Seeing is Believing: The Impact of Local Economic Conditions on Firm Expectations, Employment and Investment

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Abstract

I show that managers overweight observations of local economic conditions at firm headquarters (HQ) when forming their macroeconomic expectations. This implies that HQ local economic conditions have an excessive impact on firm investment and employment growth. Using an empirical strategy identifying the impact of local economic conditions at HQ on employment outside the HQ, I find that a 1 percentage point (p.p.) higher local unemployment rate at HQ leads to 2 p.p. lower employment growth at non-HQ establishments. I consider a number of alternative explanations such as internal capital markets reallocation or local financing, and rule these out using placebo tests and by testing the key implications of the explanations. Then, I present evidence that HQ local conditions are overweighted in managers' expectations. Worse HQ local conditions lead to more pessimistic sales forecasts and more negative macroeconomic sentiment. These findings, along with results from tests comparing firms with different sensitivities to the macroeconomic cycle, support the notion that local economic conditions bias managers' macroeconomic expectations. Finally, I show that this bias can explain differences in county economic outcomes and may lead to significant investment misallocation.

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1 Introduction

A growing body of evidence shows that individuals can be biased in their expectations of future economic outcomes. Households, managers of firms, investors, and even professional forecasters have been found to exhibit biased expectations in their forecasts of future inflation, stock market returns, or firm profitability. In other words, errors in individuals' expectations are predictable and follow systematic patterns.

One possible source of bias in expectations is that individuals place excessive weight on personal observations, compared to the relevant statistical evidence, in forming forecasts and making decisions.[1] Evidence of this behavior among individuals suggests that managers of firms may use similar heuristics in forming their forecasts and choosing firm policies. Consider how managers form expectations about the future demand facing the firm. For many firms, this requires managers to forecast the state of the macroeconomy in addition to predicting the impact of idiosyncratic firm-specific factors. While managers consult professional macroeconomic forecasts as well as detailed real-time and historical data of the firm, they may also place significant weight on personally observed or experienced economic conditions, especially given the challenges in forecasting a process as complex as the macroeconomy.

In this paper, I ask whether managers overweight personal observations and whether this affects the economic outcomes of the firm. I argue that managers overweight local economic conditions in forming their expectations of future macroeconomic conditions because personally observed local economic conditions are salient observations about the state of the macroeconomy for managers.[2] As senior managers of the firm primarily live near, and work at, the headquarters of the firm, I focus on the local economic conditions in the geographic area near company headquarters (HQ).

If managers of firms overweight local economic conditions in forming expectations, the most important implication is that local economic conditions at firm HQ will affect firm-wide investment and employment disproportionately. As an example, consider the differences in investment between two U.S. companies, Herman Miller and Kimball International. Both companies manufacture office furniture and sell nationally as well as internationally. Herman Miller is headquartered in Holland, Michigan, and Kimball has its HQ in Jasper, Indiana. From 2009 to 2011, local unemployment rates were significantly higher in Holland than in Jasper, as Michigan was more severely affected by the Great Recession. But in the years right before and after this period, Holland and Jasper had comparable unemployment rates (albeit slightly higher in Holland). And while investment levels at Herman Miller and Kimball were similar from 2005 to 2008, as well as from 2012 to 2015, Herman Miller had significantly lower investment from 2009 to 2011, with an average annual investment

[1] There are many studies on this subject, dating back to the classic work of Kahneman and Tversky (1973). See Kahneman (2011) and Taylor (1982) for surveys of studies in this area.

[2] Consider the following quote from the former CEO of Duke Energy, a large company whose stock is a component of the S&P 500 index, on a conference call in September 2007: "I dare anybody to think recession and come to Charlotte and try to buy a house, as I just did. It didn't feel recession-like, unless I'm buying at the top of the market and the whole doggone thing is just going to go into the ditch next week...you're downtown in Charlotte and you see building cranes everywhere, very strong."

level 2.17 percentage points (p.p.) lower than Kimball (scaled by total assets), compared to an investment level 0.22 p.p. *higher* from 2005 to 2008. These patterns are plotted in Figure 1. At the same time, differences in sales growth, if anything, changed in the other direction: while Herman Miller's average annual sales growth from 2005 to 2008 was lower than Kimball's, it was higher than Kimball's from 2009 to 2011.

This example illustrates a pattern that is systematic across public firms in the U.S.: in the cross-section, firms headquartered in counties with relatively higher local unemployment rates have lower investment levels than peer firms, even when controlling for other predictors of investment. Yet simple cross-sectional results face a key confound: local HQ economic conditions may also proxy for the unobserved demand conditions of the firm, since a portion of the demand the firm faces likely comes from the local area near HQ. Thus, these basic results cannot reject the null hypothesis that local economic conditions are rationally weighted by managers in choosing investment.

To address this issue, I use an empirical strategy that identifies the impact of local economic conditions at firm headquarters on firm investment – or employment growth – *not near* the HQ location. This strategy tests the significantly more restrictive prediction that firms reduce hiring in response to worse HQ local economic conditions even at establishments far away from HQ that are not affected by HQ local demand. This requires within-firm, establishment-level panel data, which I obtain using U.S. Census Bureau microdata. By considering only establishments located a significant distance away from firm HQ, I isolate the impact of HQ local economic conditions on employment growth at non-local establishments. More precisely, the empirical strategy employs establishment location-by-industry-by-year fixed effects, so the impact of HQ local conditions is identified off comparisons of establishments in the same industry and non-HQ county, but that are part of firms headquartered in different locations.[3] In other words, for two establishments located in the same county in northern California in the same industry, where one belongs to a firm headquartered in Michigan and the other to a firm headquartered in Indiana, I compare their relative employment growth at the same point in time and see if it is explained by differences between the local economic conditions of the Michigan HQ county and the Indiana HQ county.

Using this within-firm empirical strategy, I find that a 1 percentage point higher unemployment rate in the county of the firm headquarters leads to an approximately 2 p.p. decline in employment growth at non-HQ establishments. This implies that a firm at the 25th percentile of HQ unemployment will grow employment at its non-HQ establishments 4.2 p.p. faster than a firm in the 75th percentile of HQ unemployment. In addition, I find these effects are concentrated on the extensive margin of employment growth – that is, on the opening and closing of establishments.

I then present direct evidence that local economic conditions are overweighted by managers in forming their *expectations* of demand and macroeconomic conditions. First, I use data on sales guidance as a measure of managers' expectations and find that worse local economic conditions lead to more pessimistic sales guidance errors, where realized sales exceed forecast sales. This evidence is at odds with rational expectations explanations of the impact of HQ local conditions. Second,

[3] I also use establishment location-by-time fixed effects and industry-by-time fixed effects in other specifications.

using the text of firms' financial filings, I show that HQ local economic conditions have excessive impact on managers' macroeconomic expectations. I also use a machine-learning LASSO approach to construct proxies for management expectations of investment returns based on the text of firms' financial filings, and present results from this corroborative test in the Appendix. Results from all these tests support the notion that managers overweight local economic conditions in forming their expectations.

The identifying assumption of the establishment-level empirical strategy is that differences in local economic conditions at firm HQ are orthogonal to the differences in demand at establishments not near HQ. Here, I highlight a couple potential threats to identification and how I address these potential issues, leaving the rest to be fully addressed later in the paper. First, there may be direct, cross-region impacts of HQ local area demand. For example, a firm headquartered in Michigan may sell only locally in Michigan but produce its goods at establishments in California. Second, there could be reverse causality if HQ layoffs lead to higher HQ county unemployment. I conduct a number of analyses to rule out these possibilities. I restrict the sample to only firms operating in non-tradable industries (retail and restaurants) where end-demand is entirely local and find similar results in regressions on this sample. To address the reverse causality concern, I restrict the sample to firms whose HQ employment is a very small fraction of total county employment, and the results in this sample are essentially identical to those in the baseline sample.

I then consider alternative explanations for the establishment-level employment growth results. One possibility is that the results can be explained by internal capital markets reallocation across different locations of the firm (e.g., Scharfstein and Stein 2000, Giroud and Mueller 2017). In this explanation, shocks to HQ local economic conditions proxy for local demand shocks affecting firm headquarters, but the firm smooths out the effects by adjusting investment and employment at all locations, due to agency frictions or financial constraints. In response to weak HQ local economic conditions, managers rationally decrease employment growth even at establishments not near HQ and not experiencing weaker demand.

The internal capital markets explanation can be empirically differentiated from local conditions overweighting, as the former predicts that the firm will exhibit similar employment growth sensitivity to the local economic conditions of non-HQ establishments as it does to HQ local economic conditions. According to the internal capital markets explanation, there is nothing "special" about the HQ location. I conduct a number of tests for whether the local economic conditions of other establishments have similar impacts. In a placebo test, I construct a synthetic placebo HQ by aggregating a number of randomly selected non-HQ establishments to match the average HQ employment share, and repeat the baseline analysis using many randomly drawn synthetic placebo HQs.[4] The distribution of estimates I obtain from the placebo test rejects the hypothesis that the HQ impact is the same as the placebo non-HQ impact on employment growth. These placebo estimates imply that the internal capital markets explanation accounts for only 9 percent of the

[4]In a simpler placebo test, I replace HQ local conditions with the local conditions of a randomly selected non-HQ establishment within the firm and re-run the baseline specification over the many placebo draws.

baseline result. Furthermore, the placebo results support the notion that managers overweight HQ local economic conditions, even relative to the rational internal capital markets theory baseline.

Another alternative explanation for the result is that local economic conditions at firm headquarters affect the firm's ability to borrow. If firms have borrowing relationships from local banks, and worse local economic conditions lead to reduced willingness to lend by local banks, firms that borrow from these banks will face greater difficulty obtaining loans and thus reduce their investment and employment growth. Alternatively, firms may own real estate at HQ whose collateral value is lower when local economic conditions are worse. I do not find evidence supporting this financing channel explanation. For one, local economic conditions at firm HQ do not predict firm-level debt issuance. Second, firms with bond market access – that is, large firms with access to less costly non-local finance – have similar employment growth sensitivity to HQ local conditions as firms without access. Third, more financially constrained firms do not exhibit stronger sensitivity to HQ local conditions, as predicted by the local financing explanation. Finally, firms with more HQ real estate do not exhibit stronger sensitivity to HQ local conditions.

I find corroborative evidence supporting the notion that managers overweight local conditions as a signal for *macroeconomic* conditions. Comparing firms in industries with higher correlation with the state of the macroeconomy to those in industries with lower correlation, I find suggestive evidence that firms with higher macroeconomic sensitivity exhibit stronger local economic conditions overweighting.[5]

The overweighting of local economic conditions by managers has a couple of aggregate implications: it can help explain differences in county economic outcomes, and it causes misallocated investment. First, I find that counties with establishments headquartered in out-of-state areas with worse local conditions exhibit lower employment growth, fewer establishment births, and more establishment deaths. Second, local conditions overweighting can lead to the misallocation of investment and employment across firms, as it leads both to investment undertaken with lower returns than expected, as well as foregone investment that would achieve higher than expected returns. Extrapolating from the firm-level estimates, a back-of-the-envelope calculation implies that local conditions overweighting causes 3.2% of aggregate investment to be misallocated across firms, which is equivalent to $23 billion of misallocated investment in the US economy for large public firms, annually. While this estimate is only suggestive, it does point to the aggregate impacts of this bias being economically significant.

This paper adds to the literature in a number of ways. First, it is related to studies on the biased expectations among households, investors, and managers. There is a growing literature showing that households form expectations of economic variables such as stock market returns or inflation in a biased or inefficient manner (e.g., Bruine de Bruin, van der Klaauw and Topa 2011,

[5]In separate work, I find other evidence of the impact of local conditions on macroeconomic expectations. First, using data from the Michigan Survey of Consumers, I find that local unemployment rates affect household expectations for the state of the U.S. macroeconomy. Second, using data on monetary policy votes on the Federal Open Market Committee of the Federal Reserve, I find that local inflation rates affect Regional Federal Reserve Bank Presidents' monetary policy voting.

Barberis, Greenwood, Jin and Shleifer 2015, Coibion and Gorodnichenko 2015, Cavallo, Cruces and Perez-Truglia 2016). Moreover, even investors and professional forecasters form biased expectations (Coibion and Gorodnichenko 2012, Greenwood and Shleifer 2014, Bouchard, Kruger, Landier and Thesmar 2016). Gennaioli, Ma and Shleifer (2016) show that managers' expectations of earnings are overly extrapolative. A key contribution of this study is to present evidence that biased expectations directly affect the real economy through firm employment and investment policy. Separately, this paper contributes to our understanding of how managers form expectations of the macroeconomy.

Second, this paper contributes to the literature on the impact of individual experience on expectations. While there are a number of studies showing that personal history and experience matters for household or investor expectations of inflation, stock market returns, and unemployment (Kaustia and Knupfer 2008, Greenwood and Nagel 2009, Malmendier and Nagel 2011, Chiang, Hirshleifer, Qian and Sherman 2011, Piazzesi and Schneider 2012, Malmendier and Nagel 2016, Kuchler and Zafar 2016, Chernenko, Hanson and Sunderam 2016, Bailey, Cao, Kuchler and Stroebel 2017), fewer studies explore the impact of personal experience on the managers of firms (e.g., Malmendier, Tate and Yan 2011).[6]

More broadly, this paper is related to studies of biased managers, which have provided evidence on biases such as overconfidence, anchoring, miscalibration in return expectations, and excessive extrapolation (e.g., Bertrand and Schoar 2003, Malmendier and Tate 2005, Baker, Pan and Wurgler 2012, Ben-David, Graham and Harvey 2013).[7] Finally, this study contributes to the extensive literature on the determinants of firm investment and employment. The most related studies include those on within-firm investment and employment (e.g., Lamont 1997, Rajan, Servaes and Zingales 2000, Bertrand and Mullainathan 2003, Ozbas and Scharfstein 2010, Giroud 2013, Giroud and Mueller 2015, Giroud and Mueller 2017), differences in investment across geographies (e.g., Becker 2007, Dougal, Parsons and Titman 2015), the impact of creditors and real estate on firm investment (e.g., Nini, Smith and Sufi 2009, Chaney, Sraer and Thesmar 2012), and the extensive literature on the impact of cash flow on investment (e.g., Cummins, Hassett and Oliner 2006 study the relationship between cash flow, expectations, and investment). This paper shows that the biased expectations of managers are also an important determinant of firm investment.

The rest of the paper is organized as follows: Section 2 outlines the conceptual framework relating local economic conditions, expectations, and firm policy. It describes the core establishment-level empirical strategy, as well as the tests on expectations. Section 3 describes the data employed in the establishment-level tests. Section 4 presents the results of the establishment-level tests, addresses potential issues with the identification strategy, and presents tests of alternative explanations. Section 5 presents the tests of the impact of local conditions on managers' expectations. Section 6 presents evidence on the county-level impacts of local conditions overweighting, as well as its impact on investment misallocation. Section 7 concludes.

[6] Research in this area relates to the "availability heuristic," originally described by Kahneman and Tversky (1973), as well as that on the "law of small numbers" bias (e.g., Tversky and Kahneman 1971, Rabin 2002). I outline the various behavioral and psychological theories that may explain overweighted local conditions in Appendix D.

[7] See Baker and Wurgler (2013) for a comprehensive survey of recent studies on behavioral managers.

2 Empirical framework

To motivate the empirical strategy, I outline a simple conceptual framework relating local economic conditions, expectations, and investment or employment growth. I describe how this framework relates to the establishment-level employment growth and firm-level expectations empirical tests.

The establishment-level empirical strategy analyzes the impact of cross-sectional differences in local economic conditions at firm headquarters on employment growth at the firm's non-HQ establishments. The identification strategy is based on a comparison of establishments in the same county but belonging to firms headquartered in different locations. I outline potential failures of the identifying assumption and alternative explanations, but leave these to be addressed in detail in Section 4.

2.1 The firm investment problem

The firm solves

$$\mathbb{E}_t\left(q_{t+1}\right) I_{t+1} - I_{t+1} - \frac{\phi}{2} I_{t+1}^2,$$

where I_{t+1} is the total amount of investment (or hiring) the firm chooses at time t but which generates returns at time $t+1$, $\mathbb{E}_t\left(q_{t+1}\right)$ are the expected gross returns to investment I_{t+1}, and there are quadratic adjustment costs to new investment that capture, in reduced form, other costs of investment, such as those occurring due to financial constraints and costly external finance.

The first-order condition

$$I_{t+1} = \frac{\mathbb{E}_t\left(q_{t+1}\right) - 1}{\phi}, \tag{1}$$

determines the optimal level of investment.

Expectations of returns to investment are a function of signals s_1 through s_n observed at time t:

$$\mathbb{E}_t\left(q_{t+1}\right) = f\left(s_{1t}, s_{2t,} s_{3t}, ... s_{nt}\right), \tag{2}$$

where $s_1, ..., s_n$ include the many disparate signals that managers receive of future investment returns, such as macroeconomic forecasts, the firm's historical data on sales and profitability, peer firm performance, among many others. Approximating equation (2) with a linear, additive functional form, we can re-write firm investment as a function of the weights on these signals:

$$I_{t+1} \quad = \quad \theta_0 + \theta_1 s_{1t} + \theta_2 s_{2t} + ... + \theta_n s_{nt}.$$

2.2 Employment growth and local economic conditions

Consider establishment e of firm i at time t. The manager will choose investment and employment at establishment e based on signals of firm-level profitability, establishment-level signals of profitability specific to e, as well as signals of the state of the aggregate economy at time t. For example, a retailer will face both decisions on whether to build or expand a specific store, say in northern California, as well as decisions on the total number of new stores to open (or existing stores to close). While

the former decision is likely to be made primarily on the basis of detailed establishment and firm historical sales data, the latter decision will also reflect managers' outlook on the aggregate economy, as long as the macroeconomy affects the profits of the firm.

While managers' outlook on the aggregate economy will be a function of various inputs such as projections from professional forecasters, the local conditions overweighting hypothesis predicts that managers' outlook on the aggregate economy will be affected by their observed local economic conditions. This hypothesis implies that worse local economic conditions will lead managers to have a weaker macroeconomic outlook and invest less on average at the firm – e.g., open fewer stores – even if it does not affect decisions on relative allocation across the firm – e.g., whether to open a store in neighborhood a in northern California rather than neighborhood b. A simple model that formalizes this logic is presented in Appendix C.

Returning to the framework described above and assuming that managers have rational expectations, we can write the investment or employment growth at establishment e as a function of the firm-, establishment-, industry-, and aggregate-level vectors of signals:

$$I_{ei,t+1} = \Theta_1^{'}\mathbf{s}_{it}^{firm} + \Theta_2^{'}\mathbf{s}_{et}^{estab} + \Theta_3^{'}\mathbf{s}_{ind,t}^{ind} + \Theta_4^{'}\mathbf{s}_{t}^{aggregate}. \tag{3}$$

Summing across all establishments of firm i, we can express firm-level investment as:

$$\begin{aligned} I_{i,t+1} &= \sum_{e\in i} w_e I_{ei,t+1}, \\ &= \Theta_1^{'}\mathbf{s}_{it}^{firm} + \Theta_2^{'}\sum_{e\in i} w_e \mathbf{s}_{et}^{estab} + \Theta_3^{'}\mathbf{s}_{ind,t}^{ind} + \Theta_4^{'}\mathbf{s}_{t}^{aggregate}, \end{aligned}$$

where w_e is the fraction of the total firm's investment (or employment) at establishment e.

Using firm-level data, we could estimate the following firm-level regression specification:

$$I_{i,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

where ur_{ct} is the unemployment rate of the the firm's HQ county c and the proxy for local economic conditions, $\mathbf{x}_{it}$ is a vector of firm-level signals, δ_i are firm fixed effects, and $\delta_{ind,t}$ are industry-by-time fixed effects that absorb variation in industry- and aggregate-level signals. However, even under a rational expectations null hypothesis, we expect $\beta_1 < 0$ because $\text{Cov}\left(ur_{ct}, s_{ct}^{estab}\right) < 0$. In other words, cross-sectional differences in HQ local economic conditions, proxied by HQ county unemployment rates, may capture actual differences in local demand conditions for the firm in the HQ region. This implies that firm-level cross-sectional regressions cannot disentangle the overweighted local conditions hypothesis from the rational expectations null hypothesis.

2.3 Establishment-level identification strategy

To address this identification challenge, I consider the impact of firm HQ local economic conditions on the employment growth of establishments *outside* the HQ area. Local conditions overweighting

will cause firms to reduce growth even at non-HQ establishments in response to worse HQ local conditions. By restricting attention to establishments far away from HQ – outside the HQ state, or even further in robustness checks – I can plausibly avoid capturing the direct impact of HQ demand conditions on a firm's growth near its HQ. It is thus plausible that the rational expectations null hypothesis predicts zero impact of HQ local economic conditions on non-HQ establishment employment growth. However, if managers overweight local economic conditions, they will have a more pessimistic outlook on the aggregate economy in response to worse HQ local conditions, causing them to invest less, even at non-HQ establishments.

With establishment-level microdata on employment from Census, I estimate an approximation to equation (3), which relates establishment-level investment to signals of investment profitability. I estimate:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt}, \tag{4}$$

excluding all establishments in the same state as HQ county c, where e indexes establishments, l the county of the establishment, i the firm, and t the year. Firm i is headquartered in county c, and ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls for investment from Compustat measured in the first quarter of year t (Tobin's Q, cashflow, size, sales growth, and cash level), and $\Delta y_{e,t+1}$ is the growth in annual employment for establishment e, from March of year t to March of year $t+1$, defined as

$$\Delta y_{e,t+1} = \frac{emp_{e,t+1} - emp_{et}}{0.5 \times (emp_{e,t+1} + emp_{et})}.$$

This measure of employment growth is bounded by -2 and 2, and is the standard measure used in studies of establishment-level employment (e.g., Davis, Haltiwanger and Schuh 1996, Chodorow-Reich 2014). It accounts for both intensive and extensive margin changes, thus capturing establishment openings or closings. The baseline regression includes establishment fixed effects δ_e, as well as establishment location-by-year fixed effects (δ_{lt}) to capture establishment location-based signals of investment returns, along with two-digit industry-by-year fixed effects ($\delta_{ind,t}$) to capture industry and aggregate signals. In a second specification, I use establishment location-by-industry-by-year fixed effects to restrict the identifying variation to comparisons of establishments in the same industry, in the same county, at the same point in time:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{l,ind,t} + \epsilon_{eilt}. \tag{5}$$

I cluster standard errors at the firm-level in all regressions.[8]

The variation exploited in the regression corresponding to equation (5) is most easily understood with a simple hypothetical example. Consider two establishments in the same industry and located in the same county in California, but where one belongs to a firm headquartered in southern Indiana

[8] In robustness tests in Appendix Table A1, I cluster standard errors at the HQ county level, which allows for the possibility of correlated residuals across firms headquartered in the same county. The standard errors on the estimates with county-level clustering are similar to those estimated with firm-level clustering.

and the other to a firm headquartered in Michigan. I compare their relative employment growth at the same point in time and see if this is predicted by the unemployment rate in the Indiana HQ county relative to that in the Michigan HQ county.

2.3.1 Identifying assumption and alternative explanations

In baseline specifications (4) and (5), the key identifying assumption is that a rational expectations null hypothesis predicts $\beta_1 = 0$, while the local conditions overweighting hypothesis predicts $\beta_1 < 0$. Put another way, the identifying assumption is that cross-sectional differences in local conditions at firm HQ, proxied by differences in HQ county unemployment rates, are orthogonal to differences in demand and investment opportunities of similarly-located establishments not near HQ of these firms. In terms of the investment framework described by equation (3), this assumption implies that the set of residualized unobserved firm-level profit signals, $\mathbf{s}_{it}^{firm-}$, are uncorrelated with HQ unemployment rates, or $\text{Cov}\left(ur_{ct}, \mathbf{s}_{it}^{firm-}\right) = 0$. If this assumption holds, the rational expectations null hypothesis predicts that $\beta_1 = 0$.

I briefly outline the ways in which the assumption could be violated, but discuss how I address these potential violations in detail later in Section 4. First, firms may be selling in the HQ local area but producing their products at non-HQ establishments. Therefore, worse HQ local conditions still capture weaker firm-wide demand conditions that directly affect non-HQ establishments. Second, firms may have networks of establishments that are located predominantly in areas whose local economic conditions correlate with those of headquarters, leading to worse average firm-wide local economic conditions. Worse local economic conditions across the entire firm network may then lead a firm to grow employment more slowly at a particular establishment, even when compared to a peer establishment in the same county. A variant of this second concern is that firms cater to certain segments of consumers, and the local conditions at firm headquarters are capturing the economic fortunes of the firm's consumers. Hence, two establishments in the same industry and location may sell to different types of consumers whose economic fortunes are correlated with the local conditions of firm HQ. A third concern is mechanical: idiosyncratic shocks to the firm may directly affect the HQ county unemployment rate as the HQ or nearby establishments of the firm hire or lay off workers, but also cause the other non-HQ establishments to hire or lay off workers. Fourth, a more general concern is that the firm-wide investment opportunities of firms headquartered in certain areas is driven by another omitted variable correlated *only* with the vibrancy of the HQ area (and not with the vibrancy of the other non-HQ establishment locations). For example, firms headquartered in Indiana generically face favorable firm-wide investment opportunities during local booms.

