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CONSUMER PROTECTION FOR FINANCIAL INCLUSION IN LOW AND MIDDLE
INCOME COUNTRIES: BRIDGING REGULATOR AND ACADEMIC PERSPECTIVES

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Consumer Protection for Financial Inclusion in Low and Middle Income Countries: Bridging
Regulator and Academic Perspectives

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ABSTRACT

Markets for consumer financial services are growing rapidly in low and middle income countries and being transformed by digital technologies and platforms. With growth and change come concerns about protecting consumers from firm exploitation due to imperfect information and contracting as well as from their own decision-making limitations. We seek to bridge regulator and academic perspectives on these underlying sources of harm and five potential problems that can result: high and hidden prices, overindebtedness, post-contract exploitation, fraud, and discrimination. These potential problems span product markets old and new, and could impact micro- and macroeconomies alike. Yet there is little consensus on how to define, diagnose, or treat them. Evidence-based consumer financial protection will require substantial advances in theory and especially empirics, and we outline key areas for future research.

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Introduction

Markets for consumer financial services are growing and evolving rapidly in low and middle income countries (LMICs). Digital technologies and platforms in particular are fueling significant transformations to products, distribution, and business processes for financial services. Overall, from 2014 to 2017 the share of adults in LMICs depositing and borrowing at formal financial institution increased from 54 percent to 63 percent and 9 to 15 percent, respectively. Growth in digital finance is similarly rapid: the share of adults in LMICs using digital payments increased from 32 to 44 percent from 2014 to 2017, and as an example from Kenya, 13.6 percent of adults accessed a loan through digital channels in 2018 alone.²

Some of this growth has been accelerated by financial inclusion policies. For example, distributing cash transfers through debit cards in Mexico generated spillovers to retailers and non-poor households and substantially increased debit card adoption (Higgins 2020). In India, the central government mandated the opening of a bank account for every unbanked household through the Jan Dhan Yojana scheme in 2014; an estimated 80 percent of households now have an account.

These market changes and financial inclusion strategies have spurred policymakers, government regulators, and stakeholders in LMICs—who we will refer to collectively as “regulators”—to focus on consumer financial protection. Many LMIC consumers lack experience with formal financial services, much less fintech, and the experience of richer countries over the past several decades suggests that even relatively experienced consumers can be harmed by using financial services. Moreover, consumer harm can spill over to the macroeconomy (Mian and Sufi 2018).

We review academic research related to consumer financial protection with an eye toward bridging regulator and academic perspectives on key problems, viable solutions, and the most important open research questions. We focus on household credit, savings, and payments. Many of the issues and potential solutions we cover are equally relevant for insurance and entrepreneurial finance.

We organize our review around five key problems identified by regulators in LMICs: (1) High and hidden prices; (2) Overindebtedness; (3) Post-contract exploitation, with a focus on agents charging extra transaction fees, add-on of services without consent, and debt collection harassment; (4) Fraud; and, (5) Discrimination and disparate impacts on disadvantaged groups.³

We discuss each of these potential problems in four steps.

First, we describe the problem from the regulatory perspective, and discuss diagnostic challenges.

Second, we discuss the theoretical mechanisms that may underlie the problem and the existing empirical evidence (or lack thereof). We discuss what is known, and unknown, about specific market failures and consumer decision-making limitations (“decision failures,” in our parallel—if uncharitable—shorthand) that may help explain the root cause of each problem. For more traditional sources of market failure, we discuss imperfect and incomplete information (typically regarding pricing and fees), externalities, and moral hazard by firms or fraudulent actors. For decision failures, we consider a broad set, including present bias, inattention, lack of financial literacy, limited learning, and scarcity (see also Agarwal,

² Statistics in this paragraph are from Demirguc-Kunt et al. (2015; 2017), FSD Kenya (2019), and Gubbins (2019).

³ Relative to previous reviews of consumer financial protection (Campbell et al. 2011; Campbell 2016), we focus more on LMICs, on considering how behavioral industrial organization theory motivates rethinking regulatory goals and tactics, and on bridging regulator and academic perspectives to set research agendas. For reviews specific to the product markets we cover, see Karlan et al. (2014), Zinman (2015), and Suri (2017). For broader reviews of household finance see Agarwal et al. (2020) and Gomes et al. (forthcoming).

Chomsisengphet, and Lim 2017; Beshears et al. 2018). Diagnosing specific decision and market failures is difficult, as the same behavior or outcome often can be explained with different theories. Such work is vitally important for policy because distinct theories often have radically different policy implications.

Third, we review regulatory and programmatic approaches to the problem, including both traditional approaches like restricting contract terms, mandating disclosures, and providing financial education, and newer approaches like outcomes-based regulation, prohibitions on deceptive and abusive advertising and other practices, and nudging. We focus on what is known and not known about the *effectiveness* of these approaches, stopping short in most cases of assessing *efficiency* (i.e., societal welfare) for two key reasons. First, we typically lack convincing empirical evidence on many of the inputs required for quantitative welfare analysis. Concretely, for example, assessing the efficiency of an interest rate cap requires quantitative estimates of the benefit to some from lower interest rates against the loss to others from not being able to borrow at all—and obtaining those estimates requires estimates of several underlying parameters that are difficult to identify accurately and precisely. Second, potential consumer decision failures substantially complicates welfare analysis (D. Bernheim and Taubinsky 2018). If someone is present-biased, for example, which “self” is used to calculate welfare, the current impatient self or the future planning self?

Lastly for each potential problem, we highlight key open research questions that should be addressed to improve regulator capacity for successful consumer protection policies.

Three themes cut across the five problem areas and loosely correspond to the three key actors relevant for consumer protection: the consumer, the firm, and the government.

The first is the empirical importance of consumer decision failures in driving consumer and market outcomes. We find compelling evidence of their importance for our first key problem—high and hidden prices—but for the remainder we have more questions than answers.

The second theme relates to the validity of a fundamental premise of consumer protection policy: “Robust competition is the best single means for protecting consumer interests” (Muris 2002). Consumer protection regulators and researchers often overlook several bodies of theory on market and decision failures that question this premise. More competition under network externalities in platform markets like payments can produce under-supply that harms consumers. More competition under repayment externalities in credit markets can produce over-supply that harms consumers. Behavioral industrial organization theory shows that, when consumers have decision failures, more competition can backfire by drawing too many consumers into an exploitative market, redistributing surplus from struggling to sophisticated consumers, and/or incentivizing financial services providers (FSPs) to invest more in obfuscation (Heidhues and Kőszegi 2018). However, these theories are largely untested empirically. One avenue for empirical progress would test such theory’s underlying premise that specific consumer decision or market failures are economically important. Another avenue should test how changes in competition affect consumer and market outcomes. More generally, Heidhues and Koszegi elucidate the importance of studying firms’ market responses to interventions, including light-touch ones like nudges.

A third cross-cutting theme relates to limited state capacity for regulation and its enforcement (Estache and Wren-Lewis 2009).⁴ Regulatory frameworks may be less developed in LMICs, although not by as much as commonly assumed. The World Bank Group (2017) documents substantial variation in consumer protection laws and regulatory practices across 124 countries, including 45 LMICs. LMICs are roughly as likely as higher income countries to conduct supervisory activities such as complaint data collection, mystery shopping, on-site inspections, although the legal basis for financial sector intervention tends to be weaker in LMICs. A cleaner legal slate actually could be an advantage if it helps leapfrog

⁴ Most regulatory studies targeting finance are focused on prudential oversight.

“legacy” issues with, for example, ineffective policy design, limited monitoring capacity, and jurisdictional fragmentation. These issues are particularly important to address in markets where non-banks (e.g., fintechs) are emerging as competitors to traditional banks (Farhi and Tirole 2017), and in markets like payments that span borders and/or have coordination problems like interoperability (Cooper et al. 2018). Concerns about corruption often focus on LMICs but apply to richer countries as well. In short, limited state capacity is a concern for most consumer financial protection regulators around the world, and potential solutions—such as more evidence-based design of regulations and jurisdictions, regtech-enhanced monitoring and enforcement, and financial service innovations (e.g., Muralidharan, Niehaus, and Sukhtankar 2016)—are not unique to LMICs.

Two other important issues that cut across our five problems, but that we mostly ignore, are the family of issues around data security, privacy and property rights (Barth and de Jong 2017; Waldman 2018) and malfunctioning financial advice markets (D’Acunto and Rossi forthcoming; Gomes, Haliassos, and Ramadorai forthcoming).

1. High and Hidden Prices

A. Defining the Problem: Regulator perspective

Regulators often cite high and hidden prices as a key challenge in consumer financial markets. Yet diagnosing high and hidden prices is challenging. For example, regulators often point to complex product menus and contracts as a sign that providers are extracting excess revenue from confused, inattentive, or over-optimistic consumers (Greenwood and Scharfstein 2013). In classical economic models, however, rich menus are viewed as a healthy sign of tailoring products to heterogeneous consumer demand.

