The Impact of Bank Switching Costs: Evidence from a Regulatory Reform*

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Abstract

We utilize the universe of consumer bank accounts in Israel and a unique nationwide digital reform that substantially reduced the time and effort required for some customers to transfer their financial activity between banks. Employing a difference-in-differences methodology, we find that this reduction in switching costs led to a significant increase in customer mobility: affected customers arer more than twice more likely to switch banks. The effect is persistent and cannot be attributed to temporary increases in customer attention. These findings provide new evidence on switching frictions in retail banking and highlight how digital transformation can reshape customer behavior, with important implications for competition, financial stability, and bank business models.

Keywords: bank switching, switching costs, regulatory reform, digital banking, deposits
JEL Classification Codes: G21, G28, G51

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1 Introduction

Customer mobility in the banking industry is relatively low. A 2022 survey by the UK's Financial Conduct Authority (FCA) found that only 6 percent of current account holders had switched providers in the previous three years, while approximately two-thirds had maintained the same account for over a decade. This represented the lowest switching rate among the ten retail financial products examined in the survey (Financial Conduct Authority 2023). Similarly, a 2019/20 European survey revealed that just 7 percent of respondents had changed their bank account provider in the previous two years (European Comission 2021). This was again the lowest switching rate among all seven services surveyed.[1]

The low frequency of bank switching may be attributed to incumbent banks offering better conditions to existing customers, reducing incentives to change institutions. This advantage might be from banks' ability to gather soft information about customers' creditworthiness and financial capacity through ongoing relationships.[2] Yet evidence suggests that customers remain with their existing banks even when better terms are available elsewhere. For example, reports by the FCA conclude that "[m]any customers have been with their Primary bank account provider for many years despite better deals being available. Many customers...receive little or no interest on balances and pay high overdraft charges" (Financial Conduct Authority 2018), and that "[l]evels of searching and switching on current accounts have been low, despite potential consumer gains on price or quality" (Financial Conduct Authority 2022).

A possible barrier to bank switching may be the time and effort required to transition to a new institution, which can deter customers from changing banks even when dissatisfied with their current provider. In the US, for example, switching banks requires customers to manually go over all their automatic payments and deposits, and update each and every counterparty with the details of the new bank account. This includes notifying employers, government agencies (for social benefit

[1] The additional six services were insurance, gas, electricity, mobile phone, internet provision, and TV subscriptions. The equivalent fraction of switchers for these services ranged between 13 and 10 percent.

[2] Note, however, that this information advantage may allow the relationship bank to extract information rents from "locked-in" customers (Sharpe 1990; Rajan 1992; Allen et al. 2019). Degryse and Ongena (2008) refer to these relationship banking rents as "informational switching costs."

recipients), and various service providers such as utilities, insurance companies, healthcare providers, and credit card issuers.[3] The literature refers to these transition-related efforts as "transaction switching costs" (Klemperer 1995). However, low switching rates might alternatively be explained by customer inattention and lack of awareness regarding the benefits of switching. In this case, reducing transaction costs alone would not significantly affect switching behavior.

This paper examines how transaction switching costs influence bank customers' decisions to change institutions. We analyze a regulatory change in the Israeli banking sector – the "Banking Mobility Reform" (BMR) – to investigate how reduced switching costs affect customer behavior. Implemented in September 2021, the BMR's key innovation was the introduction of a free "Online Rapid Transfer System" that enables customers to easily transfer their financial activities between banks, with automatic transaction routing. Importantly, while the system was technically available to all bank accounts, during its first year of operation it was effectively less accessible to accounts with existing consumer loan balances (see Section 2 for details). Using a difference-in-difference (DiD) analysis of the universe of bank accounts in Israel, we demonstrate that following the reform, switching rates increased significantly among customers who could access the online system.

The theoretical literature suggests that switching costs have important consequences for market structure and business models of firms. Switching costs give firms a degree of market power, especially in service markets, where customers have ongoing relationships and repeated transactions with firms. A firm's current market share becomes a crucial determinant of its future profits, substantially influencing its competitive behavior and pricing strategies. Klemperer (1995) provides a comprehensive review of the theoretical literature on switching costs. In the context of financial services, switching costs can lead to higher credit interest rates, lower deposit ratess, and elevated fees.[4] Switching costs may enable banks to maintain low deposit rates even as policy rates rise, potentially affecting monetary policy transmission and bank balance sheet structures (Drechsler

[3] Numerous online guides detail these requirements. See, for example, a guide from USNews, updated to July 26, 2023: https://www.usnews.com/banking/articles/how-to-switch-banks-a-step-by-step-guide.

[4] Ausubel (1991) provides early evidence that credit card rates are higher due to switching barriers. Sharpe (1997) provides evidence that switching costs allow banks pay low interest rates on their deposits. Agarwal et al. (2014) find that that regulatory limits on credit card fees reduced overall borrowing costs without increasing interest charges or reducing the volume of credit.

et al. 2017; Drechsler et al. 2021; Segev et al. 2024).

Despite their significance, the nature of switching costs and their influence on customer switching behavior in banking remains poorly understood. Two key challenges have hindered empirical research: limited access to granular data on household bank switching patterns and the complexity of isolating how different factors affect bank switching decisions. Multiple elements can influence a customer's decision to switch banks, including transaction costs (such as time investment), search frictions (like difficulty assessing potential benefits), brand loyalty, existing banking relationships, and minimal perceived advantages of changing institutions. Consequently, previous research on bank switching has largely relied on either policy-focused studies using self-reported survey data or broad interpretations of ”switching costs” as all friction points deterring customers from changing banks (see more details on the literature).

Our unique empirical setup overcomes both these challenges. First, we utilize a unique database that includes all consumer bank accounts in Israel. To our knowledge, we are the first to use such an extensive database to estimate switching behavior. Second, since the BMR regulation was implemented specifically to reduce transaction switching barriers, we can identify the specific effect of these barriers on consumer switching. This can also allow us to examine how the digitalization of banking affects customers' mobility within this market. A critical component of our identification strategy is that accounts with an existing consumer debt balance could not use the online bank transfer platform. We use this population as a control group in our difference-in-difference setting to rule out time trends and establish a causal effect of the reform. To reduce further concerns about the endogeneity of consumer debt, we conduct a within-household specification in the spirit of Khwaja and Mian (2008) identification scheme. We also provide as large array of additional robustness tests to rule out alternative explanations and the possibility that our results are due to omitted variables.

We find that, prior to the reform, the three-year switching probability among Israeli customers was 1.6%. This figure is lower than the survey-based estimates presented above and may reflect either the more concentrated structure of the banking industry in a small country such as Israel, or a more precise estimation using account-level data. Our estimates indicate that the reform increased the three-year switching probability to 3.7%, corresponding to a 135% increase. These results are robust across a wide range of specifications and underscore the role of transaction costs as a key barrier to bank switching.

The mobility reform received extensive media coverage and was widely advertised in Israel. This raises an alternative explanation for the observed increase in bank transfers around the reform date: rather than switching costs, lack of awareness may have been the main barrier, with customers passively accepting their checking account provider as given and not actively considering alternatives. Under this view, the rise in transfers after the reform reflects heightened consumer attention generated by media coverage, rather than a reduction in switching costs.

To distinguish between the switching-cost and attention-based explanations, we conduct a dynamic analysis of monthly switching activity in the year before and after the reform. The results reveal a steady increase in switching during the first six months, followed by a stable, elevated switching probability. This pattern is consistent with a "soft launch" of the reform, followed by a persistent change in switching behavior, rather than a short-lived increase in attention.

After confirming our findings through a broad set of robustness tests, we present several additional results. First, we show that account consolidation does not mirror the pattern of account switching: there is a one-time increase in account closures as customers concentrate their banking activity in fewer accounts, but this effect is short-lived. Second, we examine heterogeneity in switching behavior. We find that younger customers more than double their switching probability relative to older ones. By contrast, we detect no effect of socioeconomic status, though this may reflect noise in our proxy, which is based on municipality-level indicators.

Finally, the reform may have benefited not only customers who actually switched banks, but also those who were able to secure better terms from their existing bank due to a credible threat of switching. Our data is limited to credit activity, and we do not observe account fees or deposit rates and conditions. As a result, we cannot provide a comprehensive assessment of the reform's overall welfare effects. Instead, we focus on credit-related outcomes, such as the size and cost of credit lines. Using the same difference-in-differences approach as before, we estimate the causal impact of the reform on these variables. While not all credit outcomes are affected, we find a significant reduction in overdraft interest rates alongside an increase in overdraft utilization. Since overdrafts are the most common form of short-term credit in Israel (with credit cards typically offering only up to 45 days of credit), and about 40% of Israeli accounts being in overdraft at any given time, this effect represents a substantial benefit for consumers.

