Curbing Shocks to Corporate Liquidity: The Role of Trade Credit*

Niklas Amberg † Tor Jacobson ‡ Erik von Schedvin § Robert Townsend ¶

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Abstract

Using data on liquidity shortfalls generated by the fraud and failure of a cash-in-transit firm, we demonstrate effects on firms' trade credit usage. We find that firms manage liquidity shortages by increasing the amount of drawn credit from suppliers and decreasing the amount issued to customers. The compounded trade credit adjustments are on average of similar magnitude as corresponding adjustments in cash holdings, suggesting that trade credit positions are economically important sources of reserve liquidity for firms. The underlying mechanism in trade credit adjustments is in part due to shifts in overdue payments.

Keywords: Liquidity management; trade credit; cash holdings; cash flow; risk sharing.

JEL: D22; G30.

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†Sveriges Riksbank. E-mail: niklas.amberg@riksbank.se.

‡Sveriges Riksbank. E-mail: tor.jacobson@riksbank.se.

§Sveriges Riksbank and CEPR. E-mail: erik.vonschedvin@riksbank.se.

¶Massachusetts Institute of Technology. E-mail: rtownsen@mit.edu.

1 Introduction

Do firms use their trade credit positions to handle shortfalls in liquidity?[1] In an upstream perspective, Wilner (2000) and Cuñat (2007) propose that firms can draw reserve liquidity from their suppliers. Their idea is that firms experiencing a shock to liquidity can offset its effect by postponing payments on the trade credit claims held by their suppliers; or, alternatively, by increasing the maturity of future trade credit contracts; and both measures will generate liquidity through increased accounts payable, without necessarily affecting the volume of input purchases.[2] Suppliers may be willing to provide such reserve liquidity given rents that are derived from the maintenance of long-term relationships. We argue that this liquidity insurance mechanism may operate symmetrically. Thus, in a downstream perspective, firms can draw reserve liquidity from their customers. That is, firms can manage the trade credit claims held on customers for this purpose, by reversing the measures that apply upstream; either by reducing net days in future trade credit contracts, or by proactive monitoring and management of outstanding contracts to avoid overdue settlement of customer debts. Hence, the firm may thus seek to reduce its accounts receivable, unchanged sales notwithstanding. The economic importance of firms' ability to extract liquidity from upstream and downstream counterparties in the supply chain to overcome liquidity shocks, may well be on par with the significance of cash reserves and bank lines of credit. However, an empirical assessment of the extent to which firms rely on adjustment capacity at the trade credit margins is challenging, foremost due to the inherent difficulty in identifying liquidity shocks that are uncorrelated with confounding factors, such as demand conditions in the supply chain.

In search of an idiosyncratic shock to corporate liquidity, we rely on the case of the Swedish cash-in-transit firm Panaxia; its fraudulent behavior initiated in the spring of 2010, and subsequent failure in September 2012—with dire consequences for the

[1] Trade credit positions give rise to sizable financial assets and liabilities on firms' balance sheets. Jacobson and von Schedvin (2015) show that the average amount of accounts receivable and accounts payable, scaled by assets, are 16 and 11 percent, respectively, for Swedish firms. Such reliance on trade credit financing prevails across countries. For instance, Rajan and Zingales (1995) show that the corresponding numbers for receivables and payables are 18 and 15 percent, respectively, for a sample of US firms.

[2] Boissay and Gropp (2013) empirically show that firms experiencing late customer payments are more likely to postpone their own payments to suppliers, illustrating that trade credit chains may function as an insurance mechanism against liquidity shocks.

clients. The fraud implied that Panaxia withheld the clients' inflows of funds in breach of the parties' contracts and hence imposed temporary liquidity shortfalls, whereas the failure imposed permanent losses. The liquidity losses were non-negligible, taken as shares of the clients' total assets, and it can be argued that the surprise element was almost complete, suggesting that these were outcomes of an event that make them close in nature to the concept of an economic shock. The Panaxia sequence of events provides an opportunity to form insights on firms' management of liquidity shortfalls. We begin our empirical analyses by evaluating adjustments in aggregate accounting measures of the three liquidity sources concerned: cash holdings; the amount of drawn trade credit from suppliers, accounts payable; and the amount issued to customers, accounts receivable. We further exploit variation in bankruptcy loss-size to assess the impact of variation in treatment, and then proceed to evaluate whether constraints for external financing determine firms' usage of the different liquidity sources in adverse circumstances. Finally, we examine the underlying mechanisms by considering if adjustments in payables are associated with postponed settlement of trade credit debt to suppliers; and similarly if adjustments in receivables are related to intensified enforcement of repayment from overdue customers.

More generally, and as a basis for the empirical evaluation, we envision that firms in a risk-sharing network are subject to idiosyncratic, firm-specific shocks and to sectoral, or macro, aggregate shocks. If there were no obstacles to risk sharing, idiosyncratic shocks would be pooled within the network, leaving management of aggregate shocks to group-level cash management, or to external formal bank relationships. No doubt in practice there are obstacles that reduce the extent of risk sharing, such as limited information and limited commitment. In particular, firms may threaten non-cooperation, e.g., to pull out of the network if they are unwilling to provide the requisite liquidity of the implicit sharing rules. But such a threat might be mitigated by potential loss of established relationships within the current supply chain, given pre-established specificity in inputs, tailored monitoring technologies, and so on. Risk sharing is more valued, the more specific such relationships are. Nevertheless, threats may not be sufficient, and on some paths of shock realizations firms will file legal claims for recovery, or be forced themselves to consider bankruptcy. In sum, we are allowing both an ex ante risk sharing perspective and an ex post contagion perspective, simultaneously.

This then is the overall framework we have in mind and plausible identification of risk sharing in data is the empirical quest of this paper.

We conduct the empirical analyses on data comprising three key components. Firstly, the identities of clients and their claims at the time of Panaxia's failure were obtained from records provided by the bankruptcy trustee and from four savings banks involved. Secondly, accounting data for the universe of Swedish corporate firms, covering the period of interest, were provided by the leading Swedish credit bureau, Upplysningscentralen (UC). Thirdly, from the credit bureau UC, we also obtained data collected by the Swedish Enforcement Agency. These data contain information on all applications for the issuance of injunctions to enforce late trade credit payments in the Swedish corporate sector, and specifically include details on the subsequent outcomes of such applications.

The nature and scope of the Panaxia sequence of events make Abadie and Imbens' (2006) nearest-neighbor matching approach a suitable empirical setup for inference. A matching approach allows us to compare the adjustments in the outcome variables in response to the liquidity shortfalls imposed on the clients (the treated firms) with the adjustments undertaken by a group of matched control firms (the counterfactuals). In this framework, we carefully assess the plausibility of the underlying identifying assumptions to mitigate endogeneity concerns. The interpretation of the results may nevertheless hinge on the composition of treated firms; both with respect to the setting of this study—Swedish firms using a cash-in-transit (CIT) firm—and the particular sequence of events which could have imposed a selection on the type of firms that were exposed to treatment. To shed light on potential selection concerns we therefore detail how the pre-bankruptcy fraud was orchestrated by Panaxia's management, and the extent to which it affected the customer base over time.

Our baseline findings confirm that firms manage liquidity shortfalls by using their cash reserves, and by increasing the amount of drawn trade credit from suppliers, as well as contracting the amount of issued trade credit to customers. In terms of economic importance, both trade credit margins play significant roles, although increases in accounts payable are more pronounced than reductions in accounts receivable. Moreover, the compounded adjustment at the two trade credit margins—the increase in drawn credit, plus the reduction in granted credit—is on average of similar magnitude as the adjustment in cash holdings, suggesting that trade credit positions make for important sources of reserve liquidity, on par with cash reserves.

The complexity of the Panaxia events gives rise to differential treatments, which can be exploited to study heterogeneity in effects. A majority of the treated firms were exposed to both the liquidity shortfalls caused by the fraud and the subsequent bankruptcy losses, whereas a subset of the treated firms were exposed to the fraud only; moreover, for the group of firms that incurred losses we observe loss-sizes. By using variation in loss-size, we confirm the intuitively appealing notion that larger adjustments in cash and trade credit positions result when firms are exposed to more liquidity distress.

Moreover, our results suggest that credit constraints matter; adjustments in cash holdings and at the two trade credit margins can primarily be attributed to firms with a low to medium credit rating, whereas highly rated firms respond to the liquidity shortfalls by expanding their bank financing. This finding suggests that idiosyncratic liquidity shocks hitting financially constrained firms to some degree are being pooled by the trade credit networks—in line with the risk sharing perspective. Another important insight is the joint reliance on cash reserves and trade credit adjustments for constrained firms. Our interpretation of the joint usage is that in situations when liquidity is scarce, credit constrained firms can by extracting liquidity from suppliers and customers preserve the necessary cash reserves for executing prompt payments, such as expenditures for salaries or taxes. In other words, firms will need sufficient liquid means to service counterparties that are unwilling to extend credit.[3] Hence, cash and trade credit adjustments are used as complements to manage liquidity.

Finally, our investigation of the mechanism underlying adjustments in trade credit positions using the data from the Swedish Enforcement Agency reveals that adjustments in accounts payable are in part due to increases in overdue payments. More specifically, the propensity to postpone settlement of trade credit payments beyond the due date increases significantly for firms that are hit by liquidity shortfalls, as re-

[3]Since trade credit is invariably bundled with purchases of input goods or services, there are limits to its usefulness for liquidity management. Even if a firm can expand its trade credit by postponing payments to its suppliers, it will still need liquidity—cash, or bank financing—to cover expenditures to counterparties that are unwilling to extend credit, such as employees and tax authorities. Moreover, it is conceivable that shocks substantially larger than those generated by the Panaxia events could trigger additional and altogether different responses, such as sales of tangible or other assets.

flected by these firms being subject to more applications for injunctions submitted by their suppliers. We are, however, unable to document significant increases in firms' propensity to enforce existing overdue payments from customers, possibly reflecting that downstream liquidity adjustments are primarily made on the extension of new trade credit.

The applications for injunctions are associated with various outcomes of the enforcement process. We find that the significant increase in overdue claims held by the suppliers of treated firms, predominantly result in a subsequent withdrawal of the case from the Enforcement Agency. Consistent with a risk sharing view, this finding suggests a prevalence of co-operative outcomes in which the parties comply with the implicit rules of the trade credit network; despite an initial and formal involvement of the Enforcement Agency.

This paper aims to contribute to the vast literature on firms' choices of cash holdings, and liquidity management in general. Influential papers include Opler, Pinkowitz, Stulz and Williamson (1999), Almeida, Campello and Weisbach (2004), and Bates, Kahle and Stulz (2009), which study firms' choices of cash holdings in light of their access to external funding. Our paper is also close to Acharya, Davydenko and Strebulaev (2012), who investigate the relationship between firms' cash holdings and their default risks, suggesting a positive one. That is, all else equal, higher default risks incentivize firms to hold more cash, to safeguard against adverse cash flow shocks. We emphasize that firms—in addition to cash holdings and external financing—have trade credit liabilities and assets that can be used to improve their liquidity positions. To better understand how firms handle liquidity shocks, it is therefore important to also consider shifts at their trade credit margins.

As noted above, the role of trade credit for firms' liquidity management has partly been put forward by Cuñat (2007), who proposes that trade credit links function as a liquidity insurance mechanism by allocating liquidity from unconstrained suppliers to constrained customers in adverse situations, through delayed repayment of trade debt.[4] Cuñat shows empirically that large declines in firms' cash holdings are cor-

[4]The literature features what is known as the financing theory for the existence of trade credit, according to which credit is redistributed in trade credit chains from unconstrained firms to constrained counterparties, see Petersen and Rajan (1997) for a seminal contribution. In addition to the financing motive, a strand of the literature emphasizes other motives for the prevalence of trade credit. For example, Smith (1987) and Long, Malitz and Ravid (1993), focus on the guarantee role played by trade credit in providing

related with increases in their accounts payable. Bakke and Whited (2012) examine the impacts of cash shortfalls triggered by mandatory pension contributions on a wide set of firm-characteristics. They find that liquidity shortfalls cause contractions in the amount of issued trade credit. Another closely related paper by Garcia-Appendini and Montoriol-Garriga (2013) make use of the recent financial crisis to gauge how an aggregate contraction in bank credit supply affected trade credit provisioning for US firms. Consistent with the redistribution view of trade credit, they find that cash-rich firms, as compared with cash-poor firms, issued more trade credit during the crisis; and that firms with cash-rich suppliers, as compared with cash-poor suppliers, received more trade credit.[5] To varying degrees, these papers all study redistribution of liquidity in trade credit chains—as we do. However, our paper provides several extensions. Firstly, we furnish insights on the impact of liquidity shocks on firms' cash holdings, accounts payable, accounts receivable, and bank financing simultaneously, thus enabling an evaluation of the relative importance of these liquidity sources for firms' management of liquidity shortfalls. Secondly, our empirical setting—where liquidity shocks affect a small subset of firms in the economy—differ distinctly from previous papers that rely on aggregate shocks for identification. Thus, the Panaxia events allow for identification using the nearest-neighbour matching approach to precisely define a presumably comparable control group of firms that were unaffected by the shocks. In contrast, identification in a setting with aggregate shocks needs to rely on exogenous variation in the impact of the shocks across firms.[6] Moreover, our empirical framework is well-suited to examine our overarching presumption: that risk-sharing in trade credit networks enables firms to pool idiosyncratic shocks; whereas there should be less scope for risk-sharing in situations where firms are exposed to shocks that are aggregate in nature. Hence, our results are complementary to earlier findings in the literature and contribute towards a deeper understanding of firms' management of idiosyncratic shocks

buyers time for verification of purchase quality. Moreover, see Giannetti, Burkart and Ellingsen (2011) for a recent, comprehensive overview of trade credit theories.

[5] Similar results are also documented by Love, Preve and Sarria-Allende (2007), who evaluate the role of trade credit financing during crisis episodes in a set of emerging economies.

[6] The difficulty in separating liquidity shocks from confounding factors is a key challenge when assessing the role of trade credit for firms' liquidity management. One such important factor is fluctuations in demand, which stem from the inherent link between trade credit arrangements and activities in the supply chain. The events considered in this paper provide a setting where the shocks are uncorrelated with conditions in the supply chain, whereas a corresponding separation becomes more cumbersome in the case of aggregate shocks.

that feature elements of liquidity shortfalls, such as cash flow shocks—which have been widely considered in the corporate finance literature.

A partly related literature considers the role of liquidity provisioning within business groups, see, e.g., Almeida, Kim and Kim (2015), Gopalan, Nanda and Seru (2007), Karaivanov, Ruano, Saurina and Townsend (2012), and Samphantharak (2009). Gopalan, Nanda and Seru (2007), for example, show that firms belonging to business groups engage in risk sharing where inter-group cash transfers is used to support distressed firms within the group. On the household side, Kinnan and Townsend (2012) use data on rural Thai households and show that indirect access to bank financing, through inter-household borrowing, mitigates income risk by reducing the association between income fluctuations and consumption. In analogy, our results suggest that firms engage in risk sharing through informal ties with their suppliers and customers in the supply chain. However, liquidity provisioning in trade credit networks is also associated with costs. Such costs have been highlighted in the financial network literature, arguing that counterparty exposures may cause shock propagation and—in extension—potential systemic failure, see, e.g., Allen and Gale (2000) and Acemoglu, Ozdaglar and Tahbaz-Salehi (2015). Empirically, in a trade credit context, Jacobson and von Schedvin (2015) use Swedish firm data similar to the current data, to study firm-failure propagation in trade credit chains. They show that suppliers who are exposed to credit losses due to failing customers, are in turn subject to an elevated risk of failure. Hence, the financial networks of suppliers and customers arising through trade credit have two closely related features; ex ante risk sharing through liquidity provisioning, on the one hand, and ex post failure propagation on the other.

The remainder of this paper is structured as follows. Section 2 outlines the Panaxia events, details our various data resources, and describes the empirical approach. Sections 3 and 4 present the empirical analyses and results outlined above, on adjustments in cash holdings and at trade credit margins, and the underlying mechanisms for the latter, respectively. Section 5 concludes.

2 The Panaxia Events, Data, and Empirical Approach

The Panaxia events were extreme outcomes of criminal offenses that caused substantial hardship for the clients involved; however, they also generated suitable data for the questions we ask. In this section we will in some detail first describe the economics of the sequence of events, and then provide an account of the construction of the data. Finally, we will explain the empirical approach subsequently pursued.

2.1 The cash-in-transit firm Panaxia, its fraud and failure

Panaxia was one of three leading cash-in-transit firms operating in Sweden. It serviced its clients—mostly, but not exclusively, in the retail sector—by collecting their daily receipts at their premises.[7] Collected receipts were then delivered to a bank depot for counting, and in one to two days, Panaxia credited the firms' bank accounts for the due funds. That is, according to the contracts between Panaxia and its clients, the latter would upon handing over the cash for transportation to the depot hold a claim on the former, until a transfer of funds to the clients' bank accounts had been carried out within a maximum of two days.

In the three-year period from 2006 to 2009, Panaxia expanded its operations forcefully; Table 1 shows that sales grew from SEK 197 million in 2006 to SEK 677 million in 2009, corresponding to a 244 percent increase. The quest for an increased market share was in part conducted through an aggressive pricing strategy, which in turn contributed towards operational losses. According to Table 1, profits started to decline in 2009 and large losses accrued in the following years. Due to the operational losses, Panaxia faced drastic contractions in the lending provided by its creditors; Table 1 shows that bank debt-to-assets in 2008 and 2009 declined from 62.2 to 42.8 percent, and further reductions in external funding occurred in 2010 and 2011.

[Insert Table 1 about here.]

To counteract the contraction in external financing, Panaxia initiated funding of its operations using the clients' funds that had been collected and counted at the de-

[7] In our final sample, 65 percent of the Panaxia clients operated in the retail sector; 16 percent in the hotel and restaurant sector; and the remaining 19 percent in sectors such as wholesale, auto mechanics, health care, and transportation.

pot, but not yet transferred to clients' bank accounts. Initially, in 2009, the scale of the scam was such that the contracted time-frame of 48 hours was not breached and clients remained unaffected.[8] However, over time the practice of delayed transfers of client funds escalated, and in the months prior to the bankruptcy that was finally declared on September 5, 2012, clients could face waiting times as long as 10 to 12 days before Panaxia transferred due funds. Figure 1 shows the average number of bank days over time required by Panaxia to transfer the due funds generated in cash collection to their clients' bank accounts. There is a distinct initial level shift; the number of bank days increased from, the agreed, two days in the beginning of 2010, to five days towards the end of that year. From the beginning of 2011 and towards the bankruptcy event, there is a slightly upward-sloping trend such that the average transfer time reached almost six days in the months prior to the failure. The sustainability of this Ponzi scheme hinged on Panaxia's ability to maintain the size of its customer base through competitive pricing.

