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The Contribution of Large and Small Employers to Job Creation in Times of High and Low Unemployment[†]

By Giuseppe Moscarini and Fabien Postel-Vinay*

We document a negative correlation, at business cycle frequencies, between the net job creation rate of large employers and the level of aggregate unemployment that is much stronger than for small employers. The differential growth rate of employment between initially large and small employers has an unconditional correlation of −0.5 with the unemployment rate, and varies by about 5 percent over the business cycle. We exploit several datasets from the United States, Denmark, and France, both repeated cross sections and job flows with employer longitudinal information, spanning the last four decades and several business cycles. We discuss implications for theories of factor demand. (*JEL* D22, E23, E32, J23, L25)

We present new empirical evidence that the net job creation of large firms or establishments ("employers") comoves negatively and more strongly with aggregate unemployment than the net job creation of small employers at business cycle frequencies. This fact holds for groups of large and small employers that account for similar shares of total employment. Specifically, we establish five facts:

Fact 1: The differential growth rate of employment between large and small US firms is strongly negatively correlated (in deviations from trend) with the contemporaneous unemployment rate, and varies by about 5 percent over the business cycle. Large employers on net destroy proportionally more jobs relative to small employers when unemployment is above trend, late in and right after a typical recession, and create more when unemployment is below trend, late in a typical expansion.

Fact 2: Fact 1 is not (only) due to different entry and exit patterns of large and small employers, but holds also for continuing firms and establishments, and for older, established firms.

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Fact 3: Fact 1 holds principally within, not across, sectors and states.

Fact 4: Fact 1 is not due to reclassification of employers into larger (smaller) size classes during an aggregate expansion (contraction). It appears, however, quantitatively stronger in datasets that lack longitudinal links, such as repeated cross sections.

Fact 5: Fact 1 is not unique to the United States: it holds in several countries of different sizes.

In order to establish these facts, we exploit a variety of datasets on employment stocks and flows by size of the employer: repeated cross sections (distribution of employment among firm size classes); semi-aggregate statistics containing limited longitudinal information, such as job flows by initial employer size; employer panels with full longitudinal information. Particularly useful proved data that we draw from the new Census Bureau's Business Dynamic Statistics (BDS), covering 1978–2009, as well as matched employer-employee datasets from Denmark and France. The different formats of these many datasets allow us to address, and relieve concerns about, the effects of two potential sources of bias. First, the *regression bias* is a well-known fallacy that creates the illusion of a negative size/growth relationship. We are not interested in the sign of that bias, but rather whether it changes over the business cycle. Second, the *reclassification bias* generates the illusion of our Fact 1, as employers are reclassified into larger-size bins as the economy grows. Longitudinal data allow us to circumvent both problems and to assess their (modest) magnitude (Fact 4).

We now discuss several implications of our findings. The firm size/growth relationship is the subject of a vast literature. Firm size is measured by either employment (as in Gibrat's 1931 seminal contribution) or assets, capital, or sales. This literature typically ignores business cycle effects. Our findings indicate that the size of a firm's employment may predict its growth depending on current labor market conditions, negatively when unemployment is high and positively when it is low. Omitting indicators of the current state of the aggregate labor market or aggregate economy may lead one to conclude that, on average, these cyclical effects wash out and size does not predict subsequent growth, which is Gibrat's law.

The new Fact 1 that we uncover is reminiscent of the idea of *cyclical upgrading of labor* in Okun (1973), a cross-industry pattern whereby employment reallocates from low- to high-paying industries in booms, and vice versa in recessions (see Bils and McLaughlin 2001 for a recent new interpretation). Instead, the phenomena that we emphasize hold within broad industries (Fact 3).

Our findings appear to contradict a well-established set of facts regarding the sensitivity of small firms to aggregate conditions and to monetary shocks. Upon closer inspection, rather than a contradiction, there is a difference in focus. The literature has studied the sensitivity of employment in firms of different sizes to identified, specific aggregate shocks, a conditional question; we document the *unconditional* behavior of large and small employers at business cycle frequencies. In addition, what we do find is a large and robust unconditional correlation around trend between the relative contributions of employers of different sizes to employment *growth* and the contemporaneous *level* of aggregate economic activity, as measured

by unemployment, but also by GDP. Conversely, GDP growth tends to be more correlated with employment growth at small employers than at large ones, but the difference with large employers is modest and not statistically significant.

In a very influential paper, Gertler and Gilchrist (1994) present evidence that small firms, which they argue are more credit constrained, are more sensitive to monetary policy shocks as measured by Romer and Romer (1989). Gertler and Gilchrist use the Quarterly Financial Report for Manufacturing Corporations (QFR), 1958–1992. This dataset defines firm size in terms of nominal sales or inventories and is a set of repeated cross section, which lacks longitudinal links, forcing Gertler and Gilchrist to make a correction to avoid the reclassification bias. Their conclusion that small manufacturing firms are more sensitive holds firmly at Romer-Romer dates. Of the six NBER-dated recessions in their sample period, only in 1970 and 1980–1983 does one see a clear collapse in the growth rate of sales and inventories of small firms relative to large ones (their Figure 1). The opposite occurs in 1961, while 1974 and 1982 appear fairly neutral.[1]

Our measure of performance and size is employment, not sales or capital, because this is what we are primarily interested in and because we have employment growth data that cover all sectors and are immune from reclassification bias, most notably the BDS for the United States and longitudinal business micro-data from other countries. Perhaps contrary to common wisdom, during the last "Great Recession"—that is, between March 2008 and March 2009—the growth rate of initially large employers was slightly more negative than that of small employers, and fell by 1.65 percent more from 2007–2008 (1.2 percent more if not detrending).

Going beyond unconditional correlations, our evidence may shed light on the nature of each specific cyclical episode. In the BDS, the relatively poorer job creation performance of large firms unfolds in each cycle for years after the trough, just like high unemployment; but in some recessions and not in others (1979, 1991) it begins during the recession itself. Going further back in time, King (1923) found that in the first quarter of 1920, the onset of the deep 1920–1921 recession in the United States, firms employing fewer than 21 workers had $\frac{1}{3}$ of total employment, but were responsible for just $\frac{1}{20}$ of the subsequent (sharp) reduction in aggregate employment.[2] The conventional wisdom that "small businesses are the engine of job creation" finds some empirical support in our data only at times of high unemployment, which are presumably when jobs are more needed. This statement clearly fails in tight labor markets.

The rest of this article is organized as follows. In Section I we illustrate conceptual reasons why one should be interested in our facts. In Section II we introduce our

[1] Sharpe (1994) replicates Gertler's and Gilchrist's findings at Romer-Romer dates for employment growth, by initial size defined in terms of net capital. He uses the NBER Manufacturing Panel from Compustat. As shown by Moscarini and Postel-Vinay (2009a), in the full Compustat panel comprising all industries, over 1975–2005, our pattern of differential growth rate by initial size emerges quite clearly at NBER-dated business cycles. In 1982, BDS data show that large firms lose more employment in the economy at large, but the opposite is observed in manufacturing. Chari, Christiano, and Kehoe (2007) also notice the distinction between Romer-Romer and NBER dates in the manufacturing QFR, and extend the analysis to the early 2000s. They focus on the growth rate of firm sales by size of a firm's assets, and do not find a differential behavior of large and small firms around NBER recessions, but rather a much higher sensitivity of small firms to Romer-Romer shocks.

[2] See King (1923, tables 6–8 and p. 31): "(. . .) these records give unequivocal evidence that it is primarily the large concern which is affected by a business depression (. . .)." We thank Mark Bils for this reference.

definitions and empirical methodology, discuss two potential biases that can affect our results, and how we cope with them. In Section III we present evidence from business micro-data that contain a longitudinal dimension and are immune from the reclassification bias, and we document our main finding for the United States. Section IV presents evidence from several other countries. Concluding remarks follow.

I. Conceptual Underpinnings

Firm size—as measured by its employment, assets, sales, inventories—has been the object of intense investigation in the economics literature. A common narrative contrasts large corporations, as major players in market economies, with a myriad of small businesses, individually vulnerable and often deemed in need of public support, but collectively endowed with vast political capital. This narrative is often colored by a romantic view of the lone, small-scale entrepreneur, the engine of innovation and growth, curbed by powerful conglomerates. The data draw a more mixed picture: large firms offer, on average, higher-paying, more stable jobs and are more productive, while small firms grow faster. Since productivity measures are typically revenue-based, the question remains whether large firms simply enjoy more market power or are indeed more efficient. On the other hand, the faster growth of small firms is at least in part just a survivorship bias, as their exit rate is also much higher.

In this paper, we continue this investigation but shift focus to aggregate fluctuations. We ask two questions: What does the firm size distribution teach us about the nature of aggregate fluctuations? And, conversely, can aggregate fluctuations shed new light on the determinants of firm size?

To answer these questions, it is useful to contrast aggregate shocks with idiosyncratic shocks of various kinds hitting a firm. While the latter are much bigger than the former, they are hardly exogenous to size. Most likely, unobserved traits affect both the dimension of a company and the idiosyncratic shocks it faces. Aggregate shocks, however, are arguably exogenous to any individual firm, no matter how big or small, thus providing identification power. Gertler and Gilchrist (1994) exploit this insight and use the Romer-Romer monetary policy shocks to empirically demonstrate the importance of credit constraints. Most extant theories of credit constraints, whether based on information asymmetry or limited commitment, predict that small firms are more likely to be constrained. Therefore, any aggregate negative shock to the availability and affordability of credit should impact more small than large firms. Such shocks, in turn, are often associated with downturns in overall economic activity. Therefore, small firms should suffer more during recessions that originate from monetary/credit tightening, and conversely when the economy expands and credit flows again.

Financial market imperfections are a type of capital adjustment cost. Moscarini and Postel-Vinay (2010a) argue that hiring and turnover frictions, a form of *labor* adjustment cost, have different predictions for the relative performance of large and small firms over business cycles. Namely, the *growth* of employment at initially large firms comoves more strongly with the *level* of aggregate unemployment than at small firms. The key insight is that large firms are typically more productive, can pay more, and thus can successfully poach workers from smaller competitors. This makes their hiring less dependent on the availability of unemployed workers, hence more brisk in late stages of expansions. In contrast, when the economy

expands and unemployment falls, small firms cannot keep pace because they find it hard to keep hiring. When the economy enters a downturn, large firms have more employment accumulated through poaching that they now want to shed. Small firms were previously more constrained in their growth by search and hiring frictions, and thus now shrink not as quickly. As in the credit frictions story, small firms are more constrained. But credit constraints bind more in recessions, when credit dries up, while hiring constraints bind more in booms, when the unemployment reservoir dries up. (See Moscarini and Postel-Vinay 2010a for a formalization of the labor-search mechanism.)

