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TOO MUCH OR TOO LITTLE (OR JUST RIGHT)?

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ABSTRACT

The intersection of research and policy on consumer credit often has a Goldilocks feel. Some researchers and policymakers posit that consumer credit markets produce too much credit. Other researchers and policymakers posit that markets produce too little credit. I review theories and evidence on inefficient consumer credit supply. For each of eight classes of theories I sketch some of the leading models and summarize any convincing empirical tests of those models. I also discuss more “circumstantial” evidence that does not map tightly into a particular model but has the potential to shed light on, or obscure, answers to key questions. Overall there is a lack of convincing evidence on whether markets err, and in which direction. We do not yet understand whether and under what conditions markets over-supply or under-supply credit, much less why.

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I. Introduction

The intersection of research and policy on consumer credit often has a Goldilocks feel.² Some researchers and policymakers posit that consumer credit markets produce too much credit. Other researchers and policymakers posit that markets produce too little credit.

Do markets actually over- or under-supply credit? If yes, to what extent, and what is the underlying source of the inefficiency- the underlying “market” failure (which, to be fair, also include the possibilities of consumer decision, and policy, failures)? These are vital threshold questions for policymaking. Without convincing answers we lack motivation for seriously considering policy interventions, and we lack guidance for designing policy that might actually improve outcomes and hence be worth subjecting to rigorous cost-benefit analyses. Yet empirical evidence on these threshold questions is thin, and mixed. Perhaps more to the point, links between theory, empirics, and welfare analysis are even thinner.

This paper reviews theories and evidence on inefficient consumer credit supply. I sacrifice depth for breadth and acknowledge the likelihood that my review may still lack some breadth. I focus mostly on the U.S. and its largest markets—mortgage, auto, student, credit card, and “small-dollar” (principally payday and bank overdraft)-- but reference other settings and markets opportunistically. I do not cover the very short-term loans that are more the province of literatures on money and payments. Nor do I explicitly consider lending to small businesses or entrepreneurs, although the considerable overlap between these agents and “consumers” seems noteworthy: most enterprises are closely held, particularly in their early stages. Indeed, there is evidence that “consumer” credit is often used to finance business activities, while “commercial” or “microenterprise” credit is often used to finance household activities.

I start with three classes of theories of credit *under-supply*. Two are canonical: market power, and regulations outlawing products that would be welfare-enhancing. The third is only slightly less canonical: varieties of asymmetric information that can lead to rationing.

Next I consider five classes of theories of credit *over-supply* (with “over-supply” being loosely defined in some cases).³ The first considers varieties of asymmetric information that can

² A rhetorical nod to Gordon and Stock (1998).

lead to over-supply: advantageous selection, and moral hazard under competition (common pool problems). A second class of theories focuses on externalities in the markets for assets used to collateralize borrowing. A third class focuses on systemic risk introduced by collateralized borrowing in concert with other factors. A fourth focuses on how deleveraging can slow or halt recoveries if initial debt loads are high enough (arguably too high in some sense). A fifth focuses on consumers who are “behavioral” in some way that makes them psychologically/cognitively predisposed to borrow “too much”.⁴

For each class of theory I sketch some of the leading models and summarize any convincing empirical tests of those models. I also discuss more “circumstantial” evidence that does not map tightly into a particular model but has the potential to shed light on, or obscure, answers to the threshold questions.

Speaking of circumstantial evidence, I also review the growing body of reduced-form evidence on the impacts of credit supply shocks on consumers. This evidence is clearly related to the threshold questions in the sense that it addresses the question of whether more or less access to credit makes people better or worse off. But mapping this evidence into policy and welfare analysis is complicated by possible non-linearities, coarse diagnosis, and equally coarse policy treatments with limited enforcement.

³ As I discuss below, many models of over-supply also have a key element of under-supply. More precisely, they generate over-supply in the 2nd-best sense that, conditional on some friction that produces binding credit constraints in some important state of the world (e.g., an economic crisis), we can end up with too much credit entering that state of the world.

⁴ I should also mention government subsidies, which have a long history of being blamed for falsely operating under the assumption of under-supply, and instead inducing over-supply. In U.S. consumer credit the main subsidies are for mortgages (via guarantees, and the interest deduction) and student loans. Interestingly, Hoff and Stiglitz (1998) show that subsidies can lead to higher prices and undersupply when there is monopolistic competition with endogenous enforcement costs. To my knowledge there is little consensus on the direction or magnitude of any distortion, although Gale (1991) and Glaeser et al (2010) find that subsidies induce nontrivial excess/inefficient supply in U.S..

II. Theories of Under-Supply

A. Market power

The major consumer credit markets in the U.S. are each served by thousands or even tens of thousands of lenders.⁵ For the most part significant entry restrictions have been absent for two decades or more.⁶ I am not aware of any compelling evidence that lenders earn excess risk-adjusted profits. In brief, I doubt that any sort of plain-vanilla market power meaningfully affects equilibrium quantities in the mortgage, auto, credit card, or student loan markets.⁷ One exception may be regulation-induced barriers to entry in the small-dollar credit market; e.g., Melzer and Morgan (2012) find that banks reduce overdraft credit limits (but also fees) when state laws restrict payday loans. See the next sub-section for further discussion of legal restrictions.

