Recent Developments in Consumer Credit and Payments*

BY MITCHELL BERLIN

On September 22-23, 2011, the Research Department and the Payment Cards Center of the Federal Reserve Bank of Philadelphia held their sixth joint conference to present and discuss the latest research on consumer credit and payments. Eighty-four participants attended the conference, which included seven research papers on the role of home equity in the decision to move to a new job; credit supply and house prices; legally mandated removal of credit remarks; policies to prevent mortgage default; adoption and use of payment instruments by U.S. consumers; liquidity constraints and consumer bankruptcy; and credit supply to bankrupt consumers. In this article, Mitchell Berlin summarizes the papers presented at the conference.

In her welcoming remarks, Loretta Mester, executive vice president and director of research at the Philadelphia Fed, noted that the recent financial crisis has uncovered a range of new issues related to household finance and payments and, further, that the Federal Reserve System has taken on a menu of new responsibilities. She stressed that the long-term research typified by the papers presented at the conference is an essential input into good regulatory policy.

Mester highlighted the variety of research approaches represented in the conference program and stressed the possibilities for integrating the various approaches. In particular, she said that the program included macroeconomic structural models that bring new perspectives that complement the findings of microeconomic studies of consumer credit. Mester argued that, in exchange, the microeconomic studies enrich the macro structural models, which rely on the parameter estimates for their calibration exercises.

Furthermore, she found the extensive use of large micro data sets in a number of the papers striking. The Philadelphia Fed has taken a leading role in managing these large data sets. In particular, the Philadelphia Fed administers RADAR,[1] a data warehouse that serves the Federal Reserve System.

HOUSE PRICES AND JOB SEARCH

In the first paper of the day, Yuliya Demyanyk, of the Federal Reserve Bank of Cleveland, reported on a study (with Dmytro Hryshko, Maria Jose Luengo-Prado, and Bent Sorensen) of the relationship between the decline in house prices and individuals' willingness or ability to move to seek employment. She emphasized that the results were very preliminary and that the audience should view them as provisional. During the recent recession, the record decline in housing prices was cited as one of the reasons for stubbornly high unemployment rates, a view that has generated conflicting reactions in the economic literature. Some economists have argued that households with negative equity have been unable to search for work in more distant labor markets because they are

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*The views expressed here are those of the author and do not necessarily represent the views of the Federal Reserve Bank of Philadelphia or the Federal Reserve System. Links to most of the papers presented can be found on the Philadelphia Fed's website at: http://www.philadelphiafed.org/research-and-data/events/2011/consumer-credit-and-payments/agenda.cfm.

[1] Risk Assessment, Data Analysis, and Research.

unable to sell their houses without defaulting. Demyanyk and coauthors did not find much evidence for this view.

The authors presented results using two methodologies. The first used regression techniques and anonymized data sets from credit bureaus. The second used a calibrated macroeconomic model.

Demyanyk and coauthors' empirical approach was to use regression methods to estimate how the probability of moving was affected by local unemployment rates and households' home equity. Specifically, Demyanyk argued that if the likelihood of moving from a poorly performing local labor market was lower for households with lower home equity, this would be evidence of a *lock-in* effect. The researchers estimated the probability of two different types of moves separately: moves to a different county within the same state and moves to a different state. Although either type of move might be associated with changing jobs — the actual employment outcome cannot be directly observed — Demyanyk argued that moves to a different state were more likely to involve movement to a different labor market.

The researchers performed regressions using two different data sets, each with its own advantages and disadvantages.[2] The first data set merged information from one of the credit reporting agencies (TransUnion) with a separate source of mortgage-loan-level data. The disadvantage of this merged data set is that it is not fully representative; the sample is dominated by subprime homeowners, and as result, prime borrowers are underrepresented and renters aren't included at all. The advantage, however, is that this data set permits the authors to estimate households' home equity with some precision. Demyanyk presented results from a 10 percent subsample of the whole data set at this conference.

