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Nonbank Lending Under Credit Registry Reform: Evidence from Israel’s Auto Loan Market

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Nonbank Lending Under Credit Registry Reform: Evidence from Israel's Auto Loan Market*

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Abstract

This paper examines how access to borrower-level credit information affects loan pricing and risk assessment. I focus on the auto loan market, which is dominated by nonbank financial institutions (NBFIs), and exploit the 2019 launch of Israel's national credit registry, which for the first time provided NBFIs with comprehensive borrower-level information. Using variation in registry usage across NBFIs, I implement a difference-in-differences design. Registry adoption reduced interest rate spreads by roughly 0.3 percentage points, with the effect exceeding one percentage point over time. Moreover, it weakened the link between pricing and loan-to-value ratios among highly leveraged loans, indicating a shift from collateral-based toward borrower-level risk pricing. Delinquency rates also declined relative to those of bank lenders. The results provide causal evidence of the impact of credit information in markets characterized by severe informational frictions, with implications for credit market regulation.

Keywords: Credit Registry, Consumer Credit, Information Asymmetry, Non-Bank Financial Institutions, Auto Loans, Risk-Based Pricing.

JEL Classification code: G21, G23, D82.

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מאגר נתוני האשראי והשפעתו על תמחור הלוואות רכב בשוק החוץ-בנקאי

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תקציר

מחקר זה בוחן כיצד גישה למידע על לווים משפיעה על תמחור אשראי ועל הערכת סיכון. המחקר מתמקד בשוק הלוואות הרכב בישראל, שבו פועלים בעיקר נותני אשראי חוץ־בנקאי, ונסמך על ניסוי טבעי: הקמתו של מאגר נתוני האשראי בישראל בשנת 2019, שסיפק לגופים אלה מידע מקיף על לווים ברמת הפרט. תוך שימוש בהבדלים בין נותני האשראי החוץ־בנקאי במידת השימוש בנתוני המאגר, מיושמת מתודולוגיית הפרש־ההפרשים (Difference-in-Differences). נמצא כי אימוץ המאגר הוביל לירידה ממוצעת של כ־0.3 נקודות אחוז במרווחי הריבית, שהעמיקה ללמעלה מנקודת אחוז לאורך זמן. ירידה זו לוותה בהיחלשות הקשר בין המחיר לבין שיעור המינוף של ההלוואה בקרב הלוואות במינוף גבוה, עדות למעבר מתמחור המבוסס על המינוף כמדד לסיכון, לתמחור מדויק יותר על בסיס מאפייני הסיכון של הלווה. במקביל, נרשמה ירידה יחסית בשיעורי הפיגור בתשלומים ביחס לאלה של הבנקים. הממצאים מספקים עדות סיבתית להשפעת מידע בשווקים המאופיינים בחיכוכי מידע משמעותיים, ותורמים להבנת תפקידם של מאגרי אשראי בעיצוב תחרות, תמחור וייעול הקצאת אשראי.

מילות מפתח: מאגר נתוני האשראי, אשראי חוץ-בנקאי, הלוואות רכב, הערכת סיכון, א-סימטריה במידע, בחירה שלילית, דירוג אשראי, תמחור מבוסס סיכון, הקצאת אשראי, תחרות בשוק האשראי.

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1. Introduction

Access to borrower-level credit information is essential for the efficient functioning of consumer credit markets. When lenders lack reliable data, credit allocation becomes inefficient, leading to adverse selection, moral hazard, and barriers to entry (Stiglitz and Weiss, 1981; Pagano and Jappelli, 1993; Padilla and Pagano, 1997; Jappelli and Pagano, 2006). These challenges have motivated the creation of credit infrastructures designed to reduce informational disparities and enable risk-based competitive lending. Although the theoretical literature on information sharing is extensive, systematic empirical evidence remains limited. Existing studies typically rely on proprietary data from a small number of lenders or focus exclusively on the banking sector. As a result, much less is known about how centralized information infrastructures affect the nonbank financial market, which has historically faced structural informational disadvantages relative to banks. Given the growing role of nonbank financial institutions (henceforth: NBFIs) in household credit markets,[1] understanding how they respond to improved data access is key to evaluating the broader effects of credit registries on market structure and credit allocation.

This paper contributes to filling that gap by examining how a reduction in informational asymmetry influences credit pricing and borrower screening in a market where informational frictions have long shaped the competitive landscape. I study a sharp, system-wide policy intervention that expanded information sharing across all consumer lenders: the 2019 rollout of Israel's centralized consumer credit registry, which provided standardized access to borrower-level data on outstanding debt and repayment histories. This reform generated an exogenous improvement in information availability that is particularly relevant for NBFIs, which had largely relied on internal relationship data and coarse risk proxies.

I focus on the auto loan market, a policy-relevant segment of Israeli consumer credit that accounts for approximately 25 percent of total consumer debt.[2] During the sample period, NBFIs originated nearly 80 percent of new auto loans, and auto lending represented the core of their consumer credit activity. Consequently, this market provides a natural and representative setting for studying how improved information affects NBFI behavior. The credit registry covers nearly the entire Israeli auto loan market, and the standardized structure of auto loans, with fixed interest rates, maturities, and collateral, further enables clean comparisons across lenders (Argyle et al., 2023). Importantly, registry usage among NBFIs was heterogeneous across lenders. Some lenders actively queried borrower credit reports to inform underwriting, while others did not utilize the data. This cross-lender variation in registry usage enables causal identification of the effects of information use on loan pricing and borrower screening within a difference-in-differences framework.

[1] See Acharya et al. (2024) for evidence on financial links between banks and NBFIs.

[2] By comparison, in the United States, auto loans represent the second largest household expenditure after housing, and constitute one of the largest segments of outstanding consumer credit (Federal Reserve Bank of New York, 2024).

The use of a credit registry may affect credit outcomes through several main channels. First, detailed credit histories, rather than coarse proxy variables, can improve underwriting efficiency by reducing misclassification in credit allocation, both by lowering the rejection of creditworthy borrowers who were previously indistinguishable from high-risk applicants, and by enabling lenders to more effectively screen out risky borrowers (Einav et al., 2013). Thus, credit registry data may mitigate adverse selection, consistent with the classic asymmetric-information models of credit markets (Akerlof, 1970; Stiglitz and Weiss, 1981). Second, enhanced access to borrower-level information may intensify competition between lenders by reducing reliance on soft or relationship-based proxies (Petersen and Rajan, 2002) and, particularly in the Israeli context, by enabling NBFIs to challenge the informational advantage traditionally held by banks. Finally, more accurate credit information may reduce overall financial distress, either by supporting risk-based pricing that allows lenders to offer better terms to safer borrowers or by increasing the reputational cost of default and strengthening repayment incentives (Vercammen, 1995; Behr and Sonnekalb, 2012).

To assess the effects of credit registry use, I begin by examining changes in loan pricing following the introduction of Israel's credit registry, a key channel through which improvements in screening and competition may operate. I find that treated NBFIs reduced interest rate spreads by roughly 30 basis points. To shed light on the sources of this effect, I then examine how the use of the credit registry altered the pricing and risk assessment practices of lenders. Specifically, I show that treated NBFIs became significantly less reliant on collateral when pricing high-LTV loans, consistent with a shift toward borrower-level risk assessment. This pattern is not observed among banks, which already had rich internal, relationship-based information on their borrower population before reform. I also document a relative decline in delinquency rates among loans issued by treated NBFIs compared to banks. This pattern is consistent with improved screening and borrower selection among lenders that had previously operated under informational disadvantages.

Overall, these results provide a policy evaluation of Israel's credit registry reform, shedding light on how expanded access to borrower-level information affected lending outcomes outside the banking sector, with implications for both pricing efficiency and borrowers' access to credit. While earlier work has documented the effects of the registry within banks (Bank et al., 2023), this paper offers the first market-wide evidence on its impact on NBFIs, a segment characterized by more severe informational frictions and explicitly targeted by the reform. More broadly, this paper is related to the literature on information sharing in credit markets. Previous research suggests that access to comprehensive borrower-level information can improve credit outcomes through several channels, including improved screening of high-risk borrowers, mitigation of adverse selection, and stronger borrower repayment incentives generated by reputational mechanisms. Using cross-country evidence, Jappelli and Pagano (2002) find that credit risk is lower in economies where lenders share information. Behr and Sonnekalb (2012) study the implementation of a credit registry in Albania and find that while it did not reduce loan pricing, it improved loan performance by encouraging repayment due to reputational concerns. In the context of auto lending, Einav et al. (2013) show that the adoption of credit scoring technology, which replaced personal interviews with a formal algorithmic screening process, led to improved borrower screening and higher profitability, amounting to approximately $1,000 per loan. Miller (2015) uses borrower-level data from a peer-to-peer lending platform to further demonstrate that superior information reduces default rates by enabling more accurate borrower selection.

