

Fuzzy Math, Disclosure Regulation and Credit Market Outcomes:  
Evidence from Truth-in-Lending Reform\*

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ABSTRACT

Consumer installment lenders prefer to market “low monthly payments” and shroud interest rates. Why not voluntarily disclose rates? We show that when an interest rate is not disclosed, most consumers substantially underestimate it using information on the monthly payment, loan principal and maturity. This “fuzzy math” or “payment/interest bias” helps explain why lenders shroud rates and thereby violate the Truth-in-Lending Act (TILA) even under the threat of fines and litigation. So does TILA have any teeth? We identify within-household interactions between policy-induced variation in the strength of TILA enforcement across lenders and time, payment/interest bias, and interest rates on actual loans. More biased households pay roughly 400 basis points more than less-biased households, but only on loans from lenders facing relatively weak TILA enforcement. TILA compliance seems costly for lenders, leaving the overall welfare effects of mandated disclosure ambiguous. The results link a cognitive bias to firm strategy and market outcomes, and show that mandated disclosure can attenuate those links.

Other keywords: household finance, disclosure regulation, consumer protection, price discrimination, consumer credit, psychology and economics, behavioral economics, behavioral industrial organization, cognitive bias, exponential growth bias

JEL codes: D01, D03, D1, D4, G2, K2, K42, L5

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Consumer installment lenders prefer to market “low monthly payments” and shroud interest rates. Why not voluntarily disclose rates? We show that when an interest rate is not disclosed, most consumers substantially underestimate it using information on the monthly payment, loan principal and maturity. This “fuzzy math” or “payment/interest bias” helps explain why lenders shroud rates and thereby violate the Truth-in-Lending Act (TILA) even under the threat of fines and litigation. So does TILA have any teeth? We identify within-household interactions between policy-induced variation in the strength of TILA enforcement across lenders and time, payment/interest bias, and interest rates on actual loans. More biased households pay roughly 400 basis points more than less-biased households, but only on loans from lenders facing relatively weak TILA enforcement. TILA compliance seems costly for lenders, leaving the overall welfare effects of mandated disclosure ambiguous. The results link a cognitive bias to firm strategy and market outcomes, and show that mandated disclosure can attenuate those links.

“Respondent...in numerous instances... 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).<sup>1</sup> The focus on APR disclosure is a direct attempt to counter pre-TILA lender practices. Prior to TILA, lenders typically marketed “low monthly payments” and either shrouded interest rates or presented alternatively defined rates that are nominally lower than APRs.<sup>2</sup> Many consumer lenders continue these practices under TILA despite the threat of fines and litigation.<sup>3</sup> The scope, content and enforcement of mandated price disclosure is central to the design of sound public policy in credit markets (e.g., Kroszner 2007).

We provide an explanation for lenders' desire to shroud APRs and emphasize payments: most people underestimate APRs when shown other loan terms. This “payment/interest bias” is pervasive and varies dramatically across individuals, who seem to be only partly aware of its effects. The bias means that consumers do not necessarily infer that shrouded prices are high prices, as in many search cost models (Salop and Stiglitz 1977). Instead, some consumers may infer a relatively low price when the interest rate is shrouded (Gabaix and Laibson 2006). Biased consumers may be more likely to take a particular shrouded loan offer instead of searching for a better price, self-financing, or forgoing current consumption.<sup>4</sup> That gives lenders incentives to price discriminate based on payment/interest bias, by shrouding APRs and emphasizing low monthly payments.

In principle, the disclosure mandated by TILA can counteract lenders’ ability to price discriminate on payment/interest bias. Our focus on payment/interest bias as a potential motive for TILA is new: prior work has focused on improving limited consumer awareness of rates, on facilitating comparisons across

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<sup>1</sup> 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.”

<sup>2</sup> Pre-TILA lender marketing practices are documented in National Commission on Consumer Finance (1972), Rubin (1991), and the references in those studies. 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.

<sup>3</sup> See, e.g., FTC Annual Reports, General Accounting Office (2004), and Fox and Guy (2005).

<sup>4</sup> Stango and Zinman (forthcoming) links payment/interest bias to a more fundamental bias (underestimation of exponential growth), provides evidence on limited awareness of the bias, and also explores how exponential growth bias affects long-term saving and wealth accumulation. The focus of that paper is different, in that it examines how *cross-sectional* variation in bias correlates with a wide range of household financial outcomes. The current study examines *within*-household changes in loan contract terms as the institutional environment (TILA) changes.

loans with different maturities, and on correcting presumably unbiased *mistakes* in how consumers make interest rate calculations.<sup>5</sup>

Because TILA enforcement is limited in practice, we can test whether TILA affects market outcomes by estimating *within*-household-level interactions between policy-induced variation in TILA enforcement, payment/interest bias, and APRs on actual installment loans.<sup>6</sup> Our data contain a household-level measure of payment/interest bias, which we parameterize using a “more biased” dummy variable  $MoreBias_h$  measuring greater vulnerability to loan marketing that obscures APRs. The data also contain detailed information about the full set of installment loans held by each household, including information on the originating lender type. Lender type matters because lenders face differential TILA enforcement. *Public* enforcement (by regulators) was weaker for non-bank finance companies than for banks throughout our sample period. *Private* enforcement through civil courts was strong for both lender types until a major reform to TILA in 1981 substantially weakened civil penalties. Because finance companies faced less active enforcement after TILA reform while banks still faced regular scrutiny, TILA reform reduced compliance incentives for finance companies relative to banks.

The data therefore permit triple-difference estimates of the within-household relationship between loan APRs, payment/interest bias and TILA enforcement across lender type and over time. We have observations for households with loans from both banks and finance companies, obtained both before and after the 1981 TILA reform. Household fixed effects control for unobserved household-level heterogeneity correlated with payment/interest bias and loan APRs. Detailed loan characteristics (amount, maturity, product purchased, year of origination) control for loan- and time-specific heterogeneity. A dummy variable indicating whether the loan is from a bank or finance company ( $Finco_l$ ) controls for unobserved heterogeneity leading to bank vs. finance company rate differences. A dummy variable indicating whether the loan was originated pre or post-TILA reform ( $NewTILA_t$ ) controls for other factors associated with rate differences over time. A full set of double interactions ( $MoreBiased_h Finco_l$ ,  $MoreBiased_h NewTILA_t$  and  $Finco_l NewTILA_t$ ) controls for other unobserved heterogeneity, such as whether finance companies price risk differently than banks, or whether TILA reform shifted the bank vs. finance company rate gap for other reasons. Conditional on all of these other factors, the triple-difference coefficient  $MoreBiased_h Finco_l NewTILA_t$  measures whether changes in TILA enforcement affect the relationship between bias and APRs in equilibrium.

