

The Impact of Liquidity on Household Balance Sheets:  
Micro Responses to a Credit Card Supply Shock

Jonathan Zinman<sup>\*</sup>  
Federal Reserve Bank of New York  
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ABSTRACT

Despite growing evidence that many US households are liquidity constrained, there remains little consensus on the quantitative importance or nature of these constraints. This paper develops a new type of evidence on the impacts of consumer credit markets on behavior by examining household-level responses to an exogenous liquidity shock. A United States Supreme Court decision effectively deregulated bank credit card interest rates in December 1978, and I find that consumers from states with binding usury ceilings before the decision became more likely to hold bank cards after the decision, relative to their counterparts in unaffected states. The marginal cardholders appear to have characteristics widely associated with credit constraints, and to borrow frequently on their new cards. Yet there is little evidence that these cardholders exploit their newfound liquidity by shifting into higher-yielding, less liquid, or riskier assets. This finding is at odds with standard models of liquidity constraints, and motivates consideration of alternative explanations for the widely observed sensitivity of consumers to liquidity.

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<sup>\*</sup> Jonathan.Zinman@ny.frb.org; tel: 212-720-1204. I am particularly grateful to Daron Acemoglu and Jon Gruber for their guidance. The author also thanks Daniel Bennett and Kurt Johnson for excellent research assistance, and Adam Ashcraft, Ricardo Caballero, Kathleen Johnson, Jim Poterba, Phil Strahan, and participants in the MIT macro and public finance lunches and the 2003 AEA Meetings for helpful comments. The views expressed do not necessarily represent those of the Federal Reserve Bank of New York or of the Federal Reserve System.

## I. Introduction

Consumer credit markets have grown dramatically over the last 30 years. Figure 1 shows that revolving consumer credit outstanding in the United States has increased sixty-fold in real terms since 1968. The development of bank credit cards has been a primary driver of this growth. The proportion of households using these cards has risen from 0.07 in 1968 to 0.68 in 1998 (Kennickell, et. al. 2001) with year 2001 aggregate outstanding credit card balances totaling about \$600 billion (Board of Governors of the Federal Reserve System, 2002). This equates to over \$5500 per household and 6% of GDP.

Various literatures are concerned with the implications of this growth in consumer credit, but fundamental puzzles remain. Growth could be caused by secular changes in demand for liquidity, and/or by technological innovations that relax liquidity constraints. Disentangling the roles of supply and demand in consumer credit markets has been a difficult task empirically, as Gross and Souleles (2002) note. Not surprisingly, then, there is little consensus on the quantitative importance of liquidity constraints and precautionary motives (Browning and Lusardi 1996).

Of course the lack of obvious exogenous variation in access to liquidity has also frustrated attempts to identify the effects of the growth of consumer credit. Meanwhile, interest in estimating these effects has grown along with recent theoretical work showing that consumer credit constraints have important implications. On the macro side, they can amplify business cycles (Hubbard and Judd 1986) and retard growth (Jappelli and Pagano 1999). More surprisingly, various works have shown that the welfare implications of expanded consumer credit markets are ambiguous, given various types of incomplete markets or nonstandard

preferences.<sup>1</sup> Interactions between easy credit and incomplete contracts can lead to social welfare losses, as Athreya (2001) demonstrates for the case of bankruptcy law. Relaxing interest rate ceilings to increase credit supply might reduce welfare if usury laws mitigate insurance market failures (Glaeser and Scheinkman 1998), or combat lender market power (Blitz and Long, 1965). Expanding consumer credit markets may provide “too much liquidity” if consumers have self-control problems (Laibson 1997), leading consumers to underestimate their credit card borrowing (Ausubel 1991) and/or to undersave (Laibson, Repetto, and Tobacman 1998). Any of these phenomena could produce optimization failures and result in welfare losses.

This paper develops new evidence on the causes and effects of liquidity growth by using the deregulation of interest rate ceilings to help identify increases in bank card use. These increases are arguably exogenous to credit demand and other unobservable determinants of household behavior. I identify states that had a binding usury ceiling (“affected” states) prior to the 1978 *Marquette* Supreme Court case that deregulated bank card interest rates, and show that following deregulation the proportion of households using bank cards increased in those states, relative to “unaffected” states that did not have binding usury ceilings prior to the case. Consideration of various potential confounds suggests that this result is not driven by unobservable differences between affected and unaffected states (or across households therein). Changes in usury law then can be used to identify the effects of shifts in access to credit on other margins of consumer behavior, including portfolio and occupational choice. This paper is thus the first study to exploit a plausibly exogenous shock to directly estimate effects of access to consumer credit on these types of outcomes; i.e., on outcomes in addition to borrowing.<sup>2</sup>

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<sup>1</sup> With complete markets, consumer credit will expand if demand increases and/or lender costs decrease. In either case the expansion will be efficient.

<sup>2</sup> There is a small literature that does look specifically at the impact of credit card usury ceilings on card use (Dunkelberg, et. al. 1981, and Goldberg 1975). But these studies utilize small samples, lack critical control

Specifically, having found that households in affected states become more likely to use a bank card, I then estimate whether households in these states become more likely to hold illiquid and/or risky assets. Both buffer stock and precautionary savings models predict that credit constraints will force consumers to be more liquid and conservative than optimal in their asset holdings (e.g., Guiso, Jappelli, and Terlizzese 1996). The joint test of whether consumers increase card use *and* shift into illiquid, riskier (and presumably higher-yielding) assets following usury deregulation offers a more complete test of the existence and impacts of liquidity constraints on consumer behavior than previous studies.

The rest of the paper proceeds as follows. The next section describes the framework used here for testing for the existence and impact of liquidity constraints. Section III describes the data on usury laws and the 1977 and 1983 Surveys of Consumer Finances (SCFs), the sources of household-level data on credit card use and other financial decisions used in this paper. Section IV details the econometric methodology, discussing threats to identification and presenting some preliminary evidence on the validity of the exclusion restriction. Section V presents results on the response of bank credit supply to interest rate deregulation, showing that bank card interest rates and consumer bank cardholding did appear to rise in states affected by *Marquette*. Section VI estimates a basic reduced-form model of the response of bank card borrowing to the liquidity shock provided by *Marquette*. The results suggest that the marginal cardholders borrowed frequently, and that they responded differently than inframarginal cardholders. The results for interest rates, card possession and borrowing all appear to be robust to various controls for household characteristics and demographic shifts. Falsification tests developed in Section VII

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variables (such as state fixed effects— see Section IV), and do not examine any impacts of credit card use itself on consumer behavior. Gross and Souleles (2002) use proprietary account-level data from credit card companies and arguably exogenous features of firm credit-granting rules to identify marginal propensities to borrow out of liquidity

further buttress the conclusion that these findings are driven by an exogenous shock to credit supply rather than unobserved shifts in demand. Section VIII tests for heterogeneity in bank card use following deregulation in an attempt to parse out countervailing structural effects obscured by the basic reduced-form estimation. Importantly, the marginal cardholder appeared to be young, poor, and minimally educated— all characteristics commonly associated with facing liquidity constraints. Section IX tests whether households, and the marginal cardholders in particular, appeared to adjust their portfolios “appropriately” in the face of increased access to liquidity. It finds little evidence that they did in fact increase illiquid or risky asset holdings, or decrease stocks of liquid assets. Section X concludes that the findings in this paper affirm the growing consensus that a great number of U.S. households have nontrivial (and very possibly substantial) marginal propensities to consume (MPCs) out of liquidity, but cast fresh doubt on the common conclusion that liquidity constraints drive these MPCs. This motivates several natural offshoots of this paper, which are sketched briefly.

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and interest rate elasticities of borrowing. The nature of their data does not permit observation of other important margins of household behavior, however, nor does the data include households without a credit card.

## II. Framework

### *A. Credit Constraints and Consumer Behavior*

This paper addresses the questions of whether liquidity constraints exist, and whether they have empirically important impacts. It proceeds in two steps, first using the deregulation of usury laws to identify plausibly exogenous variation in bank credit card use, and then using this variation to estimate the impact of bankcard use on household portfolio choice. The first step serves more than the aforementioned instrumental purpose, as it will also shed light on the pervasiveness of liquidity constraints and the impact of interest rate regulation (see also Canner and Fergus 1987).<sup>3</sup> The second step is designed to develop evidence on the welfare effects of expanded consumer credit. Under most theories of liquidity constraints, a key source of welfare loss is that consumers are forced to be overly liquid and conservative in their asset holdings in order to smooth wealth shocks. I therefore test whether, when liquidity constraints are relaxed, consumers shift out of liquid and/or safe assets and into illiquid and/or risky assets.

### *B. Credit Cards and Consumer Behavior*

Bank credit cards are a natural focal point for studying the growth and impacts of consumer credit. Empirically, bankcards have grown to dominate the other consumer credit products that preceded them: store-specific credit cards, lines of credit, and installment loans; and “traditional” consumer loans from banks and finance companies (Evans and Schmalensee, 1999). Conceptually, the very features that make bank credit cards dominant— e.g., their widespread acceptance, relatively high credit lines, and the ability to obtain cash advances— suggest that the

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<sup>3</sup> The results also bear on consumer interest rate elasticities of borrowing, and the shape of the credit supply curve (please see Section V).

growth of this type of consumer credit is more likely than any other to have reduced any pre-existing liquidity constraints, whether for good or for ill.

