The causal impact of distance on bank lending*

Christoph Herpfer†, Aksel Mjøs‡ and Cornelius Schmidt§

First Draft: March 12, 2015

This Draft: November 10, 2017

We exploit exogenous shocks to the distance between corporate borrowers and banks to analyze the role of distance in commercial bank lending. We find that a reduction in travel time due to improved infrastructure increases the likelihood of initiating a new borrowing relationship, evidence that closer distance creates a surplus from lower transaction costs. In existing lending relationships, however, banks capture a fraction of this surplus by increasing interest rates. Larger changes in distance are associated with stronger effects and banks with higher market power capture a larger fraction of the surplus.

Keywords: bank credit; lending relationship, spatial price discrimination

JEL Classification: G21, L11, L14

*We thank Sergio Correia, Rüdiger Fahlenbrach, Steven Ongena, Anthony Saunders, Glenn Schepens, Christian Thomann and participants at the University of Zurich Banking and Contract Theory Seminar, the German Finance Association Annual Meeting 2015, NHH Bergen and the European Finance Association Meeting 2016 for helpful comments. Financial support from Finans|Bergen is gratefully acknowledged. Ida Aanes and Karl Fredrik Schjøtt-Pedersen provided excellent research assistance. The views expressed in this article are personal, and do not necessarily represent those of DG Competition or of the European Commission.

†Corresponding Author. Emory University, Goizueta Business School, 1300 Clifton Road, 30322 Atlanta, USA, e-mail:christoph.herpfer@emory.edu.

‡Department of Finance, Norwegian School of Economics (NHH), 5045 Bergen, Norway. e-mail:aksel.mjos@nhh.no.

§Department of Finance, Norwegian School of Economics (NHH), 5045 Bergen, Norway and is with the European Commission (DG Competition - Chief Economist Team). e-mail:cornelius.schmidt@nhh.no.

The causal impact of distance on bank lending

We exploit exogenous shocks to the distance between corporate borrowers and banks to analyze the role of distance in commercial bank lending. We find that a reduction in travel time due to improved infrastructure increases the likelihood of initiating a new borrowing relationship, evidence that closer distance creates a surplus from lower transaction costs. In existing lending relationships, however, banks capture a fraction of this surplus by increasing interest rates. Larger changes in distance are associated with stronger effects and banks with higher market power capture a larger fraction of the surplus.

Keywords: bank credit; lending relationship; spatial price discrimination

JEL Classification: G21, L11, L14

1 Introduction

Physical distance impacts bank lending through two channels since both lenders and borrowers face transportation costs. On the one hand, a bank that is located close to a borrower enjoys significant local market power due to the higher transportation costs arising to the borrower from switching to a more distant lender (Petersen and Rajan, 1995; Degryse and Ongena, 2005). On the other hand, a bank that is actively monitoring a borrower incurs transportation costs form visiting the borrower on site (Agarwal and Hauswald, 2010). These two channels imply differing relationships between distance and interest rates. When transportation costs are borne by the borrower, banks enjoy local market power over close clients and interest rates should fall as distance increases. We will refer to this idea as the local market power hypothesis. Alternatively, if transportation costs are borne by the bank in the monitoring process, and banks price according to marginal costs, interest rates should increase with the distance between bank and borrower. We will refer to this alternative mechanism as the monitoring hypothesis.

The prior literature has found cross-sectional evidence both consistent with the local market power hypothesis (Petersen and Rajan, 2002; Degryse and Ongena, 2005) and the monitoring hypothesis (Knyazeva and Knyazeva, 2012; Bellucci et al., 2013). One potential explanation for these divergent findings in the cross section is that the matching between banks and borrowers is endogenous. Examples of matching include Berger et al. (2005), who find that large banks lend to more distant clients. Beck et al. (2016) document large heterogeneity of bank-borrower distance based on bank and firm characteristics, and Schwert (2017) finds that more bank dependent firms borrow from larger banks. To the degree that these various matching criteria are unobservable, they can bias the cross-sectional association between distance and interest rates. Finally, there is evidence that banks are choosing the location of branches strategically (Kim and Vale, 2001) introducing another selection link between distance and interest rates. If these endogeneity issues impact different samples of firms differently, they can potentially drive the divergent cross-sectional findings in the literature.

We overcome these endogeneity concerns by exploiting exogenous variations in firm bank distance from the construction of infrastructure for a unique sample of 28,000 Norwegian firms and 1300 bank branches between 2005 and 2012. We obtain a time series of driving distances between banks and borrowers by using legacy versions of route planning software that capture the state of road infrastructure at certain points in time. Similar to Giroud (2013), we exploit reductions of effective distance in a difference in differences approach and find strong support in favor of the local market power hypothesis. By analyzing the effect of a change in distance for pre-existing bank-borrower relationships we are able to identify the causal impact of distance on bank lending. Another advantage of our approach is that we posses loan level data for the entirety of the Norwegian economy as opposed to proprietary data from a single bank. We are therefore able to control for bank specific factors that might distort the relationship between distance and interest rates (Berger et al., 2005).

We begin our analysis by introducing a simple stylized model of spatial price discrimination in the spirit of Degryse et al. (2009). The model formalizes the intuition that lower distance to lenders creates a surplus for the borrower through reduced transportation costs when visiting the bank. The lender can however extract parts of this surplus through higher interest rates. The model therefore predicts that a reduction in the distance between bank and borrower should lead to higher interest rates for existing borrowers, and that this effect should be stronger in situations where banks have more bargaining power.

When we turn to the data, we find evidence that lower distance indeed seems to create a surplus for borrowers. The likelihood of a borrower establishing a new relationship with a bank increases significantly after a reduction in distance between the borrower and the closest bank branch of the lender. This result indicates that lower transportation costs make a lending relationship more attractive to borrowers, in line with the model that predicts that, on the margin, borrowers will switch to a different bank after a reduction in distance.

We then study the effect of distance on interest rates and find strong support of the local market power hypothesis. For existing relationships banks can capture part of the surplus from lower transaction costs through higher interest rates. We estimate a difference in differences model and find that a reduction in distance between banks and borrowers leads to a significant increase in interest rates: In our main specification, a reduction in driving time by on average 8 minutes (one way) leads to an increase in interest rates by 13 basis points per year, or about 2% of the average loan rate in our sample. This result is robust to controls for potentially confounding events on the region-time level and taking into account a wide variety of firm and bank characteristics. As predicted by classic monopoly pricing, this increase in the price of credit is associated with a (small) reduction in loan size.

In line with the idea that banks and borrowers bargain over the distribution of the surplus generated through lower transportation costs, interest rates increase more strongly when banks have stronger bargaining power either because they have long running relationships with clients or borrowers are poorly rated. Since transportation costs are increasing in distance, larger reductions in distance should be associated with larger increases in rates. We confirm this conjecture and show that treatment effects do indeed increase in the size of the underlying distance reduction.

One potential concern with using a black box software to calculate driving times is that differences in distance might reflect updates to the algorithm or simply noise that is somehow correlated with changes in interest rates. In our final set of results, we therefore independently confirm the results from the main analysis using the construction of specific, large infrastructure projects such as major bridges and tunnels. We find that a reduction in distance between banks and borrowers due to such projects increases interest rates, confirming the results from the main analysis. Furthermore, the estimated treatment magnitude from these large infrastructure projects is economically similar to that resulting from the largest reductions in driving time in the main sample.

Our paper contributes to the large literature on distance and information generally, and distance in banking more specifically.

The closest paper to ours is Degryse and Ongena (2005), who study the Belgian banking market. They find a strong cross sectional link between lower distances and higher interest rates. They interpret their results as evidence of spatial price discrimination, in particular since interest rates are not just decreasing in the distance between bank and borrower, but increasing in the distance between the borrower and competing banks. We add to their findings in two ways: First, we employ a causal inference design that exploits exogenous changes to the distance in existing bank-borrower relationships. This setup allows us to rule out that (partly) unobservable matching between banks and borrowers (Berger et al., 2005) or the endogenous location choice of bank branches and firms (Kim and Vale, 2001) drive the relationship of distance and interest rates. Second, our dataset covers the universe of banks and borrowers which allows us to establish that the link between distance and interest rates is not driven by specific bank characteristics, and that distance also impacts the matching of banks and borrowers.

Another paper that is close to ours is Nguyen (2017) who uses branch closings as a result of bank mergers as an exogenous shock to availability of banking services. She finds that the volume of local lending significantly falls after a drop in the number of bank branches. Her finding that distance still matters for small business lending is similar to our finding that borrowers are more likely to initiate new lending relationships with banks after a reduction in distance. Our paper differs in that our focus is less on the availability, and more on the pricing of credit and in that we are able to observe specific bank-firm relationships rather than geographic averages. Like Nguyen (2017) we find that banking markets are still local, but document that this fact impacts not just loan quantities, but also loan prices.

A number of papers have found relationships between distance and bank-borrower matching. A variety of firm characteristics, such as higher opacity, is associated with shorter bank-borrower distance (Berger et al., 2005). Similarly, various bank characteristics determine whether banks serve close or distant borrowers (Beck et al., 2016). Ono et al. (2016) exploit mergers and acquisitions as a shock to the distance between banks and borrowers and find that distance is a primary determinant of bank borrower matching. We add to this literature by providing a causal estimate of the role of distance on bank borrower matching that is unaffected by potential confounding factors associated with bank M&A (Berger et al., 1998).

Other authors investigate the effect of geographic proximity to bank branches for households rather than commercial borrowers. Celerier and Matray (2017) find that an increase in the number of local bank branches reduces the fraction of unbanked households, and Brown et al. (2017) show that geographic proximity to banks during early life increases financial inclusion.

The effect of distance on interest rates has been widely explored in the cross section. Whereas some studies find evidence in favor of local market power, i.e. that interest rates decrease in distance (Petersen and Rajan, 2002; Degryse and Ongena, 2005), others find evidence of the monitoring hypothesis and marginal pricing (Knyazeva and Knyazeva, 2012; Bellucci et al., 2013). Agarwal and Hauswald (2010) find that while lower distances increase interest rates, they seem to be associated with larger loan sizes. To the best of our knowledge our paper provides the first causal estimate of the effect of distance on interest rates and can help resolve the diverging results in the cross section.

A number of papers has found that distance and information are related more broadly in financial markets. Butler (2008) finds that local investment banks have an advantage in underwriting municipal bonds, Hollander and Verriest (2016) find that loans to geographically closer borrowers are less covenant heavy and Giannetti and Laeven (2012) documents that banks prioritized lending to domestic borrowers during the financial crisis.

Finally, our article is related to prior work that shows that the physical distance can act in similar ways as direct financing constraints. Giroud (2013) finds that reductions in travel time between firm headquarters and local plants lead to increased investment and productivity. Giroud and Müller (2015) find that financially constrained firms internally re-allocate resources to plants which receive a shock to proximity to firm headquarters, and Bernstein et al. (2015) find that reductions in distance help venture capital firms monitor portfolio firms. While our findings also document the importance of distance in the context of interactions between business parties, they highlight the role of the distribution of the surplus between parties in the context of inter-firm relationships. When both parties of the deal are part of the same organization, or one party holds significant equity in the other, the surplus from closer proximity is internalized. But when the relationship is *inter*-firm rather than *intra*-firm, the relative bargaining power of parties is crucial in sharing the surplus.

The remainder of the paper is organized as follows. Section 2 describes a simple model, while Section 3 contains a data description. Section 4 outlines our identification strategy. Section 5 presents our results and Section 6 contains additional results. Section 7 concludes.

