Can Paying Firms More Quickly Affect Aggregate Employment?*

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January 2018

Abstract

We study the impact of *Quickpay*, a federal reform that indefinitely accelerated payments to small business contractors of the U.S. government. Despite treated firms being paid just 15 days sooner, we find a strong direct effect of the reform on county-sector employment growth. Importantly, however, we also document substantial crowding out of non-treated firms' employment within local labor markets. While the overall net employment effect was positive, it was close to zero in tight labor markets – where direct effects were weaker and crowding out stronger. Our results highlight an important channel for alleviating financing constraints in small firms, but also emphasize the general-equilibrium effects of large-scale interventions, which can lead to lower aggregate outcomes depending on labor market conditions.

*Barrot: jnbarrot@mit.edu; Nanda: RNanda@hbs.edu. We are grateful to Manuel Adelino, Oriana Bandiera, Nittai Bergman, Emily Breza, Hui Chen, Erik Hurst, Mauricio Larrain, Erik Loualiche, Karen Mills, Ben Pugsley, David Robinson, Antoinette Schoar, Scott Stern, John Van Reenen, David Thesmar, Chris Woodruff, Liu Yang, Eric Zwick and participants at the NBER Entrepreneurship and Economic Growth Conference, Toulouse School of Economics , MIT Finance lunch, HEC, INSEAD, Toulouse School of Economics, LSE, Maryland Junior Finance Conference, Georgia Tech, NBER Corporate Finance, American Finance Associations meetings, Sciences Po and the NBER Summer Institute Entrepreneurship meeting for helpful feedback. We are also grateful to the US Department of Defense for sharing data on the timing of payments from their MOCAS accounting system. Barrot recognizes the support of the Kauffman Foundation Junior Faculty Fellowship and MIT Sloan. Nanda thanks the Division of Research and Faculty Development at HBS for financial support. All errors are our own.

1 Introduction

The role of financing frictions in impacting employment has received substantial interest following the 2008 financial crisis and subsequent slow recovery, where contraction in bank lending has been argued to have had a substantial impact on the real economy and particularly so on smaller firms. Policy makers interested in stimulating aggregate employment have thus largely focused on small firms' access to bank credit as a central means for alleviating potential financing constraints (Bernanke 2010; Yellen 2013; Mills 2014).

Although a focus on bank lending is important, another, more direct way that government can impact small firms' financing is through its role as their customer: federal government procurement amounts to 4% of GDP in the U.S. and includes $100 billion in goods and services purchased directly from small firms. Typical contracts require payment one-to-two months following the approval of an invoice, implying that these small businesses are effectively *lending* to the government while simultaneously having to borrow from banks to finance their payroll and working capital. Can paying small business contractors faster have a meaningful effect on their cash flows, facilitate hiring, and ultimately stimulate aggregate employment?

Theoretically, complementarities between capital and labor imply that employment is likely to be depressed when firm-level investment is held back by financing constraints. In addition, if there is a mismatch between the timing of cash flow generation and payments to labor, firms need to finance their payroll through the production process (Jermann and Quadrini 2012; Benmelech et al. 2014). A positive cash flow shock from accelerated payments could therefore have direct effects on employment for firms looking to grow, independent of the indirect effects through firm-level investment. Consistent with these arguments, recent studies have documented that firm-level employment seems to respond to the intensity of financing frictions faced by firms (Benmelech et al. 2014; Chodorow-

Reich 2014; Greenstone et al. 2014).

Nevertheless, there are reasons to also believe that faster payment might not affect aggregate employment by much, or at all: an acceleration of payment should have negligible effects in the absence of financing frictions. Further, as pointed out by Acemoglu (2010), employment growth measured at the firm level may be offset in general equilibrium due to "business stealing" effects. That is, even if affected firms grow their workforce in response to the payment acceleration, this might have negative spillovers on the employment decisions of other firms hiring from common local labor markets. The overall effect of payment acceleration on aggregate employment, if any, therefore depends both on the intensity of financing frictions and the direction and magnitude of spillovers.

Identifying direct effects and isolating them from spillovers, however, requires an exogenous source of variation in firm-level financing conditions – for an identifiable subset of firms – together with exhaustive information on employment at the local labor market level. We address these empirical challenges by exploiting the federal *Quickpay* reform, together with public-use census data on payroll and employment at the county-sector level. The *Quickpay* reform, announced in September 2011 by President Obama, indefinitely accelerated payments to a subset of small business contractors of the U.S. federal government, cutting the time taken between invoice receipt and payment by half, from 30 to 15 days. In other words, the reform permanently reduced the working capital needed to sustain a dollar of sales with the government for small businesses. $70 billion in annual contract value was accelerated and impacted a set of small businesses across virtually every industry sector and U.S. county due to the massive footprint of federal government procurement.

We analyze the effect of this policy using cross-sectional variation in the intensity of treatment across county-sector cells. More precisely, we use data on the precise location and accelerated contract value for all treated firms to compare the employment outcomes of county-sectors with high and low exposure to government contracts that were subject to the payment acceleration. We are able to control for overall exposure to government contracts, for small business presence, and for a variety of additional variables at the county-sector level that are important correlates. Since we can precisely measure both payroll and the contract value that was accelerated, our setting has the additional benefit that we can measure the elasticity of payroll to cash flow and hence implicitly 'back out' the degree of financing frictions faced by the treated firms. Secondly, because we have a well-defined subset of firms that were treated, our setting allows us to separate the direct effects of the reform from potential spillovers on untreated firms in the same commuting zone who were hiring from common labor markets. Finally, because the treatment was exogenous to the state of local labor markets, we can study heterogeneous effects of the treatment. For example, we can examine how the direct and spillover effects varied across counties where unemployment rates were particularly high at the time the reform was implemented, or where bank credit was particularly scarce.

We find a substantial impact of accelerated payment on small business employment, despite the acceleration being a 'mere 15 days'. On average, each accelerated dollar of sales led to an almost 7 cent increase in annual payroll after four years, with most of the increase coming from new hires. In other words, by permanently lowering the working capital needed to sustain a dollar of sales to the government, the policy increased firms' internal cash flow and helped them to grow. To put these magnitudes in perspective, we write a simple general equilibrium model and back-out the cost of working capital finance implied by our estimates. The implied cost of external finance for treated firms is approximately 40%, which is comparable to the cost of trade credit and of other sources of financing available to small businesses in the wake of the financial crisis.

We show that there are no prior trends, and that the effects are not driven by county-sector exposure to government contracts or to small businesses, but by exposure to accelerated contracts only. Consistent with the acceleration of the 'cash conversion cycle' driving payroll growth, we find the effect to be strongest in sectors where receivables account for a larger share of assets and in counties where financing frictions are more severe for small firms.[1] Importantly, however, we document substantial crowding out of employment growth among non-treated firms. The increased demand for labor from treated firms exerts pressure on wages, which makes it relatively harder for non-treated firms to hire, leading them to grow employment slower. Again, we show that there are no prior trends, and that the effects are not driven by county-sector exposure to government contracts or to small businesses, but by exposure to accelerated contracts only. Additionally, we provide direct evidence of actual job flows from low to high treatment sectors within a given state.

Finally, we document that positive direct employment effects are weaker and that negative spillovers are stronger in tight local labor markets, namely, in areas with many vacancies relative to unemployed workers. As a result, the overall effect of the reform is positive only in slack labor markets. Hence, while the federal payment acceleration reform did stimulate employment in areas with high unemployment at the time of its introduction, it had little or no effect elsewhere. This illustrates how the employment response to financing conditions depends on local labor market conditions, and how the general equilibrium effect of financing constraints on employment can be substantially lower than partial equilibrium estimates would suggest.

Our results are related to several strands of the literature. We first contribute to a growing stream of research focusing on the relationship between financial frictions and employment.[2] Benmelech et al. (2014) show that firm-level employment responds to bank deregulation and bank balance sheet shocks. Chodorow-Reich (2014) finds an employ-

[1] The cash conversion cycle refers to the number of days of working capital need to be financed. For example, if the firm has to pay cash on delivery of inputs, which sit in inventory for an average of 15 days, and on average, the firm is paid by its customers 30 days after the sale, then the cash conversion cycle is 45 days.

[2] The effect of financing frictions on capital investment has been studied extensively, starting with Fazzari et al. (1988), who find a strong positive relationship between cash flows and investment. Subsequent studies have complemented these findings using exogenous variations in cash flows including Blanchard et al. (1994), Lamont (1997), Rauh (2006), Faulkender and Petersen (2012), variations in collateral such as Chaney et al. (2012), or structural models including Whited (1992).

ment reduction at firms with relationships to banks exposed to the Lehman Brothers bankruptcy. Relative to these studies that examine the sample of publicly listed firms, our contribution is to analyze the response of small business employment, which we find has a much stronger response to relaxed financing constraints than the previously measured responses of large, publicly traded firms.[3] More importantly, in addition to studying the direct effects of the treatment, our context also provides a unique opportunity to study crowding out effects on non-treated firms, due to the targeted nature of the treatment, and to speak to the aggregate effects of financing conditions at the level of local labor markets.[4]

Secondly, our work sheds light on the constraints to working capital finance, a question that has only recently begun to be examined in detail. By being paid weeks after the sale of a good or a service, firms effectively provide short-term corporate financing to their customers. Such inter-firm financing is referred to as trade credit and, in aggregate, is three times as large as bank loans and fifteen times as large as commercial paper in the US.[5] Trade credit claims, recorded as accounts receivable on firms' balance sheets, are typically seen as short-term, liquid, low-risk claims that should be very easy to pledge, and that should not constrain firm growth. Yet recent research as found that long payment terms forces financially constrained firms to cut back investment (Murfin and Njoroge 2014) and exposes them to liquidity risk (Barrot 2015).[6] Our work shows that trade credit provision also constrains employment growth, even when the debtor is a low-risk customer such as the federal government.[7]

[3]Other studies have analyzed the interaction of firms employment and financing decisions including Matsa (2010), Benmelech et al. (2012), and Agrawal and Matsa (2013).

[4]In discussing the aggregate relationship between labor and credit market frictions, we relate to work by Wasmer and Weil (2004) and Petrosky-Nadeau and Wasmer (2013).

[5]As of September 2012, according to the US Flow of Funds Accounts.

[6]Other contributions to the literature on trade credit include Petersen and Rajan (1997), Biais and Gollier (1997), Wilner (2000), Demirguc-Kunt and Maksimovic (2001), Burkart and Ellingsen (2004), Frank and Maksimovic (2005), Cunat (2007), Giannetti et al. (2011), Antras and Foley (2011), Dass et al. (2011), Kim and Shin (2012), Klapper et al. (2012), Garcia-Appendini and Montoriol-Garriga (forthcoming), Murfin and Njoroge (2014) and Breza and Liberman (2016).

[7]In practice, there are also impediments in the pledgeability of government trade credit claims. We

Finally, our findings build on the literature assessing the role of policy intervention targeting businesses in the US, most of which has focused on fiscal policies including bonus depreciation (House and Shapiro 2008; Zwick and Mahon 2016) or tax refunds (Dobridge 2016). We evaluate the effect of the federal payment acceleration reform which was motivated by the need to stimulate job growth in the wake of the Great Recession. Related to this, our work is among the first to examine the role of government as a customer and its implications for the private sector.[8] We show that targeting the working capital of small businesses can be a potentially effective way for policy makers to alleviate financing constraints but this needs to be balanced against the potential crowding out of firms that are not direct contractors to the government.

The rest of the paper is structured as follows. In Section 2, we provide an overview of the *Quickpay* reform and sketch a simple theoretical model that demonstrates how accelerated payments impact labor market outcomes. In Section 3, we describe our identification strategy and provide an overview of the data we use to study the effect of *Quickpay*. Section 4 outlines our results, and relates the results from our regressions to the theoretical model to provide a perspective on the magnitudes. Finally, Section 5 concludes.

2 Financing Labor Inputs

2.1 Theoretical Considerations

In the presence of adverse selection (e.g., Stiglitz and Weiss (1981)) or moral hazard (e.g., Holmstrom and Tirole (1997)), firms may be unable to raise outside finance and may consequently need to forgo positive net present value projects. The traditional view of labor inputs is that they are 'self-financing', so that such financing constraints are thought

provide more details below.

[8]Other studies include Liebman and Mahoney (2013), Cohen and Malloy (2014), Ferraz and Finan (2015), and Goldman (2015).

to impact a firm's hiring decisions only indirectly, through the effect they have on capital investment decisions. In this case, a relaxation in financing constraints will lead to more hiring when labor and capital are complements, but might lead to a fall in employment when capital and labor are substitutes. Employment decisions might also be affected by frictions in the capital markets if labor is not a variable factor of production but rather has a fixed, or quasi-fixed cost component (Hamermesh 1989; Hamermesh and Pfann 1996; Wasmer and Weil 2004; Petrosky-Nadeau and Wasmer 2013). These adjustment costs could emerge because of hiring and training costs, for instance.