There is however mounting evidence of substantial price dispersion in consumer credit, conditional on product and consumer (risk) characteristics (see Section III-B). These patterns are consistent with lenders enjoying market power due to search and/or switch costs. Whether this leads to over- or under-supply on net remains to be identified, and presumably depends on demand elasticities and more-primitive parameters (see Section III-A).

B. Regulatory Failure

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)⁸ and payday loans or bank overdrafts (which tend to cost \gg 100% APR). The gaps are particularly striking if one focuses on *unsecured* credit (ignoring pawn and auto title loans), and all the more striking if one

⁵ The one exception is the student loan market, where presently direct and subsidized lending by the government dominates.

⁶ See Campbell (2006) for a review of the literature on credit market competition, including the extensive literature on the effects of U.S. banking dregulation.

⁷ I am quite open to the possibility, however, that this hypothesis is only true, or more true, in steady-state. E.g., it would be interesting to explore whether there has been recent (geographic) variation in financial institution failure or distress rates that could be used to test how competition affected prices, quantities, and profits during- and post-crisis.

⁸ The effective APR is $> 30\%$ for borrowers who incur penalty fees, but almost certainly still strictly less than triple digits for nearly all borrowers.

considers maturity as well as interest rate.⁹ I have yet to see a convincing analysis of the missing rungs.

One possible explanation for gaps in the lending ladder is 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.¹⁰

Another possible explanation for the missing rungs is asymmetric information. The recent spate of “big-data”-driven entrants seeking to fill the gaps seems consistent with some sort of asymmetric information problem. Forecasts of the extent to which such technological innovations, broadly defined, will change credit supply are important inputs for policy analysis.¹¹

C. *Asymmetric Information*

i. Overview

Asymmetric information has been invoked for decades as a rationale for interventions to expand credit supply, in the wake of Stiglitz and Weiss’ (1981) seminal theory of credit rationing. Yet the impact of information theory on credit policy and practice far outstrips empirical evidence on whether, how, and how much asymmetric information actually affects market outcomes. The *how* is particularly important given that different varieties of asymmetric information can have very different effects (with some even producing *over-supply*), and optimal remedies (e.g., subsidies might improve efficiency if there is adverse selection but not if there is moral hazard).

⁹ 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).

¹⁰ Generally speaking it does seem to be the case that state laws restricting high-cost consumer loans from *non-bank* providers do have teeth (Carrell and Zinman 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.)

¹¹ For some related work see, e.g., Grodzicki (2012) and Einav et al (2013).

ii. Theories

Models typically consider one (rarely more) of three varieties of asymmetric information. One variety is ex-ante “hidden information”: contract terms can change the pool of borrowers by inducing self-selection based on private information that is correlated with repayment likelihood (or, more to the point, profitability). This variety includes the now-canonical adverse selection and its lesser-known cousin, advantageous selection (see Section III-A). Sub-varieties include selection on borrower “type” (good apples vs. bad apples) and selection on project risk (good projects vs. bad projects). The second variety is ex-post “hidden action”: contract terms affect borrower incentives to repay post-disbursement of loan proceeds. Sub-varieties here include moral hazard in: project choice, project or repayment effort, and strategic (and sovereign) default. The third variety considers the interaction of hidden information and hidden action problems, where contract terms may induce borrowers to select on their tendency to engage in moral hazard (i.e., on their sensitivity to ex-post incentives).

iii. Empirical Evidence: Testing Theory

Empirical tests of asymmetric information theories remain rare, due in part to substantial identification challenges. One type of test identifies whether there is economically important asymmetric information in reduced-form, while remaining silent on the underlying variety of problem. Karlan and Zinman (2008) and Adams et al (2009) develop tests in this vein and find substantial effects of asymmetric information on default rates. Another type of test seeks to separately identify one or more of three varieties. Adams et al (2009), Einav et al (2012), and Dobbie and Skiba (2013) find evidence of substantial adverse selection in two U.S. markets, and Karlan and Zinman (2009) and Bryan, Karlan, and Zinman (2013) find evidence of substantial moral hazard in a South African market. Bryan et al also develops the only test I have seen for selection on moral hazard in a credit market, and find no evidence of it, although the sample is small and hence the estimate is imprecise. A growing literature attempts to identify the importance of strategic default in the U.S. mortgage market, but this work has not reached a consensus on the question of how many borrowers (with negative equity) default despite having the ability to repay (see, e.g., Bhutta, Dokko, and Shan 2011; Guiso, Sapienza, and Zingales 2013; Gerardi et al. 2013).

