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Fuzzy Math, Disclosure Regulation and Credit Market Outcomes*

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ABSTRACT

Disclosure regulation in credit markets is often put forth as a critical form of consumer "protection", but there is little hard evidence on why consumers need protection or whether disclosure regulation affects market outcomes. We address these two gaps. First we provide a new microfoundation for the widespread emphasis on consumer protection via mandated interest rate disclosure. The microfoundation is payment/interest bias: most consumers tend to substantially underestimate a loan interest rate when inferring it from a principal, maturity and monthly payment. This bias may provide lenders with an incentive to shroud interest rates and market "low monthly payments" when not constrained by regulation. Second and most critically, we find that an individual-level measure of payment/interest bias is correlated with rates on actual installment loans, but only on loans from lenders facing relatively lax Truth-in-Lending enforcement. Identification comes from variation across time in the general stringency of Truth-in-Lending enforcement, and from variation across lenders in the strength of enforcement. Our results hold even when we control for unobserved heterogeneity at the household level by examining households with multiple loans from lenders facing different enforcement. The results suggest that mandated interest rate disclosure can prevent lenders from catering to a cognitive bias in how consumers perceive interest rates, and highlight the importance of effective enforcement of disclosure regulation in affecting market outcomes.

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"Respondent...in numerous instances including but not limited to Exhibit A, has disseminated... advertisements that… promote the 'luxury of low payments.' Respondent's Gold Key Plus advertisements *fail to disclose the annual percentage rate* for the financing."

- Federal Trade Commission v. Herb Gordon Auto World, Inc., Docket C-3734, 1997.

I. Introduction

The United States' Truth in Lending Act (TILA) forces lenders to disclose all relevant loan terms but has a particular emphasis on the annual percentage interest rate (APR).[1] As the above example illustrates, TILA mandates APR disclosure and prohibits loan product presentations that focus exclusively on "low monthly payments." The focus on APR disclosure is a direct attempt to counter pre-TILA lender practices. Prior to TILA, lenders typically marketed monthly payments and either shrouded interest rates or presented alternatively defined rates that are nominally lower than APRs.[2] And even under TILA, many lenders continue to shroud interest rates and market "low monthly payments" despite the threat of fines and litigation.[3]

Why do lenders have strong incentives to shroud interest rates? And does mandating APR disclosure affect credit market outcomes?

These questions relate to more general ones about whether and why disclosure affects economic outcomes. These questions have concerned economists since at least Stigler's (1961) pioneering study of imperfect information, and have motivated a large theoretical literature. The proper scope and enforcement of mandated disclosure is central in current policy initiatives around subprime mortgages, auto loans, payday loans, tax refund anticipation loans, and other credit products where lenders are allegedly deceptive (Kroszner 2007). Yet there is relatively little empirical evidence on whether and why

[1] The National Commission on Consumer Finance notes that during the drafting of Truth-in-Lending law in the late 1960s, "Much of the attention and most of the heat generated by the legislation focused on requirements that the APR be calculated and disclosed." Rubin (1991) similarly emphasizes that "The Act's basic mechanism to achieve its goals was the requirement that creditors disclose the annual percentage interest rate on all consumer lending."

[2] See Gabaix and Laibson (2006) for a model of equilibrium shrouding. Pre-TILA lender marketing practices are well-documented in National Commission on Consumer Finance (1972), Rubin (1991), and the references in those papers. When lenders displayed interest rates pre-TILA they commonly reported "simple" rates that do not account for declining principal balances and consequently can be significantly lower in nominal terms than the APR. Figures 1-4 display examples of pre-TILA advertising.

[3] See, e.g., FTC Annual Reports, General Accounting Office (2004), and Fox and Guy (2005).

mandated disclosure affects market outcomes generally, and virtually no such evidence from credit markets.[4]

We seek to fill this gap by providing two types of evidence related to credit market disclosure regulation. First, we identify a new microfoundation for forcing lenders to disclose interest rates. Second, we provide some evidence on how disclosure regulation, and its costly enforcement, affects market outcomes.

We start by exploring the root cause of lender preferences for shrouding interest rates, and find a new type of microfoundation that speaks directly to lender incentives to shroud interest rates in particular, and to policymakers' emphasis on forcing APR disclosure.[5] Most consumers exhibit *payment/interest bias*: a tendency to underestimate the interest rate implied by a loan amount, maturity, and stream of monthly payments. Thus marketing "low monthly payments" and shrouding interest rates may induce more borrowing, and borrowing at higher interest rates, than when APRs are disclosed. Our companion paper details that payment/interest bias can spring from a simple and more general cognitive microfoundation: *exponential growth bias*, the well-documented tendency for individuals to dramatically underestimate the growth or decline of exponential series. Exponentiation features prominently in the mathematics of interest rates, and exponential growth bias produces payment/interest bias under general assumptions (Stango and Zinman 2007).

Thus a cognitive bias in how consumers intuit the mathematics underlying installment debt contracts yields the *possibility* that mandated APR disclosure affects market outcomes by "debiasing" consumers. Yet disclosure regulation might be economically irrelevant even in a market with biased consumers. Competition or reputations might drive loan prices toward marginal cost, and/or produce an equilibrium

[4] Notable exceptions are Mathios (2000) on salad dressing labels, and Jin and Leslie (2003) on restaurant hygiene inspections. Kroszner (2007) cites these papers and states that mandated APR disclosure is "generally believed to have improved competition and helped individual consumers", but does not cite any papers on credit market disclosure. Shaffer (1999) argues that mandated disclosure in credit cards did not change equilibrium interest rates. Mandell (1971) and Day and Brandt (1974) explore whether federal Truth-in-Lending law changed consumer "awareness" of interest rates, but do not look at any other economic outcomes.

[5] Standard theoretical microfoundations for mandated disclosure include search and shopping costs (Salop and Stiglitz 1977; Schwartz and Wilde 1982; Hynes and Posner 2002).

with voluntary disclosure.[6] Biased consumers may learn to avoid lenders that use payments marketing, and/or develop other strategies that neutralize their bias.[7] *De jure* regulation may not produce *de facto* mandated disclosure given costly enforcement. The sheer number of detected TILA violations (still numbering in the hundreds per year) indicates that many lenders find it a good gamble to violate the law – perhaps because enforcement in a market with tens of thousands of firms is difficult. So whether payment/interest bias and mandated disclosure are correlated with market outcomes in practice is ambiguous.

Accordingly the heart of this paper is an empirical study of the link between payment/interest bias, disclosure regulation, and the terms of actual loan contracts held by households. We use data from the 1983 Survey of Consumer Finance (SCF), which elicits a quantifiable household-level measure of payment/interest bias. The SCF also captures details on outstanding debts (including contract terms, product purchased, and date of origination) and household characteristics. The latter includes several measures of creditworthiness, financial condition, demographics, and preferences. Our main empirical models estimate whether payment/interest bias is correlated with the interest rates paid on actual car loans and other short-term installment loans,[8] and whether this correlation varies with the disclosure regime, conditional on observed and unobserved factors that might be correlated with both bias and interest rates.

We rely on three sources of variation to identify the effects of payment/interest bias and mandated APR disclosure on the interest rates paid by households. The first source is cross-sectional variation in the degree of payment/interest bias. Nearly all consumers underestimate the interest rate implied by a stream

[6] Several papers find that reputational incentives and/or competition will not necessarily produce voluntary disclosure or eliminate the ability of lenders to extract surplus from biased consumers (Jovanovic 1982; Farrell 1986; Shavell 1989; Fishman and Hagerty 2003).

[7] Evidence on learning about financial decisions suggests that it is important but incomplete (Agarwal, Chomsisengphet, Liu and Souleles 2006; Agarwal, Driscoll, Gabaix and Laibson 2007), and that it may be limited in the case of low-frequency decisions like installment borrowing and long-term saving (Benartzi and Thaler 2007). Evidence on consumer heuristics suggests that they are not necessarily adaptive in relatively abstract domains like math and finance, and may exacerbate rather than neutralize biases (Gilovich, Griffin and Kahneman 2002; Stanovich 2003).

[8] We ignore longer-term loans (almost of all which are 20- and 30-year mortgages) and revolving debt such as credit cards for reasons detailed in Section II-A.

of monthly payments (i.e., by principal, maturity, and payment amount). Despite that the fact that over 90% of consumers underestimate, there is substantial variation in the *degree* of bias.

The second source is cross-sectional variation in the strength of TILA enforcement by lender type. While all lenders are subject to TILA law *de jure*, banks face more stringent oversight and enforcement than non-bank finance companies *de facto*. If payment/interest bias matters for credit market outcomes, and the strength of disclosure enforcement is important, then we should see a differential relationship between rates and bias across loans from the differentially regulated lenders.

The third source is temporal variation in the relative strength of TILA enforcement for banks and finance companies. During our sample period enforcement stringency was relatively constant for banks, which face regular supervision and direct oversight by the Federal Reserve and other federal agencies. Finance companies and other non-bank lenders are policed rather than supervised, making civil actions against alleged violators more critical to enforcement. This is important because an April 1981 overhaul to Truth-in-Lending reduced penalties and greatly circumscribed the scope for recourse through the civil courts against alleged violators. The new TILA weakened enforcement and reduced the expected cost of violations for finance companies. If bias and disclosure enforcement affect market outcomes, then we expect to find a larger difference between the rate and bias correlations on bank loans and finance company loans post-1981 than pre-1981.

Taken together these three sources of variation, and loan-level data, yield a within-household triple-difference estimator of the relationship between payment/interest bias, mandated interest rate disclosure (which varies across lender type and time), and equilibrium loan interest rates. Our preferred specification takes a loan as the unit of analysis and regresses the interest rate paid on household fixed effects, loan-level characteristics, and a complete set of main effects (where applicable) and interactions among the household's payment/interest bias quintile, the loan's lender type, and the date of origination (pre- or post-TILA reform). The coefficients we focus on are the triple interactions $Bias_h \cdot Finco_l \cdot NewTILA_t$. These estimate the shift in the bias/rate relationship across TILA regime and lender type. Because

including household fixed effects limits the set of households that provide identification of the triple-difference effects, we also estimate models using the full cross-section of loans and an extensive set of household characteristics as controls in place of the household fixed effects.

Both the fixed effect and cross-section results suggest that payment/interest bias and credit market outcomes are related in empirically relevant ways, but *only on loans from finance companies in the post-TILA reform era*. More biased households pay roughly 300-400 basis points more when borrowing from lightly regulated lenders, and do not pay significantly more in other cases. The 300-400 basis point difference implies, for the typical car loan, a 6-8% loss in consumption (amount borrowed, holding payments constant) for the biased household.

Our finding that biased interest rate perceptions affect market outcomes is a novel contribution to the growing literature relating "behavioral" biases to market outcomes. Despite renewed theoretical and policy interest in the subject (Glaeser 2004; Campbell 2006; Ellison 2006), few studies have tested whether a consumer-level measure of cognitive bias is correlated with a consumer-level measure of financial contracts held in equilibrium. The studies that do exist focus on the relationship between measures of present-biased *preferences* and contract choice. In contrast we focus on the role of present-biased *perceptions* of borrowing costs (and of the opportunity cost of consumption more generally).[9]

And of course the finding that payment/interest bias has *contingent* effects on market outcomes provides a new type of motivation for studying the (relative) merits of disclosure regulation. The results are consistent with APR disclosure protecting consumers from their biased *perceptions* of borrowing costs. More standard motivations— biased expectations, or shopping costs— do not seem to drive our results. Because our analysis does not consider lender compliance costs, which can be substantial (Angell

[9] See Ashraf, Karlan, and Yin (2006) and Meier and Sprenger (2007) on present-biased preferences. Other papers show that equilibrium contracts are consistent with firms responding strategically to present-biased preferences but do not measure present-bias directly; see e.g., DellaVigna and Malmendier (2004), Oster and Scott-Morton (2005), and Shui and Ausubel (2005). See also Thaler and Benartzi (2004) for an example of successful product development based on presumed preference biases. DellaVigna (2007) provides a review of field evidence in "behavioral industrial organization." Much of the Law and Economics literature on disclosure regulation focuses on concerns about biased consumer *expectations* (Jolls and Sunstein 2006), but we are not aware of any studies that test links between consumer-level measures of biased expectations and financial contract choice. Our companion paper examines relationships between payment/interest bias (and other present-biased *perceptions* of opportunity costs of consumption) and portfolio choice (Stango and Zinman 2007).

1971; Elliehausen and Kurtz 1988), a complete welfare analysis is beyond the scope of this paper. But our results highlight the problem of costly enforcement of mandated disclosure and motivate consideration of alternative, low-cost "treatments" for biased perceptions of borrowing costs.

II. Consumer Loan Markets, Disclosure Regulation and Payment/Interest Bias

In this section we briefly describe consumer loan markets and how Truth-in-Lending law mandates APR disclosure. We then present our microfoundation for disclosure regulation and discuss its potential effects on equilibrium loan contracts.

