Why Do Borrowers Pledge Collateral?
New Empirical Evidence on the Role of Asymmetric Information

Allen N. Berger
University of South Carolina
Wharton Financial Institutions Center
CentER – Tilburg University
aberger@moore.sc.edu

Marco A. Espinosa-Vega
International Monetary Fund
mespinosa@imf.org

W. Scott Frame
Federal Reserve Bank of Atlanta
scott.frame@atl.frb.org

Nathan H. Miller
U.S. Department of Justice
nathan.miller@usdoj.gov

Abstract

An important theoretical literature motivates collateral as a mechanism that mitigates adverse selection, credit rationing, and other inefficiencies that arise when borrowers have *ex ante* private information. There is no clear empirical evidence regarding the central implication of this literature – that a reduction in asymmetric information reduces the incidence of collateral. We exploit exogenous variation in lender information related to the adoption of an information technology that reduces *ex ante* private information, and compare collateral outcomes before and after adoption. Our results are consistent with this central implication of the private-information models and support the economic importance of this theory.

JEL Classification Numbers: G21, D82, G32, G38
Keywords: Collateral, Asymmetric Information, Banks, Small Business, Credit Scoring

The views expressed are not purported to reflect those of the Federal Reserve Bank of Atlanta, the International Monetary Fund, the U.S. Department of Justice, or their staffs. The authors thank Lamont Black, Hans Degryse, Bob DeYoung, Craig Furfine, Bill Keeton, Steven Ongena, Tara Rice, Francisco Rodriguez-Fernandez, Richard Rosen, Greg Udell, Laurent Weill, Tracie Wojdtke, Alexander Zeisler, and seminar participants at the Federal Reserve System Committee on Financial Structure and Regulation, the Financial Management Association conferences in Barcelona and Orlando, the Financial Intermediation Research Society Conference in Anchorage, the Federal Reserve Bank of Chicago and DePaul University seminar series, Fordham University, University of South Carolina, University of Tennessee, Tilburg University, and University of Texas at Arlington for helpful comments. Pam Frisbee, Phil Ostromogolsky, and Shalini Patel provided outstanding research assistance.

Why Do Borrowers Pledge Collateral?
New Empirical Evidence on the Role of Asymmetric Information

I. Introduction

Although collateral is a widely observed debt contracting feature, the underlying motivation for collateral is not well understood. An important set of theoretical models explains collateral as arising from *ex ante* information gaps between borrowers and lenders. Specifically, when borrowers have private information regarding their project quality, the equilibrium may be characterized by adverse selection and credit rationing (Stiglitz and Weiss 1981, Wette 1983). The pledging of collateral may allow lenders to sort observationally equivalent loan applicants and mitigate these inefficiencies. In particular, lenders may offer a menu of contract terms such that applicants with higher-quality projects choose secured debt at lower premiums, while those with lower-quality projects select unsecured debt at higher premiums (e.g., Bester 1985, 1987, Besanko and Thakor 1987a, 1987b, Chan and Thakor 1987, Boot, Thakor and Udell 1991, Beaudry and Poitevin 1995, Schmidt-Mohr 1997).

Recent research, however, suggests that collateral may not always be optimal within the *ex ante* private information framework (Carlier and Renou 2005, 2006). Furthermore, an expansive theoretical literature invokes alternative frictions that motivate collateral as part of an optimal contract. These frictions include risk-shifting, reduced effort, and other *ex post* moral hazard concerns (e.g., Holmstrom and Tirole 1997, Aghion and Bolton 1997), limited contract enforceability (e.g., Banerjee and Newman 1993, Albuquerque and Hopenhayn 2004, Cooley, Marimon, and Quadrini 2004), or an inability of lenders to monitor project outcomes at sufficiently low cost (e.g., Townsend 1979, Gale and Hellwig 1985, Williamson 1986, Border and Sobel 1987, Mookherjee and Png 1989, Boyd and Smith 1994, Krasa and Villamil 2000, Lacker 2001, Hvide and Leite 2006).

In this paper, we isolate and test a central empirical prediction that is distinctly generated by the *ex ante* private-information models. In particular, we test whether a reduction in *ex ante* information gaps between borrowers and lenders is associated with a lower incidence of collateral. Our test exploits variation in *ex ante* lender information created by the adoption of an information-enhancing loan underwriting technology. The test isolates the private-information models by focusing only on the *ex ante* information environment (i.e., information gaps that are present when the loan is made), rather than the *ex post* frictions featured in other theoretical models. Thus, a finding that the technology is associated with a lower incidence of collateral may be interpreted as consistent with the central implication of the *ex ante* private-information literature.[1,2] Of course such a finding cannot rule out a role for *ex post* frictions – such as moral hazard, limited contract enforceability, and/or costly monitoring – in affecting observed collateral pledges.

Our data set provides an advantageous laboratory in which to test the central empirical prediction. We match the contract terms of nearly 14,000 individual newly-issued loans to small businesses between 1993 and 1997 from the Federal Reserve's Survey of Terms of Bank Lending Technology (STBL) with Call Report data on the 37 large U.S. banks that extended these credits. We also include data from a 1998 Atlanta Federal Reserve survey on whether, when, and how these banks employ small business credit scoring technology (SBCS), which provides our measure of asymmetric information. The combined data set allows for a rich set of controls at both the loan and bank level, as well as for bank and time fixed effects to account for unobserved bank heterogeneity and changes in the lending environment, respectively.

Small business credit scoring combines data on the personal credit history of the small business owner with firm financial data to generate a "score" which reflects repayment probabilities.[3] The SBCS technology may be used exclusively or in a way that augments other lending technologies – such as financial statement lending, asset-backed lending, and/or relationship lending. To isolate those cases in which SBCS technology is most likely to reduce informational asymmetries, our analysis focuses on banks that use this technology in conjunction with other lending technologies. Recent research supports the notion that SBCS improves the lender's information set when the technology is used in this fashion (e.g., Berger, Frame, and

[1] The alternative hypothesis, strictly speaking, is a combination of sub-hypotheses that includes: 1) private information contributes to collateral usage, 2) the loan underwriting technology reduces private information, and 3) the regression model is correctly specified. Thus, a finding of no relationship between the loan underwriting technology and the incidence of collateral would also be consistent with the loan underwriting technology having only modest effects on informational asymmetries; it would not determine with certainty that private information is unimportant.

[2] Inderst and Mueller (2007) provide an alternative model in which collateral arises due to informational advantages of the lending bank vis-à-vis its competitors. The model shares the prediction that an increase in the information available to the lending bank reduces the incidence of collateral.

[3] The personal information used in SBCS models (obtained from consumer credit bureaus) may include the owner's monthly income, outstanding debt, financial assets, employment tenure, home ownership, and previous loan defaults or delinquencies (Mester 1997). Although credit scoring models were applied to consumer loans well before the sample period, their application to business loans was delayed due to concerns regarding firm heterogeneity and nonstandardized documentation across firms (Berger and Frame 2007).

Miller 2005, Berger, Espinosa-Vega, Frame, and Miller 2005). The extant research also finds the adoption of SBCS to be exogenous in that it is unrelated to the bank's prior portfolio composition, financial condition, and market characteristics (e.g., Frame, Srinivasan, and Woosley 2001, Akhavein, Frame, and White 2005).

By way of preview, the results from a baseline specification suggest that the adoption of SBCS reduces the incidence of collateral by nearly 6 percentage points. The effect is statistically significant and sizeable relative certain variance measures we calculate from the data. Alternative specifications designed to address specific conceptual concerns yield a range of estimates – the impact of SBCS is sometimes smaller and sometimes greater than 6 percent – but in each case we find that SBCS is associated with a statistically significant reduction in the use of collateral. Overall, we interpret the results as providing empirical evidence that *ex ante* private information contributes to the widespread prevalence of collateral.

