

# The Benefits of Banking Relationships: Evidence from Uganda's Banking Crisis\*

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## Abstract

Four Ugandan banks, including three domestic banks, were closed between September 1998 and May 1999 because of imprudent banking practices. This paper uses a unique bank-firm matched data set to estimate the effect of losing a banking relationship on firm performance. Employing a fixed effects difference-in-differences estimation that control for unobservable firm heterogeneity, preferred estimates suggest that firms that lost a banking relationship declined by between 10-15% relative to unaffected firms over the three years following the crisis. This result is robust to reverse causation; for a fairly well identified subset of firms, I find no evidence firm decline causing banking failure. I investigate two potential explanations of this result: the information and looting views of relationship lending. Insider firms experience the sharpest decline in employment relative to all affected firms. The insider effect persists even after controlling for level of outstanding debt. I interpret this as evidence of the looting view. In the set of potential non-looting firms, I find that older affected firms have larger growth deficits than younger affected firms. In addition, affected firms that do not produce hard information have the largest growth deficit. Moreover, affected firms that produce hard information have the same growth rate as unaffected firms. Finally, affected firms are more likely to report being credit constrained in the post crisis period. I interpret this as evidence for the information view

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# 1 Introduction

Banking crises in developed and developing countries are common and costly. Caprio and Klingebiel (2002) suggest that there have been as many as 112 episodes of banking crises in 93 countries since the late 1970s. In some instances, banking crises have been estimated to cost up to 50% of GDP (Klingebiel and Honohan, 2000). Although the determinants of banking crises are typically macroeconomic in nature, microeconomic factors have been crucial in a large number of crises in developing countries. Many of these crises have occurred in the context of rapid financial liberalization and weak regulatory environments (Demirguc-Kunt and Detragiache, 1998). The sources of banking crises have taken one of two basic forms (Honohan 1997). On the one hand, banks respond to increased competition in the post reform period by aggressively expanding their market share. This usually results in the accumulation of poor credit risks and subsequently in the deterioration of bank balance sheets. A lack of political will to deal with insolvent banks usually accompanies this expansion leading to systemic bank failure (Brock et al., 2000). On the other hand, weak banking supervision allows imprudent banking practices such as insider lending to exceed stipulated limits and, in effect, fails to prevent “looting” (Akerlof et al., 1993).

Four Ugandan banks were closed between September 1998 and May 1999 as a result of imprudent banking practices. This paper uses the closure of the two largest failed banks to estimate the effect of losing a banking relationship on firm performance.<sup>1</sup> I then examine two potential channels through which the loss of a banking relationship impacts firm performance.<sup>2</sup> Firstly, if a firm is in collusion with its bank to loot deposits, we would expect the firm's performance to be proportional to the looting opportunities available.<sup>3</sup> In general, we would expect a decline in the firm's performance following the closure of the colluding bank. Secondly, if banking relationships contain private and non-transferable information about the creditworthiness of a firm, then the loss of a banking relationship has implications for the firm's ability to obtain external financing in the post-crisis period. In particular, uninformed lenders are less likely to extend financing to the affected firm. Relative to similar firms with banking relationships, we would expect weaker performance from an affected firm wholly dependent on internally generated funds (Rajan and Zingales, 1998).

The scale and nature of Uganda's banking crisis present an opportunity to answer the questions above. To begin with, the banking crisis involved the closure of the second and fifth largest banks by deposits. As a result, 30% of firms in the sample lost at least one banking relationship yielding substantial variation to permit a reasonable level of precision in estimating the impact of losing a banking relationship. The nature of the crisis admits a causal interpretation of losing a banking relationship for a well identified subset of the sample. The collapse of the banks was not the consequence of aggregate or sectoral-level shocks. In addition, lending in the closed banks was highly concentrated. Firms owned by proprietors or directors of the bank accounted for up to 50% of the banks' asset portfolio suggesting that the collapse is arguably exogenous to

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<sup>1</sup>A banking relationship is defined as the purchase of information sensitive financial products from a particular bank. These include deposit facilities, credit lines, and loans.

<sup>2</sup>A third channel, that of disruption, is investigated in the robustness checks in section 6.

<sup>3</sup>The objectives of the colluding bank could range from loan officers approving loans to their friends and implicitly to themselves, to bank owners granting loans on non-market terms to non-financial firms they control. How the looting firms use the loot is not clear ex ante but would depend on the borrowing terms and expectations about the liquidation process. In the case of a pure looting firm such as a “brief-case company”, we might not expect to observe changes in observable measures of firm performance such as employment or productivity.

all but insider firms. Furthermore, there are no clearly identifiable policy changes or macroeconomic shocks around the event window to suggest alternative explanations of the results. Finally, unlike previous studies, the data used in this paper permit a longer period over which to analyze the impact of losing a banking relationship. The three post-crisis years provide an opportunity to trace the time profile of the impact and ultimately to elucidate the sources of the effects.

A perfect credit markets model would predict that the loss of a banking relationship should not have an impact on a firm's performance. Contrary to this prediction, I find that the average annual growth rate of firms that lost a banking relationship is 2.3-4% less than the growth rate of unaffected firms over the post crisis period. I estimate this effect using a fixed effects difference-in-differences estimation strategy that controls for unobserved fixed firm characteristics. In addition, for a fairly well identified set of firms, the result is robust to reverse causality. Using this set of potential non-looting firms, the average difference in the annual growth rate between affected and unaffected firms is -2.3% over the post crisis period. The effect is concentrated in the two years after the crisis but is most pronounced in the second year. The growth deficit is large; during the same period the Ugandan economy grew by an average of 4.2%. These results are robust to a relaxation of the parallel trends assumption.

I investigate the relevance of two potential channels through which the loss of a banking relationship affects firm performance: the *information* and *looting views* of relationship lending. These two channels are not mutually exclusive and it is conceivable that both are legitimate explanations for mutually exclusive subsets of the sample.

Asymmetric information between lenders and borrowers results in the rationing of borrowers (Stiglitz and Weiss, 1981). By purchasing various "information sensitive" financial services from its bank, a firm reveals private information to its banker (Rajan, 1992).<sup>4</sup> The bank has incentives to collect this information as it can expect to extract future rents from the firm by exploiting the proprietary nature of the information. An informed bank is more likely to lend than an uninformed bank. These two ingredients form the basis of long term relationships between firms and banks. In this way, relationships serve to reduce problems of asymmetric information between banks and firms.<sup>5</sup> Two features of the *information* view of bank-mediated relationship lending are of particular relevance to this paper. The first is that the information acquired by the bank is proprietary and arguably unobservable to other lenders. This is especially so where there are no alternative stores of information such as credit rating agencies or bureaux, as is the case in the setting used in this study. Secondly, it takes time for a bank to acquire information about a new firm, since the financial products purchased are crude channels for the transmission of information about the ability/type of firm.<sup>6</sup> It follows that the loss of a banking relationship implies the loss of non-transferable information embodied in the relationship. In the absence of surviving banking relationships, the affected borrower becomes a potential "lemon" in the credit market and is less likely to obtain external financing (Sharpe 1990). The decline in

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<sup>4</sup>I focus here on bank lending as this is the dominant form of formal credit provision in developing economies. Information sensitive services include deposit and overdraft facilities as well as loans.

<sup>5</sup>See Granovetter (1985), Greif (1993) and Casella and Rauch (1997) for other cases where relationships overcome information problems in other settings.

<sup>6</sup>Diamond (1989) considers finite information flow rates in his theory on reputation building in credit markets characterized by asymmetric information.

creditworthiness is a consequence of the fact that remaining lenders are uninformed and not necessarily because of the possible stigma associated with having had a relationship with a failed bank.<sup>7</sup>

An alternative view of relationship lending has been suggested by La Porta et. al. (2003). The authors argue that under weak regulatory regimes, collusion between bankers and insider firms to loot deposits is likely to be more profitable than lending on the basis of efficiency. Using data from Mexican banks, they show that borrowers with close family ties to the directors/owners of banks have higher default rates and pay lower prices for debt. They conclude that, contrary to the predominant view on relationship lending, close banking relationships in weak regulatory settings are conduits of loot and not information. The existence of looting suggests significant efficiency costs of credit misallocation; credit flows to the firms with the weakest incentives to use these resources efficiently. The loss of a banking relationship in the looting framework is synonymous with the cessation of cheap, unsecured credit. In addition, we might expect the stigma of having banked with a failed institution to be stronger for clearly identified looting firms.

To test the plausibility of each of the channels outlined above, I divide the sample into a set of potential looting and non-looting firms. For the set of potential looting firms, I find consistent evidence of looting. Firms owned by proprietors/directors of the closed banks experience the largest decline in average log employment relative to other affected firms. The average log employment of insider firms declines by 31-35% between the pre and post crisis period relative to affected non-looting firms. In addition, the poor performance of insider firms is not explained by the size of their outstanding debt with the closed banks. Controlling for large outstanding debtors, insider firms, 60% of which are large debtors, decline by 20% relative to large debtors. The average log employment of large debtor non-insider firms declines by 15% relative to other affected firms, while small debtor non-insider firms decline by 8%. The size and robustness to other controls of the insider effect suggests that the amount of outstanding debt is not a good proxy for the amount looted prior to bank failure.<sup>8</sup>

For the set of potential non-looting firms, I find evidence in support of the information view. Older affected firms experience a larger growth deficit than younger affected firms. The decline in average log employment of an affected firm at 75th percentile relative to an affected firm at the median age is approximately 15%. In addition, using responses to survey questions on the use of external auditors and the preparation of annual budgets, I find that affected firms that do not produce “hard” information experience the largest declines in log employment. Moreover, the growth rate of affected firms that use external auditors is not statistically different from that of unaffected firms. Finally, firms that lost a banking relationship are more likely to report being credit constrained in the post crisis period than unaffected firms.

The rest of the paper is organized as follows. In section 2, I describe the banking crisis and the data used in the analysis, section 3 outlines an empirical strategy to deal with various identification problems, section 4 presents the main empirical results, section 5 presents evidence for the two channels outlined above, section 6 carries out a number of robustness checks, section 7 discusses the results and I conclude in section 8.

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<sup>7</sup>This is certainly a possibility to consider when imprudent banking practices are the basis of the bank failure. However, for firms that are not closely associated with the failed bank, I would expect the former effect to dominate.

<sup>8</sup>Given that outstanding debt is public information, the amount reported for insiders is likely under-reported.

## 2 Banking Crisis and Data Sources

### 2.1 Banking Crisis

Uganda embarked on financial sector reform in the early 1990s as part of a broader structural reform program. The main features of financial reform included the lifting of interest controls, a reduction in directed credit and the removal of barriers to entry to encourage competition in a sector previously dominated by two government owned banks. Between 1990-1995, 11 private banks were licensed resulting in a three-fold increase in the number of banks relative to the pre-reform period. The reform culminated with the passing of the Financial Institutions and Bank of Uganda Statutes in 1993 which enhanced the regulatory authority of the central bank of Uganda (BOU). These two pieces of legislation gave BOU complete regulatory authority over all financial institutions and autonomy in the conduct of monetary policy (Kasekende and Atingi-Ego, 1995).<sup>9</sup> Crucial features of the new laws included the raising of minimum capital requirements from the equivalent of \$15,000 to \$0.5 and \$1 million dollars for local and foreign banks respectively and an expansion of BOUs banking supervision capacity (Republic of Uganda, 1993).<sup>10</sup> Examples of decisive intervention of the central bank include the closure of a local bank in 1993 and the sale of two local banks with solvency problems to strategic foreign investors in 1996.

A bank wide audit in September 1998 revealed levels of insider lending above the legally stipulated limit in a number of banks.<sup>11</sup> Three banks were closed immediately.<sup>12</sup> Greenland Bank, a large privately owned domestic bank, signed a memorandum of understanding with BOU to inject more capital and reduce the level of insider lending. In December 1998, BOU discovered that Greenland Bank had illegally acquired control of the country's largest bank shortly after it had been privatized.<sup>13</sup> BOU suspended Greenland's management team and assumed the day to day running of the bank. It was closed 4 months later after attempts to turn the bank around proved futile. Finally, Cooperative Bank, the country's second largest bank, was closed suddenly on May 20 1999 following the withdrawal of the United States Agency for International Development's (USAID) support to the bank.<sup>14</sup>

Three of the banks closed had issued large amounts of credit to insiders. In the case of International Credit Bank, a small indigenous bank, 45% of the loan portfolio had been advanced to the companies

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<sup>9</sup>Prior to this, the authority to license banks and credit institutions rested with the Ministry of Finance with the regulation and supervision left with the Bank of Uganda (1969 Banking Act).

<sup>10</sup>The implementation of the new capital requirements was delayed until the end of 1996 to give banks time to adjust. Capital requirement have since increased to the equivalent of \$2 million for all banks starting January 1, 2003.

<sup>11</sup>Up to this time, BOU carried out audits every two years and relied on periodic bank returns for indicators of the health of the banking sector.

<sup>12</sup>One of these banks, Trust Bank, was closed after a run on the bank had been triggered by an announcement of the closure of its Kenyan affiliate. It was reopened two months later only to be closed in September 1999. Another bank was reopened 4 months later and continued to operate until September 2002, when it was eventually shut down.

