# Essays in Financial Intermediation

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*To Tatiana, Ava, and my whole family*

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## Abstract

Financial intermediation helps the economy allocate capital, presumably, in an efficient, safe and rational way (Manove and Padilla, 1999; Coval and Thakor, 2005). This dissertation studies if financial intermediaries behave so. Chapter 1 finds inefficiencies. I use loan-level data and bank closures to demonstrate that a distressed bank had overcharged its good-quality customers, and since they had paid these rents, switching must have been even costlier. This serves as a novel estimate of firms' switching costs and a novel identification of the hold-up problem. In Chapter 2, I match German banks' FX-denominated balance sheet exposures with transaction-level derivative exposures, and, for the first time, use such detailed data to study banks' FX risk management. I find limited evidence of hedging, which suggests insufficient risk management. In Chapter 3, I use millisecond-stamped transaction-level stock trading data to show, for the first time, that algorithms trade stocks more rationally than human traders.

## Resum

La intermediació financera ajuda a l'economia a assignar capital, presuntament d'una manera: eficient, segura i racional (Manove and Padilla, 1999; Coval and Thakor, 2005). Aquesta tesi estudia si els intermediaris financers es comporten de tal manera. El primer capítol troba ineficiències. Utilitzo dades a nivell de préstecs i de tancaments bancaris per demostrar que un banc amb problemes va sobrecargar amb interessos del crèdit els seus bons clients, i com els clients havien pagat aquestes rendes, cambiar de banc encara era més costós per a ells. Aquestes dades serveixen com una nova estimació dels costos de canvi i una nova identificació del problema de manteniment o "hold-up". En el capítol 2, aparello les exposicions a divises estrangeres en el balanç de bancs alemanys amb exposicions a derivats a nivell de transaccions, i, per primer cop, utilitzo aquestes detallades dades per estudiar la gestió del risc de divisa. Trobo evidencia limitada d'us en cobertura, el que suggereix gestió insuficient del risc. En el tercer capítol, utilitzo dades de transaccions a nivell de milisegon del mercat bursàtil per demostrar, per primera vegada, que els algoritmes compren i venen accions de manera més racional que els compradors i venedors d'accions humans.

# Preface

Financial intermediation facilitates economic growth by lubricating the flow of funds between savers and borrowers (see e.g. Greenbaum, Thakor and Boot, 2019). For example, by ex-ante screening and ex-post monitoring their borrowers, banks can transform small, liquid, short-term, and nearly risk-free deposits into large, illiquid, long-term, and risky loans. In this way, banks allocate capital presumably to its best use (i.e. efficiently) and help savers manage their risks. However, by performing asset transformation, financial intermediaries assume risks themselves. Because risk-taking incentives of managers are not always aligned with the rest of stakeholders, and because failures of financial intermediaries can have devastating consequences for societies, the risk management of financial institutions is heavily regulated. In addition, managers may possess behavioral biases, e.g. overconfidence, which can also adversely affect risk management and capital allocation. Therefore, it is important that the financial intermediation helped the economy allocate capital not only in an efficient and safe but, arguably, also rational way (see e.g. Manove and Padilla, 1999; Coval and Thakor, 2005). This dissertation studies the extent to which financial intermediaries behave in such manner. I pose three broad questions – one for each chapter – about the behavior of financial intermediaries. First, do they allocate capital efficiently? Second, how do they manage their risks? Third, are they subject to behavioral biases? I tackle these questions empirically by using novel granular datasets, and I make identification-related contributions to different branches of literature.

In Chapter 1, co-authored with Kristina Grigaitė, we show evidence that credit allocation is not always efficient, i.e. that banks can and sometimes do hinder credit access to productive firms. For identification, we use (1) loan-level data provided by the Bank of Lithuania and (2) two simultaneous closures of banks – one healthy and one distressed. We find that when a distressed bank's closure forced its good borrowers to switch, their borrowing costs immediately dropped and permanently converged to the market's average. The drop was particularly large for opaquer, i.e. smaller and younger, customers. This suggests that the bank held up and overcharged its customers, particularly opaquer ones, and since they paid these rents instead of switching, switching costs must have been even higher and stemmed primarily from information asymmetries. A healthy bank's closure shows no such evidence, which suggests that reputational concerns might discipline banks. Our primary contribution to the literature is a novel estimate of firms' lower-bound switching costs and a novel identification of the hold-up problem. To policy-makers, our evidence suggests that (1) closures of distressed banks can benefit good-quality, and, thus, productive, firms by allowing them to borrow cheaper, (2) a presence of a credit bureau is not sufficient to eliminate hold-up problems, and (3) rising interest rates charged by a bank can be an early sign of the bank's financial distress.

In Chapter 2, co-authored with Puriya Abbassi, Falk Bräuning, Luc Laeven, and José-Luis Peydró, we analyze how banks manage their foreign exchange (FX) risk. Capital requirements on FX risk make it costly for banks to hold uncovered FX positions, yet post-crisis persistent violations of covered interest parity (CIP) suggest that hedging might be costly too. We match bank-currency-month level assets and liabilities denominated in EUR, USD, GBP, JPY and CHF, provided by Deutsche Bundesbank, with daily transaction-level FX derivative exposures provided by Depository Trust & Clearing Corporation (DTCC). This detailed data allows us to identify and measure covered and uncovered FX exposures of banks with unprecedented precision, which is our primary contribution to the literature. We also test how these exposures vary depending on banks' characteristics and macroeconomic shocks. We find that banks hold large and persistent unhedged FX exposures which are somewhat reduced when FX rate uncertainty spikes. This suggests that banks' FX risk management is insufficient and overdue, and that international financial system might face systemic risk. This calls for revisions in regulations and capital requirements.

In Chapter 3, I study behavioral biases, which are particularly difficult to identify at the bank-level due to relatively infrequent and aggregated data. For example, the ECB's negative deposit facility rate offers an ideal setting to study loss aversion among banks but for identification one might need daily data on excess reserves. Therefore, in Chapter 3, I study other financial intermediaries – stock traders – and ask: can humans achieve rationality, as defined by the expected utility theory, by automating their decision making? I use millisecond-stamped transaction-level data from the Copenhagen Stock Exchange to estimate the disposition effect – the tendency to sell winning but not losing stocks – among algorithmic and human professional stock day-traders. I find that (1) the disposition effect is substantial among humans but virtually zero among algorithms, (2) this difference is not fully explained by rational explanations and is, at least partially, attributed to prospect theory, realization utility and beliefs in mean-reversion, and (3) the disposition effect harms trading performance, which further deems such behavior irrational. My results suggest that financial intermediation is at least not fully rational but, for better or for worse, can become more rational with the help of technology.

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## Chapter 1

# LOSS OF A LENDING RELATIONSHIP: PAIN OR RELIEF?

with Kristina Grigaitė*

## 1.1 Introduction

The 2007-9 financial crisis exposed the importance of firm-bank relationships. While they generally helped firms access credit (Bolton et al., 2016; Beck et al., 2018), relationships with severely hit banks were less helpful. Distressed banks cut lending (Ivashina and Scharfstein, 2010) and raised interest rates (Santos, 2011), and since bank-switching was costly, firms dependent on the distressed banks were forced to lay off staff (Chodorow-Reich, 2014), cut investment (Carvalho et al., 2015), and even shut down (Jiménez et al., 2017). In this paper we ask: how costly can bank-switching be? What causes these switching costs? Do banks exploit these switching costs to hold up their customers and extract rents from them? Although empirical evidence suggests that switching costs exist (Ioannidou and Ongena, 2010) and stem primarily from interbank information asymmetries (Bonfim et al., 2019), evidence on hold-up and rents' extraction is mixed[1]. Moreover, since switching is endogenous, i.e. firms avoid costly switching, little is known about the magnitude of switching costs[2]. We use forced switches induced by bank closures and contribute to the literature with a novel lower-bound estimate of switching costs as well as a novel identification of the hold-up problem.

Lithuania offers an ideal setting for identification due to its exhaustive credit register[3] and a sudden closure of a seemingly healthy but apparently distressed bank[4] in an

* Bank of Lithuania

[1] See, e.g., Petersen and Rajan, 1994; Berger and Udell, 1995; Angelini et al., 1998; Berlin and Mester, 1999; Dahiya et al., 2003; Schenone, 2009; Ioannidou and Ongena, 2010; Kysucky and Norden, 2015; Gobbi and Sette, 2015; Bolton et al., 2016; Lopez-Espinosa et al., 2017; Botsch & Vanasco, 2019.

[2] To the best of our knowledge, only Kim et al. (2003) estimated switching costs in the loans' market and their estimates rely on elaborate modeling of demand and supply.

[3] We observe even the smallest loans issued to the smallest, and, thus, opaquest firms. Most other credit registers have a loan size threshold, and, thus, allow analyses of only relatively large firms.

[4] The Bank of Lithuania uncovered that "Distressed bank" – the oldest bank in Lithuania – had severely misreported its asset values, and, therefore, immediately shut it down. The majority of the bank's misrepresented assets consisted of loans extended to firms closely related to the bank's major shareholder

otherwise healthy banking system. Distressed banks tend to care less about reputation than healthy banks (Boot et al., 1993) and, thus, may choose to extract more rents from locked-in customers (Sharpe, 1990). If customers pay these rents instead of switching, then their switching costs, including higher interest rates charged by outside banks due to information asymmetries (Sharpe, 1990; Rajan, 1992; von Thadden, 2004), search costs, refinancing costs, losses of benefits provided by inside banks (e.g. lower collateral requirements), and other shoe-leather switching costs (Klemperer, 1987), must be even higher. Thus, the estimation of extracted rents would reveal the lower-bound of firms' total ex-ante switching costs. We show that when "Distressed bank" closed, most of its customers switched and borrowed at lower interest rates instantly and permanently. Moreover, most of the switchers moved to better-reputation banks and borrowed on average at the same rates as all other customers of those banks. This suggests that "Distressed bank" was overcharging its good quality borrowers and since they paid these rents instead of switching, their ex-ante switching costs must have been even higher. Clients of healthy banks might face similar switching costs but a closure of such a bank would not reveal this if that bank does not overcharge its customers.[5] In our setting, coincidentally in the same quarter, a heathy but small branch of an international banking group left Lithuania due to the group's global cost restructuring plan. In line with Sharpe's (1990) reputational concerns, we find no evidence that "Healthy bank" overcharged its customers as they switched and borrowed at the same rates after the bank's closure.

We explore a number of potential explanations why borrowing costs of "Distressed bank's" customers dropped. Firstly, inside banks have more information about their borrowers, thus, outside banks face a winner's curse and are discouraged from bidding for even seemingly good quality firms (Sharpe, 1990; Rajan, 1992; von Thadden, 2004). This interbank information asymmetry makes it difficult and costly for good quality firms to switch and allows inside banks to extract rents from them. A closure of an inside bank can alleviate the winner's curse for outside banks and encourage them to compete for seemingly good quality borrowers. Secondly, "Distressed bank" was resolved by an auditor KPMG that split the bank into a "good bank" and a "bad bank". The separation of failing firms from the rest, reduced firm-bank information asymmetries, and transparency can reduce the hold-up problem (Padilla and Pagano, 1997; Jappelli and Pagano, 2002). Thirdly, if switching costs were not driven by either interbank or firm-bank information asymmetries but rather by other shoe-leather costs, e.g. search costs, loan refinancing costs or loss of some benefits provided by "Distressed bank", then "Distressed bank's" customers might have always been able to borrow at lower interest rates but chose not to until being forced. We also address other alternative explanations and endogeneity concerns.

(OECD, 2017). The closure came as a surprise even to governmental institutions, which lost large uninsured deposits (Kuodis, 2013). The bank's auditor "Deloitte Lietuva" was penalized for the perfunctory audit of the bank (Vasiliauskaitė and Gudavičius, 2014).

[5] We do not imply that healthy banks charge "fair" interest rates but, based on Boot et al. (1993) and Sharpe (1990), we presume that distressed banks may extract more rents than healthy banks. Thus, our estimated drop in borrowing costs represents the lower bound of extracted rents (which, in turn, represent the lower bound of switching costs).

We exploit the exhaustive credit register provided by the Bank of Lithuania, which reports interest rates and other characteristics of all outstanding loans in Lithuania quarterly from 2011 q4 to 2018 q1. This paper is the first to use this credit register and, to the best of our knowledge, the first to study directly how firms' loan interest rates change when banks close[6]. We analyze jointly leasing contracts, term loans and credit lines which make up 86% of all contracts in the database, but our findings are robust to using term loans and leasing contracts separately. We disregard credit unions and consider the 12 banks, which account for 95% of observations. Most Lithuanian firms are relatively small and bank-dependent. Our sample period is marked by an economic recovery after the 2007-9 crisis. In 2011 Lithuania's GDP grew by 6%, the financial system was stable and banks' total profits almost reached a record-high pre-crisis level (Bank of Lithuania, 2011). A credit bureau "Creditinfo" provided lenders with firms' ten-year-credit-histories that included interest rates, collateral values, repayment delays but no names of lenders, thus, our results might extend to other Lithuanian firms.

We analyze separately two bank closures[7]. Firstly, on February 12, 2013, the Bank of Lithuania unexpectedly closed the oldest and one of the largest domestic-capital banks "Distressed bank" due to the uncovered misreporting of assets. The bank was resolved by first netting off firms' assets and liabilities with the bank and then KPMG assigned the remaining performing and non-performing loans to the "good" and "bad" banks, respectively. This setting gives us a unique opportunity to identify poor-quality borrowers, based on ex-ante but not publicly available information, and to separate them from the rest of firms. The "bad bank" was liquidated and the "good bank" was assigned to another ("Acquiring") bank that was similar to "Distressed bank". Most firms assigned to the "good bank", however, never switched for new loans to "Acquiring bank" and instead switched to other (better) banks. Secondly, on January 30, 2013, "Healthy bank" announced about leaving Lithuania and stopped issuing new loans. It was a healthy but small branch of an international banking group, which implemented a cost restructuring plan and closed many branches around the globe. For instance, it also left Estonia but stayed in Latvia, where it had the largest and the oldest office in the Baltic countries. Old borrowers had to finish repaying their loans but could not take new loans and had to switch.[8]

We use a visual inspection of graphs and a ("reverse"[9]) difference-in-differences (DID) method to compare firms' borrowing costs before and after the shocks. Firm-quarter level borrowing costs are calculated as an average interest rate on outstanding loans weighted by loan amounts. In the post-shock period, we consider only loans issued after the shock. A firm is called a banks' customer if it had debt outstanding with that bank within one year before the shock. The treatment group comprises customers of a closed

[6] The closest study to ours is Bonfim et al. (2019). They compare non-switchers' and forced-switchers' loan interest rates post-shock: after bank branch closures. In contrast, we compare forced-switchers' rates pre-shock vs. post-shock.
[7] One more bank closed in November 2011 but due to structural changes in the database, we do not observe interest rates before 2011 q4 and, thus, do not analyze this closure.
[8] The Latvian branch of the same bank was not a feasible option due to a different currency.
[9] We call it "reverse" difference-in-differences because the treatment – being locked-in by a distressed bank – happens in the pre-shock period. In the post-shock period, customers of "Distressed bank" are released from the treatment, switch to healthier banks and, thus, resemble more the control group – all other firms borrowing from the same banks.

bank, i.e. first "Distressed" then "Healthy", and the control group comprises all other firms.

In order to maintain a constant set of firms, we consider only those that borrowed both before and after the shock. By excluding the worst quality firms, i.e. non-survivors, we may overestimate the drop in borrowing costs but we do not intend to generalize it to all firms and, instead, focus our main analysis on "good" firms due to the following reasons. Firstly, our goal is to measure switching costs, which might primarily stem from interbank information asymmetries (Bonfim et al., 2019). According to informational hold-up theories (Sharpe, 1990; Rajan, 1992; von Thadden, 2004), the worst quality firms, e.g. those unable to borrow at all, do not suffer from this type of switching costs because there are no worse quality firms that they can be mistaken with[10]. Secondly, "good" firms are particularly important for the productivity (Caballero et al., 2008) and employment (Falato and Liang, 2016) in the economy. Thirdly, "Distressed bank's" closure was caused by the "bad" firms, thus, for the rest, it was less endogenous. Fourthly, by filtering out "bad" firms from the treatment group we make it more similar to our control group and, thus, can easier generalize our results to an average Lithuanian firm. To make the two groups similar, we define "good" firms in both groups as those that were not assigned to the "bad bank" by KPMG and, after the shock, took new loans but not from "Acquiring bank". By our definition, "Bad" firms do not comply with at least one of these criteria.

We find that "Distressed bank's" "good" clients, borrowed on average at the same rates as all other firms immediately after the shock but at 1.1 pp higher rates before the shock. The overcharge was larger for "good" exclusive customers (3.1 pp), i.e. owing to one bank only, and even larger for opaquer ones, i.e. younger (3.9 pp) and smaller (3.7 pp) than median, which suggests that information asymmetries affect switching costs. Correlation with age can explain a larger overcharge found for "good" short-term exclusive clients[11] (3.9 pp). We find no change in borrowing costs for "Distressed bank's" "bad" clients[12], which again suggests that switching costs stem from information asymmetries rather than shoe-leather costs that would affect all firms. Finally, we find no evidence that benefits provided by "Distressed bank", i.e. lower collateral, longer maturities and larger loans, caused switching costs as the difference-in-differences analyses for these characteristics show no changes.

We have two major concerns regarding the endogenous firm selection. First, our treatment and control groups might be different, e.g. "Distressed bank" had on average

[10] In practice, if a firm cannot switch even when a closure of its inside bank eliminates information asymmetries between its inside and outside banks, then ex-ante switching costs caused by these asymmetries should be irrelevant.
[11] On the one hand, longer-term clients are older, less opaque and, thus, more difficult to hold-up. On the other hand, reputational concerns (Sharpe, 1990) might encourage banks to treat their long-term customers well.
[12] In theory (e.g. Sharpe, 1990), "bad" customers could also experience a drop in borrowing costs if due to a lack of information, outside banks pool them together with "good" firms. However, if outside banks have some but imperfect information about these firms, the drop can be zero for "bad-looking" firms but, due to the alleviated winner's curse, large for "good-looking" firms. Also, the transparency brought by KPMG can explain the zero drop for "bad" firms.

worse quality clients[13] than other banks. However, the DID method cancels out time-invariant differences, and the most intuitive shock-related time-varying differences point towards the underestimation of our results: e.g. (1) the deteriorating quality of "Distressed bank's" clients might have caused the bank's closure; (2) the "Distressed bank's" closure might have hurt the image of its clients; (3) declined banking competition might have particularly affected firms that were no longer locked-in by banks (Klemperer, 1987). In these examples, borrowing costs of our treatment group relative to the control group would be affected upwards, thus, we might underestimate the drop. When considering only "good" firms, we find that the post-shock trends of borrowing costs, which are of the primary importance given the "reverse" nature of our DID setting (Kim and Lee, 2018), are not only parallel but also of the same level, which suggests that ex-post the two groups are in expectation similar. Pre-shock trends are slightly diverging, which suggests that the overcharge gradually increased and, again, that we may underestimate the drop.

Second, we condition our DID analysis on post-shock measures of firm quality, i.e. surviving and switching to other banks than "Acquiring bank".[14] The shock itself may affect the true or perceived quality of firms, which may bias our results for "good" firms (e.g. Montgomery et al., 2018). On the one hand, firms' quality may improve between the shock and switching, and this would explain our results, yet, we deem this unlikely given the immediacy of the results. On the other hand, a bank's closure may hurt its clients' true or perceived quality but this would affect loan rates upwards and, thus, would lead to the underestimation of our results. Another possibility is that other equally "good" firms had not been lucky enough to switch to a good bank, and, thus, were excluded from the sample, which would overestimate results for "good" firms. To address this, we show the following. First, "good" ("bad") firms had the best (worst) average pre-shock characteristics, which suggests that luck's role was limited. Second, the results remain similar with all surviving "Distressed bank's" customers as a treatment group and when splitting them into "good" and "bad" based on pre-shock characteristics. Third, our results remain significant even in our most conservative settings using all firms and either Heckman's (1979) selection model or regression imputation. Fourth, placebo tests show no drop in borrowing costs for surviving customers of other banks, despite equally low survival rates.

Our other robustness tests show that results remain similar when using (1) only term loans, (2) only leasing contracts, (3) only clients of the most similar ("Acquiring") bank as a control group, (4) only newly issued loans in every quarter, and (5) only firms that had their liabilities with "Distressed bank" completely netted off with assets, and, therefore, were not transferred to either "good bank" or "bad bank". These firms did not benefit from an assignation to the "good bank" and were unambiguously forced to switch. Also, following Bonfim et al. (2019), we match post-shock forced-switching

[13] Borrowers of "Distressed bank" ("Healthy bank") had more (less) frequent repayment delays and higher (lower) loan interest rates (Figures 1.1.1 and 1.1.2) than clients of most other banks in Lithuania.
[14] According to KPMG's employees, our other firm quality criterion – not being assigned to the "bad bank", was largely unaffected by post-closure information because it was done urgently within a few days, and, thus, based only on pre-shock financial statements, loan agreements and other documents. When using only this split and survival as the criteria to identify "good" firms, we still observe the drop in borrowing costs for "good" but not for "bad" firms.

and non-switching loans and show that "Distressed bank's" clients and other firms borrowed at similar rates after the shock. Finally, a panel regression reveals that short-term clients tend to be overcharged more even when controlling for firms' age and other time-varying characteristics.

Overall, our results suggest that in a highly concentrated banking market with relatively small firms, switching costs can be significant[15], they stem primarily from information asymmetries, and banks, especially distressed ones, may exploit that to hold-up their good customers and to extract rents from them. This has policy implications. Firstly, information asymmetries remain despite a credit bureau providing detailed ten-year-credit-histories and, thus, regulators might as well aim to reduce these asymmetries further. For instance, a prevention of loan evergreening may improve the reliability of credit histories. Secondly, regulators could monitor banks' loan rates for early signs of banks' distress. Thirdly, although bank closures are costly (e.g. Kang et al., 2015), we provide one benefit for regulators to consider when resolving failed banks: a bank closure may help good quality firms, which are particularly important for productivity and employment (Caballero et al., 2008), borrow cheaper.

We contribute to a few strands of literature. First, we add to the literature on switching costs (e.g. Kim et al., 2003; Ioannidou and Ongena, 2010; Bonfim et al., 2019) and hold-up costs (e.g., Petersen and Rajan, 1994; Berger and Udell, 1995; Angelini et al., 1998; Berlin and Mester, 1999; Dahiya et al., 2003; Schenone, 2009; Ioannidou and Ongena, 2010; Kysucky and Norden, 2015; Gobbi and Sette, 2015; Bolton et al., 2016; Lopez-Espinosa et al., 2017; Botsch & Vanasco, 2019) in the loans' market. Second, by differentiating between "Healthy bank" and "Distressed bank", we contribute to the literature studying how banks' health affects borrowers (e.g. Ivashina and Scharfstein, 2010; Slovin et al., 1993; Ongena et al., 2003; Carvalho et al., 2015; Schnabl, 2012; Chava and Purnanandam, 2011; Khwaja and Mian, 2008) and particularly, their loan rates (Hubbard et al., 2002; Santos, 2011; Chodorow-Reich, 2014). Third, we add to the literature on bank closures, which so far has focused on the effects on aggregate economic outcomes (Bernanke, 1983; Ashcraft, 2005) and firms' investments (Minamihashi, 2011; Korte, 2015).

The rest of the paper is structured as follows. Section 1.2 discusses the theoretical framework. Section 1.3 presents the data and the institutional setting. Section 1.4 describes the closures of the banks. Section 1.5 analyzes how the closures of the banks affected firms' borrowing costs. Section 1.6 tests whether forced-switchers at new banks indeed received similar rates to those received by old customers. Section 1.7 tests the link between loan rates and lengths of relationships. Section 1.8 concludes.

## 1.2 Theoretical Framework

Lending relationships can make borrowing both cheaper and more expensive. On the one hand, repeated interactions reduce information asymmetries between firms and

[15] For instance, an average exclusive "Distressed bank's" customer could have paid 2.5 extra yearly average Lithuanian salaries in 2012 had it not been overcharged the 3.1 pp on interest payments.

banks, which may alleviate firms' borrowing costs (e.g. Diamond, 1984). On the other hand, firm-bank relationships create information asymmetries across banks. Inside banks know their borrowers better and, thus, can offer lower interest rates than outside banks, which makes switching for good-quality firms costly and leads to an adverse selection of firms willing to switch banks (Sharpe, 1990). This allows inside banks to hold up and extract rents from their good customers and, as noted by Rajan (1992) and von Thadden (2004), creates a winner's curse to outside banks. If outside banks always gave their best bids in order to attract good firms from other banks, inside banks could always respond with their own best bids in order not to lose their good customers. Outside banks would always lose either due to underbidding or due to overbidding (the winner's curse). Therefore, in equilibrium, their only solution is a mixed strategy, i.e. bidding only sometimes. In this case, inside banks would not need to always give their best bids and, thus, could extract significant rents from their good customers, although sometimes they would lose some of those customers to outside banks (Rajan, 1992; von Thadden, 2004).

Diagram 1 below represents an example of a mixed-strategy equilibrium (Rajan, 1992; von Thadden, 2004). Suppose that two banks "A" and "B" lend to "good firms" at 5% and "bad firms" at 15% interest rates. They know about firms' quality if they have lending relationships with them. If one "good" and one "bad" firm tried to switch from "Bank B" to "Bank A", the latter would receive only a noisy signal about these firms' quality. Sharpe (1990) orders break-even loan rates as follows: $r_S < r_{S'} < r_p < r_{F'} < r_F$, where $r_S$ is a break-even rate offered by a bank to a firm if the bank knows that the firm has been successful, $r_{S'}$ – if the bank receives a noisy signal that the firm has been successful, $r_p$ – if the bank has no information about the firm's past success, $r_{F'}$ – if the bank receives a noisy signal that the firm has been failing, and $r_F$ – if the bank knows with certainty that the firm has been failing. In our example: $r_S$ = 5%; $r_{S'}$ = 8%; $r_p$ = 10%; $r_{F'}$ = 12%; $r_F$ = 15%. The "bad firm" would like to switch and pay 12% at the worse-informed bank instead of the current 15%, while for the "good firm" switching would be costly because the worse-informed bank would offer 8% instead of the current 5%. This allows "Bank B" to hold up and extract rents from its "good firms" by charging them close to 8%. Since "Bank A" can attract only "bad firms", i.e. it is subject to the winner's curse, it might as well charge all approaching unknown firms 15% (or not bid at all). This allows "Bank B" to extract even more rents by charging its "good firms", for example, 14%. "Bank A" can sometimes unexpectedly bid lower, e.g. 8%, in order to take over a "good firm" from "Bank B".

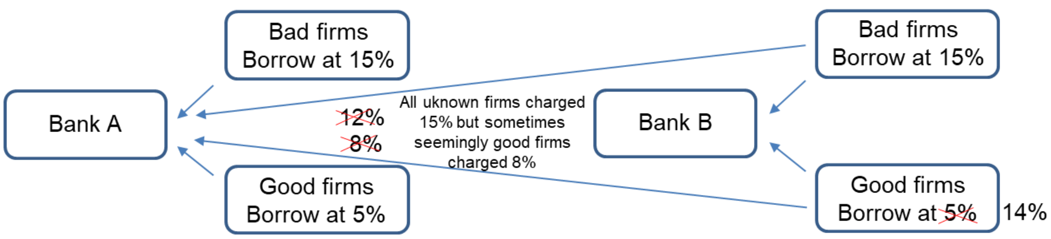



**Diagram 1.** An example of a mixed-strategy equilibrium in the theoretical framework based on Sharpe (1990), Rajan (1992) and von Thadden (2004)

If “Bank B” is distressed, it might care about its survival more than reputation (Boot et al., 1993) and, thus, might choose to extract more rents from “good firms” than other banks (Sharpe, 1990). When such a distressed “Bank B” is closed, “Bank A” is no longer subject to the winner’s curse and, thus, can charge all “seemingly good firms” 8% ($r_{S'}$) as initially intended.[16] Borrowing costs of “good” customers of “Bank B” drops from 14% to 8% and the drop of 6pp represents the lower bound of previously extracted rents of 9pp (i.e. 14pp-5pp), which in turn represent the lower bound of total ex-ante switching costs of 10pp (i.e. 15pp-5pp). Borrowing costs of the “bad” customers drop from 15% to 12%. In our empirical setting, KPMG separated “good” and “bad” firms and, thus, reduced firm-bank information asymmetries. In the theoretical example, this brings $r_{S'}$ ($r_{F'}$) closer to $r_S$ ($r_F$), which means that borrowing costs of “good” customers of “Bank B” could potentially drop even to 5%.

Note that if initially $r_{S'}$ was very close to $r_S$ and $r_{F'}$ to $r_F$, i.e. “Bank A” could identify the quality of customers of “Bank B” almost with certainty, the winner’s curse could still lead to the same mixed strategy equilibrium as described above. The closure of “Bank B” would still cause a significant drop in borrowing costs for its “good” customers but virtually no drop for “bad” customers. Also note that according to this theoretical framework, only “good firms” are subject to switching costs stemming from information asymmetries because they can be mistaken with “bad firms”. Since the goal of this paper is to measure these switching costs, we focus on “good” firms and primarily disregard non-survivors, i.e. arguably the worst-quality firms as suggested by the fact that they did not manage to borrow at all after their bank closed. Intuitively, if these firms did not manage to switch even after the reduction of interbank information asymmetries, then ex-ante switching costs stemming from these asymmetries must have been irrelevant.

## 1.3 Data and Institutional Setting

We use quarterly data on corporate loans outstanding between 2011 q4 and 2018 q1, provided by the Bank of Lithuania, and observe the following variables: year, quarter, loan id, loan type, firm id, bank id, loan issue date, loan maturity date, loan outstanding amount, loan interest rate, loan currency, loan collateral value, indicator if a firm had late repayments within a given quarter, firm’s industry and firm’s total loan amount. The database includes all debt contracts issued to all firms by all credit institutions registered in Lithuania. In addition, we observe firm id, bank id, loan initiation date and loan termination date of all loans between 1995 and 2011, which allows us to estimate lengths of all firm-bank relationships and firms’ age.

We disregard credit unions and other small lenders and consider the 12 largest banks, which account for 95% of all observations. Five of the 12 banks were funded primarily by Lithuanian capital and had none or limited cross-border activities. The other seven banks were branches or subsidiaries of foreign – mostly Scandinavian – banks. The

[16] Intuitively, when firms try to switch they unintentionally and not necessarily rightfully signal that they are unable to borrow from their well-informed inside bank. Yet, if their bank closes, they have a good excuse to switch.

banking sector was concentrated as, at the beginning of our sample period (2011 Q4), the five Scandinavian-owned banks held 82% of the outstanding credit issued to firms. The three largest banks accounted for 65% of this credit. The Herfindahl-Hirschman Index (HHI) for outstanding loans throughout our sample period varied between 1,632 and 1,992.

In our sample period, the 12 banks had 190,728 outstanding debt contracts issued to 35,905 firms, which constitutes 1,635,779 quarterly observations. These include 117,557 new contracts that were issued to 25,436 firms within our sample period. All these contracts were issued in the local currency and only between one firm and one bank.[17] Table 1.1a provides loans' summary statistics (aggregate and split by loan type). In our analyses, we use the three most popular loan types in terms of the number of contracts and the loan amount issued. They jointly constitute 86% of the total number of contracts: leasing – 69%, term loans – 13% and credit lines – 4%. The total amount issued sums up to 48 billion EUR. Of this amount, 54% is attributable to term loans, 14% to leasing contracts, 11% to credit lines and the rest to overdrafts, mortgages and other types of contracts. The average (median) loan size across all loans is EUR 0.25 million (EUR 0.026 million), the average (median) interest rate is 3.8% (3.2%), and the average (median) time to maturity of debt contracts at the time of issuance is 2.7 years (2.8 years). To avoid outliers' impact in all our regression analyses, we winsorize the top and bottom 0.5% of observations of each of these variables, but this has trivial effects on the results. Only 20% of contracts are collateralized but, for term loans and credit lines, this number reaches above 80%.

Firms in Lithuania are relatively small and reliant on banks' funding. As shown in Table 1.1b, at the beginning of the sample (2011 Q4), the average (median) outstanding debt across the 17,266 firms was almost EUR 1 million (EUR 0.06 million) and the aggregate firms' debt to banks was EUR 16.8 billion. This illustrates the firms' reliance on banks, since, according to Nasdaq Baltic monthly statistics, at the same time the stock market capitalization was EUR 3.1 billion and the market value of all publicly traded corporate bonds was EUR 1.3 billion. At the end of 2011, 77% of firms had relationships with only one bank and 68% of firms had relationships shorter than 6 years on average.[18] In our sample, the three largest sectors in terms of the number of firms were wholesale and retail (26%), transportation (12%), and manufacturing (10%). Throughout the whole sample period, 17% of all firms delayed at least one repayment.

Lithuania has been a member of the European Union since 2004 and the eurozone since 2015. The supervision of Lithuanian credit institutions follows the Basel III regulations (OECD, 2017). Since 2003, a credit bureau "Creditinfo" has been collecting information on firms' liabilities in Lithuania, which makes the Lithuanian credit market more

[17] We have dropped 2,886 loans (2%) issued in foreign currencies and 1,005 (1%) collective loans taken jointly by more than one firm. Our database does not include syndicated loans but their outstanding amount is relatively small, e.g. according to the ECB's Statistical Data Warehouse, at the end of 2011, syndicated loans to Lithuanian non-financial corporations amounted to EUR 0.7 bn, which is 4% of the loans outstanding in our dataset at the same time.

[18] In line with Ioannidou and Ongena (2010) and Bonfim et al. (2019), a firm is said to have a relationship with a bank if it had an outstanding debt with that bank at any time within the previous 12 months. We, thus, assume that after 12 months of zero debt between a firm and a bank, their relationship ties are broken.

transparent than credit markets in many other countries. Banks can access a detailed ten-year-history of their applicants' current and expired debt contracts. Information does not include bank names but does include loan types, starting and maturity dates, repayment schedules, loan amounts, interest rates, number of payments delayed, number of days delayed, total amounts delayed, etc. Nevertheless, important interbank information asymmetries remain. For example, firms are likely to keep borrowed funds in an account with the same bank, which, in turn, can observe firms' spending patterns. Furthermore, due to the possibility of loans evergreening, other banks may treat firms' credit histories with caution.

The economic environment in our sample period was marked by a sharp recovery after the 2007-2009 financial crisis. In 2011, Lithuania's GDP grew by 6%, the financial system was stable and total profits in the banking sector almost reached a record-high pre-crisis level (Bank of Lithuania, 2011). Throughout our sample period, average interest rates were gradually declining, following the expansionary monetary policies of the European Central Bank.

Our institutional setting is comparable to those of some other related papers. For instance, Bonfim et al. (2019) study another relatively small market in the eurozone – Portugal, where firms also largely rely on banks' funding, and where a few of the largest banks dominate the market. For example, in the sample period of 2012-2015, six banks held 85% of the market (Bonfim et al., 2019). Some related papers examine even smaller markets; for example, Schäfer (2018) studies 6,649 firms in Armenia in 2009-2013 while Ioannidou and Ongena (2010) study 2,805 firms in Bolivia in 1999-2003. Our setting particularly differs from theirs in terms of the credit markets' transparency, as in Portugal (Bonfim et al., 2019) and Bolivia (Ioannidou and Ongena, 2010) banks could access only 2 months of their applicants' credit history while in Armenia, a private credit bureau provided a history of 5 years. This strengthens the external validity of our results: if interbank information asymmetries matter in Lithuania, where banks can access ten years of firms' credit histories, they are likely to matter even more in less transparent markets. However, it is not clear if our results would be replicated in larger and less concentrated (more competitive) markets. On the one hand, more interbank competition makes relationship lending more important to banks (Boot and Thakor, 2000). On the other hand, interbank competition generally makes it difficult for banks to internalize benefits from lending relationships, i.e. to exploit borrowers (Petersen and Rajan, 1995; Boot and Thakor, 2000; Degryse and Ongena, 2005). Also, results might be different for larger and, thus, less opaque firms. For example, adverse selection costs were shown to be minimal in the U.S. syndicated loan market (Darmouni, 2019).

## 1.4 Closures of Banks

We use two almost simultaneous closures of banks. First, in 2013 q1 (January 30), "Healthy bank" [19] – a branch of a large international bank – announced its strategic decision to leave the Lithuanian and Estonian markets and to concentrate its business in

[19] Borrowers of "Healthy bank" had lower borrowing costs and less frequent defaults (Figures 1.1.1 and 1.1.2) than borrowers of most other banks in Lithuania.

Latvia, where it had the oldest and largest headquarters in the Baltic region. According to the bank's press release, this decision was part of the parent bank's strategic plan to save operational costs globally and to increase internal efficiency of activities in Central and Eastern Europe. After the announcement, the bank stopped issuing new loans and effectively abandoned its borrowers, who were forced to switch to other banks. Borrowing from the Latvian branch was not a feasible option since at that time Latvia and Lithuania had different currencies. Just before the announcement, at the end of 2012 Q4, the bank lent to 219 firms and was 8th in terms of corporate loans' portfolio size.

Second, "Distressed bank"[20] was a publicly traded Lithuanian bank (i.e. owned and controlled by a Lithuanian businessman) with EUR 0.3 billion lent to 1230 firms as of 2012 Q4 – the 6th largest corporate loans' portfolio. The bank's activities were stopped in 2013 q1 (February 12), due to risk mismanagement and over-reporting of its asset values, as uncovered by the Bank of Lithuania. The majority of the bank's misrepresented assets consisted of loans extended to firms closely related to the bank's major shareholder (OECD, 2017). Although the bank was commonly known to be relatively risky due to rumors and negative coverage in the media, the closure was largely unexpected not only by markets, but also by governmental institutions, which lost large uninsured deposits amounting to EUR 80 million (Kuodis, 2013). Yet, the closure did not have systemic repercussions (OECD, 2017). Financial markets reacted modestly while the total amount of deposits in the banking system even increased in the days following the shutdown (Kuodis, 2013). The bank was resolved by first netting off firms' assets and liabilities with the bank and then, during a few days after the shutdown, KPMG Baltics manually reviewed all the remaining bank's assets and split them into a "good bank", which included remaining loans that were likely to perform normally and a "bad bank", which included remaining loans that had their values misrepresented and/or were likely to default. According to KPMG's employees, the split was performed carefully but urgently and, thus, it was based merely on pre-closure information: financial statements, loan agreements and other documents. The "bad bank" was liquidated and the "good bank" was acquired by another ("Acquiring") bank. The total value of the "good bank" was EUR 0.52 billion, which included EUR 189 million of loans, EUR 126 million of fixed-income securities, EUR 106 million of cash and EUR 100 million of other assets. "Acquiring bank" took over all insured deposits of the failed bank, amounting to EUR 0.79 billion, and received a compensation of EUR 0.27 billion from the state in order to balance out the assumed assets and liabilities (Ciulada, 2013).

In many ways, "Acquiring bank" was comparable to "Distressed bank"; for example, it was a publicly traded bank with the 5th largest corporate loans' portfolio as of 2012 Q4. The largest shareholders were the European Bank for Reconstruction & Development (EBRD) and five Lithuanian companies and individuals. In 2012, there were rumors that the two banks, that is, "Acquiring bank" and "Distressed bank", might merge in order to exploit synergies stemming from similar clienteles. Both banks were known for lending to SMEs and having well-established networks of offices across the country (BNS and lrytas.lt, 2012).

[20] Borrowers of "Distressed bank" had higher borrowing costs and more frequent defaults (Figures 1.1.1 and 1.1.2) than borrowers of most other banks in Lithuania.

