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# State dependence in access to credit☆

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ABSTRACT

This paper investigates whether firms' access to credit is characterized by state dependence. We introduce a first-order Markov model of credit restriction with sample selection that makes it possible to identify state dependence in presence of unobserved heterogeneity. The results, based on a representative sample of Italian firms, show that state dependence in access to credit is a statistically and economically significant phenomenon and that this is more prominent among medium-large firms.



## 1. Introduction

It is a well-established finding that bank lending occurs in cycles. Robust evidence indicates that financial accelerator mechanisms *á la* Bernanke and Gertler (1989) and Kiyotaki and Moore (1997) are of primary importance for explaining economic fluctuations and firms' investment dynamics (Braun and Larrain, 2005; Liu et al., 2011; McLean and Zhao, 2014), and that bank lending standards are procyclical (Berger and Udell, 2004; Ruckes, 2004; Lown and Morgan, 2006; Gorton and He, 2008; de Bondt et al., 2010). Despite this broad consensus, the existence of lending cycles at the firm level is still a largely unexplored issue. This paper contributes to fill this gap by investigating whether firms' access to credit (specifically, the likelihood of applying for a loan and the outcome of loan applications) is characterized by state dependence in a representative sample of Italian manufacturing firms.

Borrowers are limited in their access to credit if they are restricted by banks in terms of quantity or price, or if they are discouraged from applying for a loan in anticipation of a future credit restriction (Jappelli, 1990). In a dynamic framework, state dependence in access to credit arises when a borrower whose loan application has been fully or partly restricted in the past exhibits a greater probability of being restricted and/or discouraged from applying for a loan in the future, relative to an identical firm whose access to credit was unrestricted.

An extensive empirical literature has investigated firms' access to credit and the determinants of loan applications and credit restrictions in a static context (Alessandrini et al., 2009; Han et al., 2009; Popov and Udell, 2012; Presbitero et al., 2014; Cole and Sokolyk, 2016). Very limited attention has been paid to whether and what extent firms can be "locked" in a state of credit restriction over time. A notable recent exception is Dougal et al. (2015,

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p. 1078), who analyze the spread on repeated loans registered in the Reuters Dealscan database from 1987 to 2008 and find that "the spread that a firm received on its most recent loan affects the spread it receives on a new loan, acting as an anchor", a pattern consistent with state dependence in firms' access to credit.[1]

The persistence in a state of credit restriction can be due to observed firm characteristics (i.e., firm size, risk, profitability), unobserved heterogeneity, and *true* state dependence (Heckman, 1981a). Unobserved heterogeneity reflects unmeasured firm attributes that may affect the likelihood of being restricted in access to credit in any period, but that are not driven by past credit constraints, such as the lack of entrepreneurial ability of managers or the lack of business opportunities. If these unobservables have some degree of persistence, they may originate the so-called *spurious* state dependence: other things being equal, past credit restrictions may turn out to be a significant predictor of the likelihood of present credit restrictions, even though they are only reflecting the influence of unobserved heterogeneity. In contrast, *true* state dependence refers to the fact that the very experience of a restriction in access to credit has a genuine causal effect on the risk of future restrictions.

Two broad, non-alternative mechanisms can explain *true* state dependence in access to credit. First, a restricted access to credit in a period can be associated with adverse changes in firms' characteristics and opportunities that make less profitable for banks to fund these firms in the future and less valuable for firms to apply for a loan. For example, an adverse shock to the borrower's productivity or to the credit supply can reduce the value of collateralizable assets (Bernanke and Gertler, 1989; Greenwald and Stiglitz, 1993; Kiyotaki and Moore, 1997). When credit markets are imperfect and applying for a loan is costly, a lower value of collateral hinders access to credit. Credit-constrained borrowers have to cut back investment and production levels, and the resulting decline in net worth further reduces their ability and willingness to borrow in the future. Another possibility is that a restriction in access to credit prevents the firm from exploiting a business opportunity and that the value of this opportunity decays over time as other competitors enter the market, making less worthy for banks to fund the project in the future (Levenson and Willard, 2000).

A second mechanism relates to information imperfections and screening technology frictions. To frame the empirical analysis, the online appendix presents a highly simplified information-based model of state dependence in access to credit. The intuition is simple. All lending decisions are made starting from some formal or informal imperfect test of borrowers' likelihood of default, like automated and semi-automated credit scoring models or soft-information-based screening technologies. To the extent that updating borrower information is costly or unfeasible (as it happens in the case of balance-sheet or other pieces of hard information), and switching banks is also a costly alternative or credit bureaus/registers reveal information on past credit ratings and rejections, banks' screening technologies result to be characterized by a degree of memory. This implies that the expected quality of a borrower that has applied for a loan in the past depends on the result of his/her previous credit-worthiness tests. Since such tests take on the lowest values for rejected borrowers, the likelihood that the current credit-worthiness test confirms the result of the previous test is higher for rejected than for non-rejected (and new) applicants. In addition, if loan application is costly, previously rejected borrowers may be discouraged from applying for a loan in the future, anticipating the higher probability of credit denial.

The investigation of phenomena that exhibit some form of persistence and the problem of isolating *true* state dependence effects are of increasing interest in many fields of economic research.[2] Besides controlling for the unobserved firm-specific characteristics and the possible dependence between the unobservables and the credit restriction status in the initial period (Heckman, 1981b), modeling state dependence in the context of access to credit requires dealing with two additional challenges. First, the use of standard binary response models might be inappropriate as firms demanding credit might be a non-random sample of population (Popov and Udell, 2012; Presbitero et al., 2014). In this view, ignoring the modeling of credit demand would produce inconsistent estimators of the transition probabilities into a credit restriction state. In addition, the economic significance of state dependence in access to credit would be understated by not considering the discouragement effect of past credit restrictions on current loan applications. A second concern is that the assumption of strict exogeneity of explanatory variables used in standard dynamic discrete-choice models is hardly tenable, and the presence of feedback effects from previous credit restrictions on future firm characteristics, such as firm size, export orientation and level of available liquidity, may hinder identification of the *true* state dependence in access to credit.

To deal with these issues, we develop a first-order Markov model for state dependence in access to credit with selection bias and unobserved heterogeneity. In each period $t$ a firm can: (i) apply for credit and receive the requested amount; (ii) apply for credit and not receive the requested amount or receive it at more onerous terms (hereafter, we label this outcome credit restriction and these firms as restricted applicants); or (iii) not to apply for credit. We specify two binary outcome equations: one for the bank lending decision and the other for the firm credit demand at time $t$. To control for the presence of unobserved heterogeneity, we adopt a random-effects approach, with a full parametrization of the variance structure. Then, in the spirit of Heckman (1981b), we control for the correlation between the unobserved effects and the initial state of restriction by specifying two more equations for credit demand and supply as initial conditions. Finally, to circumvent the assumption of strict exogeneity of observables and address the concern for possible feedback effects of previous credit restriction on firms' characteristics, we follow the approach suggested by Cappellari and Jenkins (2004) and estimate a period-to-period reduced-form model on a dataset of pooled transitions, where the observation unit is the firm observed for every possible pair of consecutive periods.

We apply our model to a representative sample of Italian manufacturing firms, surveyed by the National Institute of Statistics (ISTAT). This survey provides detailed information on firm loan demand and access to credit, location and other firm characteristics on a quarterly basis from 2008:q2 to 2009:q4. Unfortunately, survey data do not contain very specific and detailed information about the screening process, switching costs, firms' net worth and returns. Besides, the identity of surveyed companies is undisclosed to researchers, and thus we cannot add information from firms' balance-sheets and other data sources. Hence, while it is possible to provide evidence of whether and to what extent there is *true*

[1] Levenson and Willard (2000, p. 91) explicitly recognize that "credit rationing has a duration dimension", even though their empirical analysis – based on a static two-stage probit model for the probability of credit denial and loan application – does not allow to test for state dependence in access to credit.

[2] For example, models accounting for state dependence have been used to study unemployment and wage dynamics (Heckman and Borjas, 1980; Stewart and Swaffield, 1999), labor market participation (Hyslop, 1999), poverty transitions (Cappellari and Jenkins, 2004), self-assessed health condition (Carro and Traferri, 2014), remittance decisions (Bettin and Lucchetti, 2016), and households' financial distress (Giarda, 2013; Brown et al., 2014).

state dependence in access to credit, it is very difficult to distinguish empirically between the two (non-alternative) explanations for state dependence in access to credit: the adverse evolution of firms' net-worth and the existence of frictions in the screening technology.

In this respect, however, some indirect clues can be gained by looking at the type of firms which suffer more of state dependence in access to credit and at the degree of state dependence in tranquil and crisis periods.

In the net-worth explanation, state dependence in access to credit is triggered by a shock that decreases the value of firm assets, locking the firm into a long-lasting credit trap. To the extent that these shocks are more likely in crisis than in tranquil periods, state dependence in access to credit should be a more pervasive phenomenon in the former than in the latter periods. In addition, if financial frictions are more marked for small and informationally opaque borrowers, these firms are not only more likely to be unconditionally credit-constrained, but also more likely to be locked in a credit restriction state due to the decreasing value of their collateralizable wealth (Gertler and Gilchrist, 1993, 1994).

In the screening-friction explanation, state dependence in access to credit is a feature of credit markets also in tranquil periods. Moreover, small and opaque firms could be more likely to escape from a credit restriction state than large borrowers (although, unconditionally, the former have a higher probability of being credit-restricted), while they could be more likely to be discouraged from applying for a loan, once having experienced a credit restriction. This reflects the greater noisiness and lower stickiness of screening tests for small firms, that lead a bank to be more inclined to review the results of its previous assessments on old small customers currently borrowing from the bank, and less prone to accept the negative judgments on new applicants signaled by other banks that have (arguably) rejected them. In small business lending, hard information tend to be scarcer and less reliable, and banks make a large use of soft information and relation lending technologies. Soft information delivers noisy signals of the firms' credit-worthiness, and it is updated more frequently than hard information since it is typically obtained by close and continuous interactions between the lender and the borrower, thus, for both reasons, decreasing the likelihood of the latter of being locked in a bad assessment state. In addition, SMEs often rely on lending relationships with local small-sized banks (Berger et al., 1995; Strahan and Weston, 1998; Scott, 2004; Alessandrini et al., 2008; Presbitero and Zazzaro, 2011), which could be inclined to revise a negative assessment on their customers more rapidly than transactional banks lending to large enterprises. At the same time, however, the information-based model presented in the online appendix shows that the noisier the credit-worthiness signal received from the screening test, the higher the probability that a (small) firm is discouraged from applying for a loan after having experienced a credit denial. A stronger discouragement effect for small than for large firms can also be the results of higher loan application and switching costs for small than for large firms (Kon and Storey, 2003; Han et al., 2009; Barone et al., 2011).

In view of these considerations, in the empirical analysis we address three main questions. First, we test for the degree of state dependence in credit restriction at the firm level (i.e., the probability that credit supply is restricted at time $t$ conditional on having experienced a restriction in $t-1$) and for the strength of the discouragement effect (i.e., the probability that a firm does not apply for credit at time $t$ given that its application has been restricted in $t-1$), washing out other sources of persistence due to observed and unobserved heterogeneity. Second, we assess whether and to what extent state dependence in credit restriction and the discouragement effect are heterogeneous with respect to firm size. Third, we examine whether state dependence in access to credit is triggered by major global liquidity shocks or occurs also in tranquil periods.

Our main results can be summarized as follows. *First*, we find evidence that state dependence in access to credit is a statistically and economically significant phenomenon in the Italian credit market. *Second*, consistent with the information-based interpretation of state dependence in access to credit, we find that small firms are more likely to escape from a credit restriction state, but they are also more likely to be discouraged from applying for a new loan after experiencing a credit restriction. *Third*, we find that the bankruptcy of Lehman Brothers in September 2008 produced not only a crunch in the credit supply, as documented in other studies on Italy and other European countries (Puri et al., 2011; Jiménez et al., 2012; Popov and Udell, 2012; Iyer et al., 2014; Presbitero et al., 2014), but also exacerbated the persistence of credit restrictions for Italian firms. However, we also find that state dependence in access to credit is not limited to times of crisis, as firms can be locked in a state of credit restriction even in tranquil periods. Overall, although our data do not allow us to empirically discriminate between the different theories of state dependence in firms' access to credit, we interpret these results as providing evidence in favor of the relevance of the screening-failure explanation.

The rest of the paper is organized as follows: In Section 2 we introduce the first-order Markov model with sample selection and our measures of state dependence in credit denial and discouragement effect. In Section 3 we present the dataset and variables. In Section 4 we discuss the estimation results. The final section concludes.

## 2. Empirical strategy

In order to identify and consistently estimate the *true* state dependence in firms' access to credit and the discouragement effect, three specific issues need to be addressed: (i) the presence of unobserved heterogeneity; (ii) sample selection bias arising from the non-random decision to apply for a loan; (iii) possible feedback effects of past restrictions on observable firm characteristics that make the assumption of strict exogeneity problematic. Once state dependence and discouragement effect have been estimated, we quantify and test their magnitude by deriving measures that provide meaningful interpretation in terms of partial effects.

In the remainder of this section, we first discuss the critical modeling issues in a general framework, then derive a first-order Markov model that accounts for unobserved heterogeneity, sample selection and feedback effects, and finally illustrate possible measures of state dependence in credit restriction and discouragement effect.

### 2.1. Identification issues

The starting point to identify state dependence in access to credit is to specify the process

$$\begin{aligned} r_{it} &= \mathbf{x}_{it}'\boldsymbol{\tau} + \gamma R^*_{it-1} + \alpha_i + \varepsilon_{it} \\ R_{it} &= \mathbf{I}(r_{it} > 0) \quad i = 1, \ldots, N \quad t = 2, \ldots, T \end{aligned} \tag{1}$$

where $r_{it}$ is the latent propensity to be credit constrained for firm $i$ at time $t$ which we observe as $R_{it} = \mathbf{I}(r_{it} > 0)$, where the function $\mathbf{I}(\cdot)$ indicates whether firm $i$ experiences a restriction of credit availability in $t$ ($R_{it} = 1$) or not ($R_{it} = 0$). The vector $\mathbf{x}_{it} \equiv (1, x_{1it}, \ldots, x_{Kit})$ includes $K$ time-varying and time-invariant characteristics at the firm and market level. The vector $\boldsymbol{\tau}$ contains regression parameters and $\gamma$ is the state dependence parameter. In our case, $R_{it-1}$ is not observed for those firms that did not apply in $t-1$. Therefore, we substitute the lagged restriction outcome $R_{it-1}$ with an "actual-restriction" state variable $R^*_{it-1}$, that takes value 1 for firms which state they applied for credit and experienced an actual restriction in credit supply in $t-1$ and zero for non-rejected applicants and for those firms that did not apply for credit in $t-1$, whose possible rationing outcome is unobservable. In practice, we separate firms that were actually screened and negatively valued by banks in $t-1$ from those who were considered creditworthy or were not subject to any creditworthiness test by banks, irrespective of whether they had no credit needs or were discouraged from applying for a loan. Finally, the term $\alpha_i$ represents the time-invariant firm unobserved heterogeneity and $\varepsilon_{it}$ is an iid shock with zero mean and unit variance and independent of $\alpha_i$. Here we assume that both $\alpha_i$ and $\varepsilon_{it}$ are independent of $\mathbf{X}_i = [\mathbf{x}_{i1}, \ldots, \mathbf{x}_{iT}]$.

