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**ABSTRACT**

Borrowing decisions affect most households, with large stakes and implications for subfields as varied as macroeconomics and industrial organization. I review theoretical and empirical work on household debt: its prevalence, level, growth, and composition, as well as various measures of consumer choice and market (in)efficiency, elasticities, and prices, including new evidence on how borrowing heterogeneity affects the distribution of the opportunity cost of consumption. I also discuss opportunities and challenges in policy evaluation. A key takeaway is that puzzles outstrip stylized facts, and I highlight numerous avenues for further research.

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## I. Introduction

Why do research on household debt?

One reason is high stakes, in absolute and relative terms. U.S. households owe \$12-13 trillion in debt, down slightly from the peak in mid-2008 (sources: Flow of Funds, Federal Reserve Bank of New York Consumer Credit Panel). A comparable figure for borrowing by non-financial corporations is \$10 trillion (up from \$8 trillion in 2008).<sup>1</sup> Households borrow using an increasingly rich constellation of loan products, at real interest rates ranging from near zero to quadruple-digit APRs. The comparable range of non-risk-adjusted returns on the asset side of the household balance sheet is smaller by orders of magnitude.

Another reason is that these high stakes are prevalent: they affect most households, including poorer households who receive larger social welfare weights in many utility functions. More U.S. households participate in the credit card market (65%) than hold public equity (50%), and participation rates are substantial in the other big U.S. consumer debt markets—mortgages (45%), student loans (19%), and car loans (30%).<sup>2</sup>

Perhaps the most important reason is that behavior in debt markets has implications for many fields and domains beyond the liability side of the household balance sheet. Borrowing decisions are an apt arena for developing and testing intertemporal choice models that can be applied across many domains. Debt usage determines the opportunity cost of consumption or investment for most households. The combination of low-frequency, high-stakes decisions (e.g., mortgage and student loan choices) and high-frequency, lower-stakes decisions (e.g., credit card use) is reminiscent of many types of human capital investment (in education, health, child-rearing, etc.). Household debt is a great stage for applying contract theory, and for studying the interactions of (less sophisticated) consumers with (more sophisticated) firms. The absence of a robust advice market suggests the likelihood of gains from trade with literatures on expertise and delegation. The (attempted) regulation of debt markets provides illuminating examples of policymaking and its effects under enforcement constraints.

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<sup>1</sup> Source: Federal Reserve Flow of Funds 2014q1 balance sheet data, “credit market instruments” line item.

<sup>2</sup> Source: 2010 Survey of Consumer Finances Chartbook. The stock market figure counts both direct holdings and stock mutual funds held directly or through IRAs, 401(k)s, etc. The credit card figure is proportion of households who report holding one or more general-purpose credit cards.

So the returns to understanding household borrowing decisions, and the markets that mediate these decisions, are high.

Yet research on household debt has lagged behind its sister literatures on the asset side of the household balance sheet.<sup>3</sup> This imbalance is evident in terminology—both “portfolio choice” and “behavioral finance” commonly refer only to the asset side—as well as in output.<sup>4</sup> I suspect that the neglect of household debt is pronounced relative to its cousin literatures on corporate debt.<sup>5</sup>

To fix ideas on under-researched questions, let’s start with one example (among many to follow): the credit card over-borrowing puzzle. The key fact is that no extant model, whether neoclassical or behavioral, can generate enough credit card debt; e.g., even a life-cycle model with beta-delta discounting under-predicts credit card borrowing by 50% (Angeletos et al. 2001). This puzzle raises fundamental questions about the value and shape of preference parameters, the nature of expectation formation, consumers’ (biased?) perceptions of interest rates and opportunity costs, the interactions of consumers, suppliers, and marketers, and their implications for the allocation of massive amounts of resources over the various horizons that affect the macroeconomy. In these respects I find this puzzle no less interesting than, say, the equity premium puzzle. Yet orders of magnitude more ink have been spilled trying to document and explain the equity premium than the credit card “discount”.

The rest of this article lays out topics for future research as much as a literature review, since most of the key questions underpinning the economics of household debt lack definitive answers. A few notes on scope before I proceed. I do not cover extremely short-term loans that are more the province of literatures on money and payments. I give short-shrift to secondary markets, informal debt, and medical debt.<sup>6</sup> I focus on the U.S., with some opportunistic comparisons to other countries. At some junctures I will intentionally blur the lines between households,

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<sup>3</sup> I’m making the argument that household debt is a relatively neglected topic within the relatively neglected sub-field of household finance. See Campbell (2006) and Tufano (2009) for discussions of how and why household finance is neglected relative to comparably important topics in (financial) economics.

<sup>4</sup> Tufano (2009, p. 229) makes a similar point: “Arguably, most of the existing literature on consumer financial decisions is focused on the saving and investing functions”.

<sup>5</sup> “By analogy with corporate finance, household finance asks how households use financial instruments to attain their objectives” (2006, p. 1553).

<sup>6</sup> For introductions to medical debt and its relationship to health insurance access, see, e.g., Gross and Notowidigdo (2011), Finkelstein et al (2012), and Mazmuder and Miller (2014).

entrepreneurs, and small businesses, because most small firms are closely held by the households that own and operate them.

Section II provides some key facts on how much and how people borrow, with an eye towards providing a quick institutional primer on household debt and introducing some open questions at the intersection of household finance, macroeconomics, contract theory, and industrial organization. Key themes here include unexplained dramatic growth in consumer debt, the economic importance of small-dollar credit and debt collection markets, the potential importance of bundling durable purchases with financing, and unexplained variation in contracting practices (particularly across markets and countries).

Section III reviews evidence and open questions on consumer choice (in)efficiency in debt markets, bringing together various literatures that are typically thought of as disparate. Mounting evidence on the downstream impacts of credit use paints a muddled picture of whether consumers actually make themselves (weakly) better off by borrowing. Comparable evidence on consumer decisions to workout or default on debt is thinner but more encouraging. Evidence on consumer search and price dispersion suggests that many households leave substantial amounts money on the table by failing to find good deals on loans, although what drives this tendency and allows it to persist in equilibrium remains poorly understood. Evidence on allocation suggests that consumers are far more efficient at minimizing costs conditional on their set of contracts than they are at choosing debt contracts. It will be important to unpack why this is.

Section IV reviews empirical evidence on several key inputs to models of household debt. I start by providing some new evidence on the opportunity cost of consumption or investment, and show that debt creates substantial wedges between this cost and the risk-free rate. I estimate that about 75% of households face a shadow cost exceeding the risk-free rate, with 45% of households facing a cost of at least 10%. I also discuss the scant evidence on borrowing motives, and the substantial evidence on binding credit constraints. The latter is surprising given a long trend of innovations in risk-based pricing, and that economic models *under*predict consumer borrowing. I.e., consumers borrow substantially more, and more expensively, than we would predict, yet they still have excess demand on the margin. Compounding this puzzle is evidence of price elasticity in credit cards and home equity loans, although this is tempered by findings of inelastic demand in first-mortgages and car loans.

Section V draws heavily on Zinman (forthcoming) in briefly reviewing some key theories and empirical tests thereof, with a focus on the question of whether consumer credit markets produce efficient allocations. Three classes of models identify failures that lead to credit undersupply: market power, regulatory failure, and several varieties of asymmetric information. But several newer classes of models predict overborrowing: other varieties of asymmetric information, externalities in collateral asset values, deleveraging frictions, systemic risk, and behavioral biases can lead to too much borrowing in some sense. Overall there is a lack of evidentiary consensus on whether markets err, and in which direction. We do not yet have a clear understanding of whether and under what conditions markets over-supply or under-supply credit, much less why.

Section VI discusses opportunities and challenges facing policy-focused research. The amount of policy activity is trending up, motivating both theory and empirics on policy design and evaluation. But challenges old and new confront this work, including limited empirical evidence on key modeling assumptions, underpowered natural experiments, and several factors that can make it difficult to identify the rules created by policy changes: limited enforcement, regulator discretion, and “shadow regulation”.

Section VII concludes with a recap of promising avenues for future research.

