

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

JEAN-NOËL BARROT and RAMANA NANDA\*

### ABSTRACT

We study the impact of *Quickpay*, a reform that permanently 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. However, we document substantial crowding out of nontreated firms' employment within local labor markets. While the overall net employment effect is positive, it is close to zero in tight labor markets. Our results highlight an important channel for alleviating financing constraints in small firms, but emphasize the general-equilibrium effects of large-scale interventions, which can lead to lower aggregate outcomes depending on labor market conditions.

FOLLOWING THE 2008 FINANCIAL CRISIS and subsequent slow recovery, the contraction in credit supplied by financial intermediaries to nonfinancial firms had a substantial impact on the real economy, 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 focused on increasing small firms' access to bank credit to alleviate financing constraints (Bernanke (2010), Yellen (2013), Mills and McCarthy (2014)).

Beyond facilitating access to bank credit, however, government can impact small firms' financing more directly through its role as their customer. In the

\*Jean-Noël Barrot is at HEC Paris. Ramana Nanda is at Harvard Business School and a Visiting Professor at Imperial College London. We are indebted to Editor Amit Seru and two anonymous referees for helpful comments. We are grateful to Manuel Adelino; Oriana Bandiera; Nittai Bergman; Emily Breza; Hui Chen; Erik Hurst; Mauricio Larrain; Erik Loualiche; Karen Mills; Ben Pugsley; David Robinson; Antoinette Schoar; Scott Stern; John Van Reenen; David Thesmar; Chris Woodruff; Liu Yang; Eric Zwick; and participants at the NBER Entrepreneurship and Economic Growth Conference, Toulouse School of Economics, MIT Finance lunch, HEC, INSEAD, Toulouse School of Economics, LSE, Maryland Junior Finance Conference, Georgia Tech, NBER Corporate Finance, American Finance Associations meetings, Sciences Po, NBER Summer Institute Entrepreneurship meeting, and NYU for helpful feedback. We are also grateful to the U.S. Department of Defense for sharing data on the timing of payments from their MOCAS accounting system and to Paynet for providing us data on loan delinquencies. Barrot recognizes support from the Kauffman Foundation Junior Faculty Fellowship and MIT Sloan. Nanda thanks the Division of Research and Faculty Development at HBS for financial support. All errors are our own. We have read *The Journal of Finance* disclosure policy and have no conflicts of interest to disclose.

Correspondence: Jean-Noël Barrot, Department of Finance, HEC Paris, 1, rue de la Libération, 75350 Jouy-en-Josas, France; e-mail: [barrot@hec.fr](mailto:barrot@hec.fr).

DOI: 10.1111/jofi.12955

© 2020 the American Finance Association

United States, federal government procurement amounts to 4% of GDP with \$100 billion of goods and services purchased directly from small firms. Government contracts typically require payment one to two months following the approval of an invoice. Thus government contractors are effectively *lending* to the government while simultaneously having to finance their payroll and working capital through the production process or by borrowing from banks. 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 (Benmelech, Bergman, and Seru (2011), Jermann and Quadrini (2012)). 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, Bergman, and Seru (2011), Chodorow-Reich (2014), Greenstone, Mas, and Nguyen (2020)).

Nevertheless, there are reasons to also believe that faster payment might not affect aggregate employment by much, or at all: payment acceleration should have negligible effects in the absence of financing frictions. Furthermore, as Acemoglu (2010) points out, 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 impact 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 on both the intensity of financing frictions and the direction and magnitude of the effect on other firms.

We study the impact of payment acceleration on firm-level employment in the United States 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 between invoice approval and payment by half, from 30 days to 15 days. For treated firms, the reform permanently reduced the working capital needed to sustain a dollar of sales with the government. A total of \$70 billion in annual contract value was accelerated, with small businesses impacted 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) data set, which comprises establishment-level panel data that come from the Dunn & Bradstreet (D&B) registers. Our establishment-level results 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 three years following the reform, when our

establishment-level data end. In addition, and consistent with a shorter cash conversion cycle<sup>1</sup> driving employment growth, we document that treated firms also begin paying their own suppliers in a more timely manner, leading to improvements in their own payment-related credit score within the D&B registers. Placebo regressions using government contractors not exposed to the treatment show no such employment or credit score effect, 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%, which is comparable to the cost of trade credit and of other sources of financing available to small businesses in the wake of the financial crisis.

We next 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 a crowding out effect 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 accelerated payment. In additional analysis, we provide evidence of this crowding out effect, as well as 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 effects when studying the real effects of large-scale reforms aimed at relaxing firm-level financing constraints.

Our study is related to several strands of the literature. First, our work contributes to the literature on financing constraints among small, private firms. These firms account for a substantial portion of employment and output, but have received relatively little 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 interfirm financing is referred to as trade credit and, in aggregate, is three times as large as bank loans and 15 times as large as commercial paper in

<sup>1</sup> “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.

the United States.<sup>2</sup> 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 (2015)) and expose them to liquidity risk (Barrot (2015)).<sup>3</sup> Our work, which is based on firms in a broad set of industries across the United States, shows that trade credit provision also constrains employment growth, even when the debtor is a low-risk customer such as the federal government.<sup>4</sup> 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 United States. Our estimates suggest the presence of large financing frictions, which is interesting in light of 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.<sup>5</sup> Benmelech, Bergman, and Seru (2011) 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.<sup>6</sup> Moreover, in addition to providing evidence on direct effects of the treatment, we are able to shed light on crowding out effects on nontreated firms, due to the targeted nature of the treatment, and to speak to the aggregate effects of financing conditions at the local labor market level.<sup>7</sup>

<sup>2</sup> As of September 2012, according to the U.S. Flow of Funds Accounts.

<sup>3</sup> 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, Burkart, and Ellingsen (2011), Antras and Foley (2015), Dass, Kale, and Nanda (2015), Kim and Shin (2012), Klapper, Laeven, and Rajan (2012), Garcia-Appendini and Montoriol-Garriga (2013), Murfin and Njoroge (2015), and Breza and Liberman (2017).

<sup>4</sup> In practice, there are also impediments in the pledgeability of government trade credit claims. We provide more details below.

<sup>5</sup> The effect of financing frictions on capital investment has been studied extensively, starting with Fazzari, Hubbard, and Petersen (1988), who find a strong positive relationship between cash flows and investment. Subsequent studies have complemented these findings using exogenous variations in cash flows (e.g., Blanchard, Lopez-de Silanes, and Shleifer (1994), Lamont (1997), Rauh (2006), Faulkender and Petersen (2012)), variations in collateral (e.g., Chaney, Sraer, and Thesmar (2012)), or structural models (e.g., Whited (1992)).

<sup>6</sup> Other studies have analyzed the interaction of firms employment and financing decisions including Matsa (2010), Benmelech, Bergman, and Enriquez (2012), and Agrawal and Matsa (2013).

<sup>7</sup> 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).

Finally, our findings contribute to the literature on the role of policy intervention targeting businesses in the United States. Most of this literature focuses on fiscal policies, such as bonus depreciation (House and Shapiro (2008), Zwick and Mahon (2017)) 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. Our work is among the first to examine the role of government as a customer and its implications for the private sector.<sup>8</sup> 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 suppliers of goods and services to the government.

The rest of the paper is structured as follows. In Section I, 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 II, we describe our identification strategy and provide an overview of the data we use to study the effect of *Quickpay*. Section III outlines our results, and relates the results from our regressions to the theoretical model to provide a perspective on the magnitudes. Finally, Section IV concludes.

## I. Financing Labor Inputs

### A. 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), Wasmer and Weil (2004), Petrosky-Nadeau and Wasmer (2013)). These adjustment costs could emerge because of hiring and training costs, for instance.

Financing frictions can also affect employment when firms have to finance working capital (Jermann and Quadrini (2012)), as the mismatch between the timing of cash-flow generation and payments to labor requires firms to finance their payroll through the production process—before getting paid. In this case, firms may have to cut back on hiring *even in the presence of customer demand*

<sup>8</sup> Other studies include Liebman and Mahoney (2017), Cohen and Malloy (2016), Ferraz and Finan (2015), and Goldman (2020).

and adequate labor supply due to an inability to pay workers prior to receiving payment for their product or service.<sup>9</sup>

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.<sup>10</sup> 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.<sup>11</sup> In order to operate, this firm has approximately \$80,000 of cash ( $30/365 \times \$1$  million) tied up in receivables at any point 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 to use on an ongoing basis. In this extreme example where the firm is only able to support growth through internal cash flow, this will allow the firm to double in size, to \$2 million. Hence seemingly small improvements in the working capital position for constrained firms can have meaningful effects for sales and employment growth.<sup>12</sup>

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  $\mu$  and  $1 - \mu$ , that only differ in the amount of working capital they need to finance upfront.<sup>13</sup> Both sets of firms  $i \in (t, u)$  have the same decreasing returns to scale technology in labor,

$$Y = A(L^\sigma K^{1-\sigma})^\alpha, \quad (1)$$

where  $Y$  is output,  $A$  is total factor productivity,  $L$  and  $K$  are the quantity of labor and capital, respectively,  $\alpha < 1$  captures decreasing returns to scale.

