Essays on Household Finance and Credit Market Regulation

by

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Abstract

This thesis consists of three chapters on household finance and regulatory policy in consumer credit markets.

The first chapter studies the efficiency and distributional effects of credit card pricing restrictions in the 2009 Credit CARD Act. I document how two forces drive these restrictions' effects: first, the Act constrains lenders from adjusting interest rates in response to new information about default risk, which exacerbates adverse retention of risky borrowers and induces partial market unraveling on new accounts; second, the Act constrains lenders from pricing private information about demand, which reduces markups on inelastic borrowers. I develop a structural model of the US credit card market to study how heightened information problems and lower markups interact in equilibrium to determine the Act's effects. I find that equilibrium market unraveling is most severe for subprime consumers, but the reduction in markups is substantial throughout the market, so that on net, the Act's restrictions allow consumers of all credit scores to capture higher surplus on average. Total surplus inclusive of firm profits rises among prime consumers, whereas gains in subprime consumer surplus are greatest among borrowers who were recently prime.

The second chapter (co-authored with Alexander Bartik) also studies the regulation of credit market information, focusing on the use of such information in labor markets. In particular we study recent bans on employers' use of credit reports to screen job applicants. This practice has been popular among employers but controversial for its perceived disparate impact on racial minorities. Exploiting geographic, temporal, and job-level variation in which workers are covered by these bans, we analyze these bans' effects in two datasets: the panel dimension of the Current Population Survey (CPS); and data aggregated from state unemployment insurance records. We find that the bans reduced job-finding rates for blacks by 7 to 16 percent, and increased subsequent separation rates for black new hires by 3 percentage points. Results for Hispanics and whites are less conclusive. We interpret these findings in a statistical discrimination model in which credit report data, more for blacks than for other groups, send a high-precision signal relative to the precision of employers' priors.

The third chapter (co-authored with Sydnee Caldwell and Daniel Waldinger) returns to consumer credit markets and studies determinants of household borrowing behavior. Many economic models predict that consumption and borrowing decisions today depend on beliefs about risky future income. We quantify one contributor to income uncertainty and study its effects: uncertainty about annual tax refunds. In a low-income sample for whom tax refunds can be a substantial portion of income, we collect novel survey evidence on tax filers' expectations of and uncertainty about their tax refunds; we then link these data with administrative tax data, a panel of credit reports, and survey-based consumption measures. We find that while many households have correct mean expectations about their refunds, there is substantial, and accurately reported, subjective uncertainty. Households borrow moderate amounts out of expected tax refunds: for each dollar of expected refund, roughly 15 cents in revolving debt is repaid after refund receipt. This borrowing and repayment is less pronounced for more uncertain households, consistent with precautionary behavior. The unexpected component of tax refunds is not used to pay down debt, but rather induces higher debt levels. Credit report and survey evidence both suggest that these higher debt levels are driven by newly financed durable purchases such as vehicles.

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Contents

Chapter 1

Private Information and Price Regulation in the US Credit Card Market*

1.1 Introduction

Lenders typically learn new information about their borrowers over time. What are the consequences of regulation that restricts how lenders use such information for loan pricing? And what does this reveal about the role of such information in credit markets?

I study these questions in the context of the US credit card market and the Credit Card Accountability Responsibility and Disclosure (CARD) Act of 2009. The CARD Act restricted lenders' ability to raise credit card borrowers' interest rates over time and also restricted fees that could otherwise substitute for such interest rate increases. Lenders therefore became substantially less able to adjust borrowers' interest rates and other price dimensions in response to learning new information about borrowers.

Understanding the effects of the CARD Act's price restrictions is important both because of these restrictions' economic interest and because of the credit card market's central role in the US consumer credit landscape. Among the estimated 85 million US households with credit cards, roughly 60% use credit cards for at least occasional borrowing, and credit card holders collectively have access to over $3 trillion in open credit lines. Reliance on credit cards for borrowing is especially pronounced for less credit-worthy consumers, among whom the share of accounts used for at least occasional borrowing exceeds 90%. Credit card regulation

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is therefore important both for its distributional effects as well as for its implications for the efficient provision of consumer credit more generally.

In this paper, I quantify the distributional and efficiency consequences of the CARD Act's price restrictions. To understand these effects, I analyze two channels through which informational restrictions on pricing can influence credit market outcomes. First, if lenders learn over time about borrower demand, the CARD Act's price restrictions may limit lenders' ability to extract rents from inelastic borrowers. Second, such restrictions may also limit lenders' ability to adjust prices for risk, and the CARD Act may therefore exacerbate adverse selection and induce either partial or complete market unraveling. The interplay of these two channels may cause interest rates to fall for some consumers and rise for others. Total welfare may also either rise or fall.

I study these effects using two large administrative datasets. The first contains monthly account-level data from the near-universe of US credit card accounts, spanning the period before and after the CARD Act. These data have detailed price measures including both interest rates paid and fees incurred, as well as measures of outstanding consumer debt, new borrowing, and repayment. The second dataset is a large, randomly sampled panel of US consumer credit reports, also spanning the period before and after the CARD Act. These credit report data reveal patterns that cannot be measured in the account-level data – for example, which consumers are *not* credit card holders at any given time.

I first present new facts about how credit card pricing changed with the implementation of the CARD Act. I show that the class of interest rate increases restricted by the Act affected over 50% of borrowing accounts annually prior to the CARD Act, but this rate of incidence dropped to nearly zero once the Act took effect. The elimination of these interest rate increases had immediate effects on the price distribution: as lenders became unable to discretionarily raise some borrowers' interest rates, price dispersion (as measured by the inter-quartile range of interest rates) on new cohorts of mature accounts dropped immediately by approximately one third. The *bottom* of the price distribution was also compressed, albeit not immediately: within credit score, the bottom quartile of interest rates rose over time relative to the mean by over 100 basis points for most prime borrowers, and by over 200 basis points for subprime borrowers. The credit score segments that saw the greatest increase in the left tail of the price distribution also experienced the greatest rates of consumer exit. This is consistent with (partial) market unraveling as the market shifted toward greater pooling.

These results illustrate the complexity of assessing the CARD Act's equilibrium effects. Restrictions on increasing interest rates may bring lower prices to some borrowers, while other borrowers' prices will rise as they are pooled with their peers. At the same time, these relative price effects may change the composition of borrowers in the market, further shifting how lenders set rates. Tracing these effects is further complicated by the large number of contemporaneous shocks that were affecting consumer credit markets when the Act took effect in 2009. Moreover, the Act contained many *non*-price regulations as well, including additional disclosure requirements, simpler billing procedures, and "nudges" for borrowers to repay their balances.

With these empirical features in mind, I develop and estimate a detailed structural model of the credit card market to use as a tool for studying the CARD Act's price restrictions' effects. I estimate the model on the *pre*-CARD-Act equilibrium observed in the market. I then impose the CARD Act's price restrictions in the model and analyze their effects for different types of consumers and for total welfare overall. Consequently, this exercise speaks to how the market re-equilibrates in the presence of the CARD Act's price restrictions in isolation from other coincident shocks in consumer credit markets as well as other, non-price regulations contained in the Act.