## **II. How much and how people borrow: Debt growth, levels, and contracting**

This section focuses on some key facts on how much and how people borrow: the growth of consumer debt over recent decades, recent and current debt levels, and characteristics of different product markets (and contract types). I postpone discussion of household-level prevalence and heterogeneity to Section IV. My main objectives in this section are to provide a basic institutional foundation for the rest of the paper, and to introduce three important and understudied questions. Two sit at intersections of macroeconomics and household finance, and ask what explains aggregate borrowing and growth rates, within- and across-countries. The third sits at an intersection of industrial organization, contract theory, and household finance, and ask what determines the rich constellation of loan product markets that we observe in equilibrium.

## A. Growth

Household leverage has grown remarkably in real terms.<sup>7</sup> Total household debt roughly doubled between 2000 and 2007 alone, and U.S. household debt/GDP has grown about fourfold over the post-World War II period (fivefold if we measure the ratio at its peak in 2009).

What explains this growth? One likely key factor is technological change in loan production (Dynan 2009; Edelberg 2006), including but not limited to reductions in distribution costs, risk-based pricing, monitoring and repossession,<sup>8</sup> and securitization and other secondary market innovations. The degree to which such changes actually represent advances in a welfare sense is a subject of much debate that is beyond the scope of this review (e.g., Lerner and Tufano 2012), although I touch on some related issues in Section V. It also remains unclear just how important changes to the loan production function have been relative to the other factors discussed below.

A related possibility is that the technology of persuasion has improved as well. A growing body of evidence suggests that uninformative sales tactics, sometimes accompanied by nonlinear contracts that “shroud” key attributes (Gabaix and Laibson 2006), increase the quantity and/or total cost of borrowing (Agarwal and Evanoff 2013; Bertrand et al. 2010; Gine, Martinez, and Keenan 2014; Gurun, Matvos, and Seru 2014). This work does not speak to the trend in question—have lenders gotten more effective at convincing consumers to borrow?—and of course some tricks of the trade, like “monthly payments marketing”, have changed little over the decades (Stango and Zinman 2011). But other trends are quite consistent with technological change in persuasion. Direct marketing has grown dramatically in the post-war period, and especially so in the IT era (how much of this marketing is actually purely persuasive, as opposed to informative in the classical sense, is an open question) . There also seems to be far more shrouded and teaser pricing now than even 25 years ago, with bank checking account overdrafts (Stango and Zinman 2014a), credit card introductory rates and penalty fees (Agarwal, Chomsisengphet, et al. 2014; S. DellaVigna and Malmendier 2004; Heidhues and Koszegi 2010), and adjustable rate mortgages (Gurun, Matvos, and Seru 2014) among the most prominent examples. These developments suggest that there may be complementarity between innovations

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<sup>7</sup> The Federal Reserve Economic Data (FRED) repository maintained by the Federal Reserve Bank of St. Louis is a good source for time series data and data visualizations.

<sup>8</sup> See, e.g., Kevin Wack, “High Tech Repo Men are Reshaping Subprime Auto Lending”, *American Banker*, 7/28/14.

in pricing and marketing, a hypothesis that is just one among many worth testing as we seek to understand technologies of persuasion.

Another likely key factor is the rise in real house prices (Christelis, Ehrmann, and Georgarakos 2013; Dynan and Kohn 2007; Mian and Sufi 2011a). This is germane given that much of the growth in household debt has been in mortgages.

Another factor that is drawing scrutiny is income inequality. Some studies suggest that rising inequality leads to increased supply by increasing loanable funds (e.g., Kumhof, Ranciere, and Winant 2014). Others have suggested that rising inequality leads to increased loan demand through social preferences (reference points and/or peer effects); I am not aware of any papers that have attempted to fully work out the demand side, but Georgarakos et al (2014) is an interesting starting point.

Other possible factors include demographic shifts (Dynan and Kohn 2007; Christelis, Ehrmann, and Georgarakos 2013) and reduced generosity in social insurance (Hacker 2008). I am skeptical about the latter because it could easily push in the wrong direction-- lenders presumably decrease supply as the variance of applicant cash flows increases (Hsu, Matsa, and Melzer 2014)-- and precautionary saving effects could swamp emergency borrowing effects.

Cross-country comparisons should prove to be useful in teasing apart the contribution of different growth factors. Christelis et al (2013) is a start in this direction.

## *B. Levels*

Cross-country comparisons are interesting in the cross-section as well. Figure 1 depicts simple measures of debt level (total household debt/GDP) and debt mix (mortgage vs. non-mortgage), for each of the G7 countries, as of 2010-11. The size of each pie is scaled to debt/GDP, with the UK having a ratio of about 2, the U.S. and Canada at about 1,<sup>9</sup> and Italy at about 0.5. Is there something about Anglo systems or preferences that leads to higher debt levels?<sup>10</sup> Broadening the set of countries casts doubt on that hypothesis: in a study of the U.S. and 11 Euro countries (not including the U.K.) Christells et al (2013) find that the Netherlands,

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<sup>9</sup> Canada has actually risen from 0.92 in 2010q4 to 0.95 in 2013q4, while the U.S. has fallen from 0.90 to 0.81.

<sup>10</sup> Australia looks quite similar to Canada, with a ratio of 0.95 in both 2010q4 and 2013q4.

Cyprus, and Luxembourg have higher debt levels than the U.S.<sup>11</sup> Several other countries had faster growth rates than the U.S. during the 2000s (Mian and Sufi 2014b). The questions of what drives these different levels and growth rates helps motivate the growing subfield of International Comparative Household Finance (see also footnote 25).

### *C. Composition and key product markets*

Figure 1 also highlights the importance of the mortgage market, which contains >50% of outstanding household debt in every G7 country except for Italy. Focusing on the U.S., mortgages (including second mortgages) peaked at about \$10 trillion outstanding in 2008, with about \$9 trillion in 2014q1 (source: FRBNY Consumer Credit Panel). Only about \$600 million of this is on home equity lines of credit (source: FRBNY Consumer Credit Panel). Interactions between the mortgage market and housing market have of course been linked to the Great Recession, as summarized ably elsewhere (Mian and Sufi 2014b). My review focuses more on studies of contracting and consumer mortgage choice. As the sections below indicate, the amount and quality work on these questions is becoming commensurate with the market's importance.

The other debt markets among what might be called the “Big Four” in the U.S. are student loans, vehicle loans, and credit cards.

Vehicle loans are collateralized, like mortgages and unlike the other key consumer credit markets. Two trends in auto loans strike me as particularly noteworthy. First is its quick post-crisis rebound relative to mortgages. Second is the continued growth of subprime auto loans.<sup>12</sup> In all, the vehicle loan market is almost certainly understudied market relative to its economic importance.<sup>13</sup>

Student loans surpassed \$1 trillion outstanding in 2011 and now comprise the largest unsecured consumer credit market in the world. About 85% of outstandings are owned by the

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<sup>11</sup> See also Chen (2013).

<sup>12</sup> See, e.g., <http://occ.gov/publications/publications-by-type/other-publications-reports/semiannual-risk-perspective/semiannual-risk-perspective-spring-2014.pdf>.

<sup>13</sup> Exceptions include Adams et al (2009), Einav et al (2012; 2013), Stango and Zinman (2011); Stephens (2008).

U.S. government.<sup>14</sup> Student loans are increasingly implicated in policy debates and related conjectures about overpriced secondary education, the real effects of rising delinquencies, and the relatively high cost and inflexibility of private-market student loans (see Section III-C). Unfortunately economists have brought little evidence to bear on these debates; again, this market has received scant researcher attention relative to its economic importance.<sup>15</sup>

A common thread through mortgage, vehicle, and student loan markets is the tight bundling of debt and a durable purchase (this is also true of many credit card contracts in other countries, and medical debt in the U.S.). My sense is that far more work is needed to understand how this interaction affects consumer consumption v. saving and risk management decisions, equilibrium contracts, and resulting debt service.