In order to verify the health of "Healthy bank" and "Distressed bank", we compare the credit quality of their customers with the credit quality of customers of other banks in the following way. We define banks' customers in line with Ioannidou and Ongena (2010): if a firm had any amount of debt outstanding with a particular bank at any point of time within one prior year, the firm is called a customer of that bank. We identify customers of all banks at the end of 2012 q4 – just before the quarter in which both "Distressed bank" and "Healthy bank" stopped issuing loans. We exclude customers of "Distressed bank" that were assigned to the "bad bank" during the bank's resolution process. For each bank, we calculate a proportion of customers that delayed at least one repayment on any debt contract within one year before January 1, 2013 (Figure 1.1.1). Similarly, for the same groups of customers, we calculate an amount-weighted average interest rate across all contracts (term loans, leasing and credit lines) outstanding within one year before January 1, 2013 (Figure 1.1.2).[21] The two charts include customers of 11 banks since one bank was closed at the beginning of our sample period.[7]

Figure 1.1.1 indicates that before the two bank closures, customers of "Distressed bank" had the most frequent repayment delays, even when firms assigned to the "bad bank" were excluded. In contrast, customers of "Healthy bank" had the least frequent repayment troubles. These patterns are also reflected by the weighted average interest rates of each customer group prior to January 1, 2013 in Figure 1.1.2: borrowers of "Distressed bank" paid the most, while borrowers of "Healthy bank" were among those paying the least.

Table 1.1c shows that when considering term loans, credit lines and easing contracts, at the moment of the closure, "Distressed bank" had 1,204 customers, and 263 of them were assigned to the "bad bank". Out of the remaining 941 firms, 492 took new loans after the shock and thus reappeared in the credit register, which suggests that the other 449 were either not able borrow again or did not need new loans after the failure of "Distress bank". After the shock, out of the 492 firms, 243 firms took new loans from "Acquiring bank", while 249 firms switched for new loans somewhere else.

## 1.5 How Does a Bank's Closure Affect Borrowing Costs of its Customers?

### 1.5.1 Empirical Strategy

We use a visual inspection of graphs and a ("reverse") difference-in-differences (DID) method to study how borrowing costs of "Distressed bank's" customers changed when the bank was closed and the firms were forced to switch to other banks. The graphs reveal the dynamics, i.e. the sudden and permanent drop, of borrowing costs and the

[21] We ignore 15 loans (term loans, leasing or credit lines) issued between January 1, 2013 and February 11, 2013 by "Distressed bank" as information about these loans is observed after the shock - at the end of 2013 Q1. The observed values may be affected by the shock and thus might not accurately represent the situation in the pre-shock period. "Healthy bank" issued no loans (term loans, leasing or credit lines) within these dates.

DID regression formally tests if the drop is statistically significant. We then repeat the analysis with customers of "Healthy bank".

Borrowing costs, i.e. our outcome variable, are calculated for each firm, at the end of every quarter, as an amount-weighted average interest rate across outstanding loans. We consider jointly the three most popular loan types: term-loans, leasing and credit lines, in terms of both the total amount issued (79% of the sample) and the number of contracts (86% of the sample). The data sample is split into two periods: "before" considers loans issued up to December 31, 2012, while "after" considers loans issued after February 12, 2013[21] – the day when activities of "Distressed bank" were suspended.[22]

In line with Ioannidou and Ongena (2010) and Bonfim et al. (2019), a firm is defined as a bank's customer if it had debt outstanding with that bank within one prior year.[23] We call a customer "exclusive" if it had no debts with any other bank within the same prior year. As of February 12, 2013, we identify banks' customers and measure their size, age, and average length of existing relationships with banks. Firm size is proxied by the total amount of loans outstanding while firm age is proxied by the first appearance in the credit register since 1995. Firms that are smaller and younger than median are called "small" and "young", respectively, and firms with average relationships shorter than 6 years are called "short-term customers".[24]

The treatment group comprises customers of a closed bank, i.e. first "Distressed" then "Healthy", and the control group comprises customers of all other banks. In our default setting, in both groups, we consider "good" firms defined by three conditions: firms that (1) survived and, thus, took at least one new loan both before and after the shock, (2) were not assigned to the "bad bank" by KPMG and (3) after the shock never took new loans from "Acquiring bank". Although this selection of firms poses endogeneity concerns, which we discuss in sections 1.5.2.3 and 1.5.2.4, we do this for the following reasons. First, the survivorship condition is unavoidable in order to meaningfully compare borrowing costs before and after the shock. Although we exclude the worst-quality firms, i.e. those that cannot borrow at all, luckily, we do not intend to generalize our results to all firms. Our goal is to measure switching costs that likely stem from information asymmetries (Bonfim et al., 2019), and in theory (Sharpe, 1990; Rajan, 1992; von Thadden, 2004), the worst-quality firms do not suffer from such costs. In practice, for firms that cannot switch even after a bank closure alleviates information asymmetries, ex-ante switching costs stemming from these asymmetries prove irrelevant. Second, by filtering out firms which caused the bank's closure, i.e. those assigned to the "bad bank" by KPMG, we retain those to which the closure was less endogenous. Third, by selecting firms that switched for new loans to other (and, thus, better) banks than "Acquiring bank", we avoid concerns that a post-shock pricing

[22] For "Healthy bank's" analysis, we use the day the bank announced about leaving the market – January 30, 2013.

[23] In line with Ioannidou and Ongena (2010), we conservatively assume that relationship ties are broken if there is a gap without outstanding loans longer than 1 year.

[24] The cut-off of 6 years was chosen due to an interest rate pattern uncovered in Figure 1.5, which indicates that after 6 years of relationships, average interest rates start to decrease. In section 1.7, we test this pattern while controlling for firm-time (e.g. age, riskiness etc.), bank-time and firm-bank fixed effects.

strategy of this one bank drives our results. Fourth, all the three conditions make the treatment and the control groups in expectation similar as reflected by not only parallel but also same-level trends of post-shock borrowing costs, which matter given the "reverse" nature of our DID setting. Most of firms in the control group borrow from good-reputation Scandinavian banks that jointly hold more than 80% of corporate loans amount, thus, we can generalize our results to an average Lithuanian firm with reasonable confidence. Fifth, good-quality firms are particularly important for the economy due to their productivity (Caballero et al., 2008) and employment (Falato and Liang, 2016).

We hypothesize that if "Distressed bank" extracted rents from its good customers, we would see their borrowing costs decreasing more than for other firms after the bank's failure. These results should be driven by more dependent "exclusive" customers, and especially by opaquer "small" and "young" firms. Also, we expect our results to be stronger for "short-term" customers either due to the correlation with age or reputational concerns. In order to test these hypotheses, we plot average borrowing costs of the two groups in graphs and run the following five regression specifications.

$$borrowing\_costs_{f,q} = \beta_0 + \beta_1 * after_q + \beta_2 * closed_f + \boldsymbol{\beta_3 * closed_f * after_q} + FFE + TFE + \varepsilon_{f,q} \tag{1}$$

$$borrowing\_costs_{f,q} = \beta_0 + \beta_1 * after_q + \beta_2 * closed_f + \beta_3 * exclusive_f + \beta_4 * closed_f * after_q + \boldsymbol{\beta_5 * closed_f * exclusive_f * after_q} + [all\ other\ double\ interactions] + FFE + TFE + \varepsilon_{f,q} \tag{2}$$

$$borrowing\_costs_{f,q} = \beta_0 + \beta_1 * after_q + \beta_2 * closed_f + \beta_3 * exclusive_f + \beta_4 * small_f + \beta_5 * closed_f * after_q + \beta_6 * closed_f * exclusive_f * after_q + \boldsymbol{\beta_7 * after_q * closed_f * exclusive_f * small_f} + [all\ other\ double\ and\ triple\ interactions] + FFE + TFE + \varepsilon_{f,q} \tag{3}$$

$$borrowing\_costs_{f,q} = \beta_0 + \beta_1 * after_q + \beta_2 * closed_f + \beta_3 * exclusive_f + \beta_4 * young_f + \beta_5 * closed_f * after_q + \beta_6 * closed_f * exclusive_f * after_q + \boldsymbol{\beta_7 * after_q * closed_f * exclusive_f * young_f} + [all\ other\ double\ and\ triple\ interactions] + FFE + TFE + \varepsilon_{f,q} \tag{4}$$

$$borrowing\_costs_{f,q} = \beta_0 + \beta_1 * after_q + \beta_2 * closed_f + \beta_3 * exclusive_f + \beta_4 * short\_term_f + \beta_5 * closed_f * after_q + \beta_6 * closed_f * exclusive_f * after_q + \boldsymbol{\beta_7 * after_q * closed_f * exclusive_f * short\_term_f} + [all\ other\ double\ and\ triple\ interactions] + FFE + TFE + \varepsilon_{f,q} \tag{5}$$

where

- $borrowing\_costs_{f,q}$ is an average interest rate weighted by loan outstanding amounts in quarter *q* for firm *f*.

- $after_q$ is a dummy variable equal to 1 if the quarter $q$ is equal to or larger than 2013 q1, and zero otherwise.
- $closed_f$ is a dummy variable equal to 1 if firm $f$ was in a treatment group, i.e. a customer of the closed bank, and zero if firm $f$ was in a control group.
- $exclusive_f$ is a dummy variable equal to 1 if firm $f$ is a customer of only one bank, and zero otherwise.
- $small_f$ is a dummy variable equal to 1 if firm's $f$ maximum total debt to banks within one year before the shock was smaller than the median (EUR 50,000), and zero otherwise.
- $young_f$ is a dummy variable equal to 1 if as of 2013 Q1, firm $f$ was younger than median (6 years), and zero otherwise.
- $short\_term_f$ is a dummy variable equal to 1 if firm's $f$ average length of existing lending relationships before the bank's failure was shorter than 6 years, and zero otherwise.
- $FFE$ - firm-fixed effects.
- $TFE$ - time-fixed effects.

By including fixed effects, we drop variables that are not interacted but we keep them in the description above because we also re-run these regressions without fixed effects as a robustness check. In specification (1), our coefficient of interest is $\beta_3$ on the interaction term, which, if negative and statistically significant, would suggest that after the bank's failure, borrowing costs of "Distressed bank" customers decreased more than of other firms. In specification (2), the coefficient of interest is $\beta_5$ on the triple interaction, which if negative and statistically significant, would indicate that borrowing costs of "exclusive" "Distressed bank" customers dropped the most. In specifications (3), (4) and (5), the coefficient of interest is $\beta_7$ on the quadruple interaction, which if negative and statistically significant, would suggest that borrowing costs of "exclusive" "small", "exclusive" "young" and "exclusive" "short-term" "Distressed bank's" customers dropped the most.

To account for the possibility of standard errors being correlated within firms and quarters, we cluster errors multiway within both dimensions. Our results are robust if we leave errors unclustered or if we cluster only within either one of the two dimensions. Also, our results remain almost identical if we exclude either firm-fixed effects or time-fixed effects or both.

### 1.5.2 Results

#### 1.5.2.1 Default Setting (Main Results)

In our default setting with "good" firms, we have 120,756 quarterly firm-level observations of 7,067 firms, 249 of which were customers of "Distressed bank" – our treatment group.[25] Table 1.2 presents the main regression results. The coefficient on the interaction term in specification/column (1) is equal to -1.051 and is statistically significant at the 1% level, which means that after the closure of "Distressed bank", loan rates of its "good" customers decreased by 1.1 pp more than for all other "good" firms on average. This result is displayed in Figure 1.2.1: before the "Distressed bank's" closure, borrowing costs of the treatment group were significantly higher than of the control group but after the closure, they dropped and immediately converged to the control group.

As shown in Table 1.2, specification/column (2), the coefficient of interest on the triple interaction $\beta_5$ is equal to -2.544 and statistically significant at the 1% level, while the coefficient on the double interaction $\beta_4$ remains negative (-0.556) and statistically significant at the 1% level. This indicates that "exclusive" customers experienced a drop of 3.1 pp ($\beta_4 + \beta_5$) on average. This differential effect can be seen in Figure 1.2.2: the gap between "exclusive" and "non-exclusive" borrowers was large before the bank's failure, but it closed immediately after it. Interestingly, before the bank closure, rates were declining for most firms but for "exclusive" "Distressed bank's" customers they were increasing, which suggests that these customers were increasingly more overcharged.

Table 1.2, specification/column (3) shows that the coefficient of interest on the quadruple interaction $\beta_7$ is negative (-2.161) and statistically significant at the 5% level. This suggests that the "exclusive" "small" customers were overcharged the most and experienced the biggest drop of borrowing costs (-3.7 pp = $\beta_5 + \beta_6 + \beta_7$). This is reflected in Figure 1.2.3.: before the bank's closure, "exclusive" "small" "Distressed bank's" customers on average borrowed the most expensively, followed by "exclusive" "large" customers, followed by "non-exclusive" "small" customers and followed by "non-exclusive" "large" customers. After the closure, the gaps between these groups shrank and the average borrowing costs intertwined over time. Results remain similar in specifications/columns (4) and (5) where the variable $small_f$ is replaced with the variables $young_f$ and $short\_term_f$, respectively. Both groups experienced a drop in borrowing costs of 3.9 pp, the coefficients of interest on the quadruple interaction $\beta_7$ are negative and statistically significant at 5% and 1% levels, respectively. Figure 1.2.4 for "young" customers and Figure 1.2.5 for "short term" customers, show that these firms were overcharged by "Distressed bank" the most and after the bank closure experienced the largest drop of borrowing costs.

#### 1.5.2.2 Parallel Trends Assumption

A visual inspection of Figure 1.2.1 suggests that pre-shock trends of borrowing costs of the treatment and control groups were slightly diverging, but post-shock trends, which particularly matter for our "reverse" DID setting (Kim and Lee, 2018), were parallel and of the same level. This is the first evidence that the two groups were in expectation similar and we test this further using loan matching analysis in section 1.6. To test the

[25] These numbers are 149,684, 8,696 and 607, respectively, when using all surviving firms.

assumption of parallel trends formally, we apply a framework used in event studies to examine anticipation and phase-in effects. We regress our outcome variable $borrowing\_costs_{f,q}$ on interactions between the treatment variable $closed_f$ and time dummies:

$$borrowing\_costs_{f,q} = \beta_0 + \sum_{t=2012Q1}^{2018Q1} \beta_t * closed_f * dummy_t + FFE + TFE + \varepsilon_{f,q} \qquad (6)$$

We consider only “good” firms, absorb firm and time fixed effects and omit one time dummy to use it as the base. Results are presented in Table 1.3. In column (1) we omit 2011 q4 and in column (2) – 2013 q1. As compared to 2011 q4, the difference in borrowing costs between the treatment and control groups increased in the following year, but dropped immediately after the shock. This suggests that pre-shock trends were not parallel but, in the absence of the shock, borrowing costs of “Distressed bank’s” customers would have been even larger. Therefore, we might be underestimating the negative impact of the relationship break-up on borrowing costs. Column (2), where the base quarter is 2013 q1, shows that the difference in borrowing costs between the treatment and control groups has not changed significantly since the shock, which suggests parallel post-shock trends.

#### 1.5.2.3 Conditioning on Post-shock Outcomes: Results for “Bad” and All Surviving Firms

Table 1.4 shows that the coefficient on the interaction term in specification (1) is not significant when the treatment group comprises surviving but “bad” “Distressed bank’s” customers, i.e. either those assigned to the “bad bank” by KPMG (column 1) or those that switched for new loans to “Acquiring bank” (column 2) and the control group comprises all other surviving firms. Figures 1.3.1 and 1.3.2 show the respective dynamics of borrowing costs.[26] This is in line with two explanations of switching costs: (1) either banks could have always identified “good” and “bad” firms but uncertainly, i.e. firm-bank information asymmetries were low, and, thus, the winner’s curse, i.e. interbank information asymmetries, caused switching costs only for “good” firms, or (2) firm-bank asymmetries were always high and caused switching costs but KPMG reduced these asymmetries. Our results seem not to be driven by shoe-leather switching costs, which should affect both “good” and “bad” firms.

Labeling firms as “good” and “bad” based on post-shock outcomes, leaves an open possibility that “good” firms became good in the period between the shock and switching, which would explain the drop in borrowing costs. Yet, our main results occur in 2013 q1, i.e. within 6 weeks after the shock, thus, we deem unlikely that firms’

[26] Figure 1.3.1 shows that before the bank closure, customers assigned to the “bad bank” borrowed cheaper than those assigned to the “good bank”. As mentioned in section 1.4, the majority of the bank’s misrepresented assets, which were assigned to the “bad bank”, consisted of loans extended to firms closely related to the bank’s major shareholder (OECD, 2017). This explains why these firms received a preferential treatment and could borrow cheaper.

quality could have fundamentally changed, especially positively, so quickly. Another possibility is that there had been more equally "good" firms ex-ante, but some of them either (1) did not switch to a good bank or (2) did not even survive either because they were affected by the shock more adversely or due to bad luck (random variation). In this case, we would overestimate the drop in borrowing costs for "good" firms. We address these concerns as follows.

First, Table 1.1c shows that on average, "good" "Distressed bank's" customers had been the best ex-ante. Within one year before the bank closure, they had less repayment delays, lower interest rates, and lower collateral requirements than other "Distressed bank's" customers not assigned to "bad bank". Meanwhile, firms that did not take new loans after the shock were the worst in all the characteristics. This suggests that firms were labeled as "good" and "bad" not merely by luck.

Second, in Figure 1.3.3 and columns (3) and (4) in Table 1.4, we show that our main results remain similar if we split surviving "Distressed bank's" customers into "bad" and "good" based on only one pre-shock characteristic – collateralization (collateral value divided by loan outstanding amount) within one year before the shock. Collateralization should reflect how much the bank trusted its customers even if it overcharged them. We find no significant change in borrowing costs for "bad" firms (column 3), i.e. surviving "Distressed bank's" customers in the bottom quartile in terms of the lowest collateralization, and a drop of 1.1 pp in borrowing costs for "good" firms (column 4), i.e. those in the top quartile, although both groups borrowed at similar rates before the shock (see Figure 1.3.3).

Third, we use as a treatment group all "Distressed bank's" customers that comply with only one condition – survived and, thus, borrowed both before and after the shock. Column (5) in Table 1.4 shows that the coefficient on the interaction term in the regression specification (1) remains negative (-0.424) and statistically significant at 1% level, when considering jointly all surviving "Distressed bank's" customers. Figure 1.3.4. visualizes the corresponding drop in borrowing costs.

Fourth, we use all surviving firms again but to address the potential bias caused by the selection of survivors, we add Heckman (1979) correction. In order to lessen computation intensity, we reduce the number of periods to two by calculating average borrowing costs for each firm across quarters before and after the shock. Furthermore, we reduce the size of the control group by randomly selecting 20% of all other firms. As shown in Table 1.4, column (6), these adjustments have little impact on our results. Table 1.4, column (7) presents the main result of the Heckman selection model, in which the outcome equation is the regression specification (1) and the selection equation includes variables from the outcome equation and four firm-level ex-ante characteristics measured within one year before the bank closure: a dummy=1 if a firm had a repayment delay, an average ratio of collateral value over loan outstanding amount, an average loan size and an average remaining time to maturity of outstanding loans. The coefficient of interest on the interaction term remains similar (-0.359) to columns (6) and (7) and statistically significant (at 1%), even though slightly smaller in magnitude.

Fifth, column (8) in Table 1.4 shows that the results remain similar (-0.354) and statistically significant (at 1%) if instead of using Heckman correction we impute missing post-shock borrowing costs using as predictors the firm-level ex-ante characteristics from the Heckman selection equation.

Finally, we run placebo tests using surviving customers of other banks as a treatment group in regression specification (1). Out of 941 customers of "Distressed bank" not assigned to "bad bank", only 492 (52%) took new loans after the shock. While this survival rate seems low, it is very similar for other banks, especially the largest ones: 3,215/6,259 (51%); 3,677/6,219 (59%); 1,878/3,657 (51%); 1,128/2,175 (52%); 495/1,155 (43%); 335/742 (45%); 369/582 (63%); 225/367 (61%); 236/356 (66%); 139/176 (79%). This suggests that the survival rate of "Distressed bank's" customers was not extraordinary and that the bank's closure had little impact on it. If the survivorship, nevertheless, drove our results, we would expect to observe a similar drop in borrowing costs for customers of other banks which also lost a large share of their old clients. Columns (1), (2), (3), (4) and (5) in Table 1.5 show that surviving customers of the five banks with survival rates equal to or lower than "Distressed bank's" "good part" (<=52%) had no significant drops in average borrowing costs.

#### 1.5.2.4 Other Alternative Explanations and Robustness Checks

In order to alleviate some remaining endogeneity concerns, we do a series of robustness checks and present the results in Table 1.6. We rerun the regression specification (1) with a number of different alterations of the default setting, and show that our results remain qualitatively similar, i.e. there is a statistically significant drop in borrowing costs.

Firstly, to alleviate concerns that our results are driven by one particular loan type, we show that our results remain similar when using only term loans (column 1) and only leasing contracts (column 2).

Secondly, our results remain similar, when we use an alternative control group (column 3). Instead of customers of all banks other than "Distressed bank", we use customers of "Acquiring bank" – the bank which was the most similar to "Distressed bank" in terms of size, customer quality (measured as a proportion of firms with delayed repayments) and customers' average loan rates.

Thirdly, our results could be explained by old expensive loans taken by "Distressed bank's" customers and high refinancing costs. In this case, the observed drop of borrowing costs in our default setting would occur merely due to resetting loan inventories of every firm to zero at the date of the shock. Column (4) shows that the coefficient on the interaction term remains negative and statistically significant (at 5% level) if we consider only newly issued loans (leasing, term loans and credit lines) in each quarter. We treat these results as a robustness check and not as the main results because the reduced number of observations would not allow us exploiting firms' heterogeneity in terms of size, age, quality, and lengths of relationships with banks.

Nevertheless, the goal of this paper is to measure total switching costs and what part of them stems from refinancing costs is a secondary question.

Fourthly, column (5) shows that our results remain very similar to the default setting if we consider only "Distressed bank's" "good" customers (175 firms) that were not assigned either to the "bad bank" or to the "good bank" by KPMG. These firms had their loans removed from the credit register when the bank closed, which suggests that their assets and liabilities with "Distressed bank" had been netted off. As a result, these firms were unambiguously forced to switch and did not receive any potential benefits of being assigned to the "good bank".

Fifthly, the two almost simultaneous closures of "Healthy bank" and "Distressed bank" are likely to have affected the interbank market concentration, as indicated by the jump of HHI from 1,796 in 2012 Q4 to 1,959 in 2013 Q2. This would not influence the results of our difference-in-differences analysis if the concentration of banks had equal effects on firms in both the treatment group and the control group. However, according to Klemperer (1987), in markets with switching costs, "the monopoly power that firms gain over their respective market segment leads to vigorous competition for market share before consumers have attached themselves to suppliers". Thus, firms that have lost and are lacking lending relationships (i.e. our treatment group) may be affected by the interbank competition more than firms which are already locked-in by banks. A weakening competition should drive interest rates upwards, while our difference-in-difference analysis shows a steep drop. Thus, due to changes in competition, we may underestimate the negative impact of the relationship break-up on firms' borrowing costs. Nevertheless, we run a robustness test in which we make our treatment and control groups as similar as possible with respect to the sensitivity to competition. We disregard forced switching loans of "Distressed bank's" customers, i.e. the first loans after the bank closure taken by "Distressed bank's" borrowers from new banks, and consider only subsequent loans taken after these customers already started new relationships. As shown in column (6), the coefficient on the interaction term is negative and statistically significant (at 5% level).

Sixthly, we could observe a drop of loan rates if customers of "Distressed bank" either were asked to provide more collateral, or borrowed less, or borrowed with different loan maturities, after switching to other banks. We replace interest rates with other loan characteristics in the calculation of the dependent variable in our default setting, but neither the percentage of loan collateralized (columns 7) nor maturity (column 8) nor loan size (column 9) shows any statistically significant changes. This suggests that switching costs of "Distressed bank's" "good" customers had not been driven by a potential loss of other beneficial loan terms.

Seventhly, there are other reasons why the closure of "Distressed bank" might affect (or coincide with changes in) borrowing costs, but if true, these explanations would again lead us to the underestimation of our results. For example, a continuous deterioration of firms' credit quality could have caused both the bank's closure and the change in loan rates of its customers. Similarly, the bank's closure could have cast doubt for other lenders on the true quality of even the "good" customers of the failed bank. However, both of these scenarios would push borrowing costs of the "Distressed bank's" customers upwards after the bank’s closure, while we find the opposite. Thus, we might underestimate the downward impact of the relationship break-up on borrowing costs.

#### 1.5.2.5 “Healthy Bank’s” Customers as a Treatment Group

We repeat the analysis using the default setting but with customers of “Healthy bank” as a treatment group. Table 1.7 shows that in contrast to the main results of “Distressed bank’s” customers (Table 1.2), none of the coefficients of interest are negative and statistically significant. Figure 1.4 illustrates that average borrowing costs of “Healthy bank’s” customers followed the common trend without major shifts before and after the bank’s closure. This suggests that, on average, relationships with “Healthy bank” neither reduced nor inflated borrowing costs for firms. The contrasting results between the healthy and the distressed banks suggest that, in line with Sharpe (1990) and Boot et al. (1993), reputational concerns may be affected by a bank’s health and, in turn, may have a crucial impact on a bank’s decision to exploit firms’ switching costs. Yet, in order to have more conclusive evidence, we need further studies examining multiple closures of healthy and distressed banks.

## 1.6 Do Interest Rates Really Converge After the Failure of “Distressed Bank”?

Figures 1.2.1 – 1.2.5 suggested that borrowing costs of “good” “Distressed bank’s” customers dropped and immediately converged to borrowing costs of all other firms. So far, we used the difference-in-differences framework to test if the drop was statistically significant. In this section, we implement a loan matching analysis to test whether the borrowing costs indeed converged.

### 1.6.1 Empirical Strategy

Table 1.8 provides definitions of switching loans, non-switching loans and forced-switching loans. We follow the methodology of Ioannidou and Ongena (2010), who matched switching loans, i.e. loans taken from banks with which firms had no outstanding debts within one prior year, with similar non-switching loans, i.e. loans taken from banks with which firms had some outstanding debt within one prior year. Bonfim et al. (2019) follow similar methodology to compare non-switching loans with forced-switching loans defined as the first switching loans taken after closures of bank branches by firms serviced by those branches.

For comparability, we first replicate the setting of Ioannidou and Ongena (2010) matching regular-switching loans with non-switching loans, and then compare forced-switching loans of former “Distressed bank’s” customers with non-switching loans. For comparability with Bonfim et al. (2019), we also show that our results remain robust when including forced-switching loans of customers of the other two banks closed in our sample – “Healthy bank” and the bank closed in 2011 q4 (see footnote[7]).

All loans are considered only once – in a quarter in which they were issued. In total, our dataset contains 1,302 forced-switching loans, which include those of former customers of “Distressed bank”, “Healthy bank” and the bank which closed in 2011 q4, 13,133 regular-switching loans, and 81,731 non-switching loans. Table 1.9 provides average characteristics of each group of loans. As compared to non-switching loans, forced-switching loans have on average higher interest rate, larger collateral, shorter time to maturity and larger loan amount.

We apply the following procedure. First, we identify all regular-switching, forced-switching and non-switching loans. Consistently with section 1.5, we consider the three most popular loan types: leasing, term loans, and credit lines. Second, every regular-switching loan is matched with as many non-switching loans as possible, based on the set of matching variables described in Table 1.10. For each pair, we calculate an interest rate spread between a switching and a non-switching loan. The spreads are regressed on a constant, and standard errors are clustered at a switching-loan level. Third, we repeat the procedure considering forced-switching loans instead of regular-switching loans.

## 1.6.2 Results

### 1.6.2.1 Regular-switching Loans

Table 1.11 presents the results. Column (1) shows that when matching on all the loan and firm characteristics defined in Table 1.10, regular-switching loans obtain a discount of 26.3 bps (significant at 1% level) as compared to non-switching loans. This result is not surprising since firms are expected to switch only when they receive an offer which is better than one from their inside bank. Using similar sets of matching variables, Ioannidou and Ongena (2010) found a discount between 82.2 bps and 97.2 bps in Bolivia in 1999-2003. Table 1.11, column (2) shows that the results remain similar if matching restrictions are somewhat relaxed: the estimated discount is 22.2 bps and significant at the 1% level. By increasing the matching window for all continuous variables from ±30% to ±70% and dropping a few variables in column (2), we increase the number of observations from 112 to 181,260. Two loans are matched if they were issued in the same quarter by the same bank to two firms of similar size (proxied by total debt to banks), similar riskiness (proxied by a dummy equal to 1 if a firm delayed at least one repayment in one prior year, and zero otherwise), and similar average length of prior relationships, and if the two loans were of the same type, similar amount, similar proportion of loan amount collateralized, and similar maturity. When using only these variables, we have enough observations to estimate switching discounts for the three loan types separately: 22.4 bps (significant at 1% level) for leasing in column (3), 7.7 bps (significant at 5% level) for term loans in column (4) and 31.8 bps (significant at the 1% level) for credit lines in column (5).

### 1.6.2.1 Forced-switching Loans

In order to have sufficient observations to compare forced-switching loans with non-switching loans, we apply the set of matching variables used in Table 1.11 column (2). Table 1.12 presents the results. Column (1) considers former “exclusive” “Distressed bank’s” customers not assigned to “bad bank” as forced-switchers, and shows a statistically insignificant switching discount of 3.1 bps. This suggests that after losing their lending relationships with “Distressed bank” and switching to new lenders, these firms on average were offered interest rates similar to ones received by old customers of the same lenders. The average loan rate spread remains small (7.0 bps) and statistically insignificant (std. error = 5.9 bps) when including all surviving “Distressed bank’s” customers.

Bonfim et al. (2019) also find insignificant discounts for forced-switchers and interpret this as evidence that information asymmetries are the primary source of switching costs. The interpretation follows from an empirical prediction of von Thadden’s (2004) model that explains regular-switching discounts as successful randomized attempts of uninformed outside banks to attract firms from better informed inside banks. Thus, if an inside bank does not exist, there is no reason to offer a discount. If, instead, shoe-leather switching costs and the competition for the market share were causing discounts (Klemperer, 1987), a firm should receive one regardless of whether it has an inside bank or not.

In columns (2) and (3) of Table 1.12, we split the matched loan pairs into two groups and re-estimate the discounts. Column (2) (column (3)) considers loan pairs, in which non-switchers had long (short) prior lending relationships, i.e. on average longer (shorter) than 6 years. We find that forced-switchers borrowed more expensively by 19.7 bps than similar long-term customers but cheaper by 18.1 bps than similar short-term customers of the same banks. This suggests that banks overcharge their customers in the short run but undercharge them in the long run. We test this further in section 1.7.

For purposes of generalizability and comparability with Bonfim et al. (2019), columns (4) to (6) of Table 1.12 include forced-switchers from other banks – “Healthy bank” and the bank which closed in 2011 q4. The results in columns (4) to (6) remain similar to those in columns (1) to (3).

## 1.7 How Do Interest Rates Depend on Lending Relationship Length?

Our results in section 1.5 suggest that “Distressed bank” overcharged its short-term customers more than long-term customers. Similarly, the results in section 1.6 suggest that other banks on average overcharged their short-term customers but undercharged their long-term customers. In this section, we test if this pattern is generally true in the Lithuanian market. The link between relationship length and loan interest rates has been heavily studied in the relationship lending literature (e.g. Petersen and Rajan, 1994; Berger and Udell, 1995; Lopez-Espinosa et al., 2017). Few studies, however, analyzed very long relationships. To the best of our knowledge, only Lopez-Espinosa et al. (2017) tracked the complete lengths of lending relationships but studied only one

Spanish bank. They found a concave link between relationship length and interest rate, which is in line with our results in sections 1.5 and 1.6.

### 1.7.1 Empirical Strategy

Figure 1.5 displays average interest rates of all newly issued debt contracts in the first year of our sample period (2011 q4 – 2012 q3), grouped by the length of relationships between borrowers and lenders. We restrict the sample period to one year in order to avoid the influence from the downward trend of interest rates over time. The graph suggests that interest rates increase in the first years of relationships, stay elevated until the 6$^{th}$ year, and then start decreasing. This pattern could be driven by changes in other loan characteristics or by the survivorship of the best-quality firms. In order to account for these possibilities, we employ two different techniques: (1) loan matching and (2) panel regression.

#### 1.7.1.1 Loan Matching

Arguably, interest rates on loans are decided jointly with other loan characteristics, i.e. loan size, maturity, and collateral. Therefore, regressing an interest rate on a relationship length and including endogenous loan characteristics as controls could bias the results. Different studies tackle the endogeneity by using the distance between a firm and a bank as an instrumental variable (Bolton et al., 2016; Beck et al., 2018), applying propensity score matching (Li et al., 2017), using simultaneous equations (Bharath et al., 2011), omitting endogenous controls (Schäfer, 2018) or assuming that interest rates are set after deciding other loan characteristics (Bharath et al., 2011). We follow Ioannidou and Ongena (2010), who use a non-parametric approach – matching similar loans on other loan characteristics. In this way, we do not need to assume anything about the sequence of loan pricing.

We follow a procedure similar to the one described in section 1.6.1. First, we identify all newly issued switching and non-switching loans based on definitions in Table 1.8. Consistently with sections 1.5 and 1.6, we consider the three most popular loan types: leasing, term loans, and credit lines. Second, every switching loan is matched with as many as possible subsequent non-switching loans issued to the same firm by the same bank. The loans are matched if they are of the same loan type and similar maturity, loan amount and collateral size. We use repayment delays to account for changes in firms' quality over time in two ways and, thus, we run our analysis twice: first, matching two loans if the borrower either delayed at least one repayment within one year before taking each of these loans, or did not delay any repayments before taking each of these loans, and second, considering only those firms which never delayed any repayment within our sample period.

Third, for each matched pair, we calculate an interest rate spread between a non-switching loan and a switching loan and a time gap between the two loans. Based on the time gap, each pair gets assigned one of six yearly time-gap dummies: up to 1 year, between 1 and 2 years, between 2 and 3 years, between 3 and 4 years, between 4 and 5 years and more than 5 years. Estimated interest rate spreads are regressed on a set of these time-gap dummies. We control for time trends by subtracting a 3-month Euribor rate from every interest rate reported at the end of the quarter and by including switching loans' time fixed effects. Standard errors are clustered at a switching loan level.

#### 1.7.1.2 Panel Regression

In order to control better for firms' quality, age and other time-varying firm characteristics, we run a panel regression similar to Lopez-Espinosa et al. (2017) but controlling for firm-x-time fixed effects. The identification stems from firms that in the same quarter borrowed from at least two different banks with which they had different relationship lengths. In addition, we control for firm-x-bank[27] fixed effects, bank-x-time[28] fixed effects and loan type fixed effects. We estimate the following regression model using all newly issued leasing contracts, term loans and credit lines between 2011 q4 and 2018 q1.

$$interest\_rate_{l,f,b,q} = \alpha + \beta_1 \times ln\,(relationship\_length_{f,b,q}) + \beta_2 \times ln^2(relationship\_length_{f,b,q}) + \beta_3 \times time\_to\_maturity_{l,f,b,q} + \beta_4 \times perc\_collateral_{l,f,b,q} + \beta_5 \times loan\_size_{l,f,b,q} + firm \times quarter\ FE + firm \times bank\ FE + bank \times quarter\ FE + loantype\ FE + \epsilon_{l,f,b,q} \tag{7}$$

where,

- $interest\_rate_{l,f,b,q}$ is the interest rate charged for the newly issued loan $l$, taken by firm $f$, from bank $b$, in quarter $q$.
- $relationship\_length_{f,b,q}$ is the length of the relationship between firm $f$ and bank $b$ in quarter $q$ measured in quarters. Relationship lengths are measured from 1995 to 2018.
- $time\_to\_maturity_{l,f,b,q}$ is time to maturity of the issued loan.
- $perc\_collateral_{l,f,b,q}$ is the amount of the collateral divided by the size of the loan.
- $loan\_size_{l,f,b,q}$ is the outstanding amount of the loan.
- FE stands for "fixed effects".

In line with Lopez-Espinosa et al. (2017), we use the logarithm of relationship length and its square. The logarithm ensures that an extra year in a long-lasting relationship has

[27] For instance, if the manager of a firm personally knows the manager of a bank, this could lead to both longer lending relationships and lower interest rates. Firm-x-bank – fixed effects control for this and similar possibilities.

[28] For instance, if a bank occasionally engages in promotion campaigns, this could affect its relationships and interest rates at the same time. Bank-x-time – fixed effects control for this and similar possibilities.

less impact on the interest rate than an extra year in a short relationship. Including a squared logarithm allows us to capture non-linear dynamics, which could resemble the shape of dynamics between the lending relationship length and the interest rate depicted in Figure 1.5. We expect to obtain a positive coefficient on the logarithm of relationship length and a negative coefficient on its square.

### 1.7.2 Results

#### 1.7.2.1 Loan Matching

Results are presented in Table 1.13. All six estimated dummy coefficients on time-gap dummies are significant at the 1 % level and are equal to 49.3 for a time gap smaller than 1 year, 55.2 for a gap between 1 and 2 years, 46.1 for a gap between 2 and 3 years, 37.6 for a gap between 3 and 4 years, 31.3 for a gap between 4 and 5 years and -41.6 for a gap larger than 5 years. This suggests that after switching to a new bank and receiving a discount of 22.2 bps (estimated in Table 1.11, column 2) the following loans taken in the next 5 years from the same bank are on average more expensive, which is broadly in line with Ioannidou and Ongena (2010). Loans taken within the first year of a relationship are on average more expensive by 49.3 bps than the switching loan. Loans taken within the second year of the relationship are on average more expensive by 55.2 bps than the switching loan etc.

However, after two years the interest rate starts to decrease, and after five years on average it drops below rates received on the switching loan. The loan rate dynamics are depicted in Figure 1.6.1. The results are robust if we take into account only those firms which never delayed any repayment throughout our sample period (time-gap dummies are reported in the last row of Table 1.13).

#### 1.7.2.2 Panel Regression

The results of the panel regression are consistent with the results of the loan matching analysis. Table 1.14, column (4) presents the results of our benchmark regression specification (7). The coefficient on the logarithm of relationship length is positive (significant at 1% level) while the coefficient on its square is negative (significant at 10% level). The results are even more significant in column (5) (both coefficients significant at 1% level) where we additionally control for fixed effects of loan-type interacted with time, firm and bank. Column (6) shows that the results are virtually the same if we drop possibly endogenous controls for loan characteristics. Table 1.14 also reveals that it is important to control for firm-x-time fixed effects (column 3) in order to capture the nonlinear dynamics, although they are also partially captured by using non-interactive firm, bank, time and loan-type fixed effects (column 2). Without absorbing any fixed effects, the coefficients are insignificant (column 1).

Predicted values of regression specifications in columns (4) and (5) are displayed in Figure 1.6.2. They indicate that, on average, interest rates rise sharply by roughly 0.5% in the first year of a new lending relationship and then start decreasing until, after 6 years, rates fall below the initial levels. These results are in line with the interest rate dynamics estimated using the loan matching analysis.

Overall, our results suggest that even when controlling for time varying firm characteristics, banks overcharge their customers in the short run but undercharge them in the long run. This pattern is consistent with Sharpe's (1990) reputational concerns: by treating their long-term customers well, banks may signal that lending relationships eventually pay off even if they are costly in the short run.

## 1.8 Conclusion

We use bank closures and loan-level data provided by the Bank of Lithuania to examine firms' loan interest rates in order to understand how costly can it be for firms to switch banks, what causes these switching costs, and whether banks exploit these switching costs by extracting rents from their customers. We find that after the respective bank closures, loan rates of "Healthy bank's" surviving customers did not change, while loan rates of "Distressed bank's" surviving customers dropped sharply and permanently by 42 bps on average. We address concerns regarding endogenous firm selection in several ways and show that the drop is significant even in the most conservative setting with all firms and Heckman's (1979) correction. This suggests that "Distressed bank" overcharged its customers and since they paid these rents instead of switching, switching costs must have been even higher. The contrasting evidence between the two banks suggest that bank's health can affect its reputational concerns (Boot et al., 1993), which, in turn, can affect a decision to extract rents (Sharpe, 1990). Further studies using multiple bank closures are needed for more conclusive evidence on health effects.

