Marketing, Information, and Access to Credit: Evidence from Credit Card Offers

Vitaly M. Bord[1] and Jessica N. Flagg[2]

[1] Federal Reserve Board

[2] Boston College, Carroll School of Management

March 26, 2026

Abstract

This paper studies whether differences in credit card marketing contribute to racial disparities in credit access. Using data on direct mail credit card offers matched to credit bureau records, we document that Black and Hispanic consumers are 2.1 percentage points less likely to receive a credit card offer and 2.6 percentage points less likely to own a credit card than observably similar White consumers, even after controlling for credit risk and financial characteristics. Instrumenting for offer receipt with geographic and temporal variation in marketing campaigns, we establish that receiving an offer causally increases the likelihood of opening a new credit card, and that differential marketing fully explains the racial ownership gap. We argue that the differences in marketing arise from informational frictions: Black and Hispanic consumers are less likely to appear on marketing lists compiled for lenders by external data providers. Supporting this mechanism, we find that (i) marketing disparities are concentrated in new customer acquisition and do not arise in cross-selling to existing customers; (ii) larger and co-branded campaigns, which rely on data from external partners, are less likely to target Black and Hispanic consumers; and (iii) conditional on being targeted, Black and Hispanic consumers receive *more* offers than White consumers—suggesting invisibility, not discrimination, drives these results. Alternative explanations such as unobserved credit risk cannot account for our results. These findings identify a previously understudied supply-side friction affecting credit access and financial inclusion.

*The views and analysis are solely those of the authors, and do not necessarily represent the Federal Reserve Board or staff. We thank Francesco D'Acunta, Simon Blanchard, Geng Li, Kevin Moore, Michael Palumbo, Alberto Rossi, Anuj Shahani, Kamila Sommer and participants in the Federal Reserve Board Applied Micro Lunch and Junior Economist Workshop for helpful comments. Cord Barnes provided excellent research assistance. All errors are our own.

1 Introduction

Access to credit is fundamental to economic well-being. Taking advantage of opportunities such as purchasing a home, investing in education, or smoothing consumption often crucially relies on the ability to obtain credit. The most ubiquitous consumer credit product is credit cards, with more than 75% of U.S. households owning at least one card in 2023. However, significant disparities exist by race: while 82% of White households own a credit card, only 60% of Black and 65% of Hispanic households do (Federal Deposit Insurance Corporation, 2023). Understanding whether these disparities reflect consumer demand, borrower risk profiles, or frictions in how credit is supplied to different consumers is essential, and has direct implications for financial inclusion policy and the efficiency of consumer credit markets.

In this paper, we examine the drivers of the racial gap in access to credit. Using comprehensive data on direct mail credit card offers matched to credit bureau records, we show that the majority of the racial gap in credit access is due to risk: after controlling for credit score and detailed financial characteristics, Black and Hispanic consumers are only 2.6 percentage points less likely to own at least one credit card relative to observably similar White consumers. This remaining gap can be attributed to frictions in the provision of credit that arise through the channel of marketing. We document a similar magnitude of racial gap in the likelihood of receiving a credit card marketing offer and show that, because consumers who receive offers are more likely to open new credit cards, the racial gap in marketing can fully explain the racial gap in credit card ownership.

Why does this difference in credit card marketing by race arise? Because credit card lending is overwhelmingly automated and processed online, lenders generally do not observe potential borrowers' race, making purposeful discrimination unlikely. Instead, we provide evidence that the gap arises from information frictions. Lenders rely on external data providers——marketing firms, co-brand partners, and credit bureaus——to create lists of consumers to target with offers based on the extensive amounts of data these providers compile.

The information frictions occur because accurate information about Black and Hispanic consumers is more difficult to obtain. They are less likely to respond to financial surveys; their credit reports are sparser (Blattner and Nelson, 2024); and both their credit reports and the profiles maintained by data providers are more likely to contain erroneous information (Consumer Financial Protection Bureau, 2021; Neumann et al., 2024). In addition, algorithms trained primarily on White consumers may produce less predic-

tive estimates for Black and Hispanic consumers if the underlying characteristics differ substantially across groups (Fuster et al., 2022). Greater data sparseness and lower data quality may lead to noisier predictions (Blattner and Nelson, 2024) or exclusion from algorithmic processing altogether (Tucker, 2023). Consequently, information about Black and Hispanic consumers may be missing more often or less accurate, reducing their inclusion on marketing lists. Although we do not have direct data on the marketing lists, we show that the difference in marketing we identify is consistent with this information friction and inconsistent with alternative explanations such as unobservable risk differences, geographic redlining, or explicit discrimination.

Our data on credit card marketing come from Mintel Comperemedia's Credit Cards and Mortgage & Loan Databases, which tracks all financial and nonfinancial product offers received by mail by a monthly random sample of respondents. Credit card offer data are a good proxy for credit supply (Han et al., 2017), correlating well with other credit supply indicators and lenders' reported changes in lending standards (Driscoll et al., 2025). Despite the growth of online and third-party marketing channels, direct mail remains an important source of credit card applications: approximately 20% of applied-for offers were received by mail in 2023 (Federal Reserve Board of Governors, 2024).

We focus on differences by race for two reasons. First, after controlling for risk and other credit-relevant factors, race should not affect marketing or credit access, as it is not directly tied to lender profitability. This makes race an ideal dimension for identifying frictions in credit supply and marketing. By contrast, other dimensions such as income, age, or financial sophistication may directly affect access to credit through consumers' ability to repay debt and propensity to use products in ways that generate lender revenue. Second, understanding the sources of large racial disparities in credit access is essential for designing policies that promote equal credit opportunity. While an extensive literature examines racial disparities in mortgage lending, whether such differences exist in credit card access—and why—remains understudied.[1]

While we focus on direct mail marketing, the information frictions we identify likely also extend to digital channels, which have grown substantially in recent years. Insofar as lenders rely on external data providers for online marketing, the same frictions may occur. Moreover, direct mail remains important, and lenders devote a great deal of resources to this marketing channel, mailing more than 3.6 billion credit card offers in 2023. Direct mail may be particularly important for Black and Hispanic adults, who are less likely to

[1]See Bartlett et al. (2022); Bhutta et al. (2025); Munnell et al. (1996), among others, for an overview of the literature on differences by race in mortgage approvals.

have laptop or broadband internet access (Atske and Perrin, 2021; Office of the Assistant Secretary for Planning and Evaluation, 2021)[2].

Our paper yields four primary sets of results. First, we show that the likelihood of both owning a credit card and receiving a credit card offer vary with race. Controlling for credit risk and other financial characteristics explains about two-thirds of the unconditional differences. The residual, unexplained difference is small but meaningful: Black, Hispanic, and Other (BHO) respondents are approximately 2.6 percentage points less likely to own a credit card and 2.1 percentage points less likely to receive a credit card offer than similar White respondents. These findings are robust to alternative samples and specifications and, importantly, are not driven by differences in credit risk between White and BHO respondents. For marketing, our results are not driven by particular subsets of credit card offers or lenders.

Second, we show that credit card marketing causally affects credit card ownership. Using geographic and temporal variation in credit card campaigns as an instrument for offer receipt, we establish that the racial gap in credit card ownership can be fully explained by differences in marketing. Respondents who receive an offer are more likely to obtain a new credit card in the following 4 months. Moreover, receiving an offer does not increase subsequent delinquency rates, suggesting that, in our setting, credit access does not lead to excessive debt.

Our instrument is the log number of credit card campaigns in the month and county. The validity of the instrument relies on the fact that lenders do not send all campaign offers simultaneously, creating geographic and temporal variation in offer receipt that is uncorrelated with borrower characteristics but predictive of receiving an offer.

Third, we find that when BHO respondents do receive credit card offers, they receive worse offers, with higher interest rates, lower credit limits, and fewer rewards. These differences reflect the types of campaigns targeting BHO respondents—they are less likely to receive co-branded card offers and more likely to receive non-targeted, unaddressed offers.

Fourth, we examine potential explanations for the racial marketing gap and find evidence consistent with an information channel: frictions in the spread of information about BHO respondents reduce their likelihood of appearing on marketing lists.[3] Consistent with the existence of larger information frictions for BHO respondents, we show that

[2]At the same time, smartphone access rates are similar across racial groups (Atske and Perrin, 2021)

[3]As we discuss further in Section 4, these lists may be compiled by either the lenders, external data providers, co-branded partners, or credit bureaus.

Black and Hispanic consumers are less likely to respond to financial surveys, more likely to have accounts at small banks, credit unions and nonbanks, which may not always report to credit bureaus, and are more likely to have disputes of erroneous information on their credit reports.

The information channel generates several testable implications, all of which we find evidence for. The racial gap appears only in new customer acquisition campaigns, where lenders rely on external data providers, but not in cross-selling to existing customers, where lenders use internal information. Co-branded campaigns and larger national campaigns, which engage external partners more heavily, show larger racial gaps . Finally, conditional on receiving at least one offer, BHO respondents receive *more* offers than White respondents—a pattern consistent with under-representation in marketing databases rather than discrimination or risk-based targeting.

We also test and reject several alternative explanations. We find no evidence that lenders treat BHO respondents as unobservably riskier. When lenders experience higher delinquency rates, they reduce offers to borrowers with lower credit scores and those with observable risk indicators but not to BHO respondents. We also find no evidence of geographic redlining, as the racial gap persists within narrowly defined geographic areas and does not vary with neighborhood racial composition; or of explicit discrimination against racial minorities.

Our findings have implications for policy and the broader provision of credit. First, policies that rely on market forces to close the racial marketing gap by encouraging competition among lenders are unlikely to succeed because they do not address the underlying information frictions. Policies that reduce the cost of information acquistion, especially about BHO consumers, may succeed but at the cost of consumer privacy. Conversely, by increasing firms' costs of information acquisition, privacy regulations may harm underserved populations who are already underrepresented in marketing databases (Dubé et al., 2025; Goldfarb and Tucker, 2012). Consistent with this, we find that the California Consumer Privacy Act of 2018 exacerbated the racial marketing gap.

Second, our findings demonstrate that the channel through which credit supply meets demand can matter for market outcomes. In credit card markets, information frictions arise due to the reliance on external partners for marketing, generating disparities in access and pricing prior to underwriting. Other credit channels may produce different frictions.

Third, these information frictions are more likely in settings where automation

and hard information predominate. Data providers and credit bureaus rely exclusively on hard information to construct marketing lists, because this allows for the use of algorithms and the automation of credit card targeting (Liberti and Petersen, 2018).[4] Consequently, borrowers with limited hard information are disadvantaged. By contrast, in settings with loan officer discretion, investment in soft information can mitigate information frictions (Fisman et al., 2017; Frame et al., 2025).[5]

Our findings are particularly relevant given the growth of FinTech lenders that rely extensively on algorithmic underwriting. Consistent with evidence that FinTechs do not overcome racial disparities in mortgage lending (Bartlett et al., 2022) and charge higher risk premia to borrowers with limited information (Di Maggio and Yao, 2021), our results suggest FinTechs do not eliminate information-based disparities and may, in some cases, amplify them. As financial intermediation becomes more automated and data-driven, these information frictions may grow increasingly consequential.

Related literature

While prior work has documented racial and ethnic differences in credit card marketing exposure, we are the first to directly link marketing to credit card ownership and identify the underlying channel through which racial marketing gaps arise. Firestone (2014) previously showed that Black and Hispanic consumers receive fewer credit card offers using data from five large lenders during 2009-2010. Though our estimates of marketing exposure differ somewhat in magnitude, we show in Appendix A.5 that these differences reflect estimation choices and sample construction rather than differences in findings. Moreover, we are the first paper to establish a causal link between marketing and credit card ownership, demonstrating that racial disparities in card ownership can be explained by differential marketing exposure. We are also the first to provide evidence on the information channel behind the racial gap in marketing.

More broadly, our paper contributes to several strands of literature. First, there is a vast literature documenting differences in credit access by race and ethnicity in the mortgage market (Bartlett et al., 2022; Bhutta et al., 2025; Munnell et al., 1996); the small-business lending market (Blanchflower et al., 2003; Chernenko and Scharfstein, 2024;

[4]Data providers collect behavioral information, including context-dependent soft information. However, because soft information cannot be credibly transmitted (Stein, 2002), providers quantify or "harden" this information to make it transmissible and usable in algorithms, resulting in information loss (Liberti and Petersen, 2018).

[5]At the same time, discretion combined with cultural biases can result in inefficient provision of credit and worse outcomes D'Acunto et al. (2026).

Chernenko et al., 2023; Howell et al., 2024) and consumer loan market (Butler et al., 2023; Cohen-Cole, 2008; Firestone, 2014; Goodstein et al., 2018). Consistent with evidence from Bhutta et al. (2025) on the mortgage market, we find that observable risk explains much of the gap in credit card ownership and offer receipt, with a small but meaningful residual. Related to Chernenko et al. (2023), who show that application propensity explains 55% of the racial differences in the Paycheck Protection Program take-up, we show that marketing, by encouraging applications and leading to new card originations, can close the racial gap in credit access.

A related literature also finds racial differences in credit quality (Begley and Purnanandam, 2021) and pricing (Bhutta and Hizmo, 2021; Butler et al., 2023; Zhang and Willen, 2020). Complementing recent work examining whether minority mortgage borrowers face different rate menus (Bhutta and Hizmo, 2021; Zhang and Willen, 2020), we find that consumers of different race and ethnicity tend to receive different—and worse—credit card offers, with higher rates, lower limits, and fewer rewards.

Second, we contribute to the literature on information in consumer lending markets. Prior work examines how information improvements about credit risk——through better technology, credit scoring, or credit reporting——can increase access to credit by reducing information asymmetries (Livshits et al., 2016; Narajabad, 2012; Pagano and Jappelli, 1993, among others). Conversely, lenders may withhold some borrower information to prevent competitors from targeting their profitable customers (Guttman-Kenney and Shahidinejad, 2025). These papers examine information propagation about borrower *risk*. We highlight that information about borrowers' *existence*—not just risk—matters for credit access.

Third, we contribute to the literature on the consequences of the use of algorithms in consumer credit markets. While algorithms increase the speed and ease of credit access (e.g. Fuster et al., 2019), they can perpetuate or amplify underlying disparities (Fuster et al., 2022). Bartlett et al. (2022) document persistent racial disparities among FinTech lenders, showing that automation does not eliminate such gaps. Bakker et al. (2025) identify algorithmic biases in credit scores affecting racial disparities.[6] Moreover, Blattner and Nelson (2024) show that credit scores of racial and ethnic minorities are less predictive of default due to data sparsity, and Tucker (2023) argues that data sparsity may cause "algorithmic exclusion"—complete exclusion from algorithmic processing that further ex-

[6]Bakker et al. (2025) show credit scores exhibit calibration bias (conditional on score, Black borrowers default more than White borrowers) and balance bias (Black borrowers have lower scores than White borrowers with identical default outcomes). Because credit cards are algorithmically underwritten using credit scores and other *past* information, only balance bias affects credit card underwriting.

acerbates racial gaps.[7] We show that algorithmic information frictions drive racial gaps in credit card marketing and ownership. We also demonstrate that these frictions can operate prior to underwriting and thereby shape both credit access and market outcomes.

Finally, we contribute to the literature on advertising in financial markets. Due to data constraints, most studies examine marketing content without linking to outcomes (Anderson and Renault, 2006; Firestone, 2014; Mullainathan et al., 2008; Ru and Shoar, 2016). Exceptions include Grundl and Kim (2019), who examine how mortgage refinancing advertisements affect subsequent behavior; Bertrand et al. (2010), who show in a field experiment that advertising content affects demand; and Célérier and Tak (2025), who use newspaper ads and depositor records to demonstrate that increased advertising in African American newspapers raised deposit participation among Black consumers. We contribute by establishing that receiving credit card offers causally increases new card originations.

The remainder of the paper proceeds as follows. Section 2 provides institutional background on credit card marketing and describes our data. Section 3 presents our main results on racial disparities in credit access and the role of marketing. Section 4 tests potential explanations, providing evidence for the information channel. Section 5 discusses implications for policy and financial inclusion. Section 6 concludes.

2 Institutional Background and Data on the Marketing of Financial Products

In this section, we provide an overview of the marketing of financial products, before turning to a description of our dataset on mailed offers.

2.1 Marketing of consumer financial products

Why do financial institutions market their products? Under perfect information, borrowers have knowledge about all products offered by all lenders and choose those that maximize their utility. However, borrowers do not have perfect information and do not invest in obtaining that information. Most mortgage borrowers do not shop around for rates and would be better off if they shopped around more (Bhutta et al., Forthcoming). In the credit card market, 34% of cardholders classify themselves as non-shoppers who do no actively look for better offers, and only 36% consider themselves high or super shoppers

[7] The difficulty of overcoming data sparsity compounds these issues: lenders extend more credit to borrowers with existing loans, using other lenders' accounts as signals of private information (Kovrijnykh et al., 2023).

(Stango and Zinman, 2016). Even among the latter group, shopping might mean passively reviewing marketing offers sent to them rather than actively researching alternatives.

These patterns underscore the importance of direct mailings and similar outreach efforts as many consumers are unlikely to seek out better terms on their own. Over 60% of credit card applications result from offers received by mail, email, phone, or in person (Mintel Comperemedia).

Thus, data on the marketing of financial products serves two functions in this paper. First, it measures credit supply: marketing offers reveal which borrowers lenders are willing to extend credit to, how much credit, and at what price, as in Han et al. (2017). Second, it is a channel through which supply meets demand. Because consumers rarely look for better alternatives, receiving offers affects both application decisions and the extension of credit.

Financial institutions send offers for all types of credit products——credit cards, mortgages, home equity loans, personal loans——but we focus on credit cards for several reasons. First, credit cards account for the majority of credit offers, comprising over 60% of offers in our dataset. Second, the process of selecting applicants for offers is most aligned with the approval process for credit cards. Credit card approvals are largely automated, relying on self-reported income, employment, and housing status, as well as credit scores and other credit report information. Other financial products such as mortgages and home equity loans require more information and manual review.[8]

Credit card lenders use multiple marketing channels. We focus on direct mail offers to potential borrowers, which comprised of 3.6 billion offers in 2023, accounting for 20% of applied-for offers (Mintel Comperemedia).[9]

How do lenders target consumers for credit card offers?

In this subsection, we discuss the channels through which lenders receive information about potential customers whom they target with credit card offers.

Lenders obtain lists of potential borrowers from several sources. First, they mar-

[8]The extra information obtained during the underwriting of other credit products may reflect measures of risk unobserved in our data. Thus, for the other products, it is possible that differences in who receives offers do not reflect difference in who is able to obtain credit.

