The Effect of Information Sharing on Relationship Banking: Evidence from a Natural Experiment*

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Abstract

We exploit a natural experiment to provide one of the first measurements of the causal effect of new credit information on consumer-bank relationship lending. Utilizing an external shock to the amount of information available to banks about the universe of borrowers, we find that increase in information reduces the interest rates for consumer with strong relationship banking. Specifically, we show that before the information shock, borrowers which are more prone to the hold-up problem paid higher interest rates. This is consistent with theoretical models as banks extract rents from these borrowers. We then show that an exogenous reduction in asymmetric information significantly reduces the interest rate for consumers with stronger relationship banking. Our results demonstrate how regulation promoting credit information sharing can help mitigate the hold-up problem.

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1 Introduction

Information asymmetries between lenders and borrowers have long been examined in the academic literature. Financial intermediation theories offer a way to overcome this informational gap through relationship banking (Boot, 2000). Interestingly, while the literature has argued extensively that relationship lending is mostly used to deal with information frictions, to the best our knowledge, little empirical evidence exists on the actual impact information problems have on the use and importance of relationship lending. Furthermore, while relationship lending 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 in Israel, we present new evidence as to the importance of relationship banking for retail consumers.

In this study, we use the introduction of a consumer credit register and credit scores in Israel to test how changes in the information available to banks impact retail relationship lending. The introduction of the Israeli credit register increased the amount of information available regarding retail consumers. Relationship lending theory postulates that through relationships with customers, a bank gathers private information, which results in a comparative advantage in lending as opposed to non-relationship banks.[1] This could lead to the hold-up problem, whereas 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. Our findings confirm our 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 Israeli 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

[1] Please see Greenbaum et al. (2019) for a detailed recent overview of relationship lending.

section 2. This extensive dataset permits us to compare the impact that strong versus weak banking relationships have 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. We then 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 main 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 the credit register introduction. 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 (hold-up problem) (Sharpe, 1990; Rajan, 1992; Farinha and Santos, 2002; Bonfim et al., 2018). We then test the impact of a shock to retail consumer credit information. We find that the credit register's introduction significantly mitigates 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. Our findings are consistent with our main hypothesis that once information asymmetry is reduced, the hold-up problem is mitigated. 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 proceed to provide a battery of tests to show that our results are robustness 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 first include borrower-fixed-effects in our estimation. This estimation accounts for all borrowers' time-invariant characteristics. We also run our estimation on a restricted sample to alleviate selection concerns. This sample is composed of borrowers who had a loan before and after the credit register was introduced. This estimation uses within borrowers' variation, hence asymmetric changes in borrower composition across groups does not impact our estimates. Our findings are robust across all specifications, therefore alleviating endogeneity concerns and support a causal interpretation between the decrease in information asymmetry and relationship banking. In section 5, we offer additional tests demonstrating our results are robust to alternative measures of relationship banking strength.

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.[2] 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). At the same time, other studies have suggested that long borrower-lender relationships can lead to hold-up problem, as borrowers become locked-in their 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 relationship banking give 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 prior to

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

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. In contrast, in our paper we examine how relationship banking impact consumers, similar to Puri et al. (2017). Chakravarty and Scott (1999) use survey data to examine the effect of relationships on credit rationing for households. The literature so far focused mostly on loan performance and credit availability. 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 its customers. In contrast to most of this literature, we are using 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. Theortical 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 discplines borrowers as nonpayment is 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 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 loans, whereas we look at the universe of consumer non-collateralized loans. 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 main results. Section 5 shows our robustness tests, and section 6 concludes.

2 Institutional Setting and Sample Construction

2.1 Institutional details

Israel's consumer credit market is bank-based; as of December 2019, about 77% of consumer credit was granted by banks. Furthermore, this banking system is fairly concentrated. It consists of seven bank groups whereas the market share of the two largest banks exceeds 50% of the total credit allocated by in the banking system. The Israeli financial system went 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 on 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 one or both of the credit bureaus to obtain potential borrowers' credit scores and additional credit history. 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"). This data is then transmitted to two private credit bureaus, created following the law, which compute the credit

[3] For instance relevant to our paper is the estsblishment of the "Strum committee" in 2015 with the goal of increasing competition within the banking system.

scores based on such information on a case by case basis.[4]

2.2 Definition of relationship banking

A key component of the analysis is identifying the strength of relationship lending. First, we define a relationship loan as a loan granted to a borrower by the bank where she holds an existing checking account. Then, we measure the strength of this relationship using the number of banking relationships each borrower has. Specifically, we focus on borrowers who interact with one bank solely (exclusive relationships) versus borrowers who have multiple banking relationships. We denote our relationship variable as *Exclusive* which takes the value of 1 if the loan is granted by a bank where the borrower has its sole checking account.