To conclude this section, we suggest an alternative explanation of the facts that we document, within a credit constraints framework. If the monetary authority follows a Taylor rule and eases its policy when the economy slumps, then small, credit-constrained firms will benefit more. If the aggregate slump itself is originally caused by a financial shock, then on impact small firms should suffer more, and then quickly rebound thanks to the central bank reaction. If the aggregate shock is real in nature, then only the endogenous component of monetary policy should manifest itself. As the economy recovers and unemployment falls, monetary policy tightens and curbs the growth of small firms, which are then outperformed by larger, financially unconstrained competitors. While in our preferred theory, unemployment impacts differential job creation directly, in this alternative hypothesis it does so indirectly, through its effect on the monetary policy stance. If this hypothesis is correct, the evidence that we present in this paper shows that the monetary policy reaction is quite strong, possibly excessive, as small firms typically do better than large ones in the years following each recession. Moscarini and Postel-Vinay (2010b) uncover a similar pattern in US stock returns, in that the small cap premium comoves negatively with unemployment. But their estimation of a structural VAR suggests that unemployment has an independent effect on relative stock returns beyond the indirect impact through monetary policy.

II. Methodology

The main purpose of this paper is to document changes in employment by size of the employer at business cycle frequencies. In this section we lay out our empirical definitions and methodology. In the following sections we apply them to various datasets.

A. *Definitions*

Our notion of business size is employment rather than capital, assets, or sales. This choice is motivated by the theoretical work in Moscarini and Postel-Vinay (2009a, 2010a). They identify in a firm's productivity and employment level the two main determinants of the contract that the firm posts and that, in turn, determine hiring and retention, thus, ultimately, firm size. Since accurate measures of productivity are hard to obtain, and those that do exist are highly correlated with employment, in this paper we focus on the latter. By "employers" we mean either firms or establishments, depending on the dataset at hand.

The standard measure of cyclicality of a variable is the unconditional correlation of its deviations from trend with a filtered measure of aggregate economic activity,

typically GDP. When partitioning the variable of interest (in this case, employment) by some criterion that is related to the business cycle (firm size), this methodology runs into a fallacy, which we dub "reclassification bias." When the economy expands, the size of a typical employer also grows, and an increasing share of total employment appears mechanically in the group of large employers, but the identity of those employers changes over time. To circumvent this problem, we study instead the difference in employment growth rates between *initially* large and small employers, each taken as a group. If this difference is positive, then large employers grow faster, and their share of employment rises, but not vice versa, as explained above. As we will see shortly, studying the growth-size relationship has perils of its own, some known, some novel to our analysis. Employers of different sizes may add systematically more or fewer jobs, an issue of great conceptual confusion and political importance. By taking the difference in growth rates and focusing on its fluctuations around trend, rather than on its level, we sidestep this issue of the relative contribution of small businesses to job creation.

Formally, let L_{it} denote the number of employees working for employer i at (discrete observation) time t, and define a weighted-average size between $t-1$ and t:

$$L_{it-1}^{(\alpha)} = \alpha(L_{it},L_{it-1}) \cdot L_{it} + [1 - \alpha(L_{it},L_{it-1})]L_{it-1}.$$

Here, $\alpha : \mathcal{N}^2 \to [0,1]$ is the weight on end-of-period size, L_{it}, and $1-\alpha(L_{it},L_{it-1})$ on initial size, L_{it-1}, a weighting function that can depend on both numbers. Let the weighted growth rate

$$g_{it}^{(\alpha)} = \frac{L_{it} - L_{it-1}}{L_{it-1}^{(\alpha)}}.$$

A common choice of weight is to set $\alpha \equiv 1/2$. It has the twofold advantage of making the growth measure symmetric (which is identified in the statistics literature as an important property; see Törnqvist, Vartia, and Vartia 1985),[3] and to be well defined both for entrants, who have $L_{it-1} = 0 < L_{it}$, and for closing employers, who have $L_{it-1} > 0 = L_{it}$. It will be our preferred choice in this paper.

Finally, let β denote another weighting function, and $\overline{L} > \underline{L} > 0$ two integers that define "large" employers ($L_i^{(\beta)} \geq \overline{L}$) and "small" employers ($L_i^{(\beta)} \leq \underline{L}$) according to β-weighted size. We consider the growth rate between $t-1$ and t of employment at all employers that are classified at time $\tau \leq t$ as large:

$$g_{\tau,t,LARGE}^{(\alpha,\beta)} = \frac{\sum_{i:L_{i\tau}^{(\beta)} \geq \overline{L}} (L_{it} - L_{it-1})}{\sum_{i:L_{i\tau}^{(\beta)} \geq \overline{L}} L_{it-1}^{(\alpha)}}, \tag{1}$$

[3] We thank an anonymous referee for this reference.

and a similarly defined $g^{(\alpha,\beta)}_{\tau,t,SMALL}$ (with $L^{(\beta)}_{i\tau} \leq \underline{L}$) for the small size class. Notice that, using the weighting β, we can choose the size class over which to compute net job creation (the numerator) by either initial, average, or end-of-period size observed at any given date $s < t$. This initial date could be either fixed once and for all with longitudinal data, or reset every period, either at the beginning ($\tau = t - 1$) or the end ($\tau = t$) of the period. Also, we can assign to size classes the numerator (net job creation) independently of the denominator (baseline employment), using different weighting functions α and β.

For some countries (see Section IV), access to firm-level data further allows us to consider the following alternative to equation (1) as a definition of the average employment growth rate in an employer size category:

$$g^{(\alpha,\beta)}_{\tau,t,LARGE} = \frac{1}{\#\{i : L^{(\beta)}_{i\tau} \geq \overline{L}\}} \sum_{i:L^{(\beta)}_{i\tau} \geq \overline{L}} g^{(\alpha)}_{it}. \tag{2}$$

While both equations (1) and (2) are valid measures of the average growth rate in a category, (2) is an *unweighted* average, whereas each firm in (1) is weighted by its reference size, $L^{(\alpha)}_{it-1}$. As such, the behavior of a series of average growth rates based on the definition (1) is liable to be dominated by one or a few exceptionally large firms, especially if there are only a few firms with very different sizes in the "large firm" category (as will be the case in the Danish data; see Section IV). The unweighted average (2) is immune from that problem and indeed is a more accurate estimator of the performance of the *typical* firm in a size category.

We are interested in the *differential* growth rate by size class, defined as the difference in growth rates between large and small employers, based on either definition, equations (1) or (2):

$$\Delta g^{(\alpha,\beta)}_{\tau,t} = g^{(\alpha,\beta)}_{\tau,t,LARGE} - g^{(\alpha,\beta)}_{\tau,t,SMALL},$$

and in particular in how its deviations from trend correlate with the current state of the aggregate economy. Our main statistics of interest is corr $\left(\widehat{\Delta g}^{(1/2,0)}_{\tau,t}, \hat{c}_t\right)$, where hats denote absolute deviations from trend and c is an indicator of aggregate economic conditions. In line with the underlying theoretical framework, our preferred choice for c_t is the civilian unemployment rate u_t. We also experiment with real GDP and with its growth rate.

To detrend series, we use a Hodrick Prescott (HP) filter. For the unemployment rate, following Shimer (2012), we use a high smoothing parameter (8.1E6 at monthly frequency). For $\widehat{\Delta g}^{(1/2,0)}_{\tau,t}$, a high smoothing parameter is also necessary so that no obvious cyclical pattern is left visible in the fitted trend. Fitting a linear trend makes little difference in this case. All gross and net job flow rates reported later are similarly detrended. Log GDP is smoothed as usual with parameter 1,600 at quarterly frequency.

In order to compute contemporaneous correlations, we define $\hat{c}_t$ as the simple average of the deviations from trend of c at $t - 1$ and t. If c is observed at higher frequency than the (differential) growth rate—for example, the unemployment rate

is reported monthly while the time interval for employer growth is either a quarter or a year, depending on the dataset—then $\hat{c}_t$ is the average deviation of c_t from its own trend at *all* points in time between $t-1$ and t.

As we have learned from the literatures on economic growth of countries and firms, specifically Gibrat's law (Sutton 1997), the size/growth relationship is rife with statistical fallacies. Sample selection by size of the firms at either the beginning or end of the sample is not an issue for us, as we will exploit either censuses or representative samples of employers. We now discuss two potentially more serious issues.

B. *The Regression Bias*

The first potential fallacy arises if employer size L_{it} is mean-reverting. Then small employers will tend to grow more than large ones, as they all converge back to a long-run middle ground. This generates the illusion of a negative size/growth relationship, Galton's fallacy, if one uses the conventional measure of growth rate $g_{it}^{(0)} = L_{it}/L_{it-1} - 1$.

Using $g_{it}^{(1/2)}$ as the measure of growth, namely average employment between $t-1$ and t as the base for the growth rate between $t-1$ and t, is known to reduce the mean reversion fallacy (see, e.g., Davis, Haltiwanger, and Schuh 1996). Yet the relevant question for us is whether mean reversion also affects the dynamics at business cycle frequencies of the differential growth rate $\Delta g_{\tau,t}^{(\alpha,\beta)}$. This is a chiefly empirical question. Theoretically, the answer would depend on the specific statistical model of firm growth implied by the underlying structural model. To illustrate the potential problem, we take an extreme example and suppose that firm size is serially uncorrelated; i.e., size is "purely idiosyncratic." Then (normalizing to zero the unconditional mean of $\ln L_{it}$):

$$\begin{aligned}\Delta g_{t-1,t}^{(0,0)} &\simeq \mathbf{E}[\ln L_{it} - \ln L_{it-1} | L_{it-1} \geq \overline{L}] - \mathbf{E}[\ln L_{it} - \ln L_{it-1} | L_{it-1} \leq \underline{L}] \\ &= \mathbf{E}[\ln L_{it-1} | L_{it-1} \leq \underline{L}] - \mathbf{E}[\ln L_{it-1} | L_{it-1} \geq \overline{L}].\end{aligned}$$

This is both negative and decreasing in response to a mean-preserving spread of the distribution of L_{it-1}. Our difference in growth rates is thus plausibly increasing in idiosyncratic uncertainty. Then, if the variance of idiosyncratic shocks increases in slumps, as suggested by the countercyclical dispersion in various measures of business performance (Bloom 2009), this could create the illusion of cyclicality in the differential growth rate. The problem mostly arises from classification by initial size, when $\beta = 0$. To eliminate this possibility, when available we will use longitudinal data that begin at date t_0 to compute $\Delta g_{t_0,t}^{(1/2,0)}$ for all available dates $t = t_0 + 1, \cdots, t_0 + T$ after t_0. For T large enough, the effects of mean reversion would have presumably washed out. As we will see, our empirical results are robust.

