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—both upstream and downstream.

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

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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 of 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 exogenous events that are close in nature to the concept of an economic shock. The Panaxia sequence of events provides an opportunity to form causal insights on firms' management of liquidity shortfalls. We begin our empirical analyses

[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 receivables and payables, scaled by assets, are 16 and 11 percent, respectively, for Swedish firms. This 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.

by evaluating adjustments in aggregate accounting measures of the three liquidity sources concerned: cash holdings; the amount of drawn trade credit from suppliers, accounts payable; and the amount issued to customers, accounts receivable. We proceed to 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 an intensified enforcement of payments from overdue customers. 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. Finally, we evaluate whether constraints for external financing determine firms' usage of the three liquidity sources in adverse circumstances.

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 setting 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.

Our investigation of the mechanisms underlying adjustments in trade credit positions using the data from the Swedish Enforcement Agency reveals that adjustments are in part due to shifts in trade credit durations—both upstream and downstream. Thus, the propensity to postpone settlement of trade credit increases for firms that are hit by the liquidity shortfall, as reflected by them being subject to more applications of injunction submitted by their suppliers. Likewise, treated firms also tend to increase efforts to enforce payment of overdue trade credit from their customers, by submitting more applications of injunction on behalf of their customers. Both mechanisms are clearly consistent with increases in payables and reductions in receivables, as noted above.

The applications of injunction are associated with various outcomes of the enforcement process. We find that the increased instances of overdue settlement, both upstream and downstream, primarily coincide with applications that are subsequently withdrawn from the Enforcement Agency. A withdrawal implies that the final settlement of overdue credit is resolved bilaterally by the trade credit parties, without external involvement. Consistent with a risk sharing view, this finding suggests a prevalence of cooperative outcomes in which the parties comply with the implicit risk sharing rules of the trade credit network to which they belong.

Moreover, 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.

Finally, our results show that adjustments in cash holdings and at the two trade credit margins can primarily be attributed to credit constrained firms, whereas unconstrained firms respond to the liquidity shortfalls by expanding their bank financing; suggesting that idiosyncratic liquidity shocks hitting financially constrained firms 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. Hence, cash and trade credit adjustments are used as complements to manage liquidity.

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 assets and liabilities that can be transformed into liquidity. To better understand how firms handle liquidity shocks, it is therefore important to also consider liquidity extraction in trade credit networks.

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. Cuñat shows empirically that large declines in firms' cash holdings are correlated with increases in their accounts payable. In addition, 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. Our paper extends these views by providing causal insights on how liquidity shocks impact on firms' cash and trade credit positions—both upstream and downstream—simultaneously, enabling an evaluation of the relative importance of these liquidity sources.

Another closely related paper by Garcia-Appendini and Montoriol-Garriga (2013) make use of the recent financial crisis to gauge how a 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. 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. Thus, these papers study redistribution of liquidity in trade credit chains—as we do—nonetheless, our perspective is different from theirs. They use aggregate credit supply shocks to measure substitution between bank and trade credit financing; whereas the current paper considers idiosyncratic liquidity deficiencies to demonstrate the importance of liquidity extraction from counterparties in the supply chain for firms' liquidity management. 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, 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. The next section outlines the Panaxia event, details our various data resources, and describes the empirical approach. Section 3 presents the empirical analyses and results outlined above. Section 4 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.[3] Collected receipts were then delivered to a bank depot for counting, and in one to two days, Panaxia credited the firms' bank accounts for the due funds. That is, according to the contracts between Panaxia and its clients, the latter would upon handing over the cash for transportation to the depot hold a claim on the former, until a transfer of funds to the clients' bank accounts had been carried out within a maximum of two days.

In the three-year period from 2006 to 2009, Panaxia expanded its operations forcefully; Table 1 shows that sales grew from SEK 197 Million in 2006 to SEK 677 Million in 2009, corresponding to a 244 percent increase. The quest for an increased market share was in part conducted through an aggressive pricing strategy, which in turn resulted in 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.]

[3] 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 healthcare, transportation, and rental.

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.[4] 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 with Panaxia. 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 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 onward. Furthermore, 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.

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. The pre-bankruptcy period—

[4] 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."

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, the clients' extra-ordinary claims became worthless as such, which implied that they took a significant loss to their cash flows. Secondly, the extra-ordinary claim on Panaxia turned into a claim on the bankruptcy estate, which can be seen as a solvency shock on the clients; that is, a realized loss resulting in a deterioration of their assets. Thus, by tracking the Panaxia clients over the 2010–2012 period and through this sequence of events, we can empirically evaluate the consequences for them caused by the increase in payment delays and by the subsequent liquidity losses.

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 in 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. Firstly, firms that were concurrently clients of Panaxia and one of four regional savings banks were fully and almost immediately compensated for their losses in the Panaxia bankruptcy by these banks. 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. Secondly, 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, Apoteksgruppen, and convenience stores, Reitan. 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 in the pre-treatment period), the convenience store franchisees' identities and claims are included. The identities of the Reitan franchisees were obtained using the list of collection sites, *Item 2*, whereas their claims had to be approximated.[5]

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

[5] The Reitan franchisees' claims were approximated in the following way. The franchisor, Reitan, 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.

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 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 A1 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 Reitan 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.[6] 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 Reitan 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.[7]

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 analyzed below. 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 large fraction of the firm-year observations in our sample concern fiscal periods starting in the middle of a calendar year. We deal with this by interpolating the

[6] Panel A in Table A1 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 Reitan 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.

[7] 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 Reitan, 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.

financial statements so that their fiscal periods align with calendar years.[8] 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. In order to avoid detrimental effects from outlier observations, all firm-specific variables are winsorized with respect to the 1^{st} and the 99^{th} percentiles.

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 2008Q1–2013Q1, 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 liquidity assets, $Cash/Assets$, the amount of drawn trade credit from suppliers, $Payables/Assets$, and the amount of issued trade credit to customers, $Receivables/Sales$.[9] As noted in the Introduction 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-

[8] The data set, or similar versions of it, has been used extensively in previous research, c.f. Jacobson, Lindé, and Roszbach (2013), Giordani, Jacobson, Villani, and von Schedvin (2014) and Jacobson and von Schedvin (2015) for more details on the data. In particular, Giordani et al. provides an account of how the financial statements have been transformed to conform to standardized calendar year fiscal periods. 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 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.

[9] 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.

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. Through clustering of standard errors at the firm-level we account for the potential multiplicity of control firms, ensuring correct inference regarding the statistical significance of estimated treatment effects.

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, 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 favor 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 straight-forward to evaluate. We follow the practice in Imbens (2015) of normalized differences in covariates by treatment status; but also present *t*-statistics for differences in means.

We proceed to an examination of 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.

Furthermore, 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.

Finally, we 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 the bottom and top quartiles. We then estimate and compare coefficients across the two samples, to assess the role played by credit constraints.

3 Results

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, an underlying mechanism; 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), where weights are given by the fraction of treated firms in each particular 5-digit industry. To assess magnitudes in 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 and normalized differences in Panels B and C.[10] When comparing means for treated and non-treated firms

[10] Following Abadie and Imbens (2011), we calculate the normalized differences as $(\bar{X}_1 - \bar{X}_0)/\sqrt{(S_1^2 + S_0^2)/2}$, where $\bar{X}_i$ and S_i^2 are the mean and the standard deviation of a matching variable X, and subscript i refers to either of the compared groups.

in Panels A and B, the *t*-tests indicate that the treated and non-treated firms exhibit similar average cash flows and ages, whereas differences in means for the remaining variables are significantly larger than zero, suggesting that, e.g., the treated firms on average are larger, generate higher sales growth, hold less cash, and receive more and issue less trade credit. However, Abadie and Imbens (2011) suggests that the scale-free normalized difference measure of the difference in a matching variable's distribution location across groups is helpful for assessing the severity of the problem of accounting for a difference in that covariate.[11] A comparison of treated versus non-treated firms for the set of matching variables using normalized differences reverses the previous picture. Only three variables—the debt level, cash holdings, and accounts payable—yield normalized differences in absolute values of 0.356, or larger; remaining variables display modest deviations between the groups. Hence, the combined picture provided by the *t*-tests and normalized differences indicates that there are some, but not huge, statistical differences between the treated firm sample and our industry-weighted representation of non-treated firms. The presence of deviations points towards the need for matching to obtain credible counterfactuals; the fact that deviations are nonetheless mostly modest, suggests 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 only statistically significant deviations for receivables in 2008 and 2009, indicating that the matched control firms issued more trade credit as compared with the treated firms in those years. Moreover, the absolute values of the normalized differences are below a very modest 0.074 for all matching variables, except for accounts receivable, which exhibits absolute values of 0.170 and 0.142, in 2009 and in 2008, respectively, which is still fairly modest. These results indicate that the matching procedure is achieving its objective of matching treated firms to otherwise similar control firms. We will, however, pay attention to the observed differences in receivables, and apply a set of robustness tests to make sure that they do not affect the results at large.

Furthermore, Figure 2 shows differences in means between treated and control firms for the three key outcome variables in each year during: the pre-treatment period (2007–2009); the treatment period

[11] Imbens (2015) notes that a *t*-test statistic may be significant in a large sample, albeit a substantively small difference in means between two groups; whereas a large normalized difference suggest that the average covariate value in the two groups are genuinely different, with implications for the robustness of, e.g., a regression approach to estimate average treatment effects. In an empirical example Imbens (2015) characterize normalized differences below 0.3 in absolute value as modest, and we will also apply that threshold.