In line with informational hold-up theories (Sharpe, 1990; Rajan, 1992; von Thadden, 2004), the following evidence suggests that switching costs stem primarily from information asymmetries as opposed to other shoe-leather switching costs. Firstly, we find that the opaquest firms, i.e. small, young and lacking other lending relationships, were overcharged the most. For instance, an average overcharge for exclusive "Distressed bank's" customers was 3.1 pp, which considering their loan amounts, summed up to 2.5 yearly average Lithuanian salaries in 2012. Secondly, we find that, "Distressed bank" overcharged its "good" but not "bad" customers. Thirdly, we find no significant effect of the bank closure on loan characteristics other than interest rates. Fourthly, in line with Bonfim et al. (2019), we find that regular switchers received switching discounts but forced-switchers did not.

Our results shed light on loan pricing dynamics of healthy banks as well and suggest that relationships with healthy banks on average are neither beneficial nor harmful. First, we find that customers of "Healthy bank" were not affected by the bank closure, and second, old customers of healthy banks borrowed at the same rates as newly incoming forced-switchers from the closed banks. Yet, we find that banks tend to overcharge their customers in the beginning of relationships but undercharge them in the long run. This again suggests that reputational concerns matter (Sharpe, 1990).

Our setting allows us to demonstrate for the first time how hold-up costs disappear when a bank is shut down. In this way, we provide a novel identification of the hold-up problem and one of the first empirical estimates of firms' switching costs in the loan market. We expect that our results could be replicated in other loan markets, especially, characterized by small, opaque and bank-dependent firms and a high concentration of banks. Furthermore, our study is relevant for other markets where information asymmetries may cause switching costs, e.g. the labor market.

Finally, our results have policy implications. First, we show that an isolated failure of a non-systemic bank could be best resolved by a bank closure, since it would release good-quality firms from hold-up and allow them to borrow cheaper. Good-quality firms matter in particular due to their productivity (Caballero et al., 2008) and employment (Falato and Liang, 2016). Second, we show that a presence of a credit bureau, even providing detailed ten-year-credit-histories, is not enough to eliminate information asymmetries and to avoid hold-up problems. Therefore, policy-makers might as well consider ways to reduce these asymmetries further, e.g. with measures against loan evergreening. Third, our results suggest that an upward divergence of average loan rates might indicate bank distress. Thus, monitoring these rates could help policy-makers protect financial stability.

## Figures of Chapter 1

FIGURE 1.1.1

Proportion of each bank's customers with delayed repayments

Figure 1.1.1 shows the proportion of each bank's customers that delayed at least one repayment on any debt contract within one year prior to January 1, 2013. A bank's customer is defined as a firm which had any amount of debt outstanding with that bank for any period of time within one year prior to January 1, 2013.

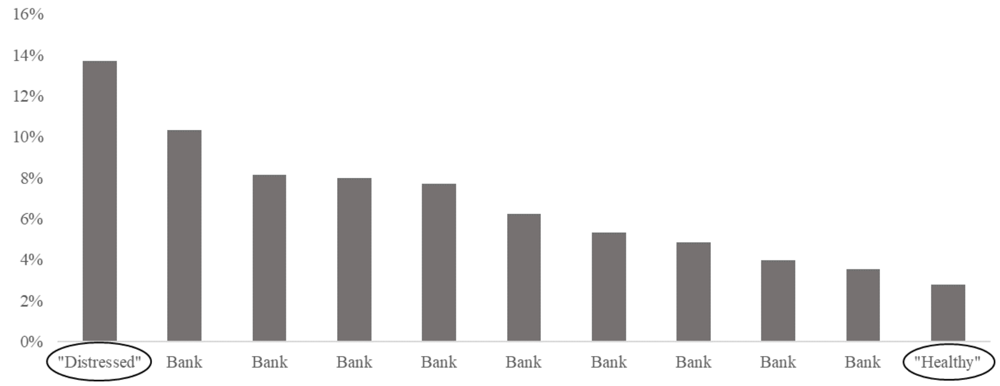



FIGURE 1.1.2

Loan rates of each bank's customer group

Figure 1.1.2 shows a weighted average interest rate (weighted by outstanding loan amount) calculated for each bank's customer group across all contracts (term loans, leasing and credit lines) outstanding and across all quarters within one year prior to January 1, 2013. A bank's customer is defined as a firm which had any amount of debt outstanding with that bank for any period of time within one year prior to January 1, 2013.

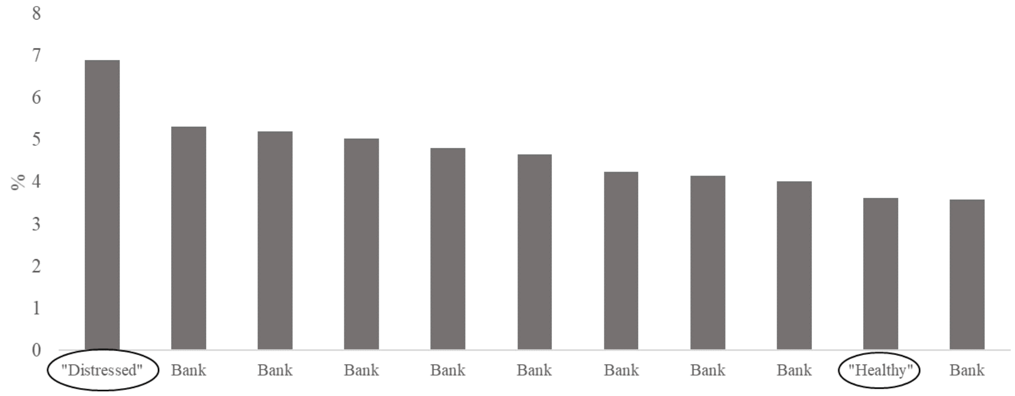


FIGURE 1.2.1

Borrowing costs of “good” customers of “Distressed bank”

Figure 1.2.1 complements the results of Table 1.2, regression specification (1). The figure shows how average borrowing costs of two groups of firms: “good” customers of “Distressed bank” and “good” customers of all other banks, evolve over time. A firm is considered a customer of a bank if it had any debt with that bank within one year prior to 2013 February 12 (failure of “Distressed bank”). This shock is marked by the vertical line. Borrowing costs for each firm equal an average interest rate weighted by loan outstanding amounts at each quarter. Leasing, term loans and credit lines are considered. After the shock, only contracts issued after the shock are considered. The chart considers only “good” firms, i.e. those that reappeared in the credit register (survived) after the shock, were not assigned to the “bad bank” by KPMG and did not borrow from “Acquiring bank” after the shock.

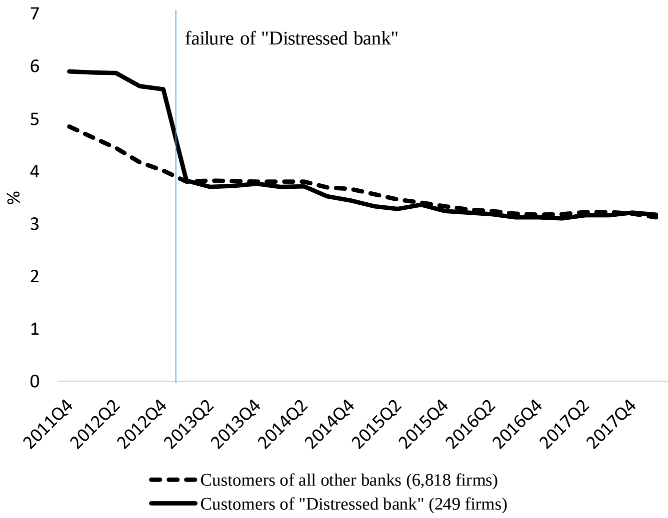



FIGURE 1.2.2

Borrowing costs of “good” “exclusive” customers of “Distressed bank”

Figure 1.2.2 complements the results of Table 1.2, regression specification (2). The figure shows how average borrowing costs of four groups of firms: “exclusive” and “non-exclusive” “good” customers of “Distressed bank” and exclusive and non-exclusive good customers of all other banks, evolve over time. A firm is considered a customer of a bank if it had any debt with that bank within one year prior to 2013 February 12 (failure of “Distressed bank”). This shock is marked by the vertical line. If a firm had debts only with that one bank, it is an “exclusive” customer. Borrowing costs for each firm equal an average interest rate weighted by loan outstanding amounts at each quarter. Leasing, term loans and credit lines are considered. After the shock, only contracts issued after the shock are considered. The chart considers only “good” firms, i.e. those that reappeared in the credit register (survived) after the shock, were not assigned to the “bad bank” by KPMG and did not borrow from “Acquiring bank” after the shock.

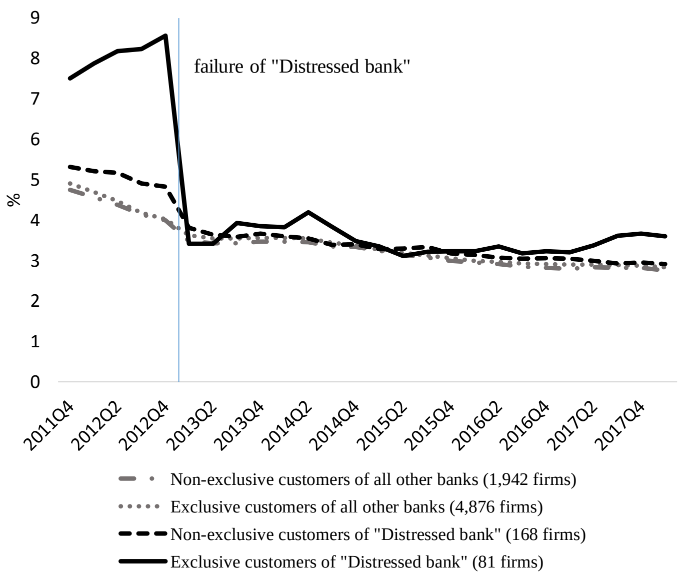


FIGURE 1.2.3

Borrowing costs of “good” “exclusive” “small” customers of “Distressed bank”

Figure 1.2.3 complements the results of Table 1.2, regression specification (3). The figure shows how average borrowing costs of four groups of firms: “exclusive” and “non-exclusive”, “large” and “small” “good” customers of “Distressed bank” evolve over time. A firm is considered a customer of a bank if it had any debt with that bank within one year prior to 2013 February 12 (failure of “Distressed bank”). This shock is marked by the vertical line. If a firm had debts only with that one bank, it is an “exclusive” customer. If a firm’s total maximum debt to banks from 2011 q4 to 2013 q1 was smaller than a median, it is a “small” customer. Borrowing costs for each firm equal an average interest rate weighted by loan outstanding amounts at each quarter. Leasing, term loans and credit lines are considered. After the shock, only contracts issued after the shock are considered. The chart considers only “good” firms, i.e. those that reappeared in the credit register (survived) after the shock, were not assigned to the “bad bank” by KPMG and did not borrow from “Acquiring bank” after the shock.

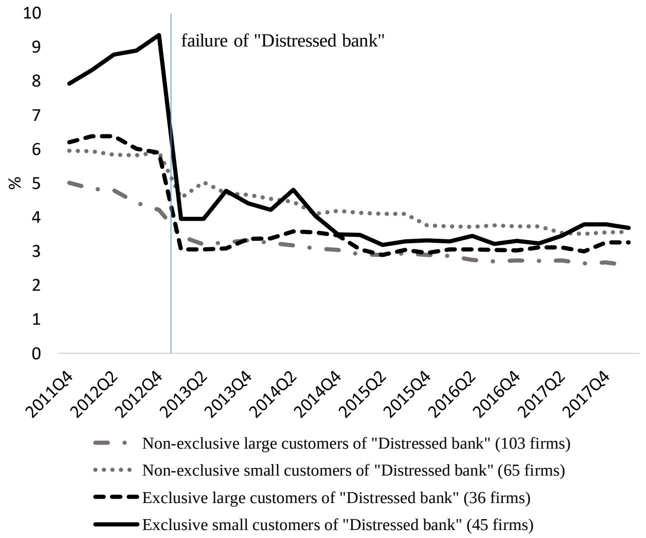
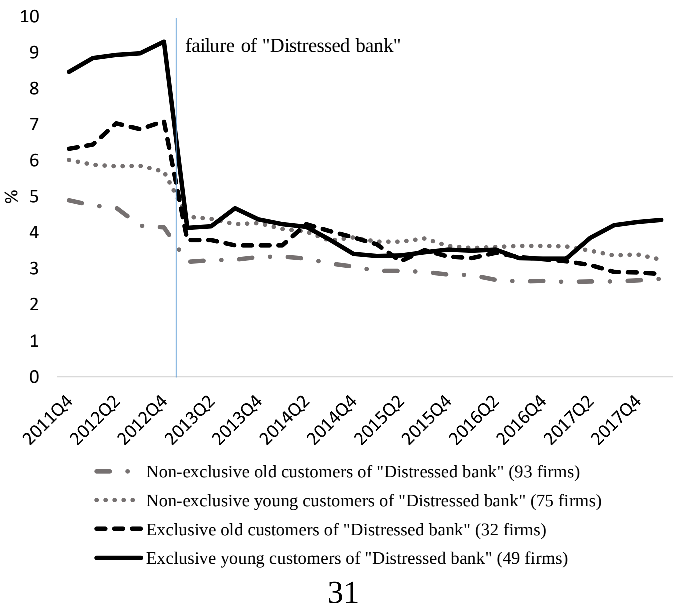



FIGURE 1.2.4

Borrowing costs of “good” “exclusive” “young” customers of “Distressed bank”

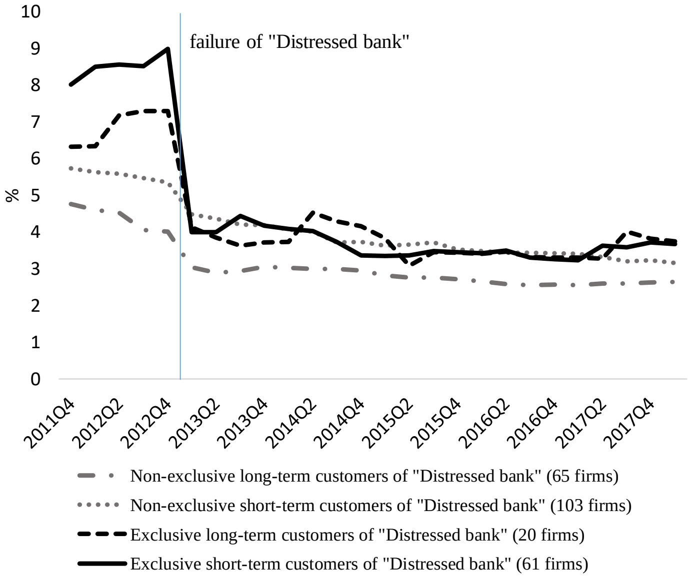
Figure 1.2.4 complements the results of Table 1.2, regression specification (4). The figure shows how average borrowing costs of four groups of firms: “exclusive” and “non-exclusive”, “old” and “young” customers of “Distressed bank” evolve over time. A firm is considered a customer of a bank if it had any debt with that bank within one year prior to 2013 February 12 (failure of “Distressed bank”). This shock is marked by the vertical line. If a firm had debts only with that one bank, it is an “exclusive” customer. If a firm’s first appearance on the credit register was less than six years ago as of 2013 q1, it is a “young” customer. Borrowing costs for each firm equal an average interest rate weighted by loan outstanding amounts at each quarter. Leasing, term loans and credit lines are considered. After the shock, only contracts issued after the shock are considered. The chart considers only “good” firms, i.e. those that reappeared in the credit register (survived) after the shock, were not assigned to the “bad bank” by KPMG and did not borrow from “Acquiring bank” after the shock.



## FIGURE 1.2.5

## Borrowing costs of “good” “exclusive” “short-term” customers of “Distressed bank”

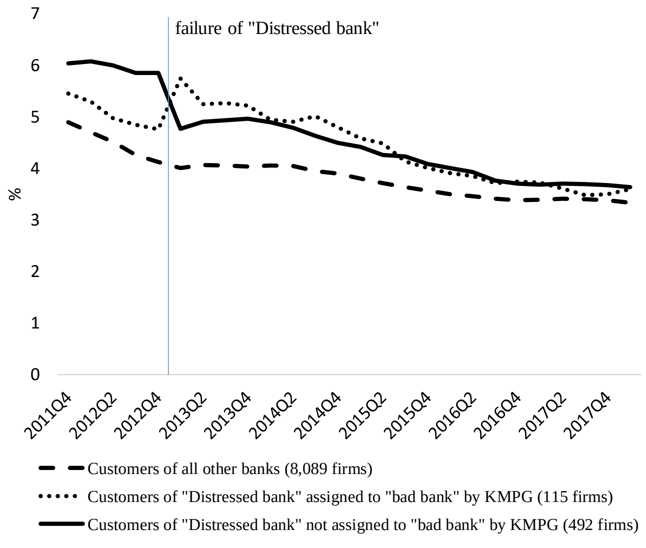
Figure 1.2.5 complements the results of Table 1.2, regression specification (5). The figure shows how average borrowing costs of four groups of firms: “exclusive” and “non-exclusive”, “long-term” and “short-term” “good” customers of “Distressed bank” evolve over time. A firm is considered a customer of a bank if it had any debt with that bank within one year prior to 2013 February 12 (failure of “Distressed bank”). This shock is marked by the vertical line. If a firm had debts only with that one bank, it is an “exclusive” customer. If a firm’s average relationship length with its banks in 2013 q1 was shorter than 6 years, it is a “short-term” customer. Borrowing costs for each firm equal an average interest rate weighted by loan outstanding amounts at each quarter. Leasing, term loans and credit lines are considered. After the shock, only contracts issued after the shock are considered. The chart considers only “good” firms, i.e. those that reappeared in the credit register (survived) after the shock, were not assigned to the “bad bank” by KPMG and did not borrow from “Acquiring bank” after the shock.




## FIGURE 1.3.1

## Borrowing costs of customers of “Distressed bank” assigned to “Good” and “Bad” banks by KPMG

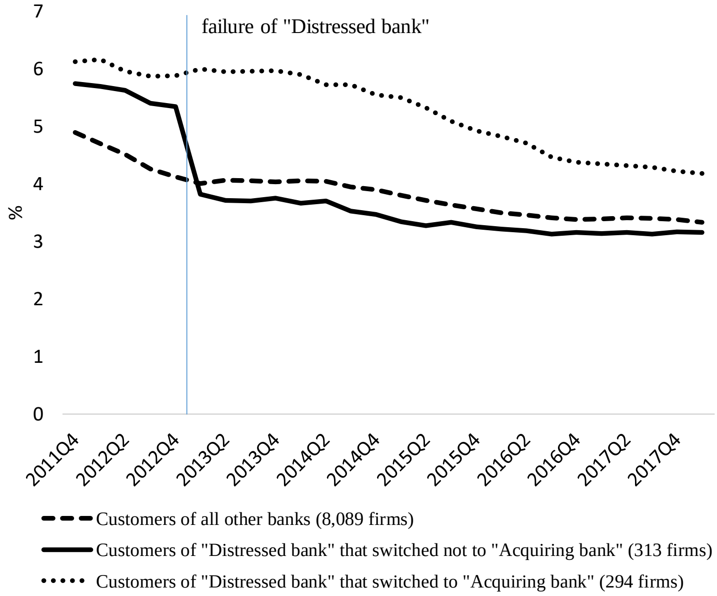
Figure 1.3.1 complements the results of Table 1.4, column (1). The figure shows how average borrowing costs of three groups of firms: customers of “Distressed bank” assigned to the “good bank” by KMPG, customers of “Distressed bank” assigned to the “bad bank” by KMPG and customers of all other banks, evolve over time. A firm is considered a customer of a bank if it had any debt with that bank within one year prior to 2013 February 12 (failure of “Distressed bank”). This shock is marked by the vertical line. Borrowing costs for each firm equal an average interest rate weighted by loan outstanding amounts at each quarter. Leasing, term loans and credit lines are considered. After the shock, only contracts issued after the shock are considered. The chart considers all firms that reappeared in the credit register (survived) after the shock.



# FIGURE 1.3.2

## Borrowing costs of customers of “Distressed bank” that switched to “Acquiring bank”

Figure 1.3.2 complements the results of Table 1.4, column (2). The figure shows how average borrowing costs of three groups of firms: customers of “Distressed bank” that stayed with the acquiring bank, customers of “Distressed bank” that switched to other banks and did not borrow from the acquirer and customers of all other banks, evolve over time. A firm is considered a customer of a bank if it had any debt with that bank within one year prior to 2013 February 12 (failure of “Distressed bank”). This shock is marked by the vertical line. Borrowing costs for each firm equal an average interest rate weighted by loan outstanding amounts at each quarter. Leasing, term loans and credit lines are considered. After the shock, only contracts issued after the shock are considered. The chart considers all firms that reappeared in the credit register (survived) after the shock.




# FIGURE 1.3.3

## Borrowing costs of ex-ante “good” and “bad” customers of “Distressed bank”

Figure 1.3.3 complements the results of Table 1.4, columns (3) and (4). The figure shows the evolution of average borrowing costs of three groups of firms: customers of “Distressed bank” in the top and bottom quartiles in terms of pre-shock lowest collateralization, measured as a ratio of collateral value over loan outstanding amount within one year before the shock, and customers of all other banks. A firm is considered a customer of a bank if it had any debt with that bank within one year prior to 2013 February 12 (failure of “Distressed bank”). This shock is marked by the vertical line. Borrowing costs for each firm equal an average interest rate weighted by loan outstanding amounts at each quarter. Leasing, term loans and credit lines are considered. After the shock, only contracts issued after the shock are considered. The chart considers all firms that reappeared in the credit register (survived) after the shock.

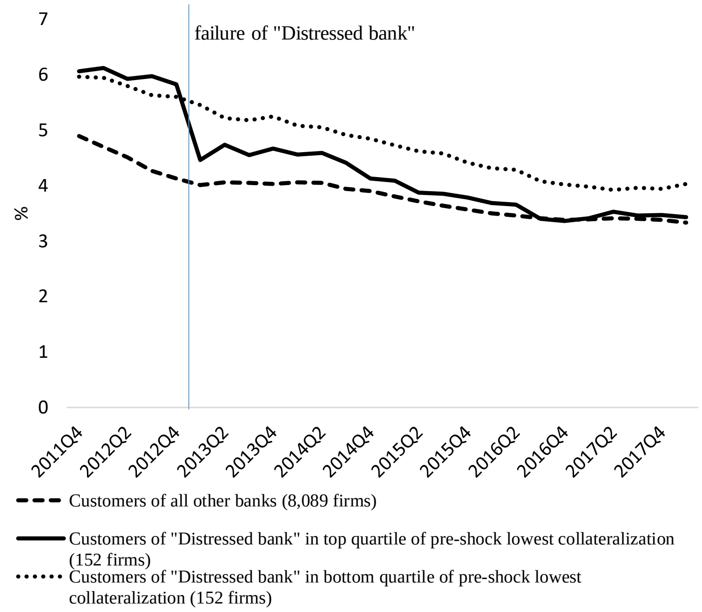


FIGURE 1.3.4

Borrowing costs of all surviving customers of “Distressed bank”

Figure 1.3.4 complements the results of Table 1.4, column (5). The figure shows how average borrowing costs of two groups of firms: all customers of “Distressed bank” and customers of all other banks, evolve over time. A firm is considered a customer of a bank if it had any debt with that bank within one year prior to 2013 February 12 (failure of “Distressed bank”). This shock is marked by the vertical line. Borrowing costs for each firm equal an average interest rate weighted by loan outstanding amounts at each quarter. Leasing, term loans and credit lines are considered. After the shock, only contracts issued after the shock are considered. The chart considers all firms that reappeared in the credit register (survived) after the shock.

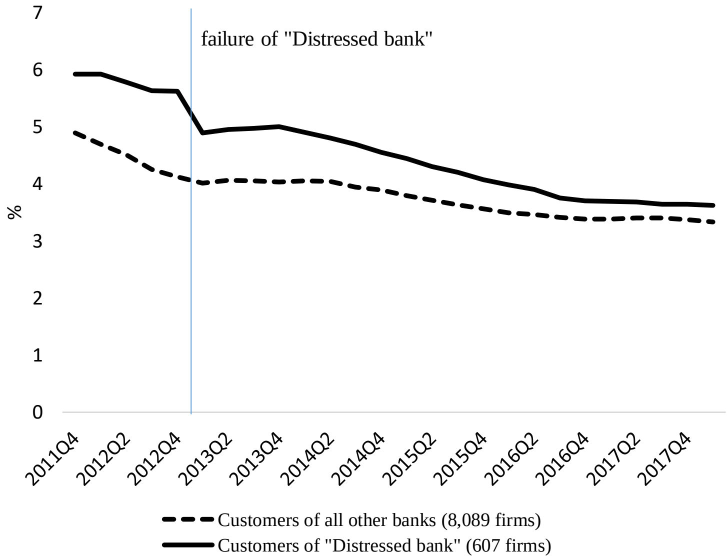



FIGURE 1.4

Borrowing costs of customers of “Healthy bank” vs. all other firms

Figure 1.4 complements the results of Table 1.7, regression specification (1). The figure shows how average borrowing costs of two groups of firms: surviving customers of “Healthy bank” and surviving customers of all other banks, evolve over time. A firm is considered a customer of a bank if it had any debt with that bank within one year prior to 2013 January 30 (the day of “Healthy bank’s” decision to stop business). This shock is marked by the vertical line. Borrowing costs for each firm equal an average interest rate weighted by loan outstanding amounts at each quarter. Leasing, term loans and credit lines are considered. After the shock, only contracts issued after the shock are considered. The chart considers only those firms that reappeared in the credit register (survived) after the shock.

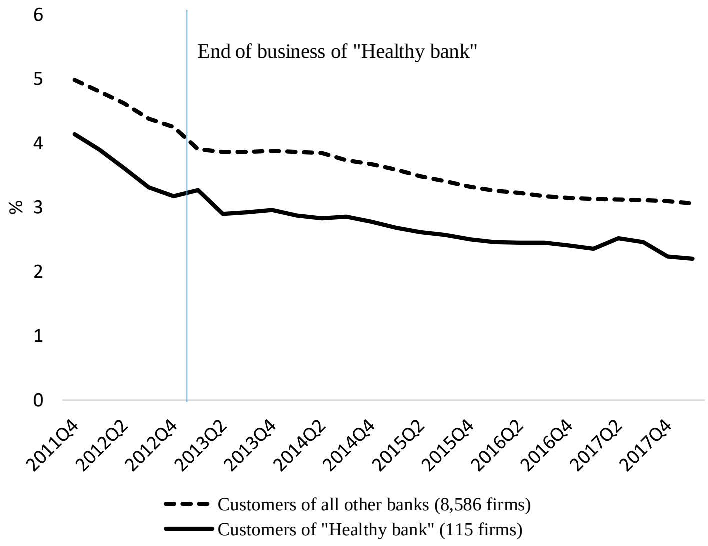


FIGURE 1.5

(Uncontrolled) Link between interest rate and firm-bank relationship length

Figure 1.5 shows the relationship between firm-bank relationship length and the interest rates charged. All new debt contracts issued in the first year of the sample, i.e. 2011 q4 – 2012 q3, were grouped by years of relationship between a lender and a borrower. The sample period in this graph is limited to one year in order to avoid the influence of the downward interest rate trend over time. Average interest rate and a 95% confidence interval is plotted for each group.

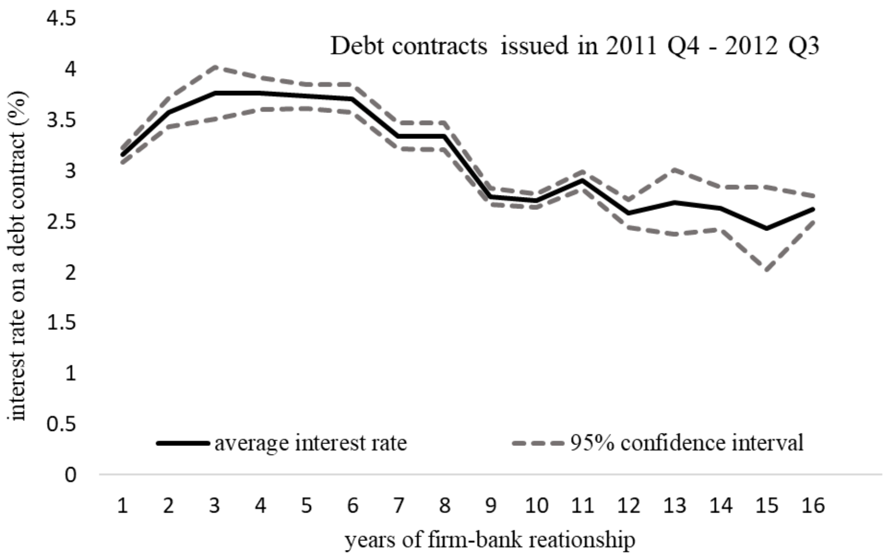



FIGURE 1.6.1

Results of the loan matching analysis: the development of interest rates after switching

Figure 1.6.1 complements the results of Table 1.11 and Table 1.13. The figure shows the development of average interest rates throughout the relationship time. The first observation (taken from Table 1.11, column 2) indicates the average discount firms receive when they voluntarily switch to other banks and start new lending relationships. The rest of observations (taken from Table 1.13) show how on average the rate at a new bank develops throughout years after switching.

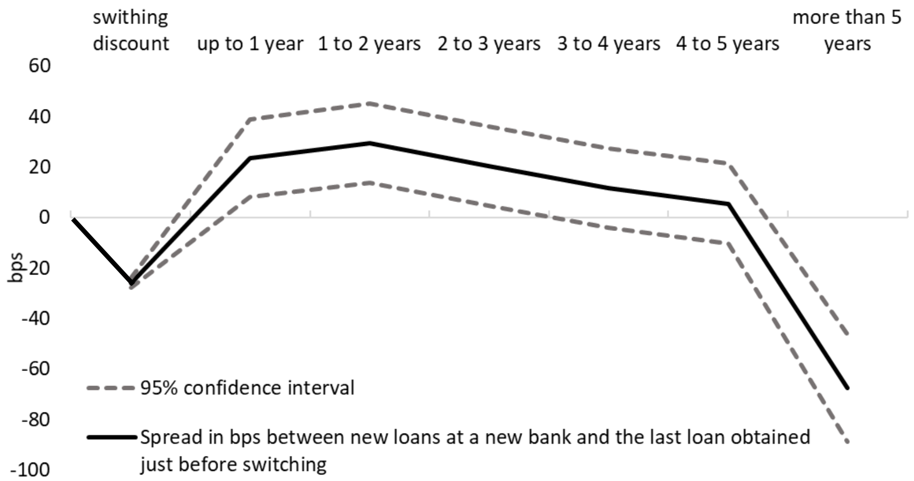
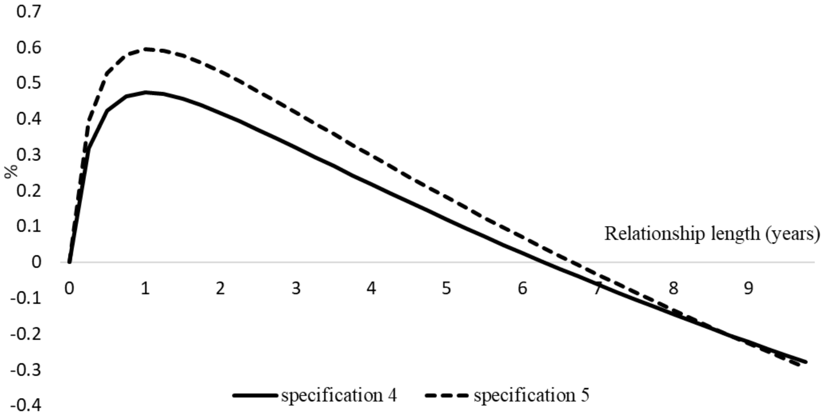


FIGURE 1.6.2

Panel regressions: relationship between interest rate and firm-bank relationship length

Figure 1.6.2 complements the results of Table 1.14. It plots predicted values of the panel regressions estimated in Table 1.14 columns 4 and 5. The regressions estimated a non-linear link between interest rate charged on newly issued loans (leasing contracts, term loans and credit lines issued between 2011 q4 and 2018 q1) and the relationship length between a bank and a firm.



# Tables of Chapter 1

TABLE 1.1a

Summary statistics of loan characteristics

Table 1.1a reports summary statistics of all the loans in the data sample. Statistics are split by loan type (the three most popular ones – leasing, term loans and credit lines – and other).

| Loantype | | Leasing | Term loans | Credit lines | Other | Total |
|---|---|---|---|---|---|---|
| Number of loans | | 131,238 | 24,507 | 7,847 | 27,136 | 190,728 |
| Number of loans collateralized | | 2,170 | 20,045 | 6,700 | 9,850 | 38,765 |
| Percentage of loans collateralized | | 2% | 82% | 85% | 36% | 20% |
| Loan size (EUR) average | | 49,443 | 1,039,184 | 696,573 | 374,643 | 249,509 |
| | *25th percentile* | 12,729 | 34,754 | 30,000 | 1,014 | 12,164 |
| | *median* | 23,364 | 113,143 | 94,127 | 10,000 | 25,809 |
| | *75th percentile* | 54,747 | 463,392 | 300,000 | 60,000 | 71,330 |
| Loan maturity (years) average | | 2.9 | 3.4 | 1.0 | 1.3 | 2.7 |
| | *25th percentile* | 1.8 | 1.3 | 0.5 | 0.5 | 1.0 |
| | *median* | 2.8 | 2.8 | 0.8 | 0.8 | 2.8 |
| | *75th percentile* | 4.3 | 4.8 | 1.5 | 1.8 | 4.0 |
| Loan interest rate (%) average | | 3.2 | 4.4 | 4.1 | 6.0 | 3.8 |
| | *25th percentile* | 1.9 | 2.9 | 2.8 | 1.4 | 2.0 |
| | *median* | 3.0 | 4.0 | 4.1 | 4.1 | 3.2 |
| | *75th percentile* | 4.2 | 5.5 | 5.4 | 8.8 | 4.8 |

TABLE 1.1b

Summary statistics of firm characteristics

The top part of Table 1.1b reports summary statistics of all the firms in the data sample. Statistics are split by firms' industry (the three most popular ones – Manufacturing, Retail/wholesale and Transportation – and other). The middle and bottom parts report firms' statistics at a fixed point of time – the beginning of the sample period – 2011 Q4.

| Firms' industry | | Manufacturing | Retail/Wholesale | Transportation | Other | Total |
|---|---|---|---|---|---|---|
| Number of firms | | 3,730 | 9,207 | 4,209 | 18,759 | 35,905 |
| Number of firms without delayed repayments between 2011-2018 | | 2,977 | 7,758 | 3,356 | 15,855 | 29,946 |
| Number of firms with delayed repayments between 2011-2018 | | 753 | 1,449 | 853 | 2,904 | 5,959 |
| Percentage of firms with delayed repayments between 2011-2018 | | 20% | 16% | 20% | 15% | 17% |
| Firm size (proxied as total debt to banks), average | | 1,633,159 | 818,025 | 912,740 | 1,605,811 | 1,325,397 |
| | *25th percentile* | 25,162 | 19,720 | 26,341 | 12,200 | 16,492 |
| | *median* | 97,476 | 57,924 | 86,440 | 40,000 | 52,896 |
| | *75th percentile* | 437,597 | 228,600 | 303,427 | 200,417 | 246,129 |
| Number of firms at the beginning of the sample - 2011Q4 | | 2,073 | 4,684 | 2,161 | 8,348 | 17,266 |
| Firm size (proxied as total debt to banks) at 2011Q4, average | | 1,143,963 | 609,510 | 643,322 | 1,215,557 | 970,929 |
| | *25th percentile* | 27,239 | 20,273 | 33,819 | 14,771 | 19,028 |
| | *median* | 101,348 | 57,784 | 101,367 | 44,779 | 59,923 |
| | *75th percentile* | 434,430 | 225,705 | 322,799 | 257,414 | 275,412 |
| Number of firms with a single relationship at 2011Q4* | | 1,446 | 3,514 | 1,512 | 6,858 | 13,330 |
| Number of firms with multiple relationships at 2011Q4* | | 627 | 1,170 | 649 | 1,490 | 3,936 |
| Number of firms with short (average<6y) relationships at 2011Q4* | | 1,267 | 3,054 | 1,499 | 5,933 | 11,753 |
| Number of firms with long (average>6y) relationships at 2011Q4* | | 806 | 1,630 | 662 | 2,415 | 5,513 |

*a firm is said to have a relationship with a bank if it had some outstanding debt with that bank within the previous 12 months

## TABLE 1.1c

## Summary statistics of pre-shock firm characteristics

Table 1.1c reports for different subgroups of firms, average characteristics measured at firm-quarter level within one year before the "Distressed bank's" closure. Our main treatment group – "good" "Distressed bank's" customers are highlighted in *italics*. The table takes into account term loans, leasing contracts and credit lines.

| | # of firms | # of firms with repayment delays | % of firms with repayment delays | Average pre-shock borrowing costs (%) | Average pre-shock collaterali-zation (collateral value/loan amount) | Average pre-shock remaining time to maturity (in quarters) | Average pre-shock loan size (in euros) | Average pre-shock firm size proxied by total borrowings (in euros) | Average pre-shock firm age proxied by first appearance in the credit register (in years) | Average pre-shock length of lending relationships (in years) |
|---|---|---|---|---|---|---|---|---|---|---|
| **"Distressed bank's" customers** | **1204** | **224** | **19%** | **6.4** | **1.16** | **7.6** | **429,509** | **1,585,700** | **5.8** | **4.6** |
| **Assigned to "bad bank"** | **263** | **72** | **27%** | **5.2** | **1.18** | **7.5** | **618,906** | **1,504,691** | **6.6** | **5.3** |
| Took new loans after shock | 115 | 3 | 3% | 4.9 | 1.19 | 8.2 | 100,164 | 467,566 | 6.2 | 4.8 |
| No new loans after shock | 148 | 69 | 47% | 5.4 | 1.17 | 6.9 | 1,021,983 | 2,310,565 | 6.1 | 4.5 |
| **Not assigned to "bad bank"** | **941** | **152** | **16%** | **6.7** | **1.15** | **7.6** | **376,574** | **1,608,341** | **6.3** | **5.1** |
| Took new loans after shock | 492 | 43 | 9% | 5.9 | 0.99 | 7.5 | 288,612 | 2,222,564 | 6.9 | 5.8 |
| *Not from "Acquiring bank"* | *249* | *13* | *5%* | *5.7* | *0.77* | *7.2* | *307,649* | *3,013,206* | *6.9* | *5.8* |
| From "Acquiring bank" | 243 | 30 | 12% | 6.0 | 1.22 | 7.9 | 269,105 | 1,412,400 | 5.5 | 4.4 |
| No new loans after shock | 449 | 109 | 24% | 7.6 | 1.33 | 7.6 | 472,960 | 935,295 | 6.2 | 4.6 |
| **All other firms** | **16416** | **2559** | **16%** | **4.8** | **0.78** | **8.9** | **298,682** | **939,713** | **6.1** | **4.6** |
| Took new loans after shock | 8089 | 495 | 6% | 4.4 | 0.80 | 8.6 | 322,592 | 1,297,339 | 6.3 | 4.7 |
| No new loans after shock | 8327 | 2064 | 25% | 5.3 | 0.76 | 9.2 | 275,460 | 592,352 | 4.8 | 4.1 |
| **Total** | **17620** | **2783** | **16%** | **5.0** | **0.81** | **8.8** | **307,623** | **983,857** | **4.8** | **4.1** |

