The causal impact of distance on bank lending*

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1 Introduction

We analyze the role of physical distance on both the formation of new lending relationships and the characteristics of existing relationships. Recent work has established the beneficial nature of close distance on monitoring in the presence of asymmetric information. Such positive information effects have been widely documented in situations where the principal holds a significant equity stake in the monitored party, such as between corporate headquarters and plants (Giroud, 2013; Giroud and Müller, 2015) or venture capitalists and portfolio companies (Bernstein et al., 2015).

We extend this literature to corporate bank lending. Banks located close to borrowers might posses superior information about those firms before granting a loan, and be better able to monitor them afterwards (Hauswald and Marquez, 2003). We will refer to this idea as the *monitoring hypothesis* for the remainder of our paper. But bank loans are *inter-firm* transactions in which the lender does not hold an equity stake in the partner firm, unlike in Giroud (2013) and Bernstein et al. (2015). The arms length transactional character adds a novel dimension to the link of distance and bank lending. Lower physical distance reduces both direct transportation costs such as fuel and, more importantly, indirect costs such as the entrepreneur's opportunity cost. If banks have negotiation power over their borrowers, they can capture this rent through higher interest rates (Hotelling, 1929; Lederer and Hurter Jr, 1986). We will refer to this idea as the *local market power hypothesis*, which predicts that lower distances between borrowers and banks result in higher interest rate due to banks' local monopoly power.

The local market power and monitoring hypotheses therefore produce contradicting predictions regarding the connection of distance and the pricing of credit: If the information channel dominates, loan rates should increase and loan size should decrease in distance since borrowers located further away from banks face higher degrees of asymmetric information. If the market power channel dominates, loan rates will decrease in distance, because firms located further from their bank will face relatively lower local market power from their lender compared to outside financing options. As banks use their local monopoly to extract higher prices, loan sizes should fall for borrowers located closer to their banks.

The existing literature on distance and bank lending arrives at contradicting conclusions: Out of the five papers which utilize actual firm-bank distances, two find that interest rates *decrease* in distance, as predicted by the local market power hypothesis, whereas three find that interest rates *increase* in distance as predicted by the monitoring hypothesis.[1]

The contradictory findings in the cross sectional literature are likely the result of endogenous location decisions by both banks and borrowers (Kim and Vale, 2001). If distance does indeed matter for bank lending, parties will be aware of this and chose optimal locations, as in Lederer and Hurter Jr (1986).

We therefore overcome the endogeneity challenge of location choice by exploiting an exogenous variation in driving time between banks and their existing borrowers. The variation we exploit stems from changes to road infrastructure in Norway. Improvements such as new tunnels, bridges or roads leave both firm and bank location unchanged but significantly decrease driving time. We utilize these reductions in distance as a natural quasi experiment to obtain estimates of the causal relationship between distance and bank lending.

We begin our investigation by focusing on the impact of a reduction in travel distance on the creation of new banking relationships. We find that firms are significantly more

[1]Table 3 provides an overview over those papers and their respective findings.

likely to initiate a new banking relationship with a lender after a reduction in driving time to that lender's closest bank branch. The positive effect of close distance on the initiation of banking relationships increases in the treatment size. Those findings demonstrate two things: First, distance appears to be a significant driver of the matching of firms and banks, which emphasizes the need for a well-identified inference strategy. Second, the positive association of close distance and relationship initiation is evidence for the existence of the benign link of proximity and asymmetric information: Since firms react positively to a reduction in travel distance, they benefit at least marginally from closer distances to lenders.

We then turn to the effect of reductions in travel time on existing bank relationships. We find strong support for the market power hypothesis. Using a sample of 28,000 Norwegian firms and 1300 different Norwegian bank branches between 2005 and 2012, we find that an average reduction in driving time between banks and borrowers by six minutes significantly increases interest rates by 14 basis points, or 2% of the pre-treatment rate. Larger reductions in travel time are associated with larger treatment effects: Reductions in travel time exceeding 20 minutes, for example, are associated with an increase in interest rates of 37 basis points. As predicted by monopolistic pricing, the higher (marginal) cost of funds are associated with a reduction in loan size.

We find further evidence in favor of the local market power hypothesis when we analyze cross sectional differences in treatment: Banks with higher ex ante negotiation power have a stronger ability to price discriminate. When borrowers are poorly rated or locked in a long run relationship, banks are able to increase interest rates further than otherwise.

Our paper contributes to several strands of the literature. Our article is primarily related to prior work that shows that the physical distance can act in similar ways as direct financing constraints. Giroud (2013) investigates the effect of the opening of new airline connections on travel time between firm headquarters and local plants and the resulting shock to productivity. He finds that reductions in travel time lead to increased investment and productivity on the plant level. Bernstein et al. (2015) document a similarly beneficial effect of reduced distance in the context of venture capital monitoring.

Both these papers share the fact that principals (headquarters, VC investors) hold a significant fraction of equity in agents (plants, portfolio companies). Those papers therefore analyze situations in which the benefits from closer physical distance are fully internalized. Our analysis is novel in that it deals with arms length contracts, in which the higher degree of information creates a rent which is then shared between banks and borrowers. Banks' market power then determines what fraction of the rent go to the bank and what fraction goes to the borrower. Our finding is that in this arms length setting, the local market power channel leads to a significant diversion of rents from firms to banks.

In addition, our paper is related to the literature that examines the effect of distance on corporate bank lending. In an important analysis of spatial price discrimination in banking, Degryse and Ongena (2005) use data from a large Belgian bank and, similar to our paper, find that interest rates are higher for borrowers located closer to lenders. They further document that the effect seems to be driven by banks' market power rather than monitoring. Other papers which document an increase of interest rates with higher distance include Petersen and Rajan (2002) and Agarwal and Hauswald (2010). Two recent papers on the other hand document reverse results for large syndicated US loans (Knyazeva and Knyazeva, 2012) and small Italian firms (Bellucci et al., 2013). Both papers conclude that loan spreads increase in firm-bank distance and decrease in the distance between firms and competing banks. Similarly, Gambacorta and Mistrulli (2014) find that while interest rates decrease in a firm's distance to its bank branch, it increases in its distance to that bank's headquarter. A number of recent papers have found additional benefits of close physical distance between borrowers and lenders: Hollander and Verriest (2016) find that banks located closer to borrowers set less restrictive covenants than those located further away. Degryse et al. (2015) document that UK borrowers of bank branches located close to bank headquarters had greater access to credit during the financial crisis. We believe that our paper can help resolve those contradictions of the earlier literature since we can address the endogenous nature of distance and location choice. For the first time, we have assembled a dataset which features a time series, rather than a cross-section of distances, as well as the whole market of corporate bank loans. In conjunction with the significant infrastructure changes in Norway this allows us to obtain an arguably causal estimate of the impact of distance on lending.

