How Costly Are Cultural Biases? Evidence from FinTech[*]

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Abstract

We study the nature and effects of cultural biases in choice under risk and uncertainty by comparing peer-to-peer loans the same individuals (*lenders*) make alone and after observing robo-advised suggestions. When unassisted, lenders are more likely to choose co-ethnic borrowers, facing 8% higher defaults and 7.3pp lower returns. Robo-advising does not affect diversification but reduces lending to high-risk co-ethnic borrowers. Lenders in locations with high inter-ethnic animus drive the results, even when borrowers reside elsewhere. Biased beliefs explain these results better than a conscious taste for discrimination: lenders barely override robo-advised matches to ethnicities they discriminated against when unassisted.

Keywords: Cultural Finance, Trust, Social Capital, Discrimination, Norms, Subjective Expectations, Robo-Advising, FinTech, Inter-ethnic Conflict, Social Conditioning, Religion, Caste.

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1 Introduction

Cultural norms, especially when based on centuries-long societal customs, can have a long-lived influence on agents' beliefs and economic decision-making (Guiso, Sapienza, and Zingales (2006), Alesina, Giuliano, and Nunn (2013), D'Acunto et al. (2019), Pursiainen (2022), Aneja and Avenancio-Leon (2023)). Norms can also produce cultural biases that make individuals' choices deviate from those of a neoclassical agent, thus lowering consumption utility (Guiso, Sapienza, and Zingales (2009)).[1] Examples of cultural biases include conscious *taste-based discrimination*, whereby discriminators take costly actions to avoid interacting with the social groups they dislike (out-group) or to increase interactions with the social groups they like (in-group, e.g., see (Becker (1957), Akerlof and Kranton (2000), Parsons et al. (2011), Hjort (2014)) and unconscious *inaccurate statistical discrimination*—ex-post systematically incorrect beliefs about the quality of counterparts based on social stereotypes (Bohren et al. (2019), Cook, Marx, and Yimfor (2022)).

Detecting and quantifying the effects of cultural biases on discriminators' economic choices is challenging because, in the absence of full information about agents' quality, belonging to a discriminated group might provide a reliable signal of quality (*statistical discrimination*) (Phelps (1972), Borjas and Goldberg (1978)), and hence discriminating might improve decision-makers' utility. Moreover, belonging to the same social group might improve principals' ability to monitor agents, which makes discrimination economically valuable to discriminators (e.g., see (Fisman et al. (2017) and Fisman et al. (2020)). Isolating and quantifying the negative effects of cultural biases on discriminators' choices, if any, requires a setting in which these channels are muted. The ideal setting also includes high-stakes economic decisions so that discriminating is costly and its costs are quantifiable.

This paper proposes a field setting to assess and quantify the nature and effects of cultural biases in decision-making under risk and uncertainty—a peer-to-peer (P2P) lending platform paired with an automated robo-advising tool. We compare decision-makers' (*lenders*) choices when unassisted and after they see automated optional robo suggestions in a context where 1) the pool of available borrowers, 2) decision makers' economic incentives, and 3) the information about borrowers decision makers observe do not change. As discussed below, the robo-advisor does not access more information than the lenders and does not use demographic information when generating proposed borrower matches.[2]

In this setting, statistical discrimination would lead to better performance than lack of discrimination, whereas performance would be worse under taste-based and inaccurate statistical discrimination. Moreover, taste-based discriminators, who were willing to take costly actions to discriminate when unassisted, should

[1] These deviations can be optimal if the agent's utility decreases when he/she makes choices that conflict with the cultural norms to which they adhere (D'Acunto (2019)). Deviations are only suboptimal if individuals would have preferred behaving like a neoclassical agent had they been aware of their cultural bias.

[2] The robo-advisor merely proposes borrowers to lenders based on borrowers' order of arrival on the P2P platform. As we show below, the proposed matches are uncorrelated with either borrowers' or lenders' demographic characteristics. This robo-advisor is not a tool that provides better information or elaborates the same information differently than lenders. The robo-advisor does not aim to maximize any objectives on behalf of lenders. Lenders are informed about what the robo-advisor does.

override robo-advice when it suggests that they lend to a borrower from a social group they dislike, given that their economic incentives with and without robo-advising are identical and overriding requires minimal effort. Inaccurate statistical discriminators, instead, who, when unassisted, discriminated unconsciously based on biased beliefs about the quality of borrowers from different social groups, would have little incentive to override robo-advised matches.

We find that when unassisted, lenders are systematically more likely to provide credit to borrowers that belong to preferred social groups conditional on borrowers' risk levels and other characteristics lenders, the platform, and the econometrician observe. This imbalance disappears after lenders observe robo-advised borrower matches, which we show are uncorrelated with borrowers' or lenders' demographics. We then find that discrimination is costly to discriminators. On average, lenders face 8% higher default rates and up to 7.3 percentage-point lower returns when choosing unassisted. Lending to low-quality borrowers belonging to favored social groups explains most of this effect.[3] Also, the vast majority of lenders do not override robo-advised suggestions, which makes it unlikely that they obtained sizable utility from discriminating consciously due to taste-based discrimination against their disfavored group or in favor of their favored group (aka, kin altruism).

The P2P platform in this paper, *Faircent*, operates in India. A set of features of this platform are important to interpret our tests and results. First, contrary to marketplace lending (Paravisini et al. (2017); Vallee and Zeng (2019); Chiu et al. (2018)), *Faircent* admits only individuals who invest their own capital. Because the capital owner and lender are the same decision maker, we can measure the direct costs of discrimination to the discriminating individual. Moreover, any potential discrimination on the platform cannot be driven by the design of principal-agent contracts between a firm/bank and its agents (employees/loan officers). For examples of how setting economic incentives in principal-agent contracts affects agents' conscious taste-based discrimination, see Hjort (2014) and Dobbie et al. (2020).

Second, the platform screens prospective borrowers based on their risk profile before borrowers are visible to lenders. If lenders were able to screen borrowers more successfully than the platform or faced lower costs of screening borrowers from favored social groups (Iyer et al. (2016); Tang (2019); Balyuk (2019)), lenders should perform weakly better when making decisions unassisted.[4]

Third, contrary to local bank branches, the platform provides barely any scope for relationship lending: 90% of the lenders disburse funds to borrowers who reside in at least 5 different Indian states, and the platform engages in screening, monitoring, and servicing after loans are issued, without any interactions between lenders and borrowers. This is why several economic channels discussed in earlier research, such as social monitoring or stigma to default on a lender belonging to the borrower's own community (e.g., see Fisman et al. (2017) and

[3]These results refer to actual choices in the field rather than hypothetical algorithmic decisions (Kleinberg et al. (2018), Tantri (2021)) and hence account for the possibility that lenders do not implement advice (Bhattacharya et al. (2012)).

[4]If lenders engaged in accurate statistical discrimination, on average, the performance of the borrowers they pick on their own should be better than that of the borrowers the tool assigns to them based on the platform's screening procedure.

Fisman et al. (2020)), are shut down in our setting.[5]

The Indian setting allows us to study two forms of cultural biases. We start with in-group vs. out-group discrimination (aka horizontal discrimination), whereby the members of two social groups favor members of their own group (in-group) and/or disfavor members of a conflicting group (out-group). In the Indian context, we study this form of discrimination between Hindus and Muslims (Brass, 2011; Tajfel et al., 1979). We then study stereotypical discrimination (aka vertical discrimination), whereby everybody discriminates against one social group—so much so that even the members of that group have excessively negative beliefs about the quality of their similar and discriminate against each other. In our setting, we argue that stereotypical discrimination arises against lower-caste borrowers (Banerjee and Munshi (2004); Banerjee and Duflo (2011)).

Studying both forms of discrimination is important because of the different policy implications. The effects of horizontal discrimination could, in principle, offset each other if the two (or multiple) conflicting social groups had similar sizes and wealth. Under vertical discrimination, instead, discriminated borrowers cannot offset the adverse effects of discrimination because nobody favors them.[6] Stereotypical discrimination based on borrowers' caste is also relevant for us in that it allows us to disentangle better the role of belonging to a social group relative to correlated confounding variables. This is because neither lenders nor our platform observes castes, and the accuracy with which certain demographic characteristics (e.g., surname, location, and occupation) predict castes varies. We can therefore compare the choices lenders make when facing two low-caste borrowers with similar demographic characteristics but whose low-caste status is easy to assess in one case and harder in the other (Bhagavatula et al. (2017)).

Figure 1 previews our baseline results in the raw data for the case of in-group vs. out-group discrimination. We compare the average share of Hindu and Muslim borrowers financed by Hindu and Muslim lenders before (black bars) and after lenders access the robo-advising tool (red bars). The data reveal three facts. First, when unassisted, both groups tend to lend more to same-religion borrowers, which is consistent with an in-group vs. out-group bias for at least one group. Second, once lenders see the borrower matches proposed by the tool, the shares of Muslim and Hindu borrowers change for both groups in opposite directions, suggesting that both religions discriminated against out-group borrowers when unassisted. Third, the shares of borrowers by religion are virtually identical across lenders after robo-advice adoption and correspond to the borrower population shares on the platform (red dashed lines). This result corroborates the notion that the robo-advising tool uses no information correlated with borrowers' religion when allocating borrowers to lenders.[7]

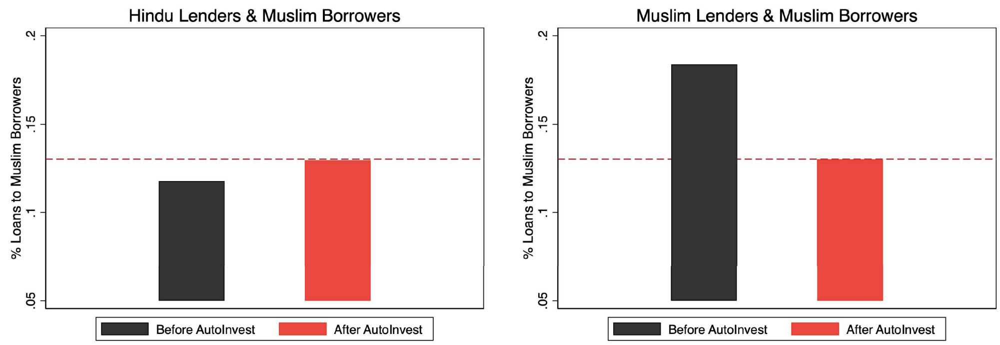
[5] As we discuss in more detail below, this is why we predict that the effects of discrimination on performance should have the opposite sign in our setting relative to Fisman et al. (2017) and Fisman et al. (2020), and indeed we do find opposite effects.

[6] Unfortunately, we cannot study choices based on borrowers' gender because the platform suggests women to indicate the identity and characteristics of a male family member on their application and we cannot know if lenders make decisions based on the characteristics of women applicants or their male family members.

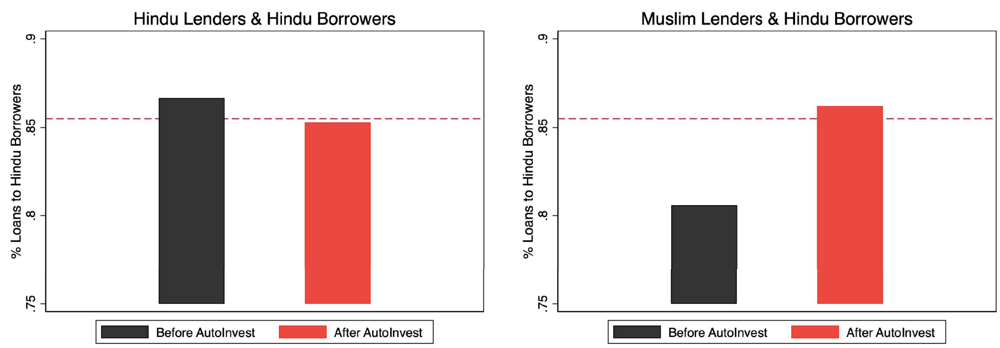
[7] As we discuss below, the robo-advising tool, as well as the selection of the borrower pool that accesses the platform, might be subject to (accurate) statistical discrimination. We are not arguing that the platform and the tool do not engage in any form of discrimination but that cultural biases do not appear to be present in the allocation choices of the tool.

Figure 1: Lending to In-Group vs. Out-Group Borrowers: Before and After Robo-Advice

Panel A. Probability of Choosing Muslim Borrowers



Panel B. Probability of Choosing Hindu Borrowers



The raw-data patterns in Figure 1 are quite robust: they do not change in multivariate analyses that control for the loan-level characteristics we observe, when we restrict the variation within lenders and hence absorb unobserved time-invariant differences across lenders such as education levels, financial literacy, and skill in screening borrowers, or when we account for different time-varying shocks before and after the robo-advising tool was available to lenders.

Note that cross-sectional or time-varying shocks that change the quality of all Hindu or all Muslim borrowers at once, such as riots against one of the two religious groups or legislation that discriminates against one of the groups relative to the others, cannot explain these results. Otherwise, both Hindu and Muslim lenders would lend relatively more to the same group of borrowers rather than adjusting their lending in opposite directions.

To corroborate our cultural-bias interpretation, we perform heterogeneity tests that capture variation in the salience of cultural stereotypes in the locations where lenders make choices, which is not where most borrowers reside. We find that biases are larger for lenders who reside in cities with a higher occurrence of Hindu-Muslim riots, in Indian states where nationalistic parties that foment inter-religious conflict obtain higher vote shares, and for lenders exposed to heightened Hindu-Muslim animus during their formative years. Recall that most loans are disbursed to out-of-state borrowers relative to lenders' location, which means that the borrowers lenders screen and choose are not exposed to the same riots as lenders and do not live in areas with the same support for nationalistic parties. Borrowers' quality and ability to repay are not determined by such sources of variation.

After detecting the presence of in-group vs. out-group discrimination, we move on to study how discrimination relates to discriminators' performance and through which channels. Under cultural biases—whether based on taste or inaccurate beliefs—when unassisted, lenders should dig deeper into the pool of in-group borrowers, who should perform worse than out-group borrowers (Agarwal et al. (2017)). The opposite should be true if lenders engage in accurate statistical discrimination.

We find that the loans lenders grant to in-group borrowers before accessing automated advice perform systematically worse than out-group borrowers' loans in terms of both default rates and returns earned. In-group-borrower loans are about 2.4 percentage points (pp) more likely to default (8% of the average default rate). High-interest-rate loans might provide high returns as long as borrowers repay something, even if they default more often. We find that this is not the case. The returns of out-group loans were higher before lenders adopted the tool, and after adoption, the returns of in-group loans increased substantially more than those of out-group loans. In back-of-the-envelope calculations, we estimate that the cost of in-group vs. out-group discrimination amounts to about 6% of the average capital lenders invested in the platform before the robo-advising tool was available.

We further document that improved in-group returns are driven mainly by a higher performance of the left tail of the distribution of in-group borrowers. Moreover, the lower delinquencies and higher returns lenders enjoy after adopting robo-advising are mostly driven by the changing risk profiles of the pools of in-group borrowers before and after lenders use the tool. Ultimately, lenders improve their performance because, when discriminating, they tend to choose high-risk in-group borrowers, which ex-post deliver lower returns on average. The tool instead barely picks any borrowers from the left tail, and when it does, it assigns such borrowers to Hindu or Muslim lenders irrespective of religion, which the tool does not observe.

In our setting, several channels that would predict a positive association between discrimination and performance are shut down by construction. For instance, homophily in monitoring borrowers and relationship lending have no scope because lenders disburse funds to borrowers all over India and do not interact with them either before or after the loans are issued (Iyer et al. (2016); Schoar (2012); Drexler and Schoar (2014); Fisman et al. (2017); Fisman et al. (2020)). For the same reasons and given that we look at uncollateralized consumer lending, the incentive effects of social collateral (Karlan, Mobius, Rosenblat, and Szeidl (2009); Diep-Nguyen and Dang (2019)), moral incentives and social image (Bursztyn et al. (2018); Bursztyn et al. (2019)), peer effects (Breza (2019)), familiarity through in-person interactions (Rao (2019)), preferences of physical appearance (Duarte, Siegel, and Young (2012); Ravina (2019)), or systematic ethnic differences in collateral value (Avenancio-León and Howard (2022); Naaraayanan (2019); Avenancio-León and Shen (2021))[8] have no scope in our setting either.

[8]Loans are not backed by collateral on the platform.

The second form of cultural bias we study is stereotypical discrimination, which, in India, arises against members of the lower Hindu caste, namely, *Shudra*.[9] This form of discrimination is especially pernicious because no decision-maker favors the discriminated group, so discriminated individuals cannot reduce the negative impact on them. We detect substantial stereotypical discrimination. Before lenders (including *Shudra* lenders) adopt the tool, *Shudra* borrowers are less likely to appear in their loan portfolios relative to their share in the borrower population. Once lenders adopt the tool, the share of *Shudra* borrowers they finance increases, and the default rates and returns of *Shudra* and other borrowers converge. Moreover, discrimination against *Shudra* borrowers is higher for lenders who reside in states where the share of crimes against lower castes is higher, which we interpret as settings in which the negative stereotypes against lower castes might be more salient to lenders at the time they make their lending decisions.

Caste-based discrimination provides an additional layer of variation for our tests due to the varying degree of recognizability of one's caste when it is not disclosed explicitly as in our platform. On Faircent, lenders can only infer borrowers' caste based on a set of individual characteristics—e.g., borrowers' surnames, locations, and occupations—and the extent to which these characteristics are a precise signal of one's caste varies. We thus design an intensive-margin test of discrimination. We build on Bhagavatula et al. (2017) and Bhagavatula et al. (2018), who design and train an algorithm that replicates an average Indian's inference problem of which caste individuals of known demographic characteristics belong. In this way, for each borrower, we obtain a continuous measure of the likelihood that lenders might recognize them as being *Shudra*. We find that discrimination against *Shudra* borrowers increases with the likelihood that the borrower is recognizable as a *Shudra*. In contrast, it virtually disappears for borrowers whose caste is difficult to assess.

The fact that *Shudra* lenders, too, discriminate against *Shudra* borrowers exclude that the results are due to kin altruism—the tendency of individuals to take costly actions to favor those who share the same demographic characteristics (e.g., see Simon (1993)). Kin altruism, like other forms of conscious taste-based discrimination, is unlikely to explain our results also because lenders do not override the tool's suggestions, which they should do if they were willingly facing the costs of helping their similar when unassisted.

In terms of performance, we detect similar facts and channels to in-group vs. out-group discrimination. First, defaults decrease, and average returns increase for discriminators after accessing the tool. Second, the improvement in returns is due mostly to the elimination of a left tail of low returns by non-Shudra borrowers.

Statistical discrimination, rather than biased discrimination, can arise in settings in which discriminated groups are a minority, and (majority) decision-makers can assess the characteristics of their similar more easily than those of minority groups (Cornell and Welch (1996)). In our setting, this explanation could be consistent with the behavior of Hindu lenders but not of Muslim lenders—who discriminate against the majority, Hindus—or Shudra lenders, for whom the favored group (non-Shudra) is the majority but does not coincide with the

[9] Unfortunately, only 0.1% of the borrowers on the platform are *Dalits* or belonging to Scheduled Castes/Schedules Tribes and hence we cannot test for discrimination against these out-caste groups.

easier-to-assess group (Shudra), against whom they discriminate.

Our paper documents a different form of discrimination relative to most earlier work on the effects of cultural biases on economic choices in the field. For instance, Hjort (2014) detects strong evidence of taste-based discrimination in a labor setting that includes no asymmetric information, in which upstream workers are aware of downstream workers' productivity. Hence, his setting has no scope for inaccurate statistical discrimination. Consistently, in Hjort (2014), economic incentives reduce biased choices when in-group agents would be affected as negatively as out-group ones. In our setting, economic incentives do not affect the extent of discrimination, and lenders stop discriminating after accessing the tool even though the economic incentives of discrimination are identical to those when choosing unassisted. We also find that the extent of lenders' in-group bias when unassisted is uncorrelated with lenders' choice of adopting the tool, which is barely consistent with taste-based discrimination.[10]

Our results also depart from forms of attention discrimination (Bartoš et al. (2016)) whereby, under asymmetric information, lenders pay more attention and effort in screening in-group borrowers rather than out-group borrowers.[11] If attention discrimination arose in our setting, the loans to in-group borrowers should have performed better than those to out-group borrowers when lenders chose unassisted, which is the opposite of what we find. Moreover, Shudra lenders should not have discriminated against Shudra borrowers but against non-Shudra borrowers who do not belong to their social group.

A nascent literature studies if and how automation affects outcome choices across various domains, including lending. Howell et al. (2023) show that lending institutions that automate the processing of loan applications were more likely to disburse Paycheck Protection Program (PPP) loans to US racial minorities during the COVID-19 crisis. In the context of consumer lending, the literature so far is inconclusive regarding the effect of automation on lending outcomes. Frame et al. (2022) find that the impact of working with a minority officer on minority application and loan outcomes is weaker at FinTech lending institutions—where several steps of the process are automated—relative to traditional lending institutions; Giacoletti et al. (2022) find that the effect of volume quotas on reducing the unexplained Black mortgage-approval gap is equally present for lenders that do or do not automate application processing, and hence automation does not help reduce biases in lending choices in their context; Bartlett et al. (2019) and Fuster et al. (2021) find that automated algorithms can in fact induce demographic-related biases in lending choices based on how algorithms are trained to learn from past data on lending outcomes.

The crucial question in this literature is whether the automated and algorithmic-based analysis of borrowers' information delivers choices that are (un)correlated with borrowers' demographic characteristics, such as their ethnicity, race, or gender. In our setting, the robo-advising tool proposes matches based on borrowers' order

[10] We thank Paola Sapienza for suggesting this argument.

