Labor Market Concentration Does Not Explain the Falling Labor Share and Other Essays in Economics

by

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ABSTRACT

This dissertation is comprised of three essays. The first and third chapter explore possible causes of the falling U.S. labor share. They show, first, that labor-market concentration is an implausible driver of the falling labor share and, second, that within-industry wages are not keeping up with output. The second chapter ins concerned with private equity and the financialization of the U.S. economy.

Using U.S. administrative data, Chapter 1 shows that the employment-weighted average labor market concentration has been declining since 1980 – the opposite of the change needed to explain the falling labor share. The relationship between wages and labor market concentration has also weakened (become less negative) over that time. Together, these results make labor market concentration an implausible driver of the falling labor share despite a strong, negative relationship between labor market concentration and wages.

Chapter 2, work with Steven J. Davis, John Haltiwanger, Kyle Handley, Josh Lerner, and Javier Miranda, studies the impact of U.S. private equity buyouts on firm-level employment, job reallocation, wages, and labor productivity. Our sample covers thousands of buyouts from 1980 to 2013, which we link to Census micro data on the target firms, their establishments, and millions of comparable firms and establishments that serve as controls. Our results uncover striking differences in the real effects of buyouts, depending on the nature of the target firm, GDP growth, and credit market conditions. Employment at target firms shrinks by nearly 13% relative to controls over two years in buyouts of

publicly listed firms but expands by 11% in buyouts of privately held firms. Slower GDP growth after the buyout brings lower employment growth at targets (relative to controls), as does a widening of credit spreads. Buyouts lead to productivity gains at target firms relative to controls nine percentage points, on average, over two years post buyout. Tighter credit conditions at the time of the buyout are associated with much larger post-buyout productivity gains in target firms. A post-buyout widening of credit spreads or slowdown in GDP growth sharply curtails or reverses productivity gains in public-to-private deals.

Chapter 3 documents that the falling labor share comes entirely from decreases of within-sector labor shares while industrial-output reallocation and changes in self-employed output have counteracted some of the effect. Additionally, the decline in Manufacturing's contribution to the aggregate labor share is almost exactly offset by the increase in Services'.

CHAPTER I

Labor Market Concentration Does Not Explain the Falling Labor Share

DISCLAIMER: "Any opinions and conclusions expressed herein are those of the author(s) and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed." (Project 1686 Disclosure Requests 7185, 7033, 7008, 6988, & 6967)

1.1 Introduction

Economists and policy makers are worried about the falling labor share and stagnating wages. In his address at the 2018 annual Fed conference in Jackson Hole, WY, Alan Krueger (*Krueger*, 2018) cites recent papers showing that labor market concentration is negatively correlated with wages. The authors of these papers extend their arguments to connect labor market concentration and the declining labor share. The non-representative nature of the data used by these studies, however, raises doubts about their conclusions concerning the contribution of labor-market concentration to changes in the labor share.

This paper examines the connection between the labor share and labor market concentration using U.S. administrative data covering all urban, private, non-farm employment, from 1980 to 2012. I find that labor market concentration is an implausible driver of the falling labor share. The employment-weighted average labor market concentration has been *declining* since 1980 – the opposite of the change needed to explain the change in the labor share (or stagnant wages). Not only has the employment-weighted average labor market concentration declined, but the relationship between labor market concentration and wages has also weakened over the period of this study. Taken together, these results show that changes in labor market concentration has not caused the falling labor share.

In order to connect labor market concentration and the labor share, I first develop a model connecting a labor market's concentration to its wages. In this labor market, firms decide how many people to employ knowing how their employment decision will affect wages and knowing other firms' desired hiring. The model implies a negative relationship between a labor market's concentration and its wages. I then test and confirm this model-implied relationship.

After confirming the relationship between labor market concentration and wages, I relate wages to the labor share. The labor share is the percent of aggregate output that goes to compensating labor and therefore is based, in part, on aggregate wages. Each labor market's wages need to be employment-weighted to properly aggregate them into average wages. By using the model-derived relationship between labor market wages and labor market concentration, I show that the employment-weighted average labor market concentration is the relevant measure to link labor market concentration and aggregate wages, and, therefore, the labor share. The time series of the employment-weighted average concentrations, however, goes in the opposite direction of that needed to explain the falling labor share. While contemporaneous papers have demonstrated the same relationship between labor market concentration and wages and have shown average labor market concentration has been decreasing, this is the first paper to explicitly connect the link between these phenomena and the labor share.

Labor market concentration's decline concurrent with product market concentration's increase, and the link between increased product market concentration and lower wages argue for the continued use of the so-called consumer welfare standard of anti-trust regulation. Adopting a regulatory standard based on labor market concentration instead of product market concentration would solve a problem (increasing labor market concentration) that does not exist in the data (*Naidu et al.*, 2018; *Marinescu and Hovenkamp*, 2018). The employment-weighted average labor market concentration is the amount of labor market concentration faced by the average U.S. worker. The time series of the employment-weighted average concentrations shows that labor market concentration is a decreasing problem for this average worker. Additionally, it could block productivity-enhancing mergers because of increased concentration in relatively small labor markets, affecting relatively few workers, and in essence, prioritize these small negative effects over national benefits.

The paper proceeds as follows: Section 1.2 describes this paper's relationship to other papers in the falling labor share and in the concentration and wages literatures; Section 1.3 develops the model relating wages to labor market concentration; Section 1.4 describes the data; Section 3.4 presents the findings of the paper; Section 1.6 presents results on sub-periods and extends the model to include product market power; and Section 3.5 concludes.

1.2 Concentration and Labor Share: Relation to the Literature

This paper contributes to a growing literature connecting the falling labor share to concentrating markets. *Autor et al.* (2017) and *Kehrig and Vincent* (2017) argue that concentrating output markets has pushed production into lower labor share firms, thereby depressing the labor share. Meanwhile *Barkai* (2016) and *De Loecker and Eeckhout* (2017) argue that concentrating output markets has increased mark ups and the additional value generated has gone into profits, thereby increasing the capital share to the detriment of the labor share. Instead of examining product market concentration, this paper focuses on labor market concentration and shows that while product markets have been getting more concentrated, labor markets have been getting less concentrated. This finding is consistent with national chains' replacing local firms. If national chains have a lower labor share than local firms, then national chains' replacing local stores would drive down the labor share as described in *Autor et al.* (2017) and *Kehrig and Vincent* (2017). This paper also provides evidence that increasing product market power can decrease wages. If increased national concentration has created more product market power, then product market power decreasing the labor share would be consistent with *Barkai* (2016) and *De Loecker and Eeckhout* (2017). This paper does not provide evidence for or against any of these studies of national concentration, but should be considered complementary to them.

A quickly growing subset of the literature examines the relationship between labor market concentration, wages, and macro trends. This paper's main contributions to this literature are its universal industrial coverage, its long panel of industries, and its direct link between labor market concentration and the labor share. It is most closely related to *Benmelech et al.* (2018), *Azar et al.* (2017), *Azar et al.* (2018), and *Rinz* (2018) but is significantly different from each in important ways. It uses more industries than *Benmelech et al.* (2018) and so provides insight into the macro-economy as opposed to a single industry; it uses a longer time series than either *Azar et al.* (2017) or *Azar et al.* (2018) and uses actual employment data rather than advertised jobs and so provides insight into the trend of employment concentration; and it directly examines the relationship between labor market concentration and the labor share so answers a different question than *Rinz* (2018) (though *Rinz* employs similar techniques and data sets).

By using administrative data from 1980-2012 for all non-farm private industries, this paper is able to examine general trends in labor market concentration instead of focusing on a particular industry. *Benmelech et al.* (2018) focuses its analysis solely on manufacturing. Because there are qualitative differences between manufacturing's average and employment-weighted average labor market concentration and the aggregate's, they mistakenly conclude that labor market concentration is an increasing problem. This qualitative difference can be understood in terms of manufacturing's declining role in U.S. employment: according to the BLS, the percentage of employment engaged in manufacturing dropped from about 22% in 1980 to just over 10% in 2012.

In addition to the broad industrial scope of this paper, its data allow it to examine the trends in labor market concentration; to calculate concentration using actual employment, independent of firm advertising behavior; and to examine the correlation of wages with the labor market concentration instead of relying on advertised wages in a selected sample of two job boards. Both *Azar et al.* (2017) and *Azar et al.* (2018) use online job posting data to calculate labor market concentration and estimate its relationship with posted (as opposed to realized) wages. They are, therefore, limited in terms of the accuracy and comprehensiveness of their sources. *Azar et al.* (2017) uses CareerBuilder.com data for 2010-2012. Beyond this paper's inability to look at trends, there are questions about the representativeness of these data and how to interpret them. Not every job is posted on Careerbuilder.com– *Azar et al.* (2017) only examine jobs in 26 SOC occupation codes–and only about 20% of postings contain wage information, raising concerns about selection into posting on the website and, conditional on posting on the website, selection into posting a wage.[1] *Azar et al.* (2018) uses a more comprehensive online job postings data from Burning Glass Technologies (BGT), but limit themselves to a single year–2016. Within these data, 40% of job postings have missing employer information.[2] While these papers define labor markets using occupation codes rather than industry codes, it is unclear which of these is the more appropriate labor market definition, or in fact, whether or not a "correct" definition is even possible. This issue will be discussed further in Section 1.4.2.

Once I have estimated the relationship between labor market concentration and wages, and demonstrated the time series trend, I use these empirical facts to examine how labor market concentration has affected the labor share over the study's timeframe. *Rinz* (2018) uses the same data as this paper and finds the same dynamics: local labor market concentration has been declining while national concentration has been increasing. He uses these facts to examine earnings outcomes of different types of workers and earnings mobility.

Rossi-Hansberg et al. (2018) uses NETS (National Establishment Time Series) to calculate national and local sales concentrations from 1990 to 2014. NETS provides microlevel data on US establishments. While NETS is closely related to the data used in this study, *Haltiwanger et al.* (2013) and *Barnatchez et al.* (2017) question its coverage. This paper

[1]This paper additionally calculates concentration as "the share of vacancies of all the firms that post vacancies in that market." Without knowing the underlying firm behavior–how is the share of job postings within CareerBuilder.com correlated with the share of employment in a given labor market?–it is impossible to interpret the concentrations they calculate. Finally, because there is no measure of labor market employment, the average concentration calculated in this study gives equal weight to small labor markets and large labor markets. The simple average presented, therefore, tells us nothing about the concentration facing average workers.

[2]It is unclear how many observations have wage data. The qualifications about differences in firm behavior and the relationship between actual wages and advertised wages also apply.

calculates concentration in employment instead of sales and uses the relationship between concentration and wages to examine how labor market concentration is related to the labor share. Despite these differences in data source, the overall trends in *Rossi-Hansberg et al.* (2018) are consistent with both *Rinz* (2018) and this paper: though nationally calculated concentration has increased over the period of study, local trends move in the opposite direction.

In addition to these recent papers, there is an older literature relating labor market concentration to wages that produced mixed results. *Boal and Ransom* (1997) survey the state of the literature until 1997 and point out that in some studies labor market concentration is highly correlated with wages while in others the correlation disappears. These studies, however, cover relatively few labor markets–the papers they cite vary from a few hundred to a few thousand labor markets–and mostly focus on either nurses or teachers. Additionally, most of these are cross-sectional studies. This paper uses a panel data set with approximately 6.9 million different markets. The panel allows the relationship between labor market concentration and wages to be identified from within-labor market variation instead of relying on cross-sectional variation–a feature driven by the theory developed in this paper. Using only the within-labor market variation also mitigates concerns that the correlation between labor market concentration and wages reflects differences between labor markets i.e. low-productivity labor markets have low wages and few firms therefore creating a correlation between the labor market concentration and wages.

1.3 Linking Labor Market Concentration and Wages

In this section I lay out a simple model of labor markets in which the market's wage depends on its level of concentration. I then discuss the basis of the model's assumptions and connect the model to the parts of this study's empirical specification regarding the choice of concentration measure and the variation used to estimate the relationship between labor market concentration and wages. Later in Section 1.6.2, the model is extended to include product market power along with labor market concentration.

1.3.1 Model

There are N firms in the labor market, indexed $i = 1, ..., N$. Firms produce output with a linear production technology in labor. Each firm receives a productivity draw, a_i around a common component , $\bar{A}$. The productivity draws across firms are mean zero, *i.i.d.*, and $a_i \in [-\bar{a}, \bar{a}]$ with $\bar{a} < \bar{A}$ so that all firms have positive productivity. Firm i's productivity is thus $A_i = \bar{A} + a_i$.

Labor supply is given by a constant-elasticity labor supply function common to all firms. Let l_i be firm i's employment and let $L = \sum_{i=1}^{N} l_i$ be the total employment in the labor market. Wages are set according to

$$w(L) = \gamma L^{\varepsilon^{-1}} \tag{1.1}$$

where $w(L)$ is the wage and ε is the elasticity of labor.

Each firm chooses its employment to maximize profits knowing the other firms' desired hiring and the labor supply curve,

$$\max_{l_i} A_i l_i - w(L) l_i. \tag{1.2}$$

This maximization leads to each firm's first order condition

$$A_i - w(L) = w'(L)l_i. \tag{1.3}$$

By substituting the labor supply function, Equation 1.3 can be rewritten as

$$\frac{A_i - w(L)}{w(L)} = \frac{A_i}{w(L)} - 1 = \varepsilon^{-1}s_i \tag{1.4}$$

where $s_i = \frac{l_i}{L}$ is firm i's share of employment in the labor market. Multiplying by s_i, further rearranging, and summing across all firms leads to

$$\frac{1}{w(L)}\left(\frac{\sum_{i=1}^{N} A_i l_i}{L}\right) = \left(1 + \varepsilon^{-1}\sum_{i=1}^{N} s_i^2\right). \tag{1.5}$$

$\sum_{i=1}^{N} s_i^2$ is the Herfindahl-Hirschman index (HHI), a commonly used measure of market concentration.[3] Taking logs and rearranging finally yields

$$\log(w) = \log(\bar{A}) + \log\left(\frac{\bar{A}L + \sum_{i=1}^{N} a_i l_i}{\bar{A}L}\right) - \log(1 + \varepsilon^{-1}\text{HHI}), \tag{1.6}$$

the relationship between the wages and the labor market concentration.

Additionally, using equations 1.1 and 1.3 to solve for total labor market employment, L, yields

$$L = \left[\frac{N\bar{A}}{\gamma(\varepsilon^{-1} - N)}\right]^{\varepsilon}. \tag{1.7}$$

In this expression, the total size of the labor market is directly related to an invertible

[3] The HHI ranges from 0 to 1 with 0 being a perfectly competitive market and 1 being a single firm in the market.

function of the total productivity in the market. This one-to-one relationship between market size and productivity will be useful later in the paper when proxying for potentially omitted variables. This expression depends only on the parameters of the model and does not depend on HHI because entry & exit are not endogenously determined. Had they been, then the concentration would influence the size of the total labor market.

1.3.2 Discussion

The preceding model establishes a direct link between wages and labor market concentration, but other features of the model are worth noting. This section will discuss these features and link the model to the empirical specification.

The firms' productivities are made up of a common component and an idiosyncratic component. The common component groups the firms' productivities while the idiosyncratic component creates a productivity distribution within the labor market. This specification is meant to match the firm sorting found in the economic geography literature: firms choose locations to be in close proximity to other firms of a similar productivity level. This phenomenon is economically important. *Gaubert* (2018) finds that nearly half the productivity advantage enjoyed by cities in France is due to firm sorting. The common component also helps determine the variation needed to estimate the model. $\log(\bar{A})$ in Equation 1.6 translates into a labor market fixed effect in the empirical specification. This fixed effect means the relationship between wages and labor market concentration is identified using the within-market variation. Section 1.5.1 will discuss in more detail the advantages of this empirical strategy.

Equation 1.4 shows that a firm's profits are related to the labor supply elasticity and to the firm's relative size in the market. The left-hand side of the equation is the mark up over wages, the only cost in the model. In this setting, as a firm becomes more productive, all else being equal, it will earn a higher mark up and employ a larger share of the labor market. The right-hand side of the equation is the product of the labor supply elasticity and the firm's share of the labor market. This expression determines how much of the additional productivity is captured by workers as a firm's share of the labor market, and therefore its productivity, increases. Weighting Equation 1.4 by the share of labor market employment and summing across all the firms leads to Equation 1.5. Here, the total per-employee profit of the labor market is related to the total labor market concentration as measured by the HHI.

Finally, Equation 1.6 shows that wages are related to the labor market's total productivity along with the market fixed effect and concentration. Unfortunately, while the data set used for the main analysis contains firm employment and wage data, it has no information about productivity. Equation 1.7 relates a product market's productivity directly to the size of the total labor market. This relationship provides a natural proxy for the potentially omitted variables. Section 1.5.1 discusses the potential bias from this omitted variable more thoroughly.

1.4 Data

1.4.1 The LBD

The main source of data for this study is the Census Bureau's Longitudinal Business Database (LBD). The LBD covers the near-universe of non-farm, private establishments with employees. For each establishment, it provides the total number of employees and the total pay of the establishment as of March 13 of the given year. Hence, only establishment-average wages can be calculated from the data. Longitudinal links are created using a variety of methods. *Jarmin and Miranda* (2002) provide full details of the construction of the LBD.

Because this study defines labor markets to exclude rural areas, it does not use the full LBD. Figure 1.1 shows the total jobs covered in each year, from 61 million jobs in 1980 to 104 million jobs in 2012. The full sample has about 850 cities and about 600 industries. It is important to note that not every city will have every industry code and not every year has 850 different cities.

Panel (a) of Table 1.1 gives summary statistics for the full sample. There are 6.94 million observations in the complete sample. Each observation is a labor market. The average HHI over the entire sample is about 0.55 and the average of the outcome variable, ln(Aggregate Wage_{mt}), is 9.833. The average ln(employment) is 3.652 which corresponds to about 37 employees in each labor market, and there are an average of 19 firms in each labor market. Finally, Annual Markets is the average number of markets in each year of the sample period. Standard deviations are below the averages in parentheses.

1.4.2 Defining the Extent of the Market

The first step in examining labor markets is defining labor markets. This paper defines labor markets as industries within an urban area. Based on this definition, firms operating multiple establishments within a labor market are counted as one and firms operating in multiple industries are counted separately. Undergirding these decisions is the premise that firms within a labor market compete with each other for labor. The rest of the section lays out the details around these choices.

This paper uses 5-digit NAICS industrial codes for its industrial classification. The NAICS industrial codes are applied to establishments based on the economic activity that the greatest number of employees are engaged in at that establishment. This classification philosophy constrasts with the older SIC classifications that assign industry codes based on what the establishments' parent company does. By way of example, employees at a warehouse whose parent company is in manufactoring would be correctly identified as working in a warehouse under NAICS codes but would be wrongly classified as manufacturing workers under the SIC coding. In fact, *Fort and Klimek* (2016) show that using SIC codes misclassifies a significant number of workers. The Census Bureau provides the establishment level industry identification developed in *Fort and Klimek* (2016) to researchers using the LBD.

The difference between NAICS and SIC codes is important for labor markets. Using classifications more closely aligned with the economic activities at the establishments allows more-similar establishments to be grouped more easily. Establishments engaged in the same economic activity presumably compete for the same employees and so are in the same labor market. This definition contrasts with studies that use SIC codes. Using SIC codes, or assigning a single NAICS code to all of a firm's establishments, is appropriate in studies of product market concentration because they are measuring the total labor that goes into making the firm's final output. Studies of labor markets, like this one, however, need to differentiate the types of labor that go into production. This study accomplishes this by using establishment level NAICS codes then aggregating to firm level employment.

While there remains significant debate around whether to use industry or occupation codes (and at which code level), the current empirical evidence shows that these differences do not threaten the conclusions of this paper. Therefore, for a study such as this, the importance of these differences may be overblown. The argument presented in Section 1.5.2 only relies on the negative relationship between labor market concentration and wages, and on the decrease in average concentration since the beginning of the analysis period. Shifting labor market definitions will certainly change the point estimates presented in Table 1.3, but these point estimates are not the main focus of this paper - the signs of the estimates are. These signs are negative in this paper; they're negative in *Rinz* (2018), which uses NAICS 4 code; they're negative in *Benmelech et al.* (2018) which uses NAICS 3 codes in manufacturing; they're negative in *Qiu and Sojourner* (2019), which uses NAICS 4 codes to impute occupational concentration; and finally they're negative in *Azar et al.* (2017) and *Azar et al.* (2018) who use jobs posting data and occupation codes. The negative correlation between wages and concentration is extremely robust and arguing about definitions does not change this basic fact. Additionally, every paper with a long enough time frame, and representative data, shows that local concentration has been decreasing over this time frame (this paper, *Qiu and Sojourner* (2019); *Rinz* (2018); *Rossi-Hansberg et al.* (2018), etc.). Taken together, these results mean that differing definitions of labor market do not threaten the main arguments and findings of this paper.

These differences, however, are extremely important for papers whose main argument focuses on the point estimate for a concentration equation. Unfortunately for these authors, it is likely impossible to select the "right" definition of labor market. The main argument has been between industry definitions and occupation definitions, however, **?** shows that both industrial and occupational human capital matter and which matters more depends heavily on the occupation and on the industry. This means that neither definition truly captures "labor markets," and point estimates that rely heavily on these definitions will always be subject to debate. I will ignore these issues in later discussions about estimates because, again, the negative relationship between labor market concentration and wages is robust to these definitions even if the point estimates are not.

The second component of labor market definitions is geographic. Firms in economically integrated areas presumably compete for the same workers while firms in areas that are not economically integrated do not. Urban areas (CBAs, CBSAs, and Micropolitan Areas, the successors to MSAs, referred to as cities from now on) make the most natural geographic definition for a labor market because they are local definitions created by the OMB based on the economic integration of cities and their surrounding areas. Recent evidence suggests this generalization is a good approximation of labor markets. Within the US, internal migration has been declining (*Partridge et al.*, 2012; *Molloy et al.*, 2014). Additionally, *Manning and Petrongolo* (2017) and *Marinescu and Rathelot* (2016) provide evidence of the local nature of job search among potential employees. Finally, moving is expensive. *Bartik* (2017) finds the adjustment costs to moving (including non-pecuniary costs) are over $100,000.

Once the local nature of labor markets is accepted, however, there are still several ways to define them: cities, commuting zone, or county definitions could all be called "local." On closer analysis, however, only the urban area definition are appropriate for a study like this one. Commuting zones are designed by the USDA to incorporate rural areas into the closest city as measured by economic integration. The city definitions, however, are already based on economic integration. Though the rural areas might be most integrated into the particular city in their commuting zone, they are not integrated enough to be included in the initial urban area definitions. That is, they are not integrated into their assigned cities and are most useful in studies that need to cover the entire U.S. land mass. *Foote et al.* (2017) shows using this over-broad definition of local labor markets can affect empirical estimates. While commuting zones are too large, counties are too small. Counties are political units. They are not based on the economic realities of the area and many cities are made up of multiple counties. Using a county-based definition of labor markets suggests that firms in different counties don't compete with each other for labor. For example each of New York City's five boroughs is its own county. County-based labor market definitions imply restaurants in Brooklyn and Manhattan do not compete for employees. Because cities' definitions are based on economic integration instead of political boundaries or geographic proximity and they allow competition within those areas of integration, they are the most natural choice for a study of labor markets. Cities themselves are not static. The OMB periodically updates its definitions of cities to reflect which areas are economically integrated in a particular year. This study uses these changing MSA definitions because they are the best measure of what the labor market was like in that particular year. Concerns that these changing geographical definitions' driving later results are addressed in Section 1.5.2.

Having a definition for labor markets allows me to define the firms operating in those markets. The Census Bureau creates a unique firm identifier that connects all of a firm's establishments (for those firms with multiple establishments) and provides enough geographic data to locate each establishment within an urban area. Many firm identifiers are linked to establishments in multiple labor markets, to multiple establishments in the same labor market, or both. For the purposes of this study, a firm is a firm identifier and labor market combination. This firm definition allows firms to operate in multiple labor markets within the same city and to compete for various types of workers. Firms (firm identifier by labor market) that operate multiple establishments within the same labor market are considered as a unit so the employment at these multiple establishments is aggregated into one firm to calculate concentration. Underlying this aggregation are 2 assumptions: first, establishments do not make hiring and pay decisions independently of each other and second, establishments owned by the same firm do not compete with each other for employees. These assumptions are in line with the discussion of wage decision making in *Bloom et al.* (2015a) where they echo the Bureau of Labor Statistics' assumption that, "the firm level is more consistent with the role of corporations as the economic decision makers than each individual establishment."

Using these definitions of labor markets and firms, I can calculate labor market concentration. This study's model suggests (Section 1.3) using the HHI as its measure of labor market concentration. In this context, the HHI is the sum of the squared shares of the labor market each firm hires. That is, for a market with N firms,

$$\text{HHI} = \sum_{i=1}^{N} s_i^2 \tag{1.8}$$

where $s_i = \frac{l_{i,M}}{L_M}$ is the fraction of employment employed at firm i in labor market M and $L_M = \sum_i^N l_{i,M}$ is the total employment in labor market M.

In addition to a measure of concentration, this study needs a measure of wages within the labor market. The theory presented earlier does not allow for wages to differ across firms. This assumption contrasts with an extensive literature on wage dispersion (see *Mortensen* (2005) for a summary of the literature to 2005). To best mimic a common component of wages that is affected by the labor market's concentration, I calculate the average wage as the employment-weighted average wage of the firms

$$\text{wage} = \sum_{i=1}^{N} \frac{l_i}{L} \frac{p_i}{l_i} \tag{1.9}$$

where p_i is the total pay at firm i. This value is alternatively calculated as the total pay within a labor market over the total labor in that market. I refer to this as the aggregate wage to distinguish it from the average wage at a particular firm $\frac{p_i}{l_i}$. This distinction is important for interpreting results later in Section 1.5.1.

1.5 Specification and Results

This section presents analysis using the data described in Section 1.4.1 and the definitions described in Section 1.4.2. The first section describes the regression analysis and shows that the relationship between labor market concentration and wages is the same in these data as suggested by the model in Section 1.3. The second section connects labor market concentration to the labor share and shows that it is an implausible driver of the decline in the labor share.

1.5.1 The Relationship between Wages and Concentration

Methodology

The theory in Section 1.3 relates the log of wages to a fixed effect, a measure of market-level productivity, and a non-linear function of the labor market's concentration. In keeping with that theory, I use a market-level regression specification for my analysis:

$$\log(w_{mt}) = \alpha + \alpha_t + \alpha_m + \beta_1 \mathrm{HHI}_{mt} + \beta_2 \mathrm{HHI}_{mt}^2 + \beta_3 \Psi_{mt} + \varepsilon_{mt} \tag{1.10}$$

where w_{mt} is the aggregate wage, α_t is a time fixed effect so the results can be interpreted in real terms, α_m is a labor market fixed effect, HHI_{mt}^2 is the square of the labor market's concentration measure to accomodate the nonlinearity of the log() function in Equation 1.6 and Ψ_{mt} are labor market level controls meant to manage the variation in average wages that comes from the second term of Equation 1.6. As controls I use the log of total labor market employment, $\log(emp_{mt})$ and the number of firms in the market, N. I cluster standard errors at the labor market level.

The controls and fixed effects alleviate some possible concerns with this regression analysis. The first possible concern is the omitted productivity term from Equation 1.6 (the second term) which shows that labor market productivity is positively correlated with the wages. If the labor market productivity is negatively correlated with the concentration (more productive markets have lower concentration levels) this will introduce a negative bias into the estimate. If, on the other hand, labor market productivity is positively correlated with the concentration, omitting the variable will introduce a positive bias in the estimate. I use the log of the total labor market employment, $\ln(emp_{mt})$, to proxy for market level productivity because the size of the labor market is a direct result of the market's aggregate productivity in the model as shown in Equation 1.7.

In addition to concerns about the omitted productivity variable, there are concerns that the negative correlation between wages and the labor market concentration is driven by some third phenomenon. The two most obvious possible confounders are the relationship between the number of available jobs and the number of firms, and between-labor market productivity.[4] If the the number of available jobs depends directly on the number of firms and is positively correlated with wages but negatively correlated with the concentration of the labor market (if there are fewer jobs available in a more concentrated labor market) then the results of regressing wages on concentration could reflect this relationship. I use the number of firms in the market, N, to control for this possibility, assuming that getting an available job is a function of N, e.g. one could worry that finding an available job in a labor market with more firms is systematically easier than finding an available job in a labor market with fewer firms, even if both markets have exactly the same concentration ratio.

