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

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Abstract

Using data on exogenous liquidity shortfalls generated by the fraud and failure of a cash-in-transit firm, we demonstrate a causal effect 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 of a similar size 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 credit durations.

Keywords: Liquidity management; Trade credit; Cash holdings; Cash flow; Risk sharing.

JEL: D22; G30

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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 to this end, by reversing the measures that apply upstream; either by reducing the 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 a clean—or exogenous, if you will—measure of shocks to corporate liquidity, we evaluate 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 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 exogenous event that is close in nature to the concept of an economic shock. The Panaxia sequence of events provides an

[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.

opportunity to form causal 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. To further strengthen our empirical identification, we exploit that some of Panaxia's clients were only exposed to the initial fraud, and did not incur losses at the bankruptcy-stage, and hence were less exposed to liquidity shortfalls. We 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 such risk sharing, idiosyncratic shocks would be pooled away, internally, 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 to and hence more limited risk sharing; obstacles 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. Which of these various forces we can plausibly identify 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 the 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 subsequent outcomes of such applications.

The nature and scope of the Panaxia sequence of events make Abadie and Imbens' (2006) nearest-neighbor matching estimator a suitable empirical setup for causal 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. The overall evaluation period spans 2007–2013 and is divided into a pre-treatment period in 2007–2009; the treatment period in 2010–2012; and the post-treatment year of 2013.

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 outstanding credit—is of a similar magnitude as the adjustment in cash holdings, suggesting that trade credit margins make for important sources of reserve liquidity, on par with cash reserves.

The complexity of the Panaxia event gives rise to differential treatments, which can be exploited for identification. A majority of the treated firms were exposed to both the liquidity shortfalls caused by the fraud and the bankruptcy losses, whereas a subset of the treated firms were exposed to the fraud only. By using this variation in treatment, and also the variation in loss-size across the firms in the sub-group receiving full treatment, 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 payments that require liquid means, 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

[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

adjustments are used as complements to manage liquidity.

Finally, our investigation of the mechanisms 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 shifts in trade credit durations. More specifically, the propensity to postpone settlement of trade credit payments increases significantly for firms that are hit by liquidity shortfalls, as reflected by these firms being subject to more applications of injunction submitted by their suppliers. In the downstream perspective, however, we are unable to document significant increases in firms' propensity to enforce overdue payments from customers to offset liquidity shortfalls; possibly reflecting that downward adjustments in accounts receivable are achieved through reduced contracted maturity in issuance of new trade credit, rather than an increased enforcement of repayment of overdue claims.

The applications of injunction 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

liquidity—cash, or bank financing—to cover expenditures to counterparties that are unwilling to extend credit, such as employees and tax authorities.

[4] Along the lines of the current paper, the existing literature highlights the financing role of trade credit, where credit is redistributed along trade credit chains from unconstrained firms to constrained counterparties in the economy, see Petersen and

are correlated with increases in their accounts payable. Bakke and Whited (2012) explore 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 causal insights on liquidity shocks' impacts on firms' cash holdings, accounts payable and 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 event allows 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. 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 can potentially contribute towards a deeper understanding of how firms manage idiosyncratic 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, Salas, and Townsend (2010), and Samphantharak (2009). Gopalan et al. (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,

Rajan (1997) for a seminal contribution. In addition to the financing motive, a strand of the literature emphasize 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 buyers time for verification of purchase quality. Moreover, see Gianetti, Burkart, and Ellingsen (2011) for a recent, comprehensive overview of existing trade credit theories.

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

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 event, 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 Event, Data, and Empirical Approach

The Panaxia event was an extreme outcome of criminal offenses that caused substantial hardship for the clients involved; however, it 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.[6] 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

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

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 depot, 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.[7] 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.]

It is a fair question to ask whether the clients did not understand what was going on, or react to the drastically increased transfer periods. They did react, but very few actually ended their contracts

[7] 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."

with Panaxia.[8] The bankruptcy trustee describes a setup where Panaxia's management instructed the customer-support staff to inform complaining clients that transfer holdups were simply due to technical problems. Along these lines, Figure A1 in the appendix shows the number of collected receipts at a monthly frequency for the period 2006–2011. The figure shows the expansion phase with a sharp increase in the number of collected receipts from January 2006 to July 2008, and a stable pattern hovering around 120,000 collected receipts in the period from July 2008 to December 2013. Hence, this figure indicates that the number of clients was stable from mid-2008 and going forward. Moreover, the general credibility of Panaxia can be appreciated by considering the fact that Sveriges Riksbank (the central bank of Sweden), even 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.[9]

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, e.g., 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

[8] 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 event period. Their statements are confirmed in Panaxia's annual financial reports for the period 2007 – 2010, which provide examples of important customers 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 customers in Panaxia's bankruptcy 2012.

[9] The interim CEO 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 seem plausible underpinnings of the lengthy Panaxia fraud.

[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 a decline in the inflow of funds. Now, Swedish accounting rules give firms discretion in the choice between booking cash-in-transfer directly under the cash account, 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.1 provides a detailed outline of the accounting practises and how their usages affect the interpretation of estimated effects on cash holdings.

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 several years of imprisonment for fraud, embezzlement, and fraudulent accounting practice.

2.2 Data

In this subsection, we first outline how the Panaxia data is 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 client firms operated in multiple locations, e.g., a retail firm running several stores. The second source is due to the four savings banks that covered the losses endured by their clients in the Panaxia bankruptcy. These banks provided the identities of their clients that were affected by the bankruptcy, as well as the sizes of the losses that were covered by them (*Item 3*). A third source is the business register *Retriever*, which contains the 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 of injunctions for settlement of 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 with very large claims on the Panaxia bankruptcy estate. It turns out that these entries correspond 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 related to the franchise group problem discussed above. Two entries on the name list, *Item 1*, correspond to parents in business groups, whereas their subsidiaries are included in the list of collection sites, *Item 2*. Hence, the appropriate entries for the final data set are the two parent firms, and to associate these parents with the consolidated financial statements pertaining to respective business group.

In total, our records cover 1,255 clients that held outstanding claims on Panaxia at the time when it

[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 customers—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 customers' 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 clients 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 clients of the four savings banks, since these firms were fully compensated.

[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.

failed; arising from collections of daily proceeds that were never transferred to the clients bank accounts, from more than 5,000 collection sites (see Table A2 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 amounted 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 averaged losses-to-assets amounted to 4.3 percent.[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 corporations in the U.S., or limited liability businesses in the U.K.. Swedish law requires every aktiebolag to hold in equity a minimum of SEK 100,000 (approximately USD 15,000) 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 the European Union standards. In Sweden, as in many other countries, firms have considerable discretion in choosing a time period for which their financial statements will apply. A substantial fraction of the firm-year observations in our sample concern fiscal periods starting in the middle of a calendar year.[16] We deal with this by interpolat-

[13] Panel A in Table A2 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.

[14] The Panaxia bankruptcy had dire consequences for its customers. 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 data set, or close versions of it, has been used extensively in previous research, cf. Jacobson, Lindé, and Roszbach (2013), Giordani, Jacobson, Villani, and von Schedvin (2014) and Jacobson and von Schedvin (2015).

[16] The shares of shorter (less than 12 months) and longer (more than 12 months) statements are both around 5 percent. Whereas shorter than the stipulated minimum of 6 months happen, statements covering a longer period than the allowed maximum of 18 months are very rare. Over time, the annual shares of shorter/longer statement periods have come down from about 8 percent to currently around 4 percent. Thus, an overwhelming majority of statements concern a period of 12 months. However, out of the 90 percent of the total number of statements, only 48 percentage points coincide with a calendar

ing the financial statements so that their fiscal periods align with calendar years.[17] Firms with total assets and real sales below SEK 100,000 (deflating by means of consumer prices, using year 2010 prices as a basis) are omitted. To avoid detrimental effects from outlier observations, all firm-specific variables are winsorized with respect to the 1^{st} and the 99^{th} 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 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 by 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

year, and hence 42 percentage points refer to other 12 month periods. In these calculations we have allowed for a given calendar year to begin in mid-December the previous year, and end in mid-January the following year.

[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 than 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.

[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 ATT for payables over cost of goods sold.

and as is evident from the presentation of our data above, the Panaxia event involved a relatively small number of firms. This suggests a matching estimator 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 estimator 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 can potentially be matched to multiple treated firms.

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-assets. 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 impacts of postponed payments, 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:

$$\tau_t^y = (\bar{y}_t^{Treated} - \bar{y}_{t-1}^{Treated}) - (\bar{y}_t^{Control} - \bar{y}_{t-1}^{Control}),\ t = 2010, ..., 2013, \tag{1}$$

where $\bar{y}_{i,t}^{Treated}$ is the mean of an outcome variable for the treated firms in year t and $\bar{y}_{i,t}^{Control}$ 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:

$$T_t^y = (\bar{y}_t^{Treated} - \bar{y}_{2009}^{Treated}) - (\bar{y}_t^{Control} - \bar{y}_{2009}^{Control}),\ t = 2010, ..., 2013. \tag{2}$$

These estimators of yearly and cumulative adjustments offer causal insights on how the liquidity shortfalls affect firms' cash and trade credit positions. The standard errors are clustered at the firm-level for

non-franchisees and at the franchisor-level for franchisees. This clustering approach accounts for the multiplicity of control firms, as well as for a possible dependence among franchisees.

Our approach to causal 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 set-up, 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 was 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 present *t*-statistics for differences in means and an assessment of the balance in covariate distributions across treated and control firms.

The complexity of the Panaxia event gives rise to differential treatments of firms, which we can exploit for identification; and which provides further indication of the plausibility of unconfoundedness. 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 2012-bankruptcy 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 adjustments in the outcome variables when firms are exposed to more liquidity distress. In this vein, we also evaluate effects conditional on variation in loss-size across the firms in the sub-group receiving full treatment.

We proceed and 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 trade credit margins. To this end, we follow Farre-Mensa and Ljunqvist (2016) and use firm size and credit ratings as measures of financial constraints. Farre-Mensa and Ljunqvist 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 corresponding to firms that are classified to be financially constrained and unconstrained.

We then estimate and compare coefficients across the two samples, to assess the role played by credit constraints.

Finally, we 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 an injunction 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, which is what would be expected if firms attempt to draw extra liquidity from suppliers and customers.

