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Abstract

We study the impact of *Quickpay*, a federal reform that indefinitely accelerated payments to small business contractors of the U.S. government. We find a strong direct effect of the reform on employment growth at the firm-level. 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 crowding out was 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.

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1 Introduction

The role of financing frictions in impacting employment has received substantial interest following the 2008 financial crisis and subsequent slow recovery, when the contraction in credit supplied by financial intermediaries to non-financial firms has been argued to have had a substantial impact on the real economy and particularly so on smaller firms (Ivashina and Scharfstein 2010; Iyer et al. 2014; Paravisini et al. 2015). 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).

Beyond facilitating access to bank credit, a 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, complementarity 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.

We study the impact of faster payment on firm-level employment in the context of the federal *Quickpay* reform of 2011. *Quickpay* indefinitely accelerated payments to a subset of small business contractors of the U.S. federal government, cutting the time taken between invoice approval and payment by half, from 30 to 15 days. For treated firms, the reform therefore permanently reduces the working capital needed to sustain a dollar of sales with the government. $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 effects of the *Quickpay* reform in two steps. We first estimate the direct effect of this policy using the National Employment Time Series (NETS) dataset, which has establishment-level panel data drawn from the Dunn & Bradstreet (D&B) registers. Our establishment-level estimations provide strong evidence of greater employment growth in treated firms after the reform, an increase that remained statistically significant and economically meaningful for at least 3 years after the reform (which is when our establishment-level data end). In addition, and consistent with a shorter 'cash conversion cycle'[1] driving employment growth, we document that treated firms also begin paying their own suppliers in a more reliable manner, leading to improvements in their own payment-related credit score within the Dunn & Bradstreet registers. Placebo regressions looking at government contractors who are not exposed to the treatment show no such employment effect or improved credit score, providing reassurance that our results are not driven by unobserved heterogeneity related to being a government contractor. The magnitude of the employment effects we estimate from our establishment-level regressions using NETS data mirror those using the more comprehensive public use Census data at the county-by-sector level; based on the elasticity of the employment response, we estimate that the implied cost of external finance for treated firms is approximately 40%, 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 then aggregate the results up to the level of local labor markets, to study whether these establishment-level results flow through to increases in aggregate employment measured at the commuting-zone level. We find that aggregate employment increases, but only in areas where unemployment is high relative to the number of vacancies. In tight labor markets, where vacancies are high relative to unemployment, we find no increase in aggregate employment. These findings suggest the presence of *negative spillovers* in tight labor markets, where the employment growth among treated firms comes at the expense of those who do not benefit from the improvement in cash flows stemming from faster payment. We then document the presence of these negative spillovers, and provide direct evidence of employment flows from low to high treatment sectors.

Taken together, our results document substantial financing frictions facing the small businesses in our sample, but also highlight the importance of accounting for equilibrium

[1]The cash conversion cycle refers to the number of days of working capital that 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.

effects when studying the real effects of large-scale reforms aimed at relaxing firm-level financing constraints.

Our results are related to several strands of the literature. First, our work contributes to the literature on financing constraints among small, private firms, that account for a substantial portion of employment and output, but have received relatively less attention due to the paucity of data on their financing. In particular, our findings point to important constraints on working capital finance for such businesses, 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 – many of which are small businesses – effectively provide short-term corporate financing to their - often large business – 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 U.S.[2] 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 has found that long payment terms force financially constrained firms to cut back investment (Murfin and Njoroge 2014) and expose them to liquidity risk (Barrot 2015).[3] Our work, which is based on a broad set of industries and firms across the U.S., shows that trade credit provision also constrains employment growth, even when the debtor is a low-risk customer such as the federal government.[4] In addition, our ability to link the accelerated contract value to the increase in employment provides us with a unique ability to quantify the size of the financing frictions faced by small business suppliers in the U.S. Our estimates suggest reasonably large financing frictions, which are interesting in light of

[2]As of September 2012, according to the U.S. Flow of Funds Accounts.

[3]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).

[4]In practice, there are also impediments in the pledgeability of government trade credit claims. We provide more details below.

other evidence highlighting how commercial banking legislation passed in the wake of the financial crisis may have inadvertently led to a disproportionate decline in small business lending (Bordo and Duca 2018).

Our work also contributes to a growing stream of research focusing on the relationship between financial frictions and employment.[5] Benmelech et al. (2014) show that firm-level employment responds to bank deregulation and bank balance sheet shocks. Chodorow-Reich (2014) finds an employment 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.[6] 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.[7]

Finally, our findings build on the literature assessing the role of policy intervention targeting businesses in the U.S., 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

[5]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).

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

[7]In discussing the aggregate relationship between labor and credit market frictions, we relate to work by Wasmer and Weil (2004), Petrosky-Nadeau and Wasmer (2013) or Hall (2017).

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 present a simple theoretical framework 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 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;

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

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 need to grow, 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.[9]

In the absence of financing frictions, a firm can 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.[10] 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 million of sales being paid 30 days after delivering its product. For simplicity, assume the firm can only grow through internal cash flow and is currently constrained from growing because it is at cash flow breakeven.[11] 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

[9]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).

[10]An alternative is to turn to factoring companies, who buy accounts receivable in exchange for cash upfront. In practice, however, the negative stigma associated with factoring companies 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.

[11]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.

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. Hence seemingly small improvements in the working capital position for constrained firms can have consequential effects for growth in sales and in payroll.[12]

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.[13] Both sets of firms $i \in (t, u)$ have the same decreasing returns to scale technology in labor:

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

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 A is 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:

[12] 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.

[13] 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.

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

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 tests we present below provide an estimate of the employment response $\frac{L^*_{i,2}}{L^*_{i,1}}$, thus allowing us to infer R. Our findings therefore shed light on the intensity of financing constraints facing firms in the U.S. at the time of the *Quickpay* reform that we describe next.

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).

In 2011, U.S. 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 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.

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.[18] The underlying idea was that "small businesses are the primary engine of job creation and job growth". Accelerating payments was intended to allow them to "reinvest that money in the economy and drive job growth".

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.[19] The con-

[14] Chapter 39 of title 31 of the United States Code
[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] Getting Money to Small Businesses Faster, White House Press Release, 2011
[19] For more details on these thresholds, see https://www.sba.gov/content/small-business-size-standards

tracting 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.[20] 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.[21] Appendix Figure A.1, Panel B, shows that the share of total government spending awarded through 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.[22] Finally, the Department of Defense also paid disadvantaged small business contractors earlier prior to the implementation of *QuickPay*.[23] 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. This heterogeneity across contract types' exposure to the reform allows us to tightly identify the effect of payment acceleration on labor market outcomes. We discuss the specifics of our identification strategy in Section 4.

[20]See Subpart 32.9 (Prompt Payment) of the Federal Acquisition Regulation.

[21]For further analyses of these two contract types, see Horton (2008), for instance.

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

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

3 Data

We combine a number of datasets to facilitate our analysis. First, we use the Federal Procurement Data System (FPDS) to identify firms that benefited from the *Quickpay* reform. The Federal Funding Accountability and Transparency Act of 2006 required the Office of Management and Budget to maintain a public website describing each federal award in great detail, 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 NAICS sector, whether the contractor is a small business or not, and the zip code where the contract is to be performed.[24] For the establishment-level analyses, these data allow us to create an indicator for whether or not the establishment was a government contractor and further, whether that establishment was treated by the *Quickpay* reform. For the analysis at the county-by-sector and commuting zone level, we go further and create a county-sector level measure of exposure to treatment, based on the average total 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 in these latter estimations, 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.[25]

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 Adminis-

[24] 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.