C. *The Reclassification Bias*

Consider $\Delta g_{t-1,t}^{(\alpha,1)} = \Delta g_{t,t}^{(\alpha,0)}$, which is the differential growth between $t-1$ and t of employers classified by their end-of-period size at t. If the economy grows,

and all employers with it, given time-invariant size cutoffs $\bar{L} > \underline{L}$, employers tend to grow in size with the economy and to jump into higher and higher size bins. It then appears that more and more job creation is attributed to larger-size classes, and the differential growth rate $\Delta g_{t,t}^{(\alpha,0)}$ is more likely to be positive. The converse is true when employment shrinks. This mechanically generates the fact that we aim to document.

Davis, Haltiwanger, and Schuh (1996) discuss the implications of this bias for measuring the relative contribution of small businesses to job creation *on average*.[4] We ask the different question of whether this bias changes at business cycle frequencies. This bias appears in different forms in the various types of datasets that we employ. Its most straightforward manifestation arises when using repeated cross sections of employment (detrended) levels, or shares, of size classes. That is, if we lack longitudinal links and only observe a time series of

$$s_{jt} = \frac{\sum_{i:L_{it}\in[\underline{L}_j,\underline{L}_{j+1})} L_{it}}{\sum_i L_{it}},$$

which is the conventional definition of the share of employment at time t working at employers of size in class j, then the change in s_{jt} is an estimate of employment growth for size class j. When small firms grow faster than large ones, their share of total employment rises. As small employers gain size, however, they are reclassified into larger size classes, so repeated cross sections are subject to reclassification bias.

This bias also appears in a more complex form in the *Business Employment Dynamics* (BED) dataset, maintained by the Bureau of Labor Statistics (BLS). Recall that Job Creation (JC) is the addition of employment positions at all units that expand, and vice versa for Job Destruction (JD), so JC − JD is net job creation. The "dynamic sizing" method adopted by the BLS to impute job flows to firm size categories in BED modifies class assignments at infra-quarterly frequency for firms crossing the line between two size classes.[5] This method obviously introduces a reclassification bias.

In order to quantify this bias in the data, we use longitudinal business micro-data, where we can first allocate an employer to an initial size class, either every period before computing growth or once and for all at the beginning of the sample t_0, and then compute $\Delta g_{t_0,t}^{(1/2,0)}$. This takes care not only of mean reversion but, even more strongly, of reclassification, as we never allow employers to change size class, even after decades. We find that the size of a firm at a point in time predicts its growth decades later in a way that depends on contemporaneous aggregate economic conditions. The downside of such long links is the possible occurrence of a survivorship bias among continuing firms.

[4] They refer to what we call the reclassification bias as the *size distribution fallacy*. They provide an illustrative example of its potential quantitative importance for the estimated size-growth relationship, but do not address its cyclical implications.

[5] For example, if firm i has $L_{it-1} = 7$, $L_{it} = 15$, then, of the eight jobs created on net, two are attributed to the size class [5,9] and six to the size class [10,19]. The denominator in the published BED job flow rates is the average $L_{it-1}^{(1/2)}$.

Nevertheless, we will also present evidence from datasets where a reclassification bias may arise either because the allocation of employment stocks and flows is not made by initial size or because we proxy growth rates of employment by size with those of employment shares from repeated cross sections. By comparing these results to those from longitudinal datasets, we will show that the reclassification bias is quantitatively substantial when the aggregate economy moves sharply, as in the recent Great Recession, and relatively modest (although still visible) at other times.

D. *Entry and Exit*

Most firms enter at the bottom of the size distribution, and the contribution of entry to net JC typically declines by size. For establishments, these patterns are similar but weaker, because existing firms open establishments of all sizes. Similarly, exit may be systematically related to the size of the firm or establishment in its last period of existence. A natural question is whether the stronger negative correlation of job creation by large firms with aggregate unemployment is due entirely or in part to differential entry and exit by large and small firms at different stages of the business cycle.

We provide evidence with and without exiting employers, to check whether our Fact 1 is driven by different patterns of exit (extensive margin) or net JC at continuing employers (intensive margin) across size classes. When the datasets at our disposal do not allow a distinction between firm exit and establishment closing by surviving firms, we either include or exclude establishment deaths, which contain both types of exit.

For entrants, the main issue is the attribution of their first growth to their initial size class. Whenever the data allow it, we present our evidence with entrants both included in and excluded from the "small" size class. We can and will distinguish between entry of a new firm and opening of a new establishment by an existing firm.

III. US Evidence

A. *Business Dynamic Statistics* (*BDS*)

The Dataset.—The primary source of information on the identity, location, employment, sales, payroll, and industry of all US businesses is the Census Bureau's Business Register (BR), formerly known as Standard Statistical Establishment List. An "establishment" is a physical location; information on BR active establishments is updated continuously from tax records. A "firm" is a a collection of one or more establishments under common ownership and control, identified by a census alpha number. Information on BR firms is updated annually through the Company Organization Survey and the Annual Survey of Manufacturers. Starting in December 2008, the Center for Economic Studies at the Census Bureau has made publicly available a set of semi-aggregate statistics from the BR, under the name of *Business Dynamic Statistics*. BDS covers approximately 98 percent of US private employment, and contains information on establishment-level employment stocks and job flows, for continuing, entering, and exiting establishments, at annual frequency for the 1976–2009 period, broken down by location and industry of the establishment,

and by age and size of the parent *firm*. Two notions of firm size are available in BDS: average size between last year and this year, partly after the job flows have taken place, and size *before* the flows are measured, the "initial firm size." These two allow us to avoid the reclassification bias and to calculate employment growth by initial size of the employer, as well as to gauge the extent of any regression bias.

More specifically, we calculate the growth rate of employment in a size class as the ratio between net job creation—namely gross JC minus gross JD—over the period running from March of year $t-1$ to March of year t, and average employment between $t-1$ and t, i.e., by $L_{it-1}^{(1/2)}$. Everything is classified by a measure of initial firm size, as of March of year $t-1$ (i.e., $L_{it-1}^{(0)}$). In BDS, *after* computing growth, firms are reclassified into their new size classes, and their new size becomes their initial size in the following period, March of year t to March of year $t+1$. One exception are new firms (which have initial age and size 0), which BDS assigns to the initial size class that corresponds to their post-entry, end-of-period size. Hence, we first reset the initial size of new firms to zero and then shift their contribution to net JC from the size class they reach ex post to the initial size class "0 employees," which is obviously part of the "small firm" group. For preexisting firms, the employment at time t by initial (year $t-1$) size class reported in BDS is the stock in year t. We recover the employment of those firms, in each initial size class, in year $t-1$ by subtracting from employment at t their net JC between $t-1$ and t. Finally, we obtain in each year the stock of employees in each size class (zero for entrants) and the net addition/subtraction to employment over the following year. We then detrend and study the differential growth rate of employment between initially large and small employers, our main focus, in our notation $\widehat{\Delta g}_{t-1,t}^{(1/2,0)}$.

As mentioned, BDS currently covers 1977–2009. Its job flow data, however, show a suspiciously high growth rate of employment at small firms in 1978 (that is, in 1977–1978), and a correspondingly small growth rate at large firms. Both anomalies disappear in 1979. Indeed, 1978 and 1979 is the only pair of years in the sample when the two growth rates move, and quite strongly, in opposite directions.[6] This suggests some kind of severe and temporary measurement error in BDS in the relative share of employment of the two groups in 1977, with the share of small employers underestimated, thus causing issues of unknown nature with its initialization. For these reasons, we use data covering 1979–2009 only.

Our choice of the size cutoffs that define "large" and "small" employers is necessarily arbitrary, but is guided by both data availability and the goal to create two groups of comparable size in terms of aggregate employment share (see Table 1). This balance avoids our time-series patterns being driven by one size class comprising most of total employment.[7] We choose cutoffs of 50 and 1,000 employees, which

[6] Stock BDS data on employment shares by firm size show an unusually large drop in the share of employment at large firms in 1977–1978, mostly explained by a huge rise in the share at medium firms, while the share at small firms rises in 1977–1978 and then drops the year afterward. To further investigate this anomaly, we use the *County Business Patterns* (CBP), which are collated from the same underlying microdata as BDS, but contain only stocks and not flows, and are organized by size of the establishment, not of the firm. The share of employment at small establishments declines slightly in 1977–1978. Also, over the overlapping 1977–2005 period, the growth rate of total employment in BDS and CBP differs in 1977–1978 by the largest amount (over 2.6 percent), against an average discrepancy of 0.4 percent and a second-highest of 1.5 percent, over the same period.

[7] We thank an anonymous referee for this observation.

Table 1—Average Establishment Size and Aggregate Employment Share by Size Class of the Parent Company

Firm size category (employees)	Mean establishment size (employees)	Employment share (percent)	Cumulated employment share (percent)
1 to 4	2.1	5.4	5.4
5 to 9	6.4	6.4	11.8
10 to 19	12.4	7.73	19.53
20 to 49	24	10.66	30.2
50 to 99	39.1	7.57	37.77
100 to 249	48.9	8.7	46.47
250 to 499	53.5	5.62	52.09
500 to 999	57.4	5.11	57.2
1,000 to 2,499	61.8	6.91	64.11
2,500 to 4,999	58.4	5.19	69.3
5,000 to 9,999	56	5.38	74.68
10,000 +	62.2	25.32	100

Source: BDS and authors' calculations.

also create two groups of essentially all (respectively) single- and multi-establishment firms. Our main results are qualitatively robust to choosing a small-firm cutoff of either 20, 100, or 500 and a large-firm cutoff of 500. Robustness checks are collected in an online Appendix.

The Aggregate Picture.—In our conceptual framework, the difference in growth rates between (initially) large and small firm groups is driven to a large extent by the level of unemployment. Figure 1 plots the differential growth rate, including the contribution of entry and exit, against the civilian unemployment rate from the BLS, both detrended, with shaded areas indicating NBER-dated recessions. Consistent with our definitions, our time convention in this and all following graphs is that growth observations in period t denote growth between $t-1$ and t. In the case of the BDS, observations occur every year in mid-March, so employment growth in year t takes place mostly during calendar year $t-1$. It is important to keep this in mind to correctly interpret the behavior of the differential growth rate during NBER-dated recessions. To guarantee that the two variables are exactly contemporaneous, the unemployment rate in year t reported in the graph is the average of monthly deviations from trend of the unemployment rate from March of the previous year to February of the current year, both included.