Cross-sectional correlation between productivity and wages could also confound estimates of the relationship between labor market concentration and wages.[5] However, be-

[4] Interestingly, search frictions are likely to be negatively correlated with labor market concentration. In a highly concentrated market, e.g. a 1 mill town, people will know exactly where to look for a job. Those jobs may not exist at the firm at that point, but that failed job-match is not due to search issues.

[5] This relationship assumes that more concentrated labor markets are less productive than less concentrated labor markets, or that there is a negative correlation between labor market concentration and productivity. While no current research exists on this relationship, there are some clues as to its existence. First, labor market size and concentration are negatively correlated. This

cause of the panel structure of these data, I am able to include the labor market fixed effect corresponding to $\log(\bar{A})$ in Equation 1.6. This fixed effect eliminates all the between-group variation from the estimate, i.e. the coefficients of interest, β_{HHI} and β_{HHI^2}, are estimated using only the time series variation within a labor market. This estimation strategy helps alleviate some of the concerns that the relationship between the wage and the level of concentration reflects the relationship between area productivity and wages. One possible concern with this strategy, however, is whether there is enough variation in the time series of the HHI within labor markets to make accurate estimates. Because the average standard deviation of the HHI within a market is 0.119, this concern is unwarranted.

Results

Table 1.3 presents the regression results from estimating Equation 1.10 with various combinations of fixed effects and controls. All of the relationships presented in the table are significant to the 1% level. The first specification uses time and labor market fixed effects and does not include the market-level control variables. These results are in Column (1). The marginal effect at the mean is -0.2434. Going from the mean concentration level (.5499) to 1 standard deviation (.3517) above the mean, all else remaining equal, is associated with a decrease in log(wages) of -.0907 or about -8.6%. The specification in Column (2) replaces the time fixed effect with time-by-city and time-by-industry fixed effects. Instead of controlling just for national trends, these additional fixed effects control for city trends and for national-industry trends, respectively. Adding these fixed effects attenuates the effect at the mean; it drops from -0.2434 to -0.1867. In this specification,

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relationship is most obviously seen in Figures 1.2 and 1.5. The equal weighted average labor market concentration is always higher than the employment weighted average labor market concentration. The weights and the concentration are therefore negatively correlated. Secondly, labor market size and productivity are positively correlated (*Gaubert*, 2018). While these opposing correlations do not guarantee that productivity and concentration will be negatively correlated, they are suggestive.

going from the mean concentration to one standard deviation above the mean, all else remaining equal, is associated with a decrease in log(wages) of .0768 which corresponds to a 7.7% decrease in wages.

Columns (3) and (4) display the results of adding controls to the fixed effect structures. Column (3) only has time and market fixed effects while column (4) has market fixed effects and time-by-industry and time-by-city fixed effects. Adding the additional controls attenuates the relationship between concentration and wages slightly: the marginal effect at the mean is about .017 smaller in Column (3) than in Column (1) and is about .02 smaller in Column (4) than in Column (2). This attenuation is in keeping with market employment's proxying for market productivity and correcting the negative bias from the omitted variable. The positive coefficients on ln(emp) also support this interpretation. The model suggests employees in more productive markets should make more money. Interestingly, the coefficients on N are negative. Holding constant the labor market's concentration and the overall employment of the market while increasing the number of firms in the market must decrease the average firm size. Lower wages are therefore correlated with smaller firms. Thus, these negative coefficients are in line with the well-known firm-size effect described in *Oi and Idson* (1999).

Because I am using a second order Taylor series expansion for $-\log(1+\varepsilon^{-1}\mathrm{HHI})$, I can use β_{HHI} and β_{HHI^2} to obtain estimates of ε^{-1}, the inverse elasticity of labor. The second order Taylor series expansion for this function is

$$-\log(1+\varepsilon^{-1}\mathrm{HHI}) \approx \gamma - \frac{\varepsilon^{-1}}{1+\varepsilon^{-1}\bar{\mathrm{HHI}}}(\mathrm{HHI}-\bar{\mathrm{HHI}}) + \frac{\varepsilon^{-2}}{2(1+\varepsilon^{-1}\bar{\mathrm{HHI}})^2}(\mathrm{HHI}-\bar{\mathrm{HHI}})^2, \tag{1.11}$$

where γ is a constant and $\bar{\mathrm{HHI}}$ is the point around which the function is expanded. Collecting terms related to the regression specification yields

$$\beta_{\text{HHI}} = -\left[\frac{\varepsilon^{-1}}{(1+\varepsilon^{-1}\bar{\text{HHI}})} + \frac{\varepsilon^{-2}\bar{\text{HHI}}}{(1+\varepsilon^{-1}\bar{\text{HHI}})^2}\right] \tag{1.12}$$

$$\beta_{\text{HHI}^2} = \frac{-\varepsilon^{-2}}{2(1+\varepsilon^{-1}\bar{\text{HHI}})^2}. \tag{1.13}$$

Solving this system of equations,

$$\varepsilon^{-1} = \frac{(2\beta_{\text{HHI}^2} - \beta_{\text{HHI}})^2}{4\beta_{\text{HHI}^2} - \beta_{\text{HHI}}}. \tag{1.14}$$

Using the estimates from Table 1.3 and Equation 1.14 delivers values for the implied inverse labor market elasticity, ε^{-1}. The coefficients in specification (1) and (3) imply $\varepsilon^{-1} = .4$ and $\varepsilon^{-1} = .23$, respectively. These correspond to an elasticity of labor between 2.5 and 4. There are two different but observationally equivalent interpretations of this elasticity. First, it can be interpreted as the average elasticity of the labor supply faced by firms. Second, it can be interpreted as the labor supply elasticity of employees making a non-dynamic labor supply decision. Further complicating the interpretation of the implied labor supply elasticity is that the unit of observation in this data set is jobs as opposed to hours. In any case, however, these estimates should not be interpreted as estimates of the Frisch elasticity of labor supply. Finally, Specifications (2) and (4) include controls for the total size of the labor market. The labor supply elasticities implied by these specifications are negative because labor supply is not allowed to adjust normally.

1.5.2 The Relationship between the Labor Share and Concentration

While the preceding section connected labor market concentration to wages, I have yet to say anything about the labor share. This section makes the connection between labor market concentration and the labor share and shows the labor market concentration is not driving the falling labor share.

The labor share is the proportion of output that goes to compensating labor

$$\alpha = \frac{wL}{PY} = \frac{w}{P}\left(\frac{Y}{L}\right)^{-1} \tag{1.15}$$

where w is average compensation, L is total employment, P is the price level and Y is total output, wL is the total compensation paid to labor while PY is the total value of output. The third expression in Equation 1.15 shows that the labor share is the ratio of real wages, $\frac{w}{P}$ to real labor productivity, $\left(\frac{Y}{L}\right)^{-1}$.

The aggregate real compensation in Equation 1.15 is the employment-weighted average of the real compensation in all of the labor markets in the U.S. That is, for M labor markets in the U.S. indexed by m,

$$\frac{w}{P} = \frac{\sum_{m=1}^{M}\omega_m}{PL} = \sum_{m=1}^{M}\frac{P_m L_m}{PL}\frac{\omega_m}{P_m L_m} \tag{1.16}$$

where ω_m is the total compensation in labor market m, and L_m is the employment in labor market m. Here, $\frac{\omega_m}{L_m}$ is the same as the wage calculated in Equation 1.6 because the model does not distinguish between wages and compensation. Inserting the relationship between concentration and labor market wages as $f(h)$[6] from 1.6 and adding time subscripts yields

$$\omega_t = \sum_{m=1}^{M}\frac{L_{mt}}{L_t}\exp(f(h_{m,t},\Gamma_{m,t})) \tag{1.17}$$

where $f(\cdot)$ is a nonlinear function, $\Gamma_{m,t}$ is a vector of variables other than the labor market

[6]The notation in the following switches from HHI to h because using HHI makes several of the following equations completely unreadable.

concentration that determine wages in market m at time t and h_t is the labor market concentration in market m at time t. Because of the non-linearity of the exp(), there is not a simple decomposition for changes in ω. However, taking the first order approximation of the expression around the points $\bar{h}$ and $\bar{\Gamma}$, and rearranging the expression yields

$$\omega_t \approx \exp(f(\bar{h},\bar{\Gamma}))(1 - f_h(\bar{h},\bar{\Gamma})\bar{h} - f_\Gamma(\bar{h},\bar{\Gamma})\bar{\Gamma}) + f_h(\bar{h},\bar{\Gamma}) \exp(f(\bar{h},\bar{\Gamma})) \sum_{m=1}^{M} \frac{L_{mt}}{L_t} h_{m,t} \quad (1.18)$$
$$+ f_\Gamma(\bar{h},\bar{\Gamma}) \exp(f(\bar{h},\bar{\Gamma})) \sum_{m=1}^{M} \frac{L_{mt}}{L_t} \Gamma_{m,t}$$

Substitute $\zeta(\bar{h},\bar{\Gamma}) \equiv \exp(f(\bar{h},\bar{\Gamma}))(1-f_h(\bar{h},\bar{\Gamma})\bar{h}-f_\Gamma(\bar{h},\bar{\Gamma})\bar{\Gamma})$, $\delta(\bar{h},\bar{\Gamma}) \equiv f_h(\bar{h},\bar{\Gamma}) \exp(f(\bar{h},\bar{\Gamma}))$ and $\psi(\bar{h},\bar{\Gamma}) \equiv f_\Gamma(\bar{h},\bar{\Gamma}) \exp(f(\bar{h},\bar{\Gamma}))$ to get

$$\approx \zeta(\bar{h},\bar{\Gamma}) + \delta(\bar{h},\bar{\Gamma}) \sum_{m=1}^{M} \frac{L_{mt}}{L_t} h_t + \psi(\bar{h},\bar{\Gamma}) \sum_{m=1}^{M} \frac{L_{mt}}{L_t} \Gamma_{m,t}. \quad (1.19)$$

Note that $\zeta(\bar{h},\bar{\Gamma})$, $\psi(\bar{h},\bar{\Gamma})$, and $\delta(\bar{h},\bar{\Gamma})$ are time invariant functions of $\bar{h}$ and $\bar{\Gamma}$. Equation 1.19 demonstrates that, to a first approximation, the aggregate compensation depends on the employment-weighted average concentration of the labor markets and the employment-weighted average of Γ, the other variables that determine wages within the labor market. Using this approximation, the change in aggregate wages due to changing labor market concentration is

$$\Delta\omega = \omega_2 - \omega_1 \approx \delta(\bar{h},\bar{\Gamma}) \left[\sum_{m=1}^{M_2} \frac{L_{m2}}{L_2} h_2 - \sum_{m=1}^{M_1} \frac{L_{m1}}{L_1} h_1 \right]. \quad (1.20)$$

Because $\psi((\bar{h},\bar{\Gamma}))$ does not depend on $h_{m,t}$, it is invariant to changes in the labor market concentration. It therefore does not factor into changes in the labor share due to change in labor market concentration.

Figure 1.2 shows the time series of the employment-weighted average labor market concentration from 1980 to 2012. The employment-weighted average labor market concentration has *decreased* since 1980. Because $f_h(h, \Gamma) < 0$, $\delta(\bar{h}, \bar{\Gamma}) < 0$. Therefore, this time series trend is the opposite of what is needed to explain the declining labor share. Of possible concern, however, is that this decline in the employment-weighted average labor market concentration is caused by the changing geographic definitions. *Rinz* (2018), however, contemporaneously finds similar local dynamics using both the Commuting Zone and the County definitions of labor markets. Since *Rinz*'s paper, several other papers including *Qiu and Sojourner* (2019); *Rossi-Hansberg et al.* (2018) have found similar dynamics using various datasets, including NETS (the National Establishment Time Series) and the LBD. These similar findings show that the direction of the changes in the employment-weighted average labor market concentration is not driven by changing geographical definitions.

Figure 1.3 shows the component of the labor share driven only by changes in the employment-weighted average labor market concentration. I calculate $\delta(\bar{h}, \bar{\Gamma})$ using the estimates of Equation 1.10 in Column (2) of Table 1.3 for the coefficients on $\bar{\text{HHI}}$, the mean of the labor market concentration for $\bar{h}$, and have set $\bar{\Gamma} = 0$ so that individual time and market components do not affect the aggregate trend. These choices lead to

$$\delta(\bar{h}, \bar{\Gamma}) = (-.0875 - 2 \cdot .0411 \cdot .55) \exp(-.0875 \cdot .55 - .0411 \cdot .55^2) = -.173. \tag{1.21}$$

The regression analysis of Section 1.5.1 links the labor market concentration with wages instead of total compensation. According to the BLS's Employment Costs for Employee Compensation (ECEC) survey,[7] however, wages made up 73% of total compensation in

[7] Available from the BLS `https://www.bls.gov/ncs/ect/#tables`

1986 (the first year of data) and 69.3% of total compensation in 2012. While wages' share of compensation has declined slightly, as long as both wages and other benefits respond to labor market concentration in the same way, this calculation makes sense. The calculation starts .63 based on the labor share in 1980 in the Penn World Table.[8] This graph makes it clear that the employment-weighted labor market concentration is moving in the wrong direction to cause the falling labor share.

The aggregate trend in employment-weighted average labor market concentration, however, masks qualitative differences in industry trends. Figure 1.4 shows the employment-weighted average labor market concentration by industry. It shows that the employment-weighted average labor market concentration has increased in retail trade, manufacturing, wholesale trade, and finance, insurance and real estate (FIRE). The employment-weighted average labor market concentration, however, has fallen in services and transport, warehousing and utilities. Because services has become a much bigger employer within the U.S. economy, its decreasing employment-weighted average labor market concentration helps drive the aggregate trend. Examining the employment-weighted average labor market concentration is tantamount to examining the labor market concentration faced by the average U.S. worker. The implication of Figure 1.2 is that the average U.S. worker is facing a lower labor market concentration today than he did 30 years ago. Additionally, the trend in manufacturing is especially interesting because it is the trend identified and explored in *Benmelech et al.* (2018). However, because they focus on manufacturing and ignore the employment weighting needed to examine the aggregate, their conclusions are incorrect.

The time series dynamics of the employment-weighted average labor market concentration is not only driven by the shifting work force. Figure 1.5 shows that the equal-weighted

[8] Available from the St. Louis Fed: `https://fred.stlouisfed.org/series/LABSHPUSA156NRUG`

average labor market concentration has also decreased from 1980 to 2012. This aggregate trend also covers qualitative differences among industries. Figure 1.6 shows the equal-weighted average labor market concentration by industry.

The preceding discussion focused entirely on the relationship between aggregate real compensation, $\frac{w}{P}$, and local labor market concentration without discussing the other piece of total compensation $\left(\frac{Y}{L}\right)^{-1}$, labor productivity. If local labor market concentration is a measure of monopsony power, and monopsony in labor markets increases labor productivity, then the employment-weighted average labor market concentration would have to increase in order to reduce the labor share. However, both the employment-weighted average and equal-weighted average labor market concentrations decrease over the time period.

1.6 Extensions

This section provides extensions to the main analysis of the paper. First, I examine the relationship between labor market concentration and wages through time and find that has weakened (become less negative). Second, I extend the model presented in Section 1.3 to incorporate product market power as well as labor market concentration. When product market power is included, the relationship between wages and labor market concentration is increased. The results of this extension are consistent with the literature on increasing product market concentration as a measure of product market power driving the falling labor share.

1.6.1 Labor Market Concentration and Wages Through Time

While Section 1.5.2 shows that the time series of labor market concentration moves in the opposite direction necessary to explain the falling labor share, this analysis was based on a static relationship between wages and labor market concentration. If, however, the relationship between labor market concentration and wages has been strengthening, that is, getting bigger in absolute value terms, then labor market concentration could still be driving the falling labor share. This change is tantamount to $\delta(\bar{h}, \bar{\Gamma})$ in Section 1.5.2 becoming more negative. To examine this possibility, I divide my sample into sub-periods and regress wages on concentration. I find that the relationship between labor market concentration and wages has also weakened over the period of study.

Table 1.2 shows the summary statistics for each sub-period: the number of observations, the average HHI[9] and the employment-weighted average HHI. The number of observations goes up monotonically, but the average HHI and employment-weighted average HHI are remarkably stable. As before, the standard deviations are in parentheses underneath the mean.

The specification I use for this analysis is

$$\log(w_{mt}) = \alpha_{it} + \alpha_{ct} + \alpha_m + \beta_1 \mathrm{HHI}_{mt} + \beta_2 \mathrm{HHI}_{mt}^2 + \varepsilon_{mt}. \tag{1.22}$$

I have dropped the additional labor market controls because they do not change the estimates much but have included the city-by-time and the industry-by-time fixed effects. The results of the regression are presented in Table 1.5. Most importantly, the marginal effect at the mean has been decreasing since the earliest sub-periods. Figure 1.7 emphasizes this result. It shows the time series of the marginal effect at the mean of HHI on wages. The

[9] I am switching back to HHI because readability is not an issue in the discussion that follows.

effect at the mean is getting closer to 0. These results allay any fears that changes in the relationship between labor market concentration and wages could be driving the declining labor share.

1.6.2 Adding Product Market Power

Another possible concern with regressing wages on labor market concentration is that product market power could contaminate the estimated relationship. In this final extension of the analysis, I control for product market power by dividing firms into local and tradable industries. Implicitly this assumes that product market power is uncorrelated with local labor market concentration for firms operating in tradable goods. I allow the output good to have a price $P(q)$ that depends on the quantity produced and $q = \sum_{i=1}^{N} q_i$. Because $P(q)$ is the inverse demand curve, $P'(q) < 0$. Firms have the same production technology and information as described in Section 1.3. Additionally, firms know how their production will affect the price of their output good. The firm's first order condition is then

$$P'(q)A_i^2 l_i + P(q)A_i - w'(L)l_i - w(L) = 0 \tag{1.23}$$

which leads to

$$\log(w) = \log(\bar{A}) + \log\left(\frac{P(q)\bar{A}L + P(q)\sum_{i=1}^{N} a_i l_i}{\bar{A}L}\right) - \log\left(1 + \left(\varepsilon^{-1} - \frac{P'(q)A_i^2 L}{w}\right)\text{HHI}\right). \tag{1.24}$$

Because of the additional marginal revenue term, the relationship between the concentration, HHI, and the wage is now more negative. Intuitively, in these markets, firms have incentives to restrict their hiring because of the profits they will earn in both the product market and the labor market.

The definitions for tradable versus local goods come from *Delgado et al.* (2015). Their methodology classifies each 6-digit NAICS classification as either tradable or local. Local industries are those whose employment is more evenly distributed throughout the country because the goods and services are location-based like food service and retail. Tradable industries, on the other hand, are more geographically concentrated. Because these industries can ship their goods, they are able to cluster more readily and benefit from agglomeration externalities. The analysis in this paper uses 5-digit NAICS and the *Delgado et al.* (2015) classifications are at the 6-digit NAICS level. This mismatch creates an additional category in this analysis: "Both." There are potentially 10 6-digit NAICS categories in each 5-digit NAICS category. If all of the 6-digit NAICS codes within a 5-digit code belong to the same classification, then the 5-digit code gets that classification. If, on the other, there are conflicting 6-digit classifications, then the 5-digit NAICS code is designated "Both."

In the regression specification, I allow all variables to vary by tradable, local, or both. The exact specification I use is

$$\begin{aligned} w_{mt} = \alpha &+ \beta_1 \mathrm{HHI}_{mt} + \beta_2 \mathbb{1}[Local] * \mathrm{HHI}_{mt} + \beta_3 \mathbb{1}[Both] * \mathrm{HHI}_{mt} + \beta_4 \mathrm{HHI}^2_{mt} + \\ &\beta_5 \mathbb{1}[Local] * \mathrm{HHI}^2_{mt} + \beta_6 \mathbb{1}[Both] * \mathrm{HHI}^2_{mt} + \beta_7 \Psi_{mt} + \beta_8 \mathbb{1}[Local] * \Psi_{mt} + \\ &\beta_9 \mathbb{1}[Both] * \Psi_{mt} + \varepsilon_{mt} \end{aligned}$$

where α is various fixed effects, $\mathbb{1}[\cdot]$ is an indicator function, and Ψ_{mt} is the set of market-level controls for total employment and the number of firms in the market. The fixed effects structures mirror those in Section 1.5.1.

Table 1.4 presents the results from this regression. In all the specifications, the estimated coefficient $\hat{\beta}_{\mathbb{1}[Local]*\mathrm{HHI}}$ is negative and significant at the 1% level indicating that, in line with the theory, wages in industries that produce local goods have a stronger relationship with the labor market concentration than wages in industries that produce tradable goods or industries that produce both. Additionally, the estimated coefficient $\hat{\beta}_{\mathbb{1}[Local]*\text{HHI}^2}$ is positive and significant at the 1% level in all specifications indicating that while local goods producers have a stronger first order effect of concentration on wages, these effects are less non-linear than in tradable goods. Column (2) shows the results of the specification without additional controls but with city-by-time and industry-by-time fixed effects. In this estimate, tradable goods' relationship between wages and labor market concentration has a small, but not statistically significant, main effect of the relationship between their wages and the labor market concentration (-.0227) but a large nonlinear effect (-.1479). Local goods, however, have the opposite relationship. The main effect of concentration on wages for local goods producing industries is large (-.1434 = -.0227 + -.1207) and the nonlinear effect is much smaller (-.0394 = -.1479 + .1085). The results in specifications (3) and (4) follow the same pattern–local goods have stronger relationship between wages and concentration than tradable goods do.

These findings are in line with the current literature on product market concentration. Product market concentration has been increasing, and if this increase in concentration is accompanied by an increase in product market power, it would decrease the labor share as in this model extension. This line of reasoning matches that in *Barkai* (2016) and in *De Loecker and Eeckhout* (2017). Additionally, the negative relationship between labor market concentration and wages is robust to controlling for product market power, even if it contaminates the initial point estimates.

1.7 Conclusion

This paper shows that labor market concentration is an implausible driver of the falling labor share. The time series trend in employment-weighted average labor market concentration moves opposite to what is needed to explain the falling labor share, and the relationship between wages and labor market concentration has not strengthed. Additionally, product market concentration is a plausible driver of the falling labor share as its time series trend moves in the right direction and product market power intensifies the relationship between wages and labor market concentration.

Taken together, these results suggest labor market concentration is a diminishing, not an increasing, problem for U.S. workers. Recent calls to update regulatory regimes to incorporate or prioritize labor market concentration in order to respond to the falling labor share and stagnating wages are, therefore, premature and unsupported by the data.

1.8 Tables and Figures

Table 1.1: Summary stats for the full sample, 1980-2012

	Full Sample
Observations	6,940,000
HHI	0.5499
	(0.3517)
ln(Aggregate Wage$_{mt}$)	9.833
	(0.9535)
ln(emp_{mt})	3.652
	(2.038)
n_{mt}	19
	(137.7)
Annual Markets	210,300
	(15,840)
Employment-weighted HHI	0.189
	0.25

Table 1.2: Observation counts, Average HHI, and Employment-weighted Average HHI for each sub-sample

Period	1980-1983	1984-1988	1989-1992	1993-1996	1997-2000	2001-2004	2005-2008	2009-2012
Observations	740,000	770,000	800,000	830,000	860,000	890,000	930,000	1,130,000
HHI	0.554	0.557	0.554	0.547	0.547	0.547	0.545	0.545
	(0.354)	(0.354)	(0.354)	(0.353)	(0.352)	(0.35)	(0.349)	(0.349)
Employment weighted HHI	0.203	0.196	0.189	0.188	0.187	0.188	0.185	0.184
	(0.27)	(0.267)	(0.259)	(0.257)	(0.252)	(0.249)	(0.246)	(0.245)

Table 1.3: The relationship between HHI and aggregate wages with controls

	Dependent Variable ln(Aggregate Wage)			
	(1)	(2)	(3)	(4)
$\hat{\beta}_{\mathrm{HHI}}$	-0.1982***	-0.0875***	-0.2257***	-0.1170***
	[0.0116]	[0.0105]	[0.0119]	[0.0105]
$\hat{\beta}_{\mathrm{HHI}^2}$	-0.0411***	-0.0902***	-0.0007	-0.0456***
	[0.0087]	[0.0078]	[0.0089]	[0.0078]
$\hat{\beta}_{\ln(\mathrm{emp})}$			0.0176***	0.0226***
			[0.0008]	[0.0008]
$\hat{\beta}_n$			-0.0006***	-0.0003***
			[0.0001]	[0.0000]
Obs	6940000	6940000	6940000	6940000
R^2	0.4432	0.5759	0.4437	0.5762
Marginal Effect at the Mean	-0.2434	-0.1867	-0.2265	-0.1672
Effect of 1 SD Increase from Mean	-0.0907	-0.0768	-0.0797	-0.0644
Time FE	X		X	
Industry×City FE	X	X	X	X
Time × City FE		X		X
Time × Industry FE		X		X

Table 1.4: The relationship between HHI and aggregate wages with controls, tradable vs local

	Dependent Variable ln(Aggregate Wage)			
	(1)	(2)	(3)	(4)
$\hat{\beta}_{\text{HHI}}$	-0.1154***	-0.0227	-0.1619***	-0.0875***
	[0.0170]	[0.0159]	[0.0171]	[0.0159]
$\hat{\beta}_{\mathbb{1}[Local]*\text{HHI}}$	-0.1665***	-0.1207***	-0.1658***	-0.0638***
	[0.0236]	[0.0215]	[0.0240]	[0.0215]
$\hat{\beta}_{\mathbb{1}[Both]*\text{HHI}}$	0.1397***	-0.0308	0.1155**	-0.0762
	[0.0512]	[0.0468]	[0.0523]	[0.0466]
$\hat{\beta}_{\text{HHI}^2}$	-0.1119***	-0.1479***	-0.0362***	-0.0686***
	[0.0125]	[0.0117]	[0.0126]	[0.0117]
$\hat{\beta}_{\mathbb{1}[Local]*\text{HHI}^2}$	0.1438***	0.1085***	0.0842***	0.0379**
	[0.0177]	[0.0161]	[0.0179]	[0.0161]
$\hat{\beta}_{\mathbb{1}[Both]*\text{HHI}^2}$	-0.0895**	0.0496	-0.0586	0.1001***
	[0.0383]	[0.0351]	[0.0393]	[0.0352]
$\hat{\beta}_{\ln(\text{emp})}$			0.0349***	0.0320***
			[0.0010]	[0.0010]
$\hat{\beta}_{\mathbb{1}[Local]*\ln(\text{emp})}$			-0.0465***	-0.0291***
			[0.0017]	[0.0017]
$\hat{\beta}_{\mathbb{1}[Both]*\ln(\text{emp})}$			0.0150***	0.0244***
			[0.0036]	[0.0035]
$\hat{\beta}_{n}$			-0.0004***	-0.0003***
			[0.0001]	[0.0000]
$\hat{\beta}_{\mathbb{1}[Local]*N}$			-0.0002**	0.0001
			[0.0001]	[0.0001]
$\hat{\beta}_{\mathbb{1}[Both]*N}$			-0.0008***	0.0001
			[0.0003]	[0.0001]
Obs	6940000	6940000	6940000	6940000
R^2	0.4432	0.5759	0.4441	0.5763
Marginal Effect At .55, Tradable	-0.238	-0.202	-0.185	-0.163
Marginal Effect At .55, Local	-0.247	-0.275	-0.187	-0.185
Time FE	X		X	
Industry×City FE	X	X	X	X
Time × City FE		X		X
Time × Industry FE		X		X

Table 1.5: The relationship between HHI and aggregate wages with controls, through time