[25] See Appendix Figure A.4 for the distribution of this treatment variable across U.S. counties, and Appendix Tables A.4, A.5, and A.6 for its distribution across sectors.

tration 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. 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 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.[26] Given that the MOCAS system is the main payment system for the Department of Defense, which itself is the agency comprising the largest share of government procurement, these figures not only validate that the treatment was implemented as outlined in the language of the reform, but also provide direct evidence of a sharp and unanticipated fall in the timing of payment (given that payment was accelerated some months prior to the public announcement of this reform by President Obama).

Our core dataset to study establishment-level outcomes is the National Employment Time Series (NETS) dataset, which is an establishment-level panel dataset based on Dunn & Bradstreet credit registry data. Created and maintained by Walls and Associates, these data are made available to researchers for a fee. We use the 2014 version of these data, the latest version available to researchers at the time of analysis. NETS data cover both employer and non-employer businesses. Every government contractor is required to register with Dunn & Bradstreet and receive a D&B ID to be eligible to bid for contracts. This makes it possible to match government contractors to establishments in the micro

[26] Fiscal year ends on September 30.

data. NETS data also include a measure of a firm's credit worthiness, known as the the Paydex score, which is developed by D&B using information from a firm's suppliers on how promptly they are paid. Since improvements in this measure are a direct indication of improvements in a firms corporate liquidity, it allows us to analyze the effect of the reform on treated firms' cash flows. In addition to NETS data, we also got access to micro-data on loan delinquencies from a fintech company that collects such information from banks it partners with. While not as comprehensive as the NETS data due to a limited set of bank partners, the fintech company got data from, we use this measure of loan delinquency as another outcome variable of interest.

For the county-sector and commuting zone analyses, we use publicly available data from the U.S. Census Quarterly Workforce Indicators (QWI).[27] QWI, which is based on micro data from the Longitudinal Employer Household Dynamics program (LEHD), allows us to measure labor market outcomes at the level of local labor markets. For each two-digit sector[28] in each county, we obtain quarterly payrolls, employment and average earnings per worker.[29] 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. Finally, 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.

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

[27]Adelino et al. (2017) also use QWI to study the local response of new firm growth to industry-level shocks.

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

[29]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.

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.

Table 1 provides descriptive statistics for our main variables. Panel A provides establishment-level descriptive statistics and Panel B provides descriptive statistics at the county-by-sector level.

4 Results

4.1 The Direct Effect of Accelerated Payments

4.1.1 Establishment-Level Estimations

We start by establishing the direct effect of the reform on treated establishments using NETS data. To do so, we proceed with the following difference-in-differences specification at the establishment-year level:

$$Y_{it} = \alpha + \beta_1.Treatment_i.post_t + \beta_2.X_{it} + \theta_i + \gamma_{st} + \lambda_{ct} + \epsilon_{it} \tag{10}$$

where Y_{it} is either the log employment, or the payment-related credit score, measured in establishment i at date $t = \{2011, 2014\}$. $Treatment_i$ is an indicator that takes a value of 1 for establishments that received eligible contracts in the two years prior to the reform, and $post_t$ is an indicator that takes a value of 1 for observations in the post-reform period. To control for unobserved heterogeneity, we include fixed effects at the establishment level (θ_i), industrial (6-digit) sector-by-year level (γ_{st}), and county-by-year level (λ_{ct}). X_{it} is a set of time-varying establishment-level controls.

As in Card (1992) and Angrist and Pischke (2008), we collapse equation 10 into the following equation in first-differences:

$$\Delta Y_i = \beta_1.Treatment_i + \beta_2.X_i^{'} + \gamma_s^{'} + \lambda_c^{'} + \epsilon_i^{'} \tag{11}$$

where ΔY_i is now the *change* in log employment or the *change* in the payment-related credit score from 2011Q1 to 2014Q1. X_i includes a dummy for whether or not the establishment received any government contracts (accelerated or not) in the two years prior to the reform, the *change* in log employment or the *change* in the Payment-related credit score from 2008Q1 to 2011, the log of employment and the payment-related credit score in 2011Q1, and the age of the establishment in 2011Q1. There are 3120 counties and 1051 6-digit sectors. The coefficient of interest is β_1, that measures the effect of the reform on the dependent variable. The identifying assumption, which is analogous to the parallel trends assumption, is that conditional on controls, treatment is orthogonal to changes in the credit scores or employment for the control group.

We report the results from equation (11) in Table 2. Panel A reports results where ΔY_i is the change in the payment-related credit Score and Panel B reports results where ΔY_i is the change in log employment. As can be seen from the table, the coefficient of interest, β_1 is positive and statistically significant, which indicates that treated firms improved their payment-related credit score, and increased employment after the payment acceleration reform.

Despite controls in equation (11), one may be worried that treated firms cluster in certain 6-digit NAICS sectors or counties that are sensitive to the business cycle in a way that might drive their employment and payment behavior after the reform. Yet the introduction of 6-digit NAICS dummies (column (2)) and county dummies (column (3)) does not affect the coefficient. Furthermore, the specification presented in column (5) includes county $\times$ 6-digit sector dummies, and thereby compares treated and control firms in the same narrowly defined industry and location. If anything the coefficient goes up slightly, which indicates that unobserved heterogeneity is unlikely to spuriously boost our estimates.

The magnitude of these effects are substantial. In Panel A, the coefficient on β_1 is approximately 0.5, which compares to an average change in payment-related credit score of -1. Treatment therefore leads to an increase by 4% of a standard deviation in the change in payment score. In Panel B, the coefficient is 0.017, indicating that treated firms increase employment by 1.7% more than control firms, which compares with an average employment growth of 0.4$ over the sample period. Treatment therefore leads to an increase by 10.4% of a standard deviation in employment growth.

4.1.2 Dynamics and Falsification Tests

In Table 3, we study the timing of the effects measured in Table 2. We re-run the most stringent specification in Table 2, that is the specification in column (5), for different long-differences running from 2009-2011 through to 2011-2014. The last column in Table 3 is identical to the last column in Table 2 while other columns show potential pre-trends and the progression in the size of the effects over time. The results in Table 3 show that for both outcome variables, there is no measurable pre-trend and that the parallel trend assumption is likely to be satisfied.

Furthermore, it is interesting to note that payment scores respond earlier than employment to the reform. The coefficient on the treatment variable for the payment-related credit score is statistically different from zero as soon as 2012, while it is statistically different from zero by 2103 for employment growth. To interpret these timings more specifically, we note that NETS data are measured as of January of each year. Given that acceleration of payment through the *Quickpay* reform took place between April and September of 2011, this means that the response in credit scores is virtually immediate given that the D&B measure is taken from creditor reports which would also need to materially change for the score to be updated. Since we measure employment in NETS every 12 months, the NETS data suggest that the change in credit scores took place within the first 6 months, while the bulk of the employment effect took place between 6 and 18 months following the reform. It is reassuring to see that the payment score response leads the employment response, since the former is a mechanical outcome of the reform, and the latter involves active decision making by the firm.

Despite of the absence of pre-trends, one might nevertheless be concerned about threats to identification. For example, the *Quickpay* 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. 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.