Aside from these potential confounds due to unobserved firm-level demand or investment opportunity factors, there are also alternative explanations that would explain the finding that $\beta_1 < 0$ in estimating specification (4) and (5). One explanation is based on internal capital markets theory, in which cross-subsidization across parts of the firm leads the firm to cut employment at non-HQ establishments in response to negative shocks to HQ area demand. A second explanation centers

around local financing: either the firm has borrowing relationships with local banks at HQ, whose credit supply shrinks with worse local economic conditions, or the firm owns real estate near HQ, whose collateral value for loans is lower when HQ local conditions are worse. A third explanation is that managers rationally weight local economic conditions in forming their macroeconomic forecasts.

2.4 Expectations and local economic conditions

Beyond the establishment-level tests of the impact of local conditions overweighting on employment growth, I also test whether HQ local economic conditions are overweighted in the formation of managerial *expectations*. A straightforward implication of the overweighting hypothesis is that relatively worse local economic conditions will make managers excessively pessimistic in their outlook. By comparing managers' reported sales forecasts with *ex post* realized sales, I can test whether worse local economic conditions lead to forecasts that undershoot realized sales more. Indeed, any rational expectations-based explanation predicts no relationship between HQ local conditions and forecast errors.

I conduct alternative tests of the impact of local conditions overweighting on expectations by recovering macroeconomic sentiment and investment return expectations from the text of firms' financial filings. I describe these tests in greater detail in Section 5.2 and Appendix Section B.2.

3 Data for establishment-level tests

Within-firm data is required to analyze the impact of HQ local economic conditions on employment growth at the non-headquarters establishments of the firm. I use establishment-level data from the Longitudinal Business Database (LBD) of the U.S. Census Bureau, and merge the Census data to Compustat data for public firms.

3.1 Local conditions data

I use the county unemployment rate at the firm HQ county to proxy for local conditions at firm HQ. Without strong prior knowledge of the specific aspects of local economic conditions that are most salient for managers, it is important to select a variable that is a strong proxy for the health of the local economy to ensure that cross-sectional differences in this variable will be sufficiently powerful in capturing differences in managers' personal observations of local economic strength. The local unemployment rate is likely to satisfy this condition. Even if managers of firms are unlikely to focus on unemployment *per se* when assessing and observing economic conditions, the local unemployment rate is a simple and powerful proxy for local economic conditions. At the national level, the unemployment rate is highly correlated with the business cycle and GDP growth rates. Granular panel data on local unemployment rates are available at the county level in the

U.S.[9]

I focus on HQ local conditions because they are the local economic conditions that are most likely to be salient for the firm's senior managers, such as the CEO. These senior managers spend a substantial amount of time at HQ, both because they primarily work there – the definition of company headquarters is the location of the executive offices – and because they likely reside in close proximity to HQ. By using the HQ unemployment rate as a proxy, I am assuming that personal observations about the strength of the economy are more negative for firms headquartered in counties with relatively higher unemployment rates.

I obtain county-level, monthly unemployment rates from the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAU) database.[10] In the analysis, I compute annual averages of monthly unemployment rates. Table 1 shows that there is significant variation in local unemployment rates in the baseline sample. In robustness tests, I use metropolitan area-level unemployment rates to proxy for local economic conditions and find similar results (see Appendix Table A2).

3.2 Firm data

The Longitudinal Business Database (LBD) contains annual establishment-level data on employer establishments in the United States. The LBD classifies establishment births and deaths and contains data on the location, firm affiliation, number of employees as of March, and annual payroll of the establishment.

For data on other controls for investment opportunities, I use quarterly data from the Compustat database. These covariates include Tobin's Q, the logarithm of total assets, annual cash flow scaled by lagged total assets, cash holdings divided by total assets, and annual sales growth relative to the prior year, all winsorized at the 1% level.

I use Compustat data on the location of firm headquarters. Compustat data on HQ location is back-filled based on the most recent HQ location. However, it has high match rates to other sources of panel data that track firm headquarters location dynamically over time (Giroud 2013). In the process of merging Compustat and LBD data, obvious instances of misclassified headquarters location are excluded from the sample. These consist of observations in which the Compustat data HQ county location merges to zero active firm establishments in the county in the LBD data. To the extent that changes in firm HQ locations introduce measurement error in the key explanatory variable of interest – local conditions at HQ – this will attenuate the estimated impact of local conditions at firm HQ.

In addition, I perform robustness tests where I use the Compact Disclosure and CRSP/Compustat merged databases in order to construct a dataset of dynamic HQ locations from 1990 onward. In these tests (shown in Appendix Table A3), I restrict the analysis to firms whose HQ location does

[9]For the years from 2001 onward where metropolitan area (MSA)-level GDP data is available, MSA-level changes in unemployment rate are strongly negatively associated with GDP growth (t-statistic of 7.9) in panel regressions with year fixed effects, which identify the cross-sectional relationship between GDP growth and change in unemployment.

[10]I obtain the data on county-level unemployment rates before 1990 directly from the BLS. From 1990 onward, county-level data can be directly downloaded from the BLS website.

not change city, and find similar results. I focus on firms with static HQ only because firms may relocate their HQ in order to be closer to their customers, leading to a potential endogeneity concern that changes in local HQ economic conditions driven by HQ relocations proxy for changes in consumer composition.[11] Moreover, the sample of firms that move headquarters may have weak power to detect local conditions overweighting, as managers may chose to move the firm headquarters to (or away from) the location where they reside. In that case, the initial (or subsequent) location's economic conditions may not be salient for the manager because she spends little time there.[12]

In the data, there is significant geographic variation in the location of firm headquarters. The number of firms headquartered in a state is shown in Figure 2.

3.3 Creating the analysis sample

I merge the Census LBD establishment-level data to the Compustat firm-level data using the Census Bureau crosswalk, which runs through 2005. The years included in the analysis are 1981 to 2005. As the employment data in the LBD is as of March of a year, I merge the LBD data in year t to Compustat data from the first quarter of year t.

I restrict the sample in a number of ways. First, I consider only firms that are headquartered in the U.S. Second, I consider only multi-establishment firms in the Census LBD database, as the empirical strategy requires that firms have at least two establishments with one outside the HQ state. I exclude financial firms (SIC codes 6000-6799) and regulated utilities (SIC codes 4900-4949), as is customary in studies of firm investment. As well, I restrict the sample to establishments not near HQ. In the baseline regressions, I define this as all establishments not in the same state as the HQ, but the results are robust to excluding all establishments in the HQ and adjacent states, or all establishments located within 250 or 500 miles of headquarters (see Appendix Table A4). The number of establishment-year observations is 4,359,000 in the baseline sample (rounded to comply with Census disclosure rules).

4 Establishment-level tests on employment growth

This section presents results of the empirical strategy described in Section 2. I show that firm employment growth at establishments far from headquarters responds to HQ local economic con-

[11] For example, a firm may be relocating headquarters from an area with worse local conditions to one with better conditions as their customers are increasingly drawn from areas similar to the new HQ location. Then, the HQ local conditions for this firm will be measured as improving just as its fundamental demand is improving. Using either a static definition of headquarters – as in the baseline analysis – or restricting the sample to non-movers avoids this concern.

[12] Both moving closer to customers and to where senior managers live are stated reasons for the relocation of firm headquarters, as Engelberg, Ozoguz and Wang (2013) document. Indeed, local conditions overweighting would predict weak effects of HQ local conditions on firm expectations and investment if the senior managers of the firm do not live near firm HQ, since local economic conditions may not be very salient. Examples of CEOs who did not reside locally at firm HQ include Ron Johnson, CEO of J.C. Penney from 2011 to 2013, who commuted from California to HQ in Texas, and Federica Marchionni, CEO of Land's End from 2015 to 2016, who spent limited time at the Dodgeville, Wisconsin HQ, but instead largely worked and resided in New York City. More generally, managers who do not reside near firm HQ will attenuate tests of local conditions overweighting using HQ local conditions.

ditions. I consider the possibility that the identifying assumption is violated due to unobserved firm-level demand or investment opportunities, but do not find any evidence that these potential confounds explain the results. I also test alternative explanations, such as internal capital markets and local financing, but do not find evidence consistent with these explanations.

4.1 Baseline results and addressing potential violations of identifying assumption

I present results from the estimation of equation (4) of the impact of HQ unemployment rate on non-HQ establishment employment growth (multiplied by one hundred to be in percent units). Column (1) of Table 2 presents these results without controls, while column (2) shows the results with firm-level controls for Tobin's Q, scaled cashflow, cash levels, sales growth, and the log of total assets. The specifications in columns (1) and (2) include establishment county-by-year, industry-by-year, and establishment fixed effects. The coefficient estimate on HQ unemployment rate (ur_{ct}) in column (2) implies that a 1 percentage point higher HQ unemployment rate leads to 2 p.p. lower employment growth, relative to other establishments in the same county and point in time. The addition of firm-level investment opportunity controls in column (2) does not significantly change the estimate relative to that from the specification without controls in column (1).

The magnitude of this estimate is economically significant. For one, we can compare the impact of HQ local conditions to other well-known predictors of investment which also affect employment growth. A one standard deviation (SD) increase in cashflow, measured in the cross-section of firms, increases employment growth by 6.90 p.p., whereas the impact of a one SD higher HQ unemployment rate is 3.15 p.p., which is 46 percent of the magnitude of the effect of cashflow.[13] Second, the standard deviation of establishment employment growth in the analysis sample is 81.2 p.p., which implies that a one SD higher HQ unemployment rate in the cross-section leads to a 3.9 percent of a SD decline in establishment growth.

Columns (3) and (4) of Table 2 present results from specification (5), which uses establishment county-by-industry-by-year fixed effects, restricting the identifying variation to establishments in the same industry-county-year cell. The coefficient estimate of -2.21 on ur_{ct} in column (4) is similar to the estimate from column (2) that uses county-by-year and industry-by-year fixed effects separately. Again, the addition of firm controls in column (4) relative to column (3) leaves the ur_{ct} coefficient effectively unchanged. Figure 3 illustrates the results from the regression in column (4) in a binned scatterplot.

The remaining columns of Table 2 use subsamples to address concerns that variation in the HQ unemployment rate is capturing unobserved variation in firm-level investment opportunities, i.e., that the identifying assumption is violated. The results shown are from the specifications using both

[13] Here, I calculate the variable standard deviations at the firm-level on the firms in the baseline regression, after residualizing the variable on industry-by-year fixed effects. This better captures the identifying variation, which is firm-level and also entirely cross-sectional because time-series variation in unemployment rates is fully absorbed by the fixed effects. Calculated this way, a one SD change in HQ unemployment rate is 1.56, and a one SD change in scaled cashflow is 0.175.

industry-by-time and location-by-time fixed effects.[14] The first such concern is that firms are selling in the HQ local area but producing their products at non-HQ establishments. For example, a firm headquartered in Michigan may sell primarily to Michigan consumers but manufacture its goods in California. Relatively worse local demand in Michigan, captured by a higher local Michigan unemployment rate, will lead the firm to cut employment growth at California establishments.

I address this issue by restricting the sample to firms operating in non-tradable industries (retail and restaurants) where end-demand is entirely locally driven (e.g., Mian and Sufi 2014). In non-tradable industries, California establishments cater only to local California demand. This directly avoids the concern about HQ location demand spillovers.[15] Column (5) of Table 2 presents estimates restricting the sample to non-tradable firms only: a one percentage point higher HQ unemployment rate leads to 3.5 p.p. lower employment growth. I also present the results restricting the sample to firms where the share of the firm's employment in the HQ state is less than 25% of total firm employment, since these dispersed firms are less likely to be selling only to the HQ region. Again, estimates of the impact of HQ unemployment rate in this subsample in column (6) of Table 2 are similar to the comparable baseline results in column (2).

A second concern is that firms may have networks of establishments that are located predominantly in areas whose local economic conditions correlate with those of HQ. For example, firms with Holland, Michigan headquarters may have a network of establishments concentrated in areas whose local economic conditions comove with those of Holland. Thus, if the Holland HQ firm faces worse local economic conditions, it will also face worse average firm-wide local economic conditions and firm-wide demand, because its *non*-Holland locations resemble Holland in terms of economic conditions. Worse local economic conditions across the entire firm network may then lead the Holland-headquartered establishment in northern California to grow employment more slowly than the Indiana-headquartered establishment in California.

This second concern can still be addressed by restricting the sample to non-tradable firms, where the demand for an establishment is entirely local. Even if worse HQ local conditions are associated with worse average firm network local conditions, this should not affect the relative local demand of a non-tradable establishment compared to another in the same location and point in time. Thus, the results in column (5), showing similar effects of the HQ unemployment rate for non-tradables, address this potential confound as well.

A related variant of this second concern is that the firm tends to locate its establishments in locations with certain types of consumers, and the local conditions at firm HQ are capturing the economic fortunes of the firm's types of consumers. Hence, even two establishments in the same industry and location may cater to different types of consumers whose economic fortunes are

[14] Results from the same regressions, but with industry-by-location-by-time fixed effects, are shown in Appendix Table A5. They are omitted from Table 2 due to the similarity of the estimates.

[15] In further robustness checks shown in Appendix Table A13, I restrict the analysis to the non-tradable establishments of non-tradable firms so that establishments such as distribution centers which serve non-local geographical areas are also excluded.

correlated with the local conditions of firm headquarters.[16]

This variant of the second concern – as well as the earlier variant – can be addressed by controlling for the average local economic conditions across all establishment locations of the firm, weighted by establishment employment. This control variable is calculated as

$$\overline{ur}_{it} = \frac{\sum_{e \in i} emp_{et} \times ur_{l_e,t}}{\sum_{e \in i} emp_{et}}, \tag{6}$$

for firm i headquartered in county c with a network of establishments indexed by e located in county l_e. I calculate the leave-own-county-out version of $\overline{ur}_{it}$ to avoid including the impact of own-county local conditions. If the average consumer of the firm is doing worse, this will be more strongly reflected in worse average economic conditions across all locations of the firm.[17] When the average local conditions of the firm $\overline{ur}_{it}$ are included as a control, the estimated coefficient on HQ local conditions ur_{ct} recovers the impact of HQ local conditions residualized by the firm network average local conditions. In other words, it recovers the impact of an "abnormal" HQ local conditions shock above and beyond its contribution to the weighted average local conditions of the firm.[18]

Column (7) of Table 2 estimates equation (4) but directly controls for the weighted average unemployment rate across all locations of the firm, $\overline{ur}_{it}$. The measured impact of HQ unemployment rates remains similar, with a coefficient of -1.76, which can be compared to the column (2) estimate, conducted on the same sample but without controlling for $\overline{ur}_{it}$, of -2.02. Worse customer economic conditions captured by HQ county unemployment rates are unlikely to explain the results, as the baseline effect of ur_{ct} remains similar, both with and without controlling for $\overline{ur}_{it}$.

A third potential confound is mechanical: idiosyncratic shocks to the firm may directly affect the HQ county unemployment rate as the HQ or nearby establishments of the firm hire or lay off workers, while simultaneously causing other non-HQ establishments to hire or lay off workers. This endogeneity issue can be ruled out in a number of ways. For one, I can restrict the sample to firms whose HQ county employment is a small fraction of total county employment so that any direct

[16] Consider the following hypothetical example: there are two types of households, high-income and low-income households. Low-income households currently suffer from worse employment prospects. The Holland, Michigan HQ firm sells primarily to low-income consumers, and Holland has a greater share of low-income households and hence worse local economic conditions than the Indiana-headquartered firm that sells primarily to high-income customers. Hence, even in the same county in northern California, the Holland HQ firm is still primarily selling to low-income consumers and the Indiana firm to high-income consumers.

[17] Firm HQs are more likely to be located in large metropolitan areas relative to non-HQ establishments, and large metropolitan areas contain more heterogeneous types of consumers. For example, counties with more firm HQs also have greater income inequality (as measured by the Gini Index in the 2010 American Community Survey), implying that HQ-heavy counties have more heterogeneous consumer types in the income dimension. In contrast, other smaller, more homogeneous counties in which the firm operates will have measured unemployment rates that better capture the economic conditions of the average consumer of the firm. Therefore $\overline{ur}_{it}$, or the average unemployment rate of the firm capturing the more homogeneous non-HQ counties in which the firm operates, is likely to be a better proxy for the employment prospects of the average consumer of the firm rather than ur_{ct}.

[18] While the ideal measure of firm network-weighted local conditions would use establishment sales weights rather than employment weights, sales weights are not available. Employment shares are likely to approximate sales weights particularly well in industries such as retail and restaurants.

or indirect effects of firm i on the HQ county unemployment rate are negligible. In column (8) of Table 2, I re-run the baseline regression restricting the sample to firms where the firm's HQ county employment as a fraction of total county employment is less than 2 percent. The estimated impact of HQ local conditions is almost exactly the same as that in column (2) on the entire sample.

In addition, I address this reverse causality concern in two other ways. First, I use the HQ metropolitan area unemployment rate leaving out the HQ county as the proxy for the local economic conditions for firms. Second, I compute a "leave-out" HQ county unemployment rate for firm i, where I assume that the change in firm i's HQ county employment from year $t-1$ to t directly contributes to the number of unemployed individuals. Appendix Table A7 presents results using these two variables as the explanatory variables of interest.

A fourth, more general concern is that the firm-wide investment opportunities of firms headquartered in certain areas is driven by another omitted variable correlated *only* with the vibrancy of the HQ area (and not with the vibrancy of the other non-HQ establishment locations). For example, this would be a concern if firms headquartered in Holland, Michigan generically face favorable investment opportunities during local booms.[19] This cannot be addressed by controlling for the weighted average local conditions of the firm, or by using placebo tests with non-HQ local economic conditions as the explanatory variable, which I discuss in Section 4.3, because the unobserved confound is specific to the HQ location only. However, this confound implies that firms with temporarily worse HQ local economic conditions will, on average, experience lower ex post firm-wide sales or profitability growth due to worse firm-wide investment opportunities. Firm-level tests of these predictions are shown in Appendix Table A6. Results with two-year sales growth or change in profits as outcomes do not support this explanation, as higher HQ unemployment rates are not associated with lower sales growth or negative changes in profitability.

4.2 Extensive versus intensive margin impact

Are the effects of HQ local conditions on establishment employment growth concentrated on the intensive or extensive margin? As the outcome variable of interest is defined to include extensive margin changes, I can decompose it into the sum of intensive and extensive margin components:

$$\begin{aligned}\Delta y_{t+1} &= \frac{emp_{t+1} - emp_t}{0.5 \times (emp_{t+1} + emp_t)} \\ &= \mathbf{1}_{\{\text{ext}\}} \Delta y_{t+1} + \left(1 - \mathbf{1}_{\{\text{ext}\}}\right) \Delta y_{t+1}.\end{aligned}$$

Thus, the impact of HQ local conditions can be decomposed into the sum of its impact on the extensive margin of employment growth, $\mathbf{1}_{\{\text{ext}\}} \Delta y_{t+1}$, consisting of openings and closings of establishments, and on the intensive margin, $\left(1 - \mathbf{1}_{\{\text{ext}\}}\right) \Delta y_{t+1}$.

Table 3 presents the results from regressions on these two components of growth. For example,

[19] Systematic differences across HQ locations in investment opportunities will be absorbed by firm establishment fixed effects. For example, if Jasper, Indiana firms always have better investment opportunities than Holland, Michigan firms, this difference will be captured by firm establishment fixed effects.

columns (1) and (2) of Table 3 decompose the result from column (2) of Table 2: the coefficient estimates on ur_{ct} in column (1) and column (2) sum to that of column (2) of Table 2. Analogously, every subsequent pair of columns from Table 3 corresponds to the decomposition of the regression from columns (4), (5), (6), and (7) of Table 2 into the extensive and intensive margin components, respectively. The decomposed results show that the baseline results are concentrated on the extensive margin. Openings and closings of establishments account for approximately 90% of the magnitude of the total employment growth effects.[20]

These findings suggest that *investment* is sensitive to HQ local conditions, as firms are opening and closing establishments in response to HQ local conditions, which is a margin of adjustment more strongly associated with investment than the margin of adding (or shedding) employees at an existing establishment.[21] Moreover, for larger firms, senior executives at firm HQ likely have significantly more input in decisions on opening and closing establishments, rather than in choosing the specific staffing level at a particular establishment.[22] These results are consistent with HQ local conditions overweighting, as the impact of HQ local conditions is concentrated on the margin of adjustment where HQ is more likely to be actively involved.

4.3 Internal capital markets explanation

In addition to the unobserved demand and investment opportunity confounds, I consider a number of alternative explanations for the baseline results. The first is that re-allocation or internal capital markets theories of the firm can explain the results. The re-allocation explanation is that the firm is moving employment to locations that have lower labor costs in response to weak local labor market conditions. If the HQ area has higher unemployment rates, it may have lower wages as well, leading firms to hire more at HQ and less at non-HQ establishments.[23]

Internal capital markets theories of the firm can potentially also explain the results. Scharfstein and Stein (2000) argue that agency frictions within the firm can lead to cross-subsidization across business divisions of the firm, whereby divisions with weaker investment opportunities are allocated a larger investment budget than otherwise optimal in a friction-less world. This theory predicts that firms cut their investment not only at HQ but also at establishments far away from HQ in response to worse demand conditions at HQ, as the HQ is cross-subsidized by parts of the firm

[20] In a further decomposition of the extensive margin effect into the impact on openings and closing, I find that just over 60% of the extensive margin effect is driven by opening new establishments (untabulated).

[21] Firm-level regressions of the impact of HQ unemployment rate on investment are shown in Appendix Table B1. I also discuss how I test whether HQ local conditions are overweighted in their impact on investment in Appendix Section B.1.

[22] For instance, Ton (2012) notes that in the retail industry store managers have significant control over payroll costs at their individual store (i.e. both hours and number of employees). However, the manager of a specific store in the retailer is unlikely to have significant control on opening or closing other stores, which are decisions made by senior managers at HQ.

[23] In Appendix Table A8, I present results from regressions where I control for HQ county employment changes. If cost-driven re-allocation explains the results, then firms expanding employment more at HQ will grow employment less at non-HQ establishments. The results controlling for HQ employment growth do not show such an effect, although caution must be taken in interpreting the results due to "bad control" issues when controlling for a possible outcome variable such as contemporaneous HQ employment growth (e.g., Angrist and Pischke 2009).

facing healthier regional demand.[24]

Crucially, the re-allocation and internal capital markets explanations can be empirically differentiated from overweighted local conditions because the latter explanation predicts that non-local employment growth sensitivity to HQ local conditions is stronger than non-local sensitivity to non-HQ establishment local conditions, while the former does not. Therefore, a number of placebo tests can be undertaken in order to see if HQ local conditions are overweighted relative to local conditions at other locations of the firm. First, I perform a simple test in which I repeatedly run the baseline specification in equation (4) but replace the HQ county unemployment rate with that of randomly selected non-HQ establishments.[25] As the average random non-HQ establishment is not as important to the firm as the HQ, I run a second variant of this placebo test in which I construct a "synthetic" placebo HQ which consists of a number of non-HQ establishments that cumulatively sum to the average HQ employment share of the firm. In other words, I randomly select establishments $e_1, e_2, ..., e_p$ until the cumulative employment share $\sum_{n=1}^{p} emp_{e_n}/emp_i$ for firm i at these p establishments is equal to the average HQ employment share in the sample.[26]

The results of these two placebo tests can be seen in Figure 4 and Figure 5, which plot the distribution of the placebo unemployment rate sensitivity for one thousand sets of randomly selected placebo HQs. In both these figures, the blue dashed line plots the magnitude of the baseline estimated sensitivity of non-local establishment growth to the HQ county unemployment rate (corresponding to column (2) of Table 2). The gray bars are counts of the frequency of the placebo non-HQ unemployment rate sensitivity estimates, plotted by coefficient magnitude on the x-axis.

The results of both the basic placebo establishment test in Figure 4 and the synthetic placebo HQ establishment test in Figure 5 strongly reject the null hypothesis that the baseline employment growth sensitivity to HQ unemployment rates is the same as the average employment growth sensitivity to non-HQ or synthetic placebo HQ unemployment rates (at the 1% level of statistical significance). These results imply that firms *overweight* HQ local economic conditions in choosing non-HQ employment growth, even compared to their employment growth sensitivity to non-HQ local conditions.