As noted in the literature, the management of a deposit provides critical information on one's cash-flows, thus naturally relates to the strength of the relationship between lenders and borrowers (Mester et al., 2007; Norden and Weber, 2010). Indeed, Puri et al. (2017) shows that having a transaction account at the bank significantly reduces consumer loans' default probability. Moreover, in Israel, a typical line of credit is an overdraft from one's main checking account. This credit line is similar to rollover credit card debt in the US (which are uncommon in Israel). Thus, this relationship is important for the bank's ability 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) notes 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 (Elsas, 2005).

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 monthly. Our sample includes all

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

non-securitized consumer loans granted by Israeli banks for the period spanning from August 2018 to February 2020.[5] This period represents the longest available period for which we have all the variables prior to the Covid-19 global pandemic.

To arrive at the final sample, we apply several filters aimed at making the sample as homogeneous as possible to reduce any bias associated with unobserved differences between exclusive and non-exclusive relationship loans. 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 have a checking account) with the 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. 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. We further restrict the sample to borrowers with credit history. That is, we exclude borrowers who took a loan in the month they opened their first checking account.

We exclude observations where there are more than two recorded borrowers.[6] We exclude any loan where the loan maturity is very short (less than one month) as these loans are unlikely to be consumer loans. We also exclude unrealistic observations where the principal amount or the annualized nominal interest rate is zero and any observations where there are missing control variables. 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 1,071,429 loans. For any estimation which uses borrower fixed effects, we keep only borrowers

[5] Since we are focusing on consumer loans we exclude from the dataset any loan where the purpose of the loan is documented as either "business" or "loan to a corporation or with a corporation" or if one of the borrowers is a licensed dealer.

[6] We apply this restriction for two reasons. First the vast majority of loans are to one or two borrowers (more than 99%) with the split between them at around 70 percents individuals and 30 percents of pair 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 5 we show that the results are robust to keeping only loans made to a singe borrower.

who had at least two loans during the sample period, which reduces the sample to approximately 700,000 loans. We also have approximately 500,000 loans that were taken by the same borrower before and after the introduction of the credit register.

Our main dependent variable is *Spread* which represents the spread between a loan's nominal annualized interest rate and the baseline Israeli interest rate.[7] In our estimation, we control for both loan specific and borrower specific characteristics. Loan controls include: loan size (*Amount*) in thousands NIS, [8] length of the loan in months at the time it was granted ($Maturity$), and the number of borrowers. Our borrower specific variables include: age group (*Age*), the socioeconomic rank of the borrower's city (*Socio*), mortgage ($Mortg$), credit line ($Credit\_Lim$), and risk (Bad_Hist).[9] 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 borrower's residence city or town. 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 10 the highest. Using this index we define *Socio* as an ordinal variable for each borrower.[11] $Mortg$ is a dummy variable which equals 1 if any of the borrowers has an outstanding mortgage. $Credit\_Lim$ is the credit line (overdraft) available to withdraw from the borrower's checking 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. 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 1 if at least one of the borrowers had

[7] The Prime lending rate is the basic debitory interest rate agreed by the banks. It currently stands at a fixed spread above the interest rate set by the Bank of Israel. This rate is the basis for setting interest rates for bank products, such as deposits and loans bearing variable interest.

[8] NIS corresponds to New Israeli Shekel and is the local currency used in Israel. 3.5 New Israeli Shekels are equivalent to approximately 1 US Dollar.

[9] Unfortunately, due to privacy issues, borrower specific variable are quite limited in the register; we are currently working on expanding these set of variables.

[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] When a loan includes two borrowers we take the minimum socio index and the minimum age group between the two borrowers. Results are almost unchanged if we take the average or the max instead.

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

a credit facility (loan/mortgage/credit card/credit line) where she was in arrears in the year before the loan was granted.[13]

Table 1 provides descriptive statistics of the main variables used in the analysis. Panel A distinguishes the pre-credit register period and the post-credit register period for all loans. Panel B includes only exclusive relationship loans and Panel C only non-exclusive relationship loans. As we can see the number of loans in each period is very similar with 50.3% of the loans given before the register introduction and the rest after.

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

Examining the borrowers' controls in Panel A, it seems that overall the two samples do not display economically meaningful differences. Borrowers with bad credit history represent 9% in period before the credit register and 8% in the period after the credit register. On average 35% of the borrowers in our sample have a mortgage in both periods. The median age-group of borrowers across the sample includes ages from 40 to 44 years old. We find that the share of exclusive loans is about 73% and 74% in the pre and post periods. We can also see that some of the loan characteristics change between the pre and the post period. For example, on average, loans given in the post-period are 3,000 NIS larger relative to loans given in the pre-period. Comparing panels B and C we see that non-exclusive borrowers tend to be on average older and have a mortgage. In addition, loans to non-exclusive borrowers tend to be larger and with longer maturity. Interestingly, while exclusive borrowers tend to be less risky than non-exclusive borrowers, on average they pay a higher spread on their loans. This observation is consistent with the main thesis of this paper. When the relationship between a bank and his borrower is exclusive, the bank is able to extract monopolistic rent from his consumers. Thus the descriptive statistics presented in Table 1 suggest exclusive borrowers pay a premia on their loans due to the hold-up problem. To test this conjecture empirically we detail proceed to detail our empirical strategy in the next section.