Our results build on an extant empirical literature that finds only mixed support for role of *ex ante* private information. We argue below that our empirical design avoids certain systemic biases that may have hampered the consistency of estimation in this literature. That said, at least two important caveats apply to our results. The first is that we draw inferences about the *probability* that individual loan applicants post collateral using data on the *proportion* of loans with collateral. This inference may be suspect if SBCS changes the loan underwriting process (e.g., accept/reject decisions) in ways that alter the mix of loan applicants and/or the mix of loan recipients. In robustness checks, we provide some empirical evidence that the observable characteristics of borrowers and loans do not change dramatically following the adoption of SBCS – these results lend credence to our empirical strategy. The second caveat is that our focus on small business loans may limit the generality of the results. Of course, the focus also conveys a potentially important advantage in evaluating the theoretical literature because small businesses tend to fit the profile of firms under conditions of asymmetric information featured in the theoretical models. The small business data may also yield the most insight regarding the policy issue of credit availability, given that these firms are likely to suffer the greatest reductions in funding when their pledgeable asset values are impaired due to external shocks.

We think it is important to identify the specific informational frictions that underlie the observed widespread use of collateral. Collateral pledges impose costs on both lenders and borrowers. This contracting mechanism requires that lenders incur screening costs associates with valuing the pledged assets; the costs of monitoring the assets; and any enforcement/disposal expenses in the event of repossession (e.g., Leeth and Scott 1989). The use of collateral also imposes opportunity costs on borrowers to the extent that it ties up assets that might otherwise be put to more productive uses. Borrowers may also suffer fluctuations in their credit availability as the values of their pledgeable assets vary.

The common application of collateral may also have macroeconomic consequences. Changes in the values of pledgeable assets that are correlated across borrowers – due to external shocks such as interest rate spikes, oil price increases, or real estate bubbles – may amplify the business cycle through procyclical changes in access to credit (e.g., Bernanke and Gertler 1989, 1990, Kiyatoki and Moore 1997). Indeed, recent empirical evidence suggests that the significant decline in real estate collateral values in Japan in the early 1990s played an important role in reducing debt capacity and investment in that nation (Gan 2007). A similar situation may be currently playing out with U.S. real estate. Moreover, the ongoing global financial crisis appears to have been amplified by collateral margin calls and increased collateral requirements in short-term funding markets.

The remainder of the paper is structured as follows. Section II reviews the existing empirical evidence and Section III outlines our econometric methodology. Section IV describes the data and variables used in these econometric tests, respectively. We present the test results in Section V and conclude in Section VI.

II. Empirical Literature Review

A number of studies examine the empirical relationship between asymmetric information and the incidence of collateral. These papers use the strength of the lender-borrower relationship – as measured by length or breadth, or whether the lender is the borrower's main or only lender – as an inverse proxy for the degree of asymmetric information. Lenders may gather proprietary information about the borrower's character, reliability, and project choice as their relationship with the borrower strengthens (e.g., Petersen and Rajan 1994, Berger and Udell 1995, Degryse and van Cayseele 2000). The empirical association between collateral and relationship strength is sometimes found to be negative as predicted by the *ex ante* private-information models (e.g., Berger and Udell 1995, Harhoff and Korting 1998, Chakraborty and Hu 2006); in other cases it is found to be positive (e.g., Machaer and Weber 1998, Elsas and Krahnen 2000, Ono and Uesegi

2005); while a third set of studies finds mixed signs (e.g., Degryse and van Cayseele 2000, Jiminez, Salas, and Saurina 2006, Menkhoff, Neuberger, and Suwanaporn 2006, Voordeckers and Steijvers 2006).

These analyses may have problems that diminish their usefulness for testing the implications of the *ex ante* private-information theory, which could also help explain the mixed empirical findings. First, the results could be biased towards a positive association between collateral and relationships to the extent that lenders sort borrowers into different lending arrangements based on their opacity. In particular, a positive coefficient may occur if lenders use relationships to evaluate more opaque small businesses (e.g., Berger and Udell 2002) – precisely those borrowers that pledge collateral in models based on moral hazard and other *ex post* frictions. Second, the results could be biased toward a negative association to the extent that collateral and relationships are substitute methods of dealing with opacity problems. For example, lenders may often require that borrowers pledge fixed assets such as real estate, motor vehicles, or equipment as collateral to resolve information problems instead of using evidence acquired through strong relationships (e.g., Manove, Padilla, and Pagano 2001, Berger and Udell 2006). Our methodology sidesteps these complications by exploiting exogenous variation in *ex ante* private information.[4]

Another set of studies examine the empirical association between borrower risk (broadly defined) and collateral, rather than that between asymmetric information and collateral. The private-information models suggest that borrowers with low <u>unobservable</u> risk may signal this through the pledging of collateral. However, the private-information models do not have a prediction regarding the correlation between <u>observable</u> risk and the incidence of collateral, although the empirical literature finds one (e.g., Leeth and Scott 1989, Berger and Udell 1990, 1995, Booth 1992, Degryse and Van Cayseele 2000). For instance, some of these studies find that collateral is associated with higher risk premiums among small business loans (e.g., Berger and Udell 1990, Degryse and Van Cayseele 2000).[5] This result seems to run counter to the

[4] The empirical association between collateral and relationship strength may also in part reflect the exercise of market power through a non-price term of credit. Some of the theoretical literature on relationship lending predicts that loan rates rise over the course of a relationship as a borrower becomes "locked-in" to its current lender because of its informational advantage over other potential lenders (e.g., Greenbaum, Kanatas, and Venezia 1989, Sharpe 1990, Rajan 1992). It is also possible that lenders may use this market power to extract collateral pledges more often from borrowers with strong relationships.

[5] Weill and Godlewski (2006) show that collateral and risk premiums may be negatively related in nations characterized by higher levels of asymmetric information – as measured by variables such as accounting standards

prediction of the private-information models that high-quality borrowers opt to pledge collateral and pay lower interest rates. However, it may be the case that collateral differences more often reflect observed quality differences, rather than unobserved differences.

In another article of evidence on the private-information models, Jiminez, Salas, and Saurina (2006) show that collateral is negatively related to *ex post* defaults on debt issued to young firms. The authors argue that *ex post* defaults may reflect high unobserved risk and hence *ex ante* private information.[6] However, because collateral may raise the cost of default, one might expect to find that secured debt is less likely to default, irrespective of whether *ex ante* asymmetric information is important. Moreover, defaults may reflect moral hazard or other frictions, and thus may not isolate the effects of *ex ante* private information. Our methodology, which exploits an exogenous shock to the level of *ex ante* informational asymmetries, avoids the problems that characterize tests based on the collateral-risk relationship. In particular, our methodology allows us to isolate the specific effect of *ex ante* private information from those of *ex post* frictions and other potentially confounding factors.

III. Outline of the Econometric Methodology

We test the central prediction of the *ex ante* private-information models regarding collateral and asymmetric information using data on the terms of individual small business loan contracts, the banks that extend these loans, and whether and how these banks employ the SBCS lending technology. We base the test on a logit model of whether collateral was pledged on the individual loans:

$$ln\, [P(COLLAT_{ijt}) / (1 - P(COLLAT_{ijt}))] = \beta_1 SCORE_{jt} + x_{ijt}'\beta_2 + \alpha_j + \gamma_t\, , \qquad (1)$$

where $P(\bullet)$ indicates probability, $COLLAT_{ijt}$ is a dummy variable that equals 1 if the loan is secured, and i, j, and t index loans, banks, and time, respectively. The key exogenous variable is $SCORE_{jt}$, which takes a value of one if bank j employs SBCS in a manner that reduces informational asymmetries in time t, and zero otherwise. The vector x_{ijt} includes other loan and

and the level of financial development of the nation – a result that is consistent with the *ex ante* private-information models.

[6] Abbring, Chiappori, Heckman, and Pinquet (2002) discuss the merits of inferring *ex ante* private information from *ex post* claims in insurance markets.

bank control variables. The scalars α_j and γ_t capture differences in the probability that collateral is pledged due to fixed effects for bank *j* and time *t*, respectively.