Furthermore, one might be concerned the results are related to unobserved heterogeneity in the types of firms that become government contractors, rather than being driven by the specific channel we are studying. To address this concern, we exploit the fact that only a subset of government contracts were eligible for accelerated payment through the *Quickpay* reform. Government contractors who were not eligible to have their payment accelerated should not see any change in their outcomes. We run falsification tests in Table 4, where we separately include an indicator of being a treated establishment as before, but now separately include an indicator for being an establishment that did have government contracts, but for whom payment was not accelerated.[30] Panel A of Table 4

[30]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 wonder whether the drop in prices could offset the

considers changes in payment-related credit scores and Panel B looks at changes in log employment. Across specifications, while the coefficient on treated establishments continues to remain statistically significant, the coefficient on placebo establishments is small and indistinguishable from zero. Moreover, tests for the difference between the treatment and placebo coefficients reject that they are equal, thus providing greater reassurance that the findings are not spuriously driven by unobserved characteristics of firms receiving government contracts.

Appendix tables A.1 and A.2 document two corroborating sets of results. Table A.1, shows that treated firms were less likely to be delinquent on their loans. Table A.2 shows that treated establishments are less likely to exit. Together, these results paint a consistent picture that the acceleration of payment through *Quickpay*, despite being a 'mere 15 days', had consequential real effects. Firms experienced an improvement in cash flow, which enabled them to pay their own suppliers faster and improve their credit scores. The reform lowered the likelihood of them being delinquent on loan payments and reduced the likelihood that they would fail. It also enabled them to grow their businesses, as evidenced by the higher employment growth following the reform among firms that were treated.

4.2 Magnitudes and Implied Financing Frictions

Having established that *Quickpay* had a direct effect on treated firms, we next turn to quantifying the size of financing frictions facing these small businesses. As noted in

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. 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. 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*.

Section 2 above, an attractive feature of our setting is that we can map our reduced form estimates to the simple model outlined in Section 2, and thereby infer the cost of financing.

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. 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 ($\theta = 0.5$).

We exploit our reduced form estimates to calibrate employment growth. The model assumes that 100% of a firm's receivables were accelerated. In practice of course, the accelerated contract value is substantially less, which means that the coefficients from our regressions will be too small relative to a situation were 100% of all their sales were subject to a payment acceleration. NETS provides sales data for a limited subset of establishment. Using the value of government contracts for treated firms available from FPDS data, we find that the median ratio of government sales to total sales is 8.5% for treated establishments. We therefore assume that a firm with 100% of accelerated sales would have increased employment by 1.7%/8.5% = 20%. Using this value to calibrate employment growth in the model, we finally infer that the corresponding model implied values for R is 0.45, as can be seen from Table 5.

We complement this approach using Census data. Specifically, we re-run equation (11) at the county-by-sector level instead of the establishment-level. We consider the following OLS regression in the cross-section of county- (two-digit) sectors:

$$\Delta LogY_{sc} = \beta_1.Treatment_{sc} + \beta_2.X'_{sc} + \gamma'_s + \lambda'_c + \epsilon'_{sc} \tag{12}$$

where $\Delta LogY_{sc}$ is the change in log payroll, log employment and log earnings from 2011Q1 to 2015Q1. The set of controls, X'_{sc}, includes the total average quarterly amount of government contracts (accelerated or not) at the county-sector level normalized by 2011Q1 payroll, as well as the unemployment rate, correlation of employment growth with U.S. 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.

Instead of an indicator variable that takes a value of 1 if an establishment benefited from the reform, $Treatment_{sc}$ is now 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. We now have a continuous measure of treatment rather than a binary measure, so that our identification comes from a variation in the degree of exposure to treated contracts. While not measured at the establishment-level, this measure has the attractive property that the 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}).

We first estimate the effect of the payment acceleration on payroll. Table 6 presents the result of this estimation. 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 2). Columns (3)-(6) decompose the payroll effect into the part stemming from increases in employment (0.057) and the part stemming from increases in earnings (0.012), although the increase in earnings is statistically indistinguishable from zero. 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 and a 0.8% increase in employment. 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.[31]

Here again, to map our estimates to the model, we need to first extrapolate the employment response of a firm with 100% of its sales affected by the reform. Table 6 provides the employment 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 (Panel A, column (4)). Given that the average ratio of payroll to 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, which is remarkably close from the 20% employment growth obtained from our establishment-level analysis. As can be seen from Table 5, this consistent with a cost of external finance of 40% annually.[32]

[31]We provide several robustness tests for this result in the Appendix. We show that the magnitudes are unchanged when we run a differences-in-differences specification rather than a first-differences specification (Appendix Table A.8), when we restrict the sample to county*sectors with positive government contracts and positive treatment (Appendix Table A.9), when we use dummies for treatment rather than a continuous treatment variable (Appendix Table A.10), when we measure 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 (Appendix Table A.11, when we control for the amount of loans granted by the Small Business Administration (Appendix Table A.12), or when we run our tests at the county-by-4-digit sector level to include tighter industry fixed effects (Appendix Table A.13). Moreover, we find in Appendix Table A.14 that the employment response is stronger in county*sectors facing tighter financing constraints. There are no prior trends in the payroll response (Appendix TableA.15), and falsification tests show that only exposure to eligible government contracts (rather than any government contracts) drive the results.

[32]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 receiv-

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 ("2/10-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).[33]

4.3 Local Labor Market Analysis

4.3.1 Commuting-zone-level Tests

Our analysis so far has documented substantial responses in creditworthiness, delinquencies, and employment to treatment, consistent with financing frictions facing the treated firms in our sample. Since the stated objective of the reform was to stimulate employment growth, we next ask whether and how these results aggregate to the labor market where these firms operate. The model presented in section 2 makes clear predictions about aggregate employment growth. The reform relaxes the financing friction of treated firms

ables from the computation of the borrowing base, unless these receivables have been properly assigned. 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". 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 or for failure to obtain necessary budgeting.

[33] One can similarly estimate the implied cash-flow elasticity of payroll 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 7 cents in additional payroll that is generated with that accelerated dollar. The implicit elasticity of 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.

whose labor demand therefore increases. However, the increased labor demand pushes up wages, thereby mitigating the employment growth at both treated and untreated firms. This mitigating effect depends on the elasticity of labor supply. Intuitively, if the supply of workers adjusts to the increased labor demand of treated firms, wages respond less, and equilibrium employment responds more. The overall effect of the reform therefore depends on this elasticity, and on the share of treated and untreated firms.

To analyze the effect that the reform had on local labor markets across the U.S., we study the employment response across commuting zones. There are a total of 709 commuting zones that cover the entire land area of the U.S. and represent labor market clusters of U.S. counties. In Table 7, we run equation (12) at the commuting zone, rather than the county-sector level.[34] We find the effect of treatment is positive and statistically distinguishable from zero even at the local labor market level (Columns 1 and 2).

We then proxy for the labor supply elasticity with local labor market tightness, defined as the ratio of vacancies to unemployed workers. Intuitively, in high unemployment areas (slack labor markets), the labor demand of treated firms will be met by the labor supply of the unemployed workers, have less of an effect on wages, and lead to a positive effect of the reform at the local labor market level. To check whether this is the case, we interact our treatment variable with dummies for high an low labor market tightness. The results in columns (3) and (4) show that the results obtained in columns (1) and (2) are driven by slack labor markets (where vacancies are high relative to unemployment). We find little or no effect on employment in tight labor markets, thereby confirming the presence of negative spillover effects that offset the direct effects of the reform.