	Dependent Variable ln(Aggregate Wage)							
Period	1980-1983	1984-1988	1989-1992	1993-1996	1997-2000	2001-2004	2005-2008	2009-2012
$\hat{\beta}_{\mathrm{HHI}}$	-0.5128***	-0.6923***	-0.5848***	-0.4027***	-0.0402**	-0.1181***	-0.2960***	-0.1915***
	[0.0561]	[0.0592]	[0.0356]	[0.0206]	[0.0176]	[0.0179]	[0.0191]	[0.0179]
$\hat{\beta}_{\mathrm{HHI}^2}$	0.1684***	0.2836***	0.3198***	0.2550***	-0.0552***	0.0047	0.1369***	0.0827***
	[0.0405]	[0.0424]	[0.0259]	[0.0160]	[0.0140]	[0.0142]	[0.0150]	[0.0139]
Obs	740000	770000	800000	830000	860000	890000	930000	1130000
R^2	0.7098	0.7001	0.6694	0.87	0.8523	0.848	0.8772	0.8688
Mean HHI Within Sample	0.554	0.557	0.554	0.547	0.547	0.547	0.545	0.545
Standard Deviation HHI Within Sample	0.354	0.354	0.354	0.353	0.352	0.35	0.349	0.349
Marginal Effect at the Mean	-0.326	-0.376	-0.23	-0.124	-0.101	-0.113	-0.147	-0.101
Industry×City FE	X	X	X	X	X	X	X	X
Time × City FE	X	X	X	X	X	X	X	X
Time × Industry FE	X	X	X	X	X	X	X	X

Figure 1.1: Total employment covered by sample, 1980-2012, in millions

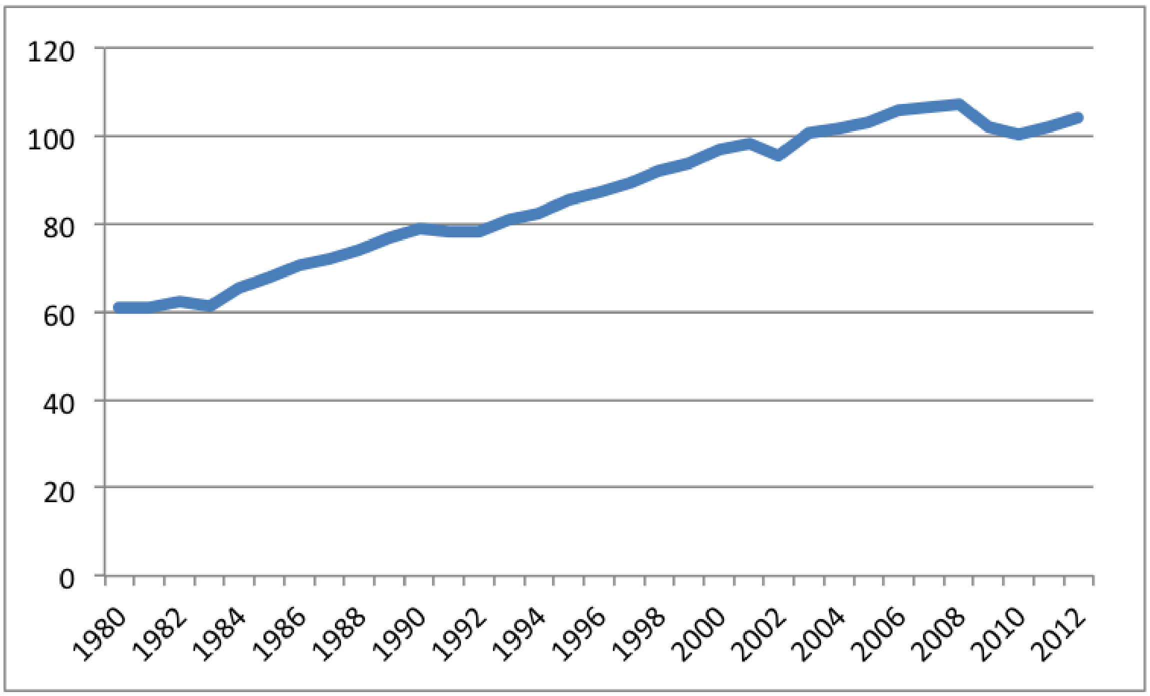


Figure 1.2: Employment-weighted average labor market concentration since 1980

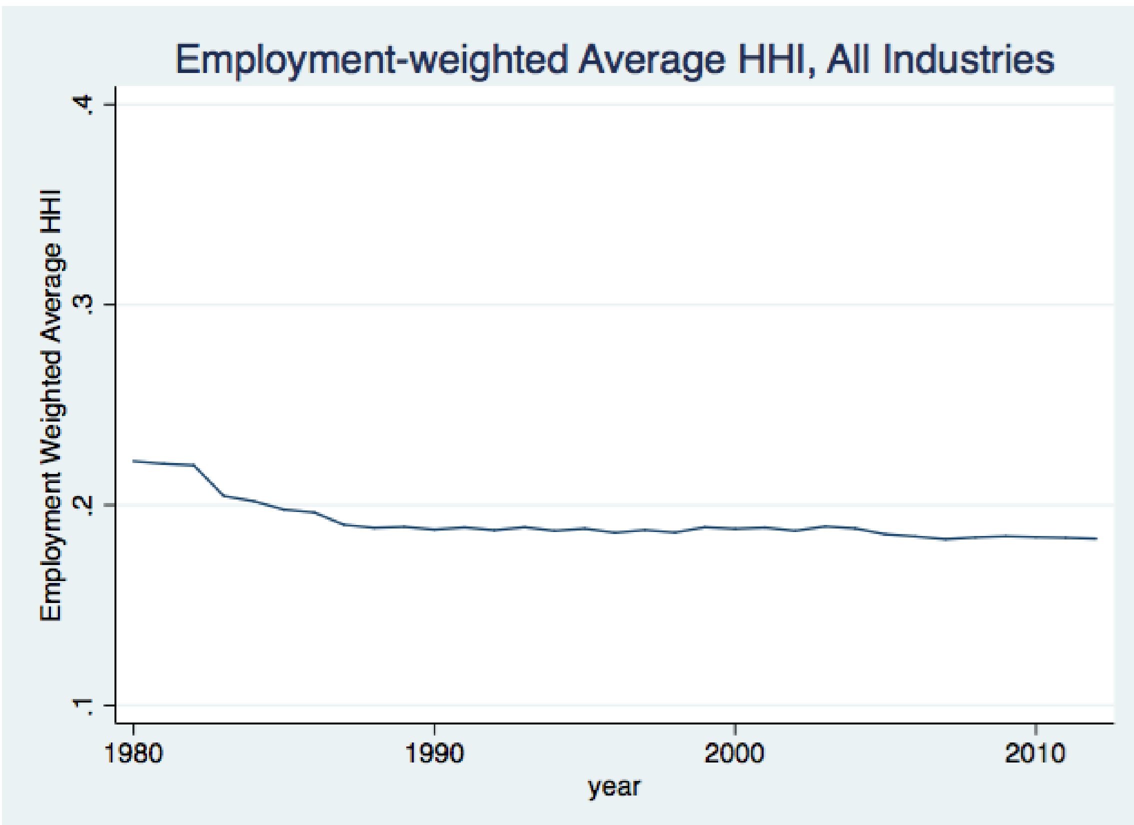


Figure 1.3: Change labor share due to changes in employment weighted average labor market concentration using estimated coefficients

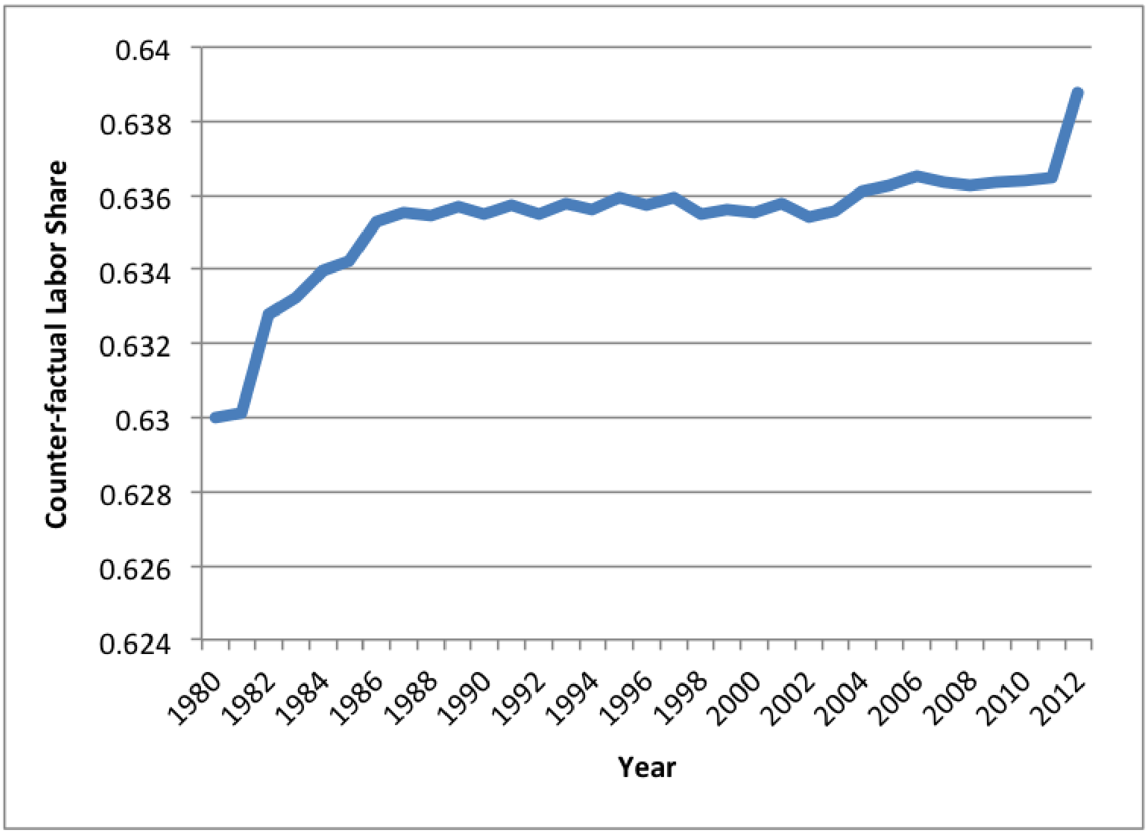


Figure 1.4: Employment-weighted average labor market concnetration since 1980 by industry

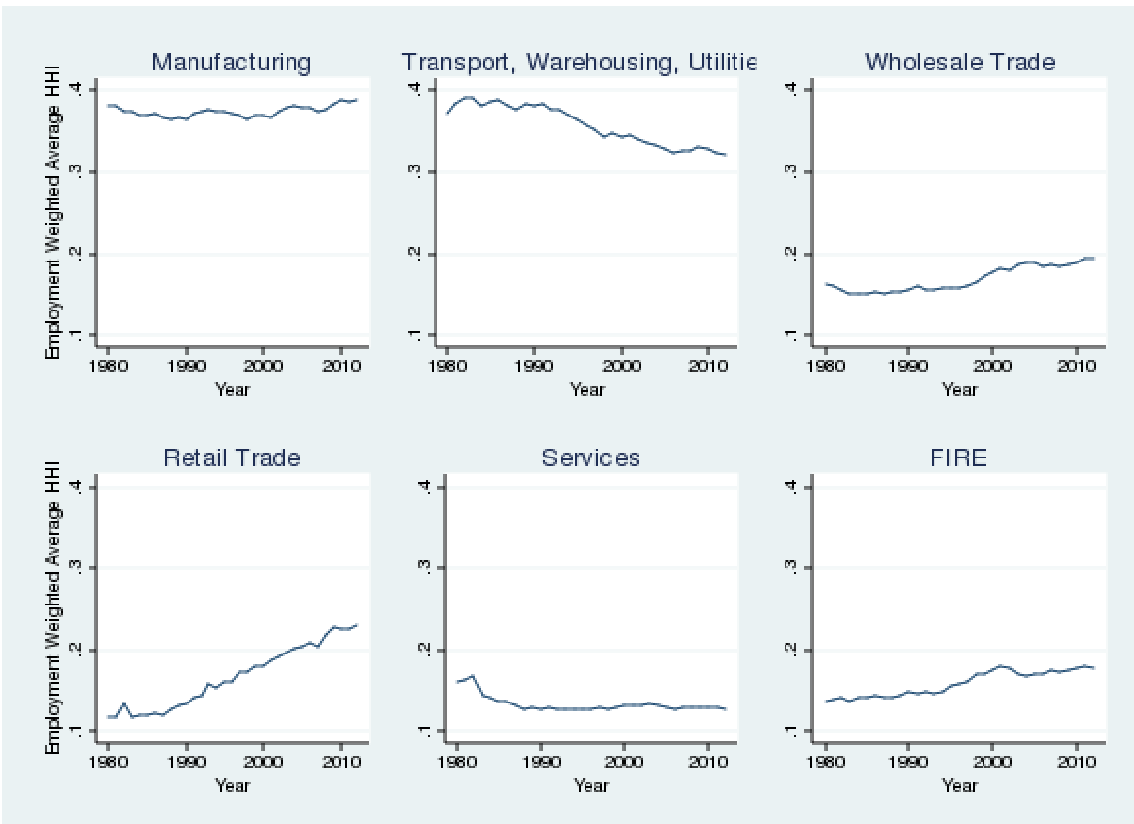


Figure 1.5: Average labor market concentration since 1980 calculated within labor markets

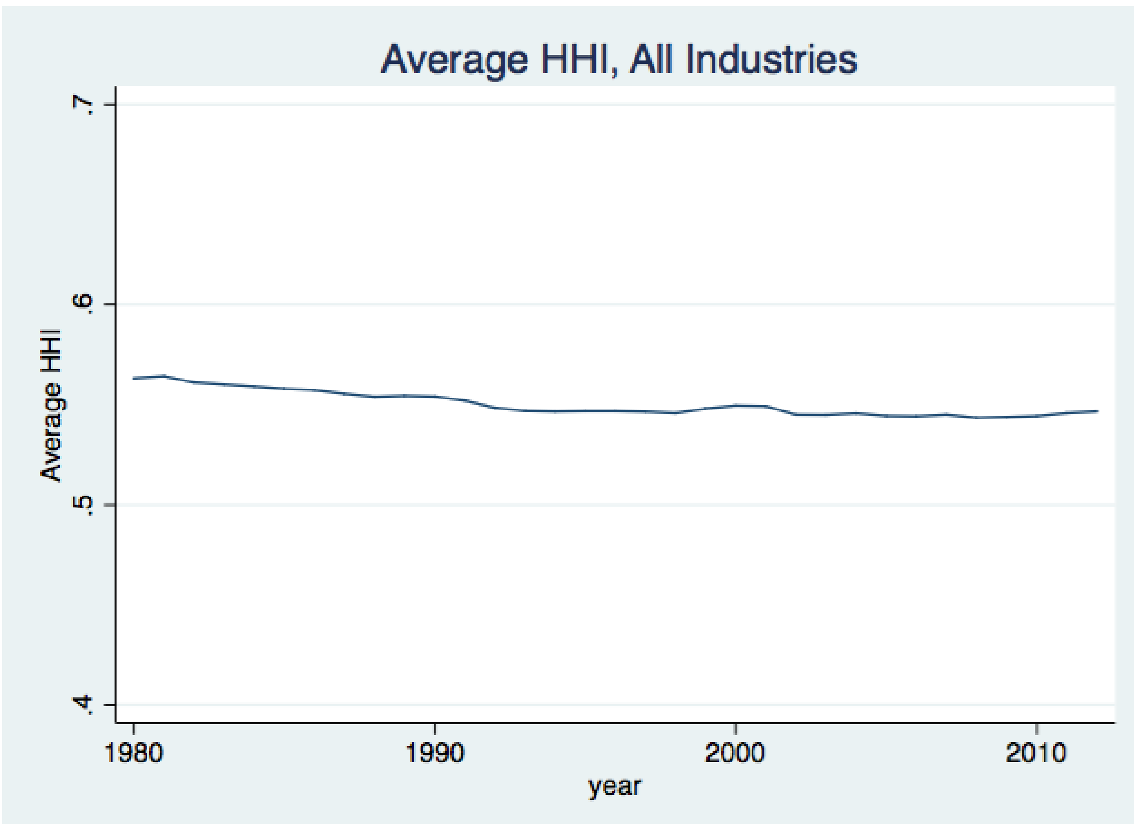


Figure 1.6: Average labor market concentration since 1980 by industry, NAICS 5

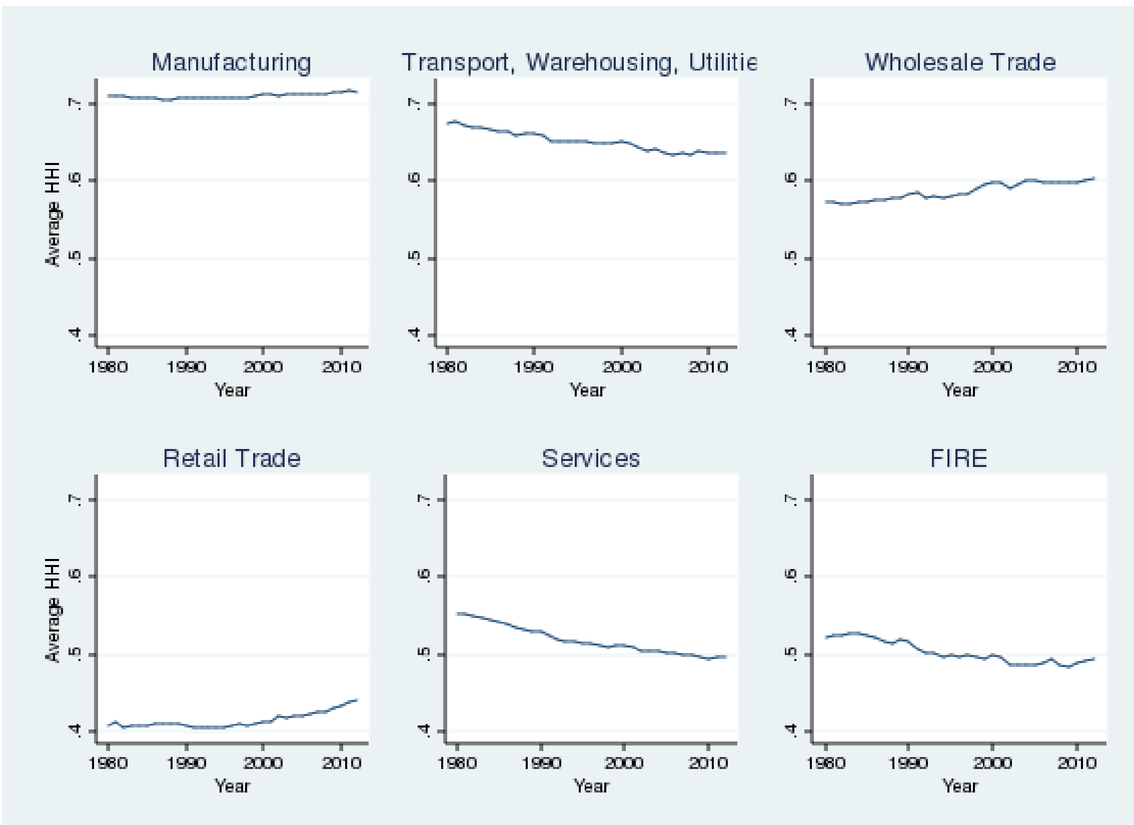
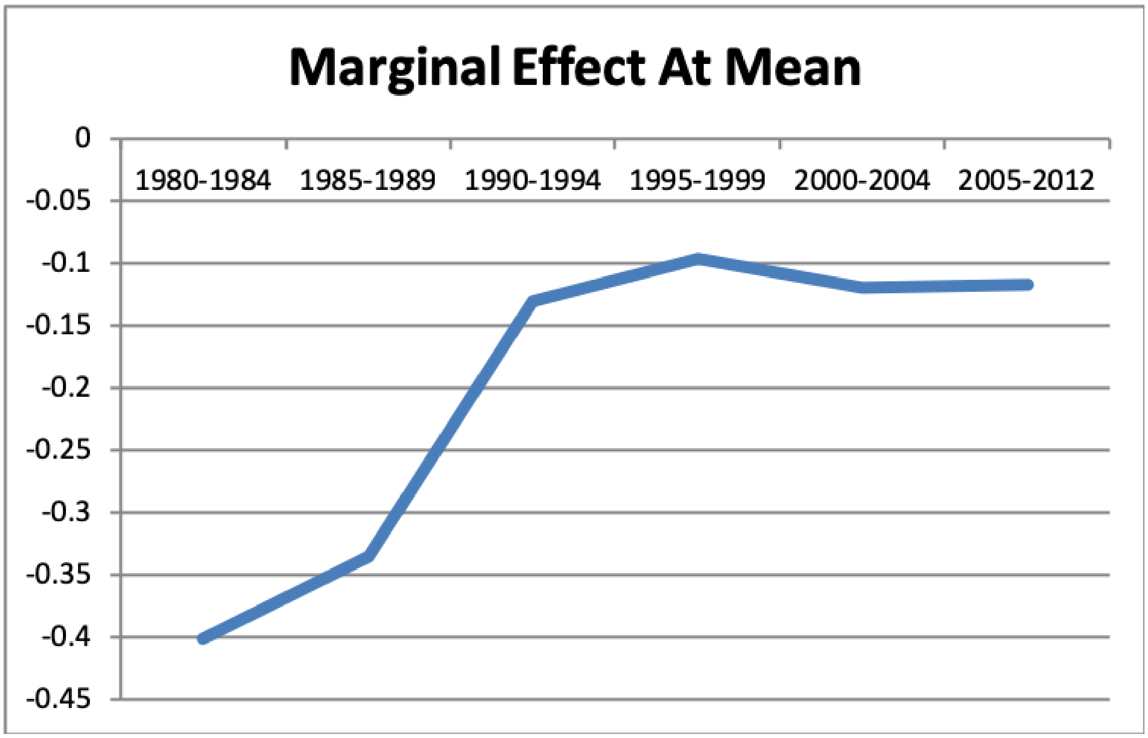


Figure 1.7: The marginal effect of labor market concentration on wages through time



CHAPTER II

The Social Impact of Private Equity Over the Economic Cycle

Work with Steven J. Davis, John Haltiwanger, Kyle Handley, Josh Lerner, and Javier Miranda[1]

[1]University of Chicago and Hoover Institution; University of Maryland; University of Michigan; Harvard University; and U.S. Bureau of the Census. Davis, Haltiwanger, and Lerner are affiliates of the National Bureau of Economic Research. Haltiwanger was also a part-time Schedule A employee at the U.S. Census Bureau during the preparation of this paper. We thank Ron Jarmin and Kirk White for helpful comments on an earlier draft and especially Alex Caracuzzo, Stephen Moon, Cameron Khansarinia, Ayomide Opeyemi, Christine Rivera, Kathleen Ryan, and James Zeitler for painstaking research assistance. Per Stromberg generously gave permission to use older transaction data collected as part of a World Economic Forum project. We thank the Harvard Business Schools Division of Research, the Private Capital Research Institute, the Ewing Marion Kauffman Foundation, and especially the Smith Richardson Foundation for generous research support. Opinions and conclusions expressed herein are the authors and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed (DRB-B0109-CDAR-2018718, DRB-B0110-CDAR-2018-0718, DRB-B0020-CED-20181128, and DRB-B0018-CED-20181126). Lerner has advised institutional investors in private equity funds, private equity groups, and governments designing policies relevant to private equity. All errors and omissions are our own.

2.1 Introduction

We study real-side outcomes at firms acquired in U.S. private equity buyouts. Our sample covers thousands of buyouts from 1980 to 2013, which we link to Census micro data on the target firms, their establishments, and millions of comparable firms and establishments that serve as controls. Our large sample, long time period, high-quality data, and ability to track firms and establishments afford an unusually informative investigation. Broadly speaking, we find that private equity (PE) buyouts accelerate job reallocation, raise productivity, and reduce wages in target firms relative to contemporaneous developments at control firms. There are, however, striking and systematic differences in the effects of buyouts, depending on the nature of the target firm, prevailing credit market conditions, and GDP growth in the broader economy. The number and volume of private equity (PE) buyouts expanded greatly in recent decades (*Kaplan and Stromberg* (2009)), directly touching a sizable share of U.S. employment (*Davis et al.* (2014)). Buyout activity collapsed during the financial crisis of 2007-09 but recovered strongly in recent years, as we show. Proponents see buyouts as engines of efficiency and value creation, fueled by the concentrated ownership of target firms, highly levered capital structures, and high-powered financial incentives (e.g., *Jensen* (1989)). Critics see a very different picture. In their view, heavy reliance of debt financing and an intense focus on investor returns lead to negative effects on target-firm performance and on the jobs and wages of their workers[2]. We develop a wealth of evidence that speaks to the claims of critics and proponents alike. Our study also speaks to broader concerns about financialization of the economy. The financial sectors share of U.S. Gross Domestic Product (GDP) rose from less than four percent in 1950 to eight percent in 2010, accelerating after 1980, as documented by *Philippon* (2015). He also

[2]For a review of the controversies, see *Appelbaum and Batt* (2014) and *Davis et al.* (2014).

provides evidence that the cost of financial intermediation has changed remarkably little since the nineteenth century, despite dramatic advances in information technology that might be expected to lower the costs of creating, pooling, holding, and trading financial assets. *Zingales* (2015) argues that the financial sector is prone to agency problems and other inefficiencies that create a range of distortions in the real economy, many of which are poorly understood and neglected by scholars. We cast new light on how one increasingly important form of financialization affects economic performance. Our study also speaks to the effects of fluctuations in credit availability, which have long worried economists (*Kindleberger* (1978)). One concern in this regard involves the incentives that drive credit decisions. In Rajan (1994)s model, for example, the desire to manage short-term earnings drives bankers to make value-destroying loans in good times and curtail lending abruptly in bad times. A second concern involves the banking systems capacity to supply credit. *Bernanke and Gertler* (1987) develop a theory in which negative shocks to bank capital cause them to forego value-creating loans. A third set of concerns surrounds the effects of credit availability on non-financial firms. According to the financial accelerator mechanism in leading macro models (e.g., *Bernanke et al.* (1999)), endogenous swings in credit availability amplify and propagate the effects of shocks to the macroeconomy. Credit availability and debt levels are a key focus in many post mortems of economic crises from the 1870s to the 2000s (e.g., *Reinhart and Rogoff* (2009); *Campello et al.* (2010); *Schularick and Taylor* (2012)). They are also a first-order concern for modern central bankers. We develop new evidence on how target-firm performance relate to credit market conditions at the time of the buyout and afterwards. We are far from the first to consider relationship between buyouts and credit cycles. Pioneering work by *N. and Stein* (1993) presents evidence that fits a specific version of the overheated buyout market hypothesis [that] the buyouts of the later 1980s [were] both more aggressively priced and more susceptible to costly financial distress. Twenty-five of 66 deals in their sample executed during the easy-credit period from 1986 to 1988 later underwent a debt default, an attempt to restructure debt, or a Chapter 11 bankruptcy filing. In glaring contrast, only one of 41 deals executed from 1980 to 1984, when credit conditions were much tighter, experienced one of these forms of financial distress. *Axelson et al.* (2013) look at a broader sample of transactions and show that credit market conditions drove leverage in buyouts far more than in publicly listed firms, where company-level characteristics were much more influential. *Kaplan and Schoar* (2005), among others, find that easier credit conditions bring greater inflows into buyout funds and lower fund-level returns[3]. In short, the literature suggests that when economic growth booms and credit spreads narrow, private equity funds attract larger inflows, their deals involve more leverage and higher valuations, and investors ultimately receive lower returns.

These empirical patterns suggest to some (e.g., *Appelbaum and Batt* (2014)) that private equity activity is too volatile, too sensitive to credit conditions, and too prone to leverage, with harmful consequences for the broader economy. In line with this view, a 2013 policy statement by U.S. banking regulators provides guidance on leveraged lending as follows: "A financial institution should have clear underwriting standards regarding leveraged transactions as these risks may find their way into a wide variety of investment instruments and exacerbate systemic risks within the general economy."[4] Similarly, European Central Bank guidance on leveraged lending states that "Underwriting of transactions presenting high levels of leverage defined as the ratio of Total Debt to EBITDA

[3]Similarly, *Gompers and Lerner* (2000) show that large inflows into venture capital funds lead to substantially higher valuations in venture investments.

[4]See Office of the Comptroller of the Currency, Board of Governors of the Federal Reserve System, and the Federal Deposit Insurance Corporation, Interagency Guidance on Leveraged Lending, March 22, 2013 at `www.federalreserve.gov/supervisionreg/srletters/sr1303a1.pdf`, pages 6-7. EBITDA is Earnings Before Interest, Taxes, Depreciation and Amortization.

exceeding 6.0 times at deal inception should remain exceptional and trigger a referral to the highest level of credit committee or similar decision-making level."[5] Likewise, *Gregory* (2013) argues that buyouts should be monitored for macro-prudential reasons, because "the increased indebtedness of such companies poses risk to the stability of the financial system."