## TABLE 1.2

## Difference-in-differences analysis default setting: "good" firms

Table 1.2 reports coefficient estimates from difference-in-differences panel regressions (specifications 1, 2, 3, 4 and 5). The data used in the analysis is at the quarter-firm level. The dependent variable "borrowing_costs" is a firm's average interest rate weighted by loan outstanding amounts at each quarter. In quarters 2011q4 - 2012q4, we consider only leasing contracts, term loans and credit lines issued up to 2012 December 31. In quarters 2013q1 - 2018q1, we consider only leasing contracts, term loans and credit lines issued from 2013 February 12 (the day of "Distressed bank's" closure). The explanatory variables are dummies: "after" - equal to 1 if an observation is from quarters 2013q1 - 2018q1, and 0 otherwise; "closed" – equal to 1 if a firm belongs to the treatment group, i.e. had any debt outstanding with the closed "Distressed bank" within one year prior to 2013 February 12, and 0 otherwise; "exclusive" – equal to 1 if a firm had debts only with one bank within the same prior year, and 0 otherwise; "small" equal to 1 if a firm's maximum total debt to bank from 2011 q4 to 2013 q1 was smaller than median, and zero otherwise; "young" equal to 1 if a firm's first appearance in the credit register was less than median as of 2013 q1, and zero otherwise; "short_term" equal to 1 if a firm's average relationship length with its banks in 2013 q1 was shorter than 6 years, and 0 otherwise. Regressions include all double and triple interactions but, for brevity, only the interactions of interest are reported. Robust standard errors are clustered multiway at the firm and quarter levels. P-values are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, two-tailed, respectively.

| | Dependent variable: borrowing_costs | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| after x closed | -1.051*** | -0.556*** | -0.593*** | -0.437*** | -0.546*** |
| | (0.000) | (0.000) | (0.000) | (0.003) | (0.000) |
| after x closed x exclusive | | -2.544*** | -0.952** | -1.071** | -0.544 |
| | | (0.000) | (0.037) | (0.026) | (0.397) |
| after x closed x exclusive x small | | | -2.161** | | |
| | | | (0.020) | | |
| after x closed x exclusive x young | | | | -2.355** | |
| | | | | (0.021) | |
| after x closed x exclusive x short_term | | | | | -2.796*** |
| | | | | | (0.006) |
| Firm-fixed effects | YES | YES | YES | YES | YES |
| Time-fixed effects | YES | YES | YES | YES | YES |
| Number of observations | 120,756 | 120,756 | 120,756 | 120,756 | 120,756 |
| Adjusted R-squared | 0.700 | 0.703 | 0.705 | 0.705 | 0.706 |

P-values in parentheses. Standard errors are clustered multiway within firms and quarters

# TABLE 1.3

## Test of the parallel trends assumption

Table 1.3 presents the results of regressing quarterly firm-level borrowing costs on the time dummies interacted with the treatment variable "closed" equal to 1 if a firm belongs to the treatment group, i.e. had any debt outstanding with the closed "Distressed bank" within one year prior to 2013 February 12, and 0 otherwise. Both columns include time-fixed effects and firm-fixed effects. The base quarter is 2011 Q4 in Column (1) and 2013 Q1 in column (2). All sample restrictions and variable definitions are in line with the default setting in Table 1.2. P-values are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, two-tailed, respectively.

| | Dependent variable: borrowing_costs | |
|---|---|---|
| | (1) | (2) |
| closed x Dummy 2011Q4 | (omitted) | 0.657*** |
| | | (0.000) |
| closed x Dummy 2012Q1 | 0.227** | 0.884*** |
| | (0.015) | (0.000) |
| closed x Dummy 2012Q2 | 0.402*** | 1.059*** |
| | (0.000) | (0.000) |
| closed x Dummy 2012Q3 | 0.425*** | 1.082*** |
| | (0.000) | (0.000) |
| closed x Dummy 2012Q4 | 0.537*** | 1.194*** |
| | (0.000) | (0.000) |
| closed x Dummy 2013Q1 | -0.657*** | (omitted) |
| | (0.000) | |
| closed x Dummy 2013Q2 | -0.661*** | -0.004 |
| | (0.000) | (0.928) |
| closed x Dummy 2013Q3 | -0.668*** | -0.011 |
| | (0.000) | (0.863) |
| closed x Dummy 2013Q4 | -0.665*** | -0.008 |
| | (0.000) | (0.912) |
| closed x Dummy 2014Q1 | -0.699*** | -0.042 |
| | (0.000) | (0.631) |
| closed x Dummy 2014Q2 | -0.742*** | -0.085 |
| | (0.000) | (0.378) |
| closed x Dummy 2014Q3 | -0.811*** | -0.154 |
| | (0.000) | (0.165) |
| closed x Dummy 2014Q4 | -0.829*** | -0.172 |
| | (0.000) | (0.174) |
| closed x Dummy 2015Q1 | -0.820*** | -0.162 |
| | (0.000) | (0.197) |
| closed x Dummy 2015Q2 | -0.797*** | -0.140 |
| | (0.000) | (0.285) |
| closed x Dummy 2015Q3 | -0.795*** | -0.137 |
| | (0.000) | (0.327) |
| closed x Dummy 2015Q4 | -0.791*** | -0.134 |
| | (0.000) | (0.347) |
| closed x Dummy 2016Q1 | -0.757*** | -0.100 |
| | (0.000) | (0.479) |
| closed x Dummy 2016Q2 | -0.740*** | -0.083 |
| | (0.000) | (0.538) |
| closed x Dummy 2016Q3 | -0.741*** | -0.084 |
| | (0.000) | (0.541) |
| closed x Dummy 2016Q4 | -0.711*** | -0.054 |
| | (0.000) | (0.691) |
| closed x Dummy 2017Q1 | -0.701*** | -0.044 |
| | (0.000) | (0.746) |
| closed x Dummy 2017Q2 | -0.697*** | -0.040 |
| | (0.000) | (0.779) |
| closed x Dummy 2017Q3 | -0.728*** | -0.071 |
| | (0.000) | (0.624) |
| closed x Dummy 2017Q4 | -0.689*** | -0.032 |
| | (0.000) | (0.822) |
| closed x Dummy 2018Q1 | -0.669*** | -0.012 |
| | (0.000) | (0.933) |
| Constant | 3.442*** | 3.420*** |
| | (0.000) | (0.000) |
| Firm-fixed effects | Yes | Yes |
| Time-fixed effects | Yes | Yes |
| Number of observations | 120,756 | 120,756 |
| Adjusted R-squared | 0.700 | 0.700 |

P-values in parentheses. Standard errors are clustered multiway within firms and quarters.
*** p<0.01, ** p<0.05, * p<0.1

## TABLE 1.4

## Difference-in-differences analysis: “bad” and all surviving firms

Table 1.4 reports coefficient estimates of difference-in-differences panel regression specification (1) for different subsamples defined in the table. The first three columns use differently defined “bad” customers of “Distressed bank” as a treatment group, column (4) uses “good” customers, and columns (5) to (8) use all “good” and “bad” customers. The firm-quarter-level dependent variable “borrowing_costs” is a firm’s average interest rate weighted by loan amounts outstanding at each quarter. In quarters 2011q4 - 2012q4, we consider only leasing contracts, term loans and credit lines issued up to 2012 December 31. In quarters 2013q1 - 2018q1, we consider only leasing contracts, term loans and credit lines issued from 2013 February 12 (the day of “Distressed bank’s” closure). The explanatory variables are dummies: “after” - equal to 1 if an observation is from quarters 2013q1 - 2018q1, and 0 otherwise; “closed” – equal to 1 if a firm belongs to the treatment group, i.e. had any debt outstanding with the closed “Distressed bank” within one year prior to 2013 February 12, and 0 otherwise. Robust standard errors are clustered multiway within firms and quarters in columns (1) to (5), and within firms in columns (6) to (8). P-values are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, two-tailed, respectively.

| | Dependent variable: borrowing_costs | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | "Bad" firms | | | "Good" firms | All firms | | | |
| Treatment group: | Surviving “Distressed bank’s” customers assigned to the “bad bank” by KPMG | Surviving “Distressed bank’s” customers that took new loans from "Acquiring bank" after shock | Surviving “Distressed bank’s” customers in the bottom quartile in terms of the smallest collateral | Surviving “Distressed bank’s” customers in the top quartile in terms of the smallest collateral | All surviving "Distressed bank's" customers | All surviving "Distressed bank's" customers (Reduced sample)[1] | All "Distressed bank's" customers (with Heckman correction)[2] | All "Distressed bank's" customers (with Regression imputation)[3] |
| Control group: | All other surviving firms | All other surviving firms | All other surviving firms | All other surviving firms | All other surviving firms | All other surviving firms | All other firms | All other firms |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| after x closed | 0.270 | 0.168 | -0.095 | -1.073*** | -0.424*** | -0.400*** | -0.359*** | -0.354*** |
| | (0.140) | (0.284) | (0.572) | (0.001) | (0.001) | (0.001) | (0.000) | (0.000) |
| Firm-fixed effects | YES | YES | YES | YES | YES | YES | YES | YES |
| Time-fixed effects | YES | YES | YES | YES | YES | YES | YES | YES |
| Number of observations | 141,275 | 144,477 | 141,831 | 141,530 | 149,684 | 4,268 | 9,046 | 9,046 |
| Adjusted R-squared | 0.690 | 0.697 | 0.695 | 0.689 | 0.693 | 0.426 | - | 0.714 |

P-values in parentheses. Standard errors are clustered multiway within firms and quarters in columns (1) to (5), and within firms in columns (6) and (8).

[1] To reduce computational intensity, in columns (6) to (8), quarterly observations are averaged across quarters within two time periods – before and after the shock, and the control group is reduced by randomly selecting and keeping 20% of firms.

[2] In column (7), we use a two-step consistent estimator (and, thus, no error clustering) to estimate Heckman (1979) selection model, in which the outcome equation is the same as regression specification (1). The selection equation includes (besides variables from the outcome equation) four firm-level ex-ante characteristics measured within one year before the bank closure: (1) a dummy=1 if a firm had a repayment delay (coefficient = 0.960; p-value = 0.000), (2) an average ratio of collateral value over loan outstanding amount (coefficient = -0.022; p-value = 0.114), (3) an average loan size in million euros (coefficient = -0.019; p-value = 0.142) and (4) an average remaining time to maturity of outstanding loans measured in quarters (coefficient = -0.007; p-value = 0.010). For brevity we report only the coefficient of interest on the interaction term from specification (1). Lambda is equal to 0.838 and statistically significant at 1% level.

[3] In column (8), we use a regression imputation method. First, we estimate a change in borrowing costs for surviving firms from one quarter to the other. Second, we regress these changes on the treatment dummy, the four firm-level ex-ante characteristics from the Heckman selection equation described above, and all possible interactions between these variables. Third, we use the estimated model to predict changes in borrowing costs for non-surviving firms and then predict their second-period borrowing costs. Fourth, we estimate regression specification (1) using the full sample.

## TABLE 1.5

## Difference-in-differences analysis: placebo tests with other banks

Table 1.5 reports coefficient estimates of difference-in-differences panel regression specification (1) for different subsamples defined in the table. Every column uses all surviving customers of one of the banks as a treatment group and all other surviving firms as a control group. The firm-quarter-level dependent variable “borrowing_costs” is a firm’s average interest rate weighted by loan amounts outstanding at each quarter. In quarters 2011q4 - 2012q4, we consider only leasing contracts, term loans and credit lines issued up to 2012 December 31. In quarters 2013q1 - 2018q1, we consider only leasing contracts, term loans and credit lines issued from 2013 February 12 (the day of “Distressed bank’s” closure). The explanatory variables are dummies: “after” - equal to 1 if an observation is from quarters 2013q1 - 2018q1, and 0 otherwise; “closed” – equal to 1 if a firm belongs to the treatment group, i.e. had any debt outstanding with the closed “Distressed bank” within one year prior to 2013 February 12, and 0 otherwise. Robust standard errors are clustered multiway within firms and quarters. P-values are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, two-tailed, respectively.

| | Dependent variable: borrowing_costs | | | | |
|---|---|---|---|---|---|
| Treatment group: | Surviving customers of a bank with a survival rate 3,215/6,259 (51%) | Surviving customers of a bank with a survival rate 1,878/3,657 (51%) | Surviving customers of a bank with a survival rate 1,128/2,175 (52%) | Surviving customers of a bank with a survival rate 495/1,155 (43%) | Surviving customers of a bank with a survival rate 335/742 (45%) |
| Control group: | All other surviving firms | All other surviving firms | All other surviving firms | All other surviving firms | All other surviving firms |
| | (1) | (2) | (3) | (4) | (5) |
| after x treatment | 0.075 | -0.053 | 0.368** | -0.060 | 0.175 |
| | (0.389) | (0.493) | (0.032) | (0.479) | (0.432) |
| Firm-fixed effects | YES | YES | YES | YES | YES |
| Time-fixed effects | YES | YES | YES | YES | YES |
| Number of observations | 149,684 | 149,684 | 149,684 | 149,684 | 149,684 |
| Adjusted R-squared | 0.694 | 0.693 | 0.693 | 0.692 | 0.692 |

P-values in parentheses. Standard errors are clustered multiway within firms and quarters.

## TABLE 1.6

## Alternative explanations and robustness checks

Table 1.6 reports coefficient estimates from difference-in-differences panel regression specification (1) for 9 alterations of the default setting (see Table 1.2): In the default setting, dependent variable is firm-quarter level “borrowing_costs” (i.e. firm’s average interest rate weighted by loan outstanding amounts at each quarter). In quarters 2011q4 - 2012q4, we consider only leasing contracts, term loans and credit lines issued up to 2012 December 31. In quarters 2013q1 - 2018q1, we consider only leasing contracts, term loans and credit lines issued from 2013 February 12 (the day of “Distressed bank’s” closure). The explanatory variables are dummies: “after” - equal to 1 if an observation is from quarters 2013q1 - 2018q1, and 0 otherwise; “closed” – equal to 1 if a firm belongs to the treatment group, i.e. had any debt outstanding with the closed “Distressed bank” within one year prior to 2013 February 12, and 0 otherwise; and the interaction between the two. Only “good” firms are considered, i.e. those that appeared in the credit register both before and after the shock, were not assigned to the “bad bank” by KPMG, and, after the shock, switched for new loans to other banks than “Acquiring bank”. The alterations of this setting are as follows (by column):

1) Only term loans: “borrowing_costs” are calculated using term loans only.
2) Only leasing contracts: “borrowing_costs” are calculated using leasing contracts only.
3) Different control group: the control group includes only those firms that were customers of “Acquiring bank” – the most similar one to “Distressed bank”.
4) Only newly issued loans: only loans issued in every given quarter are considered.
5) Assigned neither to “bad” nor “good” bank: the treatment group comprises only those “Distressed bank’s” customers which had their assets and liabilities netted off during the bank closure and thus were not assigned either to “good bank” or to “bad bank”.
6) No first switching loans: the first loans taken by “Distressed bank’s” borrowers after they lost their sole lending relationships are excluded from the sample.
7) Dep. variable: collateral: the dependent variable is calculated using a percentage of loan collateralized (i.e. collateral value divided by the loan size) instead of a loan’s interest rate.
8) Dep. variable: maturity: the dependent variable is calculated using a loan’s time to maturity (in years) instead of a loan’s interest rate.
9) Dep. variable: loan size: the dependent variable is calculated using a loan’s amount (in thousands of euros) instead of a loan’s interest rate.

Robust standard errors are clustered multiway at the firm and quarter levels. P-values are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, two-tailed, respectively.

| | Dependent variable: borrowing_costs | | | | | | Different dependent variables | | |
|---|---|---|---|---|---|---|---|---|---|
| Alterations of the default setting: | 1. Only term loans | 2. Only leasing contracts | 3. Different control group | 4. Only newly issued loans | 5. Assigned neither to "bad" nor "good" bank | 6. No first switching loans | 9. Dep. variable: collateral | 10. Dep. variable: maturity | 11. Dep. variable: loan size |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| after x closed | -1.247*** | -0.715*** | -1.623*** | -0.554** | -0.987*** | -0.662** | 0.027 | 0.260 | 34.608 |
| | (0.004) | (0.000) | (0.000) | (0.035) | (0.000) | (0.029) | (0.749) | (0.631) | (0.865) |
| Firm-fixed effects | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Quarter-fixed effects | YES | YES | YES | YES | YES | YES | YES | YES | YES |
| Number of observations | 23,677 | 85,631 | 20,424 | 19,570 | 119,891 | 118,520 | 120,756 | 120,756 | 120,756 |
| Adjusted R-squared | 0.777 | 0.722 | 0.633 | 0.621 | 0.698 | 0.698 | 0.613 | 0.527 | 0.804 |

P-values in parentheses. Standard errors are clustered multiway within firms and quarters

## TABLE 1.7

## Difference-in-differences analysis: “Healthy bank”

Table 1.7 reports coefficient estimates from difference-in-differences panel regressions (specifications 1, 2, 3, 4 and 5). The data used in the analysis is at the quarter-firm level. The dependent variable “borrowing_costs” is a firm’s average interest rate weighted by loan outstanding amounts at each quarter. In quarters 2011q4 - 2012q4, we consider only leasing contracts, term loans and credit lines issued up to 2012 December 31. In quarters 2013q1 - 2018q1, we consider only leasing contracts, term loans and credit lines issued from 2013 January 30 (the day of “Healthy bank’s” decision to stop business). The explanatory variables are dummies: “after” - equal to 1 if an observation is from quarters 2013q1 - 2018q1, and 0 otherwise; “closed” – equal to 1 if a firm belongs to the treatment group, i.e. had any debt outstanding with the closed “Healthy bank” within one year prior to 2013 January 30, and 0 otherwise; “exclusive” – equal to 1 if a firm had debts only with one bank within the same prior year, and 0 otherwise; “small” equal to 1 if a firm’s maximum total debt to bank from 2011 q4 to 2013 q1 was smaller than median, and zero otherwise; “young” equal to 1 if a firm’s first appearance in the credit register was less than median as of 2013 q1, and zero otherwise; “short_term” equal to 1 if a firm’s average relationship length with its banks in 2013 q1 was shorter than 6 years, and 0 otherwise. Regressions include all double and triple interactions but, for brevity, only the interactions of interest are reported. Robust standard errors are clustered multiway at the firm and quarter levels. P-values are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, two-tailed, respectively.

| | Dependent variable: borrowing_costs | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| after x closed | 0.058 | 0.107 | 0.080 | 0.136 | 0.040 |
| | (0.680) | (0.511) | (0.623) | (0.436) | (0.786) |
| after x closed x exclusive | | -0.325 | -0.430 | -0.534 | -0.506 |
| | | (0.255) | (0.149) | (0.152) | (0.229) |
| after x closed x exclusive x small | | | 1.276 | | |
| | | | (0.178) | | |
| after x closed x exclusive x young | | | | 0.919 | |
| | | | | (0.132) | |
| after x closed x exclusive x short_term | | | | | 0.260 |
| | | | | | (0.653) |
| Firm-fixed effects | YES | YES | YES | YES | YES |
| Time-fixed effects | YES | YES | YES | YES | YES |
| Number of observations | 150,192 | 150,192 | 150,192 | 150,192 | 150,192 |
| Adjusted R-squared | 0.694 | 0.694 | 0.695 | 0.695 | 0.695 |

P-values in parentheses. Standard errors are clustered multiway within firms and quarters

## TABLE 1.8

## Definitions of switching

Table 1.8 provides switching-related definitions used in section 1.6.

| Term | Definition |
|---|---|
| Inside bank | A bank with which a firm had any amount of debt outstanding at any point of time within 1 prior year. In line with Ioannidou and Ongena (2010), we conservatively assume that relationship ties are broken if there is a gap without outstanding loans of 1 year or longer. |
| Outside bank | A bank with which a firm had no debt outstanding at any point of time within 1 prior year. |
| Switcher | A firm which is taking a loan from an outside bank. Consistently with Ioannidou and Ongena (2010) and Bonfim et al. (2019), we exclude firms that had no debts with any bank within the last 12 months. |
| Switching loan (or regular-switching loan) | A loan taken by a switcher from an outside bank. |
| Non-switching loan | A loan taken by a firm from its inside bank. |
| Forced-switching loan | The first switching loan taken by a firm after it lost its lending relationship due to a bank closure. |

## TABLE 1.9

## Summary statistics of newly issued loans

Table 1.9 provides average characteristics of all newly issued debt contracts between 2011 q4 and 2018 q1 at the moment of issuance.

| | Non-switching loans | Regular-switching loans | Forced-switching loans | Loans to firms that had no loans in 1 prior year | Total |
|---|---|---|---|---|---|
| **Number of all newly issued debt contracts** | **81,731** | **13,133** | **1,302** | **21,391** | **117,557** |
| Interest rate (%) | 3.34 | 4.49 | 4.95 | 4.92 | 3.77 |
| Probability of a floating rate (%) | 75% | 66% | 58% | 62% | 71% |
| Probability of using collateral (%) | 17% | 32% | 51% | 27% | 21% |
| Proportion of collateralized loan amount (%) | 27% | 54% | 69% | 39% | 33% |
| Time to maturity (quarters) | 12 | 12 | 11 | 12 | 12 |
| Loan amount (euros) | 223,510 | 347,619 | 259,267 | 208,172 | 234,980 |

TABLE 1.10

Matching variables

Table 1.10 provides the descriptions of matching variables.

| Category | Matching variables | A switching loan and a non-switching loan were matched: |
|---|---|---|
| Macro | Year & quarter | if both loans were issued in the same quarter (in total 26 quarters: 2011 q4 – 2018 q1) |
| Bank | Outside bank | if the bank that issued both loans was the switcher’s new (outside) bank (in total 12 banks) |
| Firm | Firm | if both loans were issued to the same firm (in total 25,436 firms) |
| Firm | Repayment troubles last year | if either both firms delayed at least one repayment or both firms did not delay any repayments in the previous 4 quarters |
| Firm | Economic activity (sector) | if both firms operated in the same sector (in total 20 sectors) |
| Firm | Total bank debt (+-30%) | if a non-switcher’s total amount of debt in the given quarter was similar to a switcher’s total amount of debt (using a (-30%, +30%) window around switcher’s debt) |
| Loan | Loan type | if both loans were of the same type (e.g. leasing, term loans, credit lines) |
| Loan | Proportion of loan collateralized (+-30%) | if both loans had a similar proportion of the face value collateralized (using a (-30%, +30%) window around the switching loan’s collateralized proportion) |
| Loan | Loan maturity (+-30%) | if both loans had a similar maturity (using a (-30%, +30%) window around the switching loan’s maturity) |
| Loan | Loan amount (+-30%) | if both loans had a similar amount (using a (-30%, +30%) window around the switching loan’s amount) |
| Loan | Floating loan rate | if both loans had either floating or fixed interest rates (a rate is defined as floating if it varies more than 50% of the time) |
| Firm | Loan rate on prior inside loans (+-30%) | if both firms had similar interest rates (maximum across all outstanding loans) in the previous period (using a (-30%, +30%) window around the switcher’s rate) |
| Firm | Prior relationship length (+-30%) | if both firms had similar lengths of lending relationships (average across all inside banks) in the previous period (using a (-30%, +30%) window around the switcher’s length) |
| Firm | Prior multiple bank relationships | if both firms either had or did not have multiple bank relationships in the previous period |
| Firm | Prior primary lender | if both firms either had or did not have a primary lender (a bank which provided more than 50% of a firm’s debt) in the previous period |
| Firm | Prior scope of the bank relationship | if both firms either had or did not have different loan types |

TABLE 1.11

Results of the loan matching analysis: spreads between regular-switching and non-switching loans

We estimate an average spread between an interest rate on a switching loan and an interest rate on a similar non-switching loan taken by a similar firm in the same quarter from the same bank. Definitions of switching and non-switching loans and inside and outside banks are provided in Table 1.8. We pair every switching loan with as many non-switching loans as possible, based on matching variables described in Table 1.10. Columns (1) and (2) consider leasing contracts, term loans and credit lines, while column (3) considers only leasing, column (4) – only term loans, and column (5) – only credit lines. The windows used for matching continuous variables are relaxed in columns (2) to (5) from +-30% to +-70%. Estimated interest rate spreads are regressed on a constant. The estimated coefficients on the constant are reported in the bottom row. Robust standard errors are clustered at the switching loan level and reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, two-tailed, respectively.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Loan types considered | All three | All three | Leasing | Term loans | Credit lines |
| Window used for matching | +-30% | +-70% | +-70% | +-70% | +-70% |
| Year & quarter | Yes | Yes | Yes | Yes | Yes |
| Outside bank | Yes | Yes | Yes | Yes | Yes |
| Repayment troubles last year | Yes | Yes | Yes | Yes | Yes |
| Economic activity (sector) | Yes | | | | |
| Total bank debt (+-30% or 70%) | Yes | Yes | Yes | Yes | Yes |
| Loan type | Yes | Yes | Yes | Yes | Yes |
| Proportion of loan collateralized (+-30% or 70%) | Yes | Yes | Yes | Yes | Yes |
| Loan maturity (+-30% or 70%) | Yes | Yes | Yes | Yes | Yes |
| Loan amount (+-30% or 70%) | Yes | Yes | Yes | Yes | Yes |
| Floating loan rate | Yes | | | | |
| Loan rate on prior inside loans (+-30% or 70%) | Yes | | | | |
| Prior relationship length (+-30% or 70%) | Yes | Yes | Yes | Yes | Yes |
| Prior multiple bank relationships | Yes | | | | |
| Prior primary lender | Yes | | | | |
| Prior scope of the bank relationship | Yes | | | | |
| Number of switching loans | 86 | 7,295 | 6,078 | 947 | 270 |
| Number of non-switching loans | 66 | 30,010 | 28,081 | 1,570 | 359 |
| Number of observations (matched pairs) | 112 | 181,260 | 178,424 | 2,379 | 457 |
| Spread in basis points | -26.3*** | -22.2*** | -22.4*** | -7.7** | -31.8 *** |
| | (7.5) | (1.2) | (1.2) | (3.9) | (4.7) |

## TABLE 1.12

### Results of the loan matching analysis: spreads between forced-switching and non-switching loans.

We estimate an average spread between an interest rate on a forced-switching loan and an interest rate on a similar non-switching loan taken by a similar firm in the same quarter from the same bank. Definitions of forced-switching and non-switching loans and inside and outside banks are provided in Table 1.8. We pair every forced-switching loan with as many non-switching loans as possible, based on matching variables described in Table 1.10. Columns (1) and (4) consider all firms, columns (2) and (5) consider only those pairs in which non-switching firms had relationships with banks longer than 6 years on average, and columns (3) and (6) – only those pairs in which non-switching firms had relationships with banks shorter than 6 years on average. Estimated interest rate spreads are regressed on a constant. The estimated coefficients on the constant are reported in the bottom row. Robust standard errors are clustered at the switching loan level and reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, two-tailed, respectively.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Forced-switchers from which banks? | "Distressed" | "Distressed" | "Distressed" | 3 closed ones | 3 closed ones | 3 closed ones |
| Loan types considered | All three | All three | All three | All three | All three | All three |
| Window used for matching | +-70% | +-70% | +-70% | +-70% | +-70% | +-70% |
| Subsample | All firms | Non-switchers with long relation-ships (>6 years) | Non-switchers with short relation-ships (<6 years) | All firms | Non-switchers with long relation-ships (>6 years) | Non-switchers with short relation-ships (<6 years) |
| Year & quarter | Yes | Yes | Yes | Yes | Yes | Yes |
| Outside bank | Yes | Yes | Yes | Yes | Yes | Yes |
| Repayment troubles last year | Yes | Yes | Yes | Yes | Yes | Yes |
| Total bank debt (+-70%) | Yes | Yes | Yes | Yes | Yes | Yes |
| Loan type | Yes | Yes | Yes | Yes | Yes | Yes |
| Proportion of loan collateralized (+-70%) | Yes | Yes | Yes | Yes | Yes | Yes |
| Loan maturity (+-70%) | Yes | Yes | Yes | Yes | Yes | Yes |
| Loan amount (+-70%) | Yes | Yes | Yes | Yes | Yes | Yes |
| Prior relationship length (+-70%) | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of switching loans | 40 | 19 | 21 | 68 | 22 | 46 |
| Number of non-switching loans | 199 | 118 | 84 | 328 | 137 | 194 |
| Number of observations (matched pairs) | 248 | 139 | 109 | 383 | 158 | 225 |
| Spread in basis points | 3.1 | 19.7** | -18.1* | -5.4 | 19.9** | -23.1*** |
| | (8.2) | (8.5) | (9.5) | (8.0) | (7.5) | (8.3) |

TABLE 1.13

Results of the loan matching analysis: the development of interest rates over time

We estimate an average spread between an interest rate on a non-switching loan and an interest rate on a similar switching loan taken by the same firm from the same bank, when they started the lending relationship. Definitions of switching and non-switching loans and inside and outside banks are provided in Table 1.8. We pair every switching loan with as many non-switching loans as possible, based on matching variables used in Table 1.11, column (2), except that instead of matching on firms' characteristics we match on the firm's identity. Estimated interest rate spreads are regressed on a set of dummy variables which indicate yearly time gaps between the switching and the non-switching loans. We control for time trends by subtracting 3-month Euribor rate from every interest rate and by including switching loans' time fixed effects. We account for changing firm qualities by matching on the dummy "Repayment troubles last year" in the top part of the table, and by considering only firms that never delayed any repayment in our sample period in the bottom part of the table. The estimated coefficients on the time-gap dummies are reported in the bottom rows of both parts of the table. Robust standard errors are clustered at the switching loan level and reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, two-tailed, respectively.

| Time gaps between a non-switching loan and a switching loan | Up to 1 year | From 1 to 2 years | From 2 to 3 years | From 3 to 4 years | From 4 to 5 years | More than 5 years |
|---|---|---|---|---|---|---|
| Loan types considered | All three | All three | All three | All three | All three | All three |
| Window used for matching | +-70% | +-70% | +-70% | +-70% | +-70% | +-70% |
| Outside bank | Yes | Yes | Yes | Yes | Yes | Yes |
| Repayment troubles last year | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm | Yes | Yes | Yes | Yes | Yes | Yes |
| Loan type | Yes | Yes | Yes | Yes | Yes | Yes |
| Proportion of loan collateralized (+-70%) | Yes | Yes | Yes | Yes | Yes | Yes |
| Loan maturity (+-70%) | Yes | Yes | Yes | Yes | Yes | Yes |
| Loan amount (+-70%) | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of switching loans | 2,495 | 1,563 | 1,011 | 797 | 340 | 136 |
| Number of non-switching loans | 2,877 | 2,116 | 1,922 | 1,209 | 523 | 323 |
| Number of observations (matched pairs) | 33,168 | 60,495 | 145,640 | 106,740 | 5,790 | 922 |
| Coefficient on the time-gap dummy | 49.3*** | 55.2*** | 46.1*** | 37.6*** | 31.3*** | -41.6*** |
| | (7.8) | (8.0) | (8.0) | (8.0) | (8.1) | (10.8) |

Considering only those firms which never had any repayment delays:

| | | | | | | |
|---|---|---|---|---|---|---|
| Number of switching loans | 2,184 | 1,467 | 941 | 776 | 327 | 116 |
| Number of non-switching loans | 2,540 | 1,915 | 1,840 | 1,168 | 512 | 211 |
| Number of observations (matched pairs) | 32,250 | 60,132 | 145,430 | 106,688 | 5,775 | 432 |
| Coefficient on the time-gap dummy | 53.4*** | 59.8*** | 50.2*** | 41.6*** | 35.7*** | -40.9* |
| | (7.1) | (7.3) | (7.3) | (7.3) | (7.4) | (22.9) |

## TABLE 1.14

## Panel regression: relationship between interest rate and firm-bank relationship length

Table 1.14 reports coefficient estimates from the panel regressions where the dependent variable "loan rate" is an interest rate charged on a new loan *l* issued by bank *b* to firm *f* in quarter *q*, and the explanatory variable is the logarithm of the length of the relationship between firm *f* and bank *b* in quarter *q* measured in quarters. In order to capture the non-linear dynamics, we also add the square of the explanatory variable. We use newly issued leasing contracts, term loans and credit lines between 2011 q4 and 2018 q1. Relationship lengths are measured from 1995 to 2018. Each column presents coefficients obtained using different level of controls. We control for loan characteristics, i.e. time to maturity, loan amount and collateralized proportion of loan amount, firm fixed effects, quarter (time) fixed effects, bank fixed effects, loan type fixed effects and interactions between these fixed effects. Robust standard errors are clustered at the firm level. P-values are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, two-tailed, respectively.

| | Dependent variable: loan rate | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Log(relationship length) | -0.147 | 0.144*** | 0.305** | 0.598*** | 0.717*** | 0.710*** |
| | (0.504) | (0.000) | (0.010) | -0.009 | (0.000) | (0.000) |
| Log(relationship length)^2 | -0.036 | -0.035*** | -0.078*** | -0.179* | -0.216*** | -0.213*** |
| | (0.492) | (0.000) | (0.005) | (0.098) | (0.007) | (0.008) |
| Constant | 4.078*** | | | | | |
| | (0.000) | | | | | |
| Controls for loan characteristics | YES | YES | YES | YES | YES | |
| Firm - FE | | YES | | | | |
| Quarter - FE | | YES | | | | |
| Bank - FE | | YES | | | | |
| Loan type - FE | | YES | | YES | | |
| Firm x Quarter - FE | | | YES | YES | YES | YES |
| Firm x Bank - FE | | | | YES | YES | YES |
| Bank x Quarter - FE | | | | YES | YES | YES |
| Loan type x Quarter - FE | | | | | YES | YES |
| Loan type x Firm - FE | | | | | YES | YES |
| Loan type x Bank - FE | | | | | YES | YES |
| Number of observations | 95,400 | 86,045 | 58,679 | 57,769 | 56,123 | 56,130 |
| Adjusted R-squared | 0.106 | 0.803 | 0.936 | 0.950 | 0.955 | 0.955 |

P-values in parentheses. Standard errors are clustered multiway within firms and quarters

# Chapter 2

## FOREIGN EXCHANGE RISK MANAGEMENT: SUPERVISORY DATA EVIDENCE

with Puriya Abbassi*, Falk Bräuning[ϕ], Luc Laeven[ƚ], and José-Luis Peydró[◊]

## 2.1 Introduction

The 2007-08 financial crisis revealed how globally interconnected financial institutions were and how insufficient their risk management was to handle the materialization of systemic risk (Freixas, Laeven and Peydró, 2015). Today, global banks continue to hold increasingly more cross-border assets denominated in foreign currencies (Bräuning and Ivashina, 2020; Buch and Goldberg, 2017; International Monetary Fund, 2019), which makes FX risk management crucial in preventing new systemic crises (Allen and Carletti, 2013). Capital requirements for FX risk make it costly for banks to hold uncovered FX positions, and, thus, the literature often assumes that banks are fully hedged (e.g. Fender and McGuire, 2010; Ivashina, Scharfstein and Stein, 2015; Bräuning and Ivashina, 2020). However, recent persistent deviations from the covered interest parity (CIP), at least partially caused by post-crisis regulations (Du, Tepper, and Verdelhan, 2018; Cenedese, Della Corte, and Wang, 2019), might have increased hedging costs for banks. This re-raises questions that are old but still largely open due to the opaqueness of the FX OTC market (Duffie, 2012) and the long-standing absence of granular derivatives data: to what extent do banks actually hedge their FX exposures? How does hedging depend on bank characteristics and macroeconomic conditions?[29]

We contribute to the literature by tackling these questions using banks' balance-sheet and FX-derivative exposures matched at the bank-currency-month level. Our unique

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[29] The most similar paper to ours by Rampini, Viswanathan, and Vuillemey (2020) uses quarterly aggregate notional amounts of interest rate and FX derivatives "held for purposes other than trading" at the bank holding company (BHC) level and shows that after experiencing a negative shock in net worth, BHCs reduced those amounts.

dataset includes 150 largest German banks from 2014-08 to 2016-12 and is a result of merging the following databases: (1) daily transaction-level OTC FX derivatives data provided by the Depository Trust & Clearing Corporation (DTCC), (2) monthly bank-level assets and liabilities of all German banks, denominated in EUR, USD, GBP, JPY, and CHF, and (3) quarterly bank-level supervisory data on banks' size, leverage, Tier 1 capital, risk-weighted assets, Z-score and NPLs, provided by Deutsche Bundesbank. In addition, we use detailed credit and security registers provided by Deutsche Bundesbank to study real effects, i.e. how FX hedging affects bank lending and investments, e.g. by freeing-up regulatory capital. We regress bank-currency-month level FX derivative exposures on the same level balance sheet exposures interacted with the following currency-month level macroeconomic shocks: FX volatility (volatility implied from 1-month FX options, provided by Bloomberg), CIP deviations, monetary policy rate differentials, GDP and inflation forecasts, and current account balances acquired from Consensus Economics Inc. We strengthen the identification with interactive fixed-effects.

We find large persistent mismatches between banks' FX assets and liabilities and that banks do not fully hedge these mismatches with derivatives: on average, unhedged FX exposures amount to 21% of banks' equity capital. However, when FX volatility spikes up, banks hedge more: i.e. banks with large FX liabilities increase their long derivatives exposure and banks with large FX assets increase their short derivatives exposure.[30] These results are particularly pronounced for better capitalized banks. Moreover, at the extensive margin, we find that banks with large FX liabilities are more likely to use derivatives when FX volatility spikes up. Our results are robust to (1) trimming and winsorizing exposure variables, (2) using leading and contemporaneous dependent variables, (3) using time-invariant ex-ante FX balance sheet exposures estimated on the first month of our sample period (i.e. 2014-08), and (4) including a lagged dependent variable as control. Furthermore, we get comparable and even more robust findings in a setting similar to difference-in-differences when we use the largely unexpected Brexit outcome as a shock inducing large GBP volatility.

Overall, our results suggest that banks are sensitive to FX risk and are willing to protect their net worth in times of high uncertainty. This is particularly true for better capitalized banks, which is in line with Rampini and Viswanathan (2010, 2013) and Rampini, Sufi and Viswanathan (2014), who suggest that better capitalized banks can devote more collateral for hedging. Yet, we find that banks persistently hold large uncovered FX exposures, in particular long, which might be due to several reasons. First, banks may find FX risk relatively attractive despite the FX risk capital requirements. According to the standardized approach introduced by Basel Committee on Banking Supervision (BCBS, 1996, pp. 23-26; BCBS, 2019, pp. 112-114), "the capital requirement would be 8% of the higher of either the net long currency positions or the net short currency positions", which means that up to a half of uncovered FX exposures may receive no capital charge. The charge only applies if the overall net FX position exceeds 2% of the bank's own funds and there are other exemptions from this charge, e.g. for closely correlated currencies and structural FX positions held for the

[30] Results on the other macroeconomic shocks are not yet included in this draft.

protection of the capital ratio.[31] Furthermore, banks can use their discretion to hedge "net future income/expenses not yet accrued". These rules and exemptions may provide leeway for banks to take FX risk with limited capital charges. Second, post-crisis regulation, namely the minimum leverage ratio (Cenedese, Della Corte, and Wang, 2019), calculated as Tier 1 capital divided by total assets, can make FX hedging unattractive because derivative exposures substantially inflate banks' total assets in the ratio calculation (BCBS, 2014). Third, limits to arbitrage and FX demand-supply imbalances (e.g. Borio, Iqbal, McCauley, McGuire, and Sushko, 2018), reflected by CIP deviations, may also make hedging costly.

Our paper contributes to a few strands of the literature. First, we add to the rapidly growing literature on CIP deviations and FX hedging costs. According to the covered interest parity (CIP), the following two investment strategies should yield the same risk and return: (1) investing in a domestic risk-free asset yielding $i_d$ and (2) exchanging the domestic currency to a foreign one at a spot rate $S$, investing in a foreign risk-free asset yielding $i_f$, and, at maturity, exchanging back to the domestic currency at rate $F$ using a forward contract agreed upon today. In a frictionless market, the difference in yields of the two strategies, i.e. the cross-currency basis, should be zero, because every occurring gap would attract arbitragers' (e.g. banks, hedge funds etc.) respective demand for and supply of the two strategies, which would instantly close the gap again. However, since the 2007-08 financial crisis, due to a combination of (1) tightening limits to arbitrage and (2) arising imbalances between hedging demand and supply (e.g. Borio, McCauley, McGuire, and Sushko, 2016), CIP persistently fails, which suggests disadvantageous FX forward rates and, thus, costly hedging of FX risk for banks on the "wrong" side of the market. CIP deviations were found to be affected by counterparty credit risk (e.g. Ivashina et al., 2015, Borio et al., 2018), market liquidity conditions (e.g. Krohn and Sushko, 2020), interest margin differentials between countries (Iida, Kimura, and Sudo, 2018), post-crisis regulations (e.g. Du et al., 2018; Cenedese et al., 2019), etc.[32] Abbassi and Bräuning (2018) found that hedging costs in the OTC FX market also depend on hedgers' leverage and bargaining power. While most of this literature attempts to explain sources of CIP deviations and, thus, hedging costs, we contribute by testing how CIP deviations affect the actual hedging behavior.