The results suggest that TILA reform shifts the relationship between bias and APRs in an economically meaningful way. Before TILA reform, we cannot reject the hypothesis that the relationship

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<sup>5</sup> See, e.g., Mandell (1971; 1973), Day and Brandt (1974), and Durkin (1975).

<sup>6</sup> We focus on loans that finance consumer durables (primarily cars and appliances), because longer-maturity loans (e.g., most mortgages and lines of credit) are less prone to payment/interest bias (Stango and Zinman forthcoming).

between bias and interest rates is identical at all lenders.<sup>7</sup> After TILA reform, the more biased vs. less biased rate difference increases substantially, *but only on loans from lenders (finance companies) facing newly weakened enforcement*. The increase in the more biased vs. less biased rate difference is roughly 400 basis points, which translates into a consumption cost of about 6-8% of the typical car loan amount.

Although our triple-difference result suggests that mandated APR disclosure affects market outcomes among precisely those consumers who are most vulnerable to loan marketing that obscures APRs, mandated disclosure does not necessarily *improve* outcomes for more-biased borrowers on net. Weaker TILA enforcement is correlated with a general downward shift in finance company APRs, and the net effect ends up being lower APRs for less-biased borrowers and roughly equal APRs for more-biased borrowers (relative to what more-biased borrowers paid at finance companies before TILA reform). This pattern helps explain another fact about TILA reform: after reform the share of household loans from finance companies rises sharply, and rises by relatively more for less-biased households relative to more-biased households.

The full picture of our results suggests that relaxing disclosure constraints allows price discrimination on payment/interest bias, but also shifts the finance company supply curve to the right. This squares with prior evidence that TILA enforcement and compliance costs are substantial (Angell 1971; Elliehausen and Kurtz 1988). The lesson for policy is that any benefits of mandated APR disclosure may be offset by its costs.

Given this caveat, our research stops well short of identifying the optimal approach to contemporary disclosure regulation. We do not undertake a full cost-benefit analysis of TILA reform, and indeed examine only a subset of effects in one particular loan market. 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.<sup>8</sup> Moreover, retail financial markets have changed considerably since the years surrounding TILA reform in 1981. The impacts of those changes on firms' ability to exploit consumer bias(es), and the mediating role of disclosure, are unknown.<sup>9</sup>

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<sup>7</sup> Because our specification includes fixed household effects, the cross-household relationship between bias and rates (that is constant across lender type) is not identified.

<sup>8</sup> See Bertrand, Karlan, Mullainathan, Shafir, and Zinman (forthcoming) for some related evidence and discussion. Methodologically, the approach in Brown, Hossain and Morgan (2007) illustrates the type of study that would be most useful in consumer credit markets; they estimate whether unshrouding shipping charges affects consumer decisions and seller profits using experiments on online auction platforms. See also Simmons and Lynch (1991).

<sup>9</sup> On one hand, the importance of products amenable to payments marketing has expanded considerably, with the growth of 2<sup>nd</sup> mortgages, auto title loans, payday loans, and refund anticipation loans. 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.

Nevertheless our setup and findings are novel in several respects. We provide evidence on *whether* price disclosure affects market outcomes; the existing literature has focused on quality disclosure.<sup>10</sup> The evidence on payment/interest bias also provides some insight into *why* disclosure matters. As such our results help interpret stylized facts that may seem puzzling when viewed through the lens of standard models of disclosure.<sup>11</sup> Our work also adds to the literature on how cognitive biases affect market outcomes. Despite renewed theoretical and policy interest in the subject (e.g., DellaVigna and Malmendier 2004; Campbell 2006; Ellison 2006), few other studies have tested whether a consumer-level measure of cognitive bias is correlated with a consumer-level measure of financial contracts held in equilibrium.<sup>12</sup> Finally, our results highlight the importance of enforcement costs in mediating the effects of disclosure regulation. As we discuss in the conclusion, the challenge of enforcing TILA motivates consideration of related, but lower-cost, “treatments” for biased perceptions of borrowing costs.

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

In this section we briefly describe our data, consumer loan markets, and Truth-in-Lending law. We then define payment/interest bias, present our empirical evidence on its existence, and discuss how lenders might price discriminate based on bias even in competitive loan markets.

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<sup>10</sup> On the effects of consumer product quality disclosure see, e.g., Mathios (2000), Jin and Leslie (2003), and Hastings and Weinstein (forthcoming). Kroszner (2007) 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, or on price disclosure more generally. Shaffer (1999) argues that mandated disclosures did not change equilibrium interest rates for credit cards. We do not examine credit cards because they were not very prominent during our study period (the late 1970s and 1980s), and because payment/interest bias should not distort perceptions of interest rates on revolving credit lines.

<sup>11</sup> Models following Nelson (1970) often emphasize product over price disclosure, on the assumption that searching for information on price is less costly than for quality. This seems to be an accurate description for most markets, but may not be (effectively) true in loan markets if payment/interest bias increases search costs, and/or if consumers who are unaware of their bias mistakenly believe that they do not need to search for price information (because they think that they can correctly infer it from other loan terms). Models following Salop and Stiglitz (1977) predict voluntary disclosure if the marginal cost of disclosing is low, but that prediction is driven by the assumption that consumers infer that shrouded prices are high prices.

<sup>12</sup> DellaVigna (forthcoming) provides a review of field evidence in “behavioral industrial organization.” 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* (see also Stango and Zinman (forthcoming) for cross-sectional correlations between payment/interest bias and portfolio choices). Ashraf, Karlan, and Yin (2006) and Meier and Sprenger (2008) link present-biased preferences to contract choice. 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 in part on presumed preference biases. 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 contracts.