The result that close distance causally influences the matching of borrowers and banks confirms earlier results which document a cross sectional correlation between distance and the existence of banking relationships (Beck et al., 2016). Similarly, Ono and Uesugi (2016) find that firms are more likely to initiate a new relationship to a closer bank branch after a merger-induced branch closing. We add to this literature by providing the first causal evidence of the link between the establishment of banking relationships and distance.

2 Data

2.1 Data Sources

2.1.1 Data on Bank Accounts

Yearly corporate bank account information comes from the Norwegian Tax Administration.[2] The dataset contains detailed information on the end-of-year balance separately of all bank deposit and 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.

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. The dataset includes for each individual account the following data items: the account number; the name of the account holder (firm) and its organization number (a unique firm identifier); the deposit or loan balance as of 31 December; 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. The reporting bank is identified with its name and organization number.

2.1.2 Data on Firm Account Information and Firm Location

We link the bank account information to a database on firm information using a government provided, unique firm identification number. This database covers all

[2] 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.

Norwegian private and public companies for the period 1993-2012. Norwegian firms are required to have an authorized auditor.[3] 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.[4] The accounting year is required to follow the calender year. The accounting database includes the complete profit and loss account, 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. The database is further described in Berner et al. (2015).

2.1.3 Data on Bank Branch Locations

Finans Norge (Finance Norway), the financial sector's industry organization, maintains a national bank branch register. This list includes branch addresses of all bank branch offices in Norway and is updated annually. Each branch is linked to each bank as identified in the bank account and financial account databases. We use the registries for the years 2005-2012. The address information is however not always complete. In many 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.

2.2 Definition of Variables

2.2.1 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

[3]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.

[4]More information is provided at www.brreg.no.

feature historic maps, we employ several outdated vintages of Microsoft MapPoint. Norwegian maps are included in MapPoint starting with the MapPoint 2006 version. We 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 a single loan account of the firm with the bank in both years, and if the driving time between the two addresses has been reduced 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.

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 six minutes, significantly above the one minute cutoff.

[5] http://support2.microsoft.com/lifecycle/search/default.aspx?sort=PN&alpha=MapPoint&Filter=FilterNO, retrieved November 2014

[TABLE 2 here]

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]

2.2.2 Choice of treatment cutoff

A key decision for our paper is the definition of treatment. Since the calculation of driving time is noisy, and since very small reductions in driving time are unlikely to result insignificant changes in interest rates, we decide to ignore reducitons 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 two minutes and at least 10% of the original driving distance. For the median firm in our sample, driving time to their bank is 20 minutes, hence two minutes corresponds to 10% for our median firm. The general trade-off in increasing the treatment definition is that higher treatment sizes exclude large fractions of the sample from being potentially ever treated. A cutoff of five minutes, for example, corresponds to more than 50% of driving time for the 25th percentile firm in our sample. Such vast changes in relative driving time occur very infrequently (Figure 1).

In addition, we survey the literature on distance and interest rates in Table 3. 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, which we mention in 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, and if available median distance 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 a two minute reduction in traveling time. Thus, two minutes merely forms the *lower bound* of our treatment definition, i.e. we do not consider as treatment reductions of less than two minutes - the actual average treatment size is, however, three times this number at six minutes.

[TABLE 3 here]

Table 3 illustrates that even a relatively small change in driving time of two minutes is associated with significant changes in interest rates. The implied impact of a two minute change in driving time ranges from an increase in rates by 24 basis points to a drop by 8 basis points. Therefore, even an observation at the lower bound of our treatment definition should exhibit a significant reaction in interest rates.

Below, we conduct a number of exercises to check our results' robustness to variations in treatment.

2.2.3 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 2.1.1, 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 3.05% and 18.13%.

2.2.4 Measuring Other Variables

Other variables are defined in Table 4.

[TABLE 4 here]

2.2.5 Summary Statistics

We find an average driving time of 17 minutes between firms and their relationship banks. This translates to roughly 5.5 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 seven minutes than the mean distances of 17 minutes.

[TABLE 5 here]

3 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,

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

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 overcome this challenge by matching control firms to specific treatment firms in the year of treatment in Section 5.2.1.

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. 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.

There are a number of possible issues with this identification strategy which we will address below: 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.

[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.

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 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 firms. Firms might move both if their old location receives more or less infrastructure investment: Retails 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.

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

4 Results

4.1 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 eight minutes in both the mean and median driving time. The 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.

Table 6 tests this conjecture. 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 $Initiation_{fbt}$, an indicator that takes the value of one if firm f has taken out a loan from bank b 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 $Initiation_{fbt}$ as the dependent variable. The main explanatory variable is *Reduction by* 2 *minutes*, an indicator which takes the value one if the driving distance between firm f and bank b fell by at least two minutes and 10% of the value at the previous MapPoint version.[9]

[TABLE 6 about here]

[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 six minutes.

Column 1 presents the results of a regression with just controls for firm fixed effects, year fixed effects and municipality year fixed effects to control for firm, time and regional specific characteristics which might impact firm-bank matching. The estimated coefficient is 1.68, and highly statistically significant. The linear probability model 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 1.68%.[10]

Column 2 adds controls for time varying firm characteristics such as firm size (sales), firm opacity (intangibles/assets) and firm leverage. 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. The estimated impact of the number of banks in the vicinity is negative and insignificant.

Finally, Column 4 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

[10]In unreported results we repeat the analyses in Tables 6 using probit regressions instead. All results retain their economic and statistical significance.

and the number of available banks is -0.02 and statistically significantly different form 0 at the 1% level.

Taken together, Table 6 shows that a reduction in driving time increases the likelihood of initiating a borrowing relationship. The effect remains robust to the introduction of various controls. We show robustness with respect to the threshold in Section 5.1.

4.2 Loan Contract Terms

We turn to the impact of a reduction of travel time on existing lending relationships. 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 begin our analysis on a firm-year-bank level. This allows us to precisely link distance changes between a specific firm and bank branch to the corresponding accounts. In particular, it allows us to introduce firm-bank relationship dummies which capture unobserved variation of certain relationships. For example, a loan officer possesses a personal relationship with management.