At least three shortcomings of the empirical work summarized above are particularly noteworthy. One is the limited scope: taken together the papers cover only a subset of important

product markets and asymmetric information (sub-)varieties. The second is the lack of empirical evidence on the extent to which dynamic contracting mediates the effects of asymmetric information (although Karlan and Zinman (2009) has some related evidence). The third is lack of evidence on how competition mediates the effects of asymmetric information. This is partly a reflection of the theory literatures' focus on unilateral contracting, despite the fact that multilateral contracting and lender competition seem more the norm than the exception in today's markets.

iv. Empirical Evidence: Circumstantial

The influence of asymmetric information theory has been fueled more by circumstantial evidence than by sharp tests of the theories themselves. The leading circumstantial evidence takes the flavor of documenting liquidity constraints that bind for many consumers. For instance, even secured loan markets have severe rationing episodes (see, e.g., Bhutta (2013) on the mortgage origination slowdown). Many consumers report being rationed or unable to come up with \$2,000.¹² When given the chance to borrow more, many people do, exhibiting strong elasticities with respect to available credit (Gross and Souleles 2002; Mian and Sufi 2011; Bhutta and Keys 2013; Dobbie and Skiba 2013) and maturities (Attanasio, Goldberg, and Kyriazidou 2008; Karlan and Zinman 2008). Such facts are indeed compelling evidence of the economic importance of liquidity constraints. Documenting them often raises new questions; e.g., why do people respond strongly to an increased credit line even when they have seemingly ample available credit ex-ante (Gross and Souleles 2002)?

Yet it is important to recognize that this evidence is reduced-form in the sense that it does not identify the source or welfare impacts of the constraints. Indeed, these constraints may be optimal, or closer-to-optimal, if there are underlying mechanisms that push markets in the direction of over-supply. The revealed preference of consumers to borrow more need not be a sufficient statistic for welfare analysis if consumers are "behavioral" in particular ways, or if borrowing produces negative externalities. I consider such possibilities in Section III.

¹² E.g., an estimated 26% of U.S. *households* (not just would-be borrowers) report being rationed or discouraged from apply during the last 5 years in the 2010 Survey of Consumer Finances (author's calculation). 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).

v. Prospects for Better-Appling Research on Asymmetric Information to Policy

There has been some noteworthy progress in simulating the effects of policy levers in markets with an asymmetric problem.¹³ But given the influence of asymmetric information theory on credit market policy and practice—ranging from government loan guarantees to microcredit—there is relatively little empirical work that puts these theories to the test. This is an area that cries out for greater interplay between theory and empirics, and across product markets where there are clear complementarities (e.g., the credit literatures seem to be lagging a bit behind comparable literatures on insurance). The need for a richer understanding of how asymmetric information affects markets with multilateral contracting seems particularly pressing.

D. Assessment: Is Credit Under-Supplied?

We do not know. It sure seems that the answer is “yes (sometimes)”. But the evidence that leads to this inference is largely circumstantial: we still know remarkably little about the underlying causes of credit constraints/rationing. Without such knowledge it is difficult to formulate (constrained-)optimal policy interventions, or even to know whether such interventions should seek to increase the supply of credit. Indeed, as discussed below, rationing may also be consistent with *over*-supply (or constrained-optimal supply).

III. Theories of Over-Supply

A. Asymmetric Information Redux

Asymmetric information can lead to over-supply instead of under-supply. One variety is *advantageous selection*, where ex-ante willingness-to-pay is correlated with something unobserved (e.g., risk aversion) that makes those selecting in at less favorable terms *more* likely to repay ex-post (de Meza and Webb 1987; de Meza and Webb 2000). This phenomenon has been empirically documented in insurance markets (L Einav and Finkelstein 2010). The one hint of it I know of in consumer credit can be found in Einav et al (2012): the borrowers they identify as marginal, when faced with higher downpayment requirements, are *more* likely to default.

¹³ To my knowledge the focus thus far has been on bankruptcy rules (Livshits, MacGee, and Tertilt 2007; Chatterjee et al. 2007). There are many other policy options worthy of comparable scrutiny.

Another possibility in credit markets is that nonexclusive contracting generates negative externalities wherein a marginal dollar lent to a consumer by Lender A reduces the likelihood that the marginal dollar lent to the same consumer by Lender B gets repaid (Degryse, Ioannidou, and von Schedvin 2012). This produces what are variously labeled *common pool*, *common agency*, and/or *coordination* problems that have been the subject of extensive study in several other literatures (Goldstein and Razin 2013), and that may have counter-intuitive solutions (Hertzberg, Liberti, and Paravisini 2011). The link to asymmetric information problems (particularly moral hazard) is richest when contract terms themselves (including but not limited to workout situations) affect the likelihoods of repayment (Bisin and Guaitoli 2004).¹⁴ Consumer credit-focused work on these questions is quite thin, and would likely benefit from trade with literatures that are more focused on corporate finance, financial (in)stability, and international finance.