Existing empirical studies typically examine improvements in information within a single lender or platform, or focus on the banking sector and the business lending segment. In contrast, this paper studies a policy-driven expansion of information that affected an entire market, using administrative data covering nearly the entire Israeli retail credit market, including all banks and major NBFIs. Importantly, a key advantage of Israeli credit registry data is the availability of detailed loan prices alongside rich borrower and loan characteristics, enabling a direct analysis of pricing responses to improved information. The policy-driven, market-wide expansion of information availability provides exogenous variation that allows causal inference on lender behavior.

This paper is also related to the literature on how information sharing affects lender competition. Theoretical work shows that in the absence of information sharing, lenders can extract informational rents by exploiting borrower-specific private information. Padilla and Pagano (1997) show that when information is not shared across lenders, incumbent institutions are able to sustain market power and charge higher interest rates in future periods. Sharpe (1990), in turn, emphasizes a complementary mechanism operating at the relationship level, whereby lenders initially offer favorable terms in order to accumulate soft, proprietary information about borrowers, which they later exploit through higher pricing as lending relationships mature.

Consistent with this mechanism, Bank et al. (2023) show that the introduction of a credit registry weakens relationship-based pricing advantages in the banking sector, reducing the premium lenders are able to extract from captive borrowers. More broadly, Petersen and Rajan (2002) find that improvements in the availability and processing of credit information in small business lending reduce lenders' reliance on local "soft" information, enabling banks to lend at greater geographic distances and to extend credit to riskier firms that might previously have been excluded. Building on this insight, this paper provides evidence that access to borrower-level data reshapes competitive dynamics through changes in pricing behavior and risk assessment among NBFIs.

The rest of the paper is organized as follows: Section 2 provides an institutional background on the Israeli credit registry and the auto loan market. Data construction and Descriptive statistics are provided in Section 3. Section 4 outlines the empirical methodology, followed by the main findings in Section 5. Section 6 concludes.

2. Institutional Background

I utilize administrative data from the Israeli credit registry, which contains monthly loan-level information on all consumer credit extended by both banks and most NBFIs.[3] The registry covers approximately 6.3 million individuals during the sample period and is considered near-comprehensive, as opt-out requests are exceedingly rare.[4]

For years, underwriting decisions in Israel relied primarily on banks' proprietary data and on negative credit records collected under the 2002 Credit Data Law, which focused on defaults and severe delinquencies in order to mitigate public risk. This narrow framework provided limited visibility into borrowers' broader credit behavior, particularly for NBFIs that lacked adequate information about their customers. By contrast, banks already possessed rich internal relationship data on their own customers, giving them a structural informational advantage over NBFIs prior to the reform. As a result, pricing was often imprecise, access was uneven, and borrowers seeking loans outside the banking sector faced systematically higher costs, as NBFIs charged higher rates to compensate for risk. To address these limitations, Israel enacted a far-reaching credit information reform anchored in the 2016 Credit Data Law. The reform's stated objectives were to enhance competition in the retail credit market, expand access to credit, and reduce discrimination in credit allocation.

In April 2019, a centralized credit registry became operational under Bank of Israel management, offering lenders access to standardized borrower-level reports. Unlike the earlier regime, the new registry includes both positive and negative data, such as repayment history, current balances, and unused credit lines. All lenders obtain borrower credit reports on a case-by-case basis through licensed credit bureaus, which generate scores based on data centrally collected and held by the Bank of Israel.[5]

Lenders, both banks and NBFIs, were only able to access credit reports and scores starting in April 2019, when the system was formally opened for use. In contrast to banks, which were required to report on all their retail borrowers starting in 2016, NBFIs were not subject to a general aggregation requirement. Instead, they became obligated to report data only if they met at least one of the following conditions: (a) they held more than NIS 250 million in outstanding retail credit; or (b) they chose to access credit reports, in which case they became subject to reciprocity rules, requiring them to submit borrower data to the registry.[6]

[3] Either because they exceed the regulatory threshold for mandatory reporting or because they opted into the system under the reciprocity rule, as detailed below.

[4] According to the Credit Data Law (2016), borrowers may request to opt out of the registry. As of March 2020—covering the entire sample period—fewer than 0.1% of eligible individuals had submitted such a request, indicating the registry's near-universal coverage. This may reflect borrowers' incentives to remain in the registry, as exclusion can signal risk and lead to higher loan costs. Notably, the law also limits opt-out eligibility for individuals with recent evidence of noncompliance with payment obligations. For details, see the official report to the Knesset Economic Affairs Committee, pursuant to Section 113 of the Credit Data Law (2016).

[5] For institutional details on the Israeli credit registry design, see Bank et al. (2023).

[6] See Sections 19(9) of the Credit Data Law and 3 of the Credit Data (Miscellaneous Provisions) Regulations pertaining to the first case, and Section 26 of the Credit Data Law (2016) and Supervisor's Directive No. 403 to the second.

The analysis focuses on auto loans, a distinct and policy-relevant segment within the Israeli credit market. Although auto loans comprise roughly 25% of total outstanding consumer credit, their institutional structure differs sharply from that of other retail credit markets. While the markets for general-purpose consumer loans and mortgages are highly concentrated among seven banking groups—two of which hold over 50% of balances—the vehicle finance market is dominated by NBFIs, which issue over 80% of new loans. In April 2019, when credit scores became available for the first time, approximately 80% of newly issued auto loans were already granted by NBFIs. This high share remained stable through March 2025, underscoring the sustained structural dominance of NBFIs in this segment. As in the broader consumer credit market, banks typically offer auto loans only to their own customers, while NBFIs extend loans to customers across all banks.

Auto loans are collateralized, with a single vehicle formally registered as security, unlike unsecured consumer loans. In the absence of additional borrower information, a key determinant of loan pricing in this market is the loan-to-value (LTV) ratio, which reflects the degree of borrower leverage. This is especially important in auto lending, where the collateral rapidly depreciates.[7]

3. Data and Descriptive Statistics

The sample ranges from April 2019, when Israel's centralized credit registry was launched, through February 2020. This construction provides a broadly balanced window before and after the onset of significant lender use of the registry, which began around September 2019 (see §4). The sample end date of February 2020 also helps avoid potential confounding from the COVID-19 outbreak, which in March 2020 led to a sharp decline in the provision of new loans.

As noted earlier, while banks were required to report data starting in 2016, NBFIs entered the registry depending on when they exceeded the mandatory reporting threshold[8] or opted in voluntarily through reciprocity (§2). To ensure that eligibility to use the registry is exogenous, NBFIs that joined under the reciprocity principle are excluded, as their decision to participate may have reflected business considerations correlated with loan pricing, which could confound the interpretation of the results. After this restriction, I focus on 13 NBFIs and 5 banks providing auto loans, covering the largest and most active players in the market during the sample period, as these were the institutions mandated to report.[9]

The treatment group comprises lenders who actively used the credit registry after implementation. Credit registry use is measured by the number of credit report inquiries submitted by each lender, based on monthly data reported to the registry. In contrast, the control group includes lenders who,

[7] In 2017, Israel's banking supervisor issued guidelines discouraging the use of collateral values for cars older than five years and recommending a maximum recognized LTV of 60%. In practice, however, both banks and NBFIs exhibit wide variation in LTV levels, with many loans exceeding 90.

[8] As noted above, annual retail credit exceeding NIS 250 million.

[9] As of January 2025, 23 NBFIs were reporting auto loan data to the registry, some of which joined on a reciprocity basis.

although subject to reporting obligations, did not submit any inquiries during the sample period, for various reasons such as slower adoption of the system or operational considerations. [10]

Importantly, the timing of credit report inquiries is not tied to the timing of lenders' initial reports to the registry. All treated NBFIs in the sample began submitting credit report inquiries already in April 2019, even if they started reporting to the registry at a later date.[11] Moreover, because all reporting NBFIs submitted a full inventory of their outstanding loans at the time of reporting, including loan issue dates and underwriting terms, it is possible to observe all active NBFIs' loans at that date. Consequently, the sample includes loans issued before each lender's reporting date, provided that they were outstanding at the common cutoff. This design makes it possible to capture all auto loans issued from April 2019 onward that were active at the time of observation.

Building on this structure, the sampling strategy is designed to ensure comparability across lenders by adopting a unified stock-based approach that provides a consistent identification methodology. To maximize coverage while maintaining comparability across groups, slightly different sampling rules are applied for the treatment and control groups. The following paragraphs first describe the construction of the main sample and then introduce the alternative samples used for robustness checks.

For the treatment group, the sample is constructed based on all auto loans reported as outstanding in September 2019—the latest reporting entry among treated lenders. This stock-based approach ensures that all treated lenders are observed at the same time. To further mitigate survivorship bias within lenders, the sample is restricted to loans with an original maturity of at least six months.[12] For the control group, the approach is analogous, using loans reported as outstanding in February 2022, the latest reporting entry among control-group lenders. To avoid excluding short-term loans originated after April 2019, the sample is restricted to loans with an original maturity of at least 34 months.[13] On average, the lifespan of an auto loan in Israel is approximately 4.5 years (§3.1), making it unlikely that these duration-based restrictions exclude a significant share of loans. These constraints are therefore applied mainly as a precaution.