3 Baseline results on the treatment effects of liquidity shortfalls

This section presents applications of the Abadie and Imbens (2006) nearest-neighbor matching estimator to identify 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 5-digit industry. To assess magnitudes for differences in matching variables, between the treated firms on the one hand and the non-treated firms, and the matched control firms on the other hand, we calculate and report *t*-statistics for tests of differences in means in Panels B and C. When comparing means for treated and non-treated in

Panels A and B, the *t*-tests indicate that treated firms, as compared with non-treated ones, exhibit higher sales growth, hold less cash, and obtain more trade credit from suppliers, whereas differences in means for the remaining variables are not statistically separate from zero.[19] Hence, the descriptive statistics indicate some, but not huge, differences in covariates between the treated firm sample and our industry-weighted representation of non-treated firms. However, the presence of some deviation points towards a need to undertake matching to obtain credible counterfactuals; but modest deviations overall between treated and non-treated firms suggest that results obtained for the treated Panaxia firms are general and may apply more widely.

[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 *t*-tests for differences in means, there are no statistically significant 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 tests for differences in means.

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

[19] Table A4 in the Appendix reports on four measures proposed by Imbens and Rubin (2015), to assess the balance in covariate distributions: normalized differences for location; two coverage frequencies; and the logarithm of the ratio of standard deviations. Imbens and Rubin 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, they observe a maximum absolute normalized difference of 0.30 standard deviations, which contributes to an overall assessment of excellent covariate balance. In our case, Column (I) in Table A3 shows that only three variables—the debt level, cash holdings, and accounts payable—yield normalized differences in absolute values of 0.30, or larger; remaining variables display modest deviations between the groups. Moreover, the reported coverages frequencies in Columns (II) and (III) 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 identify counterfactual firms. Finally, Column (IV) shows that the differences in dispersions between the distributions are modest for all variables except for size, where the size dispersion is larger for the treated group as compared with the non-treated group.

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.]

3.2 Baseline results

We now proceed with a presentation of our baseline results. Table 3 reports estimates of the yearly and cumulative adjustments according to Equations (1) and (2), for our three key outcome variables. Panel A shows results for cash holdings, $Cash/Assets$. The estimates of the yearly adjustments effects, τ_t, in Columns (I) to (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.[20] 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, and thus, in support of unconfoundedness.[21]

[Insert Table 3 about here.]

Results for accounts payable, $Payables/Assets$, are reported in Panel B. The estimates of the yearly adjustments effects, τ_t, reported in Columns (I) to (IV) show an increase in 2011 of 1.1 percentage point and an enhanced 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 0.1 percentage points in the first year of the treatment period and an enhanced contraction of 0.6 percentage points in 2012. Accordingly, the estimates of the

[20] 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. More specifically, the convention to book cash-in-transfer under the cash account leads to an underestimation of treated firms' reliance on cash to balance the initial tranfer delays. See Appendix A.1 for a detailed discussion.

[21] 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 $E[y_{it}] = \delta + \sum_{t=2008}^{2013} \delta_t I_t + \gamma D_i + \sum_{t=2008}^{2013} \gamma_t I_t D_i$, where I_t is a time t year dummy and D is a treatment dummy. The test statistic of pre-treatment trends is a Wald test of the joint significance of γ_{2008} and γ_{2009}.

cumulative effects, T_t, show that the 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; and 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 0.085. 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 (3.7). Testing for the difference in absolute value adjustment between cash and net trade credit yields a *p*-value of 0.234, indicating that the adjustments at the two trade credit margins are jointly of a similar magnitude as the adjustments in cash holdings.[22]

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 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

[22] 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 0.035 and 0.090. Thus, the compounded adjustments and the liquidity losses are not significantly different in magnitude.

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 final 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 lending is therefore next evaluated by use of three balance sheet items: total bank debt, and short- and long-term bank debt separately. Table A5, Panels A–C, accordingly present yearly and cumulative treatment effects on these debt-measures and no systematic adjustments are recorded over the event period, indicating that firms do not turn to their banks first-hand to deal with liquidity shortfalls. We propose two potential explanations for this. 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 rely on 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 more in detail below, when we explore sources of cross-sectional heterogeneity.

To further validate our baseline results, we apply a set of robustness tests reported in Table A6. 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 5-digit industry as weights. Row (2) in Table A6 reports results where adjustments for treated firms are related to adjustments for all non-treated firms. Columns (I) to (VIII), 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); except for bank financing, which becomes positive and statistically significant. However, tests for parallel pre-treatment trends indicate deviations in cash holdings between treated and non-treated firms, emphasizing the importance of a matching approach to support a causal interpretation.

Secondly, one potential concern is that differences in characteristics influence our results. To address this concern, Row (3) reports results from bias-corrected matching estimators, where differences in the matching variable outcomes between treated and control firms are accounted for in the estimation, see Abadie and Imbens (2011). The bias-corrected effects are very similar to the baseline estimates, suggest-

ing that the latter are not confounded by differences in characteristics between the treated and control firms. As a complement to the bias-corrected estimates, we follow Hotz, Imbens, and Mitnik (2008) and restrict the estimation sample to matched pairs where differences in matching variables are small. To this end, we 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.

Thirdly, following Petersen and Rajan (1997) and Cuñat (2007), accounts payable are scaled by firms' total assets in the estimation underlying our baseline results. However, and arguably, scaling by cost of goods sold may closer reflect firms' levels of economic activity and is therefore considered next. In the case of Swedish corporate firms, only a smaller fraction reports cost of sold goods 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. In Row (5), we note a positive and significant cumulative treatment effect for payables scaled by cost of goods sold, which is consistent with our baseline results. The estimated effects for the other outcome variables show, in turn, an insignificant effect for cash holdings, but a positive adjustment in short-term bank financing, whereas the effect for receivables is inconclusive due to differences in pre-treatment trends. These results suggests that firms propensity to use accounting standards that reveal their cost of sold goods 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 matter, by instead considering a truncation at the 1st and 99th percentiles. Row (6) shows that obtained estimates on the truncated data are very similar to the baseline 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, whereas the effect on short-term bank financing is positive and statistically significant. Thus, the reliance on trade credit margins to manage the liquidity shortfalls is a common feature for the non-franchise and franchise firms alike, whereas the dependence on cash and bank financing differ across the two groups.

Sixthly, Row (8) reports results where pharmacies are included in the estimation sample. The reason why inclusion of pharmacies only lead to seven more treated firms is that most pharmacies were startups

in 2010 and 2011, cf. Table A2, 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 restriction that observations on the outcome variables must be available for both treated and control firms during the entire 2010–2013 period. Dropping this restriction increases the number of treated firms from 610 to 649. One noticeable difference is that the estimated treatment effects on payables are substantially enhanced. A potential explanation for the stronger results is that the treated firms eliminated from the balanced panel are the more distressed. Hence, these results indicate that our baseline estimates of payables-adjustments are, if anything, conservative.

Finally, Rows (10) and (11) concern aspects of the timing of the Panaxia event. For a large fraction of the firms—76 percent of the treated firms—the fiscal period ends in another month 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. One potential concern in using this approach 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 delivery times took place in the last quarter, cf. Figure 1. To investigate the significance of these circumstances, we estimate T^{y}_{2010} on two subsamples concerning treated firms with a fiscal year-end strictly in December (Row (10)), and treated firms with a fiscal year-end occurring mid-year (Row (11)). The estimates show that the adjustments in cash holdings and receivables are statistically significant for firms with a year-end in December, but no significant effects are obtained for the other group. Thus, these results render further support to the notion that our estimates capture the liquidity shortfalls imposed by the Panaxia sequence of events.

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 conceivable conjecture will be examined next and we will consider two cases: firstly, firms that incurred losses versus no losses; and secondly, incurring 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 bank in 2012; and firms that incurred losses in 2012. Thus, the firms in the two groups experienced the same fraud treatment in 2010 and 2011—delayed transfers—but a differential treatment in the bankruptcy year 2012.

Table 4 reports results for the two groups; Columns (I) to (V) cover the group of treated firms that were fully compensated in 2012 and Columns (VI) to (X) cover the group of treated firms that incurred losses in 2012. The results for the former show a downward shift in the firms' cash holdings in 2011 and a subsequent reversal in 2012, so that the estimated cumulative cash effect that year is not significantly different from zero. 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. The results for accounts receivable show that a temporary downward adjustment occurred in 2012; but the estimates of the cumulative effects are insignificant throughout the treatment and post-treatment periods, indicating that these firms did not make any significant adjustments in the amount of issued trade credit as compared with the positions held in 2009. Shifting now to the results for firms that did incur losses when Panaxia failed in 2012, we find that adjustments in cash holdings persist in 2012, and in the post-treatment year, whereas additional adjustments are observed at the two trade credit margins in 2012. Thus, the results in Table 4 suggest that the postponement fraud underlies the observed adjustments in the first two years of the treatment period and applies to both groups of firms, whereas the presence of failure losses caused the adjustments to persist, or even increase, in the last treatment year and in the post-treatment year.[23,24]

[Insert Table 4 about here.]

[23] 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 are not savings bank clients, mitigates a concern that our baseline results are driven by a credit contraction imposed by Sparbanken 1826.

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

Our analysis can take one step further using the sample of uncompensated firms that made losses in the bankruptcy, by evaluating whether the magnitudes of treatment effects depend on the size of the incurred losses, i.e., the second case mentioned above. Our conjecture is that larger losses should yield larger adjustments. Since the sample of uncompensated firms is small, we adopt an empirical approach following Bakke and Whited (2012), where they use an increasing window-width around a funding threshold to evaluate the sensitivity of their effect estimates. In our case the idea is to estimate treatment effects on successively increasing portions of the sample of uncompensated firms, where the incidence of loss-size is enhanced with portion-size. Thus, we begin by sorting treated firms according to losses in 2012 and estimate cumulative treatment effects for adjustments between 2009 and 2012 on the portion comprising the treated firms incurring the 20 percent smallest losses. We then, step-by-step, add additional firms to a portion by increasing the cut-off by 20 percentiles in the loss-size distribution, to end up with five portions such that the last one coincides with the full sample of uncompensated firms. Hence, across portions, losses increase successively, and associated treatment effects will provide insights as to the nature of their relationships.