In addition, the placebo tests quantify the proportion of the baseline estimates that are potentially caused by internal capital markets and reallocation. Based on the mean estimates of the impact of the placebo HQ or synthetic placebo HQ unemployment rates on non-local establishment employment growth, we can attribute 5 to 9 percent of the estimated baseline effect to internal

[24] Giroud and Mueller (2017) find evidence for these effects in response to local shocks during the Great Recession, and argue that a shock to investment opportunities in one part of the firm can spill over to other parts of the firm if the firm is financially constrained. Other studies find evidence in favor of internal capital markets as well (e.g., Lamont 1997, Rajan, Servaes and Zingales 2000, Xuan 2009, Ozbas and Scharfstein 2010, Giroud and Mueller 2015, Bord, Ivashina and Taliaferro 2015).

[25] As in the baseline regressions, I exclude observations of establishments in the same state as the selected placebo non-HQ establishment.

[26] Firms where the HQ employment share is very high are excluded from this sample, as there must be a sufficiently large cumulative employment share at non-HQ establishments in order to be able to construct the synthetic placebo HQ. To ensure comparability, the impact of the HQ unemployment rate to which I compare the synthetic placebo HQ estimates is re-estimated on this smaller sample of firms.

capital markets and re-allocation. While the average impact of non-HQ establishment unemployment rates on non-local establishment employment growth is negative, it is small and close to zero. This suggests that the identifying assumption of the baseline regressions – namely, that a rational expectations null hypothesis predicts no impact of HQ local conditions on non-HQ establishment employment growth – is plausible.

Finally, I run a different test where I add controls for the local conditions of the five counties of the firm with the most employees to the baseline regression. I select five counties in five separate states to avoid estimating the impact of five geographically proximate counties' local conditions. These regressions test whether HQ county unemployment rates still affect non-HQ establishment employment growth incrementally, even after controlling for the local economic conditions of the firm's largest establishments. The results are shown in Table 4. The coefficient on the HQ unemployment rate variable remains significant and similar in magnitude to the baseline estimates found in Table 2.[27]

4.4 Local financing explanations

A second alternative explanation is that local economic conditions at firm HQ proxy for the financing conditions of the firm. First, local economic conditions at firm HQ may proxy for credit availability at local banks from which firms have borrowing relationships. If worse local economic conditions lead to reduced credit supply from local banks, firms that borrow from these banks may have greater difficulty obtaining loans, and thus reduce their investment and employment. A number of studies document the links between geography and equity capital markets as well as bank lending, suggesting that differences across regions in capital availability.[28] Second, local economic conditions at HQ may proxy for the HQ real estate collateral value of the firm. If HQ local economic conditions are worse, HQ real estate will have lower value and firms will not be able to borrow as much using their real estate as collateral (e.g., Chaney, Sraer and Thesmar 2012).

I test these hypotheses in a few ways. First, I restrict the analysis to firms that have bond market access. For these large firms with bond market access, it is unlikely that their marginal investments are being funded by borrowing from small local banks. Using Compustat data on firms that have a bond rating, I define a firm i at time t as having bond market access if it has a rating at time t or earlier (e.g., Faulkender and Petersen 2006).

Estimates of the baseline regression on the subsample of firms with bond market access are shown in column (1) of Table 5. The magnitude of the impact of HQ unemployment rate (-2.22)

[27]The sample is reduced as the definition of non-local establishments must exclude not only establishment in the HQ county (or state), but also the establishments in the county (or state) of the five largest establishments. By construction, any firms without presence in at least five states are excluded. In Table 4, if the regression sample excludes the HQ county and the five largest counties, the column is labeled "Excl cty," whereas if the sample excludes the HQ state as well as the five states in which the five largest counties, the column is labeled "Excl state."

[28]For example, see Coval and Moskowitz 1999, Coval and Moskowitz 2001, Becker 2007, Hong, Kubik and Stein 2008, Becker, Ivković and Weisbenner 2011, Garcia and Norli 2012, Adhikari, Cicero and Sulaeman 2017. Separately, Dougal, Parsons and Titman (2015) argue that spillovers in urban areas may explain why firms' exhibit investment sensitivity to other local firms' Tobin's Q.

is similar to that among the entire sample of firms (-2.02). In column (2), I estimate the baseline regression among the subsample of firms without bond market access. The impact of HQ local conditions is smaller in magnitude for the sample of firms without bond market access relative to firms with bond market access, although the difference is not statistically significant.

Second, I divide the sample between firms that are more and less financially constrained. If either local bank relationships or real estate collateral explains the non-HQ employment growth sensitivity to HQ local conditions, then the effects should be stronger for the sample of firms that are more financially constrained, as those firms have higher investment-to-financing sensitivity. Splitting the sample along multiple proxies for financial constraint – dividend payers versus non-payers, more versus less levered firms, firms with more versus less cash – I do not find evidence that the impact of HQ local conditions is stronger among the more financially constrained. Results are shown in columns (3) through (8) of Table 5. Columns (3), (5), and (7) present estimates on the unconstrained (or less constrained) subsample. Across all these regressions, the impact of HQ local conditions is, if anything, stronger in the less constrained sample of firms (the differences in the HQ local conditions coefficient are significant at the 5% level across columns (3) and (4), significant at the 5% level across columns (5) and (6), and not significant across columns (7) and (8)).[29]

One possible explanation for why more financially constrained firms, such as non-dividend paying firms, have lower employment growth sensitivity to HQ local economic conditions, could be due to systematic differences across these groups of firms in their exposure to the macroeconomic cycle. Indeed, I find that non-dividend paying firms mention the macroeconomy less in their financial filings on average, suggesting perhaps that non-dividend paying firms are less sensitive to the macroeconomic cycle, and hence less likely to attempt to forecast the macroeconomy in the first place.[30] Separately, one might expect more financially constrained firms to be restricted to investing only in projects with expected returns significantly above their hurdle rate, such that the impact of HQ local economic conditions on the perceived returns of these projects is too small to affect whether they are undertaken. A simple model in Appendix C yields this prediction.

The real estate collateral variant of the explanation predicts that the effects will be concentrated among firms that own real estate or, more precisely, among firms that own real estate in the HQ area. I divide the sample between firms that own more versus less real estate, and between firms that own versus rent their headquarters real estate, using data on HQ real estate ownership in 1997 from Chaney, Sraer and Thesmar (2012). Table 6 presents the results from these subsample tests. Across different splits of the sample, I find larger effects for the subsample with lower real estate or HQ ownership, but the difference in coefficients across samples is not statistically significant.

Finally, I conduct firm-level tests of the impact of HQ local conditions on debt issuance. Results

[29] An alternative interpretation is that the more constrained firms are *extremely* financially constrained and hence their investment is *not sensitive at all* to a small relaxation of the financial constraint (e.g., Bolton, Chen and Wang 2011). However, in the same regressions, the estimated impact of cashflow on employment growth is large in magnitude and statistically significant among the subsample of more constrained firms. This is at odds with the notion that these firms are extremely financially constrained.

[30] See Section 4.5 for a discussion of other tests comparing firms in industries that are more or less correlated with the macroeconomy.

are shown in Appendix Table A9. Worse HQ local conditions do not lead to less debt issuance, as predicted by the local financing explanation.

4.5 Local conditions overweighting and macroeconomic expectations

Another explanation for the results is that managers overweight HQ local economic conditions in forming their macroeconomic expectations. This explanation predicts that local economic conditions will be more strongly overweighted by firms that are more sensitive to the macroeconomy, since these firms are more likely to have managers who actually attempt to forecast the macroeconomy.

To test this, I split the sample between firms in industries that are more sensitive to the macroeconomy, as measured by either the average sales growth-to-GDP growth beta of firms in the industry, or the frequency with which firms in the industry mention the macroeconomy in their financial filings (10-Ks and 10-Qs).[31]

Results of regressions run on separate subsamples split along a measure of industry macroeconomic correlation ("Macro correlation"), or along mentions of the macroeconomy in the financial filings ("Macro salience") are presented in Table 7. Comparing coefficients between columns (1) and (2), as well as columns (3) and (4), I find stronger results in the high macro correlation and high macro salience subsamples. However, the difference between these pairs of coefficients is not statistically significant, even if the differences between the point estimates are economically meaningful: for example, the coefficient on ur_{ct} in column (1) is almost twice the magnitude of that in column (2).

Other firm characteristics may also be associated with differences in macroeconomic sensitivity. Dividend paying firms are more sensitive to macroeconomic conditions, compared to non-payers, as measured either by macroeconomic correlation or by macroeconomic "salience."[32] In the previous Table 5, columns (3) and (4) show that the impact of HQ local conditions is stronger for dividend payers than for non-payers (significant at the 5% level). These results support the notion that local conditions overweighting is caused by its impact on managers' macroeconomic expectations.

An alternative interpretation is that managers *rationally* weight local economic conditions as a signal for macroeconomic conditions, rather than overweighting them. However, this interpretation

[31] I tabulate counts of the following macroeconomic phrases: "economic activity," "economic climate," "economic conditions," "economic environment," "economic forces," "economic outlook," "global economy," "world economy," "worldwide economy", "domestic economy," "national economy," "U.S. economy," "entire economy," "general economy," "overall economy," "the economy," "macroeconomy," "macroeconomic," "business cycle," "economic cycle," "economic expansion," "economic contraction," "economic growth," "consumer confidence," "weak economy," "weakening economy," "soft economy," "softening economy," "strong economy," "strengthening economy," "growing economy," "booming economy," and "state of the economy." I then regress an indicator for the use of macroeconomic phrases per filing per firm on time fixed effects (to remove any time trends in macro phrase usage) and industry fixed effects and use the industry fixed effects as measure of industry macroeconomic salience.

[32] In the Compustat data from 1976 to 2015, I estimate the average sales growth beta to GDP growth for different groups of firms. In these regressions, the estimated beta of average sales growth to GDP growth is larger for dividend payers relative to non-payers. The sales growth to GDP growth beta is 1.17 for dividend-paying firms and 0.48 for non-dividend paying firms. These betas are estimated using quarterly data and year-on-year growth rates from 1976 to 2015. Moreover, macroeconomic conditions are more salient for dividend paying firms relative to non-payers, as measured by average mentions of the macroeconomy in the financial filings of these firms.

is unlikely for a couple of reasons. For one, with the ready public availability of macroeconomic news, data, and forecasts, the marginal signal value of local economic conditions is likely to be negligible. Moreover, even if we assume that local economic conditions can have incremental predictive power for the macroeconomy, the rational weighting interpretation implies that firms headquartered in areas that are more correlated with macroeconomic conditions will place greater weight on local economic conditions, because the economic conditions of those areas are more predictive. To test this, I split the sample by HQ location, comparing firms headquartered in areas with larger negative correlations of local unemployment rates to U.S. GDP growth (the "more predictive" locations) versus those located in less predictive areas. Results are shown in Appendix Table A10. Columns (1), (3), and (5) show estimates for firms with HQs in locations that have stronger correlation with the macroeconomic cycle. These estimates can be compared, respectively, to those in columns (2), (4), and (6) for locations with less correlation. These comparisons do not support the notion that firms headquartered in areas with greater macroeconomic correlation weight local economic conditions more strongly, as predicted by the rational interpretation.

4.6 Additional robustness checks

I discuss a number of robustness tests. First, I add the logarithm of establishment size and establishment age as controls to the baseline regressions from equation (4). I do not include these variables in the set of baseline controls because they suffer from "bad control" issues. Both size and age variables are mechanically linked to the employment growth outcome variable for establishment births.[33] The addition of "bad controls" biases the coefficient of interest, due to a type of selection bias (Angrist and Pischke 2009). The results using establishment-level controls are shown in Appendix Table A11. The impact of HQ unemployment rate remains significant and negative on employment growth, although it is smaller in magnitude than in regressions without establishment-level controls. One would expect the estimated HQ unemployment rate effect to be smaller in magnitude once establishment-level controls are added as these controls mechanically absorb employment growth variation for new establishment openings, and the impact of HQ local conditions is concentrated on the extensive margin of employment growth (i.e., establishment openings and closings). In fact, the smaller magnitude of the effect size in the regressions with establishment controls, relative to regressions without establishment controls, is consistent with the likely sign of the bad control bias.[34]

Second, I show results from additional specifications for the subsample of non-tradables firms.

[33] If a new establishment opens between time t and $t+1$, the outcome variable for that observation will be equal to 2, while the initial size and age variables will both be 0.

[34] Consider a simplified version of the estimator, where HQ local economic conditions are either poor, $HQ_i = 1$, or normal, $HQ_i = 0$, and establishments have age A either "young" or "old," and the outcome variable Y is establishment employment growth. Controlling for establishment age, we thus estimate, for instance, for young firms: $\mathbb{E}\left[Y_i | A_i = \text{young}, HQ_i = 1\right] - \mathbb{E}\left[Y_i | A_i = \text{young}, HQ_i = 0\right]$. If we assume random assignment of HQ_i, we can decompose this estimate as

$$\underbrace{\mathbb{E}\left[Y_{1i} - Y_{0i} | A_i = \text{young}\right]}_{\text{Causal impact}} + \underbrace{\left\{\mathbb{E}\left[Y_{0i} | A_{1i} = \text{young}\right] - \mathbb{E}\left[Y_{0i} | A_{0i} = \text{young}\right]\right\}}_{\text{Selection bias}},$$

As the subsample of non-tradable firms mitigates a number of potential confounds, I present a number of alternative estimates restricting to this subsample: employing county-by-industry-by-year fixed effects, controlling for weighted average firm unemployment rate $\overline{ur}_{it}$ (defined in Section 4.1), and adding establishment-level controls. Results of these alternative specifications are shown in Appendix Table A12. In addition, to address the possibility that non-tradable firms may have business divisions that are in tradable industries, or have establishments serving non-local areas (e.g., regional distribution centers), I re-run the regressions on the subsample of non-tradable establishments of non-tradable industry firms. These results are shown in Appendix Table A13.

Third, I present results using different distance-based thresholds for excluding establishments close to HQ. In the baseline analysis, I restrict the sample to establishments not in the HQ state to avoid capturing the direct impact of HQ demand conditions on nearby establishments. In Appendix Table A4, I restrict the sample to establishments not in the HQ state nor in a geographically adjacent state, then to establishments not within 250 miles of the HQ county, and finally to establishments not within 500 miles of the HQ county. Estimates remain essentially unchanged relative to those obtained on the baseline sample (not in HQ state).

Fourth, I re-run the baseline regression but restrict the sample to the set of large, geographically dispersed firms. The firms in these samples are not concentrated geographically near HQ. Appendix Table A14 shows the results of this robustness exercise. The estimates I obtain are comparable to those on the baseline sample of all firms.

Fifth, I re-run the analyses with zipcode-by-year, or zipcode-by-industry-by-year fixed effects, to restrict the identifying variation to establishments of different firms in the same zipcode-by-year (or zipcode-by-industry-by-year) cell. The cost of these more geographically precise fixed effects is a loss in sample size due to the higher number of zipcode-by-year and zipcode-by-industry-by-year cells with one observation only that are excluded from the estimation sample. Results from this test are shown in Appendix Table A15.

Sixth, as mentioned previously, I restrict the baseline analysis to firms whose HQ does not move across metropolitan areas from 1990 onward. Results from this test are presented in Appendix Table A3.

Seventh, I use payroll growth as the outcome variable of interest, defining the outcome as

$$\Delta y_{e,t+1} = \frac{pay_{e,t+1} - pay_{et}}{0.5 \times (pay_{e,t+1} + pay_{et})},$$

where $\{Y_{1i}, Y_{0i}\}$ denote hypothetical employment growth for establishment i if $HQ_i = 1$ or $HQ_i = 0$, respectively, and similarly$\{A_{1i}, A_{0i}\}$ denote hypothetical establishment age for establishment i if $HQ_i = 1$ and $HQ_i = 0$, respectively. The first term is the causal impact of HQ local conditions on young firms. The second term captures selection bias. It consists of the difference in average hypothetical normal-HQ condition employment growth for establishments that are young (and hence exist) when HQ conditions are poor, and the average hypothetical normal-HQ condition employment growth for establishments that are young and exist when HQ conditions are normal. The former is likely to be greater than the latter, as establishments that exist even when HQ conditions are poor are likely to have unobservably better investment prospects than establishments that exist when HQ conditions are normal. This implies that the selection bias term will be positive, which leads to a less negative estimate relative to the true causal estimate, when controlling for establishment age.

and then multiplying it by one hundred. I re-run the baseline analyses, and show results from these regressions in Appendix Table A16.

5 Impact of local conditions on expectations

This section presents tests of whether HQ local economic conditions are overweighted in the formation of managers' *expectations*. I use data on management sales guidance and show that worse local economic conditions predict more pessimistic managerial forecast errors. Then, using a measure of macroeconomic sentiment based on text from firms' financial filings, I show that HQ local economic conditions have overweighted impact on macroeconomic sentiment. Separately, in Appendix Section B.2, I present evidence that HQ local conditions are overweighted in their impact on expectations, where I measure expectations based using a machine-learning approach on text data from financial filings.

5.1 Sales prediction errors

If local economic conditions are overweighted, worse HQ local conditions will cause more pessimistic managers' sales expectations *errors*.[35] In contrast, a rational expectations explanation of the firm's investment and expectations sensitivity to HQ local conditions predicts no impact of HQ local conditions on sales expectations errors.

Using data on annual management sales guidance from the Thomson Reuters I/B/E/S database from 2002 to 2015 as a proxy for management sales expectations, I define sales prediction error as

$$error_{iy} = \frac{sales_{iy} - \widehat{sales}_{iy}}{sales_{i,t-1}},$$

for firm i and annual time period y where y ends after guidance announcement period t, where $sales_{iy}$ is the *ex post* realized sales for time period y, $\widehat{sales}_{iy}$ is sales guidance, and $sales_{i,t-1}$ is the realized lagged sales for the past four quarters.[36] I multiply $error_{iy}$ by one hundred, so it is in percent units. For example, suppose a company is providing sales guidance for the upcoming 2014 year in December 2013. Then sales guidance error in December 2013 would be calculated as realized sales in 2014 minus predicted sales in 2014, divided by sales in 2013.

To test whether higher HQ unemployment rates are associated with greater sales prediction error, I estimate

$$error_{iy} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it}, \tag{7}$$

where $\mathbf{x}_{it}$ is a set of firm-level controls and ur_{ct} is the HQ unemployment rate. Figure 6 illustrates

[35] Sales expectations are likely to be the most direct proxy for management demand expectations. Moreover, short-term future sales are a quantity that managers have less discretionary control over, relative to other variables such as capital expenditures, or earnings per share that managers also forecast. Managers face strong incentives to avoid negative earnings surprises (e.g., Skinner and Sloan 2002).

[36] If sales guidance is given as a range, I use the midpoint of the range as the value of $\widehat{sales}_{iy}$.

results from this regression using a binned scatterplot, and shows that worse HQ local conditions are associated with more pessimistic sales forecast errors. Results from different variants of regression (7) are presented in Table 8. Columns (1), (2), (5) and (6) include the sample of all firms, whereas the other columns exclude financial firms and utilities. The magnitude of the estimates imply that a 1 p.p. higher HQ unemployment rate leads to 0.34 to 0.52 p.p. greater under-forecasting of *ex post* realized sales. In other words, sales forecast errors are more pessimistic when HQ local conditions are worse. Based on the estimate in column (8), a one standard deviation higher HQ unemployment rate (1.56 p.p.) causes a 4.5 percent of a standard deviation more pessimistic sales guidance error. These results are inconsistent with rational expectations explanations of the impact of HQ local conditions on real outcomes, because rational expectations explanations predict zero correlation between HQ unemployment rates and sales forecast errors.

5.2 Impact on macroeconomic sentiment

5.2.1 Definition of macroeconomic sentiment

To obtain a measure of how positive or negative managerial sentiment is towards the macroeconomy across a larger group of firms (and years), I turn to the text of firms' financial filings. These filings consist of annual and quarterly reports (10-Ks and 10-Qs), available from 1995 onward.[37] As managers have direct input, the text of the filings reflects both the information and sentiment of managers (e.g., Tetlock, Saar-Tsechansky and Macskassy 2008, Li 2010, Loughran and McDonald 2011).

I construct a measure of macroeconomic sentiment using counts of negative (and positive) economic words in the sentences of the filings discussing macroeconomic conditions. Intuitively, I seek to define a variable that captures the sentiment of managers' macroeconomic expectations as reflected in the text of the firms' filings. First, I select all sentences that contain any of the following phrases: "economic activity," "economic climate," "economic conditions," "economic environment," "economic forces," "economic outlook," "global economy," "world economy," "worldwide economy", "domestic economy," "national economy," "U.S. economy," "entire economy," "general economy," "overall economy," "the economy," "macroeconomy," "macroeconomic," "business cycle," "economic cycle," "economic expansion," "economic contraction," "economic growth," "consumer confidence," "weak economy," "weakening economy," "soft economy," "softening economy," "strong economy," "strengthening economy," "growing economy," "booming economy," and "state of the economy". I refer to these as "macroeconomic sentences," as macroeconomic conditions are an obvious topic in these sentences. Second, I count the number of the times negative or positive economic words are used in these macroeconomic sentences, limiting to the set of positive and negative words that are

[37] I thank Bill McDonald for generously sharing the raw text filing data.

commonly used to characterize macroeconomic conditions.[38]

I then perform a two-step procedure to test whether HQ local economic conditions are overweighted in their impact on macroeconomic sentiment. First, I estimate the equation:

$$sentiment_{it} = \alpha_1 ur_{ct} + \mathbf{x}_{it}^{'}\alpha_2 + \delta_i + \delta_{ind,t} + \epsilon_{it}, \tag{8}$$

for firm i and quarter t, where $\mathbf{x}_{it}$ is a set of firm-level controls, $sentiment_{it}$ is either the logarithm of the count of negative economic words from the macroeconomic sentences, normalized, or the net count of negative economic words minus positive economic words from the macroeconomic sentences, normalized, and ur_{ct} is the HQ unemployment rate.[39]

However, because the measure of macroeconomic sentiment is at the firm-level, this cross-sectional regression cannot reject a rational expectations explanation of the impact of local conditions. Therefore, in a second step, I perform a placebo test where I compare the estimated impact of HQ county unemployment rate on macroeconomic sentiment to the average impact of a randomly selected non-HQ establishment's unemployment rate. Assuming that the non-HQ impact on macroeconomic sentiment is the rational expectations baseline, this comparison tests whether HQ local economic conditions have *overweighted* impact on macroeconomic sentiment.

5.2.2 Results

Results from estimating equation (8) using the sample of all Compustat firms that merge to the SEC filing data from 1995 to 2014 are shown in Table 9. The estimate in column (1) implies that a one p.p. higher HQ unemployment rate leads to 3.17% of a standard deviation more negative macroeconomic sentiment. Restricting the sample to larger firms less likely to be locally concentrated at their HQ, the estimates are similar: in column (3), among firms that have at least $500M in assets, a one p.p. higher HQ unemployment rate leads to 2.47% of a SD more negative expectations. I obtain quantitatively similar estimates when I define macroeconomic sentiment as net negative word counts in macroeconomic sentences in columns (4) through (6).

I employ placebo tests to see whether managers *overweight* HQ local conditions in their macroeconomic sentiment. In the first placebo test, I replace the HQ unemployment rate ur_{ct} in estimating equation (8) with the county unemployment rate of a randomly selected non-HQ establishment of the firm. In the second placebo test, I replace the HQ unemployment rate ur_{ct} in equation (8) with the average unemployment rate of a synthetic placebo HQ, made up of multiple establishments whose cumulative employment share is equal to the average HQ employment share of the firm. To

[38] I classify the following as negative economic words: "downturn," "downturns," "unemployment," "recession", "recessions", "recessionary," "slowdown," "slowdowns," "crisis," "crises," "depressed," "sluggish," "sluggishness," "stagnant," "stagnation," "downward," "slow," "slowing," "slower," "slowed," "slowly," "slowness," "poor," "worsen," "worse," "worsened," "worsens," "worsening," "deterioration," "deteriorated," "deteriorate," "deteriorates," "deteriorations." The following are classified as positive economic words: "improve," "improved," "improvement," "improving," "improvements," "positive," "strong," "strength," "stronger," "strengthening," "strengthen," "strengths," "strengthened," "booming."

[39] Previous studies of text from financial filings find that negative words have stronger signal value (e.g., Loughran and McDonald 2011).

conduct these tests, I merge the Compustat and SEC filing text data to the Census LBD database. For the observations that can be linked across these datasets, I re-estimate equation (8) using HQ local conditions, and compare this estimate to the distribution of placebo estimates of equation (8), where the HQ county unemployment rate is replaced with the unemployment rate of random non-HQ establishments' counties.