A negative, statistically and economically significant estimate for the parameter β_1 would be consistent with the prediction of the private-information models that a reduction in *ex ante* asymmetric information lowers the probability that collateral is pledged.[7] Of course, such a finding cannot rule out a role for *ex post* frictions – such as moral hazard, limited contract enforceability, and/or costly monitoring – in affecting observed collateral pledges. As discussed below, we remove loan observations from the data set when the employment of SBCS has ambiguous implications with respect to reducing asymmetric information. In all cases, our empirical results are robust to changing the inclusion rules.

In equation (1), the estimate of β_1 is primarily determined by loans from banks for which *SCORE* takes on values of both 0 and 1 within the data set – i.e., banks that adopted SBCS during the sample period and have both before- and after- adoption observations available. Loans by other banks in the sample have no direct influence on the estimate of β_1 because they have no variation in *SCORE*. These other banks are of three types. First, some banks had not adopted SBCS by the end of the sample period ($SCORE_{jt} = 0$ for all *t*). Second, some banks had adopted the technology prior to the sample period and therefore had experienced any information benefits at some earlier time ($SCORE_{jt} = 1$ for all *t*). Finally, some sample banks adopted SBCS during the sample period, but have no observations available prior to adoption because one of the underlying data sets had no observations for these institutions prior to adoption ($SCORE_{jt} = 1$ for all *t* after adoption, no observations for $SCORE_{jt}$ prior to adoption). The inclusion of loans by banks with no variation in *SCORE* directly improves the estimation efficiency of the coefficients of the loan and bank control variables β_2 and the time fixed effects γ_t, and thereby indirectly contributes to improving the estimation efficiency of β_1, the *SCORE* effect.

Our empirical test is essentially equivalent to differences-in-differences estimation and presents two important econometric issues. First, the parameters are consistently estimated despite our use of fixed effects within a discrete-choice framework. The ratio of observations to parameters tends to infinity as the number of loans per bank-quarter grows large, and as the

[7] The alternative hypothesis is a combination of sub-hypotheses that includes that 1) private information contributes to collateral usage, 2) the use of SBCS reduces asymmetric information and 3) the regression model is correctly specified. A finding of no relationship between SBCS and the incidence of collateral would not determine with certainty that asymmetric information is unimportant.

number of banks and quarters rise together. Our sample features an average of 19 loans per bank-quarter, 37 banks, and 20 quarters. As a result, we are able to use nearly 14,000 observations to estimate 65 total parameters, including one parameter for $SCORE_{jt}$ (β_1), seven parameters for the control variables (β_2), 37 bank effects (α_j), and 20 time effects (γ_t).

Second, we use a clustering correction that provides consistent estimates of the *t* statistics in the presence of arbitrary correlation patterns (including autocorrelation) among loan observations from the same bank.[8] Bertrand, Duflo, and Mullainathan (2004) show that autocorrelation may cause differences-in-differences estimators to yield upwardly-biased *t* statistics that over-reject the null. However, they also note that the clustering correction we employ works well when then number of sample states – the number of sample banks in our case – is large, on the order of 50. Our baseline sample includes data on 37 banks.

IV. Data and Variables Employed in the Tests

We combine data from three sources to estimate equation (1) and test the main hypothesis about the effects of *ex ante* asymmetric information on the probability that collateral is pledged. The first source is the Federal Reserve's Survey of Terms of Bank Lending (STBL). Respondents to this survey include virtually all of the largest U.S. banks plus a stratified random sample of smaller institutions. The STBL contains details on the loan contract terms of all newly-issued domestic commercial and industrial (C&I) loans by surveyed banks during one or more days of the first week of the second month of each quarter. The terms include whether collateral is pledged – the basis for the dependent variable in equation (1) – as well as information on whether the loan is issued under commitment, the amount of the loan and commitment (if any), and whether the loan has a floating interest rate.

Our second data source is the January 1998 Survey of Small Business Credit Scoring conducted by the Federal Reserve Bank of Atlanta. This survey targets many of the same large institutions as the STBL, including 99 of the largest 200 U.S. banking organizations operating at that time. The available information includes whether lenders employed SBCS as of 1997:Q4, and if so, the date that they initially adopted the technology. The survey responses also provide data on how the adopting institutions employ the technology – specifically whether they simply

[8] Petersen (2009) finds that – of the standard error correction methods used in finance panel data sets – the clustering correction is the only that yields unbiased estimates.

use credit scores to automatically approve/reject loan applications versus using SBCS in a manner that supplements their existing underwriting techniques (Frame, Srinivasan, and Woosley 2001). The SBCS Survey data are used to construct the *SCORE* variable, and to determine whether and when this technology likely reduced asymmetric information.

Finally, we gather statistics from regulatory reports on the banks that issue the loans – items from Call Reports, Summary of Deposits, and the National Information Center. These regulatory files provide information on the financial statements, ownership, and market characteristics for virtually all U.S. banks. We use these data to construct control variables for the bank's size, age, financial condition, recent merger activity, and local market concentration.

Our regression sample is compiled by matching data from these three sources, so that each observation includes loan contract information from the STBL, data on whether, when, and how large U.S. banking organizations employed small business credit scoring from the SBCS Survey data, and statistics on the banks themselves from the regulatory files. The sample contains observations over the period 1993:Q1-1997:Q4. The SBCS technology was introduced to many U.S. large banks during this interval.

We exclude observations from the regression sample when there are ambiguities about whether the use of SBCS reduces informational asymmetries. First, we omit observations from banks that use SBCS to automatically accept/reject credit applications, rather than to supplement the information from other loan evaluation methods. Second, we exclude loans made in the two quarters following a bank's adoption of SBCS to lessen the effects of any learning curves associated with implementing this new technology. Third, we exclude loans for which the total credit (maximum of loan amount or commitment amount) is over $100,000 because SBCS is often applied by lenders only on loans up to this size, and so would have no informational effect for larger credits. Finally, we omit data on loans not issued under commitment. Prior research finds that commitment loans are more often relationship-based and therefore are likely to be associated with greater asymmetric information problems (e.g., Berger and Udell 1995).[9] We show below that our empirical results are robust to altering all of these exclusion rules.

Our main regression sample includes 13,973 loans made by 37 different large banks, 19 of which use SBCS to supplement other loan evaluation methods and 18 of which do not use this

[9] The STBL data are observed when the commitment line is drawn, not when the line is signed. The contract terms of some loans observed shortly after SBCS adoption may have been set prior to SBCS adoption. This may bias the *SCORE* coefficient towards zero, against the prediction of the private information models.

technology in any way over the sample interval. As discussed above, the estimated effect of *SCORE* is primarily determined by loans from banks that adopted the technology during the sample period and have both before- and after- adoption observations available. In our sample, 16 of the 19 adopting banks are in this category – one bank had adopted prior to the sample period and two banks adopted during the sample interval, but were added to the STBL data set only after adoption. As discussed above, the inclusion of the three adopting banks for which *SCORE* = 1 for all observations and the 18 non-adopters for which *SCORE* = 0 for all observations improve estimation efficiency.

Table 1 provides the means and standard deviations of the variables used in our main regressions. The dependent variable, *COLLAT*, is a dummy variable that equals 1 if the loan is secured. The key exogenous variable is *SCORE*, a dummy that equals one if the bank adopted SBCS at least two quarters before the loan was made. As shown, more than 80 percent of the sample loans have collateral pledged, and about 50 percent are made by banks that use SBCS in a way that is likely to reduce *ex ante* asymmetric information.

We control for two loan contract terms in our analysis: total loan size, including the amount of any commitment (*SIZE*), and a dummy variable indicating whether the loan has a floating interest (*FLOAT*). Table 1 shows that most of the loans carry floating rates and that the average loan size is just below $50,000. Recall that we limit *SIZE* to $100,000 or less because many banks use SBCS only for credits below this limit. These contract terms are potentially endogenous, as they may be determined jointly with the *COLLAT* contract term. However, we show below that our findings are robust to dropping these contract terms. We also control for five bank characteristics, gross total assets (*GTA*), bank age (*AGE*), the ratio of nonperforming loans (past due at least 30 days or nonaccrual) to gross total assets (*NPL*), whether the bank was involved in a merger in the previous year (*MERGED*), and the weighted-average market Herfindahl index of deposit concentration (*HERF*). The characteristics are constructed from the previous year's regulatory reports to mitigate potential endogeneity problems. The average *GTA* is about $16.5 billion and the average *AGE* is almost 120 years. There are no small or young banks in the sample because the SBCS survey queries only large institutions. The means of *NPL*, *MERGED* and *HERF* are 0.014, 0.429, and 0.203 respectively.