A. Consumer Loan Markets

We focus on non-mortgage, short-term consumer installment loans ("short-term loans"): these are also sometimes called closed-end loans. Closed-end loans have fixed repayment schedules, in contrast to open-end or revolving loans such as credit cards. Most short-term installment loans in our sample- 60% by dollar volume - fund new or used car purchases, with maturities of 48 or 60 months. The remainder fund purchases of household durables such as furniture, appliances, entertainment equipment, educational expenses or home improvement expenditures.

Short-term loans are an important part of the household balance sheet, both during our sample and today. In 1983 households owed $325 billion in short-term installment debt, an amount that dwarfed revolving debt. Today outstandings for the two types of debt are roughly equal; short-term installment debt outstanding is roughly $1.3 trillion, compared to $800 billion in credit card debt and $400 billion on home equity lines of credit (Federal Reserve Board G19 Statistical Releases).

We focus on short-term loans for three reasons. First, the microfoundation we identify—payment/interest bias-- only applies to the class of loans with fixed monthly payments, ruling out an analysis of revolving loans. Second, our companion paper shows that interest rate perceptions on long-term loans (almost all of which are 20- and 30-year mortgages) are unbiased in theory and practice. Third, our empirical strategy relies heavily on *within*-household variation in loan interest rates across loans from

different lenders and during different time periods. This strategy does not work for mortgages during our sample period because most households hold only one mortgage.

B. Standard Motivations for Disclosure Regulation in Installment Markets

The federal Truth-in-Lending Act (TILA) passed in 1968 and is often viewed as the first modern consumer protection law (Rubin 1991). Its legislative history points to a broad set of objectives, including promoting "economic stability," facilitating comparison shopping, and protecting consumers from deceptive billing practices.

Despite these broad and diverse goals, it quickly became apparent that mandated disclosure of annual percentage interest rates (APRs) was TILA's key—and most contentious-- provision. The author and congressional sponsor of the law, Paul Douglas, noted that his first discussions with lenders about APR disclosure were met with "a storm of indignation and protest." Retailers and automobile dealers that provided their own financing objected especially vehemently (Rubin 1991).

The proximate motivation for mandating APR disclosure was lender marketing practices that shrouded or (arguably) distorted interest rates. As noted in the introduction, prior to TILA nearly all lenders quoted terms either without any reference to any interest rate, or using "simple" or "add-on" interest rates that were roughly half the level of APRs on short-term installment loans and did not account for the effect of declining principal balances on the opportunity cost of consumption. The ads in Figures 1-4, dating from shortly before the original TILA passed in 1968, are representative. Figures 1 and 2 emphasize low monthly payments and do not report any interest rate. Figures 3 and 4 quote simple interest.

The more fundamental motivation for presuming that APR disclosure changes market outcomes was and remains less precise. Policymakers often note that APRs provide a standard unit of comparison for loans with different maturities, and for loans to savings instruments with returns stated as interest rates. And several papers from the TILA enactment and reform period establish that consumers were unaware of APRs on actual or potential loans. This lack of awareness was the focus of the prior literature and

policy discussions (National Commission on Consumer Finance 1972), and suggests that TILA might impact market outcomes by reducing search costs and facilitating comparison shopping.

But what we find most striking about the prior literature on interest rate perceptions is the evidence that consumers tended to systematically underestimate APRs, even on loans they already held.[10] This motivates our investigation of a new potential microfoundation, based on *biased* perceptions of interest rates, that may complement or supplant the more standard search/shopping cost story.

C. A New Microfoundation for APR Disclosure: Payment/Interest Bias

In this section we build a microfoundation for APR disclosure, beginning with empirical evidence from a previously untapped source on how consumers infer APRs from other loan terms: the 1983 Survey of Consumer Finances (SCFs).[11] We find that consumers systematically underestimate APRs when inferring them from other loan terms; they display *payment/interest bias*. Our household-level measure of payment/interest bias forms the basis for our analysis of the relationships between bias, mandated disclosure, and equilibrium loan interest rates.

Our measure of payment/interest bias comes from two hypothetical questions that appear in the 1983 SCF.[12] The first question is:

> *"Suppose you were buying a room of furniture for a list price of $1,000 and you were to repay the amount to the dealer in 12 monthly installments. How much do you think it*

[10] This work includes Juster and Shay (1964), National Commission on Consumer Finance (1972), Day and Brandt (1974), Parker and Shay (1974) and Kinsey and McAlister (1981). These studies tended to focus on awareness ("Do you know what the APR on your loan is?"), but also asked respondents to estimate APRs from other loan terms. In presenting results of those questions, the focus was on measuring *mistakes*, typically as the share of consumers who were correct or close to correct. Some papers do make more direct statements about bias in consumers' inference about APRs; Parker and Shay (1974), for example, note that consumers display "a strong tendency to underestimate annual percentage rates of charge by about one-half or more…." More recently, Bernheim (1995; 1998) and Moore (2003) find evidence consistent with limited understanding of loan terms, including interest rates.

[11] The 1983 SCF is a nationally representative survey of household finance. The 1983 SCF has significant content overlap with the modern, triennial version of the SCF that started asking a very consistent set of questions in 1989 (but dropped the questions we use to measure payment/interest bias). We use data on the 4,103 1983 SCF households with relatively complete data, dropping the 159 "area probability sample excluded observations" (variable b3001). See Avery, Canner, Elliehausen and Gustafson (1984) for additional information on the survey.

[12] We find a similar distribution of bias based on responses to questions on *actual* loans currently held in the 1977 SCF. But we cannot use actual loans to measure payment/interest bias in the 1983 SCF because respondents do not self-report interest rates on that survey (see Stango and Zinman 2007 for results and details).

would cost in total, for the furniture after one year -- including all finance and carrying charges?"

The response to this first question is a lump sum *repayment total* (e.g., $1200).[13] Given the predefined maturity and principal amount, the repayment total yields i^*, the *actual APR* implied by the respondent's self-supplied repayment total.[14] Figure 5a shows the distribution of the actual APR in the 1983 SCF across all households. The mean is 57 percent, which corresponds to a stream of payments over the year totaling roughly $1350. The modal actual APR is 35% ($1200), with other frequent rates corresponding to round repayment totals ($1300, $1400, $1100, etc.). The twenty-fifth percentile is 35% and the seventy-fifth is 81% ($1500).

The next question in the survey is:

"What percent rate of interest do those payments imply?"

This response is i^p, the *stated* or *perceived APR*.[15] Figure 5b shows the distribution of perceived APRs. The perceived rate distribution has a lower variance than the actual rate distribution but the perceived rate is still correlated with the actual rate; we discuss this and the related issue of *how* respondents attempt to answer the payment/interest bias questions in Section VI-B.

One natural question is how our actual and perceived rate distributions compare to market rates on 1-year consumer durable loans. We are not aware of any lender-side data. The household*loan level data in the 1983 SCF suggests that the median rate on 12-month loans (as imputed from other loan contract terms) being paid back by respondents was 19%, with a twenty-fifth percentile of 15% and seventy-fifth

[13] The survey respondent is whomever was determined to be the "most knowledgeable about family finances." We use the terms "household," "respondent", "individual", "consumer" and "borrower" interchangeably.

[14] We assume that the monthly installment payments are equal when calculating the actual APR. Different assumptions about payment arrangements do not change the qualitative results that respondents generally underestimate interest rates (even if we assume that the first eleven payments are zero, and the last completely repays the loan). More important, while such transformations change the level measure of misperception they do not alter the cross-sectional ranking in misperception. It is that ranking that helps provide identification in our empirical tests below.

[15] Although the SCF question does not specify a particular definition of "rate of interest", we use the APR as our benchmark because it has been the standard unit of comparison for borrowing costs in the U.S. since the enactment of Truth in Lending law in 1968. Using alternative benchmarks such as the Effective Annual Rate (which tends to be higher than the APR), or the "simple" or "add-on" rate (which does not account for declining principal balances on installment debt and hence is dominated by the APR as a measure of the shadow cost of foregone future consumption), does not change our results.

percentile of 23%. So actual market rates fall between the perceived and actual (as implied by the self-supplied repayment total) rates implied by responses to the SCF hypothetical.

Given that market rates tend to be lower than the actual rate implied by responses to the SCF hypothetical, we focus on *relative* differences in the extent to which consumer perceptions deviate from the actual rate, rather than in an absolute level of the difference. In order to classify respondents as more or less biased we start by calculating the level difference between the perceived and actual rates (and then bin households into quintiles). We call this level difference *payment/interest bias*.

Figure 6a presents a histogram of payment/interest bias in the 1983 SCF. The prevalence of bias is striking. Over 98% of respondents underestimate the actual rate. Roughly twenty percent of respondents give the "simple" or "add-on" rate (e.g., a repayment total of $1200 yields a perceived rate of 20%). But responses are biased even relative to this rate; those who supply something other than the add-on rate tend to underestimate relative to the add-on (Figure 6b). The size of the bias is also striking, although it is less integral to our empirical approach since the absolute magnitude is difficult to interpret given the nature of the questions. The median bias is -25 percentage points (-2500 basis points), and the mean bias is -38 percentage points.[16]

Table 1 tabulates payment/interest bias by quintiles, and provides further detail on responses. There is also a set of consumers who fail to report either a perceived APR, an actual APR or both; those are in the "n/a" column.

While we do not know of any more recent representative data measuring payment/interest bias, there is one bit of corroborating contemporary evidence. Following an internal presentation of this paper, a skeptical colleague gave an updated version of the SCF questions to students in a finance class that had recently covered discounting. Of thirty-seven students, all underestimated the APR: one gave a rate above the add-on rate, twelve gave the add-on rate, and the remainder underestimated relative to both the APR and the add-on rate.

[16] Earlier studies typically only report the share of consumers underestimating the actual rate. The one study that does allow us to infer something about the size of payment/interest bias is Juster and Shay (1964). Average bias in their sample of Consumers Union members is substantial (1500 bp) but smaller than in our samples.

D. Exponential Growth Bias: A Cognitive Microfoundation for Payment/Interest Bias

Systematic underestimation of APRs when shown other loan terms can be explained by the more general and well-documented tendency of individuals to underestimate mathematical terms involving exponentiation. We discuss that *exponential growth bias* (EG bias) in detail in our companion paper (Stango and Zinman 2007), and sketch the intuition here. Consider a consumer attempting to infer a loan interest rate i^* when confronted with a periodic payment, principal and maturity. Given a loan amount L, maturity t, and periodic payment m this implies solving:

$$m = Li^* + \frac{Li^*}{(1+i^*)^t - 1} \qquad (1)$$

A consumer with EG bias is one who underestimates the exponential term $(1+i^*)^t$. Eisenstein and Hoch (2005) show that EG bias is pervasive in its most natural economic context: intuitive assessments about the return to long-run savings. Given a present value and interest rate representing the return on savings, nearly all consumers underestimate the future value of their savings.

The relationship between exponential growth bias and payment/interest bias is a bit more subtle, in part because the interest rate is defined implicitly in the equation above. But our companion paper proves that a general form of EG bias will produce payment/interest bias. The intuition is that consumers who underestimate exponential growth fail to fully account for the effect of declining principal balances on the implied interest rate (i.e., for the effect of not getting to borrow the full principal amount for the entire maturity). Indeed, the addon rate assumes no decline in principal balance at all; it falls directly out of a specification of EG bias known as "linear bias" in which the exponential growth term $(1+i)^t$ is approximated as $(1+it)$. We also show that payment/interest bias is more severe on short-term loans. Intuitively this is because linear approximations (or more generally, underestimates of) of exponential growth are more accurate over longer maturities as the impact of declining principal balances lessens. One way to see this is by considering the limiting case of an interest-only loan; this has infinite maturity,

does not require any exponentiation to calculate the rate implied by the principal and monthly payment, and is the case in which the addon rate and APR are equal.

E. Payment/Interest Bias and Lender Behavior in Competitive Loan Markets

Now we discuss how payment/interest bias might explain the revealed preference of lenders for shrouding APRs and emphasizing monthly payments. The discussion frames our empirical strategy in Section IV by describing how a correlation between payment/interest bias and interest rates might exist in equilibrium, and be mediated by the mandated disclosure regimes detailed in Section III.

We begin with a stylized example involving a monopolist lender unconstrained by disclosure regulation. Assume that consumers will borrow if they perceive the interest rate to be less than or equal to 10%, and that they vary in their degree of payment/interest bias. Assume also that consumers make decisions based on the interest rate if it is presented, and on a perceived rate if the actual rate is not presented.[17] To simplify matters, assume further that consumers are equally risky, that risk is perfectly observable by the lender, and that repayment does not depend on the interest rate.[18]

If it can, a lender who knows that some consumers have payment/interest bias will present offers in terms of monthly payments and loan maturities, and force consumers to infer interest rates. The optimal pricing and marketing strategy depends on the observability of payment/interest bias. If bias is observable, the lender can perfectly price discriminate and design loan offers that induce biased customers to perceive a rate of 10 percent but is actually much higher. While direct observation of such bias may not be possible, many consumer lenders present and negotiate loan terms via "high touch

[17] It may be rational for liquidity constrained consumers to (largely) ignore interest rates (Adams, Einav and Levin 2007; Attanasio, Goldberg and Kyriazidou forthcoming; Karlan and Zinman forthcoming). We have several ways of controlling for liquidity constraints and comparison shopping, and detail them in Section IV.

