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URBAN AGGLOMERATIONS AND FIRMS' ACCESS TO CREDIT

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Abstract

The paper investigates whether firms have better access to bank credit in areas with a larger degree of urbanization. It uses bank-firm data drawn from the Credit Register maintained at the Bank of Italy to devise an indicator of ease of access to credit. The paper proposes an instrumental variable strategy that uses as instruments past population density and urbanization driven by considerations of political economy. The results show that urbanization affects access to credit positively for construction firms, whose collateral greatly benefits from thicker real estate markets. No impact is found for service and manufacturing firms.

JEL Classification: G21, R11, R51.
Keywords: urbanization, access to credit.

Contents

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1. Introduction[1]

We investigate whether urban agglomeration favors access to credit for firms. This may be a very relevant issue for shedding light on the economic advantages of cities. To the extent that firms located in urban markets benefit from larger financing possibilities, part of the benefits related to cities should be associated with the credit market, rather than the goods and the labor markets that have attracted the greatest attention within the literature. In contrast, if credit access is more difficult in urban centers, the advantages of urbanity might even be larger than those so far documented, as part of the returns are reaped by the local banking system.

The relationship between cities and credit availability is rather complicated. In principle, there are several channels through which urban agglomeration could impact access to credit. Some channels suggest an easier access to finance; others, on the contrary, hint that financing possibilities might be precluded in urban environments. Therefore, the sign of the net impact remains uncertain. Firstly, firms operating in urban areas are generally more productive (Rosenthal and Strange, 2001, among others). The productivity premium might be due to agglomeration economies; that is, improved access to inputs, labor market pooling, and knowledge spillovers (Holmes, 1999; Rosenthal and Strange, 2001; Ellison et al., 2010; Combes and Gobillon, 2015). However, the productivity advantages (Combes et al., 2012) might also be related to greater firm selection, as competition is tougher in cities and only the most productive firms survive. Depending on which mechanism prevails, urbanity could imply more or less credit access. For instance, in the case of selection banks will not be happy to finance firms that can be pushed out of the market by the fierceness of urban competition. Secondly, information gathering might be affected by the degree of urbanization (Kravchenko et al., 2013). Banks operating in urban areas have access to "thick" information on a wide set of entrepreneurial projects, and they can use it to assess the prospects of new projects (Lang and Nakamura, 1993). On the other hand, relationship lending - which increases the loan origination rate (Petersen and Rajan, 1995; De Young et al., 2012) - is likely to be more widespread far from urban centers, where asymmetric information induces banks to build long-term relationships with borrowers. On related grounds, recent developments in information and communication technology (ICT) - i.e. the increased availability of firm data and the improvements in credit scoring techniques - can weaken the link between the local availability of soft information and the decision to grant credit. Thus, the information channel might not make any difference between urban and non-urban locations. Thirdly, there could be a collateral channel. As real estate markets are thicker in urban areas, prices are less volatile and assets more liquid. Consequently, banks have a larger probability to provide finance, as they can more adequately price the collateral and more quickly recover loans from defaulting firms. For instance, Helsley and Strange (1991) find that the second best use of an immobile and specialized asset is more valuable in a large city than in a small city. The collateral channel is clearly relevant for all type of firms; however, its role should be even greater for construction firms, for which the entire output can be used as collateral.

[1] The views expressed in the paper are those of the authors and do not necessarily reflect those of the Bank of Italy. We would like to thank Antonio Accetturo, Antonio Bassanetti, Gaetano Basso, Vernon Henderson, Andrea Lamorgese, Marco Leonardi, Sauro Mocetti, Andrea Petrella, Paolo Sestito, two anonymous referees and the participants at the 56th ERSA Conference (Vienna, August 2016) and at Bank of Italy internal workshops for suggestions and comments.

On the whole, the impact of urbanization on firm access to credit is ultimately an empirical question. The previous literature on the topic is quite scant, with the exception of Gabriel and Rosenthal (2013) and Lee and Luca (2018). Gabriel and Rosenthal (2013) focus on access to home mortgages and find that urbanization leads to higher origination rates and loan amounts, although this effect fades over time. Lee and Luca (2018) focus on credit markets in cities around the world. By using firm-level data from the World Bank Enterprise Surveys, they find that firms in large cities (with more than 1 million inhabitants) are less likely to perceive credit constraints, although this effect declines as countries develop. Our investigation aims at getting a broad answer on whether Italian firms are better off in cities than in rural areas with respect to their ability to gain finance. Our target is therefore the net effect of urbanity, and we do not aim at pinning down exactly the individual channel through which the impact percolates. Findings, however, are presented with a sectoral breakdown. We suspect that the individual mechanisms at play might have different weights according to the tradable/non-tradable nature of the local businesses. For instance, the fortunes of non-tradable firms might depend more on local factors compared to manufacturing firms, which compete in a nationwide (even global) environment. Moreover, we present results from splitting the sample across firm characteristics, which might shed some light on the underlying mechanisms that feature urban credit markets.

The paper uses a novel indicator of credit constrains, which relies on Credit Register data. In particular, following Jiménez et al. (2012 and 2014), we exploit a proxy for new bank loan applications lodged by firms, which we contrast with the actual loans subsequently granted by the banks. Accordingly, we devise a firm-based indicator of capacity to access bank credit. The use of this indicator is a decisive improvement on currently available (survey-based) data in at least two ways: it is based on hard data instead of self-evaluations, and it is available for a potentially very large set of firms. The empirical analysis is based on a sample of about 45,000 Italian small and medium-size enterprises (SMEs), for which we observe both bank-firm level information and balance-sheet data. Focusing on Italian firms is particularly instructive for at least two reasons. Firstly, Italian firms heavily rely on bank financing (European Central Bank, 2013), with this being the only form of accessible credit for the vast majority of firms. Hence, it is likely that every firm in need of credit will only turn to banks. As we measure the capacity of firm access to credit by means of bank loan applications, this particular feature of Italian firms will reduce the risk of confounding factors (such as bond issuances) that could undermine our analysis. Secondly, a high proportion of SMEs in Italy report access to finance as their most pressing concern (European Commission, 2013), providing an ideal ground to investigate which factors might ease such difficulties.

We present a cross-labor market areas (LMAs) analysis, where we are able to fully characterize the features of the firms that face the local banking system in their quest for finance. We take the EU/OECD definition of cities and correlate the urban/non-urban feature of a firm location with our proxy for access to credit. Linear regressions show that there is some supportive evidence of a positive correlation between the urban location of a firm and its access to credit after controlling for individual and local characteristics. The positive correlation is, however, limited to the construction sector. Yet, there are a number of reasons for not interpreting linear regression results as causal. There could be a substantial measurement error since the EU/OECD definition might correspond poorly with the urban features that matter in practice. This would create attenuation bias. Moreover, there could be omitted geographical or firm characteristics along with endogeneity problems. To solve these problems, we derive two possible sources of exogenous variation for cities. As in the pioneering work of Ciccone and Hall (1996), we start by using historical variables, such as long lags of population density, to instrument for the current urban status. As explained by

Combes et al. (2010), this strategy can be deemed reasonable as long as there is some persistency in the spatial distribution of the population and the local drivers of high access to credit today differ from those of the past. By and large these requirements seem to be met in our case. However, we cannot be assured of the absence of serially correlated omitted variables, which would cause inconsistency in this set up. To tackle this potential threat, we estimate our model by using a second instrumental variable (IV), which relies on a completely different logic. We exploit some political economy considerations related to a massive social housing plan (INA-House plan), which took place in the aftermath of World War II (WWII). We show that the plan was more effective in cities that were hometowns of politicians of the ruling party. Under this setting, serially correlated omitted variables have to be related to post-WWII politician hometowns, which seems to be a stricter requirement. Both IV estimates provide a consistent picture. They confirm that urbanity provides some benefits with respect to access to credit, which are, however, reaped only by construction firms. Among the various potential channels, the one referring to the availability of valuable real-estate collateral seems to make a difference for urban areas. Finally, we characterize the two subpopulations of compliers that refer to the alternative instrumental variables employed and argue that the impact of urbanity on firm access to credit is likely to be homogeneous across local credit markets with rather different characteristics.

The paper is organized as follows. Section 2 describes the indicator used to measure the easiness of access to credit at the firm level, devised from the Bank of Italy Credit Register. Section 3 introduces the dataset, the notion of urban areas (urban LMAs) and provides some preliminary descriptive evidence. Section 4 describes the identification strategy and provides the main results, while a series of robustness exercises are performed in Section 5. In Section 6 we argue that there are possible heterogeneous effects by characterizing the subpopulations of compliers associated with the two instrumental variables employed. Section 7 provides a discussion of the possible channels that are likely to drive our results. Finally, Section 8 concludes.