<sup>13</sup>Uganda Commercial Bank (UCB), the country's largest bank was state owned until the end of 1997 when it was privatized. The winning bidder, a Malaysian investment firm, transferred its shares to Greenland Bank, which had also entered a bid, in contravention of the sale agreement. Several prominent politicians have been implicated in this transaction. Furthermore, Greenland had used UCBs foreign credit line to finance insider companies. (East African, December 1998, April 1999, February 2002).

<sup>14</sup>Cooperative bank was re-capitalized using USAID's proceeds of food aid in 1997/1998 (Nanyonjo 2001). A USAID report released in May 1999 cited large losses and poor management as reasons for withdrawing their support.

associated with/owned by proprietors of the bank. In Greenland’s case, insider lending accounted for 55% of the loan portfolio (47% of total deposits), in addition to an illegal credit line from the country’s largest bank which had been used to finance insider companies to the tune of \$14.8 million. Furthermore, large assets and liabilities were found off the bank’s balance sheet. The majority of these loans were made without sufficient security.<sup>15</sup> In the case of Cooperative Bank, the largest bank closed, losses in the previous year of \$7 million accumulated further leading eventually to insolvency to the tune of \$10 million. Large non-performing loans to various politicians and shareholders were cited as the source of the losses.<sup>16</sup> All the closed banks were operating with weak internal controls; regular internal audits portrayed the banks’ operations as sound even while the level of non-performing assets was rising. A Bank of Uganda report summarized the reasons for the closure of these banks as “insolvency brought about by imprudent banking practices and poor internal governance” (BOU 1999).

The central bank undertook to compensate all depositors the full amount of their deposits, over and above the insured limit of \$2000. The cost of this exercise was financed by government borrowing from the private sector in order to forestall inflationary pressure. In addition, BOU undertook the collection of outstanding balances from the banks’ creditors and the liquidation of other bank assets.<sup>17</sup>

A crucial outcome of the banking crisis is that it changed the market structure from one previously dominated by domestic banks (local private or government) to one dominated by foreign banks. In addition to the loss of 3 indigenous banks, the largest bank was placed under statutory management and operated under very narrow banking guidelines throughout the post crisis period relevant to this paper.

## 2.2 Data

The data used in this paper comes from the World Bank’s Regional Program for Enterprise Development (RPED) 2002/3 survey of 300 manufacturing firms in Uganda. The sample frame comprised of all manufacturing firms with 10 or more employees in the 2001/2002 Census of Business Establishments (UBOS, 2002). A stratified random sample was drawn from this frame with location, size and sector the stratification dimensions.<sup>18</sup> The stratification yielded 56 populated clusters. Table 1 in the appendix shows the structure of the sample. The sampled firms represent a sampling rate of 41% and account for about 70% of employment in the manufacturing sector.<sup>19</sup>

The survey collected information on general firm characteristics, production and sales, credit and investment, technology, labor, infrastructure and the regulatory environment. The credit section collected data on the length and nature of current relationships with up to four financial institutions. Firms were asked to

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<sup>15</sup>East African, 15-19 April 1999. BOU Press Release, 19 April 1999.

<sup>16</sup>East African, June 2-6 1999.

<sup>17</sup>BOU appointed international accounting and audit firms as the liquidators of the various banks. Cases of outstanding balances that are not paid in the time stipulated by the BOU/liquidator are referred to the commercial court.

<sup>18</sup>The following locations were selected; the Central region, which includes the capital city, South and West region and the North & East region. The central region accounts for about 70-80% of manufacturing activity. Sample stratification was designed to reflect this, although because of the insecurity in the North/East, the final sample is under-represented by firms from this region.

<sup>19</sup>Sampling probabilities are proportional to firm employment. Approximately 30% of firms had to be replaced due to non-response, non-existence and security reasons. This is comparable to a smaller survey conducted by the World Bank in 1998 where 39% of firms were replaced (Reinnika and Svensson, 2000).

provide details about their most recent loan and/or credit line, including the principle, interest, collateral requirements and duration in the case of loans. Firms were also asked whether they had a banking relationship with any of the four collapsed banks, whether any of their major trading partners had a relationship with these banks and if the collapse of these banks had affected their employment or exports.<sup>20</sup> The survey also collected information on the extent of trade credit utilization and other sources of working capital and investment finance for the previous financial year. Additional data on the identity of insider firms and firms with outstanding debt to failed banks at the time of closure, was collected separately by the author from experts in the banking industry, BOU reports and newspaper archives.<sup>21</sup>

A five year series of retrospective employment and sales data is collected for the years 1997-1998, and 2000-2002. Data for 1999, the year of the banking crisis was not collected owing to an accidental adjustment to the questionnaire.<sup>22</sup> A three year series of other potential outcomes such as exports, investment and manufacturing costs are collected for 2000-2002 which corresponds to the post crisis period.

Collecting information from firms in developing countries, where tax and regulatory compliance rates are low, is generally very difficult. Respondents refused to provide enumerators with vital accounting information despite careful training of, and enumeration by, a team from the consulting arm of the Uganda Manufacturing Association. As a result, sensitive information regarding costs, sales and other vital accounting information is missing in nearly 40% of the entire sample.<sup>23</sup>

The use of retrospective data suggests the potential for “halo” effects – respondents might be induced to report low firm size during and after the crisis period. This is not likely to be a severe problem in this data as the questions regarding the firm’s employment/sales are asked in separate sections on labor/production. For the majority of firms in the sample, the respondent for the credit and employment section is different and respondents were typically interviewed simultaneously.

The relevant sample for this study includes 219 firms that were established before 1999 and had a banking relationship between 1997 and 2002. 70 of these firms lost at least one banking relationship during the crisis. Table 2 shows characteristics of firms tabulated by whether the firms lost a banking relationship or not. From this table it is clear that there are some significant differences between affected and unaffected firms. In particular, a higher and statistically significant proportion of affected firms have owners from the central region of Uganda, a lower proportion of owners of Asian descent, a higher proportion of local banking relationships and a larger percentage of the firm owned by domestic entities.<sup>24</sup> This is in line with expectations about firm-bank matching given that 3 of the 4 closed banks were indigenous banks.

Employment data is missing for 26 firms in the pre-crisis period, 5 of which lost a banking relationship. This compares to 125 firms, of which 46 are affected firms, that do not report sales data for the pre-crisis

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<sup>20</sup> A banking relationship is defined as having an account, overdraft or loan with the collapsed bank. Firm reports of the effects of the banking crisis on exports/employment are not used in subsequent analysis as they might represent halo effects.

<sup>21</sup> An insider firm is defined as a firm owned by directors/proprietors of the closed banks.

<sup>22</sup> The lack of 1999 data prevents a more accurate assessment of the trend of the effect of the banking crisis. However, because I focus on the two banks closed in April and May of 1999, the reliability of the employment data for this purpose depends on whether it is uniformly end-of-year employment data.

<sup>23</sup> The Uganda Manufacturers Association (UMA) is the primary business association for industrial firms in Uganda. The use of UMAs consulting arm to enumerate the survey in partnership with the Private Sector Foundation, an umbrella organization for all business associations, was designed to win the confidence of respondents and ensure reasonable response rates.

<sup>24</sup> Local banking relationships refer to banking relationships with banks that are locally owned.

period.<sup>25</sup>

### 3 Empirical strategy

The primary methodological approach used to test the prediction that the loss of a banking relationship does not matter is an event study.<sup>26</sup> However, in order that inferences drawn have a causal interpretation, a number of conditions must be fulfilled. Most importantly, the event must not be triggered by the actions of the units under observation. In addition, it is important that the event be isolated in the sense that there are no other concurrent changes that could affect the outcome(s) of interest. Typically, the events that satisfy the above conditions are unanticipated announcements of bank distress or the sudden collapse of a bank. I depart from the usual event study approach by employing a difference-in-differences approach to accommodate the fact that the length of pre and post event periods are longer than the typical event study. The control group includes all firms that did not lose a banking relationship.<sup>27</sup>

Unlike the typical event study that uses stock prices as the primary outcome, I use changes in permanent employment to estimate the impact of a loss of a banking relationship on firm performance.<sup>28</sup> The use of employment as an outcome is a good measure for firm performance as we care about the ability of firms to generate employment. However, the draw back is that slow or negative growth of employment could reflect other changes associated with increases in firm productivity.

To estimate the effect of a loss of a banking relationship, I run the following difference-in-differences specification

$$y_{ijt} = \alpha_i + \mu_{jt} + \sum_{k=1998}^{t=2002} D_k * \beta_k + \sum_{k=1998}^{t=2002} D_k * T_i * \delta_k + \varepsilon_{ijt} \quad (1)$$

where  $y_{ijt}$  is the natural log of permanent employment/sales of firm  $i$  in sector  $j$  in year  $t$ ,  $D_k$  is a year dummy for year  $k$  that takes on the value of 1 when  $t = k$  and 0 otherwise.  $\alpha_i$  is a firm fixed effect while  $\mu_{jt}$  is a sector year fixed effect.<sup>29</sup>  $T_i$  is a dummy that takes on the value of 1 if the firm lost a banking relationship and 0 otherwise.

In addition I control for age and age squared in order to capture potential changes in the quality of the firm as models of learning suggest (Jovanovic, 1982, Hopenhayn 1992).

<sup>25</sup>90% of the non-responses for sales in 1997/1998 are listed as “Don’t Know”. However, between 2000-2002, “Refused to Answer” accounts for more than 20% of the missing observations. I conduct a year-by-year logit analysis to look at the pattern of missingness across affected and unaffected firms. For the employment data, affected firms are less likely to report missing data (coefficient insignificant at the usual levels) for the pre-crisis period. In the post crisis period, 2 (1) of the firms with no employment data in 2002(2001) are all affected firms. A similar analysis for the sales data shows that affected firms are more likely to report missing data in 2000, 2001 (significant at the 5% level) and in 1998 (p-value 0.12).

<sup>26</sup>James(1987), Lummer & McConnell (1989), Slovin, Sushka and Polonchek (1993), Peek and Rosengren (1997), Yamori and Murakami(1999), Ongena et. al. (2003), Bae et. al. (2000) are examples in the literature that have used banking crises as event studies.

<sup>27</sup>In the robustness section, I relax the parallel trends assumption implicit in the diff-in-diff analysis by augmenting the diff-in-diff with propensity score matching.

<sup>28</sup>Uganda’s stock market has 5 firms currently trading on the exchange and only 1 continuously throughout the period examined in this paper. I present results using output data to confirm the robustness of employment results.

<sup>29</sup>This sectoral term includes both a time-invariant term capturing differences in technology across sectors as well as idiosyncratic shocks.



$$y_{ijt} = \alpha_i + \mu_{jt} + X_{ijt}\gamma + \sum_{t=1998}^{t=2002} D_k * \beta_k + \sum_{t=1998}^{t=2002} D_k * T_i * \delta_k + \varepsilon_{ijt} \quad (2)$$

where  $X_{ijt}$  represents age and age squared. The coefficient set  $\delta_k$  captures the average difference in log firm employment/sales between affected and unaffected firms between year  $k$  and the omitted year 1997. The test of the prediction that the loss of a banking relationship does not matter is a joint test that the coefficient set  $\delta_k = 0$  for  $k > 1999$ .

Let  $\nu_{ijt} = \varepsilon_{ijt} + \alpha_i$  be the composite error term. The OLS identifying assumption for the parameters of interest  $\delta_k$  is

$$E[T_i \cdot \nu_{ijk} \mid X_{ijt}] = 0 \quad \forall k \in (1998, 2002)$$

where  $\nu_{ijk}$  contains unobserved time varying and fixed firm characteristics potentially correlated to  $T_i$ . Consistent estimation of  $\delta_k$  is problematic for a number of reasons.

- Ordinary least squares estimation of the specification above would in general produce biased estimates because a non-random set of banking relationships were dissolved. The firms that lost a banking relationship are likely to be different from firms that did not lose a relationship. An examination of table 2 in the appendix confirms that affected and unaffected firms differ along a number of important dimensions. In particular, affected firms have significantly higher domestic ownership, are more likely to be run by indigenous entrepreneurs, are less likely to have a manager with secondary school or higher education, are less likely to be in the heavy industry sector and are more likely to report that a major trading partner lost a banking relationship. These differences are likely to have significant impacts on the performance of firms even in the absence of losing a banking relationship. We would expect that in general  $E[T_i \alpha_i] \neq 0$ .

Assuming that equation 2 is correctly specified, the use of a fixed effects estimation which exploits within-firm over time variation to identify  $\delta_k$  solves this problem. The identification assumption for the fixed effects estimator to be valid is

$$E[T_i \cdot \tilde{\nu}_{ijk} \mid \tilde{X}_{ijt}] = 0 \quad \forall k \in (1998, 2002)$$

where  $\tilde{\nu}_{ijk} = \nu_{ijk} - \bar{\nu}_i$ ,  $\tilde{X}_{ijt} = X_{ijt} - \bar{X}_i$  and  $\bar{\nu}_i$ ,  $\bar{X}_i$  are firm means over the period 1997-2002. Subtracting the mean of both sides of specification 2 sweeps out fixed firm characteristics  $\alpha_i$ . This parallel trends assumption is relaxed in the robustness section.