B. Borrowing with Negative Externalities on Collateral: Fire Sales and 3L Effects

Other models of negative externalities focus on the values of assets used to collateralize borrowing. Models of *fire sales* examine the interaction between leverage and asset prices following a bad shock (e.g., Shleifer and Vishny 1992; M. K. Brunnermeier 2009). Lenders get concerned about collateral values, forcing borrowers to sell their collateral to (partially) repay their loans. When too many borrowers (relative to market liquidity) are selling, the value of the collateral can fall. If borrowers and lenders do not take this externality into account ex-ante—the effect of my (forced) sale on the value of others’ assets—this can generate too much debt (in a constrained 2nd-best sense, more on this below).¹⁵

Another potential externality works through the “location, location, location” (3L) property of real estate. Markets will generate too much debt 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.

¹⁴ Interestingly Dobbie and Skiba (2013) find evidence of what might be termed *advantageous moral hazard* for a payday lender: a larger loan size *reduces* default, all else equal.

¹⁵ See also Hart and Zingales (2011), where increasing collateralized debt increases the price of whatever good is being financed with the debt, generating a negative externality on other borrowers/consumers.

Related empirical evidence on mortgages does not sharply distinguish between fire sale and 3L effects. Mian and Sufi (2012a) emphasize the fire sale channel, and find little to support a location effect.¹⁶ Campbell et al (2011) emphasizes a 3L effect and finds little to support a fire sale effect.¹⁷

It is important to emphasize that fire sale models generate “too much” debt “only” in a constrained/2nd-best sense. The negative externality materializes only because there is actually *too little debt* at critical junctures: asset markets lack sufficient liquidity to clear at fair prices. The next two classes of models have a similar property. 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.

C. Deleveraging Slows Recovery

A new class of models highlights how deleveraging can slow recovery from an exogenous macro shock given monetary policy that is constrained by a zero bound. In Eggertsson and Krugman (2012), if leverage is high enough entering the shock, then the higher the initial leverage (debt overhang), the larger the interest rate cut needed for output to stay at potential.¹⁸ If the zero bound makes the required cuts infeasible, the contraction grows and recovery slows. Hall’s (2011) model is similar, and emphasizes how shock-induced tightening of household borrowing constraints can deepen and prolong a downturn.¹⁹ Mian and Sufi (2011; 2012b) find empirical evidence consistent with these models: the drop in demand has been more pronounced in U.S. counties and zip codes that entered the downturn with relatively high leverage. See

¹⁶ p. 17: “While there may be other channels through which foreclosures affect house prices, the evidence... suggests an important role for the foreclosure-induced expansion in the supply of inventory.... The very large increase in supply in inventory can plausibly explain the entire decline in house prices.” Note also their finding that inventory affects prices only during 2007-2009 but not 2010-11, pushing against the location effect interpretation (unless the foreclosure process got more efficient, destroying less value in the process).

¹⁷ pp. 2110-2111: the prices of forced sales have relatively little predictive power for the prices of other transactions at the zip code level, but there are spillover effects at more localized levels. Spillovers and discounts are larger in low-priced neighborhoods. “Both results suggest that spillovers may reflect physical damage to neighborhoods.”

¹⁸p. 1477: “The intuition is straightforward: the saver must be induced to make up for the reduction in consumption by the borrower. For this to happen, the real interest rate must fall, and in the face of a large deleveraging shock it must go negative to induce the saver to spend sufficiently more.”

¹⁹ See also Guerrieri and Lorenzoni (2012), and Midrigan and Philippon (2011).

Justiniano et al (2013) for a contrary view based on results from a quantitative dynamic general equilibrium model.

Note again that these models have properties of both too much and too little credit. Credit is oversupplied (or, to put it more mildly, one needs to be mindful of high leverage levels) 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.²⁰ Indeed, the findings in Mian et al (2013) suggest an important mediating role for tightened credit constraints in the consumption collapse; e.g., the marginal effect of a decline in home value on tighter constraints is significantly larger for zip codes that had a high leverage ratio entering the mortgage crisis.

D. Systemic Risk

Khandani et al (2012) shows that “the combination of rising home prices, declining interest rates, and near-frictionless refinancing opportunities can create unintentional synchronization of homeowner leverage” leading to correlated/systemic risk in the mortgage market. In particular, “cash-out refinancing is like a ratchet. It incrementally increases homeowner leverage as real-estate values appreciate without the ability to symmetrically decrease leverage by increments as real-estate values decline.” The asymmetry comes from indivisibility of the collateral (the house), and from the lack of an equity market for homeowners. Note that both of these frictions 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.

Khandani et al conduct a calibrated simulation of their model and estimate that the ratchet effect led house prices to decline \$1.4 trillion more than if no equity had been extracted during the boom. Bhutta and Keys (2013) find empirical support for the model in the form of evidence

²⁰ Despite the juxtaposition of too much and too little, Hall (2011, p. 468) infers that credit is over-supplied on balance: “The most important policy lesson is to prevent the repetition of the poor oversight of government-protected financial institutions that gave us too much housing, too many cars, and too much debt during the past decade.”

that consumers who extracted home equity later in the housing boom have been more likely to default on their mortgages than earlier extractors.