[13] It is important to note that our risk classification is imperfect since it may fail to identify other factors that lenders consider when assessing borrowers' riskiness. Ideally, we would have access to each consumer credit score calculated by the public credit register and the lender's internal score. However, we believe that our classification is at worst, underestimating borrowers' risk, classifying some borrowers as low risk despite being treated by lenders as high risk.

3 Empirical methodology

Our empirical methodology is designed to test the effect of information sharing on exclusive relationship banking. Theory suggests that in concentrated markets, stronger relationship banking leads to the hold-up problem (Petersen and Rajan, 1995). Prior to the introduction of the credit register, a bank, who maintained an exclusive relationship with a consumer, had monopoly over the information it collected through their relationship. Therefore, the bank could extract rents from such consumers. Thus, we expect to find that exclusive borrowers, all else constant, paid higher interest rates on their loans in the period before the credit register. The introduction of the credit register made consumers' credit information public. Thus, decreasing the monopolistic power of the bank over such information. Accordingly, we hypothesize the hold-up problem would attenuate for consumer most prone to it. That is, we expect that, all else constant, the interest rate paid by consumers with exclusive banking relationship would decrease after the register became available.

Our identification strategy relies on the differential effect of information shock on exclusive versus non-exclusive borrowers. To estimate this effect we use a difference-in-differences specification. Our treated group are borrowers with exclusive banking relationships. Our control group are borrowers with non-exclusive banking relationships. The information shock we are using is the introduction of the credit register in Israel. Exclusive borrowers are more prone to the hold-up problem thus we expect that once the information shock occurs they would be most effected. 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 one if borrower j has an exclusive relationship with lender k. $Post_t$ is an indicator representing the credit register introduction, it equals 1 if the observation is on or 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 two-way clustered at the month and lender levels.[14] 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. Figure 1 presents the weighted mean of the loan spread for the two groups (exclusive and non-exclusive). We can see that exclusive borrowers paid on average higher spreads throughout the entire sample. The latter is despite the fact that on average exclusive borrowers were less risky as we can see in Table 1. Second, while the spreads for both groups have an overall downward trend, the two series exhibit remarkable co-movement, suggesting the parallel trend assumption holds. We further provide evidence supporting this assumption in Section 4 below.

[Figure 1 to be added here]

Estimating Equation 1 poses several challenges. First, we need to account for the possibility that the observed lending terms are endogenous since they are conditional on selecting borrowers with specific characteristics to exclusive and non-exclusive borrowers. In Table 1, Panels B and C compare the populations of exclusive and non-exclusive borrowers respectively. 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 and credit limit. Nonetheless, it is possible that there are unobserved consumer characteristics that might be correlated with consumers having one or multiple bank relationships. Most important, if these unobserved attributes also impact loan prices, our results would be biased. To further

[14] We include time fixed effects in our baseline specification as interest rates overall changed during our sample period. We also run our estimation including *Post* as an independent term without including time fixed effects. We find that our results remain quantitatively the same.

deal with these concerns, we introduce borrower fixed effects in our specification.[15] 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 represent borrower fixed effects. To estimate this specification we restrict our sample to borrowers with at least two loans throughout the examined time period.[16] Our underlining 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 register. The latter could result from strategic timing of new lending for borrowers with specific 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' creditworthinessis most relevant for non-exclusive borrowers in our setting and has different potential pricing effect depending on

[15] Please note, when building our sample we included only borrowers who remain exclusive and non-exclusive throughout the entire time period studied. This restriction limits any impact of unobserved events that may have induced borrowers with specific characteristics to shift between the groups.

[16] 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.

households' risk level.[17] 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. [18]

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 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, we propose a more restrictive regression estimation to deal with all the issues denoted. To control for unobserved borrower characteristics, strategic timing, and screening, we limit the sample to borrowers with at least one loan prior to and at least one after the credit register introduction. This estimation uses the within-borrower variation to estimate the relative effect of exclusivity on loan spreads before and after the credit register introduction, thereby reducing any impact of asymmetric changes in borrower composition.

4 Results

4.1 Estimation 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 on average exclusive loans are more expensive than non-exclusive loans. This is consistent with exclusive borrowers been subjected to the hold-up problem. The size of the coefficient estimate on *Exclusive* suggests that before introducing the credit register, all else being equal, exclusive relationship loans paid around 40 basis points more relative to non-exclusive relationship loans. To put this number in perspective, note from Table 1 that the average spread

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

[18] We should note that we are working on obtaining data on loan applications and hope to include it in future drafts of this paper. This would permit us to further show that our results are not derived from any selection or composition concerns.

for non-exclusive loans in the pre-credit register period was around 449 basis points. Therefore, the additional premia paid by exclusive borrowers represents around an 9% increase in the price of the loan which is economically meaningful.