As a robustness check, I construct two additional samples based on this stock-based methodology. The first includes only loans outstanding as of September 2019 and excludes control lenders that had not yet begun reporting by that date. The second includes all reporting lenders but restricts

[10] The absence of inquiries is unlikely to reflect a deliberate business strategy. Given that the cost of accessing credit reports is negligible, a plausible explanation is that nonusage primarily reflected operational considerations, such as delays in system integration or adjustments to internal workflows, especially given that these NBFIs did submit inquiries at some point during the sample period. However, even if one assumes that some NBFIs viewed their internal screening practices as sufficient and therefore chose not to adopt the registry, this interpretation will only strengthen the results. If nonusers, despite their confidence in internal screening, do not achieve lower loan spreads than registry users, this will highlight the added value of the registry.

[11] Under Section 26(a)(2) of the Credit Data Law (2016), lenders are permitted to obtain credit reports prior to the commencement of their mandatory reporting, subject to a formal undertaking to begin reporting within one year and to comply with the conditions specified in the law.

[12] This corresponds to the interval between the earliest loan start date (April 2019) and the treatment group cutoff (September 2019).

[13] This corresponds to the interval between the earliest loan start date (April 2019) and the control group cutoff (February 2022).

the sample to loans outstanding as of February 2022, thereby excluding shorter-duration loans. These alternative samples confirm that the main findings are robust to the choice of cutoff date and sample construction.

The analysis focuses exclusively on auto loans, defined as loans secured by a single vehicle collateral and linked to a single transaction in the credit register. This definition ensures a clean identification of auto loans[14] and excludes cases involving multiple collaterals or pooled loan arrangements.[15] It further excludes observations with a reported nominal annual interest rate of zero, as these likely reflect input errors, and index-linked variable-rate loans, which are rare in this setting. After applying these filters, the final main sample comprises 75,091 loans, including variable-rate unindexed loans, as well as fixed-rate loans, both indexed and unindexed.

The primary outcome variable is the interest rate spread (*Spread*), defined as the difference between the loan's annualized interest rate and a maturity-matched benchmark. For fixed-rate loans, the benchmark is a maturity-matched Israeli government bond, with real yields used for indexed loans and nominal yields used for unindexed loans. For variable-rate loans, which in this market are almost exclusively prime-linked, the spread is computed relative to the Bank of Israel's policy rate (the basis for the Israeli prime rate). This spread reflects the additional premium that borrowers pay over the alternative risk-free return available to lenders[16], and primarily captures the risk premium associated with borrower-specific uncertainty, as well as lenders' operating costs (e.g., underwriting, risk management). In addition, it may include a component related to lenders' market power. Access to the credit registry could reduce all of these elements: lowering risk premia by mitigating information frictions, streamlining operational costs through readily available borrower data, and potentially diminishing the market-power component by fostering competition.

The regressions in the analysis control for both loan-specific and borrower-specific characteristics. Loan controls include the principal amount ($Principal$) in thousands of shekels, contractual maturity in months ($Maturity$), the number of coborrowers, interest structure (fixed vs. variable; indexed vs. unindexed), and collateral-related measures, including the loan-to-value ratio (LTV), defined as the principal divided by the collateral's value (the vehicle).

Borrower-level controls include age group (AGE[17]), an indicator for mortgage ownership ($Mortg$), and the municipality-level socioeconomic rank (*Socio*), which ranges from 1 (lowest) to 10 (highest)

[14] Auto loans are identified based on the collateral type, without imposing restrictions on the declared loan purpose. Among NBFIs, most vehicle-backed loans are also labeled explicitly as car-purpose loans. Among banks, however, this field is frequently missing. Nevertheless, vehicle collateralization—which involves formal registration and a legal encumbrance—is typically undertaken in the context of vehicle purchases. Supporting this interpretation, such loans are classified as auto loans in banks' financial disclosures.

[15] In the sample, approximately 13% of auto loans involve more than one collateral, while 14% of vehicle collaterals appear in multiple loans. These cases are excluded from the analysis to ensure a one-to-one mapping between a loan and its underlying asset, as such cases are less likely to represent loans intended to finance a single car purchase.

[16] The spread is measured relative to the relevant government bond yield or policy rate, and therefore does not necessarily represent the margin over lenders' funding costs.

[17] Age is grouped into 14 brackets as defined by the credit registry. 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.

and is assigned based on the borrower's place of residence, following the Central Bureau of Statistics calculation. Following Bank et al. (2023), I construct an indicator for borrower risk (Bad_Hist), which equals one if the borrower had any adverse credit history in the past 12 months—including arrears of over 30 days on any loan, bounced checks, or open enforcement or insolvency proceedings.[18] I also control for each borrower's current outstanding balance ($Utilization$), measured in shekels and capturing the maximum utilization of the checking-account credit line within a given month, as well as the borrower's total available credit line ($Credit\_Lim$), i.e., the overdraft limit on the checking account. Although the credit registry does not contain direct measures of income or wealth, both the size of the available credit line and its utilization are generally positively correlated with them. To mitigate endogeneity concerns arising from the loan being granted within the same month, both variables are lagged by one month.

Additional controls include an indicator of multiple co-borrowers ($Many\_Borrowers$), which may proxy for repayment capacity; an indicator for whether the loan was the borrower's first consumer loan (New_Bor), capturing limited credit history following Bonfim and Soares (2018); and the number of other consumer loans taken by the borrower ($Consumer\_Loans$). I further construct an indicator for whether the loan represents the borrower's first loan with the same lender (New_Cust), capturing the absence of an established borrower–lender relationship.

Furthermore, to improve the precision of capturing the borrower's leverage, I construct an adjusted loan-to-value ratio (LTV_adj). Since part of the sample is constructed based on the stock of active loans at a given date, the reported collateral value for loans originated prior to that date reflects the depreciated value at the time of observation, rather than the original value at origination.[19] To address this, I estimate the empirical depreciation curve for each vehicle, using observed price paths, and recover the likely value at issue. This correction reduces potential bias in the LTV-based controls and enhances the comparability between different loans.

Finally, *Delinquency* is defined as an indicator for whether the auto loan in the sample entered delinquency status, based on credit registry records. A loan is considered delinquent if it is reported as in default or under legal action, with at least 30 days of payment delay and a missed payment exceeding NIS 200.[20]

[18]For loans with multiple coborrowers, I assign borrower-level variables as follows: I use the lowest socioeconomic rank, the highest credit line, and the highest outstanding balance among borrowers. A borrower is flagged with adverse credit history if at least one coborrower meets the criteria. Similarly, mortgage status equals one if any co-borrower holds a mortgage. Robustness checks using alternative aggregation methods (e.g., averages or maxima) yield similar results.

[19]This adjustment affects only a minority of observations drawn from the stock sample, and the potential bias is restricted to a short time window.

[20]For this variable, I use the September subsample, in which all loans are consistently reported from that point onward. This allows me to observe delinquency starting from a stable reference date. To ensure a consistent observation window, only loans for which delinquency, if any, is first observed at least five months after origination are included (as some loans in the sample were issued as early as April 2019). I track delinquency status over a five-year horizon from origination. Since loans in the sample were issued no later than February 2020, their repayment behavior can be observed through February 2025. This design ensures comparability across all loans in the delinquency analysis.

3.1. Descriptive Statistics

Table 1 reports descriptive statistics for the full sample, disaggregated by institutional type. The table highlights structural differences between banks and NBFIs, including differences in information environments and lending practices.

Throughout the sample period, NBFI lenders issued nearly all loans (a stable share of about 98 percent) at fixed interest rates. In contrast, bank lending relied predominantly on variable-rate contracts, which are linked to the prime rate, and the share of fixed-rate loans declined markedly over time.[21] Loan sizes are consistently larger for bank-issued loans. Across periods, the weighted mean principal is about NIS 103,000 for bank loans, compared to about NIS 77,000 among NBFIs. Bank loans also exhibit higher leverage on average, with LTV ratios reaching approximately 0.83, compared to about 0.75 for loans issued by NBFIs.

Borrower characteristics further differentiate lending between institutional types. On average, bank borrowers are more likely to have an outstanding mortgage and have higher available credit through their checking accounts, with an average $Credit\_Lim$ of roughly NIS 32,000, compared to approximately NIS 25,500 among NBFI borrowers, while the median utilization of available credit limits is slightly higher among NBFI borrowers. Bank borrowers also tend to belong to somewhat higher socioeconomic groups and exhibit marginally lower rates of adverse credit history. Overall, these patterns point to systematic differences in observable borrower risk, consistent with higher baseline credit risk on average among NBFI borrowers.