- However, the use of fixed effects is unlikely to deal with other sources of omitted variable bias. In particular, any time varying covariates that are correlated with  $T_i$  would bias estimates of  $\delta_k$ . Unbiased estimation of  $\delta$  requires that there are no simultaneous policy changes that impact affected firms differentially. In particular, if these policies or aggregate demand shocks had larger negative effects on more indigenous firms, then I would wrongly infer the effect of these policies on the banking crisis. A likely policy candidate would be a more vigorous campaign to collect tax revenues from companies on

the fringe of the formal-informal divide.<sup>30</sup><sup>31</sup> Another potential source of omitted variable bias would be differential access to technology driven by differences in foreign ownership.<sup>32</sup> In the absence of direct measures of potential time varying covariates I attempt to control for this bias by including interactions of the post crisis dummy with a set of covariates  $W$  that captures potential differences in the effects of unmeasured confounding factors.  $W$  includes whether the firm is owned by an indigenous entrepreneur or whether the firm had less than 10 employees in the pre-crisis period. The source of variation used to identify  $\delta_k$  comes from within firm changes net of any differences between small and large, or indigenous and non-indigenous firms in the pre and post crisis periods.

- The most severe source of bias however, is reverse causality. A substantial number of banking crises’ descriptions typically imply the presence of reverse causality. The residual in specification 2 is potentially correlated with  $T$  if the firm’s actions and failed projects affect the survival probability of its bank. In other words, the banks collapsed because they were lending to firms whose projects went bad. If this is the case, then I would expect to observe a decline in the firm’s performance before the onset of crisis. Figure 1 in the appendix shows trends in log employment for affected, unaffected firms, insiders and borrowers. In Panel B, it is clear that insider firms, which are significantly larger than other firms, experience a decline in employment well before the crisis. An examination of the employment trend of firms that had outstanding debt when the banks were closed (outstanding debt) suggests a slight decline in the pre-crisis period. This illustrates that at least for the insiders, the banking crisis is not strictly exogenous. I investigate this possibility further in Table 3 which presents OLS estimates of a regression of pre-crisis firm growth on insider status, firms with outstanding debt and other covariates. In columns (1) and (2), where I only control for sector and location, the decline in pre-crisis employment by insiders is large and significant at the 5 and 10% levels respectively. This suggests that the decline in insider firms is not generated by a shock to a particular sector or geographic region, but rather by either an idiosyncratic shock to insider firms or wilful collusion to loot the bank. Firms with outstanding debt at the point of closure show a statistically insignificant decline in firm size in the pre-crisis period. Controlling for other covariates in columns (3) and (4) reduces the precision of the estimates, but maintains the result that insiders decline in the pre-crisis period relative to other affected firms. In columns (5) and (6), I use growth from startup to 1997 as the dependent variable. The results are broadly similar to the estimates in columns (1)-(4). Affected firms that had no debt at closure or were not insiders do not perform significantly differently from unaffected firms

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<sup>30</sup>The government of Uganda has been making concerted efforts to improve revenue collection (very low at 12% of GDP, the sub-Saharan average is nearly 25%). However, this ratio has not changed significantly over the entire period, casting some doubt on whether changes in tax policy are a potential source of omitted variable bias. Tax policy could still impact affected firms differentially if the composition of taxpayers shifts towards more informal establishments without changing revenue/GDP ratio.

<sup>31</sup>A hurdle equation of the share of revenues reported to the authorities for tax purposes indicates that affected firms are more likely to report lower levels of declared revenues. The question “what percentage of total revenues would you estimate the typical firm in your area of activity reports for tax purposes” is assumed to capture the firm’s own declaration. The selection equation estimates the probability that the firm responds to this question. I use measures likely to capture the degree of informality in the selection equation.

<sup>32</sup>Using firm responses to the question “has the firm invested in technology in the last three years” I conduct a probit estimation controlling for age, foreign ownership, and whether the firm lost a banking relationship and other firm characteristics. The coefficient on  $T$  is negative but is not significantly different from zero.

in the pre-crisis period. The foregoing confirms reports that very high levels of insider lending, up to 55% of the asset portfolio in the case of Greenland Bank, are likely to have been responsible for the collapse of the banking institutions. In this sense, the nature of the banking crisis is crucial in order for inferences to have a causal interpretation. The necessary underlying assumption is that the collapse of the bank can be attributed to a well identified subset of firms. In this sense, Uganda’s banking crisis, characterized as it was by excessive levels of concentrated lending, permits exogeneity of the crisis for the majority of affected firms.

- The final source of bias is a mis-specification of the relationship between firm size and financial position. The working specification above assumes that fixed firm characteristics affect the level of operation and not the rate of change of the level. This would not be true if past actions, which would be correlated with unobservable firm quality, matter for the scale of the operation. I investigate the implications of mis-specification in section 6.

The other econometric issue concerns measurement error. The sales data is likely to be measured with significant error as those firms that reported non-missing sales data were reluctant to provide accurate information. A large fraction of firms did not report any sales data. The employment data is not likely to suffer from serious measurement problems although the use of retrospective data is likely to be associated with recall error.<sup>33</sup> The estimation procedure outlined above assumes that all employment reported is end of year employment. This implies that for firms which lost banking relationships in 1998, the employment corresponding to this year is post crisis employment. To avoid this problem I run the regressions using a restricted sample that drops 9 firms that lost a banking relationship in 1998.<sup>34</sup>

To account for the concerns raised above I run the following specification controlling for firm fixed effects and sector year fixed effects

$$y_{ijt} = \alpha_i + \mu_{jt} + X_{ijt}\gamma + \sum_{t=1998}^{t=2002} D_k * \beta_k + \sum_{t=1998}^{t=2002} D_k * T_i * \delta_k + Post_{it} * W_i\lambda + \varepsilon_{ijt} \quad (3)$$

where  $W_i$  contains dummies for whether the firm had less than 10 employees in the pre-crisis period and whether the firm was owned by an indigenous entrepreneur in the pre-crisis period.<sup>35</sup> I run specification 3 separately for the full restricted sample, restricted sample minus insiders and a restricted sample that excludes both insiders and borrowers.<sup>36</sup> I carry out the joint test that  $\delta_k = 0$  for  $k > 1999$  as the test for whether the loss of a banking relationship affects firm performance.

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<sup>33</sup>Recall error is unlikely to be correlated with having lost a banking relationship. Further care was taken to avoid any undue correlation by posing the banking related questions to the accountant and the labor questions to the personnel manager.

<sup>34</sup>This implies that the event being studied refers to the closure of Greenland bank in April 1999 and the closure of Cooperative Bank in May 1999.

<sup>35</sup>Serial correlation in the employment/sales data would imply that the standard errors of canned fixed effects estimators would be underestimated. In addition, serial correlation of the independent “treatment” variable and interactions would magnify this problem severely (Bertrand et.al 2002). To correct for this problem, I cluster regressions at the firm level that allows for an arbitrary correlation of residuals at the firm level.

<sup>36</sup>Results using the unrestricted sample available from author at request.

## 4 Main Results

In this section I present reduced form results of the impact of the banking crisis on related firms. Panel A of figure 1 presents unconditional trends of log employment for affected and unaffected firms. The growth rate between 1997-1998 is of same magnitude for both unaffected and affected firms and is suggestive evidence for a valid assumption of parallel trends. Growth between 2000 and 1998 is approximately zero for affected firms, while growth for unaffected firms is positive. The growth difference is maximized in 2001, when unaffected firms post positive growth while affected firms show negative growth. There is no apparent growth difference between affected and unaffected firms in 2001-2002. Table 4 shows summary statistics of the variables used below.

Table 5 presents the main results of this paper. All specifications in table 5 control for firm fixed effects, include sector-year fixed effects, a time trend as well as interactions with a post crisis dummy of key covariates  $W$ . Columns (1) and (2) present the results for the full sample. The test of the benchmark model is the joint test that all interactions of the years 2000-2002 with loss of a banking relationship status are zero. This hypothesis is rejected at the 1% and 5% level in columns (1) and (2) for the unclustered and clustered specifications respectively.<sup>37</sup> The magnitude of the annual growth deficit experienced by affected firms in the post crisis period is 3%. The growth deficit is more dramatic if we restrict the period of observation to the 2 years after the crisis. Affected firms grow by 5.3% less than unaffected in each year between 1998 and 2001. The full sample is likely to suffer from potential reverse causality problems as can be confirmed in panel B of figure 1. In lieu of this, I drop all insider firms from the sample in columns (3) and (4). The magnitude of the coefficients falls by 15% relative to corresponding estimates in columns (1) and (2). The joint hypothesis that losing a banking relationship does not matter is rejected at the 5% level in column (4) but has a p-value of 0.07 in column (3) where I do not control for firm age. The growth deficit of affected firms suggested by the coefficients is 2.5% per annum throughout the post crisis period and 5% if we restrict the period of observation to 1998-2001. To purge estimates of further reverse causality bias, I drop firms that had outstanding debt with the bank at closure in addition to all insider firms in columns (5) and (6).<sup>38</sup> The precision of the estimated parameters falls. The null hypothesis that losing a banking relationship does not matter cannot be rejected at the 5% level. The p-values are 0.15 and 0.12 in columns (5) and (6) respectively. The fact that we reject the null hypothesis only at the 12% level in column (6), reflects the low power associated with dropping affected firms from the sample.<sup>39</sup> The annual growth deficit of affected firms is 2.3% for the entire post crisis period and 4.9% for 1998-2001.

The size of the impact of losing a banking relationship is large. Using the preferred specifications in columns (5) and (6) which exclude insiders and firms with outstanding debts to the failed banks, affected firms grow by between 2.3 and 2.5% per annum less than unaffected firms in the post crisis period. This effect increases when we restrict observation to the period 1998-2001. Affected firms contract by between

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<sup>37</sup>Given that the null hypothesis is a joint test of the 3 post crisis year-treatment interactions, standard errors should be clustered at the firm level. The last row in table 5 is therefore the relevant set of p-values.

<sup>38</sup>This would eliminate all reverse causality bias if the failure of the banks depends only on the quality of its current portfolio and all non-performing loans are included in the set of firms with outstanding debt.

<sup>39</sup>The standard error associated with going from the full sample to the sample used in columns (5) and (6) rises by 14%.

4.7-5.3% per annum relative to unaffected firms. The cumulative growth gap of affected firms is 10% over the entire post crisis period. Average annual growth in the economy in the post crisis period averages 4.2% in the post crisis period.

## 5 Testing the channels: Looting and Information

The results above suggest large costs associated with a loss of a banking relationship. In this section I attempt to elucidate the potential channels through which the loss of a banking relationship operates. This is an important exercise as different channels are likely to have different policy implications. A starting point is the realization that the main effects of the crisis are concentrated in the 2 years immediately after the crisis. Panels A and B in figure 2 illustrate this point. The source of the costs of losing a banking relationship must be consistent with the differential employment trends shown in figure 2. I consider in turn a two potential channels through which the loss of a banking relationship impacts firm performance.

- *The looting view.* Under this scenario, weak banking supervision permits collusion between a banker and a firm to loot deposits.<sup>40</sup> In this case, the content of a banking relationship captures a measure of the propensity for any match between a bank and its client to engage in looting. For some firms, the relationship is strong enough to generate flows of cheap credit. The closure of the bank implies the dissolution of the colluding relationship and in cases where the relationship was strong enough to generate positive flows of loot, the cessation of cheap financing. We would expect looting firms to experience the sharpest relative decline in firm size. A steep decline in employment is a necessary but not a sufficient condition to conclude that looting is the correct or predominant channel. In order to identify the looting view, we would need to observe the borrowing and investment history of all affected firms. In particular we would need to observe the price at which these firms borrowed, their default rates and other terms of the contract such as collateral requirements.<sup>41</sup> In the absence of detailed borrowing histories, I rely on much cruder measures to test this prediction. I include a dummy variable that takes on the value of 1 if the affected firm is owned by a bank director/owner (insider) and 0 otherwise. Bank owners would have high incentives to divert resources from the bank to non-financial firms they control, particularly if their share in equity in the non-financial firms exceeds that in the banks. (La Porta et. al, 2003, Johnson et. al, 2000)<sup>42</sup> In addition, I expect that collusion is more likely between firm owners and bankers that share the same ethnicity. I include a dummy that takes on the value of 1 if the affected firm's owner comes from the same region/ethnicity as the owner/managers of the

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<sup>40</sup>In order for this to be an equilibrium we require some accompanying conditions on the likelihood and size of penalty for either of the colluding parties so that collusion is preferred to the alternative strategy of lending on efficiency grounds. Low likelihoods of punishment and penalties correspond to what La Porta et al (2003) refer to as a weak regulatory environment.