2 Simple Model

We motivate our analysis using a stylized, one period model in the spirit of Degryse et al. (2009). There are two lending banks A and B located on a straight line. Assume a single borrowing firm is located between A and B, x units away from A and y units away from B, and can choose from which one bank to borrow a single Dollar. Borrowing from A, the borrower faces a total cost of $r_{Ax} + t_F x^{\alpha}$. Parameter r_{Ax} denotes the interest rate for a fixed loan amount of 1. The interaction between borrower and lender causes the borrower to incur transportation costs from visiting the lender. The linear part of these costs is captured by t_F, whereas x^{α} allows for a nonlinear cost per unit of distance x. Similarly, when borrowing from B the borrower faces cost of $r_{By} + t_F y^{\alpha}$.

As Degryse et al. (2009) we normalize the cost of funds for both banks to zero and assume that the borrower location as well as the transportation cost are known prior to the loan pricing decisions.

Lender A incurs a distance related cost for monitoring the loan, which we denote by $t_L x^{\alpha}$. It requires that its profits, $\pi_A = r_{Ax} - t_L x^{\alpha}$ are non-negative. The equivalent condition applies to lender B. The borrower is indifferent between borrowing from A and B if $r_{By} + t_F y^{\alpha} = r_{Ax} + t_F x^{\alpha}$.

The most competitive rate lender B can offer is $r_{By} = t_L y^\alpha$, i.e. a rate equal to its monitoring cost such that its profit is equal to zero. Lender A therefore maximizes its profits setting $r_{Ax} = t_F(y^\alpha - x^\alpha) + t_L y^\alpha$. The first term captures the borrower's lower transportation costs from being located closer to A than B. The second term corresponds to the fact that, if competing, lender B needs to cover its own monitoring cost. Assuming that this interest rate also covers its own monitoring cost, $t_L x^\alpha$, and that A is able to perfectly price discriminate, the profit maximizing interest rate increases in the relative proximity of lender A to the borrower, and in lender B's cost of monitoring given the borrower's location.

To relax the assumption of perfect price discrimination, we introduce a sharing rule s, which allows for a split of the cost reduction to the borrower from using the closest lender. If $s < 1$ the lender can only capture part of the borrower‘s transportation cost advantage. Formally,

$$r_{Ax} = s \cdot t_F(y^\alpha - x^\alpha) + t_L y^\alpha.$$

In case the borrower has all the bargaining power, i.e. $s = 0$, the interest rate from lender A, corresponds to the monitoring cost of lender B, assuming that its own monitoring cost are covered. On the other hand, if $s = 1$, lender A is able to capture all the benefits from borrower's transportation cost decrease.

The effect of distance on interest rates is therefore

$$\frac{\partial r_{Ax}}{\partial x} = -\alpha s t_F x^{\alpha-1}$$

and leads to

Hypothesis 1: *The interest rate increases when the distance between bank and borrower shrinks, provided that the bank has at least some bargaining power, $s > 0$.*

A drop in distance of size $-dx$ between lender A and borrower creates a surplus for the borrower from lower transportation costs equal to $t_F dx$. This borrower-surplus is then divided between the bank and the borrower. How this surplus is split depends on the bargaining power. Therefore,

Hypothesis 2: *If the bank has higher bargaining power s, it captures more of the surplus in the form of increased interest rates.*

Lender A's profits change in distance due to two channels,

$$\frac{\partial \pi_{Ax}}{\partial x} = -\alpha s t_F x^{\alpha-1} - \alpha t_L x^{\alpha-1}.$$

A drop in distance increases the profits through capturing a share of the borrower's reduced transportation costs as well as its own lower monitoring cost.

3 Data

3.1 Data Sources

Yearly corporate loan information comes from the Norwegian Tax Administration.[1] The dataset contains detailed information on the end-of-year balance separately of all bank loan accounts, and interest accrued to these accounts during the year. The sample contains account information of all Norwegian firms if they hold an account at a domestically operating bank, including Norwegian branches of foreign banks. The sample period spans from 2005 to 2012. [2] The dataset includes for each individual account the loan balance

[1] The database is confidential, but has been made available to us by the Norwegian Tax Directorate (Skattedirektoratet). Approval is gratefully received by letter dated 23 June 2014.

[2] Data quality is high. Since it is collected for tax purposes, the bank's external auditor is required to verify each annual reporting of these data to the tax administration. In addition, Norwegian banks are subject to standard regulations and supervision. We are not aware of any incentives for the banks not to report truthfully.

and interest accrued during the year. The interest accrued on loans includes, in addition to regular interest, any fees or commissions related to the loan.

We link the bank account information to a database containing firm information using a government provided, unique firm identification number.[3] The accounting database includes the complete profit and loss accounts, the balance sheet, selected items from the notes to the accounts, and other company related information such as, e.g., firm address, industry codes, and legal form.

We obtain information on bank branch locations from Finans Norge's (Finance Norway), the financial sector's industry organization, national bank branch register. This register includes branch addresses of all bank branch offices in Norway and is updated annually. We link branches to each bank as identified in the bank account and financial account databases.[4]

3.2 Definition of Variables

3.2.1 Measuring Driving Time Reductions between Banks and Firms

To detect an exogenous reduction in travel time between firms and banks we first calculate the historic driving times. Since currently available route planning software does not feature historic maps, we employ several outdated vintages of Microsoft MapPoint. Norwegian maps are included in MapPoint starting with the MapPoint 2006 version. We

[3]All Norwegian firms are required to have an authorized auditor during most of our sample. This requirement has been lifted for the smallest firms from 2011. We have no indication that this has materially reduced the quality of the accounting data in our sample and our results hold excluding 2011 and 2012. They must file their annual financial accounts with the Register of Company Accounts by the end of July in the year following the accounting year. The accounting year is required to follow the calender year. More information is provided at www.brreg.no. The accounting database is further described in more detail in Berner et al. (2015).

[4]Address information in the register is not always complete. In some instances bank addresses consist only of a postal box or sometimes only a ZIP code and a city name. For those bank branches which have usable addresses we manually link bank names in the register with the banks in our tax data.

have access to five vintages of MS MapPoint: 2006, 2009, 2010, 2011 and 2013. MapPoint version numbers do not perfectly correspond to the year of release. We use release dates of each version and assume that the map vintage used in each version corresponds to the year before the version release.[5] Table 1 details the release date and the corresponding map vintage assigned by us for each version. Appendix A describes in detail which steps we follow to calculate the driving distances.

[TABLE 1 here]

We arrive at a dataset of driving times between firms and their 20 closest bank branches in a four hour driving time radius. To assign treatment, we calculate the fastest driving time between firms and bank branches separately for each version of MS MapPoint from Table 1. We classify a firm bank pair as treated in year t if neither the firm nor bank address changed between t and $t-1$, if there did exist at least one loan account of the firm with the bank in both years, and if the driving time between the two addresses shrunk by at least 10% and more than two minutes by t. The rationale behind these cutoffs is that for firms in rural Norway, even a reduction in driving time by multiple minutes might not drastically change their total driving time. On the other hand, a firm in Oslo might experience a very large reduction in relative terms without a significant reduction in absolute travel time. We provide a detailed analysis of the effect of varying treatment sizes on treatment effects in Section 5.4.

Table 2 displays summary statistics for firm-bank pairs which experience a reduction in driving time. We note that the average reduction in driving time conditional on treatment is eight minutes or 28.5%, significantly above the cutoff.

[TABLE 2 here]

[5] The release schedule can be found at http://support2.microsoft.com/lifecycle/search/default.aspx?sort=PN&alpha=M retrieved November 2014

Figure 1 shows the dispersion for the driving time changes. We see that most of the changes are small, but there exists significant variation. We discuss the choice of the treatment size below but also include a robustness check.

[FIGURE 1 here]

3.2.2 Measuring Interest Rates

We observe for each firm and bank all separate bank accounts active at any point of time during the year of reporting but not the precise loan contract terms. As described in Section 3.1, our dataset includes the amount owed or deposited at the end of year (as separate fields) as well as interest paid or received over the course of the year on both deposits and loans. Neither the amounts deposited/owed nor the interest paid/received are netted off against each other but are reported separately. To proxy for the interest rate we take the ratio of total interest paid during the year over the balance outstanding at the end of the year. While this proxy allows us some inference regarding the interest rate, it remains a crude measure of the actual underlying interest rate. We therefore drop all interest rates calculated in this manner at the 10th and 90th percentile. This reduces the noise of our interest rate measure and yields a reasonable range of rates between 3.33% and 17.05%. We describe all other variables in Table 3.

[TABLE 3 here]

3.2.3 Summary Statistics

We find an average driving time of 19 minutes between firms and their relationship banks. This translates to roughly 6.3 miles and is consistent with prior work. For the United States the average distance between banks and borrowers is 42.5 miles (Petersen and

Rajan, 2002). For Belgium, Degryse and Ongena (2005) find that their average bank is located 1.4 miles from their lender, which translates to a 4.5 minutes drive. Norway features vast, sparely populated areas which explains why the number is larger than that for Belgium. Yet business is concentrated largely in major metropolitan areas around Oslo, Bergen, Stavanger and Trondheim which means on average distances are lower than in the less centralized United States. We also note that the average distance to the five closest competing non-relationship banks is significantly larger in both median and mean. This means relationship banks are mostly among the closest banks of a firm and underlines the importance of distance in borrower-lender matching. As Petersen and Rajan (2002), we document a wide variation in distance with far lower median distances of ten minutes than the mean distances of 19 minutes.

[TABLE 4 here]

4 Identification Strategy

To identify the causal relationship between distance and lending, we exploit changes of travel time which are independent of changes of either firm or bank addresses. Relying on a difference in differences approach, we estimate for the variable of interest y_{tfl} at the firm-year level,

$$y_{tfl} = \alpha + \beta_1 \; Treatment_{ft} + \beta_2 \; Treatment_{ft} \; X \; Post_{ft} + \Gamma X_{tfl} + \epsilon_{tfl}. \qquad (1)$$

The variable α is a vector of regional, industry, year and rating fixed effects, $Treatment_{ft}$ is an indicator variable equal to one for firms which experience a reduction in driving time to their bank at any point during our sample period and

$Treatment_{ft}$ X $Post_{ft}$ is an indicator equal to one for treatment firms in all years after they experience a reduction in driving time. The vector X_{tfl} contains controls for various firm characteristics such as firm size (sales), duration of the banking relationship, distance to banks with and without lending relationship, firm age and the ratio of intangibles to assets. Our main coefficient of interest is β_2, the estimated treatment effect on treated firms.

Note that this is only a quasi difference in differences approach, as we cannot individually estimate the effect of *Post*. The reason for this limitation is that multiple firms in the same region can be treated sequentially. If one firm is treated in 2008 and another in 2010, control firms are simultaneously part of the pre- and post-treatment sample for the two different firms, respectively. We directly address this challenge by matching control firms to specific treatment firms in the year of treatment in Section 6.1.