The second data set that Demyanyk and coauthors used is from the New York Fed's Consumer Credit Panel, a 5 percent sample of all consumers in the Equifax credit files from 1999-2011. The authors selected a random subset of those consumers and included in their data members of the same household as those consumers.[3] This data set included both prime and subprime homeowners and renters and was thus more nearly representative of households in the nation. But the authors could not directly estimate a household's home equity using this data set. Instead, they used whether local housing prices were rising or falling as a proxy for high or low home equity.

The authors' preliminary conclusion was that there is not much evidence for lock-in effects on the basis of their regression results. For the TransUnion data set, the authors found that in weak local labor markets (negative employment growth), moves to another county were less likely for households with negative equity than for households with positive equity, but moves out of state were more likely for households with negative equity. If the authors' argument that out-of-state moves are more likely to require moving to change jobs is correct, this result is inconsistent with the view that households are locked-in by negative equity. Somewhat more ambiguously, the researchers found that in somewhat stronger local labor markets (positive employment growth), moves both to another county or to another state were less likely for households with negative equity than for households with positive equity.

For the Equifax data set, the authors found no significant effect for rising or falling house prices on the probability of moving either outside the county or outside the state. While Demyanyk said that this offers no evidence for lock-in, she recognized that rising or falling housing prices in a locality are a very noisy indicator of a household's home equity.

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Demyanyk also offered some very preliminary findings from the study of a calibrated macroeconomic model, which she viewed as a way of providing more insight into the precise mechanisms through which housing shocks might affect moving to find a job. The model explicitly included the possibility of unpredictable declines in regional wage income and declines in house prices and also included the possibility of moving to seek a new job in response to both local and distant job offers. The authors conducted an experiment in which some regions experience a housing price decline, some a housing price increase, and some experience no change.

The model generated results broadly consistent with Demyanyk and coauthors' regression findings. They found that unemployed households moved whether or not housing prices had appreciated or fallen and that households with negative equity were more likely than other borrowers to take a distant job.

[2] Note that all the data used by the authors were anonymized. The data sets contain no personally identifiable information.

[3] In this data set, household members are defined as consumers ages 25-66 with the same address as an individual included in the 5 percent sample.

EASY CREDIT AND HOUSE PRICES

Manuel Adelino, of Dartmouth College, reported the results of a study (with Antoinette Schoar and Felipe Severino) that provided evidence that easy credit led to higher home prices during the housing boom. Adelino explained that it is difficult to establish the direction of causality when we observe easier credit terms and rising housing prices. While the rise in house prices might have been caused by easier credit, rising prices may create expectations that house prices will continue to rise, thus making larger mortgage loans appear less risky to lenders.

From the researcher's standpoint, the difficulty is to find some factor that affects credit terms without directly affecting house prices. Then if changes in this factor are associated with changes in house prices, the channel arguably flows through its effect on the availability of credit. The authors' approach was to examine the effects of changes in conforming loan limits during the period of rapidly rising home prices.[4] The authors argue that while the conforming limit was relaxed to reflect rising average home prices in the nation, there is substantial variation in both the level and rate of growth in house prices across local markets. Thus, changes in the conforming loan limit were unlikely to be driven by conditions in any one local market. In formal terms, Adelino and coauthors argue that the loan limits are plausibly exogenous with respect to local housing markets.

Adelino explained that the underlying assumption of their research design is that borrowing is significantly less costly for loan-to-value ratios below 80 percent. That is, a house whose price is just above 125 percent of the conforming loan limit is significantly more costly to finance than an essentially identical house that is just below 125 percent of the conforming loan limit.

The authors used data from home sales for 10 MSAs over an 11-year period (1998-2008), which includes the housing boom years. In addition to the date of sale, the address of the property, and the sale price, the data set included a number of characteristics that affect the quality — and, potentially, the price — of the house, e.g., the number of rooms, the number of bathrooms, and the age of the house, among other characteristics.