[Insert Figure 1 about here.]

Sample selection is a potential concern for the analysis of the Panaxia sequence of events. That is, the prolonged period of delayed transfers in the pre-bankruptcy period may have introduced selection on type for clients that remained in relationships with Panaxia—such as financially weak firms—which could influence the scope of the empirical analysis. It is thus a fair question to ask whether the clients understood what was going on, or reacted to the drastically increased transfer periods. They did react, but very few actually ended their contracts with Panaxia.[9] The bankruptcy trustee describes a fraud setup where Panaxia's CEO cleverly orchestrated and executed delayed transfers so as to avoid raising clients' attention and annoyance. An example is the

[8] In rather cheeky and awkward wording, the innovative financing of operations was even mentioned in Panaxia's 2009 annual report: "A strong contribution towards reducing the business-group's borrowing was made by a completely new arrangement for the funding of a large part of the cash-handling operations that entered into use in June."

[9] The bankruptcy trustee and the interim CEO, who took over management in the final months prior to the bankruptcy, independently verify by firm-names that only two firms terminated their relationships with Panaxia in the pre-bankruptcy period. Their statements are confirmed by Panaxia's annual financial reports for the period 2007–2010, which provide examples of important clients recently enlisted, or with whom new contracts had been signed. In total, 19 non-financial Swedish firms are listed over these four years, and all except for the two named firms were to become exposed clients in Panaxia's bankruptcy 2012.

instruction to the customer-support staff to inform complaining clients that transfer holdups were temporary and simply due to technical problems. Figure B1 in Appendix B shows the number of collected receipts at a monthly frequency for the period 2006–2011. The expansion phase, from January 2006 to July 2008, is associated with a sharp increase in the number of collected receipts and is followed by a stable pattern hovering around 120,000 collected receipts in the period from July 2008 to December 2011, thus including the first two fraud years. Hence, Figure B1 indicates that the number of clients remained stable from mid-2008 and going forward. The persistence in the customer base in the period running up to the bankruptcy event mitigates selection concerns.

The interim CEO, who managed Panaxia in the final stages prior to the bankruptcy, offers three main reasons that help explain why virtually all clients upheld their relationships with Panaxia, despite prolonged transfer times: (i) Panaxia's logistics worked very smoothly and the clients appreciated the way on-site collections were carried out; (ii) it is an extensive and cumbersome process to switch CIT firm; and (iii) Panaxia's owners—of which two of the main shareholders were banks: Forex Bank and Sparbanken 1826—enjoyed much and widespread credibility. Although fundamentally anecdotal in its nature, the CEO statement points to circumstances that are plausible underpinnings of the lengthy Panaxia fraud. Moreover, the general credibility of Panaxia can be further appreciated by considering the fact that Sveriges Riksbank (the central bank of Sweden), two years into the fraud episode in early 2012 signed an agreement with Panaxia for purchases of coin collection and distribution services. This agreement was in place up until the arrest of the CEO of Panaxia, shortly before the bankruptcy, although no services were ever purchased by the central bank. Finally, a common view held by clients and cited in the press following the bankruptcy, concerned the absence of any expectations for a fraud of this magnitude from a large and well-established firm like Panaxia. By and large, deception by Panaxia's management in combination with high switching costs and the general credibility of Panaxia and its main owners are important factors in explaining the stickiness of the customer base, in spite of the prolonged transfer times caused by the fraud.

The fraud and failure of Panaxia were a sequence of events resulting in gradual deterioration of its clients' liquidity positions through disruptions of their cash flows.[10] The pre-bankruptcy period—characterized by an increased widening of the time-window between collection of cash and final transferal of funds to clients' accounts—successively shifted the clients towards a low-liquidity regime. More specifically, Panaxia's prolonging of transfer time introduced lags in the inflow of clients' cash flows. This lag gave rise to a mismatch in timing between the inflow of funds and the outflow of funds, such as payment of wages. In the post-bankruptcy period, two things happened. Firstly, final transfer of client funds held by Panaxia at the time of the bankruptcy were cancelled. This implied that the clients faced an immediate and significant shock to their cash flows. Secondly, the bankruptcy also had implications for the solvency of the clients, albeit not immediately. The bankruptcy trustee faced the intricate issue of establishing the Panaxia clients' rights with respect to the assets of the bankruptcy estate, as well as the factual amount of remaining assets. The former—and unprecedented—issue required an external inquiry involving legal expertise, which implied that the final resolution of the bankruptcy was delayed well into the following year. Hence, the failure caused an immediate shock to clients' liquidity, whereas the consequences for clients' solvency were realized in the spring of 2013.

The scope of the fraud became clear in the investigation undertaken by the bankruptcy trustee for the resolution of the Panaxia bankruptcy. A fraction corresponding to 23 percent of held claims were recovered from the bankruptcy estate by the trustee. These recoveries were paid out in mid-2013 to clients that at the time were still holding claims, i.e., had not been fully, or partially, compensated by other parties. Several top-managers involved in the Panaxia fraud were convicted in the aftermath. In 2015 and 2016, the former CEO was sentenced to pay out large damages to the bankruptcy estate and to several years of imprisonment for fraud, embezzlement, and fraudulent accounting practice.

[10]The service provided by Panaxia was to transfer clients' excess cash, as generated by sales, from the transaction location—e.g., a store for a retail firm—to the clients' bank accounts. The fraud therefore resulted in partial illiquidity of firms' inflow of funds. Now, Swedish accounting rules give firms discretion in the choice between booking cash-in-transfer directly under cash holdings, or alternatively, as a short-term claim on the CIT firm. Prevalence of the former practice has implications for the measurement of adjustments in cash holdings; more specifically, our estimates may underestimate treated firms' reliance on cash to balance the liquidity shocks in 2010 and 2011, but not in 2012. Appendix A provides a detailed outline of the accounting practices and how their usages affect the interpretation of estimated effects on cash holdings.

2.2 Data

In this subsection, we first outline how the Panaxia data have been collected and structured, and then proceed by describing the data sets obtained from the Swedish credit bureau, Upplysningscentralen AB.

2.2.1 Panaxia data

We have used data from three sources to construct the final Panaxia data set. The first source is the Lindahl law firm, appointed trustee of the Panaxia bankruptcy estate. The law firm provided two basic items: (i) a name list of all firms holding claims on Panaxia, and the size of each firm's claim at the time of the bankruptcy in September 2012 (*Item 1*); and (ii) a complete list of Panaxia's collection sites on the bankruptcy date (*Item 2*). Collection sites refer to the physical locations where Panaxia collected their clients' proceeds; many Panaxia clients operated in multiple locations, e.g., retail firms running several stores. The second source is due to the four savings banks that covered the losses endured by their customers in the Panaxia bankruptcy. These banks provided the identities of the customers that were affected by the bankruptcy, as well as the sizes of the losses that were covered by the banks (*Item 3*). A third source is the business register Retriever, which contains annual financial reports for all incorporated firms in Sweden, as well as some additional firm-level information. Retriever enables matching of the firm-names provided by the law firm and 10-digit firm identities, known as organization numbers, which in turn allow for unambiguous matching with firm-level data on yearly balance sheets and applications for injunctions to settle unpaid trade credit, provided by the credit bureau UC, as described below.

Thus, the basis for the final data set is the list of names of firms that held claims on Panaxia at the time of the bankruptcy as provided by the law firm, i.e., *Item 1*. However, this list has two shortcomings. Firstly, whereas the firm-names on the list coincide to a very large extent with the unique legal and official names of the involved corporate firms, there are plentiful exceptions which required manual identification of the correct legal entity by means of internet searching, e-mails, and telephone contacts. Secondly, a number of corporate firms that were clients of Panaxia and indeed held claims at the time of the bankruptcy do not appear on the name list. The reason for this is two-fold:

(i) Firms that were indirectly clients of Panaxia, through their relationships with one of four regional savings banks, were fully and almost immediately compensated for their losses in the Panaxia bankruptcy by these savings banks.[11] Hence, the list of firms include the four savings banks holding claims ex post the bankruptcy event, but not the 286 firms that were Panaxia clients in the period of postponed transfers, 2010–2012. The identities and claim-sizes for these 286 firms were given to us directly by the four banks under the information disclosure requirements stipulated by the Sveriges Riksbank Act. (ii) The name list has two entries that held very large claims on the Panaxia bankruptcy estate. It turns out that these entries refer to two franchisor groupings of pharmacies and convenience stores. Whereas we omit pharmacy franchisees from the analyses because they were predominantly start-ups in the treatment period and hence do not have financial statements for the pre-treatment period, the convenience store franchisees' identities and claims are included. The identities of the franchisees were obtained using the list of collection sites, *Item 2*, whereas their claims had to be approximated.[12]

Furthermore, in this context it is also worthwhile to highlight another potential obstacle, which is similar to the franchise group problem discussed above. Two entries on the name list (*Item 1*) relate to parent firms in business groups, whereas their subsidiaries are included in the list of collection sites (*Item 2*). We include the two parent firms rather than their subsidiaries in the final data set, and associate these par-

[11] These firms had signed agreements directly with their savings banks, and the banks had in turn hired Panaxia to manage the transportation and depositing of the cash. Unlike the setup for other Panaxia clients—for which Panaxia collected the cash directly from the customer premises—these 286 firms delivered the cash themselves in secure deposit boxes, where Panaxia in turn collected the cash, and then counted and deposited it to the clients' bank accounts. One of the four savings banks, Sparbanken 1826, was also one of the main owners of Panaxia. This circumstance could potentially influence our identification, if the loss that the bank incurred in turn affected its supply of credit to its customers. We assess the relevance of this potential bias in the empirical analysis by applying the following sample split and logic: If our baseline results are due to a credit contraction imposed by Sparbanken 1826, we should observe larger effects in 2012 for the group of treated firms that were customers of the savings banks, relative the other treated firms; if instead, the results are due to the direct impact of the Panaxia fraud and failure, we should observe less pronounced effects in 2012 for the treated firms that were customers of the four savings banks, since these firms were fully compensated for their losses.

[12] The franchisees' claims were approximated in the following way. The franchisor informed us that they had covered 60 percent of their franchisees' losses by extending a so called market support to each firm. Now, the 2012 financial statements of the franchisees include a separate post for the amount of this market support, therefore approximate measurements of the claims held on Panaxia at the time of the bankruptcy (market support divided by 0.6) can be obtained, as well as the losses suffered by the individual firms (claim on Panaxia multiplied by 0.4). The accuracy of this loss calculation was confirmed through contacts with a sample of franchise stores.

ents with the consolidated financial statements pertaining to their respective business group.

In total, our records cover 1,255 clients that held outstanding claims on Panaxia at the time when it failed; arising from collections of daily proceeds that were never transferred to the clients' bank accounts (see Table B1 for an overview of the number of firms by type and data source). After omitting firms for which we cannot establish an identity (38); banks and financial firms (13); non-limited liability firms (173) for which we do not have accounting data; pharmacies (131) which were mostly start-ups in the period 2010–2011 due to a deregulation of the pharmacy market that took place mid-year 2009; the franchisor (1) which was indirectly exposed; and firms with missing accounting data for the period 2008–2013 (289), we obtained a final sample of 610 firms.[13] The average claim-to-assets amounts to 7.9 percent. As noted above, the claim did not translate into losses for all firms; 494 firms incurred a loss, out of which 234 franchisees were partly compensated by the franchisor, and 116 firms were fully compensated by their banks. Due to the compensation, the average losses-to-assets amounts to 4.3 percent for the group of firms that incurred losses.[14]

2.2.2 Financial statements and overdue payments

The universe of Swedish corporate firms' financial statements, provided by UC, constitute the backbone of the panel data set analysed below.[15] The panel data set is obtained through merging of the Panaxia data with data on financial statements for the stock of Swedish *aktiebolag*. *Aktiebolag* are by approximation the Swedish equivalent of corpo-

[13]Panel A in Table B1 provides an overview the number of firms by type and data source, and Panel B shows the number of non-financial corporate firms over time. It is worth noting the large inflow of pharmacies after 2009, which is due to the deregulation of the pharmacy-market; hence, we do not observe the pre-event period for most of these firms, which motivates the omission. Furthermore, unreported tests show that the results are robust to the inclusion of the franchisor. Finally, in the final sample, we have also omitted one treated firm that displayed an abnormally large number of overdue payments in 2009. For this treated firm, the number of overdue payments was amongst the largest in the entire population of Swedish firms at the time.

[14]The Panaxia bankruptcy had dire consequences for its clients. For the group of non-financial corporations that did not get compensated by the savings banks, or by the franchisor, we observe 4 failures in the last quarter of 2012, which corresponds to a quarterly bankruptcy frequency of (4/466=) 0.9 percent. This can be related to the bankruptcy frequency in the retail sector which was 0.4 percent in the same quarter, suggesting that the imposed liquidity losses led to an elevated failure risk.

[15]The financial statement data set, or close versions of it, has been used extensively in previous research, cf. Jacobson, Lindé and Roszbach (2013), Giordani, Jacobson, von Schedvin and Villani (2014), and Jacobson and von Schedvin (2015).

rations in the US, or limited liability businesses in the UK. Swedish law requires every *aktiebolag* to hold a minimum of SEK 100,000 (approximately USD 15,000) in equity to be eligible for registration at Bolagsverket, the Swedish Companies Registration Office (SCRO). Swedish corporate firms are required to submit an annual financial statement to the SCRO, covering balance-sheet and income-statement data in accordance with European Union standards. As in many other countries, Swedish firms have considerable discretion in determining the time period covered by their financial statements and a non-negligible fraction concerns fiscal periods that deviate from calendar years.[16] We deal with this by interpolating the financial statements to align fiscal periods with calendar years.[17] In addition, firms with total assets and real sales below SEK 100,000 (deflating by means of consumer prices, using 2010 as base year) are omitted. To avoid detrimental effects from outlier observations, all firm-specific variables are winsorized with respect to the 1st and the 99th percentiles. In the robustness evaluation of our baseline results, we discuss and assess the implications of the applied interpolation and winsorization schemes for our results.

Moreover, we also make use of a specialized data set provided by the credit bureau on applications for issuance of injunctions for settlement of overdue trade credit claims. These data were originally collected by the Swedish Enforcement Agency, which is the governmental institution that coordinates the administrative process of bankruptcy resolution; it is also responsible for the collection of private and public debt, and hence provides legal support to trade creditors (suppliers) for the management of their unsettled trade credit claims. For the period 2007Q1–2013Q1, we observe, at a daily frequency, all Swedish corporate customers that are subject to applications for issuance of injunctions. In these data we only observe the identity of the customer, but

[16]Financial statements for Swedish firms in general span a 12-month period, but do not necessarily coincide with calendar-years. Deviations in the length of the fiscal period may occur in the start-up year, or if the fiscal period is shifted, and in either case firms are allowed to apply a shorter or longer fiscal period (with a maximum of 18 months). It is not uncommon that fiscal periods starts in other months than January. For example, out of the 610 treated firms in the Panaxia sample, 24 percent have financial statements with fiscal periods that differ from calendar years.

[17]We apply the interpolation approach outlined by Giordani et al. (2014). More specifically, consider the case where a firm has an accounting period that ends in the middle of year t. The length of the accounting periods (in months) for the two statements that ends and starts in year t are given by N_{t_1} and N_{t_2}; the number of months that the two statements cover year t are given by n_{t_1} and n_{t_2} (such that $n_{t_1} + n_{t_2} = 12$); and Var_{t_1} and Var_{t_2} are the variables obtained from each statement. The interpolated statement is then calculated as: $(n_{t_1}/N_{t_1}) \times Var_{t_1} + (n_{t_2}/N_{t_2}) \times Var_{t_2}$ for the set of flow variables; and $(n_{t_1}/12) \times Var_{t_1} + (n_{t_2}/12) \times Var_{t_2}$ for the set of stock variables. This principle is easily extended to the few cases where three statements pertain to a given calendar year.

not the issuer (supplier). However, for a shorter period 2010Q1–2013Q1, we observe the identities of both parties for the universe of submitted applications for issuance of injunctions. Hence, for the shorter period, we can evaluate the degree to which firms try to enforce payments of overdue credit from their customers; whereas the longer period is informative about the extent to which firms postpone payments to their suppliers. Thus, the two data sets enable assessments of shifts in trade credit repayment behavior, both upstream and downstream.

2.3 Empirical Approach

Panaxia's fraudulent scheme and failure is assumed to have negatively affected the liquidity positions of its corporate clients, and we are in particular interested in the effects on cash holdings and trade credit positions. To this end, in our baseline evaluation, we will study outcome variables measuring cash and liquid assets, $Cash/Assets$, the amount of drawn trade credit from suppliers, $Payables/Assets$, and the amount of issued trade credit to customers, $Receivables/Sales$.[18] As noted in the Introduction and as is evident from the presentation of our data above, the Panaxia events involved a relatively small number of firms. This suggests a matching estimation framework in which we model the difference-in-differences in outcomes between firms exposed to the sequence of Panaxia events (the treated firms) and their counterfactuals, as obtained through matching with unexposed firms (the matched control firms). The objective is to calculate the average treatment effect for the treated firms (ATT) on the set of outcome variables, using the nearest-neighbor matching approach proposed by Abadie and Imbens (2006). The treatment period is taken to be 2010–2012, which covers the 32-month period of lasting increases in transfer delays and the subsequent losses caused by the failure in September 2012. We apply the following matching model specification. Firstly, the Mahalanobis weighting matrix is selected to control for the differences in scale between the matching variables. Secondly, we use matching with replacement, which implies that a given control firm potentially can be matched to multiple treated firms.

[18]Normalizing accounts payable by assets and accounts receivable by sales is common practice in the trade credit literature, see, e.g., Petersen and Rajan (1997), or Cuñat (2007) who, as in the current paper, evaluate effects of liquidity shocks on payables scaled by assets. For robustness we will also evaluate effects on accounts payable scaled by cost of goods sold.

Each treated firm is matched with one control firm, using a set of matching variables comprising firm-specific characteristics and a five-digit industry classifier. We select our matching variables based on covariates that are commonly used as control variables in the literatures on cash holdings and on trade credit. The selected set of matching variables is: cash flow-to-assets; log of assets; sales growth; debt-to-assets; tangible assets-to-assets; inventories-to-assets; log of firm age; cash-to-assets; payables-to-assets; and receivables-to-sales. The matching is performed with respect to the 2009-outcomes of the matching variables. We also match on 2008-outcomes of cash-to-assets, payables-to-assets, and receivables-to-sales.