V. Empirical Results

A. Main Regression Results

Table 2 presents our main regression results examining the effects of *SCORE* on the likelihood that collateral is pledged. The logit regression represented by equation (1) is estimated for four specifications that alternatively include or exclude the loan and bank control variables. Each regression includes bank and time fixed effects. Robust *t* statistics are calculated using a clustering correction for heteroskedasticity and arbitrary correlations among loan observations from the same bank.

The estimates for β_1, the coefficient on *SCORE*, are negative and statistically significant at the 1% level in all four specifications in Table 2. These findings are consistent with the central prediction of the private-information models that a reduction in *ex ante* asymmetric information lowers the probability that collateral is pledged. For the specification in column (4), which includes all of the loan and bank control variables, the *SCORE* coefficient is -0.449. The corresponding estimates in columns (1), (2), and (3) – which exclude all of the control variables, just the bank variables, and just the loan variables, respectively – the coefficients are quite similar, -0.530, -0.534, and -0.438.[10]

To evaluate whether these effects are economically significant, we convert the coefficients from the nonlinear logit model into predicted changes in the probability that collateral is pledged. In the second row of the table, we show *Predicted Δ P(COLLAT)*, which is the predicted change in the probability that collateral is pledged from changing *SCORE* from 0 to 1 at the sample means of the other exogenous variables.[11] For the full specification in column (4), *Predicted Δ P(COLLAT)* = -0.057, suggesting that the use of SBCS to augment other loan underwriting methods reduces estimated collateral incidence by roughly 6 percent. This result is robust – the figures for the other specifications shown in Table 2 are all close to 6 percent. Thus, for a loan at the sample mean *P(COLLAT)* of about 83%, the likelihood that collateral would be

[10] In an additional regression, we add ratio of the dollar value of total small commercial loans under $100,000 to total assets ("small business loan ratio") measured as of the prior June 30. The estimated effect of SBCS on collateral in this regression is similar both in magnitude (*SCORE* coefficient of -0.434) and statistical significance (*t*-statistic of 2.94). The small business loan ratio coefficient is also negative and statistically significant. We exclude the small business loan ratio from the baseline specification due to missing data problems (we observe the control for 12,201 of the loan-quarter observations).

[11] The formula for *Predicted Δ P(COLLAT)* is as follows. Let μ_X be the vector of sample means of control variable vector x_{ijt} over all i, j, and t; q_j be the proportion of loans in the sample made by bank j, and r_t be the proportion of sample loans made in year t. Define δ_1 as $\beta_1 + \mu_X'\beta_2 + \sum q_j \alpha_j + \sum r_t \gamma_t$, and define δ_0 as $\delta_1 - \beta_1$. The values shown for *Predicted Δ P(COLLAT)* are given by $[exp(d_1)/(1 + exp(d_1))] - [exp(d_0)/(1 + exp(d_0))]$, where d_1 and d_0 replace the actual coefficients with the estimated coefficients in δ_1 and δ_0, respectively.

pledged falls to about 77% when SBCS is used to reduce asymmetric information. This finding appears to be economically significant because the use of SBCS to supplement other lending technologies likely closes only a small portion of information gap between the bank and borrower. That is, the estimated 6 percentage point effect likely represents only a minor fraction of the full effect of asymmetric information on collateral decisions.[12]

Turning briefly to the control variables, only three of these variables are statistically significant in the full specification in column (4). The coefficients on *ln(SIZE)*, *ln(GTA)*, and *NPL* suggest that larger credits and larger and financially healthier banks tend to be associated with a higher incidence of collateral. In addition, Wald tests for the fixed effects (not shown) reject the null hypotheses that both the bank and the time effects are jointly zero at the 1% level in all four specifications.[13]

In the remaining discussion, we refer to the findings for the full specification shown in column (4) of Table 2 as our baseline results. These represent our best efforts at choosing the specification and sample that reflect the effects of a reduction in *ex ante* asymmetric information on the likelihood that collateral is pledged.

B. Alternative Specifications and Samples

In Table 3, we alter the specification of equation (1) in ways other than changing the control variables to examine further the robustness of the baseline results. Specifically, column (1) excludes the time effects, column (2) excludes the bank effects, and column (3) excludes both sets of effects. The loan and bank control variables are included in all of these regressions, but their coefficients are not shown in the interest of brevity. The main results are robust with respect to excluding the time fixed effects, but not the bank fixed effects. When only the time effects are excluded in column (1), the estimated coefficient on *SCORE* is -0.423, similar to the baseline coefficient of -0.449, and is statistically significant at the 1% level. The economic significance is also maintained, with only a small change in *Predicted Δ P(COLLAT)* to -0.045.

[12] To help assess the economic significance of the results, we calculate the standard deviation of each bank's average collateralization rate over the sample period. The median standard deviation among the sample banks is 0.12 (or 12 percentage points). Thus, the 6 percentage point effect of SBCS that we calculate seems sizeable relative to variance we observe in the data, in the sense that SBCS adoption reduces average collateralization at the median bank by roughly half a standard deviation.

[13] The results are also statistically and economically similar when controls for loan maturity and the bank's internal loan risk rating are added to the specification. We omit maturity from the main regressions due to potential endogeneity concerns, and we exclude the risk ratings because they are available only for the final three calendar quarters of our sample.

In contrast, the exclusion of bank fixed effects (with or without the time fixed effects) in columns (2) and (3) results in relatively small, statistically insignificant *SCORE* coefficient estimates, and much lower pseudo R-squared statistics. These findings suggest that systematic differences across banks may exist that are not captured by observables. For example, some institutions may require collateral more often than others due to their internal policies and procedures or because these banks tend to specialize in certain lending technologies that rely more heavily on collateral.

In Table 4, we examine the robustness of our baseline results with respect to the use of alternative data samples. Specifically, we examine the effects of using different bank samples, different loan samples, and excluding different numbers of quarters after SBCS adoption. Columns (1) and (2) show the effects of altering the set of banks included in the sample. In column (1), we include 22 additional banks that use credit scores to automatically approve/reject loan applications. In column (2), we restrict the sample to include only those banks present in the data in both 1993 and 1997, reducing the number of sample banks by 11. The STBL bank panel changes somewhat over the sample period due to mergers, bank growth, and other factors, which could potentially introduce sample selection issues. The results in columns (1) and (2) suggest that our baseline results are robust to these changes in bank samples. In both cases, the coefficients on *SCORE* remain negative and statistically significant at the 1% level, and the value of *Predicted Δ P(COLLAT)* is reasonably close to the -0.057 found for the baseline regression.[14,15]

We next show the results from regressions using alternative loan samples. Specifically, we use observations on loans not issued under commitment in column (3), loans of total size up

[14] In an extension of column (1), we include a separate scoring dummy for those banks that use SBCS technology to automatically accept/reject loan applicants. The effect for these banks is small (coefficient of -0.234) and not statistically significant (*t*-statistic of -0.95). By contrast, the original *SCORE* effect is relatively larger (coefficient of -0.305) and statistically significant (*t*-statistic of -1.94). The finding is exactly what one would expect under the private information hypothesis because SBCS generates greater reductions in informational asymmetries when it is used to supplement the information from other loan evaluation techniques than when it is used to automatically accept/reject applicants.