(2010–2012); and the post-treatment period (2013). The confidence bands in the figure shows that average cash holdings coincided for treated and control firms in the pre-treatment period. However, in the treatment period we see a downward shift for the difference in average cash holdings, i.e., average cash for treated firms is significantly smaller than average cash for controls. A similar-sized treatment shift, but in the other direction, is observed for accounts payable; differences in averages are insignificant in the pre-treatment period, followed by an upward shift during the treatment period. Consistent with the descriptive statistics in Table 2, the figure further shows that treated firms held significantly less accounts receivable on average in all periods. Nevertheless, the figure displays a distinct downward trend in receivables during the treatment period, suggesting that treated firms altered their receivables more than control firms did. Thus, Figure 2 provides initial evidence that treated firms used their cash holdings and trade credit margins to overcome the Panaxia liquidity shortfalls. Moreover, one potential concern is that cash holdings for treated firms may display a downward trend in the pre-treatment period, and thereby compromise identification. We investigate this formally below, and verify that treated and control firms display trends for the outcome variables in the pre-treatment period that are not significantly different.[12]

[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 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. Columns (V) to (VIII) report 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 ones reported in Columns (I) to (IV), suggesting that the latter are not confounded by differences in characteristics between the treated and control firms. In addition, to gauge the plausibility of the unconfoundedness assumption, we test for differences in trends across treated and control firms in the pre-treatment period

[12] As a complement to the differences in means depicted in Figure 2, we present normalized trends of the three outcome variables for the treated and control firms over the period 2007–2013 in Figure A2. The figure shows that the three outcome variables exhibit parallel-trends in the pre-treatment period and substantial divergences in trends in treatment and post-treatment years.

2007–2009. Column (IX) shows test results indicating parallel cash holding trends, and thus, in support of unconfoundedness.[13]

[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 effects, T_t, of 2.8 percentage points in 2012 and 2.8 percentage points in the post-treatment year. Columns (V) to (VIII) show that the results persist for the bias-corrected estimates and Column (IX) indicates that treated and control firms have 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 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. The bias-corrected estimators reported in Columns (V) to (VIII) show that treatment effects for receivables are somewhat enhanced, once we account for the slight differences in the amount of issued trade credit between treated and controls in the pre-treatment period, cf. Table 2. Finally, the similarity in pre-trends, documented in Column (IX), is in support of the underlying unconfoundedness assumption.

The point estimates of the cumulative adjustments in 2012, T_{2012}, suggest that the magnitude of upstream adjustments is larger than that of downstream adjustments. 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 close to the estimate for sales-scaled receivables of -0.010 (-3.4). A statistical test for the difference in absolute adjustment between payables-to-assets and receivables-to-assets, show that adjustments in payables indeed dominate receivables, with a *p*-value of 0.068. 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

[13] 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}.

size of adjustments in cash holdings. The estimated cumulative adjustment (*t*-value) in net trade credit in 2012 is 0.038 (3.3). Testing for the difference in absolute adjustment between cash and net trade credit yields a *p*-value of 0.285, indicating that the adjustments at the two trade credit margins are jointly of a similar magnitude as the adjustments in cash holdings.[14]

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 from customers. We will shortly study these underlying adjustment mechanisms closer—both upstream and downstream—but first challenge our baseline results in a number of directions.

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 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 A3, 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 to deal with liquidity shortfalls. We propose two potential explanations for this. Firstly, the firms under consideration may on average have 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 evaluate these explanations more in detail below, when we explore sources of cross-sectional

[14] We can further compare the average loss of 4.3 percent, cf. Table 2, to 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 95-percent confidence bands spanning between 0.032 and 0.091. The compounded adjustments are thus of a similar magnitude as the liquidity losses.

heterogeneity.

Next we will examine the extent to which our baseline results are influenced by using a matching procedure. This is done by estimating cumulative adjustments using all non-treated firms instead of the matched control firms. As in Table 2, weighted means for the non-treated firms are calculated using the fraction of treated firms in each 5-digit industry as weights. Table A4 reports results where adjustments for treated firms are related to adjustments for all non-treated firms. Columns (III) to (VI), show that the effects for all outcome variables are statistically significant in 2012. They have the same signs, but are slightly smaller, as compared with the baseline estimates in Table 3. However, yearly adjustments in the pre-treatment period, reported in Columns (I) and (II), show deviations in pre-treatment trends between treated and weighted non-treated firms, which highlights the importance of our matching approach for a causal interpretation.

To further validate our baseline results, we apply a set of robustness tests reported in Table A5. Firstly, Panel A provides complementary results to the bias-adjusted estimates dealing with differences in receivables between treated and control firms in the pre-treatment period, cf. Table 2. Following Crump, Hotz, Imbens, and Mitnik (2008), we 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 eliminating all statistically significant differences in the matching variables between the treated and control firms. Consequently, for the restricted sample, there are no statistically significant differences in means between the treated and control firms in accounts receivable; and corresponding absolute values of the normalized differences for accounts receivable are reduced to 0.048 and 0.007, in 2009 and 2008, respectively. The estimated treatment effects obtained in the restricted sample largely confirm the baseline results, and lead us to conclude that pre-treatment differences in matching variables in the full sample are not a concern.

Secondly, following Petersen and Rajan (1997) and Cuñat (2007), in our baseline results accounts payable are scaled by firms' total assets. However, 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 report cost of sold goods in their financial statements, which reduces our estimation sample to 109 treated firms when including pairs of treated and matched control firms where both parties convey this information in the treatment period. Nevertheless, in Panel B, we note a positive and significant cumulative adjustment effect in 2012 for payables scaled by cost of goods sold, which is consistent with our baseline results.

Thirdly, we evaluate whether our choice to winsorize the variables matter, by instead consider a truncation at the 1^{st} and 99^{th} percentiles. Panel C shows that obtained estimates on the truncated data are very similar to the baseline results. Fourthly, 234 of the treated firms are Reitan franchisees. To gauge the extent to which the franchisees influence the baseline results, we re-estimate our models omitting the Reitan franchisees. Panel D reports results showing that the estimated effects are slightly smaller, but largely in line with the baseline results. Fifthly, Panel E 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 are startups in 2010 and 2011, cf. Table A1, which implies that a large share of the them have missing accounting information for parts of the 2008–2013 period. However, when including the pharmacies for which we have adequate information, we obtain estimated effects that are very similar to the baseline results. Finally, Panel F 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. One explanation for the stronger results is that the treated firms eliminated in the balanced panel are the more distressed. Hence, these results indicate that our baseline estimates of payables-adjustments are, if anything, conservative.

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 for 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 Underlying mechanisms

So far, we have demonstrated that liquidity shortfalls cause the observed adjustments in treated firms' trade credit positions. We will now explore our key presumption for the nature of firms' behavior underlying these adjustments: namely, that reserve liquidity is extracted upstream by postponement of trade credit payments to suppliers, and by enforcing repayment of outstanding debt to customers downstream. Whereas these two types of actions may well be privately conducted between trade credit parties, they 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 dishonoring 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.

The EA data are somewhat restricted in that we can only observe applications faced by treated and control firms—as customers—in the full period 2008Q1–2013Q4. For this period we can disaggregate the application data in two dimensions. Firstly, we observe applications that led to settlement immediately after the firms received notification from the EA, and denote these as *Applications*. Secondly, we also observe applications for which the customers did not settle the debt after the notification, and denote these as *Defaults*. However, for the shorter sample period 2010Q1–2013Q1, the application 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 the various outcomes underlying *Applications*. That is, *Applications* are associated with the following three outcomes: (i) the supplier and customer can bilaterally reach an agreement, which usually results in a withdrawal of the application from the EA, denoted as *Withdrawals*; (ii) the customer can also settle the claim by paying the EA, denoted as *Payments to EA*; and (iii) the customer can contest the claim, which happens if there is a disagreement between the two parties, denoted as *Contested claims*.

In the analyses below, our main focus is on *Applications* and its three components. The reason for this is that increases in *Applications* can be interpreted as a manifestation of a firm´s decision to strategically prolong payment periods, when looking upstream. Likewise, when looking downstream, *Applications* can be seen as outcomes of firms' decisions to strategically increase management of late payments. *Defaults*, on the other hand, are more extreme outcomes. For instance, when examining downstream adjustments, we do not necessarily expect the customers of the treated firms to default considerably more than the customers of the control firms do, i.e., the relative quality should be unchanged. Instead, we expect to see increases in *Applications* which would be consistent with that the treated firms more actively try to manage late customer payments.

It is important to note that the results obtained for the shorter sample period should be interpreted with some caution due to the following circumstances. The lack of data coverage for the pre-treatment period, implies that we cannot confirm that the pre-event trends are parallel. Moreover, when using the first quarter of 2010 as the pre-event period, estimates may be affected due to, e.g., seasonal variation in the outcome variables under consideration. Whereas the former is hard to overcome given data restrictions, we will conduct robustness tests to address the latter concern.