<sup>9</sup> 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)).

<sup>10</sup> An alternative is to turn to factoring companies, that 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, nonrecourse factoring (where the factor takes on the full counterparty 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 toward firm growth.

<sup>11</sup> 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.

<sup>12</sup> 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.

<sup>13</sup> 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.



Firms maximize profit  $\Pi$ ,

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

where  $w$  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  $p = 1$ .  $\gamma_i$  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, labor demand is given as

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

Using this expression, we can express the elasticity of labor demand to a change in payment terms as

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

As expected, labor demand decreases with  $\gamma_i$ , that is, 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 of labor demand increases with returns to scale.

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

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

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

$$C \leq wL + \Pi(L). \quad (6)$$

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

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

where  $\theta$  is the labor supply elasticity.<sup>14</sup> 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

<sup>14</sup>  $\zeta$  is a standard scaling factor for labor, but because utility is modeled as being separable in consumption and labor, it gets differenced out and hence does not end up playing a consequential role in the model.

the change in the optimal quantity of labor when going from  $\gamma_{i,1}$  prior to *Quickpay* to  $\gamma_{i,2}$  afterward. We express employment growth for treated firms ( $i = t$ ) across the two steady states, prior and after *Quickpay* as

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

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 crowding out of labor demand through wage increases.

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

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

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

An appealing feature of equation (8) is that we can calibrate all parameters except for  $\frac{L_{i,2}^*}{L_{i,1}^*}$  and  $R$ , the cost of financing. Since the empirical tests we present below provide an estimate of the employment response  $\frac{L_{i,2}^*}{L_{i,1}^*}$ , this allows us to infer  $R$ . Our findings therefore shed light on the intensity of financing constraints facing firms in the United States at the time of the *Quickpay* reform that we describe next prior to our empirical analysis.

### B. 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 upward 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.<sup>15</sup> 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.<sup>16</sup>

<sup>15</sup> Chapter 39 of title 31 of the U.S. Code.

<sup>16</sup> See Memorandum M11-32 of the Office of Management and Budget, 2011.



The reform was formally announced on September 14, 2011 with the goal of achieving payments acceleration in all federal agencies by November 1, 2011. 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.<sup>17</sup> 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.<sup>18</sup> 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 1,500 employees.<sup>19</sup> The contracting officer in any given federal agency is in charge of checking whether the contractor is a small business firm and whether it is therefore eligible to accelerated payments. Internet Appendix Figure IA.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.<sup>20</sup>

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.<sup>21</sup> 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 prenegotiated price. Under cost-plus contracts, contractors are paid for their expenses up to a set limit, plus profit.<sup>22</sup> Internet Appendix Figure IA.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

<sup>17</sup> See Memorandum 2011-O0007 of the Office of the Under Secretary of Defense, 2011.

<sup>18</sup> Getting Money to Small Businesses Faster, White House Press Release, 2011.

<sup>19</sup> For more details on these thresholds, see <https://www.sba.gov/content/small-business-size-standards>.

<sup>20</sup> The Internet Appendix may be found in the online version of this article.

<sup>21</sup> See Subpart 32.9 (Prompt Payment) of the Federal Acquisition Regulation.

<sup>22</sup> For further analyses of these two contract types, see Horton (2008), for instance.

cost-plus contracts within 15 days.<sup>23</sup> Finally, the Department of Defense also paid disadvantaged small business contractors earlier prior to the implementation of *QuickPay*.<sup>24</sup> In the rest of the paper, we use the term “noneligible” 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 III.

## II. Data

The data used in our analysis come from several sources. 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 detail, including information on contracts, grants, direct payments, and loans. This website was launched in 2007 and includes FPDS data back to 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 North American Industry Classification System (NAICS) sector, whether the contractor is a small business or not, and the zip code where the contract is to be performed.<sup>25</sup> For the establishment-level analyses, these data allow us to create an indicator for whether the establishment was a government contractor and whether it was treated by the *Quickpay* reform. For the analyses at the county-by-sector and commuting zone levels, we create a county-sector level measure of exposure to treatment that is equal to the average total quarterly value of contracts awarded to small businesses in that cell over the period 2009Q1 to 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 fewer than five employees in 2011Q1, or where the ratio of government contracts to payroll is larger than four.<sup>26</sup>

FPDS data do not include information on speed of payment. To verify that the reform was effectively implemented, we obtained proprietary cash-flow information from the Department of Defense’s main payment system, the Mechanization of Contract Administration Services (MOCAS). For all receipts processed from 2010Q3 to 2014Q3, we obtain the date of receipt and payment as well as contract characteristics including the contract identifier, which

<sup>23</sup> See Subpart 232.906 of the Department of Defense Supplement to the Federal Acquisition Regulation (DFARS), 48 CFR Chapter 2.

<sup>24</sup> See Subpart 232.903 of the Department of Defense Supplement to the Federal Acquisition Regulation (DFARS), 48 CFR Chapter 2.

<sup>25</sup> We also obtain the contractor’s location. While this is a less well-measured data point, we find similar results when we use this variable instead, most likely because both locations are the same in a vast majority of cases.

<sup>26</sup> See Internet Appendix Figure IA.2 for the distribution of this treatment variable across U.S. counties, and Internet Appendix Tables IA.I, IA.II, and IA.III for its distribution across sectors.

allows us to merge this information with the FPDS data. Figure 1 presents average payment terms, measured as the difference in days between the receipt and payment of the invoice around implementation of the payment acceleration. From Panel A, we see that payment terms faced by small businesses with fixed-price contracts experience a decline. By contrast, Panel B shows that the 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. In Internet Appendix Figure IA.3, we further show 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.<sup>27</sup> 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 the reform by President Obama).

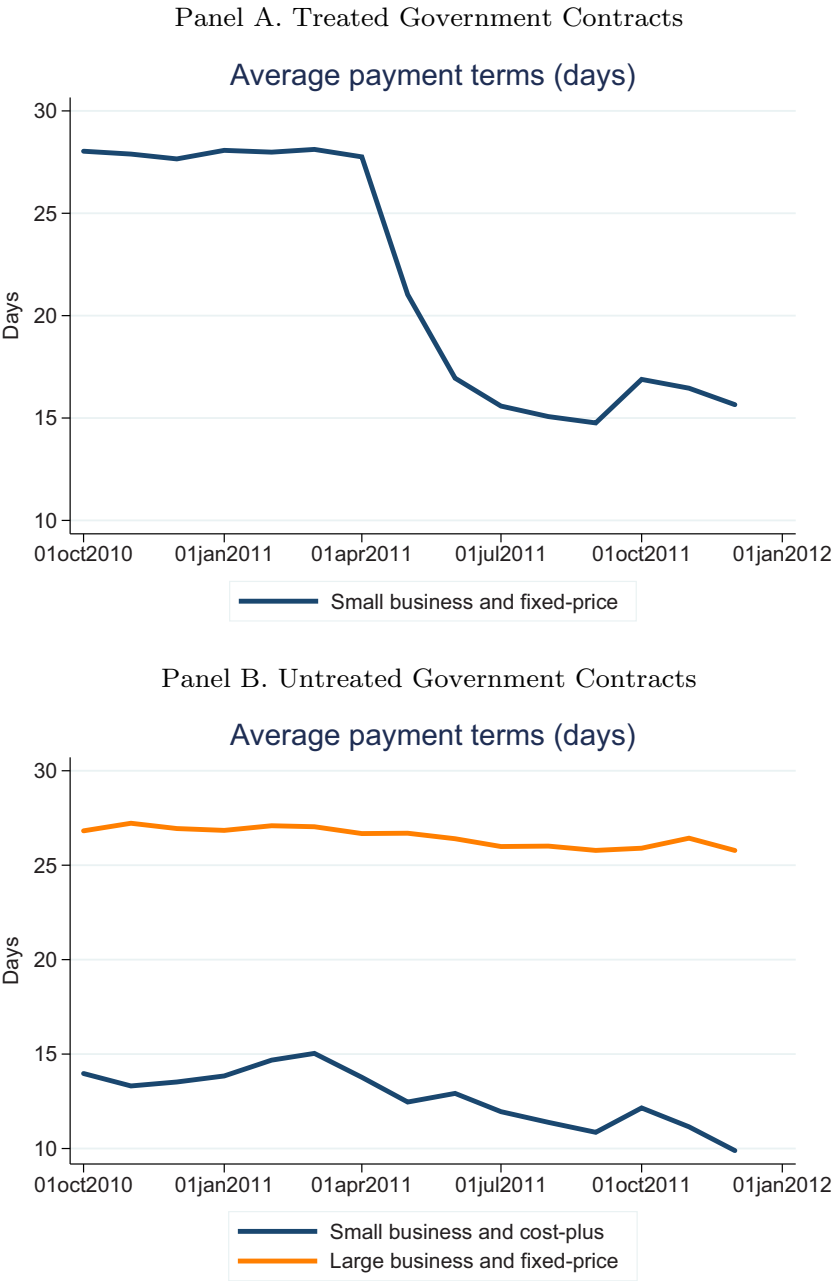
Our core data source to study establishment-level outcomes is the NETS data set, which is an establishment-level panel data set based on D&B credit registry data. Created and maintained by Walls and Associates, these data are made available to researchers for a fee. We use the 2014 version of these data, the latest version available to researchers at the time of analysis. NETS data cover both employer and nonemployer businesses. Every government contractor is required to register with D&B 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 microdata. NETS data also include a measure of a firm's credit worthiness, known as 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 firm's corporate liquidity, it allows us to analyze the effect of the reform on treated firms' cash flows. In addition to NETS data, we obtain microdata on loan delinquencies from Paynet, a fintech company that collects such information from the banks it partners with. 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).<sup>28</sup> QWI, which is based on microdata 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<sup>29</sup> in each county, we

<sup>27</sup> The fiscal year ends on September 30.