The “for good” scenario is relatively obvious-- under traditional (time-consistent) preferences, relaxing liquidity constraints will be efficient, as supply-side innovations in consumer credit add to the space of Arrow-Debreu markets. The “for bad” scenario is more controversial, but gaining currency as models which incorporate self-control problems using quasi-hyperbolic preferences are formalized. Under such a model, relaxing credit constraints may create “too much liquidity” (Laibson 1997) by permitting time-inconsistent consumers to indulge their current (time  $t$ ) selves by splurging, at the expense of later consumption (and their time  $t+n$  selves). This can create welfare losses relative to a benchmark where the  $t=0$  self is able to commit his future selves to implement his optimal consumption plan.<sup>4</sup>

Importantly, consumers with self-control problems most likely face greater difficulties with bankcards— which can be used virtually anywhere to make purchases or obtain cash advances from ATMs<sup>5</sup>-- than with a store card. In the latter case, the sophisticated consumer need only avoid a particular establishment to control his consumption, whereas controlling bankcard spending might require more costly commitment devices.<sup>6</sup>

### *C. Usury Law and Bank Card Use*

More instrumentally (pun intended), idiosyncratic variation in the regulation of bank card interest rates can be used to help identify arguably exogenous shocks to the supply of credit.

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<sup>4</sup> Welfare losses need not result if consumers are (partly) sophisticated about their self-control problems (see, e.g., O’Donoghue and Rabin 2001; DellaVigna and Malmendier 2001) and possess commitment devices that effectively constrain future selves (e.g., Laibson, et. al. 1998). I broach possible impacts of “commitment constraints” in the concluding section.

<sup>5</sup> An extreme example is the prevalence of ATMs in casinos.

Specifically, the confluence of state usury laws and a United States Supreme Court ruling, in *Marquette National Bank v. First of Omaha Service Corporation*, 439 US 299 (1978), created a quasi-experiment where banks suddenly could charge discretely higher interest rates to consumers in several states beginning in December 1978. These were states that maintained binding interest rate (usury) ceilings on bank credit cards as of that date.<sup>7</sup> (I define “binding” as less than 18%, since this has historically been both the modal rate charged, and the modal ceiling where ceilings existed.<sup>8</sup>) The *Marquette* decision gave banks the authority to “export” the bankcard interest rates permitted by their home state to customers in other states. Banks located in a state with a high or no ceiling could then charge high rates to consumers residing in other states. This opened the door to mass interstate marketing, and within two years leading bankcard issuers such as Citibank and MBNA had relocated to high interest states South Dakota and Delaware, respectively (Athreya 2001). *Marquette* thus quickly functionally deregulated bankcard interest rates by enabling out-of-state banks to circumvent the remaining strict state-level usury ceilings (and by putting banks chartered in those states at a competitive disadvantage, prompting actual state-level deregulation in most cases).<sup>9</sup>

Accordingly, it seems plausible that *Marquette* increased the supply of bankcards to households residing in those states that had binding usury ceilings at the time of the decision.

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<sup>6</sup> Ausubel (1991) and others have popularized the anecdote where consumers entomb their cards in ice and store them in the freezer to prevent impulsive purchases. Bertaut and Haliassos (2002) consider how credit limits might be used by consumers as a commitment device against overspending on bank cards.

<sup>7</sup> Penalties for violating these usury ceilings were typically severe, and violators faced potentially massive exposure to civil judgements (Illig, 1978).

<sup>8</sup> 18% was the modal rate charged on (bank) credit card balances in states that had ceilings of 18% or higher in both years considered in this study (1977 and 1983), and it remains the modal rate in the most recently published (1998) Survey of Consumer Finances. (Ausubel 1991 examines the stickiness of bankcard interest rates.) 36 of 38 states represented in the 1977 Survey of Consumer Finances placed some restriction on bankcard interest rates at the time of the survey, and 22 of these states had 18% as their ceiling.

<sup>9</sup> This view of *Marquette*'s impact is widely held by both legal scholars and economists. See also, e.g., Ausubel (1991), or Evans and Schmalensee (1999).



The specific exclusion restriction that must hold for this shock to identify increases in bankcard use that are exogenous to other behaviors of interest is discussed in Section IV.

### III. Data

#### *A. Survey of Consumer Finances*

I draw microdata on household credit card use, assets, and demographics from the 1977 and 1983 Surveys of Consumer Finances (SCF), primarily.<sup>10</sup> The SCF provides the best available nationally representative data on credit card use (and on household balance sheets in general), but has increasingly well-documented limitations. The samples are small (2,563 households in 1977, and 3,665 in 1983 if one excludes the high-income oversample). Credit card use is underreported-- Blanchflower, Evans, and Oswald estimate that 1983 SCF respondents understated their bankcard balances by a factor of 2, and their number of credit card accounts by a factor of 1.5. Nevertheless the SCF provides some important advantages over the issuer-based data used in Gross and Souleles (2002) and Ausubel (1999). Most obviously, the SCF is publicly available (although geographic identifiers are not, after 1983), contains more comprehensive data on household characteristics, and permits direct examination of the impact of credit card use on margins of consumer behavior other than credit card borrowing. More subtly, perhaps, SCFs contain data on households without credit cards, avoiding the concerns about selection due to entry and attrition that are inherent to the use of Gross and Souleles' account-level data.

I also use data from the 1968 and 1970 Surveys of Consumer Finances to examine pre-treatment trends (see Section IV) and conduct falsification tests (see Section VII). These surveys

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<sup>10</sup> The 1977 survey was originally entitled the "Consumer Credit Survey", and was sponsored by various bank regulating agencies (including the Federal Reserve Board). It was designed to provide some continuity with the earlier, annual SCFs from 1947-1970 that had been sponsored by the Federal Reserve Board. The 1983 survey was more comprehensive, and updated most of the variables collected in 1977.

are comparable in size (they contain 2,677 and 2,576 observations, respectively) and content to the 1977 and 1983 surveys, although both earlier surveys lack data on credit card interest rates and the 1968 survey contains relatively few details on credit card use.

### *B. Bankcard Usury Laws*

I determined whether each state that appears in the SCF had a binding bankcard usury law as of the 1977 survey date (July 1977) by referring to the appropriate superceded state statutes. I then confirmed that my reading of the statutes was correct (e.g., that there were no legal loopholes or enforcement practices that might effectively raise *de jure* low ceilings, or lower *de jure* high ceilings), by consulting secondary sources (including Gushee, various years; American Bankers' Association, various years; and dozens of law review articles). Oregon's bankcard usury law was both unique in construction and in a state of flux in 1976-77, so I drop the 47 Oregon households in the 1977 SCF from my estimation sample.

## IV. Econometric Methodology

### *A. Reduced-form model*

The basic reduced-form model is estimated using Ordinary Least Squares (OLS) or probit as follows:

$$(1) Y_{ist} = a + \beta X_{st} + \chi W_{ist} + \delta_s + \phi_t + \varepsilon_{ist}$$

$Y$  is a measure of bankcard use or asset holding from the SCF by household  $i$ , living in state  $s$ , at time  $t$  (where  $t$  is either 1977 or 1983).  $X$  is an indicator variable taking the value of 1 if banks had clear authority to charge 18% or higher on bankcard balances to residents of state  $s$  at

time  $t$ .  $X_{st}$  therefore takes the value of zero only for 1977 households in states with binding usury ceilings in 1977, since the *Marquette* decision of 1978 effectively deregulated bankcard interest rates by 1983 (see Section II).  $W$  is a vector of control variables, and includes household and state-level characteristics.  $\delta_s$  and  $\phi_t$  condition on state- and year-specific means, respectively, of the dependent variable. Standard errors are adjusted for the fact that the variation of interest occurs at the state-year level by allowing for clustering within state-year cells.

I also will use (1) to verify that usury deregulation did in fact permit higher interest rates, by setting  $Y_{ist}$  equal to the (bank) credit card interest rate.

The coefficient  $\beta$  will capture the causal effect of  $X_{st}$ , the usury law (or “deregulation”) variable, on  $Y$  if there are no unobserved, differential *trends* in  $Y$  across households in the two groups of states  $X_{st}$  classifies—those that had binding usury ceilings in 1977 (and therefore were plausibly affected by *Marquette*) and those that did not (and therefore were not plausibly affected by *Marquette*).<sup>11</sup> Note the emphasis on unobservable *trends*; any persistent differences across states (e.g., some are debt-loving, others are debt-hating) are captured by the state fixed effects  $\delta_s$ . In other words, (1) will capture the within-state variation in  $Y$  due to  $X$ , for households in states that had binding usury ceilings in 1977 relative to households in states that did not, if the identifying assumption holds. The raw data suggests that it does. Figure 2 reveals little evidence of differential trends in bank card use across affected and unaffected states *before* 1977, and suggests breaks from trend in affected states (relative to unaffected states) only *after* 1977 (presumably due to *Marquette*).<sup>12</sup> Figure 3 indicates that variables that should *not* have been

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<sup>11</sup> No states made material changes to their usury ceilings between the 1977 SCF survey date and the 1978 *Marquette* decision.

<sup>12</sup> The aggregate slowdown in bank card growth evident after 1977 has been attributed to a combination of the early 1980s recession and industry growing pains (Mandell 1991).

affected by *Marquette* (these are used in the falsification tests of Section VII) do *not* in fact appear to break from trend after 1977. Table 1 shows few observable differences in demographic characteristics or economic conditions between affected and unaffected states in 1977, but a stark difference in bank card interest rates (which presumably were depressed in affected states by binding usury ceilings).