### *A. Data from the 1983 Survey of Consumer Finances*

Our data come from the 1983 Survey of Consumer Finances (SCF).<sup>13</sup> This is the only dataset we know of with information on both payment/interest bias and the terms of loans originated before and after the Truth in Lending Act (TILA) reform of 1981.<sup>14</sup>

### *B. Consumer Installment Loan Markets*

Our empirics examine installment loans financing the purchase of household durable goods: these are also called closed-end loans. Closed-end loans have fixed repayment schedules, in contrast to open-end or revolving loans such as credit cards. Roughly 60% of the loans in our sample fund new or used car purchases. The remainder fund purchases of furniture, appliances, entertainment equipment, education or home improvement (Table 1).

Installment loans comprise a large share of household liabilities, both during our study period and today. In 1983 households owed \$325 billion in installment debt, an amount that dwarfed revolving debt. Even today, installment debt exceeds other types of discretionary borrowing; short-term installment debt outstanding in 2007 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 installment loans for three reasons. First, as noted above, installment credit was the most important source of discretionary borrowing during our sample period. Second, our prior work shows that the effect of payment/interest bias on consumers' perceptions of APRs is only economically relevant on loans with maturities less than 10 years. Nearly all installment loans have maturities of 10 years or less (Table 1); revolving credit and mortgages tend to have longer (effective) maturities that render payment/interest bias irrelevant.<sup>15</sup> Third, our empirical strategy exploits *within*-household variation in APRs from different lenders and during different time periods, on loans with otherwise similar characteristics (maturity, loan amount, and the durable financed). That strategy does not work for mortgages during our sample period, because very few households hold a second mortgage.

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<sup>13</sup> 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 consistent set of questions in 1989 (but dropped the questions we use to measure payment/interest bias). We start with 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.

<sup>14</sup> See Lusardi and Tufano (2009) for contemporary evidence on payment/interest bias, measured using questions that are framed differently than ours. Payment/interest bias was measured extensively prior to 1983 (although not labeled as a bias *per se*), and we review that evidence below.

<sup>15</sup> See Stango and Zinman (forthcoming) for theory and evidence on the bias/maturity relationship. Intuitively, bias leads to failure to consider the impact of declining principal balances on the cost of borrowing. On short term loans that failure really matters, because principal balances decline quickly. It matters less on long term loans where principal balances decline very slowly, and is irrelevant on interest-only loans where principal remains constant.

### *C. The Original TILA, its Motivations, and Prior Work*

The 1968 Truth-in-Lending Act (TILA) 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 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 or contracting that shrouded or 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 over the life of the loan. 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. These ads offer insight into the equilibrium that would prevail if lenders faced no regulatory constraints. While some theories yield an equilibrium with voluntary disclosure, credit markets do not fit that model.<sup>16</sup>

The more fundamental motivation for mandating APR disclosure was less precise. Policymakers often note that APRs allow one to compare the prices of loans with different maturities, or to compare loan prices to asset returns. Several papers from the TILA enactment and reform period establish that consumers were unaware of APRs, either in the sense of knowing fair market rates or of knowing APRs on loans they actually held.<sup>17</sup> Both factors – the general notion that APRs are useful to know, and the empirical evidence that APRs were not widely known - framed the policy debate (National Commission on Consumer Finance 1972). Based largely on those factors rather than more direct evidence, advocates

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<sup>16</sup> Lenders often argue that consumers *want* to see loan offers quoted as payments, to help with monthly budgeting. That may very well be true, but it fails to explain why lenders *hide* APRs, particularly after TILA, when shrouding violates the law. Whether the incomplete disclosure favored by lenders sufficiently informs consumers is a separate empirical question, and one we examine below.

<sup>17</sup> This work includes Due (1955), 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. More recently, Bernheim (1995; 1998), Moore (2003), and Lusardi and Tufano (2009) find evidence consistent with limited understanding of loan terms, including interest rates. See also footnote 23.

of TILA asserted that mandated disclosure would reduce search costs, facilitate comparison shopping and improve market outcomes.

Below we develop payment/interest bias as a more complete underpinning for TILA. The systematic tendency of consumers to underestimate the cost of borrowing from other loan terms can explain why lenders choose to hide APRs, why some consumers do not accurately infer interest rates from other loan terms, and how mandated APR disclosure might affect market outcomes.

Our contribution relative to prior work is tightening the focus, and broadening the scope, on the role of biased perceptions of borrowing costs in consumer credit markets. Other work contains evidence that consumers systematically underestimate an interest rate implied by other loan terms, but rarely states it as such: most often, the emphasis is on whether consumers make *mistakes* in assessing APRs, rather than whether errors are *biased*.<sup>18</sup> The work that does state a finding similar to ours does not test the implications of payment/interest bias for market outcomes and the effects of mandated disclosure.

#### *D. Payment/Interest Bias: Evidence and Origins*

We define *payment/interest bias* as a tendency to underestimate an APR when attempting to calculate it based on other loan terms. Our household-level measure of payment/interest bias comes from two hypothetical questions that appear in the 1983 SCF.<sup>19</sup> 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 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).<sup>20</sup> Given the predefined maturity and principal amount, the repayment total yields the *actual APR* implied by the respondent’s self-supplied repayment total.<sup>21</sup> 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

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<sup>18</sup> 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...”

<sup>19</sup> 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 (forthcoming) for a discussion.

<sup>20</sup> 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.

<sup>21</sup> 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.

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 the *stated* or *perceived* APR. Figure 5b shows the distribution of perceived APRs. In order to classify respondents as more or less biased we calculate the level difference between the perceived and actual APRs, with negative values indicating greater bias. We call this level difference *payment/interest bias*. Figure 6a presents a histogram of the bias. The share of respondents underestimating the actual APR is 98%. The median bias is -25 percentage points (-2500 basis points), and the mean bias is -38 percentage points. Roughly twenty percent of respondents give the add-on rate (e.g., a repayment total of \$1200 yields a perceived rate of 20%). But responses are heavily biased even relative to the add-on rate; nearly all 80% who supply something other than the add-on rate underestimate relative to the add-on (Figure 6b).

