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

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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. The reform more than doubled the annual probability of bank switching, increasing it from 0.6% to approximately 1.4%. The effect is persistent and cannot be attributed to a temporary increase 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.

JEL Classification Codes: G21, G28, G51

Keywords: bank switching, switching costs, regulatory reform, digital banking, deposits.

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השפעת רפורמת "מעבר בקליק" על ניידות לקוחות והתחרות במערכת הבנקאית בישראל

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תקציר

מחקר זה בוחן את ההשפעה של רפורמת מעבר מבנק לבנק בקליק שנכנסה לתוקפה בישראל בחודש ספטמבר 2021 אשר הפחיתה באופן משמעותי את הזמן והמאמץ הנדרשים מחלק מהלקוחות להעביר את פעילותם הפיננסית בין בנקים. באמצעות מתודולוגיית הפרש-הפרשים (Difference-in-Differences) אנו מוצאים כי הירידה בעלויות המעבר הובילה לעלייה מובהקת בניידות הלקוחות. בפרט, הרפורמה יותר מהכפילה את ההסתברות השנתית למעבר בין בנקים – מ־0.6% לכ־1.4%. האפקט נמצא מתמשך ואינו ניתן לייחוס לעלייה זמנית בתשומת הלב של הלקוחות. ממצאים אלו מספקים עדות חדשה בדבר חסמי מעבר בבנקאות הקמעונאית, ומדגישים כיצד טרנספורמציה דיגיטלית עשויה לעצב מחדש את התנהגות הלקוחות, תוך השלכות משמעותיות על רמת התחרות, היציבות הפיננסית ומודלי הפעילות של הבנקים.

מילות מפתח: רגולציה, בנקאות דיגיטלית, עלויות מעבר, מעבר בין הבנקים.

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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 three 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 (Petersen and Rajan 1994; Berger and Udell 1995). This advantage might be from banks' ability to gather soft information about customers' creditworthiness and financial capacity through ongoing relationships (Mester et al. 2006).[2] Yet evidence suggests that customers remain with their existing banks even when better terms are available elsewhere. In a recent study, Basten and Juelsrud (2023) find that existing bank depositors receive lower rates on their deposits and pay higher risk-adjusted rates on their loans. Accordingly, 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, as well as in other countries, switching banks requires

[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."

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 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, known as 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 through their current bank's website, 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.[4] 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

[3]Numerous online guides detail these requirements. See, for example, a guide from USNews, updated to December 10, 2025: https://www.usnews.com/banking/articles/how-to-switch-banks-a-step-by-step-guide.

[4]The tendency of customers to stay with their current provider is also known in the literature as "inertia" and, in the case of products, as "brand loyalty."

services, switching costs can lead to higher credit interest rates, lower deposit rates, and elevated fees.[5] 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 et al. 2017; Drechsler et al. 2021; Segev et al. 2024).

Despite their importance, the nature of switching costs and their impact on customer mobility in banking remain poorly understood. Empirical research has been constrained by two main challenges: limited access to granular data on household-level bank switching behavior and the difficulty of disentangling the effects of the various factors that influence switching decisions. Customers' decisions to switch banks may be shaped by multiple elements, including transaction costs (e.g., time and administrative effort), search frictions, brand loyalty, existing banking relationships, and low perceived benefits from switching. As a result, much of the existing literature on bank switching has relied either on policy-oriented studies using self-reported survey data or on broad interpretations of "switching costs" that encompass all frictions discouraging customers from changing banks (see the literature review for further details).

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 a large array of additional

[5] Ausubel (1991) provides early evidence that credit card rates are higher due to switching barriers. Sharpe (1997) provides evidence that switching costs allow banks to 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. Another example of inertia is the failure of mortgage borrowers to refinance when interest rates decline, resulting in excessive interest payments (Keys et al. 2016; Andersen et al. 2020).

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 1.8%. This figure is lower than the UK and European 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 4.1%, corresponding to a 130% 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.

To evaluate the robustness of our baseline difference-in-differences results, we conduct a series of additional tests addressing concerns related to sample composition, identification, and measurement. We examine sensitivity to customer entry/exit and to movement between treatment and control status by estimating the model separately for customers who remain in a given group throughout the sample period and those who transition between groups. We then implement placebo tests with alternative event dates to rule out seasonal patterns or pre-trends. Next, we verify that the results are not driven by our customer definition by restricting the sample to individuals with a one-to-one mapping between customers and accounts. Finally, we test robustness to alternative samples and switching windows. Across all specifications, the estimated effect of the reform on bank switching remains economically meaningful and statistically significant.

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.[6]

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

[6]Anecdotal evidence suggests that the reform encouraged banks to offer rate-free overdraft. See more details in Section 7.

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)). 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]."[7]

An examination of the reform's first year by the Bank of Israel's Supervision Department revealed that existing debt was indeed a major barrier to the use of the online transfer system. Of approximately 18,000 transfer requests cancelled by banks through September 30, 2022, about 47 percent were rejected for technical reasons (such as identification issues or inactive accounts), while roughly 38 percent were rejected due to unpaid debts at the originating bank. In response, the Supervision Department asked banks to reassess their treatment of transfer requests involving existing loans and to implement mechanisms that would allow transfers to proceed despite the existence of credit (Bank of Israel 2022). Based on anecdotal evidence that these measures subsequently improved system usage among customers with outstanding credit, we restrict our identification strategy to the reform's first year of operation.