[9]Other channels include digital solicitations sent by email, social media, or through lender websites/apps (one-third of applied-for offers); in-branch cross-selling to existing customers (15% of applied-for offers); and other channels, including third-party comparison sites and affiliate marketing such as airline miles blogs (20% of applied-for offers). Digital channels and affiliate marketing have grown in recent years. See Federal Reserve Board of Governors (2024) and the Consumer Financial Protection Bureau (2023) for additional details.

ket to existing account holders. This reduces information asymmetries, as lenders have more information about these customers' risk, profitability, and product usage. Acquiring accounts through existing customers may also be cheaper than acquiring new customers.[10]

Second, for co-branded credit cards that earn rewards tied to airline miles, hotel points, or specific retail stores, lenders rely on the co-brand partner organization to provide member lists, since members are more likely to open co-branded cards. Third, lenders rely on credit bureaus, particularly for pre-selected offers sent to individuals based on their credit scores.[11]

Fourth, lenders use marketing data providers that specialize in identifying potential customers. Since lenders screen on credit risk using credit scores, they seek lists of consumers likely to apply if offered a card and likely to use it if approved. Data providers such as Acxiom, Data Axle, and Melissa Data compile large swathes of data, use proprietary algorithms to estimate expected behavior, and then provide this information to credit card lenders, as well as other firms. Data they provide include demographic and income information, homeowner/renter status, expected purchasing behavior, and even estimated responsiveness to offers and expected usage, among other information.

Appendix B contains excerpts from a sample customer report showing the information data providers compile. Fields include personal identifiers (name, email, IP address, physical address history); personal characteristics (gender, birth date, marital status, income); financial information (credit card and bank account ownership, credit score range); and education and employment. Data providers also use proprietary algorithms estimate buying activity across product categories (for example, gourmet, furnishings, travel) and provide "inferences" about characteristics ranging from body mass index to discretionary spending allocation. Relevant for our paper, they estimate responsiveness to financial product solicitations such as credit card or mortgage offers.

The use of external partners—loyalty programs, credit bureaus, and marketing data providers—by credit card lenders raises the possibility that some consumers may be omitted from marketing lists for reasons unrelated to risk or profitability. In this paper, we focus on race as a characteristic that, controlling for risk, should not affect offer receipt under perfect information. However, racial minorities may be less likely to receive offers for two reasons. First, they may be underrepresented on lists that data vendors provide to lenders because

[10]Basten and Juelsrud (2023) show that deposit account holders are 20 percentage points more likely to subsequently take out a loan from the lender over the following 14 years.

[11]These are also known as "pre-approved" or "pre-screened" offers. Different lenders use different terminology.

information about racial minorities may be harder to obtain (see Section 4). Second, data providers sell estimated rather than actual data. Prior literature has established that algorithms may perform worse for historically underserved groups (Fuster et al., 2022) and consumers with sparser or lower quality data (Tucker, 2023). If estimated data quality for racial minorities is lower, data providers may be unwilling to sell it, excluding these groups from customer lists.

2.2 Data

We next turn to an overview of the data sources we use.

2.2.1 Data sources

Our main data source is the Mintel Comperemedia Credit Cards and Mortgage & Loan Databases, hereafter "Mintel." Mintel maintains a pool of 22,000 customers, sampling approximately 8,000-9,000 monthly, with around 2,700 participating by collecting and digitizing all received mailings. Mailings include offers for new financial products (credit cards, bank accounts, insurance, mortgages, loans), notices and statements for existing products, and non-financial advertisements. The pool is balanced on region, income, age, and household size to be representative of the U.S. Census. For more information on the Mintel data collection process, see Han et al. (2017).

Mintel data are a useful proxy for credit supply (Han et al., 2011). Unlike approval-based measures, offer data are not conditional on applications and thus include discouraged borrowers. Han et al. (2017) show that credit card offers track well with other credit supply measures such as new accounts.

Mintel contains three types of variables. First, Mintel collects demographic survey data, including the head of household's age, race, education and employment status, household composition, income, and home-ownership status.[12] Mintel also provides geographic information including county and zip code for nearly all respondents and census tract for most. Second, Mintel records characteristics from digitized offers: sector (credit cards, insurance, mortgage), mailing type (statement, new offer, notice), whether the recipient is a new or existing customer, incentives, and lender name. For credit cards, the data include terms such as annual fees, promotional interest rates, balance transfer fees, minimum and maximum interest rates, and other features.

[12]Some variables including education and employment are reported for both the male and female head of household, if both exist.

Each credit card offer is part of a campaign. Credit card campaigns are lender marketing initiatives for specific credit card products. Campaigns are generally rolled out over multiple months and geographic areas but have the same or similar defined characteristics: interest rates, credit limits, fees, and rewards. Thus all consumers who are targeted for the campaign and receive a credit card offer as part of the campaign receive same or similar offer terms.

Third, each household member's data are subsequently linked to a credit bureau, and information from the consumer's credit report is included as of two months prior. Credit history information includes key measures of a consumer's credit status, such as the type and amount of debt the consumer holds, delinquency status, and the number of recent credit inquiries. It also includes the VantageScore 1.0 credit risk score model—referred to as credit score hereafter—a credit scoring model used to assess borrower's financial responsibility. It ranges from 501 to 990. It is important to note that credit report and mailing information are individual-specific, while the demographic survey data, including race, reflect the head of household.

Our sample spans 2007 to 2023, as credit scores are available starting in 2007. We exclude observations without credit report matches (28% of the full Mintel sample). The match rate is marginally lower for Black, Hispanic, or Other respondents (70%) than for White respondents (73%). We further exclude individuals in very rural counties (fewer than 25 individuals surveyed over the 17-year period) and individuals younger than 20 or older than 75.[13]

Our final sample contains 558,813 individual-month pairs—our main level of analysis — reflecting 309,078 respondents in 138,698 households. The dataset is an unbalanced panel, with almost 60% of observations corresponding to respondents who are observed multiple times. As we discuss in Section 3.3, the panel structure allows us to track part of the sample over time to examine changes in credit report variables.[14]

We supplement the Mintel data with several data sources. First, from Call Reports, we obtain bank balance sheet information including asset size and 30- and 90-day delinquency rates. We matched Mintel lender names to Call Report rssd ids using fuzzy matching.[15] We matched 350 of 1,150 lenders (30%) sending credit card offers, accounting

[13]These restrictions do not meaningfully affect our results.

[14]The Mintel dataset provides a household identifier that is generally consistent over time, with some exceptions. Using individual credit report information such as birth date and census tract, we are able to create a time-consistent individual identifier.

[15]We used the fedmatch R package and verified matches using geographic matching using bank branch data, Levenshtein distance comparisons on names, and manual review. We excluded credit unions due to difficulty comparing non-profit, mutually owned institutions with other financial institutions.

for 94% of the one million credit card offers.

Second, we obtain census tract demographics from the Census and census tract low and moderate income (LMI) status from the Federal Financial Institutions Examination Council (FFIEC) data related to the Community Reinvestment Act (CRA). Finally, we obtain racial animus data by designated market area from the website of Seth Stephens-Davidowitz (Stephens-Davidowitz, 2014).

2.2.2 Summary statistics

Table 1 presents summary statistics for our sample. Panel A shows self-reported demographic information by race for the head of household. Several points stand out. First, White respondents generally report higher income than Black and Hispanic respondents. While only 6% of White respondents have income below $15,000, 16% of Black and 8% of Hispanic respondents report this income. Differences exist across most income bins and other characteristics. For example, 66% of White respondents live in married or domestic partnership households, compared to 38% of Black and 62% of Hispanic respondents.

Second, Asian respondents' financial characteristics more closely resemble White respondents and exceed them for some variables. For example, the share of Asian respondents in each income bin is lower than White respondents for bins below $100,000 and higher for bins above $100,000.

Third, 85% of respondents report their race as White. Han et al. (2017) similarly found 85% White respondents in their 2007-2014 sample, compared to 80% in a comparable Survey of Consumer Finances (SCF) sample. This likely reflects that our Mintel sample requires credit report matches, and racial minorities are more likely to lack credit reports (Brevoort et al., 2016).[16]

Panel B presents credit report variables by race. White respondents generally have better financial indicators than Black or Hispanic respondents. For example, White respondents have credit scores approximately 100 points higher than Black respondents and 60 points higher than Hispanic respondents. As before, Asian respondents have financial health measures as high or higher than White respondents. Finally, White and Asian respondents are more likely to receive mailed credit card offers: 46% of White and 48% of Asian respondents receive at least one offer monthly on average, compared to 37% of Black and 44% of Hispanic respondents.

[16]The age restrictions we put in place also partly explains why the share of White respondents is so high. The share of individuals younger than 20 who are White is lower.

3 Credit Card Ownership and Marketing

We establish and quantify gaps in both credit card ownership and credit card marketing by race. We show that, on the extensive margin, the differences in ownership can be explained by differences in marketing. In addition, when racial minorities receive credit card offers, they are generally targeted with offers with lower limits, higher interest rates, and less rewards.

3.1 Credit card ownership

To examine whether credit card ownership differs by race, we estimate an OLS regression of the form:

$$I\{CC_{i,t}\} = \beta Race_i + \gamma Controls_{it} + \lambda_t + \mu_c + \eta_g + \epsilon_{it} \tag{1}$$

where $I\{CC_{i,t}\}$ is an indicator for whether respondent i owned at least one credit card at time t.

Throughout most of the paper, we focus on the extensive margin of credit card ownership. The first credit card provides a clear measure of credit access with well-defined benefits. Additional cards do not necessarily improve financial well-being, as greater credit access can lead to higher debt burdens (Bord, Kovacs, and Moran, 2026). For this reason, our main analysis emphasizes the extensive margin, while the intensive margin is discussed separately in Section 4.

$Race_i$ are indicators equal to one for households reporting their race as Black, Hispanic, Asian, or Other. $Controls_{it}$ includes household characteristics reported to Mintel and financial health measures from matched credit bureau data (Table 1). We include month fixed effects (λ_t) and county fixed effects (μ_c). Given the large demographic differences identified in Table 1, our preferred specifications include granular age, household composition, credit score, and income fixed effects. Standard errors are clustered at the county level.

Table 2 presents the results. Specification (1) includes only month fixed effects. Controlling for only the date, Black households are approximately 18 percentage points less likely to have a credit card than White respondents, while Hispanic and Other respondents are 6 and 10 percentage points less likely, respectively. By contrast, Asian respondents are about 5 percentage points more likely to own a credit card. These magnitudes are slightly smaller than the FDIC Unbanked/Underbanked survey findings of 22, 17, and 4 percentage

points for Black, Hispanic, and Asian respondents, respectively, consistent with our sample being limited to respondents with credit reports.

Specification (2) includes indicators for housing status (renter, owner, single-family home, mobile home; omitted category is other), household employment status (full-time, part-time, unemployed; omitted category is retired), age and household composition fixed effects (same bins as in Table 1), and county fixed effects. These controls reduce the coefficients on the race indicators somewhat.

Specification (3) adds granular income and credit score bins. Income bins match Table 1; credit score bins are 50-point bins between 500 and 600, 20-point bins between 600 and 700 and 10 point bins between 700 and 990. We also add a battery of financial controls: log total debt, log number of trades, and indicators for mortgage, retail credit, personal loan, installment loan and recent credit inquiry in the past 6 months; indicators for whether the credit report includes a personal bankruptcy, derogatory public records or a 90+ day delinquency; and measures of recent usage: indicators for 30+ day delinquency in the past 2 years, for a new file with fewer than 5 years of history, and for a recent file updated in the past year.

These controls generally have expected signs. Controlling for homeownership, respondents with mortgages or installment loans are less likely to own credit cards, while those with retail debt are more likely to. This likely reflects lenders targeting consumers with unsecured debt who may revolve balances. Similarly, the total debt increases the probability of owning a credit card. Derogatory records, recent delinquency, and new files decrease credit card ownership. Individuals with recently updated credit files are significantly more likely to own credit cards, indicating that recent credit information improves credit access.

Controlling for risk and other financial characteristics, reduces the coefficients on race by over two-thirds. The magnitude of the coefficients decline to -4.2 percentage points for Black respondents, -1.0 for Hispanic, and -1.6 for Other, while the coefficient on Asian is reduced to 2.2 percentage points. This is consistent with Bhutta et al. (2025), who similarly find that observable characteristics explain most of the difference in mortgage approval rates between White and minority households.

A potential concern is that our findings are driven by differences in the distributions of financial characteristics documented in Table 1. To address this, specification (4) limits the sample to a propensity-score matched sample of similar White, Black, Hispanic, Asian, and Other respondents (see Appendix A.1). Results are similar, and the coefficient on

Asian is no longer significant.

Although coefficients on Black, Hispanic, and Other vary across specifications (3) and (4), differences are minor compared to specification (1). For the rest of the paper, we group Black, Hispanic, and Other respondents together as BHO. Because Asian respondents are as likely or more likely to own credit cards as White respondents, we group Asian and White respondents together as the omitted category.

Specification (5) shows that BHO respondents are approximately 2.6 percentage points less likely to own a credit card than similar White and Asian respondents. While small relative to the 76% baseline credit card ownership rate, this coefficient is economically meaningful. The magnitude is comparable to having a recent credit inquiry or personal bankruptcy history, and exceeds coefficients on homeownership and employment status.

We next investigate how differences in credit card ownership vary by income, credit score, and other financial characteristics.

Heterogeneity by income, credit score, and other characteristics

Credit card ownership differences by race vary across income, credit score, and other characteristics. Panel A of Figure 1 shows interactions between race and income bins. Black respondents are less likely than White respondents to own credit cards across the income distribution, though the difference is not statistically significant above $100,000. For Hispanic respondents, differences concentrate in the middle of the income distribution. Asian respondents are more likely than White respondents to own credit cards in the $60,000-$200,000 range.

Panel B shows similar patterns by credit score. Black respondents are less likely to own credit cards across the credit score distribution, while for Hispanic respondents, the effect concentrates among those with scores of 850-950.

Panel C shows patterns by consumer type. Black respondents are less likely to own credit cards among owners, renters, risky consumers, non-risky consumers, and those with both new and older credit files.[17] Asian respondents are more likely to own credit cards across all categories. For Hispanic respondents, effects appear mainly among non-risky consumers and those with older files.

[17]Risky consumers are defined as having utilization above 90%, bankruptcy, derogatory records, having ever had a 90+ day delinquency, or a 30+ day delinquency in the past 24 months. New credit files are those created in the previous 5 years. Older files are older than 5 years.

Robustness

In Panel A of Table A2, we show that our findings are not driven by our sample construction. Our results are robust to: using the Han et al. (2017) sample which excludes respondents over age 65 and those with incomes below $10,000 or above $200,000 (specification (1)); excluding rural counties (specification (2)); and using respondents observed again in the data between 3 and 6 months later, the same sample used in Section 3.3 (specification (3)); and using the Mintel-provided demographic weights to weight respondents (specification (4)).

3.2 Credit card marketing

Differences in credit card ownership can arise for various reasons, including credit supply—lenders not offering credit cards to BHO respondents at the same rate as White respondents. In this section, we examine this potential reason, using credit card offers to measure credit supply, following Han et al. (2011). Consistent with prior literature including Han et al. (2011) and Firestone (2014), we find that BHO respondents are less likely to receive credit card offers than similar White respondents.

Table 3 replicates Table 2 with an indicator for receiving at least one credit card offer as the dependent variable. Specification (1) includes month fixed effects; specification (2) adds demographic controls; specification (3) adds credit score and income fixed effects and credit report variables. Beyond the credit report variables in Table 2, we include log of the total number of trades and indicators for having at least one active credit card and for high utilization above 90 percent. Specification (4) restricts to the propensity-score matched sample.

As in Table 2, controls and fixed effects substantially reduce the coefficients, implying that different risk profiles explain much of the racial gap in marketing. Unconditionally, Black respondents are 9.1 percentage points less likely than White respondents to receive offers; this difference falls to 2.2 percentage points with controls. Similarly, coefficients on Hispanic and Other decrease by nearly half. Unconditionally, Asian respondents are more likely than White respondents to receive offers, but this is largely driven by financial characteristics; the coefficient is not statistically significant after including controls.

Specification (5) estimates a 2.1 percentage point coefficient on BHO, slightly smaller than but similar in magnitude to the coefficient in Table 2. It is also similar to the coefficient on having a bankcard, an important driver of credit history and credit eligibility.

As in Table 2, the coefficient on recent credit file activity is large: respondents whose files were updated within the past year are more likely to receive offers. Respondents with new files are also more likely to receive offers, consistent with more active or information-rich credit histories being targeted.

Heterogeneity by income, credit score, and other characteristics

As with credit card ownership, credit card offer receipt by race varies across income and credit score. Panel A of Figure 2 shows interactions between race and income bins. The difference between Black and White respondents appears primarily for incomes above $40,000. Differences between Hispanic and White and between Other and White respondents are also larger at higher income bins. Panel B shows that Black respondents are less likely to receive credit card offers across the credit score distribution, while for Hispanic and Other respondents, the effect concentrates among those with scores of 650-850. Panel C shows that Black respondents are less likely to receive offers across all consumer categories: owners, renters, risky consumers, non-risky consumers, and those with both new and older credit files. For Hispanic and Other respondents, effects appear mainly among non-risky consumers and those with older files, same as with the patterns on credit card ownership.

Robustness

Our results are not driven by sample construction or particular lender or mailing types. Panel B of Table A2 replicates Panel A using offer receipt as the dependent variable, showing similar robustness.

Table A3 shows that the racial gaps in marketing appear in mailings both large and small banks (specifications 1-4) and from banks with and without nearby branches (specifications 5-6). Panel B similarly shows gaps across all offer types: pre-selected (screened on credit report information), general (same offer as available on the lender's website), general, co-branded, and non-co-branded (specifications 1-4). Results are also similar when excluding unaddressed offers and when limiting to the main household respondent who answered the demographic questions (specifications 5-6).

Finally, Table A4 shows these gaps are unlikely to be driven by differences in BHO respondents' general likelihood of receiving mail (specifications 1-2), and that BHO respondents are less likely to receive offers for business credit cards and offer bank deposit products (specifications 3-4). Detailed results appear in Appendix A.2.

Having established racial gaps in both credit card ownership and marketing, we

next examine whether differences in marketing can explain the differences in ownership.

3.3 Can marketing explain differences in credit card ownership?

Do respondents who do not receive credit card offers open fewer new credit cards? The prevalence of credit card marketing suggests yes: according to Mintel Comperemedia, more than 60% of credit card applications result from offers received by mail, email, phone, or in person. This suggests that the receipt of marketing offers affects who subsequent origination, and that the marketing gaps documented in Section 3.2 may help explain the ownership gaps in Section 3.1.

To test this hypothesis, we exploit the panel structure of our data. Almost 60% of our observations correspond to respondents observed multiple times. We limit our analysis to respondents observed at least twice, with the second observation 3-6 months after the first. Because credit report data are lagged 2 months, this means credit reports are observed 1-4 months after credit card offers are received. We denote the first observation period as t and the second as t'.

We proxy for whether the respondent opened a new credit card by whether the number of credit cards increases between t and t', *conditional* on the months since the last card opening being less than $t' - t$.[18] Our dependent variable is $I\{NewCC_{i,t'}\}$, an indicator equal to 1 if at least one card was opened. Our main variable of interest is $I\{Offer_{i,t}\}$, an indicator for whether the respondent received at least one credit card offer at time t, relative to a baseline rate of about 5%.

Specification (1) of Table 4 presents the OLS results. We include all controls and fixed effects from Table 13, plus fixed effects for the number of months between observations. The results indicate that respondents who received a credit card offer at time t were 0.5 percentage points more likely to open at least one credit card by time t', relative to a baseline of approximately 5 percent.

The main threat to a causal interpretation is that offer receipt may not be exogenous. Lenders may target households with immediate credit needs—for example, those experiencing income growth with high expenses but low savings. Conversely, lenders may also target higher-income, higher-credit-score borrowers who are lower risk but may not need credit and are less likely to apply. In both cases, the relationship between offers and subsequent card openings would reflect selection rather than causation, but in the first

[18] We define new cards this way because the change in the number of cards is a noisy proxy for new originations due to account closures.

case, OLS would overstate the causal effect, while in the second, it would understate it.