Adelino described the logic of the authors' research design as follows. Imagine a home that was sold for slightly *less than* 125 percent of the conforming loan limit in 1999. Now, imagine that a very similar home in the same neighborhood sold in 2000 and that the conforming loan limit had risen during the year. Although other factors may explain the difference between the prices of these two very similar homes, the change in the conforming limit would not, since it was not binding for either home in either year.

Now, imagine another pair of very similar homes in the same neighborhood. The first was sold for slightly *more than* 125 percent of the conforming loan limit in 1999. The second was sold in 2000, again for more than 125 percent of the 1999 conforming loan limit, but for less than the actual conforming loan limit in 2000. Thus, the only material difference between those two homes is that the second one could be purchased with lower-cost financing due to the increase in the conforming loan limit.

Using a difference-in-difference approach, the authors compared the difference between the sale prices of the first two homes described above to the difference in the sale prices of the second two homes. They hypothesized that since more potential borrowers would qualify to buy the more expensive home in 2000 than in 1999, demand for such homes would increase. Thus, the sale price of those homes would tend to rise; in particular, it should rise more than the sale price of homes that were initially (and that remained) less expensive than the conforming limit.

Indeed, this is what they found in their main regression: If two houses were sold in subsequent years in the same zip code, the value per square foot was $1.10 lower for the house with a price above the cutoff in the earlier year. Furthermore, the effect was stronger in the earlier part of the period (1998-2001). According to the authors, this finding was consistent with their hypothesis, because the conforming loan limit became less important as households' access to second liens and to jumbo loans in the latter part of the sample period lowered financing costs.

In addition to their main regressions, in which the researchers controlled only for house size and neighborhood, they also ran regressions taking into account other factors that might affect the house's price. In particular they estimated hedonic regressions, in which the house price (or,

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[4] Only loan sizes above the conforming loan limit can receive guarantees from the GSEs. In addition to the value of the guarantee against default, the market for mortgage-backed securities composed of conforming loans is much deeper than for nonconforming loans.

alternatively, house value per square foot) was broken down into two parts: one part that can be explained by a host of observable characteristics, e.g., the number of rooms and bathrooms, among other factors, and another part that can't be explained by these characteristics, the *residual*. Using this residual as an alternative measure of home value, they found that value per square foot was $0.65 lower if the house price was above the cutoff. In light of this finding, Adelino argued that unmeasured quality differences among houses were not likely to be the explanation for their main results.

The authors also found that the effect of being above the cutoff was stronger in those zip codes in which household income growth was negative. They argue that in such localities, households are more likely to be credit constrained, strengthening the argument that it is changes in the cost of credit that drive their results.

SHOULD CREDIT REMARKS BE FORGOTTEN?

Marieke Bos, of the Swedish Institute for Social Research, discussed the results of her study (with Leonard Nakamura) of the effect of legal mandates to drop credit remarks from individuals' credit files after a specified period of time. The study's main conclusion was that creditworthiness and access to credit increased when credit remarks were removed and that, for most consumers, the effects were long-lasting. Bos emphasized that her results were preliminary. She explained that while 90 percent of the 113 countries with credit bureaus do expunge credit remarks after some period of time, the amount of time varies significantly. In Sweden, credit remarks are removed after three years.[5]

[5] In the U.S., reported delinquencies are expunged after seven years and bankruptcy filings after 10 years.

To help motivate her empirical work, Bos cited Ronel Elul and Piero Gottardi's (2011) model of the optimal policy for "forgetting" a default. In that model, expunging credit remarks increases the likelihood that an individual will make risky decisions prior to defaulting, but once an individual has actually defaulted, forgetting improves his or her subsequent incentive to make prudent decisions. The optimal time to forgetting balances these two forces.