The paper contributes to several strands of the literature. First, it contributes to the literature that tries to identify and measure switching costs. The term "consumer switching costs" is a general name for many reasons that prevent consumers from switching between competitors, and includes many possible barriers for switching between competitors or reasons for maintaining "brand loyalty." Among these costs are transaction costs of switching service providers, loss of consumer reputation, costs of learning to work with a new service provider, uncertainty about the quality of a new and untested provider, as well as psychological costs such as "cognitive dissonance" or costs of attention (for a detailed general discussion on switching costs, see (Klemperer 1995) Section 2). Previous empirical studies are not able to distinguish between different costs and therefore use firm-level or aggregate data to measure the their total amount (see, for example, Greenstein (1993), Kim et al. (2003), and Shy (2002)). We, in contrast, are able to identify and measure the effect of a specific type, transaction switching costs.

In the context of banking, previous research has investigated the impact of switching costs and the determinants of bank switching by retail consumers mostly by focusing on survey data and by examining how switching behavior is affected by invetors' properties (Kiser 2002; Calem et al. 2006; Zhao et al. 2013; Brunetti et al. 2016; van der Cruijsen and Diepstraten 2017; Diepstraten and van der Cruijsen 2019; Brunetti et al. 2020; Gerritsen and Bikker 2020). We add to this literature by exploiting a quasi-experiment and actual switching data that allows us to provide causal identification.

In this literature, the paper most closely related to ours is Brunetti et al. (2020), which utilize a legal reform in Italy that reduced mortgage switching costs to investigate the role of transaction switching costs on bank switching behavior. We use a similar identification scheme but differ from their paper in a number of ways. First, while Brunetti et al. (2020) focus on a specific product, this paper presents evidence about switching the entire array of financial products between banks.

Second, Brunetti et al. (2020) use self-reported survey data, while we have access to the universe of bank accounts. To our knowledge, this paper is the first to use account-level data to investigate retail consumer bank switching behavior empirically.

The present paper is also related to the large literature that deals with the determinants of banking relationship (Ongena and Smith 2001; Farinha and Santos 2002; Gopalan et al. 2011) and specifically how relationship banking impacts bank switching (Chakravarty et al. 2004; Degryse et al. 2011; Brown et al. 2020). We show that improving customers' ability to switch bank accounts may impact both the duration and the number of customer-bank relationships. Additionally, while the vast majority of studies on relationship banking focus on firm-bank relationships, in this study, we examine retail consumers.

Finally, while the the online bank transfer in Israel was inacted by the regulator, new branchless digital banks and "neobanks" are disrupting the banking system and offer a complete online experience. The present paper is therefore relevant for examining the effect of bank digitalization on switching behavior and competitiveness in general. For example, Koont et al. (2024) suggest that digitization in the banking sector, by making depositors less sticky, reduces banks' deposit franchise. We provide additional empirical support to the hypothesis that digitization can impact banks competitiveness by showing that transaction switching costs matter, and that online system significantly impacts retail consumer bank switching behavior.

The rest of the paper is arranged as follows: Section 2 provides institutional background on the "Banking Mobility Reform." Section 3 describes the data and the empirical methodology. Section 4 presents the results, and Section 5 delves into robustness checks. Finally, Section 7 provides suggestive implications of the reform on the overall customer conditions in the Israeli banking system and Section 8 concludes.

2 Institutional Background: the Banking Mobility Reform

In March 2018, the Israeli parliament passed an amendment to the Banking (Service to the Customer) Law, requiring the establishment of a secure and convenient online platform that enables customers to transfer their financial activities between institutions. On September 22, 2021, the Bank of Israel and the Ministry of Finance implemented a new online system allowing customers to switch banks at no cost. Through this platform, customers could initiate transfers by submitting requests directly to their preferred bank online, eliminating the need for branch visits. The receiving bank would then manage the entire transfer process, completing it within seven business days of receiving a valid request.

The system automatically transfers all major financial activities, including: credit or debit account balances in both shekels and foreign currencies, authorized debits, checks, Israeli and foreign securities, payment card activities, and standing orders. Additionally, it features a "follow me" routing mechanism that automatically redirects any incoming transactions from the old account to the new account following the transfer date.

Importantly, the new banking directive allows the original bank to reject transfer requests *only* when account holders have existing unsecured debt balances. While the directive encourages the original bank to facilitate loan repayment through automatic debits from the customer's new account, it also states that "If the original bank decides not to allow continued repayment of the loan in this manner and has reached no other agreement with the customer on how to handle the loan, or if the customer has given no instruction, *the bank-switching process shall be halted* [emphasis added]."[5]

An examination of the reform's first year by the Bank of Israel's Supervision Department revealed that existing debt was indeed a significant barrier to using the online transfer system. Of approximately 18,000 transfer requests cancelled by banks through September 30, 2022, about 38 percent were rejected due to unpaid debts at the original bank. In response to these findings, the Supervision Department directed banks to review their handling of transfer requests involving existing loans and to explore options that would allow transfers to proceed despite outstanding credit (Bank of Israel 2022). We have anecdotal evidence that this direction indeed improved the use of the online system by customers with outstanding credit, and therefore we use our identification method (as described below) only for the reform's first year of operations.

[5] See Bank of Israel Proper Conduct of Banking Business directive 448 (September 2021) page 15.

3 Methodology and Data

3.1 Data and sample construction

Our main data source is the Israeli Consumer Credit Register, established in 2016 as part of the "Credit Data Law". The Bank of Israel maintains the credit registry, and it includes all consumer credit data for the entire population of borrowers in Israel. Specifically, all banks and credit card companies are required to report all their new and outstanding credit data on a monthly basis.[6]

For our empirical estimation, we use a panel data set of all bank-consumer deposit (checking/transaction) accounts in Israel. Banks are required to report information on a monthly basis on every checking account in one of two cases: (i) if the account has an overdraft credit line (with or without any draw-downs). Overdraft debt is a credit line that banks grant their clients on their checking accounts from which they can withdraw funds up to some limit. Overdraft is a very popular method of rolling over debt by households in Israel. Banks will offer most of their clients a credit line option and around 40% of Israel's households have an overdraft at least once a year. (ii) Even if an account does not have a credit line available, it will still be reported to the credit registry if there was a physical check payment from the account and/or any other form of payment order (e.g., payment with a credit card). Since the vast majority of checking accounts in Israel have either an available credit line or payment order activity, the credit registry covers the vast majority (around 80%) of retail bank checking accounts.

Our data set is at the account-month level. We assign a unique customer ID (identity) For each individual or multiple individuals who share the same bank account. The unique customer ID is the same across all bank accounts and periods, so we can identify for each customer ID all bank accounts and any bank switching behavior.[7] For most empirical estimations, we focus on the year

[6] the Bank of Israel gathers and holds all the credit data used to compute Israelis credit scores ("credit register"). This data is then transmitted to private credit bureaus, created following the law, which compute the credit scores based on such information on a case-by-case basis. The Bank of Israel provides a website where each consumer can obtain their credit history. This data, alongside additional information regarding the Israeli Credit Data Register, are available at https://www.creditdata.org.il/en.

[7] We provide additional details on the method of assigning a unique borrower ID for each bank account. If a single person has bank accounts where she is the only account holder while also sharing another bank account, these accounts will receive different borrower IDs. For example, if borrower A has an account in Bank 1 and an additional account with Bank 2 where she is the sole account holder, while also sharing an account with person B in Bank 2 the

before and after the implementation of the bank mobility reform, i.e., the period of October 2020 through September 2022. We drop accounts where the account holders changed during that period and limit to only identities with at least one checking account throughout the entire sample period. The full data set includes information on over 103 million account-month observations belonging to 4.5 million individuals. Because of the size of the data, we run most of the estimations on a random sample of 10% of the customer ID from the full data.[8] We define an account switching when an account closes within a month of the the same customer ID opening a new checking account, at a different bank.

Our identification scheme builds on the effective restriction on customers with an outstanding consumer loan that were not able to use the online transfer system to automatically close and transfer their account products to a different bank. Specifically, we define an account as "treated" in months the account customers do not have an outstanding consumer loan with the same bank and "control" otherwise.

Table 1 presents descriptive statistics of our random sample. Panel A is at the account-month level, showing statistics for the 10% random sample and splitting between treated and control accounts. *Switch* is the share of accounts that moved to a different bank in every month. *Credit Lim* shows the share of accounts with an existing overdraft credit line available and the amount of the credit line from the accounts with such an option. *Overdraft* is the share of accounts with a credit line overdraft utilization at the end of the month and the size of the outstanding balance. *Credit Card* is the share of accounts where the customer also had a credit card with the bank and the end of the month credit card balance.[9] *Payment Orders* are the number of payment orders and physical checks presented during the month. For the control accounts (accounts where the customer has an outstanding loan in that bank), the table also presents the number of consumer loans and

identification will work as follows: the first two accounts will be assigned the borrower ID A while the last account will be assigned a different ID composed of both account holders, A-B. In Section 5.3 we provide more details on the reason for this identification and provide robustness tests showing that the results do not depend on this specific method. Note that from this point, we refer to the unique ID as the "customer ID" or "identity," though it may actually represent more than one person.

[8]In a sensitivity analysis in Section 5, we show the main results are almost identical when using the full database.