[Table 2 to be added here]

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. We find it is negative and significant at 10% across all three columns. That is the premia paid by exclusive borrowers compared to non-exclusive borrowers was cut by approximately a third (14 basis points) once the credit register became public. This finding shows that the hold-up problem is been attenuated after the credit register. Thus, our results demonstrate that once information asymmetry between banks and households decreases, rents extracted by banks from exclusive borrowers significantly decease.

From Table 2 we also learn the effect of our control variables on the spread. As expected on average having a bad credit history 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 risk in columns 2 and 3, overall we find that the direction of the coefficients estimates is consistent with column 1. On average exclusive borrowers with good credit history pay 38.4 basis points more than non-exclusive borrowers. Exclusive borrowers with bad credit history pay on average 46.2 basis points more than non-exclusive borrowers with bad credit history. We further show that β_2 is negative and significant for both borrowers with good and bad credit history. 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 the credit register could have improved banks' screening ability which most likely impacted non-exclusive borrowers more than exclusive borrowers. Thus potentially biasing our results. Our findings in Table 2 suggest otherwise, especially if we focus on column 2. That is, if our findings were solely driven by changes to 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 the non-exclusive low risk borrowers only weaken our findings. In fact, we could view the estimated β_2 as the lower bound of our effect. 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 difference in the approval probability between exclusive and non-exclusive borrowers. Accordingly, our findings in column 2 further reinforce our causal interpretation of β_2. Overall the results from Table 2 are consistent with the prevalence of the hold-up problem. Banks on average charge exclusive borrowers a higher interest rate on their consumer loans. We then show that once the credit register is available and credit information becomes public, the premia paid by exclusive borrowers significantly decreases.

To further examine the dynamic shift in the impact of stronger relationship lending following the credit register, we reestimate Equation (1), where the interaction between the exclusive dummy and Post was replaced with a set of interactions between *Exclusive* and a dummy for each month in our sample period. The coefficient estimates of the 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 2, along with 90% confidence bands. For comparison, the figure also plots the coefficient of the exclusive dummy presented in Table 2.

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

From the plot, we see that the impact of exclusive loans was quite volatile before the credit register and did not show any clear direction, moving around the estimated impact from Table 2. However, immediately after the credit register became operative, we see a drop and a clear and 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. The results from the estimation of Equation 2 are presented in Table 3. As noted above, since we include borrower fixed effects, the coefficient on *Exclusivity* (β_1), as well as some of the time-invariant borrower controls, are absorbed.

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

Our main coefficient of interest is β_2, the coefficient on the interaction term between *Exclusive* and *Post*. Similar to our baseline estimation, we find that β_2 is negative and statistically significant. 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 (*Exclusivity*).

In order to further show that our results are not biased by any selection concerns discussed in Section 3, we restrict our sample to borrowers with loans before and after April 2019. Table 4 displays the coefficient estimates of Equation 2 using this restricted sample. Here as well, our main cofficient of interest is β_2, the coefficient on the interaction term between *Exclusive* and *Post*. Similar to the results in the previous tables, we find it is negative and statistically significant in columns 1 and 2. The coefficient estimate in column 3 is negative but no longer significant at 10%. Nonetheless, we believe that our main results are supported by these empirical evidence. We find a negative and significant effect of the register for the overall sample and borrowers with good credit history. These results provide further empirical support that our main results were not merely driven by borrowers' selection and banks' screening.

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

Taken together the findings in Tables 3 and 4 show that information sharing reduced exclusive borrowers loans' prices relative to non-exclusive borrowers. Therefore, these tables reinforce our claim that once information asymmetry decreases the hold-up problem attenuates.

Overall, the results presented in this section suggest that prior to the credit registy exclusive borrowers were subject to the hold-up problem and paid a higher interest rate on their loans compared to non-exclusive borrowers. We then provide empirical evidence that a decrease in information asymmetry between lenders and consumers 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 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. Thus, further supporting our hold-up story.

5 Robustness

So far, we used *Exclusivity* as our measurement for relationship banking strength. Alongside, multiple banking relationships, other measures have been offered in the literature to proxy for relationship banking. A commonly used proxy for relationship lending is the length of the bank-borrower relationship (see for example, Petersen and Rajan (1994); Berger and Udell (1995) among many others). Therefore, we propose alternative definitions for exclusivity which account for the time dimension as well. First, we expand our definition so that an exclusive borrower is one who did not have a different bank-borrower relationship for at least a year before the loan was granted ($Alternative_1$). This restriction reduces any possibility of changes to the number of relationships close to the time the loan was granted. Next, following Berger and Black (2011) we combine checking account exclusivity and the relationship's length to measure the relationship's strength. Specifically, we define a strong bank-borrower relationship as one where the borrower has a checking account only with the lending bank and the account has been opened for at least a year ($Alternative_2$).