Aside from adjustment costs, financing frictions can have a consequential effect on employment when firms have to finance working capital (Jermann and Quadrini 2012). This is particularly true among small or young firms that are in growth mode, as the mismatch between the timing of cash flow generation and payments to labor requires firms to finance their payroll through the production process - in advance of getting paid - and means that firms may have to cut back on hiring *even in the presence of customer demand and adequate labor supply*, due to an inability to pay workers in advance of receiving cash for their product or service. Survey evidence indeed suggests that over 90% of small businesses pay their employees twice a month or more frequently, with nearly half paying their employees weekly (Dennis 2006).

In the absence of financing frictions, a firm will be able to borrow fully against future cash flows, leading any change in the working capital cycle to have small or no effects on a firm's hiring decisions.[9] In the presence of financing frictions, however, even small improvements in cash collection can have large direct effects on hiring due to the multiplier effect of working capital. To see why, consider the stark example of a firm with $1

[9] An alternative for them is to turn to factoring companies, who buy accounts receivable in exchange for cash upfront. In practice, however, the negative stigma associated with factors leads small firms to go to them only as a last resort: customers have been known to pull back on demand upon learning that receivables were factored as this could suggest firms are on their last legs and hence can lead to issues with supply going forward. In addition, non-recourse factoring (where the factor takes on the full counter-party risk) has become far less prevalent for small firms, so that even if small firms did use factors, this would not free up a large amount of cash for them to put towards firm growth.

million of sales being paid 30 days after delivering its product. For simplicity, assume that the firm can only grow through internal cash flow and is currently constrained from growing because it is at cash flow breakeven.[10] In order to operate, this firm has to have approximately $80,000 of cash (30/365*$1 million) 'tied up' in receivables at any moment in time. A shift in the payment regime from 30 days to 15 days would only require the firm to have $40,000 of cash tied up in receivables and would therefore *permanently* unlock $40,000 of cash for the firm on an ongoing basis. In this extreme example where the firm is only able to support growth through internal cash flow, this will allow the firm to double in size, to $2 million, showing how seemingly small improvements in the working capital position for constrained firms can have consequential effects for growth in sales and in payroll.[11]

We formalize this intuition with a one-period general equilibrium model. Firms use labor and capital to produce and sell continuously throughout the period but only receive payments after selling their output, so that they have to finance their inputs in advance. The economy consists in two sets of firms t and u, with respective mass μ and $1-\mu$, that only differ in the amount of working capital they need to finance upfront.[12] Both sets of firms $i \in (t, u)$ have the same decreasing returns to scale technology in labor:

$$Y = A\left(L^{\sigma} K^{1-\sigma}\right)^{\alpha} \tag{1}$$

[10] That is, if it tried to grow, it would require additional cash to support the growth in sales which it cannot do due to only being able to grow from internal cash flow.

[11] It is important to note here that this is only true if there is a change in the payment regime, which permanently shifts payment from 30 days to 15 days. If there was a one-time change to 15 days that then reverted back to 30 day payment, the firm would need to fall back to its original $1 million of sales in order to avoid bankruptcy.

[12] Having two sets of competitive firms in the model allows us to separately consider the effect of payment acceleration on treated firms and other firms.

where $\alpha < 1$ captures the decreasing returns to scale. Firms maximize profit:

$$\max_{L,K} \Pi(L, K) = pY - wL - rK - R\gamma_i(wL + rK) \tag{2}$$

where total factor productivity, L and K are the quantity of labor and capital, w is the competitive wage, r the user cost of capital and R is the cost of financing. We take output as the numeraire such that $p = 1$. γ_i is the fraction of annual input cost that firms of type i have to finance in advance, measured as a fraction of the number of days in the year. Using the first order conditions for the maximization of profit with respect to labor and capital we obtain labor demand as:

$$L_i^* = w^{\frac{1-(1-\sigma)\alpha}{\alpha-1}} \left(\frac{1 + R\gamma_i}{\sigma A\alpha}\right)^{\frac{1}{\alpha-1}} \left(\frac{\sigma}{1-\sigma}\frac{r}{w}\right)^{\frac{(1-\sigma)\alpha}{\alpha-1}} \tag{3}$$

Using this expression, we can express the elasticity of labor demand to a change in payment terms measured with γ_i:

$$\frac{\partial L_i^*}{\partial \gamma_i}\frac{\gamma_i}{L_i^*} = -\frac{R}{(1 + R\gamma_i)(1-\alpha)} \tag{4}$$

As expected, labor demand decreases with γ_i, i.e., with the amount of working capital that needs to be financed ahead of sales. Moreover, the higher the cost of external financing, R, the stronger is the response of labor demand. Finally, the elasticity increases with higher returns to scale.

We next consider the households' problem and assume they maximize the following utility function:

$$U(C, L) = C - \zeta\frac{L^{1-\frac{1}{\theta}}}{1-\frac{1}{\theta}} \tag{5}$$

where C is the numeraire, L is labor supply, subject to the budget constraint:

$$C \leq wL + \Pi(L) \tag{6}$$

The first order conditions of this problem allow us to express labor supply as:

$$L_s^* = \left(\frac{w}{\zeta}\right)^{-\theta} \tag{7}$$

where θ is the labor supply elasticity. We finally obtain the equilibrium wage w^* from the market clearing condition, by equating demand and supply on the labor market. Our empirical analysis considers the response of employment to a change in the number of days receivables. Within the model, we can compare the change in the optimal quantity of labor when going from $\gamma_{i,1}$ prior to *Quickpay* to $\gamma_{i,2}$ afterwards. We express employment growth for treated firms ($i = t$) across the two steady states, prior and after *Quickpay* as

$$\frac{L_{t,2}^*}{L_{t,1}^*} = \left(\frac{1 + R\gamma_{t,2}}{1 + R\gamma_{t,1}}\right)^{\frac{1}{\alpha-1}} \left(\frac{w_2^*}{w_1^*}\right)^{\frac{1-(1-\sigma)\alpha}{\alpha-1}} \tag{8}$$

The first term on the right hand side of equation 8 captures the effect of higher labor demand triggered by the reform. The second term captures the negative effect on demand through wage increases.

For untreated firms, $\gamma_{u,2} = \gamma_{u,1}$ and employment growth reduces to

$$\frac{L_{u,2}^*}{L_{u,1}^*} = \left(\frac{w_2^*}{w_1^*}\right)^{\frac{1-(1-\sigma)\alpha}{\alpha-1}} \tag{9}$$

which is decreasing in wage growth. Untreated firms are thus negatively affected by the increase in wage triggered by the higher demand of treated firms.

The nice feature of equation 8 is that we can calibrate all parameters but $\frac{L_{i,2}^*}{L_{i,1}^*}$ and R, the cost of financing. The empirical exercise we present below delivers $\frac{L_{i,2}^*}{L_{i,1}^*}$, thus allowing us to pin down R. Our findings therefore shed light on the intensity of financing constraints facing firms in the US at the time of the *Quickpay* reform that we describe next. Moreover, the model allows us to compute the aggregate effect of *Quickpay* and compare it with our estimates, as we discuss below.

2.2 The Quickpay Reform

Although the economy began recovering from the trough of the Great Recession in June 2009, employment growth was sluggish, in what is now commonly referred to as the 'jobless recovery'. Bank lending following the financial crisis also continued to lag, particularly for small businesses. Alternative channels of finance were expensive, with interest rates typically upwards of 25% even when these firms could access credit (Mount 2012).

Faster payment to small business contractors of the federal government was initially promoted by the President's Council on Jobs and Competitiveness and supported by the Small Business Administration (SBA). The main motivation for undertaking this payment acceleration reform was to stimulate job creation as clearly evidenced in the White House press release announcing the reform.[13] The underlying idea was that "small businesses are the primary engine of job creation and job growth". Given that the Federal Government purchases approximately \$100 billion each year for goods and services from small businesses, accelerating payments was intended to allow them to "reinvest that money in the economy and drive job growth".

In 2011, US federal agencies started accelerating payments to their small business contractors, a reform named *Quickpay*. Prior to the reform, payments were typically made within 30 days from when an agency received an invoice, in accordance with the Prompt Payment Act.[14] If an agency did not pay a vendor the amount due by the required payment date, it was required to pay the vendor a late-payment interest penalty. Under the new policy, agencies were ordered to make payments as quickly as possible and within 15 days of receiving proper documentation, including an invoice for the amount due and

[13]Getting Money to Small Businesses Faster, White House Press Release, 2011

[14]Chapter 39 of title 31 of the United States Code

confirmation that the goods or services have been received and accepted.[15] The reform was formally announced on September 14, 2011 with the goal of achieving payments acceleration in all federal agencies by November 1, 2011.[16] However, some agencies anticipated the reform by a few months. In particular, the Department of Defense, the largest contributor to federal procurement by far, started accelerating payments as of April 27, 2011.[17] Accelerated dollars over the subsequent four years (our window of analysis) amounted to $70 billion per year.

For the purpose of this policy, small businesses are defined according to SBA's thresholds. These thresholds vary significantly across industries: the upper limit varies from 0.75 million to 38.5 million in annual receipts, or from 100 to 1500 employees.[18] The contracting officer in any given federal agency is in charge of checking whether the contractor is a small business firm and whether it is therefore eligible to accelerated payments. Appendix Figure A.1, Panel A, shows that the share of total government spending awarded to small businesses is close to 20% and stable throughout the sample period.

While all contracts awarded to small businesses were paid within 15 days after the reform, some contracts were already typically paid sooner than 15 days, and remained unaffected by the policy change. First, contracts pertaining to the delivery of meat food products, fresh or frozen fish, perishable commodities and dairy products were typically paid sooner than 10 days even prior to the reform.[19] Second, government contracts fall under two broad categories: fixed-price and cost-plus. Under fixed-price contracts, contractors agree to deliver the product or service at a pre-negotiated price. Under cost-plus contracts, contractors are paid for their expenses up to a set limit, plus profit.[20] Appendix Figure A.1, Panel B, shows that the share of total government spending awarded through

[15] See Memorandum M11-32 of the Office of Management and Budget, 2011
[16] See Memorandum M11-32 of the Office of Management and Budget, 2011
[17] See Memorandum 2011-O0007 of the Office of the Under Secretary of Defense, 2011.
[18] For more details on these thresholds, see https://www.sba.gov/content/small-business-size-standards
[19] See Subpart 32.9 (Prompt Payment) of the Federal Acquisition Regulation.
[20] For further analyses of these two contract types, see Horton (2008), for instance.

fixed-price contracts is close to 60% and stable throughout the sample period. The Department of Defense, which accounts for approximately two thirds of federal procurement, was already paying its cost-plus contracts within 15 days.[21] Finally, the Department of Defense also paid disadvantaged small business contractors earlier prior to the implementation of *QuickPay*.[22] In the rest of the paper, we use the term "non-eligible" contracts to refer to contracts that were already paid within 15 days prior to the reform. As we discuss later, this heterogeneity across contract types' exposure to the reform allows us to tightly identify the effect of payment acceleration on labor market outcomes. Appendix Table A.2 shows the distribution of eligible and non-eligible contracts across industries and firm-type. We discuss the specifics of our identification strategy in the following section.

One might argue that government contractors should easily find external financing for their receivables in the form of working capital loans or factoring, so that the reform should have little or no effect. This is turns out not to be true for at least two reasons. First, under the Federal Government Assignment of Claims Act (FACA), the credit provider must give timely written notice of the assignment to both the agency's contracting officer and its disbursing officer, and obtain written confirmation both in order to obtain a security interest on a government receivable. Loan agreements typically exclude government receivables from the computation of the borrowing base, unless these receivables have been properly assigned.[23] Moreover, while it is probably true that the government is a safer customer than many firms in the economy, typical government contracts include provisions allowing the purchasing agency to arbitrarily terminate the contract for convenience

[21]See Subpart 232.906 of the Department of Defense Supplement to the Federal Acquisition Regulation (DFARS), 48 CFR Chapter 2.

[22]See Subpart 232.903 of the Department of Defense Supplement to the Federal Acquisition Regulation (DFARS), 48 CFR Chapter 2.

[23]See page 34 of https://www.sec.gov/Archives/edgar/data/1336691/000119312510010871/dex44.htm. Eligible accounts exclude those "from the United States or any department, agency or instrumentality thereof (unless there has been compliance, to Banks satisfaction, with the Federal Assignment of Claims Act of 1940, as amended"

or for failure to obtain necessary budgeting. Relatedly, payments can be significantly delayed in the event of government shutdowns, as was the case in the fall of 2013.[24]

3 Empirical design

3.1 Identification Strategy

To understand the change in labor market outcomes driven by the *Quickpay* reform, we start with the following specification at the county-sector-year level:

$$LogY_{sct} = \gamma_{st} + \lambda_{ct} + \eta_{sc} + \beta_1.(Treatment_{sc}.d_t) + \beta_2.X_{sct} + \epsilon_{sct} \tag{10}$$

where Y_{stc} is either total payroll, total employment or average earnings, measured in county c and sector (or industry) s at date $t = \{2011Q1, 2015Q1\}$. d_t is a dummy for observations in 2015Q1. $Treatment_{sc}$ is defined as $\frac{FA_{sc}}{Y_{sc2011}}$ where FA_{sc} is the average quarterly amount of eligible government contracts to be performed in a given county×sector between 2009Q1-2011Q1. This includes all contracts awarded to small businesses, excluding non-eligible contracts as described in subsection 2.2. Y_{sc2011} is quarterly payroll measured 2011Q1. Our measure of treatment therefore captures the intensity of exposure to "Treated Contracts" in the quarter preceding the reform. As can be seen in Figure 1, treatment spans virtually every county in the US and exhibits substantial within-state variation, even without accounting for within county variation at the sector level. Moreover, since we measure treatment at the county-sector-year level, this allows us to identify the effect of the treatment while including fixed effects at sector-year level (γ_{st}), the county-year level (λ_{ct}) and county-sector level (η_{sc}). X_{sct} is a set of time-varying controls at the industry-sector level including government spending at the county-sector level.