[15] Although the sample banks are all relatively large, there are significant differences among them. To investigate this heterogeneity, we re-run the baseline regression splitting the banks that adopt SBCS during the sample period separately by their *GTA* and by their ratio of the dollar value of total small commercial loans under $100,000 to total assets ("small business loan ratio"). All of the control banks are included in each regression. The *SCORE* coefficient estimated from banks above the median in terms of *GTA* is -0.357 and the coefficient estimated from banks below the median is -0.623. The *SCORE* coefficient estimated from banks above the median in terms of small business loan ratio is -0.597 and the coefficient estimated from banks below the median is -0.142. The coefficients are all negative, consistent with the private information hypothesis, and the first three coefficients are also statistically different than zero.

to $50,000 in column (4), and loans of total size between $50,000 and $100,000 in column (5). The sample in our baseline regression includes only loans issued under commitment, which are expected to be associated with greater asymmetric information problems, and credits of all sizes up to $100,000, the maximum size on which many lenders use the SBCS technology. In all three alternative samples, the coefficients on *SCORE* are negative, statistically significant, and of economically significant magnitude.[16] Again, the findings support the robustness of the baseline results and suggest that our finding that the adoption of SBCS is associated with less collateral is not due to specific loan sample restrictions.

Columns (6), (7), and (8) give the findings when we exclude different numbers of quarters after SBCS adoption: zero quarters (column (6)), one quarter (column (7)), and four quarters (column (8)). The sample used in the baseline regression excludes two quarters to reduce the effects of any learning curve associated with implementing the technology. The *SCORE* coefficients are all again negative and statistically significant, consistent with the baseline regression. However, the value of *Predicted Δ P(COLLAT)* is smaller when zero quarters are excluded, which suggests that the new technology may take some time to significantly improve lender information.

C. Compositional Shifts

We motivate the baseline specification under the assumption that it is possible to draw inferences about the probability that individual loan applicants post collateral using data on the proportion of loans with collateral. The assumption may be suspect if the adoption of SBCS changes the loan underwriting process (e.g., accept/reject decisions) in ways that alter the mix of loan applicants and/or the mix of loan recipients. Although we cannot rule out the possibility that such compositional shifts are important, we find little evidence that the observable characteristics of borrowers and loans change dramatically following the adoption of SBCS.

To start, we investigate the effect of SBCS technology on the total number of loans, the total number of collateralized loans, and the total number of non-collateralized loans. One could

[16] Perhaps surprisingly, the estimate for loans not issued under commitment was larger than the estimate for loans issued under commitment. We believe that this may largely be due to the following phenomenon. As mentioned previously, for commitment loans, there can be a significant lag between when loan terms are established and when the loan is actually booked. So for loan commitments made prior to SBCS adoption but drawn after adoption, we would not expect to observe an effect of the technological innovation resulting in some bias toward zero for commitment loans. As a result, non-commitment loans may have a stronger measured effect than commitment loans – even if the true effect may be smaller.

plausibly argue that the main results are more likely to reflect changes in the probability that loan applicant's post collateral if: 1) the total number of loans does not change, 2) the number of collateralized loans falls, and 3) and the number of non-collateralized loans increases. To test these hypotheses, we regress the three variables on *SCORE*, bank fixed effects, and time fixed effects. We employ OLS and cluster the standard errors at the bank level. The unit of observation is at the bank-quarter level.[17] We find no statistically significant effect of SBCS technology on the total number of loans; the *SCORE* coefficient is negative (-30.33) but imprecisely estimated (*t* statistic of -1.35).[18] By contrast, we find that SBCS technology decreases the number of collateralized loans and increases the number of non-collateralized loans, and that the changes are statistically significant. The SCORE coefficients are –35.73 (*t* statistic of 1.65) and 5.39 (*t* statistic of 3.15), respectively.

Next, we examine the effect of SBCS technology on two important contract terms: the maturity of the loan and the total loan size. One could argue that compositional shifts among loan applicants and recipients are less likely to drive the main results if these terms are relatively unaffected by SBCS technology. To conduct the tests, we regress the two variables on *SCORE*, bank fixed effects, and time fixed effects. We employ OLS and cluster the standard errors at the bank level. The unit of observation is at the loan-quarter level. We find little effect of SCBS technology on either contract term. With respect to maturity, the *SCORE* coefficient is small (0.15) and not significant (*t* statistic of 0.68). The *SCORE* coefficient is again small (-2.28) and not significant (*t* statistic of -0.72) in the loan size regression.[19]

Finally, we examine the effect of SBCS technology on the average risk of borrowers. We make use of the banks' internal loan risk ratings, which are available in the STBL starting in 1997:Q2. Again, one could argue that compositional shifts among loan applicants and recipients are less likely to drive the main results if risk ratings are relatively unaffected by SBCS

[17] We weight observations by the total number of bank-quarter loans to make the results comparable to the baseline logit results, which exploit variation at the loan-quarter level.
[18] The finding that SBCS technology is uncorrelated with the total number of small business loans among banks that do not use the technology to automatically accept/reject applicants is consistent with the previous empirical findings of Berger, Frame and Miller (2005) regarding the effect of SBCS on the small business loan ratios for these banks.
[19] These results should be interpreted with caution. Economic theory suggests that loan maturity is related to borrower risk (both observed and unobserved); and that this relationship may be nonmonotonic (e.g., Flannery 1986, Diamond 1991). We estimate only the net effect. Also, we restrict our sample to credits of total size less than \$100,000 because many banks employ SBCS only for these smaller credits. The size regression captures movement within this range. We do not capture, for example, loan commitments that increase size beyond the \$100,000 cut-off.

technology. We regress the risk ratings on *SCORE*, the bank and loan control variables, and time fixed effects. Given the shorter sample period of data availability, the *SCORE* coefficient is not identifiable in the presence of bank fixed effects (only two banks adopt during 1997:Q2-1997:Q4), and we exclude bank fixed effects from the regression. The risk rating characterizes loans as being of “minimal,” “low,” “moderate,” and “acceptable” risk, and we use ordered logit to capture the effect of SBCS on risk accordingly. The resulting *SCORE* coefficient is small (0.089) and not statistically significant (*t* statistic of 0.110). Overall, these empirical results provide some evidence that compositional shifts may not drive the main results of the paper.

D. Remaining Issues

We now address two remaining empirical issues. First, we explore the implicit assumption in the baseline specification that SBCS has a level effect on collateralization – rather than the measured effect spuriously reflecting a trend away from loan collateralization. We create a variable TREND1 that equals 0 for non-scorers and equals 1 for banks in their second quarter after the adoption of credit scoring, 2 for banks in their third quarter after adoption, and so on. (Recall that observations for the quarter of adoption and the quarter following are omitted from our analysis.) We estimate a collateral regression including SCORE and TREND1, in addition to our standard loan variables, bank variables, bank fixed effects, and time fixed effects. Table 5 presents the results. As shown in column (1), the SCORE coefficient remains negative (--0.464) and statistically significant (*t* statistic of -3.52) and corresponds to a 5.8 predicted percentage point reduction in collateralization. By contrast, the coefficient for TREND1 is positive, small and statistically insignificant.

In a closely related test, we attempt to capture trends in collateralization both before and after the adoption of SBCS. We create a variable TREND2 that is a linear time trend. For banks that adopted SBCS in quarter T, $TREND2_{T-t} = -t$ and $TREND2_{T+t} = t-1$ with t=0 and t=1 dropped from the analysis as before. TREND2 always equals zero for banks not adopting SBCS during the sample. We estimate a collateral regression including SCORE, TREND2, and SCORE*TREND2 in addition to our standard loan variables, bank variables, bank fixed effects, and time fixed effects. As shown in column (2) of Table 5, the SCORE coefficient remains negative (-0.326) and statistically significant (*t* statistic of -3.18) with a predicted change in the probability of collateral of 4.1%. TREND2 is also negative (-0.028) and statistically significant at the 10% level (*t* statistic of -1.85); while the interaction term SCORE*TREND2 is positive

(0.027) but statistically insignificant (*t* statistic of 1.25). Taken together, these results support the conjecture that the adoption of SBCS has a level effect on the incidence of collateral, consistent with baseline specification.

