Markups and Business Dynamism across Industries

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Recent research connects rising measured market power to other macroeconomic trends in the U.S., including decades-long declines in measures of "business dynamism," such as job reallocation and business entry. Intuitively, factors that raise market power may also reduce entry, and firms with more market power are less responsive to shocks. Such theories predict a negative correlation between market power and business dynamism. We use industry-level data to study long-run trends and annual patterns of market power and dynamism. Using multiple measures of each, we find no compelling evidence that rising market power is associated with declining dynamism. In fact, we sometimes observe the opposite relationship. Our results suggest that market power does not explain the decline in dynamism.

JEL-Classification: D22, D40, L11, L26, M13

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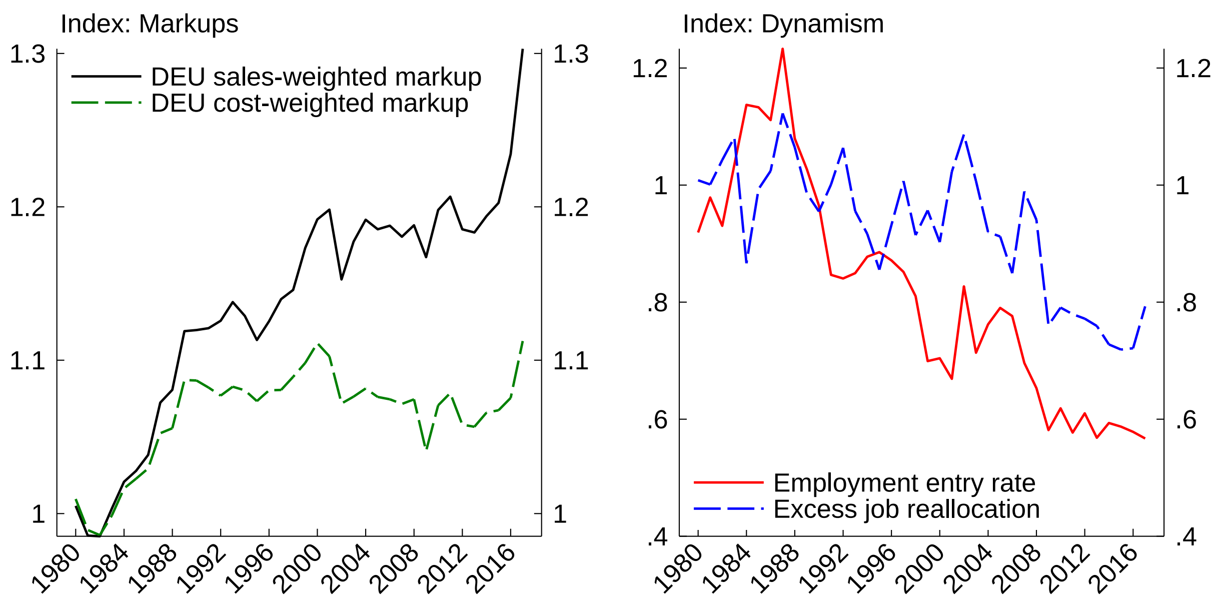
1 Introduction

Over the past four decades, the U.S. economy has experienced two notable trends that have drawn attention from researchers and policymakers, illustrated by figure 1. First, there is some evidence that an important measure of market power—the average markup—has risen significantly in recent decades (left panel; De Loecker, Eeckhout, and Unger 2020). Second, common measures of "business dynamism"—such as new business entry rates and excess job reallocation—have seen significant declines (right panel).[1] The concurrent timing of these trends raises an important question about whether these patterns are related.



Note: Index is relative to 1980-1984 average by series. Entry rate is employment based.
Source: Business Dynamics Statistics; De Loecker, Eeckhout, & Unger (2020); and Compustat.

Figure 1: Markups and business dynamism, 1980-2017

In this paper, we ask whether rising markups can explain for the observed decline in U.S. business dynamism. While the aggregate time series evidence suggests a potential

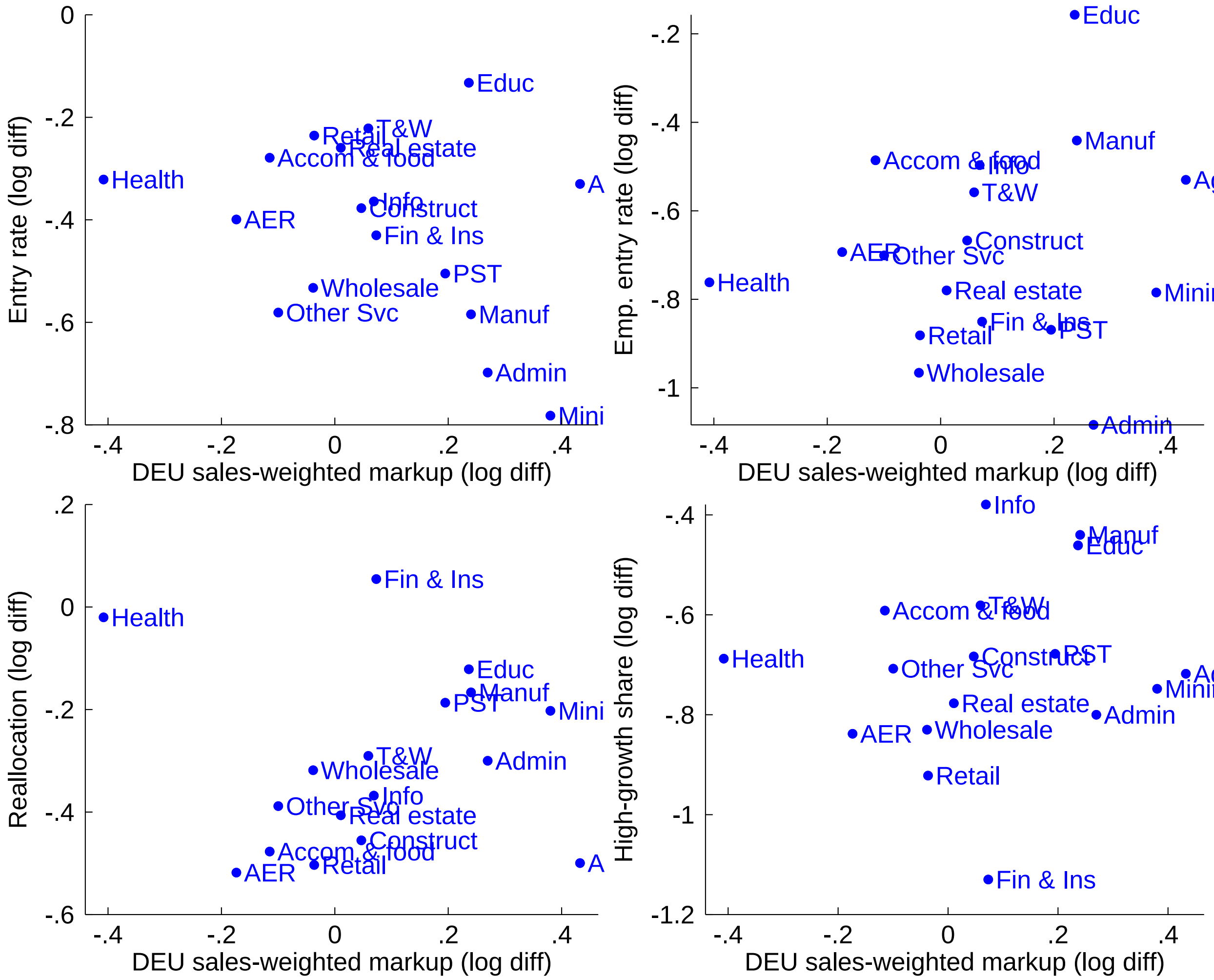
1. For examples, see Hyatt and Spletzer (2013), Decker et al. (2014), Karahan, Pugsley, and Sahin (2019), Decker et al. (2020), and Akcigit and Ates (2023).

connection, we take a more granular approach by analyzing *cross-sectional* evidence at both low and high frequencies. Our analysis is motivated by a simple premise: if rising markups (or something else that directly causes rising markups) are driving the decline in dynamism, we should observe a strong negative correlation between changes in markups and changes in dynamism at the industry level.

Contrary to the hypothesis suggested by the time series, we find almost no supporting evidence at the industry level. Our analysis, which employs multiple measures of both dynamism and markups and examines both long-run changes and annual fluctuations, reveals no systematic negative correlation between rising markups and declining dynamism. In fact, we are more likely to find evidence of a positive relationship.

Figure 2 provides a visual summary of our findings by plotting sector-level changes across four measures of business dynamism—firm entry rates, employment-weighted firm entry rates, excess job reallocation, and the share of employment at high-growth firms. These dynamism measures are plotted against changes in a popular average markup measure developed by De Loecker, Eeckhout, and Unger (2020). No clear negative pattern catches the eye; the sectors in which markups have risen most are not necessarily the sectors that have seen large declines in dynamism. Instead, the variation appears to be random, as if changes in markups and changes in dynamism have been driven by different sources. If an underlying negative relationship between market power and dynamism were a significant driver of aggregate patterns, we would expect to see a more systematic relationship at the sector level.

We further show that drilling down to narrower industry detail, using several alternative measures of markups, exploring different regression weighting specifications, and exploiting different time series structure does not typically yield the negative relationship



Note: Difference, 2013-2017 average vs. 1980-1984 average. NAICS 22 omitted.
Source: Business Dynamics Statistics; De Loecker, Eeckhout, & Unger (2020); and Compustat.

Figure 2: Change in markups and dynamism, broad sectors

seen in the aggregate time series, with relatively few possible exceptions, and sometimes yields a positive relationship.[2]

2. As we will show, in certain (but not all) econometric specifications, a modestly negative markup/dynamism relationship is observed for the unweighted firm entry rate—a measure that is susceptible to measurement issues and lacks the economic significance of our other dynamism measures. A modestly negative relationship is also sometimes seen for the markup measured specifically as the inverse of the labor share—though not for inverse energy or materials shares, nor for the widely used production function-based markup measures used by De Loecker, Eeckhout, and Unger (2020). The labor share finding seems relevant for the voluminous labor share literature but, given its marginal statistical significance and the results from our many other measures, does not seem particularly relevant for the question of rising product market power and business dynamism. These exceptional results highlight the importance

Our empirical results—or, perhaps more accurately, non-results—are not trivial or inevitable. The theoretical connection between dynamism and average market power is straightforward. For example, market power makes firms less responsive to shocks, which dampens job reallocation. More generally, under common assumptions, factors that reduce the number of potential entrants to a market could be expected to manifest as both lower dynamism and higher average markups. All research to date on the connection between dynamism and markups directly or indirectly includes such a mechanism that drives markups up and dynamism down (Akcigit and Ates 2021, 2023; De Loecker, Eeckhout, and Mongey 2022; De Ridder 2024).

Our empirical approach and inference depend heavily on measurement considerations. For markups, we first follow the seminal work of De Loecker, Eeckhout, and Unger (2020) (DEU), which constructed markup estimates using sales and cost data along with revenue function estimation for the universe of publicly traded firms. Since this markup measure—which we call "DEU markups"—is available at the firm level in publicly available data, we can construct average markups at any arbitrary level of industry detail; given sample size, we focus on the 2-digit and 3-digit NAICS levels. By following the methodology of De Loecker, Eeckhout, and Unger (2020) we can construct these markups starting in the early 1980s—the starting point for our dynamism measures—and running through 2017. This markup measure is not without controversy, but it has been used in widely cited papers and continues to inform discussions about market power among both researchers and policymakers.

To complement our analysis of DEU markups, we use industry-level measures constructed from the BEA-BLS Integrated Industry-level Production Accounts (better known as "KLEMS"). The KLEMS data offer several key advantages: comprehensive industry

of considering a range of dynamism and markup measures and judging overall patterns across multiple specifications.

coverage inclusive of both public *and private* firms, rigorous adherence to proper output measurement, and consistent industry definitions throughout the sample period. These data run from 1988 through our last year of analysis, 2019. From these data, we construct several "markup" measures—admittedly using the term "markup" loosely—including basic cost share approaches and more sophisticated Hall (2018)-style instrumental variable-based estimates.

For dynamism, we examine employment-based entry rates (i.e., the share of employment accounted for by new firm entrants), excess job reallocation rates, and the prevalence of high-growth firms measured in the Census Bureau's Business Dynamics Statistics (BDS) for the near-universe of private nonfarm employer businesses.[3] These measures are available at various levels of industry detail and can cover the time period from the early 1980s through 2019.

Across these various measures, our results show a striking disconnect between aggregate time series trends and cross-sectional evidence. When studying "long differences"—industry-level changes in markups and dynamism from the 1980s through the late 2010s—we do not generally observe that industries with large gains in markups saw larger declines in dynamism, and the opposite pattern holds in many specifications. At annual frequency, using impulse response functions from local projections, we find generally noisy and statistically insignificant results over 3 to 4 year horizons.

Our results have important implications for the literatures on both dynamism and market power. While several important studies hypothesize a negative relationship between dynamism and market power (e.g., Akcigit and Ates 2021, 2023; De Loecker, Eeckhout, and Mongey 2022; De Ridder 2024) our results challenge this view. Importantly, we recognize that the theoretical relationship need not be causal; various other factors could

3. As noted above, we also study simpler unweighted firm entry rates, which have been used in a vast literature, though we do not focus on this measure for reasons discussed below.

affect both markups and dynamism to produce a negative reduced-form relationship. However, the absence of this relationship in the cross-sectional data suggests that market power likely plays at most a minimal role in explaining declining dynamism, or that any relationship involves more complex channels than previously theorized. Perhaps most tellingly, we find that the major increases in markups and decreases in dynamism have largely occurred in different industries.

This paper builds on an earlier short preview note (Albrecht and Decker 2024) in which we reported a smaller set of exercises focused on industry-level long differences similar to those shown on figure 2. That short note was limited in both dynamism measures and markup measures, and we did not explore annual-frequency time series relationships. The present paper is a far wider-ranging study—albeit with results confirming those of the older short note.[4]

The remainder of this paper proceeds as follows. Section 2 briefly reviews the literature on declining dynamism and rising markups, highlighting the theoretical channels through which these phenomena might be connected. Section 3 describes our data sources and measurement approaches. Section 4 presents our main empirical analysis of the long-run relationship between markups and dynamism across industries. Section 5

4. In particular, in the present paper we explore an additional dynamism measure: the prevalence of high-growth firms, a measure featured in older dynamism literature (Decker et al. 2016b) and receiving renewed attention recently (Kim et al. 2024). We also add numerous additional markup variables arising from the national accounts data, which address the significant limitations of the public firms-based markup measures used by Albrecht and Decker (2024); these additional measures provide evidence that the lack of a negative relationship between markups and dynamism found in Albrecht and Decker (2024) is not simply an artifact of specific markup measures while also pointing to a potential story around the labor share (the subject of its own large literature). Part of the value of these additional markup measures is that they are available at a detailed industry level that is likely to be far more robust than the detailed industry tabulations used in Albrecht and Decker (2024), which, relying on Compustat data limited to publicly traded firms, often featured sparsely populated industry categories. Finally, while in Albrecht and Decker (2024) we focused only on long-run changes in dynamism and their relation to long-run changes in markups, in the present paper we add annual-frequency time series analysis to uncover comovement of dynamism and markups within shorter time windows. These large additions to our analysis of this critical question provide new insights and confirm the robustness of our earlier result. In addition, in the present paper we summarize results from a large number of robustness exercises around both our long-run change results and our annual-frequency results.

examines the annual-frequency relationships using local projections to generate impulse response functions. Section 6 concludes.