[24]See www.nytimes.com/2013/10/19/us/shutdown-to-cost-us-billions-analysts-say-while-eroding-confidence.html

As in Card (1992) and Angrist and Pischke (2008), given that we analyze data for two periods, we collapse equation 10 into the following equation in first-differences:

$$\Delta LogY_{sc} = \gamma_s^{'} + \lambda_c^{'} + \beta_1.Treatment_{sc} + \beta_2.X_{sc}^{'} + \epsilon_{sc}^{'} \tag{11}$$

where $\Delta LogY_{sc}$ is the change in log payroll from 2011Q1 to 2015Q1. The set of controls, $X_{sc}^{'}$, include average quarterly government spending at the county-sector level normalized by 2011Q1 payroll, as well as the unemployment rate, correlation of employment growth with US employment growth, log employment, log average earnings, past three year employment growth, past three year earnings growth, past ten year employment growth, log average establishment size, and the employment share of small establishments, all measured as of 2011Q1.

The main coefficient of interest, β_1, measures the sensitivity of payroll growth from 2011Q1 to 2015Q1 to the county-sector share of accelerated contracts in total payroll. Because $\Delta LogY_{sc}$ approximates $\frac{Y_{sc2015}-Y_{sc2011}}{Y_{sc2011}}$, and recalling that $Treatment_{sc} = \frac{FA_{sc}}{Y_{sc2011}}$, β_1 can also be interpreted as a cash-flow elasticity of payroll, namely, the additional \$ of payroll spent for each accelerated \$ of sales (FA_{sc}).

Our identifying assumption, which is analogous to the parallel trends assumption, is that conditional on controls, treatment is orthogonal to labor market changes for the control group. There are several potential concerns about this assumption. First, county-sectors with high treatment intensity might be more exposed to government spending, and might therefore be on a different trend. We address this concern in several ways. We control for overall exposure to government contracts in all regressions. We also control for both ten-year employment growth as well as the correlation of employment growth in each county-sector with US employment growth in the past ten years. Finally, in placebo regressions, we check that county-sector exposure to government contracts that were not accelerated, either because they were already paid within 15 days, or because they were awarded to large businesses, does not drive employment growth.

Another concern is that county-sectors with high treatment intensity might be more exposed to small businesses, who might also have a different sensitivity to the business cycle. In addition to controlling for long term employment growth and correlation with the business cycle, we also control for the employment share of small establishments and the average establishment size in all regressions. If exposure to small businesses is driving the results, these variables should absorb the effect. In addition, we directly check in placebo regressions whether government contracts awarded to small businesses that were not accelerated following *Quickpay*, because they were already paid within 15 days, had any impact on payroll growth.

One might also worry that procurement policy might have changed after the reform in ways that could explain the results, irrespective of the payment acceleration. Appendix Figure A.1 indicates that the share of aggregate government spending going to small businesses (Panel A) is stable over the sample period. Even if there are no trends in government contracting in the aggregate, one might still worry that firms in areas highly exposed to treatment might have been awarded significantly more government contracts after the change. We check in Appendix Table A.1 whether county-sector exposure to treatment is associated with a change in average quarterly government contracts in the four years after *Quickpay* relative to the pre-period, normalized by 2011Q1 payroll. We find that this is not the case, so that results are unlikely to be driven by a surge of government contracts targeting areas where payments were also accelerated.[25]

A related concern is that other contract terms might have changed endogenously as a results of the reform. In particular, prices might have gone down as a result of the more aggressive bidding by small businesses after payments are accelerated. One may

[25]We also re-run our regressions using a measure based on contemporaneous acceleration of contracts and find the results to be unchanged. Note, however, that we do not attempt to distinguish how much of the effects we are capturing are coming from firms that were government contractors before the introduction of *Quickpay* and benefit from the payment acceleration, of from firms that became government contractors or were created after its introduction.

wonder whether the drop in prices could offset the increased liquidity associated with the acceleration. If it were the case, then this would prevent us from finding any effect of the reform on payroll. While we do not observe prices, we check whether government auctions are more likely to be awarded to small businesses and find no evidence for this.[26] Alternatively, if the time between invoicing and payment was used by federal agencies to check the quality of the goods being delivered, the shorter time period might allow small businesses to produce lower quality output, and might lead the government to shift its procurement away from them (Breza and Liberman 2016). Again, this would probably go against finding any positive effect of *Quickpay* on small business payroll.

Finally, one might worry that the payment acceleration reform might be correlated with other policies undertaken at the time to support the economy, such as the American Recovery and Reinvestment Act (ARRA) that was initiated in 2009. Fortunately, the procurement data we obtain includes all government contracts, including those awarded under ARRA, which were subject to the same acceleration policy. Hence there is little reason to think that ARRA-related procurement might affect our estimates. Moreover, we find estimates very similar to our baseline in Appendix Table A.6 where we control for the total amount of SBA-sponsored loans at the county and 2-digit NAICS level normalized by payroll.

3.2 Data

We combine a number of datasets to facilitate our analysis. The two core datasets we use are the publicly available US Census Quarterly Workforce Indicators (QWI) and the Federal Procurement Data System (FPDS). The QWI, which is based on micro data from the Longitudinal Employer Household Dynamics program (LEHD), allows us to measure

[26]Moreover, while small businesses can theoretically revert to their reservation profits after *Quickpay* by lowering prices, they might still grow payroll and employment in the process, thereby achieving the same level of profit with higher employment.

labor market outcomes at the county-sector level. For each two-digit sector[27] in each county, we obtain quarterly payrolls, employment and average earnings per worker.[28] The focus of our analysis is the change in these outcomes from 2011Q1 to 2015Q1. The data allow us to separately analyze job creations and separations. We also take advantage of a recently released supplement to the QWI, the job-to-job flows data. In each quarter, we obtain the number of workers of a given State changing jobs from one sector to another.

We measure treatment exposure using publicly available data on government contracts. The Federal Funding Accountability and Transparency Act of 2006 requires the Office of Management and Budget to maintain a public website describing each federal award in great details, including contracts, grants, direct payments and loans. This website was launched in 2007 and includes archives from the Federal Procurement Data System (FPDS) since 2000. For each contract, we obtain the contract identifier, amount and date when the contract is signed, the contract type (cost-plus or fixed-price), the name of the contractor and its six-digit sector, whether the contractor is a small business or not, and the zip code where the contract is to be performed.[29] As noted above, these data allow us to create a county-sector level measure of exposure to treatment, based on the average quarterly value of contracts awarded to small businesses in that cell over the period 2009Q1-2011Q1, scaled by payroll in that cell in 2011Q1. To minimize the role of outliers,we drop county-sector cells with less than 5 employees in 2011Q1, or where the ratio of government contracts to payroll is larger than 4.

Our control variables at the county-sector level are derived from the QWI and the County Business Patterns (CBP) data published by the US Census Bureau and based on the Longitudinal Business Database (LBD). Finally, to measure labor market tightness,

[27]Sectors are defined according to the National American Industry Classification System (NAICS).

[28]Unfortunately, these data do not allow us to measure wages. Earnings per worker are defined as the product of hourly wage and the number of hours of work per month.

[29]We also obtain the place of location of the contractor. While this is a less well measured data point, we find similar results when we use this information instead, most likely because both locations are the same in a vast majority of cases.

we follow the literature and compute the ratio of the number of vacancies to the number of unemployed workers for each local labor market in 2010. The former is obtained from the Conference Board HWOL data, and the latter is obtained from the Bureau of Labor Statistics (BLS). Higher values of this ratio indicate a tight labor market, where unemployment is low.

FPDS data does not incorporate payment speed information. To verify that the reform was effectively implemented, we obtained proprietary cash flow information from the Department of Defense's main payment system, the Mechanization of Contract Administration Services (MOCAS). For all receipts processed from 2010Q3 to 2014Q3, we obtain the date between receipt and payment as well as contract characteristics including the contract identifier that allow us to merge this information with FPDS data. Appendix Figure A.2 presents average payment terms, measured as the difference in days between the receipt and payment and the invoice around the implementation of the acceleration. From Panel A, we see that payment terms faced by small businesses with fixed-price contracts experience fall sharply. By contrast, Panel B shows that payment terms faced by large businesses do not change. Moreover, small businesses with cost-plus contracts are already paid within 15 days before the reform and experience little or no acceleration on average. We also show in Appendix Figure A.3 that the aggregate accounts payable of the federal government, including agencies for which we do not have the contract level descriptive data, go down starting in fiscal year 2011.[30]

Table 1 provides descriptive statistics for our main variables. As can be seen from Table 1, on average, the quarterly dollar value of accelerated contracts was 2.2% of the county-sector's 2011Q1 payroll. The average county-sector has 2,237 employees, average annual earnings per worker of $32,599 and average payroll a little over $100 million. County-sectors in the sample have on average 136 establishments in 2011, of which 15 had government contracts, and 8 had government contracts that were accelerated. Panel

[30]Fiscal year ends on September 30.

B of Table 1 also shows that accelerated contracts spanned every industry in our sample. Appendix Table A.2 further details the average annual $ value of of government contracts awarded to small and large businesses. Manufacturing and Professional Services account for over half the $ value of accelerated contracts, with Construction, Administration and Wholesale also accounting for a further third of the contract value. Overall, treatment seems spread out across sectors.

4 Results

4.1 The Direct Effect of Accelerated Payments

We first estimate the effect of the payment acceleration on payroll. As we show in equation 11, we aggregate both the exposure to the shock and the labor market outcomes of interest to the county-sector level. We consider the four-year change in total payroll from 2011Q1 to 2015Q1 as a function of the exposure to treatment. Table 2 presents our baseline results. Across specifications, the coefficient on treatment is positive and significant. The introduction of controls does attenuate the coefficient slightly, which remains stable around 0.08. In the most conservative specifications which includes the full set of controls as well as industry- and county fixed effects, we obtain a coefficient of 0.07 (Column 5).

Given that treatment is 2.2% on average, our findings suggest that county-sector payroll increases by 0.15% (2.2%*0.07) on average following the payment acceleration, relative to the baseline in the absence acceleration. Panel B reports the magnitudes in terms of a standardized treatment, and shows that a one standard deviation increase in treatment corresponds to a 1% increase in payroll. In Panel C, we weight regression by county-sector employment in 2011Q1. The coefficient drops to 0.056 but remains significant. This suggest that the effects tend to be slightly stronger in larger county-sectors. By permanently cutting in half the working capital needed to sustain a dollar of sales to the government, the policy thus led to a significant growth in payroll.

Measuring the implied cash-flow elasticity of payroll can be done by recognizing that a dollar of sales with the government prior to *Quickpay* requires 30 days of working capital. This implies that 30/365=8.2 cents are tied up in accounts receivables at any point in time. Moving to 15 days permanently frees up 4.1 cents of cash that can be compared with the 5.6 to 7 cents in additional payrolls. The implicit elasticity of 1.35 to 1.7 is higher than the few existing estimates from prior work focusing on publicly listed firms in Compustat and summarized in Schoefer (2015) who shows they range between 0.2 and 1. This should not come as a surprise given that the focus of our study is on small businesses that face more severe financing frictions. In particular, treated firms in our sample are much smaller than Compustat with a median of 6 employees (Compustat=500) and $0.6M in annual sales (Compustat=$150M). Moreover, rather than a one time windfall in cash flow, the payment acceleration is a permanent decrease in asset intensity, i.e., a shock to the amount of assets needed to produce a $ of sales. It is therefore not directly comparable to a one-time cash flow shock, and is likely to trigger a more significant response.