In building the model, I begin with a pair of reduced-form analyses of the key forces driving the CARD Act price restrictions' effects. The first of these analyses shows that the Act changed how the credit card market prices risk, and that these changes led to the adverse retention of risky borrowers over time. I show that prior to the CARD Act, interest rates were strongly responsive to changes in risk, as observed through changes in credit score after origination. In fact, the price gradient of these interest rate changes (as measured in interest rate basis points per point of credit score) was nearly identical to the price gradient of risk observable at the time of origination. In that sense, there was a single average price of risk in the market, which applied equally to risk at origination and risk that emerged over time. In contrast, I find that after the CARD Act, interest rates were less responsive to changes in risk, so that a sizable gap emerged between these two gradients. Newly emergent risk became nearly 75% cheaper for a borrower, per FICO score point, than risk observable at origination. Examining how these relative price effects changed the selection of consumers into and out of borrowing, I estimate that for every one percentage point reduction in interest rates charged to newly risky borrowers, these borrowers responded with a 0.7 percentage point decrease in quarterly attrition rates – a sizable effect given that average attrition rates range from 10 to 15% per quarter.

The second of these reduced-form analyses highlights that the Act also restricted lenders from adjusting interest rates in response to new information about borrowers' price sensitivity. I find that two of the borrower behaviors that most commonly triggered interest rate increases – late payments of less than 30 days, and transactions in excess of a borrower's credit limit – were used as signals that borrowers were price inelastic, and that lenders then levied price increases on these accounts to earn *higher* returns than they earned on other, identically risky accounts. In contrast, after the introduction of the CARD Act's restrictions, lenders' excess returns on these accounts were either eliminated or sharply reduced, depending on the signal and the credit-worthiness of the borrower. The Act thus made it difficult for lenders to extract rents in response to signals of relative price inelasticity, leading to a decline in rents from inelastic borrowers.

A reduced-form decomposition shows that signals of borrower price inelasticity drove the majority of interest rate increases on prime accounts, while the majority of interest rate increases on subprime accounts were in response to signals of borrower default risk. A similar decomposition holds for fee revenue. This decomposition suggests the possibility that the

CARD Act's price restrictions mostly led to lower lender rents among prime accounts, and mostly exacerbated information problems through unpriced risk among subprime accounts. However, caution is warranted in relying only on this reduced-form decomposition: since consumers' credit scores change over time, information asymmetries on subprime accounts can affect prime borrowers' rates, and even a small amount of unpriced risk can lead to severe market unraveling. This further motivates my use of a model that can predict how the market re-equilibrates overall in order to study these restrictions' effects.

The structural model features consumers with time-varying risk, differentiated lenders who acquire private information about borrowers over time, and flexible correlation between borrower risk and demand. In estimating the model, I estimate several key parameters related to the workings of the US credit card market that, to my knowledge, are not available in previous academic work. I use a novel source of quasi-experimental price variation – occasional, portfolio-wide repricing by certain lenders – to estimate borrowers' sensitivities to price. I find that riskier borrowers are less price elastic, consistent with the market being adversely selected. I also provide estimates of the extent to which lenders possess private information about their borrowers' preferences and risk. I find that such private information plays an important role in the credit card market, as my measure of lender private information is nearly as predictive of subsequent default (in per-standard-deviation terms) as borrower credit scores. Other estimates on the demand side of the model indicate that consumers' set-up costs for opening new credit card accounts are relatively high, consistent with only a subset of consumers taking advantage of promotional or "teaser" interest rates by refinancing balances with new credit cards. Finally, on the supply side of the market, the estimates of lender costs recovered from first-order conditions in the model match closely to industry reports of these costs – for example, the cost of marketing and customer acquisition for new credit card accounts.

After thus estimating the model on the observed pre-CARD-Act equilibrium, I impose the CARD Act's price restrictions in the model and study how the market responds. Specifically, I study the new equilibrium that emerges when lenders best-reply to each other under a new regulatory regime that does not allow them to change a borrower's price of borrowing over time, except through promotional or "teaser" rates that were still allowed under the Act.

The results of this exercise reveal a number of interrelated effects of the CARD Act's price restrictions. First, induced adverse selection is most severe among subprime accounts, while reduced lender rents are evident throughout the market. These patterns are consistent with the results of the earlier reduced-form decomposition. On net, average transacted prices fall throughout the market. This reflects, in part, that many borrowers who previously could access the cheapest credit within their credit score segment face higher prices and exit. Therefore the higher prices they would have faced are censored from transacted prices. This type of partial unraveling is especially pronounced among subprime consumers. Nonetheless, given the importance of lower prices for consumers with the strongest demand for credit, consumer surplus rises throughout the market. Among subprime consumers, the rise in consumer surplus is mostly offset by a fall in lender profits; among prime consumers, total surplus rises. Much of this surplus gain is due to the insurance value of these restrictions for consumers whose credit scores deteriorate over time. While this insurance is most relevant for prime borrowers, it also affects the interpretation of surplus gains among subprime borrowers. The subprime borrowers who benefit most are those whose credit score has recently fallen below prime, since these restrictions allow them to retain favorable pricing from loans originated at prime scores. In contrast, subprime borrowers looking to open a new credit card – for example, a young borrower or a long-time subprime consumer – feel the effects of market unraveling more severely.

This paper makes a number of contributions relative to existing literature. Agarwal et al. (2014) also study how the Act affected credit card pricing and find that a measure of the average, fee-inclusive cost of borrowing fell on consumer credit cards after the Act, among the set of consumers who still chose to borrow on credit cards. The primary differences between their study and this paper is that I focus on the mechanisms of how dynamic pricing regulation can affect equilibrium prices by restricting what information is possible to price; I study rich heterogeneity of price effects across borrower types, including borrowers who chose to leave the market after the Act; and I isolate the effects of the Act's price regulation separately from other omnibus regulation included in the Act, for example restrictions that aimed to make interest charge calculations and due dates more transparent for consumers. Additionally, I use a starkly different empirical strategy: while Agarwal et al. (2014) use business credit cards as a comparison group in a difference-in-differences strategy, I use a combination of reduced-form analyses that leverage cross-account comparisons, and a detailed structural model to help study the Act's effects in isolation from other contemporaneous shocks in financial markets. Other research on the CARD Act includes Keys and Wang (2016), who study the Act's "nudges" for borrowers to pay more than their minimum required payment each month, Jambulapati and Stavins (2014) and Santucci (2015), who describe patterns of account closures and credit line changes coinciding with the Act and the Great Recession, Debbaut et al. (2016), who focus on the Act's particular restrictions to protect young borrowers, and Han et al. (2015), who compare credit cards' with other financial products' direct-mail offers before and after the CARD Act to conclude, consistent with my results on partial market unraveling among subprime accounts, that the Act partially curtailed supply among subprime credit cards.[1]

This paper also joins a long literature examining the competitiveness of, and sources of market power in, the credit card industry. After seminal work by Ausubel (1991) showed credit card lenders tended not to pass through changes in the cost of funds to their borrowers,[2] a number of papers explored whether and why the industry may be imperfectly competitive,

[1]There is also a small body of theoretical work focused on the CARD Act's price restrictions in particular, including Hunt and Serfes (2013) and Pinheiro and Ronen (2015), who present theoretical models of the effects of repricing restrictions, and some research on restrictions to credit card interest rate increases in the law literature (Levitin (2011) and Bar-Gill and Bubb (2011)).