Apart from the effect of observable covariates, this specification contains both the sources of persistence in $r_{it}$: the *true* state dependence $\gamma$, that is the genuine effect of experiencing a credit restriction in $t-1$ on $Pr(R_{it} = 1)$, and the *spurious* state dependence, that is the propensity to experience a credit restriction event at all times, represented by $\alpha_i$ (Heckman, 1981a). With respect to the static context, where the unobserved heterogeneity is either eliminated by conditioning the joint probability of $\boldsymbol{R_i} = [R_{i1}, \ldots, R_{iT}]$ on a sufficient statistic for $\alpha_i$ or integrated out, the presence of $\alpha_i$ in a dynamic framework additionally raises the so-called "initial conditions problem", that derives from the correlation between the unobserved heterogeneity and the initial realization of the process $R_{i1}$. In other words, if we were to assume that the initial state of firms concerning access to credit is independent of $\alpha_i$, we could estimate a dynamic model for access to credit without having to deal with the initial conditions problem. However, the exogeneity assumption could be only appropriate when the length of the time-series is adequate for asymptotics. With $T$ fixed and relatively short-length – as in our sample, covering eight quarters –, the assumption of independence between the firm's initial state and its unobservable specific characteristics is barely tenable.

Also, a possible source of misspecification in (1) is the presence of sample selection bias: some firms can non-randomly decide not to apply for a loan, giving rise to a possible source of selection bias when estimating the probability of being credit restricted. To address this problem, we need to specify an additional process of the form

$$\begin{aligned} d_{it} &= \mathbf{w}_{it}'\boldsymbol{\xi} + \phi R^*_{i,t-1} + \eta_i + u_{it} \\ D_{it} &= \mathbf{I}(d_{it} > 0) \quad \text{for} \quad i = 1, \ldots, N \quad t = 2, \ldots, T, \end{aligned} \tag{2}$$

where $d_{it}$ is the latent propensity to apply for a loan which is observed as $D_{it} = \mathbf{I}(d_{it} > 0)$ and $R_{it}$ in (1) is observed only if $D_{it} = 1$, the vector $\mathbf{w}_{it} \equiv (1, x_{1it}, \ldots, x_{Kit}, w_{1it}, \ldots, w_{Mit})$ consists of the $K$ covariates in $\mathbf{x}$ and $M$ suitable exclusion restriction variables affecting the firm decisions to apply for a loan but not directly influencing bank willingness to grant the requested amount of credit, and $R^*_{i,t-1}$ is defined as in (1). The vector $\boldsymbol{\xi}$ contains the regression parameters and $\phi$ represents the discouragement effect. The unobserved error terms $\eta_i$ and $u_{it}$ have the same interpretation as $\alpha_i$ and $\varepsilon_{it}$ in (1), respectively. Furthermore, $u_{it}$ is independent of $\eta_i$ and we assume that both the error terms are independent of $\mathbf{W}_i = [\mathbf{w}_{i1}, \ldots, \mathbf{w}_{iT}]$. A sample selection problem arises because there is cross-equation dependence due to the correlation in the time-invariant and time-varying error terms, meaning that $\mathrm{E}[\alpha_i\eta_i] \neq 0$ and $\mathrm{E}[\varepsilon_{it}u_{is}] \neq 0$, for $t, s = 2, \ldots, T$.

Finally, we must take into account the possible presence of feedback effects of past credit restrictions on observable covariates that would not allow for the identification of state dependence and discouragement effect in access to credit. Standard approaches to the estimation of dynamic models require the assumption of strict exogeneity (conditional on time-invariant unobserved effects) to hold, that is $Pr(R_{it}|\mathbf{X_i}, R^*_{i,t-1}, \alpha_i) = Pr(R_{it}|\boldsymbol{x}_{\mathbf{it}}, R^*_{i,t-1}, \alpha_i)$. In practice, past values of the response variable are thought not to influence current values of covariates. In our case, this assumption is likely to be violated since the experience of being restricted in credit availability in the past may affect the current values of some firms' characteristics (for instance, liquidity, production level and even firm size) that, in turn, could influence the bank's decision to accept or reject the current loan applications.

### 2.2. A first-order Markov model for state dependence in access to credit

In order to address the identification issues (i)–(iii) discussed above, we specify a model that accounts for selectivity bias, endogenous initial conditions and allows us to identify the state dependence in presence of unobserved heterogeneity and possible feedback effects. Precisely, in the spirit of Cappellari and Jenkins (2004), we specify a first-order Markov model for credit demand and supply based on a dataset of pooled transitions, where the observation unit is the firm observed for every possible pair of consecutive quarters.[3] Then, we formulate the two-period model for firm transitions between different states in the credit market in $t-1$ and $t$. In each period, any firm is in one of the following states: (i) it applies to a bank for a loan and receives the requested amount; (ii) it applies for a loan, but its application is not fully accepted by the bank in terms of quantity and/or interest rate (hereafter, we define such firms as credit restricted); and (iii) it does not apply for credit to any bank.

We specify two binary outcome equations at time $t$, one for credit restriction and one for credit demand. We account for the initial conditions problem by following the reduced-form approach with a linearized index function of Heckman (1981b), that is we specify two additional equations for credit demand and supply in $t-1$, where $t-1$ is the initial period in the first-order Markov model. In this framework we are able to account for the presence of unobserved heterogeneity and sample selection; covariates are allowed to be predetermined since strict exogeneity is circumvented by truncating the process at the second period and using the values of covariates in the first of the two periods considered.

In symbols, our model is specified as

$$r_{it} = \boldsymbol{x}_{it-1}'\boldsymbol{\beta} + \gamma R^*_{it-1} + \alpha_i + \varepsilon_{it} \tag{3}$$

$$d_{it} = \boldsymbol{w}_{it-1}'\boldsymbol{\delta} + \phi R^*_{it-1} + \eta_i + u_{it} \tag{4}$$

$$r_{it-1} = \boldsymbol{z}_{it-1}'\boldsymbol{\pi} + \theta\alpha_i + \nu_{it-1} \tag{5}$$

$$d_{it-1} = \boldsymbol{q}_{it-1}'\boldsymbol{\lambda} + \psi\eta_i + \vartheta_{it-1} \quad i = 1, \ldots, N \quad t = 1, \ldots, T, \tag{6}$$

where the left-hand side terms are defined as in (1) and (2) and $R_{it}$, $D_{it}$, $R_{it-1}$, and $D_{it-1}$ follow the same observational rules. To solve the initial conditions problem, we specified two reduced-form equations (for the application decision and the primary outcome) with a linearized index function following Heckman (1981b) where $\mathbf{z}_{it-1} \equiv (1, z_{1it-1}, \ldots, z_{Kit-1})$ and $\mathbf{q}_{it-1} \equiv (1, z_{1it-1}, \ldots, z_{Kit-1}, q_{1it-1}, \ldots, q_{Mit-1})$ are vectors of covariates (that may include exclusion restrictions) and $\boldsymbol{\pi}$ and $\boldsymbol{\lambda}$ are regression parameters. The parameters $\theta$ and $\psi$ capture the correlation between the unobserved heterogeneity, $\alpha_i$ and $\eta_i$, and the initial observations $R_{it-1}$ and $D_{it-1}$, respectively. The error terms $\varepsilon_{it}$ and $u_{it}$ follow the same assumptions laid out in Section 2.1. In addition, $\nu_{it-1}$ and $\vartheta_{it-1}$ are iid zero mean error terms independent of $\alpha_i$, $\eta_i$, and of the model's covariates.

[3] Cappellari and Jenkins (2004) specify a trivariate dynamic probit model for poverty transitions between two consecutive periods with sample attrition. Their trivariate specification includes a selection equation to control for the bias generated by sample attrition. By accounting for the selection bias generated by the credit demand, we extend their framework so as to include the selection equation in the initial conditions as well.

In order to estimate the parameters in (3)–(6), we rely on full information maximum likelihood, and therefore a distributional assumption on the error terms has to be made. In this respect, we assume that the vector of composite error terms, $[\alpha_i+\varepsilon_{it}, \eta_i+u_{it}, \theta\alpha_i+\nu_{it-1}, \psi\eta_i+\vartheta_{it-1}]$ is a quadrivariate normal random variable with zero mean and an appropriate $4\times 4$ covariance matrix. Since we are considering a two-period model, not all the parameters can be identified. For instance, with only two periods we cannot disentangle the variance of $\alpha_i$ and the correlation between $\varepsilon_{it}$ and $\nu_{it-1}$, that is we are not able to identify whether persistence in unobservables is due to the time-invariant or to the time-varying unobserved heterogeneity.[4] In addition, diagonal elements need to be normalized to unity (see Cappellari and Jenkins, 2004, for further details). The off-diagonal correlations are as follows:

- $\rho_{21}=\mathrm{E}[(\theta\alpha_i+\nu_{it-1})(\psi\eta_i+\vartheta_{it-1})]$ and $\rho_{43}=\mathrm{E}[(\alpha_i+\varepsilon_{it})(\eta_i+u_{it})]$ capture the correlation due to the selection bias in $t-1$ and $t$, respectively (i.e., the correlation between the error terms in (5) and (6) and in (3) and (4)).
- $\rho_{31}=\mathrm{E}[(\eta_i+u_{it})(\psi\eta_i+\vartheta_{it-1})]$ and $\rho_{42}=\mathrm{E}[(\alpha_i+\varepsilon_{it})(\theta\alpha_i+\nu_{it-1})]$ measure the correlation between the firm's status in $t-1$ and $t$ in credit demand and in credit restriction respectively (i.e., the correlation between the error terms in (4) and (6) and in (3) and (5)).
- $\rho_{32}=\mathrm{E}[(\eta_i+u_{it})(\theta\alpha_i+\nu_{it-1})]$ and $\rho_{41}=\mathrm{E}[(\alpha_i+\varepsilon_{it})(\psi\eta_i+\vartheta_{it-1})]$ capture the sample selection correlation across $t$ and $t-1$ (i.e., the correlations between the error terms in (4) and (5) and in (3) and (6)).

This way, individual unobserved heterogeneity is accounted for and fully parametrized by the above correlation structure, and the parameters of interest $\gamma$ and $\phi$ remain identified.

Under the assumption of joint normality, the first-order Markov model (3)–(6) is estimated as a quadrivariate probit model where (4) and (6) are selection equations. We estimate the parameter vector $[\boldsymbol{\beta}', \gamma, \boldsymbol{\delta}', \phi, \boldsymbol{\pi}', \boldsymbol{\lambda}', \mathrm{vech}(\mathbf{C})']'$, where $\mathbf{C}$ is the lower triangular Cholesky of $\boldsymbol{\Sigma}=unvech(1, \rho_{21}, \rho_{31}, \rho_{41}, 1, \rho_{32}, \rho_{42}, 1, \rho_{43}, 1)$, by Simulated Maximum Likelihood (SML). The contribution of firm $i$ to the log-likelihood is $\ell_i=\ln(P_i)$ with

$$\ell_i=\ln(P_i)=\ln[\Phi_4(\mathbf{a}_i, \mathbf{b}_i, \mathbf{C})] \tag{7}$$

where $\Phi_4(\cdot)$ is the quadrivariate standard normal distribution function. Full expressions for the integral bounds $\mathbf{a}_i$ and $\mathbf{b}_i$ are given in the online appendix B. The probability $\Phi_4(\cdot)$ is simulated using the GHK algorithm, with 200 replications (Geweke, 1989; Keane, 1994; Hajivassiliou and McFadden, 1998). Standard errors are obtained using the sandwich formula. In the first-order Markov model formulation, the estimation of a quadrivariate probit model on pooled transitions is equivalent to estimating a random effect sample selection dynamic probit with $T=2$ and linearized initial conditions as in Heckman (1981b), where the use of GHK instead of standard quadrature methods is necessary since unobserved heterogeneity is accounted for by a suitable parametrization of the correlation structure.

Other estimators for dynamic binary (short)-panel data models proposed in the literature that allow for the covariates to be predetermined rather than strictly exogenous suffer from some limitations that make them not easily applicable to our context.[5] First, the approach proposed by Wooldridge (2000) to jointly estimate the parameters of the model for the response variable and predetermined covariates cannot be easily extended to a model with two response variables (credit restriction and loan application) and requires the formulation of a model for feedback effects for all the predetermined covariates, whose number is very large in our case.[6] Second, the approach proposed by Honoré and Lewbel (2002) requires that a continuous exogenous instrument is available, a condition which we are not able to meet in our dataset as present values of all the available firms' characteristics could be in principle influenced by past restriction events. Finally, the semiparametric estimator of Arellano and Carrasco (2003) considers only a small number of explanatory variables and it is hardly feasible with a set of regressors as large as ours (see also Biewen, 2009, for a review).

Our empirical approach is not without costs. First, by assuming that the process of credit restriction events lasts two periods, we do not fully exploit the longitudinal structure of the dataset, inducing a loss of estimation efficiency. Second, we cannot identify feedback effects, and third, we cannot disentangle the persistence in time-varying unobserved heterogeneity from the persistence due to time-invariant unobserved heterogeneity. Nevertheless, since our main objective is to test for and quantify state dependence and discouragement effect in access to credit, these shortcomings are not problematic in our context.