We repeat our estimations from the previous section using these two alternative definitions. Tables 5 through 10 report these coefficients estimates using $Alternative_1$ and $Alternative_2$. For both alternative definitions we find that the coefficients on the interaction terms $Exclusivity * Post$ are negative and significant. Interestingly, our results are stronger using these alternative definitions.

Examining $Alternative_2$ the more restrictive alternative definition, we find that on average exclusive borrowers pay approximately 32 basis points more than non-exclusive borrowers (the coefficient estimate on β_1). As we noted above this finding is consistent with the existence of the hold-up problem. Once the credit register is introduced, we find that this price difference significantly decreases by approximately two-thirds (21.5 basis points). This effect is larger than the effect we find in Table 2. This finding is consistent with the conjecture that consumers with longer relationships with their banks are more prone to the hold-up problem. Therefore, once the problem is mitigated by the introduction of the credit register, the observed effect for these borrowers is larger.

A possible concern our main specification does not address is if any bank specific time-varying factors bias our results. 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.[19] To address this issue, we add bank-time fixed-effects which accounts for time-varying bank-specific factors that may influence the interaction between the strength of bank-borrower relationships and consumer loan interest rates.[20] Results are presented in Table 11. Here 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.

Finally, recall that our sample includes loan to both individuals and pairs. This required certain 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 additional differences which we do not account for. To address this concern, we restrict our sample to loans with only one borrower. This sample is not subject to the same measurement concerns. The results from this estimation are reported in Table 12. 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.

[19] We should 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 notably was a regulation mandating the separation of credit card companies from banks. This regulation affected so far Hapoalim and Leumi, Israel's two largest banks.

[20] In order to keep the number of tables reasonable in our robustness tests, we repeat our estimations using our most restrictive specification - estimating Equation 2 using our restricted sample. Our results however hold using any of the other less restrictive specifications.

6 Conclusion

In conclusion, our paper provides new empirical evidence as to the importance of relationship banking in the household context. As the theoretical literature suggests we find empirical evidence confirming retail relationship banking is used to mitigate information asymmetry between lenders and borrowers. So far the empirical literature has mostly focused on firms and commercial lending. Our paper provides novel evidence on the effect of relationship banking for households.

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 the credit register is introduced, we find that this price difference between exclusive and non-exclusive borrowers significantly decreases. This results 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 relationship banking could have on retail consumers' loan prices. With the rapidly changing financial landscape, especially in the past decades with the increase financial products and their complexity (Campbell, 2006), our paper points to novel evidence as to one of the most basic lending channel. 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 takes advantage of a novel dataset provided by the introduction of the credit register in Israel. We believe that there are ample opportunities to examine important questions relating to household finance and the banking industry using this unique dataset. At first stage, we hope to obtain further information from the register so to further strengthen the results in this paper. At a later stage, we hope to take advantage of this data to better our understanding of banks and households' financial decisions more broadly.

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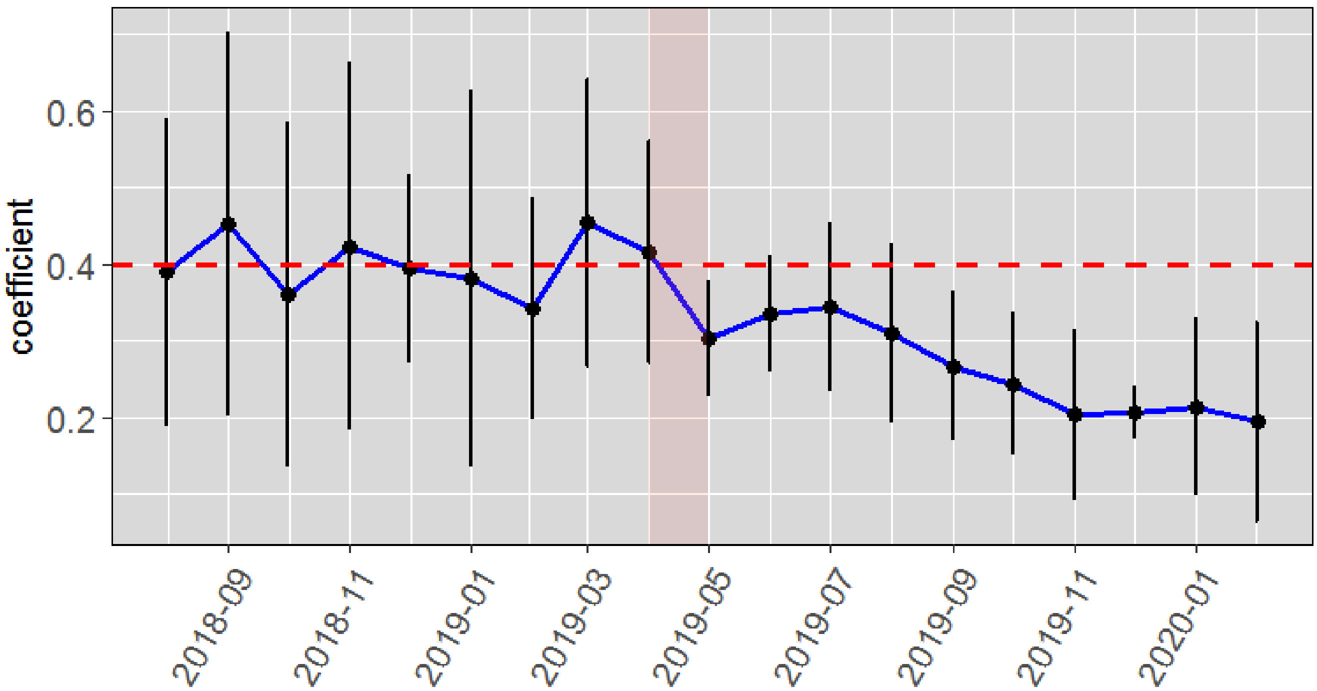
A Figures

