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Relationship Banking and Credit Scores: Evidence from a Natural Experiment[1]

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Discussion Paper 2023.05
March 2023

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1 The views expressed here do not necessarily reflect those of the Bank of Israel. For their valuable input we thank Alon Raviv (discussant), Padma Sharma (discussant), Rajashri Chakrabarti (discussant), Eugene Kandel, Francesco D'Acunto, and the seminar participants at the: Bank of Israel, Hebrew University School of Business Administration, Tel-Aviv University Coller School of Management Finance Seminar, Tel-Aviv University Coller School of Management Business Economics and Strategy Seminar, Israel Competition Authority, Israel National Economic Council, University of California Berkeley Haas School of Business, University of California Irvine Department of Economics, Ben-Gurion University Department of Economics, Bar-Ilan University Department of Economics, Haifa University Department of Economics, Raichman University Department of Economics and the CEPR Household Finance Seminar. The conference participants at the MFA, ALEA, Women in Empirical Microeconomics. We are very grateful for the great people at the Bank of Israel Information and Statistics Department for their invaluable help, hard work and dedication to this project. We are especially grateful to Shlomi Bardagan for his assistance with the data.

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Tali Bank Nimrod Segev and Maya Shaton

Abstract

We show the effect of credit scores' introduction on consumer credit prices. Utilizing a novel dataset of the universe of loans in Israel, we find that a decline in information asymmetry, following the introduction of credit scores introduction, led to a decrease in loan prices for households with strong relationship banking. Prior to that, when banks held a monopoly on potential borrowers' credit history, they charged higher interest rates, all else equal, as predicted by theoretical models. We further show that these informational rents significantly decrease once credit scores are introduced, resulting in a decline in the hold-up problem. To the best of our knowledge, this paper is the first to show the causal impact of credit scoring on households' loan pricing. Our results underscore the importance of information sharing in consumer credit markets, and have important public policy implications.

Keywords: Credit Scores, Relationship Lending, Relationship Banking, Hold-up Problem, Consumer Credit, Information Sharing, Credit Register.
JEL Classification code: G21, G28.

דירוג אשראי וקשרי מלווה-לווה: ממצאים ממאגר נתוני האשראי

טלי בנק נמרוד שגב ומאיה שטאון

תקציר

מאמר זה בוחן את ההשפעה של הגידול במידע העומד לרשות המלווים כתוצאה מהקמת מאגר האשראי על קשרי הבנק עם לקוחותיו ובפרט על מחירי האשראי הקמעונאי בישראל. נמצא שלפני הקמת המאגר, לקוחות אשר ניהלו חשבון עו״ש בבנק בודד שילמו כחצי אחוז יותר על אשראי צרכני לעומת לקוחות אשר החזיקו מספר חשבונות עו״ש בבנקים שונים. כמו כן, לאחר הקמת המאגר, פער זה בריביות הצטמצם בכשליש. ממצאים אלו מהווים אינדיקציה לכך ששיתוף נתונים דרך מאגר האשראי פועל לצמצום הפערים במחיר האשראי דרך הקטנת התלות של לקוחות בבנק בו הם מנהלים את חשבון העו״ש (בעיית הלקוח השבוי).

1 Introduction

Asymmetric information between borrowers and lenders has long been discussed in the academic literature as leading to suboptimal credit allocation. Financial intermediation theories offer a way to overcome this informational gap through relationship banking (Boot 2000). Another way to overcome this asymmetry is through information sharing between lenders (Pagano and Jappelli 1993). Credit reports and credit scores represent a very common way through which lenders share information. While credit scores have become widely popular in the consumer credit markets, there is very little evidence as to how information sharing impacts relationship banking. Furthermore, while relationship banking in business lending has been studied extensively, empirical evidence on relationship lending in consumer credit is extremely limited (Puri and Rocholl 2008). Utilizing a unique exogenous increase in consumer credit information, we present the first empirical evidence as to the effect of information sharing on retail relationship banking and loan pricing.

In this study, we use the introduction of a consumer credit register and credit scores in Israel to test how a decrease in information asymmetry between borrowers and lenders impacts retail relationship lending. The introduction of the Israeli credit register increased the amount of information available regarding retail consumers. Relationship lending theory postulates that a bank gathers private information through customer relationships, resulting in a comparative advantage in lending relative to non-relationship banks (Sharpe 1990; Rajan 1992). This potentially leads to the hold-up problem, where banks could extract monopolistic rents from their consumers, especially in concentrated markets (Petersen and Rajan 1995). Therefore, we hypothesize that once information asymmetry is reduced, the hold-up problem would attenuate. That is, as banks' ability to extract monopolistic rents from households decreases, we expect loan prices to decline. Our findings confirm this conjecture. Specifically, we show that the introduction of credit scores significantly impacts loan prices for borrowers with stronger banking relations relative to borrowers with weaker relationships.

Our data includes confidential administrative details on debt for the universe of banks' retail consumers in Israel from 2018 to 2020. For each borrower, we observe all credit facilities obtained from all banks. In contrast to consumer credit data obtained from credit bureaus in the US, our data also includes loan prices alongside some borrower-specific characteristics as detailed in Section 2. This novel dataset permits us to estimate the informational rents extracted by banks and how these are impacted by information sharing. Specifically, we compare the impact of strong versus weak banking relationships on loans' outcomes before and after the information asymmetry is reduced. Similar to Puri et al. (2017), we define a relationship bank as a bank where the borrower manages a checking account. As noted by Norden and Weber (2010), checking account activity provides important information enabling banks to assess credit risk. Accordingly, we quantify relationship strength by the exclusivity of the relationship. That is, a strong bank-borrower relationship is one in which the borrower holds a checking account solely in one bank. Israel provides a great setting to test questions relating to relationship banking, as banks are the primary source of consumer credit. Furthermore, the availability of a centralized credit register, which includes interest rates, provides us with a unique opportunity to identify the impact of information on relationship banking.

To test the impact of relationship banking on loan pricing, we use a difference-in-differences approach. Specifically, we compare the changes in loan pricing for exclusive and non-exclusive borrowers before and after credit scores were introduced. We first show that exclusive relationship borrowers paid higher interest rates before credit scores were introduced. This result is consistent with banks extracting rents from exclusive relationship borrowers, the hold-up problem. We then test the impact of a shock to retail consumer credit information. We find that credit scores significantly mitigate the hold-up problem. All else equal, we show that the difference in the interest rates for exclusive and non-exclusive borrowers decreased by about 10 to 13 basis points. This represents a 30 percent reduction in the interest-rate-difference between the two groups relative to the prior period. When we further restrict our definition of exclusive relationship to include only checking accounts of length longer than 12 months, we find that this gap decreases by almost 90 percent. Our findings are consistent with our main hypothesis that the hold-up problem is mitigated once information asymmetry is reduced. To the best of our knowledge, this study is the first to show this causal relationship between consumer information sharing and relationship banking in the household setting.

We then provide a battery of tests to show that our results are robust to different challenges. A possible concern is that our results are driven by endogenous selection and time-varying differences across exclusive and non-exclusive relationship borrowers. To deal with selection concerns, we estimate our specification with a restricted sample composed of borrowers who had a loan before and after the introduction of credit scores. The latter permits us to include borrower fixed effects in our estimation, thus accounting for all borrowers' time-invariant characteristics. This estimation uses within borrowers' variation, hence asymmetric changes in borrower composition across groups do not impact the estimates. Our findings are robust across all specifications, therefore alleviating endogeneity concerns and supporting a causal interpretation between the decrease in information asymmetry and relationship banking. In Section 4, we offer additional tests demonstrating our results are robust to other possible concerns.

Our paper relates to the vast literature on relationship banking - in particular, how relationship banking influences credit availability and loan prices in the retail setting. So far the literature has proposed two possible, and at times opposing, implications of relationship banking on contract terms.[1] A number of studies have shown that relationship banking can benefit borrowers through increased credit availability while also benefiting banks by improving screening ability (Petersen and Rajan 1994; Berger and Udell 1995; Agarwal et al. 2018).[2] At the same time, other studies have suggested that long borrower-lender relationships can lead to the hold-up problem, as borrowers become locked into their

[1] See Kysucky and Norden (2016) for a brief summary of this literature.