[18] This abstracts from issues of risk-based pricing (Edelberg 2006) under asymmetric information (Edelberg 2004; Adams, Einav and Levin 2007; Karlan and Zinman 2007b).

marketing" and face-to-face negotiation; in the largest segment (auto loans) there is evidence that price discrimination on both loan and non-loan terms is common.[19]

Even without the ability to observe or learn about individuals' payment/interest bias, a lender can present a menu of loan offers that induces borrowers to self-select based on bias. For example, on a $10,000 new car loan the lender might offer "either 10 percent, or 48 low monthly payments of $278," where the monthly payments imply an actual rate of 15 percent. Unbiased customers will prefer the first offer. Customers with substantial payment/interest bias will perceive a rate lower than 10 percent on the second offer, and prefer it. Figure 7 shows an example of this sort of loan marketing.

While the monopoly example provides intuition it abstracts from actual market structure in consumer loans. Tens of thousands of banks and finance companies offer consumer loans, and in 1983 the mean (median) county was served by 35 (9) financial institution establishments (source: County Business Patterns). There are few barriers to entry, and by most accounts the market has been competitive since before enactment of the original TILA.[20] This raises the possibility that loan markets are competitive enough to render payment/interest bias irrelevant by driving loan rates to marginal cost.

While we do not develop a model of loan market competition with payment/interest bias here, other models are similar enough to highlight the assumptions necessary for payment/interest bias to matter even in a free-entry equilibrium. Gabaix and Laibson (2006) show that shrouding can exist even in highly competitive markets if some consumers are unaware of their bias (in that model, the bias is underestimation of add-on prices). Partial awareness seems to be an apt description in our setting, since while payment/interest bias seems to have substantial effects on financial condition on average, the use of outside advice rises sharply with bias, and eliminates its effects (Stango and Zinman 2007). Bias can also be viewed as generating differences in willingness to pay for loan contracts (depending on how the terms

[19] Some recent evidence suggests that contemporary auto loan finance companies often mark-up loans in "on the spot" negotiations (Charles, Hurst and Stephens 2006; Cohen 2006). For evidence of price discrimination on car prices, see Busse, Simester, and Zettelmeyer (2007). We find no evidence in our data that lenders offset higher loan rates with lower purchase prices (see Section VI-D for details).

[20] By competitive we mean that the marginal entrant earns zero economic profit.

are framed). Such differences can generate equilibrium price discrimination either across firms or within firms if consumers' cross-price demand elasticities vary.[21]

Apart from this theoretical ambiguity, mandated disclosure might have its intended effect of countering lenders' desire to shroud APRs. If so, lenders will disclose APRs and payment/interest bias should not be correlated with the terms of actual loan contracts.

In short, while both theory and the institutional facts about how loans are marketed suggest that lenders cater to payment/interest bias in a way that leads to a correlation between bias and loan rates, whether such a correlation is empirically relevant is ambiguous.

III. Consumer Credit Markets and Disclosure Regulation

In this section we discuss the institutional history of the Truth in Lending Act (TILA), focusing on two differences in the strength of enforcement: across lender type and over time. This allows us to sharpen our empirical tests regarding the relationship between payment/interest bias, disclosure regulation and credit market outcomes.

The first difference in enforcement is by lender type. While TILA applies equally to all lenders *de jure*, a key feature of the law is its assignment of jurisdiction for enforcement. Banks and other depository institutions are under the purview of the Federal Reserve System and other bank supervisory agencies. Banks are monitored and examined regularly for safety and soundness purposes, and TILA compliance was incorporated into this process (Walter 1995). In contrast, enforcement authority for non-bank "finance companies" lies with the Federal Trade Commission (FTC). The FTC is a law enforcement rather than a supervisory agency and consequently has tended to lack the staff and imprimatur to conduct regular exams of finance companies.

The second source of differences in enforcement is over time. In response to confusion about what constituted compliance with the law, and concern about escalating caseloads and lender liability, the

[21] See Borenstein (1985) and Holmes (1989) or theoretical models of price discrimination in free-entry markets. Borenstein (1991) and Shepard (1991) show that price discrimination exists in retail gasoline markets.

Truth-in-Lending Simplification and Reform Act was signed into law on March 31, 1980 (effective beginning April 1, 1981). The changes to TILA were more an overhaul than a reform, prompting the Federal Reserve Board to label the 1980 law a "new Truth-in-Lending-Act" (Federal Reserve Board 1981).

Both legal scholars and the Board itself found that the new TILA greatly limited the size and enforcement of penalties.[22] The original TILA "was enforced with tough civil penalties" (e.g., Peterson 2003, p. 880). Consumers and their advocates filed over 17,000 civil lawsuits in federal courts against lenders for alleged violations during 1969-80. TILA cases represented as much as 2% of the entire federal court caseload in some years. Some of these suits resulted in large damage awards for plaintiffs. Many additional cases settled out of court (Federal Reserve Board 1981; Willenzik and Schmelzer 1981).

The new TILA, on the other hand, dictated that penalties be imposed only for "significant" violations. It clarified the cap on maximum recovery for multiple class action. And it broadened and strengthened the ability of lenders to avoid punishment for violations by taking remedial actions. In short, it greatly limited the scope for private enforcement.

In concert, these differences in enforcement provide a difference-in-difference in the strength of TILA as a means of disciplining lender behavior. The jurisdictional difference means that banks in general have plausibly faced stronger enforcement than non-bank finance companies, over our entire sample period. The passage of the new TILA in 1981 reduced the scope for private enforcement through the civil courts and financial markets.[23] Because public enforcement remained essentially constant, this created a relative weakening of enforcement for finance companies relative to banks.

The available descriptive data squares with the interpretation that the new TILA represented a greater reduction in compliance incentives for finance companies than for banks. The TILA caseload dropped almost immediately to "relatively sparse" levels (Fonseca and Fonseca 1986; Keest and Klein 1995).

[22] For additional legal details on the penalties and enforcement provisions discussed in the next two paragraphs, see, e.g., Boyd (1981), Federal Reserve Board (1981), Prigden (1990), Keest and Klein (1995), and Peterson (2003).

[23] In addition to circumscribing the scope and penalties for violations as described in the preceding paragraph, the new TILA also limited liability to loan originators in most cases. This reduced incentives for monitoring by secondary market participants.

Bank supervisory agencies continued with regular exams and overall it seems that bank compliance was fairly complete in the 1980s, with most violations characterized as mistakes rather than willfully deceptive practices (Willenzik and Schmelzer 1981; Elliehausen and Kurtz 1988; Barefoot 1990; Jackins and Gates 1990). In contrast the FTC did not begin to fully supplant private enforcement until after our sample period. A campaign begun in 1985 to improve TILA compliance in auto loan advertising turned up thousands of noncompliant finance companies. Eight percent of these lenders did not comply even after being contacted by the FTC. The FTC proceeded to file lawsuits against a small fraction of the noncompliers (Fortney 1986; Federal Trade Commission various years). Figures 8 and 9 provide anecdotal evidence of the differential effects of TILA on banks and finance companies. Figure 8 shows a post-TILA finance company ad emphasizing payments, while Figure 9 shows a post-TILA bank ad emphasizing rates.

IV. Empirical Strategy

Identifying the relationships between payment/interest bias, disclosure regulation and consumer loan interest rates is the primary empirical question at hand.[24] Below we detail our econometric strategy and identification issues. Then we present and discuss the results in Section V.

A. A Cross-Sectional Model of Loan Interest Rates

Let the reduced-form cross-sectional relationship describing the loan interest rate r on loan l, obtained by household h at time t, be:

$$(1)\quad r_{hlt} = \beta_1 Bias_h Finco_l NewTILA_t + \beta_2 Bias_h Finco_l + \beta_3 Finco_l NewTILA_t + \beta_4 Bias_h NewTILA_t + \beta_5 Bias_h + \beta_6 Finco_l + \beta_7 NewTILA_t + f(X_h) + g(Z_l) + \varepsilon_{hlt}$$

The triple-interaction term $Bias_h Finco_l NewTILA_t$ asks the primary empirical question in the paper: how does the correlation between bias and loan rates vary across lender type and TILA regime? We specify

[24] We also consider relationships between bias, disclosure, and the propensity to borrow from finance companies in Section V-E.

bias using an indicator for the quintile of household-level bias as shown in Table 1, or a dummy for non-response; this means that any interaction term containing $Bias_h$ is actually a vector containing the five quintile dummies and the "no answer" indicator.[25] Lender type is measured by a loan-level indicator $Finco_l$ for whether the loan comes from a finance company. TILA regime is measured via an indicator for whether the loan was obtained after TILA reform. The full model also includes the double interactions $[Bias_h Finco_l, Bias_h NewTILA_t, Finco_l NewTILA]_t$ as well as the level effects of the single terms $[Bias_h, Finco_l, NewTILA_t]$.

The triple-difference approach partials out a number of confounding influences on interest rates. The level effect of bias $Bias_h$ measures correlations between payment/interest bias and loan rates that are constant across lender type and TILA regime. These may reflect a primary bias/rate relationship that is constant across lenders and regime, but will also capture unobserved household-level characteristics correlated with both bias and loan rates (and not captured by household-specific variables in X_h), and hence must be interpreted cautiously. Similarly, the lender type dummy $Finco_l$ measures the average level difference in rates between banks and finance companies. This presumably captures differences in customer mix (including credit risk) and in other aspects of loan production functions. Finally, the TILA reform indicator $NewTILA_t$ will measure the average shift in rates across all institutions following TILA reform, but also reflect the influence of other time-varying effects (like the substantial time series variation in market rates during our sample period). The double interaction terms will measure differences in the bias/rate relationship at finance companies $(Bias_h Finco_l)$, differences in finance company interest rates after TILA reform $(Finco_l NewTILA_t)$, and differences in the bias/rate relationship after TILA reform $(Bias_h NewTILA_t)$. We discuss the interpretation of these effects below.

[25] We have used other functional forms (linear, log-linear, and quadratic) with similar results but prefer the less parametric form offered by the vector of quintile dummies. We have also estimated specifications treating add-on responses distinctly from others; the add-on answer coefficients are not significant in any of our empirical models.

The next set of controls is a vector X_h of household-specific variables, measured on the survey date. These include household-level characteristics such as education, race, gender, state of residence, employment status and income, asset and debt levels, job title, industry of primary employment, financial attitudes and preferences, expectations of future income, several measures of credit risk and liquidity constraints (including categorical variables for job tenure, recent denial of credit, recent late payments, and possession of a credit card), and a variable assessing whether a household shops for loans based on monthly payments or APRs.[26] This set of variables is meant to be exhaustive, even at the risk of "over-controlling" that might underestimate the true bias/rate relationship.[27]

Table 2 presents unconditional relationships between many of these variables and our payment/interest bias categories.[28] Education, income, and wealth are all highly correlated with bias. Bias is also correlated with health, creditworthiness and measures of financial sophistication (ATM/credit card use). In addition to these standard measures of household demographics, financial condition and well-being, bias is also highly correlated with our measures of preferences.[29]

These correlations suggest two things. First, they illustrate that our measure of bias is not random; it is clearly strongly correlated with what both inputs and outputs to household financial condition. Second, it highlights the importance of controlling as completely as possible for household-specific heterogeneity that might be correlated with both payment/interest bias and loan interest rates. This motivates our use of

[26] The SCF asks consumers this question: "... in choosing an automobile loan, which of the credit terms listed on this card would be most important to you if you were going to use credit to purchase a car?" Consumers list their top three choices from a list of over ten. The most popular responses are "interest rate" and "size of the monthly payment," which together comprise roughly half of all responses. Others include: the total size of interest/loan payments, the size of the loan, and fees for late or early payment. We classify a household as "shopping on payments" if it lists payments among the top two characteristics but not interest rates. We have used a number of other definitions with no effect on the results.

[27] See Angrist and Krueger (1999) for a discussion of over-controlling. We may be over-controlling with other covariates as well here; e.g., with balance sheet variables such as wealth and its components. Stango and Zinman (2007) estimates relationships between bias and these variables.

[28] See http://www.dartmouth.edu/~jzinman/Papers/Stango&Zinman_FuzzyMath_Web%20Appendix.pdf for further detail on variable definitions and construction.

[29] Our companion paper finds that the *conditional* correlation between payment/interest bias and preferences is weak. In contrast the SCF preference measures are significantly and highly correlated with financial decision in the expected ways, conditional on our other control variables (Stango and Zinman 2007).

household fixed effects in the primary empirical specification, although we also estimate models using the household characteristics in Table 2 (as well as those listed in the notes to Table 2) as covariates.