Our empirical strategy for measuring the effects of TILA relies on *ranking* households as more or less biased, and tests how *degrees* of bias matter empirically. Consequently, our choice of the APR rather than the add-on or some other rate as the benchmark interest rate is not empirically relevant; any definition of “interest rate” yields essentially the same ranking of households based on bias.<sup>22</sup> We classify households as “less biased” if they are in the lowest quintile of payment/interest bias, and as “more biased” if they are in quintiles 2-5 or did not answer one or both survey questions.<sup>23</sup> Appendix Table 3 shows that the results are robust to alternative functional forms of bias, and also robust to treating add-on responses as different from other responses with equal bias.

Prior work also finds or contains evidence of payment/interest bias. The evidence is robust to different ways of framing the question,<sup>24</sup> and the bias is evident on actual loans as well as in hypotheticals (Stango and Zinman forthcoming). Bias does not arise because people respond with individual assessments of “fair” current or past market prices; such responses would not generate *internal*

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<sup>22</sup> Other than the APR or add-on rate, the most common definition of “interest rate” is the *effective interest rate*, which is the continuously compounded rate. The effective rate is even higher than the APR, and hence using it as the actual rate increases the measured level of payment/interest bias.

<sup>23</sup> Our payment/interest bias nonresponders typically answer the first question but not the second. Demographically nonresponders are most similar to households in the highest quintile of bias, and nonresponders are actually have lower educational attainment and income than households in highest-bias quintile.

<sup>24</sup> Earlier studies with any information about the direction of mistakes typically report only 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 basis points) but smaller than in our samples. Lusardi and Tufano (2009), using data from 2007, “confirms the evidence reported in Stango and Zinman that individuals are systematically biased toward underestimating the interest rate out of a stream of payments”.

*inconsistency* between the payment and interest rate responses.<sup>25</sup> Nor does the fair market rate story square with the empirical finding that bias on similar questions is relatively constant across different decades, despite substantial differences over time in the mean and volatility of market rates.<sup>26</sup>

The fact that consumers' perceived rates are *incorrect* is unsurprising; since calculating an APR is difficult even with a calculator. What is striking is that perceived rates are *biased*: mistakes overwhelmingly lie in a particular direction. That is unusual; the "wisdom of crowds" implies that answers to even very difficult questions are often correct on average (Surowiecki 2005).

The likely source of payment/interest bias is a general and well-documented *exponential growth bias* (EG bias) in how individuals assess mathematical terms involving exponentiation. Our focus in this paper is on the *implications* of payment/interest bias; Stango and Zinman (forthcoming) examines the origins of the bias. Here is the intuition. Consider a consumer attempting to infer a loan's periodic interest rate  $i$  given a loan amount  $L$ , maturity  $t$ , and periodic payment  $m$ . The periodic rate is:

Consider a consumer attempting to infer a loan's periodic interest rate  $i$  given a loan amount  $L$ , maturity  $t$ , and periodic payment  $m$ . The periodic rate is:

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

A consumer with EG bias is one who underestimates the exponential term  $(1 + i)^t$ . Doing so leads to an underestimate of the APR.

Payment/interest bias can also stem from a simple and common failure to understand that declining principal balances increase the interest rate on an installment loan (this failure can be due to EG bias, or to some other cognitive process). The most common incorrect answer in our data is the add-on rate, which assumes no decline in principal balance at all: 20% *would be* correct if someone borrowed \$1000 for a full year and repaid \$1200 at the end of the year. But equal monthly payments of \$100 reduce the principal balance during the year, increasing the cost of borrowing to 35% APR.

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

Now we discuss how lenders who shroud APRs and emphasize monthly payments might be able to price discriminate based on payment/interest bias.

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<sup>25</sup> Actual interest rates on actual 12-month loans in the 1983 SCF lie between the perceived and actual rates supplied by respondents holding those loans; it does not appear that perceived rate responses are simply equal to consumers' APRs on actual loans. The median rate on 12-month loans is 19%, with a twenty-fifth percentile of 15% and seventy-fifth percentile of 23%.

<sup>26</sup> Stango and Zinman (forthcoming) show that bias in the 1977 SCF is just as prevalent as bias in 1983, despite the fact that 1977 followed a period of low and stable market rates, while 1983 followed a period of high and volatile market rates. It is also similar in 1968.

One threshold question is whether competition in credit markets makes such discrimination impossible.<sup>27</sup> While we cannot directly test whether lenders have sufficient pricing power to segment based on payment/interest bias, there is considerable evidence that price discrimination can occur in highly competitive markets.<sup>28</sup>

To see how a lender with some ability to price discriminate might do so based on bias, consider a 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 APR if that is disclosed and on the perceived rate if the APR is not disclosed.<sup>29</sup> 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.

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 mechanics of that process depend on whether the lender observes payment/interest bias or must induce consumers to self-select. If bias is observable, the lender can perfectly price discriminate and design loan offers with true rates that are higher than 10 percent perceived by biased customers. Even without the ability to observe or learn about a given individual's payment/interest bias, a monopolist lender can present a menu of loan offers that induces self-selection 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.

One limitation of our data is that it does not reveal the precise channel through which lenders attract customers and set loan terms. Lenders could implement price discrimination via "high touch" on-the-spot negotiation; for auto loans, which are the bulk of loans in our data, recent evidence suggests that such

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<sup>27</sup> 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 consumer credit markets are competitive in the sense that marginal entrants earn zero economic profit.

<sup>28</sup> See Borenstein (1985) and Holmes (1989) for theoretical models of price discrimination in free-entry markets. Borenstein (1991) and Shepard (1991) show that price discrimination exists in retail gasoline markets.