3.3.1 Marketing Campaigns as an Instrument

To address these concerns, we instrument for offer receipt with the log number of credit card campaigns in the respondent's county during the month. We use a leave-one-out methodology, counting only campaigns sent to other respondents' households.[19] As discussed in Section 2.1, campaigns are initiatives for specific credit card products.

The instrument exploits the fact that campaigns are rolled out over several months, sometimes with geographic variation in the subset of states and counties targeted each month. FFFFFF Update For example, a campaign lasting 5 months covers 17 states on average, but each month, consumers in only 5 states receive offers. Geographic targeting can also occur for other reasons. For instance, when an airline introduces more flights from a city, the co-branded credit card lender may launch a campaign with higher incentives in that city.

Our instrument thus uses variation in offer receipt from living in counties more or less exposed to marketing campaigns based on which campaigns are rolled out to the county in a given month. The exclusion restriction requires that the number of campaigns in a county-month affects whether a respondent opens a new credit card only through the channel of receiving an offer. Potential violations of the exclusion restriction could arise if campaign intensity is correlated with local credit demand or lender approval decisions. While we cannot test this directly, Panel A of Table A7 examines whether our instrument correlates with respondent characteristics. Specification (1) regresses income on the instrument and all controls and fixed effects except income bins. Specifications (2)-(4) use as dependent variables income growth between t and t', credit score growth, and an indicator for risky status. In all cases, $LogCCCampaigns$ is uncorrelated with respondent characteristics, supporting the exclusion restriction.

Specifications (2) and (3) of Table 4 present the IV results, using *Log CC Campaigns* as the instrument for $I\{Offer_{i,t}\}$ and BHO $\times$ *Log CC Campaigns* as the instrument for BHO $\times I\{Offer_{i,t}\}$. The first stage results, presented in Appendix Table A6 for brevity, show that the instruments are highly correlated with the instrumented variables. Specifications (2) and (3) show that respondents who receive a credit card offer are approximately 1.5 percentage points more likely to open a new credit card. Additionally, the interaction

[19] To minimize the influence of small campaigns on our findings, we drop all campaigns for which fewer than 3 offers are observed. This restriction does not affect our results.

term indicates that BHO respondents who receive offers are not differentially more likely to open cards than White respondents, alleviating concerns that differential underlying demand drives our results.

Next, we examine whether the difference in credit card offer receipt can explain the difference in credit card ownership documented in Table 4. Using the same sample of respondents observed at least twice, specifications (4) and (5) of Table 4 use as the dependent variable $I\text{CC}_{i,t'}$, an indicator for whether the respondent had a credit card as of t'.[20] The OLS results in specification (4) show that BHO respondents are about 2.9 percentage points less likely to own a credit card, consistent with the 2.6 percentage point gap in specification (5) of Table 2.

The IV results in specification (5) show that this effect is concentrated among respondents who do not receive an offer. The interaction between BHO and $I\{Offer_{i,t}\}$ is positive and significant, and the sum of the BHO coefficient and BHO $\times$ $I\{Offer_{i,t}\}$ is not statistically different from zero. This indicates that by t', BHO and White respondents who received an offer at time t have the same likelihood of owning at least one credit card. In other words, the racial ownership gap is entirely eliminated among respondents who receive offers.

3.3.2 Coefficient Magnitude and Selection

It is notable that the IV coefficient in specification (2) is much larger than the OLS coefficient in specification (1). This suggests that borrowers whom lenders select to receive offers are less likely to open new cards than those who receive offers for more exogenous reasons. Panel B of Table A7 in the Appendix presents direct evidence of this selection effect. Specifications (1) and (2) re-run the OLS and IV regressions using only respondents with $t' - t < 3$, that is, they are observed again less than 3 months after the first observation. Crucially, because credit report data are lagged two months, any cards opened by t' in this subsample must have been opened before or concurrent with offers received at time t. Despite this timing, the OLS results in specification (1) show a positive and significant correlation between $I\{Offer_{i,t}\}$ and $I\{CC_{i,t'}\}$. This "pre-trend" suggests that lenders disproportionately target respondents who have recently opened credit card account. Because these respondents have recently opened a new credit card, they may be less likely to open another one subsequently, explaining the smaller OLS coefficient in Table 4. How-

[20] We exclude the following credit card controls from these regressions: the indicator for having a credit card and the indicator for high credit card utilization.

ever, instrumenting with *Log CC Campaigns* allows us to purge this selection effect: the IV coefficient in specification (2) of Table A7, Panel B is much smaller and not statistically significant. Specifications (3)-(4) similarly show a correlation between $I\{Offer_{i,t}\}$ and $I\{CC_{i,t'}\}$, which can be mitigated by using the instrument.

3.3.3 Long-Run Financial Health Implications

Access to credit can have both positive and negative implications. Although owning a credit card allows borrowers to smooth consumption, it can also lead to over-borrowing and negative consequences such as default. Thus, whereas exposure to credit card offers allows some BHO respondents to obtain credit cards, the long-run implications for their financial health are ambiguous.

Specifications (1)-(3) examine the medium-term welfare effects by limiting to respondents whose credit reports are observed again 5-12 months after their credit card offers are observed.[21] In specification (1), we regress an indicator for whether the respondent is 30 days delinquent on $I\{Offer_{i,t}\}$, again instrumenting with *Log CC Campaigns*. We find no evidence that respondents who receive offers are subsequently more likely to become delinquent, and importantly, there is no differential effect for BHO respondents, whether or not they receive a credit card offer. In specification (2), our dependent variable is the change in credit score between t and t'. Whereas BHO respondents who do not receive a credit card offer see their credit score decline marginally by 6 points, those who receive a credit card offer at time t experience no effect, as the sum of BHO and BHO x $I\{Offer_{i,t}\}$ is essentially zero.

Credit cards are a particularly important financial instrument for building credit history and obtaining a credit score, and they are often a borrower's first lending product. In specification (3), we use as the dependent variable an indicator for whether the respondent has a credit score and find that respondents who receive an offer at time t are 2 percentage points more likely to have a credit score at time t'. In specifications (4)-(6), we repeat the analysis for respondents whose credit reports are observed again 13-24 months after their first observation in the data, and find qualitatively similar results[22].

Having established that on the extensive margin, BHO respondents are less likely to receive a credit card offer, we next examine whether the offers they do receive differ from those received by White respondents.

[21] That is $t' - t \in [7, 14]$, with credit report information lagged 2 months.
[22] That is $t' - t \in [15, 26]$

3.4 Credit Card Offer Terms

We next examine how terms and characteristics differ between credit card offers received by BHO and White respondents.

Table 6, Panel A, uses offered interest rate as the dependent variable in specifications (1)-(3) and minimum credit limit in specifications (4)-(6). Specifications (1) and (4) include only month fixed effects; specifications (2) and (5) add all controls and fixed effects; and specifications (3) and (6) add campaign fixed effects.

Specifications (1)-(2) and (4)-(5) show BHO respondents receive slightly higher interest rates and slightly smaller limits, even with credit report controls and all fixed effects. As in Tables 2 and 3, credit score and income controls are important: unconditional differences of 96 basis points in interest rate and $210 in minimum limit drop to 20 basis points and $63 with controls.[23] These differences are driven purely by the types of offers and campaigns received by BHO and White respondents. With campaign fixed effects, differences in interest rate and limit are precisely estimated zeros, as terms vary little within campaigns.

Panel B examines offer characteristics. BHO respondents are more likely to receive pre-selected offers targeted based on credit score (specification 1) or non-personalized, unaddressed mass-mailed offers (specification 2). BHO respondents' higher likelihood of receiving these types of offers means they are less likely to receive personalized offers such as for co-branded cards (specification 3).[24] BHO respondents' offers are also less likely to have rewards, particularly miles or points (specifications 4-6), consistent with receiving fewer co-branded cards. The lower prevalence of non-pre-selected and co-branded offers suggests BHO respondents are less likely to be targeted when external firms, such co-brand partners or marketing firms, create target lists of potential borrowers. We discuss this further in the next section.

4 Potential Channels

Having established that racial differences in credit card marketing exist, we examine the potential channels through which they arise. Our evidence supports an information chan-

[23] Because interest rate varies strongly and mechanically with credit score, we exclude credit score bins from the regressions in this table. Including credit score bin yields coefficients of 0.08 and -28 in specifications (2) and (5), respectively.

[24] Note that since these regressions include only offers received, these results do not mean that BHO respondents are more likely than a White respondent to receive a pre-selected or even non-personalized card. Table A3 in the Appendix shows that they are not.

nel: independent of their credit risk, BHO respondents are less likely to appear on the lists that data providers compile for lenders to identify potential borrowers. We reject two alternative explanations: that BHO respondents are unobservably riskier than White respondents, or that lenders engage in geographic targeting to avoid high minority areas.

4.1 Information Channel

According to the information channel, BHO respondents are less likely to receive credit card offers due to frictions in how information about them reaches lenders through external data providers. These frictions operate through three main pathways.

The first pathway is for co-branded cards, which comprise almost 20% of the offers in our sample. For these cards, lenders rely on membership lists provided by loyalty partners such as airline miles and hotel points programs. If BHO consumers are less likely to join these programs—for example, due to lower willingness to share personal information or lower participation in activities these programs reward—they will be underrepresented on these lists.

The second pathway is marketing databases. As discussed in Section 2.1, lenders often rely on external data providers to compile lists of potential borrowers. These providers aggregate information from multiple sources to predict which consumers are likely to respond to credit card offers. If BHO consumers are less willing to share their information with surveys, retailers, or other data collection efforts, they will be underrepresented in these databases.

Table 7, Panel A, provides suggestive evidence that Black and Hispanic consumers are less likely to share personal financial information, which may help explain why they are underrepresented on loyalty program rosters and marketing provider lists. The panel shows non-response rates from the 2023 FDIC National Survey of Unbanked and Underbanked Households, a survey with detailed questions about financial product ownership and usage. Non-response is measured as the share of individuals, by race, who completed the monthly Current Population Survey (CPS) but either refused to complete the FDIC's financial supplement or dropped out before finishing. Black and Hispanic respondents exhibit significantly higher non-response rates than White or Asian respondents, suggesting differential willingness to share financial information.

In addition, the data compiled by external data providers is more likely to be erroneous for BHO consumers. Neumann et al. (2024) show that the data broker information is less accurate for racial minorities, likely due to their smaller digital footprint. Inaccuracies

may be particularly prevalent in how BHO consumers will use a credit card, information that is especially important for lenders. For example, as Appendix B shows, the data vendors provide estimates of discretionary income and purchasing behavior, that credit card lenders rely on for targeting offers. [25] Surveys of payments have shown that Black and Hispanic households are much more likely to use cash, relative to White households (Greene et al., 2023). Since data on cash expenditures is more difficult to obtain, purchasing behavior of Black and Hispanic potential borrowers may be underestimated, reducing the likelihood of their inclusion in marketing lists.

The use of algorithms may further exacerbate these issues. Data providers sell estimated rather than actual consumer characteristics, such as predicted purchasing behavior or creditworthiness. The algorithms that calculate these characteristics may perform worse for racial minorities, particularly when trained primarily on majority-group data (Fuster et al., 2022). Data sparsity may further compounds this problem: Blattner and Nelson (2024) show that sparser credit reports for disadvantaged groups reduce the predictability of their outcomes. When predictions are less reliable or unable to be made, data providers and lenders may exclude these consumers from marketing lists altogether.

Similarly, Tucker (2023) argues that missing and sparse data may lead to "algorithmic exclusion", the exclusion of these consumers from algorithms. As Tucker (2023) points out, differences by race in the rates of matching consumers across databases point to the prevalence of this problem. As discussed in Section 2.2, the match rate between Mintel offer data and credit bureau data for BHO consumers is 70%, lower than the 73% match rate for White respondents.

The third way information frictions may affect the marketing is through credit reports. Lenders rely on credit bureaus particularly for pre-selected offers, which are screened on credit score and make up almost 60% of the offers in our sample. However, Black and Hispanic consumers are significantly more likely to be "credit invisible"—that is, to lack a credit record, relative to White or Asian consumers, and among those with credit records, they are more likely to lack a credit score (Consumer Financial Protection Bureau, 2015). Consumers without credit files or scores are likely to be systematically excluded from prescreened offer lists.

Even when credit reports exist, they may be sparser for BHO consumers. As shown in Table 1, Black and Hispanic consumers have fewer credit trades on their reports

[25] The data providers generally do not release the sources of their data. Purchasing behavior is most likely based on surveys, data on internet browsing behavior, and inference based on detailed geographic information.

on average. This sparseness arises from multiple sources. First, BHO consumers may have fewer formal credit accounts overall. Second, they are more likely to use alternative financial services such as payday loans and auto title loans that do not report to credit bureaus (Federal Deposit Insurance Corporation, 2023). Third, they may be more likely to bank with smaller lenders, credit unions, or nonbank financial institutions that report less consistently to credit bureaus. In particular, smaller institutions often report only delinquencies, while accounts in good standing may not be reported to credit bureaus. This differential sparseness reduces the information available to data providers and lenders, contributing to the marketing gap we observe.

Panel B of Table 7 presents evidence of this friction. In specifications (1)-(2), the dependent variable is the log of the number of trades on a consumer's credit report. Because the number of trades is directly affected by our credit report variables such as credit card ownership, and directly affects credit scores, we exclude credit report controls and credit score bins from these regressions. Specification (1) shows that BHO respondents have approximately 2 percent fewer trades than similar White or Asian respondents. In specification (2), we examine whether this difference can be explained by the composition of lenders the respondent has accounts with. Although we cannot observe lender types on the credit report, the Mintel data includes statements and notices mailed to existing customers, allowing us to identify each respondent's lenders for this subset of accounts with mailed statements. We group small banks, credit unions, and nonbanks together and classify the lenders as either large banks or not. Specification (2) shows that controlling for the composition of lenders, the difference in the log number of trades is not statistically significant. Moreover, accounts with large banks are much more strongly associated with credit report trades than accounts with smaller institutions. A one percent increase in the number of large bank accounts is associated with a 0.36 percent increase in credit report trades, compared to only 0.08 percent for small banks, credit unions, and nonbanks.[26] This pattern is consistent with large banks reporting more consistently to credit bureaus.

Specifications (3) and (4) further show that BHO respondents are more likely to have accounts with small banks, credit unions, and nonbanks, and less likely to have accounts at large banks, which explains why the BHO coefficient is not statistically significant, after controlling for the composition of lenders, in specification (2).

Finally, information on a credit report may also be wrong. Existing literature has shown that consumers living in majority Black and Hispanic neighborhoods are more

[26]The coefficients are small likely because the number of accounts we observe in the data, based on statements and notices, is a small fraction of the total number of accounts the respondents owns.

likely to have disputes of erroneous information appear on their credit reports (Consumer Financial Protection Bureau, 2021). Specification (5) of Table 7, Panel B, confirms this in our data. We regress an indicator for whether the respondent has a dispute on their credit report on demographics and find that BHO respondents are approximately one percentage point more likely to have a dispute, relative to an average of 9 percent.

In all these cases, the information gap between White and BHO respondents arises from outside the lender. When lenders possess information about potential borrowers, they actively use it for targeting. Ru and Shoar (2016), for example, show that banks target less-sophisticated borrowers with more steeply back-loaded fees, both by targeting based on observable characteristics and by creating menus of offers to separate more and less sophisticated borrowers. If lenders possessed information about White and BHO respondents, it would likely be more profitable to use this information to target different customer types with tailored offers rather than passively avoiding certain groups.[27]

The information channel has several testable implications for credit card offers, which we turn to next.

4.2 Testing the information channel

We next examine three testable implications of the information channel. First, BHO respondents should be less likely to receive new customer acquisition offers but not cross-selling offers to existing customers. This is because lenders rely on external data providers for new customer targeting but use internal information for cross-selling. Second, campaigns that rely more heavily on external partners—co-branded campaigns and larger campaigns—should show larger racial disparities. Third, conditional on receiving an offer, BHO respondents should receive as many or more offers than similar White respondents.

Cross-selling offers versus new customer offers

Under the information channel, BHO respondents should be less likely to receive credit card offers from new lenders, but not from lenders they already have an account with. This is because when cross-selling to existing customers lenders rely on internal information rather than external data providers or credit bureaus.[28]

[27] As discussed in the appendix, it is somewhat unclear whether explicitly using race in the extension of credit card offers would run afoul of fair lending laws.

[28] For example, some lenders do not pull a credit score when considering credit card applications from existing customers, relying instead on internal risk measures. Lenders also use internal data on credit card usage, spending, and revolving behavior to target profitable customers who actively use their cards and revolve balances (Adams et al., 2022).

Table 8 tests this hypothesis. In specification (1), the dependent variable is an indicator for whether the respondent received a cross-selling offer—an offer from a lender they already have an account with.[29] Because only respondents with an existing relationship with a lender are able to receive a cross-selling offer, specification (2) limits the sample to respondents with at least one existing lender relationship. In both cases, there is no difference between BHO and White respondents in the likelihood of receiving a cross-selling offer.

By contrast, our findings are entirely driven by new customer acquisition offers. In specification (3), the dependent variable indicates whether the respondent received a new customer offer from a lender they do not have an existing relationship with. Specification (4) restricts the sample to respondents with at least one existing lender relationship in the data. In both specifications, BHO respondents are significantly less likely to receive new customer offers, with coefficients similar in magnitude to those in Table 3.

Campaigns and share of offers sent to BHO respondents

Second, the information channel implies that the share of offers sent to BHO respondents should be lower in co-branded campaigns, which tend to rely on mailing lists compiled by the co-brand partner, and in larger campaigns, where lenders are more likely to use external marketing firms to achieve broader reach. To test this, we conduct an analysis at the campaign level.[30] Table 9 presents the results, with each observation corresponding to a campaign. The dependent variable is the share of offers sent to BHO respondents. We control for whether it is a pre-selected campaign and for lender size to ensure that campaign size effects are not simply capturing differences between large and small lenders. Campaign size is measured as the log of the number of offers sent and lender size is calculated as the total number of offers sent by the lender.[31] The coefficients on both *Co-branded* and *Campaign size* are negative and significant.

In specification (2), we include lender fixed effects, and the results are very similar, suggesting that even within lender, co-branded and larger campaigns are less likely to send offers to BHO respondents. Specifications (3) and (4) use the mail-volume-weighted share

[29] To identify whether the offer is a cross-selling offer or a new customer offer, we use information from Mintel on whether the offer is an acquisition or a cross-sell, supplemented with information on whether lender had ever sent a statement or notice to the respondent before for any type of financial product.. Our findings are robust to using only one of these definitions.

[30] We restrict our analysis to campaigns with at least 2 observations in our data. This restriction does not change the results.

[31] Results are similar if using assets or other measures of size.

of offers sent to BHO respondents as the dependent variable. This weighting accounts for the fact that our dataset captures only a fraction of the approximately 3 billion credit card offers sent annually—Mintel estimates the number of unobserved offers corresponding to each observed offer. The mail-volume-weighted results are consistent with the unweighted specifications.