Second, by testing how other shocks, i.e. FX volatility, monetary policy rates' differentials, GDP forecasts etc., affect banks' foreign currency risk management, and subsequent lending in different currencies (e.g. through freed-up capital), we contribute to the literature studying cross-border transmission of shocks. For instance, Bräuning and Ivashina (2020), Morais, Peydró, Roldán-Peña and Ruiz-Ortega (2019), and Cetorelli and Goldberg (2012) show how monetary policy in one country affects lending of global banks in other currencies. Peek and Rosengren (1997, 2000) demonstrate how a Japanese bank reduced lending in the U.S. due to a sharp decline of stock prices in

[31] For example, if a bank holds FX assets and hedges them with FX derivatives, an appreciation in FX would increase the size of the balance sheet without increasing the equity, which would reduce the capital ratio. In order to protect the capital ratio, banks are allowed to hold some structural FX positions unhedged.

[32] Other research on CIP deviations include (among others) Avdjiev, Du, Koch, and Shin (2019); Liao (2020); Rime, Schrimpf, and Syrstad (2019); Hong, Oeking, Kang, and Rhee (2019); Gabaix and Maggiori (2015); Gromb and Vayanos (2018).

Japan. Ongena, Schindele and Vonnak (2019) find a differential effect of domestic and foreign monetary policies on domestic lending in different currencies. Mueller, Tahbaz-Salehi and Vedolin (2017) find evidence that monetary policy uncertainty in the U.S. affects foreign exchange markets. Bruno and Shin (2015) relate foreign exchange rates and financial stability by showing that banking sectors increase leverage when local currencies appreciate (i.e. foreign currencies depreciate).

Third, our paper enriches the knowledge of why and how institutions, particularly banks, manage risk with financial derivatives. According to Allayannis, Ihrig and Weston (2001), financial hedging is a crucial component for the FX risk management to benefit shareholders, but hedging incentives depend on many factors. Smith and Stulz (1985) propose that managerial compensation schemes using stock options and stocks (that effectively have a payoff of a call option) reduce incentives to hedge because options are more valuable when the underlying in risky. Yet, managers with more concentrated ownership may hedge more as they hold a less diversified portfolio. More levered firms may also hedge more because of being disciplined by debtholders and because smoother cash flows lower the probability of bankruptcy and financial distress, which creates value for shareholders (Smith and Stulz, 1985). Similarly, Froot, Scharfstein and Stein (1993) propose that firms which have more investment opportunities and/or are more financially constrained, e.g. smaller, highly leveraged, undercapitalized etc., would choose to hedge more in order to avoid raising expensive external capital necessary for investments and maintaining dividends. In contrast, Rampini and Viswanathan (2010, 2013) and Rampini, Sufi and Viswanathan (2014) argue that since hedging requires collateral, firms with lower net worth, e.g. undercapitalized, would hedge less in order to save their collateral for financing needs. Rampini, Viswanathan, and Vuillemey (2020) find empirical evidence among financial institutions supporting this theory. Our findings that better capitalized banks react to spiking FX volatility by hedging more is also in line with this theory. Other incentives for hedging include the reduction of expected tax liabilities (Nance, Smith, and Smithson, 1993), the expansion of debt capacity (Leland, 1998; Graham and Rogers, 2002) and the reallocation of risks (Froot and Stein, 1998; Schrand and Unal, 1998). The risk reallocation hypothesis is particularly relevant for banks, since by hedging their interest rate risk (Brewer, Minton, and Moser, 2000) and exchange rate risk (Deng, Elyasiani and Mao, 2017), they can afford to increase their exposures to credit risk, i.e. their main expertise. However, despite all potential benefits of hedging, Martin and Mauer (2003) show that banks remain significantly exposed to foreign exchange risk, which is in line with our findings. Abbassi and Bräuning (2019) demonstrate that a realization of FX risk may lead to a reduction of lending.

The rest of the paper is structured as follows. Section 2.2 describes the data, section 2.3 outlines our regression specifications, section 2.4 presents the results and section 2.5 concludes.

## 2.2 Data

Our unique dataset comprises month-end balance-sheet and FX-derivative exposures split by five currencies, i.e. EUR, USD, GBP, JPY, and CHF, of 150 largest German banks between 2014-08 and 2016-12.[33] It also includes detailed data on banks' loans, security holdings and some supervisory data, e.g. T1 capital and RWA. The dataset is obtained by merging five databases: one provided by DTCC and the others – by Deutsche Bundesbank.

The daily transaction-level data on OTC FX derivatives is provided by the Depository Trust & Clearing Corporation (DTCC). European Market Infrastructure Regulation (EMIR) states that from February 12$^{th}$, 2014, every counterparty in the European Union that enters a derivative contract, has to report it to one of the six authorized trade repositories (Osiewicz, Fache Rousová and Kulmala, 2015). In 2015 and 2016, i.e. during most of our sample period, DTCC has been the largest one in terms of the total notional amount of FX derivatives, and included between 60% and 80% of the total notional value (and between 30% and 50% of the total number of trades) distributed across the six repositories (Ascolese, Molino, Skrzypczyński, Cerniauskas, and Pérez-Duarte, 2017).[34] We observe every trade reported in the repository where one of the counterparties was registered in Germany. By focusing on only one repository, we do not necessarily observe derivatives of all German banks, but since every counterparty is required to report only to one repository, we observe all FX derivatives for those banks that chose to report to DTCC.

For visualization purposes, Figure 2.1 presents an excerpt from the database with fake values of all variables. For every trade we observe trade ID, reporting counterparty ID, ID of the other counterparty, whether the reporting counterparty was acting on its own behalf or as a broker, whether it acted as a buyer or a seller, mark-to-market value of the derivative updated daily, derivative type (i.e. currency forward, currency option, etc.), notional amounts and currencies of each leg, maturity date, trade date and reporting date. In total, we observe 250 distinct German counterparties, out of which 80 were banks (accounting for 66% of all trades). Most of the contracts were forwards (85%), which also include separate legs of swap contracts, and options (12%). In our analysis we use only forward contracts and drop non-banks. The total number of remaining contracts by currency are USD/EUR (1,087,946), GBP/EUR (108,709), CHF/EUR (79,749), and JPY/EUR (58,948). Most contracts, i.e. 50%, have maturity shorter than 3 months and 20% of contracts have maturity above 1 year.

Across all the six repositories, there are well known data issues, that were grouped into three categories by Serena-Garralda and Tissor (2018): "(1) incomplete coverage in terms of market segments and/or instruments, (2) absence of counterparty information, and (3) missing details on critical elements of the derivatives transactions". The first category problem appears to be limited (Serena-Garralda and Tissor, 2018), as for the second category, the only counterparty information we use is the ID of reporting counterparty, and, as for the third category, we work around the missing details on derivative transactions in the following way. One of the biggest issues in our data is the inconsistent reporting of the long and short legs. Luckily banks updated daily mark-to-market values of their positions rather rigorously. In order to confirm whether a reporting counterparty was long or short in a foreign currency, for every contract, we calculate the correlation between daily mark-to-market values and daily exchange rate

[33] The sample period will be updated up to 2020 in the upcoming versions.
[34] We are in the process of including other trade repositories.

between the two reported currencies. For instance, if the mark-to-market value increases when the foreign currency appreciates, a bank is assumed to be long in the foreign currency. Since we only need exposures on the last day of each month, and banks sometimes skip some days of reporting, we select all contracts occurring in the database within each month and keep contracts with maturity dates beyond the end of that month. Finally, we aggregate notional amounts at the bank-currency-month level to estimate the net notional amounts, which can be either long or short, in each currency.

The derivative data is then merged with the other four databases provided by Deutsche Bundesbank. First, we use total banks' assets and liabilities outstanding at the end of each month, aggregated by currency in which they are denominated ("Auslandsstatus"). Second, we use the supervisory data on banks' size, leverage, Tier 1 capital, risk-weighted assets, Z-score and non-performing loans, all reported at quarter-ends. We use linear interpolation between quarters-ends to obtain the missing month-end observations. Third, in order to study whether and how banks increase lending and investment in securities, as a result of reducing their FX risk via hedging, we use the credit and security registers. The credit register provides loan outstanding amounts reported quarterly at the borrower level for all borrowers with the total credit exposure above EUR 1 million. The register covers more than 70% of the total credit volume in Germany. Although we do not observe loan-specific variables, e.g. interest rates, maturities etc., we do observe some borrower's characteristics, e.g. industry code and country of residence. Finally, we have access to the security register, where banks report all their security holdings at the ISIN level on the monthly basis. We observe the number of securities held, their last available price, yield-to-maturity, maturity, and rating. From every database we select the same 150 largest banks in terms of total assets as of 2014-08, i.e. the beginning of our sample period. These banks include the 80 banks that use FX derivatives and report them to DTCC.

## 2.3 Methodology

In order to tackle our research questions, i.e. to what extent banks hedge their FX exposures and how hedging depends on bank characteristics and macroeconomic conditions, we estimate the following regression models. At this stage, we only use one macroeconomic shock - FX volatility (volatility implied from 1-month FX options, extracted from Bloomberg). First, we estimate the following probit model to test whether banks with large exposures in FX assets and/or FX liabilities are more likely to use derivatives in times of high FX volatility:

$$Use\_of\_deriv\_dummy_{b,t,c} = \beta_1 * FX\ Assets_{b,t,c} + \beta_2 * FX\ Liabilities_{b,t,c} + \beta_3 * FX\ Assets_{b,t,c} * FX\ Volatililty_{t,c} + \beta_4 * FX\ Liabilities_{b,t,c} * FX\ Volatililty_{t,c} + Bank\text{-}x\text{-}Time\text{-}FE + Bank\text{-}x\text{-}Currency\text{-}FE + Currency\text{-}x\text{-}Time\text{-}FE + \varepsilon_{b,t,c} \quad (1)$$

Where

- $Use\_of\_deriv\_dummy_{b,t,c}$ is a dummy variable equal to 1 if a bank *b* had non-zero net FX derivative exposure in currency *c* at the end of month *t*.

- $FX\ Assets_{b,t,c}$ is the total value (in EUR) of bank's $b$ assets denominated in currency *c*, reported at the end of month *t*.
- $FX\ Liabilities_{b,t,c}$ is the total value (in EUR) of bank's *b* liabilities denominated in currency *c*, reported at the end of month *t*.
- $FX\ Volatililty_{t,c}$ is the VIX index calculated using implied volatility of 1-month FX options for currency *c*, reported by Bloomberg at the end of month *t*.
- $FE$ stands for "fixed effects".

The interactive fixed effects considerably strengthen our identification because we control for all observable and unobservable bank-specific time-varying, currency-specific time-varying and bank-currency-specific time-invariant characteristics.

Next, we replace the dependent variable with gross long and gross short exposures in FX derivatives, and use OLS to test whether and in which direction the interaction between FX balance sheet exposures and FX volatility affects derivative exposures:

$$\boldsymbol{Short\_deriv\_exposure_{b,t,c}} = \beta_1 * FX\ Assets_{b,t,c} + \beta_2 * FX\ Liabilities_{b,t,c} + \beta_3 * FX\ Assets_{b,t,c} * FX\ Volatililty_{t,c} + \beta_4 * FX\ Liabilities_{b,t,c} * FX\ Volatililty_{t,c} + Bank\text{-}x\text{-}Time\text{-}FE + Bank\text{-}x\text{-}Currency\text{-}FE + Currency\text{-}x\text{-}Time\text{-}FE + \varepsilon_{b,t,c} \quad (2)$$

$$\boldsymbol{Long\_deriv\_exposure_{b,t,c}} = \beta_1 * FX\ Assets_{b,t,c} + \beta_2 * FX\ Liabilities_{b,t,c} + \beta_3 * FX\ Assets_{b,t,c} * FX\ Volatililty_{t,c} + \beta_4 * FX\ Liabilities_{b,t,c} * FX\ Volatililty_{t,c} + Bank\text{-}x\text{-}Time\text{-}FE + Bank\text{-}x\text{-}Currency\text{-}FE + Currency\text{-}x\text{-}Time\text{-}FE + \varepsilon_{b,t,c} \quad (3)$$

Where

- $Short\_deriv\_exposure_{b,t,c}$ is the total value of forward contracts' notional amounts of currency *c* that bank *b* has committed to sell in the future, reported at the end of month *t*.
- $Long\_deriv\_exposure_{b,t,c}$ is the total value of forward contracts' notional amounts of currency *c* that bank *b* has committed to buy in the future, reported at the end of month *t*.

We then augment specifications (2) and (3) with the triple interactions using one bank characteristic at a time:

$$\boldsymbol{Short\_deriv\_exposure_{b,t,c}} = \beta_1 * FX\ Assets_{b,t,c} + \beta_2 * FX\ Liabilities_{b,t,c} + \boldsymbol{\beta_3} * \boldsymbol{FX\ Assets_{b,t,c}} * \boldsymbol{FX\ Volatililty_{t,c}} * \boldsymbol{BankChar_{b,t}} + \boldsymbol{\beta_4} * \boldsymbol{FX\ Liabilities_{b,t,c}} * \boldsymbol{FX\ Volatililty_{t,c}} * \boldsymbol{BankChar_{b,t}} + [all\ possible\ double\ interactions] + Bank\text{-}x\text{-}Time\text{-}FE + Bank\text{-}x\text{-}Currency\text{-}FE + Currency\text{-}x\text{-}Time\text{-}FE + \varepsilon_{b,t,c} \quad (4)$$

$$\boldsymbol{Long\_deriv\_exposure_{b,t,c}} = \beta_1 * FX\ Assets_{b,t,c} + \beta_2 * FX\ Liabilities_{b,t,c} + \boldsymbol{\beta_3} * \boldsymbol{FX\ Assets_{b,t,c}} * \boldsymbol{FX\ Volatililty_{t,c}} * \boldsymbol{BankChar_{b,t}} + \boldsymbol{\beta_4} * \boldsymbol{FX\ Liabilities_{b,t,c}} * \boldsymbol{FX\ Volatililty_{t,c}} * \boldsymbol{BankChar_{b,t}} + [all\ possible\ double\ interactions] + Bank\text{-}x\text{-}Time\text{-}FE + Bank\text{-}x\text{-}Currency\text{-}FE + Currency\text{-}x\text{-}Time\text{-}FE + \varepsilon_{b,t,c} \quad (5)$$

Where

- $BankChar_{b,t}$ is one of the following three characteristics of bank $b$ at the end of month $t$: (1) size, i.e. total assets, (2) E/A ratio, i.e. total equity divided by total assets, and (3) T1 ratio, i.e. T1 capital divided by risk-weighted assets.

Finally, we reduce the regression specifications (2) and (3) and combine them into one by using net FX derivative exposure as the dependent variable and net FX assets as the explanatory variable:

$$Net\ FX\ derivatives_{b,t,c} = \beta_1 * Net\ FX\ Assets_{b,t,c} + \beta_2 * Net\ FX\ Assets_{b,t,c} * FX\ Volatililty_{t,c} + Bank\text{-}x\text{-}Time\text{-}FE + Bank\text{-}x\text{-}Currency\text{-}FE + Currency\text{-}x\text{-}Time\text{-}FE + \varepsilon_{b,t,c} \quad (6)$$

Where

- $Net\ FX\ derivatives_{b,t,c}$ is the difference between $Long\_deriv\_exposure_{b,t,c}$ and $Short\_deriv\_exposure_{b,t,c}$.
- $Net\ FX\ Assets_{b,t,c}$ is the difference between $FX\ Assets_{b,t,c}$ and $FX\ Liabilities_{b,t,c}$.

In order to strengthen the evidence on the causal relationship between FX volatility and banks' hedging, we implement an analysis similar to difference-in-differences using the largely unexpected Brexit outcome on June 23, 2016, as a shock inducing large volatility of the GBP/EUR exchange rate. We restrict the sample period to 6 months: 3 month-ends before the shock (i.e. March, April and May) and 3 month-ends after the shock (June, July and August), and estimate the following two regression specifications using assets and liabilities denominated in GBP and forward contracts in which one of the notional amounts is denominated in GBP.

$$\boldsymbol{Short\_deriv\_exposure_{b,t}} = \beta_1 * After_t * Big\ FX\ Assets_b + \beta_2 * After_t * Big\ FX\ Liabilities_b + Bank\text{-}FE + Time\text{-}FE + \varepsilon_{b,t,c} \quad (7)$$

$$\boldsymbol{Long\_deriv\_exposure_{b,t}} = \beta_1 * After_t * Big\ FX\ Assets_b + \beta_2 * After_t * Big\ FX\ Liabilities_b + Bank\text{-}FE + Time\text{-}FE + \varepsilon_{b,t,c} \quad (8)$$

Where

- $After_t$ is a dummy variable equal to 1 if period $t$ is after the Brexit vote, and zero otherwise.
- $Big\ FX\ Assets_b$ is a dummy equal to 1 if bank's $b$ value of GBP assets measured at the end of March 2016 was above median among banks used in this analysis, and zero otherwise.
- $Big\ FX\ Liabilities_b$ is a dummy equal to 1 if bank's $b$ value of GBP liabilities measured at the end of March 2016 was above median among banks used in this analysis, and zero otherwise.

In specifications (9) and (10) we also include the triple interactions with one of the following three bank characteristics measured at the end of March 2016: size, E/A ratio and T1 ratio:

$$\boldsymbol{Short\_deriv\_exposure_{b,t}} = \beta_1 * After_t * Big\ FX\ Assets_b * BankChar_b + \beta_2 * After_t * Big\ FX\ Liabilities_b * BankChar_b + [all\ double\ interactions] + Bank\text{-}FE + Time\text{-}FE + \varepsilon_{b,t,c} \quad (9)$$

$$\boldsymbol{Long\_deriv\_exposure_{b,t}} = \beta_1 * After_t * Big\ FX\ Assets_b * BankChar_b + \beta_2 * After_t * Big\ FX\ Liabilities_b * BankChar_b + [all\ double\ interactions] + Bank\text{-}FE + Time\text{-}FE + \varepsilon_{b,t,c} \quad (10)$$

## 2.4 Results

### 2.4.1 Stylized Facts

First, we use charts to analyze banks' FX exposures at aggregate levels and then we present our regression results. Figure 2.2 shows assets and liabilities denominated in the four foreign currencies, reported in euros, and aggregated across the 150 largest German banks used in our sample. Total FX assets persistently amount to roughly twice as much as total FX liabilities, which raises a question – do banks hedge the FX funding gap? This would require aggregate short exposures in FX forwards to be significantly larger than aggregate long exposures.

Figure 2.3 presents aggregate FX derivative exposures for each of the four currencies split into four time-to-maturity buckets. Blue (red) columns above (below) the x-axis represent long (short) exposures, i.e. notional amounts of foreign currency that banks committed to buy (sell). For USD and JPY, aggregated long and short exposures are roughly the same across all maturities, and thus virtually cancel each other out. For GBP and CHF, long and short exposures in most of the maturity buckets are also similar, yet short exposures are somewhat larger, particularly in contracts expiring within 30 days. This suggests that in aggregate, German banks do not trade FX derivatives to fully hedge net FX assets and possibly trade derivatives, particularly with notional amounts denominated in USD and JPY, as market makers.

Figure 2.4 plots aggregate net FX balance sheet exposures (i.e. FX assets minus FX liabilities) in blue, and aggregate net FX derivatives exposures (i.e. FX notional amounts committed to buy minus the amounts committed to sell) in red. In line with Figure 2.3, for GBP and CHF, and more recently also for JPY, derivatives are used to at least partially offset FX balance sheet exposures, yet, large uncovered FX exposures remain. As for USD, net FX derivative exposures are persistently positive, which means that on top of large net USD-denominated assets, banks add even more USD exposure using derivatives. When analyzing Figure 2.4, it is important to remember that some of the 150 largest German banks might be reporting their derivative trades to other repositories than DTCC, yet we have no reason to suspect that their use of derivatives would be systematically different.

Figure 2.5 presents scatter plots with net FX balance sheet and net FX derivative exposures measured at the beginning of our sample (2014-08) at the individual bank level. The number of banks (blue dots) appears relatively low because the smallest banks cluster at the intersection of the two axes while larger banks stand out. In line with previous graphs, Figure 2.5 indicates that most large banks at least partially (but not fully) offset their GBP and CHF denominated net assets with short FX forwards. However, banks tend to have positive USD exposures in both assets and derivatives.

Overall, we find that German banks do not hedge their FX exposures, especially USD, fully. This suggests that despite the regulatory capital requirements, FX risk is attractive, and/or hedging it is costly. But do banks hedge more at least when the uncertainty in the FX market increases? Figure 2.6 shows that spikes in FX volatility (i.e. volatility implied from 1-month FX options) of each currency tend to be accompanied by spikes in the number of OTC FX forwards newly initiated by banks. In order to better understand whether such increases in banks' activity are driven by their own demand for hedging FX risk and not merely by market making, we turn to the regression results.

### 2.4.2 Regression Results

We report only our most conservative estimates of regression coefficients obtained by absorbing as much as possible variation with fixed effects and by clustering standard errors multiway, however, our results remain similar and, in most cases, even more statistically significant with fewer fixed effects and different error clusters. Table 2.1 reports regression coefficients of specifications (1), (2), (3) and (6) in columns (1), (2), (3), and (4), respectively. Column (1) shows the results at the extensive margin. The coefficient on the interaction term between FX Liabilities and FX Volatility is positive and statistically significant at 5% level, which suggests that banks with large liabilities denominated in a certain foreign currency are more likely to use FX derivatives in that currency, when implied volatility of that currency spikes up. The coefficient on the interaction term between FX Assets and FX Volatility is very close to zero and not statistically significant. One explanation for the asymmetric effect of assets and liabilities could be that banks have much more FX assets than FX liabilities (see Figure 2.2). Due to the relatively high magnitude of assets, banks may be already using derivatives independently of FX volatility, while due to the relatively low magnitude of liabilities, banks may only worry about hedging them when volatility spikes up. This is supported by the positive and statistically significant (at 10% level) coefficient on FX Assets.

As for the intensive margin, in column (2) ((3)), a positive and statistically significant (at 5% (10%) level) coefficient on the interaction term between FX Assets (FX Liabilities) and FX Volatility suggests that in times of high FX Volatility, banks with large FX Assets (FX Liabilities) tend to increase their short (long) derivative exposure, i.e., hedge more. Similarly, in column (4), a negative and statistically significant coefficient on the interaction term between Net FX Assets and FX Volatility suggests that the longer banks are in FX Assets in times of high volatility, the shorter they go in FX derivatives, which also suggests a reduction in the overall exposure to FX risk.

In Table 2.2, columns (3) and (6), the positive and statistically significant coefficients on the triple interactions suggest that our results are stronger for better capitalized banks as measured by T1 ratio. This is in line with Rampini and Viswanathan (2010, 2013) and Rampini, Sufi and Viswanathan (2014), who suggest that better capitalized banks hedge more because they can devote more collateral for hedging. Our results are somewhat mixed for other bank characteristics. For example, in times of high FX volatility, smaller banks hedge their FX assets more than larger banks (column 1), but larger banks hedge their FX liabilities more than smaller banks (column 4). The E/A ratio does not seem to affect banks' hedging of FX assets (column 2), but banks with lower E/A ratio appear to hedge their FX liabilities more in times of high FX volatility (column 5).

We get similar and, in fact, even more consistent results when analyzing how banks' FX derivative exposures were affected by the volatility spike induced by the Brexit vote (the spike can be seen in Figure 2.6, top right graph). Table 2.3, column (1) shows that after the Brexit vote, banks with larger GBP-denominated assets tended to increase their short derivative exposure, while banks with larger GBP-denominated liabilities tended to decrease their short derivative exposure, which suggests an overall reduction in FX risk. Similarly, column (5) shows that after the Brexit vote, banks with larger GBP liabilities tended to increase their long derivatives exposure, while banks with larger GBP assets tended to decrease their long derivative exposure, although the regression coefficient on the latter effect is not statistically significant. Columns (2)-(4) and (6)-(8) consistently show that our results are stronger for banks that are smaller and better capitalized in terms of both E/A ratio and T1 ratio.

### 2.4.3 Robustness Tests and Limitations

Our main results remain robust after (1) trimming and winsorizing exposure variables, (2) using leading (one period ahead) dependent variables, (3) using time-invariant FX balance sheet exposures estimated on the first month of our sample period (i.e. 2014-08), and (4) including dependent variables lagged by one period as controls. Nevertheless, there are some limitations that need to be addressed.

First, FX balance sheet exposures and FX derivative exposures in our dataset do not match each other perfectly. For example, FX derivative exposures represent future cash flows (notional amounts) while FX balance sheet exposures represent present values of assets and liabilities. Therefore, the direct comparison between the two in Figures 2.4 and 2.5 might seem unfair. However, net FX assets expressed as future cashflows would likely be even larger, and, thus, the uncovered FX exposures would be larger too. Another example is maturity mismatches between FX derivative exposures and FX balance sheet exposures, as for latter we do not observe maturities at all. However, matching contract maturities when hedging FX risk has limited impact on the quality of hedge, assuming that one can immediately re-enter into a new derivative contract when an old one matures. This assumption seems innocuous since even regulators ignore maturities when summing up FX exposures (BCBS, 2019, pp. 112-114).

Second, changes in implied FX volatility are normally accompanied or triggered by FX rate movements, which immediately create gains and losses for uncovered FX positions. It is, therefore, possible that banks hedge more not as a response to an increased volatility, as suggested by our findings, but in response to sudden gains and losses. The same applies to our setting using the Brexit vote as a shock inducing FX volatility. In order to disentangle the effect of volatility from the effect of gain and losses, in future research we need to control for gains and losses instantly created by FX rate movements. In any case, our results demonstrate that banks get concerned about the FX risk management too late, i.e. only after major movements in FX rate occur.

## 2.5 Conclusion

The OTC FX derivatives market has long been opaque, and, thus, relatively little is known about banks' FX risk management. While, related literature often assumes that regulatory capital requirements force banks to fully hedge their FX risks (e.g. Fender and McGuire, 2010; Ivashina, Scharfstein and Stein, 2015; Bräuning and Ivashina, 2020), post-crisis persistent CIP violations suggest that FX hedging might be costly. It is, therefore, not clear to what extent and under which circumstances banks actually hedge their FX risks. Recently, since 2014, it became possible to study these questions with relatively high precision due to the FX derivatives data made available by the European Market Infrastructure Regulation (EMIR). In this paper, we merge the newly available data on FX derivatives with FX balance sheet exposures provided by Deutsche Bundesbank, and for the first time use it to study how banks manage their FX risk.

We find that in aggregate, German banks hold at least twice as much FX-denominated assets as FX-denominated liabilities, and the banks' usage of FX derivatives leaves the mismatch largely uncovered, especially for the USD. This suggests that banks trade derivatives for other reasons than hedging, e.g. market making. This also suggests either (1) that FX risk hedging is excessively costly, e.g. due to the FX demand-supply mismatches and limits to arbitrage, as reflected by CIP deviations, or due to the new post-crisis regulations, in particular, the minimum leverage ratio requirement, or (2) that FX risk is attractive and existing FX risk capital requirements are insufficient to discourage banks from being exposed to it, or (3) both. This poses a question for regulators whether FX risk capital requirements and the new post-crisis regulations should not be revised.

We do find, however, that banks are somewhat sensitive to FX risk, as they use derivatives to hedge it more when the FX rate uncertainty increases. In line with Rampini and Viswanathan (2010, 2013) and Rampini, Sufi and Viswanathan (2014), these findings are stronger for better capitalized banks, and in line with Froot, Scharfstein and Stein (1993), for smaller banks. However, at this point we do not rule out the possibility that banks increase they hedging as a result of gains and losses incurred from sudden FX movements, rather than due to coinciding hikes in implied volatility.

All in all, our results suggest that banks tend to leave significant FX exposures, especially long ones, unhedged and that banks get concerned about the FX risk too late, i.e. after FX rate shocks. This poses a threat to the international financial stability and calls for revisions of both FX capital requirements, which might currently be insufficient, and other post-crisis regulations, in particular, the leverage ratio, which might currently make hedging costly.

## Figures of Chapter 2

FIGURE 2.1

An example of the OTC FX derivatives data from DTCC

Figurer 2.1 shows an excerpt from the OTC FX derivatives database provided by the Depository Trust & Clearing Corporation (DTCC). The values of every variable are fake and are presented only for visualization purposes.

| counterparty id | id of the other counterparty | trading capacity | counterparty side | MtM value | currency of MtM value | product id 1 | product id 2 | notional currency 1 | notional currency 2 | trade id | notional 1 | notional 2 | MtM value date | maturity date | initiation date | report date |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| BANK11111 | BANK22222 | Personal | Sell | - 401,859 | USD | Currency | Forward | GBP | USD | 00001XYZ | 50,000,000 | 78,500,000 | 18-Dec-15 | 15-Aug-16 | 03-Apr-15 | 03-Apr-15 |
| BANK33333 | BANK44444 | Agent | Buy | 50,857 | EUR | Currency | Option | CHF | EUR | 00002XYZ | 50,000,000 | 78,500,000 | 18-Dec-15 | 15-Aug-16 | 03-Apr-15 | 03-Apr-15 |

FIGURE 2.2

Aggregate FX balance sheet exposure

Figure 2.2 shows aggregate assets (in blue) and liabilities (in red) denominated in one of the four foreign currencies, i.e. USD, GBP, JPY and CHF, reported in billion euros monthly by 150 largest German banks (bank size measured as of 2014-08).

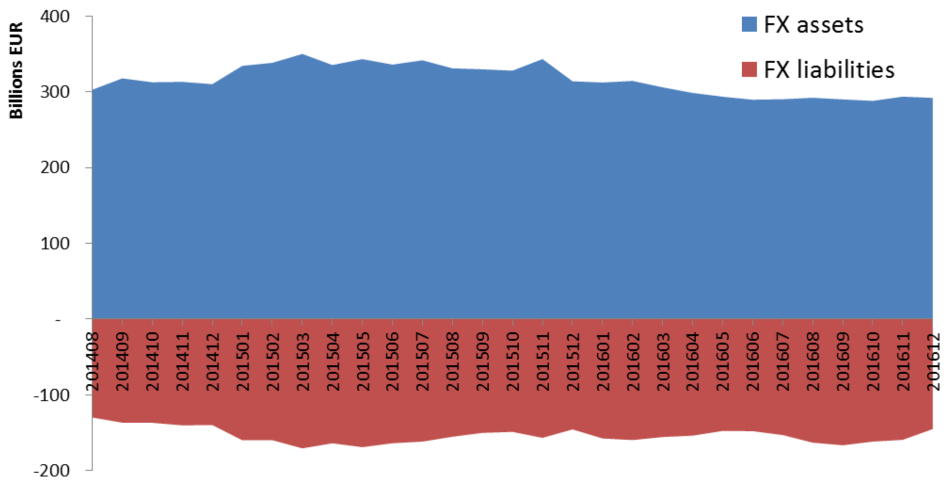


FIGURE 2.3

Aggregate FX derivatives exposure by maturity

Figure 2.3 comprises four graphs – one for each foreign currency: USD, GBP, JPY and CHF – that show aggregate notional amounts of foreign currency that banks were committed to buy (long exposure – in blue) and sell (short exposure – in red) using OTC FX forward contracts, reported in billion euros at 2014-08 by 150 largest German banks (bank size measured as of 2014-08). All exposures were split into 4 columns based on contracts' remaining maturity: (1) 0-30 days, (2) 30-60 days, (3) 60-120 days, and (4) more than 120 days.

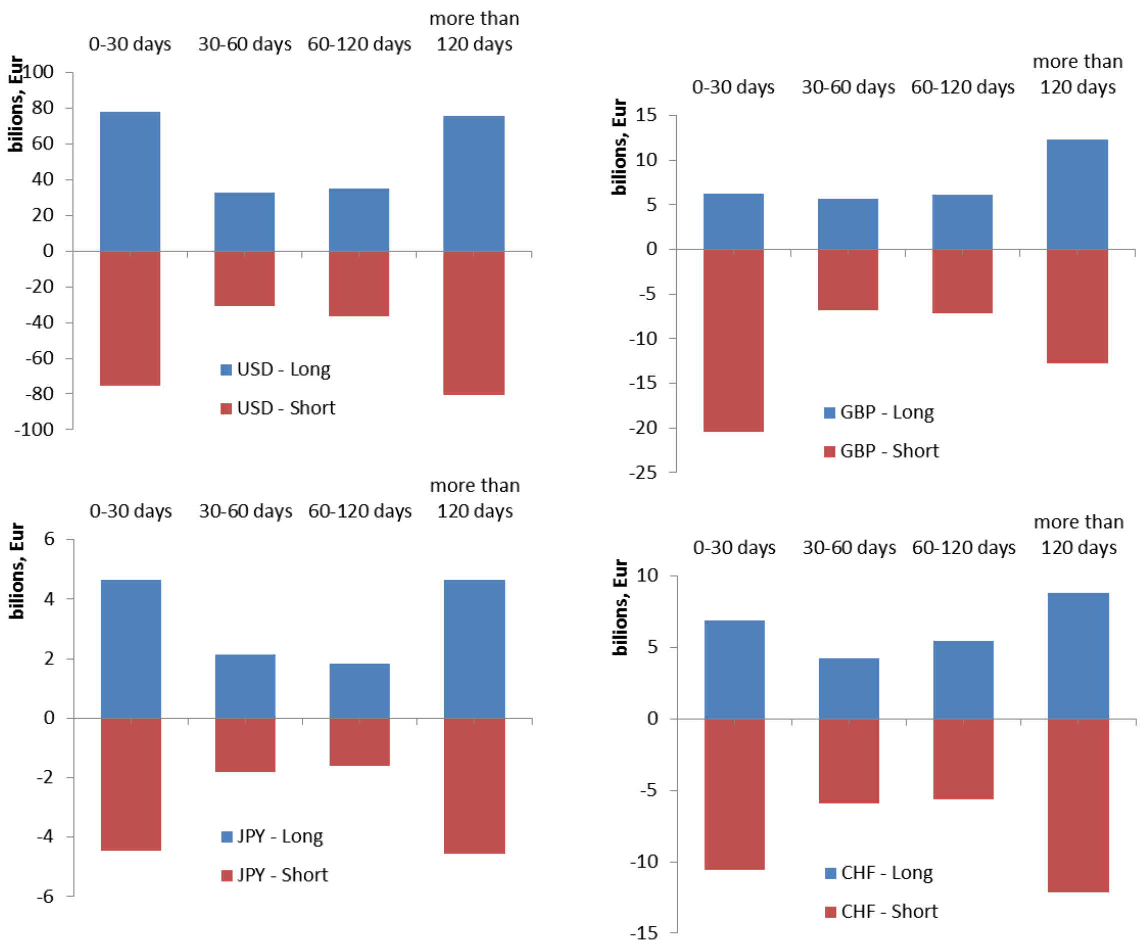


FIGURE 2.4

Aggregate net FX balance sheet exposure and net FX derivatives exposure

Figure 2.4 comprises four graphs – one for each foreign currency: USD, GBP, JPY and CHF – that show aggregate net FX balance sheet exposure (in blue) and aggregate net FX derivative exposure (in red) of the 150 largest German banks (bank size measured as of 2014-08) reported monthly in billion euros. Net FX balance sheet exposure is calculated by subtracting FX liabilities from FX assets presented in Figure 2.2, and net FX derivatives exposure is calculated by subtracting short exposures (i.e. notional amounts committed to sell using FX forward contracts) from long exposures (i.e. notional amounts committed to buy using FX forward contracts) presented in Figure 2.3.

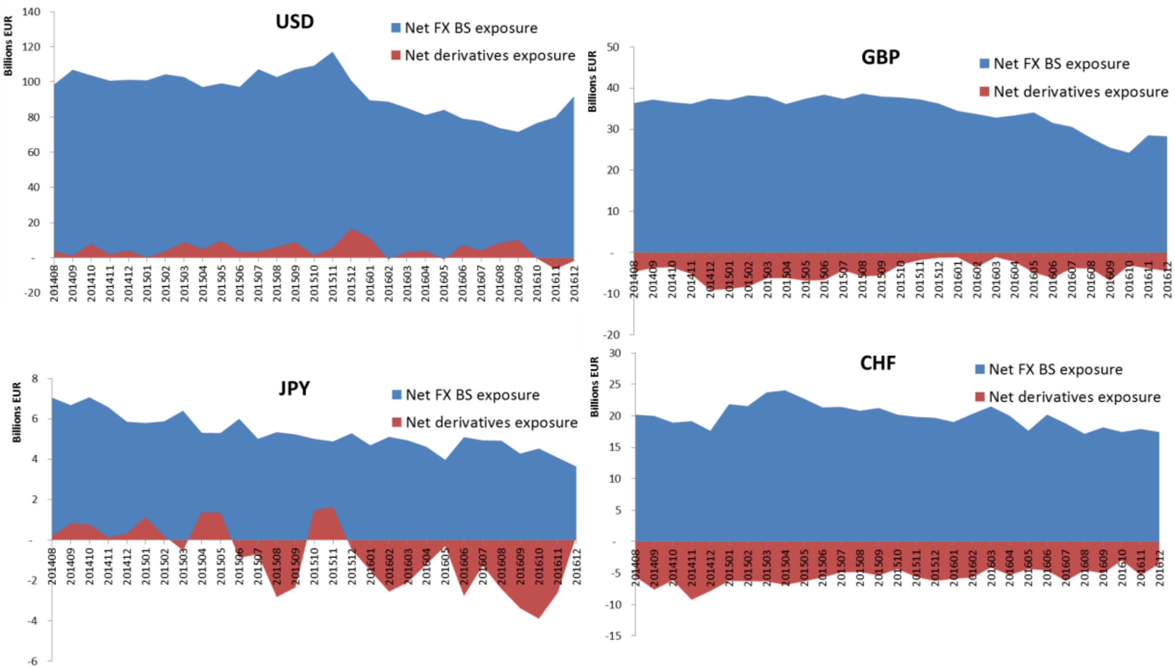


FIGURE 2.5

Bank-level net FX balance sheet exposure and net FX derivatives exposure

Figure 2.5 comprises four graphs – one for each foreign currency: USD, GBP, JPY and CHF – that show bank-level (each dot represents one bank) net FX balance sheet exposure (on x-axis) and net FX derivative exposure (on y-axis) of the 150 largest German banks (bank size measured as of 2014-08) reported at 2014-08 in billion euros. Net FX balance sheet exposure is calculated by subtracting FX liabilities from FX assets presented in Figure 2.2, and net FX derivatives exposure is calculated by subtracting short exposures (i.e. notional amounts committed to sell using FX forward contracts) from long exposures (i.e. notional amounts committed to buy using FX forward contracts) presented in Figure 2.3.