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.[8]

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

[8]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

For our empirical analysis, we use a panel dataset covering all consumer deposit (checking) bank accounts reported to the Israeli credit registry. Banks are required to submit monthly information on each checking account under one of two conditions. First, an account is reported if it has an overdraft credit line, regardless of whether the credit line is actively used. An overdraft is a revolving credit facility attached to a checking account that allows customers to withdraw funds up to a predetermined limit. Overdraft borrowing is a common form of short-term household credit in Israel: banks typically offer overdraft facilities to most customers with payroll deposits, and approximately 40% of Israeli households utilize such credit at least once per year. Second, even in the absence of an overdraft facility, an account is reported if it is used for check payments or other payment orders (e.g., credit card transactions). Because the vast majority of checking accounts satisfy at least one of these criteria, the credit registry covers approximately 80% of retail checking accounts in Israel.

Our data set is at the account-month level. We assign a unique customer ID 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.[9] In what follows we refer to the unique ID as the "customer ID" or "identity," though it may actually represent more than one person. For most empirical estimations, we focus on the year 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 before and after the sample period. The full data set includes information on over 97 million account-month observations belonging to 4.5 million individuals. Because of the size of the data, we run

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.

[9] 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 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.

most of the estimations on a random sample of 20% of the customer ID from the full data.[10] We define an account switch as the closure of a checking account within one month of the same customer ID opening a new checking account at a different bank.

Our identification strategy leverages the institutional feature that customers with an outstanding consumer loan at a given bank were excluded from using the online transfer system to automatically close and transfer their account products. This constraint generates time-varying exposure to the reform at the account level. We therefore classify an account as "treated" in months in which the account holder does not have an outstanding consumer loan with the same bank, and as "control" otherwise.

Table 1 presents descriptive statistics of our random sample. Panel A presents account-month–level statistics for the 20% sample, separately for treated and control accounts. *Switch* denotes the monthly share of accounts that moved to a different bank (see precise definition below in Section 3.2). *Credit Lim* denotes the overdraft credit line attached to the account; we report the share of accounts with an authorized credit line in a given month and the average credit limit among those accounts. *Overdraft* captures overdraft utilization at month-end; we similarly report the share of accounts with active utilization and the average outstanding balance among those accounts. *Credit Card* indicates the share of accounts for which the customer also holds a credit card with the same bank and the end-of-month credit card balance.[11] *Payment Orders* measures the number of payment orders and physical checks processed during the month. For control accounts – those with an outstanding consumer loan at the bank – the table also reports the number of consumer loans and their outstanding balances.

Panel B reports descriptive statistics at the customer ID level.[12] *Socio* denotes the socioeconomic indicator of the customer's municipality of residence. The Israeli Central Bureau of Statistics assigns

[10]Section 5.5 shows that the main results are almost identical when using the full database.

[11]In Israel, credit cards are issued either directly by banks or by independent credit card companies. The variable in Table 1 refers only to bank-issued credit cards linked to the specific account.

[12]A customer ID may belong to the control group in some months (when it has a positive consumer loan balance) and to the treatment group in other months (when no such balance exists). Consequently, the total number of unique customer IDs is smaller than the sum of treated and control customer IDs. In Subsection 5.4, we show that our results are robust when restricting the sample to customer IDs that remain in either the treatment or the control group throughout the sample period.

each local council or municipality a socioeconomic index ranging from 1 (lowest) to 10 (highest). *Age* is reported in 14 categorical groups, with higher values corresponding to older age cohorts.[13] For each customer ID, we assign the minimum socioeconomic indicator and age group among its account holders. The panel also reports the mean and median number of individuals per customer ID, as well as the average number of bank accounts held per 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 (%)	0.06	2.41	0	0.01	0.88	0	0.08	2.85	0
Accounts with Credit Lim (%)	61.23	48.72	100	80.51	39.61	100	52.44	49.94	100
Credit Lim (Thousand NIS)	9.07	12.60	4	14.02	14.03	10	6.81	11.19	1
Accounts with Overdraft (%)	22.20	41.56	0	44.15	49.66	0	12.18	32.71	0
Overdraft (Thousand NIS)	3.30	9.81	0	5.87	10.62	0.005	2.13	9.18	0
Accounts with Credit Card (%)	68.75	46.35	100	76.12	42.64	100	65.39	47.57	100
Credit Card Balance (Thousand NIS)	4.12	6.35	1.26	5.98	7.47	3.38	3.28	5.56	0.59
Payment Orders	3.64	4.45	2	4.85	5.31	3	3.08	3.88	2
Number of Consumer Loans	0.49	0.93	0	1.56	1.05	1	0	0	0
Consumer Loans Current Balance (Thousand NIS)	19.97	44.98	0	63.75	60.55	44.70	0	0	0
Account-month Observations		19,526,719			6,117,671			13,409,048	
Panel B: Customer ID Characteristics									
Number of Bank Accounts	1.21	0.47	1	1.24	0.51	1	1.19	0.45	1
Socio	5.50	2.19	6	5.21	2.15	5	5.63	2.19	6
Age Group	7.36	3.34	7	7.06	2.77	7	7.50	3.57	7
Number of People on the Account	1.30	0.46	1	1.32	0.47	1	1.30	0.46	1
Unique Customer ID		865,731			327,425			716,370	

Notes: This table presents the descriptive statistics of a random sample composed of 20% 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.