[2]See Grodzicki (2012) for evidence on how the patterns identified in Ausubel (1991) have become less pronounced in more recent data.

including for reasons of search costs (Berlin and Mester (2004)), consumer irrationality (Brito and Hartley (1995)),[3] and adverse selection for firms that cut prices (Stavins (1996)). My work integrates many of these potential sources of market power in a single model – including switching costs across firms, adverse selection, as well as lender private information – and provides an estimation framework that helps identify the relative importance of each of these. My results on the particular importance of switching costs across firms join a growing recent literature on the importance of switching costs in selection markets, including Handel (2013) and Illanes (2016).

I also provide new evidence on consumer demand for credit card borrowing and how consumers respond to changes in their terms of credit. To date, much of the research on this front has focused on how spending or borrowing responds to changes in credit limits (Gross and Souleles (2002), Agarwal et al. (2015b), and Gross et al. (2016)), and how credit limits affect consumers' holdings of cash on hand (Telyukova and Wright (2008) and Fulford (2015)). In contrast to this work on credit limits, research on how borrowers respond to interest rates and fees has been more limited.[4] To help fill this gap, I estimate borrower price elasticities across a range of borrower risk types, and also estimate primitives of a rich demand model – including switching costs, liquidity costs, and disutility from price – that predict how price elasticities change non-locally as pricing changes.[5] Estimates of these primitives help not just for understanding the CARD Act's price restrictions, but for other applied work in the credit card market as well.

This paper is organized as follows. In Section 1.2, I provide background on the credit card market, the CARD Act and the two datasets that I use in my analysis. I also present summary statistics from these datasets to highlight key changes in the credit card market around the implementation of the Act. In Section 1.3, I report reduced-form analyses of how lenders used CARD-Act-restricted repricing prior to the Act and how the market responded to the implementation of the Act. I develop and estimate my model of the credit card market in Section 1.4. Section 1.5 presents results from using the model to study how the CARD Act's pricing restrictions affect prices, borrowing and welfare in equilibrium. Section 1.6 concludes.

[3]Research on behavioral consumers in the credit card market has remained quite active, including work by Angeletos et al. (2001), DellaVigna and Malmendier (2004), Grubb (2009), Heidhues and Koszegi (2010), Heidhues and Kőszegi (2015), and Ru and Schoar (2016). Related work focuses on how consumers learn over time how to avoid apparent mistakes with credit cards (Agarwal et al. (2008), Agarwal et al. (2009)), and how the probability of mistakes also falls as consumers face higher stakes, e.g. higher balances borrowed (Agarwal et al. (2015a)).

[4]The available evidence does find a nontrivial elasticity of borrowing with respect to interest rates, although this evidence tends to use price variation generated either by (1) the pre-scheduled expiration of promotional interest rates (Gross and Souleles (2002)), which may predominantly affect a particularly price-sensitive subset of borrowers who serially shop for promotional rates, or (2) within-account interest rate changes over time (Alexandrov et al. (2017)), which, as I detail in Section 1.3.3, can arise endogenously as lenders respond to shifts in individual borrowers' risk or demand.

[5]Other modeling work specific to the credit card market includes Drozd and Serrano-Padial (2014).

1.2 Background and Data

1.2.1 Institutional Background

The Credit Card Industry

Credit cards are well known as a means of transaction. For many households they are also an important source of credit. Credit cards provide over $3 trillion in open credit lines for unsecured borrowing, and survey estimates suggest that roughly 60% of US households that hold credit cards actively use credit cards to borrow, i.e., do not pay their balance due in full and hence incur interest charges (Bricker et al. (2017)).[6] The importance of credit cards as a source of credit is especially strong among less credit-worthy consumers, where the prevalence of at least occasional borrowing rises to roughly 85% among accounts held by near-prime consumers and over 95% for subprime consumers.

The credit card market was also relatively unregulated in the period prior to the CARD Act. After US Supreme Court cases in 1978 and 1996 curtailed state regulation of credit card interest rates and fees (Evans and Schmalensee (2005), Hyman (2011)), credit card lending became concentrated among large, national banks that faced few restrictions on pricing strategies or the terms of credit offered to borrowers (Mandel (1990)). Simultaneously, advances in credit scoring and computing power increased the sophistication of pricing and underwriting, with prices becoming tailored to borrowers' individual risk, price sensitivity, and even shopping habits (Edelberg (2006), FRB (2010)).

Prior to the CARD Act, lenders' pricing strategies rested on two main sources of information. One is consumer credit bureaus, which collect data on consumer borrowing history across a wide range of loan products and then use these data to predict consumers' likelihood of future default. The bureaus transform these predicted default likelihoods to a more familiar credit score on an integer scale with higher numbers corresponding to safer borrowers; one common example is a FICO score.[7] These scores and the underlying data are sold to credit card issuers to prospect and underwrite new accounts and also to monitor risk on mature accounts.[8] Because this information is typically available to all firms in the market, this information is best thought of as *public* information for the purposes of studying firm behavior.

The second key source of information for a lender is a consumer's own behavior with a credit card after origination. Much of this information is private for the lender because it is not reported to consumer credit bureaus and is not otherwise observable to competitors, including a consumer's purchase volume, shopping behavior, prevalence of borrowing, repayment rates, and monthly payment timing. For some consumers lenders may receive

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[6]The account-level administrative data I study in this paper corroborate this survey evidence, as I find that 70% of active credit card accounts are used for borrowing in at least three months of the year.

[7]Further information on the contents and uses of credit report data is provided in Section 1.2.2.

[8]See Grodzicki (2014) for a discussion of the information that credit card issuers use in prospecting new accounts.

additional private information as well. For example, consumers may signal their riskiness through interactions with call center representatives – say, explaining an idiosyncratic reason for a late payment when requesting a late fee to be forgiven – or through additional information provided when requesting a credit limit increase, such as updated employment and income information. This private information is generally learned through a relationship with a borrower *after* origination.

Prior to the CARD Act, lenders could use a number of price dimensions to respond to new information learned after origination. First an account's interest rate for borrowing – which in the credit card market is represented as an annual percentage rate (APR)[9] – could change "at any time for any reason" according to stock language included in nearly all credit card contracts.[10] Credit card contracts also typically delineated a set of "triggers," such as late payments and over-limit transactions, that would cause the card issuer to consider an interest rate increase. Roughly 52% of borrowers in pre-CARD-Act data experienced a discretionary increase in their card's interest rate over the course of a year, with about half of these increases coinciding with behaviors typically specified as repricing triggers.[11] Thus lenders found it optimal to upwardly reprice the interest rate on many, but not all, borrowers as new information arrived over the course of lending to a consumer.

In addition to these interest rate repricings, credit card pricing also responded to borrower behavior through behavior-contingent fees, such as fees for late payments or over-limit transactions. For an average account prior to the CARD Act, revenue from these fees was 32% as large as interest charges, and on subprime accounts it was 46% of interest charge revenue.[12]

[9]The APR concept was developed by the Truth in Lending Act (TILA) rather than by industry. TILA's implementing regulation specifies that the APR is "determined by multiplying the unit-period rate by the number of unit-periods in a year," so APRs are annualized without compounding even though credit card interest typically compounds monthly. See 12 CFR Part 1026.