[34] All controls are defined at the commuting zone level. We standardize treatment variable by its cross-sectional standard deviations.

4.3.2 Testing for Spillovers

We next directly test for the presence and the direction of spillovers. To do so, we augment our baseline specifications at the county-sector level reported in Table 6 with a measure of total accelerated dollars at the commuting zone level. More precisely, we construct the variable *Treatment: CZ* defined as the average quarterly amount of eligible government contracts in the county-sector's commuting zone between 2009Q1-2011Q1, but excluding the focal county-sector. We normalize this measure 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 impact of treatment on *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 is negative if spillovers 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. As for the direct effect, we include the total average quarterly amount of government contracts (accelerated or not) at the commuting-zone level normalized by 2011Q1 payroll. 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.[35]

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

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

with a drop by 1% in payrolls and 0.8% in employment. Little or no effect is found on earnings.[36] This is consistent with the payment acceleration having substantial crowding out effect on the employment decisions of firms in the neighborhood of treated firms.

Key to the identification of the coefficient on $Treatment : CZ$ is the parallel trend assumption. In the absence of the reform, there should be no difference in the employment behaviors of firms as a function of treatment. While this assumption cannot be formally tested, we can check whether there is any differential trends *prior* to the implementation of *Quickpay*. In Table 9, we run the same regressions as those in Table 8 over several windows surrounding the implementation of the reform. We fail to find any pre-trends in employment growth, neither in terms of direct effects and nor in terms of spillovers. No effects are picked up prior to the payment acceleration reform. The direct effect of the reform becomes statistically significant in 2012, and the spillover effects in 2013. In Panel B of Figure 1, we also run the specification presented in Table 9, 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 employment 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. Taken together, these tests largely attenuate the concern that the parallel trends assumption might not be satisfied.

We provide several robustness tests for our estimates of spillovers in the Appendix. First, we show in Table A.17 that spillovers are observed in the establishment sample, and that they also show up with a lag relative to the direct effects. We run falsification tests in Appendix Table A.18 and find that the negative employment response is only found in commuting zones with a large exposure to government contractors that were eligible to the payment acceleration. Conversely, the presence of non-eligible government contractors

[36] Changes in earnings include both changes in wages and changes in hours worked. While wages are likely to increase with the presence of treated firms, hours should should decrease, like employment.

does not affect employment.[37] Taken together, theses analyses 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.

The model presented in section 2 suggests that negative spillovers should concentrate in tight local labor markets. We therefore segment again the commuting zones in our sample into those with relatively high and low labor market tightness, measured with the ratio of the number of vacancies to the number of unemployed workers in 2010.[38] Table 10 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, the presence of treated firms negatively affects the employment growth of local firms. In other words, there is substantial crowding out of non-treated firms employment.

Finally, we directly identify the crowding out effect of treatment, with recently released data on job-to-job flows from the Longitudinal Employer-Household Dynamics database that includes 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 as follows:

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

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

[37]The coefficient on the placebo variable is not statistically different from zero and is positive, although it is not statistically different from the treatment variable at conventional levels.

[38]labor market tightness is strongly negatively related with unemployment rates and our results are unchanged when we use unemployment rates instead.

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 treatment intensity between the destination and origin sectors and shows that the difference strongly predicts job-flows. Hence, the reform clearly led to a reallocation of labor from low to high treatment sectors. 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.

5 Conclusion

In this paper we analyze the impact of the *Quickpay* reform of 2011. We show that despite being paid only 15 days sooner, a fall in the need to finance working capital through the production process has substantial effects on employment. We trace the effect of this improved corporate liquidity to a greater consistency in paying their own suppliers, a fall in loan delinquencies, a lower likelihood of firm failure and greater employment growth relative to firms that did not benefit from the reform. A unique element of our setting is that we can precisely measure the dollar value of accelerated payments, which together with the size of the employment response allows us to estimate the size of financing frictions facing the firms in our sample. We estimate an implied cost of external finance of 40%.

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 can 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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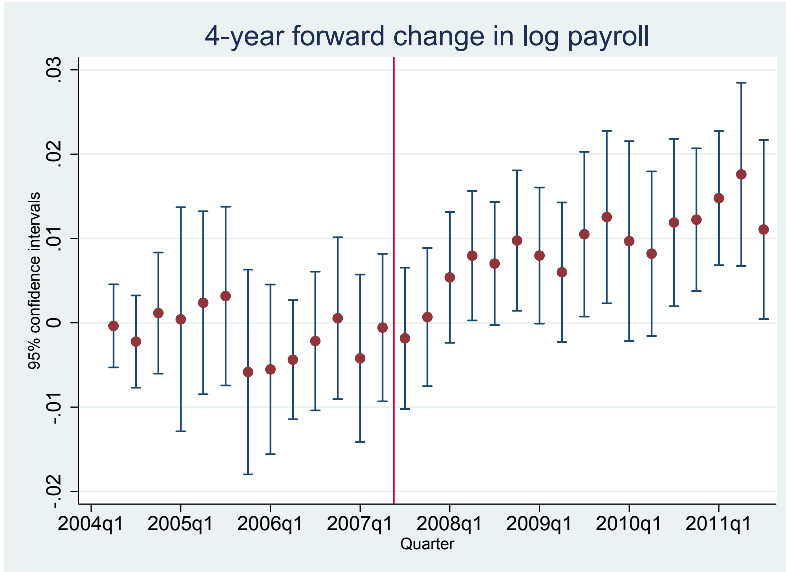
Panel A. Direct effects