Notwithstanding these concerns and policy initiatives, claims about excessive cyclicality and leverage in PE deals may be overstated, outdated, or misplaced. Large PE buyouts completed from 2005 to 2008 led to relatively few bankruptcies during or after the global financial crisis (*Primack* (2015)). Compared to other similarly leveraged firms, PE-backed firms were no more likely to default during the financial crisis, and they tended to resolve financial distress more efficiently (*Hotchkiss et al.* (2014)). According to practitioner accounts, the ties that PE firms have developed with the banking industry strengthened the capacity of their portfolio firms to weather financial strains. Close banking ties enabled PE-backed firms to borrow more cheaply, negotiate more favorable "covenant light" agreements,[6] and continue tapping credit during crises (*Ivashina and Kovner* (2011)). *Bernstein et al.* (2018) show that a sample of approximately 400 British PE-backed firms cut investments less than peers during the global financial crisis and had greater equity and debt inflows. Moreover, buyout funds established before 2006 earned greater returns than publicly listed equities, while funds established after 2005 experienced returns similar to public equities (*Jenkinson et al.* (2016)).

[5]See Guidance on Leveraged Transactions, ECB Banking Supervision Division, May 2017, at `https://www.bankingsupervision.europa.eu/ecb/pub/pdf/ssm.leveraged_transactions_guidance_201705.en.pdf`. For a useful comparison of U.S. and European regulatory guidance regarding leveraged lending, see Shearman & Sterling LLP, Leveraged Lending: Summary of ECB Guidance Compared to US Guidance, June 21, 2017, at `www.shearman.com/~/media/Files/NewsInsights/Publications/2017/06/Leveraged-Lending-Summary-of-the-ECB-Guidance-compared-to-the-US-Guidance-FN-062117.pdf`.

[6]Covenant light is a type of financing that places fewer restrictions on borrowers.

As this summary suggests, previous research has considered how the financial characteristics of and returns to PE investments vary over time. There has been little investigation, however, into the broader social effects of PE buyouts over the economic cycle. Do the cyclical patterns in buyout deals carry over to changes in the employment, productivity, and wage outcomes of portfolio firms? In particular, do deals undertaken when GDP growth is high, or credit conditions are easy, have fewer beneficial effects (or more harmful effects) on portfolio firms and their employees? Does a surge of highly leveraged buyout deals set the stage for a painful crunch if and when the economy contracts and credit conditions tighten?

To address these questions, we examine non-financial outcomes for roughly 5,100 U.S. buyouts.[7] Using an improved version of the large-sample methodology in *Davis et al.* (2014), we explore the extent to which PE buyouts affect employment levels, the pace of job reallocation, wages, and productivity, all expressed relative to contemporaneous outcomes at comparable firms not backed by private equity. We focus on how outcomes unfold at PE buyout targets relative to control units over the first two years after the buyout. Some of our main findings follow:

- Target firms are more productive than controls before buyouts, and the differential widens by 9 percentage points over the first two years post buyout. Productivity gains are concentrated in private-to-private and public-to-private buyouts.

- The pace of job reallocation across facilities rises in target firms relative to controls post buyout. Much of this increase involves extra merger and acquisition activity.

[7] Early studies on the real-side firm-level outcomes associated with private equity buyouts include *Kaplan* (1989) and *Lichtenberg and Siegel* (1990). More recent work considers much larger samples, often by exploiting government databases. See *Bernstein et al.* (2016); *Bernstein and Sheen* (2016); *Boucly et al.* (2011); *Cohn et al.* (2014, 2017); *Davis et al.* (2014); *Fracassi et al.* (2018). *Davis et al.* (2014) also summarize several case studies.

- Employment outcomes vary hugely by type of buyout. In public-to-private deals, target employment contracts nearly 13% relative to controls over two years. Divisional sales involve job losses of about 11%. Private-to-private and secondary deals show a strikingly different pattern: target-firm employment expands by 12% and 8% relative to controls in the first two years after the buyout.[8]

- Buyouts executed amidst wider credit spreads experience more intra-firm job reallocation and much greater productivity gains.

- Expanding credit spreads and slow GDP growth post buyout bring slower employment growth for targets relative to controls. For public-to-private deals, they also bring smaller productivity gains.

- Buyouts bring a small, statistically insignificant drop in average earnings per worker at target firms relative to controls.

Wage losses are largest in private-to-private buyouts, while wage changes are positive in divisional sales. These findings point to several broader implications:

1. The social impact of PE buyouts does not lend itself to an easy, simple summary characterization. The real-side effects of buyouts on target firms differ greatly by deal type, with external credit conditions, over the economic cycle, and between existing and greenfield facilities. There are also large differences by industry sector (*Davis et al.* (2014)).

2. Our evidence that tighter credit conditions bring greater productivity gains at targets (relative to controls) suggests a degree of substitution in the levers by which PE groups create value for their investors. In particular, when debt is more expensive (or

[8]Secondary deals refer to the acquisition of a portfolio firm from another PE firm.

less available), PE groups select buyouts that rely more on operating improvements to create value for their investors and less on financial engineering.

3. The relative impact of PE buyouts on employment at target firms is pro-cyclical. Downturns intensify the job losses associated with PE buyouts (relative to controls), particularly for private-to-private and secondary deals.

4. Our results reinforce concerns about public-to-private deals, which account for 10% of PE buyouts from 1980 to 2013 and 32% of employment in target firms. Public-to-private deals exhibit large post-buyout employment losses, a concentration of deals around market peaks, and poor productivity performance during downturns.

The next section describes the creation of our sample. Section II discusses our empirical methodology. Section III presents our baseline results on the social effects of PE buyouts, and Section IV considers how the effects vary with cyclical and credit conditions. Section V concludes.

2.2 Creating Our Samples of Private Equity Buyouts

2.2.1 Identifying Private Equity Buyouts

Our study builds on the data work and analysis in *Davis et al.* (2014).[9] We consider later-stage changes in ownership and control, executed and partly financed by PE firms. In these deals, the (lead) PE firm acquires a controlling equity stake in the target firm

[9] This effort originated as part of the World Economic Forum effort to assess the long-term effects of private equity, and also included a study of the demographics of private equity (published as *Kaplan and Stromberg* (2009)), growth buyouts in France (*Boucly et al.* (2011)) the relationship between private equity and innovation (*Lerner et al.* (2011)), and management practices and private equity (*Bloom et al.* (2015b)).

and retains significant oversight until it exits by selling its stake. The buyout event typically involves a shift toward greater leverage in the capital structure of the target firm and, sometimes, a change in its management. As indicated by our quotations of U.S. and European regulators above, bank loans are key sources of the credit that facilitates the leveraged nature of PE buyouts. We made major efforts to construct our sample of buyouts and ensure its integrity, expending thousands of research assistant hours. Specifically, we undertook a two-part effort, following *Stromberg* (2008). The first part drew on the CapitalIQ database to create a base sample of PE-sponsored leveraged buyout transactions. We selected all M&A transactions in CapitalIQ classified as a leveraged buyout, management buyout, or JV/LBO (joint venture/leverage buyout) and closed between January 1, 1980 and December 31, 2013. To this sample, we added all M&A transactions undertaken by a financial sponsor classified as investing in buyouts. We excluded management buyouts not sponsored by a PE firm and startup firms backed by venture capitalists. Although CapitalIQ has back-filled its database using various sources since starting its data service in 1999, its coverage remains incomplete in the early years of our sample. For this reason, the second part of our sample construction efforts relied on other databases,[10] the business press, and transaction lists for the 1980s compiled by other researchers. The largest source of discrepancies between our CapitalIQ sample and these lists are pure management buyouts: LBO transactions not sponsored by a buyout fund or other financial institution but, instead, undertaken by management itself. Since these management buyouts are not the object of our study, we exclude them from our sample. Naturally, the overlap between our CapitalIQ-based sample and lists compiled by other researchers is greater for LBOs with a financial sponsor. For instance, 62 of the 77 transactions in *Kaplan* (1989) hand-selected sample of LBOs completed between 1980 and 1986 are captured by our CapitalIQ

[10]These include Dealogic, Preqin, and Thomson Reuters.

sample, a coverage rate of 81%. We added these 15 missing transactions to our sample, as we did for other PE buyouts identified using various sources beyond CapitalIQ. In the course of our investigations, we discovered that CapitalIQ classifies certain buyout fund transactions as private placements rather than acquisitions. In most cases, these private placements involve minority stakes or follow-on investments and, hence, are unsuitable for inclusion in our sample. Still, the distinction between buyouts and private placements is not always clear. In addition, some transactions reported as LBO deals were actually venture capital investments, which are not the object of our study. We sought to err on the side of caution by excluding ambiguous transactions and, as a result, may miss some bona fide LBOs. We also excluded acquisitions that were announced but not yet completed by the end of 2013, acquisitions of non-control stakes (typically associated with growth and venture transactions, not classic buyouts), purchases of firms with foreign headquarters, stakes in public companies that remained publicly traded (PIPES), and other misclassified transactions. We identified these transactions through the careful review of text fields in CapitalIQ records and our own detailed research using other commercial databases, securities filings, and media accounts. The resulting sample contains 9,794 PE-led leveraged buyouts of U.S. companies from January 1, 1980 to December 31, 2013.[11] We sort the sample transactions into four main deal types: the buyout of an independent, privately

[11] *Ayash and Rastad* (2017) criticize our approach to distinguishing LBOs from growth equity buyouts, because data service providers have difficulty differentiating between leverage buyouts and growth equity buyouts. They advocate a transaction value cutoff approach, based on the idea that smaller deals are more likely to be growth equity buyouts. The cutoff approach yields two types of errors: (a) in deals larger than the cutoff, the improper inclusion of all growth equity buyouts, and (b) in deals smaller than the cutoff, the improper exclusion of all leveraged buyouts. While some early papers in the PE literature used size cut-offs, they did so due to a lack of data on smaller transactions (e.g., *Kaplan* (1989), footnote 3). Most major papers in the recent literature on PE rely on classification methodologies resembling ours to a greater or lesser extent. Examples include *Axelson et al.* (2013); *Faccio and HSU* (2017); *Fang et al.* (2013); *Ivashina and Kovner* (2011).

held firm (private-to-private); the buyout of a publicly listed firm (public-to-private); the buyout of part of a firm (divisional); and the buyout of a portfolio firm from another PE firm (secondary). We derive our classifications from the textual descriptions of the transactions in CapitalIQ, as well as our own reviews of other databases, press accounts, and securities filings.

2.2.2 Inspecting the Full Sample (Before Linking to Census Data)

Figure 2.1 displays quarterly counts of PE-sponsored buyouts in our sample for these four deal types.[12] As noted in earlier studies, PE buyout activity grew enormously in recent decades. The expansion is especially striking for private-to-private buyouts, which saw a huge increase in deal flow over time. The flow of new PE buyouts crashed in 2008 as the financial crisis gathered force, credit conditions tightened, and the economy contracted. Interestingly, the flow of new public-to-private buyouts dropped off well before the onset of the financial crisis, and remained at modest levels through the end of our sample. Counts for private-to-private deals and secondary sales rebounded sharply as the economy recovered from the 2008-09 recession and maintained a robust pace until the end of our sample in 2013. To set the stage for the analysis below, Table 2.1 presents evidence on how deal flow relates to economic and credit conditions. We regress the natural log of the quarterly buyout count on deal-type indicators interacted with market conditions at the buyout close (top panel) and over the next two years (bottom panel). We use real GDP growth to characterize economic conditions and the yield spread between below-investment-grade corporate bonds and one-month LIBOR for credit conditions.[13] Both regressions include

[12]Because we lack non-Census data on deal size for much of our sample, especially in more recent years, we cannot construct a size-weighted version of Figure 2.1 without matching to Census micro data. Once we match, however, we become subject to Census disclosure rules that preclude a granular depiction of deal flow as in Figure 1. The same point applies to Tables 2.1 and 2.2 below.

[13]See Section IV for the precise definition of these measures.

controls for deal type and a linear time trend. The results are striking. The top panel says that deal volumes are higher when real GDP growth is above its sample median and credit spreads are narrower than the median. Buyout counts are 28 log points (32%) higher for private-to-private deals, 66 log points (93%) higher for public-to-private deals, and 41 (51%) log points higher for divisional sales in periods with above-average GDP growth, conditional on the credit-spread interaction variables and the controls. Buyout counts are 18-26 log points lower when credit spreads are wider than average, conditional on the other regressors. *Axelson et al.* (2013), among others, also document the relationship of credit spreads to buyout activity and to the extent of leverage and valuations. The bottom panel in Table 2.1 says that periods with high buyout volume are associated with rising credit spreads over the next two years and, except for secondary sales, higher than average GDP growth over the next two years. Again, the associations are large in magnitude. For example, buyout counts are 20-68 log points higher in periods that precede above-average increases in credit spreads. This result says that target firms are more likely than not to face a tightening of credit conditions post buyout, an issue that we explore below. Table 2.2 shows how the industry mix of PE buyouts differs by deal type. For instance, public-to-private deals are relatively prevalent in Consumer Staples (e.g., food and household products) and Healthcare, while divisional deals are relatively prevalent in Information Technology and in Utilities. A Pearson chi-squared test rejects the null hypothesis that the industry distribution of buyouts is independent of deal type. The distributions of PE buyouts by industry, firm size, and firm age also differ greatly from the corresponding distributions of private sector employment, as shown in *Davis et al.* (2014). Given the patterns in Tables 1 and 2 and our earlier work, our econometric investigations below compare buyout targets to control units defined by buyout period, industry, firm size, and firm age.

2.2.3 Matching Private Equity Buyouts to Census Micro Data

The Longitudinal Business Database (LBD) is a longitudinal version of the Census Bureaus comprehensive Business Register (BR), which contains annual data on U.S. businesses with paid employees. The LBD covers the entire nonfarm private sector and, in recent years, has roughly 7 million establishment records and 5 million firm records per year.[14] It draws on a wide range of administrative records and survey sources for data inputs. Firms are defined based on operational control, and all establishments majority owned by a parent firm are included in the parents activity measures. Core data items include employment, payroll, four-digit Standard Industrial Classification (SIC) or six-digit North American Industrial Classification (NAICS), employer identification numbers, business names, and location information. To merge our data on buyouts to Census data on firms and establishments, we match business name and address information for the buyout targets to the name and address records in the BR. The Online Appendix describes our matching process in detail. The process yields a mapping to one or more firms in the BR for about 7,600 of the 9,794 U.S. buyouts that we identified from CapitalIQ and other sources. Of these 7,600 buyouts, about 4,100 match to BR identifiers for a single firm, while the other 3,500 map to identifiers for multiple firms. We resolved about 2,000 of these 3,500 cases to a unique match, leaving about 6,000 buyouts that we confidently match to a unique firm in the BR in the period from 1980 to 2013. The main reason we cannot confidently resolve the other 1,500 cases to a unique firm in the BR is because many targets undergo a complex reorganization during the buyout, or shortly thereafter. The reorganization can involve the sale of multiple firm components to multiple parties, the emergence of multiple new firm IDs, and the introduction of a complex array of holding company structures.

[14] An establishment is a physical location where economic activity occurs. A firm is a legal entity that owns and operates one or more establishments.

These cases present considerable matching challenges. There are other challenges as well, as discussed in the Online Appendix. Rather than include matches of dubious quality, we exclude them from our analysis. Once matched to the BR, we can identify all establishments owned by the target firm as of its buyout year. LBD longitudinal links let us compute employment changes for establishments and firms and track their entry, exit, and ownership changes. We supplement the LBD with firm-level revenue data drawn from the Census BR to obtain a revenue-enhanced version of the LBD (RE-LBD). The revenue data, available from 1996 to 2013, let us study the impact of PE buyouts on labor productivity, defined as real revenue per worker. About 20 percent of LBD firm-year observations cannot be matched to BR revenue data because firms can report income under EINs that may fall outside the set of EINs that Census considers part of that firm for employment purposes. *Haltiwanger et al.* (2017) provide additional information about the revenue data.

2.2.4 Treatment of Timing Matters

Given our interest in employment dynamics, the relationship of the LBD employment measure to the timing of PE buyouts requires careful treatment. The LBD reports total employment in the payroll period containing the week of March 12. Accordingly, for buyouts that close before October 1, LBD employment in March of the same calendar year serves as our contemporaneous employment measure. We assign transactions that close on or after October 1 in calendar year t to the LBD employment value in March of $t + 1$. October is the natural cutoff because it lies midway between March-to-March employment changes in the LBD.[15] Henceforth, our references to buyout activity in year t

[15] Fractional-year mistiming of buyout deals is unavoidable when matching to the LBD, given its annual frequency. When buyouts are uniformly distributed over the year, an October cutoff minimizes the mean absolute mistiming gap. See Davis et al. (2018) for additional discussion. As an empirical matter, buyout transaction dates are distributed fairly evenly over the calendar year.

refer to deals that closed from October of calendar year $t-1$ through September of calendar year t. In particular, buyouts that closed in October, November or December of 2013 are shifted forward to 2014, beyond the time span covered by our LBD data. As a result, these matched targets are not part of our analysis sample. All told, we are left with about 6,000 matched target firms acquired in PE buyouts from 1980 to 2013. These firms operated about 177,000 establishments as of the buyout year and had nearly 7 million workers on their payrolls as of March in the buyout year.

2.2.5 Tracking Firms after the Buyout and Forming Our Analysis Sample

Of necessity, much of our analysis restricts attention to target firms that we can track after the buyout. While we can readily track establishments over time in the LBD, tracking firms is more challenging for two main reasons: the disappearance of firm identifiers (IDs), and irregularities in Census Bureau tracking of PE targets involved in certain divisional sales. We elaborate on these two reasons in turn.

Firm ID Disappearance The disappearance of a firm ID in the LBD can occur for various reasons. One is the death of a firm and the closure of all of its establishments. Firm death in this sense presents no problem: we capture such events whether they involve target or control firms. A more difficult situation involves a target firm ID in the buyout year that disappears in later years, even though some of the establishments owned by the firm (as of the buyout year) continue to operate. This situation can arise when the various components of the original firm are acquired by multiple existing firms. It is inherently difficult to define and measure firm changes when the original legal entity ceases to exist, and we exclude these cases from our firm-level longitudinal analyses.[16] To reduce the number of observations lost for this reason

[16] Even establishments are challenging to track in some circumstances. Every five years, the

and other challenges in tracking firms over time, we restrict our longitudinal analyses to the buyout year and the next two years.

Divisional Transactions . In principle, the Annual Company Organization Survey lets Census accurately track the business units involved in divisional sales. However, we discovered divisional sales in which the firm ID of the (new) target firm remained the same as the firm ID of the selling firm. This situation indicates that the new firm created in the course of the divisional buyout did not receive a new firm ID, at least not in a timely manner. This problem does not preclude an establishment-level analysis, because we can often use an alternative identifier the Employer Identification Number (EIN) to accurately identify, as of the buyout year, the establishments involved in divisional sales. Unfortunately, EINs are unsuitable for tracking firms through time, because new and acquired establishments may obtain new EINs. Thus, we exclude divisional buyouts from our firm-level longitudinal analyses when the LBD lacks an accurate firm ID for the newly created target firm. Table2.3 summarizes our sample of PE buyouts matched to Census micro data. Panel A reports the number of establishments operated by our 6,000 matched target firms and their employment, with breakdowns by deal type. Panel B considers the 5,100 matched buyouts that closed from 1980 to 2011. Compared to the 1980-2003 sample in Davis et al. (2014), our new 1980-2011 analysis sample has 2.3 times as many matched targets, reflect-

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Census Bureau obtains a full list of establishments owned by multi-unit firms from the Economic Censuses. It also obtains a full list of establishments owned by large multi-unit firms (250 or more employees before 2013) from the Annual Company Organization Survey (COS). However, the COS samples smaller multi-unit firms in a targeted manner based on information that they underwent rapid growth or organizational change. Thus, Census may not promptly recognize the ownership of establishments operated by small, multi-unit firms in intercensal years. To address this matter, the LBD retimes the intercensal entry and exit of some establishments operated by small multi-unit firms. Still, the timing of M&A activity for small multi-units not covered by the COS or other Census surveys exhibits some bunching in Economic Census years.

ing high deal flow after 2003. Private-to-private deals account for about half of our 1980-2011 sample, as in our earlier work. But the 22% share of secondary sales is nearly twice as large as in our earlier work, reflecting a large flow of these deals in recent years. The share of divisional buyouts is somewhat smaller in our new sample. These compositional changes over time can also be seen in Figure 2.1. Panel C compares matched buyouts in our new sample to those in *Davis et al.* (2014) for the overlapping period from 1980 to 2003. Our new sample has about 20% fewer buyouts in the overlapping period, which reflects the more rigorous matching criteria that we now apply. Our new sample of two-year continuer targets, excluding EIN cases, has only 10% fewer matched buyouts. The mix of buyout types in our new 1980-2003 sample is similar to that in our earlier work, but our new sample has considerably fewer establishments and less employment.

2.3 Empirical Methods and Identification Assumptions

This section describes several important aspects of our empirical methods. The first relates to how we track business outcomes over time. While we focus on firm-level outcomes, we exploit the establishment-level data in the LBD in several ways: to distinguish organic changes at the firm level from acquisitions and divestitures, to capture new facilities opened post buyout, and to decompose firm-level employment changes into the gross job creation and destruction components associated with growing and shrinking establishments, respectively. The LBDs capacity to isolate each of these adjustment margins is one of its major strengths.

A second aspect relates to aggregation and the measurement of growth rates. Let E_{it} denote employment at establishment or firm i in year t i.e., the number of workers on payroll in the pay period covering March 12. We measure the employment growth rate of unit i from $t-k$ to t as $g_{it,t-k} = (E_{it} - E_{i,t-k})/X_{it,t-k}$, where$X_{it,t-k} = 0.5(E_{it} + E_{i,t-k})$. This growth rate measure is symmetric about zero and lies in the interval [-2, 2], with endpoints corresponding to death and birth.[17] Employment growth at higher levels of aggregation is then given by $g_{t,t-k} = \sum_i (X_(it, t-k/X_{t,t-k})g_{it,t-k}$, where $X_{t,t-k} = \sum_i X_{it,t-k}$. Using these formulas, we can easily and consistently aggregate from establishments to firms, from individual units to industries, and over time periods. This approach to growth rates and aggregation also works for gross job creation and destruction, job reallocation, and employment changes on particular margins such as acquisitions and divestitures or continuing establishments.

A third aspect relates to the selection of control units for comparison to buyout targets in our regression models. We need suitable control units because the distribution of private equity buyouts across industries and business characteristics is not random. Target firms are larger and older than the average firm and disproportionately concentrated in manufacturing, information technology, accommodations, and food services (*Davis et al.*, 2014). They also differ by deal type, as shown above. Moreover, growth and volatility vary greatly by firm size and age, and the workplace and production process differ greatly by industry.[18] In view of these facts, we sort target firms into cells defined by industry, size, age, multi-unit status, and buyout year. We then identify all firms not backed by private equity that fall into the same cell as the given target firm(s), and treat those firms as control units for the target firm(s) in that cell. Specifically, we define our control cells as

[17]This growth rate measure has become standard in analyses of establishment and firm dynamics, because it shares some useful properties of log differences while also handling entry and exit. See *Davis et al.* (1998) and *Tornqvist et al.* (1985).

[18]Much previous research highlights sharp differences in employment growth and the pace of job reallocation by firm size, firm age and industry. See, for example, *Davis et al.* (1998); *Haltiwanger et al.* (2013).

the full cross product of about 90 industries (3-digit NAICS), 10 firm size categories, 6 firm age categories, a dummy for firms with multiple establishments, and 32 distinct buyout years from 1980 to 2011.[19] This classification yields over 10,000 control cells per year. Of course, many cells are unpopulated, but the flexibility and richness of our approach to control units is clear.

Fourth, we estimate the effects of buyouts using a difference-in-difference approach. That is, we compare changes in jobs, wages, and productivity at target firms in the wake of buyouts to contemporaneous changes at their matched control units.[20] This approach, in combination with our rich set of controls, facilitates an apples-to-apples comparison when estimating the effects of buyouts.

A fifth aspect pertains to how we weight observations in the estimation. In this regard, we are mindful that buyout effects can vary with firm characteristics and economic conditions and by industry, deal type, and time period. Indeed, we find material differences in the effects of buyouts on some of these dimensions, as discussed below. However, there is surely more heterogeneity in treatment effects than we can estimate with precision. Faced with this heterogeneity, our goal is to obtain a consistent estimate for the activity-weighted mean treatment effect on treated units under two common identification assumptions in regression studies of treatment effects:

CMI (conditional mean independence) Conditional on controls and the treatment

[19]We define industry for multi-unit firms based on the modal industry of their establishments, computed on an employment-weighted basis. Our firm size categories are 1-4, 5-9, 10-19, 20-49, 50-99, 100-249, 250-499, 500-999, 1000-2499, 2500-4999, 5000-9999, and 10000 or more employees. Our firm age categories are 0-5 years, 6-10, 11-15, 16-20, and 21 or more years. Following *Davis et al.* (2014), when a firm first appears in the LBD, we assign it the age of its oldest establishment. We then increment the firms age by one year for each year it continues as a legal entity in the LBD. In this way, we avoid arbitrary increases or decreases in firm age due to the sale and purchase of establishments.

[20]In *Davis et al.* (2014), we find that propensity score matching estimators yield very similar results. We stick with the control cell approach in this paper for simplicity.

indicator, outcomes for treated and non-treated units are independently distributed within cells.

SUTVA (stable unit treatment value) Treating one unit has no effect on the outcomes of other units.[21]

To achieve our estimation goal, we adopt two principles in weighting the observations:[22]

TS (target-share weighting) Weight each target (and each target cell) by its share of aggregate target activity, where aggregate refers to the sum over all buyouts in the regression sample.

SCT (set control weights to targets) Set the sum of weights on controls in a given cell to the cells target activity share.

To be precise, suppose we have two target firms in two separate control cells, and we are interested in target-control comparisons from t to $t+k$. The targets have activity levels $X_{(1,t+k,t)} = 0.5(E_{(1,t+k)} + E_{1t})$ and $X_{(2,t+k,t)} = 0.5(E_{(2,t+k)} + E_{2t})$. The first targets share of aggregate target activity is $\omega_{(1,t+k,t)} \equiv X_{(1,t+k,t)}/(X_{(1,t+k,t)} + X_{(2,t+k,t)})$, and the seconds share is $\omega_{(2,t+k,t)} \equiv X_{(2,t+k,t)}/(X_{(1,t+k,t)} + X_{(2,t+k,t)})$. Since each control cell has a single target, these are also the control cell weights.[23] Principle SCT requires $\sum_j^{(\mathbb{C}=1)} \omega_{(j,t+k,t)} = \omega_{(1,t+k,t)}$ and $\sum_j^{(\mathbb{C}=2)} \omega_{(j,t+k,t)} = \omega_{(2,t+k,t)}$, where $\mathbb{C}$ indexes control cells, and j indexes control units in the cell.

[21]See Chapter 18 in *Wooldridge* (2002) for an extended discussion of CMI and SUTVA in panel regression studies of treatment effects.

[22]Neither equal weighting nor simple activity weighting of regression observations recovers the average treatment effect of interest.

[23]Note that we define a units activity level as the average of its employment at the start and end of the time interval under consideration. This practice conforms to our overall approach to aggregation and growth rate measurement, as discussed above in the main text.

Principle TS helps recover an average treatment effect that reflects the distribution over cells of target activity levels. Principle SCT has a similar motivation. It also ensures that the influence of control units on the coefficient estimates for covariates reflects the distribution over cells of target activity levels. Principle SCT is silent on exactly how to set control unit weights within cells, so long as they sum to the cells share of aggregate target employment. In practice, we weight each control unit in proportion to its share of employment among the control units in the cell. After obtaining these proportions, we rescale then to satisfy SCT. We experimented with other approaches to weighting control units that comply with SCT. In particular, we tried equal weights for all control units within a given cell. We also tried winsorizing the weights of very large control units before rescaling to comply with SCT.[24] These alternative approaches to weighting control units led to results similar to the ones reported below.