[11] In Bartoš et al. (2016), the attention spent in screening in-group vs. out-group agents flips when the decision is about excluding the worst individuals from a pool. Still, this case does not arise in our setting because the platform pre-screens low-quality borrowers and lenders are aware that the worst borrowers are excluded from the pool.

of arrival on the platform that are by construction (and as we document in the data) unrelated to borrower and/or lenders' demographics. Our algorithm does not make any use of borrowers' and/or lenders' demographic information, either directly or indirectly, and for this reason our results can barely inform the issue of whether the algorithmic screening of borrowers biases choices. Rather, we use our setting to assess the nature and effects of human decision-makers' biases exploiting robo-advised matches as benchmarks. In particular, our setting is uniquely designed to assess whether (human) cultural biases are based on conscious taste-based discrimination or unconscious inaccurate statistical discrimination, because the two forms of discrimination have different predictions about whether decision-makers should follow robo-advised suggestions or override them. If we were simulating hypothetical unbiased choices to compare with actual choices (e.g., see Tantri (2021)), we would not be able to perform our assessment of cultural biases because we would never know if a decision-maker that faced the hypothetical option would take it or override it.

Overall, our findings emphasize an unintended role of robo-advice (D'Acunto and Rossi (2020)), which is diffusing around the world to facilitate consumers' spending (D'Acunto et al. (2019)), saving (Gargano and Rossi (2023)), borrowing (Agarwal et al. (2019); Chak et al. (2022)), and lending decisions. We show that robo tools can help discriminating agents avoid the financial losses they face when making culturally biased choices, of which they might be unaware. Robo-advising might be a viable substitute for supply-side interventions (e.g., disclosure requirements) and demand-side interventions (e.g., provision of financial literacy) because, contrary to such interventions, it does not require that agents understand fully the problems they face (Adams et al. (2019); D'Acunto et al. (2022); D'Acunto et al. (2019)).

2 Institutional Setting

The setting for our analysis is Faircent, a large FinTech platform that specializes in P2P lending in India.[12]

2.1. Borrowers' Screening by the Platform

Several platform features are important for the design and interpretation of our tests. First and foremost, the platform screens applicant borrowers before admitting them to the pool, and lenders observe the results of this screening. Even though the platform does not use racial variables, its automated screening procedure might incorporate statistical discrimination (e.g., see Bartlett et al. (2019); Bhutta et al. (2021); Cowgill and Tucker (2020); Fuster et al. (2021); Rambachan et al. (2020)).

Once a prospective borrower signs up, he/she submits a loan application that includes the proposed amount and motivation of the loan, the borrower's credit score, occupation, geographic location, and whether the borrower has dependents. Borrowers need to provide evidence of a financial account, which excludes unbanked

[12]This setting is reminiscent of the recent literature on FinTech adoption in developing countries (e.g., see Agarwal et al. (2019); Crouzet, Gupta, and Mezzanotti (2023); Higgins (2022); D'Andrea and Limodio (2019)).

applicants. The first screening step is an automated algorithmic-based assessment of the borrower's credit viability, which is largely based on the borrower's credit score, proposed loan amount and maturity, and occupation.[13] Applicants whose credit viability falls below a fixed threshold are dismissed.

The Online Appendix reports raw-data evidence on the outcomes of the automated screening procedure (see Figure A.1). Credit scores are monotonically related to loans' annual interest rate, maturity, and loan amounts, all of which are assigned by the platform (see Figure A.2). It is important to stress that the interest rates the platform assigns to borrowers cannot be modified. Lenders' choices are therefore limited to quantities (whether to finance a loan and, if so, by how much) and not to loan prices.

The prospective borrowers who are approved and accept the parameters proceed to the verification step, whereby Faircent's employees verify borrowers' self-provided information and documents: borrowers' identity (identity cards and a personal picture), two income paystubs, or incoming transactions in a bank account under the borrower's name, utility payments, the picture of the borrower's housing and working locations. Borrowers who pass this verification step are admitted to the pool that lenders can browse.

Lenders observe borrowers' demographic characteristics and the qualitative and quantitative risk assessment from the screening process (we discuss the information lenders see in section *2.2.* below).

In this setting, lenders make choices *after* a substantive risk assessment of borrowers, whose outcomes lenders observe. Decoupling borrowers' risk screening from lending decisions, which departs from earlier research that studied the choices of loan officers, reduces the scope for accurate statistical discrimination on the part of lenders. Lenders might believe that they can screen borrowers better than the platform (Balyuk and Davydenko (2019); Vallee and Zeng (2019)). If true, lenders' unassisted choices should perform better than robo-advised choices, which is the opposite of what we find.

To avoid money laundering concerns, the platform imposes that each loan is financed by at least 5 lenders, and each lender can finance up to 20% of any loans. Lenders, who reside all over India, cannot communicate within the platform or observe each others' identity, which virtually excludes the possibility of forming coalitions or coordinating loan financing across lenders.

Our setting is quite different from local-branch relationship lending by professional loan officers to local borrowers, in which soft information about borrowers and social norms/social pressure might influence borrowers' repayment behavior: as Figure A.5 of the Online Appendix shows, more than 90% of lenders choose borrowers who live across at least 5 different Indian states. The median lender disburses funds to borrowers who reside in 13 different Indian states. Importantly, this geographic dispersion of lender-borrower matches is similar before and after the introduction of the robo-advising tool. This feature allows us to exploit variation in inter-ethnic animus across the locations in which lenders make choices about borrowers that do not reside in the same locations, so that local inter-ethnic animus cannot proxy for unobserved characteristics of the borrower population.

[13]Due to confidentiality reasons, we cannot describe how this information is combined in the automated screening.

Execution, servicing, and monitoring after loan approval are also managed by the platform. The loan agreement is a private contract between the borrower and each lender, but the platform produces the electronic forms that lenders and borrowers have to sign. No lenders enjoy any form of seniority. Upon execution, Faircent tells borrowers their equated monthly installment (EMI)—the monthly payment—and services the loan. Faircent monitors the status of loans each month. Loans' status is declared "closed" after full repayment or after repeated delinquency. Borrowers whose loans are closed while delinquent are dismissed from the platform. Faircent's loans are subject to the same regulatory policies and oversight as the loans issued by traditional financial institutions in India.

2.2. Robo-Advising Tool (Auto Invest)

The second important feature of the platform is that lenders can make their choices unassisted or under the assistance of a robo-advising tool called Auto Invest.

For unassisted choices, lenders can browse the borrower pool at any point in time. Lenders observe the coarse risk category assigned by the platform (low, medium, or high risk), the detailed risk assessment of the loan (interest rate, maturity, overall loan amount), and a set of borrowers' demographic characteristics that include names and surnames, location of residence, occupation, education levels, and the number of dependents. Upon clicking on the borrowers' profile, lenders can see the verification report about their identity. Lenders can decide who, if anybody, they want to fund and by how much, subject to the 20% limit discussed above. Lenders need to have the funds they want to commit deposited on the platform before they can finance a loan.

The second mode of investment is with the assistance of an automated robo-advising tool, Auto Invest, which was introduced on the platform in the second half of 2018. After its release, lenders can adopt Auto Invest at any time. We report the screenshot of Auto Invest in Figure A.3 of the Online Appendix. Upon adoption, lenders decide the share of their overall funds deposited on the platform for which they want to use Auto Invest and the share of funds they want to keep investing unassisted. Lenders are then asked to allocate the funds for which they seek assistance by Auto Invest across the same three borrower-risk categories they see when making unassisted decisions—low, medium, and high risk.

For each risk category, the tool matches lenders to borrowers based on borrowers' order of admission to the pool. Auto Invest does not choose borrowers explicitly based on their interest rate, level of risk within each risk category, or demographic characteristics. Below, we show that the proposed matches by Auto Invest are uncorrelated with lenders' or borrowers' demographic characteristics, and hence verify that Auto Invest does not target borrower characteristics that happen to be correlated with religion or ethnicity. Ultimately, Auto Invest matches borrowers to lenders almost at random conditional on borrowers' risk categories. In contrast with how the platform advertises the tool, Auto Invest does not select the best-performing borrowers on the platform faster than would be possible with manual choices—we detect no economically or statistically different performance for borrowers whose loans are funded faster or slower either by Auto Invest or through manual choices.

Auto Invest might affect lenders' loan portfolios in many ways unrelated to cultural biases, such as diversification. While in Section *6.2.1* we show that lenders' portfolio diversification does not change after adopting Auto Invest, one could be concerned that Auto Invest may affect lenders' portfolios in other ways that relate to performance. For this reason, when assessing the cost of cultural biases, we will compare lender-level returns on favored-group borrowers to the same lenders' returns on discriminated borrowers, whose improvement might capture potential positive effects of Auto Invest unrelated to demographics and hence that are distributed across all groups.

Once Auto Invest proposes a set of borrowers, the lender decides if she wants to proceed with the suggested allocation or change it in part or in full. To make changes, she browses the pool of borrowers and finds replacements. This step is crucial for interpreting our results because, under conscious taste-based discrimination, we would expect lenders to override robo-advised suggestions when matched with demographic groups they dislike, given that the cost of doing so is minimal.

2.2.1 **Adoption of Auto Invest.** A lender's choice to adopt Auto Invest is not exogenous. Unfortunately, the company did not engage in randomized testing or advertising of the tool, which would allow us to exploit a quasi-exogenous source of variation in adoption across lenders. For this reason, most of our empirical analyses will only consider lenders who adopted the tool at some point and exploit the differential timing of adoption across lenders. Still, understanding the patterns of adoption of Auto Invest is important to assess whether the population adopting lenders is selected in ways that would affect the interpretation of our results.

We first consider the timing of adoption. Figure A.4 in the Online Appendix plots the number of lenders who adopt Auto Invest each month-year our data cover and the number of new lender signups to the platform over the same period. Overall, we detect a pattern of increasing adoption over time, which roughly follows the pattern of new lender signups to the platform. Although the two patterns are not always parallel, we do not detect any obvious clusters of Auto Invest adoption in specific month-years, which suggests that the choice of adopting Auto Invest is unlikely to be driven by economy-wide shocks lenders face.

We then assess if any of the limited lender-level characteristics we observe correlate with the decision to adopt the tool. Beyond demographics, an important dimension we can measure is the extent of lenders' bias against out-group borrowers before Auto Invest is introduced, which we can compare across adopters and non-adopters to understand if the adoption decision relates to the extent to which lenders discriminate against out-group borrowers. We define this "pre-adoption bias" as the difference between the share of out-group borrowers on the platform and the share of out-group borrowers in each lender's loan portfolio before adopting Auto Invest (and throughout the sample period for non-adopters).

Table A.1 in the Online Appendix shows that neither the extent to which lenders are biased against out-group borrowers nor lenders' religion predicts a higher or lower likelihood of adoption.[14] Moreover, none of the two religious groups is more sensitive to their in-group bias in terms of adopting Auto Invest. These non-results are unchanged if we only compare lenders who reside in the same Indian state and lenders who were born in the same year (lenders' cohorts). Location and cohorts are the only demographic characteristics of lenders the platform collects and verifies because the platform does not need to assess lenders' riskiness.

Overall, we detect no obvious patterns of adoption over time across lenders of different demographic groups. In particular, the extent of bias against the opposite religious group before adoption does not predict lenders' decision to adopt Auto Invest.

We conclude this section by discussing two points about how the endogeneity of the choice to adopt Auto Invest relates to the interpretation of our results. First, we note that virtually any potential unobserved dimension that affects the viability of each group of borrowers over time, and especially before and after the introduction of Auto Invest, would predict that all lenders change their lending behavior in a similar way for the same group of borrowers. For instance, if time-varying economic shocks made one borrower group relatively more or less creditworthy than the others, all lenders should react in the same direction in terms of lending to such borrower group rather than reacting in opposite ways based on lenders' own demographics, which is instead the prediction of a model of demographic-based discrimination. Even on the lenders' side, potential unobserved time-varying shocks that affect all lenders or lenders of a specific religion or caste would lead lenders to modify their lending behavior similarly across borrowers of different ethnic groups unless lending choices faced demographic-based discrimination.

Second, note that our paper does not aim to isolate and estimate the causal effect of adopting this specific form of robo-advising on lenders' choices and performance. This question would have limited scope given that its answer would be specific to the design of Auto Invest on the Faircent platform and would not generalize to other forms of robo-advice. Rather, we aim to verify in the data that the choices lenders make after accessing this tool are uncorrelated with their own or borrowers' religion or caste and thus compare the performance of lenders' choices when demographic dimensions correlate with such choices and when they do not to measure the extent of cultural biases in decision making and how cultural biases relate to performance. We could not tackle these questions by simply comparing actual choices with simulated choices uncorrelated with borrowers' demographics: we need to observe lenders' actual choices when both correlated and uncorrelated with borrowers' demographics to assess if biased choices are consistent with a conscious form of taste-based discrimination or an unconscious form of inaccurate statistical discrimination.

[14] Although our full sample includes 2,818 unique lenders, for this analysis, we can only include lenders for whom the religion and caste of all borrowers in their portfolios can be retraced, which is 1,567 unique lenders.

3 Data and Summary Statistics

To perform our analyses, we use seven data sets, each covering a different feature of the lending process at Faircent. Our data span the period between January 1, 2018, and March 30, 2020, although, given the sizeable monthly growth of the platform, 60% of the loans in our sample were issued in 2019 and another 19% in the first three months of 2020. Variation in the timing of loan issuance is, therefore, limited. We limit the sample to the end of March 2020 to avoid covering the COVID-19 pandemic period.

Two data sets—the *Lenders' characteristics data set* and the *Borrowers' characteristics data set*—include cross-sectional data with one observation per individual. Each lender and borrower is assigned a unique identifier, which allows us to link lenders' and borrowers' characteristics across data sets. For each lender, we observe name and surname, the state of residence,[15] and the date of birth. On top of these characteristics, the borrowers' sample also includes the borrower's residence type (whether owned or rented), number of dependents, employment type (whether self-employed or not), and credit score.

Faircent does not collect borrowers' religion or caste, but lenders can infer these dimensions based on borrowers' demographic information they observe. We will exploit the extent to which lenders can easily infer castes based on borrowers' characteristics in our analysis. We, as econometricians, infer these variables using the *Marriage registry data set* (see Bhagavatula et al. (2017)), which includes demographic information about religion and caste elicited at marriage for a random sample of 2,481,158 Indians. It includes names and surnames, date of marriage, state of birth, city of residence, height (in centimeters), religion, and caste.

Assigning religions is quite straightforward, implying that lenders should also find it easy to infer borrowers' religion because religion barely varies across individuals with the same surname. Eighty-nine percent of the unique pairs of surnames and dates of birth in the registry (everybody who was born the same day and shares the surname) belong to same-religion individuals. When we only consider Hindus and Muslims, 96% of the surname-date-of-birth pairs are matched uniquely to one of the two religions. For these reasons, we assign religions to lenders and borrowers based on surname and date of birth.

The assignment of castes is less straightforward. The caste information in the registry is dispersed and includes about 540 narrowly-defined partitions, which often merely correspond to the individual's surname. To make our analysis of castes meaningful, we need a reliable way to assess which of the four main castes borrowers and lenders belong to.[16] None of the combinations of characteristics we observe in both the Faircent data and the registry—names, surnames, and dates of birth—restrict the set of castes enough to proceed in the same way as with religions. To infer borrower and lenders' varnas, we thus rely on research in computer science (Bhagavatula et al. (2017), and Bhagavatula et al. (2018)) use the procedure we discuss in Section *5.1.*.

[15] For a subset of lenders, we also observe the city of residence, but whereas choosing the state from a pre-determined list is compulsory, writing down the city in a blank box is not required.

[16] We provide a primer on Indian castes and how they relate to our analysis below.

Fourth, we use the *Lender-Borrower Mapping*—a cross-sectional data set at the level of lender-borrower-loan triads. This information is critical to merge individual characteristics to borrowers and lenders who match through a loan that is in part funded by the lender. The data are also critical to merge loan characteristics and performance information to each unique lender-borrower-loan triad.

Loan-level information is in cross-sectional and panel formats. The *Loan characteristics data set* is a cross-sectional data set at the loan level that reports the borrower identity, the total amount lent, interest rate, maturity, and proposed monthly payment. We also observe the loan's status as of March 31, 2020 (active or closed) and whether the last payment happened within the 31 days before closure (i.e., whether the loan was in good standing). The *Loan performance data set* is an unbalanced panel at the loan-month level. Unfortunately, we were able to obtain the monthly payments (including zero for missed payments) only for a random subset of the loans in our main sample.

Finally, the *Auto Invest data set* is a cross-sectional data set at the lender level that reports whether lenders have ever activated the robo-advising tool and, if yes, the activation date and the share of the funds for which the lender wanted matching proposals from the tool.

3.1. Sample Selection

To study in-group vs. out-group discrimination, we create a sample at the borrower-lender-loan level that includes all the Hindu and Muslim borrowers and lenders in the data for whom we observe no missing information on loan characteristics, the usage of Auto Invest, or loan performance.

A common concern when studying the adoption of a new technology is that unobserved characteristics and shocks might, at the same time, cause lenders' adoption as well as a behavioral change after adoption so that this change cannot be attributed causally to using the new technology. Note that, as we discussed in Section *2.2.*, we find that none of the observable lenders' characteristics predict adoption in our setting, including the extent of lenders' bias against out-group borrowers before adoption.

Moreover, in our setting, this endogeneity concern is less compelling than in others because we test whether different lenders change their behavior in opposite ways after adopting the robo-advising tool. For instance, if Hindu and Muslim borrowers faced different economic shocks over time and for this reason they became more or less viable borrowers, all lenders should react in the same direction in terms of choosing more or less of one of the two categories of borrowers, because borrower-level shocks affect the borrowers available in the pool in the same way, irrespective of whether they end up being chosen by a Hindu or a Muslim lender.

Similarly, lender-level shocks might affect the extent to which lenders engage with the platform or the amount of money they invest on the platform over time, but once these dimensions are fixed at the lender level, they should not impact the allocation of funds across borrowers of different religions. One possibility is that the lenders who think they have been performing worse on the platform are more likely to adopt the Auto Invest earlier, but then at a minimum, these lenders were not realizing that cultural biases were the source of their low performance or otherwise they would have changed their allocation across religions and castes before the introduction of Auto Invest. Even in this case, comparing the allocation of unassisted choices with the allocation of choices implemented through Auto Invest would allow us to measure lender-level cultural biases.

Nonetheless, to reduce concerns about the endogenous adoption of Auto Invest, we follow the literature[17] and only consider lenders who activated Auto Invest at some point between its introduction in 2018 and the end of the sample period (March 31, 2020). In this way, we do not compare the choices of lenders who did not adopt Auto Invest with those of lenders who adopted it at some point in time. At each point in time, the sample includes lenders who have already activated the tool and others who have not yet activated it.

A second way in which we reduce concerns about the endogeneity of adoption is by exploiting the intensive margin of the use of Auto Invest: we compare lenders who allocate a higher or lower share of their funds to the robo-advising tool, and hence lenders that might have activated the tool at the same point in time but who make a higher or lower fraction of choices unassisted.

The second working sample allows us to study the effects of stereotypical biases. We first select the sample using the same steps discussed above. Then, we further restrict the sample to only include Hindus because other religious groups do not partake in the caste hierarchy. Moreover, we only include borrowers for whom we can retrace caste information in the form of one of the four varnas based on the marriage registry data.

3.2. Summary Statistics

The first working sample includes 113,283 unique lender-borrower-loan triads, which are based on 2,818 unique lenders. Panel A of Table 1 reports summary statistics. Borrowers' religion, consistent with the split between Hindu and Muslim individuals in the general Indian population, is tilted toward Hindus—13% of the borrowers are Muslim. The religious imbalance is starker on the lenders' side—99% of lenders are Hindu. Despite the small share of Muslim lenders (1% of the sample), the number of observations is large enough to allow a statistically meaningful analysis of Muslim lenders' choices. A potential reason for this imbalance is that the precepts of *Sharia* condemn the earning of financial interest on loans and might discourage Muslims from signing up as lenders.[18]

About 45% of the loans were issued when lenders had activated the robo-advising tool. The share of funds allocated to the tool is about 60% on average, but substantial cross-sectional variation exists across lenders. As far as loan characteristics are concerned, the average maturity (tenure) is 22 months, and the median maturity is 24 months. The average loan amount is slightly above 130,000 rupees, which corresponds to about $1,770,[19]

[17]For instance, see D'Acunto et al. (2019), D'Acunto et al. (2020), and Gargano and Rossi (2021), among others.

[18]For this reason, the Muslim lenders who sign up might not be less attached to the religious precepts of Islam. Our tests are based on Muslim ethnic and social identity, to which Muslims should relate whatever their religiosity.

[19]This conversion does not adjust for varying the monthly exchange rate between the US dollar and Indian rupee.

with a large standard deviation. The average annual interest rate is 24%—similar to the yearly APRs for credit cards in the US over the same period.[20]

The summary statistics so far refer to all unique lender-borrower-loan triads—the level of observation in most of our analyses. Considering the lender level, we find that the share of Muslims on the platform is stable throughout the sample period. We cannot reject the null that the shares are equal before and after Auto Invest is available. Also, recall from section *2.2.* that lenders' religion does not correlate with the decision to adopt robo-advice, that is, Hindu and Muslim lenders adopt at similar rates. Lenders' engagement with the platform does not vary, either: the average lender issues 40 loan offers to any borrowers both before and after accessing Auto Invest (p-value of t-test for equality=0.683). In terms of loan characteristics, when computed at the lender level, the average interest rate of loans issued before using Auto invest is higher than that of loans issued after using Auto Invest (25% vs. 23%, p-value of t-test for equality=0.003), and the default rates lenders face in the raw data are substantially higher before they use Auto Invest (33% vs. 16%, p-value of t-test for equality$<$ 0.1%). Also, lenders offer lower average amounts per loan after accessing Auto Invest (₹2,980 vs. ₹2,123, p-value of t-test for equality$<$ 0.1%). As we will see in the last part of the paper, the fact that the loans issued through Auto Invest are less risky, on average, than those issued through unassisted choices is important to explain why culturally-biased choices deliver lower returns to lenders.

Our second working sample, which only includes Hindus whose caste we can infer, has 62,831 unique lender-borrower-loan triads. Panel B of Table 1 reports the summary statistics for this sample, in which 39% of borrowers belong to the discriminated *Shudra* varna. Despite the smaller size, the summary statistics for the main variables of interest in this sample are similar to those described in Panel A. The patterns for the statistics at the lender level, including the lack of changes in the composition of borrower pools before and after Auto Invest is available, as well as the lower risk of loan offers lenders make when assisted by Auto Invest, are also the same as for the Hindu-Muslim sample.