2. Measuring access to credit

Measuring the extent of access to credit at the firm level is a difficult task. Even when disaggregated information at the bank-firm level involving both credit granted and credit disbursed (which is typically the case for Credit Register data) is available, drawing information on access to credit from such hard data is rather cumbersome, as loan amounts jointly reflect both demand and supply factors. In order to disentangle the role of credit demand and credit supply, most studies rely on survey-based data, where firms are directly asked about their capability of accessing credit. While survey data generally allow for a more precise measurement of credit access, they also suffer from a wide set of limitations: since surveys are both time consuming and costly, the sample is often too small and unsuitable for local level analysis; moreover, the sample could suffer from attrition problems, self-report bias, and selection.

We use a novel measure of credit accessibility that exploits hard data at the bank-firm level provided by the Credit Register maintained at the Bank of Italy. This measure has been used in several recent papers (Jiménez et al. 2012 and 2014, exploit a similar Credit Register at the Bank of Spain; for Italy, see Albareto et al., 2016; Albertazzi et al., 2017; Galardo et al., 2019). The measure exploits the fact that banks may ask the Credit Register for information about potential new borrowers by paying a fee, whose amount is, however, negligible. This service, known as "servizio di prima informazione" (preliminary information request), allows banks to obtain information on the current credit position of the potential borrower *vis a vis* the banking system; this information helps bank officers to make a decision about the loan application.[2] By matching the bank-firm level data on such preliminary information requests with subsequent bank-firm level data on loans granted, we are able to verify whether a borrower was successful (loan approval, i.e. good access to credit) or not (loan denial, i.e. bad access to credit) in obtaining new loans.

Operationally, we proceed as follows:

- we select all firms for which at least one bank issued a preliminary information request in 2013;[3]
- for each of these firms we check whether such banks granted credit within three months following the inquiry;[4]
- if at least one bank has granted a loan to the firm, we claim that the firm has good access to credit, i.e. the variable measuring the easiness of access to credit takes the value 1 for that firm. Otherwise it takes the value 0.

Our indicator of access to credit is available for a very large set of firms. Potentially, any firm that is registered (i.e. that has outstanding bank loans for an amount that is larger than the threshold of 30,000 euro) might be part of our dataset, overcoming many of the limitations stemming from the use of survey-based evidence. As a matter of fact, our empirical results are based on a sample of roughly 45,000 observations. The type of information received from "Servizio di prima informazione" is broadly consistent with that from survey data. Lucci (2017) focuses on the firms participating in the "Survey on Industrial and Service Firms (SISF)" carried out at the Bank of Italy in the years 2010-2014 and matches the measure of self-reported credit rationing with that recovered from the Credit Register. She finds a positive and statistically significant relationship between the two variables.

While the Credit register indicator is admittedly the best available proxy of access to credit, it comes at the cost of possible measurement errors. In particular, and because enquiring the Credit register is a voluntary decision, it could either overestimate or underestimate the easiness of access to credit. In the Appendix we provide a detailed discussion of such potential measurement errors and argue that, in our case, they do not represent a threat.

3. Data

Our data draws on three main sources: (1) the Central Credit Register, containing monthly information at the bank-firm level on the amount of granted loans, which we exploit to devise our measure of access to credit; (2) the Cerved Archive, containing balance sheet data for all Italian limited companies (as access to credit is measured in 2013, we select balance sheet data referring to 2012); (3) the National Institute of Statistics (Istat), mapping Italian municipalities into LMAs.

[2] Notice that while every preliminary information request issued by a bank on a firm applying for a bank loan is recorded in the Credit Register, the amount of the loan requested by the firm is not.

[3] We chose only one year in order to focus on the same credit cycle; however, as we show in the robustness section, the results hold when choosing a different year.

[4] The choice to consider a three-month period is in line with Jiménez et al. (2012), Albareto et al. (2016), Albertazzi et al. (2017) and Galardo et al. (2019), who use the same information. In the robustness section, we also provide the results for a six-month window.

Our definition of urban areas is based on the concept of population density. In particular, an urban area includes cities and their commuting zones. The definition of a city follows the criteria indicated by the European Commission and the OECD (OECD, 2012) and is based on the presence of an ‘urban center’, a relatively recent spatial concept based on high-density population grid cells. The procedure consists of four basic steps: step 1: all grid cells with a density of more than 1,500 inhabitants per square kilometer are selected; step 2: the contiguous high-density cells are then clustered, gaps are filled and only the clusters with a minimum population of 50,000 inhabitants are kept as an ‘urban center’; step 3: all the municipalities with at least half their population inside the urban center are selected as candidates to become part of the city; step 4: the city is defined ensuring that (a) there is a link to the political level; (b) at least 50% of the city population lives in an urban center and (c) at least 75% of the population of the urban center lives in a city. Drawing from this city definition, an urban area consists of the city and its surrounding commuting zone (local labor system, LMA). Applied to Italy’s spatial distribution of population, the EU/OECD definition identifies 73 urban areas (urban LMAs) over a total of 611 LMAs (see figure 1).[5] The main advantage of using the EU/OECD definition is that it is now standard in Europe and facilitates cross-country comparisons.

As expected, urban LMAs have a higher bank branch density than non-urban ones. The cost of credit to firms, however, is similar across urban and non-urban LMAs. In particular, at the end of 2013 the short-term interest rate to firms was 6.3 percent in urban areas and 6.2 in non-urban areas. The difference in the long-term interest rate was larger (0.7 percentage points; see table A1).

As shown in figure 2, firms headquartered in urban LMAs show better access to credit with respect to firms located in non-urban LMAs: the capability of access to credit is about 12% larger for the first group of firms. This evidence arguably reflects both supply side factors and characteristics of the firms. Concerning the latter, urban LMAs are characterized, on average, by more productive firms: firm productivity, measured as the ratio between sales and employment costs, is 15% larger than that characterizing non-urban locations. Urban firms also show a higher level of R&D, proxied by the weight of intangible assets over total assets: the share is 24% larger for firms located in urban LMAs relative to that of non-urban firms. Regarding the role of bank debt as source of financing, the two groups of firms do not significantly differ.

We model firm access to credit controlling for a wide set of firm characteristics (size, risk, profitability, leverage); we also include the number of banks making an inquiry, controls defined at the LMA level that refer to the degree of concentration in the LMA credit market[6] and the average funding gap of banks operating in the LMA. These indicators are defined in table A2, while table A3 provides some descriptive statistics about our estimation sample. Two-thirds of firms are headquartered in urban LMAs. 13% of them were granted a loan from one of the banks making the Credit Register inquiry within three months from the request. Profitability was only positive for construction firms; these firms also differ from service and manufacturing firms in terms of sales and leverage: construction firms are smaller and are characterized by a higher degree of indebtedness than the other ones. On average, about 2

[5] Labor market areas (LMAs) are sub-regional geographical areas where the bulk of the labor force lives and works, and where establishments can find the largest amount of the labor force necessary to occupy the offered jobs. For more details see http://www.istat.it/en/archive/142790.

[6] Cetorelli and Strahan (2006) show that a higher degree of concentration in the banking sector leads to greater difficulty for potential industry entrants to access credit because banks with market power protect the profitability of their existing borrowers.

banks make a Credit Register inquiry for each firm in the sample; this number is very similar among sectors. Slightly less than 30% of the firms in our estimation sample are headquartered in the South and the Islands, a share similar to that of the universe of Italian limited liability companies (table A4).

4. Identification strategy and main results

We first present our linear probability model (LPM) results. Then, we tackle identification issues by relying on two different IV strategies.

4.1 *LPM regressions*

We estimate the following firm level equation:

$$y_{is} = \alpha + \beta \; urban\_LMA_s + \Gamma x_i + \mathrm{K}\theta_s + \Lambda\phi_i + \Psi\lambda_p + \varepsilon_{is} \tag{1}$$

where *y* is a dummy variable taking the value 1 if firm *i*, headquartered in the Local labor area (LMA) *s*, has good access to credit; *urban_LMA* is a dummy taking the value 1 if the firm is headquartered in an urban LMA; x is a vector of firm level variables; θ is a vector of LMA level variables reflecting supply side factors in the loan application decision (the Herfindahl-Hirschman index, measuring the degree of concentration in the LMAs credit market and the bank funding gap, averaged across each LMAs); $\phi$is a vector of firm sector dummies using a coarse (3 sectors) classification, and λ is a vector of provincial dummies, capturing local economy characteristics.