The vector Z_l contains loan-specific characteristics. As with the household-specific characteristics, our goal is to control for loan-specific heterogeneity that might be correlated with bias, lender type and TILA regime. The loan characteristics include the amount borrowed, loan maturity (using a vector of dummies for months of maturity), one of fourteen product purchase categories (e.g., "used car," "furniture"), and a vector of indicators for year and month of loan origination.[30]

B. Unobserved Household-Specific Characteristics and Household Fixed Effects

Because our unit of observation is a loan, and because many households hold loans from different lender types that were originated at different times, we can also include household fixed effects as an additional control for unobserved, time-invariant household heterogeneity that might be correlated with loan rates, lender type, and payment/interest bias. This approach helps deal with the concern that payment/interest bias is a measure of financial sophistication and therefore correlated with credit risk. If credit risk is imperfectly captured by our other controls, and also induces borrowing from finance companies at higher rates, then we might see a spurious effect on interest rates, as measured by the double interaction $(Bias_h Finco_l)$. In some of our specifications we would like to conduct inference on this variable and hence household fixed effects are critical to our identification strategy. Our fixed effects model is:

$$r_{hlt} = \beta_1 Bias_h Finco_l NewTILA_t + \beta_2 Bias_h Finco_l + \beta_3 Finco_l NewTILA_t + \beta_4 Bias_h NewTILA_t + \beta_5 Bias_h + \beta_6 Finco_l + \beta_7 NewTILA_t + \xi_h + g(Z_l) + \varepsilon_{hlt}$$

Estimating the fixed effects model requires dropping the household-specific covariates. It also prevents identification of the level bias effects $Bias_h$. The fixed effects model also limits the set of loans that provide identification of the interaction terms to those from households with multiple loans (and

[30] In the results we show, include a vector of year dummies and a vector of month dummies. We have also estimated the model with a full set of year/month dummies. The results do not change.

heterogeneity in lender type and/or TILA regime). We therefore also estimate the cross-sectional model detailed above and compare results from the two specifications.

C. Risk-Based Pricing Across Lender Type and Regime

While it should not affect the triple-difference interactions, unobserved risk may cloud interpretation of the double interaction $(Bias_h Finco_l)$. Adding household fixed effects should control completely for the level effect of unobserved household-specific risk. We also try to control directly for the possibility that risk differentially affects pricing by banks and finance companies. We do this by interacting two household-level measures of default risk (a recent denial of credit by a lender, and whether the household has made any late debt payments in the last year) with the bias, lender type and TILA regime variables. In some specifications we interact these variables with the triple interaction $Bias_h Finco_l NewTILA_t$ as well. In other specifications we estimate models using only the subsample of households with observably "good" credit: those with neither a credit denial nor a late debt payment within the last year.

D. Unobserved Time-Varying Risk

A final concern is that a component of unobserved risk is varying over time at the household level, and correlated with lender type and interest rates. However, such correlations seem unlikely. They would require that the *level* of bias (measured at one point in time) is correlated with the *variance* of any unobserved credit risk over time, after conditioning on loan-specific characteristics used to price that risk. They would moreover require that such a correlation between level bias and the variance of time-varying credit risk have different correlations with interest rates on finance company loans, and that this difference changed over the two TILA regime periods. At a minimum this would require that banks and finance companies priced risk differently, and that this difference changed before vs. after the TILA reform. We are not aware of any evidence (anecdotal or otherwise) that would corroborate this story.

V. Results

This section presents the results obtained from estimating our cross-sectional and fixed effect models detailed above. But first we describe our samples of loans and households in greater detail.

A. *Descriptive Statistics*

Table 3 shows descriptive statistics for the full sample of outstanding non-mortgage installment loans owed by households in the 1983 SCF.[31] Row 1 shows the distribution of the 1929 households with any loan, across our categories of payment/interest bias. The next rows show the total number and average of loans held by households in each bias category. There is no evident pattern between bias and the number of loans in this raw data. In all there are 3102 loans. Of these 3094 have sufficient information to use in our fixed effect specifications reported in Table 4 below. Missing household characteristics further reduce the sample to 2,973 loans for the cross-sectional specifications reported in Table 5 below.

Rows 4-12 describe loan characteristics. Mean loan sizes are large and decline sharply as bias increases; while the difference in means is large, it is affected by a small number of very large loans in the low-bias quintiles. The 90th percentile of loan size is $25,000 in quintile 1, $15,000 in quintile 2, and between $8,000 and $10,000 in each of the other categories. Not surprisingly, median loan sizes are much smaller and decline less sharply as bias increases. Rows 6 and 7 show that mean and median maturity are flat in bias; nearly all loans in our sample have maturities between 12 and 72 months, a pattern that is similar across bias categories. Rows 8-11 describe our primary dependent variable, the loan APR, by bias quintile, lender type and TILA regime. The pattern in the raw data is suggestive: there is a bank/finco rate gap that is greater for more-biased households, and it grows larger after the TILA regime change.

Recall that all specifications also include a vector of dummy variables for loan purpose and year of origination. Summary data for these variables are in Appendix Table 1.

[31] Recall from Section II-A that we have theoretical and practical reasons to restrict the sample to such loans. First, payment/interest bias affects borrowing decisions only for relatively short maturities, both in theory and in practice (Stango and Zinman 2007). In contrast mortgage loans were nearly all 20- and 30-year maturities during our sample period. Second, the second mortgage market barely existed during our sample period, and hence very few families held more than one mortgage. This precludes estimating our household fixed effect model on mortgage loans.

B. Fixed Effect Model Results

Table 4 presents estimates from several different specifications of our fixed effects model. The sample includes the 3094 short-term loans described directly above.[32] Column 1 includes the complete set of interactions, with the triple-difference coefficients shown in the top rows. Recall that the level bias effects are subsumed in the household fixed effects, and that the *NewTILA* main effect is subsumed by the year of origination dummies. We omit Bias quintile 1, and measure the effects for more-biased categories relative to the baseline effects *Finco*, *NewTILA* and *FincoNewTILA*. The coefficients on the triple-interaction variables identify the extent to which lenders facing lighter TILA enforcement (i.e., finance companies under the post-reform TILA regime) vary loan pricing with payment/interest bias.

The results in column 1 suggest that borrowers in quintiles 2-5 have loans with interest rates 200-500 basis points more than their least-biased counterparts when borrowing from lightly regulated lenders. Three out of the five triple-interactions are significantly at 10% or better, and the triple-interaction variables are jointly significant as well. The bottom rows also show p-values for exclusion of sets of variables. Only the triple-difference variables are jointly significant, and the restriction that neither the single or double interaction terms are different from zero can not be rejected. Column (2) therefore shows a version of the model dropping all but the triple-difference variables. The estimated effects are more precisely estimated, and still jointly significant.

Results in columns (3)-(7) check robustness and provide further detail on variation in the data that provides identification. Columns (3) and (4) are analogous to (1) and (2) but also interact the triple-difference variables with variables measuring creditworthiness. This increases the size of the point estimates, though not in a statistically significant way. The sign of the double interaction *FincoNewTILA* changes, suggesting (along with the p-value for the interactions) that we can not reject an overall change in how banks and finance companies price risk after the TILA regime change. Model (5) drops

[32] One could envision estimating a first stage selection equation that estimates the likelihood that a household borrows. While we do find that biased households are more likely to be borrowers, we do not pursue the two-stage strategy because there are no good candidates for the exclusion restriction in the first stage.

households that we define as facing relatively severe credit constraints. Again, this increases the point estimates. Models (6) and (7) drop the triple-difference and retain the double interactions.

While we do not discuss the results here, we have also estimated a variety of alternative models with different functional forms for payment/interest bias. Appendix Table 2 shows these results, which are generally robust.

Several other patterns in Table 4 seem noteworthy. Our R-squareds are fairly high: each specification explains around 50% of the within-household variation in loan rates. The household fixed effects are always jointly and highly significant. So too are the loan characteristics, with the exception of year of origination.

In all the fixed effect results are consistent with the interpretation that finance companies were *relatively* free to exploit payment/interest bias (by shrouding interest rates) due to weakened incentives for compliance under the new TILA.

C. Cross-Section Model Results

For the purpose of comparison, we also estimate our main model without household fixed effects, but with the set of household-specific covariates discussed earlier. The advantage of this model is that it estimates the effects of interest using a broader sample. The disadvantage is that the model is more vulnerable to omitted (household-specific) variable bias.

Table 5 presents estimates from several different specifications. The first two columns follow the fixed effects table. Here none of the interactions are jointly significant in the full model (Column 1). However, when we drop the second-level interactions and main effects (none of which are jointly significant) the triple-different coefficients become individually and jointly significant. The point estimates in Column 2 imply that borrowers in bias quintiles 2-5 paid about 200 basis points more than their least-biased counterparts when borrowing from finance companies in the new TILA period (relative to pre-reform). Again, the next columns include each set of interactions individually to illustrate patterns of variation in the data. Essentially, a good deal of identification comes from bank/finco differences that

are largely constant across time. There is weaker evidence that TILA reform induced a level shift in the bias/rate relationship across all lender types. The level bias indicators are neither large nor statistically significant. Column 6 shows that the triple-interaction results do not change if we drop observably poor credit risks from the sample. Column 7 shows results for the cross-section model using only the set of households with multiple loans from different lender types or TILA regimes (these are the households that provide identification in the household fixed effect model). The point estimates on the triple interaction terms are not statistically different from those in the full model. This suggests that the household fixed effect results are not being driven solely by differences in the characteristics of households with multiple loans and may be valid for the cross-section more broadly.

In all the cross-section estimates are generally consistent with the qualitative and quantitative findings of the fixed effect model. Payment/interest bias seems to have economically significant impacts on loan market outcomes, and effects that are mediated by disclosure regulation. On the whole the difference between borrowing from lightly- vs. heavily-regulated lenders is about 200-500 basis points greater for households in bias quintiles 2-5 compared to their least-biased counterparts.

D. Interpreting the Magnitudes: Bias, High Rates, and Foregone Consumption

What does a 200-500 basis point "bias markup" imply in terms of foregone consumption? Table 6 explores this question. Following the fixed effect results we assume that the more biased households (e.g., those in quintiles 4 and 5, and the "no quiz answer" category) will pay 400 basis points more when borrowing lightly regulated lenders, relative to borrowing from more regulated lenders, than less-biased households do.

Table 6 applies our bias markup to median loans in the four most common purchase categories: home improvement, new car, used car, and other household durable (principally appliances). We assume that "less-biased" borrowers pay 14% APR, and "more-biased" borrowers pay 18% APR (these are the round figures closest to the sample mean). Loan amounts are median values for the 1983 SCF. The third row

shows the impact the 400 basis point markup has on the monthly payment, holding the loan amount and maturity constant at the product category medians. The fourth row shows the additional interest paid over the life of the median loan.

The last three rows translate these effects into implied changes in loan amount. The "implied loan increase" is the additional amount one could borrow at the less-biased rate and the more-biased monthly payment. Since this is a present value it captures the consumption foregone by paying the bias markup (again in 1983 dollars). The next two rows scale this by the loan amount and a presumed household income of $25,000 (recall that Table 2 presents the median income for each bias category).

These magnitudes suggest that the potential economic effects of payment/interest bias, and the potential mediating impacts of disclosure, are both substantial. For example, if it is indeed the case that disclosure regulation helps many households avoid losing about 1% of consumption this would be a substantial gross benefit (that would of course need to be weighed against the costs of regulation).

E. A Word on Marketing Mechanisms: Exploiting Bias on Extensive and/or Intensive Margins?

Section II-D discussed how lenders that are not prevented from shrouding can exploit payment/interest bias using contract menus and/or high-touch marketing/negotiation. A related question is whether lightly regulated lenders *attract* more biased borrowers (the extensive margin), and/or whether they simply *extract* more from biased borrowers that happen to show up.

Appendix Table 3 casts some doubt on the importance of the extensive margin. Here we estimate several models that explore the question of whether borrowing from finance companies increases with bias (post-TILA reform). The unconditional, significant positive correlations between finance company borrowing and bias (see also Table 3) do not survive the inclusion of additional controls. And we do not find any evidence that more-biased households increased their borrowing from finance companies following the TILA reform.

VI. Alternative Interpretations

In motivating our empirical strategy above we detailed what we take to be the greatest threats to identification. These threats all stem from a possible correlation between our measure of payment/interest bias and (time-varying) unobserved credit risk – though as we note in Section IV, that correlation would itself need to have changed following TILA reform in order to explain our empirical results. In this section we address some other concerns regarding the interpretation of our bias measure and the results from our empirical models.