Another diagnostic approach focuses on non-compliance with existing regulations like the disclosure mandates discussed below. This approach is conceptually limited in focusing on processes rather than the desired outcomes, and practically limited by resource and legal constraints.

A promising diagnostic development is increased reliance on audits using mystery shoppers, often conducted in partnership with academic researchers. Professional auditors or regular consumers are hired to visit several providers to inquire as clients. These studies show that provider staff maximize commissions and fees rather than making recommendations in the customer’s best interest. For example, Giné and Mazer (2020) finds that FSP staff selling various products in Ghana, Mexico and Peru provided little information about financial products voluntarily but disclosed information truthfully when asked. Less than a third of the total cost of products was disclosed voluntarily, and the amount disclosed was uncorrelated with the total cost of the product. Auditors were always offered more expensive products and almost never offered a no-cost account that met their needs (see also Mowl and Boudot 2015). In a series of field experiments in India, Anagol et al (2017) finds that insurance agents steered purchasers towards unsuitable, strictly dominated products that provided agents with high commission revenues. Audits can thus be used to detect consumer protection problems and to gauge regulatory compliance of FSPs.

B. Defining the Problem: Academic theory and empirical evidence

Besides the audit studies describe above, another indication that consumers may be overpaying is price dispersion: observably identical individuals paying very different prices for the same product (Giné and Mazer 2020; Karlan and Zinman 2018). Although part of the observed price dispersion may be due to discrimination (see Section 6), most papers in this literature emphasize the importance of search costs and heterogeneity in consumer shopping behavior.

Search frictions can generate rents for providers via market power (Allen, Clark, and Houde 2018; Galenianos and Gavazza 2019). Agarwal et al.(2020) emphasize an important wrinkle for credit (and insurance) markets that is introduced by screening: the possibility of application rejection increases the borrower’s effective search costs and can generate adverse selection, even if riskier borrowers do not have

higher willingness to pay for credit, producing a *positive* correlation between search and price paid. Conversely, consumers with high approval probability have the traditional negative correlation.

An alternative set of explanations for high prices emphasizes strategic price obfuscation that preys on consumer confusion, inattention, and/or over-optimism about avoiding contingent fees for paying late, overages, etc. (See Grubb (2015) for a review). A key prediction of Gabaix et al. (2016) is that an increase in the number of competitors may not lower prices towards marginal costs, suggesting that increasing competition will not necessarily improve outcomes if consumers have decision failures.

Another key prediction of these models is that consumers will be (relatively) insensitive to shrouded prices. Alan et al. (2018) finds evidence consistent with this in a field experiment with a large bank in Turkey. SMS messages promoting a discount on overdraft fees *decreased* overdraft usage—which violates the classical law of demand—suggesting that consumers were unaware of or underestimated overdraft fees. However, messages that mentioned overdraft availability without highlighting the price increased demand. Neither message campaign caused long-run differences in overdraft usage, providing further evidence for limited memory and attention that could hinder consumers from experiential learning.

Other key predictions of these models are that advertising will be more persuasive than informative and that firms lack incentives to “debias” customers (Gabaix and Laibson 2006). Hastings et al. (2017) explores the nature of advertising of Mexican pension plans by developing a model where workers respond to prices and brand preferences when choosing their plans. The model can account for two stylized facts: workers are not price sensitive as they do not choose the plan that minimized management costs, and firms that invested heavily in advertising have both high prices and large market shares, suggesting that competition on advertising and nonprice attributes substituted for competition on price.

C. Interventions and evidence on their effectiveness

Price ceilings and bans

Interest rate caps are as old as written history (Temin and Voth 2008). At present, more than 70 countries worldwide have interest rate caps (see Maimbo and Gallegos 2014; and for a review, Ferrari, Masetti, and Ren 2018). Most caps are defined in terms of an annual percentage rate (APR), which includes all fees and charges. But other times the cap only applies to the interest rate, thus encouraging lenders to increase non-interest income via fees and commissions. And yet other times a cap applies to only a (typically politically) selected subset of similar products.

Critics of caps argue that lenders are not earning excess profits, and instead caps reduce credit access and quantities. Regulators may view reducing access and/or quantities as desirable (see Overindebtedness section), or may view lower prices for some as important enough to accept reduced access for others.

Case studies analyzed in Ferrari et al (2018) suggest that interest rate caps reduced credit availability, increased costs for low-income borrowers, and reduced financial product transparency and diversity. We highlight three examples. In West African countries, the imposition of interest rate caps on microfinance loans has been associated with microfinance institutions withdrawing lending to the poorest borrowers and to remote areas. In South Africa, several financial institutions seem to have evaded the interest rate caps by charging credit life insurance and other service fees, reducing the transparency of the total cost of credit. Lastly, in Japan, the supply of formal credit has contracted and that of informal credit has increased since the imposition of a cap.

In richer-country markets, several natural experiments find reductions in credit supply from caps (e.g., Fekrazad 2020). An important LMIC study is Cuesta and Sepúlveda (2019), which finds that lowering caps in Chile reduced both prices and credit supply, and consequently (with a model of classically rational

consumers), estimates reduced consumer welfare on average with larger losses for riskier borrowers. In the United States, Allcott et al (2020) finds that caps harm behaviorally-biased consumers as well.

Interestingly, behavioral industrial organization theory shares the same concerns as classical theory about price caps on *base* prices. Armstrong et al. (2009), for example, shows that a base price cap can reduce consumer search, dampening price competition for consumers. In contrast, behavioral industrial organization theory reviewed in Heidhues and Koszegi (2018) is far more bullish on capping or prohibiting contingent or *add-on* prices: “The same case as for safety regulations can be made: if a contract feature is likely to induce many mistakes and has little economic purpose, then banning it is welfare-increasing.” We know of examples of such bans—for example on prepayment penalties and on upfront fees for debt workout assistance that is contingent on the consumer making a stream of additional payments—but not of any empirical evidence of their effects on consumers, providers, or the market.

Disclosure mandates and prohibitions on deception

Disclosure mandates are also common, based on the rationale that they will reduce consumer search costs and/or confusion, and promote competition. An influential example is the U.S. Truth in Lending Act of 1968 (TILA), which requires standardized disclosures including the calculation of interest and upfront fees as an APR.

Theory and empirics both point to the limitations of traditional disclosures. Regulations may not cover all key costs, especially those that depend on consumer usage (such as late fees, line of credit draws, and rollovers). Point-of-sale provision may happen too late in the consumer search and decision process (Ben-Shahar and Schneider 2011; Loewenstein, Sunstein, and Golman 2014). Strategic responses by providers—in disclosure form design, for example, or in pricing and sales tactics—can undo regulations’ intended effects (Duarte and Hastings 2012; Anagol and Kim 2012), particularly when there is limited and selective enforcement (Stango and Zinman 2011).

Even well-designed and implemented disclosures may not have their intended effects, particularly if consumers have limited attention, other decision limitations like narrow bracketing, and/or face high (perceived) search or switch costs. A randomized evaluation in Mexico that used a sample of borrowers already carrying large credit card balances tested several TILA-like disclosures alongside messages comparing the debt levels and riskiness of cardholders relative to their peers, and finds limited and temporary effects on balances, defaults or account closings (Seira, Elizondo, and Laguna-Müggenburg 2017). Similarly, a randomized evaluation with UK savings accounts finds little effect from behaviorally informed disclosures (P. D. Adams et al. 2019).

There is some evidence, however, that redesigning disclosures with consumer decision failures in mind can improve outcomes, or at least change decisions. Bertrand and Morse (2011) tests new approaches to summarizing payday loan costs and find effects on consumer behavior (which we discuss in more detail in Section 3, on overindebtedness). Giné et al. (2017) finds that providing a simplified fact sheet to low-income consumers from peri-urban Mexico and Peru in a lab-like setting induces greater price-sensitivity, suggesting that improved consumer understanding could lead to more price competition.

Recently many countries have instituted broad prohibitions on deceptive advertising (World Bank Group 2017). We are not aware of any compelling evidence on their effects.

D. Open questions

There is relatively strong evidence that providers succeed in charging high or hidden prices by exploiting decision failures. The key open questions therefore revolve around prevention.

Starting with traditional levers, one key open set of questions is whether interventions that seek to promote competition (for example, by informing consumers of different products available and their costs

and terms) actually do so, and whether success in increasing competition leads to better outcomes. Another is whether anything can be done to salvage traditional approaches to product-specific disclosure; the empirical evidence thus far is not particularly encouraging, but disclosure variations are relatively easy to test and it may therefore be worth continuing to develop innovations in content, timing, medium, and source. A third, and particularly challenging, set of key questions to address empirically is the effects of capping or banning prices on secondary, add-on, or contingent features of financial services. A relatively promising avenue here is for theory to generate clearer guidance on the specific market and consumer decision-making conditions under which caps or bans are likely to improve welfare, and for empiricists to set about measuring those conditions.