Credit cards have been the locus of some of the most interesting innovations in consumer finance in the post-war period, transforming both the payments system and lending (Ausubel 1991; Evans and Schmalensee 2005). On the lending side, credit card issuers have been at the forefront of risk-based pricing (D. B. Gross and Souleles 2002a; Stango and Zinman 2014b), flexible and nonlinear contracting (Agarwal, Driscoll, et al. 2013; 2014), direct marketing (Han, Keys, and Li 2013), and product differentiation (with affinity co-branding, rewards programs, prestige tiering, etc.). About 68% of households held a credit card in 2010, down from 73% in in 2007 (Bricker et al. 2012). Outstandings peaked at just over \$1 trillion in 2007q4, and have been around the low \$800millions since mid-2010 (source: G.19).<sup>16</sup>

Of course, outstanding debt is not the only measure of economic importance. For example, borrowing costs also matter greatly. By this metric, the gap between, say, credit cards and mortgages closes from an order of magnitude to a factor of 3 or so.<sup>17</sup>

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<sup>14</sup> See, e.g., <http://www.consumerfinance.gov/newsroom/student-debt-swells-federal-loans-now-top-a-trillion/>.

<sup>15</sup> Exceptions include Cadena and Keys (2012); Lochner and Monge-Naranjo (2012); Marx and Turner (2014); and Rothstein and Rouse (2011).

<sup>16</sup> Noisy estimates that deal with data limitations suggest that a large fraction of these balances are “revolving” (accruing interest)—see Brown et al (2011), Zinman (2009), and the data on credit card borrowing costs in footnote 17.

<sup>17</sup> Credit card issuer (lender) revenues from finance charges and fees have amounted to roughly \$120-\$150 billion in recent years, depending on how interchange fees are accounted for (<http://www.cuinsight.com/press-release/2013-card-industry-revenue-another-downtick> ). I am not aware of a comparable figure for consumer mortgage lenders, but if the \$8 trillion in outstanding first-mortgage

We should consider the incidence of borrowing costs as well as the level. This is particularly important in “small-dollar” loan markets that serve subprime, and often low-income, households at APRs that typically start in the triple digits. Payday loan borrowers spent \$7.4 billion<sup>18</sup> to borrow \$40 billion in 2010 on maturities of mostly 2-4 weeks.<sup>19</sup> A typical payday loan amount is a few hundred dollars, and the Consumer Financial Protection Bureau (CFPB) finds that over 80% of loans are rolled over or followed by another loan within 14 days (Burke et al. 2014).<sup>20</sup> Checking account holders spent \$32 billion on overdraft fees in 2013 (source: Moebs), down from a peak of \$37 billion in 2009, with the CFPB estimating that 8% of holders incur 75% of the fees (Bakker et al. 2014). A typical overdraft amount is \$20-\$25. Both payday loans and bank overdrafts require a checking account, but other small-dollar products do not. The number of pawn shops has grown 50% since the start of the Great Recession, after years of decline, with over 10,000 outlets in the U.S. currently. Rent-to-own has also grown dramatically since the onset of the Great Recession, with CNN Money estimating that about 9,000 U.S. stores earned about \$8 billion in 2012. The auto title loan market, whereby the borrower obtains a loan secured by a free-and-clear auto title, is also growing rapidly. The Center for Responsible Lending (2013) estimates that auto title borrowers pay about \$4 billion per year to borrow about \$2 billion. The average loan is about \$1,000, with the typical loan having a single balloon payment 30 days after origination.<sup>21</sup>

Incidence and prevalence both help motivate work on the debt collection industry. 35% of people with credit files in the U.S. have an account in collection, with an average amount of \$5,178 (Ratcliffe et al. 2014). Many of these accounts are owned by specialized third-party collectors that purchase debts from originators, or from other collectors. This market poses a wealth of unanswered questions on market structure, contracting, and the effects of collection

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balances is *earning* a mean balance-weighted APR of 5%, that would be \$400 billion in revenue for lenders and costs for borrowers.

<sup>18</sup> Payday loans are often “secured” by a post-dated check or ACH debit authorization, and the \$7.4 billion does not include overdraft or other fees incurred if there are insufficient funds in the account when the lender makes its claim.

<sup>19</sup> This volume of lending and borrowing costs is substantially depressed by regulations in several states that effectively prohibit payday loan contracts (Kaufman 2013).

<sup>20</sup> See also Skiba and Tobacman (2008).

<sup>21</sup> Other types of small-dollar markets are important in many developing countries—microcredit in its various forms, and various types of informal credit from friends, family members, social networks, and moneylenders/loan sharks.

practices and policies on consumer welfare (for an introduction see, e.g., Fedaseyeu and Hunt (2014)).

Zooming back to the constellation of loan product markets highlights several more open questions.<sup>22</sup> One is why the U.S. market is missing several “rungs” in the “lending ladder” between credit cards (which tend to top out around 30% APR for subprime borrowers)<sup>23</sup> and the triple-digit APR small-dollar products discussed in the previous paragraph. The gaps are all the more striking if one considers maturity as well as interest rate.<sup>24</sup> Another question is why contracts and usage varies so much across countries. Even products that are nominally the same (mortgages, credit cards, bank overdrafts, etc.) can have very different contractual features and pricing in different countries. Understanding what drives this—Regulation? History? Market conditions? Consumer preferences?—seems like a worthy focus for future research.<sup>25</sup> Yet another question is what explains the lack of a market for delegation: why don’t we have a “liability management” industry that helps consumers choose among the complex and expensive array of product options? This void is all the more puzzling in light of the evidence on consumer choice inefficiency catalogued in the next section.

### **III. Choice (in)efficiency in debt terms and levels**

Today’s credit markets present households with choices among hundreds of products and product variations offered by thousands of lenders. How well do households choose when borrowing? This section considers four different indicators of choice efficiency. The first two focuses on the downstream impacts of borrowing or debt relief on overall financial condition and subjective well-being. The second two focus on margins of cost minimization: ex-ante contract choice, and ex-post debt allocation.

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<sup>22</sup> This paragraph draws heavily from Zinman (forthcoming).

<sup>23</sup> The effective APR can be  $> 30\%$  for borrowers who incur penalty fees but is likely still strictly less than triple digits for nearly all borrowers.

<sup>24</sup> Maturities can be quite long for credit cards, which tend to be structured as open-end lines of credit with modest minimum monthly payments, and are nearly always quite short for payday loans (1-4 weeks).

<sup>25</sup> The Oxford-Harvard-Sloan Initiative is a new resource for researchers interested in International Comparative Household Finance.

### *A. Downstream impacts of credit access*

One way of measuring choice efficiency is by identifying the downstream effects of borrowing decisions on more-ultimate outcomes of interest like overall financial condition and subjective well-being. In a classical world, such analysis would be trivial in a qualitative sense—we would infer that borrowing makes borrowers weakly better off, in expectation, by revealed preference. But a gamut of behavioral possibilities makes it plausible that some—perhaps many—households “overborrow” (Section V).<sup>26</sup> To this end, a burgeoning literature seeks to identify the downstream impacts of small-dollar credit on U.S. borrowers. It finds mixed results: some studies find large positive impacts, others find large negative impacts, and others null effects (Zinman forthcoming).<sup>27</sup> Whether this pattern is due to true underlying heterogeneity (e.g., in samples or settings), or merely to heterogeneity in study quality, is difficult to discern at this point and vital to unpack going forward.

### *B. Credit as insurance and exercising an option to default*

A related question is how well people take advantage of opportunities to extricate themselves from problematic debt burdens. Defaulting on debt is a multifaceted option that is complicated to value (White 1998), particularly when unsecured debt is involved. In that case, a consumer faces choices regarding not just whether to default, but how. She can default informally (just stop paying and wait to see whether and how lenders pursue remedies), or file for bankruptcy protection. If taking the latter route there are decisions to make about how to file (which Chapter, etc.)

Dobbie and Song (2013) find striking evidence that bankruptcy protection leads to very strong increases in income and decreases in mortality and foreclosure, suggesting that consumers

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<sup>26</sup> Oft-overlooked is the possibility that even behavioral borrowers do better with expanded formal credit access, given that their counterfactual may be continued use of alternatives that are even more expensive and less flexible. See Zinman (forthcoming) for a discussion.