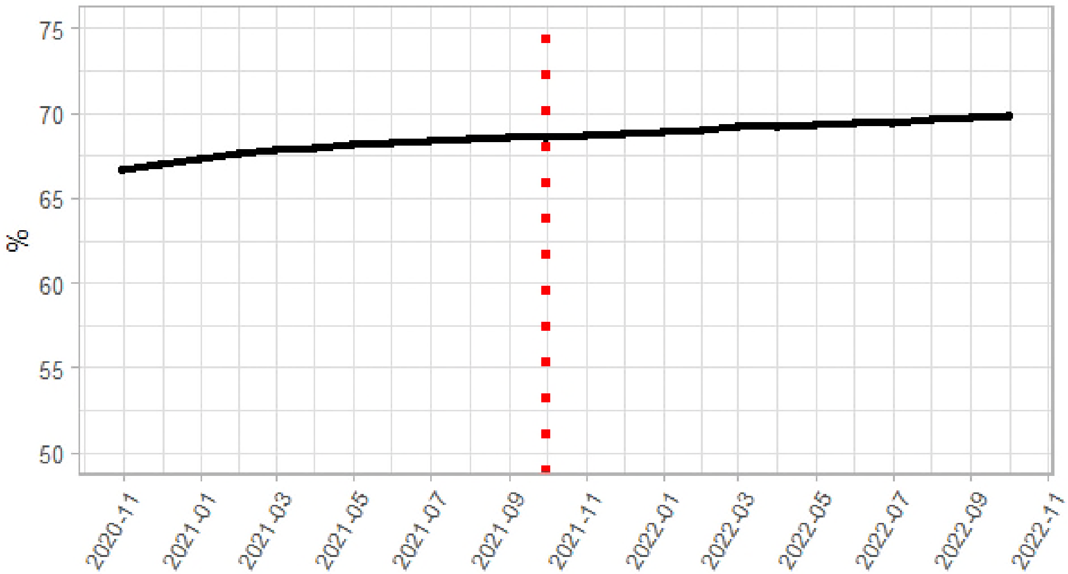
Table 1 reveals several notable differences between treated and control accounts. On average, treated accounts are less likely to have an overdraft credit line, and when they do, the credit limit is substantially smaller (NIS 6.81k compared to NIS 14.02k). Treated accounts are also much less likely to use overdraft facilities (12% versus 44% for control accounts) and hold smaller overdraft balances. In addition, treated accounts have lower credit card balances and process fewer payment orders and checks per month (3.08 versus 4.85). At the customer level, treated customers tend to reside in municipalities with slightly higher socioeconomic indices (mean of 5.63 versus 5.21)

[13] Ages 18–21 are coded as 1; 22–24 as 2; 25–29 as 3; 30–34 as 4; 35–39 as 5; 40–44 as 6; 45–49 as 7; 50–54 as 8; 55–59 as 9; 60–64 as 10; 65–69 as 11; 70–74 as 12; 75–79 as 13; and ages above 79 as 14.

and belong to marginally older age groups (7.50 versus 7.06). Finally, over the full sample period, the share of accounts that switch banks is eight times higher among treated accounts than among control accounts (0.08% versus 0.01%). The key identifying assumption underlying our empirical strategy is that, absent the reform, these differences between treated and control accounts would have remained stable over time.

Figure 1 plots the monthly share of *treated* accounts in the full database. Approximately 66% of accounts do not have an outstanding consumer loan, and this share exhibits strong persistence over time. The absence of any visible change around the reform date indicates that the reform did not affect customers' borrowing decisions. This stability supports the identifying assumption that the presence of an outstanding consumer loan, and thus treatment assignment, is exogenous to the implementation of the reform.

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.

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:

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

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 three month before or after[14]. *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. They include indicators for credit line, overdraft balance, and connected credit card, as well as the number of payment orders and checks presented during the month.

Following much of the literature, we examine the dynamic relationship between treatment status and account switching by replacing the *Post* indicator with a series of event-time dummy variables $\{D_{-12}, \ldots, D_{12}\}$ which capture the months from 12 periods before to 12 periods after the reform. September 2021 is omitted and serves as the reference 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.

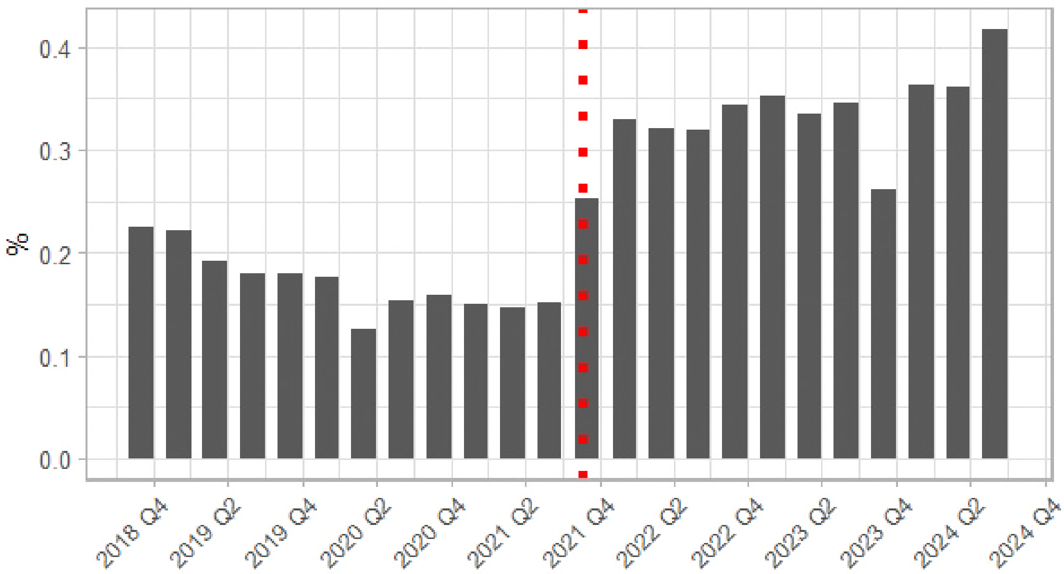
[14] In Section 5.5 we show that our results hold even when the switching period is allowed to be one or two months.

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 customers ID that switched a bank every quarter from 2018 Q4 through 2023 Q3.