[2] The importance of relationship banking has been further discussed in the context of economic downturns. Bolton et al. (2016) show that relationship banks provide continuous lending in a crisis. Cohen et al. (2021) provide evidence that the collapse of banking relationships had a significant impact on economic activity during the Great Depression.

banking relationship (Sharpe 1990; Rajan 1992). This is especially the case when switching costs are high (Ioannidou and Ongena 2010) and consumers do not have many alternatives to their relationship banking (Degryse and Ongena 2005). In their cross-country analysis, Kysucky and Norden (2016) suggest that corporate borrowers are more prone to not benefit from relationship banking in countries where banking competition is low. We contribute to this literature by directly testing the effect of relationship banking on loan prices in the household setting. Our findings suggest that relationship banking gives rise to the hold-up problem, which is mitigated once the information asymmetry between banks and borrowers diminishes.

Puri and Rocholl (2008) note that retail relationship banking has been less examined in the literature due to severe data limitations in the context of appropriate experimental design. Our paper provides a perfect setting, alongside a unique dataset, to examine retail relationship banking, thus expanding the research from the household finance perspective. Puri et al. (2017) use German data and show that retail customers who have a relationship with their savings bank before applying for a loan default significantly less often than customers with no prior relationship. Agarwal et al. (2018), examine retail credit consumers in one bank and find that relationship banking offers significant potential benefits to banks in mitigating credit risk. Chakravarty and Scott (1999) use survey data to examine the effect of relationships on credit rationing for households. We further our understanding of this area by providing novel findings regarding the impact of relationship banking on prices, specifically on banks' ability to extract rents from their customers. Also in contrast to most of this literature, we use a natural experiment where a shock to information has occurred. Thus, we expand this literature by providing empirical evidence where causal inference can be drawn.

Our paper also relates to papers investigating the influence of information sharing on credit pricing and performance. Theoretical models suggest that credit information sharing schemes can help lenders and borrowers overcome asymmetric information problems. Credit registries provide information to banks permitting better screening (Pagano and Jappelli 1993; Bennardo et al. 2014). At the same time, it disciplines borrowers as nonpayment issues are made public (Padilla and Pagano 1997). Bos et al. (2018) document how credit information affects borrowers' access to credit. Jappelli and Pagano (2002), using a cross-country survey, find that credit risk is lower in countries where lenders share information. However, overall evidence on the effect of public credit registries on credit supply is ambiguous (Djankov et al. 2007). Einav et al. (2013) show that the adoption of credit scoring by an auto finance company has benefited the lender, partially due to better screening of high-risk borrowers. They focus on a particular lender and type of loan, whereas we look at the universe of consumer non-collateralized loans. Sutherland (2018) examines how information sharing influences relationship lending for businesses. He finds that information sharing does influence contract terms. In contrast to our setting, he does not examine retail consumers nor does he have pricing information. Similar to our paper, Behr and Sonnekalb (2012) use the introduction of a credit register in Albania to test the effect of information sharing. However, our paper is different from their work for several reasons. Most importantly, they focus on SME firms, whereas we focus on households. Second, they examine data from one bank, whereas we have data for the universe of consumers loans. Our paper contributes to this literature by providing novel empirical evidence as to the impact of credit registries on relationship banking.

The remainder of this paper is organized as follows. Section 2 describes the institutional details and the data. Section 3 presents our empirical strategy. In Section 4 we present our results and robustness tests, and Section 5 concludes.

2 Institutional Setting and Sample Construction

2.1 Institutional Details

Israel's consumer credit market is bank-based; as of December 2019, about 85% of consumer credit was granted by banks. Furthermore, this banking system is fairly concentrated. It consists of seven bank groups, where the market share of the two largest banks exceeds 50% of the total credit allocated by the banking system. The Israeli financial system has gone through several reforms in the past two decades.[3] Most relevant to our paper are the regulatory steps taken to promote competition in the banking system. As part of such reforms, credit scores were first introduced in Israel in April 2019.

The institution of the Israeli credit register is part of such reforms and was enacted in 2016 in the Credit Data Law. The proclaimed goals of the register are: (1) Enhance competition in the retail credit market; (2) Expand access to credit; (3) Reduce discrimination in credit supply; (4) Establish a credit register database to facilitate the carrying out of the Bank of Israel's functions. Following the passage of the Credit Data Law, all Israeli banks were required to transfer all credit data for the entire population of borrowers to the Bank of Israel. The requirement started in 2016, whereas credit scores became available starting from April 2019. From April 2019, any lending institution could contact any of the credit bureaus to obtain potential borrowers' credit reports and scores. We should note, that in contrast to the US, where credit data used to compute households' credit scores are collected and held by private credit bureaus (such as Equifax, Experian, and TransUnion), in Israel the law prescribes that the Bank of Israel gathers and holds all the credit data used to compute Israeli credit scores ("credit register").[4] This data is then transmitted to private credit bureaus, created following the law, which compute the credit scores based on such information on a case by case basis.[5]

2.2 Definition of Relationship Banking

An essential aspect of our analysis concerns the determination of the strength of relationship lending. First, we define a relationship loan as a loan granted to a borrower by the bank where she maintains a

[3]Relevant to our paper is the establishment of the "Strum Committee" in 2015 with the goal of increasing competition within the banking system.

[4]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

[5]See Jappelli and Pagano (2002) for a review of different types of credit bureaus and credit registers around the world.

checking account. We then proceed to assess the intensity of this relationship by examining the number of banking relationships that each borrower has established. Specifically, our study focuses on borrowers who exclusively interact with one bank, thereby maintaining an exclusive relationship, in comparison to those who maintain relationships with multiple banks. We denote our relationship variable as *Exclusive* which takes the value of 1 in cases where the borrower is granted the loan by the same bank where they hold a sole checking account.

The management of a checking account provides important information on one's cash-flows and thus relates to the strength of the relationship between lenders and borrowers (Mester et al. 2007; Norden and Weber 2010). Puri et al. (2017) show that having a transaction account at the bank significantly reduces consumer loans' default probability. Moreover, in Israel, the typical consumer first line of credit is the overdraft from one's main checking account. This credit line is similar to rollover credit card debt in the US (which is uncommon in Israel). Similar to credit cards in the US, banks grant their clients a credit limit on their checking accounts up to which they can withdraw funds. Thus, through their checking accounts' activities banks are able to identify one's credit usage and overall creditworthiness. The number of banking relationships each borrower holds is also noted in the literature as a key component of the bank-borrower relationship. For example, Berger et al. (2005) note that bank exclusivity promotes the development of close relationships through unique accesses and accumulation of information. However, exclusivity could also give rise to the hold-up problem between the bank and the borrower.

2.3 Data and Descriptive Statistics

The credit data register contains information on all (new and outstanding) consumer credit facilities such as consumer loans, credit cards, credit lines, and mortgages, on a monthly basis. Our sample includes all non-securitized consumer loans granted by Israeli banks for the period spanning from August 2018 to February 2020. This period represents the longest available period for which we have all the variables prior to the COVID-19 global pandemic.

We apply several filters to make the sample as homogeneous as possible, which reduces concerns about any bias associated with unobserved differences between exclusive and non-exclusive relationship loans. First, since our primary focus is on the difference between exclusive and non-exclusive relationship lending, we exclude all loans granted to consumers who do not have any relationship (do not maintain a checking account) with the lending bank. It is important to note that, in Israel, loans given to consumers who do not hold a checking account with the bank are less common and represent only 10% of consumer loans originated by banks. Additionally, these loans tend to be very different relative to relationship loans in terms of structure and purpose, suggesting that these loans should indeed be excluded. Also, we exclude any borrower who switched between exclusive and non-exclusive relationships during the sample period.[6] We further restrict the sample to borrowers with credit history. That is, we exclude borrowers

[6] This restriction ensures that there is no movement between the treated and the control group and limits any impact of unobserved events that may have induced borrowers with specific characteristics to shift between the groups.

who took a loan in the same month that they opened their first checking account.