<sup>27</sup> A parallel literature takes the same approach with respect to microcredit in developing countries. Several randomized evaluations have found modestly positive results (Banerjee, Karlan, and Zinman 2014), with the one set of very encouraging results coming from the one lender studied thus far that explicitly makes consumer loans and does not target microentrepreneurs (Karlan and Zinman 2010). I am not aware of any study, in any setting, that convincingly identifies effects on inframarginal borrowers, which is a key consideration in environments where policymakers are considering restricting access to credit.

are getting it right to at least some important extent. But whether consumers default optimally remains an open question, and one that begs the broader questions how credit and insurance markets interact, and what those interactions imply for the design of debt forgiveness policy and practice. An emerging literature studies recent debt forgiveness interventions in developing countries,<sup>28</sup> and I hope we soon see more studies of similar activities in the U.S. (Agarwal, Amromin, et al. 2013; Gerardi and Li 2010).

### *C. Contract choice and price dispersion*

Another metric of choice efficiency is the extent to which, in a market with price dispersion, households choose deals at the cost-minimizing frontier.<sup>29</sup> Although not every paper described here takes the same approach, to fix ideas think of the key question as being: how much borrower-level price dispersion<sup>30</sup> persists after controlling for borrower credit risk and other product attributes (Stango and Zinman 2014b)?<sup>31</sup> A useful thought experiment is to consider two consumers with the same credit characteristics (e.g., credit score, LTV, etc.), taking up the same product (e.g., a 30-year fixed-rate mortgage). Do these two borrowers take loans with substantially different APRs? The answer seems to be “yes, often”, raising questions about the economic consequences of this heterogeneity for borrowers, and about how consumer heterogeneity interacts with lender competition to produce price dispersion in equilibrium.

Most of the work along these lines has focused on mortgages. Several papers find evidence suggesting that many millions of mortgagors pay hundreds or even thousands of dollars in

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<sup>28</sup> A handful of papers study recent debt forgiveness interventions in developing countries; see, e.g., Kanz (2013).

<sup>29</sup> One can also estimate the ex-ante efficiency of usage-contingent contracts from the same menu; see, e.g., Agarwal et al (2013).

<sup>30</sup> Other loan terms are interesting in their own right, including the choice of fixed vs. adjustable interest rate in the mortgage markets (Badarinza, Campbell, and Ramadorai 2013; Bucks and Pence 2008) and of course loan amount, including the decision of whether to borrow at all (Sections III-A and V; Cadena and Keys (2012)).

<sup>31</sup> Given the potential importance of non-price attributes, I focus on studies that examine choices within a product market. Other papers examine contract choice across product markets, with the goal of ascertaining whether users of small-dollar products might instead be able to access cheaper and more flexible mainstream products. Bhutta et al (forthcoming) find little evidence of this: people apply for payday loans when they have limited access to mainstream credit. Agarwal and Bos (2014) find a similar pattern for Swedish pawn borrowers.

markups they could avoid with a seemingly modest amount of additional shopping effort or sophistication at origination (for cites see Campbell et al (2011), and the last paragraph in this sub-section ). It also appears that many borrowers leave money on the table in comparison to an optimal refinancing benchmark (Agarwal, Driscoll, and Laibson 2013), with some evidence that errors of commission are somewhat common (refinancing too soon), and that errors of omission are particularly large (Agarwal, Rosen, and Yao 2012; Keys, Pope, and Pope 2014).<sup>32</sup>

In credit cards, Stango and Zinman (2014b) estimate a residual APR interquartile range of several hundred basis points, even after controlling for borrower credit risk and other card attributes. In student loans, there is mounting hue and cry about students being duped into taking inferior deals in the private market before maxing out their subsidized federal loan allocation, but I have not seen this quantified.<sup>33</sup>

What drives all of this dispersion? Stango and Zinman (2014b) point to heterogeneity in credit card pricing models coupled with heterogeneity in consumer search costs. Several mortgage papers point to some combination of uninformative sales tactics and shrouding that facilitate price discrimination (Agarwal and Evanoff 2013; Gurun, Matvos, and Seru 2014; Woodward and Hall 2012).<sup>34</sup> Charles et al (2008) find evidence suggesting that auto finance companies have price discriminated on race. Stango and Zinman (2011) find evidence suggesting that auto finance companies have used monthly payments marketing to price discriminate on a behavioral bias that induces underestimation of the shadow cost of borrowing (see also Section V). Agarwal et al (2009) find U-shaped patterns of cost minimization by borrower age. Agarwal and Mazmuder (2013) find evidence consistent with a role for numeracy. But much work remains to unpack why consumers seem to be so heterogeneous in their shopping behavior and efficiency (variation in experience, cognitive functioning, time costs, behavioral factors etc.), and how lender offer dispersion persists in equilibrium (standard search costs, shrouding, competition on persuasive sales tactics, etc.)

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<sup>32</sup> Those making serious errors of commission are prevalent enough to be categorized by participants in the mortgage-backed securities industry, as “woodheads”. See also Andersen et al (2014) on Denmark.

<sup>33</sup> The argument is that federal loans are both cheaper, and more flexible in repayment options, than private loans. (However, it is less clear that public loans dominate private loans in bankruptcy situations.) The modest penetration of the private student loan market—about 3 million borrowers—creates an upper bound on the current prevalence of this potential problem, although to be fair much of the policy concern is based on the potential rather than the actual size of the private market.

<sup>34</sup> See also Gine et al’s (2014) audit study in Mexico.

#### *D. Debt Allocation*

Ex-post debt allocation is another important margin of choice (in)efficiency. The question here is: given a set of debt contracts, how efficiently does the household minimize borrowing costs? The sharpest way to address this question is to look within a product class. This approach is particularly useful in the U.S. credit card market, where most households hold multiple cards with different APRs (Stango and Zinman 2014b), and theories abound for why people would not follow a strictly cost-minimizing strategy (Ponce, Seira, and Zamarripa 2014). Yet Stango and Zinman (2014b) find that nearly everyone allocates debt to their lowest-cost card, subject to credit limit constraints.<sup>35</sup>

Cross-product comparisons are much trickier due to non-price heterogeneity. For example, making monetized comparisons between using a home equity vs. credit card line of credit requires additional assumptions or data on risk preferences and expectations about future credit constraints. A more-scrutinized example is what I have dubbed “borrowing high and lending low”, a phenomenon brought to modern prominence by Gross and Souleles (2002b). But Telyukova (2013) shows that nearly all simultaneous revolving of credit card debt and holdings of highly liquid assets can be rationalized by transaction frictions.<sup>36</sup> Becker and Shabani (2010) consider a broader set of assets and liabilities, taking into account the proposition that the return to paying down debt is typically strictly greater than the (risk-free) return on investing in assets, and find little inefficiency.

All told, it seems that the weight of the available evidence leads to a pair of striking inferences: households are remarkably efficient and homogenous at allocating debt across the contracts they already have, but remarkably inefficient and heterogeneous in choosing those

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<sup>35</sup> This data is from 2006-2008 and includes the balance transfer problem studied in Agarwal et al (2009) and Agarwal and Mazmuder (2013), which was much narrower in scope—it affected those who transferred a balance, in full, and then made new “convenience” purchases that they could float on the old card, during the teaser-pricing period of the balance transfer. The problem stemmed from the practice of “negative payment hierarchy”, whereby issuers not only priced new purchases strictly higher than the transferred balance during the teaser period, but also applied any repayments to lower-APR balances first. The CARD Act has largely outlawed negative payment hierarchy. Ponce et al (2014) find substantially less cost minimization in Mexico.

<sup>36</sup> Another way of seeing that even small differences in liquidity between credit cards and checking accounts could reconcile the apparent puzzle is to note that most U.S. households probably run incredibly “lean” in their checking accounts. Stango and Zinman (2014a) find that 83% of account-months have a minimum balance < \$100. See Section IV-C for some related evidence on liquidity constraints.

contracts. More work is needed to unpack and whether and why this is the case; Stango and Zinman (2014b) offer some speculation.

#### **IV. Evidence on modeling inputs: Intertemporal prices and substitution**

This section provides and reviews empirical evidence on several key inputs to models of household debt, starting with intertemporal prices and then turning to various measures of, and motives for, intertemporal substitution.