3.3.1 Upstream adjustments

As a starting point, we consider differences in upstream adjustments through applications for injunction to settle overdue payments faced by treated and control firms. Figure 3 offers a graphical illustration of the extensive (Panel A) and intensive (Panel B) margins. Panel A suggests that in the period between 2010Q3 and 2012Q1, the fraction of firms that delayed trade credit payments increased amongst treated relative to control firms. Trends in the number of applications, reported in Panel B, show an even more pronounced divergence between treated and control firms, starting after 2010Q3 and persisting through 2013Q2. These patterns are consistent with our baseline results showing upward adjustments in payables during 2011 and 2012, cf. Table 3.

[Insert Figure 3 about here.]

Table 4 reports cumulative effects, T_t, on the following set of outcome variables: *Applications*; *Withdrawals*; *Payments to EA*; *Contested claims*; and *Defaults*. We take 2009 as the pre-treatment period for the variables for which we have data over the longer period of 2008Q1–2013Q4; and use 2010Q1 as the pre-treatment period for the variables for which we have data for the shorter period of 2010Q1–2013Q1. Row (1) reports results for a dummy variable indicating whether, or not, the firms faced *Applications*. The estimated effect for 2011 shows that a significantly larger fraction of treated firms were facing *Applications* in that year. In Row (2) we consider the number of *Applications*. The results indicate a significant difference between treated and control firms in 2012, such that the treated firms faced a larger number of applications. Moreover, in Row (3) we re-estimate the model reported in Row (2), but instead of using 2009 as the pre-treatment period, we take 2010Q1 to be our pre-treatment period. The purpose is to examine how sensitive our results are to the change in pre-treatment period. As can be seen in Row (3), the estimated effects are significant in 2011, 2012, and 2013, and somewhat enhanced in comparison with corresponding estimates in Row (2). Hence, although some caution is warranted when interpreting the size of effects obtained from models estimated on the shorter sample period, we nevertheless deem estimates based on the shorter period as being informative about which of the three outcomes—*Withdrawals*, *Payments to EA*, and *Contested claims*—that underlie and explain the observed increases in *Applications*.

[Insert Table 4 about here.]

Rows (4) to (6) report results for the three outcomes of *Applications*. The main picture emerging is that that increases in *Applications* primarily are due to *Withdrawals,* whereas no significant effects are obtained for *Payments to EA*, nor for *Contested claims*. These result suggest that the treated firms to a larger extent than the control firms strategically postponed their trade credit payments, but once the EA becomes involved an agreement is reached. *Withdrawals* dominating *Payments to EA*—direct payment to suppliers, rather than via the EA—can be interpreted as firms trying to maintain an ongoing relationship with their suppliers, albeit the instance of late payment. Hence, despite the involvement of EA, cooperative outcomes appear to prevail. Finally, Row (7) reports results for the number of *Defaults*. The coefficients show a positive and significant effect in 2010. However, the test for parallel pre-trends is significant, which indicates divergences between treated and control firms in 2009, and hence these treatment effect results are ambiguous.

Following recent work by Boissay and Gropp (2013) and Jacobson and von Schedvin (2015), showing that trade credit networks facilitate shock propagation, we attempt to widen the perspective and consider whether firms further up the supply chain were affected by the Panaxia event. In the data, we observe 487 corporate suppliers that submitted an application to the EA—including both *Applications* and *Defaults*—to enforce late payments from treated firms in the 2010Q1–2013Q1 period; and correspondingly we observe 331 corporate suppliers submitting applications with respect to control firms. For the 2010Q1–2013Q4 period, we find that 66 percent of the suppliers of treated firms and 63 percent of the suppliers of control firms in turn postponed one, or more, payments to suppliers at the next level. Hence, suppliers of treated and control firms that face a late payment are equally likely to, in turn, postpone payments to their suppliers. Nevertheless, since a larger number of suppliers of treated firms were subject to late customer payments, we see a significantly larger number of postponed payments emanate from these suppliers. On average for the 2010Q1–2013Q4 period, we observe that 111 suppliers of treated firms postponing supplier payments in each quarter, in contrast with 82 suppliers of control firms postponing supplier payments, where the difference is statistically significant. Our interpretation of these results is that firms in general may routinely be subject to liquidity shocks that will result in postponed settlement of trade credit debt, as reflected by the applications to the EA from 331 suppliers of the control firms. The treated firms are also subject to such general liquidity shocks, but these firms were also—over and above—affected by the Panaxia liquidity shortfalls, and the combined effect of general and specific liquidity shocks resulted in a larger number of delayed payments from the suppliers of treated firms to their suppliers. Hence, these results suggest that the Panaxia event triggered a contagion spread effect in the trade credit network, which had implications for suppliers further up the supply chain.[15]

3.3.2 Downstream adjustments

We now turn the evaluation to downstream adjustments by considering *Applications* submitted by treated and control firms. Figure 4 shows the fraction of treated and control firms that submitted applications in each quarter (Panel A) and the average number of applications (Panel B). Over the event period, the fractions of treated firms that in each quarter acted to enforce payment of overdue debt increased more than the fractions of control firms. Likewise, when considering the number of *Applications*, we find that in the treatment period, treated firms consistently increased the number of applications more than the control firms did, with the exception of one quarter, 2011Q1.

[Insert Figure 4 about here.]

Table 5 reports cumulative treatment effects, T_t, for submitted *Applications*, *Withdrawals*, *Payments to EA*, *Contested claims*, and *Defaults*. The results presented in Panel A are from models based 2010Q1 as the pre-treatment period, whereas in Panel B all of 2010 has been assigned to pre-treatment. Row (1) reports treatment effects for a variable indicating whether, or not, firms submitted *Applications* to enforce payments. The coefficient in 2010 is positive and statistically significant indicating an increase in fraction of the treated firms that used EA to enforce late trade credit payments. Row (2) show results for the number of *Applications*. The coefficient for 2012 is positive and statistically significant indicating that the treated firms on average increased the number of applications to enforce payments in that year.

[15] In the quarter of the Panaxia failure and in the three quarters following, 2012Q3–2013Q2, we observe 12 bankruptcies among the 487 suppliers of treated firms and 5 bankruptcies among the 331 suppliers of control firms. In accordance with Jacobson and von Schedvin (2015), the elevated number of bankruptcies for the suppliers of treated firms indicates that liquidity extraction along the trade credit chains may result in ex post outcomes that could be detrimental for upstream counterparties.

This result is consistent with our baseline results, cf. Table 3, showing a large receivables contraction in 2012.

[Insert Figure 5 about here.]

Furthermore, Rows (3) to (5) show that the observed effect in the number of *Applications* primarily can be attributed to *Withdrawals*, whereas no significant effects are due to *Payments to EA*, nor to *Contested claims*. This result is consistent with the observed outcomes for upstream adjustments, by suggesting that the applications result in cooperative solutions between the firms and their customers. Moreover, the coefficients for the number of *Defaults*, reported in Row (6), are insignificant. This result suggests that differences in the quality of the treated and control customers remain unchanged over the event period. Hence, the treated firms' documented increase in attempting enforcement of late payments does not appear to be due to a deterioration in the quality of their customers, but rather to an effort by the treated firms to reduce late payments. Finally, the results in Rows (7) to (12) show that the results persist when we use 2010 as the pre-event period, instead of 2010Q1. Hence, mitigating the concern that seasonal variations, attributed to the first quarter, in the outcome variables drive the results reported in Panel A.

In sum, the upstream and downstream analyses of the mechanisms underlying the documented adjustments in payables and receivables suggest that adjustments are indeed associated with shifts in trade credit maturity. In coherence with a risk sharing perspective, the dominance of withdrawals of applications indicate the cooperative nature of outcomes, such that suppliers and customers provide requisite liquidity according to implicit sharing rules. However, based on these results it is difficult to conclude how much of the adjustments in payables and receivables that are due to maturity adjustments in the underlying trade credit contracts. Adjustments along other contract dimensions—such as the instance of direct payments and the pricing of the contracts—are also conceivable. It is nevertheless important to note that our measure of overdue credit only capture rather long payment delays, and it is likely that most delays do not result in an application to the EA, which suggest that we do not fully capture the role played by postponed payments.

3.4 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 6 reports results for the two groups; Columns (I) to (V) correspond to the group of treated firms that were fully compensated in 2012 and Columns (VI) to (X) correspond to 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 for the two trade credit margins in 2012. Thus, the results in Table 6 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 existence of failure losses caused the adjustments to persist, or even increase, in the last treatment year and in the post-treatment year.

[Insert Table 6 about here.]

We can further deepen the analysis by evaluating the intensive margins according to which the size of the adjustments depends on size of incurred losses, i.e., the second case mentioned above. To this end we estimate cumulative adjustment effects between 2009 and 2012, in models conditional on the size of the loss. Also, to account for non-linarites, we report average marginal effects from polynomial extensions of the models, where quadratic- and cubic-terms of the loss-variable are included.[16] Columns (I) and (II)

[16] For the two years 2009 and 2012, we estimate models on the form: $E[y_{i,t}] = \beta_0 + \beta_1 X_i \times I_t + \beta_2 X_i + \beta_3 I_t + \boldsymbol{\beta}_4 \mathbf{Z}_i$; where X_i is the size of the claim over assets in 2012, $Loss/Assets_i$; I_t is an indicator variable set to unity in 2012, and zero in 2009; and $\mathbf{Z}_i$ is a vector of 2009-outcomes of the continues matching variables. A quadratic polynomial expansion is obtained when replacing X_i by $(X_i + X_i^2)$, and a cubic polynomial when replacing X_i by $(X_i + X_i^2 + X_i^3)$.

in Table 7 report insignificant treatment effects from the loss-variable on cash holdings for the linear and quadratic models. However, when accounting for higher order non-linarites by applying a cubic polynomial extension, cf. Column (III), we find a negative and statistically significant effect on cash holdings. Columns (IV) to (IX) report corresponding results for accounts payable and receivable. The overall insight provided by these estimates is that larger losses are associated with significantly larger increases in payables, as well as decreases in receivables; consistent across all model specifications, including the linear one. Thus, our results indicate that trade credit margins indeed played important roles in absorbing the impacts of the incurred losses, and the larger the loss, the larger are resulting adjustments. The average marginal effects estimated in polynomial extensions of the model are substantially larger, suggesting the presence of non-linear relationships between loss-size and resulting adjustments.[17]

[Insert Table 7 about here.]