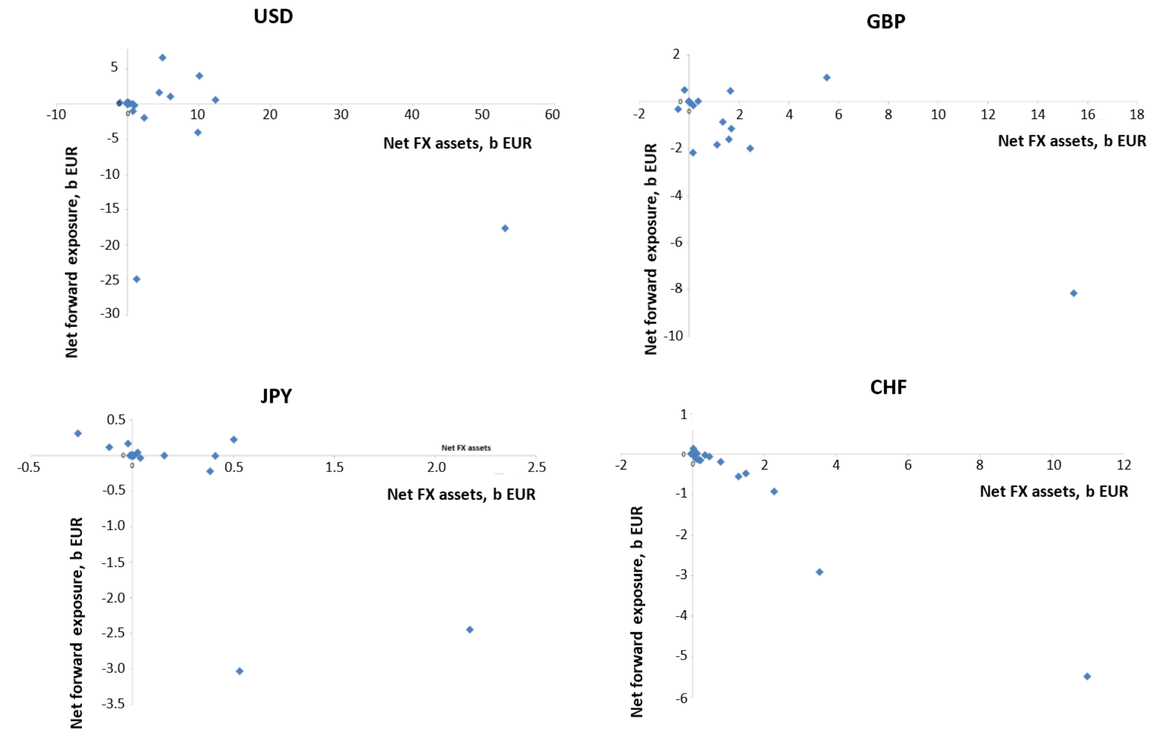
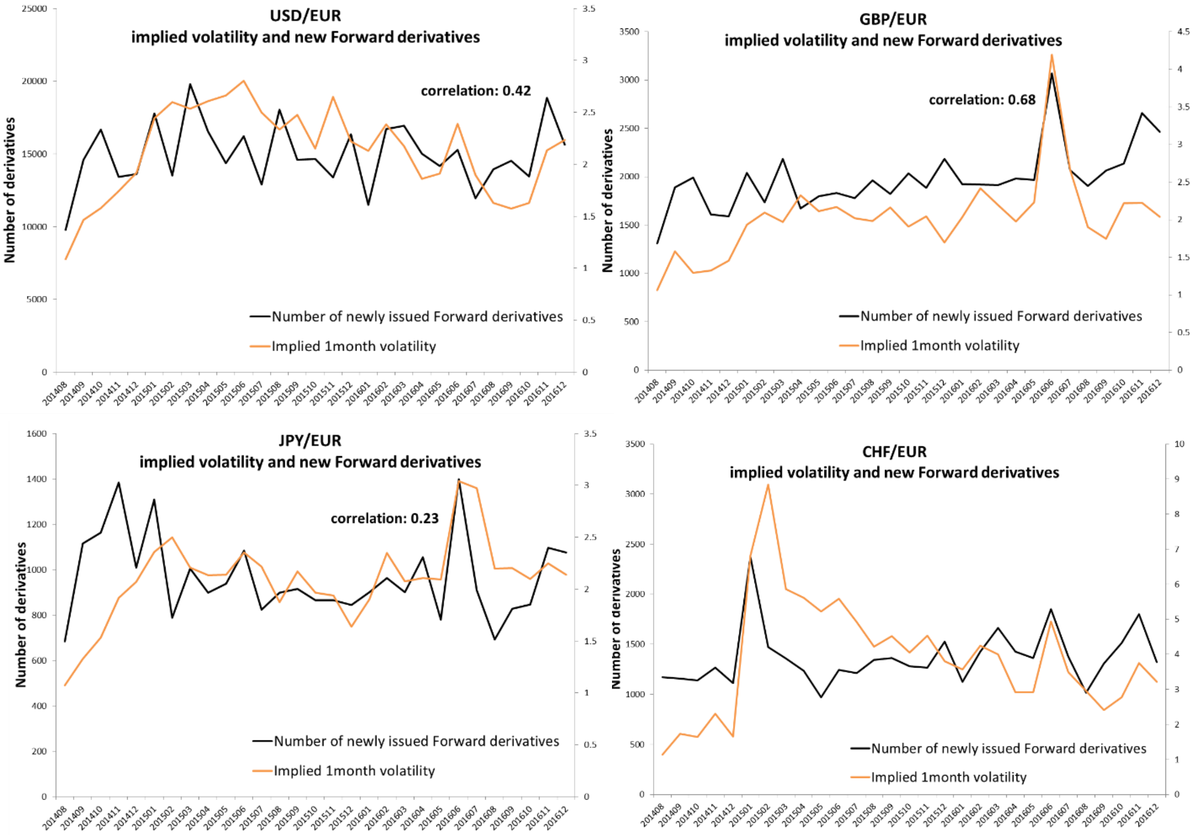


FIGURE 2.6

Implied FX volatility and usage of derivatives

Figure 2.6 comprises four graphs – one for each foreign currency: USD, GBP, JPY and CHF – that show monthly (average across daily observations within each month) FX volatility (i.e. volatility implied from 1-month FX options, extracted from Bloomberg), and the number of new OTC FX forward derivatives initiated within each month by all German banks reporting their derivative exposures to DTCC.



## Tables of Chapter 2

TABLE 2.1

Main results: usage of derivatives in times of FX volatility spikes

Table 2.1 presents the results of regression specifications (1), (2), (3) and (6) in columns (1), (2), (3) and (4), respectively. Every specification has a different dependent variable defined at the bank-currency-time level: in specification (1) – "Use of derivatives dummy" equal to 1 if a bank had non-zero net FX derivative exposure in a certain currency at a certain month end; in specification (2) ((3)) – "Short derivative exposure" ("Long derivative exposure") defined as a total notional amount of a foreign currency that a bank has committed to sell (buy) in the future using OTC FX forward contracts; in specification (4) – "Net FX derivatives" equal to "Long derivative exposure" minus "Short derivative exposure". Explanatory variables in specifications (1), (2) and (3) are bank-currency-time level "FX Assets" ("FX Liabilities") equal to total assets (liabilities) of a bank denominated in a certain foreign currency, currency-time level "FX Volatility" equal to volatility implied by 1-month FX options, and interactions between these variables. Note that "FX volatility" variable is absorbed by Currency-x-Time fixed effects. Explanatory variables in Specification (4) are bank-currency-time level "Net FX Assets" equal to "FX Assets" minus "FX Liabilities", and its interaction with "FX volatility". We consider 150 largest German banks (bank size measured as of 2014-08), four foreign currencies, i.e. USD, GBP, JPY and CHF, and monthly periods from 2014-08 to 2016-12.

| | Dependent variable: | | | |
|---|---|---|---|---|
| | Use_of_deriv_dummy | Short_deriv_exposure | Long_deriv_exposure | Net FX derivatives |
| | (1) | (2) | (3) | (4) |
| FX Assets | 0.006* | -0.051 | -0.207** | |
| | (0.074) | (0.374) | (0.020) | |
| FX Liabilities | 0.000 | 0.029 | 0.016 | |
| | (0.837) | (0.614) | (0.790) | |
| FX Assets x FX Volatility | -0.001 | 0.477** | 0.342 | |
| | (0.763) | (0.047) | (0.242) | |
| FX Liabilities x FX Volatility | 0.007** | -0.193 | 0.317* | |
| | (0.029) | (0.165) | (0.082) | |
| Net FX Assets | | | | -0.059 |
| | | | | (0.490) |
| Net FX Assets x FX Volatility | | | | -0.337* |
| | | | | (0.066) |
| Bank-x-Time FE | YES | YES | YES | YES |
| Bank-x-Currency FE | YES | YES | YES | YES |
| Currency-x-Time FE | YES | YES | YES | YES |
| Observations | 15,866 | 5,126 | 5,126 | 5,126 |
| R-squared | 0.913 | 0.879 | 0.884 | 0.736 |

P-values in parentheses. Standard errors are clustered multiway within banks and months.

*** p<0.01, ** p<0.05, * p<0.1

## TABLE 2.2

## Usage of derivatives in times of FX volatility spikes. Differential effects by bank characteristics

Table 2.2 presents the results of regression specifications (4) and (5) in columns (1)-(3) and (4)-(6), respectively. Dependent variables are defined at the bank-currency-time level: in specification (4) ((5)) – "Short derivative exposure" ("Long derivative exposure") is defined as a total notional amount of a foreign currency that a bank has committed to sell (buy) in the future using OTC FX forward contracts. Explanatory variables are bank-currency-time level "FX Assets" ("FX Liabilities") equal to total assets (liabilities) of a bank denominated in a certain foreign currency, currency-time level "FX Volatility" equal to volatility implied by 1-month FX options, bank-time level "BankChar" which is equal to "bank size" in columns (1) and (4), "E/A ratio" in columns (2) and (5), and "T1 ratio" in columns (3) and (6), and interactions between these variables. Note that "FX volatility" variable is absorbed by Currency-x-Time fixed effects and "BankChar" is absorbed by Bank-x-Time fixed effects. We consider 150 largest German banks (bank size measured as of 2014-08), four foreign currencies, i.e. USD, GBP, JPY and CHF, and monthly periods from 2014-08 to 2016-12.

| | Dependent variable: | | | | | |
|---|---|---|---|---|---|---|
| | Short_deriv_exposure | | | Long_deriv_exposure | | |
| BankChar: | size | E/A ratio | T1 ratio | size | E/A ratio | T1 ratio |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| FX Assets | -0.112 | -0.085 | -0.029 | -0.117* | -0.028 | -0.321*** |
| | (0.182) | (0.404) | (0.595) | (0.091) | (0.799) | (0.003) |
| FX Liabilities | -0.031 | 0.160* | 0.016 | -0.004 | -0.023 | 0.117 |
| | (0.785) | (0.066) | (0.801) | (0.967) | (0.820) | (0.224) |
| FX Assets x FX Volatility | 0.865*** | 0.244 | 0.059 | 0.876*** | -0.064 | 0.428 |
| | (0.001) | (0.221) | (0.680) | (0.001) | (0.820) | (0.131) |
| FX Liabilities x FX Volatility | -0.396 | -0.324* | 0.025 | -0.162 | 0.508** | -0.190 |
| | (0.101) | (0.094) | (0.896) | (0.427) | (0.012) | (0.524) |
| FX Assets x BankChar | 0.159 | 0.043 | 0.011 | -0.176 | -0.264* | 0.263** |
| | (0.234) | (0.764) | (0.907) | (0.196) | (0.057) | (0.042) |
| FX Liabilities x BankChar | 0.174 | -0.200 | 0.002 | 0.077 | 0.117 | -0.137 |
| | (0.361) | (0.169) | (0.985) | (0.701) | (0.411) | (0.236) |
| FX Volatility x BankChar | 9.178 | -6.327 | -5.675 | 2.882 | 0.206 | -10.041 |
| | (0.118) | (0.249) | (0.285) | (0.666) | (0.968) | (0.152) |
| FX Assets x FX Volatility x BankChar | -0.686* | 0.429 | 0.567** | -0.754** | 0.771** | -0.067 |
| | (0.051) | (0.189) | (0.047) | (0.046) | (0.030) | (0.876) |
| FX Liabilities x FX Volatility x BankChar | 0.098 | -0.029 | -0.234 | 0.476* | -0.832*** | 0.651* |
| | (0.776) | (0.928) | (0.326) | (0.057) | (0.001) | (0.053) |
| Bank-x-Time FE | YES | YES | YES | YES | YES | YES |
| Bank-x-Currency FE | YES | YES | YES | YES | YES | YES |
| Currency-x-Time FE | YES | YES | YES | YES | YES | YES |
| Observations | 5,126 | 5,126 | 5,126 | 5,126 | 5,126 | 5,126 |
| R-squared | 0.880 | 0.880 | 0.879 | 0.885 | 0.885 | 0.885 |

P-values in parentheses. Standard errors are clustered multiway within banks and months.

*** p<0.01, ** p<0.05, * p<0.1

TABLE 2.3

The effect of the Brexit vote on hedging

Table 2.3 presents the results of regression specifications (7), (8), (9) and (10) in columns (1), (5), (2)-(4), and (6)-(8), respectively. Dependent variables are defined at the bank-currency-time level: in columns (1)-(4) ((5)-(8)) – "Short derivative exposure" ("Long derivative exposure") is defined as a total notional amount of GBP that a bank has committed to sell (buy) in the future using OTC forward contracts. Explanatory variables are: a bank level dummy variable "Big FX Assets" ("FX Liabilities") equal to 1 if a bank had above median value of GBP-denominated assets (liabilities) among other banks as of 2016-03, a dummy variable "After" equal to 1 if an observation occurs after the Brexit vote, a bank level variable "BankChar", which is equal to "bank size" in columns (2) and (6), "E/A ratio" in columns (3) and (7), and "T1 ratio" in columns (4) and (8), as of 2016-03, and interactions between these variables. Note that bank level and month level variables are absorbed by either bank-fixed effects or time-fixed effects, respectively. We consider 150 largest German banks (bank size measured as of 2014-08) and monthly periods from 2016-03 to 2016-08, i.e. three month-ends before Brexit vote and three month-ends after.

| | Dependent variable: | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Short_deriv_exposure | | | | Long_deriv_exposure | | | |
| BankChar: | - | size | E/A ratio | T1 ratio | - | size | E/A ratio | T1 ratio |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| After x Big FX Assets | 0.275*** | 0.591*** | 0.076 | 0.070 | -0.211 | -0.397 | 0.123 | 0.133 |
| | (0.001) | (0.002) | (0.348) | (0.617) | (0.201) | (0.188) | (0.404) | (0.445) |
| After x Big FX Liabilities | -0.431*** | -0.576*** | -0.143 | -0.166 | 0.316** | 0.499*** | -0.066 | -0.259 |
| | (0.000) | (0.000) | (0.158) | (0.443) | (0.050) | (0.000) | (0.757) | (0.473) |
| After x BankChar | | 3.919 | 1.139 | 0.099 | | 4.493 | 1.457 | -3.540 |
| | | (0.172) | (0.441) | (0.951) | | (0.324) | (0.640) | (0.288) |
| After x Big FX Assets x BankChar | | -0.485** | 0.285** | 0.320* | | 0.685** | -0.622** | -0.422* |
| | | (0.029) | (0.010) | (0.066) | | (0.014) | (0.013) | (0.093) |
| After x Big FX Liabilities x BankChar | | 0.273* | -0.347*** | -0.339 | | -0.984*** | 0.521** | 0.661* |
| | | (0.073) | (0.002) | (0.131) | | (0.000) | (0.031) | (0.083) |
| Bank FE | YES | YES | YES | YES | YES | YES | YES | YES |
| Time FE | YES | YES | YES | YES | YES | YES | YES | YES |
| Observations | 234 | 234 | 234 | 234 | 234 | 234 | 234 | 234 |
| R-squared | 0.942 | 0.944 | 0.943 | 0.943 | 0.831 | 0.836 | 0.834 | 0.833 |

P-values in parentheses. Standard errors are clustered within banks

*** p<0.01, ** p<0.05, * p<0.1

# Chapter 3

# HUMAN VS. MACHINE: DISPOSITION EFFECT AMONG ALGORITHMIC AND HUMAN DAY-TRADERS

## 3.1 Introduction

Human efforts to raise productivity, marked by the technical progress (e.g. Rosenberg and Nathan, 1982), has brought the world to the Fourth Industrial Revolution (Schwab, 2017). Today's industries increasingly automate not only physical tasks but also decision making, which will likely contribute to the productivity and economic growth (Acemoglu and Restrepo, 2018), raising inequality (Berg et al., 2018) and the disruption of labor markets (Autor, 2015). In the long-run, technological changes may shape institutional frameworks, cultural norms, mental models of reality of individuals and their decision-making (North, 1994).[35] Therefore, it is important to understand the advantages and disadvantages of decisions implemented by algorithms over on-the-spot decisions made by humans. This understanding would help anticipate which industries are the most subject to change and how, and what type of behavior future generations may learn from their environments. Importantly, by comparing humans and machines, we may learn about humans' decision making, which is crucial for economic theory, mostly centered around the rationality assumption (Hogarth and Reder, 1987; Hirshleifer, 2001; Thaler, 2016).

An ideal setting for making this comparison is the stock market, where both professional human and algorithmic day-traders make frequent high-stake buy and sell decisions under uncertainty in an attempt to profit from short term price movements. In this paper we ask: do machines make more rational[36] decisions than humans, and if so, does that help them perform better? We focus on one of the most extensively documented puzzles in behavioral finance – the disposition effect – the tendency to sell

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[35] E.g. If newborns in the future will be constantly exposed to automated decision making (e.g. self-driving cars), it seems plausible that such an environment might teach them to make more machine-like decisions.

[36] We call a behavior "rational" if it complies with the expected utility theory, axiomatized by von Neumann and Morgenstern (1947): a representative rational agent is risk averse and makes choices that maximize expected utility derived from wealth levels (see, e.g., Machina 1987) (For other definitions, measures and interpretations of rationality see e.g. Marschak, 1950; Simon, 1978; Apesteguia and Ballester, 2015; Nagel, 2016)

winning stocks too early and to hold losing stocks for too long (Shefrin and Statman, 1985). We use the millisecond-frequency transaction-level data from January 2016 to December 2017 provided by NASDAQ OMX Copenhagen Stock Exchange and track all trades executed by every trader. We observe if a trader was a human or an algorithm[37], if it acted as a broker or traded on its proprietary account, if a trade provided or removed liquidity, the trade execution time, stock name, stock price and the traded number of shares. We focus our analysis on the most frequently trading human and algorithmic day-traders, which makes the two groups comparable in terms of their trading activity, namely, trading frequency, turnover, portfolio size, trading horizon and the selection of the most traded stocks. We follow Odean (1998) in measuring the disposition effect and find that it is substantial among professional human stock day-traders but virtually zero among algorithmic traders. The difference is not fully explained by rational motives such as portfolio rebalancing, contract-induced incentives, transaction costs or mechanics of limit orders. Meanwhile, we find support for less rational explanations, namely, the prospect theory, realization utility and beliefs in mean-reversion. We also find that the disposition effect harms the already poor performance of human traders, which further supports the irrational explanations. This suggests that human behavior systematically violates the expected utility theory, and that algorithms have an advantage of making more optimal trading decisions.

It has been argued that algorithms make decision-making more cost-effective and less noisy, i.e. more consistent (Kahneman et al., 2016). In addition, there is evidence that trading algorithms benefit from their speed advantage (Brogaard et al., 2015; Budish et al., 2015; Baron et al., 2018) and better access to information (Chordia et al., 2018; Biais et al., 2015). Do algorithms also make more rational decisions? Interviews suggest that programmers attempt to curb emotions and behavioral biases when coding trading algorithms (Borch and Lange, 2017). This is consistent with the conventional wisdom among trading professionals who use discipline, e.g. stop-loss strategies (Henderson et al., 2018), to minimize costs from irrational behavior (Locke and Mann, 2005). However, algorithms may suffer from certain biases too, inherited either from developers or from biased training data (see e.g. Cowgill and Tucker, 2019). Thus, it is not clear whether programmers manage to achieve the claimed discipline. To our knowledge, this is the first paper to compare humans and algorithms in terms of trading behavior and performance, and to provide evidence that algorithms in fact do trade more rationally and more successfully.

There has been an ongoing debate between rationalists and behavioralists on the "correct" way of economic modeling (see, e.g., Hogarth and Reder, 1987). The expected utility theory, axiomatized by von Neumann and Morgenstern (1947), characterized how a representative rational agent should make risky choices and became central to modern economic modeling. Kahneman and Tversky (1979) demonstrated that people systematically violate the rationality axioms and proposed an alternative descriptive

[37] NASDAQ OMX Copenhagen requires its members to register their trading accounts as "Personal" if the account is used for manual trading (user ID typically indicating the first few letters of traders' first and last names), and as "Algo" (user ID starting with PTRxxx, AUTDxx or LPSxxx) if the account is used by algorithms with no human involvement, i.e. "a computer algorithm automatically determines individual parameters of orders such as whether to initiate the order, the timing, price or quantity of the order or how to manage the order after its submission". (Nasdaq, 2018)

theory of risky choice – the prospect theory. It predicts, in contrast to the expected utility theory, that people (1) assign different weights to probabilities of outcomes, (2) maximize utility drawn from gains and losses rather than from final wealth, (3) are risk-averse when facing gains and risk-seeking when facing losses, and (4) suffer from losses more than they enjoy adequate gains. This spurred the debate on rationality further (Thaler, 2016).

The prospect theory paired with mental accounting (Thaler, 1985) have provided a long-standing preference-based explanation of the disposition effect (e.g. Shefrin and Statman, 1985; Odean, 1998; Weber and Camerer, 1998; Henderson et al., 2018): if investors view every stock as a separate mental account, and are risk-seeking when facing losses but risk-averse when facing gains, they would prefer to continue gambling with losing investments and to sell winning investments in order to lock in gains. Another preference-based theory – realization utility (Barberis and Xiong, 2009, 2012; Ingersoll and Jin, 2013; Frydman et al., 2014) claims that utility, i.e. pleasure and pain, is drawn directly from the realization of gains and losses. Pleasure and pain are related to a number of elements: e.g. cognitive dissonance, i.e. psychological costs of admitting to mistakes (Chang et al., 2016), pride and regret (Shefrin and Statman, 1985; Strahilevitz et al. 2011; Frydman and Camerer, 2016), self-control problems, i.e. planner-doer conflict whereby a doer (but not a planner) experiences the urge to postpone regret and hasten pride of past decisions (Shefrin and Statman, 1985; Fischbacher et al., 2017), the salience of the stock purchase price (Frydman and Wang, 2019; Dierick et al., 2019; Frydman and Rangel, 2014) and affect, i.e. "hot" immediate reaction to recent events (Loewenstein, 2005). Since both preference-based explanations view outcomes, i.e. gains and losses, relative to a reference point, they contradict the rational agent of the expected utility theory.

Beliefs offer alternative (rational and irrational) explanations of the disposition effect (see e.g. Ben-David and Hirshleifer, 2012). For example, investors may believe in mean-reversion and, thus, keep stocks when prices fall and sell stocks when prices rise. Similarly, investors may believe they have private information, which has not been incorporated into the stock price yet. If the stock price falls, investors may either rationally or due to overconfidence believe that it is just a temporary setback and continue to hold losing investments until the market incorporates that private information. If the stock price rises, investors may believe that the private information has been incorporated as expected, and thus sell the investments at a gain. An opposite effect, whereby a gain (loss) reinforces (hurts) confidence in the private information and urges to buy more (to sell) stock, is also possible (Ben-David and Hirshleifer, 2012). However, empirically, both belief-based explanations found little support in the literature (Weber and Camerer, 1998; Odean, 1998; Kaustia, 2010). Moreover, even if they do drive the disposition effect, such beliefs have been shown to be irrational, due to past winners persistently outperforming past losers (Odean, 1998; Frazzini, 2006; Strahilevitz et al., 2011).

The literature on the disposition effect also considers the following rational explanations. (1) Portfolio rebalancing (Odean, 1998; Kaustia, 2010): gains (losses) increase (decrease) the weight of certain stocks in a portfolio, and to restore the well-diversified balance investors may sell a part of the winning stocks (keep or buy more losing stocks). (2) Mechanics of limit orders (Linnainmaa, 2010): if an investor sold a stock using a limit order, the counterparty must have crossed the bid-ask spread and pushed the price up, which makes it more likely that the sold stock was a winner than a loser. (3) Earnings management or contract-induced incentives (Beatty and Harris, 1999): e.g. banks were found to smooth their reported taxable earnings by strategically realizing gains and losses from securities. (4) Transaction costs (Odean, 1998): low-priced stocks may have relatively higher transaction costs; thus, investors might be reluctant to trade stocks after their prices decrease. (5) Tax considerations (Lakonishok and Smidt, 1986; Odean, 1998): investors have incentives to realize losses in order to reduce taxable income and, in turn, tax payable, but this would generate the reverse disposition effect.

All these rational and irrational theories potentially could explain why we observe a substantial disposition effect among human traders but virtually no disposition effect among algorithms. Firstly, human traders make on-the-spot decisions under stress while developers have time to “think slow” (Kahneman, 2011) and to calmly pass on their deliberate logic to algorithms, keeping in mind that their coded principles would be used for multiple buy and sell decisions in the future. By “thinking slow”, i.e. using System 2, developers may avoid behavioral biases, heuristics and other cognitive features of System 1 such as attachments to reference points and loss aversion, which are at the heart of the prospect theory (Kahneman, 2011). Secondly, at the moment of coding, developers are unlikely to feel any pleasure or pain from defining selling decisions, which makes algorithms less dependent on realization utility and other related elements such as cognitive dissonance, pride and regret, and salience of the purchase price. Also, by coding, algorithmic traders effectively pre-commit to their future buy and sell decisions and thus avoid self-control problems and “hot” reactions. Thirdly, algorithmic traders, equipped with better access to information (Chordia et al., 2018; Biais et al., 2015) and the ability to continuously analyze market data, may have different beliefs than humans in mean-reversion and private information. Fourthly, algorithmic traders may use fundamentally different trading strategies than human traders and thus might care less about the portfolio rebalancing. For instance, market making and cross-market arbitrage strategies, once carried out by humans, have been replaced by algorithms (Danish FSA, 2016). Fifthly, if algorithms use relatively fewer limit orders than humans, this could, at least partially, explain the difference in the disposition effect. Sixthly, human traders may have different career concerns and compensation schemes than programmers of trading algorithmic, and depending on accounting rules, may have stronger incentives to report realized gains (losses) as large (small) as possible. Seventhly, market venues compete to attract algorithmic traders by offering favorable transaction costs (Danish FSA, 2016), which might make algorithms less sensitive to them. We argue that if there are other rational motives to realize gains and losses, that are common to both algorithms and humans, e.g. taxes, developers should take them into account when coding trading algorithms, and thus, they should not cause the observed difference in the disposition effect between humans and algorithms.

**Results.** Our estimates of the substantial disposition effect among humans and the virtually zero disposition effect among algorithms remain similar when considering (1) only long daily positions, (2) only short daily positions, (3) only those positions that are short (long) from a daily perspective but long (short) from a two-year perspective, and

(4) when considering only full but not partial closures of existing positions. Furthermore, we find that humans use relatively more market orders and less limit orders than algorithms. These findings suggest that the aforementioned rational motives fail to explain the large difference in the disposition effect between humans and algorithms. Meanwhile, we find evidence supporting the less rational explanations, namely, (1) the realization utility, (2) the prospect theory and (3) beliefs in mean-reversion. Specifically, we find that (1) humans but not algorithms trade more aggressively, i.e. use disproportionally more market orders, when realizing losses, as if they were nervous and trying to "get over it quickly", (2) the disposition effect among humans but not among algorithms reacts to the exogenous factor – the weather, and (3) humans but not algorithms tend to open new long (short) positions after stock price drops (hikes). Finally, we find that if a human (algorithmic) trader had been forced to stop trading at any point of the day, 8 trading hours later, his or her frozen daily positions would have lost EUR 435 (gained EUR 259) on average. This superior performance of algorithmic traders cannot be attributed to the execution speed advantage and suggests that algorithms are better at predicting price movements over the next 8 trading hours. The 8-hour profits would have been significantly higher (lower) for both humans and algorithms, if traders were forced to realize all their paper losses (gains) just before freezing their portfolios. The fact that humans persistently realize more gains than losses despite this behavior harming their performance further suggests the irrationality of the disposition effect (Odean, 1998).

**Literature and contribution.** Our paper contributes to a few lines of research, including (1) algorithmic trading, (2) disposition effect, (3) weather effects on financial markets, (4) algorithmic bias and (5) the debate on the rationality assumption in economics.

The literature on algorithmic trading so far has focused on studying algorithmic traders' speed advantage (Budish et al., 2015; Baron et al., 2018), informational advantage (Chordia et al., 2018; Biais et al., 2015), trading strategies (Hagströmer and Nordén, 2013; Menkveld, 2013; Malinova et al., 2014; O'Hara, 2015), and impact on market quality (Hendershott et al., 2011), especially, liquidity (Hendershott and Riordan, 2013; Brogaard et al., 2015; Ait-Sahalia and Saglam, 2017; Brogaard et al., 2018;), volatility (Hasbrouck and Saar, 2013; Kirilenko et al., 2017), and price efficiency (Carrion, 2013; Brogaard et al., 2014; Chaboud et al., 2014; Brogaard et al., 2019; Conrad et al. 2015; Weller, 2017). We contribute by demonstrating that rationality, or lack of behavioral biases, is another economically significant advantage of algorithmic traders. Algorithmic trading has been proliferating across financial markets (Kirilenko and Lo, 2013), which suggests that these markets on average have been becoming more rational. Furthermore, to our knowledge, this is the first paper to compare the behavior and performance between algorithmic and human traders.

The literature on the disposition effect has documented the effect in different markets, e.g. stocks (Odean, 1998), stock options (Heath et al., 1999), futures of currencies and commodities (Locke and Mann, 2005), and real estate (Genesove and Mayer, 2001), and among different market participants, e.g. individual investors (Odean, 1998), institutional investors (Grinblatt and Keloharju, 2001), mutual funds (Cici, 2012) and professional futures' day-traders (Locke and Mann, 2005). A long-standing explanation of the disposition effect has been the prospect theory (Shefrin and Statman, 1985; Odean, 1998; Weber and Camerer, 1998; Henderson 2012; Li and Yang, 2013; Henderson et al., 2018; Meng and Weng, 2018), however, more recently, a particular focus has been set on identifying other explanations theoretically (Barberis and Xiong, 2009; Barberis and Xiong, 2012; Ingersoll and Jin, 2013) and empirically (Kaustia 2010; Weber and Welfens, 2011; Ben-David and Hirshleifer, 2012; Frydman et al., 2014; Frydman and Rangel, 2014; Chang et al., 2016; Frydman and Camerer, 2016; Fischbacher et al., 2017; Frydman et al., 2017; Frydman and Wang, 2019; Dierick et al., 2019). Other papers examine the impact of the disposition effect on asset prices (Grinblatt and Han, 2005; Frazzini, 2006; An, 2015; Birru, 2015). We contribute by documenting, for the first time, the lack of the disposition effect among algorithmic traders – an important group of traders that constituted roughly half of trading volume at Nasdaq Copenhagen in the beginning of our data sample period (Danish FSA, 2016). To the best of our knowledge, this is also the first paper to document the disposition effect among professional stock day-traders at the intraday horizon. Furthermore, we contribute by identifying irrational explanations of the disposition effect using novel strategies such as the exogenous effect of the weather and the use of liquidity absorbing orders.

This paper also contributes to the behavioral finance literature studying how the weather affects financial markets. For instance, weather has been shown to affect stock returns (Hirshleifer and Shumway, 2003; Goetzmann et al., 2014), behavior of individual (Schmittmann et al., 2014) and institutional (Goetzmann et al., 2014) investors, and behavior and performance of loan-officers (Cortés et al., 2016). We contribute with evidence that weather affects the disposition effect.

We also add to the literature on algorithmic bias and fairness (Cowgill and Tucker, 2019). For instance, algorithms have been shown to make biased and discriminatory decisions in lending (Bartlett et al., 2019), criminal sentencing (Dressel and Farid, 2018) and ad targeting (Datta et al., 2015). We provide the first evidence that algorithms can make more rational decisions, as defined by von Neumann and Morgenstern (1947), and that this leads to a better performance.

Finally, by providing novel evidence of subrational behavior of human traders, we contribute to the debate on the rationality assumption in economics (Hogarth and Reder, 1987; Hirshleifer, 2001; Thaler, 2016).

## 3.2 Data

We use the millisecond-stamped transaction-level trade data from 9 am., i.e. the stock market’s opening time, January 1, 2016 to 5 pm, i.e. the stock market’s closing time, December 31, 2017 provided by the NASDAQ OMX Copenhagen Stock Exchange. We observe the following information about every trade executed by every approved member of the stock exchange: (1) the execution date and time at millisecond precision, (2) the name of the traded stock, (3) the indicator of whether shares were bought or sold, (4) the share price of the traded stock, (5) the number of shares traded, (6) the indicator of whether a trade added or removed liquidity, (7) the indicator of whether a trade was executed on a trader's own proprietary account or on behalf of the trader's client (i.e. a trader acted as a broker) (8) the name of a trader's institution, i.e. a member of the stock exchange, (9) the indicator of whether a trader's account is used by a human or an algorithm, (10) the user account name (first three letters of a trader's name and surname for humans and PTRxxx, AUTDxx or LPSxxx for algorithms), and (11) the name of a counterparty's organization. Conveniently, every trade enters the dataset twice, treating each counterparty as a primary one. The name of an organization in combination with the user account name provides a unique trader's id.

While we do not know how exactly trading algorithms are coded, what strategies every of them follows and how complex they are, e.g. if they are self-learning and adjust depending on their trading experience, we do know that they are programmed to make the following decisions without human involvement: "whether to initiate the order, the timing, price or quantity of the order or how to manage the order after its submission" (Nasdaq, 2018). These are the requirements of the NASDAQ Copenhagen when issuing "Algo"-type accounts starting with PTRxxx, AUTDxx or LPSxxx to its members. For an overview of the algorithmic trading on the NASDAQ Copenhagen, refer to the report of the Danish Financial Supervisory Authority released in February 2016 – at the beginning of our sample period (Danish FSA, 2016). The report summarizes algorithms' trading strategies, algorithms' benefits and risks to the market, the recent trends in trading volume of algorithms and humans, regulations, etc.

In total, our dataset contains 102,553,306 transactions. Since we cannot identify traders that access the stock market through the brokerage services provided by the exchange's members, we focus only on the proprietary trades of the exchange members. This leaves us with 39,740,156 transactions in 159 different stocks: 32,243,301 transactions executed by 91 algorithmic trading accounts from 33 member institutions and 7,496,855 transactions executed by 597 human trading accounts from 54 member institutions. The trading frequency across both human and algorithmic traders is very heterogenous (see Figure 3.1). In this paper, we focus on day-traders, i.e. those that trade the most frequently, for three reasons. Firstly, to the best of our knowledge, this is the first paper to analyze the intraday disposition effect in the stock market. Secondly, most of the algorithms in our database trade frequently throughout the day. For instance, more than two thirds of algorithms (63 of 91) trade on average at least once in every 10 minutes (i.e. 48 times per day). Thirdly, we want to identify algorithms that are the least likely to be affected by the direct human intervention. For instance, a seldomly trading algorithm might be launched by a human only when he or she desires to trade particular stocks, while continuously trading algorithms allow less time for a human to intervene.

In order to identify day-traders, in our default setting, we consider only those human and algorithmic traders that on average execute at least 1 trade in every two minutes (at least 240 trades per day). However, our results are qualitatively similar if we use different thresholds, e.g. at least 1 trade in every 10 minutes (48 trades per day) or at least 1 trade in every 1 minute (480 trades per day) (See the two tables in Appendix). Moreover, the most frequently trading human executes 1,523 trades per day on average, thus, in order to make the two groups of traders comparable, we exclude algorithmic traders that trade more frequently than 1,530 times per day on average. In our default setting, this leaves us with 11,097,306 transactions: 5,899,279 of them executed by 31 algorithmic traders from 14 member institutions and 5,198,027 trades executed by 34 human day-traders from 13 member institutions.

How comparable are these two groups of traders? We estimate the following variables at the trader-day level: (1) total number of trades; (2) total turnover; (3) portfolio size, calculated as an average stock inventory (grossing both long and short stock positions) valued at 5-minute intervals throughout a day at original purchase (sale, for short positions) prices; and (4) trading horizon in days, calculated, similarly to "Inventory days", as a ratio of average portfolio size over the total value of shares sold (repurchased, for short positions) throughout a day valued at purchase (sale, for short positions) prices. Also, for each trader-day, we identify (1) 10 most traded stocks in terms of total turnover, (2) the member institution type, e.g. international bank, local bank etc., and (3) the city of its headquarters. As shown in Table 3.1.A, humans and algorithms trade similarly. Humans on average execute 695 trades per day, while algorithms execute 68 trades more. This difference is not statistically significant. An average daily turnover of a human trader is EUR 5.7 m and is not statistically different from an average turnover of an algorithm (EUR 5.1 m). The difference between an average portfolio size of a human (EUR 1.4 m) and of an algorithm (EUR 1.1 m) is also not statistically significant. For both humans and algorithms, it would take almost 3 (2.7 for humans and 2.8 for algorithms) days on average to close their positions opened throughout a day. Finally, humans on average generate 90% and algorithms 86% of turnover by trading 10 favorite stocks of a day. This difference is not statistically significant. Table 3.1.B shows that humans and algorithms trade the same stocks. The table presents the list of the 10 most popular stocks for both humans and algorithms. It is based on the number of times that every stock enters an individual trader's top 10 in terms of daily turnover. Most (22 of 34 for humans and 24 of 31 for algorithms) of the analyzed proprietary day-traders are employed by large international banks such as BNP Paribas, Barclays, Credit Suisse, Deutsche Bank, Goldman Sachs, Merrill Lynch, Citigroup, Societe Generale, Nordea, Danske Bank, SEB, HSBC and JP Morgan. The rest of traders work for small investment banks or local commercial banks. Algorithmic traders are located in London (20), Paris (7), Stockholm (2), Copenhagen (1) and Dublin (1), while human traders are based in London (8), Randers (7), Paris (6), Copenhagen (6), Stockholm (3), Silkeborg (2) and Aabenraa (2).

## 3.3 Methodology

In the default setting, we consider 31 algorithmic and 34 human day-traders that trade on their proprietary accounts, and make between 240 and 1530 trades per day on average.[38] In line with Locke and Mann (2005), Coval and Shumway (2005), Baron et al. (2018), we assume that traders start with zero inventory every day[39] and by trading

[38] As argued in "Data" section, in this way we focus on the comparable algorithmic and human day-traders. Our results are robust to including algorithms that trade more frequently than 1,530 per day on average and to using other minimum thresholds instead of 240, e.g. 48 or 480 trades per day (i.e. at least 1 trade in every 10 or in every 1 minute, respectively). As a robustness check, in the two tables in Appendix, we present the main results from Table 3.2 but using these different thresholds.

[39] Our results are qualitatively similar if we assume zero starting inventory on the first day and accumulate inventories, gains and losses over the two-year sample period.

build up their long and short positions throughout a day.[40] Having a timeline of all trades in the market, and using a volume-weighted average purchase price (WAPP) as a reference purchase price[41], we calculate total gain for every trader-stock position at every point in time. Total gain consists of cumulative realized gain and outstanding paper gain. Outstanding paper gain is calculated by multiplying the number of shares outstanding by the difference between the last observed stock price in the market and WAPP. Realized gain occurs when traders either fully or partially close their position, and is calculated by multiplying the number of shares sold (or repurchased, in case of short positions) by the difference between the selling (repurchasing) price and WAPP. Cumulative realized gain is calculated by accumulating realized gains throughout a day. Following Odean (1998), we measure the disposition effect at every point of time for every trader as the proportion of gains realized (PGR) minus the proportion of losses realized (PLR). PGR (PLR) equals trader's cumulative realized gains above (below) zero summed up across all trader-stock positions divided by total gains above (below) zero summed up across all trader-stock positions.[42]

## 3.4 Results

Figure 3.2.A shows PGR and PLR at the end of every hour throughout the day, averaged across traders and days within both groups, i.e. humans and algorithms. The graph shows that by the end of the day, algorithms realize on average 32% of gains and 32% of losses, while humans realize 35% of gains but only 20% of losses. Due to the assumption of zero starting inventory, these gains and losses can be interpreted as incrementally caused by actions taken throughout the day. Table 3.2 Panel A shows that the average disposition spread, i.e. the average difference between PGR and PLR across all traders, days and hours, is 1 pp and not statistically significant from zero for algorithms, and 12 pp and statistically significant at 1% level for humans.[43] Figure 3.2.B (3.2.C) and Table 3.2 Panel B (C) shows that when considering only long (short) positions, the disposition spread is 1 pp (1 pp) and not statistically significant for algorithms and 15 pp (13 pp) and statistically significant at 1% level for humans. Finally, Figure 3.2.D and Table 3.2 Panel D shows that human day-traders do but algorithms do not realize significantly more gains than losses when considering long-term portfolios, i.e. when we assume zero starting inventory on the first trading day and accumulate inventories throughout the whole two-year sample period. The average

[40] Although in the default setting, we use both long and short positions, we show that our results hold for both long and short positions separately.