[9]Credit cards in Israel are available either directly from banks where one holds an account or from three credit card companies. The variable in Table 1 refers only to the banks credit cards directly linked to the specific account.

the loan's current balance.

In Panel B, we present statistics at the customer ID level[10]. *Socio* is the socio-economic indicator based on the customers municipality (home address). The Israeli Central Bureau of Statistics provides a socioeconomic index ranging from 1 to 10 for each local council or municipality, where 1 represents the lowest socioeconomic ranking and 10 the highest *Age* is reported in 14 groups, where higher number implies an older age[11]. For each account we assign the lowest socioeconomic indicator value and age group from the account owners. The panel also presents the average and median number of individuals in each customer ID, and the number of bank accounts that they have on average in every month.

Table 1: Descriptive statistics

	All			Control			Treated		
	Mean	St. Dev	Median	Mean	St. Dev	Median	Mean	St. Dev	Median
Panel A: Account Characteristics									
Switch (%)	0.050	2.239	0	0.007	0.848	0	0.072	2.679	0
Accounts with Credit Lim (%)	65.59	47.51	100	82.63	37.88	100	56.99	49.51	100
Credit Lim (Thousand NIS)	10.07	13.68	5	14.67	14.92	10	7.75	12.39	2
Accounts in Overdraft (%)	23.87	42.63	0	45.14	49.76	0	13.14	33.78	0
Overdraft (Thousand NIS)	3.04	8.64	0	5.89	10.52	0.01	1.60	7.09	0
Accounts with Credit Card (%)	70.23	45.72	100	76.23	42.56	100	67.19	46.95	100
Credit Card Balance (Thousand NIS)	4.54	7.15	1.64	6.36	8.47	3.58	3.62	6.18	0.90
Payment Orders	3.21	3.57	2	4.05	4.19	3	2.79	3.13	2
Number of Consumer Loans	0.54	1.06	0	1.61	1.27	1	0	0	0
Comsumer Loans Current Balance (Thousand NIS)	23.56	76.00	0	70.28	118.08	45.50	0	0	0
Account-month Observations		17,556,909			5,886,629			11,670,280	
Unique Accounts		795,893			327,600			608,783	
Panel B: Customer ID Characteristics									
Number of Bank Accounts	1.21	0.46	1	1.24	0.50	1	1.20	0.45	1
Socio	5.58	2.19	6	5.28	2.17	5	5.73	2.19	6
Age Group	7.56	3.26	7	7.13	2.75	7	7.77	3.47	7
Number of People on the Account	1.33	0.49	1	1.33	0.47	1	1.34	0.50	1
Unique Customer ID		671,746			296,663			535,233	

Notes: This table presents the descriptive statistics of a random sample composed of 10% of Customer ID used in the empirical estimation. All observations are at the bank account month level in Panel A and the Customer ID level in Panel B. The Sample period is October 2020 to September 2022. See Section 3.1 for details on the sample construction and the variables. Mean, standard deviation and median are presented for each variable.

[10] Note that a customer ID can be part of the control group in some months (when it has a positive consumer loan balance) and part of the the treatment group in other months (when it does not have such balance). Thus, the total number of unique customers IDs is smaller than the sum of the treated and control customer IDs. In Subsection 5.4 we show that our results hold when we only include customer IDs appear in either the treatment or control groups throughout the sample.

[11] Ages 0-21 are coded as 1; ages 22-24 are coded as 2; ages 25-29 are coded as 3; ages 30-34 are coded as 4; ages 35-39 are coded as 5; ages 40-44 are coded as 6; ages 45-49 are coded as 7; ages 50-54 are coded as 8; ages 55-59 are coded as 9; ages 60-64 are coded as 10; ages 65-69 are coded as 11; ages 70-74 are coded as 12; ages 75-79 are coded as 13; and ages above 79 are coded as 14

Table 1 reveals several notable differences between treated and control accounts. On average, treated accounts exhibit a lower likelihood of having an overdraft (34% compared to 50% for control accounts) and smaller average overdraft balances. Similarly, treated accounts have a lower credit card balance, slightly lower access to a credit limit (57% vs. 83%) and smaller average credit limits (NIS 7.75k vs. NIS 14.67k). The number of payment orders and checks per month is also lower for treated accounts (2.8 vs. 4.1). At the customer level, treated customers tend to reside in municipalities with slightly higher socioeconomic indices (mean of 5.73 vs. 5.28 for control customers) and have a slightly higher average age group (7.77 vs. 7.13). These patterns highlight meaningful differences in financial behavior and demographic characteristics between treated and control accounts. Finally, the share of switching accounts over the entire sample period is 10 times higher for treated accounts compared to control accounts (0.07% vs. 0.007%). The key assumption of our identification strategy is that the differences between the groups would have remained constant in the absence of the reform.

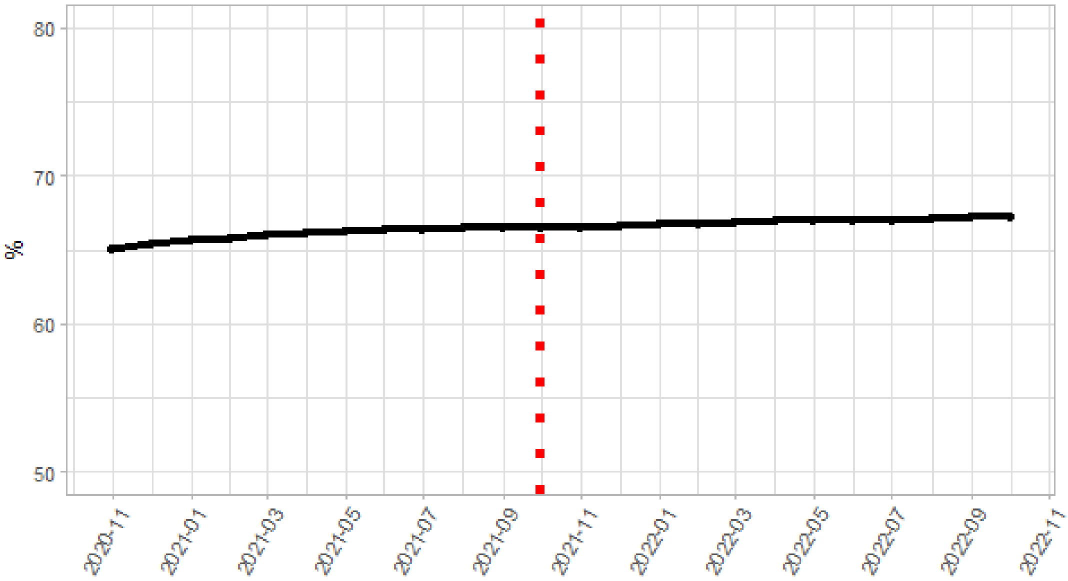
Figure 1 presents the share of *treated* accounts in every month from the full database. As we can see around 67% of bank accounts do not have an existing consumer loan balance with very strong persistence over the sample period suggesting the reform did not change customers debt balances. That is we can infer that the existence of the debt balance in every account was exogenous to the implementation of the reform.

3.2 Empirical specification

We estimate the effects of the Banking Mobility Reform using a difference-in-differences (DiD) research design, comparing accounts without outstanding debt (treatment group) and bank accounts with an outstanding loan (control group). Specifically, we estimate the following linear probability panel regression estimation:

$$Switch_{i,j,k,t} = \alpha_j + \delta_k + \gamma_t + \psi treat_{i,t} + \beta Post_t \times treat_{i,t} + \theta X_{i,t-1} + \epsilon_{i,j,k,t}, \tag{1}$$

Figure 1: Share of treated accounts



Note: This figure reports the share of treated accounts, i.e. accounts where the customers do not have an existing customer loan with the same bank, from October 2020 to September 2022. Dashed red line is in September 2021, the month the "Banking Mobility Reform" (BMR) was implemented.

where *Switch* is binary variable taking the value one if account i of customer j at bank k was closed in month t and additionally customer j opened a new account in a different bank during the same month or up to one month before or after[12]. *Post* is a dummy variable that takes the value of one for every month after September 2021. α_j, δ_k and γ_t are customer, bank, and month fixed effects, respectively. *treat* is a dummy equal to one if the account holders do not have an outstanding consumer loan balance in that bank. X is a set of account level controls that may also impact customers' tendency to close the account, which include existing credit line, overdraft balance, credit card balances with the bank and the number of payment orders and checks presented during the month.