E. Behavioral Consumers (facing Neoclassical Suppliers)

i. Overview of Behavioral Explanations

A 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? Generally speaking I focus here on models where there consumers have some *bias* that can lead to excessive borrowing.²¹ 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 economists finding evidence (often lab-based, and from other social sciences) that motivates a different specification of one or more pieces of an otherwise standard economic model. The pieces themselves are what is standard: preferences, prices (and perceptions thereof, which can be biased in a behavioral model), expectations about various future parameters (including preferences and prices), and decision rules (i.e., whether/how someone solves a problem, conditional on parameter values).

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 (more on this below).²² 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

²¹ 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.

²² See also Ellison (2006) and DellaVigna (2009) for reviews of what is variously referred to as behavioral or boundedly rational industrial organization.

unbiased advice.²³ 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 2012). Social learning can produce herding and inefficient equilibria [Banerjee; Rabin and co-authors].

ii. Circumstantial evidence re: consumer borrowing behavior

Before proceeding to a brief summary of different behavioral theories and evidence, I start with some of the circumstantial evidence on borrowing behavior that helps to motivate behavioral work and policy interest therein.²⁴ One striking observation is that no extant model, behavioral or otherwise, can generate even ½ of the credit card debt U.S. households hold in steady-state (Angeletos et al. 2001). Other studies, on loans with triple-digit APRs, find that consumers respond strongly to uninformative advertising (Marianne Bertrand et al. 2010), substantially reduce borrowing when exposed to subtle shocks that draw their attention to high fees (Stango and Zinman 2013a), and borrow repeatedly on expensive short-term loans before defaulting (Skiba and Tobacman 2008). Descriptive evidence shows a lack of lender disclosure of, and consumer shopping on, bank overdraft fees even when said fees constitute the lion's share of consumer checking account costs (Armstrong and Vickers 2012; General Accounting Office 2008). "Overpaying" may be as important as "overspending": there is substantial price dispersion in mortgage and credit card markets, conditional on credit risk and other contract terms (Woodward and Hall 2012; Stango and Zinman 2013b). Consumers behave as if search and switch costs are quite substantial, which could of course be rationalized with a standard time-cost explanation, but may ultimately be better explained by a behavioral model of (non-)shopping.

²³ There has been much 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).

²⁴ Re: policy interest, see e.g., Sunstein (2006) and Bar-Gill and Warren (2008) for two of the many law review articles marshaling behavioral arguments for regulation.

iii. Preference-based models

Perhaps the best-known class of behavioral model²⁵ posits that consumers have a time-inconsistent taste for immediate gratification. Following Laibson (1997) and O'Donoghue and Rabin (1999), economists often parameterize this taste by adding an additional discounting parameter (beta), capturing a relatively strong taste for current consumption, to a standard intertemporal choice model. This parameterization can generate quasi-hyperbolic discounting behavior, in contrast to the exponential discounting predicted by models with time-consistent preferences.²⁶

Despite the influence of the beta-delta model on research output and policy discussions on consumer credit, there is actually little sharp empirical evidence linking time-inconsistent preferences to (over)borrowing. In fact I am not aware of any nationally representative evidence other than the calibration exercise in Laibson et al (2003), which shows that beta-delta preferences can rationalize the simultaneous holding of illiquid (retirement-savings) assets and expensive (credit card) debt. The market for commitment devices that would help consumers avoid overborrowing is thin (there are alternative explanations for this fact, including other behavioral biases that dampen demand). There are some interesting findings that suggest a role for beta-delta (Meier and Sprenger 2010; Skiba and Tobacman 2008), but overall empirical work has only begun to scratch the surface of field-testing whether time-inconsistent preferences actually drive (over-)borrowing.

The lack of empirical evidence becomes less surprising upon close scrutiny of the now-standard beta-delta model and its implications. Most financed purchases are durables, not instantaneous consumption per se.²⁷ Obtaining a loan often involves some unpleasant upfront tasks (comparison shopping, paperwork) that many beta-delta consumers would procrastinate.

But Heidhues and Koszegi (2010) show how an enriched version of the beta-delta model can generate overborrowing. As long as consumers are not perfectly sophisticated about their self-

²⁵ No slight intended to loss aversion, which I also discuss below.

²⁶ Recent work on intra-household decision making shows that preference heterogeneity among time-consistent household members can produce time-inconsistent behavior at the household level (Hertzberg 2012; Schaner 2013).

²⁷ The question of how long the “instantaneous” or “current” consumption period lasts is an important one that has received little attention aside from Gine et al (2013).

control problems (specifically, as long as they underestimate their time-inconsistency by just a little bit), firms will offer back-loaded credit contracts in equilibrium. These contracts exploit the tendency of imperfectly sophisticated consumers to procrastinate *repayment*, thereby underestimate the cost of borrowing, and overborrow relative to the benchmark of unbiased forecasting of one's behavior. I am not aware of any empirical tests of this model.