Our aim is to gauge the impact of the postponed transfers, and subsequent losses, on treated firms. For this purpose, we consider the following difference-in-differences estimator of yearly adjustments in the treatment and post-treatment periods for the outcome variables:

$$\tau_t^y = \left(\bar{y}_t^{(1)} - \bar{y}_{t-1}^{(1)}\right) - \left(\bar{y}_t^{(0)} - \bar{y}_{t-1}^{(0)}\right), \quad t = 2010, ..., 2013, \tag{1}$$

where $\bar{y}_t^{(1)}$ is the mean of an outcome variable for the treated firms in year t and $\bar{y}_t^{(0)}$ is the mean of the same outcome variable for the matched control firms in year t. We calculate the yearly adjustments for the treatment period 2010–2012, and for the post-treatment year 2013. In addition to yearly adjustments, we also calculate difference-in-differences estimators of cumulative adjustments over multiple years for the treatment and post-treatment periods:

$$T_t^y = \left(\bar{y}_t^{(1)} - \bar{y}_{2009}^{(1)}\right) - \left(\bar{y}_t^{(0)} - \bar{y}_{2009}^{(0)}\right), \quad t = 2010, ..., 2013. \tag{2}$$

These estimators of yearly and cumulative adjustments offer insights on how the liquidity shortfalls affect firms' cash and trade credit positions. Following Cameron and Miller (2015), the standard errors are adjusted for clusters in the following two dimensions. Firstly, standard errors are adjusted at the firm-level for non-franchisees, and at the franchisor-level for franchisees. This accounts for the multiplicity of control firms, as well as for a possible dependence among franchisees. Secondly, the standard errors are also adjusted at the level of matched pairs, to account for potential dependences within pairs of treated and control firms.[19]

Our approach to inference is within a potential outcome framework and rests on two identifying assumptions; that of unconfoundedness and that of an overlap in covariate distributions, see Imbens and Wooldridge (2009) for a comprehensive overview. The unconfoundedness assumption asserts that treatment assignment is independent of potential outcomes, conditional on observable covariates. In our difference-in-differences setup, this is to say that in the absence of treatment (not observable) changes in the outcome variables for the treated firms in the treatment period should coincide with (observed) changes for the control firms in this period. While the unconfoundedness assumption is untestable, its plausibility can be assessed. To this end we examine the trends in the outcome variables for treated and control firms in the pre-treatment period; statistically indistinguishable trends favour the plausibility of unconfoundedness. If treated and control firms developed similarly in a period when factually neither were subject to treatment, then it is more plausible that they would have done so also in the treatment period had there been no treatment. The assumption of overlap in covariate distributions is more straightforward to evaluate. For this purpose, we follow Imbens and Rubin (2015) towards an assessment of the balance in covariate distributions across treated and control firms.[20]

The complexity of the Panaxia events gives rise to differential treatments of firms, which we can exploit to study heterogeneity in effects. That is, a sub-group of the treated firms were only exposed to the fraudulent scheme undertaken by Panaxia, but did not suffer any losses in the bankruptcy in 2012 since they were fully compensated by their banks. We use this differential in treatment—comparing firms that received partial treatment with those receiving full treatment—to examine if we observe larger

[19] In a matching approach, the commonality in characteristics of a treated and its matched control firm implies that we should expect a dependence in outcomes over the treatment period—that is, absent treatment they are presumed to develop in a similar fashion. By cluster adjusting the standard errors at the level of matched pairs, we control for this dependence. In a recent paper, de Chaisemartin and Ramirez-Cuellar (2019) show that estimators may be biased if dependencies at the matched pair level are not accounted for by means of cluster-adjusted standard errors.

[20] Our empirical setup follows the commonly applied two-step procedure discussed by Ho, Imai, King and Stuart (2007), combining a pre-processing matching step to achieve covariate balance, with a second-step regression estimator. In very recent work, Abadie and Spiess (2019) propose an approach to account for uncertainty in the matching step by first resorting to matching without replacement, and then to calculate standard errors adjusted for clustering at the level of matched pairs in the second step. To ensure that our results withstand control for the matching step uncertainty, we include the Abadie and Spiess approach as an alternative specification in our analysis.

adjustments in outcome variables when firms are exposed to more liquidity distress. In this vein, we also evaluate effects conditional on variation in loss-size.

We proceed to examine cross-sectional heterogeneity in firm-characteristics using sample-splits for differential impacts of liquidity shortfalls on treated firms' liquidity management. Here we explore the notion that credit constraints matter for firms' reliance on adjustments in cash and at the trade credit margins. We follow Farre-Mensa and Ljungqvist (2016) and use firm size and credit ratings as measures of financial constraints. Farre-Mensa and Ljungqvist show that small private firms and high-risk firms are more likely to face limited access to external financing. More specifically, for each split-variable, we sort the firms into empirical distributions based on the 2009-outcomes of the split-variable and construct two samples of firms that are classified as financially constrained and unconstrained, respectively. We then estimate and compare coefficients across the two samples, to assess the role played by credit constraints.

Finally we propose to gauge the mechanisms underlying adjustments in payables and receivables, by considering a set of outcome variables related to overdue trade credit payments—both upstream and downstream. To this end, we use data from the Swedish Enforcement Agency on applications for the issuance of injunctions to settlement of outstanding claims. These data provide an opportunity to assess whether the treated firms to a larger extent than the control firms delayed payments to suppliers, i.e., engaged in upstream adjustments. In other words, we examine if treated firms' upstream suppliers submitted more applications for issuance of an injunction to recover late payments, than did the upstream suppliers of control firms. Symmetrically, we can also assess whether treated firms to a greater extent than control firms, submitted applications for injunction issuance to recover customers' overdue debt, i.e., engaged in downstream adjustments. This analysis provides insights on whether adjustments in payables and receivables are associated with shifts in the enforcement of overdue payments on the underlying trade credit contracts.

3 Baseline Results on the Treatment Effects of Liquidity Shortfalls

This section presents applications of the Abadie and Imbens (2006) nearest-neighbor matching approach to estimate treatment effects on the Panaxia clients that were affected by the liquidity shortfalls generated in the fraud and subsequent failure. We first establish a set of baseline results, and then consider, in turn, the relationship between treatment size and effect, and the role of financial constraints.

3.1 Sample compositions for treated, non-treated, and matched control firms

Descriptive statistics for the matching variables are reported in Table 2; Panels A, B, and C cover the treated firms, the non-treated firms, and the matched control firms, respectively. The non-treated firm-category refers to a weighted cross-industry average of the entire population of Swedish corporate firms, subject to the same eligibility restrictions that we apply to the treated firms and the matched control firms. The industry weights are given by the fraction of treated firms in each particular five-digit industry. As noted above, we follow the guidelines in Imbens and Rubin (2015) for the appraisal of overlap in covariate distributions. Therefore, to assess magnitudes of differences in matching variables, between the treated firms and the non-treated firms on the one hand, and between the treated firms and the matched control firms on the other hand, we calculate and report normalized differences, $\Delta_{co,tr}$, in Panels B and C. When comparing covariate distributions for treated and non-treated firms in Panels A and B, the normalized differences indicate non-negligible deviations in tangible assets, cash holdings and accounts payable.[21] Hence, the descriptive statistics indicate some, but not huge, differences in covariates between treated firms and our industry-weighted representation of non-treated firms.[22] However, the presence of some deviation points towards a

[21] Imbens and Rubin (2015) compare outcomes in normalized differences as obtained in four distinct data sets; three covering observation data and one experimental data. For the LaLonde (1986) experimental data with random assignment, Imbens and Rubin observe a maximum absolute normalized difference of 0.30 standard deviations, which contributes to their overall assessment of excellent covariate balance.

[22] Table B3 in Appendix B reports three additional measures for the assessment of overlap: two coverage frequencies; and the logarithm of the ratio of standard deviations. The reported coverage frequencies in Columns (I) and (II) show that the covariate distributions are overlapping to considerable extent for the treated and non-treated firms, which suggest that there is scope for a matching procedure to accurately

need to undertake matching to obtain credible counterfactuals.

[Insert Table 2 about here.]

Consistent with the overlap assumption, the results reported in Panel C show that the matched control firms are very similar to the treated firms. In terms of normalized differences, there are only minor deviations between the treated and matched control firms. These results indicate that the matching procedure is achieving its objective of matching treated firms to otherwise similar control firms. Nevertheless, we will subsequently apply a set of robustness tests to account for potential differences that may not necessarily be detected in a balance assessment.

Furthermore, Figure 2 presents normalized means of the three outcome variables, for the treated, non-treated, and matched control firms in each year during: the pre-treatment period (2007–2009); the treatment period (2010–2012); and the post-treatment period (2013). Two features are apparent. Firstly, when comparing treated with non-treated firms, the figure shows distinct deviations for cash holdings and accounts payable in the pre-treatment period, which again highlights the need for matching to acquire credible counterfactual firms. Secondly, in the comparison of treated and control firms, we find that all three outcome variables display similar trends in the pre-treatment period. Thereafter, in the treatment period, there is divergence in means between treated and control firms. We observe a relative increase in accounts payable for the treated firms, as well as relative declines in accounts receivable and cash holdings. Thus, Figure 2 provides initial evidence suggesting that treated firms used their cash holdings and trade credit margins to overcome the Panaxia liquidity shortfalls. Moreover, in the evaluation below we report formal tests of divergences in trends, and verify that treated and control firms display trends in the outcome variables that are not significantly different in the pre-treatment period.

[Insert Figure 2 about here.]

identify counterfactual firms. In addition, Column (III) shows that the differences in dispersion between the distributions are modest for all variables.

3.2 Baseline results

We now proceed with a presentation of our baseline estimation results. Table 3 reports the yearly and cumulative adjustments according to Eqs. (1) and (2) for our three key outcome variables. Panel A shows results for cash holdings, $Cash/Assets$. The estimates of the yearly adjustment effects, τ_t, in Columns (I)-(IV) show statistically significant reductions in cash holdings in the first two years of the treatment period. The immediate response in 2010 is consistent with the prolonging of the transfer period, which reached five days already in December 2010, cf. Figure 1.[23] The cumulative effect estimates, T_t, show that the yearly declines in cash in 2010 and 2011 result in persistently lower cash holdings in the final year of the treatment period and in the post-treatment year. In addition, to assess the plausibility of the unconfoundedness assumption, we test for differences in trends across treated and control firms in the pre-treatment period 2007–2009. Column (V) shows test results indicating parallel cash holding trends, which supports unconfoundedness.[24]

[Insert Table 3 about here.]

Results for accounts payable, $Payables/Assets$, are reported in Panel B. The estimates of the yearly adjustment effects, τ_t, reported in Columns (I)-(IV) show an increase in 2011 of 1.1 percentage point and a further increase of 1.8 percentage points in 2012. These yearly effects result in a cumulative adjustment effect, T_t, of 2.8 percentage points in 2012 and 2.8 percentage points in the post-treatment year. Moreover, Column (V) indicates that treated and control firms follow parallel pre-treatment trends with respect to accounts payable.

Panel C reports results for accounts receivable, $Receivables/Sales$. The estimates of the yearly adjustment effects point to an initial contraction of 0.3 percentage points in the first year of the treatment period and a further contraction of 0.6 percentage points in 2012. Accordingly, the estimates of the cumulative effects, T_t, show that the

[23] Variation in choice of accounting practice across the treated firms may affect the measurement of cash adjustments in 2010 and 2011, but not in 2012. In particular, the convention to book cash-in-transfer under cash holdings leads to an underestimation of treated firms' reliance on cash to balance the initial transfer delays. See Appendix A for a detailed discussion.

[24] We apply the test of parallel pre-trends proposed by Mora and Reggio (2015). More specifically, for the period 2007–2013, we estimate the model $E\left[y_{it}\right] = \delta + \sum_{t=2008}^{2013} \delta_t I_t + \gamma D_i + \sum_{t=2008}^{2013} \gamma_t I_t D_i$, where I_t is a time t year dummy and D is a treatment dummy. The Wald test statistic for parallel pre-treatment trends concerns the joint significance of $\hat{\gamma}_{2008}$ and $\hat{\gamma}_{2009}$.

downward trend in receivables amounts to an accumulated reduction of 1 percentage point in 2012, which persists in the post-treatment year. Finally, the similarity in pre-trends, documented in Column (V), is in support of the underlying unconfoundedness assumption.

The point estimates of the cumulative adjustments in 2012, T_{2012}, suggest that the magnitude of the upstream adjustment is larger than that of the downstream adjustment. One obvious concern in a comparison of relative size for the two effects is that payables are scaled with assets, whereas receivables are scaled with sales. Scaling accounts receivable by assets instead, provides a better ground for such a comparison; in estimation using receivables-to-assets we obtain a cumulative effect (t-value) in 2012, T_{2012}, of –0.010 (–1.9), which is similar to the estimate for sales-scaled receivables of –0.010 (–3.3). A statistical test for the difference in absolute adjustment between payables-to-assets and receivables-to-assets, shows that adjustments in payables indeed dominate receivables, with a p-value of 0.069. Furthermore, to gauge the relative importance of cash versus trade credit margins, we can compare the size of compounded adjustments in net trade credit positions (i.e., $(Payables - Receivables)\,/Assets$) with the size of adjustments in cash holdings. The estimated cumulative adjustment (t-value) in net trade credit in 2012 is 0.039 (3.8). Testing for the difference in absolute value adjustment between cash and net trade credit yields a p-value of 0.215, indicating that average adjustments at the two trade credit margins are jointly of a similar magnitude as average adjustments in cash holdings.[25]

Although firms clearly make use of both upstream and downstream liquidity extraction—independently or simultaneously—it is conceivable that operating the accounts payable margin may provide a more effective measure to raise liquidity and explains why we find that upstream dominate downstream adjustments. Through upstream adjustments, firms can readily offset liquidity shocks by immediate postponement of due payments to suppliers, and withhold money until additional inflows of funds are obtained. If the amount of liquidity extracted upstream proves insufficient to

[25] We can further compare the average loss of 4.3 percent, cf. Table 2, with the sum of the absolute adjustments in cash, payables, and receivables (scaling receivables with assets instead of sales), which amounts to ($|T_{2012}^{Cash/Assets}| + |T_{2012}^{Payables/Assets}| + |T_{2012}^{Receivables/Assets}| =$) 0.062, with a 95-percent confidence band spanning 0.036 and 0.089. Thus, the liquidity losses and compounded adjustments are of similar magnitude.

offset the shock, the firm may continue to roll over its overdue trade credit debt until the impact of the original liquidity shock is neutralized. Intuitively, the ability for firms to roll over overdue trade credit debt hinges on their suppliers' willingness to overlook late payments, that is, on the absence of obstacles to the functioning of (implicit) risk sharing networks. In downstream adjustments, firms can extract liquidity by reducing the trade credit maturities in new contracts to prompt faster future payments from customers. But that will free up liquidity only with a lag. An alternative measure is to proactively manage outstanding claims, to avoid late payments from customers. The nature of firms' trade credit margin adjustments warrants a closer study and we will therefore return to the matter of the underlying mechanisms in the next section below.

A rather obvious and potentially important liquidity source for firms is bank lines of credit, see, e.g., Sufi (2009). Whether the liquidity shortfalls considered here also yield effects on firms' bank borrowing is therefore next evaluated by use of three balance sheet items: total bank debt, and short- and long-term bank debt separately. Appendix Table B4, Panels A-C, accordingly present yearly and cumulative treatment effects on these debt-measures; no systematic adjustments are recorded over the event period, indicating that the firms do not turn to their banks first-hand to deal with liquidity shortfalls. We propose two potential explanations. Firstly, the firms under consideration may on average be subject to binding financial constraints that limits their access to bank financing, therefore forcing them to instead use their cash holdings and trade credit margins. Secondly, Lins, Servaes and Tufano (2010) argue that firms mainly use cash to handle cash flow shocks, whereas credit lines are primarily used to ensure funding for future investments. We will study these explanations in more detail below, when we explore sources of cross-sectional heterogeneity.

To further validate our baseline results, we consider a set of alternative specifications reported in Table 4. For these robustness analyses, we report the estimated cumulative treatment effects in 2012, which capture the full impact of the sequence of events related to the fraud and failure of Panaxia. Firstly, we examine the extent to which our baseline results are influenced by the use of a matching procedure. This is carried out by estimating cumulative adjustments using all non-treated firms instead of the matched control firms as counterfactuals. Analogously to the calculations underlying Table 2 and Figure 2, weighted means for the non-treated firms are calculated using the fraction of treated firms in each five-digit industry as weights. Row (2) in Table 4 reports results where adjustments for treated firms are related to adjustments for all non-treated firms. Columns (I)-(VI) show that the estimated effects for all outcome variables are statistically significant in 2012. The estimates carry the same signs, but are slightly smaller as compared with the baseline estimates, cf. Row (1). However, tests for parallel pre-treatment trends indicate deviations in cash holdings between treated and non-treated firms, emphasizing the importance of applying a matching approach.

[Insert Table 4 about here.]

Secondly, a potential concern is that remaining differences in characteristics post-matching may influence our results. To address this matter, we report results from bias-corrected matching estimators, where differences in matching-variable outcomes between treated and control firms are accounted for, see Abadie and Imbens (2011). Specifically, based on the set of matched control firms only, we estimate the linear regression function, $\mu_0\left(X_i\right)$, on the thirteen matching-covariates in Table 2, and enter control firms into the regression with the same frequency as they occur in matched pairs. The outcome variable for the control firms is then adjusted using the estimated function $\hat{\mu_0}\left(X_i\right)$.[26] Results in Row (3) show that the bias-corrected effects are very similar to the baseline estimates, suggesting that the latter are not confounded by differences in characteristics across treated and control firms. In the proceeding accounting ratio analysis, we complement the baseline estimates with bias-adjusted estimates to demonstrate that covariate deviations in matched observations do not affect the results. In addition to the bias-corrected estimates, we follow Crump, Hotz, Imbens and Mitnik (2009) and restrict the estimation sample to matched pairs where differences in matching variables are small. We therefore consider the 50 percent closest matched pairs, with the purpose of further ensuring that the characteristics of the treated firms closely align with the ones for the matched control firms. Row (4) shows that the estimated treatment effects obtained in the restricted sample largely conform to the baseline results.

[26]In the calculations underlying Eq. (2), the outcome variable for the matched control firms, $\Delta y_i^{(0)}$, is adjusted as follows: $\Delta y_i^{(0)} + \left(\hat{\mu_0}\left(X_i\right) - \hat{\mu_0}\left(X_\ell\right)\right)$, where X_ℓ denotes the covariate outcome for the control firm and X_i denotes the pair-specific covariate outcome for the treated firm. This adjustment thus controls for variation in the outcome variable that can be attributed to differences in covariates between the treated and matched control firms.