Following much of the literature, we further examine the dynamic relationship between the *treat* the account switching statues by replacing *Post* with a series of dummy variables $\{D_{-12}, \ldots, D_{12}\}$ which indicate 12 months before to 12 months after the beginning of the interest rate increase,

[12]In Section 5.5 we show that our results hold even when the switching period is allowed to be 2 or 3 months.

omitting September 2021 which is used as the base month.

$$Switch_{i,j,k,t} = \alpha_j + \delta_k + \gamma_t + \psi treat_{i,t} + \sum_{t=-12}^{12} \beta_t (D_t \times treat_{i,t}) + \theta X_{i,t-1} + \epsilon_{i,j,k,t} \tag{2}$$

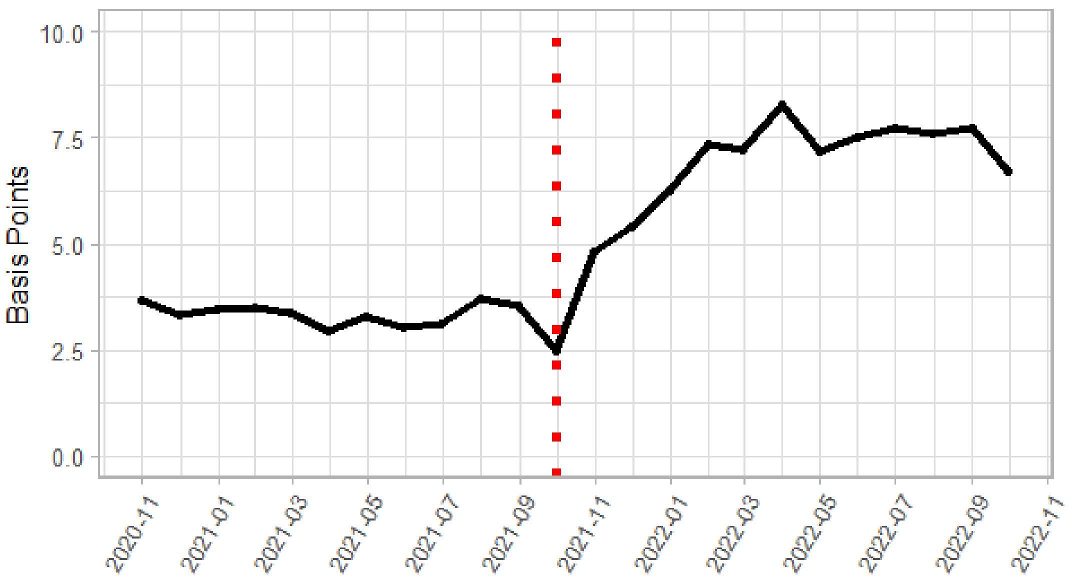
The dynamic specification allows us to investigate whether customers with and without outstanding debt were experiencing different pre-existing trends in bank switching behavior prior to the reform implementation, and also examine the post-reform trend.

4 Results

4.1 Descriptive results

We start with a simple uncontrolled visualization of the over-time changes in customer switching behavior. Figure 2 plots the share of bank accounts that switched every month between October 2020 and September 2022.

Figure 2: Share of account switching



Note: This figure reports the share of accounts that switched an account (closed an existing bank account and opened a new one in a one-month period) from October 2020 to September 2022.

The figure shows a clear increase in switching immediately after the implementation of the reform: the share of the accounts that switch each month increase from 3.7 basis points to 7.5 basis points. While the figure suggests a strong impact of the reform, it does not necessarily imply that the reform is what *caused* the increase in bank switching. For example, it is possible that the media coverage of the reform increased customers' awareness of the benefits and possibilities of switching banks. That is, the increase might not have been the result of reduced switching costs due to the online system, but rather a result of increased customers' attention to the subject. The next section investigates the impact of the reform more formally to establish causality.

4.2 Main results

Table 2 presents the results of our baseline estimation: the dependent variable is the *Switch* binary variable and columns represent different combinations of fixed effects and controls. The coefficient of the *treat* variable is positive and significant in all columns, suggesting that even before the reform treated accounts were more likely to switch relative to control accounts. The coefficient implies that, before the reform, the monthly probability of switching was higher in 4 to 27 basis points in accounts without existing debt balance compared to account with debt balance.

Our main coefficient of interest is the interaction between the *treat* and *Post* dummy. This coefficient measures the effect of the reform on switching costs above and beyond all other explanatory variables, when the control group is used to rule out any time trend. As can be seen, this coefficient is positive, significant, and has the same size in all three specifications.It suggests that after the implementation of the online transfer reform, the probability of account switching has increased by about 5 basis points.

To get a sense of the economic significance of our results we can compare the interaction coefficient estimates from Table 2 to the general share of switching before the reform, as seen in Figure 2. Prior to the reform, the monthly account switching probability was about 3.7 basis points (bp), so an increase of 5bp is a 135% increase. Over a three-year period, this represents an increase from 1.33% before the reform to 3.1% afterwards. Moreover, these results are in account level, and in order to look at customer ID level, which is a proxy for household level, we have to take into account the fact that a customer ID has 1.2 bank accounts in average (Table 1). Thus, the results represent the the three-year probability of households to switch banks has increased from 1.6% before the reform to 3.7% afterwards.

In column (3) of Table 2 we present the results of an extended specification that includes time-varying account-level control variables that may also impact the probability of account switching.[13] Specifically, we add three dummy variables denoting whether the account has an available credit line, an outstanding overdraft balance, and a connected bank credit card.[14] A fourth variable measures the number of monthly payments and checking orders drawn from the account. These four variables are all related to an account's activity level. They may affect the results if one believes that accounts with greater activity are less likely to be closed. The results show, first, that the interaction coefficient remains unchanged, implying the robustness of our specification. These additional variables, however, do have a significant effect on switching behavior. Greater account activity is indeed negatively correlated with the probability of account switching. Note that the positive coefficient on the credit line indicator implies that for customers with an available credit line but *no* overdraft utilization there is a higher probability of bank switching.

Figure 3 presents the results of the dynamic specification by plotting the β_t coefficients from Eq. (2) along with a 95% confidence bands with the coefficient of September 2021 normalized to zero. We can see that before the reform there is no clear trend or significant difference in the tendency of treated and control accounts to switch banks relative to the baseline month. Following September 2021, there is a significant increase in the probability of treated accounts to switch. The first six months after the reform show a steady increase in switch probability. This is in line with anecdotal evidence about a "soft launch" of the reform, with public advertising and targeted campaigns starting only after the launch.[15] We then see a strong and persistent effect, which remains until the

[13]We do not add time-fixed account-level control variables to the regression as they are subsumed by the customer fixed effect.

[14]Results are mostly unchanged if we use the monetary amount of the binary variables instead of the dummy indicator.

[15]An executive in one of the major banks has told us that this was due to the technical complexity of the online automatic transfer system. IT personnel were worried that the system would not work or fail if there is a large number of transfer requests, and the banks asked the regulator not to publicise too much before the launch.

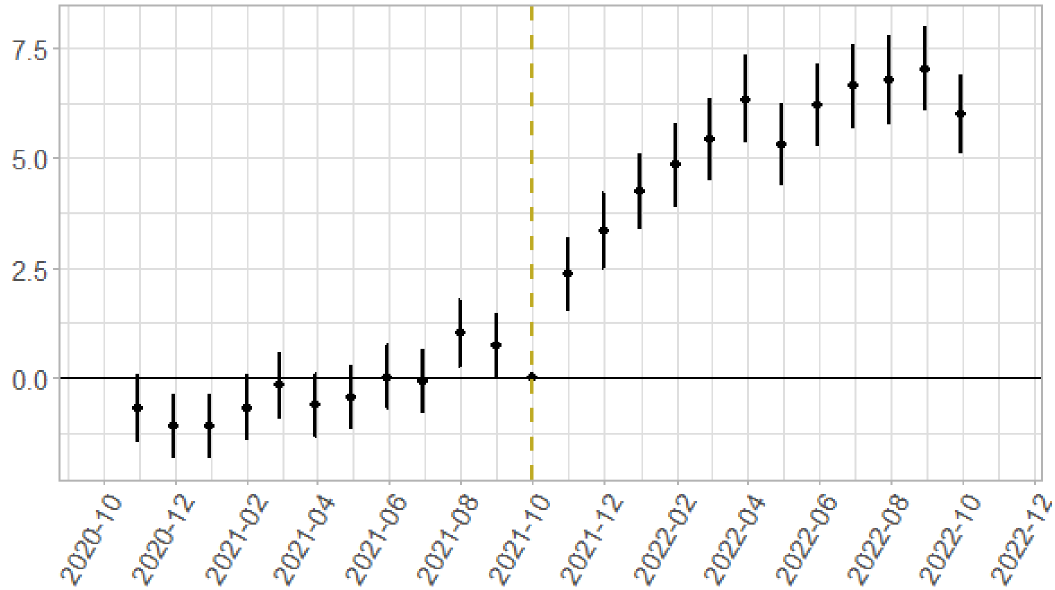
Table 2: Baseline estimations

	Switch		
	(1)	(2)	(3)
treat	4.127***	26.849***	26.194***
	(0.100)	(0.533)	(0.537)
treat × *Post*	4.715***	5.584***	5.585***
	(0.173)	(0.191)	(0.192)
Credit lim			0.153***
			(0.021)
Overdraft			−0.096***
			(0.014)
Payment Order			−0.401***
			(0.051)
Credit Card			−0.391***
			(0.017)
Customer FE	N	Y	Y
Time FE	Y	Y	Y
Bank FE	Y	Y	Y
Obs.	17,556,909	17,556,909	17,556,909
R^2	0.0003	0.041	0.041

Notes: This table reports the coefficient estimates of Equation 1. all coefficients are scaled up by 10,000 to represent impact in basis points. Time period is October 2020 through September 2022. Standard errors, clustered at the bank level are reported in parenthesis. *p<0.1; **p<0.05; ***p<0.01

end of the sample period. This sustained impact indicates that the reform brought about lasting changes in behavior or market dynamics, and had a deeper structural influence beyond the initial surge in activity.