Recall that we aim to recover the average treatment effect on the treated (buyout) firms under CMI and SUTVA. A standard approach is to fit a regression model with heterogeneous treatment effects, average over the treatment effect estimates, and compute the standard error for the average treatment effect by the delta method. (See, e.g., Chapter 18 in *Wooldridge* (2002).) That is the approach we took in *Davis et al.* (2014). Weighting principles TS and SCT afford a simpler econometric approach that recovers the average

[24]Three concerns motivated our experimentation with alternative schemes that give less weight to larger control units, while still adhering to principle SCT. First, very large employment values for certain control units could reflect measurement error. This concern might apply to targets as well, but since our sample has only a few thousand targets, we scrutinize them carefully. We believe we have identified (and corrected) gross errors in target outcomes. A similarly careful approach for controls is infeasible, since there are so many of them. Second, it is often hard to fit very large firms into a particular industry category, even at the three-digit NAICS level. The classification challenges presented by such large firms raise concerns about the suitability of the treatment-control comparison. Third, the very largest control firms can be much larger than the corresponding target firm. The vast difference in size raises a different source of concern about the suitability of the treatment-control comparison. By applying equal weights to control units in a given cell or winsorizing the weights, we mitigate these concerns.

treatment effect of interest from a specification with a homogenous treatment effect. Under this simpler approach, we need not resort to the delta method to obtain standard errors. We can instead obtain them directly from the standard output for weighted least squares regressions in STATA and other widely used statistical packages. That is the approach we take here.

2.4 Analysis of Social Impact

2.4.1 The Regression Specification and Additional Remarks about Identification

Our firm-level regression analysis considers the same type of semi-parametric specifications as our earlier paper. To be precise, we estimate specifications of the following form by least squares, weighting each observation as detailed in Section 2.3:

$$Y_{(i,t+2)} = \alpha + \sum_c D_{cit}\theta_c + \lambda_1 LEST_{it} + \lambda_2 FIRM_{it} + \gamma PE_{it} + \varepsilon_{it},$$

where $Y_{(i,t+2)}$ is the change in the outcome variable of interest from buyout year t to two years later for firm i. The D_{cit} are cell-level dummy variables defined on the full cross product of buyout year t, the firms three-digit NAICS, its size category, its age category, and an indicator for whether it has one or multiple establishments. $LEST_{it}$ and $FIRM_{it}$ are controls for the firms pre-buyout growth history. To construct $LEST_{it}$, we consider the set of establishments owned by firm i in buyout year t and compute their employment growth rate from $t-3$ to $t-1$. To construct $FIRM_{it}$, we consider the parent firm that owned these establishments in $t-3$ and compute its growth rate from $t-3$ to $t-1$. If ownership was split across multiple firms in $t-3$, we select the firm with the largest share of employment among these establishments. Often, but not always, these two control variables take on the same value.

PE_{it} is a dummy variable equal to 1 for a target firm. Per our discussion of weighting in Section 2.3, the coefficient γ recovers a consistent estimate of the weighted average treatment effect on treated units (i.e., buyout targets) under assumptions CMI and SUTVA. Our rich set of controls lends greater plausibility to the CMI assumption than in most previous work on PE buyouts. Even if CMI fails, our results throw light on the economic role of private equity, and provide useful evidence for formulating and evaluating theoretical models of PE behavior and its effects. The SUTVA assumption could fail if treatment effects on targets systematically alter market equilibrium outcomes for controls through demand and supply channels or by competitive pressures that stimulate productivity gains at controls. Since buyout targets account for modest activity levels compared to controls, standard market equilibrium effects are unlikely to be important in our setting, especially within our two-year post-buyout time frame.

2.4.2 Average Treatment Effects Over All Buyouts

Table 2.4 presents our first set of regression results. The sample contains firms that underwent buyouts from 1980 to 2011 and matched control firms in the same cells. The top row in Panel A says that employment at target firms shrinks by a statistically insignificant 1.4 percentage points relative to control units in the two years after the buyout. The second row says that target-firm employment shrinks by a statistically significant 4.4 points relative to controls when omitting post-buyout acquisitions and divestitures. These bottom line effects of PE buyouts on target firm employment are moderately larger than we found in *Davis et al.* (2014): -0.9 percentage points overall, and -3.7 points for organic growth.

The other rows in Panel A break down the overall employment change into several margins of adjustment. Continuers refer to establishments that operate under ownership of the same firm (target or control) throughout the period from t to $t + 2$. Continuer employment at target firms shrinks by (a statistically insignificant) 1.5% relative to control counterparts in the two years after buyout. The rate of employment change at growing continuers is essentially identical for buyouts and controls, as indicated by the Creation results. In contrast, contracting continuers shrink more rapidly, as indicated by the Destruction results. Target firms experience 4.0% larger employment losses from shuttered establishments (Deaths) and 1.2% greater employment gains due to new facilities (Births). They also add 3.7% more jobs through acquisitions.

Because the regressions are employment weighted, we can sum the coefficients over the margins. Consider first the results for "Continuers" and "Deaths" which capture all employment changes for establishments owned and operated by targets and controls in the buyout year. Summing these two components yields a two-year employment growth rate differential of -5.6 percentage points (-1.53 4.03) for targets. That is, establishments operated by target firms as of the buyout year shed 5.6% of employment relative to controls over the next two years, largely through establishment shutdowns. Factoring in the greater propensity of target firms to create more new jobs at new establishments adds 1.2 points to this sum. That yields a net differential of -4.4 percentage points for targets, the same as the organic growth change in the second row. Further factoring in the role of acquisitions and divestitures adds 3.0 points, yielding an overall buyout effect on firm-level employment of -1.4 percentage points over two years.

Panel A also provides evidence that buyouts raise job reallocation. Compared to controls, target firms exhibit greater job destruction through establishment shutdowns, more job creation through establishment births, more employment losses through divestitures, and greater employment gains through acquisitions. In short, targets undergo a faster pace of job reallocation after buyouts than controls. We delve more deeply into the reallocation effects of buyouts shortly.

How buyouts affect wages has long been controversial. Critics argue that buyouts lead to lower wages, as formalized by *Shleifer and Summers* (1988). Indeed, *Lichtenberg and Siegel* (1990) find that buyouts lead to lower compensation for white-collar workers. More recently, *Agrawal and Tambe* (2016) suggest that buyouts can enhance human capital in target firms, particularly by developing employee knowledge of information technology. Survey evidence in *Gompers et al.* (2016) is consistent with this view.

Panel B in Table 2.4 provides new evidence on the wage effects of PE buyouts using a much larger sample than previous studies. Our wage measure is the firms gross annual compensation per employee.[25] We consider the same sample as before, except for dropping firms that close all establishments by t+2, because we cannot calculate wage changes for firms that die. (There are very few such firms among targets.) The first row in Panel B reports a modest, statistically insignificant wage drop of -0.3% at target firms relative to controls over two years post buyout. The next two rows in Panel B show that target firms pay a wage premium of about 3% in the buyout year and two years later. Thus, we find no evidence that PE buyouts have systematic effects on wages at least when aggregating

[25] *Barth et al.* (2014) provide a detailed description of the LBD wage measure: The data follow the definition of salaries and wages used for calculating the federal withholding tax. They report the gross earnings paid in the calendar year to employees at the establishment prior to such deductions as employees social security contributions, withholding taxes, group insurance premiums, union dues, and savings bonds. Included in gross earnings are all forms of compensation such as salaries, wages, commissions, dismissal pay, paid bonuses, vacation and sick leave pay, and the cash equivalent of compensation paid in kind. Salaries of officers of the establishment, if a corporation, are included. Payments to proprietors or partners, if an unincorporated concern, are excluded. Salaries and wages do not include supplementary labor costs such as employers Social Security contributions and other legally required expenditures or payments for voluntary programs. Thus, our wage measure includes management compensation except for stock option grants, which are typically constructed to defer tax obligation until exercise or sale. Buyouts often tilt the compensation of senior management toward stock options (*Leslie and Oyer*, 2008), which means we may slightly understate the true wage change at target firms.

over deal types and time periods.

Panel C reports results for firm-level revenue productivity, measured as the log of Real Revenue per Worker using the industry-level price deflators described in *Haltiwanger et al.* (2017). As noted above, the revenue productivity data are available for about 80 percent of the firm-level observations from 1996 onwards. To address the potential selection bias introduced by missing productivity observations, we construct inverse propensity score weights for the observations as in *Haltiwanger et al.* (2017). These weights ensure that the re-weighted RE-LBD is representative of the LBD universe with respect to the size, age, employment growth rate, industry sector, and multi-unit status of firms. We apply these weights in the regression analysis of productivity growth in addition to the activity weights described in Section 2.3.

The second row of Panel C says that target firms are 35 log points more productive than control firms as of the buyout year, a very large gap. The gap widens by 9 log points over the next two years after the buyout, according to the productivity change regression reported in the top row of Panel C, and by 6 log points when comparing the productivity level regression in $t + 2$ to the one in t.[26] Our earlier work in *Davis et al.* (2014) finds that PE buyouts lead to smaller TFP gains at target firms relative to controls in the manufacturing sector. Here, we find a larger effect of PE buyouts on labor productivity when looking across all industry sectors.[27]

Table 2.5 reports the estimated impact of PE buyouts on two measures of reallocation activity. The overall job reallocation rate for a firm is the sum of its gross job gains due

[26] Our propensity score weights that adjust for the missing productivity observations differ across the three regressions in Panel C. That is why the level and change regressions yield somewhat different estimates for the effect of buyouts on productivity.

[27] *Foster et al.* (2006) show that gross output per worker and TFP are highly correlated within industries, presumably because materials and capital shares are similar across firms within industries. Because our control variables include industry-by-year effects, we effectively perform within-industry comparisons in our productivity growth regressions.

to new, expanding, and acquired establishments and its gross job losses due to exiting, shrinking, and divested establishments. A firms excess reallocation rate is the difference between its job reallocation rate and the absolute value of its net growth rate.[28] If a firm changes employment in the same direction at all of its establishments, then its excess reallocation is zero. To the extent that a firm expands employment at some units and contracts employment at others, it has positive excess reallocation. If the firm adds jobs at some of its establishments and cuts an equal number of jobs at other establishments, then excess reallocation equals overall job reallocation.

According to Table 2.5, the overall job reallocation rate is 7.1 percentage points higher at targets for organic employment changes over the two years after the buyout and 11.5 points higher when including acquisitions and divestitures, both highly significant. These results confirm our previous inference that PE buyouts accelerate the pace of reallocation at target firms, more so when including acquisitions and divestitures. The excess reallocation rate is 5.0 percentage points higher at target firms for all changes, but (insignificantly) lower for organic changes. The implication is that the faster pace of job reallocation induced by buyouts mainly involves greater reallocation of across firms rather than within target firms. That is, PE buyouts lead to net job losses at some target firms (relative to control units) and net job gains at other target firms. The extra between-firm reallocation of jobs induced by PE buyouts equals 6.5 (11.5 - 5.0) percent of initial employment over the first two years after the buyout.

[28]This concept of excess reallocation is often used in the literature on gross job flows to analyze the nature of job reallocation within and between industries or sectors. Examples include *Dunne et al.* (1989) and *Davis and Haltiwanger* (1992, 1999). Our approach here applies the concept to the reallocation of jobs across units within firms.

2.4.3 Treatment Effects by Buyout Type

There are sound reasons to expect the social impacts of PE buyouts to vary by deal type. Public-to-private deals involve target firms with highly dispersed ownership. These firms may suffer from poor corporate governance and face a strong need for cost cutting. Buyouts of privately held firms may be more often motivated by a desire to professionalize management or gain better access to financing. Some divisional sales involve units that fit poorly with the pre-buyout parent firm. In other divisional sales, the parent firm recognizes a need for downsizing but outsources that unpleasant task to new PE owners in an effort to shield its public image and employee morale in the rest of the firm. Some secondary sales reflect an incomplete effort by the initial PE owner to improve the operations and profitability of the target firm, often truncated by the desire to have a successful exit prior to raising a new fund.

In light of these observations, Table 2.6 reports regression results by deal type. The outcome variables and specifications parallel the ones in Tables 2.4 and 2.5. As seen in the top row of Panel A, the employment effects of PE buyouts differ dramatically by deal type. Target employment contracts nearly 13 percent relative to controls over two years post buyout in public-to-private deals. This result, along with the high visibility and large employment share (31% of target employment from 1980 to 2013) of public-to private deals, helps explain concerns about job losses in PE buyouts. Divisional sales also involve large job losses relative to controls about 11 percent over two years. The similarities between public-to-private and divisional deals are perhaps unsurprising, given that both typically involve sellers who are publicly traded entities. In sharp contrast, target employment jumps by 13% relative to controls in private-to-private deals (26% of target employment) and by 10% in secondary deals (19%). Buyout effects also differ sharply for organic changes: -10%

and -16% for public-to-private and divisional deals versus +4% and +6% for private-to-private and secondary deals.

Turning to Panel B, buyouts bring large upticks in overall job reallocation for all deal types, with magnitudes ranging from 9% of buyout-year employment in secondary sales to 19% in divisional sales. However, the character of the extra buyout-induced reallocation differs among deal types. Job losses in public-to-private and divisional deals largely reflect establishment closures and, for divisional deals, job cuts in continuing establishments. Buyout-induced job gains in private-to-private and secondary deals reflect the important roles of acquisitions and establishment births and, for secondary sales, a boost in job creation at continuers. For public-to-private deals, essentially all of the extra job reallocation reflects a downsizing of some target firms (relative to controls) and an upsizing of others. In other words, targets show virtually no uptick in excess reallocation in public-to-private deals. In contrast, an uptick in excess reallocation at target firms accounts for one-half to two-thirds of the extra buyout-induced job reallocation in the other deal types. For divisional sales, most of the extra excess job reallocation occurs on organic margins.

Panel C focuses on wage differences and effects associated with PE buyouts. At the time of buyouts, employees in public-to-private and divisional targets receive sizable wage premia relative to their counterparts in control firms, while employees in secondary targets receive a discount. More noteworthy for our purposes, earnings per worker rise by 11% in divisional targets relative to controls over two years post buyout, while falling by 6% in private-to-private deals. We find smaller, statistically insignificant wage declines for public-to-private and secondary deals.

Large post-buyout wage gains at divisional targets may partly reflect what practitioners call job title upgrading: When a corporate division becomes a new stand-alone firm, the divisional general manager (or his replacement) becomes CEO, the divisional controller becomes CFO, and so on. The new titles and firm-wide responsibilities often come with (much) higher pay. The Carlyle Groups divisional buyout of DuPont Performance Coatings (renamed Axalta Coating Systems) in February 2013 offers a case in point.[29]

Panel D considers productivity changes. Again, we find large differences in buyout effects by deal type. Target firms in private-to-private deals experience a gain in revenue per worker of 14 log points over two years post buyout relative to control counterparts. Targets in public-to-private deals enjoy even larger gains, but the imprecise estimate precludes a strong inference.

In summary, Table 2.6 says the social impacts of PE buyouts vary greatly by deal type, as anticipated. The pattern of results is broadly consistent with the limited body of evidence compiled in previous research on the real-side effects of PE buyouts. (The literature on private equity is voluminous but mainly speaks to financial characteristics and outcomes.)

Private-to-private deals exhibit high post-buyout employment growth (largely but not entirely due to acquisitions), wage reductions, and large productivity gains. These results align with the view that private equity eases financing constraints at target firms, enabling their expansion (*Boucly et al.*, 2011). The large productivity gains align with evidence in *Bloom et al.* (2015b) that PE buyouts bring better management practices. Their sample contains buyouts of middle-market firms for which private-

[29]The top five personnel of Axalta received compensation in 2013 of $17.2 million, including the aggregate fair value of stock option awards as of the grant date. While the reporting of the value of the option grants may differ for tax purposes (and hence in our data), even the total non-option compensation of the five individuals was $6.1 million. We cannot directly observe the compensation of the top five employees of DuPont Performance Coatings in 2012, but web sites such as Glassdoor suggest that senior divisional managers at DuPont received contemporaneous compensation packages in the mid-six figures. See Axalta Coating Systems, Schedule 14A, March 23, 2015 and *Lerner and Tuzikov* (2018). Thus, the compensation of top Axalta personnel in 2013 was much greater than what they, or their counterparts, likely earned as senior divisional managers before the buyout.

to-private deals are likely to predominate.

Public-to-private deals exhibit large job losses, often through facility closures, and large (imprecisely estimated) productivity gains. The large job losses in these deals (and in divisional sales) may partly reflect the workforce recontracting hypothesis of *Shleifer and Summers* (1988). They may also partly reflect a concentration of these deals in advance of credit-market tightening, a topic we consider in the next section.

Divisional deals also involve large job losses, through both facility closures and cutbacks at continuers, but large gains in compensation per worker.

Secondary deals exhibit high target employment growth, largely organic, and few discernable effects otherwise. This pattern resonates with *Degeorge et al.* (2016), who find positive financial performance in secondary deals. It is reasonable to hypothesize in many cases, that the previous PE owner undertook considerable restructuring, setting the stage for rapid employment growth after the secondary buyout.

2.5 How the Impact of Buyouts Varies with Market Conditions

We now investigate how the social impact of PE buyouts varies with market conditions. To do so, we estimate expanded regression specifications of the form,

$$Y_{(i,t+2)} = \alpha + \sum_{c} D_{cit}\theta_c + \lambda_1 EST_{it} + \lambda_2 FIRM_{it} + \gamma PE_{it} + \beta PE_{it} * MktCondition_t + \varepsilon_{it},$$

where the new term, $\beta PE_{it} MktCondition_t$, captures the interaction between buyout status and market conditions. When using intra-year variation in market conditions, we also include the $MktCondition_t$ main effect. When using only annual variation, we cannot separately identify the main effect since our cell-level controls encompass annual time effects.

Table 2.7 considers two measures of market conditions when the buyout closed: the log change in real GDP over the four-quarter interval ending in the quarter of the buyout closing (using U.S. Bureau of Economic Analysis data) and the spread in the buyout month between the yield to maturity in the Bank of America Merrill Lynch U.S. High Yield Index for corporate bonds and the one-month LIBOR. Tables 2.8 and 2.9 instead consider how market conditions evolve after the close. We measure post-buyout changes in market conditions from March (or the first quarter) of year t to March (first quarter) of year $t+2$.[30] In short, Table 2.7 tells us how targets fare post buyout (relative to control firms) as a function of market conditions near the deal close, while Tables 2.8 and 2.9 tell us how targets fare as a function of the evolution in market conditions after the buyout.

Turning first to the Table 2.7 results, we find no evidence that the post-buyout performance of target firms (again, relative to controls) varies with GDP growth in the four quarters leading up to the buyout close. The β coefficients on the interaction term are imprecisely estimated and statistically insignificant for each dependent variable in columns (1) to (5). In contrast, higher credit spreads at the close are associated with large, statistically significant post-buyout increases in excess reallocation and productivity growth at target firms. These increases come on top of the baseline effects seen in the top row. The last row in Table 2.7 reports the product of the β coefficient and a unit standard deviation change in market conditions. Raising the credit spread by one standard deviation corresponds to a post-buyout gain of 21.7 log points for targets relative to controls and an

[30] Similar results obtain when using the change from the buyout closing date in year t to March of year $t+2$.

increase in excess reallocation of 4.6 percent of buyout-year employment.

Thus, the credit spread effect on target-firm productivity growth is quite large and is accompanied by a sizable increase in the pace of job reallocation inside target firms. This pattern suggests that PE buyouts foster productivity gains by catalyzing creative destruction within target firms. In unreported results, we directly examine the post-buyout relationship between productivity growth and excess job reallocation and find that targets with higher excess job reallocation enjoy higher productivity growth. These results echo one of the chief findings in *Davis et al.* (2014) despite our use of a different productivity measure, different empirical methods, and data for a much broader set of industries. Our earlier study finds that buyouts lead to TFP gains at target firms in the manufacturing sector, mainly due to the reallocation of activity from less productive plants to more productive ones. Here, we find that high credit spreads at the time of the buyout lead to greater productivity growth and greater reallocation activity in target firms in the two years after the buyout. Both sets of results link buyout-induced productivity gains to an accelerated, purposefully directed reallocation of activity within target firms.

Figure 2.2 illustrates how post-buyout productivity growth and excess reallocation at target firms vary with credit spreads at the time of buyout. Evaluated at the sample mean credit spread, Table 2.7 says that buyouts raise productivity by about 15 log points over two years at targets relative to controls. The buyout-related productivity boost is more than twice as large when the credit spread is one standard deviation about its sample mean. Post-buyout excess reallocation also rises with the credit spread at target firms relative to controls, as discussed above.

One interpretation of these patterns is that PE groups have multiple tools for earning investment returns on their portfolio firms. When credit is cheap and easy, it is more attractive to rely on financial engineering tools to generate returns, e.g., by issuing new debt to fund additional dividend payments to equity holders. When credit is costly and tight, financial engineering is less attractive and PE groups focus more on generating returns by cultivating operational improvements that raise productivity in portfolio firms. This substitution between financial engineering and operational improvements may work through the selection of buyout targets, through the way PE firms and senior managers in portfolio firms allocate their time and attention, or through a combination of the two.

Turning to Table 2.8, faster GDP growth in the two-year interval after buyouts is associated with faster post-buyout employment growth at targets relative to controls and greater excess reallocation. The effects are large, as seen in the last row: A unit standard deviation rise in the post-buyout GDP growth rate comes with a gain in relative employment growth at targets of 3.2 log points and a relative increase of 3.0 percent in excess employment reallocation. A rise in credit spreads after buyouts involves slower employment growth at targets relative to controls, slower organic growth, slower excess reallocation, higher wage growth, and lower productivity growth. These shifts are statistically significant on every margin. While the credit spread results in Table 8 are stronger than the results for GDP growth, the outcome response pattern is the same. In unreported results, we also find a broadly similar pattern when using equity market valuations to measure external conditions.

Figure 2.3 illustrates how post-buyout employment growth and excess reallocation at target firms vary relative to controls with the post-buyout evolution of external market conditions. The baseline employment growth effects depicted in the center bars are of modest size, in line with our results in Section 2.4. However, the relative post-buyout employment performance of targets is highly sensitive to the evolution of market conditions. For example, a post-buyout widening of credit spreads by two standard deviations lowers the relative employment growth of targets by 5 log points. Excess reallocation rates at target firms are also highly sensitive to the post-buyout evolution of market conditions. While not illustrated in the figure, the results in Table 2.8 also imply that post-buyout productivity growth at targets rises strongly with an improvement in economic conditions (faster GDP growth or shrinking credit spreads).

Post-buyout wage growth at targets is also sensitive to the evolution of credit conditions. According to Column (9) in Table 2.8, a unit standard deviation widening of the credit spread (440 basis points) in the two years after the buyout is associated with a relative wage gain at targets of 1.4 log points. Whether this result reflects a compositional shift in the workforce (e.g., layoffs concentrated among low-wage workers) or wage gains for employees at target firms relative to those at control firms is an open question.

As the reader will have noted, high credit spreads when the buyout closes and widening credit spreads after the buyout closes have very different relationships to the post-buyout performance of targets. As we saw in Table 2.7, deals done during periods of high credit spreads prove to have more productivity gains. But if credit spreads further increase after the buyout, the effect goes the other way, as revealed in Table 2.8. This contrast might seem anomalous. As noted above, PE groups may react to tight credit conditions by choosing transactions that are conducive to operational improvements. If credit conditions deteriorate post-buyout, however, they appear unable to switch gears to improving productivity. Rather, the deteriorating conditions seem to translate into fewer productivity gains (and more job losses). Given the pervasiveness of road-maps for future operational plans prepared by PE groups as part of the due diligence process (e.g., *Gompers et al.* (2016)), one possibility is that they get locked into a particular strategy, hampering their ability to promptly shift course later if market conditions change after the purchase. As we saw earlier, a post-buyout widening of credit spreads also brings slower pace of excess reallocation at targets relative to controls.

Given these intriguing results, Table 2.9 considers the role of post-buyout market conditions by deal type. As we saw in Table 2.6, the employment response to buyouts differs dramatically across deal types. Moreover, Figure 2.1 and Table 2.1 show that the mix of buyouts by deal type varies over the economic cycle. These remarks suggest that the sensitivity of targets to the post-buyout evolution of external market conditions may also differ by deal type.

As seen in Panel B of Table 2.9, a post-buyout widening of credit spreads brings relative employment drops at target firms in private-to-private and secondary deals. A one standard deviation rise in the spread over two years after the buyout is associated with a relative employment drop of about 5 log points. The drop involves organic employment changes in secondary deals. Lower post-buyout GDP growth is also associated with lower employment growth (except for public-to-private deals), but the effects are not statistically significant when cutting the sample by deal type.

The post-buyout evolution of market conditions shows a stronger relationship to excess reallocation rates in target firms. In five out of eight reported regressions, we see that a deterioration in external market conditions brings a significant decline in excess reallocation at targets relative to controls. In only one (statistically insignificant) case does the effect go in the opposite direction. Excess reallocation in target firms is especially sensitive to the post-buyout evolution of market conditions for public-to-private and divisional deals.

The wages and productivity results in Table 2.9 highlight the special character of public-to-private deals. In particular, in these deals a post-buyout deterioration in market conditions brings greater wage growth and slower productivity growth at target firms compared to controls. These effects are statistically significant and quite large, as seen in the last row of each panel. Given the size and heavy debt loads of target firms in public-to-private deals (*Axelson et al.*, 2013), downturns place may place great stress on their restructuring plans.

The combination of adverse market conditions and heavy indebtedness may hamper their efforts to undertake productivity-improving changes. Normally, we might anticipate that financial pressures on the firm would also translate into wage and benefit concessions by workers (e.g., *Matsa* (2010), and *Benmelech et al.* (2012)). But in this setting the dynamics may be more complex. For instance, a firm may be unable to sell units with high labor costs when external conditions are weak, and it may lack the resources to finance a shift in operations to new facilities with lower labor costs when debt burdens are too heavy.

2.6 Conclusion

In his presidential address to the American Finance Association, Zingales (*Zingales*, 2015) makes the case that we "cannot argue deductively that all finance is good [or bad]. To separate the wheat from the chaff, we need to identify the rent-seeking components of finance, i.e., those activities that while profitable from an individual point of view are not so from a societal point of view." Our study takes up that challenge for private equity buyouts, a financial enterprise that critics see as dominated by rent-seeking activities with little in the way of societal benefits.

Our results show that it is highly misleading to speak about "the" social impact of private equity. The real-side effects of buyouts vary greatly by deal type and with market conditions. The effects of public-to-private buyouts are especially sensitive to market conditions. Tighter credit market conditions when buyouts take place are associated with greater post-buyout productivity gains at target firms (relative to control firms). This result suggests a degree of substitution in the levers by which private equity groups create value for their investors. Furthermore, our evidence that the relative impact of private equity on employment is pro-cyclical, particularly for private-to-private and secondary investments, suggests a "PE multiplier effect" that may accentuate cyclical swings in economic activity.

Our paper also points to some important unanswered questions. Foremost among these are whether and how the social impact of buyouts varies among private equity groups themselves. In particular, do buyout effects vary with the experience and size of the private equity group? *Kaplan and Schoar* (2005) find that the financial performance varies across private equity firms in a manner that persists from fund to fund. This pattern suggests that real-side effects are also likely to differ across private equity firms in a persistent manner.

2.7 Tables and Figures

Table 2.1: Market Conditions and Private Equity Buyout Frequency by Deal Type, 1980-2013

We regress the natural log of the count of PE buyouts in quarter t on deal-type indicators interacted with market conditions at buyout close (top panel) and over the following two years (bottom panel), while controlling for deal type and a linear time trend. To characterize contemporaneous market conditions for buyouts that close in quarter t, we consider whether the credit spread in t is above or below its sample median value and whether real GDP growth from t-4 to t is above or below its median. Similarly, to characterize the evolution of market conditions over the next two years, we consider whether the change in the credit spread and real GDP from t to t+8 are above or below their median values. Each regression has 454 observations. *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$.