4 In-group vs. Out-group Discrimination: Hindu vs. Muslims

We first analyze in-group vs. out-group discrimination: agents tend to favor members of their social group (in-group) over members of conflicting social groups (out-group), where social groups' boundaries are defined based on cultural cleavages (Tajfel et al. (1979); Hewstone et al. (2002); Jenkins (2014)).[21]

The cultural-bias hypothesis predicts not only that lenders should be more likely to pick in-group borrowers but also that the in-group borrowers they choose should perform *worse* than the out-group borrowers they choose. These predictions are in contrast to what we should find if lenders' favoritism toward in-group borrowers were

[20] Over our sample period, the nominal interest rate of reference set by the Reserve Bank of India ranged between 4.5% and 6%.

[21] Unfortunately, we cannot provide a comprehensive description of all the facets and decades-long academic debate about this family of theories in this paper, but due to space constraints, we need to focus on the most relevant implications in terms of what we can test directly in our setting. For more comprehensive reviews, see, for example, Hewstone et al. (2002) and Jenkins (2014).

due to lenders' ability to screen and monitor in-group members better than out-group members.

We consider the religious conflict between Hindus—Indian religious majority—and Muslims, one of the religious minorities in post-independence India. Acts of in-group vs. out-group discrimination between these two religious groups are deeply rooted in history and pre-date the independence of modern India in 1947 as well as the British rule on the Indian subcontinent (Lorenzen (1999)). Not only have Hindu and Muslim identities developed in contrast over time, but the identity clash has also manifested in acts of conflict, including violent conflict and riots, for decades (e.g., see Engineer (1997)).

The Hindu-Muslim conflict has been exacerbated over the last two decades (Graff et al. (2012)) and erupted in violent riots, such as the anti-Muslim pogrom in the state of Gujarat in 2002 (Ghassem-Fachandi (2012)). Several political scientists and sociologists argue this conflict was exacerbated because right-wing political parties, such as the BJP, support Hindu nationalism (see Kaul (2017) among others). For instance, the approval of the *Citizenship Amendment Act* by the BJP in 2019 has produced a widely covered wave of riots and violence between Hindus and Muslims (Bhat (2020); (Mitra and Ray, 2014)).

We first assess the extent to which Hindu lenders might have been more inclined to finance Hindu borrowers and Muslim lenders to finance Muslim borrowers when making their decisions autonomously. Then, we compute the change in the propensities to lend to Hindus and Muslims after robo-advising adoption. Third, we assess whether the extent of lenders' in-group vs. out-group bias was stronger for lenders who resided in areas with higher Hindu-Muslim animus, for whom the Hindu-Muslim conflict was arguably more salient when making lending decisions.

4.1. In-group vs. Out-group Lending Before and After Robo-advising

In the introduction, figure 1 plots the average share of Hindu and Muslim borrowers in Hindu and Muslim lenders' portfolios when lenders made all loan decisions unassisted and after robo-advising adoption. The top graphs consider the choice to lend to Muslim borrowers before (black bar) and after (red bar) using Auto Invest. The bottom graphs repeat the analysis for Hindu borrowers.

Three broad patterns are worth noticing. First, consistent with the presence of an in-group vs. out-group bias for at least one of the two groups, both groups choose a higher share of borrowers of their own religion when unassisted: 86% of Hindu lenders' borrowers are Hindu, whereas only 80% of Muslim lenders' borrowers are Hindu. Conversely, the share of Muslim borrowers is 12% for Hindu lenders and 18% for Muslim lenders.

Second, after using the robo-advising tool, the shares of Muslim and Hindu borrowers change for all lenders in opposite directions: Hindu borrowers decrease from 86% to 84% of Hindu lenders' choices, whereas the share of Muslim borrowers increases from 12% to 13%. At the same time, Hindu borrowers move from 80% to 86% of Muslim lenders' choices, whereas the share of Muslim borrowers drops from 18% to 13%.

Third, Figure 1 shows that the share of borrowers of different religions is equalized for Hindu and Muslim lenders after using Auto Invest (red dashed horizontal lines). Unobserved channels that might make in-group borrowers more profitable to lenders than out-group borrowers are thus unlikely to exist in our setting. Moreover, this result corroborates that the tool does not use information about religion (which it does not have) to allocate borrowers across Hindu and Muslim lenders. Ultimately, the tool equalizes the share of borrowers of each religion in each lender's portfolio to the share of borrowers of each religion in the broader population of borrowers.

The averages in Figure 1 compare lending behavior before and after lenders adopt the robo-advising tool. Still, Auto Invest allows lenders to choose the share of their funds on the platform they want to allocate through Auto Invest's suggestions and the share they want to allocate unassisted. We can thus compare lenders who adopt the tool at the same time but make a different share of their choices based on the tool's suggestions.

Figure 2 reports the results of this intensive-margin analysis in the raw data.[22] We sort lenders based on the share of funds they allocate to Auto Invest, which is strictly larger than zero and lower than or equal to 1. The solid blue line reports smoothed nonparametric estimates of the relationship between the share of Hindu borrowers in lenders' borrower portfolios (measured on the right y-axis), and the percentage of funds lenders allocate to Auto Invest. Grey bandwidths refer to 95% confidence intervals around the point estimates of the slope of the curve for each percentage of fund allocation.

We find that a more intensive use of Auto Invest is associated with a lower in-group vs. out-group bias in lending. The bias drops substantially for lenders who assign at least half of their available resources on the platform to be allocated through Auto Invest.[23]

The univariate results discussed thus far suggest that automating lending choices reduces lenders' favoritism toward choosing in-group borrowers over out-group borrowers. One might worry that systematically different time-varying shocks Hindu and Muslim borrowers face might explain the differential shares of lending to borrowers of different religions, but this explanation requires that economic shocks make a group of borrowers, say Muslims, a worse match for Hindus but a better match for Muslims, which seems quite implausible.

To assuage the relevance of this concern, in Table 2, we report the results for estimating variations of the following multivariate specification:

$$\begin{aligned} Muslim\ Borrower_{i,j,t} =& \alpha + \beta\ Auto\ Invest_{j,t} + \gamma\ Hindu\ Lender_j + \\ & \delta\ Hindu\ Lender_j \times Auto\ Invest_{j,t} + \zeta\ \boldsymbol{x}_{i,t} + \eta_j + \eta_t + \epsilon_{i,j,t}, \end{aligned} \tag{1}$$

where $Muslim\ Borrower_{i,j,t}$ is equal to 1 if borrower i receives funding from lender j in year t is Muslim, and 0 otherwise; $Auto\ Invest_{j,t}$ is equal to 1 if the lender made the loan after activating Auto Invest, and 0 otherwise; $Hindu\ Lender_j$ is equal to 1 if lender j is Hindu; $\boldsymbol{x}_{i,t}$ is a vector of loan-level characteristics that are direct

[22]We only study the intensive margin for Hindu lenders because we do not have enough Muslim lenders in the sample to obtain a meaningful mass of them at each value of the percentage of funds allocated to Auto Invest.

[23]We detect a mirroring pattern for Muslim borrowers (green dashed line), which is mechanical given that the sample includes only Hindu and Muslim borrowers.

proxies for the risk profiles of the loans that lenders extend to borrowers—loan maturity (measured in months), loan amount, and the interest rate associated with the loan. Importantly, these characteristics are *not* chosen by the lender; instead, the company's algorithm assigns them to borrowers when the loan requests are vetted, borrowers' risk profile is estimated, and requests are approved. Finally, η_j is a full set of lender fixed effects and η_t is a full set of year fixed effects, which we use in our most restrictive specifications to only exploit variation within lenders and/or within years.

Unfortunately, we do not observe whether each individual loan contribution is made by the lender unassisted or through Auto Invest. For this reason, we cannot compare the choices the same lender makes manually with those based on Auto Invest's suggestions at the same point in time by absorbing lender-by-time variation in our multivariate analyses. Moreover, by construction, we cannot absorb borrower fixed effects because the outcome variable does not vary within borrowers.

In terms of statistical inference, in Table 2 we cluster standard errors at the lender level to allow for correlation across the lender-borrower matches that include the same lender. In Table A.2 of the Online Appendix, we show that the results are quite similar if we make different assumptions, such as allowing for double clustering by both lender and borrower, i.e., allowing for correlation also across the residuals of matches within the same loans, allowing for triple clustering at the lender, borrower, and month-of-issuance level, as well as for triple clustering at the level of lender family communities (captured by sharing the same surnames), borrower family communities, and month of issuance.

The coefficient estimates of interest in Table 2 are $\hat{\gamma}$—the likelihood that Hindu lenders had Muslim borrowers in their portfolios relative to Muslim lenders—and $\hat{\delta}$—the change in the probability that Hindu lenders lend to Muslim borrowers after activating the robo-advising tool.

Column (1) of Table 2 reports the baseline estimates without adding any control variables. This specification assesses the statistical significance of the univariate results. The coefficient estimate $\hat{\gamma}$ is negative and statistically as well as economically significant, indicating Hindu lenders were about 5.8 pp less likely to lend to Muslim borrowers than Muslim lenders before using the robo-advising tool. The constant term—18%—captures the share of Muslim borrowers in Muslim lenders' portfolios. The insignificant estimate of β shows that the share of Muslim borrowers on the platform is not systematically different before and after lenders access the tool. Moreover, $\hat{\delta}$ is positive and significant, indicating Auto Invest increases the likelihood that Hindu lenders lend to Muslim borrowers by about 4.5 pp. The lack of full debiasing is consistent with the fact that adopting lenders might keep making some loan choices unassisted based on the amount of resources for which they ask for robo-advised matches (see Figure 2).

In the second column of Table 2, we find the coefficient on the interaction between $Auto\ Invest_j$ and $Hindu\ Lender_j$ has the same statistical significance and point estimate as the baseline result, which excludes that the bias before the use of robo-advising was driven by heterogeneity in objective proxies for the riskiness of borrowers, such as loans' interest rates and maturity.

We then move on to restricting the variation within lenders by adding a lender fixed effect (column (3)). In this way, we assess the scope for discrimination after absorbing systematic time-invariant differences across lenders, such as financial literacy, cognitive skills, education levels, and skill in screening borrowers. Our estimates stay almost identical, which suggests that they are driven mostly by within-lender variation over time before and after robo-adoption rather than by variation across lenders. Absorbing time-varying shocks in column (4) also fails to influence the estimated coefficient $\hat{\delta}$ which, as discussed in section *2.2.*, is not surprising given that time-varying economic shocks would predict changes in lending in the same direction for all lenders rather than changes in the composition of borrowers' characteristics.

The last two columns of Table 2 assess the intensive margin by comparing lenders who allocate less than 40% of their funds to Auto Invest (column (5)) and others (column (6)).[24] $\hat{\delta}$ is small and insignificant for lenders who do not use Auto Invest intensively, while positive and significant for others.

4.2. Heterogeneous Inter-Religious Animus Where Lenders Reside

To further assess the role of cultural biases in our results, based on earlier research in psychology and sociology, we isolate sources of heterogeneity in the extent to which the Hindu-Muslim conflict should be salient to lenders while they make their lending decisions. Beyond providing an additional test for the effects of cultural biases, these heterogeneity tests further reduce concerns about the endogeneity of adoption: unobserved drivers of both adoption and choices are unlikely to vary systematically with variation in the salience of Hindu-Muslim animus in the lenders' location because the vast majority of borrowers lenders vet and choose do not reside in the same locations.

***4.2.1* Hindu-Muslim Riots Across Cities.** We first consider spatial heterogeneity in Hindu-Muslim riots (Oza (2007); Ticku (2015)). Proximity to the violent riots and local media coverage are likely to make Hindu-Muslim animus more salient to exposed lenders (D'Acunto et al. (2019)). We thus test if lenders residing in states that have faced more Hindu-Muslim riots display a stronger in-group bias when making all choices unassisted.[25] Panel A of Figure 3 depicts the cross-state variation we employ based on the incidence of riots (Ticku (2015)): dark-green states (Gujarat, Marahashtra, Karnataka, and Uttar Pradesh) are those in which Hindu-Muslim riots have been most prevalent. To make the (many) coefficients of interest across subsamples easier to compare, we report the results of our multivariate heterogeneity tests in graphical form.

Figure 4 plots the relevant coefficients when estimating equation (1) separately for lenders in states with

[24] We chose this threshold based on the shape of the relationship we described in Figure 2, which varies systematically above 40%, but the results are similar irrespective of the choice of threshold.

[25] As discussed above, all lenders are mapped to an Indian state but not necessarily to a city. Moreover, regulations and policies are often implemented at the state level; for example, see the case of "anti-conversion laws" (Jenkins (2008); Dhattiwala and Biggs (2012)). Also, deep-rooted local cultural norms persistently relate to present-day interreligious violence in India (Jha (2014)).

a low incidence of Hindu-Muslim riots during 1980-2000 and other lenders. The bias is about twice as large for Hindu lenders in states with a higher incidence of Hindu-Muslim riots (6.4 pp) and statistically different from zero, but small and insignificant for other lenders. The left plot of Panel B shows $\hat{\delta}$, which captures the drop in bias after adopting Auto Invest for the same sets of lenders. Lenders in states with a high incidence of Hindu-Muslim riots debias by more (4.8 pp, p<0.01). Statistically, though, we cannot reject the null that the estimated coefficients are equal across subsamples: the χ^2 statistic for a Wald-test of equality of the coefficients across the two subsamples is 0.18 in this first split.

4.2.2 Electoral Support for the BJP Across States.

We move on to consider a second dimension that earlier research has associated with the salience of the Hindu-Muslim conflict: the local vote share for the *Bharatiya Janata Party* (BJP). The BJP's ideological roots have always been based on the notion of *hindutva*, which implies a coincidence between the spheres of Indian culture and traditional Hindu values (e.g., see Berglund (2004); Chhibber and Verma (2018); Prakash (2007); and Chidambaram et al. (2020)). The BJP is the result of mergers of post-independence parties and has shared a leading role with the *Indian National Congress* since independence (e.g., see Ziegfeld (2020)).[26] We exploit state-level variation in BJP vote shares to capture variation in Hindu lenders' exposure to an ideological bias against Muslims.[27] To this aim, we compute the BJP candidates' vote share in each election cycle from 1977 to 2015 and state and then the average BJP vote shares within states.[28] Panel B of Figure 3 reports the average BJP vote share distribution.

In the middle plots of Figure 4, the extent of bias against Muslim borrowers by Hindu lenders—Panel A—was 9.4 pp before robo-advising in high-BJP-vote share states, but only 3.5 pp for lenders in other states. After robo-advising, Hindu lenders in high-BJP-vote-share states do not appear biased again Muslims borrowers. A χ^2 test confirms that de-biasing is statistically higher in areas with high BJP support (χ^2=10.65, p<0.01).

4.2.3 Cross-Cohort Exposure to Hindu-Muslim Conflict.

The last dimension we consider uses variation across lender cohorts. We exploit the fact that the electoral support for the BJP has increased substantially since the early 2000s (e.g., see Menon and Nigam (2007)) and reached its peak with the increased visibility and popularity of Narendra Modi after becoming Prime Minister of India in 2014 (Chhibber and Verma (2014)). BJP's rise to national power has pushed the issue of Hindu-Muslim relations to the top of the agenda in the Indian political discourse. We exploit variation across cohorts of lenders who were exposed to the rise of BJP during their formative years or were only exposed to this phenomenon in adulthood when their political

[26]In contrast to the BJP, the Indian National Congress has been proposing instances of secularization and has been less supportive of conflict between the Hindu majority and Muslim minority (e.g., see Ganguly (2003), and Verma (2016)).

[27]We do not argue that the BJP vote share is a precise measure of the extent to which each lender supports the Hindu-Muslim conflict, but that, on average, it captures variation in the extent to which the conflict is salient to lenders.

[28]We obtain data on the official number of voters, residents, and votes cast for various parties for all elections to the national congress and state-level elections from 1977 to 2015 from Bhavnani (2014), whose data set is based on information from the Indian electoral commission.

beliefs were likely already cemented (Malmendier and Nagel (2011)). Specifically, we compare lenders born after 1990 (24 or younger when Narendra Modi became Prime Minister) to others.[29]

The right plots of Figure 4 report the results: pre-robo-advising bias is larger (7.1 pp) and statistically significant for lenders born after 1990, whereas the bias is small and statistically insignificant for others. Panel B shows the same patterns for bias reduction. These two estimated effects are not only economically but also statistically different (χ^2=4.43, p<0.01).

5 Stereotypical Discrimination: Shudra (Low-Caste) Borrowers

The second type of cultural bias we study is stereotypical discrimination—the fact that decision-makers systematically discriminate against certain social groups, because society attaches negative stereotypes to them (Becker (1957); Akerlof and Kranton (2000); D'Acunto et al. (2019); and Payne et al. (2019)). Stereotypical discrimination differs from in-group vs. out-group bias because even the members of the discriminated group discriminate against each other (Jost and Banaji (1994); Nosek et al. (2002); Pritlove et al. (2019)).[30]

The Indian setting is well suited to the study of stereotypical discrimination because of the centuries-long negative stereotypes attached to members of lower castes. Based on a set of traditional and foundational Hindu writings, the Indian society has been divided into five broad social groups for centuries: four *varnas*, or castes, and a fifth group of "outcasts" or untouchables (Fox (1969); Dumont (1980); Sinha and Sinha (1967)).[31] In the traditional interpretation, these social groups have a strict hierarchical relation to one other. *Brahmins*, the highest caste, traditionally included Hindu clerics and those who dedicated their lives to studying and contemplative activities. The second caste (*Kshatriyas*) historically covered governmental and military positions. The third caste, the *Vaishyas*, included farmers, traders, and merchants.[32] Against the three top varnas stands the (*Shudra*) caste, which has historically included laborers, peasants, and servants (Ambedkar (1947)). This caste was ranked lowest among other varnas and its members were employed in roles that benefited higher castes, which contributed to the diffusion of persistently negative stereotypes attached to (*Shudras*).[33]

5.1. Variation in the Recognizability of Borrowers' Caste

Castes are not always easily recognizable based on observational characteristics such as names and surnames, physical appearance, and occupation (Muthukumar (2020)). Variation in caste recognizability provides a natural

[29]The results are similar if we split the sample based on different years around 1990.

[30]For example, research finds that not only men but also women tend to rate women's quality and performance in leadership roles lower than men's, even when objective measures of performance across genders are similar (Bertrand et al. (2005); Brownstein (2015)).

[31]Here, we refer to the traditional scriptures-based notion of *varnas*. It does not coincide with the notion of *jati*, which is a richer and more complex sociological system based on which Hindus are further divided into other castes, tribes, and local social groups.

[32]Historians have emphasized similarities between the notion of *Vaishyas* and the *bourgeoisie* in pre-revolutionary France, for instance.

[33]Note that members of the outcast group, the *Dalits*, have faced even stronger discrimination and segregation over the centuries (Maikkēl (1999)). Less than 2% of the borrowers in our platform are *Dalits*, which hinders us from analyzing this group empirically.

test for the salience of borrowers' caste in lenders' choices, which would not exist in settings where social groups are easily recognizable. We exploit predictable variation in caste recognizability to test for the effects of stereotypical discrimination in lending. To capture the extent of recognizability of each borrower's caste, we build on an off-the-shelf algorithm by Bhagavatula et al. (2017) that assigns last names and other characteristics to castes and is designed to mimic the decision that a human would make based on the information at hand.[34] The procedure relies on two features of Indian castes: they are endogamous—marriages occur mainly between individuals belonging to the same caste—and last names are indicative of castes.

In the first step, the procedure collects data from 2.5 million individuals registered on online matrimonial agencies. This data contains information on individuals' last names and varna. The possibility of misreporting is virtually non-existent because prospective spouses search for same-caste matches through matrimonial agencies. In the second step, the procedure assigns one or more castes to each last name. We compute the probability of a surname belonging to a given caste as the share of matrimonial website users in each state holding that surname who belongs to the caste. In the third step, we assign a caste (and its probability) to each borrower and lender on our platform based on their last name and location. Figure A.7 in the Online Appendix plots the distribution of the probability of being *Shudra* for the borrowers in our sample for whom such a probability is strictly larger than zero—about 80% of all borrowers. Except for a right tail (17%) of borrowers whose probability of being *Shudra* is close to 1, the probability is distributed throughout the support.

5.2. Stereotypical Lending Before and After Robo-advising

The top graphs of Figure 5 plot the percentage of *Shudra* borrowers within all lenders' portfolios. The leftmost graph considers the full set of borrowers including those whose caste is barely recognizable. In this case, we detect no difference in the share of *Shudra* borrowers before and after lenders use the robo-advising tool: both shares equal 31%, which is the share of *Shudras* in the borrower population. This result is consistent with the possibility that no discrimination exists against *Shudra* borrowers when castes are barely identifiable.

We obtain different results when we restrict the sample to subgroups in which the borrowers' caste is more easily recognizable. The pre-Auto Invest lending to *Shudra* borrowers is 27% in subfigure (b) and only 17% in subfigure (c), which only includes borrowers whose caste is highly recognizable. On the other hand, lending after robo adoption does not change with caste recognizability.

As discussed above, stereotypical discrimination also arises among members of the same group. And indeed, in the bottom graphs of Figure 5, in which we restrict the sample to *Shudra* lenders, *Shudra* borrowers are even more discriminated against, possibly because *Shudra* lenders can more easily recognize their similar than the

[34] We thank Manaswini Bhalla for graciously running the algorithm developed in Bhagavatula et al. (2017) and Bhagavatula et al. (2018) on our data. Note that we could not reach out to lenders directly and ask them to assess borrowers' caste because this priming procedure might have affected their lending behavior on the platform, thus invalidating our analysis.

average Indian lender. We confirm the robustness of these results with the following multivariate specification:

$$Shudra\ Borrower_{i,j,t} = \alpha + \beta\ Auto\ Invest_{j,t} + \zeta\ \boldsymbol{x}_{i,t} + \eta_j + \eta_t + \epsilon_{i,j,t}, \tag{2}$$

where the regressors are defined as in equation 1. Because we want to focus on several subsamples of the data based on caste recognizability for both the baseline and heterogeneity results, to facilitate the comparison of many relevant coefficients across specifications, we report these results in graphical form. Panel A of Figure 6 contains four bars representing different estimates of β in equation (2) across different subsamples of the data.