### 2.3. Measuring state dependence in credit restriction and discouragement effect

Other than testing for state dependence in access to credit, we are interested in evaluating the magnitude of state dependence in credit restriction in terms of probability changes. We compute state dependence in credit restriction as the average difference between the probability that the loan application has not been fully accepted by banks at time $t-1$ and, alternatively, the probability of credit restriction in $t$ conditional on not having applied for a loan $t-1$ ($\overline{SD}_{R_{t-1}=0}$) or the probability of having requested and obtained credit in $t-1$ ($\overline{SD}_{D_{t-1}=0}$). Using the notation of model (3)–(6):

$$\overline{SD}_{R_{t-1}=0}=\frac{1}{N}\sum_{i=1}^{N}\left[\frac{Pr(R_{it}=1, D_{it}=1, R_{it-1}=1, D_{it-1}=1)}{Pr(R_{it-1}=1, D_{it-1}=1)}-\frac{Pr(R_{it}=1, D_{it}=1, R_{it-1}=0, D_{it-1}=1)}{Pr(R_{it-1}=0, D_{it-1}=1)}\right] \tag{8}$$

$$\overline{SD}_{D_{t-1}=0}=\frac{1}{N}\sum_{i=1}^{N}\left[\frac{Pr(R_{it}=1, D_{it}=1, R_{it-1}=1, D_{it-1}=1)}{Pr(R_{it-1}=1, D_{it-1}=1)}-\frac{Pr(R_{it}=1, D_{it}=1, D_{it-1}=0)}{Pr(D_{it-1}=0)}\right] \tag{9}$$

Similarly, we compute the discouragement effect as the average difference between the probability of not applying for credit in $t$ conditional on having experienced a credit restriction in $t-1$

[4] For instance, the random-effects dynamic probit models put forward by Hyslop (1999) and Keane and Sauer (2009) accommodate both permanent unobserved heterogeneity $\alpha_i$, parametrized in the covariance matrix by its variance (say, $\sigma_\alpha^2$), and autocorrelation in the time-varying error term. Clearly, when the time series length is equal to 2, the two can not be identified separately.

[5] Fixed-effects or conditional approaches for estimating dynamic binary choice models all require the model covariates to be strictly exogenous conditional on the unobserved heterogeneity (Chamberlain, 1985; Honoré and Kyriazidou, 2000; Wooldridge, 2005; Bartolucci and Nigro, 2010), whereas standard random-effects approaches (Heckman, 1981b; Hyslop, 1999; Keane and Sauer, 2009) would become extremely expensive to implement in software given the high dimension of the problem. To the best of our knowledge, there is no clear evidence on the behavior of random-effects models when the strict exogeneity assumption is violated.

[6] For instance, Biewen (2009) derives his joint model for the response variable and only two binary predetermined regressors, we should consider production, liquidity, size, export, etc. which, in our dataset, are measured as continuous, binary or ordinal.

and, alternatively, the probability of not applying for credit in $t$ conditional on having obtained credit in $t-1$ ($\overline{DE}_{R_{t-1}=0}$), or the probability of no application in either $t$ or $t-1$ ($\overline{DE}_{D_{t-1}=0}$):

$$\overline{DE}_{R_{t-1}=0} = \frac{1}{N}\sum_{i=1}^{N}\left[\frac{Pr(D_{it}=0, R_{it-1}=1, D_{it-1}=1)}{Pr(R_{it-1}=1, D_{it-1}=1)} - \frac{Pr(D_{it}=0, R_{it-1}=0, D_{it-1}=1)}{Pr(R_{it-1}=0, D_{it-1}=1)}\right] \quad (10)$$

$$\overline{DE}_{D_{t-1}=0} = \frac{1}{N}\sum_{i=1}^{N}\left[\frac{Pr(D_{it}=0, R_{it-1}=1, D_{it-1}=1)}{Pr(R_{it-1}=1, D_{it-1}=1)} - \frac{Pr(D_{it}=0, D_{it-1}=0)}{Pr(D_{it-1}=0)}\right] \quad (11)$$

We can test the statistical significance of state dependence in access to credit by calculating the standard errors for expressions (8)–(11) using the Delta Method.

It is worth noting that these indicators of state dependence – which correspond to the Genuine State Dependence computed by Cappellari and Jenkins (2004) – control for individual heterogeneity, because they are functions of differences in individual probabilities, which are then averaged over the whole sample. These measures differ from aggregate measures of state dependence and discouragement effects (hereafter ASD and ADE), which can be computed taking the differences between the model transition rates:

$$ASD_{R_{t-1}=0} = \left[\frac{\sum_{i=1}^{N} R_{it}R_{it-1}Pr(R_{it}=1, D_{it}=1|R_{it-1}=1, D_{it-1}=1)}{\sum_{i=1}^{N} Pr(R_{it-1}=1, D_{it-1}=1)}\right] - \left[\frac{\sum_{i=1}^{N} R_{it}(1-R_{it-1})Pr(R_{it}=1, D_{it}=1|R_{it-1}=0, D_{it-1}=1)}{\sum_{i=1}^{N} Pr(R_{it-1}=0, D_{it-1}=1)}\right] \quad (12)$$

$$ASD_{D_{t-1}=0} = \left[\frac{\sum_{i=1}^{N} R_{it}R_{it-1}Pr(R_{it}=1, D_{it}=1|R_{it-1}=1, D_{it-1}=1)}{\sum_{i=1}^{N} Pr(R_{it-1}=1, D_{it-1}=1)}\right] - \left[\frac{\sum_{i=1}^{N} R_{it}(1-D_{it-1})Pr(R_{it}=1, D_{it}=1|D_{it-1}=0)}{\sum_{i=1}^{N} Pr(D_{it-1}=0)}\right] \quad (13)$$

$$ADE_{R_{t-1}=0} = \left[\frac{\sum_{i=1}^{N}(1-D_{it})R_{it-1}Pr(D_{it}=0|R_{it-1}=1 D_{it-1}=1)}{\sum_{i=1}^{N} Pr(R_{it-1}=1, D_{it-1}=1)}\right] - \left[\frac{\sum_{i=1}^{N}(1-D_{it})(1-R_{it-1})Pr(D_{it}=0|R_{it-1}=0, D_{it-1}=1)}{\sum_{i=1}^{N} Pr(R_{it-1}=0, D_{it-1}=1)}\right] \quad (14)$$

$$ADE_{D_{t-1}=0} = \left[\frac{\sum_{i=1}^{N}(1-D_{it})R_{it-1}Pr(D_{it}=0|R_{it-1}=1 D_{it-1}=1)}{\sum_{i=1}^{N} Pr(R_{it-1}=1, D_{it-1}=1)}\right] - \left[\frac{\sum_{i=1}^{N}(1-D_{it})(1-D_{it-1})Pr(D_{it}=0|D_{it-1}=0)}{\sum_{i=1}^{N} Pr(D_{it-1}=0)}\right]. \quad (15)$$

By comparing $\overline{SD}$ and $\overline{DE}$ with ASD and ADE, respectively, we can draw indications about the relative importance of state dependence effect and firms' observed and unobserved heterogeneity in explaining the overall persistence in the probability of being credit restricted and of being out of the credit market. In particular, if $\overline{SD}$ ($\overline{DE}$) resulted to be similar to ASD (ADE), the greater difficulties encountered by firms experiencing a credit restriction to access credit in the future should be ascribed to a large extent of a state dependence effect.

## 3. Data and variables

We draw the data from the monthly "Survey on manufacturing firms' confidence" run by the ISAE (Institute of Studies and Economic Analysis), now part of the ISTAT (Italian Institute of Statistics). These data were recently re-engineered for comparability with data released by other European institutions, such as the Ifo Business Climate Survey, consistent with maintaining a focus on the traditional sectors of Italian specialization (Malgarini et al., 2005). The representative sample is stratified by geographical area, economic activity and number of employees. The survey covers about 4000 Italian manufacturing firms with at least five employees, interviewed from March 2008 to March 2010. Data are available at the firm level, on a quarterly basis (March, June, September and December releases).[7] The availability of a survey run at a quarterly frequency is a better setting to investigate state dependence in access to credit than standard structural surveys run on an annual or pluri-annual basis, as many events that can affect the firm entry and exit from the credit restriction and discouragement states may occur over such a long time frame.

The dataset provides information about several firm characteristics, even though, for reasons of confidentiality, there are no firm identifiers and no information on lending banks. Therefore it is impossible to match the data with other sources in order to obtain, for example, firm and bank balance sheet information. Thanks to the availability of data about firm location, at the administrative province level, we link the ISAE/ISTAT dataset with monthly data on bank branch openings and closures (at the bank-province level) compiled by the Bank of Italy, and with data on regional real GDP published by ISTAT.

Excluding 2010 because of outliers and observations with missing values in the variables of interest, we have 3893 firms observed quarterly between 2008:q1 to 2009:q4 (unbalanced). The estimation of the model (3)–(6), presented in Section 2, is based on the sample of pooled transitions. Therefore, we reshape our dataset such that the observation unit is the firm observed for every possible pair of consecutive quarters $t$ and $t-1$ from 2008:q2 to 2009:q4. In this way, we end up with a sample of 24,080 observations.

The survey has a specific section on firms' access to credit with information on firm demand for credit and bank lending decisions, so that we can distinguish between the demand for and supply of bank credit. We measure demand for credit by an indicator variable ($D$) which assumes the value 1 for firms that directly contacted one or more banks in the previous quarter in order to apply for credit (i.e., we exclude firms stating that they just went to the bank to ask for information).[8]

Restricted applicants are only among firms that applied for a loan in a given quarter and they are identified by a dummy variable for credit restriction that is equal to one for firms that did not obtain the desired amount of bank credit ($R$).[9] Lacking loan-level data, our variables on loan demand and credit restriction do not refer to a specific bank–firm relationship.

Table 1 summarizes the sample composition by firm state in $t$ and $t-1$ for the complete sample of pooled transitions. The marginal frequency of restricted applicants at time $t$ in our sample (of applicant firms) is 20.1%, while the credit restriction frequency in $t$ conditional on having experienced past credit restriction is

[7] Additional information on the survey is available here: http://siqual.istat.it/SIQual/visualizza.do?id=888894.

[8] To be precise, the survey section on access to bank credit includes three main questions:

**Table 1**
Sample composition by state transitions. We use data from a survey conducted by the ISAE (Institute of Studies and Economic Analysis), recently becoming part of the ISTAT (Italian Institute of Statistics). About 4000 Italian manufacturing firms, with a minimun of 5 employess, are interviewed monthly from March 2008 to February 2010. The data are available at the firm–level, but only on a quarterly basis (the March, June, September and December releases). The ISAE dataset is linked with monthly data on bank branch openings and closures compiled by the Bank of Italy. These data are at the bank–province level. After excluding 2010 because of outliers and missing values, our sample consists 3893 firms observed quarterly between 2008:1 and 2009:4 (unbalanced). In order to estimate the proposed model, we build a dataset of pooled transitions: the observation unit is the firm observed for every possible pair of consecutive quarters $t$ and $t-1$ from 2008:q2 to 2009:q4. This table summarizes the sample composition by firms' state in $t$ and $t-1$ for the final sample of 24,080 pooled transitions.

| Quarter $t-1$ | Quarter $t$ | | | Obs. |
|---|---|---|---|---|
| | No demand | Not rationed | Rationed | |
| No demand | 78.92% | 17.13% | 3.94% | 16,861 |
| Not rationed | 53.99% | 41.13% | 4.87% | 5910 |
| Rationed | 46.68% | 18.79% | 34.53% | 1309 |
| Obs. | 17,109 | 5566 | 1405 | 24,080 |

34.6%. This means that 80% of restricted applicants in the quarter $t$ come from the pool of firms that were credit restricted in the previous quarter. The conditional frequency of credit restricted applicants is constantly higher than that of the marginal frequency, even though the two frequencies follow the same pattern over time, characterized by a sharp increase after the Lehman's collapse (Fig. 1).

The set of regressors includes variables at the firm and credit market level. Definitions and sample means of covariates are shown in Table 2. The main variable of interest is firm size, measured by the logarithm of the number of employees (*SIZE*). Following standard arguments (and evidence) in the banking literature, we assume that small firms are informationally more opaque than large firms, and that banks provide small business lending on a relational basis more than by using hard-information credit scoring technologies (Berger and Udell, 1998; Degryse et al., 2009). Consequently, pledgeable collateral is of primary importance for small firms' access to credit (Degryse and Van Cayseele, 2000; Steijvers and Voordeckers, 2009). This should make small firms more likely to be credit restricted in any period, but also more likely to be locked in a credit restriction state due to the self-reinforcing deterioration of their net worth produced by the negative impact that the inability to borrow has on investment, production and sales (Gertler and Gilchrist, 1993, 1994). However, the greater information

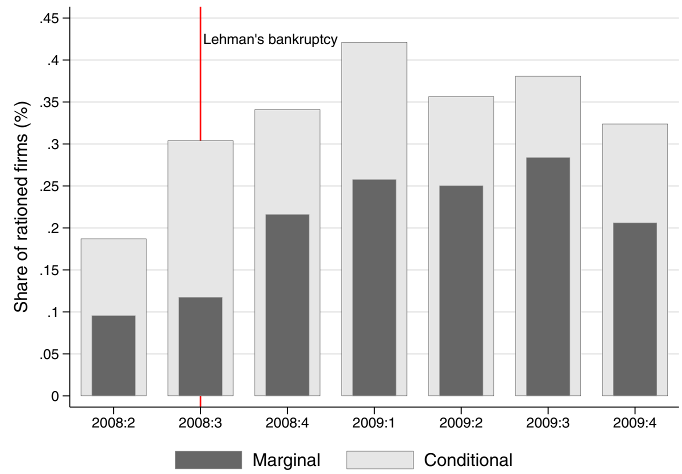



**Fig. 1.** Restricted applicants: marginal and conditional frequencies, by quarter.

opaqueness of small firms and their wider resort to relationship lending also imply that the bank screening technology for those firms is noisier, less informative and less sticky than for large firms.[10] Thus, while unconditionally expected to be more likely to be credit restricted in access to credit, small firms can also be more likely to escape from (or entry into) a state of credit restriction than large firms, as negative and positive assessments on small firms tend to be quickly revised by relationship lenders prepared to take advantage of any new (and volatile) pieces of soft information as they become available (see Eq. (A.8) and Result 2 in the online appendix).[11]

At the same time, the greater opaqueness and information volatility of small firms increase the discouragement effects produced by the difficulties previously experienced in accessing credit. In addition, for small firms loan application and switching costs are higher (Barone et al., 2011), due to their simplified and less professional organizational structure and the limited availability of public information about them. As a result, small firms can be more inclined to be discouraged from applying for a loan than large firms both unconditionally and conditional on a past loan rejection (see Result 3 in the online appendix).

The set of firm-level variables includes a dummy for exporter firms (*EXPORT*), identified as firms that sold at least some of theirs products abroad. This variable, taken as a proxy for productivity, is expected to affect positively the demand for credit and negatively the likelihood of credit restriction. The categorical variable *LIQUIDITY* captures firm financial health on the basis of a question about the level of liquidity with respect to the operational needs, which respondents can evaluate as good, neither good nor bad, or bad. To the extent that this variable is an indicator of financial needs and riskiness, it is expected to be negatively correlated with the demand for credit and positively correlated with credit availability.[12] We control for differences in access to credit across sectors adding a set of 10 industry dummies (*INDUSTRY*), and we include a dummy

Q43. How do you consider the access to bank credit with respect to three months ago?
1. improved
2. the same as before
3. worsened
4. do not know

Q44. Is your previous judgment the result of direct contact with a bank aimed at requesting/increasing a line of credit for your firm, or is it just an opinion which has nothing to do with specific contacts with banks?
1. it derives from direct contact with banks (go to Q45)
2. it is an opinion unrelated to direct contacts with banks (no other questions in this section)

Q45. (for firms answering 1. at Q44). Did you get from the bank the requested amount of credit?
1. yes, at the same conditions (no other questions in this section)
2. yes, but at more onerous terms
3. no
4. I just went to the bank to ask for information (no other questions in this section)

We define $D=1$ if Q44 = 1 and Q45 = 1, 2 or 3; and $D=0$ if Q44 = 2 or Q45 = 4.