Figure 3 reports treatment effects—with 95 percent confidence intervals—for the three outcome variables across the five portions. Results for cash holdings, reported in Panel A, show insignificant effects for the two portions based on the 20 and 40 percentile cut-offs. Increasing the cut-offs further are associated with enhanced and statistically significant treatment effects on cash. Panel B show similar results for accounts payable. The estimated treatment effects increase as the portions include more firms with larger losses and become statistically significant for the 80 percentile cut-off and for the full sample portion. Effects on accounts receivable, reported in Panel C, show a slightly different relationship. These effects become statistically significant from the 40 percentile cut-off and the magnitude of estimates are fairly stable across portions. One potential explanation for this is that adjustments in issued trade credit—due to, e.g., efforts to enforce payment of overdue claims on customers—in response to the fraud were maintained over time also for the treated firms that only incurred small losses at the bankruptcy stage. The overall picture emerging from this exercise is that the compounded responses at all margins are indeed related to the size of incurred losses.[25]

[Insert Figure 3 about here.]

[25] 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. Table A7 reports results for these firms. The cumulative effects on accounts payable and receivable indicate increases in the amount of received trade credit and contradictions 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.

In sum, these results shed additional light on the credibility of the unconfoundedness assumption. 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, affirm our claim that overall we are capturing adjustments in the outcome variables that are due to increased liquidity needs. Hence, the documented intensity effects offer further assurance of the validity of the underlying identification assumption.

3.4 The roles 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 how they manage liquidity, and 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.[26] 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 are classified 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. 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 other studies that consider public firms. The reported estimates concern cumulative treatment effects in 2012, intended to capture the full impact of the Panaxia sequence of events.

Panel A in Table 5 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 ones as unconstrained. The first result emerging in Row (1) 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 the two samples. Results for accounts payable in Row (2) are similar, constrained firms increase their payables, whereas no adjustments are observed for unconstrained ones. However, the estimated effects are not significantly different across the two samples. For accounts receivable in Row (3), we note significant treatment effects for both groups, although the point estimates are larger for the constrained firms as compared with unconstrained ones. Again, we find that the effects are not statistically different. Row (4)

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

reports the compounded effect at both trade credit margins. The point estimate is larger for constrained firms, but the difference is not statistically significant. Finally, the last outcome variable considered is short-term financing, reported in Row (5). The coefficient is negative and insignificant for constrained firms and positive and insignificant for unconstrained ones. However, comparing the two coefficients indicate that they are different at a 10-percent level, indicating a relative increase in bank financing for unconstrained firms, as compared to constrained ones, which is consistent with the presumption that the applied size-split to some degree captures access to external financing.

[Insert Table 5 about here.]

Panel B shows results for sample-splits based on firms' credit ratings, where firms associated with medium to high risk are classified as constrained, and low-risk firms are classified as unconstrained. The effects align with the ones reported for the size-split, but display a more pronounced difference between the two sub-samples. For cash holdings, reported in Row (1), 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) and (3) show that constrained firms increase the amount of drawn trade credit and contract the amount of issued trade credit, whereas the coefficients for the 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 the constrained firms, which also is reflected by the compounded effects reported in Row (4). Finally, estimates in Row (5) show that unconstrained firms tend to draw significantly more short-term bank financing, as compared with the constrained firms.

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 credit from suppliers and customers so as to sustain sufficient cash reserves for the purpose of executing direct 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 cause the observed adjustments in treated firms' trade credit positions. In this section we will probe our key presumption for the nature

of firms' behaviour underlying these adjustments: namely, that reserve liquidity is extracted through shifts in trade credit durations, both upstream and downstream. Upstream 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 durations, as will active attempts to enforce payment on due credit extended to customers. For lack of data on trade credit contracts, we resort to study temporary defaults and enforcements of payment related to trade credit.

4.1 Measurement of mechanisms

Whereas postponement of payments to suppliers and enforcement of customers' trade credit payment 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 event is straight forward. 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 can 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 as *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 as *Defaults*. Secondly, we also observe applications that led to settlement immediately after the firms received notification from the EA, and denote these outcomes as *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 enforced payments from downstream customers. Secondly, we also observe a set of various outcomes underlying *Settlements*. That is, *Settlements* are 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 as *Withdrawals*; the customer can also settle the claim by paying the EA, denoted as *Payments to EA*; and the customer can contest the claim, which happens if there is a disagreement between the two parties, denoted as *Contested claims*.

We structure the outcome variables—*Defaults*, *Settlements*, and *Withdrawals*, *Payments to EA*, *Contested claims*, respectively—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 incidents occurred, or not; and their intensive margins by measuring the number of specific incidents 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:

$$y_{i,t} = \beta_0 + \beta_1 \times Event_t + \beta_2 \times Treated_i + \beta_3 \times Event_t \times Treated_i + \varepsilon_{i,t}, \tag{3}$$

where y corresponds to one of the dependent variables described above; $Event$ 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 2010Q2–201204 period, and zero otherwise, when the model is estimated on the shorter sample; and $Treated$ is a variable that takes the value one for the treated firms and zero for the matched control firms. Thus, the β_3-coefficient 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. The motive for using this approach rather than estimating Equations (2) and (3), is that the dependent variables derived from the EA data represent flows rather than stocks, which intuitively makes cumulative effects less tractable.

Figure 4 offers a graphical illustration of how the average incidents 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 (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 firm 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 that treated firms seek to reduce actual payment periods by more actively managing *Late payments*; or a combination of the two.

[Insert Figure 4 about here.]

4.2 Mechanism results

Table 6 reports results for Eq. (3), where estimates from a linear probability model (LPM) is provided in Column (I) and estimates from a Tobit model, accounting for the zero lower bound in the number of outcomes, is presented in Column (II). Panels A and B report results for the postponement of payments to suppliers and the enforcement of late payments from customers, respectively.

[Insert Table 6 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 effect, we can relate it to the pre-treatment period frequency in *Late payments* of 4.7 percent, indicating a considerable increase 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 result 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 that the overdue claims did not results in outright defaults.

Rows (1) to (3) in Column (II) 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.[27]

Next, Rows (4) to (6) in Columns (I) and (II) 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 causal interpretations are unwarranted since we lack data for the pre-treatment period and cannot undertake tests for parallel trends.[28] Nevertheless, the coefficients reported in Rows (4) to (6) serve a purpose in shedding additional light on the underlying drivers of the causal effects documented in Rows (1) to (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*.[29] The background for a *Withdrawal* of an injunction is that either the customer makes a direct payment for the overdue debt to the supplier, or that the two 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 the evaluation of mechanisms underlying downstream 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 causal interpretations are unwarranted. Rows (7) to (8) show that the estimated effects for *Late payments*, and its two sub-components *Defaults* and *Settlements*, are positive, but 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 sup-

[27] The test for parallel trends in the pre-treatment period demonstrates a significant difference in growth rate between treated and control firms for the number of *Defaults*, cf. Row (2) in Column (II), which prevents a causal interpretation of the estimated treatment effect. The erratic behaviour displayed by the number of *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).

[28] If we apply the shorter 2010Q1-2012Q4 period with 2010Q1 as the pre-treatment period for Settlements, we obtain estimates (*t*-values) of 0.018 (1.5) and 0.285 (1.3) 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 the *t*-values drop substantially in magnitude.

[29] Figure A2 provides further support for this conclusion. The relative increase in *Settlements* for treated firms, relative to control firms, appears primarily to be due to shifts in *Withdrawals*.

port 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 suggest the following. The upstream analysis of the mechanisms underlying the previously documented adjustments in accounts payable suggests that these are indeed associated with shifts in trade credit maturities. 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 for this maturity shifting process.[30] 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 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 that the fraud and failure of a large Swedish cash-in-transit firm imposed on its clients. These unique events provide an opportunity to derive causal inference on the roles played by cash holdings, and trade credit margins to handle liquidity

[30] 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, the customers mostly choose not to default on the supplier claim, 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 the business relationship.

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 (payables), and by decreasing the amount of issued credit to suppliers (receivables). Secondly, in terms of magnitudes, upstream adjustments dominate downstream adjustments; and the compounded adjustment at the two trade credit margins is found to be of the same order as adjustments in cash holdings, suggesting that trade credit positions indeed provide important sources of reserve liquidity. Thirdly, by exploring the underlying mechanism of the trade credit adjustments, we find evidence suggesting that the observed changes are due to shifts in the time dimension, where firms in need of liquidity increase durations on the trade credit contracts upstream and reduce them downstream. Finally, adjustment capacity in cash holdings and trade credit margins appear to be complements, and where in particular credit constrained firms rely on combinations of these sources to handle liquidity shocks.

As Cuñat (2007) points out, establishing the role of trade credit in firms' liquidity management may provide important insights in the understanding of 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 confirm that such 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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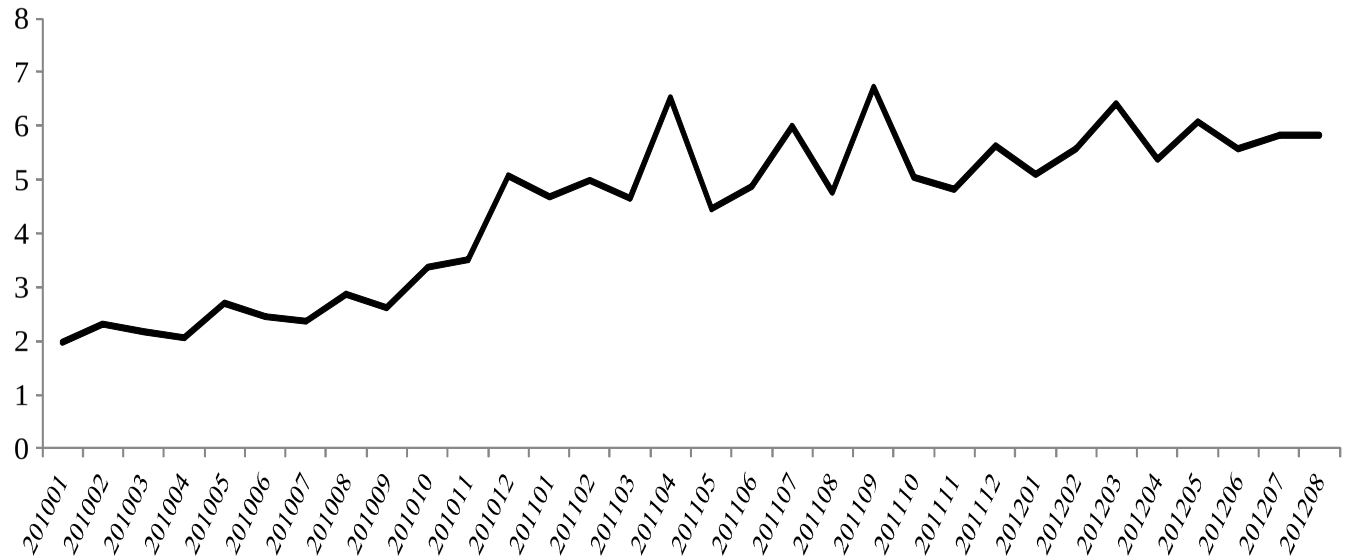
Figure 1