The other canonical behavioral model—preference-based or otherwise—is loss aversion. Loss aversion is sometimes invoked as potential obstacle to consumers *reducing* their debt loads (Karlan and Zinman 2012)-- or, more broadly, to consumers *increasing* their savings rates (Benartzi and Thaler 2004)-- but I have yet to see this intuition worked out theoretically, or tested empirically.

Another intriguing, preference-based model is Laibson's (2001) theory of Pavlovian cues. Some of the evidence in Bertrand et al (2010) is consistent with this model, but again field tests of the model's distinct predictions are lacking.

iv. Expectations about repayment

There is growing concern in policy and “consumer advocacy” circles that consumers are, at least functionally, overly optimistic about their likelihood of repaying debt. As discussed above, Heidhues and Koszegi's model generates this sort of overoptimism via over-optimism about self-control. Brunnermeier and Parker (2005) rationalize overly optimistic expectations about income with anticipatory utility, and show that this interaction between preferences and expectations can produce more consumption (less saving) earlier in the life-cycle, but I have not seen this model extended to flesh out implications for borrowing or debt contracts. Mullainathan, Shafir, and co-authors (2012; 2013) suggest that scarcity in the budget constraint leads to consumers to pay excessive attention to solving problems that alleviate that scarcity (such as obtaining cash, perhaps via borrowing) and little attention to other problems (such as how they will repay the loan).

Empirical work has just begun exploring links between optimism and borrowing. Hyytinen and Putkuri (2012) explore correlations between a survey-based measure of forecast *errors* about financial condition and household borrowing; to me their most striking finding is that forecasts are *unbiased* on average. Similarly, Mann (2013) finds that payday borrowers are roughly

unbiased in forecasting how long it will take them to repay.²⁸ If we were focused on entrepreneurial finance we would certainly consider the possibility of over-confidence in one's abilities (in absolute and/or relative terms); perhaps over-confidence is relevant in consumer credit as well.²⁹ But overall the empirical links between biased expectations of repayment prospects and borrowing behavior are tenuous at best, and arguably non-existent.

v. Other forms of inattention

Consumers may not attend to contingent fee schedules if they are overly optimistic about avoiding such fees. Heidhues and Koszegi generate such optimism through naivete about preferences for instant gratification. Gabaix and Laibson (2006) impose such optimism on consumer expectations (“Me, overdraft? Nah.”), and show that the shrouding of such fees can persist in competitive equilibrium, if, as seems plausible, a debiasing firm does not fully capture the returns to debiasing (due, e.g., to switch costs). I am not aware of any sharp tests of the shrouding model re: borrowing behavior, although the evidence in Bucks and Pence (2008) and in Stango and Zinman (2013a) is consistent with it.

Karlan et al (2012) explores how consumption v. savings behavior will respond if consumers tend to forget “exceptional” (infrequent, and relatively large) expenditure needs/*opportunities a la* Sussman and Alter (2012). Karlan et al speculates that debt may be “salience-privileged” relative to saving because it is available on-demand, at the late moment the exceptional expense becomes salient and it is too late to save up for it.

vi. Biased price perceptions

As discussed above, consumers can end up underestimating the cost of borrowing due to biased (overly optimistic) expectations about their self-control (Heidhues and Koszegi 2010) or

²⁸ Fritzdixon et al (2013) compare repayment expectations to a population average in their sample of auto title loan borrowers, and find evidence of some, but “not severe” underestimation of repayment time. Of course, this inference depends on their sample being representative of the comparison population, along with other factors that are not explored in the paper. Lacking an individual-level measure of optimism, the paper makes no attempt to directly estimate correlations between optimism and borrowing behavior.

²⁹ Grubb (2009) shows how consumer overconfidence in demand forecasts give firms incentives to offer contracts with quantities at zero marginal cost (e.g., teaser rates) followed by steep marginal charges (e.g., penalty fees).

more generic underestimation of their demand for services that will incur costly “add-on” prices (Gabaix and Laibson 2006).

Consumers might also underestimate prices more directly, even when there is no uncertainty. I.e., there may be a distinction between the vector of prices economists typically use to capture the shadow cost of consumption in intertemporal choice models, and how the consumer *perceives* that vector of prices. Stango and Zinman (2009) show that the flip side of the well-known underestimation of compound growth is an underestimation of how quickly principal is paid back on installment debt, with a more general *exponential growth bias* explaining both tendencies. They also find some evidence that more-biased households do hold more installment debt, and pay higher prices on it (Stango and Zinman 2011). Soll et al (2013) find that people underestimate how long it takes to eliminate a debt when payments barely covered interest owed. The latter two papers, and Bertrand and Morse (2011), find some evidence that disclosures designed to debias consumer underestimates of finance charges (and/or borrowing likelihoods, in the case of Bertrand and Morse) reduce loan demand.