A firm's total cost of borrowing include both the actual loan costs (interest rates and fees) as well as the transaction costs borne by the firm. A reduction in travel time directly reduces transaction costs. The market power hypothesis predicts that a reduction in travel time creates a rent through lower transaction and monitoring costs. This rent can then be captured by the bank through an increase in the price of credit, i.e. the cost of borrowing. Formally this means that:

$$T(Distance_{b_i t}) + Lr_{b_i t} \leq T(Distance_{b_j t}) + Lr_{b_j t} \; \forall j \in \mathbf{B}, \tag{2}$$

where $T(Distance_{b_i t})$ denotes transportation costs as a function of the distance between the firm and bank i, L is the loan size desired and $r_{b_i t}$ the interest rate on the loan. **B** is the set of all competing banks j different from i. The total cost of borrowing from the relationship bank i must be lower than the total cost of any competing bank. Consider for simplicity that only the driving time to the relationship bank is reduced. Since this relationship must hold equally after a change to driving time and since the cost of borrowing is strictly positive, it follows that:

$$T(Distance_{b_i t}) - T(Distance_{b_i t+1}) \leq Lr_{b_i t+1} - Lr_{b_i t} \tag{3}$$

The rent that can be extracted by the bank can not exceed the benefits derived by the borrower from the lower distance. For illustration consider a reduction in driving time by 7.5 minutes (one way). If the entrepreneur visits the bank once each quarter, the resulting saving in time equals one hour per year. Assuming opportunity costs of \$200 per hour, the increase in interest rates cannot raise annual interest payments by more than \$200.

We report the results of regressions of interest rates on changes in travel time in Table 7.

[TABLE 7 about here]

We regress interest rates on the treatment dummy with just individual municipality and year fixed effects but not their joint effect in Column 1. A firm-bank relationship is treated if driving time has decreased by at least 10% over the sample period. As before we exclude reductions in driving time of less than two minutes. After the treated firms get treated, the interest rate rises by 10 basis points on average. The treatment group exhibits on average 18 basis points lower interest rates than the control group, a difference that is statistically significant at the 1% level. We then proceed to add controls for local economic development in the form of joint municipality-year fixed effects in Column 2. The estimated impact of treatment is an increase in rates by 0.14 basis points, significant at the 1% level. We proceed to add further loan specific in Column 3. The coefficient on relationship duration is negative and significant. Once firm age is controlled for in Column 5, the estimated coefficient on relationship duration turns positive and significant. The cross sectional coefficient on distance to the relationship bank is -0.14 and statistically significant at the 1% 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 -0.02 and statistically insignificant. 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 coefficient is negative but insignificant, a result lying somewhere between the two findings.

Column 4 adds further firm specific controls. The estimated impact of treatment on interest rates remains at 15 basis points while the estimated pre-treatment difference between treatment and control firms shrinks to 9 basis points. The negative and significant point estimates on firm age and -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 and rating fixed effects as well as both firm- and loan specific controls. The estimated treatment effect is 14 basis points, and statistically significant at the 1% level.

At a median loan size of NOK 1.096 million, the coefficient of 14 basis points translates into annual expenses of NOK 1,534, or roughly $185 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 six minutes (one way). Assuming quarterly visits to the bank, this yields annual time savings of 48 minutes which implies hourly opportunity cost of about $231 for the owner. The estimated opportunity cost is a reasonable number 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. In addition there are smaller savings from "shoe leather costs".[11]

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 six minutes and the resulting increase in interest rates is 14 basis points.

In a standard monopoly setting, the monopolist increases the price of the product compared to the market clearing price to extract rents. The price discrimination hypothesis therefore predicts that the increased interest rate is accompanied by a reduction in loan size (Hoover, 1937). In Table 8 we therefore proceed to test the second implication of monopoly pricing. For bank loans, we expect smaller loan sizes given the rising interest rates.

[TABLE 8 about here]

Table 8 repeats the analysis from Table 7, with the difference being that the dependent variable is loan size rather than interest rates. The results confirm that treatment is

[11] We disregard the effects of tolls commonly applied to new infrastructure developments. If anything tolls wold reduce the economic effects of reduced driving distances.

indeed associated with reduced loan size: The estimated coefficient on *Treatment X Post* ranges between -0.15 and -0.20 percent of assets among the various specifications, and is statistically significant at the 1% level. At the median loan size of about 1.08 Million NOK the corresponding nominal reduction in loansize is NOK 2,160, or about $270. While this is an economically insignificant reduction, the monitoring hypothesis would predict an *increase* in loan size, which makes the statistically significant reduction in size meaningful.

This section therefore finds strong evidence in favor of the market power hypothesis. A reduction in travel time between firms and banks is associated with a significant increase in interest rates and lower loan amounts.

4.3 Market Power or Monitoring?

Our previous results are strong evidence for the existence of the local market power channel of distance: When distance between borrowers and firms is exogenously reduced, market power of relationship banks increases and they extract at least part of the associated savings in travel time through higher rates. In this section, we investigate whether there are any cross sectional differences in treatment between firms.

First, 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. We measure market power through two proxies: First, 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). Similarly, firms with better credit ratings pose less of a risk and might have an easier time switching to other lenders.

We therefore use the duration of a firm's relationship with its bank as well as its credit rating as proxies for the bank's negotiation power.

Second, we address the fact that our prior evidence of the market power channel does not rule out the existence of the monitoring channel. Both channels might coexist and our finding might be evidence that the effects of market power simply dominate those of monitoring. To answer this question, we investigate whether treatment effects are larger for firms which require more monitoring: Smaller firms and those with a higher fraction of intangibles over assets.

Table 9 shows the results.

[TABLE 9 about here]

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*) . The market power hypothesis 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). The local market power hypothesis therefore predicts that banks with long lending relationships should increase interest rates more after a reduction in distance. Indeed Column 1 of Table 9 shows that the interaction between treatment, post-project and the length of the relationship is positive and significant at the 5% 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 borrowers 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 significantly smaller for firms with a good credit rating. This results again confirms the prediction by the market power hypothesis that when banks have larger negotiation strength, they will capture larger parts of the rents generated by a reduction in driving time.

The result in Column 2 is not just evidence in favor of the market power hypothesis, but also evidence against the monitoring hypothesis: Firms with bad credit ratings might need more monitoring and hence should arguably profit more from closer distance to banks. The fact that we find the exact opposite effect, i.e. that firms with worse credit ratings experience a sharper *increase* in rates is strong evidence against a monitoring channel.