2 Related Literature

A large empirical literature documents the sustained decline in dynamism in the U.S. since the 1980s.[5] This decline appears across multiple dimensions of business activity. Entry rates and the pace of job reallocation have fallen substantially, as shown in figure 1 and documented extensively in the literature (Decker et al. 2014; Decker et al. 2016a; Karahan, Pugsley, and Sahin 2019; Alon et al. 2018).

Closely related has been a more recent decline of entry in the high-tech sector and lower prevalence of high-growth young firms (Decker et al. 2016b; Haltiwanger, Hathaway, and Miranda 2014; Guzman and Stern 2020; Kim et al. 2024). The decline in entry has coincided with declining gross job reallocation and within-firm employment volatility (Davis et al. 2006; Decker et al. 2014; Decker et al. 2016b), worker flows (Hyatt and Spletzer 2013; Davis and Haltiwanger 2014), and internal migration (Molloy et al. 2016). Falling job reallocation has been associated with weaker responsiveness of firms and establishments to productivity shocks, with potentially significant implications for aggregate productivity growth (Decker et al. 2020).

In tandem with the declining dynamism literature, a widely noted literature on rising market power has rapidly expanded. De Loecker, Eeckhout, and Unger (2020) (DEU) provide a commonly cited estimate of rising markups in the U.S. economy, showing that the average markup increased from 1.2 in 1980 to 1.6 in 2016. Their paper uses an approach to estimating markups that comes from Hall (1988) and De Loecker and Warzynski (2012).

5. Most of this literature predates the recent pandemic; Decker and Haltiwanger (2023, 2024) provide evidence of elevated dynamism during the pandemic, though it is unclear whether this marks a durable reversal of the longer-run trend. We deliberately abstract from the pandemic era.

The markup concept requires data on expenditures on any variable input along with total sales and the output elasticity of the variable input. The data that DEU use come from Compustat, which includes only publicly-traded firms and provides data on sales as well as cost of goods sold, which the authors use as their expenditure on variable costs. Their estimates have informed ongoing debates about market power's role in broader economic trends, and our study adopts their benchmark data to examine its connection with declining dynamism. While the validity of the DEU methodology is the subject of a substantial debate, we abstract from these concerns and focus on the empirical implications for industry-level relationships between markups and dynamism.[6]

Our approach to studying markups also builds on Hall (2018), who develops a framework for measuring markups using industry-level BEA-BLS productivity data (KLEMS). These data provide comprehensive national account-consistent coverage of all industries and carefully constructed measures of inputs and outputs. Hall develops a framework to directly measure marginal costs using these detailed industry-level productivity data, providing an alternative to the production function approach of De Loecker, Eeckhout, and Unger (2020). Intuitively, marginal cost is constructed as the elasticity of total costs to sales; to avoid endogeneity between these variables, Hall (2018) employs certain national defense spending categories and oil prices as instruments. Hall finds rising markups in recent decades. We adapt Hall's approach and refer to the resulting markup estimates as "Hall-style markups".

There is by now a large literature on aggegrate markups, each relying on slightly different models or data. Traina (2018) uses Compustat data for public firms but includes sales and administrative expenses as a variable cost (this is a broader definition of variable costs than in De Loecker, Eeckhout, and Unger 2020). Edmond, Midrigan, and Xu (2023)

6. There has been a large econometric debate about possible issues with the DEU approach to estimating markups. See Flynn, Gandhi, and Traina (2019), Kirov and Traina (2023), Bond et al. (2021), Doraszelski and Jaumandreu (2019, 2021), De Loecker (2021), and De Ridder, Grassi, and Morzenti (2024).

also use the data on public firms but develop a model of oligopoly in which the proper measure of misallocation is a cost-weighted average markup instead of a sales-weighted markup as in DEU; using cost weights, the average markup has increased by much less than the sales-weighted version. Autor et al. (2020) find there has been a reallocation toward "superstar" firms with higher markups, a pattern also found in De Loecker, Eeckhout, and Unger (2020). Foster, Haltiwanger, and Tuttle (2022) use Census Bureau data on the universe of manufacturing establishments—a sector that is important for the public firm-based markup rise—and find a smaller increase in average markups when revenue functions are estimated with more industry detail. We view these measurement questions as important, but for our purposes we approach markup measurement by using a range of measures, including the DEU markups, the Hall-style markups, and looser measures constructed as the inverse of cost shares (for each of energy, materials, labor, and overall "variable" costs). That is, we do not take a stand on the best measure of markups, choosing instead to look for patterns across many measures.

The parallel trends in dynamism and market power have motivated a growing theoretical and empirical literature linking the two.[7] Decker et al. (2020) suggest the possibility that rising market power is related to declining dynamism based in part on simple model intuition: rising market power is often modeled as increasing curvature of revenue functions, which reduces firm- or establishment-level shock responsiveness and, therefore, aggregate job reallocation. De Loecker, Eeckhout, and Unger (2020), while focused on measuring market power through markups, suggests the rising market power that they find as a likely explanation for declining dynamism.

7. There are other proposed causes of the dynamism decline. These include declining labor force or population growth (Karahan, Pugsley, and Sahin 2019; Hathaway and Litan 2014; Ozimek and Wurm 2017), increasing stringency of regulations (Davis and Haltiwanger 2014; Autor, Kerr, and Kugler 2007; Johnson and Kleiner 2020; Goldschlag and Tabarrok 2018), changing business models like retail consolidation (Decker et al. 2016b; Foster, Haltiwanger, and Krizan 2006), or a shift toward more nonemployer activity (Bento and Restuccia 2022; Abraham et al. 2019).

De Loecker, Eeckhout, and Mongey (2022) provide the most direct examination of this relationship, developing a model where declining potential entry simultaneously reduces observed entry and raises incumbent markups. When calibrated and simulated, their model can more than explain the full decline in aggregate job reallocation. Other papers propose different channels through which these phenomena might be connected: a rise in the use of intangible capital (De Ridder 2024), IT technology (Aghion et al. 2019; Lashkari, Bauer, and Boussard 2019), changes in knowledge diffusion (Akcigit and Ates 2023; Olmstead-Rumsey 2022), or demographics (Peters and Walsh 2019).

3 Data

We combine three primary datasets for our analysis, covering business dynamics (the BDS), DEU markups (Compustat), and KLEMS markups (KLEMS).[8]

3.1 Dynamism Data

We obtain measures of business dynamism from the Census Bureau's Business Dynamics Statistics (BDS), which are publicly available tabulations from the confidential Longtidudinal Business Database (LBD) microdata. The BDS are a workhorse public-use data source for studying firm dynamics in the U.S., with annual data spanning from the late-1970s and currently runs through 2022. It includes tabulations by firm size and age, establishment

8. In addition to industry exclusions noted below, in all data sources we omit funds, trusts, and other financial vehicles (NAICS 525), lessors of nonfinancial intangible assets (NAICS 533), monetary authorities (NAICS 521), and management of companies and enterprises (NAICS 55), all of which are conceptually problematic and sometimes feature uninterpretable extreme outliers.

industry (up to 4-digit NAICS), and other categories.[9] Most of the literature on changing business dynamism in the U.S. relies on the BDS or the confidential LBD.

BDS data are annual with a March reference month; for example, reported job growth for the year 2015 in the BDS is a measure of job growth from April 2014 to March 2015. We adjust our other datasets—Compustat and KLEMS—to match this timing as closely as possible; in particular, we add one to the recorded year in Compustat and KLEMS prior to matching those data to the BDS. In other words, for example, we match the 2014 observations in Compustat and KLEMS to the 2015 observation in BDS. Throughout the paper, references to years on expressed on this BDS basis.

We focus on four measures of business dynamism, each capturing a distinct dimension of firm and job dynamics:

1. Employment entry Rate. The employment entry rate measures the share of total employment accounted for by new firms (those with age 0). That is,

$$eer_t = \frac{e_t^0}{\frac{1}{2}(e_t + e_{t-1})}, \tag{1}$$

where e_t^0 is employment among firms with age 0 (i.e., new entrants) in year t, and e_t is total employment among all firms in year t. The total employment is adjusted using the Davis-Haltiwanger-Schuh (DHS) denominator (Davis, Haltiwanger, and Schuh 1996) to ensure longitudinal consistency. This measure of entry—sometimes referred to as the

9. BDS data cover the near-universe of private nonfarm employer establishments, excluding only "farms" (NAICS 111 and 112), railroads (NAICS 482), private households (NAICS 814), and some other smaller categories of establishments. Importantly, the BDS are based on high-quality firm identifiers that permit tracking of firm age, where a "firm" is distinct from an "establishment." In Census Bureau parlance, an establishment is a single operating location of a business, while a firm is a collection of one or more establishments under common operational control or ownership. Firm age in the BDS is defined consistent with most U.S. business dynamics literature (e.g. Haltiwanger, Jarmin, and Miranda 2013): upon the first observation of a firm identifier, the firm is assigned the age of its oldest establishment—where an establishment is age zero in the first year in which it has reported (March) employment.

employment-based or employment-weighted entry rate—measures the economic magnitude of new entrants and is our preferred measure, as it captures the economic magnitude of firm entry and is robust to difficulties counting small firms and establishments.

2. Excess Job Reallocation Rate. The excess job reallocation rate is given by:

$$ejr_t = \frac{jc_t + jd_t - |jc_t - jd_t|}{\frac{1}{2}(e_t + e_{t-1})}, \tag{2}$$

where jc_t is gross job creation (total job gains among entering and expanding establishments), jd_t is gross job destruction (total job losses among downsizing and exiting establishments), and e_t is total employment. Excess job reallocation is a measure of the gross job flows that exceed what is necessary to facilitate net job growth and can be thought of as a second moment of the establishment employment growth distribution. In this paper, we use the terms "excess job reallocation," "job reallocation," and "reallocation" interchangeably.

3. High-Growth Employment Share. The high-growth employment share measures the share of total employment accounted for by firms classified as "high growth." Tabulations of firms, employment, and other variables by annual growth rate categories are available in a recently introduced public-use BDS dataset. We define "high-growth firms" as those with DHS growth rates of at least 0.8.[10] Formally, the high-growth employment share is

10. DHS growth rates are given by $(e_{f,t} - e_{f,t-1})/(0.5e_{f,t-1} + 0.5e_{f,t})$, where $e_{f,t}$ is employment at firm f in year t. For example, a firm that had 50 employees in year $t-1$ would have to have gained another 67 employees by year t to have a DHS growth rate of at least 0.8. Notably, new firms have DHS growth rate of 2 and are therefore included, by construction, in our definition of high-growth firms; while the BDS tabulations allow for excluding new firms from this set, such tabulations produce data suppression within industry cells. DHS growth is measured on an "organic" basis to avoid issues with merger and acquisition activity; intuitively, establishments owned by a firm in time t (and only those establishments) count toward firm growth from $t-1$ to t (see Kim et al. 2024).

defined as:

$$\text{HGFS}_t = \frac{\sum_{f \in HG_t} e_{f,t}}{\sum_f e_{f,t}}, \tag{3}$$

where HGFS_t is the high-growth employment share in year t, HG_t is the set of high-growth firms in year t, $e_{f,t}$ is the employment at firm f in year t, and the denominator sums employment across all firms. The numerator captures total employment at high-growth firms, while the denominator reflects total employment across the entire economy (or industry). Declining high-growth firm activity has been seen as one of the indicators of declining dynamism (Decker et al. 2016b; Kim et al. 2024).

4. Simple entry rate. In addition, in appendix material we report some results for a fourth measure, the simple or unweighted entry rate (often called the startup rate). This entry rate measures the share of *firms* in the economy that are new entrants, regardless of their size:

$$er_t = \frac{f_t^0}{\frac{1}{2}(f_t + f_{t-1})}, \tag{4}$$

where f_t^0 is the number of new firms in year t. This measure has been widely used in the dynamism literature but has two key limitations. First, the simple entry rate is heavily influenced even by very small entrants and is therefore less indicative of the overall economic magnitude of business entry than is the employment entry rate described above. Second, the simple entry rate is not robust to increasingly well known measurement challenges associated with identifying and appropriately categorizing small firms and establishments; small business units are the subject of considerable (and rising) disagreement between the Census Bureau's business register (underlying the BDS) and the Bureau of Labor Statistics (BLS) business register.[11] The two data sources are much closer for em-

11. For example, by 2021 there were roughly 2 million more establishments in BLS data than in Census Bureau data. While this issue has been studied in the past, it has recently gained renewed interest. A large portion of the discrepancy is related to differing classification rules for social assistance businesses; issues

ployment figures, prompting us to prefer the employment entry rate to the simple entry rate.

3.2 Compustat-Based Markup Measures

For our first set of markup measures, we use the benchmark estimates from De Loecker, Eeckhout, and Unger (2020) ("DEU markups"). DEU markups are calculated with a production function approach where the markup is given by:

$$\text{Markup} = \text{Output Elasticity of Variable Input} \times \frac{\text{Revenue}}{\text{Cost of Variable Input}}.$$

For their variable input, De Loecker, Eeckhout, and Unger (2020) use Cost of Goods Sold (COGS), but Traina (2018) argues for using COGS plus Selling, General and Administrative Expenses (SGA). In theory, any variable input works for the estimation, although Raval (2023), using Census data on manufacturing, rejects that the markup distributions are the same whether labor or materials are used. Foster, Haltiwanger, and Tuttle (2022) argue for using material costs in the manufacturing sector.

We obtain these estimates by using DEU's published replication files and Compustat data. While these files provide the revenue elasticities necessary for markup construction, they do not include code for the original revenue function estimation. Consequently, our sample extends through 2017, the end point of their replication files' coverage, though we begin in 1980 to align with BDS data availability. When aggregating across firms, we rely on the DEU benchmark sales-weighted markup for most exercises but also sometimes report results using a cost-weighted markup.

with tracking of business flows also play a role. See discussion slides here: https://rdeckernet.github.io/website/2024CRIW_discussion_CHMS.pdf.

A large literature has grown up debating bias and identification issues with these estimates.[12] In addition to limitations associated with production function estimation issues, these estimates rely on Compustat data covering only publicly traded firms. The firms in Compustat account for roughly half of aggregate private sales, and this share varies widely across narrow industries (even running well above 100 percent in some cases; see Decker and Williams 2023).[13] Existing literature finds that the business dynamics of privately held firms differ materially from those of publicly traded firms, both in the cross section and over time (Davis et al. 2006; Dinlersoz et al. 2018).