For the first bar to the left, we do not impose any sample restrictions. The $\hat{\beta}$ estimate is positive (0.011) and statistically significant at the 5% level (t=2.54), indicating that using Auto Invest increases the probability of lending to Shudra borrowers. On the flip side, the result suggests that, before using Auto Invest, lenders were discriminating against Shudra borrowers. The second bar imposes that the probability of caste recognition is at least 40% and shows an effect similar to the first, both economically and statistically.

The last two columns restrict the estimating sample to lenders whose belonging to the Shudra caste is more easily recognizable—with probability larger than 70% and 80%, respectively—in this case, lending to Shudras after adopting Auto Invest more than doubles. These estimated effects are statistically significant at the 1% level, with t-statistics in excess of 3.5.

5.3. Heterogeneous Salience of Negative Stereotypes Attached to Lower Castes

Recent research documents substantial hatred crimes against members of lower castes, which has been heterogeneous across space (Sharma (2015); Bapuji and Chrispal (2020)). We conjecture that the stereotypical discrimination of *Shudra* borrowers might be higher in areas where the conflict between higher and lower castes is more salient due to the higher incidence and reporting of acts of violence against lower castes. To operationalize this conjecture, we collect the number of crimes against lower-caste victims per 100,000 inhabitants of Indian states from the annual report of the *National Crime Records Bureau (NCRB)* (NCRB (2019)). Figure 7 plots the cross-sectional variation in crimes against lower-caste victims.

We estimate equation (2) for lenders in Indian states above and below the median rate of crimes against lower castes per 100,000 inhabitants (18.8). We report the results in Panel B of Figure 6. For low levels of caste recognizability, we find no evidence of discrimination across space: the $\hat{\beta}$ estimates are not statistically different from zero. As recognizability increases, we start to observe a wedge between the two areas, which becomes more and more marked at higher levels of recognizability. Above 70% recognizability, the share of Shudra borrowers chosen by lenders in areas with high crimes against Shudras increases by 2.7 pp with robo adoption, which is about 12% more of the average share of Shudra borrowers in lenders' portfolios before accessing the tool (unreported in the graph). The effect is less than half for other lenders and statistically insignificant. These patterns become even starker when we condition on higher levels of caste recognizability.

6 Cultural Biases and Lenders' Performance

Under cultural biases, lenders should reach deeper into the pool of borrowers of the religion (or caste) they favor when choosing to whom they lend their money. As a result, they should lend to less creditworthy borrowers of the preferred religion (or caste). In contrast, they should reject more creditworthy borrowers of the religion (or caste) against whom they discriminate. Therefore, we conjecture that the performance of favored-group borrowers is worse than that of other borrowers before lenders access the robo-advising tool, and this difference drops after adoption.

Our setting allows assessing this conjecture while abstracting from the economic channels studied in earlier research that predict the opposite effect of cultural biases on performance. For instance, it abstracts from the potential screening and monitoring roles of in-group lending (Fisman, Paravisini, and Vig (2017) and Fisman, Sarkar, Skrastins, and Vig (2020)) because it connects lenders and borrowers all over India who do not live or work in the same small social environment.

6.1. The Cost of Cultural Biases: Sign, Size, and Channels

We first consider a measure of the extensive margin of lending performance—whether borrowers default on their loans. Loan defaults are a commonly used measure of performance and have also been studied as an outcome for moral financial decision-making (e.g., see Guiso et al. (2013)). In our case, loan defaults allow testing whether in-group borrowers are less likely to default on in-group lenders than out-group borrowers due to stigma from the in-group community, which is the opposite of what cultural biases predict.

For high-interest loans, lenders' returns might be high even in case of defaults as long as the borrower pays high interests before defaulting. We will thus also estimate the effects of cultural biases on loan returns.

6.1.1 **Loan Defaults.** To assess loans' default, we consider all the loans in our sample that were closed by the last month we have available—March 2020—and categorize as defaulted those loans that had been delinquent for more than 90 days at the time of closure.[35]

We start by considering in-group vs. out-group discrimination. For Hindu lenders, we estimate variations of

[35] This definition is close to the regulatory definition by the Reserve Bank of India (RBI) as well as to the economic definition of a defaulted loan, whereby the borrower did not pay back in full the loan's future value to the lender at the time the platform stopped monitoring the borrower. Note that we do not observe whether lenders engaged in litigation to collect borrowers' debentures after the loan was closed. If lenders were ultimately able to obtain a higher repayment than what is registered in the company's accounts, unfortunately, we cannot know.

the following specification by OLS:

$$
\begin{aligned}
Delinquent\ Loan_{i,j,t} = \alpha + \gamma\ Muslim\ Borrower_j \\
&+ \delta\ Muslim\ Borrower_{i,j} \times Auto\ Invest_{j,t} \\
&+ \theta\ Hindu\ Borrower_{i,j} \times Auto\ Invest_{j,t} + \zeta\ \boldsymbol{x}_{i,t} + \eta_j + \eta_t + \epsilon_{i,j,t}, \qquad (3)
\end{aligned}
$$

where $Delinquent\ Loan_{i,j,t}$ is equal to 1 if the loan associated with borrower i and lender j is closed as delinquent, and all other variables are defined as discussed above.

On average, Muslim borrowers are less likely to default relative to Hindu borrowers in Hindu lenders' portfolios *before* Auto Invest is used ($\hat{\gamma} < 0$). We report this result both graphically and in table format.

In panel A of Figure 8, we plot the estimated coefficient $\hat{\gamma}$ for the full sample of Hindu lenders ("All") as well as when estimating Equation 3 separately for the subsamples based on the heterogeneous salience of Hindu-Muslim conflict we discussed in the previous section. Not only are Muslim borrowers, on average, 4.6 percentage points less likely to default than Hindu borrowers before lenders access Auto Invest (about 16% of the average rate of default in the sample, i.e., 29%), but the size of this estimate is larger for lenders in states with a higher incidence of Hindu-Muslim riots, those in states with higher support for the BJP, and younger lenders. Columns (1)-(2) of Table 3 confirm these results and show their statistical significance using two alternative specifications: column (1) does not allow for the interaction between auto-invest and borrower religion, while column (2) does.

The second implication of biased discrimination is that, after Hindu lenders access Auto Invest, we should observe a greater reduction in the likelihood of default for Hindu borrowers than for Muslim borrowers, that is, estimates of θ should be smaller (i.e., more negative) than δ in Equation 3. Note that Auto Invest might provide additional benefits in terms of performance. For instance, it might provide lenders with a more diversified portfolio of borrowers. For this reason, the likelihood of default by Muslim borrowers might also decline with robo-advising. Discrimination, though, undoubtedly predicts that the likelihood of default should decline more for Hindu than Muslim borrowers. Results in column (2) of Table 3 are consistent with this implication: Muslim borrowers reduce their likelihood of default by 7.3 percentage points whereas Hindu borrowers by 11.2 percentage points—the effect is 53% larger for Hindu borrowers than for Muslim borrowers.

The third implication we bring to the data relates to the channels that should explain the difference in performance by Hindu and Muslim borrowers before and after robo-advising. If Hindu lenders were willing to dig deeper in the pool of Hindu borrowers and pick riskier borrowers in that group, because on our platform the highest-risk borrowers are disproportionally more likely to default than other borrowers (see Figure A.6 for the relation between ex-post default probabilities and interest rates), our results should be driven by a change in the composition of risky borrowers within lenders' portfolios after robo-advice adoption. But then, if we kept constant proxies for borrowers' risk characteristics–interest rate, maturity, and loan amount—the average differences in defaults across Hindu and Muslim borrowers should disappear.

This prediction provides a natural falsification test for our interpretation of the results. Column (3) of Table 3 shows results that align with the prediction. Once we control for borrower-level risk, neither Muslim borrowers default less than Hindu borrowers before robo-advising, nor do Hindu borrowers appear to improve their performance more than Muslim lenders after robo-advising. Also, the two point estimates for the decline in default rates—7.2 and 7 percentage points—are indistinguishable from the estimated improvement of Muslim borrowers in column (2), which is 7.3 percentage points.

Moving on to stereotypical discrimination, we assess whether Shudra borrowers were less likely to default than other borrowers before robo-advising, whether the improvement in default after robo-advising was larger for non-Shudra borrowers, and whether controlling for borrower-level risk characteristics reduces the differences in defaults.

Graphically, Panel B of Figure 8 shows that not only were Shudra borrowers less likely to default before lenders accessed Auto Invest, but this difference in default rates increased with borrowers' recognizability as members of the Shudra caste. The left plot shows that Shudra borrowers whose probability of being recognized was high (above 80%) were about 13 percentage points less likely to default than others. In contrast, when barely recognizable Shudra borrowers enter the sample, the lower likelihood of default was only 4.5 percentage points. This result is starker for lenders who reside in states with a higher incidence of crime against Shudras (right plot). Among highly recognizable Shudras, those picked by lenders in high-crime states were about twice less likely to default than other Shudra borrowers.

We also find evidence consistent with the second implication in column (5) of Table 3. Non-Shudra borrowers' performance improved by more than Shudra borrowers' performance, although the difference between these two effects (16 and 14.8 percentage points) is not statistically significant. Finally, column (6) shows that, once we control for borrower-level risk, both estimated effects decline and are aligned to about 10 percentage points.[36]

6.1.2 Fraction of Loan Repaid.

The second dimension of performance we consider is the fraction of the overall amount due (including principal and interest) the borrower pays back to the lender. This variable aims to capture the extent to which borrowers are willing to default on lenders at the intensive margin.

The share of the repaid amount is capped at 1 for borrowers who repay their loan in full. In principle, the share can be as low as 0 if a borrower does not repay anything, but in our data the number of borrowers who repay less than 20% of their amount due is minimal because the platform expels borrowers who pay less than 20% of the amount on any outstanding loans.

Panel A of Figure 9 focuses on in-group vs. out-group discrimination. The graph to the left plots the cumulative distribution functions (CDFs) for the share of the loans paid by Hindu borrowers (solid green line)

[36] Even in this case, before robo-advising, the lower likelihood of default of Shudra borrowers is lower although it does not become insignificant as we found for the case of Muslim borrowers in column (3).

and Muslim borrowers (orange dashed line) before Hindu lenders use Auto Invest. The evidence is consistent with the conjecture that Hindu lenders might dig deeper into the pool of Hindu borrowers than into the pool of Muslim borrowers when making choices on their own: Muslim borrowers repay larger shares of their amount due before the loan is closed. In fact, no Muslim borrowers picked by Hindu lenders paid less than 80% of the amount due in our sample. On the contrary, the repayment behavior of Hindu lenders is more volatile: about 20% of them repay less than 40% of the amounts due. Even when considering those who pay at least 80%, the CDF of Hindu borrowers is flatter than that of Muslim borrowers.[37]

The right graph of Figure 9 plots the CDFs for the shares of the amount due repaid by Hindu and Muslim borrowers to Hindu lenders after Hindu lenders start to use Auto Invest. Hindu borrowers improve disproportionally more than Muslim borrowers throughout the distribution. For instance, the share of Hindu borrowers who pay back more than 90% of their loans among those picked by Hindu lenders under Auto Invest increases to about 40% from 30% before Auto Invest.

We consider stereotypical discrimination in Panel B of Figure 9. The left graph splits borrowers whose probability of being Shudra is below the median (solid green line) and above the median (orange dashed line) before lenders access Auto Invest. Throughout the support, and especially in the left part of the distribution, borrowers who are more recognizable as Shudra tend to repay a higher fraction of their loans.

After accessing Auto Invest (right graph of Panel B of Figure 9), the distance between the CDFs decreases, and the convergence is largely driven by an improvement of the low-probability Shudra borrowers' repayment behavior—the solid green line shifts to the right. Even in the case of stereotypical discrimination, lenders seem to impose higher standards on highly recognizable Shudra borrowers than on others when making unassisted choices. After robo-adoption, the standards applied to Shudra and non-Shudra borrowers converge.

6.2. Loan Returns

We then consider loan returns given that high-interest loans might provide lenders with high cash flows and hence high returns even if borrowers default at some point and fail to repay the principal and interest due in full.

A potential concern when considering loan returns, though, is that returns might increase if lenders' portfolios become more diversified after they start using Auto Invest. In this case, an improvement in returns would not necessarily suggest that lenders choose higher-quality borrowers after activating the robo-advising tool but might be a byproduct of higher diversification in their portfolios.

Based on these arguments, we first assess whether lenders' portfolios are more diversified after lenders activate Auto Invest compared to before and, if so, which groups of borrowers (favored or discriminated) drive this improvement. We then assess how returns change with Auto Invest and which groups of borrowers increase

[37] Note that the share of Hindu borrowers who repay intermediate amounts between 50% and 80% is negligible.

lenders' returns, if any.

6.2.1 Portfolio Diversification.

The main challenge in assessing how portfolio diversification varies over time is to define portfolio diversification in the context of a portfolio of individual loans, many of which are issued at different times and do not produce cash flows at the same time. We, therefore, create two portfolios of loans for each lender, i.e., a pre-robo portfolio that includes all the loans issued before activating Auto Invest and a post-robo portfolio that provides for all the loans issued under the assistance of Auto Invest.

The first proxy we propose is the standard deviation of the loan size (rupee value) each lender issues before and after activating Auto Invest. The rationale is that if Auto Invest increased portfolio diversification by letting a lender issue several loans with standardized characteristics, loan size would be one of such characteristics, and we would observe an increase in the similarity of loan sizes after robo-adoption. Loans of the same size suggest equal weights in terms of lenders' exposure to different borrowers. Second, we consider the standard deviation of a lender's loan returns, the rationale being that a more diversified portfolio should lead lenders to make less extreme returns on the individual loans they issue. Third, we consider a measure of similarity of the cash flows lenders obtain from loans—the average standard deviation of the size of the monthly payments they receive from each borrower. Intuitively, if all borrowers paid exactly the monthly payments set at loan issuance throughout the loan's life, the standard deviation across the payments of each borrower and the average across borrowers would be zero. The higher the average standard deviation, instead, the more irregular the cash flows lenders obtain from their portfolio of loans each month.

In Table A.3, we report the results for estimating specifications similar to column (1) of Table 3. Across the board, we fail to detect systematic differences between lenders' portfolio diversification before and after activating Auto Invest.

Recall that our main prediction of interest is whether different groups of borrowers contribute more or less to any improvements in lenders' performance. Even though on average, diversification does not improve after the adoption of robo-advising, favored religious and caste groups might still contribute positively to an improvement in diversification and others negatively, and hence an effect through diversification could still explain our performance results. In Table 4, we thus estimate specifications similar to column (2) of Table 3. In columns (1)-(3), we do not detect any systematic patterns in terms of Hindu or Muslim borrowers contributing differently to the change in diversification in Hindu lenders' portfolios (which did not change on average after adoption). Similarly, we do not detect any systematic patterns for Shudra and non-Shudra borrowers in contributing to lenders' portfolio diversification.

6.2.2 Average Loan Returns.

We move on to consider lenders' returns. We obtained the detailed monthly-servicing information for a random sample of the loans on the platform, which, added to the sample restriction of only using closed loans as discussed in the analysis of defaults, unfortunately, reduces the sample size for the analysis in this section substantially relative to the analyses in the first part of the paper. Although the smaller sample size reduces statistical power, we show that we can still reject the null hypotheses we bring to the data both economically and statistically.

We consider two types of specifications. First, like in the analysis of defaults, we assess the return changes at the loan level before and after lenders access Auto Invest in OLS specifications of the following form:

$$\begin{aligned} Loan\ Return_{i,j,t} = \alpha + \gamma\ Muslim\ Borrower_j \\ + \delta\ Muslim\ Borrower_j \times Auto\ Invest_{j,t} \\ + \theta\ Hindu\ Borrower_j \times Auto\ Invest_{j,t} + \zeta\ \boldsymbol{x}_{i,t} + \eta_j + \eta_t + \epsilon_{i,j,t}, \end{aligned} \tag{4}$$

where $Loan\ Return_{ij}$ is the return associated with the loan borrower, i obtains from lender j at the time the loan servicing is closed (whether repaid in full or delinquent). We standardize returns to ease the interpretation of the magnitudes of the results. All other variables are defined as discussed above.

The predictions of cultural biases we test are analogous to those discussed for the case of loan defaults, except that the predicted signs of coefficients are flipped for the case of loan returns. First, cultural biases would imply that Muslim borrowers provided Hindu lenders with higher returns than Hindu borrowers before robo-advice adoption ($\gamma > 0$), because Hindu lenders systematically overestimated the expected returns they would obtain from Hindu borrowers and underestimated those from Muslim borrowers.

Second, after robo-advice adoption, Hindu borrowers' returns should increase more than Muslim borrowers' returns ($\theta > \delta$). Note that, as we discussed in section *2.2.*, the tool is likely to improve lenders' loan portfolios above and beyond its effect on cultural biases. For instance, the tool might increase the diversification of lenders' loan portfolios. For this reason, if cultural biases are present, Muslim borrowers' returns might also increase after access to robo-advising, but by a lower amount than Hindu borrowers' returns. The difference between the change in the average returns of Hindu and Muslim borrowers would bound the return value of cultural biases because, by construction, any improvement in Muslim borrowers' returns cannot be explained by cultural biases but might be explained by other effects of robo-advising common to both groups of borrowers.

As in our analysis of defaults, the third "falsifying" prediction we bring to the data is that, if Hindu borrowers' improvement is driven by a systematic change in the riskiness of Hindu borrowers that are matched with Hindu lenders by the robo-advising tool, once we keep constant proxies for the riskiness of loans, the differences in loan returns across Hindu and Muslim borrowers should be muted. Otherwise, channels other than a systematic difference in the riskiness of the Hindu and Muslim borrowers chosen by Hindu lenders when unassisted might explain the differential performance.

In Table 5, we report the results when bringing these three predictions to the data. Columns (1)-(3) consider in-group vs. out-group discrimination. We find that, on average, Muslim borrowers' loans earned Hindu lenders higher returns before lenders adopted robo-advising—about half a standard deviation higher returns, or 12.5 percentage-point higher returns. At the same time, Hindu lenders' returns increased on average after adoption by about 20% of a standard deviation, or five percentage points.

Column (2) reveals that the improvement in Hindu lenders' returns after robo-advising adoption is almost exclusively driven by an increase in the returns earned on loans issued to Hindu borrowers (22% of a standard deviation higher, or 5.5 percentage points). By contrast, Muslim borrowers' loan returns, which were already performing better before adoption, did not change in any economically or statistically significant manner after robo-advice adoption. These patterns are consistent with the conjectures about performance under in-group vs. out-group discrimination by Hindu lenders.

Moreover, consistent with the third conjecture, once we absorb dimensions that capture the riskiness of loans in column (3), we fail to detect systematic differences in the change of loan returns between Hindu and Muslim borrowers after lenders adopt the robo-advising tool. This result suggests that the asymmetric improvement in performance across groups of borrowers we detected in column (2) is explained by a systematic change in the riskiness of the two pools of borrowers matched to lenders after robo-advice adoption.

We then move on to stereotypical discrimination. Column (4) of Table 5 reports the results for the baseline specification. Even in this case, the results align with the predictions of cultural debiasing. First, Shudra borrowers, who were discriminated against before robo-advice adoption, provided higher loan returns to lenders when lenders made their choices unassisted. Shudra borrower returns were almost one-quarter of a standard deviation higher than non-Shudra borrower's returns, which in the sample of Hindu borrowers we use in this analysis corresponds to about 6.5 percentage-point higher returns.

Moreover, lenders increased their average returns after accessing robo-advice relative to before. Column (5) compares the contribution of Shudra and non-Shudra borrowers to the increase in average returns after robo-advice and shows that, indeed, Shudra borrowers improve more than non-Shudra borrowers after Auto-Invest adoption.

When we absorb the proxies for the riskiness of loans in column (6), consistent with our third prediction, we find that the estimated improvements in returns after robo-advising are similar in magnitude and statistical significance across the two groups of borrowers. This result corroborates the interpretation that a systematically different composition of risk profiles across groups of borrowers drives the improvement in returns after robo-advising.

***6.2.3* Loan Return Distributions.** The results on the average returns of loans issued to different groups of borrowers before and after robo-advice adoption show the important role of cultural biases. Yet, the results based on sample averages do not reveal which parts of the distribution of loan returns are responsible for the improvement. Suppose lenders tended to pick very risky borrowers from their favorite group before accessing robo-advising, and the tool does not pick such risky borrowers. In that case, we should observe that the improvement in performance associated with adopting robo-advice originates from eliminating these loans from the lenders' portfolios. If, instead, lenders tended to pick riskier in-group borrowers throughout the distribution, we should not detect any differential role of the tails relative to the rest of the borrowers' riskiness distribution.

To assess which parts of the loan return distribution drive our results, we follow D'Acunto and Rossi (2022) and estimate a set of quantile regressions of the following form:

$$Q_\tau(Returns_{i,j}) = \alpha(\tau) + \beta(\tau)\ Auto\ Invest_j +\ X'_{i,j}\ \zeta(\tau) + \epsilon_{i,j}, \tag{5}$$

whose outcome variable is quantile Q_τ of the distribution of the return associated with borrower i and j throughout the sample period. All other variables are defined as in equation (3).

To interpret the estimates of equation (5), consider the special case of the median, which is the 50^{th} percentile of the distribution. The coefficient $\hat{\beta}(50)$ estimates that the median return was $\hat{\beta}(50)$ higher after lender j moved to Auto Invest relative to before. A positive $\hat{\beta}(50)$ would suggest that the median of the distribution has shifted to the right. The advantage of estimating quantile regressions is that we can assess how the whole intensive margin (distribution) has changed rather than focusing on specific moments, such as the conditional mean.

We report the results for estimating the baseline OLS specification in columns (1)-(2) of Panel A of Table 6 and the quantile regression estimates in columns (3)-(8).