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Prior empirical research by David Musto on the effects of removing a bankruptcy flag from credit files in the U.S. yields pessimistic results. In Musto's sample, individuals' access to credit improves when the bankruptcy flag is removed but most of those consumers subsequently experience declines in creditworthiness. In contrast to Musto's focus on removing bankruptcy flags, Bos and Nakamura focus on removing credit remarks, which, Bos argued, could easily arise from an oversight, a legal dispute, or more generally, from temporary factors outside the individual's control.

Bos and Nakamura's data set includes the credit files for individuals in Sweden for a six-year period, from February 2000 to October 2005. First, the authors examine the outcomes for individuals who had a remark removed (the *removal group*) compared with all individuals without a credit remark during the sample period.

Focusing first on the short-term effects of removing the credit remark, the authors found that individuals' credit scores improved significantly and that both applications for credit and access to credit increased. The improvements in credit scores were most striking for those individuals with credit scores in the middle range before the derogatory credit remark was removed. Loan applications increased just prior to removal of the remark for many borrowers — which, Bos suggested, might reflect individuals' uncertainty about the precise timing of removal — and remained high.

The authors then turned to longer-run outcomes. Bos noted that in contrast to Musto's findings, the initial improvement in credit scores for most consumers was not reversed in the longer term. She and Nakamura also found that removal led to a long-term increase in both applications for credit and access to credit. Furthermore, while the likelihood of delinquency was substantially higher for this group than for other individuals, the delinquency rate was not very high.

The results described so far are based on a comparison of outcome variables for consumers before and after a derogatory credit remark is removed. Bos noted that such a comparison is not a natural experiment that might isolate the effects of removing the derogatory credit remark from other time-varying factors that might affect individuals' outcomes. Nor can a true natural experiment be constructed. Instead, the authors compared the outcomes of the removal group with the outcomes for a control group of individuals, similar to the removal group. Specifically, the authors used the propensity score matching technique to

identify individuals who were similar to the removal group at the time the remark was removed.

Bos and Nakamura compared the change in various measures of creditworthiness and credit availability at different time horizons for the removal group and the control group. Relative to the pattern for the control group, they found that among members of the removal group, credit scores improved immediately after the removal of the derogatory remark and that the boost in creditworthiness lasted up to two years. Loan applications increased immediately prior to removal, and there was a differential effect of up to three and a half years. They also found that various measures of access to credit increased with removal. Following an initial decline, which the authors argue reflects a lag between applications for credit and the receipt of funds, the number of loans increased, as did credit limits and outstanding balances, for up to 30 months. The average increase in outstanding credits was SEK 21,000 (about $3,100), a large increase.

The authors then considered default behavior over time. They found that the removal group had a significantly higher probability of delinquency than other individuals; up to 24 percent of the removal group was delinquent after 36 months, compared with 9 percent among the individuals with no remark and 11 percent among the matched sample. Nonetheless, the likelihood of subsequent delinquency was significantly lower than that found in Musto's sample.

HOUSING PRICES AND DEFAULT

Leonardo Martinez, of the International Monetary Fund, explained the results of a macroeconomic modeling exercise (with Juan Carolos Hatchondo and Juan Sanchez) that focused on the implications of housing price risk for household behavior. Among other things, they used the model to examine the effects of minimum down payment restrictions and laws that permit lenders to garnish the income of defaulting homeowners.

Martinez explained that in their model, households have limited opportunities to hedge against declines in their labor income or to sudden declines in housing prices. Although other researchers have examined similar models with and without explicit housing decisions, the main innovations of this paper were to include a realistic long-term mortgage contract and to allow the major contract terms, e.g., interest rates and down payments, to arise endogenously through supply and demand in a competitive market.