We provide several robustness tests for our baseline specification that we present in the Appendix. In Panel A of Appendix Table A.4, we run a differences-in-differences specification analogous to Equation 10, rather than a first-differences specification. As shown in Equation 10, this allows us to also control for industry-year, sector-year and county-sector fixed effects. The magnitudes of coefficients are almost unchanged, suggesting that our specification in first-differences accounts for the relevant sources of unobserved heterogeneity. Panel B shows that the results hold when we use a dummy taking the value of one for county-sector above the treatment median, and zero otherwise, rather than the continuous version of treatment that we use in the baseline analysis. This suggests that our findings are unlikely to be driven by outliers. In Panel C, we restrict the sample to county-sector cells with positive government contracts and control for the share of treated contracts in total government contracts. Our main coefficient is unaffected, which suggests that what matters is the ratio of treated contracts to total payroll. In Panel A of Appendix Table A.5 we further show that measuring treatment based on ex post (i.e. actual, but endogenous) accelerated contracts as opposed to our exogenous measure of exposure to acceleration based on pre-period contracts yields similar magnitudes. In Appendix Table A.7 we run our specifications at the county-by-4-digit NAICS to include tighter industry fixed effects. Because data is not available for all county-sectors at this level of disaggregation, the magnitudes vary slightly, but the results remain highly significant.

4.1.1 Dynamics and Falsification Tests

We next examine the dynamics around the reform. Table 3 replicates columns 5 in Table 2, but with different long-difference intervals so as to analyze the evolution of the effects between 2009 and 2015. Column 1 of Table 3 looks at the change in payroll from 2009Q1 to 2011Q1 and column 2 looks at the change from 2010Q1 to 2011Q1. Both Columns 1 and 2 show that, there is no evidence of a pre-trend. Columns 3-6 look at the post-period, with increasing windows from the introduction of the reform. Column 3 shows that about half the measured effect in Table 2 was realized in the first year, although this is not precisely estimated. Further, but diminishing marginal growth is seen in the subsequent two years. These dynamics are reassuring, as they highlight that the timing of the effects are consistent with the accelerated payments causing the increase in payroll.

Another way to assess the presence of pre-trends is to run our cross-sectional county-sector regressions at different points in time. We run the specification presented in Table 11 Panel B, Column 4 in every quarter from 2004 to 2011. We present the point estimates in Panel A of Figure 2 along with 95% confidence intervals. The red line denotes 2007Q1, the point in time after which 4-year forward changes in log payroll include the post-reform period. Prior to this point, the estimates are not different from zero. Afterwards, the point estimates increase until they reach their highest value for 2011Q1, as expected. This clearly indicates that the findings are not the spurious outcomes of prior trends.

From an identification perspective, a nice feature of the *Quickpay* reform was that it accelerated a subset of small business contracts and left all large business contracts unchanged. This allows us to run falsification tests where we split total government spending at the county-sector level into its constituent parts and see whether county-sector exposure to non-eligible contracts awarded to small businesses, or to contracts awarded to large businesses are also associated with payroll growth. If this were so, it would suggest that something related broadly to government exposure, or a particular contract-type, rather than the *Quickpay* reform per se, was responsible for our findings. We report the results from these falsification regressions in Table 4, which are similar to those in Table 2. Only exposure to eligible contracts awarded to small businesses causes an increase in aggregate payroll. Payrolls are not affected by exposure to contracts awarded to small businesses that were not accelerated, nor by exposure to contracts awarded to large businesses. This provides compelling evidence that the effects we a picking up are caused by the reform.

4.1.2 Plant Level Evidence

A concern may be that we cannot control for county×sector fixed effects since our treatment variable is defined at this level. To check whether we are not picking up specific county×sector trends despite of the controls we add to the regressions, we exploit firm level data from the National Establishment Time-Series (NETS) database that collects employment information for establishments across the country.[31] We merge this data

[31] The NETS dataset is drawn from the Dun and Bradstreet archival establishment data. It provides longitudinal data on firm-level employment and includes a Dun and Bradstreet identifier for each plant, also available from FPDS. These data are not a true 'census' of establishments and unfortunately, many of the employment numbers are imputed or are updated infrequently. While this dataset provides us with a useful way to do robustness checks, we prefer to base the magnitudes of our effects on the more comprehensive Census data.

with government data and construct the treatment variable as the average quarterly amount of government contracts awarded to the plant between 2009Q1-2011Q1, normalized by 2011Q1 employment. We regress the change in log employment between 2011Q1 and 2015Q1 on treatment as well as total government contracts normalized by 2011Q1 employment. Appendix Table A.8 shows that the effect of treatment is positive and significant, and that the introduction of County×Sector (6 digit) fixed effects does not affect the coefficient. It is thus very unlikely that specific county×sector trends are driving our findings.

4.1.3 Heterogeneous Treatment Effects

The results in Table 3 and 4 together document that accelerated payments stimulate payroll growth. We next check whether the effects are larger in some industries and areas. Since our treatment variable is scaled by payroll, we would expect to find a larger effect in industries where payroll is larger relative to sales. For the same level of our treatment variable, a firm is more likely to grow if accelerated contracts are a larger share of its total sales – which our treatment variable will capture if the ratio of payroll to sales is larger. We find that this is the case in Column 1 of Table 5, where we interact the treatment variable with a dummy for high payroll-to-sales sectors, based on BEA industry accounts. Moreover, if these effects are driven by a drop in asset intensity generated by the reform, we would expect them to be stronger in sectors where receivables form a larger share of total assets. If a firm needs a lot of other assets to operate, then a reduction in its accounts receivable is unlikely to have a major effect on its ability to grow. This is what we find in Column 2 of Table 5, where we interact the treatment variable with a dummy for high receivables-to-assets sectors, based on Compustat.

Moreover, we would expect the effects to be stronger where financing frictions are more severe. In Columns 3 and 4 of Table 5, we estimate the coefficient on the treatment variable separately in industries with high and low degrees of pledgeability, measured with the ratio of fixed assets to assets obtained from Compustat, and in counties with high or low issuances of small business loans per establishments in 2010, based on Community Reinvestment Act and County Business Patterns data. We find the effects to be more pronounced in low pledegeability industries and counties with fewer small business loans, where credit availability is likely to be lower. Although the differences in coefficients are not statistically different at conventional levels, these findings suggest that the effect of payment acceleration on payroll is driven by a relaxation in financing constraints.

4.1.4 Employment Response

The QWI data allow us to separate payroll growth into the share arising from employment growth (extensive margin) versus growth in average earnings (intensive margin).[32] We report the results in Table 6, which is equivalent to Table 2, except that $\Delta LogY_{sc}$ now measures the county-sector change in total employment and in average earnings between 2011Q1 and 2015Q1. As with Table 2, the coefficients attenuate slightly with the inclusion of fixed effects. They remain statistically significant and economically meaningful even with the inclusion of the full set of fixed effects, as evidenced in Column 5. Comparing the magnitude of the coefficients in Panel A and Panel B to those found in Table 2, we conclude that most (80%) of the change in payroll growth comes from the extensive margin through growth in employment, with the remaining fifth arising at the intensive margin from a change in earnings. Such an increase in employment might be driven by an increase in new hires or a decrease in separations. A unique feature of the QWI data is that it separately reports new hires and separations, which are typically not available in aggregated datasets. We show in Appendix Table A.9 that the treatment increases the hiring rate but not the separation rate, leading to more net hires. This suggests that rather than delaying layoffs, the reforms triggered the creation of actual new jobs.

[32] Again, earnings growth can either be explained by a increase in wages or an increase in hours worked, and our data does not allow us to differentiate between the two.

4.1.5 Extent of Financing Constraint Implied by Estimates

As noted in Section 2 above, an attractive feature of our setting is that we are able to map our reduced form estimates to the simple model outlined in Section 2, and thereby infer the cost of financing facing the small businesses in our setting. Recall that Equation 8 allows us to estimate the degree of constraints faced by firms, because it only depends on employment growth and the change in γ_i that we both observe empirically, model parameters that we can calibrate with standard values, as well as R, the cost of financing, that we can therefore infer. More precisely, we assume that for the subset of firms t affected by the change, the fraction of input costs that needs to be financed in advance, $\gamma_{t,1} = 30/365 = .8$ and $\gamma_{t,2} = 15/365 = .4$. By contrast, for the subset of firms u unaffected by the reform, $\gamma_{u,1} = \gamma_{u,2} = 30/365 = .8$. We use standard parameter value for the labor share ($\sigma = 2/3$), the returns to scale parameter ($\alpha = 0.9$) and the elasticity of labor supply ($\theta = 0.5$). Table 7 presents the values for employment growth implied by our theoretical framework as a function of the cost of financing R.

The reduced form estimates presented in Table 6 give the response of a firm receiving accelerated contracts amounting to 100% of its *payroll*. For such a firm, the acceleration leads to a 5.7% increase in employment over the next four years. Given that the average share of payroll in total sales is 33% in the BEA input-output data, a firm with 100% of its *sales* affected by the reform would therefore experience a 0.057*3=17% increase in employment. As can be seen from Panel A of Table 7, our results are therefore consistent with a cost of external finance of 40% annually. This estimate compares relatively well with the implicit interest rate on trade credit contracts. These typically allow the buyer to pay within 30 days, and to get a 2% discount in the event where the payment occurs before 20 days ("2/20-net 30"). A 2% discount for 10 days implies a annual interest rate of 37%, close to our own estimates. Our estimate for R is also relatively close, albeit slightly larger than the rates charged by asset-backed lenders that typically ranged between 4 and

5% monthly, or 18 to 30% annually at the time of the reform (Mount 2012).

4.2 Spillover Effects

We next turn to the analysis of spillovers to firms not directly affected by the reform. Our theoretical framework highlights that these firms face an increase in equilibrium wages from the rising labor demand of treated firms, that in turn reduces their demand for labor. To test for the presence and the direction of spillovers, we augment our baseline specification with a measure of total accelerated dollars at the commuting zone level, which is the appropriate level to capture labor market dynamics.[33] More precisely, we construct the variable *Treatment: CZ* defined as the average quarterly amount of eligible government contracts to be performed in a given commuting zone between 2009Q1-2011Q1, excluding the focal county-sector, normalized by aggregate quarterly payrolls in 2011Q1, also excluding the focal county-sector. Controlling for the treatment at the county-sector level, this measure therefore picks up the intensity of treatment in *other* county-sectors in a given commuting zone relative to the focal county-sector. The coefficient on this variable is positive if spillovers are positive, and conversely if they are negative. We standardize the county-sector level treatment and the commuting zone level treatment by their respective cross-sectional standard deviations to be able to compare their economic magnitudes. Finally, we augment our baseline specification with several commuting zone level controls including the unemployment rate, the share of small establishments, the average establishment size and the log of total employment and average earnings measured in 2011Q1.[34]

As evidenced in the first row of Table 8, treatment at the county-sector level continues to maintain its significance and economic magnitude. A one standard deviation in

[33] There are a total of 709 commuting zones that cover the entire land area of the US and represent labor market clusters of US counties.

[34] Because we now include a county-level measure as a regressor, we do not include county fixed effects and include State fixed effects instead.

Treatment is associated with a 1% increase in payrolls, a 0.9% increase in employment and a 0.1% in earnings – although the effect on earnings is not statistically different from zero. The inclusion of *Treatment: CZ* comes in with a negative and significant coefficient, implying that a one standard deviation in Treatment at the commuting zone level is associated with a drop by 0.8% in payrolls and 0.6% in employment. Little or no effect is found on earnings. This is consistent with the payment acceleration having substantial crowding out effect on the employment decisions of firms in the neighborhood of treated firms.

In Table 9, we run the same regressions as those in Table 3, but with the inclusion of *Treatment: CZ* along side the county-sector treatment. Again, it can be seen that there are no pre-trends in payroll growth, neither in terms of direct effects and nor in terms of the crowding out effects. In Panel B of Figure 2, we run the specification presented in Table 8, Column 3, in every quarter from 2004 to 2011. We present the point estimates along with 95% confidence intervals. The red line denotes 2007Q1, the point in time after which 4-year forward changes in log payroll include the post-reform period. Prior to this point, the estimates are not different from zero. Afterwards, the point estimates decrease until they reach their lowest value for 2011Q1, as expected.

Table 10 runs the same falsification regressions as in Table 4 and shows that crowding out effects are only found in commuting zones with a large exposure to small firm with eligible contracts. By contrast, exposure to small business contractors with non-eligible contracts, and exposure to large business contractors do not drive spillovers. As with the direct effects, the results in Tables 9 and 10 provide confidence that the effects we are seeing are being driven by the reform, and moreover, are not due to any systematic differences in counties with exposure to either government contracts or small businesses in general.

We then search for direct evidence of employment flows from low to high treatment firms. We use the recently released data on Job-to-Job flows from the Longitudinal

Employer-Household Dynamics database to examine the origin and destination sectors of people changing jobs within a given state. We run OLS cross-sectional regressions at the State × Origin sector × Destination sector of the form

$$JobFlow_{s,o,d} = \beta_0 + \beta_1.Treatment_o + \beta_2.Treatment_d + \beta_3.X_{s,o} + \beta_4.X_{s,d} + \eta_s + \omega_d + \zeta_o + \epsilon_{s,o,d}$$

where $JobFlow_{s,o,d}$ is defined as total job flows from origin sector o to destination sector d in State s from 2011Q2 to 2015Q1 normalized by 2011Q1 employment in sector d in State s. $Treatment_{s,o}$ is the treatment for origin sector o in State s, and $Treatment_{s,d}$ is the treatment for destination sector d in State s. As can be seen from Panel A of Table 11, destination sectors exposed to high treatment are more likely to see an inflow and origin sectors exposed to high treatment are less likely to see an outflow of workers. Panel B of Table 11 takes the difference in the treatment between the destination and origin sectors and shows that the difference strongly predicts job-flows. Together with the results in Tables 8-10, this is compelling evidence of firms across sectors competing in common local factor markets, leading to crowding out effects when some firms face a reduction in financing constraints.