Figure 1: Spreads

Note: The figure shows the monthly weighted average spread of new consumer loans for exclusive and non-exclusive borrowers. See Section 2.3 for details on construction of sample and variables. The time period is August 2018-February 2020.

Figure 2: Impact of exclusive relationship by month



Note: This figure reports the impact of exclusive relationship lending on loan spreads by month. The parameter estimates reported are for estimating Equation 1 with interactions between *exclusive* and monthly dummies for each month from August 2018-February 2020. 90% confidence bands are also presented as well as the coefficent of the interaction between *Exclusive* and *Post* estimated in Table 2.

B Tables

Table 1: Descriptive Statistics

	Pre				Post			
	n	Mean	St. Dev	Median	n	Mean	St. Dev	Median
Panel A. all loans								
Exclusive	642,820	0.73	0.44	1	636,725	0.74	0.44	1
Spread (%)	642,820	5.22	3.28	5.85	636,725	4.92	3.11	5.4
Amount (Thousand NIS)	642,820	39.14	69.1	20	636,725	40.52	55.53	24
Maturity (Month)	642,820	43.01	27.79	37	636,725	43.36	28.49	37
Bad_Hist	642,820	0.09	0.28	0	636,725	0.08	0.27	0
Mortg	642,820	0.35	0.48	0	636,725	0.35	0.48	0
Socio	537,162	5.41	2.15	6	534,267	5.4	2.14	6
Age	642,820	6.17	2.81	6	636,725	6.09	2.81	6
Credit_Lim (Thousand NIS)	642,820	16.96	18.44	12	636,725	17.35	17.58	12.8
Borrowers	642,820	1.33	0.47	1	636,725	1.34	0.47	1
Panel B. only exclusive loans								
Spread (%)	468,891	5.49	3.26	6.35	470,792	5.16	3.11	5.75
Amount (Thousand NIS)	468,891	37.35	66.33	20	470,792	38.52	51.17	20
Maturity (Month)	468,891	41.82	26.55	36.5	470792	42.07	27.24	36.5
Bad_Hist	468,891	0.05	0.23	0	470,792	0.05	0.21	0
Mortg	468,891	0.29	0.45	0	470,792	0.3	0.46	0
Socio	393,679	5.36	2.13	6	396,117	5.35	2.13	6
Age	468,891	6.01	2.81	6	470,792	5.94	2.81	6
Credit_Lim (Thousand NIS)	468,891	15.16	16.29	10	470,792	15.59	16.05	10
Borrowers	468,891	1.29	0.45	1	470,792	1.29	0.45	1
Panel C. only non-exclusive loans								
Spread (%)	173,929	4.49	3.22	4.5	165,933	4.25	3.01	4
Amount (Thousand NIS)	173,929	43.96	75.86	25	165,933	46.19	66.02	30
Maturity (Month)	173,929	46.23	30.64	48	165,933	47	31.47	48
Bad_Hist	173,929	0.18	0.38	0	165,933	0.16	0.37	0
Mortg	173,929	0.49	0.5	0	165,933	0.5	0.5	1
Socio	143,483	5.53	2.2	6	138,150	5.55	2.18	6
Age	173,929	6.58	2.78	6	165,933	6.52	2.76	6
Credit_Lim (Thousand NIS)	173,929	21.79	22.56	18	165,933	22.33	20.52	20
Borrowers	173,929	1.46	0.5	1	165,933	1.48	0.5	1

Notes: This table presents the descriptive statistics for the main variables used in Equation 1. See Section 2.3 for details on construction of sample and variables.