Results from the first placebo test are shown in Figure 7. Comparing the baseline estimate (blue dashed line) to the distribution of placebo estimates, we find that the HQ effect is more negative than the 1st percentile of estimated non-HQ placebo effects. Results from the second test using synthetic placebo HQ local conditions are shown in Figure 8. Here as well, the HQ effect is more negative than the 1st percentile of estimated non-HQ synthetic placebo effects.[40]

Using the mean placebo estimate, I calculate the extent to which managers overweight HQ local conditions in forming their macroeconomic sentiment, assuming that the non-overweighted impact of local conditions is captured by the mean impact of the placebo non-HQ local conditions. Based on the two placebo distributions, this implies that 78 to 89 percent of the estimated impact of HQ local conditions on macroeconomic sentiment in Table 9 can be attributed to *overweighting* HQ local economic conditions. This estimate is calculated by subtracting the mean placebo HQ sensitivity from the estimate of HQ local conditions sensitivity, and dividing this difference by the HQ sensitivity estimate. Thus, a one SD higher HQ unemployment rate (1.56 p.p.) causes 3.8 to 4.4 percent of a SD worse macroeconomic sentiment due to overweighted local conditions.

6 Aggregate implications

I present evidence that managers' overweighting of HQ local economic conditions affects county-level economic outcomes. Moreover, overweighting HQ local economic conditions will lead to misallocated investment between firms. I provide a simple back-of-envelope estimate for the potential extent of investment misallocation.

6.1 Impact of overweighted local conditions on county economies

The existence of local conditions overweighting among large, geographically dispersed firms suggests the possibility that it could help explain regional differences in economic performance across the U.S. If counties have different exposures to the average local conditions of *other counties* via the headquarters of establishments in their own county, can that help explain differences in county economic outcomes?[41]

[40] The estimate of the baseline impact of HQ unemployment rate on macroeconomic sentiment differs between the two placebo tests because the samples are different. In Appendix Figure B4, the placebo synthetic HQ regressions can be estimated only for firms with sufficient non-HQ employment share. Hence the impact of HQ unemployment rate on macroeconomic sentiment is re-estimated on the same subsample in order to be exactly comparable to the placebo synthetic HQ estimates.

[41] General equilibrium effects may offset the impact of local conditions overweighting on regions, as competitor firms respond by adjusting their investment and employment in response. Competitor firms may invest more when in the same market as overly pessimistic firms (and vice versa when competing with excessively optimistic firms). However, firms may not internalize that their competitors are affected by this bias.

To answer this question empirically, I estimate the following regression:

$$y_{c,t+1} = \beta_1 \overline{HQur}_{ct} + \mathbf{x}_{ct}^{'}\beta_2 + \beta_3 y_{ct} + \delta_c + \delta_t + \epsilon_{ct}. \tag{9}$$

$\overline{HQur}_{ct}$ is calculated as the establishment employment-weighted average HQ county unemployment rate of all Compustat-linked establishments in county c that are not headquartered in the state of county c, $\mathbf{x}_{ct}$ is a vector of controls, including industry employment shares in county c, as well as establishment employment-weighted firm controls of Tobin's Q, cashflow, size, sales growth, and cash level, and y_{ct} is the lagged outcome variable. δ_c and δ_t are county and time fixed effects, respectively. The county-level outcomes $y_{c,t+1}$ are the employment growth rate and the establishment birth minus establishment death rate, calculated in the Census LBD data using all active employer establishments in county c. I use establishment births and deaths as an outcome, because the firm-level results show that the effects of overweighted local conditions are concentrated on the extensive margin of growth. As the analysis relies on Compustat data on HQ locations, I restrict the sample to county observations where the firms in the merged Compustat-LBD sample contain a greater-than-median share of county employment to increase the power of the tests. Standard errors are clustered at the county level.

These county-level regressions face additional and different potential confounds compared to the establishment-level tests in Section 4 that exploit variation within county-by-year cells. Here, a key new concern is that underlying patterns of spatial correlation in economic outcomes across counties unrelated to expectations overweighting of HQ local conditions may affect the results. For example, county c economic outcomes may be correlated with those of counties where the establishments of county c are headquartered due to geographic proximity. I attempt to address this concern in a few ways. First, $\overline{HQur}_{ct}$ is calculated using only establishments which are headquartered outside the state of county c, thus excluding close counties. Second, I add the lagged outcome variable y_{ct} to control for potential correlation between the local conditions at the headquarters of establishments in county c with local conditions in county c itself. Finally, I re-run the analysis with the outcome variable y defined only for the non-tradable establishments in county c, to avoid concerns that the results may be driven by demand from HQ counties being met by production in county c. In those regressions, I define the explanatory variable, $\overline{HQur}_{ct}^{NT}$, as the weighted average HQ county unemployment rate of all Compustat-linked firms with non-tradable establishments.[42]

The results of these regressions are shown in Table 10. Columns (1) through (4) present estimates of equation (9) where all employer establishments in a county are included. A higher average HQ unemployment rate leads to lower employment growth as well as a lower birth minus death rate of establishments. The estimate in column (2) implies that a 1 p.p. higher average HQ unemployment rate for establishments in county c leads to 0.29 p.p. lower county employment growth. As one standard deviation of employment growth in the sample is 9.5 percent, this implies that a

[42] I also perform county-level placebo tests, by estimating equation (9) but replacing $\overline{HQur}_{ct}$ with the unemployment rate of a randomly selected out-of-state county. These placebo tests (results not shown) strongly reject the hypothesis that the average placebo county unemployment rate impact is equal to the estimated effect of $\overline{HQur}_{ct}$.

1 SD higher $\overline{HQur}_{ct}$ leads to 3.1% of a SD lower employment growth rate. Similarly, the estimate in column (4) implies that a 1 SD higher $\overline{HQur}_{ct}$ leads to 3.2% of a SD lower establishment birth minus establishment death rate.

The results in columns (5) through (8) of Table 10 present the results where outcome variables are defined for establishments in non-tradable industries only. The estimates suggest similar-sized effects of average HQ local conditions. For example, column (6) implies that the impact of a 1 p.p. higher average HQ unemployment rate for non-tradable establishments in county c leads to 0.24 p.p. lower non-tradable employment growth (3.5% of a standard deviation), and the estimate in Column 8 implies that a 1 p.p. higher $\overline{HQur}_{ct}^{NT}$ leads to 0.22 p.p. lower birth minus death rates of non-tradable establishments (3.3% of a standard deviation).

We can compare the county-level regressions to the establishment-level results in Table 2, even if there are a number of reasons why the estimates may differ. For instance, the county-level coefficients will include any potential spillovers on other establishments arising from the direct firm-level impact of local conditions overweighting. However, it is reassuring that the county-level estimate of employment growth in column (2) of Table 10 is similar to the the -2.02 p.p. estimate in column (2) of Table 2, after adjusting for the fact that the average county in the sample of column (2) of Table 10 has approximately 18 percent linked Compustat employment share. In other words, $\overline{HQur}_{ct}$ measures the HQ local conditions for 18% of employment of an average county in the sample. If I simply extrapolate the establishment-level estimate (2.02 p.p. decline in employment growth for a 1 p.p. higher HQ unemployment rate), it suggests that county-level employment growth should decline 0.36 p.p. for a 1 p.p. increase in average HQ unemployment rate for 18% of the county. This is close to the 0.29 p.p. estimate from column (2) of Table 10, suggesting that the county-level results are consistent, in terms of order-of-magnitude, with the results of the establishment-level regressions.

If differences in expectations due to differences in HQ local conditions affect county economic outcomes, this suggests the existence of another channel via which regional fiscal stimulus can potentially affect other regions.[43] In particular, localized fiscal stimulus targeting areas with many headquarters of large, dispersed firms may affect the investment at other non-local establishments of these firms due to its impact on the expectations of managers at HQ. This channel of regional stimulus will not be captured by the cross-sectional empirical strategies used in recent studies (e.g., Chodorow-Reich, Feiveson, Liscow and Woolston 2012, Nakamura and Steinsson 2014, Shoag 2015).

[43] Chodorow-Reich (2017) discusses one potential channel which is caused by a direct income effect leading local households to consume more output produced by other regions. In addition, there are a number of studies identifying the impact of regional fiscal stimulus by exploiting cross-sectional fiscal shocks (see Chodorow-Reich 2017 for a summary). Local conditions overweighting may attenuate estimates of cross-sectional multipliers, because increased fiscal stimulus in the "treated" area leads to higher investment in the "control" areas as well, potentially increasing the output of counterfactual control regions.

6.2 Misallocated corporate investment

If managers overweight local economic conditions in their investment and employment decisions, firms with strong HQ local conditions will overinvest more (or underinvest less) relative to firms facing worse local conditions.[44] Consequently, if there are diminishing marginal returns to investment, then the marginal investment productivity will be relatively lower for firms with stronger local conditions. I present evidence in Appendix Table A17 that shows that worse HQ local economic conditions are associated with higher marginal investment productivity, as proxied by changes in sales-to-fixed asset and sales-to-employment ratios.[45]

Here, I provide a back-of-the-envelope quantification of the aggregate amount of misallocated investment due to the overweighting of local conditions. To arrive at this aggregate figure, I use the firm-level estimates of the impact of local conditions overweighting on investment (results of which are described in Appendix Section B.1 and shown in Appendix Table B1, along with Appendix Figure B1 and B2) and make a few additional assumptions.

I illustrate the approach to estimating the aggregate amount of misallocated investment in Figure 9. In Panel A, I plot both the empirically estimated relationship between HQ local conditions and firm investment in the data (the red solid line), along with the counterfactual impact of HQ local conditions on firm investment if HQ local conditions are not overweighted (the gray dashed line). The red solid line is derived from regression estimates shown in Appendix Table B1, and plotted in Panel B of Figure 9 as a binned scatterplot. The no-bias counterfactual relationship between HQ local conditions and firm investment remains negative, due to the direct demand effects of HQ local conditions on HQ area investment and internal capital markets reallocation. The slope of the gray dashed line (the no-bias counterfactual) corresponds to the empirically estimated average synthetic placebo HQ effect on firm investment (shown separately in Appendix Figure B2). Total misallocated investment is captured by the shaded areas between the two lines: the area to the left of intersection of the red and gray dashed lines represents overinvestment due to strong HQ local conditions, whereas the area to the right represents underinvestment due to poor local conditions. I make the conservative assumption that there is no misallocation due to local conditions overweighting at the cross-sectional average of HQ local conditions (i.e., the point where the red and gray lines intersect), which yields the smallest estimate of total misallocated investment.

I then calculate the estimated contribution of local conditions overweighting to firm investment misallocation for each firm in the sample. For firm i headquartered in county c, I calculate the estimated misallocated investment as $\beta \times LCfrac \times (ur_{ct} - \overline{ur}_t)$. β is the estimated impact of the HQ unemployment rate on investment from column (6) of Appendix Table B1 of -0.0356. $LCfrac$ is the fraction of the estimated firm-level effect that can be attributed to local conditions

[44] Even if all firms suffer from under-investment (or over-investment) due to other agency or financial frictions, local conditions overweighting causes firms with strong local conditions to underinvest less (or overinvest more).

[45] Results from placebo tests, where ur_{ct} is replaced with the local conditions of a random placebo non-HQ firm establishment, reject the hypothesis that the effects shown in Table A17 are equally explained by the impact of non-HQ establishment local conditions. Results of these placebo tests are shown in Appendix Figures A1, A2, A3, and A4.

overweighting, recovered by calculating the share of the HQ local conditions impact on investment that is not attributed to the average synthetic placebo HQ impact. I obtain $LCfrac = 54.5\%$. Finally $ur_{ct} - \overline{ur}_t$ is the difference between the unemployment rate of firm i with HQ in county c at time t relative to the sample average HQ unemployment rate (across all firms) at time t.

This back-of-envelope calculation makes a number of restrictive assumptions. The first is that a firm with average HQ local conditions ($\overline{ur}_t$) has no misallocated investment. Second, I assume there are no general equilibrium effects of overweighted local conditions. If competitor firms recognize the over- or under-investment of their peer firms, they may "lean against the wind" with their investment, attenuating the aggregate amount of misallocated investment. On the other hand, if firms interpret the investment decisions of their peer firms as a signal of investment attractiveness, this will amplify the effects of the bias.

Using these assumptions, I calculate the percentage of investment that is misallocated due to local condition overweighting across Compustat firms with over $500M in assets, excluding financial and utility firms. The estimated amount of misallocated investment comprises 3.2% of total investment of these firms. As these firms invested a total of $715B in 2013, this back-of-the-envelope calculation suggests that misallocation due to local conditions overweighting led to $23B of misallocated capital expenditures in 2013 for these large public firms.

An estimate of total misallocated investment does not provide a complete picture of the extent of aggregate misallocation. Understanding the foregone aggregate output from misallocated investment requires an answer to a second question of *how* misallocated investment is for each unit of investment. This requires information on how much lower the returns on misallocated investment are relative to their use elsewhere. If we assume that the average investment has a useful life of 10 years, and that misallocated investments could earn 5% more annually if properly allocated, this implies that the annual $23B of misallocated capital expenditures represents $8.9B in foregone present value in profit.[46]

7 Conclusion

This study argues that managers overweight personal observations about the local economy in forming their expectations about the macroeconomy, and that this bias affects firm employment and investment policy. Exploiting within-firm Census data, I find that higher local unemployment rates at firm HQ lead to lower employment growth at establishments not close to the HQ. These effects are concentrated on the extensive margin, that is, opening and closing establishments. I consider a number of ways that the identifying assumption – that differences in HQ local conditions are irrelevant for differences in investment opportunities across non-HQ establishments in the same county, industry, and year – may be violated, and provide evidence that these do not explain the results. I also test key alternative explanations such as internal capital markets, using a placebo test, and local financing, using comparisons across different subsamples, and do not find evidence

[46] I use a discount rate of 5%.

that these explain the results. I find suggestive evidence that the effects are stronger for firms which are more strongly correlated with the macroeconomy that are more likely to be making macroeconomic forecasts.

I then show that local economic conditions are overweighted in their impact on managers' expectations. Higher local unemployment rates at firm HQ lead to more pessimistic sales forecast errors, which is directly at odds with rational expectations explanations. I also find that HQ local conditions have overweighted impact on managers' macroeconomic sentiment, as recovered from the text of financial filings.

Managers' biased weighting of local economic conditions has broader economic impacts. It helps explain differences in economic outcomes across counties. As well, it causes additional misallocation in investment across firms, with firms headquartered in worse-than-average locations under-investing relative to firms with better-than-average HQ local conditions. A back-of-the-envelope estimate implies that around $23B of investment of large public companies is misallocated every year due to local conditions overweighting.

In companion work, I find evidence of the same bias in different contexts. First, I find that households report more pessimistic expectations of national macroeconomic conditions when the local area where they reside faces worse economic conditions. Households overweight local economic conditions in forming macroeconomic expectations as well. Second, I find that differences in local inflation rates affect the the monetary policy votes of the presidents of regional Federal Reserve Banks who sit on the Federal Open Markets Committee (FOMC): relatively higher local inflation, in the cross-section, is associated with more "hawkish" voting.

These complementary results on household macroeconomic expectations and FOMC voting suggest that there are other relevant economic contexts in which local economic conditions will affect both the expectations of economic agents as well as their behavior in ways that may deviate from the rational expectations framework. These related questions I hope to explore in concurrent and future research.

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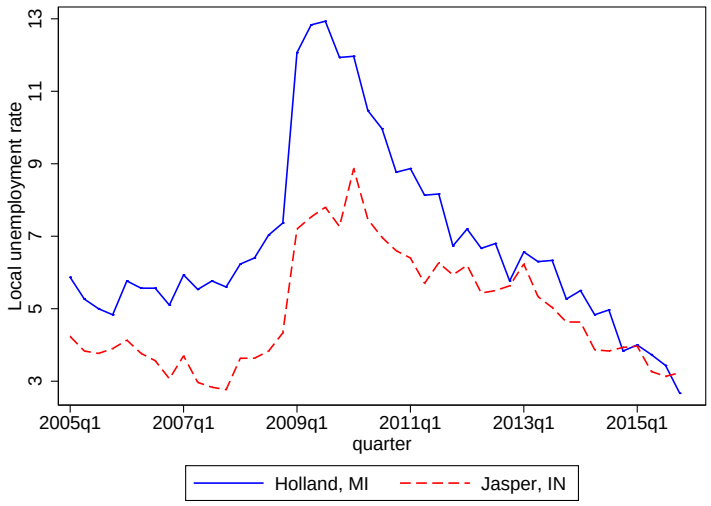
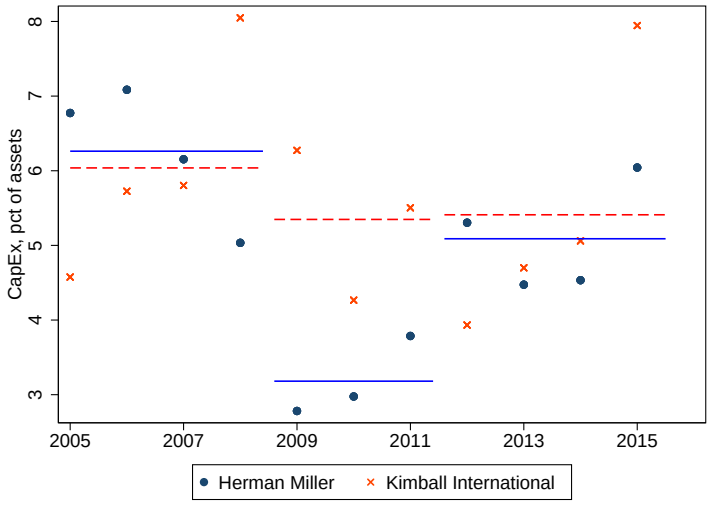
Figures

Figure 1: Herman Miller versus Kimball International
Panel A: Local unemployment rates

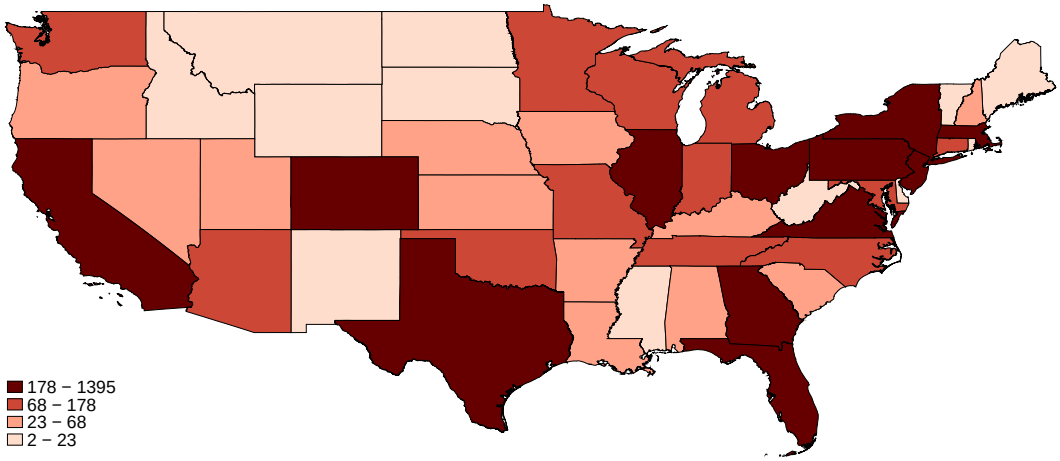


Panel B: Average investment levels



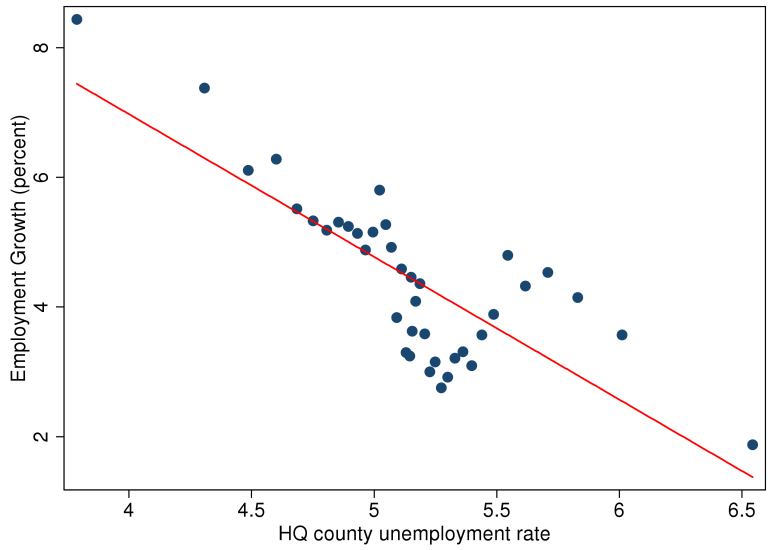
Notes: In Panel A, the county unemployment rate in Holland, Michigan (Ottawa county) and in Jasper, Indiana (Dubois county), where the headquarters of Herman Miller and Kimball International are located, respectively, are plotted. In Panel B, the average annual investment level of Herman Miller and Kimball are shown as a scatterplot, and average annual investment is calculated as the equal-weighted average of quarterly capital expenditures divided by lagged quarter assets of a calendar year, annualized. Herman Miller quarter-ends are shifted one month forward (e.g., calendar year 2014 represents December 2013 through November 2014). Multi-year averages from 2005 to 2008, from 2009 to 2011, and from 2012 to 2015 are plotted using horizontal lines.

Figure 2: Map of US with counts of HQ locations, by state



Notes: This figure plots the number of firm headquarters by state for the continental United States. The firms are restricted to all firms in the Compustat analysis sample from 1983 to 2014 with at least $100 million in total assets (2008 dollars), and are in the regression sample of column (5) of Appendix Table B1.

Figure 3: Impact of HQ local conditions on non-local employment growth: binned scatterplot

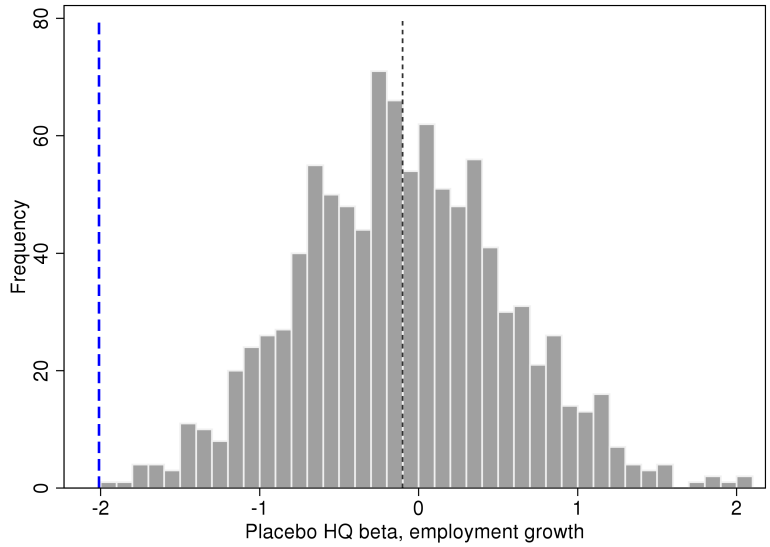


Notes: This figure plots binned scatterplot results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{l,ind,t} + \epsilon_{eilt},$$

in the sample of Compustat firms merged to Census LBD data, as described in Section 3.3. Establishments in the same state as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times \left(emp_{e,t+1} + emp_{et}\right)}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, and $\delta_{l,ind,t}$ county-by-industry-by-year fixed effects. Firm controls include Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. This binned scatterplot corresponds to the regression estimate presented in column (4) of Table 2.

Figure 4: Placebo HQ local conditions impact on establishment employment growth

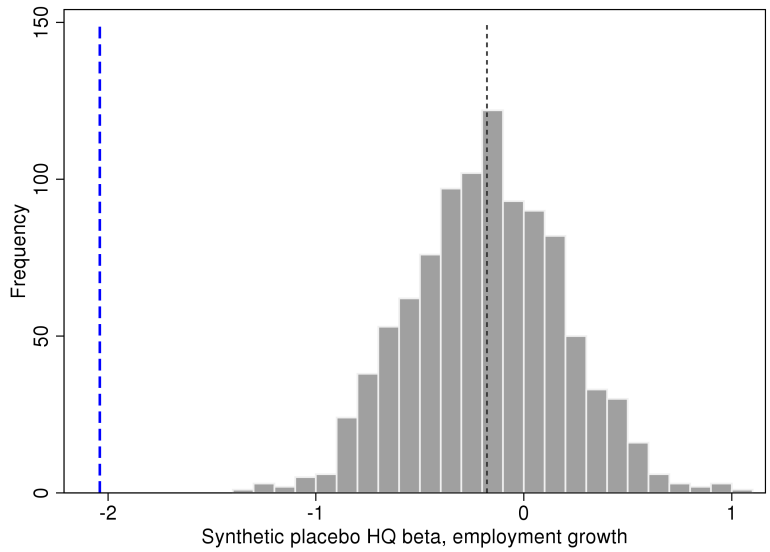


Notes: This figure plots the frequency distribution of estimates of β_1 from running 1000 regressions corresponding to equation (4):

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

but where HQ unemployment rate ur_{ct} is replaced with the unemployment rate of a randomly selected non-HQ establishment of the firm (the "placebo HQ"). In each placebo regression, establishments in the same state as the randomly selected non-HQ establishment are excluded. The x-axis of the figure plots the magnitude of the β_1 estimates. The baseline estimated impact of the HQ county unemployment rate on establishment growth, estimated in column (1) of Table 2, is plotted as the thicker blue dashed line, while the mean placebo estimate (averaging across the 1000 estimates) is plotted as the thin black dashed line.