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 larger 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.

3.5 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.[18] More specifically, for each split-variable, we sort the firms into an empirical distribution based on their 2009-outcomes of the split-variable and then construct two sub-samples related to the 1^{st} and the 3^{rd} quartiles of the distribution.

[17] 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.

[18] 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.

Panel A in Table 8 shows results when splitting the sample with respect to the size of treated firms. The first result emerging is that the negative effects for cash holdings can be attributed to small firms, whereas no significant effects are observed for large firms, whose point estimates are close to zero. The reported *p*-values indicate that treatment effects are significantly different for the two samples. Results for accounts payable are similar, small firms increase their payables in 2012, whereas no adjustments are observed for large firms. However, the estimated effects are not significantly different across the two samples. For accounts receivable, we note significant treatment effects for both groups, although the point estimates, in most years, are larger for the small firms as compared with the large ones. Again, we find that the effects are not statistically different in any of the years. Finally, the last outcome variable considered is short-term bank debt over assets, and in 2012 the treatment effect for large firms is positive and significantly larger than zero—it is also significantly larger than corresponding small firm estimate that year. Small firms do not adjust their short-term bank debts significantly in any year.

[Insert Table 8 about here.]

Panel B shows results for sample-splits based on firms' credit ratings. The 1^{st} Quartile sub-sample corresponds to low-risk firms and the 3^{rd} Quartile corresponds to high-risk firms. The results for the rating variable coincide closely with to the ones reported for firm size in the sense that adjustments in cash holdings, payables, and receivables only are observed for high-risk firms, whereas low-risk firms primarily tend to adjust the amount of short-term bank debt. However, the reported *p*-values indicate that most of the effects are not statistically different across the two samples, except for 2013, when there are statistically significant differences in payables and receivables.

In sum, the documented effects suggest that financially unconstrained firms may access external financing to handle liquidity shocks. Financially constrained firms, on the other hand, primarily rely on adjustment capacity in cash-holdings and the two trade credit margins; and in particular by drawing liquidity from their trade credit margins in adverse situations enable them to complement the liquidity buffer in their cash reserves. 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. Our results are thus consistent with the interpretation that 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.

4 Conclusions

Resent 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 impact of liquidity shortfalls that the fraud and failure of a large Swedish cash-in-transit firm imposed on its clients. This unique event provides an opportunity to derive causal inference on the roles played by cash holdings, and trade credit margins to handle liquidity shortfalls.

Our contribution can be summarized by the following main findings. Firstly, firms handle adverse liquidity shortfalls by drawing down on their cash holdings, by increasing the amount of drawn credit from suppliers (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, where 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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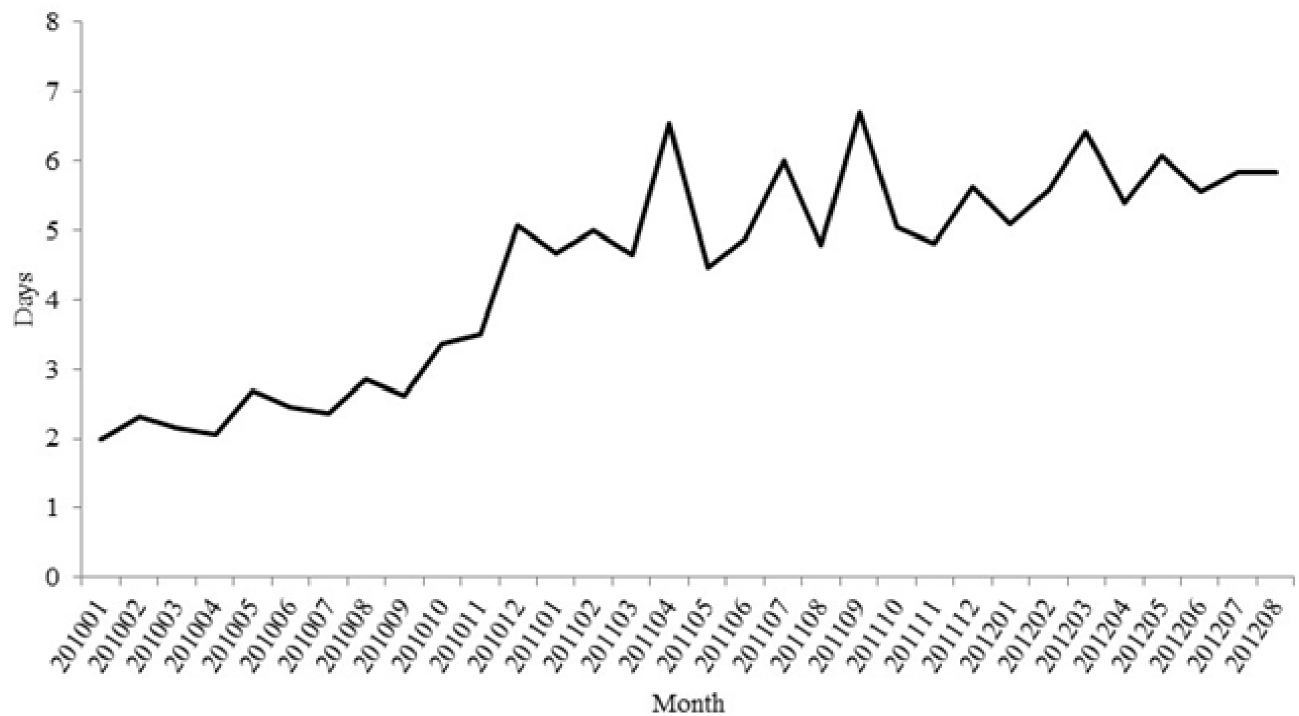
Figure 1

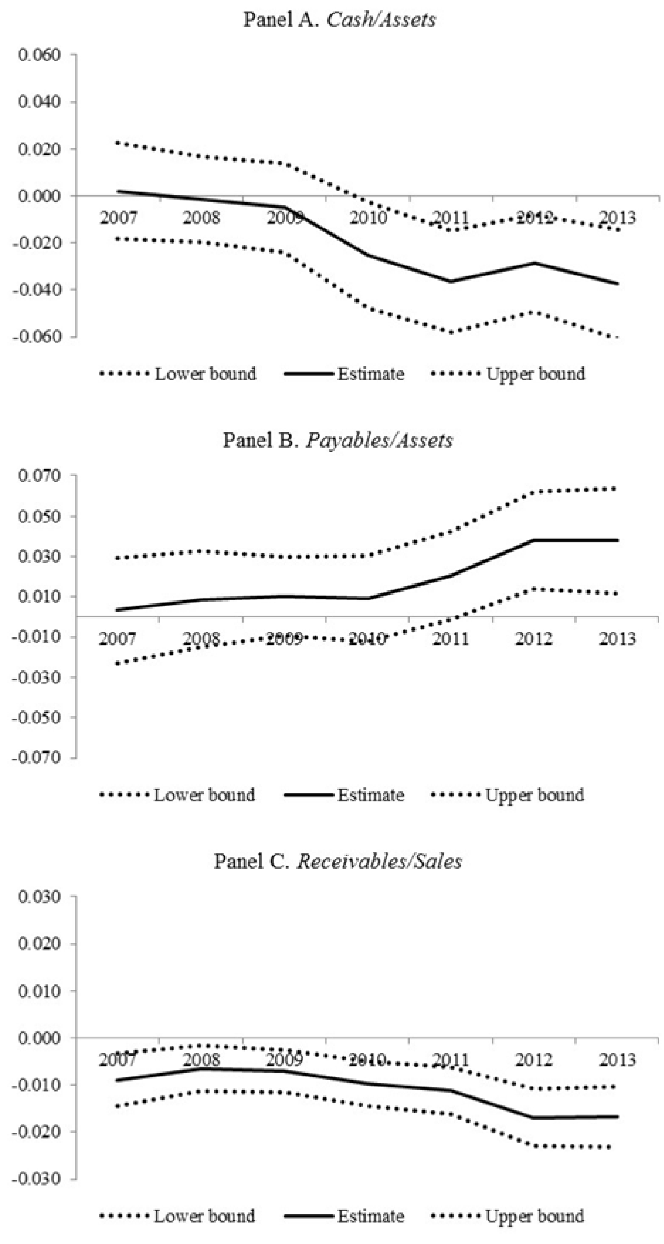
PANAXIA; TIME FROM COLLECTION TO TRANSFER