Figure 2: Share of households that switched banks



Note: This figure reports the share of households that switched a bank in every quarter from 2018 Q4 to 2024 Q3. Dashed red line is in September 2021, the month the "Banking Mobility Reform" (BMR) was implemented.

The figure shows a sharp and persistent increase in switching immediately following the implementation of the reform. The quarterly switching rate rises from approximately 15 basis points before the reform to about 33 basis points afterward, implying that the three-year share of households that switched banks increased from roughly 1.8% to 4%.

The observed pattern does not, by itself, establish causality, as it cannot rule out the influence of unobserved time-varying factors. Moreover, even if the change is attributable to the reform, the increase in switching may reflect heightened customer awareness generated by extensive media coverage rather than a reduction in switching costs enabled by the online transfer system.

The following section evaluates the impact of the reform using a difference-in-difference empirical framework designed to identify its causal effect.

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 on *treat* is positive and significant among all specification, suggesting that even before the reform accounts without an outstanding loan were more likely to switch relative to control accounts. In the fully controlled specification (column 3), the pre-reform monthly switching probability is 27 basis points higher for accounts without an outstanding debt balance.

Our main coefficient of interest is the interaction between the *treat* and *Post* indicators, which captures the effect of the reform on switching behavior net of common time trends absorbed by the control group. This coefficient is positive, statistically significant, and stable across specifications. The estimates imply that following the implementation of the online transfer reform, the monthly probability of account switching increased by approximately 6.4 basis points. This magnitude is vary close to the raw increase in quarterly switching observed in Figure 2.

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.[15] Specifically, we add three dummy variables indicating whether the account has an available credit line, an outstanding overdraft balance, and a connected bank credit card.[16] 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

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

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

activity is indeed negatively correlated with the probability of account switching.

Table 2: Baseline estimations

	Switch		
	(1)	(2)	(3)
treat	4.584***	29.666***	27.529***
	(0.103)	(0.547)	(0.548)
treat × *Post*	5.410***	6.443***	6.438***
	(0.173)	(0.199)	(0.199)
Credit lim			−2.912***
			(0.665)
Overdraft			−1.572***
			(0.266)
Payment Order			0.011
			(0.037)
Credit Card			−14.858***
			(0.611)
Customer FE	N	Y	Y
Time FE	Y	Y	Y
Bank FE	Y	Y	Y
Obs.	19,526,719	19,526,719	19,526,719
R^2	0.0003	0.055	0.056

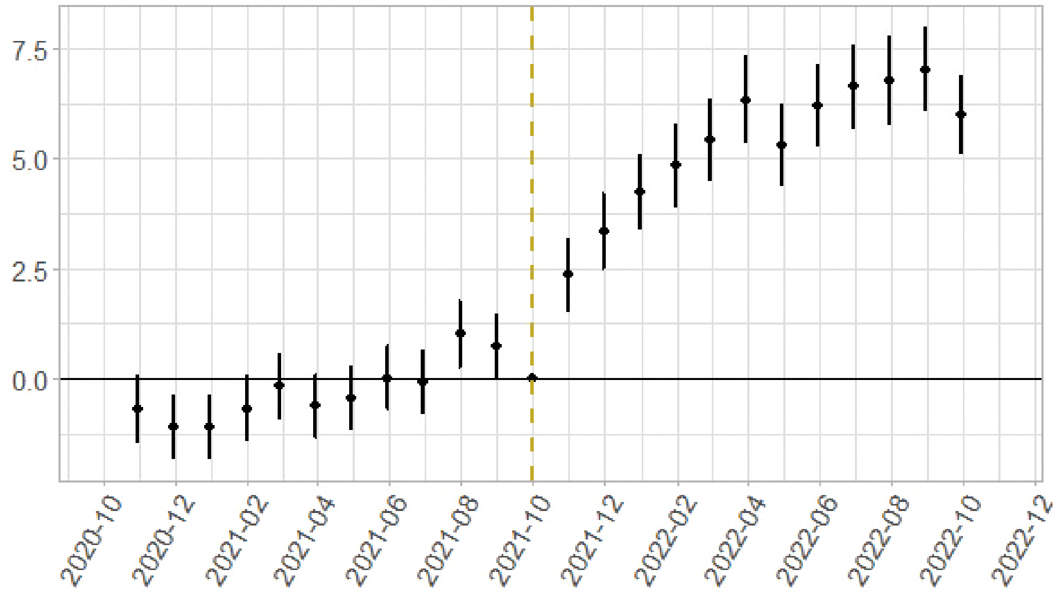
Notes: This table reports the coefficient estimates of Equation (1). All coefficients are scaled up by 10,000 to represent impact in basis points. Time period is October 2020 through September 2022. Standard errors, clustered at the bank level are reported in parenthesis. *p<0.1; **p<0.05; ***p<0.01

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.[17] We then see a strong effect, which is similar in size to the coefficient in Table 2. The effect remains until the end of the sample period, indicating that the reform brought about lasting changes in behavior or market dynamics, and had a deeper structural

[17] 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.

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 evaluates the robustness of our baseline results to alternative samples, definitions, and identification assumptions.

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. 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	8.924***	10.723***	9.303***
	(0.517)	(0.616)	(0.616)
treat × *Post*	11.727***	14.331***	14.292***
	(0.927)	(1.043)	(1.044)
Customer FE	N	Y	Y
Time FE	Y	Y	Y
Bank FE	Y	Y	Y
Controls	N	N	Y
Obs.	1 ,804,943	1,804,943	1,804,943
R^2	0.001	0.036	0.036

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. All coefficients are scaled up by 10,000 to represent impact in basis points. Time period is October 2020 through September 2022. *p<0.1; **p<0.05; ***p<0.01

5.2 Placebo tests

Another potential concern is that our results are driven by within-year seasonal patterns. To address this issue, we conduct placebo tests by estimating Equation (1) over two-year windows centered on September 2019, 2020, 2022, and 2023. For each placebo sample, the estimation mirrors the baseline specification in all respects, except that the *Post* indicator is redefined to capture months after September of the corresponding placebo year. Table 4 reports the results of these placebo tests using the same 20% customer ID sample as in the baseline analysis presented in Table 2. In addition, we estimate the dynamic specification in Equation (2) for each placebo period; the corresponding interaction coefficients and 95% confidence intervals are shown in Figure 4.