Despite these limitations, DEU markup measures are widely cited in the literature and policy circles and so are objects worth studying closely. For econometric purposes, we simply take DEU markups as given data and not as an estimated object.[14] Readers should interpret our empirical results in the context of both the limitations mentioned above and the importance these markup estimates have had in academic, policy, and media discussion.

3.3 KLEMS Industry Data

In addition to the popular DEU markup measure, we use a variety of separate measures that rely on the BEA-BLS Integrated Industry-level Production Accounts. This is also known as the "KLEMS" dataset—the acronym stands for inputs of capital (K), labor (L),

12. See Flynn, Gandhi, and Traina (2019), Kirov, Mengano, and Traina (2023), Bond et al. (2021), Doraszelski and Jaumandreu (2019, 2021), De Loecker (2021), and De Ridder, Grassi, and Morzenti (2024)

13. Firms in Compustat are labeled with detailed NAICS industry codes of varying NAICS vintage; we adjust these codes to match the NAICS 2017 vintage as described in Albrecht and Decker (2024). We drop industries that cannot be easily mapped to NAICS 2017 format (these include NAICS 233, 234, 235, 421, 422, and 513). We also note that some Compustat industries have few or no firms in some or all years and therefore fall out of some calculations (these include NAICS 55—which we omit from all calculations for more general conceptual reasons—as well as NAICS 113 and 485).

14. This also means that we do not address the concern of generated regressors (Murphy and Topel 1985; Oxley and McAleer 1993), so our standard errors are likely understated.

energy (E), materials (M), and services (S).[15] The data are annual beginning in 1988 (defined on BDS basis as described above), and we use the version covering roughly 60 distinct non-overlapping industries. KLEMS industry codes map to standard NAICS industries at varying levels of detail; for example, within the "2-digit" NAICS manufacturing sector (NAICS 31-33) there are nearly 20 KLEMS industries, while the 2-digit NAICS educational services sector (NAICS 61) is a single KLEMS industry.[16] We are not concerned about this variation in the level of industry detail across sectors (which is also a feature of the standard NAICS industry taxonomy) as most of our results are constructed on an activity-weighted basis (i.e., employment or sales weighted). KLEMS industries are defined such that data formatted with standard NAICS industry codes can be matched to KLEMS codes through appropriate aggregation; this is convenient given that the BDS and Compustat are both on a NAICS basis. We use KLEMS data from 1988-2019 to avoid any changes related to the pandemic. This timeframe is slightly different from our Compustat coverage, starting later and extending a bit more recently.

The KLEMS data offer several key advantages compared to data used in earlier markup measurement work. First, the data maintain rigorous adherence to proper measurement of output and other variables (resulting from their origin in the national accounts) using appropriate methodologies to track levels and growth.[17] Second, they employ uniform and modern NAICS industry definitions throughout the sample period, and the level of available industry detail does not present concerns about sparsely populated cells as in

15. The BLS and BEA provide extensive technical documentation of the data at https://www.bls.gov/productivity/articles-and-research/bea-bls-integrated-production-accounts.htm

16. As mentioned above, we omit NAICS 521 (monetary authorities) from all exercises. The KLEMS industry setup combines NAICS 521 with NAICS 522 (credit remediation and related activities) into the KLEMS industry 521CI, which we omit from all KLEMS exercises. Similarly, we omit NAICS 533 (lessors of nonfinancial intangible assets) from all exercises. The KLEMS industry setup combines NAICS 533 with NAICS 532 (rental and leasing services) into the KLEMS industry 532RL, which we omit from all KLEMS exercises.

17. DEU markups can be corrected to match more features of the national accounts. See Hasenzagl and Pérez (2023).

Compustat data. Third, they provide a comprehensive breakdown of inputs into five categories: capital, labor, energy, materials, and services.

These measurement advantages make the KLEMS data particularly well-suited for examining markups, despite being aggregated to the industry level rather than providing firm-level variation. The tradeoff is between more granular but less comprehensive data from individual firms (and associated firm-level production function estimation) versus broader but more carefully measured industry aggregates. Given our focus on industry-level relationships, the measurement advantages of KLEMS are worth considering as an alternative to DEU firm-level markup measures.

KLEMS Inverse Cost-Share Markups

We construct several KLEMS-based markup measures. Our first approach exploits input cost shares to infer markups based on the ratio of (nominal) revenue to (nominal) expenditures on a specific input category.

$$\text{Inverse Cost-Share Markup} = \frac{\text{Industry Revenue}}{\text{Industry Cost of Variable Input}}.$$

The variable inputs we consider are labor, materials, energy, and total "variable" costs (which we define as the sum of labor, materials, energy, and services).

The cost share approach makes two key assumptions: first, that first-order cost minimization conditions hold on average for all inputs; second, that returns to scale are constant. While these are strong assumptions, the cost share approach has advantages over alternatives—notably not requiring output quantity data or imposing more restrictions, such as Hicks-neutral productivity. Intuitively, if firms have market power, their revenue shares will be lower than their cost shares, with the ratio providing a measure of the markup. When there is no market power, cost and revenue shares are equal.

Notably, when expressed as log differences over time—as we do in most of our main exercises—these inverse cost shares are equivalent to the DEU markup if output elasticities are constant.

KLEMS Hall Markups

Using KLEMS data and following Hall (2018), we specify an industry-level regression that allows for a time trend in industry-specific markups:

$$\sum_i \alpha_{i,t} \Delta x_{i,t} = (\phi - \psi t) \Delta \log y_t - a_t, \tag{5}$$

where $\alpha_{i,t}$ is the input share for factor i, $x_{i,t}$ is a (real) factor input quantity, y_t is real industry output, t is time, and we omit industry index subscripts for simplicity.[18] The term on the left-hand side sums across all inputs and can therefore be easily obtained as the difference between total output and the Solow total factor productivity residual, which are both available in KLEMS.

Hall (2018) shows the implied functional form for the ratio of price to marginal cost is:

$$\mu_t = \frac{1}{\phi - \psi t}. \tag{6}$$

Critically for our purposes, the term ψ summarizes the change in industry-level markups over time; if $\psi > 0$ then markups increase over time. Rather than study the full markup formula (6), we simply focus on ψ to measure the direction and magnitude of industry-level markup growth.

Of course, a simple estimate of the regression in equation 5 suffers from endogeneity between inputs and output. Hall (2018) adopts an instrumental variables approach, using

18. The regression we describe in equation 5 matches equation (19) from Hall (2018).

as instruments military purchases of (separately) equipment, ships, and software; military expenditures on research and development; and the West Texas Intermediate crude oil price (annual average). We follow this estimation approach at the level of KLEMS industries. In exercises described below, we report results for both the version estimated with instrumental variables and the simple OLS version. Since these markup change estimates emerge from linear regression, they are useful for studying long-run changes in markups, but we cannot use them in annual time series analysis.

4 Long-Run Industry Trends

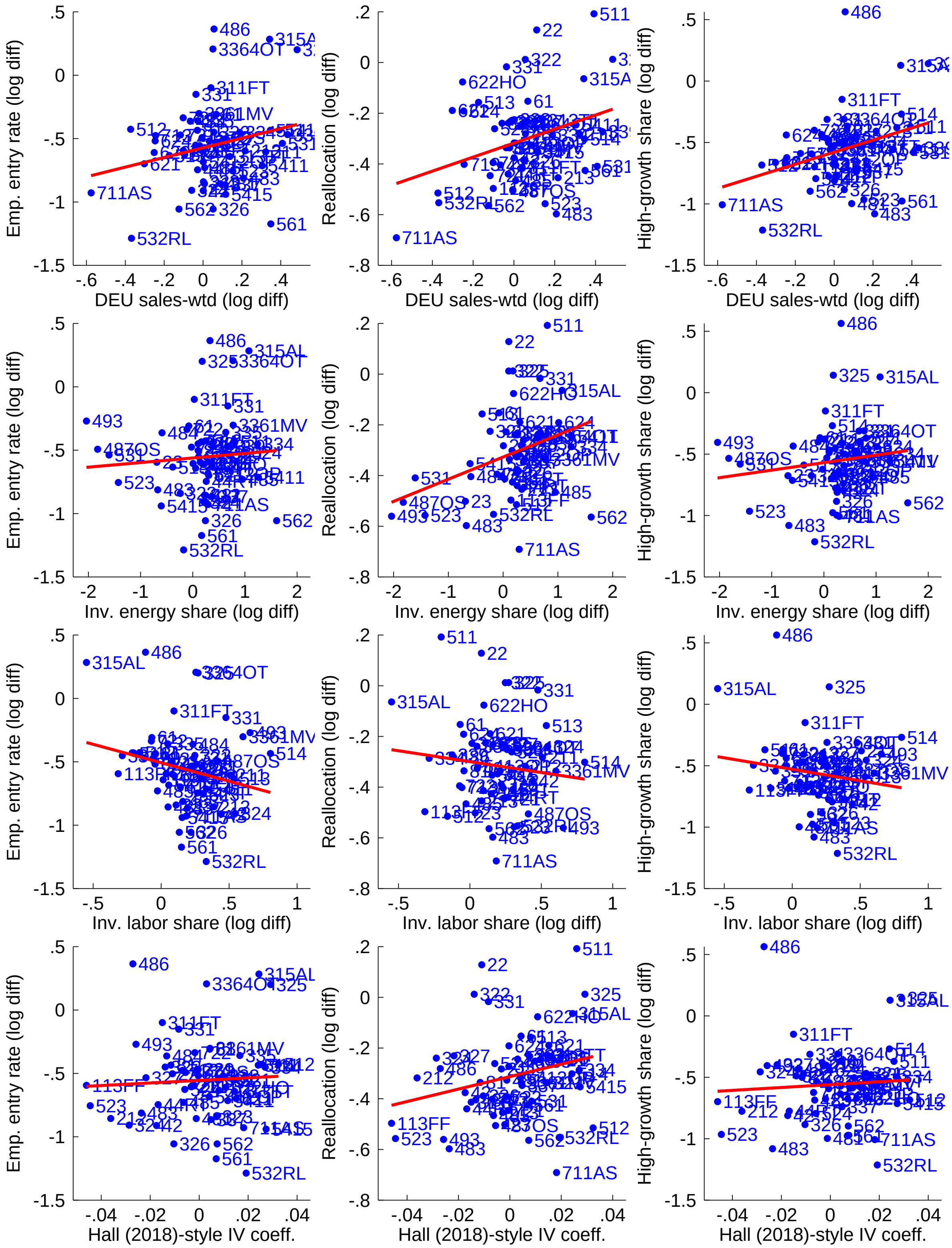
Figure 1 showed opposite movements between aggregate measures of business dynamics (entry and excess job reallocation) and markups, with the strong rise in markups from 1980-2017 matched by declines in entry and reallocation. It is this time series pattern that has led to research exploring a potential relationship between trends in dynamism and market power, a relationship that can be easily generated by standard theory (De Loecker, Eeckhout, and Mongey 2022). For example, declining competition resulting from fewer potential entrants results in lower entry and reallocation alongside higher average markups. If this kind of mechanism is an important explanatory factor for declining dynamism and rising markups from the 1980s through the 2010s, we should expect industries with larger increases in markups to also exhibit larger declines in dynamism over this period. In this section we present our primary empirical tests of this prediction using cross-sectional industry-level data.

4.1 Plotting Long-Run Industry Trends

Figure 3 shows the reduced-form relationship between our three main business dynamism measures and four different markup specifications within detailed industries. For this figure, industries are defined on a KLEMS basis, the most detailed industry taxonomy available for all of our markup measures. Since, in this section, we are interested in long-run trends, each variable is expressed as a log difference between the 2015-2019 average (or 2015-2017 for DEU markups) and the 1988-1992 average (the earliest period available for KLEMS data). The exception is the Hall (2018)-style markup, which is simply measured as the estimated slope coefficient as described above. In the figure, each row of panels corresponds to a different markup measure—here we show the DEU sales-weighted markup, the inverse energy cost share, the inverse labor cost share, and the Hall (2018)-style markup coefficient estimated using instrumental variables. The columns correspond to our main three dynamism measures—the employment entry rate, excess job reallocation, and the high-growth firm share of employment.

Rather than examining whether markup levels in an early time period predict dynamism levels in a later period, this exercise shows how the accumulation of markup changes relates to the cumulative change in dynamism measures. This approach captures the possible accumulation of both rising market power and declining dynamism. If underlying changes to industry structure are causing lower dynamism and higher market power, then industries that experienced larger increases in markups over these decades should show correspondingly larger declines in measures of dynamism.

We can focus on the top-left panel, which relates the change in the employment entry rate to the change in the DEU sales-weighted markup. In contrast to the theory in papers like Akcigit and Ates (2023), De Loecker, Eeckhout, and Mongey (2022), and De Ridder (2024), we observe a striking *positive* relationship between entry and markups. Going back to the broad sectors in figure 2, we can gain a sense of why the data do not support the standard theory. For example, some broad sectors with large declines in employment entry rates (shown on the top right panel of figure 2)—such as healthcare and social assistance services—actually saw declines in markups, despite the aggregate



Note: Difference, 2015-2019 average vs. 1988-1992 except the DEU markup (which uses the 2015-2017 average) and the Hall-style markup growth coefficent.
Source: Business Dynamics Statistics; Compustat; KLEMS.

Figure 3: Change in dynamism and markups, KLEMS industries

increasing markup pattern. The wholesale and retail trade sectors both saw very large declines in employment entry rates, as well as excess job reallocation and high-growth firm shares, but these sectors saw roughly no change in markups whatsoever. On the other hand, some sectors with large increases in markups—such as manufacturing and education services—saw relatively little decline in employment entry rates. Looking again at figure 3, with its greater industry detail, we can observe a similar pattern; many industries with large declines in dynamism saw declines in markups, while industries with large gains in markups often feature smaller declines (or even increases) in dynamism measures. And we observe a wide range of dynamism outcomes among those industries with little or no markup change. We observe similar patterns across all dynamism measures for three of the four markup measures shown on figure 3.