We exclude observations where there are more than two recorded borrowers.[7] We exclude any loan where the loan maturity is very short (less than three month) or very long (over 360 months) as these loans are most likely business loans. For the same reason, we also consider only loan with a principle amount between 1k to 300k NIS. We also exclude observations where the principal amount or the annualized nominal interest rate is zero as these are likely errors. Finally, we exclude uncommon types of consumer loans such as fixed-rate loans, linked loans, and loans made in foreign currency. Variable interest-rate loans represent the vast majority of consumer loans in Israel, therefore our sample is restricted to these. These filters reduce the sample to 1,408,225 loans. For any estimation which uses borrower fixed effects, we keep only borrowers who had at least two loans, at least one before and at least one after the the introduction of the credit register, which reduces the sample to 610,545 loans.

Our main dependent variable is *Spread* which represents the spread between a loan's nominal annualized interest rate and the baseline Israeli interest rate.[8] In our estimation, we control for both loan-specific and borrower-specific characteristics. Loan controls include: loan size (*Amount*) in thousands of New Israeli Shekel (NIS),[9] length of the loan in months at the time it was granted ($Maturity$), and the number of borrowers. A loan is taken by a household. A household could be composed of one or two individuals. Typically, if a loan is taken by a household composed of two people they co-sign the loan. Accordingly, we control for the number of borrowers in our estimations. Our borrower specific variables include: age group (*Age*), the socioeconomic rank of the borrower's city (*Socio*), mortgage ($Mortg$), credit line (*Credit_Lim*), credit line utilization (*Utilization*), and risk (*Bad_Hist*).

The credit register provides only the age group of the borrower (14 age groups). Therefore, we define an ordinal variable for each of these categories.[10] Our socioeconomic indicator is based on the municipality where the borrower resides. The Israeli Central Bureau of Statistics provides a socioeconomic index ranging from 1 to 10 for each local council or municipality, where one represents the poorest socioeconomic conditions and ten the highest. Using this index, we define *Socio* as an ordinal variable for each borrower.[11] $Mortg$ is a dummy variable which equals one if any of the borrowers has an outstanding mortgage. *Credit_Lim* is the credit line (overdraft) available to withdraw from the borrower's checking

[7]We apply this restriction for two reasons. First, about 99% of the loans in our sample have one or two borrowers. Approximately, 70% of loans have a single borrower, and about 30% have two borrowers. Additionally, we are interested in including borrower fixed effects in our estimations. When we limit our sample to individuals and pairs, we are able to identify and track individuals across time. In Section 4 we show that the results are robust to keeping only loans made to a single borrower.

[8]The Prime Rate is the basic debitory interest rate agreed upon by the banks and serves as the baseline rate for most loans.

[9]During the relevant time period, 1 USD was equal approximately 3.5 NIS.

[10]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.

[11]In cases where a loan involves two borrowers, we allocate the lowest socioeconomic indicator value and age group of the two borrowers to the households. Notably, we find that the results remain largely unaffected by using either the average or maximum values instead.

account.[12] While the credit register does not have any information on income or wealth, these variables tend to be positively correlated with the credit line's magnitude. *Utilization* is the ratio between the total amount drawn from all available credit lines and the total amount of available credit lines for the household. Both *Utilization* and *Credit_Lim* are lagged one month to reduce endogeneity concerns resulting from the loan having been granted during the same month. *Bad_Hist* denotes our risk indicator. Similar to Bonfim and Soares (2018), we use borrowers' recent credit history to assess their riskiness. This dummy variable equals one if the borrower was in arrears on any credit facility, or if the borrower had a check that was not processed due to non-sufficient funds, in the 12 months prior to obtaining the loan.[13] Table 1 provides descriptive statistics of the main variables used in the analysis. Panel A distinguishes between the period prior to the introduction of credit scores and following their introduction for all loans. Panel B includes only exclusive relationship loans and Panel C only non-exclusive relationship loans. Panels D and E include loans to consumers with good and bad credit histories respectively. In Panel F we compare between exclusive and non-exclusive borrowers. Panel A-E show that overall the number of loans remained stable before and after the introduction of credit scores.

[**Table 1 to be added here**]

From Panel A shows that there is no economically meaningful differences in our controls variables before and after the introduction of credit scores. Borrowers with bad credit history represent 19% in the pre-period and 17% in the post-period. On average, 36% of the borrowers in our sample have a mortgage in both periods. In addition, the median age group of borrowers across the sample is 6 which represents ages from 40 to 44 years old. Our analysis reveals that approximately 68% of loans were granted to exclusive borrowers in both the pre- and post- periods. The average loan size was 40,380 NIS, with a slight increase to 41,910 NIS in the post-period. The average maturity remained consistent across both periods, at approximately 42 months. Furthermore, households had an average monthly credit line of around 18,000 NIS, of which they utilized approximately 60%. It is worth noting that this high utilization rate is not surprising, as overdrafts from checking accounts are commonplace in Israel and are often used as the primary method for rolling over household debts. Overall, our findings suggest that the main covariates in the sample were not significantly affected by the introduction of the credit register.

As anticipated, when comparing panels D and E, borrowers with good credit history are found to pay a lower spread on their loans on average, and the average loan amount is slightly higher. While the borrower-specific controls such as age and sociodemographic indicator are similar across both groups. Comparing panels B, C, and F, we observe that non-exclusive borrowers tend to be older and more likely to have a mortgage. Loans to non-exclusive borrowers also tend to be larger in size and have

[12] If the loan has two borrowers with two separate checking accounts, we take the largest credit line between the two.

[13] If a loan is approved for a household with two borrowers, this variable will have a value of 1 if either borrower has been in arrears within the last 12 months. For the purpose of this definition, a credit facility would include any consumer loan, mortgage, credit card or credit line.

longer maturities. Interestingly, while exclusive borrowers are, on average, less risky than non-exclusive borrowers, they pay a higher spread on their loans. This finding aligns with the central thesis of our paper, which posits that banks can extract monopolistic rent from their consumers when the bank-borrower relationship is exclusive. The descriptive statistics presented in Table 1 support this notion, as they indicate that exclusive borrowers pay a premium on their loans due to the hold-up problem. To test this conjecture empirically, we outline our empirical strategy in the following section.

3 Empirical methodology

Our empirical methodology is designed to test the effect of information shock on consumer loan prices. According to theory, stronger relationship banking leads to the hold-up problem (Petersen and Rajan 1995). Prior to the introduction of credit scores, banks that maintained exclusive relationships with their consumers, held a monopoly over the information collected through those relationship, allowing them to extract rents from consumers. As a result, we expect that, all else being equal, exclusive borrowers paid higher interest rates on their loans before the advent of credit scores. The introduction of credit reports and scores made consumer credit information publicly available, thus reducing banks' monopolistic power over such information. Accordingly, we hypothesize that the hold-up problem will attenuate for consumers who are most susceptible to it. Specifically, we expect that the interest rates paid by consumers with exclusive banking relationships will decrease once credit scores become available, all else being equal.

To test this hypothesis, our identification strategy relies on the differential effect of information shocks on exclusive versus non-exclusive borrowers. To estimate this effect we use a difference-in-differences specification. Our treated group is composed of borrowers with exclusive banking relationships. Our control group is composed of borrowers with non-exclusive banking relationships. The information shock we are using is the introduction of credit scores in Israel. Since exclusive borrowers are more prone to the hold-up problem, we expect that once the information shock occurs they would be most affected. Accordingly, our baseline specification is as follows:

$$Spread_{i,j,k,t} = \gamma_k + \delta_t + \beta_1 Exclusive_{j,k} + \beta_2 Exclusive_{j,k} * Post_t + \beta_3 X_i + \beta_4 Z_{j,t} + e_{i,j,k,t} \quad (1)$$

where subscripts represent loan i given to borrower j, and reported by lender k at time t. The dependent variable, *Spread*, is the spread between the nominal annualized interest rate and the baseline Israeli interest rate. *Exclusive* is a binary variable that takes the value 1 if borrower j has an exclusive relationship with lender k. $Post_t$ is an indicator representing the period after credit scores were introduced. It equals 1 if the observation is after April 2019 and 0 otherwise. X_i and $Z_{j,t}$ are loan and borrower characteristics, respectively. The terms γ_k and δ_t represent lender and month fixed effects, respectively. Standard errors are clustered at the month level throughout all of our estimations. β_1 and β_2 represent the relative effect of exclusive relationship lending on credit spread. β_1 represents the average spread exclusive borrowers pay on new consumer loans relative to non-exclusive borrowers. Our main coefficient of interest is β_2 which represents the causal effect of the information shock on exclusive loans' spreads relative to non-exclusive loans.