PANAXIA; TIME FROM COLLECTION TO TRANSFER

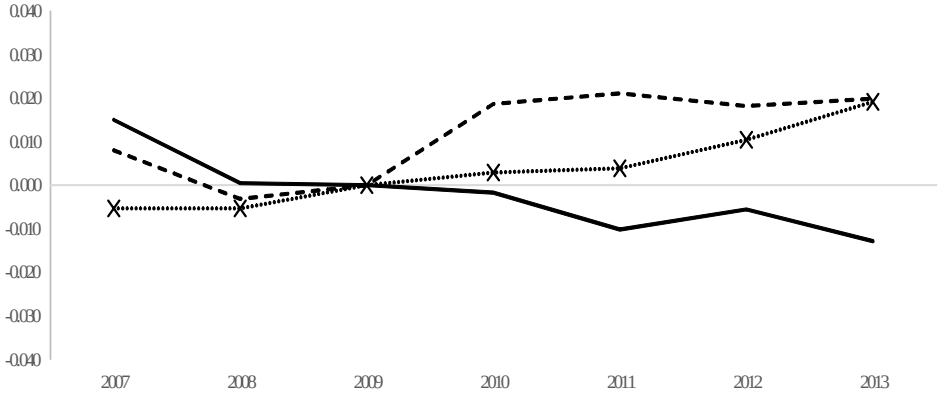


This figure illustrates, at a monthly frequency, the average number of days that Panaxia held on to their clients' proceeds before transfer, in the period running up to the bankruptcy. The time period covers January, 2010 to August, 2012.

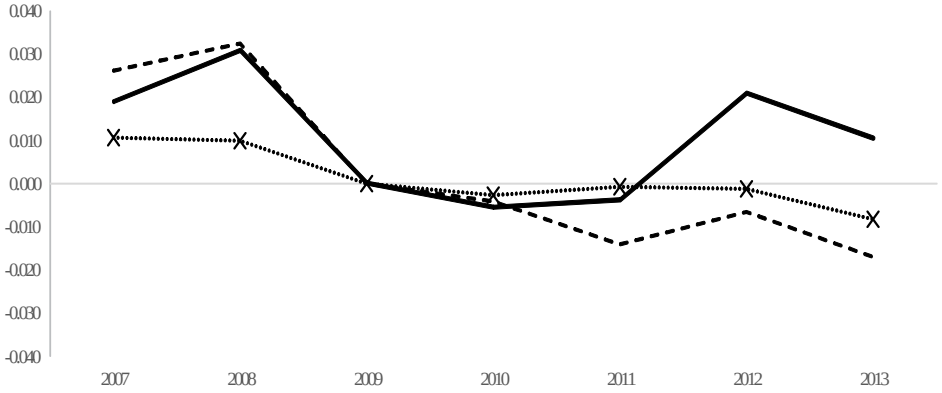
Figure 2

MEANS IN OUTCOME VARIABLES

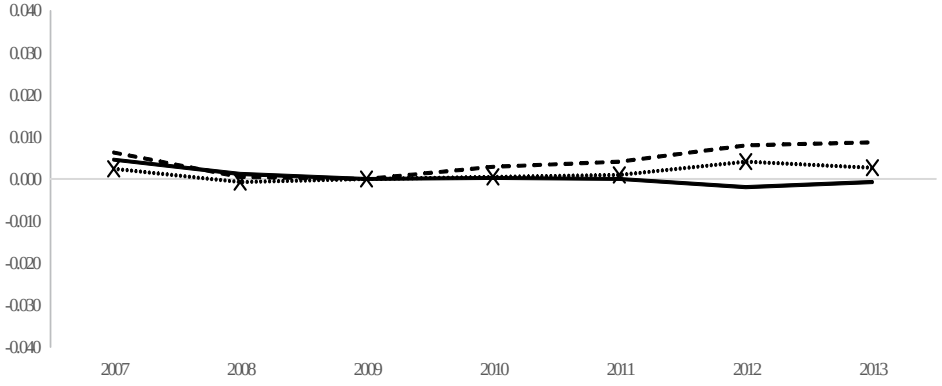
Panel A. *Cash/Assets*



Panel B. *Payables/Assets*



Panel C. *Receivables/Sales*

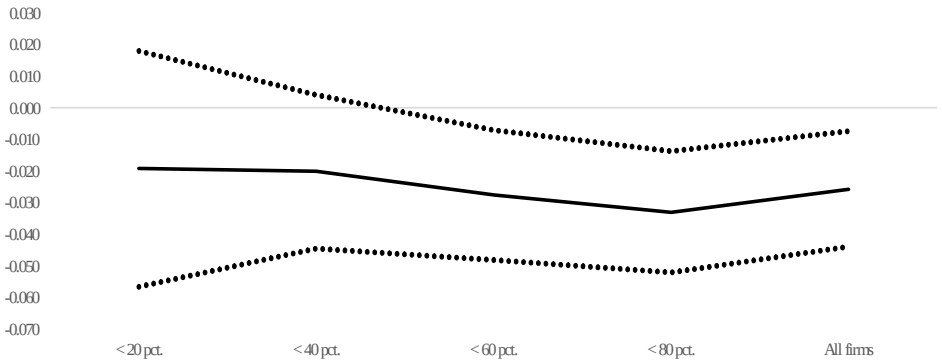


This figure reports normalized means in the three outcome variables: cash-to-assets, payables-to-assets, and receivables-to-sales, over the 2007–2013 period, for the treated (solid line), non-treated (dotted line with crosses) and matched control (dashed line) firms. The values are normalized by 2009-outcomes. In each year, only pairs for which there are data on both the treated and the control firms are included. Means for non-treated firms are calculated using weights corresponding to the fraction of treated firms in each particular 5-digit industry.

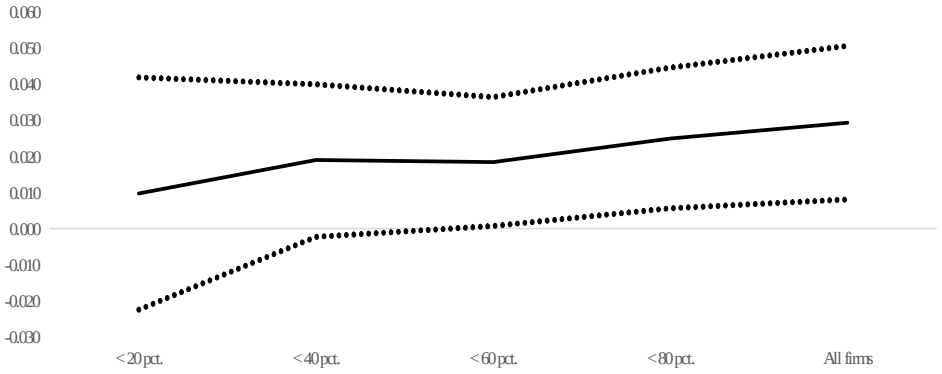
Figure 3

TREATMENT EFFECTS CONDITIONAL ON LOSS-SIZE

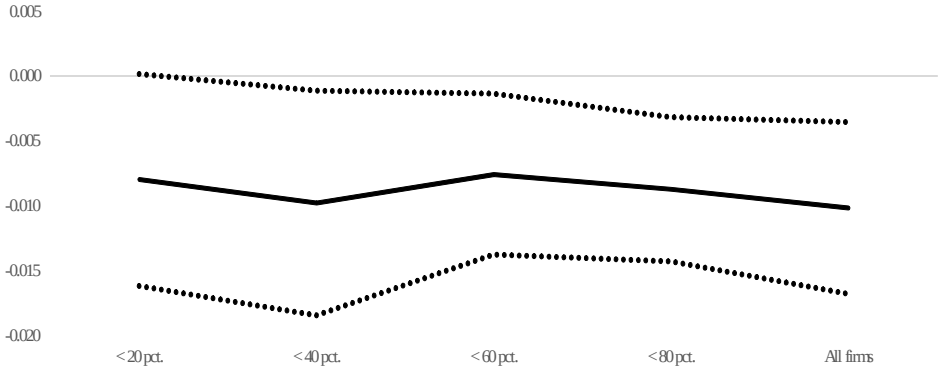
Panel A. *Cash/Assets*



Panel B. *Payables/Assets*



Panel C. *Receivables/Sales*

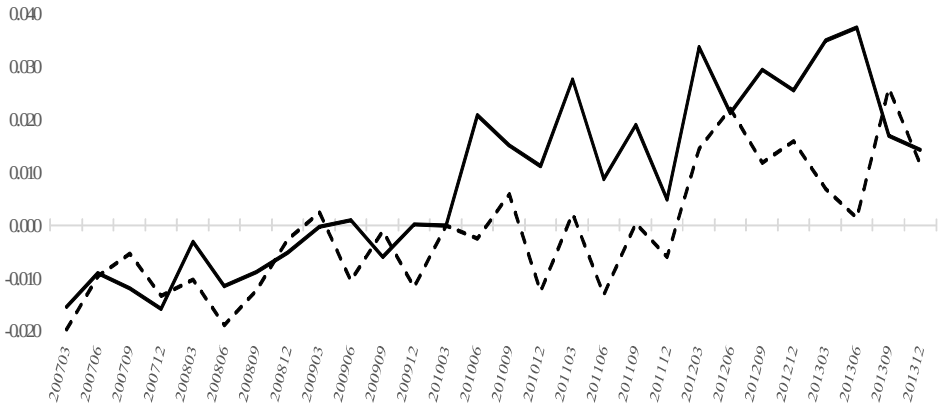


This figure reports cumulative adjustments, Eq. (2), in cash holdings, accounts payable, and accounts receivable in 2012. Calculations are based on the sample of 494 treated firms that incurred losses. The horizontal axis indicates the samples used for estimation: the bottom 20 percent of treated firms in the Loss/Asset-distribution; the bottom 40 percent of treated firms in the Loss/Asset-distribution; the bottom 60 percent of treated firms in the Loss/Asset-distribution; the bottom 80 percent of treated firms in the Loss/Asset-distribution; and all treated firms that incurred losses in the bankruptcy. In each sub-sample, we include corresponding matched control firms for the selection of treated firms at hand. The solid lines mark the estimates of cumulative adjustment and the dotted lines indicate 95-percent confidence intervals. Standard errors are clustered at the firm-level to account for the multiplicity of control firms.