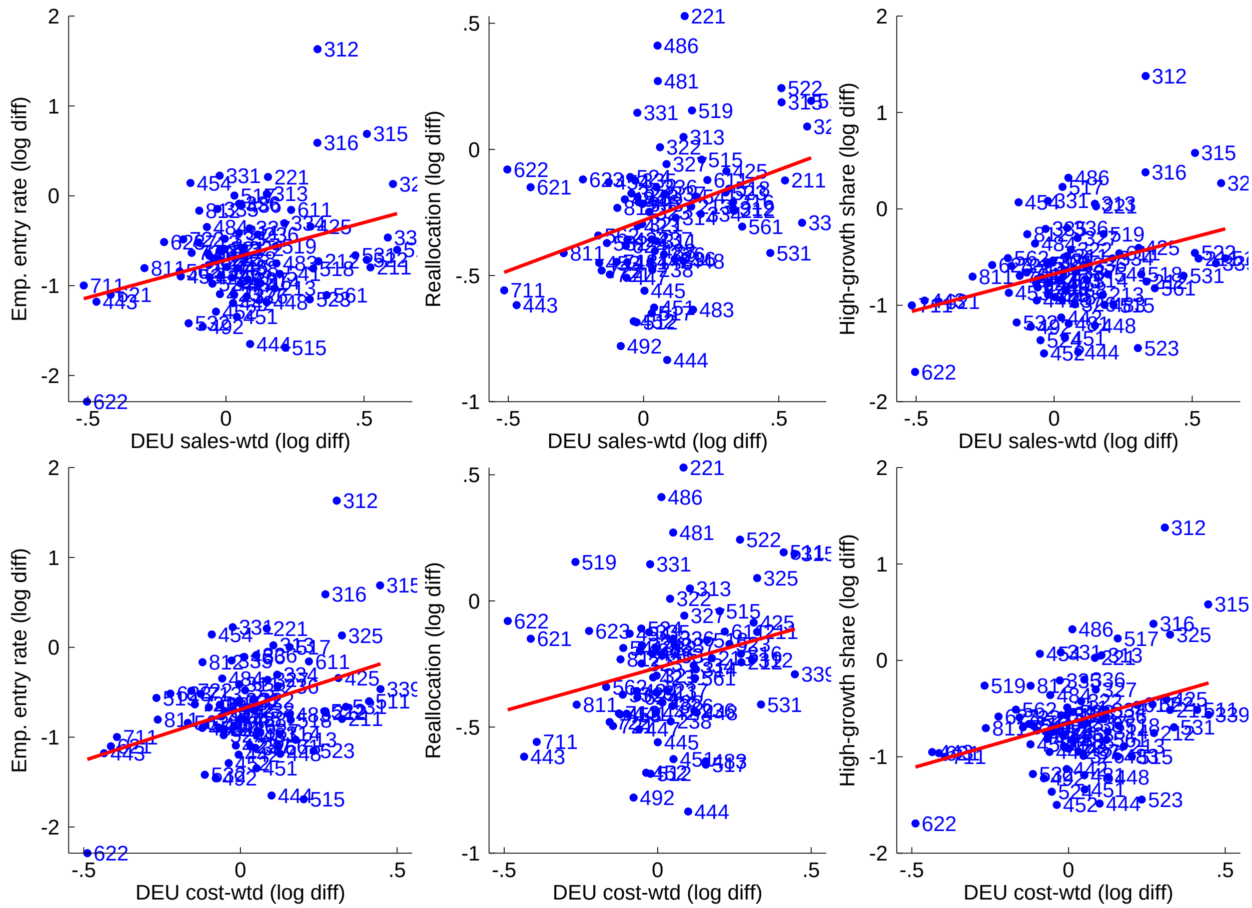
The noteworthy exception is the inverse labor share (the third row of the figure), which actually does appear to have a negative relationship with the dynamism measures.[19] We discuss this more below.

That said, none of the other markup measures exhibit this negative relationship with dynamism.[20] And more evidence that market power and dynamism are not negatively correlated, in general, can be seen on figure 4, which reports scatterplots (for the DEU markups only) at the 3-digit NAICS industry level; this affords somewhat more disaggregation and a longer time series (starting in 1980) than the KLEMS industry-based ap-

19. Additionally, an exception among the dynamism variables is the *unweighted* firm entry rate. Appendix figure A2 reports scatterplots showing the long (log) difference in unweighted entry rates against changes in all eight of our markup measures. Some of these show a modestly negative slope, though in unreported regressions using many different specifications only a few produce statistically significant negative relationships.

20. This can be further seen on appendix figure A1, which shows scatterplots for the remaining markup measures: the DEU cost-weighted markup, the inverse materials share, the inverse variable cost share, and the OLS version of the Hall (2018)-style markup coefficient. On this appendix figure, a flat or positive relationship is observed in each case, with the closest call being the variable cost.

proach of figure 3.[21] With this greater industry detail and longer time period, we observe consistent *positive* correlations throughout.



Note: Difference, 2013-2017 average vs. 1980-1984 average.
Source: Business Dynamics Statistics; Compustat.

Figure 4: Change in dynamism and markups, 3-digit NAICS industries

4.2 Limitations

Here we pause to emphasize two limitations of our empirical exercises. First, of course, we are not uncovering causal relationships between markups and dynamism measures.

21. After the industry omissions described above, the 3-digit NAICS scatterplots feature 74 industries. The KLEMS scatterplots feature 56 industries.

Both markups and dynamism are endogenous to various other economic forces—including, for example, the mass of *potential* entrants as in De Loecker, Eeckhout, and Mongey (2022)—and are ultimately jointly determined. Our scatterplots and regression results are simply reduced-form moments that are naturally implied by rich models of firm dynamics. In a sense we are providing reduced-form empirical tests of such models using cross-sectional variation. If, indeed, the same underlying causal mechanisms are driving higher market power and lower business dynamism, we should observe a negative reduced-form correlation between changes in markups and changes in dynamism at the industry level.

Second, our markup measures are subject to a range of measurement limitations. And some of them (the DEU markups and the Hall-style coefficients) are econometrically estimated objects with their own sampling variation; considerations for "imputed regressors" described by Murphy and Topel (1985) likely apply to these markups. We will next turn to formal regressions, which we will estimate with commonly used "robust" standard errors, but we acknowledge that we may be underestimating the standard errors given the nature of the markup measures. For these reasons, we study this issue with a large number of empirical specifications, including not only multiple "markup" measures but many different econometric setups, as we will show below.

4.3 Baseline Regression Specifications

In table 1, we present baseline regression results examining the relationship between changes in markups and dynamism measures across industries. The table reports coefficients from separate regressions of long differences in dynamism measures on long differences in various markup measures at the KLEMS industry level; these correspond closely with the regression lines displayed on figure 3, with a key exception: in these regressions we systematically omit outliers, that is, industries whose respective markup change is in the top or bottom 2 percent of industries (we relax this constraint for robustness discussions further below). Slope coefficients from both unweighted (panel A) and employment-weighted (panel B) regression specifications are shown.

Panel A, reporting unweighted regressions, is consistent with the results of figure 3 already shown. Slopes are generally positive, except for the inverse labor share markup measure. In the table we can also observe that most slopes are not statistically significant, with the main exception pertaining to the inverse energy share which appears to have statistically significantly positive relationships with dynamism variables. The negative slopes on the inverse labor share are not statistically significant.

Importantly, though, our motivations for this paper—summarized by figure 1—are the aggregate patterns. The regression results of panel A could simply reflect small industries that are not important for aggregate economic activity. Panel B therefore reports employment-weighted regressions, such that industries with larger employment shares have larger influence on regression coefficients.[22] This adjustment clearly matters: coefficients are generally lower in panel B than in panel A, with some of panel A's positive coefficients even flipping to negative. We observe a bit less statistical significance for positive coefficients than in the unweighted regressions, and we now observe statistical significance for two of the three negative coefficients on the inverse labor share markup—especially in the case of high-growth firm shares. On balance, the weighted regressions still cast considerable doubt on theories linking market power and dynamism, but the labor share exception is noteworthy.

22. Weighted regressions also eliminate concerns about arbitrary industry definitions or levels of detail. For example, if an industry taxonomy features greater detail in, say, manufacturing than in services, then manufacturing industries will unduly influence unweighted coefficients in industry-level regressions. Weighted regressions alleviate this concern by ensuring that small industries—which may be "small" simply because they are targeted by narrower industry definitions—have appropriately small influence on regression coefficients.

Table 1: Long run markups vs. dynamism: Baseline specification

	Employment Entry Rate	Reallocation	High-Growth Share
A. Unweighted regressions			
DEU sales-weighted markup	0.20	0.16	0.33
	(0.24)	(0.14)	(20)
Observations	53	53	53
Inverse energy share	0.12^{*}	0.11^{***}	0.12^{**}
	(0.06)	(0.03)	(0.05)
Observations	54	54	54
Inverse labor share	-0.19	-0.05	-0.13
	(0.18)	(0.12)	(0.17)
Observations	54	54	54
Hall-style IV markup	0.82	2.85^{*}	1.26
	(3.39)	(1.54)	(3.43)
Observations	54	54	54
B. Employment-weighted regressions			
DEU sales-weighted markup	-0.37	-0.17	-0.14
	(0.39)	(0.16)	(0.28)
Observations	53	53	53
Inverse energy share	0.01	0.11^{***}	0.03
	(0.05)	(0.03)	(0.04)
Observations	54	54	54
Inverse labor share	-0.47^{**}	-0.13	-0.41^{***}
	(0.20)	(0.12)	(0.15)
Observations	54	54	54
Hall-style IV markup	1.29	5.54^{***}	2.90
	(3.47)	(1.48)	(2.69)
Observations	54	54	54

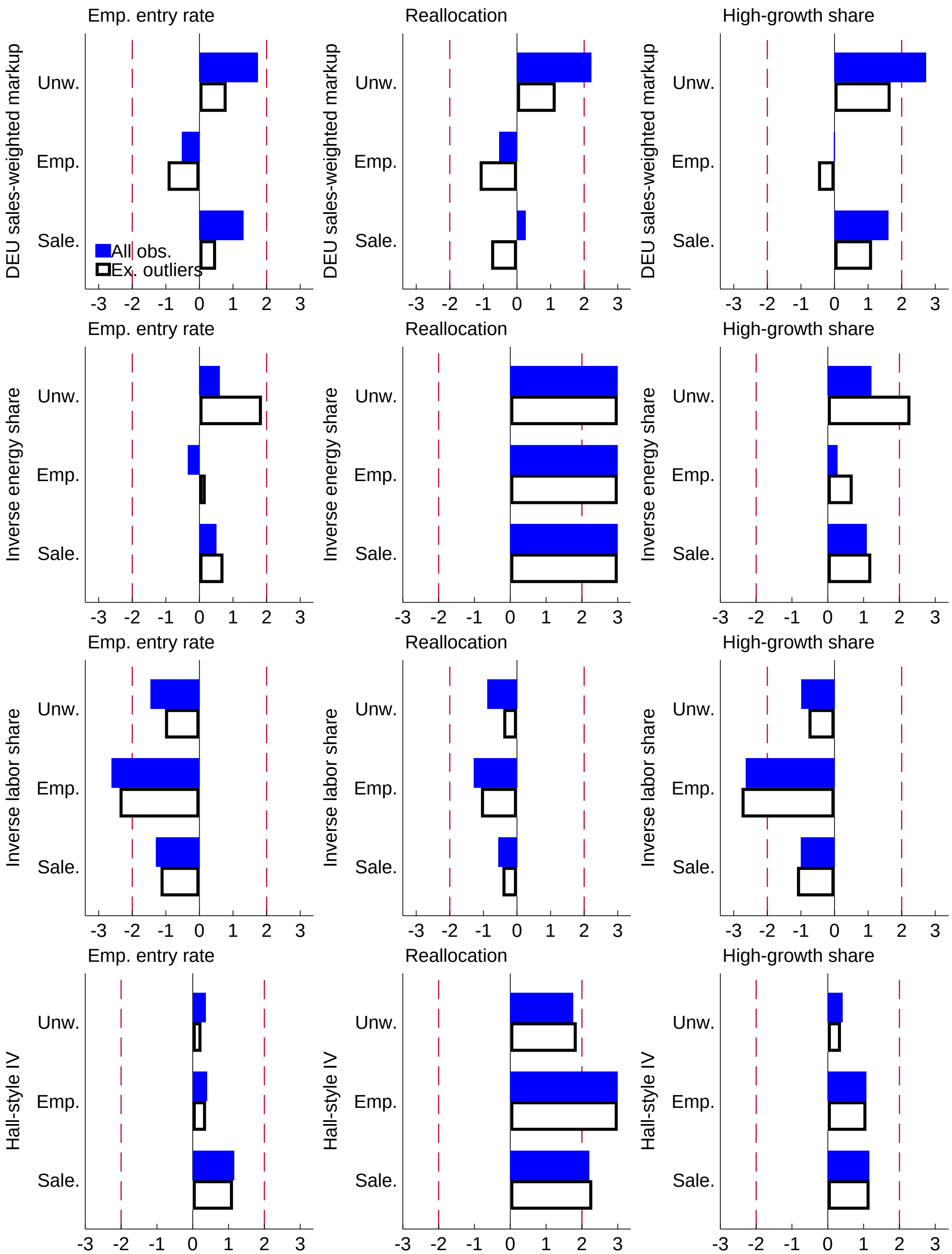
Note: SE in parentheses; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$
Long differences at KLEMS industry level. Period: 2015-2019 vs 1988-1992 (DEU: 2015-2017).
Weighted regressions use avg. employment 2015-2019 (DEU: 2015-2017) and 1988-1992.
Outlier markup changes omitted.

4.4 Alternative Regression Specifications

These baseline results are robust to numerous alternative specifications. We estimate these regressions on KLEMS industries with and without outliers and with three difference regression weighting schemes (unweighted, employment weighted, and real sales weighted).[23] The results are summarized on figure 5, which corresponds to the markup variables shown on table 1 (DEU sales-weighted markups, the inverse energy share, the inverse labor share, and the Hall-style IV coefficients). In this figure, we report t statistics from a number of regressions, allowing for quick analysis of both the direction and the statistical significance of each regression's estimated markup/dynamism relationship. Each panel of the figure corresponds to a single dynamism variable and a single markup variable and reports three sets of bars corresponding to unweighted regressions ("Unw."), employment-weighted regressions ("Emp."), and real sales-weighted regressions ("Sale."). Within each weighting scheme, we report t statistics from regressions using all observations (solid bars, "All obs.") and regressions in which 2 percent outliers are excluded (hollow bars, "Ex. outliers"). We include vertical dashed lines indicating t statistics of -2 and 2, a rule of thumb for statistical significance.

To understand figure 5, start on the top left panel. The first hollow bar (corresponding with the unweighted "Unw." row of the panel) shows the t statistic from an unweighted regression of the (log differenced) employment entry rate on the (log differenced) DEU sales-weighted markup, with outliers excluded. This bar corresponds exactly to the first regression coefficient reported on table 1. This top left panel generally suggests a lack of statistical significance of any of the regressions of employment entry rates on DEU sales-weighted markups, as all the bars are well within the bounds of the dashed lines indicating t statistics of -2 and 2. The inclusion or exclusion of outliers matters in some re-

23. We deflate industry nominal sales data using the U.S. GDP deflator. This deflation does not matter for these exercises.



Note: Each panel shows t statistics from unweighted, employment-weighted, and sales-weighted regressions (described in text). t statistics truncated below -3 and above 3. KLEMS industry-level regressions.
Source: Business Dynamics Statistics; Compustat; KLEMS.

Figure 5: Long-run markup vs. dynamism coefficients t statistics, KLEMS industries

gressions but not others; but overall, outliers are not relevant to the question of statistical significance.

Looking across the panels of figure 5 generally, we observe that, regardless of regression weights and outlier inclusion, results are usually statistically indistinguishable from zero.[24] The most glaring exception comes from the middle panel of the second row, corresponding to the regressions of reallocation rates on the inverse energy share, all of which feature large, highly significant, positive t statistics. A couple exceptions can be seen on the third row, corresponding to the inverse labor share and discussed above, though in broader context these exceptions are not so striking or persuasive.