We employ this quasi difference in differences approach since we expect that firms and banks strategically choose their location.[6] In addition, firms and banks match with each other and their location can be one determinant of the matching, but not the only one. Schwert (2017), for example, documents that bank dependent firms borrow from more well capitalized banks. If non dependent firms are indifferent with respect to the capitalization of their bank, this will create a mechanical association of bank dependence with higher (cross sectional) distances between borrowers and banks. To the extend that bank dependence is (partially) unobservable, this association will bias the effect of distance on interest rates upward. Another potentially confounding matching might be that

[6] Kim and Vale (2001) for example provide evidence for strategic branch opening decisions for Norwegian banks around the time of the Norwegian financial crisis. Analyzing data from 1988 to 1995, they document that the total number of bank branches exhibited significant variation. In 1988, they document 2166 individual bank branches in the country. This number subsequently falls to 1604 branches in 1993, a reduction of more than 500 branches, or 26%. This number then increased to 1740 branches or 8.5% until 1995. Brickley et al. (2003) find that small banks with local decision power are at an advantage to serve small borrowers.

low quality firms or CEOs with little effort might pick closely located banks. We propose exploiting reductions in travel time between firms and banks caused by improvements in infrastructure such as new roads, tunnels or bridges to overcome those challenges. This strategy resembles that employed in Giroud (2013) who uses the introduction of novel airline routes as variations in travel time between firm headquarters and plants.

We address potential issues with this identification strategy as follows: First, the construction of new infrastructure might be either the cause or the result of the development of the local economy. This problem is similar to the one faced by Giroud (2013): As in his analysis, there is a potential simultaneity between the construction of new infrastructure and local economic development. There are two channels through which infrastructure and loan terms can interact: On the one hand, a booming regional economy can attract infrastructure spending to keep up with the increased economic activity. In this case the booming economy causes both a change in loan conditions as well as the change in travel time. Second, infrastructure investment (e.g. by the government) can lead to both economic development and travel time reductions.

Our identification strategy relies on identifying the impact of changes in travel time on loan terms. Since local economic development can impact both of these variables it is crucial to control for it in our empirical analysis. We therefore include time-region fixed effects in our regressions to capture this local development. We use the 120 Norwegian commuter regions which are geographic groups of postcodes built on economic areas.[7]

Second, not all distance changes can be treated as exogenous. For example it is likely that a firm that is moving its place of business is different from one which stays. Firms which move because they are growing will have different financing needs than stagnant

[7] We employ Sergio Correia's STATA *reghdfe* routine for the calculation of high dimensionality fixed effects.

firms. Firms might move both if their old location receives more or less infrastructure investment: Retail firms will tend to leave an area which is experiencing a negative development with increased crime and lower wages. Manufacturing firms might leave an area that is experiencing the opposite effect due to higher rents.

A similar logic applies to banks: A change in branch address might be associated with changes inside the bank such as expansion, downsizing or different business strategy. A final reason for changes in distance through relocation of branches are bank mergers. The resulting relocation of branches is endogenous for two reasons: First, the merged entity will be more likely to close poorly performing branches and those with an overlapping branch network. Second, bank mergers are not exogenous, e.g. poorly performing banks might both offer worse loan conditions and be more likely takeover targets. We therefore exclude all distance changes which are caused by a change in either the firm or the bank address and only analyze changes of distance for existing firm bank pairs.

5 Results

5.1 Distance and Relationship Initiation

The summary statistics show that the distance between firms and their relationship banks is lower than the average distance to the five closest non relationship banks. The difference is about ten minutes in both the mean and median driving time. A similar cross-sectional link between distance and banking relationships is documented in Beck et al. (2016) and suggests that distance is an important consideration in firm-bank matching.

In Table 5, we proceed to test the causal impact of distance on the matching of firms and borrowers. We explore the impact of an exogenous reduction in travel time on the initiation of banking relationships. To the best of our knowledge, this is the first analysis of the causal link between changes in distance and the initiation of borrowing relationships.

We start by determining for all years the set of unique bank branches which can be reached from a firm's address in less than four hours drive.[8] We then determine for each of those banks whether the firm has a loan with this bank in that particular year.

Using this panel, we construct $New\ Relationship_{tfl}$, an indicator that takes the value of one if firm f has taken out a loan from lender l in year t and has not had a loan in the year before, $t-1$. To ease exposition we multiply the resulting variable by 100. We then run linear probability model regressions with $New\ Relationship_{fbt}$ as the dependent variable. The main explanatory variable is $Treatment\ x\ Post$, an indicator which takes the value one if the driving distance between firm f and bank b fell by at least 10% and two minutes.[9]

We then proceed to estimate

$$New\ Relationship_{tfl} = \alpha + \beta_1\ Treatment_{ft} + \beta_2\ Treatment_{ft}\ X\ Post_{ft} + \Gamma X_{tfl} + \epsilon_{tfl} \quad (2)$$

and present the results in Table 5.

[TABLE 5 about here]

Column 1 presents the results of a regression with just controls for industry fixed effects, year fixed effects and municipality year fixed effects to control for industry, time and regional specific characteristics which might impact firm-bank matching. The estimated coefficient is 1.24, and highly statistically significant. The linear probability model

[8] For details on the procedure see Appendix A.

[9] We require a minimum reduction of at least two minutes since lower reductions are unlikely to have a significant impact. The average treatment size is significantly larger than two minutes, at eight minutes.

allows for an intuitive interpretation of this coefficient as the effect of treatment being an increase in the likelihood of initiating a banking relationship by 1.24%.

Column 2 adds controls for time varying firm characteristics such as firm size (sales), firm opacity (intangibles/assets), firm leverage and employs a combined municipality/year fixed effect. The estimated impact of distance changes on relationship initiation remains unchanged in both economic and statistical significance. The point estimates on sales and leverage are positive and significant, which suggests that larger firms and those with more debt are more likely to initiate new lending relationships. The negative and significant coefficient on intangibles/assets reflects the fact that more opaque firms have a harder time initiating new lending relationships, possibly due to higher asymmetric information.

In the third column, we add banking specific controls. First, we add bank fixed effects to capture the varying propensity of banks to acquire new clients. In addition, we control for the distance both between the existing banks and the borrower, and the distance between the borrower and competing banks. Both estimates are positive and significant. Column 4 shows the regression results when adding both bank specific factors as well as firm characteristics. The magnitude of the reduction of treatment decreases but retains statistical and economic significance.

Finally, Column 5 adds the interaction of the number of available bank branches with treatment. Firms with a larger set of potential lenders will likely react less to a reduction in distance to any specific lender. Indeed, the coefficient on the interaction of treatment and the number of available banks is -0.02 and statistically significantly different form 0 at the 1% level.

Taken together, Table 5 shows that a reduction in driving time increases the likelihood of initiating a borrowing relationship.

5.2 Loan Contract Terms

We turn to the impact of a reduction of travel time on existing lending relationships. Hypothesis 1 predicts that a reduction in travel time between firm and bank will increase the interest rate, due to the lender's higher local market power. To test Hypothesis 1, we limit our sample to firm bank pairs with a borrowing relationship of at least one year since new lending relationships are potentially different from old ones, for example because of teaser rates used to attract new customers. We therefore estimate the effect of a reduction in travel time on interest rates:

$$Loan\ Interest\ Rate_{tl} = \alpha + \beta_1\ Treatment_{ft} + \beta_2\ Treatment_{ft}\ X\ Post_{ft} + \Gamma X_{tfl} + \epsilon_{tl}. \quad (3)$$

Table 6 reports the results of estimating 3.

[TABLE 6 about here]

We estimate regressions of interest rates on the treatment indicator with just industry and combined municipality/year fixed effects in Column 1. We estimate that, after the treated firms get treated, the interest rate rises by 14 basis points. We also find that, before treatment, the treatment group exhibits on average 13 basis points lower interest rates than the control group, a difference that is statistically significant at the 1% level. We then proceed to add bank fixed effects in Column 2 as well as relationship specific controls in Column 3. The estimated impact of treatment is an increase in rates by 0.14 basis points, significant at the 1% level. The cross sectional coefficient on distance to the relationship bank is -0.08 and marginally significant at the 10% level. This negative cross sectional relationship of distance and interest rates mirrors our causal estimate that a reduction in distance is associated with an increase in interest rates. Finally, the estimated effect of the distance of *competing* banks is -0.1 and statistically significant with 90% confidence. Degryse and Ongena (2005) find a positive and significant association between interest rates and distance of competing banks which they interpret as a competition effect. Bellucci et al. (2013) on the other hand document a *negative* and significant relationship between distance to competing banks and interest rates. Our estimated cross sectional coefficient is negative and marginally significant, a result lying somewhere between the two papers.

Column 4 adds further firm specific controls in lieu of time-varying relationship specific controls. The estimated impact of treatment on interest rates remains at 14 basis points while the estimated pre-treatment difference between treatment and control firms shrinks to 13 basis points. The negative and significant point estimates on firm age and firm size suggest that older and larger firms pay lower interest rates. Finally, Column 5 presents results for the richest specification including municipality-year fixed effects, industry, rating and bank fixed effects as well as both firm- and bank specific controls. The estimated treatment effect is 13 basis points, and statistically significant at the 1% level.

One potential concern is that changes to infrastructure might simultaneously affect the distance not just to lending banks, but also to competing banks. In unreported results we explicitly test this hypothesis. We define a second treatment indicator that takes the value of one if one of the 5 closest *competing* banks moved closer to a firm. We then repeat the analysis in Table 6 with this additional control variable. Throughout all specifications, we find that the treatment effect of a relationship bank moving closer is positive and significant as before. The effect of a competing bank moving closer on interest rates is negative, but economically smaller and statistically insignificant compared to the effect of a relationship bank moving closer.

We proceed to evaluate whether the economic magnitude of this estimate is plausible. At a median loan size of NOK 1.064 million, the coefficient of 13 basis points translates into annual expenses of NOK 1383, or roughly $165 per annum. We can compare these expenses to the reduction in travel time for the owner in a back of the envelope calculation. Mean travel time reduction for treated firms is about eight minutes (one way). Assuming quarterly visits to the bank, annual time savings are 64 minutes which implies hourly opportunity cost of about $155 for the business owner or manager. While we want to be careful not to put too much emphasis on this analysis, the back-of-the-envelope calculation yields a reasonable estimate of the hourly opportunity costs of managers, in particular if one takes into account that the owner does not only lose his own labor but also fails to supervise her employees during that time.

Our causal estimate is furthermore remarkably close to the cross-sectional findings in Degryse and Ongena (2005): They find that a cross sectional difference in driving time of seven minutes less is associated with an increase in the loan rate by 18 basis points. In our analysis the mean treatment size is eight minutes and the resulting increase in interest rates is 13 basis points.

While our simple model has no prediction on loan size, standard monopoly pricing (Hoover, 1937) predicts that increases in prices (interest rates) are accompanied by reductions in quantities (loan size).[10] In Table 7 we therefore proceed to test whether a reduction in distance also affects loan size.

[10]This effect holds under the assumption of a single pricing schedule, i.e. there is a single interest rate for the loan. In reality, banks can extract rents through higher fees, effectively charging a two part tariff that does not distort quantities. Our data do not allow us to differentiate between fees and actual rates and we therefore want to be careful not to put too much emphasis on this result.

[TABLE 7 about here]

Table 7 repeats the analysis from Table 6, with the difference being that the dependent variable is loan size rather than interest rate. The results confirm that treatment is indeed associated with reduced loan size: The estimated coefficient on *Treatment X Post* ranges between -0.13 and -0.16 percent of assets among the various specifications, and is statistically significant at the 1% level.[11]

5.3 The Effect of Bank Market Power

Hypothesis 2 predicts that the effect of proximity on interest rates depends on the bargaining power of both bank and firm. We test this prediction by interacting the treatment effect with three different proxies for the relative bargaining power of banks and borrowers.