<sup>29</sup> This presumes that consumers use APRs rather than monthly payments (or perhaps add-on rates) when making borrowing decisions and comparing loans. If they can make effective decisions without referring to an APR, then APR disclosure regulation should be empirically irrelevant; we test that hypothesis below. It may be rational for liquidity constrained consumers to (largely) ignore interest rates (Attanasio et al. 2008; Karlan and Zinman 2008; Adams et al. 2009). We have several ways of controlling for liquidity constraints and comparison shopping, and detail them in Sections IV and V.

price discrimination is common.<sup>30</sup> Alternatively, lenders could attract customers via print or other advertisements emphasizing low monthly payments. Anecdotally, lenders employ a combination of those strategies.<sup>31</sup>

Both lender strategies require shrouding, so another threshold question is why consumers do not infer that shrouded interest rates are high rates. Indeed, that inference generates voluntary disclosure in models following Salop and Stiglitz (1977). But those models have Bayesian consumers. If consumers are prone to payment/interest bias and are not fully aware of it, then they will not necessarily infer that shrouded prices are high prices.<sup>32</sup> Gabaix and Laibson (2006) show that shrouding can exist even in highly competitive markets if some consumers are unaware of their bias and debiasing is costly. Payment/interest bias might also drive search costs up. Search cost models typically assume that the cost of acquiring price information is low, but this is not necessarily the case for consumers with payment/interest bias. Such consumers may not even recognize the value of search if they are not aware of their bias.

In short, theory and evidence suggest that lenders *could* price discriminate on payment/interest bias. Whether they *do* discriminate on bias is an empirical question. And even if lenders can price discriminate on bias when unconstrained, the other empirical question is whether mandated disclosure changes that relationship. We now provide some institutional details on Truth in Lending, before turning to our empirical analysis of whether TILA affects the equilibrium relationship between bias and APRs.

### **III. Truth-in-Lending, its Enforcement, and a Natural Experiment**

Here we discuss the history of the Truth in Lending Act (TILA), focusing on two differences in the strength of its enforcement: across lender type and over time. We later use this variation to help identify the links between TILA, payment/interest bias and loan APRs.

The first difference in TILA enforcement is by lender type. Two types of lenders make installment loans: depository institutions (banks) and non-bank finance companies. While TILA mandates that all lenders disclose APRs, it assigns enforcement jurisdiction in a way that can change incentives for compliance. Banks and other depository institutions are under the purview of the Federal Reserve System and other bank supervisory agencies. Those agencies monitor and examine banks regularly for safety, soundness and TILA compliance (Walter 1995). In contrast, TILA assigns enforcement authority for non-

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<sup>30</sup> Some recent evidence suggests that contemporary auto loan finance companies often mark-up loans in “on the spot” negotiations (Cohen 2007). 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 V-B for details).

<sup>31</sup> E.g., lenders may use ads to “prime” biased consumers to be receptive to payments-based offers made on-the-spot.

<sup>32</sup> Stango and Zinman (forthcoming) find evidence consistent with limited consumer awareness of payment/interest bias.

bank finance companies to the Federal Trade Commission (FTC). The FTC is a law enforcement agency rather than a supervisory agency. It responds to consumer complaints but does not regularly examine finance companies for TILA compliance. So banks and finance companies face differential *public* enforcement throughout our sample period.

The second difference in TILA enforcement is over time. In response to confusion about what constituted compliance with the law, and concern about escalating caseloads and lender liability, Congress passed the Truth-in-Lending Simplification and Reform Act that limited the scope of *private* (primarily judicial) enforcement. The new TILA provisions took effect April 1, 1981, and the extent of the changes led the Federal Reserve Board to label it a “new Truth-in-Lending-Act” (Federal Reserve Board 1981).

The central effect of the new TILA was to limit the size and enforcement of civil penalties.<sup>33</sup> 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 more 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 suits. 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.<sup>34</sup> Meanwhile the new TILA left public enforcement essentially unchanged. Because of its emphasis on private enforcement, TILA reform applied primarily to finance companies (where enforcement had operated primarily through the courts) rather than banks (where enforcement had operated primarily through bank supervisors).

TILA and its reform therefore provide a difference-in-difference in the enforcement of mandated APR disclosure. The jurisdictional difference between banks and finance companies holds throughout our sample period. The 1981 TILA reform leads to a *relative* shift in the strength of enforcement on different lender types, because private enforcement weakened, relaxing constraints on finance companies, while public enforcement remained essentially constant, leaving bank constraints unchanged.

Descriptive evidence squares with the interpretation that the new TILA reduced compliance incentives for finance companies but not for banks. The overall TILA caseload dropped almost

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<sup>33</sup> 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).

<sup>34</sup> 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.

immediately to “relatively sparse” levels (Fonseca and Fonseca 1986; Keest and Klein 1995). 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 willful deception (Willenzik and Schmelzer 1981; Elliehausen and Kurtz 1988; Barefoot 1990; Jackins and Gates 1990). In contrast, an FTC campaign begun in 1985 (two years after our sample period ends) 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 violators (Fortney 1986; Federal Trade Commission various years), and also increased its active enforcement, but that change took place well after our sample period. Figures 8 and 9 provide anecdotal evidence of the typical differential effects of TILA enforcement 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 the APR.

#### IV. Testing for Links Between TILA, Bias and Loan APRs

Identifying the empirical relationships between payment/interest bias, disclosure regulation and consumer loan interest rates is the primary empirical question at hand. Below we detail our identification strategy, describe the data and address some econometric issues.

##### A. Empirical Specification

Our empirical model uses household-level payment/interest bias, along with loan-level variation in TILA enforcement, to identify whether mandated disclosure changes lenders’ ability to price discriminate on payment/interest bias. The unit of observation is an installment loan  $l$  with APR  $r$ , obtained by household  $h$  at time  $t$ ,

$$r_{hlt} = \beta_1 \text{MoreBiased}_h \text{Finco}_l \text{NewTILA}_t + \beta_2 \text{MoreBiased}_h \text{Finco}_l + \beta_3 \text{MoreBiased}_h \text{NewTILA}_t \\ + \beta_4 \text{Finco}_l \text{NewTILA}_t + \beta_5 \text{MoreBiased}_h + \beta_6 \text{Finco}_l + \beta_7 \text{NewTILA}_t \\ + \xi_h + g(Z_l) + \varepsilon_{hlt}$$

The indicator  $\text{MoreBiased}_h$  equals zero if the household is in the lowest quintile of payment/interest bias, and equals one otherwise. We present results with other functional forms for bias, and differential treatment of add-on rate responses, in Appendix Table 3. The indicator  $\text{Finco}_l$  equals one if the loan is from a finance company and zero if it is from a bank. The indicator  $\text{NewTILA}_t$  equals one if the loan was originated after TILA reform, and zero otherwise. Enforcement varies both across lender type (bank or finance company), and across TILA regime (pre- or post-reform). The triple-interaction term  $\text{MoreBiased}_h \text{Finco}_l \text{NewTILA}_t$  exploits both sources of variation in enforcement to answer the

primary empirical question: how does the correlation between bias and APRs vary as TILA enforcement changes?