Figure 6 reports t statistics associated with 3-digit NAICS-level regressions. Again, these only permit use of the DEU markups, but they allow for slightly more industry detail and a longer time sample. In these regressions, all coefficients are positive, and several are statistically significant.

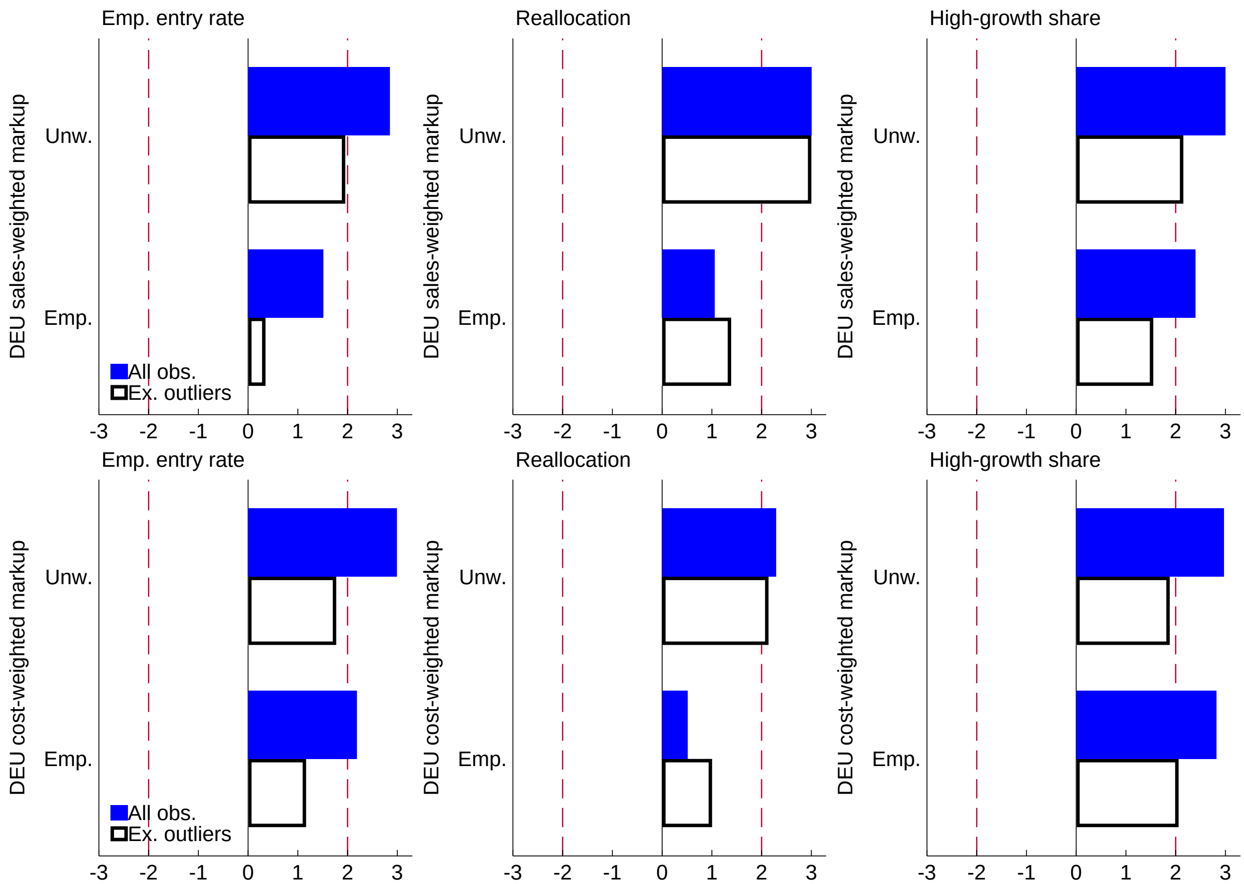
While figures 5 and 6 show little or no relationship between markups and dynamism across a wide range of regression and measurement specifications, these figures still do not fully capture the many specifications we have investigated. Other specifications include:

- All specifications in both the KLEMS industry dataset and the 3-digit NAICS industry dataset (instead of the KLEMS dataset), where only DEU markup variables are available but for a period starting in 1980.

- All specifications with the simple (unweighted) entry rate dynamism variable.

- All specifications expressed in long *level* differences (instead of log differences).

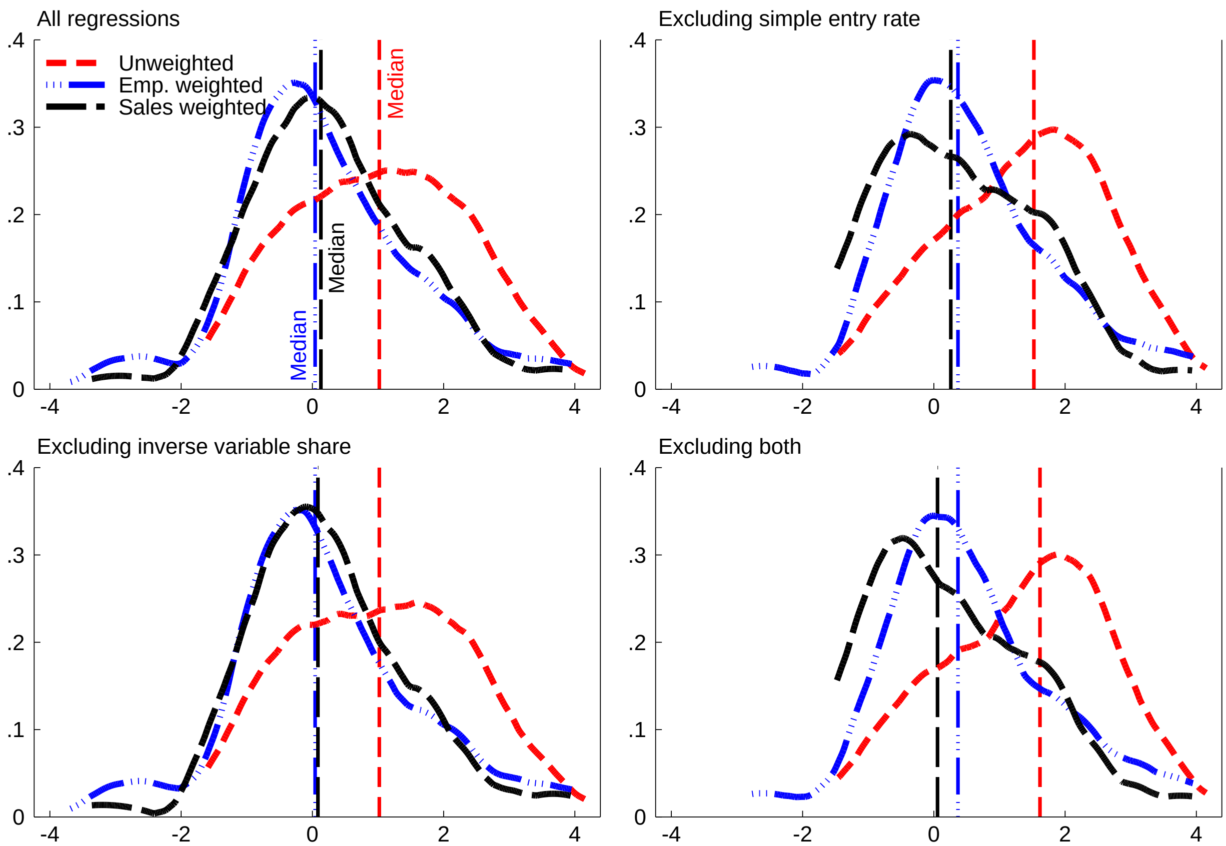
24. Appendix figure A3 is similar to figure 5 but corresponds to our remaining four markup variables in KLEMS data: DEU cost-weighted markups, the inverse materials share, the inverse variable cost share, and the OLS version of the Hall-style markup growth coefficient. The only statistically significant bars on figure A3 are in the positive direction, suggesting that industries with larger markup gains saw smaller dynamism declines.



Note: Each panel shows t statistics from unweighted, employment-weighted, and sales-weighted regressions (described in text).
t statistics truncated below -3 and above 3. 3-digit NAICS level regressions.
Source: Business Dynamics Statistics; Compustat; KLEMS.

Figure 6: Long-run markup vs. dynamism coefficients t statistics, 3-digit NAICS industries

Rather than report dozens more regressions one by one, we summarize *all* regression t statistics—including those we have already shown—using kernel densities, shown on figure 7.



Note: Kernel density of t statistics from long difference regressions in two datasets: KLEMS industries with all markup variables (384 regressions) and 3-digit NAICS industries with DEU markup variables (64 regressions). Separate densities shown for unweighted regressions, employment-weighted regressions, and sales-weighted regressions. Regression specifications include level differences and long differences; including and excluding outliers. All dynamism variables included. Vertical lines indicate median t statistic across regressions.
Source: Business Dynamics Statistics, Compustat, and KLEMS.

Figure 7: Kernel density of t statistics from long difference regressions

The top left panel of figure 7 reports kernel densities of t statistics, separately by regression weighting scheme, for all regression specifications (over 400 of them). Along with the standard kernel densities, we report medians of each distribution. Roughly speaking, the distribution of unweighted regression t statistics has a clear majority of its mass above 0 and a solid portion even above 2, the rule of thumb for statistical significance. Weighted regression distributions are shifted to the left of the unweighted one, centered almost exactly on zero. That is, among hundreds of regressions, the coefficients relating markups and dynamism have a central tendency very close to zero.

The top right panel of figure 7 excludes the simple (unweighted) entry rate and therefore is limited to the other three dynamism variables on which we have focused in the main text. This shifts the distributions a bit to the right, suggesting more prevalence of positive relationships between markups and dynamism. The bottom left panel reports all regressions excluding the inverse variable cost share, which we exclude due to the heavy roll of payroll in this markup concept (though this panel does include regressions using payroll specifically). Finally, the bottom right panel excludes both the simple entry rate regressions and the inverse variable cost share regressions.

Observe several additional patterns exhibited in figure 7. Notably, across all panels, there is almost no density below t statistics of -2. This reinforces our main finding that evidence for a relationship between rising markups and declining dynamism is extremely limited. A nontrivial amount of density appears above 2, but the bulk of the t-statistic distributions falls between -2 and 2, indicating that many of our estimated relationships are not statistically significant. If we focus in on the medians (shown by vertical lines), they differ across weighting schemes: employment-weighted and sales-weighted specifications tend to produce t-statistics centered close to zero compared to positive-median unweighted specifications. This suggests that the positive relationship we saw in some scatterplots is being driven by smaller industries; once industries are weighted, the median specification finds essentially no positive or negative correlation between markups and dynamism.

Wrapping up these "long difference" analyses of industry-level trends in markups and dynamism, across hundreds of regression specifications we find minimal evidence for the negative reduced-form relationship between dynamism and markups that is posited by some theories. We find nontrivial but limited evidence for positive relationships. But overall, the headline result is that these hundreds of regressions across many variables and specifications point to *no significant relationship* between dynamism and markups. That said, a reasonable counterargument is that we are studying too long a time period; over multiple decades, a range of macroeconomic or industry-level shocks could affect both dynamism and markups, possibly "washing out" a causal relationship linking the two. We now turn to "high-frequency" exercises, in which we will look for a simultaneous relationship between markups and dynamism within much shorter time windows.

4.5 The Special Case of the Inverse Labor Share

The negative relationship we find between the inverse labor share measure and dynamism metrics—particularly in the employment-weighted specifications—warrants further discussion, as it connects to a broader literature on the secular decline in labor's share of income. Recent work by Autor et al. (2020), Karabarbounis and Neiman (2014), and others documents this decline and explores various potential explanations including technological change, globalization, and changes in market structure.

While our inverse labor share results might appear to support theories linking market power to declining dynamism, we interpret that result cautiously for several reasons. First, the labor share can decline for many reasons unrelated to product market power, such as capital-biased technological change, increased automation, or changes in the relative prices or availability of capital and labor. Second, if declining labor shares primarily reflected rising product market power, we would expect to see similar negative relationships with our other markup measures, which we do not.[25] Third, the broad sectors driving the declining labor share in the broader literature and our data do not have clear

25. An important reminder is that, in these log long difference specifications, each of our markup variables—assuming they capture the revenue share of variable inputs—is equivalent to the more rigorous DEU markup concept under the assumption of constant production function elasticities.

systematic patterns in relation to declining dynamism; for example, while retail trade sees a large decline in the labor share and in some dynamism measures, manufacturing sees a large decline in the labor share with a relatively small decline in our main three dynamism measures. Thus, while our labor share results are intriguing and connect to important trends in factor shares, they do not appear likely to be driven by broader theories attempting to link product market power and business dynamism.

5 High-Frequency Analysis: Impulse Response Functions

To analyze the contemporaneous, high-frequency comovement of dynamism and markup variables, we employ local projections following Jordà (2005). This allows us to assess the short-to-medium run comovement predicted by theories linking market power and dynamism. Notably, we have annual variation in all of our dynamism variables and in all of our markup variables except for the Hall-style market coefficients.

The local projection method involves estimating a sequence of regressions at each forecast horizon h, where the outcome variable of interest is regressed on the contemporaneous shock and relevant controls. Specifically, for each h, we estimate the following equation:

$$y_{i,t+h} - y_{i,t-1} = \alpha_i + \tau_t + \sum_{l=0}^{L} \beta_l \Delta x_{i,t-l} + \sum_{l=1}^{L} \gamma_l \Delta y_{i,t-l} + \epsilon_{i,t+h}, \tag{7}$$

where $y_{i,t}$ is the dynamism variable of interest (e.g., employment entry rates, reallocation, or the high-growth share) for industry i in year t, and $x_{i,t}$ represents the markup variable (both y and x are expressed in logs). Industry fixed effects, α_i, and year fixed effects, τ_t, control for unobserved persistent heterogeneity across industries and macroeconomic shocks. The inclusion of lagged changes in x and y ensures that the regressions control for potential persistence in both shocks and outcomes. The coefficient β_h measures the cumulative response at horizon h, capturing both immediate and lagged effects of the shock.

Our local projection explicitly accommodates concurrent effects at $h = 0$, which allow for the immediate impact of shocks, in line with a theory where a shock moves markups and dynamism at the same time (De Loecker, Eeckhout, and Mongey 2022). For our main results, we set the maximum horizon H at 3, balancing our interest in observing as wide a window as practical against the short nature of our time series datasets; we later discuss results for $H = 4$. We set the maximum lag L at 3 but also discuss results for $L = 2$.[26] And our main results feature employment-weighted regressions, consistent with our interest in understanding drivers of aggregate patterns; we explore this specification choice below as well.