Table 2: Baseline regressions

	Spread		
	All	Good Hist.	Bad Hist
	(1)	(2)	(3)
Exclusive	0.399***	0.384***	0.462***
	(0.117)	(0.117)	(0.144)
Exclusive ∗ Post	-0.137*	-0.137*	-0.093*
	(0.079)	(0.082)	(0.054)
Amount	-0.006***	-0.006***	-0.003***
	(0.002)	(0.002)	(0.001)
Maturity	-0.003	-0.002	-0.007
	(0.006)	(0.006)	(0.006)
Bad_Hist	0.860***		
	(0.184)		
Mortg	-0.681***	-0.692***	-0.604***
	(0.051)	(0.054)	(0.072)
Socio	-0.134***	-0.135***	-0.114***
	(0.015)	(0.015)	(0.020)
Age	0.024	0.022	0.048
	(0.028)	(0.027)	(0.039)
Credit_lim	-0.003***	-0.003***	-0.002***
	(0.008)	(0.008)	(0.007)
n_Borr	-0.511***	-0.526***	-0.254
	(0.197)	(0.193)	(0.269)
Bank FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Observations	1,071,429	981,399	89,956
R^2	0.262	0.268	0.156

Notes: The table reports the results of estimating Equation 1. See Section 2.3 for details on construction of sample and variables. The time period is August 2018-February 2020. Standard errors clustered by bank and time are reported in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table 3: Estimation with borrower FE

	Spread		
	All	Good Hist.	Bad Hist
	(1)	(2)	(3)
$Exclusive * Post$	-0.114***	-0.100**	-0.171*
	(0.035)	(0.035)	(0.095)
$Amount$	-0.006***	-0.006***	-0.004***
	(0.002)	(0.002)	(0.001)
$Maturity$	-0.003	-0.003	-0.003
	(0.003)	(0.003)	(0.003)
Bad_Hist	0.162**		
	(0.065)		
$Mortg$	-0.102***	-0.112***	-0.055
	(0.043)	(0.036)	(0.144)
$Credit\_Lim$	-0.002	-0.001	-0.002
	(0.001)	(0.001)	(0.003)
Borrower controls	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes
Observations	697,523	644,057	53,471
R^2	0.840	0.846	0.839

Notes: The table reports the coefficient estimates of Equation 2. The sample is limited to borrowers with at least two loans during our sample period. See Section 2.3 for details on construction of sample and variables. The time period is August 2018-February 2020. Standard errors clustered by bank and time are reported in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table 4: Estimation with restricted sample

	Spread		
	All	Good Hist.	Bad Hist
	(1)	(2)	(3)
$Exclusive * Post$	-0.113***	-0.100**	-0.171
	(0.040)	(0.041)	(0.127)
$Amount$	-0.006***	-0.007***	-0.005***
	(0.002)	(0.002)	(0.001)
$Maturity$	-0.003	-0.003	-0.003
	(0.003)	(0.003)	(0.004)
Bad_Hist	0.182**		
	(0.065)		
$Mortg$	-0.067*	-0.075**	-0.098
	(0.043)	(0.036)	(0.198)
$Credit\_Lim$	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.003)
Bank FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes
Observations	465,281	431,902	33,379
R^2	0.814	0.822	0.827

Notes: The table reports the coefficient estimates of Equation 2 when restricting the sample 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 construction of sample and variables. The time period is August 2018-February 2020. Standard errors clustered by bank and time are reported in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table 5: $Alternative_1$ - Baseline specification

	Spread		
	All	Good Hist.	Bad Hist
	(1)	(2)	(3)
Exclusive	0.434***	0.407***	0.600***
	(0.104)	(0.105)	(0.125)
Exclusive ∗ Post	-0.148**	-0.147**	-0.102*
	(0.069)	(0.073)	(0.053)
Amount	-0.006***	-0.007***	-0.003***
	(0.002)	(0.002)	(0.001)
Maturity	-0.003	-0.002	-0.007
	(0.006)	(0.006)	(0.005)
Bad_Hist	0.882***		
	(0.182)		
Mortg	-0.680***	-0.691***	-0.618***
	(0.050)	(0.054)	(0.068)
Socio	-0.131***	-0.134***	-0.104***
	(0.015)	(0.015)	(0.019)
Age	0.023	0.022	0.043
	(0.028)	(0.027)	(0.043)
Credit_lim	-0.032***	-0.032***	-0.026***
	(0.008)	(0.008)	(0.008)
n_Borr	-0.495***	-0.514***	-0.210
	(0.198)	(0.192)	(0.276)
Bank FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Observations	1,052,065	957,680	94,385
R^2	0.257	0.264	0.153

Notes: The table reports the coefficient estimates from Equation 1 using $Alternative_1$ as our definition for relationship banking . See Section 2.3 for details on construction of sample and variables. The time period is August 2018-February 2020. Standard errors clustered by bank and time are reported in parentheses.