We postulate that banks with larger power over their borrowers before the decrease in distance will have a stronger bargaining position and will extract a larger fraction of the rents. It is well established in the literature that longer relationship duration gives relationship banks an informational advantage over competitors and results in them having more power over borrowers (see for example Ioannidou and Ongena, 2010). The treatment effect should therefore be stronger for firms with long banking relationships, as their banks are able to raise rates more. Similarly, firms with better credit ratings pose less of a risk and might have an easier time switching to other lenders. Accordingly, the treatment effect should be larger for firms with a poor credit rating.

Table 8 shows the results.

[TABLE 8 about here]

[11] While this effect is statistically significant, it is economically weak: At the median loan size of about 1.064 million the corresponding nominal reduction in loan size is NOK 1700, or about $140.

Column 1 presents the results from a specification including an indicator whether a firm is in the treatment group (*Treatment*), the interaction between treatment and an indicator for the time period after treatment (*Treatment x Post*) and finally a triple interaction between the treatment group indicator, the post indicator and the duration of banking relationship (*Treatment x Post x Duration relationship*). Hypothesis 2 predicts that banks with more negotiation power over their borrowers will be able to capture a larger share of the surplus generated by the reduction in travel time. The previous literature suggests that relationship banks gain an informational monopoly regarding their borrowers over time which gives them stronger negotiation power (Von Thadden, 2004; Ioannidou and Ongena, 2010). Thus banks with long lending relationships should increase interest rates more after a reduction in distance. Indeed Column 1 of Table 8 shows that the interaction between treatment, post-project and the length of the relationship is positive and significant at the 10% level. That result suggests that when banks have superior inside knowledge about borrowers and therefore larger bargaining power, they are able to extract larger rents from a reduction in traveling time.

Column 2 confirms this result using a different measure of negotiation power: The borrower's credit rating. We use Bisnode's internal credit ratings on a scale from 1 to 5, with 1 corresponding to firms rated "C" and 5 corresponding to the best possible rating of "AAA". If borrowers with good credit rating have an easier time switching lenders, we expect their banks to increase interest rates less as a result from a reduction in distance. The negative point estimate of the interaction of treatment group, post project and rating quality suggests that the increase in interest rates after treatment is indeed significantly smaller for firms with a good credit rating. This results again confirms the prediction by Hypothesis 2 that when banks have larger negotiation strength, they will capture larger parts of the rents generated by a reduction in driving time.

If banks are willing to invest in relationships with borrowers in their core industry, they might be less willing to extract rents from borrowers after a reduction in distance. We therefore investigate whether the treatment effect is smaller for firms operating in the industry in which their bank is specialized. Column 3 shows that firms which operate in industries in which the bank developed expertise generally pay lower interest rates. If the distance between firms and banks is reduced, the bank captures a smaller fraction if the firm operates in the bank's core industry.

The results in Columns 2 and 3 are particularly interesting as they do not just corroborate the market power hypothesis, but also provide direct evidence against the monitoring hypothesis: If firms with bad credit rating require more frequent monitoring than those with a good credit rating, a reduction in travel time should lower monitoring costs more for those firms and therefore lead to lower interest rates (or alternatively put, a smaller increase in rates) compared to firms with a good credit rating. Similarly, a bank that is specialized in the particular industry of a client might monitor this client less frequently. As a result, the savings from lower driving time would be lower for those clients and interest rates should go up more for firms operating in an industry in which the bank is specialized. In both cases, we find the opposite of this conjecture, which we interpret as additional evidence in favor of the market power hypothesis driving the relationship between distance and interest rates.

5.4 Interest Rates and Treatment Magnitude

In this section we study the role of treatment size on the resulting change in interest rates. Since the calculation of driving time is noisy, and since very small reductions in driving time are unlikely to result in significant changes in interest rates, we decide to ignore reductions in driving time below a certain cutoff. In our main specification we assign firms to the treatment group if they experience a reduction in driving time of at least 10%. In addition to this definition based on a *relative* change in driving time, we also ignore cases in which the *absolute* reduction is smaller than two minutes. The general trade-off in increasing the treatment definition is that higher treatment sizes exclude large fractions of the sample from being potentially treated.

To put our treatment definition into perspective, we survey the literature on distance and interest rates in Table 9. We identify five papers which explicitly estimate the impact of distance (either in terms of space or in terms of driving time) on interest rates (Column 1). Column 2 shows the country from which the sample was taken. There are three papers using US data, one paper using Italian data and one paper using data from Belgium. Column 3 lists the sample mean for firm bank pairs in each paper as well as the measuring unit employed. Column 4 presents for each paper the estimated cross sectional effect of changes in the respective distance measure on interest rates. Finally, we standardize each paper's estimate by calculating the (implied) change in interest rates associated with an eight minute reduction in traveling time which corresponds to our average reduction in traveling time conditional on treatment.

[TABLE 9 here]

Table 9 illustrates that even relatively small changes in driving time are associated with economically significant changes in interest rates. The implied impact of a eight minute change in driving time ranges from an increase in rates by 96 basis points to a drop by 31 basis points, with our estimate of a causal effect of 13 basis points being roughly in the middle of the previous findings.

Investigating the role of treatment size on interest rates is not just a robustness exercise. Our stylized model predicts that the treatment effect from lower distance is increasing in treatment size. Larger reductions in distance should therefore be associated with larger increases in interest rates. Table 10 presents results from running the most complete specification in Column 5 of Table 6 with various changes in the treatment definition:

[TABLE 10 about here]

The point estimate for the interaction of treatment group and post-project in Column 5 of Table 6 is 13 basis points. When the bar for treatment is increased to five minutes, the resulting point estimate shrinks to just 10 basis points in Column 2. Once treatment magnitude is increased to ten minutes (Column 3), the point estimate increases to 19 basis points, significant at the 5% level. The coefficient's magnitude is larger than that for all lower treatment thresholds. Once the treatment size is increased even further to fifteen and twenty minutes in Columns 4 and 5 respectively, the estimated coefficient on treatment and post project increases each time in both economic and statistical significance. The point estimate for 15 minutes is 38 basis points, while for 20 minutes it is 40 basis points. Both estimates are statistically significantly different from zero at the 1% and the 5% level, respectively.

Table 10 demonstrates that our result is robust to variations in the definition of treatment. Furthermore, all point estimates with the exception of that for the five minutes threshold are increasing monotonically: Smaller reductions in driving time are therefore associated with smaller changes in interest rates, and larger reductions are associated with larger increases in interest rates.

6 Additional Tests

6.1 Matching

The structure of our dataset, in which many firms are being treated in different regions at different times limits our ability to implement a standard difference in differences setup. Since treated firms are treated at different times, it is not possible to assign a "post project" indicator to control group firms.

In this section, we overcome this methodological challenge via a matching procedure. We construct a panel of treatment and control firms with matching taking place in the year before treatment using the Abadie and Imbens (2002) matching procedure. We require perfect matching on the industry classification as well as the same geographical region as defined by first postcode digits.[12] In addition to those exact matches, we also match on all continuous variables used in the most complete regression specification in Column 5 of Table 6.

In a next step, we plot the development of interest rates for treatment and control groups to visually confirm that they exhibit parallel trends. Figure 2 displays the mean interest rate paid by treatment and control firms relative to the treatment year. Since different projects were completed in different years, we normalize the time dimension of treatment. The x-axis plots "timeline", the time relative to the completion of the project which we assign a value of zero. The y-axis represents the interest rate paid by firms.[13]

[Figure 2 about here]

[12]The postcode method yields ten geographically large regions. Exact matching on the 120 commuter region results in many firms without a corresponding control firm in the same industry and year in the more rural regions.

[13]Since treatment can happen at various times during our seven year sample period, firms with earlier treatment will be disproportionately represented in the later "post treatment years", whereas firms with treatment in the later years will form a larger part of the pre-treatment sample.

Figure 2 reveals that treatment group firms exhibit lower interest rates than control group firms in all years preceding treatment. While there is a difference in the *level* of interest rates pre-treatment, the *trends* appear very much parallel: Interest rates for both groups rise and fall without a clear trend. Interestingly both treatment and control groups move in the same direction in all years before treatment, either both rising or falling in lockstep. This co-movement is broken for the first time in the treatment year (year 0). Whereas the interest rate paid by the control group drops slightly by 5 basis points, that of the treatment group increases by about 15 basis points. The resulting difference of 20 basis points is close to the estimated treatment effect from the main sample of about 13 basis points. Interest rates for the treatment group remain higher than those for the control group in all five years following the treatment year, reversing the pattern from the pre treatment period where their interest rate was relatively lower. Both groups experience a decline in overall interest rates in the post treatment years which reflects the generally decreasing rates in Norway in the years after the financial crisis. The spread widens in later years. It is important to note that the increase in the spread in later years is not what drives our results because it reflects a much smaller group of observations: Observations five years post treatment can only consist of those firms treated at the very start of our sample in 2007. While there are between 1,800 and 2,900 observations for this treatment year and one year pre- and post treatment, there are merely 300 observations in year $t + 4$ and $t + 5$.

The econometric analysis presented in Table 11 confirms the visual impression:

[TABLE 11 about here]

Column 1 presents results from a raw difference in differences regression without any control variables. The coefficient on the indicator variable *Treatment* is -16 basis points and significant at the 5% level. The estimated coefficient of -30 basis points for the post treatment period reflects the decline in interest rates during our sample. Finally, the estimated treatment effect from the interaction of *Treatment x Post* is 32 basis points and statistically significant at the 1% level. Although we constructed our panel by matching on various firm characteristics, we add a variety of control variables in Columns 2 to 5. Column 2 adds industry and municipality year fixed effects. The estimated treatment effect shrinks to 19 basis points and is statistically significant at the 5% level. When we add bank related controls such as the distance to the relationship and to competing banks in Column 3, the estimated treatment effect remains 19 basis points and retains its statistical significance. It does so also when we instead control for firm level characteristics such as size and age in Column 4. Finally Column 5 combines all bank- and firm related control variables and reports results from running the same regression specification as in Column 5 of Table 6 on our matched sample. The estimated treatment effect is 17 basis points, almost the same as in the large sample results and it is statistically significant at the 5% level.

The results from the matching analysis therefore confirm our findings from the Difference in Differences specifications in the main analysis. The results in the matched panel mirror those from the large sample analysis and confirm our conclusion that a reduction in driving time between banks and their borrowers is associated with an increase in interest rates, as predicted by the market power hypothesis.

6.2 Large infrastructure Projects

There are four potential concerns with the approach in the previous analyses: First, the use of black box software for distance calculations can induce noise in our treatment measure. Microsoft purchased maps for MapPoint from third parties. There is no information on whether new versions of MS MapPoint contains updated map material, or whether updates cover only parts of the country. The lack of information could potentially lead to significant mis measurement of distance changes if maps were only selectively updated. The second source of noise can stem from changes to the algorithm which calculates driving time: If Microsoft updates the algorithm between two versions of MapPoint, our approach of taking the difference between driving times across software versions can lead to a false positive conclusion that driving time has changed when in fact it was simply the algorithm changing. While including region-year fixed effects alleviates the concern that the results are driven by changes in the algorithm, we cannot rule fully rule out that our analysis picks up this noise. The third concern is that MapPoint versions are unevenly spaced in time. There is no annual update to the software which means for years without updates we have no way of assigning new treatment. The final concern is that many changes calculated from MapPoint are relatively small. While we demonstrate that our estimation is robust to using different treatment intensities, some doubts might remain with respect to the significance of the treatment.