LPM results are reported in table 1. Overall, our estimates show no impact of urbanity on firms' access to credit. When we estimate the model separately by sector, a positive and statistically significant impact emerges for construction firms, while there is no effect for the other two sectors. In particular, construction firms headquartered in urban LMA have a probability to access bank loans which is 16% higher than the average. As expected, firm riskiness negatively affects access to credit, while larger and more profitable firms have easier access to bank loans. Finally, the number of banks making an inquiry positively affects the probability that the firm obtains the loan.

In order to be able to interpret equation (1) in a causal sense we need to tackle a few identification challenges. In particular, our estimate of β could be biased due to the omission of relevant explanatory variables, with an unknown direction of bias. A second source of endogeneity could stem from reverse causality (i.e. firms might take into account the extent of bank credit availability in their decision about where to locate), leading to an upward bias of our OLS estimate. Finally, urban areas could be measured with error, as the EU/OECD methodology relies on a series of unavoidable working assumptions and correction rules; in this case our OLS estimate would be downward biased.

4.2 *IV using past density*

We follow an instrumental variable approach. We start by using as an instrument the density of the LMA in 1861, the oldest data we could gather at the municipality level. In our context, the instrument is a strong predictor for an LMA being classified as an urban center by the EU/OECD procedure. To be valid the instrument needs to be correlated with the outcomes of interest only through the current urban status of the LMA. The argument that the error term in equation (1) should be uncorrelated with our instrument relies on the fundamental changes that affected the Italian economy since 1861. These include the two world wars, the fascist dictatorship, and the more general transformation from a largely rural economy at the start of the 20th Century. Other research has also shown that past population density yields results similar to alternative instruments based on geology (Combes et al., 2010) and are robust to the inclusion of many local characteristics. The adoption of historical population variables as instruments has a strong tradition in urban economics since Ciccone and Hall (1996). Therefore, this standard choice might allow easier comparisons of our results.

Results are reported in table 2. IV estimates confirm the baseline LPM findings: we find a positive statistically significant impact only for construction firms. The coefficient of the dummy *urban_LMA* is now larger, indicating that our OLS estimates are downward biased. According to these estimates, the probability of urban construction firms accessing bank loans is about 70% higher than the average.

4.3 *IV using political economy considerations*

The use of lagged values of population density as an instrumental variable could, however, be problematic if the omitted variables are serially correlated. To tackle this potential threat, we also estimate our model using an alternative instrumental variable strategy. Namely, we exploit the role played by the main Italian party, Democrazia Cristiana, henceforth DC, in shaping the urban landscape in the second post-war period in Italy. In 1948 the DC won the political elections, gaining the absolute majority in Parliament. The following year, under law number 43 of 28 February 1949, the government launched the so-called "INA-House plan", a massive investment plan in social housing (900 billion Liras overall), which lasted 14 years and significantly affected the urban growth in Italy.

In those 14 years construction sites were opened in about 5,000 municipalities (out of a total of around 8,000), and about 350,000 houses were built, with a total of 1,920,000 rooms. The incidence of the houses built because of the “INA-House plan” was significant, reaching, on average, 10% of the houses built in Italy between 1951-1961; the incidence was higher in the South, with the highest in Calabria (18.5%). As suggested by historians (see Di Biagi, 2001; Bottini, 2001 and Frontera, 2012), the massive social housing program promoted urbanization (and overall employment and growth, as the buildings favored inflows of rural migrants into higher productive urban locations). At the same time, it was pivotal to reward and strengthen the political consensus of the leading party. To derive a plausible exogenous source of variation we exploit the presence of local political patronage and hometown political favoritism[7]: places that gave birth to a DC senator were disproportionately favored by the social housing program.

[7] While hometown political favoritism largely characterizes authoritarian regimes (see for instance Do et al., 2017), it is not uncommon in democratic countries. For instance, Besley et al. (2012) use Indian data and show that politicians favor their own villages, which in turn support them in elections. Regarding European countries, Fiva and Halse (2016) use data from regional elections in Norway for the period 1976-2011 and find that politicians are able to obtain public spending to benefit their hometowns. Hodler and Raschky (2014) use data from about 40,000 regions in 126 countries and find that nighttime light is more intense in regions where the current political leader was born. Arguably, Italy is not an exception (see Carozzi and Repetto, 2016).

To corroborate our assumption of political patronage, we look at the number of houses and investments made over the 14 years of the INA-House plan at the province[8] level, as LMA level data on the INA-House plan are not available. We consider three variables: the number of homes per capita built over the 14 years of the plan; the number of rooms per capita; the expenditure per capita, measured in both the first and second 7-year periods (in which the data available are framed). Our variable of interest is given by the number of DC Senators with their hometown in the province. As the incidence of the INA-House plan was larger in the South, we also include a dummy for Southern Italy. The estimates, displayed in table A5, show that the number of homes per capita was affected by the number of DC Senators having their hometown in the province; a similar result is found when considering expenditure per capita; we also find a positive correlation between the number of rooms and the number of DC senators, although the coefficient is marginally non-statistically significant. As a matter of fact, the South dummy is mostly positive, but not statistically significant. These results are highly suggestive of the presence of a "political channel" underlying the unfolding of the INA-House plan. They motivate the choice of our instrument, which is a dummy variable taking the value 1 for those LMAs that contain the hometown of one of the senators of the DC party elected in 1948.

The exclusion restriction implied by our approach is that, conditional on the controls included in the regression, being the hometown of a DC senator in 1948 has no effect on access to credit for a firm today, other than through the urbanization channel. It is possible to think of causes for failure of the exclusion restriction: for instance, in a dynastic political system the descendants of a 1948 DC Senator (to the extent that they are born in the same place) might still be in charge of transferring resources to the hometown and thus impacting the credit market. However, such possibilities seem to be really unlikely, given also the political turnaround Italy experienced since 1992. In particular, the so-called "Mani pulite" scandal (1992-1994) led to the complete disappearance of the DC, while new parties with different agendas gained parliamentary seats (see Barone et al., 2016). Building on these arguments, we consider this instrumental variable as our preferred one. Moreover, besides relying on much more credible assumptions behind exclusion restrictions, the instrument based on the political favoritism argument is characterized by a larger share of compliers (see below, Section 6).

As depicted in table 2, IV results based on our preferred instrumental variable confirm our previous findings. The first stage F-statistic highlights that 1948 hometown patronage is a sufficiently strong predictor of today's urban status. Again, no effect is found for manufacturing and services, while the probability of construction firms headquartered in urban LMAs to access bank loans is 30% greater than the average. Moreover, the IV estimates deliver a consistent picture, irrespective of the instrument used. This suggests that the effect of urbanity on credit access might be relatively homogenous across cities (see Section 6).

While the IV estimates of the impact of urbanization on access to credit are still larger than the OLS estimates, the difference is now much lower than that associated with the previous instrument. Yet, such attenuation bias suggests that our key identification variable might suffer from measurement error. In particular, since the EU/OECD methodology relies on a series of unavoidable working assumptions and correction rules to estimate whether an LMA is urban or not, such a measure is likely to be an error-ridden proxy of the true level of urbanization. Along with the measurement error, the difference between OLS and IV estimates could be associated with omitted variables bias. In this case the direction of the bias

[8] This corresponds to 92 units at the NUTS-3 geographical level.

is in principle unknown. For instance, a greater competition among firms applying for bank loans, which is arguably larger in urban areas, could lead to a downward bias of the OLS estimates as banks could cherry pick firms. On the other hand, a larger presence of more skilled entrepreneurs, possibly characterizing urban areas, would lead to an upward bias of the OLS, since skilled managers can arguably better negotiate bank loan terms. Overall, we argue that in our case the downward bias of the OLS estimates could reflect the prevalence of unobservables leading to a downward bias with respect to those leading to an upward bias.

5. Robustness

In this section we probe the robustness of the previous findings. To save space, we only present LPM and IV estimates obtained using the instrument inspired by political economy considerations (however, using historical population density would deliver very similar results).

Our estimates have so far focused on a single year: 2013. There is a risk that our results might reflect the consequences of the severe economic and financial conditions of that period. During the Lehman Brothers and the sovereign debt crises, banks' supply schedules became tighter (Del Giovane et al., 2013). To the extent that the credit crunch was heterogeneous across sectors and localities (see Barone et al., 2016), we can mistakenly attribute to the urban status an effect that is essentially driven by the crisis. To check whether this issue has an impact on our findings, we re-estimate the model using a pre-crisis year, 2007 (in this case our sample becomes much larger: it now includes around 86,000 firms, due to the fact that 2007 was the peak of a long-lasting credit expansion in Italy). Reassuringly, the IV estimates reported in table 3, panel (a), confirm our previous findings.