A. The Difficulty of the APR Questions, and Interpretation

One general concern starts with the observation that we define bias based on the answer to a very difficult problem: calculating an APR. The problem is not intractable however. A simple heuristic—doubling the add-on rate— would get an SCF respondent close to the correct answer and into our least-biased category. Moreover difficulty does not necessarily produce bias on average, even when respondents resort to guesses. The "wisdom of crowds" has been documented extensively (Surowiecki 2005).[33]

Also, recall that we find substantial payment/interest bias on actual loans as well (Stango and Zinman 2007). This suggests that variation in payment/interest bias is not the mechanical byproduct of hypotheticals.

A related concern is that the difficulty of APR inference introduces substantial noise into the responses. But our measure of bias is clearly not random—it is strongly correlated not only with our outcomes, but also with the most plausible covariates (such as income and education).

Finally, recall that our identification strategy relies on cross-sectional variation in the size of bias; it does not require anyone to be correct. In fact, a simpler question answered correctly by more consumers would yield less information.

[33] For brief accounts of a seminal and a recent example see "Sweet Success Shows you can Count on the Public" at: http://www.nature.com/nature/journal/v444/n7115/full/444014a.html .

B. Willingness to Pay, not Bias

A related concern is that since calculating the interest rate implied by one's repayment total is difficult, respondents may effectively answer a different question. In particular it is natural to wonder whether our measure of bias actually measures willingness to pay (WTP) for debt rather than variation in interest rate perceptions. This is particularly important given that it may be rational for the loan demand of credit constrained borrowers to be more sensitive to monthly payments than to interest rates (Adams, Einav and Levin 2007; Attanasio, Goldberg and Kyriazidou forthcoming; Karlan and Zinman forthcoming).

WTP fails to explain why respondents' answers are *internally inconsistent*, however. There is no clear motive or cognitive microfoundation for consumers supplying WTP for their actual rate (calculated from their loan repayment total), and something much lower (presumably a "fair" market rate rather than WTP) when asked for a perceived rate.

Interpreting actual rates as WTP and perceived rates as perceptions about fair rates is equivalent to saying that consumers are not attempting to solve the problem as posed. The data suggest otherwise. To take two examples from Stango and Zinman (2007): 1) the data fit a standard functional form found in lab experiments on exponential growth bias where researchers have been able to monitor and study problem-solving approaches; 2) actual and perceived rates are correlated; e.g., among those with actual APRs below the median, the correlation between actual and perceived rates is 0.46 in the 1983 data.

Finally, recall that our main specification identifies the relationships of interest from *within*-household variation in loan sources and rates obtained at different times. Consequently WTP would need to be time-varying, and relatively highly correlated with finance company loans in the post-TILA reform period, to explain the results. Most dubiously, respondents would have to answer the payment/interest bias questions such that our measure of bias is positively correlated with *variance* in the probability that credit constraints bind – and, the strength of that correlation would need to be correlated with TILA reform.

C. Unobserved Heterogeneity in Preferences

Another concern is that our cross-sectional variation in payment/interest bias reflects unobserved heterogeneity in preferences that is correlated with loan interest rates.

But again recall that we identify the difference in the correlations between bias and interest rates on finance company vs. bank loans using *within*-household variation in loan source and rates, over different loans taken out close in time. Consequently preferences will confound the interpretation only if they are time-varying, at high frequencies.

Also note that the disclosure regime should be irrelevant if preferences drive the observed correlations between bias, loan source, and interest rates. More generally any explanation for our findings should account for the fact that the correlations between our measure of bias, loan source, and loan rates changed after TILA reform.

D. Unobserved Tradeoffs Between Loan Terms and Purchase Price

A final concern is that lenders might trade purchase prices against loan terms, meaning that total costs for a product might not be higher even with higher interest rates. While we do not report the results, we do observe purchase price of the product being financed for a subset of loans (car and home improvement loans). Adding this information to our vector of loan characteristics does not change the regression results reported above. Furthermore, in order to explain our results the price/rate tradeoff would, again, have to be correlated not only with bias but also with lender type by TILA reform.

VII. Conclusion

Our main findings are on two fronts.

First, we provide a tighter microfoundation for disclosure regulation than has previously been articulated in loan markets. Biased perception of borrowing costs may not be the only foundation, but the policy focus on APR disclosure has a clear basis in the strong tendency for consumers to underestimate the interest rate on short-term installment debt when the rate is shrouded.

Second, we show that an easily observed (by researchers) metric of misperceptions about APRs helps explain interest rates on actual loan contracts held in equilibrium, but only when disclosure regulation is weak. This suggests that disclosure regulation has its intended effects when it is enforced.

More generally, our findings provide unique evidence of a link between biased consumer perceptions and market outcomes, and add to the small literature on the link between disclosure, regulation and market outcomes.

While our findings suggest that Truth in Lending disclosure improved market outcomes for some borrowers, we emphasize that our research stops well short of identifying the optimal approach to contemporary disclosure regulation. We do not observe disclosures or their impact on choices directly, and the logic of psychological research on consumer biases suggests that it is critical to evaluate decision making treatments directly, in the contexts of interest.[34] Our data are a bit outdated, and retail financial markets have changed considerably since 1983. The impacts of these changes on firms' ability to exploit consumer bias(es), and the mediating role of disclosure, are unknown.[35]

Nor do our findings provide any motivation for restricting consumer access to even high-interest consumer credit. Consumers may still benefit from borrowing even when they pay too much relative to an unbiased benchmark, especially when a realistic alternative is borrowing from expensive sources that would likely escape regulation (e.g., loan sharks, overdraft protection, rent-to-own, pawn shops) rather than not borrowing at all. The available evidence on the impacts of expensive consumer credit on (consumer) welfare is limited, and mixed (Karlan and Zinman 2007a; Melzer 2007; Morgan and Strain 2007; Morse 2007; Skiba and Tobacman 2007).

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[34] See Bertrand, Karlan, Mullainathan, Shafir, and Zinman (2007) for some related evidence and discussion. Methodologically, the approach in Hossain and Morgan (2006) illustrates the type of study that would be most useful practically in consumer credit markets; they estimate whether unshrouding (shipping charges in this case) impacts consumer decisions and seller profits using field experiments on Yahoo's online auction platform in Taiwan. See also Simmons and Lynch (1991).

[35] On the one hand, the importance of products amenable to payments marketing has expanded considerably, with the growth of 2nd mortgages, auto title loans, payday loans, and refund anticipation loans. Also direct marketing and risk-based pricing have become more sophisticated. The increasing complexity of financial products also makes designing effective disclosure more difficult. On the other hand consumers have greater and cheaper access to decision aids and expert advice.

Instead we hope that the main impact of our findings is a rethinking of the motivation and approach to consumer protection in retail financial markets. In one sense we have clarified the motivation for mandating and improving APR disclosure, by providing a new cognitive microfoundation for why consumer decisions might be distorted when APRs are shrouded. Payment/interest bias has a solid normative basis for being treated (unlike biased preferences), and is easily identifiable (unlike biased expectations). But in another sense our findings highlight a critical limit of disclosure regulation: the cost of enforcement.

Given the incentive problems that are seemingly inherent to implementing effective disclosure regulation, a complementary strategy might be to proactively "debias" consumers. Simple decision rules and decision aids disseminated by more incentive-compatible agents (e.g., nonprofit and government agencies) might be sufficient to improve consumer financial decision making. A decision aid reduced exponential growth bias in laboratory studies (Arnott 2006). Eisenstein and Hoch (2005) show that a quick tutorial on the Rule of 72 improves estimates of future values. In consumer loan markets, doubling the simple interest rate produces a reasonable estimate of the shadow cost of borrowing over a large range of maturities.

But much work remains to be done on identifying the nature and prevalence of cognitive biases that might affect financial decision making, and on designing and testing cost-effective methods for treating them.

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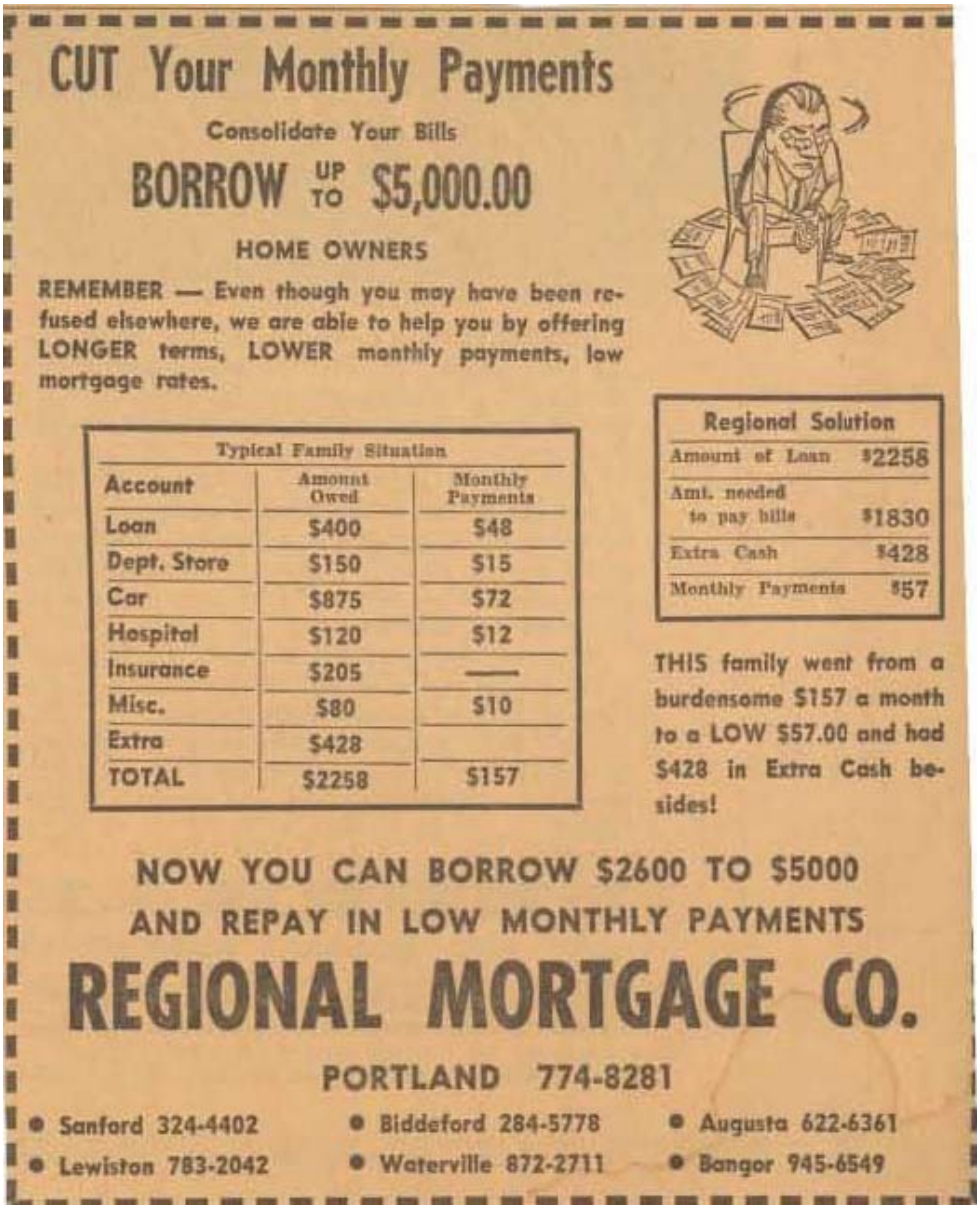


Figure 1. Pre-TILA finance company loan ad emphasizing monthly payments and omitting APR.