[10]See ConsumerAction (2007) for details on the prevalence of these any-time-any-reason terms. Examples include "All terms, including the APRs and fees...may change based on information in your credit report, market conditions, business strategies, or for any reason", and "We have the right to change the rates, fees, and terms at any time, for any reason...These reasons may also include competitive or market-related factors.", and "APRs may change to higher APRs, fixed APRs may change to variable APRs, or variable APRs may change to fixed APRs. We may change the terms (including APRs) at any time for any reason."

[11]In addition to such discretionary interest rate increases, 36% of borrowers saw a promotional interest rate either introduced or expired during the year. The expiration of a promotional rate differs from a discretionary change in interest rate because it is pre-scheduled at the time the promotion is introduced. Promotional rates are especially common at the time of origination, and hence are often referred to as introductory or "teaser" rates. Prior to the CARD Act, 35% of originations included some kind of promotional rate, and among accounts used for borrowing this share reached 71%. These rates were often offered below lenders' costs; in particular I estimate that 81% of promotional rates were a 0% APR, allowing interest-free borrowing. The profitability of these "teaser" contracts rested on borrowers continuing to borrow after the promotional period ended, and indeed I estimate that 86% of consumers who borrowed during an introductory promotion were still borrowing on their card three months after the end of their promotion. (In principle, promotional rates could also be profitable if borrowers incurred sufficient fees during their interest-period. However, I estimate that fee revenue on cards with promotional balances was only 1.24% annualized as a share of those cards' balances – insufficient even to cover lenders' costs of funds for much of the pre-CARD-Act period.)

[12]All major categories of fees were contingent on one or more borrower behaviors revealed after origination,

The responsiveness of credit card pricing to borrower behavior became an important motivation for the CARD Act, as consumer advocates and policy-makers both saw an inherent "unfairness"[13] in price increases that targeted some borrowers rather than others. As I detail in the following section, what emerged from policy debates around the CARD Act were strong restrictions on contingent pricing, i.e. pricing that depended on what lenders learned about borrowers over time, and very limited restrictions on pricing based on information available to lenders at the time of account origination.

The Credit CARD Act

Much of the policy debate around the CARD Act focused on the responsiveness of credit card pricing to borrower behavior. One perspective emphasized that discretionary interest rate repricing and contingent fees could "opportunistically" raise the cost of borrowing for consumers with the most pronounced demand for credit, in effect, extracting rents from those consumers with price-inelastic demand (Levitin (2011)). At the other end of the debate, industry advocates highlighted the importance of raising prices on borrowers revealed to be riskier than expected, so as not to instead make safer borrowers bear the cost of this risk (ABA (2013)).

Ultimately the Act did place strong restrictions on how credit card pricing responds to borrower behavior. First, discretionary increases in interest rates on outstanding balances were almost completely eliminated; the one major exception that was allowed to lenders has, in practice, proved to be an exception lenders rarely choose to use.[14] Second, over-limit fees were one of the most common contingent fees prior to the CARD Act and were likewise almost completely eliminated.[15] Third, the other most commonly used contingent fee, late fees, were effectively capped by a safe-harbor ceiling of $25 (or $39 for subsequent incidences within 6 months)."[16] On net, these restrictions strongly restricted lenders from adjusting prices in response to information revealed through borrower behavior over time, while placing little to no restriction on the interest rate set on the account at the time of origination.[17]

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with the exception of annual fees, which made up less than 10% of all fee revenue in pre-CARD-Act data.

[13]Fairness, while not defined in the CARD Act, is invoked both in the Act's preamble and five separate times in the text of the Act itself.

[14]This exception allows for the upward repricing of balances on accounts that are 60 or more days delinquent. In the debates leading up to the CARD Act, industry commentators presented evidence that repricing at this point of delinquency would not be profitable, as such balances are already at high risk of default (FRB (2008)); subsequent experience has borne this out, and lenders today rarely reprice balances that are 60 days late despite being allowed to do so (see Figure 1).

[15]While in principle these fees were still allowed if borrowers opt-in to allow these fees, they have virtually disappeared from the market (see Figure 1).

[16]Other pricing restrictions, which affect, for example, the number of fees that can be charged simultaneously or near the time of account origination, are detailed in CFPB (2013).

[17]Besides these price restrictions, the CARD Act also included a bevy of restrictions that sought to make credit card borrowing more predictable and transparent for borrowers. Lenders were banned, for example, from changing borrowers' statement due dates from month to month, or from imposing a cutoff time on

These interest rate repricing restrictions and over-limit fee restrictions took effect in February 2010 and late fee restrictions take effect in August 2010.[18] These implementation dates followed after a compressed period of policy debate surrounding the Act's passage. First in December 2008, as a precursor the Act the Federal Reserve issued a rule (originally scheduled to take effect in July 2010) that would have implemented a weaker version of the CARD Act interest rate repricing restrictions and fee restrictions. The CARD Act, introduced in Congress a month later in January 2009, superseded these restrictions and strengthened them to their present form. The Act was then passed and signed into law several months later in May 2009.

Given the Act's staggered congressional debates, passage, and implementation, I for much of my analysis will focus on a pre-CARD-Act period stretching from July 2008 through June 2009, and a post-CARD-Act period from July 2011 to June 2014. I focus on these full-12-month periods, both beginning in July, in order to avoid overemphasizing any seasonality, such as holiday consumption and subsequent debt repayment timed to the receipt of tax refund payments, that would appear in some months and not in others.

1.2.2 Data Sources and Summary Statistics

I use two main datasets in my analysis. One dataset contains the near-universe of US credit card accounts in a monthly account-level panel. The second dataset is a large random sample of consumer credit reports, showing all credit cards and other non-credit-card-loans held by a panel of consumers over time. Both are anonymized, administrative datasets furnished by industry and maintained by the Consumer Financial Protection Bureau (CFPB).[19] In this section I introduce both datasets and present summary statistics that highlight key dynamics in the credit card industry before and after the CARD Act.

CCDB Account-Level Dataset

The first dataset I use is the CFPB's Credit Card Database (CCDB), a near-universe of de-identified credit card account data in a monthly panel from 2008 to present. The data include all open credit card accounts held by 17 to 19 large and midsize credit card issuers under the supervisory authority of either the OCC or the CFPB, which together cover roughly 90%

due dates that came before 5 PM. Lenders were also required to include additional information on account statements that emphasized how long it would take to pay off a balance at various monthly payment sizes. Changes in account terms were also required to be disclosed to borrowers with 45 days of advance warning rather than the previous 15 day limit. A full review of these restrictions is available in CFPB (2013). However, industry trade association statements suggest the most important part of the Act from industry's perspective was the restriction on interest rate increases: the American Bankers Association referred to these restrictions as "the core, most important provision of the CARD Act" (ABA (2013)).

[18] A limited number of other provisions, including the requirement of earlier disclosure for account changes, took effect soon after the Act's passage, in mid-2009.