In the first line of each column, we focus on specifications that *do not* control for the risk characteristics of loans, such as interest rates and amount, allowing for the possibility that differences in loan risks drive differences in returns before and after access to robo-advising.

Based on the hypothesis that Hindu lenders picked in-group borrowers of worse quality before moving to Auto Invest, we should find that they improve their performance after accessing Auto Invest. The same should be true for Muslim lenders, who, under cultural biases, should have picked in-group borrowers of worse quality than otherwise available out-group borrowers. And indeed, the first line of columns (1)-(2) of Panel A of Table 6 reveals that both Hindu and Muslim lenders improve their performance in terms of average loan returns.

Moving on to the quantile regression results (columns (3)-(8)), they reveal that most of the returns' improvement is driven by substantially higher returns in the left tail of the distribution, as can be seen by the fact that the size of the estimated coefficients is larger for the 25^{th} and 50^{th} percentiles of the return distribution and declines as we move towards the right (the 75^{th} percentile).

In the second line of each column, we add borrower-level loan characteristics as controls to further assess whether any changes before and after Auto Invest might be driven by systematically different choices in terms of borrower risk—for instance, whether lenders tended to choose systematically riskier in-group borrowers relative to out-group borrowers when making unassisted choices.

The evidence is consistent with a risk channel that explains the performance improvement lenders enjoy when moving to Auto Invest. Once we absorb differences in the riskiness of loans, conditional returns do not differ when lenders make choices on their own or when the robo-advising tool makes choices on their behalf, either on average or in terms of the different parts of the distribution of loan returns, including the left tail of the distribution.

We perform the same analysis for the case of stereotypical discrimination. The first line of Panel B of Table 6 estimates the relationship for Shudra borrowers without controlling for the loans' risk characteristics. In contrast, the second line estimates the relationship conditional on borrower-level risk proxies.

The patterns we uncover for stereotypical discrimination are the same as for the case of in-group vs. out-group discrimination. The bulk of the return improvement is driven by eliminating the left tail of low-return borrowers. Moreover, once we estimate the differential returns conditional on loans' risk measures, we detect no systematic differences before and after Auto Invest, which is consistent with the conjecture that the return improvement under Auto Invest is due to the selection of a less risky set of borrowers.

Ultimately, our analysis of performance suggests that eliminating cultural biases improves lenders' performance, and this improvement is driven by a change in the composition of the borrower pool that reduces lenders' risk exposure. This reduction is largely driven by eliminating a left tail of low-return loans and is consistent with the conjecture that culturally-biased lenders dig deeper into the pool of in-group borrowers and hence select riskier (and lower return) borrowers when making unassisted choices.

7 Quantifying the Cost of Cultural Biases: Lender-Level Returns

In this section, we propose a quantification of the aggregate effects of cultural biases at the aggregate lender level rather than a reduced-form analysis at the lender-loan level, on which our analysis has focused thus far.

We start by computing the change in the returns each lender made on their overall invested amounts before and after accessing the automated robo-advising tool, both in general as well as separately for the amounts lenders disbursed to in-group vs. out-group religions as well as to Shudra vs. non-Shudra borrowers. For each lender, we define the total return on the investment before and after Auto Invest as follows:

$$Lender\ Total\ Return_{i,t} = 100 \times \frac{\sum_j Amount\ Disbursed_{i,j,t} \times Loan\ Return_{j,t}}{\sum_j Amount\ Disbursed_{i,j,t}}, \quad (6)$$

where $Lender\ Total\ Return_{i,t}$ is the overall return on the aggregate investment made by lender i earned either before access to Auto Invest ($t = PRE$) or after access to Auto Invest ($t = POST$); $Amount\ Disbursed_{i,j,t}$ is the amount (in rupees) lender i disbursed to loan j, which was issued either before or after access to Auto Invest (t); $Loan\ Return_{j,t}$ is the return of loan j to which lender i contributed.

The quantities defined by equation (6) thus capture the total returns lenders realized before and after using

Auto Invest. We then compute the lender-level change in total return across the two conditions:

$$Change\ Lender\ Return_i = Lender\ Total\ Return_{i,POST} - Lender\ Total\ Return_{i,PRE}, \tag{7}$$

where a positive value indicates that lender i earned a higher total return on their investment after accessing Auto Invest relative to before, and a negative value the opposite.

In Figure 10, we plot the density of the distributions of $Change\ Lender\ Return_i$ for lenders in the in-group vs. out-group discrimination sample (panel A) and those in the stereotypical discrimination sample (panel B). For each distribution, we indicate the mean of the distribution with a solid vertical line and compare it to a dashed vertical line that indicates no change in returns. We find that the average lender in the in-group vs. out-group discrimination sample earned a 4.5-percentage-point higher total return after accessing Auto Invest relative to before. In contrast, the average lender in the stereotypical discrimination sample earned a 7.3-percentage-point higher return.

Our regression results at the lender-borrower-loan level suggested that most of the loan defaults and repayment behavior improvement derived from borrowers belonging to demographics that lenders tended to favor before moving to Auto Invest. As expected, this pattern holds in terms of lender-level total returns. For instance, if we compute the change in lender returns defined in equation 6 separately for Hindu lenders based on the amounts they lent to Hindu borrowers or Muslim borrowers before and after Auto Invest, we find Hindu lenders, on average, gained a 4.3-percentage-point higher return on the amounts disbursed to Hindu borrowers, whereas they actually on average "lost" (an insignificant) 0.5 percentage points in returns on the amounts disbursed to Muslim borrowers. Virtually the whole improvement in the average lender-level returns derives from higher returns earned on the loans disbursed to favored demographic groups.

To capture the rupee-level change in performance and hence the lender-level and the aggregate value of cultural biases, we need a measure of lender-level performance in which returns are value-weighted—they are weighted by the rupee amounts each lender disburses to borrowers on the platform. A challenge to define such a measure is that the amounts lenders disbursed before and after accessing Auto Invest might differ for many reasons, which are potentially unrelated to cultural biases, lender characteristics, or platform characteristics. We, therefore, compute the following:

$$\begin{aligned} &Change\ Lender\ Value_i \\ &\quad = [Amount\ Disbursed_{i,POST} \times Return_{i,POST} - Amount\ Disbursed_{i,PRE} \times Return_{i,PRE}] \\ &\quad - [Amount\ Disbursed_{i,POST} \times Return_{i,POST} - Amount\ Disbursed_{i,PRE} \times Return_{i,POST}]. \end{aligned} \tag{8}$$

The expression defined in equation (8) allows us to purge the difference in total rupee-value earnings at the lender level merely due to the fact that lenders might disburse different amounts before and after accessing Auto

Invest, irrespective of the returns they earn in the two periods.

Note that equation (8) is equivalent to $Amount\ Disbursed_{i,PRE} \times Change\ Lender\ Return_i$, and hence this measure can be interpreted as the change in lender-level return after accessing Auto Invest relative to before weighted by the rupee amount the lender disbursed on the platform before accessing Auto Invest, which cannot have been determined by the returns the lender earned after starting to use the tool. Ultimately, this value captures the incremental rupee amount each lender would have earned in the period before accessing Auto Invest had they realized the post-Auto Invest adoption returns.

We find that the average change in lenders' rupee value for lenders in the in-group vs. out-group discrimination sample is ₹457, which is about 6% of the average amount of resources disbursed by each lender in the period before accessing Auto Invest (₹7,543).[38] When we consider the stereotypical discrimination sample, we find that the average change is higher: ₹862, which represents about 12% of the average amount lenders in this sample disbursed before accessing Auto Invest (₹7,091). Overall, the estimated cost of cultural biases appears to be sizable for both types of biases we study.

Note that the calculation proposed in equation (8) does not account for the possibility that the rupee amounts disbursed to each demographic group would have been different had cultural biases not influenced lenders' choices in the period before accessing Auto Invest. To assess whether accounting for this difference could influence our estimates of the costs of cultural biases substantially, we thus propose a modified version of the definition in equation (8) for robustness purposes. We separate the amounts disbursed and returns earned in the pre- and post-periods by Hindu lenders coming from Hindu and Muslim borrowers for the case of in-group vs. out-group discrimination and from Shudra and non-Shudra borrowers for the case of stereotypical discrimination.

To obtain a counterfactual for the first period, we split the amounts Hindu lenders disbursed through the platform in the pre-period among the two borrower groups (Hindus vs. Muslims and Shudra vs. non-Shudra) based on the shares of the post-period funds lenders attribute to each group rather than the true shares they attributed to them in the pre-period. In this way, we keep the true total amounts lenders disbursed through the platform in the pre-period fixed, but we assume that, if lenders faced no cultural biases in the pre-period, they would have split such amounts between borrowers based on the post-period shares.

Economically, the change in the shares of Hindu borrowers in Hindu lenders' portfolios cannot be large, given that most borrowers on the platform are Hindu. Indeed, the share of Hindu borrowers in Hindu lenders' portfolios moves from 89.7% in the pre-period to 88.7% in the post-period. For this reason, we would not expect that the correction we propose in this second method will deliver estimates for the cost of in-group vs. out-group bias that are substantially different from those discussed above. And indeed, we find that the average lender-level change in earnings for Hindu lenders based on this correction is ₹382, which is quite similar to the value estimated above.

[38] Note that the average size of loans on the platform is substantially larger (₹90,523), because, as we discussed when introducing our setting, each loan borrowers received is financed by multiple lenders.

The same correction, instead, is likely to imply a larger estimated cost of the bias for the case of stereotypical discrimination because the share of Shudra borrowers in Hindu lenders' portfolios goes from 41.8% in the pre-period to 46.2% in the post-period. When accounting for this composition change in the pre-period, we obtain an average cost of bias of ₹2,254 at the lender level, which is more than twice as large as the estimate that does not use this correction.

8 Conclusions

We propose a field setting to test for and quantify the extent and cost of cultural biases to discriminators in high-stake economic choices. Our setting allows us to compare the choices the same decision makers make before and after observing optional suggestions from a robo-advising tool. We detect evidence that in-group vs. out-group and stereotypical discrimination are prevalent and economically sizable. These forms of discrimination make discriminating individuals—in our case, lenders—worse in terms of consumption utility because by discriminating, they finance loans by borrowers who perform worse than other borrowers available on the lending platform. Our results are most consistent with inaccurate statistical discrimination—biased beliefs about borrowers' quality—because lenders do not override robo-advised suggestions of lending to previously-discriminated borrowers and economic incentives to not affect the extent of discrimination.

Our tests and results also suggest a new role that algorithmic-based robo-advising tools might have for future research in economics—they can provide a benchmark to assess the types and sizes of agents' biases in decision-making. For instance, by coding robo-advising tools that embed different forms of biases or rules of thumb detected in the literature and by comparing decision-makers' unassisted choices with those they make after accessing such tools, one could disentangle the role of alternative biases and quantify them.

Moreover, future research should study whether exposure to robo-advising suggestions lets decision-makers learn about optimal choices and develop rules of thumb that can also assist them when a robo-advisor is unavailable. Interactive robo-advising tools might teach borrowers how to create goal-setting strategies (Gargano and Rossi (2023)) or provide just-in-time simple financial literacy contents (Burke et al. (2021)).

More broadly, our results beget additional work across several fields on understanding how human and machine-based decision-making interact and complement or substitute each other in a world where the combination of the two forms of decision-making is becoming ubiquitous in all daily economic decision-making problems agents face.

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Figure 2: Lending to In-Group vs. Out-Group Borrowers: Intensive Margin

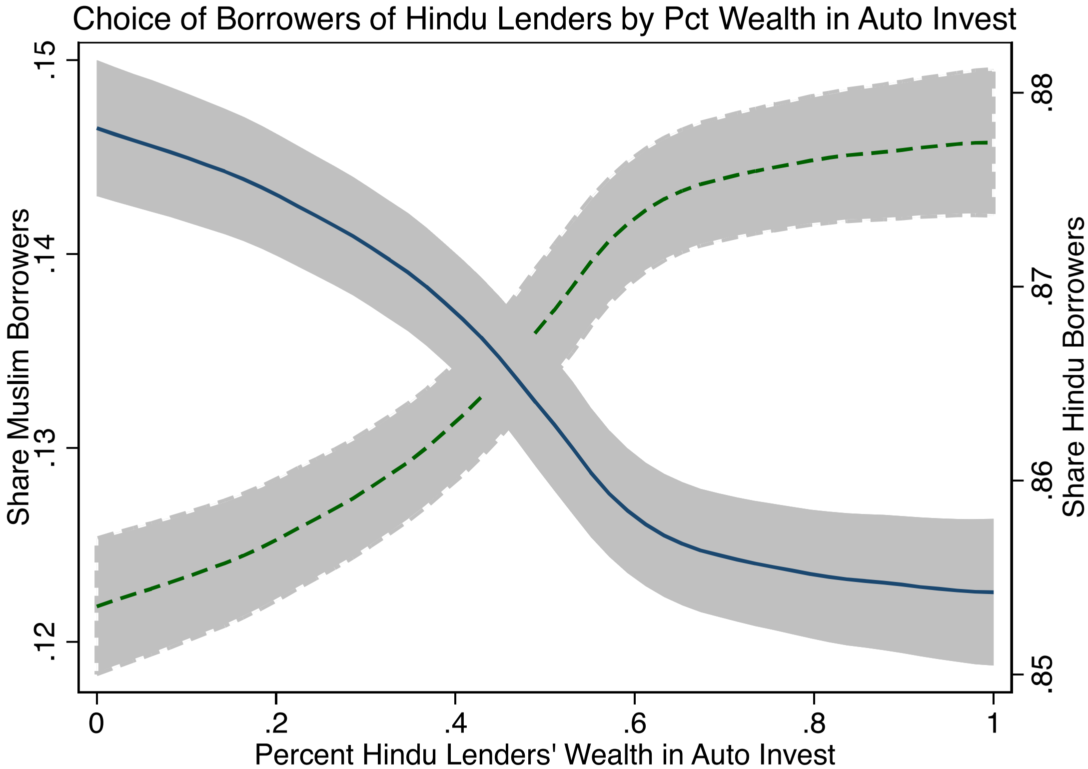


Figure 2 plots the coefficient estimates of kernel-weighted local mean smoothing regressions for whether borrowers are Hindu (blue, solid line, measured on the right y-axis) and whether borrowers are Muslim (green, dashed line, measured on the left y-axis) on the share of their available funds Hindu borrowers who moved to the robo-advising lending tool (Auto Invest) allocated to such tool. This share represents the intensive margin of usage of Auto Invest by Hindu borrowers. Grey bandwidths correspond to 95% confidence intervals around the estimated coefficients. We use an Epanechnikov kernel and evaluate the relation at 50 grid points.

Figure 3: Spatial Heterogeneity of In-group vs. Out-group Conflict

Panel A. Hindu-Muslim Riots, 1980-2000

Panel B. Average Vote Shares for the Bharatiya Janata Party (BJP) 1977-2015

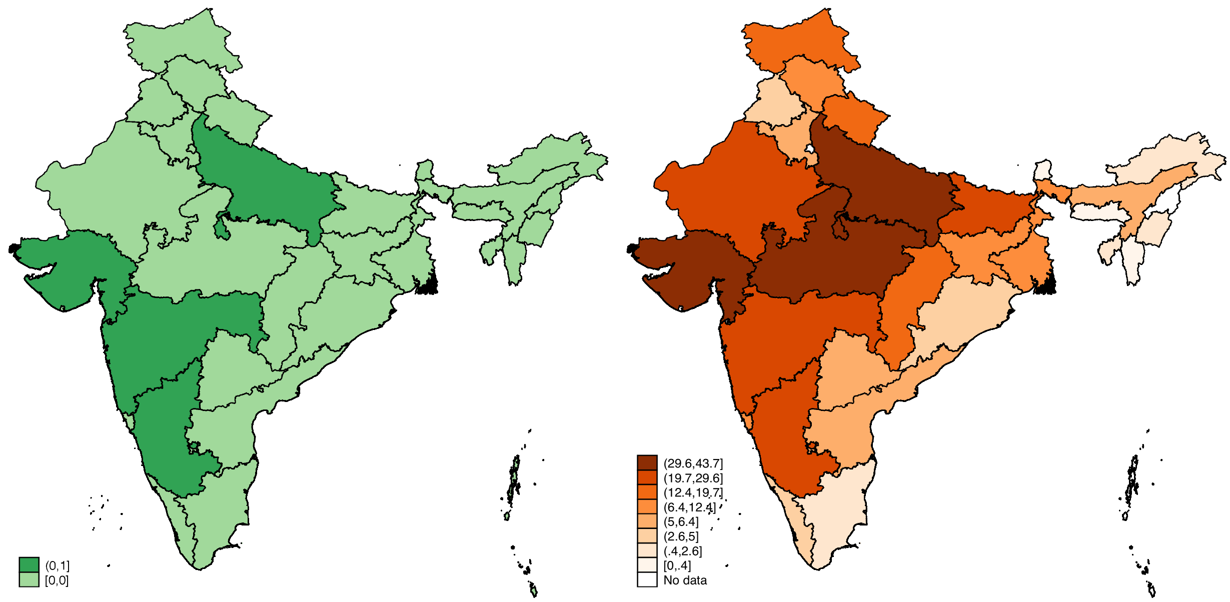
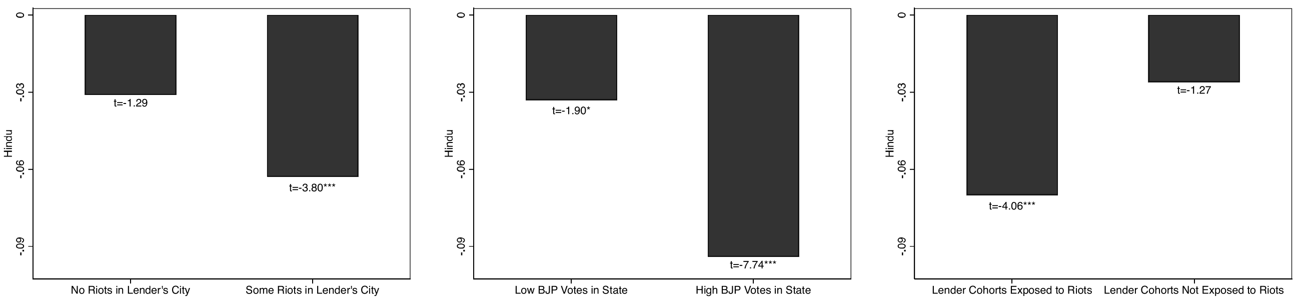


Figure 3 depicts the spatial variation of proxies for the vividness of Hindu-Muslim conflict across Indian states. Panel A compares states in which large-scale riots between Hindus and Muslims and/or pogroms against the Muslim minority happened between 1980 and 2000. Dark green states (Gujarat, Uttar Pradesh, Delhi, Marahashtra, and Karnataka) are states where such events happened based on Ticku (2015). Panel B compares states based on the average vote share of BJP candidates to national and local elections between 1977 and 2015. We obtain candidate-level election results from 1977 to 2015 from Bhavnani (2014). We first compute the voting shares for each election in each state and then average these shares within states. The darker a state, the higher the average BJP vote share.

Figure 4: Change in Lending to Out-group Borrowers: Salience of Hindu-Muslim Conflict

Panel A. Bias Before Auto Invest ($\hat{\gamma}$ Coefficient)



Panel B. De-Biasing After Auto Invest ($\hat{\delta}$ Coefficient)

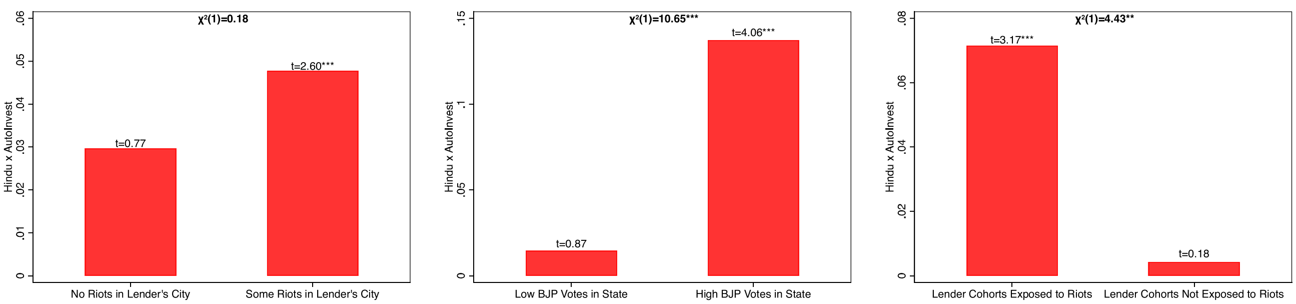


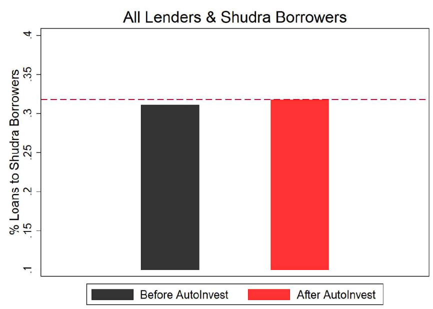
Figure 4 reports the results of estimating the following specification by ordinary least squares across different subsamples reported on top of each column:

$$
\begin{aligned}
Muslim\ Borrower_{i,j,t} = {} & \alpha + \beta\ Auto\ Invest_{j,t} + \gamma\ Hindu\ Lender_j \\
& + \delta\ Hindu\ Lender_j \times Auto\ Invest_{j,t} + \zeta\ \boldsymbol{x}_{i,t} + \eta_j + \eta_t + \epsilon_{i,j,t}
\end{aligned}
$$

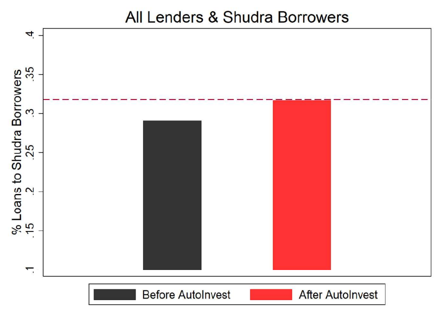
where $Muslim\ Borrower_{i,j,t}$ is equal to 1 if the borrower i who receives funding from lender j in year t is Muslim, and zero otherwise; $Auto\ Invest_{j,t}$ is equal to 1 if the lender made the loans after activating Auto Invest and 0 otherwise; $Hindu\ Lender_j$ is equal to 1 if lender j is Hindu; and $\boldsymbol{x}_{i,t}$ is a vector of loan-level characteristics that are direct proxies for the risk profiles of the loans lenders extend to borrowers—loan maturity (measured in months), loan amount, and the annual interest rate associated with the loan. These loan-level characteristics are assigned to borrowers by the platform's algorithm when the loan requests are vetted before borrowers access the borrower pool. η_j is a full set of lender fixed effects and η_t is a full set of year fixed effects, which we use in our most restrictive specifications to only exploit variation within lenders and/or within years. We cluster standard errors at the lender level. Panel A plots estimated coefficient $\hat{\gamma}$, which captures the extent of lender bias before accessing Auto Invest. Panel B plots estimated coefficient $\hat{\delta}$, which captures the de-biasing effect of Auto Invest.