Panel B. Spillover effects

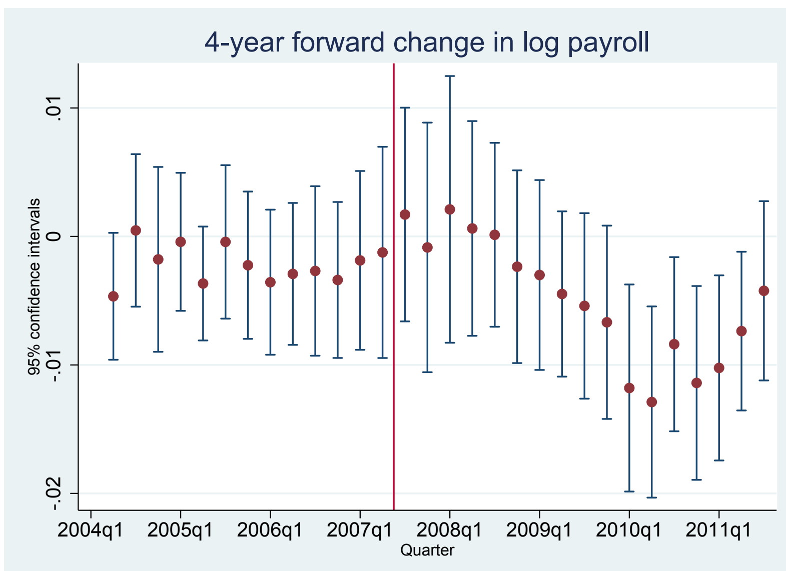


Figure 1: **Direct and spillover effects.** This figure shows the direct effects (Panel A) and spillover effects (Panel B) of payment acceleration on 4-year forward change in log payroll. In each quarter from 2004 to 2011, we measure the direct effect by running a regression at the county×sector level 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: plant level sample				
	Obs.	Mean	Sd	Median
Treatment (dummy)	8943717	0.001	0.036	0.000
Government contractor (dummy)	8943717	0.002	0.042	0.000
2011 payment score	5962341	73.950	13.793	80.000
Δ payment score	3998159	-1.093	12.584	0.000
Log employment	8943717	1.320	1.239	1.099
Δ log employment	7129157	0.004	0.163	0.000
Panel B: county×sector level sample				
	Obs.	Mean	Sd	Median
Treatment	44499	0.022	0.132	0.000
Government contracts	44499	0.053	0.249	0.000
Δ log payrolls	44382	0.139	0.381	0.129
Δ log employment	44382	0.036	0.314	0.034
Δ log earnings	44498	0.103	0.196	0.096
Unemployment rate	44499	9.492	3.018	9.300
Corr with US emp growth	44499	0.120	0.275	0.122
Average establishment size	44499	2.227	0.868	2.191
Share of small establishments	44499	0.994	0.024	1.000
Emp share of small establishments	44499	0.825	0.261	1.000
Long term employment growth	44499	0.063	0.141	0.019
Employment	44499	2237.510	9810.074	266.000
Annualized earnings ('000)	44499	32.599	19.402	28.692
Annualized payrolls ('000)	44499	101502.811	871446.079	7557.516
Log total employment	44499	5.756	1.862	5.583
Log average earnings	44499	7.766	0.530	7.779
Log average payrolls	44499	13.523	2.051	13.353

Table 2: Direct effect of payment acceleration: establishment-level baseline

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the establishment level of the change in payment score and log employment on a dummy measuring whether the establishment received government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment (dummy)* is a dummy taking the value of one if the establishment received eligible government contracts in the two years preceding the reform. Control variables include a dummy for whether or not the establishment received any government contracts (accelerated or not) in the two years prior to the reform, the *change* in log employment or the *change* in the Payment-related credit score from 2008Q1 to 2011Q1, as well as the log of employment and the Payment-related credit score in 2011Q1, and the age of the establishment in 2011Q1. There are 3120 counties and 1051 6-digit sectors. 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 Payment score (relative to 2011Q1)				
Treatment (dummy)	0.458***	0.511***	0.525***	0.545***	0.553***
	(0.134)	(0.134)	(0.132)	(0.132)	(0.182)
Controls	Yes	Yes	Yes	Yes	Yes
6-digit Sector FE	No	Yes	No	Yes	No
County FE	No	No	Yes	Yes	No
County × 6-digit Sector FE	No	No	No	No	Yes
Observations	3376225	3376225	3376225	3376225	3376225
R^2	0.106	0.113	0.113	0.119	0.270
	Panel B: Δ Log employment (relative to 2011Q1)				
Treatment (dummy)	0.015**	0.015**	0.014**	0.015**	0.017**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
Controls	Yes	Yes	Yes	Yes	Yes
6-digit Sector FE	No	Yes	No	Yes	No
County FE	No	No	Yes	Yes	No
County × 6-digit Sector FE	No	No	No	No	Yes
Observations	7129157	7129157	7129157	7129157	7129157
R^2	0.003	0.003	0.005	0.005	0.145

Table 3: Direct effect of payment acceleration: establishment-level dynamics

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the establishment level of the change in payment score and log employment on a dummy measuring whether the establishment received government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment (dummy)* is a dummy taking the value of one if the establishment received eligible government contracts in the two years preceding the reform. Control variables include a dummy for whether or not the establishment received any government contracts (accelerated or not) in the two years prior to the reform, the *change* in log employment or the *change* in the Payment-related credit score from 2008Q1 to 2011Q1, as well as the log of employment and the Payment-related credit score in 2011Q1, and the age of the establishment in 2011Q1. There are 3120 counties and 1051 6-digit sectors. 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 Payment score (relative to 2011Q1)				
	[t-2,t]	[t-1,t]	[t,t+1]	[t,t+2]	[t,t+3]
Treatment (dummy)	0.103	0.095	0.471***	0.411**	0.553***
	(0.112)	(0.124)	(0.179)	(0.180)	(0.182)
Controls	Yes	Yes	Yes	Yes	Yes
County × 6-digit Sector FE	Yes	Yes	Yes	Yes	Yes
Observations	4456842	4382124	4117792	3791391	3376225
R^2	0.758	0.461	0.212	0.231	0.270
	Panel B: Δ Log employment (relative to 2011Q1)				
	[t-2,t]	[t-1,t]	[t,t+1]	[t,t+2]	[t,t+3]
Treatment (dummy)	0.005	0.002	0.005	0.012*	0.017**
	(0.005)	(0.004)	(0.004)	(0.007)	(0.008)
Controls	Yes	Yes	Yes	Yes	Yes
County × 6-digit Sector FE	Yes	Yes	Yes	Yes	Yes
Observations	8943717	8943717	8339454	7880364	7129157
R^2	0.655	0.356	0.128	0.139	0.145

Table 4: Direct effect of payment acceleration: establishment-level falsification tests

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the establishment level of the change in payment score and log employment on a dummy measuring whether the establishment received government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment (dummy)* is a dummy taking the value of one if the establishment received eligible government contracts in the two years preceding the reform. *Non-eligible (dummy)* is a dummy taking the value of one if the establishment received government contracts that were not eligible to the payment acceleration in the two years preceding the reform. Control variables include the *change* in log employment or the *change* in the Payment-related credit score from 2008Q1 to 2011Q1, as well as the log of employment and the Payment-related credit score in 2011Q1, and the age of the establishment in 2011Q1. There are 3120 counties and 1051 6-digit sectors. 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 Payment score (relative to 2011Q1)				
Treatment (dummy)	0.501***	0.577***	0.565***	0.589***	0.516***
	(0.082)	(0.081)	(0.079)	(0.076)	(0.103)
Non-eligible (dummy)	0.043	0.066	0.040	0.045	-0.037
	(0.114)	(0.111)	(0.111)	(0.107)	(0.149)
Controls	Yes	Yes	Yes	Yes	Yes
6-digit Sector FE	No	Yes	No	Yes	No
County FE	No	No	Yes	Yes	No
County × 6-digit Sector FE	No	No	No	No	Yes
Observations	3376225	3376225	3376225	3376225	3376225
R^2	0.106	0.113	0.113	0.119	0.270
P-value Treatment=Non-eligible	0.001	0.000	0.000	0.000	0.002
	Panel B: Δ Log employment (relative to 2011Q1)				
Treatment (dummy)	0.020***	0.019***	0.018***	0.017***	0.018***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Non-eligible (dummy)	0.005	0.004	0.003	0.003	0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Controls	Yes	Yes	Yes	Yes	Yes
6-digit Sector FE	No	Yes	No	Yes	No
County FE	No	No	Yes	Yes	No
County × 6-digit Sector FE	No	No	No	No	Yes
Observations	7129157	7129157	7129157	7129157	7129157
R^2	0.003	0.003	0.005	0.005	0.145
P-value Treatment=Non-eligible	0.029	0.027	0.038	0.034	0.032

Table 5: Model 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.