##### *A. Intertemporal prices*

The price of consumption (return on investment) is a key parameter in most models of intertemporal choice. Household debt affects this price when there is a wedge between savings yields and borrowing costs. Following the discussion of portfolio choice at the level of the household balance sheet in Section III-D, it bears reiterating that, in a proximate sense, the price of consumption or investment is *not* necessarily dictated by a risk-free rate of return on a financial asset (I dub this the “traditional” risk-free rate) . It may instead be determined by a strictly higher risk-free rate of return from paying down debt (the “true” risk-free rate).

So how many households face a true rate that exceeds the traditional rate? And how big are the wedges? Figures 2a and 2b present some new summary tabulations, based on 2010 Survey of Consumer Finances data, that take each household and assign it a marginal cost based on  $\text{Max}\{30\text{-year Treasury rate, Max}(\text{interest rate on outstanding debt})\}$ . Figure 2a shows that an estimated 70% of U.S. households are borrowing at an interest rate  $>$  traditional risk-free rate, with about 25% facing a true shadow cost of at least 10%. Figure 2b attempts to incorporate credit constraints and hence is more accurate in my view. Here I take those who report being credit-rationed or discouraged from applying and place them in the uppermost bin ( $\geq 19\%$ ). This leads to about 75% of households facing a true rate  $>$  the traditional rate, with about 45% facing an interest rate of at least 10%.

Figure 3 decomposes the source of the shadow cost for the (would-be) borrowers in Figure 2b. 31% of borrowing households are classified based on being rationed and/or discouraged from

applying for credit. 22% and 11% have their price determined by credit cards or payday loans (these are almost certainly lower bounds, as discussed in the next paragraph). 30% have their price determined by secured credit, with over half of these due to a 1<sup>st</sup> mortgage.

Three measurement issues suggest that these Figures understate the true mass at the extremes. One is the SCF's incomplete coverage of small-dollar borrowing.<sup>37</sup> Two is the substantial underreporting of unsecured, expensive debt in surveys (Zinman 2009; Zinman 2010).<sup>38</sup> Three is that I use pre-tax interest rates that do not take into account the favorable tax treatment on mortgages for those who itemize. This likely affects a nontrivial but certainly not enormous set of households; e.g., Figure 3 estimates that only 17% of *debtors* have their opportunity cost determined by a first mortgage.<sup>39</sup>

In any case, the results here and elsewhere (Becker and Shabani 2010; Chetty and Szeidl 2007; Davis, Kubler, and Willen 2006) suggest that it is perilous for intertemporal choice modelers to ignore household borrowing costs and the heterogeneity therein. Many households face high, risk-free returns to paying down debt as a consequence of their earlier borrowing decisions. Conversely, household borrowing decisions also reveal something about (perceived) rates of return to consumption smoothing, human capital investment, or other types of investment. They must be quite high for many households to justify the prevalence of high-cost borrowing. But life-cycle modelers have yet to determine a specification of perceived rates of return that reproduces the actual extent of high-cost borrowing in steady-state (Angeletos et al. 2001; Carroll 2001). This is the credit card debt puzzle I mentioned at the outset, but it presumably also applies to other long-term or serial borrowing in products like subprime auto or small-dollar loans.

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<sup>37</sup> FINRA's National Financial Capability Study offers better coverage, and estimates that, as of 2012, about 30% of U.S. households had used an expensive small-dollar product within the previous five years: <http://www.usfinancialcapability.org/>.

<sup>38</sup> Much of the underreporting is on the extensive margin, for small-dollar loans. This could lead to substantial underestimation (several percentage points) of the proportion of households in the highest-cost bin. See also Karlan and Zinman (2008a) on underreporting in South Africa.

<sup>39</sup> Given non-borrowers, non-itemizers, and those borrowing at post-tax rates > risk-free, I would guess that Figures 3a and 3b understate the prevalence of households facing the Treasury rate by about 5 percentage points or so.

## *B. Why do households borrow?*

What generates high perceived returns to household debt: why do households borrow? Empirical evidence on this questions yields clues about potential solutions to the puzzlingly high prevalence and level of high-cost debt, and informs modeling decisions about how to specify borrowing motives. E.g, in Guerrieri and Lorenzoni (2011) borrowers smooth transitory income fluctuations, and in Eggertsson and Krugman (2012) borrowers are less patient than savers. I suspect that substantial amounts of household borrowing are financing investment in closely held businesses (Robb and Robinson forthcoming), suggesting a potential link to the private equity premium puzzle (Moskowitz and Vissing-Jorgensen 2002).

Given the size of the mortgage and auto loan markets, it is tempting to infer that most marginal spending must be on durables (and human capital, in the student loan case). But given the fungibility of money, and collateral constraints, this need not be the case. Moreover, much of the puzzling debt is in unsecured credit markets. There is little direct evidence on where households spend their marginal dollars of debt financing,<sup>40</sup> and circumstantial evidence paints a muddled picture of the importance of different motives.<sup>41</sup>

## *C. Credit constraints: Measurement and interpretation*

The continued prevalence of credit constraints (Figures 2b and 3) is noteworthy and somewhat puzzling in its own right: for all of the advances in risk-based pricing, mechanism design, nonlinear contracting etc., prices are still quite far from clearing consumer credit markets! This seems true, at least episodically, even in secured credit markets; see, e.g., Bhutta (2013) on the mortgage origination slowdown during the Great Recession.

Measuring credit constraints precisely remains a challenge—should we focus on marginal borrowing costs, rejected applications, self-reports of discouragement, high credit line utilization,

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<sup>40</sup> See Karlan et al (2014) for a detailed discussion, and various approaches to identification applied in the Philippines.

<sup>41</sup> In addition to the studies of durables markets discussed above, note also the studies of downstream impacts (many of which are consistent with transitory smoothing, or overborrowing), and studies of interactions between borrowing and social insurance (Sullivan 2008; Hsu, Matsa, and Melzer 2014)

the (in)ability to come up with a substantial amount of liquidity to deal with emergency,<sup>42</sup> and/or something else? How should we account for the possibility of buffer stocks in unused credit lines?

Besides direct elicitation, researchers continue to make progress in describing liquidity constraints by inferring them from behaviors. For examples focused on borrowing responses following supply shocks, see Adams et al (2009), Gross and Souleles (2002b), and Mian and Sufi (2011a). An oft-overlooked but informative approach is to estimate maturity elasticities: strong ones are consistent with binding liquidity constraints (Attanasio, Goldberg, and Kyriazidou 2008).<sup>43</sup> The most popular approach to identifying constraints relies on tracking spending responses to income changes (Jappelli and Pistaferri 2010). Some particularly striking findings from this literature come from papers that have data to directly examine borrowing responses in the wake of income shocks. Agarwal et al (2007) find that, following the 2001 federal income tax rebates, the average consumer first paid down credit card debt, but soon afterward increased her spending, leading to a net increase in spending of about 40% of the rebate amount. High-utilization and low-limit cardholders increased their spending, while debt repayment (savings) rose for plausibly less-constrained cardholders.<sup>44</sup> Aaronson et al (2012) find that an increase in the minimum wage has a multiplier effect on spending that is driven by a small number of households making large, debt-financed automobile purchases. Several papers find that the magnitude of spending declines during the Great Recession increases with ex-ante household leverage (Baker 2014; Mian, Rao, and Sufi 2013; Mian and Sufi 2014a).

But we still have a long way to go to understand the normative impacts of credit constraints. In the not too-distant past, prevalent credit constraints would have been interpreted as a sign of inefficiency—most often, as a symptom of undersupply borne of asymmetric information problems. These days, we approach credit constraints with more nuance and ambivalence, in light of various theories of overborrowing (Section V). Credit constraints can be a good thing, if behavioral tendencies, externalities, and/or certain varieties of asymmetric information lead

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<sup>42</sup> In the 2009 TNS Global Economic Crisis survey nearly one-half of Americans report being certainly or probably not able to come up with \$2,000 in 30 days to deal with an unexpected shock (Lusardi, Schneider, and Tufano 2011).

<sup>43</sup> See also Karlan and Zinman (2008b) for evidence from South Africa.