Intensive Margin

Third, the information channel predicts that conditional on receiving at least one offer, BHO respondents should receive as many or more offers than similar White respondents. Under the information channel, BHO consumers are underrepresented in marketing databases relative to the overall population, conditional on credit risk. If lenders randomly sample from these databases when compiling lists for lenders, conditional on begin in the database, BHO and White consumers should receive similar numbers of offers on average. If lenders oversample certain demographic characteristics to make their mailings more representative of the broader population—for example, oversampling renters, a status more common among BHO consumers—then BHO consumers in the databases would receive more offers than White consumers in the databases. Moreover, this effect should occur only among new customer offers. As in the previous subsection, the number of cross-selling offers should not differ by race because lenders rely on internal rather than external information for existing customers.

Table 10 tests this prediction by restricting the sample to respondents who received at least one offer and using the number of offers received as the dependent variable. As in Section 2.1, the effect is small in magnitude with a BHO respondent receiving, on average, 0.05 more offers, relative to the unconditional means of 2.4 and 2.3 offers for BHO and White respondents, respectively.[32] Nevertheless, the effect is positive and statistically significant, consistent with the information channel.

Specifications (2)-(5) decompose total offers into cross-selling versus new customer offers. Specification (2) uses the number of cross-selling offers as the dependent variable, while specification (3) additionally restricts to respondents with at least one existing lender relationship. Specifications (4) and (5) repeat this analysis for new customer offers. Consistent with the information channel, we find no difference by race in cross-selling offers but a positive and statistically significant difference in new customer offers.

[32]Since the number of offers is a count variable, in specification (1) of Table A8, we use a Poisson pseudo-likelihood regression.

The next subsection examines the alternative explanation that unoberved risk drives our results.

4.3 Unobservable Risk

Having provided evidence consistent with the information channel, we next examine and rule out alternative channels. The main alternative is that BHO status correlates with unobservable risk factors not captured by our controls. Although we control for observable risk measures including granular credit score bins, some risk that is unobservable in our data but observable to lenders may remain. In this subsection, we discuss why the risk channel is inconsistent with the findings of the previous section and then test and reject an additional implication of the risk channel.

The risk channel is inconsistent with our findings on both the extensive and intensive margins. Under the risk channel, the racial gap in the extensive margin of marketing could be explained if BHO consumers are unobservably riskier than White consumers. However, in specifications (2) and (4) of Table 8, we restrict to respondents who have an account with at least one lender in our sample. For these consumers, lenders sending cross-selling offers possess more information about customer risk than lenders sending new customer offers. Thus, if BHO consumers are riskier, we should observe larger racial gaps in cross-selling offers than in new customer offers. Instead, we find racial gaps only in new customer offers, inconsistent with the risk channel. [33]

Similarly, the risk channel cannot explain the intensive margin findings. Under the risk channel, BHO respondents receiving more offers conditional on receiving any could be explained by differential selection: if the threshold for a BHO respondent to receive at least one offer is higher than for a similar White respondent, then conditional on receiving at least one, BHO respondents would be less risky and receive more offers. However, if this selection mechanism were operating, it should be strongest for cross-selling offers, where lenders have better measures of customer risk. Instead, we find that BHO respondents receive more offers only in new customer campaigns, inconsistent with the risk channel.

Next, we provide direct evidence that the risk channel is inconsistent with the data by examining how banks change their targeting after credit losses. When banks experience losses, they tend to reduce credit supply, especially to riskier borrowers. If BHO respondents are unobservably riskier, banks should disproportionately reduce offers

[33] An alternative explanation would be that banks *incorrectly* believe BHO respondents to be riskier and this belief is corrected with greater information about these respondents, after they become account holders.

to these borrowers after losses.

To test this, we aggregate credit card offer data to the bank-year level and match it to lagged bank balance sheet information from the Call Reports. Table 11, Panel A, presents the results. We regress the log number of credit card offers on lagged measures of credit card losses, including bank and year fixed effects as well as controls for log assets, the Tier 1 capital ratio, advertising expenses, and total number of credit card offers (all lagged one year). Our measure of credit card losses is the share of credit card loans 90 days or more past due in the previous year in specification (1) and 30 days or more past due in specification (2). Both specifications confirm that total offers decline after losses: a one percentage point increase in 90+ day delinquencies reduces offers by 11 percentage points. Specifications (3)-(4) show that the share of offers sent to BHO respondents does not decline—banks reduce total supply but do not disproportionately reduce offers to BHO respondents. This is inconsistent with the hypothesis that BHO respondents are unobservably riskier.

Panel B shows the opposite pattern for observably risky borrowers. We define risky borrowers as nonprime (specification 1-2) or as having high utilization, bankruptcy, derogatory records, or recent delinquencies (specification 3-4). After losses, banks significantly reduce the share of offers sent to these observably risky groups. This contrast demonstrates that banks adjust targeting based on observable risk but not based on BHO status, rejecting the hypothesis that unobservable risk explains the racial marketing gap.

Overall, we find no evidence that unobserved risk explains the lower likelihood of BHO respondents receiving credit card offers.

4.4 Other Potential Channels

In this section, we examine and rule out other potential explanations for our results, including differential responsiveness, geographic redlining, and explicit discrimination or racial animus.

Differential Responsiveness

To optimize their advertising budgets, lenders send credit card offers to consumers most likely to respond and apply. Marketing firms often estimate expected responsiveness using proprietary algorithms and provide this information to lenders (see Section 2.1). An alternative explanation for our results is that lenders may not target BHO respondents because BHO consumers respond less frequently to credit card offers.

However, this explanation is inconsistent with our findings in Table 4. We show that the relationship between receiving an offer and subsequently opening a new credit card is the same for BHO and White respondents, suggesting that differential responsiveness cannot explain the marketing gap.

"Redlining"

An alternative explanation is that lenders avoid sending offers to high-minority geographic areas, a practice known as "redlining." While documented in mortgage markets, redlining is less likely for credit cards, which are marketed nationally using automated underwriting.[34] Nevertheless, we test whether geographic targeting explains our results.

Table 12, specifications (1)-(3), tests for redlining. Specification (1) includes census tract BHO share and its interaction with BHO. If redlining explains the results then after controlling for the local BHO share, BHO status should not be statistically significant. Specification (2) similarly tests if lenders avoid lower-income areas by including an indicator for lower- or moderate-income (LMI) tracts (median income below 80% of MSA median) and its interaction with BHO. Neither interaction term is statistically significant, indicating that the racial gap does not vary with neighborhood composition. Moreover, the main effects of tract BHO share and LMI status are not statistically significant, indicating that lenders do not avoid high-minority or low-income areas overall.

Finally, specification (3) includes census tract fixed effects, comparing White and BHO respondents within the same census tract. The coefficient on BHO remains similar in magnitude and statistical significance, which suggests that geographic exclusion does not play a role in our findings.

Racial Animus

Racial animus, or "taste-based discrimination," is unlikely in the credit card market given that offer mailings and approvals are fully automated. Moreover, Asian respondents are just as likely to receive offers as White respondents, suggesting that structural factors rather than discrimination against minorities explain the BHO gap. This contrasts with mortgage markets, which documents that Black, Hispanic, *and* Asian borrowers are less likely to receive a mortgage (Bhutta et al., 2025).

Nevertheless, we test explicitly for racial animus using data on racially charged

[34]See Aaronson et al. (2021); Fishback et al. (2024, 2020), among others, for evidence of redlining in mortgage markets.

internet searches from Stephens-Davidowitz (2014), measured at the designated market area (DMA) level during 2004-2007. Specification (4) of Table 12 includes DMA fixed effects and an interaction between BHO status and an indicator for above-median racially charged search. The interaction is not statistically significant, indicating that the marketing gap does not vary with local racial animus. This finding supports the information channel rather than explicit discrimination.

5 Discussion and Policy Implications

In this section, we discuss the implications of our findings for policy and financial inclusion.

Competition

The findings discussed in the previous sections reveal a market inefficiency: creditworthy Black and Hispanic borrowers are less likely to receive credit card offers and obtain credit cards. These are potentially profitable customers who should receive credit but do not for reasons unrelated to risk. Competition is often proposed to solve such inefficiencies (Becker, 1957). However, competition is unlikely to ameliorate this outcome because it does not address the underlying information frictions and does not affect how information about potential borrowers propagates. If lenders are unaware that certain Black and Hispanic consumers exist because they do not appear on marketing lists, competition cannot help, especially in a setting where potential borrowers not shopping around, as discussed in section 2.1.

Table 13 tests whether competition affects the share of BHO offers. The analysis is at the campaign level and the dependent variable is the share of offers sent to BHO respondents (as in Table 9). We measure competition using the Herfindahl-Hirschman Index (HHI) across the geographies of each campaign. Specification (1) calculates HHI across all credit card offers in the state in the previous year. Specifications (2) and (3) calculate HHI separately for income and credit score bins. We aggregate HHI to the campaign level by weighting by the number of offers sent into each state or state-bin. All specifications include lender fixed effects and controls from Table 9: pre-approved indicator, co-branded indicator, and campaign size. In specification (4), we test whether the share of BHO offers is affected by lender market power, measured as the lender's share of credit card offers, across the geographies of a campaign.

In all specifications, the measures of competition are not statistically significant,

suggesting that the share of BHO offers is unaffected by local competition or lender market power.

We next examine whether the difficulty of information acquisition due to data privacy laws affects the racial gap in marketing.

Information

What policy prescriptions can mitigate the racial gap in marketing? Because underlying frictions curtail information spread, improving information about potential borrowers should help. Since soft information is not used in the credit card market, improving information necessitates more hard information becoming available. However, there is a trade-off between greater information and consumer privacy (Tucker, 2012). As online marketing and targeted advertising have expanded, some states have passed more stringent privacy laws. While these laws improve consumer privacy and protection, by making information acquisition more expensive, they may exacerbate racial marketing gaps (Dubé et al., 2025). In addition, just as they are less likely to respond to financial surveys, BHO consumers may be more likely to opt out of information sharing, further reducing data providers' information about them.

We test this hypothesis using the California Consumer Privacy Act (CCPA), passed in 2018 and effective January 2020. The CCPA established rights to access, delete, and opt out of personal data sales for California residents and guaranteed non-discrimination protections for consumers exercising these rights. California passed additional privacy legislation in 2020, effective in 2023, and 18 other states passed privacy laws in 2023-2024. However, because our sample ends in 2023, we only examine the CCPA. To evaluate the effects of the CCPA effects, we create a dataset at the campaign-Census division level. While there are nine Census divisions, we split the Pacific division in two, putting California into its own division. We then estimate a difference-in-differences regression:

$$\text{BHO Share}_{it} = \beta Cali_i x Post2020_t + \gamma Controls + \eta_i + \tau_t + \epsilon_{it} \tag{2}$$

The dependent variable is the share of offers sent to BHO respondents. Our coefficient of interest is β, the interaction between the California indicator, $Cali_i$, and the post-2020 indicator. We include division fixed effects (η_i), year fixed effects (τ_t) for the first year the campaign is active, and controls for pre-approved status, co-branded status, campaign size, and lender size. Table 14 presents the results, with specification (2) adding lender fixed effects, specification (3) adding campaign fixed effects, and specification (4)

limiting to campaigns lasting no more than one calendar year.

Across all specifications, the share of BHO offers is approximately 7-8 percentage points lower after the CCPA took effect. Before 2020, the BHO offer share was 19% in California and 11% in the rest of the U.S. After the CCPA, it was 11% in both California and the rest of the U.S., despite California's greater racial diversity.[35]

Figure 3 plots yearly interactions between $Cali_i$ and year indicators. The figure suggests parallel trends are satisfied: the decline in BHO offer share appears only after 2020; there is no difference in the share of BHO offers between California and the other Census divisions prior to 2020.

The findings in this section suggest that information privacy laws may exacerbate the racial gap in marketing and thus have a negative effect on financial inclusion. It is important to note that our findings are suggestive rather than definitive. It is possible that the decline we document is not caused by the CCPA, since 2020 also saw the start of the COVID-19 pandemic, which had massive, permanent effects on consumer credit markets. Establishing the causal effect of privacy regulation on the racial gap in marketing is beyond the scope of this paper and we leave it for future research.

6 Conclusion

This paper examines whether differences in credit card marketing contribute to racial disparities in credit access. Using data on direct mail credit card offers matched to credit bureau records, we establish that Black and Hispanic consumers are less likely to receive offers than observably similar White consumers. We show this marketing gap causally affects credit card ownership and can explain the racial disparity in card ownership. We provide evidence that these differences arise from information frictions: Black and Hispanic consumers are systematically underrepresented in the data sources that external marketing providers use to compile target lists.

Our findings have important policy implications. Because they make information acquisition more expensive, privacy regulation may exacerbate the racial disparities we document. Indeed, we find that California's Consumer Privacy Act appears to have widened the racial marketing gap.

More broadly, our findings demonstrate that the channel through which credit supply meets demand matters for market outcomes. Through their reliance on data providers,

[35] In 2024, only 33% of the California population was White, relative to 57% for the entire U.S.

consumer lenders introduce information frictions into credit provision that lead to under-provision of credit cards to racial minorities. Addressing these information frictions is essential for promoting equal credit access and financial inclusion.

Financial Disclosure

The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.

References

Aaronson, Daniel, Daniel Hartley, and Bhashkar Mazumder, "The Effects of the 1930s HOLC "Redlining" Maps," *American Economic Journal: Economic Policy*, November 2021, *13* (4), 355–92.

Adams, Robert, Vitaly M. Bord, and Bradley Katcher, "Credit Card Profitability," 2022. Feds Notes.

Anderson, Simon P. and Régis Renault, "Advertising Content," *American Economic Review*, March 2006, *96* (1), 93–113.

Anenberg, Elliot, Andrew C. Chang, Serafin Grundl, Kevin B. Moore, and Richard Windle, "The Branch Puzzle: Why Are there Still Bank Branches?," 2018. Feds Notes.

Atske, Sara and Andrew Perrin, "Home Broadband Adoption, Computer Ownership Vary by Race, Ethnicity in the U.S.," July 2021. Pew Research Center.

Bakker, Trevor J, Stefanie DeLuca, Eric A English, James S Fogel, Nathaniel Hendren, and Daniel Herbst, "Credit Access in the United States," Working Paper 34053, National Bureau of Economic Research July 2025.

Bartlett, Robert, Adair Morse, Richard Stanton, and Nancy Wallace, "Consumer-lending discrimination in the FinTech Era," *Journal of Financial Economics*, 2022, *143* (1), 30–56.

Basten, Christoph and Ragnar Juelsrud, "Cross-Selling in Bank-Household Relationships: Mechanisms and Implications for Pricing," *The Review of Financial Studies*, 08 2023, p. hhad062.

Becker, Gary S, *The economics of discrimination*, University of Chicago press, 1957.

Begley, Taylor and Amiyatosh Purnanandam, "Color and credit: Race, regulation, and the quality of financial service," *Journal of Financial Economics*, 2021, *141* (1), 48–65.

Bertrand, Marianne, Dean Karlan, Sendhil Mullainathan, Eldar Shafir, and Jonathan Zinman, "What's Advertising Content Worth? Evidence from a Consumer Credit Marketing Field Experiment*," *The Quarterly Journal of Economics*, 02 2010, *125* (1), 263–306.

Bhutta, Neil and Aurel Hizmo, "Do minorities pay more for mortgages?," *Review of FInancial Studies*, 2021, *34* (2), 763–789.

_ , **Andreas Fuster, and Aurel Hizmo**, "Paying too much? Borrower Sophistication and Overpayment in the U.S. Mortgage Market," *Journal of Finance*, Forthcoming.

_ , **Aurel Hizmo, and Daniel Ringo**, "How Much Does Racial Bias Affect Mortgage Lending? Evidence from Human and Algorithmic Credit Decisions," *The Journal of Finance*, 2025, *80* (3), 1463–1496.

Blanchflower, David G., Phillip B. Levine, and David J. Zimmerman, "Discrimination in the Small-Business Credit Market," *The Review of Economics and Statistics*, 11 2003, *85* (4), 930–943.

Blattner, Laura and Scott Nelson, "How Costly is Noise? Data and Disparities in Consumer Credit," 2024. Working Paper.

Brevoort, Kenneth P., Philipp Grimm, and Michelle Kambara, "Credit Invisibles and the Unscored," *Cityscape*, 2016, *18* (2), 9–34.

Butler, Alexander W., Erik J. Mayer, and James P. Weston, "Racial Disparities in the Auto Loan Market," *Review of Financial Studies*, 2023, *136*, 1–41.

Chernenko, Sergey and David Scharfstein, "Racial disparities in the Paycheck Protection Program," *Journal of Financial Economics*, 2024, *160*, 103911.

_ , **Nathan Kaplan, Asani Sarkar, and David S Scharfstein**, "Applications or Approvals: What Drives Racial Disparities in the Paycheck Protection Program?," Working Paper 31172, National Bureau of Economic Research April 2023.

Cohen-Cole, Ethan, "Credit Card Redlining," 2008. Federal Reserve Bank of Boston Working Paper.

Consumer Financial Protection Bureau, "Data point: Credit invisibles," 2015.

_ , "Disputes on Consumer Credit Reports," 2021.

_ , "The Consumer Credit Card Market," 2023.

Crump, Richard K., V. Joseph Hotz, Guido W. Imbens, and Oscar A. Mitnik, "Dealing with limited overlap in estimation of average treatment effects," *Biometrika*, 01 2009, *96* (1), 187–199.

Célérier, Claire and Purnoor Tak, "Finance, Advertising, and Race," *The Review of Financial Studies*, 06 2025, *38* (11), 3149–3204.

Di Maggio, Marco and Vincent Yao, "Fintech Borrowers: Lax Screening or Cream-Skimming?," *The Review of Financial Studies*, 2021, *34* (10), 4565–4618.

Driscoll, John C., Benjamin S. Kay, Geng Li, and Cindy M. Vojtech, "The Channel through which Banks Tighten Credit Card Standards," 2025. Working Paper.

Dubé, Jean-Pierre, John G. Lynch, Dirk Bergemann, Mert Demirer, Avi Goldfarb, Garrett Johnson, Anja Lambrecht, Tesary Lin, Anna Tuchman, and Catherine Tucker, "Frontiers: The Intended and Unintended Consequences of Privacy Regulation for Consumer Marketing," *Marketing Science*, 2025, *44* (5), 975–984.

D'Acunto, Francesco, Pulak Ghosh, and Alberto G. Rossi, "How costly are cultural biases? Evidence from FinTech," *Journal of Financial Economics*, 2026, *175*, 104202.

Federal Deposit Insurance Corporation, "FDIC National Survey of Unbanked and Underbanked Households," 2023.

Federal Reserve Board of Governors, "Report to Congress on the Profitability of Credit Card Operation of Depository Institutions," 2024.

Firestone, Simon, "Race, Ethnicity, and Credit Card Marketing," *Journal of Money, Credit, and Banking*, 2014, *41* (6), 1205–1224.

Fishback, Price, Jonathan Rose, Kenneth A. Snowden, and Thomas Storrs, "New Evidence on Redlining by Federal Housing Programs in the 1930s," *Journal of Urban Economics*, 2024, *141*, 103462.

Fishback, Price V., Jessica LaVoice, Allison Shertzer, and Randall Walsh, "Race, Risk, and the Emergence of Federal Redlining," 2020. NBER Working Paper.

