Comment

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Western Europe and the United States suffered a severe financial (notably banking) crisis, followed by a strong economic recession. These phenomena are not unique: banking crises are recurring phenomena, triggering deep and long-lasting recessions (Kindleberger 1978; Reinhart and Rogoff 2009). In a crisis, the main channel by which weaknesses in the balance sheets of banks affect the real sector is via a reduction of credit supply (Bernanke 1983). Moreover, banking crises—importantly—are not random phenomena, but come after periods of very strong bank credit growth (Kindleberger 1978; Schularick and Taylor 2012; Gourinchas and Obstfeld 2012; Bordo and Meissner 2012).

The academic literature has put more emphasis on credit supply restrictions during crises than on the precrisis behavior of credit. However, as the previous papers suggest, high precrisis aggregate credit growth is a very good predictor of banking crises. In fact, recent evidence by the International Monetary Fund (IMF) shows that ex ante credit growth is a strong robust correlate of future financial crises using ample cross-country data from approximately 170 countries (Dell'Ariccia et al. 2012). Other papers restrict their analysis to few countries but use long historical data showing a strong correlation between ex ante credit growth and ex post likelihood of banking crises (Schularick and Taylor 2012; Jordà, Schularick, and Taylor 2011; Bordo and Meissner 2012).

Yet there are many credit booms that do not end up in a financial crisis. Of course, credit booms may just follow strong economic fundamentals (credit demand driven). The IMF evidence, in fact, suggests that two-thirds of credit booms do not end up in a financial crisis.

The historical episodes or the cross-country analysis do not use detailed micro data sets due to data limitations, thus they can only analyze

aggregate precrisis credit growth, and the implications of their analyses are that strong pre-crisis credit growth is a key predictor of banking crisis. On the other hand, the majority of credit booms do not end up in a crisis. That is, credit booms support investment and consumption, but they can frequently end up in damaging financial crises whose ex post costs may greatly exceed the benefits associated with the boom. Therefore, it is clear that not all credit booms are the same and there must be factors that differentiate them, but these cannot pick up by an analysis of aggregate credit data.

Disaggregated micro data sets on credit are, on the contrary, crucial to understanding the driving forces of the boom and, hence, the likelihood of financial crises. In particular, one can understand whether the credit boom is due to: (a) credit supply or demand driven: for example, due to financial innovation (securitization) and liberalization or, on the other hand, due to better nonfinancial borrowers' net worth and risk; or (b) moral hazard issues in the banking sector due to suboptimal regulation and compensation schemes versus behavioral aspects as neglected risks. Therefore, micro data sets on credit help to understand the determinants of risk-taking and to provide some ex ante information on whether the credit boom is going to end up in a crisis.[1]

Besley, Meads, and Surico (henceforth BMS) use a very detailed micro loan-level data set on UK mortgage loans over 1975 to 2005, matched with loan and borrower variables. The authors estimate a credit supply function that allows for heterogeneity in risk pricing (through quantile regressions), using changes in the tax system for housing transactions to instrument for loan demand.

Though there is a growing literature on papers using micro data sets on credit (in this discussion I cite some of those), the new elements by BMS are to analyze a long time series of microdata on credit (crucial to comparing credit supply before and after the different important banking shocks, which go back to the liberalizations in the 1980s) and to analyze it with a quantile regression focusing on unobservable heterogeneity in risk.

Besley, Meads, and Surico have the following results: over 1975 to 2005 riskier borrowers were increasingly penalized in loan rates for borrowing more. However, the period of 1995 to 2005 was characterized by a sharp fall in risk pricing and little evidence of heterogeneity, consistent with a relaxation of credit standards prior to the financial crisis (higher bank risk-taking). Moreover, securitization seems to be the driving force.

This is a very interesting paper, which uses a very detailed micro data set with a long time series for the micro data sets on credit. In addition, the micro-based results are new. However, the results are not surprising. The reduction of spreads, especially for the riskier borrowers, was highlighted by policymakers before the start of the crisis for sovereign, financial, and nonfinancial borrowers (European Central Bank [ECB] 2006). Though in all these markets banks are important (and possibly banking deregulation), securitization is not. My main comments will link the reduction of risk heterogeneity to bank shocks—notably, on bank securitization and competition.

First, it is important to understand what is (and what is *not*) in the micro data set used by BMS to understand the strengths and weaknesses of the analysis. They use a loan-level data set, with 600,000 mortgage loans from the United Kingdom over 1975 to 2005. There are some data issues that were not clear: the frequency of the data (yearly, quarterly, monthly data); the number of observations in each year; whether they use the universe of loans or a random sample; and so forth. Moreover, 2006 and 2007 were missing and indeed these two years witnessed the period of strongest securitization, a potential key factor in the softening of lending standards.