The evidence that banks have fewer secured loans in the period preceding the adoption of SBCS (i.e., TREND2 < 0 in the prior regression) raises the question of whether SBCS adoption is actually exogenous. We argue that the assumption of exogeneity is reasonable in this case. Previous research finds little connection between SBCS adoption and the bank's prior portfolio composition, financial condition, and market characteristics (e.g., Frame, Srinivasan, and Woosley 2001, Akhavein, Frame, and White 2005, Berger, Frame, and Miller 2005). We also note that if borrowers become less able to provide collateral over time and this causes SBCS adoption, then one might also expect SBCS technology to be associated with changes in borrower risk. The risk rating regressions discussed above suggest that this did not occur.

Second, the baseline specification includes observations from 18 banks that do not use SBCS technology during the sample period. We assume that these banks are similar to those banks that adopt SBCS (aside from having different bank fixed effects). Under this assumption, the inclusion of these banks improves the estimation efficiency of *SCORE* coefficient and the control variables. However, if these banks are fundamentally different, then the baseline *SCORE* coefficient could be asymptotically inconsistent.[20] As a robustness check, we run the baseline regression excluding banks that do not use SBCS technology during the sample period. The resulting *SCORE* coefficient remains negative (-0.229) and is statistically significant (*t* statistic of 2.05); the estimate corresponds to a 2.4 percentage point drop in collateralization due to SBCS. This regression provides additional support for the baseline results.

VI. Conclusions

The theoretical literature identifies collateral as a key contracting tool employed by lenders to reduce problems associated with asymmetric information. In particular, an important set of models suggests that collateral may mitigate adverse selection and reduce credit rationing when borrowers have *ex ante* private information regarding the quality of their project. The central implication of these *ex ante* private-information models is that an attenuation of the

[20] This could happen if the prevalence of collateral increases/decreases *faster* for banks that do not adopt SBCS. We are not aware of any stylized facts that support such an effect. Banks that do not use SBCS average 1.5 percent of their assets in small business loans relative to 1.0 percent for banks that do use SBCS.

information gap between borrowers and lenders should reduce the incidence of collateral. Previous findings regarding this implication are mixed, and may be hampered by issues relating to endogenous selection and other biases.

In this paper, we sidestep the problems of the existing empirical literature by employing data on an exogenous technological innovation that was not introduced to most large U.S. banks until the mid-1990s. Specifically, we use data on whether, when, and how large U.S. banks employed small business credit scoring (SBCS) over the period 1993:Q1-1997:Q4, focusing on cases in which this technology supplements other loan evaluation techniques to reduce asymmetric information. We combine the SBCS data with information on collateral and other contract terms on about 14,000 newly-issued small business loans and data on the banks themselves.

The empirical results support the central prediction of the *ex ante* private-information models. The data are consistent with a fall in the use of collateral when banks adopt SBCS and use it to supplement information from other lending technologies. The findings are both statistically and economically significant and are robust to a number of alternative specifications and changes in sample. The results suggest that banks that used the new technology to reduce information gaps during our sample interval lessened their need for collateral on a significant number of small business loans. The findings further imply that the employment of SBCS may have reduced lender and borrower costs and improved the efficiency of a segment of the small business lending market.

Our empirical application examines the effects of just one new lending technology on credits to one class of borrower over one time interval. Nonetheless, our findings may have more general implications. The results suggest that any market advances (e.g., new technologies, financial contracting tools) or policy innovations (e.g., improved disclosure rules/enforcement) that appreciably reduce information gaps between borrowers and lenders may improve the efficiency of debt markets by reducing reliance on costly collateral. Such developments may also bring about substantially greater credit availability for some potential borrowers – particularly those with severe asymmetric information problems or without access to pledgeable collateral – as the need for collateral is reduced. Any improvements in information that substantially reduce dependence on collateral may also reduce procyclicality and other adverse macroeconomic consequences associated with external shocks to asset values.

References

Abbring, Jaap H., Pierre-Andre Chiappori, James J. Heckman, and Jean Pinquet (2003). "Adverse Selection and Moral Hazard in Insurance: Can Dynamic Data Help to Distinguish," *Journal of the European Economic Association*, 1, 512-521.

Aghion, Philippe and Patrick Bolton (1997). "A Theory of Trickle-Down Growth and Development," *Review of Economic Studies,* 64, 151-172.

Albuquerque, Rui and Hugo A. Hopenhayn (2004). "Optimal Lending Contracts and Firm Dynamics," *Review of Economic Studies,* 71, 285-315.

Akhavein, Jalal, Frame, W. Scott Frame, and Lawrence J. White (2005). "The Diffusion of Financial Innovations: An Examination of the Adoption of Small Business Credit Scoring by Large Banking Organizations," *Journal of Business*, 78, 577-96.

Banerjee, Abhijit V. and Andrew F. Newman (1993). "Occupational Choice and the Process of Development," *Journal of Political Economy*, 101, 274-298.

Beaudry, Paul and Michel Poitevin (1995). "Competitive Screening in Financial Markets when Borrowers and Firms can Recontract," *Review of Economic Studies*, 62, 401-423.

Berger, Allen N., Marco Espinosa-Vega, W. Scott Frame, and Nathan Miller (2005). "Debt, Maturity, Risk, and Asymmetric Information," *Journal of Finance*, 60, 2895-2923.

Berger, Allen N. and W. Scott Frame (2007). "Small Business Credit Scoring and Credit Availability," *Journal of Small Business Management*, 47, 5-22.

Berger, Allen N., W. Scott Frame, and Nathan Miller (2005). "Credit Scoring and the Availability, Price, and Risk of Small Business Credit," *Journal of Money, Credit, and Banking*, 37, 191-222.

Berger, Allen N. and Gregory F. Udell (1990). "Collateral, Loan Quality, and Bank Risk," *Journal of Monetary Economics*, 25, 21-42.

Berger, Allen N. and Gregory F. Udell (1995). "Relationship Lending and Lines of Credit in Small Firm Finance," *Journal of Business*, 68, 351-381.

Berger, Allen N. and Gregory F. Udell (2002). "Small Business Credit Availability and Relationship Lending: The Importance of Bank Organizational Structure," *Economic Journal*, 112, F32-F53.

Berger, Allen N. and Gregory F. Udell (2006). "A More Complete Conceptual Framework for SME Finance," *Journal of Banking and Finance,* 30, 2945-2966.

Bernanke, Ben and Mark Gertler (1989). "Agency Costs, Net Worth, and Business Fluctuations," *American Economic Review*, 79, 14-31.

Bernanke, Ben S. and Mark Gertler (1990). "Financial Fragility and Economic Performance," *Quarterly Journal of Economics*, 105, 87-114.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan (2004). "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics*, 119, 249-275.

Besanko, David and Anjan V. Thakor (1987a). "Collateral and Rationing: Sorting Equilibria in Monopolistic and Competitive Credit Markets," *International Economic Review*, 28, 671-689.

Besanko, David and Anjan V. Thakor (1987b). "Competitive Equilibrium in the Credit Market under Asymmetric Information," *Journal of Economic Theory*, 42, 167-183.

Bester, Helmut (1985). "Screening vs. Rationing in Credit Markets with Imperfect Information," *American Economic Review*, 75, 850-855.

Bester, Helmut (1987). "The Role of Collateral in Credit Markets with Imperfect Information," *European Economic Review*, 106, 60-75.

Boot, Arnoud W.A., Anjan V. Thakor, and Gregory F. Udell (1991). "Secured Lending and Default Risk: Equilibrium Analysis, Policy Implications and Empirical Results," *Economic Journal*, 101, 458-472.

Booth, James R. (1992). "Contract Costs, Bank Loans, and the Cross-Monitoring Hypothesis," *Journal of Financial Economics*, 31, 25-42.

Border Kim C. and Joel Sobel (1987). "Samurai Accountant: A Theory of Auditing and Plunder," *Review of Economic Studies*, 54, 525-540.