The other particularly notable feature of Table 1 is the evident lingering effects of the 1981-82 recession. Credit card growth, which had surged throughout the 1970s, slowed dramatically in the late 1970s and early 1980s (see also Figure 1), and unemployment remained high in 1983. These macroeconomic effects arguably stack the deck against finding effects of usury deregulation in a statistical sense, since there is probably less variation in card use than there would have been counterfactually. But the recession should not otherwise contaminate the results, since the year effects capture time series conditions common to the entire sample, and household- and state-specific control variables capture local conditions.

Of course, the deeper question to consider regarding the identification issue is why several states maintained binding usury ceilings as late as the *Marquette* case while others did not. The political economy of usury regulation is poorly understood (and the drivers of usury *deregulation* even less so), but the fact that the deregulation considered in this paper occurred as the result of a federal intervention, and a court case at that, mitigates concerns that subsequent behavior in affected states might be driven by unobserved changes in consumer demand rather than bank supply. Nevertheless I condition on various household and state characteristics that could be correlated with both usury law status as of 1977 and changes in demand for credit and various assets. This strategy is detailed in Section V. Section VII then presents several falsification tests designed to detect any spurious correlation between deregulation and demand shocks.

## B. Structural Models

Although (1) captures the structural relationship of interest quite well in the case of interest rates and card possession, many of the other outcomes ( $Y_{ist}$ 's) considered in this paper have additional structural parameters of interest. For example, the structural equation of interest for bankcard borrowing is:

$$(2) B_{ist} = a + \beta_1 H_{ist} + \beta_2 r_{ist} + \chi W_{ist} + \delta_s + \phi_t + \epsilon_{ist}$$

Where  $H$  and  $r$  are the endogenous regressors of interest, with  $H$  measuring whether household  $i$  has a bankcard, and  $r$  measuring the interest rate  $i$  faces if it borrows on its bankcard.<sup>13</sup>

(2) reveals that estimating (1), the reduced-form, for  $B_{ist}$  masks important heterogeneity, since (1) pools two very different types of cardholders—the marginal ones, for whom new access to card represents a decrease in the cost of borrowing, and the inframarginal ones, who likely experience no change (if they live in an unaffected state) or an *increase* (if they live in an affected state) in the cost of borrowing. Put differently, to estimate a true interest rate elasticity of bankcard borrowing, one needs to control for selection into bankcard holding, and therefore one needs to instrument for both  $H$  and  $r$ .

This presents a problem. The results in Section V suggest that  $X_{st}$  (the deregulation variable) can serve as one instrument, but another is required. Ongoing work seeks to develop well-

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<sup>13</sup> Note that the interest rate should not have an independent effect on demand for card possession (as opposed to borrowing) under standard preferences, since consumers can choose whether to finance balances, and there are several other reasons to hold cards (including: the option to borrow, the free float on balances paid in full after one billing cycle, payment services). If consumers have self-control problems, however, all bets are off. Sophisticated consumers with self-control problems might well exhibit an interest rate elasticity of cardholding, since they may forgo cardholding in order to commit not to borrow at high rates.

identified structural models of bankcard borrowing and portfolio choice using additional instruments.

For now, I rely on an alternative approach to put a bit more structure on the reduced-form results; namely, adding interactions of household characteristics with the deregulation variable to (1). With reference to existing evidence on which types of consumers are likely to be liquidity constrained (e.g., Jappelli 1990), these results will test for heterogeneity in responses across different types of consumers and help identify the marginal card user (see Section VIII).

## V. Impacts of Rate Deregulation on Card Interest Rates & Possession

The next two sections discuss estimates of equation (1) for various outcomes related to credit card use. The results suggest that deregulation increased both bankcard holding and borrowing. Robustness and falsification tests in Section VII generally support the interpretation that deregulation caused these changes by increasing the supply of bankcards.

### *A. Interest Rates*

Table 2 presents estimates of the effect of deregulation on credit card interest rates. The variables of interest are constructed from SCF questions asking respondents for the interest rate they pay on bank or store card balances that are not paid in full (i.e., that are carried beyond the free float period). The SCF does not ask about bankcard rates in particular, so I construct a crude approximation by limiting the sample to those who report an interest rate, but have only a bankcard (not a store card). Each row presents results for a different dependent variable, and each cell contains results on the deregulation variable from a different regression. As such each column presents results for a different specification, as follows:

- column 1 regressions include only the deregulation variable, state and year effects
- column 2 adds variables capturing the race and age of the household head
- column 3 adds household structure, the household head's education and employment (including self-employment status), characteristics of the head's spouse, household income, housing tenure, and a home mortgage indicator
- column 4 adds the log of aggregate state income, the Gini coefficient on state income, and the state employment rate.
- column 5 adds interactions of the year dummy and individual covariates, to capture any time-varying influence of a household's characteristics on its financial decisions.

The general approach here is to ensure that any observed relationship between deregulation and the outcomes of interest is not driven somehow by demographic shifts in affected states, either due to coincidence, or to a political economy story whereby increasing demand in affected states set in motion the legal process that culminated in the *Marquette* decision.<sup>14</sup> The state aggregate variables are motivated in part by Glaeser and Scheinkman's (1998) findings that the likelihood of a usury ceiling may be increasing with equality and decreasing in income growth, and in part by the importance of network externalities in bankcard supply (Evans and Schmalensee, 1999). These factors suggest that card issuer (mass) marketing strategy might depend on state-level characteristics not entirely captured by the micro data.

The results in Table 2 suggest that deregulation did in fact increase bankcard interest rates. The approximated annual bankcard rate rises by about 130 basis points in affected states relative

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<sup>14</sup> *Marquette*-related proceedings were in fact initiated by a bank in a state with a binding usury ceiling (Minnesota)— in 1976. If a potentially confounding secular increase in demand were driving this proceeding, we might then expect to see a break from trend in card use before the 1977 SCF, in affected states relative to unaffected states. This does not appear to be the case (Figure 2).

to unaffected states (this would imply a 9% increase over the 1977 mean of 14.4% in affected states). Taking the log of the dependent variable yields similar magnitudes, with implied increases of about 9%. Both the level and log results are consistent across specifications, and all are statistically significant by a comfortable margin (with t-statistics of 3 or greater). The combined bank and store card rate increases less, not surprisingly, but still substantially, by about 90 basis points (implying a 6.4% increase over the 1977 base mean of 14.1% in affected states). The logged results are insignificant, with slightly larger standard errors than their logged bankcard rate counterparts, and much smaller point estimates clustered around 0.04. In all, it seems plausible, at least, that *Marquette* did in fact increase the interest rate banks charged on credit card balances held by households in affected states. This finding provides support for Ausubel's (1991) "upward-quick" model of credit card interest rates.

### *B. Card possession*

Table 3 presents analogous results for bankcard possession from linear probability estimates of (1) (probits produced nearly identical results). Possession is arguably the bankcard behavior of greatest interest, since it is measured with less error than bankcard borrowing (Blanchflower, Evans, and Oswald), and provides a better summary of the benefits available to cardholders than actual borrowing. This is because a bankcard provides services that plausibly decrease liquidity constraints *regardless of whether the cardholder actually borrows*. These services include payments, free float, and the option to borrow. The results suggest that the proportion of households using a bankcard rose between 3.5 and 5.5 percentage points in affected states, relative to unaffected states, between 1977 and 1983. These are large changes— they would imply a 10 to 15 percent increase over the base period mean of 0.36 in affected states, and



account for 49 to 77 percent of the time series growth in bank card possession in affected states during this period (Table 1). The estimates are marginally significant, with t-statistics of about 2 in every case. Similar inferences are obtained for the number of bankcards held by a household (this can be thought of as a proxy for available credit). Row 2 presents results with the dependent variable topcoded at 2 to reduce the influence of outliers (this censors 2.3% of the estimation sample), and Row 3 presents results with the number of bankcards topcoded at 3 (censoring 0.7% of the estimation sample). These point estimates imply increases of 0.07 to 0.12 bankcards per household in affected states, or about a 15% increase over the base period means. This percentage increase is similar to that obtained for card possession, suggesting the much of the increase in the number of cards may actually be driven by the extensive margin.

The analogous results for all credit cards (where the count does not include gas cards, and therefore is comprised primarily of bank and store cards) are sensitive to the censoring rule, with significant increases found when topcoding at 10 cards, but not at 5. The increases of 0.21 to 0.31 of a card found in Row 6 would imply a 9 to 13 percent increase over the base period mean of 2.4 cards in affected states. There is no evidence of an effect on the extensive margin of holding any card (Row 4), nor is there a significant effect on holding a store card in particular (Table 5, Row 1).

The relationship between household characteristics and bank card possession (not reported) confirm most of the findings of Blanchflower, Evans, and Oswald's analysis of pooled 1977-1995 SCF data (which did not include state fixed effects or the deregulation variable). Conditional on the other observables, white, rich, educated, married, middle-aged, female-headed, homeownership households with fewer members appear most likely to hold bank cards.

### *C. Summary*

In all, the results presented thus far suggest that deregulation did in fact increase both the number of households holding bank cards, and the number of bank cards in circulation. Deregulation does not appear to have had a significant effect, however, on overall cardholding. These results raise the questions of whether bankcards were substitutes for store cards, and/or if the marginal cardholder produced by deregulation was in fact liquidity constrained. I explore these questions in the next three sections.