The central finding of this paper is visually clear and confirmed by the statistically significant correlation of -0.52 (p-value of 0.003) between the large- to small-employer growth differential and unemployment:[8]

Fact 1: *The differential growth rate of employment between large and small US firms is strongly negatively correlated* (*in deviations from trend*) *with the contemporaneous unemployment rate, and varies by about 5 percent. Large employers on net destroy proportionally more jobs relative to small employers when*

[8] Unless otherwise indicated, all correlations reported in the paper are statistically significant. In the interest of space, we do not systematically report p-values, which we keep available on request.

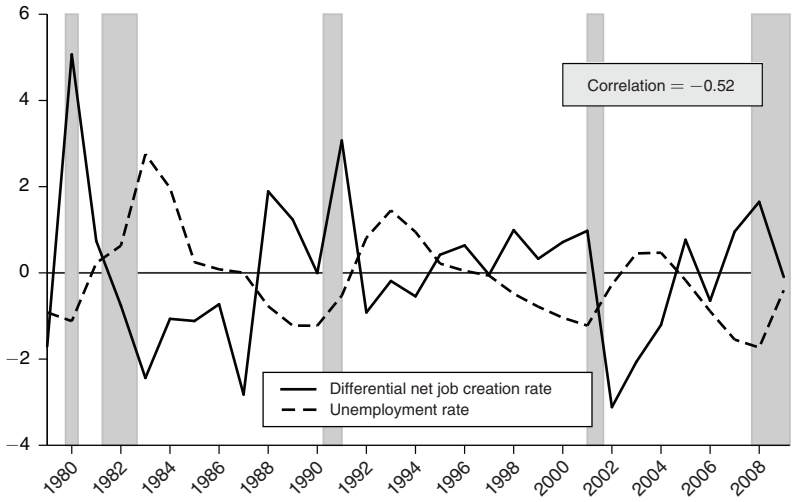


FIGURE 1. DIFFERENTIAL GROWTH AND UNEMPLOYMENT

Notes: Categories defined each year as < 50 and $> 1{,}000$. Shaded areas indicate NBER contractions. Both series detrended.

Source: BDS and authors' calculations.

unemployment is above trend, late in and right after a typical recession, and create more when unemployment is below trend, late in a typical expansion.

This unconditional correlation is our main object of interest. As we will see, this finding is very robust.

To further examine the lead/lag structure of that correlation, Table 2 shows correlation coefficients between the large- to small-employer growth differential, $\widehat{\Delta g}^{(1/2,0)}_{t-1,t}$, and the contemporaneous and lagged values of the cyclical component of the unemployment rate. (Once again, $\hat{u}_t$ denotes the average detrended unemployment rate between March of year $t-1$ and February of year t.) The table further shows correlations of either $\hat{u}_t$ or $\hat{u}_{t-1}$ with, separately, the detrended growth rates of employment at initially large and small firms, $\hat{g}^{(1/2,0)}_{t-1,t,LARGE}$ and $\hat{g}^{(1/2,0)}_{t-1,t,SMALL}$. While the growth differential is negatively correlated with both current and lagged unemployment, those negative correlations have different origins: when contemporaneous unemployment is high, large firms do particularly poorly relative to trend, whereas when lagged unemployment is high, it is small firms that do particularly well. Both patterns are consistent with our conceptual framework, which emphasizes that small firms benefit from high unemployment, as that relaxes hiring constraints.

Finally, to shed more light on what is behind the correlation highlighted in Fact 1, it is useful to zoom in on individual cyclical episodes. This "conditional" analysis of individual business cycle episodes is conceptually distinct from, and necessarily more speculative than, our unconditional Facts 1–5. In Figure 2 we plot the net employment growth rates of initially small and large firms separately. The latter is higher in the few years preceding each of the four NBER peaks and lower in the few years after. Zooming in on individual recessions, large firms suffered much more

Table 2—Unconditional Correlations, BDS

	$\widehat{\Delta g}^{(1/2,0)}_{t-1,t}$	$\hat{g}^{(1/2,0)}_{t-1,t,LARGE}$	$\hat{g}^{(1/2,0)}_{t-1,t,SMALL}$
$\hat{u}_t$	−0.52 (0.003)	−0.45 (0.010)	−0.02 (0.934)
$\hat{u}_{t-1}$	−0.36 (0.054)	0.22 (0.245)	0.53 (0.002)

Note: p-values in parentheses.

during the 1982 and 2001 recessions, in both cases taking years to recover relative to small firms. Even in the Great Recession of 2008–2009, the growth rate of employment at initially large firms declined by 1.65 percent more. The 1991 recession, the mildest in the sample period, appears to break the pattern. One-third of the March 1990–March 1991 employment growth reported in Figure 2 as the 1991 observation took place between March and July 1990, when the economy was still expanding. Theories about the cause of the 1990–1991 recession range from real shocks, such as the downsizing in military spending due to the end of the Cold War, to financial shocks, either the Savings and Loan crisis or the Romer-Romer episode of 1989. The latter interpretation may explain the short-lived but sharp contraction of small firms' employment in 1990–1991. In 1979, small firms definitely suffered more, in line with the common wisdom of the Volcker recession, the result of monetary tightening. In both the 1979 and 1991 episodes the poorer performance of small firms lasted just one year, and in every recovery in the sample, large firms remained more sluggish for years. Overall, this picture corroborates only in part the common wisdom that small businesses are the engine of job creation: small firms appear to create more jobs as a fraction of their employment only when unemployment is high (which is, arguably, when jobs are most needed).

Figure 3 decomposes the differential (initially large minus small firms) net JC rates of Figure 2 into differential gross JC and JD rates. In March 1979 to March 1980, namely the 1980 datapoint in the graph, large firms created more jobs and destroyed fewer than small firms, each group relative to their own trends, again consistent with the conventional wisdom that a monetary shock initiated the recession. The pattern quickly reversed during the later phase of the double dip. The action was all on the destruction side in the year leading to March 1983; namely, contracting large firms shed a much larger proportion of their payroll than contracting small firms. Less dramatic but qualitatively similar is the pattern after the 1991 recession. In 2001, both JC and JD contributed to the worse performance of large employers, more represented in the IT sector, which suffered the brunt of that shock.[9] In 2008–2009, the gross JC of large firms, which was markedly higher coming into the recession, dropped sharply. Between recessions, it is a surge in gross JC by large firms relative to small ones late in expansions that accounts for their better performance in those phases, which provides further support to the theoretical framework underlying our empirical exercise.

Alternative Indicators of Aggregate Economic Conditions.—Our conceptual framework emphasizes the level of unemployment as the determinant of the relative

[9] We thank an anonymous referee for suggesting this interpretation.

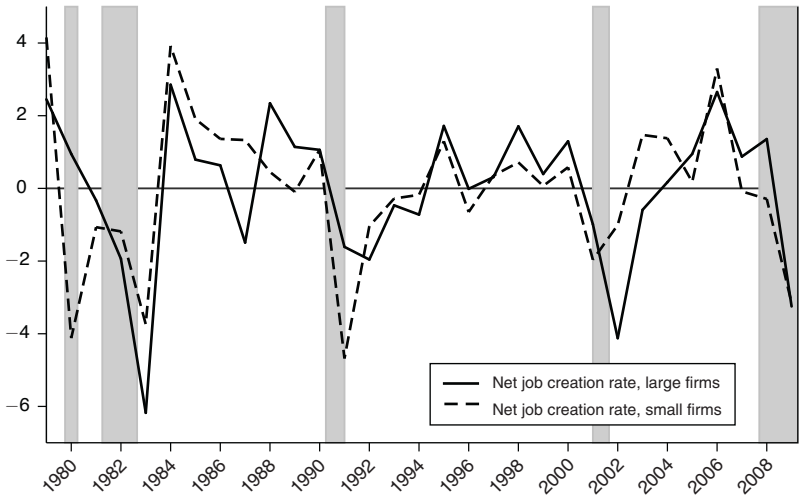


FIGURE 2. NET JOB CREATION RATES

Notes: Categories defined each year as < 50 and $> 1{,}000$. Shaded areas indicate NBER contractions. Both series detrended.

Source: BDS and authors' calculations.

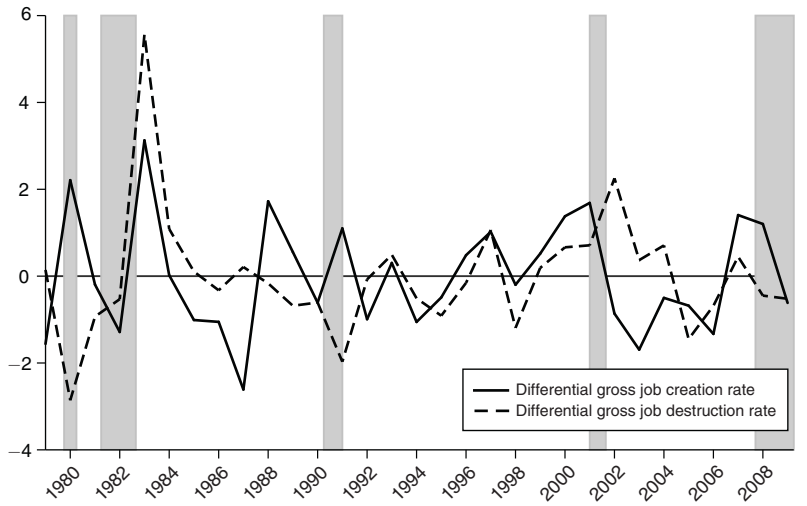


FIGURE 3. DIFFERENTIAL GROSS JOB FLOWS

Notes: Categories defined each year as < 50 and $> 1{,}000$. Shaded areas indicate NBER contractions. Both series detrended.

Source: BDS and authors' calculations.

contributions of large and small employers to total job creation. Accordingly, Fact 1 refers to unemployment. Going beyond our conceptual framework, for other purposes it is also potentially interesting to explore more conventional GDP-based measures of aggregate economic activity. For example, the vast literature on Gibrat's law has never considered a potential role for the business cycle.

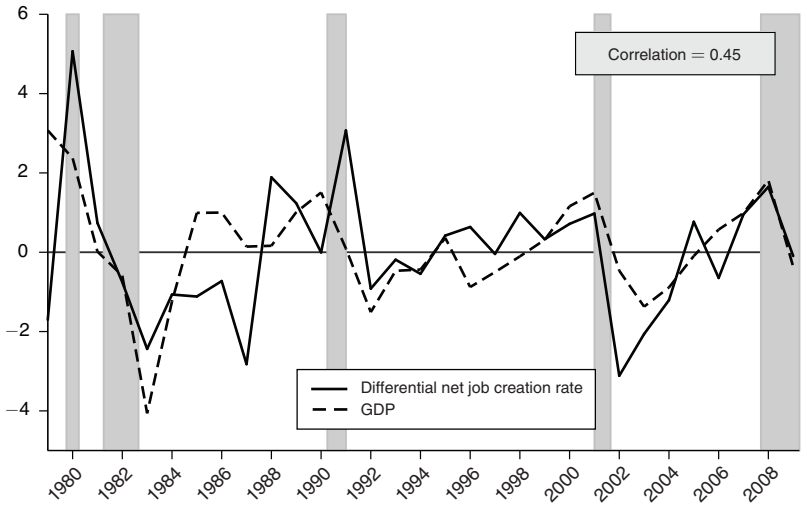


FIGURE 4. DIFFERENTIAL GROWTH AND GDP

Notes: Categories defined each year as < 50 and $> 1{,}000$. Shaded areas indicate NBER contractions. Both series detrended.