A less traditional, but promising, approach to pricing problems is to educate consumers about shrouded fees. Even if some FSPs lack incentives to educate or debias consumers, other FSPs (such as personal financial management apps), NGOs, and government agencies may have incentives more aligned with consumers. We discuss some closely related challenges and opportunities in the discussion of fraud in Section 5.

Another less traditional, but promising, approach that is gaining traction with regulators is price comparison tools. Market provision has not necessarily improved outcomes (Ellison and Ellison 2009), leading some regulators to consider public moderation of an FSP shopping platform. Of course, FSPs could still respond strategically to mute or undo intended effects, so it will be important to identify how both providers and consumers respond to shopping and search interventions.

2. Overindebtedness

A. Defining the problem: Regulator perspective

As noted at the outset, many LMIC consumers are borrowing for the first time from formal financial institutions and many existing borrowers are taking on more debt. This growth includes both “down-market” expansions of now-traditional products like mortgages and credit cards and the increased availability of seemingly newer “digital” products that are sometimes linked to payment platforms.

With such growth comes concerns about “overindebtedness”—about consumers taking on more debt than they should reasonably expect to pay back without incurring undue hardship.

One common approach that regulators use to diagnose overindebtedness is to define it “directly”, using, for example, a debt-to-income (DTI) ratio cutoff. Conceptually, drawing the line is difficult: How high is too high for DTI? This is especially thorny given theory and evidence suggesting that those willing to take on large debt burdens are often facing a stark need. Overindebtedness may be merely a symptom of a deeper problem, not a cause, and DTI cutoffs may restrict access for those who benefit the most from it, at least in the short run.

Practically, even if one knew the magic threshold—and we assert this is not the case—there are definitional and measurement issues in implementing DTI cutoffs. For example, focusing on the DTI numerator, what counts as “debt”? All recurring and outstanding obligations, including utility and rent payments, or just those considered “loans” in a formal sense? All loans, or just those that report to credit bureaus? Further complicating any such rule, some LMICs lack credit bureaus entirely, while bureaus in richer countries still have substantial gaps in coverage for the types of “alternative” products used most intensively by lower-income consumers.

Another common approach is to diagnose overindebtedness based on symptomology, which involves noting an outcome in observational data and inferring that its proximate cause is overindebtedness. Default is a prominent example. Regulators sometimes look at more or less the same symptoms (e.g., high default rates) and make very different diagnoses (e.g., efficient risk-based pricing that serves high-

risk borrowers or overindebtedness). Sometimes they look at very different symptoms (e.g., both low and high default rates) and make more or less the same diagnosis (e.g., of overindebtedness). Low default rates are potentially indicative of a debt trap in which borrowers keep refinancing instead of defaulting (Karlán, Mullainathan, and Roth 2019; Skiba and Tobacman 2018). High default rates are potentially indicative of overindebtedness based on the deceptively simple intuition: “How can the transaction be good for the consumer, if the lender does not expect to get repaid per the original amortization schedule?” However, this intuition ignores canonical insights from finance: Flexibility has value for the borrower and shifts risk from the borrower to the lender, benefiting the borrower in most cases. Indeed, the microcredit movement has an odd relationship with default as a metric of success; while it is heralded for high repayment rates (often at or near 100 percent), at such high repayment rates lenders are unlikely diversifying risk across borrowers, and thus high-risk, high-return projects are likely going unfunded. Some default is a sign of a healthy market. All told, regulators are likely to go astray if they try to diagnose overindebtedness from default rates.

Conflicting diagnoses also arise from observing that consumers want to borrow more and do so when given the opportunity. Empirical evidence abounds that liquidity constraints are binding, thus leading many regulators to infer that market failures like asymmetric information lead to people being *underindebted*, which then renders people unable to make potentially profitable investments, smooth consumption, or buy desired durable goods. Indeed, the microcredit movement is predicated on this inference. However, extrapolating from this inference to promoting credit access requires the additional assumption that consumers make borrowing decisions in their own best interest. As we discuss next, that assumption pushes against the core concern of consumer financial protection policy.

B. Defining the Problem: Academic theory and empirical evidence

How do academics diagnose overindebtedness, or lack thereof? Absent a clear normative definition, academics focus on diagnosing the consumer decision failures and market failures that can cause overindebtedness.

i. Theories of consumer decision failures

Formal academic models have shown that several types of decision failures can, in theory, generate overborrowing.

One type is present focus, formerly known as present bias (D. Bernheim and Taubinsky 2018). The present-focused consumer is always “living for today”, exhibiting relative impatience when making tradeoffs between the short run and longer run, while being more patient with respect to tradeoffs between different points in time during the longer run. In this sense, present focus is only a decision failure from the perspective of the “long-run self”.

Laibson (1997) formalizes how present focus can generate undersaving (the mirror image of overborrowing). Concerns particular to consumer credit markets include increasingly frictionless take-up; the relatively easy “on-boarding” of digital credit, for example, should make it more attractive to present-focused consumers than traditional microcredit. Present focus can also induce borrowers to postpone repayment (e.g., Allcott et al. 2020), raising concerns about overindebtedness from failure to pay down debt on an optimal trajectory.

Two other potential behavioral biases, exponential growth bias (EGB) and limited attention, can lead to underestimation of borrowing costs, which can in turn lead to overborrowing through price elasticity. Exponential growth bias (EGB) can affect both underestimation of borrowing costs and undervaluation of savings returns (Stango and Zinman 2009; Levy and Tasoff 2016). Inattention to shrouded and complex

prices is covered in Section 2, and can lead to lower levels of elasticity with respect to price than would be observed with full attention.

Many other commonly cited decision failures, including loss aversion (e.g., Page 2017), overconfidence (e.g., Brunnermeier and Parker 2005), lack of financial literacy (Lusardi and Mitchell 2014), and scarcity (Mullainathan and Shafir 2013), have underdeveloped or unclear theoretical predictions on overborrowing. For example, a lack of financial literacy could deter a consumer from borrowing, but it could also lead a consumer to make mistakes that sometimes lead to taking on too much debt, and other times to taking on too little debt.

A key question about decision limitations is the extent to which consumer meta-awareness dampens any harmful tendencies (in this case toward overindebtedness). An influential body of theory predicts that meta-awareness of one's present focus is unlikely to substantially dampen tendencies toward undersaving; specifically, this work finds that anything less than perfect sophistication about one's present focus leads to large consumer welfare losses (e.g., Heidhues and Köszegi 2010).

However, recent work shows that this prediction only holds when the future is easily predictable; under the more realistic assumption that consumers face substantial uncertainty about the future (specifically, about the value of consumption or liquidity to them), consumer choices and welfare change smoothly, not discretely, with changes in the degree of meta-awareness (Allcott et al. 2020). The empirical implication is that it is important to diagnose the *degree* of consumer naivete about their decision-making limitations, not just whether they have any naivete at all. This work is important because a growing body of theory shows that the degree to which individuals learn about their limitations over time can be very limited (e.g., Gagnon-Bartsch, Rabin, and Schwartzstein 2020).

ii. Diagnosing specific consumer decision failures

In part because of the difficulty attributing behaviors to specific decision failures (Kremer, Rao, and Schilbach 2019), empirical tests of links between specific decision failures and overindebtedness in LMICs is scant at best. Two pertinent findings are: (1) a lesser-known behavioral bias, preference for certainty, is correlated with less loan delinquency in Afghanistan (Callen et al. 2014); (2) in India and the Philippines, paying off the debt of individuals who borrowed repeatedly from moneylenders did not lead to reduced levels of debt a year later, and no particular proxy for a decision failure, nor a financial education intervention, mediated the likelihood that the borrower returned to moneylender debt (Karlan, Mullainathan, and Roth 2019).