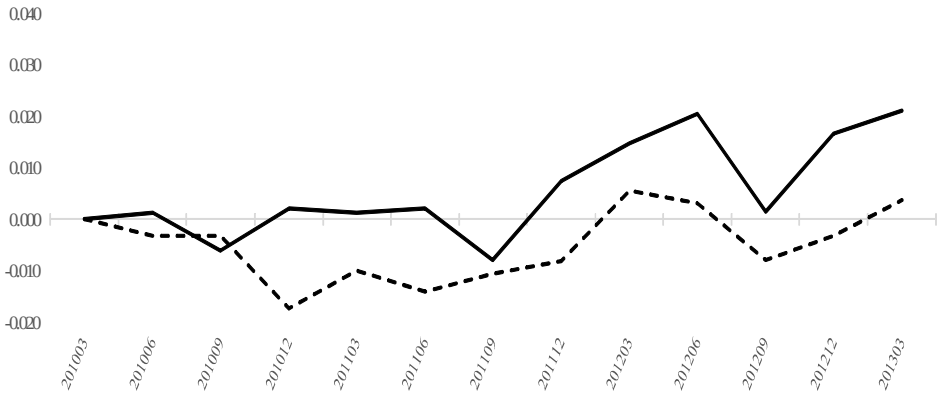
Figure 4

LATE PAYMENTS AND ENFORCMENT OF LATE PAYMENTS

Panel A. Late payments; Upstream perspective



Panel B. Enforcment of late payments; Downstream perspective



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

Table 1

PANAXIA AB; PERFORMANCE AND EXTERNAL FINANCING

Variables	2006	2007	2008	2009	2010	2011
1. Performance						
Total sales (In M SEK)	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 M SEK)	268.2	515.3	914.5	852.8	899.8	854.3
Net income (In M SEK)	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%
2. External financing						
Bank debt (In M SEK)	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 2006–2011 period.

Table 2

DESRIPTIVE STATISTICS FOR TREATED, NON-TREATED, AND MATCHED CONTROL FIRMS

	Panel A. Treated firms		Panel B. Non-treated firms (weighted)			Panel C. Matched control firms		
					t-test			*t*-test
	Mean	SD	Mean	SD	(*p*-val.)	Mean	SD	(*p*-val.)
Variables	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
1. Event characteristics								
Claim/Assets$_{2012}$	0.079	0.108	-	-	-	-	-	-
Loss/Assets$_{2012}$	0.043	0.051	-	-	-	-	-	-
2. Firm characteristics								
Cash flow/Assets$_{2009}$	0.083	0.144	0.087	0.177	(0.768)	0.087	0.141	(0.771)
Assets$_{2009}$ (In M SEK)	240.307	3,087.629	28.608	1018.081	(0.172)	76.759	413.432	(0.294)
Sales growth$_{2009}$	0.047	0.297	0.017	0.352	(0.076)	0.027	0.269	(0.318)
Debt/Assets$_{2009}$	0.168	0.247	0.230	0.270	(0.336)	0.175	0.235	(0.914)
Tangible assets/Assets$_{2009}$	0.200	0.234	0.302	0.279	(0.134)	0.216	0.241	(0.815)
Inventories/Assets$_{2009}$	0.276	0.203	0.248	0.244	(0.288)	0.278	0.206	(0.947)
Age$_{2009}$	14.887	16.796	15.971	13.566	(0.776)	14.093	14.992	(0.839)
Cash/Assets$_{2009}$	0.179	0.173	0.251	0.229	(0.000)	0.184	0.183	(0.663)
Payables/Assets$_{2009}$	0.242	0.158	0.162	0.150	(0.083)	0.232	0.155	(0.828)
Receivables/Sales$_{2009}$	0.021	0.041	0.033	0.073	(0.127)	0.028	0.042	(0.392)
Cash/Assets$_{2008}$	0.179	0.170	0.246	0.226	(0.000)	0.181	0.181	(0.908)
Payable/Assets$_{2008}$	0.273	0.191	0.172	0.157	(0.123)	0.264	0.184	(0.896)
Receivable/Sales$_{2008}$	0.022	0.046	0.033	0.070	(0.204)	0.029	0.045	(0.450)
N. Obs.	610		49,633			610		
N. Unique firms	610		49,633			482		

This table reports descriptive statistics for the 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 5-digit industry. The p-values refer to tests for differences in means, where standard errors are clustered at the firm-level for non-franchisees and at the franchisor-level for franchisees. The tests for differences in means in Panel B compare outcomes for treated firms with the sample of non-treated firms; and Panel C compares treated firms with the matched control firms. Variable definitions are provided in Table A3.

Table 3

BASELINE ESTIMATES; AVERAGE TREATMENT EFFECTS FOR TREATED FIRMS

	Treatment period			Post-treatment period	Test of parallel pre-trends
	(I)	(II)	(III)	(IV)	(V)
Variables	2010	2011	2012	2013	(*p*-val.)
Panel A. *y = Cash/Assets*					
τ_t^y	-0.020^{**}	-0.011^{*}	0.008	-0.009	(0.858)
	(-2.3)	(-1.8)	(1.2)	(-0.7)	
T_t^y	-0.020^{**}	-0.031^{***}	-0.024^{***}	-0.032^{***}	
	(-2.3)	(-3.7)	(-2.9)	(-2.7)	
Panel B. *y = Payables/Assets*					
τ_t^y	-0.001	0.011^{**}	0.018^{*}	-0.000	(0.712)
	(-0.2)	(2.4)	(1.7)	(-0.0)	
T_t^y	-0.001	0.010	0.028^{***}	0.028^{**}	
	(-0.2)	(1.1)	(3.0)	(2.5)	
Panel C. *y = Receivables/Sales*					
τ_t^y	-0.003^{**}	-0.002	-0.006^{***}	0.000	(0.430)
	(-2.1)	(-1.0)	(-2.5)	(0.1)	
T_t^y	-0.003^{**}	-0.004^{**}	-0.010^{***}	-0.010^{***}	
	(-2.1)	(-2.3)	(-3.3)	(-3.1)	
N. Treated firms			610		-
N. Control firms			610		-
N. Unique control firms			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 on the 2007–2009 period, and follow the approach proposed by Mora and Reggio (2015). Variable definitions are provided in Table A3. The standard errors are clustered at the firm-level for non-franchisees and at the franchisor-level for franchisees. ***, **,* denote statistically distinct from 0 at the 1, 5 and 10 percent level, respectively.

Table 4

NO LOSSES VS. INCURRED LOSSES

	No losses in 2012					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)
Variables	2010	2011	2012	2013	(*p*-val.)	2010	2011	2012	2013	(*p*-val.)
Panel A. *y* = *Cash/Assets*										
τ_t^y	−0.009	−0.032**	0.025**	−0.014	(0.756)	−0.023**	−0.006	0.003	−0.008	(0.858)
	(−0.7)	(−2.4)	(2.0)	(−1.0)		(−2.3)	(−0.9)	(0.5)	(−0.5)	
T_t^y	−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)	(−3.0)	(−2.8)	(−2.5)	
Panel B. *y* = *Payables/Assets*										
τ_t^y	0.011	0.010	−0.000	0.002	(0.767)	−0.004	0.012**	0.022**	−0.001	(0.543)
	(1.5)	(1.2)	(−0.0)	(0.2)		(−0.5)	(2.2)	(2.0)	(−0.0)	
T_t^y	0.011	0.021**	0.021	0.023		−0.004	0.008	0.029***	0.029**	
	(1.5)	(2.1)	(1.5)	(1.4)		(−0.5)	(0.8)	(2.7)	(2.2)	
Panel C. *y* = *Receivables/Sales*										
τ_t^y	−0.001	0.003	−0.010**	−0.002	(0.531)	−0.003**	−0.003	−0.005*	0.001	(0.526)
	(−0.4)	(1.5)	(−2.5)	(−0.4)		(−2.2)	(−1.5)	(−1.8)	(0.2)	
T_t^y	−0.001	0.002	−0.008	−0.010		−0.003**	−0.006***	−0.010***	−0.010***	
	(−0.4)	(0.6)	(−1.5)	(−1.3)		(−2.2)	(−2.9)	(−3.0)	(−2.8)	
N. Treated firms			116					494		
N. Control firms			116					494		
N. Unique control firms			116					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) report results for the sub-sample of treated firms that incurred losses in 2012. The tests of 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 A3. Standard errors are clustered at the firm-level for non-franchisees and at the franchisor-level for franchisees. ***, **,* denote statistically distinct from 0 at the 1, 5 and 10 percent level, respectively.

Table 5

RESPONSES CONDITIONAL ON CREDIT CONSTRAINTS

Cumulative effects (T^{y}_{2012})		Constraint criteria:											
		Panel A. Firm size						Panel B. Rating					
		Constrained		Unconstrained		*t*-test		Constrained		Unconstrained		*t*-test	
		(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)	(X)	(XI)	(XII)
Row	Outcome variables (y)	Coef.	(*t*-val.)	Coef.	(*t*-val.)	H_0	*p*-val.	Coef.	(*t*-val.)	Coef.	(*t*-val.)	H_0	*p*-val.
(1)	*Cash/Assets*	-0.036^{***}	(-3.3)	-0.001	(-0.0)	U $<$ C	0.021	-0.029^{***}	(-2.9)	-0.017	(-0.9)	U $<$ C	0.284
(2)	*Payables/Assets*	0.035^{***}	(2.6)	0.016	(1.3)	C $<$ U	0.136	0.037^{***}	(2.8)	0.012	(0.9)	C $<$ U	0.071
(3)	*Receivables/Sales*	-0.011^{**}	(-2.4)	-0.008**	(-2.4)	U $<$ C	0.276	-0.013^{***}	(-2.8)	-0.003	(-1.2)	U $<$ C	0.027
(4)	*Bank debt/Assets*	-0.003	(-0.7)	0.007	(1.1)	U $<$ C	0.100	-0.004	(-0.9)	0.010^{**}	(2.1)	U $<$ C	0.012
(5)	N. Frims	346/346/232		148/148/140				347/347/243		147/147/137			

This table reports estimates of cumulative adjustments, Eq. (2), for cash holdings, accounts payable, accounts receivable, and short-term bank debt, in 2012. The model is estimated on sub-samples classified with respect to the total assets of the firms (Panel A) and the credit ratings of the firms (Panel B). t-values calculated using robust standard errors, clustered at the firm-level for non-franchisees and at the franchisor-level for franchisees, are reported within parenthesis. p-values refer to one-sided tests for differences in coefficients between the sub-samples. Variable definitions are provided in Table A3. ***, **,* denote statistically distinct from 0 at the 1, 5 and 10 percent level, respectively.