[19] Consistent with the CFPB's confidentiality rules, this paper only presents results that are sufficiently aggregated so as to not identify any specific individuals or institutions. Additionally, the data used contain no direct consumer identifiers.

of outstanding general-purpose US credit card balances.[20] For each account in each month, the data show totals of all aggregate quantities that would appear on a monthly account statement, including total purchases in dollars, amount borrowed and repaid, interest charges and fees by type of interest or fee, payment due dates and delinquencies. The dataset also includes some fields that are maintained by the lender but not always included on account statements, such as the consumer's current FICO score and a flag for whether the account holder keeps other accounts with the same bank, for example a mortgage. These same data fields are typically used by lenders for day-to-day account management.[21]

These data represent a superset of the credit card data used in Agarwal et al. (2014) and Agarwal et al. (2015b), including 9 to 10 additional midsize issuers that cover an additional 17% to 23% of outstanding balances. The advantage of using this superset is the inclusion of a more diverse set of firms, especially issuers with relatively concentrated market shares in important submarkets such as subprime or super-prime accounts. While these data are relatively new to academic research, they have been used previously in Keys and Wang (2016), Gross et al. (2016), and Alexandrov et al. (2017),[22] as well as several CFPB market-monitoring publications (CFPB (2013), CFPB (2015)).

More generally, an advantage of using these data is the ability to study an entire *industry's* behavior under different regulatory regimes using detailed account-level data. Large sample sizes – hundreds of millions of panel observations from credit cards actively used for borrowing in the pre-CARD-Act period, for example – make it possible to estimate rich heterogeneity in borrower demand characteristics and to study how these demand characteristics correlate with default risk, even among borrower types for whom ex-post default is rare. My use of account-level data for this purpose in many ways follows the example of Einav et al. (2012), who illustrate in the related context of subprime auto lending how account-level data can be used to estimate a rich model of credit demand where demand characteristics covary with risk.

For reasons of panel balance and data availability, I restrict my analysis to a subset of CCDB lenders that hold over 88% of all credit card balances observed in the CCDB in 2008-2009. This subset includes all of the issuers studied previously in Agarwal et al. (2014) and several additional issuers, including a large issuer with relative specialization in prime and super-prime lending. Given the presence of some mid-size and regionally-focused issuers in this sample, I also pool data from the smallest issuers into a single "fringe" issuer, as in Somaini (2011), when estimating my model.

[20]A total of 6 lenders enter or exit at some point in the sample period.

[21]See Trench et al. (2003) for one relevant industry study on this front.

[22]Respectively these papers study the CARD Act's "nudges" for borrowers to pay more than their minimum payments each month, propensities to consume out of changes in credit limits, and the responsiveness of balance size and late payments to interest and fees.

CCP Borrower-Level Dataset

The second database I use is the CFPB's Consumer Credit Panel (CCP), a large, randomly sampled panel of consumer credit reports showing all credit card accounts and other non-credit-card loans for a set of anonymized consumers over time. The non-credit card loans in these data include mortgages, auto loans, student loans, lines of credit, and installment loans held by a given consumer. The data also include non-loan items such as a measure of past loan applications, defaulted debts in collection, and public records such as bankruptcies.[23]

The panel is a 1-in-48 random sample, drawn from one of the three nationwide consumer credit reporting agencies.[24] This panel is observed quarterly beginning in 2004, with additional observations at an annual frequency from 2001 to 2004.[25] The CCP therefore has the advantages of showing a large representative sample of consumers, following these consumers over a longer time frame than is available in the CCDB, and reporting all credit card and non-credit-card accounts for a given consumer. The CFPB CCP data have been used previously in Brevoort and Kambara (2015), Brevoort et al. (2016), and Brevoort et al. (2017).[26]

In comparison to other credit report data often used in research, in particular the Federal Reserve Bank of New York's Consumer Credit Panel, the CFPB CCP has the unique feature of being a *loan*-level dataset rather than a borrower-level dataset for credit card accounts. For example, the CFPB CCP shows the quarterly balance on each of a borrower's credit cards, rather than the total balance summed across all credit cards. The availability of account-level credit report data makes it possible to study how borrowers allocate balances across multiple credit cards and other loans, and how borrower behavior evolves over time across multiple accounts. Additionally, the CCP makes it possible to study borrower entry and exit in the credit card market, as the dataset includes individuals not holding credit cards at any given point in time.

The CCP and CCDB both provide panel data on the credit card market before and after the CARD Act. The CCP has longer panel length and richer borrower-level information, and the CCDB has richer pricing information and lender-level information. Neither accounts nor account-holders can be linked between the CCDB and CCP.

Summary Statistics

In this subsection I use the CCP and CCDB to illustrate the mechanical effects of the Act on three specific price dimensions, to contrast these mechanical effects with the overall changes

[23]For further background on data included in consumer credit reports and the uses of these data, see Avery et al. (2003).

[24]These three are Equifax, Experian and Transunion.

[25]Additionally, the panel frequency increases to monthly in 2013, although I do not use the monthly data in this paper.

[26]Respectively these three papers study medical collections' predictive power for loan default, the prevalence and correlates of not having a credit report file or credit score, and the impact of Medicaid expansions on financial health.

in the cost of borrowing in equilibrium before and after the Act, and to document changes in borrowing behavior that coincided with these price shifts.

Figure 1 shows the mechanical effects of the Act on three price dimensions that the Act regulated most directly: interest rate repricing, over-limit fees, and late fees. First, Panel A shows the incidence of interest rate increases on current borrowers over time. Forty-eight to fifty-four percent of borrowers experienced a discretionary interest rate increase at least once a year before the CARD Act.[27] The incidence of interest rate increases then dropped sharply, and nearly to zero, when the CARD Act repricing restrictions went into effect. Panel B documents a similar drop in the incidence of over-limit fees, which affected roughly 7% of accounts in an average *month* prior to the CARD Act, and then fell sharply to nearly zero when the Act's over-limit fee restrictions went into effect. Panel C shows the drop in total late fee revenue at the time the Act's reasonable-and-proportional late fee restrictions took effect, a decrease of roughly 40%. These three results show that the Act's restrictions were binding on the price dimensions the Act targeted most directly, and that the Act's restrictions affected pricing on a sizable majority of accounts.

Figure 2 next shows that these price restrictions' implementation coincided with an immediate compression in the distribution of interest rates across accounts. The figure shows the inter-quartile range (IQR) of interest rates after controlling for origination FICO score, with one data point presented for each quarterly origination cohort.[28] For cohorts reaching maturity before the Act's repricing restrictions went into effect, these IQRs are consistently equal to nearly 8 percentage points; for cohorts reaching maturity after these restrictions took effect, these IQRs fell sharply to less than 6 percentage points.[29] To be clear, this evidence is only an event-study analysis. However, the sharpness of this change around the time of the CARD Act's implementation suggests that the Act, rather than other coincident changes in the credit card market, caused this fall in price dispersion.

Table 1 presents further evidence on which percentiles of the price distribution compressed and shifted. Each column of the table corresponds to a given statistic of credit card pricing (for example, the 25th percentile of interest costs), and each row highlights a different market segment (for example, borrowers with subprime FICO scores of 620-639). The statistics presented are changes in each measure from pre-CARD-Act data (2008Q3 through 2009Q2) to post-CARD-Act data (2011Q3 through 2014Q2). Effective interest rates[30] and

[27]I focus here on the type of rate increases restricted by the CARD Act, namely rate increases *not* caused by the expiration of a promotional interest rate or by changes in an indexed base rate, and also rate increases not coinciding with a delinquency of 60 days or more.

[28]I focus here on the age of accounts' maturity, i.e., the age by which all promotional teaser rates from the time of origination have usually expired, because a substantial amount of price dispersion emerges around the time of promotional rates expiring. In order to focus on within-FICO price dispersion, the IQRs plotted in the figure are for residual borrowing costs after partialling out FICO-score fixed effects.