Reduced form coefficient	0.009	0.011	0.013	0.015	0.017	0.019	0.021
Implied employment growth for a 100% treated firm ($\Delta L_t^* - 1$)	0.11	0.13	0.15	0.17	0.20	0.22	0.24
Model implied R	0.25	0.30	0.35	0.40	0.45	0.50	0.55

Table 6: Direct effect of payment acceleration: county×sector 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 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. In Panel B, the treatment variable is normalized by its cross-sectional standard deviation. 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: baseline					
	Δ log payroll		Δ log employment		Δ log earnings	
Treatment	0.095***	0.070**	0.078***	0.057**	0.017	0.012
	(0.032)	(0.031)	(0.027)	(0.026)	(0.014)	(0.012)
County×sector controls	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	No	Yes	No	Yes	No	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	44484	44484	44484	44484	44484	44484
R^2	0.159	0.184	0.140	0.173	0.172	0.244
	Panel B: standardized treatment					
	Δ log payroll		Δ log employment		Δ log earnings	
Treatment (std)	0.013***	0.010**	0.011***	0.008**	0.002	0.002
	(0.005)	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)
County×sector controls	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	No	Yes	No	Yes	No	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	44484	44484	44484	44484	44484	44484
R^2	0.159	0.184	0.140	0.173	0.172	0.244

Table 7: 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

Table 8: Spillover effect of payment acceleration: county×sector 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	-0.012***	-0.010**	-0.008***	-0.008***	-0.004**	-0.002
	(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	0.048	0.074	0.104	0.178

Table 9: Spillover effect of payment acceleration: county×sector 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	-0.000	-0.003	-0.006***	-0.007***	-0.008***
	(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	0.544	0.267	0.038	0.050	0.064	0.074

Table 10: 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	0.010***	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			-0.005	-0.006
			(0.004)	(0.004)
Treatment: CZ × high			-0.018***	-0.018***
			(0.005)	(0.006)
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	0.073	0.048	0.074

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

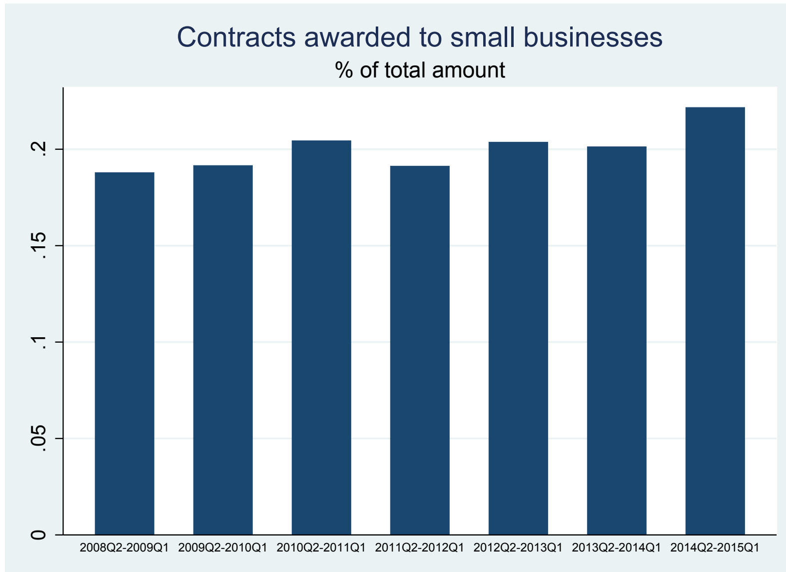
This table presents the results of a difference-in-difference estimation in first-differences. We run OLS cross-sectional regressions at the State×Origin sector × Destination sector. The dependent variable, Job flows, is defined as total job flows from origin sector to destination sector in a given State 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)
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
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

Appendix

The Employment Effects of Faster Payment: Evidence from the Federal Quickpay Reform

Panel A. Share of contracting dollars awarded to small businesses



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

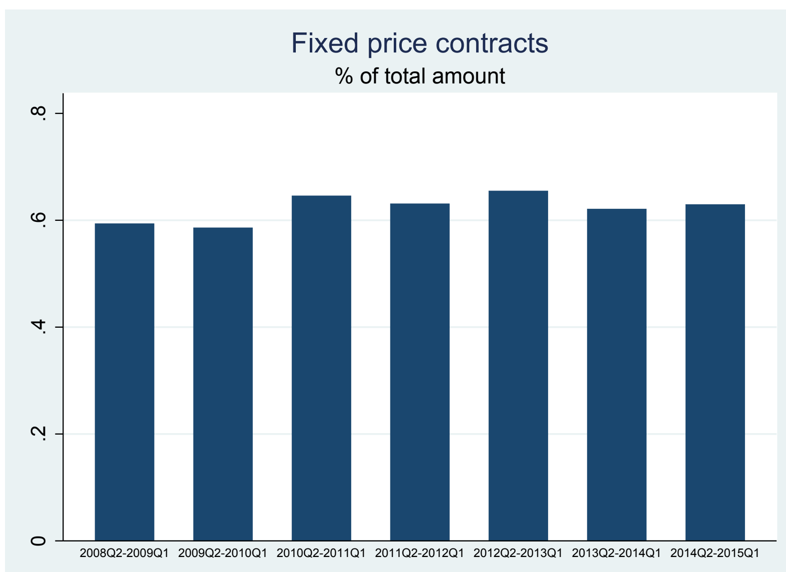
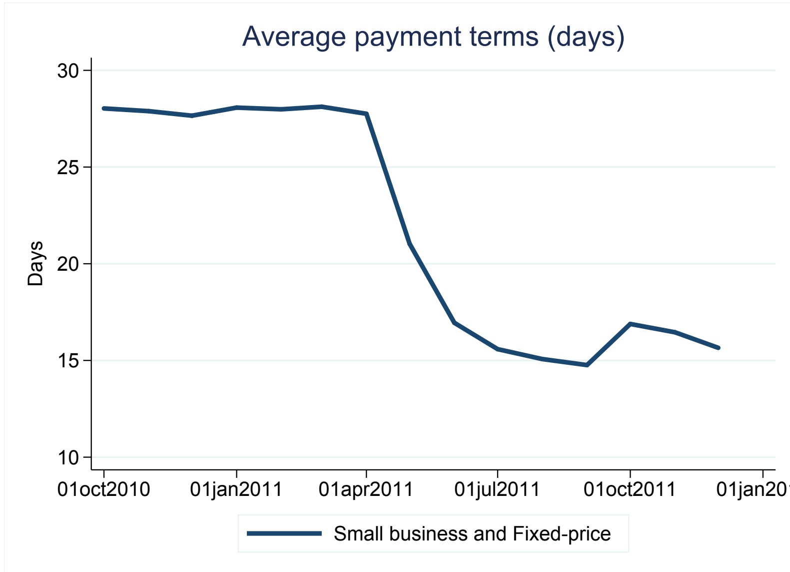


Figure A.1: **Trends in government contracting.** This figure shows trends in government contracting between 2009 and 2015. Panel A presents the share of total government contracts awarded to small business, on a dollar-weighted basis. Panel B presents the share of total government contract awarded through 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