Figure 5: Lending to Discriminated Borrowers:
Shudra Caste Borrowers Before and After Robo-Advising

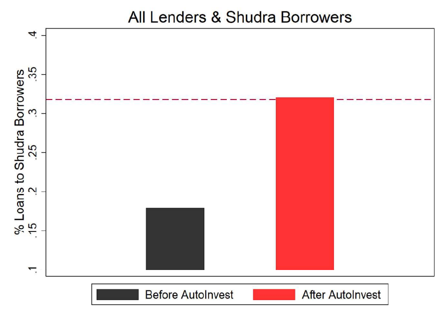
Panel A. All Lenders



(a) Caste Barely Recognizable (Prob≥0%)

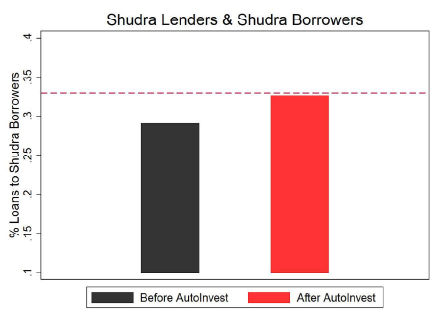


(b) Caste Somewhat Recognizable (Prob>50%)

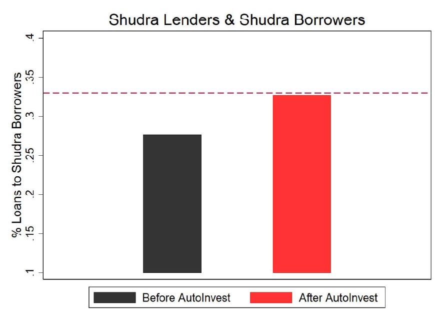


(c) Caste Easily Recognizable (Prob>70%)

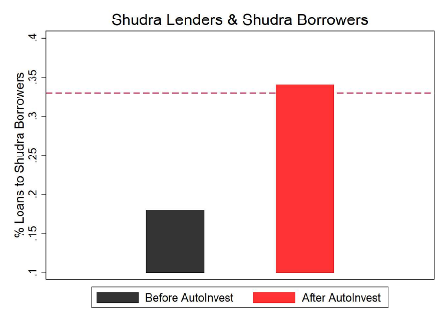
Panel B. Only Shudra Lenders (Easier to Recognize Own Caste)



(d) Caste Barely Recognizable (Prob≥0%)



(e) Caste Somewhat Recognizable (Prob>50%)

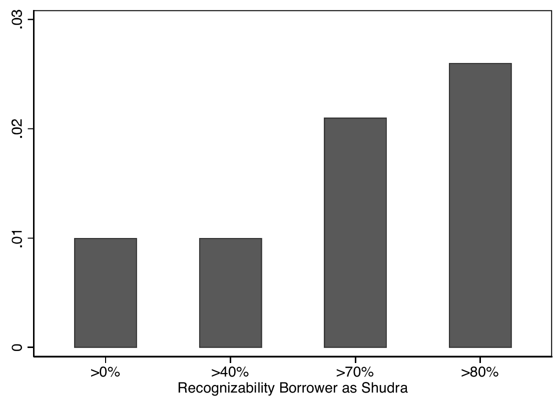


(f) Caste Easily Recognizable (Prob>70%)

Figure 5 plots the average share of borrowers in Hindu lenders' portfolios who are Shudra before lenders moved to the robo-advising tool (Auto Invest, black bars) and after lending decisions are made by Auto Invest (red bars). Panel A considers all Hindu lenders on the platform whereas Panel B only includes Shudra Hindu lenders, for whom recognizing the caste of Shudra borrowers might be weakly easier. In each panel, the left graph considers all borrowers in lenders' portfolios; the middle graph only considers borrowers whose caste can be recognized by a human with a probability above 50% as defined by the algorithm designed by Bhagavatula et al. (2018); the right graph only considers borrowers whose caste can be recognized with a probability above 70% based on the same algorithm.

Figure 6: Change in Lending to Discriminated Borrowers—Shudra Caste Borrowers

Panel A. De-Biasing After AutoInvest by Recognizability of Shudra Borrowers



Panel B. Heterogeneous De-biasing After AutoInvest by Salience of Shudra Discrimination

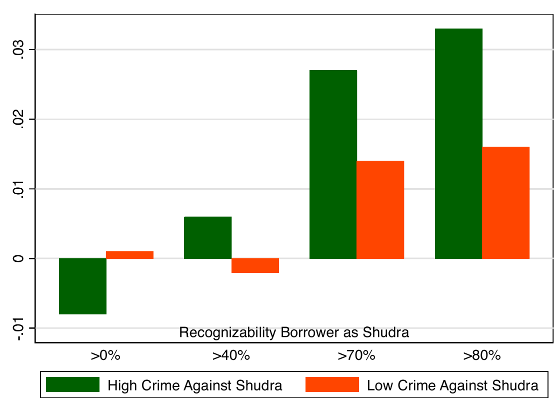


Figure 6 reports the results of estimating the following specification by ordinary least squares:

$$Shudra\ Borrower_{i,j,t} = \alpha + \beta\ Auto\ Invest_{j,t} + \zeta\ \boldsymbol{x}_{i,t} + \eta_j + \eta_t + \epsilon_{i,j,t}$$

where $Shudra\ Borrower_{i,j,t}$ is equal to 1 if the borrower i who receives funding from lender j in year t is Shudra and zero otherwise; $Auto\ Invest_{j,t}$ is equal to 1 if the lender made the loans after activating Auto Invest and 0 otherwise; and $\boldsymbol{x}_{i,t}$ is a vector of loan-level characteristics that are direct proxies for the risk profiles of the loans lenders extend to borrowers—loan maturity (measured in months), loan amount, and the annual interest rate associated with the loan. These loan-level characteristics are assigned to borrowers by the platform's algorithm when the loan requests are vetted before borrowers access the borrower pool. Panel A reports the $\hat{\beta}$ coefficients across Shudra borrowers with different recognizability. Panel B further divides the estimates across states with high and low crimes against Shudra. We cluster standard errors at the lender level.

Figure 7: Spatial Heterogeneity of Salience of Stereotypical Discrimination

Crimes Against Scheduled Castes per inhabitant (2018)

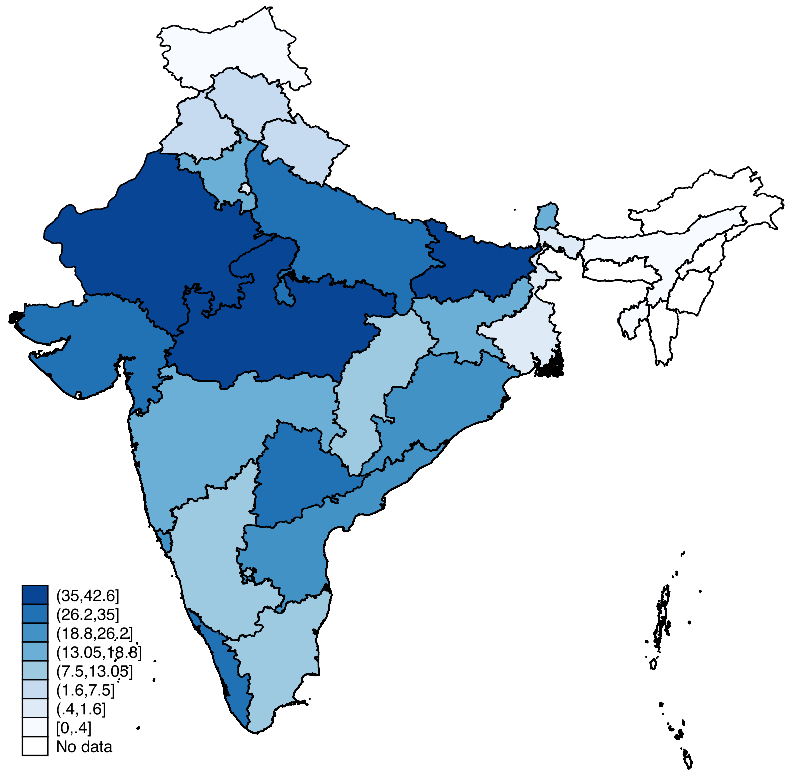
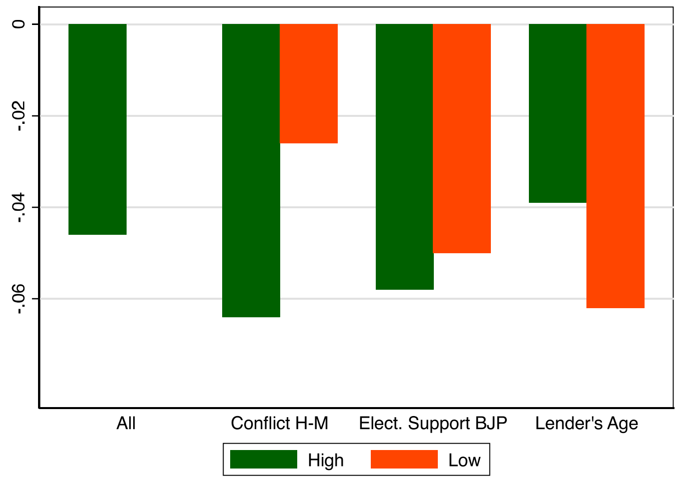


Figure 7 depicts the spatial variation of a proxy for the salience of discrimination against lower castes by Hindus across Indian states, that is, the number of crimes against Scheduled Castes (which includes members of the Shudra varna as well as those belonging to lower castes) per 100,000 inhabitants in 2018 based on the official data from the Indian National Crime Records Bureau (NCRB (2019)). The darker a state, the higher the number of crimes against Schedules Classes per inhabitant in the state.

Figure 8: Lower Default of Discriminated Borrowers Relative to Others Before Auto Invest

Panel A. In-group vs. Out-group Discrimination



Panel B. Stereotypical Discrimination

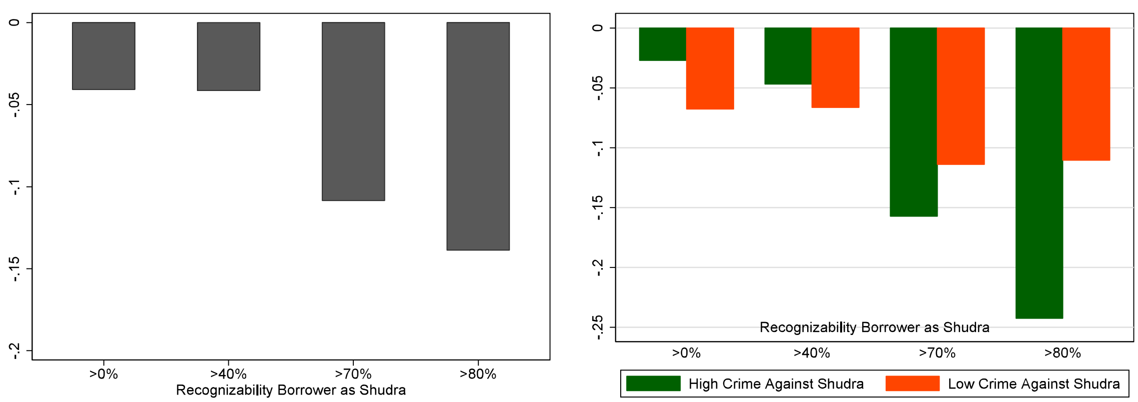
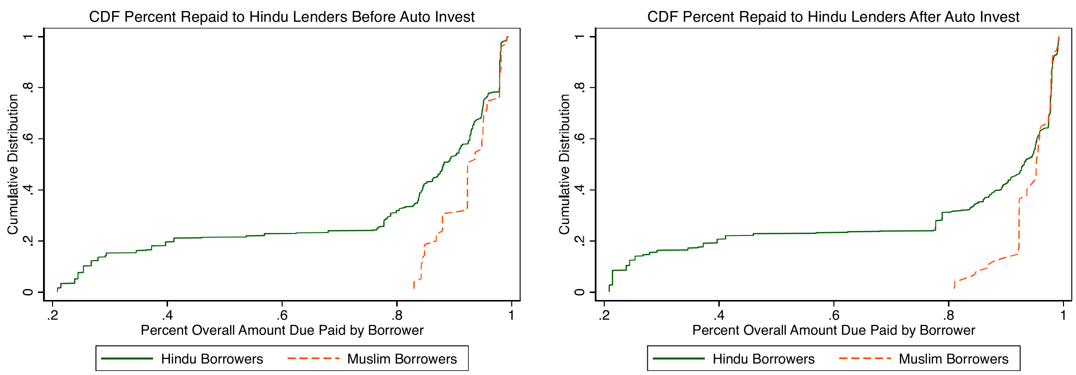


Figure 8 plots the relative default of Muslim borrowers relative to Hindu borrowers in Hindu lenders' portfolios (Panel A) and of Shudra borrowers relative to other Hindu borrowers in all Hindu lenders' portfolios before lenders moved to the robo-advising tool (Auto Invest) and across different subsamples. Panel A includes borrowers in Hindu lenders' portfolios. In Panel B, the probability of caste recognition of Shudra borrowers is based on the algorithm developed by Bhagavatula et al. (2018).

Figure 9: Fraction of Loan Repaid Before and After Robo-Advising

Panel A. In-group vs. Out-group Discrimination



Panel B. Stereotypical Discrimination

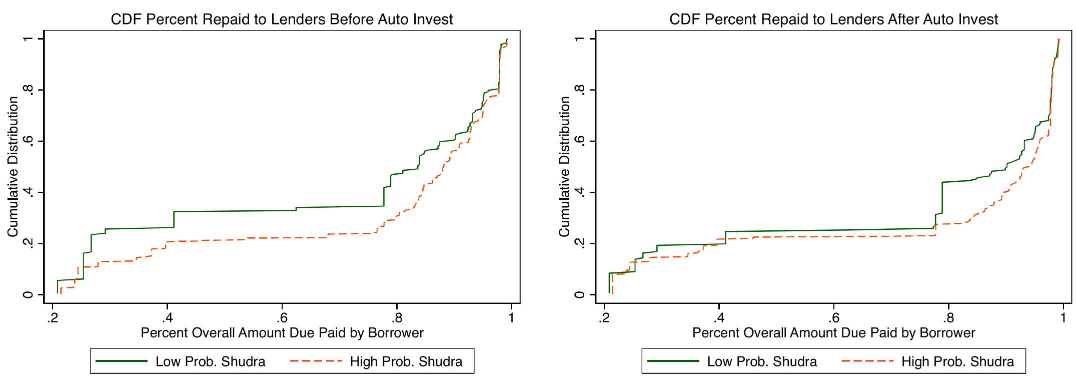
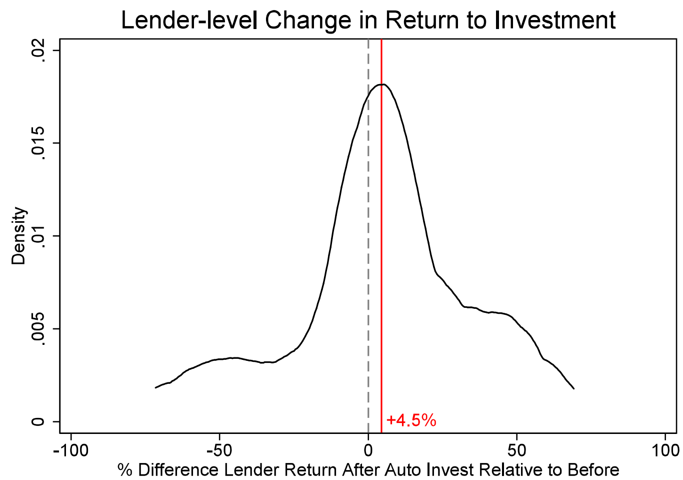


Figure 9 plots a set of cumulative distribution functions (CDFs) for different groups of borrowers over the share of the overall amounts due (principal plus interest) that borrowers repaid by the time their loan account was closed. In all Panels, the left graph refers to the CDFs of borrowers in lenders' portfolios before moving to the robo-advising tool (Auto Invest). In contrast, the right graph refers to the CDFs after moving to Auto Invest. Panel A includes borrowers in Hindu lenders' portfolios. The solid green lines are the CDFs for Hindu borrowers, and the orange dashed lines are for Muslim borrowers. Panel B only includes Shudra borrowers in Hindu lenders' portfolios. The solid green lines are the CDFs for Shudra borrowers whose probability of caste recognition is below 15% based on the algorithm developed by Bhagavatula et al. (2018), and the orange dashed lines for other Shudra borrowers.

Figure 10: Lender-level Change in Returns After Robo-Advising

Panel A. In-group vs. Out-group Discrimination



Panel B. Stereotypical Discrimination

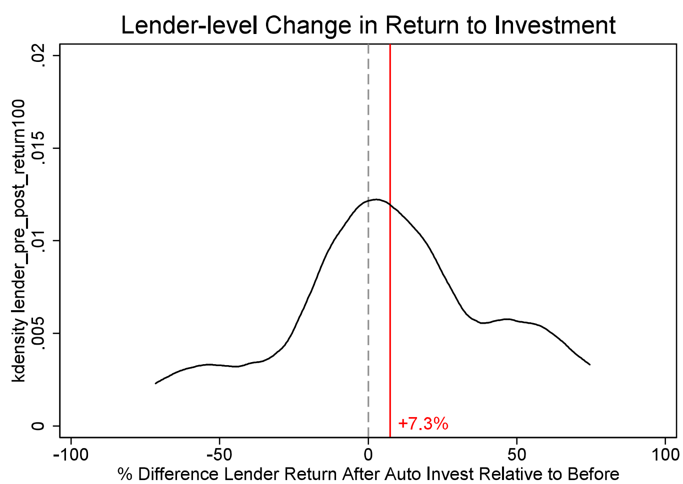


Figure 10 plots the density of the lender-level average change in the returns to loans originated on the platform after using Auto Invest relative to before. To compute the change, we first compute the overall return to the amounts invested by each lender separately before and after using Auto Invest across all loans they originated. We then subtract the two returns and average these changes across lenders. Loan-level returns are thus value-weighted within lenders, and lender-level returns are equally weighted across lenders. Panel A refers to Hindu lenders' loans to Hindu borrowers (in-group vs. out-group discrimination), whereas Panel B refers to all Hindu lenders' loans to Shudra borrowers (stereotypical discrimination).

Table 1. Summary Statistics

Panel A. In-group vs. Out-group Discrimination Sample

	N. obs.	Mean	St. dev.	25*th* perc.	Median	75*th* perc.
Muslim Borrower	113,283	0.13	0.34	0.00	0.00	0.00
Hindu Lender	113,283	0.99	0.11	1.00	1.00	1.00
Auto Invest	113,283	0.45	0.50	0.00	0.00	1.00
Auto Invest allocation (%)	113,283	0.59	0.37	0.22	0.60	1.00
Tenure (months)	113,283	22.08	8.98	15.00	24.00	24.00
Loan Amount (rupees)	113,283	131,074	102,575	50,000	100,000	188,000
Interest Rate	113,283	0.24	0.07	0.20	0.23	0.27
Delinquent	113,127	0.29	0.45	0.00	0.00	1.00

Panel B. Stereotypical Discrimination Sample

	N. obs.	Mean	St. dev.	25*th* perc.	Median	75*th* perc.
Shudra Borrower	62,831	0.39	0.49	0.00	0.00	1.00
Auto Invest	62,831	0.43	0.49	0.00	0.00	1.00
Auto Invest allocation (%)	62,831	0.58	0.37	0.22	0.57	1.00
Tenure (months)	62,831	22.03	9.02	12.00	24.00	30.00
Loan Amount (rupees)	62,831	131,797	105,994	50,000	100,000	200,000
Interest Rate	62,831	0.24	0.07	0.20	0.23	0.26
Delinquent	62,736	0.28	0.45	0.00	0.00	1.00

Table 1 reports summary statistics for the main variables in the analysis across the two datasets used in the analysis of in-group vs. out-group discrimination (Panel A) and stereotypical discrimination (Panel B). In both panels, the unit of observation is a lender-borrower-loan triad. Borrower-lender characteristics include the religion/caste of borrowers and lenders. *Auto Invest* is a dummy variable that equals 1 if the lender uses the robo-advising lending tool, whereas *Auto Invest allocation* is the share of funds lenders have available on the P2P platform that they allocate to the robo-advising tool. Loan-level characteristics include the loans' tenure, size, and annual interest rate, as well as a dummy variable that equals 1 if the loan was delinquent at the time it was closed and zero otherwise.