Casting the net more broadly to include evidence from richer countries, Lusardi and co-authors find that lower financial literacy is correlated with more use of high-cost credit (Lusardi and Scheresberg 2013) and self-assessed overindebtedness (Lusardi and Tufano 2015). Carvalho et al. (2020) shows that lower "decision-making ability" (DMA), as measured by exhibiting coherent preferences and avoidance of dominated choices in stylized tasks, is correlated with more use of high-cost credit in Iceland.⁵ Meier and Sprenger (2010) finds that present-focused money discounting is correlated with more credit card debt. Allcott et al. (2020) finds that payday borrowers are substantially present focused but, on average, have a high degree of sophistication about their present focus. However, other results in that paper, and others (e.g., Acland and Levy 2015), suggest that learning about one's limitations may be highly context- and

⁵ One might hypothesize that low financial literacy and low DMA are related, since they might both measure tendencies to make (financial) mistakes, but the results in Stango and Zinman (2020b) suggest otherwise.

product-specific, implying that regulators should not assume that consumer experience will not readily extrapolate to new market conditions or other product markets.⁶

Notably absent from this limited body of work linking decision failures to borrowing decisions is any field evidence that scarcity or related phenomena negatively affects the quality of borrowing decisions (although see Burke et al. (2020) for some circumstantial evidence). It also bears emphasizing that scarcity could in theory lead to *better* decision-making about debt, if liquidity is the key margin of scarcity and hyper-focus. Also absent is any evidence linking loss aversion to potentially problematic borrowing decisions; if anything, circumstantial evidence points in the opposite direction.⁷

iii. Theory and evidence on market failures that produce oversupply of credit

Why might lenders oversupply credit to consumers? Although many of the most canonical market (and regulatory) failures predict *undersupply*, certain market failures can lead to oversupply (see Zinman (2014) for a more comprehensive review). Some relate to profiting from consumer decision-making limitations, as discussed above in Section 2. Another, that more directly impacts credit quantities, is a negative repayment externality where a loan from Lender A increases the likelihood the borrower defaults on a (already outstanding) loan from Lender B. The one empirical paper we know of on this important question finds large positive externalities for high-credit-score borrowers, and large negative externalities for low-score borrowers, in the Mexican credit card market (De Giorgi, Drenik, and Seira 2020).

iv. Circumstantial evidence on decision and market failures, and its limitations

A small body of empirical evidence identifies plausibly causal downstream effects of increased access to consumer credit on proxies for consumer welfare, such as financial condition and subjective well-being. Focusing on LMICs, Karlan and Zinman (2010) finds that access to high-cost installment loans in South Africa improves a range of downstream outcomes but increases stress.⁸ We are not aware of any randomized evaluations on the effects of digital credit, although a number are in progress. A regression discontinuity evaluation of a digital credit expansion in Kenya finds improved consumer financial resilience (Bharadwaj, Jack, and Suri 2019). The randomized evaluations of traditional microcredit find positive effects on a right tail but typically null average effects and, importantly for the overindebtedness issue, no presence of a fat left tail (Banerjee, Karlan, and Zinman 2015; Meager 2019; 2020).

Although encouraging, the evidence that more consumer credit access leads to better consumer outcomes does not actually produce clear policy prescriptions, due to four often overlooked yet important limitations. First, even if we were to take the findings at face value, the question would remain: How much should access be expanded? For example, there may well be non-linearities, such as an inverted U-shaped relationship between access and consumer welfare. Second, and again taking results at face value, improved consumer welfare from *one* form of credit does not imply that the *optimal* policy response is to expand access to credit. Even consumers subject to decision failures might benefit from expanded access to one form of credit (e.g., formal consumer loans) by switching away from more expensive forms of credit (e.g., moneylenders). If this channel is driving the results, the best policy option may be to restrict access to *all* forms of credit, although that requires un-siloed rules and enforcement capacity. Third, the affected consumers in these studies might differ from those affected by new policy restrictions—in the

⁶ See also Stango and Zinman's (2020a) findings that behavioral biases tend to be stable within-person over time.

⁷ Chapman et al. (2019) and Stango and Zinman (2020b) find evidence that loss aversion is *positively* correlated with cognitive skills, in contrast to most other behavioral biases, in representative U.S. samples.

⁸ A small related literature uses randomized evaluations to examine the effects of expanding credit access on the take-up of policy-favored durables and related downstream impacts; see e.g., Tarozzi et al. (2014) on bed nets and Berkouwer and Dean (2020) on energy-efficient cookstoves.

studies cited here, for example, many of the marginal consumers are getting new access, whereas with new policy restrictions many of the marginal consumers would be existing borrowers who lose access. Fourth, no generally accepted summary statistic measure exists for consumer (financial) well-being, raising the question whether this literature is measuring (all of) the right downstream outcomes. This is a particularly important issue if there are tradeoffs, with some consumer welfare proxies improving with expanded access and others declining (Karlan and Zinman 2010).

A second body of evidence examines the effects of restricting, rather than expanding, credit access. Studying restrictions, to date, requires examining natural experiments (rather than conducting randomized evaluations). Breza and Kinnan (2020) is of particular importance, and finds strong evidence of negative general equilibrium consequences from industry-wide shutdowns of microcredit, as a result of the microcredit crises in Andhra Pradesh. Other work on restrictions of credit has focused on payday loans in richer countries and found mixed results (see, e.g., Allcott et al. (2020) for a recent review).

A third body of evidence examines the effects of debtor protection policies such as bankruptcy on borrowers.⁹ This literature has focused on quasi-experimental studies of richer countries and finds large positive effects on outcomes like earnings, foreclosure, and mortality, most strikingly in Dobbie and Song's (2015) U.S. study.¹⁰ This evidence, albeit important, has many of the same limitations described above. Moreover, evidence that debt reduction helps distressed borrowers on the "back end" (ex-post, in academic parlance) does not imply that policymakers should restrict credit access. Instead, consumers may be making sound upfront decisions, experiencing bad luck, and then benefitting from the insurance that debtor protections, including limited liability, are designed to provide. Filing for bankruptcy, or otherwise defaulting, is no more clearly a bad outcome for the consumer than filing an insurance claim.

A fourth piece of field experiment evidence documents the powerful effects of persuasive advertising on high-interest loans in South Africa (Bertrand et al. 2010). Such effects are difficult to square with classically rational demand for debt, particularly given that the study's sample had previous experience with the advertised product and advertising lender.

Perhaps the most compelling circumstantial evidence comes from bringing observational data to quantitative structural models of expensive borrowing. Laibson and co-authors (2020; 2020) find that a model with substantial present focus and complete naivete thereof can reproduce key empirical regularities of U.S. household balance sheets, including high levels of credit card debt, while classical time-consistent discounting cannot.¹¹ Having said that, the complete naivete required for this inference is a strong assumption; evidence suggests that consumers learn, at least about specific products, when experience produces clear, repeated and consistent feedback (e.g., Allcott et al. 2020). This makes one wonder whether other decision failures besides present-focus drive decisions around (over-)borrowing.

C. Interventions and evidence on their effectiveness

We review common and newer policy and programmatic approaches that focus on preventing overborrowing.¹² First, price and loan size caps, which seek to reduce the quantity of credit supplied, are

⁹ Some countries also have legally-sanctioned, industry-coordinated debt consolidation and restructuring programs, like consumer credit counseling services in the U.S. (see, e.g., Dobbie and Song 2020).

¹⁰ There is a related literature, again focused on the quasi-experimental studies of richer countries, on the effects of debt overhang on consumer outcomes. See, e.g., DiMaggio et al. (2020) for a recent review.

¹¹ See Kremer et al. (2019) and Beshears et al (2018) for discussions of related puzzles around household consumption dynamics and balance sheets that reach a similarly speculative conclusion on the likely importance of consumer decision-making limitations.

¹² See Section 2.C for interventions focused on lowering borrowing costs, and Section 3.B-iv for a brief review of debt forgiveness interventions.

common but have not been evaluated with randomized evaluations. Nor have restrictions on DTI or repeat borrowing, although Allcott et al.'s (2020) simulations suggest that the latter are more likely to benefit present-focused borrowers than price or loan size caps.

Turning to regulations and programs that provide information and/or nudges, Seira et al. (2017) tests traditional disclosures, and debiasing and social comparison nudges, on a large sample of credit card borrowers in Mexico, and finds little evidence of effects on behavior. Adams et al (2018) shows that a nudge away from making minimum credit card payments in the U.K. affects behavior but not outcomes; payment amounts to the nudging lender increase, but overall repayments and other outcomes do not change substantially. Similarly, Bertrand and Morse (2011) shows that providing innovative disclosures to first-time borrowers, in the form of information about costs of product usage and the likelihood of repeat borrowing, reduces borrowing from the nudging lender.¹³

Financial education interventions provide some combination of actionable information, teaching of basic principles, and advice. Kaiser et al.'s (2020) meta-analysis finds that such interventions have modest average effects (0.04 standard deviations) on various borrowing behaviors across 22 randomized evaluations, many of which were conducted in LMICs. We are unaware, however, of findings with a strict focus on overindebtedness that would validate the use of financial education for this specific issue.

D. Open questions

We view research on diagnosing and treating overindebtedness to be wide open, and briefly summarize our views on which questions merit particular focus.

One is outcome measurement. In principle, it is possible to directly measure overindebtedness, and researchers would do well to take methodological guidance from the painstaking efforts of Benjamin, Heffetz, Kimball and co-authors (e.g., 2020) to improve the measurement and interpretation of subjective well-being. In the meantime, useful starting points are the questions developed by Lusardi and Tufano (2015), and the CFPB's Financial Well-Being battery (CFPB 2017).