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

Figure 2

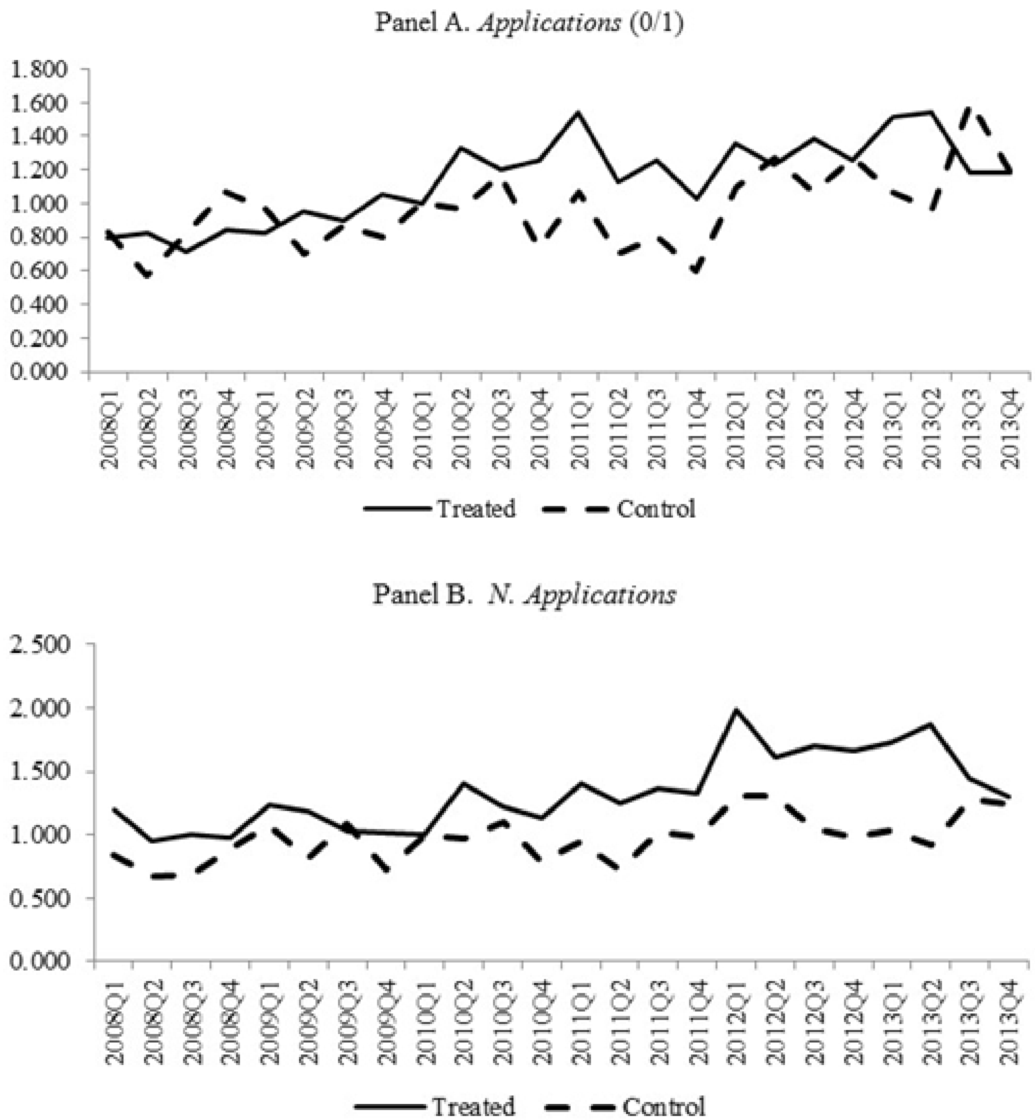
DIFFERENCES IN MEANS DURING THE PRE-TREATMENT, TREATMENT, AND POST-TREATMENT PERIODS



This figure shows yearly differences in means between the treated and matched control firms, over the period 2007–2013, for cash holdings, accounts payable, and accounts receivable. The period comprises the pre-treatment period, 2007–2009, the treatment period, 2010–2012, and the post-treatment period, 2013. For each year, only pairs for which there is data on both the treated and control firms are included. The solid lines mark the differences in means between the two groups and the dotted lines indicate 90-percent confidence bands.

Figure 3

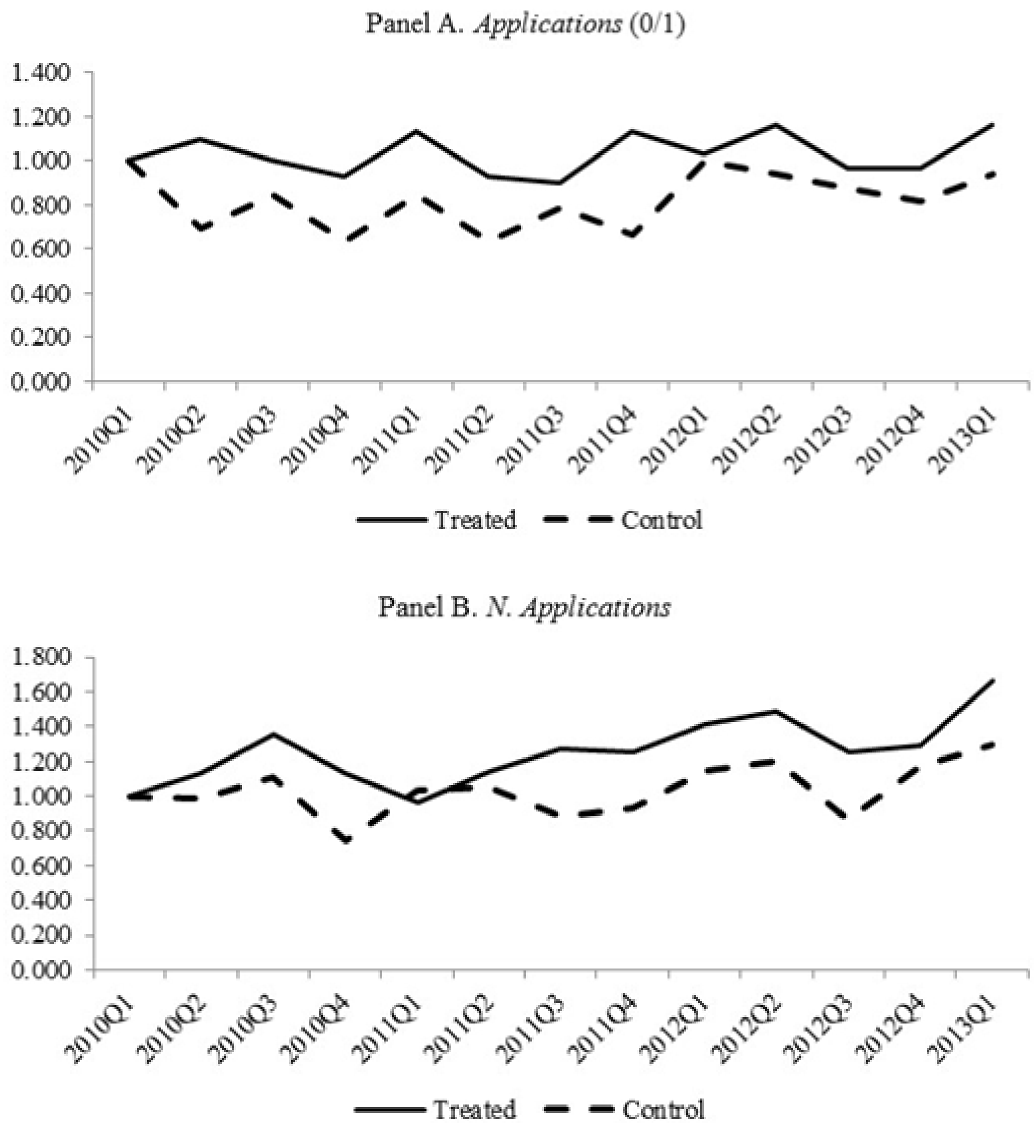
UPSTREAM PERSPECTIVE; LATE PAYMENTS



This figure shows applications for injunction to settle overdue payments faced by firms, where we have normalized by 2010Q1-outcomes throughout. Panel A shows the fractions of treated and control firms, that in a given quarter were subject to one, or several applications for injunctions to settle a late trade credit payment, and Panel B shows the average number of applications facing firms, in the period 2008Q1–2013Q4.

Figure 4

DOWNSTREAM PERSPECTIVE; ENFORCMENT OF LATE PAYMENTS



This figure shows applications for injunction to settle overdue payments issued by firms, where we have normalized by 2010Q1-outcomes throughout. Panel A shows the fractions of treated and control firms that in a given quarter applied for an injunction to enforce late payments from their customers, and Panel B shows the average number of submitted applications, in the period 2010Q1–2013Q1.