[29]For evidence on price dispersion in the credit card market from a slightly earlier time period than is observable in the CCDB data, see Stango and Zinman (2015).

[30]The effective interest rates presented here are calculated by dividing total interest charges by the average amount borrowed, and then annualizing. This is not a fee-inclusive cost or "total" cost, but rather a measure of interest costs. Due to intricacies of how lenders assess interest, these can differ from slightly from the

fee-inclusive borrowing costs[31] both compressed from the pre-CARD-Act period to the post-CARD-Act period. For both price measures, the table reveals increases of several hundred basis points in the 25th percentile for most prime borrowers (FICO scores at or over 660) or in the 10th percentile for most subprime borrowers (FICO scores under 660), while the 75th and 90th percentiles usually fell, sometimes on the order of hundreds of basis points, or at least rose by less than the lower tail rose.

Overall the table shows that most credit scores saw compression in the left tail of the distribution as well as the right tail, and that compression in the price distribution was most pronounced among subprime consumers. Indeed, subprime consumers saw their IQRs of effective interest rates and fee-inclusive borrowing costs both typically fall by over 500 basis points, while the very bottom of the subprime price distribution sometimes rose by over 300 basis points. This compression in the left tail of the distribution cannot be a merely mechanical effect of the CARD Act's repricing restrictions, which only restricted interest rate *increases* after origination. Rather, this compression is suggestive of an equilibrium outcome whereby borrowers in the left tail of the price distribution faced higher prices as the CARD Act's repricing restrictions pooled them with their peers.

Figure 3 suggests that these relative price shifts may also have changed borrowing behavior. I focus on the extensive margin of credit card borrowing, both the share of consumers who hold a credit card at all and the share of active credit card accounts used for borrowing instead of transacting.[32] The figure shows that the share of consumers who have any credit card at all fell by up to 10 percentage points in the subprime market, while the share of consumers using cards for borrowing remained broadly unchanged. On net then, there was substantial consumer exit from the credit card market in the same market segments that saw, with the passage of the Act, higher prices in the low-cost left tail of the price distribution. While these patterns are only suggestive, they help motivate my analysis of whether the Act led to partial market unraveling.

I close this section with basic summary statistics that help with understanding the credit card market in the pre-CARD-Act equilibrium. Table 2 shows various statistics of credit card pricing across its columns, while the table's rows correspond to different market segments and

stated APR on the account. Additionally, several APRs may be in effect on an account at any given time, for example, one APR for a promotional balance, one APR applied to a balance accrued through a cash-advance, and another APR applied to non-promotional purchases. This measure of effective interest provides the arguably most representative average of these different APRs.

[31]To calculate a measure of the fee-inclusive price of borrowing, I sum interest charges and fee revenue on a given account and divide by the amount borrowed over a given period, such as a month or quarter, and then annualize. I refer to this measure as the fee-inclusive borrowing cost or price, or average borrowing cost. This is the same price measure used previously in research on the credit card market, including by Agarwal et al. (2014), and is equal to the "total cost of credit" as defined by CFPB (2013). Although this is not a *marginal* price for an additional dollar borrowed, it is the relevant marginal cost to consider on the extensive margin of borrowing.

[32]The share of consumers who hold a credit card at all is taken from the CCP, and the share of active accounts used for borrowing is taken from the CCDB data, both described above. Credit scores in the CCP data are non-FICO scores, but they are presented on the same axis because the two scores are designed to be similarly predictive of default, and because the two scores have the same range.

the extent to which these different segments use credit cards for borrowing. The prevalence of borrowing is quite high among active accounts: 96% of credit card accounts with subprime FICO scores of 620-639 are used for borrowing at least three months of the year, and even among prime (resp. super-prime) accounts in the 720-739 (resp. 780+), the prevalence of borrowing at least three months of the year is 67% (resp. 42%). Note also that fee-inclusive prices decrease sharply across the range of FICO scores from roughly 21 percentage points annualized among the subprime accounts shown, on average, down to 10 percentage points among the super-prime accounts shown. There is also a risk gradient in the share of revenue coming from fees: at the subprime end of the market, 5 percentage points out of the total 21% average borrowing cost is generated by contingent fees such as late fees or over-limit fees, while at the super-prime end, less than 1 percentage point out of the total 10% average borrowing cost comes from fee revenue.

1.3 Reduced Form Evidence

In this section I show in more detail *who* faced relative price changes as a result of the Act. I show that relatively safe borrowers faced higher prices and relatively risky borrowers faced lower prices, and that this engendered a dynamic form of adverse selection whereby lenders retained riskier borrowers over time. Consistent with partial market unraveling, lenders also set higher interest rates on average for all borrowers at origination. However I also show that in some parts of the market – especially prime accounts – the *majority* of the repricing that was restricted by the Act enabled lenders to charge higher markups over the cost risk, not just to adjust prices for risk. Lenders' excess returns on these marked-up accounts then fell sharply or were reversed after the Act.

1.3.1 Risk Pricing and Adverse Selection

This subsection examines how credit card lenders price risk that is observable at the time of origination, which I term "origination risk," and how this compares to the pricing of risk that becomes observable later, which I term "emergent risk." The CARD Act restricted how lenders price emergent risk but not origination risk, and I show that the Act generated a gap between the pricing of these two types of risk which led to lenders' adverse retention of riskier borrowers over time.

I first estimate the price gradient of origination risk as a linear relationship between interest rates $r_{i,0}$ and FICO scores at origination, $\text{FICO}_{i,0}$:

$$r_{i,0} = a + b\text{FICO}_{i,0} + e_{i,0} \tag{1.3.1}$$

I plot this gradient in pre-CARD-Act data as the dashed line in Figure 4 against the left and bottom axes, along with an accompanying binscatter.[33] There is a consistent relationship

[33] A binscatter plots the conditional mean of the dependent variable at each percentile of the regressor,

between price and risk throughout the FICO distribution: the average price of risk is roughly 32 basis points in annualized interest for every 10 FICO points of expected default risk.

I then estimate the pre-CARD-Act price gradient of emergent risk using a similar linear model, where I estimate the relationship between interest rates and *change* in FICO score since origination,

$$r_{i,t} = \alpha_{\tau_{i,t}} + \alpha_{\text{FICO}_{i,0}} + \beta\left(\text{FICO}_{i,t} - \text{FICO}_{i,0}\right) + \epsilon_{i,t} \tag{1.3.2}$$

This regression also includes fixed effects α for origination FICO score, $\text{FICO}_{i,0}$, which are included to absorb variation in interest rates $r_{i,0}$ from the time of origination,[34] as well as fixed effects for account age $\tau_{i,t}$, which absorb average changes in interest rates over the life of an account due to, for example, promotional rates expiring over time. Given the presence of these fixed effects, the estimated coefficient β then shows the correlation between changes in FICO score since origination and changes in (average) interest rate since origination.

In the same figure I then plot the estimate of β from this second regression with an accompanying binscatter. These are plotted on the opposite set of axes (right and top axes), which have the same scaling as the main axes for sake of comparability. Both plotted gradients are nearly the same: for both origination risk and emergent risk, borrowers on average face a difference in price of about 30 basis points in annualized interest for every 10 FICO-point difference in risk. This points to the credit card market setting a consistent price of risk, on average, in the pre-CARD-Act data, regardless of whether the risk was evident at origination or emergent later.