Figure 5: Placebo synthetic HQ local conditions impact on establishment employment growth

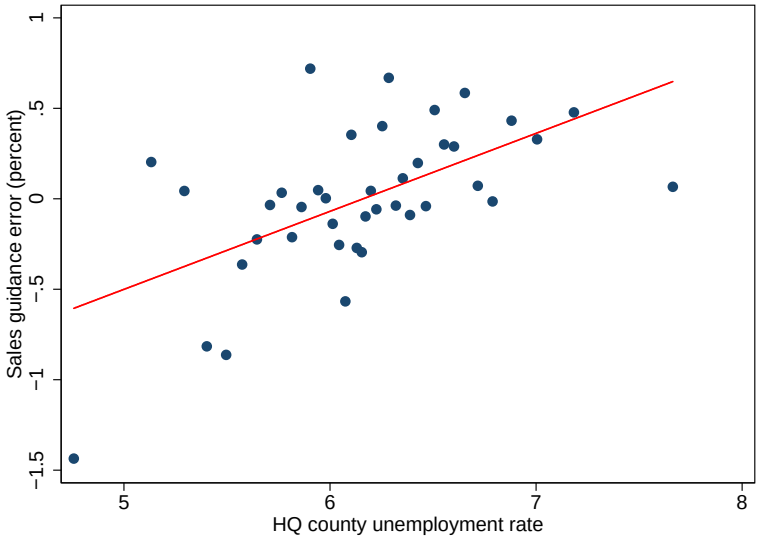


Notes: This figure plots the frequency distribution of estimates of β_1 from running 1000 regressions corresponding to equation (4):

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

but where HQ unemployment rate ur_{ct} is replaced with the weighted average unemployment rate of a number of randomly selected non-HQ establishments of the firm (the "synthetic placebo HQ"), whose employment share cumulatively sums to the average HQ employment share in the sample. Firms for which the HQ employment share is extremely high are excluded from this sample, as there must be sufficiently large cumulative employment share at non-HQ establishments in order to be able to construct the synthetic placebo HQ. In each regression, establishments in the same county as any of the randomly selected non-HQ establishments are excluded. The x-axis of the figure plots the magnitude of the β_1 estimates. The baseline estimated impact of the HQ county unemployment rate on establishment growth corresponds to estimating equation (4) but for a directly comparable sample to that used in the synthetic placebo tests (i.e., excluding firms with very high HQ employment share and excluding establishments in the HQ county). Therefore, the estimate will not exactly match the estimate shown in Figure 4. The baseline estimated impact of the HQ county unemployment rate on establishment growth is plotted as the thicker blue dashed line, while the mean placebo estimate (averaging across the 1000 estimates) is plotted as the thin black dashed line.

Figure 6: Impact of HQ local conditions on sales forecast errors: binned scatterplot

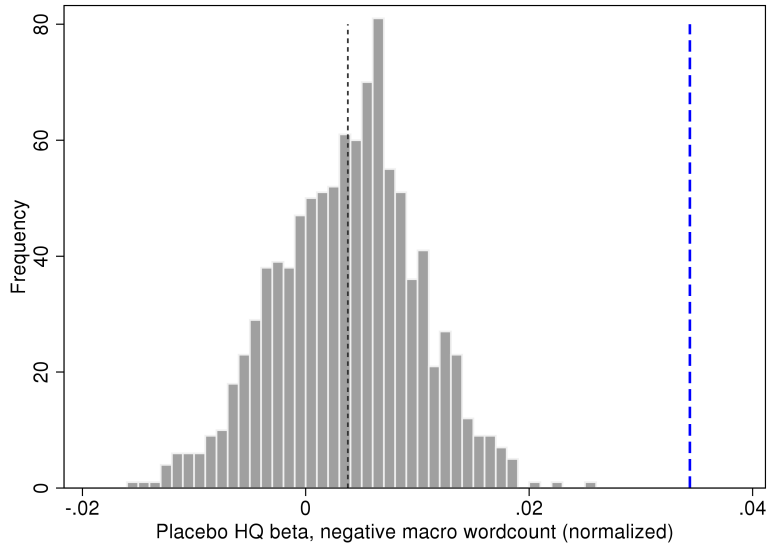


Notes: This figure plots binned scatterplot results from running regressions of the form:

$$error_{iy} = \beta_1 ur_{ct} + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

in the quarterly sample of Compustat firms merged to I/B/E/S guidance data from 2002 to 2015, as described in Section 5.1. $error_{iy}$ is the sales forecast error for firm i in guidance (forecast) year y, scaled by lagged four-quarter sales and multiplied by 100, winsorized at 1%, as described in Section 5.1, ur_{ct} is the average local unemployment rate in firm HQ county c for the lagged four quarters including quarter t, δ_i are firm fixed effects, and $\delta_{ind,t}$ are industry-by-quarter fixed effects. This binned scatterplot corresponds to the regression estimate presented in column (5) of Table 8.

Figure 7: Placebo HQ local conditions impact on firm macroeconomic sentiment

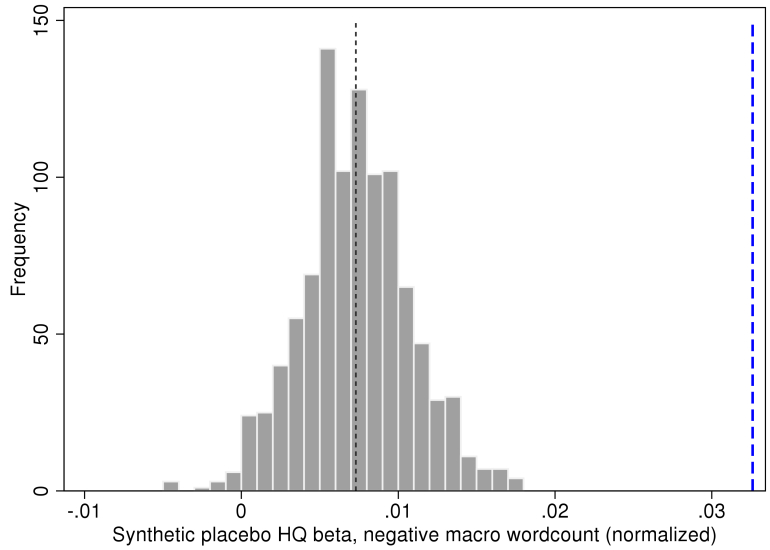


Notes: This figure plots the frequency distribution of estimates of β_1 from running 1000 regressions corresponding to equation (5.2):

$$sentiment_{it} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

but where HQ unemployment rate ur_{ct} is replaced with the unemployment rate of a randomly selected non-HQ establishment of the firm (the "placebo HQ"), and $sentiment_{it}$ is defined as the logarithm of negative economic words in macroeconomic sentences, normalized. The x-axis of the figure plots the magnitude of the β_1 estimates. The baseline estimated impact of the HQ county unemployment rate on macroeconomic sentiment, is plotted as the thicker blue dashed line, while the mean placebo estimate (averaging across the 1000 estimates) is plotted as the thin black dashed line.

Figure 8: Placebo synthetic HQ local conditions impact on firm macroeconomic sentiment



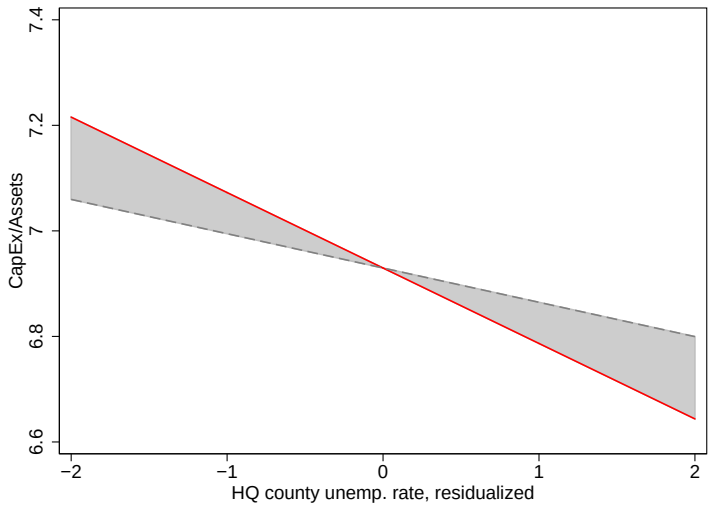
Notes: This figure plots the frequency distribution of estimates of β_1 from running 1000 regressions corresponding to equation (5.2):

$$sentiment_{it} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

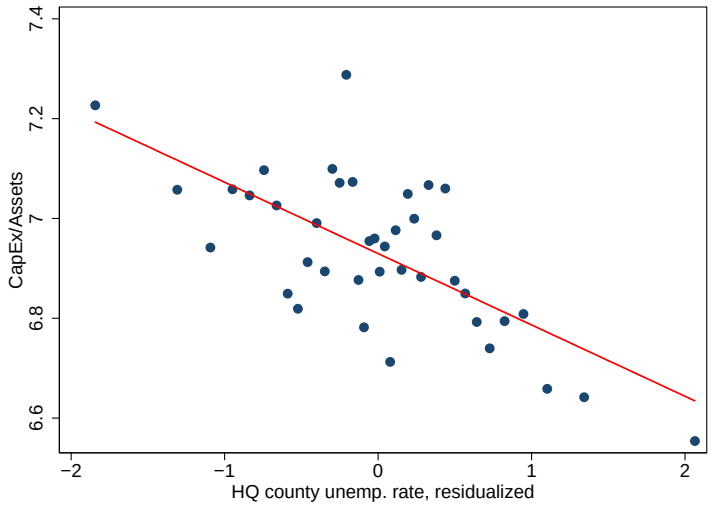
but where HQ unemployment rate ur_{ct} is replaced with the weighted average unemployment rate of a number of randomly selected non-HQ establishments of the firm (the "synthetic placebo HQ"), whose employment share cumulatively sums to the average HQ employment share in the sample, and $sentiment_{it}$ is defined as the logarithm of negative economic words in macroeconomic sentences, normalized. Firms for which the HQ employment share is extremely high are excluded from this sample, as there must be sufficiently large cumulative employment share at non-HQ establishments in order to be able to construct the synthetic placebo HQ. In each regression, establishments in the same county as any of the randomly selected non-HQ establishments are excluded. The x-axis of the figure plots the magnitude of the β_1 estimates. The baseline estimated impact of the HQ county unemployment rate on macroeconomic sentiment corresponds to estimating equation (8) but for a directly comparable sample to that used in the synthetic placebo tests (i.e., excluding firms with very high HQ employment share and excluding establishments in the HQ county). Therefore, the estimate will not exactly match the estimate shown in Figure 7. The baseline estimated impact of the HQ county unemployment rate on macroeconomic sentiment is plotted as the thicker blue dashed line, while the mean placebo estimate (averaging across the 1000 estimates) is plotted as the thin black dashed line.

Figure 9: Calculating misallocated investment from local conditions overweighting

Panel A: Misallocated investment due to overweighted local conditions



Panel B: Binned scatterplot of HQ local conditions impact on investment



Notes: In Panel A, the red line plots the regression estimate plotted in Panel B. The dashed gray line plots the counterfactual impact of the HQ unemployment rate on total firm investment with no local conditions overweighting, as described in Section 6.2. The shaded gray areas represent the total quantity of misallocated investment due to local conditions overweighting.
Panel B plots the binned scatterplot for the regression estimating:

$$Inv_{i,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

in the quarterly sample of Compustat firms from 1983 to 2014 with at least \$500M in total assets (2008 dollars). It corresponds to the estimate shown in column (6) of Appendix Table B1, but where $Inv_{i,t+1}$ is annualized. $Inv_{i,t+1}$ is capital expenditures in quarter $t+1$ (annualized) divided by quarter t assets in percent, winsorized at the 1%, ur_{ct} is the average local unemployment rate in firm HQ county c for the lagged four quarters including quarter t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat, δ_i are firm fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. Firm-level controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%.

Tables

Table 1: Summary Statistics

Variable	**Mean**	**Standard Deviation**	***N***
Employment growth (percent)	4.4	81.2	4359000
HQ unemployment rate (ur_{ct})	5.156	1.910	4359000
Tobin's Q	1.775	1.055	4359000
Size (log)	7.456	1.773	4359000
Cash level / Assets	0.073	0.090	4359000
Cashflow / Assets	0.120	0.096	4359000
Sales Growth	0.120	0.228	4359000
Weighted average local conditions ($\overline{ur}_{it}$)	5.660	1.451	4359000

Table 2: Impact of HQ local conditions on non-local establishment employment growth

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

in the sample of Compustat firms merged to Census LBD data, as described in Section 3.3. Establishments in the same state as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1}-emp_{et}}{0.5\times\left(emp_{e,t+1}+emp_{et}\right)}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. In columns (3) and (4), δ_{lt} and $\delta_{ind,t}$ are replaced with $\delta_{l,ind,t}$, county-by-industry-by-year fixed effects. Other firm controls include log of asset size, cash levels (scaled by assets), and sales growth for the past four quarters, all winsorized at 1%. In column (5), the sample is restricted to firms in non-tradables industries. In column (6), the sample is restricted to firms with HQ state employment share of less than 25%. In column (7), weighted average local conditions of the firm $\overline{ur}_{it}$ are added as a control (see equation (6) in Section 4.1 for calculation details). In column (8), the sample is restricted to firms with HQ county employment of less than 2% of total county employment. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr
ur_{ct}	-1.93***	-2.02***	-2.03***	-2.21***	-3.48***	-3.17***	-1.76***	-2.09***
	(0.612)	(0.595)	(0.700)	(0.678)	(1.07)	(0.771)	(0.640)	(0.626)
$\overline{ur}_{it}$							-1.21	
							(1.19)	
Cashflow		39.4***		41.1***	51.9***	39.7***	39.3***	40.4***
		(5.52)		(5.96)	(14.1)	(6.65)	(5.54)	(5.52)
Tobin's Q		2.19***		1.70***	1.81**	1.73**	2.17***	2.02***
		(0.622)		(0.638)	(0.843)	(0.766)	(0.622)	(0.594)
Other firm controls	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Estab location X Year FE	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Industry X Year FE	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Location X Ind X Year FE	No	No	Yes	Yes	No	No	No	No
Sample	Baseline	Baseline	Baseline	Baseline	Non-trad	$\frac{HQ}{firm}$<25%	Baseline	$\frac{HQ}{county}$<2%
Obs.	4359000	4359000	4047000	4047000	1780000	3403000	4359000	3984000
R^2	0.278	0.282	0.381	0.384	0.284	0.299	0.282	0.289

Table 3: Impact of HQ local conditions on intensive and extensive margin of non-local employment growth

The table displays results from running regressions of the form:

$$\mathbf{1}_{\{\text{ext}\}}\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt}$$

or

$$\left(1 - \mathbf{1}_{\{\text{ext}\}}\right)\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt}$$

in the sample of Compustat firms merged to Census LBD data, as described in Section 3.3. Establishments in the same state as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times \left(emp_{e,t+1} + emp_{et}\right)}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, $\mathbf{1}_{\{\text{ext}\}}$ is an indicator for extensive margin changes, $1 - \mathbf{1}_{\{\text{ext}\}}$ is an indicator for intensive margin changes, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. Columns labeled "Ext" use the extensive margin employment growth outcome, columns labeled "Int" use the intensive margin employment growth outcome. In columns (3) and (4), δ_{lt} and $\delta_{ind,t}$ are replaced with $\delta_{l,ind,t}$, county-by-industry-by-year fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. In columns (5) and (6), the sample is restricted to firms in non-tradables industries. In columns (7) and (8), the sample is restricted to firms with HQ state employment share of less than 25%. In columns (9) and (10), weighted average local conditions of the firm $\overline{ur}_{it}$ are added as a control (see equation (6) in Section 4.1 for calculation details). Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1) Ext	(2) Int	(3) Ext	(4) Int	(5) Ext	(6) Int	(7) Ext	(8) Int	(9) Ext	(10) Int
ur_{ct}	-1.80***	-0.220	-2.00***	-0.203	-3.10***	-0.378	-2.83***	-0.338	-1.55**	-0.210
	(0.571)	(0.212)	(0.646)	(0.250)	(1.02)	(0.411)	(0.739)	(0.305)	(0.623)	(0.244)
$\overline{ur}_{it}$									-1.16	-0.0507
									(1.17)	(0.338)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Estab location X Year FE	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry X Year FE	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Location X Ind X Year FE	No	No	Yes	Yes	No	No	No	No	No	No
Sample	Baseline	Baseline	Baseline	Baseline	Non-trad	Non-trad	$\frac{HQ}{firm}$<25%	$\frac{HQ}{firm}$<25%	Baseline	Baseline
Obs.	4359000	4359000	4047000	4047000	1780000	1780000	3403000	3403000	4359000	4359000
R^2	0.275	0.167	0.383	0.266	0.283	0.161	0.293	0.183	0.275	0.167

Table 4: Impact of HQ local conditions and local conditions at largest establishments on non-local establishment employment growth

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{ur}_{it}^{rank'}\Gamma + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

in the sample of Compustat firms merged to Census LBD data, as described in Section 3.3. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times (emp_{e,t+1} + emp_{et})}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{ur}_{it}^{rank}$ is a vector of the unemployment rates of the 5 largest establishments, by employment, of 5 different states (rank 1 is the largest), $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. In column (3), δ_{lt} and $\delta_{ind,t}$ are replaced with $\delta_{l,ind,t}$, county-by-industry-by-year fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. In column (4), the sample is restricted to firms in non-tradables industries. In column (5), the sample is restricted to firms with HQ state employment share of less than 25%. In column (6), weighted average local conditions of the firm $\overline{ur}_{it}$ are added as a control (see equation (6) in Section 4.1 for calculation details). Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)
	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr
ur_{ct}	-1.54**	-1.57**	-1.39**	-2.80**	-2.12***	-1.49**
	(0.636)	(0.703)	(0.692)	(1.14)	(0.791)	(0.664)
ur_{it}^{rank1}	-0.0332	-0.389	-0.256	0.552	-0.307	0.0203
	(0.334)	(0.387)	(0.399)	(0.608)	(0.427)	(0.401)
ur_{it}^{rank2}	0.00637	0.0846	-0.00832	-0.417	-0.221	0.0382
	(0.299)	(0.330)	(0.328)	(0.746)	(0.435)	(0.339)
ur_{it}^{rank3}	0.0440	0.229	0.0704	-0.0105	-0.118	0.0641
	(0.260)	(0.291)	(0.285)	(0.454)	(0.357)	(0.280)
ur_{it}^{rank4}	0.0241	-0.121	-0.0434	0.893*	-0.168	0.0397
	(0.240)	(0.257)	(0.254)	(0.514)	(0.336)	(0.244)
ur_{it}^{rank5}	0.231	0.163	0.0907	-0.0295	-0.159	0.243
	(0.224)	(0.246)	(0.258)	(0.483)	(0.315)	(0.221)
$\overline{ur}_{it}$						-0.410
						(1.55)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Estab location X Year FE	Yes	Yes	No	Yes	Yes	Yes
Industry X Year FE	Yes	Yes	No	Yes	Yes	Yes
Location X Ind X Year FE	No	No	Yes	No	No	No
Sample	Excl cty	Excl state	Excl cty	Non-trad	$\frac{HQ}{firm}$<25%	Excl cty
Obs.	3783000	2314000	3469000	1516000	2855000	3783000
R^2	0.297	0.315	0.409	0.304	0.318	0.297

Table 5: Impact of HQ local conditions on firms with high or low financial constraints

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

for different samples of firms. Establishments in the same state as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times (emp_{e,t+1} + emp_{et})}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. Firm i at time t has bond market access if it has a credit rating at time t or before. A firm is a dividend payer if it paid any dividends in year t. For leverage and cash level, I split firms based on whether they are above- or below-median in a particular year, where leverage is the ratio of total debt outstanding to the sum of debt outstanding and stock market capitalization, and cash level is the cash fraction of total assets. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Bond market access		Pay dividends		Leverage		Cash levels	
	Yes	No	Yes	No	Low	High	High	Low
ur_{ct}	-2.22***	-1.05	-2.40***	0.194	-3.43***	-0.458	-2.75***	-1.49**
	(0.836)	(1.04)	(0.710)	(0.964)	(0.943)	(0.925)	(0.960)	(0.659)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location X Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry X Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	2761000	1254000	2669000	1625000	1926000	2054000	1747000	2405000
R^2	0.304	0.345	0.289	0.350	0.321	0.342	0.356	0.334

Table 6: Impact of HQ local conditions on firms with high or low real estate ownership

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

for different samples of firms. Establishments in the same state as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times (emp_{e,t+1} + emp_{et})}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. Real estate ownership is measured as the fraction of net real estate-related assets divided by net fixed assets, based on Compustat data, and the sample is split on whether a firm is above- or below-median in a particular year. A firm is classified as a HQ owner using the data of Chaney, Sraer and Thesmar (2012) for HQ ownership in 1997, which is based on financial filings. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)
	Real estate ownership		Own HQ		Own HQ (1987-2005)	
	Low	High	No	Yes	No	Yes
ur_{ct}	-1.89***	-0.416	-2.49***	-1.22	-2.58**	-0.852
	(0.725)	(1.14)	(0.840)	(1.27)	(1.20)	(1.43)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Location X Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry X Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	555000	622000	1289000	1126000	1143000	1032000
R^2	0.368	0.372	0.322	0.324	0.335	0.330

Table 7: Impact of HQ local conditions on firms with high or low by industry macro correlation

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

for different samples of firms. Establishments in the same state as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times \left(emp_{e,t+1} + emp_{et}\right)}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. Industry macroeconomic correlation is determined by the equal-weighted average sales growth to GDP growth beta for all Compustat firms in the same 2-digit SIC industry from 1976 to 2015; "High" indicates an above-median industry by sales-to-GDP beta. Industry macroeconomic salience is determined by the average presence of macroeconomic phrases in the 10-K and 10-Q SEC filings of firms in an industry, after removing time fixed effects. I classify the following as macroeconomic phrases: "economic activity," "economic climate," "economic conditions," "economic environment," "economic forces," "economic outlook," "global economy," "world economy," "worldwide economy", "domestic economy," "national economy," "U.S. economy," "entire economy," "general economy," "overall economy," "the economy," "macroeconomy," "macroeconomic," "business cycle," "economic cycle," "economic expansion," "economic contraction," "economic growth," "consumer confidence," "weak economy," "weakening economy," "soft economy," "softening economy," "strong economy," "strengthening economy," "growing economy," "booming economy," and "state of the economy." "High" indicates an above-median industry by count of macroeconomic phrases. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)
	Ind macro correlation		Ind macro salience	
	High	Low	High	Low
ur_{ct}	-2.85***	-1.47**	-2.28***	-1.39
	(0.902)	(0.751)	(0.756)	(0.953)
Other firm controls	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes
Estab location X Year FE	Yes	Yes	Yes	Yes
Industry X Year FE	Yes	Yes	Yes	Yes
Obs.	1645000	2686000	2641000	1705000
R^2	0.296	0.289	0.285	0.302

Table 8: Impact of HQ local conditions on sales forecast errors

The table displays results from running firm-level regressions of the form:

$$error_{iy} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

in the quarterly sample of Compustat firms merged to I/B/E/S guidance data from 2002 to 2015, as described in Section 5.1. $error_{iy}$ is the sales forecast error for firm i in guidance (forecast) year y, scaled by lagged four-quarter sales and multiplied by 100, winsorized at 1%, as described in Section 5.1, ur_{ct} is the average local unemployment rate in firm HQ county c for the lagged four quarters including quarter t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat, δ_i are firm fixed effects, and $\delta_{ind,t}$ are industry-by-quarter fixed effects. Columns (1) through (4) use δ_t time fixed effects instead of $\delta_{ind,t}$. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1) Sales guide error	(2) Sales guide error	(3) Sales guide error	(4) Sales guide error	(5) Sales guide error	(6) Sales guide error	(7) Sales guide error	(8) Sales guide error
ur_{ct}	0.522***	0.410**	0.485**	0.398**	0.431**	0.346*	0.413**	0.360*
	(0.185)	(0.175)	(0.191)	(0.180)	(0.193)	(0.185)	(0.199)	(0.190)
Other firm controls	No	Yes	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	No	No	No	No
Industry X Quarter FE	No	No	No	No	Yes	Yes	Yes	Yes
Sample	All	All	No Fin/Util	No Fin/Util	All	All	No Fin/Util	No Fin/Util
Obs.	42442	39882	40358	38107	42442	39882	40358	38107
R^2	0.0256	0.0515	0.0261	0.0522	0.414	0.423	0.403	0.413