We check whether our findings continue to hold if we allow for a larger time window (6 months instead of 3) for banks to grant credit. The results, reported in table 3, panel (b), are very similar to the previous findings. Next, we replicate our estimates also controlling for a measure of firm productivity, proxied by the value added to labor costs ratio. The inclusion of productivity unduly restricts our sample (which goes down to 30,000 firms) due to the availability of labor cost data for only a subset of firms. Quite surprisingly productivity does not enter significantly into our regressions, suggesting that this aspect is already captured by the remaining right hand-side controls. Crucially, the results shown in table 3, panel (c), confirm again our findings.

We also estimate the model by controlling for one additional banking sector characteristic: the share of loans granted by banks belonging to the top 5 groups. This variable allows us to proxy for the incidence of non-local banks, whose lending policies rely less on soft information and on retail funding than other banks. Moreover, lending policies of these banks might reflect strategic decisions at the group level. For instance, the Bank of Italy regional survey on banks on the terms of credit supply and on the demand for credit showed that, in the period covered by our data, larger banks provided easier credit conditions with respect to smaller banks. After controlling for the share of loans granted by the top 5 bank groups our results are fully confirmed (table 3, panel (d)).

Finally, we estimate the model by adding other bank balance sheet indicators: the average values of the tier 1 ratio and the ratio of non-performing loans (NPLs) to total loans of the banks operating in the LMA. These variables allow us to control for the fact that banks characterized by a lower degree of capitalization and credit quality could adopt more restrictive credit policies (table 3, panel (e)). Irrespective of the estimation method used our results continue to hold, while the coefficients associated with these bank indicators are never significant.

6. Compliers groups, heterogeneous effects and external validity

Since we rely on an IV identification strategy, our estimates refer to those firms whose localization in an urban LMA was induced by the instrument (so-called "compliers"), leading to the estimation of a local average treatment effect (LATE), rather than the average across the full sample. In this section we exploit the presence of two subpopulations of compliers (one for the political patronage IV and one for the past population density IV) to investigate whether the effect of urbanity on firm access to credit is heterogeneous across local credit markets and firm characteristics. Since the IV estimates associated with the two instruments both suggest a positive (and similar in size) impact of urbanity on firm access to credit for construction firms only, if the related compliant subpopulations are different we could argue that the estimated effects of urbanity are homogeneous (Angrist and Pischke, 2009). In turn, this would also support the claim of greater external validity for our results.

While it is generally not possible to identify complier firms, in the case of binary instruments and binary treatment variables, both the population of treated firms which are compliers and their observable characteristics can be easily estimated (Angrist and Pischke, 2009). As our treatment variable is a dummy taking the value 1 for firms headquartered in an urban LMA, our identification strategy automatically falls exactly within this scenario for our preferred instrumental variable (dummy variable taking the value 1 for those LMAs that contain the hometown of one of the senators of the DC party elected in 1948). On the other hand, the instrument based on the past population density is continuous; in order to also characterize the population of compliers for this second instrument, we recoded it as binary[9].

In the case of the hometown political favoritism instrument, the compliers are about one third of the population of "treated" firms, while in the case of the past population density instrument the share falls to about 13%. This evidence supports our choice to consider the first instrument as the preferred one; moreover, from a policy point of view, it is reassuring that our evidence is not representative of just a marginal group of firms. Although compliers cannot be individually identified, it is possible to describe how the characteristics of the complier subpopulations compare with those of the full sample (Angrist and Pischke, 2009). In order to perform this task, we select a set of pre-treatment binary variables measuring both the local credit market characteristics (bank concentration, bank funding policy) and the firm observables (risk, profitability, assets, leverage) and estimate the relative likelihood that a complier firm shares any of such characteristics. These estimates are reported in table 4. Considering our preferred instrumental variable, a complier firm is much more likely (74% more likely) to operate in an LMA where the bank concentration is high with respect to the average firm, while it is less likely to operate in an LMA where banks have a large funding gap; similarly, it is less likely to be in an LMA where the first banking groups have the largest share of loans. When we consider our second instrument, we obtain a rather different picture:

[9] In order to recode the instrument as binary we estimate a probit model where the dependent variable is the dummy urban and the independent variable is the LMA's density in 1861 and define a dummy variable equal to 1 when the predicted probability for an LMA to be urban is above the mean and 0 when it falls below. Using such instrument leads to estimates which are almost identical to those obtained using the baseline instrument based on past LMA's density (estimates not reported but available upon request).

in particular, complier firms are now more likely to operate in an LMA where banks have a large funding gap with respect to the average firm (25% more likely), and the relative likelihood to be in a bank-concentrated LMA is now much higher. When we consider firm characteristics (table 4, panel (b)) the estimated relative likelihoods suggest that the favorite IV-complier firm is very similar to the average firm; moreover, the two instruments provide very similar estimates.

Overall, while firm characteristics of the two groups of compliers are very similar, we find remarkable differences in their local credit markets, being more concentrated for the second group of compliers (past population density IV). As the IV estimates are very similar using the two alternative instruments, despite the differences in the compliers' local credit markets, we argue that the impact of urbanization is not likely to be heterogeneous across local credit markets with different features. On the other hand, we cannot exclude that heterogeneous effects are instead at work when firms with different characteristics from those in the sample are considered.

Our sample involves only firms for which banks issued a preliminary information request in 2013 (about 10% of the population of Italian limited companies), as for these firms only we can assess the capacity of access to credit. Hence, the external validity of our results could be undermined by potential selection being at work. If the absence of a request of preliminary information is correlated with the firm capacity of access to credit, our results will not be generalizable to the population of firms. To reassure on the external-validity side, we will show that, conditional on the set of observables employed in our model, the probability of being included in the sample is random. On a theoretical ground, since the cost faced by banks to issue a preliminary information request is negligible and all firm variables that provide insights on firms' merit of credit are included in our model, this assumption seems perfectly reasonable, although clearly not testable. In order to support our claim, we consider a linear probability model where the dependent variable is the probability that a firm is sampled. Conditional on the controls used in our baseline regressions, we then assess the correlation between such probability and four different observables: the dummy urban (our "treatment" variable); the amount of sales (used as proxy of firm size); the incidence of non-tangible assets (used as proxy for R&D investments); productivity, proxied by the ratio of sales to labor costs. The results, reported in table A6, show that the probability that a firm is included in our sample is not correlated with any of those variables.

7. A discussion of the findings

The impact of urbanity on credit access for firms in industry and service is - consistently across specifications and methods - estimated to be zero. In Table 5 we re-run our preferred IV regressions by splitting the sample across the (high/low) productivity and the (young/old) age dimensions. The results are basically inconclusive (only for manufacturing firms we find that young firms have a penalization in terms of access to credit, although the effect is only marginally significant). Overall, neither the productivity/selection channel nor the information channel seem to provide a meaningful characterization of urban credit markets.

Only construction firms seem to be able to reap financing benefits by dwelling in urban markets. Our findings therefore highlight that an explanation based on collateral might be at the core of the agglomeration advantages in the credit market. While the collateral channel is in principle relevant for all firms that have real estate properties on their balance sheets, it is clearly much more important for the firms in the building and construction sector, as the benefits of thicker real estate markets percolate to the whole firm output that is usable as guarantee. The last panel of Table 5 splits the sample according to the value of inventories in the firm balance-sheets. In the case of construction firms, inventories include unsold properties. We find that the impact of urbanity is larger for construction firms that have a larger stock of unsold properties. Inventories also matter for service and manufacturing firms, even though the estimated coefficient of the dummy urban has a larger p-value. Thus, the collateral might also be important, to a certain extent, when it includes finished goods or raw materials. Thicker real estate markets imply less price volatility and high asset liquidity. For instance, the number of months on market is 24% larger for houses located in non-urban areas (Banca d'Italia, 2018). Therefore, the value of the housing collateral is maximized for the lending institutions, which can more easily mark it. Because of the substantial urban premium in housing evaluations (Manzoli and Mocetti, 2016), real estate properties represent a larger share of the balance sheet for urban companies. Therefore, both sides of the credit relationship seem to take advantage of the urban status.

8. Conclusions

In this paper we investigate whether urban agglomeration favors access to credit for firms. There are several channels through which urban agglomeration could positively affect firm access to credit. In urban areas, for instance, firms are generally more productive and can raise more collateral. However, opposite factors - such as fiercer competition in cities, leading to greater firm selection, as well as more widespread use of the so-called relationship lending model far from urban centers - might reduce the urban area premium and favor access to credit in rural and non-urban areas.