Thirdly, following Petersen and Rajan (1997) and Cuñat (2007), accounts payable are scaled by firms' total assets in the estimations underlying our baseline results. However, an alternative scaling is by cost of goods sold ($COGS$), see, e.g., Garcia-Appendini and Montoriol-Garriga (2013), that may closer reflect firms' levels of economic activity and in particular better capture durations in underlying trade credit contracts. In the case of Swedish corporate firms, only a subset reports cost of goods sold in their financial statements, which reduces our estimation sample to 109 treated firms when retaining pairs of treated and matched control firms where both parties convey this information in 2009 and 2012.[27] In Row (5), we note a positive and significant cumulative treatment effect for payables scaled by cost of goods sold, thus consistent with our baseline results.[28] The estimated effects for the other outcome variables show an insignificant effect for cash holdings; whereas the effect for receivables is negative but inconclusive, due to differences in pre-treatment trends. Unreported results for cumulative adjustments in short-term bank financing for this subsample, indicates a positive and statistically significant estimate (t-value) of 0.006 (2.0). These results suggests that firms propensity to use an accounting method that discloses their costs of goods sold is potentially correlated with factors associated with their access to bank financing, which would also explain the adjustments in short-term bank financing rather than cash holdings.

Fourthly, we evaluate whether our choice to winsorize the variables is of consequence, and alternatively consider a truncation at the 1st and 99th percentiles. Row (6) shows that obtained estimates on truncated data are very similar to the baseline

[27] Swedish firms can choose between the cost of sales method and the nature of expense method, when accounting for cash flows in their financial statements. The former method involves reporting cost of goods sold, the latter does not. In the treated group, 255 firms (42 percent) apply the cost of sales method.

[28] In a similar vein, we also consider two alternative specifications. Firstly, to evaluate the full number of treated firms that report $COGS$, we re-match targeting treated and control firms that report $COGS$ using the original set of matching-variables and pre-outcomes (2008 and 2009) of $Payables/COGS$, resulting in 255 treated and matched control firms. Due to post-matching differences in $Payables/COGS$ in 2009 ($\Delta_{c,t} = 0.414$), we apply a bias-adjustment using the fifteen matching-covariates—following the same approach as for the results in Row (3). The obtained estimate (t-value) of the cumulative adjustment, T_{2012}, for $Payables/COGS$ amounts to 0.020 (3.9). Secondly, we also consider accounts payable scaled by expenses (operating expenses minus salary expenses and other non-goods costs). We re-match using the original set of thirteen covariates, the pre-outcomes of $Payables/Expenses$, and an indicator for the accounting method used. To control for post-matching differences, we apply a bias-adjustment using all matching covariates, except for the accounting indicator, which is exactly matched. The 594 treated and matched control firms yield a cumulative adjustment, T_{2012}, for $Payables/Expenses$ of 0.012, with a t-value of 2.6.

results.

Fifthly, 234 of the treated firms are franchisees. To gauge the extent to which the franchisees influence the baseline results, we re-estimate our models omitting these firms. Row (7) reports results showing that the estimated effects for the two trade credit margins are slightly smaller, but largely in line with the baseline results. The effect on cash holdings is negative, but statistically insignificant.[29] Thus, the reliance on trade credit margins to manage the liquidity shortfalls is a common feature for the non-franchise and franchise firms alike.

Sixthly, Row (8) reports results where pharmacies are included in the estimation sample. The reason why inclusion of pharmacies adds seven more treated firms is that most pharmacies were start-ups in 2010 and 2011, cf. Table B1, which implies that a large share has missing accounting information for parts of the 2008–2013 period. However, when including the pharmacies for which we do have adequate information, we obtain estimated effects that are very similar to the baseline results.

Seventhly, Row (9) concerns results for an unbalanced panel, where we relax the baseline eligibility restriction that observations on outcome variables must be available for both treated and control firms in every year of the treatment and post-treatment periods and instead impose that outcome variables must be non-missing in 2012, which increases the number of treated firms from 610 to 641. There is a marked difference in that the estimated treatment effect on payables is substantially enhanced for the unbalanced panel. A potential explanation for the stronger results is that the treated firms eliminated from the unbalanced panel were more distressed. Hence, these results indicate that our baseline estimates of payables adjustments are, if anything, conservative.

Eighthly, for a large fraction of firms—24 percent of the treated firms—the fiscal period ends in a month other than December. To account for this we use interpolated financial statements, so that fiscal periods align with calendar years, see discussion in Subsection 2.2.2. To ensure that the interpolation procedure does not affect our results, we estimate cumulative effects on non-standardized data. Row (10) shows that the obtained effects from this exercise are very close to the baseline estimates. Furthermore,

[29] The p-value of cash holdings is 0.12, and the estimate is not statistically different from the baseline effect reported in Row (1). Unreported estimates (t-value) show an increase in short-term bank financing of 0.008 (1.8), suggesting that the group of non-franchise firms also used bank financing to manage liquidity shortfalls.

Rows (11) and (12) concern aspects of timing for the Panaxia events. One potential worry in using interpolated accounting statements is that the timing of the liquidity shortfalls may not be fully captured by our baseline estimates. For instance, effects in 2010 should primarily be observed for treated firms for which the fiscal period ends in December, since the marked, upward shift in transfer times took place in the last quarter that year, cf. Figure 1. To investigate the significance of these circumstances, we estimate T^y_{2010} on two subsamples concerning treated firms with fiscal year-ends in December, Row (11), and treated firms with fiscal year-ends occurring in other months than December, Row (12). Consistent with the baseline effects reported in Table 3, the estimates reported in Rows (11) and (12) show that the adjustments in cash holdings and receivables are statistically significant for firms with fiscal year-ends in December, but no significant effects are obtained for the other group. Thus, these results render further support to the notion that our estimates indeed capture the liquidity shortfalls imposed by the Panaxia fraud.

Finally, Abadie and Spiess (2019) propose that uncertainty regarding the matching process can be accounted for by first applying matching without replacement and then calculating cluster-adjusted standard errors at the level of matched pairs. Following their suggestion, Row (13) reports results from a propensity score matching without replacement—using the same set of matching variables as in the baseline specification—with standard errors adjusted in two dimensions: firstly, at the matched pair level; and secondly, at the firm-level for non-franchisees and franchisor-level for franchisees. To account for post-matching deviations in covariate outcomes between treated and matched control firms, we apply the bias-correction outlined above, cf. the description of the results in Row (3). The results reported in Row (13) are consistent with the baseline results in showing statistically significant downward shifts in cash holdings and receivables, and an upward shift in payables.

To sum up, our baseline results show that the retention of client funds and the subsequent bankruptcy-related losses caused Panaxia's clients to reduce their cash holdings, increase the amount of drawn trade credit from suppliers, and contract the amount of issued trade credit to customers. In terms of magnitudes, the joint impact at the two trade credit margins is on par with adjustments in cash holdings; and upstream trade credit adjustments dominate downstream adjustments. Thus, trade credit is an important source of reserve liquidity for firms.

3.3 Responses conditional on loss-size

Magnitudes of adjustments in cash and at the trade credit margins should depend positively on the sizes of firms' incurred losses in the Panaxia failure. That is, whereas the fraud in postponing transfers of funds to client accounts is certainly expected to have a negative impact on firms' liquidity positions, the point-in-time realization of a large loss when Panaxia finally went bankrupt should yield a larger negative and more persistent impact. This conjecture will be examined next and we will consider two cases: firstly, firms that incurred losses versus no losses; and secondly, firms' responses conditional on the size of their losses. For the first case we divide the treated firms into two groups: firms that were fully compensated by their banks in 2012; and firms that incurred losses in 2012. Thus, the two groups experienced similar fraud treatments in 2010 and 2011—delayed transfers—but a differential treatment in the bankruptcy year 2012. However, the small number of compensated firms, 116 observations, introduces limitations for the analysis in restricting statistical power.

Panel A in Table 5 reports cumulative treatment effects in 2012 for the two groups; Columns (I) and (II) cover treated firms that were fully compensated in 2012 and Columns (III) and (IV) treated firms that incurred losses in 2012. Rows (1)-(3) report estimates for the baseline specification, and Rows (4) and (5) estimates for the baseline specification with bias-adjustment. The results show more pronounced adjustment effects on all three outcome variables for the group of firms that incurred losses, as compared with the group of compensated firms.[30] Nevertheless, although statistically significant effects are primarily observed for the group of firms that incurred losses, effects are not statistically larger for firms that incurred losses, cf. Columns (V) and (VI).

[Insert Table 5 about here.]

For a broader picture of the responses to differential treatments in the two groups, Table B5 in Appendix B reports yearly adjustments and cumulative effects over the full

[30]The largest of the four savings banks, Sparbanken 1826, was as noted above one of the largest owners of Panaxia—which may implicate our identification approach. However, the results showing that effects in 2012 primarily pertain to the group of treated firms that were not savings bank customers, mitigate a concern that our baseline results in Table 3 are influenced by a potential credit contraction imposed by Sparbanken 1826.

treatment and post-treatment periods. The table shows that the group of compensated firms displayed a downward shift in cash holdings in 2011, and a subsequent reversal in 2012. A similar pattern is observed for accounts payable, where the cumulative adjustments indicate an increase in 2011, followed by an insignificant accumulated effect in 2012. These results thus suggest that the group of compensated firms responded to the liquidity shortfalls induced by the initial fraud treatment. For the group of firms that incurred losses, the results show initial adjustments along all three margins during the fraud treatment in 2010 and 2011, followed by further adjustments along the two trade credit margins in response to the bankruptcy event in 2012.[31,32]

Our analysis can take one step further by evaluating whether the magnitudes of treatment effects depend on the size of the incurred losses, i.e., the second case of differential treatment mentioned above. Our conjecture is that larger losses are associated with larger adjustments at the three margins. To assess this conjecture, we estimate the following version of the baseline difference-in-differences specification:

$$\begin{aligned} y_{i,t} =& \beta_0 + \beta_1 \times Event_t + \beta_2 \times Loss/Assets_{i,2012} \\ &+ \beta_3 \times Event_t \times Loss/Assets_{i,2012} + \varepsilon_{i,t}, \end{aligned} \tag{3}$$

where $y_{i,t}$ denotes one of the three dependent variable; $Event_t$ is a dummy variable that takes the value one in 2012, and zero otherwise; and $Loss/Assets_{i,2012}$ is firm i's incurred bankruptcy loss scaled by total assets in 2012. The model is estimated on data from 2009 and 2012 for the full sample of firms. The coefficient of interest, β_3, thus captures the relationship between loss-size and subsequent adjustment in the dependent variable. Furthermore, to account for nonlinearities, results are also reported for an augmented version of the model including a squared term of the loss variable. Two-way

[31] Following the vast literature related to the cash flow sensitivity of investments, we have also considered the presence of real effects by exploring cumulative adjustments in investments. In the post-treatment year we observe no effects on tangible assets for the fully compensated firms, whereas firms that incurred losses exhibit a statistically significant reduction relative to the control firms. Hence, the failure losses are also associated with real effects for affected firms.

[32] A sub-group of the firms that did incur losses in the 2012 bankruptcy went on to receive final disbursements from the remaining assets of the bankruptcy estate in 2013, amounting to 23 percent of their claims at the bankruptcy date. Unreported results for these firms on cumulative effects at the two trade credit margins indicate increases in the amount of received trade credit and contractions in the amount of issued trade credit in 2012. However, in 2013, corresponding point estimates are smaller and statistically insignificant, which is consistent with a mitigating effect from the disbursements that this sub-group received in that year.

cluster-adjusted standard errors are calculated according to our baseline specification.

Panel B in Table 5 shows estimation results for Eq. (3). The linear version of the model is reported in Columns (I)-(III) and the version of the model augmented with a squared term in Columns (IV)-(VI). To enhance interpretability of the effect magnitudes obtained from the nonlinear model, we complement the coefficient estimates with marginal effects calculated at the mean (MEM), where the mean is set to 4.3 percent—which is the mean loss for the group of firms that incurred losses, cf. Table 2. Column (I) shows an insignificant relationship between the size of a loss and associated adjustment in cash holdings, whereas Columns (II) and (III) show that larger losses are associated with significantly larger increases in payables as well as larger decreases in receivables, in a statistical sense. Moreover, the results in Columns (IV)-(VI) suggest that nonlinearities matter. For accounts payable, as shown by the MEMs, the positive relationship is substantially larger as compared with the linear model, whereas the effects at the cash and accounts receivable margins are similar to the estimates from the linear model.[33] Hence, these results indicate that the trade credit margins indeed played an important role in absorbing the impact of the incurred losses and the larger the loss, the larger were resulting adjustments.[34]

In sum, these results shed additional light on the consequences of the bankruptcy event for the exposed firms. Diminishing effects in 2012 for the group of firms that were only exposed to the fraud, in combination with more pronounced effects on the outcome variables for firms that incurred larger losses, corroborate the presumption that overall we are capturing adjustments in the outcome variables that are associated with increased liquidity needs.

[33]Comparing the R^2 for the linear model in Column (II) with the nonlinear model in Column (V) shows an increase from 8.5 to 10.7 percent, which according to an F-test indicates a statistically significant increase at the 1-percent level. Controlling for nonlinearities thus matters for the inference of the accounts payable margin.

[34]A potential concern when estimating the more elaborate Eq. (3) is that the loss variable is correlated with firm-specific factors, such as firm size. This could imply that that the loss variable reflects adjustments for treated firms with a specific set of characteristics, rather than the actual impact of the incurred loss. One way to control for this is to estimate Eq. (3) with matched pair$\times$time-fixed effects. These fixed effects absorb adjustments that are particular to each treated firm and its matched control firm. Appendix Table B6 shows that, if anything, the effects along all margins become more pronounced once we account for time-varying matched pair fixed effects.

3.4 The role of financial constraints

In this sub-section, we set out to investigate the idea that firms' ability to access external funding may be important for their liquidity management, and for shocks to liquidity in particular. To this end, we apply a set of sample-splits to the sample of treated firms that incurred losses in the Panaxia bankruptcy and estimate Eq. (2) for sub-samples differing in the degree of credit constraints, as measured by firm size and credit rating.[35] More specifically, we sort the firms into an empirical distribution based on their 2009-outcomes of the split-variable and then construct two sub-samples; for each split-variable, firms in the top three deciles of the distribution are classified as unconstrained and firms in the bottom seven deciles as constrained. The main reason for using the full sample—and not the more commonly applied approach to compare the top three deciles against the bottom three—is to preserve the number of observations in an already small sample, in the interest of preserving statistical power. Another reason is that, due to the sample composition, firms in the bottom seven deciles of our sample would most likely be classified as constrained when applying cut-offs used in studies that consider public firms. Our reported estimates concern cumulative treatment effects in 2012—capturing the full impact of the Panaxia sequence of events—using the baseline specification, with and without bias-adjustment. For robustness, in Table B7 in Appendix B, we also report results for a symmetric sample split, comparing firms in the top three deciles with firms in the bottom three deciles of the size- and rating-distributions. These results are briefly discussed below.

Panel A in Table 6 shows results when splitting the sample with respect to the size of treated firms, where small and medium-sized firms are classified as constrained and large firms as unconstrained. The first result emerging in Rows (1) and (5) is that the negative effects for cash holdings can be attributed to constrained firms, whereas no significant effects are observed for unconstrained firms, whose point estimates are close to zero. The reported p-value indicates that treatment effects are significantly different for small and medium-sized firms versus large firms. However, test results for the two trade credit margins reported in Rows (2), (3), (6), and (7), respectively, show no statistically significant differences in effects between the two groups.

[35] We select our split-variables based on Farre-Mensa and Ljungqvist (2016), who show that small private firms and high-risk firms are likely to be subject to external funding constraints.

[Insert Table 6 about here.]

Panel B shows results for sample-splits based on firms' credit ratings; firms associated with high bankruptcy risk are classified as constrained, whereas low-risk firms are classified as unconstrained. The estimated effects display a pronounced difference between the two sub-samples. For cash holdings, reported in Rows (1) and (5), the coefficients are negative and statistically significant for constrained firms and insignificant for unconstrained ones. The estimates are nevertheless not statistically different from each other. Rows (2), (3), (6), and (7) show that constrained firms increase the amount of drawn trade credit and contract the amount of issued trade credit, whereas the coefficients for unconstrained firms are close to zero and insignificant. The t-tests indicate that the effects at the two trade credit margins are significantly more pronounced for constrained firms. Finally, estimates in Rows (4) and (8) show that unconstrained firms tend to use significantly more short-term bank financing, as compared with the constrained firms.

In Table B7 in Appendix B, we report results for the alternative sample-split classification that compares effect outcomes for constrained firms in the bottom-three deciles with unconstrained firms in the top-three. These are broadly in line with the results in Table 6 and show that for both constraint measures, the magnitudes of the estimated effects tend to increase for constrained firms when applying the stricter classification. However, the reported t-tests for differences in estimated effects across the two groups of firms become slightly less pronounced. For example, the difference in treatment effect on accounts payable between constrained and unconstrained firms for the rating constraint measure, becomes statistically insignificant using the unadjusted baseline specification, whereas it remains significant for the bias-adjusted estimates.

In sum, although not conclusive, these results are consistent with the presumption that financially unconstrained firms may access external financing to handle liquidity shocks, whereas constrained firms have to rely on internal funds in combination with liquidity extraction from suppliers and customers. That is, constrained firms facing the task of managing liquidity shocks, may draw extra liquidity from suppliers and customers so as to sustain sufficient cash reserves for the purpose of executing prompt payments, such as ongoing expenses for salaries and taxes. In other words, constrained firms balance liquidity extraction from counterparties in the supply chain with the use of liquid assets to handle payments where liquid means are required—indicating that these liquidity sources operate as complements.

4 Mechanisms

In the previous section, we demonstrated that liquidity shortfalls are related to adjustments in treated firms' trade credit positions. In this section we will probe the underlying duration adjustments in trade credit arrangements. More specifically, in an upstream perspective, a duration shift can be obtained by a prolongation of the trade credit contract maturity, but also effectively through a temporary default on due outstanding debt. Symmetrically, shorter maturities on new contracts downstream will reduce trade credit duration, as will active attempts to enforce payment on due credit extended to customers. For lack of data on trade credit contracts we cannot examine shifts in contracted net days; hence, we resort to study temporary defaults and enforcements of payment related to trade credit.

The analysis in this section is close in spirit to the one by Boissay and Gropp (2013), who document that firms pass on liquidity shocks through chains of defaults. Our analysis differs with respect to the nature of the shocks considered—in our case originating outside of the supply chain and therefore uncorrelated with demand conditions—and in the assessment of how overdue claims are resolved.