While the results of Columns 1 and 2 in Table 9 further strengthen the market power hypothesis, Columns 3 and 4 conduct additional test for the existence of a monitoring channel. First, we test whether larger firms experience smaller treatment effects. Under Norwegian law, larger firms are required to produce more complete financial reports and face stronger requirements regarding auditing. Therefore small firms might require more monitoring and should profit more from a reduction in traveling time. The corresponding interaction term between treatment and firm size should therefore be positive. The point estimate reported in Column 3 is indeed negative at -0.01, however statistically insignificant.

Finally, Column 4 reports results from including the interaction between treatment and the ratio of intangibles among firms' assets. As firms with less tangible assets might require more monitoring, the monitoring hypothesis predicts that treatment should be associated with lower interest rates for firms with more intangible assets. The estimated coefficient on the interaction of treatment and intangible assets presented in Column 4 of Table 9 is indeed negative, albeit again statistically insignificant.

Overall, the results in Table 9 on the cross sectional variation in treatment effects provide evidence in favor of the market power hypothesis and little to none evidence in favor of a monitoring channel.

5 Additional Tests

5.1 Relationship Initiation and Treatment Magnitude

One possible concern with the preceding analysis might be that the treatment threshold of two minutes is low and somewhat arbitrary. We therefore proceed to re-estimate the final two specifications in Columns 3 and 4 of Table 6 using two larger treatment cutoffs: The variable *Reduction by* 5 *minutes* takes the value of one if the driving time between firms and banks was reduced by at least five minutes in between two distance calculations. The variable *Reduction by* 7.5 *minutes* is defined similarly with a cutoff of seven and a half minutes. Table 11 displays the results of this analysis.

[TABLE 11 about here]

The estimated treatment effect for a reduction in driving time by at least five minutes is 1.92 and statistically significant at the 1% level. The economic magnitude is significantly larger than in the case of a two minute treatment cutoff, which had an estimated coefficient of 1.52 in Column 3 of Table 6. Once the interaction with the number of available banks is added, the point estimate on treatment increases further to 4.21, and the corresponding point estimate on the interaction term is -0.03, both values are larger in absolute size than in the case of a two minute treatment cutoff.

Columns 3 and 4 repeat these regressions using a seven and a half minute treatment cutoff. As before, the point estimates on treatment (4.50) and its interaction with the number of available banks (-0.03) are economically larger then those for treatment using a 5 minute cutoff and those for the two minute treatment cutoff. These results suggest that the estimated treatment effects in Table 6 increase in magnitude as treatment increases in magnitude.

5.2 Interest Rates and Treatment Magnitude

We also investigate whether our results concerning the interest rate depend on the definition of treatment. Table 12 presents results from running the most complete specification in Column 4 of Table 7 with various changes in the treatment definition:

[TABLE 12 about here]

The point estimate for the interaction of treatment group and post-project in Column 4 of Table 7 is 14 basis points. When the treatment cutoff is lowered to one minute, the coefficient decreases to 12 basis points as displayed in Column 1 of Table 12. The estimate is statistically significantly different from zero at the 1% level. However, when the bar for treatment is increased to five minutes, the resulting point estimate shrinks to just 8 basis points in Column 2. The estimated effect is both smaller in magnitude than those for the one and two minute definitions and statistically insignificant. Once treatment magnitude is increased further to ten minutes (Column 3), the point estimate increases to 19 basis points, significant at the 10% 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 36 basis points, while for 20 minutes it is 37 basis points. Both estimates are statistically significantly different from zero at the 5% level.

Table 12 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 with the increase in the underlying change in driving time from 12 basis points for treatments of at least one minute, to 37 basis points for the largest treatment definition of a reduction of twenty minutes: 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.

5.2.1 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. Assume for example a case in which two firms are treated in 2008 and 2010, respectively. Untreated firms in the same region would then be in the "post" group for the first firm in 2008, but in the "pre" group for the second firm. Since we therefore cannot reliably assign control firms into a post treatment period, our earlier analysis abstained from estimating the "post" effect separately.

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 Drukker (2004) 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 4 of Table 7.

In a next step, we plot the development of interest rates for treatment and control groups to visually confirm that they exhibit parallel trends. 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 4 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.

[Figure 2 about here]

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*

[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.

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 14 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 13 confirms the visual impression:

[TABLE 13 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 -32 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 33 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 4. Column 2 adds industry and municipality year fixed effects. The estimated treatment effect shrinks to 20 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 20 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 4 of Table 7 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 this section confirm our findings from 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.

5.3 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 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. 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 10 about here]

Table 10 shows the results of a difference in differences regression analogous to those in Table 13. 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 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 1% 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 (Column 4 in Tables 7 and 10) are 0.14% 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 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 12 reports estimated treatment effects of 0.36% and 0.37% 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.

6 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 two theories predict opposing impacts of distance on pricing and size of credit: The monitoring hypothesis predicts interest rates to rise as distance increases since asymmetric information issues worsens. The local market power view, on the other hand, predicts interest rates to fall in distance as banks enjoy less of a monopoly position. 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.

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Appendix for
The causal impact of proximity 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 post code 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 post code areas which can be reached from the firm's post code 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