4.3 Heterogeneity across Local Labor Markets

Since our mechanism depends on labor demand among non-treated firms falling due to an increase in equilibrium wages, we should expect the effects to vary with labor market tightness. As can be seen from Figure A.4, there was substantial heterogeneity in the unemployment rates across commuting zones in the time period of our analysis, with a 3 percentage point difference between the commuting zones in the 25th and 75th percentiles of the distribution. We therefore segment the commuting zones in our sample into those with relatively high and low labor market tightness, measured at the commuting zone level as the ratio of the number of vacancies to the number of unemployed workers in 2010. Labor market tightness is strongly negatively related with unemployment rates and our results are unchanged when we use unemployment rates instead. Table 12 presents the results of the spillover regression where the treatment variables are interacted with a dummy for high and low labor market tightness. In commuting zones with low tightness (high unemployment), we find that the direct impact of acceleration is felt more strongly and that there is no measured effect in terms of spillovers. On the other hand, in commuting zones with tight labor markets in 2010, there is substantial crowding out of non-treated firms employment.

We next aggregate the analysis at the commuting zone level. In Table 13, we run the same cross-sectional regression as in Table 12 at the commuting zone, rather than the county-sector level. Here, the treatment variable jointly captures the direct and spillover effects at the local labor market level. On average, the effect of treatment is positive (Columns 1 and 2). However, it is not significantly different from zero in tight labor markets (Columns 3 and 4). The policy has little or no effect on employment in these areas, presumably because the spillover effects fully offset the direct effects.

While spillover effects appear to be negative on average, we explore whether there might not be mitigated by positive demand externalities. In particular, in the presence of nominal rigidities, if firms in a commuting zone start hiring due to the payment acceleration, this might generate some additional demand for local non-tradable industries. We check whether this is the case by separately estimating the coefficient on the treatment variable measured at the commuting zone level for tradable and non-tradable industries.[35] As can be seen in Table A.10, the negative spillover effect is largely mitigated for non-tradable industries, which is consistent with the idea that they also benefit from a boost in demand that offsets the crowding out effect they experience on their local labor market.

[35]Non-tradable industries include health care, hospitality, food service, education, retail, and construction.

5 Conclusion

In this paper we analyze the impact of the *Quickpay* reform of 2011. We show that even in the presence of moderate financing frictions, a fall in the need to finance working capital through the production process can have substantial effects on firms' employment decisions. Importantly, we find that the resulting employment growth of treated firms can have significant negative spillovers on firms that compete in common labor markets. In tight local labor markets, these spillovers completely negate the positive effects of the reform on firms that benefit from the treatment, although the net effect remains positive in areas with initially higher levels of unemployment. More generally, this crowding out effect has important consequences for policy makers: while accelerating payments seems to be a way for the government to reduce financing constraints for small businesses, the overall effect of a reduction in financing constraints is likely to be significantly smaller when firms compete for talent, particularly in local labor markets where unemployment rates are already low.

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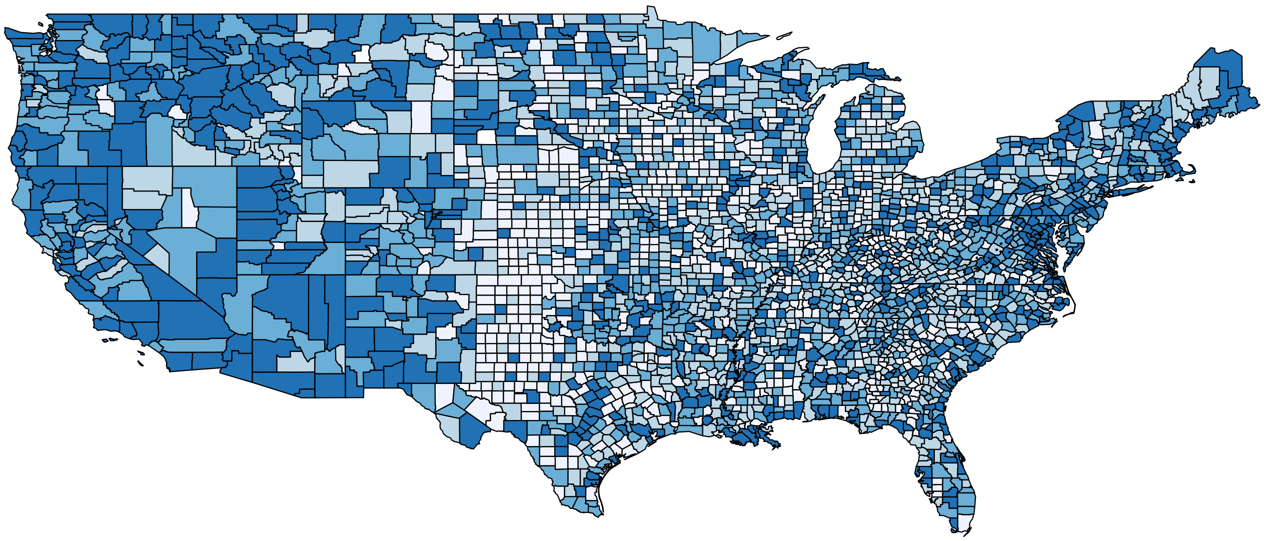
Panel A. Total government contracts



Panel B. Accelerated government contracts

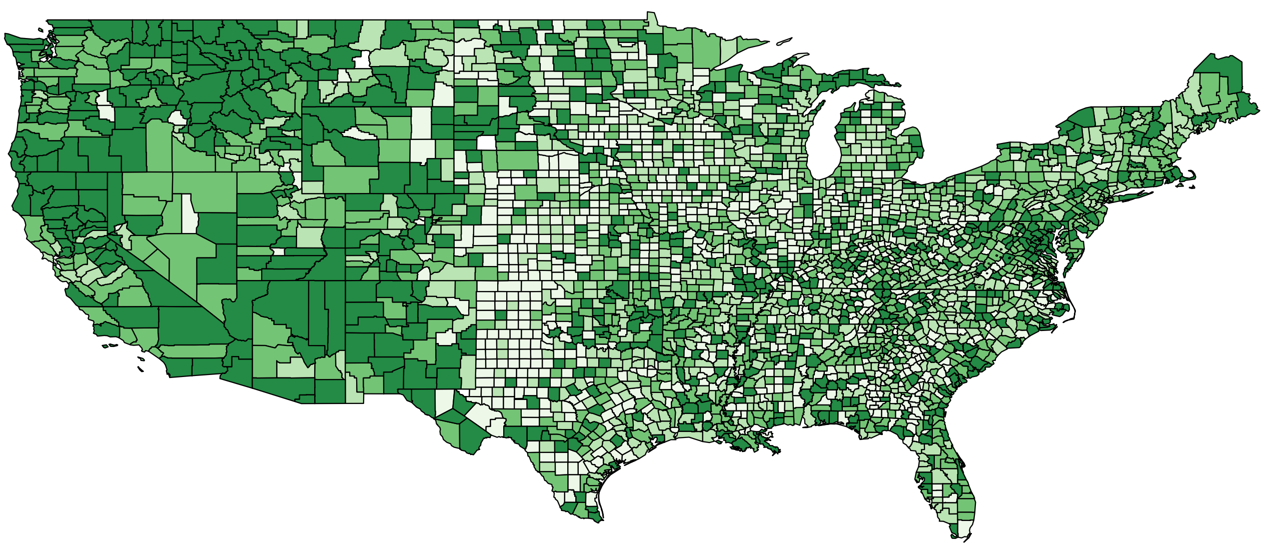
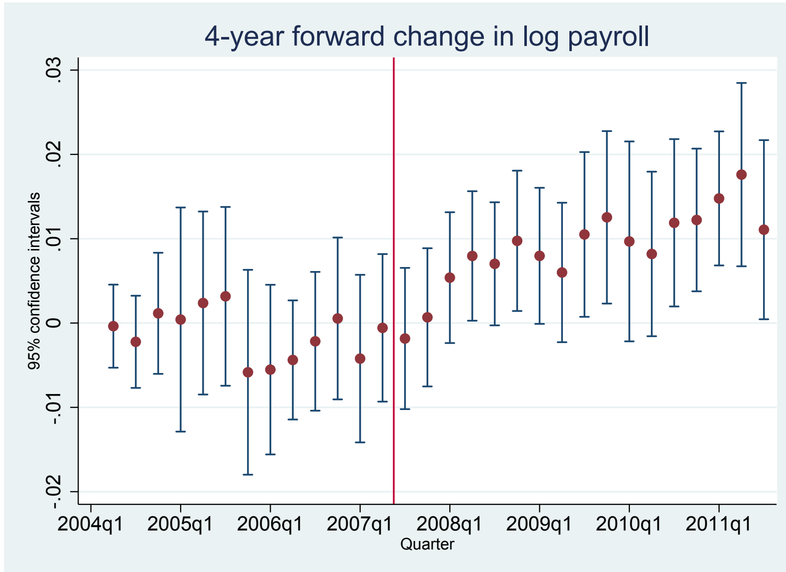


Figure 1: **Distribution of government contracts across US counties.** This figure shows the distribution of total government contracts (Panel A) and government contracts eligible to acceleration (Panel B) aggregated at the county level in the two years prior to the reform, normalized by total county payrolls. Darker shades indicate larger intensity county level exposure.

Panel A. Direct effects



Panel B. Spillover effects

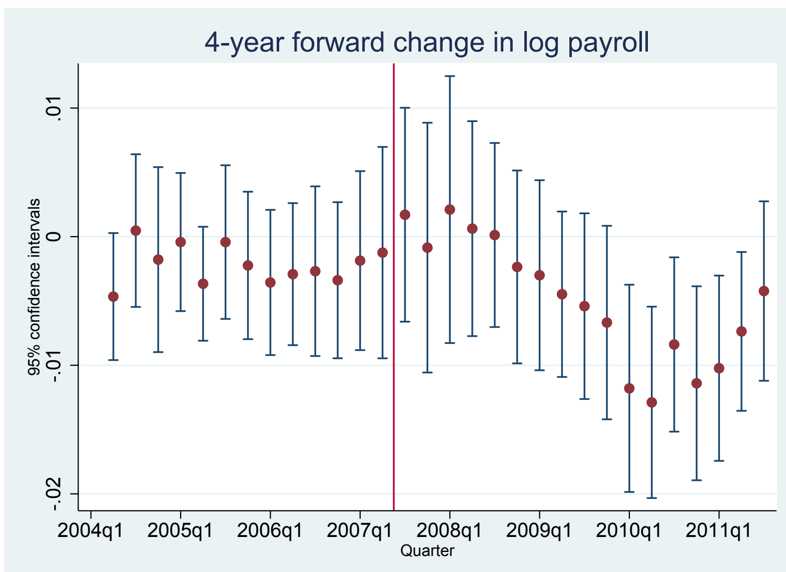


Figure 2: **Direct and spillover effects.** This figure shows the direct effects (Panel A) and spillover effects (Panel B) of payment acceleration on 4-year forward change in log payroll. In each quarter from 2004 to 2011, we measure the direct effect by running a regression at the county×sector level of the change in log payroll on the treatment variable as well as control variables and county fixed effects. *Treatment* is the average quarterly amount of eligible government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll. We measure the indirect effect by running a similar regression augmented with *Treatment: CZ*, measured at the commuting zone level rather than the county×sector level, and excluding the focal county×sector. *Treatment* and *Treatment: CZ* are normalized by their cross-sectional standard deviation. The point estimates are presented along with 95% confidence intervals.

Table 1: Summary statistics

Panel A of this table presents summary statistics for the key outcome and control variables, measured at the county×sector level we consider in our analysis. There are 3120 counties and 18 industries. Treatment is the average quarterly amount of eligible government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by average quarterly payrolls measured in 2011Q1. Variables of interest include payroll, employment and earnings growth rates between 2011Q1 and 2015Q1. Controls variables include the average quarterly amount of all government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by average quarterly payrolls during the same period, as well as additional county×sector controls including the share of small establishment, the average establishment share, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth in a given county×sector with aggregate employment growth, total employment, total annualized payrolls and average earnings measured in 2011Q1. Panel B presents the distribution of the treatment variable across two-digit sectors.