We therefore independently verify our results using a different approach: We focus on large infrastructure projects such as bridges and tunnels. We obtain a list of all large infrastructure projects completed in Norway during our sample period from the Norwegian Public Roads Administration ("Statens Vegvesen"). Data include each project's name as well as its year of completion. We then hand match each project to postal codes in the vicinity of its start- and endpoint. We determine for each firm whether their relationship bank in the years prior to the project's completion is located on the same side as their firm or the other side. Firms which are clients of banks on the other side of the project experience a reduction in travel time after the initial opening of the project. Those firms form the treatment group. Conversely, firms which are located on the same side as their banks form the control group.

Figure 3 visualizes the locations of the various projects used in our analysis. As can be seen from the map, projects are dispersed over the entire country. The majority of projects is related to bridges and tunnels in the more densely populated south of the country.

[FIGURE 3 about here]

In a next step, we plot the development of interest rates for treatment and control groups to visually confirm that they exhibit parallel trends, similar to the analysis in the matching analysis for the MapPoint sample. Figure 4 displays their development over time. Since different projects were completed in different years, we normalize the time dimension of treatment. The x-axis plots “timeline”, the time relative to the completion of the project which we mark as year 0 on the timeline. On the y-axis we plot the interest rate paid by firms.

[FIGURE 4 about here]

The interest rate paid by firms in the treatment group is generally lower or equal to that of the control group for the five years preceding the treatment year. While the rate for the treatment group exhibits larger variance leading up to the treatment year compared to the control group, there is no significant difference in pre-trends.

Post treatment, interest rates for both the treatment and control groups increase sharply, but more so for the treatment group. The common increase in rates for both treatment and control firms is a sign that large infrastructure construction is associated with significant concurrent economic trends. For the five years following treatment, interest rates for treated firms are equal to or larger than those for control group firms in four years.

[TABLE 12 about here]

We then test this visual interpretation more formally. Table 12 presents the results of a difference in differences regression analogous to those in Table 11. The dependent variable is the interest rate on loans taken out by firms, measured in percentage points. The treatment group is defined as consisting of firms located on a different side of the project than their banks before the project is finished. The *Post Project* indicator takes the value of one in the years after completion of the project. Column 1 shows the results of a regression which features just the two indicators *Post Project*, *Treatment* as well as their interaction. The estimated coefficient on *Post Project* is 0.71 and statistically significant at the 1% level. The positive coefficient reflects the visual finding from earlier that interest rates increased for both treatment and control groups after the completion of projects. Similarly, firms in the treatment group are estimated to pay significantly lower interest rates overall, with the point estimate on the treatment group being -1.7% and statistically significantly different from 0 at the 1% level. Finally, the estimated treatment effect stemming from the interaction of the *Post Project* and *Treatment* indicators is estimated to be 0.49% and statistically significant at the 5% level.

The statistical analysis therefore confirms the visual impressions from Figure 4: Treatment firms enjoy lower interest rates before the infrastructure project is finished. Both treatment and control firms experience an increase in interest rates post-completion, but the increase is sharper for treatment firms.

Columns 2 to 5 confirm this conclusion by sequentially adding further control variables. Column 2 adds industry fixed effects as well as controls for bank relationship duration, and distance to both relationship and non relationship bank branches. Importantly, Column 2 adds geography fixed effects for each of the projects in the sample. The findings remain both statistically and economically unchanged. Column 3 adds firm level control variables, including firm size, age and the ratio of intangibles over assets. In addition, Column 3 adds rating fixed effects. Results remain largely unchanged, with the estimated treatment effect at 0.52% and statistically significant at the 5% level. Finally, Column 4 presents the most complete specification including industry, region and rating fixed effects in addition to all firm and bank level control variables from the previous columns. The estimated effects on *Post Project*, *Treatment* and their interaction retain their direction, magnitude and statistical significance with an estimated treatment effect of 0.57%.

Interestingly, the estimated coefficients are significantly larger in the big infrastructure projects sample compared to the MapPoint analysis. The point estimates on *Post Project X Treatment* in the most extensive specifications (Columns 5 and 4 in Tables 6 and 12) are 0.13% and 0.57% in the MapPoint and big project analyses, respectively. The estimates from the big project sample are therefore economically about four times as large as those in the large sample when we choose a treatment definition of 10% and dropped changes below two minutes. Since those large projects are likely associated with significantly larger changes in driving time, their impact should be more similar to that of the 15 and 20 minute definitions of treatment. Table 10 reports estimated treatment effects of 0.38% and 0.40% for treatment sizes of 15 and 20 minutes, respectively. As expected, those estimates are more in line with those from the large infrastructure project sample.

The results in this section independently confirm the relevancy of distance on bank lending. When travel time between borrowers and their banks decreases, banks capture the resulting rents from reduced travel time as predicted by the local market power hypothesis. The effects are economically more significant for large infrastructure projects and comparable to the estimates from the largest treatment sizes in the MapPoint sample.

7 Conclusion

A number of recent papers study the impact of changes in transportation time between economic agents. Many find that closer distance enables the collection of soft information and so allows for superior monitoring. The resulting surplus is internalized when monitoring principals hold equity stakes in agents (Giroud, 2013; Bernstein et al., 2015). We extend this line of research to the inter-firm setting of bank lending. Since banks do not hold an equity stake in borrowers, distance plays a dual role: Closer distance is associated with better access to information, yet also provides banks with local market power. The existing banking literature finds contradicting results to this question, likely due to the endogenous nature of location choice. We overcome this identification challenge by exploiting exogenous variations in lender-borrower distance from the construction of new infrastructure. We find that borrowers are significantly more likely to initiate a new banking relationship with a bank after the travel distance to its branch has fallen. For existing relationships on the other hand we find that banks capture a fraction of the created surplus by increasing interest rates.

References

Abadie, A., and G.W. Imbens, 2002, Simple and bias-corrected matching estimators for average treatment effects, *Working paper, National Bureau of Economic Research Cambridge* .

Agarwal, Sumit, and Robert Hauswald, 2010, Distance and private information in lending, *Review of Financial Studies* 23, 2757–2788.

Beck, Thorsten, Steven Ongena, and Ilkay Sendeniz-Yuncu, 2016, Keep walking? geographical proximity, religion, and relationship banking, *Working Paper, University of Zurich* .

Bellucci, Andrea, Alexander Borisov, and Alberto Zazzaro, 2013, Do banks price discriminate spatially? Evidence from small business lending in local credit markets, *Journal of Banking & Finance* 37, 4183–4197.

Berger, Allen N, Nathan H Miller, Mitchell A Petersen, Raghuram G Rajan, and Jeremy C Stein, 2005, Does function follow organizational form? Evidence from the lending practices of large and small banks, *Journal of Financial Economics* 76, 237–269.

Berger, Allen N, Anthony Saunders, Joseph M Scalise, and Gregory F Udell, 1998, The effects of bank mergers and acquisitions on small business lending, *Journal of Financial Economics* 50, 187–229.

Berner, Endre, Aksel Mjøs, and Marius Olving, 2015, Norwegian corporate accounts - documentation and quality assurance of snf's and nhh's database of accounting and company information for norwegian companies, *SNF Working Paper 10/16* .

Bernstein, Shai, Xavier Giroud, and Richard R Townsend, 2015, The impact of venture capital monitoring, *Journal of Finance* 71, 1591–1622.

Brickley, James A, James S Linck, and Clifford W Smith, 2003, Boundaries of the firm: evidence from the banking industry, *Journal of Financial Economics* 70, 351–383.

Brown, James R, J Anthony Cookson, and Rawley Heimer, 2017, Growing up without finance, *Working Paper* .

Butler, Alexander W, 2008, Distance still matters: Evidence from municipal bond underwriting, *Review of Financial Studies* 21, 763–784.

Celerier, Claire, and Adrien Matray, 2017, Bank branch supply and the unbanked phenomenon, *Working Paper* .

Degryse, Hans, Luc Laeven, and Steven Ongena, 2009, The impact of organizational structure and lending technology on banking competition, *Review of Finance* 13, 225–259.

Degryse, Hans, and Steven Ongena, 2005, Distance, lending relationships, and competition, *Journal of Finance* 60, 231–266.

Giannetti, Mariassunta, and Luc Laeven, 2012, The flight home effect: Evidence from the syndicated loan market during financial crises, *Journal of Financial Economics* 104, 23–43.

Giroud, Xavier, 2013, Proximity and investment: Evidence from plant-level data, *Quarterly Journal of Economics* 128, 861–915.

Giroud, Xavier, and Holger Müller, 2015, Capital and labor reallocation within firms, *Journal of Finance* 70, 1767–1804.

Hollander, Stephan, and Arnt Verriest, 2016, Bridging the gap: the design of bank loan contracts and distance, *Journal of Financial Economics* 119, 399–419.

Hoover, Edgar M, 1937, Spatial price discrimination, *Review of Economic Studies* 4, 182–191.

Ioannidou, Vasso, and Steven Ongena, 2010, Time for a change: Loan conditions and bank behavior when firms switch banks, *Journal of Finance* 65, 1847–1877.

Kim, Moshe, and Bent Vale, 2001, Non-price strategic behavior: the case of bank branches, *International Journal of Industrial Organization* 19, 1583–1602.

Knyazeva, Anzhela, and Diana Knyazeva, 2012, Does being your banks neighbor matter?, *Journal of Banking & Finance* 36, 1194–1209.

Nguyen, Hoai-Luu Q, 2017, Are credit markets still local? evidence from bank branch closings, *Working Paper, Haas School of Business* .

Ono, Arito, Yukiko Saito, Koji Sakai, and Iichiro Uesugi, 2016, Does geographical proximity matter in small business lending? Evidence from changes in main bank relationships, *Unpublished working paper, Institute of Economic Research, Hitotsubashi University* .

Petersen, Mitchell A, and Raghuram G Rajan, 1995, The effect of credit market competition on lending relationships, *Quarterly Journal of Economics* 407–443.

Petersen, Mitchell A, and Raghuram G Rajan, 2002, Does distance still matter? the information revolution in small business lending, *Journal of Finance* 57, 2533–2570.

Schwert, Michael, 2017, Bank capital and lending relationships, *Journal of Finance, forthcoming* .

Von Thadden, Ernst-Ludwig, 2004, Asymmetric information, bank lending and implicit contracts: The winner's curse, *Finance Research Letters* 1, 11–23.

Appendix for
The causal impact of distance on bank lending

A Details on the calculation of driving times

To use MapPoint we standardize and clean all firm and bank addresses. Since firm data are compiled from multiple sources, there is no common format for firm addresses. Both the order of information (ZIP Codes, Street Address etc.) and the information itself (abbreviations) vary significantly. In addition, large Norwegian firms often have unique ZIP codes assigned to them. This means that some firms will have a unique ZIP code that is different from the ZIP code of neighboring houses. This is an issue since in particular older route planning software will often not recognize those unique ZIP codes and calculations will fail in those cases. Similar issues arise with the bank addresses.

To standardize the addresses we rely on Google Map's autocorrection algorithm. For each address of firms or bank branches, we send a search-query to Google maps containing the street name, house number and city of the address, followed by the letters ", Norway". Google's algorithm automatically corrects spelling mistakes and incorrect (unique) ZIP codes. In addition, it expands all abbreviations and returns a suggested address. We then record the returned address suggestion as the corrected address.

To ease the computational intensity, we avoid to calculate the driving distance between all sample firms (28,000) and all bank branches (1,300) using our five different MapPoint versions. Instead, we use the following multi step approach:

[FIGURE 3 here]

We begin by identifying neighboring one-digit postcode areas. Figure 3 shows those areas. We then calculate the driving time between all four digit postcodes contained in any neighboring single digit region. For example we calculate the driving time between any postcode starting with an 8 and any postcode starting with either the number 7 or 9. Doing this yields the approximate driving time between the center of one postcode and the the center of another one.