Table 6

LATE PAYMENTS; UPSTREAM AND DOWNSTREAM

		Specification:		Estimation	Pre-treatment
		Yes/No (0/1)	Ln(1+*N*)	period	period
Row	Outcome variables (*y*)	(I)	(II)	(III)	(IV)
Panel A. Upstream					
(1)	*Late payments*	0.017**	0.188	07Q1-12Q4	07Q1–09Q4
		(2.3)	(1.5)		
		[0.632]	[0.682]		
(2)	*Defaults*	−0.003	−0.296	07Q1-12Q4	07Q1–09Q4
		(−1.0)	(−0.9)		
		[0.148]	[0.000]		
(3)	*Settlements*	0.018***	0.227*	07Q1-12Q4	07Q1–09Q4
		(2.6)	(1.9)		
		[0.532]	[0.563]		
(4)	*Withdrawals*	0.022*	0.393*	10Q1–12Q4	10Q1
		(1.9)	(1.7)		
(5)	*Payments to EA*	0.001	0.126	10Q1–12Q4	10Q1
		(0.2)	(0.2)		
(6)	*Contested claims*	−0.001	−0.177	10Q1–12Q4	10Q1
		(−0.1)	(−0.4)		
Panel B. Downstream					
(7)	*Late payments*	0.013	0.382	10Q1–12Q4	10Q1
		(1.4)	(1.4)		
(8)	*Defaults*	0.002	0.031	10Q1–12Q4	10Q1
		(0.2)	(0.1)		
(9)	*Settlements*	0.012	0.449	10Q1–12Q4	10Q1
		(1.4)	(1.5)		
(10)	*Withdrawals*	0.021**	0.891***	10Q1–12Q4	10Q1
		(2.4)	(2.7)		
(11)	*Payments to EA*	−0.007	−0.726	10Q1–12Q4	10Q1
		(−1.6)	(−1.6)		
(12)	*Contested claims*	−0.005	−0.313	10Q1–12Q4	10Q1
		(−0.9)	(−0.8)		

This table reports difference-in-differences estimates, Eq. (3). Panel A reports results for applications faced by the firms (upstream perspective); and Panel B reports results for applications issued by the firms (downstream perspective). Column (I) reports results for a linear probability model and Column (II) report results for a Tobit model. Variable definitions are provided in Table A3. 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 A3. The standard errors are clustered at the firm-level for non-franchisees and at the franchisor-level for franchisees. ***, **,* denote statistically distinct from 0 at the 1, 5 and 10 percent level, respectively.

Appendix

A.1 Accounting Practises and the Measurement of Cash Adjustments

The accounting rules in Sweden—which are in line with international standards—do not indicate one 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; of which two are very close, but for clarity we will mention both.

Firstly, the least cumbersome way for the firm is to not re-book, but simply let the cash-in-transit remain a part of cash-holdings on the books, until notice is received about the transfer to the bank account having been completed (denoted as Practice 1A). At this point, the firm will then book the funds as bank-holdings rather than cash-holdings. Secondly, the firm can book the money picked up by the cash-in-transit firm on a "cash-in-transit" account, i.e., a sub-account under cash-holdings, whilst the money is on its way to the bank account (denoted as Practice 1B). That is, in the accounts make a distinction between cash and cash-in-transit, during the transfer period. Once the funds hit 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-transfer as a short-term claim on the cash-in-transit firm, and then re-book as a bank-holding once the money is obtained from the cash-in-transit firm Panaxia (denoted as Practice 2).

To illustrate how these different accounting practises affect the measurement of cash holdings in firms financial statements, we present an illustrative simplified example. Consider a firm with cash flow, CF_t, which corresponds to the difference between the inflows of funds, $Inflow_t$, and outflows of funds, $Outflow_t$. In an initial step, we assume that the firm balances all fluctuations in cash flow using its cash holdings, CH_t, such that: $\Delta CH_t = CH_t - CH_{t-1} = CF_t$. In other words, we abstract from the influence of other financing sources, such as trade credit or bank financing. Columns (I) in Table A1 shows how cash holdings evolve over the 2009-2012 period for a firm which is not subject to fraud.

Let us now consider the case of the Panaxia fraud, where a fraction α_t of the firm's $Inflow_t$ was withheld each year. Columns (II) and (III) in Panel A show how the cash and short-term claims accounts in the financial statement evolved for the cases when Practice 1A, or 1B, are used and the same columns in Panel B show how they evolved for the case when Practice 2 is applied.[31] Column (IV) shows the difference in cash holdings between the fraud (Column (II)) and the counterfactual of no fraud (Column (I)).

[31] The cash account in the financial statements primarily cover cash- and bank-holdings. If Practice 1B is applied, cash-in-transfer would show up under the cash account, and we would not be able to distinguish the cash-in-transfer from cash- and bank-holdings. Thus, given the level at which the accounts are aggregated in our data, Practises 1A and 1B will have the same implications for cash-holdings.

For Practices 1A and 1B, Column (IV) in Panel A shows that since the firm books the cash-intransfer under the cash account, there is no difference in cash holdings between the fraud and no fraud cases in 2010 and 2011.[32] In 2012, however, there is a relative decline in cash-holdings when the firm that is exposed to the fraud incurs a loss as Panaxia enters bankruptcy. That is, the bankruptcy loss leads the firm to write off that amount of its cash-holdings. For Practice 2, Column (IV) in Panel B, the cash-in-transfer is booked under a short-term claims account, which results in a relative decline in cash holdings from the point in time when Panaxia starts to delay transfers of cash-in-transit to the firm's bank account. That is, the relative decline starts in 2010, and continues in 2011 and 2012—where the decline in each year is proportional to the increase in the amount withheld, α_t. Thus, the accounting practices result in different implications for relative cash holdings in 2010 and 2011, but not in 2012. We will now proceed and discuss how the practices may influence the interpretation of our results.

Under the special case outlined above, there is a one-to-one relationship between cash holdings and cash flow; in other words, firms rely completely on cash to manage variations in cash flow. However, the picture changes if we introduce other financing sources. For example, consider trade credit and bank financing. By postponing trade credit payments, accounts payable, a firm could balance parts, or the full, Panaxia-withheld inflow of funds, α_t, by postponing a portion of $Outflow_t$ directed to its suppliers. Similarly, by using a bank line of credit the firm could balance parts, or the full, withheld inflow of funds. Another potential measure by the firm to balance the withheld inflow, is to reduce maturities on extended trade credit, accounts receivable, which would lead to an upward push for $Inflow$ in the year that trade credit maturity is reduced. Thus, we will only see a relative decline in cash for firms that rely on cash to balance the withheld inflow and no decline for firms that rely on other financing sources.[33] One caveat in our analysis is that for Practices 1A and 1B, we will underestimate the reliance on cash 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 balances the amount withheld, $\alpha_t Inflow_t$, by drawing down on its bank line of credit. This would imply that the $Outflow_t$ influencing the cash account is reduced by $\alpha_t Inflow_t$ in 2010 and 2011.[34] The reduction in $Outflow_t$ would lead to a relative increase in cash holdings of $\alpha_t Inflow_t$ in 2010 and 2011 (Rows (2) and (3) in Column (IV)). In other words, the fraud does not lead to a mechanical decline in cash—rather, the interpretation is that

[32] Note that $\alpha_t Inflow_t$, the illiquid share of $Inflow_t$ in t, becomes liquid in $t+1$, hence $CH_t = Inflow_t - Outflow_t$, and not $CH_t = (1-\alpha_t)Inflow_t + \alpha_t Inflow_t - Outflow_t$, when entered into the calculation of CH_{t+1}.

[33] This reasoning aligns with the one presented by Almeida et al. (2004), who explore the cash flow sensitivity of cash, and propose that a positive relationship between cash flow and cash only should be observed for financially constrained firms.

[34] Using a bank line of credit imposes a disconnect between the $Outflow$ affecting CF in Column (I) and the $Outflow$ affecting CH in Column (III); more specifically, covering expenses with a bank line of credit leaves the $Outflow$ affecting the cash flow, CF, unchanged, but reduces the $Outflow$ affecting the cash account, CH.

a decline is reflecting a firm's decision to use cash to balance the withheld funds due to transfer delays. Note that under Practises 1A and 1B, an underestimation of the treatment effect on cash in 2010 and 2011 is at hand, but not so in 2012.

Which practise 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., Practices 1A or 1B. However, when transfer times increase in duration, it becomes conceptually less clear that cash-in-transfer should remain booked under the cash account, but instead should 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 4 (firms that incurred a loss) show that the decline in cash is strongest in the beginning of the treatment period and that there is no effect in 2012. Furthermore, results in the left-hand side of Table 4 (firms that were compensated) show a statistically significant increase in cash-holdings in 2012. This is also consistent with firms on average having booked the cash-in-transfer under short-term claims and then having filled up cash again upon being compensated in 2012. In addition, a shift from Practices 1A and 1B to Practice 2 could potentially contribute to the pronounced effect for cash in 2011; 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 practise.