## VI. The Impact of Interest Rate Deregulation on Credit Card Borrowing

### *A. Unconditional Borrowing*

I continue by estimating the reduced-form effect of interest rate deregulation on credit card borrowing. Table 4, Row 1 presents results for whether a household reported having a bankcard balance that was incurring finance charges as of the month prior to the SCF survey date. (This binary parameterization of borrowing is motivated by concerns about outliers and measurement error.) The estimates suggest a marginally significant, 3 percentage point increase in the number of households carrying bankcard balances. Taking the point estimates literally, this is a large increase compared to the estimated 3.5 to 5.5 percentage point possession increases reported in Table 3. It suggests that *at least* 55% of the marginal cardholders were borrowing on their bankcards (the full sample mean in the 1983 SCF is 52%). Of course the true proportion is likely somewhat higher, since, as Section IV outlines, the reduced-form for unconditional borrowing captures the net effect of marginal cardholders borrowing more (since one can't borrow without a card), and inframarginal cardholders borrowing less (since they now face a higher interest rate). Row 2 estimates the reduced-form effect on credit card balances generally (this measure is

dominated by store and bank cards, and does not include gasoline cards). The results are virtually identical to those obtained for bank card balances only. This is reassuring-- to the extent there are changes in overall card borrowing, they should be driven by bank cards (i.e., if the increase in overall credit card borrowing was significantly larger than for bank card borrowing, this would arouse suspicion). Row 3 reports results for whether the household reports that it typically borrows on its bank or store card. The results are generally insignificant, with somewhat larger standard errors and smaller coefficients than the other measures of unconditional borrowing (this could well be due to a relatively big underreporting problem with this variable).

Unfortunately, the 1977 SCF lacks balance data on most other types of borrowing, so it is not possible to directly calculate the extent to which, if any, new bank card borrowing crowds out other sources of debt. However, preliminary estimates provide little evidence of crowd-out on the extensive margin of likely substitutes (revolving credit and installment debt from stores, consumer loans from banks).

### *B. Conditional Borrowing*

Rows 4 and 5 present estimates for the logarithms of bank and credit card balances, respectively, and thus condition on borrowing. Although one should not attribute a causal effect to deregulation in this context, since we know that deregulation also effects that probability that one borrows, it may be worth noting that the results here appear consistent with a substantial decrease in borrowing by inframarginal cardholders; i.e., with a nontrivial interest rate elasticity of borrowing. Alternately, marginal cardholders may borrow less on average than their predecessors.

### *C. Interpretation*

In all, the reduced-form results on credit card borrowing raise two important possibilities. One is that the marginal cardholder was quite credit constrained; the results on unconditional borrowing are consistent with very high probabilities of borrowing for marginal bank cardholders. The second hints that responses to deregulation were heterogeneous, as the results on unconditional borrowing point to potentially important differences between marginal and inframarginal cardholders. This further motivates richer models that can distinguish the mechanisms underlying responses by marginal and inframarginal cardholders. Ongoing work seeks to develop structural models of the sort described in Section IV. Another approach is to enhance the reduced-form model. This is undertaken in section VIII.

## VII. Falsification Tests

Before exploring heterogeneity in the responses of card use to interest rate deregulation, I first conduct several falsification tests in an attempt to rule out demand-driven explanations for the observed correlations between interest rate deregulation and bank card use. Table 5 presents the results of falsification tests where I simply replace the dependent variables of equation 1 with variables that should *not* be affected by bankcard interest rate deregulation. Finding a “false positive” here would raise concerns that the “shock” provided by *Marquette* is actually correlated with unobserved determinants of demand; i.e., that households in states affected by *Marquette* actually had a secular uptick in demand between 1977 and 1983 relative to unaffected states.

The probability of holding a *store* card should be uncorrelated with bank card deregulation, since *Marquette* did not apply to store cards, and since none of the states affected by *Marquette* deregulated store card rates between 1977 and 1983. The results in Table 5, row 1 provide no evidence to the contrary. This is hardly airtight evidence in support of a causal effect of the deregulation variable on bankcard use, since bank and store cards could be substitutes (or less likely, complements). Nevertheless one would be concerned if store cards increased along with the deregulation variable. Row 2 shows no significant effect on first mortgage interest rates. Rows 3 and 4 display correlations between the deregulation variable and beliefs about the benefits of consumer credit that are *negative*, if anything; i.e., the proportion of households responding that consumer credit is a “good thing”, or that it is “all right to borrow” to finance discretionary purposes, appears to *drop* following deregulation.<sup>15</sup> It is not yet clear how we should interpret these results (see the previous footnote), or whether we should take attitudinal questions seriously at all. But if nothing else these results further alleviate concern that deregulation might be correlated with unobserved increases in demand.

Table 6 presents estimates for a timing falsification model where (1) is estimated on 1970 and 1977 SCF data for the card possession and lending variables of interest (credit card interest rates are not available in 1970), with deregulation falsely coded as taking effect in 1977 for households in states that still had binding usury ceilings at that time. This test creates a selection problem, unfortunately, since many states changed their usury ceilings between 1970 and 1977,

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<sup>15</sup> These results are intriguing, particularly since the dominant reasons households offer for consumer credit or credit cards being a “bad thing” evoke self-control problems (“encourages impulse buying... too easy to buy now, pay later.... buy things don’t want or need”, “buy more than you can pay for”). The results hint that there may have been some (social) learning about the self-control problems posed by bankcard possession, and that households updated their beliefs about bankcards’ costs and benefits as cards became more prevalent. The results do *not* appear to be driven by inframarginal cardholders responding to higher interest rate, since “costs too much” is a separate, mutually exclusive response. (Other alternative explanations must be explored, of course; e.g., what if consumer groups targeted consumer credit “awareness” campaigns to affected states in the wake of *Marquette*?) Future work

and I am forced to limit the sample to those states that had stable interest rate regulation during this period. The results therefore should be interpreted with caution. Estimates are ultimately based on a sample of households from 27 states (as opposed to the 37 states represented in every other sample used in this paper), of which only 5 states are labeled as affected (as opposed to 7 elsewhere). The deregulation variable should be uncorrelated with the outcomes of interest in these tests if deregulation was in fact an exogenous shock to bank card supply. Indeed, there is virtually no suggestion of a statistically significant relationship in this table. It should be noted, however, that the standard errors are large enough in most cases to admit the possibility of correlations that would raise concerns about mean reversion.

### VIII. Heterogeneity: In Search of the Marginal Bank Card User

The reduced-form models presented thus far may obscure important heterogeneity in responses between marginal cardholders (for whom borrowing on bank cards became less expensive, or at least feasible) and inframarginal cardholders (for whom borrowing might have become more expensive) in states affected by usury deregulation. In this section I attempt to identify any important sources of heterogeneity by adding interactions between the deregulation variable ( $X$ ) and each of the household control variables (in matrix  $W$ ) to equation (1). This increases the main effect on the deregulation variable approximately twofold for the case of bankcard possession (compare to Table 3, row 1, column 4); however, the standard errors jump as well (with a p-value of 0.155 for any bank card, and .081 for the number of bank cards). The interaction terms suggest that marginal cardholders are much more likely to be young (a whopping 26 percentage point increase in deregulated states relative to the oldest households),

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on the nature of consumption responses to increased bankcard use (smooth, or splurge?) will examine these results more closely.

have only a high school education (a 9 point increase relative to heads without a high school education), poor (9 to 13 point increases for the poorest group relative to richer groups), and a mortgagee.<sup>16</sup> Each of these characteristics except for the latter have been widely associated with liquidity constraints (e.g., Jappelli 1990).

The main effect of deregulation on the proxy for bank card interest rates jumps dramatically when the interactions are added (coefficient=3.14, standard error=1.49; compare to Table 2, row 1, column 4). Working households with children, male heads, and very little education appear more likely to face higher rates after deregulation.

Adding the interactions eliminates the main effect of deregulation on binary bankcard borrowing (coefficient=0.006, standard error=0.045; compare to Table 4, row 1, column 4). Only young households (compared to the oldest households), married households, and those with some college education (compared to the least-educated) appear to borrow more, and the effects are only marginally significant. Conditional borrowing falls dramatically but is imprecisely estimated (coefficient= -0.89, standard error= 0.70), with the self-employed and those with some college appearing more likely to borrow.

In all, the large increases in bank cardholding (and, to a lesser extent, on borrowing) among classes of borrowers widely thought to be liquidity constrained would seem to bode well for identifying the effects of liquidity on portfolio choice in the next section.

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<sup>16</sup> Income categories are based on four approximately equal-sized groups in the 1977 data (this approach is necessitated by the categorical nature of the data). I then deflate the 1983 income variable (which is continuous) to 1976 dollars and use the 1977 break points to define the 1983 categories as well.

## IX. Interest Rate Deregulation and Asset Choice

The results thus far suggest that interest rate deregulation delivered bank cards to a nontrivial number of households on the margin. Many of these households borrowed actively on their cards, and many of the marginal cardholders were plausibly credit constrained *ex ante*. Standard models of buffer stocks and precautionary saving predict that liquidity constraints force agents to be sub-optimally liquid and conservative in their asset holdings. Accordingly, this section presents estimates of reduced-form models that test whether households in states affected by the deregulation of interest rate ceilings, and in particular those households that appear to be the marginal cardholders in Section VII, do in fact shift into higher-yielding illiquid and risky assets.

This exercise is hampered somewhat by data limitations in the 1977 SCF, which reports only categorical values for most asset types. Although the natural variables of interest here are ratios of asset types to total assets, it is impossible to directly construct these proportions with any precision in the 1977 SCF.<sup>17</sup> Consequently I am forced to parameterize asset holdings as binary variables, based either on the extensive margin or arbitrary cutoffs. The combination of these measurement problems and the small net increase in bank cardholding in affected states (3.5 to 5.5 percentage points) dims the prospect of finding significant effects in the basic reduced-form (equation 1).