Fisman, Raymond, Daniel Paravisini, and Vikrant Vig, "Cultural Proximity and Loan Outcomes," *American Economic Review*, 2017, *107* (2), 457–492.

Frame, W. Scott, Ruidi Huang, Erica Xuewei Jiang, Yeonjoon Lee, Will Shuo Liu, Erik J. Mayer, and Adi Sunderam, "The Impact of Minority Representation at Mortgage Lenders," *The Journal of Finance*, 2025, *80* (2), 1209–1260.

Fuster, Andreas, Matthew Plosser, Philipp Schnabl, and James Vickery, "The Role of Technology in Mortgage Lending," *The Review of Financial Studies*, 04 2019, *32* (5), 1854–1899.

_ , Paul Goldsmith-Pinkham, Tarun Ramadorai, and Angsgar Walther, "Predictably Unequal? The Effects of Machine Learning on Credit Markets," *The Journal of Finance*, 2022, *77* (1), 5–47.

Goldfarb, Avi and Catherine Tucker, "Privacy and Innovation," *Innovation Policy and the Economy*, 2012, *12*, 65–90.

Goodstein, Ryan, Alicia Lloro, Sherrie L.W. Rhine, and Jeffrey Weinstein, "What Accounts for Racial and Ethnic Differences in Credit Use?," 2018. FDIC Division of Depositor and Consumer Protection Working Paper.

Greene, Claire, Julian Perry, and Joanna Stavins, "Consumer Payment Behavior by Income and Demographics," 2023. FRB of Boston Working Paper.

Grundl, Serafin and You Suk Kim, "Consumer mistakes and advertising: The case of mortgage refinancing," *Quantitative Marketing and Economics (QME)*, June 2019, *17* (2), 161–213.

Guttman-Kenney, Benedict and Andres Shahidinejad, "Unraveling Information Sharing in Consumer Credit Markets," 2025. Working Paper.

Han, Song, Benjamin J. Keys, and Geng Li, "Credit Supply to Personal Bankruptcy Filers: Evidence from Credit Card Mailings," 2011. Federal Reserve System Feinance and Economics Discussion Series Working Paper.

_ , _ , and _ , "Unsecured Credit Supply, Credit Cycles, and Regulation," *The Review of Financial Studies*, 10 2017, *31* (3), 1184–1217.

Howell, Sabrina T., Theresa Kuchler, David Snitkof, Johannes Stroebel, and Jun Wong, "Lender Automation and Racial Disparities in Credit Access," *The Journal of Finance*, 2024, *79* (2), 1457–1512.

Kovrijnykh, Natalia, Igor Livshits, and Ariel Zetlin-Jones, "Building Credit Histories," 2023. Working Paper.

Liberti, José María and Mitchell A Petersen, "Information: Hard and Soft," *The Review of Corporate Finance Studies*, 11 2018, *8* (1), 1–41.

Livshits, Igor, James C. Mac Gee, and Michele Tertilt, "The Democratization of Credit and the Rise in Consumer Bankruptcies," *The Review of Economic Studies*, 2016, *83* (4), 1673–1710.

Mullainathan, Sendhil, Joshua Schwartzstein, and Andrei Shleifer, "Coarse Thinking and Persuasion*," *The Quarterly Journal of Economics*, 05 2008, *123* (2), 577–619.

Munnell, Alicia H., Geoffrey M. B. Tootell, Lynn E. Browne, and James McEneaney, "Mortgage Lending in Boston: Interpreting HMDA Data," *The American Economic Review*, 1996, *86* (1), 25–53.

Narajabad, Borghan N., "Information technology and the rise of household bankruptcy," *Review of Economic Dynamics*, 2012, *15* (4), 526–550.

Neumann, Nico, Catherine E. Tucker, Levi Kaplan, Alan Mislove, and Piotr Sapiezynski, "Data Deserts and Black Boxes: The Impact of Socio-Economic Status on Consumer Profiling," *Management Science*, 2024, *70* (11), 8003–8029.

Office of the Assistant Secretary for Planning and Evaluation, "Persons in Low-Income Households Have Less Access to Internet Services," March 2021. U.S. Department of Health & Human Services.

Pagano, Marco and Tullio Jappelli, "Information Sharing in Credit Markets," *The Journal of Finance*, 1993, *48* (5), 1693–1718.

Ru, Hong and Antoinette Shoar, "Do Credit Card Companies Screen for Behavioral Biases?," 2016. NBER Working Paper No. w22360.

Skanderson, David and Dubravka Ritter, "Fair Lending Analysis of Credit Cards," 2014. FRB of Philadelphia Working Paper.

Stango, Victor and Jonathan Zinman, "Borrowing High versus Borrowing Higher: Price Dispersion and Shopping Behavior in the U.S. Credit Card Market," *The Review of Financial Studies*, April 2016, *29* (4), 979–1006.

Stein, Jeremy C., "Information Production and Capital Allocation: Decentralized versus Hierarchical Firms," *The Journal of Finance*, 2002, *57* (5), 1891–1921.

Stephens-Davidowitz, Seth, "The cost of racial animus on a black candidate: Evidence using Google search data," *Journal of Public Economics*, 2014, *118*, 26–40.

Tucker, Catherine, "Algorithmic exclusion: The fragility of algorithms to sparse and missing data," 2023. Brookings Center on Regulation and Markets Working Paper.

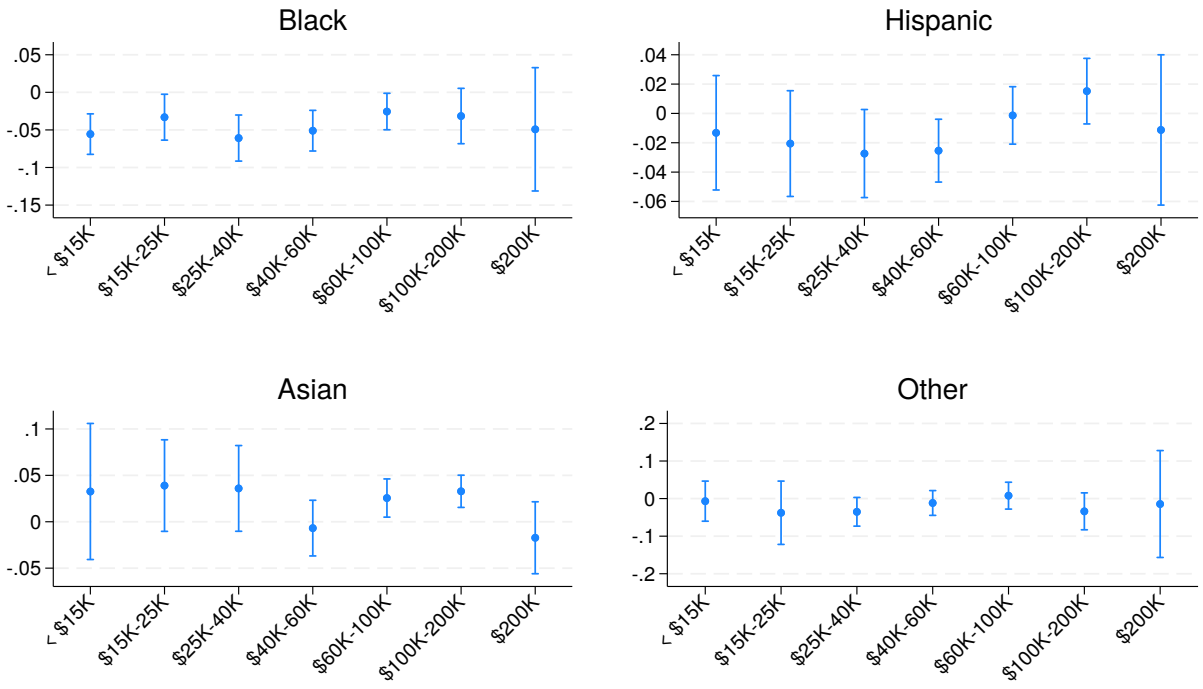
Tucker, Catherine E., "The economics of advertising and privacy," *International Journal of Industrial Organization*, 2012, *30* (3), 326–329. Selected Papers, European Association for Research in Industrial Economics 38th Annual Conference, Stockholm, Sweden, September 1-3, 2011.

Zhang, David and Paul Willen, "Do Lenders Still Discriminate? A Robust Approach for Assessing Differences in Menus," 2020. Federal Reserve Bank of Boston Working Paper.

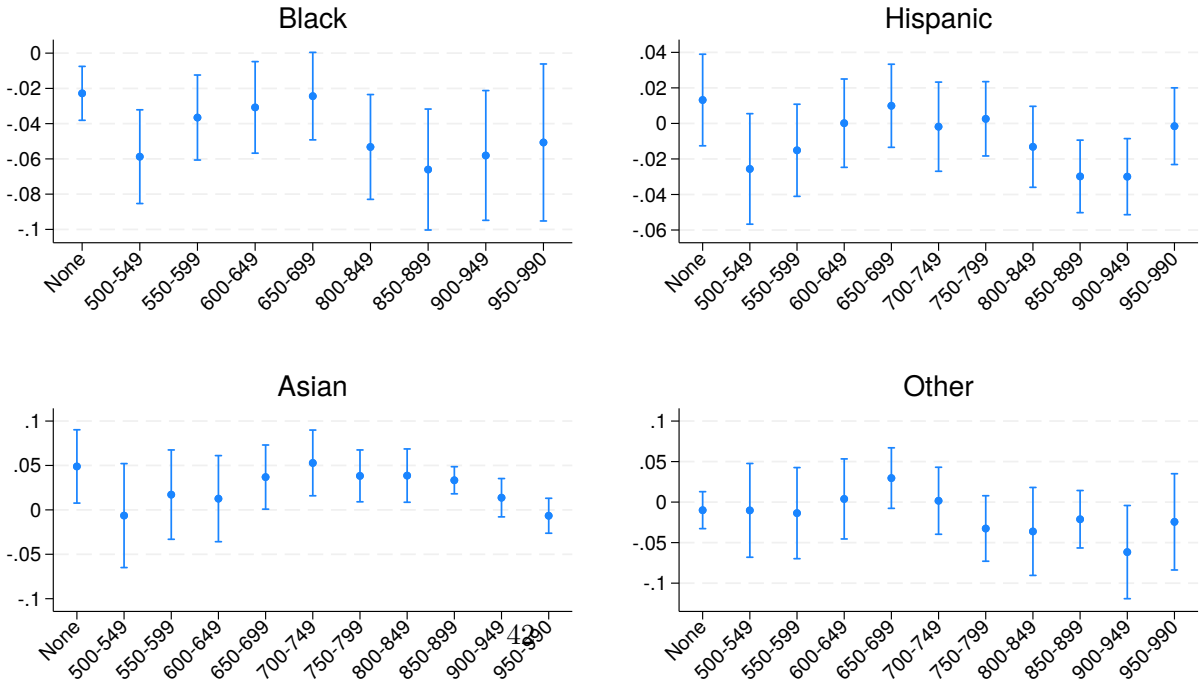
Figure 1: Credit card ownership by respondent characteristics

This figure plots the coefficients of the interactions between the race indicators and financial characteristics.

Panel A: Income



Panel B: 50-point credit score bins



Panel C: Other characteristics

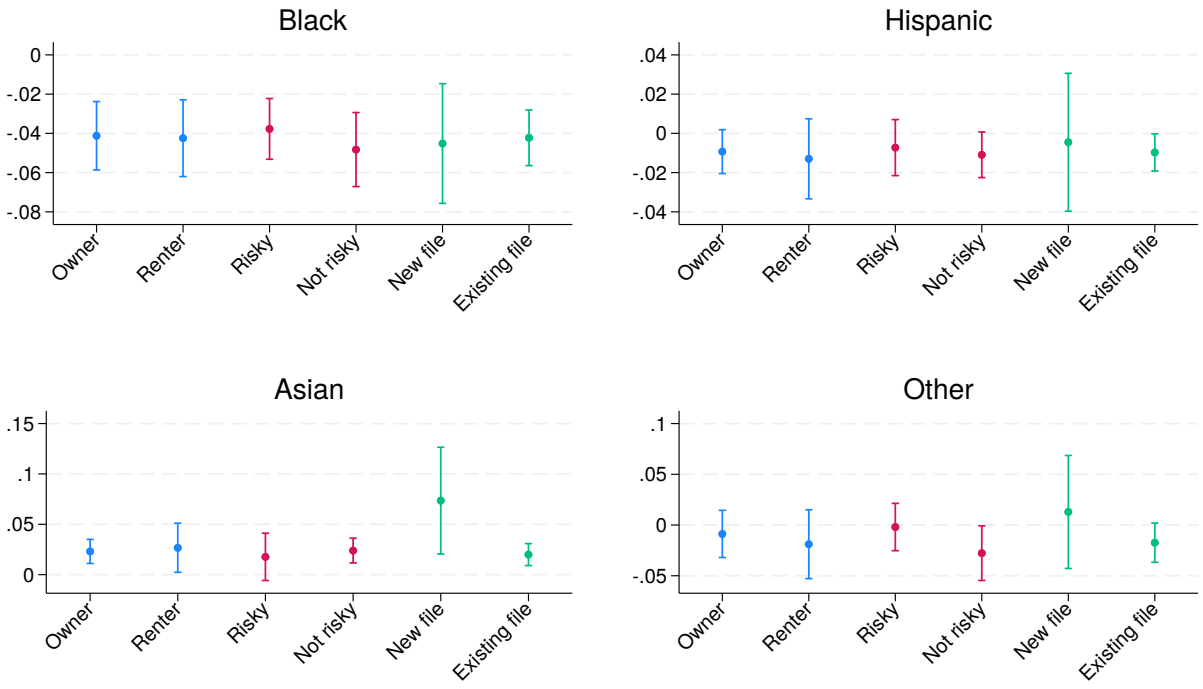
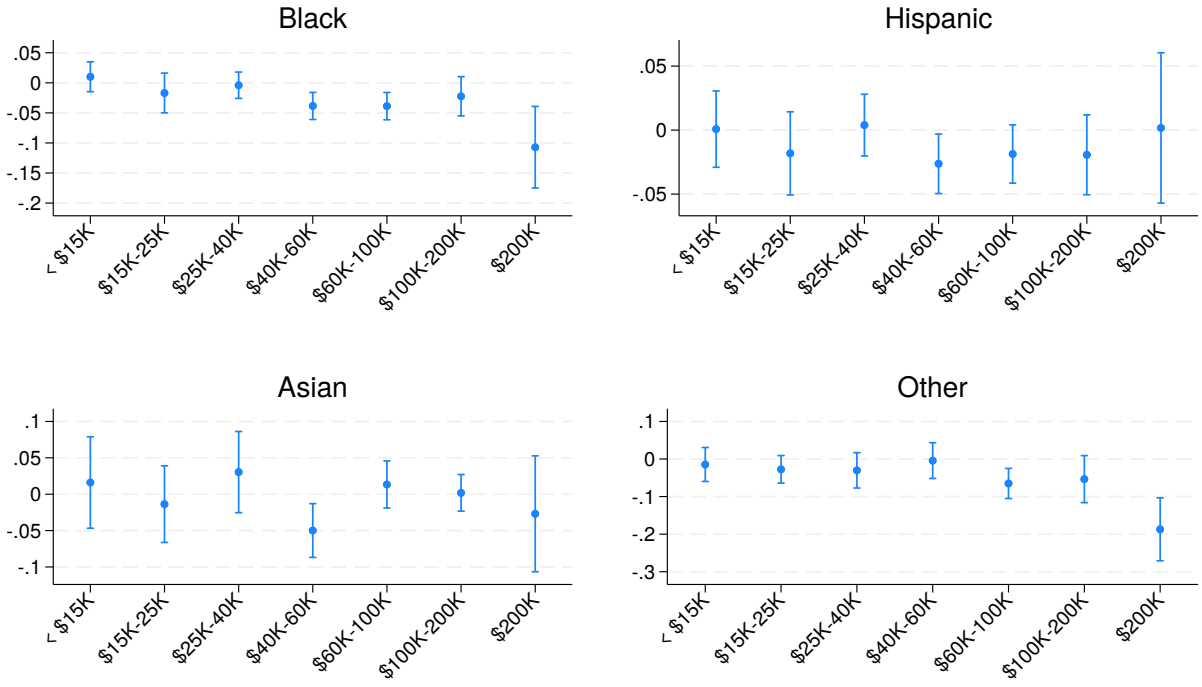


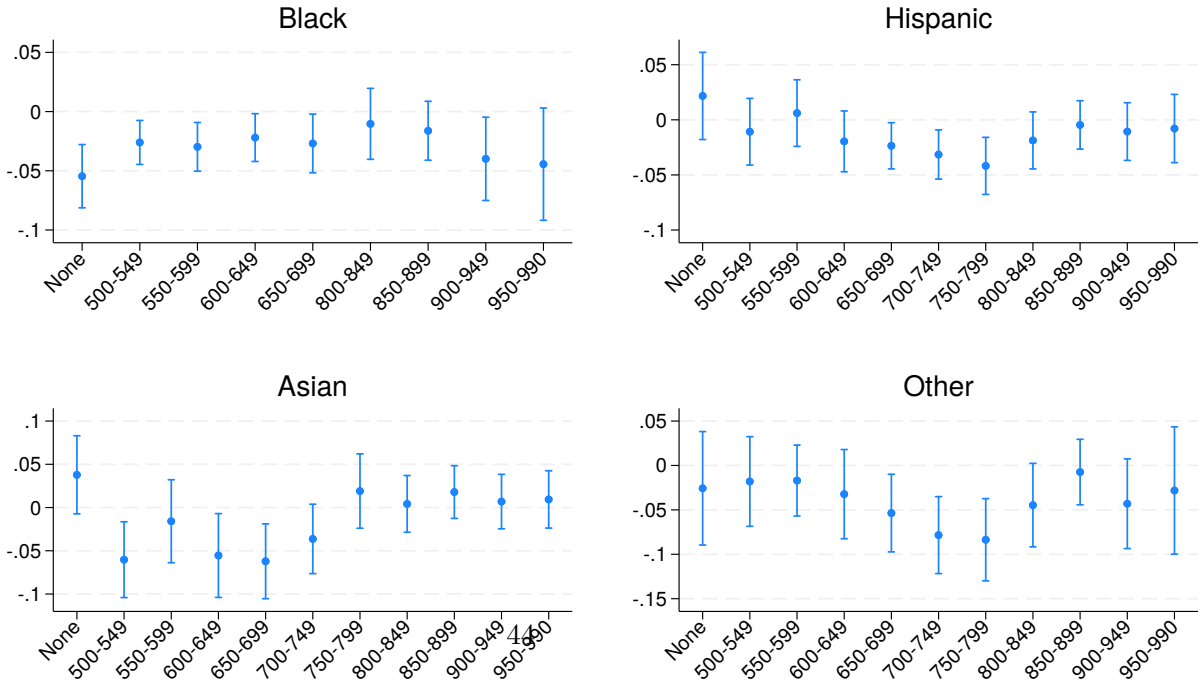
Figure 2: Credit card marketing by respondent characteristics

This figure plots the coefficients of the interactions between the race indicators and financial characteristics.