<sup>41</sup>Although the official publications of the Bank of Uganda do suggest the prevalence of unsecured lending, I do not have any data to confirm this. The investment pattern of a looting firm is likely to vary depending on the "borrowing" terms and expectations about the liquidation process. For firms receiving unsecured/poorly secured financing, we might expect positive investment as the potential liquidators would be unable to hold the looting firm accountable beyond the weak borrowing terms. Whether the firm invests or consumes immediately would depend on rates of time preference, the nature of the investment and expectations of liquidator action.

<sup>42</sup>Given the capital requirements needed to operate a bank, we would expect that bankers would tend to have larger equity in non-financial firms.

collapsed bank and 0 otherwise.<sup>43</sup> Further, I expect that firms that were looting did not anticipate the collapse of the banks and therefore the set of firms with outstanding debt at the time of closure is likely to be correlated with a set of looting firms.<sup>44</sup> I include a dummy that takes on the value of 1 if the affected firm had outstanding debt with the collapsed bank at the time of closure and 0 otherwise. I also include a dummy that is 1 if a firm had more than the median level of outstanding debt and 0 otherwise. This allows me to distinguish between a large debtor effect and the independent effect of being an insider.<sup>45</sup>

- *The information view.* Under this scenario, the banking relationship embodies information about the firm’s creditworthiness that is not transferable to other lenders. The dissolution of the relationship implies a loss of non-transferable information embodied in the banking relationship. The affected firm becomes a potential lemon in the credit market and is less likely to obtain external financing from uninformed lenders. The loss of a banking relationship is likely to have a bigger (more negative) impact on “soft” information firms that can not reproduce information about their creditworthiness to other lenders readily.<sup>46</sup> The ideal data to test this prediction would be a measure of the information embodied in the lost relationship in conjunction with measures of the “softness” of information of affected firms. I would expect that “soft” information firms with the most information lost are likely to be the worst affected. In the absence of data on the concentration of firm borrowing or length of the relationship with closed banks, I use proxies for the amount and type of information lost in the relationship. I include interactions of  $T$  with firm age to capture a measure of information lost, an interaction with a dummy that is 1 for firms with less than 10 employees in the pre-crisis period and 0 otherwise. Small firms have a comparative disadvantage in the production of “hard” information as they can not afford hard information services such as external auditors. In this direction, I include two direct measures of the ability of a firm to produce “hard” information using the firms’ responses to the questions, “Do you prepare an annual budget?” and “Does the firm use external auditors?”<sup>47</sup> The annual budget dummy takes on a value of 1 if the firm does not prepare an annual budget and 0 otherwise. Likewise, the external audit dummy is 1 if the firm is not audited by an external agency and 0 otherwise.<sup>48</sup> These variables are potentially endogenous to the loss of a banking relationship. Although it is unlikely that firms using external auditors/preparing annual budgets would switch from these practices, it is possible that in response to the crisis, firms could have started preparing annual budgets and using external auditors.<sup>49</sup> However, it is reasonable to assume that firms that do not prepare annual budgets

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<sup>43</sup>I use region of birth for the owners/directors of the 3 indigenous banks and ethnicity for the foreign bank. This is a similar to one of the measures of related lending used by La Porta et. al (2003) albeit cruder.

<sup>44</sup>This is one of the findings of La Porta et. al. (2003) who show that related lending increases as the fragility of the bank increases.

<sup>45</sup>The assumption is that the effect of reverse causality is likely to be similar for firms with the same level of outstanding debt, but insiders are likely to have received more cheap funding over their lifetimes than firms with the same level of outstanding debt.

<sup>46</sup>A firm is defined to be a “soft” information firm if information about the firm cannot be easily reduced to a set of numbers or index. This is in the spirit of Stein (2002), Berger et. al (2003) Petersen (2002).

<sup>47</sup>The responses to these questions reflect current firm practices contemporaneous with when the survey was conducted.

<sup>48</sup>While the preparation of an annual budget might reflect entrepreneurial ability, the use of external auditors is costly. This is more so in a small economy dominated by the big 5 accounting firms.

<sup>49</sup>This is particularly so in light of the regime shift from a financial sector dominated by domestic banks to one dominated

or use external auditors currently, did not do so in the pre-crisis period. I investigate the likelihood that more able firms affected by the crisis switched to producing hard information. An examination of the distribution of firms that do not prepare annual budgets or use external auditors reveals that the proportion of firms not producing hard information is only slightly higher amongst affected firms. 40% of unaffected firms do not prepare annual budgets compared to 49% of affected firms. Similarly, 30% of unaffected firms do not use external auditors compared to 41% of affected firms. Within the group of affected firms, “soft” information firms have a higher share of domestic equity, are less likely to be run by a manager with secondary school or higher education and were less likely to export in the pre-crisis period. In addition, affected “soft” information firms were significantly smaller and younger than affected “hard” information firms in the pre-crisis period. These pre-crisis differences support the claim that the current distribution of affected firms producing hard information is unlikely to have changed much from the pre-crisis distribution. I exclude insiders and firms with outstanding debt at closure in order not to confound the looting and information views.

Further, the information view suggests that banks would ration credit to firms as a function of the information they had on the firms. Therefore I would expect that firms that lost a banking relationship to be more credit constrained than similar unaffected firms. Using contemporaneous credit data I test directly for whether affected firms report being more credit constrained. Firms that were currently borrowing were asked if they would be willing to borrow more at the prevailing cost of credit. I extend the traditional definition of being credit constrained by including firms that had a loan application rejected in the post crisis period as these are revealed credit constrained firms. Assuming that information deficits resulting from the banking crisis are not quickly erased, the loss of a banking relationship is a good instrument for the strength of a firm’s current relationships.<sup>50</sup>

The basic framework in section 3 well suited to test the predictions outlined above. In order to do this I include interactions of  $T$  with the covariates outlined above to test the various implications of the looting and information views. To test the validity of the information view, I drop the set of potential looting firms from the sample. I use the specification below to test the plausibility of each explanation.<sup>51</sup>

$$y_{ijt} = \alpha_i + \mu_{jt} + after * \beta + after * T_i * \delta + after * T_i * Z_i * \pi + X_{ijt}\gamma + \varepsilon_{ijt} \quad (4)$$

where  $Z_i$  represents a set of firm characteristics and the coefficient set  $\pi$  captures the average differential change in log firm size between affected firms with the characteristics  $Z$  and affected firms that do not have the attributes captured by  $Z$ . *after* is a dummy that takes the value of 1 if  $t > 1999$  and 0 otherwise. The other variables are as defined in section 3.

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by foreign banks. Mian (2003) argues that foreign firms rely primarily on “hard” information as the basis for their lending decisions while domestic banks are better able to deal use soft information.

<sup>50</sup>95% of the firms that lost a banking relationship either started a new relationship or had continuous banking relationships with non-failed banks.

<sup>51</sup>I drop the separate coefficients for each post crisis year in this general specification.

To test the prediction of the information view that affected firms are more likely to report being credit constrained, I run the following logit analysis.

$$\Pr(c_i = 1) = X_i\beta + T_i\gamma + T_i * R_i\tau + \varepsilon_i \quad (5)$$

where  $c_i$  is the self-reported credit constrained status for firm  $i$ ,  $\varepsilon_i$  represents the residual which includes unobservable firm characteristics,  $X$  captures observable firm characteristics thought to affect demand for credit such as firm age and assets.  $X$  also includes covariates along which affected and unaffected firms differ such as business group affiliation and ownership by entrepreneurs of Asian descent.  $T$  is a dummy capturing whether the firm lost a lending relationship or not. I include interactions of covariates  $R$  that capture the capacity of firm  $i$  to produce hard information and test for differential effects within the affected firms. The identification of  $\gamma$  and  $\tau$  requires that

$$E[T_i\varepsilon_i] = 0$$

However, given that these parameters are estimated in the cross section, residual differences in unobservables between affected and unaffected firms could potentially bias these estimates.

## 5.1 Looting view results

Using the crude proxies identified above, I estimate the effect of the banking crisis on insiders, firms with outstanding debt at closure of the banks and an instrument for potential looters. Table 6 presents results of specifications which control for firm fixed effects, sector-year fixed effects and a time trend. Clustered standard errors at the firm level are shown in square brackets. The coefficient on the interaction between  $T$ , and a dummy capturing insiders is negative, large but not significantly different from zero in columns (1) and (3). The point estimate is large and of the expected sign. The size of coefficient in column (1) suggests that relative to non-looting firms, the change in average log firm size of insider firms between the pre- and post-crisis period is -24%. However, owing to the small number of insiders in the sample, it is imprecisely estimated (p-value of 0.14). The coefficient on the interaction between  $T$  and outstanding debt status is negative but imprecisely estimated in column (2). The size of the point estimate is slightly larger than half the corresponding estimate for insider firms in column (1). The lack of precision of this point estimate is likely to be driven by sample size problems. Controlling for both insider and outstanding debtor status in column (3) the size of the coefficient for insiders remains very large. Insiders with outstanding debt decline by 32% between the pre and post-crisis periods while debtors decline by an average of 11% relative to affected non-looting firms. In column (4), I include a dummy for firms with more than the median level of outstanding debt. 60% of all insiders are large debtors by this definition.<sup>52</sup> The coefficient on the interactions with insiders remains large and of the same magnitude as in columns (1) and (3). Relative to large debtors, average log employment of insider firms declines by nearly 21%. Average log employment of large debtors declines by approximately 15% relative to affected non-looting firms, while log employment of

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<sup>52</sup>The median level of outstanding debt is equivalent to \$60,000 1998 dollars. The mean level of outstanding debt is approximately \$250,000 1998 dollars.



small debtors declines by only 8%. In column (5), the coefficient on the interaction between  $T$  and a variable capturing the shared ethnicity/region of birth of lenders and borrowers is positive and insignificant. One possible explanation for this result is that the measure used relies on region of birth which is likely to be a weak measure of close ties. The size and sign of the coefficient on insiders is consistent with the looting view. Even after controlling for large debtor status, the independent effect of being an insider remains large. It is much less clear whether all firms with outstanding debt or only the firms with large amounts of outstanding debt meet the predictions of the looting view. On balance, these results do not dispute the claim that a good fraction of these banks' lending was being looted. Anecdotal evidence and official declarations support this claim. An examination of panel B in figure 1 suggests an alternative explanation. Insiders and firms with outstanding debt were experiencing a decline in the pre-crisis period.<sup>53</sup> These employment trends are consistent with a standard reverse causality story that precludes looting. Moreover, there is no obvious trend break in the employment trend of insider firms to suggest stronger evidence for the looting story. However, for this pure reverse causality story to be consistent, we require that the amount of outstanding debt for insider firms is under-reported. For the trend to be consistent with looting we would need to show that bank performance started to deteriorate well before the crisis precipitating the pre-crisis decline in employment we observe. There is a noticeable downward shift in the employment trend of firms with outstanding debt after the crisis, however.

## 5.2 Information view results

According to this view, the loss of information embodied in banking relationships implies that affected firms cannot easily borrow from uninformed lenders. I test the predictions of this view using the sample of potential non-looting firms; observations corresponding to firms identified as insiders or debtors are dropped. I use a dummy for firms with less than 10 employees in the pre-crisis period and dummies for whether firms do not prepare annual budgets or use external auditors as measures of the softness of a firm's information. In addition, I interact firm age with treatment status as a measure of the amount of information lost.<sup>54</sup> The testable hypotheses are that firms with softer information or firms that lose the most information should experience larger growth deficits relative to other affected firms. Table 7 presents results of the specifications in which I control for firm fixed effects, sector-year fixed effects and a time trend. Standard errors clustered at the firm level are shown in square brackets. Column (1) presents the main effect of losing a banking relationship. The sign of the coefficient underlines the basis for a non-looting interpretation of the results in this subset of the sample. In column (2) I include an interaction with a dummy that takes on the value of 1 if the firm had less than 10 employees in the pre-crisis period. The coefficient on the interaction is positive but insignificant at the usual levels. In column (3) I include an interaction with a dummy for firms that do not prepare an annual budget. The coefficient on the interaction is negative and significant at the 1% level for the unclustered regression but is insignificant when standard errors are clustered. Moreover, there is no

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<sup>53</sup>In this sense, like insider lending in early industrial New England, the bank failure might simply be the consequence of group/owner specific shock without the perverse incentives implied by the looting view (Lamoureaux, 1996).

<sup>54</sup>I do not have information on the length of the relationship with collapsed banks. However for unaffected firms the correlation between main bank relationship length and firm age is 0.4 (significant at the 1% level).

differential in growth rates between affected firms that prepare annual budgets and unaffected firms in the post crisis period. In column (4) where I control for the interaction with the external audit dummy, the coefficient is negative, large and significant at the 1% (unclustered) and 10% (clustered) levels respectively. As in column (3), the growth of affected firms that use external auditors is not significantly different from that of unaffected firms. In column (5), I include interactions with age and age squared. I expect that older firms are likely to have accumulated the largest amount of information with the collapsed banks, however, I also expect that older firms have a longer track record that is publicly observable. The coefficient on the interaction with age is negative and significant at the 5% level (unclustered) but is insignificant when standard errors are clustered. The coefficient on the squared term is positive and suggests a u-shaped profile that is minimized at 97 (out of sample). In column (6) I include all interactions of  $T$  with pre-crisis firm size, external audit use and firm age and age squared. The coefficient on the interaction with firm size is now positive and significant for the unclustered specification (insignificant when standard errors are clustered). The interaction with the external audit dummy remains negative and significant at the 1% (unclustered) and 10%(clustered) levels respectively. The coefficient on the interaction with age is still negative but only significant at the 10% level (unclustered) and insignificant at the usual levels when standard errors are clustered. Holding age constant, affected firms producing hard information do not have a statistically distinguishable growth path from unaffected firms.