Our empirical methodology relies on the assumption that without introducing the credit register, the difference in loan pricing between exclusive and non-exclusive relationship lending would have remained constant. That is, the parallel trend assumption holds in this case. To show that this assumption holds in our data we modify Equation (1) and replace our interaction variable $Exclusive * Post$ with a set of interactions between $Exclusive$ and a dummy for each month in our sample period. The coefficient estimates of these interaction variables reflect the dynamics of the effect of exclusive relationship versus non-exclusive relationship on loan pricing. To the extent that the parallel trend assumption holds the coefficients on the interaction terms in the months before the information shock should not have any particular trend and start to persistently fall after the shock.

Interpreting causally the coefficients estimates from Equation 1 could pose several challenges. First, we need to account for the possibility that the observed lending terms are endogenous, as they are conditional on selecting borrowers with specific characteristics to exclusive and non-exclusive borrowers. In Table 1, Panels B, C, and F compare the populations of exclusive and non-exclusive borrowers. As noted, the two populations diverge on different observable characteristics. To deal with these differences, we include controls for such observable characteristics: age, socioeconomic level, risk, mortgages, credit limit, and utilization. Nonetheless, it is possible that there are unobserved consumer characteristics that might be correlated with consumers having one or multiple bank relationships. Most importantly, if these unobserved attributes also impact loan prices, our results would be biased. To further deal with these concerns, we introduce borrower fixed effects in our specification. We modify the specification presented in Equation 1 to include borrower fixed effects:

$$Spread_{i,j,k,t} = \gamma_k + \delta_t + \mu_j + \beta_2 Exclusive_{j,k} * Post_t + \beta_3 X_i + \beta_4 Z_{j,t} + e_{i,j,k,t} \tag{2}$$

where μ_j represents borrower fixed effects. To estimate this specification we restrict our sample to borrowers with at least two loans, one prior to and one following the introduction of credit scores.[14] Our underlying assumption in these tests is that any unobservable borrower-characteristics, which could lead to any of the selection issues mentioned, are time-invariant during the sample. In this case, borrower fixed effects alleviate concerns that our results are due to some unobserved characteristics and selection.

An additional concern is that our sample risk composition changed with the introduction of the credit scores. The latter could result from strategic timing of new lending for borrowers with specific

[14] In this specification, $Z_{j,t}$ will include all time-variant borrowers' characteristics, any time-invariant variables is dropped as it is absorbed by the borrower fixed effects. For the same reason $Exclusive$ is not included in the specification as it is borrower specific and time invariant.

characteristics. It is possible that riskier borrowers feared that the credit register would reveal their bad credit information thus hindering their access to credit. Therefore they may have preemptively applied for new loans from lenders with weak relationships before April 2019. At the same time, relatively creditworthy borrowers may have postponed borrowing to the period after the register, if they anticipated it would reduce their cost of credit. In this case, the quality and overall composition of borrowers before and after the credit register will be different and may impact our results. Furthermore, the credit register most likely improved banks' screening ability which could influence loan approval and pricing. If the credit register induced banks to change their screening and loan approval practices, this might have changed borrower composition and impacted the results. Banks' ability to better assess households' creditworthiness is most relevant for non-exclusive borrowers in our setting and has different potential pricing effect depending on households' risk level.[15] Low-risk high-quality borrowers potentially benefit from the additional public information banks obtain about them, whereas high-risk borrowers may suffer from higher prices and credit rationing.

We deal with these concerns in several ways. First, we introduce a control for risk in our regression estimation, as we described in Section 2.3. In addition, we split our sample between high-risk and low-risk borrowers. To the extent that high-risk borrowers are more prone to both strategic timing and screening by lenders, a sample with only low-risk borrowers would be less vulnerable to these biases. Finally, the estimation of Equation 2 also deals with the issues denoted. This specification uses within-borrower variation to estimate the relative effect of exclusivity on loan spreads before and after credit scores' introduction, thereby reducing any impact of asymmetric changes in borrower composition.

4 Main Results

Table 2 presents the results from the estimation of Equation 1. Column 1 shows the results for all loans, while columns 2 and 3 present results for a sample split between borrowers with good and bad credit history. We find that β_1, the coefficient on *Exclusive*, is positive and statistically significant across all three columns. That is, ceteris paribus, exclusive loans are more expensive than non-exclusive loans. This is consistent with exclusive borrowers being subjected to the hold-up problem. The size of the coefficient estimate on *Exclusive* suggests that before introducing the credit register, exclusive relationship loans paid around 37.1 basis points more relative to non-exclusive relationship loans. To put this number in perspective, note from Table 1 that the average spread for non-exclusive loans before the introduction of credit scores period was approximately 451 basis points. Therefore, the additional premium paid by exclusive borrowers represents around an 8% increase in the loan price, which is economically meaningful.

[**Table 2 to be added here**]

[15] The underlining assumption throughout our analysis is that banks have better credit information for their exclusive versus non-exclusive borrowers.

Our main coefficient of interest is β_2. This coefficient represents the causal effect of the shock to information asymmetry on loan pricing for exclusive borrowers relative to non-exclusive borrowers. β_2 is negative and significant at 1% across all columns. That is, the premium paid by exclusive borrowers compared to non-exclusive borrowers decreased by approximately a third (13 basis points) once the credit register became public. This finding shows that the hold-up problem is been attenuated after credit reports and scores are available to lenders. Thus, our results demonstrate that once the information asymmetry between banks and households is mitigated, the informational rents extracted by banks decrease significantly.

From Table 2 we also learn the effect of our control variables on the spread. As expected, on average, having a bad credit history and higher utilization of credit lines significantly increases loan pricing. At the same time, living in an area with a higher socieconomic index, having a mortgage, and higher credit limit negatively impact loan spreads. Examining the sample split based on borrowers' risk in columns 2 and 3, we see that overall, the direction of the coefficients estimates is consistent with column 1. On average, exclusive borrowers with good credit history pay 32.8 basis points more than non-exclusive borrowers. Exclusive borrowers with bad credit history pay on average 51.8 basis points more than non-exclusive borrowers with bad credit history. For both borrowers with good and bad credit history, we find that β_2 is negative and significant.

A possible concern is that this observed decrease in interest rate of exclusive relative to non-exclusive borrowers is the result of changes in the spread of non-exclusive borrowers. Recall from our discussion in Section 3, that credit scores could have improved banks' screening ability which most likely impacted non-exclusive borrowers more than exclusive borrowers. Accordingly, the observed narrowing of the interest rate gap between the two groups could result from an increase in interest rates charged to non-exclusive borrowers rather than a decrease in the premium charged to exclusive borrowers. The latter could potentially bias the interpretation of our findings. However, the results in Table 2 column 2 suggest otherwise. That is, if our findings were solely driven by changes to the interest rate for non-exclusive low-risk borrowers then we would expect to find a significant increase in the relative interest rate for exclusive versus non-exclusive borrowers. We find the opposite effect as we show that β_2 is negative and significant. Therefore, any effect the credit register may have on non-exclusive low risk borrowers only weakens our findings. In fact, we could view the estimated β_2 as a lower bound estimate for the total effect of information sharing on the hold-up premium. Furthermore, we argue above that borrowers with good credit history are less likely to be impacted by bank credit rationing. Therefore, borrowers with good credit history are less prone to identification issues related to time-varying differences in the approval probability between exclusive and non-exclusive borrowers. Accordingly, our findings in column 2 further reinforce our causal interpretation of β_2.

To provide further support that our results are not driven by a significant decrease in the interest rates of non-exclusive borrowers, we decompose the decrease in interest rates following the introduction of credit scores for exclusive versus non-exclusive borrowers. To do so, we add an interaction term

$NonExclusive * Post$ to our specification in Equation 1. Table 3 displays the coefficient estimates when adding the interaction term $NonExclusive * Post$. In column 1, we find that the coefficient estimate on the interaction term $NonExclusive * Post$ is not statistically significant. That is, we do not observe a significant decrease in interest rates for non-exclusive borrowers once credit scores are introduced. Consistent with our previous results, we find that the interest rate of exclusive borrowers significantly decreased in the post period. While the coefficient estimates in this regression cannot be interpreted causally, they do provide further empirical support for the causal interpretation of β_2 in our main specification.