<sup>28</sup> Adelino, Ma, and Robinson (2017) also use QWI to study the local response of new firm growth to industry-level shocks.

<sup>29</sup> Sectors are defined according to the National American Industry Classification System (NAICS).



**Figure 1. 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 a fixed-price rather than a cost-plus basis. Under fixed-price contracts, contractors agree to deliver the product or service at a prenegotiated price. Under cost-plus contracts, contractors are paid for their expenses up to a set limit plus a profit. (Color figure can be viewed at [wileyonlinelibrary.com](http://wileyonlinelibrary.com))

obtain quarterly payrolls, employment, and average earnings per worker.<sup>30</sup> 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 comes from the Conference Board Help Wanted OnLine (HWOL) data, and the latter from the Bureau of Labor Statistics (BLS). Higher values of this ratio indicate a tighter labor market, that is, lower unemployment.

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

### III. Results

#### A. The Direct Effect of Accelerated Payments

##### A.1. Establishment-Level Estimations

We start by documenting 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:

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

where  $Y_{it}$  is either 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 one 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 postreform period. To control for unobserved heterogeneity, we include fixed effects at the establishment level ( $\theta_i$ ), industrial (six-digit) sector-by-year level ( $\gamma_{st}$ ), and county-by-year level ( $\lambda_{ct}$ ).  $X_{it}$  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:

$$\Delta Y_i = \beta_1.Treatment_i + \beta_2.X'_i + \gamma'_s + \lambda'_c + \epsilon'_i, \quad (11)$$

where  $\Delta Y_i$  is now the *change* in log employment or the *change* in payment-related credit score from 2011Q1 to 2014Q1.  $X_i$  includes a dummy for whether

<sup>30</sup> 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.

**Table I**  
**Summary Statistics**

Panel A of this table presents summary statistics for the outcome and control variables in the establishment-level sample. *Treatment (dummy)* is a dummy that takes the value of one if the establishment received eligible government contracts in the two years preceding the reform. Variables of interest include changes in payment score and log employment from 2011Q1 to 2014Q1. Control variables include a dummy for whether the establishment received any government contract (accelerated or not) in the two years prior to the reform, the change in log employment or the change in payment-related credit score from 2008Q1 to 2011Q1, the log of employment and the payment-related credit score in 2011Q1, and the age of the establishment in 2011Q1. Panel B of this table presents summary statistics for the key outcome and control variables, measured at the county  $\times$  sector level. The sample contains 3,120 counties and 18 industries. *Treatment* is the average quarterly amount of eligible government contracts to be performed in a given county  $\times$  sector between 2009Q1 and 2011Q1, normalized by average quarterly payroll measured in 2011Q1. Variables of interest include payroll, employment, and earnings growth rates between 2011Q1 and 2015Q1. Control variables include the average quarterly amount of all government contracts to be performed in a given county  $\times$  sector between 2009Q1 and 2011Q1, normalized by average quarterly payroll during the same period, as well as county  $\times$  sector controls, which include the share of small establishments, the average establishment share, 2000Q1 to 2011Q1 average annual employment growth, the correlation between employment growth in a given county  $\times$  sector and aggregate employment growth, total employment, total annualized payroll, and average earnings measured in 2011Q1.

	Obs.	Mean	SD	Median
Panel A: Plant-Level Sample				
Treatment (dummy)	8,943,717	0.001	0.036	0.000
Government contractor (dummy)	8,943,717	0.002	0.042	0.000
2011 payment score	5,962,341	73.950	13.793	80.000
$\Delta$ payment score	3,998,159	-1.093	12.584	0.000
Log employment	8,943,717	1.320	1.239	1.099
$\Delta$ log employment	7,129,157	0.004	0.163	0.000
Panel B: County $\times$ Sector-Level Sample				
Treatment	44,499	0.022	0.132	0.000
Government contracts	44,499	0.053	0.249	0.000
$\Delta$ log payroll	44,499	0.176	0.421	0.167
$\Delta$ log employment	44,499	0.052	0.349	0.049
$\Delta$ log earnings	44,499	0.124	0.212	0.117
Unemployment rate	44,499	9.492	3.018	9.300
Corr with U.S. emp. growth	44,499	0.120	0.275	0.122
Average establishment size	44,499	2.227	0.868	2.191
Share of small establishments	44,499	0.994	0.024	1.000
Emp. share of small establishments	44,499	0.825	0.261	1.000
Long-term employment growth	44,499	0.063	0.141	0.019
Employment	44,499	2,238	9,810	266
Annualized earnings ('000)	44,499	32.599	19.402	28.692
Annualized payrolls ('000)	44,499	101,503	87,1446	7,558
Log total employment	44,499	5.756	1.862	5.583
Log average earnings	44,499	7.766	0.530	7.779
Log average payrolls	44,499	13.523	2.051	13.353

Table II

**Direct Effect of Payment Acceleration: Establishment-Level Baseline**

This table presents results of a difference-in-difference estimation in first-differences. We run establishment-level ordinary least squares (OLS) regressions of the change in payment score and log employment on a dummy indicating whether the establishment received government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment (dummy)* is a dummy that takes 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 the establishment received any government contract (accelerated or not) in the two years prior to the reform, the *change* in log employment or the *change* in payment-related credit score from 2008Q1 to 2011Q1, the log of employment and the payment-related credit score in 2011Q1, and the age of the establishment in 2011Q1. The sample contains 3,120 counties and 1,051 six-digit sectors. Standard errors presented in parentheses are clustered at the commuting zone level. \*, \*\*, and \*\*\* denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: $\Delta$ Payment score (Relative to 2011Q1)					
Treatment (dummy)	0.458*** (0.134)	0.511*** (0.134)	0.525*** (0.132)	0.545*** (0.132)	0.553*** (0.182)
Controls	Yes	Yes	Yes	Yes	Yes
Six-digit sector FE	No	Yes	No	Yes	No
County FE	No	No	Yes	Yes	No
County $\times$ Six-digit sector FE	No	No	No	No	Yes
Observations	3,376,225	3,376,225	3,376,225	3,376,225	3,376,225
$R^2$	0.106	0.113	0.113	0.119	0.270
Panel B: $\Delta$ Log employment (Relative to 2011Q1)					
Treatment (dummy)	0.015** (0.007)	0.015** (0.007)	0.014** (0.007)	0.015** (0.007)	0.017** (0.008)
Controls	Yes	Yes	Yes	Yes	Yes
Six-digit sector FE	No	Yes	No	Yes	No
County FE	No	No	Yes	Yes	No
County $\times$ Six-digit sector FE	No	No	No	No	Yes
Observations	7,129,157	7,129,157	7,129,157	7,129,157	7,129,157
$R^2$	0.003	0.003	0.005	0.005	0.145

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. Our sample contains 3,120 counties and 1,051 six-digit sectors. The coefficient of interest,  $\beta_1$ , 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 II. Panel A reports results based on the change in the payment-related credit score and Panel B reports results based on the change in log employment. As can be seen from the table, the coefficient of interest,  $\beta_1$ , 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 the controls in equation (11), one may be worried that treated firms cluster in certain six-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 six-digit NAICS dummies (column (2)) and county dummies (column (3)) does not affect the coefficient. Furthermore, when we include county  $\times$  six-digit sector dummies (column (5)), and thereby compare treated and control firms in the same narrowly defined industry and location, we find that if anything the coefficient goes up slightly, which indicates that unobserved heterogeneity is unlikely to spuriously drive our estimates.

The magnitude of these effects are substantial. In Panel A, the coefficient on  $\beta_1$  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.