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 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 large components, such as claims related to tax payments. To illustrate this, average other short-term claims scaled by assets amounts to 22% for the treated and control firms in 2009. Nevertheless, when estimating the cumulative treatment effects for the outcome variable other short-term claims-to-assets we obtain estimates (t-values) 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 estimates are nevertheless small—if Practice 2 prevails, we would expect coefficients that exceed adjustments in cash holdings—which could be due to the event also affecting other components booked on the 'other short-term claims' account. For instance, the cumulative effect in post-treatment year 2013 is large and significant, which

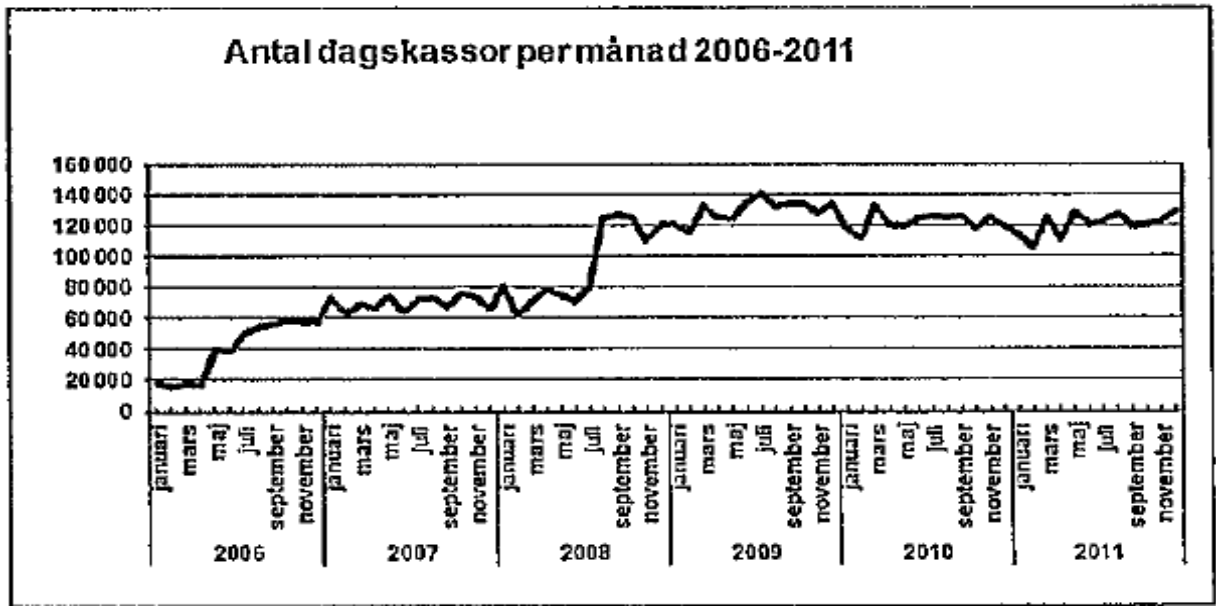
is obviously unrelated to shifts in cash-in-transfer.

Taken together, due to a fraction of the treated firms having potentially applied Practises 1A and 1B, we should caution our interpretation of the cash effects in 2010 and 2011; cash-in-transfer may be 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 should not matter for the cash estimates.

A.2 Figures and Tables

Figure A1

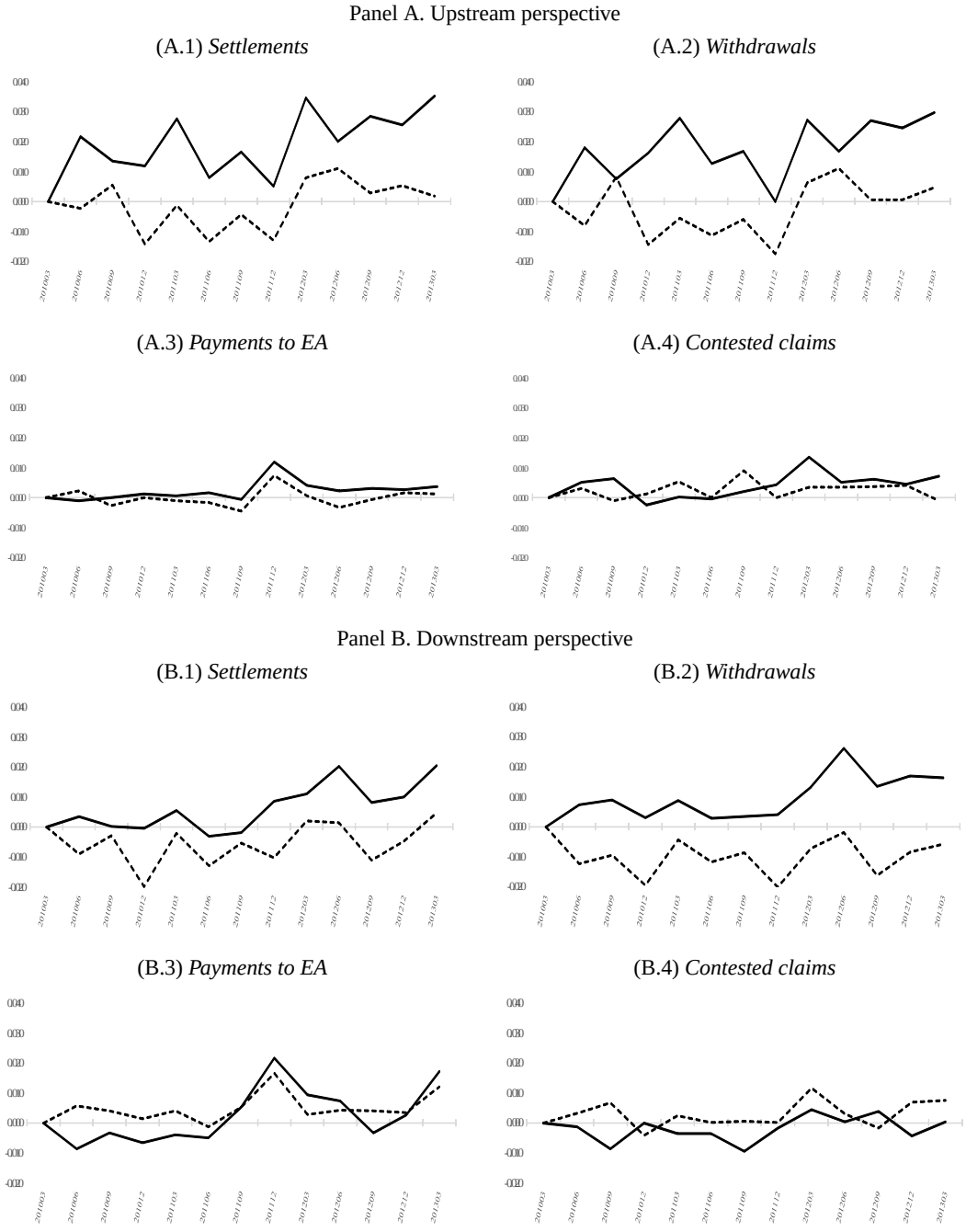
THE NUMBER OF COLLECTED RECEIPTS DURING THE 2006–2011 PERIOD



This figure is obtained from Panaxia's bankruptcy report. It shows the number of collected receipts in each month in the period 2006–2011.

Figure A2

SETTLEMENTS AND ITS COMPONENTS



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 (solid line) and matched control (dashed line) firms; and Panel B shows outcomes for settlements for enforcements imposed by the treated (solid line) and matched control (dashed line) firms. Variable definitions are provided in Table A3.

Table A1

ILLUSTRATIVE EXAMPLE OF DIFFERENT ACCOUNTING PRACTISES' CONSEQUENCES

FOR CASH-HOLDINGS UNDER NO FRAUD/FRAUD

		No fraud	Panaxia fraud		
Row	Year (t)	CH_t (I)	CH_t^{α} (II)	*Short-term claim*$_t$ (III)	Dif. $CH_t^{\alpha} - CH_t$ (IV)
Panel A. Accounting practise 1A and 1B					
(1)	2009	$CH_{2008} + CF_{2009}$	$CH_{2008} + Inflow_{2009} - Outflow_{2009}$	0	0
(2)	2010	$CH_{2009} + CF_{2010}$	$CH_{2009} + (1 - \alpha_{2010})Inflow_{2010} + \alpha_{2010} Inflow_{2010} - Outflow_{2010}$	0	0
(3)	2011	$CH_{2010} + CF_{2011}$	$CH_{2010} + (1 - \alpha_{2011})Inflow_{2011} + \alpha_{2011} Inflow_{2011} - Outflow_{2011}$	0	0
(4)	2012	$CH_{2011} + CF_{2012}$	$CH_{2011} + (1 - \alpha_{2012})Inflow_{2012} - Outflow_{2012}$	0	$-\alpha_{2012}$Inflow$_{2012}$
Panel B. Accounting Practise 2					
(5)	2009	$CH_{2008} + CF_{2009}$	$CH_{2008} + Inflow_{2009} - Outflow_{2009}$	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$_{2012}$

The table shows how different accounting practises influence 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.

Table A2

SAMPLE CHARACTERISTICS—NUMBER OF PANAXIA CLIENTS

Panel A. Type of firm	Total	Uncompensated firms (*Item 1*)	Franchisees (*Item 2*)	Compensated by savings banks (*Item 3*)	Pharmacies (*Items 1 & 2*)
1. Unidentified firms	38	18	20	0	0
2. Financial firms	13	13	0	0	0
3. Unincorporated firms	173	43	0	130	0
4. Pharmacies	131	0	0	0	131
5. Non-financial corporations					
Franchisor	1	1	0	0	0
With missing accounting information	289	74	175	40	0
With accounting information (final sample)	610	260	234	116	0
Sum	1255	409	429	286	131

Panel B.	Year						
	2007	2008	2009	2010	2011	2012	2013
1. Non-financial corporations (excl. pharmacies)							
N. Firms	599	692	819	856	884	899	897
N. New firms	55	93	127	37	28	15	0
N. Failures	0	0	0	0	0	2	5
N. Firms in final analysis	543	610	610	610	610	610	610
2. Pharmacies							
N. Firms	6	23	25	107	127	131	129
N. New firms	0	17	2	82	20	4	0
N. 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, and Panel B reports the number of non-financial firms (the franchisor excluded) and pharmacies over the 2007–2013 period.