I begin by estimating whether deregulation appears to induce households to reduce liquid asset holdings. There has been a longstanding particular interest in whether credit card use reduces liquid asset holdings (dating at least to White 1976), motivated primarily by the question of whether the payments feature of credit cards reduces the transactions demand for money (see also Duca and Whitsell 1995, and Blanchflower, Evans, and Oswald). Of course credit cards

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<sup>17</sup> The next draft will include asset share estimates based in part on 1977 holdings that are predicted from a model estimated on 1970 data, conditional on the asset interval (which is observed in both years).



might reduce liquid asset holdings through another channel as well— if they relax liquidity constraints and thereby reduce the need to hold buffer stocks (which must be kept relatively liquid for emergencies). Table 7, Rows 1-3 present the reduced-form estimates for checking account balances (row 1), savings account balances (row 2), and a binary variable for savings account ownership (row 3). The model delivers the expected signs for the savings account variables but not for checking balances, and the standard errors are too large to identify the plausibly small effects one would expect on these binary outcomes. I encounter a similar problem with illiquid assets (certificates of deposit and government savings bonds) and a risky asset (stocks). Results are presented for the extensive margin in each case, but binary variables based on the various categorical cutoffs posed by the 1977 survey fared no better.

The basic reduced-form model accordingly provides little leverage for testing the prediction that interest rate deregulation should induce portfolio shifts (via relaxation of liquidity constraints). Part of the power problem stems from the fact that, as in the case of borrowing, the reduced-form model may obscure countervailing effects on marginal and inframarginal cardholders in affected states. In contrast, the large effects on cardholding and borrowing for certain, plausibly credit-constrained groups (Section VII) suggest that the enhanced reduced-form model with interactions holds some promise for identifying effects of deregulation (and bank card use) on asset choice.<sup>18</sup>

Indeed, adding the interaction terms to the equations for asset holdings yields some significant results— and little support for the prediction that marginal, liquidity-constrained households should adjust their portfolios in favor of more illiquid, risky assets upon accessing

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<sup>18</sup> The limitations of the reduced-form again motivate structural approaches. Ongoing work suggests that certain structural parameters should appear in asset choice models that are absent from the borrowing model. For example, it would be useful to distinguish the impact of the option value of borrowing on portfolio choice from the impact of actual borrowing (or convenience use).

credit. The youngest households in affected states do appear to become much more likely to hold a government savings bond and less likely to have savings account (relative to the oldest households) after deregulation, which jibes with the canonical predictions. However they seem far less likely to hold stock or be self-employed, findings that are at odds with the canonical predictions. The poorest households appear to become more likely to be self-employed or to hold a government savings bond, certificate of deposit, or stock than their richer counterparts, but they also seem to increase their liquid asset holdings (this is a case where it would be particularly useful to have data on the ratios of each of these different types of assets to total assets). There does not appear to be significant variation in portfolio choice by education level in deregulated states, in contrast to bank card usage results. Males seem to begin holding more assets in their savings accounts, and are less likely to be self-employed (these could be inframarginal effects of males facing the higher interest rates observed in Section VII). Working households become more likely to hold both stock and liquid assets. Married households become more likely to hold a CD and substantial amounts of stock, but also more likely to hold high checking account balances after deregulation. Blacks appear to enter self-employment and hold larger savings account balances, despite no observable changes in card use.

Although measurement issues discourage drawing firm conclusions from the results in this section, it nevertheless seems fair to say that the findings here do not jibe easily with standard models of liquidity constraints.

## X. Summary and Implications

This paper develops a new type of evidence on the impacts of liquidity on consumer behavior. The results suggest that binding interest rate ceilings in several states during the late 1970s kept perhaps 4 or 5 out of every 100 households in these states from obtaining bank credit cards, and that at least 55% of these households borrowed on bank credit cards when given the opportunity. Moreover these marginal households were probably even *less* likely to consume out of liquidity than the 57% of households that remained without bank credit cards in the 1983 SCF (Jappelli, Pischke, and Souleles 1998). The results here are thus consistent with a clear majority of U.S. households having a nonzero marginal propensity to consume (MPC) out of liquidity. The fact that it is possible to identify borrowing increases at all in data reported by consumers (who are notorious for understating their credit card borrowing) suggests that these MPCs could be substantial, as in Gross and Souleles (2002).

But an MPC to consume out of liquidity is not a sufficient condition for establishing the existence or importance of liquidity constraints. A stricter test for the presence of liquidity constraints is whether consumers respond to increased liquidity by shifting into illiquid, riskier assets. The results in this paper offer no evidence that they do, despite the fact that the marginal cardholder appears to be young, poor, and relatively poorly educated— all characteristics commonly associated with facing binding liquidity constraints. In other words, a substantial number of households that appeared to be credit constrained suddenly obtained access to liquidity as the result of interest rate deregulation, and they did not adjust their portfolios as standard models of liquidity constraints would predict. Data limitations prevent drawing firm conclusions, but the results are suggestive and motivate consideration of alternative explanations.

One type of alternative (or complementary) model postulates that consumers face what might be termed “commitment constraints”. The possibility that some consumers are prone to splurge, with potentially adverse consequences, in the absence of effective commitment devices is gaining currency.<sup>19</sup> Recent simulations, for example, have shown that preferences which allow for self-control problems explain household balance sheets much better than standard preferences (Angeletos, et. al. 2001; Laibson, Repetto, and Tobacman forthcoming).

Far more work can be done to study the impact of liquidity, and or any lack thereof, in consumer credit markets. One offshoot of this paper is estimating the impact of *Marquette*-induced liquidity on the level, composition, and time-path of consumption— do households smooth or splurge when they obtain a credit card?— and will proceed to estimate any long-run impacts on savings rates and savings adequacy. This new work should light on the nature and magnitude of excess sensitivity, and on the structure of consumer preferences and high-frequency intertemporal choice.

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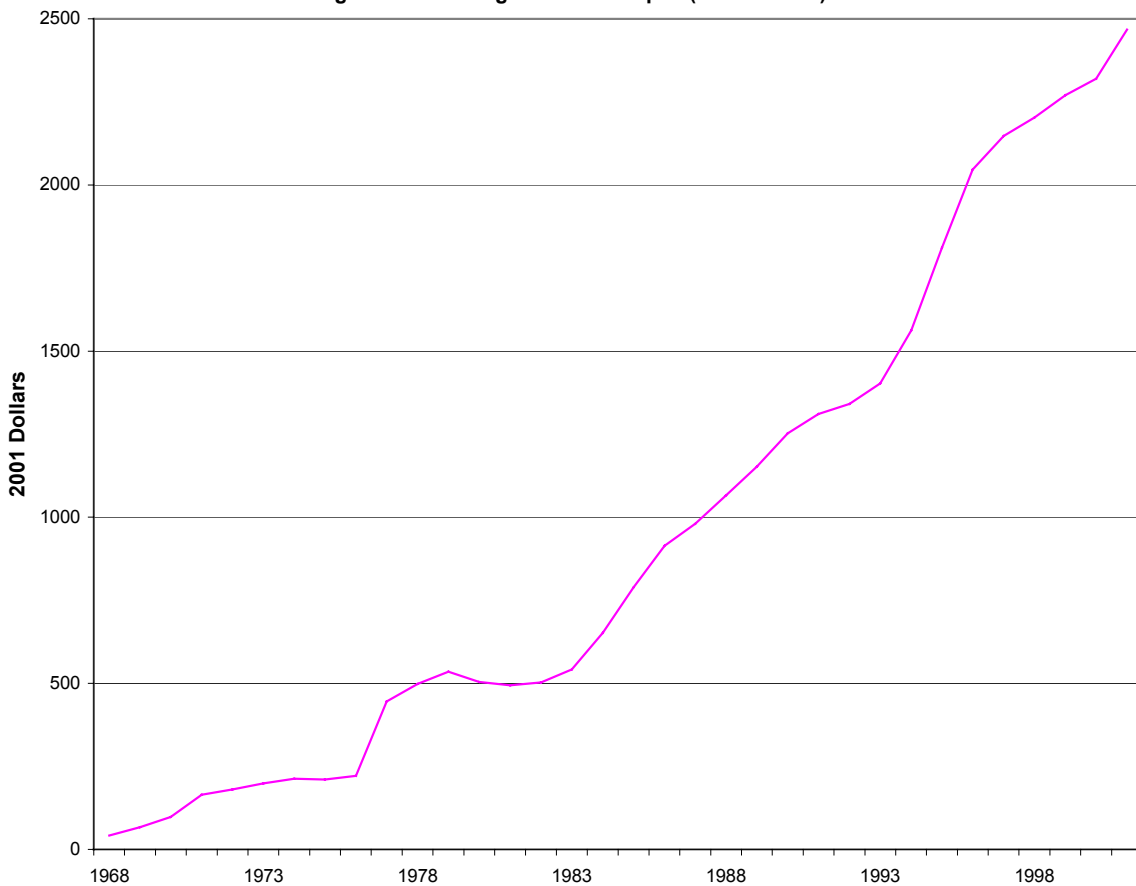
<sup>19</sup> Zinman (2003) explores whether the explosive growth of debit card use over the last decade is due (in part) to debit use serving as a commitment device against “overborrowing” on credit cards.