[41] The results are robust if we use first-in-first-out method to determine the reference purchase price (see the figure in Appendix).

[42] Originally, Odean (1998) measures the disposition effect for long term investors who trade less frequently. Realized gains (losses) are counted daily as a number of different stocks sold at a gain (loss) and paper gains (losses) are counted daily as a number of different stocks held at a gain (loss) but not sold. To get closer to the original measure, we calculate for every trader hourly PGR (PLR) as a number of shares sold at a gain (loss) within a given hour divided by the total number of winning (losing) shares held in that hour, i.e. the shares sold at a gain (loss) within a given hour plus the winning (losing) shares remaining at the end of the hour. Our results are qualitatively similar when using this alternative measure of PGR and PLR.

[43] In order to account for autocorrelation within trader's observations, standard errors are clustered at the trader level.

disposition spread is 1 pp and not statistically significant for algorithms, and 13 pp and statistically significant at 1% level for humans.

### 3.4.1 Rational Explanations

Firstly, in order to examine if the “portfolio rebalancing” story drives our results, we re-run the main analysis using only those realizations of gains and losses that close the position entirely and not just partially. According to Odean (1998), “investors who are rebalancing will sell a portion, but not all, of their shares of winning stocks. A sale of the entire holding of a stock is most likely not motivated by the desire to rebalance”. After eliminating the partial realizations, which might be motivated by rebalancing, our results remain qualitatively similar to the default setting (see Figure 3.2.E and Table 3.2 Panel E).

Secondly, it is plausible that accounting rules paired with career concerns or compensation schemes incentivize human traders to realize their gains and losses differently from algorithmic traders. For instance, banks have been shown to manage, e.g. smooth, their reported earnings by strategically realizing gains and losses from securities (see e.g. Dong and Zhang, 2017; Beatty and Harris, 1999; Ahmed and Takeda, 1995). To test this possibility, we consider those gains and losses that occur mentally, but are not reported in any way – i.e. missed opportunities to gain and lose. For instance, suppose a trader owns 100 shares and sells one. If the price goes up (down), the actual and reportable value of the portfolio increases (decreases), but the trader may consider the missed opportunity to earn (lose) money on the sold share as a loss (gain). The trader can “realize” this “loss” (“gain”) by repurchasing the sold share at the new higher (lower) price, but this “realization” would not be reflected in the actual profits. Our estimates of the disposition effect for both humans and algorithms are robust when considering only these mental “gains” and “losses” (See Figure 3.2.F and Table 3.2 Panel F). This result is consistent with Strahilevitz et al. (2011) who study how regret affects the repurchase of stocks previously sold. Specifically, we calculate cumulative inventories of every trader-stock position over the two-year period, and use only those trader-stock-days in which a long-term position, i.e. cumulative from day 1, remains long (short) throughout the whole given day, but the short-term position, i.e. cumulative from the beginning of the given day, is short (long). In this case, an upward (downward) price move brings gains (losses) from the long-term portfolio perspective, but losses (gains) from the narrower daily portfolio perspective. Thus a “daily” loss (gain) is not an actual loss (gain) that can be reported but a missed opportunity to gain (lose).

Thirdly, it is plausible that after losing, compensation schemes incentivize human traders to take extra risks, and if investors believe that low-priced stocks are more volatile than high-priced stocks (e.g. Ohlson and Penman, 1985; Dubofsky, 1991), they might prefer to hold on to stocks that recently decreased in price and caused losses. Similarly, it is possible that a low stock price makes traders reluctant to trade due to relatively high transaction costs. However, these explanations are plausible only when considering long positions, since with short positions they predict a reverse disposition effect. As can be seen in Figures 3.2.B and 3.2.C and Table 3.2 Panels B and C, long and short positions exhibit very similar disposition effects.

Fourthly, if human traders used relatively more limit orders than algorithms, especially when closing their positions to realize gains and losses, this could explain the difference of the disposition effect between the two groups (Linnainmaa, 2010). However, Figure 3.3.A shows that in fact humans use relatively less limit orders and more market (liquidity taking, aggressive) orders than algorithms when deepening positions and even more so when closing positions to realize gains and losses.

### 3.4.2 Less Rational Explanations

Firstly, Figure 3.3.B and Table 3.3 show that humans trade particularly aggressively, i.e. use relatively more liquidity absorbing market orders, when realizing losses as compared to when realizing gains or when deepening positions (i.e. not realizing either gains or losses). Meanwhile, algorithms trade almost equally aggressively when realizing losses and when deepening positions. This suggests that human traders are more nervous when realizing their losses as predicted by realization utility theory. Since the realization of losses is a painful procedure, human traders might urge to “get over it” quickly, and, thus, use more liquidity taking market orders. Following the argumentation of Linnainmaa (2010), if one sells a stock to realize a gain or a loss using an aggressive market order, one has to cross the bid-ask spread and thus the stock is more likely to be sold at a loss and less likely at a gain. These simple mechanics help explain why for algorithms in Figure 3.3.B the line representing the aggressive loss (gain) realization is slightly above (below) the line of non-realization. For humans, however, the loss realization line is far above other lines, which suggest there are other forces explaining why human traders use relatively more aggressive orders when realizing losses than when realizing gains or when not realizing either.

Secondly, we test if the gap between PGR and PLR is sensitive to the weather. We use hourly data on sunshine duration, temperature, precipitation and air pressure in the cities where traders in our dataset are located, provided by Meteoblue. Table 3.4 Panel A shows that human traders exhibit a larger disposition spread during sunny hours than on cloudy hours, while algorithms show no reaction to the weather (Table 3.4 Panel B). This result can be explained by the prospect theory. During sunny hours, human traders might be more distracted from work and thus rely more on System 1, which is subject to cognitive features such as attachments to reference points and loss aversion (Kahneman, 2011). These results, however, should be treated with caution, as they lack economical significance and are not very robust to different fixed effects and error clustering.

Thirdly, if traders believed in mean-reversion they would expect a stock price to increase after seeing it dropping and to decrease after seeing it rising, even if currently they have no position in that stock (Ben-David and Hirshleifer, 2012). We consider only those trades which open, but do not increase or decrease the existing, long or short daily trader-stock positions, assuming that every day starts with zero inventory. Figure 3.4 shows that humans, but not machines, tend to open their daily positions by selling recent (previous 60 minutes) winners and buying recent losers. This suggests that humans but not algorithms tend to believe in mean-reversion, which may contribute to the disposition effect.

### 3.4.3 Performance

Our evidence suggests that rational explanations such as portfolio rebalancing, contract-induced incentives, transaction costs and limit order mechanism cannot fully explain the large difference in disposition effect between humans and algorithms. Meanwhile, we find evidence that less rational explanations such as the prospect theory, the realization utility theory and beliefs in mean-reversion contribute to the difference. Independently on whether preferences or beliefs drive the disposition effect, if such behavior helps traders perform better, it would be justified and rational (Odean, 1998). However, if traders continue to exhibit disposition effect despite persistent evidence that it hurts their performance, this behavior would be irrational (Odean, 1998). In order to estimate the harm/benefit of the disposition effect we do the following exercise for the same groups of traders as before: 31 algorithmic and 34 human day-traders.

As before, we assume zero starting inventories every day and construct portfolios for every trader considering trades that they executed throughout the day. At the end of every trading hour we freeze portfolios' compositions (we call them the "Actual portfolios") and use stock prices prevailing 8 trading hours later to calculate how much profits every trader would have made over those next 8 trading hours had they not executed any more trades. Then, for every trader, at every moment of the freeze, we construct a hypothetical "Realization portfolio", which is formed by trades that would be necessary in order to realize all existing paper losses. Assuming constant compositions of "Realization portfolios" we calculate profits over the same next 8 trading hours. Adding up the "Actual portfolio" and the "Realization portfolio" gives us a "Combined portfolio" – a hypothetical portfolio that a trader would be holding at the moment of the freeze had he or she just realized all paper losses.

Figure 3.5.A shows profits earned within the next 8 trading hours by the "Actual", "Realization" and "Combined" portfolios frozen at various times of the day averaged across traders and days. Figure 3.5.A and Table 3.5 suggests that human traders on average would persistently make losses (on average 404 euros) over the next 8 trading hours if they stopped trading at any point of the day. Yet, on average they would earn more than that (421 euros) and break-even over the same 8 trading hours if they realized all their losses. The best time to realize all losses appears to be at around 1 pm since the "Realization portfolio" would have earned 596 euros on average over the next 8 trading hours. Realization of losses would allow human traders to avoid persistent losses since the "Combination portfolio" would earn on average 63 euros over the 8-hour period. Figure 3.5.B and Table 3.5 Panel B shows that algorithms are better at choosing stocks than humans as their "Actual portfolio" on average earns positive (even though not statistically significant) profits of 134 euros over the 8 hours[44]. However, they would benefit from realizing more losses too as their "Combined portfolio" would earn 271 euros on average. Interestingly, these findings suggest that algorithms can predict better

[44] When including algorithms that trade more frequently than 1,530 times per day on average, algorithms' "Actual portfolio" earns 260 euros on average - a positive profit statistically significant at 10%.

than humans, which stocks will be profitable during the next 8 trading hours. This difference in performance cannot be explained by algorithms' execution speed advantage, which is only the matter of milliseconds.

Results are different in magnitude but similar qualitatively when instead of 8-hour horizon we use 1, 2, 4, 16 and 24 hours: the "Realization portfolio" always generates gains and helps both humans to offset their loses and algorithms to increase their gains. We see a similar picture (Figure 3.5.C for humans and 3.5.D for algorithms) when looking at returns, i.e. when we divide portfolio profits by the initial portfolio values at the time of freezing. As shown in Table 3.5, both profits and returns of "Realization portfolio" are positive and statistically different from zero.

Using the same logic, we form "Realization portfolios" with trades that would realize all gains instead of losses (Table 3.6 and Figure 3.6.A for humans and 3.6.B for algorithms). In this case the "Realization portfolio" incurs negative profits – on average -173 euros for human and -283 euros for algorithms in the next 8 hours. This leads to "Combined portfolio" performance being worse than "Actual portfolio" performance on average. The same applies when analyzing returns instead of profits (Figure 3.6.C for humans and 3.6.D for algorithms). All in all, this evidence suggests that both the realization of gains and the non-realization of losses are detrimental to the trading performance, which suggests the disposition effect to be irrational behavior. In addition, since algorithms' average performance over the intraday horizon is always better than humans', independently on what time the portfolios are frozen, this serves as evidence that algorithms, either due to informational advantage, rationality or other reasons, are better at picking stocks for day trading and would outperform humans even without their execution speed advantage.

## 3.5 Conclusion

In this paper we ask: do machines make more rational decisions, as defined by the expected utility theory, than humans, and if so, does that help them perform better? We use two years of transaction-level millisecond-stamped trade data from the NASDAQ OMX Copenhagen Stock Exchange to compare the disposition effect between two groups of proprietary day-traders: algorithms and humans. In order to ensure the comparability between the two groups in terms of trading frequency, turnover, portfolio size, trading horizon and favorite stocks, and in order to minimize the likelihood that a human could directly impact trading decisions of algorithms, we focus our analysis on traders that trade the most frequently, namely the 31 algorithms and 34 humans that on average execute between 240 and 1530 trades per day. We also show that our results are qualitatively similar when changing the lower bound from 240 to 48 and 480 trades per day.

We find a substantial disposition effect among humans but virtually no disposition effect among algorithms. This difference cannot be fully explained by the popular rational explanations, such as portfolio rebalancing, contract-induced incentives, transaction costs or mechanics of limit orders. However, we find evidence that it is at least partially explained by less rational explanations such as the prospect theory, realization utility and beliefs in mean-reversion. The evidence of the irrationality of the disposition effect is reinforced by our finding that the realization of gains and the non-realization of losses systematically hurt future performance of both humans and algorithms. Finally, we find that algorithms have a better ability than humans to predict the stock price movements in the next 1, 2, 4, 8, 16 and 24 hours, which suggests that algorithms would outperform humans even without their advantage of execution speed.

These results suggest that professional human day-traders do not behave fully rationally as defined by the expected utility theory, even though more rational behavior, i.e. more equal realization of gains and losses, would lead to larger trading profits. Our findings also suggest that rationality can be achieved by automating the decision-making process. For example, by "thinking slow" (using System 2) programmers might avoid behavioral biases and heuristics. Also, while programming their decisions, which may or may not be executed in the future, depending on future situations, programmers can minimize their pleasure and pain derived from these decisions. Furthermore, by pre-committing to the future decisions, programmers can avoid self-control problems and "hot" reactions.

Our results suggest that, in addition to making faster, better-informed, less noisy and more cost-effective decisions than humans, algorithms have an additional advantage – they have a potential to make more rational decisions. This advantage may widen the scope of industries that could benefit from and be changed by the automation of decision-making. In the long run, future generations, surrounded by more rational decision-making executed by machines, might learn to behave in a more rational manner as well. Whether this is something to strive for depends, among other things, on ethics, people's priorities, and what machines are programmed to optimize, e.g. shareholders' profits, consumers' happiness, well-being of the society as whole etc.

## Figures of Chapter 3

FIGURE 3.1

Number of traders and total turnover ordered by traders' average trading frequency

Figure 3.1 shows the distribution of 91 algorithmic and 597 human traders ordered by their average trading frequency per day (blue columns, lhs). For example, a large part of both algorithmic (28) and human (427) traders trade relatively seldomly – less than 48 times per day (i.e. less than 1 trade in every 10 minutes) on average. The orange line (rhs) shows the aggregate turnover in euros generated by traders in each trading frequency category throughout the two-year sample period.

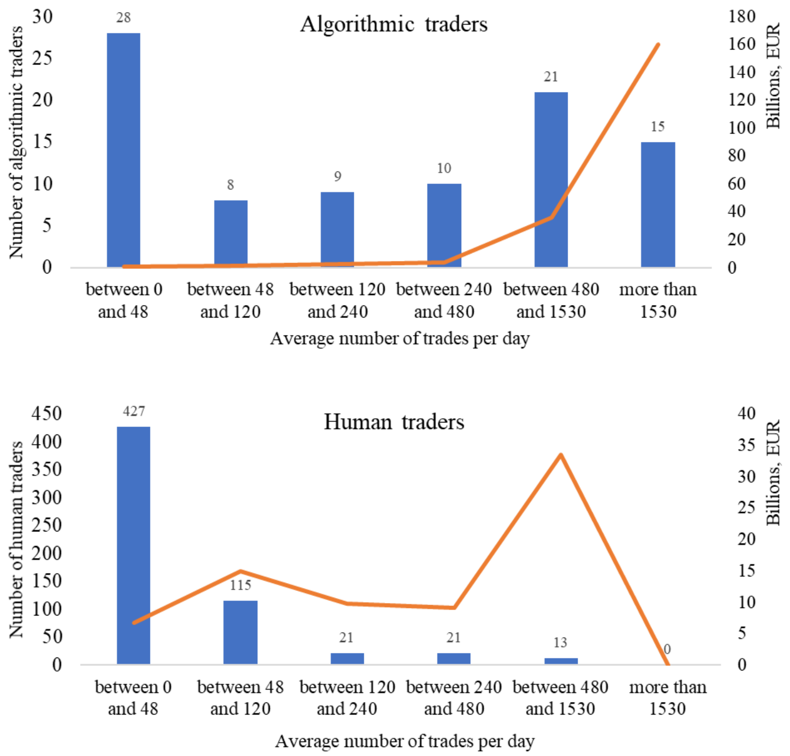


FIGURE 3.2.A

Realization of gains and losses throughout a day – default setting

Figure 3.2.A shows the proportion of gains realized (PGR) and the proportion of losses realized (PLR) at the start of every hour of a day, averaged across trading days and across traders in the two groups, i.e. humans and algorithms. The graph considers 31 algorithmic and 34 human proprietary traders that on average execute between 240 and 1,530 trades per day. Individual PGR and PLR for every trader are calculated as follows. Traders are assumed to start every day with zero inventory (at 9 am) and by trading to build their long and short positions in stocks throughout a day. For every trader-stock position at every point of time we calculate *total gain*, which consist of *cumulative realized gain* and *outstanding paper gain*. *Outstanding paper gain* is calculated by multiplying remaining inventory by the difference between the last observed stock price and the volume-weighted average purchase price (WAPP). *Realized gain* is calculated by multiplying the number of shares sold (or repurchased, in case of short positions) by the difference between the selling (repurchasing) price and WAPP. *Cumulative realized gain* is calculated by accumulating *realized gains* throughout a day. At any point of time, a trader's PGR (PLR) equals *cumulative realized gains* above (below) zero summed up across trader-stock positions divided by *total gains* above (below) zero summed up across trader-stock positions.

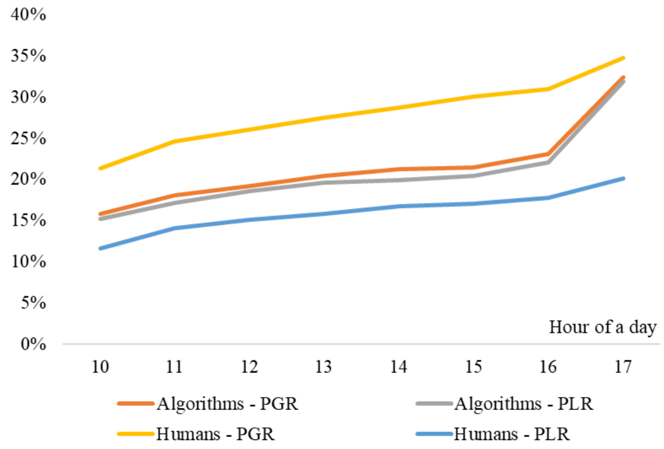



FIGURE 3.2.B

Realization of gains and losses throughout a day – only long positions

Figure 3.2.B shows the proportion of gains realized (PGR) and the proportion of losses realized (PLR) at the start of every hour of a day, averaged across trading days and across traders in the two groups, i.e. humans and algorithms. The graph considers 31 algorithmic and 34 human proprietary traders that on average execute between 240 and 1,530 trades per day. Individual PGR and PLR for every trader are calculated as follows. Traders are assumed to start every day with zero inventory (at 9 am) and by trading to build their long and short positions in stocks throughout a day. **In this chart we only consider long positions**. For every trader-stock position at every point of time we calculate *total gain*, which consist of *cumulative realized gain* and *outstanding paper gain*. *Outstanding paper gain* is calculated by multiplying remaining inventory by the difference between the last observed stock price and the volume-weighted average purchase price (WAPP). *Realized gain* is calculated by multiplying the number of shares sold by the difference between the selling price and WAPP. *Cumulative realized gain* is calculated by accumulating *realized gains* throughout a day. At any point of time, a trader's PGR (PLR) equals *cumulative realized gains* above (below) zero summed up across trader-stock positions divided by *total gains* above (below) zero summed up across trader-stock positions.

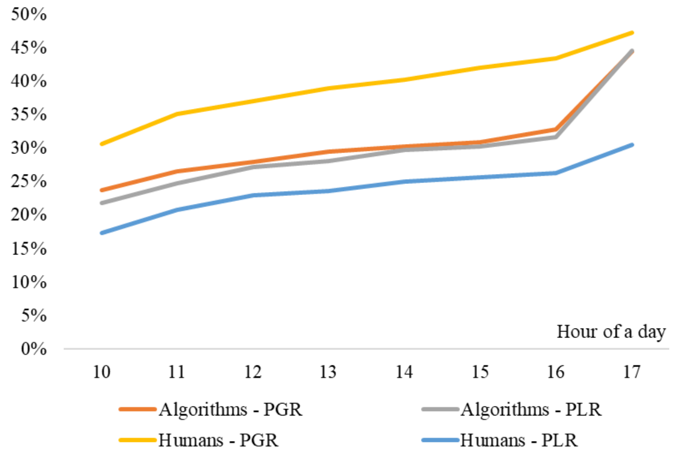


FIGURE 3.2.C

Realization of gains and losses throughout a day – only short positions

Figure 3.2.C shows the proportion of gains realized (PGR) and the proportion of losses realized (PLR) at the start of every hour of a day, averaged across trading days and across traders in the two groups, i.e. humans and algorithms. The graph considers 31 algorithmic and 34 human proprietary traders that on average execute between 240 and 1,530 trades per day. Individual PGR and PLR for every trader are calculated as follows. Traders are assumed to start every day with zero inventory (at 9 am) and by trading to build their long and short positions in stocks throughout a day. **In this chart we only consider short positions**. For every trader-stock position at every point of time we calculate *total gain*, which consist of *cumulative realized gain* and *outstanding paper gain*. *Outstanding paper gain* is calculated by multiplying remaining inventory by the difference between the last observed stock price and the volume-weighted average purchase price (WAPP). *Realized gain* is calculated by multiplying the number of shares repurchased by the difference between the repurchase price and WAPP. *Cumulative realized gain* is calculated by accumulating *realized gains* throughout a day. At any point of time, a trader's PGR (PLR) equals *cumulative realized gains* above (below) zero summed up across trader-stock positions divided by *total gains* above (below) zero summed up across trader-stock positions.

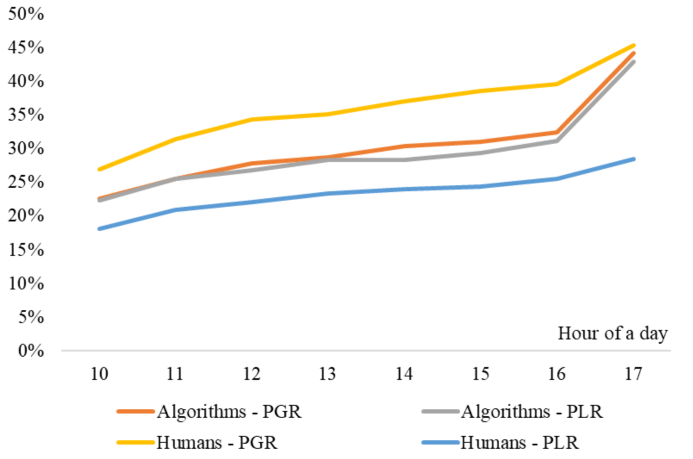



FIGURE 3.2.D

Realization of gains and losses throughout the two years sample period

Figure 3.2.D shows the proportion of gains realized (PGR) and the proportion of losses realized (PLR) at the end of every quarter, averaged across traders in the two groups, i.e. humans and algorithms. The graph considers 31 algorithmic and 34 human proprietary traders that on average execute between 240 and 1,530 trades per day. Individual PGR and PLR for every trader are calculated as follows. Traders are assumed to start with zero inventory on the first trading day and to build their long and short positions in stocks by trading throughout the two years. For every trader-stock position at every point of time we calculate *total gain*, which consist of *cumulative realized gain* and *outstanding paper gain*. *Outstanding paper gain* is calculated by multiplying remaining inventory by the difference between the last observed stock price and the volume-weighted average purchase price (WAPP). *Realized gain* is calculated by multiplying the number of shares repurchased by the difference between the repurchase price and WAPP. *Cumulative realized gain* is calculated by accumulating *realized gains* throughout the two years. At any point of time, a trader's PGR (PLR) equals *cumulative realized gains* above (below) zero summed up across trader-stock positions divided by *total gains* above (below) zero summed up across trader-stock positions.

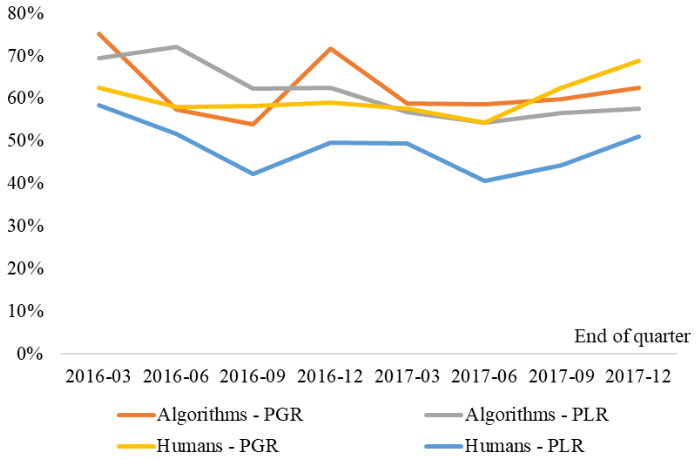


FIGURE 3.2.E

Realization of gains and losses throughout a day – without partial realizations

Figure 3.2.E shows the proportion of gains realized (PGR) and the proportion of losses realized (PLR) at the start of every hour of a day, averaged across trading days and across traders in the two groups, i.e. humans and algorithms. The graph considers 31 algorithmic and 34 human proprietary traders that on average execute between 240 and 1,530 trades per day. Individual PGR and PLR for every trader are calculated as follows. Traders are assumed to start every day with zero inventory (at 9 am) and by trading to build their long and short positions in stocks throughout a day. For every trader-stock position at every point of time we calculate *total gain*, which consist of *cumulative realized gain* and *outstanding paper gain*. *Outstanding paper gain* is calculated by multiplying remaining inventory by the difference between the last observed stock price and the volume-weighted average purchase price (WAPP). *Realized gain* is calculated by multiplying the number of shares sold (or repurchased, in case of short positions) by the difference between the selling (repurchasing) price and WAPP. **In this chart we consider only those sales (repurchases), which completely closed trader-stock positions**. *Cumulative realized gain* is calculated by accumulating *realized gains* throughout a day. At any point of time, a trader's PGR (PLR) equals *cumulative realized gains* above (below) zero summed up across trader-stock positions divided by *total gains* above (below) zero summed up across trader-stock positions.

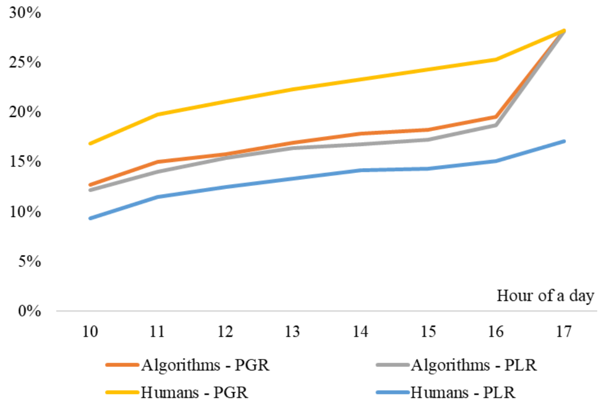


FIGURE 3.2.F

Realization of gains and losses throughout a day – mental "gains" and "losses"

Figure 3.2.F shows the proportion of gains realized (PGR) and the proportion of losses realized (PLR) at the start of every hour of a day, averaged across trading days and across traders in the two groups, i.e. humans and algorithms. The graph considers 31 algorithmic and 34 human proprietary traders that on average execute between 240 and 1,530 trades per day. Individual PGR and PLR for every trader are calculated as follows. Traders are assumed to start every day with zero inventory (at 9 am) and by trading to build their long and short positions in stocks throughout a day. **We call these positions "daily" positions.** For every trader-stock position at every point of time we calculate *total gain*, which consist of *cumulative realized gain* and *outstanding paper gain*. *Outstanding paper gain* is calculated by multiplying remaining inventory by the difference between the last observed stock price and the volume-weighted average purchase price (WAPP). *Realized gain* is calculated by multiplying the number of shares sold (or repurchased, in case of short positions) by the difference between the selling (repurchasing) price and WAPP. *Cumulative realized gain* is calculated by accumulating *realized gains* throughout a day. At any point of time, a trader's PGR (PLR) equals *cumulative realized gains* above (below) zero summed up across trader-stock positions divided by *total gains* above (below) zero summed up across trader-stock positions. **We also calculate "overall" trader-stock positions assuming zero inventory at 9 am of day 1, and accumulating inventories throughout the two years. In this chart we only consider those trader-stock positions, which are either long throughout the whole day from the "overall" perspective and short from the "daily" perspective or short throughout the whole day from the "overall" perspective and long from the "daily" perspective. Thus, "daily" losses (gains) are not actual losses (gains) but missed opportunities to gain (lose) "overall".**

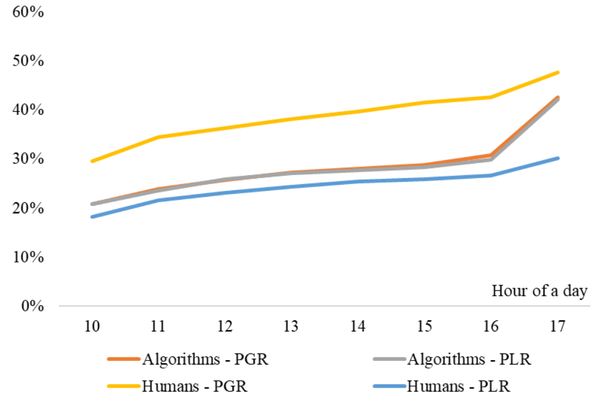
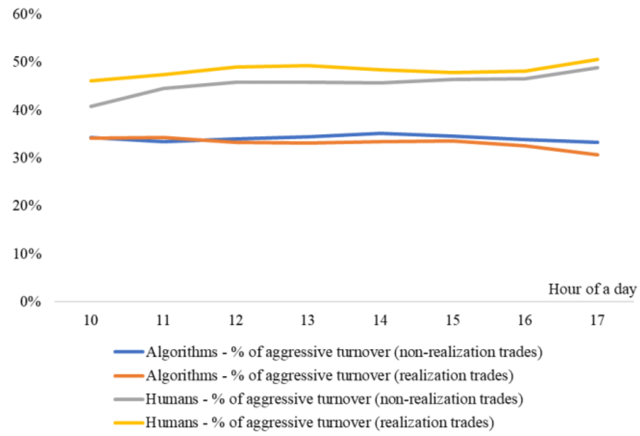



FIGURE 3.3.A

Aggressiveness of trades – realization and non-realization trades

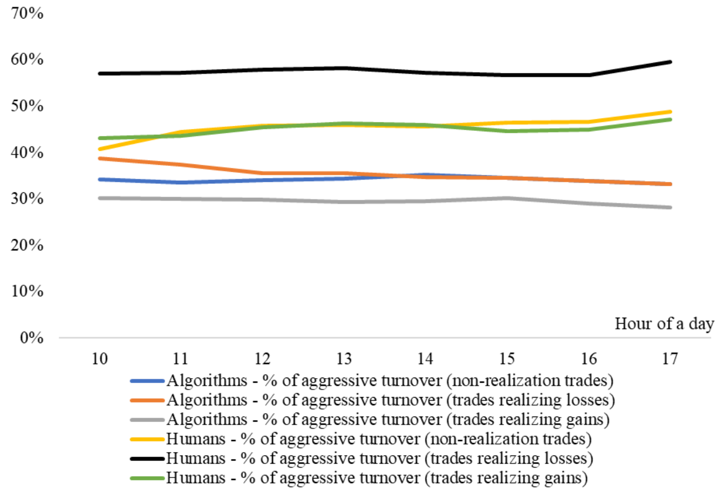
Figure 3.3.A shows the average ratio of trader's hourly turnover that was executed with market orders over the sum of hourly turnover executed using both market and limit orders. The ratio is averaged across trading days and across traders in the two groups, i.e. humans and algorithms. We consider separately (1) trades that opened or deepened existing positions, i.e. non-realization trades, and (2) trades that closed (partially or fully) existing positions, i.e. realization trades. The graph considers 31 algorithmic and 34 human proprietary traders that on average execute between 240 and 1,530 trades per day.



## FIGURE 3.3.B

## Aggressiveness of trades – loss realization, gain realization and non-realization trades

Figure 3.3.B shows the average ratio of trader's hourly turnover that was executed with market orders over the sum of hourly turnover executed using both market and limit orders. The ratio is averaged across trading days and across traders in the two groups, i.e. humans and algorithms. We consider separately (1) trades that opened or deepened existing positions, i.e. non-realization trades, (2) trades that closed (partially or fully) existing positions at a loss, i.e. trades realizing losses and (3) trades that closed (partially or fully) existing positions at a gain, i.e. trades realizing gains. The graph considers 31 algorithmic and 34 human proprietary traders that on average execute between 240 and 1,530 trades per day.




## FIGURE 3.4

## Opening daily positions by buying and selling recent winners and losers

Figure 3.4 shows the average number of times per day that traders opened their daily positions (assuming zero starting inventory every day) by buying and selling recent winners, i.e. stocks that increased in price during the previous 60 minutes, and recent losers, i.e. stocks that decreased in price during the previous 60 minutes. The black lines are 95% confidence intervals. The graph shows that human traders tend to open their positions by selling recent winners and buying recent losers, which is in line with the beliefs in mean-reversion. Algorithms tend to do the opposite – open their positions by buying recent winners and selling recent losers, which is in line with trend following. However, the result for algorithms is not statistically significant, as all the confidence intervals overlap.

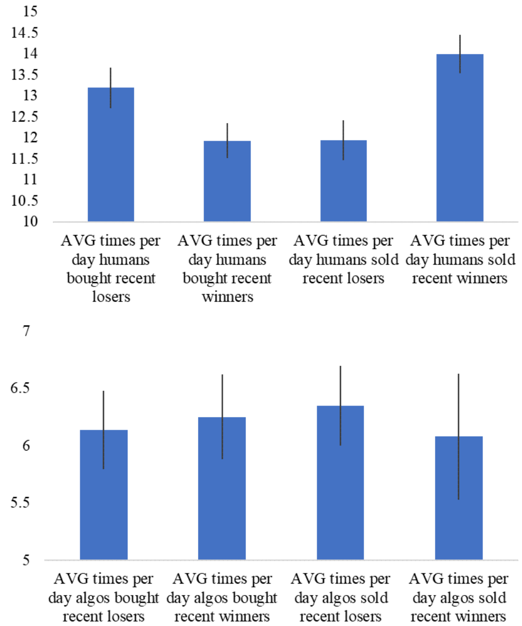


FIGURE 3.5.A

## Gains of frozen portfolios of humans over the next 8 hours – case of loss realization

Figure 3.5.A shows average profits in euros earned over the next 8 hours by three types of portfolios frozen at different times of the day. The average is calculated across human traders and trading days. Dashed lines of corresponding colors represent confidence intervals. Individual trader's "Actual portfolio" is constructed by assuming zero starting inventory every day and executing actual trades up to the moment of the freeze. The composition of the "Actual portfolio" is frozen at every hour of a trading day. Individual trader's "Realization portfolio" is a hypothetical portfolio constructed by executing trades necessary to realize all existing paper losses at the moment of the freeze. Individual trader's "Combined portfolio" is a combination of both "Actual portfolio" and the "Realization portfolio", thus, it is a hypothetical portfolio that a trader would hold at the moment of the freeze had he just realized all paper losses. The gain of every portfolio is calculated by comparing stock prices at the moment of the freeze and eight trading hours later, holding the portfolios' compositions constant. The graph considers 34 human proprietary traders that on average execute between 240 and 1,530 trades per day.

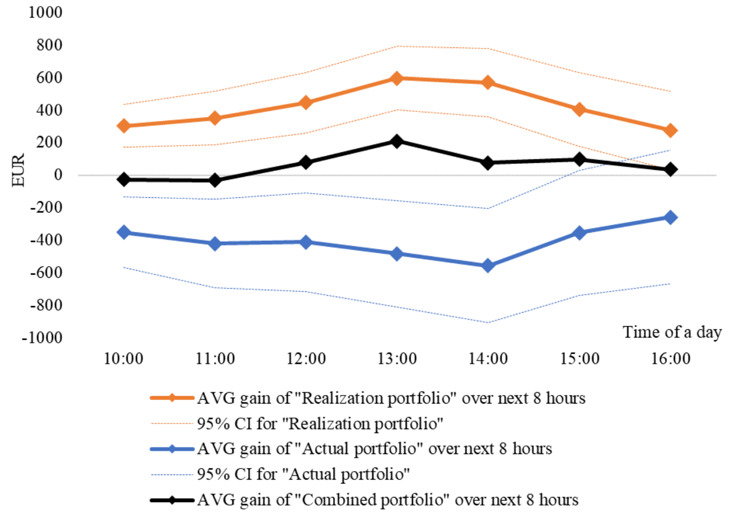
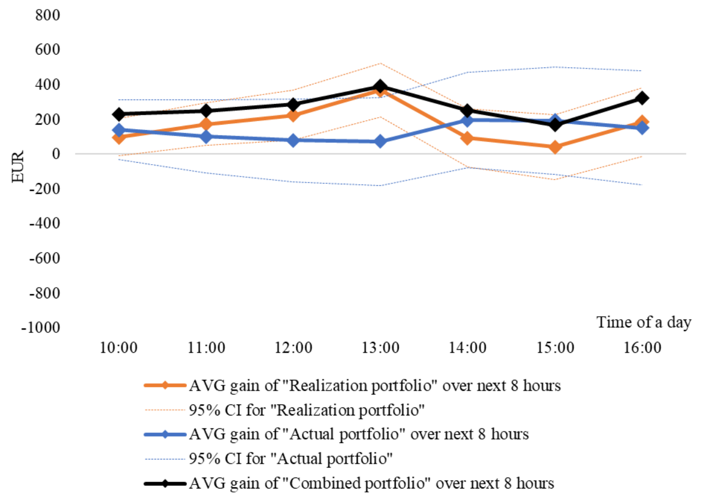



FIGURE 3.5.B

## Gains of frozen portfolios of algorithms over the next 8 hours – case of loss realization

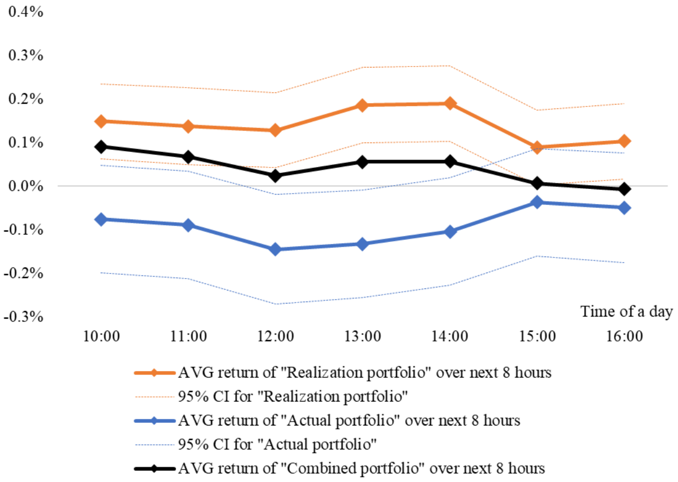
Figure 3.5.B shows average profits in euros earned over the next 8 hours by three types of portfolios frozen at different times of the day. The average is calculated across algorithmic traders and trading days. Dashed lines of corresponding colors represent confidence intervals. Individual trader's "Actual portfolio" is constructed by assuming zero starting inventory every day and executing actual trades up to the moment of the freeze. The composition of the "Actual portfolio" is frozen at every hour of a trading day. Individual trader's "Realization portfolio" is a hypothetical portfolio constructed by executing trades necessary to realize all existing paper losses at the moment of the freeze. Individual trader's "Combined portfolio" is a combination of both "Actual portfolio" and the "Realization portfolio", thus, it is a hypothetical portfolio that a trader would hold at the moment of the freeze had he just realized all paper losses. The gain of every portfolio is calculated by comparing stock prices at the moment of the freeze and eight trading hours later, holding the portfolios' compositions constant. The graph considers 31 algorithmic proprietary traders that on average execute between 240 and 1,530 trades per day.