<sup>44</sup> See also Leth-Petersen (2010) on Denmark, and Agarwal and Qian (2014) on Singapore.

markets to produce too much debt. The normative properties of credit constraints may even vary over time: desirable, in a 2<sup>nd</sup>-best sense, in steady-state, but disastrous in certain crises (Zinman forthcoming).

#### *D. Demand Elasticities*

Given the prevalence of liquidity constraints and high (perceived) internal rates of return, one might expect low price sensitivity in consumer credit markets. In fact the evidence, while limited, sketches a more muddled picture. DeFusco and Paciorek (2014) find nearly zero price sensitivity in mortgage originations. Attanasio et al (2008) find a similar lack of price sensitivity in auto loans. However they find that demand increases strongly with maturity, Adams et al (2009) finds that demand increases strongly with lower downpayments in the subprime car market, and several papers find strong elasticities with respect to available loan amounts (D. B. Gross and Souleles 2002b; Dobbie and Skiba 2013; Mian and Sufi 2011b). Altogether these findings are strongly suggestive of binding liquidity constraints that depress the price sensitivity of demand. Yet other findings suggest (nearly) elastic price responses: Gross and Souleles (2002b) in credit cards,<sup>45</sup> and Bhutta and Keys (2013) in home equity lines. Reconciling these different estimates will likely require better measurement of total (net) borrowing and estimates of longer-run elasticities.<sup>46</sup> It may also be important to account for the presence or absence of a durable purchase that is tied to the financing, and/or for the possibility that sensitivity varies nonlinearly with stakes.<sup>47</sup>

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<sup>45</sup> Ponce et al (2014) find elasticities of similar magnitude to Gross and Souleles, in the Mexico credit card market. But Alan and Loranth (2013) find little sensitivity to price increases in U.K. credit cards, and Karlan and Zinman (2008b) find very strong price sensitivity to a price increase in a South African small-dollar market.

<sup>46</sup> Karlan and Zinman (2014) take this approach, in the Mexico microcredit market.

<sup>47</sup> E.g., by incorporating optimization frictions a la Chetty's work on labor supply (see Chetty (2012) and cites therein).

## V. How to explain the facts? Theories and theory-testing

How do we explain the litany of facts presented above? There is a shortage of unifying explanations but no shortage of theories that focus on important pieces of the puzzle. This section provides very brief overview of key classes of theories and theory-testing that draws heavily on Zinman (forthcoming), which provides a more detailed review.

One way to make sense of theoretical work on household debt is to organize our thinking around what I take to be the two most important threshold questions animating research and policy: do consumer credit markets produce efficient allocations? Why or why not?

Three classes of theories flesh out mechanisms that lead to under-supply. One is bad-old-fashioned market power. There is little evidence that plain-vanilla market power is important, but the evidence on price dispersion (Section III-C) is consistent with lenders enjoying market power due to search and/or switch costs (see also Knittel and Stango (2003) on tacit collusion). Whether this leads to over- or under-supply on net remains to be identified, and presumably depends on demand elasticities and more-primitive parameters. E.g., search and switch costs arising from behavioral factors (I don't shop because I procrastinate) could indicate over-supply rather than under-supply (Section V). A second is regulatory failure. E.g., it may be that missing rungs in the lender ladder are due to state regulations that outlaw the very products that would fill the gaps. I have heard market participants make this argument, but have yet to see a comprehensive accounting of the relevant laws or a convincing analysis of the impacts of the laws on entry.<sup>48</sup>

The third class of theory of under-supply is asymmetric information that produces credit rationing a la Stiglitz and Weiss (1981). There are actually multiple classes here, for different varieties of asymmetric information: hidden information, hidden action, and interactions between the two (principally, selection on moral hazard). These theories have helped motivate countless interventions to increase credit supply—subsidies, guarantees, direct lending, etc.-- and empirical evidence on whether, how, and how much asymmetric information actually affects market

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<sup>48</sup> Generally speaking it does seem to be the case that state laws restricting high-cost consumer loans from *non-bank* providers do have teeth (Kaufman 2013; McKernan, Ratcliffe, and Kuehn 2013). The bank vs. non-bank distinction is legally significant because the *Marquette* Supreme Court decision in 1978 upheld the ability of national banks to “export” rates from states with more favorable regulation to states with less favorable regulation. And the distinction is economically significant because banks have tended to stay out of product markets with APRs higher than credit cards, presumably because they are discouraged from doing so by their supervisors/regulators. (The one exception is checking account-linked products like overdraft and cash advance, which are viewed differently by the law and by bank supervisors for various reasons, some of which are more framing than substance.)

outcomes is finally catching up to theory and practice. Examples include Adams et al (2009) and Dobbie and Skiba (2013) on subprime auto and payday loans, several papers on mortgage securitization (e.g., Keys et al. 2010; Bubb and Kaufman 2014), and a growing literature on strategic default by mortgagors (e.g., Mayer et al. forthcoming).

Several classes of theories flesh out mechanisms that can produce too much debt. Advantageous selection (de Meza and Webb 1987; 2000) and moral hazard under nonexclusive contracting (Bisin and Guaitoli 2004; Degryse, Ioannidou, and von Schedvin 2012; Bizer and DeMarzo 1992; Goldstein and Razin 2013) can both fit the bill. Einav et al (2012) find some evidence consistent with the former, and Dobbie and Skiba (2013) with the latter. Distressed home sellers may also generate “location, location, location” negative externalities if more debt leads to more foreclosures, foreclosures destroy neighboring property value(s) as well as own-property value, and loan contracts do not internalize this externality (Campbell, Giglio, and Pathak 2011). Various fire sale models can generate too much debt in the sense that liquidity constrained sellers exert negative externalities on asset prices (e.g., Shleifer and Vishny 1992; Brunnermeier 2009). Mian and Sufi (2014) find evidence consistent with a fire sale channel effect of foreclosures.

It is also important emphasize that fire sale models generate too much debt “only” in a constrained/2<sup>nd</sup>-best sense. The negative externality materializes only because there is actually *too little debt* at critical junctures, when a shock makes it such that asset markets lack sufficient liquidity to clear at fair prices.

Two new classes of model have a similar property. One shows how deleveraging can slow recovery from an exogenous macro shock given monetary policy that is constrained by a zero bound (Eggertsson and Krugman 2012; Hall 2011). So debt levels can be too high conditional on there being a bad shock that induces substantial deleveraging, and conditional on frictions that make some agents liquidity constrained. Remove the liquidity constraints (or mute the surprise, perhaps by mitigating asymmetric information) and high leverage is unlikely to exacerbate downturns (Mian, Rao, and Sufi 2013). Mian and Sufi (2011b; 2012) also find empirical evidence consistent with these models, although see Justiniano et al (2013) for a contrary view based on results from a quantitative dynamic general equilibrium model.

Another class of model shows that near-frictionless mortgage refinancing can lead to systemic risk by increasing homeowner leverage as real estate values appreciate without the ability to symmetrically decrease leverage as collateral values decline (Khandani, Lo, and Merton 2012). Bhutta and Keys (2013) find empirical support for the model. Note that both of the frictions generating inefficiency in the model—collateral indivisibility and the lack of an equity market for homeowners-- could, in principle, be mitigated with financial innovations that addressed the underlying source of the constraints (moral hazard?). So again we have a model that has too much debt entering a downturn, and too little debt/liquidity during the downturn. A critical question for welfare and policy analysis is whether we should take those liquidity constraints—frictions that bind, at the very least, during times of crisis-- as given.

The puzzlingly high equilibrium debt burdens and interest rates discussed in Section II, along with strong responses to plausibly uninformative stimuli like reminders (Stango and Zinman 2014a) and some advertising (Bertrand et al. 2010) have helped motivate the growing body of work posits that consumers are “behavioral” in ways that predispose them to over-borrow (under-save) relative to some benchmark. What is the benchmark? I focus on models where consumers have some *bias* that can lead to excessive borrowing.<sup>49</sup> So the benchmark is “unbiased”, and hence often neoclassical.