The data set have the following loan characteristics: loan size, the rate of interest charged, purchase price (house value), down payment (which BMS refer to as housing wealth), and loan-to-value (LTV). The borrower characteristics are age, income, previous tenure of the household, and the region. Previous tenure status includes information on any prior track record as a mortgage borrower; that is, a variable on lack of credit history. "No credit history" could be an interesting ex ante risky variable that the authors were not using. It would be important to know the correlations across the loan and borrower variables (for example, LTV and income, and others).

The data set have limitations, in particular they are important variables not present in the data set. These limitations include loan characteristics: the granting of loan applications (rejections), the value of collateral (value of house plus the wealth of the family given recourse loans, also other loans from the household), loan maturity, whether loans are securitized or not, and ex post loan defaults. Other limitations are borrower characteristics: risk and net worth of the household. On the supply of credit (lenders') characteristics: the identity of the lender, characteristics of the lender as bank capital, liquidity, size and others, as

bank wholesale funding costs (retail deposit rates are not the marginal funding rates for banks as used in the paper).

A direct way to analyze risk pricing is to regress loan rates on ex ante borrower risk—for example, credit history, credit score, or rating (the variable u of the model). This could, in fact, be the second-stage regression of a selection model where the granting of loan applications gives the first-stage regression. Besley, Meads, and Surico do not have ex ante borrower risk measures or loan applications, so they use a different approach. In fact, there is a variable about "no credit history," but it is not used in the paper. It is important to note that not using the first-stage regression on the granting of loan applications can bias the results on the analysis on loan outcomes of granted loans (Jiménez et al. 2012b).

The empirical strategy followed by the paper is the following: BMS regress loan rate on loan volume, controlling for other loan and borrower variables, and add time fixed effects (based on a model of credit supply and risk heterogeneity). If borrower-region-time variables control well for risk, then the higher the loan volume, the higher should be the loan rate (based on unobserved risk by the econometrician).

However, BMS do not mention that loan size L may depend on risk ($= L(u)$) through credit rationing (Stiglitz and Weiss 1981). It would be nice to have a discussion on this. It could be that credit rationing is only important in loan application rejections. However, the results of BMS could be rationalized as there is credit rationing in loan size but not in applications at the time of securitization (i.e., securitization affects differently the extensive and intensive margin of lending, see evidence on this by Jiménez et al. 2012). In this case, granted loan applications with higher credit volume would not have higher loan rates since only better borrowers have access to those larger loans.

It is important also to note that loan rates depend on the degree of banking competition (not only on borrower risk)[2] and, moreover, there are important banking competition shocks across the thirty years of data (a period with important banking liberalization shocks). Importantly, time dummies may not be enough to control for these shocks as, for example, lack of bank competition is stronger for opaque (riskier) borrowers who can be held up by banks (see Rajan 1992). In consequence, time dummies cannot control perfectly for banking competition as competition may affect different types of borrowers and banks. Moreover, shocks to bank capital and liquidity may affect credit supply

and this may vary over the business and monetary cycle and depends differently for each lender (see, e.g., Jiménez et al. 2012a).

Besley, Meads, and Surico use an instrument on tax changes for loan demand. But it was not very clear whether it was the level or the changes of taxes, nor was there a discussion on the exclusion restriction, since, for example, these taxes could affect bank profits and hence loan rates. Moreover, LTV is also endogenous (not only loan volume) since the down payment not only depends on the household wealth but also on the lending standards applied by banks (the supply of credit). All in all, the instrument is not the strength of this paper, but the strength is the excellent data set and how the authors use them (through quantile regressions) to understand the change in lending standards over the last thirty years.

In the thirty-year period of UK data, there are important time-varying shocks. There are several structural shocks in the UK banking system and mortgages during those thirty years. The authors describe some shocks on banking competition and financial innovation. There are several important shocks on bank competition. The chart measuring competition is based on an HHI (Herfindahl–Hirschman index), which is not ideal (and in fact suggests that competition goes down, not up, in the 1990s to the 2000s, when there was important banking deregulation and liberalization shocks). Moreover, financial innovation (securitization) has increased massively during the 2000s. An important shock in the 1990s and 2000s was financial globalization, which is missing in the paper, but is important for the United Kingdom (see, e.g., Kalemli-Ozcan, Peydró, and Papaioannou 2010 and 2012). Another important factor could have been changes in banking supervision and regulation (see Freixas and Rochet 2008).