For each firm address, we then use this postcode driving matrix to identify all postcode areas which can be reached from the firm's postcode area in at most four hours drive. We choose this cutoff since for longer distances, managers are likely to switch to flying. We then calculate for each firm the driving distance between the firm's address and any bank branch located inside any postal zone which can be reached in at most four hours. This does allow for actual driving time both above and below four hours for two reasons:

First, in large, rural postal zones the distance between the zone's center and the outer fringes can be significant. Second, geographical features can impact driving time: If a postal zone covers both sides of a fjord or mountain, reaching the far side of the fjord might require a lot more time than reaching the zone's center even if the surface area of the zone is relatively small.

Hence there might be some bank branches which can be reached in four hours from a firm's address but if they were lying at the fringe of a postcode who's center can not actually be reached in four hours, we might exclude them from our search. Vice versa, we will sometimes include banks in our search which are located a lot further than four hours drive from a firm's address. At this stage we are more concerned with type two errors of omitting bank branches that might be relevant, than with type one errors of calculating driving time for bank branches far away. We therefore choose the rather generous four hour cut off for postal zones mentioned above.

For firms in dense urban areas such as Oslo, this procedure yields dozens of banks whereas for firms in rural north Norway even the relatively generous four hour cutoff only yields a handful of banks. To keep our data to a manageable size, we limit the number of bank branches to the closest 20 banks found in the above fashion for each firm address. Similar to Degryse and Ongena (2005) we argue that even with heterogeneous offers from banks, most firms will find an offer suitable for them among twenty branches. Finally, we only consider the single closest branch for each bank. To illustrate, consider there are two branches of DNB Bank near a firm, in five and ten minutes driving time respectively. We will then only consider the branch in 5 minutes time based on the assumption that two branches of the same bank will act as perfect substitutes.

Tables and Figures

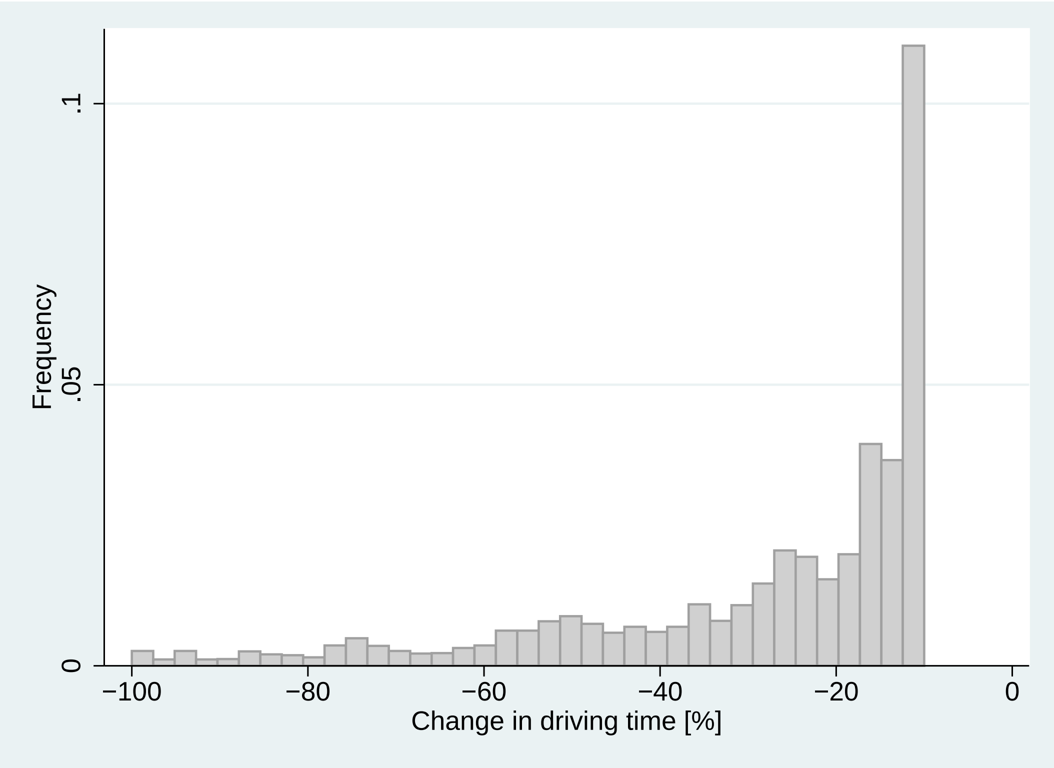

Figure 1: Frequency of driving time reductions
This figure plots the frequency of relative driving time reductions in percent for treated observations for the MapPoint sample.

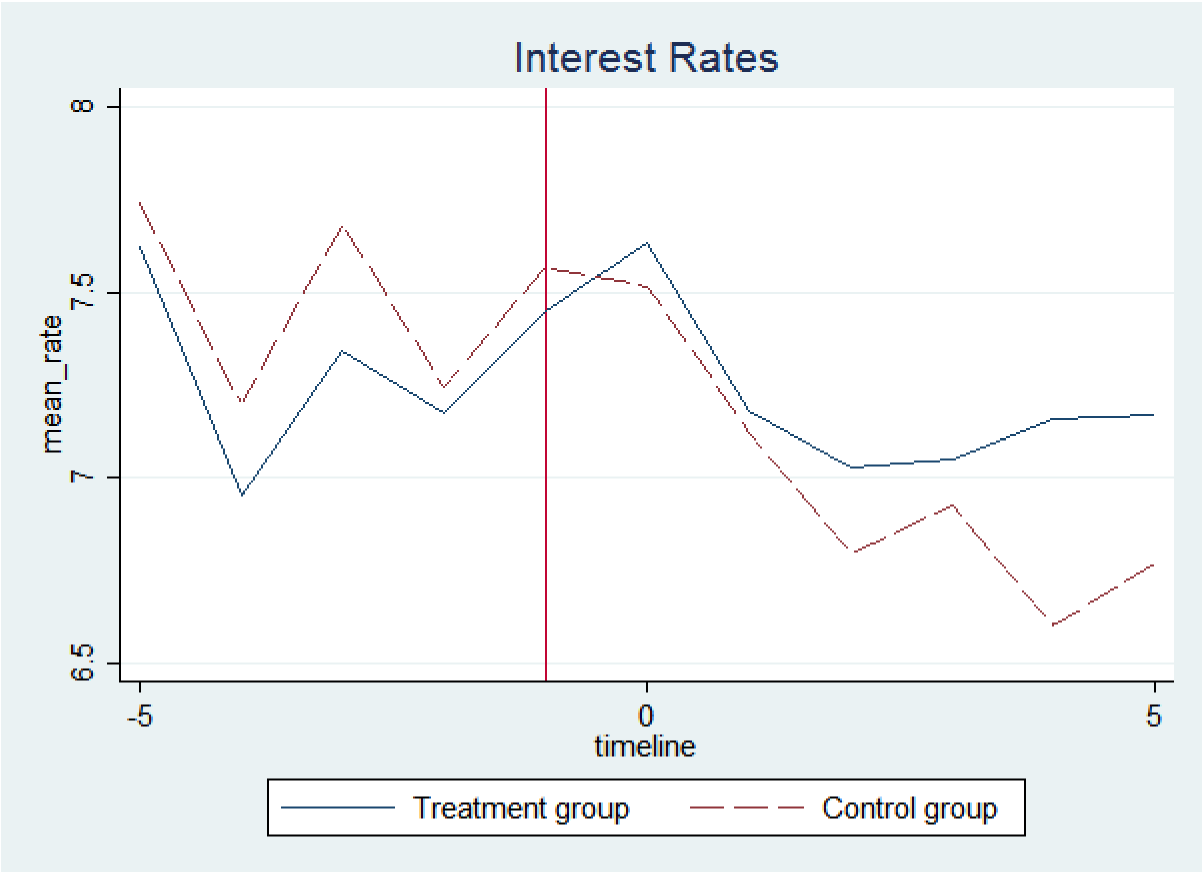

Figure 2: Parallel Trends: MapPoint Sample
This figure plots the mean interest rate of treatment and control firms from the matched MapPoint sample against time to treatment.

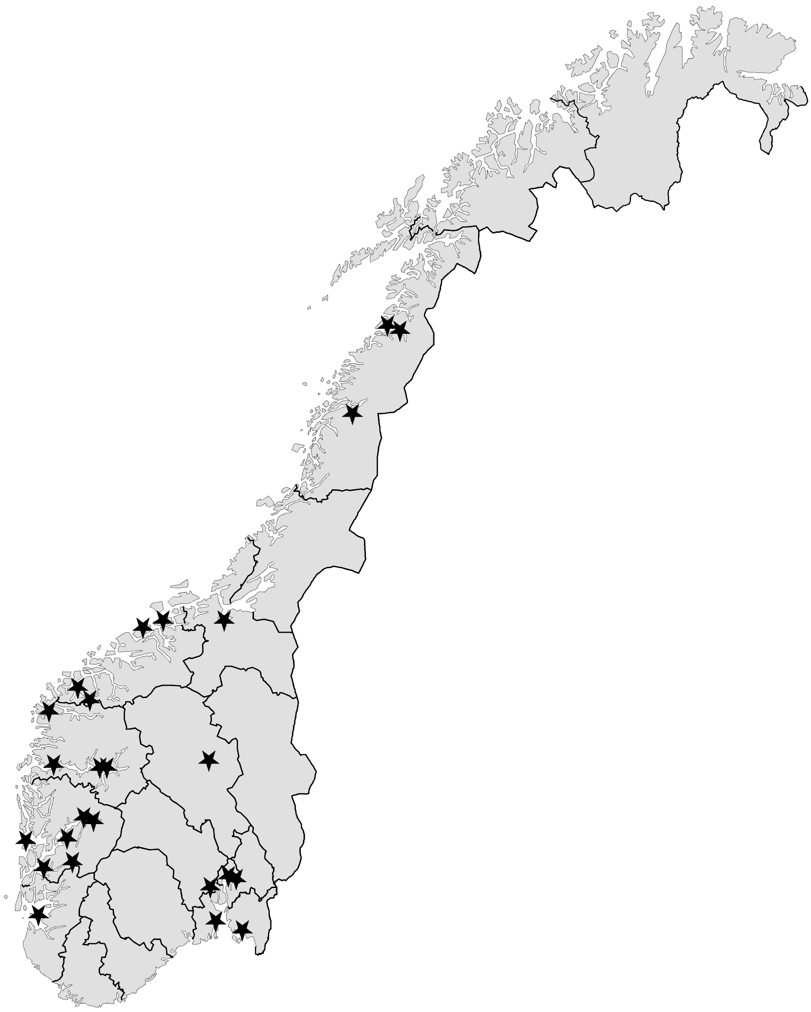

Figure 3: Map of Norway and location of large infrastructure projects
This figure shows the geography of Norway and the location of the large infrastructure projects used in the Big Project Sample. The borders correspond to the one-digit postcode areas.