What sorts of behavioral biases are thought to matter, and how do economists model them? One way of understanding behavioral economics, methodologically speaking, is as a specification problem in an otherwise standard economic model. The types of pieces are standard, but even sometimes suggests that we should consider shaping them differently. Preferences may be time-inconsistent (Heidhues and Koszegi 2010; Laibson, Repetto, and Tobacman 2003; Meier and Sprenger 2010), price perceptions may tilt toward making borrowing look deceptively cheap (Bertrand and Morse 2011; Gabaix and Laibson 2006; Stango and Zinman 2009), expectations about various future parameters may tend toward optimism (M. Brunnermeier and Parker 2005; Hyttinen and Putkuri 2012; Mann 2013), and decision rules (i.e.,

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<sup>49</sup> In contrast, a lack of knowledge (e.g., a lack of financial literacy) seems to me unlikely to produce overborrowing in the absence of biases. I suspect that a rational actor who lacks knowledge will in some cases get her debt level right on average (making mistakes in both directions that cancel out), and in other cases borrow less if a risk associated with borrowing leads her to opt-out of the market, a la Calvet et al (2007) on financial asset markets.

whether/how someone solves a problem, conditional on parameter values) may rely on crude heuristics or vary with attention shocks (Stango and Zinman 2014a).

Why don't the standard forces of competition, delegation, and/or learning mitigate or neutralize the effects of any behavioral biases? A growing literature models how behavioral consumers contract with sophisticated firms, and finds equilibria where firms profit from exploiting behavioral consumers rather than helping them overcome their biases.<sup>50</sup> Casual empiricism suggests that the advice market for liabilities is limited in scope, of dubious quality, and interacts in interesting ways with low consumer willingness to pay for unbiased advice.<sup>51</sup> Opportunities for learning from one's own experience may be limited; e.g., many households obtain a mortgage only at decennial frequencies. And new theories suggest that consumers may not learn about their biases even when faced with ample opportunities to do so (Ali 2011; Benjamin, Rabin, and Raymond 2013; Eil and Rao 2011; Schwartzstein forthcoming). Social learning can produce herding and inefficient equilibria (Banerjee 1992; Eyster and Rabin 2010).

Links between behavioral biases, equilibrium contracts, and consumer debt levels are intriguing but remain largely speculative. Overall the work is characterized by bias-/model-proliferation, and a lack of empirical work testing distinct testable predictions of one or more of the behavioral explanations.

Zooming back out to the eight classes of theory, there is some evidence for each class, but no consensus on which class of model(s) is most descriptive. We still do not have a clear sense of whether consumer credit markets produce too much, too little, or just the right amount of debt.

## **VI. Research and policy: Some key issues**

With the advent of the Consumer Financial Protection Bureau and increased activity by other federal and state actors in the wake of the financial crisis, policy-focused research on household debt is a growth stock.

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<sup>50</sup> See also Ellison (2006) and DellaVigna (2009) for reviews of what is variously referred to as behavioral, or boundedly rational, industrial organization.

<sup>51</sup> There has been more work estimating the quality of financial advice on the asset side of the household balance sheet, and the results are not encouraging. See, e.g., Malkiel (2013) and Inderst and Ottaviani (2012).

Policy approaches to consumer credit markets befit the unresolved tensions described in the preceding section, at least when it comes to interventions that seek to directly affect prices and/or quantities. Some seek to expand access; subsidies or guarantees are common levers to this end.<sup>52</sup> Others seek to restrict access; price caps and restrictions on underwriting criteria are common levers to this end.<sup>53</sup> As discussed above, evidence on the effects and welfare implications of these sorts of interventions is limited, and mixed.

The other traditional policy pillar in consumer finance is mandated, point-of-sale disclosure. Heavy reliance on this approach has come under fire for various reasons, including enforcement costs (Stango and Zinman 2011) and behavioral factors (Loewenstein, Sunstein, and Golman 2014). Nevertheless there is some evidence that certain disclosures—both traditional and nontraditional—may mitigate behavioral biases (Bertrand and Morse 2011; Stango and Zinman 2011), suggesting that further research and development are vital.

A third policy pillar is emerging: promoting financial literacy. Yet despite increasingly abundant evidence on correlations between measures of literacy and financial outcomes, evidence on causality is thin (see Lusardi and Mitchell (2014) for a more bullish review). Nor is there strong evidence showing that programmatic or policy interventions can reliably increase financial literacy effectively, much less cost-effectively (Hastings, Madrian, and Skimmyhorn 2013; Fernandes, Lynch, and Netemeyer 2014). Having said that, I suspect there are opportunities to build and test bridges between literacy-promotion and the other two pillars. Classical work on disclosure equilibria (Milgrom 2008) suggests that some oft-overlooked components of financial literacy could be particularly (socially) beneficial: awareness of the distribution of supplier quality, and “skepticism” or “wariness” of firm incentives (Dranove and Jin 2010). Behavioral work suggests yet more oft-overlooked components of financial literacy: meta-awareness of one’s biases or tendencies to overborrow, and awareness of the many new

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<sup>52</sup> Examples here include interest rate subsidies on student loans, implicit price subsidies on non-jumbo mortgages via the secondary market activities of Fannie Mae and Freddie Mac, and Federal Housing Authority mortgage guarantees.

<sup>53</sup> Examples here include many state-level provisions and the federal Military Lending Act re: small-dollar credit (see footnote 46), the CARD Act’s limitations on credit card fees, and CFPB’s recent amendments to the Truth in Lending Act that affect mortgages by limiting prepayment penalties and requiring lenders to make a good-faith determination of an applicant’s ability to repay.

products that are designed to mitigate them (commitment contracts, behaviorally-informed personal financial management software, etc.)

Policy researchers face several fascinating challenges. Deriving optimal contracts and related policy responses—there are particularly active literatures regarding mortgages, and bankruptcy provisions—typically requires assumptions about many of the contested issues in Section V. (Is there asymmetric information, and of what variety? Are consumers behavioral, and in which way or ways?) Enforcement of a given rule can vary idiosyncratically (Agarwal, Lucca, et al. 2014; Stango and Zinman 2011). Regulators may even have discretion over what the rules actually are—I have taken to calling this “shadow regulation”-- due to principles-based regulation (like “UDAAP”)<sup>54</sup> and a political climate where regulatory or supervisory threats are credible. Empirical evaluation of federal policy changes is further hindered by the usual shortcomings of event studies, including the possibility of anticipatory effects and the likelihood of low power (due to the Moulton problem and serial correlation).

In short, it seems probable the researchers will be faced with a proliferation of policy changes that offer the possibility of natural experiments. But it seems even more likely that substantial methodological and substantive progress will be required to improve the evaluation and design of consumer financial policies.

## **VII. Conclusion**

Household borrowing choices and markets offer an abundance of fascinating facts and puzzles. I close by recapping some particularly fruitful questions for further research. Many of these sit at intersection with other subfields like industrial organization, contract theory, behavioral economics, and macroeconomics, and highlight the potential for gains from trade across literatures.

Persistently high debt levels of expensive unsecured debt—whether in credit cards or small-dollar credit products-- remain confounding for any extant model. The evidence on choice inefficiency suggests that a good chunk of the unexplained portion could be due to overpaying

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<sup>54</sup> UDAAP === Unfair, Deceptive or Abusive Practices. See, e.g., Farrell (2013).

for debt rather than overspending on consumption per se (Stango and Zinman 2014b), but this hypothesis remains to be tested. I suspect that there are similar puzzles in secured debt markets, perhaps due to bundling with durables purchases that induces people to overpay and hence overborrow. There also appears to be a puzzle in how different choice (in)efficiencies fit together: it seems that households leave substantial money on the table when choosing contracts, including whether and how to refinance, yet allocate their balance sheet efficiently conditional on their existing set of contracts.

The extent of household choice inefficiencies also raises questions about the apparent low penetration and low quality of the advice market for household liabilities. Why don't third parties help households make better choices and share in the savings?

The long-run upward trend and recent explosion of consumer debt also present puzzles. It seems likely that innovations in contracting and persuasion have made significant contributions. But the nature, extent, and implications of these changes are not yet well understood. Nor is it clear why contracts (and sales tactics?) seem to vary so much across countries. Do lending technologies actually vary substantially across settings, and if so why?