[9] Precisely, we define $R=1$ if Q45 = 2 or 3; and $R=0$ if Q45 = 1.

[10] Using symbols of the information-based model presented in the online appendix, $x$ is greater, while $\mu_s$ and $\lambda$ are lower.

[11] Banks can be readier to revise their negative assessment on SMEs' creditworthiness for other reasons, related, for example, to the lower size of loans and the resulting lower solvency risk, and to the lower capital requirements on small business lending, that benefits from a capital discount (even though the SME Supporting Factor is only entered into force with Basel III, while during the period that we analyze it was only at the proposal stage). However, for these very same reasons, banks can have less interests to revise their positive assessments on SMEs, such that the average impact on state dependence in SMEs' access to credit could be negligible.

[12] The survey question is: "*Currently, the level of liquidity with respect to operational needs is good, neither good nor bad, or bad?*"

**Table 2**
Variables: definitions and descriptives. This table shows the description and sample means based on pooled transitions of the variables used in the model estimation. We include covariates both at the firm and local credit market level. Statistics are based on 24,080 observations.

| Variable | Description | Mean |
|---|---|---|
| *D* | Dummy equal to one for firms which report direct contacts with one or more banks in the previous quarter in order to seek credit (i.e., we exclude firms stating that they just went to the bank to ask for information). | 28.9 |
| *R* | Dummy equal to one for firms which stated that they did not obtain the desired amount of bank credit provided they contacted one or more banks. | 20.1 |
| *SIZE* | The logarithm of the firm's number of employees. | 3.13 |
| *EXPORT* | Dummy equal to one if the firm is an exporter, and zero otherwise. | 0.471 |
| *SOUTH* | Dummy equal to one if the firm is located in the southerns regions of Italy, and zero otherwise. | 0.176 |
| *HHI* | Herfindahl–Hirschman index calculated on bank branches in the province where the firm is located. | 1.061 |
| *BRANCHES* | Number of branches for 10,000 inhabitants by province. | 6.273 |
| *GDP GROWTH* | Regional real GDP growth rate (ISTAT), expanded using quarterly national data on GDP and expenditure components by Chow and Lin (1971)'s interpolation. | 0.089 |
| *TIME* | Quarter dummies from 2008:q2 to 2009:q4. | |
| *LIQUIDITY* | Level of liquidity: | |
| *GOOD* = 1 | | 0.241 |
| *NEITHER GOOD OR BAD* = 2 | | 0.559 |
| *BAD* = 3 | | 0.200 |
| *INDUSTRY* | 2 digit 2002 ATECO classification: | |
| 1: *extractive* | | 0.020 |
| 2: *food* | | 0.094 |
| 3: *textile* | | 0.214 |
| 4: *wood and other* | | 0.114 |
| 5: *paper plants and paper processing* | | 0.055 |
| 6: *fuel, chemistry and plastic* | | 0.160 |
| 7: *steel* | | 0.145 |
| 8: *mechanics* | | 0.092 |
| 9: *electrics and electronics* | | 0.072 |
| 10: *transportation machinery* | | 0.034 |
| *ORDER* | Level of orders and demand: | |
| *HIGH* = 1 | | 0.059 |
| *NORMAL* = 2 | | 0.470 |
| *LOW* = 3 | | 0.471 |
| *LABOR COST* | Percentage change in labor cost per employee in the past 12 months. | 1.285 |

variable identifying firms located in southern regions (*SOUTH*), to take into account the effect that differences in the levels of economic and financial development between the North and the South of Italy could have on access to credit.

Finally, a variable measuring the level of orders (*ORDER*) is used as an exclusion restriction in the initial conditions (5) and (6). The survey asks about the level of orders and demand in the current quarter, which can be assessed as low, normal or high. Since we consider a two-period model for each transition, it can be conjectured that this information may affect only the initial decision to apply for a loan and the initial bank response, while it should not provide additional information in the following quarter. In addition, the trend in labor costs during the previous 12 months *LABOR COST* is used as an exclusion restriction for the credit demand equations (4) and (6), as in Presbitero et al. (2014).[13]

As a measure of the structure of the local credit market at the provincial level (NUTS 3), we include the number of branches per 10,000 inhabitants (*BRANCHES*) and the Herfindahl–Hirschman index (*HHI*) of market concentration, computed on the share of branches held by banks operating in the province where the firm is located, as a measure of the degree of credit market competition in the province. To the extent that banks' market power and credit standards increase with the credit market concentration, the probability of credit restriction and *HHI* can be expected to be positively correlated (see the model in appendix).

We control for regional variation in the business cycle, which may affect the demand for and the supply of credit by adding the regional real GDP growth rate (*GDP GROWTH*). We expand the annual GDP data published by ISTAT using quarterly national data on GDP and expenditure components, following Chow and Lin (1971)'s interpolation. Aggregate common shocks are taken into account by adding quarterly time dummies.

Fig. 2 shows the frequencies (both marginal and conditional on the credit restriction state in $t-1$) of restricted applicants at time $t$, by firm and credit market characteristics. Quantities are disaggregated for the two values in the binary variables of interest and for the 1st and 3rd quartiles in the distributions of *SIZE*, *HHI* and *BRANCHES*. It is worth noting that, consistent with the screening-failure explanation discussed in the online appendix, when considering firm size and export, the patterns of the marginal and conditional frequencies are reversed: small and non-exporting firms are, on average, more likely to have their credit application not fully received by the banks, while large, exporter firms show a higher degree of state dependence in credit denial.

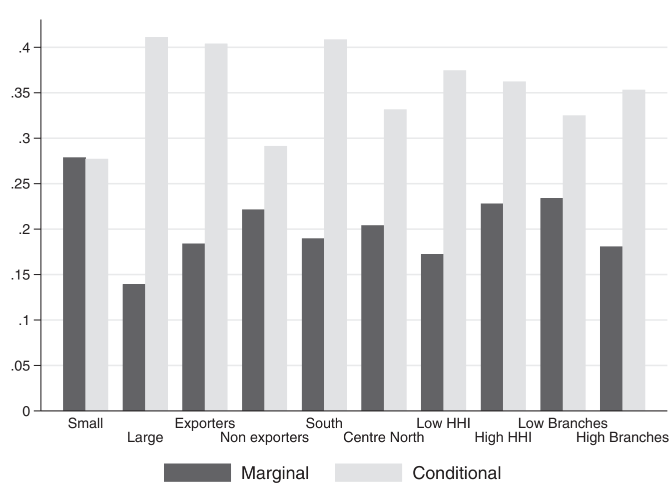



**Fig. 2.** Restricted applicants: marginal and conditional frequencies, by firm and credit market characteristics.

[13] With regard to labor costs the exact survey question is: "*In percentage, how much has the cost of labor changed in the last 12 months?*"

## 4. Estimation results and discussion

### 4.1. The baseline model

#### 4.1.1. Identifying state dependence

Table 3 shows our main results and compares the estimates of the first-order Markov model with the ones obtained with alternative estimation techniques for state dependence in access to credit, ignoring firm unobserved heterogeneity and sample selection. The first four columns report the estimated parameters of Eqs. (3)–(6) and the correlation coefficients of the first-order Markov model proposed in Section 2 (Model (1) henceforth). Columns 5 and 6 report the estimation results of a probit model with sample selection in which the probability of a firm being credit restricted is jointly estimated with the demand (selection) equation, but in which the initial state of credit restriction is considered exogenous and the effects of unobserved heterogeneity in $t-1$ are neglected. The last column shows the result of a simple probit model estimation of the supply equation (3), in which both the sample selection and the initial conditions are not addressed.

In Model (1), the key parameter $\gamma$, associated to $R^*_{t-1}$ in the credit restriction equation, is positive and statistically significant (column 4), suggesting that once a firm has been restricted in access to credit in $t-1$, its probability of experiencing a new restriction in $t$ is, on average, higher than for firms that have not been credit restricted or did not apply for a loan in $t-1$. At the same time, we find that $\phi$ in (4) is negative and statistically significant (column 3): all else being equal, firms which have experienced a loan denial in $t-1$ are

**Table 3**

Estimation results: first-order Markov model, probit model with sample selection, and probit model, baseline specification. The first two columns report the estimated parameters for the initial condition equations (6) and (5). The second two columns report the estimated parameters of the demand and credit restriction equation in $t$, and the correlation coefficients of the first-order Markov model. The fifth and sixth columns show the estimation results of a probit model with sample selection. The last column shows the results of a simple probit model for the credit restriction equation without sample selection and initial conditions. Standard errors are reported in parentheses. Each specification includes a constant term and dummies for *INDUSTRY*. The bottom rows show the values for the state dependence and discouragement effect computed as in (9) and (10) and results of tests for absence of state dependence, expogeneity of initial conditions and joint exogeneity. The test for absence state dependence is a Wald test: under the null, the parameters associated with $R^*_{t-1}$ in (4), (3) should be jointly zero. The test of exogeneity of initial conditions is a LR test: under the null, the correlations between demand and rationing in $t$ and demand and rationing in $t-1$ should be zero, $H_0:\rho_{31}=\rho_{41}=\rho_{32}=\rho_{42}=0$. Finally, the null hypothesis of the test of joint exogeneity is that all correlation coefficients are jointly zero.

| | First order Markov Model (1) | | | | Probit with sample selection | | Probit model |
|---|---|---|---|---|---|---|---|
| | Eq. (6) $D_{t-1}$ | Eq. (5) $R_{t-1}$ | Eq. (4) $D_t$ | Eq. (3) $R_t$ | $D_t$ | $R_t$ | $R_t$ |
| $R^*_{t-1}$ | | | −0.273 (.153)* | 0.927 (.137)*** | 0.605 (.037)*** | 0.236 (.072)*** | 1.163 (.055)*** |
| *SIZE* | 0.045 (.008)*** | −0.093 (.017)*** | 0.047 (.008)*** | −0.083 (.014)*** | 0.050 (.008)*** | −0.093 (.012)*** | −0.099 (.019)*** |
| *EXPORT* | 0.156 (.020)*** | −0.067 (.041)* | 0.147 (.020)*** | −0.126 (.028)*** | 0.141 (.020)*** | −0.122 (.026)*** | −0.002 (.043) |
| *HHI* | 0.004 (.017) | 0.020 (.034) | −0.004 (.017) | 0.022 (.021) | −0.005 (.018) | 0.025 (.022) | 0.035 (.039) |
| *BRANCHES* | 0.012 (.005)** | −0.038 (.009)*** | 0.011 (.005)** | −0.020 (.007)*** | 0.013(.005)*** | −0.024 (.006)*** | −0.032 (.010)*** |
| *SOUTH* | −0.029 (.023) | 0.020 (.044) | −0.027 (.023) | −0.005 (.030) | −0.027 (.024) | −0.010 (.030) | −0.057 (.052) |
| *GDP GR* | −0.025 (.008)*** | −0.007 (.015) | −0.012 (.008) | 0.005 (.010) | −0.009 (.008) | 0.003 (.010) | −0.010 (.017) |
| *TIME* | | | | | | | |
| 2008:q2 | −0.157 (.032)*** | 0.007 (.074) | | | | | |
| 2008:q3 | −0.120 (.033)*** | −0.006 (.071) | 0.066 (.032)** | −0.019 (.049) | 0.080 (.032)** | −0.026 (.044) | 0.108 (.078)* |
| 2008:q4 | −0.306 (.044)*** | 0.278 (.086)*** | −0.064 (.034)* | 0.239 (.095)** | −0.054 (.034) | 0.252 (.055)*** | 0.475 (.079)*** |
| 2009:q1 | −0.389 (.061)*** | 0.312 (.116)*** | −0.123 (.043)*** | 0.263 (.091)*** | −0.123 (.044)*** | 0.277 (.062)*** | 0.418 (.098)*** |
| 2009:q2 | −0.407 (.069)*** | 0.273 (.128)** | −0.129 (.061)** | 0.232 (.092)** | −0.126 (.061)** | 0.242 (.079)*** | 0.316 (.133)*** |
| 2009:q3 | −0.464 (.053)*** | 0.470 (.100)*** | −0.278 (.069)*** | 0.395 (.110)*** | −0.277 (.068)*** | 0.412 (.089)*** | 0.459 (.149)*** |
| 2009:q4 | | | −0.257 (.053)*** | 0.288 (.071)*** | −0.263 (.053)*** | 0.301 (.069)*** | 0.223 (.119)** |
| *LIQUIDITY* | | | | | | | |
| *NEITHER* | 0.211 (.022)*** | | 0.224 (.023)*** | −0.070 (.106) | 0.201 (.022)*** | −0.023 (.048) | 0.395 (.060)*** |
| *BAD* | 0.493 (.028)*** | | 0.520 (.035)*** | −0.120 (.212) | 0.389 (.028)*** | 0.057 (.083) | 0.868 (.065)*** |
| *LABOR COST* | 0.016 (.003)*** | | 0.010 (.004)** | | 0.009(.002)*** | | |
| *ORDER* | | | | | | | |
| *NORMAL* | −0.202 (.035)*** | 0.204 (.079)*** | | | | | |
| *LOW* | −0.227 (.036)*** | 0.306 (.078)*** | | | | | |
| *Correlation Coeff.* | | | | | | | |
| $\rho_{21}$ | −0.627 (.156)*** | | | | | | |
| $\rho_{31}$ | 0.429 (.018)*** | | | | 0 | | |
| $\rho_{41}$ | −0.424 (.030)*** | | | | 0 | | |
| $\rho_{32}$ | −0.000 (.128) | | | | 0 | | |
| $\rho_{42}$ | 0.042 (.117) | | | | 0 | | |
| $\rho_{43}$ | −0.967 (.106)*** | | | | −0.954 (.026)*** | | |
| *Pooled transitions* | 24,080 | | | | 24,080 | | 6971 |
| *Censored obs.* | 17,090 | | | | 17,090 | | |
| *Log-likelihood* | −33,582.03 | | | | −16,931.12 | | −2878.50 |
| $\overline{SD}_{R_{t-1}=0}$ | 0.252 (.060)*** | | | | | | |
| $\overline{SD}_{D_{t-1}=0}$ | 0.264 (.065)*** | | | | | | |
| $\overline{DE}_{R_{t-1}=0}$ | −0.100 (.058)** | | | | | | |
| $\overline{DE}_{D_{t-1}=0}$ | −0.333 (.055)*** | | | | | | |
| *State dependence* ($\chi^2_2$) | $W=53.962$ | | | | | | |
| *Initial conditions* ($\chi^2_4$) | $LR=1173.746$ | | | | | | |
| *Joint exogeneity* ($\chi^2_6$) | $LR=1201.773$ | | | | | | |

* $p<0.10$, ** $p<0.05$, *** $p<0.01$.

less likely to apply for bank credit in $t$. Taken together, these results indicate a strong dependence in access to credit, expressed in the propensity of banks to keep the negative assessment on firm creditworthiness from one period to the other, and in the discouragement effect on credit restricted applicants.