The empirical evidence that consumers tend to directly underestimate borrowing costs is provocative, but comes with several caveats. The body of work lacks a unifying theoretical framework that analyzes equilibrium consumption-savings and/or contracting outcomes. Bertrand and Morse does not measure price perceptions directly, and Stango and Zinman (2009) and Soll et al lack data on other behavioral factors, leaving open the question of whether price perceptions might be correlated with some other bias that drives behavior. Stango and Zinman (2011) does not observe disclosures directly. Bertrand and Morse measures borrowing from only one payday lender providing out-of-equilibrium disclosures.

vii. Summary: The State of Behavioral Evidence on Over-Borrowing

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.

F. Assessment: Is Credit Over-Supplied?

My brief assessment here parallels the one re: under-supply. We do not know if credit is over-supplied. Again, it sure seems that the answer is “yes (sometimes)”. But again the evidence that leads to this inference is largely circumstantial: we still know remarkably little about the drivers of any over-supply. The evidence is particularly limited if we restrict consideration to models that potentially deliver unambiguous predictions of welfare-reducing over-supply— asymmetric information (advantageous selection, common agency), behavioral consumers, and location-based externalities in real estate.

The other models—fire sales, deleveraging, ratcheting to systemic risk—share the property that the optimal level of debt can depend on the state of the world. Specifically, debt can be too high entering a downturn, yet too low during the downturn. As such these models have implications for macroeconomic management—fiscal policy and monetary policy (see also Lippi and Trachter (2012))—as well as for the regulation of specific credit markets. These models also raise the question of which objective policymakers should adopt in calmer times: should they try to reduce credit market frictions, in anticipation of the next downturn? Or should they assume that frictions will always be severe enough to exacerbate downturns, and introduce frictions during calmer times to keep leverage from being too high when the next downturn strikes?

IV. Reduced-Form Evidence on the Goldilocks Question

Another approach to addressing the goldilocks question is to address the “impacts” question: what happens to economic entities when credit access shifts (due to a plausibly exogenous shock like a policy change or experiment that shifts credit supply)? Do economic conditions and overall well-being improve, or deteriorate, as credit supply increases or decreases?

A. Effects of expanded consumer credit supply on consumers

A growing literature seeks to identify the impacts of changes in “small-dollar” credit access on the financial condition and well-being of consumers. I focus here on the U.S. studies, in part because small-dollar credit in developing countries (“microcredit”), and studies thereof, has

focused on microentrepreneurs.³⁰ Small-dollar credit markets in the U.S.—principally payday loans—are attractive settings for addressing the impacts question for several reasons. First, the stakes are large, particularly compared to the relatively low incomes of most borrowers. Loan volumes are in the tens of billions of dollars each year, and APRs are typically in the triple digits. Second, borrowing is prevalent among populations with relatively high social welfare weights: there are more payday loan outlets than MacDonaldis and Starbucks combined. Third, shocks to the market itself are unlikely to have effects in the aggregate: the market is not *that* big, and most small-dollar loans are uncollateralized (auto title loans being the key exception these days). Fourth, there is substantial policy variation-- at the state, within-state, and federal levels—that can help with identification.

Results from the U.S. literature are quite mixed. Some prior studies find that, on average, expensive consumer loans help borrowers smooth negative shocks (Morse 2011) or better manage liquidity to alleviate financial distress (Zinman 2010; Morgan, Strain, and Seblani 2012). Other studies find that increased access to expensive credit increases financial distress (Melzer 2011; D. Campbell, Tufano, and Martinez-Jerez 2012) and bankruptcy (Skiba and Tobacman 2011; Morgan, Strain, and Seblani 2012), and decrease job performance (Carrell and Zinman 2013). Two other studies find no impacts on credit scores (Bhutta 2012; Bhutta, Skiba, and Tobacman 2012). It remains unclear how much of the variation in evidence is due to variation in methodology vs. substance. Carrell and Zinman speculate the latter, and in particular variation in consumer choice sets, is critical: given a pool of productive and counter-productive (perhaps behavioral) borrowers, productive borrowers may be more likely to refrain from borrowing when outside options are worse, leaving counter-productive borrowers to dominate the pool.³¹

Besides the mixed bag of results and lingering questions about identification, the extant impacts literature has several other limitations that limit its direct applicability to policy design. There is not yet any generally accepted, directly measurable summary statistic for consumer welfare (although in my view many of the approaches taken thus far are quite sensible). Existing

³⁰ Karlan and Zinman (2010) is an exception, and finds that expanded access to 4-month consumer loans at 200% APR substantially increases job retention and well-being in South Africa. See Banerjee (2013) for a summary of evidence on the impacts of credit targeted to microentrepreneurs.

³¹ Carrell and Zinman's finding that payday loan-induced job performances declines are larger in high unemployment areas is consistent with this story.

methodologies have not been powerful enough to identify any non-linearities (e.g., perhaps the relationship between welfare and credit supply is upside-down u-shaped).