Panel A: Income



Panel B: 50-point credit score bins



Panel C: Other characteristics

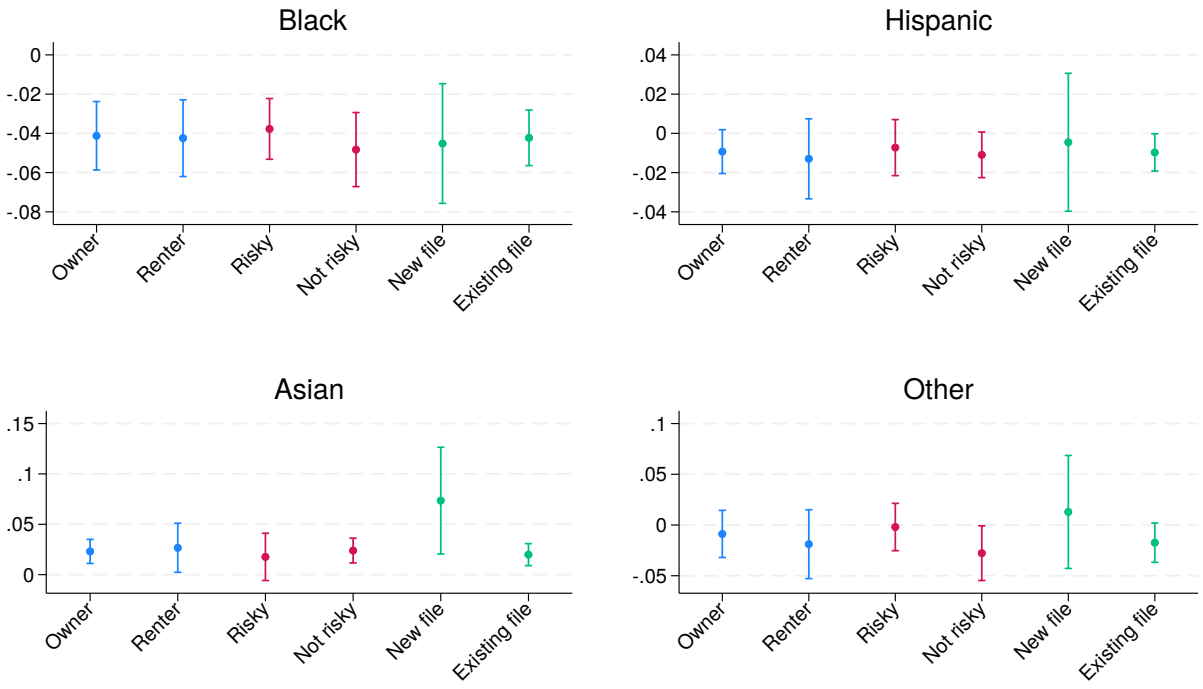


Figure 3: The Effects of the California Consumer Privacy Act (2018)

This figure plots the coefficients of the interaction between year indicators and the indicator for California.

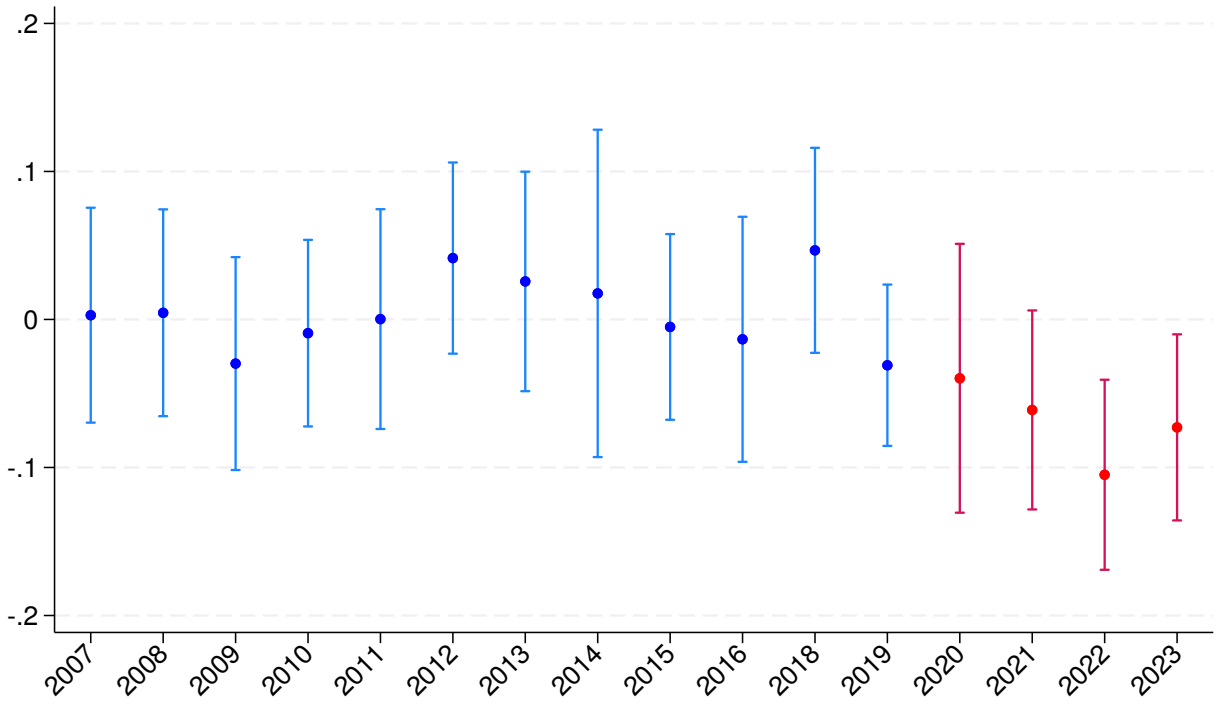


Table 1: Summary statistics

This table presents summary statistics for demographic and credit report variables for our sample, broken down by self-reported race category. All demographic variables are self-reported and reflect the demographics of the head of household. Credit report variables are as of approximately 60 days prior to the survey date.

Panel A: Demographic Variables

Demographic Variables	All	White	Black	Hispanic	Asian
< $15,000	0.067	0.059	0.163	0.084	0.032
$15,000-25,000	0.072	0.069	0.116	0.078	0.035
$25,000-40,000	0.137	0.134	0.183	0.153	0.087
$40,000-60,000	0.177	0.178	0.187	0.190	0.117
$60,000-100,000	0.311	0.319	0.216	0.302	0.311
$100,000-200,000	0.208	0.212	0.126	0.164	0.351
> $200,000	0.028	0.028	0.009	0.030	0.068
Female Alone	0.111	0.105	0.212	0.077	0.095
Female Head Living with Others Related	0.084	0.073	0.215	0.136	0.069
Female Living with Others Not Related	0.027	0.027	0.023	0.026	0.031
Male Alone	0.084	0.083	0.093	0.073	0.097
Male Head Living with Others Related	0.031	0.028	0.058	0.054	0.046
Male Living with Others Not Related	0.019	0.019	0.023	0.015	0.010
Male and Female Married	0.565	0.584	0.330	0.559	0.562
Married / Domestic Partnership	0.078	0.080	0.046	0.061	0.090
Household size	2.645	2.637	2.512	3.003	2.938
Owner	0.819	0.839	0.632	0.741	0.784
Renter	0.161	0.144	0.330	0.232	0.196
Single-family home	0.826	0.837	0.729	0.800	0.797
Mobile Home	0.042	0.043	0.030	0.036	0.011
Has child	0.254	0.247	0.250	0.352	0.372
<25	0.025	0.025	0.021	0.026	0.032
25-29	0.048	0.048	0.039	0.058	0.060
30-34	0.057	0.055	0.053	0.073	0.080
25-29	0.074	0.071	0.078	0.096	0.122
40-44	0.093	0.090	0.096	0.124	0.122
45-49	0.094	0.092	0.093	0.115	0.126
50-54	0.119	0.118	0.128	0.127	0.126
55-59	0.137	0.138	0.144	0.125	0.121
60-64	0.156	0.159	0.159	0.117	0.119
65+	0.198	0.204	0.191	0.139	0.092
Observations	558814	488832	34079	30685	16028

Panel B: Credit report Variables

Credit Report Variables	All	White	Black	Hispanic	Asian
Credit Score	815.480	822.644	724.298	779.041	846.254
S011: Number of trades	7.528	7.553	7.073	7.571	8.199
Log Recent Debt	9.110	9.167	8.297	9.035	9.508
Has a mortgage trade	0.402	0.410	0.308	0.387	0.439
Has at least one active bankcard	0.764	0.777	0.598	0.715	0.834
Has a retail trade	0.274	0.272	0.326	0.296	0.219
Has personal finance trade	0.270	0.263	0.350	0.309	0.248
Has an active installment trade	0.449	0.446	0.505	0.487	0.367
Utilization	0.324	0.310	0.518	0.400	0.249
Debt to Income	1.322	1.284	1.495	1.570	1.771
Revolving Debt to Income	0.160	0.160	0.158	0.178	0.152
Recent inquiry	0.288	0.280	0.375	0.338	0.278
Has Personal Bankruptcy	0.054	0.052	0.080	0.073	0.039
Has Derogatory Records	0.087	0.081	0.168	0.115	0.062
Highest Delinq ever on a trade is 90+	0.200	0.179	0.454	0.297	0.147
30 dpd in past 24 mos	0.172	0.156	0.369	0.250	0.131
New File within 5y	0.035	0.034	0.044	0.048	0.048
Last updated within 1y	0.961	0.965	0.915	0.949	0.974
Received CC offer	0.454	0.461	0.373	0.436	0.475
Credit Card Offers	1.046	1.059	0.893	1.043	1.078
Observations	558814	488832	34079	30685	16028

Table 2: Credit card ownership and race

This panel presents the results of the regression in equation (1). The dependent variable is an indicator for whether the consumer owns a credit card. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

	$I\{CC_{i,t}\}$				
				PSM	
	(1)	(2)	(3)	(4)	(5)
Black	-0.180***	-0.127***	-0.042***	-0.053***	
	(0.008)	(0.009)	(0.007)	(0.009)	
Hispanic	-0.062***	-0.050***	-0.010**	-0.015*	
	(0.009)	(0.007)	(0.005)	(0.008)	
Asian/Pacific Islander	0.053***	0.030***	0.022***	0.025	
	(0.011)	(0.008)	(0.006)	(0.036)	
Other	-0.100***	-0.069***	-0.016*	-0.009	
	(0.015)	(0.013)	(0.010)	(0.013)	
BHO					-0.026***
					(0.004)
Owner		0.092***	0.015*	0.013	0.015*
		(0.012)	(0.009)	(0.017)	(0.009)
Renter		-0.048***	0.013	0.017	0.013
		(0.014)	(0.009)	(0.015)	(0.009)
Single-family home		-0.017***	-0.011***	-0.017**	-0.011***
		(0.006)	(0.004)	(0.009)	(0.004)
Mobile Home		-0.154***	-0.026***	-0.036**	-0.026***
		(0.010)	(0.007)	(0.015)	(0.007)
Employed FT		0.060***	0.012**	-0.023	0.013**
		(0.009)	(0.006)	(0.015)	(0.006)
Employed PT		-0.001	0.012	-0.020	0.012
		(0.011)	(0.008)	(0.018)	(0.008)
Unemployed		-0.025**	0.016**	-0.009	0.016**
		(0.010)	(0.007)	(0.016)	(0.007)
Log Recent Debt			0.067***	0.066***	0.067***
			(0.000)	(0.001)	(0.000)
Has a mortgage trade			-0.195***	-0.177***	-0.195***
			(0.004)	(0.009)	(0.004)
Has a retail trade			0.077***	0.112***	0.077***
			(0.002)	(0.008)	(0.002)
Has personal finance trade			-0.005*	-0.034***	-0.005**

			(0.003)	(0.008)	(0.003)
Has an active installment trade			-0.112***	-0.174***	-0.112***
			(0.003)	(0.009)	(0.003)
Recent inquiry			0.031***	0.050***	0.031***
			(0.002)	(0.004)	(0.002)
Has Personal Bankruptcy			0.021***	0.038***	0.021***
			(0.007)	(0.013)	(0.007)
Has Derogatory Records			-0.068***	-0.062***	-0.069***
			(0.005)	(0.009)	(0.005)
Highest Delinq ever on a trade is 90+			-0.100***	-0.071***	-0.100***
			(0.004)	(0.008)	(0.004)
30 dpd in past 24 mos			-0.013***	-0.036***	-0.014***
			(0.003)	(0.006)	(0.003)
New File within 5y			-0.050***	-0.041***	-0.050***
			(0.005)	(0.009)	(0.005)
Last updated within 1y			0.153***	0.074***	0.154***
			(0.006)	(0.013)	(0.006)
Month FE	Yes	Yes	Yes	Yes	Yes
County, Age, HH FE		Yes	Yes	Yes	Yes
Income Bin FE			Yes	Yes	Yes
Credit Score Bin FE			Yes	Yes	Yes
Observations	558813	558813	558813	45297	558813
R-Squared	0.019	0.102	0.402	0.513	0.402

Table 3: Credit card marketing and race

This panel presents the results of the regression in equation (1). The dependent variable is an indicator for whether the consumer has a credit card offer. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

	$I\{Offer_{i,t}\}$				
				PSM	
	(1)	(2)	(3)	(4)	(5)
Black	-0.091***	-0.062***	-0.022***	-0.037***	
	(0.007)	(0.007)	(0.007)	(0.008)	
Hispanic	-0.030***	-0.029***	-0.015***	-0.035***	
	(0.007)	(0.006)	(0.006)	(0.008)	
Asian/Pacific Islander	0.025***	0.002	0.000	-0.060	
	(0.009)	(0.009)	(0.008)	(0.053)	
Other	-0.078***	-0.062***	-0.041***	-0.048***	
	(0.012)	(0.012)	(0.010)	(0.016)	
BHO					-0.021***
					(0.005)
Owner		0.048***	0.012	0.061***	0.012
		(0.009)	(0.009)	(0.016)	(0.009)
Renter		-0.008	0.016*	0.042**	0.017*
		(0.010)	(0.009)	(0.017)	(0.009)
Single-family home		-0.005	-0.001	-0.001	-0.001
		(0.005)	(0.005)	(0.007)	(0.005)
Mobile Home		-0.057***	-0.004	-0.010	-0.003
		(0.008)	(0.008)	(0.016)	(0.007)
Employed FT		0.025***	0.005	0.018	0.006
		(0.007)	(0.007)	(0.016)	(0.007)
Employed PT		-0.005	0.002	-0.013	0.002
		(0.009)	(0.008)	(0.020)	(0.009)
Unemployed		-0.029***	-0.010	-0.003	-0.010
		(0.008)	(0.007)	(0.018)	(0.007)
Log Recent Debt			0.006***	0.005***	0.006***
			(0.001)	(0.001)	(0.001)
Log trades			0.029***	0.019***	0.029***
			(0.003)	(0.007)	(0.003)
Has a mortgage trade			-0.008**	-0.019**	-0.008**
			(0.003)	(0.009)	(0.003)
Has at least one active bankcard			0.024***	0.037***	0.025***

			(0.003)	(0.008)	(0.003)
Has a retail trade			0.022***	0.018**	0.022***
			(0.002)	(0.007)	(0.002)
Has personal finance trade			-0.006**	-0.005	-0.006**
			(0.003)	(0.007)	(0.003)
Has an active installment trade			-0.004	-0.014*	-0.004
			(0.003)	(0.008)	(0.003)
High credit card utilization: 80+			-0.017***	-0.020**	-0.017***
			(0.003)	(0.008)	(0.003)
Recent inquiry			0.000	-0.011**	0.000
			(0.002)	(0.004)	(0.002)
Has Personal Bankruptcy			-0.001	-0.008	-0.001
			(0.007)	(0.012)	(0.007)
Has Derogatory Records			-0.055***	-0.034***	-0.055***
			(0.005)	(0.010)	(0.005)
Highest Delinq ever on a trade is 90+			-0.015***	-0.005	-0.015***
			(0.004)	(0.007)	(0.004)
30 dpd in past 24 mos			-0.033***	-0.015**	-0.033***
			(0.003)	(0.007)	(0.003)
New File within 5y			0.087***	0.057***	0.087***
			(0.005)	(0.011)	(0.005)
Last updated within 1y			0.082***	0.073***	0.082***
			(0.008)	(0.014)	(0.008)
Month FE	Yes	Yes	Yes	Yes	Yes
County, Age, HH FE		Yes	Yes	Yes	Yes
Income Bin FE			Yes	Yes	Yes
Credit Score Bin FE			Yes	Yes	Yes
Observations	558813	558813	558813	45297	558813
R-Squared	0.039	0.065	0.100	0.183	0.100

Table 4: Can marketing explain credit card ownership?

In specifications (1)-(3), the dependent variable is an indicator for at least one new credit card opened. In specifications (4)-(5), it is whether respondent owns at least one credit card at time t'. We limit to the sample of respondents that are observed again between 2 and 6 months after they are observed the first time. The instrument for the IV columns is the log of the number of credit card campaigns in the same county and month. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

	$I\{Open_{i,t}\}$			$I\{CC_{i,t'}\}$	
	OLS	IV	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)
$I\{Offer\}$	0.005***	0.015***	0.016***		0.020**
	(0.002)	(0.005)	(0.005)		(0.009)
$BHOxI\{Offer\}$			0.006		0.050***
			(0.010)		(0.017)
BHO			-0.003	-0.028***	-0.048***
			(0.005)	(0.009)	(0.012)
CC Controls	Yes	Yes	Yes		
Other Controls	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	99094	99094	99094	99094	99094
R-Squared	0.055	0.015	0.010	0.414	0.159

Table 5: Delinquency and credit score

In this table, we examine whether respondents who receive offers (and open cards) have long-run negative effects on their credit reports. In Specifications (1)-(3), we limit to respondents whose credit reports are observed again 7-14 months after their offers are observed (thus their credit report variables are observed 5-12 months after the offer. In specifications (4)-(6), we limit to respondents whose credit reports are observed 13-24 months after their credit card offers are initially observed. Offer receipt is instrumented using the log number of credit card campaigns in the respondent's county-month (leave-one-out). $BHOxI\{Offer\}$ is instrumented using BHO × Log(CC Campaigns). Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

	7-12 Months			13-24 Months		
	30-Day Delinquency	Chg in CS	Have a CS	30-Day Delinquency	Chg in CS	Have a CS
	(1)	(2)	(3)	(4)	(5)	(6)
$I\{Offer\}$	-0.002	0.725	0.022***	0.003	-3.117	0.032***
	(0.002)	(1.004)	(0.004)	(0.004)	(2.169)	(0.008)
BHO	0.003	-5.622***	0.003	0.005	-10.157***	0.007
	(0.003)	(1.184)	(0.004)	(0.004)	(3.034)	(0.009)
$BHOxI\{Offer\}$	0.006	4.772**	-0.012	-0.009	14.477**	-0.017
	(0.004)	(2.377)	(0.008)	(0.006)	(6.142)	(0.020)
All Controls, FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	130228	119895	130228	61858	56537	61858
R-Squared	0.014	0.013	0.016	0.007	0.016	0.021

Table 6: Credit card offer terms and characteristics

This table examines differences in the terms and characteristics of the offers received by White respondents and the offers received by BHO respondents.

Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

Panel A: Interest Rate and Limit

	Interest Rate			Min. Credit Limit		
	(1)	(2)	(3)	(4)	(5)	(6)
BHO	0.962***	0.193***	0.000	-210.423***	-63.443***	0.009
	(0.062)	(0.047)	(0.000)	(12.786)	(12.323)	(0.014)
Date FE	Yes			Yes		
All Controls, FE		Yes			Yes	
Campaign x Date FE			Yes			Yes
Observations	532431	532430	532431	356116	356113	356116
R-Squared	0.006	0.125	0.000	0.003	0.039	0.000

Panel B: Card Characteristics

	Pre-selected	Non-personalized	Co-branded	Rewards - Any	Rewards - Miles	Rewards - Points
	(1)	(2)	(3)	(4)	(5)	(6)
BHO	0.008***	0.011***	-0.011***	-0.022***	-0.013***	-0.010***
	(0.002)	(0.003)	(0.002)	(0.004)	(0.003)	(0.003)
All Controls, FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	532430	532430	532430	532430	532430	532430
R-Squared	0.007	0.006	0.004	0.017	0.004	0.003

Table 7: Frictions in information spread

Panel A. Non-Response Rates by Race/Ethnicity from the 2023 FDIC National Survey of Unbanked and Underbanked Households

Race/Ethnicity	**Non-Response Rate**
Black	36.3%
Hispanic	31.8%
Asian Pacific Islander	29.9%
White	28.7%
Other	29.1%

Panel B. Credit reports, lenders size and disputes

This panel examines differences in credit report information and lender composition across respondents. In specifications (1)-(2), the dependent variable is the log number of trades on the respondent's credit report, while in specifications (3)-(4), it is the log of the number of the respondent's lender relationships observed in the sample, separated into small banks/credit unions/nonbanks and large banks, and in specification (5) it is an indicator for whether the respondent has a dispute recorded on their credit report. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

	Log Num. Trades		Log Lenders		$I\{Dispute\}$
			Small/CU/Nonbank	Large	
	(1)	(2)	(3)	(4)	(5)
BHO	-0.021*	-0.001	0.009*	-0.016**	0.011***
	(0.011)	(0.011)	(0.005)	(0.007)	(0.004)
Log small banks/CU/nonbanks		0.077***			
		(0.006)			
Log large banks		0.357***			
		(0.005)			
Controls	Yes	Yes	Yes	Yes	Yes
Credit Report Controls			Yes	Yes	Yes
CS Bin FE			Yes	Yes	Yes
Other FE	Yes	Yes	Yes	Yes	Yes
Observations	558813	558813	558813	558813	557802
R-Squared	0.175	0.257	0.270	0.436	0.070

Table 8: New and existing borrowers

In this table, we split the the dependent variable by whether the respondent received a cross-selling offer (specifications (1)-(2)) or a new customer offer (specifications (3)-(4)). In specifications (2) and (4), we limit the sample to respondents that receive a statement or notice for an existing account. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

	Cross-selling Offer		New Cust. Offer	
	All	Have an account	All	Have an account
	(1)	(2)	(3)	(4)
BHO	-0.004	-0.006	-0.020***	-0.019***
	(0.003)	(0.005)	(0.004)	(0.005)
All Controls, FE	Yes	Yes	Yes	Yes
Observations	558813	345657	558813	345657
R-Squared	0.051	0.060	0.095	0.096

Table 9: Credit card campaigns

In this table, the dependent variable is the share of offers sent to BHO consumers. In specifications (3) and (4), we limit the sample to campaigns that were received by at least 20 respondents in our sample. Standard errors are shown in parentheses and are clustered at the lender level. * p<0.10, ** p<0.05, *** p<0.01.

	Share BHO		Share BHO, Mailvolume Wtd.	
	(1)	(2)	(3)	(4)
Co-branded	-0.035***	-0.018***	-0.038***	-0.019***
	(0.006)	(0.006)	(0.006)	(0.006)
Pre-selected	0.005	0.003	0.006	0.003
	(0.010)	(0.012)	(0.011)	(0.013)
Campaign size	-0.008***	-0.006**	-0.006**	-0.004*
	(0.003)	(0.003)	(0.003)	(0.002)
Lender size	-0.002		-0.002	
	(0.003)		(0.003)	
Lender FE		Yes		Yes
Observations	48075	48075	48075	48075
R-Squared	0.010	0.053	0.009	0.051

Table 10: Intensive margin

This table examines the intensive margin of number of credit card offers received, and we limit to respondents who received at least one offer. In specification (1), the dependent variable is the number of offers, while in specifications (2)-(3), it is the number of cross-selling offers and in specifications (4)-(5), is is the number of new customer offers. In specifications (3) and (5), we limit the sample to respondents that receive a statement or notice for an existing account. In specification (6), the dependent variable is the number of credit cards the respondent owns. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

	Num. Offers	Cross-selling Offer		New Cust. Offer	
			Have an account		Have an account
	(1)	(2)	(3)	(4)	(5)
BHO	0.059**	0.016	0.026	0.043**	0.054***
	(0.023)	(0.016)	(0.022)	(0.017)	(0.021)
All Controls, FE	Yes	Yes	Yes	Yes	Yes
Observations	253827	253827	179200	253827	179200
R-Squared	0.111	0.072	0.068	0.099	0.113

Table 11: Credit supply tightening and credit card offers

This table examines banks' extension of credit card offers following higher delinquency rates. "Pct. 90+ dpd CC loans" and "Pct. 90+ dpd CC loans" are the shares of credit card loans that are at least 90 and 30 days past due, respectively. All explanatory variables are measured at a one-year lag relative to the dependent variable. In panel A, the dependent variable in specifications (1)-(2) is the log of the number of credit card offers and in specifications (3)-(4), it is the share of offers sent to BHO respondents. In Panel B, the dependent variable in specifications (1)-(2) is the share of credit card offers sent to nonprime respondents with credit score below 700 and in specifications (3)-(4), it is the share of offers sent to risky respondents, which we define as those with any of the following indicators: utilization above 90 percent, personal bankruptcy, any derogatory public records, whether the respondent was ever 90 days or more past due, and whether they were 30 days or more past due in the previous 24 months. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

Panel A: Share of offers to BHO respondents

	Log number CC offers		Pct. BHO offers	
	(1)	(2)	(3)	(4)
Pct. 90+ dpd CC loans	-11.864***		-0.589	
	(2.893)		(0.562)	
Pct. 30+ dpd CC loans		-4.535**		-0.058
		(2.141)		(0.237)
Log number CC offers	0.588***	0.597***	0.015***	0.015***
	(0.048)	(0.049)	(0.005)	(0.005)
Log assets	0.188*	0.186*	-0.013	-0.014*
	(0.107)	(0.107)	(0.008)	(0.009)
Tier1 ratio	-0.001	0.000	-0.001*	-0.001
	(0.006)	(0.006)	(0.001)	(0.001)
Advertising Expense/Assets	23.406	18.249	-5.149	-5.255
	(25.713)	(25.247)	(4.298)	(4.452)
Year FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Observations	1030	1030	1030	1030
R-Squared	0.859	0.858	0.668	0.668

Panel B: Share of offers to risky respondents

	Pct. nonprime offers		Pct. risky offers	
	(1)	(2)	(3)	(4)
Pct. 90+ dpd CC loans	-2.973***		-1.690**	
	(0.755)		(0.831)	
Pct. 30+ dpd CC loans		-1.379***		-0.447
		(0.336)		(0.418)
Log number CC offers	0.012*	0.014**	0.016**	0.018**
	(0.006)	(0.007)	(0.007)	(0.007)
Log assets	-0.012	-0.011	-0.008	-0.010
	(0.019)	(0.018)	(0.017)	(0.018)
Tier1 ratio	0.000	0.000	0.002	0.003*
	(0.001)	(0.001)	(0.002)	(0.001)
Advertising Expense/Assets	1.128	-0.383	-1.792	-2.347
	(3.514)	(3.693)	(5.287)	(5.524)
Year FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Observations	1030	1030	1030	1030
R-Squared	0.659	0.659	0.610	0.608

Table 12: Alternative channels

This table examines alternative explanations for our results. The dependent variable is whether the respondent received at least one credit card offer. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

	$I\{Offer_{i,t}\}$			
	(1)	(2)	(3)	(4)
BHO	-0.017**	-0.021***	-0.016**	-0.018***
	(0.008)	(0.006)	(0.008)	(0.006)
Pct BHO in Tract	-0.011			
	(0.010)			
BHO x Pct BHO in Tract	-0.003			
	(0.016)			
LMI Tract		-0.007		
		(0.004)		
BHO x LMI Tract		0.007		
		(0.009)		
BHO x Racially Charged Search Above Median				-0.007
				(0.009)
County FE	Yes	Yes		Yes
Tract FE			Yes	
DMA FE				Yes
All Other Controls, FE	Yes	Yes	Yes	Yes
Observations	436999	436999	436999	545567
R-Squared	0.101	0.101	0.254	0.100

Table 13: Competition and credit card offers

This table presents the correlation between the probability of receiving a credit card and local market competition. The dependent variable is whether the respondent received at least one credit card offer. CC HHI is the county-level HHI calculated using credit card offers sent in the prior year, whereas deposit HHI is calculated using deposits. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

	Share BHO			
	(1)	(2)	(3)	(4)
CC Offer HHI	-0.012			
	(0.048)			
CC Offer x Income Bin HHI		0.019		
		(0.046)		
CC Offer x CS Bin HHI			-0.003	
			(0.034)	
Share of CC Offers				0.024
				(0.035)
Lender FE	Yes	Yes	Yes	Yes
Observations	48075	48075	48075	48075
R-Squared	0.053	0.053	0.053	0.053

Table 14: The Effects of the California Consumer Privacy Act (2018)

In this table, the dependent variable is the share of offers sent to BHO consumers. The year for each campaign is the year of the first time an offer is sent as part of the campaign. The results are very similar using the year of the last time an offer is sent. In specification (4), we only include campaigns that take place within a single year. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

Dependent Variable: Share BHO

	All Campaigns			Single-year Campaigns
	(1)	(2)	(3)	(4)
CA x Post 2020	-0.084***	-0.084***	-0.077***	-0.078***
	(0.017)	(0.017)	(0.019)	(0.020)
Co-brand	-0.029***	-0.021***		
	(0.004)	(0.007)		
Pre-selected	0.006	0.004		
	(0.008)	(0.012)		
Campaign size	-0.011**	-0.008*		
	(0.005)	(0.004)		
Lender size	-0.002			
	(0.002)			
Year, Census Div. FE	Yes	Yes	Yes	Yes
Lender FE		Yes		
Campaign FE			Yes	Yes
Observations	56921	56883	46248	42041
R-Squared	0.086	0.106	0.391	0.396

A Appendix

A.1 Propensity-matched sample

Although we use granular income, credit score, household composition, and age fixed effects, this is not necessarily enough to align the distributions of the credit report variables. To address this concern, we create a propensity-matched (PSM) sample of similar White and BHO respondents.

To do this, we estimate a propensity score of a respondent being Black, Hispanic, Asian, or Other on the controls and fixed effects, and match each respondent to a White respondent requiring an exact match on credit score bin, income bin, age bin, household composition, household size, and quartiles of debt to income and log number of trades. We use nearest neighbor matching and only keep observations in the common support of the propensity score distribution and those with propensity score between 0.1 and 0.9, following Crump et al. (2009). Table A1 presents the summary statistics. Although differences between some of the categories still exist, they are much smaller than in Table 1. For example, Black respondents in the PSM sample have a credit score of only 16 points lower than White respondents, on average, while Hispanic respondents have a higher credit score. Similarly, whereas in the main sample, debt to income was higher for Black and Hispanic respondents relative to White respondents, in the PSM sample, it is lower.

Table A1: Summary statistics

This table presents summary statistics for demographic and credit report variables for our sample, broken down by self-reported race category. All demographic variables are self-reported and reflect the demographics of the head of household. Credit report variables are as of approximately 60 days prior to the survey date.

Panel A: Demographic Variables

Demographic Variables	All	White	Black	Hispanic	Asian
< $15,000	0.164	0.139	0.219	0.119	0.000
$15,000-25,000	0.112	0.108	0.130	0.092	0.000
$25,000-40,000	0.208	0.206	0.217	0.200	0.020
$40,000-60,000	0.190	0.200	0.177	0.209	0.060
$60,000-100,000	0.229	0.250	0.181	0.285	0.281
$100,000-200,000	0.091	0.095	0.075	0.093	0.464
> $200,000	0.005	0.003	0.001	0.002	0.175
Female Alone	0.233	0.211	0.302	0.142	0.205
Female Head Living with Others Related	0.148	0.138	0.174	0.131	0.029
Female Living with Others Not Related	0.021	0.025	0.017	0.026	0.053
Male Alone	0.105	0.091	0.123	0.084	0.046
Male Head Living with Others Related	0.024	0.023	0.027	0.029	0.007
Male Living with Others Not Related	0.010	0.008	0.013	0.007	0.007
Male and Female Married	0.448	0.495	0.336	0.569	0.628
Married / Domestic Partnership	0.010	0.009	0.010	0.013	0.026
Household size	2.408	2.529	2.134	2.728	2.797
Owner	0.595	0.641	0.544	0.601	0.579
Renter	0.363	0.324	0.409	0.361	0.411
Single-family home	0.703	0.732	0.672	0.730	0.472
Mobile Home	0.035	0.044	0.022	0.033	0.000
Has child	0.262	0.281	0.202	0.330	0.369
<25	0.024	0.027	0.020	0.025	0.061
25-29	0.052	0.057	0.033	0.067	0.111
30-34	0.062	0.066	0.046	0.073	0.165
25-29	0.086	0.087	0.071	0.091	0.235
40-44	0.106	0.110	0.086	0.123	0.160
45-49	0.104	0.109	0.090	0.110	0.131
50-54	0.128	0.129	0.131	0.135	0.033
55-59	0.138	0.133	0.158	0.129	0.033
60-64	0.134	0.128	0.163	0.112	0.029
65+	0.166	0.155	0.203	0.134	0.041
Observations	45462	23015	14806	9930	701

Panel B: Credit report variables

Credit Report Variables	All	White	Black	Hispanic	Asian
Credit Score	701.901	703.937	687.020	712.267	911.339
S011: Number of trades	5.049	5.349	4.599	5.231	6.357
Log Recent Debt	7.362	7.680	6.820	7.435	9.709
Has a mortgage trade	0.251	0.267	0.221	0.255	0.482
Has at least one active bankcard	0.534	0.573	0.463	0.549	0.907
Has a retail trade	0.271	0.278	0.275	0.269	0.101
Has personal finance trade	0.310	0.309	0.313	0.308	0.238
Has an active installment trade	0.443	0.457	0.431	0.456	0.083
Utilization	0.534	0.526	0.573	0.518	0.102
Debt to Income	0.825	0.864	0.748	0.826	1.046
Revolving Debt to Income	0.117	0.131	0.105	0.108	0.051
Recent inquiry	0.367	0.375	0.360	0.362	0.252
Has Personal Bankruptcy	0.108	0.120	0.092	0.109	0.006
Has Derogatory Records	0.200	0.206	0.212	0.178	0.019
Highest Delinq ever on a trade is 90+	0.492	0.477	0.538	0.468	0.058
30 dpd in past 24 mos	0.388	0.390	0.414	0.362	0.047
New File within 5y	0.066	0.066	0.065	0.074	0.081
Last updated within 1y	0.869	0.893	0.835	0.865	0.976
Received CC offer	0.376	0.408	0.330	0.380	0.524
Credit Card Offers	0.885	0.976	0.762	0.886	1.107
Observations	45462	23015	14806	9930	701

A.2 Robustness of ownership and marketing results to sample construction

Table A2 shows our results are robust to alternative samples. Panel A shows robustness for specification (5) of Table 2 and Panel B shows robustness for specification (5) of Table 3. Our results are robust to: using the Han et al. (2017) sample which excludes respondents over age 65 and those with incomes below $10,000 or above $200,000 (specification (1)); excluding rural counties (specification (2)); and using respondents observed again in the data between 3 and 6 months later, the same sample used in Section 3.3 (specification (3)); and using the Mintel-provided demographic weights to weight respondents (specification (4)).

Table A2: Robustness to alternative samples

This table presents robustness for Tables 2 and 3. In each panel, specification (1) uses the sample from Han et al. (2017), which includes only respondents aged 20-60 and with income between $10,000 and $200,000, and specification (2) excludes rural counties. Specification (3) limits the sample to respondents who are observed again between 3 and 6 months later, and specification (4) weights respondents by the Mintel-provided demographic weights. In all panels, standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

Panel A: Ownership

The dependent variable is an indicator for whether the respondents has at least one credit card.

	HKL (2017) Sample (1)	Excl Rural Co. (2)	IV Sample (3)	Weighted (4)
BHO	-0.019***	-0.026***	-0.036*	-0.028***
	(0.004)	(0.004)	(0.018)	(0.005)
All Controls, FE	Yes	Yes	Yes	Yes
Observations	665656	534018	5569	465144
R-Squared	0.420	0.399	0.563	0.427

Panel B: Marketing

The dependent variable is an indicator for whether the respondents received at least one credit card offer.

	HKL (2017) Sample (1)	Excl Rural Co. (2)	IV Sample (3)	Weighted (4)
BHO	-0.016***	-0.020***	-0.031	-0.020***
	(0.004)	(0.005)	(0.026)	(0.006)
All Controls, FE	Yes	Yes	Yes	Yes
Observations	665656	534018	5569	465144
R-Squared	0.105	0.097	0.283	0.097

A.3 Gaps in marketing by race are not driven by particular lenders or mailing types

Table A3 shows the racial gap in marketing exists across lender and mailing types.

Panel A splits offers by lender size (specifications 1-4), and shows that BHO respondents are less likely to receive offers from both small and large banks. Specifications (5)-(6) split offers by whether the lender has a branch in the same county. Relationship banking literature suggests branches facilitate customer interactions (Anenberg et al., 2018). Banks might therefore send more

offers where they have branches and existing customer relationships. However, BHO respondents are less likely to receive offers from both lenders with nearby branches and out-of-county lenders.

Panel B splits mailing by offer type. Specifications (1)-(2) examine the likelihood of receiving a pre-selected offer (screened on credit report information) or a general offer (same offer as available on the lender's website). In specifications (3)-(4), the dependent variable is receipt of a co-branded offer or non-co-branded offer, respectively. BHO respondents are less likely to receive all offer types. Specification (5) shows that the results of specification (5), Table 3, are similar when excluding non-personalized mass-mailings and specification (6) reruns the analysis limiting to the main household respondent who answered demographic questions.

Table A3: Robustness of findings on gaps by race in marketing

Panel A: Types of lenders

Panel A presents the results for different subsets of lenders. In specifications (1) and (2), we split lenders by whether they have assets greater than $100B. In specifications (3)-(4), we split lenders into the top 6 credit card lenders (JP Morgan Chase, Capital One, American Express, Citigroup, Bank of America, and Discover). In specification (5)-(6), we split the the dependent variable by whether the offer received was from lender with or without local branches in the same county.

	Bank Size				Nearby Branch	
	(1)	(2)	(3)	(4)	(5)	(6)
BHO	-0.018***	-0.009***	-0.018***	-0.006**	-0.010***	-0.016***
	(0.005)	(0.003)	(0.005)	(0.003)	(0.003)	(0.004)
All Controls, FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	558813	558813	558813	558813	558813	558813
R-Squared	0.090	0.097	0.109	0.081	0.094	0.104

Panel B: Types of mailings

In specifications (1), the dependent variable is an indicator for whether the respondent received a pre-selected offer; in specification (2), it is a general offer. Specification (3) and (4) use indicators for co-brand and not co-brand offers, respectively. Specification (5) excludes non-personalized offers, while in specification (6), we limit the sample just to respondents who are heads of household, since the demographic controls for the head of household are used each household member.