Figure 5 re-estimates both of these price gradients in post-CARD-Act data. Here there is evidence of the CARD Act's repricing restrictions causing a divergence between the two gradients: whereas origination risk is priced at 26 basis points annualized per 10 points of FICO score difference, lenders are only able to price risk that emerges after origination at less than a third of that rate, at 7 basis points per 10 FICO points.[35]

The gap between these gradients leads to weaker incentives for newly risky borrowers to

helping illustrate the shape of the relationship between the two across the distribution of the data. This can also be extended to regressions with controls by first partialling out controls from both the dependent variable and the regressor. See Stepner et al. (2013).

[34]This specification is equivalent to a long-differences specification in price and risk (without controls for origination risk) if the above error terms $e_{i,0}$ and $\epsilon_{i,t}$ are independent. The long-differences specification cannot be estimated directly, as $r_{i,0}$ is typically unobserved in the data for accounts originated prior to 2008. However, results are robust to an alternative, first-differences specification, which *can* be estimated.

[35]One intriguing question is why the post-CARD-Act price gradient of emergent risk in sloped at all, and furthermore, why it is not kinked at zero, seeing as the Act did not restrict interest rate decreases for borrowers who became safer over time. The likely answers to this particular questions are related. First, the Act still allowed several channels through which lenders are able to update interest rates as borrower risk evolves: lenders could change interest rates on future balances, albeit not on current balances; lenders could pass through base rate increases to borrowers but could also selectively choose to cancel these increases; and lenders could still offer promotional rates to borrowers, even on mature accounts. However, with the exception of a scheduled expiration of such a promotional rate, the Act provided no means for a lender to "claw back" any rate decrease for a borrower after offering that decrease, so lenders' incentive to offer rate decreases to newly safe borrowers was blunted by dynamic considerations.

attrite from borrowing, and likewise gives newly safe borrowers stronger incentives to attrite. I look for evidence of this type of dynamic adverse selection by estimating the relationship between borrower retention and changes in FICO score since origination, using a specification similar to equation 1.3.2,

$$A_{i,t} = \alpha_{\tau_{i,t}} + \alpha_{\text{FICO}_{i,0}} + \beta\left(\text{FICO}_{i,t} - \text{FICO}_{i,0}\right) + \eta_{i,t} \tag{1.3.3}$$

where $A_{i,t}$ is an indicator for attrition from borrowing, and, as in equation 1.3.2, the fixed effects α control for age $\tau_{i,t}$ since origination and FICO score at origination, $\text{FICO}_{i,0}$. The equation is again estimated at a quarterly frequency. The β coefficient therefore captures how quarterly linear-probability hazards from borrowing to non-borrowing change as a function of FICO score differences since origination.

I estimate this attrition model separately in the pre-CARD-Act and post-CARD-Act data and show corresponding binscatters in Figure 6. The gap between the two plotted relationships shows the difference between attrition hazards at each credit score. The gaps show that borrowers who become safer over time become more likely to attrite from borrowing after the Act relative to before. Similarly, borrowers who become riskier over time become less likely to attrite than before the Act. The estimates imply that for every one percentage point by which emergent risk is mispriced relative to origination risk, borrowers respond with a 0.7 percentage point change in the quarterly hazard of attrition from borrowing.

These two core results – the divergence between emergent and origination risk and the ensuing adverse retention of risky borrowers – are also robust to a number of alternative specifications. These specifications include the following cases: if fees are included in addition to interest rates in the definition of the "price" of borrowing; if only very young (i.e., recently originated) accounts are included to estimate the origination price-risk gradient; if the sample only includes accounts old enough that all originated *prior* to the CARD Act; if a short-differences specification is used to relate quarterly changes in interest rates to quarterly changes in FICO score; if attrition from accounts is extended to include charge-off; if accounts with promotional rates are included in the sample used to estimate origination price-risk gradients; and if a Cox proportional hazard model is used instead of a linear probability model to estimate these attrition hazards.

1.3.2 Price Elasticity Signals and Lender Rents

Consumer behavior on credit cards may reveal information not just about risk, but also about borrowing demand characteristics. In this section I provide evidence on which consumer behaviors reveal price elasticities of borrowing demand – behaviors that I term "price elasticity signals." To do so, I analyze heterogeneity in lender returns across accounts that exhibit different behaviors in pre-CARD-Act data, and I identify which behaviors predict *higher* returns relative to returns on other, equally risky accounts that exhibit no such particular behavior. These higher returns are evidence of lenders' learning about borrowers' price elasticities, as they indicate lenders are able to adjust loan pricing in excess of what would compensate the lender for any change in borrower risk.

My core finding in this exercise is that two of the most common causes of interest rate repricing the pre-CARD-Act data – transactions exceeding an account's credit limit, and delinquencies of less than 30 days – were in fact price elasticity signals in many FICO-score segments. In particular, delinquencies of less than 30 days predicted excess returns as high as 500 basis points at some FICO scores. I also confirm that, for accounts exhibiting either of these two behaviors, lenders' excess returns were either sharply reduced or eliminated after the Act. In contrast, all other behaviors that were typically denoted as potential causes for repricing in pre-CARD-Act credit card contracts predict greater default rates and (often sharply) lower returns in the pre-CARD-Act period.

Using ex-post returns to identify price elasticity signals is an appealing approach because such signals are otherwise inherently difficult for a researcher to identify in the CCDB data. This is true for at least two reasons. First, there is no analog of a FICO score that can be used to track changing demand, rather than risk, over time. Second, lenders' endogenous price responses to such signals can make the borrowers in question appear less, not more, likely to borrow than their peers. However even when these endogenous price changes lead to higher attrition, they still lead to higher ex-post returns if lenders are profit-maximizing and if a behavior is indeed revealing of higher price inelasticity.

To categorize borrower behaviors as price elasticity signals, I calculate the expected present value of lender revenues minus default losses among accounts that exhibit a certain behavior s in period $t = 0$, relative to the expected present value of balances lent on the same accounts, and I compare this measure of returns to the corresponding returns on accounts that do not exhibit any such particular behavior. Concretely this measure of expected returns is,

$$\hat{\mathbb{E}}[Y|s] = \sum_{t=0}^{T} \frac{\sum_{i:b_0(i)=s} R_{it} - L_{it}}{\sum_{i:b_0(i)=s} B_{it}/12} \tag{1.3.4}$$

where $b_t(i)$ is the behavior exhibited by consumer i in period t, and respectively R_{it}, L_{it}, and B_{it} are revenues, default losses, and revolved balances for that consumer. I then classify s as a price elasticity signal if,

$$\hat{\mathbb{E}}[Y|s] > \hat{\mathbb{E}}[Y|0] \tag{1.3.5}$$

where the behavior "0" on the right-hand-side of the inequality signifies that an account displayed "normal" behavior in that period, or more precisely, exhibited none of the signals I study.

I conduct this exercise for all behaviors that were typically included in pre-CARD-Act credit card contracts as causes for either a penalty fee of some kind or a potential change in interest rate: over limit transactions, delinquencies in paying a monthly bill of various severity (less than 30 days, 30 to 60 days, and over 60 days), as well as changes in FICO score or other credit report information. I also consider several interactions of these behaviors, for example late payment that coincides with an over-limit transaction in the same billing cycle.