4.1 Measurement of mechanisms

Whereas postponement of payments to suppliers and enforcement of customers' trade credit payments may well be privately conducted matters between trade credit parties, such actions will ever so often involve a third party, the Swedish Enforcement Agency (Kronofogdemyndigheten; EA), and leave behind publicly available records. The EA offers legal support to Swedish trade creditors (suppliers) for the management of their unsettled trade credit claims. The creditor can submit an application to the EA for the issuance of an injunction to settlement of the outstanding claim. If approved, the EA will then notify the debtor for prompt payment within a fortnight, and take further measures to enforce payment should the debtor persist in dishonouring the claim after notification. Applying for an injunction to settlement is normally the creditor's last resort and typically occurs when a claim has been overdue for an extended period—several weeks, or longer.

We have, from the EA, obtained data on applications for the issuance of injunctions to settlement of outstanding claims, submitted by the universe of Swedish corporate firms. The data include details on the date of submission and the identities of involved parties so that unambiguous merging with the treated and control firms of the Panaxia events is straightforward. The merged data set provides an opportunity to assess whether treated firms to a greater extent than control firms have been subjects to applications for injunction issuance due to unpaid trade credit, i.e., the upstream perspective. We can also consider the downstream perspective and examine whether treated firms to a greater extent than control firms submitted applications for injunction issuance, i.e., took action to enforce repayment of overdue trade credit.

For the full sample period 2007Q1–2013Q4, the EA data are somewhat restricted in that we only observe applications faced by treated and control firms, not issued by them. That is, we observe the customers, but not the suppliers involved. We denote all claims that have been registered at EA $Late\ payments$. For the full sample period we can further disaggregate $Late\ payments$ in two dimensions. Firstly, we observe applications for which the customers did not settle the debt after the notification, and denote these outcomes $Defaults$. Secondly, we also observe applications that led to settlement immediately after the firms received notification from the EA, and denote these outcomes $Settlements$. However, for the shorter sample period 2010Q1–2013Q1, the data set is more detailed. Firstly, we observe the identity of both counterparties involved in an application, i.e., both the supplier and the customer, which means that we can use these data to explore differences in the extent to which treated and control firms attempted to enforce payments from downstream customers. Secondly, we also observe the various outcomes underlying $Settlements$. That is, $Settlements$ is associated with the following three outcomes: the supplier and customer can bilaterally reach an agreement, which usually results in a withdrawal of the application from the EA, denoted $Withdrawals$; the customer can also settle the claim by way of paying directly to the EA, denoted $Payments\ to\ EA$; and the customer can contest the claim, which happens if there is a disagreement between the two parties, denoted $Contested\ claims$.

We structure the outcome variables—$Late\ payments$, $Defaults$, $Settlements$,

$Withdrawals$, $Payments\ to\ EA$, and $Contested\ claims$—obtained from the EA data at a quarterly level. For all outcome variables we measure their extensive margins by use of dummy variables capturing whether the specific event occurred, or not; and their intensive margins by measuring the number of specific events that occurred.

To assess whether the sequence of Panaxia events affected the treated firms' propensity to postpone payments to suppliers and enforce late payments from customers, we apply the following difference-in-differences specification for the sample of treated and matched control firms:

$$y_{i,t} = \gamma_0 + \gamma_1 \times Event_t + \gamma_2 \times Treated_i + \gamma_3 \times Event_t \times Treated_i + \varepsilon_{i,t} \tag{4}$$

where $y_{i,t}$ denotes one of the six EA-dependent variables described above; $Event_t$ is a dummy variable that takes the value one in the 2010Q1–2012Q4 period, and zero otherwise, when the model is estimated on the full sample, and one in the 2010Q2–2012Q4 period, and zero otherwise, when the model is estimated on the shorter sample; and $Treated_i$ is a variable that takes the value one in case of a treated firm and zero for a matched control firm. Thus, the coefficient γ_3 provides an estimate of the average shift in an EA-outcome variable for treated firms in relation to control firms, throughout the entire treatment period. Two-way cluster-adjusted standard errors are calculated according to our baseline specification.

Figure 3 offers a graphical illustration of how the average incidence of $Late\ payment$ developed over time for treated and control firms; measured as the natural logarithm of one plus the number of late payments. Panel A shows postponed payments to suppliers—the upstream perspective. Outcomes in $Late\ payments$ across treated firms (solid line) and control firms (dashed line) are very similar in the pre-treatment period, but after the onset of treatment in 2010 a pronounced divergence between the groups is evident. The steeper rise in $Late\ payments$ for treated firms is consistent with our baseline result showing upward adjustments in their accounts payable, cf. Table 3. Furthermore, Panel B illustrates supplier attempts toward enforcement of late payments from customers—the downstream perspective. The figure shows that treated firms increase the number of attempts to enforce late payments more than the control firms do during the event period, which is consistent with the baseline result showing a downward shift in accounts receivable, cf. Table 3. In light of this baseline result, an increase in the enforcement of late payments can either be due to a reduction in contracted trade credit maturities triggering customers to default more on due debt, or treated firms seeking to reduce actual payment periods by more actively managing late payments; or a combination of the two.

[Insert Figure 3 about here.]

4.2 Mechanism results

Table 7 reports results for Eq. (4), where estimates from a linear probability model (LPM) are provided in Columns (I) and (VI), and estimates from a model that measures the number of outcomes are presented in Columns (II) and (VII). To further account for the zero lower bound in the number of outcomes, Tobit model estimates are reported in Columns (III) and (VIII). Panels A and B report results for the postponement of payments to suppliers and the enforcement of late payments from customers, respectively.

[Insert Table 7 about here.]

Starting with the upstream perspective, Row (1) in Column (I) shows that treated firms' propensity to postpone payments increased by 1.7 percentage points relative to control firms, during the treatment period. To provide an idea of the economic significance of this estimated effect, we can relate it to the pre-treatment period frequency in $Late\ payments$ of 4.7 percent, which indicates a considerable increase for treated firms amounting to $(1.7/4.7 =)$ 35.9 percent.

Rows (2) and (3) in Column (I) show estimates for the two sub-components of $Late\ payments$: $Defaults$ and $Settlements$. The estimated effects show that the increase in $Late\ payments$ for treated firms in the treatment period can be primarily attributed to an upward shift in $Settlements$, whereas the effect for $Defaults$ is very small and statistically insignificant. These results indicate that the treated firms on average engaged in liquidity extraction from their suppliers through maturity extensions on their trade credit debt by means of withholding payments past their due dates, but the overdue claims did not result in outright defaults.[36]

[36]For the group of treated and control firms in our sample, default is a fairly infrequent outcome; the

Rows (1)-(3) in Columns (II) and (III) concern results related to the intensive margin of the outcome variables. The estimated effects are largely consistent with the extensive margin results reported in Column (I), showing that the number of settlements increased significantly more for treated firms, relative to control firms, in the treatment period.[37]

Next, Rows (4)-(6) in Columns (I)-(III) report results for the three sub-components of $Settlements$: $Withdrawals$, $Payments\ to\ EA$, and $Contested\ claims$. It is important to note that these estimates are obtained for the shorter sample period, implying that strong interpretations are unwarranted since we lack data for the pre-treatment period and cannot undertake tests for parallel pre-trends.[38] Nevertheless, the coefficients reported in Rows (4)-(6) serve a purpose in shedding additional light on the underlying drivers of the effects documented in Rows (1)-(3). The main picture emerging is that increases in $Settlements$ primarily appear to be associated with increases in $Withdrawals$, whereas no significant effects are obtained for $Payments\ to\ EA$, nor for $Contested\ claims$.[39] The background for a withdrawal of an injunction is either that the customer makes a direct payment for the overdue debt to the supplier, or the two parties agree on an extension of maturity. In either case, the supplier will consequentially cancel the formal enforcement process. Both cases can be interpreted as reflecting firms trying to preserve and maintain an ongoing relationship, albeit the instance of an overdue claim. Hence, despite the initial involvement of the enforcement agency, co-operative outcomes appear to prevail.

We now turn to Panel B and the evaluation of mechanisms underlying downstream

average quarterly default rate in the pre-treatment period is 0.5 percent, as compared with 4.6 percent for settlements. This may raise concerns about the power of our tests involving $Defaults$ as outcome variable. Therefore, our empirical assessment does not rule out a statistically significant effect for defaults if a larger sample were at hand. Nevertheless, abstracting from statistical significance, the magnitude of the coefficient does not point in the direction of a sharp rise in the frequency of defaults.

[37]The test for parallel trends in the pre-treatment period demonstrates a significant difference in growth rate between treated and control firms for $Defaults$, cf. Row (2) in Column (III), which prevents a strong interpretation of the estimated treatment effect. The erratic behaviour displayed by $Defaults$ could be a source of distortion that also affects the intensive margin estimate for $Late\ payments$, which in turn may explain why the intensive margin estimate is statistically insignificant, cf. Row (1) in Column (II), as opposed to a statistically significant estimate of the extensive margin, cf. Row (1) in Column (I).

[38]If we consider the shorter 2010Q1-2012Q4 period with 2010Q1 as the pre-treatment period for Settlements, we obtain estimates (t-values) of 0.018 (1.4) and 0.285 (1.2) for the models in Columns (I) and (II), respectively. Hence, the point estimates are fairly close to the ones obtained when using the full period, 0.018 (2.6) and 0.227 (1.9), but t-values drop substantially in magnitude.

[39]Figure B2 in Appendix B provides further support for this conclusion. The increase in $Settlements$ for treated firms, relative to control firms, appears primarily to be due to shifts in $Withdrawals$.

adjustments by considering injunctions for overdue claims submitted by treated and control firms in the capacity of suppliers. Again, for this analysis we rely on the shorter sample period, and strong interpretations are thus unwarranted. Rows (1)-(3) show that the estimated effects for $Late\,payments$, and its two sub-components $Defaults$ and $Settlements$, are statistically insignificant. Moreover, for the three sub-components of $Settlements$ we find—consistent with upstream mechanisms—positive and statistically significant estimates for $Withdrawals$ at both the extensive and intensive margins, but statistically insignificant estimates for $Payments\;to\;EA$ and $Contested\;claims$. However, the significant increase in $Withdrawals$ does not feed into a significant effect for $Settlements$, nor in turn for $Late\,payments$. Thus, these results do not lend support to the presumption that treated firms, relative to control firms, attempt to enforce more late payments in the treatment period.

A summary of the insights gained from the analyses of the EA-data set suggests the following. The upstream analysis of the mechanisms underlying the previously documented adjustments in accounts payable indicates that these are associated with shifts in overdue payments. That is, treated firms extract liquidity from their suppliers by postponing payments on trade credit debt. In coherence with a risk sharing perspective, the dominance of withdrawals as final outcomes of applications to the enforcement agency points towards an inherently co-operative nature of this maturity shifting process.[40] Turning to the downstream analysis of mechanisms, our results do not provide conclusive evidence for treated firms increasing enforcements of late payments from customers. This may be due to the treated firms' reduction of accounts receivable—documented in the previous section—being primarily achieved through a shortening of contracted net days on issued trade credit, rather than an increased enforcement of overdue payments. Moreover, in this context it is worth noting that our

[40] In line with the research on efficient informal insurance arrangements constrained by limited commitment, discussed by Ligon, Thomas and Worrall (2002) and Kocherlakota (1996), there may be limitations to the amount of extra liquidity that suppliers are able or willing to supply to distressed customers in adverse situations. If the liquidity shortfalls are sufficiently large, we should observe an increased number of cases in which suppliers have reached and surpassed the constraint on the amount of extra liquidity that they are willing to supply, and by involving the EA they signal this to the distressed firms. However, even though formalized enforcement through the EA is at hand, most of the claims are withdrawn by the suppliers, which suggests that the suppliers and customers have been able to reach mutual agreements. That is, customers mostly choose not to default on supplier claims, or in other words, they mostly choose to adhere to the informal rules of the network and not renege. So, an apparently non-cooperative equilibrium involving outside enforcement support from the EA, nevertheless typically ends in a way that benefits both parties and enables a continuation of their business relationship.

measure of overdue credit—derived from the EA-data—presumably tends to capture rather long payment delays, and accordingly it is likely that many overdue claims on slow-paying customers do not result in formal applications to the EA, which suggests that we do not fully capture the treated firms propensity to postpone payments to suppliers, nor their attempts to foster or enforce prompt repayments from customers.

5 Conclusions

Recent research has shown that the buffer motive plays a prominent role for firms' choices of cash holdings. Another conceivably important source of reserve liquidity is adjustment capacity at the trade credit margins—accounts payable and receivable—on firms' balance sheets. In this paper, we empirically gauge how trade credit positions, next to cash holdings, are used by firms to curb the impacts of shortfalls in liquidity. To this end, we evaluate the effects of liquidity shortfalls generated in the fraud and failure of a large Swedish cash-in-transit firm and imposed on its clients. These unique events provide an opportunity to derive inference on the roles played by cash holdings, and trade credit margins to handle liquidity shortfalls.

Our contribution can be summarized by the following main findings. Firstly, firms handle adverse liquidity shortfalls by drawing down on their cash holdings, by increasing the amount of drawn credit from suppliers (accounts payable), and by decreasing the amount of issued credit to suppliers (accounts receivable). Secondly, in terms of average magnitudes, upstream adjustments dominate downstream adjustments; and the compounded adjustment at the two trade credit margins is found to be of a similar order as adjustments in cash holdings, suggesting that trade credit positions indeed constitute important sources of reserve liquidity. Thirdly, adjustment capacity in cash holdings and at the trade credit margins appear to be complements, and in particular credit constrained firms rely on combinations of these sources to handle liquidity shocks. Finally, by exploring the underlying mechanism of the trade credit adjustments, we find evidence that the observed changes are due to shifts in overdue payments—firms in need of liquidity increase duration on their trade credit upstream by postponing payments beyond the due date.

As Cuñat (2007) points out, establishing the role of trade credit in firms' liquidity management may provide important insights to the widespread use of trade credit. More specifically, recent research has asked the question why trade credit is so widely used despite appearing very costly in some cases. The findings in this paper corroborate the view that such implicit costs in the underlying trade credit contracts could well be motivated by the insurance properties embedded in the risk-sharing arrangements in trade credit networks.

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Table 1: Panaxia AB—Performance and external financing

	2006	2007	2008	2009	2010	2011
A. Performance						
Total sales (in MSEK)	197.0	409.5	517.4	677.1	729.6	574.1
Sales growth	—	107.9%	26.4%	30.9%	7.8%	–21.3%
Total assets (in MSEK)	268.2	515.3	914.5	852.8	899.8	854.3
Net income (in MSEK)	7.4	8.6	29.7	–7.2	–85.4	–36.8
Net income/Assets	2.8%	1.7%	3.2%	–0.8%	–9.5%	–4.3%
B. External financing						
Bank debt (in MSEK)	140.7	255.5	568.4	365.3	334.3	235.5
Bank debt/Assets	52.5%	49.6%	62.2%	42.8%	37.2%	27.6%
Change in bank debt	—	81.7%	122.4%	–35.7%	–8.5%	–29.6%

This table reports information on the performance and external financing of Panaxia AB, obtained from the consolidated financial statements over the period 2006–2011.

Table 2: Descriptive statistics for treated, non-treated, and matched control firms

	A. Treated firms		B. Non-treated (weighted)			C. Matched control firms		
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
	Mean	SD	Mean	SD	$\Delta_{co,tr}$	Mean	SD	$\Delta_{co,tr}$
1. Exposure								
$Exposure_{2012}/Assets_{2012}$	0.079	0.108	—	—	—	—	—	—
$Loss_{2012}/Assets_{2012}$	0.043	0.051	—	—	—	—	—	—
2. Firm characteristics								
$Cash\ flow/Assets_{2009}$	0.083	0.144	0.087	0.177	–0.027	0.087	0.141	–0.033
$Assets_{2009}$ (in MSEK)	33.355	76.413	8.851	91.406	0.291	27.446	69.623	0.081
$Sales\ growth_{2009}$	0.047	0.297	0.017	0.352	0.093	0.027	0.269	0.071
$Debt/Assets_{2009}$	0.168	0.247	0.230	0.270	–0.239	0.175	0.235	–0.029
$Tangible\ assets/Assets_{2009}$	0.200	0.234	0.302	0.279	–0.397	0.216	0.241	–0.069
$Inventories/Assets_{2009}$	0.276	0.203	0.248	0.244	0.127	0.278	0.206	–0.009
Age_{2009}	14.887	16.796	15.971	13.566	–0.071	14.093	14.992	0.050
$Cash/Assets_{2009}$	0.179	0.173	0.251	0.229	–0.356	0.184	0.183	–0.028
$Payables/Assets_{2009}$	0.242	0.158	0.162	0.150	0.518	0.232	0.155	0.065
$Receivables/Sales_{2009}$	0.021	0.041	0.033	0.073	–0.206	0.028	0.042	–0.170
$Cash/Assets_{2008}$	0.179	0.170	0.246	0.226	–0.331	0.181	0.181	–0.007
$Payables/Assets_{2008}$	0.273	0.191	0.172	0.157	0.576	0.264	0.184	0.046
$Receivables/Sales_{2008}$	0.022	0.046	0.033	0.070	–0.178	0.029	0.045	–0.142
Number of observations	610		49,633			610		
Number of unique firms	610		49,633			482		

This table reports descriptive statistics for treated firms (Panel A), non-treated firms (Panel B), and matched control firms (Panel C). The descriptive statistics for non-treated firms in Panel B are constructed using weights corresponding to the fraction of treated firms in each particular five-digit industry. The loss-variable is calculated based on the group of treated firms that incurred losses in 2012. $\Delta_{co,tr}$ denotes a normalized difference and is calculated as: $\left(\bar{X}_{tr} - \bar{X}_{co}\right)/\sqrt{\left(S^2_{tr} + S^2_{co}\right)/2}$, where $\bar{X}$ is the mean, S is the standard deviation, and sub-indices tr and co denote treated firms and control firms, respectively. The normalized differences in Panels B and C compare covariate outcomes for treated firms with those of non-treated firms and matched control firms, respectively. Variable definitions are provided in Table B2.