Regression of ln(buyout count) on deal type interacted with measures of market conditions, 1980-2013

	Coefficient on Market Conditions (row) interacted with Deal-Type Indicator (column)					
Market Conditions	Private to Private	Public to Private	Divisional Sales	Secondary Sale	R^2	Equality of Coefficients (p-value)
A. At Buyout Close						
High GDP Growth	0.282***	0.660***	0.412***	0.0174		
	[0.0946]	[0.161]	[0.156]	[0.144]		0.000
Wide Credit Spread	-0.207**	-0.266*	-0.181	-0.249*	0.74	
	[0.0993]	[0.147]	[0.149]	[0.150]		0.019
B. Over Next 2 Years						
High GDP Growth	0.119	0.449***	0.523***	-0.407***		
	[0.112]	[0.147]	[0.163]	[0.153]		0.000
Widening Credit Spread	0.212*	0.678***	0.325**	0.200	0.75	
	[0.112]	[0.142]	[0.148]	[0.139]		0.000

Table 2.2: Private Equity Buyouts by Industry Sector and Deal Type, 1980-2013

Each column reports the percentage breakdown of buyouts for the indicated deal type, using the Standard & Poors 2018 Global Industry Classification Standard (GICS).

		Buyout Type				
Sector	*GICS code*	*Private-to-Private*	*Public-to-Private*	*Divisional*	*Secondary*	*Total*
Energy	10	2.9	2.2	2.6	2.2	2.6%
Materials	15	8.1	5.7	9.3	8.6	8.3%
Industrials	20	28.9	19.0	23.4	28.6	26.5%
Consumer staples	25	18.6	24.6	18.8	20.7	19.6%
Consumer discretionary	30	7.4	4.6	4.0	6.2	6.0%
Health care	35	10.1	12.0	8.0	10.3	9.7%
Financials	40	3.9	4.7	4.7	2.7	3.9%
Information technology	45	11.5	15.8	17.7	12.3	13.7%
Communications services	50	7.2	7.5	8.1	7.4	7.5%
Utilities	55	0.6	1.0	2.1	0.8	1.1%
Real estate	60	0.8	3.1	1.3	0.2	1.0%
		100.0%	100.0%	100.0%	100.0%	100.0%

NB: A test of the null hypothesis that the industry distribution of buyouts is independent of deal type yields a Pearson Chi-squared statistic of 260.7 with a p-value of 0.000.

Table 2.3: Summary Statistics for Private Equity Buyouts Matched to Census Micro Data

Panel A considers all matched targets in our 1980-2013 sample period. Panel B considers buyouts in the 1980-2011 period, for which we can follow targets and their control units for two years post buyout. Panel C considers the same period (1980-2003) covered by the analysis sample in Davis et al. (2014). Two-year continuers include target firms that shut down all establishments by the second year after the buyout year.

	Number of Matched Buyouts (Target Firms)	Number of Target Establishments in the Buyout Year	Employment at Target Establishments in the Buyout Year
A. All, 1980-2013	6,000	177,000	6,890,000
Private-to-private	2,600	42,000	1,800,000
Public-to-private	600	67,000	2,130,000
Divisional Sales	1,300	25,000	1,120,000
Secondary Sales	1,300	31,000	1,280,000
Unknown Type	200	12,000	560,000
B. All, 1980-2011	5,100	164,000	6,400,000
Excluding EIN cases	4,500	144,000	5,690,000
Two-year continuers,	3,600	127,000	4,970,000
Excluding EIN cases:			
Private-to-private	1,800	32,000	1,450,000
Public-to-private	500	58,000	1,800,000
Divisional Sales	400	11,000	470,000
Secondary Sales	800	20,000	920,000
Unknown Type	100	6,000	330,000
C. All, 1980-2003	1,800	69,000	2,990,000
Excluding EIN cases	1,500	59,000	2,630,000
Two-year continuers,	1,200	49,500	2,210,000
Excluding EIN cases:			
Private-to-private	600	21,000	900,000
Public-to-private	200	16,000	690,000
Divisional Sales	200	5,000	210,000
Secondary Sales	150	3,600	180,000
Unknown Type	80	3,900	230,000

Table 2.4: Buyout Effects on Employment, Wages, and Productivity

The sample contains firms that underwent private equity buyouts from 1980 to 2011 and matching control firms in the same cells. Each row reports a single weighted least-squares regression. The top row in Panel A reports the estimated buyout effect on the employment growth rate of target firms from the buyout year to two years later. The next row isolates the buyout effect on the organic employment growth rate (i.e., excluding post-buyout acquisitions and divestitures), and the remaining rows break out particular employment adjustment margins. Panels B and C report buyout effects on firm-level wage and revenue productivity growth. All specifications include a full set of cell-level fixed effects and controls for pre-buyout growth histories, as described in the main text. See Section II in the main text for an explanation of how and why we weight observations. The regressions in panels B and C exclude firms that exit within two years after the buyout. Huber-White robust standard errors in brackets. *** p¡0.01, ** p¡0.05, * p¡0.1.

Panel A: Employment		
Dependent Variable:		
	Buyout Effect	R^2
Firm-level Growth Rate from Buyout Year t to t+2	-0.0135 [0.0217]	0.32
Firm-Level Growth Rate from t to t+2, Organic Changes Only	-0.0438** [0.0190]	0.29
By Adjustment Margin		
Continuers	-0.0153 [0.0115]	0.28
Creation	*0.0020* *[0.0041]*	*0.34*
Destruction	*-0.0173** *[0.0096]*	*0.27*
Deaths	-0.0403*** [0.0124]	0.30
Births	0.0117** [0.0051]	0.34
Acquisitions	0.0369*** [0.0097]	0.38
Divestitures	-0.0065 [0.0041]	0.26

Panel B: Wages

Dependent Variable	Buyout Effect	R^2
Change in Wages from t to t+2	-0.0028	0.41
	[0.0168]	
Wage Level in t	0.0321**	0.74
	[0.0141]	
Wage level in t+2	0.0293	0.69
	[0.0200]	

Panel C: Productivity

Dependent Variable	Buyout Effects	R^2
Change in Rev Prod from t to t+2	0.0937*	0.46
	[0.0497]	
Rev Prod in t	0.3544***	0.64
	[0.1034]	
Rev Prod in t+2	0.4158***	0.60
	[0.0865]	

Table 2.5: Buyout Effects on Firm-Level Job Reallocation and Excess Reallocation

The sample contains firms that underwent private equity buyouts from 1980 to 2011 and matching control firms in the same cells. Each row reports a single weighted least-squares regression. The dependent variable is the excess reallocation rate or job reallocation rate computed from establishment-level employment changes between the buyout year t and t+2. The key independent variable is a dummy equal to one for buyout targets. All specifications include a full set of cell-level fixed effects and controls for pre-buyout growth histories, as described in the text. See Section II in the text for an explanation of how and why we weight observations. All Margins captures the Organic Margins plus post-buyout acquisitions and divestitures. Huber-White robust standard errors in brackets. *** p≤0.01, ** p≤0.05, * p≤0.1.

Estimated Effect on Targets Relative to Controls from Buyout Year t to t+2		
Dependent Variable	Buyout Effect	R^2
Firm-Level Excess Reallocation Rate, All Margins	0.0495*** [0.0114]	0.40
Firm-Level Excess Reallocation Rate, Organic Margins (Births, Deaths & Continuers)	0.0061 [0.0154]	0.35
Firm-Level Job Reallocation Rate, All Margins	0.1147*** [0.0182]	0.39
Firm-Level Job Reallocation Rate, Organic Margins (Births, Deaths & Continuers)	0.0713*** [0.0176]	0.39

Table 2.6: How Buyout Effects Vary by Deal Type

This table follows Tables 4 and 5 except for reporting results by deal type, as indicated in the column headings. See notes to Tables 4 and 5 for additional information. *** p≤0.01, ** p≤0.05, * p≤0.1.

Panel A: Employment

Dependent Variable:	Private-to-private		Public-to-private		Divisional Sales		Secondary Sales	
	Buyout Effect	R^2	Buyout Effect	R^2	Buyout Effect	R^2	Buyout Effect	R^2
Firm-level Employment Growth Rate from Buyout Year t to t+2	0.1275***	0.37	-0.1263***	0.38	-0.1145**	0.32	0.0989***	0.32
	[0.0250]		[0.0285]		[0.0470]		[0.0254]	
Organic Growth	0.0309**	0.33	-0.0997***	0.38	-0.1599***	0.29	0.0609***	0.30
	[0.0152]		[0.0242]		[0.0424]		[0.0227]	
By Adjustment Margin								
Continuers	0.0055	0.30	-0.0159	0.33	-0.0764***	0.29	0.0263**	0.36
	[0.0104]		[0.0120]		[0.0274]		[0.0128]	
Creation	0.0027	0.36	0.0023	0.29	-0.0086	0.28	0.0210*	0.43
	[0.0057]		[0.0056]		[0.0096]		[0.0108]	
Destruction	0.0028	0.32	-0.0182*	0.32	-0.0678***	0.33	0.0053	0.29
	[0.0077]		[0.0099]		[0.0245]		[0.0102]	
Deaths	0.0003	0.34	-0.0626***	0.44	-0.0976***	0.28	-0.007	0.29
	[0.0104]		[0.0205]		[0.0200]		[0.0158]	
Births	0.0251***	0.40	-0.0213***	0.33	0.0142	0.37	0.0416***	0.42
	[0.0077]		[0.0071]		[0.0120]		[0.0122]	
Acquisitions	0.0953***	0.44	0.004	0.42	0.0332**	0.38	0.0329***	0.39
	[0.0259]		[0.0057]		[0.0154]		[0.0096]	
Divestitures	0.0027	0.20	-0.0301***	0.35	0.0102**	0.23	0.0036	0.22
	[0.0053]		[0.0104]		[0.0049]		[0.0061]	
Observations (000s)	3,900		400		2,300		600	

Panel B: Reallocation

Dependent Variable	Private-to-Private Buyout Effect	Private-to-Private R^2	Public-to-private Buyout Effect	Public-to-private R^2	Divisional Sales Buyout Effect	Divisional Sales R^2	Secondary Sales Buyout Effect	Secondary Sales R^2
Firm-Level Excess Reallocation All Margins	0.0547**	0.42	0.0171	0.39	0.0995***	0.44	0.0714***	0.45
	[0.0228]		[0.0156]		[0.0185]		[0.0235]	
Firm-Level Excess Reallocation Births, Deaths & Continuers	-0.0378	0.40	-0.017	0.36	0.0765***	0.37	0.0422	0.40
	[0.0345]		[0.0185]		[0.0226]		[0.0279]	
Firm-Level Job Reallocation All Margins	0.1174***	0.39	0.0959***	0.45	0.1940***	0.43	0.0935***	0.39
	[0.0271]		[0.0233]		[0.0448]		[0.0270]	
Firm-Level Job Reallocation Births, Deaths & Continuers	0.0248	0.44	0.0617***	0.44	0.1711***	0.41	0.0643**	0.41
	[0.0191]		[0.0203]		[0.0441]		[0.0277]	
Observations (000s)	3,900		400		2,300		600	

Panel C: Wages

Dependent Variable:	Private-to-private Buyout Effect	Private-to-private R^2	Public-to-private Buyout Effect	Public-to-private R^2	Divisional Sales Buyout Effect	Divisional Sales R^2	Secondary Sales Buyout Effect	Secondary Sales R^2
Change in Wages from t to t+2	-0.0588*	0.13	-0.0183	0.81	0.1102***	0.41	-0.0301	0.37
	[0.0335]		[0.0156]		[0.0343]		[0.0250]	
Wage Level in t	0.0387	0.77	0.0472*	0.77	0.0675**	0.74	-0.0700**	0.62
	[0.0264]		[0.0252]		[0.0276]		[0.0319]	
Wage level in t+2	-0.0201	0.56	0.0289	0.84	0.1777***	0.72	-0.1001**	0.55
	[0.0360]		[0.0235]		[0.0373]		[0.0397]	
Observations (000s)	2,100		200		1,500		300	

Panel D: Productivity

Dependent Variable	Private to Private Buyout Effect	Private to Private R^2	Public To Private Buyout Effect	Public To Private R^2	Divisional Sales Buyout Effect	Divisional Sales R^2	Secondary Sales Buyout Effect	Secondary Sales R^2
Change in Productivity from t to t+2	0.1440***	0.436	0.2674	0.5046	-0.0429	0.3762	0.0296	0.425
	[0.0431]		[0.1691]		[0.0584]		[0.0596]	

Table 2.7: How Buyout Effects Vary with Market Conditions Near the Closing Date

The sample contains firms that underwent private equity buyouts from 1980 to 2011 and matching control firms in the same cells. Each column reports a single weighted least-squares regression. Moving from left to right across columns, the dependent variable is the firm-level employment growth rate from buyout year t to t+2, the organic part of the firm-level employment growth rate over the same interval, the excess job reallocation rate from t to t+2, the log change in the wage (annual earnings per worker) from t to t+2, and the log change in revenue productivity from t to t+2. The key independent variables are a dummy for whether the observation is a buyout target, a measure of market conditions when the buyout closed, and the interaction between the two. Columns (1)-(5) use real GDP growth to measure market conditions, and columns (6)-10) use the credit spread). We adjust the measures of market conditions to mean zero in the regression sample. All specifications include a full set of cell-level fixed effects and controls for pre-buyout growth histories. See Section II in the text for an explanation of how and why we weight observations. The final row presents the estimated effect of a unit standard deviation positive shock to market conditions on the dependent variable for buyout targets relative to control units. Huber-White robust standard errors in brackets. *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$

	Market Conditions Measure: Real GDP Growth Rate over The Four-Quarter Period Ending When the Buyout Closed					Market Conditions Measure: Credit Spread In the Month the Buyout Deal Closed				
	(1) Emp. Growth	(2) Organic Growth	(3) Excess Reall.	(4) Wage Growth	(5) Prod. Growth	(6) Emp. Growth	(7) Organic Growth	(8) Excess Reall.	(9) Wage Growth	(10) Prod. Growth
Buyout	-0.0136	-0.0435**	0.0491***	-0.0047	0.0946**	-0.0117	-0.0435**	0.0545***	-0.0028	0.1656**
	[0.0210]	[0.0178]	[0.0115]	[0.0159]	[0.0449]	[0.0221]	[0.0195]	[0.0115]	[0.0162]	[0.0708]
MktConditions	-6.4790	-9.642	-1.141	10.9300	-24.70*	0.0995	0.0995	-0.0131	0.0056	-0.6737**
	[9.137]	[8.889]	[4.161]	[9.940]	[14.67]	[0.0611]	[0.0618]	[0.0346]	[0.0713]	[0.2848]
Buyout * MktConditions	-0.2394	0.1441	-0.6586	-0.6516	-3.67	0.0028	-0.0012	0.0132***	0.0066	0.0627**
	[1.279]	[1.081]	[0.6881]	[0.7838]	[4.905]	[0.0077]	[0.0062]	[0.0045]	[0.0062]	[0.0301]
Observations (000s)	6,400	6,400	6,400	3,700	910	6,400	6,400	6,400	3,700	910
R-squared	0.3177	0.2959	0.401	0.4113	0.4622	0.3184	0.2957	0.4043	0.41	0.4753
MktConditions Mean	0.0258	0.0258	0.0258	0.0278	0.028	5.158	5.158	5.158	4.752	5.54
MktConditions St. Dev.	0.0175	0.0175	0.0175	0.0165	0.019	3.494	3.494	3.494	3.057	3.459
Effect of 1 St. Dev. Shock	-0.0042	0.0025	-0.0115	-0.0108	-0.0697	0.0098	-0.0042	0.0461	0.0202	0.2169

Table 2.8: How Buyout Effects Vary with Changes in Market Conditions in the Two Years after Closing

The sample contains firms that underwent private equity buyouts from 1980 to 2011 and matching control firms in the same cells. Each column reports a single weighted least-squares regression. Moving from left to right across columns, the dependent variable is the firm-level employment growth rate from buyout year t to t+2, the organic part of the firm-level employment growth rate over the same interval, the excess job reallocation rate from t to t+2, the log change in the wage (annual earnings per worker) from t to t+2, and the log change in revenue productivity from t to t+2. The key independent variables are a dummy for whether the observation is a buyout target and an interaction between the buyout dummy and a measure of the change in macroeconomic conditions from the buyout year t to t+2. after the transaction closing date. Regressions in the first (second) five columns use the GDP growth rate (credit spread) to measure macroeconomic conditions. We adjust the market conditions measures to mean zero in the regression sample. All specifications include a full set of cell-level fixed effects and controls for pre-buyout growth histories. See Section II in the text for an explanation of how and why we weight observations. The final row presents the estimated effect of a one standard deviation positive shock to the macroeconomic variable on the dependent variable for buyout targets relative to control units. Huber-White robust standard errors in brackets; *** p≤0.01, ** p≤0.05, * p≤0.

	Market Conditions Measure: Annualized Real GDP Growth Rate from the Buyout Year to Two Years later					Market Conditions Measure: Change in the Credit Spread from the Buyout Year to Two Years Later				
	(1) Emp. Growth	(2) Organic Growth	(3) Excess Reall.	(4) Wage Growth	(5) Prod. Growth	(6) Emp. Growth	(7) Organic Growth	(8) Excess Reall.	(9) Wage Growth	(10) Prod. Growth
Buyout	-0.0040	-0.0405**	0.0583***	-0.0052	0.1248**	-0.0056	-0.0397**	0.0584***	-0.0061	0.1468**
	[0.0242]	[0.0203]	[0.0110]	[0.0168]	[0.0559]	[0.0233]	[0.0202]	[0.0121]	[0.0176]	[0.0616]
Buyout * MktConditions	0.9560*	0.3382	0.8823***	-0.2411	1.818	-0.0057*	-0.003	-0.0064***	0.0033*	-0.0175*
	[0.5410]	[0.3993]	[0.2806]	[0.4057]	[1.129]	[0.0030]	[0.0026]	[0.0018]	[0.0020]	[0.0089]
Observations (000s)	6,400	6,400	6,400	3,700	910	6,400	6,400	6,400	3,700	910
R-squared	0.3189	0.2942	0.4045	0.4096	0.4613	0.3187	0.2946	0.4058	0.4101	0.4675
MktConditions Mean	0.0463	0.0463	0.0463	0.0466	0.0533	0.3800	0.38	0.38	0.7460	-0.181
MktConditions St. Dev.	0.0337	0.0337	0.0337	0.0343	0.0363	4.9130	4.913	4.913	4.3710	4.257
Impact of Unit St. Dev. Positive Shock	0.0322	0.0114	0.0297	-0.0083	0.0660	-0.0280	-0.0147	-0.0314	0.0144	-0.0745

Table 2.9: How Buyout Effects by Deal Type Vary with Changes in Market Conditions in the Two Years after Closing

The sample contains firms that underwent private equity buyouts from 1980 to 2011 and matching control firms in the same cells. Each column reports a single weighted least-squares regression for the indicated dependent variable, deal type and post-buyout change in market conditions. See notes to Table 8 for additional information. *** p≤0.01, ** p≤0.05, * p≤0.1.

Panel A: Change in Real GDP

	Employment Growth Rate from Buyout Year t to t+2				Organic Employment Growth Rate from t to t+2			
	Private to Private	Public to Private	Divisional Sales	Secondary Sales	Private to Private	Public to Private	Divisional Sales	Secondary Sales
Buyout	0.1284***	-0.1276***	-0.0951*	0.0901***	0.0270**	-0.1007***	-0.1473***	0.0519**
	[0.0259]	[0.0390]	[0.0533]	[0.0240]	[0.0137]	[0.0285]	[0.0465]	[0.0212]
Buyout * MktConditions	0.2795	-0.0460	1.8230	0.8187	-1.211***	-0.035	1.1760	0.8389
	[0.6737]	[0.7233]	[1.139]	[0.6354]	[0.3393]	[0.5330]	[0.8442]	[0.5191]
Observations (000s)	3,900	400	2,300	600	3,900	400	2,300	600
R-squared	0.3737	0.3828	0.3262	0.3254	0.3369	0.3885	0.3034	0.3109
MktConditions Mean	0.0446	0.0537	0.0526	0.0245	0.0446	0.0537	0.0526	0.0245
MktConditions St. Dev.	0.0344	0.0275	0.0348	0.0380	0.0344	0.0275	0.0348	0.038
Impact of 1 SD Shock	0.0096	-0.0013	0.0634	0.0311	-0.0417	-0.0010	0.0618	0.0206

	Excess Reallocation Rate from t to t+2				Wage Growth Rate from t to t+2			
	Private to Private	Public to Private	Divisional Sales	Secondary Sales	Private to Private	Public to Private	Divisional Sales	Secondary Sales
						-		
Buyout	0.0529**	0.0463***	0.1173***	0.0650***	-0.0583*	0.0511***	0.1060***	-0.0325
	[0.0222]	[0.0179]	[0.0194]	[0.0238]	[0.0326]	[0.0195]	[0.0362]	[0.0234]
Buyout * MktConditions	-0.557	1.0282***	1.6722**	0.5963	0.5433	-1.418***	-0.4162	0.7131
	[0.3959]	[0.3457]	[0.7420]	[0.4568]	[1.257]	[0.5263]	[0.5169]	[0.7191]
Observations (000s)	3,900	400	2,300	600	2,100	200	1,500	300
R-squared	0.4255	0.3946	0.4493	0.4485	0.1286	0.8147	0.4087	0.3662
MktConditions Mean	0.0446	0.0537	0.0526	0.0245	0.0423	0.0482	0.0522	0.0320
MktConditions St. Dev.	0.0344	0.0275	0.0348	0.038	0.0374	0.0295	0.0354	0.0332
Impact of 1 SD Shock	-0.0192	0.0283	0.0582	0.0227	0.0203	-0.0418	-0.0147	0.0237

	Productivity Change from t to t+2			
	Private to Private	Public to Private	Divisional Sales	Secondary Sales
Buyout	0.1588***	0.4980**	-0.0079	0.0245
	[0.0459]	[0.2153]	[0.0713]	[0.0606]
Buyout * MktConditions	-2.127	8.753**	1.572	-1.284
	[1.308]	[3.778]	[1.573]	[1.118]
Observations (000s)	410	20	620	40
R-squared	0.4446	0.5498	0.3804	0.4275
MktConditions Mean	0.0406	0.0489	0.0584	0.0367
MktConditions St. Dev.	0.0456	0.0338	0.0373	0.0332
Impact of One SD Shock	-0.0970	0.2959	0.0586	-0.0426

Panel B: Change in Credit Spread

	Employment Growth Rate from Buyout Year t to t+2				Organic Employment Growth Rate from t to t+2			
	Private to Private	Public to Private	Divisional Sales	Secondary Sales	Private to Private	Public to Private	Divisional Sales	Secondary Sales
Buyout	0.1362***	-0.1102***	-0.1210***	0.0788***	0.0288*	-0.0868***	-0.1637***	0.0422**
	[0.0250]	[0.0304]	[0.0466]	[0.0246]	[0.0156]	[0.0248]	[0.0429]	[0.0211]
Buyout * MktConditions	-0.0104**	-0.0064	0.0062	-0.0075**	0.0025	-0.0051	0.0036	-0.0070***
	[0.0048]	[0.0039]	[0.0066]	[0.0030]	[0.0025]	[0.0034]	[0.0056]	[0.0025]
Observations (000s)	3,900	400	2,300	600	3,900	400	2,300	600
R-squared	0.3773	0.3857	0.3221	0.3271	0.3326	0.3913	0.3004	0.3126
MktConditions Mean	0.1860	0.7200	-0.0925	4.4830	0.186	0.72	-0.0925	4.483
MktConditions St. Dev.	4.7130	4.1240	3.3960	7.4070	4.713	4.124	3.396	7.407
Impact of One SD Shock	-0.0490	-0.0264	0.0211	-0.0556	0.0118	-0.0210	0.0122	-0.0518

	Excess Reallocation Rate from t to t+2				Wage Growth Rate from t to t+2			
	Private to Private	Public to Private	Divisional Sales	Secondary Sales	Private to Private	Public to Private	Divisional Sales	Secondary Sales
Buyout	0.0563**	0.0294*	0.1115***	0.0563**	-0.0585*	- 0.0459***	0.1134***	-0.0295
	[0.0232]	[0.0173]	[0.0190]	[0.0221]	[0.0333]	[0.0144]	[0.0338]	[0.0249]
Buyout * MktConditions	-0.0019	-0.0049*	-0.0114**	-0.0057*	0.0019	0.0113***	-0.0041	0.0026
	[0.0022]	[0.0025]	[0.0046]	[0.0029]	[0.0022]	[0.0023]	[0.0033]	[0.0024]
Observations (000s)	3,900	400	2,300	600	2,100	200	1,500	300
R-squared	0.4242	0.3922	0.4524	0.4502	0.1282	0.8190	0.4098	0.3657
MktConditions Mean	0.186	0.72	-0.0925	4.483	1.1500	0.8250	0.1830	2.1130
MktConditions St. Dev.	4.713	4.124	3.396	7.407	4.5940	3.8590	3.3930	5.9950
Impact of One SD Shock	-0.0090	-0.0202	-0.0387	-0.0422	0.0087	0.0436	-0.0139	0.0156

	Productivity Growth Rate from t to t+2			
	Private to Private	Public to Private	Divisional Sales	Secondary Sales
Buyout	0.1018***	0.5028***	-0.013	0.012
	[0.0319]	[0.1872]	[0.0635]	[0.0616]
Buyout * MktConditions	0.0163	-0.0711***	-0.0087	0.0190***
	[0.0103]	[0.0270]	[0.0083]	[0.0051]
Observations (000s)	410	20	620	40
R-squared	0.4463	0.5884	0.3802	0.4413
MktConditions Mean	0.248	0.258	0.11	1.603
MktConditions St. Dev.	5.452	5.202	2.537	4.833
Impact of One SD Shock	0.0889	-0.3699	-0.0221	0.0918

Figure 2.1: Quarterly Buyout Counts by Deal Type, 1980 to 2013.

Each panel shows buyout closings for the indicated deal type in quarter t, overlaid with the contemporaneous credit spread and the log change in real GDP from t-4 to t. We exclude about 300 buyouts that we cannot classify as to deal type. See Section 1.A for an explanation of how we construct of our sample of 9,794 leveraged buyouts sponsored by private equity firms.

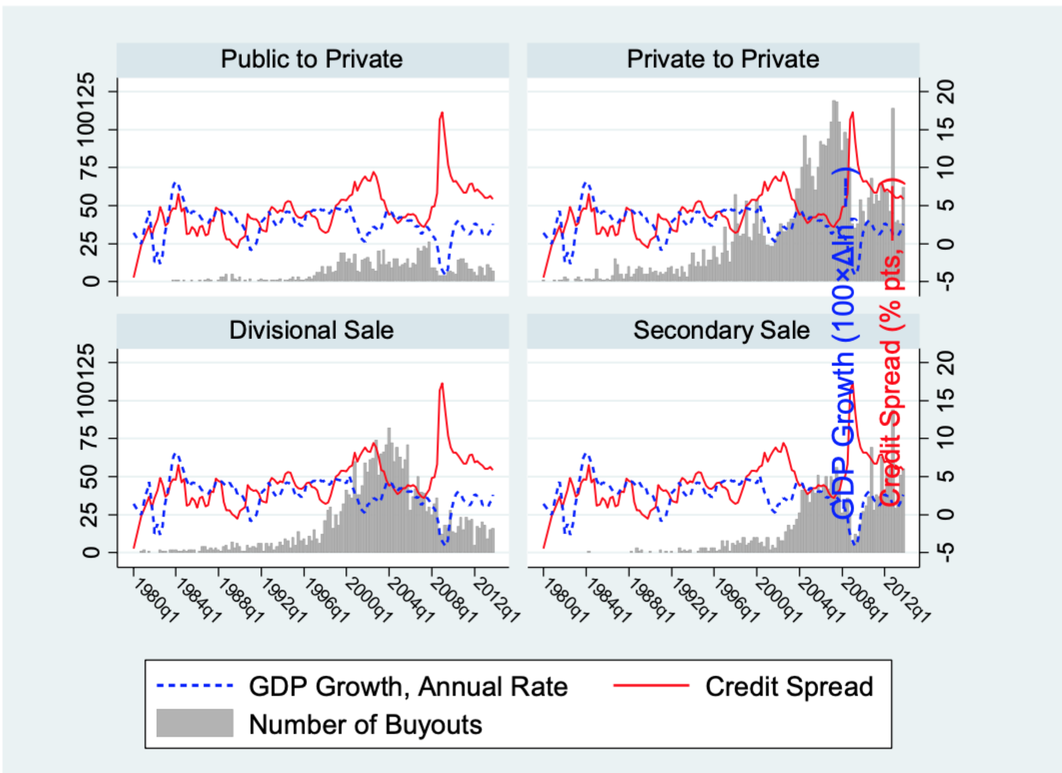


Figure 2.2: How the Post-Buyout Rates of Productivity Growth and Excess Reallocation of Targets Vary with the Credit Spread on the Buyout Date

This chart uses the estimated coefficient on the credit spread interaction effects in Table 7 to depict how target outcomes over the two years after the buyout vary with credit spreads at the time of the buyout. Center bars show baseline effects on rates of productivity growth and excess reallocation when the credit spread equals its sample mean. The other bars show buyout effects on targets for credit spread values -2, -1, 1 and 2 standard deviations about the sample mean.