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Figure 1. Revolving Credit Per Capita (2001 Dollars)



Source: Series G-19, Board of Governors of the Federal Reserve System

Figure 2. Trends in Bank Card Use, Affected vs. Unaffected States

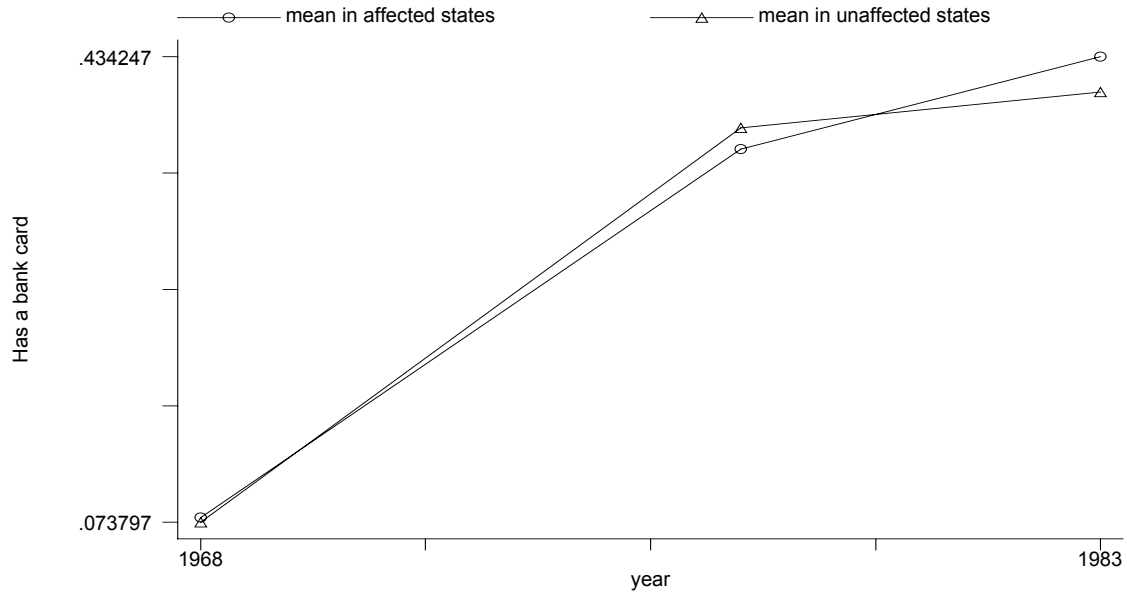


Figure 2 compares pre-deregulation trends and post-*Marquette* breaks in bank card use across affected vs. unaffected states. All data points are group-year means from the given year's Survey of Consumer Finances, where group membership is based on whether the household lived in a state that had a binding usury ceiling on bank cards prior to deregulation ("affected" states), or in an "unaffected" state that did not. The top figure presents the proportion of households holding a bank card, the middle figure the mean number of bank cards held by households, and the bottom figure the proportion of households that reported carrying a balance (paying interest) on a bank card in the month prior to the survey date.