At the same time, differences in borrower-lender relationships are particularly salient. NBFIs disproportionately extend credit to borrowers who are new to the institution itself: on average, roughly three-quarters of NBFI loans are issued to first-time customers of the lender (New_Cust), compared to about one-quarter among banks. These borrowers, however, typically exhibit active credit usage elsewhere. Borrowers served by NBFIs hold a larger number of concurrent consumer loans on average ($Consumer\_Loans$) and are less likely to be taking their first consumer loan (New_Bor), indicating higher credit intensity rather than borrower inexperience.

Taken together, these features characterize the institutional conditions under which NBFIs have limited scope to accumulate relationship-based information. Before the introduction of the credit registry, NBFI lending therefore took place in the absence of both internal relationship data and comprehensive external credit information. This combination may give rise to adverse selection and "lemons" dynamics, as lenders face difficulty distinguishing high-quality from low-quality borrowers ex-ante, discouraging lower-risk borrowers and resulting in a borrower pool tilted toward higher-risk types, as reflected in the observed differences in borrower characteristics. The descriptive patterns

[21] The average spread among bank loans decreased from 4.57 percentage points before the registry to 2.08 after its introduction. This decline partly reflects a compositional shift toward prime-linked loans. Given the substantial variation in spreads across interest rate types and indexation, the regression analysis includes fixed effects for both interest rate structure and indexation type.

thus underscore why access to centralized borrower-level information can be particularly valuable for NBFIs.

Table 2 compares the NBFIs that actively used the credit registry (treatment) with those that did not (control). These groups constitute the treatment and control samples used in the empirical analysis. While treated lenders issue slightly larger loans on average and serve borrowers with somewhat higher credit limits and utilization, differences in leverage, maturity, and other observable borrower characteristics are modest. These descriptive patterns support the comparability of treated and control lenders after accounting for lender fixed effects and borrower and loan-level controls. They also help alleviate concerns that any estimated effects are driven by differential changes in borrower composition or credit exposure rather than by registry usage, after accounting for the borrower and loan controls included in the regression analysis.

Table 3 presents descriptive statistics for robustness samples constructed in a uniform way for both groups. These samples are designed to further assess whether the main results are sensitive to alternative sample construction choices. The first robustness sample is based on the September 2019 loan stock and includes only lenders that had begun reporting by that date. While this stock covers a smaller set of lenders, it captures a broad cross-section of outstanding auto loans, as it retains all loans originated since April 2019 that remain active as of September 2019. Given typical auto loan maturities, loans that fully amortize within five months are rare, and this restriction is therefore unlikely to materially affect borrower composition. The second robustness sample is based on the February 2022 loan stock, which includes all reporting lenders but mechanically excludes shorter-duration contracts by construction, as only loans still outstanding at that date are observed.

Across both samples, borrower and loan characteristics remain broadly stable between the pre and post periods. As expected, the average contractual maturity in the February sample is higher by construction, reflecting the restriction to loans outstanding at that date, while other borrower and loan characteristics are comparable in magnitude and dispersion to those observed in the main sample.

Table 1: Descriptive Statistics by Institution and Period

Panel A: Pre Period

	Bank				**Nonbank**			
	n	*Mean*	*SD*	*Median*	*n*	*Mean*	*SD*	*Median*
Spread (%)	7764	4.57	3.89	3.25	25494	8.42	2.44	8.82
Maturity (Month)	7764	54.13	19.36	59.00	25494	54.56	20.98	60.00
LTV	7764	0.78	0.28	0.78	25494	0.75	0.20	0.78
LTV_adj	7764	0.78	0.28	0.78	25494	0.75	0.21	0.77
Principal (Th. NIS)	7764	95.17	63.55	80.95	25494	75.73	50.32	64.00
Bad_Hist	7764	0.08	0.27	0	25494	0.08	0.27	0
Socio	7764	5.40	2.16	5	25494	5.02	2.10	5
Mortg	7764	0.36	0.48	0	25494	0.29	0.45	0
Consumer_Loans	7764	2.82	7.91	2	25494	3.16	6.25	2
Credit_Lim (Th. NIS)	7764	27.50	39.8	15	25494	23.95	36.87	15
Utilization (Th. NIS)	7764	20.56	43.01	6.25	25494	19.83	40.91	7.95
Borrowers	7764	1.23	0.42	1	25494	1.09	0.29	1
New_Bor	7764	0.24	0.43	0	25494	0.16	0.37	0
New_Cust	7764	0.20	0.40	0	25494	0.80	0.40	1
Age	7764	7.15	2.60	7	25494	6.87	2.56	7
Fixed Loans (%)	35.8%				97.9%			

Panel B: Post Period

	Bank				**Nonbank**			
	n	*Mean*	*SD*	*Median*	*n*	*Mean*	*SD*	*Median*
Spread (%)	7148	2.08	2.54	0.60	34685	8.60	2.45	9.09
Maturity (Month)	7148	52.83	15.75	59.50	34685	54.16	20.87	60.00
LTV	7148	0.84	0.31	0.85	34685	0.75	0.20	0.78
LTV_adj	7148	0.84	0.31	0.85	34685	0.76	0.20	0.78
Principal (Th. NIS)	7148	111.96	66.24	100.00	34685	77.95	52.84	64.00
Bad_Hist	7148	0.06	0.25	0	34685	0.09	0.28	0
Socio	7148	5.83	2.10	6	34685	4.98	2.13	5
Mortg	7148	0.42	0.49	0	34685	0.28	0.45	0
Consumer_Loans	7148	2.77	9.82	1	34685	3.25	6.39	2
Credit_Lim (Th. NIS)	7148	31.92	44.61	20	34685	24.52	39.23	14
Utilization (Th. NIS)	7148	20.93	44.84	5.6	34685	20.66	43.13	7.75
Borrowers	7148	1.35	0.48	1	34685	1.09	0.29	1
New_Bor	7148	0.28	0.45	0	34685	0.16	0.37	0
New_Cust	7148	0.26	0.44	0	34685	0.77	0.42	1
Age	7148	7.39	2.64	7	34685	6.78	2.57	6
Fixed Loans (%)	8.4%				97.4%			

Table 2: Descriptive Statistics by Treatment Status and Period

Panel A: Pre Period

	Control				**Treatment**			
	n	*Mean*	*SD*	*Median*	*n*	*Mean*	*SD*	*Median*
Spread (%)	2115	8.20	2.81	8.78	23379	8.44	2.41	8.83
Maturity	2115	54.67	10.15	60.00	23379	54.55	21.69	60.00
LTV	2115	0.76	0.18	0.78	23379	0.75	0.21	0.77
LTV_adj	2115	0.76	0.21	0.77	23379	0.75	0.21	0.77
Principal (Th. NIS)	2115	59.24	28.13	55.00	23379	77.23	51.60	64.00
Bad_Hist	2115	0.10	0.30	0.00	23379	0.08	0.27	0.00
Socio	2115	5.11	2.03	5.00	23379	5.01	2.11	5.00
Mortg	2115	0.29	0.45	0.00	23379	0.29	0.45	0.00
Consumer_Loans	2115	2.74	5.73	2.00	23379	3.20	6.29	2.00
Credit_Lim (Th. NIS)	2115	20.50	29.56	12.00	23379	24.27	37.45	15.00
Utilization (Th. NIS)	2115	16.25	30.36	6.20	23379	20.15	41.72	8.10
Borrowers	2115	1.11	0.32	1.00	23379	1.09	0.29	1.00
New_Bor	2115	0.20	0.40	0.00	23379	0.16	0.37	0.00
New_Cust	2115	0.98	0.13	1.00	23379	0.78	0.41	1.00
Age	2115	6.95	2.57	7.00	23379	6.87	2.56	7.00

Panel B: Post Period

	Control				**Treatment**			
	n	*Mean*	*SD*	*Median*	*n*	*Mean*	*SD*	*Median*
Spread (%)	2903	8.57	2.26	8.83	31782	8.60	2.47	9.14
Maturity	2903	54.90	10.13	60.00	31782	54.09	21.59	60.00
LTV	2903	0.74	0.18	0.76	31782	0.75	0.20	0.79
LTV_adj	2903	0.75	0.21	0.76	31782	0.76	0.20	0.79
Principal (Th. NIS)	2903	58.16	27.50	52.50	31782	79.76	54.21	65.00
Bad_Hist	2903	0.09	0.28	0.00	31782	0.08	0.28	0.00
Socio	2903	5.04	2.03	5.00	31782	4.97	2.14	5.00
Mortg	2903	0.26	0.44	0.00	31782	0.28	0.45	0.00
Consumer_Loans	2903	2.87	9.81	2.00	31782	3.29	5.98	2.00
Credit_Lim (Th. NIS)	2903	20.66	34.33	11.00	31782	24.87	39.63	14.60
Utilization (Th. NIS)	2903	16.91	37.82	6.15	31782	21.01	43.57	7.90
Borrowers	2903	1.12	0.33	1.00	31782	1.09	0.29	1.00
New_Bor	2903	0.20	0.40	0.00	31782	0.16	0.37	0.00
New_Cust	2903	0.91	0.28	1.00	31782	0.76	0.43	1.00
Age	2903	6.68	2.51	6.00	31782	6.79	2.58	6.00