Source: BDS and authors' calculations.

Figure 4 replicates Figure 1 but replaces detrended unemployment with detrended GDP, specifically with the quarterly HP-filtered logarithm of GDP (chain-weighted, 2005 prices), averaged from the second quarter of year $t - 1$ to the first quarter of year t. This timing coincides almost perfectly with the March-to-March growth rate of employment in BDS. Now our Fact 1 should appear as a positive comovement, which is indeed quite visible in the graph, even beyond the correlation between the two series, equal to 0.45 and statistically significant at conventional levels.

As mentioned, the relationship between employment levels of size classes and the level of aggregate activity is likely plagued by reclassification bias. We can, however, take first log differences of this relationship and correlate our differential growth rate by initial size with the *growth rate* of GDP, again from second quarter to first quarter. This correlation is a measure of the relative cyclicality of large and small employers, which is interesting per se, and differs from our primary focus. Figure 5 does precisely that. Now the positive correlation breaks down both visually and statistically: the contemporaneous correlation is -0.24, not statistically significant (p-value of 0.20). We also experimented extensively with different firm-size cutoffs and subsample periods, excluding the last recession either from the sample altogether or from the sample used to fit a linear trend (some of those results are reported in the online Appendix). We found that, although consistently negative, the correlation between GDP growth and our differential growth rate by initial employer size is small in magnitude, most often statistically nonsignificant, and highly sensitive to the choice of size cutoffs, detrending method, or sample period. While that correlation can be found to be statistically significant in some cases (for example, when detrending the growth rates of large and small firms by using only 1979–2005 data to build the linear trend, then extrapolating to 2006–2009), this result appears rather fragile. In contrast, our Fact 1 remains

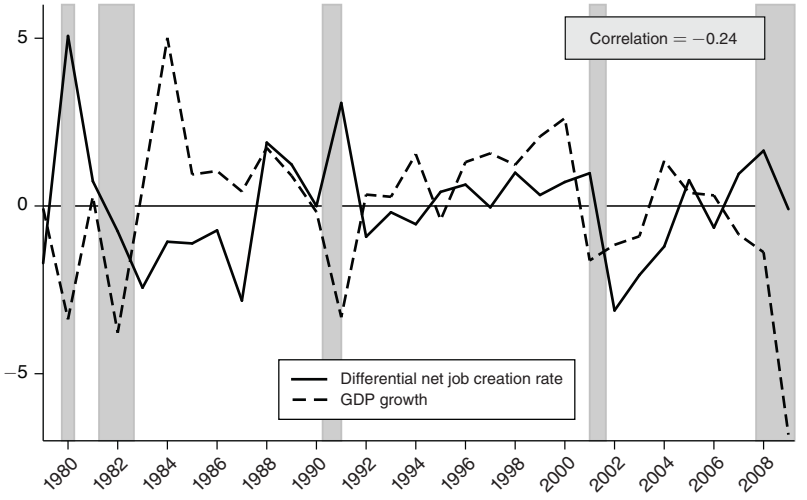


FIGURE 5. DIFFERENTIAL GROWTH AND GDP GROWTH

Notes: Categories defined each year as < 50 and $> 1{,}000$. Shaded areas indicate NBER contractions. Both series detrended.

Source: BDS and authors' calculations.

strongly significant, both quantitatively and statistically, with any of the sample dispositions that we have tried. In line with the conceptual framework described in the previous section, the only robust relation that we find in the data is between growth rates by firm size and level of contemporaneous aggregate economic activity.

Entry and Exit.—We can isolate the contributions of entry and exit by eliminating from our computations new firms, new establishments, and/or closing establishments. In the interest of brevity we do not report plots but only correlations between the differential growth rate of employment of large versus small employers and the unemployment rate over the period 1979–2009. When we exclude new firms whose initial employment is zero (so they belong in the "small" group, fewer than 50 employees) but which create much new employment, this correlation is -0.54. When we also exclude new establishments, even if added by preexisting firms, the correlation is -0.58. To exclude closing establishments, we need to subtract their employment from the initial stock, as well as their JD from the total employment growth, for each size class. The correlation then equals -0.37. Finally, when we exclude both new and closing establishments, the correlation is -0.51. Qualitatively, our main Fact 1 is robust to all possible treatments of entry and exit.

Recent work on business micro-data (e.g., Foster, Haltiwanger, and Syverson 2008) as well as established theories of firm dynamics (Jovanovic 1982) point to the age of a firm as a major predictor of its future behavior. Older firms are much less volatile and fast-growing, conditional on survival, than younger firms. Furthermore, our underlying theoretical framework focuses on firm size as a proxy for productivity. This approximation is likely to be more accurate for older firms, which are closer to their steady state size, while very young firms are still learning their true quality.

Thus, we would expect Fact 1 to emerge even more strongly among established firms. We repeat our computation by restricting attention to firms that are initially at least three years old, where the age of a firm is the age of the oldest establishment it owns, and is typically not reset to zero by mergers and acquisitions. This restriction automatically excludes entrants, and focuses on the behavior of more established companies. We find a correlation of −0.61 between the differential growth rate of employment in initially large versus small older firms and the unemployment rate. As expected, Fact 1 is strengthened. If we exclude the contribution to JC by new establishments opened by these older firms, the correlation is −0.53. If we exclude the contribution to JD by closing establishments, the correlation is −0.51. Finally, if we exclude new and closing establishments, and focus only on continuing ones, the correlation is −0.41. For firms that are at least four years old, the results are virtually unchanged.

Fact 2: *Fact* 1 *is not due* (*only*) *to the different entry and exit patterns of large and small firms, but holds also for continuing firms and establishments, as well as for older, established firms.*

Industry Patterns.—We now dig deeper and check whether the basic Fact 1 that we uncover, that employment at initially larger firms is more strongly correlated with aggregate economic activity, holds within or across geographical locations and industries. One important proviso is that, unlike initial size, which refers to the parent company, the location and industry in BDS refer to the establishment. It is of course impossible to attribute a unique location and industry to most large firms that have establishments in many US states and industries. Moreover, looking within industries or states requires using cross-tabulations by two or three different criteria (e.g., net JC by initial firm size, within each industry or state, and by firm age to reassign entrants to the "small firm" category). Unfortunately, a number of observations are suppressed by the Census Bureau from such three-way tabulations for confidentiality reasons. While missing observations are likely to cover a relatively small number of jobs (otherwise they would not be omitted), they may still account for a substantial fraction of net job flows. With those caveats in mind, we now proceed, beginning with industries and continuing with geographical units.

One natural question is whether Fact 1 just reflects a more pronounced sensitivity of sectors with an above-average firm and establishment size (like manufacturing) to aggregate economic condition. We emphasize that the substance of our finding would not be diminished even if it were indeed due to a composition effect, although it may appear less surprising in light of what we already know about the average firm size and cyclical sensitivity of different sectors. But it turns out that Fact 1 is, by and large, a phenomenon that occurs within, rather than between, industries.

We partition the economy into eight broad sectors, which are reasonably consistent through the changeover from Standard Industrial Classification to the North American Industry Classification System in 1998.[10] With this classification, we

[10] We omit data for a ninth sector, namely Agricultural Services, Forestry, and Fishing, as the BDS leaves agricultural production workers out (among a few other categories of workers). Overall in our BDS sample, that

Table 3—Industry-Level Correlations between Average Unemployment over Past Year and Differential Firm Growth, BDS

TCPU	−0.579 (0.001)	Services	−0.214 (0.247)
FIRE	−0.559 (0.001)	Wholesale trade	−0.275 (0.135)
Construction	−0.527 (0.002)	Manufacturing	−0.197 (0.289)
Retail trade	−0.473 (0.007)	Mining	−0.045 (0.809)

Notes: *p*-values in parentheses. TCPU stands for Transportation, Communication, Public Utilities. FIRE stands for Finance, Insurance, Real Estate.

find that 93 out of 248 sector-year cells are affected to some degree by suppressed observations, always in the large firm groups, where we are likely to exaggerate JC by the total employment of new firms that show up immediately as large after one year since entry (small firms are never an issue in this data). The number of missing observation per year is not cyclical, however (its correlation with the unemployment rate is −0.005), and numbers of employees as small as few thousands are reported and not suppressed by the Census Bureau.

In Table 3 we report the correlations between the differential growth rate of (initially) large minus small firms in each broad sector and the civilian unemployment rate, as usual both detrended. The small and large firm cutoffs are set here to < 50 and $> 1{,}000$ employees for all industries. As an alternative way of looking at within-industry correlations, we also project the detrended industry-specific differential (large minus small firms) employment growth rate on the (national) civilian unemployment rate from the BLS. The estimated regression coefficient is −1.19, with a robust standard error of 0.28. Similar calculations using, as cutoffs, the first and third terciles of the average (over the entire period) firm size distribution within each sector—to allow differentiation of sectors that have exceedingly high average establishment size, such as manufacturing, from much of the rest of the economy—are available on request, and draw a very similar picture. The familiar pattern emerges also, more or less strongly, within all sectors, including the larger ones.

Finally, we perform the reverse exercise. We classify and rank sectors by mean firm size over the period. Then we calculate the employment growth of the largest three sectors and that of the smallest three, and take the difference. This is a between-industry measure of employment reallocation from sectors that have, on average, larger or smaller employers. The pattern is much less clear, as the growth differential has a correlation with the unemployment rate of −0.140 and is not statistically significant (*p*-value of 0.452).

Geographical Patterns.—We next look within states. We note that the BDS file presenting job flows and stocks by initial size, state, and age of the firm that we used to compile results in this section has 1,581 state-year cells, out of which 994 are affected by missing observations in *some* size class among the 4 classes (1,000 to 2,499 employees; 2,500 to 4,999; 5,000 to 9,9999; and greater than 10,000) aggregated into the "large" group, to a degree that is impossible to gauge beyond what

particular industry accounts for less than 0.7 percent of total employment. It is also responsible for over 20 percent of the missing observations in the full dataset.

we already reported earlier. Again, by definition missing observations have low employment count and really only matter for the rare firms that enter and become very large within a year.