Table 4: Placebo tests

	Switch		
	(1)	(2)	(3)
Panel A: September 2019			
treat	6.458***	24.185***	20.068***
	(0.143)	(0.522)	(0.512)
treat × *Post*	−1.375***	−0.987***	−0.717***
	(0.185)	(0.195)	(0.195)
Obs.	18,214,998	18,214,998	18,214,998
R^2	0.0002	0.050	0.051
Panel B: September 2020			
treat	5.105***	20.473***	17.273***
	(0.119)	(0.473)	(0.463)
treat × *Post*	−0.487***	0.146	0.240
	(0.157)	(0.169)	(0.169)
Observations	18,968,168	18,968,168	18,968,168
R^2	0.0002	0.053	0.053
Panel C: September 2022			
treat	10.064***	49.145***	47.189***
	(0.141)	(0.685)	(0.693)
treat × *Post*	1.578***	4.364***	4.418***
	(0.208)	(0.234)	(0.235)
Obs.	20,112,771	20,112,771	20,112,771
R^2	0.0003	0.048	0.048
Panel D: September 2023			
treat	11.640***	58.508***	57.102***
	(0.156)	(0.763)	(0.777)
treat × *Post*	−0.050	3.397***	3.474***
	(0.222)	(0.247)	(0.247)
Obs.	19,822,534	19,822,534	19,822,534
R^2	0.0003	0.047	0.047
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

Across all panels of Table 4 the coefficient on *treat* is positive and statistically significant, consistent with treated accounts exhibiting higher switching probabilities than control accounts throughout the sample period.

By contrast, the interaction coefficient $treat \times Post$ shows no systematic evidence of an effect around the placebo dates. It is insignificant in the September 2020 placebo (Panel B) and negative with small magnitude in the September 2019 placebo (Panel A). While the September 2022 and 2023 placebos (Panels C and D) yield positive interaction coefficients, these effects are smaller than those observed for September 2021.

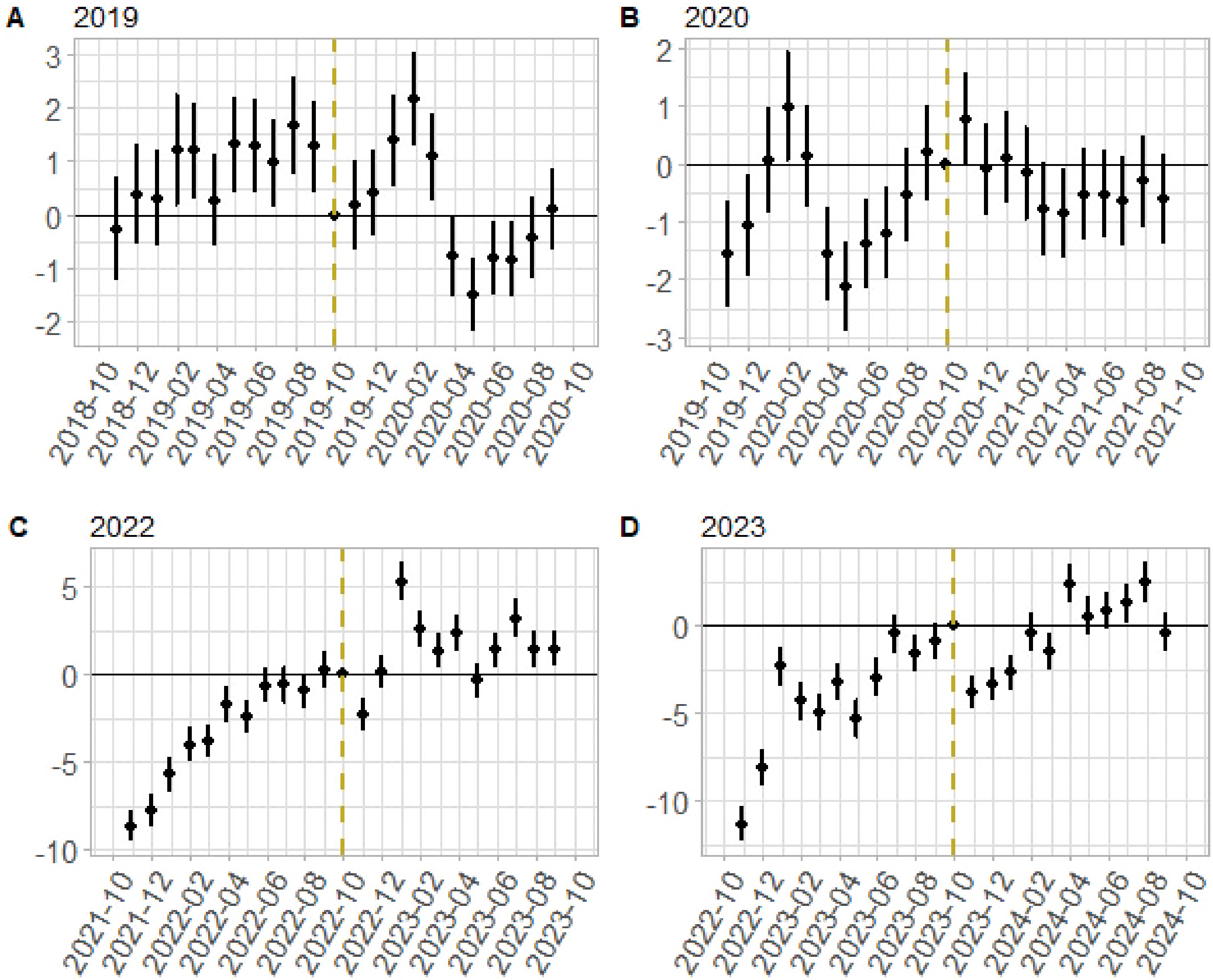
Figure 4 shows that none of the placebo specifications exhibits an event-time pattern comparable to that observed around the actual reform date. The positive coefficients in Panels C and D reflect a gradual upward trend in switching probabilities before the event dates. In Panel C in particular, this pattern is likely driven by the continued increase in switching probabilities following September 2020. Taken together, these results support the interpretation that the increase in switching observed in September 2021 is attributable to the reform rather than to seasonal effects or underlying trends.