Panel A: Key variables				
	Mean	Sd	p5	p95
Treatment	0.022	0.132	0.000	0.081
Government contracts	0.053	0.249	0.000	0.224
Δ log payrolls	0.139	0.381	-0.366	0.676
Δ log employment	0.036	0.314	-0.389	0.477
Δ log earnings	0.103	0.196	-0.163	0.389
Unemployment rate	9.492	3.018	4.700	14.500
Corr with US emp growth	0.120	0.275	-0.330	0.580
Average establishment size	2.227	0.868	0.916	3.730
Share of small establishments	0.994	0.024	0.966	1.000
Emp share of small establishments	0.825	0.261	0.270	1.000
Long term employment growth	0.063	0.141	-0.040	0.371
Employment	2237	9810	19	9194
Annualized earnings ('000)	32.599	19.402	11.136	66.936
Annualized payrolls ('000)	101503	871446	393	359495
Log total employment	5.756	1.862	2.944	9.126
Log average earnings	7.766	0.530	6.833	8.627
Log average payroll	13.523	2.051	10.396	17.215
Panel B: Distribution of treatment across sectors				
Accommodation	0.003	0.031	0.000	0.006
Administrative and support	0.048	0.203	0.000	0.216
Agriculture	0.042	0.204	0.000	0.195
Arts	0.003	0.022	0.000	0.006
Construction	0.092	0.291	0.000	0.481
Education	0.019	0.120	0.000	0.073
Finance	0.001	0.016	0.000	0.000
Health care	0.003	0.016	0.000	0.009
Information	0.014	0.124	0.000	0.034
Manufacturing	0.036	0.146	0.000	0.153
Mining	0.012	0.137	0.000	0.022
Other services	0.006	0.046	0.000	0.020
Professional services	0.031	0.115	0.000	0.141
Real estate	0.040	0.158	0.000	0.179
Retail	0.002	0.013	0.000	0.009
Transportation	0.013	0.095	0.000	0.039
Utilities	0.007	0.070	0.000	0.013
Wholesale	0.018	0.098	0.000	0.061
Total	0.022	0.132	0.000	0.081

Table 2: Direct effect of payment acceleration on payrolls

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the county×sector level of the change in log payroll on county×sector exposure to government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment* is the average quarterly amount of eligible government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll. Control variables include the average quarterly amount of all government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll, as well as additional county×sector controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. There are 3120 counties and 18 industries. Panel A presents the baseline estimation. In Panel B, the treatment variable is normalized by its cross-sectional standard deviation. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Δ Log payroll (2011Q1-2015Q1)				
	Panel A: Baseline				
Treatment	0.145***	0.085***	0.071**	0.095***	0.070**
	(0.032)	(0.030)	(0.029)	(0.032)	(0.031)
County×sector controls	No	Yes	Yes	Yes	Yes
Sector FE	No	No	Yes	No	Yes
County FE	No	No	No	Yes	Yes
Observations	44484	44484	44484	44484	44484
R^2	0.002	0.048	0.066	0.159	0.184
	Panel B: Standardized treatment				
Treatment (std)	0.020***	0.012***	0.010**	0.013***	0.010**
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
County×sector controls	No	Yes	Yes	Yes	Yes
Sector FE	No	No	Yes	No	Yes
County FE	No	No	No	Yes	Yes
Observations	44484	44484	44484	44484	44484
R^2	0.002	0.048	0.066	0.159	0.184
	Panel C: Regressions weighted by employment				
Treatment	0.094***	0.077**	0.048	0.082**	0.056*
	(0.036)	(0.034)	(0.033)	(0.032)	(0.030)
County×sector controls	No	Yes	Yes	Yes	Yes
Sector FE	No	No	Yes	No	Yes
County FE	No	No	No	Yes	Yes
Observations	44484	44484	44484	44484	44484
R^2	0.003	0.057	0.094	0.228	0.268

Table 3: Direct effect of payment acceleration: dynamics

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the county×sector level of the change in log payroll on county×sector exposure to government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment* is the average quarterly amount of eligible government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll. Control variables include the average quarterly amount of all government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll, as well as additional county×sector controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. There are 3120 counties and 18 industries. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Δ Log payroll (relative to 2011Q1)					
	[t-2,t]	[t-1,t]	[t,t+1]	[t,t+2]	[t,t+3]	[t,t+4]
Treatment	0.014	0.003	0.037	0.042	0.055*	0.070**
	(0.022)	(0.021)	(0.024)	(0.028)	(0.031)	(0.031)
County×sector controls	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	44369	44349	44368	44344	44367	44484
R^2	0.571	0.321	0.127	0.146	0.164	0.184

Table 4: Direct effect of payment acceleration: falsification tests

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the county×sector level of the change in log payroll on county×sector exposure to government contracts normalized by 2011Q1 payroll. *Small* and *Large* refer to contracts awarded to small and large business respectively. *Eligible* (non-eligible) refers to contracts that were paid in 30 days (sooner than 30 days) prior to the reform. Control variables include the average quarterly amount of all government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll, as well as additional county×sector controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. There are 3120 counties and 18 industries. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Δ Log payroll (2011Q1-2015Q1)				
Small and eligible	0.132***	0.078***	0.054**	0.105***	0.061**
	(0.026)	(0.025)	(0.024)	(0.027)	(0.027)
Small and non-eligible	-0.043	-0.031	-0.060	0.034	-0.013
	(0.083)	(0.072)	(0.072)	(0.069)	(0.069)
Large	-0.012	-0.006	-0.016	0.009	-0.008
	(0.013)	(0.012)	(0.012)	(0.013)	(0.013)
County×sector controls	No	Yes	Yes	Yes	Yes
Sector FE	No	No	Yes	No	Yes
County FE	No	No	No	Yes	Yes
Observations	44484	44484	44484	44484	44484
R^2	0.002	0.048	0.066	0.159	0.184

Table 5: Direct effect of payment acceleration: heterogeneous treatment effects

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the county×sector level of the change in log payroll on county×sector exposure to government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment* is the average quarterly amount of eligible government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll. The treatment variable is interacted successively with a dummy for high payroll/sales sectors (based on BEA industry accounts), high receivables/assets sectors (based on Compustat), high pledgeability (fixed assets/assets based on Compustat), and high small business loans per establishments (based on Community Reinvestment Act and County Business Patterns data). Control variables include the average quarterly amount of all government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll, as well as additional county×sector controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. There are 3120 counties and 18 industries. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Δ Log payroll (2011Q1-2015Q1)			
	Treatment intensity		Access to external finance	
	Sector mean payroll/sales	Sector mean receivables/assets	Sector mean pledgeability	Small bus. loans per establishment
Treatment × high	0.093**	0.096***	0.034	0.026
	(0.038)	(0.034)	(0.072)	(0.039)
Treatment × low	0.047	-0.016	0.082**	0.101**
	(0.050)	(0.069)	(0.033)	(0.045)
County×sector controls	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	44484	41533	41533	44484
R^2	0.184	0.190	0.190	0.184

Table 6: Direct effect of payment acceleration: employment and earnings

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the county×sector level of the change in log employment (Panel A) and log average earnings (Panel B) on county×sector exposure to government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment* is the average quarterly amount of eligible government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll. Control variables include the average quarterly amount of all government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll, as well as additional county×sector controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. There are 3120 counties and 18 industries. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Panel A: Δ log employment (2011Q1-2015Q1)				
Treatment	0.097***	0.071***	0.061**	0.078***	0.057**
	(0.025)	(0.025)	(0.024)	(0.027)	(0.026)
County×sector controls	No	Yes	Yes	Yes	Yes
Sector FE	No	No	Yes	No	Yes
County FE	No	No	No	Yes	Yes
Observations	44484	44484	44484	44484	44484
R^2	0.001	0.034	0.060	0.140	0.173
	Panel B: Δ log earnings (2011Q1-2015Q1)				
Treatment	0.047***	0.014	0.010	0.017	0.012
	(0.013)	(0.012)	(0.011)	(0.014)	(0.012)
County×sector controls	No	Yes	Yes	Yes	Yes
Sector FE	No	No	Yes	No	Yes
County FE	No	No	No	Yes	Yes
Observations	44484	44484	44484	44484	44484
R^2	0.001	0.094	0.165	0.172	0.244

Table 7: Direct effect of payment acceleration: implied financing frictions

This table presents the values for employment growth implied by our theoretical framework presented in section 2 as a function of the cost of financing R. We also present the corresponding reduced form coefficient, where we divide the model prediction by 3 to reflect the fact that our treatment variable is scaled by payroll rather than sales.

	Predicted employment growth as a function of R						
R	0.25	0.30	0.35	0.40	0.45	0.50	0.55
Implied employment growth at treated firms $\Delta L_t^* - 1$	0.10	0.12	0.14	0.17	0.19	0.21	0.23
Corresponding reduced form coefficient	0.034	0.041	0.048	0.055	0.062	0.070	0.077

Table 8: Spillover effect of payment acceleration: baseline

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the county×sector level of the change in log payroll, log employment and log earnings on county×sector exposure to government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment* is the average quarterly amount of eligible government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll. *Treatment: CZ* is the same variable, measured at the commuting zone level rather than the county×sector level, and excluding the focal county×sector. *Treatment* and *Treatment: CZ* are normalized by their cross-sectional standard deviation. County×sector control variables include the average quarterly amount of all government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll, as well as additional county×sector controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. CZ controls include the unemployment rate, the share of small establishments, the log average establishment size, log total employment and log average earnings in 2011Q1. There are 3120 counties and 18 industries. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Δ log payroll		Δ log employment		Δ log earnings	
Treatment	0.012***	0.010**	0.010***	0.009***	0.002	0.001
	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.001)
Treatment: CZ	-0.010***	-0.008**	-0.007**	-0.007**	-0.003**	-0.001
	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)
County×sector controls	Yes	Yes	Yes	Yes	Yes	Yes
CZ controls	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	No	Yes	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	44484	44484	44484	44484	44484	44484
R^2	0.065	0.085	0.047	0.074	0.104	0.178

Table 9: Spillover effect of payment acceleration: dynamics

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the county×sector level of the change in log employment on county×sector exposure to government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment* is the average quarterly amount of eligible government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll. *Treatment: CZ* is the same variable, measured at the commuting zone level rather than the county×sector level, and excluding the focal county×sector. *Treatment* and *Treatment: CZ* are normalized by their cross-sectional standard deviation. County×sector control variables include the average quarterly amount of all government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll, as well as additional county×sector controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. CZ controls include the unemployment rate, the share of small establishments, the log average establishment size, log total employment and log average earnings in 2011Q1. There are 3120 counties and 18 industries. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Δ log employment (relative to 2011Q1)					
	[t-2,t]	[t-1,t]	[t,t+1]	[t,t+2]	[t,t+3]	[t,t+4]
Treatment	0.003	0.001	0.004*	0.005*	0.007**	0.009***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Treatment: CZ	0.000	-0.001	-0.003	-0.005**	-0.007***	-0.007**
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)
County×sector controls	Yes	Yes	Yes	Yes	Yes	Yes
CZ controls	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	44369	44349	44368	44344	44367	44484
R^2	0.545	0.267	0.038	0.051	0.064	0.074

Table 10: Spillover effect of payment acceleration: falsification tests

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the county×sector level of the change in log employment on commuting zone exposure to government contracts normalized by 2011Q1 payroll. *Small* and *Large* refer to contracts awarded to small and large business respectively. *Eligible* (non-eligible) refers to contracts that were paid in 30 days (sooner than 30 days) prior to the reform. All three variables are standardized by their cross-sectional standard deviation. County×sector control variables include the average quarterly amount of all government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll, as well as additional county×sector controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. CZ controls include the unemployment rate, the share of small establishments, the log average establishment size, log total employment and log average earnings in 2011Q1. There are 3120 counties and 18 industries. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Δ log employment (2011Q1-2015Q1)				
Small and eligible: CZ	-0.007***	-0.006***	-0.006**	-0.006***	-0.006**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Small and non-eligible: CZ	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Large: CZ	0.003	0.009	0.007	0.009	0.014
	(0.036)	(0.036)	(0.036)	(0.036)	(0.037)
County×sector controls	No	Yes	No	Yes	Yes
CZ controls	Yes	Yes	Yes	Yes	Yes
Sector FE	No	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Observations	44484	44484	44484	44484	44484
R^2	0.014	0.047	0.037	0.047	0.074

Table 11: Spillover effect of payment acceleration: Job-to-job flows

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS cross-sectional regressions at the State×Origin sector × Destination sector. The dependent variable, Job flows, is defined as total job flows from origin sector to destination sector in a given State from 2011Q2 to 2015Q1 normalized by 2011Q1 employment in the destination sector. Treatment is the average quarterly amount of eligible government contracts to be performed in a given sector and State between 2009Q1-2011Q1, normalized by 2011Q1 quarterly payroll. In Panel A, both treatment variables for the origin and destination sectors enter the regressions separately, while we use the difference between the two in Panel B. Control variables include the average quarterly amount of all government contracts to be performed in a given State×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll, as well as additional State×sector controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Job flows (2011Q2-2015Q1) % of 2011Q1 employment				
Treatment, destination	0.190***	0.048***	0.021	0.065***	0.028
	(0.046)	(0.015)	(0.018)	(0.018)	(0.017)
Treatment, origin	0.037	-0.043**	-0.016	-0.027	-0.010
	(0.024)	(0.018)	(0.016)	(0.021)	(0.015)
Observations	14990	14689	14689	14689	14689
R^2	0.271	0.540	0.593	0.553	0.599
	Job flows (2011Q2-2015Q1) % of 2011Q1 employment				
Difference in treatment	0.076***	0.046***	0.019**	0.046***	0.019**
	(0.017)	(0.007)	(0.009)	(0.007)	(0.009)
Observations	14990	14689	14689	14689	14689
R^2	0.265	0.540	0.593	0.553	0.599
Controls (origin State-sector)	No	Yes	Yes	Yes	Yes
Controls (destination State-sector)	No	Yes	Yes	Yes	Yes
Origin State-sector FE	No	No	Yes	No	Yes
Destination State-sector FE	No	No	Yes	No	Yes
State FE	No	No	No	Yes	Yes