In order to test the hypothesized relationship we exploited information contained in the Bank of Italy Credit Register which allows us to draw information on credit access for a wide set of firms. In particular, we exploit the fact that banks and other financial institutions may ask the Credit Register for information about potential new borrowers. By matching the bank-firm level data on such preliminary information requests with subsequent bank-firm level data on loans granted, we are able to verify whether a borrower was successful (with a loan approval) or not (loan denial) in obtaining new loans.

We estimated a linear probability model (firm level data), where the dependent variable takes the value 1 if at least one of the banks that made the request to the Credit Register on a certain firm granted a loan to that firm within 3 months from the request. In order to deal with endogeneity, we followed an instrumental variable approach that exploits as instruments past population density and urbanization driven by political economy considerations. We find that construction firms headquartered in urban areas have better access to credit compared to those headquartered in non-urban areas. However, we fail to find evidence of urban financing premia for manufacturing or service firms. Our analysis suggests that the impact of urbanity on access to credit is mainly driven by the collateral channel through the real estate market. In particular, construction firms operating in urban areas benefit more than those operating in non-urban areas due to the thickness of the real estate market in cities, leading to less price volatility and greater asset liquidity. As a consequence, banks can more easily price the collateral and recover loans from defaulting firms, being more willing to provide finance.

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Tables and figures

Table 1. Baseline OLS estimates

VARIABLES	Total		Construction		Service		Manufacturing	
urban LMA	-0.00618	-0.00273	0.0252**	0.0263**	-0.000785	0.00133	-0.0221	-0.0226
	(0.00485)	(0.00491)	(0.0120)	(0.0122)	(0.00575)	(0.00577)	(0.0143)	(0.0146)
fgap LMA	-0.000936***	-0.000913**	-0.00172*	-0.00162*	-0.00100**	-0.000973**	-0.000395	-0.000365
	(0.000356)	(0.000355)	(0.000903)	(0.000894)	(0.000407)	(0.000406)	(0.000850)	(0.000860)
hhi LMA	-0.00726	-0.00730	-0.144	-0.116	0.0798	0.0661	-0.00413	0.0162
	(0.0775)	(0.0769)	(0.210)	(0.209)	(0.0841)	(0.0838)	(0.215)	(0.213)
tang. assets (log)		0.00376***		0.00939***		0.00405**		-0.00568***
		(0.00125)		(0.00346)		(0.00162)		(0.00211)
sales (log)		0.00953***		0.0129***		0.0125***		0.00779**
		(0.00121)		(0.00329)		(0.00145)		(0.00306)
risk		-0.00176*		-0.00501*		-0.00181**		0.00213
		(0.000905)		(0.00268)		(0.000900)		(0.00277)
interests paid		--0.00002		-0.00249**		0.00182		-0.00629
		(0.00038)		(0.00106)		(0.00248)		(0.00394)
roa.Q2		0.0295***		0.0259*		0.0285***		0.0448***
		(0.00430)		(0.0134)		(0.00529)		(0.0130)
roa.Q3		0.0484***		0.0550***		0.0455***		0.0606***
		(0.00620)		(0.0170)		(0.00638)		(0.0144)
roa.Q4		0.0392***		0.0460***		0.0340***		0.0645***
		(0.00510)		(0.0147)		(0.00512)		(0.0163)
leverage.Q2		0.0203***		-0.00006		0.0219***		0.0355***
		(0.00358)		(0.0163)		(0.00409)		(0.0120)
leverage.Q3		0.0282***		0.0284*		0.0307***		0.0275
		(0.00565)		(0.0164)		(0.00628)		(0.0180)
leverage.Q4		0.0181***		0.0215		0.0228***		-0.00727
		(0.00528)		(0.0144)		(0.00663)		(0.0157)
no. of requests		0.00749***		0.00548		0.00718***		0.0117***
		(0.00143)		(0.00430)		(0.00207)		(0.00375)
provincial dummies (103)	yes	yes	yes	yes	yes	yes	yes	yes
sector dummies	yes	yes	no	no	no	no	no	no
Constant	0.163***	0.0691***	0.245***	0.127***	0.141***	0.0198	0.179***	0.0749**
	(0.00673)	(0.0146)	(0.0265)	(0.0353)	(0.00976)	(0.0142)	(0.0184)	(0.0318)
Observations	45153	45153	5311	5311	32647	32647	5276	5276
R-squared	0.006	0.018	0.028	0.043	0.007	0.019	0.024	0.038

Notes: Linear probability model. See table A2 for the description of all variables. ROA and leverage are expressed by means of four dummies, indicating whether the firm belongs to four quartiles, with Q4 being the highest and Q1 being the lowest. Clustered (at the LMA level) standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2. Baseline IV estimates

VARIABLES	Total		Construction		Service		Manufacturing	
	IV past density (1)	*IV hometown fav. (2)*	*IV past density (1)*	*IV hometown fav. (2)*	*IV past density (1)*	*IV hometown fav. (2)*	*IV past density (1)*	*IV hometown fav. (2)*
urban LMA	0.0118	0.00523	0.108*	0.0483*	-0.000039	0.0182	-0.0687	-0.0684
	(0.0240)	(0.0111)	(0.0593)	(0.0270)	(0.0205)	(0.0155)	(0.0513)	(0.0446)
fgap LMA	-0.00132*	-0.00114**	-0.00368**	-0.00217**	-0.000933	-0.00146**	0.00108	0.00107
	(0.0007)	(0.000461)	(0.0017)	(0.00105)	(0.0007)	(0.000602)	(0.0017)	(0.00165)
hhi LMA	-0.0145	-0.0112	-0.0777	-0.106	0.0669	0.0564	0.0981	0.0975
	(0.0810)	(0.0783)	(0.2360)	(0.212)	(0.0847)	(0.0877)	(0.2220)	(0.220)
tang.assets (log)	0.00383***	0.00380***	0.00973***	0.00948***	0.00404**	0.00415**	-0.0058***	-0.00581***
	(0.0013)	(0.00125)	(0.0034)	(0.00343)	(0.0016)	(0.00162)	(0.0021)	(0.00210)
sales (log)	0.00949***	0.00951***	0.0123***	0.0128***	0.0125***	0.0125***	0.00774**	0.00774**
	(0.0012)	(0.00120)	(0.0033)	(0.00325)	(0.0014)	(0.00144)	(0.0030)	(0.00303)
risk	-0.00180**	-0.00178**	-0.00514*	-0.00504*	-0.00181**	-0.00186**	0.00222	0.00222
	(0.0009)	(0.000900)	(0.0026)	(0.00264)	(0.0009)	(0.000896)	(0.0028)	(0.00274)
interests paid	-0.00002	-0.00002	-0.00238**	-0.00246**	0.00182	0.00180	-0.00621	-0.00621
	(0.00039)	(0.00038)	(0.00101)	(0.00104)	(0.00248)	(0.00248)	(0.00394)	(0.00392)
roa.Q2	0.0296***	0.0296***	0.0275**	0.0263**	0.0285***	0.0287***	0.0451***	0.0451***
	(0.0043)	(0.00429)	(0.0132)	(0.0132)	(0.0053)	(0.00527)	(0.0130)	(0.0129)
roa.Q3	0.0485***	0.0484***	0.0574***	0.0556***	0.0455***	0.0456***	0.0602***	0.0602***
	(0.0062)	(0.00619)	(0.0172)	(0.0169)	(0.0064)	(0.00637)	(0.0143)	(0.0143)
roa.Q4	0.0393***	0.0393***	0.0473***	0.0463***	0.0340***	0.0340***	0.0651***	0.0651***
	(0.0051)	(0.00509)	(0.0146)	(0.0146)	(0.0051)	(0.00512)	(0.0162)	(0.0162)
leverage.Q2	0.0205***	0.0204***	0.00173	0.000421	0.0219***	0.0221***	0.0357***	0.0357***
	(0.0036)	(0.00358)	(0.0164)	(0.0162)	(0.0041)	(0.00408)	(0.0119)	(0.0119)
leverage.Q3	0.0283***	0.0282***	0.0306*	0.0290*	0.0307***	0.0307***	0.0274	0.0274
	(0.0056)	(0.00564)	(0.0160)	(0.0162)	(0.0063)	(0.00627)	(0.0177)	(0.0177)
leverage.Q4	0.0184***	0.0182***	0.0224	0.0218	0.0227***	0.0233***	-0.00826	-0.00825
	(0.0052)	(0.00527)	(0.0144)	(0.0143)	(0.0067)	(0.00664)	(0.0157)	(0.0156)
no. of requests	0.00750***	0.00749***	0.00511	0.00538	0.00718***	0.00719***	0.0115***	0.0115***
	(0.0014)	(0.00143)	(0.0043)	(0.00425)	(0.0021)	(0.00206)	(0.0037)	(0.00371)
provincial dummies (103)	yes	yes	yes	yes	yes	yes	yes	yes
sector dummies	yes	yes	no	no	no	no	no	no
Constant	0.0654***	0.0671***	0.0986***	0.119***	0.0202	0.0148	0.0870**	0.0869***
	(0.0164)	(0.0151)	(0.0361)	(0.0347)	(0.0159)	(0.0153)	(0.0349)	(0.0332)
Observations	45153	45153	5311	5311	32647	32647	5276	5276
R-squared	0.018	0.018	0.038	0.043	0.019	0.019	0.036	0.037
First stage F	21.27	19.28	25.55	24.41	19.3	17.11	15.53	16.15