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 more than one "customer" group (see footnote 9). 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 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

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 20% sample. 95% confidence bands computed using standard errors, clustered at the bank level, are also presented.

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	5.415***	32.087***	29.473***
	(0.136)	(0.690)	(0.691)
treat × *Post*	5.101***	6.097***	6.090***
	(0.219)	(0.255)	(0.255)
Customer FE	N	Y	Y
Time FE	Y	Y	Y
Bank FE	Y	Y	Y
Controls	N	N	Y
Obs.	13,597,619	13,597,619	13,597,619
R^2	0.0003	0.058	0.058

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.[18]

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 of the baseline specification. The estimated effect of the reform, captured by the interaction coefficient *treat* × *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

[18]It 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 period after the reform.

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	3.179***	20.559***	15.601***
	(0.101)	(0.988)	(1.017)
treat × *Post*	3.562***	4.193***	4.281***
	(0.163)	(0.179)	(0.179)
Customer FE	N	Y	Y
Time FE	Y	Y	Y
Bank FE	Y	Y	Y
Controls	N	N	Y
Observations	15,462,803	15,462,803	15,462,803
R^2	0.0002	0.063	0.063

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 reports the baseline specification estimated on the full database rather than the 20% random sample used previously. The coefficients are similar in magnitude and statistical significance to the baseline results, indicating that the findings are not driven by sampling variability and generalize to the full population.

Table 8 reports results from a specification that restricts the sample to a shorter window spanning six months before and six months after the reform. This specification serves two purposes. First, shows that the estimates are not sensitive to the choice of event window. Second, it limits the analysis 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 monetary tightening period, this specification reduces potential confounding effects related to monetary policy. The results remain consistent with the baseline findings, reinforcing the causal interpretation of the reform's impact.

Table 7: Baseline Estimation - Full Database

	Switch		
	(1)	(2)	(3)
treat	4.579***	28.982***	26.838***
	(0.046)	(0.241)	(0.241)
treat × *Post*	5.340***	6.319***	6.316***
	(0.077)	(0.089)	(0.089)
Customer FE	N	Y	Y
Time FE	Y	Y	Y
Bank FE	Y	Y	Y
Observations	97,627,408	97,627,408	97,627,408
R^2	0.0003	0.054	0.055

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

Finally, Table 9 reports results using alternative time windows to identify switching events. In the baseline specification, switching is defined as the closure of an account followed by the opening of a new account within a three-month window, reflecting the concern that prior to the reform customers may have maintained overlapping accounts for several months before fully transitioning. To show that the results are not sensitive to this choice, we re-estimate the model using a one-month window (Columns 1-3) and a two-month window (Columns 4-6). The estimated coefficients remain consistent across specifications, indicating that the findings are not sensitive to the choice of switching identification window.

Table 8: One year window

	Switch		
	(1)	(2)	(3)
treat	4.477***	32.926***	30.763***
	(0.140)	(0.916)	(0.928)
treat × *Post*	4.644***	5.172***	5.178***
	(0.236)	(0.250)	(0.250)
Customer FE	N	Y	Y
Time FE	Y	Y	Y
Bank FE	Y	Y	Y
Controls	N	N	Y
Obs.	9 ,747,745	9,747,745	9,747,745
R^2	0.0003	0.094	0.094

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

Table 9: Switching window

	$Switch^{1-months}$			$Switch^{2-months}$		
	(1)	(2)	(3)	(4)	(5)	(6)
treat	2.681***	20.015***	19.440***	3.731***	25.829***	24.503***
	(0.077)	(0.445)	(0.453)	(0.091)	(0.508)	(0.512)
treat × *Post*	4.664***	5.408***	5.397***	5.225***	6.131***	6.123***
	(0.140)	(0.163)	(0.163)	(0.158)	(0.184)	(0.184)
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	N	N	Y
Obs.	19,526,719	19,526,719	19,526,719	19,526,719	19,526,719	19,526,719
R^2	0.0003	0.053	0.053	0.0003	0.054	0.054

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 Switching behavior and household characteristics

Customer characteristics are likely to influence both the decision to switch banks and the extent to which individuals respond to the reform. Customers with higher socioeconomic status may have higher incomes and greater financial literacy, resulting in stronger incentives to optimize their financial arrangements and greater responsiveness to reductions in switching costs. By contrast, lower-income customers may face informational and digital barriers that discourage mobility and have lower expected gains from switching. Age may also play an important role: younger customers tend to be more flexible, technologically adept, and less entrenched in long-term banking relationships, whereas older customers may exhibit greater inertia due to habit formation, trust in their existing 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.[19]

Table 10 reports the results of the heterogeneity analysis. Differences between high- and low-socioeconomic groups are relatively small, both before and after the reform. This finding suggests either that socioeconomic status is not a key determinant of switching behavior or that our proxy is measured with noise.[20] In contrast, age emerges as an important source of heterogeneity. Younger customers were already more than twice as likely to switch banks prior to the reform (35 versus 14.9 basis points per month), and they experienced a substantially larger reform effect (8.7 versus 3.6 basis points). These results underscore the role of age in shaping responses to digitally enabled banking reforms.