It is also important to wrestle with the fact of prevalent credit constraints. E.g., I estimate that 75% of U.S. households face an opportunity cost of consumption or investment that strictly exceeds a risk-free of return on a financial asset. Is this a symptom of too much debt, where behavioral factors and/or externalities lead people to overborrow? Or is this a symptom of too little debt, where asymmetric information, market power, and/or regulation create holes in the lending ladder and rationing on the margins of its rungs? More broadly, much work remains to refine and test theories that shed light on whether and why markets (do not) produce optimal allocations of credit (Zinman forthcoming). Questions about how credit and insurance markets interact are also important and understudied, along with their implications for the design of debt forgiveness practices and policy.

Another important question is whether and how credit constraints co-exist with strong price sensitivities of demand. If opportunity costs of consumption are indeed high, yet people still want to borrow, that suggests high private rates of return to borrowed money. But if this is the case then why would people be price elastic on the margin? Are we uncovering information

about the true underlying distribution of (perceived) internal rates of return, about some behavioral factor that makes price sensitivity highly context-specific, or something else?

Recent history and the current regulatory climate suggest that policy and programmatic changes will continue apace, at various levels of government and practice. Using these changes as natural experiments will require methodological progress in calculating standard errors, identifying anticipatory responses by lenders, and account for limited enforcement and various types of regulator discretion. Feeding evidence back into policy and program design will require continued interplay between theory and empirics that wrestles with the incomplete and often conflicting evidence on why, how, how much, and how well households borrow.

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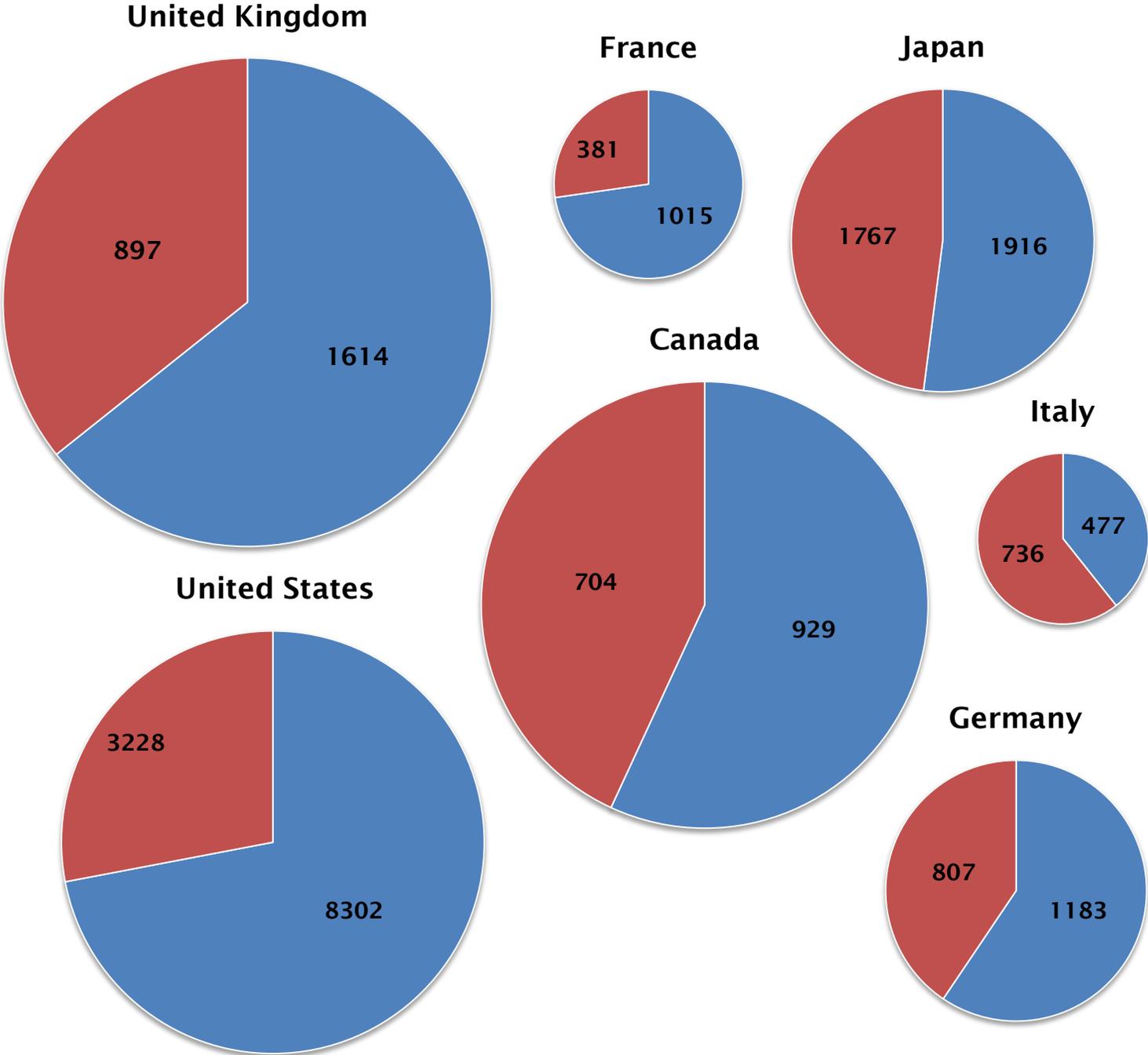
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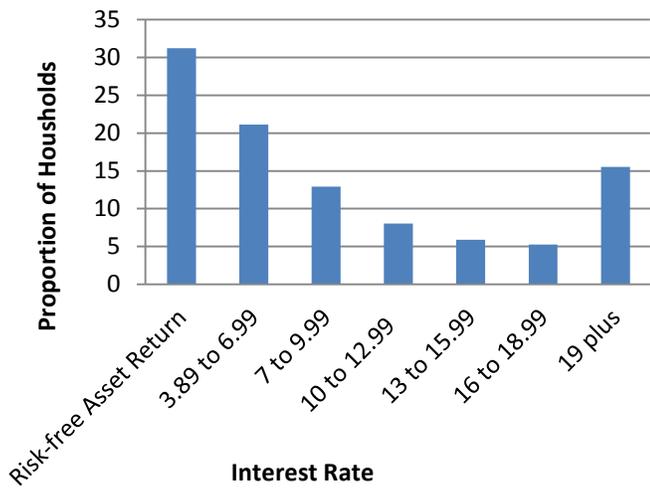
# Figure 1. Household Debt in G7 Countries



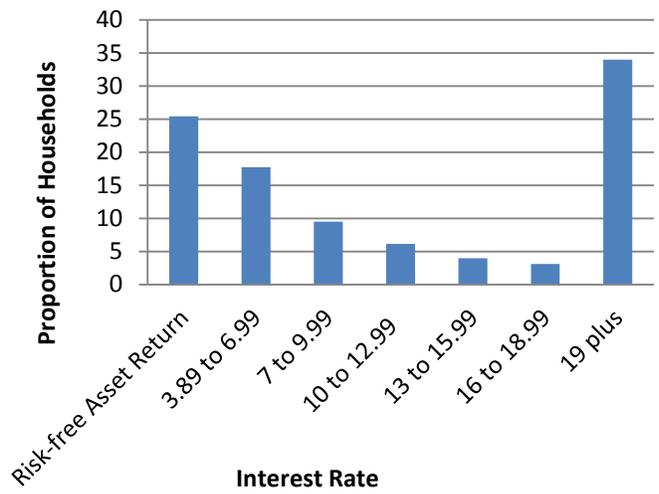
■ Household debt due to mortgage in billions of USD  
■ Other household debt in billions of USD

Scaled by household debt to GDP ratio  
*Data from years 2010 and 2011*

**Figure 2a. Opportunity Cost of Consumption/Investment v1**



**Figure 2b. Opportunity Cost of Consumption/Investment v2**



Based on weighted data from the 2010 Survey of Consumer Finances. Risk-free asset return of 3.88% is the yield on 30-year treasuries as of July 1, 2010 (annual average return on money market funds in 2010 was 0.04%). In v1, interest rates greater than the risk-free asset return are the rate on the household's most expensive debt if they have debt. In v2, households that report being rationed or discouraged from applying are in the 19+ bin.