Table A3

VARIABLE DEFINITIONS

Variable names	Definitions (Data source)
1. Event variables	
Claim	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).
2. Outcome variables	
Cash	Cash and short-term investments (Financial statements).
Payables	Accounts payable (Financial statements).
Receivables	Accounts receivable (Financial statements).
Total bank debt	Total bank debt (Financial statements).
Short-term bank debt	Short-term bank debt (Financial statements).
Long-term bank debt	Long-term bank debt (Financial statements).
Late payments	Applications for the issuance of injunctions to settlement of outstanding claims (Enforcement Agency).
Defaults	Applications that remain unsettled after a fortnight from the time of notification (Enforcement Agency).
Settlements	Applications that are settled through a Withdrawal, Payment to EA, or Contested claim (Enforcement Agency).
Withdrawals	Applications that were withdrawn from the EA by the supplier (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).
3. Control variables	
Cash flow	Earnings after interest expenses and taxes, but before depreciation and amortization (Financial statements).
Assets	Book value of 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 registration as a corporate (Credit bureau).
CGS	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 variables.

Table A4

ASSESSING BALANCE

	Panel A. Non-treated (weighted)				Panel B. Matched control firms			
	Normalized difference ($\Delta_{c,t}$)	Coverage frequencies $\pi_c^{.95}$	$\pi_t^{.95}$	Log of ratio of SD ($\Gamma_{c,t}$)	Normalized difference ($\Delta_{c,t}$)	Coverage frequencies $\pi_c^{.95}$	$\pi_t^{.95}$	Log of ratio of SD ($\Gamma_{c,t}$)
Variables	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Cash flow/Assets$_{2009}$	−0.027	0.975	0.924	−0.209	−0.033	0.941	0.957	0.022
Assets$_{2009}$ (In M SEK)	0.092	0.918	0.880	1.109	0.074	0.966	0.979	2.011
Sales growth$_{2009}$	0.093	0.964	0.932	−0.171	0.071	0.951	0.954	0.100
Debt/Assets$_{2009}$	−0.239	0.510	0.629	−0.091	−0.029	0.489	0.634	0.048
Tangible assets/Assets$_{2009}$	−0.397	0.989	0.796	−0.178	−0.069	0.967	0.920	−0.029
Inventories/Assets$_{2009}$	0.127	0.938	0.577	−0.182	−0.009	0.931	0.890	−0.012
Age$_{2009}$	−0.071	0.872	0.965	0.214	0.050	0.882	0.915	0.114
Cash/Assets$_{2009}$	−0.356	0.997	0.816	−0.279	−0.028	0.962	0.921	−0.056
Payables/Assets$_{2009}$	0.518	0.920	0.761	0.051	0.065	0.948	0.941	0.022
Receivables/Sales$_{2009}$	−0.206	0.618	0.659	−0.572	−0.170	0.598	0.757	−0.016
Cash/Assets$_{2008}$	−0.331	0.993	0.834	−0.286	−0.007	0.964	0.926	−0.062
Payable/Assets$_{2008}$	0.576	0.856	0.785	0.198	0.046	0.936	0.957	0.040
Receivable/Sales$_{2008}$	−0.178	0.603	0.685	−0.414	−0.142	0.584	0.734	0.031
N. Treated/N. Control	610/49,633				610/610			
N. Unique control firms	49,633				482			

This table reports four measures of balance proposed by Imbens and Rubin (2015): normalized differences; coverage frequencies; and the logarithm of the ratio of standard deviations. Panel A compares outcomes for treated firms with the sample of non-treated firms; and Panel B compares treated firms with the matched control firms. 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 A3.

Table A5

BANK FINANCING

	Average treatment effects for treated firms (*ATT*)				
	Treatment period			Post-treatment period	Test of parallel pre-trends
	(I)	(II)	(III)	(IV)	(V)
Variables	2010	2011	2012	2013	(*p*-value)
Panel A. *y = Total bank debt/Assets*					
τ_t^y	0.000	-0.007^*	0.007^*	-0.012	(0.385)
	(0.1)	(-1.9)	(1.6)	(-1.6)	
T_t^y	0.000	-0.007	0.000	-0.011	
	(0.1)	(-1.3)	(0.1)	(-1.3)	
Panel B. *y = Short-term bank debt/Assets*					
τ_t^y	0.002	-0.003	0.004^{**}	-0.001	(0.528)
	(0.9)	(-1.6)	(2.4)	(-0.7)	
T_t^y	0.002	-0.002	0.003	0.001	
	(0.9)	(-0.6)	(1.0)	(0.5)	
Panel C. *y = Long-term bank debt/Assets*					
τ_t^y	-0.001	-0.003	0.002	-0.012^*	(0.206)
	(-0.2)	(-0.9)	(0.5)	(-1.7)	
T_t^y	-0.001	-0.004	-0.002	-0.013	
	(-0.2)	(-0.8)	(-0.3)	(-1.6)	
N. Treated firms	610				
N. Control firms	610				
N. Unique control firms	482				

This table reports estimates of yearly adjustments, Eq. (1), and cumulative adjustments, Eq. (2), in total bank debt, short-term bank debt, and in long-term bank debt, over the treatment and post-treatment periods. The tests of parallel pre-trends are conducted using the 2007–2009 period, and follow the approach proposed by Mora and Reggio (2015). Variable definitions are provided in Table A3. The standard errors are clustered at the firm-level to account for multiplicity of matched control firms. ***, **,* denote statistically distinct from 0 at the 1, 5 and 10 percent level, respectively.

Table A6

ROBUSTNESS ANALYSES; BASELINE RESULTS

Cumulative effects (T^{y}_{2012})			Outcome variables (y):								
			Cash		*Payables*		*Receivables*		*Short-term bank debt*		
Row	Specification	year (t)	Coef. (I)	(t-val.) (II)	Coef. (III)	(t-val.) (IV)	Coef. (V)	(t-val.) (VI)	Coef. (VII)	(t-val.) (VIII)	N. Firms (IX)
(1)	Baseline	2012	-0.024^{***}	(-2.9)	0.028^{***}	(3.0)	-0.010^{***}	(-3.3)	0.003	(0.9)	610/610/482
(2)	Non-treated as control group	2012	-0.016^{***}	$(-3.0)^{+}$	0.022^{***}	(4.2)	-0.006^{***}	(-3.9)	0.007^{***}	(2.9)	610/49,633
(3)	Bias-adjusted estimators	2012	-0.025^{***}	(-2.7)	0.029^{***}	(3.4)	-0.012^{***}	(-4.2)	0.003	(0.9)	610/610/482
(4)	50 perc. closest matches	2012	-0.041^{***}	(-3.8)	0.029^{**}	(2.4)	-0.009^{**}	(-2.0)	0.005	(1.1)	305/305/245
(5)	*Payables/CGS*	2012	-0.016	(-1.2)	0.025^{***}	(3.1)	-0.001^{***}	$(-4.9)^{+}$	0.006^{**}	(2.2)	109/109/44
(6)	Truncated	2012	-0.019^{**}	(-2.0)	0.032^{***}	(2.8)	-0.009^{**}	(-2.5)	-0.001	(-0.3)	521/521/402
(7)	Franchisees omitted	2012	-0.015	(-1.4)	0.020^{**}	(2.6)	-0.008^{***}	(-2.7)	0.008^{*}	(1.8)	376/376/362
(8)	Pharmacies included	2012	-0.022^{***}	(-2.8)	0.028^{***}	(3.1)	-0.010^{***}	(-3.2)	0.003	(0.9)	617/617/487
(9)	Unbalanced panel	2012	-0.026^{***}	(-3.1)	0.069^{***}	(6.4)	-0.013^{***}	(-5.0)	0.001	(0.4)	649/649/517
(10)	Acc. period ends in Dec.	2010	-0.027^{***}	(-2.7)	-0.003	(-0.3)	-0.004^{***}	(-2.6)	0.001	(0.6)	463/463/339
(11)	Acc. period ends prior to Dec.	2010	0.002	(0.2)	0.003	(0.4)	0.002	(1.2)	0.004	(0.7)	147/147/146

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 5-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 (CGS), where the sample is restricted to the treated and control firms that report CGS; 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 cumulative effects in 2010 for the sub-sample of treated firms with an accounting period that ends in December; and Row (11) reports cumulative effects in 2010 for the sub-sample of treated firms with an accounting period that ends in the middle of the year. Variable definitions are provided in Table A3. The standard errors are clustered at the firm-level for non-franchisees and at the franchisor-level for franchisees. ***, **,* denote statistically distinct from 0 at the 1, 5 and 10 percent level, respectively. + denotes statistically destinct deviations in pre-treatment trends at the 10 percent level.

Table A7

DISBURSEMENTS FROM THE BANKRUPTCY ESTATE IN 2013

	Treatment period			Post-treatement period	Test of parallel pre-trends
	(I)	(II)	(III)	(IV)	(V)
Variables	2010	2011	2012	2013	(*p*-value)
Panel A. *y* = *Cash/Assets*					
τ_t^y	0.002	−0.008	−0.009	0.005	(0.878)
	(0.2)	(−0.9)	(−1.2)	(0.7)	
T_t^y	0.002	−0.006	−0.014	−0.009	
	(0.2)	(−0.5)	(−1.2)	(−0.7)	
Panel B. *y* = *Payables/Assets*					
τ_t^y	−0.001	0.007	0.014**	−0.009	(0.350)
	(−0.2)	(1.0)	(2.1)	(−1.5)	
T_t^y	−0.001	0.005	0.019**	0.010	
	(−0.2)	(0.7)	(2.1)	(1.1)	
Panel C. *y* = *Receivables/Sales*					
τ_t^y	−0.002	0.001	−0.007*	0.005	(0.993)
	(−1.1)	(0.5)	(−1.9)	(1.2)	
T_t^y	−0.002	−0.002	−0.008**	−0.003	
	(−1.1)	(−0.7)	(−2.3)	(−0.8)	
N. Treated firms			260		
N. Control firms			260		
N. Unique control firms			246		

This table reports estimates of yearly adjustments, Eq. (1), and cumulative adjustments, Eq. (2), in cash holdings, accounts payable, and in accounts receivable, over the treatment and post-treatment periods, for the sub-sample of treated firms that received final disbursements from the remaining assets of the bankruptcy estate in 2013. The tests of parallel pre-trends are conducted using the 2007–2009 period, and follow the approach proposed by Mora and Reggio (2015). Variable definitions are provided in Table A3. The standard errors are clustered at the firm-level to account for multiplicity of matched control firms. ***, **,* denote statistically distinct from 0 at the 1, 5 and 10 percent level, respectively.