Table 3: Robustness Samples: September 2019 and February 2022

Panel A: September 2019 Loan Stock

	Pre				**Post**			
	n	*Mean*	*SD*	*Median*	*n*	*Mean*	*SD*	*Median*
Spread (%)	26,205	8.47	2.46	8.93	35,313	8.62	2.46	9.13
Maturity	26,205	54.23	20.99	60.00	35,313	53.90	20.90	60.00
LTV	26,205	0.75	0.20	0.77	35,313	0.75	0.19	0.78
LTV_adj	26,205	0.75	0.20	0.77	35,313	0.75	0.19	0.78
Principal (Th. NIS)	26,205	75.17	49.96	62.10	35,313	77.39	52.62	63.00
Bad_Hist	26,205	0.08	0.27	0.00	35,313	0.08	0.27	0.00
Socio	26,205	5.01	2.10	5.00	35,313	4.96	2.12	5.00
Mortg	26,205	0.29	0.45	0.00	35,313	0.27	0.45	0.00
Consumer_Loans	26,205	3.15	6.20	2.00	35,313	3.24	6.34	2.00
Credit_Lim (Th. NIS)	26,205	23.52	35.19	14.00	35,313	24.01	36.97	13.60
Utilization (Th. NIS)	26,205	19.29	37.31	7.85	35,313	19.96	38.80	7.60
Borrowers	26,205	1.09	0.29	1.00	35,313	1.09	0.29	1.00
New_Bor	26,205	0.16	0.37	0.00	35,313	0.16	0.37	0.00
New_Cust	26,205	0.80	0.40	1.00	35,313	0.77	0.42	1.00
Age	26,205	6.85	2.55	6.00	35,313	6.76	2.57	6.00

Panel B: February 2022 Loan Stock

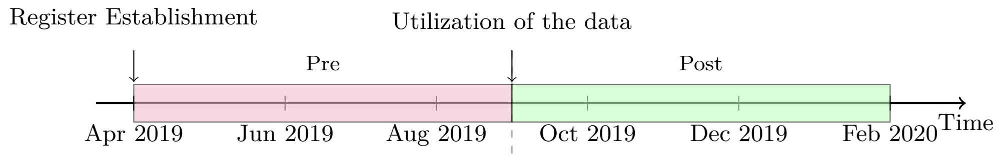
	Pre				**Post**			
	n	*Mean*	*SD*	*Median*	*n*	*Mean*	*SD*	*Median*
Spread (%)	11,182	7.88	2.74	8.53	14,249	7.97	2.81	8.69
Maturity	11,182	57.42	19.78	60.00	14,249	56.86	19.13	60.00
LTV	11,182	0.78	0.19	0.80	14,249	0.79	0.18	0.80
LTV_adj	11,182	0.78	0.21	0.79	14,249	0.79	0.19	0.80
Principal (Th. NIS)	11,182	80.43	49.30	67.30	14,249	84.83	53.51	70.10
Bad_Hist	11,182	0.09	0.28	0.00	14,249	0.09	0.29	0.00
Socio	11,182	5.16	2.11	5.00	14,249	5.13	2.15	5.00
Mortg	11,182	0.30	0.46	0.00	14,249	0.30	0.46	0.00
Consumer_Loans	11,182	3.05	5.16	2.00	14,249	3.38	7.29	2.00
Credit_Lim (Th. NIS)	11,182	24.60	36.87	15.00	14,249	26.57	42.17	15.00
Utilization (Th. NIS)	11,182	20.17	41.68	8.06	14,249	22.45	47.25	8.45
Many_Borrowers	11,182	1.10	0.30	1.00	14,249	1.11	0.31	1.00
New_Bor	11,182	0.17	0.37	0.00	14,249	0.16	0.37	0.00
New_Cust	11,182	0.84	0.36	1.00	14,249	0.80	0.40	1.00
Age	11,182	7.11	2.56	7.00	14,249	7.02	2.57	7.00

4. Empirical Methodology

The empirical methodology is designed to evaluate the impact of an information shock on auto loan pricing, focusing on NBFIs, which had a structural disadvantage in their access to borrower information prior to the introduction of the registry.

Although all NBFIs in the sample were legally required to report to the registry and were formally eligible to access credit reports, practical adoption required internal adjustments, including technical integration and staff training. In practice, several lenders had not yet begun to use registry data during the sample period.[22] Loans issued by these institutions serve as a credible counterfactual for loans issued by institutions that use the registry in the difference-in-differences analysis.

Based on conversations with industry participants, full implementation was typically achieved by September 2019. To capture this shift in usage, I define September as the effective treatment onset and construct a symmetric sample window of five months before and after, spanning April 2019 to February 2020, while avoiding confounding from COVID-related disruptions.



This timeline is supported by administrative data, which show a sharp increase in the ratio of credit report inquiries to the number of auto loans[23] between April and July 2019, followed by a stabilization in subsequent months, suggesting a gradual adoption process (see Figure 1). Consistent with this pattern, the event study estimates (see §5) indicate that pricing effects emerged only in the final quarter of 2019. Similar temporal lags are well documented in prior research. For example, Einav et al. (2013) note that although a formal policy was introduced at a fixed date, behavioral adoption evolved gradually, and Miller (2015) finds delays between registry access and measurable changes in market outcomes.

I estimate the following difference-in-differences specification:

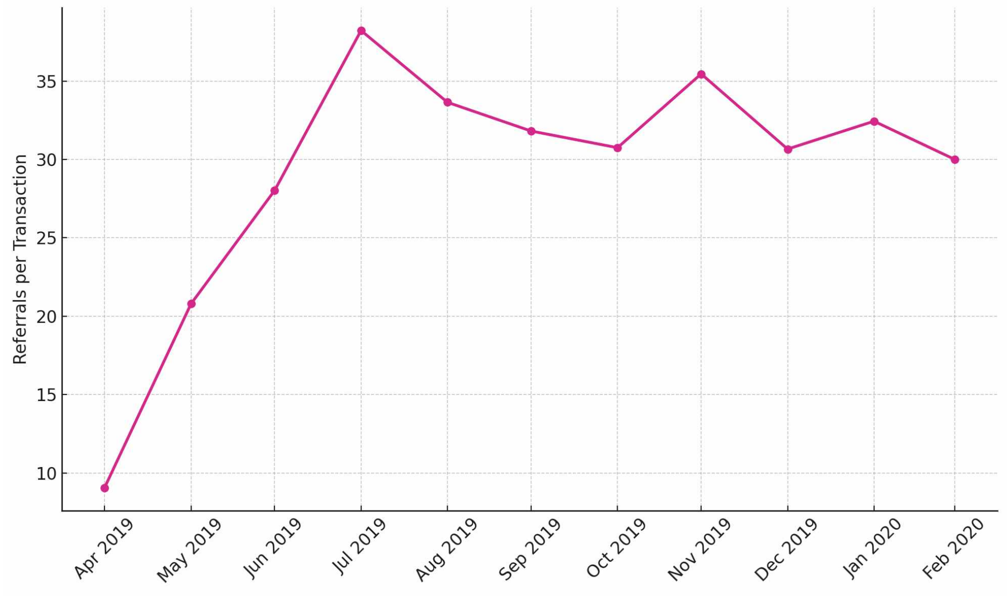
$$Spread_{i,j,t} = \alpha + \beta_1 \cdot (Post_t \times Treat_j) + \beta_2 \cdot X_i + \gamma_j + \delta_t + \varepsilon_{i,j,t}, \tag{1}$$

where $Spread_{i,j,t}$ is the interest rate spread for loan i, issued by lender j in month t, defined as the difference between the loan's annualized rate and an appropriate benchmark (see §3). $Post_t$ is an indicator for the post-treatment period (September 2019 onward), and $Treat_j$ indicates whether

[22] The absence of inquiries is unlikely to reflect a deliberate business strategy. It likely reflects operational considerations, such as system integration and workflow adjustments, given that these institutions are subject to reporting requirements and that the cost of credit reports is negligible. Even if some NBFIs had a business rationale for not using the data, believing that their internal screening practices were sufficient, the results are only strengthened. If nonusers, despite their confidence, do not offer lower loan spreads than registry users, it highlights the added value of the registry.

[23] The inquiries shown are those of the treated group, as control lenders made no credit report inquiries during this period.