The results above provide evidence for the information view. The coefficients on the annual budget and external audit dummies are in line with expectations that firms that do not produce hard information are likely to be the worst affected by the loss of a banking relationship. In fact, the average growth rate of affected firms that produce hard information is not significantly different from that of unaffected firms. The result survives when controls for firm size in the pre-crisis period are included, suggesting that the measure of soft information used is not picking up a firm size effect unrelated to the capacity to produce easily reproducible information. Similarly, the results on firm age are consistent with the information view, in that we would expect the loss of private information to be larger for firms that had longer relationships with the collapsed banks. Our expectation that the firm's age is a public measure of quality and therefore older firms should not be as affected as firms of intermediate age is not borne out in the data.<sup>55</sup>

I test an implication of the information view using results from a logit regression of firms self reported credit constrained status on  $T$ , interactions with  $T$  and other controls. Other controls include firm assets, measures of cash flow and covariates along which affected and unaffected firms differ. If information flow rates are finite, I should expect firms that lost a banking relationship to be more likely to be credit constrained. The results in table 8 confirm that this is indeed the case. Holding other factors constant, the odds ratio of being credit constrained increases 3 fold for affected firms relative to unaffected firms in column (1). The interaction term with the external audit dummy in columns (3) -(6) is negative, albeit insignificant. Firms that do not prepare annual budgets and lost a banking relationship are more likely to be credit constrained. The coefficient is, however, insignificant at the usual levels. Affected firms that were less than 10 years old in the pre-crisis period are less likely to be credit constrained (insignificant).

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<sup>55</sup>It is possible that in an environment of cozy relationships between politicians and firms, firm age is likely to be a noisy signal of quality.

## 6 Robustness Checks

In this section, I review a number of robustness checks: I investigate the plausibility of a cash flow disruption as an alternative interpretation of the results, relax the parallel trends assumption and test that the model used throughout the paper is not mis-specified. Although it does not appear likely that cash flow disruption adequately describes the time pattern of the effect, it is possible that in conjunction with a permanent loss in market share and/or trading partners, firms unable to access their retained earnings might suffer longer term effects. To test this interpretation, we would require information on the duration of the elapsed period between the closure of the banks and recovery of the firm's cash as well as the amount of retained earnings tied up in the failed banks.<sup>56</sup> Information on retained earnings in surviving banks would be necessary to assess the severity of this effect for affected firms. In the absence of such data, I exploit variation in the degree to which the closure of the banks was anticipated. Cooperative bank was shut quite suddenly. Greenland bank, on the other hand, was under statutory management for four months before it was shut. I include an interaction with  $T$  and a dummy for firms that were banking with Cooperative bank. I expect that the coefficient on this interaction to be negative if the cash flow disruption interpretation is correct. I also interact  $T$  with a dummy that takes the value of 1 if the firm belongs to a business group and 0 otherwise. I expect that business group affiliates would be able to accommodate a cash flow shock better than non-business group firms. Finally, I include an interaction with  $T$  of a dummy that takes on the value of 1 if the affected firm is owned by an entrepreneur of Asian descent and 0 otherwise.<sup>57</sup> I would therefore expect the effect of the banking crisis to be less severe for firms with good access to trade credit if the cash flow explanation has the greatest traction.

I drop observations corresponding to firms identified as insiders or firms with outstanding debt at closure. Table 9 presents the results. All specifications include controls for firm fixed effects, sector year fixed effects and a time trend. Standard errors clustered at the firm level are shown in square brackets. The interaction of  $T$  and a dummy capturing business group affiliation is negative, very small and insignificant in columns (1), (3) and (5). Business group affiliates that lost a banking relationship perform as poorly as other affected firms providing little support of the cash flow view. Affected firms owned by entrepreneurs of Asian descent (and arguably those with better access to trade credit) perform better than other affected firms. However, the point estimate is not significant. Finally, I include a dummy that takes the value of 1 if the affected firm banked with Cooperative bank. The point estimate is of the right sign albeit insignificant in columns (4) and (5). Widespread reports that Greenland bank, the other collapsed bank in the restricted sample, would not be closed as it was linked to influential politicians demand that I exercise caution in interpreting this result as evidence for/against the cash flow view since the distinction between anticipated and unanticipated closures is not clear. In sum, there does not appear to be strong evidence for a cash flow disruption effect.

The difference-in-differences estimation strategy relies on the assumption that the employment growth trajectory of affected and unaffected firms is the same. This parallel trends assumption is less likely to hold

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<sup>56</sup>The BOU arranged reimbursement of depositors so that small depositors were paid first and large depositors later. All depositors were paid within 3 months of the banks closing (BOU 1999).

<sup>57</sup>A recent literature on manufacturing in Africa suggests that a primary advantage of non-indigenous firms is their greater use of and access to trade credit (Fafchamps 1997, Fisman 1999).

when the firms that lose banking relationships are very different from unaffected firms. The evidence in figures 1 and 2 is suggestive of a valid parallel trends assumption. However, given that we only have 2 data points for the pre-crisis period, we cannot be certain of its validity. To overcome this problem we use propensity score matching techniques to create a more valid control group. In particular we run a probit model on a number the firm characteristics shown in table 2 and use the predicted propensity of losing a banking relationship as a control in the specification used in table 5. Table 11 presents the results of propensity score corrected analysis that is analogous to the specifications used in table 5. The standard errors in this table are clustered at the firm level. The estimated propensity score is significant in all specifications. The test that loss of a banking relationship does not matter is rejected at the usual levels in all specifications except (5). These results corroborate the validity of the parallel trends assumption.

The specifications used throughout this paper assume that unobservable fixed firm characteristics such as the ability of the owner affect the scale of operation and not the rate of change of the scale. This assumption would be invalid if unobservable characteristics entered a cumulative production function in which past inputs matter. To see this, assume that the correct specification is as follows

$$y_{ijt} = \sum_{s=0}^t X_{ijs} \beta_s + \sum_{s=0}^t \alpha_{is} \delta_s + \mu_{jt} + \sigma_t + \varepsilon_{ijt} \quad (6)$$

where  $\sigma_t$  represents a time trend and all other variables are defined as before. The specification above posits a relationship in which past inputs affect the firm's current scale of operation. In general, the conditions under which this specification can be identified are fairly stringent.<sup>58</sup> To see the implications of a cumulative production function, assume that only unobservable owner ability applied in the previous and current periods have an impact on current scale. We can write this simpler specification as

$$y_{ijt} = X_{ijt} \beta + \alpha_i \delta_{t-1} + \alpha_i \delta_t + \mu_{jt} + \sigma_t + \varepsilon_{ijt} \quad (7)$$

Taking first differences, we obtain the following specification

$$y_{ijt} - y_{ijt-1} = (X_{ijt} - X_{ijt-1}) \beta + \alpha_i (\delta_t - \delta_{t-2}) + (\mu_{jt} - \mu_{jt-1}) + (\sigma_t - \sigma_{t-1}) + (\varepsilon_{ijt} - \varepsilon_{ijt-1}) \quad (8)$$

Abstracting from omitted variable bias, specification 8 above implies that changes in the scale of operation are affected by unobservable fixed effects in the general case when  $\delta_t \neq \delta_{t-2}$ . This would imply that a fixed effects estimation in levels would not yield unbiased estimates. In particular, given the negative correlation between  $T$  and  $\alpha_i$  I would expect that a regression with the dependent variable in levels would bias results towards finding a negative impact of the crisis. Assuming that  $(\delta_t - \delta_{t-2})$  is constant for all  $t$ , then a fixed effects estimation in changes in level would produce unbiased estimates. I examine the extent to which mis-specification of this kind might be a problem for the models run above by using employment growth as the dependent variable in the following fixed effects specification

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<sup>58</sup>Either, we require data for every period since the firm was established or we would need to make assumptions that either inputs from distant periods did not affect current outcomes or changes in outcomes or that a lagged measure of  $y_{ijt}$  was a sufficient statistic for all past inputs (Wolpin and Todd, 2003).

$$g_{ijt} = \alpha_i + \mu_{jt} + Post * \beta_k + Post * T_i * \delta + X_{ijt}\gamma + Post_{it} * W_i + \varepsilon_{ijt} \quad (9)$$

where  $g_{ijt}$  is growth of firm  $i$  in sector  $j$  between  $t$  and  $t - 1$ ,  $Post$  is a dummy that takes the value of 1 in the post crisis period and 0 otherwise,  $X$  represents time varying firm characteristics and  $W$  represents a vector of covariates along which affected and unaffected differ significantly. Table 10 shows fixed effects estimates of  $\delta$ . Although the coefficient is estimated imprecisely, the sign and magnitude of the estimate is consistent with the results obtained using the levels specification. The average growth deficit of affected firms varies between 3.3% in the full sample to 7.1% when insiders and borrowers are dropped. This suggests that mis-specification is unlikely to be driving the results of section 4.

## 7 Discussion

We might worry that the size of the estimated impact of losing a banking relationship is too large relative to the depth of debt markets in an economy such as Uganda. To check the sensibility of the estimates, I calculate the average share of debt in firm value using the 1998 manufacturing firm survey carried out by the World Bank (Reinnika and Svensson, 2000). The average share of debt in total assets is 17%. Assuming no technological change, a depreciation rate of 5% and post-crisis investment rates of 10%, the proportion of growth explained by debt's share in firm value is 0.85%.<sup>59</sup> This is about 40% of the effect I find. However, this calculation assumes that the market share of an affected firms remains constant. Given that bank failure affects a minority of firms, we might expect a decline in market share to accompany the loss of a banking relationship and potentially deepen its effects. This possibility is corroborated by firms' responses to the question "why is your main challenger is more competitive than the you". 41% of affected firms state that a "better financial position" is one of the two most important reasons that their main challenger is more competitive compared to 31% amongst unaffected firms.<sup>60</sup>

The fact that 93% of current loans and 60% of credit lines are backed by physical collateral, questions the role of information in the financing decision. Information should have no role if credit is backed by sufficient collateral. However, a statement by the director of a leading foreign-owned bank in Uganda suggests an explanation

"Security (collateral) in Uganda can be difficult. There is little market for property outside Kampala and in the event of failure these properties prove difficult to sell. There is a propensity by some valuers to overvalue since valuation fees are based on the value attached to the property. Few valuers carry any, or sufficient, professional indemnity insurance on which banks can rely in the event of a dispute".

The same banker goes on to say that

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<sup>59</sup> Assuming no changes in productivity, growth of firm employment is equal to growth in firm capital in a constant returns to scale technology.

<sup>60</sup> Average current market share held by the unaffected firms is about 6.5% greater than affected firms, however, this is not statistically significant.

“..for banks to consider a financing proposal, cash flow forecasts and budgets are usually required. If customers are unable to produce them themselves, banks will usually look to a book-keeper or accountant to assist them”<sup>61</sup>

confirming the role for “hard” information in the lending decision and banks’ incentives to collect this information when necessary. Thin markets for collateral, and perverse incentives in the provision of valuation introduce uncertainty in the value of collateral and raise the importance of information in the lending decision.

The empirical strategy employed to test the plausibility of the information and looting views assumes that the choice of sample determines the predominant channel in operation. However, the fact that each view relies on close relationships between lenders and borrowers makes it difficult to draw clear implications using this classification. While the classification of insiders as potential looters is not controversial, including all outstanding debtors as looting firms, rules out the possibility that some of these firms had outstanding debt because they had exceeded the information threshold of the failed banks. In addition, relying on the use external auditors or preparation of annual budgets as evidence for the information view does not address the possibility that these firms do not keep good records because they were looting.<sup>62</sup> Similarly, the result that older firms experience larger declines in growth could reflect the fact that these are firms with more connections and hence greater access to loot. It is not possible, with the data that I have, to robustness of the looting and non-looting samples used above.

The time profile of the impact of losing a banking relationship is consistent with features of the credit market in the post crisis period. Figure 3 shows trends in asset holdings of commercial banks over the period relevant to this analysis. Growth of credit to the private sector between 1999 and 2000 is just under 2% compared to growth in government security holdings of over 50%. Credit to the private sector recovers between 2000 and 2001, registering a growth of nearly 30%.<sup>63</sup> This is also the year in which the growth difference between affected and unaffected firms is most pronounced. Whether this is further evidence for the information view depends on whether potential looting firms run down accumulated loot slowly so that the patterns in the credit market are only spuriously correlated to the employment trend.