The model includes household fixed effects  $\xi_h$ , which control for any unobserved time-invariant factors that would affect equilibrium loan rates. The fixed effects are identified because many households hold loans from different lender types, and loans that were originated both pre- and post-TILA reform (Table 1). The household fixed effects address the concern that payment/interest bias is correlated with other household-level shifters of credit risk or loan demand, including but not limited to income, education, discount rates or financial sophistication.

The vector  $Z_l$  is a set of loan characteristics. These include fixed effects for product purchase category (e.g., “used car,” “furniture”), fixed effects for year of loan origination, dummies for loan maturity categories, and the natural logarithm of amount borrowed.<sup>35</sup> These variables control for loan- and purchase-specific characteristics that are correlated with loan APRs. The year effects control for variation over time in market rates.

The other controls include a full set of level effects and double interactions of the three indicators in the triple-difference. The level effect  $MoreBiased_h$  controls for the average relationship between bias and APRs across TILA regime and lender type, and is subsumed in the household fixed effects. The TILA reform effect  $NewTILA_t$  controls for the average shift in loan APRs after TILA reform across bias category and lender type, and is subsumed in the loan year-of-origination effects. The lender type indicator  $Finco_l$  captures the mean rate difference between finance companies and banks.

The double interaction terms measure the difference in the relationship between bias and APRs at finance companies ( $MoreBiased_h Finco_l$ ), a shift in finance company interest rates after TILA reform ( $Finco_l NewTILA_t$ ), and a shift in the relationship between bias and APRs after TILA reform ( $MoreBiased_h NewTILA_t$ ).

The empirical model therefore estimates how the interaction of TILA enforcement and bias affects APRs, controlling for unobserved household heterogeneity, unobserved lender type heterogeneity, and unobserved TILA reform-related heterogeneity. It also allows for loan APRs to vary systematically by product purchased, maturity and loan amount. The double interactions rule out subtler confounding influences. We discuss some of those below.

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<sup>35</sup> The results are robust to less-parametric controls for loan size (such as loan amount decile). And as discussed below in Section V-B, we also observe the purchase price of the product being financed for a subset of loans (car and home improvement loans). Adding this information to the vector of loan and purchase characteristics does not change the results.

## *B. Econometric Issues and Identification*

### 1. Unobserved Risk

The most natural econometric concern is that bias is correlated with unobserved household-level credit risk. The household fixed effects control for the level effect of time-invariant unobserved household-specific risk on APRs. The double interaction  $MoreBiased_h Finco_l$  controls for the possibility that banks and finance companies price unobserved risk correlated with payment/interest bias differently. The double interaction  $MoreBiased_h NewTILA_t$  controls for the possibility that risk-based pricing by all lenders to more-biased households changes after TILA reform (relative to pricing to less-biased households). We have also estimated models that add interactions between a proxy for default risk and the level effects and double interactions, with no effect on the results (see Section V-B).

Time-varying unobserved risk is a more subtle concern. Suppose that household risk varies over time and is correlated with lender type and loan APR. For that correlation to cloud even the double interaction  $MoreBiased_h Finco_l$  it would have to be true that the *level* of payment/interest bias is correlated with the *variance* of unobserved credit risk over time, after conditioning on loan- and time-specific characteristics used to price that risk. Moreover, that correlation will only affect the triple-difference if the correlation itself changes after TILA reform.<sup>36</sup> While we cannot rule out that change, it is unlikely, and we have found no evidence of such a change.

### 2. Selection into Lender Type and the Sample

Another concern is that lender type is endogenous – that biased households are more likely to pay higher rates, and more likely to borrow from finance companies. The household fixed effects should control for that possibility, which might otherwise lead to a positive relationship between APR and  $Finco_l$ . More subtle possibilities (such as a correlation between household risk over time and selection into finance companies) are discussed above. We have also estimated the correlation between bias and borrowing from finance companies using cross-sectional models, and report those results in Appendix Table 2.<sup>37</sup> Nearly all of the explainable variation in borrowing from finance companies is due to loan amount and the set of loan purpose (product) dummy variables. We do find a small positive correlation

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<sup>36</sup> Note also that nearly all of the loans in our sample had interest rates that were fixed over the life of the loan and hence did not vary with changes in borrower characteristics or market rates.

<sup>37</sup> One could envision estimating a first stage selection equation that estimates the (perhaps loan-specific) likelihood that a household borrows from a finance company. The problem with this strategy is the lack of good candidates for the exclusion restriction in the first stage. That limitation forces us to assume strict exogeneity, meaning that conditional on the observables and the household fixed effects, time-specific shocks to loan APRs are uncorrelated with borrowing from finance companies in all time periods. Because product purchased and loan amount are the primary determinant of finance company borrowing, one could also view our exogeneity assumption as equivalent to an assumption that past shocks to loan APRs (again, conditional on the household fixed effect and past loan characteristics) do not affect future product choices.

between bias and the likelihood of borrowing from a finance company. However, there is no relationship between bias, TILA reform and the likelihood of borrowing from finance companies in a model with household fixed effects.

A minor concern is that our sample composition is affected by loan attrition from refinancing or prepayment. Such events were uncommon during our sample period, and it is unlikely that they would be correlated with the triple-difference interaction.<sup>38</sup>

Sample selection, and in particular our model's reliance on household with multiple loans for identification, also raises an external validity issue. We discuss this in Section V-B, after detailing our main results.

### 3. Confounding Changes in Equilibrium APRs after TILA Reform

Because identification of enforcement strength comes from TILA reform, it is important to allow for the possibility that TILA reform affects bank and/or finance company loan pricing in ways unrelated to pricing payment/interest bias. The level effect  $NewTILA_t$  allows for a general shift in loan pricing across all lender types. The double interaction  $Finco_t NewTILA_t$  allows for a differential shift in pricing at finance companies relative to banks. The latter is particularly important, as TILA reform's focus on enforcement at finance companies certainly admits the possibility of such a shift.