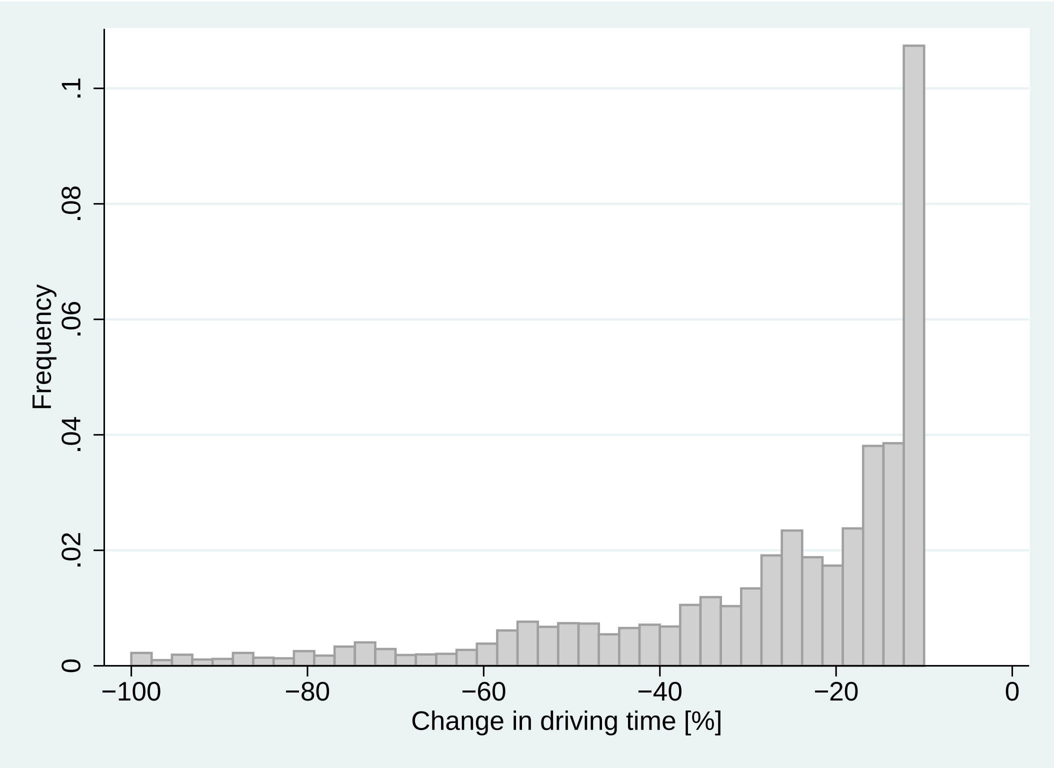

Figure 1: Frequency of driving time reductions
This figure plots the frequency of relative driving time reductions in percent for treated observations for the MapPoint sample.

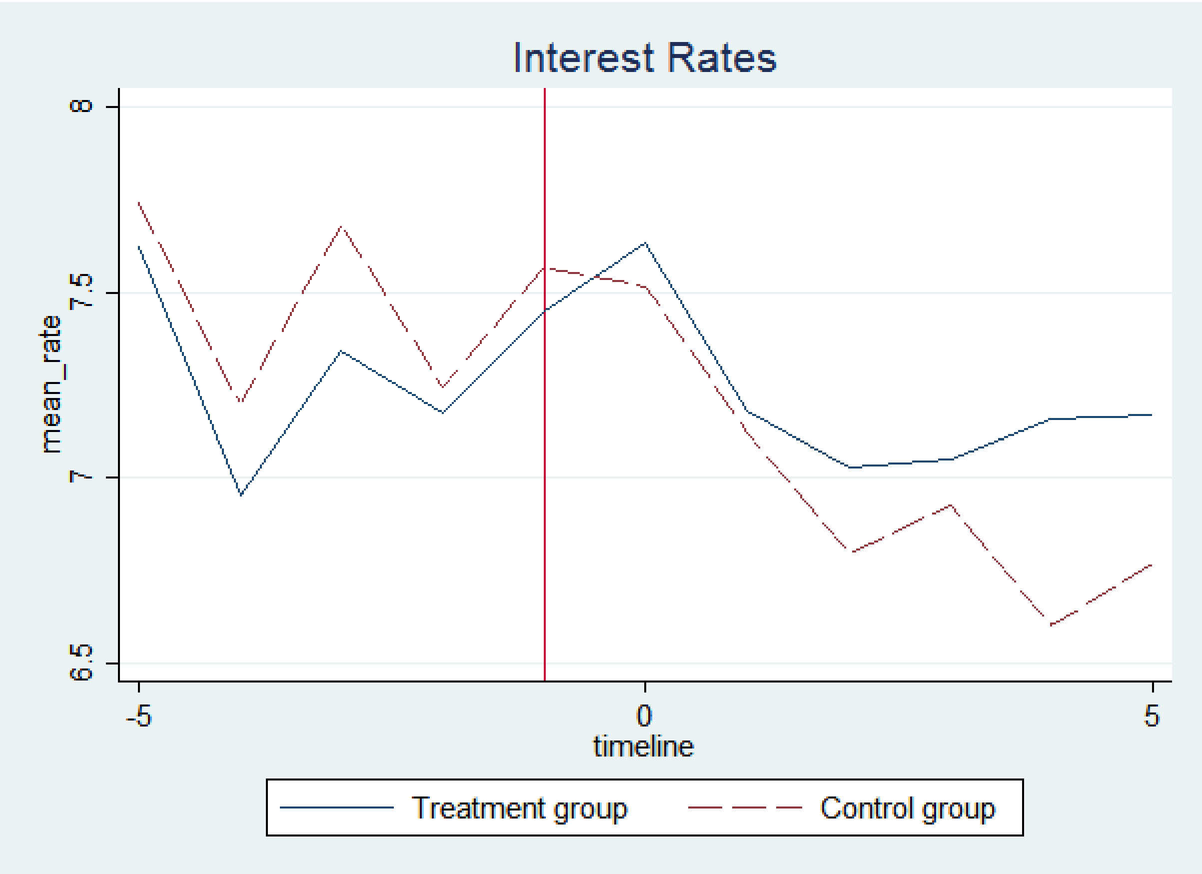

Figure 2: Parallel Trends: MapPoint Sample
This figure plots the mean interest rate of treatment and control firms from the matched MapPoint sample against time to treatment.