A second key line of inquiry is linking specific decision and/or market failures to borrowing decisions. As discussed above, empirical evidence lags far behind large bodies of influential theory. A related question is how various decision and/or market failures interact to affect consumer welfare and its implications for policy. One proposed approach is to presume that any consumer decision failures have reinforcing effects on outcomes (for example, all pushing toward overindebtedness) and use a "reduced form sufficient statistics" approach to welfare and policy analysis (e.g., Chetty (2015)). This assumption has not been well-validated empirically, though Stango and Zinman (2020b) makes a start in this direction, and these models are only beginning to accommodate the cross-consumer heterogeneity that we suspect is important empirically (e.g., Allcott, Lockwood, and Taubinsky 2019). Another approach theorizes that limited attention is a foundational bias that produces many others (Gabaix 2019); this is important to test as it implies that treating attention could cure many presumed decision-making ills.

As noted at the outset, understanding decision failures is absolutely critical for policy design, in part because decision failures can turn on its head the canonical result that increasing competition improves outcomes. As such, empirical studies of decision failures and provider responses to competition, in separate and in tandem, will be most useful; as Heidhues and Koszegi (2018) concludes, "the theoretical

¹³ Donnelly et al. (2020) finds that a product feature giving consumers the option to allocate repayments to particular purchases, tested with a large credit card issuer in Australia as a novelty but widely available in many other countries, increases repayments to that issuer. However, results using only the random variation engineered by the experiment are imprecise.

side of behavioral industrial organization is far more developed than the empirical side”. The repayment externalities discussed in part B-iii also motivate further scrutiny of competition’s effects on consumer and market outcomes, since more competition—specifically more lenders—could exacerbate any negative externalities and lenders’ incentives to meet consumer demand for overborrowing.

A third key line of inquiry is unpacking the (mal-)functioning of advice markets, whether they presently cure any decision failures, and how they might be redesigned or regulated to produce better outcomes. Most research and practice has focused on the asset side of household balance sheet, but, as Zinman (2015) argues, debt is where the money is for low and moderate income consumers.

Another key line of inquiry is measuring the extent to which pro-savings interventions affect debt loads and overall financial condition. We emphasize the importance of viewing such interventions as potential *preventative* measures when it comes to overindebtedness and targeting them to people who are *not* currently borrowing expensively. Conversely, nudging or shoving consumers who are borrowing expensively to start saving at relatively low rates of return, as many microfinance institutions and “asset-building” programs do, is a dubious strategy in the absence of empirical evidence that such interventions produce very powerful savings habits.

Finally, although we hesitate to recommend policy experimentation without a firmer empirical evidence base on the extent and nature of overindebtedness problems, we do concede that it could be fruitful to test innovative approaches suggested by theory. Examples include waiting periods,¹⁴ consumer licensing, and restrictions on repeat borrowing.

3. Post-contract Exploitation

A. Defining the Problem: Regulator perspective

In addition to the set of “upfront” *pricing* problems that arise during the marketing of financial services addressed in Section 2, various “post-contract” problems can arise after consumers take-up services. Examples include agents charging extra transaction fees, add-on of services without consent, and debt collection harassment. Misconduct may violate contracts with consumers and/or be illegal, and also may result from intentional corporate practice or client-facing employees acting independently. The malfeasance may be costly to individual consumers, but also creates broader societal costs by reducing trust in financial institutions. We later introduce complaints resolution as a potential solution to these problems; however, the under-provision of complaints channels and resolution mechanisms may itself be considered another post-contract violation.

The growth of LMIC consumer financial markets has been fueled in part by the expansion of agent networks, such as small retailers doubling as lower-cost alternatives to bank branches and enabling customers to make deposits, withdrawals, money transfers, and payments of loans more conveniently. FSPs face a trade-off between broadening their agent network and effectively training and monitoring their agents (Agent Network Accelerator 2016), leading to growing concerns among regulators about the quality of the service delivered.

Supervising companies often either allow agents to set their own fees or don’t provide oversight to prevent this behavior. In the early days of mobile money, agents in the Philippines and Tanzania were allowed to set their own fees to foster competition. More recently, even agents who are formally given

¹⁴ Burlando et al.’s (2020) regression discontinuity evidence from Mexico, which finds that doubling digital credit disbursement time from ten to twenty hours substantially reduces default rates, provides some empirical motivation for experimenting with waiting periods.

tariff structures often informally set their own prices for commissions, as fees are paid in cash and not recorded in the system (Davidson and Leishman 2012). In Nigeria, an estimated 45 percent of agents decide and set customer prices, ignoring those set by FSPs (EFInA 2018).¹⁵

Service add-ons without clear customer consent is also prevalent, although empirical evidence on this is limited and comes mostly from the relatively small set of regulator enforcement actions. Anecdotally, some add-ons are imposed through opt-out defaults that exploit disclosure and consumer decision limitations discussed above. Others are more proactive; India's Bharti Airtel, for example, was sanctioned by regulators for opening Airtel Payments Bank accounts without customer consent and linking those accounts to customers' national digital ID so that—unbeknownst to the customer—public subsidy payments would flow to the accounts by default (Singh 2017).

Abusive debt collection practices also seem prevalent. Prohibited or discouraged practices include attempts to collect debts that have been repaid or forgiven, dunning, public shaming, threats, contacting family members or co-workers, house visits, and failing to disclose delegation to a third-party collector. Abusive collection of digital credit debts in Kenya has attracted global attention, though the continued prevalence of enforcement action in richer countries also suggests that their long-standing protections have not fully deterred prohibited conduct.

B. Defining the Problem: Academic theory and empirical evidence

We characterize these post-contract problems as reflecting *supply-side moral hazard*, by which we mean that problems arise because the provider does not adhere to the terms of the contract or the law with regards to how they charge fees, register clients for services, or collect on loans.¹⁶ The supply-side moral hazard problems addressed here broadly arise from imperfect contracting between customers and firms, and more specifically may reflect the market power of individual firms, principal-agent problems related to corporate governance, and limited accountability arising from weak regulatory and legal institutions.¹⁷

Market Failures: Theory and Empirical evidence

Although competition levels vary widely across LMIC markets, certain stylized facts are common. Rural consumers have limited payment, credit, and savings choices, and local mobile money agents and bank branches may enjoy local monopolies. This market structure has led to policy interventions such as prohibition of exclusivity clauses in agent-mobile money operator contracts in Kenya and Uganda. Additionally, mobile money tends to be dominated by one or two large firms per country. Even in Kenya and Tanzania, where digital credit startups have proliferated in recent years, the markets are still dominated by just two and three top firms, respectively (Izaguirre, Kaffenberger, and Mazer 2018).

Several classes of theories suggest that more competition does not necessarily produce better market outcomes, and so increased competition alone may not be sufficient to address post-contract exploitation. We discuss the potential impact of consumer decision failures and repayment externalities in other sections (including Section 2, above), and focus here on network externalities. These are potentially important in payments and other platform markets (e.g., Crouzet, Gupta, and Mezzanotti 2020). The literature on “contestability” versus competition demonstrates that market concentration in network

¹⁵ Note that this charging of extra fees by the agent also relates to the theme of price shrouding mentioned above, but differs slightly in that it often violates company policy on tariffs and, therefore, could not have been disclosed at the time of registration.

¹⁶ We note that there may be cases where an agent of the institution does not violate the terms of the contract per se, but does manipulatively guide the client to an option or service that is clearly not in their best interest.

¹⁷ Extra fees charged by agents could also theoretically be explained by high switching costs and models with limited consumer comparison as covered in the previous section.

industries does not necessarily imply poor consumer experience, as even monopoly providers must compete to forestall entry threats.¹⁸ However, concerns exist about anti-competitive behavior in digital economies. For example, mobile money competitors have been blocked from accessing USSD channels; digital credit lenders have failed to comply with credit information sharing systems; and, anti-competitive behavior by internet platforms have led to anti-trust actions by European and U.S. governments.¹⁹

Of course, agent pricing practices could reflect pro-competitive pressures rather than rent extraction or unintended consequences of competition. There is evidence of tight agent profit margins, and self-reports indicating lack of commercial viability if limited to the official tariff structure (e.g., Brown et al. 2019).

C. Interventions and evidence on their effectiveness

Evidence from various interventions provides insight on how to make complaints channels more accessible, dispute resolution more effective, and market monitoring more efficient. We also share some limited evidence on how enhanced retail competition can benefit consumers, how deepening of consumer finance usage may lead to self-regulation by retail agents, and how seemingly uncontroversial interventions on debt collection can have negative welfare impacts.

Complaints channels: Complaints processes are important for at least four reasons: solving customer problems, helping firms identify areas of improvement, assisting regulators in market monitoring, and discouraging malfeasance in the first place. However, there is evidence that financial customers in LMICs use formal complaints channels infrequently (e.g., Kantar InterMedia 2016), and that LMIC regulators are far less likely than high-income country counterparts to mandate that FSPs handle complaints, report complaints to the government, or inform customers of dispute resolution mechanisms (World Bank 2017). Technologies such as social media and chatbots can democratize access for communicating complaints and can streamline the process of FSPs responding with support services. Mazer and Onchieku (2019) identified and prioritized consumer concerns with financial services through the #KOT Kenyans on Twitter community, and Tully & Madrid Morales (2020) demonstrate the broad use of social media by consumers to communicate complaints.