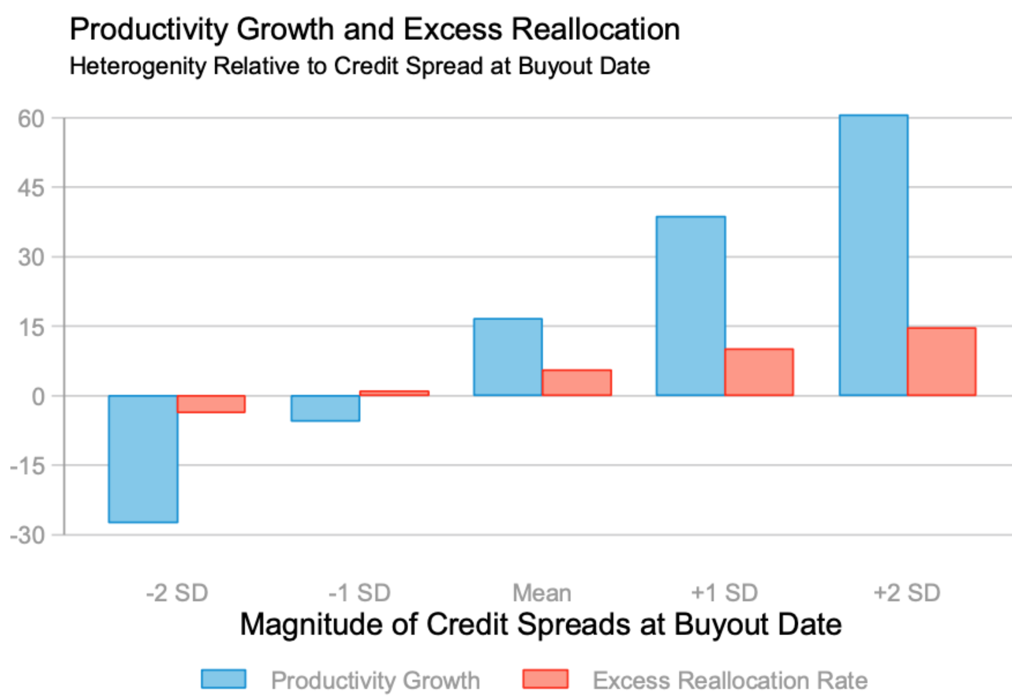
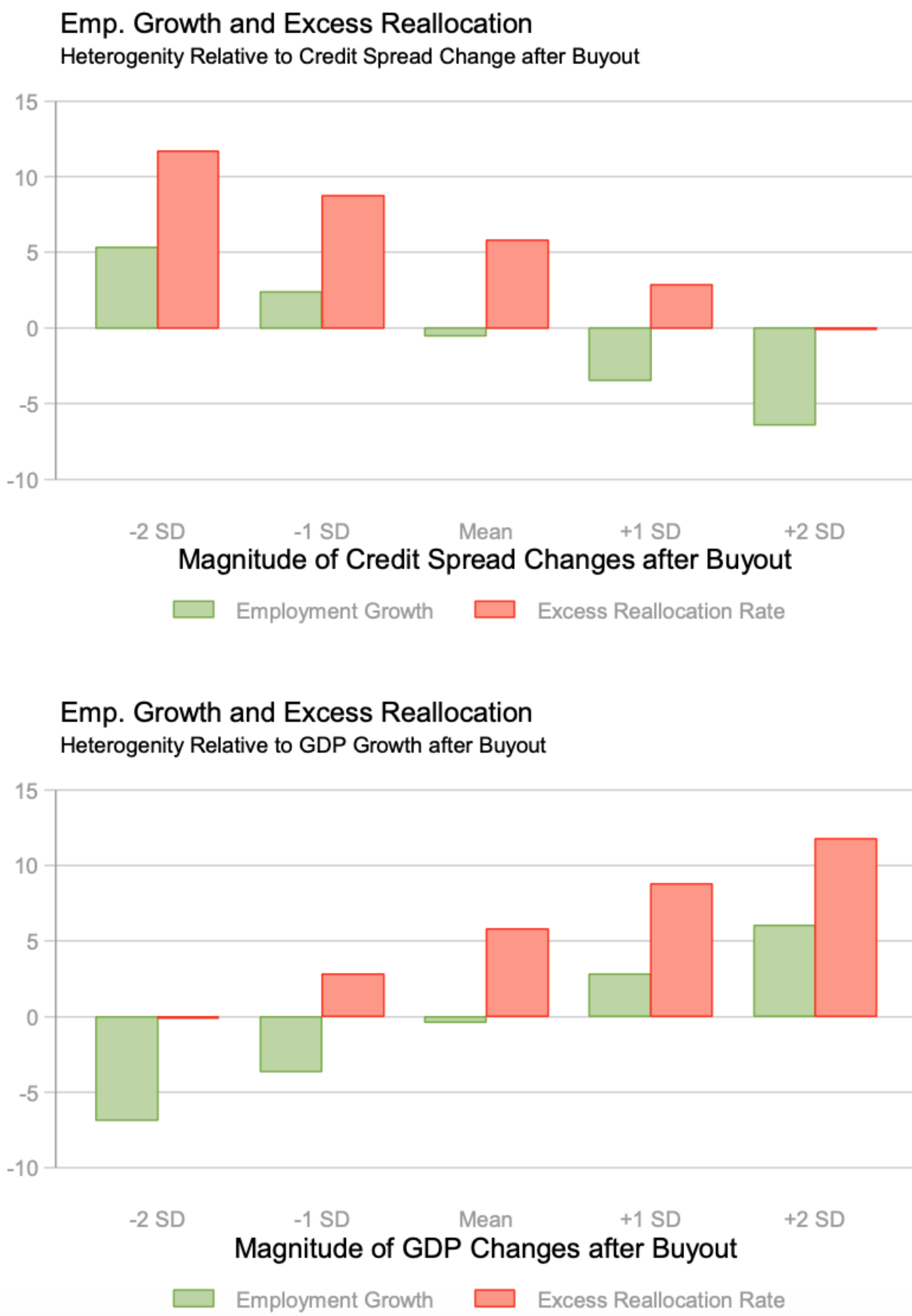


Figure 2.3: How the Post-Buyout Rates of Employment Growth and Excess Reallocation of Targets Vary with the Post-Buyout Evolution of Market Condition

The top panel shows the impact of changes in credit spreads on outcomes. The bottom panel shows the impact of GDP changes. These results are based on Table 8. The bars graphing the effects are centered on the Average Treatment Effect at the mean 2 standard deviations.



CHAPTER III

U.S. Industrial Composition and the Labor Share

3.1 Introduction

The falling labor share is a well documented phenomenon by now (*Elsby et al.*, 2013; *Karabarbounis and Neiman*, 2013). While, researchers have been trying to uncover the economic phenomena underlying this decline, they have yet to find many convincing answers. One possible explanation for the falling labor is the evolving make-up of the U.S. economy; the shift of output from manufacturing to services could reallocate output from high labor share to low labor share sectors of the economy. This paper examines this explanation for the falling labor share.

To do so, this paper first examines some of the assumptions that go into creating the Penn World Table's measure of U.S. labor share.[1] The assumptions underlying the PWT's treatment of the self-employed is an important component of the labor share calculation and inflates the labor share above just compensation paid to employees. The paper then decomposes changes in the labor share two ways - first into changes because of the shifting

[1]the PWT's labor share calculation is one of the most frequently cited.

sectoral composition of the U.S. economy and then into changes due to changing compensation ratios, due to changing self-employed output and changes due to output allocation.

Using BEA data on sectoral output and compensation to examine the composition of the U.S. economy shows that the falling labor share is not attributable to the U.S. economy's shift from a manufacturing to a services economy. This finding should assuage fears that the U.S.'s shift towards services is harming workers (in terms of aggregate labor share); there are, of course, other possible problems with this economic shift. A counterfactual shows that the biggest part of the labor share's decline due to manufacturing is its within-sector labor share as opposed to economic shifts away from manufacturing. After examining the shifting composition of the economy, the paper goes on the examining the other components of the changing labor share.

Over the period of study, the labor share's fall is due entirely to within-industry changes in the compensation ratio. Both self-employed output and the shifting composition of the economy act to bolster the labor share. This finding suggests that any theory on the falling labor share must first look within industries and second explain why wages are failing to grow at the same rate as nominal output, e.g. there must be some explanation for the wages' failure to adjust for higher productivity, in the case of increased technology, or otherwise introduce some sort of labor market friction that prevents wages' adjustment.

The rest of the paper is as follows: Section 3.2 discusses related work; Section 3.3 describes the construction of the various decompositions in the paper; Section 3.4 presents results and analysis; and Section 3.5 concludes.

3.2 Relationship to the Literature

This paper shows that the labor's decline is entirely due to changes in within-industry compensation and that the reallocation of the U.S. economy towards services has actually dampened the labor share's fall. Additionally, because of the BEA data used it covers the entire economy. *Karabarbounis and Neiman* (2013)'s study, among the first to spot the falling global labor share, focuses exclusively on the corporate sector to avoid the issues of measuring the labor shares of the self employed and of home produced output. Using KLEMS data and a regression decomposition, they show that the labor share's fall is largely within the industries they examine but they do not have full industrial coverage nor do they show output reallocation's positive effect on the labor share. They further argue that cheaper investment prices have shifted production towards more capital intensive modes of production and this shift has hurt the labor share. *Bentolila and Saint-Paul* (2003) argue similarly.

Elsby et al. (2013) state that approximately a third of the decline in the labor share comes from mismeasured self-employed output. They further argue that the stability of the labor share in the past masked within-industry heterogeneity and that workers' bargaining power does not drive the falling labor share. These results are broadly in line with the results of this study. My study also shows heterogeneity across industries and demonstrates its importance for the overall analisys of the labor share. I go farther and demonstrate how the within-industry heterogeneity in labor share directly affects the aggregate.

Some recent papers have argued that industrial concentration explains this decreased worker bargaining power. *Autor et al.* (2017) were the first to show that industrial output has been getting more concentrated and present a model in which this concentration leads to lower aggregate fixed costs - and a lower labor share. However, they do not address bargaining power directly and examine industrial concentration at a nation, as opposed to local, level. *Rinz* (2018) and *Lipsius* (2018) show that when measured locally, concentration has been going down. Whether or not nationally calculated industrial concentrations are related to employee bargaining power is an open question. If bargaining power really is a driver of the falling labor share, then economists must offer and analyze an explanation outside of labor-market concentration, such as de-unionization, or explain why local concentration is a less good indicator of labor market power than national concentration.

Most closely related to this paper is *Rodriguez and Jayadev* (2010). While they say they show that falls in the labor share are mainly driven by within-industry changes, their analysis is focused primarily on manufacturing. This result misses the dynamics in other industries such as services. Manufacturing itself is an ever-shrinking part of the U.S., and global, economies and therefore dynamics there do not necessarily reflect broader economic phenomena. This study uses all the sectors of the U.S. economy to bypass these worries.

3.3 Data and Concepts

The main source of data for this analysis is the BEA's GDP-by-Industry components data series. These data contain the total value added produced by U.S. industries and then divides them into three sub-categories: total labor compensation, taxes and subsidies, and total surplus. These categories are exhaustive and mutually exclusive so that within each industry, i, and for each year, t,

$$VA_{it} = C_{it} + S_{it} + T_{it}$$

where VA_{it} is total value added, C_{it} is labor compensation, S_{it} is surplus and T_{it} is taxes. These data are available back to 1987. There is a break in the data in 1997. The post-

'97 data were harmonized with the 2012 NAICS (North American Industrial Classification System) industrial reclassification while the pre-'97 data have not been. The main results for this paper use these inconsistent data series but all regressions include a pre-'97 fixed effect to absorbed this difference.

This paper divides the economy into eight sectors: manufacturing; services; agriculture, forestry, fishing, hunting, and mining (rural industries); transport, warehousing, utilities, and construction (TWUC); wholesale trade; retail trade; finance, insurance, and real estate (FIRE); and government, which includes local, state and federal expenditures. These sectors are meant to reflect the sectors in **?**.[2] The BEA data, however, provide more granular industrial data for some of these sectors. Table 3.1 shows how the industries within the BEA data are aggregated into the appropriate sectors. Within each sector, the industries' components of value added are summed to create the sector's value added and its subsequent components. The output-weighted average of each sector's compensation ratio, $\frac{C_{it}}{VA_{it}}$ is the economy-wide compensation ratio:

$$\frac{C_t}{VA_t} = \sum_{i=1}^{N} \frac{VA_{it}}{VA_t} \frac{C_{it}}{VA_{it}}. \tag{3.1}$$

These compensation ratios, however, are not labor shares. Compensation ratios need to be adjusted for what *Feenstra et al.* (2015) call "mixed income." As they explain "Mixed income is the total income earned by self-employed workers, so it is a combination of capital and labor income." Mixed income data, however, do not exist for individual industries. Instead, this paper calculates a national-level adjustment that can be applied to the BEA compensation ratio data and, mathematically, applies to each industry individually.

[2] **?** uses manufacturing, services, retail trade, wholesale trade, utilities and transportation, and finance.

The first step in calculating this compensation ratio adjustment factor is to examine compensation ratios from both the BEA data and from the Penn World Table (PWT). Figure 3.1a shows the compensation shares for the full economy calculated from the BEA data and from the PWT. The bottom dashed line shows the time series of BEA full economy compensation ratio. *Feenstra et al.* (2015) explains that when calculating the labor share for the PWT, "net taxes on products are excluded since this is not income accruing to any of the factor inputs but a direct transfer to the government." The top solid line shows the BEA compensation ratio adjusted for taxes. In keeping with the PWT, the value added is adjusted by subtracting taxes from it. After the adjustment, the compensation ratio becomes

$$\frac{C_t}{VA_t - T_t}.$$

This adjustment then increases the percentage of output that goes to compensating labor, explaining the increase in the compensation ratio. The final, dotted line lying between the two BEA data series is the PWT compensation ratio. While this series does not line up directly with the adjusted BEA compensation ratio, its correlation with the series is .999. Figure 3.1b shows the total compensation share and the adjusted compensation share calculated only for private industries (excluding government output and employment), and the PWT compensation share. The private industry compensation share lies below the PWT compensation share both before and after the tax adjustment. Because the PWT series lies between the full economy compensation series and strictly above the private industry, compensation series, this study will use the full economy, tax adjusted compensation ratio to create the labor share series.

From the compensation ratios, there are several possible adjustments for mixed income and the particular adjustment appropriate to an economy depends heavily on the particular features of that economy. The PWT chooses to assume that the labor share for mixed income is the same as the labor share for compensation income. This assumption leads it to adjust the economy-wide value of GDP by subtracting all of the output generated from the self employed. The labor share, then, is defined as

$$\alpha = \frac{Comp_t}{GDP_t - M_t},$$

where α is the labor share, $Comp_t$ is the total compensation to labor in time t, GDP_t is the tax-adjusted total output in t and M_t is total mixed income in t. Neither the BEA data nor the PWT give information about the amount of output generated from the self-employed. However, because it provides both the compensation share $\frac{Comp_t}{GDP_t}$ and the labor share, the PWT provides enough information to back out the economy-wide percentage of output generated by the self employed. The difference between the labor share and the compensation share is equal to the compensation share times the ratio of self-employed output to output produced by compensated employees:

$$\frac{Comp_t}{GDP_t - M_t} - \frac{Comp_t}{GDP_t} = \frac{Comp_t}{GDP_t}\frac{M_t}{GDP_t - M_t}. \tag{3.2}$$

Writing M_t as a percentage of output, $M_t = \gamma_t GDP_t$ or $\gamma_t = \frac{M_t}{GDP_t}$, simplifies equation 3.2 to

$$\frac{Comp_t}{GDP_t - M_t} - \frac{Comp_t}{GDP_t} = \frac{Comp_t}{GDP_t}\frac{\gamma_t}{1 - \gamma_t}. \tag{3.3}$$

Adding the compensation share back into the equation, rearranging terms and solving for

γ_t yields:

$$\frac{Comp_t}{GDP_t - M_t} = \frac{Comp_t}{GDP_t}\frac{1}{1-\gamma_t} \tag{3.4}$$

$$\gamma_t = 1 - \left(\frac{Comp_t}{GDP_t - M_t}\right)^{-1}\frac{Comp_t}{GDP_t} \tag{3.5}$$

γ_t, the percentage of total output generated by the self employed is 1 minus the ratio of the compensation share to the labor share. Figure 3.2 shows how γ_t, calculated from the PWT data, has evolved over the period of study. While the percentage of output generated by the self employed is always between 6% and 8%, it never drops below 7% after the mid '90s. $\frac{1}{1-\gamma_t}$ is the ratio of the total economy to the percentage of output not generated by the self-employed. This value will act as the inflator transforming the compensation ratios into labor shares. The proper adjustment is implied by equation 3.4. This adjustment factor is increasing in γ_t. Applying it to the BEA compensation ratio creates the implied BEA labor share. Figure 3.3 graphs the implied BEA labor share and the PWT labor share. The gap between the two series represents two things, the initial gap between the compensation ratios presented in Figure 3.1a and the slight change in correlation between the two series. While the BEA adjusted-compensation ratio has a .999 correlation with the PWT compensation ratio, the BEA implied labor share has a .954 correlation with the PWT U.S. labor share. Both series show a sharp drop in the labor from 2000 to 2014: the BEA implied labor share drops from .66 to .62 while the PWT labor share drops from .64 to about .60. Additionally, both series are lower in 2014 than they are in 1987, though the difference is smaller than the difference from the highs.

The BEA implied labor share is, then, the product of the adjustment factor in equation 3.4 and the data structure from the BEA data presented in equation 3.1

$$LS_t^{BEA} = \delta_t \frac{C_t}{VA_t}$$

where $\delta_t = \frac{1}{1-\gamma_t}$. Constructing the labor share this way implies that changes in the labor share are driven by changes in the economy's compensation ratio and the amount of total output that is produced by the self-employed. These changes in the labor share decompose exactly into

$$\begin{aligned} \Delta LS_t^{BEA} &= LS_t^{BEA} - LS_{t-1}^{BEA} \\ &= \delta_t \left(\Delta \frac{C_t}{VA_t} \right) + \frac{C_{t-1}}{VA_{t-1}} \Delta \delta_t. \end{aligned} \tag{3.6}$$

The terms in equation 3.6 are the change in the labor share due to the change in the underlying compensation ratio weighted by the adjustment factor and due to the change in the adjustment factor weighted by initial compensation ratio. This decomposition says that as either the percentage of output generated by the self-employed increases ($\delta_t > \delta_{t-1}$) or as the compensation ratio increases, so does the labor share.

The changes in the compensation ratio can be further decomposed into the changes due to within-sector changes in the compensation ratio and the changes due to between-sector reallocation of output. Because the compensation ratio is the output-weighted average of the compensation ratios of the different sectors of the economy,

$$\frac{C_t}{VA_t} = \sum_{i=1}^{N} \frac{VA_{it}}{VA_t} \frac{C_{it}}{VA_{it}},$$

the Olley-Pakes decomposition as described by *Melitz and Polanec* (2015) yields

$$\frac{C_t}{VA_t} = \frac{\bar{C}_t}{VA_t} + \sum_{i=1}^{N} \left(\frac{C_{it}}{VA_{it}} - \frac{C_t}{VA_t} \right) \left(\frac{VA_{it}}{VA_t} - \frac{V\bar{A}_{it}}{VA_t} \right).$$

This definition of the aggregate compensation ratio translates changes in the compensation ratio into

$$\Delta\frac{C_t}{VA_t} = \Delta\frac{\bar{C}_t}{VA_t} + \Delta\sum_{i=1}^{N} \left(\frac{C_{it}}{VA_{it}} - \frac{C_t}{VA_t} \right) \left(\frac{VA_{it}}{VA_t} - \frac{V\bar{A}_{it}}{VA_t} \right). \tag{3.7}$$

The first term of this decomposition is the change in the sectors' average level of the compensation ratio. It measures how within-sector changes in the compensation share affect the aggregate compensation ratio. *Melitz and Polanec* call the second term the covariance between output share and compensation ratio. Calling this the covariance is a slight abuse of notation but *Melitz and Polanec* point out"since $\left[\frac{VA_{it}}{VA_t}\right]$ are market shares, they essentially already incorporate the division by the number of [sectors]." The covariance term measures how the aggregate composition ratio changes because of between-sector reallocation of output. Shifting output from low compensation ratio sectors to high compensation ratio sectors will increase the aggregate compensation ratio while shifting output away from high compensation ratio sectors towards lower compensation ratio sectors will lower the aggregate compensation ratio. Section 3.4 will examine how compensation ratio and output shares have evolved over the time of the study and examine how much each of these components have contributed to the overall change in the labor share.

Combining equations 3.6 and 3.7, the change in the labor share is composed of three terms, a term driven by changes in average compensation ratio, a term driven by changes in the allocation of output, and finally, a term driven by changes in the amount produced by the self-employed:

$$\Delta LS_t^{BEA} = \delta_t \Delta \frac{\bar{C}_t}{VA_t} + \delta_t \Delta \sum_{i=1}^{N} \left(\frac{C_{it}}{VA_{it}} - \frac{C_t}{VA_t} \right) \left(\frac{VA_{it}}{VA_t} - \frac{\bar{VA}_{it}}{VA_t} \right) + \frac{C_{t-1}}{VA_{t-1}} \Delta \delta_t. \quad (3.8)$$

While the changing labor share can be decomposed into the three components just discussed. It can also be decomposed into industry changes. The aggregate labor share is the output weighted average of the labor shares within each industry:

$$\frac{\text{output}_i}{\sum_{i=1}^{8} \text{output}_i} \cdot \text{Labor Share}_i \quad (3.9)$$

for each industry i. Here, the two components of change are an industry's weight within the economy and the industry's labor share itself. Section 3.4.1 will examine how the changing composition of the U.S. economy contributes to the falling labor share. A further examination of how macroeconomic conditions affect particular industries and their labor shares is beyond the scope of this paper but is an interesting topic.

3.4 Results

3.4.1 The Changing Economy

Figure 3.4 shows the within-industry changes in the compensation ratio and labor share. By construction, the labor share is always above the compensation share. Other than Services (3.4a) and Government (3.4h), all industries experience falls in the compensation ratio. The most significant falls come in Manufacturing and Wholesale Trade. Figures 3.4c and 3.4d show these time series. Manufacturing's compensation ratio falls from about .65 to about .50 over the analysis period. Wholesale Trade's falls from about .71 to about .55. In both of these industries, employees' compensation, as a percentage of total output, fell by

15+ percentage points. Other notable features of the various time series are the precipitous drop in FIRE compensation share that coincides with the 2009 financial crisis (3.4f) and the fall in Rural Industries' compensation share seems to coincide with the tech-bubble in the early 2000s but Rural Industries' compensation share has recovered slightly since the financial crisis. Both Services and Government have had slight increases in compensation ratio over the time examined.

Figure 3.5 shows the percent of the economy each industry comprises. Here we can see how the U.S. economy has changed significantly over the last 30 years. In 1987, Manufacturing output made up almost 20% of total U.S. output but by 2014 that was down to about 12% (3.5c). Over the same period, the share of output comprised of Services rose from about 24% to about 32% (3.5a). This 8 percentage point increase in Services almost exactly equals Manufacturing's decline. FIRE gains a little under 3% of output-share (3.4f). Its output share grows more quickly than Services' and peaks in the early 2000s. At the time of the dot-com bubble (∼2000) and the time of the financial crisis (∼2009) there are big drops in FIRE's output share which only recover by 2014. Wholesale Trade (3.5d) gains less than 1 percentage point of output while Retail; Utilities, Construction, and Transportation; and Government all lose about less than 2 percentage points of output. Interestingly, Rural industries are almost flat. After a large decrease in the ́90s, these industriesśhare of output recovers completely and by the end of the sample period exceeds its starting value, albeit by less than half a percentage point.

Figure 3.6 shows how the results in Figures 3.4 & 3.5 interact. It shows the time series of each industries contribution to the labor share i.e. Figure 3.6 plots

$$\frac{\text{output}_i}{\sum_{i=1}^{8} \text{output}_i} \cdot \text{Labor Share}_i \tag{3.10}$$

for each industry i. Panel 3.6c shows that Manufacturing is contributing almost 7 percentage points less to labor share in 2014 than it did 1987. However, this fall is counter balanced by a nearly identical rise in Services' overall labor share contribution (Panel 3.6a). Outside of Services, however, only FIRE (Panel 3.6f) has a higher contribution to labor share in 2014 than in 1987. All other sectors (other than Rural Industries), act as a drag on the aggregate labor share. (Panels 3.6d, 3.6b, 3.6e, 3.6h,& 3.6f). By the end of the period the sector with the smallest change in labor-share contribution is Rural Industries. This fact, however, masks a huge dip early '90s and then a steady climb in labor share contribution since. That said, Rural Industries are a relatively minor component of the labor share, contributing about 1/20th the amount of labor share that Services does.

Breaking out the Sectors' contributions like this allows us to think of counterfactuals. If, for example, there was no decline in Manufacturing's within-sector labor share, then, all else equal, the labor share in 2014 would be about 2.5 percentage points higher and people might not be so worried about the falling labor share. Similarly, if the sectoral make up of the U.S. economy remained the same as in 1987 and only Manufacturing's within-sector labor share changed, the aggregate labor share in 2014 would be almost 5 percentage points higher. Here we see that the main driver of the falling labor share is not the shift away from high-labor share industries but is within industry dynamics.

3.4.2 Components of a Changing Labor Share

Figure 3.7 shows that the, approximately, 2 percentage point fall in the labor share from 1987 to 2014 is due entirely to the falling compensation ratio within industries, which has been partially counteracted by increases in labor share due to reallocation towards higher compensation ratio industries and increases in labor share due to self-employed output. The first bar in the graph (blue) shows the overall change in the labor share over the covered period. Overall, the labor share had declined about 2 percentage points in the period. The second bar (orange) is the change in the labor share due to the change in the covariance term in Equation 3.8. It shows that the output reallocation has actually counteracted other drags on the labor share by pushing the labor share up over 1 percentage point. This result should assuage fears that (part of) the falling labor share has to do with the U.S. economy's shift away from manufacturing towards services. If this were the case, then changes in the labor share due to reallocation would be negative. Instead, the evidence suggests that the move away from manufacturing has actually bolstered the labor share over this period. The third bar (green) shows the fall in labor share attributable to changes in the within-industry average compensation ratio. The fall in the compensation ratio over this time is almost 5 percentage points. The final bar (red) is the change in labor share due to changes in the percent of output created by the self employed. The shift to self employment has also counteracted the drag on the labor share of average compensation ratios and has boosted the labor share by over 1 percentage point as well. These taken together, then, demonstrate that the falling labor share since 1987 is entirely due to changes in compensation ratios within industries, counteracted by reallocation and self-employment.

The results for the whole period, however, do mask some heterogeneity. Figure 3.8 divides 1987-2014 period into 6 sub-periods and displays the change in the labor share and its constituent components in each. While the labor share does not fall in each subperiod, the labor share changes are driven mostly by changes in the composition share (green). In each subperiod the change due to the compensation share is the component with the biggest absolute value. The change in the compensation ratio is positive in only 2 of the 6 subperiods ('87-'91 and '97-'01). Additionally, changes in variance and changes due to self-employed output seem to act as a counterbalance to changes in the compensation ratio, most of the time. In 4 of the 6 subperiods, both of these changes have the opposite sign of the changes in compensation ratio ('87-'91, '92-'96, '07-'11, and '12-'14). Interestingly, changes in covariance are only a headwind to the labor share in the very earliest period of the study ('87-'91). For the whole rest of the studied period, it acts as a tail wind, pushing the labor share up.