[19] 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.

[20] The socioeconomic indicator is defined at the municipality level. In particular, large cities often contain substantial within-municipality heterogeneity, which may reduce the precision of this measure for part of the sample.

Table 10: Customer Heterogeneity

	Switch			
	Low Socio-Economic	High Socio-Economic	Low Age	High Age
	(1)	(2)	(3)	(4)
treat	28.472***	26.101***	35.059***	14.910***
	(0.710)	(0.862)	(0.788)	(0.665)
treat × *Post*	6.363***	6.586***	8.675***	3.557***
	(0.265)	(0.306)	(0.321)	(0.208)
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.	11,206,233	8,320,486	10,868,886	8,657,833
R^2	0.055	0.056	0.056	0.056

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

6.2 Account consolidation

While the primary objective of the banking mobility reform was to intensify competition by reducing switching frictions across banks, it also lowered the transaction costs associated with account consolidation. Specifically, the reform facilitated the transfer of activity from one bank account into an existing account at another institution, 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; Bank et al. 2023). 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 a 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 estimate Eq.(1) using $Merge$ as the dependent variable, defined as a binary indicator equal to one if account i of customer-ID j at bank k was closed at month t, and no other account was opened by that customer-ID within a three-month window. All other variables are defined as in the baseline specification. The results of this estimation are reported in Table 11. We also estimate the dynamic specification in Equation (2) with $Merge$ as the dependent variable; the corresponding interaction coefficients and 95% confidence intervals are shown in Figure 5.

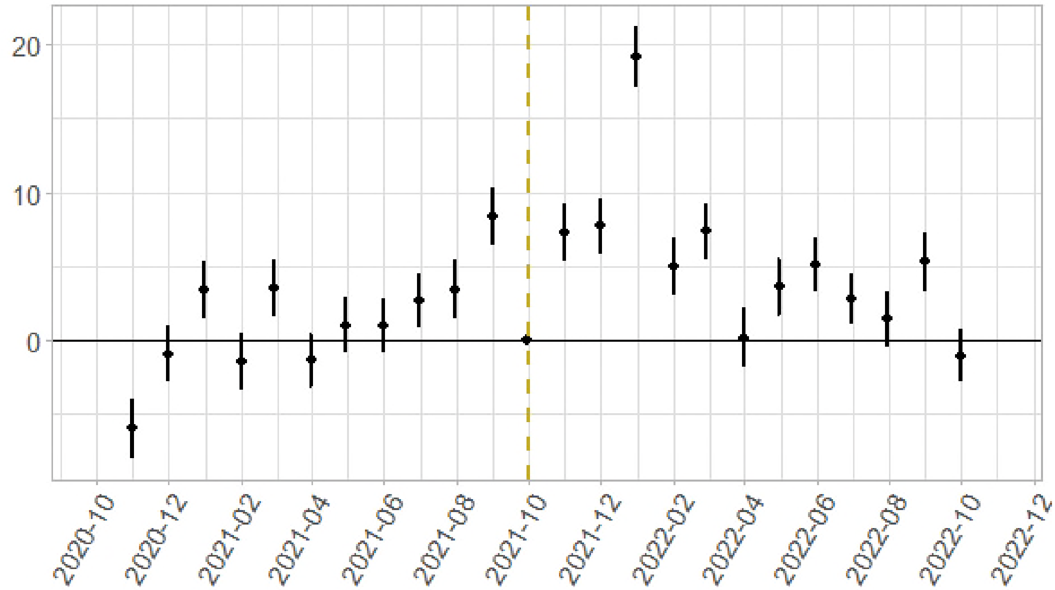
Table 11: Account Consolidation

	Merge		
	(1)	(2)	(3)
treat	22.393***	95.664***	61.760***
	(0.274)	(1.049)	(1.000)
treat × *Post*	−0.494	4.040***	4.233***
	(0.382)	(0.419)	(0.423)
Credit lim			−122.970***
			(1.537)
Overdraft			2.440***
			(0.445)
Payment Order			−6.840***
			(0.136)
Credit Card			−122.999***
			(1.389)
Customer FE	N	Y	Y
Time FE	Y	Y	Y
Bank FE	Y	Y	Y
Observations	19,526,719	19,526,719	19,526,719
R^2	0.001	0.086	0.093

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 11 shows that the coefficient on *treat* is positive and statistically significant, indicating that, as in the case of switching, account closures are more common for customer IDs without an active consumer loan. The interaction coefficient on $treat \times Post$ implies that, following the reform, the probability of account consolidation increased by approximately 4 basis points for treated accounts. However, Figure 5 reveals that this effect is short-lived and concentrated in the months immediately following the reform, most notably in December 2021. The dynamic estimates suggest that the increase in consolidation activity is temporary, consistent either with a transitory increase in customer attention to the ease of closing accounts or with a one-time adjustment toward a lower optimal number of accounts following the reform.

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.

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 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.