	Mailing Type					
	Prescreened	General	Cobrand	Not Cobrand	Excl. Non-personalized	Head of HH
	(1)	(2)	(3)	(4)	(5)	(6)
BHO	-0.009**	-0.014***	-0.009***	-0.017***	-0.021***	-0.026***
	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)	(0.006)
All Controls, FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	558813	558813	558813	558813	558813	292727
R-Squared	0.090	0.079	0.064	0.095	0.101	0.110

Table A4 addresses concerns that BHO respondents receive less physical mail generally.

Specification (1) uses an indicator for receiving non-financial mail (phone, wireless, shipping, travel, or retail advertisements) as the dependent variable. Specification (2) examines whether respondents with at least one credit card received spending, balance transfer, or reduced rate offers on existing cards. If BHO respondents receive fewer mailed credit card offers , they would also be less likely to receive other mail or offers. In both specifications, the coefficient on BHO is a precisely estimated zero.[36]

Specifications (3)-(4) find similar racial gaps for small business credit cards and bank account offers. The latter is particularly notable: deposit accounts carry minimal risk— of mainly fraud, not default—suggesting unobservable risk differences do not drive our results.

Table A4: Other Mailings

In specification (1), the dependent variable is an an indicator for whether the respondent received a non-financial mailed offer. In specification (2), it is an indicator for whether the respondent received a credit card statement or notice. In specifications (3)-(4), the dependent variables are whether the respondent received a business credit card offer or bank offer, respectively.

	Non-Fin. Offer	Notice	Bus. CC	Bank
	(1)	(2)	(3)	(4)
	(1)	(2)	(3)	(4)
BHO	0.001	-0.002	-0.004***	-0.011***
	(0.003)	(0.003)	(0.001)	(0.002)
All Controls, FE	Yes	Yes	Yes	Yes
Observations	558813	426948	558813	558813
R-Squared	0.075	0.052	0.035	0.067

A.4 Differences in marketing by race and fair lending laws

Credit card lenders generally do not observe potential customers' race and may not be aware of the differences in Table 3. Even if aware, these differences may not violate fair lending laws. While lenders cannot consider prohibited classes when extending credit under the disparate treatment and impact provisions of the Equal Credit Opportunity Act of 1974 (ECOA) §202.4(b), whether these rules apply to credit card marketing is unclear, as most of §202.4(b) concerns borrowers and applicants Skanderson and Ritter (2014).

The Official Staff Interpretation to ECOA §202.4(b) states the section "covers acts or practices directed at prospective applicants that could discourage a reasonable person, on a

[36] A related concern is that BHO households may opt out of receiving offers more frequently. Under the Fair Credit Reporting Act, consumers can opt out of pre-screened credit card and insurance offers (both simultaneously). BHO respondents are more, not less, likely to receive mailed insurance offers, suggesting differential opt-out does not drive our results.

prohibited basis, from applying for credit." However, examples focus on active discouragement: statements that applicants should not apply, advertising that expresses discriminatory preferences, and interview scripts that discourage applications on prohibited bases.[37]

A.5 Comparison with Firestone (2014)

The closest paper to ours is Firestone (2014), who examines Mintel data for 5 large lenders during the August 2009 to October 2010 period and finds a much larger effect of race and ethnicity on the probability of a consumer receiving an offer: "Blacks were approximately 27% less likely to receive offers from credit card lenders during the sample period...[and] Hispanics were 17% less likely to receive an offer, after including controls."

Table A5: Comparison with Firestone (2014)

This table presents the comparison between Firestone (2014) and our results. Specifications (1)-(4) run a logit regression using state and month fixed effects. In specification (1), we replicate the results of Firestone (2014). In specification (2), we extend the sample to the full 2007-2023 period, while in specification (3), we limit to respondents with credit report information. In specification (4), we use the same set of fixed effects we use throughout the paper. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

	Logit			OLS
	Firestone Sample 2009-2010	Firestone Sample 2007-2023	Full Sample 2007-2023	Full Sample 2007-2023
Black	-0.205***	-0.123***	-0.118***	-0.021***
	(0.056)	(0.016)	(0.014)	(0.005)
Hispanic	-0.166***	-0.061***	-0.052***	-0.014**
	(0.050)	(0.015)	(0.015)	(0.006)
Marginal Effect: Black	-0.041	-0.026	-0.025	-0.021
	(.011)	(.003)	(.003)	(.005)
Marginal Effect: Hispanic	-0.033	-0.013	-0.011	-0.014
	(.01)	(.003)	(.003)	(.006)
State, Month FE	Yes	Yes	Yes	
All FE				Yes
Observations	53562	467939	558814	558813
R-Squared	0.099	0.076	0.075	0.100

Differences between our results and Firestone (2014)stem from interpretation and sample construction. First, we report coefficients from a linear probability model, while Firestone's main specification uses a logit model. The 27% and 17% effects cited above are odds ratios, not

[37]The Official Staff Interpretation also explicitly allows lenders to "affirmatively solicit or encourage members of traditionally disadvantaged groups to apply for credit," but is silent on lenders doing the converse. See Regulation B, 12 C.F.R. §202, Official Staff Interpretations to §202.4(a).

marginal effects, and are not directly comparable to our estimates. Second, Firestone examines only August 2009-October 2010, when lenders were recovering from the Great Recession, and includes respondents not matched to credit reports, assuming they have no tradelines or debt.[38]

Table A5 shows that Firestone's larger magnitudes are driven by sample period and the inclusion of unmatched respondents. Specification (1) replicates Firestone's findings using a logit regression with the top 6 banks, the August 2009-October 2010 period, and the controls used in Firestone (2014): quartiles of utilization, debt-to-income, revolving debt-to-income, months since delinquency, indicators for completed education, age, age squared, and indicators for personal bankruptcy and renter status. Specification (2) uses the full 2007-2023 period; specification (3) excludes respondents without credit reports; specification (4) uses OLS for comparison and our preferred set of controls. .

Marginal effects from Firestone's regressions are somewhat larger than ours for 2009-2010 but attenuate over 2007-2023. Excluding unmatched respondents yields marginal effects very similar to ours, with essentially no difference between logit and OLS estimation.

[38]There is no way to distinguish between respondents without a credit report and respondents that have a credit report but were not matched to it during the matching process with credit bureau data.

A.6 IV Robustness

Table A6: Marketing Campaigns as IV: First Stage

This table presents the first stage of the IV regressions shows in Table 4. Errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

	$I\{Offer_{i,t}\}$	BHO x $I\{Offer_{i,t}\}$
	(1)	(2)
Log CC Campaigns	0.204***	0.007***
	(0.004)	(0.002)
BHO x Log CC Campaigns	-0.007**	0.133***
	(0.003)	(0.010)
All Controls, FE	Yes	Yes
Observations	99094	99094
R-Squared	0.214	0.456

Table A7: Selection and Respondent Characteristics

Panel A: Correlations with Economic and Financial Characteristics

In this panel, we regress financial characteristics on our instrument, Log CC Campaigns. In specification (2), the growth in income is measured from the first time the respondent, t, is observed to the second, t'. We limit to the sample of respondents whose credit records are observed between 0 to 6 months after their credit card offers are observed. Specifications (3) and (4) use as the dependent variable the credit score and an indicator for whether the borrower is risky, respectively. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

	Income (1)	Growth in Income (2)	Growth in Credit Score (3)	I{Risky} (4)
Log CC Campaigns	173.585	16.674	-2.585***	0.001
	(295.355)	(36.031)	(0.436)	(0.001)
Demographic Controls	Yes	Yes	Yes	Yes
Income FE	Yes	Yes		Yes
Credit Score FE		Yes	Yes	Yes
Other FE	Yes	Yes	Yes	Yes
Observations	99094.000	99094.000	95181.000	99094.000
R-Squared	0.492	0.066	0.688	0.804

Panel B: New Credit Cards and Selection

In this panel, we limit to respondents who are observed less than 3 months after the first observation. Because the credit report variables are lagged, for this sample, the new credit cards are opened before the offer $I\{Offer_{i,t}\}$ is received. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

	$I\{Open_{i,t}\}$		$I\{CC_{i,t'}\}$	
	OLS (1)	IV (2)	OLS (3)	IV (4)
$I\{Offer\}$	0.004**	-0.001	0.022***	0.017
	(0.002)	(0.006)	(0.004)	(0.013)
CC Controls	Yes	Yes		
Other Controls	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes
Observations	49012	49012	49012	49012
R-Squared	0.058	0.009	0.474	0.181

A.7 Other Robustness Results

Table A8: Intensive Margin Robustness

This table presents robustness results for Table 10 using Poisson pseudo-likelihood regression in all specifications. Standard errors are shown in parentheses and are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

	Num. Offers	Cross-selling Offer		New Cust. Offer	
			Have an account		Have an account
	(1)	(2)	(3)	(4)	(5)
BHO	0.025**	0.034	0.040	0.022**	0.027**
	(0.010)	(0.036)	(0.035)	(0.009)	(0.011)
All Controls, FE	Yes	Yes	Yes	Yes	Yes
Observations	253827	252631	178594	253827	179198
R-Squared	0.047	0.091	0.064	0.043	0.052

B. Example Consumer Marketing Information Report

Demographics and Identifiers

01. Personal Identifiers / 01. Name / Name (005)
01. Personal Identifiers / 01. Name / Name (006)
01. Personal Identifiers / 01. Name / Name (007)
01. Personal Identifiers / 02. Address / Address (001)
01. Personal Identifiers / 02. Address / Address (001) / Description (001)
01. Personal Identifiers / 02. Address / Address (001) / Description (002)
01. Personal Identifiers / 02. Address / Address (002)
01. Personal Identifiers / 02. Address / Address (002) / Description
01. Personal Identifiers / 02. Address / Address (002) / Last Verified Date (001)
01. Personal Identifiers / 02. Address / Address (002) / Last Verified Date (002)

01. Personal Identifiers / 02. Address / Address (006)	[redacted]	01. Personal Identifiers / 10. Other Identifiers / IP Address (056)	69.112.168.242 (Last Activity Date:20240417)
01. Personal Identifiers / 02. Address / Address (006) / Description	Current	01. Personal Identifiers / 10. Other Identifiers / IP Address (057)	69.140.248.196 (Last Activity Date:20230618)
01. Personal Identifiers / 02. Address / Address (006) / Distance of Move	0.18	01. Personal Identifiers / 10. Other Identifiers / IP Address (058)	70.106.202.173 (Last Activity Date:20230516)
01. Personal Identifiers / 02. Address / Address (006) / Last Verified Date (001)	20231218	01. Personal Identifiers / 10. Other Identifiers / IP Address (059)	70.240.205.53 (Last Activity Date:20230430)
01. Personal Identifiers / 02. Address / Address (006) / Last Verified Date (002)	202011	01. Personal Identifiers / 10. Other Identifiers / IP Address (060)	71.178.177.237 (Last Activity Date:20230922)

Financial Information and Inferences

02. Personal Characteristics / 17. Financial Information / Income Range (004)	$175,000-$199,999
02. Personal Characteristics / 17. Financial Information / Income Range - Household	$100,000-$149,999
02. Personal Characteristics / 74. Inferences-Financial Information / Net Worth Household	$100,000-$249,999
03. Personal Property or Buying Activity|Interest / 11. Personal Attributes / Household - Relationship	Male Head of Household
03. Personal Property or Buying Activity|Interest / 17. Financial Information / Credit - High Credit Range	700-749
03. Personal Property or Buying Activity|Interest / 17. Financial Information / Credit Card - Owner - Bank	Yes
03. Personal Property or Buying Activity|Interest / 17. Financial Information / Credit Card - Owner - General	Yes

10. Inferences / 72. Inferences-Email Information / Household - Deliverability (005)

10. Inferences / 72. Inferences-Email Information / Household - Open Propensity (001)

10. Inferences / 72. Inferences-Email Information / Household - Open Propensity (002)

10. Inferences / 74. Inferences-Financial Information / Ability to Pay - Household	889 (1=Lowest to 1000=Highest)
10. Inferences / 74. Inferences-Financial Information / Ability to Pay - Household / Ability to Pay - Household - Assignment Level	Household Level Match
10. Inferences / 74. Inferences-Financial Information / Credit Card - Balance Transfers	0 (0=Lowest to 5000000=Highest)
10. Inferences / 74. Inferences-Financial Information / Credit Card - Likely to Respond	51 (0=Lowest to 100=Highest)
10. Inferences / 74. Inferences-Financial Information / Credit Card - Likely to Respond - High	83 (0=Lowest to 100=Highest)
10. Inferences / 74. Inferences-Financial Information / Credit Card - Likely to Respond - Low Interest Rate	6595 (0=Lowest to 100=Highest)
10. Inferences / 74. Inferences-Financial Information / Credit Card - Low Delinquent Risk	True

10. Inferences / 75. Inferences-Buying Activity Behavior / Household Dollars Spent - Apparel (007)

10. Inferences / 75. Inferences-Buying Activity Behavior / Household Dollars Spent - Apparel (008)

10. Inferences / 75. Inferences-Buying Activity Behavior / Household Dollars Spent - Dining Out (001)

10. Inferences / 75. Inferences-Buying Activity Behavior / Household Dollars Spent - Dining Out (002)

10. Inferences / 75. Inferences-Buying Activity Behavior / Household Dollars Spent - Dining Out (003)

10. Inferences / 74. Inferences-Financial Information / Mortgage - Likely to Respond - First Time	652 (0=Lowest to 1000=Highest)
10. Inferences / 74. Inferences-Financial Information / Mortgage - Likely to Respond - New	652 (0=Lowest to 1000=Highest)
10. Inferences / 74. Inferences-Financial Information / Mortgage - Likely to Respond - New and First Time	1303 (0=Lowest to 2000=Highest)
10. Inferences / 74. Inferences-Financial Information / Mortgage - Likely to Respond - Refinance	591 (0=Lowest to 1000=Highest)
10. Inferences / 74. Inferences-Financial Information / Net Worth Household (001)	[redacted]
10. Inferences / 74. Inferences-Financial Information / Net Worth Household (002)	[redacted]

10. Inferences / 75. Inferences-Buying Activity Behavior / Monthly Likelihood to Spend Retail - Costco - Offline	30 (000=Unlikely; 001=Most Likely to 100=Least Likely)
10. Inferences / 75. Inferences-Buying Activity Behavior / Monthly Likelihood to Spend Retail - Costco - Online	28 (000=Unlikely; 001=Most Likely to 100=Least Likely)
10. Inferences / 75. Inferences-Buying Activity Behavior / Monthly Likelihood to Spend Retail - Costco - Online|Offline - Total Spend	29 (000=Unlikely; 001=Most Likely to 100=Least Likely)
10. Inferences / 75. Inferences-Buying Activity Behavior / Monthly Likelihood to Spend Retail - Crate & Barrel	25 (000=Unlikely; 001=Most Likely to 100=Least Likely)
10. Inferences / 75. Inferences-Buying Activity Behavior / Monthly Likelihood to Spend Retail - Cumberland Farms	81 (000=Unlikely; 001=Most Likely to 100=Least Likely)
10. Inferences / 75. Inferences-Buying Activity Behavior / Monthly Likelihood to Spend Retail - DSW	12 (000=Unlikely; 001=Most Likely to 100=Least Likely)

Other Inferences

03. Personal Property or Buying Activity|Interest / 57. Lifestyle Interest / Gourmet (001)	5 (1=Lowest to 9=Highest)
03. Personal Property or Buying Activity|Interest / 57. Lifestyle Interest / Gourmet (002)	6 (1=Lowest Interest to 9=Highest Interest)
03. Personal Property or Buying Activity|Interest / 57. Lifestyle Interest / Home Decorating Furnishings (001)	3 (1=Lowest to 9=Highest)
03. Personal Property or Buying Activity|Interest / 57. Lifestyle Interest / Home Decorating Furnishings (002)	4 (1=Lowest Interest to 9=Highest Interest)
03. Personal Property or Buying Activity|Interest / 57. Lifestyle Interest / Home Living (001)	3 (1=Lowest to 9=Highest)
03. Personal Property or Buying Activity|Interest / 57. Lifestyle Interest / Home Living (002)	3 (1=Lowest Interest to 9=Highest Interest)
03. Personal Property or Buying Activity|Interest / 57. Lifestyle Interest / Internet (001)	6 (1=Lowest to 9=Highest)

03. Personal Property or Buying Activity|Interest / 60. Buying Activity Behavior-Books and Magazines / Magazine Subscriber - News and Financial	Yes
03. Personal Property or Buying Activity|Interest / 60. Buying Activity Behavior-Books and Magazines / Magazine Subscriber - Sports	Yes
03. Personal Property or Buying Activity|Interest / 60. Buying Activity Behavior-Books and Magazines / Purchase Date - Date Last Known - Books or Magazines	[redacted]
03. Personal Property or Buying Activity|Interest / 63. Buying Activity Behavior-Food / Number of Unique Companies Purchased from - Food	2
03. Personal Property or Buying Activity|Interest / 64. Buying Activity Behavior-General / Last Dollars Amount Purchased	[redacted]

10. Inferences / 65. Buying Activity Behavior-Health and Beauty / Diet Lifestyle	7 (1=Very Poor to 7=Very Healthy)
10. Inferences / 71. Inferences-Models / Alcohol Consumption	7 (1=None to 7=Heavy)
10. Inferences / 71. Inferences-Models / Body Mass	1 (6=High BMI to 1=Low BMI)
10. Inferences / 71. Inferences-Models / Data Received Date	20230406
10. Inferences / 71. Inferences-Models / Data Received Date / Charitable Contributions Decile (001)	4 (1=Lowest to 10=Highest)
10. Inferences / 71. Inferences-Models / Data Received Date / Charitable Contributions Decile (002)	9 (1=Lowest to 10=Highest)

10. Inferences / 71. Inferences-Models / Data Received Date / Life Insurance Purchase Propensity Rank (002)
10. Inferences / 71. Inferences-Models / Data Received Date / Vacation Property Ownership Propensity Decile (001)
10. Inferences / 71. Inferences-Models / Data Received Date / Vacation Property Ownership Propensity Decile (002)

10. Inferences / 71. Inferences-Models / Exercise Frequency (002)
10. Inferences / 71. Inferences-Models / Extreme Sports Enthusiasts
10. Inferences / 71. Inferences-Models / Fast Food|Snack Food Consumption (001)

10. Inferences / 71. Inferences-Models / Fast Food|Snack Food Consumption (003)
10. Inferences / 71. Inferences-Models / Healthy Lifestyle
10. Inferences / 71. Inferences-Models / Monthly Likelihood to Engage Online - Arts & Entertainment: Music & Audio
10. Inferences / 71. Inferences-Models / Monthly Likelihood to Engage Online - Arts & Entertainment: Music & Audio - Religious Music
10. Inferences / 71. Inferences-Models / Monthly Likelihood to Engage Online - People & Society: Social Issues & Advocacy
10. Inferences / 71. Inferences-Models / Overall Wellness (001)
10. Inferences / 71. Inferences-Models / Overall Wellness (002)