Resolution: For small complaints, customer support or self-service FAQs may suffice. For example, the RegTech for Regulators Accelerator worked with the Central Bank of the Philippines to build a customer complaint chatbot to process customer complaints and found evidence that the prototype led to faster response turnaround time (di Castri, Grasser, and Kulenkampff 2019), although evidence of effects on outcomes is lacking. Resolution of larger disputes may require broader development of alternative dispute resolution systems and the legal system. There is some promising evidence on the impacts of improved legal representation for poor LMIC citizens. In an RCT in Kenya, Aberra & Chemin (2019) finds that free legal representation increased the security of property rights, which translated into greater credit access and agricultural production. Similarly, Sadka et al.'s (2018) randomized evaluation in Mexico City's labor courts finds that information about potential outcomes and conciliation services improves settlements for workers, but only if the plaintiff is present, suggesting attorneys do not share this information with their clients. The benefits of other approaches may be more dubious; mandated arbitration, for example, is a prevalent alternative resolution system and remains controversial—at least in part because it is often an attribute that is shrouded in consumer contracts.

Market Monitoring: Technological progress in the digitization of complaints channels, widespread use of mobile phones, and increasingly accessible high-powered analytical solutions, such as machine learning,

¹⁸ See Tirole (2017, 397–400) for an accessible description of *contestability*.

¹⁹ USSD (Unstructured Supplementary Service Data) is a protocol used to send text messages commonly used among LMIC mobile money providers.

may improve the ability of regulators to monitor consumer problems and improve enforcement. Tully and Madrid-Morales (2020) illustrates that social media monitoring can provide useful insights on consumer concerns. An experiment employing a phone-based system to monitor government support for farmers in India demonstrated substantial reduction in leakage with high returns on investment (Muralidharan et al. 2018) and examples from social protection monitoring in Andhra Pradesh, India, illustrate the potential of integrated digital solutions (Aadil et al. 2019)

Competition: Evidence shows that policy-induced increases in retail competition can benefit consumers through reduced prices and improved quality. Busso and Galiani (2014) randomizes entry of existing small neighborhood convenience stores into the network of retailers certified to accept welfare food vouchers in the Dominican Republic, and finds impacts on both prices and quality with larger price drops in areas with more entrants. Such retailers closely fit the profile of mobile money retail agents in many countries and serve the poorer population segments that are often the target of extra fees.

Consumer learning and agent self-regulation: In the near-term, extra agent fees may warrant intervention given that they are disproportionately paid by less empowered customers, including women, low-literacy citizens, and individuals living in remote areas (Brown et al. 2019). Over the long term, however, markets may simply mature out of this problem as consumers learn and agents self-regulate in response to more sophisticated consumers. An experiment with garment workers in urban Bangladesh demonstrated that workers with digitized wage payments were more likely to personally execute transfers through their own phones, avoiding the use of agents altogether, which led agents to change behavior (Breza, Kanz, and Klapper 2020). Another experiment among mobile money customers and agents in Ghana found that simply informing consumers about the official fees for common local transactions not only improved the experience of the treated customers, but also led agents to reduce their own misconduct (Annan 2020).

Debt collection harassment: Many countries have laws prohibiting abusive debt collection practices, though enforcement may be lax and weak judiciaries can hinder recourse for victims (World Bank 2017). Evidence on the efficacy of interventions to curb abusive debt collection is limited. Breza and Kinnan (2020) examines the impacts of an emergency ordinance that drastically reduced the ability of microfinance lenders to collect on loans, ostensibly to curb abusive lending and collection practices at the height of what is referred to as the Andhra Pradesh microfinance crisis (Cole and Saleman 2015). The policy was followed by almost universal default, which the authors find caused subsequent reductions in credit supply and led to significant decreases in household wage earnings and consumption. Interventions specifically targeted at collection practices might perform better, of course.

D. Open Questions

We identify three broad policy areas for further research: complaint and support systems; service quality and fees of agents; and debt collection practices.

Within open questions on complaint systems, we discuss three aspects: accessibility, causal impact, and institutional and operational design. Technology can transform complaint systems, making them more accessible, more timely, and ultimately more effective. However, much remains to be learned about how to achieve those goals, particularly for the most vulnerable. On the one hand, data costs, differential internet access, and language barriers may render digital platforms less accessible. On the other hand, the informality and lower cost of digital (versus in-person) complaints may offer greater visibility on vulnerable consumers. Further investigation on the selection of different consumer segments into and out of alternative complaints channels is needed.

We also need to learn more about the causal impacts of new systems on provider behavior, dispute resolution, trust in financial institutions, use of financial services, and general consumer welfare. To the

extent evidence suggests trust is in fact a positive externality in the spirit of public goods or coordination problems, for example, there may be a case public subsidy and/or coordination of the complaint system. Supporting this view, Stremmler & Osman (2015) argues that elders and traditional authorities' involvement in Somaliland has helped build trust in mobile money, despite low institutional capacity. In a potentially similar vein, the Reserve Bank of India maintains a financial service Ombudsman, though we are unaware of rigorous evidence on the consumer welfare impacts of these alternative dispute resolution schemes. Potentially furthering the public goods aspect, complaint systems could be analyzed and curated to produce real-time or even predictive insights on risks to consumers and markets more broadly.

Lastly on complaint systems, much could be learned on institutional and operational design. Testing, different institutional arrangements for market monitoring—whether through, for example, non-government civil society institutions, citizen elected special representative, or appointed ombudspersons—may suggest alternatives to standard government regulatory arrangements and illuminate some of the factors that might otherwise constrain regulator capacity, such as civil servant technical sophistication, budget limitations, interagency conflict, or industry capture. On operational design, experimentation with subsidized legal representation for poor consumers or accessible fair consumer finance arbitration processes when complaints are not satisfactorily resolved by FSPs would be instructive. Similarly, research on the use of, selection into, and satisfaction with automated complaint resolution technologies, such as chatbots, automated voice activated systems, and the backend artificial intelligence that run them would aid in informing which “regtech” technologies may warrant public investment and/or open source development, and which policymakers should mandate for FSP adoption.

Problems with FSP agent service quality and fees also offer an exciting focus for further research. Alternative approaches to inducing greater retail competition through both government policy and FSP corporate policy, and the impacts of competition on agent business models and consumer experience, are fertile grounds for new work. For example, more evidence from additional geographies on whether more competition leads to better consumer outcomes or more cutthroat consumer treatment would be valuable. Another key set of questions revolves around if and why FSPs are pricing services in a way that threatens agent economics and subsequently consumer experience, and how pricing could be better set to benefit social welfare. Whether deeper penetration of digital financial services increases dependence on agents or reduces dependence through mechanisms such as improved technological literacy (Breza, Kanz, and Klapper 2020) or less use of cash deserves further attention. The effectiveness of technical and behavioral solutions for better informing consumers about mandated fees and service quality, such as automatic voice calls or SMS when transfer amounts are slightly less than the common round number expected due to extra fees, are understudied. How to improve the agent experience for women and vulnerable customers also remains an important topic. Early evidence on expanding the role of women as agents to reduce the higher incidence of misconduct among female consumers is mixed (Annan 2020), though more research is clearly needed.

More generally, if and how downward pressure on extra agent fees affects the sustainability of the agent business model remains an important research question, as limitations on incentives to expand the distribution networks can impose social welfare costs by constraining financial inclusion among those still out of reach of formal financial services.

Finally, too little is known about debt collection and how to systematically diagnose harmful practices. Improved complaints and market monitoring approaches may aid in better describing the incidence and extent of harassment. New experimentation, for example with contract design for in-house collection staff and third-party collectors, would aid in guiding regulators and corporate managers while also addressing compelling theoretical questions on incentives and optimal contracting.

4. Fraud

A. *Defining the Problem: Regulator perspective*

Fraud is amorphous, and thus the detection challenge for regulators is complicated. Some frauds operate at the institutional level. This could be a Ponzi scheme, in which the entire entity is a fraud, a large-scale phishing operation, or internal corruption at a financial institution that puts consumer deposits at risk. Other frauds are perpetrated by individuals, such as fraudsters engaging in informal, fraudulent transactions; faking SMS messages to engender trust and extract money or goods; impersonating a formal financial institution in order to abscond with funds; creating a personal lending app that lures users to pay fees to obtain access to cheap loans, but which offer no actual service; or phishing for passwords or account numbers. There can also be problems with the fraudulent agents of legitimate institutions, such as mobile money agents who cheat clients, or bank employees that in some way steal from clients and/or the firm (Buku and Mazer 2017).