## FIGURE 3.5.C

## Return of frozen portfolios of humans over the next 8 hours – case of loss realization

Figure 3.5.C shows average returns earned over the next 8 hours by three types of portfolios frozen at different times of the day. The average is calculated across human traders and trading days. Dashed lines of corresponding colors represent confidence intervals. Individual trader's "Actual portfolio" is constructed by assuming zero starting inventory every day and executing actual trades up to the moment of the freeze. The composition of the "Actual portfolio" is frozen at every hour of a trading day. Individual trader's "Realization portfolio" is a hypothetical portfolio constructed by executing trades necessary to realize all existing paper losses at the moment of the freeze. Individual trader's "Combined portfolio" is a combination of both "Actual portfolio" and the "Realization portfolio", thus, it is a hypothetical portfolio that a trader would hold at the moment of the freeze had he just realized all paper losses. The return of every portfolio is calculated by subtracting the portfolio value at stock prices prevailing at the time of the freeze from the portfolio value at stock prices prevailing 8 trading hours later (holding the portfolios' compositions constant), and dividing the difference by the former portfolio value. The graph considers 34 human proprietary traders that on average execute between 240 and 1,530 trades per day.




## FIGURE 3.5.D

## Return of frozen portfolios of algorithms over the next 8 hours – case of loss realization

Figure 3.5.D shows average returns earned over the next 8 hours by three types of portfolios frozen at different times of the day. The average is calculated across algorithmic traders and trading days. Dashed lines of corresponding colors represent confidence intervals. Individual trader's "Actual portfolio" is constructed by assuming zero starting inventory every day and executing actual trades up to the moment of the freeze. The composition of the "Actual portfolio" is frozen at every hour of a trading day. Individual trader's "Realization portfolio" is a hypothetical portfolio constructed by executing trades necessary to realize all existing paper losses at the moment of the freeze. Individual trader's "Combined portfolio" is a combination of both "Actual portfolio" and the "Realization portfolio", thus, it is a hypothetical portfolio that a trader would hold at the moment of the freeze had he just realized all paper losses. The return of every portfolio is calculated by subtracting the portfolio value at stock prices prevailing at the time of the freeze from the portfolio value at stock prices prevailing 8 trading hours later (holding the portfolios' compositions constant), and dividing the difference by the former portfolio value. The graph considers 31 algorithmic proprietary traders that on average execute between 240 and 1,530 trades per day.

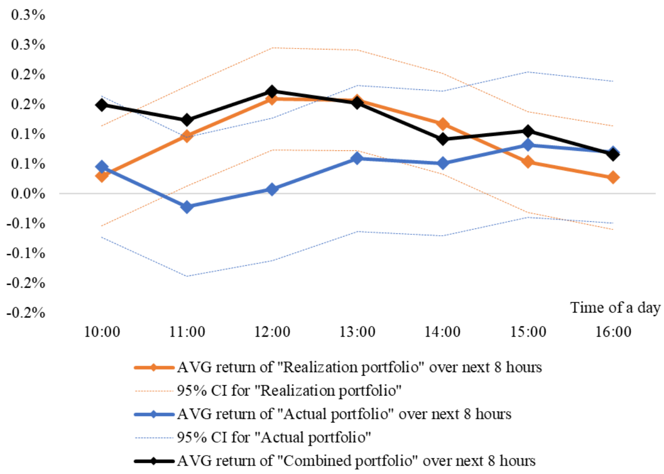


FIGURE 3.6.A

Gains of frozen portfolios of humans over the next 8 hours – case of gain realization

Figure 3.6.A shows average profits in euros earned over the next 8 hours by three types of portfolios frozen at different times of the day. The average is calculated across human traders and trading days. Dashed lines of corresponding colors represent confidence intervals. Individual trader's "Actual portfolio" is constructed by assuming zero starting inventory every day and executing actual trades up to the moment of the freeze. The composition of the "Actual portfolio" is frozen at every hour of a trading day. Individual trader's "Realization portfolio" is a hypothetical portfolio constructed by executing trades necessary to realize all existing paper gains at the moment of the freeze. Individual trader's "Combined portfolio" is a combination of both "Actual portfolio" and the "Realization portfolio", thus, it is a hypothetical portfolio that a trader would hold at the moment of the freeze had he just realized all paper gains. The gain of every portfolio is calculated by comparing stock prices at the moment of the freeze and eight trading hours later, holding the portfolios' compositions constant. The graph considers 34 human proprietary traders that on average execute between 240 and 1,530 trades per day.

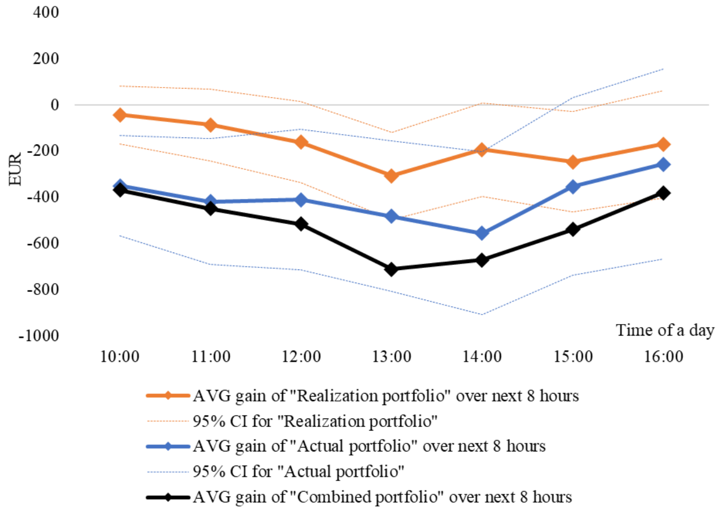
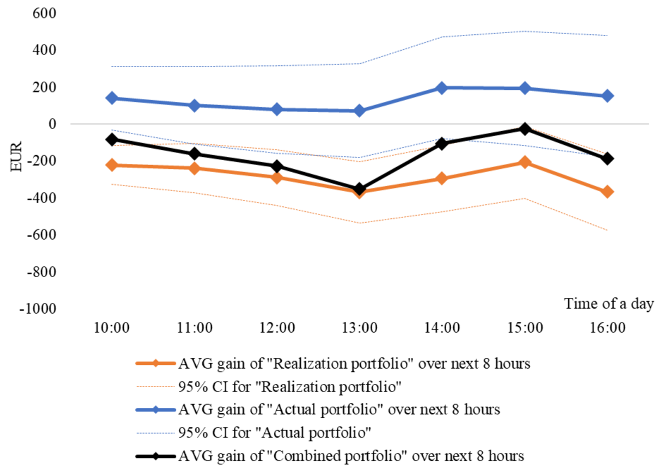



FIGURE 3.6.B

Gains of frozen portfolios of algorithms over the next 8 hours – case of gain realization

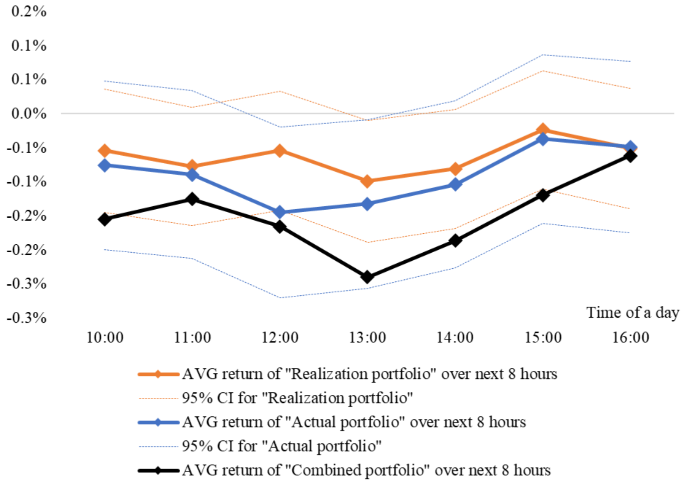
Figure 3.6.B shows average profits in euros earned over the next 8 hours by three types of portfolios frozen at different times of the day. The average is calculated across algorithmic traders and trading days. Dashed lines of corresponding colors represent confidence intervals. Individual trader's "Actual portfolio" is constructed by assuming zero starting inventory every day and executing actual trades up to the moment of the freeze. The composition of the "Actual portfolio" is frozen at every hour of a trading day. Individual trader's "Realization portfolio" is a hypothetical portfolio constructed by executing trades necessary to realize all existing paper gains at the moment of the freeze. Individual trader's "Combined portfolio" is a combination of both "Actual portfolio" and the "Realization portfolio", thus, it is a hypothetical portfolio that a trader would hold at the moment of the freeze had he just realized all paper gains. The gain of every portfolio is calculated by comparing stock prices at the moment of the freeze and eight trading hours later, holding the portfolios' compositions constant. The graph considers 31 algorithmic proprietary traders that on average execute between 240 and 1,530 trades per day.



## FIGURE 3.6.C

## Return of frozen portfolios of humans over the next 8 hours – case of gain realization

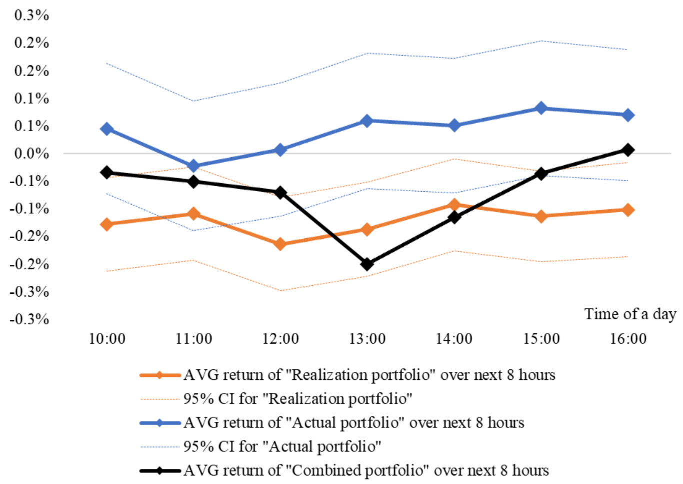
Figure 3.6.C shows average returns earned over the next 8 hours by three types of portfolios frozen at different times of the day. The average is calculated across human traders and trading days. Dashed lines of corresponding colors represent confidence intervals. Individual trader's "Actual portfolio" is constructed by assuming zero starting inventory every day and executing actual trades up to the moment of the freeze. The composition of the "Actual portfolio" is frozen at every hour of a trading day. Individual trader's "Realization portfolio" is a hypothetical portfolio constructed by executing trades necessary to realize all existing paper gains at the moment of the freeze. Individual trader's "Combined portfolio" is a combination of both "Actual portfolio" and the "Realization portfolio", thus, it is a hypothetical portfolio that a trader would hold at the moment of the freeze had he just realized all paper gains. The return of every portfolio is calculated by subtracting the portfolio value at stock prices prevailing at the time of the freeze from the portfolio value at stock prices prevailing 8 trading hours later (holding the portfolios' compositions constant), and dividing the difference by the former portfolio value. The graph considers 34 human proprietary traders that on average execute between 240 and 1,530 trades per day.




## FIGURE 3.6.D

## Return of frozen portfolios of algorithms over the next 8 hours – case of gain realization

Figure 3.6.D shows average returns earned over the next 8 hours by three types of portfolios frozen at different times of the day. The average is calculated across algorithmic traders and trading days. Dashed lines of corresponding colors represent confidence intervals. Individual trader's "Actual portfolio" is constructed by assuming zero starting inventory every day and executing actual trades up to the moment of the freeze. The composition of the "Actual portfolio" is frozen at every hour of a trading day. Individual trader's "Realization portfolio" is a hypothetical portfolio constructed by executing trades necessary to realize all existing paper gains at the moment of the freeze. Individual trader's "Combined portfolio" is a combination of both "Actual portfolio" and the "Realization portfolio", thus, it is a hypothetical portfolio that a trader would hold at the moment of the freeze had he just realized all paper gains. The return of every portfolio is calculated by subtracting the portfolio value at stock prices prevailing at the time of the freeze from the portfolio value at stock prices prevailing 8 trading hours later (holding the portfolios' compositions constant), and dividing the difference by the former portfolio value. The graph considers 31 algorithmic proprietary traders that on average execute between 240 and 1,530 trades per day.



## Tables of Chapter 3

TABLE 3.1.A

Comparison of trading activity between algorithms and humans

Table 3.1.A shows the results of regressing trader-day level observations of five different variables on a constant and a dummy *Algorithm*, which is equal to one if a trader is an algorithm and zero if it is a human. We consider 31 algorithmic and 34 human proprietary traders that on average execute between 240 and 1530 trades per day. The four dependent variables are calculated as follows: (1) "N of trades" is a total number of trades that a trader executed in a given day; (2) "Turnover" is a total turnover in euros traded by a trader in a given day; (3) "Portfolio size", measured in euros, is calculated by assuming that every trader starts every day with zero inventory and builds long and short stock positions by trading throughout the day. Every 5 minutes, i.e. 96 times per day, we calculate values of every short and long trader-stock position by multiplying the outstanding number of shares by the original purchase (sale, for short positions) price, and sum up gross values of all positions to arrive at 96 daily observations for each trader. "Portfolio size" is an average across the 96 daily observations. (4) "Inventory days", measured in days, is calculated by dividing "Portfolio size" by the total value of shares sold (repurchased, for short positions) during a given day valued at purchase (sale, for short positions) prices. (5) "Turnover top10" is a ratio of daily turnover in the most traded 10 stocks throughout the day over the total daily turnover. The table suggests that the differences between humans and algorithms are not statistically significant in any of these five categories.

| | Dependent variable: | | | | |
|---|---|---|---|---|---|
| | N of trades | Turnover | Portfolio size | Inventory days | Turnover top10 |
| Algorithm | 67.9 | -598,733 | -346,552 | 0.1 | -0.040 |
| | (0.566) | (0.616) | (0.102) | (0.948) | (0.148) |
| Constant | 694.5*** | 5,710,481*** | 1,416,845*** | 2.7*** | 0.902*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Observations | 121,720 | 121,720 | 121,720 | 112,832 | 121,552 |

P-values in parentheses. Standard errors are clustered at trader's level

*** p<0.01, ** p<0.05, * p<0.1

TABLE 3.1.B

Comparison of trading activity between algorithms and humans: most traded stocks

Table 3.1.B presents the list of the 10 most popular stocks for both humans and algorithms. It is based on the number of times that every stock enters an individual trader's top 10 in terms of daily turnover.

| Humans | | Algorithms | |
|---|---|---|---|
| Number of times that a stock is among trader's top 10 in terms of daily turnover | Stock name | Number of times that a stock is among trader's top 10 in terms of daily turnover | Stock name |
| 5159 | NOVO B | 6072 | NOVO B |
| 4794 | VWS | 5099 | VWS |
| 4588 | GEN | 4725 | DANSKE |
| 4582 | PNDORA | 4374 | PNDORA |
| 4421 | DANSKE | 4327 | GEN |
| 3627 | MAERSK B | 4076 | MAERSK B |
| 2832 | DSV | 3904 | DSV |
| 2736 | CARL B | 3617 | CARL B |
| 2545 | COLO B | 3414 | COLO B |
| 2419 | NZYM B | 3242 | NZYM B |

## TABLE 3.2

## Realization of gains and losses

Table 3.2 shows the results of regressing hourly (end of hour) trader-level observations of the spread between the proportion of gains realized (PGR) and the proportion of losses realized (PLR) on a constant and a dummy *Algorithm*, which is equal to one if a trader is an algorithm and zero if it is a human. When regressing the spread on a constant only, we split the sample into two groups – humans and algorithms. Standard errors are clustered at a trader level. We consider 31 algorithmic and 34 human proprietary traders that on average execute between 240 and 1,530 trades per day. Individual PGR and PLR for every trader at the end of every hour are calculated as follows. In Panels A, B, C, E and F traders are assumed to start every day with zero inventory and by trading to build their long and short positions in stocks throughout a day. In Panel D, traders are assumed to start the first trading day with zero inventory and to accumulate inventory throughout the full two-year sample period. For every trader-stock position at every point of time we calculate *total gain*, which consist of *cumulative realized gain* and *outstanding paper gain*. *Outstanding paper gain* is calculated by multiplying remaining inventory by the difference between the last observed stock price and the volume-weighted average purchase price (WAPP). *Realized gain* is calculated by multiplying the number of shares sold (or repurchased, in case of short positions) by the difference between the selling (repurchasing) price and WAPP. *Cumulative realized gain* is calculated by accumulating *realized gains* over time. At any point of time, a trader's PGR (PLR) equals *cumulative realized gains* above (below) zero summed up across trader-stock positions divided by *total gains* above (below) zero summed up across trader-stock positions. The dependent variable is the difference between PGR and PLR. Panels A and D consider both long and short trader-stock positions, while Panels B and C consider only long and short positions, respectively. Panel E is similar to Panel A, but considers only those realizations of gains and losses that fully closed positions, i.e. it ignores those stock sales (or repurchases, in case of short positions) which realized only part of a gain or a loss. Panel F considers only those trader-stock positions, which are either long throughout the whole day from the 2-year perspective and short from the daily perspective or short throughout the whole day from the 2-year perspective and long from the daily perspective.

| | Dependent variable: PGR-PLR spread | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Panel A: all daily positions | | | Panel B: long daily positions | | | Panel C: short daily positions | | |
| Subsample: | Algorithms | Humans | Both | Algorithms | Humans | Both | Algorithms | Humans | Both |
| Algorithm | | | -0.112*** | | | -0.145*** | | | -0.118*** |
| | | | (0.002) | | | (0.001) | | | (0.004) |
| Constant | 0.009 | 0.121*** | 0.121*** | 0.009 | 0.154*** | 0.154*** | 0.011 | 0.129*** | 0.129*** |
| | (0.620) | (0.000) | (0.000) | (0.694) | (0.000) | (0.000) | (0.630) | (0.000) | (0.000) |
| Observations | 57,982 | 54,674 | 112,656 | 51,803 | 47,912 | 99,715 | 51,921 | 48,981 | 100,902 |
| | Panel D: all 2-year positions | | | Panel E: only full realizations | | | Panel F: only "mental" gain and loss | | |
| Subsample: | Algorithms | Humans | Both | Algorithms | Humans | Both | Algorithms | Humans | Both |
| Algorithm | | | -0.118** | | | -0.087*** | | | -0.140*** |
| | | | (0.030) | | | (0.004) | | | (0.001) |
| Constant | 0.011 | 0.129*** | 0.129*** | 0.007 | 0.093*** | 0.093*** | 0.003 | 0.143*** | 0.143*** |
| | (0.762) | (0.003) | (0.002) | (0.658) | (0.001) | (0.000) | (0.883) | (0.000) | (0.000) |
| Observations | 95,990 | 107,225 | 203,215 | 57,982 | 54,673 | 112,655 | 51,747 | 48,090 | 99,837 |

P-values in parentheses. Standard errors are clustered at trader's level

*** p<0.01, ** p<0.05, * p<0.1

## TABLE 3.3

## Aggressiveness of trades when realizing losses

Table 3.3 shows the results of regressing hourly calculated trader-level %ALRT-%ANRT spread on a constant and a dummy *Algorithm*, which is equal to one if a trader is an algorithm and zero if it is a human. When regressing the spread on a constant only, we split the sample into two groups – humans and algorithms. Standard errors are clustered at a trader level. We consider 31 algorithmic and 34 human proprietary traders that on average execute between 240 and 1,530 trades per day. %ALRT (proportion of aggressive loss realization turnover) is equal to a trader's hourly turnover that was executed when realizing losses (i.e. partially or fully closing losing positions) using market orders divided by the hourly turnover executed when realizing losses using both market and limit orders. %ANRT (proportion of aggressive non-realization turnover) is equal to a trader's hourly turnover that was executed when opening new or deepening existing positions using market orders divided by the hourly turnover executed when opening new or deepening existing positions using both market and limit orders. The table shows that algorithms trade virtually equally aggressively when realizing losses and when opening or deepening positions, while humans are more likely to use market orders when realizing losses than when opening or deepening positions.

| | Dependent variable: %ALRT-%ANRT spread | | |
|---|---|---|---|
| Subsample: | Algorithms | Humans | Both |
| Algorithm | | | -0.059*** |
| | | | (0.005) |
| Constant | -0.002 | 0.057*** | 0.057*** |
| | (0.826) | (0.004) | (0.003) |
| Observations | 40,530 | 28,110 | 68,640 |

P-values in parentheses. Standard errors are clustered at trader's level
*** p<0.01, ** p<0.05, * p<0.1

## TABLE 3.4

## Disposition effect sensitivity to the weather

Table 3.4 shows the results of regressing hourly (end of hour) trader-level observations of the spread between the proportion of gains realized (PGR) and the proportion of losses realized (PLR) on weather variables. The PGR-PLR spread is defined as in Table 3.1. In these regressions we consider only those observations where PGR-PLR spread is positive, i.e. we test if the disposition effect is sensitive to the weather provided that there is a disposition effect. The hourly trader-specific (depending on the city in which a trader is located) variable of interest is "sunshine duration" (minutes of sunshine during a given hour), and "sunshine dummy" which is equal to 1 if a variable is larger than its monthly average and zero otherwise. We also use three other similarly constructed weather dummy variables as controls: (1) temperature (in Celsius at the beginning of a given hour), (2) precipitation (milliliters of water per square meter of surface), and (4) air pressure (average hectopascal at sea level during a given hour). We use other weather controls in columns 3-6. In columns 1-3 we control for trader fixed effects and time (i.e. date-hour) fixed effects. In columns (4-6) we control for time fixed effects and trader x hour fixed effects in order to account for the possibility that the time of the day may be correlated with both the weather and traders' tiredness of some traders. Robust standard errors are unclustered in columns 1-4, clustered at a trader's level in column 5 and clustered at trader x date level in column 6. Panel A (B) considers 34 human (31 algorithmic) proprietary traders that on average execute between 240 and 1530 trades per day.

| | Dependent variable: PGR-PLR spread (percentage points) | | | | | |
|---|---|---|---|---|---|---|
| | Panel A: Humans | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| sunshine dummy | | 0.793** | 0.919** | 1.016*** | 1.016* | 1.016* |
| | | (0.038) | (0.019) | (0.009) | (0.078) | (0.053) |
| sunshine duration (minutes) | 0.015** | | | | | |
| | (0.044) | | | | | |
| Constant | 31.844*** | 31.849*** | 31.368*** | 31.453*** | 31.453*** | 31.453*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Temperature, precipitation and pressure controls | | | Yes | Yes | Yes | Yes |
| Trader fixed effects | Yes | Yes | Yes | | | |
| Time fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Trader x hour fixed effects | | | | Yes | Yes | Yes |
| Observations | 32,022 | 32,022 | 32,022 | 32,018 | 32,018 | 32,018 |
| Adjusted R-squared | 0.190 | 0.190 | 0.190 | 0.195 | 0.194 | 0.195 |
| | Panel B: Algorithms | | | | | |
| | (7) | (8) | (9) | (10) | (11) | (12) |
| sunshine dummy | | 0.274 | 0.299 | 0.125 | 0.125 | 0.125 |
| | | (0.676) | (0.656) | (0.852) | (0.882) | (0.886) |
| sunshine duration (minutes) | 0.005 | | | | | |
| | (0.661) | | | | | |
| Constant | 21.903*** | 21.914*** | 22.757*** | 22.961*** | 22.961*** | 22.961*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Temperature, precipitation and pressure controls | | | Yes | Yes | Yes | Yes |
| Trader fixed effects | Yes | Yes | Yes | | | |
| Time fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Trader x hour fixed effects | | | | Yes | Yes | Yes |
| Observations | 26,466 | 26,466 | 26,466 | 26,465 | 26,465 | 26,465 |
| Adjusted R-squared | 0.162 | 0.162 | 0.162 | 0.172 | 0.172 | 0.172 |

P-values in parentheses. Robust standard errors are unclustered in columns 1-4, clustered at trader's level in column 5 and clustered at trader x hour leve in column 6

*** p<0.01, ** p<0.05, * p<0.1

## TABLE 3.5

## Average profits and returns of frozen portfolios over the 8-hour period – case of loss realization

Table 3.5 shows the results of regressing (only on a constant) hourly trader-level observations of profits (Panels A and B) and returns (Panels C and D) over the following 8-hour period earned by frozen "Realization" (column 1), "Actual" (column 2) or "Combined" (column 3) portfolios. Panels A and C consider human traders and Panels B and D consider algorithms. Standard errors are clustered at a trader level. We consider 31 algorithmic and 34 human proprietary traders that on average execute between 240 and 1,530 trades per day. The frozen "Realization", "Actual" and "Combined" portfolios are constructed in the following way. Individual trader's "Actual portfolio" is constructed by assuming zero starting inventory every day and executing actual trades up to the moment of the freeze. The composition of the "Actual portfolio" is frozen at the end of every hour of a trading day. Individual trader's "Realization portfolio" is a hypothetical portfolio constructed by executing trades necessary to realize all existing paper **losses** at the moment of the freeze. Individual trader's "Combined portfolio" is a combination of both "Actual portfolio" and the "Realization portfolio", thus, it is a hypothetical portfolio that a trader would hold at the moment of the freeze had he just realized all paper **losses**. The gain of every portfolio is calculated by comparing stock prices at the moment of the freeze and eight trading hours later, holding the portfolios' compositions constant. The return of every portfolio is calculated by subtracting the portfolio value at stock prices prevailing at the time of the freeze from the portfolio value at stock prices prevailing 8 trading hours later (holding the portfolios' compositions constant), and dividing the difference by the former portfolio value.

| | Dependent variable: Portfolio profit over the 8-hour period (EUR) | | | | | |
|---|---|---|---|---|---|---|
| | Panel A: humans' profit | | | Panel B: algorithms' profit | | |
| Portfolio type | "Realization" | "Actual" | "Combined" | "Realization" | "Actual" | "Combined" |
| Constant | 420.577*** | -403.512** | 63.228 | 168.819*** | 133.729 | 271.410** |
| | (0.000) | (0.026) | (0.522) | (0.003) | (0.401) | (0.018) |
| Observations | 52,381 | 52,381 | 52,381 | 54,124 | 54,124 | 54,124 |

| | Dependent variable: Portfolio return over the 8-hour period (%) | | | | | |
|---|---|---|---|---|---|---|
| | Panel C: humans' return | | | Panel D: algorithms' return | | |
| Portfolio type | "Realization" | "Actual" | "Combined" | "Realization" | "Actual" | "Combined" |
| Constant | 0.141*** | -0.090** | 0.042 | 0.092** | 0.042 | 0.123*** |
| | (0.000) | (0.030) | (0.158) | (0.028) | (0.513) | (0.004) |
| Observations | 48,656 | 50,381 | 49,565 | 51,039 | 52,608 | 51,885 |

P-values in parentheses. Standard errors are clustered at trader's level

*** p<0.01, ** p<0.05, * p<0.1

TABLE 3.6

Average profits and returns of frozen portfolios over the 8-hour period – case of gain realization

Table 3.6 shows the results of regressing (only on a constant) hourly trader-level observations of profits (Panels A and B) and returns (Panels C and D) over the following 8-hour period earned by frozen "Realization" (column 1), "Actual" (column 2) or "Combined" (column 3) portfolios. Panels A and C consider human traders and Panels B and D consider algorithms. Standard errors are clustered at a trader level. We consider 31 algorithmic and 34 human proprietary traders that on average execute between 240 and 1,530 trades per day. The frozen "Realization", "Actual" and "Combined" portfolios are constructed in the following way. Individual trader's "Actual portfolio" is constructed by assuming zero starting inventory every day and executing actual trades up to the moment of the freeze. The composition of the "Actual portfolio" is frozen at the end of every hour of a trading day. Individual trader's "Realization portfolio" is a hypothetical portfolio constructed by executing trades necessary to realize all existing paper **gains** at the moment of the freeze. Individual trader's "Combined portfolio" is a combination of both "Actual portfolio" and the "Realization portfolio", thus, it is a hypothetical portfolio that a trader would hold at the moment of the freeze had he just realized all paper **gains**. The gain of every portfolio is calculated by comparing stock prices at the moment of the freeze and eight trading hours later, holding the portfolios' compositions constant. The return of every portfolio is calculated by subtracting the portfolio value at stock prices prevailing at the time of the freeze from the portfolio value at stock prices prevailing 8 trading hours later (holding the portfolios' compositions constant), and dividing the difference by the former portfolio value.

| | Dependent variable: Portfolio profit over the 8-hour period (EUR) | | | | | |
|---|---|---|---|---|---|---|
| | Panel A: humans' profit | | | Panel B: algorithms' profit | | |
| Portfolio type | "Realization" | "Actual" | "Combined" | "Realization" | "Actual" | "Combined" |
| Constant | -173.020** | -403.512** | -518.472*** | -283.064*** | 133.729 | -162.156** |
| | (0.012) | (0.026) | (0.000) | (0.008) | (0.401) | (0.018) |
| Observations | 52,381 | 52,381 | 52,381 | 54,124 | 54,124 | 54,124 |

| | Dependent variable: Portfolio return over the 8-hour period (%) | | | | | |
|---|---|---|---|---|---|---|
| | Panel C: humans' return | | | Panel D: algorithms' return | | |
| Portfolio type | "Realization" | "Actual" | "Combined" | "Realization" | "Actual" | "Combined" |
| Constant | -0.063*** | -0.090** | -0.150*** | -0.120*** | 0.042 | -0.071 |
| | (0.005) | (0.030) | (0.000) | (0.003) | (0.513) | (0.105) |
| Observations | 48,890 | 50,381 | 49,576 | 51,299 | 52,608 | 51,697 |

P-values in parentheses. Standard errors are clustered at trader's level
*** p<0.01, ** p<0.05, * p<0.1

# Appendix of Chapter 3

TABLE 3.2 (Trading frequencies 48-1530)

Realization of gains and losses

Table 3.2 (Trading frequencies 48-1530) shows the results of Table 3.2 but using a different subsample – those 63 algorithmic and 170 human traders that on average executed between 48 and 1,530 trades per day throughout our two-year sample period.

This table shows the results of regressing hourly (end of hour) trader-level observations of the spread between the proportion of gains realized (PGR) and the proportion of losses realized (PLR) on a constant and a dummy *Algorithm*, which is equal to one if a trader is an algorithm and zero if it is a human. When regressing the spread on a constant only, we split the sample into two groups – humans and algorithms. Standard errors are clustered at a trader level. We consider 63 algorithmic and 170 human proprietary traders that on average execute between 48 and 1,530 trades per day. Individual PGR and PLR for every trader at the end of every hour are calculated as follows. In Panels A, B, C, E and F traders are assumed to start every day with zero inventory and by trading to build their long and short positions in stocks throughout a day. In Panel D, traders are assumed to start the first trading day with zero inventory and to accumulate inventory throughout the full two-year sample period. For every trader-stock position at every point of time we calculate *total gain*, which consist of *cumulative realized gain* and *outstanding paper gain*. *Outstanding paper gain* is calculated by multiplying remaining inventory by the difference between the last observed stock price and the volume-weighted average purchase price (WAPP). *Realized gain* is calculated by multiplying the number of shares sold (or repurchased, in case of short positions) by the difference between the selling (repurchasing) price and WAPP. *Cumulative realized gain* is calculated by accumulating *realized gains* over time. At any point of time, a trader's PGR (PLR) equals *cumulative realized gains* above (below) zero summed up across trader-stock positions divided by *total gains* above (below) zero summed up across trader-stock positions. The dependent variable is the difference between PGR and PLR. Panels A and D consider both long and short trader-stock positions, while Panels B and C consider only long and short positions, respectively. Panel E is similar to Panel A, but considers only those realizations of gains and losses that fully closed positions, i.e. it ignores those stock sales (or repurchases, in case of short positions) which realized only part of a gain or a loss. Panel F considers only those trader-stock positions, which are either long throughout the whole day from the 2-year perspective and short from the daily perspective or short throughout the whole day from the 2-year perspective and long from the daily perspective.

Dependent variable: PGR-PLR spread

| | Panel A: all daily positions | | | Panel B: long daily positions | | | Panel C: short daily positions | | |
|---|---|---|---|---|---|---|---|---|---|
| Subsample: | Algorithms | Humans | Both | Algorithms | Humans | Both | Algorithms | Humans | Both |
| Algorithm | | | -0.053** | | | -0.082*** | | | -0.067** |
| | | | (0.022) | | | (0.008) | | | (0.020) |
| Constant | 0.019 | 0.072*** | 0.072*** | 0.021 | 0.103*** | 0.103*** | 0.022 | 0.088*** | 0.088*** |
| | (0.241) | (0.000) | (0.000) | (0.325) | (0.000) | (0.000) | (0.291) | (0.000) | (0.000) |
| Observations | 75,959 | 113,123 | 189,082 | 64,021 | 85,808 | 149,829 | 63,651 | 85,871 | 149,522 |

| | Panel D: all 2-year positions | | | Panel E: only full realizations | | | Panel F: only "mental" gain and loss | | |
|---|---|---|---|---|---|---|---|---|---|
| Subsample: | Algorithms | Humans | Both | Algorithms | Humans | Both | Algorithms | Humans | Both |
| Algorithm | | | -0.039 | | | -0.041** | | | -0.081** |
| | | | (0.286) | | | (0.035) | | | (0.010) |
| Constant | 0.008 | 0.048** | 0.048** | 0.015 | 0.056*** | 0.056*** | 0.017 | 0.098*** | 0.098*** |
| | (0.788) | (0.024) | (0.023) | (0.276) | (0.000) | (0.000) | (0.431) | (0.000) | (0.000) |
| Observations | 143,214 | 472,388 | 615,602 | 75,939 | 113,129 | 189,068 | 63,171 | 83,326 | 146,497 |

P-values in parentheses. Standard errors are clustered at trader's level

*** p<0.01, ** p<0.05, * p<0.1

## TABLE 3.2 (Trading frequencies 480-1530)
## Realization of gains and losses

Table 3.2 (Trading frequencies 480-1530) shows the results of Table 3.2 but using a different subsample – those 21 algorithmic and 13 human traders that on average executed between 480 and 1,530 trades per day throughout our two-year sample period.

This table shows the results of regressing hourly (end of hour) trader-level observations of the spread between the proportion of gains realized (PGR) and the proportion of losses realized (PLR) on a constant and a dummy *Algorithm*, which is equal to one if a trader is an algorithm and zero if it is a human. When regressing the spread on a constant only, we split the sample into two groups – humans and algorithms. Standard errors are clustered at a trader level. We consider 21 algorithmic and 13 human proprietary traders that on average execute between 480 and 1,530 trades per day. Individual PGR and PLR for every trader at the end of every hour are calculated as follows. In Panels A, B, C, E and F traders are assumed to start every day with zero inventory and by trading to build their long and short positions in stocks throughout a day. In Panel D, traders are assumed to start the first trading day with zero inventory and to accumulate inventory throughout the full two-year sample period. For every trader-stock position at every point of time we calculate *total gain*, which consist of *cumulative realized gain* and *outstanding paper gain*. *Outstanding paper gain* is calculated by multiplying remaining inventory by the difference between the last observed stock price and the volume-weighted average purchase price (WAPP). *Realized gain* is calculated by multiplying the number of shares sold (or repurchased, in case of short positions) by the difference between the selling (repurchasing) price and WAPP. *Cumulative realized gain* is calculated by accumulating *realized gains* over time. At any point of time, a trader's PGR (PLR) equals *cumulative realized gains* above (below) zero summed up across trader-stock positions divided by *total gains* above (below) zero summed up across trader-stock positions. The dependent variable is the difference between PGR and PLR. Panels A and D consider both long and short trader-stock positions, while Panels B and C consider only long and short positions, respectively. Panel E is similar to Panel A, but considers only those realizations of gains and losses that fully closed positions, i.e. it ignores those stock sales (or repurchases, in case of short positions) which realized only part of a gain or a loss. Panel F considers only those trader-stock positions, which are either long throughout the whole day from the 2-year perspective and short from the daily perspective or short throughout the whole day from the 2-year perspective and long from the daily perspective.

| | Dependent variable: PGR-PLR spread | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Panel A: all daily positions | | | Panel B: long daily positions | | | Panel C: short daily positions | | |
| Subsample: | Algorithms | Humans | Both | Algorithms | Humans | Both | Algorithms | Humans | Both |
| Algorithm | | | -0.111** | | | -0.138** | | | -0.117** |
| | | | (0.020) | | | (0.018) | | | (0.020) |
| Constant | -0.010 | 0.101** | 0.101** | -0.014 | 0.124** | 0.124** | -0.013 | 0.105** | 0.105** |
| | (0.536) | (0.038) | (0.024) | (0.524) | (0.034) | (0.021) | (0.541) | (0.035) | (0.022) |
| Observations | 47,077 | 30,657 | 77,734 | 44,212 | 27,781 | 71,993 | 44,157 | 28,308 | 72,465 |
| | Panel D: all 2-year positions | | | Panel E: only full realizations | | | Panel F: only "mental" gain and loss | | |
| Subsample: | Algorithms | Humans | Both | Algorithms | Humans | Both | Algorithms | Humans | Both |
| Algorithm | | | -0.166** | | | -0.092** | | | -0.138** |
| | | | (0.040) | | | (0.025) | | | (0.015) |
| Constant | -0.028 | 0.138* | 0.138** | -0.010 | 0.082** | 0.082** | -0.014 | 0.124** | 0.124** |
| | (0.472) | (0.067) | (0.050) | (0.448) | (0.049) | (0.034) | (0.519) | (0.030) | (0.017) |
| Observations | 65,949 | 50,373 | 116,322 | 47,078 | 30,654 | 77,732 | 44,343 | 27,952 | 72,295 |

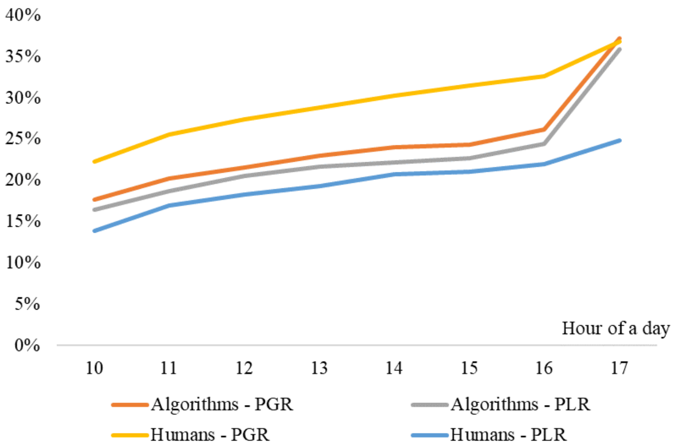
P-values in parentheses. Standard errors are clustered at trader's level
*** p<0.01, ** p<0.05, * p<0.1

FIGURE 3.2.A (FIFO method)

Realization of gains and losses throughout a day – default setting

Figure 3.2.A (FIFO method) shows the same result as Figure 3.2.A but using a first-in-first-out (FIFO) method instead of weighted average purchase price (WAPP) in order to determine the reference purchase (selling, in case of short positions) stock price.

The figure shows the proportion of gains realized (PGR) and the proportion of losses realized (PLR) at the start of every hour of a day, averaged across trading days and across traders in the two groups, i.e. humans and algorithms. The graph considers 31 algorithmic and 34 human proprietary traders that on average execute between 240 and 1,530 trades per day. Individual PGR and PLR for every trader are calculated as follows. Traders are assumed to start every day with zero inventory (at 9 am) and by trading to build their long and short positions in stocks throughout a day. For every trader-stock position at every point of time we calculate *total gain*, which consist of *cumulative realized gain* and *outstanding paper gain*. *Outstanding paper gain* is calculated by multiplying remaining inventory by the difference between the last observed stock price and the original purchase (selling, in case of short positions) price of each stock using the first-in-first-out (FIFO) method. *Realized gain* is calculated by multiplying the number of shares sold (or repurchased, in case of short positions) by the difference between the selling (repurchasing) price and the original purchase (selling, in case of short positions) price of each stock using the first-in-first-out (FIFO) method. *Cumulative realized gain* is calculated by accumulating *realized gains* throughout a day. At any point of time, a trader's PGR (PLR) equals *cumulative realized gains* above (below) zero summed up across trader-stock positions divided by *total gains* above (below) zero summed up across trader-stock positions.



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