Formally, we test for state dependence in the demand and rationing equation computing the Wald test statistic for the null hypothesis of $\phi$ and $\gamma$ in (4) and (3) being jointly equal to zero. The value of the statistic is 53.96 which, compared with a $\chi^2_2$, indicates that the null hypothesis of absence of state dependence can be rejected.

Looking at the results of the probit model with sample selection (columns 5 and 6) makes it clear that a pooled model, not properly taking into account the unobserved time-invariant firm heterogeneity and its correlation with the lagged dependent variable, leads to biased results. In particular, the state dependence parameters change substantially when initial conditions are not accounted for. First, previously credit restricted firms look more likely to be restricted again in the next period, but the estimated value of the parameter $\gamma$ is much lower than the one estimated by the first-order Markov model, hiding the effects of the sample selection in $t-1$.[14] Second, and more noticeably, when ignoring the initial conditions, the coefficient for $R^*_{t-1}$ in the demand equation is significantly positive, suggesting that experiencing a credit restriction in $t-1$ would result in spurring rather than discouraging firms to apply for credit in $t$.

We test for the exogeneity of the initial conditions computing a LR test for the null hypothesis of $\rho_{31} = \rho_{41} = \rho_{32} = \rho_{42} = 0$. The value of the test is 1173.75, such that the null hypothesis is rejected (the value of the test needs to be compared with a $\chi^2_4$).

The estimated correlation coefficients of Model (1) indicate that the selection bias in modeling the rationing probability needs to be accounted for: the estimates of $\rho_{43}$, the correlation between demand and rationing in $t$, and $\rho_{21}$, the correlation between demand and rationing in $t-1$, are high in absolute value and statistically significant. The negative values of the correlation coefficients between the credit demand and supply equations suggest that some firms do not apply for credit as they expect to have a high probability of being restricted by banks and prefer to take themselves out of the credit market. Neglecting the selection bias leads to severely biased estimates of the regression coefficients, especially of the state dependence parameter that would prove extremely large (see column 7).

Interestingly, we also find that the correlation between the error terms in the credit restriction equations ($\rho_{31}$) is positive and statistically significant, suggesting that there are time-invariant unobservable characteristics which affect the individual probability of being credit restricted. Differently, we do not find a similar effect for the credit demand equations, as $\rho_{42}$ is not statistically different from zero.

From the estimates of the first-order Markov model we can calculate the state dependence in credit restriction and discouragement effects, as presented in Section 2.3. On average, firms experiencing a credit restriction in $t-1$ are 25.2% more likely to be credit restricted again in $t$ than borrowers that were not restricted by banks in $t-1$ ($\overline{SD}_{R_{t-1}=0}$) and 26.4% more likely to be restricted with respect to firms which did not applied for a loan in the previous period ($\overline{SD}_{D_{t-1}=0}$). This difference is statistically significant and large enough to be also economically significant, given that the sample frequency of rationed firms is 20%. Likewise, the discouragement effect proves to be statistically and economically significant, as the probability of not applying for credit conditional on a previous credit restriction is 10% ($\overline{DE}_{R_1=0}$) higher than for non-restricted borrowers and 33.3% higher than for new applicants ($\overline{DE}_{D_1=0}$).

The quantities $\overline{SD}$ and $\overline{DE}$ measure the probability of experiencing a credit restriction or the probability of being discouraged, conditional on credit restriction in the previous period, net of the individual observed and unobserved heterogeneity (Section 2.3). We could gauge how much of the persistence in access to credit is actually due to state dependence, rather than to the firms' observed and unobserved heterogeneity comparing $\overline{SD}$ and $\overline{DE}$ with the aggregate measures of state dependence and discouragement effects, ASD and ADE. The latter are displayed at the bottom of Table 4, which reports the average estimated probabilities of being in one of the three possible states at time $t$, conditional on coming from each state in $t-1$. The comparison clearly shows that most of the persistence in credit restriction and in being out of the credit market is due to state dependence while the persistence explained by firms' heterogeneity is much lower.

#### 4.1.2. Comparing different models: goodness-of-fit

As a first measure of the goodness of fit of Model (1), we compare the predicted transition rates reported in Table 4 with the sample conditional frequencies displayed in Table 1 and we find an almost perfect coincidence between the two set of probabilities.

To assess the accuracy of the different models we look at the Receiver Operating Characteristic (ROC) curve and at the area under the curve (AUROC). The latter is a measure of the predictive power of the model that is independent of the cutoff probability used to classify the model predictions.[15] The AUROC provides a simple test against the null value of 0.5 (a complete uninformative model).

Fig. 3 shows the ROC for the probabilities of credit restriction in $t$ (panel a), and of being credit constrained (panel b). For the first-order Markov model, probabilities are assigned according to the state in $t-1$. In panel (a) the probability of credit restriction for the first-order Markov model and for the probit model with sample selection are conditional on applying for credit in $t$, for comparability with the pooled probit model without sample selection. In panel (b), the probability of being credit restricted is computed as the marginal probability of not applying for loan, $P(D_t = 0)$, plus the joint probability of applying and being restricted by banks, $Pr(D_t = 1, R_t = 1)$. We exclude the simple probit model from this second analysis since, in this model, it is assumed that credit demand and denial are independent.

Fig. 3 confirms that the first-order Markov model describes access to credit more accurately than the pooled probit model with and without sample selection. The shape of the ROC curves and the values of AUROC in both panels suggest that controlling for initial conditions considerably and significantly improves the predictive power of the first-order Markov model. At least for a wide range of cutoff probabilities, the ROC curves indicate that, for a given share

[14] Typically, neglecting initial conditions results in overstating rather than understating the magnitude of state dependence. However, in this case the biases in the demand equation and supply equations in $t$ depend on the correlation between the unobserved heterogeneity and both the initial outcomes.

[15] Under the standard classification rule, the probability cutoff is set at 0.5, implying that Type 1 and Type 2 errors are equally bad. However, varying the cutoff probability will reduce the chances of making one type of error at the expense of increasing the other type of error. The ROC curve tells us exactly how this trade-off works for all possible cutoffs, plotting the true positive rate of the model against its false positive rate. The $y$-axis captures 'Sensitivity' which is the probability of correctly predicting when the outcome is equal to one (i.e., the loan application is actually denied) The $x$-axis is 1−Specificity, where 'Specificity' is the probability of correctly predicting the outcome variable equal to zero (i.e., the loan application is not rejected). It is easy to see that the further the ROC curve is away from the 45 degree line the better the model predicts both states (i.e., credit availability and denial). When the area under the ROC curve is 1, the model predicts everything correctly. For a recent use of the AUROC in a finance context, see Bharath and Dittmar (2010).

**Table 4**
Transition matrix of estimated probabilities for Model (1) and aggregate state dependence in access to credit. This table reports in the top panel the estimated transition rates for the sample of pooled transitions using quarters 2008:q2–2009:q4. They are probabilities of being in one of the three possible states in quarter $t$ (*No demand*, *Not rationed*, *Rationed*) conditional on being in one of the states in quarter $t-1$. They are evaluated at the parameter estimates of Model (1). The bottom panel reports the aggregate measures of state dependence (*ASD*) and discouragement effects (*ADE*), calculated taking the differences between the estimated transition rates.

| Quarter $t-1$ | Quarter $t$ | | |
|---|---|---|---|
| | No demand | Not rationed | Rationed |
| No demand | 78.98% | 17.25% | 3.92% |
| Not rationed | 54.24% | 40.84% | 5.02% |
| Rationed | 46.54% | 19.03% | 33.97% |

| Aggregate state dependence and discouragement effect | |
|---|---|
| $ASD_{R_{t-1}}$ | 0.340 – 0.052 = 0.289 |
| $ASD_{D_{t-1}}$ | 0.340 – 0.039 = 0.300 |
| $ADE_{R_{t-1}}$ | 0.465 – 0.542 = –0.077 |
| $ADE_{D_{t-1}}$ | 0.465 – 0.790 = –0.334 |

of true positives, the simple probit models call a larger share of false alarms that the first-order Markov model. In particular, in panel (a) the difference in the AUROC when using the first-order Markov model and the probit, with and without sample selection, is large and statistically significant, while the AUROCs of the two probit models are almost identical. This suggests that the increase in accuracy comes from the capacity to deal with unobserved heterogeneity, more than from modeling sample selection.

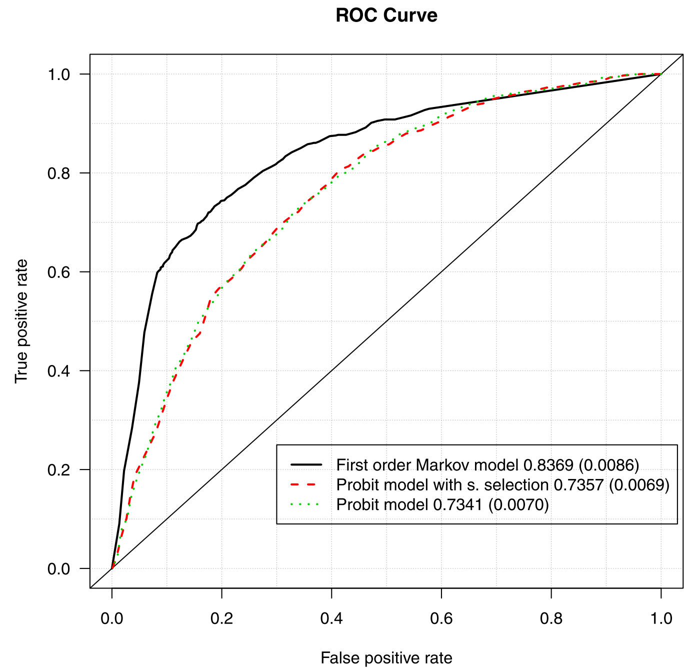
### 4.1.3. The other determinants of credit access

In this section we discuss the effect of other variables on the probabilities of loan application and credit restriction, referring to the estimates of Model (1).

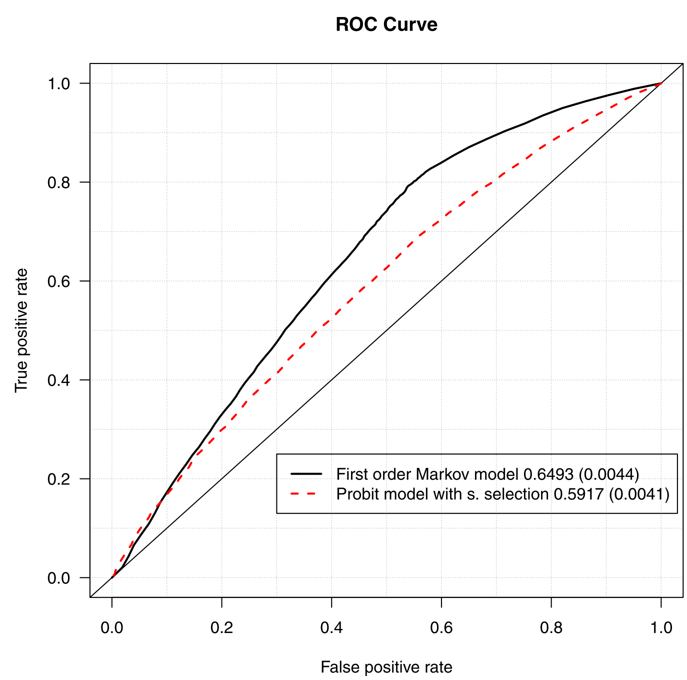
We find that small and exporting firms are more likely to apply for a loan and to suffer credit restrictions by banks than large and domestic-oriented firms. This result is not only consistent with the extensive literature on firms' financing constraints on survey data for Italy and other countries (Beck et al., 2005, 2006; Alessandrini et al., 2009; Brown et al., 2011; Presbitero et al., 2014; Ferri and Murro, 2015), but also with the predictions of the model in the online appendix, under the reasonable assumption that the screening technology used by banks for lending to small and exporting firms is more imperfect and noisy than that used to lend to large and domestic-oriented firms. As expected, firm liquidity needs are positively correlated with the demand for credit, but they do not show any significant association with the probability of experiencing a credit restriction.

Firms facing a low demand for their products have a higher probability of staying out of the credit market, either because they do not demand credit or because they do not obtain it. This finding is consistent with our model, predicting that when project returns ($Y$) are low bank credit standards ($\hat{p}_g$) are tighter, leading to a higher probability of credit restriction and to a stronger discouragement effect.

Moving on to the credit market structure, we find that the degree of credit market concentration is associated with a lower probability of credit demand and a higher probability of credit restriction, even if the coefficients for *HHI* are not statistically significant. By contrast, firms located in more financially developed provinces (i.e., where the number of bank branches per capita is higher) are more likely to apply for credit and to have their application accepted. To the extent that a greater presence of branches in the market involves lower application costs for borrowers and lower market




(a) Probability of credit restriction at time $t$




(b) Probability of being out of the credit market at time $t$

**Fig. 3.** Goodness-of-fit. *Notes*: Elaborations based on results from Table 3.

power for banks, these empirical findings speak to the predictions of our theoretical model (see the online appendix).

Once firm- and market-specific characteristics are taken into account, our results indicate that being located in the less developed southern regions or in regions where GDP growth rate is lower is not significantly associated with worse access to credit.