Table 9: Impact of HQ local conditions on macroeconomic sentiment

The table displays results from running firm-level regressions of the form:

$$sentiment_{it} = \alpha_1 ur_{ct} + \mathbf{x}_{it}^{'}\alpha_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

in the quarterly sample of Compustat firms merged to SEC financial filing data from 1995 to 2014, as described in Section 5.2. $sentiment_{it}$ is a measure of macroeconomic sentiment for firm i in quarter t, which in columns (1) through (3) is defined as the log number of negative economic words in macroeconomic sentences, normalized to unit standard deviation, and in columns (4) through (6) is defined as the net count of negative economic words (negative minus positive) in macroeconomic sentences, normalized to unit standard deviation, ur_{ct} is the average local unemployment rate in firm HQ county c for the lagged four quarters including quarter t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat as well a control for the number of 10-K forms filed by firm i in quarter t, δ_i are firm fixed effects, and $\delta_{ind,t}$ are industry-by-quarter fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1) Neg macro sent.	(2) Neg macro sent.	(3) Neg macro sent.	(4) Net neg macro sent.	(5) Net neg macro sent.	(6) Net neg macro sent.
ur_{ct}	0.0317***	0.0251***	0.0247**	0.0283***	0.0228***	0.0258**
	(0.00584)	(0.00799)	(0.0117)	(0.00616)	(0.00860)	(0.0127)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry X Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	100M+	500M+	All	100M+	500M+
Obs.	279322	170348	96956	279322	170348	96956
R^2	0.493	0.520	0.529	0.339	0.363	0.377

Table 10: Impact of average non-local HQ local conditions on aggregate county outcomes

The table displays results from running county-level regressions of the form:

$$y_{c,t+1} = \beta_1 \overline{HQur}_{ct} + \mathbf{x}_{ct}^{'}\beta_2 + \beta_3 y_{ct} + \delta_c + \delta_t + \epsilon_{ct},$$

or

$$y_{c,t+1} = \beta_1 \overline{HQur}_{ct}^{NT} + \mathbf{x}_{ct}^{'}\beta_2 + \beta_3 y_{ct} + \delta_c + \delta_t + \epsilon_{ct},$$

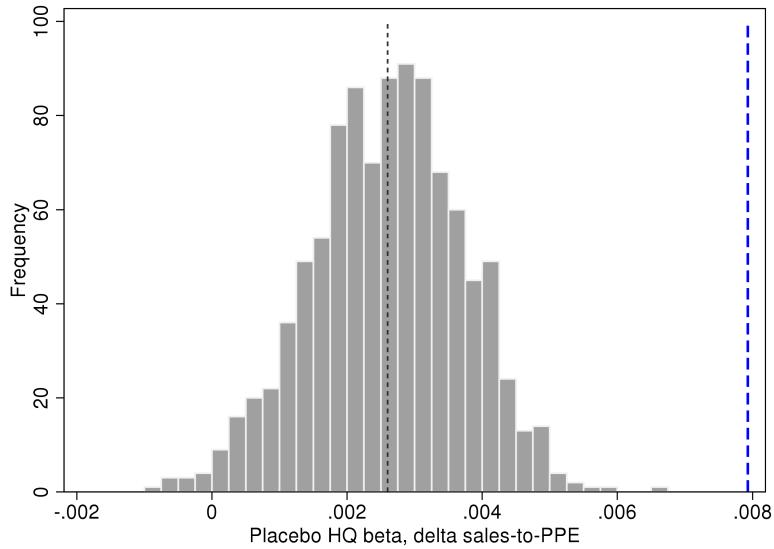
for counties from 1981 to 2005, as described in Section 6.1. $y_{c,t+1}$ is either: 1) county employment growth from year t to $t+1$, 2) the establishment birth rate minus the establishment death rate (all based on t to $t+1$ births or deaths). In columns (1) through (4), $y_{c,t+1}$ is calculated for all employer establishments in county c; in columns (5) through (8), $y_{c,t+1}$ is calculated using all non-tradables establishments in county c, $\overline{HQur}_{ct}$ the establishment employment-weighted average HQ county unemployment rate of all Compustat-linked establishments in county c that are not headquartered in the same state as county c, $\overline{HQur}_{ct}^{NT}$ is the establishment employment-weighted average HQ county unemployment rate of all Compustat-linked non-tradable establishments in county c that are not headquartered in the same state as county c, $\mathbf{x}_{ct}$ includes industry share controls, as well as establishment employment-weighted average firm controls from Compustat, δ_c are county fixed effects, and δ_t are year fixed effects. Average firm controls include establishment employment-weighted average Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters. The sample is restricted to county-year observations where the linked Compustat firms with HQ data contain a greater-than-median share of county employment. Robust standard errors are clustered by county. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1) Emp Gr	(2) Emp Gr	(3) Birth-Death	(4) Birth-Death	(5) Emp Gr	(6) Emp Gr	(7) Birth-Death	(8) Birth-Death
$\overline{HQur}_{ct}$	-0.365***	-0.288**	-0.178***	-0.147***				
	(0.116)	(0.115)	(0.0580)	(0.0564)				
$\overline{HQur}_{ct}^{NT}$					-0.277**	-0.243**	-0.248***	-0.221***
					(0.112)	(0.112)	(0.0596)	(0.0587)
Lagged Y	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ind share controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Av. firm controls	No	Yes	No	Yes	No	Yes	No	Yes
Sample	All	All	All	All	Non-trad	Non-trad	Non-trad	Non-trad
Obs.	22000	21000	22000	21000	20000	20000	20000	20000
R^2	0.140	0.141	0.743	0.752	0.126	0.126	0.722	0.723

A Appendix A: Additional results and robustness checks

In this section, I present figures and tables that show results from additional tests.

Figure A1: Placebo HQ local conditions impact on marginal investment productivity (delta sales-to-fixed assets)

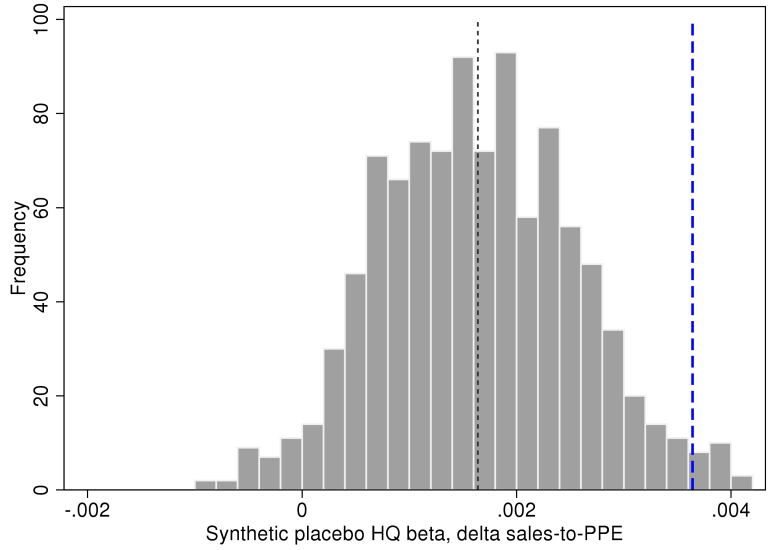


Notes: This figure plots the frequency distribution of estimates of β_1 from running 1000 regressions corresponding to equation:

$$\Delta \log \left(\frac{sales_{i,t+1}}{PPE_{it}} \right) = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'} \beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

but where HQ unemployment rate ur_{ct} is replaced with the unemployment rate of a randomly selected non-HQ establishment of the firm (the "placebo HQ"). The x-axis of the figure plots the magnitude of the β_1 estimates. The baseline estimated impact of the HQ county unemployment rate on firm investment productivity corresponds to estimating the equation shown in Table A17 but for a directly comparable sample to that used in the placebo tests (i.e., based on the sample of merged Compustat-Census firms). The baseline estimated impact of the HQ county unemployment rate on investment productivity is plotted as the thicker blue dashed line, while the mean placebo estimate (averaging across the 1000 estimates) is plotted as the thin black dashed line.

Figure A2: Placebo synthetic HQ local conditions impact on marginal investment productivity (delta sales-to-fixed assets)

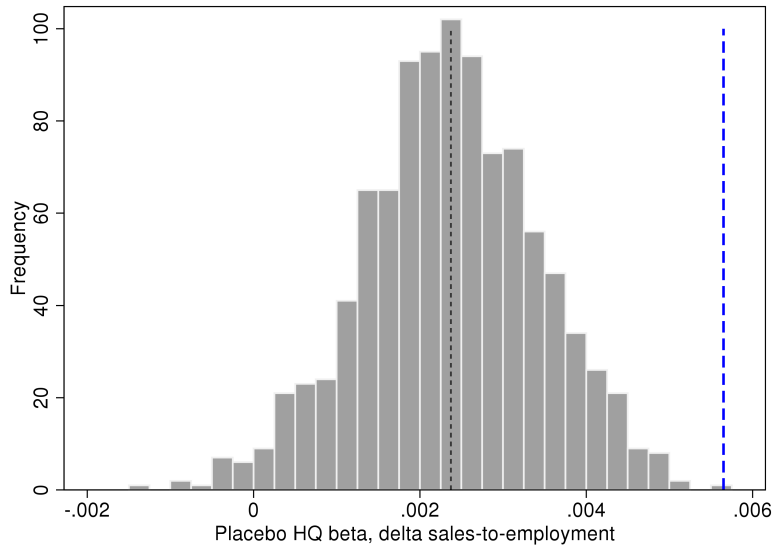


Notes: This figure plots the frequency distribution of estimates of β_1 from running 1000 regressions corresponding to equation:

$$\Delta \log \left(\frac{sales_{i,t+1}}{PPE_{it}} \right) = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'} \beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

but where HQ unemployment rate ur_{ct} is replaced with the weighted average unemployment rate of a number of randomly selected non-HQ establishments of the firm (the "synthetic placebo HQ"), whose employment share cumulatively sums to the average HQ employment share in the sample. Firms for which the HQ employment share is extremely high are excluded from this sample, as there must be sufficiently large cumulative employment share at non-HQ establishments in order to be able to construct the synthetic placebo HQ. The x-axis of the figure plots the magnitude of the β_1 estimates. The baseline estimated impact of the HQ county unemployment rate on firm investment productivity corresponds to estimating the equation shown in Table A17 but for a directly comparable sample to that used in the placebo tests (i.e., based on the sample of merged Compustat-Census firms and excluding firms with very high HQ employment share). Therefore, the estimate will not exactly match the estimate shown in Figure A1. The baseline estimated impact of the HQ county unemployment rate on investment productivity is plotted as the thicker blue dashed line, while the mean placebo estimate (averaging across the 1000 estimates) is plotted as the thin black dashed line.

Figure A3: Placebo HQ local conditions impact on marginal investment productivity (delta sales-to-employment)

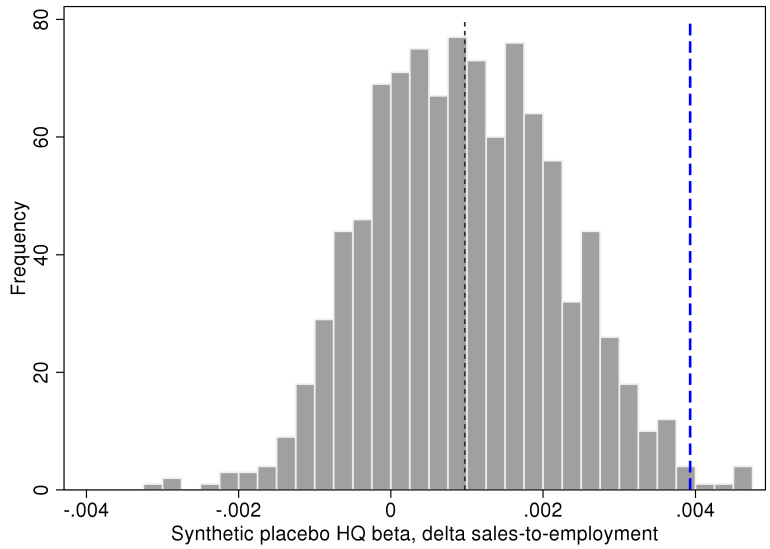


Notes: This figure plots the frequency distribution of estimates of β_1 from running 1000 regressions corresponding to equation:

$$\Delta \log \left(\frac{sales_{i,t+1}}{Emp_{it}} \right) = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'} \beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

but where HQ unemployment rate ur_{ct} is replaced with the unemployment rate of a randomly selected non-HQ establishment of the firm (the "placebo HQ"). The x-axis of the figure plots the magnitude of the β_1 estimates. The baseline estimated impact of the HQ county unemployment rate on firm investment productivity corresponds to estimating the equation shown in Table A17 but for a directly comparable sample to that used in the placebo tests (i.e., based on the sample of merged Compustat-Census firms). The baseline estimated impact of the HQ county unemployment rate on investment productivity is plotted as the thicker blue dashed line, while the mean placebo estimate (averaging across the 1000 estimates) is plotted as the thin black dashed line.

Figure A4: Placebo synthetic HQ local conditions impact on marginal investment productivity (delta sales-to-employment)



Notes: This figure plots the frequency distribution of estimates of β_1 from running 1000 regressions corresponding to equation:

$$\Delta \log \left(\frac{sales_{i,t+1}}{Emp_{it}} \right) = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'} \beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

but where HQ unemployment rate ur_{ct} is replaced with the weighted average unemployment rate of a number of randomly selected non-HQ establishments of the firm (the "synthetic placebo HQ"), whose employment share cumulatively sums to the average HQ employment share in the sample. Firms for which the HQ employment share is extremely high are excluded from this sample, as there must be sufficiently large cumulative employment share at non-HQ establishments in order to be able to construct the synthetic placebo HQ. The x-axis of the figure plots the magnitude of the β_1 estimates. The baseline estimated impact of the HQ county unemployment rate on firm investment productivity corresponds to estimating the equation shown in Table A17 but for a directly comparable sample to that used in the placebo tests (i.e., based on the sample of merged Compustat-Census firms and excluding firms with very high HQ employment share). Therefore, the estimate will not exactly match the estimate shown in Figure A3. The baseline estimated impact of the HQ county unemployment rate on investment productivity is plotted as the thicker blue dashed line, while the mean placebo estimate (averaging across the 1000 estimates) is plotted as the thin black dashed line.

Table A1: Impact of HQ local conditions on non-local establishment employment growth, HQ county clustered standard errors

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

in the sample of Compustat firms merged to Census LBD data, as described in Section 3.3. Establishments in the same state as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times \left(emp_{e,t+1} + emp_{et}\right)}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. In columns (3) and (4), δ_{lt} and $\delta_{ind,t}$ are replaced with $\delta_{l,ind,t}$, county-by-industry-by-year fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. In column (5), the sample is restricted to firms in non-tradables industries. In column (6), the sample is restricted to firms with HQ state employment share of less than 25%. In column (7), weighted average local conditions of the firm $\overline{ur}_{it}$ are added as a control (see equation (6) in Section 4.1 for calculation details). In column (8), the sample is restricted to firms with HQ county employment of less than 2% of total county employment. Robust standard errors are clustered by HQ county. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr
ur_{ct}	-1.93***	-2.02***	-2.03***	-2.21***	-3.48***	-3.17***	-1.76***	-2.09***
	(0.613)	(0.621)	(0.682)	(0.702)	(1.06)	(0.791)	(0.672)	(0.616)
$\overline{ur}_{it}$							-1.21	
							(1.23)	
Other firm controls	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Estab location X Year FE	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Industry X Year FE	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Location X Ind X Year FE	No	No	Yes	Yes	No	No	No	No
Sample	Baseline	Baseline	Baseline	Baseline	Non-trad	$\frac{HQ}{firm}$<25%	Baseline	$\frac{HQ}{county}$<2%
Obs.	4359000	4359000	4047000	4047000	1780000	3403000	4359000	3984000
R^2	0.278	0.282	0.381	0.384	0.284	0.299	0.282	0.289

Table A2: Impact of HQ MSA unemployment rate on non-local establishment employment growth

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{mt} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

in the sample of Compustat firms merged to Census LBD data, as described in Section 3.3. Establishments in the same state or metropolitan area as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times \left(emp_{e,t+1} + emp_{et}\right)}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, ur_{mt} is the average local unemployment rate in firm HQ metropolitan area m in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. In column (2), δ_{lt} and $\delta_{ind,t}$ are replaced with $\delta_{l,ind,t}$, county-by-industry-by-year fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. In column (3), the sample is restricted to firms in non-tradables industries. In column (4), the sample is restricted to firms with HQ state employment share of less than 25%. In column (5), weighted average local conditions of the firm $\overline{ur}_{it}$ are added as a control (see equation (6) in Section 4.1 for calculation details). In column (6), the sample is restricted to firms with HQ county employment of less than 2% of total county employment. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)
	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr
ur_{mt}	-1.89***	-1.98***	-2.98**	-2.85***	-1.60**	-1.92***
	(0.655)	(0.757)	(1.28)	(0.866)	(0.698)	(0.693)
$\overline{ur}_{it}$					-1.41	
					(1.17)	
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Estab location X Year FE	Yes	No	Yes	Yes	Yes	Yes
Industry X Year FE	Yes	No	Yes	Yes	Yes	Yes
Location X Ind X Year FE	No	Yes	No	No	No	No
Sample	Baseline	Baseline	Non-trad	$\frac{HQ}{firm}$<25%	Baseline	$\frac{HQ}{county}$<2%
Obs.	4358000	4017000	1781000	3403000	4358000	3983000
R^2	0.282	0.385	0.285	0.299	0.282	0.289

Table A3: Impact of HQ local conditions on non-local establishment employment growth, non-mover HQ firms only

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

in the sample of Compustat firms merged to Census LBD data, as described in Section 3.3. Establishments in the same state as the firm HQ are excluded, and only firms where the HQ is located in the same county in 1990 as they are in the most recent Compustat sample are included. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times \left(emp_{e,t+1} + emp_{et}\right)}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. In column (2), δ_{lt} and $\delta_{ind,t}$ are replaced with $\delta_{l,ind,t}$, county-by-industry-by-year fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. In column (3), the sample is restricted to firms in non-tradables industries. In column (4), the sample is restricted to firms with HQ state employment share of less than 25%. In column (5), weighted average local conditions of the firm $\overline{ur}_{it}$ are added as a control (see equation (6) in Section 4.1 for calculation details). In column (6), the sample is restricted to firms with HQ county employment of less than 2% of total county employment. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)
	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr
ur_{ct}	-1.67***	-1.78**	-2.87**	-2.75***	-1.33**	-1.73**
	(0.635)	(0.741)	(1.11)	(0.835)	(0.679)	(0.674)
$\overline{ur}_{it}$					-1.44	
					(1.42)	
Other firm controls	No	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Estab location X Year FE	Yes	No	Yes	Yes	Yes	Yes
Industry X Year FE	Yes	No	Yes	Yes	Yes	Yes
Location X Ind X Year FE	No	Yes	No	No	No	No
Sample	Baseline	Baseline	Non-trad	$\frac{HQ}{firm}$<25%	Baseline	$\frac{HQ}{county}$<2%
Obs.	3629000	3314000	1571000	2764000	3629000	3300000
R^2	0.284	0.394	0.288	0.303	0.284	0.292

Table A4: Impact of HQ local conditions on non-local establishment employment growth, excluding establishments close to HQ by distance

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

in the sample of Compustat firms merged to Census LBD data, as described in Section 3.3. In columns (1) through (3), establishments in the same state as the firm HQ, as well as immediately adjacent states, are excluded; in columns (4) through (6) all establishments in counties within 250 miles of the HQ county are excluded, and in columns (7) through (9) all establishments in counties within 500 miles of the HQ county are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times (emp_{e,t+1} + emp_{et})}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. In columns (2), (5), and (8), δ_{lt} and $\delta_{ind,t}$ are replaced with $\delta_{l,ind,t}$, county-by-industry-by-year fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. In columns (3), (6) and (9), the sample is restricted to firms in non-tradables industries. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Excl own/adj state			250+ miles			500+ miles		
	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr
ur_{ct}	-2.23***	-2.29***	-3.82***	-2.07***	-2.09***	-3.58***	-2.20***	-2.16***	-3.90***
	(0.621)	(0.704)	(1.12)	(0.605)	(0.687)	(1.10)	(0.646)	(0.709)	(1.14)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location X Yr FE	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
Industry X Yr FE	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
Location X Ind X Yr FE	No	Yes	No	No	Yes	No	No	Yes	No
Sample	Baseline	Baseline	Non-trad	Baseline	Baseline	Non-trad	Baseline	Baseline	Non-trad
Obs.	3622000	3315000	1430000	3888000	3564000	1551000	2968000	2697000	1136000
R^2	0.288	0.393	0.294	0.286	0.390	0.292	0.294	0.400	0.301

Table A5: Impact of HQ local conditions on non-local establishment employment growth, location-by-industry-by-time fixed effects

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{l,ind,t} + \epsilon_{eilt},$$

in the sample of Compustat firms merged to Census LBD data, as described in Section 3.3. Establishments in the same state as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times \left(emp_{e,t+1} + emp_{et}\right)}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, and $\delta_{l,ind,t}$ are county-by-industry-by-year fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. In column (3), the sample is restricted to firms in non-tradables industries. In column (4), the sample is restricted to firms with HQ state employment share of less than 25%. In column (5), weighted average local conditions of the firm $\overline{ur}_{it}$ are added as a control (see equation (6) in Section 4.1 for calculation details). In column (6), the sample is restricted to firms with HQ county employment of less than 2% of total county employment. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)
	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr
ur_{ct}	-2.03***	-2.21***	-3.97***	-3.45***	-1.92***	-2.30***
	(0.700)	(0.678)	(1.10)	(0.892)	(0.711)	(0.726)
$\overline{ur}_{it}$					-1.42	
					(1.48)	
Other firm controls	No	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Location X Ind X Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Baseline	Baseline	Non-trad	$\frac{HQ}{firm}$<25%	Baseline	$\frac{HQ}{county}$<2%
Obs.	4047000	4047000	1718000	3116000	4047000	3681000
R^2	0.381	0.384	0.349	0.402	0.384	0.392

Table A6: Impact of HQ local conditions on sales and profit growth: firm-level regressions

The table displays results from running firm-level regressions of the form:

$$\Delta S_{i,t+4} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

in the quarterly sample of Compustat firms from 1983 to 2014. $\Delta S_{i,t+4}$ is either growth in annual sales from quarter $t+4$ through $t+7$ relative to sales from quarter $t-4$ to $t-1$, winsorized at 1%, or change in annual cash flow from quarter $t+4$ through $t+7$ relative to cash flow in quarter $t-4$ to $t-1$, scaled by assets at $t-4$, winsorized at 1%, ur_{ct} is the average local unemployment rate in firm HQ county c for the lagged four quarters including quarter t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat, δ_i are firm fixed effects, and $\delta_{ind,t}$ are industry-by-quarter fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)
	Sales Gr	Sales Gr	Sales Gr	Δ cashflow	Δ cashflow	Δ cashflow
ur_{ct}	-0.00147	0.00378	0.00359	0.00322**	0.00282**	0.00188
	(0.00493)	(0.00385)	(0.00445)	(0.00149)	(0.00110)	(0.00127)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry X Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	100M+	500M+	All	100M+	500M+
Obs.	380285	217409	117379	346351	200719	109223
R^2	0.349	0.447	0.488	0.265	0.309	0.330

Table A7: Impact of HQ local conditions on employment growth, using "leave-out" HQ county or MSA unemployment rate

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct}^{LO} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

in the sample of Compustat firms merged to Census LBD data, as described in Section 3.3. Establishments in the same state as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times \left(emp_{e,t+1} + emp_{et}\right)}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t , ur_{mt}^{-c} is the average local unemployment rate in the MSA m of the firm HQ in year t, excluding county c which the firm HQ is located, ur_{ct}^{LO} the average local unemployment rate in county c adjusting for the direct impact of changes in HQ employment for firm i from year $t-1$ to year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. In columns (2) and (7), δ_{lt} and $\delta_{ind,t}$ are replaced with $\delta_{l,ind,t}$, county-by-industry-by-year fixed effects. Firm-level controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. In columns (3) and (8), the sample is restricted to firms in non-tradables industries. In columns (4) and (9), the sample is restricted to firms with HQ state employment share of less than 25%. In columns (5) and (10), the weighted average local conditions of the firm $\overline{ur}_{it}$ are added as a control (see equation (6) in Section 4.1 for calculation details). Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr
ur_{mt}^{-c}	-1.92***	-2.29***	-2.94**	-2.94***	-1.68**					
	(0.658)	(0.769)	(1.34)	(0.878)	(0.669)					
ur_{ct}^{LO}						-1.86***	-1.93***	-2.14***	-2.38***	-1.62***
						(0.367)	(0.409)	(0.552)	(0.491)	(0.383)
$\overline{ur}_{it}$					-1.53					-1.95
					(1.25)					(1.23)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Estab location X Year FE	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Industry X Year FE	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Location X Ind X Year FE	No	Yes	No	No	No	No	Yes	No	No	No
Sample	Baseline	Baseline	Non-trad	$\frac{HQ}{firm}$<25%	Baseline	Baseline	Baseline	Non-trad	$\frac{HQ}{firm}$<25%	Baseline
Obs.	3814000	3497000	1623000	3050000	3814000	3974000	3661000	1628000	3097000	3974000
R^2	0.283	0.390	0.285	0.299	0.283	0.288	0.390	0.294	0.305	0.288