All these structural shocks affect bank risk-taking (risk heterogeneity in credit supply) and there is a large literature in banking on this (see the overview by Freixas and Rochet 2008) and, therefore, time dummies are not enough to control for these changes, which affects banks differentially.

Moreover, there are also time-varying shocks at higher frequency—the business and monetary cycle. Lending conditions thus depend on the business cycle (Ciccarelli, Maddaloni, and Peydró 2012). They also depend on borrower and lender net worth and risk and these balance sheet characteristics vary every quarter or month.

Therefore, my main suggestion is to control for (quarter or year) time-varying coefficients. Both for controlling for time-varying shocks

and for understanding the causes of the reduction of heterogeneity in risk pricing, coefficients (in at least some regressions) should be (every quarter or year) time varying. That is, to run cross-sectional regressions every quarter (month or year) and plot the coefficients over time.

Then one can check when (and why) there is the decrease of heterogeneity. In particular, one can check the correlation of the time-varying coefficients with changes in banking competition, securitization and globalization, and also with the business and monetary cycle. My gut feeling is that it could be that the large reduction of heterogeneity was due to changes in competition. As explained before, competition and risk-taking are very interlinked in banking (Freixas and Rochet 2008).

The paper seems to suggest that financial innovation/securitization is or may be the driving force in the softening of lending standards. Securitization influences in at least two ways: (1) the banker may monitor (and/or screen, and/or price) less because bank securitizes (sells) the loans; (2) there may be a reduction of funding costs for all loans by relaxing regulatory bank capital constraints and increasing bank liquidity (Maddaloni and Peydró 2011), and even the capacity to securitize loans may matter in softening of lending standards, not only the actual securitization (Jimenez et al. 2012). Given the credit supply function formula by BMS, the effect should be stronger for the riskier loans.

Controlling for time-varying coefficients may not be enough, as one caveat is that actual securitization may be not high but still securitization (through the capacity to securitize assets) may crucially matter for credit supply (see Diamond and Rajan [2001] for models showing that expectations over bank liquidity may matter, not only actual liquidity). Moreover, an even more serious concern is identification through time series may just give correlations but not causality.

An alternative, better identification strategy is to use microidentification at the loan or bank level. Bank level is not feasible due to lack of data. However, it could still work through loan-level if there are any (even slight) restrictions for securitization depending on the LTVs. For example, in Spain it is easier to securitize MBS if the LTVs are lower than 80 percent. If there were any cost associated to securitization depending on LTV in the United Kingdom, one could use it to get identification exploiting LTV variation within the same time period within the discontinuity of the LTV. Alternatively, one could use region-level identification as in Mian and Sufi (2009) if there are data on securitization at the county level.

Finally, there is still substantial price heterogeneity in loan rates in

1995 to 2005, therefore a crucial question is what the determinants are. The paper shows that there is no heterogeneity on loan rates on volume, but what about the other measures of risk as income, age, and LTV? That is, it would be nice to report the time-varying coefficients of the other variables in the split of the three decades as it is done for the over-all sample (ideally as well for year or quarter time-varying coefficients). Maybe the LTV margin was more important than loan size in the 2000s and, hence, one needs to look at alternative measures and identification margins—as I said before, not only loan volume is endogenous but also LTV. Moreover, because mortgages are with recourse, maybe the action is in other loans—for example, in Spain, where mortgage loans are also with recourse, bank risk-taking is mainly on business loans.

All in all, BMS is a very important and interesting paper: the majority of papers are on aggregate credit (credit booms) but very few discuss the heterogeneity and detailed microanalysis of credit. Moreover, crucial to the question, I do not know of any other paper using a detailed microcredit data set going back to 1975. In addition, the question is crucial. Results are very interesting and point to softer credit conditions prior to the crisis. Also one can use this analysis in real time to try to forecast future problems. Though there is a growing literature on papers using micro data sets on credit, the new elements by BMS are to analyze a long time series of microdata on credit and to analyze it with a quantile regression focusing on unobservable heterogeneity in risk. My main comment is that the paper can improve on the identification linking the softer credit conditions to the different bank shocks.

Endnotes

1. For an estimator in real time of credit booms caused by credit supply factors, see Jiménez et al. (2012).

2. See, for example, Rochet and Freixas (2008); Matutes and Vives (1996, 2000); Broecker (1990); Ruckes (2004); Dell'Ariccia and Marques (2006); Hellman, Murdock, and Stiglitz (2000).

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