#### A.2. Dynamics and Falsification Tests

In Table III, we study the timing of the effects measured in Table II. We rerun the most stringent specification in Table II, that is the specification in column (5), for different long-differences running from 2009 to 2011 through to 2011 to 2014. The last column in Table III is identical to the last column in Table II while other columns show potential pretrends and the progression in the size of the effects over time. The results in Table III show that for both outcome variables, there is no measurable pretrend and that the parallel trend assumption is likely to be satisfied. Since there is attrition in the sample, there are fewer observations in column (5) than in column (1). To ensure that the absence of pretrends is not the by-product of sample attrition, we run the same specifications in the sample of firms that survive throughout the period, and confirm the absence of effect prior to the implementation of *Quickpay* in Internet Appendix Table IA.IV.

As can be seen in Table III, payment scores respond to the reform earlier than employment: the coefficient on the treatment variable for payment-related credit score is statistically different from zero as soon as 2012, while employment growth is statistically different from zero by 2013. To interpret these timings, we note that NETS data are measured as of January of each year. Given that the shift in payment acceleration under 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 change materially for the score to change. Since we measure employment in NETS every 12 months, the NETS

**Table III**  
**Direct Effect of Payment Acceleration: Establishment-Level Dynamics**

This table presents results of a difference-in-difference estimation in first-differences. We run establishment-level ordinary least squares (OLS) regressions of the change in payment score and log employment on a dummy indicating whether the establishment received government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment (dummy)* is a dummy that takes 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 the establishment received any government contract (accelerated or not) in the two years prior to the reform, the *change* in log employment or the *change* in payment-related credit score from 2008Q1 to 2011Q1, the log of employment and the payment-related credit score in 2011Q1, and the age of the establishment in 2011Q1. The sample contains 3,120 counties and 1,051 six-digit sectors. Standard errors presented in parentheses are clustered at the commuting zone level. \*, \*\*, and \*\*\* denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: $\Delta$ Payment score (Relative to 2011Q1)					
	$[t - 2, t]$	$[t - 1, t]$	$[t, t + 1]$	$[t, t + 2]$	$[t, t + 3]$
Treatment (dummy)	0.103 (0.112)	0.095 (0.124)	0.471*** (0.179)	0.411** (0.180)	0.553*** (0.182)
Controls	Yes	Yes	Yes	Yes	Yes
County $\times$ Six-digit sector FE	Yes	Yes	Yes	Yes	Yes
Observations	4,456,842	4,382,124	4,117,792	3,791,391	3,376,225
$R^2$	0.758	0.461	0.212	0.231	0.270
Panel B: $\Delta$ Log employment (Relative to 2011Q1)					
	$[t - 2, t]$	$[t - 1, t]$	$[t, t + 1]$	$[t, t + 2]$	$[t, t + 3]$
Treatment (dummy)	0.005 (0.005)	0.002 (0.004)	0.005 (0.004)	0.012* (0.007)	0.017** (0.008)
Controls	Yes	Yes	Yes	Yes	Yes
County $\times$ Six-digit sector FE	Yes	Yes	Yes	Yes	Yes
Observations	8,943,717	8,943,717	8,339,454	7,880,364	7,129,157
$R^2$	0.655	0.356	0.128	0.139	0.145

data suggest that the change in credit scores took place within the first six 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 can be seen as a more mechanical outcome of the reform for financially constrained firms, while the latter involves more active decision making by the firm.

Despite the absence of pretrends, one may nevertheless be concerned about threats to identification. For example, the *Quickpay* reform might be correlated with other policies undertaken to support the economy at the time, such as the American Recovery and Reinvestment Act (ARRA) that was initiated in 2009. However, the procurement data that we obtain include 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 may also be concerned that procurement policy may have changed after the reform in ways that could

help explain the results, irrespective of the payment acceleration. Internet Appendix Figure IA.1 indicates that the share of aggregate government spending going to small businesses (Panel A) is stable over the sample period.

One may further be concerned that the results are driven by unobserved heterogeneity in the types of firms that become government contractors, rather than by the specific channel that we study. 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 not eligible for payment acceleration should not see any change in their outcomes. We test this by running falsification tests in Table IV, where we separately include an indicator for being a treated establishment as before, but we now also include an indicator for being an establishment that had government contracts, but for whom payment was not accelerated.<sup>31</sup> Panel A of Table IV reports results for changes in payment-related credit scores and Panel B for 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 treatment and placebo coefficients reject the hypothesis that they are equal, thus providing greater reassurance that the findings are not spuriously driven by unobserved characteristics of firms receiving government contracts.

Since the *Quickpay* reform targets small businesses, one may also be concerned that our treatment variable is picking up the differential sensitivity of small businesses to the business cycle.<sup>32</sup> While all specifications include linear control for size, we confirm in Internet Appendix Table IA.V that our results on payment scores and employment hold after including dummies for establishment size deciles. In Internet Appendix Table IA.VI, we further check that the results continue to hold *within* the universe of small establishments. More specifically, we run our baseline specification after successively excluding firms with more than 250, 100, 50, 25, and 10 employees. Across these specifications, the effect of treatment on payment scores and employment remains strongly significant.

<sup>31</sup> A related concern is that other contract terms might have changed endogenously as a result 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 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 (2017)). Again, this would probably go against finding any positive effect of *Quickpay*.

<sup>32</sup> We thank an anonymous referee for flagging this concern and suggesting a way to test for it.

**Table IV**  
**Direct Effect of Payment Acceleration: Establishment-Level**  
**Falsification Tests**

This table presents results of a difference-in-difference estimation in first-differences. We run establishment-level ordinary least squares (OLS) regressions of the change in payment score and log employment on a dummy indicating whether the establishment received government contracts that were accelerated following the implementation of the federal payment reform of 2011. *Treatment (dummy)* is a dummy that takes value of one if the establishment received eligible government contracts in the two years preceding the reform. *Noneligible (dummy)* is a dummy that takes the value of one if the establishment received government contracts that were not eligible for payment acceleration in the two years preceding the reform. Control variables include the *change* in log employment or the *change* in payment-related credit score from 2008Q1 to 2011Q1, the log of employment and the payment-related credit score in 2011Q1, and the age of the establishment in 2011Q1. The sample contains 3,120 counties and 1,051 six-digit sectors. Standard errors presented in parentheses are clustered at the commuting zone level. \*, \*\*, and \*\*\* denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: $\Delta$ Payment score (Relative to 2011Q1)					
Treatment (dummy)	0.501*** (0.082)	0.577*** (0.081)	0.565*** (0.079)	0.589*** (0.076)	0.516*** (0.103)
Noneligible (dummy)	0.043 (0.114)	0.066 (0.111)	0.040 (0.111)	0.045 (0.107)	-0.037 (0.149)
Controls	Yes	Yes	Yes	Yes	Yes
Six-digit sector FE	No	Yes	No	Yes	No
County FE	No	No	Yes	Yes	No
County $\times$ Six-digit sector FE	No	No	No	No	Yes
Observations	3,376,225	3,376,225	3,376,225	3,376,225	3,376,225
$R^2$	0.106	0.113	0.113	0.119	0.270
p-Value treatment = Noneligible	0.001	0.000	0.000	0.000	0.002
Panel B: $\Delta$ Log employment (Relative to 2011Q1)					
Treatment (dummy)	0.020*** (0.004)	0.019*** (0.004)	0.018*** (0.004)	0.017*** (0.004)	0.018*** (0.005)
Noneligible (dummy)	0.005 (0.005)	0.004 (0.005)	0.003 (0.005)	0.003 (0.005)	0.001 (0.006)
Controls	Yes	Yes	Yes	Yes	Yes
Six-digit sector FE	No	Yes	No	Yes	No
County FE	No	No	Yes	Yes	No
County $\times$ Six-digit sector FE	No	No	No	No	Yes
Observations	7,129,157	7,129,157	7,129,157	7,129,157	7,129,157
$R^2$	0.003	0.003	0.005	0.005	0.145
p-Value Treatment = Noneligible	0.029	0.027	0.038	0.034	0.032

Internet Appendix Tables IA.VII and IA.VIII document two corroborating sets of results. Table IA.VII shows that treated firms were less likely to be delinquent on their loans. Table IA.VIII shows that treated establishments are less likely to exit. Taken 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

Table V  
Model Implied Financing Frictions

This table presents the values for the cost of financing  $R$  implied by our theoretical framework presented in Section I as a function of employment growth.

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

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.

B. Magnitudes and Implied Financing Frictions

Having established that *Quickpay* had a direct effect on treated firms, we next quantify the size of financing frictions facing these small businesses. As noted in Section I, an attractive feature of our setting is that we can map our reduced-form estimates to the simple model outlined in Section I, and thereby infer the cost of financing.

Equation (8) allows us to estimate the degree of financing constraints faced by firms because it only depends on employment growth and the change in  $\gamma_i$ , both of which we 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 reform, the fraction of input costs that needs to be financed in advance,  $\gamma_{t,1} = 30/365 = 0.08$  and  $\gamma_{t,2} = 15/365 = 0.04$ . By contrast, for the subset of firms  $u$  unaffected by the reform,  $\gamma_{u,1} = \gamma_{u,2}$ . 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 all of a firm’s receivables were accelerated. In practice of course, the accelerated contract value is substantially less than 100%, which means that the coefficients from our regressions will be too small relative to a situation in which all of a firm’s sales was subject to 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 infer that the corresponding model-implied value for  $R$  is 0.45, as can be seen from Table V.