Table A8: Impact of HQ local conditions on employment growth, controlling for firm HQ county employment growth

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \beta_3 \Delta y_{i,t+1}^{HQ} + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

in the sample of Compustat firms merged to Census LBD data, as described in Section 3.3. Establishments in the same state as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times \left(emp_{e,t+1} + emp_{et}\right)}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, $\Delta y_{i,t+1}^{HQ}$ is the employment growth measure but calculated for the HQ county establishment employment (and not scaled by 100), ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. In column (2), δ_{lt} and $\delta_{ind,t}$ are replaced with $\delta_{l,ind,t}$, county-by-industry-by-year fixed effects. Firm-level controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. In column (3), the sample is restricted to firms in non-tradables industries. In column (4), the sample is restricted to firms with HQ state employment share of less than 25%. In column (5), weighted average local conditions of the firm $\overline{ur}_{it}$ are added as a control (see equation (6) in Section 4.1 for calculation details). Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)
	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr
ur_{ct}	-2.15***	-2.46***	-3.50***	-3.35***	-1.81***
	(0.595)	(0.682)	(1.08)	(0.776)	(0.658)
$\Delta y_{i,t+1}^{HQ}$	15.5***	15.2***	15.0***	13.6***	15.5***
	(1.61)	(1.75)	(2.88)	(1.76)	(1.61)
$\overline{ur}_{it}$					-1.51
					(1.27)
Other firm controls	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes
Estab location X Year FE	Yes	No	Yes	Yes	Yes
Industry X Year FE	Yes	No	Yes	Yes	Yes
Location X Ind X Year FE	No	Yes	No	No	No
Sample	Baseline	Baseline	Non-trad	$\frac{HQ}{firm}$<25%	Baseline
Obs.	4178000	3875000	1742000	3252000	4178000
R^2	0.286	0.388	0.289	0.302	0.286

Table A9: Impact of HQ local conditions on debt issuance: firm-level regressions

The table displays results from running firm-level regressions of the form:

$$Debtissue_{i,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

in the quarterly sample of Compustat firms from 1983 to 2014. $Debtissue_{i,t+1}$ is either defined as the total amount of debt issued (scaled by lagged assets) in quarter $t+1$ in percent, winsorized at the 1%, or an indicator for whether the firm does any debt issuance from $t+1$ to $t+4$, ur_{ct} is the average local unemployment rate in firm HQ county c for the lagged four quarters including quarter t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat, δ_i are firm fixed effects, and $\delta_{ind,t}$ are industry-by-quarter fixed effects. Firm-level controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1) Debt issued	(2) Debt issued	(3) Debt issued	(4) Any issue	(5) Any issue	(6) Any issue
ur_{ct}	0.0283	0.0731	0.0286	0.00165	0.00391	0.00153
	(0.0301)	(0.0455)	(0.0496)	(0.00194)	(0.00246)	(0.00320)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry X Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	100M+	500M+	All	100M+	500M+
Obs.	414501	229500	121633	369629	206548	110641
R^2	0.253	0.275	0.281	0.479	0.505	0.474

Table A10: Impact of HQ local conditions on firms with high or low regional area macro correlation

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

for different samples of firms. Establishments in the same state as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times \left(emp_{e,t+1} + emp_{et}\right)}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. A firm is classified as "high" local area macroeconomic correlation if the estimated beta of the HQ metropolitan area unemployment rate to GDP growth from 1976 to 2015 is more negative than the median metropolitan area (columns (1), (3), and (5)). In columns (3) and (4), δ_{lt} and $\delta_{ind,t}$ are replaced with $\delta_{l,ind,t}$, county-by-industry-by-year fixed effects. In columns (5) and (6), the sample is restricted to firms in non-tradables industries. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline sample		Baseline sample		Non-tradables	
	High	Low	High	Low	High	Low
ur_{ct}	-1.09	-2.63***	-1.35	-2.76**	-3.52**	-3.21**
	(0.842)	(0.952)	(1.04)	(1.15)	(1.69)	(1.51)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Estab location X Year FE	Yes	Yes	No	No	Yes	Yes
Industry X Year FE	Yes	Yes	No	No	Yes	Yes
Location X Ind X Year FE	No	No	Yes	Yes	No	No
Obs.	2458000	1821000	2191000	1615000	960000	788000
R^2	0.297	0.307	0.417	0.419	0.309	0.315

Table A11: Impact of HQ local conditions on non-local establishment employment growth, including establishment-level controls

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{eit}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

in the sample of Compustat firms merged to Census LBD data, as described in Section 3.3. Establishments in the same state as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1}-emp_{et}}{0.5\times\left(emp_{e,t+1}+emp_{et}\right)}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{eit}$ is a vector of firm-level controls from Compustat measured in Q1 of year t as well as establishment-level controls, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. In column (2), δ_{lt} and $\delta_{ind,t}$ are replaced with $\delta_{l,ind,t}$, county-by-industry-by-year fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. Establishment controls include the log of establishment employment and the log of establishment age. In column (3), the sample is restricted to firms in non-tradables industries. In column (4), the sample is restricted to firms with HQ state employment share of less than 25%. In column (5), weighted average local conditions of the firm $\overline{ur}_{it}$ are added as a control (see equation (6) in Section 4.1 for calculation details). In column (6), the sample is restricted to firms with HQ county employment of less than 2% of total county employment. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)
	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr
ur_{ct}	-1.34***	-1.30**	-2.71***	-2.00***	-1.03**	-1.41***
	(0.469)	(0.534)	(0.839)	(0.628)	(0.525)	(0.475)
$\overline{ur}_{it}$					-1.44*	
					(0.837)	
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Establishment controls	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Estab location X Year FE	Yes	No	Yes	Yes	Yes	Yes
Industry X Year FE	Yes	No	Yes	Yes	Yes	Yes
Location X Ind X Year FE	No	Yes	No	No	No	No
Sample	Baseline	Baseline	Non-trad	$\frac{HQ}{firm}$<25%	Baseline	$\frac{HQ}{county}$<2%
Obs.	4359000	4047000	1780000	3403000	4359000	3984000
R^2	0.565	0.625	0.594	0.578	0.565	0.575

Table A12: Impact of HQ local conditions on employment growth for non-tradable firms only

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{eit}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

in the sample of Compustat firms merged to Census LBD data, as described in Section 3.3. Establishments in the same state as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times \left(emp_{e,t+1} + emp_{et}\right)}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{eit}$ is a vector of firm-level controls from Compustat measured in Q1 of year t as well as establishment-level controls, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. In columns (1) and (3), δ_{lt} and $\delta_{ind,t}$ are replaced with $\delta_{l,ind,t}$, county-by-industry-by-year fixed effects. Firm-level controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. Establishment controls include the log of establishment employment and the log of establishment age. In columns (2) and (4), weighted average local conditions of the firm $\overline{ur}_{it}$ are added as a control (see equation (6) in Section 4.1 for calculation details). All samples restrict to firms in non-tradables industries. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)
	Emp Gr	Emp Gr	Emp Gr	Emp Gr
ur_{ct}	-3.97***	-3.64***	-2.88***	-2.95***
	(1.10)	(1.06)	(0.868)	(0.935)
$\overline{ur}_{it}$		0.970		1.43
		(3.14)		(2.05)
Other firm controls	Yes	Yes	Yes	Yes
Establishment controls	No	No	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes
Estab location X Year FE	No	Yes	No	Yes
Industry X Year FE	No	Yes	No	Yes
Location X Ind X Year FE	Yes	No	Yes	No
Sample	Non-trad	Non-trad	Non-trad	Non-trad
Obs.	1718000	1780000	1718000	1780000
R^2	0.349	0.284	0.629	0.594

Table A13: Impact of HQ local conditions on employment growth for non-tradable establishments only

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

in the sample of Compustat firms merged to Census LBD data, as described in Section 3.3. Establishments in the same state as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times \left(emp_{e,t+1} + emp_{et}\right)}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. In columns (2), (4), (6) and (8), δ_{lt} and $\delta_{ind,t}$ are replaced with $\delta_{l,ind,t}$, county-by-industry-by-year fixed effects. Firm-level controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. In columns (5) through (8), weighted average local conditions of the firm $\overline{ur}_{it}$ are added as a control (see equation (6) in Section 4.1 for calculation details). All samples restrict to establishments classified in non-tradables industries. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	NT estabs of NT firms		NT estabs of all firms		NT estabs of NT firms		NT estabs of all firms	
	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr
ur_{ct}	-3.43***	-3.93***	-2.29***	-2.85***	-3.58***	-3.95***	-2.28***	-2.76***
	(1.05)	(1.07)	(0.847)	(0.939)	(1.03)	(1.03)	(0.872)	(0.931)
$\overline{ur}_{it}$					0.888	0.0830	-0.0784	-0.471
					(3.15)	(4.07)	(2.08)	(2.79)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Estab location X Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Industry X Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Location X Ind X Year FE	No	Yes	No	Yes	No	Yes	No	Yes
Obs.	1665000	1605000	2368000	2210000	1665000	1605000	2368000	2210000
R^2	0.276	0.344	0.286	0.379	0.276	0.344	0.286	0.379

Table A14: Impact of HQ local conditions on non-local establishment employment growth, large or geographically dispersed firms only

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

in the sample of Compustat firms merged to Census LBD data, as described in Section 3.3. Establishments in the same state as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times \left(emp_{e,t+1} + emp_{et}\right)}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. In column (1), firms with lower-than-median assets across firms in that year are excluded; in column (2), firms with fewer than \$500M (2008 dollars) in assets are excluded; in column (3), firms with fewer than \$1B (2008 dollars) in assets are excluded; in column (4), firms located in twelve (the median number of locations) or fewer states are excluded; in column (5), firms with higher-than-median geographic concentration, as measured using an employment share Herfindahl index, are excluded. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)
	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr
ur_{ct}	-2.31***	-2.13***	-1.92**	-2.00***	-2.01***
	(0.639)	(0.707)	(0.771)	(0.633)	(0.607)
Other firm controls	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes
Location X Year FE	Yes	Yes	Yes	Yes	Yes
Industry X Year FE	Yes	Yes	Yes	Yes	Yes
Sample	>Median Size	\$500M+ assets	\$1B+ assets	>12 States	High Geo disp.
Obs.	3896000	3483000	2931000	4086000	4205000
R^2	0.286	0.292	0.297	0.284	0.284

Table A15: Impact of HQ local conditions on non-local establishment employment growth, with zipcode fixed effects

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

in the sample of Compustat firms merged to Census LBD data, as described in Section 3.3. Establishments in the same state as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{emp_{e,t+1} - emp_{et}}{0.5 \times \left(emp_{e,t+1} + emp_{et}\right)}\right] \times 100$, where emp_{et} is the employment of establishment e at March 12 of year t, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, δ_{lt} are establishment zipcode-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. In column (2), δ_{lt} and $\delta_{ind,t}$ are replaced with $\delta_{l,ind,t}$, zipcode-by-industry-by-year fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. In column (3), the sample is restricted to firms in non-tradables industries. In column (4), the sample is restricted to firms with HQ state employment share of less than 25%. In column (5), weighted average local conditions of the firm $\overline{ur}_{it}$ are added as a control (see equation (6) in Section 4.1 for calculation details). In column (6), the sample is restricted to firms with HQ county employment of less than 2% of total county employment. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)
	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr	Emp Gr
ur_{ct}	-1.99***	-2.45***	-3.42***	-3.08***	-1.77***	-2.02***
	(0.574)	(0.781)	(0.990)	(0.751)	(0.610)	(0.607)
$\overline{ur}_{it}$					-1.02	
					(1.17)	
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Estab zipcode X Year FE	Yes	No	Yes	Yes	Yes	Yes
Industry X Year FE	Yes	No	Yes	Yes	Yes	Yes
Zipcode X Ind X Year FE	No	Yes	No	No	No	No
Sample	Baseline	Baseline	Non-trad	$\frac{HQ}{firm}$<25%	Baseline	$\frac{HQ}{county}$<2%
Obs.	4244000	2936000	1707000	3278000	4244000	3871000
R^2	0.341	0.535	0.369	0.366	0.341	0.350

Table A16: Impact of HQ local conditions on non-local establishment payroll growth

The table displays results from running regressions of the form:

$$\Delta y_{eil,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_e + \delta_{lt} + \delta_{ind,t} + \epsilon_{eilt},$$

in the sample of Compustat firms merged to Census LBD data, as described in Section 3.3. Establishments in the same state as the firm HQ are excluded. In the regression, $\Delta y_{eil,t+1} = \left[\frac{pay_{e,t+1} - pay_{et}}{0.5 \times (pay_{e,t+1} + pay_{et})}\right] \times 100$, where pay_{et} is the annual payroll of establishment e in year t, ur_{ct} is the average local unemployment rate in firm HQ county c in year t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat measured in Q1 of year t, δ_e are establishment fixed effects, δ_{lt} are establishment county-by-year fixed effects, and $\delta_{ind,t}$ are industry-by-year fixed effects. In columns (3) and (4), δ_{lt} and $\delta_{ind,t}$ are replaced with $\delta_{l,ind,t}$, county-by-industry-by-year fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. In column (5), the sample is restricted to firms in non-tradables industries. In column (6), the sample is restricted to firms with HQ state employment share of less than 25%. In column (7), weighted average local conditions of the firm $\overline{ur}_{it}$ are added as a control (see equation (6) in Section 4.1 for calculation details). In column (8), the sample is restricted to firms with HQ county employment of less than 2% of total county employment. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pay Gr	Pay Gr	Pay Gr	Pay Gr	Pay Gr	Pay Gr	Pay Gr	Pay Gr
ur_{ct}	-1.61***	-1.67***	-1.66***	-1.78***	-2.41***	-2.48***	-1.34**	-1.59***
	(0.541)	(0.536)	(0.596)	(0.597)	(0.920)	(0.700)	(0.576)	(0.570)
$\overline{ur}_{it}$							-1.51	
							(1.13)	
Other firm controls	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Estab location X Year FE	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Industry X Year FE	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Location X Ind X Year FE	No	No	Yes	Yes	No	No	No	No
Sample	Baseline	Baseline	Baseline	Baseline	Non-trad	$\frac{HQ}{firm}$<25%	Baseline	$\frac{HQ}{county}$<2%
Obs.	4359000	4359000	4047000	4047000	1780000	3403000	4359000	3984000
R^2	0.494	0.496	0.567	0.569	0.546	0.520	0.496	0.499

Table A17: Impact of HQ local conditions on marginal investment productivity

The table displays results from running firm-level regressions of the form:

$$\Delta \log \left(\frac{sales_{i,t+1}}{A_{it}} \right) = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'} \beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

in the sample of Compustat firms merged to Census LBD data. $\Delta \log \left(\frac{sales_{i,t+1}}{A_{it}} \right)$ is the change in the logarithm of the ratio of sales in the four quarters starting in $t+1$ to net property, plant and equipment at t minus the same ratio four quarters previously, winsorized at the 1%, or the change in the logarithm of the ratio of sales in the four quarters starting in $t+1$ to total LBD employment at t minus the same ratio four quarters previously, winsorized at the 1%, ur_{ct} is the average local unemployment rate in firm HQ county c for the lagged four quarters including quarter t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat, δ_i are firm fixed effects, and $\delta_{ind,t}$ are industry-by-quarter fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Δ Sales/PPE	Δ Sales/PPE	Δ Sales/PPE	Δ Sales/PPE	Δ Sales/Emp	Δ Sales/Emp	Δ Sales/Emp	Δ Sales/Emp
ur_{ct}	0.0104***	0.0109***	0.00854***	0.00812***	0.00746***	0.00900***	0.00475***	0.00524***
	(0.00155)	(0.00180)	(0.00161)	(0.00189)	(0.00158)	(0.00195)	(0.00163)	(0.00203)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	No	No	Yes	Yes	No	No
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry X Quarter FE	No	No	Yes	Yes	No	No	Yes	Yes
Sample	100M+	500M+	100M+	500M+	100M+	500M+	100M+	500M+
R^2	0.153	0.180	0.337	0.378	0.0627	0.0698	0.239	0.267
Obs.	152000	87000	151000	87000	152000	88000	151000	87000

B Appendix B: Firm-level tests on investment and expectations

B.1 The impact of local conditions overweighting on investment

I present additional results from tests using firm-level data on investment. These tests are firm-level, and thus cannot exploit the empirical strategy used for establishment-level employment growth in Section 2.

I estimate the regression:

$$Inv_{i,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it}, \tag{10}$$

where i indexes firms, and t the quarter. Firm i is headquartered in county c. $Inv_{i,t+1}$ is capital expenditures in quarter $t+1$, scaled by lagged assets, ur_{ct} is the average local unemployment rate in firm HQ county c in the last four quarters ending in t, $\mathbf{x}_{it}$ is a vector of firm-level controls. This regression uses industry-by-quarter fixed effects $\delta_{ind,t}$ along with firm fixed effects δ_i. I cluster standard errors at the firm-level. In a separate specification, I run a variant using only firm and time fixed effects:

$$Inv_{i,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_i + \delta_t + \epsilon_{it}. \tag{11}$$

In both regressions, the impact of HQ local conditions ur_{ct} is identified off cross-sectional differences in local conditions.

Appendix Table B1 presents results from running regressions (10) and (11) with investment as the outcome variable. Across all specifications and different samples of firms (including the sample of larger firms with over \$500M in total assets), worse HQ local economic conditions are strongly associated with lower investment levels. However, because these regressions do not exclude the direct impact of HQ area economic conditions on local HQ establishments, these estimates will also capture the rational response to relatively worse local HQ demand and investment opportunities.

Therefore, to test whether managers overweight HQ local conditions in choosing investment, I employ two placebo tests. In the first test, I replace the HQ unemployment rate ur_{ct} in estimating equation (10) with the local unemployment rate of a randomly selected non-HQ establishment of the firm. In the second test, I replace the HQ unemployment rate ur_{ct} in equation (10) with the average unemployment rate of a synthetic placebo HQ, made up of multiple establishments whose cumulative employment share is equal to the average HQ employment share of the firm. To conduct these placebo tests, I merge the Compustat data to the Census LBD database, as Census data are required to link establishment locations and employment shares to the firm-level investment data.

Results from the first placebo test are shown in Appendix Figure B1. The dashed line plots the magnitude of the impact of HQ county unemployment rate on firm investment. The bars represent the frequency distribution of estimates of the impact of randomly selected non-HQ establishment unemployment rates on investment, based on one thousand placebo runs. Comparing the baseline estimate to the distribution of placebo estimates, I find that the HQ effect is more negative than

the 1st percentile of estimated non-HQ placebo effects. Results from the second test using synthetic placebo HQ local conditions are shown in Appendix Figure B2. Here as well, the HQ effect is more negative than the 1st percentile of estimated non-HQ synthetic placebo effects.[47]

B.2 Recovering expectations from text data: LASSO approach

B.2.1 Financial filings text data

To obtain a measure of how positive or negative managerial expectations are across a large sample of firms (and years), I turn to data from the text of the financial filings of firms, as in section 5.2. Here, I use a machine-driven LASSO variable selection model to recover the text associated with expectations.

To construct the dataset, I first select all future-oriented sentences from the text of 10-K and 10-Q filings from 1995 to 2014, following the methodology of Muslu, Radhakrishnan, Subramanyam and Lim (2015). Future-oriented sentences are sentences containing words such as "will", "management expects", or "next year."[48] Then, I construct word count variables for all the words contained in these future-oriented sentences. I restrict the set of word count variables to the most common words classified as "negative" or "positive," in the financial filing context, by Loughran and McDonald (2011). I restrict to the top one hundred negative words and top fifty positive words by frequency in order to reduce the dimensionality of the predictor space. These account for approximately 80 percent of the total occurrences of negative and positive words.

B.2.2 LASSO method of recovering expectations

With a dataset of word counts for the most common words, I use a modified LASSO variable-selection method to select the words most strongly associated with expectations of the returns to investment. As there is no directly observed measure of expectations, I assume that unobserved expectations of investment returns at date t, $\mathbb{E}_t\left(q_{t+1}\right)$, are positively and monotonically related to investment at date $t+1$, I_{t+1}, as predicted by most models of investment. In other words, in the underlying structural relationship between expectations and investment,

$$I_{t+1} = b\mathbb{E}_t\left(q_{t+1}\right) + \epsilon_{t+1},$$

[47] The estimate of the baseline impact of HQ unemployment rate on investment differs between the two placebo tests because the samples are different. In Appendix Figure B2, the placebo synthetic HQ regressions can be estimated only for firms with sufficient non-HQ employment share. The impact of HQ unemployment rate on expectations is re-estimated on the same subsample in order to be exactly comparable to the placebo synthetic HQ estimates.

[48] For a full list of words, phrases, and expressions that identify future-oriented sentences, I refer the reader to the Appendix of Muslu et al. (2015). I use the same list, and allow the phrases denoting future time periods to be more general (e.g., both "next year" and "next two years" are included).

b is positive. Then, I assume that if a word count variable $word_t$ at date t is associated with more investment at date $t+1$, the word reflects positive expectations.[49]

I use a modified LASSO linear regression method in order to select the text variables that have incremental explanatory power for future investment, while including standard financial variables as well as firm and time fixed effects. Intuitively, the LASSO linear regression method solves the OLS minimization problem, but penalizes additional explanatory variables in order to reduce overfitting and hence improve out-of-sample predictive power.[50]

Concretely, I proceed in a number of steps in order to derive the text-based measure of expectations. The first step is to minimize the following objective function:

$$\sum_{i=1}^{I}\sum_{t=1}^{T}\left(y_{i,t+1}-\mathbf{w}_{it}^{'}\beta_1-\mathbf{x}_{it}^{'}\beta_2\right)^2+\lambda\sum_{\omega=1}^{W}\left|\beta_{1\omega}\right|, \tag{12}$$

for firm i and quarter t, where $y_{i,t+1}$ is investment (scaled by total assets) over the next 4 quarters, $\mathbf{w}_{it}$ is a vector of normalized word counts for the most common words for the filing from quarter t, $\mathbf{x}_{it}$ is a vector of financial variables and fixed effects that includes Tobin's Q, annual cashflow, log of total assets, cash-on-hand, sales growth, firm fixed effects, and quarter fixed effects. λ is the penalty parameter, and the optimal value of λ for minimizing out-of-sample prediction error is estimated by using ten-fold firm-level cross-validation.[51] The minimand shown in equation (12) is modified from the typical LASSO minimand because the financial and fixed effect indicator variables ($\mathbf{x}_{it}$) are not penalized–regardless of the value of the λ penalty parameter, these variables are always included. Solving this minimization problem yields a vector of coefficients β_1 for all word count variables, where some of the elements of β_1 are zero due to penalization.

Second, I construct the text-based measure of expectations as

$$expect_{it}=\hat{y}_{i,t+1}^{text}-\hat{y}_{i,t+1}^{acct}.$$

Here, $\hat{y}_{i,t+1}^{acct}=\mathbf{x}_{it}^{'}\hat{\beta}_2^{acct}$ is the predicted value of investment for firm i at time $t+1$, $y_{i,t+1}$, based on regressing $y_{i,t+1}$ solely on $\mathbf{x}_{it}$, the accounting variables at time t, along with firm and time fixed effects. $\hat{y}_{i,t+1}^{text}=\mathbf{w}_{it}^{*'}\hat{\beta}_1+\mathbf{x}_{it}^{'}\hat{\beta}_2^{text}$ is the predicted value of $y_{i,t+1}$ based on a regression of the selected word count variables $\mathbf{w}^*$ from the first step LASSO minimization problem – i.e. all the words ω

[49] This assumption holds as long as $\mathrm{Cov}\left(word_t, Inv_{t+1}\right)-\mathrm{Cov}\left(word_t,\epsilon_{t+1}\right)>0$. The covariance of positive management words with unforeseen negative shocks to future investment cannot exceed the positive covariance of these words with future investment.

[50] See, for example, Hastie et al. (2009) for a comprehensive description of the LASSO methodology.