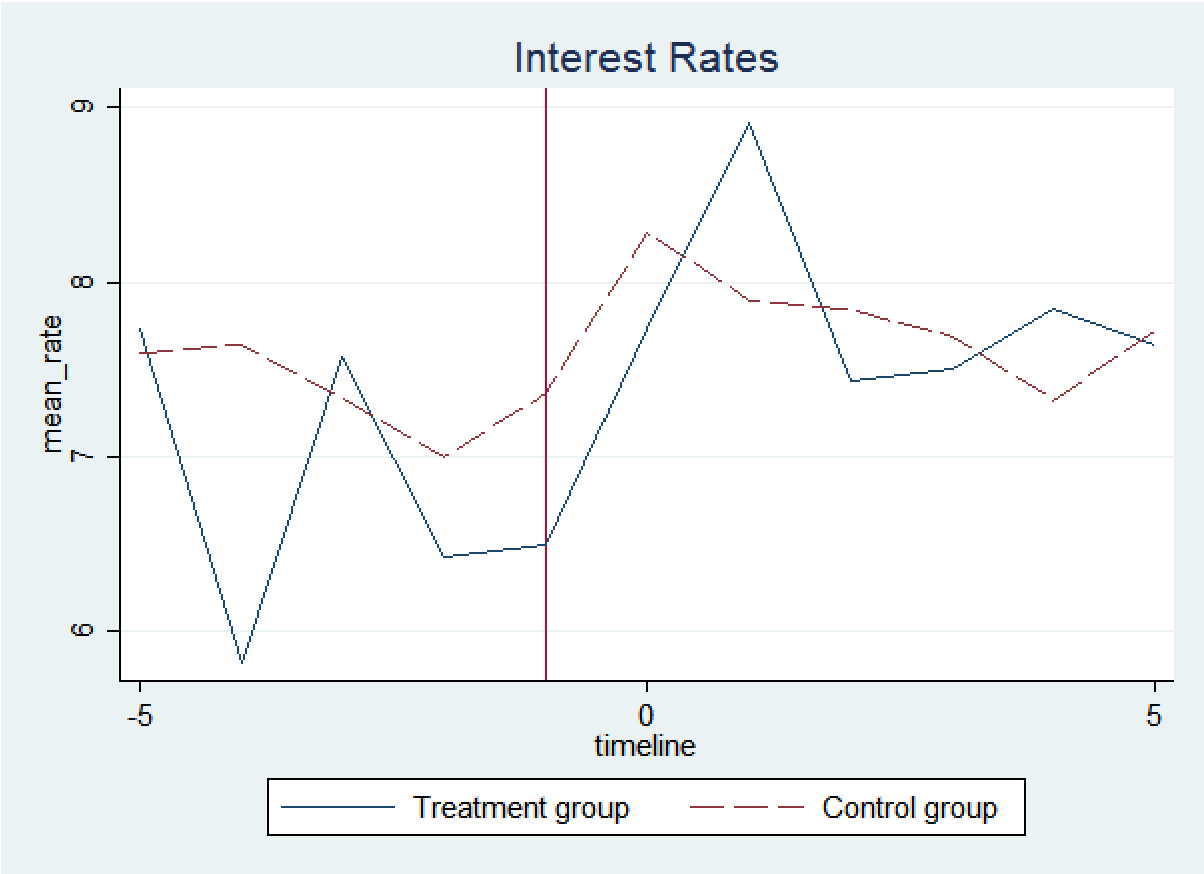

Figure 4: Parallel Trends: Big Projects
This figure plots the mean interest rate of treatment and control firms from the big project sample against time to treatment.

Table 1: Map Sources and Map Vintages
This table shows the release dates of the different MapPoint versions and which version we assigned to which map vintage.

Version	Release Date	Map Vintage
MS MapPoint 2006	6/30/2006	2005
MS MapPoint Europe 2009	12/30/2008	2008
MS MapPoint Europe 2010	1/10/2010	2009
MS MapPoint Europe 2011	8/23/2011	2010
MS MapPoint Europe 2013	1/3/2013	2012

Table 2: Driving Time Changes for Treated Firms

This table shows summary statistics for changes in driving time in hours for a given firm bank pair between the years 2005, 2008, 2009, 2010 and 2012. We report both the driving time before treatment and the driving time after treatment. Treatment is defined as a reduction in driving time by more than 10% and by more than two minutes.

Variable	Mean	SD	P25	Median	P75	Min	Max	N
Driving Time Pre-Treatment [in h]	0.58	0.48	0.23	0.42	0.83	0.03	3.00	5449
Driving Time Post-Treatment [in h]	0.45	0.42	0.14	0.33	0.64	0.00	2.65	5449
Change in Driving Time [in h]	-0.13	0.14	-0.15	-0.08	-0.05	-1.00	-0.03	5449
Change in Driving Time [in %]	-28.51	21.14	-37.49	-19.66	-12.22	-100.00	-10.00	5449

Table 3: Variable Definitions

The tables defines the variable definitions.

Variable	Units	Definition
Age	Years	Firm age since founding.
Assets	NOK '000	Total assets (both fixed and current).
Book leverage		1-fraction of equity in total assets
Distance Relationship Banks	Hours	Average driving time between the firm's address and its loan relationship banks' addresses. Calculated for the fastest driving connection.
Distance Banks Without Relationship	Hours	Average driving time between the firm's address and the addresses of the five closest banks which are not relationship banks. Calculated for the fastest driving connection.
Duration Relationship Banks	Years	Average number of years since first account reported between firm and bank. Winsorized at seven years.
Industry Fixed Effects	Binary	Indicator indicating one of twelve different industries (data item bransjegr_07)
Industry specialisation		Fraction of bank's total lending in customer industry (code bransjegr_07)
Intangibles / Assets	%	Intangible total assets composed of activated R&D expenditure, patents, goodwill and deferred tax assets.
Loan Amount	NOK '000	End of year balance on loan account.
Loan Interest Rate	%	Interest rate, calculated as the fraction of interest accrued over loan amount outstanding at year end.
Municipality Fixed Effects	Binary	Indicator indicating one of 120 commuter regions.
Number of Relationship Banks		Number of different banks with which firm has at least one loan account for each year.
Number Banks without Relationship		Number of banks within driving time radius
Rating quality	0-5	Rating quality measure between 1 (low quality) and 5 (high quality). A value of 0 is assigned to unrated companies.
Sales	NOK '000	Annual revenues
Treatment	Binary	Indicator equal to one if the driving time between the firm and the bank address has experienced a reduction of at least 10% and at least two minutes.

Table 4: Summary Statistics
This table reports sample summary statistics for all observations (Columns 1-6), for treated (Columns 7-9) and non-treated observations (Columns 10-12) separately. All variables are defined in the data appendix. The sample period spans the years from 2005 to 2012.

	All						Treatment			No Treatment		
Variables	Mean	SD	P25	Median	P75	N	Mean	Median	N	Mean	Median	N
Age	16	13	7	13	20	62626	18	15	5360	15	13	57266
Assets	18355	36602	2998	6133	14960	62626	20067	6709	5360	18194	6077	57266
Book leverage	0.76	0.21	0.63	0.77	0.89	62626	0.74	0.75	5360	0.76	0.77	57266
Employees	13	17	4	7	14	62626	13	8	5360	13	7	57266
Distance Relationship Banks	0.32	0.34	0.09	0.17	0.42	62626	0.47	0.34	5360	0.31	0.16	57266
Distance Banks without Relationship	0.49	0.44	0.14	0.34	0.75	62626	0.71	0.64	5360	0.47	0.31	57266
Duration Relationship Banks	5.45	1.94	4.00	7.00	7.00	62626	5.93	7.00	5360	5.40	6.67	57266
Industry specialisation	0.11	0.12	0.04	0.07	0.10	62626	0.10	0.07	5360	0.11	0.07	57266
Intangibles/Assets	0.02	0.07	0.00	0.00	0.02	62626	0.02	0.00	5360	0.02	0.00	57266
Loan amount (logs)	7.35	1.57	6.33	7.31	8.32	62626	7.47	7.44	5360	7.34	7.30	57266
Loan Interest Rate	7.45	2.57	5.60	6.99	8.77	62626	7.47	7.05	5360	7.45	6.98	57266
Number Banks w/o Relationship	67.64	26.40	47.00	68.00	89.00	62626	71.24	72.00	5360	67.30	68.00	57266
Number of Relationship Banks	1.08	0.29	1.00	1.00	1.00	62626	1.09	1.00	5360	1.08	1.00	57266
Rating Quality	3.52	1.09	3.00	4.00	4.00	62626	3.52	4.00	5360	3.52	4.00	57266
Sales	24443	41307	4634	10126	24318	62626	26282	11114	5360	24271	10049	57266

Table 5: Relationship Initiation

This table presents the results for regressions of the initiation of banking relationships on changes in driving time. The dependent variable is a dummy equal to 1 if a firm has taken out a loan from a bank for the first time in that year. T-statistics in parentheses are based on robust standard errors which are clustered on the region-year level. Statistical significance at the 1%, 5% and 10% level is indicated by ***, ** and *, respectively.

	New Relationship *100				
	(1)	(2)	(3)	(4)	(5)
Treatment x Post	1.24***	1.25***	1.26***	0.73***	2.22***
	(4.39)	(4.34)	(4.37)	(2.61)	(3.61)
Treatment	0.01	0.01	-0.07*	-0.00	-0.00
	(0.26)	(0.17)	(-1.77)	(-0.11)	(-0.11)
Treatment x Post x Number Banks w/o Relationship					-0.02***
					(-2.61)
Number Banks w/o Relationship				-0.00***	-0.00***
				(-6.78)	(-6.54)
Sales (logs)		0.04***		0.04***	0.04***
		(5.03)		(5.80)	(5.82)
Intangibles/Assets		0.10		0.16	0.16
		(0.47)		(0.72)	(0.73)
Book leverage		0.09*		0.10*	0.10*
		(1.66)		(1.82)	(1.81)
Distance Relationship Banks			0.49***	0.47***	0.47***
			(6.79)	(6.49)	(6.48)
Distance Banks w/o Relationship			0.30***	0.29**	0.29**
			(2.72)	(2.39)	(2.39)
N	953730	953729	953729	953729	953729
adj. R^2	0.001	0.001	0.002	0.009	0.009
Bank Fixed Effects	No	No	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No	No	No
Municipality Fixed Effects	Yes	No	No	No	No
Municipality & Year Fixed Effects	No	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes
Rating Fixed Effects	No	Yes	No	Yes	Yes

Table 6: Interest rates

This table reports difference in differences results for loan interest rates exploiting changes in driving time between banks and firms. The dependent variable is loan interest rate. Treatment equals one if any of the relationship banks moves closer (is treated). Sample period spans 2005 to 2012. All other variables are defined in the Data Appendix (Table 3). T-statistics in parentheses are based on robust standard errors which are clustered on the region-year level. Statistical significance at the 1%, 5% and 10% level is indicated by ***, ** and *, respectively.

	Loan Interest Rate				
	(1)	(2)	(3)	(4)	(5)
Treatment X Post	0.14***	0.13***	0.14***	0.14***	0.13***
	(2.60)	(2.65)	(2.90)	(2.94)	(2.77)
Treatment	-0.13***	-0.17***	-0.15***	-0.13***	-0.12***
	(-3.16)	(-4.02)	(-3.59)	(-3.27)	(-2.97)
Distance Relationship Banks			-0.09*		-0.08*
			(-1.75)		(-1.78)
Distance Banks w/o Relationship			-0.11**		-0.10*
			(-1.96)		(-1.88)
Duration Relationship Banks			-0.03***		0.03***
			(-5.26)		(5.31)
Age				-0.01***	-0.01***
				(-10.10)	(-11.50)
Sales (logs)				-0.05***	-0.05***
				(-8.93)	(-9.13)
Intangibles/Assets				0.25	0.27*
				(1.53)	(1.67)
N	62556	62535	62535	62535	62535
adj. R^2	0.121	0.137	0.138	0.170	0.170
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes
Municipality & Year Effects	Yes	Yes	Yes	Yes	Yes
Rating Fixed Effects	No	No	No	Yes	Yes
Bank Fixed Effect	No	Yes	Yes	Yes	Yes

Table 7: Loan size

This table reports difference in differences results for loan size exploiting changes in driving time between banks and firms. The dependent variable is the loan amount, measured as the logarithm of the nominal loan amounts. Treatment equals one if any of the relationship banks moves closer (is treated). Sample period spans 2005 to 2012. All other variables are defined in the Data Appendix (Table 3). T-statistics in parentheses are based on robust standard errors which are clustered on the region-year level. Statistical significance at the 1%, 5% and 10% level is indicated by ***, ** and *, respectively.