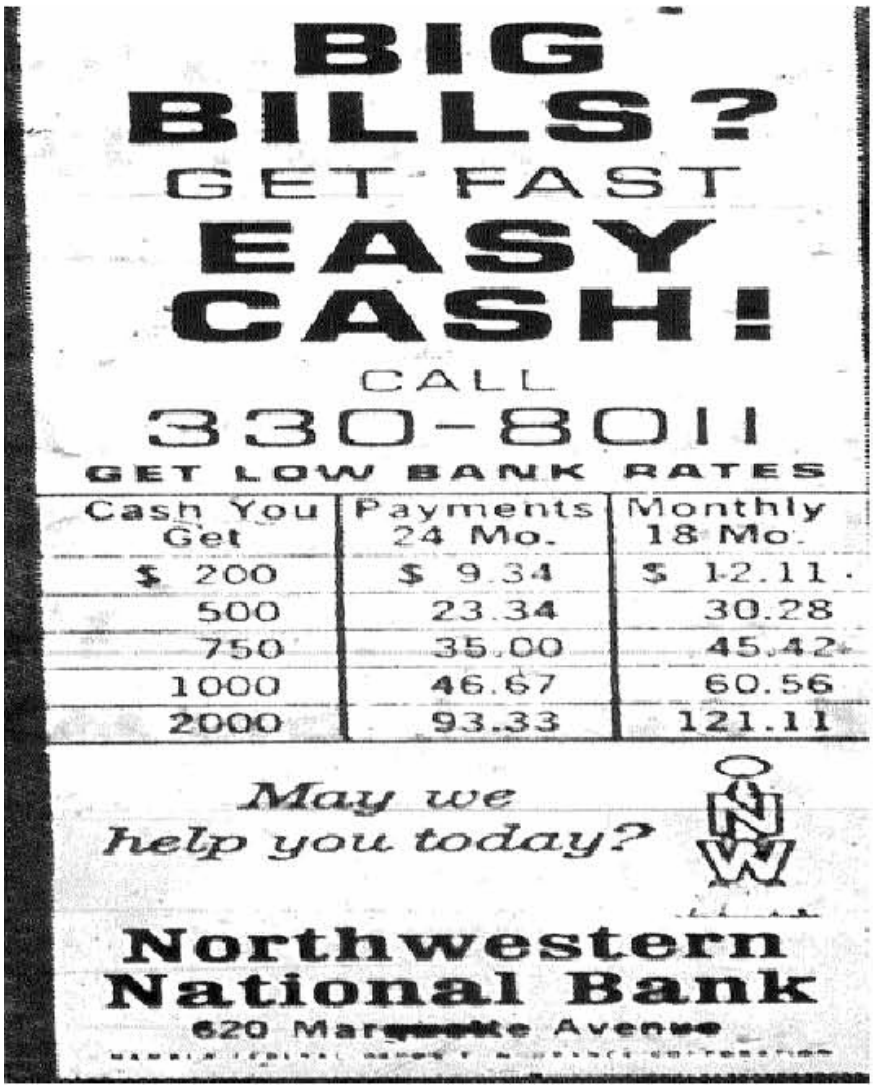
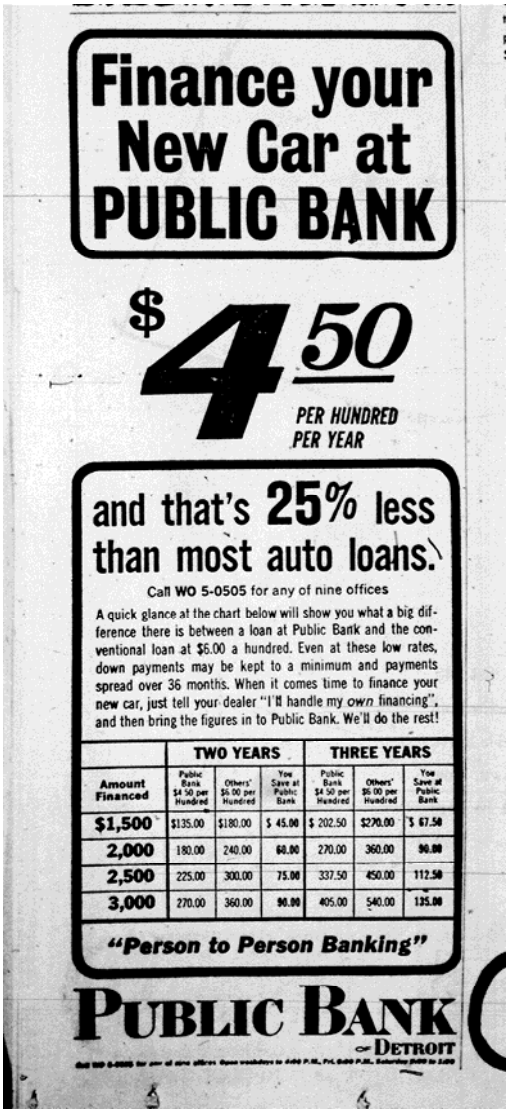


Figure 2. Pre-TILA bank installment loan ad emphasizing monthly payments.

Figure 3. Pre-TILA bank loan ad emphasizing add-on rate.



Finance your New Car at PUBLIC BANK

$4.50

PER HUNDRED PER YEAR

and that's 25% less than most auto loans.

Call WO 5-0505 for any of nine offices

A quick glance at the chart below will show you what a big difference there is between a loan at Public Bank and the conventional loan at $6.00 a hundred. Even at these low rates, down payments may be kept to a minimum and payments spread over 36 months. When it comes time to finance your new car, just tell your dealer "I'll handle my *own* financing", and then bring the figures in to Public Bank. We'll do the rest!

Amount Financed	TWO YEARS			THREE YEARS		
	Public Bank $4.50 per Hundred	Others' $6.00 per Hundred	You Save at Public Bank	Public Bank $4.50 per Hundred	Others' $6.00 per Hundred	You Save at Public Bank
$1,500	$135.00	$180.00	$ 45.00	$ 202.50	$270.00	$ 67.50
2,000	180.00	240.00	60.00	270.00	360.00	90.00
2,500	225.00	300.00	75.00	337.50	450.00	112.50
3,000	270.00	360.00	90.00	405.00	540.00	135.00

"Person to Person Banking"

PUBLIC BANK of DETROIT

Figure 4. Pre-TILA bank loan ad emphasizing add-on rate and monthly payments.

Figures 5a and 5b. Actual and Perceived Rates on Hypothetical Loans in the 1983 SCF

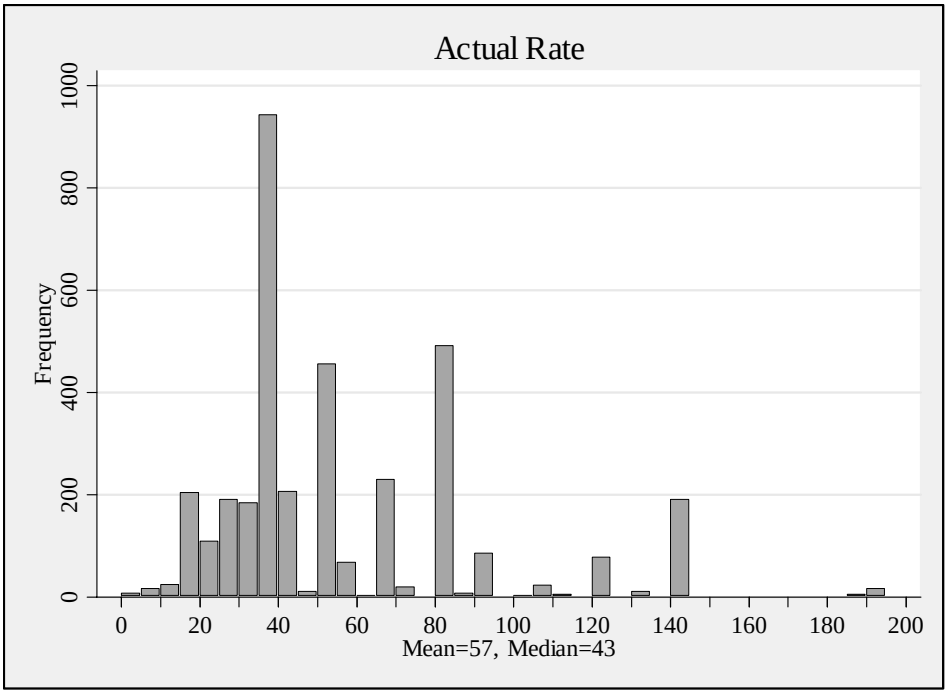


Figure 5a

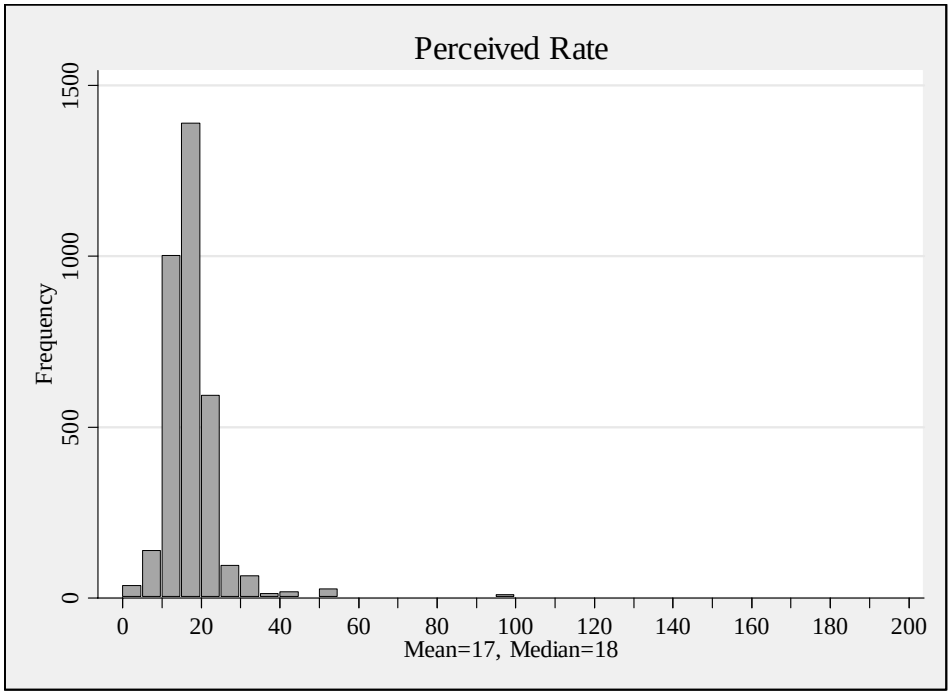


Figure 5b

Notes: “Actual rate” is the APR calculated using the consumer’s self-supplied repayment total on a hypothetical $1000, 12-month installment loan. “Perceived rate” is the rate inferred by the consumer given the same terms.

Figures 6a and 6b. Payment/Interest Bias in the 1983 SCF

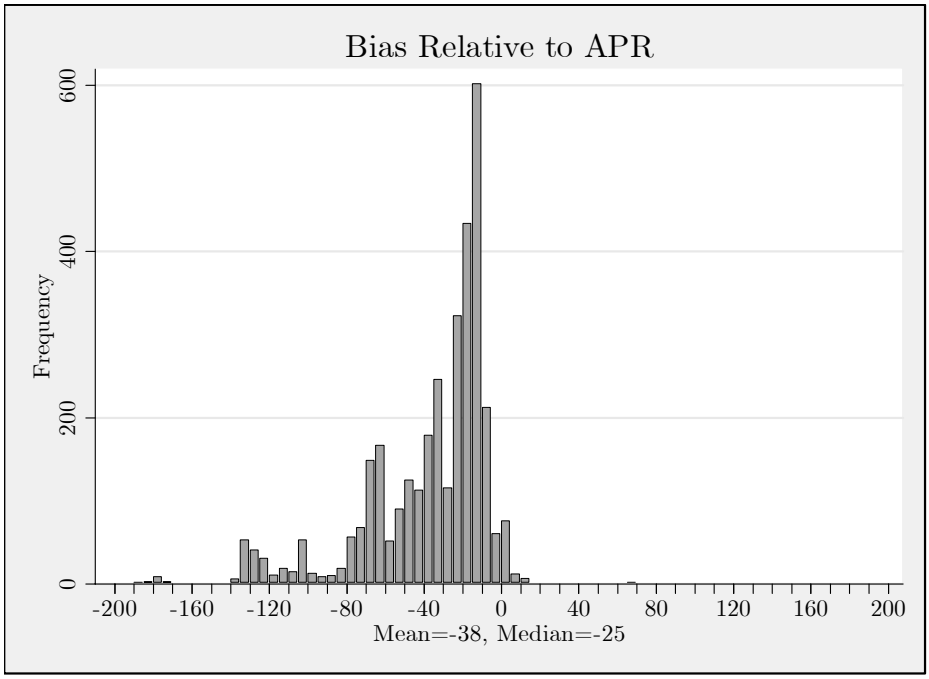


Figure 6a

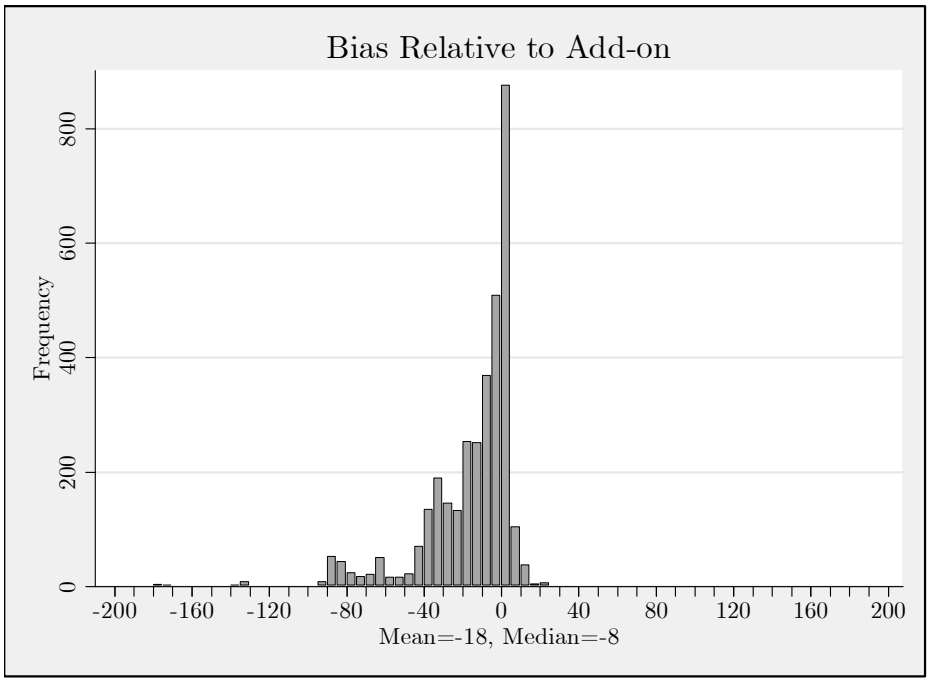


Figure 6b

Notes: Figure 6a shows the distribution of payment/interest bias (the difference between the Perceived and Actual APRs) across households. Figure 6b measures bias as the difference between the Perceived and Add-on rates.