Figure 3: Dynamic estimation



Note: This figure reports the dynamic impact of *treat* estimated from Eq. (2) with 95% confidence bands computed using standard errors, clustered at the bank level. The coefficient for September 2021 is normalized to zero.

5 Robustness

This section provides robustness tests to the main specification

5.1 Within-customer estimation

Our identification strategy leverages the different behavior of customers with and without consumer loans, who were affected differently by the reform. One potential concern is that the propensity to have a consumer loan might be correlated with unobserved variables that could drive the results. For example, if the likelihood of having a loan is linked to education level, and education influences customer reactions to the banking reform (e.g., through greater financial literacy), then the observed effect could reflect pre-existing differences between the treatment and control groups rather than the impact of the reform itself.

To address this concern, we conduct a robustness check by restricting the sample to customers who maintain both treated and control accounts during the sample period. This approach allows us to exploit customer-level fixed effects, ensuring within-customer identification similar to the within-firm identification framework used by Khwaja and Mian (2008). By comparing treated and control accounts for the same individuals, we effectively control for unobserved characteristics at the customer level. The results, presented in Table 3, confirm that the reform had a significant impact on bank account closures and switching. In fact, the interaction coefficient is significantly higher for theses customers. Other coefficients are qualitatively similar to those in the baseline specification (Table 2). These findings provide additional evidence that the reform played a causal role in increasing bank switching, independent of unobserved differences between the treatment and control groups.

Table 3: Within customer estimation

	Switch		
	(1)	(2)	(3)
treat	8.081***	9.572***	9.015***
	(0.490)	(0.591)	(0.599)
treat × *Post*	10.898***	13.336***	13.246***
	(0.888)	(1.007)	(1.009)
Credit lim			0.133***
			(0.033)
Overdraft			−0.125***
			(0.276)
Payment Order			−0.276***
			(0.090)
Credit Card			−0.237***
			(0.042)
Customer FE	N	Y	Y
Time FE	Y	Y	Y
Bank FE	Y	Y	Y
Controls	N	N	Y
Obs.	1,734,347	1,734,347	1,734,347
R^2	0.001	0.034	0.034

Notes: This table reports the coefficient estimates of Equation 1 with sample restricted to borrowers with at least one treated and one control account during the sample period. Time period is October 2020 through September 2022. *p<0.1; **p<0.05; ***p<0.01

5.2 Placebo tests

Another possible concern is that our results are driven by within-year seasonal patterns. To mitigate this concern, we conduct placebo tests by estimating Equation (1) using the two-year periods around

September 2019/2020/2022, where for each placebo sample, all estimation details are the same as in the main specification, except that *Post* is a dummy variable that marks the months after September of that sample's year.

We also examine bank switching around April 2019, when the Israeli public credit registry started to operate. In a recent paper on the Israeli public credit registry, Bank et al. (2023) find that loans for customers with a single bank account have higher interest rates compared to those with multiple accounts, and that this "hold-up" premium fell significantly once a public credit registry was implemented in April 2019. If maintaining multiple banking accounts had a significant value for borrowers before the introduction of the registry and had a lower value afterwards, we might expect to see account consolidation after April 2019, and this can have long term implications on the probability of account closing that we observe around the bank mobility reform that was implemented 2.5 years latter.

Table 4 presents the placebo results using the same 10% customer ID sample that was used for the baseline sample specification in Table 2. We can see that in all panels the *treat* variable is positive and significant. This makes sense, as we expect customers to switch treated account with higher probability than control account throughout the sample period. However, and in contrast to the baseline scenario, the interaction coefficient $treat \times Post$ does not in general exhibit a consistent size and sign throughout different columns within a single test (panel).[16] This supports our claim that the effect in September 2021 was due to the reform.

We also run our placebo tests using the dynamic specification of Equation (2). Figure 4 presents the results of the dynamic specification for each placebo sample. As can be seen, for all placebo samples, there is no clear pattern around the "event date."

5.3 Alternative customer definition

Recall from Section 3.1 that in our baseline specification, we define a "customer" as a unique set of individuals who jointly own a bank account. Consequently, some individuals may belong to

[16] The September 2022 test shows a positive effect, yet much smaller than the one in September 2021. While we do control for time fixed effects, this result may be impacted by the fact fast increase in the Bank-of-Israel during that period.

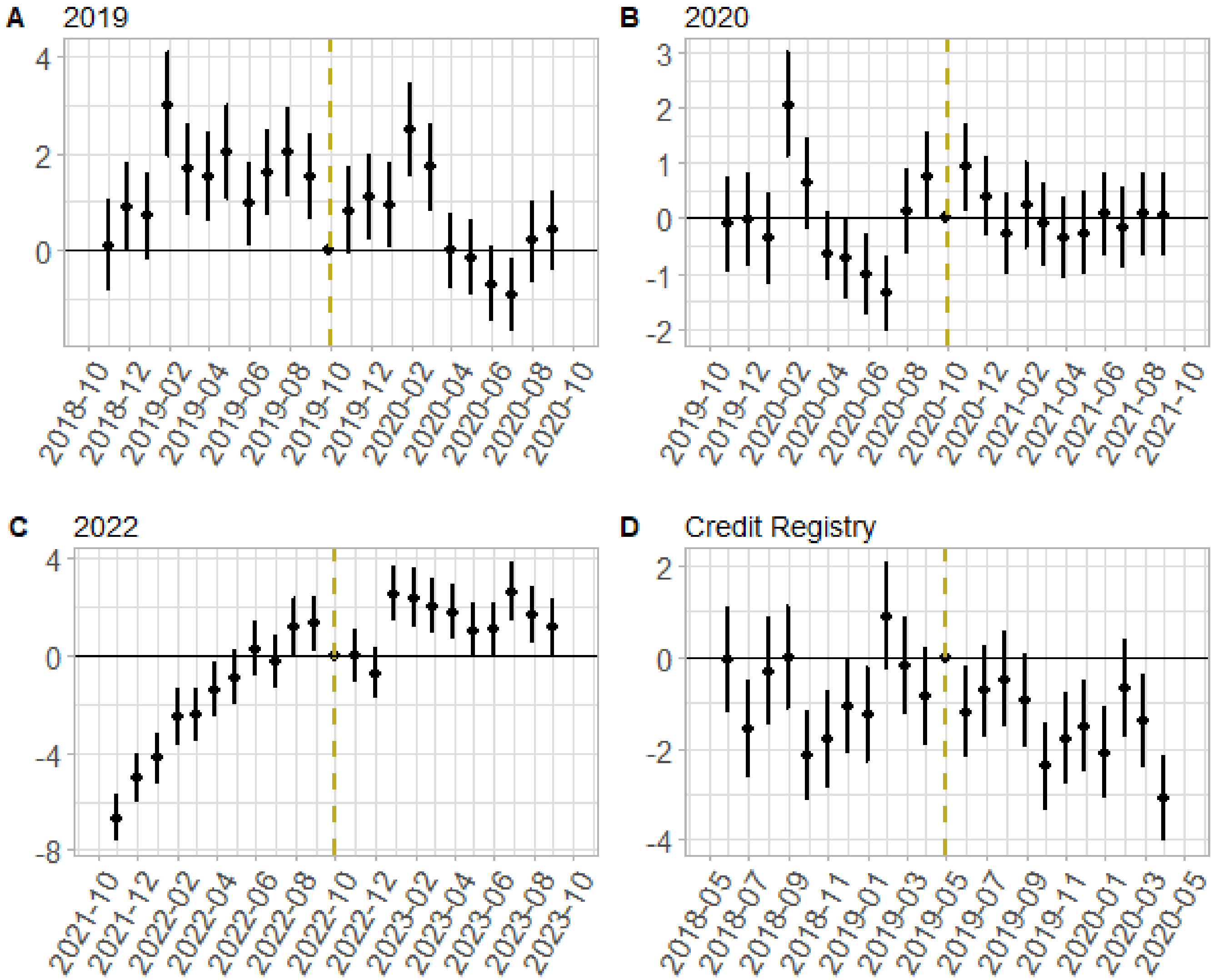
Table 4: Placebo tests

	Switch		
	(1)	(2)	(3)
Panel A: September 2019			
treat	4.126***	17.606***	16.884***
	(0.133)	(0.513)	(0.517)
treat × *Post*	−1.106***	−0.832***	−0.786***
	(0.176)	(0.181)	(0.181)
Obs.	11,998,702	11,998,702	11,998,702
R^2	0.0001	0.039	0.039
Panel B: September 2020			
treat	3.126***	14.510***	13.878***
	(0.108)	(0.442)	(0.445)
treat × *Post*	−.241*	0.068	0.115
	(0.147)	(0.151)	(0.151)
Obs.	13,200,071	13,200,071	13,200,071
R^2	0.0001	0.039	0.039
Panel C: September 2022			
treat	6.803***	37.416***	37.935***
	(0.145)	(0.684)	(0.702)
treat × *Post*	1.296***	2.856***	3.067***
	(0.216)	(0.230)	(0.232)
Obs.	13,266,457	13,266,457	13,266,457
R^2	0.0003	0.041	0.041
Panel D: April 2019 (Credit register established)			
treat	4.583***	20.259***	19.522***
	(0.143)	(0.552)	(0.557)
treat × *Post*	−1.024***	−0.814***	−0.792***
	(0.190)	(0.194)	(0.194)
Obs.	11,607,983	11,607,983	11,607,983
R^2	0.0002	0.039	0.039
Customer FE	N	Y	Y
Time FE	Y	Y	Y
Bank FE	Y	Y	Y
Controls	N	N	Y

