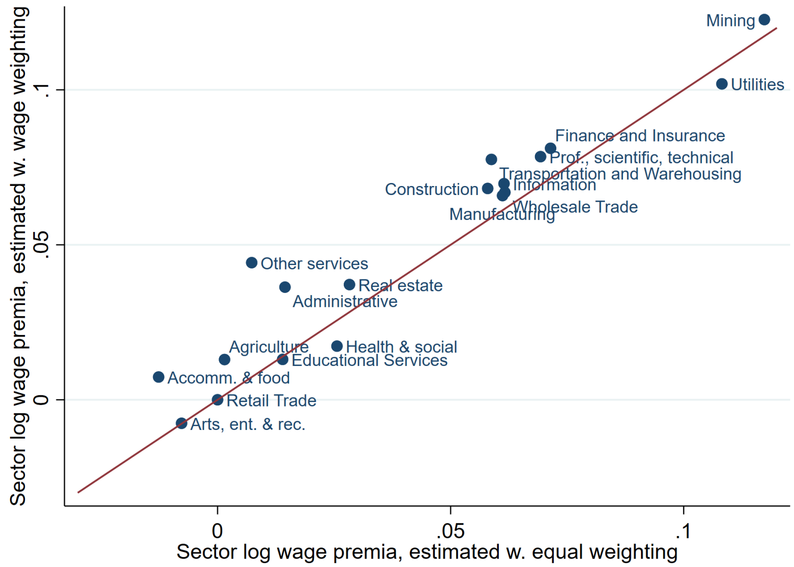



**Figure B.8:** Comparison of estimated industry log wage premia, relative to Retail Trade (2016), estimated with equal weighting in regressions vs. wage weighting in regressions

### B.1.7 Increasing variance of industry-level profitability

As we note in the body of the paper, even if we interpret industry wage premia as rents to labor, a decline in the dispersion of industry rents going to labor could be a result of (some combination) of three factors: (1) a fall in the rent-sharing coefficient holding total rents constant, meaning total rents to labor fall, as workers at high-rent industries no longer do as well as they did before; (2) a reallocation of workers from industries with high labor rents (either because of high rent-sharing or high rents) to industries with low labor rents, which would mean that total rents to labor have fallen, but only because of structural changes in the economy; or (3) a fall in the dispersion of rents across industries, holding the rent-sharing coefficient constant, meaning total rents to labor may not have fallen. In Figures B.9 and B.10 below, we show that the dispersion of rents does not appear to have

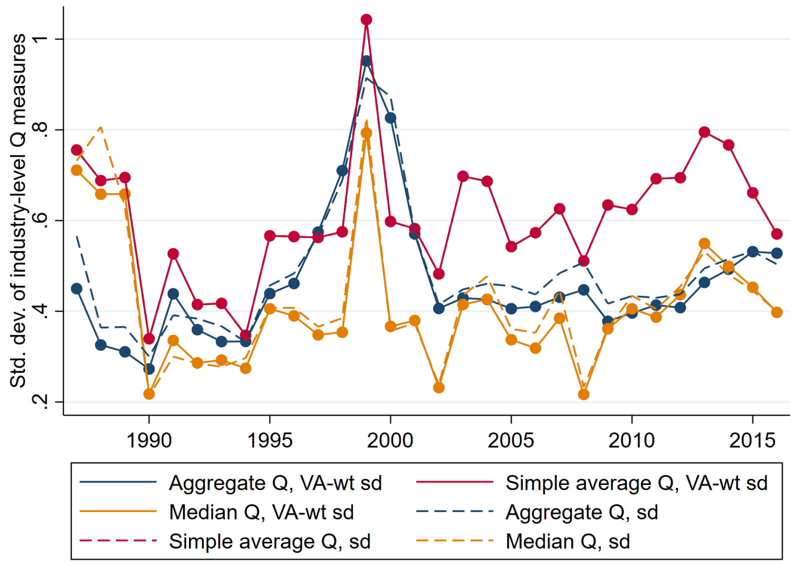



**Figure B.9:** Standard deviation of industry-level measures of Q

fallen across industries (for BEA industries); in fact, the dispersion of profits per worker and average Q appears if anything to have risen somewhat over the period.

## B.2 Calculation of labor rents

### B.2.1 Baseline: nonfinancial corporate sector

Industry rents: To create our series of sector-level wage premia, to use to calculate industry rents: For years 1984-2019, we use our estimates of sector-level wage fixed effects from the CPS-ORG as outlined above. We estimate these for SIC sectors and for NAICS sectors separately (See the final section of this Appendix for details as to how we crosswalk the ind1990 industry code in the CPS IPUMS data into SIC and NAICS sectors). For years 1982-1983, we use estimates of sector-level

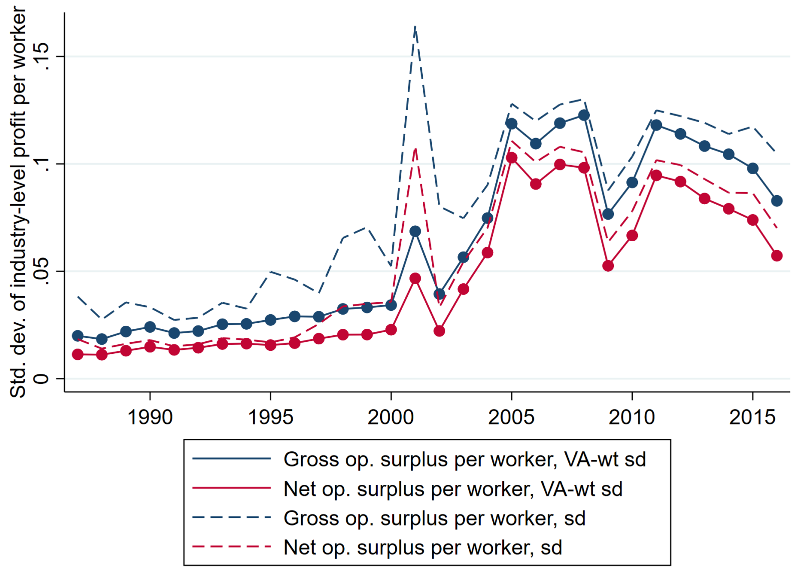



**Figure B.10:** Standard deviation of industry-level profits per worker

wage fixed effects from the CPS-ORG, estimated without the union control (which is only introduced in 1984), and rescaled. Specifically, we rescale the estimated fixed effects for 1982-1983 using the ratio of the fixed effects without union controls to the fixed effects with union controls over 1984-2019 (in practice, the estimates are very similar).

We then convert these sector-level wage fixed effects into our estimated sector wage premia for each sector S by setting the wage premium for Retail Trade to zero, then taking half the difference between the fixed effect for sector S and the fixed effect for Retail Trade.

To calculate aggregate industry rents for years 1982-1986, we use BEA NIPA compensation by sector at the SIC level, along with our SIC level sector wage premium estimates. To calculate aggregate industry rents for each year from 1987 to 2016, we use BEA NIPA compensation by industry at the NAICS level, along with our NAICS level industry wage premium estimates.

For our baseline calculations for the nonfinancial corporate sector, we exclude the SIC sector "Finance, insurance, and real estate" (for 1982-1986) and we exclude the NAICS sectors "Finance and insurance" and "Real estate, rental, and leasing" (for 1987-2016). We also estimate industry rents for SIC industries for 1987 to 1997 to understand the degree to which the SIC-based and NAICS-based series are comparable. The series move almost identically together, but the SIC series is slightly higher than the NAICS series. To adjust for this, we take the average ratio of the NAICS labor rents series to the SIC labor rents series over 1987-1997, and scale the SIC series down by this ratio for the years 1982 to 1987.

We have the further issue that our BEA compensation by industry data is for the entire private sector, not just the corporate sector. We therefore then take our estimate of total industry rents and scale it down by the ratio of total compensation in all private industries (excluding finance, insurance, and real estate) to total compensation in the nonfinancial corporate sector.

Union rents: We estimate the union coverage rate for all private sector workers in the CPS-ORG for years 1984-2019, excluding those in Finance, Insurance, or Real Estate. We extend this backwards to 1982 by applying the annual rate of change in the union coverage rate for all private sector workers (from unionstats.com) for 1982-83 and 1983-84. We estimate our own union wage premia from the CPS-ORG for years 1984-2019, as outlined in the first section of this Appendix. We then use the Blanchflower and Bryson (2004) series of union wage premia for the years 1982 and 1983. As shown in Figure A1, the series are very similar for the years that they overlap, and we use very similar controls to estimate the series, suggesting that this imputation is legitimate. We estimate total union rents for the nonfinancial corporate sector using the estimated union wage premia, estimates of the union coverage rate for nonfinancial private sector workers, and compensation for the nonfinancial corporate sector from the BEA NIPA.

Firm size rents: We estimate firm size premia from the CPS ASEC for years 1990-2019, as outlined in the first section of this Appendix We use the Census Bureau's SUSB data set to calculate the total payroll share by firm size category in each year, for three categories: less than 100, 100-499, and 500+ workers. We then apply these payroll shares to total compensation for the nonfinancial corporate sector in each year, from the BEA NIPA, to obtain estimated shares of compensation in each firm size category. We estimate total firm size rents for the nonfinancial corporate sector for 1990-2019 using our firm size premia and these estimated compensation shares. To calculate firm size rents for the years 1982-1989, we refer to Levine et al (2002) who show estimates of the distribution of employment by firm size in 1979 and 1993 (their Table 4.1), and the estimated firm size log wage effect in 1979 and 1993 (their Table 4.3), for firms of 100-999 workers and 1,000+ workers. We estimate the change in firm size rent share over 1979-1993 which would be implied by their estimates, which is around 0.4 percentage points. We then note that in our data, the firm size rent share does not change much between 1990 and 1993. We therefore use the estimated decline in the firm size rent share of 0.4 percentage points to impute total firm size rents in 1979 to be equal to total firm size rents in 1990 + 0.4 percentage points. We linearly interpolate the firm size rents in each intervening year 1980-1989.

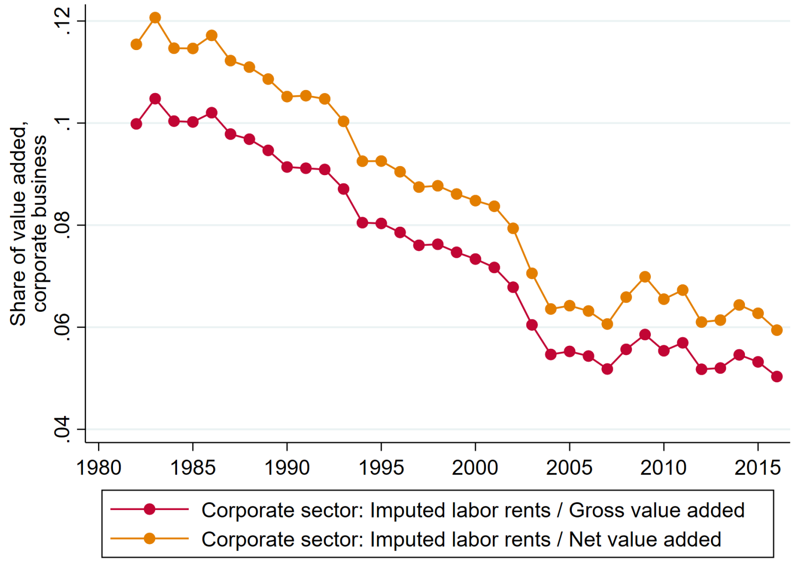



**Figure B.11:** Labor rents as share of corporate business value added

### B.2.2 Alternative calculation: corporate sector

Our baseline estimates of labor rents (described above) are for the nonfinancial corporate sector. Here we replicate our calculation for total labor rents, but for the entirety of the corporate sector: that is, the calculation includes the finance industry. We do not find a substantially different pattern for the corporate sector relative to the nonfinancial corporate sector, as shown in Figure B.11. Figure B.12 breaks out the differences in the series by source, showing that union rents are slightly higher as a share of nonfinancial corporate value added as compared to corporate value added, but industry rents slightly lower.

Differences between the corporate sector and nonfinancial corporate sector estimates of labor rents are as follows:

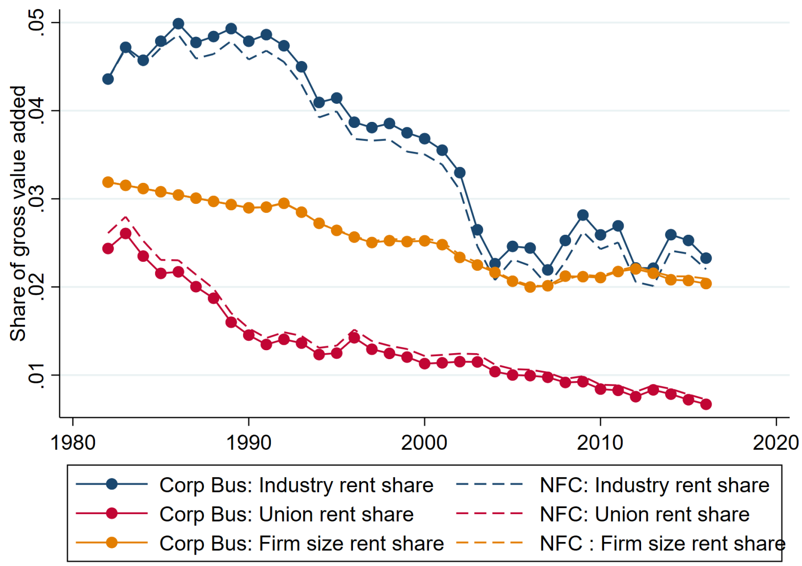



**Figure B.12:** Labor rents as share of corporate and nonfinancial corporate business ("NFC") value added, by source

1. Union rents: The nonfinancial corporate sector series uses our estimate of the unionization rate excluding finance, insurance, and real estate. The corporate sector series uses the unionization rate across all private industries. Because unionization in finance, insurance, and real estate is lower than average, this means that union rents as a share of value added in the corporate sector is slightly lower than union rents as a share of value added in the nonfinancial corporate sector.

2. Industry rents: Our estimate of industry rents for the nonfinancial corporate sector excludes any rents in finance, insurance, or real estate. Our estimate of industry rents for the corporate sector includes labor rents in finance, insurance, and real estate (though the magnitude of estimated industry rents in real estate is too small to affect the overall calculation

much). There are three forces operating on our series for industry rents in the corporate sector: (1) industry rents in finance were lower than the average industry rents outside of finance for our whole sample period, acting to make the level of total industry rents as a share of value added lower for the corporate sector as a whole than for the nonfinancial corporate sector; (2) the share of finance in value added and compensation grew from the 1980s to the 2010s; and (3) industry rents fell much more slowly in the financial sector than in nonfinancial sectors, which would operate to make the decline in overall industry rents as a share of value added less steep for the corporate sector than the nonfinancial corporate sector.[2]

3. Firm size rents: Firm size rents as a share of value added is by definition the same in both sectors, as we use the same methodology for both sectors.

Top-coding and high earners in the financial sector: *One major caveat to these estimates of labor rents in the nonfinancial corporate* sector is that - since the CPS earnings data is top-coded (e.g. at $2,885 per week for the 2000s and 2010s) - we will not pick up the very large increases in compensation for the highest earning financial sector workers over the period, which may well reflect rents (see, e.g. Phillipon and Reshef 2006). This means that our series of industry rents should be thought of as a series measuring industry rents that flow to the majority of workers.

Because the top-coding thresholds jump, the share of CPS respondents who are top-coded varies

[2] Industry rents in finance - as we estimate them from the CPS - fell very gradually in the 1980s and 1990s, and then levelled off at around 1.5% of financial sector value added in the 2000s. Industry rents in non-financial sectors fell much more sharply in the 1980s and 1990s, and then levelled off at around 2% of nonfinancial sector value added in the 2000s. Over the same period, the financial sector grew as a share of both compensation and value added: in the BEA NIPA Industry Accounts data, the share of total compensation (in private industries) in finance, insurance, and real estate rose from around 9% in the late 1980s to around 11% by the 2010s.

over time. In our CPS-ORG data, the (weighted) share of workers in Finance, Insurance, and Real Estate who had top-coded earnings rose from 2% in 2000 to 9% by 2019. (The share in the overall data for private sector rises from 1% in 2000 to just under 5% by 2019). If the finance wage premium for high-earning financial professionals is growing over time over the 2000s and 2010s even as it slightly declines for the majority of finance sector workers - which seems possible - then our estimates of labor rents overestimate the decline in labor rents going to all workers, but are a good estimate of labor rents going to the majority of workers.

A quick counterfactual estimate, however, suggests that the degree to which the exclusion of top-earning workers in finance might affect our calculations is relatively limited. Assume for example that a rise in rents going to top earners drastically changed the average wage premium in Finance and Insurance, meaning that it rose from 11% to 15% over 1987-2016, rather than falling from 11% to 8%. In this case, our estimate of total industry rents as a share of value added in the corporate sector would have fallen from 3.9% to 2.3%, rather than from 3.9% to 2.0%. over 1987-2016.

### B.2.3 Labor rents for college and non-college workers

We estimate labor rents for college and non-college workers separately. Our estimates go from 1984-2016 (rather than 1982-2016 for the aggregate calculation) because we are only able to obtain estimates of union wage premia and unionization rates by education group for 1984 onwards. We estimate that labor rents to non-college workers, as a share of net value added in the nonfinancial corporate sector, fell substantially over 1987-2016, while labor rents to college workers rose slightly (Figure B.13). This, however, is the result of two effects: a compositional effect as the share of the labor force without a college education fell over this period, and a within-group effect as labor rents fell by more for college workers than for non-college workers (Figure B.14).

Industry rents: For industry rents, we estimate sector wage premia in the CPS separately for workers with a four year college degree and for workers without a four year college degree. We also use the CPS to estimate the total share of earnings by education group within each sector in each year. Using these earnings shares and the BEA NIPA Industry Economic Accounts, we estimate total compensation by education group and sector, and apply our sector-by-education-by-year wage premia.

Union rents: We estimate union wage premia and union coverage rates in the CPS separately for workers with a four year college degree and for workers without a four year college degree. We also use the CPS to estimate the total share of earnings by education group in each year. We use these, and total compensation for the nonfinancial corporate sector from the BEA NIPA, to estimate total union rents for college and non-college workers for each year.

Firm size rents: We estimate firm size wage premia in the CPS separately for workers with a four year college degree and for workers without a four year college degree. We also use the CPS to estimate the total share of earnings by education group and firm size class in each year. We use these, the payroll shares by firm size class from the Census Bureau SUSB, and the total compensation for the nonfinancial corporate sector from the BEA NIPA, to estimate total firm size rents for college and non-college workers for each year from 1990 onwards. We are unable to calculate our own estimates of firm size rents for years pre-1990. However, our estimates of firm size rents as a share of total compensation for each education group, for the years 1990-2019, show very consistent and

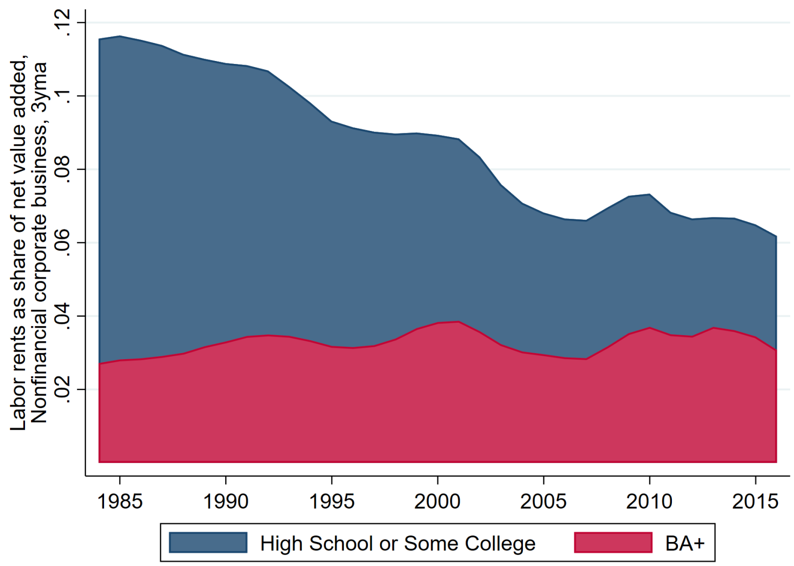



**Figure B.13:** Estimated labor rents as share of net value added, nonfinancial corporate sector, by education group (3-year moving average)

strongly divergent trends, so we interpolate these same trends backwards through 1984 (as shown in Figure B.15) and use these trends to impute firm size rents for years 1984-1989. This imputation is consistent with our imputation of firm size rents at the aggregate level using data from Levine et al (2002), as described in the second section of this Appendix (in terms of the change in total firm size rents as a share of value added).