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In their model, households can decide to either buy or rent — by assumption, renting is intrinsically less attractive than buying for all households — and households take out long-term fixed-rate mortgages to finance their home purchases. Mortgages can be refinanced and households may default. In the model, households make all decisions knowing that their future wage income or house prices might rise or fall in any period. Households know how income and house prices move together on average, but they can't predict precisely what will happen in any particular period. Even though the authors assume that households can't purchase explicit insurance against declines in labor income or house prices, they can protect themselves through prudent savings decisions, their decisions about how big a house to buy, how much money to put down, etc. That is, households can *self-insure*.

The authors used calibration techniques to fix the model's parameters. They chose a number of the model's parameters, for example, households' aversion to risk and the correlation between house prices and personal income, from the existing literature. Then authors chose values for the remaining model parameters with the goal of matching three targeted factors that can be measured from published data: the average house-price to income ratio, the median net-worth to income ratio, and the homeownership rate from the 2004 Survey of Consumer Finances.

The authors then simulated the fully calibrated model to see how closely it could match certain empirical features of housing markets. Martinez reported that the model was relatively successful in matching the distribution of down payments across the population of homeowners, as well as the homeownership rates for households of different age groups. The model's ability to match these factors with some accuracy provides a rationale for viewing the model as a useful representation of the real world.

Next, the authors examined how successfully households could self-insure in a world where income might fall without warning precisely when house prices are also falling, a potential disaster in a world in which households prefer to avoid risk. Despite

households' lack of explicit insurance opportunities in the model, the authors found that households were able to self-insure just as well in a model economy with housing risk as they were in an otherwise identical model economy without housing.

Martinez and his coauthors then used their model economy to analyze the effects of two policy experiments. In the first, they examined the effect of imposing a 20 percent down payment requirement for all mortgages. They found that this policy had only a modest effect on homeownership rates and led to a reduction in default rates and interest rates. While higher down payments reduced the well-being of renters and younger households, who were forced to wait longer to purchase a home, the authors argue that most households would gain from such a policy.

The second policy allowed lenders to garnish defaulting households' income above some predetermined floor. They modeled garnishment in a stylized way: Households can make binding pledges of future income to service debts without imposing large collection costs on lenders. This policy increased homeownership rates, reduced default rates, and lowered mortgage rates. Martinez suggested that this policy would be welfare-enhancing for nearly all households. Unlike a policy of minimum down payments, this policy increased the availability of mortgage credit for younger households who might not have sizable enough savings to make a down payment.

THE ADOPTION AND USE OF PAYMENTS INSTRUMENTS

Scott Schuh, of the Federal Reserve Bank of Boston, presented results from a study (with Sergei Koulayev, Marc Rysman, and Joanna Stavins) of the adoption and usage patterns of payments instruments — e.g., cash, check, and debit, among others — by U.S. consumers. Schuh emphasized that the results were preliminary. He explained that payments systems are changing rapidly and that we know relatively little about what an optimal payments system might look like. Nonetheless, policymakers are making regulatory decisions that have an impact on the payment choices of consumers.

The authors estimated a structural model of consumer decision-making that explicitly separates adoption decisions ("Do I open a credit card account?") and usage decisions ("Do I use credit or debit to buy this TV?"). This permitted the authors to analyze how households might respond to market-driven or regulatory changes that affect the cost or usefulness of various payment instruments. In particular, Schuh explained that they can use this model to examine some of the potential effects of regulatory ceilings on debit card interchange fees paid by merchants mandated under the Durbin Amendment of the Dodd-Frank Act of 2010.[6] Schuh noted that some banks had increased debit card fees or reduced rewards for consumers in response to the regulatory change and that this model could be used to see how customers might respond and to measure how the change might affect their well-being.

The researchers estimated the model using a data set, called the Survey of Consumer Payment Choice, jointly constructed by the Federal Reserve Bank of Boston and the Rand Corporation. To construct this data set, the Boston Fed and Rand asked 1,500 households to fill out a detailed survey that asked which payment instruments the consumers used and for what types of purchases. Respondents also answered questions about their attitudes toward the various instruments, for example, the ease of adoption, the speed with which transactions could be completed, and the relative security of using an instrument. The data set also includes demographic information about the household, e.g., income, marital status, and education, among other factors. This is a continuing survey; the authors estimated the model using information from the 2008 survey. For this study, the authors limited their attention to households with a checking account, yielding a sample of 997 households.