Notes: This table reports the coefficient estimates of Equation 1 using alternative (placebo) time periods with the same customers used in the sample of the main time period. *p<0.1; **p<0.05; ***p<0.01

more than one "customer" group (see footnote 7). This approach works well in scenarios where joint ownership of bank accounts arises for reasons unrelated to switching costs. For example, in a household where two spouses share a joint account for daily needs while each maintains a separate personal account for individual budgeting, the decision to have multiple accounts is unrelated to switching costs, and our definition does not introduce bias. However, if the separate accounts are inactive or legacy accounts, failing to group them under the same customer ID could lead to the

Figure 4: Dynamic estimation for placebo samples (Switch)



Note: This figure reports the dynamic impact of *treat* estimated from Eq. (2) using alternative (placebo) time periods and a random 10% sample. 95% confidence bands computed using standard errors, clustered at the bank level, are also presented.

misidentification of switching events.

To address this potential issue, we perform an additional analysis using a restricted sample comprising only individuals who, throughout the sample period, are part of a single customer ID. This ensures a one-to-one mapping between customer IDs and bank accounts, eliminating complexities introduced by household structures and account management practices. The results, presented in Table 5, show that the findings remain both qualitatively and quantitatively consistent with the baseline specification. These results confirm that our main conclusions are robust to potential biases arising from the customer ID definition.

Table 5: Restriction to one-to-one mapping of customers to accounts

	Switch		
	(1)	(2)	(3)
treat	4.899***	28.896***	28.193***
	(0.134)	(0.675)	(0.680)
treat $\times$ *Post*	4.584***	5.343***	5.314***
	(0.223)	(0.245)	(0.246)
Customer FE	N	Y	Y
Time FE	Y	Y	Y
Bank FE	Y	Y	Y
Controls	N	N	Y
Obs.	11,828,767	11,828,767	11,828,767
R^2	0.0003	0.041	0.041

Notes: This table reports the coefficient estimates of Equation 1 with sample restricted to borrowers who are the only account holder in all their accounts during the sample period. The time period is October 2020 through September 2022. *p<0.1; **p<0.05; ***p<0.01

5.4 Controlling for moves from treatment to control

The fact that account IDs may move between the treatment and the control group each month as they repay a consumer loan or take a new one allowed us to control for possible unobservable difference between the to treatment and control groups by looking at a within-customer effect in Section 5.1. This analysis, however, implicitly assumes that the move between the treatment and control groups is orthogonal to the switching decision. If customers take into account their willingness to switch a bank when they make decisions on taking or repaying a customer loan, this may create a bias.[17]

To address this concern, we perform a robustness check by restricting the sample to customers who remain in either the treatment or the control group throughout the entire sample period. The results, reported in Table 6, are consistent with those from the baseline specification. The estimated

[17] Is should be noted that before the reform customers were likely not aware that an active customer loan will make it harder to transfer their account, as explained in Section 2. Such effect, if existed, should appear only in the month after the reform.

effect of the reform, captured by the interaction coefficient $treat \times Post$, is somewhat smaller than in the original sample analysis. This is in line with the findings in Table 3, which uses a sample of customers who do switch between treatment and control groups during the study period and reveals a stronger effect. Taken together, the fact that the effect remains statistically significant in both subsamples provides strong evidence of the robustness of our results.

Table 6: Restriction to accounts that are treated/control throughout the sample period

	Switch		
	(1)	(2)	(3)
$treat$	2.787***	17.199***	15.478***
	(0.095)	(0.922)	(0.949)
$treat \times Post$	2.701***	3.397***	3.391***
	(0.160)	(0.169)	(0.169)
Customer FE	N	Y	Y
Time FE	Y	Y	Y
Bank FE	Y	Y	Y
Controls	N	N	Y
Obs.	13,762,541	13,762,541	13,762,541
R^2	0.0002	0.041	0.041

Notes: This table reports the coefficient estimates of Equation 1 with sample restricted to accounts that are treated/control throughout the sample period. The time period is October 2020 through September 2022. *p<0.1; **p<0.05; ***p<0.01

5.5 Additional Tests

Table 7 presents the results for the baseline specification applied to the full database, rather than the 10% random sample used in previous analyses. The coefficients remain consistent in both significance and magnitude with those obtained from the baseline analysis, demonstrating the robustness of the findings to the expanded dataset. These results provide additional confidence that the observed effects are not driven by sampling variability but are reflective of broader patterns in the population.

Table 8 presents the results when restricting the analysis to a shorter time frame, covering six months before and six months after the regulatory change. This approach serves two purposes: (i) it demonstrates that the results are not sensitive to the choice of the event window, ensuring

Table 7: Baseline estimations - full database

	Switch		
	(1)	(2)	(3)
treat	4.259***	28.164***	27.521***
	(0.046)	(0.243)	(0.247)
treat × *Post*	4.996***	5.983***	5.997***
	(0.078)	(0.088)	(0.088)
Credit lim			0.168***
			(0.012)
Overdraft			−0.097***
			(0.007)
Payment Order			−0.324***
			(0.074)
Credit Card			−0.442***
			(0.009)
Customer FE	N	Y	Y
Time FE	Y	Y	Y
Bank FE	Y	Y	Y
Obs.	89,736,876	89,736,876	89,736,876
R^2	0.0003	0.044	0.044

Notes: This table reports the coefficient estimates of Equation 1 using the full database instead of the random 10% sample. Time period is October 2020 through September 2022. Standard errors, clustered at the bank level are reported in parenthesis. *p<0.1; **p<0.05; ***p<0.01

robustness across different time frames; and (ii) it limits the sample to a period before April 2022, when the Bank of Israel began rapidly increasing interest rates in response to rising inflation. By excluding observations during the interest rate hike period, this analysis reduces potential biases associated with monetary policy's impact on consumer switching behavior. The results remain consistent with the baseline findings, reinforcing the causal impact of the reform.

In our baseline specification, we define bank switching as the closure of an old bank account followed by the opening of a new one within a one-month window. One might claim that prior to the reform bank switching may have taken longer than one month, with customers often maintaining overlapping accounts for several months before fully transitioning. If this were the case, our baseline specification might under-identify switching events in the pre-reform period, potentially biasing the results. To address this concern, Table 9 presents results using extended time windows for identifying switching events. Columns 1-3 use a two-month window, while columns 4-6 use a three-month window. As the table shows, the results remain consistent, confirming that the observed

Table 8: One year window

	Switch		
	(1)	(2)	(3)
treat	3.991***	30.013***	29.688***
	(0.139)	(0.911)	(0.931)
treat × *Post*	3.865***	4.275***	4.312***
	(0.239)	(0.245)	(0.246)
Customer FE	N	Y	Y
Time FE	Y	Y	Y
Bank FE	Y	Y	Y
Controls	N	N	Y
Obs.	8,791,215	8,791,215	8,791,215
R^2	0.0003	0.077	0.077

Notes: This table reports the coefficient estimates of Equation 1 using a shorter time period. Time period is April 2021 through March 2022. Standard errors, clustered at the bank level are reported in parenthesis. *p<0.1; **p<0.05; ***p<0.01

effects are not sensitive to the choice of switching identification window.

Table 9: Switching window

	$Switch^{1-months}$			$Switch^{2-months}$		
	(1)	(2)	(3)	(4)	(5)	(6)
treat	2.445***	18.161***	18.285***	3.411***	23.357***	23.086***
	(0.076)	(0.435)	(0.445)	(0.090)	(0.495)	(0.502)
treat × *Post*	4.321***	4.916***	4.952***	4.603***	5.343***	5.363***
	(0.143)	(0.159)	(0.160)	(0.160)	(0.178)	(0.178)
Customer FE	N	Y	Y	N	Y	Y
Time FE	Y	Y	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y	Y	Y
Controls	N	N	Y			
Obs.	17,556,909	17,556,909	17,556,909	17,556,909	17,556,909	17,556,909
R^2	0.0003	0.040	0.040	0.0003	0.040	0.040

Notes: This table reports the coefficient estimates of Equation 1 using a shorter time period. Time period is April 2021 through March 2022. Standard errors, clustered at the bank level are reported in parenthesis. *p<0.1; **p<0.05; ***p<0.01

6 Additional Results

6.1 Account Consolidation

While the primary objective of the banking mobility reform was to promote competition by enhancing customers' ability to switch their banking activities across institutions, a side effect was that it also lowered the costs of consolidation. Specifically, the reform facilitated the transfer of activity from one bank account into an existing account at another institution, thereby enabling customers to concentrate their banking relationships within a smaller number of banks.