Table 12: Spillover effect of payment acceleration: labor market tightness

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the county×sector level of the change in log employment on county×sector exposure to government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment* is the average quarterly amount of eligible government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll. *Treatment: CZ* is the same variable, measured at the commuting zone level rather than the county×sector level, and excluding the focal county×sector. *Treatment* and *Treatment: CZ* are normalized by their cross-sectional standard deviation. County×sector control variables include the average quarterly amount of all government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll, as well as additional county×sector controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. CZ controls include the unemployment rate, the share of small establishments, the log average establishment size, log total employment and log average earnings in 2011Q1. There are 3120 counties and 18 industries. Labor market tightness is measured at the CZ level as the ratio of the number of vacancies to the number of unemployed workers in 2010. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

High versus low labor market tightness *Above or below median*	Δ log employment (2011Q1-2015Q1)			
Treatment × low	0.011***	0.009**	0.011***	0.009**
	(0.004)	(0.004)	(0.004)	(0.004)
Treatment × high	0.009	0.008	0.009	0.008
	(0.006)	(0.006)	(0.006)	(0.006)
Treatment: CZ × low			-0.004	-0.004
			(0.004)	(0.004)
Treatment: CZ × high			-0.017***	-0.017***
			(0.005)	(0.005)
County× sector controls	Yes	Yes	Yes	Yes
CZ controls	Yes	Yes	Yes	Yes
Sector FE	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes
Observations	44477	44477	44477	44477
R^2	0.047	0.074	0.048	0.074

Table 13: Employment effects at aggregated levels

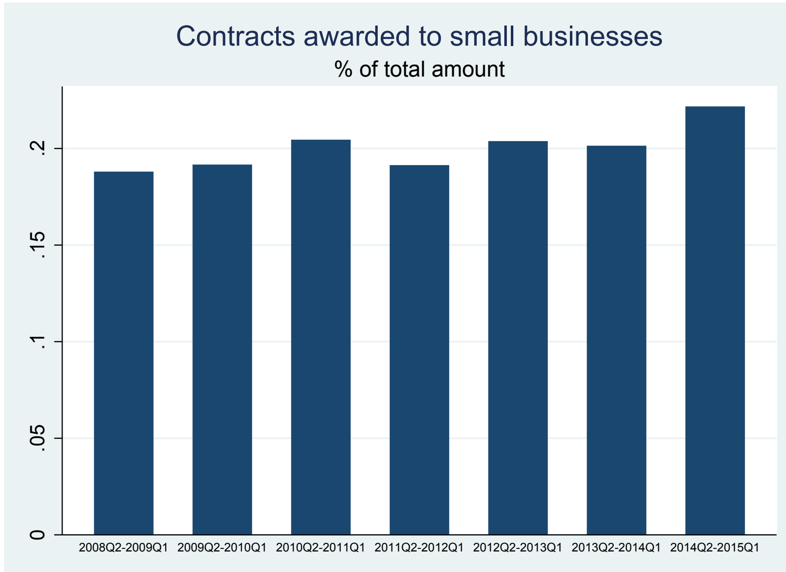
This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the commuting zone level of the change in log employment on commuting zone exposure to government contracts that were accelerated following the implementation of the federal payment reform of 2011. Treatment is the average quarterly amount of eligible government contracts to be performed in a given commuting zone between 2009Q1-2011Q1, normalized by 2011Q1 payroll. Treatment is normalized by its cross-sectional standard deviation. Control variables include the average quarterly amount of all government contracts to be performed in a given commuting zone between 2009Q1-2011Q1, normalized by 2011Q1 payroll, as well as additional commuting zone level controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. Labor market tightness is measured at the CZ level as the ratio of the number of vacancies to the number of unemployed workers in 2010. Robust standard errors presented in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

High versus low labor market tightness				
Above or below median				
	Δ log employment (2011Q1-2015Q1)			
Treatment	0.016*	0.026***		
	(0.009)	(0.009)		
Treatment × low			0.029***	0.032***
			(0.009)	(0.009)
Treatment × high			-0.002	0.010
			(0.010)	(0.011)
CZ level controls	No	Yes	No	Yes
Observations	693	693	693	693
R^2	0.008	0.245	0.048	0.255

Appendix

Can Paying Firms More Quickly Affect Employment?

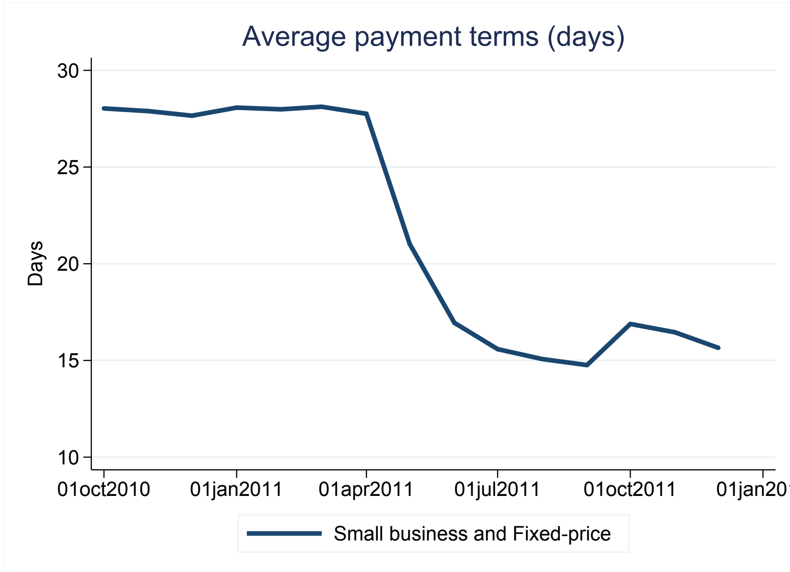
Panel A. Share of contracting dollars awarded to small businesses



Panel B. Share of contracting dollars awarded through fixed-price contracts

Figure A.1: **Trends in government contracting.** This figure shows trends in government contracting between 2009 and 2015. Panel A presents the share of total government contracts awarded to small business, on a dollar-weighted basis. Panel B presents the share of total government contract awarded through fixed-price contracts, on a dollar-weighted basis. Under fixed-price contracts, contractors agree to deliver the product or service at a pre-negotiated price. Under cost-plus contracts, contractors are paid for their expenses up to a set limit, plus profit.

Panel A. Treated government contracts



Panel B. Untreated government contracts

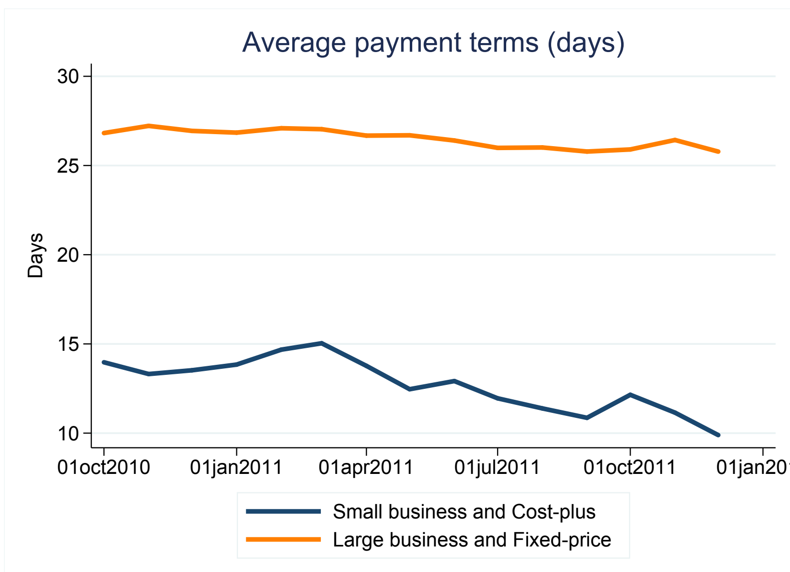


Figure A.2: **Department of Defense payment terms.** This figure shows the average number of days between receipt and payment of invoices in the MOCAS payment system of the Department of Defense. Panel A presents the difference between payments associated with contracts awarded to small versus large businesses. Panel B presents the difference between contracts awarded on fixed-price rather than a cost-plus basis. Under fixed-price contracts, contractors agree to deliver the product or service at a pre-negotiated price. Under cost-plus contracts, contractors are paid for their expenses up to a set limit, plus profit.

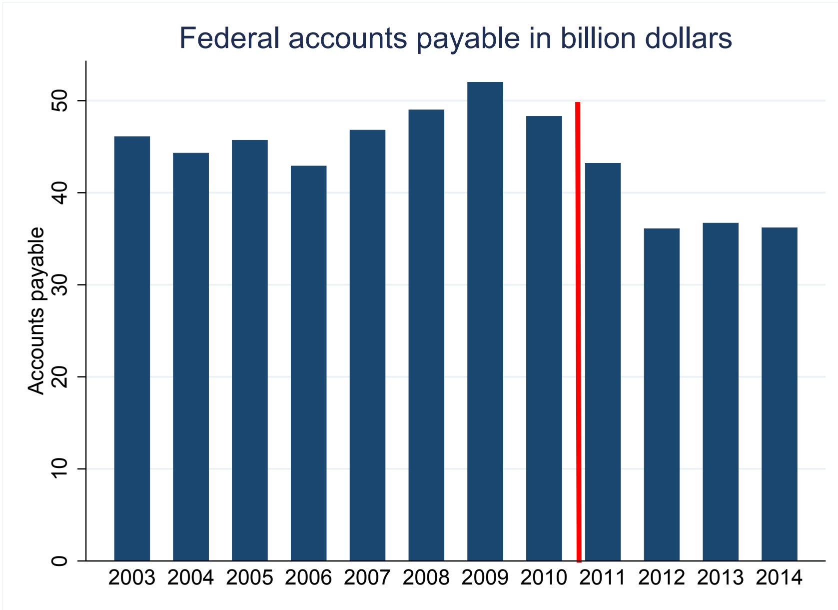


Figure A.3: **US federal accounts payable.** This figure presents total accounts payables across agencies of the US federal government for fiscal years 2003 to 2014, obtained from United States Government Notes to the Financial Statements. These aggregates exclude agencies that did not report payables consistently across the period.

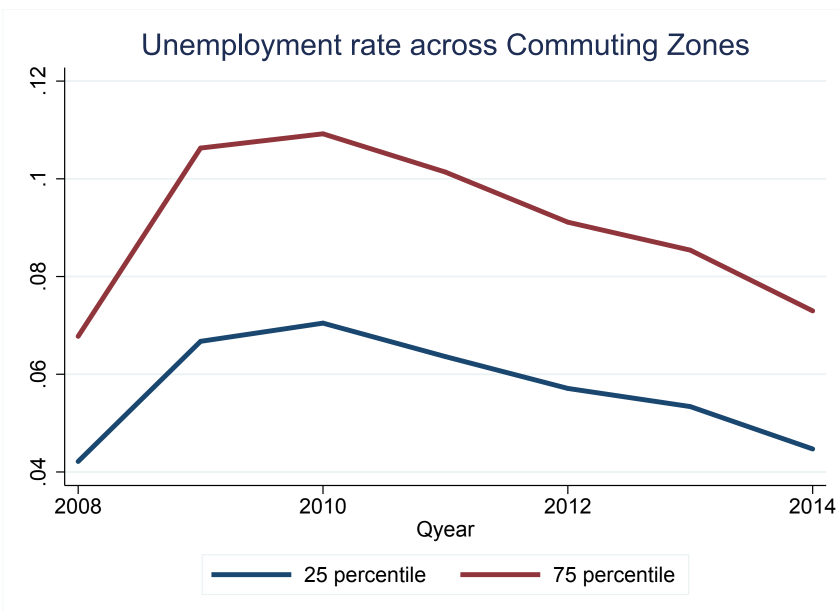


Figure A.4: **Unemployment rates across CZs .** This figure presents the 25th and 75th percentile of the distribution of unemployment rates across commuting zones in the US, from 2008 to 2014.