[**Table 3 to be added here**]

Overall the results from Table 2 are consistent with the prevalence of the hold-up problem. When credit scores were not available, banks used their monopoly over consumers' credit information and on average charged exclusive borrowers a higher interest rate on consumer loans. As credit scores become available the premium paid by exclusive borrowers significantly decreases. This is consistent with banks' monopoly over consumers' credit information diminishing with the introduction of credit scores. To the best of our knowledge, this is the first empirical work that shows the causal effect of credit reports and scores on households' prices of credit.

To further examine the dynamic shift in the impact of stronger relationship lending following the credit register, we re-estimate Equation 1, where the interaction between the exclusive dummy and Post is replaced with a set of interactions between *Exclusive* and a dummy for each month in our sample period. The coefficient estimates of these interaction variables reflect the dynamics of the effect of exclusive relationship versus non-exclusive relationship on loan pricing. The estimated coefficients are plotted in Figure 1, along with 90% confidence bands. For comparison, the figure also plots β_1, the coefficient of the exclusive dummy presented in Table 2. The red column represents the month when credit scores were introduced.

[**Figure 1 to be added here**]

Examining Figure 1, we observe that the relative price of exclusive versus non-exclusive loans is quite volatile before credit scores became available and does not show any clear trend, moving around the estimated premium (β_1) from Table 2. However, immediately after credit scores are introduced, we see a drop and a smooth downward trend in the coefficients' size. This suggests that in the period after the credit register, the effect of exclusivity on loan pricing consistently and persistently diminished. The figure also supports our assumption that the reduction in the importance of strong relationship loans started to decline only after the information shock, i.e., it supports the parallel trend assumption.

As discussed in Section 3, the coefficient estimates in Table 2 may be biased due to endogeneity and selection concerns. To remedy such concerns, we introduce borrower fixed effects in Equation 2. Including borrower fixed effects ensures that borrowers' specific time-invariant differences are not driving the results. To estimate this specification, we restrict our sample to borrowers with at least one loan before and one loan after April 2019. The results from the estimation of Equation 2 are presented in Table 4.

[**Table 4 to be added here**]

Recall that our main coefficient of interest is β_2, the coefficient on the interaction term between *Exclusive* and *Post*. Similar to our baseline estimation, β_2 is negative and statistically significant across all columns. From Table 4 we observe that the premium charged to exclusive borrowers decreases by approximately 15 basis points once credit scores are available. These results provide empirical support that our findings in Table 2 were not merely driven by unobserved borrowers' characteristics correlated with our relationship banking measure (*Exclusive*). Thus, our restricted sample findings further reinforce our claim that once information asymmetry in credit markets is mitigated, informational rents extracted by banks decrease significantly.

Our baseline specification focuses on the exclusivity component of relationship banking. Exclusivity is an important driver of the hold-up problem. Banks, through exclusive relationships, learn about their consumers' creditworthiness, thus obtain monopolistic power over such information. Such power allows them to extract informational rents from borrowers. Another important driver of the relationship's strength is the length of the relationship. In order to learn about their consumers, lenders have repeated interactions with their clients over a period of time. A commonly used proxy for relationship lending is the length of the bank-borrower relationship (see, for example, Petersen and Rajan (1994) and Berger and Udell (1995) among many others). Accordingly, we will combine both the exclusivity of the relationship and its length to offer an additional measure of relationship strength. The first data points in the credit register are from July 2017. The sample period used in this paper begins on August 2018. Therefore, we account for the length of the relationship by restricting the sample to borrowers that had a checking account with the lending bank for at least 12 months before the loan was granted.

Furthermore, restricting both our treated and control groups to borrowers with a longer relationship reduces possible concerns that any promotions at the time of the account opening may be driving our results. In addition, this restriction limits the effect of any unobserved events that may have induced non-exclusive borrowers to open additional checking accounts. Accordingly, this refined definition of exclusivity permits us to better isolate the informational rents extracted by banks resulting from information asymmetry in credit markets. Therefore, we expect that the effect of the introduction of credit scores would be more pronounced under this specification.

Table 5 reports the estimation of Equation 1 after restricting the sample to borrowers who had a relationship at least 12 months prior to the loan, both for exclusive and non-exclusive borrowers. The coefficient estimate on *Exclusive* (β_1) is positive and significant. However, this coefficient is of smaller magnitude compared to our baseline specification. The coefficient estimate on the interaction term $Exclusive * Post$ (β_2) is significant and negative. The relative magnitude of the estimated β_1

and β_2, for the overall sample in column 1, is larger compared to our baseline estimation. In fact, we find a 90 percent decrease in the hold-up premium exclusive versus non-exclusive borrowers pay once credit scores are introduced. Interestingly, when we observe the coefficient estimate for borrowers with good credit history, in column 2, we find that the hold-up premium disappears once credit scores are available. In column 3, we find that for borrowers with bad credit history the premium only decreases by approximately 50%. Taken together, the results reported in Table 5 suggest that once relationship's length is accounted for, the introduction of credit scores reduced most of the hold-up premium. We show that the premia between low risk exclusive and non-exclusive borrowers in this specification is nearly zero in the post period. Consistent with our hypothesis, consumers with longer relationships are more prone to the hold-up problem as their bank is the only lender that is able to assess their creditworthiness. With the introduction of the credit register, these consumers are posed to benefit the most from the decrease in information asymmetry, as more lenders are able to assess their credit information. This effect is most pronounced for lenders with good history.

[**Table 5 to be added here**]

Overall, the results presented in this section suggest that when credit scores were unavailable, banks were able to extract informational rents from exclusive borrowers. Across all specifications in this section, we find that exclusive borrowers paid a higher interest rate on their loans compared to non-exclusive borrowers. That is, consistent with theoretical models, exclusive borrowers were subject to the hold-up problem. We then provide empirical evidence that a decrease in information asymmetry between lenders and consumers resulting from the introduction of credit scores mitigates the hold-up problem. Our baseline estimation is further reinforced by our sample split and more restrictive estimations. Alternative explanations, such as better screening by banks, would predict that the interest rate should decrease for non-exclusive good borrowers. However, we show that the effect of the credit register is negative and significant for borrowers with good credit history across all specifications. Economically, the decrease in the hold-up premia is quite large. In our baseline estimation it is around 30 %. However, when we better isolate the relationship banking strength by restricting the relationship length to at least 12 months, we find that the premia mostly disappears, especially for borrowers with good credit history.

A potential concern with our main specification is that there may be bank specific time-varying factors that are not accounted for in the baseline specification. While borrower fixed-effects and bank fixed effects account for possible borrower-specific and bank-specific time invariant variables, any unobserved bank time-variant factors could bias our results.[16] To address this issue, we add bank-time fixed-effects, which account for time-varying bank-specific factors that may influence the interaction between the strength of bank-borrower relationships and consumer loan interest rates. Results are presented in Table 6. Here

[16]We note that as part of the attempt to promote competition in the banking industry, specific banks went through some structural changes during the sample period. These changes could have potentially impacted their lending strategies. Most notable was a regulation mandating the separation of credit card companies from banks. This regulation has affected so far Hapoalim and Leumi, Israel's two largest banks.

as well we find that the coefficient estimate β_2 is negative and statistically significant. Therefore, our results are robust to the introduction of these more restrictive fixed-effects.

[**Table 6 to be added here**]

In addition, recall that our sample includes loans with one or two borrowers. As noted in Section 2.3, the latter required us to make various decisions on measurements of borrower characteristics like age and available credit line. Throughout our analysis, we control for the number of borrowers, however, these loans may have other differences for which we do not account. To address this concern, we restrict our sample to loans with only one borrower. Thus, this sample is not subject to the same measurement concerns. The results from this estimation are reported in Table 7. We find that here as well, β_2 is negative and statistically significant. Therefore, it appears our results are not merely driven by any of the measurement choices we made when structuring our sample of loans with multiple borrowers.

[**Table 7 to be added here**]

5 Conclusion

In conclusion, our paper provides the first empirical evidence as to the causal impact of credit scores on consumer loan pricing. As suggested by the theoretical literature, we find that banks extract monopolistic rents from households with strong relationship banking. Credit scores help remedy the asymmetric information problem between lenders and borrowers in consumer credit markets. Thus, credit scores mitigate the monopoly power banks hold over consumers' credit information. Consistent with the latter, we find that the introduction of credit scores led to a significant decrease in the hold-up premia charged by banks.