### 4. Which Loan Maturities Provide Identification?

Our inclusion of maturity dummies in the empirical model means that coefficients are only identified from pre-/post-TILA reform comparisons of loans with identical maturities. The timing of the survey (which begins in February 1983) and TILA reform (effective April 1981) means that shorter maturity loans (e.g., those <24 months) taken out in the pre-TILA period will be relatively underrepresented, and hence will not provide much identification. This should not be a big concern, since most installment loans during our sample period (at least in terms of dollar volume) had maturities of three years or more. Thus we have ample overlap across pre- and post-TILA reform for the most important maturities in our sample (Table 1).

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<sup>38</sup> In the present refinancing is more common (e.g., of home equity loans), and it could be the case that a weakening of TILA would induce some biased borrowers to refinance at relatively high rates. This would only affect the consistency of the triple-difference coefficient if the tendency of borrowers to refinance loans that had relatively high rates *to begin with* varied with payment/interest bias and lender type; in principle the sign of this correlation is ambiguous, and could be zero. One way to avoid this issue would be to obtain data at the loan origination level (as opposed to our data, which measure the stock of loans outstanding at a particular time).

## 5. Broader Interpretations of “Payment/Interest Bias”

Table 4 presents evidence testing whether our measure of payment/interest bias simply proxies for something broader like credit risk, cognitive ability (education), price sensitivity (income), or search costs.<sup>39</sup> Section V-B discusses these results.

### **V. Results**

Before presenting the results we briefly review the sample and our variables of interest. Table 1 shows descriptive statistics for the full sample of outstanding non-mortgage installment loans owed by households in the 1983 SCF.<sup>40</sup> The top rows show the distribution of the 1929 households and 3094 loans across our categories of payment/interest bias. Subsequent rows describe loan characteristics: APR, lender type, TILA regime, loan size, product purchased and maturity. Although the household fixed effects control for household-level characteristics, for descriptive purposes we summarize the differences across more-biased vs. less-biased households for income, education and debt-to-income ratio in Appendix Table 1.<sup>41</sup>

#### *A. Main Results*

Table 2 presents estimates of equation (2). In all specifications, the household fixed effects are significantly correlated with loan APRs. Those effects subsume the main effect of payment/interest bias, and capture the mean effect of all other unobserved time-invariant household characteristics. The purchase and loan characteristics are also significant; those subsume the main effect of the post-TILA reform period. The within-household R-squared is high (roughly 0.5), primarily due to the inclusion of loan characteristics.

Column (4) is our main specification and includes the triple difference interaction. That coefficient has a p-value of 0.06 and implies that more-biased borrowers paid 410 basis points more than less-biased borrowers at finance companies after TILA reform, on otherwise similar loans. 410 basis points is a large difference; it represents nearly 30% of the mean APR in our sample. Table 3 translates the effect size into foregone consumption by estimating how much more a more-biased consumer could borrow at 14%

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<sup>39</sup> Stango and Zinman (forthcoming) presents evidence suggesting that payment/interest bias captures something distinct from standard measures of broader financial sophistication.

<sup>40</sup> Recall from Section II-B 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 forthcoming). 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 mortgages.

<sup>41</sup> See Stango and Zinman (forthcoming) for more detail on these cross-sectional relationships and others.

instead of 18%, holding monthly payments constant. We do this exercise for the four most common purchase types. On the two most common purchase types – used and new cars – the “bias markup” costs more-biased consumers an estimated \$175 and \$452 (1983 dollars), which are 6% and 8% of the median loan amounts for those product categories.

The  $MoreBiased_h Finco_l$  coefficient in Table 2 Column (4) is not significantly different from zero; we cannot reject the hypothesis that pre-TILA reform pricing at finance companies was no different than that at banks. This is consistent with TILA enforcement constraining price discrimination. Before TILA reform, finance companies were constrained by threat of litigation. After TILA reform weakened civil penalties, finance companies were freer to price discriminate on bias and prices changed. There is no evidence that TILA reform changes the more biased vs. less biased APR difference at banks: the  $MoreBiased_h NewTILA_t$  coefficient is not significantly different from zero. That is not surprising given that TILA reform left bank compliance incentives essentially unchanged.

The level effect  $Finco_l$  in Column (4) shows that finance company loan APRs were significantly higher than bank APRs for less biased borrowers in our pre-TILA reform period. This indicates cost or demand differences across lender type, or a demand difference coupled with a greater overall ability to price discriminate.

The  $Finco_l NewTILA_t$  effect in Column (4) is large and negative, with a p-value of 0.07. This result is interesting for two reasons. First, conditional on the other variables in our triple-difference specification, it suggests that the difference between finance company and bank rates shrinks for *less*-biased households after TILA reform, relative to the pre-TILA reform difference. This is what one would expect for a low willingness-to-pay group when a constraint on price discrimination is relaxed. Second, together with the triple-difference coefficient (which has roughly the same magnitude and the opposite sign), the  $Finco_l NewTILA_t$  result also suggests a more general decline in finance company rates after TILA reform. Without such a decline, a greater degree of price discrimination would imply lower APRs to less-biased households but higher APRs to more-biased households. But the  $Finco_l NewTILA_t$  coefficient completely offsets the  $MoreBiased_h Finco_l NewTILA_t$  coefficient, meaning that APRs paid by *more*-biased borrowers at finance companies after TILA reform are roughly equal to their pre-TILA reform levels. That zero change combined with the APR decline for less-biased borrowers implies that the average finance company APR across all loans falls after TILA reform. One explanation for that change is that TILA reform reduced regulatory burden for all finance companies, reducing costs overall even as it allowed greater price discrimination on payment/interest bias. The fact that the share of household borrowing from finance companies rose sharply after TILA reform, and that the share increased more for less-biased borrowers (Table 1), is consistent with this interpretation. In all our results suggest that TILA reform shifted the finance company supply curve to the right.