Importantly, while impluse response functions (IRFs) resulting from local projection specifications often take on a natural causal interpretation, in our setting we do not claim to be recovering exogenous shocks and causal responses. We employ the local projection methodology and IRFs simply to uncover high-frequency reduced-form comovement of markups and dynamism variables when controlling for persistent industry heterogeneity and aggregate temporal shocks. This allows us to isolate these comovements in a much narrower window than our long difference exercises above and to abstract from prominent sources of variation that could cloud the reduced-form relationship between markups and dynamism.

26. The rich lead and lag structure of local projections puts the dataset under considerable stress, since leads and lags reduce the effective number of annual observations that can be used. Since local projections are estimated using multiple separate regressions (one each for $h \in 0, 1, ..., H$), we carefully ensure that the exact same dataset is used for each regression within a local projection model (otherwise, the inclusion of lag and lead variables would change the effective beginning and endpoints of each regression dataset).

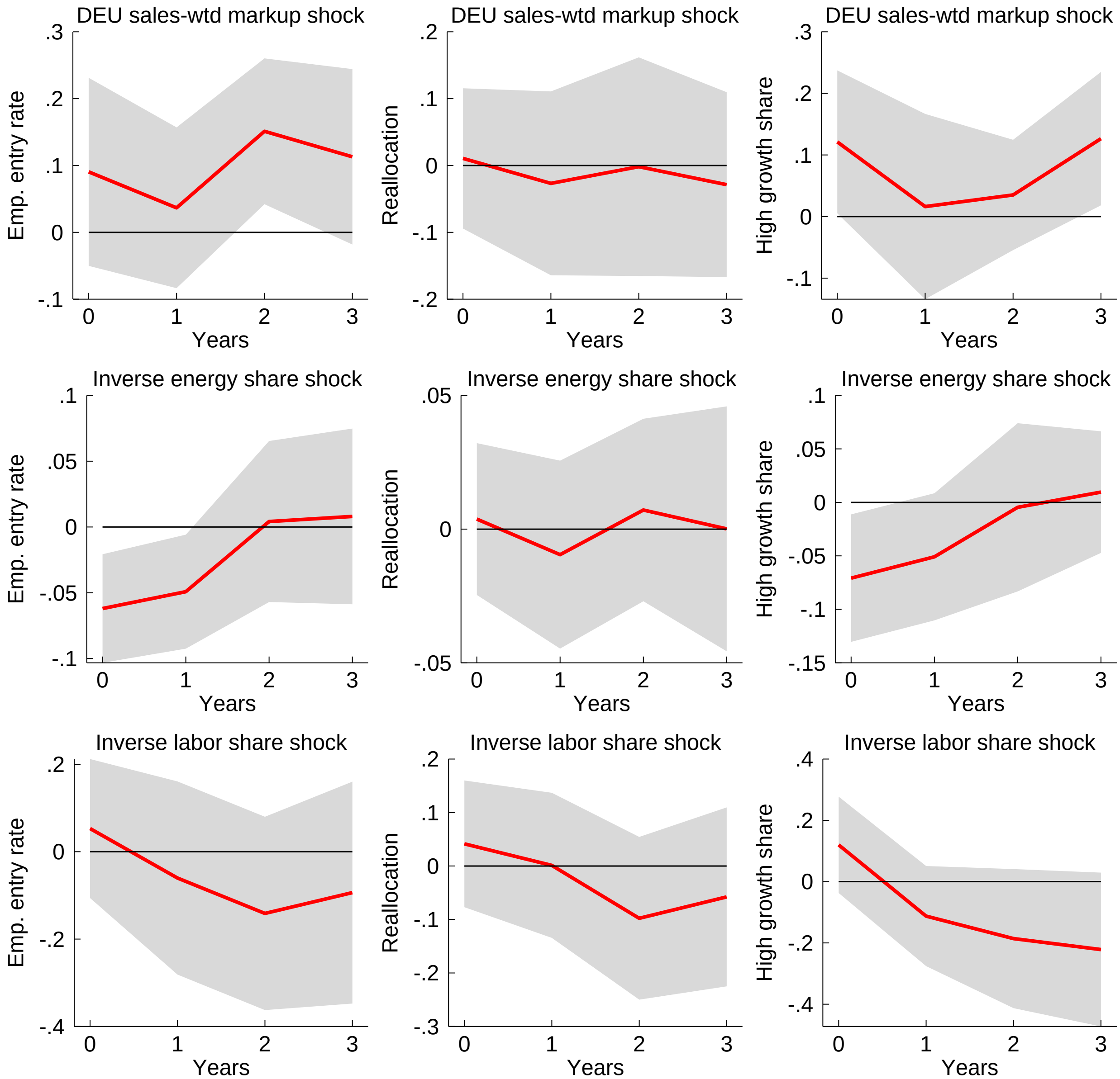
5.1 Baseline Impulse Response Specifications

The results of the local projections using the KLEMS industry-level data are presented in figure 8, which plots the cumulative IRFs of industry-level dynamism measures to markup "shocks". The panels show responses of employment entry rates (first column), reallocation (second column), and the high-growth share (third column) to three markup shocks: DEU sales-weighted markup shocks (first row), inverse energy share shocks (second row), and inverse labor share shocks (third row).

For DEU markup shocks (top row), we observe some variation in responses over time. Employment entry rates and high-growth firm shares both rise immediately in response to a contemporaneous increase in DEU markups, though with only marginal statistical significance. Reallocation does not respond at all. Inverse energy share shocks (second row) are associated with contemporaneous, statistically significant drops in entry rates and high-growth firm shares, though these responses are quite short-lived (consistent with our long difference results, with find an eventual positive, and often statistically significant, relationship between dynamism and inverse energy shares). Inverse labor share shocks (third row) prompt no significant response of employment entry rates and reallocation, though a borderline statistically significant negative response of high-growth firm shares is detectable.[27]

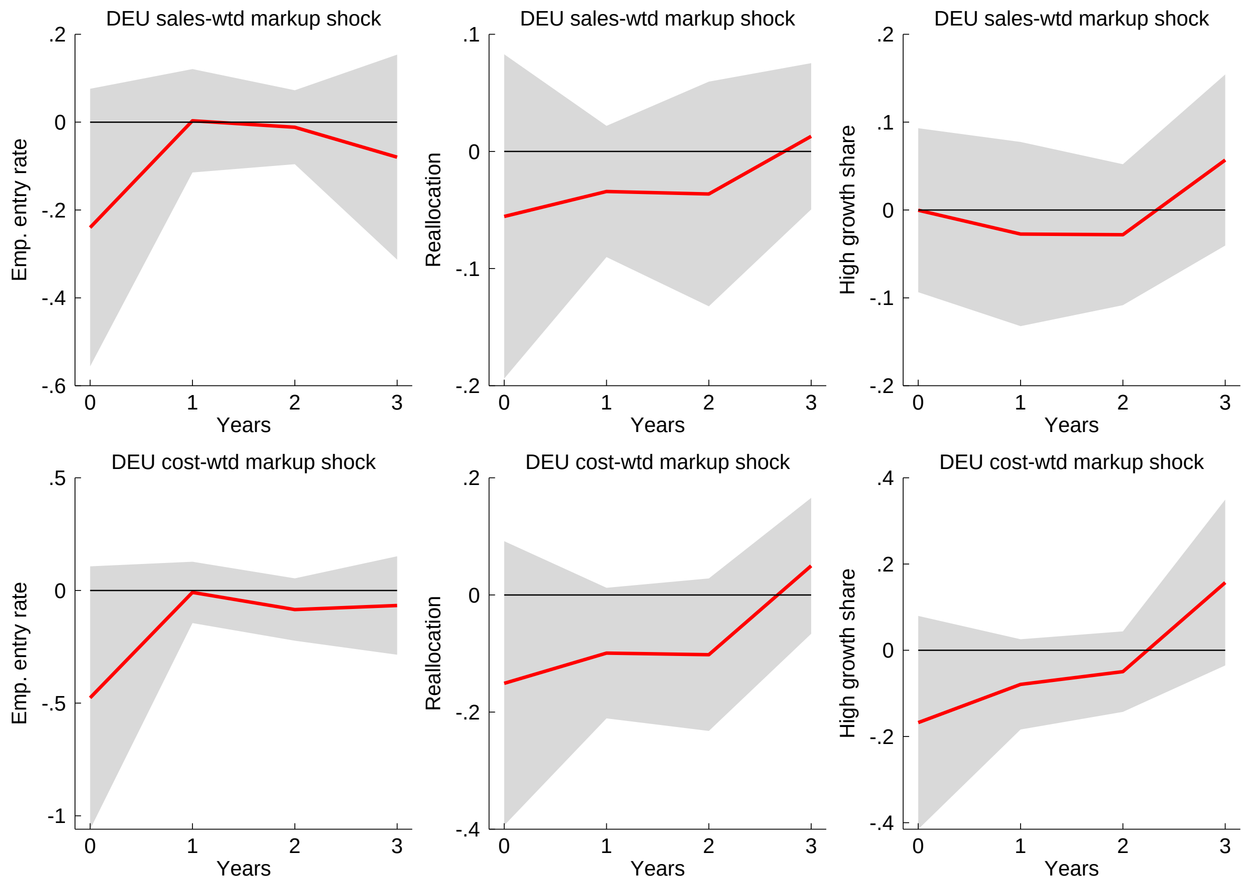
While figure 8 focuses on the KLEMS industry dataset, we can exploit a longer time series and somewhat finer industry detail in our 3-digit NAICS dataset, where only the DEU markups are available. IRFs from this dataset are shown on figure 9. None of the relationships shown there are statistically significant.

27. In the appendix, figure A4 presents a similar variety of results using alternative markup measures: DEU cost-weighted markups, inverse material share shocks, and inverse variable cost share shocks. With these alternative measures, we still observe largely non-statistically significant results, with some modest exceptions. We also estimate these IRFs for the simple (unweighted) entry rate, reported on appendix figure A5. The entry rate response to markups is typically positive though not typically statistically significant.



Note: Cumulative impulse response of dynamism variable to markup variable (both in log differences); employment-weighted regressions with industry and year fixed effects and 3 lags. DEU: 1988-2017; others: 1988-2019. KLEMS industries
Source: Business Dynamics Statistics; Compustat; KLEMS.

Figure 8: Cumulative impulse responses of dynamism measures to DEU markup and cost share shocks, KLEMS industries



Note: Cumulative impulse response of dynamism variable to markup variable (both in log differences); employment-weighted regressions with industry and year fixed effects and 3 lags. 1980-2017. 3-digit NAICS industries.
Source: Business Dynamics Statistics; Compustat.

Figure 9: Cumulative impulse responses of dynamism measures to DEU markup shocks, 3-digit NAICS industries

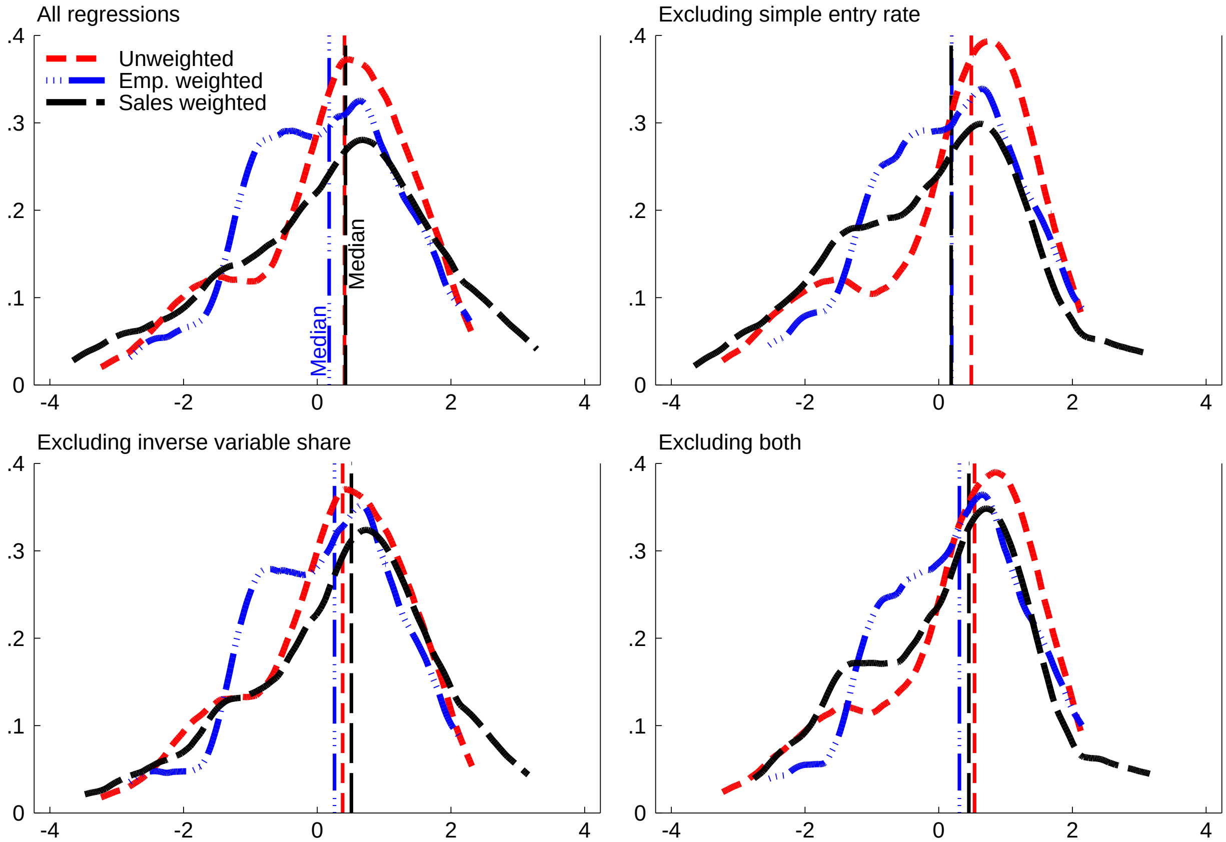
On balance, the IRFs across the two datasets point to statistically minor high-frequency responses of dynamism variables to markups, with little evidence of negative relationships aside from the inverse labor share—which we discussed at length above. In short, we do not observe compelling, widespread negative relationships as posited by theory.

5.2 Alternative Specifications

As with our long difference regressions, though, we estimate these IRFs under a number of alternative specifications beyond those described above. These include:

- With and without year fixed effects.
- Unweighted and real sales-weighted regressions in addition to the employment-weighted regressions of the main results.
- Without allowing contemporaneous effects of markups on dynamism measures (i.e., require the first effect to be lagged one year).
- Extending the maximum lead horizon H to 4 (allowing for a more comprehensive time window of observation).
- Shortening the maximum lag L to 2 (allowing for a slightly longer available dataset coverage period).

Once again we use kernel densities to display the range of outcomes from these many specifications; we focus on the t statistic associated with the IRF coefficient timed at the maximum H horizon ($H = 3$ in our main exercises). That is, we study the range of possibilities for the end-of-window cumulative "effect" of markups on dynamism. Figure 10 reports kernel densities in the same format as figure 7 described above. The top left panel shows the t statistic distributions across all specifications under the three different regression weighting schemes.