Due to data limitation, the effect of credit scores on relationship banking in the household context has not been thoroughly examined in the literature. Our setting alongside the detailed dataset, provided us with a unique opportunity to contribute to this important literature. We show that households with stronger relationship lending paid higher interest rates on their loans prior to the credit register. This result is consistent with the hold-up problem. Once credit scores become available, we find that this price difference between exclusive and non-exclusive borrowers decreases significantly. This result holds across several specifications and robustness tests. Accordingly, we believe that our results show that once credit information becomes public, the hold-up problem is mitigated. This is an important finding, which as far as we know, was not previously shown empirically for households.

Our paper also contributes to the growing literature examining household financial decisions. In particular, our paper points to the possible effects that relationship banking could have on retail consumers' loan prices. With the rapidly changing financial landscape, especially in the past decades with the increase in financial products and their complexity (Campbell 2006), our paper points to novel evidence as to one of the most basic lending channels. The latter is important, as in order to better our understanding of complex financial interactions, we first need to have a clear understanding of the more basic household lending channels. Furthermore, our findings suggest how regulations aimed at increasing transparency, availability and verifiability of borrowers' credit information can help mitigate informational frictions in financial markets which could eventually improve retail consumers' financial health. Finally, our paper provides a glimpse into the future of banking as information is becoming more accessible with technological improvements. Our results suggest that in the age of information the role of relationship banking may be changing. The latter could have important implications for the banking industry business model.

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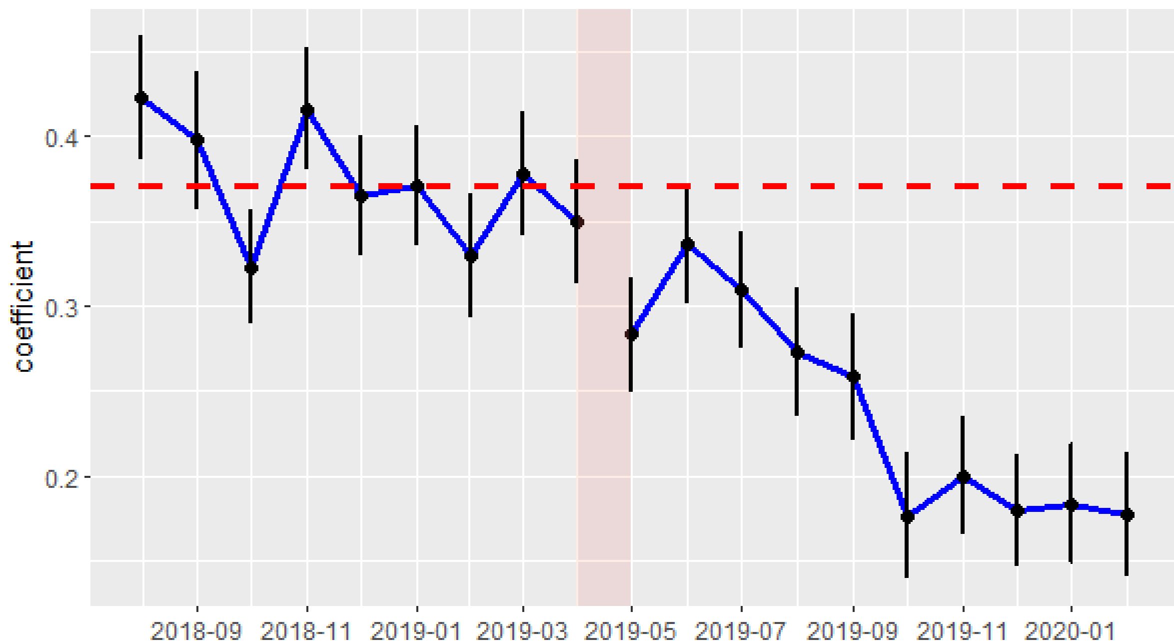
A Tables and Figures

Figure 1: Impact of exclusive relationship by month



Note: This figure reports the impact of exclusive relationship lending on loan spreads by month. The coefficient estimates reported are from the estimation of Equation 1 with interactions between *Exclusive* and monthly dummies for each month from August 2018-February 2020. 90% confidence bands are presented in black. The red dashed line represents estimated coefficient of the interaction between *Exclusive* and *Post* in the baseline specification shown in Table 2.

Table 1: Descriptive Statistics

	Pre				Post			
	n	Mean	St. Dev	Median	n	Mean	St. Dev	Median
Panel A. All Loans								
Exclusive	709831	0.68	0.47	1	698394	0.69	0.46	1
Bad_Hist	709831	0.19	0.39	0	698394	0.17	0.37	0
Spread (%)	709831	5.18	3.20	5.60	698394	4.90	3.04	5.25
Amount (Thousand NIS)	709831	40.38	41.68	25	698394	41.91	42.26	25
Maturity (Month)	709831	43.26	26.28	39.50	698394	43.36	26.34	39.50
Borrowers	709831	1.34	0.47	1	698394	1.35	0.48	1
Credit_Lim (Thousand NIS)	709831	18.30	18.55	13	698394	18.39	18.40	13.50
Utilization	709831	0.62	0.41	0.81	698394	0.60	0.42	0.78
Mortg	709831	0.36	0.48	0	698394	0.36	0.48	0
Age	709831	6.59	2.76	6	698394	6.51	2.76	6
Socio	709831	5.34	2.13	6	698394	5.33	2.13	5
Panel B. Only Exclusive Borrower-Loans								
Bad_Hist	479516	0.12	0.33	0	480194	0.11	0.32	0
Spread (%)	479516	5.50	3.22	6.20	480194	5.19	3.06	5.75
Amount (Thousand NIS)	479516	37.55	39.67	21	480194	38.89	40.18	24
Maturity (Month)	479516	42.28	25.55	36.50	480194	42.22	25.50	36.50
Borrowers	479516	1.28	0.45	1	480194	1.29	0.45	1
Credit_Lim (Thousand NIS)	479516	15.05	15.71	10	480194	15.27	15.76	10
Utilization	479516	0.60	0.42	0.80	480194	0.58	0.43	0.76
Mortg	479516	0.29	0.45	0	480194	0.29	0.45	0
Age	479516	6.40	2.76	6	480194	6.32	2.77	6
Socio	479516	5.28	2.10	5	480194	5.26	2.10	5
Panel C. Only Non-Exclusive Borrower-Loans								
Bad_Hist	230315	0.32	0.47	0	218200	0.29	0.45	0
Spread (%)	230315	4.51	3.07	4.50	218200	4.27	2.89	4.10
Amount (Thousand NIS)	230315	46.25	45.01	30	218200	48.55	45.81	30
Maturity (Month)	230315	45.31	27.63	48	218200	45.88	27.96	48
Borrowers	230315	1.46	0.50	1	218200	1.48	0.50	1
Credit_Lim (Thousand NIS)	230315	25.07	21.89	20	218200	25.27	21.64	20
Utilization	230315	0.66	0.38	0.84	218200	0.63	0.39	0.80
Mortg	230315	0.51	0.50	1	218200	0.52	0.50	1
Age	230315	7	2.70	7	218200	6.93	2.69	7
Socio	230315	5.49	2.19	6	218200	5.50	2.18	6
Panel D. Only Borrowers With Good History								
Exclusive	576766	0.73	0.44	1	581643	0.73	0.44	1
Spread (%)	576766	5	3.23	5.25	581643	4.74	3.06	5
Amount (Thousand NIS)	576766	40.50	41.97	25	581643	42.30	42.64	26
Maturity (Month)	576766	43.16	26.35	37	581643	43.36	26.35	39.50
Borrowers	576766	1.34	0.47	1	581643	1.35	0.48	1
Credit_Lim (Thousand NIS)	576766	18.23	18.26	13	581643	18.37	18.12	14
Utilization	576766	0.58	0.41	0.74	581643	0.57	0.42	0.70
Mortg	576766	0.36	0.48	0	581643	0.36	0.48	0
Age	576766	6.66	2.79	6	581643	6.57	2.79	6
Socio	576766	5.41	2.11	6	581643	5.40	2.11	6