Table 3: Baseline estimates

	Treatment period			Post-treatment period	Test of parallel pre-trends
	(I)	(II)	(III)	(IV)	(V)
	2010	2011	2012	2013	p-val.
A. $y = Cash/Assets$					
(1) τ_t	–0.020**	–0.011*	0.008	–0.009	0.832
	(–2.4)	(–1.9)	(1.2)	(–0.7)	
(2) T_t	–0.020**	–0.031***	–0.024***	–0.032***	
	(–2.4)	(–3.8)	(–3.1)	(–2.8)	
B. $y = Payables/Assets$					
(3) τ_t	–0.001	0.011**	0.018*	0.000	0.648
	(–0.2)	(2.4)	(1.7)	(0.0)	
(4) T_t	–0.001	0.01	0.028***	0.028**	
	(–0.2)	(1.2)	(3.2)	(2.6)	
C. $y = Receivables/Sales$					
(5) τ_t	–0.003**	–0.002	–0.006**	0.000	0.291
	(–2.1)	(–1.0)	(–2.5)	(0.1)	
(6) T_t	–0.003**	–0.004**	–0.010***	–0.010***	
	(–2.1)	(–2.4)	(–3.3)	(–3.1)	
Number of firms		610/610/482			

This table reports estimates of yearly adjustments, Eq. (1), and cumulative adjustments, Eq. (2), in cash holdings, accounts payable, and accounts receivable, over the treatment and post-treatment periods. The tests of parallel pre-trends are conducted for the 2007–2009 period, and follow the approach proposed by Mora and Reggio (2015). Variable definitions are provided in Table B2. The numbers of firms reported in the bottom line refer to treated firms, matched control firms, and unique matched control firms, respectively. t-values, reported in parenthesis, are calculated using robust standard errors adjusted for clusters in two dimensions: firstly, at the firm-level for non-franchisees and at the franchisor-level for franchisees; and secondly, at the level of matched pairs. ***, **,* denote statistically distinct from 0 at the 1, 5, and 10 percent levels, respectively.

Table 4: Alternative specifications

		$y = Cash/Assets$		$y = Payables/Assets$		$y = Receivables/Sales$		
		(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
	t	T_t^y	t-val.	T_t^y	t-val.	T_t^y	t-val.	No. of firms
(1) Baseline estimates	2012	–0.024***	(–3.1)	0.028***	(3.2)	–0.010***	(–3.3)	610/610/482
(2) Non-treated as control group	2012	–0.016***,+	(–3.0)	0.022***	(4.2)	–0.006***	(–3.9)	610/49,633
(3) Bias-adjusted estimates	2012	–0.025***	(–3.2)	0.029***	(3.3)	–0.012***	(–4.0)	610/610/482
(4) 50 percent best matches	2012	–0.041***	(–4.1)	0.029**	(2.4)	–0.009**	(–2.0)	305/305/245
(5) $Payables$ scaled by $COGS$	2012	–0.016	(–1.2)	0.025***	(2.8)	–0.001***,+	(–4.4)	109/109/44
(6) Truncated	2012	–0.019**	(–2.1)	0.032***	(2.9)	–0.009**	(–2.6)	521/521/402
(7) Franchisees omitted	2012	–0.015	(–1.6)	0.020***	(2.8)	–0.008***	(–2.8)	376/376/362
(8) Pharmacies included	2012	–0.022***	(–2.9)	0.028***	(3.2)	–0.010***	(–3.3)	617/617/487
(9) Unbalanced	2012	–0.019**	(–2.4)	0.043***	(4.6)	–0.010***	(–3.4)	641/641/505
(10) Non-standardized acc. data	2012	–0.024***	(–2.6)	0.026***	(2.8)	–0.008**	(–2.5)	610/610/482
(11) Acc. period ends in Dec.	2010	–0.027***	(–2.7)	–0.003	(–0.3)	–0.004***	(–2.6)	463/463/339
(12) Acc. period ends prior to Dec.	2010	0.002	(0.3)	0.003+	(0.5)	0.002	(1.1)	147/147/146
(13) Bias-adjusted pscore matching	2012	–0.017**	(–2.5)	0.039***	(5.6)	–0.007***	(–3.4)	610/610/610

This table reports estimates of cumulative adjustments, Eq. (2), in 2012. Row (1) reports the baseline results from Table 3; Row (2) reports results where the non-treated firms are used as control group (means for non-treated firms are calculated using weights corresponding to the fraction of treated firms in each particular five-digit industry); Row (3) reports bias-adjusted estimators according to Abadie and Imbens (2011); Row (4) reports results for the 50 percent closest matches; Row (5) reports results for payables scaled by the cost of goods sold ($COGS$), where the sample is restricted to the pairs of treated and matched control firms that report $COGS$; Row (6) reports results for a sample where the variables are truncated at the 1st and 99th percentiles; Row (7) reports results where franchisee firms are omitted; Row (8) reports results when pharmacies are included; Row (9) reports results for an unbalanced panel; Row (10) reports results using non-standardized accounting data; Row (11) reports cumulative effects in 2010 using non-standardized accounting data for the sub-sample of treated firms with accounting periods that end in December; Row (12) reports cumulative effects in 2010 using non-standardized accounting data for the sub-sample of treated firms with accounting periods that end in other months than December; and Row (13) reports results from propensity score matching, implemented with bias-adjustment and without replacement. Variable definitions are provided in Table B2. The numbers of firms reported in the bottom line refer to treated firms, matched control firms, and unique matched control firms, respectively. t-values, reported in parenthesis, are calculated using robust standard errors adjusted for clusters in two dimensions: firstly, at the firm-level for non-franchisees and at the franchisor-level for franchisees; and secondly, at the level of matched pairs. ***, **,* denote statistically distinct from 0 at the 1, 5, and 10 percent levels, respectively. + denotes statistically distinct deviations in pre-treatment trends at the 5 percent level.

Table 5: Treatment effects conditional on loss-size

Panel A.	Incurred bankruptcy losses in 2012					
	No		Yes		t-test	
	(I)	(II)	(III)	(IV)	(V)	(VI)
	T^{y}_{2012}	t-val.	T^{y}_{2012}	t-val.	H_0	p-val.
Baseline specification						
(1) $y = Cash/Assets$	–0.015	(–0.8)	–0.026***	(–2.9)	No loss $\leq$ Loss	0.297
(2) $y = Payables/Assets$	0.021	(1.5)	0.029***	(2.9)	Loss $\leq$ No loss	0.319
(3) $y = Receivables/Sales$	–0.008	(–1.6)	–0.010***	(–3.0)	No loss $\leq$ Loss	0.353
Baseline specification with bias-adjustment						
(4) $y = Cash/Assets$	–0.016	(–0.9)	–0.027***	(–3.1)	No loss $\leq$ Loss	0.283
(5) $y = Payables/Assets$	0.020	(1.4)	0.028***	(2.8)	Loss $\leq$ No loss	0.314
(6) $y = Receivables/Sales$	–0.008*	(–1.7)	–0.012***	(–3.4)	No loss $\leq$ Loss	0.274
Number of firms	116/116/116		494/494/367			

Panel B.	Outcome variable					
	$Cash/$ $Assets$	$Payables/$ $Assets$	$Receivables/$ $Sales$	$Cash/$ $Assets$	$Payables/$ $Assets$	$Receivables/$ $Sales$
	(I)	(II)	(III)	(IV)	(V)	(VI)
$Event_t \times Loss/Assets_{2012}$	–0.002	0.206**	–0.052**	–0.157	0.949***	–0.063
	(–0.0)	(2.3)	(–2.0)	(–0.5)	(5.4)	(–1.2)
$Event_t \times Loss/Assets^2_{2012}$				0.996	-4.789***	0.072
				(0.5)	(–3.5)	(0.2)
Marginal effect at the mean	—	—	—	–0.071	0.537***	–0.057*
				(–0.6)	(6.5)	(–2.0)
Number of firms	610/610/482					

Panel A reports estimates of cumulative adjustments, Eq. (2), in 2012 for the sub-sample of treated firms that were fully compensated for bankruptcy losses in 2012, Columns (I) and (II), and for the sub-sample of treated firms that incurred losses in 2012, Columns (III) and (IV). Rows (1) to (3) report estimates for the baseline specification, and Rows (4) and (5) for the baseline specification with bias-adjustment. p-values refer to one-sided tests for differences in coefficients between the sub-samples. Panel B reports results from estimations of Eq. (3). Variable definitions are provided in Table B2. The numbers of firms reported in the bottom lines of each panel refer to treated firms, matched control firms, and unique matched control firms, respectively. t-values, reported in parenthesis, are calculated using robust standard errors adjusted for clusters in two dimensions: firstly, at the firm-level for non-franchisees and at the franchisor-level for franchisees; and secondly, at the level of matched pairs. ***, **,* denote statistically distinct from 0 at the 1, 5, and 10 percent levels, respectively.

Table 6: Treatment effects conditional on credit constraints

	A. Firm size						B. Rating					
	Constrained		Unconstrained		t-test		Constrained		Unconstrained		t-test	
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)	(X)	(XI)	(XII)
	T^y_{2012}	t-val.	T^y_{2012}	t-val.	H_0	p-val.	T^y_{2012}	t-val.	T^y_{2012}	t-val.	H_0	p-val.
Baseline specification												
(1) $y = Cash/Assets$	–0.036***	(–3.4)	–0.001	(–0.0)	U < C	0.012	–0.029***	(–3.1)	–0.017	(–1.0)	U < C	0.277
(2) $y = Payables/Assets$	0.035***	(2.8)	0.016	(1.6)	C < U	0.110	0.037***	(2.9)	0.012	(1.1)	C < U	0.041
(3) $y = Receivables/Sales$	–0.011**	(–2.4)	–0.008**	(–2.5)	U < C	0.274	–0.013***	(–2.8)	–0.003	(–1.4)	U < C	0.025
(4) $y = Short\text{-}term\ bank\ debt/Assets$	–0.003	(–0.7)	0.007	(1.0)	U < C	0.101	–0.004	(–1.1)	0.011**	(2.2)	U < C	0.006
Baseline specification with bias-adjustment												
(5) $y = Cash/Assets$	–0.036***	(–3.3)	–0.011	(–0.9)	U < C	0.066	–0.033***	(–3.5)	–0.016	(–0.9)	U < C	0.206
(6) $y = Payables/Assets$	0.026**	(2.1)	0.023**	(2.3)	C < U	0.421	0.036***	(2.9)	0.012	(1.1)	C < U	0.055
(7) $y = Receivables/Sales$	–0.013***	(–2.9)	–0.008***	(–2.7)	U < C	0.180	–0.015***	(–3.2)	–0.003	(–1.3)	U < C	0.010
(8) $y = Short\text{-}term\ bank\ debt/Assets$	0.001	(0.2)	0.010	(1.5)	U < C	0.106	–0.002	(–0.5)	0.012**	(2.4)	U < C	0.013
Number of firms	346/346/232		148/148/140				347/347/243		148/148/137			

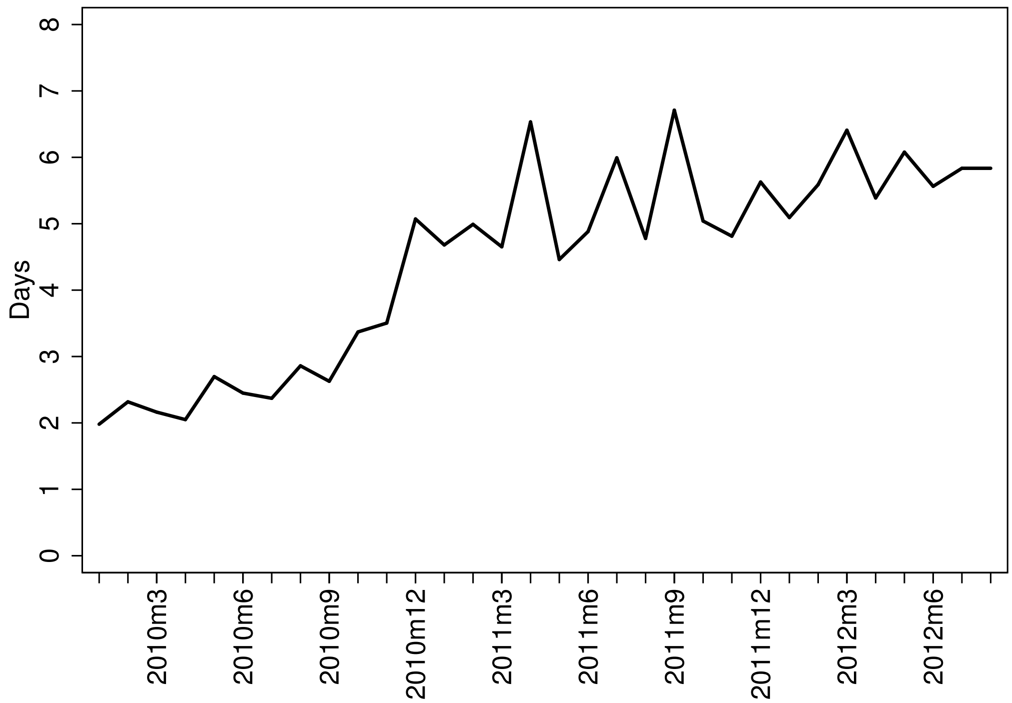
This table reports estimates of cumulative adjustments, Eq. (2), for cash holdings, accounts payable, accounts receivable, and short-term bank debt, in 2012. Rows (1) to (4) report estimates for the baseline specification. Rows (5) and (8) report estimates for the baseline specification with bias-adjustment. The models are estimated on sub-samples classified with respect to treated firms' total assets (Panel A) and credit ratings (Panel B). For each classification variable, firms in the bottom 7 deciles are classified as constrained and firms in the top 3 as unconstrained. p-values refer to one-sided tests for differences in coefficients between the sub-samples. Variable definitions are provided in Table B2. The numbers of firms reported in the bottom line refer to treated firms, matched control firms, and unique matched control firms, respectively. t-values, reported in parenthesis, are calculated using robust standard errors adjusted for clusters in two dimensions: firstly, at the firm-level for non-franchisees and at the franchisor-level for franchisees; and secondly, at the level of matched pairs. ***, **,* denote statistically distinct from 0 at the 1, 5, and 10 percent levels, respectively.

Table 7: Late payments—upstream and downstream

	A. Upstream					B. Downstream				
	Specification					Specification				
	No/Yes (0/1)	$ln(1+N)$		Estimation	Pre-treatment	No/Yes (0/1)	$ln(1+N)$		Estimation	Pre-treatment
	OLS	OLS	Tobit	period	period	OLS	OLS	Tobit	period	period
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)	(X)
(1) *Late payments*	0.017**	0.013	0.188	07Q1-12Q4	07Q1-09Q4	0.013	0.011	0.382	10Q1-12Q4	10Q1
	(2.3)	(1.5)	(1.5)			(1.4)	(1.2)	(1.5)		
	[0.642]	[0.874]	[0.719]			—	—	—		
(2) *Defaults*	–0.003	–0.003	–0.296	07Q1-12Q4	07Q1-09Q4	0.002	–0.002	0.031	10Q1-12Q4	10Q1
	(–0.9)	(–1.0)	(–0.8)			(0.2)	(–0.3)	(0.1)		
	[0.274]	[0.192]	[0.000]			—	—	—		
(3) *Settlements*	0.018**	0.015*	0.227*	07Q1-12Q4	07Q1-09Q4	0.012	0.012	0.449	10Q1-12Q4	10Q1
	(2.6)	(1.9)	(1.9)			(1.4)	(1.3)	(1.5)		
	[0.502]	[0.832]	[0.576]			—	—	—		
(4) *Withdrawals*	0.022*	0.021**	0.393*	10Q1-12Q4	10Q1	0.021**	0.021**	0.891***	10Q1-12Q4	10Q1
	(1.8)	(2.0)	(1.7)			(2.5)	(2.3)	(2.9)		
(5) *Payments to EA*	0.001	0.003	0.126	10Q1-12Q4	10Q1	–0.007	–0.003	–0.726	10Q1-12Q4	10Q1
	(0.2)	(0.6)	(0.3)			(–1.5)	(–0.7)	(–1.6)		
(6) *Contested claims*	–0.001	0.001	–0.177	10Q1-12Q4	10Q1	–0.005	–0.005	–0.313	10Q1-12Q4	10Q1
	(–0.1)	(0.2)	(–0.4)			(–0.9)	(–0.8)	(–0.8)		
Number of firms	610/610/482									

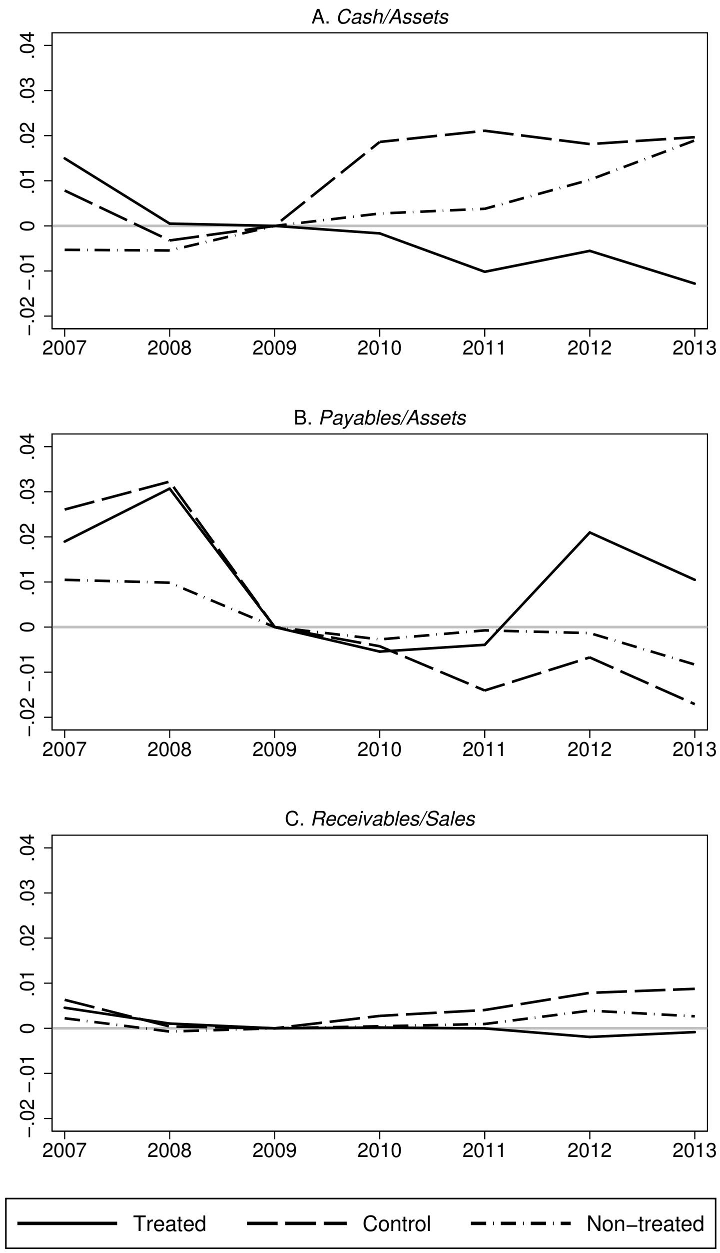
This table reports difference-in-differences estimates from Eq. (4). Panel A reports results for applications faced by firms (upstream perspective) and Panel B for applications issued by firms (downstream perspective). The tests of parallel pre-trends are conducted using the 2007Q1–2009Q4 period, and follow the approach proposed by Mora and Reggio (2015); results are reported as p-values within square brackets. Variable definitions are provided in Table B2. The numbers of firms reported in the bottom line refer to treated firms, matched control firms, and unique matched control firms, respectively. t-values, reported in parenthesis, are calculated using robust standard errors adjusted for clusters in two dimensions: firstly, at the firm-level for non-franchisees and at the franchisor-level for franchisees; and secondly, at the level of matched pairs. ***, **,* denote statistically distinct from 0 at the 1, 5, and 10 percent levels, respectively.