### B.2.4 State-level labor rents calculations

We estimate state-level labor shares using the Regional Economic Accounts from the Bureau of Economic Analysis. We use the state-level estimates of GDP and compensation for all Private Industries to calculate the labor share (compensation/GDP). To calculate our labor rents measure at the state

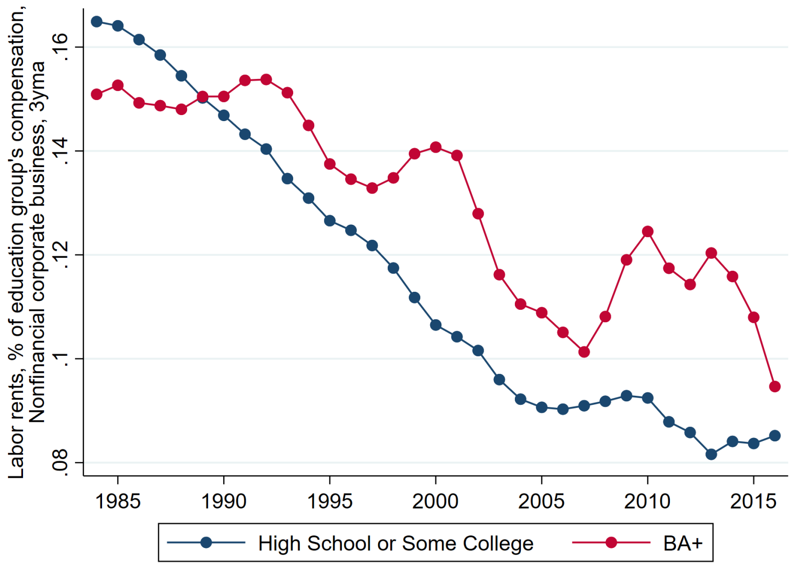



**Figure B.14:** Estimated labor rents as share of compensation, by education group (non-financial corporate sector)

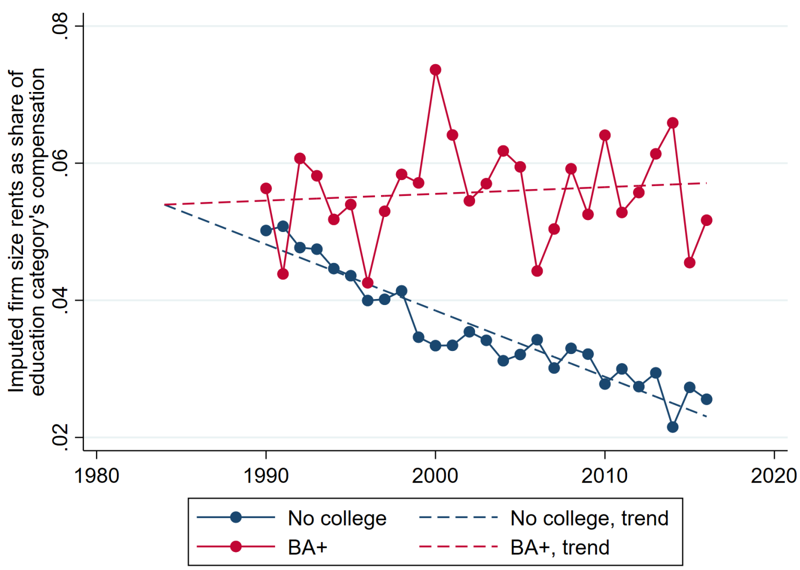



**Figure B.15:** Firm size rents as a share of compensation, by education group: estimates and trend level, we first use the CPS-ORG data to estimate industry-by-state, firm size-by-state, and union-by-state wage premia for each year 1984-2019, using the full set of controls described in the first section of this Appendix.

Industry rents: We estimate industry wage premia by state at both the NAICS sector level and the SIC sector level, using the crosswalk from Census industry codes to NAICS and SIC sectors described in the final section of this Appendix. We obtain state-level compensation at the industry level from the BEA Regional Economic Accounts data, which provides data on NAICS industries for 1997 onwards and for SIC industries for years up to and including 1997. We calculate industry rents with the formula in the main body of the paper, using NAICS compensation by industry and the industry wage premia estimated using NAICS codes for 1997-2017, and SIC compensation by industry and the industry wage premia estimated using SIC codes for 1984-1996. We then take the ratio of estimated industry rents at the state level using NAICS industries, relative to the estimated industry rents using SIC industries, in 1997, and apply this backwards over 1984-1996 to create a roughly consistent series over time.[3]

Union rents: We estimate the union coverage rate by state from the CPS, and calculate union rents using the state-level union coverage rate and union wage premium.

Firm size rents: We estimate the firm size wage premium by state from the CPS ASEC for years 1990-2019, use data on the distribution of payroll by firm size and state from the Census Bureau SUSB database to estimate payroll share by firm size class, and then apply this to compensation

[3] The state level calculations use all private industries' GDP and compensation as their baseline as we do not have state-level data on the nonfinancial corporate sector only. As such, we include industry rents for financial industries in this calculation.

by state from the BEA Regional Economic Accounts to estimate firm size rents by state for each year from 1990-2016. To estimate firm size rents from 1984-1989, we use the data from Levine et al (2002), whose data suggests that firm size rents fell by 0.4% of value added from 1979 to 1993 at the national level. As in our national level estimates, we therefore apply a fall of 0.4% of state-level GDP to our firm size rents calculations for each state over 1979-1993, with a linear interpolation for each year between these dates.

#### B.2.5 Industry-level labor rents calculations

Our calculation of industry rents largely follows the methodology for the aggregate level outlined above, with the notable exception that we do not calculate firm size rents as we do not have payroll data by firm size at the industry level.

Industry rents: We estimate industry wage fixed effects by state at the level of 51 industries (BEA industry code  NAICS 3-digit level), over 1987-2016, in the CPS-ORG. We then calculate the industry wage premium as half of the difference between the estimated fixed effect for each industry, relative to the lowest-wage large industry, which is Food Services and Drinking Places. This industry had 12.3 million employees as of February 2020, and average hourly earnings of $15.25 as of October 2019 (the latest data available from the BLS at time of writing). We obtain industry-level compensation at the NAICS 3-digit level from the BEA Industry Economic Accounts, and aggregate this to the BEA industry code level.

Union rents: We use our estimates of the union wage premium at the national level, and estimate the union coverage rate by industry using the CPS. Our results are not sensitive to estimating the union wage premium separately for each individual industry, rather than using the aggregate union wage premium.

Firm size rents: We do not include firm size rents in our measure of total labor rents by industry, because we do not have data on compensation by firm size class and 3-digit NAICS industry.

### B.2.6 Were labor rents redistributed or destroyed? Cross-industry analysis

In the main body of the paper, we start to address the question: were labor rents redistributed or destroyed? We predict that, if the decline of labor rents is because rents in specific industries were destroyed as a result of globalization or increased competition, then one would expect (1) that returns to capital would fall alongside rents to labor, and (2) that the total rents in the industry - profits, plus labor rents - would be falling.

We look at 51 industries over 1987-2016 to establish whether this was the case (industries at the BEA industry code/roughly NAICS 3-digit level, as in the industry-level analysis in our paper). We measure three items to answer these questions:

- The profit rate to capital, which we measure as net operating surplus minus a rough measure of the cost of capital,[4] over fixed assets;
- The rent rate to labor, for which we use our measure of labor rents, divided by fixed assets;

[4] We use the 3-month Treasury rate, plus a 5% fixed equity premium, minus a backward-looking measure of inflation expectations (a 3-2-1 weighted average of PCE inflation over the previous three years). This does not account for differential costs of capital in different industries, perhaps caused by differential risk across industries.

and

- The total rent rate, which is the sum of the profit rate to capital and the rent rate to labor.

We calculate these three statistics for each of the 51 industries under consideration and compare their average values in 1987-91 and 2012-16, the start and end of our sample period.[5]

The industries of apparel manufacturing and wholesale trade give particularly striking examples of our heuristic to distinguish between rent destruction vs. rent redistribution, shown in Figures B.16 and B.17. In apparel manufacturing, the profit rate to capital and rent rate to labor both fell very substantially over 1987-2016 (the figure shows 5-year centered moving averages). This suggests that in apparel manufacturing, the dominant trend was a destruction of rents – as would accord with the substantial rise in low-wage import penetration over the period. On the other hand in wholesale trade, the decline in the rent rate to labor was more than matched by an increase in the profit rate to capital over the period, suggesting that the dominant trend was a redistribution of rents from labor to capital.

We can analyze this more systematically by categorizing industries into groups based on the changes in the profit rate to capital and rent rate to labor over the period, as shown in Table B.1. For each category, we show the number of industries in this category and the share of the private sector workforce in 2018 which was employed in these industries (which we calculate using the BEA NIPA employment by industry database).[6]

[5] This is the longest sample period for which we have industry-level data for consistently defined NAICS-based industry codes.

[6] Note that the totals do not add up to 100%, because this industry-level analysis excludes financial industries to be consistent with our main analysis in the paper.

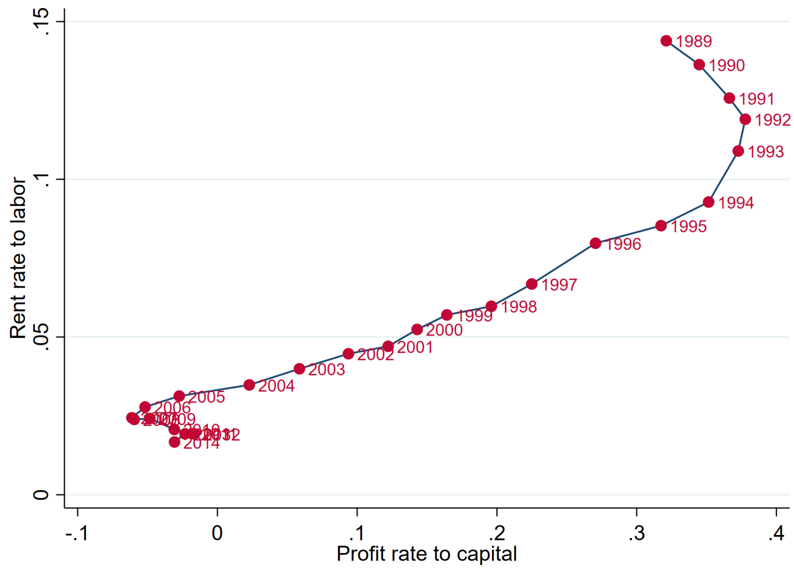



**Figure B.16:** Imputed profit rate to capital and rent rate to labor, Apparel Manufacturing

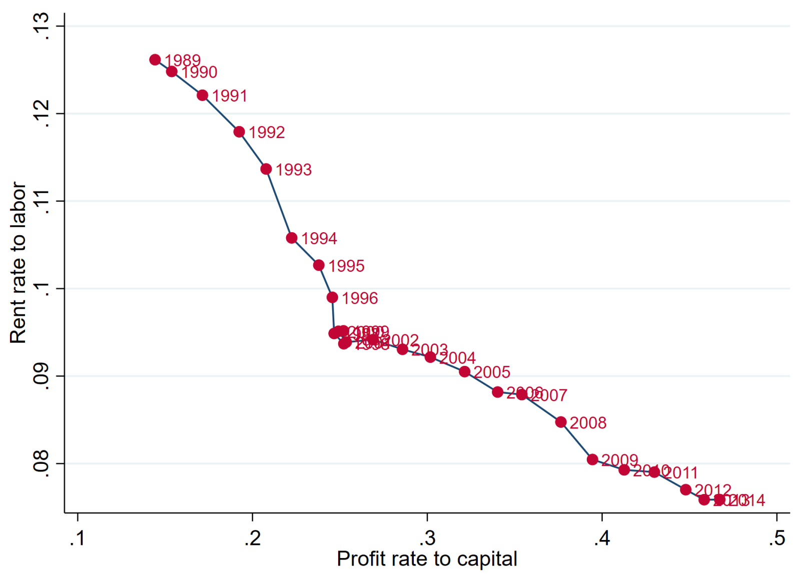



**Figure B.17:** Imputed profit rate to capital and rent rate to labor, Wholesale Trade

**Table B.1:** Industries, categorized by changes in profit rate to capital and rent rate to labor, 1987-2016

| | | Profit rate to capital | | |
|---|---|---|---|---|
| | | rose | roughly the same | fell |
| Rent rate to labor | | | | |
| rose | *industries:* | 1 | 0 | 0 |
| | *(% of p.s. workforce)* | (<1%) | (0%) | (0%) |
| roughly the same | *industries:* | 5 | 0 | 0 |
| | *(% of p.s. workforce)* | (13%) | (0%) | (0%) |
| fell | *industries:* | 29 | 2 | 14 |
| | *(% of p.s. workforce)* | (29%) | (18%) | (30%) |

Note: We define "roughly the same" as being within 0.5 percentage points of its 1987-91 level in 2012-16. "p.s. workforce" stands for private sector workforce.

We find that in 29 industries, employing around 30% of the private sector workforce, the profit rate to capital rose even while the rent rate to labor fell over 1987-2016. Together, these 29 industries were responsible for 73% of the total decline in labor rents over the period. Further, in the majority of these industries - 21 industries, employing around 24% of the private workforce - returns to capital rose by more than rents to labor fell over 1987-2016, implying that the total underlying profits generated by these industries rose, even as rents to labor fell (i.e., the total rent rate rose). These industries - those where the increase in profits to capital was greater than the decline in rents to labor - were responsible for 38% of the total decline in labor rents over 1987-2016. These calculations suggest a substantial role for redistribution in the decline in labor rents.

Figures B.18, B.19, and B.20 break down these statistics at the industry level, showing the profit rate to capital and rent rate to labor in each of 1987-91 and 2012-16.

Figure B.18, showing manufacturing industries, shows that for the majority of manufacturing industries the total rent rate changed very little over the period, but the distribution of those rents changed substantially as rents were redistributed from labor to capital - see, for example, the cases of autos and transportation equipment (Dur_transp), plastics (Nondur_plastic), fabricated metal products (Dur_fab_metal), chemical products (Nondur_chemical), paper products (Nondur_paper), or printing (Nondur_printing). In some industries, total rents were destroyed, with both labor and capital seeing rent destruction: apparel and furniture manufacturing (Nondur_apparel and Dur_furniture) being the most prominent cases (and two of the most exposed to low-wage import competition over the period). A handful of sectors actually saw total rents rise substantially even as labor rents fell, in particular food, beverage and tobacco products (Nondur_food) and petroleum products (Nondur_petro).

Figure B.19 shows the same statistics for Trade, Transportation, Construction, and Utilities industries. Here, the picture is more mixed (and note the different scale on the axis, relative to the manufacturing graph). Retail trade, trucking, and construction (Retail_trade, Transp_truck, Construction) saw relatively large decreases in their total rent rate over this period, but this decrease appeared to have been entirely borne by labor, with little decrease in the profit rate to capital (and even an increase in the profit rate to capital in the case of trucking). Mining industries (Min_oil_and_gas, Min_ex_oil), and passenger transportation (Transp_passenger) saw large increases in their profit rate without an increase in the rent rate to labor. Wholesale trade saw a large increase in its total rent rate even as the rent rate to labor substantially decreased.

Finally, Figure B.20 breaks down the trends for service sector industries. It excludes financial and legal services, since their very low fixed assets means their ratios are off the scale on the graph. Particularly notable here are the substantial declines in the labor rent rates in administrative and support

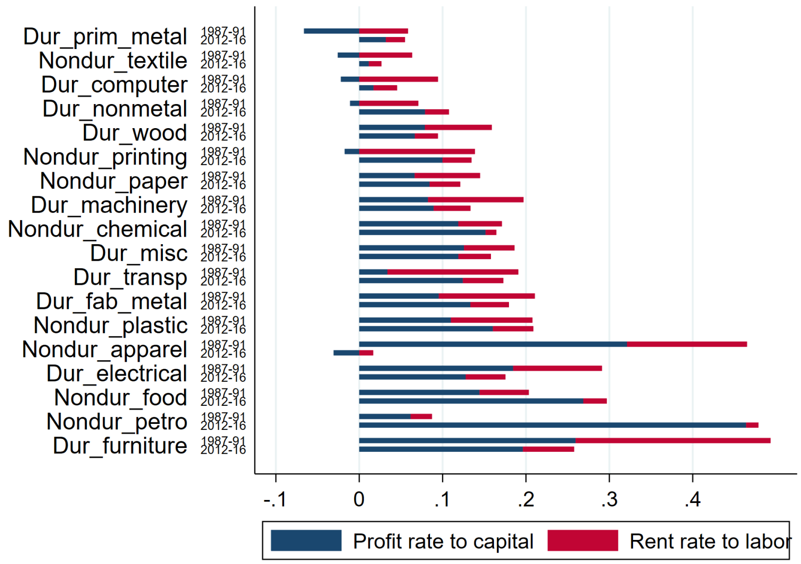



**Figure B.18:** Manufacturing industries: profit rate to capital and rent rate to labor, 1987-91 and 2012-2016

services (Adm_support), publishing (Inf_publishing), and data processing (Inf_data) - which came alongside large decreases in the total rent rate and the profit rate to capital. One industry bucking the trend is computer services (Computer_serv), which saw a large increase in the rent rate to labor over the period.

In Figure B.21, we show the breakdown of the share of the total decline in labor rents accounted for by each industry. The majority of the decline in labor rents can be accounted for by industries in manufacturing, retail and wholesale trade, construction, utilities, and transportation industries.

### B.2.7 Manufacturing industries, rents, and low-wage import penetration

In the main body of the paper, we show that manufacturing industries with bigger increases in low-wage import penetration over 1989-2007 were not the industries with the biggest drops in labor

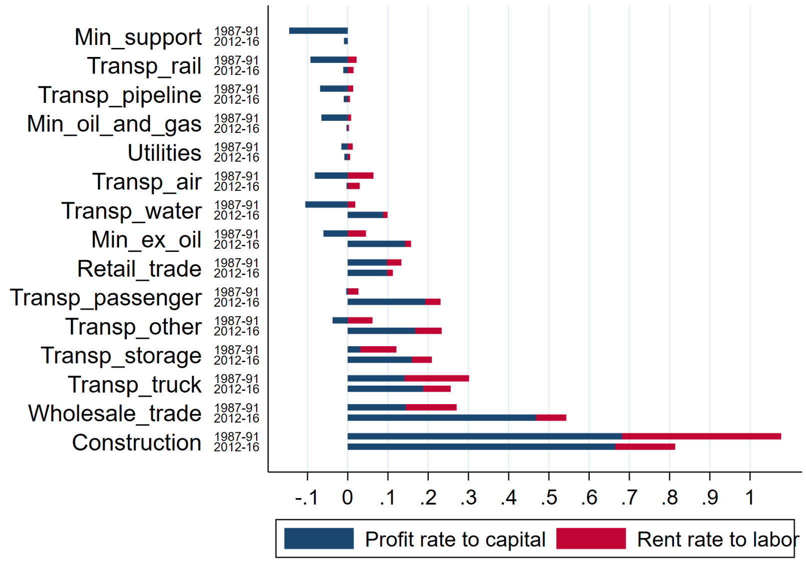



**Figure B.19:** Trade, transportation, construction, and utilities: profit rate to capital and rent rate to labor, 1987-91 and 2012-2016

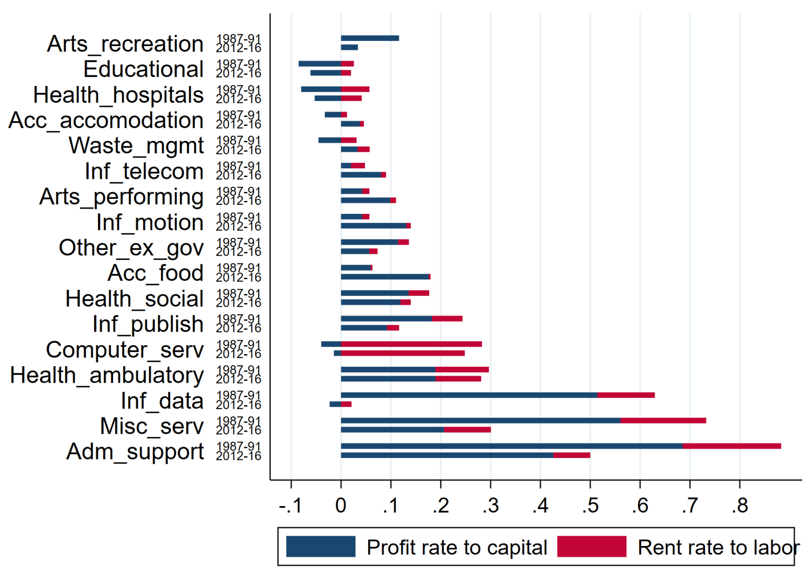



**Figure B.20:** Services: profit rate to capital and rent rate to labor, 1987-91 and 2012-2016

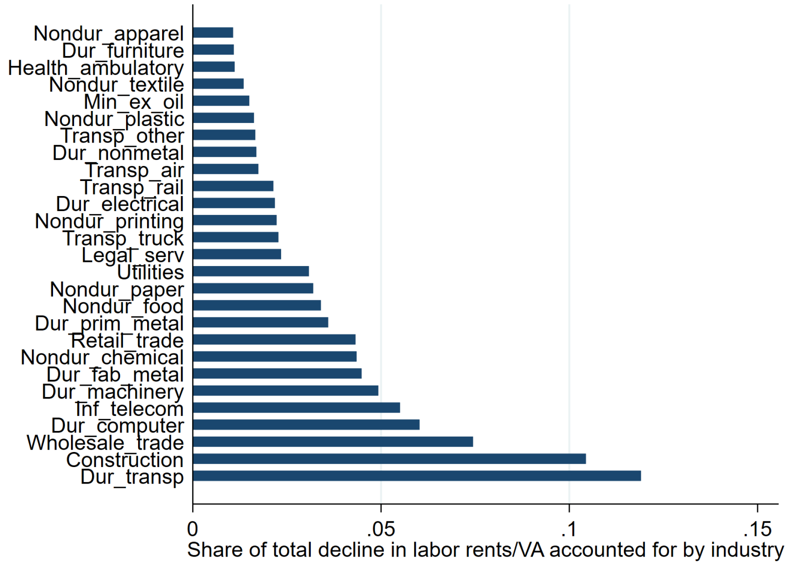



**Figure B.21:** Share of total decline in labor rents as a share of value added, accounted for by each industry (over 1987-91 to 2012-2016)

rents over the period, which would tend to cast doubt on the idea that globalization was primarily responsible for the decline in labor rents over this period. Our data on import penetration from low-wage countries over 1989-2007 is from Bernard, Jensen, and Schott (2006), updated by Peter Schott in 2011 and available on his website. Low-wage import penetration is calculated as the share of domestic sales within each industry represented by imports from low-wage countries, defined as countries with GDP per capita less than 5% of the U.S. level. We study 1989-2007 as this is the period for which we have consistently-defined data on low-wage import penetration. The data is at the NAICS 3-digit industry level, which corresponds to our 18 industry definitions in manufacturing.) with changes in profitability and the log industry wage premium, for 18 manufacturing industries.