In their modeling approach, Schuh and coauthors viewed households as making a two-stage decision. In the *adoption* phase, they choose to adopt a bundle of payments instruments, i.e., a checking account plus any or all of the following: cash, debit card, credit card, stored-value card, online bill payment, direct bank deduction, and income deduction. Households make the initial adoption decision knowing the various types of purchases they are going to make in the future and, thus, their future choice of payment instruments, the

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[6] Section 1075 of Pub. L. 111-203.

usage stage. The authors estimated two separate equations jointly: One equation represented the household's usage among the payment instruments from the bundle initially chosen, and a second represented the household's adoption decision, that is, the initial choice among bundles.

Schuh explained that their modeling approach was flexible, in the sense that it permitted a wide range of interactions among usage patterns by different households. A possibly significant limitation of their approach was the assumption that the adoption of one instrument does not affect the cost of adopting another instrument. While this may be an unrealistic assumption — made for technical reasons — the researchers' approach does permit the adoption of one instrument to affect the consumer's cost or value of *using* another instrument. So, in their model, adopting a credit card doesn't make it cheaper to also adopt a debit card, but it could make it easier to use the debit card.

Schuh then highlighted some of the insights from the model. Focusing first on the usage equation, Schuh and coauthors found that consumers' income was strongly positively related to usage of all payment instruments except for stored-value (prepaid) cards. Consumer ratings were also important determinants of usage, with ease of use and cost of use being particularly important, while security was a relatively unimportant concern for households. Schuh argued that this was an unexpected result, evidence of the value of the researchers' structural modeling approach.

Turning to the adoption equation, the authors found that credit cards were the least costly to adopt, followed by debit cards. The authors also found that adoption costs were negatively related to income for all instruments, but the negative relationship between income and credit card adoption costs was particularly strong. Schuh suggested that this may reflect the role of underwriting in the supply of unsecured credit.

Schuh then discussed the effects of the Durbin Amendment, which placed a ceiling on debt card interchange fees paid by merchants.[7] First, the authors estimated the usage benefits and adoption costs for debit cards. Schuh showed that usage benefits were roughly the same for consumers with different incomes, while adoption costs were significantly lower for higher-income consumers. Schuh and coauthors concluded that policies that increase debit adoption costs are likely to have a disproportionate effect on low-income households, at least those with checking accounts.

Next, the authors used their model to simulate how consumers might respond to an increase in adoption costs or an increase in usage costs that reduced the market share of debit cards by 1 percent. The authors considered both short- and long-term effects of such changes. In the short run, in which consumers cannot immediately adjust their bundles of payment instruments, they shift a significant portion of their transactions to cash, with a somewhat smaller shift to checks and credit cards. The results for the long run, in which consumers can choose a different bundle, are similar. The authors also found that low-income customers with checking accounts would suffer larger declines in well-being compared with the declines experienced by consumers with higher incomes. That is because households with higher incomes tend to use more payment instruments, thereby incurring lower costs of adjusting to the new environment.

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LIQUIDITY CONSTRAINTS AND BANKRUPTCY

Tal Gross, of Columbia University, reported the results of an empirical study (with Matthew Notowidigdo and Jialan Wang) of the effects of tax rebates on bankruptcy filings. Their main finding was that tax rebates increased Chapter 7 filings, evidence that many households without ready cash were unable to file bankruptcy unless they could pay the required court costs and lawyers' fees.