Perhaps the most important limitation of the existing impacts evidence is that it leaves us without a *diagnosis* of the mechanism(s) underlying any failure in market allocations. Another way of framing this issue is to consider the challenge of inferring even a policy *objective* (much less a specific mechanism for achieving that objective) from a given qualitative result on consumer impacts. Say the bulk of the evidence found that more credit makes consumers better-off. This is consistent with credit being under-supplied of course. But it is also consistent with credit being over-supplied, at least in a first-best sense, as it may be the case that the credit expansion serves behavioral consumers who would otherwise borrow from inferior sources. This is an important consideration if it is feasible to cost-effectively regulate certain types of credit suppliers (e.g., large payday lenders) but not others (small payday lenders, loan sharks).³² Now say instead that the bulk of the evidence found that credit expansions have null effects on consumers. Besides the obvious interpretation, we should also consider the possibilities that impacts are heterogeneous in ways that might invite targeting and screening, and that other market failures interact with credit market failures in important ways (Emran and Stiglitz 2007).³³

B. Long-run effects of expanded consumer credit supply on economies?

In principle it would be informative to study more-aggregated impacts of access to consumer credit as well. This sort of evidence would internalize some potential spillovers (Angelucci, Karlan, and Zinman 2013), while providing an aggregate analog to the consumer-impacts literatures (Section IV-A) and a long-run/steady-state analog to the leveraging and deleveraging literatures (Section III-C). Despite the extensive literature on links between financial development and growth, both across-country (e.g., Levine 1997), and within-country (e.g., Jayaratne and Strahan 1996), I am not aware of any evidence that sharply identifies the impacts

³² See Stango and Zinman (2011) for some related evidence on disparate enforcement (across banks vs. non-banks) of mandated disclosure in the auto loan market.

³³ The inferential challenge even extends to robust evidence of negative impacts, as it could be the case that (enough) consumers are effectively risk-loving. This actually does not seem totally implausible given limited liability, and/or the option value of changing industries in the labor market (Neumuller 2013).

of *consumer* credit per se. I suspect this is due in large part to the difficulty of disentangling the impacts of consumer credit from other sorts of financial intermediation.

V. Conclusion

Many policy questions regarding consumer credit begin with a presumption about whether, and to what extent, markets fail to supply an efficient quantity of credit. Theories abound for both over-supply and under-supply. Several classes of models have elements of both; in particular, they predict that credit will be over-supplied heading into a downturn, and then under-supplied during the downturn. If this inference about state-contingency is accurate then it further complicates optimal policy design. E.g., it raises the question of which objective policymakers should adopt in calmer times: should they try to reduce credit market frictions, in anticipation of the next downturn? Or should they assume that frictions will always be severe enough to exacerbate downturns, and introduce frictions during calmer times to keep leverage from being too high when the next downturn strikes?

These and other fundamental questions remain open ones because empirical evidence is mixed, and limited. We are still a long way from diagnosing specific failures—market, decision, governmental, etc.-- that could and should be treated with policy interventions.

I suspect that diagnoses and treatments will vary substantially across product markets. Different product markets may attract different (behavioral) types of borrowers, and the mix of types can have important welfare and policy implications (e.g., J. Y. Campbell 2006). Variation in opportunity sets might affect the mix of borrower types as well; e.g., productive borrowers may opt-out of a market when productive investments in human capital or consumption smoothing are lacking, leaving counter-productive borrowers to dominate the pool. The contracting equilibrium also merits scrutiny; e.g., it seems more likely to me that an equilibrium with symptoms of shrouded pricing (e.g., checking account overdrafts) is sub-optimal than an equilibrium with more-transparent pricing (like payday loans).³⁴ Finally, the distinction between collateralized and uncollateralized loans seems likely to be important. To highlight just two

³⁴ Although see Bertrand and Morse (2011) for some evidence suggesting that one-off price disclosures (“\$15 per \$100”, 400% APR, etc.) may not be sufficient, perhaps because some borrowers underestimate the longer-term costs and/or likelihoods of borrowing.

distinctions among many, collateralized loans may be subject to more externalities (via collateralized asset values), while unsecured debt interacts more strongly with bankruptcy rules.

In short, policymakers should approach consumer credit markets with humility. There is a lack of convincing evidence on whether markets err, and in which direction. We do not yet understand whether and under what conditions markets over-supply or under-supply credit, much less why. Under the current evidentiary conditions, traditional approaches to policymaking will likely do more harm than good in many cases.

To be clear, I am not positing a binary policy choice between doing something ill-conceived and doing nothing. The opportunity set is richer than that, and endogenous to regulator choices. A key is to make choices that generate evidence while minimizing path-dependence. This is relatively difficult for policies aimed at stabilization and systemic risk. But policymakers concerned more with long-run and steady-state credit supply have viable options. E.g., as I have argued elsewhere, it would behoove policymakers to do more “beta-testing” and less “ready, fire, aim!”³⁵

³⁵ http://www.dartmouth.edu/~jzinman/Papers/Evidence_Based_Policymaking_SDC.pptx . See also, e.g., Sunstein (2011).

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