We complement the approach above using Census data. Specifically, we rerun equation (11) at the county-by- two-digit sector level instead of the

establishment level:

$$\Delta \text{Log}Y_{sc} = \beta_1 \cdot \text{Treatment}_{sc} + \beta_2 \cdot X'_{sc} + \gamma'_s + \lambda'_c + \epsilon'_{sc}, \quad (12)$$

where  $\Delta \text{Log}Y_{sc}$  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 10-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 one if an establishment benefited from the reform,  $\text{Treatment}_{sc}$  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  $\times$  sector between 2009Q1 and 2011Q1. This includes all contracts awarded to small businesses, excluding noneligible contracts as described in subsection I.B.  $Y_{sc2011}$  is quarterly payroll measured in 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 main coefficient of interest,  $\beta_1$ , measures the sensitivity of payroll growth from 2011Q1 to 2015Q1 to the county-sector share of accelerated contracts in total payroll. Because  $\Delta \text{Log}Y_{sc}$  approximates  $\frac{Y_{sc2015} - Y_{sc2011}}{Y_{sc2011}}$ , and recalling that  $\text{Treatment}_{sc} = \frac{FA_{sc}}{Y_{sc2011}}$ ,  $\beta_1$  can also be interpreted as a cash-flow elasticity of payroll, namely, the additional payroll spent for each accelerated dollar of sales ( $FA_{sc}$ ).

We first estimate the effect of payment acceleration on payroll. Table VI 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) to (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 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 the working capital needed to sustain a dollar of sales to the government in half, the policy thus led to a significant growth in payroll.<sup>33</sup>

<sup>33</sup> We provide several robustness tests of this result. First, we separately estimate the effect of the reform on job creations and separations. In the Internet Appendix, we show that most of the effect comes from the former rather than the latter (Table IA.IX). We show that the magnitudes are

Table VI  
Direct Effect of Payment Acceleration: County × Sector Baseline

This table presents results of a difference-in-difference estimation in first-differences. We run county × sector-level ordinary least squares (OLS) regressions 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 and 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 and 2011Q1, normalized by 2011Q1 payroll, as well as additional county × sector controls, which include the share of small establishments, the log of average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation between employment growth and aggregate employment growth, log total employment, and log average earnings. In Panel B, the treatment variable is normalized by its cross-sectional standard deviation. The sample contains 3,120 counties and 18 sectors. Standard errors presented in parentheses are clustered at the commuting zone level. \*, \*\*, and \*\*\* denote significance at the 10%, 5%, and 1% level, respectively.

	Δ log payroll		Δ log employment		Δ log earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline						
Treatment	0.095*** (0.032)	0.070** (0.031)	0.078*** (0.027)	0.057** (0.026)	0.017 (0.014)	0.012 (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	44,484	44,484	44,484	44,484	44,484	44,484
R <sup>2</sup>	0.159	0.184	0.140	0.173	0.172	0.244
Panel B: Standardized Treatment						
Treatment (std)	0.013*** (0.005)	0.010** (0.004)	0.011*** (0.004)	0.008** (0.004)	0.002 (0.002)	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	44,484	44,484	44,484	44,484	44,484	44,484
R <sup>2</sup>	0.159	0.184	0.140	0.173	0.172	0.244

unchanged when we run a differences-in-differences specification rather than a first-differences specification (Table IA.X), when we restrict the sample to county\* sectors with positive government contracts and positive treatment (Table IA.XI), when we use dummies for treatment rather than a continuous treatment variable (Table IA.XII), 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 preperiod contracts (Table IA.XIII, when we control for the amount of loans granted by the SBA (Table IA.XIV), or when we run our tests at the county-by-four-digit sector level to include tighter industry fixed effects (Table IA.XV). Moreover, we show in Table IA.XVI that the employment response is stronger in county\* sectors facing tighter financing constraints. We find no prior trends in the payroll response (Table IA.XVII), and falsification tests show that only exposure to eligible government contracts (rather than any government contracts) drive the results.



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 VI 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 Bureau of Economic Analysis (BEA) input-output data, a firm with 100% of its *sales* affected by the reform would therefore experience a  $0.057 \times 3 = 17\%$  increase in employment, which is remarkably close to the 20% employment growth obtained from our establishment-level analysis. As can be seen from Table V, this suggests a cost of external finance of 40% annually.

This estimate compares relatively well with the implicit interest rate on trade credit contracts. These typically allow the buyer to pay within 30 days, and to receive a 2% discount if payment occurs before 20 days (“2/10-net 30”). A 2% discount for 10-day acceleration implies an annual interest rate of 37%, which is close to our estimate. Our estimate for  $R$  is also relatively close to, albeit slightly larger than the rate charged by asset-backed lenders, which typically ranged between 4% and 5% monthly or 18% to 30% annually at the time of the reform (Mount (2012)).<sup>34</sup>

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 argument does not hold, however, for at least two reasons. First, under the Federal Government Assignment of Claims Act of 1940 (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 of both in order to obtain a security interest on a government receivable. Loan agreements typically exclude government receivables from the computation of the borrowing base, unless these receivables have been properly assigned. Eligible accounts often exclude those “from the United States or any department, agency or instrumentality thereof (unless there has been compliance, to Bank’s satisfaction,

<sup>34</sup> 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 six 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, that is, 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.

with the Federal Assignment of Claims Act of 1940, as amended”).<sup>35</sup> Second, while it is likely true that the federal government is a more reliable customer than many firms in the economy, typical government contracts include provisions that allow the purchasing agency to arbitrarily terminate the contract for convenience or for failure to obtain the necessary budget.

### *C. Local Labor Market Analysis*

#### *C.1. Tests at the Commuting Zone Level*

Our analysis so far documents substantial responses to treatment in creditworthiness, delinquencies, and employment, consistent with the treated firms in our sample facing financing frictions. Since the stated objective of the reform was to stimulate employment growth, we next examine whether these results aggregate to the labor market as a whole. The model presented in Section I makes clear predictions about aggregate employment growth. In particular, the model predicts that by relaxing treated firms' financing frictions, it increases their labor demand therefore. However, the increases in labor demand pushes up wages, mitigating employment growth at both treated and untreated firms. The extent of 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 should respond less and equilibrium employment should respond more. The overall effect of the reform therefore depends on this elasticity as well as on the share of treated versus untreated firms.

To analyze the effect that the reform had on local labor markets across the United States, we study the employment response across commuting zones. The entire land area of the United States comprises a total of 709 commuting zones, which represent labor market clusters of U.S. counties. In Table VII, we rerun equation (12) at the commuting zone, rather than the county-sector level.<sup>36</sup> We find that the effect of treatment remains positive and statistically distinguishable from zero even at the local labor market level (columns (1) and (2)).

We next 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 unemployed workers, will have less of an effect on wages, and therefore will 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 and low labor market tightness. The results, reported in columns (3) and (4), show that the results obtained in columns (1) and (2) are driven by slack labor markets (where vacancies are

<sup>35</sup> As an example, see page 36 of the loan agreement between Silicon Valley Bank and Motricity, Inc., dated as of April 13, 2009, and available from the Securities and Exchange Commission's website, [www.sec.gov](http://www.sec.gov).

<sup>36</sup> All controls are defined at the commuting zone level. We standardize treatment variable by its cross-sectional standard deviation.

**Table VII**  
**Employment Effects at Aggregated Levels**

This table presents results of a difference-in-difference estimation in first-differences. We run commuting-zone-level ordinary least squares (OLS) regressions 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 and 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 and 2011Q1, normalized by 2011Q1 payroll, as well as additional-commuting-zone-level controls, which include the share of small establishments, the log of average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation between employment growth and aggregate employment growth, log total employment, and log average earnings. Labor market tightness is measured at the commuting zone level as the ratio of the number of vacancies to the number of unemployed workers in 2010. High (low) labor market tightness commuting zones are those above (below) the sample median. Robust standard errors are presented in parentheses. \*, \*\*, and \*\*\* denote significance at the 10%, 5%, and 1% level, respectively.

	$\Delta$ log employment (2011Q1 to 2015Q1)			
Treatment	0.016*	0.025**		
	(0.009)	(0.010)		
Treatment $\times$ low labor market tightness			0.029***	0.032***
			(0.009)	(0.009)
Treatment $\times$ high labor market tightness			-0.002	0.006
			(0.010)	(0.012)
CZ level controls	No	Yes	No	Yes
Observations	693	693	693	693
R <sup>2</sup>	0.008	0.228	0.048	0.241

high relative to unemployment). We find little to no effect on employment in tight labor markets, consistent with crowding out effects offsetting the direct effects of the reform.