Gross noted first that a number of other empirical studies had found that liquidity constraints have significant effects on consumption decisions. He and his coauthors explored whether liquidity constraints might also limit households' access to social insurance programs — programs designed to protect households against catastrophic declines in consumption levels — when these programs require a household to pay a fee. Bankruptcy is a particular type of social insurance program designed to reduce a household's debt payments when they become too large relative to income, but court fees are $300 and Chapter 7 lawyers'

[7] Note that in all simulations in this paper, it is assumed that merchants would continue to accept the forms of payment they accepted prior to the policy experiment.

fees fall between $500 and $1500. Gross suggested that these fees might represent a significant barrier to using bankruptcy for households in financial distress and without cash on hand.

The authors' approach was to use a natural experiment to examine the effects of the tax rebates of 2001 and 2008 on bankruptcy filings. They found that bankruptcy filings increased after households received the rebates for both episodes. But this increase occurred only for Chapter 7 filings and not for filings under Chapter 13.

The authors' approach exploited a feature of the tax rebates that make them an ideal natural experiment; the timing of the rebates was based solely on the last two digits of the recipient's Social Security number. The key is that the last two digits of a recipient's Social Security number are essentially random; it is a characteristic that is unrelated to any other factor that might plausibly affect the recipient's economic behavior, such as income, marital status, age, etc. The authors used court records to identify the Social Security number of households that entered bankruptcy in 2001 and 2008 from 72 of the 90 bankruptcy courts in the U.S., a sample that included 74 percent of the bankruptcy filings and 95 percent of the U.S. population.

The authors then used a difference-in-difference framework to determine whether tax rebates affected the number of households filing for bankruptcy. Specifically, in any two-week period, the authors added up the number of filings for those individuals whose Social Security numbers indicated that they might have received tax rebates in that two-week period and compared this with the number of households that could not have received tax rebates during that period. The authors found that for the 2001 rebate, the number of Chapter 7 bankruptcy filings was nearly 4 percent higher for Social Security number groups that had received rebates. For the 2008 rebate, the comparable figure was even higher, nearly 5 percent. Gross noted that this was interesting because the 2005 bankruptcy act had been explicitly designed to make it more difficult for households with above-average incomes to qualify for Chapter 7.

The authors' results suggest that fees are ordeal mechanisms; that is, they pose a hurdle that makes it harder for liquidity-constrained households to file for bankruptcy.

In contrast, the authors found only a small negative effect on Chapter 13 filings in 2001 and no effect in 2008. Gross argued that this finding was consistent with the view that relaxed liquidity constraints were the true cause of the rise in bankruptcy filings, because only Chapter 7 filers are required to pay the filing fee immediately. Chapter 13 filers are permitted to pay fees over time as part of their repayment program.

The authors conducted a simple falsification test to ensure that their results could not have arisen by chance or because of some factor other than the tax rebates. They conducted identical experiments for each of the other years between 1998 and 2008 and found that there was no evidence of a similar timing effect in those years when tax rebates were not sent out. Furthermore, Gross noted that their empirical estimates of the effects of the rebates were probably conservative, because not all individuals with the same last two digits of their Social Security numbers actually received rebates.

Gross concluded by drawing out the policy implications of his research. He noted that one could not automatically conclude that policymakers should seek to make bankruptcy filings easier. The authors' results suggest that fees are ordeal mechanisms; that is, they pose a hurdle that makes it harder for liquidity-constrained households to file for bankruptcy. In principle, this might be justified if it improves households' financial incentives to act prudently and to make decisions that lower the probability of bankruptcy. Nonetheless, if policymakers do not want to penalize liquidity-constrained households by limiting access to the bankruptcy courts, the researchers' results suggest that simplified procedures that require lower out-of-pocket costs for filers might be desirable.

THE SUPPLY OF CREDIT TO BANKRUPT HOUSEHOLDS

Song Han, of the Federal Reserve Board, reported on the results of a study (with Benjamin Keys and Geng Li) of the supply of credit to bankrupt individuals. Using a data set that monitors credit card mailings to a sample of households to measure the supply of credit, their main results were that bankrupt individuals *(filers)* continued to receive offers of credit; the terms of the credit card offers were less favorable for filers than those offered to individuals who had not gone bankrupt *(nonfilers)*; and recent filers were more likely to receive an offer of credit than filers who were about to have the bankruptcy flag in their credit files removed.