Note: Kernel density of t statistics for cumulative response at end of 3 years in two datasets: KLEMS industries with all markup variables (576 estimated models) and 3-digit NAICS industries with DEU markup variables (128 models). Separate densities shown for unweighted regressions, employment-weighted regressions, and sales-weighted regressions. All dynamism variables included. Includes models with and without year effects. Includes models with simultaneous response to markup shocks and one-year lagged response. Vertical lines indicate median t statistic across regressions.
Source: Business Dynamics Statistics, Compustat, and KLEMS.

Figure 10: Kernel density of t statistics from cumulative impulse responses of dynamism measures to markup shocks (horizon H = 3)

The medians of all distributions are slightly positive but close to zero, indicating that the cumulative response of dynamism shocks after 3 years tends to be close to zero. Appendix figure A6 reports the same kind of kernel densities but for the longer cumulative time horizon, $H = 4$. The longer time horizon does result in distributions shifted slightly to the left—with a couple median lines now below zero. But the general message is the same: across hundreds of specifications, the cumulative response of dynamism measures to markup shocks tends around zero, with a nontrivial distribution that is largely between the rule-of-thumb t statistic values of -2 and 2.[28]

These annual-frequency results largely corroborate our long-run analyses: there is little or no robust, systematic relationship between industry-level markups and dynamism.

Before concluding, we make a short digression: As noted in numerous places above, theories linking dynamism with markups need not feature direct causality from markups to dynamism. A third factor could cause both dynamism to fall and markups to rise. The example from De Loecker, Eeckhout, and Mongey (2022) is a decline in the number of potential entrants, which both reduces entry and, in a Cournot setting, raises markups due to fewer competitors. While we cannot observe the number of potential entrants, we can observe firm counts by industry. In unreported results, we estimate impulse response functions of markup measures to firm count shocks. Results are decidedly mixed and heavily dependent on specification, with cumulative effects that are sometimes positive, sometimes negative, and typically not statistically significant—though a few results are marginally so. When firm counts are normalized by employment (i.e., firms per employee, the inverse of average firm size), DEU markups see a positive and nontrivially statistically significant response; that is, DEU markups rise when firms get smaller. This

28. In unreported results, we also run all specifications under a regression setup in which the right-hand-side variables of equation 7 (the markup and the lags of the dynamism variable) are specified in (log) levels instead of (log) differences. This alternative approach results in broadly similar kernel densities, still centered around zero with most of the mass within the range of $(-2, 2)$.

latter result is not observed in KLEMS-based markups. A more thorough investigation of this topic is beyond the scope of this paper but an interesting avenue for further research.

6 Conclusion

The U.S. economy has experienced several noteworthy trends over the past few decades: productivity growth has slowed, profit shares have increased, the labor share has fallen, and the high pace of business and labor market dynamics commonly associated with the U.S. economy has declined. Many researchers and policymakers have commented on relationships between these various trends. This paper adds to the empirical literature by studying two trends in particular: rising markups and declining business dynamism. Instead of focusing on the time-series evolution of aggregate average markups and aggregate dynamism, which could comove for any number of reasons (or for spurious reasons), we exploit industry-level variation.

Our analysis, employing multiple measures of markups derived from both Compustat data on publicly traded firms and industry-level data from the BEA-BLS Integrated Industry-level Production Accounts (KLEMS), reveals a notable disconnect between theoretical predictions and empirical evidence at the industry level. The prevailing theory suggests that increased market power, manifested in higher markups, should correlate with reduced firm responsiveness and cause (or be associated with) lower business entry, thereby dampening business dynamism. However, across various specifications and markup measures, including sales-weighted and cost-weighted estimates from De Loecker, Eeckhout, and Unger (2020) and cost-share and Hall (2018)-style instrumental variable estimates from KLEMS data, we find almost no support for this negative relationship across industries between the 1980s and 2010s. In fact, several specifications relating long-run trends in these variables suggest a positive correlation, indicating that industries experiencing larger increases in measured markups often saw smaller declines, or even increases, in various measures of business dynamism. Our high-frequency analysis using local projections further reinforces these findings. And we show clearly that our results are not artifacts of certain measurement or econometric specifications; they are apparent in a large number of alternative empirical approaches.

This finding highlights the danger of explaining comovements of aggregate time series using even well-designed theories. To oversimplify a bit, any theory linking long-run trends in dynamism and market power must explain why the two trends appear to be occurring in separate industries. Our results also highlight challenges in empirically linking aggregate trends with industry-level dynamics and underscores the difficulties of measuring market power. While the DEU markup measure, based on Compustat data, has been influential in shaping discussions around rising markups, its limitations-—such as the focus on publicly traded firms and the reliance on cost of goods sold as a proxy for variable costs——may warrant caution when interpreting its relationship with economy-wide dynamism. Similarly, while the KLEMS data offer comprehensive industry coverage and rigorous output measurement, the aggregated nature of the data may mask important within-industry heterogeneity and lack the comfortable structural underpinnings of firm-level production function-based estimates.[29]

We do find hints of an interesting relationship between the declining U.S. labor share and declining business dynamism. A negative relationship between the inverse labor share and dynamism appears in both our long-run specifications and our annual-frequency analyses, though in both cases the statistical significance is marginal. While we do not interpret this result as showing the clear negative relationship between product market

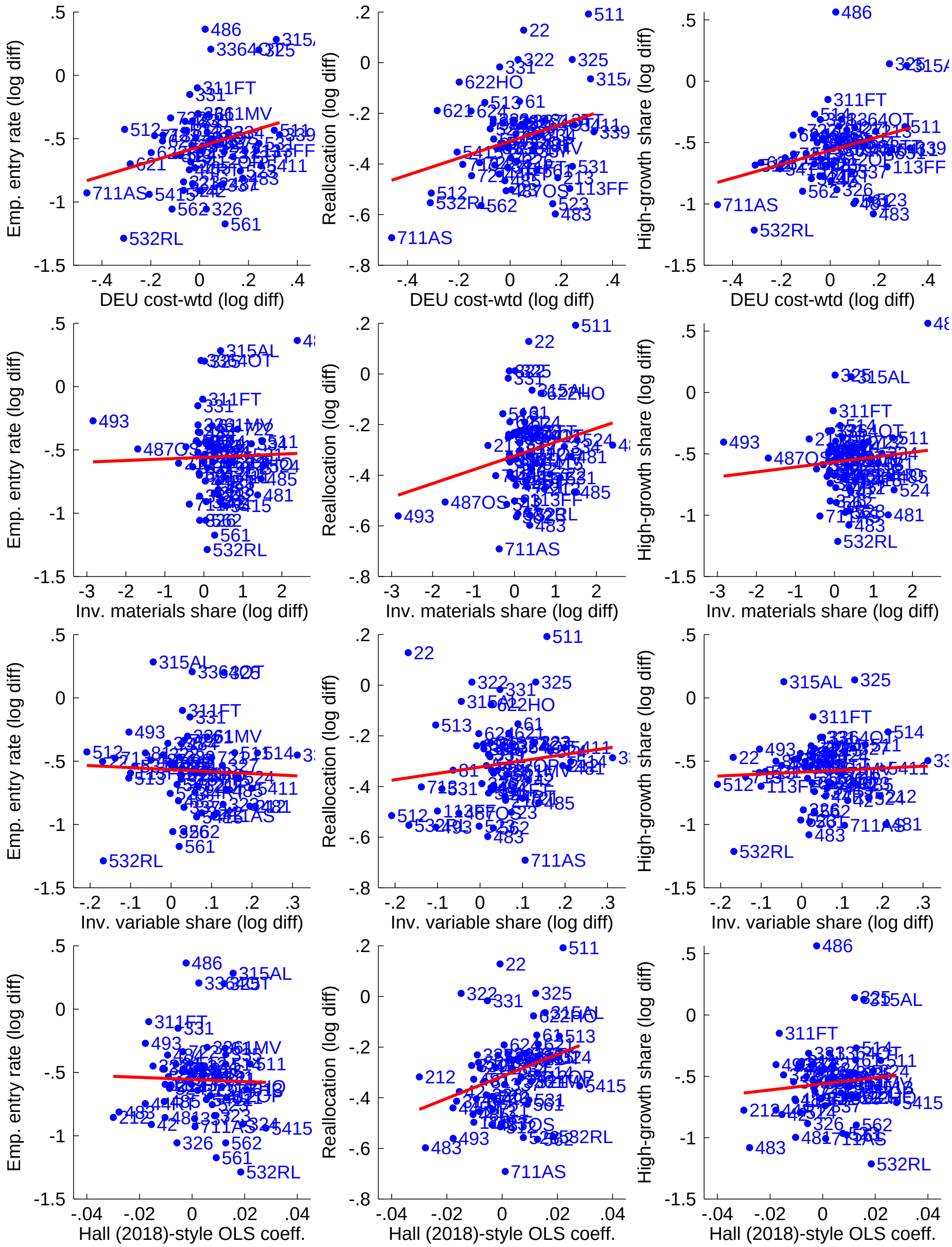
29. Additionally, the validity of instruments used in Hall-style estimations remains a subject for careful consideration.

power and dynamism that has been posited in other literature, the labor share/dynamism story merits further investigation.

We abstract from the recent pandemic period, which has featured wide-ranging discussions about both business dynamism and the role of market power in price setting. We prefer to focus on the long-run trends that predate the pandemic and sparked large literatures, but we view the pandemic period as an important arena in which to study market power and business dynamism in future work.

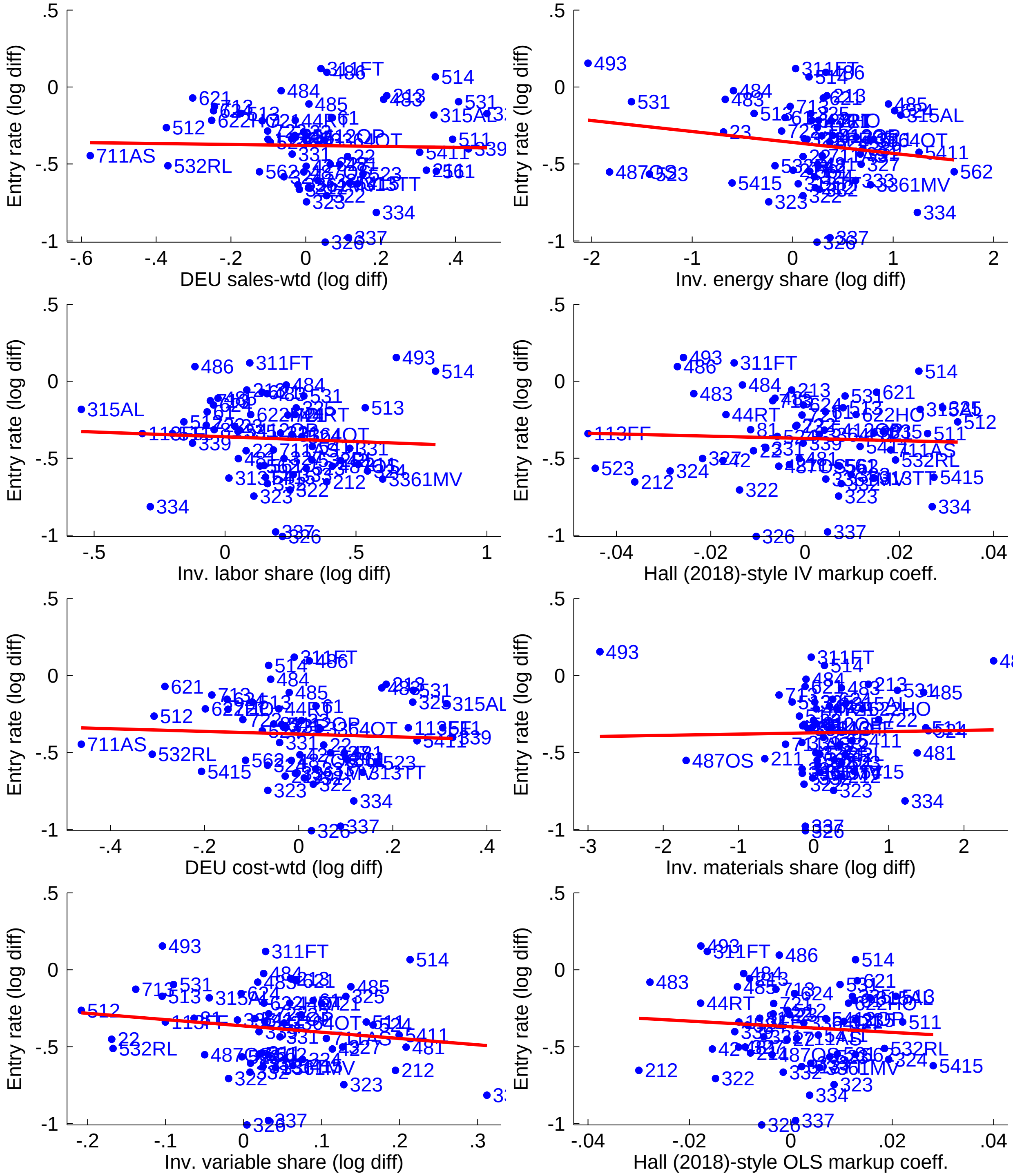
In sum, our empirical tests, based on cross-sectional industry data and ranging across many measurement and econometric specifications, simply do not support the prediction that higher markups have led to lower dynamism in the U.S.