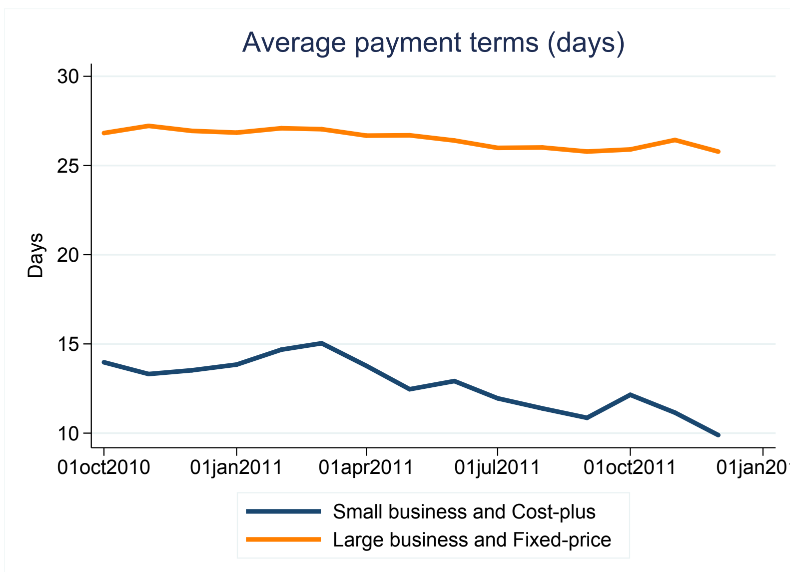


Figure A.2: **Department of Defense payment terms.** This figure shows the average number of days between receipt and payment of invoices in the MOCAS payment system of the Department of Defense. Panel A presents the difference between payments associated with contracts awarded to small versus large 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.

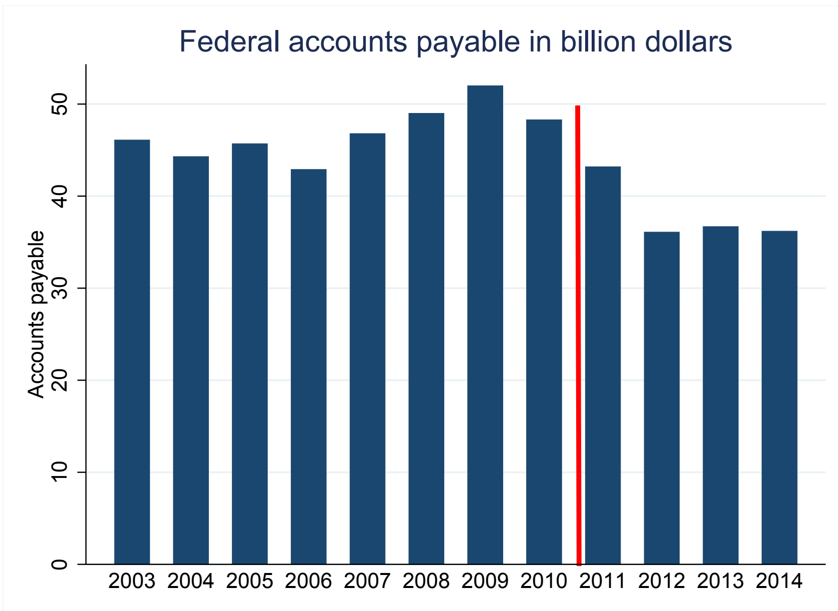
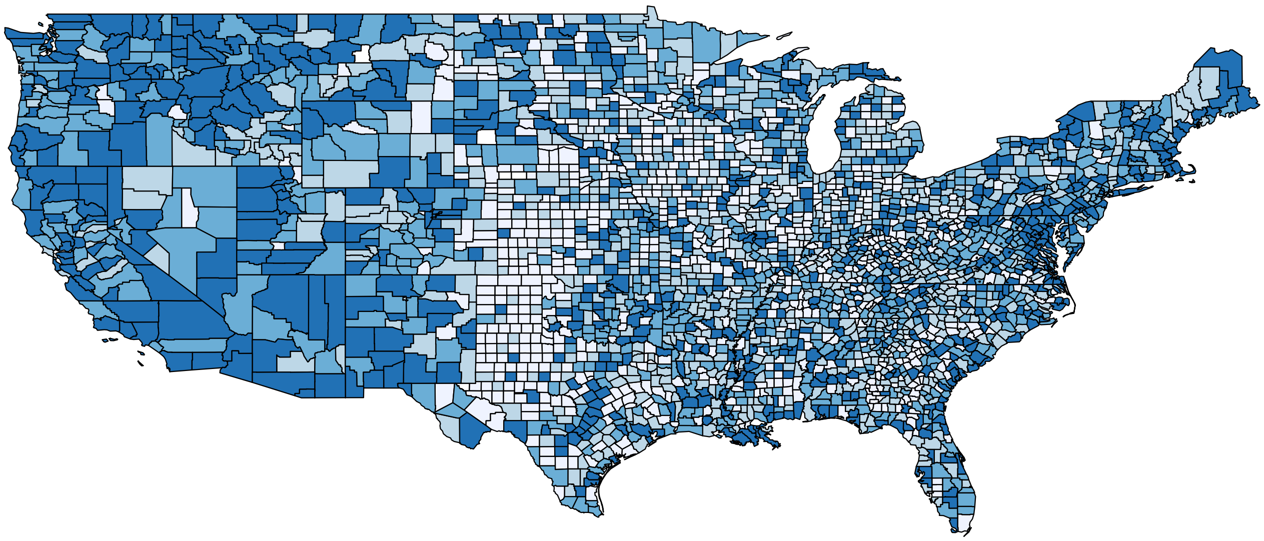


Figure A.3: **US federal accounts payable.** This figure presents total accounts payables across agencies of the US federal government for fiscal years 2003 to 2014, obtained from United States Government Notes to the Financial Statements. These aggregates exclude agencies that did not report payables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

Panel A. Total government contracts



Panel B. Accelerated government contracts

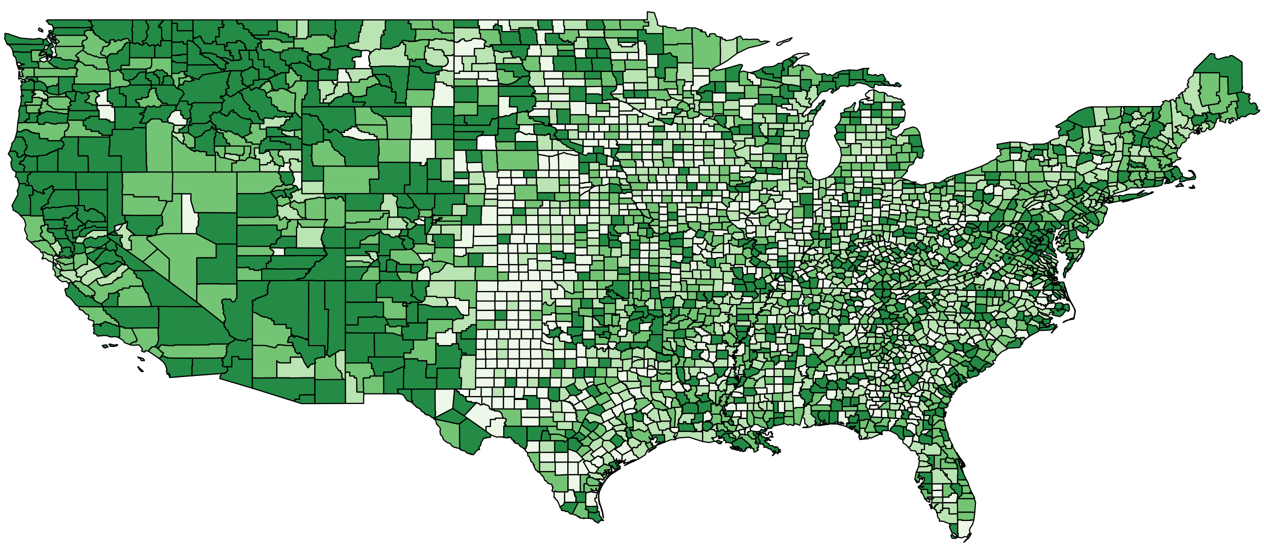


Figure A.4: **Distribution of government contracts across US counties.** This figure shows the distribution of total government contracts (Panel A) and government contracts eligible to acceleration (Panel B) aggregated at the county level in the two years prior to the reform, normalized by total county payrolls. Darker shades indicate larger intensity county level exposure.