Figure 7. Finance company ad showing different offers (in four lower boxes) quoted as payments and rates.

Figure 8. Post-TILA finance company ad
emphasizing monthly payments and omitting APR (in violation of TILA)

.

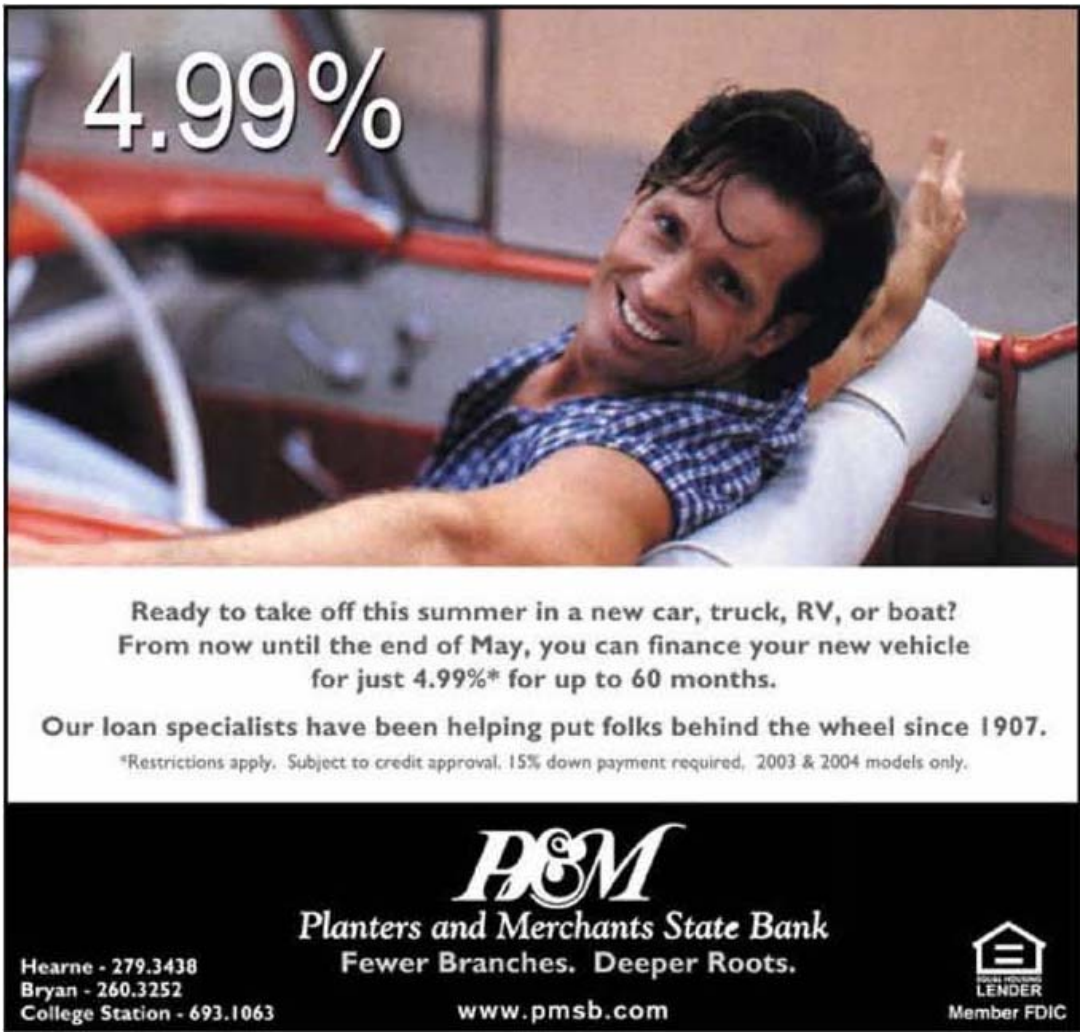


Figure 9. Post-TILA bank ad emphasizing APR.

Table 1. Payment/Interest Bias in the 1983 SCF

	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5	No answer
Stated repayment total (P+I)	1135	1200	1255	1398	1772	1492
Actual APR	24	35	44	66	114	76
Perceived APR	16	18	17	18	15	16
Payment/Interest Bias = Perceived APR - Actual APR	-8	-16	-27	-48	-99	–
Share supplying add-on rate	0.58	0.42	0.09	0.02	0	–
Range of bias in quintile	[-100, 14]	[14, 20]	[20, 33]	[33, 63]	[63, 290]	–
Number of households	698	713	662	729	612	689

Notes: Sample includes all households in the 1983 SCF. Rates and bias are in hundreds of basis points. Payment, APR and bias measures are means by quintile. Quintiles are by bias relative to APR. “No answer” bin includes households who fail to supply either a repayment total or a perceived APR, or report neither. Observations per quintile differ due to clustered values of bias.

Table 2. Payment/Interest Bias and Selected Household Characteristics

	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5	No answer
No HS education	0.08	0.08	0.14	0.19	0.27	0.49
HS degree	0.22	0.27	0.37	0.35	0.34	0.28
Some college	0.19	0.24	0.24	0.23	0.2	0.14
College degree	0.51	0.41	0.25	0.24	0.19	0.1
Income, median ($)	39170	35000	25000	25350	20000	14000
Total assets, median ($)	89900	65025	38100	38336	26600	20135
Total debt, median ($)	24825	24220	13465	10802	8106	3782
Homeowner	0.76	0.74	0.66	0.65	0.62	0.66
Mortgage holder	0.57	0.61	0.53	0.48	0.45	0.38
Age	45	42	40	40	40	47
Male household head	0.91	0.90	0.85	0.82	0.75	0.71
Married	0.8	0.79	0.73	0.73	0.69	0.64
Household size	3.05	3.09	3.08	3.11	3.19	3.13
White	0.91	0.91	0.87	0.84	0.80	0.62
African-American	0.05	0.05	0.10	0.12	0.16	0.31
Hispanic	0.01	0.03	0.02	0.03	0.02	0.06
Asian/Native American	0.03	0.01	0.01	0.01	0.02	0.01
Employed	0.81	0.84	0.82	0.81	0.74	0.59
Years in current job	5.97	5.83	5.30	5.24	4.72	4.20
Self-employed	0.34	0.31	0.28	0.24	0.17	0.14
Spouse employed	0.42	0.49	0.45	0.47	0.4	0.34
Health: ``Excellent''	0.52	0.52	0.50	0.41	0.43	0.30
Health: ``Good''	0.4	0.39	0.37	0.40	0.40	0.35
Health: ``Fair''	0.06	0.08	0.10	0.15	0.11	0.22
Health: ``Poor''	0.02	0.01	0.03	0.04	0.06	0.13
Spouse's health: ``Excellent''	0.43	0.43	0.36	0.31	0.29	0.19
Spouse's health: ``Good''	0.29	0.28	0.26	0.31	0.26	0.25
Spouse's health: ``Fair''	0.06	0.06	0.08	0.09	0.09	0.14
Spouse's health: ``Poor''	0.01	0.02	0.03	0.02	0.06	0.05
Recently denied credit	0.13	0.19	0.19	0.20	0.25	0.22
Recent late debt payment	0.15	0.20	0.21	0.23	0.22	0.26
Shops on payments	0.46	0.56	0.59	0.62	0.64	0.60
Has a credit card	0.88	0.86	0.76	0.69	0.61	0.48
Has an ATM card	0.28	0.26	0.25	0.24	0.19	0.10
Takes substantial financial risks	0.10	0.05	0.06	0.08	0.06	0.04
Takes > average financial risks	0.21	0.17	0.14	0.13	0.10	0.06
Takes average financial risks	0.39	0.48	0.43	0.40	0.34	0.24
Not willing to take any financial risks	0.30	0.30	0.37	0.39	0.50	0.65
Thinks buying on credit is good idea	0.46	0.50	0.47	0.47	0.44	0.36
Thinks buying on credit is good and bad	0.32	0.31	0.32	0.31	0.31	0.28
Thinks buying on credit is bad idea	0.22	0.19	0.20	0.22	0.25	0.32
Will tie up money long-run for substantial returns	0.16	0.17	0.12	0.15	0.11	0.09
Will tie up money med. run for > average returns	0.39	0.34	0.29	0.28	0.24	0.13
Will tie up money short-run for average returns	0.26	0.30	0.33	0.33	0.29	0.24
Will not tie up money at all	0.18	0.17	0.23	0.25	0.36	0.46
Uses external financial advice	0.50	0.57	0.56	0.58	0.52	0.43

Notes: Sample includes households with any installment debt in the 1983 SCF. Values are averages across households. Regressions shown in Table 5 use deciles for wage income, assets, and debt rather than the levels shown above, and also includes age squared. Not shown, but also included as household-level covariates in the cross-section: industry (14 categories), occupation (8 categories), pension income (10 categories), beliefs about inheritance, job tenure and pension income (34 categories), state of residence fixed effects.

Table 3. Bias, borrowing and loan interest rates by bias quintile

	Payment/Interest Bias					
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5	No answer
Households with loans	329	391	323	385	305	196
Total loans	534	697	528	613	448	282
Loans per household	1.62	1.78	1.63	1.59	1.47	1.44
Loan size ($, mean)	37785	27648	12725	6244	6458	5454
Loan size ($, median)	4721	4050	3192	3046	2502	2024
Loan maturity (months, mean)	39	42	40	40	41	35
Loan maturity (months, median)	36	36	36	36	36	30
Average loan interest rate:						
Bank, pre-TILA reform	14.3	14.1	15.9	12.9	16.5	15.9
Finco, pre-TILA reform	17.3	16.7	17.4	18.1	17.6	16.7
Bank, post-TILA reform	14.2	15.5	15.3	15.4	16.3	16.7
Finco, post-TILA reform	17.5	19.6	19.4	20.0	19.4	20.0
Share of loans from finance companies	0.20	0.24	0.27	0.28	0.36	0.40

Notes: Loans per household are average over households in quintile. Loan size, maturity and interest rates are averages over loans in quintile. Rates are in percentage points. See Table A1 for summary data on loan purpose and year of origination.