Consolidation of banking activities into fewer accounts following the reform may occur for several reasons. First, when switching costs are high, maintaining multiple banking relationships can serve as a hedge against hold-up problems (Farinha and Santos 2002; Gopalan et al. 2011). As switching costs decline, the incentive to sustain multiple accounts diminishes. Second, high switching costs and the difficulty of closing accounts may lead households to accumulate accounts simply to avoid the inconvenience of terminating inactive ones. In this context, an online, user-friendly transfer platform can encourage households to close dormant accounts, further contributing to consolidation.

To examine the impact of the BMR on account consolidation, we define account mergers as cases in which the closure of an existing account is not accompanied by the opening of a new one. Recall that our sample includes only customer IDs with active bank accounts both before and after the two-year window. This design eliminates cases in which account closures reflect an exit from the credit registry. Consequently, for customers with continuous banking activity, any account closure must reflect either bank switching or account consolidation.

We then re-estimate Eq.(1) using $Merge$ as the dependent variable which is a binary variable taking the value one if account i of customer j at bank k was consolidated to a different existing account in month t. All other variables are the same as in the original specification.

Table 10: Impact of Accounts Consolidation

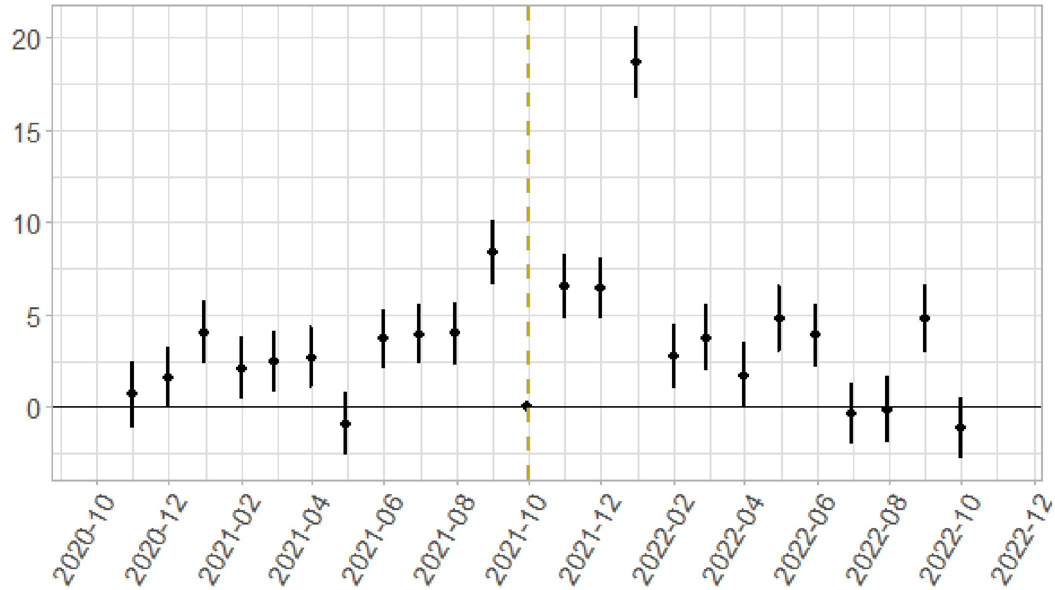
	Merge		
	(1)	(2)	(3)
treat	14.692***	87.115***	65.963***
	(0.236)	(1.006)	(0.961)
$treat \times Post$	1.947***	3.785***	1.661***
	(0.343)	(0.367)	(0.371)
Credit lim			− 3.418***
			(0.062)
Overdraft			−0.050***
			(0.040)
Payment Order			−11.006***
			(0.149)
Credit Card			−2.327***
			(0.041)
Customer FE	N	Y	Y
Time FE	Y	Y	Y
Bank FE	Y	Y	Y
Obs.	17,556,909	17,556,909	17,556,909
R^2	0.001	0.034	0.039

Notes: This table reports the coefficient estimates of Equation 1 using $Merge$ as the dependent variable. Time period is October 2020 through September 2022. Standard errors, clustered at the bank level are reported in parenthesis. *p<0.1; **p<0.05; ***p<0.01

Table 10 presents the results when the dependent variable is the $Merge$ binary variable. The interaction coefficent on the $treat \times Post$ imply that probability of account merging has increases for treated accounts by 1.6-3.8 basis points. We also examine the impact on account merging using the dynamic specification of Eq.(2), again with $Merge$ as the dependent variable. The results are presented in Figure 5. It is clear that the effect of the reform is limited to a December 2020, which present a sharp increase in consolidation. All other months before and after the reform are the same. Specifically, the effect is not lasting.

The results of 5 imply that the effect is temporary, either due to an increased attention to the possibility of closing an account easily, or because the reform decreased the optimal number of accounts, and consolidation activity represents convergence to the new optimum.

Figure 5: Dynamic estimation on account merging



Note: This figure reports the dynamic impact of *treat* estimated from Eq. (2), when the dependent variable is $Merge$. Coefficients are reported with 95% confidence bands computed using standard errors, clustered at the bank level. The coefficient for September 2021 is normalized to zero.

6.2 Switching behavior and household characteristics

We examine two customer characteristics that may influence customers' ability and willingness to switch banks: income and age. Customers with higher socioeconomic status may possess greater financial literacy, more resources to manage the switching process, and stronger incentives to optimize their financial arrangements, making them more responsive to reductions in switching costs. By contrast, lower-income customers may face informational barriers, limited expected gains from switching, or institutional frictions, such as reliance on overdraft facilities, that discourage mobility.

Age may also play a role: younger customers tend to be more flexible, technologically adept, and less entrenched in long-term banking relationships, whereas older customers may display greater inertia due to habit formation, trust in their current bank, or the perceived complexity of transferring financial arrangements.

To test these hypotheses, we split the sample by the socioeconomic indicator of the customer's municipality and by customer age group (see Section 3.1). For both dimensions, we partition the sample at the median and estimate Eq. 1 separately for each sub-sample.[18]

Table 11: Customer Heterogeneity

	Switch			
	Low Socio-Economic	High Socio-Economic	Low Age	High Age
	(1)	(2)	(3)	(4)
treat	27.447***	24.304***	33.339***	14.533***
	(0.710)	(0.818)	(0.779)	(0.645)
treat × *Post*	5.849***	5.257***	7.825***	3.025***
	(0.262)	(0.286)	(0.321)	(0.196)
Customer FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Bank FE	Y	Y	Y	Y
Account level controls	Y	Y	Y	Y
Obs.	9,816,953	7,739,956	9,403,860	8,153,049
R^2	0.041	0.040	0.041	0.039

Notes: This table reports the coefficient estimates of Equation 1 splitting the sample either by socio-economic indicator (columns 1-2) or the customer age group (columns 3-4) using for both sub-samples the sample median. Standard errors, clustered at the bank level are reported in parenthesis. *p<0.1; **p<0.05; ***p<0.01

Table 11 reports the results of this estimation. The differences between high- and low-socioeconomic groups are relatively small, both before and after the reform. This may suggest either that socioeconomic status is not a major determinant of switching behavior, or that our proxy is too noisy.[19] By contrast, age appears to be an important source of heterogeneity. Younger customers were already more than twice as likely to switch banks prior to the reform (33.4 vs. 14.5 basis points

[18] As shown in Table 1, the median age falls in group 7 (ages 45–49), and the median socioeconomic indicator is 6 on a scale from 1 to 10.

[19] The socioeconomic indicator is defined at the municipality level. In particular, large cities often contain a diverse population in socioeconomic terms, which may reduce the precision of this measure for part of the sample.

per month), and the effect of the reform was significantly larger for them (7.8 vs. 3.0 basis points). These results highlight the importance of age when assessing the broader implications of digitally enabled banking reforms.