We report some additional correlations in the data here. First, as one would expect, the indus-

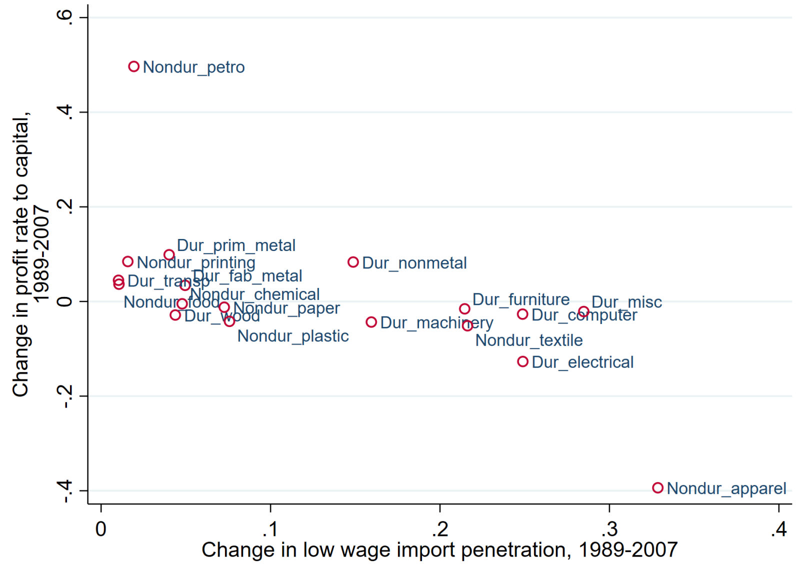



**Figure B.22:** Profit rate to capital and import penetration, manufacturing, 1989-2007

tries which saw bigger increases in import competition saw the biggest declines in the profit rate to capital (Fig. B.22). Why, then, were they not the industries that saw the biggest declines in rents to labor? The answer is that the industries which saw the biggest rises in low-wage import penetration over 1989-2007 were industries which for the most part already had relatively low labor rents in the late 1980s (Figure B.23). Industries with high initial labor rent shares had more labor rents to lose over the 1989-2007 period, as shown in Figure B.24 (though, as a percent of total labor rents, almost all manufacturing industries lost a relatively similar share: about 30%-50% of their labor rents over the period, as shown in Figure B.25). Note, however, that even when controlling for the initial level of labor rents in 1989, there is still no significant relationship between the increase in low-wage import penetration and the change in labor rents over 1989-2007 (and the coefficient is in fact positive).


**Figure B.23:** Initial rent share and change in import penetration, manufacturing, 1989-2007

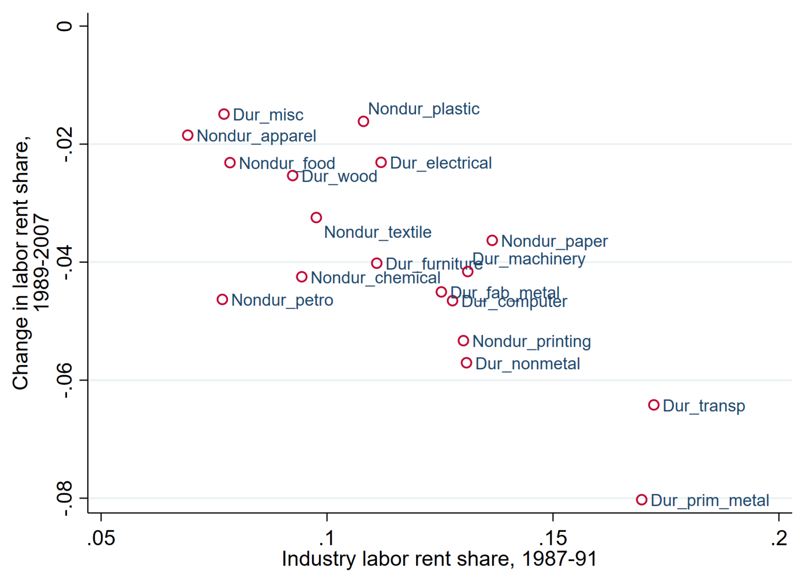



**Figure B.24:** Initial industry labor rent share and change in labor rent share, manufacturing, 1989-2007

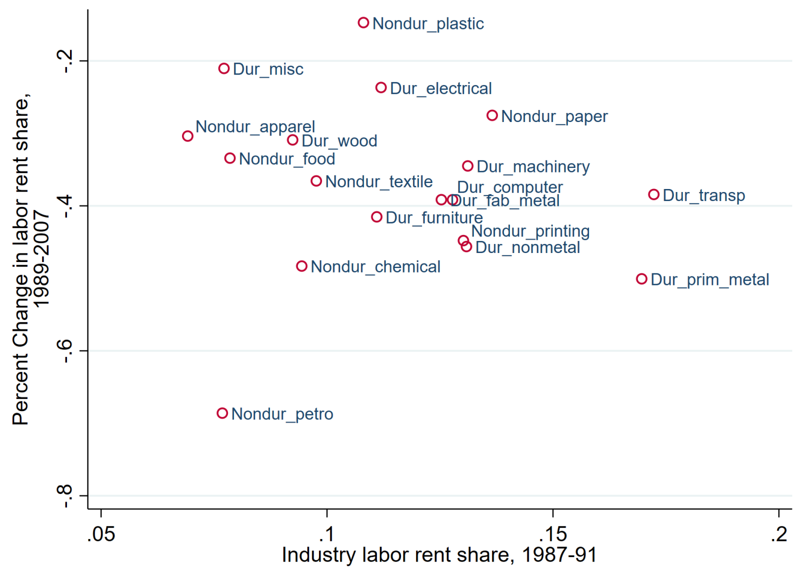



**Figure B.25:** Initial industry labor rent share and percentage change in labor rent share, manufacturing, 1989-2007

## B.3 Additional figures and tables

### B.3.1 Decline in unionization by sector

We calculate unionization rates by SIC (1987) major sector in the CPS-ORG for each year from 1984 to 2019 inclusive. We show the raw declines in unionization by sector in Figure B.26. Indexing the unionization rate in 1984-86 to 100, we show the 3-year moving average of the unionization rate over 1984-2019 in Figure B.27. As can be seen, the proportional rate of decline in unionization was strikingly similar across almost all sectors - Mining, Manufacturing, Transportation and Utilities, Retail Trade, Wholesale Trade, and Construction - particularly until the mid-2000s. Note also that of the three sectors whose unionization rate did not decline as much - Agriculture, FIRE, and Services - Agriculture and FIRE had started with such low initial unionization levels that it would have

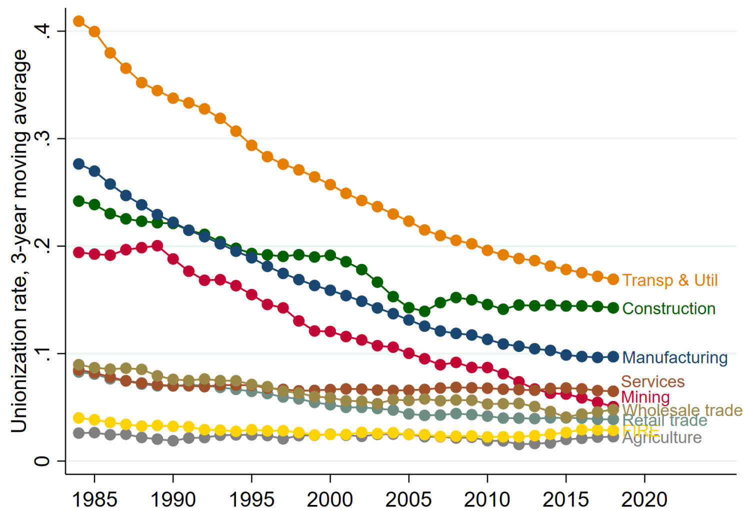



**Figure B.26:** Decline in unionization rates by sector, 1984-2019 (3-year moving average)

been difficult to decline much further. Within Services, the arrest of the decline in unionization rates since 2000 was - compositionally - a result of slow decline in unionization in health, offset by an increase in unionization in education.

### B.3.2 Relationship between wage premia and concentration

Figure B.28 shows the correlation, at the SIC sector level, between average top 20 sales concentration and our estimated log wage premium, for 5-year periods over 1982 to 2012. As the dashed lines of best fit suggest, workers in more concentrated sectors receive higher wage premia on average, but this relationship appears to have weakened. The sector wage premium is calculated as half of the sector log wage fixed effect which we estimate from the CPS-ORG as detailed in the paper. Average concentration in the sector is defined as the (sales-weighted) average Top 20 Sales Concentration Ra-

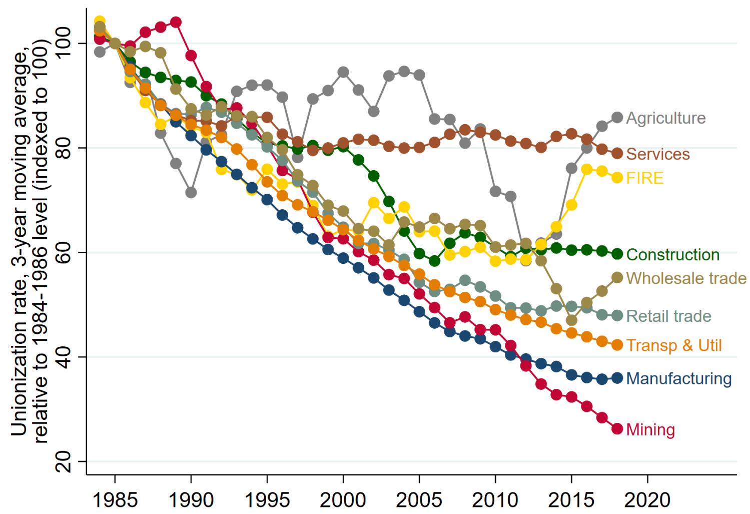



**Figure B.27:** Decline in unionization rates by sector, 1984-2019 (3y ma, indexed to 1984-86)

tio across SIC industries within each sector, 5-yearly from 1982 to 2012. The concentration data is calculated by Autor et al (2020) from Census data; we obtain it from their Figure 4 using WebPlot-Digitizer (Rohatgi 2019). A similar relationship holds if we use the top 4 firm concentration ratio rather than the top 20 firm concentration ratio.

In regressions of concentration on wage premia at the level of our BEA industries (roughly NAICS 3-digit), we find smaller coefficients on concentration in the later period, but the difference is not statistically significant. (We use data on concentration from Covarrubias, Gutierrez, and Philippon (2019), calculated from Compustat data for 1982-2016 and from Census data for 1997, 2002, 2007, and 2012.) Running a similar regression for the NBER CES manufacturing industries (NAICS 6-digit level) over 1997-2012 - regressing the level of top 20 sales concentration (import adjusted) on log average compensation per worker - we find that the coefficient also falls, but the

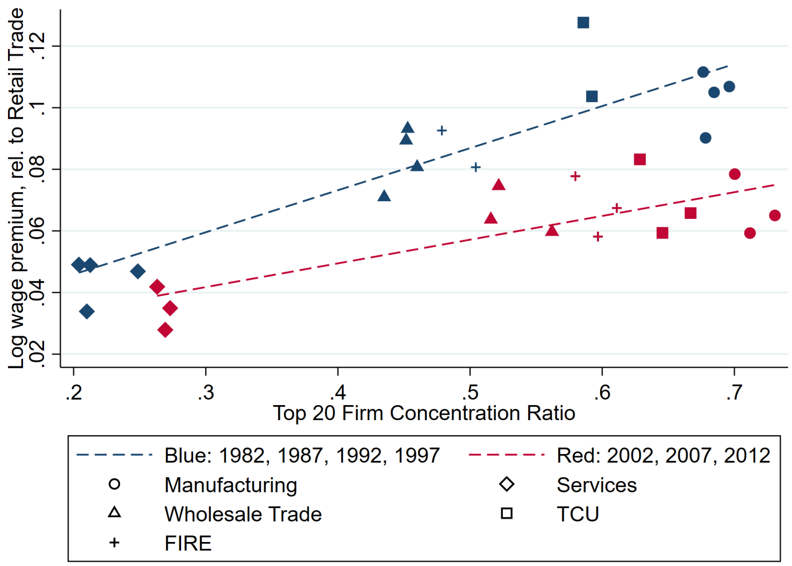



**Figure B.28:** Sector-level relationship between concentration and wage premium
Note: Each point is the sector average log wage premium relative to Retail Trade, calculated from the CPS-ORG, plotted against the average top 20 sales concentration ratio in the sub-industries of each major sector, calculated from Economic Census data by Autor et al (2019).

change is once again not statistically significant.

### B.3.3 Quantifying the rise in outsourcing of cleaning, security, and logistics work

In Figure B.29, we recreate Dorn, Schmieder, and Spletzer's (2018) estimates of the share of workers in cleaning, security, and logistics occupations who were working in the business services sector (indicating having had their work outsourced). Dorn, Schmieder, and Spletzer (2018) identify occupation and industry codes in the Census data which indicate outsourcing to business services, according to both the 1950 and the 1990 occupation and industry codes. Note that the series have

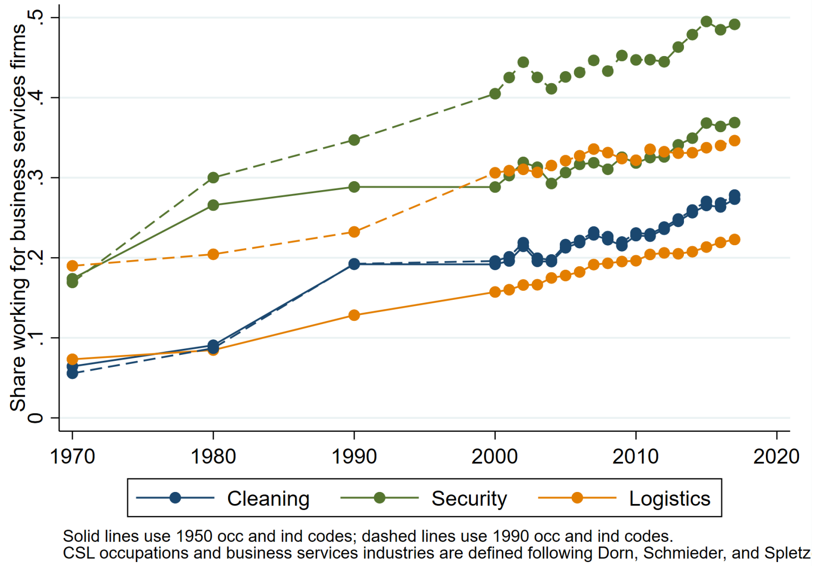



**Figure B.29:** Share of workers in cleaning, security, and logistics working for business services firms (recreation of figure in Dorn, Schmieder, and Spletzer 2018)

different levels as they are calculated using 1950 and 1990 industry and occupation codes, but that the time series trend in both is similar.

### B.3.4 Allocation of workers to high-rent industries

If labor rents have declined, a natural question is whether workers are no longer working in the industries which produce rents. One way to visualize this is to show the share of workers working in industries with different degrees of profitability. Figure B.30 shows the share of workers in industries with different levels of median Tobin's Q (as measured by Covarrubias et al 2019 for publicly-listed companies): there is a noticeable rightward shift, as the median Tobin's Q across industries has increased over the period.[7] Figure B.31 shows the share of workers in industries with different values of gross operating surplus over fixed assets: this shows a slight downward shift, as gross operating surplus / fixed assets was lower in many industries over 2012-2016 than it was in 1987-1991. Both figures show a marked increase in the dispersion of industry profitability across workers.

One might think that part of the rise in inequality within labor as a group has been the result of a change in the distribution of workers across industries with high/low rents. There is suggestive evidence that workers with less education are more likely to work in firms with low rents now than in the past (because of the increased evidence of sorting between high fixed effect workers and high fixed effect firms from AKM models such as Song et al 2019). Does this also happen at the industry level? A preliminary analysis suggests it has not happened at the industry level. Figure B.32 shows the share of college-educated or non-college educated workers employed in industries at each quartile of the distribution of median industry Q (as calculated from Compustat by Covarrubias et al 2019). Similarly, Figure B.33 shows the share of college-educated or non-college educated workers employed in industries at each quartile of the distribution of profitability (gross operating surplus to fixed assets). By these metrics, it does not appear to be the case that there has been a sorting of lower education workers into lower-rent industries.

[7] The pattern is very similar for the equal-weighted and value added-weighted Q across firms within each industry.