Figure 1: Panaxia—Time from collection to transfer



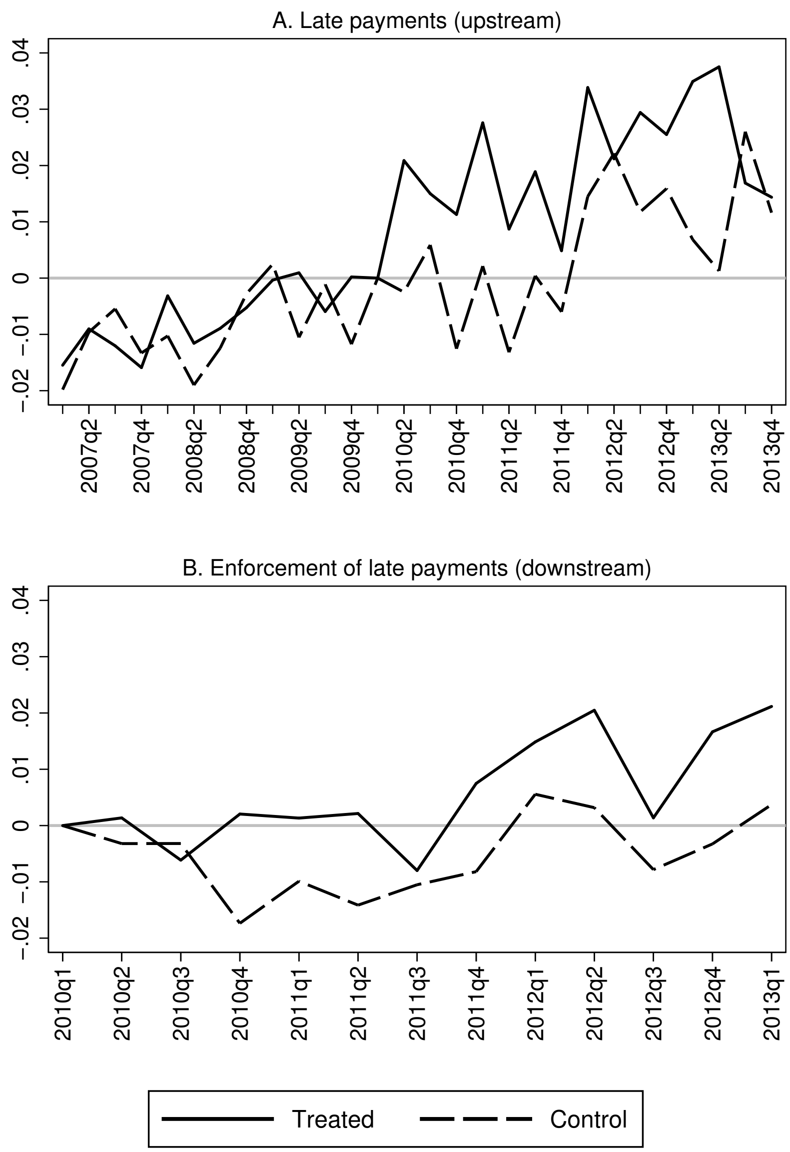
This figure illustrates, at a monthly frequency, the average number of days that Panaxia held on to their clients' proceeds before transfer, in the period running up to the bankruptcy. The time period covers January, 2010, to August, 2012.

Figure 2: Means of balance-sheet outcome variables



This figure reports normalized means for the three main outcome variables: $Cash/Assets$, $Payables/Assets$, and $Receivables/Sales$, over the period 2007–2013, for treated firms (solid line), non-treated firms (dashed line), and matched control firms (dashed-dotted line). The values are normalized by 2009-outcomes. In each year, only pairs for which there are data on both treated and control firms are included. Means for non-treated firms are calculated using weights corresponding to the fraction of treated firms in each five-digit industry.

Figure 3: Late payments and enforcement of late payments



This figure reports the natural logarithm of one plus the number of late payments. Panel A shows late payments by treated firms (solid line) and matched control firms (dashed line) for the period 2007Q1–2013Q4. Panel B shows the enforcement of late payments by treated firms (solid line) and matched control firms (dashed line) for the period 2010Q1–2013Q1.

Appendix A

Accounting Practices, Measurement of Cash Adjustments, and Implications for ATT on Cash Holdings

The accounting rules in Sweden—which adhere to the International Financial Reporting Standards (IFRS)—do not indicate a single appropriate measure for a firm to correctly book cash which is in transit. There are in principle three possibilities open to firms for accounting for cash-in-transit; two of these are very close, but for clarity and completeness we will distinguish between them in what follows.

Firstly, the least cumbersome way for the firm is to not re-book, but simply let the cash-in-transit remain a part of the bills and coins account on the books, until notice is received about the transfer to the bank account having been completed (denoted Practice 1A); where both the bills and coins account and the bank account are sub-accounts of the cash account. Secondly, the firm can book the money picked up by the cash-in-transit firm on a cash-in-transit account, i.e., another sub-account under the cash account, whilst the money is on its way to the bank account (denoted Practice 1B). That is, the firm makes a distinction between cash-in-transit and other components under the cash account during the transfer period. Once the funds reach the bank account, they are re-booked as bank-holdings and cease to be cash-in-transit holdings. Finally, the third possible accounting measure is for the firm to book the cash-in-transit as a short-term claim on the cash-in-transit firm, and then re-book it as bank-holdings under the cash account once the money is obtained from Panaxia (denoted Practice 2). By and large, Practices 1 and 2 differ in that under Practice 1, cash-in-transit remains booked under the cash account throughout, whereas under Practice 2 the funds are temporarily booked as short-term claims when in Panaxia's hands. Practices 1A and 1B differ in that under 1A funds are not re-booked while in transit, whereas for 1B cash-in-transit is temporarily re-booked to a sub-account under the cash account while being in transit.[A1]

[A1] Swedish firms anticipating a potential future write-off, should rebook a claim with a low likelihood of repayment as a reservation. This accounting practice is common for doubtful accounts receivable; for claims on non-paying customers that are 60 days, or more, past their due dates, reservations should be made. However, it is unlikely that Panaxia's clients made reservations on their cash-in-transit claims during the fraud period prior to the bankruptcy, since the transfer periods in 2010 and 2011, although considerably prolonged, were around 5 to 6 days. The funds withheld by Panaxia were continuously and

To illustrate how Practices 1 and 2 differently affect the measurement of cash holdings on firms' accounting statements, we now present a simplified example. Consider a firm's cash flow, CF_t, i.e., the difference between its inflows of funds, $Inflow_t$, and its outflows of funds, $Outflow_t$. Initially, we will assume that the firm balances all fluctuations in cash flow using its cash holdings, CH_t, only. This implies that $\Delta CH_t = CH_t - CH_{t-1} = CF_t$. In other words, we initially abstract from the presence of other potential liquidity sources—such as trade credit or bank financing—available to the firm. Column (I) in Table A1 shows how cash holdings evolve over the period 2009–2012 for a firm which is not subject to a cash-in-transit firm fraud.

Shifting focus to the case of the Panaxia fraud, a fraction α_t of $Inflow_t$ is unduly withheld in contract violation in each year of the treatment period. Columns (II) and (III) in Panel A show how the cash holdings and short-term claims accounts on the accounting statement evolved under Practices 1A and 1B, and the same columns in Panel B show the cash and short-term claims accounts under Practice 2. Column (IV) shows the differences in cash holding outcomes between the case of fraud (Column (II)) and the counterfactual of no fraud (Column (I)). Column (IV) in Panel A shows that under Practices 1A and 1B, there are no differences in the accounting measure of cash holdings between the fraud and no fraud cases in 2010 and 2011, since the firms subject to fraud book cash-in-transit under cash holdings. In 2012, however, there is a relative decline in cash holdings for fraud-exposed firms incurring losses when Panaxia enters bankruptcy. That is, the realized bankruptcy losses in 2012 induce firms to write off the withheld amounts from their cash accounts.

Under Practice 2, Column (III) in Panel B shows that fraud-exposed firms book cash-in-transit under a short-term claims account. This results in a relative decline in cash holdings from the point in time when Panaxia starts to delay transfers of cash-in-transit, cf. Columns (II) and (IV), Panel B. That is, the relative decline starts in 2010, and continues throughout 2012. The decline in each year is proportional to the increase in

consistently transferred to the clients' bank accounts, but with a time lag—long enough to matter for clients' liquidity positions, but not long enough to raise concerns for a looming failure and subsequent losses. Had clients begun to anticipate potential losses due to a forthcoming Panaxia failure, they would presumably have aborted purchases of Panaxia services immediately, and not merely resorted to reservations. This issue is related to the setting of the fraud and the sustainability of the Ponzi-like scheme implemented by Panaxia's management, which hinged on its ability to preserve the customer base over time, cf. the discussion in Section 2.1.

the fraction withheld, α_t.[A2] Thus, depending on choice of accounting practice, implications for relative cash holdings in 2010 and 2011 differ, but not so in 2012. In 2012, due to the Panaxia bankruptcy, withheld cash-in-transit results in a loss to be written off irrespective of whether the funds were booked under cash holdings (Practice 1A and 1B), or under short-term claims (Practice 2), and thus induces a change in cash holdings either way. We will now proceed to a discussion on how the accounting practices may influence the interpretation of our results.

In the simplified example outlined above, a one-to-one relationship between cash holdings and cash flow was assumed; in other words, firms rely completely on cash to manage variations in cash flow. However, this picture changes when we more realistically introduce other liquidity sources at firms' disposal. For example, let us consider trade credit and bank financing. By postponing trade credit payments, accounts payable, a firm can balance parts, or the full, Panaxia-withheld inflow of funds, $\alpha_t Inflow_t$, by postponing parts of its outflows directed to suppliers. Similarly, by using a bank line of credit the firm can balance parts, or the full, withheld inflow of funds. Another potential measure available to the firm is to reduce maturities on extended trade credit, accounts receivable, which would then lead to an upward push for $Inflow_t$ in that year. Thus, in this multi-source scenario we can only observe a relative decline in cash holdings for firms that indeed rely on cash to balance withheld inflows, and need not necessarily observe any decline in cash holdings for firms that rely on other financing sources.[A3] One caveat in our analysis is that for Practices 1A and 1B, we will underestimate the reliance on cash holdings in 2010 and 2011; usage of other financing sources could even lead to an upward push of cash holdings in 2010 and 2011. To see this, let us assume that the firm completely balances the amount withheld, $\alpha_t Inflow_t$, by postponing payments to suppliers. This means that $Outflow_t$—which affects cash holdings through $\Delta CH_t \equiv CF_t$—is reduced by $\alpha_t Inflow_t$. In this example, the reduc-

[A2]Note that when Panaxia finally transfers withheld cash-in-transit, the firm's cash account is credited (by way of the sub-account bank-holdings), and the short-term claims account is debited with the withheld amount. This explains why we can use the same notation for cash holdings, CH_t, in Columns (I) and (II). More specifically, for the determination of the values of CH_t^α for 2011 and 2012 in Column (II), withheld cash-in-transit in the previous year becomes liquid and part of cash holdings in the current year, such that $CH_{t-1} = CH_{t-1}^\alpha + \alpha_{t-1} Inflow_{t-1}$.

[A3]This reasoning aligns with Almeida, Campello and Weisbach (2004), who examine the cash flow sensitivity of cash, and propose that a positive relationship between cash flow and cash holdings should only be observed for financially constrained firms.

tion in $Outflow_t$, amounting to $\alpha_t Inflow_t$, leads to a corresponding relative increase in cash holdings of the same size. The important implication of this is that the fraud cannot give rise to a mechanical decline in cash holdings in the presence of alternative liquidity sources affecting CF_t, and therefore ΔCH_t. Hence, declines in accounted cash holdings reflect firms' decisions to use their cash holdings to balance withheld funds due to Panaxia's delayed transfers.

To conclude, the above suggests a caveat in our analysis in that for firms applying Practices 1A and 1B, we will underestimate their reliance on cash in 2010 and 2011 because their accounted cash holdings include withheld and therefore illiquid funds. Moreover, in the presence of multiple liquidity sources, there cannot be a mechanical fraud effect on firms' cash holdings.

Which practice do Swedish firms use? The general view among professional and academic accountants is that under normal circumstances—when transfer times are well within the contracted two days—cash-in-transfer most likely will remain booked under the cash account, i.e., Practice 1A or 1B. However, when transfer times increase in duration, it becomes conceptually less clear that cash-in-transfer should continue to be booked under the cash account, but should instead be booked as a short-term claim, i.e., Practice 2, reflecting the increased illiquidity. The results in Section 3 are consistent with the use of Practice 2 during the treatment period. More specifically, the results in Table 3 and in the right-hand side of Table B5 (firms that incurred a loss) show that the decline in cash is strongest in the beginning of the treatment period and no effect in 2012. Furthermore, results in the left-hand side of Table B5 (firms that were compensated for their losses) show a statistically significant increase in cash holdings in 2012. This result suggests that firms on average booked the cash-in-transfer under short-term claims and then filled up cash holdings again upon being compensated in 2012. In addition, a shift from Practices 1A or 1B to Practice 2 could potentially contribute to the pronounced effect for cash in 2011, cf. Table 3; that is, the upward shift in delivery times at the end of 2010 affects booked cash holdings in the year after, due to a shift in accounting practice.

If Practice 2 prevails, we should observe an upward shift in one of the short-term claims accounts on the balance sheet, where the cash-in-transfer is booked. The short-term claims in the accounting statements in our data consist of three gross components: 'accounts receivable'; 'short-term claims on group firms'; and 'other short-term claims'. Thus, intuitively, cash-in-transfer should be booked under the account referred to as 'other short-term claims'. This is however a residual account that contains other potentially large components, such as claims related to tax payments. This is illustrated by 'other short-term claims' scaled by total assets on average amounting to 22 percent for treated and control firms in 2009. Nevertheless, when estimating cumulative treatment effects for the outcome variable other short-term claims-to-assets, we obtain estimates (t-value) of 0.018 (2.4), 0.026 (3.1), 0.012 (0.8), and 0.040 (4.1) for years 2010, 2011, 2012, and 2013, respectively. The upward shift in 'other short-term claims' in 2010 and 2011 is consistent with cash-in-transfer being booked under this account. The estimated effects are small in magnitude, however. If Practice 2 indeed prevails, we would expect coefficients that exceed adjustments in cash holdings. Our results may reflect that the events also affected other components on the 'other short-term claims' account. For instance, the cumulative effect in 2013 is large and significant, which is obviously unrelated to shifts in cash-in-transfer.

Taken together, due to a fraction of treated firms having potentially applied Practices 1A and 1B, we caution the interpretation of estimated cash effects in 2010 and 2011; cash-in-transfer may have been booked under the cash account, which would imply that our estimates understate the treatment effect on cash. In 2012, however, the choice of accounting practice does not matter for the cash estimates.

Table A1: Illustrative example of the consequences of different accounting practices in cases of fraud and no fraud

	No fraud	Fraud		
	(I)	(II)	(III)	(IV)
	CH_t	CH_t^α	$Short\text{-}term\ claim_t$	$CH_t^\alpha - CH_t$
A. Accounting practices 1A and 1B				
(1) 2009	$CH_{2008} + CF_{2009}$	$CH_{2008} + CF_{2009}$	0	0
(2) 2010	$CH_{2009} + CF_{2010}$	$CH_{2009} + CF_{2010}$	0	0
(3) 2011	$CH_{2010} + CF_{2011}$	$CH_{2010} + CF_{2011}$	0	0
(4) 2012	$CH_{2011} + CF_{2012}$	$CH_{2011} + (1 - \alpha_{2012})Inflow_{2012} - Outflow_{2012}$	0	$-\alpha_{2012}Inflow_{2012}$
B. Accounting practice 2				
(5) 2009	$CH_{2008} + CF_{2009}$	$CH_{2008} + CF_{2009}$	0	0
(6) 2010	$CH_{2009} + CF_{2010}$	$CH_{2009} + (1 - \alpha_{2010})Inflow_{2010} - Outflow_{2010}$	$\alpha_{2010}Inflow_{2010}$	$-\alpha_{2010}Inflow_{2010}$
(7) 2011	$CH_{2010} + CF_{2011}$	$CH_{2010} + (1 - \alpha_{2011})Inflow_{2011} - Outflow_{2011}$	$\alpha_{2011}Inflow_{2011}$	$-\alpha_{2011}Inflow_{2011}$
(8) 2012	$CH_{2011} + CF_{2012}$	$CH_{2011} + (1 - \alpha_{2012})Inflow_{2012} - Outflow_{2012}$	0	$-\alpha_{2012}Inflow_{2012}$

This table shows how different accounting practices influence the measurement of relative adjustments in cash holdings in a comparison of a firm that experienced the Panaxia fraud with the counterfactual outcome of a firm that did not experience the fraud. The example abstracts from influences of trade credit and bank financing.

Appendix B

Supplementary Tables and Figures

Table B1: Sample characteristics—Number of Panaxia clients

Panel A.	Total	Uncompensated firms (*Item 1*)	Franchisees (*Item 2*)	Compensated by savings bank (*Item 3*)	Pharmacies (*Items 1 and 2*)
1. Unidentified firms	38	18	20	0	0
2. Financial firms	13	13	0	0	0
3. Non-incorporated entities	173	43	0	130	0
4. Pharmacies	131	0	0	0	131
5. Non-financial corporations					
5.1. Franchisor	1	1	0	0	0
5.2. With missing accounting data	289	74	175	40	0
5.3. With accounting data (final sample)	610	260	234	116	0
Total number of firms	1255	409	429	286	131

Panel B.	2007	2008	2009	2010	2011	2012	2013
1. Non-financial corporations							
1.1. Continuing firms	599	692	819	856	884	899	897
1.2. New firms	55	93	127	37	28	15	0
1.3. Failures	0	0	0	0	0	2	5
1.4. Firms in final sample	543	610	610	610	610	610	610
2. Pharmacies							
2.1. Continuing firms	6	23	25	107	127	131	129
2.2. New firms	0	17	2	82	20	4	0
2.3. Failures	0	0	0	0	0	2	0

This table reports the number of Panaxia clients identified in our records. Panel A reports the number of firms by type and data source, while Panel B reports the number of non-financial firms (excluding the franchisor) and pharmacies over the period 2007–2013.