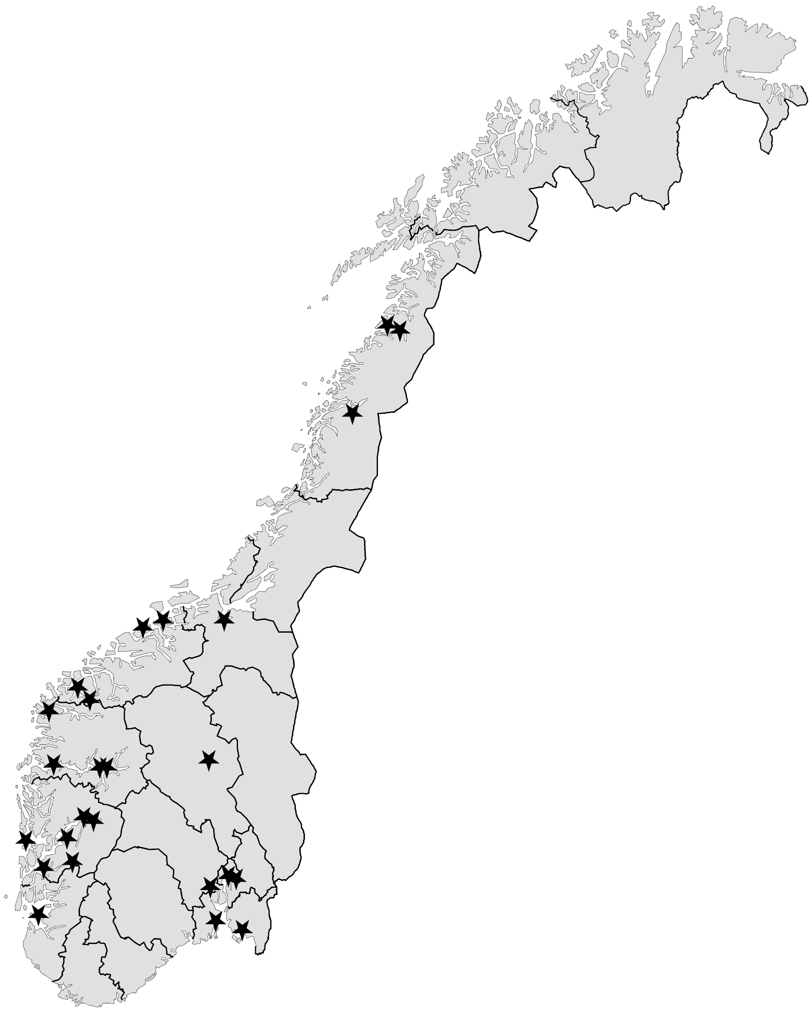

Figure 3: Map of Norway and location of large infrastructure projects
This figure shows the geography of Norway and the location of the large infrastructure projects used in the Big Project Sample.

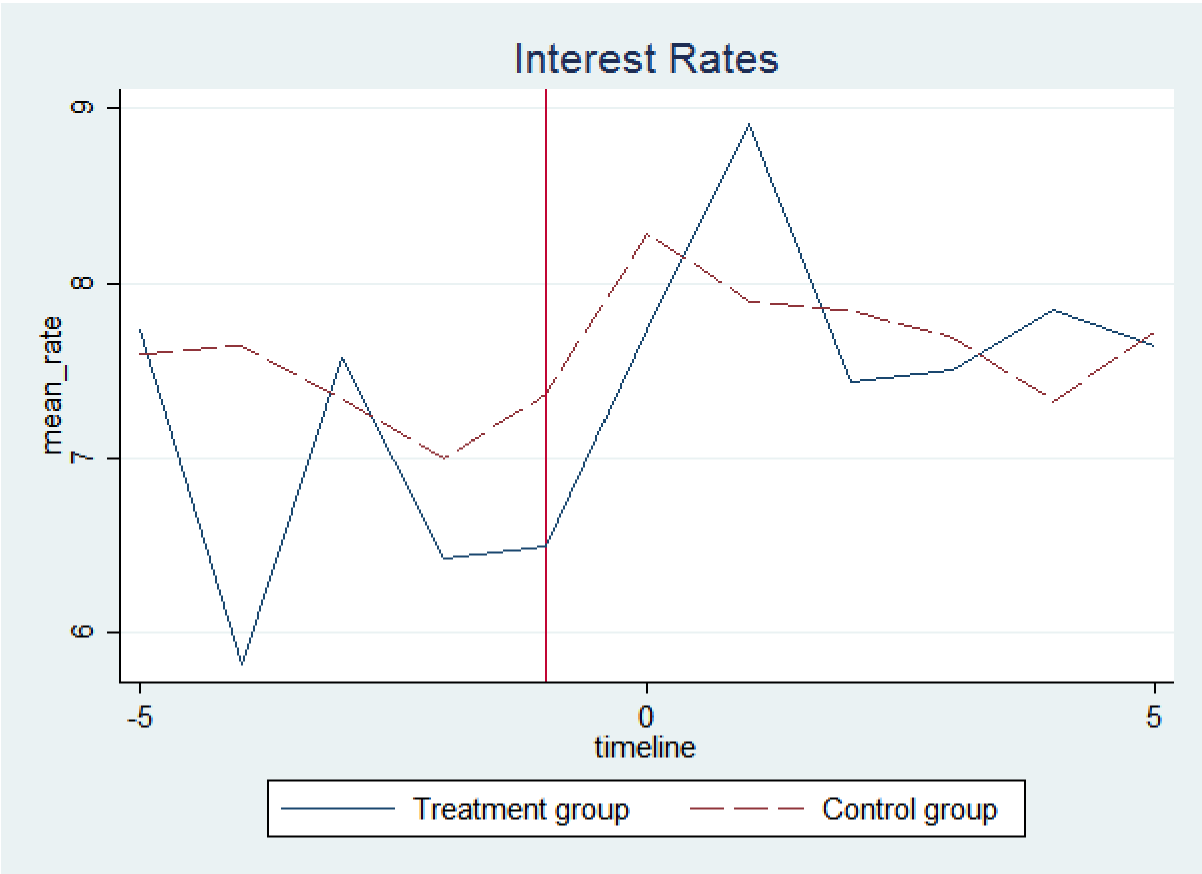

Figure 4: Parallel Trends: Big Projects
This figure plots the mean interest rate of treatment and control firms from the big project sample against time to treatment.

Table 1: Map Sources and Map Vintages

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 more than two minutes.

Variable	Mean	SD	P25	Median	P75	Min	Max	N
Driving Time Pre-Treatment [in h]	0.51	0.49	0.14	0.33	0.74	0.02	3.00	3391
Driving Time Post-Treatment [in h]	0.40	0.42	0.10	0.25	0.55	0.00	2.54	3391
Change in Driving Time [in h]	-0.11	0.14	-0.14	-0.06	-0.04	-1.00	-0.02	3391
Change in Driving Time [in %]	-27.58	19.68	-35.32	-20.03	-12.55	-100.00	-10.00	3391

Table 3: 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 (median) of distance and the estimated effect of a reduction in distance, it provides a standardized measure of the implied cross sectional relationship between a 2 minute reduction in driving time and interest rates.