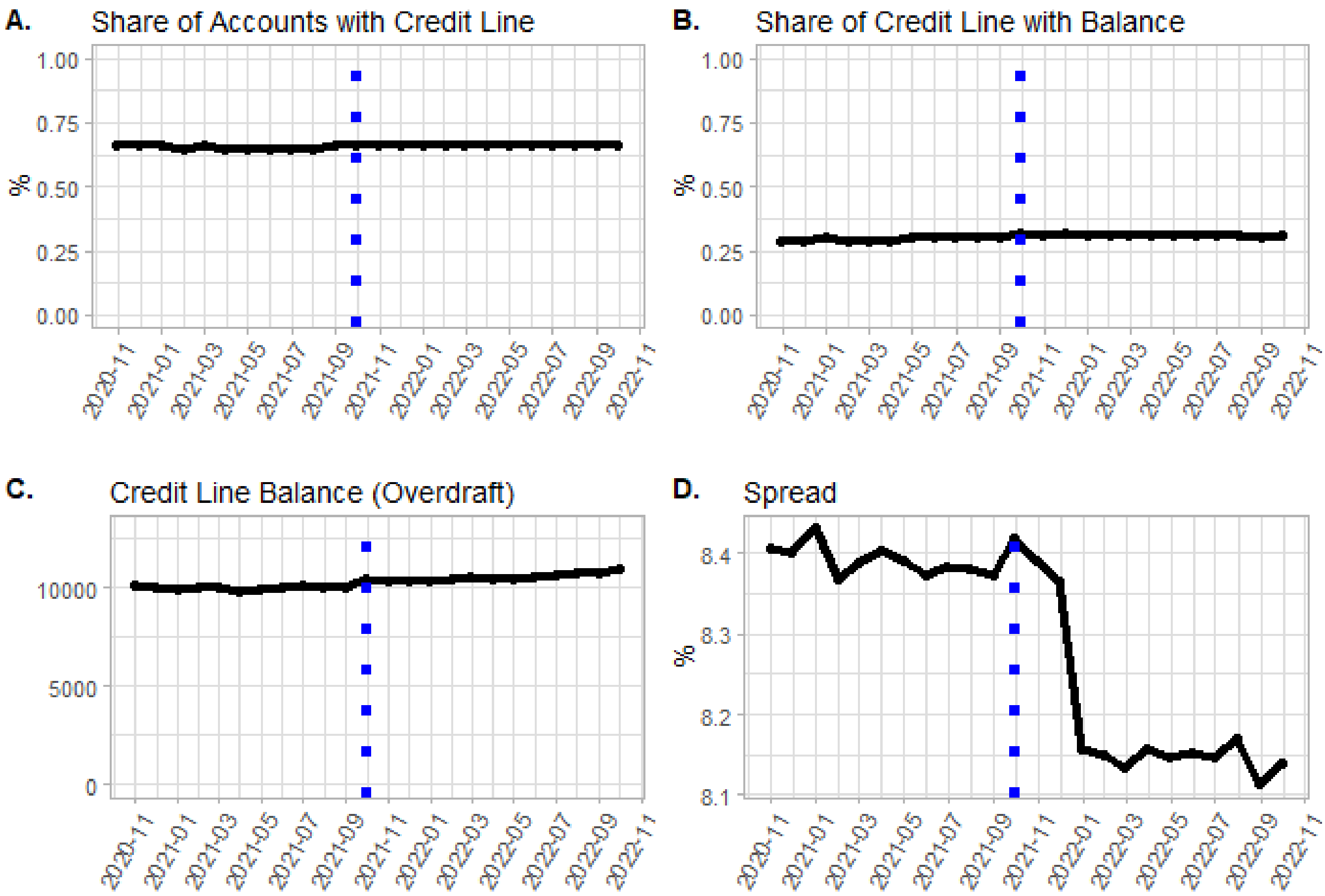
7 Aggregate implications

The previous sections demonstrated that reducing switching costs increases the likelihood of customers to change banks. While this increase suggests that some customers perceived benefits in switching, it remains unclear what these benefits were and whether the reduction in switching barriers translated into broader improvements in consumer welfare. This ambiguity is particularly relevant given that, although switching rates increased post-reform, the absolute number of individuals who actually moved their accounts remains very low.

From a theoretical perspective, if the perceived threat of customer mobility is sufficiently credible, banks may respond by improving conditions for their incumbent customers, either all or only those that threat to switch. In this context, the reform may have intensified competitive pressure not only through realized switching but also via the increased plausibility of switching. Such competitive responses could manifest in various ways, including lower fees, higher deposit rates, or more favorable lending terms.

Due to data limitations, we are unable to observe fees or deposit rates directly. Instead, we examine changes in credit-related variables: specifically, credit line availability, size, utilization, and cost. Figure 6 presents trends in several credit line-related outcomes over time. Panels A through C show that the availability of credit lines, the share of credit lines in use (i.e., with a positive balance), and the average overdraft balance all remain relatively stable around the time of the reform (marked by the vertical dashed line). If anything, there is a slight increase in credit line utilization following the reform. In contrast, Panel D reveals a marked and immediate decline in the spread charged on overdraft balances. This sharp reduction in borrowing costs suggests that the reform may have intensified competition among banks, leading to improved lending terms for consumers, even in the absence of major changes in credit access or usage.

Figure 6: Aggregate implications - consumer deposit account credit line utilization and spreads



Note: The figure presents trends in several credit line–related outcomes over the sample time period.

If the reform indeed increased competitive pressures, we would expect that changes in credit-related variables will be more salient for accounts that were treated by the reform relative to those that were not. This is because customers of treated accounts, that is, those without an active consumer loan, will be at a better position to bargain with the bank compared to costomer in the control group.

To assess this hypothesis, we estimate the following regression:

$$y_{i,j,k,t} = \alpha_j + \delta_k + \gamma_t + \psi treat_{i,t} + \beta Post_t \times treat_{i,t} + \epsilon_{i,j,k,t}, \tag{3}$$

where y represents one of several account-level outcomes related to credit line conditions: (i) A dummy variable equal to one if the account has an available credit line; (ii) Among accounts with a credit line, the logarithm of the credit line size; (iii) Among accounts with a credit line, a dummy variable indicating whether the account is in overdraft (i.e., has a negative balance); (iv) Among accounts with an overdraft, the logarithm of the overdraft amount; (v) Among accounts with an overdraft, the interest rate spread paid by the customer on the overdraft balance.

The variables *Treat* and *Post* are defined as in the baseline specification, capturing treatment status and the post-reform period, respectively. All regressions include customer fixed effects, time fixed effects, and bank fixed effects to control for unobserved heterogeneity at these levels.

Table 12: Aggregate implications

	Credit Lim. Dummy	log(Credit Lim)	Overdraft Dummy	log(Overdraft)	Spread
	(1)	(2)	(3)	(4)	(5)
treat	-0.117^{***}	-0.118^{***}	-0.033^{***}	-0.279^{***}	0.089^{***}
	(0.001)	(0.003)	(0.001)	(0.006)	(0.011)
treat $\times$ *Post*	0.0001	-0.029^{***}	0.001	0.028^{***}	-0.042^{***}
	(0.0004)	(0.001)	(0.001)	(0.003)	(0.006)
Customer FE	Y	Y	Y	Y	Y
Time f.e	Y	Y	Y	Y	Y
Time f.e	Y	Y	Y	Y	Y
Obs.	17,556,909	11,514,871	11,514,871	4,190,462	4,190,462
R^2	0.861	0.931	0.610	0.650	0.860

Notes: This table reports the coefficient estimates of Equation 3. Standard errors, clustered at the bank level are reported in parenthesis. *p<0.1; **p<0.05; ***p<0.01

Table 12 reports how credit-related variables were affected by the reform. The coefficients on *treat* indicate that, even prior to the reform, customers in the treatment group were less likely to hold credit lines or use overdraft facilities, consistent with the notion that they generally relied less on credit (see also Table 1 and the accompanying discussion). Interestingly, however, treated customers paid higher overdraft spreads before the reform. One possible explanation is that, because they were less frequently in overdraft, they paid less attention to such charges and were less inclined to negotiate fees.

The interaction term *treat* $\times$ *Post* shows that, after the reform, treated customers held smaller credit lines relative to the control group. More importantly, the reform significantly affected overdraft usage and pricing: treated customers were more likely to obtain lower overdraft interest rates, while simultaneously holding larger overdraft balances. This pattern is consistent with an increase in their bargaining power vis-à-vis banks.

8 Conclusions

This study examines the impact of a regulatory reform in Israel that significantly reduced the costs of switching banks through the implementation of an online transfer system. Using a comprehensive dataset of bank accounts and employing a difference-in-differences methodology, we demonstrate that the reform led to a substantial increase in bank switching activity. The findings highlight that customers who were able to utilize the online system were much more likely to switch banks, reflecting a clear causal relationship between reduced switching costs and customer mobility. Furthermore, the observed persistence of increased switching behavior suggests that the reform brought about lasting structural changes in the banking market.

Beyond the rise in switching, our analysis reveals the broader implications of reduced switching costs for customer financial conditions. Specifically, the study suggests that bank switching post-reform was associated with better credit conditions, especially when focusing on overdraft fees. These findings imply that banks responded to increased competition by offering more attractive terms, benefiting consumers directly and fostering a more dynamic financial market.

This research contributes to the literature on consumer switching costs, banking competition, and digital transformation in financial services. By isolating the effects of reduced transactional barriers, we provide robust evidence that digitalization can enhance customer mobility, reshape banking relationships, and challenge existing market dynamics. The findings have important policy implications, emphasizing the potential of regulatory interventions to improve consumer welfare and market efficiency in the banking sector.

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