Finally, the coefficients on the time dummies indicate that Italian firms experienced a credit crunch after the Lehman bankruptcy. The point estimates show a significant reduction in the demand for credit but, even accounting for this effect, there is evidence of

**Table 5**
Balance sheet indicators: definitions and descriptives. This table shows the description and sample means based on pooled transitions of the balance sheet indicators used in the estimation of Model (2), Table 6. Differently from Bonaccorsi di Patti and Sette (2016), all banks with available balance sheet information are kept, excluding only those with missing values in the variable of interest. The final sample consists of 683 banks in 2008 and 687 in 2009. The three indicators are then averaged at the province level, weighted by the number of branches and matched with the pooled transitions at the province level. Statistics are based on 24,080 observations. Balance sheet items are drawn from *Bilbank*, a dataset maintained by the Italian Banking Association (ABI).

| Variable | Description | Mean |
|---|---|---|
| *INTERBANK* | Interbank borrowing divided by total assets | 9.995% |
| *SECURITIZE* | Volume of securitized loans divided by total assets | 1.005% |
| *CHARGEOFF* | Loan loss provisions and charge-offs divided by total assets | 1.674% |
| *LIQUID ASSETS* | Liquid assets (cash and sovereign bonds) divided by total assets | 0.807% |
| *CAPITAL* | Tier 1 capital divided by risk-weighted assets | 11.605% |

**Table 6**
Estimation results: first-order Markov model, Model (2). This table reports the estimation results of the first-order Markov model for a specification including the credit supply-side indicators: the ratio of interbank borrowing over total assets (*INTERBANK*), the ratio of securitized loans over total assets (*SECURITIZE*), the ratio of loan loss provisions and charge-offs over total assets (*CHARGEOFF*), the ratio of liquid assets over total assets (*LIQUID ASSETS*), and the Tier 1 capital ratio (*CAPITAL*). Estimation results of initial condition equations are not reported but are available upon request. The estimated coefficients on the constant term, *LIQUIDITY* dummies, and *INDUSTRY* dummies are not reported for brevity. Standard errors are reported in parentheses. The bottom of the table reports the values for the state dependence and discouragement effect and the test statistics for absence of state dependence, exogeneity of initial conditions, and joint exogeneity (see Table 3 for further details).

| | Model (2) | |
|---|---|---|
| | $D_t$ | $R_t$ |
| $R^*_{t-1}$ | −0.267 (.156)* | 0.926 (.141)*** |
| *SIZE* | 0.047 (.008)*** | −0.085 (.015)*** |
| *EXPORT* | 0.144 (.020)*** | −0.124 (.028)*** |
| *SOUTH* | −0.068 (.032)** | 0.008 (.045) |
| *HHI* | 0.019 (.021) | 0.009 (.028) |
| *BRANCHES* | 0.005 (.006) | −0.013 (.008) |
| *INTERBANK* | −0.002 (.004) | 0.001 (.005) |
| *SECURITIZE* | 0.007 (.016) | 0.011 (.023) |
| *CHARGEOFF* | −0.042 (.022)* | 0.032 (.026) |
| *LIQUID ASSETS* | −0.060 (.047) | 0.058 (.058) |
| *CAPITAL* | −0.001 (.007) | 0.014 (.010) |
| *GDP GR* | −0.015 (.009)* | 0.006 (.011) |
| *TIME* (*ref*: 2008:q2) | | |
| 2008:q3 | 0.067 (.032)** | −0.017 (.049) |
| 2008:q4 | −0.067 (.034)** | 0.246 (.097)** |
| 2009:q1 | −0.121 (.049)** | 0.236 (.088)*** |
| 2009:q2 | −0.136 (.068)** | 0.205 (.093)** |
| 2009:q3 | −0.290 (.076)*** | 0.371 (.111)*** |
| 2009:q4 | −0.261 (.059)*** | 0.260 (.074)*** |
| *Correlation coeff.* | | |
| $\rho_{21}$ | | −0.613 (.164)*** |
| $\rho_{31}$ | | 0.429 (.019)*** |
| $\rho_{41}$ | | −0.425 (.033)*** |
| $\rho_{32}$ | | 0.008 (.132) |
| $\rho_{42}$ | | 0.038 (.125) |
| $\rho_{43}$ | | −0.964 (.112)*** |
| *Log-likelihood* | −33,567.51 | |
| $\overline{SD}_{R_{t-1}=0}$ | 0.249 (.062)*** | |
| $\overline{SD}_{D_{t-1}=0}$ | 0.260 (.067)*** | |
| $\overline{DE}_{R_{t-1}=0}$ | −0.100 (.060)** | |
| $\overline{DE}_{D_{t-1}=0}$ | −0.333 (.058)*** | |
| *State dependence* ($\chi^2_2$) | $W$ = 50.737 | |
| *Initial conditions* ($\chi^2_4$) | $LR$ = 1171.673 | |
| *Joint exogeneity* ($\chi^2_6$) | $LR$ = 1197.952 | |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

**Table 7**
Estimation results: first-order Markov model, Model (3). This table reports the estimation results of the first-order Markov model for a specification including the interaction of $R^*_{t-1}$ with *SIZE*. Estimation results of initial condition equations are not reported but are available upon request. Results for the constant term, *LIQUIDITY* dummies, and *INDUSTRY* dummies are not reported for brevity. Standard errors are reported in parentheses. The bottom of the table reports the values for the state dependence and discouragement effect and the test statistics for absence of state dependence, exogeneity of initial conditions, and joint exogeneity (see Table 3 for further details).

| | Model (3) | |
|---|---|---|
| | $D_t$ | $R_t$ |
| $R^*_{t-1}$ | −0.433 (.152)*** | 0.680 (.175)*** |
| *SIZE* | 0.044 (.008)*** | −0.094 (.019)*** |
| *EXPORT* | 0.146 (.020)*** | −0.120 (.030)*** |
| *SOUTH* | −0.028 (.023) | −0.012 (.033) |
| *HHI* | −0.004 (.017) | 0.025 (.023) |
| *BRANCHES* | 0.012 (.005)** | −0.022 (.007)*** |
| *GDP GR* | −0.011 (.008) | 0.004 (.011) |
| *TIME* (*ref*: 2008:q2) | | |
| 2008:q3 | 0.068 (.032)** | −0.006 (.051) |
| 2008:q4 | −0.062 (.034)* | 0.277 (.087)*** |
| 2009:q1 | −0.122 (.043)*** | 0.296 (.085)*** |
| 2009:q2 | −0.128 (.061)** | 0.256 (.091)*** |
| 2009:q3 | −0.275 (.068)*** | 0.426 (.107)*** |
| 2009:q4 | −0.254 (.053)*** | 0.301 (.073)*** |
| *SIZE* × $R^*_{t-1}$ | 0.074 (.032)** | 0.088 (.039)** |
| *Correlation coeff.* | | |
| $\rho_{21}$ | | −0.627 (.175)*** |
| $\rho_{31}$ | | 0.424 (.018)*** |
| $\rho_{41}$ | | −0.417 (.035)*** |
| $\rho_{32}$ | | −0.028 (.133) |
| $\rho_{42}$ | | 0.056 (.125) |
| $\rho_{43}$ | | −0.941 (.122)*** |
| *Log-likelihood* | −33,571.98 | |
| $\overline{SD}_{R_{t-1}=0}$ | 0.248 (.060)*** | |
| $\overline{SD}_{D_{t-1}=0}$ | 0.261 (.065)*** | |
| $\overline{DE}_{R_{t-1}=0}$ | −0.099 (.056)** | |
| $\overline{DE}_{D_{t-1}=0}$ | −0.332 (.054)*** | |
| *State dependence* ($\chi^2_4$) | $W$ = 61.350 | |
| *Initial conditions* ($\chi^2_4$) | $LR$ = 1170.852 | |
| *Joint exogeneity* ($\chi^2_6$) | $LR$ = 1195.518 | |

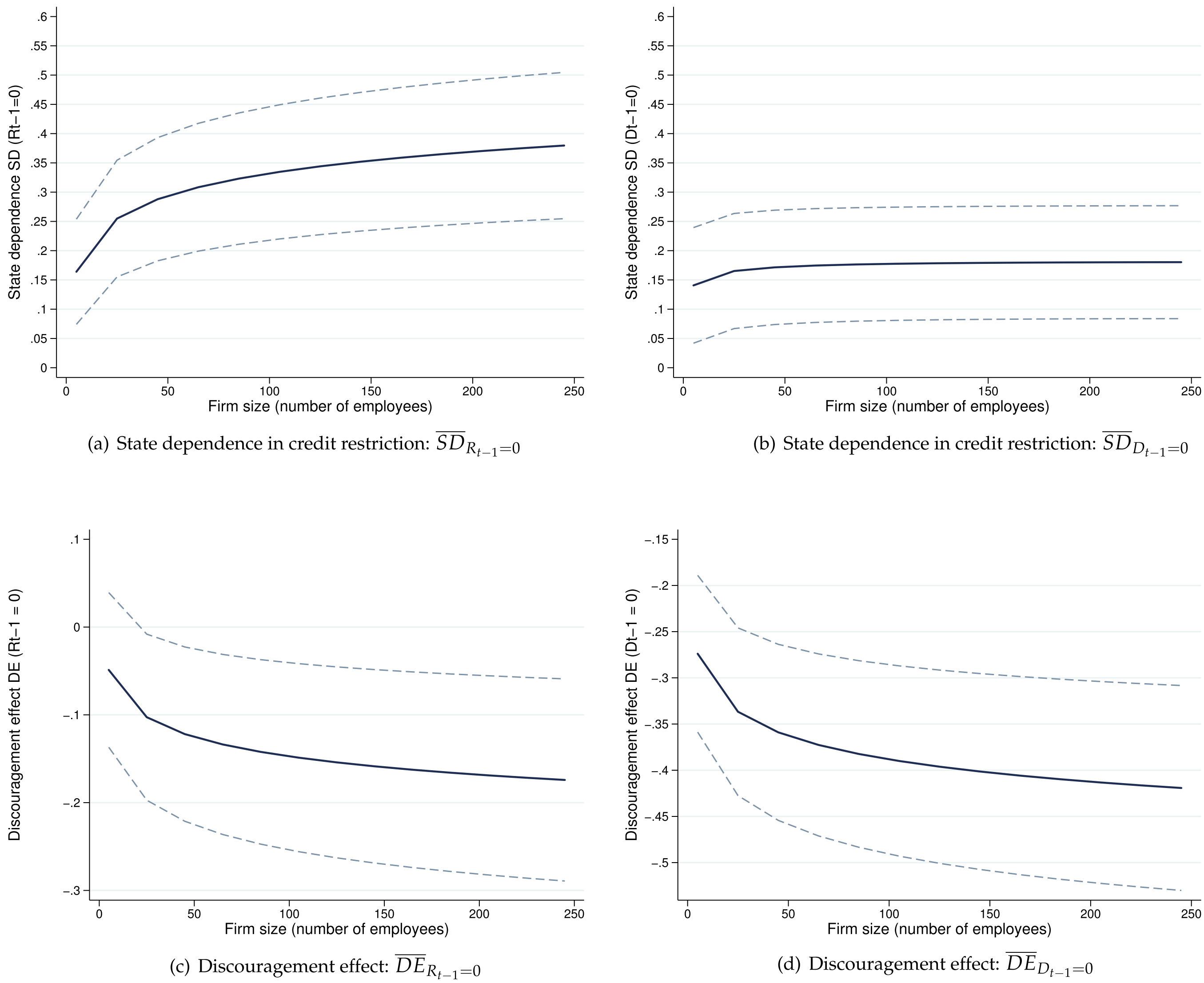
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

a large and statistically significant reduction in the likelihood of firms obtaining the required credit, consistent with the existing literature for Italy (Del Giovane et al., 2011; Gobbi and Sette, 2014; Presbitero et al., 2014) and with the descriptive statistics (Fig. 1).

#### 4.1.4. Controlling for changes in credit supply

During the 2007–2008 global financial crisis, the instability in the interbank market caused a reduction of bank lending, especially for banks more reliant on short-term debt and wholesale funding (Ivashina and Scharfstein, 2010; Puri et al., 2011; Santos, 2011; Kapan and Minoiu, 2015). In the case of Italy, Bonaccorsi di Patti and Sette (2016) document that firms borrowing from banks with a larger share of securitized loans experienced a stronger tightening in credit supply. In the light of this evidence, the persistence of credit restriction over time and the discouragement effects could reflect worse supply conditions due to bank liquidity constraints rather than *true* state dependence in access to credit.

Since our data do not allow to identify the lending banks, we can account for possible supply side factors only very indirectly by including indicators of asset quality and exposure to liquidity shocks, calculated at the provincial-year level using market shares in terms of branches as weights. Namely, we measure the exposure




(a) State dependence in credit restriction: $\overline{SD}_{R_{t-1}=0}$

(b) State dependence in credit restriction: $\overline{SD}_{D_{t-1}=0}$

(c) Discouragement effect: $\overline{DE}_{R_{t-1}=0}$

(d) Discouragement effect: $\overline{DE}_{D_{t-1}=0}$

**Fig. 4.** State dependence in access to credit and firm size. *Notes*: Elaborations based on results from Table 7 (Model 3). The diagrams show the values of $\overline{SD}_{R_{t-1}=0}$, $\overline{SD}_{D_{t-1}=0}$, $\overline{DE}_{R_{t-1}=0}$, and $\overline{DE}_{D_{t-1}=0}$ (solid lines) and the 90% confidence interval (dotted line) for different values of the number of employees. Standard errors are computed via Delta Method.

to the interbank market as the ratio of gross interbank borrowing to total asset (*INTERBANK*); the ratio of securitized loans over total assets measures that exposure to a securitization market freeze; and the quality of the loan portfolio is measured by the ratio of loan loss provisions and charge-offs over total assets (*CHARGEOFF*). We also control for bank liquidity and capitalization, which may affect the capacity of banks to extend credit, especially during crisis periods (Jiménez et al., 2012; Iyer et al., 2014): the liquidity indicator is the ratio of cash and sovereign bonds over total assets (*LIQUID ASSETS*) and capitalization is defined by the ratio of Tier 1 capital over risk-weighted assets (*CAPITAL*).[16]

The inclusion of these additional control variables – which are generally not statistically significant – does not affect the estimated coefficients associated with credit restriction in the previous period, $R^*_{t-1}$, as well as the effects of the other explanatory variables (with the sole exception of *BRANCHES*, see Table 6). To the extent that our aggregate indicators reflect at least partly the actual conditions credit supply, this result suggests that state dependence in access to credit is not driven by bank characteristics affecting credit supply.

### 4.2. State dependence, discouragement effect, and firm size

Two major explanations for *true* state dependence in access to credit can be advanced: (a) the adverse effects of credit restriction on firms' net-worth and (b) the negative signaling of credit restriction about firms' riskiness under imperfect screening technology. Even though the lack of information on firms' wealth and value of pledgeable collateral does not allow for a direct test of these explanations, we can try to discriminate between the two mechanisms looking at firm size.

Both the net-worth explanation of state dependence and the screening-friction one predict that informationally opaque small firms borrowing on a relational basis (i.e., using the symbols of the model in the online appendix, firms with higher value of $x$, $\mu_s$ and $c$ and lower value of $\lambda$) are unconditionally more likely to be restricted in access to credit and more likely to be discouraged

[16] In the choice and definition on these indicators we follow closely Bonaccorsi di Patti and Sette (2016). Definitions, sources, and descriptive statistics are available in Table 5.