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 performance and external financing, obtained from Panaxia AB's 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			
Variables	Mean	SD	Mean	SD	t-test (p-value)	Normalized difference	Mean	SD	t-test (p-value)	Normalized difference
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	(0.488)	−0.027	0.087	0.141	(0.591)	−0.033
$Assets_{2009}$ (In M SEK)	240.307	3,087.629	28.608	1018.081	(0.090)	0.092	76.759	413.432	(0.195)	0.074
$Sales\ growth_{2009}$	0.047	0.297	0.017	0.352	(0.020)	0.093	0.027	0.269	(0.232)	0.071
$Debt/Assets_{2009}$	0.168	0.247	0.230	0.270	(0.000)	−0.239	0.175	0.235	(0.662)	−0.029
$Tangible\ assets/Assets_{2009}$	0.200	0.234	0.302	0.279	(0.000)	−0.397	0.216	0.241	(0.327)	−0.069
$Inventories/Assets_{2009}$	0.276	0.203	0.248	0.244	(0.001)	0.127	0.278	0.206	(0.883)	−0.009
Age_{2009}	14.887	16.796	15.971	13.566	(0.123)	−0.071	14.093	14.992	(0.455)	0.050
$Cash/Assets_{2009}$	0.179	0.173	0.251	0.229	(0.000)	−0.356	0.184	0.183	(0.660)	−0.028
$Payables/Assets_{2009}$	0.242	0.158	0.162	0.150	(0.000)	0.518	0.232	0.155	(0.385)	0.065
$Receivables/Sales_{2009}$	0.021	0.041	0.033	0.073	(0.000)	−0.206	0.028	0.042	(0.010)	−0.170
$Cash/Assets_{2008}$	0.179	0.170	0.246	0.226	(0.000)	−0.331	0.181	0.181	(0.907)	−0.007
$Payable/Assetss_{2008}$	0.273	0.191	0.172	0.157	(0.000)	0.576	0.264	0.184	(0.551)	0.046
$Receivables/Sales_{2008}$	0.022	0.046	0.033	0.070	(0.000)	−0.178	0.029	0.045	(0.028)	−0.142
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 of differences in means and normalized differences are calculated according to Abadie and Imbens (2011). The tests of differences in means and normalized differences 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 A1.

Table 3

BASELINE ESTIMATES; AVERAGE TREATMENT EFFECTS FOR TREATED FIRMS

	Average treatment effects for treated firms (ATT)				ATT with bias adjustment				
	Treatment period			Post-treatment period	Treatment period			Post-treatment period	Test of parallel pre-trends
	(I)	(II)	(III)	(IV)	(I)	(II)	(III)	(IV)	(IX)
Variables	2010	2011	2012	2013	2010	2011	2012	2013	(*p*-value)
Panel A. $y = Cash/Assets$									
τ_t^y	−0.020**	−0.011**	0.008	−0.009	−0.020***	−0.009	0.004	−0.011	(0.666)
	(−2.4)	(−2.0)	(1.2)	(−0.9)	(−2.8)	(−1.4)	(0.6)	(−1.2)	
T_t^y	−0.020**	−0.031***	−0.024***	−0.032***	−0.020***	−0.029***	−0.025***	−0.035***	
	(−2.4)	(−3.5)	(−2.8)	(−3.0)	(−2.8)	(−3.4)	(−2.7)	(−3.2)	
Panel B. $y = Payables/Assets$									
τ_t^y	−0.001	0.011**	0.018***	−0.000	−0.002	0.012**	0.018***	−0.000	(0.757)
	(−0.2)	(2.1)	(3.0)	(−0.0)	(−0.3)	(2.0)	(3.0)	(−0.0)	
T_t^y	−0.001	0.010	0.028***	0.028**	−0.002	0.010	0.029***	0.029***	
	(−0.2)	(1.3)	(3.1)	(2.5)	(−0.3)	(1.4)	(3.4)	(2.9)	
Panel C. $y = Receivables/Sales$									
τ_t^y	−0.003**	−0.002	−0.006***	0.000	−0.003**	−0.002	−0.006***	0.000	(0.417)
	(−2.1)	(−1.0)	(−2.6)	(0.1)	(−2.2)	(−1.4)	(−2.6)	(0.1)	
T_t^y	−0.003**	−0.004**	−0.010***	−0.010***	−0.003**	−0.005***	−0.012***	−0.012***	
	(−2.1)	(−2.3)	(−3.4)	(−3.1)	(−2.2)	(−2.9)	(−4.2)	(−3.5)	
N. Treated firms	610				610				-
N. Control firms	610				610				-
N. Unique control firms	482				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 bias-adjusted estimators in Columns (V) to (VIII) control for differences in outcomes, between the treated and matched control firms, in the set of continuous matching-variables (Abadie and Imbens (2011)). 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 A1. The standard errors in Columns (I) to (IV) 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 4

UPSTREAM PERSPECTIVE; AVERAGE TREATMENT EFFECTS FOR TREATED FIRMS

Cumulative effects (T_t)					Post-treatment period	Test of parallel pre-trends	Sample period
		Treatment period					
		(I)	(II)	(III)	(IV)	(IX)	(X)
Row	Dependent variables (y)	2010	2011	2012	2013	(*p*-value)	
(1)	*Applictions* (0/1)	0.011	0.022***	0.007	0.009	(0.374)	2008Q1−2013Q4
		(1.2)	(2.6)	(0.7)	(0.7)		
(2)	*N. Applictions*	0.005	0.028	0.056*	0.041	(0.762)	2008Q1-2013Q4
		(0.3)	(1.3)	(1.9)	(1.3)		
(3)	*N. Applictions*	0.041	0.064*	0.084**	0.097**	-	2010Q1−2013Q1
		(1.4)	(1.9)	(2.2)	(2.3)		
(4)	*N. Withdrawals*	0.048*	0.056**	0.070**	0.082**	-	2010Q1−2013Q1
		(1.9)	(2.0)	(2.2)	(2.2)		
(5)	*N. Payments to EA*	−0.010	0.002	−0.005	−0.015	-	2010Q1−2013Q1
		(−1.0)	(0.2)	(−0.4)	(−0.8)		
(6)	*N. Contested claims*	0.004	0.005	0.018	0.030	-	2010Q1−2013Q1
		(0.4)	(0.4)	(1.2)	(1.4)		
(7)	*N. Defaults*	0.007**	0.003	−0.009	0.002	(0.065)	2008Q1−2013Q4
		(2.0)	(0.5)	(−0.8)	(0.2)		
N. Treat./Cont./Un. Cont.			610/610/482				-

This table reports estimates of cumulative adjustments, Eq. (2). Results are reported for a variable indicating whether, or not, the firm faced applications (Row (1)); the number of applications faced by the firm (Rows (2) and (3)); the number of applications that were subject to a withdrawal (Row (4)); the number of applications that resulted in a payment to EA (Row (5)); the number of applications that were contested (Row (6)); and the number of applications that resulted in a default (Row (7)). The models reported in Rows (1), (2), and (7) use 2009 as the pre-treatment period and the models in Rows (3) to (6) use 2010Q1 as the pre-treatment period. The tests of parallel pre-trends are conducted using the 2008Q1–2009Q4 period, and follow the approach proposed by Mora and Reggio (2015). Variable definitions are provided in Table A1. 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 5

DOWNSTREAM PERSPECTIVE; AVERAGE TREATMENT EFFECTS FOR TREATED FIRMS

Cumulative effects (T_t)		Treatment period			Post-treatment period
		(I)	(II)	(III)	(IV)
Row	Dependent variables (y)	2010	2011	2012	2013Q1
Panel A. 2010Q1 as pre-treatments period					
(1)	*Applictions* (0/1)	0.015*	0.016	0.007	0.011
		(1.7)	(1.6)	(0.6)	(1.0)
(2)	*N. Applictions*	0.348	0.259	0.565*	1.015
		(1.0)	(1.3)	(1.8)	(1.4)
(3)	*N. Withdrawals*	0.308	0.128	0.469**	0.649
		(1.1)	(1.5)	(2.0)	(1.5)
(4)	*N. Payments to EA*	−0.001	0.124	0.070	0.307
		(−0.0)	(1.2)	(1.1)	(1.2)
(5)	*N. Contested claims*	0.041	0.007	0.026	0.059
		(0.7)	(0.3)	(0.7)	(0.8)
(6)	*N. Defaults*	0.148	0.369	0.492	0.785
		(0.7)	(1.1)	(1.2)	(1.2)
Panel B. 2010Q1−2010Q4 as pre-treatment period					
(7)	*Applictions* (0/1)	-	0.004	−0.005	−0.000
			(0.7)	(−0.7)	(−0.0)
(8)	*N. Applictions*	-	−0.002	0.304**	0.754
			(−0.0)	(2.1)	(1.5)
(9)	*N. Withdrawals*	-	−0.103	0.238*	0.418
			(−0.8)	(2.0)	(1.6)
(10)	*N. Payments to EA*	-	0.124	0.070	0.307
			(1.4)	(1.6)	(1.3)
(11)	*N. Contested claims*	-	−0.023	−0.005	0.028
			(−0.8)	(−0.2)	(0.8)
(12)	*N. Defaults*	-	0.258	0.381	0.674
			(1.3)	(1.4)	(1.3)
N. Treat./Cont./Un. Cont.			610/610/482		

This table reports estimates of cumulative adjustments, Eq. (2). Results are reported for a variable indicating whether, or not, the firm issued applications (Rows (1) and (7)); the number of applications issued by the firm (Rows (2) and (8)); the number of applications that were subject to a withdrawal (Rows (3) and (9)); the number of applications that resulted in a payment to EA (Rows (4) and (10)); the number of applications that were contested (Rows (5) and (11)); and the number of applications that resulted in a default (Row (6) and (12)). The models in Panel A use 2010Q1 as the pre-treatment period and the models in Panel B use 2010 as the pre-treatment period. Variable definitions are provided in Table A1. 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 6

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*-value)	2010	2011	2012	2013	(*p*-value)
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	(0.727)
	(−0.7)	(−2.4)	(2.0)	(−1.0)		(−2.3)	(−1.0)	(0.5)	(−0.7)	
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)	(−2.9)	(−2.7)	(−2.7)	
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	(0.614)
	(1.5)	(1.2)	(−0.0)	(0.2)		(−0.5)	(1.9)	(3.1)	(−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)	(2.8)	(2.1)	
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	(0.513)
	(−0.4)	(1.5)	(−2.5)	(−0.4)		(−2.2)	(−1.5)	(−1.9)	(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 A1. Standard errors are clustered at the firm-level to account for the multiplicity of matched control firms. ***, **,* denote statistically distinct from 0 at the 1, 5 and 10 percent level, respectively.

