Comment

Bernard Salanié, Columbia University

While securitization was once touted as a revolution in risk-sharing, it has figured as a prominent culprit in many popular accounts of the developments that led to the Great Recession. Several recent papers have in fact documented the serious flaws of the originate-to-distribute (OTD) model. Some of the evidence is indirect: Mian and Sufi (2009) showed that zip codes with a higher proportion of subprime borrowers had higher credit growth in the years preceding the crisis, despite their lower income growth, and that this credit growth came only from securitized mortgage credit. Purnanandam (2010) matched banks by institutional and borrower characteristics and showed those that made more use of OTD had a higher default rate. In a series of papers, Keys et al. (2010) and Keys, Seru, and Vig (2012) used the fact that GSE guidelines indicated that a FICO score below 620 was “a strong indication that the borrower’s credit reputation is not acceptable.” They showed that loans to borrowers who scored just above 620 were indeed much more likely to be securitized, and more quickly, than loans just below 620, and that they also performed worse. This is a strong indication of either adverse selection, in that these loans were negatively selected by the originators to be resold, and/or lender moral hazard, with originators not monitoring loans once they were off their books. Piskorski, Seru, and Vig (2010) also showed that securitized loans that are sixty or more days delinquent are more likely to be foreclosed than loans that are on the books of the originator.

All of the papers just cited focus on the United States; there is much less evidence on other countries, and this is a first welcome contribution of this paper. While the UK housing market has had a boom and bust similar to that in the United States, there are also some striking differ-
ences. As the authors point out, lenders have more recourse than in the United States in case of default, and indeed defaults have increased much less than in the United States. Perhaps more importantly, securitized credit played a very small role in the UK mortgage market until at least 2000 (figure C1); and while it expanded fast, it remained below the levels reached in the United States. There is also no UK equivalent to Fannie and Freddie, and thus to the distinction between agency and nonagency securitization that plays such an important role in studies of the process in the United States.

The second contribution of this paper is its focus on unobservable heterogeneity in risk: factors that are unobserved to the econometrician. Some of these factors are observed by both lender and borrower, so that they can be built into the terms of the loan: they are collected in the variable \( u \) in the paper. Others are observed by the borrower only. Borrowers who ask for larger loans tend to be riskier (conditional on \( u \)). This correlation is not modeled in the paper, but it may stem from borrower moral hazard (it is harder to pay back a larger loan) or from adverse selection (riskier borrowers tend to go in over their heads). As a consequence, lenders ask for higher interest rates on larger loans. This generates the inverse supply curve on the first panel of figure C2, which is a sorting device: the riskier borrower 2 has preferences that lead him to self-select into a larger loan \( L \) at a higher interest rate \( R \). Securitization allows originators to offload some of the risk by reselling the loans, so that they are willing to make larger loans at any interest rate. As this is more relevant for larger and riskier loans, the inverse supply curve should both shift outwards and become flatter over time, as illustrated in the second panel of figure C2. Each type of borrower borrows more, at a lower interest rate.

Fig. C1. Proportion of mortgage credit funded by securitization in the United Kingdom
Source: FSA (2009).
The paper uses a database of mortgage loans over 1975 to 2005 (a very long time period by the standards of this literature) to show that this seems to be what happened. While the inverse supply curve was upwards sloping until the 1990s, it became almost flat in the years preceding the crisis—more on this later.