Paper	Country	Mean (median) dist.	Estimated effect	Red. 2 Min. effect[a]
Petersen and Rajan (2002)	USA	115 (9) Miles	-37bp/mile[a]	+24bp
Degryse and Ongena (2005)	Belgium	6.9 (4.29) Min.	-18bp/mile[a]	+12bp
Agarwal and Hauswald (2010)	USA	10.25 (≈7.5) Min.	-15bp/mile[a]	+10bp
Knyazeva and Knyazeva (2012)	USA	744 (581) Miles	+1bp/mile[c]	-2bp
Bellucci et al. (2013)	Italy	4.98 (NA) Km	+6.7bp/km	-7.8bp
Our findings	Norway	16.8 (7.2) Min.	-2.12/Min.[d]	+4.24bp

[a] Cross-sectional effect of a *reduction* in driving time by 2 minutes assuming speed of 20 mph (35 kmh)
[b] Calculation from Degryse et al. (2009)
[c] Effect for a one standard deviation increase in log distance starting as neighbor from distance 0
[d] Treatment effect of 14 bp and average treatment size of 6.6 minutes

Table 4: Variable Definitions

Variable	Units	Definition
Age	Years	Firm age since founding.
Assets	NOK ’000	Total assets (both fixed and current).
Distance Relationship	Hours	Average driving time between the firm’s address and it’s loan relationship banks’ addresses. Calculated for the fastest driving connection.
Distance Banks Without Relationships	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.
Intangibles / Assets	%	Intangible total assets composed of activated R&D expenditure, patents, goodwill and deferred tax assets.
Interest Rate	%	Interest rate, calculated as the fraction of interest accrued over loan amount outstanding at year end.
Loan Size	NOK ’000	End of year balance on loan account.
Loan Size / Assets	%	End of year balance on loan account over total assets.
Number of relationship banks		Number of different banks with which firm has at least one loan account for each year.
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 2 minutes.
Industry Fixed Effects	Binary	Indicator indicating one of twelve different industries (data item bransjegr_07)
Municipality & Year Fixed Effects	Binary	Cross dummy for each of the 120 commuter regions and 7 sample years (2006-2012)

Table 5: 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
Assets	21727	45503	2958	6208	16036	100045	25064	7209	9626	21372	6105	90419
Distance Relationship Banks	0.28	0.37	0.06	0.12	0.34	100045	0.37	0.22	9626	0.27	0.11	90419
Distance Banks without Relationship	0.45	0.47	0.11	0.27	0.66	100045	0.59	0.47	9626	0.44	0.25	90419
Duration Relationship Banks	5	2	4	7	7	100045	6	7	9626	5	7	90419
Loan Interest Rate	7.61	3.03	5.36	7.01	9.14	81194	7.57	7.04	7782	7.61	7.01	73412
Loan Size	10859	101695	321	1096	3482	100045	15448	1247	9626	10371	1081	90419
Loan Size/Assets	0.26	0.23	0.07	0.21	0.40	100045	0.26	0.21	9626	0.26	0.21	90419
Number of Relationship Banks	1.07	0.28	1.00	1.00	1.00	100045	1.09	1.00	9626	1.07	1.00	90419
Sales	27966	50286	4771	10407	25724	100045	30570	11779	9626	27688	10278	90419

Table 6: 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)
Reduction by 2 min	1.68***	1.68***	1.52***	3.34***
	(12.68)	(12.68)	(7.61)	(4.87)
Reduction by 2 min X number banks				-0.02***
				(-2.83)
Sales (logs)		0.03***	0.04*	0.04*
		(2.83)	(1.71)	(1.72)
Intangibles/Assets		-0.70**	-1.14	-1.13
		(-1.97)	(-1.39)	(-1.38)
Bookleverage		0.28***	0.25	0.25
		(3.51)	(1.23)	(1.23)
Distance Relationship Banks			2.08***	2.08***
			(9.96)	(9.97)
Distance Banks w/o Relationship			2.09***	2.09***
			(15.01)	(15.00)
Number banks			-0.04	-0.02
			(-0.00)	(-0.00)
N	1286557	1286420	479705	479705
adj. R^2	0.007	0.007	0.021	0.022
Firm Fixed Effects	Yes	Yes	Yes	Yes
Bank Fixed Effects	No	No	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Municipality&Year Fixed Effects	Yes	Yes	Yes	Yes

Table 7: 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 4). 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 Rates				
	(1)	(2)	(3)	(4)	(5)
Treatment X Post	0.10*	0.14***	0.15***	0.15***	0.14***
	(1.96)	(2.86)	(3.10)	(3.16)	(2.98)
Treatment	-0.18***	-0.13***	-0.11***	-0.09**	-0.08**
	(-3.44)	(-3.19)	(-2.76)	(-2.36)	(-2.03)
Distance Relationsship Banks			-0.14***		-0.13**
			(-2.64)		(-2.56)
Distance Banks w/o Relationship			-0.02		-0.03
			(-0.35)		(-0.63)
Duration relationship			-0.03***		0.03***
			(-5.62)		(5.26)
Age				-0.01***	-0.01***
				(-11.20)	(-12.75)
Sales (logs)				-0.05***	-0.05***
				(-8.64)	(-8.81)
Intangibles/Assets				0.10	0.12
				(0.59)	(0.74)
N	61261	61263	61263	61263	61263
adj. R^2	0.139	0.123	0.123	0.158	0.159
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes
Municipality & Year Effects	No	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No	No	No
Municipality Fixed Effects	Yes	No	No	No	No
Rating Fixed Effects	No	No	No	Yes	Yes

Table 8: 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 4). 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	-0.15***	-0.16***	-0.19***	-0.19***	-0.20***
	(-4.36)	(-4.69)	(-5.33)	(-5.78)	(-5.96)
Treatment	0.31***	0.28***	0.27***	0.24***	0.24***
	(7.61)	(12.60)	(12.25)	(11.75)	(11.39)
Distance Relationsship Banks			0.14***		0.12***
			(4.48)		(4.19)
Distance Banks w/o Relationship			-0.17***		-0.09***
			(-4.97)		(-2.86)
Duration relationship			0.06***		0.03***
			(17.46)		(6.39)
Age				0.02***	0.01***
				(33.37)	(25.07)
Sales (logs)				0.16***	0.16***
				(18.33)	(18.26)
Intangibles/Assets				0.72***	0.74***
				(7.19)	(7.30)
N	63218	63223	63223	63223	63223
adj. R^2	0.144	0.124	0.129	0.177	0.177
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes
Municipality & Year Effects	No	Yes	Yes	Yes	Yes
Rating Fixed Effects	No	No	No	Yes	Yes

Table 9: 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 4). 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.