## 8 Conclusion

Using the closure of four banks, I estimate the effect of losing a banking relationship on growth in firm employment. Controlling for firm fixed effects and sector-year effects, average annual growth over the 5 year period is 4.0% for the unaffected firms compared to virtually no growth for affected firms over the same period. For the set of potentially non-looting firms, I find no evidence of firm decline leading to the failure of the banks. For this restricted sample, the average annual growth rate of affected firms in the post

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<sup>61</sup>Frank Griffiths, Managing Director, Barclays Bank in “Proceedings of the Symposium on Modalities for Financing SMEs in Uganda” UNCTAD 2002.

<sup>62</sup>Including an interaction of shared ethnicity of firm owners and bankers does not change the result that soft information firms have the largest declines in employment.

<sup>63</sup>In the post crisis period growth in treasury bills holding far outstrips growth in credit to the private sector. The build-up in government borrowing during this time to pay for the costs of the banking crisis and increased insecurity, is likely to have enhanced the effects of relationship loss on affected firms as it reduced the amount of loanable funds in the banking system.

crisis period is 2.3% less than that of unaffected firms. I investigate two potential explanations of these results. I find evidence that insider firms and firms with outstanding debt at closure experience the largest growth declines relative to other affected firms. Relative to the set of potential non-looting firms, insider firms contract by 24% in the post crisis period. I interpret this as evidence that these firms were engaged in looting of the banks in line with anecdotal and official declarations. For the set of potential non-looting firms, I find that affected firms that do not use external auditors or prepare annual budgets experience the largest growth deficit. In fact, the growth path of affected firms that produce “hard” information is not statistically distinguishable from that of unaffected firms. Older firms that lost a banking relationship experience larger growth declines relative to younger affected firms. Finally, using self-reported evidence on credit constrained status, I find that affected firms are more likely to report being credit constrained. I interpret this as evidence for the information view of bank-mediated lending.

To the extent that banks are repositories of borrower information and we find some evidence that this is the case, they play an important role in correcting an important market imperfection. This is particularly so in economies where information flows are poor. Given the fragility of the financial system implied by rapid financial liberalization and weak regulatory institutions, there is a need for long-lived stores of information to mitigate the effects of banking crises. The existence of credit bureaux would go a long way in preserving the records of firms that are most vulnerable to bank failure. The tentative evidence in support of the information view provides as yet an unexplored rationale for more rigorous banking regulation. More effective banking regulation that polices looting preserves the accumulation of borrower information and is likely to enhance the developmental role of banking advanced by Lewis (1950) and Gershenkron (1962).

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## APPENDIX

**Table 1: Basic sample structure**

SIZECLASS	Agroindustry	Chemicals and Paints	Construction Materials	Furniture	Metals	Paper, publishing and Printing	Plastic	Textile and Leather Product	Wood	Grand Total
11 - Small	43	7	5	17	13	13	3	7	4	112
	18.5	16	17.6	18.3	18.9	20.9	22.0	22.3	19.3	19.0
12 - Medium	19	6	3	3	9	2	5	3	1	51
	63.7	60.0	74.7	58.3	67.8	75.0	59.2	59.3	58.0	63.9
13 - Large	32	4	1	1	7	4	2	5	1	57
	507.5	199.3	280.0	135.0	180.6	256.5	165.0	343.6	238.0	386.5
CENTRAL	94	17	9	21	29	19	10	15	6	220
	194.1	74.6	65.8	29.6	73.1	76.2	69.2	136.8	62.2	124.6
21 – Small	7		4	8	2			2	1	24
	18.7		15.5	13.8	14.0			23.5	10.0	16.2
22 – Medium	1						1	2		4
	83						71	60.5		68.75
23 – Large								4		4
								204.25		204.25
NORTH-EAST	8		4	8	2		1	8	1	32
	26.8		15.5	13.8	14.0		71.0	123.1	10.0	46.3
31 – Small	7		3	6	2	1		2	2	23
	12.6		25.0	12.0	15.0	27.0		13.0	26.0	16.1
32 – Medium	2	1	1		1				1	6
	55	85	82		60				50	64.5
33 – Large	19		2		1			1		23
	709.3		231.5		230.0			112.0		620.9
SOUTH-WEST	28	1	6	6	4	1		3	3	52
	488.4	85.0	103.3	12.0	80.0	27.0		46.0	34.0	289.2
TOTAL	130	18	19	35	35	20	11	26	10	304
	247.2	75.2	67.1	22.9	70.5	73.8	69.4	122.1	48.5	144.5

Notes for Table 1:

The table above shows the structure of the sample. The sample frame included all manufacturing firms with more than 10 employees. 3 regions of activity were chosen: Central –1; North-East – 2 and South-West-3. The number of firms is shown in the upper cell with the average firm size in that cluster in the cell below. Small firms are defined as firms with less than 50 employees, medium firms have between 50-100 employees and large firms have 100 or more employees.

Table 2: Means of Selected Firm Characteristics

	Unaffected firms	Affected firms	Difference
	Mean	Mean	Mean
Current age of firm	15.21 (1.09) [10.00]	18.66 (2.34) [11.00]	3.45 (2.58)
Fraction of firms in Agro-industry <sup>a</sup>	0.36 (0.04)	0.47 (0.06)	0.11 (0.07)
Fraction firms banking with distressed banks in post crisis period	0.36 (0.04)	0.33 (0.06)	-0.03 (0.07)
Average firm employment in pre-crisis period	115.21 (50.84) [11.25]	71.81 (23.29) [11.00]	-43.41 (55.96)
Fraction of firms affiliated with a business group	0.34 (0.04)	0.26 (0.05)	-0.08 (0.07)
Fraction of firms formerly owned by government	0.09 (0.02)	0.07 (0.03)	-0.02 (0.04)
Average percentage of firm owned domestic nationals	69.81 (3.60)	82.96 (4.41)	13.14 (5.69)*
Fraction of firms with owners of Asian descent. <sup>b</sup>	0.24 (0.04)	0.11 (0.04)	-0.13 (0.05)*
Fraction of firms with owners of African descent. <sup>c</sup>	0.49 (0.04)	0.70 (0.06)	0.21 (0.07)**
Fraction of firms with owners whose region of birth is Central Uganda	0.30 (0.04)	0.41 (0.06)	0.12 (0.07)+
Fraction of firms whose manager have secondary school or tertiary education	0.79 (0.03)	0.67 (0.06)	-0.11 (0.07)+
Number local, durable pre-crisis banking relationship	0.55 (0.06)	0.60 (0.10)	0.05 (0.11)
Proportion of firms with no banks in immediate post-crisis period	0.11 (0.03)	0.19 (0.05)	0.07 (0.05)
Fraction of firms exporting pre-crisis	0.17 (0.03)	0.13 (0.04)	-0.05 (0.05)
Fraction of firms in heavy industry/high import industries <sup>d</sup>	0.30 (0.04)	0.14 (0.04)	-0.16 (0.06)**
Fraction firms reporting major trading partner that lost banking relationship	0.02 (0.01)	0.12 (0.04)	0.1 (0.04)*
Number of firms	<b>149</b>	<b>70</b>	

**Notes:**

Standard errors in parentheses, median in square brackets. Significantly different from zero at 90 (+); 95 (\*); 99 (\*\*) percent confidence. The sample of firms used in this comparison includes firms established before 1999 (the crisis year) and had a banking relationship between 1997 and 2002.

<sup>a</sup> Agro industry includes non-food agricultural products processing such as tea, coffee or tobacco, food processing and beverages. This sector accounts for nearly 40% of the sample.

<sup>b</sup> Owners of Asian descent are predominantly from India and Pakistan.

<sup>c</sup> Owners of African descent defined as individuals/families from Sub-Saharan Africa.

<sup>d</sup> High import Industries is defined as the proportion of firms in sectors that import more than 20% of their inputs directly. These include Chemicals & paints, metals, plastics and publishing & printing

Table 3: Test for reverse causality

	Dependent variable, Employment growth 1997-98				Dependent variable, Growth startup-1997	
	(1)	(2)	(3)	(4)	(5)	(6)
Firm lost relationship	-0.002 (0.068)	0.012 (0.080)	-0.039 (0.056)	-0.036 (0.062)	0.010 (0.062)	0.000 (0.061)
Insider	-0.251 (0.116)*	-0.227 (0.119)+	-0.055 (0.111)	-0.048 (0.115)	-0.177 (0.131)	-0.192 (0.145)
Borrower at closure		-0.078 (0.094)		-0.019 (0.069)		0.047 (0.134)
Other controls	N	N	Y	Y	Y	Y
Observations	161	161	157	157	143	143
R-squared	0.08	0.09	0.18	0.18	0.06	0.06

Notes for Table 3

Robust standard errors in parentheses. Significantly different from zero at 90 (+); 95 (\*); 99 (\*\*) percent confidence. Dependent variable defined as growth in the pre-crisis period. Reported coefficients estimated from an OLS regression of pre-crisis growth on treatment dummy and other controls. Other controls include a dummy if firm was an exporter in the pre-crisis period, a dummy for firms with owners of Asian descent, a dummy for business group affiliation and the natural log of firm age in 1997.

Table 4: Summary Statistics

Variable	N	Mean	Median	Minimum	Maximum
Log employment	1010.00	2.83	2.71	0.00	8.70
Log sales	604.00	19.47	19.19	12.82	25.43
Credit constrained	219.00	0.09	0.00	0.00	1.00
Access to loans or overdrafts, current	219.00	0.38	0.00	0.00	1.00
Firm age	219.00	16.31	10.00	5.00	93.00
Firm affiliated to a business group	219.00	0.31	0.00	0.00	1.00
Log (plant equipment + buildings owned)	205.00	19.31	19.41	10.82	26.10
Firm owner of Asian descent	219.00	0.20	0.00	0.00	1.00
Firm controlled by foreigners	219.00	0.22	0.00	0.00	1.00
Firm owned by directors/owners of closed banks	219.00	0.02	0.00	0.00	1.00
Lost all banking relationships or had none pre-crisis <sup>a</sup>	219.00	0.14	0.00	0.00	1.00
Firm banking with Cooperative Bank	219.00	0.19	0.00	0.00	1.00
Firm had outstanding debt with collapsed banks	219.00	0.07	0.00	0.00	1.00
Related firm/bank owner of same ethnicity	219.00	0.17	0.00	0.00	1.00
Firm with less than 10 employees in 1998	196.00	0.49	0.00	0.00	1.00
Firm less than 5 years old in 1998	219.00	0.51	1.00	0.00	1.00
Firms does not prepare an annual budget	219.00	0.42	0.00	0.00	1.00
Firms does not use external auditors	219.00	0.32	0.00	0.00	1.00
Firm not located in capital city	219.00	0.29	0.00	0.00	1.00
Firms did not own any land in the pre-crisis period	219.00	0.37	0.00	0.00	1.00
Firms did not own any buildings in the pre-crisis period	219.00	0.52	1.00	0.00	1.00

Notes for Table 4

The summary statistics are generated using the unrestricted sample which includes all firms that were established before 1999 and had a banking relationship between 1997-2002.

<sup>a</sup> Lost all banking relationships is a dummy variable that is 1 if the firm lost all its banking relationships in 1999 or had no banking relationship in 1999 and 0 otherwise.

Table 5: Testing Hypothesis: Does loss of a banking relationship matter

	Dependent Variable: Log Permanent employees					
	Full Sample		No Insiders		No Insiders/borrowers	
	(1)	(2)	(3)	(4)	(5)	(6)
1998 *Firm lost relationship	-0.081 (0.079) [0.080]	-0.102 (0.079) [0.075]	-0.065 (0.082) [0.085]	-0.092 (0.082) [0.080]	-0.056 (0.090) [0.100]	-0.082 (0.090) [0.092]
2000 *Firm lost relationship	<b>-0.167</b> <b>(0.080)*</b> <b>[0.086]+</b>	<b>-0.166</b> <b>(0.080)*</b> <b>[0.085]+</b>	<b>-0.136</b> <b>(0.083)</b> <b>[0.090]</b>	<b>-0.146</b> <b>(0.083)+</b> <b>[0.089]</b>	<b>-0.098</b> <b>(0.091)</b> <b>[0.099]</b>	<b>-0.104</b> <b>(0.091)</b> <b>[0.098]</b>
2001 *Firm lost relationship	<b>-0.278</b> <b>(0.080)**</b> <b>[0.118]*</b>	<b>-0.268</b> <b>(0.080)**</b> <b>[0.123]*</b>	<b>-0.246</b> <b>(0.083)**</b> <b>[0.124]*</b>	<b>-0.250</b> <b>(0.083)**</b> <b>[0.128]+</b>	<b>-0.229</b> <b>(0.090)*</b> <b>[0.142]</b>	<b>-0.228</b> <b>(0.091)*</b> <b>[0.149]</b>
2002 *Firm lost relationship	<b>-0.240</b> <b>(0.080)**</b> <b>[0.125]+</b>	<b>-0.220</b> <b>(0.080)**</b> <b>[0.130]+</b>	<b>-0.197</b> <b>(0.083)*</b> <b>[0.129]</b>	<b>-0.195</b> <b>(0.083)*</b> <b>[0.134]</b>	<b>-0.183</b> <b>(0.090)*</b> <b>[0.148]</b>	<b>-0.176</b> <b>(0.091)+</b> <b>[0.156]</b>
Age controls	N	Y	N	Y	N	Y
Other controls	Y	Y	Y	Y	Y	Y
Observations	902	898	882	878	834	830
Number of Firms	186	186	182	182	172	172
R-squared	0.17	0.18	0.17	0.18	0.17	0.19
F-test: relation loss not important	4.63	4.07	3.25	3.25	2.47	2.32
P-value	0.00	0.01	0.02	0.02	0.06	0.07
P-value (clustered standard errors)	0.04	0.04	0.07	0.05	0.15	0.12

Notes for Table 5

Standard errors in parentheses. Clustered standard errors at firm level in square brackets. Significantly different from zero at 90 (+); 95 (\*); 99 (\*\*) percent confidence. Dependent variable defined as log of firm's permanent employees. Reported coefficients estimated from a fixed effects regression of log employment on treatment dummy and other controls. Other controls include sector-year fixed effects, year dummies and interactions of *After* with a dummy for indigenous firms, and firms that had less than 10 employees in the pre-crisis period. F-test performs the joint test that growth differentials between affected and unaffected firms in the post crisis period are significantly different from zero. Age controls include age of the firm and age squared.