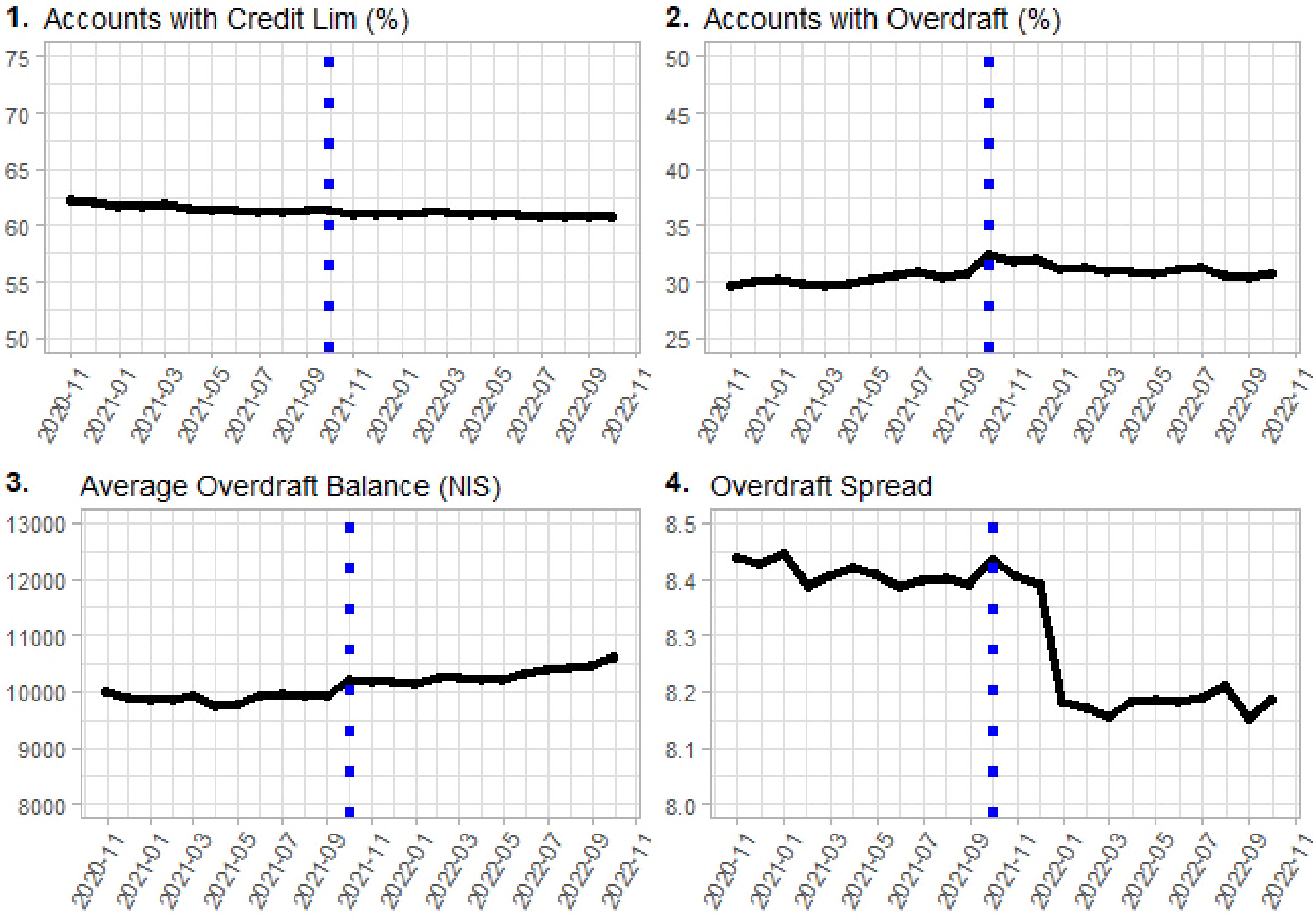
Due to data limitations, we are unable to observe fees or deposit rates directly. Instead, we examine changes in credit-related variables: specifically, credit line availability, size, utilization, and cost. Figure 6 presents trends in several credit line-related outcomes over time. Panels A through C show that the share of accounts with authorized overdraft credit lines, the share of accounts with utilized overdraft, 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 (Panel C).

In contrast, Panel D shows a sharp and immediate decline in the spread between overdraft interest rates and the Bank of Israel policy rate. This pronounced 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 substantial changes in credit access or utilization. Consistent with this interpretation, in January 2022 Israel Discount Bank – the fourth-largest bank in Israel – introduced interest-free overdrafts of up to NIS 2,000. This initiative received extensive media coverage and was followed by similar competitive responses from other banks seeking to attract customers. Local media explicitly linked these developments to the banking mobility reform.[21]

If these changes are driven by increased competitive pressures induced by the reform, we would expect credit-related outcomes to respond more strongly for accounts directly affected by the reform

[21] See, for example, *Globes*, "Discount Bank Offers Interest-Free Overdrafts," https://en.globes.co.il/en/article-discount-bank-offers-interest-free-overdrafts-1001397743).

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.

than for those that were not. This is because customers without an active consumer loan are in a stronger bargaining position with their bank than customers 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 $y_{i,j,k,t}$ denotes an account-level credit outcome. Specifically, we consider: (i) an indicator equal to one if the account has an available credit line; (ii) conditional on having a credit line, the logarithm of the credit limit; (iii) conditional on having a credit line, an indicator for overdraft utilization (i.e., a negative balance); (iv) conditional on overdraft utilization, the logarithm of the overdraft balance; and (v) conditional on overdraft utilization, the interest rate spread paid 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	-0.113^{***}	-0.111^{***}	-0.034^{***}	-0.279^{***}	0.077^{***}
	(0.001)	(0.003)	(0.001)	(0.005)	(0.011)
treat × *Post*	0.001^{***}	-0.031^{***}	-0.0002	0.030^{***}	-0.027^{***}
	(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.	19,526,719	11,983,929	11,983,929	4,350,926	4,350,926
R^2	0.869	0.933	0.611	0.659	0.859

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* × *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 and suggests that the observed effects on overdraft utilization and costs may reflect reduced switching frictions rather than alternative mechanisms.

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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