Table B2: Variable definitions and data sources

Variable names	Definitions (data source)
A. Event variables	
$Exposure$	Claims held on Panaxia at the time of the bankruptcy in 2012 (Bankruptcy trustee and savings banks)
$Loss$	Uncovered claims in 2012 (Bankruptcy trustee and savings banks)
B. Outcome variables	
$Cash$	Total amount of cash and liquid assets (Financial statements)
$Payables$	Accounts payable (Financial statements)
$Receivables$	Accounts receivable (Financial statements)
$Bank\ debt$	Total bank debt (Financial statements)
$Short\text{-}term\ bank\ debt$	Short-term bank debt (Financial statements)
$Long\text{-}term\ bank\ debt$	Long-term bank debt (Financial statements)
$Applications$	Applications for the issuance of injunctions to settlement of outstanding claims (Enforcement Agency)
$Withdrawals$	Applications that were withdrawn by the supplier from the EA (Enforcement Agency)
$Payments\ to\ EA$	Applications that resulted in a payment to EA (Enforcement Agency)
$Contested\ claims$	Applications that were contested by the customer (Enforcement Agency)
$Defaults$	Applications that were unsettled within a fortnight from the time of notification (Enforcement Agency)
C. Control variables	
$Cash\ flow$	Earnings after interest expenses and taxes, but before depreciation and amortization (Financial statements)
$Assets$	Book value of total assets (Financial statements)
$Sales\ growth$	The log difference between sales in periods $t-1$ and t (Financial statements)
$Debt$	Total liabilities excluding payables (Financial statements)
$Tangible\ assets$	Property, plant, and equipment (Financial statements)
$Inventories$	Inventories (Financial statements)
Age	Years since firm was registered as a corporation (Credit bureau)
$COGS$	Cost of goods sold (Financial statements)
$Rating$	Probability of default (PD) estimated by the Swedish credit bureau UC (Credit bureau)

This table reports definitions of all variables used in the empirical analysis.

Table B3: Assessing balance

	A. Non-treated (weighted)			B. Matched control firms		
	(I)	(II)	(III)	(IV)	(V)	(VI)
	Coverage freq.		Log of ratio	Coverage freq.		Log of ratio
	$\pi_{co}^{.95}$	$\pi_{tr}^{.95}$	of SD ($\Gamma_{co,tr}$)	$\pi_{co}^{.95}$	$\pi_{tr}^{.95}$	of SD ($\Gamma_{co,tr}$)
$Cash\ flow/Assets_{2009}$	0.975	0.924	–0.209	0.941	0.957	0.022
$Assets_{2009}$ (in MSEK)	0.918	0.880	–0.179	0.966	0.979	0.093
$Sales\ growth_{2009}$	0.964	0.932	–0.171	0.951	0.954	0.100
$Debt/Assets_{2009}$	0.510	0.629	–0.091	0.489	0.634	0.048
$Tangible\ assets/Assets_{2009}$	0.989	0.796	–0.178	0.967	0.920	–0.029
$Inventories/Assets_{2009}$	0.938	0.577	–0.182	0.931	0.890	–0.012
Age_{2009}	0.872	0.965	0.214	0.882	0.915	0.114
$Cash/Assets_{2009}$	0.997	0.816	–0.279	0.962	0.921	–0.056
$Payables/Assets_{2009}$	0.920	0.761	0.051	0.948	0.941	0.022
$Receivables/Sales_{2009}$	0.618	0.659	–0.572	0.598	0.757	–0.016
$Cash/Assets_{2008}$	0.993	0.834	–0.286	0.964	0.926	–0.062
$Payables/Assets_{2008}$	0.856	0.785	0.198	0.936	0.957	0.040
$Receivables/Sales_{2008}$	0.603	0.685	–0.414	0.584	0.734	0.031
Number of firms	610/49,633/49,633			610/610/482		

This table reports three measures of balance proposed by Imbens and Rubin (2015): two coverage frequencies and the logarithm of the ratio of standard deviations. Panels A and B compare outcomes for treated firms with those for non-treated firms and matched control firms, respectively. Means and standard deviations for non-treated firms are calculated using weights corresponding to the fraction of treated firms in each particular 5-digit industry. Variable definitions are provided in Table B2. The numbers of firms reported in the bottom line refer to treated firms, matched control firms, and unique matched control firms, respectively.

Table B4: Bank financing

	Treatment period			Post-treatment period	Test of parallel pre-trends
	(I)	(II)	(III)	(IV)	(V)
	2010	2011	2012	2013	p-val.
A. $y = Total\ bank\ debt/Assets$					
(1) τ_t	0.000	–0.007*	0.007	–0.012*	0.410
	(0.1)	(–1.8)	(1.5)	(–1.7)	
(2) T_t	0.000	–0.007	0.000	–0.011	
	(0.1)	(–1.1)	(0.1)	(–1.2)	
B. $y = Short\text{-}term\ bank\ debt/Assets$					
(3) τ_t	0.002	–0.003	0.004**	–0.001	0.590
	(0.9)	(–1.5)	(2.3)	(–0.7)	
(4) T_t	0.002	–0.002	0.003	0.001	
	(0.9)	(–0.5)	(0.9)	(0.5)	
C. $y = Long\text{-}term\ bank\ debt/Assets$					
(5) τ_t	–0.001	–0.003	0.002	–0.012	0.215
	(–0.2)	(–0.8)	(0.5)	(–1.6)	
(6) T_t	–0.001	–0.004	–0.002	–0.013	
	(–0.2)	(–0.7)	(–0.3)	(–1.4)	
Number of firms		610/610/482			

This table reports estimates of yearly adjustments, Eq. (1), and cumulative adjustments, Eq. (2), in total bank debt, short-term bank debt, and long-term bank debt, over the treatment and post-treatment periods. The tests for parallel pre-trends are conducted on the 2007–2009 period, and follow the approach proposed by Mora and Reggio (2015). Variable definitions are provided in Table B2. The numbers of firms reported in the bottom line refer to treated firms, matched control firms, and unique matched control firms, respectively. t-values, reported in parenthesis, are calculated using robust standard errors adjusted for clusters in two dimensions: firstly, at the firm-level for non-franchisees and at the franchisor-level for franchisees; and secondly, at the level of matched pairs. ***, **,* denote statistically distinct from 0 at the 1, 5, and 10 percent levels, respectively.

Table B5: No losses vs. incurred losses

	A. No losses in 2012					B. Incurred bankruptcy losses in 2012				
	Treatment period			Post-treatement period	Test of parallel pre-trends	Treatment period			Post-treatement period	Test of parallel pre-trends
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)	(X)
	2010	2011	2012	2013	p-val.	2010	2011	2012	2013	p-val.
A. $y = Cash/Assets$										
(1) τ_t	–0.009	–0.032**	0.025**	–0.014	0.630	–0.023**	–0.006	0.003	–0.008	0.805
	(–0.7)	(–2.6)	(2.0)	(–0.9)		(–2.3)	(–1.0)	(0.5)	(–0.5)	
(2) T_t	–0.009	–0.040**	–0.015	–0.029		–0.023**	–0.029***	–0.026***	–0.033***	
	(–0.7)	(–2.3)	(–0.8)	(–1.3)		(–2.3)	(–3.1)	(–2.9)	(–2.6)	
B. $y = Payables/Assets$										
(3) τ_t	0.011	0.010	0.000	0.002	0.657	–0.004	0.012**	0.022**	–0.001	0.448
	(1.4)	(1.2)	(–0.0)	(0.2)		(–0.5)	(2.2)	(2.0)	(–0.0)	
(4) T_t	0.011	0.021**	0.021	0.023		–0.004	0.008	0.029***	0.029**	
	(1.4)	(2.1)	(1.5)	(1.4)		(–0.5)	(0.8)	(2.9)	(2.2)	
C. $y = Receivables/Sales$										
(5) τ_t	–0.001	0.003	–0.010**	–0.002	0.503	–0.003**	–0.003	–0.005*	0.001	0.328
	(–0.4)	(1.5)	(–2.4)	(–0.4)		(–2.1)	(–1.5)	(–1.8)	(0.2)	
(6) T_t	–0.001	0.002	–0.008	–0.01		–0.003**	–0.006***	–0.010***	–0.010***	
	(–0.4)	(0.7)	(–1.6)	(–1.3)		(–2.1)	(–2.8)	(–3.0)	(–2.9)	
Number of firms			116/116/116					494/494/367		

This table reports estimates of yearly adjustments, Eq. (1), and cumulative adjustments, Eq. (2), in cash holdings, accounts payable, and accounts receivable, over the treatment and post-treatment periods. Columns (I) to (V) report results for the sub-sample of treated firms that were fully compensated for bankruptcy losses in 2012, and Columns (VI) to (X) for the sub-sample of treated firms that incurred losses in 2012. The tests for parallel pre-trends are conducted on the 2007–2009 period, and follow the approach proposed by Mora and Reggio (2015). Variable definitions are provided in Table B2. t-values, reported in parenthesis, are calculated using robust standard errors adjusted for clusters in two dimensions: firstly, at the firm-level for non-franchisees and at the franchisor-level for franchisees; and secondly, at the level of matched pairs. ***, **,* denote statistically distinct from 0 at the 1, 5, and 10 percent levels, respectively.

Table B6: Treatment effects conditional on loss-size—Alternative specification

	Outcome variable					
	$Cash/$ $Assets$	$Payables/$ $Assets$	$Receivables/$ $Sales$	$Cash/$ $Assets$	$Payables/$ $Assets$	$Receivables/$ $Sales$
	(I)	(II)	(III)	(IV)	(V)	(VI)
$Event_t \times Loss/Assets_{2012}$	–0.150	0.346**	–0.132***	–0.658**	1.233***	–0.173**
	(–1.2)	(2.4)	(–3.2)	(–2.5)	(4.2)	(–2.2)
$Event_t \times Loss/Assets^2_{2012}$				3.466**	–6.053***	0.279
				(2.4)	(–3.2)	(0.6)
Marginal effect at the mean	—	—	—	–0.360**	0.712***	–0.150***
				(–2.2)	(4.3)	(–3.2)
Number of observations	610/610/482					

This table reports results from estimations of Eq. (3) augmented with matched pair×time-fixed effects. Variable definitions are provided in Table B2. The numbers of firms reported in the bottom line of Panel A refer to treated firms, matched control firms, and unique matched control firms, respectively. t-values, reported in parenthesis, are calculated using robust standard errors adjusted for clusters in two dimensions: firstly, at the firm-level for non-franchisees and at the franchisor-level for franchisees; and secondly, at the level of matched pairs. ***, **,* denote statistically distinct from 0 at the 1, 5, and 10 percent levels, respectively.

Table B7: Treatment effects conditional on credit constraints—Alternative sample split

	A. Firm size						B. Rating					
	Constrained		Unconstrained		t-test		Constrained		Unconstrained		t-test	
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)	(X)	(XI)	(XII)
	T^y_{2012}	t-val.	T^y_{2012}	t-val.	H_0	p-val.	T^y_{2012}	t-val.	T^y_{2012}	t-val.	H_0	p-val.
Baseline specification												
(1) $y = Cash/Assets$	–0.052***	(–3.8)	–0.001	(–0.0)	U < C	0.002	–0.035**	(–2.2)	–0.017	(–0.9)	U < C	0.236
(2) $y = Payables/Assets$	0.038*	(1.9)	0.016	(1.6)	C < U	0.159	0.041*	(1.7)	0.011	(0.9)	C < U	0.152
(3) $y = Receivables/Sales$	–0.012*	(–1.8)	–0.008**	(–2.5)	U < C	0.277	–0.022**	(–2.1)	–0.003	(–1.4)	U < C	0.036
(4) $y = Short\text{-}term\ bank\ debt/Assets$	–0.004	(–1.1)	0.007	(1.0)	U < C	0.078	–0.004	(–0.6)	0.011**	(2.3)	U < C	0.045
Baseline specification with bias-adjustment												
(5) $y = Cash/Assets$	–0.060***	(–4.4)	–0.011	(–0.9)	U < C	0.004	–0.047***	(–2.9)	–0.016	(–0.9)	U < C	0.115
(6) $y = Payables/Assets$	0.039**	(2.1)	0.023**	(2.3)	C < U	0.216	0.059**	(2.4)	0.012	(1.1)	C < U	0.054
(7) $y = Receivables/Sales$	–0.018***	(–2.7)	–0.008***	(–2.7)	U < C	0.085	–0.024**	(–2.4)	–0.003	(–1.3)	U < C	0.018
(8) $y = Short\text{-}term\ bank\ debt/Assets$	0.001	(0.4)	0.010	(1.5)	U < C	0.124	0.000	(0.1)	0.012**	(2.4)	U < C	0.107
Number of firms	149/149/103		148/148/140				149/149/117		148/148/137			

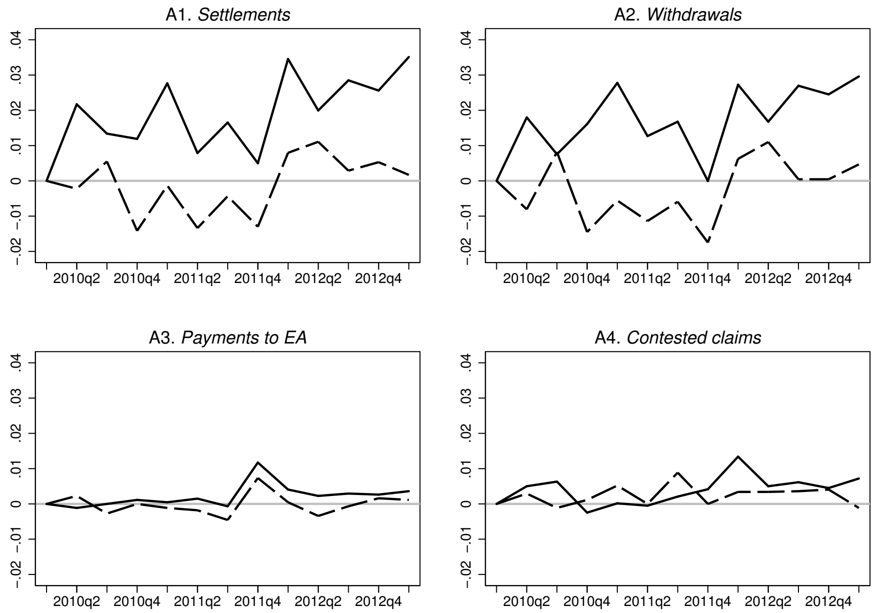
This table reports estimates of cumulative adjustments, Eq. (2), for cash holdings, accounts payable, accounts receivable, and short-term bank debt, in 2012. Rows (1) to (4) report estimates for the baseline specification. Rows (5) and (8) report estimates for the baseline specification with bias-adjustment. The models are estimated on sub-samples classified with respect to treated firms' total assets (Panel A) and credit ratings (Panel B) of the treated firm in each matched pair. For each classification variable, firms in the bottom 3 deciles are classified as constrained and firms in the top 3 as unconstrained. p-values refer to one-sided tests for differences in coefficients between the sub-samples. Variable definitions are provided in Table B2. The numbers of firms reported in the bottom line refer to treated firms, matched control firms, and unique matched control firms, respectively. t-values, reported in parenthesis, are calculated using robust standard errors adjusted for clusters in two dimensions: firstly, at the firm-level for non-franchisees and at the franchisor-level for franchisees; and secondly, at the level of matched pairs. ***, **,* denote statistically distinct from 0 at the 1, 5, and 10 percent levels, respectively.

Figure B1: Panaxia—Number of daily collections per month, 2006–2011

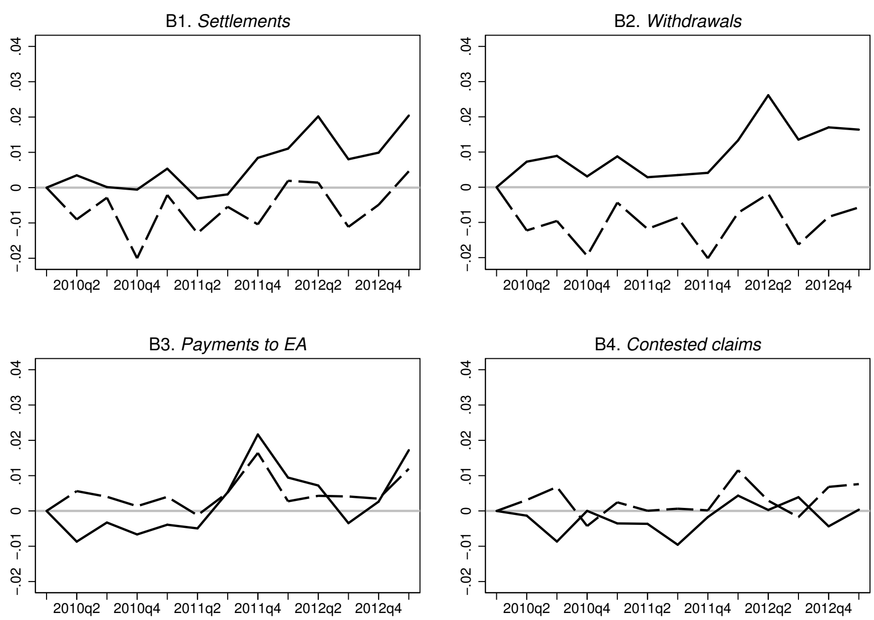
This figure is a modified version of a figure appearing in the report covering Panaxia's bankruptcy estate. It shows the number of daily collections in each month during the period 2006–2011.

Figure B2: Settlements and its components

A. Upstream perspective



B. Downstream perspective



This figure shows $Settlements$ and its three components: $Withdrawals$; $Payments\ to\ EA$; and $Contested\ claims$. Panel A shows outcomes for settlements related to enforcements faced by the treated firms (solid line) and matched control firms (dashed line). Panel B shows outcomes of settlements for enforcements imposed by the treated firms (solid line) and matched control firms (dashed line). Variable definitions are provided in Table B2.