Table 4. Disclosure Regulation, Payment/Interest Bias and Loan Rates with household fixed effects

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Finance Company (Finco)*New TILA	-3.88**	-0.62	2.15	4.96*	-0.51		
	(1.85)	(0.92)	(2.99)	(2.58)	(1.07)		
Bias Q2*Finco*New TILA	4.46*	2.61**	4.74*	2.84**	3.31**		
	(2.54)	(1.15)	(2.52)	(1.15)	(1.36)		
Bias Q3*Finco*New TILA	2.03	2.19*	1.87	2.15*	2.55*		
	(2.49)	(1.24)	(2.48)	(1.24)	(1.47)		
Bias Q4*Finco*New TILA	4.81**	3.30***	5.13**	3.54***	5.72***		
	(2.38)	(1.20)	(2.38)	(1.22)	(1.51)		
Bias Q5*Finco*New TILA	3.52	2.04	3.18	1.73	1.11		
	(2.52)	(1.31)	(2.52)	(1.33)	(1.71)		
No Response*Finco*New TILA	8.12***	3.92**	7.49***	3.59**	3.89**		
	(2.89)	(1.52)	(2.89)	(1.54)	(1.91)		
Finco	3.66**		3.81**			0.52	
	(1.70)		(1.69)			(0.88)	
Bias Q2*Finco	-2.48		-2.55			1.34	
	(2.29)		(2.28)			(1.08)	
Bias Q3*Finco	0.15		0.21			1.89	
	(2.22)		(2.21)			(1.16)	
Bias Q4*Finco	-0.92		-1.08			2.76**	
	(2.18)		(2.17)			(1.17)	
Bias Q5*Finco	-1.57		-1.55			1.47	
	(2.25)		(2.24)			(1.26)	
No Response*Finco	-5.22**		-5.06*			1.24	
	(2.59)		(2.58)			(1.44)	
Bias Q2*New TILA	0.86		0.88				1.65*
	(0.99)		(0.98)				(0.92)
Bias Q3*New TILA	-0.10		-0.05				0.37
	(1.11)		(1.11)				(1.01)
Bias Q4*New TILA	-0.79		-0.78				0.28
	(1.06)		(1.05)				(0.95)
Bias Q5*New TILA	0.22		0.12				0.75
	(1.31)		(1.30)				(1.09)
No Reponse*New TILA	0.72		0.93				2.63*
	(0.19)		(0.19)				(0.22)
N	3094	3094	3094	3094	2061	3094	3094
R-squared (within)	0.48	0.46	0.48	0.47	0.53	0.47	0.45
Household fixed effects	yes (0.00)	yes (0.00)	yes (0.00)	yes (0.00)	yes (0.00)	yes (0.00)	yes (0.00)
Loan amount, product dummies	yes (0.00)	yes (0.00)	yes (0.00)	yes (0.00)	yes (0.00)	yes (0.00)	yes (0.00)
Loan year of origination dummies	yes (0.35)	yes (0.16)	yes (0.46)	yes (0.22)	yes (0.60)	yes (0.17)	yes (0.20)
Loan maturity dummies	yes (0.00)	yes (0.00)	yes (0.00)	yes (0.00)	yes (0.00)	yes (0.00)	yes (0.00)
Finco*New TILA*Bias interactions	no	no	yes (0.02)	yes (0.02)	no	no	no
Finco*New TILA*Bias effects=0	0.08	0.07	0.09	0.06	0.01	n/a	n/a
Finco*Bias effects=0	0.29	n/a	0.31	n/a	n/a	0.32	n/a
New TILA*Bias effects=0	0.70	n/a	0.67	n/a	n/a	n/a	0.25
Model 2/4 vs. model 1/3	0.33		0.33		n/a	n/a	n/a
Only loans held by unrationed HHs?	no	no	no	no	yes	no	no

Notes: Dependent variable is level interest rate on a consumer installment loan. Right-hand side variables include those listed, household fixed effects, and loan-specific covariates listed in rows below the r-squared. "Yes" indicates that the set of controls was included in the model, and the value in parentheses is the p-value for the exclusion restriction on that set of covariates. Models (3) and (4) interact the triple-difference term with indicators for recent credit denial and recent late loan payment, as well as ln(loan amount). Model (5) uses only the subsample of loans held by households with no recent credit denial or late loan payment.

Table 5. Payment/Interest Bias, Disclosure and Loan Interest Rates in the Cross-sectio

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Finco*New TILA	-2.42	-0.34				-0.46	-0.04
	(1.67)	(0.61)				(0.77)	(1.09)
Bias Q2*Finco*New TILA	3.39*	2.35***				2.28**	2.04
	(2.04)	(0.78)				(1.05)	(1.35)
Bias Q3*Finco*New TILA	1.92	2.06***				2.44***	1.43
	(1.91)	(0.72)				(0.89)	(1.26)
Bias Q4*Finco*New TILA	2.09	2.27***				3.33***	3.19**
	(1.94)	(0.75)				(0.98)	(1.31)
Bias Q5*Finco*New TILA	2.05	1.80**				1.88*	2.63*
	(1.94)	(0.82)				(1.06)	(1.58)
No Response*Finco*New TILA	3.20	2.25***				2.50**	4.34**
	(2.07)	(0.85)				(1.06)	(2.15)
Finance Company (Finco)	2.20		0.38				
	(1.51)		(0.58)				
Bias Q2*Finco	-1.30		1.63**				
	(1.77)		(0.71)				
Bias Q3*Finco	0.64		1.63**				
	(1.70)		(0.67)				
Bias Q4*Finco	0.42		1.91***				
	(1.74)		(0.70)				
Bias Q5*Finco	-0.55		1.25*				
	(1.67)		(0.72)				
No Response*Finco	-1.18		1.18				
	(1.77)		(0.76)				
Bias Q2*New TILA	-0.04			0.84**			
	(0.70)			(0.39)			
Bias Q3*New TILA	0.21			0.22			
	(0.78)			(0.42)			
Bias Q4*New TILA	-0.06			0.52			
	(0.73)			(0.40)			
Bias Q5*New TILA	0.61			0.78*			
	(0.78)			(0.43)			
No Reponse*New TILA	1.21			0.91*			
	(0.94)			(0.49)			
Bias Q2	0.31				0.57*		
	(0.51)				(0.31)		
Bias Q3	-0.70				-0.09		
	(0.62)				(0.35)		
Bias Q4	-0.15				0.31		
	(0.56)				(0.32)		
Bias Q5	-0.28				0.37		
	(0.61)				(0.35)		
No Quiz Answer	-0.99				0.07		
	(0.73)				(0.41)		
N	2973	2973	2973	2973	2973	1970	687
R-squared	0.53	0.52	0.52	0.51	0.51	0.58	0.64
Finco*New TILA*Bias effects=0	0.61	0.03	n/a	n/a	n/a	0.03	0.14
Finco*Bias effects=0	0.55	n/a	0.12	n/a	n/a	n/a	n/a
New TILA*Bias effects=0	0.70	n/a	n/a	0.19	n/a	n/a	n/a
Bias effects=0	0.42	n/a	n/a	n/a	0.29	n/a	n/a
Model 2 vs. Model 1	0.29		n/a	n/a	0.30	n/a	n/a
Only loans held by unrationed HHs?	no	no	no	no	no	yes	no
Only loans identifying trip-diff within HH	no	no	no	no	no	no	yes

Notes: Dependent variable is level interest rate on a consumer installment loan. Models (1)-(5) use the sample of loans from the 1983 SCF for which all covariates are observed. Model (6) uses only the sample of loans held by households not facing credit constraints. Model (7) uses only the sample of loans that identifies the triple-difference effects in Table 4, Model (1). Standard errors are clustered by household. Rows below r-squared report p-values for F-tests of exclusion restrictions for sets of dummy variables listed. Covariates are those listed in the row headings, plus the full set of loan-specific controls described in Table 3 and Table A1, plus the full set of household-level controls described in Table 2 and the notes thereto.

Table 6. Effects of Payment/Interest Bias for Typical Loans in the Sample

Variable	Home Imp.	New Car	Used Car	Durable
Monthly payment (unbiased)	$88	$164	$103	$62
Monthly payment (high bias)	97	176	108	64
Difference in monthly payment	9	12	6	2
Increased interest, life of loan	485	590	213	34
Implied loan increase	347	452	175	33
Implied loan increase (%)	10%	8%	6%	3%
Implied loan increase as % of income	1.39%	1.81%	0.70%	0.13%

Notes: 1983 dollars. Typical loans have sample median amount borrowed and maturity. Median home improvement loan is $3800 repaid over 60 months. Median new car loan is $6000 repaid over 48 months (for comparison's sake, the median new car loan in the 2004 SCF was $23,000 repaid over 60 months). Median used car loan is $3000 repaid over 36 months. Median household durable loan is $1000 repaid over 18 months. Less-biased rate is 14% APR, more-biased rate is 18%. Implied loan increase is the increase in amount borrowed at 14% that renders monthly payments equal to those at 18%. For the last row we assume that more-biased households have total income of $25,000; see Table 2.

Table A1. Counts of Loan Purpose (Product) and Year of Origination by Bias Quintile

	Payment/Interest Bias						
	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5	No answer	Total
Loan Purpose:							
Primary home purchase	24	25	18	18	11	5	101
Mobile home purch	1	2	4	2	4	2	15
Home improvement/maint.	31	62	23	52	45	26	239
New vehicle purchase	113	136	87	114	66	31	547
Used vehicle purchase	108	152	146	154	123	81	764
Durables including furniture	44	76	80	75	79	64	418
Rec equip, boats	12	17	17	19	15	4	84
Other real estate	16	14	5	5	3	3	46
Other investments	56	58	27	21	9	7	178
Travel/vacation	3	9	4	2	2	3	23
Medical/dental	16	21	27	48	34	12	158
Education	63	70	48	60	33	17	291
Living expenses, other event	47	49	41	42	24	27	230
Total	534	691	527	612	448	282	3,094
Year of origination:							
1978 or earlier	81	90	62	74	40	31	378
1979	43	53	37	41	25	21	220
1980	62	83	71	86	63	36	401
1981	118	164	107	118	79	47	633
1982	159	221	174	218	186	104	1,062
1983	71	86	77	76	55	43	408
Total	534	697	528	613	448	282	3,102

Notes: These variables are included as controls, along with maturity categories and log loan size (see Table 3), in the empirical models used in Tables 4 and 5.

Table A2. Alternative Functional Forms of Bias

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Biased*Finco*New TILA	2.14***	2.71***						
	(0.44)	(0.98)						
ln(Bias)*Finco*New TILA			0.45***	0.75				
			(0.13)	(0.51)				
Bias*Finco*New TILA					0.03***	0.01	0.06***	0.05
					(0.01)	(0.01)	(0.02)	(0.04)
Bias squared*Finco*New TILA							-0.00*	-0.00
							(0.00)	(0.00)
Finco*New TILA		-0.61		-1.04		0.98		0.35
		(0.92)		(1.73)		(0.61)		(0.89)
N	3094	3094	2739	2739	2812	2812	2812	2812
R-squared (within)	0.46	0.46	0.46	0.46	0.46	0.46	0.46	0.46

Notes: Each model is a variant of the fixed effect specification from Table 4, Column (1) using a different functional form for bias. Models (1) and (2) use a binomial indicator in which ``biased'' encompasses quintiles 2-5 and ``no answer.'' Models (3) and (4) use ln(bias), which drops observations for which bias is less than or equal to zero. Models (5) and (6) use the level of bias, and Models (7) and (8) also include the squared level of bias. The latter six models drop ``no answer'' observations for which the level of bias is unmeasured.

Table A3. Payment/Interest Bias and Borrowing from Finance Companies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bias Q2	0.02	0.00	-0.01	-0.01		0.12	0.13	0.14
	(0.03)	(0.03)	(0.03)	(0.03)		(0.11)	(0.12)	(0.10)
Bias Q3	0.09**	0.03	-0.04	-0.03		-0.10	-0.04	-0.10
	(0.04)	(0.04)	(0.04)	(0.04)		(0.12)	(0.13)	(0.10)
Bias Q4	0.10***	0.06*	0.00	0.03		-0.06	-0.02	-0.06
	(0.04)	(0.04)	(0.04)	(0.04)		(0.12)	(0.12)	(0.10)
Bias Q5	0.22***	0.10**	0.08*	0.05		0.04	-0.04	0.08
	(0.05)	(0.04)	(0.05)	(0.05)		(0.13)	(0.13)	(0.11)
No Quiz Answer	0.20***	0.07	0.02	-0.01		0.02	-0.05	-0.03
	(0.06)	(0.05)	(0.06)	(0.06)		(0.15)	(0.16)	(0.12)
New TILA	0.14***		0.09***					
	(0.03)		(0.03)					
Bias Q2*New TILA	0.03	0.00	0.03	0.00	-0.03			
	(0.05)	(0.04)	(0.05)	(0.05)	(0.07)			
Bias Q3*New TILA	-0.03	-0.03	0.04	0.01	0.01			
	(0.05)	(0.05)	(0.05)	(0.05)	(0.08)			
Bias Q4*New TILA	-0.03	-0.06	0.01	-0.03	-0.10			
	(0.05)	(0.05)	(0.05)	(0.05)	(0.08)			
Bias Q5*New TILA	-0.10*	-0.05	-0.06	-0.03	-0.10			
	(0.06)	(0.05)	(0.06)	(0.06)	(0.09)			
No Reponse*New TILA	-0.02	-0.00	-0.02	0.03	0.07			
	(0.07)	(0.06)	(0.07)	(0.07)	(0.11)			
N	3102	3094	2973	2973	3094	1847	1849	4103
Loan characteristics	No	Yes	No	Yes	Yes	n/a	n/a	n/a
Household covariates	No	No	Yes	Yes	No	Yes	Yes	Yes
Household fixed effects	No	No	No	No	Yes	n/a	n/a	n/a

Notes: (1)-(5) are linear probability models using the loan as an observation. The dependent variable is equal to one if the loan is from a finance company. Model (1) includes only the covariates shown (and a constant term). Model (2) includes the covariates shown as well as loan-specific characteristics (year, product, amount, maturity). Model (3) omits loan-specific characteristics and includes the full set of household-specific covariates used in Table 5. Model (4) includes loan-specific characteristics and household characteristics. Model (5) includes loan characteristics and household fixed effects.

Models (6) and (7) are probit models estimated at the household level, with a dependent variable equal to one if the household has any loans from a finance company (6) or any post-TILA reform loans from a finance company (7), using the sample of households with at least one loan. Model (8) is a probit estimated at the household level, with a dependent variable equal to one if the household has any loans from a finance company, using the entire SCF sample. Models (1)-(5) cluster standard errors at the household level. Note that the New TILA level coefficient is identified only when loan characteristics (which include year of origination) are omitted.