A Appendix



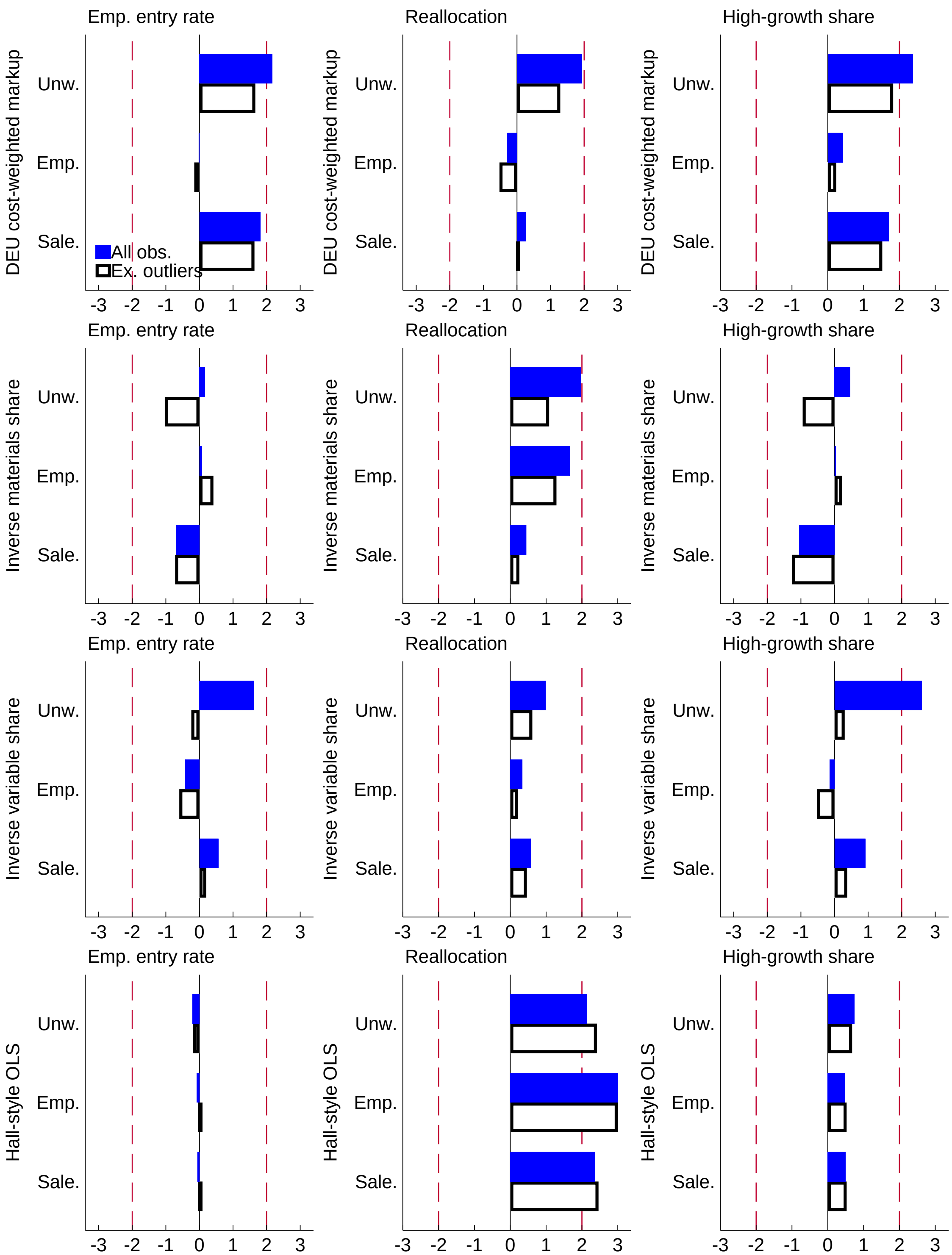
Note: Difference, 2015-2019 average vs. 1988-1992 except the DEU markup (which uses the 2015-2017 average) and the Hall-style markup growth coefficent.
Source: Business Dynamics Statistics; Compustat; KLEMS.

Figure A1: Change in dynamism and markups, KLEMS industries (alternative markup measures)



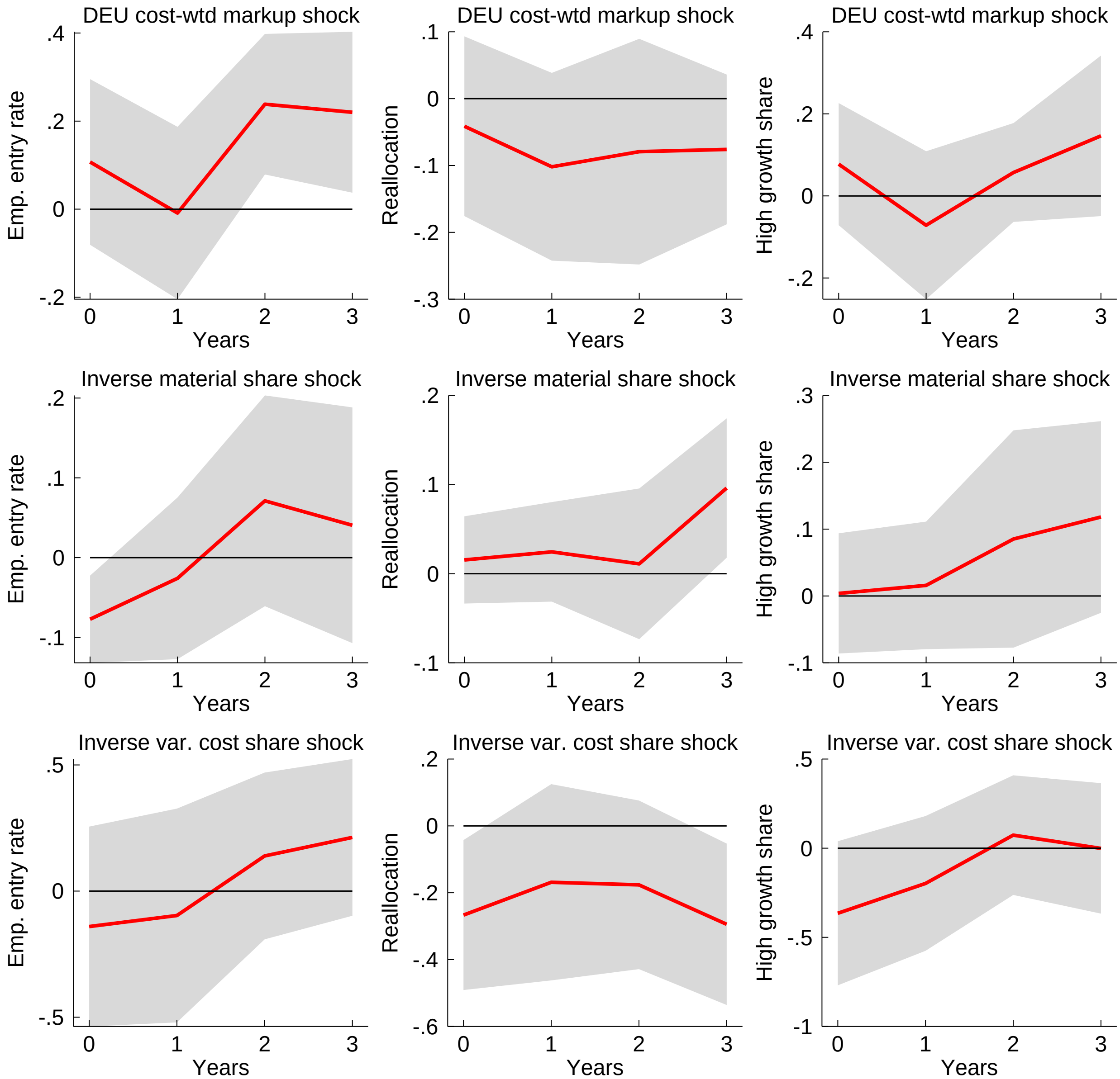
Note: Difference, 2015-2019 average vs. 1988-1992 except DEU markups (which use the 2015-2017 average) and markup growth coefficents.
Source: Business Dynamics Statistics; Compustat; KLEMS.

Figure A2: Change in unweighted entry rates and markups, KLEMS industries



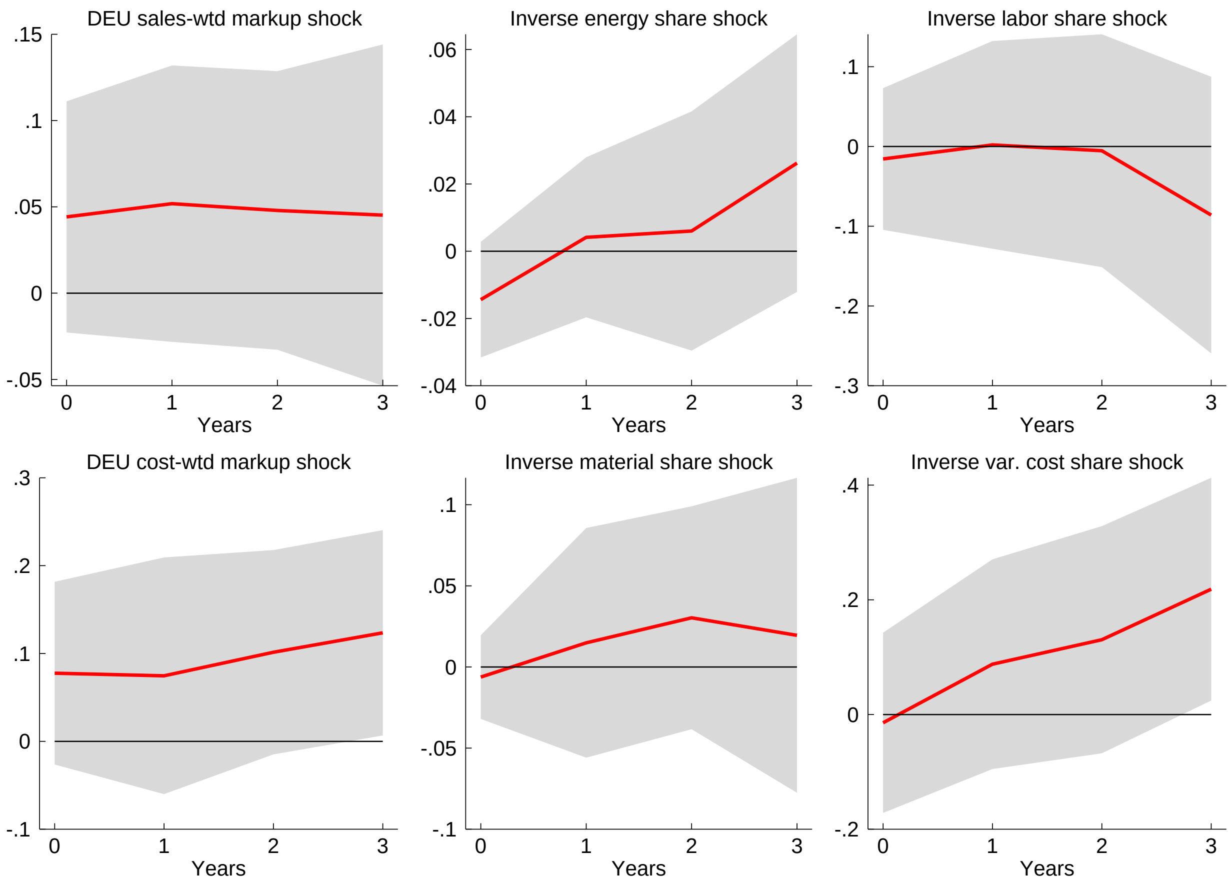
Note: Each panel shows t statistics from unweighted, employment-weighted, and sales-weighted regressions (described in text). t statistics truncated below -3 and above 3. KLEMS industry-level regressions.
Source: Business Dynamics Statistics; Compustat; KLEMS.

Figure A3: Long-run markup vs. dynamism coefficients t-statistics, KLEMS industries (alternative markup measures)



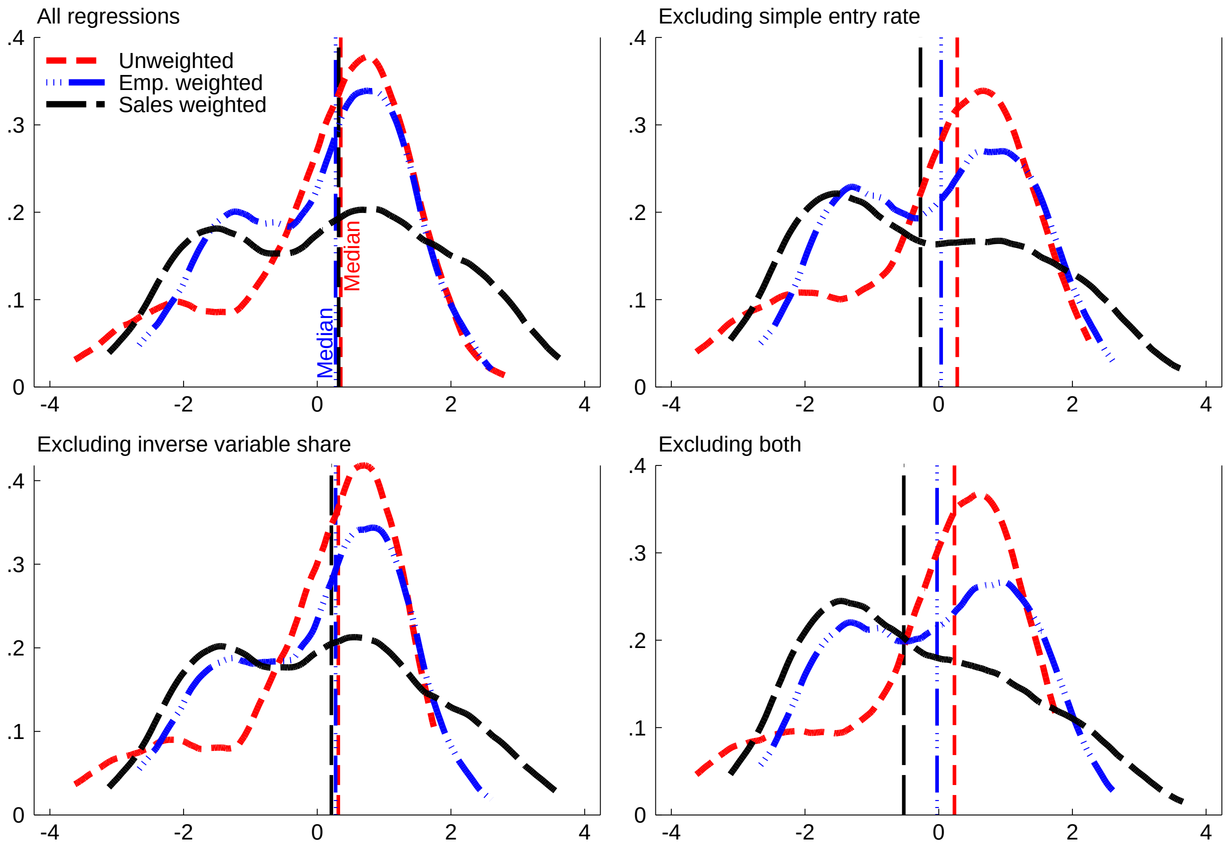
Note: Cumulative impulse response of dynamism variable to markup variable (both in log differences); employment-weighted regressions with industry and year fixed effects and 3 lags. DEU: 1988-2017; others: 1988-2019. KLEMS industries Source: Business Dynamics Statistics; Compustat; KLEMS.

Figure A4: Cumulative impulse responses of dynamism measures to DEU markup and cost share shocks, KLEMS industries (alternative markup measures)



Note: Cumulative impulse response of dynamism variable to markup variable (both in log differences); employment-weighted regressions with industry and year fixed effects and 3 lags. 1980-2017. KLEMS industries.
Source: Business Dynamics Statistics; Compustat.

Figure A5: Cumulative impulse responses of simple (unweighted) entry rate to DEU markup and cost share shocks, KLEMS industries



Note: Kernel density of t statistics for cumulative response at end of 4 years in two datasets: KLEMS industries with all markup variables (576 estimated models) and 3-digit NAICS industries with DEU markup variables (128 models). Separate densities shown for unweighted regressions, employment-weighted regressions, and sales-weighted regressions. All dynamism variables included. Includes models with and without year effects. Includes models with simultaneous response to markup shocks and one-year lagged response. Vertical lines indicate median t statistic across regressions.
Source: Business Dynamics Statistics, Compustat, and KLEMS.

Figure A6: Kernel density of t statistics from cumulative impulse responses of dynamism measures to markup shocks (horizon H = 4)

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