Table 2. Change in Lending to Out-group Borrowers: Hindu vs. Muslim

Dependent variable: Muslim Borrower					Low Use Auto Invest	High Use Auto Invest
	(1)	(2)	(3)	(4)	(5)	(6)
Hindu Lender × Auto Invest	0.045**	0.046**	0.045**	0.043**	0.009	0.052*
	(2.51)	(2.51)	(2.07)	(1.96)	(0.23)	(1.94)
Hindu Lender	-0.058***	-0.058***				
	(-3.52)	(-3.54)				
Auto Invest	-0.026	-0.025	-0.030	-0.033	0.011	-0.048*
	(-1.45)	(-1.40)	(-1.43)	(-1.53)	(0.29)	(-1.83)
Maturity		0.012***	0.009***	0.010***	0.010**	0.009**
		(4.78)	(3.20)	(3.35)	(2.31)	(2.31)
Loan Amount (₹000)		-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
		(-10.34)	(-10.81)	(-10.92)	(-5.47)	(-9.27)
Interest Rate		-0.009	-0.015	-0.012	0.013	-0.035
		(-0.47)	(-0.74)	(-0.62)	(0.47)	(-1.25)
Constant	0.181***	0.163***	0.119***	0.120***	0.099***	0.137***
	(11.14)	(8.74)	(11.72)	(11.77)	(6.77)	(9.60)
Lender Fixed Effects			✓	✓	✓	✓
Year Fixed Effects				✓	✓	✓
N. obs.	113,284	113,283	113,283	113,283	39,366	72,104

Table 2 reports the results of estimating the following specification by ordinary least squares:

$$Muslim\ Borrower_{i,j,t} = \alpha + \beta\ Auto\ Invest_{j,t} + \gamma\ Hindu\ Lender_j + \delta\ Hindu\ Lender_j \times Auto\ Invest_{j,t} + \zeta\ \boldsymbol{x}_{i,t} + \eta_j + \eta_t + \epsilon_{i,j,t},$$

where $Muslim\ Borrower_{i,j,t}$ is equal to 1 if the borrower i who receives funding from lender j in year t is Muslim, and zero otherwise; $Auto\ Invest_{j,t}$ is equal to 1 if the lender made the loans after activating Auto Invest and 0 otherwise; $Hindu\ Lender_j$ is equal to 1 if lender j is Hindu; and $\boldsymbol{x}_{i,t}$ is a vector of loan-level characteristics that are direct proxies for the risk profiles of the loans lenders extend to borrowers—loan maturity (measured in months), loan amount, and the annual interest rate associated with the loan. These loan-level characteristics are assigned to borrowers by the platform's algorithm when the loan requests are vetted before borrowers access the borrower pool. η_j is a full set of lender fixed effects and η_t is a full set of year fixed effects, which we use in our most restrictive specifications to only exploit variation within lenders and/or within years. We cluster standard errors at the lender level.

Table 3. Loan Defaults Before and After Robo-Advising

Dependent variable:	**In-group vs. Out-group Discrimination**			**Stereotypical Discrimination**		
Loan-level Delinquency	Hindu Lenders			All Lenders		
	(1)	(2)	Falsification (3)	(4)	(5)	Falsification (6)
Auto Invest	-0.108***			-0.157***		
	(-5.08)			(-6.25)		
Hindu Borrower× Auto Invest		-0.112***	-0.072***			
		(-5.21)	(-4.55)			
Muslim Borrower× Auto Invest		-0.073**	-0.070***			
		(-2.49)	(-2.82)			
Muslim Borrower	-0.024**	-0.046**	-0.002			
	(-2.02)	(-2.58)	(-0.10)			
Non-Shudra Borrower× Auto Invest					-0.160***	-0.100***
					(-6.04)	(-4.39)
Shudra Borrower× Auto Invest					-0.148***	-0.104***
					(-4.54)	(-3.32)
Shudra Borrower				-0.038***	-0.041***	-0.030**
				(-3.03)	(-2.89)	(-2.14)
Constant	0.499***	0.501***	0.403***	0.490***	0.491***	0.421***
	(30.95)	(30.84)	(17.69)	(32.13)	(32.25)	(7.77)
Loan Risk Characteristics			✓			✓
Lender Fixed Effects			✓			✓
Year Fixed Effects			✓			✓
N. obs.	16,985	16,985	16,985	6,821	6,821	6,821
R-Square	0.012	0.012	0.263	0.020	0.020	0.357

Table 3 reports the results of estimating variations of the following specification by ordinary least squares:

$$\begin{aligned} \textit{Delinquent Loan}_{i,j,t} = \alpha &+ \gamma\ \textit{Muslim Borrower}_{i,j} \\ &+ \delta\ \textit{Muslim Borrower}_{i,j} \times \textit{Auto Invest}_{j,t} \\ &+ \theta\ \textit{Hindu Borrower}_{i,j} \times \textit{Auto Invest}_{j,t} + \zeta\ \boldsymbol{x}_{i,t} + \eta_j + \eta_t + \epsilon_{i,j,t}, \end{aligned}$$

where $\textit{Delinquent Loan}_{i,j,t}$ is equal to 1 if the loan associated with borrower i and lender j in year t is closed as delinquent; $\textit{Auto Invest}_j$ is equal to 1 if the lender made the loans after activating Auto Invest and 0 otherwise; $\textit{Muslim Borrower}_{i,j}$ ($\textit{Hindu Borrower}_{i,j}$) is equal to 1 if the borrower i who receives funding from lender j is Muslim (Hindu), and zero otherwise; $\boldsymbol{x}_{i,t}$ is a vector of loan-level risk characteristics—loan maturity (measured in months), loan amount, and the annual interest rate associated with the loan. These loan-level characteristics are assigned to borrowers by the platform's algorithm when the loan requests are vetted before borrowers access the borrower pool; and η_j and η_t are full sets of lender and year fixed effects. We cluster standard errors at the lender level.

Table 4. Lenders' Portfolio Diversification Before and After Robo-Advising

Dependent variable:	**In-group vs. Out-group Discrimination**			**Stereotypical Discrimination**		
Portfolio-level Diversification (*see columns*)	St.dev Loan Sizes (1)	St.dev Loan Returns (2)	St.dev Payments (3)	St.dev Loan Sizes (4)	St.dev Loan Returns (5)	St.dev Payments (6)
Share Hindu Borrowers× Auto Invest	-0.004 (-1.11)	-0.023 (-0.31)	0.002 (1.39)			
Share Muslim Borrowers× Auto Invest	-0.003 (-0.18)	-0.301 (-0.72)	-0.006 (-0.82)			
Share Muslim Borrowers	0.014 (0.67)	-0.351 (-0.96)	0.005 (0.80)			
Share Non-Shudra Borrowers× Auto Invest				-0.006 (-0.84)	-0.215 (-1.51)	-0.002 (-1.01)
Share Shudra Borrowers× Auto Invest				-0.022** (-2.09)	0.178 (0.82)	0.003 (0.94)
Share Shudra Borrowers				0.012 (0.89)	-0.773*** (-3.81)	-0.002 (-0.64)
Constant	0.045*** (17.04)	0.632*** (10.77)	0.040*** (36.95)	0.038*** (7.55)	0.926*** (8.22)	0.043*** (26.14)
N	2,586	399	2,707	998	292	979
R-Square	0.001	0.018	0.001	0.014	0.033	0.001

Table 4 reports the results of estimating variations of the following specification by ordinary least squares at the lender's level:

$$\begin{aligned} Diversification\ Portfolio_{j,t} = \alpha &+ \gamma\ Share\ Muslim\ Borrowers_{j,t} \\ &+ \delta\ Share\ Muslim\ Borrowers_{j,t} \times Auto\ Invest_{j,t} \\ &+ \theta\ Share\ Hindu\ Borrowers_{j,t} \times Auto\ Invest_{j,t} + \epsilon_{j,t}, \end{aligned}$$

where $Diversification\ Portfolio_{j,t}$ is computed at the lender level j before and after Auto Invest (t) in three ways: the standard deviation of the sizes of individual loans in lender j's portfolio before and after Auto Invest (columns (1) and (4), the standard deviation of the returns of the loans originated in lender j's portfolio before and after Auto Invest (columns (2) and (5)), and the standard deviation of the size of the monthly payments lender j received from their loans before and after Auto Invest; $Auto\ Invest_j$ is equal to 1 if the lender made the loans after activating Auto Invest and 0 otherwise; $Share\ Muslim\ Borrowers_{j,t}$ ($Share\ Hindu\ Borrowers_{j,t}$) is the share of Muslim (Hindu) borrowers in lender j's portfolio before and after Auto Invest; In this case, because the level of observation is a lender, we cluster standard errors at the city level. All the results are similar if we cluster at the lender level, i.e. compute simply heteroskedasticity-robust standard errors.

Table 5. Loan Returns Before and After Robo-Advising

Dependent variable:	**In-group vs. Out-group Discrimination**			**Stereotypical Discrimination**		
Loan-level Return	Hindu Lenders			All Lenders		
			Falsification			Falsification
	(1)	(2)	(3)	(4)	(5)	(6)
Auto Invest	0.201***			0.300***		
	(3.02)			(2.14)		
Hindu Borrower× Auto Invest		0.222***	0.098			
		(3.07)	(0.78)			
Muslim Borrower× Auto Invest		-0.012	-0.028			
		(-0.18)	(-0.19)			
Muslim Borrower	0.282***	0.438**	0.313***			
	(6.25)	(5.13)	(4.44)			
Non-Shudra Borrower× Auto Invest					0.307***	0.245
					(2.98)	(1.12)
Shudra Borrower× Auto Invest					0.283**	0.192
					(2.33)	(0.79)
Shudra Borrower				0.106*	0.122	0.257**
				(1.77)	(1.49)	(2.40)
Constant	0.361***	0.349***	-0.061	0.314***	0.310***	-1.261***
	(5.09)	(4.68)	(-0.37)	(3.05)	(2.80)	(-5.51)
Loan Risk Characteristics			✓			✓
Lender Fixed Effects			✓			✓
Year Fixed Effects			✓			✓
N. obs.	2,134	2,134	2,134	859	859	859
R-Square	0.263	0.264	0.575	0.338	0.338	0.705

Table 5 reports the results of estimating variations of the following specification by ordinary least squares:

$$\begin{aligned} Loan\ Return_{i,j,t} &= \alpha + \gamma\ Muslim\ Borrower_{i,j} \\ &+ \delta\ Muslim\ Borrower_{i,j} \times Auto\ Invest_{j,t} \\ &+ \theta\ Hindu\ Borrower_{i,j} \times Auto\ Invest_{j,t} + \zeta\ \boldsymbol{x}_{i,t} + \eta_j + \eta_t + \epsilon_{i,j,t}, \end{aligned}$$

where $Loan\ Return_{i,j,t}$ is the standardized return of the loan associated with borrower i and lender j in year t at loan closure; $Auto\ Invest_j$ is equal to 1 if the lender made the loans after activating Auto Invest and 0 otherwise; $Muslim\ Borrower_{i,j}$ ($Hindu\ Borrower_{i,j}$) is equal to 1 if the borrower i who receives funding from lender j is Muslim (Hindu), and zero otherwise; and $\boldsymbol{x}_{i,t}$ is a vector of loan-level risk characteristics—loan maturity (measured in months), loan amount, and the annual interest rate associated with the loan. These loan-level characteristics are assigned to borrowers by the platform's algorithm when the loan requests are vetted before borrowers access the borrower pool; and η_j and η_t are full sets of lender and year fixed effects. We cluster standard errors at the lender level.

Table 6. Loan Returns Before and After Robo-Advising:
Quantile Regressions

Panel A. In-group vs. Out-group Discrimination

Dependent variable:	**OLS**		**25**th *percentile*		**Median**		**75**th *percentile*	
Loan's Return	Hindu (1)	Muslim (2)	Hindu (3)	Muslim (4)	Hindu (5)	Muslim (6)	Hindu (7)	Muslim (8)
Auto-Invest	0.217***	0.065**	0.250*	0.173**	0.274***	0.121***	0.109***	-0.004
(Without Risk Controls)	(4.92)	(2.25)	(1.82)	(2.23)	(11.22)	(3.84)	(11.66)	(-0.29)
Auto-Invest	-0.063**	-0.006	-0.008	-0.002	-0.035	-0.000	0.000	0.008
(With Risk Controls)	(-2.20)	(-0.30)	(-1.01)	(-0.38)	(-0.04)	(-0.04)	(0.02)	(1.04)
N. obs.	2,326	220	2,326	220	2,326	220	2,326	220

Panel B. Stereotypical Discrimination

Dependent variable:	**OLS**		**25**th *percentile*		**Median**		**75**th *percentile*	
Loan's Return	Low Prob Shudra (1)	High Prob Shudra (2)	Low Prob Shudra (3)	High Prob Shudra (4)	Low Prob Shudra (5)	High Prob Shudra (6)	Low Prob Shudra (7)	High Prob Shudra (8)
Auto-Invest	0.079***	0.068***	0.485***	0.080*	0.088***	0.072***	0.030***	0.025***
(Without Risk Controls)	(2.89)	(4.54)	(4.61)	(1.73)	(3.02)	(8.13)	(5.64)	(9.29)
Auto-Invest	-0.053	-0.039	-0.001	0.000	-0.011	0.014**	0.020	0.020***
(With Risk Controls)	(-0.82)	(-1.04)	(-0.07)	(0.05)	(-0.55)	(1.99)	(0.55)	(4.83)
N. obs.	462	1,158	462	1,158	462	1,158	462	1,158

Table 6 reports the results of estimating the following set of quantile regressions:

$$Q_\tau(Loan's\ Return_{i,j}) = \alpha(\tau) + \beta(\tau)\ Auto\ Invest_j +\ X'_{i,j}\ \zeta(\tau) + \epsilon_{i,j},$$

whose outcome variable is quantile Q_τ of the distribution of the (standardized) loan return associated with borrower i and lender j throughout the sample period; $Auto\ Invest_j$ is equal to 1 if the lender made the loans after activating Auto Invest and 0 otherwise; and $X_{i,j}$ is a vector of loan-level characteristics that are direct proxies for the risk profiles of the loans lenders extend to borrowers—loan amount, and the annual interest rate associated with the loan. These loan-level characteristics are assigned to borrowers by the platform's algorithm when the loan requests are vetted before borrowers access the borrower pool. In each panel, the first row reports the estimates of $\beta(\tau)$ *without* controlling for loans' risk characteristics. The second row reports the estimates of the same specifications when risk controls are included. We cluster standard errors at the lender level.

Online Appendix:

How Costly Are Cultural Biases? Evidence from FinTech

Francesco D'Acunto, Pulak Ghosh, Alberto G. Rossi

Not for Publication

Figure A.1: Platform's Screening of Borrowers 1:
Credit Scores of Approved and Rejected Borrowers

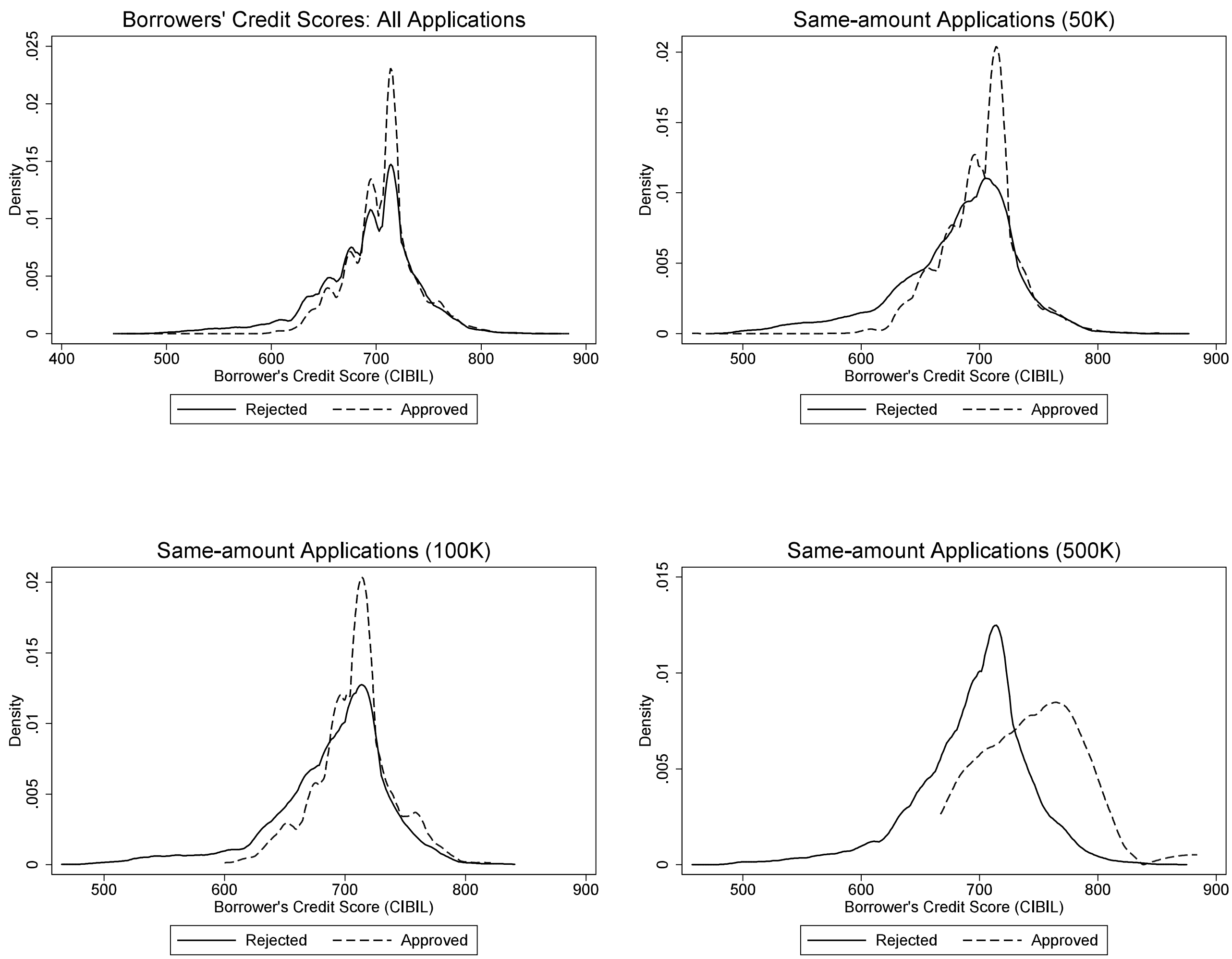


Figure A.2: Platform's Screening of Borrowers 2:
Interest Rates, Maturities, and Loan Amounts by Credit Score

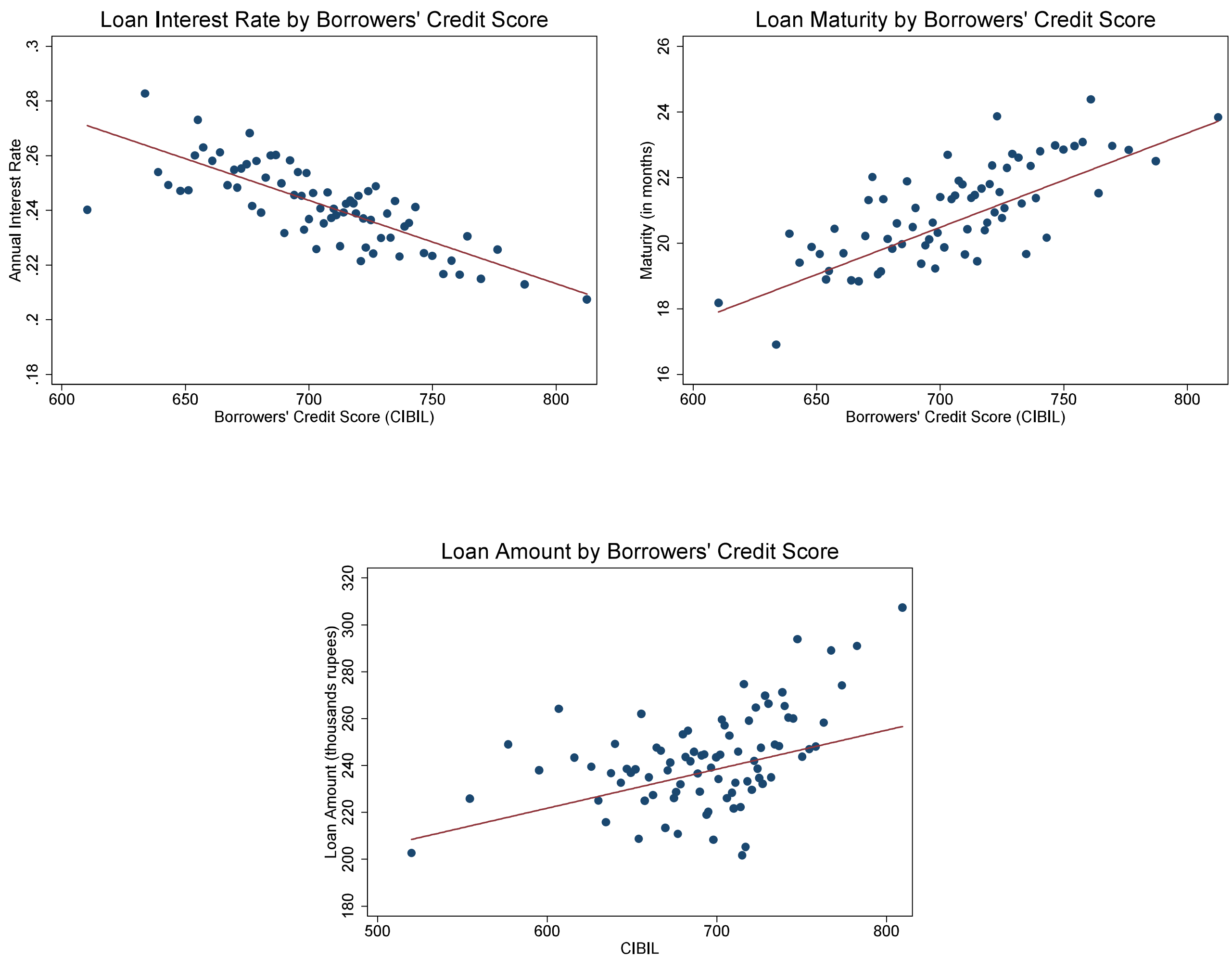
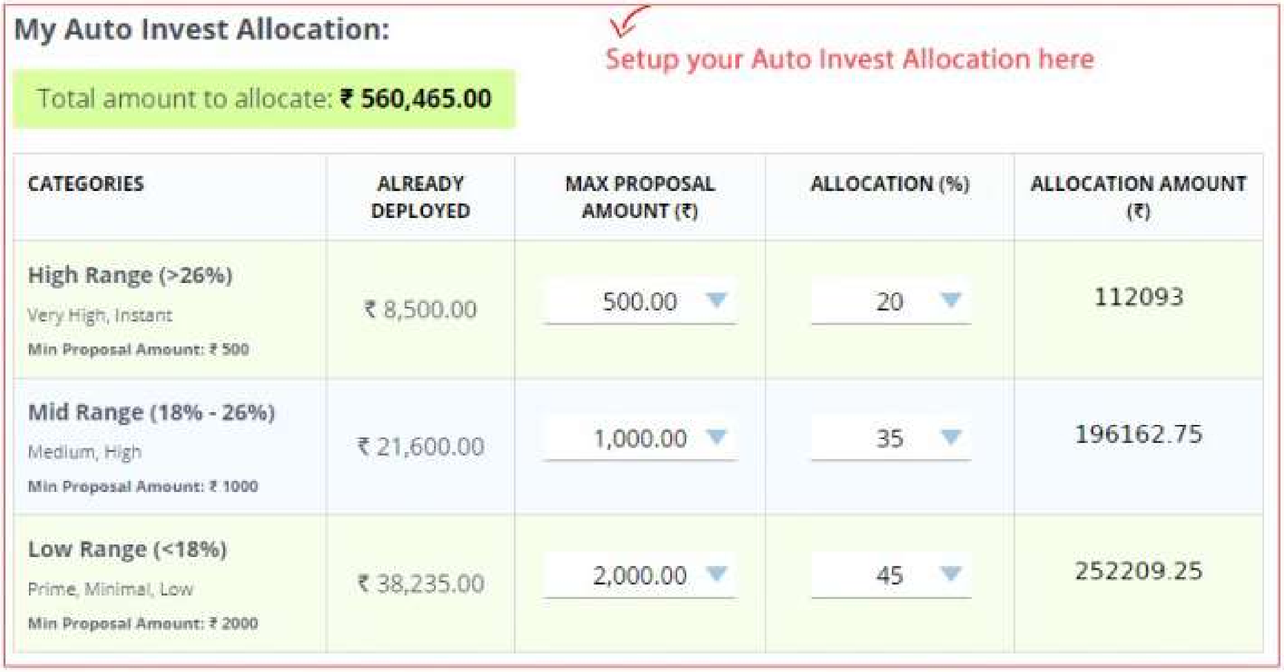