### *C.2. Crowding Out Effects*

We next look for direct evidence of such crowding out. To do so, we augment our baseline specifications at the county-sector level reported in Table VI with a measure of total accelerated dollars at the commuting zone level. More specifically, 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 and 2011Q1, but excluding the focal county-sector. We normalize this measure by aggregate quarterly payroll in 2011Q1, also excluding the focal county-sector. Controlling for the treatment at the county-sector level, this measure therefore picks up the effect of treatment on *other* county-sectors in a given commuting zone relative to the focal county-sector. If crowding out is salient, then treatment in other county-sectors within the same commuting zone will negatively impact employment growth in the focal county-sector, in which case we should obtain a negative coefficient in

Table VIII  
Crowding Out Effect of Payment Acceleration: County × Sector  
Baseline

This table presents results of a difference-in-difference estimation in first-differences. We run county × sector-level ordinary least squares (OLS) regressions 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 and 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 and 2011Q1, normalized by 2011Q1 payroll, as well as additional county×sector controls, which include the share of small establishments, the log of average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation between employment growth and aggregate employment growth, log total employment, and log average earnings. CZ controls include the unemployment rate, the share of small establishments, the log of average establishment size, log total employment, and log average earnings in 2011Q1. The sample contains 3,120 counties and 18 sectors. Standard errors presented in parentheses are clustered at the commuting zone level. \*, \*\*, and \*\*\* denote significance at the 10%, 5%, and 1% level, respectively.

	Δ log payroll		Δ log employment		Δ log earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.012*** (0.004)	0.010** (0.004)	0.010*** (0.003)	0.009*** (0.003)	0.002 (0.002)	0.001 (0.001)
Treatment: CZ	−0.012*** (0.004)	−0.010** (0.004)	−0.008*** (0.003)	−0.008*** (0.003)	−0.004** (0.002)	−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	44,484	44,484	44,484	44,484	44,484	44,484
R <sup>2</sup>	0.065	0.085	0.048	0.074	0.104	0.178

our estimations. 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.<sup>37</sup>

As can be seen from the first row of Table VIII, treatment at the county-sector level maintains its significance and economic magnitude. A one

<sup>37</sup> Because we now include a commuting-zone-level measure as a regressor, we do not include county fixed effects and instead include state fixed effects.

**Table IX**  
**Crowding Out Effect of Payment Acceleration: County  $\times$  Sector Dynamics**

This table presents results of a difference-in-difference estimation in first-differences. We run county  $\times$  sector-level ordinary least squares (OLS) regressions of the change in log employment on county  $\times$  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  $\times$  sector between 2009Q1 and 2011Q1, normalized by 2011Q1 payroll. *Treatment: CZ* is the same variable, measured at the commuting zone level rather than the county  $\times$  sector level, and excluding the focal county  $\times$  sector. *Treatment* and *Treatment: CZ* are normalized by their cross-sectional standard deviation. County  $\times$  sector control variables include the average quarterly amount of all government contracts to be performed in a given county  $\times$  sector between 2009Q1 and 2011Q1, normalized by 2011Q1 payroll, as well as additional county  $\times$  sector controls, which include the share of small establishments, the log of average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation between employment growth and aggregate employment growth, log of total employment, and log average earnings. CZ controls include the unemployment rate, the share of small establishments, the log of average establishment size, log total employment, and log average earnings in 2011Q1. The sample contains 3,120 counties and 18 sectors. Standard errors presented in parentheses are clustered at the commuting zone level. \*, \*\*, and \*\*\* denote significance at the 10%, 5%, and 1% level, respectively.

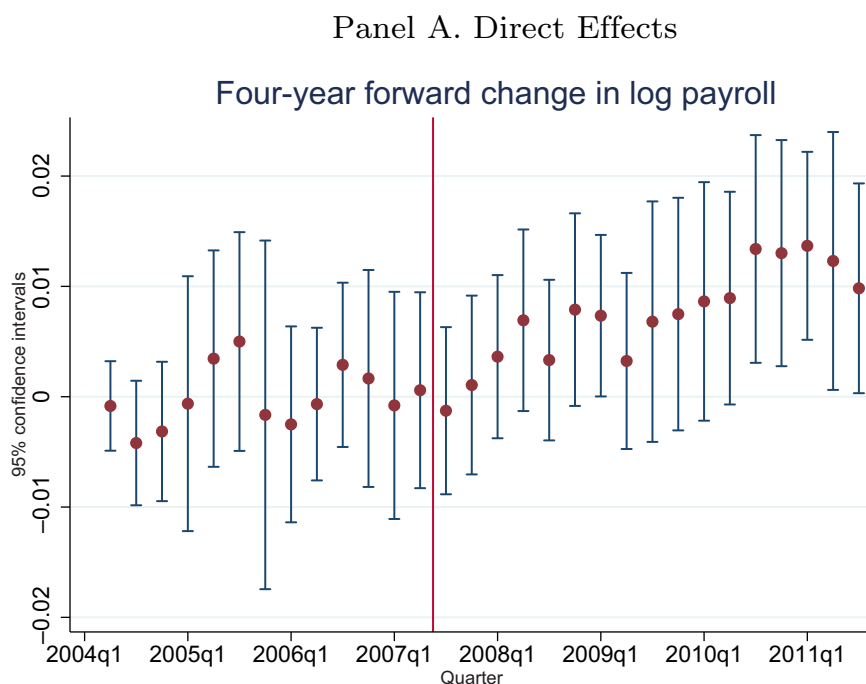
	$\Delta$ log employment (Relative to 2011Q1)					
	$[t - 2, t]$	$[t - 1, t]$	$[t, t + 1]$	$[t, t + 2]$	$[t, t + 3]$	$[t, t + 4]$
Treatment	0.003 (0.002)	0.001 (0.002)	0.004* (0.002)	0.005* (0.003)	0.007** (0.003)	0.009*** (0.003)
Treatment: CZ	0.000 (0.001)	-0.000 (0.001)	-0.003 (0.002)	-0.006*** (0.002)	-0.007*** (0.002)	-0.008*** (0.003)
County $\times$ 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	44,369	44,349	44,368	44,344	44,367	44,484
$R^2$	0.544	0.267	0.038	0.050	0.064	0.074

standard deviation in *Treatment* is associated with a 1% increase in payroll, a 0.9% increase in employment, and a 0.1% increase 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 increase in treatment at the commuting zone level is associated with a 1% decrease in payroll and 0.8% decrease in employment in the focal county-sector. Little or no effect is found on earnings.<sup>38</sup> These results thus provide direct evidence of crowding out.

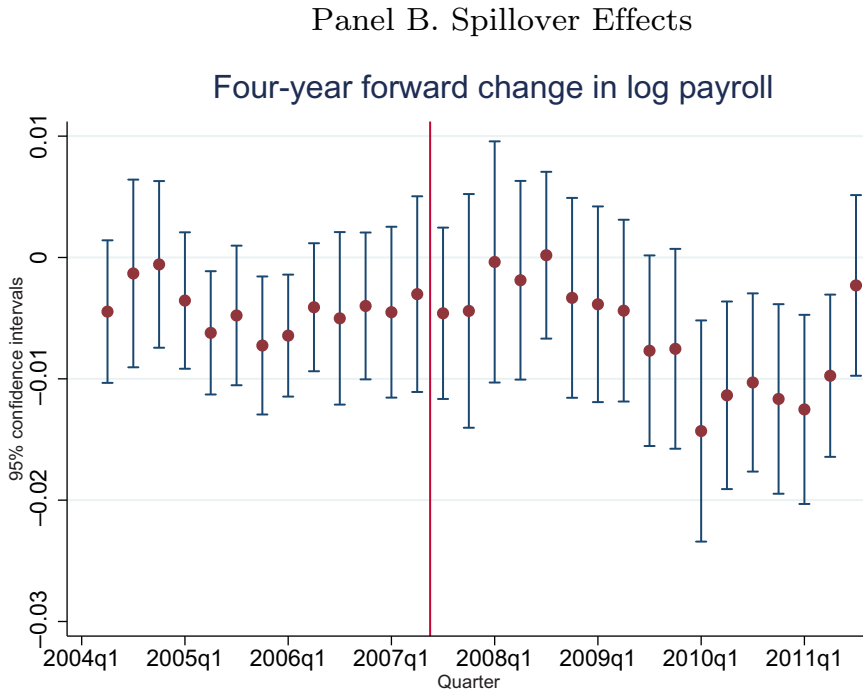
Key to the identification of the coefficient on *Treatment: CZ* is the parallel trend assumption. In the absence of the reform, there should be no difference

<sup>38</sup> 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 decrease, like employment.

in the employment behavior of firms as a function of treatment. While this assumption cannot be formally tested, we can check whether any differential trends exist *prior* to the implementation of *Quickpay*. In Table IX, we run the same regressions as in Table VIII over several windows surrounding the implementation of the reform. We fail to find any pretrends in employment growth, either in terms of direct effects or in terms of crowding out. The direct effect of the reform becomes statistically significant in 2012, and the spillover effects in 2013. In Panel B of Figure 2, we present results for the specification presented in Table IX, column (3), for each quarter from 2004 to 2011. We present the point estimates along with 95% confidence intervals. The red line denotes 2007Q1, the quarter after which four-year forward changes in log employment include the postreform period. Prior to this point, the estimates are not



**Figure 2. Direct and spillover effects.** This figure shows the direct effects (Panel A) and spillover effects (Panel B) of payment acceleration on four-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  $\times$  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  $\times$  sector between 2009Q1 and 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  $\times$  sector level, and excluding the focal county  $\times$  sector. *Treatment* and *Treatment: CZ* are normalized by their cross-sectional standard deviation. The point estimates are presented along with 95% confidence intervals. (Color figure can be viewed at [wileyonlinelibrary.com](http://wileyonlinelibrary.com))



**Figure 2.** *Continued*

different from zero. Afterward, the point estimates decrease until they reach their lowest value in 2011Q1, as expected.