The other columns shed light on the sources of variation in APRs across bias, lender and TILA reform categories. Column (1) includes household fixed effects, loan characteristics, the  $Finco_l$  indicator, and the  $Finco_l NewTILA_t$  interaction; it omits all of the bias-related variables. The  $Finco_l$  coefficient is positive and significant; APRs charged by finance companies are higher *within household* even after controlling for loan characteristics (p-value=0.002). That difference reflects the mean of APR differences across lender types, for all borrowers in the sample. The  $Finco_l NewTILA_t$  interaction is not significant. The coefficient on  $MoreBiased_h Finco_l$  in column 2 shows that across TILA regimes, finance company APRs are higher to more-biased households (p-value=0.07). Column (3) drops the  $MoreBiased_h Finco_l$  interaction and replaces it with a  $MoreBiased_h NewTILA_t$  interaction. The coefficients again indicate a premium in rates on finance company loans (p-value=0.002). The coefficient on  $Finco_l NewTILA_t$  is insignificant, meaning that TILA reform is not correlated with a shift in finance company pricing; that coefficient is imprecisely estimated, however. Nor do we find a significant overall increase in the APRs paid by more-biased borrowers post-TILA reform, although the confidence interval on  $MoreBiased_h NewTILA_t$  is wide.

### B. Robustness

Column 5 of Table 2 restricts the sample to include only the 1,384 loans owed by 529 households with either loans from both lender types or loans from both TILA regimes, since our identifying variation comes in part from these households. The results are similar.<sup>42</sup>

Appendix Table 3 shows that the results are similar if we treat add-on responses differently or measure payment/interest bias using other functional forms. Column (1) adds an additional interaction:  $Finco_l NewTILA_t Addon_h$ ; it is not significant. Column (2) defines less-biased households as those falling in the bottom two quintiles (instead of just the bottom quintile). Column (3) defines less-biased as falling in the bottom three quintiles. Column (4) uses our standard definition (less biased = bottom quintile), but excludes respondents with correct answers, negative bias or non-responses. Columns (5) and (6) use the log and the square root of bias, taking its negative so that positive values indicate greater bias; both specifications discard nonresponse observations and responses with (now) negative bias, and the former also discards the 33 correct (zero bias) responses. In all but model (3), the coefficient on the triple interaction is significant in economic and statistical terms.

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<sup>42</sup> The point estimates are also similar, but very imprecisely estimated, if one restricts the sample further to the 446 loans held by households for which the triple-difference is completely identified. Those households have loans both before and after TILA reform, and from both a bank and from a finance company.

We also observe the purchase price of the product being financed for a subset of loans (car and home improvement loans). Adding this information to the vector of loan and purchase characteristics does not change the results.

Table 4 explores whether our measure of payment/interest bias, and effects thereon, might in fact be capturing something broader. The second-to-last row of the table casts some prima facie doubt on this hypothesis: it shows that the unconditional correlation between bias and each of these other characteristics is actually fairly weak. Nevertheless we explore the hypothesis more formally by adding interactions between a broader characteristic (recent credit denial, education, income, or loan comparison shopping), TILA regime, and lender type to our standard specification. None of these additional interactions are individually or jointly significant. Nor does adding the additional interactions change inferences on the variables including payment/interest bias: the bias interactions remain jointly significant, and the magnitude on our variable of greatest interest,  $MoreBiased_h Finco_l NewTILA_t$ , remains essentially unchanged.

### C. External Validity

A final issue is out-of-sample validity. Since the fixed effects model gets identification from households with multiple loans, our results apply directly only to such households. A natural question is whether they also apply to single-loan households. To explore that question, albeit indirectly, we have also estimated our empirical model in Table 2 without household fixed effects, retaining all of the other variables and adding household-level covariates. We estimate that model separately for single- and multi-loan households. The household-level covariates include the level of payment/interest bias, income, education, other demographics, measures of preferences, broader financial sophistication, loan shopping, and liquidity constraints. For multi-loan households the cross-section results are similar to the fixed effect results in Table 2, in sign and significance. But for single loan households the cross-section results indicate essentially no relationship between payment/interest bias, disclosure enforcement and loan APRs. The overall pattern is not conclusive, but it suggests that our results apply only to frequent borrowers; these borrowers hold 63% of the loans in our data. One interpretation of the pattern is that some households are aware of their bias, and consequently get fewer loans and avoid higher APRs when they do borrow.

## VI. Conclusion

Our main findings are on two fronts. First, we show that consumers' biased perceptions of borrowing costs can explain why lenders shroud interest rates, and that mandated APR disclosure can counter lenders' ability to price discriminate on payment/interest bias, in principle. Second, we show that a TILA

enforcement matters: variation in the strength of enforcement explains interest rates on actual loan contracts held in equilibrium, but only among those consumers with greater payment/interest bias. When disclosure regulation becomes weaker, the change in the more-biased vs. less biased finance company loan rate difference is roughly 400 basis points. This suggests that disclosure regulation, when enforced, does in fact change market outcomes by constraining the ability of lenders to price discriminate on a cognitive limitation. More generally, our findings provide evidence of a link between a cognitive bias and market outcomes, and add to the small literature on the links between disclosure, regulation and market outcomes.

Although our findings suggest that weaker TILA enforcement leads to greater price discrimination on payment/interest bias, the net effect of weaker TILA enforcement is a general decline in finance company APRs. In fact, the net effect of weaker TILA enforcement on APRs is zero on more-biased borrowers, and negative for less-biased borrowers. Concurrently the share of household loans from finance companies rose sharply after TILA reform, with a greater increase for less-biased households than for more-biased households. The full picture of our results suggests that relaxing disclosure constraints shifted the finance company supply curve to the right.

In this sense our findings highlight a critical limit of disclosure regulation: the cost of enforcement. This issue and others discussed in the introduction preclude us from drawing clear conclusions on the welfare effects of (relaxing) mandated APR disclosure. Instead we hope that our work will spark a careful rethinking of the motivation and approach to consumer protection in retail financial markets. A key point is that payment/interest bias has a solid normative basis for being treated (unlike biased preferences), and is easily identifiable (unlike biased expectations).

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 cost-effective ways to improve consumer financial decision making. Decision aids reduce 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 that heuristic is used by only a few people in our sample.

Clearly much work remains to be done on identifying and understanding cognitive biases that might affect firm strategies and consumer decisions, and on designing and testing cost-effective debiasing methods that improve market outcomes.

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