Table A.1: Establishment level regressions: loan delinquencies

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the establishment level of a dummy for whether the establishment becomes delinquent on a loan in 2012-2014 on a dummy measuring whether the establishment received government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment (dummy)* is a dummy taking the value of one if the establishment received eligible government contracts in the two years preceding the reform. Control variables include a dummy for whether or not the establishment received any government contracts (accelerated or not) in the two years prior to the reform, the *change* in log employment, as well as the log of employment in 2011Q1, and the age of the establishment in 2011Q1. There are 3120 counties and 1051 6-digit sectors. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively. Source: Paynet. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Probability to be delinquent on a loan 2012-14				
Treatment (dummy)	-0.043**	-0.042**	-0.041**	-0.044**	-0.073*
	(0.016)	(0.018)	(0.017)	(0.018)	(0.044)
Controls	Yes	Yes	Yes	Yes	Yes
6-digit Sector FE	No	Yes	No	Yes	No
County FE	No	No	Yes	Yes	No
County $\times$ 6-digit Sector FE	No	No	No	No	Yes
Observations	13982	13982	13982	13982	13982
R^2	0.375	0.437	0.415	0.476	0.850

Table A.2: Establishment level regressions: exit

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the establishment level of a dummy for whether the establishment exits at any time between 2012-2013 on a dummy measuring whether the establishment received government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment (dummy)* is a dummy taking the value of one if the establishment received eligible government contracts in the two years preceding the reform. Control variables include a dummy for whether or not the establishment received any government contracts (accelerated or not) in the two years prior to the reform, the *change* in log employment, as well as the log of employment in 2011Q1, and the age of the establishment in 2011Q1. There are 3120 counties and 1051 6-digit sectors. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

	Probability of establishment exit in 2012-13				
Treatment (dummy)	-0.039***	-0.039***	-0.038***	-0.037***	-0.037***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Controls	Yes	Yes	Yes	Yes	Yes
6-digit Sector FE	No	Yes	No	Yes	No
County FE	No	No	Yes	Yes	No
County × 6-digit Sector FE	No	No	No	No	Yes
Observations	8943717	8943717	8943717	8943717	8943717
R^2	0.016	0.020	0.025	0.028	0.140

Table A.3: 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.4: Distribution of treatment across sectors

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.

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 A.5: 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.6: Top and bottom 20 4-digit NAICS industries

This table 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.7: 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.8: 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.9: Baseline in alternative samples

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 in the full sample. Standard errors presented in parentheses are clustered at the commuting-zone-level. *, **, and *** denote significance at the 10%, 5%, and 1%, respectively.

			Sample restricted to			
	Full sample		County×sectors with non zero gov contracts		County×sectors with positive treatment	
Treatment	0.095***	0.070**	0.081**	0.070**	0.078**	0.069*
	(0.032)	(0.031)	(0.035)	(0.034)	(0.037)	(0.036)
County×sector controls	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	No	Yes	No	Yes	No	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	44484	44484	23149	23149	20670	20670
R^2	0.159	0.184	0.259	0.280	0.280	0.300

Table A.10: Robustness: Baseline using alternative functional forms

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: 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 B: dummies for high versus medium versus low treatment				
Medium treatment	-0.009	0.023***	0.024***	0.028***	0.022***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
High treatment	0.020***	0.031***	0.015**	0.045***	0.015**
	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)
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	0.067	0.160	0.184

Table A.11: 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.12: 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.13: 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.14: Direct effect of payment acceleration: county×sector 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 A.15: 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 A.16: 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)				
Treatment	0.132***	0.078***	0.054**	0.105***	0.061**
	(0.026)	(0.025)	(0.024)	(0.027)	(0.027)
Non-eligible	-0.013	-0.007	-0.017	0.010	-0.009
	(0.013)	(0.012)	(0.012)	(0.012)	(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	0.002	0.048	0.066	0.159	0.184
P-value Treatment = Non-eligible	0.000	0.005	0.015	0.003	0.026

Table A.17: Establishment level regressions: spillovers

This table presents the results of a difference-in-difference estimation in first-differences. We run OLS regressions at the establishment level of the change in payment score and log employment on a dummy measuring whether the establishment received government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment (dummy)* is a dummy taking the value of one if the establishment received eligible government contracts in the two years preceding the reform. *Treatment (average): CZ* is the average of $Treatment$ dummies across all establishments in the commuting zone of the focal establishment, excluding the focal establishment. Control variables include a dummy for whether or not the establishment received any government contracts (accelerated or not) in the two years prior to the reform, the *change* in log employment from 2008Q1 to 2011Q1, as well as the log of employment in 2011Q1, and the age of the establishment in 2011Q1. Commuting zone level controls include the share of establishments that received any government contracts (accelerated or not) in the two years prior to the reform, the average *change* in log employment from 2008Q1 to 2011Q1, log employment in 2011Q1, and the average age of establishments in 2011Q1. There are 3120 counties and 1051 6-digit sectors. 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)				
	Panel A: Baseline				
Treatment (dummy)	0.022***	0.015**	0.015**	0.015**	0.015**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Treatment (average): CZ	-2.582***	-2.879***	-2.643***	-1.762***	-0.951*
	(0.690)	(0.678)	(0.581)	(0.564)	(0.492)
Establishment controls	No	Yes	Yes	Yes	Yes
CZ controls	No	No	No	Yes	Yes
State FE	No	No	Yes	No	Yes
Observations	7129157	7129157	7129157	7129157	7129157
R^2	0.000	0.003	0.003	0.003	0.003
	Panel B: Dynamics				
	[t-2,t]	[t-1,t]	[t,t+1]	[t,t+2]	[t,t+3]
Treatment (dummy)	0.002	-0.001	0.004	0.010*	0.015**
	(0.005)	(0.004)	(0.004)	(0.006)	(0.007)
Treatment (average): CZ	-0.211	-0.171	0.031	-0.379	-0.951*
	(0.226)	(0.191)	(0.230)	(0.329)	(0.492)
Establishment controls	Yes	Yes	Yes	Yes	Yes
CZ controls	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Observations	8943717	8943717	8339454	7880364	7129157
R^2	0.602	0.252	0.001	0.002	0.003

Table A.18: 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 fixed-price: CZ	-0.007***	-0.007***	-0.006**	-0.007***	-0.007***
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
Non-eligible: CZ	0.011	0.017	0.016	0.017	0.025
	(0.031)	(0.032)	(0.031)	(0.032)	(0.033)
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	0.015	0.048	0.037	0.048	0.074
P-value Treatment: CZ = Non-eligible: CZ	0.953	0.781	0.860	0.781	0.652

Table A.19: 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