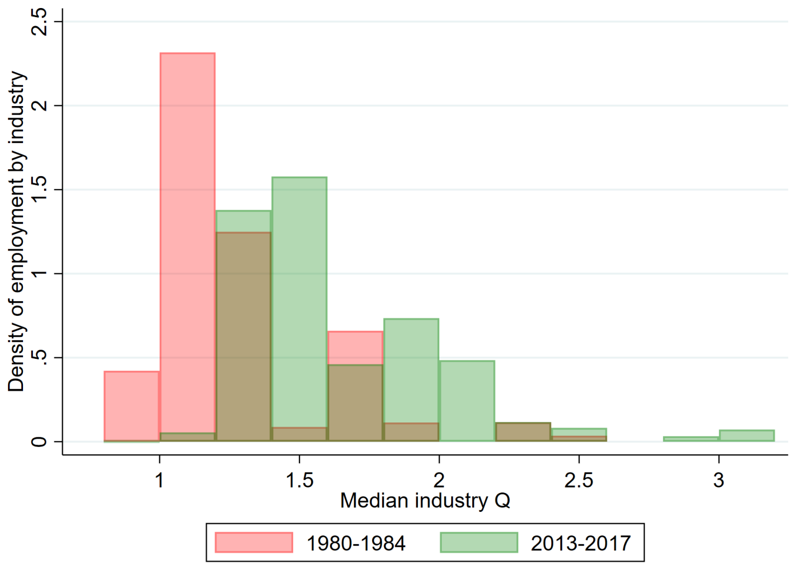



**Figure B.30:** Allocation of employment by industry median Q, 1980-84 and 2013-17

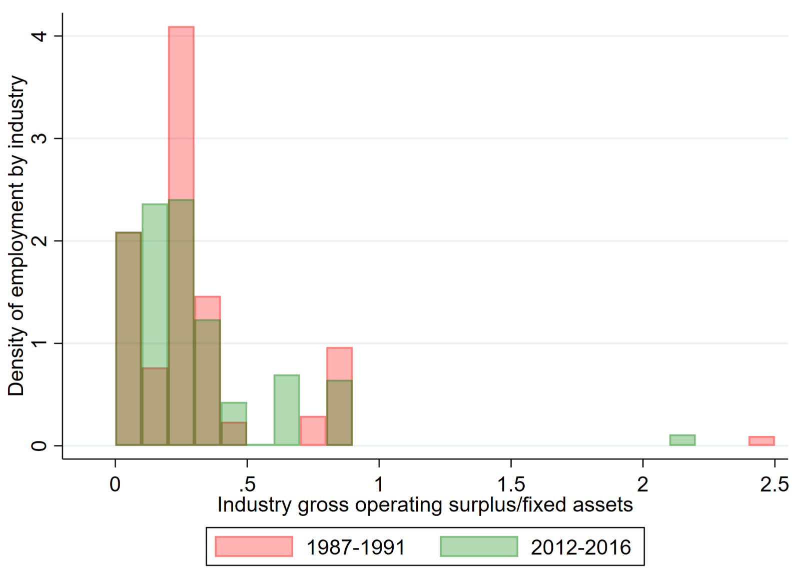



**Figure B.31:** Allocation of employment by industry gross profit rate, 1980-84 and 2013-17

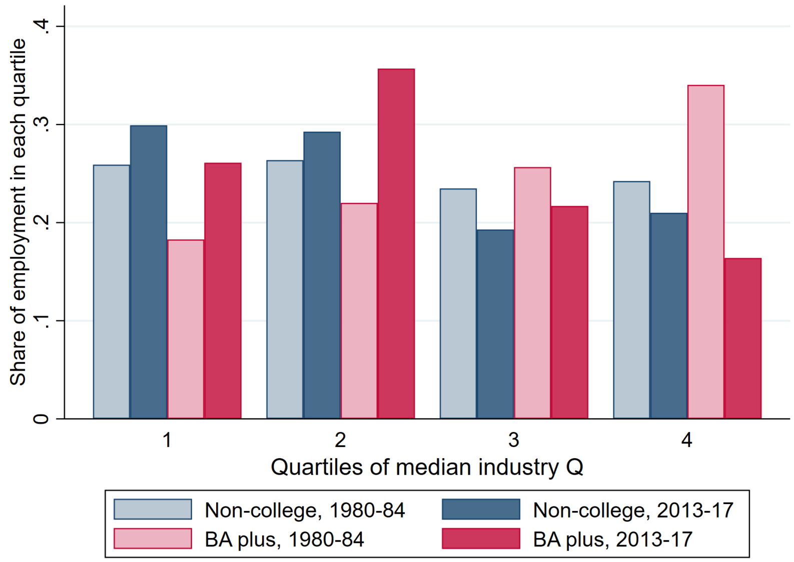



**Figure B.32:** Employment of non-college and college educated workers, by industry median Q, 1980-84 and 2013-17

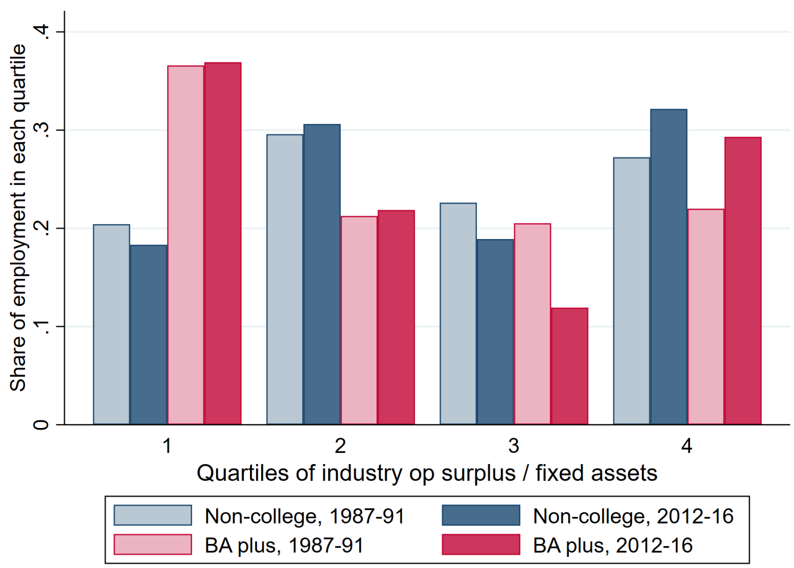



**Figure B.33:** Employment of non-college and college educated workers, by industry gross profitability, 1987-91 and 2012-16

### B.3.5 Labor rents by college/non-college

The decline in rent-sharing has affected non-college workers more than college workers. While industry rents declined in a relatively similar way for college and non-college workers, non-college workers were disproportionately affected by the fall in unionization and by the fall in large firm wage effects.

Figure B.34 shows the private sector union coverage rate for workers with high school or with some college education, vs. workers with a four year college degree or postgraduate education. The decline has been much sharper for workers without college education. Figure B.35 shows the estimated private sector union log wage premium for workers with no college vs. four-year college education (estimated from the CPS-ORG using the methodology described in the first section of this Appendix). Non-college workers have substantially higher union wage premia than college-educated workers, which makes the decline in unionization more costly for them.

Figure B.36 shows the private sector firm wage effect, split by college and non-college workers. While the wage effect from medium sized firms (100-499 workers) stayed roughly constant for both groups over the period, the entire fall in the large firm wage premium (500+ workers) was concentrated on non-college workers.

Figure B.37 shows that industry wage premia mostly moved in tandem for workers with and without college educations.

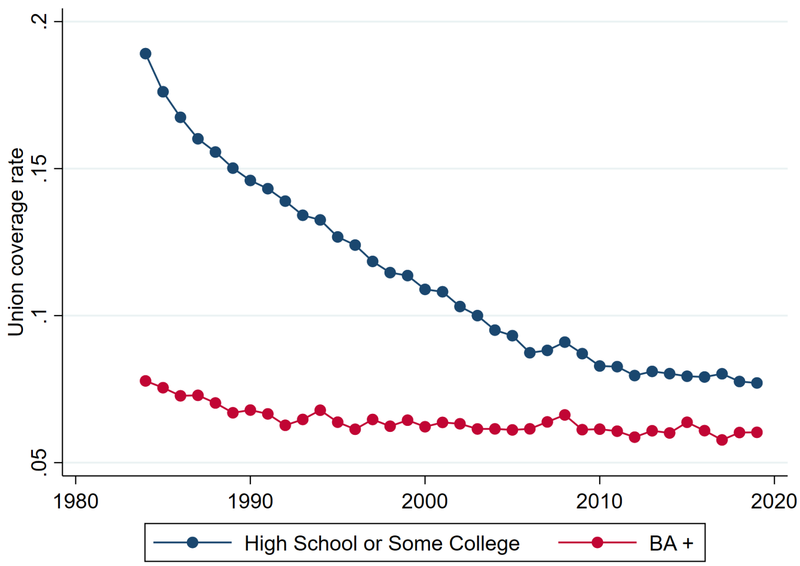



**Figure B.34:** Private sector union coverage rates, by education group

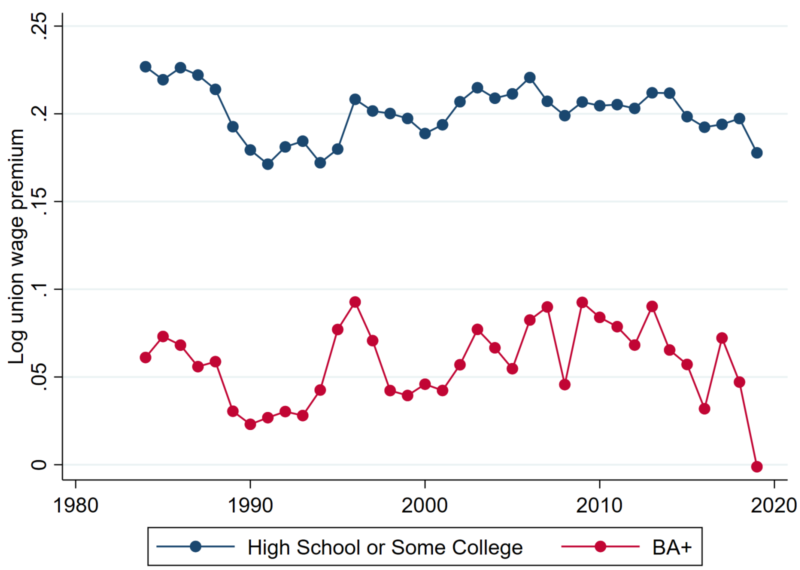



**Figure B.35:** Private sector union log wage premium, by education group

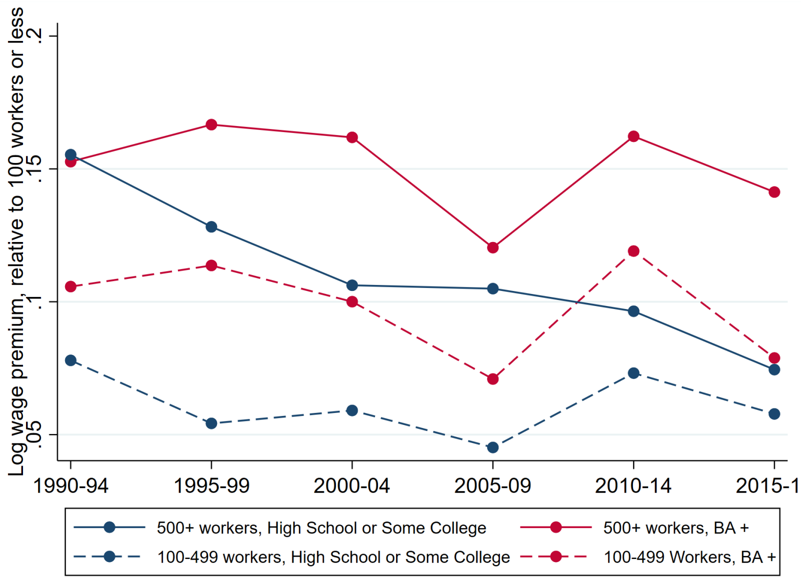



**Figure B.36:** Private sector large firm wage effects, by education group

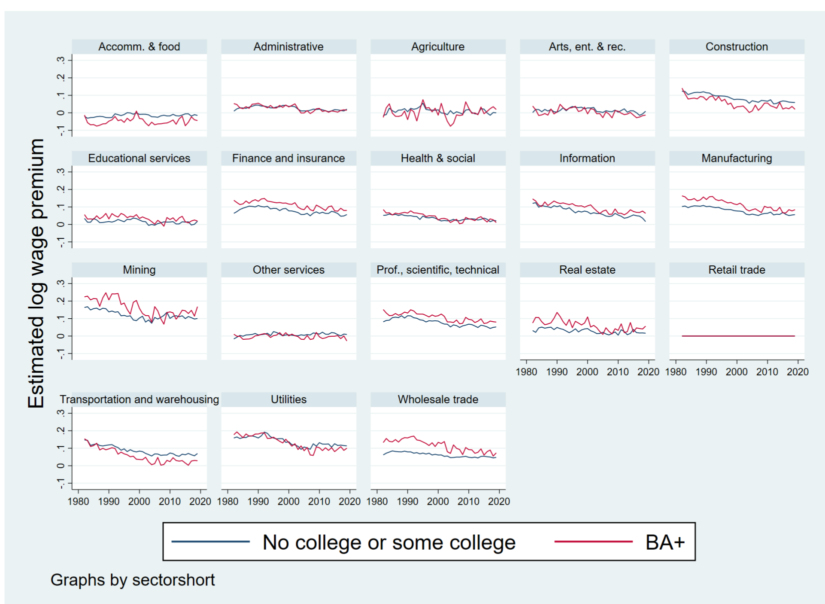



**Figure B.37:** Industry wage premia, by education group

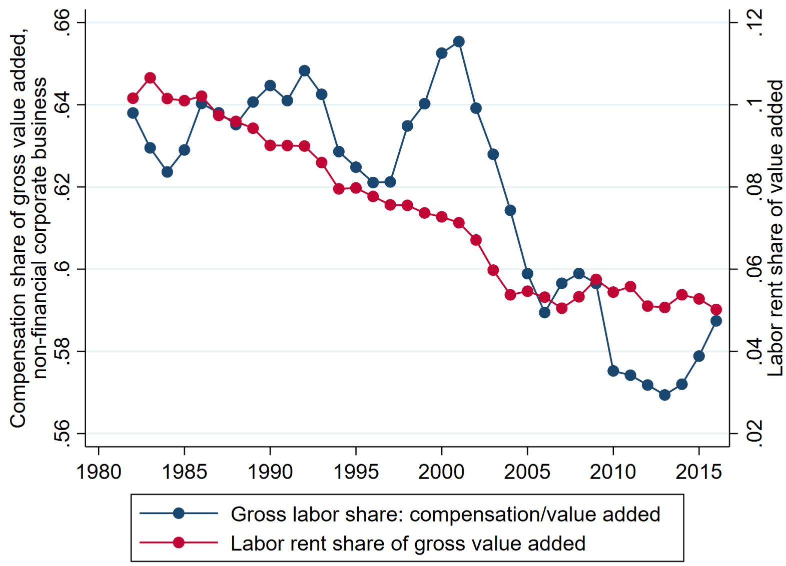



**Figure B.38:** Labor rent share and compensation share of gross value added, nonfinancial corporate

### B.3.6 Decline in labor share, rise in capital share, and decline in investment-profits: gross measures

In the paper, we focus on measures of the labor share, capital share, and investment-profit ratio net of depreciation, for the nonfinancial corporate sector. Figures B.38, B.39, and B.40 below replicate Figures 2.7, 2.8, and 2.18 in the paper, but for gross measures (without incorporating the effects of depreciation).

### B.3.7 Industry-level analysis: unionization

In Table B.2, we replicate Table 2.4 in the main paper, but using as our measure of worker power the industry unionization rate instead of our measure of imputed labor rents. The general pattern of

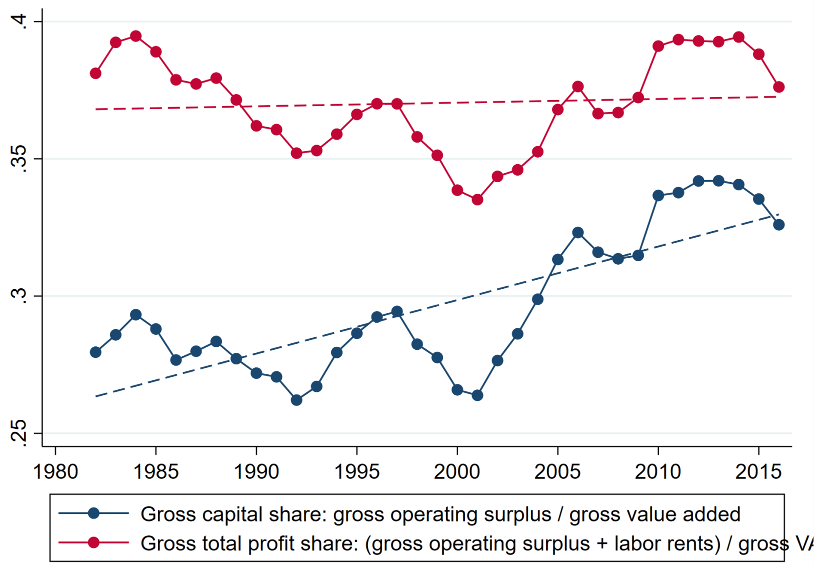



**Figure B.39:** Capital share and "total profit" share of gross VA, nonfinancial corporate

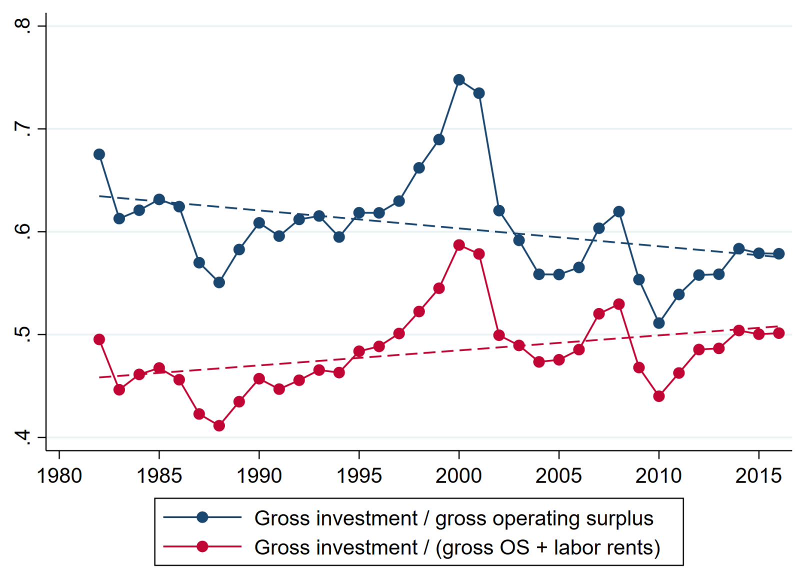



**Figure B.40:** Gross investment to operating surplus, nonfinancial corporate results is similar to that in the main paper using labor rents.

We note in particular that our regressions in Table B.2 show a positive, relatively large, and statistically significant relationship between the industry unionization rate and labor share over 1987-2016. This is similar to the findings in Young and Zuleta (2017), albeit with a slightly different industry definition, timeframe, and regression specification.

Elsby, Hobijn, and Sahin (2013) find a positive correlation between the change in the union coverage rate and the change in the payroll share of value added over 1987-2011 across industries. They find that a 1 percentage point decline in the union coverage rate over the period was non-significantly associated with a 0.2 percentage point decline in the labor share, and argue that the point estimate suggests that the decline in unionization can only explain a small amount of the variation in the decline in the labor share. Since they do not emphasize the role of unions, based on their findings, it is worth comparing our results with theirs.

Therefore, we also carry out a very similar regression to Elsby et al (2013), over our slightly longer sample period - regressing the percentage point change in the compensation share of gross and net value added from 1987-91 to 2012-16 on the percentage point change in the union coverage rate from 1987-91 to 2012-16 at the our BEA industry code level, weighting each observation by the industry's average share of value added over 1987-2016. The point estimates are 0.54 for the gross labor share, and 0.66 for the net labor share, both regressions with a p-value of 0.001 and R-squared of 0.19 - suggesting that nearly 20% of the variation in the industry-level labor share over the period can be explained by changes in unionization alone. This is visualized in Figure B.41.

**Table B.2:** Industry-Level Regressions, using unionization instead of imputed labor rents

*Panel A: Regressions of labor shares and investment-profit on labor rent share and Compustat concentration. N = 1,189 (41 industries, 1987-2016)*

| Dependent variable: | Labor share of gross value added | | | | Labor share of net value added | | | | Investment to profit ratio | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Industry unionization rate | 0.09 | 0.03 | 0.43** | 0.05 | 0.24 | 0.19 | 0.58** | 0.27 | 0.15 | 0.14 | 0.31 | 0.34 |
| | -0.22 | -0.24 | -0.1 | -0.13 | -0.27 | -0.29 | -0.19 | -0.18 | -0.31 | -0.33 | -0.26 | -0.61 |
| Avg top 20 sales concentration, | -0.20* | -0.19* | -0.05 | -0.04 | -0.13 | -0.12 | -0.11 | -0.09 | 0.28 | 0.29 | -0.17 | -0.16 |
| imp-adj (Compustat) | -0.08 | -0.08 | -0.07 | -0.07 | -0.1 | -0.1 | -0.12 | -0.13 | -0.21 | -0.22 | -0.22 | -0.23 |
| Fixed effects | None | Year | Ind | Yr, Ind | None | Year | Ind | Yr, Ind | None | Year | Ind | Yr, Ind |

*Panel B: Regressions of profitability on labor rent share and Compustat concentration. N=1,189 (41 industries, 1987-2016)*

| Dependent variable: | Gross profit rate | | | | Aggregate Q | | | | Median Q | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Industry unionization rate | -0.30+ | -0.33+ | -0.02 | -0.12 | -1.45** | -1.37** | -0.93* | 0.55 | -1.14** | -1.01** | -1.39** | -0.02 |
| | -0.15 | -0.17 | -0.14 | -0.16 | -0.39 | -0.41 | -0.43 | -0.65 | -0.31 | -0.32 | -0.42 | -0.5 |
| Avg top 20 sales concentration, | -0.07 | -0.07 | 0.03 | 0.03 | 0.19 | 0.17 | -0.31 | -0.3 | 0.32* | 0.30+ | 0.15 | 0.16 |
| imp-adj (Compustat) | -0.11 | -0.11 | -0.13 | -0.13 | -0.15 | -0.15 | -0.31 | -0.31 | -0.15 | -0.15 | -0.21 | -0.2 |
| Fixed effects | None | Year | Ind | Yr, Ind | None | Year | Ind | Yr, Ind | None | Year | Ind | Yr, Ind |

*Panel C: Regressions of labor shares and investment-profit on labor rent share and Census concentration. N = 174 (45 ind. for 1997, 2002, '07, '12)*

| Dependent variable: | Labor share of gross value added | | | | Labor share of net value added | | | | Investment to profit ratio | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Industry unionization rate | -0.06 | -0.1 | 0.43** | 0.43** | 0.05 | 0.01 | 0.76** | 0.90** | -0.14 | -0.25 | 1.36 | 1.58 |
| | -0.25 | -0.27 | -0.13 | -0.14 | -0.3 | -0.32 | -0.26 | -0.28 | -0.89 | -0.98 | -0.86 | -1.12 |
| Avg top 20 sales concentration, | -0.46** | -0.45** | -0.34** | -0.37** | -0.33* | -0.32+ | -0.72** | -0.80** | 0.61 | 0.65 | -1.09 | -1.16 |
| imp-adj (Census) | -0.12 | -0.13 | -0.11 | -0.11 | -0.16 | -0.16 | -0.16 | -0.15 | -0.62 | -0.65 | -0.68 | -0.74 |
| Fixed effects | None | Year | Ind | Yr, Ind | None | Year | Ind | Yr, Ind | None | Year | Ind | Yr, Ind |

*Panel D: Regressions of profitability measures on labor rent share and Census concentration. N = 174 (47 ind. for 1997, 2002, '07, '12)*

| Dependent variable: | Gross profit rate | | | | Aggregate Q | | | | Median Q | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Industry unionization rate | -0.70* | -0.77* | -0.08 | -0.45* | -0.96+ | -1.22* | 1.08 | -0.15 | -0.96* | -0.93* | -0.64+ | -0.67 |
| | -0.34 | -0.38 | -0.21 | -0.2 | -0.53 | -0.55 | -0.8 | -1 | -0.38 | -0.4 | -0.36 | -0.51 |
| Avg top 20 sales concentration, | -0.29 | -0.27 | 0.41 | 0.55+ | -0.27 | -0.16 | -1.51+ | -0.83 | 0.13 | 0.14 | -0.76 | -0.3 |
| imp-adj (Census) | -0.27 | -0.27 | -0.28 | -0.28 | -0.28 | -0.29 | -0.83 | -0.72 | -0.21 | -0.22 | -0.45 | -0.4 |
| Fixed effects | None | Year | Ind | Yr, Ind | None | Year | Ind | Yr, Ind | None | Year | Ind | Yr, Ind |

Robust standard errors, clustered at industry level, in parentheses. + p<0.10, * p<0.05, ** p<0.01.