	Loan Amount (Log NOK)				
	(1)	(2)	(3)	(4)	(5)
Treatment X Post	-0.13***	-0.13***	-0.15***	-0.15***	-0.16***
	(-3.97)	(-4.02)	(-4.72)	(-5.07)	(-5.26)
Treatment	0.26***	0.27***	0.27***	0.24***	0.23***
	(11.25)	(12.34)	(11.96)	(11.05)	(10.76)
Distance Relationship Banks			0.11***		0.11***
			(3.70)		(3.86)
Distance Banks w/o Relationship			-0.16***		-0.10***
			(-4.73)		(-3.19)
Duration Relationship Banks			0.06***		0.03***
			(17.32)		(6.40)
Age				0.01***	0.01***
				(31.73)	(23.84)
Sales (logs)				0.16***	0.16***
				(20.23)	(20.17)
Intangibles/Assets				0.67***	0.69***
				(6.85)	(6.96)
N	62556	62535	62535	62535	62535
adj. R^2	0.128	0.149	0.154	0.204	0.205
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes
Municipality & Year Effects	Yes	Yes	Yes	Yes	Yes
Rating Fixed Effects	No	No	No	Yes	Yes
Bank Fixed Effect	No	Yes	Yes	Yes	Yes

Table 8: Interest Rates Interactions
This table reports difference in differences results for loan interest rates, exploiting cross sectional heterogeneity in treatment firms. The dependent variable is loan interest rate. Treatment equals one if any of the relationship banks moves closer (is treated). Sample period spans 2005 to 2012. All other variables are defined in the Data Appendix (Table 3). T-statistics in parentheses are based on robust standard errors which are clustered on the region-year level. Statistical significance at the 1%, 5% and 10% level is indicated by ***, ** and *, respectively.

	Loan Interest Rate		
	(1)	(2)	(3)
Treatment	-0.12***	-0.12***	-0.08**
	(-2.95)	(-2.92)	(-2.02)
Treatment × *Post*	-0.14	0.41***	0.19***
	(-0.88)	(3.41)	(3.23)
Treatment × *Post* × *Duration relationship*	0.04*		
	(1.84)		
Treatment × *Post* × *Rating quality*		-0.08**	
		(-2.50)	
Treatment × *Post* × *Industry specialisation*			-0.48**
			(-2.16)
Duration Relationship Banks	0.03***		
	(4.75)		
Rating quality		-0.34***	
		(-31.54)	
Industry specialisation			-0.76***
			(-5.28)
Distance Relationship Banks	-0.08*	-0.10**	-0.14***
	(-1.77)	(-2.07)	(-2.73)
Distance Banks w/o Relationship	-0.10*	-0.09	-0.03
	(-1.88)	(-1.58)	(-0.61)
Age	-0.01***	-0.01***	-0.01***
	(-11.52)	(-10.88)	(-11.05)
Sales (logs)	-0.05***	-0.05***	-0.05***
	(-9.13)	(-9.33)	(-8.78)
Intangibles/Assets	0.27	0.46***	0.06
	(1.64)	(2.74)	(0.37)
N	62535	62535	62556
adj. R^2	0.170	0.164	0.156
Industry Fixed Effects	Yes	Yes	Yes
Municipality & Year Effects	Yes	Yes	Yes
Rating Fixed Effects	Yes	No	Yes
Bank Fixed Effect	Yes	Yes	Yes

Table 9: Geographical distances in the literature
This table reports findings from the prior literature on the nature of distance and interest rates. In addition to the country of investigation, the sample mean of distance and the estimated effect of a reduction in distance, it provides a standardized measure of the implied cross sectional relationship between a eight minutes reduction in driving time and interest rates.

Paper	Country	Mean dist.	Estimated effect	8 Min. effect[a]
Petersen and Rajan (2002)	USA	115 Miles	-37bp/mile	+96bp
Degryse and Ongena (2005)	Belgium	6.9 Min.	-18bp/mile	+47bp
Agarwal and Hauswald (2010)	USA	10.25 Min.	-15bp/mile	+39bp
Knyazeva and Knyazeva (2012)	USA	744 Miles	+1bp/mile[b]	-3bp
Bellucci et al. (2013)	Italy	4.98 Km	+6.7bp/km	-31bp
Our findings	Norway	19.2 Min.	-1.63/Min.[c]	+13bp

[a] Cross-sectional effect of a *reduction* in driving time by 8 minutes assuming speed of 20 mph (35 km/h)
[b] Effect for a one standard deviation increase in log distance starting as neighbor from distance 0
[c] Treatment effect of 13 bp and average treatment size of 8 minutes

Table 10: Interest rates and treatment magnitude
This table reports difference in differences results for loan interest rates exploiting changes in driving time between banks and firms. The dependent variable is loan interest rate. "Treatment X" equals one if any of the relationship banks moves closer (is treated) by at least X minutes. Sample period spans 2005 to 2012. All other variables are defined in the Data Appendix (Table 3). T-statistics in parentheses are based on robust standard errors which are clustered on the region-year level. Statistical significance at the 1%, 5% and 10% level is indicated by ***, ** and *, respectively.

	Loan Interest Rate			
	(1)	(2)	(3)	(4)
Treatment 5 × *Post*	0.10*			
	(1.74)			
Treatment 5	-0.16***			
	(-3.91)			
Treatment 10 × *Post*		0.19**		
		(2.02)		
Treatment 10		-0.27***		
		(-3.91)		
Treatment 15 × *Post*			0.38***	
			(2.94)	
Treatment 15			-0.24**	
			(-2.55)	
Treatment 20 × *Post*				0.40**
				(2.57)
Treatment 20				-0.41***
				(-3.54)
Distance Relationship Banks	-0.07	-0.08	-0.09*	-0.09*
	(-1.43)	(-1.61)	(-1.80)	(-1.79)
Distance Banks w/o Relationship	-0.11*	-0.10*	-0.11**	-0.11*
	(-1.91)	(-1.85)	(-2.03)	(-1.92)
Duration relationship	0.03***	0.03***	0.03***	0.03***
	(5.37)	(5.35)	(5.31)	(5.33)
Age	-0.01***	-0.01***	-0.01***	-0.01***
	(-11.50)	(-11.55)	(-11.57)	(-11.58)
Sales (logs)	-0.05***	-0.05***	-0.05***	-0.05***
	(-9.13)	(-9.12)	(-9.15)	(-9.15)
Intangibles/Assets	0.27*	0.27*	0.27*	0.27*
	(1.67)	(1.65)	(1.66)	(1.66)
N	62535	62535	62535	62535
adj. R^2	0.170	0.170	0.170	0.170
Industry Fixed Effects	Yes	Yes	Yes	Yes
Municipality & Year Effects	Yes	Yes	Yes	Yes
Rating Fixed Effects	Yes	Yes	Yes	Yes
Bank Fixed Effect	Yes	Yes	Yes	Yes

Table 11: Interest Rates and matching
This table reports difference in differences results for loan interest rates. We employ propensity score matching to match firms which experience a reduction in distance to their banks by at least two minutes and 10% of the pre-treatment driving time with a group of control firms which did not. The dependent variable is loan interest rate. Treatment equals one if any of the relationship banks moves closer (is treated). Sample period spans 2005 to 2012. All other variables are defined in the Data Appendix (Table 3). T-statistics in parentheses are based on robust standard errors which are clustered on the region-year level. Statistical significance at the 1%, 5% and 10% level is indicated by ***, ** and *, respectively.

	Loan Interest Rate				
	(1)	(2)	(3)	(4)	(5)
Treatment Group	-0.16**	-0.12*	-0.12*	-0.13**	-0.13**
	(-2.43)	(-1.88)	(-1.88)	(-2.09)	(-2.07)
Post	-0.30***	-0.31***	-0.30***	-0.28***	-0.29***
	(-3.35)	(-3.90)	(-3.85)	(-3.53)	(-3.79)
Treatment x Post	0.32***	0.19**	0.19**	0.16**	0.17**
	(3.39)	(2.25)	(2.34)	(1.97)	(2.07)
Distance Banks w/o Relationship			-0.23*		-0.26*
			(-1.70)		(-1.91)
Distance Relationship Banks			0.15		0.16
			(1.50)		(1.61)
Duration Relationship			-0.02		0.03*
			(-1.38)		(1.91)
Age				-0.00*	-0.00**
				(-1.78)	(-2.37)
Sales (logs)				-0.08***	-0.08***
				(-7.22)	(-6.88)
Intangibles/Assets				-0.42	-0.00
				(-1.30)	(-0.00)
N	13465	13328	13314	13328	13314
adj. R^2	0.002	0.160	0.191	0.195	0.226
Industry Fixed Effects	No	Yes	Yes	Yes	Yes
Municipality & Year Effects	No	No	Yes	Yes	Yes
Rating Fixed Effects	No	No	No	Yes	Yes
Bank Fixed Effect	No	No	Yes	No	Yes

Table 12: Interest rates: Big infrastructure projects
This table reports difference in differences results for loan interest rates exploiting changes in driving time between banks and firms caused by large infrastructure projects. The dependent variable is loan interest rate. Treatment equals one if any of the relationship banks moves closer (is treated) due to the completion of a large infrastructure project. Sample period spans 2005 to 2012. All other variables are defined in the Data Appendix (Table 3). T-statistics in parentheses are based on robust standard errors which are clustered on the region-year level. Statistical significance at the 1%, 5% and 10% level is indicated by ***, ** and *, respectively.

	Loan Interest Rate			
	(1)	(2)	(3)	(4)
Post Project x Treatment	0.49**	0.50**	0.52**	0.57**
	(2.05)	(2.13)	(2.24)	(2.51)
Post Project	0.71***	0.66***	0.68***	0.63***
	(4.49)	(4.31)	(4.38)	(4.19)
Treatment	-1.70***	-1.79***	-1.61***	-1.75***
	(-4.30)	(-4.69)	(-4.17)	(-5.01)
Distance Banks w/o Relationship		-0.55*		-0.56*
		(-1.69)		(-1.78)
Distance Relationship Banks		-0.07		-0.04
		(-0.32)		(-0.21)
Duration Relationship Banks		0.04		0.07***
		(1.61)		(3.00)
Age			-0.01	-0.01*
			(-0.89)	(-1.70)
Sales (logs)			-0.03	-0.04
			(-1.20)	(-1.51)
Intangibles/Assets			0.54	0.69
			(0.62)	(0.79)
N	4630	4630	4630	4630
adj. R^2	0.170	0.171	0.175	0.177
Industry Fixed Effects	No	Yes	Yes	Yes
Region Fixed Effects	No	Yes	Yes	Yes
Rating Fixed Effects	No	No	Yes	Yes