Continued on next page

Table 1 *(continued)*

	Pre				Post			
	n	Mean	St. Dev	Median	n	Mean	St. Dev	Median
Panel E. Only Borrowers With Bad History								
Exclusive	133065	0.45	0.50	0	116751	0.46	0.50	0
Spread (%)	133065	5.95	2.94	6.70	116751	5.73	2.77	6.25
Amount (Thousand NIS)	133065	39.83	40.40	25	116751	39.96	40.28	25
Maturity (Month)	133065	43.71	25.99	47	116751	43.36	26.31	40
Borrowers	133065	1.32	0.47	1	116751	1.34	0.47	1
Credit_Lim (Thousand NIS)	133065	18.61	19.78	12.50	116751	18.49	19.73	12.10
Utilization	133065	0.77	0.37	1	116751	0.75	0.38	0.99
Mortg	133065	0.37	0.48	0	116751	0.37	0.48	0
Age	133065	6.30	2.59	6	116751	6.20	2.61	6
Socio	133065	5.07	2.20	5	116751	5.02	2.21	5
Panel F. All Loans Exclusive VS. Non-Exclusive Borrowers								
	Exclusive				Non-Exclusive			
Bad_Hist	959710	0.12	0.32	0	448515	0.30	0.46	0
Spread (%)	959710	5.34	3.14	6	448515	4.40	2.99	4.40
Amount (Thousand NIS)	959710	38.22	39.93	22.60	448515	47.37	45.42	30
Maturity	959710	42.25	25.52	36.50	448515	45.59	27.79	48
Borrowers	959710	1.28	0.45	1	448515	1.47	0.50	1
Credit_Lim (Thousand NIS)	959710	15.16	15.74	10	448515	25.17	21.77	20
Utilization	959710	0.59	0.43	0.78	448515	0.65	0.39	0.82
Mortg	959710	0.29	0.45	0	448515	0.51	0.50	1
Age	959710	6.36	2.77	6	448515	6.96	2.70	7
Socio	959710	5.27	2.10	5	448515	5.49	2.18	6

Notes: This table presents the descriptive statistics of the observations in our sample. All observations are loan-borrower observations. These include all variables we use in the estimation of our main specification represented in Equation 1. Panel A displays the descriptive statistics for our entire sample. Panel B displays the descriptive statistics of loans originated to exclusive borrowers. Panel C displays the descriptive statistics of loans originated to non-exclusive borrowers. Panel D and E display the descriptive statistics for loans originated to borrowers with good and bad credit history, respectively. In Panels A-E, columns 1 through 4 present loans originated in the period prior to the introduction of credit scores (August 2018 - April 2019). Columns 5-8 present the loans originated in the period following the introduction of credit scores (May 2019 - February 2020). Panel F presents the descriptive statistics of the loans originated to exclusive borrowers (columns 1-4) and non-exclusive borrower (columns 5-8) for the entire sample period. See Section 2.3 for details on the construction of the sample and the variables.

Table 2: Baseline Estimations

	Spread		
	All	Good Hist.	Bad Hist
	(1)	(2)	(3)
Exclusive	0.371^{***}	0.328^{***}	0.518^{***}
	(0.009)	(0.011)	(0.018)
Exclusive $*$ *Post*	-0.133^{***}	-0.131^{***}	-0.153^{***}
	(0.010)	(0.012)	(0.023)
Amount	-0.015^{***}	-0.016^{***}	-0.013^{***}
	(0.0001)	(0.0001)	(0.0002)
Maturity	0.005^{***}	0.006^{***}	-0.001^{*}
	(0.0001)	(0.0002)	(0.0003)
Bad_Hist	0.794^{***}		
	(0.008)		
Mortg	-0.647^{***}	-0.648^{***}	-0.661^{***}
	(0.007)	(0.008)	(0.015)
Socio	-0.117^{***}	-0.119^{***}	-0.102^{***}
	(0.002)	(0.002)	(0.003)
Age	0.057^{***}	0.054^{***}	0.088^{***}
	(0.001)	(0.001)	(0.003)
Credit_lim	-0.031^{***}	-0.034^{***}	-0.021^{***}
	(0.0002)	(0.0003)	(0.0004)
Borrower_Number	-0.487^{***}	-0.493^{***}	-0.362^{***}
	(0.007)	(0.008)	(0.015)
Utilization	1.423^{***}	1.489^{***}	0.965^{***}
	(0.008)	(0.008)	(0.018)
Bank F.E	Y	Y	Y
Time F.E	Y	Y	Y
Observations	1,408,225	1,158,409	249,816
R^2	0.315	0.328	0.206

Notes: This table reports the coefficient estimates of Equation 1: $Spread_{i,j,k,t} = \gamma_k + \delta_t + \beta_1 Exclusive_{j,k} + \beta_2 Exclusive_{j,k} * Post_t + \beta_3 X_i + \beta_4 Z_{j,t} + e_{i,j,k,t}$. The sample in column 1 includes all the loans in our sample. The samples in columns 2 and 3 are restricted to borrowers with good and bad credit histories, respectively. See Section 2.3 for details on the construction of the sample and the variables. The time period is August 2018-February 2020. Standard errors clustered by borrower are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table 3: Decomposition of the Premia Decrease

	Spread		
	All	Good Hist.	Bad Hist
	(1)	(2)	(3)
Exclusive	0.371^{***}	0.327^{***}	0.519^{***}
	(0.009)	(0.011)	(0.018)
$Exclusive * Post$	-0.155^{***}	-0.147^{***}	-0.214^{***}
	(0.006)	(0.006)	(0.017)
$Non-Exclusive * Post$	-0.023^{***}	-0.019^{*}	-0.060^{***}
	(0.009)	(0.010)	(0.016)
Amount	-0.015^{***}	-0.016^{***}	-0.013^{***}
	(0.0001)	(0.0001)	(0.0002)
Maturity	0.005^{***}	0.006^{***}	-0.0005^{*}
	(0.0001)	(0.0002)	(0.0003)
Bad_Hist	0.796^{***}		
	(0.008)		
Mortg	-0.648^{***}	-0.649^{***}	-0.662^{***}
	(0.007)	(0.008)	(0.015)
Socio	-0.117^{***}	-0.119^{***}	-0.102^{***}
	(0.002)	(0.002)	(0.003)
Age	0.058^{***}	0.055^{***}	0.089^{***}
	(0.001)	(0.001)	(0.003)
Credit_lim	-0.031^{***}	-0.034^{***}	-0.021^{***}
	(0.0002)	(0.0003)	(0.0004)
Borrower_Number	-0.487^{***}	-0.493^{***}	-0.362^{***}
	(0.007)	(0.008)	(0.015)
Utilization	1.425^{***}	1.491^{***}	0.968^{***}
	(0.008)	(0.008)	(0.018)
Bank F.E	Y	Y	Y
Observations	1,408,225	1,158,409	249,816
R^2	0.314	0.327	0.205

Notes: This table reports the coefficient estimates of the following regression: $Spread_{i,j,k,t} = \gamma_k + \beta_1 Exclusive_{j,k} + \beta_2 Exclusive_{j,k} * Post_t + \beta_3 NonExclusive_{j,k} * Post_t + \beta_4 X_i + \beta_5 Z_{j,t} + e_{i,j,k,t}$. See Section 2.3 for details on the construction of the sample and the variables. The time period is August 2018-February 2020. Standard errors clustered by borrower are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table 4: Estimations Including Borrower Fixed Effects

	Spread		
	All	Good Hist.	Bad Hist
	(1)	(2)	(3)
Exclusive ∗ Post	-0.147^{***}	-0.111^{***}	-0.175^{***}
	(0.011)	(0.013)	(0.030)
Amount	-0.009^{***}	-0.009^{***}	-0.008^{***}
	(0.0001)	(0.0001)	(0.0003)
Maturity	0.0002	0.001^{***}	-0.002^{***}
	(0.0002)	(0.0002)	(0.001)
Bad_Hist	0.124^{***}		
	(0.014)		
Mortg	0.113^{***}	0.102^{***}	0.123
	(0.033)	(0.036)	(0.098)
Credit_lim	0.002^{***}	0.001^{*}	0.003^{**}
	(0.001)	(0.001)	(0.001)
Utilization	0.327^{***}	0.299^{***}	0.447^{***}
	(0.012)	(0.013)	(0.037)
Bank F.E	Y	Y	Y
Time F.E	Y	Y	Y
Observations	610,545	511,248	99,297
R^2	0.808	0.829	0.801