Table 7

RESPONSES CONDITIONAL ON LOSS-SIZE

	Dependent variables:								
$y =$	$Cash/Assets$			$Payables/Assets$			$Receivables/Sales$		
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)
Variable	Coef.	dy/dx	dy/dx	Coef.	dy/dx	dy/dx	Coef.	dy/dx	dy/dx
$Loss/Assets_{2012}$	0.008	−0.133	−0.457**	0.227*	0.390***	0.438*	−0.071***	−0.090**	−0.171**
	(0.1)	(−1.1)	(−2.2)	(1.9)	(3.0)	(1.9)	(−2.8)	(−2.3)	(−2.5)
R^2									
Model	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic	Linear	Quadratic	Cubic
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N. Treat./Cont./Un. Cont.	494/494/367								

This table reports coefficients and average marginal effects (dy/dx) for the relationships between the cumulative adjustment over the period 2009 to 2012 and the size of the incurred losses, for cash holdings, accounts payable, and accounts receivable. The models are described in Footnote 13. Variable definitions are provided in Table A1. Standard errors are clustered at the firm-level to account for the multiplicity of matched control firms. ***, **,* denote statistically distinct from 0 at the 1, 5 and 10 percent level, respectively.

Table 8

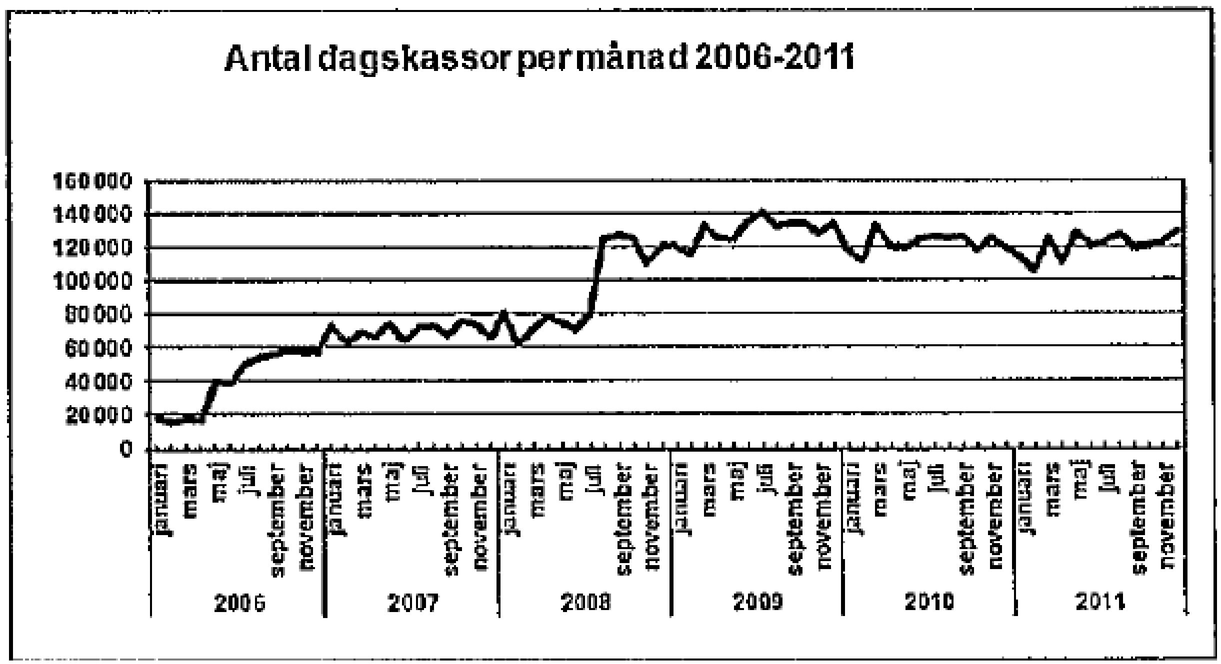
RESPONSES CONDITIONAL ON CREDIT CONSTRAINTS

	Panel A. Sample split by Firm size				Panel B. Sample split by *Rating* (PD)			
	Treatment period			Post-treatment period	Treatment period			Post-treatment period
	(I)	(II)	(III)	(IV)	(I)	(II)	(III)	(IV)
Variables	2010	2011	2012	2013	2010	2011	2012	2013
(1.) $y = Cash/Assets$								
1^{st} Quartile	−0.048**	−0.054***	−0.053***	−0.054***	−0.012	−0.018	−0.019	−0.024
	(−2.4)	(−2.9)	(−3.1)	(−2.9)	(−0.9)	(−1.0)	(−1.0)	(−1.2)
3^{rd} Quartile	0.001	−0.012	−0.012	−0.005	−0.057*	−0.065**	−0.041**	−0.032
	(0.1)	(−0.9)	(−0.8)	(−0.3)	(−1.9)	(−2.5)	(−2.0)	(−1.6)
p-value	0.033	0.071	0.066	0.050	0.145	0.109	0.334	0.694
(2.) $y = Payables/Assets$								
1^{st} Quartile	0.007	0.024	0.047**	0.025	−0.005	−0.000	0.019	0.005
	(0.4)	(1.2)	(2.0)	(0.9)	(−0.6)	(−0.0)	(1.6)	(0.3)
3^{rd} Quartile	0.004	0.006	0.019	0.008	0.006	0.027	0.048*	0.052**
	(0.5)	(0.7)	(1.6)	(0.6)	(0.3)	(1.2)	(1.9)	(2.3)
p-value	0.883	0.407	0.285	0.589	0.678	0.224	0.303	0.060
(3.) $y = Receivables/Sales$								
1^{st} Quartile	−0.002	−0.009***	−0.012*	−0.018**	−0.004	−0.002	−0.004	0.001
	(−1.3)	(−2.7)	(−1.9)	(−2.3)	(−1.5)	(−0.6)	(−1.3)	(0.2)
3^{rd} Quartile	−0.007**	−0.003	−0.007**	−0.004	−0.000	−0.007	−0.019*	−0.021**
	(−2.3)	(−0.9)	(−2.0)	(−0.9)	(−0.2)	(−1.5)	(−1.8)	(−2.2)
p-value	0.130	0.236	0.525	0.144	0.380	0.290	0.159	0.044
(4.) $y = Short\text{-}term\ bank\ debt/Assets$								
1^{st} Quartile	−0.001	−0.006	−0.002	−0.004	0.006**	0.005	0.009**	0.004
	(−0.3)	(−1.6)	(−0.5)	(−1.0)	(2.6)	(1.2)	(2.0)	(1.1)
3^{rd} Quartile	0.001	0.004	0.013*	0.010	0.002	−0.005	0.001	0.001
	(0.1)	(0.5)	(1.8)	(1.3)	(0.3)	(−0.7)	(0.2)	(0.1)
p-value	0.800	0.235	0.064	0.102	0.730	0.352	0.558	0.799
N. Firms	[124/124/89;123/123/115]				[123/123/114; 122/122/97]			

This table reports estimates of cumulative adjustments, Eq. (2), in cash holdings, accounts payable, accounts receivable, and short-term bank debt, over the treatment and post-treatment periods. 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, are reported within parenthesis. p-values refer to tests of difference in coefficients between the 1st and 3rd Quartile sub-samples. Variable definitions are provided in Table A1. ***, **,* denote statistically distinct from 0 at the 1, 5 and 10 percent level, respectively.