Figure A.3: Robo-Advising Tool—Auto Invest



My Auto Invest Allocation:

Setup your Auto Invest Allocation here

Total amount to allocate: **₹ 560,465.00**

CATEGORIES	ALREADY DEPLOYED	MAX PROPOSAL AMOUNT (₹)	ALLOCATION (%)	ALLOCATION AMOUNT (₹)
High Range (>26%) Very High, Instant Min Proposal Amount: ₹ 500	₹ 8,500.00	500.00	20	112093
Mid Range (18% - 26%) Medium, High Min Proposal Amount: ₹ 1000	₹ 21,600.00	1,000.00	35	196162.75
Low Range (<18%) Prime, Minimal, Low Min Proposal Amount: ₹ 2000	₹ 38,235.00	2,000.00	45	252209.25

Figure A.4: Number of Lenders Adopting Auto Invest by Month-Year

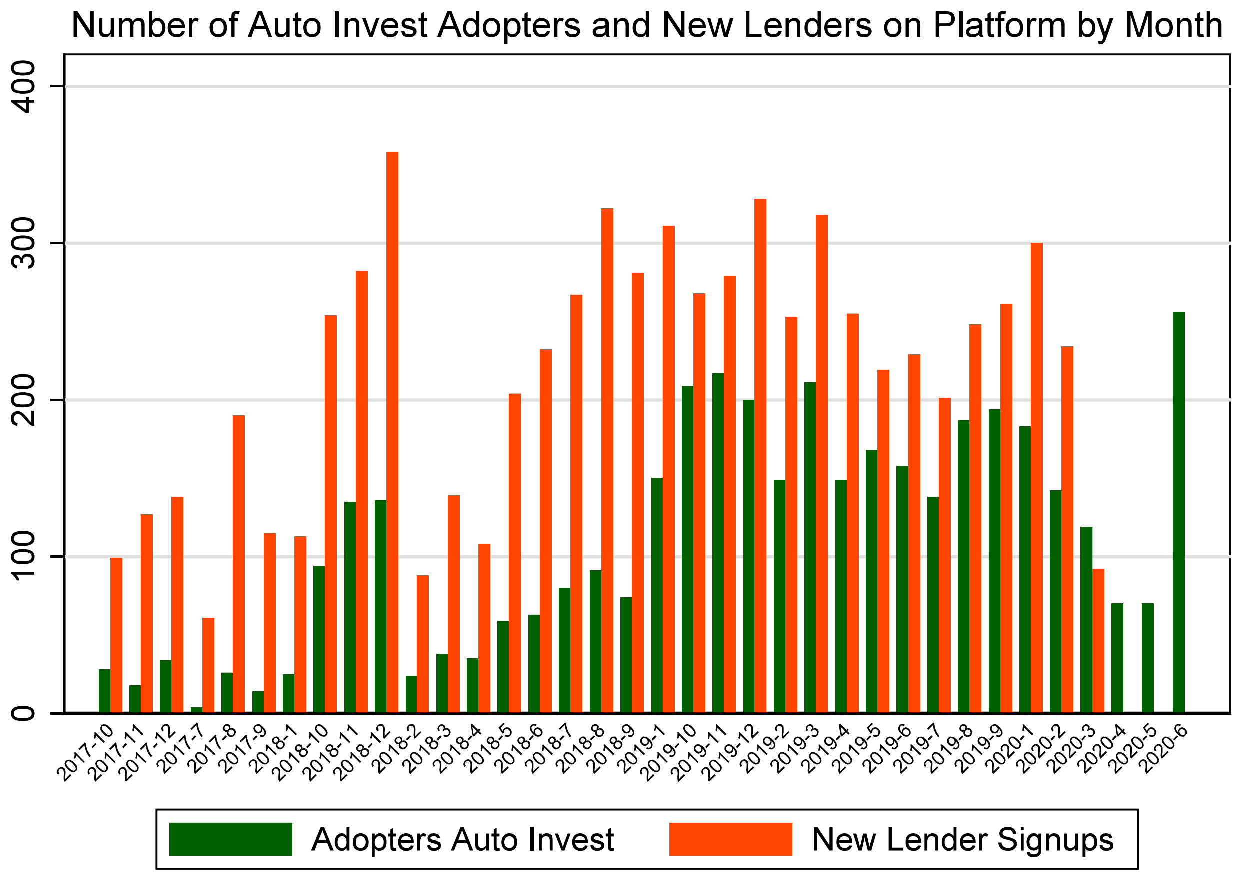


**Figure A.5: Geographic Distribution of Lending:
Number of Indian States in which Each Lender Disburses Funds**

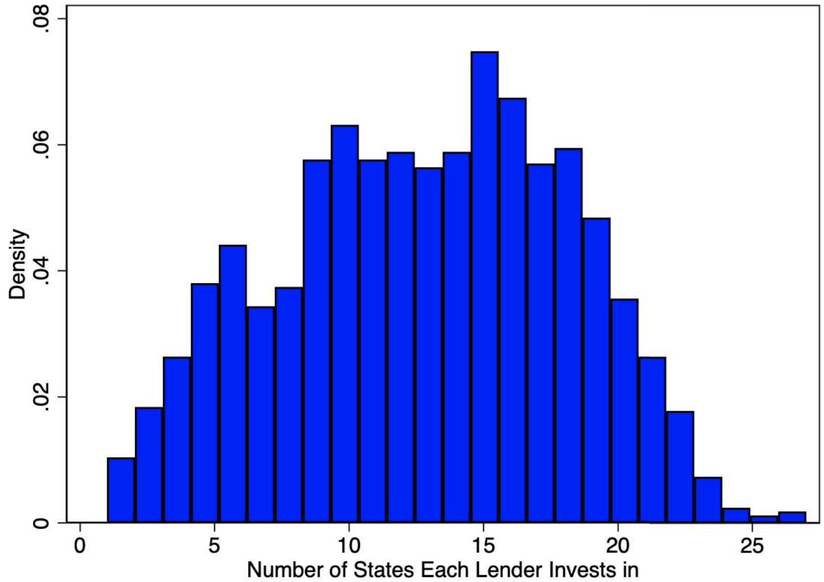


Figure A.6: Ex-post Likelihood of Default and Interest Rates

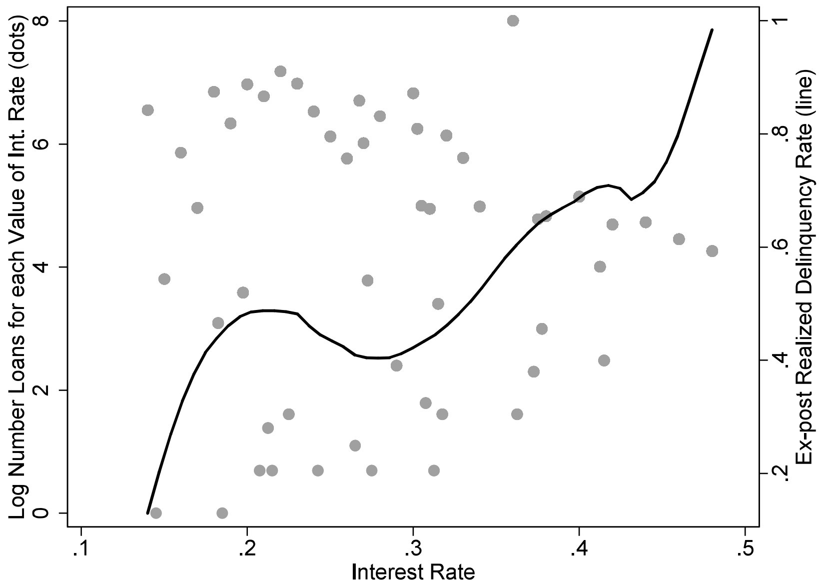


Figure A.7: Distribution of Probabilities that Borrowers are Shudra

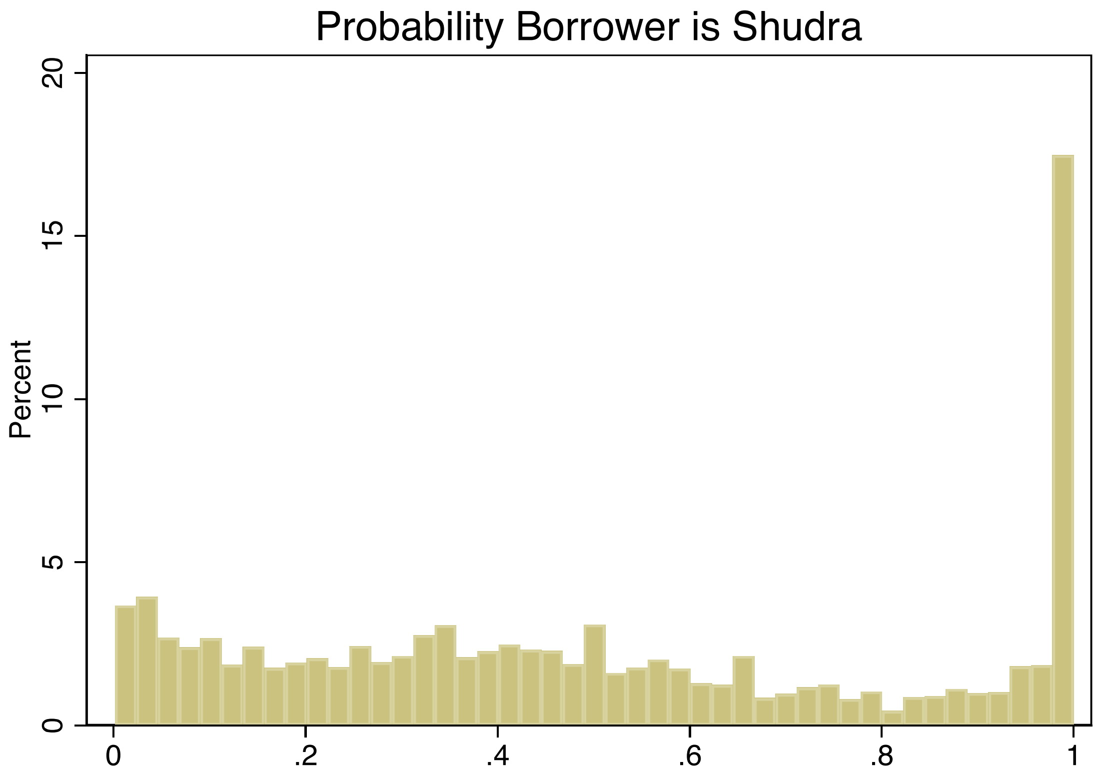


Figure A.8: Intensive Margin Performance Before and After Auto Invest: Full Sample

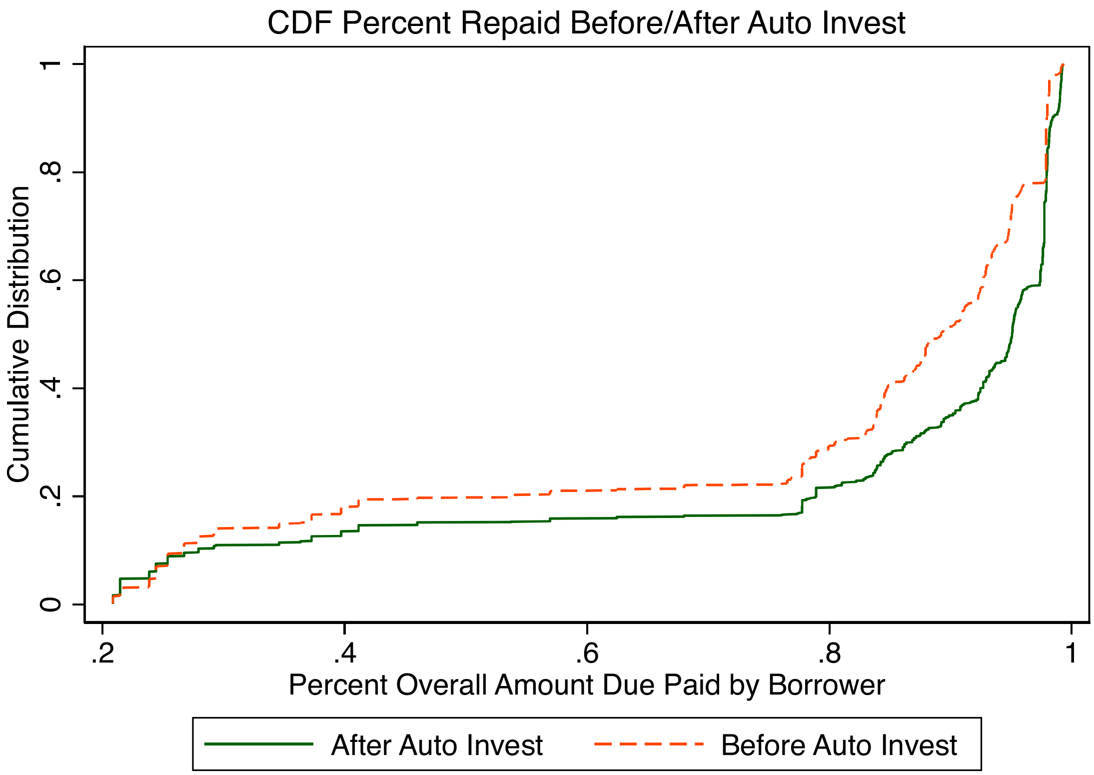


Table A.1. Determinants of Robo-Advice Adoption

Dependent variable: Ever Adopted Robo-Advising Tool	(1)	(2)	(3)	(4)
In-group Bias	-0.060	-0.056	0.007	0.007
	(-0.88)	(-0.72)	(0.02)	(0.02)
Hindu Lender			-0.094	-0.143
			(-0.30)	(-0.43)
In-group Bias× Hindu Lender			-0.076	0.015
			(-0.24)	(0.04)
Constant	0.850***	0.751***	0.950***	0.896***
	(14.01)	(6.87)	(3.13)	(2.66)
Lender-State Fixed Effects		✓		✓
Lender-Cohort Fixed Effects		✓		✓
N. obs.	1,567	1,233	1,567	1,233
R-square	0.00	0.06	0.00	0.06

Table A.1 reports the results of estimating the following specification—estimated at the level of lender j—by ordinary least squares:

$$
\begin{aligned}
Ever\ Adopted\ Robo_j = \alpha + \beta\ In-group\ Bias_j + \gamma\ Hindu\ Lender_j \\
+ \delta\ In-group\ Bias_j \times Hindu\ Lender_j + \epsilon_j
\end{aligned}
$$

This specification includes the full set of Faircent lenders rather than only the lenders who adopted the robo-advising tool at some point during our sample period, which constitutes the population of all our other empirical analyses. $Ever\ Adopted\ Robo_j$ is a dummy variable that equals 1 if lender j has ever adopted the robo-advising tool at any point during our sample period, and zero otherwise; $In-group\ Bias_j$ is the difference between the share of out-group borrowers in the overall pool and the share of that group's population in lender j's loan portfolio. For instance, for Hindu lenders, $In-group\ Bias_j = Share\ Muslim\ Borrower\ Pool - Share\ Loans\ to\ Muslim\ Borrowers$.

Table A.2. Change in Lending to Out-group Borrowers: Robustness, Statistical Inference

Dependent variable: Muslim Borrower					Low Use Auto Invest	High Use Auto Invest
	(1)	(2)	(3)	(4)	(5)	(6)
Hindu Lender × Auto Invest	0.045	0.046	0.045	0.043	0.009	0.052
By Lender	(2.51)**	(2.51)**	(2.07)**	(1.96)**	(0.23)	(1.94)*
By Lender and Borrower	(2.24)**	(2.24)**	(2.00)**	(1.90)*	(0.23)	(1.93)*
By Lender, Borrower, and Month	(2.13)**	(2.14)**	(2.51)**	(2.19)**	(0.21)	(2.71)***
By Lender Surname, Borrower Surname, and Month	(2.16)**	(2.18)**	(2.01)**	(2.03)**	(0.22)	(1.94)*
Lender Fixed Effects			✓	✓	✓	✓
Year Fixed Effects				✓	✓	✓
N. obs.	113,284	113,283	113,283	113,283	39,366	72,104

Table A.2 reports the results of estimating the following specification by ordinary least squares:

$$\begin{aligned} Muslim\ Borrower_{i,j,t} &= \alpha + \beta\ Auto\ Invest_{j,t} + \gamma\ Hindu\ Lender_j \\ &+ \delta\ Hindu\ Lender_j \times Auto\ Invest_{j,t} + \zeta\ \boldsymbol{x}_{i,t} + \eta_j + \eta_t + \epsilon_{i,j,t} \end{aligned}$$

where $Muslim\ Borrower_{i,j,t}$ is equal to 1 if borrower i who receives funding from lender j is Muslim, and zero otherwise; $Auto\ Invest_j$ is equal to 1 if the lender made the loans after activating Auto Invest and 0 otherwise; $Hindu\ Lender_j$ is equal to 1 if lender j is Hindu; and $\boldsymbol{x}_{i,t}$ is a vector of loan-level characteristics that are direct proxies for the risk profiles of the loans lenders extend to borrowers—loan maturity (measured in months), loan amount, and the annual interest rate associated with the loan. These loan-level characteristics are assigned to borrowers by the platform's algorithm when the loan requests are vetted before borrowers access the borrower pool; and η_j and η_t are full sets of lender and year fixed effects. In each line, we report the t-statistics estimated with the indicated level of clustering of standard errors.

Table A.3. Lenders' Portfolio Diversification Before and After Robo-Advising

Dependent variable:	**In-group vs. Out-group Discrimination**			**Stereotypical Discrimination**		
Diversification (*see columns*)	Stdev Loan Sizes (1)	Stdev Loan Returns (2)	Stdev Payments (3)	Stdev Loan Sizes (4)	Stdev Loan Returns (5)	Stdev Payments (6)
Auto Invest	-0.002	-0.046	0.001	-0.012***	-0.053	-0.000
	(-0.78)	(-0.71)	(0.98)	(-4.07)	(-0.62)	(-0.08)
Share Muslim Borrowers	0.006	-0.535**	0.001			
	(0.49)	(-2.49)	(0.20)			
Share Shudra Borrowers				0.004	-0.500**	0.000
				(0.54)	(-2.38)	(0.20
Constant	0.044***	0.646***	0.040***	0.041***	0.817***	0.042***
	(19.14)	(12.00)	(41.43)	(13.53)	(7.02)	(29.69)
N	2,586	399	2,707	998	292	979
R-Square	0.000	0.017	0.000	0.012	0.030	0.000

Table A.3 reports the results of estimating variations of the following specification by ordinary least squares at the lender's level:

$$Diversification\ Portfolio_{j,t} = \alpha + \gamma\ Share\ Muslim\ Borrowers_{j,t} + \delta \times Auto\ Invest_{j,t} + \epsilon_{j,t},$$

where $Diversification\ Portfolio_{j,t}$ is computed at the lender level j before and after Auto Invest (t) in three ways: the standard deviation of the sizes of individual loans in lender j's portfolio before and after Auto Invest (columns (1) and (4), the standard deviation of the returns of the loans originated in lender j's portfolio before and after Auto Invest (columns (2) and (5)), and the standard deviation of the size of the monthly payments lender j received from their loans before and after Auto Invest; $Auto\ Invest_j$ is equal to 1 if the lender made the loans after activating Auto Invest and 0 otherwise; $Share\ Muslim\ Borrowers_{j,t}$ ($Share\ Shudra\ Borrowers_{j,t}$) is the share of Muslim (Shudra) borrowers in lender j's portfolio before and after Auto Invest; In this case, because the level of observation is a lender, we cluster standard errors at the city level. All the results are similar if we cluster at the lender level, i.e. compute simply heteroskedasticity-robust standard errors.