Notes: This table reports the coefficient estimates of Equation 2: $Spread_{i,j,k,t} = \gamma_k + \delta_t + \mu_j + \beta_2 Exclusive_{j,k} * Post_t + \beta_3 X_i + \beta_4 Z_{j,t} + e_{i,j,k,t}$. The sample is restricted to borrowers with at least one loan in the pre-credit register period and at least one in the post-credit register period. See Section 2.3 for details on the construction of the sample and the variables. The time period is August 2018-February 2020. Standard errors clustered by borrower are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table 5: Long Banking Relationship

	Spread		
	All	Good Hist.	Bad Hist
	(1)	(2)	(3)
Exclusive	0.150^{***}	0.111^{***}	0.330^{***}
	(0.011)	(0.012)	(0.022)
Exclusive ∗ Post	-0.141^{***}	-0.137^{***}	-0.160^{***}
	(0.012)	(0.013)	(0.028)
Amount	-0.014^{***}	-0.014^{***}	-0.011^{***}
	(0.0001)	(0.0001)	(0.0002)
Maturity	0.009^{***}	0.010^{***}	0.004^{***}
	(0.0002)	(0.0002)	(0.0003)
Bad_Hist	0.603^{***}		
	(0.009)		
Mortg	-0.583^{***}	-0.585^{***}	-0.579^{***}
	(0.008)	(0.009)	(0.018)
Socio	-0.109^{***}	-0.111^{***}	-0.095^{***}
	(0.002)	(0.002)	(0.004)
Age	0.065^{***}	0.062^{***}	0.092^{***}
	(0.001)	(0.001)	(0.003)
Credit_lim	-0.033^{***}	-0.036^{***}	-0.022^{***}
	(0.0003)	(0.0003)	(0.0005)
Borrower_Number	-0.375^{***}	-0.379^{***}	-0.255^{***}
	(0.008)	(0.009)	(0.017)
Utilization	1.743^{***}	1.744^{***}	1.625^{***}
	(0.009)	(0.009)	(0.027)
Bank F.E	Y	Y	Y
Time F.E	Y	Y	Y
Observations	1,099,905	932,527	167,378
R^2	0.325	0.337	0.217

Notes: This table reports the coefficient estimates of Equation 1 when we restrict the sample to borrowers who held a checking account with the lending bank for at least a year prior to the loan's origination date. See Section 2.3 for details on the construction of the sample and the variables. The time period is August 2018 - February 2020. Standard errors clustered by borrower are reported in parentheses.*p<0.1; **p<0.05; ***p<0.01

Table 6: Bank-Time Fixed Effects

	Spread		
	All	Good Hist.	Bad Hist
	(1)	(2)	(3)
Exclusive	0.358***	0.317***	0.504***
	(0.009)	(0.011)	(0.018)
Exclusive ∗ Post	−0.108***	−0.110***	−0.130***
	(0.011)	(0.012)	(0.023)
Amount	−0.015***	−0.016***	−0.013***
	(0.0001)	(0.0001)	(0.0002)
Maturity	0.004***	0.005***	−0.001***
	(0.0001)	(0.0002)	(0.0003)
Bad_Hist	0.794***		
	(0.008)		
Mortg	−0.642***	−0.643***	−0.659***
	(0.007)	(0.008)	(0.015)
Socio	−0.117***	−0.119***	−0.102***
	(0.002)	(0.002)	(0.003)
Age	0.058***	0.055***	0.089***
	(0.001)	(0.001)	(0.003)
Credit_lim	−0.031***	−0.034***	−0.021***
	(0.0002)	(0.0003)	(0.0004)
Borrower_Number	−0.497***	−0.503***	−0.368***
	(0.007)	(0.008)	(0.015)
Utilization	1.415***	1.480***	0.961***
	(0.008)	(0.008)	(0.018)
Bank-time F.E.	Y	Y	Y
Observations	1,408,225	1,158,409	249,816
R^2	0.319	0.332	0.212

Notes: This table reports the coefficient estimates of Equation 1 including bank-time fixed effects instead of bank F.E and time F.E. See Section 2.3 for details on the construction of the sample and the variables. The time period is August 2018-February 2020. Standard errors clustered by borrower are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table 7: Sample Restricted to Households Comprised of One Borrower

	Spread		
	All	Good Hist.	Bad Hist
	(1)	(2)	(3)
Exclusive	0.578***	0.561***	0.636***
	(0.017)	(0.021)	(0.027)
*Exclusive * Post*	−0.263***	−0.262***	−0.273***
	(0.019)	(0.025)	(0.034)
Amount	−0.018***	−0.019***	−0.013***
	(0.0001)	(0.0001)	(0.0003)
Maturity	0.007***	0.007***	0.003***
	(0.0002)	(0.0002)	(0.0004)
Bad_Hist	0.785***		
	(0.011)		
Mortg	−0.633***	−0.624***	−0.611***
	(0.017)	(0.019)	(0.040)
Socio	−0.125***	−0.126***	−0.115***
	(0.002)	(0.002)	(0.005)
Age	0.062***	0.061***	0.086***
	(0.002)	(0.002)	(0.003)
Credit_lim	−0.044***	−0.048***	−0.026***
	(0.0005)	(0.001)	(0.001)
Utilization	1.096***	1.163***	0.611***
	(0.010)	(0.011)	(0.023)
Bank F.E	Y	Y	Y
Time F.E	Y	Y	Y
Observations	685,226	570,185	115,041
R^2	0.262	0.282	0.140

Notes: This table reports the coefficient estimates of Equation 1 when we restrict our sample to loans originated to households comprised of one borrower. See Section 2.3 for details on the construction of the sample and the variables. The time period is August 2018-February 2020. Standard errors clustered by borrower are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table 8: Sample Split by Socioeconomic Rank

	Spread			
	Low	Low-Medium	Medium	High
	(1)	(2)	(3)	(4)
Exclusive	0.493^{***}	0.360^{***}	0.334^{***}	0.214^{***}
	(0.019)	(0.020)	(0.017)	(0.019)
Exclusive ∗ Post	-0.139^{***}	-0.135^{***}	-0.162^{***}	-0.057^{***}
	(0.021)	(0.023)	(0.019)	(0.021)
Amount	-0.015^{***}	-0.018^{***}	-0.016^{***}	-0.013^{***}
	(0.0002)	(0.0002)	(0.0001)	(0.0002)
Maturity	0.002^{***}	0.006^{***}	0.006^{***}	0.005^{***}
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Bad_Hist	0.769^{***}	0.787^{***}	0.823^{***}	0.824^{***}
	(0.014)	(0.016)	(0.015)	(0.018)
Mortg	-0.728^{***}	-0.626^{***}	-0.648^{***}	-0.593^{***}
	(0.016)	(0.015)	(0.013)	(0.015)
Socio	-0.193^{***}	-0.080^{***}	-0.171^{***}	-0.236^{***}
	(0.010)	(0.013)	(0.012)	(0.023)
Age	0.088^{***}	0.072^{***}	0.044^{***}	0.032^{***}
	(0.002)	(0.002)	(0.002)	(0.003)
Credit_lim	-0.030^{***}	-0.040^{***}	-0.033^{***}	-0.026^{***}
	(0.0005)	(0.001)	(0.0005)	(0.0004)
Borrower_Number	-0.582^{***}	-0.350^{***}	-0.444^{***}	-0.532^{***}
	(0.015)	(0.015)	(0.013)	(0.016)
Utilization	1.111^{***}	1.377^{***}	1.611^{***}	1.653^{***}
	(0.014)	(0.016)	(0.014)	(0.017)
Bank F.E	Y	Y	Y	Y
Time F.E	Y	Y	Y	Y
Observations	372,046	329,854	420,657	285,668
R^2	0.286	0.299	0.315	0.310

Notes: The table reports the coefficient estimates of Equation 1, splitting the sample by the socioeconomic rank of the borrower's town; Low is ranking 1-3, Low-Medium is ranking 4-5, Medium is ranking 6-7, and High is ranking 8-10. See Section 2.3 for details on the construction of the sample and the variables. The time period is August 2018-February 2020. Standard errors clustered by borrower are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01