Table 6: Test of the Looting Hypothesis

	Dependent Variable: Log of Permanent employees				
	(1)	(2)	(3)	(4)	(5)
Firm Lost Relationship*After	-0.138 (0.050)** [0.104]	-0.126 (0.054)* [0.118]	-0.203 (0.066)** [0.156]	-0.118 (0.054)* [0.119]	-0.162 (0.070)* [0.174]
Firm Lost Relationship*After*Insider	-0.241 (0.157) [0.163]			-0.211 (0.160) [0.167]	-0.193 (0.161) [0.163]
Firm Lost Relationship*After*Borrower		-0.131 (0.100) [0.153]		-0.108 (0.101) [0.154]	-0.111 (0.101) [0.152]
Lost Relationship*After*same ethnicity			0.093 (0.081) [0.180]		0.083 (0.082) [0.181]
Age/Other controls	Y	Y	Y	Y	Y
Observations	962	962	962	962	962
Number of Firms	209	209	209	209	209
R-squared	0.18	0.18	0.17	0.18	0.18
F-test: sum of interaction =0	2.34	1.73	1.31	1.74	1.50
Prob>F	0.13	0.19	0.25	0.18	0.21
Prob> F (clustered standard errors)	0.14	0.39	0.61	0.30	0.41

Notes for Table 6

Standard errors in parentheses. Clustered standard errors at firm level in square brackets. Significantly different from zero at 90 (+); 95 (\*); 99 (\*\*) percent confidence. Dependent variable defined as log of the firm's permanent employment. Reported coefficients estimated from a fixed effects regression of log employment on treatment dummy and other controls. Other controls include sector-year fixed effects and year dummies. F-test performs the joint test that sum of triple of interaction and double interaction with *After* are significantly different from zero.

Insider is a dummy that takes on the value of 1 if the firm is owned by directors/owners of the collapsed bank and 0 otherwise. Borrower takes on the value of 1 if the firm had outstanding debt with the collapsed bank at closure and 0 otherwise. Same ethnicity takes on the value of 1 if the firm's owner comes from the same region/is of same ethnicity as directors/owners of collapsed bank and 0 otherwise. Age controls include age of the firm and age squared.

Table 7: Test of the Information channel

	Dependent variable: Log of Permanent employees							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Firm Lost Relationship*After	-0.172 (0.084)* [0.222]	-0.168 (0.066)* [0.129]	0.041 (0.078) [0.137]	0.080 (0.072) [0.106]	-0.107 (0.098) [0.174]	-0.113 (0.071) [0.153]	-0.093 (0.084) [0.184]	0.134 (0.104) [0.168]
Firm lost relationship*After*Firm < 10 employees pre-crisis	0.076 (0.103) [0.259]							
Firm lost relationship*After*Firm not located in capital		0.163 (0.124) [0.311]						
Firm lost relationship*After*Firm does not prepare budget			-0.305 (0.111)** [0.223]					-0.099 (0.125) [0.222]
Firm lost relationship*After*No external auditors				-0.444 (0.112)** [0.238]+				-0.388 (0.126)** [0.250]
Firm lost relationship*After*Firm < 10 yrs old pre-crisis					-0.012 (0.117) [0.226]			
Firm lost relationship*After*Firm did not own land pre-crisis						-0.027 (0.111) [0.226]	-0.003 (0.152) [0.318]	-0.009 (0.151) [0.281]
Firm lost relationship*After*Firm did not own bldg pre-crisis							-0.054 (0.149) [0.325]	-0.066 (0.147) [0.283]
Age, age squared and other controls	Y	Y	Y	Y	Y	Y	Y	Y
Observations	832	894	894	894	894	894	894	894
Number of Firms	173	195	195	195	195	195	195	195
R-squared	0.19	0.19	0.19	0.20	0.18	0.19	0.19	0.21

Notes for Table 7

Standard errors in parentheses. Clustered standard errors at firm level in square brackets. Significantly different from zero at 90 (+); 95 (\*); 99 (\*\*) percent confidence. Dependent variable defined as log of the firm's permanent employment. Reported coefficients estimated from a fixed effects regression of log employment on treatment dummy and other controls. Other controls include sector-year fixed effects and year dummies. All insider firms and firms with outstanding debt at closure are dropped.

Table 8: Maximum Likelihood Logistic estimation of being credit constrained

	Dependent variable: Credit Constrained					
	(1)	(2)	(3)	(4)	(5)	(6)
Firm lost relationship	1.092 (0.537)*	1.750 (0.788)*	1.885 (0.783)*	2.398 (1.033)*	1.896 (1.035)+	3.027 (1.265)*
No other durable banking relationships		0.944 (1.000)		1.658 (1.375)	1.590 (1.398)	1.333 (1.424)
Firm lost relationship*No durable bank in pre-crisis period		-0.666 (1.270)		-0.987 (1.861)	-0.665 (1.892)	-0.108 (1.933)
Firm has no external audit			-1.320 (1.263)	-1.742 (1.849)	-1.603 (2.065)	-1.663 (2.048)
Firm lost relationship*Firm has no external audit			-0.499 (1.583)	-0.767 (2.419)	-1.631 (2.678)	-1.972 (2.422)
Firm does not prepare budget					-0.504 (1.400)	-0.638 (1.510)
Firm lost relationship*Firm does not prepare budget					1.783 (1.573)	1.681 (1.738)
Firm < 10 yrs old pre-crisis						-1.518 (1.961)
Firm lost relationship*Firm < 10 yrs old pre-crisis						-1.259 (1.246)
Other controls	N	N	N	Y	Y	Y
Size class, sector dummies	Y	Y	Y	Y	Y	Y
Observations	202	174	174	152	152	152
Log likelihood	-52.88	-44.62	-43.76	-36.70	-36.08	-35.01
Pseudo R2	0.05	0.16	0.18	0.25	0.26	0.28

Notes for Table 8

Robust standard errors in parentheses. Significantly different from zero at 90 (+); 95 (\*); 99 (\*\*) percent confidence. The dependent variable, Credit constrained, is a dummy that takes on the value 1 if firms that are currently borrowing wanted to borrow more, if a firm has had a loan application rejected recently and 0 otherwise. Reported coefficients estimated from a maximum likelihood logit of credit constrained on treatment dummy, interactions, log firm age, sector and size class dummies and other controls. No durable banks pre-crisis is a dummy variable that is 1 if the firm lost all its banking relationships in 1999 or had no banking relationship in 1999 and 0 otherwise. Other firm controls include log of firm assets, a dummy that takes the value of 1 if firm is controlled by foreigners and zero otherwise, a business group dummy and an Asian owner dummy.

Table 9: Testing the Cash flows hypothesis

	Dependent Variable: Log Permanent employment				
	(1)	(2)	(3)	(4)	(5)
Firm lost relationship*After	-0.105 (0.066) [0.163]	-0.118 (0.059)* [0.129]	-0.116 (0.068)+ [0.168]	-0.071 (0.088) [0.170]	-0.078 (0.096) [0.195]
Firm lost relationship*After*Business group affiliate	-0.014 (0.123) [0.204]		-0.010 (0.124) [0.207]		-0.009 (0.124) [0.208]
Firm lost relationship*After*Owner of Asian descent		0.173 (0.195) [0.253]	0.194 (0.196) [0.274]		0.190 (0.196) [0.264]
Firm lost relationship*After*Cooperative bank				-0.062 (0.103) [0.219]	-0.057 (0.103) [0.217]
Age/Other controls	Y	Y	Y	Y	Y
Observations	894	894	894	894	894
Number of Firm	195	195	195	195	195
R-squared	0.18	0.18	0.18	0.18	0.18
F-test: sum of interactions =0	0.62	1.67		0.36	0.43
Prob>F	0.43	0.20		0.55	0.73
Prob > F (clustered standard errors)	0.64	0.31		0.78	0.88

Notes for Table 9.

Standard errors in parentheses. Clustered standard errors at firm level in square brackets. Significantly different from zero at 90 (+); 95 (\*); 99 (\*\*) percent confidence.

Dependent variable defined as log of the firm's permanent employment. Reported coefficients estimated from a fixed effects regression of log employment on treatment dummy and other controls. Other controls include age, age squared, sector-year controls and year dummies.

F-test performs the joint test that sum of triple of interaction and double interaction with *After* are significantly different from zero.

All insider firms and firms with outstanding debt at closure are dropped.

Table 10: Changes specification

	Dependent variable: employment growth			
	(1)	(2)	(3)	(4)
Firm lost relationship*After	-0.033 (0.056) [0.086]	-0.040 (0.057) [0.090]	-0.046 (0.059) [0.098]	-0.071 (0.064) [0.120]
Borrowers included	Y	Y	Y	N
Insiders included	Y	Y	N	N
Age, age squared	N	Y	Y	Y
Other controls	Y	Y	Y	Y
Observations	714	714	698	660
Number of Firms	186	186	182	172
R-squared	0.08	0.08	0.08	0.09
F-test: Treatment*After =0	0.34	0.49	0.61	1.22
Prob>F	0.56	0.48	0.44	0.27
Prob>F (clustered standard errors)	0.70	0.66	0.64	0.55

Notes for Table 10.

Standard errors in parentheses. Clustered standard errors at firm level in square brackets. Significantly different from zero at 90 (+); 95 (\*); 99 (\*\*) percent confidence. Dependent variable defined as growth in the firm's permanent employment. . Reported coefficients estimated from a fixed effects regression of employment growth on treatment dummy and other controls. Other controls include year dummies, interactions of *After* with a dummy for indigenous owned firms, and firms that had less than 10 employees in the pre-crisis period and sector-year controls. F-test performs the test that growth of affected firms significantly different from zero. Age controls include age of the firm and age squared.

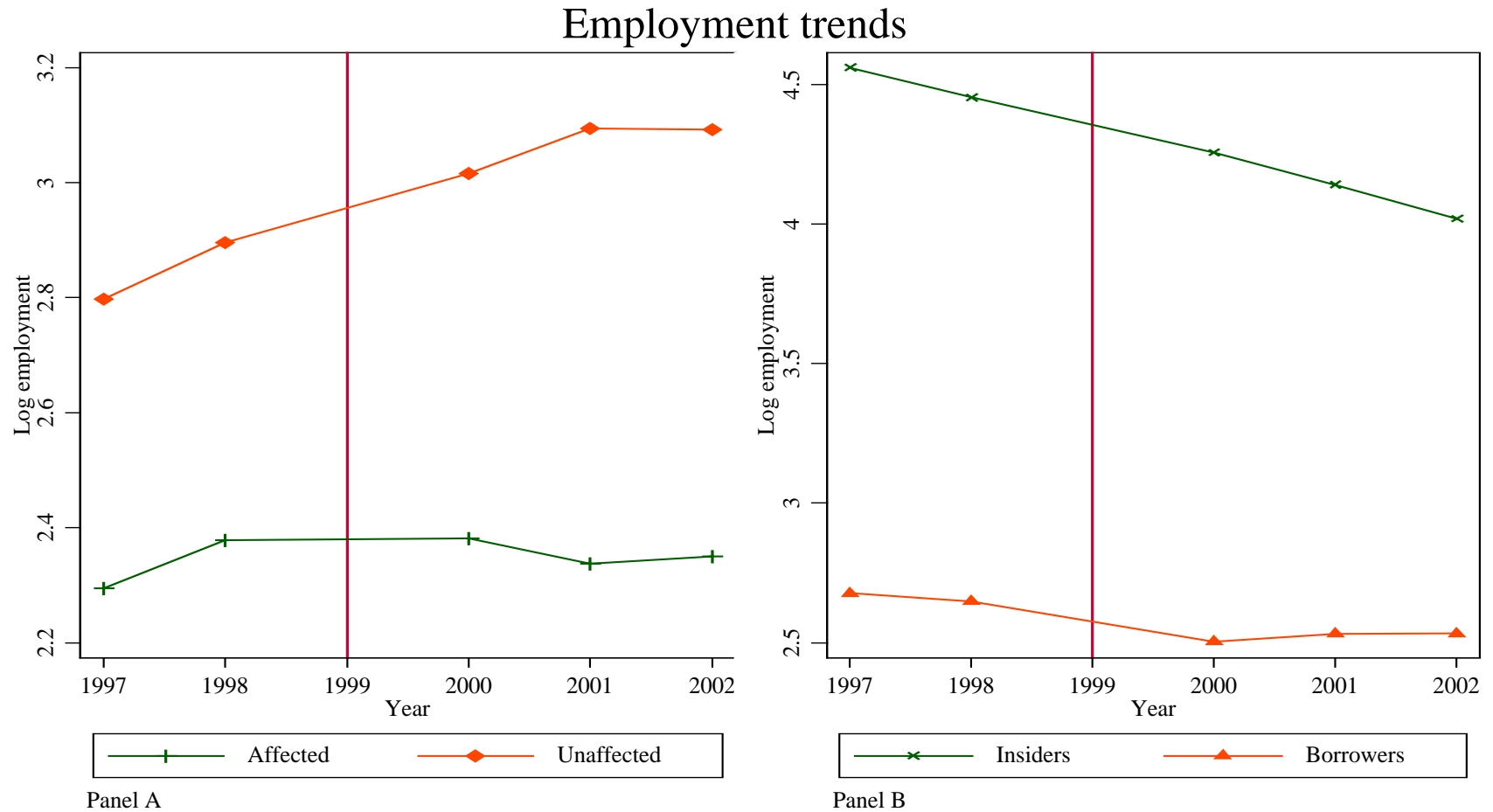
Table 11: Testing Hypothesis: Does loss of a banking relationship matter