	(1)	(2)	(3)	(4)
Treatment	-0.08**	-0.08**	-0.08**	-0.08**
	(-2.02)	(-2.02)	(-2.04)	(-2.03)
Treatment X Post	-0.18	0.37***	0.21*	0.15***
	(-1.17)	(3.11)	(1.83)	(3.11)
Treatment X Post X Duration relationship	0.05**			
	(2.27)			
Treatment X Post X Rating quality		-0.06**		
		(-2.11)		
Treatment X Post X Sales (logs)			-0.01	
			(-0.66)	
Treatment X Post X Intangibles/Assets				-0.37
				(-0.75)
Distance Relationsship Banks	-0.13**	-0.13***	-0.13**	-0.13**
	(-2.55)	(-2.60)	(-2.56)	(-2.55)
Distance Banks w/o Relationship	-0.03	-0.03	-0.03	-0.03
	(-0.64)	(-0.52)	(-0.63)	(-0.63)
Duration relationship	0.03***	0.03***	0.03***	0.03***
	(4.58)	(4.85)	(5.26)	(5.29)
Age	-0.01***	-0.01***	-0.01***	-0.01***
	(-12.78)	(-13.11)	(-12.76)	(-12.75)
Sales (logs)	-0.05***	-0.05***	-0.05***	-0.05***
	(-8.82)	(-9.10)	(-8.76)	(-8.81)
Intangibles/Assets	0.11	0.33**	0.12	0.15
	(0.71)	(2.04)	(0.75)	(0.88)
N	61263	61263	61263	61263
adj. R^2	0.159	0.153	0.159	0.159
Industry Fixed Effects	Yes	Yes	Yes	Yes
Municipality & Year Effects	Yes	Yes	Yes	Yes
Rating Fixed Effects	Yes	Yes	Yes	Yes

Table 10: 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 4). 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 Rates			
	(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 Relationsship Banks		-0.07		-0.04
		(-0.32)		(-0.21)
Distance Banks w/o Relationship		-0.55*		-0.56*
		(-1.69)		(-1.78)
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

Table 11: Relationship Initiation and treatment magnitude
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)
Reduction by 5 min	1.92***	4.21***		
	(7.02)	(4.36)		
Reduction by 5 min X Number banks		-0.03**		
		(-2.54)		
Reduction by 7.5 min			2.18***	4.50***
			(5.96)	3.69
Reduction by 7.5 min X Number banks				- 0.03**
				(-2.06)
Sales (logs)	0.04*	0.04*	0.04*	0.04*
	(1.72)	(1.73)	(1.71)	(1.72)
Intangibles/Assets	-1.15	-1.14	-1.13	-1.13
	(-1.40)	(-1.40)	(-1.38)	(-1.38)
Bookleverage	0.25	0.25	0.25	0.25
	(1.25)	(1.25)	(1.26)	(1.25)
Distance rel banks	2.08***	2.08***	2.07***	2.08***
	(9.95)	(9.96)	(9.94)	(9.95)
Distance Banks w/o Relationship	2.09***	2.09***	2.09***	2.09***
	(14.97)	(14.98)	(14.97)	(14.97)
Number banks	-0.03	-0.01	-0.03	-0.05
	(-0.00)	(-0.00)	(-0.00)	(-0.00)
N	479705	479705	479705	479705
adj. R^2	0.021	0.022	0.021	0.021
Firm Fixed Effects	Yes	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Municipality&Year Fixed Effects	Yes	Yes	Yes	Yes

Table 12: 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 min" 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 4). 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 Rates				
	(1)	(2)	(3)	(4)	(5)
Treatment 1 min X Post	0.12***				
	(2.65)				
Treatment 1 min	-0.11***				
	(-3.16)				
Treatment 5 min X Post		0.08			
		(1.43)			
Treatment 5 min		-0.12***			
		(-2.67)			
Treatment 10 min X Post			0.19*		
			(1.73)		
Treatment 10 min			-0.21***		
			(-2.87)		
Treatment 15 min X Post				0.36**	
				(2.45)	
Treatment 15 min				-0.15	
				(-1.49)	
Treatment 20 min X Post					0.37**
					(2.45)
Treatment 20 min					-0.32***
					(-3.18)
Distance Banks w/o Relationship	-0.03	-0.03	-0.03	-0.04	-0.03
	(-0.51)	(-0.57)	(-0.54)	(-0.71)	(-0.59)
Duration relationship	0.03***	0.03***	0.03***	0.03***	0.03***
	(5.33)	(5.36)	(5.35)	(5.33)	(5.34)
Distance Relationsship Banks	-0.12**	-0.11**	-0.12**	-0.13**	-0.12**
	(-2.49)	(-2.23)	(-2.36)	(-2.54)	(-2.45)
Age	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***
	(-12.59)	(-12.73)	(-12.76)	(-12.79)	(-12.81)
Sales (logs)	-0.05***	-0.05***	-0.05***	-0.05***	-0.05***
	(-8.77)	(-8.78)	(-8.78)	(-8.81)	(-8.80)
Intangibles/Assets	0.12	0.12	0.12	0.12	0.12
	(0.74)	(0.74)	(0.72)	(0.75)	(0.74)
N	61263	61263	61263	61263	61263
adj. R^2	0.159	0.159	0.159	0.159	0.159
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes
Municipality & Year Effects	Yes	Yes	Yes	Yes	Yes
Rating Fixed Effects	Yes	Yes	Yes	Yes	Yes

Table 13: 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 4). 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 Rates				
	(1)	(2)	(3)	(4)	(5)
Treatment X Post	0.33***	0.20**	0.20**	0.17**	0.17**
	(3.60)	(2.45)	(2.47)	(2.11)	(2.14)
Treatment	-0.16**	-0.12**	-0.12**	-0.13**	-0.13**
	(-2.51)	(-2.02)	(-2.06)	(-2.14)	(-2.16)
Post	-0.32***	-0.31***	-0.29***	-0.28***	-0.28***
	(-3.61)	(-4.00)	(-3.77)	(-3.62)	(-3.64)
Distance Banks w/o Relationship			-0.14		-0.20
			(-1.11)		(-1.54)
Distance Relationsship Banks			0.19*		0.22**
			(1.89)		(2.24)
Duration relationship			-0.02*	0.02*	0.02*
			(-1.83)	(1.67)	(1.80)
Age				-0.01***	-0.01***
				(-3.26)	(-3.31)
Sales (logs)				-0.08***	-0.08***
				(-7.04)	(-7.07)
Intangibles/Assets				-0.39	-0.40
				(-1.25)	(-1.29)
N	13109	12960	12960	12960	12960
adj. R^2	0.002	0.162	0.162	0.198	0.199
Industry Fixed Effects	No	Yes	Yes	Yes	Yes
Municipality & Year Effects	No	Yes	Yes	Yes	Yes
Rating Fixed Effects	No	No	No	Yes	Yes