As a compact summary of the state-level results, we regress the detrended state-level differential (large minus small firms) employment growth rate on the local (state-level) civilian unemployment rate from the BLS. The estimated regression coefficient is -0.15, with a standard error of 0.05. Thus, the phenomenon that we identify takes place within states, and is not (solely) driven by employment moving from locations with small firms to locations with large firms in a boom, and vice versa in recessions.[11]

We summarize the findings of this and the previous subsections in:

Fact 3: *Fact* 1 *holds principally within, not across, sectors and states.*

B. *Business Employment Dynamics* (*BED*)

A different but still nearly exhaustive source of information on the distribution of employment and on job flows by employer size in the US private sector is available from the BED program at the BLS. This program collects information accrued from the states' unemployment insurance programs. A firm is identified by a federal Employer Identification Number (EIN). This is a narrower definition than the Census Bureau's alpha, which is based on ownership and control. In particular, a large multiestablishment firm that operates in multiple states often has multiple EINs, so it appears as one firm in BDS and as multiple, often medium-sized firms, in BED. Indeed, the distribution of employment in BED is much less skewed than in BDS: over the overlapping time period 1992–2009 between the two datasets, the share of employment at small firms (< 50 employees) in BED and BDS is virtually the same, about 30 percent, while the share of employment at large firms ($> 1{,}000$ employees) in BED is 37 percent against 43 percent in BDS, with an offsetting difference in the medium size group. This limits somewhat the usefulness of BED for studying firm size.

As the name suggests, BED is primarily a dataset of job flows. Although size classification is dynamic, thus subject to the reclassification bias (see Section II), the small magnitude of that bias found in BDS, and documented later in this section, suggests that valuable information may be contained also in BED flows, as broken down by dynamically classified size. BED only begins in 1992:III, and runs through 2010:III at the time of writing, but BED has one advantage over BDS: a quarterly frequency. For our purposes, BDS is clearly superior in time span, definition of a firm, and classification method by (initial) size.

All BED series exhibit, even after seasonal adjustment, marked high-frequency variation, which dilutes the correlation with unemployment, although it does not mask the comovement visually. For this reason, we further smooth all (detrended) BED time series with a symmetric five-quarter moving average filter. A bandpass

[11] To save on space, we do not report correlations for each individual state. Those correlations (available in the online Appendix) are negative in the vast majority of cases, although they are only borderline significant in roughly 20 percent of states, where the point estimate is "most negative."

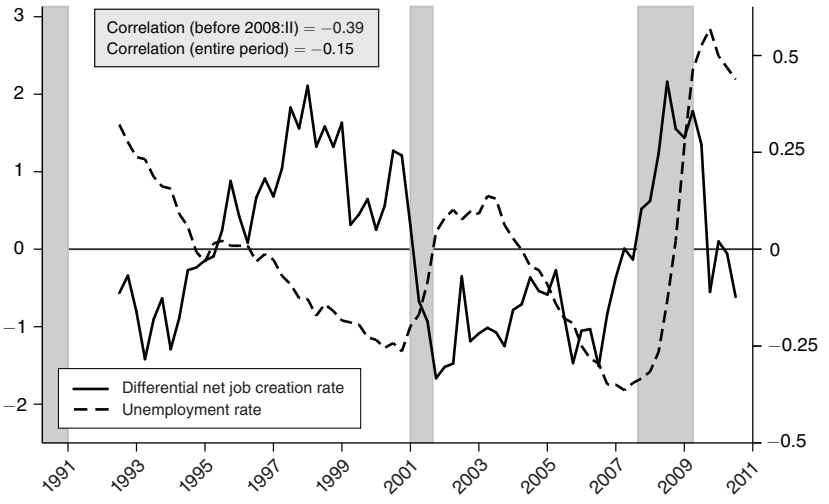


FIGURE 6. DIFFERENTIAL GROWTH AND UNEMPLOYMENT

Notes: Categories defined each year as < 50 and $> 1{,}000$. Shaded areas indicate NBER contractions. Differential net job creation rate detrended and MA-smoothed; unemployment rate detrended.

Source: BDS and authors' calculations.

filter would lose the last few observations, which pertain to the last recession and current recovery.

Figure 6 plots the differential quarterly growth rate between large and small firms according to the usual size cutoffs and the unemployment rate (deviations from trend averaged over the three months in the same quarter as growth rates), and shades NBER recessions. The two series mirror each other, except in the last recession, which shows the telltale mark of a credit crunch, hitting small firms harder. Before this recent episode (of obvious statistical importance in such a short sample), the correlation between the two series over the first 18 years of observation is -0.39, and in 2001 large firms clearly received the stronger blow, thus conveying the same message as the BDS data. If we include the last recession in the sample, however, the correlation between the unemployment rate and growth rate differential drops to a statistically insignificant -0.15.

To gain further insight into what happened during the last, unusually severe recession, in Figure 7 we illustrate the comovement between the unemployment rate and the difference in net growth rates between large and small *continuing* firms: that is, we leave out the job flows caused by firm entry and exit.[12] Then, the last recession does not stick out nearly as much: the correlation between the two series is consistently negative over the whole 1992–2010 period at -0.34. For the large firm group, entry and exit of firms account for negligible (less that 1 percent) and acyclical fractions of JC and JD. For the small firm group, the share of gross JC explained by entry is substantial (28 percent on average) and roughly acyclical.

[12] Closings in the firm-size BED data refer to firms' shutdowns, subject to the caveats on the BED definition of a firm. See www.bls.gov/news.release/cewfs.tn.htm for details. In BDS and similar Census Bureau datasets, closings always refer to establishments.

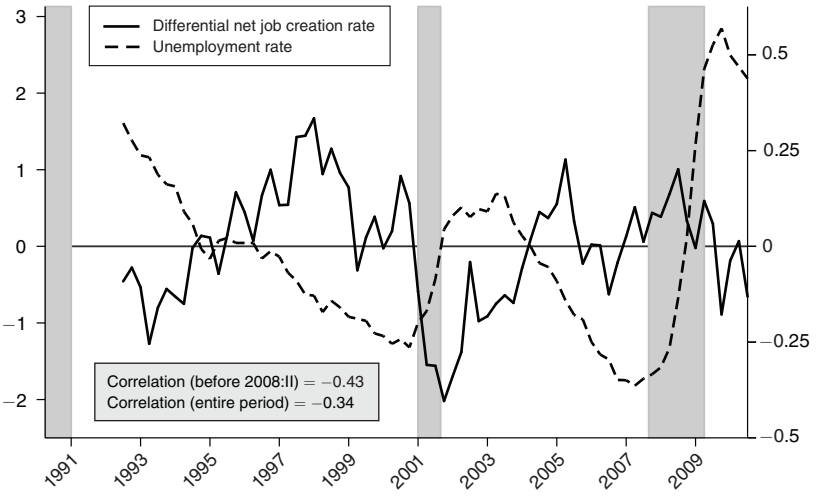


FIGURE 7. DIFFERENTIAL GROWTH AND UNEMPLOYMENT, CONTINUING FIRMS

Notes: Categories defined each year as < 50 and $> 1{,}000$. Shaded areas indicate NBER contractions. Differential net job creation rate detrended and MA-smoothed; unemployment rate detrended.

Source: BDS and authors' calculations.

The share of JD explained by exit is also important (27 percent on average), and this is where the last recession appears special. The share of gross JD by small firms due to exit is markedly *pro*cyclical until the end of 2005, meaning that total JD is more countercyclical than JD due to firm exit until that date. Then the same share suddenly becomes countercyclical after 2006, rising sharply in 2008–2009. In short, the last recession hit small firms harder, which is unusual, and it appears to have done so mostly by forcing them to exit in unusually high proportions. The BLS's dynamic sizing method implies that any closing firm of size 50 and above generates a loss of 49 jobs imputed to exit by firms in the small group. Thus, in the last, severe recession reclassification may be a significant issue in BED data: the exit of a large firm suddenly declining from $> 1{,}000$ to 0 employees within a quarter is likely more frequently observed, especially in such a sharp contraction, than the growth of an entrant from 0 to $> 1{,}000$, an event that we know from BDS data to be very rare. Based on these considerations and on the initial-size BDS evidence, we conclude that BED job flows dynamically allocated to firm sizes are problematic to understand their fluctuations, especially during severe contractions such as in 2008–2009.

C. *The Distribution of Employment by Employer Size at Business Cycle Frequencies*

As discussed in Section II, the growth rate of the employment share of a given size class approximates our main object of interest, the growth rate of employment in the set of firms *initially* in that size class, only up to the reclassification bias. We now report evidence on changes in the employment distribution from repeated cross sections, both to gauge (by comparison with our previous results) the magnitude of the reclassification bias, and because the data required to construct employment

shares are much more widely available across countries than longitudinal datasets, such as BDS, that make it possible to correct for reclassification.

The BDS also provides employment stocks by current firm size, thus allowing construction of employment shares of size classes each year. We can thus repeat the exercise of Section IIIA only using the growth rates of employment shares across size classes, whose correlation with the growth rates of employment by initial firm size is 0.84. The message is essentially unchanged. The correlation of the differential growth rate of employment shares of large and small firms with unemployment is −0.77, somewhat more negative than in Figure 1, as we would expect because of reclassification, which pushes more firms into (and hence imputes more growth to) the large firm size categories when unemployment is low, and vice versa.

Fact 4: *Fact* 1 *is not due to reclassification of employers into larger* (*smaller*) *size classes during an aggregate expansion* (*contraction*). *Fact* 1, *however*, *appears quantitatively stronger in datasets that lack longitudinal links*, *such as repeated cross sections.*

IV. International Evidence

Business micro- or semi-aggregated data (by employer size) on employment are available in many countries. We now present evidence of the following, final stylized fact:

Fact 5: *Fact* 1 *is not unique to the United States: it holds in several countries of different sizes.*

A. *The Size/Growth Relationship at Business Cycle Frequencies*

Proper longitudinal micro-data from Denmark and France that we have been able to access allow us to replicate and to extend the exercise that we performed for the United States with BDS, as well as to fix firm size once and for all at the beginning of the sample.[13] Results in this subsection are mainly based on the unweighted definition (2), although we will also mention the results based on equation (1).[14]

Denmark.—The Danish register-based matched employer-employee dataset *Integreret Database for Arbejdsmarkedsforskning* (Integrated Database for Labor Market Research (IDA)) contains basic socioeconomic information collected on

[13] See also Moscarini and Postel-Vinay (2009b) for additional evidence from Brazil, and Bachmann and David (2010) for evidence from West Germany. In both cases the evidence is based on administrative sources and strongly corroborates our Fact 5.