Two Methodological Difficulties

Figure C2 suggests a very simple estimation strategy: regress $R$ on $L$ to obtain the slope of the inverse supply curve $R'(L)$, and relate its changes over time to market developments and in particular to the expansion of securitization documented in figure C1. Unfortunately, figure C2 is conditional on information $u$ that is shared by lender and borrower but unobserved to the authors of the paper. Denote its equation (assuming linearity) as

$$R(L; u) = a(u) + b(u)L,$$

then it is easy to see that regressing $R$ on $L$ would give an estimate of

$$\tilde{b} = \frac{\text{cov}(R, L)}{VL} = (1 - R^2) \left( Eb(u) + \frac{\text{cov}(b(u), V(L | u))}{EV(L | u)} \right),$$

where $R^2$ is the share of the variance of loan size $L$ that is explained by the semipublic information $u$. Even if $b(u)$ was independent of $u$, the pooled estimate would be affected by an attenuation bias; and changes in $R^2$ over time could be confused for changes in the slope of the inverse supply curve. A smaller $\tilde{b}$, for instance, would be compatible with an

Fig. C2. Equilibrium in the origination mortgage market, before and after the expansion of securitization
unchanged inverse supply curve if the ability of lenders to screen borrowers had increased over time, reducing the \((1 - R^2)\) factor. Heterogeneity in \(b(u)\) would blur the picture further by introducing the usual aggregation problem: relatively better screening (a lowered \(V(L|u)\)) of borrowers with a higher \(b(u)\) would also spuriously suggest a flatter inverse supply curve.

The authors use quantile regressions to solve this first difficulty. Since \(u\) is just an index we can normalize it to be uniformly distributed on \([0,1]\), so that it is a quantile of risk. Assuming that \(R(L;u)\) is monotonic in \(u\), quantile regressions recover estimates of \(b(u)\) for all quantiles \(u\). This is clearly the right approach and I commend the authors for using it; almost by construction, the shape of the inverse supply curve must depend on semipublic information \(u\) in a nontrivial way. Quantile regressions paint a picture of risk pricing that is much more interesting than the poor and misleading information the least-squares estimates give—see figure 15 in the paper.

Endogeneity concerns are a tougher nut to crack. They come in two shapes. The first one is that identifying the coefficients \(b(u)\) in the quantile regressions require an instrument \(Z\) that changes the loan size chosen by the borrower but is “unrelated” to its \(u\). To illustrate this issue, assume for simplicity that there is only a finite set of possible loan sizes, \(\ell \in M\); and that given a scheme \(R(L;u)\), a borrower with semipublic \((u,)\) and privately observed \((C)\) chooses the size of his loan by maximizing

\[
\max_{C} w(C, R(C;u), Z) + u + C
\]

over \(\ell \in M\). This makes the chosen size \(L\) a complicated function of the \((u,)\) as well as the \((\theta,)\) and \(Z\); and since \(L\) gives information on the relative sizes of the \((u,)\), this rather appealing model violates the conditions set out by Chernozhukov–Hansen (2005).

It seems very hard to think of a variable \(Z\) that can serve as a convincing instrument in this model: it would need to change loan size in a way that does not depend on the semipublic \(u\). Neither macroeconomic conditions nor borrower or bank characteristics fit the bill. In their section IX and appendix C, the authors describe their attempt to use the stamp rate as an instrument, which was much more prominent in the paper presented at the conference. Since the stamp rate has a piecewise constant schedule, one can make an argument that if a borrower who faced a constant stamp rate \(Z_1\) over the range \(M\) of his plausible choices of loan size is shifted to a different stamp rate \(Z_2\) that is also constant over \(M\), then the change in the borrower’s loan size will be dominated by the
change in the stamp rate rather than the borrower’s $u$. This is a clever idea but unfortunately, this instrument turned out to be pretty weak, as the stamp rate is low over the period for most borrowers. The results are accordingly shaky, with implausibly large effects as the authors admit.

The second endogeneity concern is that the inverse supply function $R(L;u)$ itself presumably solves an adverse selection problem. As such, it depends on the distribution $(\theta_i)$ of the private information of the borrowers conditional on $u$, and on the way it enters payoffs of both lenders and borrowers. It makes it even harder to find an instrument: a change in the stamp rate, for instance, should lead lenders to change their supply behavior. But more generally, it raises difficulties about the interpretation of changes in the estimated slope of the inverse supply schedule, as they incorporate changes in the preferences of borrowers.