There is considerable overlap from a regulator's perspective between these types of fraudulent activities and the post-contracting exploitation issues discussed in Section 4. The difference is severity and intent: here we discuss situations where an individual or institution is trying to consummate a transaction fraudulent at its core—likely criminal (rather than deceptive) and certainly manipulative.

We recognize, but do not discuss, fraud that falls under the purview of prudential regulators (such as willful disregard for liquidity, leverage, or risk management rules), and as such also do not discuss deposit insurance in low-income countries, an important protection for consumers depositing with regulated institutions (Demirgüç-Kunt, Karacaovali, and Laeven 2005).

B. *Defining the Problem: Academic theory and empirical evidence*

Fraud occurs when market and legal forces exacerbate or otherwise fail to counterbalance the greed and corruption that underlies motivations for fraudsters. Consequences of fraud can be examined at two levels: direct effects and indirect market-level effects.

The first-level effect are the direct costs to the consumer from loss of funds and time, and stress. Such direct costs are particularly of concern given the likelihood that low-income or discriminated-against consumers are the more likely victims of fraud. Over 20 percent of banked respondents in Uganda, Kenya, and Nigeria reported losing money to fraud or scams, or paying illegal extra fees (such as bribes) in the prior six months in Kantar InterMedia's 2016 Financial Inclusion Insights Survey. Two Ponzi schemes in Colombia in 2008 had over half a million customers, with invested funds equal to 1.2 percent of annual GDP (Hofstetter et al. 2018). In Uganda, staff and agents at a mobile money provider pilfered millions of dollars from mobile money accounts (Morawczynski 2015).

With many experiencing direct fraud, it is likely—with face-to-face gossip and social media—that almost everyone knows someone who has been defrauded. This direct effect may therefore cause an indirect negative-externality effect: a general equilibrium market failure where “good” transactions are not consummated because consumers cannot distinguish good from bad actors. This then means, for example, that consumers who want to save in a financial institution do not, or that those who want to send money via a mobile agent do not. These foregone, mutually beneficial trades imply reduced social welfare.

The available evidence suggests that indirect effects are also prevalent and costly. For example, the Mexico World Values Survey finds that trust in banks is especially low among the poor: 71 percent of those with less than a primary school education report low trust in banks, compared to 55 percent of those who completed primary school, and 46 percent of those who completed university. In addition, 24 percent of a sample of low income households that receive a government cash transfer reported not saving in the bank account that receives the transfer because they did not trust the bank (Bachas et al. 2020). In Kenya,

a survey in three rural areas found that 8 percent of people who knew of a particular local bank did not trust it (Dupas et al. 2016). In Uganda and Malawi, in a sample of poor, rural households, 16 percent in Uganda and 5 percent in Malawi report knowing someone who has lost money deposited in a bank and 60 percent in Uganda and 16 percent in Malawi report having little or no trust in banks (Dupas et al. 2018). Similarly, and also in Uganda, only 44 percent of a sample of young adults groups believed savings in a bank would definitely not be stolen (Horn et al. 2020). Further evidence come from a myriad of sources, although differences in sampling and question wording make comparison difficult. Experimental evidence on such indirect effects come from the USA mortgage market, in which “good” offers were met with suspicion by potential borrowers, implying that borrowers were unable to separate good from untrustworthy mortgage offers (Johnson, Meier, and Toubia 2019).

Social media can be used to quickly and cheaply detect fraud and spread information on untrustworthy entities. It empowers individuals by amplifying their voice and also reduces the cost dramatically for regulators to hear from people about potential cases of fraud. Naturally, this requires a regulatory entity that tracks social media to identify fraud, then investigates and adjudicates, as finally returns to social media to communicate effectively and rapidly to stop further growth of the fraud.

However social media can also exacerbate the general equilibrium problem, a classic herding problem (Banerjee 1992). Disinformation can originate from the fraudster, aiming to spread viral information about false, positive outcomes (such as lottery payouts). Disinformation can also take good actors down, for instance converting an innocuous story of consumer disagreement into a viral, negative story. Social learning has always posed risks for localized disinformation, but social media can exacerbate such effects.

The challenge for regulators using social media is thus two-fold, with the added complexity of coordinating around a free-rider obstacle: how to build a platform that guides people to trust what they should trust, and mistrust what they should mistrust. Ultimately, the challenge of generating proper incentives to report credible information at scale, and to make it easily and rapidly accessible and believable is no different than the challenge media literacy advocates face across the world. Recent work from the political context suggests that limited attention (as opposed to mere confirmation bias) is a key factor in the spread of fake news and other disinformation (Pennycook and Rand 2020). This finding is encouraging, as it suggests that increasing the profile of accurate sources can shift both trusting and distrusting people towards more accurate beliefs about what to trust and what not to trust.

C. Interventions and evidence on their effectiveness

Broadly speaking, classical economic theory suggests that competition could reduce fraud. However, competition need not reduce fraud if consumer have decision failures, as discussed in the Introduction. We are not aware of any empirical evidence in either direction.

Interventions that more directly address fraud can be divided into four types: institutional oversight such as banking supervision (which, as explained above, is not within the scope of this review); consumer complaint and redress processes (within the scope of this review, but covered earlier in Section 4), and interventions which aim to inform consumers upfront, before fraud occurs, to help separate well-intentioned actors from fraudulent ones. We focus on the latter.

Evidence on information campaigns to build trust are scant, but—with increasing government use of mobile money for cash transfer payments and government oversight of agent systems—such interventions are increasingly viable and important. In Peru, a randomized evaluation of a three-hour training session designed specifically to build trust in financial institutions for beneficiaries of a conditional cash transfer program finds a 33 percent increase in trust and a corresponding increase of 1.6 percentage points in the savings rate from the cash transfers (Galiani, Gertler, and Navajas-Ahumada 2020). A recent meta-review

of financial education programs finds an overall effect size of about 0.20 standard deviations on “financial knowledge”, which does suggest promise for inclusion of more content on trust into financial education efforts (Kaiser et al. 2020). There are also opportunities to glean from the social learning literature (e.g., Chandrasekhar, Larreguy, and Xandri 2020).

Consumers can also learn through direct experiences. In Mexico, for the cash transfer recipients cited above, for example, recipients left small amounts in their accounts after receiving debit cards and repeatedly checked the account balance to ensure that their money was still there. Over two years with the card, the number of account balance checks fell substantially and beneficiaries’ probability of reporting a lack of trust in the bank fell by 33 percent (Bachas et al. 2020). Similarly, being given fee-free savings accounts decreased the mistrust of poor, rural households from 60 percent to 50 percent in Uganda but not in Malawi, where the base mistrust rate was an already-low 16 percent (Dupas et al. 2018).

In the Uganda young adult sample mentioned above, we learn about both information campaigns and direct experience, via a three-arm study comparing financial education (which includes components on how to learn which financial institutions are trustworthy), savings account access, and both (Horn et al. 2020). The financial education only treatment arm generated important increases in trust both one and five years after the treatment, whereas the treatment arm that offers account access only had no effect. The treatment arm with both did have larger effects than the financial education only treatment arm at one year, although at five years the difference was large but no longer statistically significant. This suggests that financial education alone can improve trust, and that experience using a savings account may amplify such effects.

D. Open questions

Evidence on detecting, preventing, and treating fraud, as well as on building a good balance of trust and wariness in consumers, is heavy on anecdotes and light on systematic, rigorous evidence.

For detecting and addressing fraud directly, new regtech opportunities abound both for new approaches to regulation and for research to test their effectiveness. Regulators with the proper capacity could scrape social media to identify fraud, investigate cases, and then use social media again to warn the public about specific cases. More comprehensively, regulators could feed such information to a public report card platform (such as an app or a website) that identifies both bad and good actors. This could also incorporate issues raised in the Post-Contract Exploitation section. Research can draw from computer science to build tools for scraping and analyzing natural language posts in social media, as well as from social science and law to learn how best to promote and evaluate such efforts. Further work, likely from a combination of computer science and accounting, can also develop methods for firms to conduct stronger internal audits of transactions to identify potential agent fraud (West and Bhattacharya 2016).

For educating and persuading consumers, opportunities abound to meet three key programmatic and research challenges head-on. One is measuring trust, and—given that more trust is not always better—developing success metrics for interventions. Another opportunity is learning how to strike a better balance between context-relevance for a given study versus universality for cross-study comparability. Existing measures, such as the World Value Survey questions, ask about overall instinct aiming to capture more of a personality measure, and laboratory experiments such as the trust game have proven questionable for measuring trust (e.g., Schechter 2007). A third opportunity is testing to find the right balance between providing actionable advice (“These are the types of providers you should (mis)trust.”) and teaching consumer skills, frameworks, and principles to apply in novel situations. Financial literacy and advice interventions grapple with similar issues, so presumably there are gains from trade between intervention designers in related domains. It also will be critical to test whether and how various factors

mediate the effects of content designed to improve trust in trustworthy actors and increase wariness of untrustworthy ones. Potential mediators include content medium, information source, endorsements, and baseline (mis)information environment.