	Dependent Variable: Log Permanent employees					
	Full Sample		No Insiders		No Insiders/borrowers	
	(1)	(2)	(3)	(4)	(5)	(6)
1998 *Firm lost relationship	-0.027 (0.075)	-0.053 (0.070)	-0.011 (0.079)	-0.042 (0.075)	0.005 (0.094)	-0.024 (0.088)
2000 *Firm lost relationship	<b>-0.149</b> <b>(0.084)+</b>	<b>-0.152</b> <b>(0.082)+</b>	<b>-0.117</b> <b>(0.087)</b>	<b>-0.132</b> <b>(0.086)</b>	<b>-0.072</b> <b>(0.095)</b>	<b>-0.084</b> <b>(0.093)</b>
2001 *Firm lost relationship	<b>-0.282</b> <b>(0.118)*</b>	<b>-0.274</b> <b>(0.121)*</b>	<b>-0.249</b> <b>(0.122)*</b>	<b>-0.257</b> <b>(0.126)*</b>	<b>-0.230</b> <b>(0.140)</b>	<b>-0.233</b> <b>(0.145)</b>
2002 *Firm lost relationship	<b>-0.239</b> <b>(0.124)+</b>	<b>-0.220</b> <b>(0.127)+</b>	<b>-0.195</b> <b>(0.128)</b>	<b>-0.196</b> <b>(0.132)</b>	<b>-0.179</b> <b>(0.146)</b>	<b>-0.175</b> <b>(0.151)</b>
Estimated propensity score	<b>-11.425</b> <b>(0.000)**</b>	<b>-4.450</b> <b>(0.820)**</b>	<b>1.444</b> <b>(0.079)**</b>	<b>-5.766</b> <b>(1.086)**</b>	<b>2.214</b> <b>(0.043)**</b>	<b>-5.965</b> <b>(1.715)**</b>
Age controls	N	Y	N	Y	N	Y
Other controls	Y	Y	Y	Y	Y	Y
Observations	893	889	873	869	825	821
R-squared	0.97	0.97	0.97	0.97	0.97	0.97
F-test: relation loss not important	2.83	3.04	2.53	2.83	1.89	2.10
P-value (clustered standard errors)	0.04	0.03	0.06	0.04	0.13	0.10

Notes for Table 11

Clustered standard errors in parentheses. Significantly different from zero at 90 (+); 95 (\*); 99 (\*\*) percent confidence. Dependent variable defined as log of firm's permanent employees. Reported coefficients estimated from a fixed effects regression of log employment on treatment dummy and other controls. Other controls include sector-year fixed effects, year dummies and interactions of *After* with a dummy for indigenous firms, and firms that had less than 10 employees in the pre-crisis period. F-test performs the joint test that growth differentials between affected and unaffected firms in the post crisis period are significantly different from zero. Age controls include age of the firm and age squared.

Figure 1:

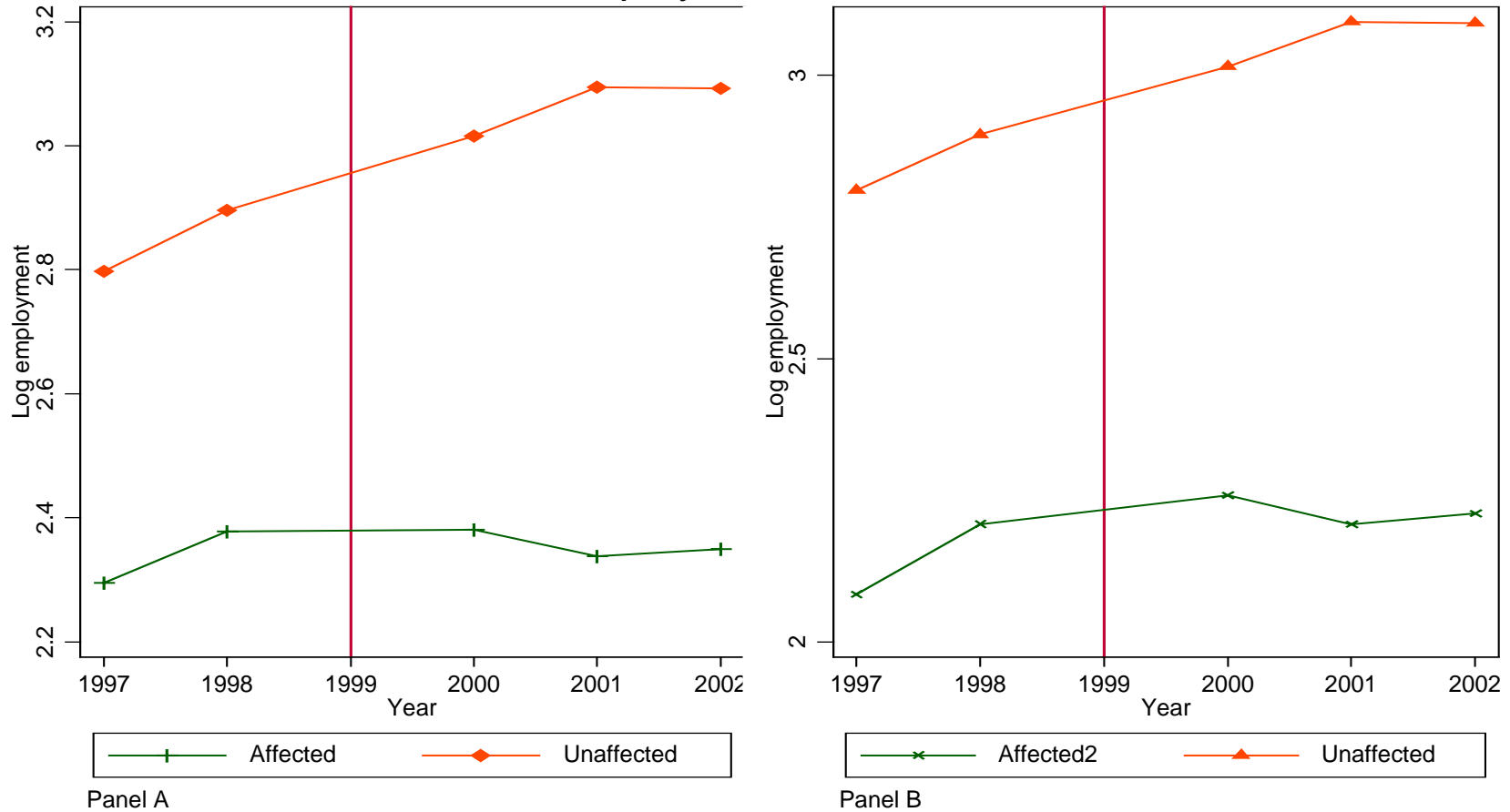


Source: RPED Uganda 2002

**Notes for Figure1:** The sample of firms used to construct this figure includes all firms that were established before 1999 and had a banking relationship between 1997-2002 and had all 5 years of employment data. 9 firms that lost a banking relationship in 1998 are dropped.

Figure 2:

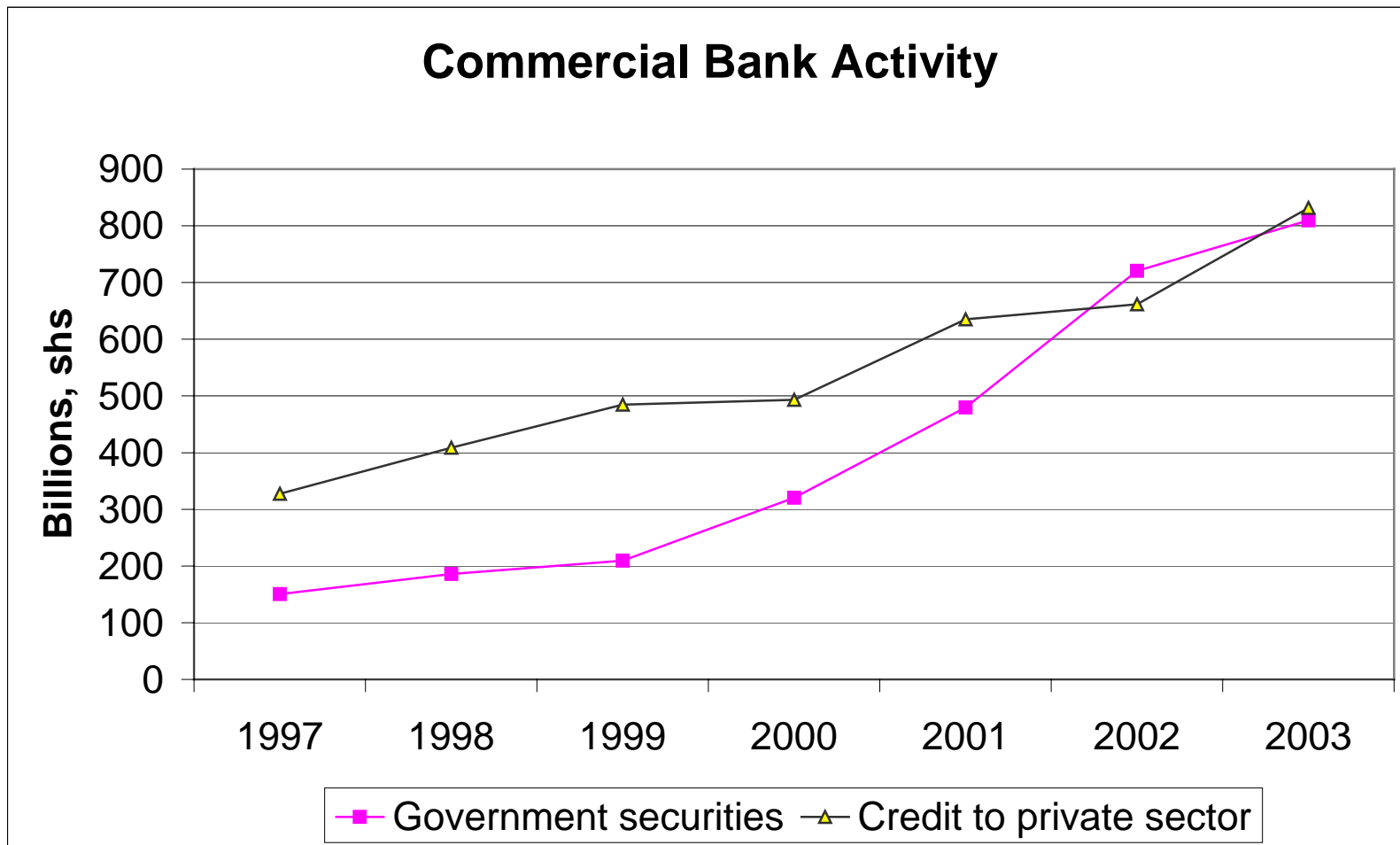
## Employment trends



**Notes for Figure2:** The sample of firms used to construct this figure includes all firms that were established before 1999 and had a banking relationship between 1997-2002 and had all 5 years of employment data. 9 firms that lost a banking relationship in 1998 are dropped. Affected firms defined as firms that lost a banking relationship during the crisis. Affected2 is the subset of affected firms that were not insiders/borrowing at closure.



Figure 3:



Source: Bank of Uganda (BOU)

Notes for Figure 3: The figure above shows the composition of commercial bank assets between 1997 and 2003. Government securities refers to holdings of treasury bills or BOU bonds. Credit to the private sector includes advances and loans.