Note: This table is a replica of Table 2.4 in the main paper, but using the industry unionization rate instead of imputed labor rents as the measure of worker power at the industry level. Investment-profits are 98% winsorized. Regressions are for 41/45 industries because we do not have concentration data for all 51 non-financial industries.

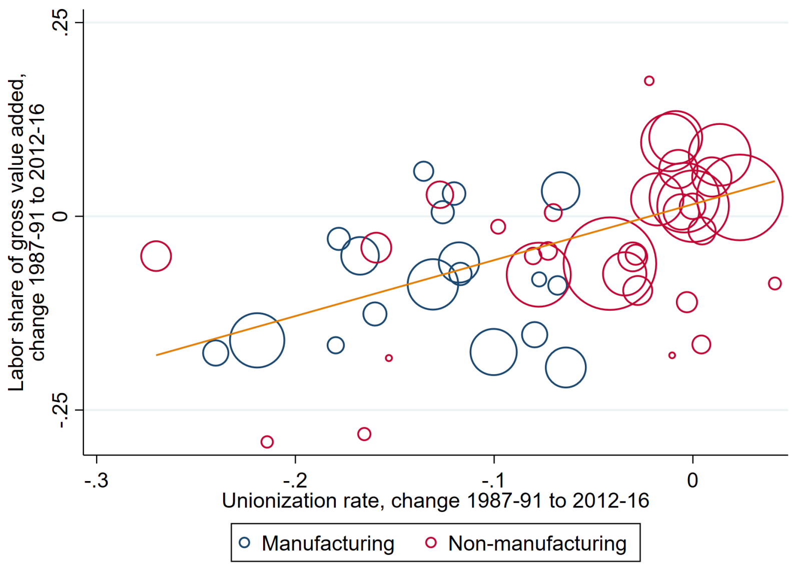



**Figure B.41:** Change in unionization rate and change in labor share, by industry, 1987-2016
Note: Bubble size represents industry share of value added, average over 1987-2016.

### B.3.8 Relationship between industry, union, and firm size rents

We note in the paper that our measure of the union rent share only captures the direct effect of unions on unionized workers' wages, relative to non-unionized workers' wages. It is possible that our estimates of the industry and firm size rent shares pick up the "union threat effect", by which the possibility of unionization, or norms set by unions, raise wages even at non-unionized firms.

Industry- and firm-level trends in industry, union, and firm size rent shares are consistent with this. Industries which saw bigger declines in their union rent share also saw bigger declines in their industry rent share (Figure B.42). At the level of 52 BEA industries, regressing the change in the imputed industry rent share over 1987-91 to 2012-16 on the change in the imputed union rent share over the same period gives a coefficient of 1.01, with a standard error of 0.16 and an R-squared of 43%. This also holds when regressing the industry rent share on the union rent share at an annual frequency, controlling for industry and year fixed effects and with standard errors clustered at the industry level: the coefficient estimate is 0.84, with a standard error of 0.11.

At the state level, we can perform similar analyses. Regressing the change in the imputed industry rent share over 1984-88 to 2012-16 on the change in the imputed union rent share over the same period gives a coefficient of 0.70, with a standard error of 0.23 and an R-squared of 16%. Regressing the change in the imputed firm size rent share over 1984-88 to 2012-16 on the change in the imputed union rent share over the same period gives a coefficient of 0.49, with a standard error of 0.12 and an R-squared of 25%. These results are visualized in Figures B.43 and B.44. Regressions of the industry and firm size rent share on the union rent share at an annual frequency, controlling for state and year fixed effects and with standard errors clustered at the state level, gives coefficients (standard errors) of 0.37 (0.15) and 0.28 (0.09) respectively.

### B.3.9 Labor rents, unemployment, and labor market tightness

In the main body of the paper we present evidence of a significant negative relationship between labor rents and unemployment at the state and industry level. Following Figura and Ratner (2015), we can also use JOLTS data on vacancy rates over 2000-2019 to test whether industries with bigger falls in our measures of labor power saw bigger increases in labor market tightness (vacancies/unemployment). The JOLTS database only reports data on relatively aggregated industries - 15 in total. As shown in Figures B.45 and B.46, the industries with the biggest falls in unionization rates over 2000/01-2018/19, or biggest falls in labor rent shares over 2000/01-2015/16, also saw the biggest increases

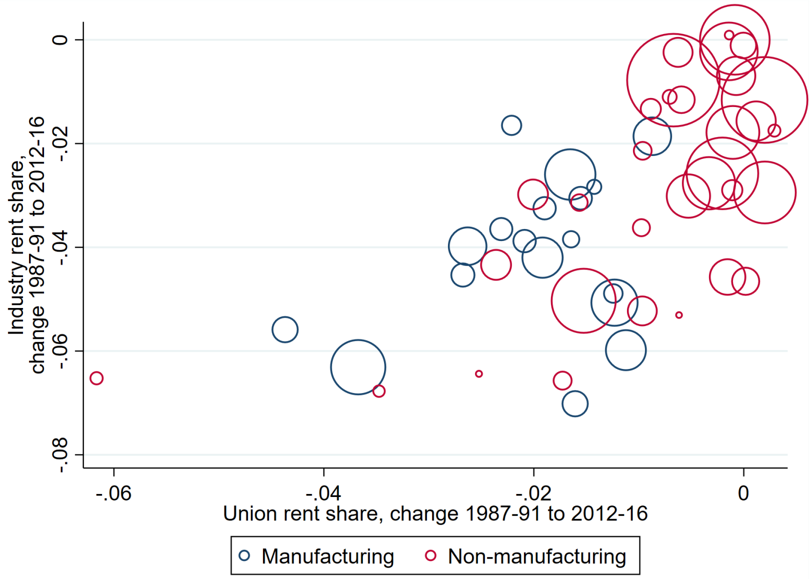



**Figure B.42:** Change in industry rent share and union rent share, by industry, 1987-2016
Note: Bubble size represents industry share of employment, 2012-2016 average. Graph shows 52 industries at the BEA industry code level.


**Figure B.43:** Change in industry rent share and union rent share, by state, 1987-2016

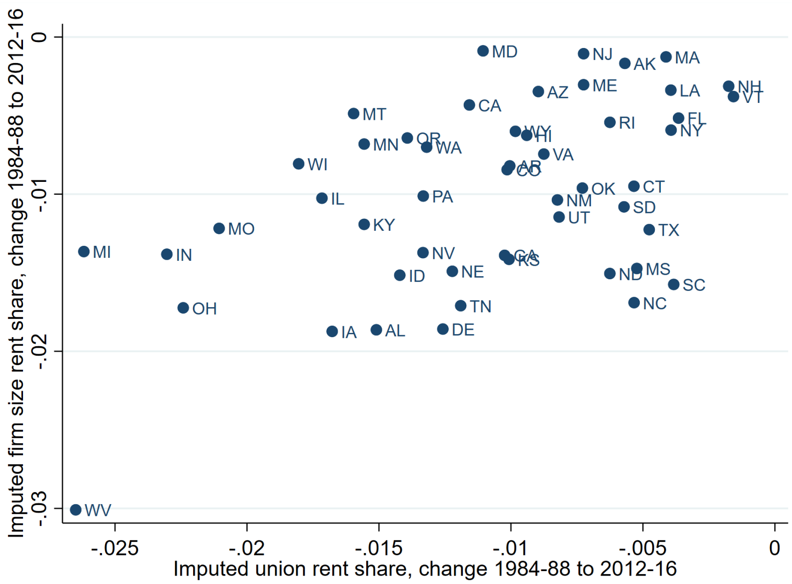



**Figure B.44:** Change in firm size rent share and union rent share, by state, 1987-2016

in labor market tightness. (Note that 2000/01 and 2018/19 are particularly appropriate years to compare because aggregate V/U and unemployment was very similar in the two periods). In annual regressions of labor market tightness on measures of worker power over 2000-2016, with industry fixed effects, we similarly find that lower unionization rates or labor rent shares are significantly associated with higher vacancy-unemployment ratios. The coefficients suggest that the average fall in unionization was associated with a 10pp higher VU ratio, and the average fall in imputed labor rent share was associated with a 15.7pp higher VU ratio.

We also show in Figure B.47 that industries with bigger falls in their unionization rate tended to see bigger falls in their unemployment rate over 1984-2019. (Analogous to Figure 15 in the main paper, which shows that industries with bigger falls in their imputed labor rent share tended to see bigger falls in their unemployment rate over 1988-2016).

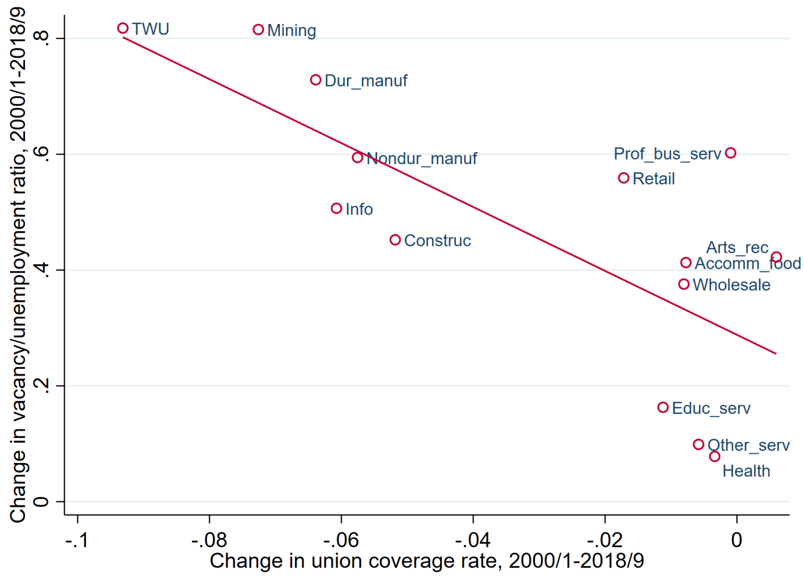



**Figure B.45:** Change in labor market tightness and the unionization rate, 2000-2019, by industry

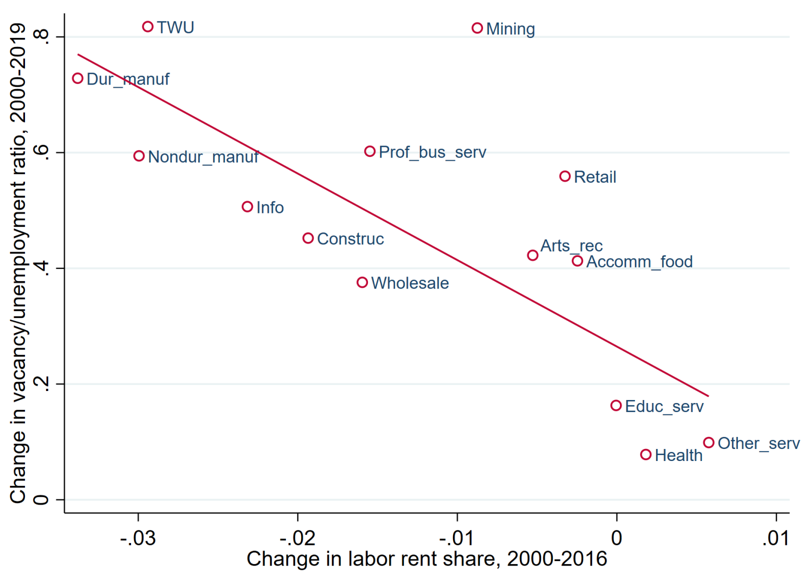



**Figure B.46:** Change in labor market tightness and imputed labor rent share, by industry

Notes to Figures B.45 and B.46: Each dot is an industry (at the level of 15 JOLTS industry categories). Note that the observations for 2000-2001 are averages of monthly data from December 2000-December 2001 inclusive, as the JOLTS data only starts in December 2000. The observations for 2018-2019 are averages of monthly data from January 2018 to October 2019 inclusive. Red line is an employment-weighted line of best fit.

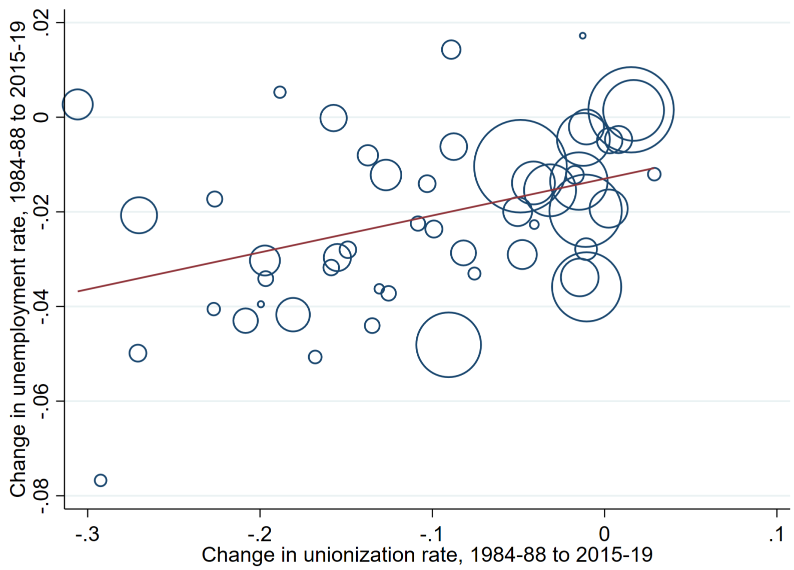



**Figure B.47:** Change in unemployment and the unionization rate, by industry

### B.3.10 Investment to profits and labor rents

In the main body of the paper we show that at the aggregate level, the decline in labor rents can explain the apparent decline in the ratio of investment to operating surplus in the nonfinancial corporate sector, and show that the ratio of investment to total profits - operating surplus, plus labor rents - has hardly declined at all. In Figure B.48, we show that at the industry level, industries with larger declines in their labor rent share also saw larger relative falls in their investment-to-operating surplus ratio over 1988-2016. On the other hand, there is no relationship between top 20 import-adjusted sales concentration and the investment-to-profit ratio (Figure B.49).


**Figure B.48:** Change in investment-profits and imputed labor rent share, by industry
Notes: Each bubble is an industry (at the BEA industry code level), where the size of the bubble represents industry average employment over 2012-2016. The red line is an employment-weighted line of best fit.

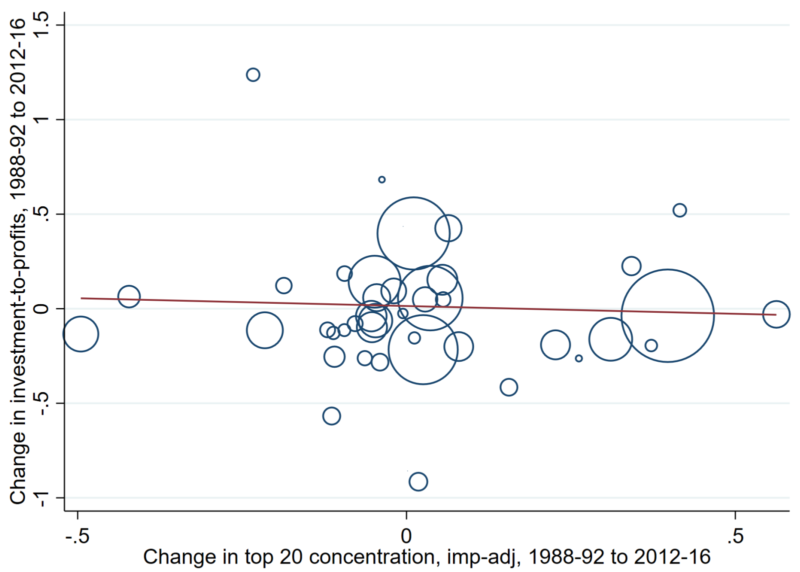



**Figure B.49:** Change in investment-profits and top 20 sales concentration (imp-adj), by industry
Notes: Each bubble is an industry (at the BEA industry code level), where the size of the bubble represents industry average employment over 2012-2016. The red line is an employment-weighted line of best fit.

### B.3.11 Can the decline of labor rents account for the rise in the income share of the top 1%?

We calculate that labor rents as a % of gross value added in the nonfinancial corporate sector were 10.1% in 1982 and 5.0% in 2016. Nonfinancial corporate sector gross value added was a little less than 2/3 of national income over this period (65% in 1982, 58% in 2016, according to the BEA NIPA), which implies that labor rents declined by 3.7% of national income over 1982 to 2016.

Different authors come to quite different estimates for the magnitude of the increase in the income share of the top 1% over the last forty years, and the estimates are quite dependent on a number of methodological choices. Rather than take a stance on these choices, we use two of the most prominent recent estimates: Auten and Splinter (2019) estimate that the top 1% pre-tax income share rose by 4.9 percentage points over 1979 to 2014, while Piketty, Saez, and Zucman (2018) estimate that it rose by 9 percentage points.[8]

We perform two exercises with these data.

1. We assume that all labor rents that we measure accrued to the bottom 99% in the past, and were redistributed to the top 1% (whether as capital or labor income). In this case, our measure of the decline in labor rents could account for 3.7 of the 4.9 to 9 percentage points increase in the income share of the top 1%, so from 41% to 76% of the increase.

2. We assume that labor rents were redistributed as capital income across the entire income distri-

[8] Auten and Splinter (2019) note that, since top income shares are very cyclical, one should compare similar points in the business cycle when looking at long changes in top income shares. They therefore suggest comparing 1979 and 2014.

bution (rather than just to the top 1%), in proportion to the distribution of capital income arising from firm ownership in 2016 (as estimated by Piketty, Saez, and Zucman 2018). Since the top 1% received 59% of total capital income in 2016, this would imply an increase in labor rents to the top 1% of income earners of 3.7*0.59 = 2.2% of national income, accounting for 24%-45% of the increase in the income share of the top 1% over recent decades.

Note that we are inclined to think our measure of the decline of labor rents in the nonfinancial corporate sector may be an underestimate of the decline of labor rents as a share of national income, since (a) labor rents may also have fallen in finance and in the non-corporate business sector, (b) union premia for non-wage compensation are greater than that for wages, but we applied the union wage premium to non-wage compensation, and (c) the evidence appears to suggest that the union threat effect has positive spillover effects on other non-union wages, and our estimates may not capture all of this. This would suggest that our calculations above may be underestimates of the degree to which the decline in labor rents could account for the rise in the top 1% income share.

## B.4 Further details on modified Farhi/Gourio accounting decomposition

### B.4.1 Detailed writeup of modified Farhi/Gourio accounting decomposition

This section contains a more detailed writeup of our modified accounting decomposition based on Farhi and Gourio (2018). We are grateful that Farhi and Gourio provided their replication data and code online, such that it was easy to carry out our modified version of their decomposition.