Boyd, John H. and Bruce D. Smith (1994). "The Equilibrium Allocation of Investment Capital in the Presence of Adverse Selection and Costly State Verification," *Economic Theory*, 3, 427-451.

Carlier, Guillaume and Ludovic Renou (2005). "A Costly State Verification Model with Diversity of Opinions," *Economic Theory*, 25, 497-504.

Carlier, Guillaume and Ludovic Renou (2006). "Debt Contracts with *Ex-ante* and *Ex-post* Asymmetric Information: An Example," *Economic Theory*, 28, 461-473.

Chakraborty, Atreya and Charles X. Hu (2006). "Lending Relationships in Line-of-Credit and Nonline-of-Credit Loans: Evidence from Collateral use in Small Business," *Journal of Financial Intermediation*, 15, 86-107.

Chan, Yuk-Shee, and Anjan V. Thakor (1987), "Collateral and Competitive Equilibria with Moral Hazard and Private Information," *Journal of Finance*, 42, 345-363.

Cooley, Thomas, Ramon Marimon, and Vincenzo Quadrini (2004). "Aggregate Consequences of Limited Contract Enforceability," *Journal of Political Economy*, 112, 817-847.

Degryse, Hans and Patrick Van Cayseele (2000). "Relationship Lending within a Bank-Based System: Evidence from European Small Business Data," *Journal of Financial Intermediation*, 9, 90-109.

Diamond, Douglas W (1991). "Debt Maturity Structure and Liquidity Risk." *Quarterly Journal of Economics* 106, 709-738.

Elsas, Ralf and Jan Pieter Krahnen (2000). "Collateral, Default Risk, and Relationship Lending: An Empirical Study on Financial Contracting." CEPR Discussion Paper 2540.

Flannery, Mark J. (1986). "Asymmetric Information and Risky Debt Maturity Choice." *Journal of Finance* 41, 19-37.

Frame, W. Scott, Aruna Srinivasan, and Lynn Woosley (2001). "The Effect of Credit Scoring on Small Business Lending," *Journal of Money, Credit, and Banking*, 33, 813-825.

Gale, Douglas and Martin Hellwig (1985). "Incentive-Compatible Debt Contracts: The One-Period Problem," *Review of Economic Studies*, 52, 647-663.

Gan, Jie (2007). "Collateral, Debt Capacity, and Corporate Investment: Evidence from a Natural Experiment," *Journal of Financial Economics*, 85, 709-734.

Greenbaum, Stuart I., George Kanatas, and Itzhak Venezia (1989). "Equilibrium Loan Pricing under the Bank-Client Relationship," *Journal of Banking and Finance*, 13, 221-235.

Harhoff, Dietmar and Timm Korting (1998). "Lending Relationships in Germany – Empirical Evidence from Survey Data," *Journal of Banking and Finance*, 22, 1317-1353.

Holmstrom, Bengt and Jean Tirole (1997). "Financial Intermediation, Loanable Funds, and the Real Sector," *Quarterly Journal of Economics*, 62, 663-691.

Hvide, Hans K. and Tore Leite, (2006). "Optimal Debt Contracts under Costly Enforcement," Manuscript, University of Aberdeen Business School.

Inderst, Roman, and Holger M. Mueller (2007). "A Lender-Based Theory of Collateral," *Journal of Financial Economics*, 84, 826-859.

Jiminez, Gabriel, Vincente Salas, and Jesus Saurina, 2006. "Determinants of Collateral." *Journal of Financial Economics*, 81, 255-281.

Kiyatoki, Nobuhiro and John Moore (1997). "Credit Cycles," *Journal of Political Economy*, 105, 211-248.

Krasa, Stefan and Anne P. Villamil (2000). “Optimal Contracts when Enforcement is a Decision Variable,” *Econometrica*, 68, 119-134.

Lacker, Jeffrey M. (2001). “Collateralized Debt as the Optimal Contract,” *Review of Economic Dynamics*, 4, 842-859.

Leeth, John D., and Jonathan Scott (1989). “The Incidence of Secured Debt: Evidence from the Small Business Community,” *Journal of Financial and Quantitative Analysis*, 24, 379-394.

Machauer, Achim and Martin Weber (1998). “Bank Behavior Based on Internal Credit Ratings of Borrowers,” *Journal of Banking and Finance*, 22, 1355-1383.

Manove, Michael, A. Jorge Padilla, and Marco Pagano (2001). “Collateral versus Project Screening: A Model of Lazy Banks,” *Rand Journal of Economics*, 32, 726-744.

Menkhoff, Lukas, Doris Neuberger, and Chodechai Suwanaporn (2006). “Collateral-based Lending in Emerging Markets: Evidence from Thailand,” *Journal of Banking and Finance*, 30, 1-21.

Mester, Loretta (1997). “What’s the Point of Credit Scoring?” Federal Reserve Bank of Philadelphia Business Review, 3-16.

Mookherjee, Dilip and Ivan Png (1989). “Optimal Auditing, Insurance, and Redistribution,” *Quarterly Journal of Economics*, 104, 399-415.

Petersen, Mitchell A. (2009). “Estimating Standard Errors in Finance Panel Data Sets: Comparing Approaches,” *Review of Financial Studies*, 22, 435-480.

Petersen, Mitchell A. and Raghuram G. Rajan (1994). “The Benefits of Lending Relationships: Evidence from Small Business Data,” *Journal of Finance*, 110, 407-443.

Rajan, Raghuram G. (1992). Insiders and Outsiders: The Choice between Informed and Arm’s-Length Debt,” *Journal of Finance*, 47, 1367-1400.

Schmidt-Mohr, Udo (1997). “Rationing Versus Collateralization in Competitive and Monopolistic Credit Markets with Asymmetric Information,” *European Economic Review*, 41, 1321-1342.

Sharpe, Steven A. (1990). “Asymmetric Information, Bank Lending, and Implicit Contracts: A Stylized Model of Customer Relationships,” *Journal of Finance*, 45, 1069-1087.

Stiglitz, Joseph E. and Andrew Weiss (1981). “Credit Rationing in Markets with Imperfect Information,” *American Economic Review*, 71, 393-410.

Townsend, Robert M. (1979). "Optimal Contracts and Competitive Markets with Costly State Verification," *Journal of Economic Theory*, 21, 265-293.

Voordeckers, Wim and Tensie Steijvers (2006). "Business Collateral and Personal Commitments in SME Lending," *Journal of Banking and Finance,* 30, 3067-3086.

Weill, Laurent and Christophe Godlewski (2006). "Does Collateral Help Mitigate Adverse Selection? A Cross-Country Analysis," MPRA Working Paper 2508, University Library of Munich, Germany.

Wette, Hildegarde C. (1983). "Collateral in Credit Rationing in Markets with Imperfect Information: Note." *American Economic Review*, 73, 442-445.

Williamson, Stephen D. (1986). "Costly Monitoring, Financial Intermediation and Equilibrium Credit Rationing," *Journal of Monetary Economics*, 18, 159-179.

Table 1
Variables and Summary Statistics

Means and standard deviations for variables used in subsequent estimation. The sample combines loan observations from 19 large banks that use small business credit scoring technology over 1993:Q1-1997:Q4, but not to automatically approve/reject loans, with loan observations from 18 large banks that do not use this technology in any capacity during this interval. Loan observations from the first two quarters following credit scoring adoption are excluded. *COLLAT* is a dummy that equals 1 if the loan is secured. *SCORE* is a dummy that equals 1 if the bank uses small business credit scoring technology when the loan is made. *SIZE* is the maximum of the loan amount and the amount of commitment. *FLOAT* is a dummy that equals one if the loan has a floating interest rate. *GTA* is the gross total assets of the bank. *AGE* is the age of the bank. *NPL* is the bank's ratio of nonperforming loans (past due at least 30 days or nonaccrual) to *GTA*. *MERGED* is a dummy that equals one if the bank was involved in a merger the previous year. *HERF* is the bank's weighted-average market Herfindahl index of deposit concentration. Bank variables are constructed from the previous year's regulatory reports. The loans considered have *SIZE* less than or equal to $100,000 and are issued under commitment. The total sample size is 13,973. Sources: Federal Reserve's Survey of Terms of Bank Lending (STBL) for *COLLAT*, *SIZE* and *FLOAT*; January 1998 Federal Reserve Bank of Atlanta survey on the use of credit scoring for *SCORE*; bank regulatory reports (Call Reports, Summary of Deposits, National Information Center) for *GTA*, *AGE*, *NPL*, *MERGED* and *HERF*.