Figure 1. : Ratio of credit report inquiries to number of auto loans as a function of time.



lender j began using the credit registry for underwriting decisions. $\beta_2 \cdot X_i$ represents a vector of loan- and borrower-level covariates. γ_j and δ_t, are lender and calendar month fixed effects, respectively. Standard errors are clustered at the lender level throughout all estimations. My coefficient of interest is β_1, which captures the differential change in pricing among lenders who used the registry relative to those who did not.[24] I focus on the interest rate spread, rather than on the nominal interest rate itself, as it provides a more consistent basis for comparing loan pricing across contracts. Although my sample period is relatively short and characterized by stable monetary policy, spreads remain essential for controlling for variation in loan terms and structures, particularly differences in maturity, indexation, and rate type. By netting out the relevant benchmark yield, the spread captures the effective risk premium applied by lenders. In the regression analysis, I additionally control for interest rate structure and indexation to account for any remaining variation in pricing terms.

To assess the credibility of my identifying assumptions and explore the dynamics of the treatment effect, I complement Equation (1) with an event study specification. In particular, I interact the treatment group indicator ($Treat$) with a set of calendar month dummies. This approach allows me to test the parallel trends assumption, which is that without the registry's adoption, the pricing gap between treated and control lenders should remain stable in the pretreatment period, with systematic divergence emerging only after the practical integration of registry use. Moreover, I note that the timing of registry usage was neither publicized nor easily predictable.[25] Therefore,

[24]Because identification relies on differential registry usage across lenders, the estimates capture the relative effect among users versus nonusers. If registry availability also altered market-wide competitive conditions, including among nonusers, the estimates may represent a lower bound on the reform's overall impact.

[25]Furthermore, data from Ifat Media on Financial Advertising indicate no changes in Bank of Israel communications or advertising regarding the registry around the time when credit report inquiries began to increase.

strategic manipulation of loan characteristics or borrower composition around the treatment period is unlikely.

In order to ensure that my results are not sensitive to the data construction method, I apply a unified sample construction approach across all lenders. I restrict the sample to loans observed in the stock as of September 2019 (the treatment reference point) and originally issued with a maturity of at least six months (see §3). While this restriction reduces the number of participating lenders, it is unlikely to induce systematic selection in the sample, since auto loans are typically long-term in nature. Complementarily, I construct a parallel sample based on February 2022, aligned with the control group (nonusers of the registry), and restrict the sample to loans with maturities of at least 34 months to minimize survivor bias. This specification retains the full set of lenders but includes only long-duration loans.

I further test the sensitivity of the results to defining September 2019 as the treatment date, which corresponds to the onset of meaningful registry usage. Any limited usage prior to this date within the baseline period would mechanically attenuate the estimated treatment effects rather than bias them upward. I nevertheless estimate the key specifications using July 2019 as an alternative treatment date, consistent with the observed inflection point in inquiry activity (see Figure 1). Furthermore, I conduct an additional robustness exercise based on a "clean window" design. Specifically, I compare outcomes in a strictly preregistry period (October 2018 through March 2019), when no lender could access registry data, to those in a strictly post-implementation period (September 2019 onward), when registry usage had already become operational among treated lenders. I exclude the intermediate months between April and August 2019, which correspond to the learning and adjustment phase documented in the inquiry data. This design eliminates contamination of the pretreatment period by early users and ensures that the post-treatment period reflects stable operational usage.[26]

In addition, I address potential bias arising from measurement error in collateral values by introducing an adjusted LTV measure that estimates collateral values at origination (see §3).

I extend the analysis to explore the mechanisms through which the credit registry may have influenced loan pricing, focusing on improved risk assessment and subsequent loan performance.

4.1. Pricing Sensitivity to LTV

If improved credit information enables lenders to more accurately assess borrower risk, I expect their reliance on observable proxies, such as LTV, to decline. In particular, the sensitivity of loan pricing to LTV should weaken after registry adoption, as lenders shift toward more data-driven

[26] I rely on the stock of loans reported at each lender's registry entry date to identify loans issued from October 2018 onward. Extending the window further back in time reduces the usable sample and increasingly limits it to loans that remained active at the reporting date. I therefore treat this specification as a robustness exercise rather than my baseline, and expect the estimates to be noisier.

assessments of borrower quality. Because I analyze secured auto loans, pricing tends to be less sensitive to LTV at low leverage levels, where collateral coverage is ample and expected loss given default is small. Loan risk becomes economically meaningful as leverage rises. I therefore restrict the LTV sensitivity tests to loans with $LTV > 0.7$, focusing the analysis on loans where credit risk is more material.[27]

I begin by estimating the following model on the treated set within the $LTV > 0.7$ subsample, including both banks and NBFIs that adopted the registry:

$$Spread_{ijt} = \alpha + \beta_1 LTV_{ijt} + \beta_2 \left(Post_t \times LTV_{ijt}\right) + \mathbf{X}'_{ijt}\gamma + \delta_t + \mu_j + \varepsilon_{ijt}. \tag{2}$$

Banks and NBFIs differ in baseline access to borrower information, so pooling can obscure treatment effects. I therefore estimate Equation (2) for banks separately. Because all banks adopted the registry simultaneously, they serve as a benchmark without cross-sectional treatment variation. Banks already held extensive borrower information prior to the reform, so the registry added little new risk information. Accordingly, I expect the interaction coefficient on $Post_t \times LTV_{ijt}$ to be close to zero.

To identify the effect for NBFIs, I exploit adoption variation and estimate a triple-differences model within the $LTV > 0.7$ subsample:

$$Spread_{ijt} = \alpha + \beta_1 \left(Treat_j \times Post_t\right) + \beta_2 LTV_{ijt} + \beta_3 \left(Treat_j \times Post_t \times LTV_{ijt}\right) + \mathbf{X}'_{ijt}\gamma + \delta_t + \mu_j + \varepsilon_{ijt}. \tag{3}$$

Here, $Treat_j$ indicates whether lender j adopted registry usage. The coefficient β_3 captures the differential post-adoption change in the LTV–spread slope for treated NBFIs relative to untreated NBFIs. A negative β_3 indicates reduced reliance on LTV as a risk proxy in the high-LTV range, consistent with improved risk-based pricing driven by enhanced borrower-level information.

4.2. Delinquency Rates

As discussed above (see §1), registry data may improve borrower selection prior to loan origination, either by helping lenders identify safer borrowers or by allowing them to more effectively exclude riskier applicants, and strengthen borrowers' incentives to repay once the loan is issued.

To assess whether either of these mechanisms affects loan performance, I examine delinquency rates as an ex-post outcome. I estimate the following difference-in-differences specification, comparing treated NBFIs to banks:

$$Delinq_{ijt} = \alpha + \theta_1 NBFI_j \times Post_t + \mathbf{X}'_{ijt}\gamma + \delta_t + \mu_j + \varepsilon_{ijt}. \tag{4}$$

[27] The 0.7 cutoff is below both the mean and median LTV across groups, see Section 3.

Here, $Delinq_{ijt}$ is an indicator equal to one if loan i becomes delinquent (see §3). A negative and significant θ_1 would indicate that delinquency rates declined more among NBFIs than among banks following registry adoption.

5. Main Results

5.1. Effect on pricing

Table 4 presents the results from estimating Equation (1). The main coefficient of interest, β_1, corresponding to the interaction term $Post \times Treat$, is negative and statistically significant at the 5% level. This coefficient represents the causal effect of credit registry usage on loan pricing. The estimated effect implies a decline of 0.31 percentage points in loan spreads charged by treated lenders relative to untreated lenders.

Table 4 also reports the effects of key control variables on loan pricing. As expected, having a bad credit history and a higher current outstanding balance are both associated with significantly higher spreads. In contrast, having an existing mortgage or a higher credit limit, being a first-time borrower, or sharing the loan with additional coborrowers are all associated with lower spreads. I view these patterns as consistent with standard risk-based pricing, and use them primarily to validate that the specification behaves as expected. Similar associations were documented by Bank et al. (2023).

To further explore the temporal dynamics of the credit registry's impact on loan pricing, I re-estimate Equation (1), replacing the interaction between the $Treat$ dummy and $Post$ with a series of interactions between the $Treat$ indicator and monthly dummies across the sample period. The resulting coefficients capture the month-by-month evolution of the treatment effect on loan pricing, relative to the baseline month of April 2019. Figure 2 displays these coefficients alongside 95% confidence intervals.

The results reveal no immediate effect following the introduction of the credit registry. However, a gradual and persistent decline in loan spreads emerges several months after the baseline, suggesting that lenders required time to incorporate the newly available information into their pricing decisions. This delayed yet sustained effect supports the interpretation that registry usage promoted more competitive loan pricing over time. Importantly, the absence of a pretreatment trend differential between treated and untreated lenders lends support to the parallel trends assumption.