Figure A1

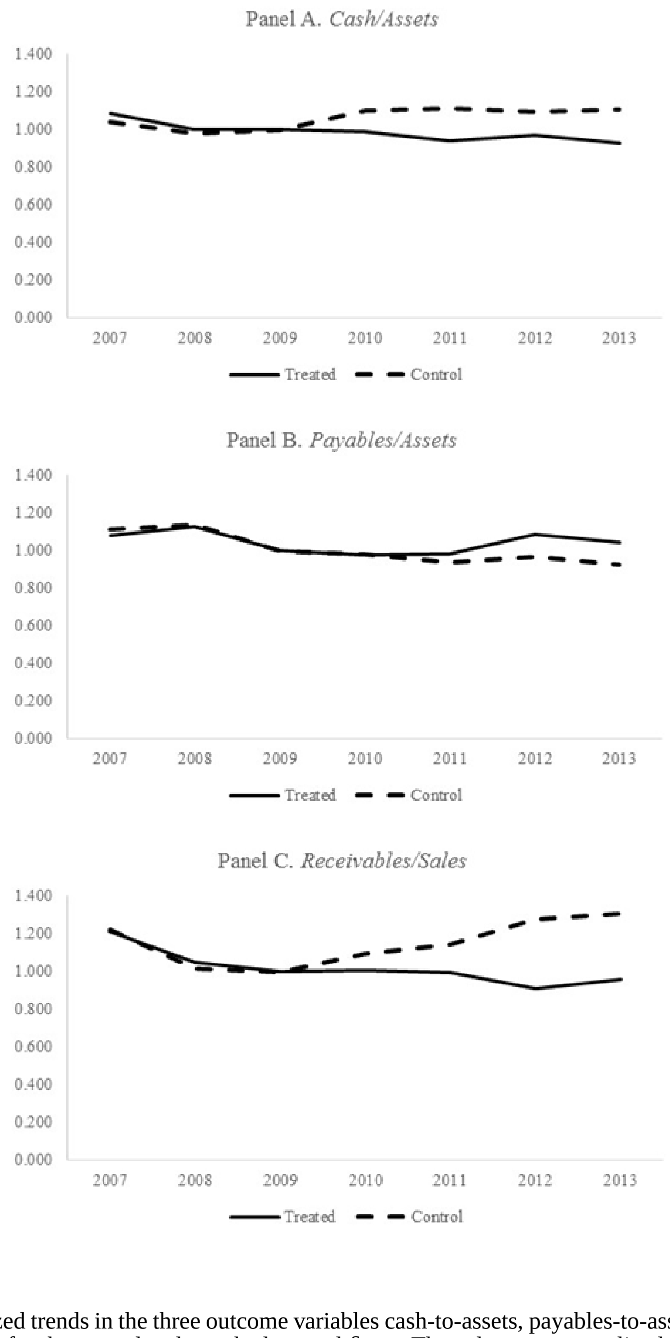
THE NUMBER OF COLLECTED RECEIPTS DURING THE 2006–2011 PERIOD



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

TRENDS IN OUTCOME VARIABLES



This figure reports normalized trends in the three outcome variables cash-to-assets, payables-to-assets, and receivables-to-sales, over the 2007–2013 period, for the treated and matched control firms. The values are normalized with 2009-outcomes.

Table A1

SAMPLE CHARACTERISTICS—NUMBER OF PANAXIA CLIENTS

Panel A. Type of firm	Total	Uncompensated firms (*Item 1*)	Reitan 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					
Reitan (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 (Reitan franchisor excluded) and pharmacies over the 2007–2013 period.

Table A2

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).
*Application*s	Applications for the issuance of injunctions to settlement of outstanding claims (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).
Defaults	Applications that remain unsettled after a fortnight from the time of notification (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 A3

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. $y = Short\text{-}term\ bank\ debt/Assets$					
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. $y = Long\text{-}term\ bank\ debt/Assets$					
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 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 A1. 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 A4

NON-TREATED FIRMS AS CONTROL GROUP

	Pre-treatment period		Treatment period			Post-treatment year included
Groups	2008–2007	2009–2008	2010–2009	2011–2009	2012–2009	2013–2009
Panel A. $y = Cash/Assets$						
Treated	−0.017***	−0.001	−0.002	−0.010**	−0.006	−0.013**
	(−3.8)	(−0.1)	(−0.5)	(−2.3)	(−1.1)	(−2.2)
Non-treated	−0.000	0.005***	0.003	0.004*	0.010***	0.019***
	(−0.1)	(3.0)	(1.5)	(1.6)	(4.0)	(6.2)
Difference	−0.017***	−0.006	−0.004	−0.014***	−0.016***	−0.032***
	(−3.4)	(−1.4)	(−1.1)	(−2.8)	(−2.8)	(−4.9)
Panel B. $y = Payables/Assets$						
Treated	0.011**	−0.031***	−0.005	−0.004	0.021***	0.010*
	(2.4)	(−6.7)	(−1.5)	(−1.0)	(4.5)	(1.9)
Non-treated	−0.001	−0.010***	−0.003**	−0.001	−0.001	−0.008***
	(−0.5)	(−7.9)	(−2.2)	(−0.4)	(−0.7)	(−3.9)
Difference	0.012**	−0.021***	−0.003	−0.003	0.022***	0.019***
	(2.5)	(−4.4)	(−0.7)	(−0.8)	(4.4)	(3.2)
Panel C. $y = Receivables/Sales$						
Treated	−0.004***	−0.001	0.000	−0.000	−0.002**	−0.001
	(−3.1)	(−0.9)	(0.2)	(−0.0)	(−2.3)	(−0.8)
Non-treated	−0.003***	0.001	0.000	0.001	0.004***	0.003***
	(−6.1)	(1.0)	(0.6)	(1.1)	(3.8)	(2.8)
Difference	−0.001	−0.002	−0.000	−0.001	−0.006***	−0.003**
	(−0.6)	(−1.3)	(−0.3)	(−0.8)	(−4.4)	(−2.4)
N. Treated			610			
N. Non-treated			45,659			

This table reports difference-in-differences in cash holdings, accounts payable, and accounts receivable, over the treatment and post-treatment periods, between treated and non-treated firms. Means 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 A1. ***, **,* denote statistically distinct from 0 at the 1, 5 and 10 percent level, respectively.

Table A5

ROBUSTNESS ANALYSES; BASELINE RESULTS

	Cumulative effects (T_t)					
	Treatment period			Post-treatment period	Test of parallel pre-trends	N. Treat./ Cont./ Un. Cont.
	(I)	(II)	(III)	(IV)	(V)	(VI)
Variables	2010	2011	2012	2013	(p-value)	
Panel A. 50 perc. best matches						
$y = Cash/Assets$	−0.031**	−0.044***	−0.041***	−0.037***	(0.988)	305/305/245
	(−2.2)	(−3.4)	(−3.7)	(−2.8)		
$y = Payables/Assets$	0.003	0.020*	0.029**	0.031**	(0.883)	305/305/245
	(0.3)	(1.8)	(2.4)	(2.2)		
$y = Receivables/Sales$	−0.000	−0.003	−0.009**	−0.012**	(0.478)	305/305/245
	(−0.4)	(−1.2)	(−2.0)	(−2.3)		
Panel B. Alternative payable measure						
$y = Payables/CGS$	−0.006	0.002	0.025*	0.018	(0.666)	109/109/44
	(−0.9)	(0.2)	(1.9)	(1.5)		
Panel C. Truncated variables						
$y = Cash/Assets$	−0.018*	−0.023**	−0.019**	−0.027**	(0.343)	521/521/402
	(−1.7)	(−2.3)	(−2.0)	(−2.4)		
$y = Payables/Assets$	0.002	0.013	0.032***	0.028**	(0.951)	521/521/404
	(0.2)	(1.5)	(3.3)	(2.3)		
$y = Receivables/Sales$	-0.003**	-0.007***	-0.009***	-0.009**	(0.192)	521/521/406
	(-2.2)	(-2.8)	(-2.6)	(-2.5)		
Panel D. Reitan firms omitted						
$y = Cash/Assets$	-0.001	-0.016*	-0.015	-0.015	(0.722)	376/376/362
	(-0.2)	(-1.8)	(-1.4)	(-1.3)		
$y = Payables/Assets$	0.003	0.010*	0.020**	0.014*	(0.597)	376/376/362
	(0.5)	(1.7)	(2.6)	(1.7)		
$y = Receivables/Sales$	−0.002	−0.000	−0.008***	−0.005	(0.954)	376/376/362
	(−1.2)	(−0.2)	(−2.7)	(−1.5)		

Table A5 continues on the next page

Table A5—CONTINUED

	Cumulative effects (T_t)					
	Treatment period			Post-treatment period	Test of parallel pre-trends	N. Treat./ Cont./ Un. Cont.
	(I)	(II)	(III)	(IV)	(V)	(VI)
Variables	2010	2011	2012	2013	(p-value)	
Panel E. Pharmacies included						
$y = Cash/Assets$	−0.019**	−0.031***	−0.022**	−0.031***	(0.596)	617/617/487
	(−2.2)	(−3.5)	(−2.7)	(−3.0)		
$y = Payables/Assets$	−0.001	0.011	0.028***	0.027**	(0.769)	617/617/487
	(−0.2)	(1.4)	(3.1)	(2.4)		
$y = Receivables/Sales$	−0.003**	−0.004**	−0.010***	−0.010***	(0.351)	617/617/487
	(−2.4)	(−2.4)	(−3.4)	(−3.3)		
Panel F. Unbalanced panel						
$y = Cash/Assets$	-0.000	-0.026***	-0.026***	-0.033***	(0.732)	649/649/517
	(-0.0)	(-3.0)	(-3.0)	(-3.0)		
$y = Payables/Assets$	0.023***	0.042***	0.069***	0.058***	(0.983)	649/649/517
	(2.8)	(4.8)	(6.7)	(5.0)		
$y = Receivables/Sales$	-0.003**	-0.005***	-0.013***	-0.016***	(0.354)	649/649/517
	(-2.2)	(-2.9)	(-5.2)	(-4.5)		

This table reports estimates of cumulative adjustments, Eq. (2). Panel A reports results for the 50 percent closest matches; Panel B reports results for payables scaled by the cost of goods sold; Panel C reports results for a sample where the variables are truncated at the 1st and 99th percentiles; Panel D reports results where Reitan firms are omitted; Panel E reports results when pharmacies are included; and Panel F reports results for an unbalanced panel. 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 A1. 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

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 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 A1. 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.