[51] I normalize word counts using the common "tf-idf" weighting scheme, because raw word counts are unlikely to be the best measure of a word's information content (e.g., Loughran and McDonald 2011). If a word ω has raw word count $w_\omega>0$ in a filing at time t, the tf-idf weighted word count is

$$\hat{w}_\omega \quad = \quad \frac{1+\log\left(w_\omega\right)}{1+\log\left(a_t\right)}\times\log\frac{N}{df_\omega},$$

where a_t is the average word count of the document at time t, N is the total number of documents in the sample, and df_ω is the total number of documents containing at least one instance of word ω. If $w_\omega=0$, then $\hat{w}_\omega=0$ as well. This weighting scheme attenuates the impact of high frequency words.

for which $|\beta_{1\omega}| > 0$ from the first step – as well as $\mathbf{x}_{it}$. Intuitively, $expect_{it}$ is the incremental text-predicted expectations of investment returns for firm i at time t.[52]

To test whether HQ local conditions affect expectations, I estimate:

$$expect_{it} = \alpha_1 ur_{ct} + \mathbf{x}_{it}^{'}\alpha_2 + \delta_i + \delta_{ind,t} + \epsilon_{it}, \tag{13}$$

for firm i and quarter t, where $\mathbf{x}_{it}$ includes the same set of firm-level financial variables. However, because the measure of expectations is firm-level, the regression in (13) cannot clearly reject a rational expectations explanation of the impact of local economic conditions on macroeconomic sentiment. Therefore, in a second step, I perform a placebo test where I compare the baseline estimate of the impact of HQ local economic conditions on firm-level expectations, to the average effect of a randomly selected non-HQ establishment's local economic conditions. This tests whether HQ local economic conditions are overweighted in their impact on expectations, relative to the impact of comparable non-HQ local economic conditions.

B.3 LASSO expectations and local conditions overweighting

Results from estimating equation (13) are shown in Appendix Table B2, where $expect_{it}$ is normalized to have unit standard deviation. Column (1) presents the baseline estimates using the sample of all Compustat firms that merge to the SEC filing data from 1995 to 2014. A 1 p.p. higher HQ unemployment rate leads to 2.9% of a standard deviation more negative expectations. Restricting the sample to larger firms which are less likely to be locally concentrated in the HQ area, the estimates are similar: in column (3), among firms that have at least \$500M in assets, a 1 p.p. higher HQ unemployment rate leads to 4.5% of a SD more negative expectations.

Since I cannot exclude the direct impact of HQ area economic conditions on local HQ establishments in the results in Appendix Table B2, I employ placebo tests to see whether managers overweight HQ local conditions in forming expectations. In the first test, I replace the HQ unemployment rate ur_{ct} in estimating equation (13) with the local unemployment rate of a randomly selected non-HQ establishment of the firm. In the second test, I replace the HQ unemployment rate ur_{ct} in equation (13) with the average unemployment rate of a synthetic placebo HQ, made up of multiple establishments whose cumulative employment share is equal to the average HQ employment share of the firm. To conduct these tests, I merge the Compustat and SEC filing text data to the Census LBD database.

Results from the first placebo test are shown in Appendix Figure B3. The dashed line plots the magnitude of the impact of HQ county unemployment rate on expectations. The bars represent the frequency distribution of estimates of the impact of randomly selected non-HQ establishment unemployment rates on expectations, based on one thousand runs. Comparing the baseline estimate

[52] As a validity check, I find that $expect_{it}$ is positively associated with survey measures of CFO optimism about the financial prospects of their firm for the much smaller sample of firms with CFO expectations data (for a description of the survey data, see Graham and Harvey 2001). I am grateful to John Graham and Campbell Harvey for generously sharing the CFO survey data.

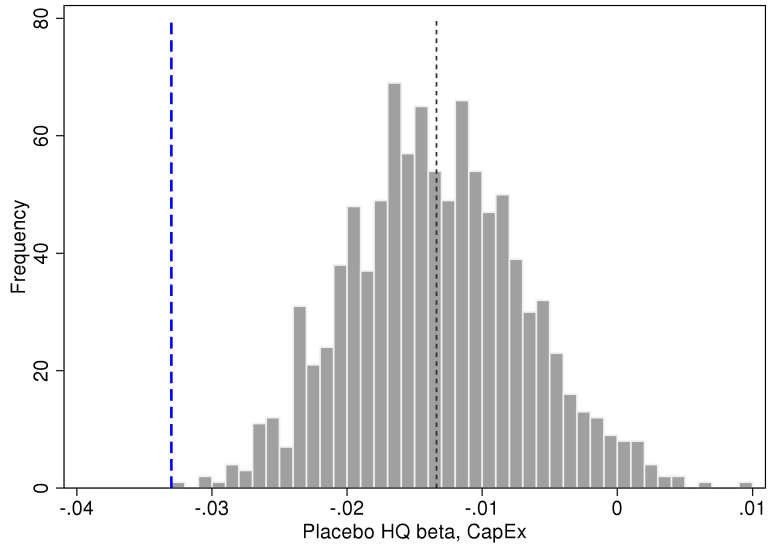
to the distribution of placebo estimates, we find that the HQ effect is more negative than the 1st percentile of estimated non-HQ placebo effects. Results from the second test using synthetic placebo HQ local conditions are shown in Appendix Figure B4. Here as well, the HQ effect is more negative than the 1st percentile of estimated non-HQ synthetic placebo effects.[53]

Using the mean of the placebo estimates, I calculate the extent to which managers overweight HQ local conditions in forming expectations, assuming that the non-overweighted impact of local conditions is captured by the mean estimated impact of the placebo local conditions.[54] Based on the mean of the two placebo distributions, 52 to 63 percent of the estimated impact of HQ local conditions on expectations in Appendix Table B2 can be attributed to *overweighting* HQ local conditions, calculated as the difference between the estimate of HQ local conditions sensitivity and mean placebo HQ sensitivity, and dividing this difference by the HQ sensitivity estimate. Thus, a one SD higher HQ unemployment rate (1.56 p.p.) causes 2.3 to 2.8 percent of a SD worse expectations due to overweighted local conditions.

[53]The estimate of the baseline impact of HQ unemployment rate on expectations differs between the two placebo tests because the samples are different. In Appendix Figure B4, the placebo synthetic HQ regressions can be estimated only for firms with sufficient non-HQ employment share. The impact of HQ unemployment rate on expectations is re-estimated on the same subsample in order to be exactly comparable to the placebo synthetic HQ estimates.

[54]Stated another way, this assumption says that there is no rational expectations reason for the firm to respond differently to HQ local conditions relative to those of a similarly important non-HQ establishment. I discuss how this assumption may not hold due to other factors such as HQ area borrowing relationships in Section 4, and I find no evidence consistent with the failure of this assumption.

Figure B1: Placebo HQ local conditions impact on firm investment

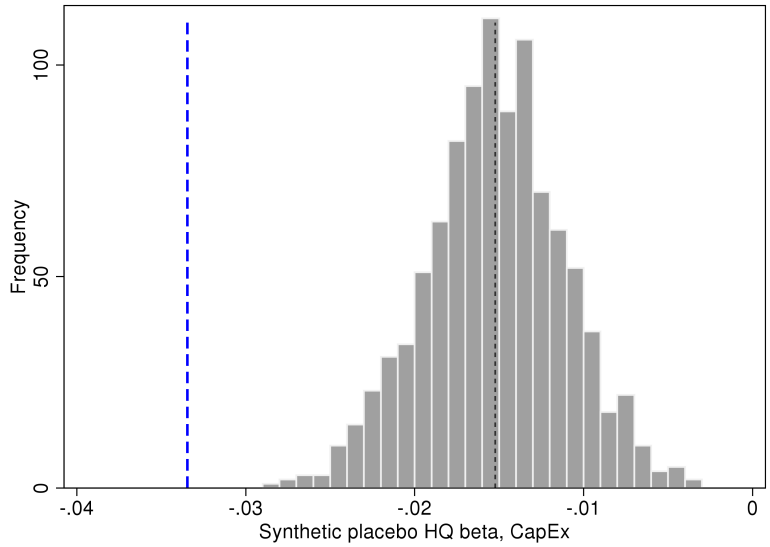


Notes: This figure plots the frequency distribution of estimates of β_1 from running 1000 regressions corresponding to equation (10):

$$Inv_{i,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

but where the HQ unemployment rate ur_{ct} is replaced with the unemployment rate of a randomly selected non-HQ establishment of the firm (the "placebo HQ"). The x-axis of the figure plots the magnitude of the β_1 estimates. The baseline estimated impact of the HQ county unemployment rate on firm investment corresponds to estimating equation (10) but for a directly comparable sample to that used in the placebo tests (i.e., based on the sample of merged Compustat-Census firms). The baseline estimated impact of the HQ county unemployment rate on investment is plotted as the thicker blue dashed line, while the mean placebo estimate (averaging across the 1000 estimates) is plotted as the thin black dashed line.

Figure B2: Placebo synthetic HQ local conditions impact on firm investment

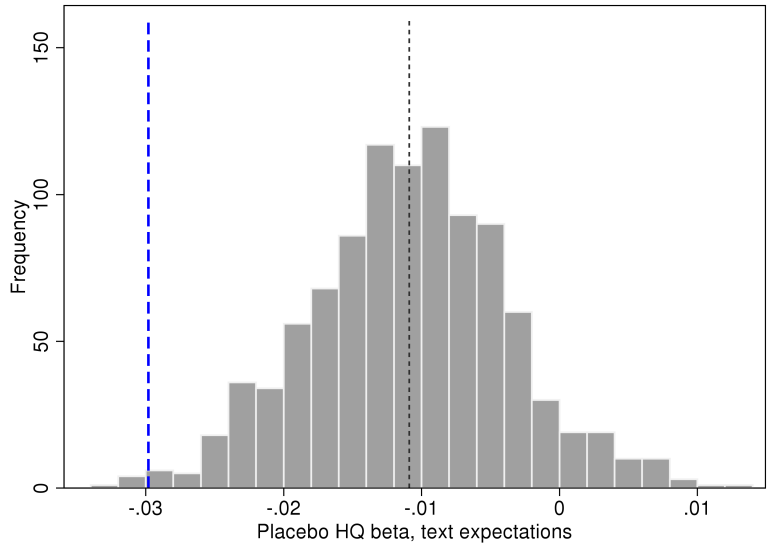


Notes: This figure plots the frequency distribution of estimates of β_1 from running 1000 regressions corresponding to equation (10):

$$Inv_{i,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

but where the HQ unemployment rate ur_{ct} is replaced with the weighted average unemployment rate of a number of randomly selected non-HQ establishments of the firm (the "synthetic placebo HQ"), whose employment share cumulatively sums to the average HQ employment share in the sample. Firms for which the HQ employment share is extremely high are excluded from this sample, as there must be sufficiently large cumulative employment share at non-HQ establishments in order to be able to construct the synthetic placebo HQ. The x-axis of the figure plots the magnitude of the β_1 estimates. The baseline estimated impact of the HQ county unemployment rate on firm investment corresponds to estimating equation (10) but for a directly comparable sample to that used in the placebo tests (i.e., based on the sample of merged Compustat-Census firms and excluding firms with high HQ employment share). Therefore, the estimate will not exactly match the estimate shown in Figure B1. The baseline estimated impact of the HQ county unemployment rate on investment is plotted as the thicker blue dashed line, while the mean placebo estimate (averaging across the 1000 estimates) is plotted as the thin black dashed line.

Figure B3: Placebo HQ local conditions impact on firm expectations

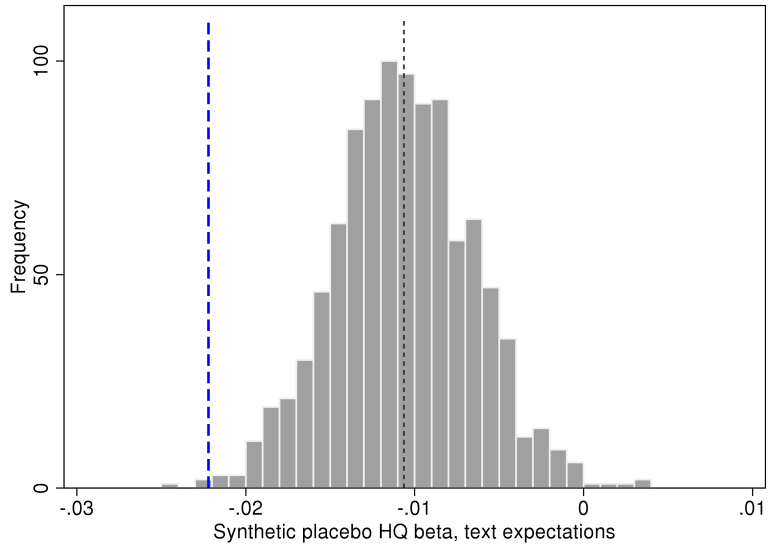


Notes: This figure plots the frequency distribution of estimates of β_1 from running 1000 regressions corresponding to equation (13):

$$expect_{it} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

but where the HQ unemployment rate ur_{ct} is replaced with the unemployment rate of a randomly selected non-HQ establishment of the firm (the "placebo HQ"). The x-axis of the figure plots the magnitude of the β_1 estimates. The baseline estimated impact of the HQ county unemployment rate on expectations corresponds to estimating equation (13) but for a directly comparable sample to that used in the placebo tests (i.e., based on the sample of merged Compustat-Census firms). The baseline estimated impact of the HQ county unemployment rate on expectations is plotted as the thicker blue dashed line, while the mean placebo estimate (averaging across the 1000 estimates) is plotted as the thin black dashed line.

Figure B4: Placebo synthetic HQ local conditions impact on firm expectations



Notes: This figure plots the frequency distribution of estimates of β_1 from running 1000 regressions corresponding to equation (13):

$$expect_{it} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

but where HQ unemployment rate ur_{ct} is replaced with the weighted average unemployment rate of a number of randomly selected non-HQ establishments of the firm (the "synthetic placebo HQ"), whose employment share cumulatively sums to the average HQ employment share in the sample. Firms for which the HQ employment share is extremely high are excluded from this sample, as there must be sufficiently large cumulative employment share at non-HQ establishments in order to be able to construct the synthetic placebo HQ. The x-axis of the figure plots the magnitude of the β_1 estimates. The baseline estimated impact of the HQ county unemployment rate on expectations corresponds to estimating equation (13) but for a directly comparable sample to that used in the placebo tests (i.e., based on the sample of merged Compustat-Census firms and excluding firms with very high HQ employment share). Therefore, the estimate will not exactly match the estimate shown in Figure B3. The baseline estimated impact of the HQ county unemployment rate on expectations is plotted as the thicker blue dashed line, while the mean placebo estimate (averaging across the 1000 estimates) is plotted as the thin black dashed line.

Table B1: Impact of HQ local conditions on investment: firm-level regressions

The table displays results from running firm-level regressions of the form:

$$Inv_{i,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_i + \delta_t + \epsilon_{it}$$

or

$$Inv_{i,t+1} = \beta_1 ur_{ct} + \mathbf{x}_{it}^{'}\beta_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

in the quarterly sample of Compustat firms from 1983 to 2014. $Inv_{i,t+1}$ is capital expenditures in quarter $t+1$ (scaled by quarter t assets), winsorized at the 1%, ur_{ct} is the average local unemployment rate in firm HQ county c for the lagged four quarters including quarter t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat, δ_i are firm fixed effects, δ_t are quarter fixed effects, and $\delta_{ind,t}$ are industry-by-quarter fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1)	(2)	(3)	(4)	(5)	(6)
	Invest.	Invest.	Invest.	Invest.	Invest.	Invest.
ur_{ct}	-0.0463***	-0.0478***	-0.0673***	-0.0284***	-0.0276***	-0.0357***
	(0.00784)	(0.00933)	(0.0113)	(0.00753)	(0.00901)	(0.00981)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	No	No	No
Industry X Quarter FE	No	No	No	Yes	Yes	Yes
Sample	All	100M+	500M+	All	100M+	500M+
Obs.	431721	241940	129109	430742	241140	128373
R^2	0.0415	0.0864	0.101	0.422	0.547	0.614

Table B2: Impact of HQ local conditions on expectations, proxied by incremental text-predicted future investment

The table displays results from running firm-level regressions of the form:

$$expect_{it} = \alpha_1 ur_{ct} + \mathbf{x}_{it}^{'}\alpha_2 + \delta_i + \delta_t + \epsilon_{it}$$

or

$$expect_{it} = \alpha_1 ur_{ct} + \mathbf{x}_{it}^{'}\alpha_2 + \delta_i + \delta_{ind,t} + \epsilon_{it},$$

in the quarterly sample of Compustat firms merged to SEC financial filing data from 1995 to 2014, as described in Appendix Section B.2. $expect_{it}$ is the firm expectations for firm i in quarter t, normalized to have unit standard deviation, as calculated using the method described in Appendix Section B.2.2, ur_{ct} is the average local unemployment rate in firm HQ county c for the lagged four quarters including quarter t, $\mathbf{x}_{it}$ is a vector of firm-level controls from Compustat, δ_i are firm fixed effects, δ_t are quarter fixed effects, and $\delta_{ind,t}$ are industry-by-quarter fixed effects. Firm controls are Tobin's Q, log of asset size, cash levels (scaled by assets), cash flow (scaled by assets) for the past four quarters, and sales growth for the past four quarters, all winsorized at 1%. Robust standard errors are clustered by firm. $*p < 0.1, **p < 0.05, ***p < 0.01$

	(1) Expect.	(2) Expect.	(3) Expect.	(4) Expect.	(5) Expect.	(6) Expect.
ur_{ct}	-0.0353***	-0.0362***	-0.0536***	-0.0290***	-0.0286***	-0.0449***
	(0.00758)	(0.00975)	(0.0132)	(0.00762)	(0.00987)	(0.0137)
Other firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	No	No	No
Industry X Quarter FE	No	No	No	Yes	Yes	Yes
Sample	All	100M+	500M+	All	100M+	500M+
Obs.	248750	156537	90770	247984	155940	90343
R^2	0.000676	0.0115	0.0220	0.0336	0.0736	0.115

C Appendix C: A simple model of local economic conditions and investment

A firm i has a continuum of equally-sized establishments, indexed on the unit interval $[0, 1]$. The firm faces a discrete investment problem at each establishment: it chooses whether to make an investment of 1 (or 0) at each establishment. For the particular establishment e, the firm will invest if expected profits π_e exceed $\bar{\pi}$, where $\bar{\pi}$ captures the firm's investment return hurdle rate.

Expected profits π_e are a function of two signals of profitability, s_e and m, where s_e is the establishment-specific profitability signal, and m is the signal of the aggregate state of the economy, or

$$\pi_e = s_e + b_i m,$$

where b_i captures the firm's loading on the state of the macroeconomy. In other words, the expected returns of each establishment depends both on establishment-specific factors as well as a component due to the aggregate economy. Both signals are observed by the firm at the time it makes the investment decision.

Establishment-specific profitability signals s_e are distributed normally across establishments, with mean zero and unit variance. Then the total investment of the firm, I_i, will consist of the sum of investments at all the establishments where the expected profitability exceeds the return hurdle – i.e., all establishments at which $\pi_e = s_e + b_i m \geq \bar{\pi}$. Therefore, the chosen level of total firm investment will be:

$$\begin{aligned} I_i &= \int_{\bar{\pi}-b_i m}^{\infty} \phi(s) ds \\ &= 1 - \Phi\left(\bar{\pi} - b_i m\right) \end{aligned}$$

where $\phi(s)$ is the standard normal probability density function, and $\Phi(s)$ is the normal cumulative density function.

Suppose that the manager of the firm overweights local economic conditions at HQ in forming macroeconomic expectations m, so that $m = m^{'} + l_c$, where l_c captures the local economic conditions at HQ location c, and $m^{'}$ captures the other macroeconomic signals the manager observes (e.g., professional macroeconomic forecasts).

This framework yields a few predictions:

1. Firm investment increases with better HQ local economic conditions.

Taking a derivative of total firm investment with respect to l_c yields:

$$\frac{\partial I_i}{\partial l_c} = \phi\left(\bar{\pi} - b_i\left(m' + l_c\right)\right) b_i > 0,$$

since better local economic conditions increase the average perceived returns to investing across all establishments.

2. If the firm's profitability is more sensitive to the state of the macroeconomy, the

responsiveness of investment to HQ local economic conditions will be higher.

Taking the cross-partial derivative of investment with respect to HQ local economic conditions l_c and the sensitivity of firm profitability to the the macroeconomy, b_i, yields:[55]

$$\frac{\partial I_i}{\partial l_c \partial b_i} = \phi\left(\bar{\pi} - b_i\left(m' + l_c\right)\right) + b_i \phi\left(\bar{\pi} - b_i\left(m' + l_c\right)\right)\left(\bar{\pi} - b_i\left(m' + l_c\right)\right)\left(m' + l_c\right) > 0.$$

[56]

3. If the firm is more financially constrained, the sensitivity of firm investment to HQ local conditions will be lower.

$$\frac{\partial I_i}{\partial l_c \partial \bar{\pi}} = -b_i \times \phi\left(\bar{\pi} - b_i\left(m' + l_c\right)\right)\left(\bar{\pi} - b_i\left(m' + l_c\right)\right) < 0$$

While this result depends on the specific distribution of the establishment-specific signal s_e, an alternative way of characterizing financial constraints will yield the same prediction more robustly (i.e., without the same reliance on a specific distribution of s_e). If financially constrained firms have a limited capital budget $\bar{I}$, such that $I_i < \bar{I} < 1 - \Phi\left(\bar{\pi} - b_i\left(m' + l_c\right)\right)$, constrained firms will (locally) exhibit no investment sensitivity to local economic conditions, i.e., $\frac{\partial I_i}{\partial l_c} = 0$. In contrast, unconstrained firms will still have positive investment sensitivity to local economic conditions, i.e., $\frac{\partial I_i}{\partial l_c} > 0$.

D Appendix D: Behavioral foundations of local conditions overweighting

The hypothesis that managers overweight personally observed local economic conditions in forming their expectations of the state of the macroeconomy is closely related to a few theories of decision-making from the behavioral economics literature.

One closely related behavioral bias is the availability heuristic. Kahneman and Tversky (1973) define the availability heuristic as a mental shortcut in which individuals evaluate a concept or decision based on easily recalled instances or examples. In the context of this study, one may consider direct or secondhand observations about the local economy the easily recalled examples for managers attempting to forecast the state of the future macroeconomy. And while the relevant variable to forecast is, for example, the probability distribution of aggregate growth, observations on the health of the local economy are overweighted by managers because they are personally observed or overheard. Moreover, personal observations about the strength of the local economy

[55] I assume that $(\bar{\pi} - b_i(m' + l_c)) > 0$ for possible values of m' and l_c.

[56] Alternatively, if we assume that managers view s_e and m as separate but noisy signals of the same underlying fundamental profitability of the establishment f_e, we can then interpret b_i as the parameter that dictates the optimal weight placed on m as a function of the relative noisiness of the signals s_e and m. This follows from a standard Bayesian updating problem where a Bayesian agent optimally updates her posterior estimate of the underlying variable, based on multiple noisy signals of the same underlying variable. This slightly altered interpretation of the model suggests the separate prediction that if the signal of establishment profitability s_e is noisier (i.e., b_i is higher), then the responsiveness of firm investment to HQ local conditions will be higher.

are likely to be easier to recall than the details of professional macroeconomic forecasts, and the ease of recall is a crucial component of the availability heuristic (Schwarz, Bless, Strack, Klumpp, Rittenauer-Schatka and Simons 1991).

However, overweighting local conditions not only reflects the impact of salient examples and personal experiences, but also that of *recent* ones. The empirical design of my study does not consider the effect of a salient one-off historical local economic shock, but rather that of recent local economic conditions, which change over time. This aspect of local conditions overweighting is related to the recency bias documented in the psychology literature, where individuals' decisions are found to be more sensitive to recent experiences (e.g., Hertwig, Barron, Weber and Erev 2004). It is also connected to studies showing that individuals form expectations of the future by extrapolating recent trends (e.g., Greenwood and Shleifer 2014).

Another related bias is the "law of small numbers," wherein individuals overestimate the extent to which small samples resemble the population at large (e.g., Tversky and Kahneman 1971, Camerer 1987, Rabin 2002). Tversky and Kahneman (1971) argue that this bias is best explained by the representativeness heuristic, in which individuals assess the similarity of the sample to the population along essential characteristics, but largely ignore sampling variation. If the local economy is seen as a small sample of the national economy (the "population"), the fact that managers overweight observations about the local economy's strength in inferring the strength of the national economy may be an example of the law of small numbers. However, managers may not view their observations of the local economy as a small data sample for the same underlying variable of the national economy. Managers may observe the strength of local consumer demand or overhear anecdotes about the tightness of the local labor market, but ultimately wish to forecast a different national variable such as GDP growth. In other words, the local "sample" may be drawn from a different underlying population distribution than the actual population the manager desires to forecast.