Notes: See table A2 for the description of all variables. (1) Instrumental variable estimates using the LMA population density in 1861. – (2) Instrumental variable estimates using the political hometown favoritism hypothesis. ROA and leverage are expressed by means of four dummies, indicating whether the firm belongs to four quartiles, with Q4 being the highest and Q1 being the lowest. Clustered (at the LMA level) standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3. Robustness checks

VARIABLES	Total (1)		Construction		Service		Manufacturing	
				(a) estimates on 2007 data				
	OLS	*IV (2)*	*OLS*	*IV (2)*	*OLS*	*IV (2)*	*OLS*	*IV (2)*
urban LMA	-0.00239	0.000141	-0.00161	0.0315*	-0.00359	-0.00025	-0.00463	-0.0302
	(0.00349)	(0.00757)	(0.00843)	(0.0190)	(0.00401)	(0.00875)	(0.00831)	(0.0208)
				(b) access to credit assessed over 6 months following the bank inquiry				
	OLS	*IV (2)*	*OLS*	*IV (2)*	*OLS*	*IV (2)*	*OLS*	*IV (2)*
urban LMA	-0.00353	0.00833	0.0270**	0.0505*	-0.00029	0.0217	-0.0145	-0.0495
	(0.00544)	(0.0110)	(0.0134)	(0.0270)	(0.00596)	(0.0150)	(0.0152)	(0.0414)
				(c) controlling for productivity				
	OLS	*IV (2)*	*OLS*	*IV (2)*	*OLS*	*IV (2)*	*OLS*	*IV (2)*
urban LMA	-0.00029	-0.0114	0.0303**	0.0491*	0.00208	-0.00391	-0.0205	-0.0754
	(0.00534)	(0.0124)	(0.0135)	(0.0287)	(0.00660)	(0.0148)	(0.0155)	(0.0461)
				(d) controlling for the share of top 5 groups				
	OLS	*IV (2)*	*OLS*	*IV (2)*	*OLS*	*IV (2)*	*OLS*	*IV (2)*
urban LMA	-0.00297	0.00481	0.0265**	0.0484*	0.00101	0.0175	-0.0218	-0.0669
	(0.00493)	(0.0113)	(0.0123)	(0.0269)	(0.00578)	(0.0157)	(0.0145)	(0.0444)
				(e) controlling for bank balance sheets indicators				
	OLS	*IV (2)*	*OLS*	*IV (2)*	*OLS*	*IV (2)*	*OLS*	*IV (2)*
urban LMA	-0.00357	0.00515	0.0247*	0.0483*	0.00045	0.0176	-0.0248*	-0.0717
	(0.00490)	(0.0112)	(0.0128)	(0.0287)	(0.00577)	(0.0155)	(0.0143)	(0.0438)

Notes: All regressions include credit supply and firm level variables as well as 103 provincial dummies. - (1) Also includes 4 sectoral dummies. Clustered (at the LMA level) standard errors in parentheses. (2) Instrumental variable estimates using the political hometown favoritism hypothesis. *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Characteristics of the compliers

	hometown favoritism instrument	past population density instrument
	panel A: local credit market	
bank concentration (HHI) (1)	1.74	2.28
bank funding gap (2)	0.53	1.25
share of first 5 banking groups (3)	0.80	1.00
	panel B: firm characteristics	
firm risk (4)	1.02	1.10
firm profitability (5)	0.99	0.98
firm leverage (6)	1.04	1.21
firm assets (7)	1.01	0.96

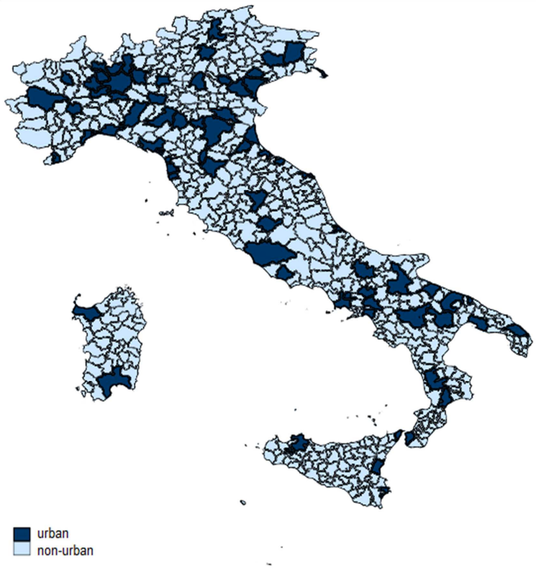
Notes: Relative likelihood that complier firms have the characteristic indicated in each row with respect to the firms in the sample, for both the instrument of hometown political favoritism and the instrument of past population density. (1) Binary indicator taking the value 1 for those LMAs characterized by a bank loan HHI greater than the average computed on the full sample; (2) binary indicator taking the value 1 for those LMAs characterized by a bank funding gap greater than the average; (3) binary indicator taking the value 1 for those LMAs characterized by a share of top 5 banking groups greater than the average; (4) binary indicator taking the value 1 for risky firms; (5) binary indicator taking the value 1 for more profitable firms; (6) binary indicator taking the value 1 for firms with high leverage; (7) binary indicator taking the value 1 for those firms whose amount of assets is greater than the average.

Table 5. Sample splits results

	Total		**Construction**		**Service**		**Manufacturing**	
Firm productivity	above median	below median	above median	below median	above median	below median	above median	below median
urban LMA	-0.01405	-0.01091	0.01245	0.06843	0.00446	-0.01138	-0.08247	-0.05633
	(0.01870)	(0.01882)	(0.04075)	(0.04454)	(0.02192)	(0.02370)	(0.05257)	(0.06742)
Observations	16708	16270	1806	2170	12363	11255	1975	2235
First stage F	22.50	23.27	30.90	22.37	19.41	21.18	15.76	13.35
Firm age	above median	below median	above median	below median	above median	below median	above median	below median
urban LMA	0.01948	-0.00961	0.05355	0.02929	0.02168	0.01708	0.00710	-0.17710*
	(0.01652)	(0.01838)	(0.03941)	(0.03918)	(0.0196667)	(0.02084)	(0.04293)	(0.10624)
Observations	24816	19707	2430	2834	18387	13867	3028	2112
First stage F	18.38	20.34	19.62	28.48	17.57	16.80	16.45	13.48
Stock of inventories	above median	below median	above median	below median	above median	below median	above median	below median
urban LMA	0.028917*	-0.01510	0.08905**	-0.02400	0.04598*	-0.00867	-0.04072	-0.13272*
	(0.01667)	(0.01824)	(0.03591)	(0.05144)	(0.0239964)	(0.02148)	(0.04952)	(0.07958)
Observations	21447	23275	2987	2286	14034	18379	3672	1565
First stage F	23.43	15.36	32.48	13.71	20.98	14.59	18.48	9.10

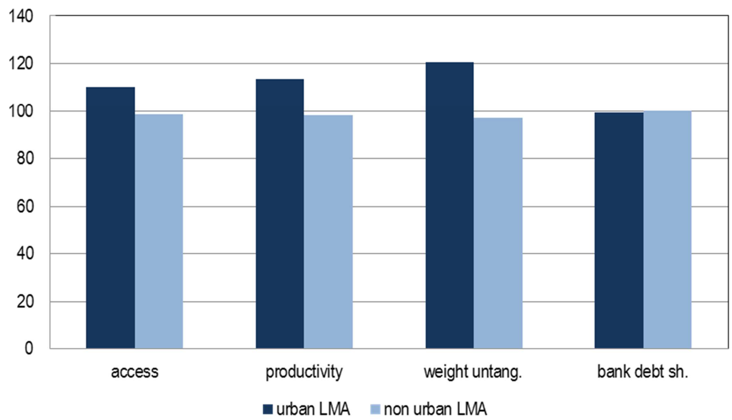
Notes: Instrumental variable estimates using the political hometown favoritism hypothesis. All regressions include credit supply and firm level variables as well as 103 provincial dummies. *** p<0.01, ** p<0.05, * p<0.1.