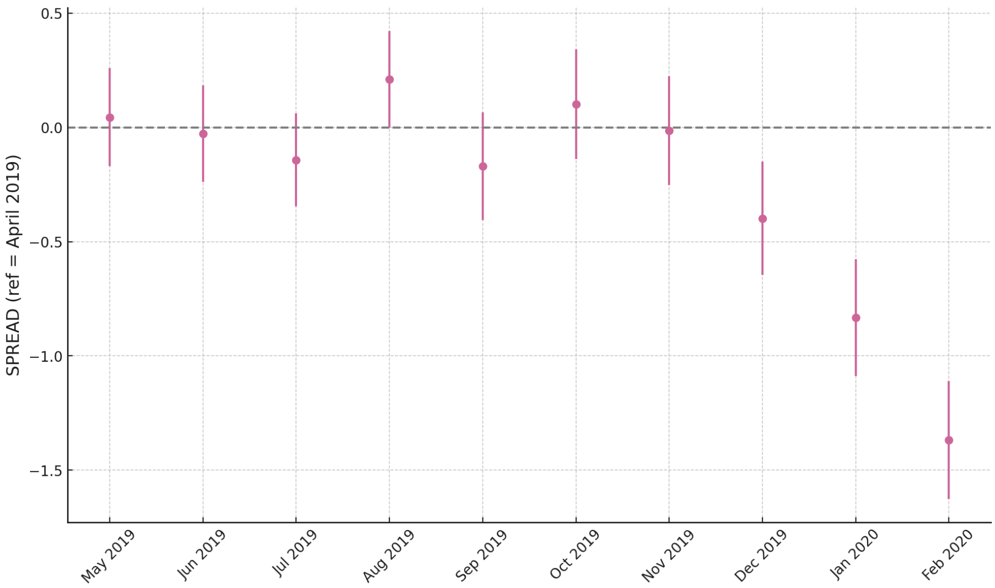
I verify that the main pricing results are robust to alternative sample construction and timing assumptions. Across all specifications, the estimated treatment effect of registry usage on loan spreads remains negative and statistically significant. Full results are reported in Appendix A.

Table 4: Baseline Estimations

	(1)	(2)	(3)
LTV	0.651	0.265	0.135
	(0.607)	(0.356)	(0.329)
Maturity	0.003	-0.014**	-0.015***
	(0.018)	(0.005)	(0.005)
Principal (Th. NIS)	-0.009**	-0.005***	-0.005***
	(0.004)	(0.001)	(0.001)
Credit_Limit (Th. NIS)			-0.006***
			(0.001)
Current_Balance (Th. NIS)			0.003***
			(0.0005)
Utilization (Th. NIS)			0.003***
			(0.0003)
Bad_Hist			0.392***
			(0.025)
Mortg			-0.241***
			(0.022)
Many_Borrowers			-0.147***
			(0.024)
New_Cust			-0.352**
			(0.145)
New_Bor			-0.234***
			(0.055)
Post	0.639***		
	(0.203)		
Treat	-1.175*		
	(0.711)		
Post : *Treat*	**-0.401***	**-0.331****	**-0.311****
	(0.223)	(0.122)	(0.115)
Loan controls	✓	✓	✓
Borrower controls	✗	✗	✓
Lender FE	✗	✓	✓
Time FE	✗	✓	✓
Observations	60,179	60,179	60,179
Adjusted R^2	0.485	0.659	0.680

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

Figure 2. : Impact of Treatment on Spread by month



5.2. Pricing mechanisms: LTV sensitivity

I next examine how registry usage changed the mapping from observable risk proxies to prices, focusing on collateral-based pricing in the high-LTV segment. As shown in Table 5, pricing sensitivity is examined in loans with $LTV > 0.7$, where collateral buffers are thin and risk assessment becomes central to pricing. This threshold lies close to the sample mean LTV of 0.75, thereby capturing the upper part of the LTV distribution (see §3).

Pooling all treated lenders together, both banks and NBFIs exhibit a significant change in the LTV–spread relationship (Column 1). However, this average effect masks important heterogeneity. For banks alone, the estimated change is small and statistically insignificant (Column 2), consistent with their longstanding access to rich internal borrower data. The registry provided little incremental information for pricing.

By contrast, among NBFIs the triple-differences specification isolates the post-adoption change in LTV sensitivity for registry users relative to nonusers. The coefficient on the triple interaction $Post \times Treat \times LTV$ is negative and statistically significant across specifications (Columns 3–5). In the baseline specification with lender and month fixed effects, the estimate implies a reduction of about 1.3 percentage points in the slope of the spread–LTV relationship. This suggests that, within the high-LTV range, registry usage reduced reliance on collateral as a pricing input and shifted pricing toward borrower-level risk information. Put differently, among loans with relatively high leverage, which are typically viewed as riskier, the availability of registry data enabled lenders to better separate high-risk borrowers from those with otherwise strong creditworthiness, allowing the latter to obtain more favorable terms despite high leverage.

Table 5: Registry Adoption and LTV Sensitivity: Pooled, Banks and NBFIs (LTV > 0.7 Sample)

	All	Banks	NBFIs	NBFIs	NBFIs
	(1)	(2)	(3)	(4)	(5)
LTV	0.732	2.826***	3.316*	-0.712	-0.613
	(0.697)	(0.118)	(1.836)	(0.759)	(0.641)
Post			-0.055		
			(0.406)		
Treat			1.937**		
			(0.953)		
Maturity	-0.020***	-0.009	-0.005	-0.020***	-0.020***
	(0.004)	(0.009)	(0.016)	(0.004)	(0.004)
Principal (Th. NIS)	-0.005***	-0.005***	-0.009**	-0.005***	-0.006***
	(0.001)	(0.001)	(0.004)	(0.001)	(0.001)
Credit_Lim (Th. NIS)	-0.006***	-0.006***			-0.005***
	(0.001)	(0.002)			(0.0004)
Current_Balance (Th. NIS)	0.005***	0.007**			0.003***
	(0.001)	(0.002)			(0.001)
Max_Utilization	0.002***	0.002*			0.002***
	(0.0003)	(0.001)			(0.0004)
Bad_Hist	0.442***	0.943**			0.290***
	(0.113)	(0.324)			(0.014)
Post × *LTV*	-0.674***	-0.407	0.816	0.431	0.399
	(0.120)	(0.282)	(0.569)	(0.411)	(0.356)
Post × *Treat*			1.397***	0.873**	0.854**
			(0.505)	(0.361)	(0.306)
Treat × *LTV*			-3.639**	0.181	0.111
			(1.840)	(1.005)	(0.911)
Post × *Treat* × *LTV*			**-2.053*** **	**-1.358*** **	**-1.307*** **
			(0.614)	**(0.417)**	**(0.364)**
Loan Controls	✓	✓	✓	✓	✓
Borrower Controls	✓	✓	✗	✗	✓
Lender FE	✓	✓	✗	✓	✓
Time FE	✓	✓	✗	✓	✓
Observations	47,560	9,976	40,821	40,821	40,821
R^2	0.830	0.800	0.528	0.666	0.683
Adjusted R^2	0.830	0.799	0.528	0.666	0.683

* p<0.1, ** p<0.05, *** p<0.01. All columns estimated on a sample with *LTV* > 0.7

5.3. Loan Performance: Delinquency Rate

I further examine whether registry adoption was associated with changes in loan performance, as measured by delinquency rates (see §3 for definition). Table 6 reports results from estimating Equation 4, comparing changes in delinquency between NBFIs and banks before and after the registry launch. Although both groups had access to the registry, NBFIs entered the reform period with more severe informational disadvantages. The negative and statistically significant coefficient on the interaction term $NBFI_j \times Post_t$ indicates that delinquency rates declined more among non-bank lenders than among banks following the registry's introduction. This pattern suggests a narrowing of the performance gap between NBFIs and banks, consistent with the view that access to comprehensive borrower information improved screening among previously information-constrained lenders.

Taken together, the results indicate that improved access to borrower-level information substantially altered NBFI lending behavior. Registry usage enabled lenders to rely less on collateral-based proxies such as LTV, particularly for high-leverage loans, consistent with a shift toward more precise borrower-level risk assessment. This improvement in risk assessment translated into lower loan spreads. Evidence on loan performance further suggests that these changes were accompanied by enhanced screening among lenders that previously faced informational disadvantages, as evidenced by a relative decline in delinquency rates compared to bank lenders.

Table 6: Delinquency rates

	(1)	(2)	(3)
LTV_wins	0.041***	0.039***	0.029***
	(0.008)	(0.008)	(0.006)
Maturity	0.0003***	0.0003***	0.0003***
	(0.0001)	(0.0001)	(0.0001)
Principal (Th. NIS)	0.00005	0.0001	0.0001*
	(0.0001)	(0.00005)	(0.00003)
Post	-0.004		
	(0.004)		
NBFI	-0.006		
	(0.004)		
Credit_Limit (Th. NIS)			-0.001**
			(0.0002)
Current_Balance (Th. NIS)			-0.00002
			(0.0001)
Utilization			0.001**
			(0.0003)
Bad_Hist			0.085***
			(0.008)
Sector (*ind_v*3)			-0.024***
			(0.003)
New_Bor			-0.006*
			(0.003)
New_Cust			-0.014**
			(0.005)
$Post \times NBFI$	**-0.009***	**-0.011***	**-0.015****
	(0.005)	(0.006)	(0.006)
Loan controls	✓	✓	✓
Borrower controls	✗	✗	✓
Lender FE	✗	✓	✓
Time FE	✗	✓	✓
Observations	69,820	69,820	69,820
R^2	0.010	0.013	0.039
Adjusted R^2	0.009	0.013	0.038

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

6. Conclusions

This paper studies how improved access to borrower-level credit information affects loan pricing and risk assessment by lenders, two central dimensions of competitive consumer credit markets. I exploit the 2019 launch of Israel's centralized credit registry, which provided lenders with standardized data on individuals' debt balances and repayment histories. While prior studies have focused primarily on the banking sector, I examine the reform's impact on NBFIs, which had previously operated under significant informational disadvantages and were a primary focus of the reform.