Farhi and Gourio (2018) document six stylized macro-finance facts over recent decades:

1. Falling real risk-free interest rates

2. Rising profitability of private capital

3. Increasing valuation ratios

4. Slight fall in investment/output and investment/capital ratios

5. Slowing TFP and investment-specific productivity growth, and falling employment-population ratio

6. Falling labor share

They then decompose the degree to which these can be explained by five different factors: rising market power, rising unmeasured intangibles, rising risk premia, increased savings supply, and a slowdown of technological progress. Their model is an otherwise standard neoclassical growth model which incorporates macroeconomic risk, monopolistic competition, and the potential for mismeasurement of intangible capital. Their framework, however, does not take into account the possibility that workers may share in some of the rents generated by product market power, and that the degree of rent-sharing may have changed over time.

We incorporate a simple version of rent-sharing into the baseline Farhi/Gourio accounting framework (which does not include intangible capital). Farhi and Gourio find that rising market power plays a role in explaining the macro-finance facts of recent decades, but they implicitly hold the degree of worker rent-sharing constant in their analysis (at zero). We do the opposite: we hold the degree of firm output market power constant in our analysis (setting the average markup at 1.15, the level that Farhi/Gourio estimate for the 2001-2016 period), and allow the degree of worker rent-sharing to vary.

We incorporate rent-sharing between labor and capital in the simplest way possible: the monopolistic firm still maximizes profits as before, hiring labor and capital in a competitive market. It then shares the rents or 'pure profits' between capital and labor, with share $\pi_L$ going to labor. A decline in rent-sharing is modeled by a decline in $\pi_L$. This reduced-form approach can be micro-founded with an efficient bargain type model (Solow and MacDonald 1981) where workers, seeking to maximize total pay to labor, and shareholders, seeking to maximize their profits, jointly bargain over the firm's production decisions. Alternatively, the firm could be considered to be jointly managed in the (weighted average) interests of workers and shareholders.

Firm production decisions are the same in our framework as they would be in the Farhi/Gourio model without rent-sharing. This means that only a few equations change relative to the Farhi/Gourio model. We show these below: Equation (20), the labor share:

$$s_L = \frac{w_t N_t}{Y_t} + \frac{\pi_L \left(Y_t - w_t N_t - R_t K_t\right)}{Y_t} = \frac{1 - \alpha + \pi_L \left(\mu - 1\right)}{\mu}$$

Equation (21), the measured capital share:

$$s_K = 1 - s_L = \frac{\alpha + \left(1 - \pi_L\right)\left(\mu - 1\right)}{\mu}$$

The measured capital share can be decomposed into the share representing monopoly rents, $s_\Pi$, and the 'true' capital share corresponding to remuneration for capital ownership, $s_C$:

$$s_\Pi = \frac{(1-\pi L)(\mu-1)}{\mu} s_C = \frac{\alpha}{\mu}$$

Equation (24), Tobin's Q:

$$\text{Tobin's Q} = \frac{P_t}{K_t/Q_t} = (1+g_T)\left(1+(1-\pi_L)\frac{\mu-1}{\alpha}\frac{r^*+\delta+g_Q}{r^*-g_T}\right)$$

Equation (26), the marginal product of capital:

$$MPK_t = \frac{\Pi_t}{K_t/Q_t} = \left(\frac{(1-\pi_L)(\mu-1)+\alpha}{\alpha}\right)\left(r^*+\delta+g_Q\right)$$

Equation (27), the spread between the marginal product of capital and the risk-free rate:

$$MPK - r_f = \delta + g_Q + \frac{(1-\pi_L)(\mu-1)}{\alpha}\left(r^*+\delta+g_Q\right) + r^* - r_f$$

The implications of our modifications for the comparative statics are shown in Table B.3. As can be seen, there are only two differences in sign for key measurable moments of the data: lower-rent sharing is not predicted to affect the investment-output or capital-output ratios, whereas higher markups cause them to fall. In the US data, the investment-output ratio has fallen only very slightly and the share of non-residential investment in GDP has not fallen at all over 1984 to 2016. Meanwhile, the capital-output ratio has risen slightly (see Farhi and Gourio Table 1).

**Table B.3:** Different predictions of FG vs. SS

| | Higher Markups $\mu$ | Lower rent-sharing $\pi_L$ |
|---|---|---|
| Labor share | $\downarrow$ | $\downarrow$ |
| 'True' capital share | $\downarrow$ | no change |
| Pure profit share | $\uparrow$ | $\uparrow$ |
| Investment-output ratio | $\downarrow$ | no change |
| Capital-output ratio | $\downarrow$ | no change |
| Spread between ret. on K and RF rate | $\uparrow$ | $\uparrow$ |
| Tobin's Q | $\uparrow$ | $\uparrow$ |

Farhi and Gourio estimate nine key parameters in their model, targeting nine key moments, for the periods 1984-2000 and 2001-2016. We denote their baseline accounting decomposition "FG". The parameters they estimate are:

- $\beta$, the discount factor
- p, the probability of an economic crisis or "disaster"
- $\delta$, the depreciation rate of capital
- $\alpha$, the Cobb-Douglas parameter
- $g_P$, the growth rate of the population
- $g_Z$, the growth rate of TFP
- $g_Q$, the growth rate of investment-specific productivity
- $\bar{N}$, the labor supply parameter
- $\mu$, the markup

These parameters are estimated targeting nine moments:

- Gross profitability $\frac{\Pi}{K}$
- Gross share of income going to capital $\frac{\Pi}{Y}$
- Investment-capital ratio $\frac{I}{K}$
- Risk-free rate RF
- Price dividend ratio PD
- Growth rate of population
- Growth rate of TFP
- Growth rate of investment prices
- Employment-population ratio

We replicate the baseline Farhi/Gourio ("FG") decomposition. We then modify the Farhi/Gourio approach to allow for changing rent-sharing between capital and labor, instead of changing total rents (monopoly power). To do this, we hold the markup constant at 1.15, which is the level of the markup that Farhi/Gourio estimate for the second period in their study (2001-2016). We instead allow the parameter governing rent-sharing with labor to change ($\pi_L$), and estimate this alongside the other 8 Farhi/Gourio parameters, targeting the same empirical moments. We denote this approach as "SS" going forward.

Identification in our modified accounting decomposition is nearly identical to that in Farhi/Gourio. As with theirs, the identification is nearly recursive. Some parameters are obtained directly, as their counterparts are assumed to be observed: population growth $g_N$, investment price growth (the inverse of $g_Q$), and the employment-population ratio $\bar{N}$. The growth rate $g_Z$ is chosen to roughly match measured TFP (but also depends on $\alpha$, the estimated Cobb-Douglas parameter). The depreciation rate $\delta$ is chosen to match $\frac{I}{K}$ using the balanced growth relation (eq. 18 in F/G), and the Gordon growth formula is used to infer the expected return on risky assets $r^*$.

Our approach differs from Farhi/Gourio only when we identify the parameters $\alpha$ and $\pi_L$, using our modified versions of equations (20) and (27) above. The labor share $s_L$ and the marginal product of capital (approximated by average profitability of capital $\frac{\Pi}{K}$) are the observables, and we set the markup $\mu$=1.15. Since we have estimates for $r^*$,$\delta$,$g_Q$, we can identify $\alpha$ and $\pi_L$ from this pair of equations.

Identification then continues as in Farhi/Gourio, using data on the risk-free rate to infer the equity premium, and separately inferring discount factor $\beta$, risk aversion "$\theta$, and quantity of risk $\xi$ (making assumptions about these variables and the intertemporal elasticity of substitution exactly as in the paper). Note that these choices do not affect inferences about $\alpha$ or $\pi_L$.

Table B.4 compares the parameter estimates in the Farhi/Gourio baseline model ("FG") compared to our model ("SS"). (Table 2 in the main paper is a truncated version of this table. In Table 2, we only show the parameters which were estimated to have changed). Note that the majority of estimated parameters are identical or very similar across the two specifications, reflecting the recursive identification procedure described above. The only differences are in the rent-sharing parameter and markup parameter (by construction), and in the Cobb-Douglas parameter $\alpha$ and TFP growth parameter $g_Z$.

In the Farhi/Gourio model, the markup is estimated to rise from 1.08 in the period 1984-2000 to 1.15 in the period 2001-2016 (implicitly holding rent-sharing constant at zero in both periods). In our model, holding the markup constant at 1.15 in both periods, rent-sharing with labor is estimated to fall from 0.44 in the period 1984-2000 to 0.02 in the period 2001-2016. In contrast to the Farhi/Gourio model, our model also features a small decline in the Cobb-Douglas parameter $\alpha$, suggesting a small amount of labor-biased technical change (FG estimates no change in $\alpha$). Our model estimates a smaller decline in the rate of TFP growth than the FG model. Common to both models are an increase in the discount factor, reflecting higher savings supply; an increase in macroeconomic risk (disaster probability); and an increase in the rate of depreciation. Note that these factors are identical in both exercises by construction of our modification exercise.

In Table B.5 we show the estimated contribution of each parameter to changes in the model-implied moments, replicating Table 4 of Farhi/Gourio. For these decompositions we use the method that Farhi and Gourio use to estimate the contributions of each parameter to each change in the key moments. As Farhi/Gourio note: "because our model is non-linear, this is not a completely straightforward task; in particular, when changing a parameter from a first subsample value to a second subsample value, the question is at which value to evaluate the other parameters (for example, the first or second subsample value). If the model were linear, or the changes in parameters were small, this would not matter; but such is not the case here, in particular for the price-dividend ratio". They therefore report the average contribution over all possible orders of changing parameters, as

**Table B.4:** Estimated Parameters and Changes between samples

| Parameter | Symbol | Model | First Sample (1984-2000) | Second Sample (2001-2016) | Difference |
|---|---|---|---|---|---|
| Discount factor | $\beta$ | FG | 0.961 | 0.972 | 0.012 |
| | | SS | 0.961 | 0.972 | 0.012 |
| Disaster probability | p | FG | 0.034 | 0.065 | 0.031 |
| | | SS | 0.034 | 0.065 | 0.031 |
| Depreciation | $\delta$ | FG | 2.778 | 3.243 | 0.465 |
| | | SS | 2.778 | 3.243 | 0.465 |
| Cobb-Douglas | $\alpha$ | FG | 0.244 | 0.243 | -0.000 |
| | | SS | 0.260 | 0.244 | -0.016 |
| Population growth | $g_P$ | FG | 1.171 | 1.101 | -0.069 |
| | | SS | 1.171 | 1.101 | -0.069 |
| TFP growth | $g_Z$ | FG | 1.298 | 1.012 | -0.286 |
| | | SS | 1.233 | 1.010 | -0.223 |
| Investment in technical growth | $g_Q$ | FG | 1.769 | 1.127 | -0.643 |
| | | SS | 1.769 | 1.127 | -0.643 |
| Labor supply | $\bar{N}$ | FG | 62.344 | 60.838 | -1.507 |
| | | SS | 62.344 | 60.838 | -1.507 |
| Rent-sharing with labor | $\pi_L$ | FG | – | – | – |
| | | SS | 0.441 | 0.022 | -0.419 |
| Markup | $\mu$ | FG | 1.079 | 1.146 | 0.067 |
| | | SS | – | – | – |

Note: in the "SS" estimation, $\mu$ is held constant at 1.15. In the "FG" estimation, $\pi_L$ is implicitly held constant at 0. The "FG" estimates in this table correspond to the baseline parameter estimates in Table 2 of Farhi and Gourio (2018).

we move from the first to the second subsamples. In both the "FG" and the "SS" case, the decline in the risk free rate is primarily explained by a rise in savings supply (decline in discount factor $\beta$) and an increase in disaster risk p. This increase in savings supply should, all else equal, decrease average profitability of capital $\frac{\Pi}{K}$ by 2 percentage points. In reality, the average profitability of capital has risen a little. The baseline Farhi/Gourio model reconciles the rise in savings supply and small rise in average profitability of capital with a combination of higher macroeconomic risk and higher markups. In the "SS" case, instead, the two are reconciled with higher macro risk and lower rent-sharing with labor. In the "FG" case, the change in markups accounts for the bulk of the increase in price-earnings ratios and in Tobin's Q over the period. In the "SS" case, this is instead achieved by the fall in rent-sharing with labor. The "SS" model accounts for the rise in the Price-Dividend ratio slightly differently as compared to the "FG" model, with a slightly larger role for the decline in the Cobb-Douglas parameter $\alpha$ and a slightly smaller role for the decline in TFP growth $g_Z$.

The increase in the share of income going to capital (the "measured capital share") and its counterpart, the decline in the labor share, is entirely explained by higher markups in the "FG" case: higher markups create a wedge between the marginal product and the return for both labor and capital, pushing down the labor share and "pure" capital share, but increasing the "pure" profit share. In the "SS" case, the increase in the measured capital share/decline in the labor share is primarily explained by lower rent-sharing with labor; at the same time, the decline in the Cobb-Douglas parameter $\alpha$ acts to increase the labor share and reduce the capital share, partly offsetting the decline in the labor share that would have occurred from the estimated decline in rent-sharing alone.

Finally, in the "FG" case the capital-output ratio, investment-output ratio, and growth rates of output and investment are lower than they otherwise would have been if markups had not risen. In contrast in the "SS" case, the degree of rent-sharing between capital and labor in our model does not affect firms' production or investment decisions.

### B.4.2 Plausibility of estimated rent-sharing parameter in Farhi/Gourio accounting decomposition

Our estimation suggests that the rent-sharing parameter was 0.44 in the 1980s-1990s. How plausible is this? To compare this to estimates from the literature, we need to translate it into the rent-sharing elasticities estimated in the empirical literature.

Following Card et al (2018), note that under certain assumptions the elasticity of wages with respect to an increase in total rents (pure profits), $\xi_R$, is equivalent to the share of labor rents in wages. Then, the elasticity of wages with respect to value added is $\xi_{VA} = \xi_R \cdot \frac{VA}{Rents}$.

In our accounting decomposition, the equilibrium share of rents in wages in the first period (1984-2000) is 0.09, implying an elasticity of wages with respect to rents of $\xi_R = 0.09$ and an elasticity of wages with respect to value added of $\xi_{VA}$=0.44. These estimates are not implausibly high compared to the (few) well-identified empirical estimates of rent-sharing elasticities in the US (many of which are summarized in Card et al (2018)).

Blanchflower, Oswald, and Sanfey (1996), for example, found an elasticity of 0.01-0.06 for the transmission of industry-level profits per worker into wages in U.S. manufacturing industries. Estevao and Tevlin (2003) also studied U.S. manufacturing industries, instrumenting for shocks to

**Table B.5:** Contributions of estimated parameters to model moments

| | | Model-implied moments | | | Contributions of each parameter | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Model | 1984-2000 | 2001-2016 | Difference | $\beta$ | p | $\delta$ | $\alpha$ | $g_P$ | $g_Z$ | $g_Q$ | $\bar{N}$ | $\pi_L$ | $\mu$ |
| MPK-RF spread | FG | 11.22 | 15.24 | 4.02 | -0.66 | 2.39 | 0.68 | 0.00 | 0.00 | -0.10 | -1.05 | 0.00 | | 2.76 |
| | SS | 11.22 | 15.24 | 4.02 | -0.66 | 2.39 | 0.68 | 0.26 | -0.00 | -0.08 | -1.05 | 0.00 | 2.48 | |
| ...of which, depreciation | FG | 4.55 | 4.37 | -0.18 | 0.00 | 0.00 | 0.47 | 0.00 | 0.00 | 0.00 | -0.64 | 0.00 | | 0.00 |
| | SS | 4.55 | 4.37 | -0.18 | 0.00 | 0.00 | 0.47 | 0.00 | 0.00 | 0.00 | -0.64 | 0.00 | 0.00 | |
| ...of which, market power | FG | 3.39 | 5.55 | 2.17 | -0.59 | 0.24 | 0.21 | 0.00 | -0.00 | -0.09 | -0.35 | 0.00 | | 2.73 |
| | SS | 6.05 | 5.68 | -0.37 | -0.76 | 0.31 | 0.28 | 0.34 | -0.00 | -0.09 | -0.45 | 0.00 | 0.00 | |
| ...of which, risk premium | FG | 3.15 | 5.23 | 2.08 | -0.05 | 2.14 | 0.00 | -0.00 | -0.00 | -0.01 | -0.00 | 0.00 | | 0.00 |
| | SS | 3.15 | 5.23 | 2.08 | -0.05 | 2.14 | 0.00 | -0.00 | -0.00 | -0.01 | -0.00 | 0.00 | 0.00 | |
| Equity return | FG | 5.85 | 4.90 | -0.96 | -1.22 | 0.56 | 0.00 | -0.00 | -0.00 | -0.19 | -0.10 | 0.00 | | 0.00 |
| | SS | 5.85 | 4.90 | -0.96 | -1.22 | 0.56 | 0.00 | -0.04 | -0.00 | -0.15 | -0.11 | 0.00 | 0.00 | |
| Equity premium | FG | 3.07 | 5.25 | 2.18 | 0.00 | 2.18 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | | 0.00 |
| | SS | 3.07 | 5.25 | 2.18 | 0.00 | 2.18 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | |
| Risk-free rate | FG | 2.79 | -0.35 | -3.14 | -1.22 | -1.62 | 0.00 | -0.00 | -0.00 | -0.19 | -0.10 | 0.00 | | 0.00 |
| | SS | 2.79 | -0.35 | -3.14 | -1.22 | -1.62 | 0.00 | -0.04 | -0.00 | -0.15 | -0.11 | 0.00 | 0.00 | |
| Price-dividend ratio | FG | 42.34 | 50.11 | 7.78 | 30.67 | -13.19 | 0.00 | -0.02 | -1.86 | -5.07 | -2.76 | 0.00 | | 0.00 |
| | SS | 42.34 | 50.11 | 7.78 | 30.56 | -13.13 | 0.00 | -0.96 | -1.85 | -3.97 | -2.87 | 0.00 | 0.00 | |
| Price-earnings ratio | FG | 17.85 | 25.79 | 7.94 | 10.16 | -4.57 | -0.35 | 0.00 | -0.59 | -1.47 | -0.34 | 0.00 | | 5.08 |
| | SS | 17.85 | 25.79 | 7.94 | 10.11 | -4.54 | -0.35 | 0.23 | -0.58 | -1.15 | -0.37 | 0.00 | 4.58 | |
| Tobin's Q | FG | 2.50 | 3.84 | 1.34 | 1.05 | -0.48 | 0.11 | 0.00 | -0.08 | -0.28 | -0.31 | 0.00 | | 1.34 |
| | SS | 2.50 | 3.84 | 1.34 | 1.03 | -0.47 | 0.11 | 0.09 | -0.08 | -0.22 | -0.32 | 0.00 | 1.20 | |
| Labor share | FG | 70.11 | 66.01 | -4.10 | 0.00 | 0.00 | 0.00 | 0.03 | 0.00 | 0.00 | 0.00 | 0.00 | | -4.13 |
| | SS | 70.11 | 66.01 | -4.10 | 0.00 | 0.00 | 0.00 | 1.35 | 0.00 | 0.00 | 0.00 | 0.00 | -5.46 | |
| 'Pure' capital share | FG | 22.59 | 21.24 | -1.35 | 0.00 | 0.00 | 0.00 | -0.03 | 0.00 | 0.00 | 0.00 | 0.00 | | -1.33 |
| | SS | 22.59 | 21.24 | -1.35 | 0.00 | 0.00 | 0.00 | -1.35 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | |
| 'Pure' profit share | FG | 7.30 | 12.76 | 5.46 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | | 5.46 |
| | SS | 7.30 | 12.76 | 5.46 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 5.46 | |
| K/Y | FG | 2.13 | 2.28 | 0.15 | 0.29 | -0.12 | -0.11 | -0.00 | -0.00 | 0.04 | 0.18 | -0.00 | | -0.13 |
| | SS | 2.13 | 2.28 | 0.15 | 0.29 | -0.12 | -0.11 | -0.13 | 0.00 | 0.04 | 0.18 | -0.00 | 0.00 | |
| I/Y | FG | 17.28 | 16.50 | -0.78 | 2.20 | -0.90 | 0.23 | -0.02 | -0.16 | -0.52 | -0.59 | -0.00 | | -1.03 |
| | SS | 17.28 | 16.50 | -0.78 | 2.20 | -0.90 | 0.23 | -1.15 | -0.16 | -0.41 | -0.61 | -0.00 | 0.00 | |
| Detrended Y (% change) | FG | – | – | -0.30 | 4.18 | -1.70 | -1.52 | -0.07 | -0.00 | 0.65 | 2.56 | -2.45 | | -1.95 |
| | SS | – | – | -2.36 | 4.38 | -1.78 | -1.59 | -4.16 | 0.00 | 0.54 | 2.69 | -2.45 | 0.00 | |
| Detrended I (% change) | FG | – | – | -4.95 | 17.18 | -6.98 | -0.12 | -0.20 | -0.94 | -2.45 | -0.96 | -2.45 | | -8.02 |
| | SS | – | – | -7.01 | 17.38 | -7.05 | -0.19 | -10.93 | -0.94 | -1.91 | -0.91 | -2.45 | 0.00 | |

Note: in the "SS" estimation, $\mu$ is held constant at 1.15. In the "FG" estimation, $\pi_L$ is implicitly held constant at 0. The "FG" estimates in this table correspond to the decomposition estimates in Table 4 of Farhi and Gourio (2018).

industry demand using increases in output of large downstream sectors: they found a rent-sharing elasticity of 0.29 for value added per worker and 0.14 for profits per worker (as reported in Card et al (2018)). Barth, Bryson, Davis, and Freeman (2016) use the Longitudinal Business Database, instrumenting for demand shocks using output of the same industry in other regions, and find an elasticity of wages with respect to sales per worker of 0.16. Kline, Petkova, Williams, and Zidar (2019) use the granting of patents to firms as an instrument for a profit/rent shock, and estimate an average rent-sharing parameter of 0.3. Lamadon, Mogstad, and Setzler (2019) find that a 10% increase in firm value added results in 1.4% higher wages.