Figure 1. Urban LMAs



Notes. The figure shows the 611 Italian LMAs, defined as sub-regional geographical areas where the bulk of the labor force lives and works, according to the Italian statistical institute 2011 definition (see http://www.istat.it/en/archive/142790). Urban LMAs are represented in dark.

Figure 2. Access to credit and firm characteristics by urban vs non-urban LMAs



Notes. The figure shows the average capacity of access to credit, the average productivity, the average share of untangible assets over total assets and the share of total bank debts over total financial debts for firms headquartered in urban LMAs and those headquartered in non-urban LMAs. Firm characteristics are expressed as indexes, with 100 being the average value characterising the full sample. Access to credit is measured in 2013, while balance sheet data refer to year 2012.

Appendix

A. Additional tables

Table A1. Bank branch density and cost of credit to firms at the end of 2013

	urban LMA	non-urban LMA
Bank branch density		
branch per square km	24.7	6.0
Cost of credit to firms		
short-term lending rate	6.3	6.2
long-term lending rate	3.6	4.3

Table A2. Description of variables

name	*variable*	*definition*
access	Access to credit	Dummy variable equal to 1 if the firm has good access to credit
urban LMA	Urbanization	Dummy variable equal to 1 if the firm is headquartered in an urban local labor system
fgap LMA	Banks funding gap	Average funding gap of the banks operating in a local labor system
hhi LMA	Banks HHI	Average Herfindahl-Hirschman index of the banks operating in the local labor system
tang. assets	Tangible assets	Log of tangible assets
sales	Sales	Log of sales
risk	Risk (z-score)	Dummy variable equal to 1 if the firm is "risky", i.e. has a z-score greater than 6
ROA	Return on Assets	ROA Index (quartiles)
leverage	Leverage ratio	Debt to equity ratio (quartiles)
no. of requests	requests of first information	Number of requests of first information by banks
interests paid	interest/g.o.r	Net interest payments over gross operating revenue
productivity	Productivity index	Sales to employment costs ratio
share 5groups	Banks top groups loans	Share of bank loans originated by banks belonging to the top 5 groups
No. of senators	DC Senators	Number of Democrazia Cristiana Senators elected in 1948
South	South area	Dummy variable equal to 1 if the firm is headquartered in the South

Table A3. Descriptive statistics

	Total		*Construction*		*Service*		*Manufacturing*	
	mean	s.d	mean	s.d	mean	s.d	mean	s.d
access	0.13	0.34	0.16	0.36	0.12	0.33	0.14	0.35
urban LMA	0.68	0.47	0.63	0.48	0.71	0.45	0.61	0.49
fgap LMA	11.4	11.9	10.0	11.7	11.6	11.9	11.9	11.4
hhi LMA	0.04	0.04	0.04	0.04	0.04	0.04	0.03	0.03
tang. assets	469	10,185	151	1,945	215	3,782	765	5,981
sales	2,521	121,086	689	3,867	2,238	136,116	4,596	89,147
risk	0.28	0.45	0.3	0.4	0.3	0.4	0.3	0.5
roa	-0.37	140.94	3.7	30.7	-0.9	136.2	-1.5	231.9
leverage	32.0	234.4	35.9	243.1	31.7	248.2	26.4	161.6
no. of requests	1.74	1.34	1.7	1.3	1.7	1.3	1.8	1.6
no. of firms	45,153		5,311		32,647		5,276	

Notes: Access to credit is measured in 2013; balance sheet variables refer to 2012. See table A2 for the description of all variables.

Table A4. Firm distribution by region

	Estimation sample		*Universe of Italian limited liability companies*	
	No. of Firms	*%*	*No. of firms*	*%*
Piedmont	2,216	4.91	31,517	5.94
Aosta Valley	81	0.18	1,100	0.21
Lombardy	8,981	19.89	109,717	20.67
Trentino - Alto Adige	420	0.93	8,498	1.60
Veneto	3,863	8.56	49,688	9.36
Friuli Venezia Giulia	736	1.63	10,151	1.91
Liguria	920	2.04	10,646	2.01
Emilia Romagna	3,484	7.72	45,571	8.58
Tuscany	2,610	5.78	34,574	6.51
Umbria	530	1.17	7,313	1.38
Marche	825	1.83	13,624	2.57
Latium	7,499	16.61	70,681	13.31
Center & North	**32,165**	**71.24**	**393,080**	**74.04**
Abruzzo	889	1.97	9,926	1.87
Molise	179	0.4	1,757	0.33
Campania	4,428	9.81	45,468	8.56
Puglia	2,355	5.22	27,222	5.13
Basilicata	301	0.67	3,439	0.65
Calabria	936	2.07	9,475	1.78
Sicily	2,915	6.46	29,004	5.46
Sardinia	985	2.18	11,529	2.17
South & Islands	**12,988**	**28.76**	**137,820**	**25.96**
Total	**45,153**	**100**	**530,900**	**100**

Table A5. INA-House Plan investments

	homes per capita (1)	rooms per capita (1)	expend. 1st period per capita (2)	expend. 2nd period per capita (3)	expend. tot per capita (3)
no. of DC senators (4)	0.389*	1.855	0.379	0.799**	1.177**
	(0.228)	(1.223)	(0.248)	(0.377)	(0.583)
south (dummy)	0.123	0.789	-0.248	0.934	0.686
	(0.554)	(2.976)	(0.604)	(0.918)	(1.420)
constant	6.724***	36.78***	6.422***	10.95***	17.37***
	(0.390)	(2.093)	(0.425)	(0.645)	(0.999)
observations	92	92	92	92	92
R-squared	0.032	0.026	0.027	0.059	0.046

Notes: (1) Investments made over the 14 years of the INA-House plan at the province (92 units at the NUTS-3 geographical level) level. – (2). Investments made over the first 7 years of the INA-House plan at the province level. – (3). Investments made over the second 7 years of the INA-House plan at the province level. - (4) Number of of Democrazia Cristiana Senators elected in 1948 having their hometown in the province. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A6. Probability of being sampled

VARIABLES	(1)	(2)	(3)	(4)
urban LMA	0.0478			
	(0.0364)			
sales		0.00020		
		(0.00136)		
incidence of tang. ass			0.00037	
			(0.00037)	
productivity				0.00003
				(0.00097)
fgap LMA	-0.216	-0.187	-0.177	-0.168
	(0.154)	(0.139)	(0.133)	(0.129)
hhi LMA	12.26***	12.31***	12.20***	12.27***
	(1.525)	(1.514)	(1.473)	(1.442)
risk	0.00340***	0.00355***	0.00340***	0.00039
	(0.000985)	(0.00105)	(0.00103)	(0.00103)
sales (log)	-0.00576***	-0.00583***	-0.00671***	-0.00669***
	(0.00087)	(0.00096)	(0.00095)	(0.00093)
tang. assets (log)	-0.00497***	-0.00526***	-0.00634***	-0.00545***
	(0.00045)	(0.00056)	(0.00057)	(0.00064)
interests paid	-0.00016***	-0.00016***	-0.00020***	-0.00022***
	(0.00005)	(0.00006)	(0.00006)	(0.00007)
roa.Q2	0.00115	0.000736	0.00127	0.00131
	(0.00104)	(0.00114)	(0.00132)	(0.00125)
roa.Q3	-0.00056	-0.00120	-0.00213	-0.00322**
	(0.00108)	(0.00125)	(0.00140)	(0.00158)
roa.Q4	0.00879***	0.00837***	0.00743***	0.00641***
	(0.00151)	(0.00165)	(0.00200)	(0.00188)
leverage.Q2	-0.00517**	-0.00537**	-0.0113***	-0.0161***
	(0.00261)	(0.00265)	(0.00311)	(0.00352)
leverage.Q3	-0.0294***	-0.0300***	-0.0362***	-0.0399***
	(0.00396)	(0.00407)	(0.00451)	(0.00467)
leverage.Q4	-0.0432***	-0.0439***	-0.0495***	-0.0507***
	(0.00508)	(0.00520)	(0.00554)	(0.00557)
provincial dummies (103)	yes	yes	yes	yes
sector dummyes	yes	yes	yes	yes
Constant	-0.0935**	-0.0544**	-0.0296	-0.0331
	(0.0375)	(0.0243)	(0.0248)	(0.0242)
Observations	514315	513221	460451	403847
R-squared	0.381	0.378	0.381	0.39