Disentangling these effects is admittedly a tall order; it may require a structural model of a market with adverse selection, and despite recent progress (e.g., Perrigne–Vuong 2011) identifying such models remains a hard task. It is made even more difficult on credit markets by the “common values” nature of the problem: the lender cares not only about the terms of a loan, but also about the likelihood that the borrower will repay. Still, the discussion of “interpretations” in section VIII leaves demand factors aside, and I hope that the authors will attempt to widen the perspective in future work.

Is It Securitization?

This brings me to the results; let me focus on figure 15, which plots least squares and quantile regression estimates of $b(u)$ for the three decades between 1975 and 2005. The flattening of the inverse supply curve can be seen in two ways: for any risk quantile $u$, the value of $b(u)$ gets smaller over time; and in addition the derivative $b’(u)$ of the quantile regression estimates gets smaller. Thus the inverse supply curves do not only get flatter, they also “fan in,” as shown in figure C3.

Setting aside the caveats I listed before, this seems to be strong evidence that lenders cared less about pricing risk, both of the semipublic $(u)$ and the private $(\theta)$ type. Yet the results are puzzling in some ways. First, it is hard to reconcile the model in the paper and the estimates. Remember that (equation [6] in the paper) the interest spread is

$$R = (1 + \rho)^{-1/T} \left( \frac{\Psi}{T} \right)^{(-1/T)} - 1$$
where $\psi$ is a function of maturity $T$ and the parameter $\gamma$. Now the latter is just $(1 - d)$, where $d$ is a summary “delinquency” factor that multiplies the probability of default and the proportion of the loan that is not recovered upon default:

$$d = (1 - \beta)(1 - \alpha).$$

The example in section IV, subsection A of the paper has $d = 0.005$, so it seems safe to take a first-order expansion around $d = 0$. (Figure C4 confirms that before the crisis, default rates were also quite small in the United States.) This gives

$$R \approx (1 + \rho)d \frac{T + 1}{2T},$$

which is close to $d/2$. This is a simple model, to be sure; yet it makes sense that the variation in the interest rate with loan size (as measured by $b(u)$) should be commensurate with the variations of the delinquency parameter. Now given the estimates in the top panel of figure 15, for the median risk in 1975 to 1985 an increase of 1 percent in loan size should cost about 15 basis points. Given the approximate previous formula, it would take an increase in $d$ of about 0.003. This seems very small, but it represents an increase of 60 percent in delinquency for an increase of 1 percent in the size of the loan. With a median loan size of about £40,000, it is hard to believe that a borrower asking for a loan that is only £400 would be considered as much more likely to go delinquent.

The second surprising feature of the results is that the inverse supply curve becomes quite flat already for most borrowers in 1986 to 1995, when securitization was all but unknown in the United Kingdom (see Fig. C3. Changes in the inverse supply curves over time
If securitization is the reason why the curve is flat after 1996, then the authors owe readers an explanation of why it was already flat before securitization arrived in the United Kingdom. What accounted for the change from the first to the second subperiod? As I emphasized before, both supply-side factors and supply-side reactions to demand-side factors may lie behind it. I look forward to reading more from the authors on this subject.

**Concluding Remarks**

Once again, I applaud this paper for emphasizing the importance of unobserved risk factors in the debate over securitization. While the lack of a proper instrument was a serious limitation of this paper, I hope that their ideas will stimulate further contributions in the same spirit. Richer data sets, with default history, would bring very useful additional information. Finally, it seems to me that in what is essentially a contracting problem, a model of loan demand is necessary to disentangle changes on both sides of the market.

**Fig. C4.** Early payment defaults in the United States

Sources: Mayer, Pence, and Sherlund (2009). Calculations from First American Loan Performance data.
Endnotes

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1. The “Collapsed” column of table A3 is the relevant one since the variation in the stamp rate schedule only has a time dimension.

References