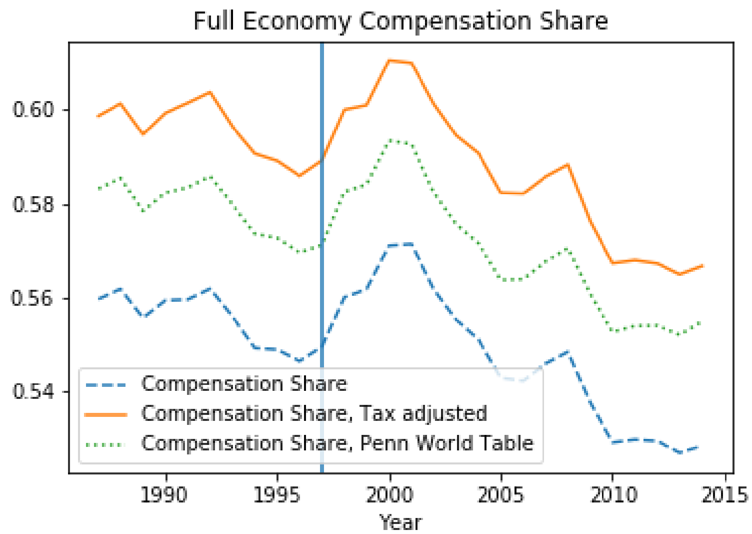
3.5 Conclusion

Questions about the falling labor share are important for economists to answer. This paper attempts to examine the relationship between the falling labor share and other macroeconomic indicators by decomposing the labor share and examining the constituent pieces. In doing so, this paper shows that the falling labor share is due to decreases in within industry labor share while output reallocation and self-employed output have worked towards bolstering the labor share.

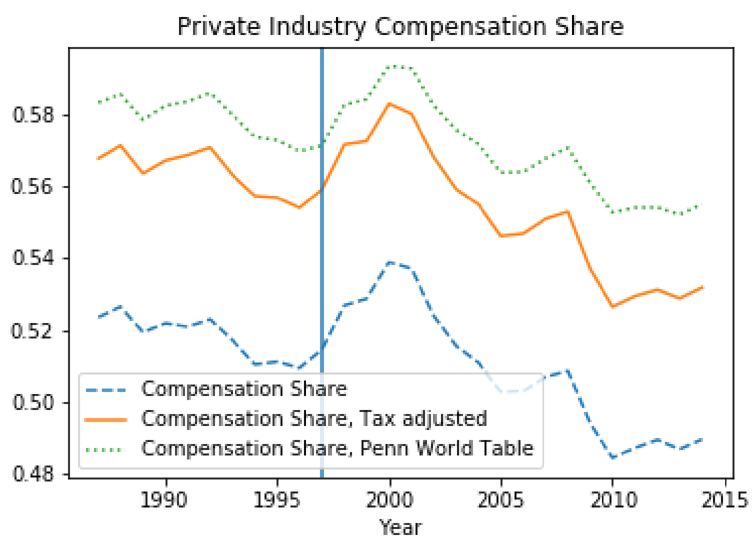
3.6 Tables and Figures

Table 3.1: Industry Aggregation

Manufacturing	Manufacturing
Services	Information Professional and business services Educational services, health care, and social assistance Arts, entertainment, recreation, accommodation, and food services Other services, except government
Rural Industries	Agriculture, forestry, fishing, and hunting Mining
TWUC	Transportation and warehousing Utilities Construction
Wholesale Trade	Wholesale Trade
Retail Trade	Retail Trade
FIRE	Finance, insurance, real estate, rental, and leasing
Government	Federal State and Local



(a) Total Economy



(b) Private Industries

Figure 3.1: Change in Compensation Share

Figure 3.2: Labor Share Inflator

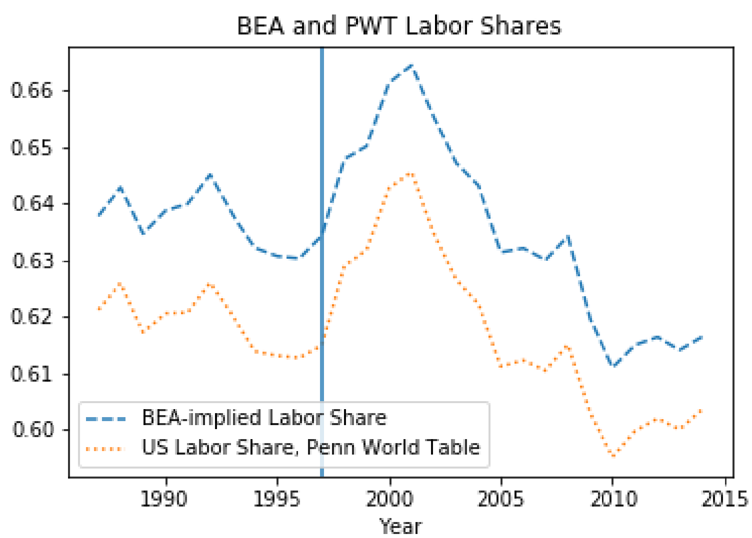
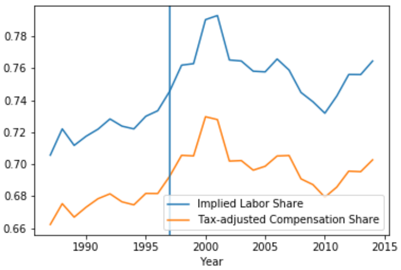
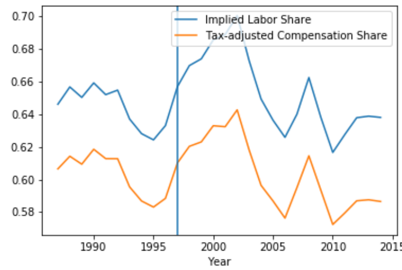


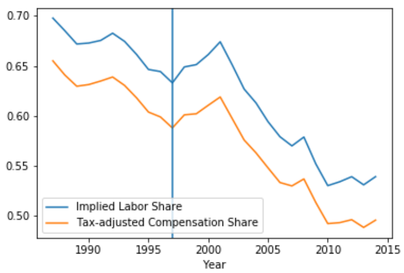
Figure 3.3: Economy Wide Labor Share



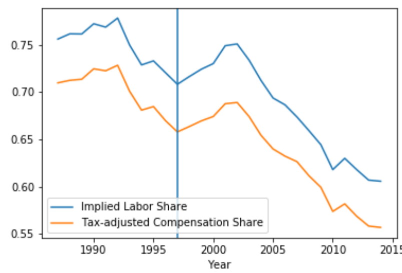
(a) Services



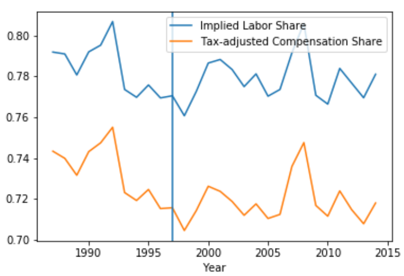
(b) Utilities, Construction, and Transportation



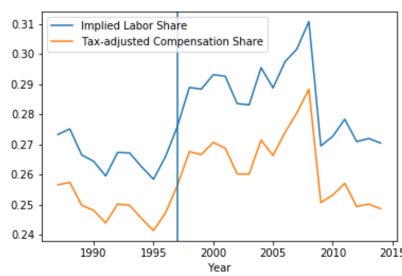
(c) Manufacturing



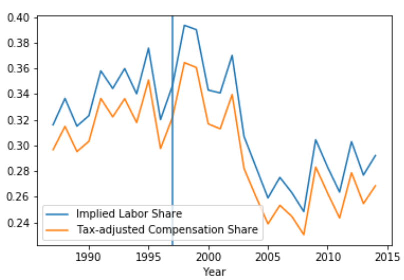
(d) Wholesale Trade



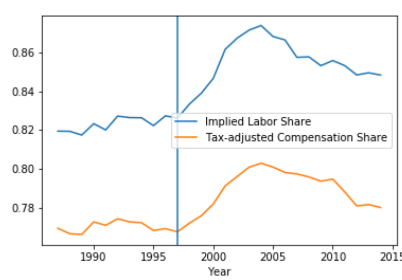
(e) Retail Trade



(f) FIRE

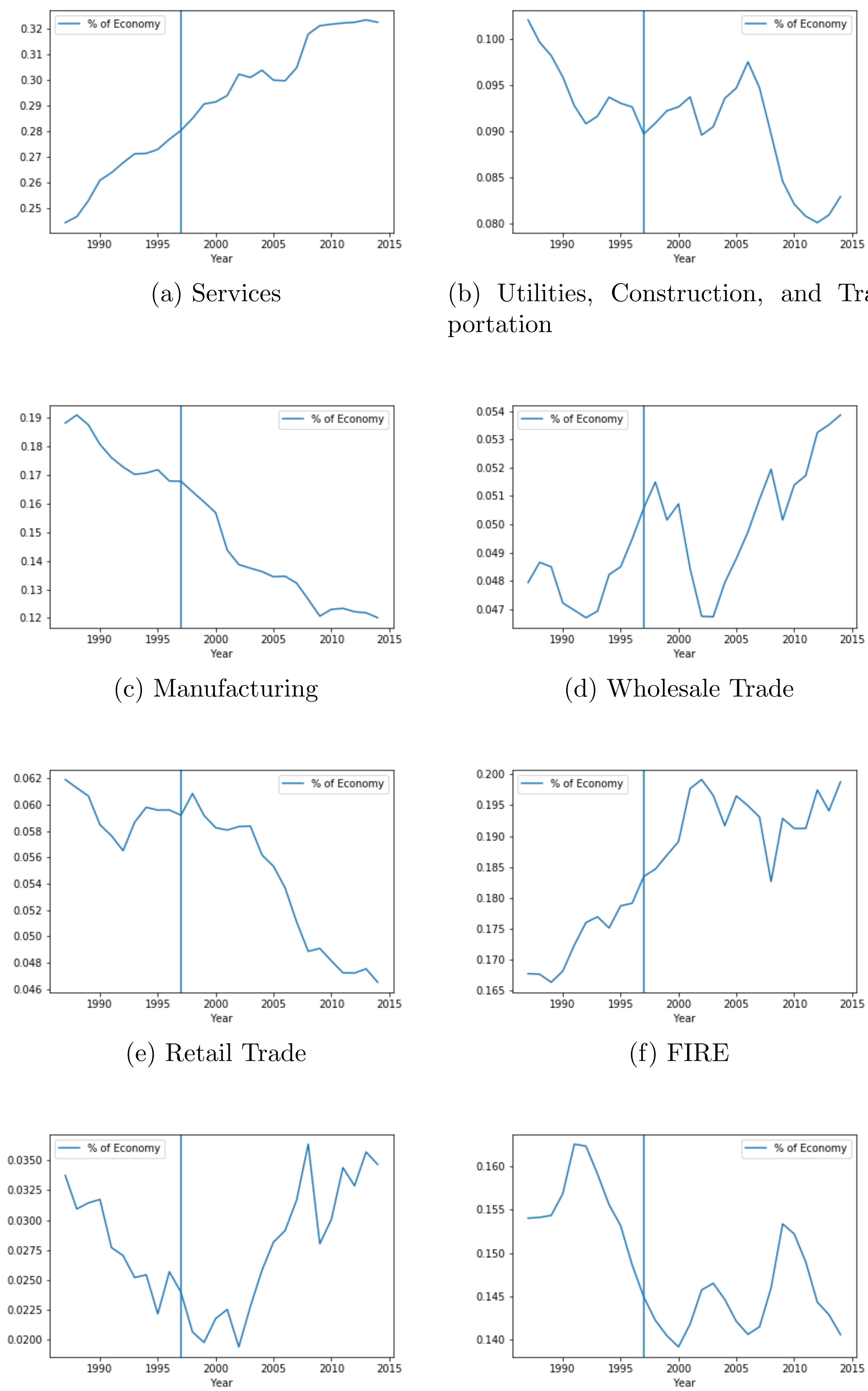


(g) Rural Industries



(h) Government

Figure 3.4: Sector Labor Shares



(a) Services

(b) Utilities, Construction, and Transportation

(c) Manufacturing

(d) Wholesale Trade

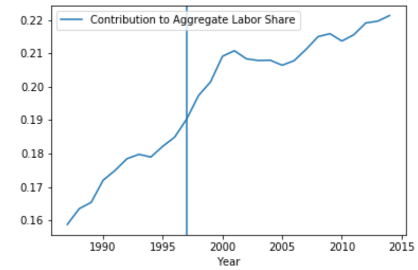
(e) Retail Trade

(f) FIRE

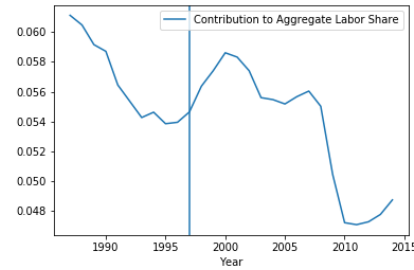
(g) Rural Industries

(h) Government

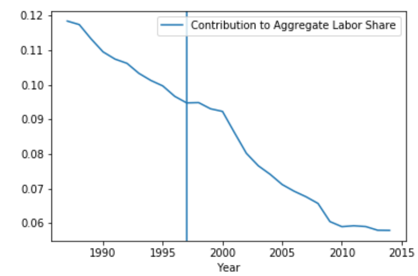
Figure 3.5: Sector Economy Shares



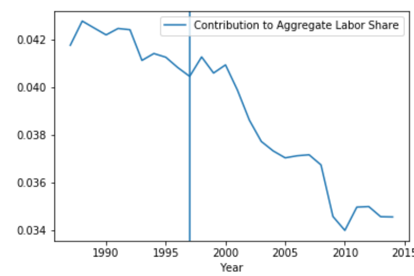
(a) Services



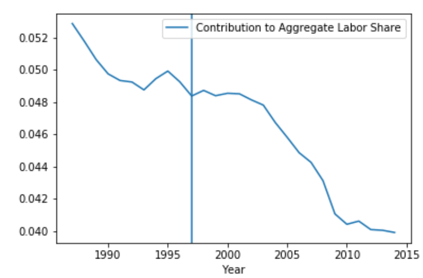
(b) Utilities, Construction, and Transportation



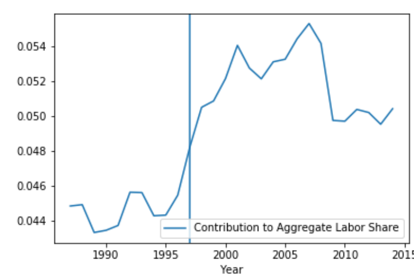
(c) Manufacturing



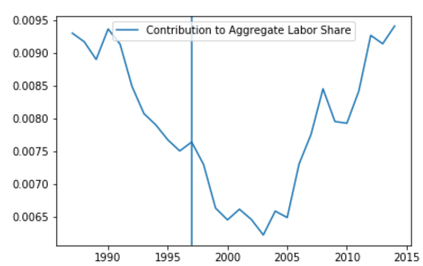
(d) Wholesale Trade



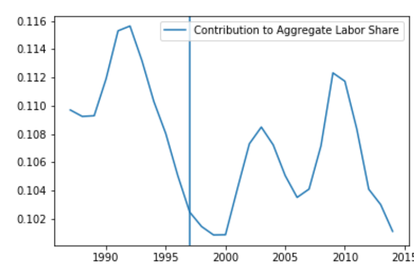
(e) Retail Trade



(f) FIRE



(g) Rural Industries



(h) Government

Figure 3.6: Contribution of each Sector to Aggregate Labor Share

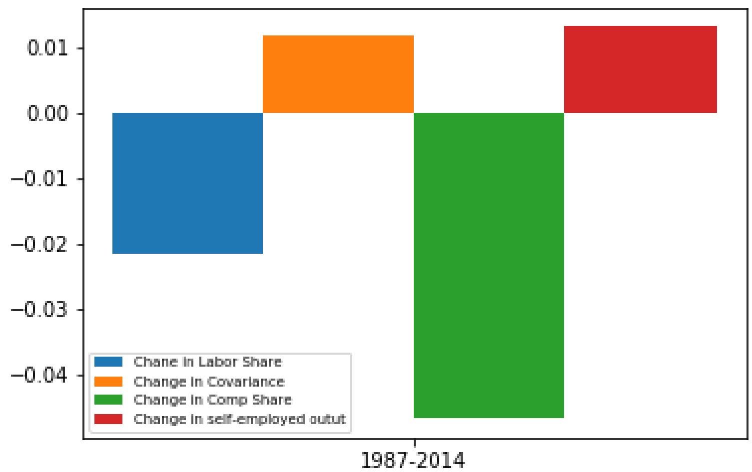


Figure 3.7: Total Change in Labor Share and Constituents

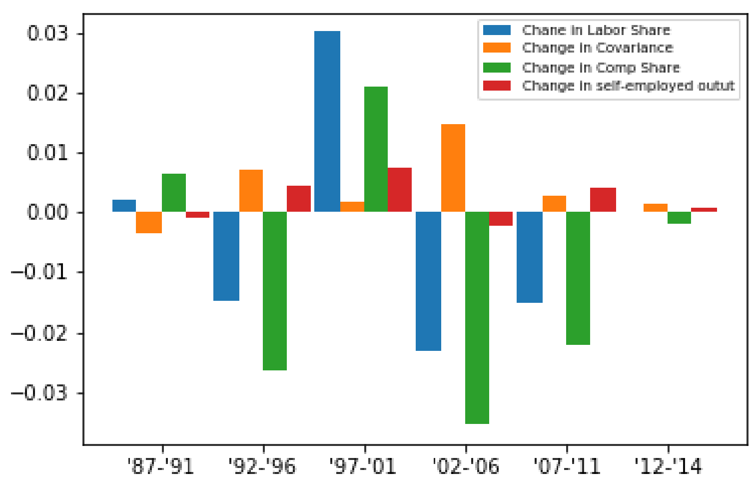


Figure 3.8: Total Change in Labor Share and Constituents, Subperiods

APPENDIX

APPENDIX A

Chapter II supporting material

A.1 Sample Construction and Matching

A.1.1 Overview

We combine information on private equity buyouts from CapitalIQ and other sources with firm-level and establishment-level data held by the U.S. Census Bureau. We start by matching buyout deals to target firms and their establishments in the Census Bureaus comprehensive Business Register (BR). Our basic approach is as follows. First, we use name and address information to match a particular deal to a specific unit in the BR. Because the matching algorithm relies partly on address information, this step identifies a specific establishment owned by the target firm, which is often but not always a headquarters facility. Second, we use the BR link between that establishments ID and its parent firm ID to identify the target firm in the BR. In most cases, this method identifies the target firm in the BR and all of its establishments.

After matching to the BR, we use the Longitudinal Business Database (LBD) essentially a longitudinal version of the BR to follow target firms and their establishments over time. We also use the LBD to identify control units (comparable firms and establishments) and to follow them over time as well. In addition, we exploit common alphanumeric identifiers to incorporate other Census micro data for some aspects of our analysis.

The LBD tracks establishments and parent firms using a combination of administrative records and survey collections that include the Company Organization Survey (COS), the Economic Censuses, and the Annual Surveys of Businesses (e.g., the Annual Survey of Manufactures). Information about company structure is incorporated into the LBD by attaching firm identifiers to records for establishments. Ownership changes are identified when establishments switch parent firm through mergers, acquisitions, and divestitures.

The Census Bureau assigns a unique firm ID to all establishments under common ownership and control in a given year, including establishments that belong to subsidiaries under control of the parent corporation. This firm ID is distinct from a taxpayer ID such as the employer identification number (EIN). The relationships among the various IDs are as follows. In any given year, an establishment is uniquely associated with a single taxpayer ID and a single firm ID. Moreover, each taxpayer ID is uniquely associated with a firm ID. For multi-establishment firms, a parent firm ID has multiple affiliated establishment IDs and potentially multiple EINs. Put differently, the EIN as a unit of observation is somewhere between an establishment and a firm.

A.1.2 Matching Buyout Targets to the Business Register (BR)

From Capital IQ and other sources, we obtain several pieces of information about the acquired entity in a private equity buyout. These pieces include the name of the seller, the name of the acquisition target, the targets address, and the acquisition date. The seller and target are typically the same in whole-firm acquisitions but not in partial-firm acquisitions for example, when the private equity firm acquires one division of a multi-division company.

We match acquisition targets to firms in the BR using the data matching algorithms that are part of the SAS DQMatch procedure. This is an improved version of the matching code we developed and implemented algorithm used in *Davis et al.* (2014). Our DQMatch implementation proceeds through 16 rounds of matching from the strictest criteria (requiring a perfect match on name and address) to progressively looser criteria that allow for fuzzier matching (exact name and fuzzy address, fuzzy name and exact address, exact name and zip code, etc.) Results from each pass are flagged and the results are stored for use in later analyses. For brevity, we do not discuss the DQMatch matching criteria and the algorithm used to identify matches in detail.[1] Here, we describe our overall matching strategy, explain how we resolve buyout deals that match to multiple target firm candidates in the BR, and discuss issues that arise in tracking firms over time.

A Simple Case

Suppose a private equity firm acquires firm A in its entirety during year t and places it under new ownership, possibly with a new name. A simplified version of our matching algorithm in this case works as follows: First, we find an establishment in the BR as of year t located at the target address and owned by a firm with the target name. Second, with this match in hand, we use the firm-establishment links in the BR to identify the full set of establishments operated by the target firm in t. From this point, we can measure the

[1] Programs to implement the DQMatch algorithm and master batch files to run them are available on the computing cluster servers in the Federal Statistical Research Data Centers.

activity of the target firm in t and follow the firm (and its establishments) forward from t using the LBD.

Challenges that Arise in the Matching Process

In practice, several challenges arise in the matching process. First, because name and address data are noisy, we may find multiple BR firms that are candidate matches for the acquisition target.[2] All but one of these candidates, and perhaps all of them, are false positives.

Second, to cope with timing differences between datasets, we search for matches in the BR over a three-year window centered on the buyout year. While this approach can pick up good matches that we would otherwise miss, it can also introduce additional false positive matches. Whenever we have multiple candidate matches, we need some way to resolve to a unique match. When we cannot do so with sufficient confidence, we drop the acquisition target from our analysis.

Third, it can be hard to distinguish the seller firm from the acquisition target in some cases. For example, suppose a private equity firm acquires establishments e_1 and e_2 from firm A to form a new firm B in year t. In this case, the activity of establishments e_1 and e_2 are associated with both firms A and B in t, because each firm files tax records that cover e_1 and e_2 for part of the year. Thus, when we match the target address to an establishment, that establishment may link to two parent firms in the BR in the buyout

[2] We use both physical and mailing address from the Business Register when available to generate matches. There is some noise in the addresses for new units in the Business Register that is typically resolved in an Economic Census. Our use of a multi-year window should help overcome some of this source of noise. Note that we did not find that our match rates had peaks in Census years suggesting this is not a major issue.

year. In this situation as well, we need some way to resolve to a unique match.

Fourth, some private equity buyouts involve complex reorganizations of target entities that lead to the creation of multiple new firms or the piecemeal sale of the target entity to multiple parties. In these cases, even when we successfully match the target address to an establishment and correctly identify that establishments parent firm, we may identify and track only some of the establishments acquired as part of the buyout. Indeed, there can be multiple true successor firms to the target entity in such cases, and we may capture and track only one of them.

Fifth, another challenge involves divisional buyouts, whereby the private equity firm acquires only part of a multi-division firm. For divisional buyouts, we could not always identify the correct target firm in the BR after matching the deal to a specific establishment. These instances arose because, in some cases, the Census firm ID associated with the matched establishments did not change to reflect the ownership change of the division involved in the buyout deal. We identified these problematic cases by observing that the matched target establishment remained affiliated with the parent seller firm even after the buyout transaction. It is our understanding that the Census Bureau on occasion had difficulty tracking the new firm in divisional buyouts because of nonresponse on the COS or other survey instruments.

We thus had two types of divisional cases. The first are those where we could accurately identify the target firm using our main method, and the second where we could not. Even in those cases, we were able to link the matched establishment to at least a part of the target firm through the EIN (taxpayer ID). The complete target firm may or may not be identified in such cases, because the divisional business involved in the buyout may have operated with multiple EINs. In the main text and this appendix, we refer to such cases as EIN cases. In these EIN cases, we can accurately identify a part of the target firm in the transaction year and at least some of the corresponding target establishments, but we cannot be confident that we captured the entire target firm. We exclude EIN cases in our firm-level longitudinal analyses, because the EIN is not suitable for tracking firms over time. For example, if a target firm (i.e., an EIN case) creates or acquires a new establishment, it may obtain a new EIN for that establishment for accounting or tax reasons. In such cases, we would not know that the new establishment is part of the target firm.

We develop a methodology that takes advantage of the timing of the acquisition event and the time series properties of the associated firms to identify the target. Our strategy then requires we match the target name and address to a window of years around the acquisition event. For each target we match their information to the BR at time of acquisition, t, and to $t + 1$ and $t - 1$. In addition to the history of activity, we also exploit the employer tax identifier, the EIN, of the firms associated with the target.[3]

How We Proceed

We describe our process for de-duplicating the buyout transactions that are matched to multiple Census firm IDs by separating them into a set of mutually exclusive cases.

<u>No Matches</u> In about 2000 of the 9794 deals in CapitalIQ, no companies within the BR matched even using the loosest matching criterion.

<u>Unique Matches</u> As previously described, the search algorithm first proceeds through 16

[3]The EIN is an employer tax identifier that may or may not change when ownership changes. It is often helpful in matching and tracking target firms and establishments involved in complex reorganizations.

rounds of matching using progressively less strict match criteria. A unique match is a match to a single firm identifier in the strictest match criterion available for that deal. For example, suppose a buyout transaction target from CapitalIQ matches to a single BR entry in round 4 of our algorithm. If it also matches to multiple firms in subsequent, less strict rounds, but had no matches until round 4, then this entry is a unique match. From the initial 9794 deals in CapitalIQ, we find about 4000 unique matches.

Non-Unique Matches and De-Duplications The remaining set of about 3500 deals from CapitalIQ match to multiple firms within the round where the strictest match criterion is applied. This could happen, for example, if we exact match on address, but there are multiple firms in a single building with similar company names. We use several methods to arrive at a unique match between the CapitalIQ and a Census firm ID.

The first method for de-duplicating our dataset is to check the EINs of the matched firms. In about 25 percent of the duplicate, matches matched to multiple firms with the same EIN. This possibility arises given our use of the three-year window and is an indicator that M&A and/or reorganization activity is underway. This enables us to link the multiple matches and we follow the continuing firmid when calculating employment growth rates, etc.

The second method for de-duplicating or data is to exploit the timing pattern of the matches. We look for cases with at least two firms associated with the same deal. A common pattern is that one of them is a birth of a new firm ID at time t or t+1 and the other is a death at time t-1 or t. In this context, a birth is when a firm ID appears at time t or $t + 1$ and that firm ID did not exist in the preceding years ($t - 1$ if the birth is in t or $t-1$ and t if the birth is in $t+1$) and a death is when a firm ID disappears in time t or $t+1$. We investigated these patterns and determined they are likely to indicate PE-precipitated reorganization. These firm IDs were already matched using name and addressing matching criteria, so that they are not simply spurious patterns in the data. This second step uniquely resolves about 200 additional firm IDs to a transaction.

The third method follows four rules we developed to help resolve duplicate matches.

Rule 1: Within a set of duplicates, we choose the firm ID that has the strictest match criteria. For example, if we have a duplicate match and one firm identifier has an exact name and address match and a different firm identifier only matches on the name, we resolve the duplicate by keep the highest quality match.

Rule 2: We choose the firm IDs with the strictest match criteria and condition on survival to period t+1 in the LBD.

Rule 3: We apply Rule 2, but then also include resolutions from Rule 1 that may not have survived into t+1.

Rule 4: We change the order of operations. We condition on survival to period t+1, then choose the match that satisfies the strictest matching criteria.

When all four of these rules resolve to the same firm, we consider that firm to be the match and use it in our analysis. These rules uniquely resolve about 1000 additional deals to a Census firm ID. Combined, these resolution criteria yield approximately 2000 additional matched deals. This gives us the total sample of approximately 6000 matched deals.

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