5. Discrimination

Discriminatory practices towards women, minority groups, and other potentially vulnerable groups rightly concerns regulators (e.g., Toronto Centre 2019). Given its overlap with the voluminous literature in other fields in economics and law (e.g., see Charles and Guryan 2011; Bertrand and Duflo 2017) as well as gender policy (e.g., see Jayachandran 2015), we limit this section to a broad overview with pointers to pertinent literatures.

A. *Defining the Problem: Regulator perspective*

Pernicious discrimination happens when an FSP treats individuals worse because of their group affiliation (n.b. group affiliation may be misperceived, by the FSP). Examples include discouraging individuals from engaging with the FSP, lowering approval rates, excluding outright, or charging more. Although discriminating based on creditworthiness is accepted practice (risk-based pricing), many countries prohibit the use of personal demographic factors in approval and pricing decisions (Morse and Pence 2020; World Bank 2017). Despite digital finance's relative youth, many FSPs have already been scrutinized and/or sanctioned for discriminating (Annan 2020; Matiwane 2019).

The most common regulatory approach to defining and detecting discrimination in consumer financial services is not based on pernicious intent—which is often difficult to ascertain, as discussed below—but rather on “disparate impacts”. Under this standard, the key question is whether a protected group gets worse outcomes (e.g., loan approval rate or contract terms) after controlling for all other relevant characteristics.

An additional concern for regulators is that that increased reliance on technology may exacerbate inequalities. Access to a mobile phone is usually a prerequisite for using digital financial services and—although mobile phone ownership is on the rise globally—women are 6.6 percentage points less likely than men to own a mobile phone (GSMA 2020). The Economist Intelligence Unit's Inclusive Internet Index 2020 finds a 34.5 percentage point gender gap in access to the internet in the least-developed countries. In addition, an ID is a requirement for accessing financial and public services, and 44 percent of women in lower-income countries do not have an ID, compared with 28 percent of men (World Bank 2019). The poor are also less likely to have access to phones, internet, and IDs. Regulators worry about barriers that reliance on technology will create for these populations if underlining gaps are not addressed. At the same time, sharing information like photos and phone numbers online or with agents has risks for women and marginalized populations in some countries including harassment, blackmail, and shaming (Sambasivan et al. 2019).

B. *Defining the Problem: Academic theory and empirical evidence*

There are two main models of discrimination in the economics literature. In “taste-based discrimination”, decisionmakers dislike marginalized groups. In “statistical discrimination”, the differential treatment of members of the discriminated group is due to imperfect information, with decisionmakers making inferences based on group characteristics rather than individual characteristics, thus making discrimination the result of a signal extraction problem. Bohren et al. (2019) argues that often what is called “statistical discrimination” is actually rooted in inaccurate beliefs that are themselves biased against the discriminated group (see also Bordalo et al. 2016 for a model of inaccurate stereotypes). This distinction is quite important for thinking about how to address the issue of disparate treatment: if the “profit-maximizing” policy is in fact discriminatory (as with accurate statistical discrimination), this

suggests a deeper societal challenge, one that requires regulation, monitoring, and/or perhaps subsidy, to address. If on the other hand firms or their agents forgo profits to discriminate based on inaccurate beliefs or, worse-yet, “taste”, then more direct punitive regulatory policy may be appropriate.

Discrimination can happen at various stages in the contracting of financial products. First, in the customer acquisition or discouragement stage, through decisions about e.g., physical location, advertising, and customer service. Second, in the contracting stage (see also Sections 2, 3, and 5 above). Third, in the post-contracting stage (see also Sections 4 and 5).

Detecting discrimination in customer acquisition stage faces the particular challenge of lacking data on those discouraged from engaging with an FSP due to expected or realized discrimination. The two main methods for addressing this challenge are audit studies (discussed in Section 4 and below) and the Becker (1957) “outcome test”. The latter estimates whether services extended to a protected group have relatively high marginal profits, controlling for all other relevant characteristics. Higher marginal profits indicate that the FSP is discriminating by not competing for clients in the protected group to the point where contract terms are set per a competitive equilibrium, as e.g., Dobbie et al. (forthcoming) find is the case in U.K. consumer lending.

Algorithmic or machine learning assessment of financial standing has the potential to reduce discrimination in face-to-face assessments, but recent evidence from richer countries finds that these models also can unintentionally generate differential treatment by race and gender (Fuster et al. 2017). In some cases, algorithms may be fairer than humans as they remove some of the personal bias that comes through in face-to-face interactions. In the US, Bartlett et al. (2019) compares algorithmic and face-to-face lending and finds that face-to-face lenders charge otherwise-equivalent minority borrowers 7.9 basis points higher for mortgage loans, costing \$750 million annually. Algorithmic lending also discriminates, but by 40 percent less than face-to-face lenders.

Evidence from LMICs is scant. Dvara Research finds that use of artificial intelligence in credit underwriting may disadvantage traditionally vulnerable segments of the population in India. BJORKEGREN & GRISSIN (2018) find that alternative credit scoring—using information like utility bills, mobile money history, social media and other online behavior, number of contacts, and psychometric evaluations—is expanding credit access to populations that may not have the documentation or collateral traditionally used during the loan approval process.

C. Interventions and evidence on their effectiveness

In 2017, 65 percent of 124 jurisdictions surveyed had laws prohibiting discrimination against certain populations in finance, while only 44 percent of low-income jurisdictions did (World Bank 2017). Such laws can be enforced through regulatory examinations that are part of the supervision process, through audits, or through complaints and the legal system. Countries including South Africa, India, and the United Kingdom cite financial services ombudsmen as helping to combat discrimination in finance (Izaguirre 2020). To our knowledge these approaches have not been rigorously evaluated in LMICs.

Some studies have evaluated debiasing campaigns for FSP staff and have found mixed results at best. For example, Montoya et al (2020) finds that male loan officers in Chile did not reduce their existing levels of discrimination against female loan applicants when presented with evidence that female borrowers have higher loan repayment rates than male borrowers. Nor did loan officers in the Philippines lend more to low-income households when presented with evidence that they are as creditworthy as wealthier borrowers and trained with a scorecard to estimate income quickly. In fact, there is some evidence that the training in the scorecard increased lending to wealthier individuals (Karlan, Osman, and Zinman 2019).

With respect to artificial intelligence and algorithms in fintech, regulation in LMICs to date has focused on data privacy and disclosing to consumers how their personal information will be used, and whether and how it will be shared (World Bank Group 2017). Due to the black box nature of algorithms, and the

fact that they evolve, developing frameworks for fairness and some degree of transparency is not straightforward. Researchers have made progress towards frameworks for ethical artificial intelligence and machine learning algorithms, but these have not yet been widely applied, in LMICs or elsewhere.²⁰

D. Open research questions

This is an open—and high priority—area of research, as much of what we discussed above is speculative. In particular, more research is needed on detecting discrimination in product markets where refusals or discouragement are hard to observe due to lack of data. Using audits that change the identity of the customer in terms of gender, race or ethnicity could be a fruitful avenue of research. Although much policy in LMICs is focused on gender issues, in many countries there are important oppressed groups (political, ethnic, or religious, for example) that warrant attention as well.

We also need further studies of agent or branch engagements. Are decisions driven solely by profit considerations, or do FSPs try to avoid certain areas that may be profitable but have a high density of oppressed groups? Likewise, to the extent that discrimination is agent-based—such as, for example, in mobile money operations—interventions that aim to shift social norms may also be appropriate, although they are outside the scope of this review.

Regulating black-box algorithmic lending (or other products) to prevent discrimination is an important and growing area, with attention from computer science and legal theory as well as economics. This is clearly a case where solutions ought to be able to travel across borders, from poorer to wealthier and vice versa.

6. Conclusion

We cover five consumer financial protection issues of growing concern to various stakeholders in LMICs: high and hidden prices, overindebtedness, post-contract exploitation, fraud, and discrimination. We highlight many tensions between how regulators and academic diagnose the extent and nature of these problems, with some growing common ground in the use of audit studies and consumer complaint data. We also find that academic evidence tends to cast doubt on the effectiveness of standard policy approaches like mandated disclosure, restricting contract terms (such as interest rate caps), and price-focused competition policy, although again we find some common ground in restricting add-on pricing practices, improving consumer redress channels, and protecting against discrimination or bias. Open questions far outnumber those answered definitively enough to provide actionable evidence for policy, and for each problem we highlight particularly important lines of inquiry regulators and researchers should pursue collaboratively going forward.

²⁰ There is a large literature on this, but starting points include Bartlett et al. (2019), Corbett-Davies and Goel (2018), and Pandey and Caliskan (2020).

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