**Table 8**
Estimation results: first-order Markov model, Model (4) and Model (5). This table reports the estimation results of the first-order Markov models for two specifications including the interaction of $R^*_{t-1}$, *SIZE*, *CCB* and *LARGE*, respectively. Estimation results of initial condition equations are not reported but are available upon request. Results for the constant term, *LIQUIDITY* dummies, and *INDUSTRY* dummies are not reported for brevity. Standard errors are reported in parentheses. The bottom of the table reports the values for the state dependence and discouragement effect and the test statistics for absence of state dependence, exogeneity of initial conditions, and joint exogeneity (see Table 3 for further details).

| | Model (4) | | Model (5) | |
|---|---|---|---|---|
| | $D_t$ | $R_t$ | $D_t$ | $R_t$ |
| $R^*_{t-1}$ | −0.523 (.196)*** | 0.797 (.070) | −0.373 (.445) | −0.528 (.601) |
| *SIZE* | 0.055 (.011)*** | −0.092 (.018)*** | 0.002 (.027) | −0.082 (.054) |
| *EXPORT* | 0.146 (.020)*** | −0.120 (.030)*** | 0.146 (.020)*** | −0.119 (.032)*** |
| *SOUTH* | −0.043 (.024)* | 0.002 (.034) | −0.045 (.024)* | −0.008 (.038) |
| *HHI* | −0.015 (.017) | 0.034 (.024) | 0.007 (.018) | 0.020 (.025) |
| *BRANCHES* | 0.018 (.006)*** | −0.029 (.008)*** | 0.016 (.005)*** | −0.023 (.008)*** |
| *GDP GR* | −0.005 (.009) | −0.002 (.011) | −0.009 (.008) | 0.002 (.011) |
| *TIME* (*ref*: 2008:q2) | | | | |
| 2008 : *q*3 | 0.068 (.032)** | −0.005 (.051) | 0.055 (.032)* | 0.002 (.052) |
| 2008 : *q*4 | −0.054 (.034) | 0.270 (.086)*** | −0.076 (.034)** | 0.282 (.093)*** |
| 2009 : *q*1 | −0.099 (.045)** | 0.276 (.086)*** | −0.129 (.043)*** | 0.296 (.089)*** |
| 2009 : *q*2 | −0.085 (.064) | 0.217 (.095)** | −0.127 (.061)** | 0.252 (.093)*** |
| 2009 : *q*3 | −0.225 (.073)*** | 0.380 (.111)*** | −0.270 (.068)*** | 0.419 (.110)*** |
| 2009 : *q*4 | −0.219 (.056)*** | 0.268 (.076)*** | −0.254 (.053)*** | 0.299 (.074)*** |
| *CCB* | 0.036 (.206) | 0.245 (0.311) | | |
| $CCB \times R^*_{t-1}$ | 0.665 (1.02) | −0.849 (1.34) | | |
| $CCB \times SIZE$ | −0.080 (.057) | −0.019 (.094) | | |
| $SIZE \times R^*_{t-1}$ | 0.125 (.057)** | 0.031 (.070) | | |
| $CCB \times SIZE \times R^*_{t-1}$ | −0.399 (.350) | 0.450 (.461) | | |
| *LARGE* | | | −0.138 (.186) | 0.045 (.234) |
| $LARGE \times R^*_{t-1}$ | | | −0.203 (.786) | 2.278 (1.03)** |
| $LARGE \times SIZE$ | | | 0.082 (.051) | −0.026 (.084) |
| $SIZE \times R^*_{t-1}$ | | | −0.162 (.135) | 0.531 (.191)*** |
| $LARGE \times SIZE \times R^*_{t-1}$ | | | 0.450 (.254)* | −0.829 (.340)** |
| *Correlation coeff.* | | | | |
| $\rho_{21}$ | | −0.630 (.174)*** | | −0.626 (.165)*** |
| $\rho_{31}$ | | 0.425 (.018)*** | | 0.428 (.018)*** |
| $\rho_{41}$ | | −0.418 (.035)*** | | −0.417 (.039)*** |
| $\rho_{32}$ | | −0.023 (.133) | | −0.004 (.130) |
| $\rho_{42}$ | | 0.052 (.124) | | 0.052 (.128) |
| $\rho_{43}$ | | −0.939 (.122)*** | | −0.939 (.137)*** |
| *Log-likelihood* | −33,564.67 | | −33,551.10 | |
| $\overline{SD}_{R_{t-1}=0}$ | 0.248 (.060)*** | | 0.246 (.059)*** | |
| $\overline{SD}_{D_{t-1}=0}$ | 0.261 (.064)*** | | 0.259 (.065)*** | |
| $\overline{DE}_{R_{t-1}=0}$ | −0.098 (.056)** | | -0.094 (.055)** | |
| $\overline{DE}_{D_{t-1}=0}$ | −0.331 (.054)*** | | −0.326 (.053)*** | |
| *State dependence* ($\chi^2_8$) | *W* = 65.83 | | *W* = 84.79 | |
| *Initial conditions* ($\chi^2_4$) | *LR* = 1168.59 | | *LR* = 1172.44 | |
| *Joint exogeneity* ($\chi^2_6$) | *LR* = 1193.22 | | *LR* = 1197.03 | |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

from applying for a loan after having experienced a previous credit restriction. However, unlike under the net-worth hypothesis, small firms will be less likely to be locked in a credit restriction state over time in presence of screening-frictions.

Hence, to discriminate between these two hypotheses we augment the baseline specification (Model 1) interacting the indicator $R^*_{t-1}$ with firm size. The positive sign of coefficients for the interaction terms $SIZE \times R^*_{t-1}$ in the supply and demand equations indicate, respectively, that small firms are less likely to be locked in a credit restriction state than large firms, but are more likely to be discouraged from applying for credit in the future (Table 7).[17]

Given that in non-linear models the sign and magnitude of marginal effects associated with interaction terms are not directly interpretable (Ai and Norton, 2003), we compute state dependence in credit restriction and the discouragement effect, as indicated in expressions (9) and (10), for different values of firm size. The results are plotted in Fig. 4. State dependence in credit restriction is increasing nonlinearly in firm size and is especially low for micro and small enterprises. In addition, this nonlinearity is stronger if state dependence is computed with respect to non-applicants (panel b), while state dependence with respect to non-restricted borrowers increases more steadily in firm size (panel a). The discouragement effect is decreasing nonlinearly in firm size (Fig. 4, panels c and d). Interestingly, all these findings are consistent with the model reported in the online appendix,[18] suggesting that, besides deterioration of firms' balance sheets and projects' market potential, imperfections in banks' screening technologies are a major determinant of state-dependence in access to credit at the firm level.

[17] The null hypotheses of absence of state dependence, exogeneity of initial conditions and joint exogeneity are rejected as in Model (1). In addition, estimated coefficients for control variables and estimated average state dependence and discouragement effects are in line with results in Table 3.

[18] See expressions (A.8) and (A.9), recalling that $\mu_{s|s \geq \tilde{s}} > \mu_s$, and (A.11).

Variations in the state dependence and discouragement effects due to firm size are economically (and statistically) significant. For a firm with 5 employees, the penalty due to having been credit restricted in $t-1$ on the current likelihood of credit restriction is 18%, while it increases to 26.8% for a firm with 25 employees and to 38% for a larger firm with 200 employees (Fig. 4, panel a). The variation in the discouragement effect due to firm size is also considerable: the discouragement effect increases, in absolute terms, from −5% for a firm with five employees to −10% and −17% for a firm with 25 and 200 employees, respectively (Fig. 4, panel c).

Besides higher noise in the screening technology, stronger state dependence for SMEs can reflect the fact that these firms are more likely to borrow from local and small-sized banks on a relational basis. As relational lenders, local banks could be prepared to revise their negative assessments on small firms more frequently than large, distant transaction banks. In this case, lower state dependence for SMEs would reflect a beneficial effect of their bank relationships rather than the stronger noise in the screening technology. An indirect way to try to discriminate between these two possible explanations is to include measures of the size structure of the local banking industry (Berger et al., 2007), and test whether the difference in state dependence across small and large firms is greater in provinces disproportionally populated by small, cooperative banks.

Then, we estimate our baseline specification adding the triple interaction term between *SIZE*, $R^*_{t-1}$ and, alternatively, the share of branches owned by credit cooperative banks (*CCB*) and the share of branches owned by the five largest banking groups (*LARGE*), both varying at the province-year level.

Results – reported in Table 8 – show that the effect of $SIZE \times R^*_{t-1}$ is still positive and statistically significant in the demand equation, whereas the interaction terms involving *CCB* have no significant effects either on the probability of applying for a loan, or on the probability of having the application rejected. Differently, results indicate that in provinces where there is a higher share of large banks, firms have a higher probability of being rationed if their application has been already rejected in the previous quarter. Moreover, larger firms continue to exhibit a higher persistence in credit rationing, although this effect is attenuated for firms located in provinces with a high share of large banks.

To measure the magnitude of these results, we compute the impact on state dependence and discouragement effect of a discrete change in the number of employees (from 10 to 100) and we evaluate them at the quartiles of the distributions of *CCB* and *LARGE*.[19] Table 9 shows that in provinces where the share of cooperative banks is high, the difference between the state dependence measures for firms with 10 and 100 employees is slightly lower than in provinces where the presence of cooperative bank is low, even if not statistically significant. This finding contrasts with the relational-lending motives for the greater state dependence of small firms, which predict a higher difference in state dependence between small and large firms in provinces disproportionally populated by cooperative banks. However, the decreasing (absolute) values of the discouragement effect corroborates the relational-lending interpretation. Likewise, the lower difference in state dependence and the higher (in absolute value) one in the discouragement effects between small an large firms in provinces with a higher share branches of large banks are consistent with the relational-lending interpretation, although none of these variations are statistically significant.

On the whole, even if only indirectly, these results suggest that the relational-lending motives may not provide an exhaustive explanation of the differential in state dependence and the discouragement effect between small and large firms.

**Table 9**
Variations in state dependence: Model (4) and Model (5). This table shows the effect on the measures of state dependence of the discrete change in firm size from 10 to 100. The quantities are computed following the expression in footnote 19 and evaluated at the quartiles of *CCB* (0.062, 0.117, 0.186) and *LARGEG* (0.419, 0.521, 0.606). Quantities are computes using the parameter estimates of Model (4) and Model (5) available upon request. Standard errors are computed by Delta Method.

| | Variation in state depedence | s.e. | c.i.95% |
|---|---|---|---|
| *Model (4) CCB* | | | |
| $\Delta^{0.25}\overline{SD}_{R_{t-1}=0}/\Delta x$ | 0.134*** | 0.041 | (0.054; 0.214) |
| $\Delta^{0.50}\overline{SD}_{R_{t-1}=0}/\Delta x$ | 0.131*** | 0.032 | (0.068; 0.194) |
| $\Delta^{0.75}\overline{SD}_{R_{t-1}=0}/\Delta x$ | 0.124*** | 0.037 | (0.051; 0.197) |
| $\Delta^{0.25}\overline{SD}_{D_{t-1}=0}/\Delta x$ | 0.126*** | 0.041 | (0.046; 0.206) |
| $\Delta^{0.50}\overline{SD}_{D_{t-1}=0}/\Delta x$ | 0.122*** | 0.032 | (0.059; 0.185) |
| $\Delta^{0.75}\overline{SD}_{D_{t-1}=0}/\Delta x$ | 0.115*** | 0.037 | (0.042; 0.188) |
| $\Delta^{0.25}\overline{DE}_{R_{t-1}=0}/\Delta x$ | −0.099*** | 0.038 | (−0.173; −0.025) |
| $\Delta^{0.50}\overline{DE}_{R_{t-1}=0}/\Delta x$ | −0.080*** | 0.031 | (−0.141; −0.019) |
| $\Delta^{0.75}\overline{DE}_{R_{t-1}=0}/\Delta x$ | −0.053* | 0.037 | (−0.126; 0.020) |
| $\Delta^{0.25}\overline{DE}_{D_{t-1}=0}/\Delta x$ | −0.112*** | 0.037 | (−0.185; −0.039) |
| $\Delta^{0.50}\overline{DE}_{D_{t-1}=0}/\Delta x$ | −0.092*** | 0.031 | (−0.153; −0.031) |
| $\Delta^{0.75}\overline{DE}_{D_{t-1}=0}/\Delta x$ | −0.064** | 0.036 | (−0.135; 0.007) |
| *Model (5) LARGE* | | | |
| $\Delta^{0.25}\overline{SD}_{R_{t-1}=0}/\Delta x$ | 0.145*** | 0.041 | (0.065; 0.225) |
| $\Delta^{0.50}\overline{SD}_{R_{t-1}=0}/\Delta x$ | 0.137*** | 0.032 | (0.074; 0.200) |
| $\Delta^{0.75}\overline{SD}_{R_{t-1}=0}/\Delta x$ | 0.119*** | 0.041 | (0.039; 0.199) |
| $\Delta^{0.25}\overline{SD}_{D_{t-1}=0}/\Delta x$ | 0.136*** | 0.041 | (0.056; 0.216) |
| $\Delta^{0.50}\overline{SD}_{D_{t-1}=0}/\Delta x$ | 0.128*** | 0.031 | (0.067; 0.189) |
| $\Delta^{0.75}\overline{SD}_{D_{t-1}=0}/\Delta x$ | 0.111*** | 0.040 | (0.033; 0.189) |
| $\Delta^{0.25}\overline{DE}_{R_{t-1}=0}/\Delta x$ | −0.028 | 0.041 | (−0.108; 0.052) |
| $\Delta^{0.50}\overline{DE}_{R_{t-1}=0}/\Delta x$ | −0.074*** | 0.031 | (−0.135; −0.013) |
| $\Delta^{0.75}\overline{DE}_{R_{t-1}=0}/\Delta x$ | −0.107*** | 0.035 | (−0.176; −0.038) |
| $\Delta^{0.25}\overline{DE}_{D_{t-1}=0}/\Delta x$ | −0.039 | 0.041 | (−0.119; 0.041) |
| $\Delta^{0.50}\overline{DE}_{D_{t-1}=0}/\Delta x$ | −0.087*** | 0.031 | (−0.148; −0.026) |
| $\Delta^{0.75}\overline{DE}_{D_{t-1}=0}/\Delta x$ | −0.121*** | 0.035 | (−0.190; −0.052) |

* $p<0.10$, ** $p<0.05$, *** $p<0.01$.

### 4.3. State dependence in tranquil and crisis times

During our sample period the Italian economy was severely hit by the global financial crisis triggered by the collapse of Lehman Brothers. Since five out of the seven quarters in our sample are crisis periods, a possible concern with our analysis is that state dependence in access to credit could be due to the global liquidity shock and limited to the crisis period.