Our main specifications are run in long-differences. In Internet Appendix Table IA.XVIII, we test for the presence of pretrends in year-by-year first-difference specifications in the nine years around the reform. More specifically, we run ordinary least squares (OLS) regressions of yearly changes in employment on the treatment variables interacted with year dummies. We find no direct effect nor any crowding out effect on employment before the reform. Moreover, consistent with the findings in Table IX and Figure 2, the crowding out effect shows up slightly later than the direct effect, and most of the employment effects of *Quickpay* accumulate in the first two years.

We provide several robustness tests for our estimates of crowding out in the Internet Appendix. First, in Table IA.XIX, we show that crowding out is observed in the establishment sample, and that they are also observed with a lag relative to the direct effects. In Table IA.XX, we run falsification tests and find that crowding out is only found in commuting zones with a large share of government contractors that were eligible to the payment acceleration. Conversely, the presence of noneligible government contractors does not crowd out



Table X  
Crowding Out Effect of Payment Acceleration: Labor Market Tightness

This table presents results of a difference-in-difference estimation in first-differences. We run county  $\times$  sector-level ordinary least squares (OLS) regressions of the change in log employment on county  $\times$  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  $\times$  sector between 2009Q1 and 2011Q1, normalized by 2011Q1 payroll. *Treatment: CZ* is the same variable, measured at the commuting zone level rather than the county  $\times$  sector level, and excluding the focal county  $\times$  sector. *Treatment* and *Treatment: CZ* are normalized by their cross-sectional standard deviation. County  $\times$  sector control variables include the average quarterly amount of all government contracts to be performed in a given county  $\times$  sector between 2009Q1 and 2011Q1, normalized by 2011Q1 payroll, as well as additional county  $\times$  sector controls, which include the share of small establishments, the log of average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation between employment growth and aggregate employment growth, log total employment, and log average earnings. CZ controls include the unemployment rate, the share of small establishments, the log of average establishment size, log total employment, and log average earnings in 2011Q1. Labor market tightness is measured at the commuting zone level as the ratio of the number of vacancies to the number of unemployed workers in 2010. High (low) labor market tightness commuting zones are those above (below) the sample median. The sample contains 3,120 counties and 18 sectors. Standard errors presented in parentheses are clustered at the commuting zone level. \*, \*\*, and \*\*\* denote significance at the 10%, 5%, and 1% level, respectively.

	$\Delta$ log employment (2011Q1 to 2015Q1)			
Treatment $\times$ low	0.010***	0.009**	0.011***	0.009**
labor market tightness	(0.004)	(0.004)	(0.004)	(0.004)
Treatment $\times$ high	0.009	0.008	0.009	0.008
labor market tightness	(0.006)	(0.006)	(0.006)	(0.006)
Treatment: CZ $\times$ low			-0.005	-0.006
labor market tightness			(0.004)	(0.004)
Treatment: CZ $\times$ high			-0.018***	-0.018***
labor market tightness			(0.005)	(0.006)
County $\times$ 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	44,477	44,477	44,477	44,477
R <sup>2</sup>	0.047	0.073	0.048	0.074

growth in employment.<sup>39</sup> Taken together, these analyses provide confidence that the effects we document 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 I suggests that crowding out should concentrate in tight local labor markets. We therefore also classify the commuting zones in our sample into those with relatively high versus low labor market

<sup>39</sup> 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.

Table XI

**Crowding Out Effect of Payment Acceleration: Job-to-Job Flows**

This table presents results of a difference-in-difference estimation in first-differences. We run ordinary least squares (OLS) cross-sectional regressions at the level of state  $\times$  origin sector  $\times$  destination sector. The dependent variable, *Job flows*, is defined as total job flows from the origin sector to the 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 and 2011Q1, normalized by 2011Q1 quarterly payroll. In Panel A, the 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  $\times$  sector between 2009Q1 and 2011Q1, normalized by 2011Q1 payroll, as well as additional state  $\times$  sector controls, which include the share of small establishments, the log average establishment size, 2000Q1 to 2011Q1 average annual employment growth, the correlation between employment growth and aggregate employment growth, log total employment, and log average earnings. Standard errors are presented in parentheses and are clustered at the commuting zone level. \*, \*\*, and \*\*\* denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Job Flows (2011Q2 to 2015Q1), Separate Treatment Variables					
Treatment, destination	0.190*** (0.046)	0.048*** (0.015)	0.021 (0.018)	0.065*** (0.018)	0.028 (0.017)
Treatment, origin	0.037 (0.024)	-0.043** (0.018)	-0.016 (0.016)	-0.027 (0.021)	-0.010 (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	14,990	14,689	14,689	14,689	14,689
$R^2$	0.271	0.540	0.593	0.553	0.599

Panel B: Job Flows (2011Q2 to 2015Q1), Difference in Treatment Variables					
Difference in treatment (destination-origin)	0.076*** (0.017)	0.046*** (0.007)	0.019** (0.009)	0.046*** (0.007)	0.019** (0.009)
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	14,990	14,689	14,689	14,689	14,689
$R^2$	0.265	0.540	0.593	0.553	0.599

tightness, as measured by the ratio of the number of vacancies to the number of unemployed workers in 2010.<sup>40</sup> Table X presents the results of these regressions 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

<sup>40</sup> Labor market tightness is strongly negatively related with unemployment rates and our results are unchanged when we use unemployment rates instead.

strongly and that there is no measured effect in terms of crowding out. On the other hand, in commuting zones with tight labor markets in 2010, the presence of treated firms does crowd out the employment growth of local firms.

Finally, we trace the movement of workers using recently released data on job-to-job flows from the LEHD database which includes the origin and destination sectors of people changing jobs within a given state. We run cross-sectional OLS regressions at the state  $\times$  origin sector  $\times$  destination sector as follows:

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

where  $JobFlow_{s,o,d}$  is defined as total job flows from origin sector  $o$  to destination sector  $d$  in state  $s$  from 2011Q2 to 2015Q1 normalized by 2011Q1 employment in sector  $d$  in State  $s$ .  $Treatment_{s,o}$  is the treatment for origin sector  $o$  in State  $s$ , and  $Treatment_{s,d}$  is the treatment for destination sector  $d$  in State  $s$ . As can be seen from Panel A of Table XI, 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 XI examines the difference in treatment intensity between the destination and origin sectors and shows that the difference strongly predicts job flows. These results indicate that the reform led to a reallocation of labor from low to high treatment sectors. More generally, the results provide strong evidence that firms in different sectors compete in common local factor markets, leading to crowding out effects when some firms face a reduction in financing constraints.

#### IV. Conclusion

In this paper, we analyze the impact of the *Quickpay* reform of 2011. We show that despite being paid only 15 days more quickly, the decrease in the need to finance working capital through the production process had substantial effects on employment. We trace the effect of this improvement in corporate liquidity to an improved ability to pay their suppliers on a timely basis and greater employment growth relative to firms that did not benefit from the reform. A unique aspect 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 in treated firms can crowd out employment growth in other firms that compete in common labor markets. In tight local labor markets, such crowding out can completely offset the direct 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 implications for policy makers. While accelerating payments is a direct way for governments 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.