Variable	Description	Mean	Std Dev	25%	50%	75%
Dependent variable:						
COLLAT	Loan is secured (1=yes)	0.825	0.380	1.000	1.000	1.000
Credit scoring dummy:						
SCORE	Bank uses credit scoring (1=yes)	0.505	0.500	0.000	1.000	1.000
Loan variables						
SIZE	Loan size ($000)	48.544	28.734	24.466	47.087	72.005
FLOAT	Floating interest rate (1=yes)	0.917	0.277	1.000	1.000	1.000
Bank variables						
GTA	Gross total assets ($000)	16,718,600	20,827,050	3,878,491	9,558,315	27,057,860
AGE	Age of the bank (years)	119.062	23.332	112.000	119.000	130.000
NPL	Nonperforming loans ÷ *GTA*	0.015	0.008	0.010	0.013	0.019
MERGED	Merged last year (1=yes)	0.445	0.497	0.000	0.000	1.000
HERF	Average market Herfindahl	0.203	0.051	0.180	0.193	0.224

Table 2

Main Collateral Regressions

Logit regressions for *COLLAT*, a dummy variable that equals one if the loan is secured. The sample combines loan observations from 19 large banks that use small business credit scoring technology over 1993:Q1-1997:Q4, but not to automatically approve/reject loans, with loan observations from 18 large banks that do not use this technology in any capacity during this interval. The loans considered have *SIZE* of less than or equal to $100,000 and are issued under commitment. Loans made during the first two quarters following credit scoring adoption are excluded. Robust *t* statistics are calculated using a clustering correction for heteroskedasticity and arbitrary correlations among loan observations from the same bank. *Predicted Δ P(COLLAT)* indicates the predicted change in the probability that collateral is pledged from changing *SCORE* from 0 to 1 at the means of the other exogenous variables. Significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)
Credit scoring dummy:				
SCORE	-0.530***	-0.534***	-0.438***	-0.449***
	(-3.42)	(-3.60)	(-2.91)	(-3.10)
Predicted Δ P(COLLAT)	-0.066	-0.066	-0.056	-0.057
Loan variables:				
ln(SIZE)		0.356***		0.353***
		(3.95)		(3.88)
FLOAT		-0.374*		-0.321
		(-1.74)		(-1.44)
Bank variables:				
ln(GTA)			0.393***	0.384***
			(3.55)	(3.67)
ln(AGE)			15.488	15.379
			(1.58)	(1.62)
NPL			-8.374**	-7.560*
			(-2.03)	(-1.72)
MERGED			-0.035	-0.064
			(-0.33)	(-0.63)
HERF			0.340	0.672
			(0.20)	(0.41)
Bank fixed effects	yes	yes	yes	yes
Time fixed effects	yes	yes	yes	yes
Pseudo R-Squared	0.096	0.106	0.098	0.108
Number of obs.	13,973	13,973	13,973	13,973

Table 3

Robustness Tests: Additional Alternative Specifications

Logit regressions for *COLLAT*, a dummy variable that equals one if the loan is secured. The sample combines loan observations from 19 large banks that use small business credit scoring technology over 1993:Q1-1997:Q4, but not to automatically approve/reject loans, with loan observations from 18 large banks that do not use this technology in any capacity during this interval. The loans considered have *SIZE* of less than or equal to $100,000 and are issued under commitment. Loans made during the first two quarters following credit scoring adoption are excluded. Robust *t* statistics are calculated using a clustering correction for heteroskedasticity and arbitrary correlations among loan observations from the same bank. *Predicted Δ P(COLLAT)* indicates the predicted change in the probability that collateral is pledged from changing *SCORE* from 0 to 1 at the means of the other exogenous variables. Significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	(1)	(2)	(3)
Credit scoring dummy:			
SCORE	-0.423***	0.085	0.274
	(-2.83)	-0.27	-0.76
Predicted Δ P(COLLAT)	-0.045	0.012	0.038
Loan variables	yes	yes	yes
Bank variables	yes	yes	yes
Bank fixed effects	yes	no	no
Time fixed effects	no	yes	no
Robust *t* statistics	yes	yes	yes
Pseudo R-Squared	0.106	0.033	0.026
Number of obs.	13,973	13,997	13,997

Table 4
Robustness Tests: Alternative Samples

Logit regressions for *COLLAT*, a dummy variable that equals one if the loan is secured. The baseline sample combines loan observations from 19 large banks that use small business credit scoring technology over 1993:Q1-1997:Q4, but not to automatically approve/reject loans, with loan observations from 18 banks that do not use this technology in any capacity during this interval. Unless otherwise noted, loans have *SIZE* less than or equal to $100,000, are issued under commitment, and are not made during the first two quarters following credit scoring adoption. Robust *t* statistics are calculated using a clustering correction for heteroskedasticity and arbitrary correlations among loan observations from the same bank. *Predicted Δ P(COLLAT)* indicates the predicted change in the probability that collateral is pledged from changing *SCORE* from 0 to 1 at the means of the other exogenous variables. Significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	Different bank samples:		Different loan samples:			Different # of quarters excluded after adoption:		
	Includes banks that use credit scoring to automatically approve/reject	Includes only banks that are present in both 1993 and 1997	Loans not issued under commitment	Loans of up to $50,000	Loans of $50,000-$100,000	None	One	Four
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Credit scoring dummy:								
SCORE	-0.422***	-0.458***	-0.532**	-0.397***	-0.567***	-0.327**	-0.405***	-0.445***
	(-2.81)	(-3.06)	(-2.49)	(-3.01)	(-2.76)	(-2.26)	(-2.71)	(-2.81)
Predicted Δ P(COLLAT)	-0.072	-0.055	-0.114	-0.044	-0.077	-0.032	-0.050	-0.052
Loan variables	yes	yes	yes	yes	yes	yes	yes	yes
Bank variables	yes	yes	yes	yes	yes	yes	yes	yes
Bank fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Time fixed effects	yes	yes	yes	yes	Yes	yes	yes	yes
Pseudo R-Squared	0.238	0.105	0.179	0.165	0.053	0.105	0.107	0.107
Number of obs.	21,980	12,858	8,807	8,582	5,339	14,780	14,357	13,087

Table 5
Robustness Tests:
Trends in Collateralization Before and After SBCS Adoption

Logit regressions for *COLLAT*, a dummy variable that equals one if the loan is secured. The sample includes loan observations from 19 large banks that adopt small business credit scoring technology over 1993:Q1-1997:Q4, but not to automatically approve/reject loans. The loans considered have *SIZE* of less than or equal to $100,000 and are issued under commitment. Loans made during the first two quarters following credit scoring adoption are excluded. Robust *t* statistics are calculated using a clustering correction for heteroskedasticity and arbitrary correlations among loan observations from the same bank. *Predicted Δ P(COLLAT)* indicates the predicted change in the probability that collateral is pledged from changing *SCORE* from 0 to 1 at the means of the other exogenous variables. Significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

	(1)	(2)
Credit scoring dummy:		
SCORE	-0.464***	-0.326***
	(-3.52)	(-3.18)
Predicted Δ P(COLLAT)		
TREND1	0.008	---
	(0.46)	---
TREND2	---	-0.028*
	---	(-1.85)
*TREND2*SCORE*	---	0.027
	---	(1.25)
Loan variables	yes	yes
Bank variables	yes	yes
Bank fixed effects	yes	no
Time fixed effects	no	yes
Robust *t* statistics	yes	yes
Pseudo R-Squared	0.108	0.108
Number of obs.	13,973	13,973