Notes: Linear probability model, where the dependent variable is a dummy equal to 1 if the firm is part of the estimation sample and 0 otherwise. See table A2 for the description of all variables. ROA and leverage are expressed by means of four dummies, indicating whether the firm belongs
to four quartiles, with Q4 being the highest and Q1 being the lowest. Clustered (at the LMA level) standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

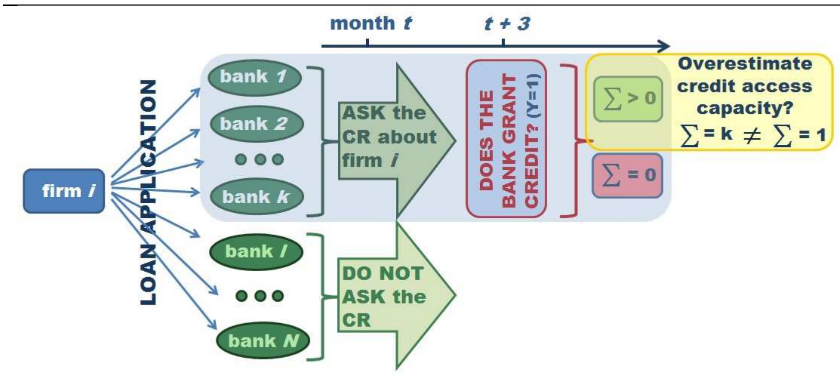
B. Measurement error of the dependent variable

Our dependent variable could suffer from measurement errors, which could lead to either overestimate or underestimate firm easiness of access to credit.

Overestimation and underestimation of easiness of access to credit

Let us discuss the overestimation case first. Consider, for instance, the case where firm i applies for a loan to N banks and that, amongst them, k banks turn to the service of "preliminary information request". In order to measure the easiness of access to credit for firm i, we check whether at least one of the k banks have granted credit within 3 months from the preliminary info request. If at least one of those banks granted the credit, our dependent variable takes value 1; otherwise, it is set to 0. While having a sharp (binary) measure of access to credit is convenient, it is important to notice that such procedure leads us to attach the same probability of access to credit to a firm when it obtains a loan from one bank out of k only (with all the remaining k-1 banks declining the credit), and when it obtains a loan offer from all k banks (figure B.1) We end up by overestimating firm i's easiness of access to credit in the first case. The overestimation could be even more serious, if the single bank approving the loan is offering an amount of credit that is well below the one initially asked by the firm. Even in this third case, our dependent variable would be set equal to 1.

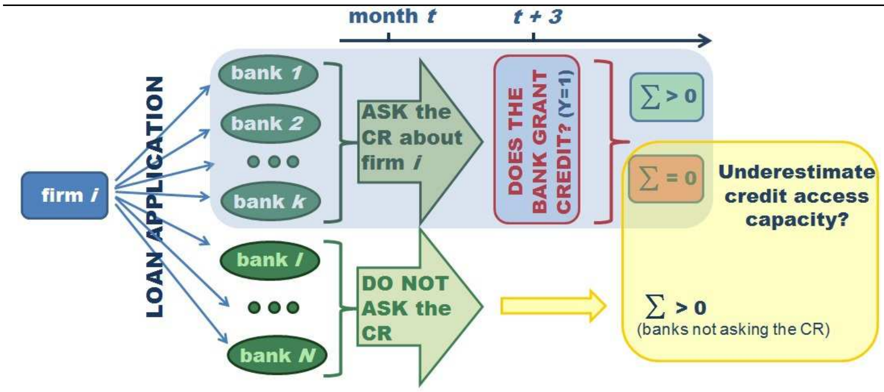
Figure B.1 – Overestimation of easiness of access to credit



Since the extent of potential overestimation of the easiness of access to credit is directly proportional to the number of k banks (i.e., the set of banks requesting info for firm i), we looked at the distribution of k in our dataset. In more than 80 percent of the cases we record only one request of first information for each firm (k=1), while the remaining cases have mostly k=2. Hence, we conclude that the risk of overestimating firm easiness of access to credit can be deemed as negligible.

Let us now focus on the risk of underestimating firm easiness of access to credit. As described in section 2, the "preliminary information request" procedure is costly and not mandatory for banks; hence, intermediaries can freely decide not to make an enquiry about the firm. This might happen, for instance, when the bank already has information about the potential borrower firm because it is (or it was) already borrowing from the bank, or because the firm is indirectly known to the bank (i.e., by means of another bank having lending relationship with the counterpart and belonging to the same banking group). If the firm borrows only from one of the banks that did not ask for preliminary information, our dependent variable would be set to zero, but the firm actually did get bank credit (figure B.2).

Figure B.2 – Underestimation of easiness of access to credit



Differently from the previous case, underestimation of the dependent variable is likely to be at work. In the reminder of this section we discuss more in depth the potential consequences and show why underestimating easiness of access to credit does not undermine our estimation framework.

Consequences of measurement error of the dependent variable

Since our dependent variable is measured with error, our estimates might be affected. In particular, the measurement error will appear in the new error term, with two consequences: (1) it will inflate the estimated variance, reducing the power of our statistical tests and inference (we may not find a significant effect even though it might be there in reality); (2) depending on whether the measurement error is systematically correlated or not with our independent variables, our OLS estimates will be or not be biased (Wooldridge, 2002).

While the only way to tackle the first consequence is to increase the sample size and collect more information, it is crucial to discuss the conditions under which the measurement error does not affect our estimates.

Let us focus on the (relevant) case where we underestimate the easiness of access to credit. As we explained above, this might happen if only a subset of banks decides to make the enquiry to the Credit register and the firm gets the loan from one of the banks that did not make the enquiry (because presumably the firm was already known by this bank). It is reasonable to assume that the underestimation error correlates with firm characteristics: for instance, more profitable, solid and less risky firms will have greater chances to get credit from the banks that already know them and that do not make a preliminary information request.

As the main aim of this work is to investigate the relationship between the firm being headquartered in a city (urban_LMA =1) and her capability to access bank credit, we would still get an unbiased estimate of the parameter of interest, β in equation (1), if (i) the measurement error is uncorrelated with the variable urban_LMA and (ii) the variable urban_LMA is uncorrelated with the other explanatory variables which are correlated with the measurement error (the latter is a weaker form of the requirement that the measurement error of the dependent variable should be uncorrelated with the full set of explanatory variables for the OLS estimate to be unbiased (Wooldridge, 2002).

In order to draw information on whether condition (i) holds, we devise a proxy for measurement error: such proxy is set equal to 1 for firms classified as having bad access to credit but whose total credit granted by all banks (regardless of whether they issued or not a request of preliminary information) grows within three months after the time of the request. The underestimation error does not show an evident correlation with the dummy urban_LMA. On the other hand, as expected, underestimation of access to credit correlates with other firm characteristics, such as the merit of credit. In particular, underestimation is larger when we consider low-risk firm, who presumably are well-known to the banks, which, in turn, rely less on the service of preliminary information to ground their decision of loan origination.

Concerning condition (ii), we can test it by looking at the pairwise correlation coefficients table. Table B.1 shows that the pairwise correlation coefficients between urban_LMA and the other explanatory variables are indeed all very close to zero, and the same result holds when we look at the pairwise correlations between urban_LMA and the sub-regional dummies.

Table B.1 - Pairwise correlations

	access	urban LMA	fgap LMA	hhi LMA	tang. assets	sales	risk	interest/g.o.r	roa	leverage	no of req
access	1										
urban LMA	-0.0278	1									
fgap LMA	-0.0287	0.2599	1								
hhi LMA	0.0058	-0.0409	-0.2253	1							
tang. assets	0.0464	-0.0632	-0.0246	0.0029	1						
sales	0.0648	0.0508	0.0054	-0.0309	0.3767	1					
risk	-0.0589	-0.0078	0.0217	-0.0017	-0.0205	-0.1921	1				
interest/g.o.r	0.0001	0.0013	-0.0012	0.0008	-0.0087	-0.008	-0.0061	1			
roa	0.015	-0.0021	-0.0002	0.0016	0.0271	0.0426	-0.1094	0.0022	1		
leverage	0.0005	-0.0086	-0.0045	0.0073	0.0054	-0.0326	0.0531	0.0007	0.0008	1	
no of req	0.0392	0.0037	0.0246	-0.0169	0.0924	0.1387	0.02	-0.0096	0.0043	0.0089	1

Notes: Access to credit is measured in 2013; balance sheet variables refer to 2012. See table A2 for the description of all variables.

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