The empirical setting is the auto loan market, which offers a particularly informative case for evaluating the impact of the registry. Auto loans comprised roughly 25 percent of total household credit during the sample period, with over 80 percent of new loans issued by NBFIs. Auto lending also represents the core of NBFIs' consumer credit activity, making this segment well-suited for studying the effects of improved access to borrower information among informationally constrained lenders.

I leverage institutional heterogeneity in registry usage: Although all major NBFIs were required to report credit data and were authorized to access it, only a subset actively used credit reports during the study period. This variation enables a difference-in-differences design, comparing treated (registry-using) and untreated NBFIs before and after the registry became operational.

The results indicate that registry adoption led to a significant reduction in interest rate spreads—about 30 basis points—among treated NBFIs, with the effect reaching roughly one percentage point by the end of the sample period. I also document a shift in pricing behavior, as loan pricing became less sensitive to LTV ratios among higher-leverage loans, indicating reduced reliance on collateral-based proxies and greater use of borrower-level information. This pattern is not observed among banks, consistent with their longstanding access to borrower information through existing customer relationships. In addition, I document a relative decline in delinquency rates among treated NBFI lenders compared to bank lenders following registry adoption, consistent with improved borrower screening and a partial narrowing of the informational gap between nonbank and bank lenders. These findings are robust to a range of alternative specifications, including different sample definitions, adjusted LTV measures accounting for vehicle depreciation, and alternative treatment timing based on observed registry usage patterns.

Unlike much of the existing literature, which relies on data from individual lenders, this study uses comprehensive administrative records covering nearly the entire Israeli credit market, with particular coverage of NBFIs. In addition, the paper provides a unique case study of a centralized credit market characterized by pronounced informational asymmetries. This context offers new insight into how improved access to borrower-level credit information reshapes lender pricing behavior and risk assessment in constrained credit markets. Beyond this contribution, the paper delivers a rare empirical evaluation of the main objective of a major financial reform in Israel, with direct implications for credit market regulation and policy design. More broadly, the findings suggest that improving access to reliable borrower-level information can mitigate informational market failures and translate into more accurate risk-based pricing, with tangible benefits for borrowers in segments previously served by information-constrained lenders. While the analysis focuses on borrower outcomes, the findings may also have implications for market structure and competition, opening important directions for future research.

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Appendix A. Robustness Checks

A1. Alternative Sample Construction

I estimate the baseline specification using loan stocks constructed uniformly across lenders, with September 2019 and February 2022 as reference points. Table A1 presents the corresponding results, which are consistent with the main estimates.

Table A1: Alternative Sample Construction: September 2019 and February 2022

	Sep (1)	Sep (2)	Feb (3)	Feb (4)
LTV	0.258***		0.154**	-0.438
	(0.075)		(0.066)	(0.477)
Maturity	-0.014***	-0.014***	-0.021**	-0.021**
	(0.001)	(0.001)	(0.008)	(0.008)
Principal (Th. NIS)	-0.005***	-0.005***	-0.006***	-0.006***
	(0.0003)	(0.0003)	(0.001)	(0.001)
Credit_Limit (Th. NIS)		-0.007***		-0.006***
		(0.0005)		(0.001)
Current_Balance (Th. NIS)		0.003***		0.003***
		(0.001)		(0.001)
Max_Utilization (Th. NIS)		0.004***		0.003***
		(0.001)		(0.001)
Bad_Hist		0.391***		0.393***
		(0.024)		(0.034)
Mortg		-0.250***		-0.262***
		(0.016)		(0.022)
New_Bor		-0.223***		-0.193***
		(0.021)		(0.059)
New_Cust		-0.351***		-0.490**
		(0.021)		(0.200)
$Post \times Treat$	**-0.232***	**-0.213***	**-0.269***	**-0.261***
	(0.116)	**(0.108)**	**(0.144)**	**(0.146)**
Loan Controls	✓	✓	✓	✓
Borrower Controls	✗	✓	✗	✓
Lender FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Observations	61,518	61,518	25,431	25,431
R^2	0.659	0.680	0.757	0.757
Adjusted R^2	0.659	0.680	0.756	0.757

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

A2. Alternative Treatment Timing: July 2019

As an additional robustness check, I re-estimate the baseline specification using July 2019 as the treatment threshold. This alternative timing is motivated by a sharp increase in the number of credit report inquiries observed in administrative data during that month (see Figure 1 and discussion in §4). As presented in Table A2, the results under this specification remain materially unchanged.

Table A2: Regression Results Relative to July 2019

	(1)	(2)
LTV	0.264	0.134
	(0.355)	(0.328)
Maturity	-0.014**	-0.015***
	(0.005)	(0.005)
Principal (Th. NIS)	-0.005***	-0.005***
	(0.001)	(0.001)
Credit_Limit (Th. NIS)		-0.006***
		(0.001)
Current_Balance (Th. NIS)		0.003***
		(0.0005)
Utilization (Th. NIS)		0.003***
		(0.0003)
Bad_Hist		0.392***
		(0.025)
Mortg		-0.242***
		(0.022)
Many_Borrowers		-0.147***
		(0.024)
New_Bor		-0.234***
		(0.055)
$Post \times Treat$	**-0.290****	**-0.267****
	(0.100)	**(0.089)**
Loan controls	✓	✓
Borrower controls	✗	✓
Lender FE	✓	✓
Time FE	✓	✓
Observations	60,179	60,179
R^2	0.659	0.680
Adjusted R^2	0.659	0.680

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

A3. Clean Window Comparison

Table A3 presents estimates from the clean window comparison between a strictly pre-registry period and a strictly post-implementation period. The estimated effect remains negative and statistically significant, with greater magnitude but less precision than the baseline.

Table A3: Clean Window Comparison (2018–2019)

	(1)	(2)
LTV	0.286	0.126
	(0.392)	(0.354)
$Maturity$	-0.019***	-0.019***
	(0.005)	(0.005)
$Principal$	-0.004***	-0.005***
	(0.001)	(0.001)
$Credit\_Lim(Th.NIS)$	-0.001*	-0.006***
	(0.001)	(0.001)
$Current\_Balance(th)$		0.007***
		(0.0005)
Bad_Hist		0.496***
		(0.018)
$Mortg$		-0.254***
		(0.021)
$Many\_Borrowers$		-0.126***
		(0.041)
New_Bor		-0.276***
		(0.041)
New_Cust		-0.291**
		(0.108)
$Post \times Treat$	-0.596*	-0.607*
	(0.319)	(0.298)
Loan controls	✓	✓
Borrower controls	✗	✓
Lender FE	✓	✓
Time FE	✓	✓
Observations	50,683	50,683
R^2	0.623	0.646
Adjusted R^2	0.622	0.645

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

A4. Adjusted LTV Measure

I replace the baseline LTV measure with an adjusted version that accounts for depreciation in collateral values. I obtain qualitatively comparable estimates, both in the baseline specification and when using July 2019 as the treatment threshold.

Table A4: Robustness: Adjusted LTV

	(1) Sep 2019 threshold	(2) Jul 2019 threshold
Adjusted_LTV	0.141	0.134
	(0.309)	(0.328)
Maturity	-0.015***	-0.015***
	(0.005)	(0.005)
Principal (Th. NIS)	-0.005***	-0.005***
	(0.001)	(0.001)
Credit_Limit (Th. NIS)	-0.006***	-0.006***
	(0.001)	(0.001)
Current_Balance (Th. NIS)	0.003***	0.003***
	(0.0005)	(0.0005)
Utilization (Th. NIS)	0.003***	0.003***
	(0.0003)	(0.0003)
Bad_Hist	0.392***	0.392***
	(0.025)	(0.025)
Mortg	-0.242***	-0.242***
	(0.022)	(0.022)
Many_Borrowers	-0.147***	-0.147***
	(0.024)	(0.024)
New_Bor	-0.233***	-0.234***
	(0.055)	(0.055)
New_Cust	-0.352**	-0.352**
	(0.146)	(0.146)
$Post \times Treat$	**-0.309****	**-0.267****
	(0.114)	**(0.089)**
Loan controls	✓	✓
Borrower controls	✓	✓
Lender FE	✓	✓
Time FE	✓	✓
Observations	60,179	60,179
R^2	0.680	0.680
Adjusted R^2	0.680	0.680

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