Han explained that it is typically difficult to empirically disentangle the effects of changes in the supply of credit from changes in the demand for credit simply by observing credit terms. Theoretically, the supply of credit to a

filer might decrease if the bankruptcy flag reveals higher credit risk. But it could also increase because bankruptcy eliminates existing debt and places legal limits on future filings. Han argued that it is essential to understand how the supply of credit is affected by bankruptcy to understand the bankruptcy decision.

To conduct their study, the authors used a data set that includes a more direct measure of the supply of credit. A sample of 3,000 households from July 2009 to August 2010 sent the data provider all credit card mailings they had received within the previous month. The information about the number and the terms of the offers was then linked to data from individuals' credit bureau files, which include the date on which some individuals filed for bankruptcy, as well as a range of other information about the individual's finances.[8] The bankruptcy flag in the data set file did not distinguish whether the individual entered Chapter 7 proceedings — in which all debts are written off — or Chapter 13 proceedings — in which the individual agrees to a repayment plan.

Han first presented anecdotal evidence that bankrupt individuals received credit card offers targeted specifically to households that had just exited bankruptcy proceedings. He then presented summary statistics indicating that the percentage of filers who had opened an account was nearly the same as for nonfilers, while, on average, offered interest rates were substantially higher, credit limits substantially lower, and accounts substantially more likely to bear annual fees for filers.[9] He said that these offers were typically of the "credit building" variety; that is, the offer had annual fees but without the rewards typical of "premium rewards" offers.

The researchers then examined how the supply of credit evolved over time after a bankruptcy filing. One factor that might affect the supply of credit is the restriction that filers can't file for bankruptcy for eight years (while bankruptcy markers are dropped from credit files after 10 years). Unsecured lenders might view recent filers as a relatively lower risk, everything else equal, given the restrictions on filing again. Consistent with this view, Han and coauthors found that, over time, the probability of getting a card offer declined following bankruptcy. But among filers who did receive offers, interest rates and some other credit terms in those offers improved modestly as the time elapsed since the bankruptcy increased.

Han and coauthors then carried out a formal regression analysis, estimating the effects of filing on the probability of receiving an offer and on the credit terms received by filers. These regressions also took into account the individual's credit score, demographic information, and information about the individual's balance sheet. Broadly consistent with the summary statistics reported above, the authors found that filers were only 7 percentage points less likely than nonfilers to receive an offer in any given month and individuals who had filed in the previous two years were as likely to receive an offer as a nonfiler.

Conditional on receiving an offer, the probability that the offer required an annual fee was 13 percentage points higher — a large difference, since only 26 percent of nonfilers' offers contained an annual fee. In addition, filers were offered interest rates that were 77 basis points higher than rates offered to comparable nonfilers. Filers were offered a minimum credit limit that was $470 (29 percent) lower than that offered to comparable nonfilers.

Finally, the authors examined the possibility that card issuers included less generous terms in the fine print of the mailing — where they were presumably less likely to be noticed — a practice known as *shrouding* in the economic literature. Indeed, they found that offers to filers were more likely to include higher fees or interest rates on balance transfers and higher minimum payments. Additional fees and other more onerous contract features were more commonly included in the fine print of the offers made to filers than to nonfilers.

[8] Note that the data used by the authors were anonymized. No personally identifiable information is contained in the data set.

[9] In their analyses of differences in interest rates offered to filers and nonfilers, the authors focus on the "go to" rate, that is, the interest rate charged on revolving balances after any promotional interest rates have expired. This is a conservative approach, since nonfilers are much more likely than filers to receive generous promotional rate offers.

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