[14] In both the Danish and French data that we use, a firm is identified by a tax register number. Those identifiers are not entirely stable over time since it is possible for firms to change their tax register number (or obtain several such numbers at the same time), which they sometimes do for reasons that have nothing to do with their human resource decisions. In particular, some of the firm dynamics observed in those datasets may reflect mergers and acquisitions activity. An advantage of the unweighted average growth rate (2) is that it is less sensitive to the issue of spurious firm entry and exit generated by those changes of identification number. Another advantage is that it is less sensitive to outliers where there are relatively few large firms, typically in small economies, such as Denmark.

workers annually in the last week of November, and some background information on employers (including employer identifiers). It covers the entire Danish population aged 16 to 69.[15] As a part of the IDA program, Statistics Denmark maintains an employer-level panel that contains all the basic information on employers, including workforce size as of the last week of November. The current panel length is 28 years, from 1981 to and including 2008, and the sample that we use excludes public-sector and not-for-profit employers.

As the IDA firm file is a panel of employers, we can assign any particular employer to a fixed size class for as many years as we like. It is possible, in particular, to replicate the structure of the US BDS data, where firms are assigned to a fixed size class over rolling two-year windows. Figure 8 plots for 1981–2008 the (detrended) Danish unemployment rate and growth rate of large minus small firms, where size is fixed a year in advance, as in the BDS. The familiar pattern emerges: initially large employers grow faster when unemployment is unusually low, and vice versa. The correlation between the two series is −0.59, very similar to the United States.

In Figure 9 we allocate firms to the two size classes in 1981 and, without ever reclassifying them, track the differential employment growth rate of these two groups over the following 20 years. The pattern holds strikingly well all the way to 2008. Remarkably, a firm's initial size predicts its growth as far as three decades down the line.

Firm size often serves as an indirect measure of productivity, based on the well-documented positive relationships between employment size and revenue-based measures of either productivity or wages. For IDA, both relationships have been confirmed by Lentz and Mortensen (2008). Since IDA contains wage information, we also consider initial wage, rather than initial size, as a measure of underlying productivity. It is plausible that wages reflect productivity better than size, especially for young firms that are still in their initial growth phases. In Figure 10 we allocate firms once and for all to low- and high-paying bins according to mean wage earned by their employees in 1981. We then compute, detrend, and plot with the detrended unemployment rate the growth rate of employment at initially high- and low-paying firms, where the former are on average larger. Again, the two series are correlated negatively (with an overall correlation of −0.22, which becomes much stronger after an initial and suspiciously large blip in the differential growth series in 1981).

As indicated above, Figures 8 to 10 are based on the unweighted definition of average firm growth, equation (2). Correlations between the unemployment rate and weighted average differential growth based on equation (1), while still negative at −0.06, −0.16 and −0.10, respectively, are not statistically significant. The differential growth series based on equation (1) turns out to be very noisy for Denmark, where the number of firms in the "large" category is both small and variable (between 150 and 250).

[15] The Statistics Denmark website has information in English about the IDA set: www.dst.dk/HomeUK/Guide/documentation/Varedeklarationer/emnegruppe/emne.aspx?sysrid=1013. See also Bagger et al. (2009) for a more detailed description. We take this opportunity to thank Jesper Bagger for his very generous help in getting access to and using the IDA data.

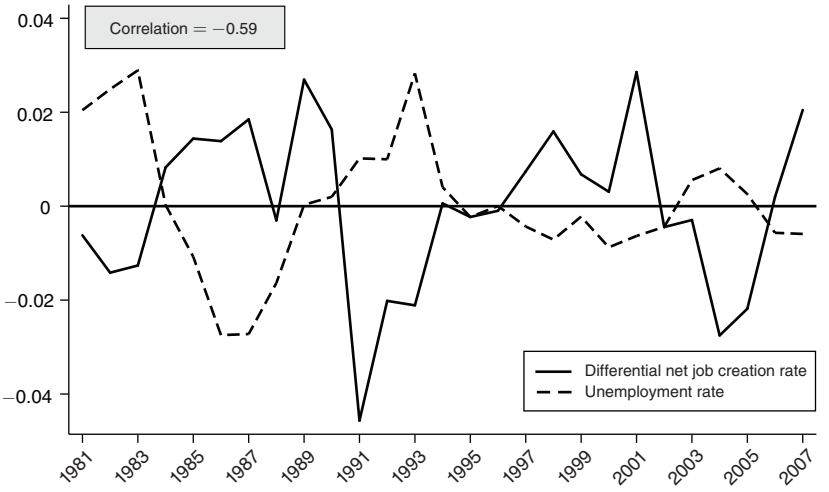


FIGURE 8. DIFFERENTIAL GROWTH AND UNEMPLOYMENT, DENMARK

Notes: Categories defined each year as < 20 and > 500. Both series detrended.

Source: IDA, Organisation for Economic Co-operation and Development (OECD), and authors' calculations.

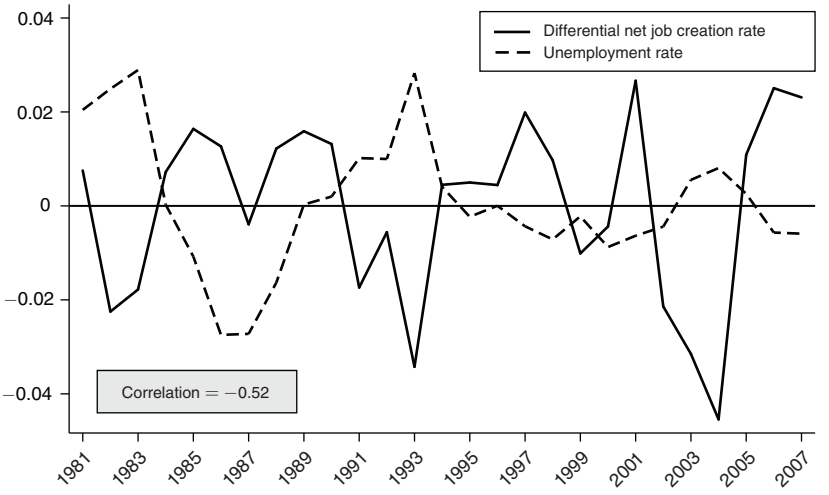


FIGURE 9. DIFFERENTIAL GROWTH AND UNEMPLOYMENT, DANISH PRIVATE COMPANIES CLASSIFIED BY SIZE IN 1981

Notes: Categories defined based on size in 1981 as < 20 and > 500. Both series detrended.

Source: IDA, OECD, and authors' calculations.

France.—The French Institute for National Statistics (INSEE) collects and maintains a panel of firm-level data, extracted from the *Bénéfices Réels Normaux* (BRN) register, which conveys standard annual accounting information on all private companies (*not* establishments) with an annual sales turnover in excess of (roughly) €550,000 and liable to corporate taxes. From the BRN dataset, we were able to access an exhaustive panel of private firms covering the years 1985–2005

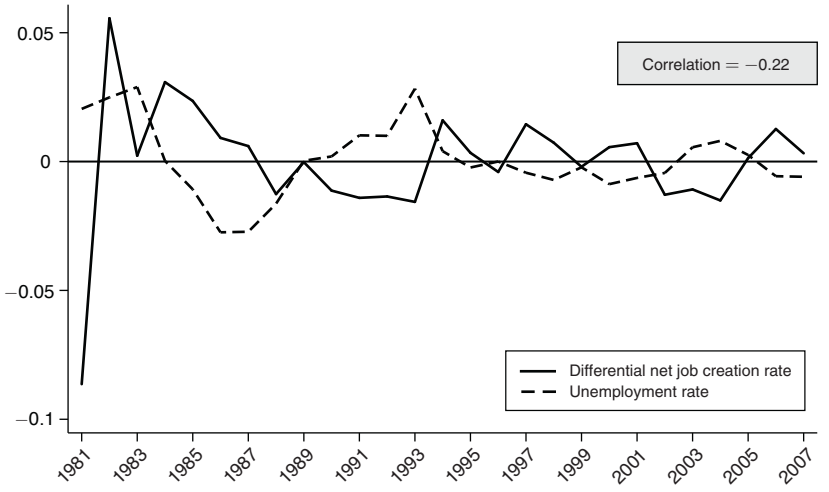


Figure 10. Differential Growth and Unemployment, Danish Private Companies Classified by Mean Wage in 1981

Notes: Categories based on mean wage in 1981 and defined as < DKK 58,000/year and > DKK 83,000/year. Both series detrended.

Source: IDA, OECD, and authors' calculations.

with information about end-of-year employment size (as of December 31st each year) and value added.[16]

By its nature and design, the French BRN panel is very similar to the Danish IDA firm file. We thus use it in exactly the same way: Figures 11, 12, and 13 are the French counterparts of the Danish Figures 8, 9, and 10, the only difference being that we classify French firms using value added per worker (used here as an alternate measure of productivity) in Figure 13, as opposed to mean wages in the Danish case (Figure 10).[17] As usual, in all figures we also show a plot of the HP-filtered unemployment rate (taken from INSEE).

The familiar pattern of differential growth emerges again from all three figures: initially large employers (in size or in value added per worker) grow faster when unemployment is unusually low, and vice versa. Again Figures 11 to 13 are based on the unweighted definition of average growth. The correlation coefficients obtained by using the weighted series are −0.50, −0.55, and −0.36, respectively, are all statistically significant. The volatility issue that we flagged in the Danish data appears much less of a problem in the French data, where the number of "large" firms is relatively stable around 1,300. While we currently have no way of checking, we conjecture that this volatility problem should be relatively mild in the even bigger US economy, where the count of large firms with more than 1,000 employees averages over 9,000.

[16] The INSEE website has information in French about a public-access (nonpanel) version of BRN: www.insee.fr/fr/themes/detail.asp?ref_id=ir-images08&page=irweb/images08/dd/doc/infosuse.htm#L1. Access to the full BRN data is restricted to authorized researchers. We are grateful to Linas Tarasonis of University Paris I and CREST-INSEE for performing the data extraction and analysis for us.

[17] Specifically, in Figure 13 we split firms once and for all into high- and low-productivity classes according to their mean value added per worker over the initial period 1985–88. This time-averaging is warranted by the notorious volatility of firm-level value added per worker.