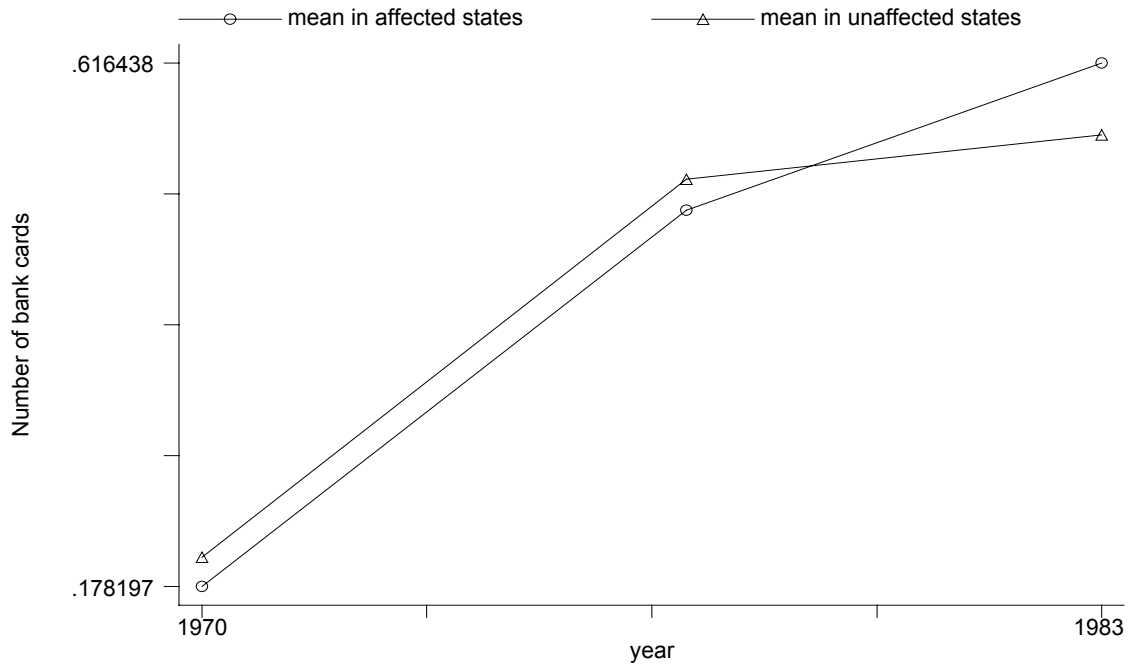




Figure 2, continued

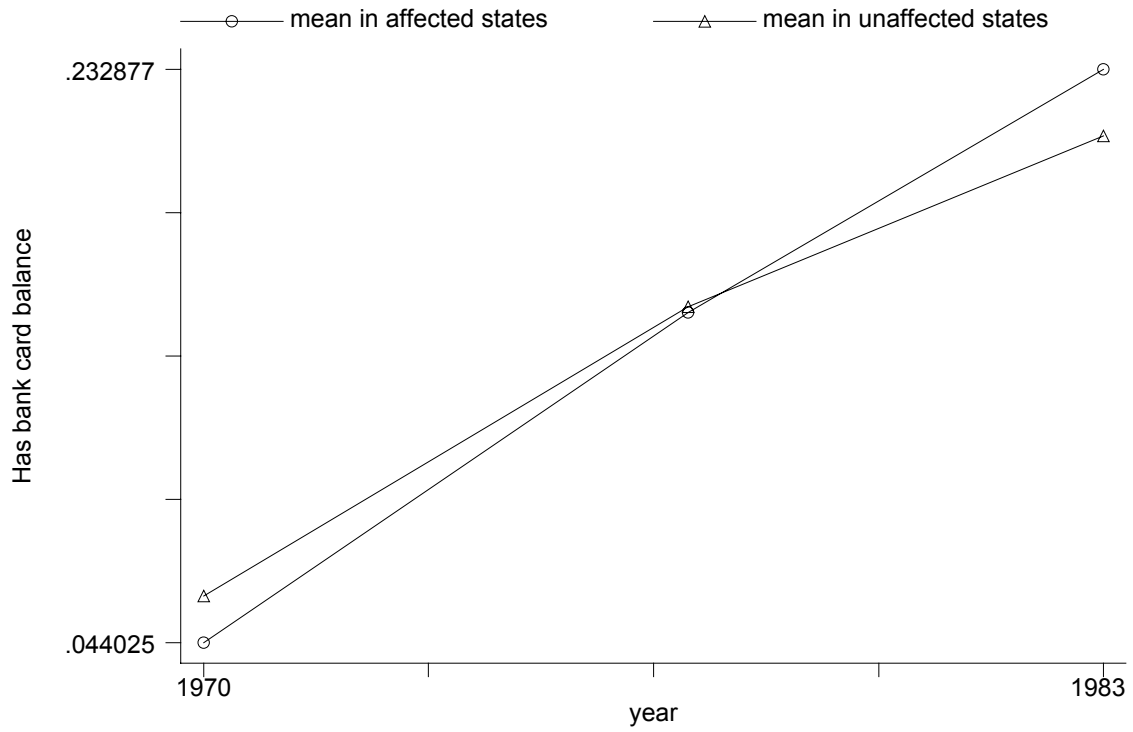


Figure 3. Trends in Falsification Variables, Affected vs. Unaffected States

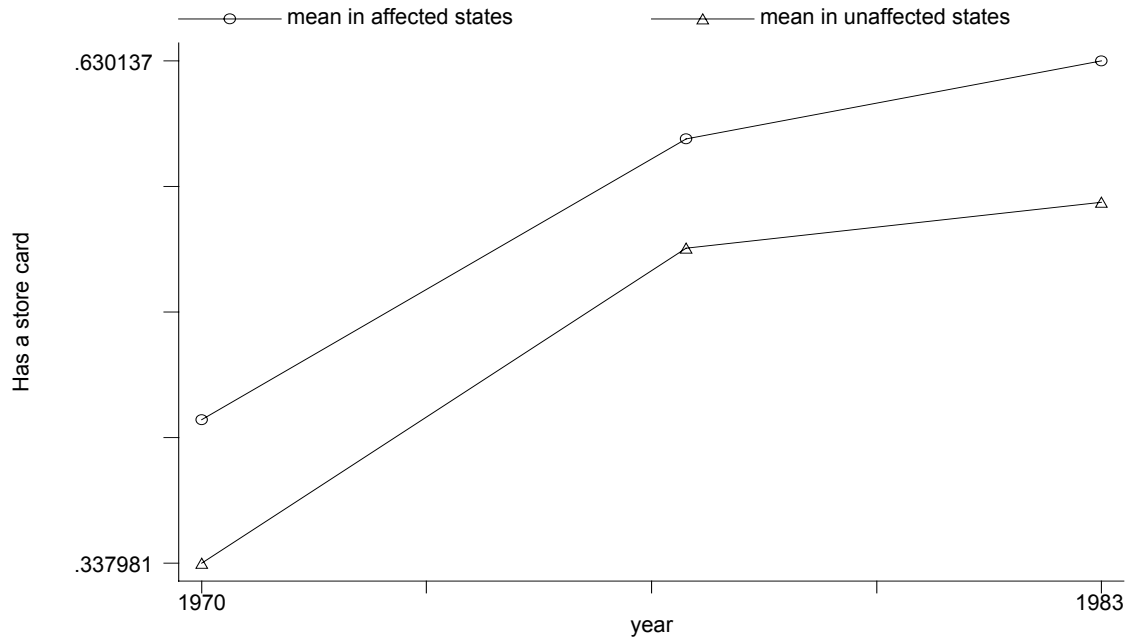


Figure 3 compares pre-deregulation trends and post-*Marquette* breaks (or lack thereof) in two falsification variables across affected vs. unaffected states. We expect the relative trends in these variables to be unperturbed by *Marquette*. As in Figure 2, all data points are group-year means from the given year's Survey of Consumer Finances, where group membership is based on whether the household lived in a state that had a binding usury ceiling on bank cards prior to deregulation ("affected" states), or in an "unaffected" state that did not. The top figure presents the proportion of households holding a store card, and the bottom figure the mean interest rate reported by households on first home mortgages.

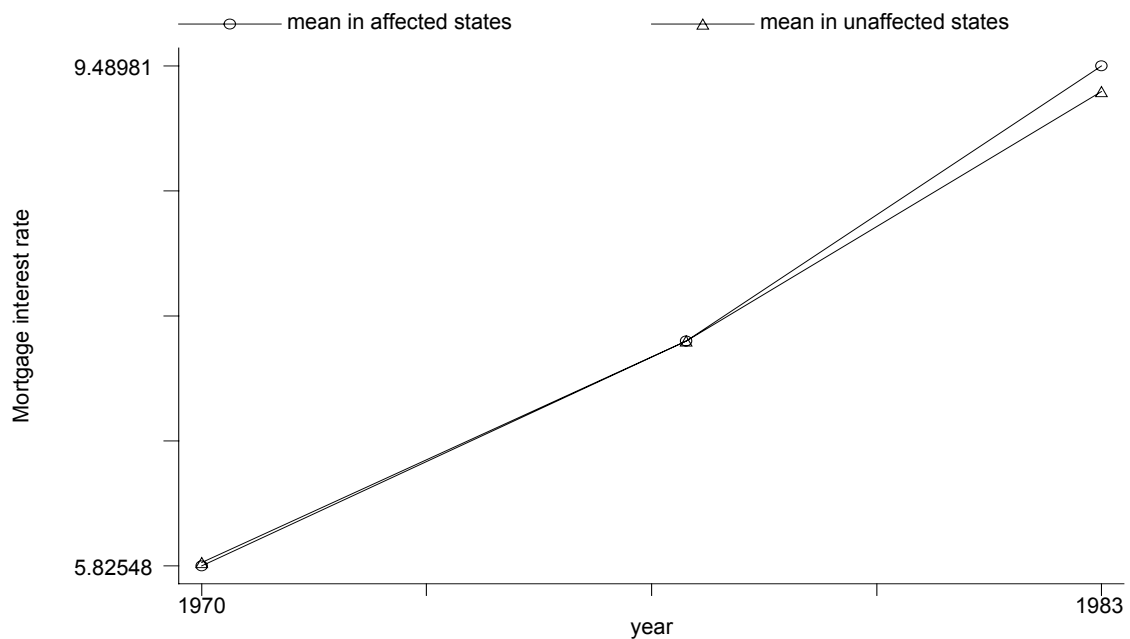


Table 1. Summary Statistics, Selected Variables from the 1977 &amp; 1983 SCFs

Variable	1977	1983	1977 affected	1977 unaffected	1983 affected	1983 unaffected
Any bank card	.376 (.485)	.412 (.492)	.363 (.481)	.380 (.486)	.434 (.496)	.407 (.491)
Number of bank cards	.515 (.726)	.568 (.746)	.493 (.715)	.521 (.729)	.616 (.776)	.556 (.738)
Paying interest on bank card	.155 (.361)	.215 (.411)	.153 (.360)	.155 (.362)	.233 (.422)	.211 (.408)
Interest rate on bank card	16.4 (3.4)	17.9 (3.0)	14.4 (3.8)	17.0 (3.1)	17.0 (3.2)	18.1 (2.9)
Interest rate on credit card	16.2 (3.6)	17.6 (3.5)	14.1 (3.7)	16.8 (3.4)	16.2 (4.1)	17.9 (3.3)
Any credit card	.597 (.491)	.627 (.484)	.643 (.479)	.585 (.493)	.682 (.466)	.613 (.487)
Any savings account	.784 (.411)	.611 (.487)	.826 (.380)	.773 (.419)	.633 (.482)	.606 (.489)
Any CD	.138 (.346)	.197 (.398)	.145 (.353)	.137 (.343)	.226 (.419)	.190 (.393)
Any stock	.255 (.436)	.286 (.452)	.279 (.449)	.248 (.432)	.301 (.459)	.282 (.450)
Male head	.775 (.418)	.738 (.440)	.762 (.426)	.779 (.415)	.742 (.438)	.737 (.440)
High school educated or less	.323 (.470)	.291 (.454)	.298 (.458)	.338 (.473)	.273 (.446)	.295 (.456)
Unemployed head	.029 (.168)	.075 (.263)	.038 (.191)	.027 (.161)	.060 (.238)	.079 (.269)
Wife works	.327 (.469)	.321 (.467)	.355 (.479)	.320 (.467)	.335 (.472)	.317 (.465)
Age of head	46.9 (17.3)	46.6 (17.3)	47.0 (17.5)	46.9 (17.2)	46.8 (17.1)	46.5 (17.4)
Less than \$7500 income (\$1976)	.223 (.416)	.325 (.469)	.208 (.407)	.227 (.419)	.316 (.465)	.327 (.469)
Gini coefficient on income, state	.399 (.019)	.409 (.017)	.401 (.022)	.399 (.018)	.403 (.015)	.410 (.017)
Employment rate, state	.463 (.026)	.487 (.036)	.454 (.023)	.465 (.026)	.480 (.034)	.489 (.036)
N	2417	3665	504	1913	730	2935

## Notes to Table 1.

Cells present (sub-)sample means, with standard deviations in parentheses. Selected cells include the number of nonmissing observations; 1983 variables have few if any missing observations due to imputations by Federal Reserve Board staff. Bank card interest rates are observed only for those households with a bank card but not store card. “Affected” households are those living in states that had binding usury ceilings on bank credit card interest rates as of the 1977 SCF. “Unaffected” households are those living in states that did not have binding usury ceilings as of the 1977 SCF. The last row gives the number of observations in the full (sub-)sample (I exclude the high income oversample from the 1983 data, and observations from Oregon or with unknown state of residence from the 1977 data).

Table 2. The Effect of Interest Rate Deregulation on Credit Card Interest Rates

	1	2	3	4	5
Bank card interest rate	1.39 (0.37) 1936	1.34 (0.39) 1936	1.36 (0.40) 1933	1.25 (0.35) 1933	1.18 (0.36) 1933
Log of bank card interest rate	0.099 (0.027) 1936	0.093 (0.029) 1936	0.094 (0.03) 1933	0.084 (0.028) 1933	0.077 (0.028) 1933
Bank or store card interest rate	0.969 (0.451) 2755	0.884 (0.454) 2755	0.944 (0.452) 2750	0.893 (0.5) 2750	0.845 (0.485) 2750
Log of bank or store card interest rate	0.042 (0.037) 2755	0.034 (0.037) 2755	0.041 (0.037) 2750	0.038 (0.043) 2750	0.034 (0.042) 2750
State and year effects	Y	Y	Y	Y	Y
Race and age of household head	N	Y	Y	Y	Y
Complete set of household covariates	N	N	Y	Y	Y
State-level covariates	N	N	N	Y	Y
Interactions of year dummy and household covariates	N	N	N	N	Y

Each row presents results for a different dependent variable, and each cell contains results on the deregulation variable from a different specification of equation (1). As such each column presents results for a different specification, as follows: column 1 regressions include only the deregulation variable, state and year effects; column 2 adds variables capturing the race and age of the household head; column 3 adds household structure, the household head's education and employment (including self-employment status), characteristics of the head's spouse, household income, housing tenure, and a home mortgage indicator; column 4 adds the log of aggregate state income, the Gini coefficient on state income, and the state employment rate; column 5 adds interactions between the year indicator and each of the household covariates.