Initial submission: February 2, 2017; Accepted: July 2, 2019  
Editors: Stefan Nagel, Philip Bond, Amit Seru, and Wei Xiong

## REFERENCES

- Acemoglu, Daron, 2010, Theory, general equilibrium, and political economy in development economics, *Journal of Economic Perspectives* 24, 17–32.
- Adelino, Manuel, Song Ma, and David Robinson, 2017, Firm age, investment opportunities, and job creation, *Journal of Finance* 72, 999–1038.
- Agrawal, Ashwini K., and David A. Matsa, 2013, Labor unemployment risk and corporate financing decisions, *Journal of Financial Economics* 108, 449–470.
- Angrist, Joshua D., and Jörn-Steffen Pischke, 2008, *Mostly Harmless Econometrics: An Empiricist's Companion* (Princeton University Press, Princeton).
- Antras, Pol, and C. Fritz Foley, 2015, Poultry in motion: A study of international trade finance practices, *Journal of Political Economy* 123, 853–901.
- Barrot, Jean-Noel, 2015, Trade credit and industry dynamics: Evidence from trucking firms, *Journal of Finance* 71, 1975–2016.
- Benmelech, Efraim, Nittai K. Bergman, and Ricardo J. Enriquez, 2012, Negotiating with labor under financial distress, *Review of Corporate Finance Studies* 1, 28–67.
- Benmelech, Efraim, Nittai K. Bergman, and Amit Seru, 2011, Financing labor, NBER Working Paper 17144.
- Bernanke, Ben S., 2010, Restoring the flow of credit to small businesses, Federal Reserve Meeting Series: “Addressing the Financing Needs of Small Businesses,” July 12.
- Biais, Bruno, and Christian Gollier, 1997, Trade credit and credit rationing, *Review of Financial Studies* 10, 903–937.
- Blanchard, Olivier Jean, Florencio Lopez-de Silanes, and Andrei Shleifer, 1994, What do firms do with cash windfalls? *Journal of Financial Economics* 36, 337–360.
- Bordo, Michael, and John Duca, 2018, The impact of the Dodd-Frank Act on small businesses, NBER Working Paper 24501.
- Breza, Emily, and Andres Liberman, 2017, Financial contracting and organizational form: Evidence from the regulation of trade credit, *Journal of Finance* 72, 291–324.
- Burkart, Mike, and Tore Ellingsen, 2004, In-kind finance: A theory of trade credit, *American Economic Review* 94, 569–590.
- Card, David, 1992, Using regional variation in wages to measure the effects of the federal minimum wage, *Industrial & Labor Relations Review* 46, 22–37.
- Chaney, Thomas, David Sraer, and David Thesmar, 2012, The collateral channel: How real estate shocks affect corporate investment, *American Economic Review* 102, 2381–2409.
- Chodorow-Reich, Gabriel, 2014, The employment effects of credit market disruptions: Firm-level evidence from the 2008–9 financial crisis, *Quarterly Journal of Economics* 129, 1–59.
- Cohen, Lauren, and Christopher Malloy, 2016, Mini West Virginias: Corporations as government dependents, SSRN Working Paper 2758835.
- Cunat, Vicente, 2007, Trade credit: Suppliers as debt collectors and insurance providers, *Review of Financial Studies* 20, 491–527.
- Dass, Nishant, Jayant Kale, and Vikram Nanda, 2015, Trade credit, relationship-specific investment, and product-market power, *Review of Finance* 19, 1867–1923.
- Demircuc-Kunt, Asli, and Vojislav Maksimovic, 2001, Firms as financial intermediaries: Evidence from trade credit data, World Bank Working Paper Series 2696.
- Dennis, William, 2006, National small business poll: Payroll, NFIB Small Business Poll 6.

- Dobridge, Christine L., 2016, Fiscal stimulus and firms: A tale of two recessions, Federal Reserve Board, Finance and Economics Discussion Series 2016-013.
- Faulkender, Michael, and Mitchell Petersen, 2012, Investment and capital constraints: Repatriations under the American Jobs Creation Act, *Review of Financial Studies* 25, 3351–3388.
- Fazzari, Steven M., R. Glenn Hubbard, and Bruce C. Petersen, 1988, Financing constraints and corporate investment, *Brookings Papers on Economic Activity* 1988, 141–206.
- Ferraz, Claudio, and Frederico Finan, 2015, Procuring firm growth: The effects of government purchases on firm dynamics, NBER Working Paper 21219.
- Frank, Murray Z., and Vojislav Maksimovic, 2005, Trade credit, collateral, and adverse selection, SSRN Working Paper 87868.
- Garcia-Appendini, Emilia, and Judit Montoriol-Garriga, 2013, Firms as liquidity providers: Evidence from the 2007-2008 financial crisis, *Journal of Financial Economics* 109, 272–291.
- Giannetti, M., M. Burkart, and T. Ellingsen, 2011, What you sell is what you lend? Explaining trade credit contracts, *Review of Financial Studies* 24, 1261–1298.
- Goldman, Jim, 2020, Government as customer of last resort: The stabilizing effect of government purchases on firms, *Review of Financial Studies* 33, 610–643.
- Greenstone, Michael, Alexandre Mas, and Hoai-Luu Nguyen, 2020, Do credit market shocks affect the real economy? Quasi-experimental evidence from the Great Recession and “normal” economic times, *American Economic Journal: Economic Policy* 12, 200–225.
- Hall, Robert E., 2017, High discounts and high unemployment, *American Economic Review* 107, 305–330.
- Hamermesh, Daniel S., 1989, Labor demand and the structure of adjustment costs, *American Economic Review* 79, 674–689.
- Hamermesh, Daniel S., and Gerard A. Pfann, 1996, Adjustment costs in factor demand, *Journal of Economic Literature* 34, 1264–1292.
- Holmstrom, Bengt, and Jean Tirole, 1997, Financial intermediation, loanable funds, and the real sector, *Quarterly Journal of Economics* 112, 663–691.
- Horton, John J., 2008, Procurement, incentives and bargaining friction: Evidence from government contracts, SSRN Working Paper 1094622.
- House, Christopher L., and Matthew D. Shapiro, 2008, Temporary investment tax incentives: Theory with evidence from bonus depreciation, *American Economic Review* 98, 737–768.
- Ivashina, Victoria, and David Scharfstein, 2010, Bank lending during the financial crisis of 2008, *Journal of Financial Economics* 97, 319–338.
- Iyer, Rajkamal, Jose-Luis Peydro, Samuel da Rocha-Lopes, and Antoinette Schoar, 2014, Interbank liquidity crunch and the firm credit crunch: Evidence from the 2007–2009 crisis, *Review of Financial Studies* 27, 347–372.
- Jermann, Urban, and Vincenzo Quadrini, 2012, Macroeconomic effects of financial shocks, *American Economic Review* 102, 238–271.
- Kim, Se-Jik, and Hyun Song Shin, 2012, Sustaining production chains through financial linkages, *American Economic Review* 102, 402–406.
- Klapper, Leora, Luc Laeven, and Raghuram Rajan, 2012, Trade credit contracts, *Review of Financial Studies* 25, 838–867.
- Lamont, Owen, 1997, Cash flow and investment: Evidence from internal capital markets, *Journal of Finance* 52, 83–109.
- Lieberman, Jeffrey B., and Neale Mahoney, 2017, Do expiring budgets lead to wasteful year-end spending? Evidence from federal procurement, *American Economic Review* 107, 3510–3549.
- Matsa, David A., 2010, Capital structure as a strategic variable: Evidence from collective bargaining, *Journal of Finance* 65, 1197–1232.
- Mills, Karen, and Brayden McCarthy, 2014, The state of small business lending: Credit access during the recovery and how technology may change the game, Harvard Business School Working Paper 15-004.
- Mount, Ian, 2012, When banks won't lend, there are alternatives, though often expensive, *New York Times*, August 1.
- Murfin, Justin, and Ken Njoroge, 2015, The implicit costs of trade credit borrowing by large firms, *Review of Financial Studies* 28, 112–145.

- Paravisini, Daniel, Veronica Rappoport, Philipp Schnabl, and Daniel Wolfenzon, 2015, Dissecting the effect of credit supply on trade: Evidence from matched credit-export data, *Review of Economic Studies* 82, 333–359.
- Petersen, Mitchell A., and Raghuram G. Rajan, 1997, Trade credit: Theories and evidence, *Review of Financial Studies* 10, 661–691.
- Petrosky-Nadeau, Nicolas, and Etienne Wasmer, 2013, The cyclical volatility of labor markets under frictional financial markets, *American Economic Journal: Macroeconomics* 5, 193–221.
- Rauh, Joshua D., 2006, Investment and financing constraints: Evidence from the funding of corporate pension plans, *Journal of Finance* 61, 33–71.
- Schoefer, Benjamin, 2015, The financial channel of wage rigidity, Harvard Graduate School of Arts & Sciences Dissertation.
- Stiglitz, Joseph E., and Andrew Weiss, 1981, Credit rationing in markets with imperfect information, *American Economic Review* 71, 393–410.
- Wasmer, Etienne, and Philippe Weil, 2004, The macroeconomics of labor and credit market imperfections, *American Economic Review* 94, 944–963.
- Whited, Toni M., 1992, Debt, liquidity constraints, and corporate investment: Evidence from panel data, *Journal of Finance* 47, 1425–1460.
- Wilner, Benjamin S., 2000, The exploitation of relationships in financial distress: The case of trade credit, *Journal of Finance* 55, 153–178.
- Yellen, Janet, 2013, Interconnectedness and systemic risk: Lessons from the financial crisis and policy implications, AEA/AFA Joint Luncheon, January 4.
- Zwick, Eric, and James Mahon, 2017, Tax policy and heterogeneous investment behavior, *American Economic Review* 107, 217–248.

### Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

**Appendix S1:** Internet Appendix.  
**Replication code.**