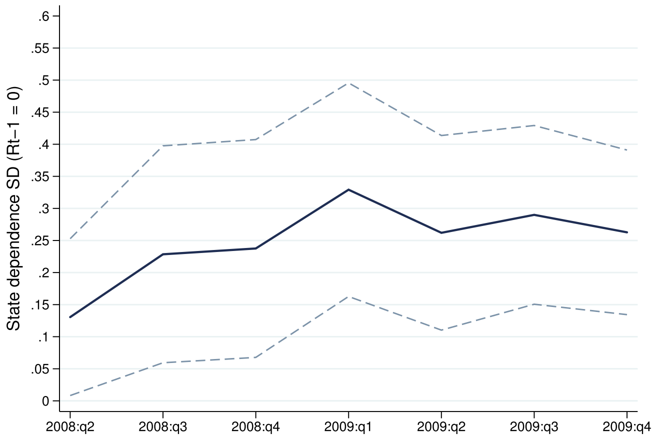
In order to test whether state dependence in credit restriction and the discouragement effect are specific features of malfunctioning credit markets in crisis periods, we interact $R^*_{t-1}$ with quarter dummies to allow for the effect of $R^*_{t-1}$ on the probabilities of demanding credit and having the loan application restricted to vary over time. Since Lehman Brothers filed for Chapter 11 in September 2008, we consider the quarters 2008:q4–2009:q4 the post-Lehman period.[20]

[19] In formulas:

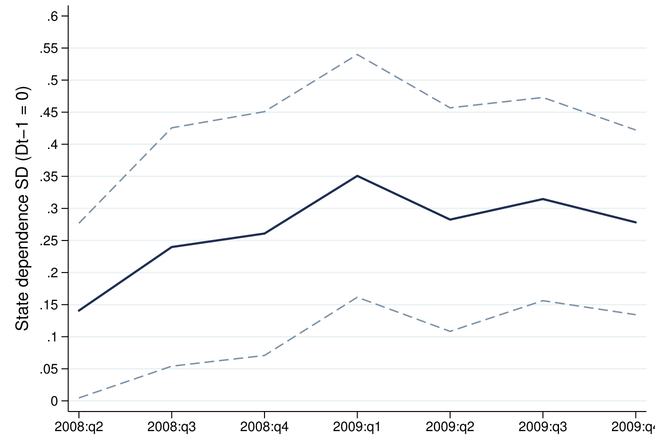
$$\frac{\Delta^q\overline{SD}_{R_{t-1}=0}}{\Delta x} = \frac{1}{N}\sum_{i=1}^{N}\left[\frac{\Delta P_{1i}}{\Delta x} - \frac{\Delta P_{0i}}{\Delta x}\right]$$

where $P_{1i} = \frac{Pr(R_{it}=1, D_{it}=1, R_{it-1}=1, D_{it-1}=1)}{Pr(R_{it-1}=1, D_{it-1}=1)}$ and $P_{0i} = \frac{Pr(R_{it}=1, D_{it}=1, R_{it-1}=0, D_{it-1}=1)}{Pr(R_{it-1}=0, D_{it-1}=1)}$, $\Delta x = -90$, and $q$ denotes the quartiles of the distribution of *CCB* or *LARGEG* at which $\frac{\Delta^q\overline{SD}_{R_{t-1}=0}}{\Delta x}$ is evaluated. In the same way we derive $\Delta^q\overline{SD}_{D_{t-1}=0}/\Delta LB$, $\Delta^q\overline{DE}_{R_{t-1}=0}/\Delta LB$ and $\Delta^q\overline{DE}_{D_{t-1}=0}/\Delta LB$.

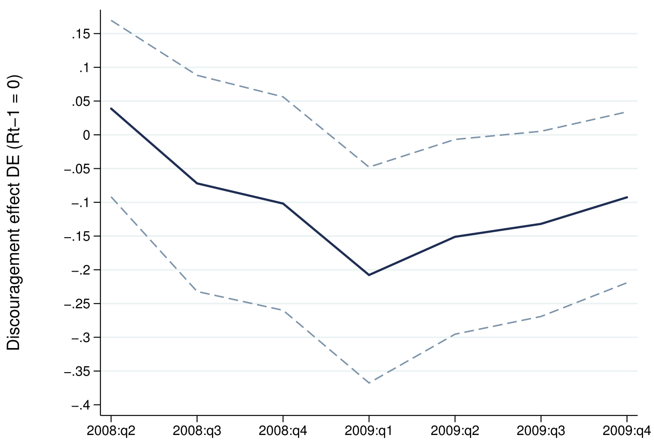
[20] We are aware that many authors have considered the period 2008:q1–2008:q3 as a crisis time following the troubles in the subprime market for mortgages



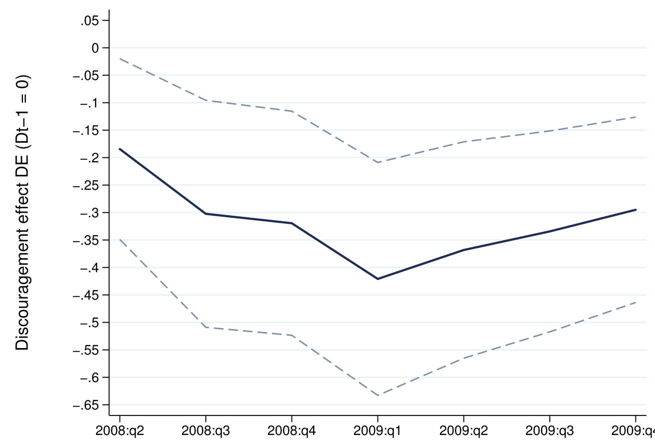

(a) State dependence in credit constraints: $\overline{SD}_{R_{t-1}=0}$




(b) State dependence in credit constraints: $\overline{SD}_{D_{t-1}=0}$




(c) Discouragement effect: $\overline{DE}_{R_{t-1}=0}$




(d) Discouragement effect: $\overline{DE}_{D_{t-1}=0}$

**Fig. 5.** State dependence in access to credit and time. *Notes*: Elaborations based on results from Table 10 (Model 6). The diagrams show the values of $\overline{SD}_{R_{t-1}=0}$, $\overline{SD}_{D_{t-1}=0}$, $\overline{DE}_{R_{t-1}=0}$, and $\overline{DE}_{D_{t-1}=0}$ (solid lines) and the 90% confidence interval (dotted line) for each quarter in the sample. Standard errors are computed via Delta Method.

The results, reported in Table 10, reject the hypothesis that state dependence in access to credit is an exclusive occurrence in times of crisis. The coefficients on $R^*_{t-1}$ confirm the presence of state dependence in access to credit in tranquil periods, while those on the interaction terms suggest that the discouragement effect decreased during the crisis, probably because of the crucial importance of obtaining access to credit in this period.

In order to have a clear picture of the evolution of state dependence in firms' access to credit during the sample period, we compute the values of SD and DE quarter by quarter and test for their significance. Fig. 5 shows that state dependence in credit restriction and the discouragement effect are present throughout the sample.[21]

The confidence bands in Fig. 5 show that the variations in state dependence and discouragement effects from one quarter to the next are generally not statistically significant. However, when we compute the variation in SD and DE by aggregating quarters in pre- and post-Lehman periods,[22] we observe that state dependence in credit restriction is 9.7% higher in the post- than in the pre-Lehman period, while the discouragement effect decreases by almost 12.5% after the Lehman collapse (Table 11).

(Ivashina and Scharfstein, 2010; Puri et al., 2011); however, it is a widely accepted opinion that the impact of the subprime crisis on the Italian banking system was limited before the collapse of Lehman Brothers (Gobbi and Sette, 2014; Presbitero et al., 2014).

[21] To be precise, the value of state dependence in the pre-Lehman quarter 2008:q2 is equal to 0.13 and statistically significant (panels a and b). In the same quarter, $\overline{DE}_{R_{t-1}=0} = 0.04$ and it is not significantly different from 0 (panel c), but the discouragement effect computed with respect to non-applicant firms in $t-1$ amounts to $-0.184$ and is significant at the 5% level (panel d).

[22] The effect on the state dependence in credit restriction measures of the discrete change between the pre- and post-Lehman Brothers quarters ($\Delta LB$) is calculated by setting to zero/one the quarter dummies for the pre/post-Lehman periods:

$$\frac{\Delta \overline{SD}_{R_{t-1}=0}}{\Delta LB} = \frac{1}{N}\sum_{i=1}^{N}\left[\frac{\Delta P_{1i}}{\Delta LB} - \frac{\Delta P_{0i}}{\Delta LB}\right]$$

where $P_{1i} = \frac{Pr(R_{it}=1, D_{it}=1, R_{it-1}=1, D_{it-1}=1)}{Pr(R_{it-1}=1, D_{it-1}=1)}$ and $P_{0i} = \frac{Pr(R_{it}=1, D_{it}=1, R_{it-1}=0, D_{it-1}=1)}{Pr(R_{it-1}=0, D_{it-1}=1)}$. We test for the statistical significance of $\frac{\Delta \overline{SD}_R}{\Delta x}$ via delta method. In the same way we derive $\Delta \overline{SD}_{D_{t-1}=0}/\Delta LB$, $\Delta \overline{DE}_{R_{t-1}=0}/\Delta LB$ and $\Delta \overline{DE}_{D_{t-1}=0}/\Delta LB$.

**Table 10**
Estimation results: first-order Markov model, Model (6). This table reports the estimation results of the first-order Markov model for a specification that includes the interaction terms between of $R^*_{t-1}$ and the quarter dummies. Estimation results of initial condition equations are not reported but are available upon request. Results for the constant term, *LIQUIDITY* dummies, and *INDUSTRY* dummies are not reported for brevity. Standard errors are reported in parentheses. The bottom of the table reports the values for the state dependence and discouragement effect and the test statistics for absence of state dependence, exogeneity of initial conditions, and joint exogeneity (see Table 3 for further details).

| | Model (6) | |
|---|---|---|
| | $D_t$ | $R_t$ |
| $R^*_{t-1}$ | −0.739 (.190)*** | 1.010 (.188)*** |
| *SIZE* | 0.046 (.008)*** | −0.079 (.024)*** |
| *EXPORT* | 0.149 (.020)*** | −0.130 (.032)*** |
| *HHI* | −0.004 (.017) | 0.020 (.022) |
| *BRANCHES* | 0.011 (.005)** | −0.019 (.009)** |
| *SOUTH* | −0.027 (.023) | −0.001 (.033) |
| *GDP GR*. | −0.012 (.008) | 0.006 (.009) |
| *TIME* (*ref*: 2008:q2) | | |
| 2008 : $q3$ | 0.054 (.032)* | −0.022 (.055) |
| 2008 : $q4$ | −0.078 (.034)** | 0.222 (.154) |
| 2009 : $q1$ | −0.156 (.044)*** | 0.262 (.136)* |
| 2009 : $q2$ | −0.153 (.062)** | 0.233 (.129)* |
| 2009 : $q3$ | −0.295 (.070)*** | 0.373 (.146)** |
| 2009 : $q4$ | −0.264 (.054)*** | 0.273 (.074)*** |
| 2008 : $q3 \times R^*_{t-1}$ | 0.220 (.149) | −0.037 (.185) |
| 2008 : $q4 \times R^*_{t-1}$ | 0.307 (.140)** | −0.113 (.241) |
| 2009 : $q1 \times R^*_{t-1}$ | 0.584 (.127)*** | −0.163 (.219) |
| 2009 : $q2 \times R^*_{t-1}$ | 0.425 (.124)*** | −0.188 (.185) |
| 2009 : $q3 \times R^*_{t-1}$ | 0.395 (.128)*** | 0.065 (.194) |
| 2009 : $q4 \times R^*_{t-1}$ | 0.297 (.132)** | 0.095 (.172) |
| *Correlation coeff.* | | |
| $\rho_{21}$ | | −0.638 (.151)*** |
| $\rho_{31}$ | | 0.439 (.018)*** |
| $\rho_{41}$ | | −0.428 (.038)*** |
| $\rho_{32}$ | | 0.041 (.127) |
| $\rho_{42}$ | | 0.023 (.131) |
| $\rho_{43}$ | | −0.980 (.155)*** |
| *Log-likelihood* | −33,565.57 | |
| $\overline{SD}_{R_{t-1}=0}$ | 0.564 (.349)** | |
| $\overline{SD}_{D_{t-1}=0}$ | 0.574 (.355)** | |
| $\overline{DE}_{R_{t-1}=0}$ | −0.557 (.031)*** | |
| $\overline{DE}_{D_{t-1}=0}$ | −0.780 (.042)*** | |
| *State dependence* ($\chi^2_{14}$) | $W$ = 86.465 | |
| *Exogeneity of initial conditions* ($\chi^2_4$) | $LR$ = 1206.585 | |
| *Joint exogeneity* ($\chi^2_6$) | $LR$ = 1178.311 | |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

**Table 11**
Variations in state dependence: Model (6). This table shows the effect on the measures of state dependence of the discrete change from the Pre-Lehman to the Post-Lehman situation. The quantities are computed following expression (16) and by setting to zero/one the quarter dummies for the Pre/Post-Lehman period. Quantities are computes using the parameter estimates of Model (6) displayed in Table 10. Standard errors are computed by Delta Method.

| | Variation in state depedence | s.e. |
|---|---|---|
| $\Delta\overline{SD}_{R_{t-1}=0}/\Delta x$ | 0.097*** | 0.037 |
| $\Delta\overline{SD}_{D_{t-1}=0}/\Delta x$ | 0.107*** | 0.038 |
| $\Delta\overline{DE}_{R_{t-1}=0}/\Delta x$ | −0.125*** | 0.043 |
| $\Delta\overline{DE}_{D_{t-1}=0}/\Delta x$ | −0.104*** | 0.040 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## 5. Conclusions

The ongoing financial crisis has made it clear once again that financial frictions constitute a key determinant of prolonged recessions. As a result, a growing literature – mainly based on general equilibrium theory – is investigating the real effect of financial frictions, building on the seminal contributions by Bernanke and Gertler (1989) and Kiyotaki and Moore (1997). Given the micro foundation of these macro models, the limited empirical evidence on the actual presence of persistence in financial frictions at the firm level is somewhat surprising. This paper is a first contribution to fill this gap investigating state dependence in access to credit on a representative sample of Italian manufacturing firms.

We use a first-order Markov model to estimate state dependence. We take into account the possible biases arising from sample selection and from the endogeneity of the initial conditions, jointly modeling the probability of applying for bank credit and of being credit denied in two consecutive periods. Considering that in each period a firm may: (i) be credit restricted, (ii) not demand bank credit, and (iii) have full access to bank credit, we are able to estimate the degree of state dependence in credit restriction and the strength of the discouragement effect.

Keeping in mind some caveats due to data limitations, our results show that firm access to credit is characterized by state dependence in credit restriction and discouragement effects. State dependence in access to credit varies across firm characteristics. Once credit restricted, small and informationally opaque firms are less likely to apply for credit than large firms (i.e., the discouragement effect is stronger), but they are less subject to state dependence in credit restriction. Finally, we document that state dependence, although heightened by global liquidity shocks, is not an exclusive feature of crisis periods.

## Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at http://dx.doi.org/10.1016/j.jfs.2016.08.003.

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