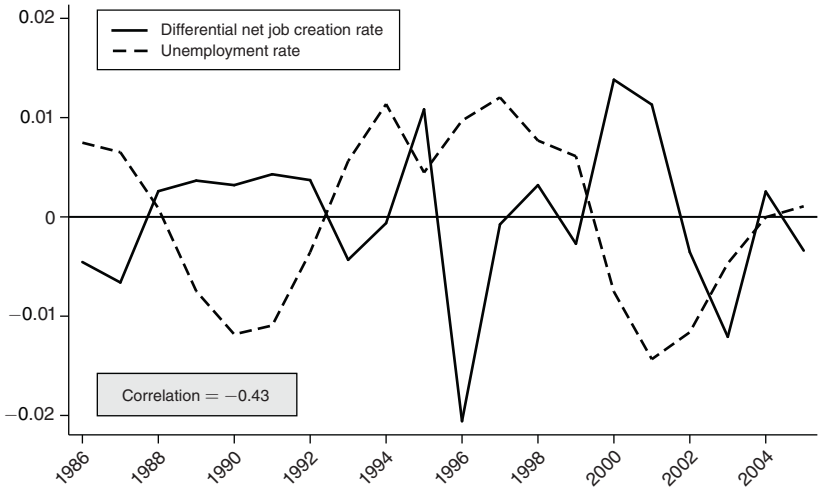


FIGURE 11. DIFFERENTIAL GROWTH AND UNEMPLOYMENT, FRANCE

Notes: Categories defined each year as < 20 and > 500. Both series detrended.

Source: BRN, OECD, and authors' calculations.

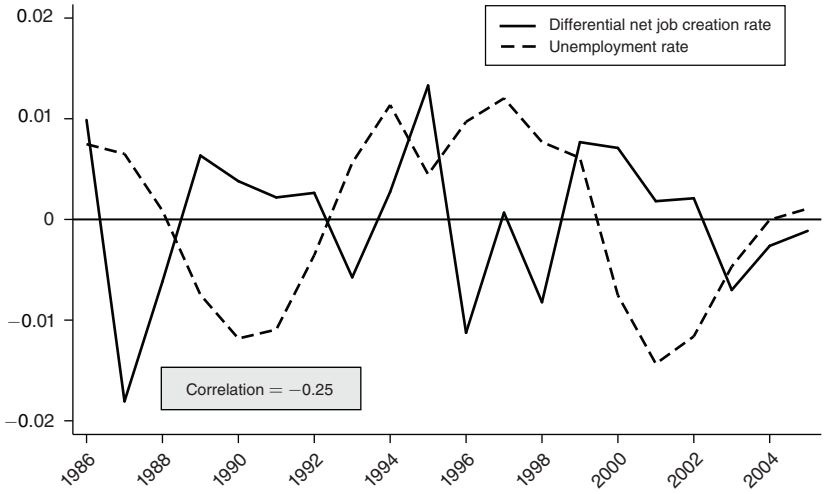


FIGURE 12. DIFFERENTIAL GROWTH AND UNEMPLOYMENT, FRENCH PRIVATE COMPANIES CLASSIFIED BY SIZE IN 1986

Notes: Categories defined in 1986 as < 20 and > 500. Both series detrended.

Source: BRN, OECD, and authors' calculations.

B. *Distribution of Employment by Employer Size at Business Cycle Frequencies*

The type of data easiest to access is the distribution of employment among employers of different sizes. Although this is only indirect evidence, as shown in previous sections the cyclical behavior of employment shares by size classes in the United States is informative of the underlying pattern of growth by initial size, because reclassification bias appears to be quantitatively modest at annual or higher frequencies. We present evidence from two other countries that the

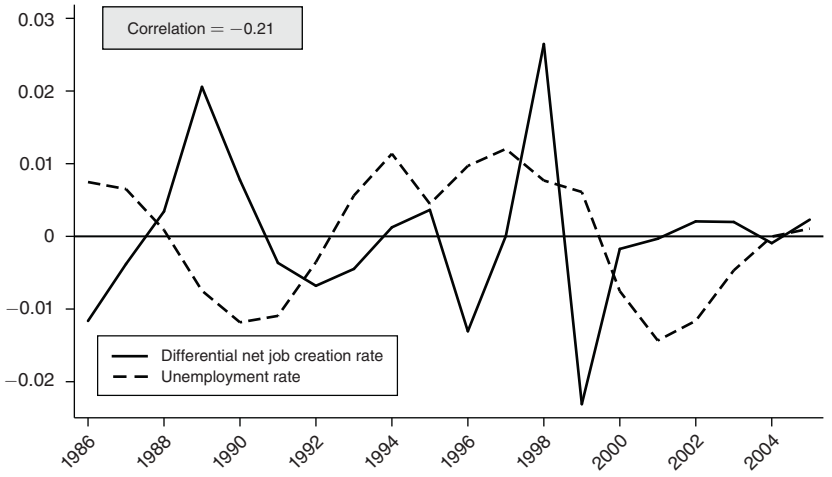


FIGURE 13. DIFFERENTIAL GROWTH AND UNEMPLOYMENT, FRENCH PRIVATE COMPANIES CLASSIFIED BY MEAN VALUE ADDED PER WORKER IN 1985–1988

Notes: Categories based on mean VA/worker over 1985–1988 and defined as < FRF 150,000/year and > FRF 220,000/year. Both series detrended.

Sources: BRN, OECD, and authors' calculations.

differential growth rate of the employment shares of large and small employers indeed comoves with unemployment. This exercise can probably be replicated in many other countries using publicly available data.

United Kingdom.—The UK Department for Business, Innovation and Skills publishes a table of employment shares by classes of firm size at annual frequency.[18] We found data for 1994–2006. The size cutoffs are < 20 and > 249 employees. We compute and detrend employment share growth rates and plot the cross-size-class difference thereof against the detrended UK unemployment rate (from the UK Office of National Statistics). The correlation between those two series over the 1994–2006 observation period is -0.24, smaller in absolute value than what we found for other countries but still negative, as visually clear in Figure 14.

Canada.—As a part of its report on "Business Dynamics in Canada," Statistics Canada has compiled annual employment shares by firm size categories over the two decades 1983–2003.[19] The largest size category available is 500. From these data, we can compute employment growth rates, with reclassification and including entrants. In Figure 15 we plot the differential growth rate (> 500 employees minus < 20 employees) against detrended unemployment (from the OECD), and we find a familiar negative correlation of -0.42.

[18] See http://stats.berr.gov.uk/ed/sme/.
[19] See Table 5a at www.statcan.gc.ca/bsolc/olc-cel/olc-cel?catno=61-534-XWE&lang=eng.

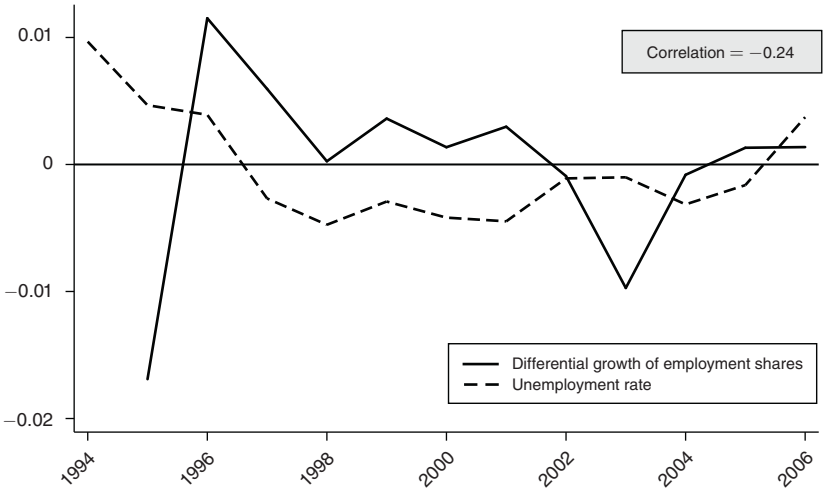


FIGURE 14. DIFFERENTIAL GROWTH OF EMPLOYMENT SHARES AND UNEMPLOYMENT, UNITED KINGDOM

Notes: Categories defined each year as < 20 and > 249, without correction for reclassification. Both series detrended.

Sources: UK Department for Business, Innovation and Skills, UK Office for National Statistics, and authors' calculations.

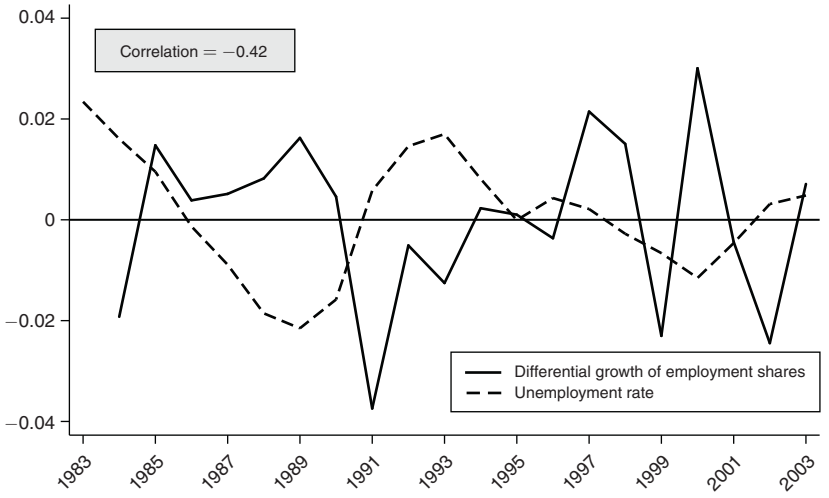


FIGURE 15. DIFFERENTIAL GROWTH OF EMPLOYMENT SHARES AND UNEMPLOYMENT, CANADA

Notes: Categories defined each year as < 20 and > 500 without correction for reclassification. Both series detrended.

Sources: Statistics Canada (Business Dynamics in Canada), OECD, and authors' calculations.

V. Concluding Remarks

We present substantial and diverse empirical evidence that the differential growth rate of employment at initially large and small firms is strongly negatively correlated with the aggregate unemployment rate. Large firms grow faster, relative to small firms,

when unemployment is low, and vice versa. This pattern is robust to a variety of measures of differential employment growth, employer size and classification by size, treatments of entry and exit of firms and establishments, industry, geographical and firm age breakdowns. Very similar patterns are observed in other, diverse countries. Beyond unconditional correlations, the relative performance of small and large employers is informative about the nature of the last five recessions. Maybe surprisingly, in the last Great Recession the net job creation of large employers slowed down much faster.

From a conceptual viewpoint, the data require a theoretical framework to make sense of the patterns that we uncover. Indeed, our measurement is based entirely on the theoretical work in Moscarini and Postel-Vinay (2009a, 2010a). There, a "firm" is identified with a wage policy, and our facts are explained by competition for workers among heterogeneous firms in frictional labor markets. Alternative definitions of a firm, based on technology (scale of operation, capital adjustment costs), span of control, borrowing constraints, and others, can be similarly embedded in an equilibrium framework to produce predictions that can be confronted with our facts.

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