### B.5 Quantitative implications of the decline in worker power for the NAIRU

In the main body of the paper, we argue that the decline in worker power should have been expected to reduce the NAIRU. Here, we use a number of simple exercises to illustrate the possible magnitude of the decline in the NAIRU induced by the decline in worker power.

First, we use the model in Summers (1988) which argues that the equilibrium rate of unemployment is a function of the degree of worker rent-sharing power (indexed by the share of workers with power $\beta$ and the wage premium they receive $\mu$), the level of the value of unemployment relative to the value of work b, and the efficiency wage parameter $\alpha$ (the elasticity of worker productivity to the relative wage):

$$u = \frac{\alpha + \mu\beta}{(1-b)(1+\mu\beta)}$$

Summers sets $\alpha = 0.06$ and $b = 0$. Using these, and plugging in the changes in the unionization rate (for $\beta$) and union wage premium (for $\mu$) over 1982 to 2019, would predict a 3.5 percentage point decline in the equilibrium unemployment rate. Using the larger changes in the degree of rent-sharing that we estimate in prior sections - rather than just the decline in unionization - or using larger values for b, the value of unemployment, or smaller values for $\alpha$, the efficiency wage parameter, would predict even larger declines in equilibrium unemployment. A second exercise uses the model in Johnson and Layard (1986). They lay out a model of wait unemployment, where the availability of high wage union jobs incentivizes workers to search for union jobs rather than accept a lower-paid job in the competitive sector. In their simple model, the NAIRU is determined as follows:

$$U = \frac{1}{\frac{\delta(1-\rho)}{QmP} + 1}$$

where P is the unionization rate, m is the union wage premium (markup over the competitive wage), Q is the rate at which unionized workers leave their jobs, $\delta$ is the discount rate, and $\rho$ is the replacement rate of unemployment benefits (the ratio of the value of being unemployed relative to the competitive wage). Plugging the decline in the unionization rate and union wage premium from 1982 to 2019 into this simple equation, alongside a replacement rate of benefits of 0.5, discount rates of between 3 and 8 percent, and a separation rate of unionized workers of between 2 and 4 percent, would yield a fall in the NAIRU of 0.9-2.2 percentage points. Once again, using the full estimated reduction in labor rents would increase this estimate, whereas a higher replacement rate of benefits would reduce the fall in the NAIRU.

Finally, Akerlof, Dickens, and Perry (1996) specify a Phillips curve equation where inflation $\pi$ is a product of the expected inflation rate $\pi^E$, unemployment u, the worker rent-sharing parameter a (in a simple bargaining-over-surplus model), firms' product market markup $\frac{\beta-1}{\beta}$, and a function of the degree of downward nominal wage rigidity S:

$$\pi_t = \pi_t^E + c - au_t + \frac{\beta}{\beta - 1} S_t$$

In the absence of downward nominal wage rigidity, this suggests that the slope of the Phillips curve is equivalent to the degree of worker rent-sharing power. The decline in the slope of the Phillips curve estimated by Blanchard, Cerutti, and Summers (2015) was 0.23 from the 1960s until the 2010s. This would be consistent with the magnitude of the decline in worker rent-sharing that we have identified earlier in this paper. The decline in the worker rent-sharing parameter that was estimated to be consistent with changes in other macro variables like the labor share, in our accounting decomposition, was 0.42 over the 1980s to 2010s; and our estimated decline in imputed labor rents would have been consistent with a decline in the worker rent-sharing parameter of between 0.22 and 0.41 over the 1980s to 2010s (under the assumption of a constant aggregate markup of between 1.1 and 1.2 over the period).

What do these exercises suggest? While these models are by design not able to provide precise estimates, they suggest that in very loosely disciplined models with several free parameters it is very easy to obtain very large impacts of a decline in worker power - of the magnitude we have observed - on the NAIRU and the slope of the Phillips Curve.

## B.6 Markup measurement and labor rents

Baqaee and Farhi (2020) outline three measures for firm-level markups in the U.S. data: (1) the accounting profits approach, (2) the user cost approach, (3) the production function estimation approach. All three of these methods for measuring markups include some measure of costs, including labor costs, in the denominator. This means that if rent-sharing with labor falls, measured labor costs will fall, leading to an increase in measured markups without any changes in the underlying product market power of a firm. This is illustrated in more detail below.

Accounting profits approach: This is the simplest approach to markup measurement. If one assumes that operating income is equal to profits, this implies that markups are equal to sales divided by costs. Baqaee and Farhi (2020) use the expression

$$\text{operatingsurplus}_{i,t} = \left(1 - \frac{1}{\mu_{i,t}}\right) \text{sales}_{i,t}$$

to back out firm-level markups $\mu$, measuring operating surplus as operating income minus depreciation from Compustat data. Firm-level operating income is measured as revenue minus costs, where costs include labor costs. If rent-sharing with labor falls, payments made to workers for a given unit of work will fall, leading measured labor costs to fall. This leads mechanically to an increase in measured markups.

User cost approach: The user cost approach, used by Gutierrez and Philippon (2017) and Baqaee and Farhi (2020), is similar to the accounting profits approach – but, with a more sophisticated consideration of the cost of capital. In this approach markups are estimated as the ratio of sales to total average costs, which are calculated as operating expenses plus an imputed cost of capital. Markups can be estimated from the expression

$$\text{operatingsurplus}_{i,t} = r_{kt,t}K_{i,t} + \left(1 - \frac{1}{\mu_{i,t}}\right)\text{sales}_{i,t}$$

where $\text{operatingsurplus}_{i,t}$ is the operating income of the firm after depreciation and minus income taxes, $r_{kt,t}$ is the user-cost of capital and $K_{i,t}$ is the quantity of capital used by firm i in period t. Once again, since firm-level operating income is measured as revenue minus costs, where costs include labor costs, then if rent-sharing with labor falls, measured markups will mechanically increase.

Production function estimation approach: The production function estimation approach, used by De Loecker, Eeckhout, and Unger (2020) (and based on De Loecker and Warzynski (2012)), estimates firm-level markup as a function of the estimated elasticity of output with respect to variable inputs, $\theta^{v}_{it}$, and the ratio of sales to variable costs, $\frac{P_{it}Q_{it}}{P^{V}_{it}V_{it}}$:

$$\mu_{it} = \theta^{v}_{it}\frac{P_{it}Q_{it}}{P^{V}_{it}V_{it}}.$$

To measure variable costs in the (imperfect) U.S. firm level data, De Loecker, Eeckhout, and Unger (2020) use Cost of Goods Sold (COGS). Other authors have used instead COGS+SGA (Cost of Goods Sold, plus Sales, General, and Administrative) expenses (see, for example, Traina (2018)). The elasticity of output with respect to variable inputs is estimated using the production function estimation technique as outlined in Appendix A of De Loecker, Eeckhout, and Unger (2020). They note that most of the rise in aggregate markups they identify is still present holding the elasticity term constant: that is, it comes from an increase in the ratio of sales to variable costs, rather than a change in the elasticity of output with respect to variable inputs. But once again, if firms earn some rents, and workers' compensation includes at least some of these rents, then some rents to labor will be included as part of measured labor costs. Since the Cost of Goods Sold (COGS) often includes some portion of the firm's labor costs (typically, the portion which is directly tied to production), a decline in rent-sharing with labor would show up under this measure as a decline in COGS relative to sales, and therefore would lead to a mechanical increase in the measured markup with no change in the underlying product market power of the firm.

## B.7 Industry codes

### B.7.1 Sector codes (NAICS and SIC)

For our calculations of the aggregate magnitude of labor rents, and the magnitude of labor rents by state, we use estimates of the industry wage premium at the sector level. At the aggregate level, NAICS level sector compensation data is available from the BEA for 1987-2016, and SIC level sector compensation data is available until 1997. At the state level, NAICS level sector compensation data is available from the BEA for 1997-2016, and SIC level until 1997. This means that we must estimate industry wage premia for both NAICS sectors and SIC sectors. It is relatively straightforward to estimate industry wage premia for SIC sectors in the CPS-ORG, because the CPS uses Census industry codes, which are based on SIC codes (we use the IPUMS-provided consistent code ind1990). It is less straightforward to crosswalk the industry codes in the CPS-ORG to the NAICS sectors. We first map the ind1990 code, based on Census 1990 industry codes, into NAICS 3-digit codes (as described below), then aggregate this up into NAICS sectors.

### B.7.2 Industry codes (BEA industry code, roughly NAICS-3 digit)

For our industry-level analyses, we use the same industry categorizations as Covarrubias, Gutierrez, and Philippon (2019), whose industry classifications are primarily based on BEA industry codes. Data on value added, compensation, gross operating surplus, depreciation, investment, and fixed assets are available from the BEA at the level of these BEA industry codes from 1987-2016.

For our industry-level measures of labor rents, and wage premia, which are estimated from the CPS, we map the ind1990 code (provided by IPUMS as a consistent industry code over time, based on Census 1990 industry codes) into NAICS 3-digit industry codes (as described in more detail below), then map these into BEA industry codes and group them following Covarrubias et al (2019) (detailed in Table 10 in their paper).

Mapping ind1990 codes into NAICS 3-digit industry codes: We start with the Census NAICS industry crosswalk provided by the U.S. Census Bureau. This maps many of our ind1990 codes into NAICS 3-digit industry codes directly. There are some ind1990 industries which map into more than one NAICS code. For these, we start by considering workers in the CPS IPUMS data in 2003 and later, who are assigned Census 2000 industry codes as well as the time-consistent ind1990 code. Many of these Census 2000 industry codes do map directly into one NAICS code, and we use this accordingly. For workers in the data before 2003, we impute their NAICS code using the information from the workers post 2003: for each industry-occupation cell (ind1990 by occ1990), we calculate the share of workers in 2003 and later who are mapped into each NAICS code. We then randomly assign workers pre-2003 in each of those industry-occupation cells to those NAICS codes, with the probability that they receive each NAICS code corresponding to the share of workers post-2003 in their same ind1990-occ1990 cell who are mapped to that NAICS code. A small number of ind1990 codes are not mapped: the biggest are Manufacturing, n.s., and Metal industries, n.s., which correspond to a number of different possible codes with no obvious way of allocating people between them.

# C

# Appendix to Chapter 3

**Table C.1:** HMRC investigations: self-correction (UK)

| | Total arrears identified | % of arrears self-corrected | Number of firms with arrears | % of firms who paid arrears through self-correction |
|---|---|---|---|---|
| 2018/19 | £24.4m | 41% | 1,456 | 28% |
| 2017/18 | £15.6m | 38% | 1,116 | 25% |
| 2016/17 | £10.9m | 55% | 1,313 | 32% |
| 2015/16 | £10.3m | 45% | 1,040 | 17% |

Source: Government Evidence on Minimum Wage Compliance (for arrears) and Freedom of Information request to HMRC (for number of firms). The outcomes of all Freedom of Information requests made as a part of the UK research for this project can be found at WhatDoTheyKnow.com under user profile "amstansbury".
Notes: This Table shows the share of arrears which were self-corrected, and share of firms who paid arrears through self-correction, in HMRC investigations from 2015/16 to 2018/19.

**Table C.2:** Minimum wage cases in online employment tribunals decision database, which feature positive penalty, costs award or compensation relating to minimum wage claim

| Case | No. of workers | Unpaid wages | Other awards | Penalty, costs, compensation | Details |
|---|---|---|---|---|---|
| Ms J Wang v Wangping Travel Ltd: 4122579/2018 | 1 | £1,806 | – | Comp: £100 | The unpaid wage award was for a combination of breaches of National Minimum Wage, holiday pay, and unpaid arrears. Compensation was awarded since the Claimant had to take out credit to cover her expenses while she was being underpaid. |
| Mr Kieran Pattni v Hitshomes Ltd and Others: 2600130/2018 | 1 | £3,287 | £1,802 | Comp: £107 | The other award was for breach of contract. Compensation was awarded pursuant to section 24(2) of the Employment Rights Act 1996. |
| Mr V Atanasui and Mrs M Atanasui v Mr Samir Gad Salama: 2302880/2016 and 2302881/2016 | 2 | £1,453 | £758 | Costs: £284 | The case had two claimants: amounts listed here are total awards across both claimants. The other awards were for unpaid holiday pay and notice pay. Cost award was for preparation time and expenses. |
| Mr Elek Bottlik and Ms Melinda Berecz v Gurdial PJ Ltd: 3331452/2018 and 3331453/2018 | 2 | £29,982 | £19,545 | Costs: £8,000 | The case had two claimants: amounts listed here are total awards across both claimants. The other awards were for compensation for unfair dismissal of both workers. The cost award was 4,000 for each worker. |

**Table C.2:** (continued)

| Case | No. of workers | Unpaid wages | Other awards | Penalty, costs, compensation | Details |
|---|---|---|---|---|---|
| Don Amarasekara and Ahangama Ahangama v Pirathini Elanchcheliyan and Manickam Jasokaran: 1411564/2015 | 2 | £34,51 | £19,762 | Penalty: £5,000 | The case had two claimants: amounts listed here are total awards across both claimants. The other awards were basic and compensatory awards for unfair dismissal, compensation for wrongful dismissal, unpaid holiday pay, unpaid notice pay, compensation for failure to provide statement of terms and conditions, compensation for failure to inform and consult under TUPE. |
| Mr G Warley v Metro Lodgings Ltd: 2501301/2018 | 1 | £1,029 | £1,251 | Comp: £308 | Compensation was awarded pursuant to section 24(2) of the Employment Rights Act 1996 for various bank charges and overdraft fees incurred as a result of the non-payment of wages. The other award was for the failure to give a written statement of employment particulars. |
| Miss R Latif v Eminant Childcare Ltd T/a Laugh 'n' Learn: 1301220/2017 | 1 | £1172 | – | Comp: £180 | Compensation was for bank charges incurred as a result of late payment of wages. |
| Mr A Jones v Sportfact Ltd: 3303557/2018 | 1 | £43 | £483 | Comp: £200 | Compensation for breach of contract for non-payment of the minimum wage of £200 was awarded. The additional awards were for failure to pay holiday pay, and for breach of contract for unpaid notice pay. |
| Mrs B Belkadi v Edward Jones Estate Agents and others: 2601614/2018 | 1 | £1,566 | £2,229 | Comp: £350 | Compensation awarded pursuant to section 24(2) of the Employment Rights Act 1996. Additional awards were for unpaid holiday pay, failure to provide written pay statement, and failure to provide a statement of employment particulars. |
| Ms D Rose v Paula Deans: S/4117252/2018 | 1 | £269 | – | Comp: £33, Costs: £66 | Compensation awarded pursuant to section 24(2) of ERA 1996 for costs incurred taking photographs and sending recorded delivery letters to proceed with case. Cost order made for 2 hours' preparation time. |
| Miss AP Read v Aftala Norfolk Ltd T/a Papa John's Pizza and Whitestone Norwich Ltd T/a Papa John's Pizza: 3400414/2017 | 1 | £478 | £12,243 | Costs: £4,250 | Additional awards were for unfavourable treatment as a result of pregnancy, and for unpaid holiday pay. |

**Table C.2:** (continued)

| Case | No. of workers | Unpaid wages | Other awards | Penalty, costs, compensation | Details |
|---|---|---|---|---|---|
| Mr G Jones v Cupio Vehicle Management Ltd: 2401585/2017 | 1 | £3,174 | £15,700 | Costs: £2,550 | Additional awards were for disability discrimination and for unpaid holiday pay. |

Source: Author's analysis of Ministry of Justice Employment Tribunal database. Notes: This table shows all minimum wage cases in the employment tribunal database (February 2017-August 2019) which listed any penalty, cost award, or compensation relating to the minimum wage underpayment. The value of awards are rounded to the nearest pound.

**Table C.3:** Examples of director disqualification after minimum wage arrears (UK)

1. A 2019 case in the High Court, *Antuzis -v- DJ Houghton Catching Services Limited*, found two company directors of a chicken-catching firm in breach of their requirement to "act in good faith so as to promote the success of the company". The case involved numerous egregious breaches of employment law, including minimum wage violations, excessive hours, failure to pay holiday pay, unsanitary and unacceptable working conditions. (The company had previously had its license revoked as part of a GLA investigation, which called it "the worst gangmaster ever". In 2016 it was found guilty of various counts of labour exploitation in the High Court, and agreed to a settlement of over £1 million).

2. Joanne Ward, owner of nursery *Cygnets to Swans* in Manchester, was disqualified as a company director for 6 years. Her nursery had underpaid 10 staff a total of £11,789. The company went into insolvency without paying the arrears or penalty levied by HMRC. Over the period, Ms Ward had received personal benefits from the company of £157,601.

3. Kenneth Nnaemeka Ikerunanwa, the sole director of both *Widescope Security Services Limited*, and *Atlas Manned Guarding Security Limited* was disqualified for 9 years after an insolvency Service investigation found he paid employees at below the national minimum wage and submitted a series of under-declared VAT returns to HM Revenue & Customs.

4. Shakil Ahmed, director of *Euro Contracts Services Limited*, was disqualified for 7 years after repeatedly failing to pay workers the minimum wage. An HMRC investigation in 2009 found wage arrears of £65,000. These were repaid, but a second HMRC investigation in 2010-11 found wage arrears of more than £110,000. The money owed was still unpaid by the time the company entered liquidation in 2016.

Source: Internet searches of UK government website (gov.uk) and media records. Sources for individual disqualifications are: https://www.lexology.com/library/detail.aspx?g=5f83ead8-e27a-4649-bdd3-7dda6d22ee50, https://www.gov.uk/government/news/failure-to-pay-the-minimum-wage-sees-manchester-nursery-owner-banned-for-six-years, https://www.gov.uk/government/news/security-company-director-given-9-year-ban-for-exploiting-workers, https://www.gov.uk/government/news/director-banned-after-failing-to-pay-minimum-wage-to-farm-labourers

Notes: This table shows five examples of director disqualifications after minimum wage arrears. These are the only examples the author could find after an extensive internet search in August 2019.

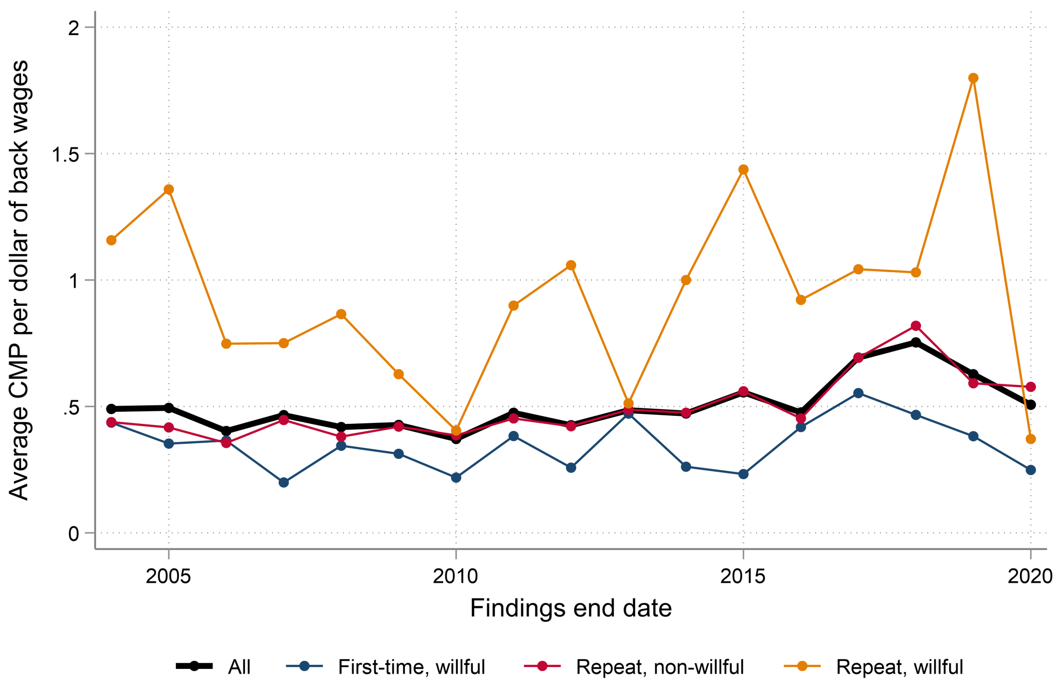



**Figure C.1:** Average civil monetary penalty per dollar of back wages levied in DOL investigations of FLSA wage and hour violations (US)

Source: Author's analysis of Department of Labor WHISARD database.