*p<0.1; **p<0.05; ***p<0.01

Table 6: $Alternative_1$ - Estimation with borrower FE

	Spread		
	All	Good Hist.	Bad Hist
	(1)	(2)	(3)
$Exclusive * Post$	-0.125***	-0.108***	-0.209*
	(0.033)	(0.036)	(0.114)
Controls	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes
Observations	710,359	650,780	59,579
R^2	0.837	0.844	0.831

Notes: The table reports the coefficient estimates from Equation 2 using $Alternative_1$ as our definition for relationship banking with borrower FE. See Section 2.3 for details on construction of sample and variables. The time period is August 2018-February 2020. Standard errors clustered by bank and time are reported in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table 7: $Alternative_1$ - Estimation with restricted sample

	Spread		
	All	Good Hist.	Bad Hist
	(1)	(2)	(3)
$Exclusive * Post$	-0.124***	-0.108***	-0.209*
	(0.031)	(0.034)	(0.112)
	(0.002)	(0.001)	(0.003)
Controls	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes
Observations	474,224	436,256	37,968
R^2	0.811	0.820	0.817
Adjusted R^2	0.734	0.743	0.678

Notes: The table reports the estimation results of Equation 2 using $Alternative_1$ as our definition for relationship banking. Our 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 construction of sample and variables. The time period is August 2018-February 2020. Standard errors clustered by bank and time are reported in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table 8: $Alternative_2$ - Baseline specification

	Spread		
	All	Good Hist.	Bad Hist
	(1)	(2)	(3)
Exclusive	0.317***	0.299***	0.484***
	(0.100)	(0.098)	(0.130)
$Exclusive * Post$	-0.215***	-0.219***	-0.114***
	(0.057)	(0.061)	(0.027)
Amount	-0.006***	-0.007***	-0.003***
	(0.002)	(0.002)	(0.001)
Maturity	-0.003	-0.002	-0.007
	(0.006)	(0.006)	(0.005)
Bad_Hist	0.836***		
	(0.184)		
Mortg	-0.689***	-0.701***	-0.625***
	(0.051)	(0.054)	(0.063)
Socio	-0.134***	-0.137***	-0.105***
	(0.015)	(0.015)	(0.021)
Age	0.021	0.020	0.041
	(0.028)	(0.027)	(0.043)
Credit_lim	-0.032***	-0.032***	-0.027***
	(0.008)	(0.008)	(0.007)
n_Borr	-0.504***	-0.520***	-0.242
	(0.198)	(0.193)	(0.277)
Bank FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Observations	1,052,065	957,680	94,385
R^2	0.257	0.264	0.153

Notes: The table reports the coefficient estimates from Equation 1 using $Alternative_2$ as our definition for relationship banking . See Section 2.3 for details on construction of sample and variables. The time period is August 2018-February 2020. Standard errors clustered by bank and time are reported in parentheses.

*p<0.1; **p<0.05; ***p<0.01

Table 9: *$Alternative_2$* - Estimation with borrower FE

	Spread		
	All	Good Hist.	Bad Hist
	(1)	(2)	(3)
Exclusive ∗ Post	-0.135***	-0.122***	-0.139*
	(0.033)	(0.036)	(0.084)
Controls	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes
Observations	678,164	620,301	57,863
R^2	0.837	0.844	0.830

Notes: The table reports the coefficient estimates from Equation 2 using *$Alternative_2$* as our definition for relationship banking with borrower FE. See Section 2.3 for details on construction of sample and variables. The time period is August 2018-February 2020. Standard errors clustered by bank and time are reported in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table 10: *$Alternative_2$* - Estimation with restricted sample

	Spread		
	All	Good Hist.	Bad Hist
	(1)	(2)	(3)
Exclusive ∗ Post	-0.133***	-0.119***	-0.140∗∗
	(0.030)	(0.033)	(0.079)
Controls	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes
Observations	447,427	410,789	36,638
R^2	0.811	0.820	0.815

Notes: The table reports the estimation results of Equation 2 using *$Alternative_2$* as our definition for relationship banking. Our 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 construction of sample and variables. The time period is August 2018-February 2020. Standard errors clustered by bank and time are reported in parentheses.
*p<0.1; **p<0.05; ***p<0.01

Table 11: Robustness: Bank-time fixed effects

	Spread		
	All	Good Hist.	Bad Hist
	(1)	(2)	(3)
Exclusive ∗ Post	-0.093^{***}	-0.088^{***}	-0.130^{*}
	(0.030)	(0.031)	(0.073)
Controls	X	X	X
Bank-Time FE	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes
Observations	462,716	430,337	32,379
R^2	0.816	0.823	0.832

Notes: The table reports the coefficient estimates from Equation 2 including bank-time fixed effects. Our 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 construction of sample and variables. The time period is August 2018-February 2020. Standard errors clustered by bank and time are reported in parentheses.

*p<0.1; **p<0.05; ***p<0.01

Table 12: Robustness: One borrower

	Spread		
	All	Good Hist.	Bad Hist
	(1)	(2)	(3)
Exclusive ∗ Post	-0.157^{***}	-0.127^{**}	-0.191^{**}
	(0.058)	(0.063)	(0.090)
Controls	X	X	X
Bank FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes
Observations	289,682	268,544	21,138
R^2	0.821	0.829	0.846

Notes: The table reports the coefficient estimates from Equation 2. Our 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 construction of sample and variables. The time period is August 2018-February 2020. Standard errors clustered by bank and time are reported in parentheses.

*p<0.1; **p<0.05; ***p<0.01