Table A.1: Change in government contract intensity

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the county×sector level of the change in average quarterly government contracts between the two year prior and the three years after the payment acceleration, scaled by 2011 payroll, on county×sector exposure to government contracts that were accelerated following the implementation of the federal payment reform of 2011. Treatment is the average quarterly amount of eligible government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll. Control variables include the average quarterly amount of all government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized normalized by 2011Q1 payroll, as well as additional county×sector controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. There are 3120 counties and 18 industries. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Δ Average quarterly government contract, scaled by 2011Q1 payroll				
Treatment	-0.058	-0.060	-0.062	-0.070	-0.074
	(0.112)	(0.113)	(0.113)	(0.111)	(0.112)
County×sector controls	No	Yes	Yes	Yes	Yes
Sector FE	No	No	Yes	No	Yes
County FE	No	No	No	Yes	Yes
Observations	44484	44484	44484	44484	44484
R^2	0.193	0.193	0.195	0.268	0.271

Table A.2: Distribution of procurement contracts across sectors

This table presents the cross-sector distribution of average yearly amounts of Government contracts granted between 2009 and 2015, in billion dollars. *Eligible* (non-eligible) refers to contracts that were paid in 30 days (sooner than 30 days) prior to the reform. There are 18 industries.

	Average yearly amounts 2009-2015, in billion dollars			
	All businesses	Small businesses		Large businesses
Sector		Eligible	Ineligible	
Manufacturing	135.49	15.29	0.95	119.25
Professional services	100.48	15.77	4.67	80.05
Construction	26.95	7.49	0.01	19.45
Admin	25.48	4.44	0.30	20.74
Wholesale	11.83	3.95	0.24	7.65
Finance	10.49	0.30	0.00	10.19
Transportation	8.71	1.30	0.02	7.39
Information	8.68	1.71	0.15	6.82
Health care	4.75	1.07	0.01	3.67
Education	3.63	0.73	0.02	2.88
Retail	3.02	0.99	0.23	1.80
Real estate	2.50	1.10	0.00	1.39
Other services	2.09	0.48	0.02	1.59
Utilities	1.50	0.15	0.00	1.34
Accomodation	0.79	0.18	0.01	0.60
Agriculture	0.31	0.23	0.01	0.08
Mining	0.26	0.08	0.00	0.18
Arts	0.05	0.04	0.00	0.02

Table A.3: Top and bottom 20 4-digit NAICS industries

This tabe presents the top 20 and bottom 20 4-digit NAICS industries based on treatment, measured as the average quarterly amount of eligible government contracts to be performed in a given industry between 2009Q1-2011Q1, normalized by quarterly payroll in 2011Q1 . There are 287 4-digit NAICS industries.

Rank	Naics 4	Description
1	3366	Ship and Boat Building
2	1153	Support Activities for Forestry
3	5612	Facilities Support Services
4	3162	Footwear Manufacturing
5	2379	Other Heavy and Civil Engineering Construction
6	3159	Apparel Accessories and Other Apparel Manufacturing
7	5629	Remediation and Other Waste Management Services
8	3149	Other Textile Product Mills
9	2362	Nonresidential Building Construction
10	4831	Deep Sea, Coastal, and Great Lakes Water Transportation
11	6114	Business Schools and Computer and Management Training
12	3112	Grain and Oilseed Milling
13	4812	Nonscheduled Air Transportation
14	4247	Petroleum and Petroleum Products Merchant Wholesalers
15	5311	Lessors of Real Estate
16	3169	Other Leather and Allied Product Manufacturing
17	3333	Commercial and Service Industry Machinery Manufacturing
18	3329	Other Fabricated Metal Product Manufacturing
19	5415	Computer Systems Design and Related Services
20	3325	Hardware Manufacturing
268	8132	Grantmaking and Giving Services
269	7131	Amusement Parks and Arcades
270	5331	Lessors of Nonfinancial Intangible Assets (except Copyrighted Works)
271	4453	Beer, Wine, and Liquor Stores
272	6223	Specialty (except Psychiatric and Substance Abuse) Hospitals
273	4248	Beer, Wine, and Distilled Alcoholic Beverage Merchant Wholesalers
274	4521	Department Stores
275	5511	Management of Companies and Enterprises
276	5211	Monetary Authorities-Central Bank
277	2122	Metal Ore Mining
278	4861	Pipeline Transportation of Crude Oil
279	4879	Scenic and Sightseeing Transportation, Other
280	7132	Gambling Industries
281	7224	Drinking Places (Alcoholic Beverages)
282	3161	Leather and Hide Tanning and Finishing
283	1132	Forest Nurseries and Gathering of Forest Products
284	3122	Tobacco Manufacturing
285	5232	Securities and Commodity Exchanges
286	5259	Other Investment Pools and Funds
287	4851	Urban Transit Systems
288	4869	Other Pipeline Transportation

Table A.4: Robustness: Baseline using alternative specifications

This table presents alternative specifications. Panel A presents a difference-in-difference estimation of the three year change in log payroll on the interaction of a post dummy taking the value of one for the 2011-14 period, and zero for the 2008-11 period with a treatment variable measured as the average quarterly amount of eligible government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll. Panel B presents OLS regressions of the change in log payroll on a dummy taking the value of one for county×sector with treatment above the sample median. Control variables include the average quarterly amount of all government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll, as well as additional county×sector controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the corr elation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. There are 3120 counties and 18 industries. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Panel A: two period DID, 2007-11, 2011-15				
Post × Treatment	0.378***	0.079**	0.062	0.091**	0.072*
	(0.073)	(0.040)	(0.039)	(0.041)	(0.039)
County×sector controls	Yes	Yes	Yes	Yes	Yes
County×sector FE	Yes	Yes	Yes	Yes	Yes
Sector×year FE	No	No	Yes	No	Yes
County×year FE	No	No	No	Yes	Yes
Observations	90352	90352	90352	90382	90382
R^2	0.460	0.799	0.803	0.822	0.826
	Panel B: dummy for high versus low treatment				
High treatment	0.011**	0.029***	0.018***	0.040***	0.018***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
County×sector controls	No	Yes	Yes	Yes	Yes
Sector FE	No	No	Yes	No	Yes
County FE	No	No	No	Yes	Yes
Observations	44484	44484	44484	44484	44484
R^2	0.001	0.049	0.066	0.160	0.184
	Panel C: controlling for the share of treated contracts				
Treatment	0.141***	0.077**	0.075**	0.078**	0.067*
	(0.032)	(0.030)	(0.030)	(0.035)	(0.034)
Share of treated contracts	0.004	-0.001	-0.001	-0.001	-0.000
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Government contracts	-0.014	-0.012	-0.017	0.011	-0.001
	(0.013)	(0.012)	(0.012)	(0.013)	(0.013)
County×sector controls	No	Yes	Yes	Yes	Yes
Sector FE	No	No	Yes	No	Yes
County FE	No	No	No	Yes	Yes
Observations	23745	23745	23745	23745	23745
R^2	0.004	0.056	0.072	0.256	0.277

Table A.5: Robustness: Baseline results using 2011Q2-2015Q1 government contracts

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the county×sector level of the change in log payroll on county×sector exposure to government contracts that were accelerated following the implementation of the federal payment reform of 2011. Treatment is the average quarterly amount of eligible government contracts to be performed in a given county×sector between 2011Q1-2015Q1, normalized by 2011Q1 payroll. Control variables include the average quarterly amount of all government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll, as well as additional county×sector controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. There are 3120 counties and 18 industries. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Panel A: Δ log payroll (2011Q1-2015Q1)				
Treatment (realized)	0.181***	0.122***	0.096***	0.141***	0.091***
	(0.033)	(0.032)	(0.032)	(0.035)	(0.034)
County×sector controls	No	Yes	Yes	Yes	Yes
Sector FE	No	No	Yes	No	Yes
County FE	No	No	No	Yes	Yes
Observations	44438	44438	44438	44438	44438
R^2	0.001	0.048	0.066	0.159	0.184
hline					
	Panel B: Δ log employment (2011Q1-2015Q1)				
Treatment (realized)	0.117***	0.090***	0.066**	0.107***	0.062**
	(0.029)	(0.028)	(0.028)	(0.031)	(0.030)
County×sector controls	No	Yes	Yes	Yes	Yes
Sector FE	No	No	Yes	No	Yes
County FE	No	No	No	Yes	Yes
Observations	44438	44438	44438	44438	44438
R^2	0.001	0.034	0.061	0.140	0.174
	Panel C: Δ log earnings (2011Q1-2015Q1)				
Treatment (realized)	0.064***	0.032**	0.029**	0.033**	0.029*
	(0.016)	(0.015)	(0.014)	(0.016)	(0.015)
County×sector controls	No	Yes	Yes	Yes	Yes
Sector FE	No	No	Yes	No	Yes
County FE	No	No	No	Yes	Yes
Observations	44438	44438	44438	44438	44438
R^2	0.001	0.094	0.164	0.171	0.244

Table A.6: Robustness: Baseline results, controlling for SBA loan intensity

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the county×sector level of the change in log payroll on county×sector exposure to government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment* is the average quarterly amount of eligible government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll. Control variables include the average quarterly amount of all government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll, as well as additional county×sector controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. There are 3120 counties and 18 industries.Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Δ Log payroll (2011Q1-2015Q1)				
	Controlling for SBA loans given in				
	2009-10	2009-11	2009-12	2009-13	2009-14
Treatment	0.070**	0.069**	0.069**	0.070**	0.069**
	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)
SBA loans over payrolls	0.001	0.002*	0.002***	0.001*	0.001
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
County×sector controls	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Observations	44484	44484	44484	44484	44484
R^2	0.184	0.185	0.185	0.185	0.184

Table A.7: Robustness: County×four-digit sector

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the county×4-digit sector level of the change in log payroll on county×sector exposure to government contracts that were accelerated following the implementation of the federal payment reform of 2011. Treatment is the average quarterly amount of eligible government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll. Control variables include the average quarterly amount of all government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll, as well as additional county×sector controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. There are 3120 counties and 18 industries. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Δ log payroll (2011Q1-2015Q1)				
Treatment	0.078***	0.054***	0.053***	0.049**	0.035*
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
County×sector controls	No	Yes	Yes	Yes	Yes
Sector (4 digit) FE	No	No	Yes	No	Yes
County FE	No	No	No	Yes	Yes
Observations	194209	194209	194209	194209	194209
R^2	0.000	0.024	0.068	0.059	0.121

Table A.8: Robustness: Firm-level regressions

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the firm level of the change in log employment on county×sector exposure to government contracts that were accelerated following the implementation of the federal payment reform of 2011. Treatment is measured as the average quarterly amount of government contracts awarded to the plant between 2009Q1-2011Q1, normalized by 2011Q1 employment. All regressions control for the average quarterly amount of all government contracts awarded to the plant between 2009Q1-2011Q1, normalized by 2011Q1 employment. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Δ log employment (2011Q1-2014Q1)				
Treatment	0.780***	0.780***	0.788***	0.781***	0.792***
	(0.210)	(0.210)	(0.210)	(0.210)	(0.235)
Sector (6 digit)	No	Yes	No	Yes	No
County FE	No	No	Yes	Yes	No
Sector (6 digit) × County FE	No	No	No	No	Yes
Observations	8458477	8458477	8458477	8458477	8458477
R^2	0.004	0.004	0.003	0.005	0.132

Table A.9: Employment growth: job creations and destructions

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the county× two-digit sector level of average quarterly job creations and job destruction rates on county×sector exposure to government contracts that were accelerated following the implementation of the federal payment reform of 2011. Treatment is the average quarterly amount of eligible government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll. Control variables include the average quarterly amount of all government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll, as well as additional county×sector controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. There are 3120 counties and 18 industries. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Average quarterly rate (2011Q2-2015Q1)	
	Job creations	Job destructions
Treatment	0.009***	0.002
	(0.003)	(0.002)
County×sector controls	Yes	Yes
Sector FE	Yes	Yes
County FE	Yes	Yes
Observations	44484	44484
R^2	0.368	0.326

Table A.10: Spillover effect of payment acceleration: tradable and non tradables

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the county×sector level of the change in log payroll on county×sector exposure to government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment* is the average quarterly amount of eligible government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll. *Treatment: CZ* is the same variable, measured at the commuting zone level rather than the county×sector level, and excluding the focal county×sector. *Treatment* and *Treatment: CZ* are normalized by their cross-sectional standard deviation. County×sector control variables include the average quarterly amount of all government contracts to be performed in a given county×sector between 2009Q1-2011Q1, normalized by 2011Q1 payroll, as well as additional county×sector controls including the share of small establishment, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation of employment growth with aggregate employment growth, the log of total employment and the log average earnings. CZ controls include the unemployment rate, the share of small establishments, the log average establishment size, log total employment and log average earnings in 2011Q1. Non-tradable industries include health care, hospitality, food service, education, retail, and construction. There are 3120 counties and 18 industries. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Δ log employment (2011Q1-2015Q1)	
Treatment: CZ × Non tradable	-0.001	-0.002
	(0.004)	(0.004)
Treatment: CZ × Tradable	-0.010***	-0.010**
	(0.004)	(0.004)
County× sector controls	Yes	Yes
CZ controls	Yes	Yes
Sector FE	No	Yes
State FE	Yes	Yes
Observations	44484	44484
R^2	0.049	0.074