Notes: This figure shows the average civil monetary penalty per dollar of back wages for repeat and willful violations of the FLSA, of the concluded Wage and Hour Division actions between FY 2005 and January 2021. "Findings end date" refers to the latest date in which the DOL found violations, rather than the year in which the investigation was concluded.

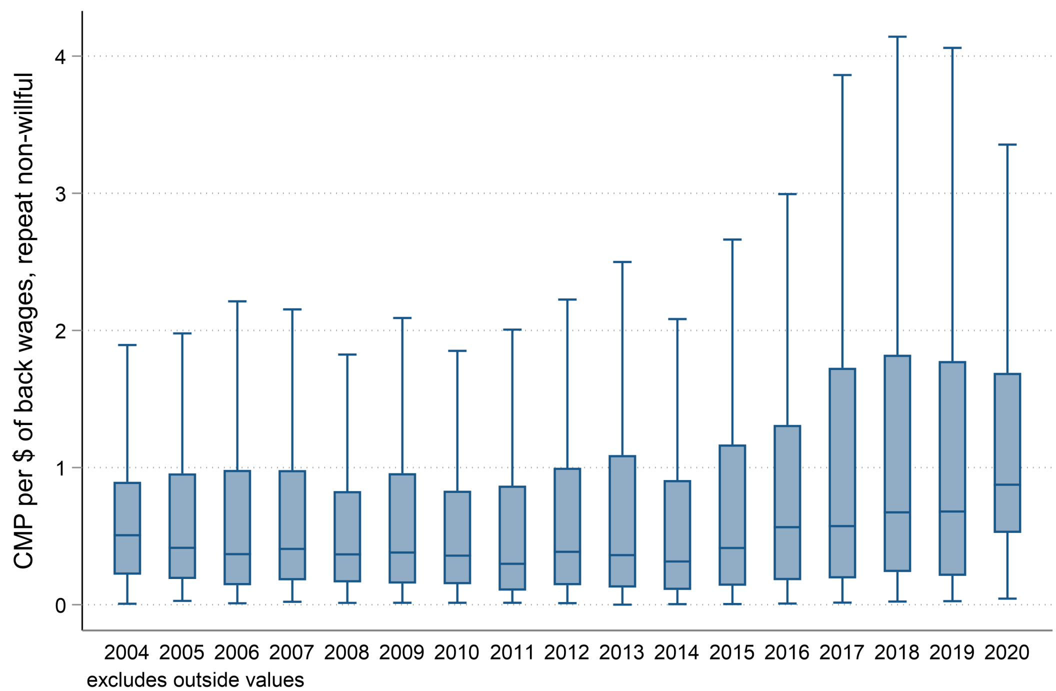



**Figure C.2:** Repeat, non-willful – Distribution of civil monetary penalty per dollar of back wages levied in DOL investigations of FLSA wage and hour violations (US)
Source: Author's analysis of Department of Labor WHISARD database.
Notes: This figure shows the average civil monetary penalty per dollar of back wages for repeat and willful violations of the FLSA, of the concluded Wage and Hour Division actions between FY 2005 and January 2021. "Findings end date" refers to the latest date in which the DOL found violations, rather than the year in which the investigation was concluded.

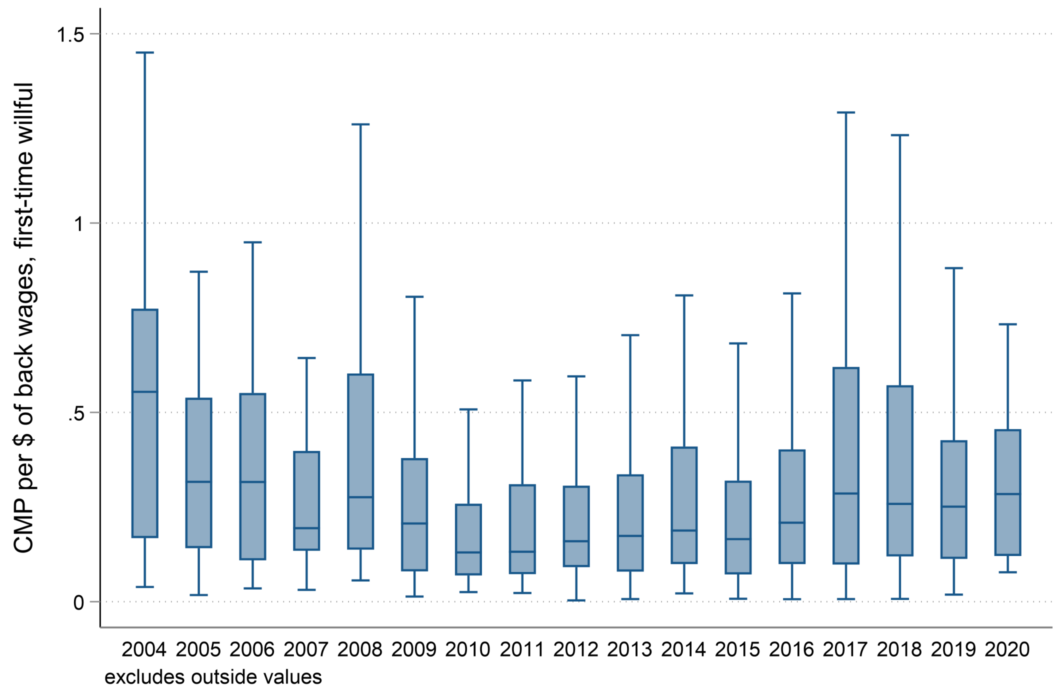



**Figure C.3:** Repeat, non-willful – Distribution of civil monetary penalty per dollar of back wages levied in DOL investigations of FLSA wage and hour violations (US)

Source: Author's analysis of Department of Labor WHISARD database.

Notes: This figure shows the average civil monetary penalty per dollar of back wages for repeat and willful violations of the FLSA, of the concluded Wage and Hour Division actions between FY 2005 and January 2021. "Findings end date" refers to the latest date in which the DOL found violations, rather than the year in which the investigation was concluded.

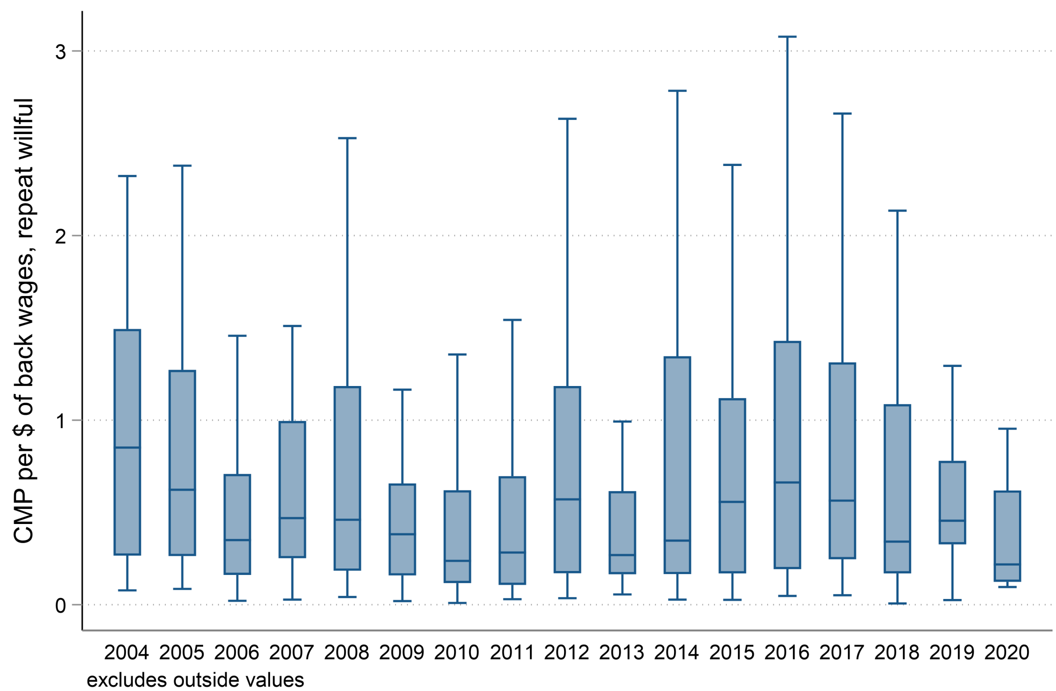



**Figure C.4:** Repeat, non-willful – Distribution of civil monetary penalty per dollar of back wages levied in DOL investigations of FLSA wage and hour violations (US)
Source: Author's analysis of Department of Labor WHISARD database.
Notes: This figure shows the average civil monetary penalty per dollar of back wages for repeat and willful violations of the FLSA, of the concluded Wage and Hour Division actions between FY 2005 and January 2021. "Findings end date" refers to the latest date in which the DOL found violations, rather than the year in which the investigation was concluded.

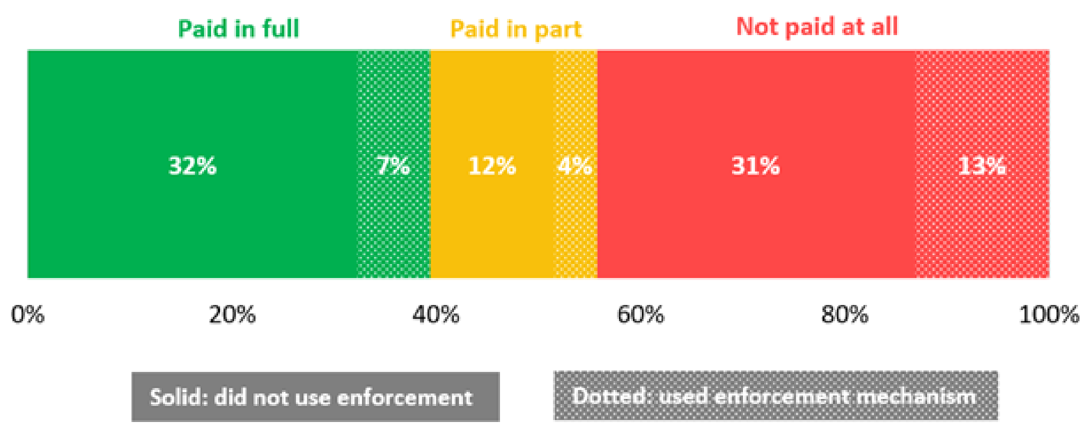



**Figure C.5:** Payment of employment tribunal awards for unpaid wages (UK)
Source: Author's analysis of data in BIS (2013).
Notes: This figure shows the share of employment tribunal awards for unpaid wages which were paid in full, paid in part, or not paid at all, according to a survey on payment of employment tribunal awards conducted by BIS.

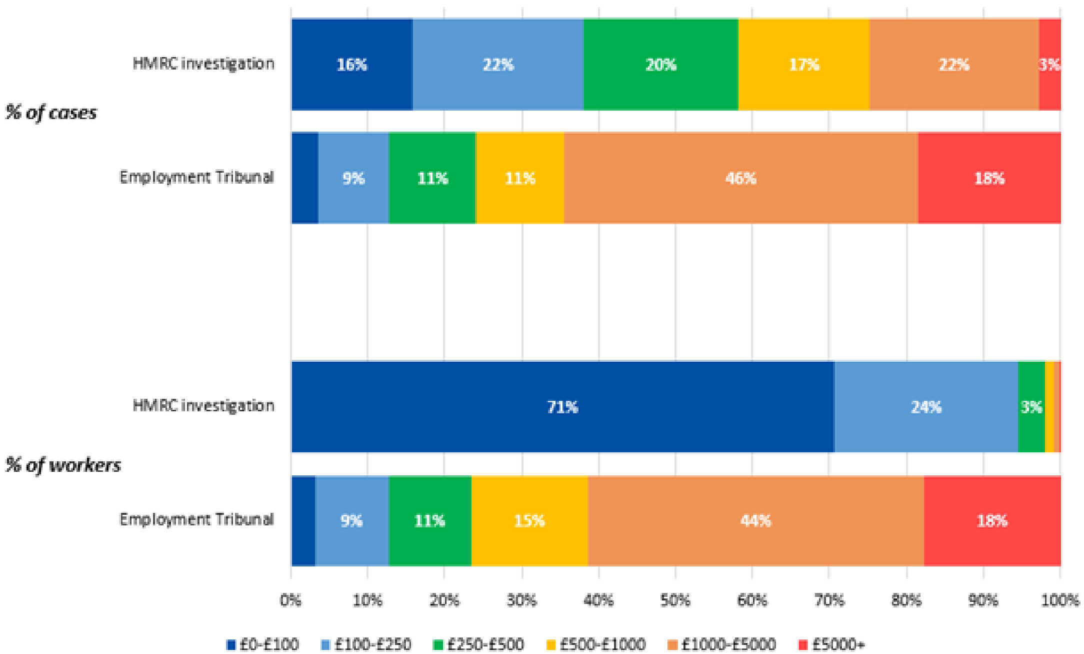



**Figure C.6:** Average arrears per worker in HMRC and Employment Tribunal minimum wage cases (UK)
Source: Author's analysis of publicly available data from BEIS Naming Scheme in 2017 and 2018 (which includes all HMRC-detected minimum wage violations except those offered self-correction), and Ministry of Justice Employment Tribunal database February 2017-August 2019.
Notes: This figure shows the average arrears per worker by case and by workers in HMRC investigations and in employment tribunal minimum wage cases.

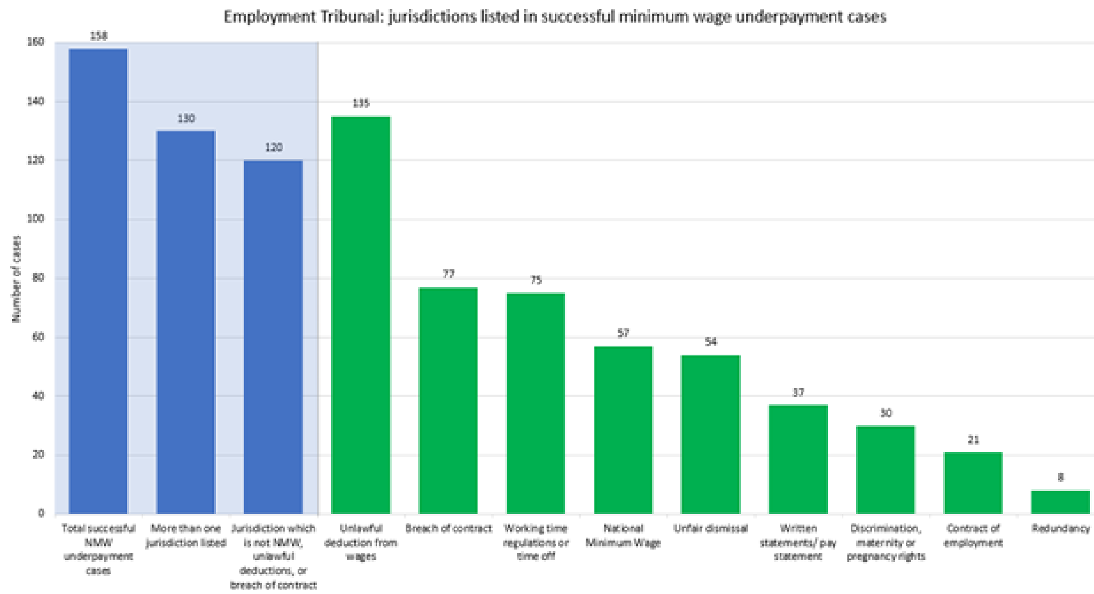



**Figure C.7:** Jurisdictions in successful minimum wage underpayment employment tribunal cases (UK)
Source: Author's analysis of Ministry of Justice Employment Tribunal database.
Notes: This figure shows the number of successful minimum wage cases in the Ministry of Justice Employment Tribunal database (between the start of the database in February 2017 and the date analysis was conducted in August 2019), by the jurisdiction listed. Cases can list multiple jurisdictions. If a minimum wage case also lists a jurisdiction like "unfair dismissal", this means that the employment tribunal case was brought both for minimum wage underpayment and for this other reason (e.g. unfair dismissal). Note that minimum wage underpayment itself can be brought under National Minimum Wage, unlawful deduction from wages, or breach of contract, but that unlawful deductions or breach of contract can also include other employment law violations.

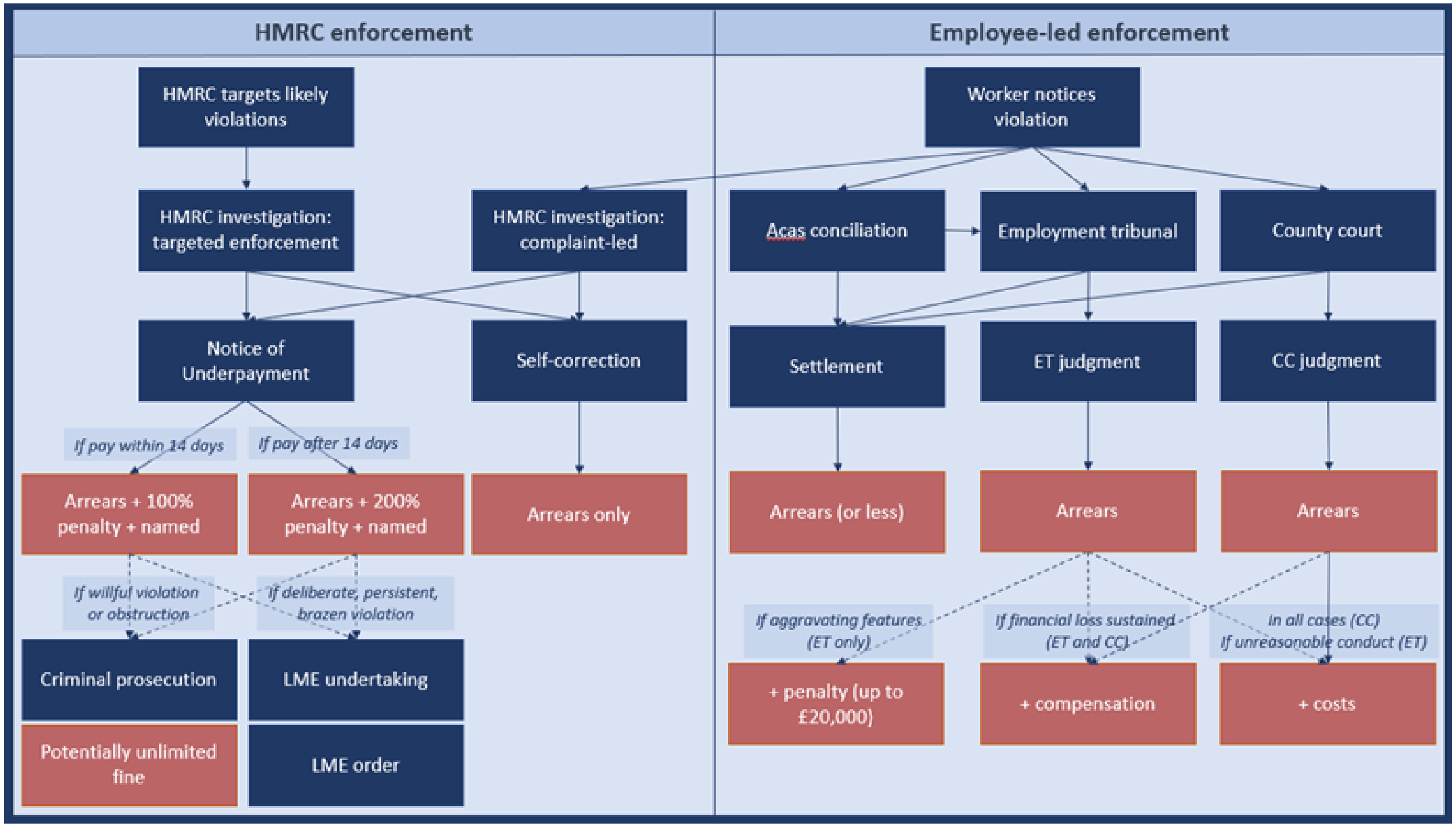



**Figure C.8:** Enforcement channels and possible penalties for UK minimum wage violations

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