Table 3. The Effect of Interest Rate Deregulation on Credit Card Possession

	1	2	3	4	5
Has bank card	0.055 (0.025) 6059	0.047 (0.025) 6059	0.035 (0.02) 6037	0.039 (0.018) 6037	0.038 (0.02) 6037
Number of bank cards (topcoded at 2)	0.103 (0.043) 6059	0.092 (0.043) 6059	0.071 (0.039) 6037	0.070 (0.031) 6037	0.068 (0.031) 6037
Number of bank cards (topcoded at 3)	0.118 (0.047) 6059	0.107 (0.046) 6059	0.083 (0.040) 6037	0.080 (0.033) 6037	0.077 (0.033) 6037
Has credit card	0.011 (0.021) 6063	0.002 (0.022) 6063	-0.002 (0.015) 6041	0.004 (0.013) 6041	0.004 (0.013) 6041
Number of credit cards (topcoded at 5)	0.133 (0.085) 6033	0.099 (0.089) 6033	0.044 (0.062) 6012	0.054 (0.062) 6012	0.066 (0.060) 6012
Number of credit cards (topcoded at 10)	0.350 (0.133) 6033	0.306 (0.139) 6033	0.209 (0.104) 6012	0.232 (0.088) 6012	0.252 (0.084) 6012
State and year effects	Y	Y	Y	Y	Y
Race and age of household head	N	Y	Y	Y	Y
Complete set of household covariates	N	N	Y	Y	Y
State-level covariates	N	N	N	Y	Y
Interactions of year dummy and household covariates	N	N	N	N	Y

Each row presents results for a different dependent variable, and each cell contains results on the deregulation variable from a different specification of equation (1). As such each column presents results for a different specification, as follows: column 1 regressions include only the deregulation variable, state and year effects; column 2 adds variables capturing the race and age of the household head; column 3 adds household structure, the household head's education and employment (including self-employment status), characteristics of the head's spouse, household income, housing tenure, and a home mortgage indicator; column 4 adds the log of aggregate state income, the Gini coefficient on state income, and the state employment rate; column 5 adds interactions between the year indicator and each of the household covariates.

Table 4. Reduced-form Effects of Interest Rate Deregulation on Credit Card Borrowing

	1	2	3	4	5
Has bank card balance	0.028 (0.016) 6082	0.029 (0.016) 6082	0.028 (0.015) 6057	0.030 (0.013) 6057	0.029 (0.012) 6057
Has credit card balance	0.026 (0.021) 6082	0.027 (0.020) 6082	0.033 (0.019) 6057	0.031 (0.015) 6057	0.026 (0.016) 6057
Reports typically carrying a balance	0.012 (0.022) 6020	0.012 (0.021) 6020	0.022 (0.020) 5997	0.029 (0.015) 5997	0.028 (0.016) 5997
Log of bank card balance	-0.185 (0.084) 1162	-0.250 (0.080) 1162	-0.249 (0.072) 1161	-0.327 (0.088) 1161	-0.351 (0.107) 1161
Log of credit card balance	-0.054 (0.116) 1885	-0.065 (0.112) 1885	-0.079 (0.090) 1882	-0.077 (0.089) 1882	-0.058 (0.096) 1882
State and year effects	Y	Y	Y	Y	Y
Race and age of household head	N	Y	Y	Y	Y
Complete set of household covariates	N	N	Y	Y	Y
State-level covariates	N	N	N	Y	Y
Interactions of year dummy and household covariates	N	N	N	N	Y

As in Tables 2 and 3, each row presents results for a different dependent variable, and each cell contains results on the deregulation variable from a different specification of equation (1). As such each column presents results for a different specification, as follows: column 1 regressions include only the deregulation variable, state and year effects; column 2 adds variables capturing the race and age of the household head; column 3 adds household structure, the household head's education and employment (including self-employment status), characteristics of the head's spouse, household income, housing tenure, and a home mortgage indicator; column 4 adds the log of aggregate state income, the Gini coefficient on state income, and the state employment rate; column 5 adds interactions between the year indicator and each of the household covariates.

Table 5. Falsification: Testing for “False Positives”

	1	2	3	4	Mean of Dependent Variable
Has store card	0.015 (0.022) 6062	0.007 (0.023) 6062	0.003 (0.016) 6040	0.012 (0.011) 6040	.585 (.493) 501
First mortgage interest rate	0.234 (0.173) 2190	0.183 (0.169) 2190	0.144 (0.154) 2187	0.142 (0.144) 2187	7.47 (1.77)
Log of mortgage interest rate	0.026 (0.022) 2190	0.019 (0.023) 2190	0.013 (0.021) 2187	0.016 (0.019) 2187	
Consumer credit is a “good thing”	-0.015 (0.036) 6018	-0.017 (0.022) 6018	-0.022 (0.039) 5995	-0.045 (0.026) 5995	.407 (.491)
“All right to borrow” to finance discretionary items	-0.041 (0.025) 6008	-0.041 (0.026) 6008	-0.046 (0.026) 5984	-0.067 (0.02) 5984	.627 (.484)
State and year effects?	Y	Y	Y	Y	
Race and age of household head?	N	Y	Y	Y	
Complete set of household covariates	N	N	Y	Y	
State-level covariates	N	Y	Y	Y	

As in previous tables, each row presents results for a different dependent variable, and each cell contains results on the deregulation variable from a different specification of equation (1). As such each column presents results for a different specification, as follows: column 1 regressions include only the deregulation variable, state and year effects; column 2 adds variables capturing the race and age of the household head; column 3 adds household structure, the household head’s education and employment (including self-employment status), characteristics of the head’s spouse, household income, housing tenure, and a home mortgage indicator; column 4 adds the log of aggregate state income, the Gini coefficient on state income, and the state employment rate. The key difference in this table, of course, is that we expect the deregulation variable to be uncorrelated with the dependent variables tested here. Dependent variable means are calculated for observation in affected states only, in 1977, to provide a sense of the “base period” value.



Table 6. Falsification: Timing Tests

	1	2	3	4	5
Has bank card	-.039 (.026)	-.024 (.028)	-.005 (.028)	.020 (.021)	-.014 (.019)
Number of bank cards (topcoded at 2)	-.045 (.042)	-.023 (.046)	.010 (.046)	.052 (.036)	-.003 (.032)
Number of bank cards (topcoded at 3)	-.051 (.043)	-.029 (.046)	.005 (.046)	.049 (.037)	-.009 (.032)
Has credit card	-.023 (.033)	-.001 (.035)	.019 (.028)	.019 (.030)	.009 (.028)
Number of credit cards (topcoded at 5)	-.107 (.112)	-.030 (.115)	.099 (.099)	.103 (.104)	-.002 (.105)
Number of credit cards (topcoded at 10)	-.228 (.122)	-.123 (.121)	.069 (.119)	.100 (.129)	-.081 (.136)
Has bank card balance	-.03 (.025)	-.029 (.024)	-.024 (.024)	-.008 (.023)	-.018 (.021)
Has credit card balance	-.013 (.029)	-.014 (.027)	-.012 (.027)	-.019 (.028)	-.016 (.028)
Observations	4084	4084	4063	4063	4063
State and year effects	Y	Y	Y	Y	Y
Race and age of household head	N	Y	Y	Y	Y
Complete set of household covariates	N	N	Y	Y	Y
State-level covariates	N	N	N	Y	Y
Interactions of year dummy and household covariates	N	N	N	N	Y

Notes to Table 6 overleaf.

## Notes to Table 6.

Results are presented for estimates of equation (1), the basic reduced-form model, using 1970 and 1977 data, with households in states that still faced binding usury ceilings in 1977 falsely labeled as having deregulated. We therefore expect the deregulation variable here to be uncorrelated with card usage. Sample sizes are lower than for previous estimates due to the elimination of 10 states that made material changes to their bank card interest rate usury laws between 1970 and 1977. As in previous tables, each row presents results for a different dependent variable, and each cell contains results on the deregulation variable from a different specification of equation (1). As such each column presents results for a different specification, as follows: column 1 regressions include only the deregulation variable, state and year effects; column 2 adds variables capturing the race and age of the household head; column 3 adds household structure, the household head's education and employment (including self-employment status), characteristics of the head's spouse, household income, housing tenure, and a home mortgage indicator; column 4 adds the log of aggregate state income, the Gini coefficient on state income, and the state employment rate.

Table 7. Reduced-form Effects of Interest Rate Deregulation on Asset Choice

	1	2	3	4
Checking account balances above 1977 median	0.032 (0.028) 5775	0.023 (0.031) 5775	0.009 (0.026) 5764	0.014 (0.025) 5764
Savings account balances above 1977 median	-0.033 (0.026) 5683	-0.037 (0.025) 5683	-0.041 (0.024) 5672	-0.053 (0.022) 5672
Has savings account	-0.019 (0.024) 6027	-0.025 (0.025) 6027	-0.021 (0.021) 6012	-0.007 (0.019) 6012
Has government savings bond	0.025 (0.02) 6025	0.019 (0.02) 6025	0.016 (0.021) 6007	-0.005 (0.019) 6007
Has certificate of deposit	0.024 (0.017) 6017	0.026 (0.017) 6017	0.019 (0.017) 5999	0.020 (0.017) 5999
State and year effects?	Y	Y	Y	Y
Race and age of household head?	N	Y	Y	Y
Complete set of household covariates	N	N	Y	Y
State-level covariates	N	N	N	Y

As in Tables 2, 3, 5, and 6, each row presents results for a different dependent variable, and each cell contains results on the deregulation variable from a different specification of equation (1). As such each column presents results for a different specification, as follows: column 1 regressions include only the deregulation variable, state and year effects; column 2 adds variables capturing the race and age of the household head; column 3 adds household structure, the household head's education and employment (including self-employment status), characteristics of the head's spouse, household income, housing tenure, and a home mortgage indicator; column 4 adds the log of aggregate state income, the Gini coefficient on state income, and the state employment rate.