Note that I do not require an account to never exhibit behavior s in order to be included in the sum over $\{i : b_0(i) = s\}$; I only require that the account not exhibit s in period 0. In my baseline results, I take $T = 24$ to correspond to a 2-year horizon, which is a standard horizon over which to evaluate outcomes in consumer credit; results are also robust to taking $T = 12$. Given the front-loading of revenue relative to losses, the shorter-horizon specification leads to additional behaviors being classified as price elasticity signals as well.

Figure 7 shows the difference in expected returns, $\hat{\mathbb{E}}[Y|s] - \hat{\mathbb{E}}[Y|0]$, for two primary signals that I identify as price elasticity signals: over-limit transactions not coinciding with delinquencies, and delinquent payments that are late by less than 30 days. Over-limit transactions are generally price elasticity signals on subprime accounts, while late payments of less than 30 days are generally price elasticity signals on prime accounts.[36] Such late payments may be indicative of less price-elastic demand for a number of reasons, including credit constraints, a higher cost of time, or borrower inattention.[37]

Table 3 then shows the results of this exercise for all other behaviors not classified as price elasticity signals. As shown in the table, each of these other signals predicts greater lender losses over the next two years. For example, among near-prime accounts with credit scores of 660-679, a quarterly FICO score drop of 30 to 59 points predicts lower annual returns by 3.66 percentage points off a baseline return of 5.09%, whereas late payments of 60 to 89 days predict lower returns by 42 percentage points.

1.3.3 Decomposition of Contingent Pricing

In this section I find that such price elasticity signals drove the majority of repricing on prime accounts, but not subprime accounts. I find that this result holds whether one considers interest rate repricing in response to contract-specified triggers, or any-time-any-reason interest rate repricing,[38] or pricing through fees rather than interest rates. This decomposition suggests, and my model results later confirm, that the CARD Act price restrictions' primary effect for *prime* borrowers is to restrict lenders from pricing information about borrower demand characteristics. In contrast, among subprime consumers the Act restricts the pricing of more risk-relevant information.

Figure 8 decomposes the share of interest rate increases in the pre-CARD-Act period that coincide with various contract-specified repricing triggers, for example transactions in excess

[36]This difference between prime and subprime accounts comports with some basic institutional features of the credit card market: credit limits on prime accounts are typically high enough that an over-limit transaction for a prime consumer would suggest severe liquidity needs, likely predictive of substantial risk; in contrast late payments of less than 30 days on prime accounts may signal inattention and hence lower price sensitivity, whereas any late payment on subprime accounts may signal a liquidity shortfall.

[37]As further evidence that late payments of less than 30 days may indicate inattention among some borrowers, I find in CCP data that these payments are positively correlated with borrowers reporting having a credit card misplaced or stolen.

[38]See Section 1.2.1 for more details on any-time-any-reason repricing.

of an account's credit limit. This decomposition is done separately for subprime accounts in the left panel and prime accounts in the right panel, and each trigger is colored to emphasize whether I identified it as a price elasticity signal in section 1.3.2 above. Price elasticity signals (colored in green) are by far the dominant cause of interest rate increases on prime accounts; in contrast, other triggers (colored in red) dominate on subprime accounts.

To investigate whether this basic pattern also appears in *fee* revenue rather than interest rate increases, Table 4 next shows the share of fee revenue coming from various signals across various FICO score groups. The share of fee revenue attributable to price elasticity signals again depends on FICO score. Among prime accounts, over 70% of all contingent fee revenue comes from a behavior I find to be a price elasticity signal, delinquencies of less than 30 days. Among subprime accounts, only about 20% of fee revenue comes from the behavior I find to be a price elasticity signal in this market segment, over-limit transactions not coinciding with delinquencies. These patterns suggest that, for fee revenue just as for interest rate increases, the CARD Act price restrictions primarily restricted the pricing of risk-relevant information in the subprime market, whereas they primarily restricted the pricing of demand-relevant information in the prime market.

In Figure 9 I next ask whether a similar pattern holds for non-triggered interest rate increases, i.e. any-time-any-reason repricing.[39] The figure shows that non-triggered price increases primarily occur at times when a borrower's FICO score is in fact *increasing*, i.e. default risk is falling. Hence roughly the same decomposition seen in triggered interest rate increases and in fee revenue can also be seen in non-triggered interest rate increases. That is, in the subprime market, the types of pricing restricted by the CARD Act had been primarily responsive to signals of borrower risk, while in the prime market, these pricing restrictions primarily limited price response to signals of borrower demand.

The Need for a Model

The results in the preceding subsections 1.3.1 and 1.3.2 point to a key tradeoff emerging from the CARD Act's pricing restrictions. On the one hand, restricting lenders' ability to raise prices on borrowers in response to a signal of borrowers' price elasticity can lower markups on some borrowers, bringing prices closer in line with marginal costs and reducing the deadweight loss associated with these markups. On the other hand, restricting lenders' ability to raise prices in response to risk information can engender adverse selection (at any price), which brings deadweight loss of its own. For consumers, the net effect of the Act on pricing depends on which of these two forces dominates in equilibrium, and for total surplus in the market, the net effect of the Act depends on the relative sizes of these two deadweight losses.

Empirically assessing the relative sizes of these effects empirically is difficult for two

[39]The data do not actually assign an interest rate increase to a particular cause, so these non-triggered repricings are inferred based on which behaviors or potential causes are seen to coincide with an interest rate increase within the preceding quarter.

reasons. First, the CARD Act substantially changed the composition of borrowers in the credit card market. This makes the Act's price effects difficult to measure for borrowers who were induced to leave the market. Second, the implementation of the Act coincided with a number of other credit market reforms[40] and with a time of unique turbulence in consumer financial markets, and the Act itself contained a number of policy changes unrelated to the repricing restrictions that I focus on here. For all of these reasons, it is a fraught exercise to isolate the effects of the Act's repricing restrictions per se in any data taken from after the Act's implementation.

These empirical difficulties notwithstanding, it *is* still an empirical question whether exacerbated information problems or lower lender markups were dominant when the Act's pricing restrictions took effect. Intuitively, the key issue underlying this question is the whether the information restricted by the Act resolved more uncertainty about borrower risk or demand. The more this information was relevant for borrower demand, then the greater were the Act's effects on markups. This can be shown through a graphical example, where I stylize the Act's pricing restrictions as requiring two borrower types who previously could be priced differently to instead be pooled. The more these two borrower types differed in terms of their demand elasticities, the more overall prices in the market fall as a result of the Act's restrictions, and the more does total surplus increase; conversely, the more these two borrower types differed in terms of their default risk, the worse is the resulting adverse selection problem and the more does total surplus fall.

In the following sections, I extend the intuition from that two-borrower example into a more realistic model of the credit card market, including multiple firms, private information, and a dynamic setting where lenders attempt to poach profitable borrowers from each other while borrower types also change over time. As I emphasize in section 1.4.1, each of these features plays a crucial role in a model designed to predict the CARD Act repricing restrictions' effects; for example, private information and dynamic borrower types are important in light of how the CARD Act restricted the pricing of information that either changes or is revealed privately over time.

1.4 A Model of the Credit Card Market

In this section I develop and estimate a model of the credit card market. I estimate the model on the equilibrium observed in pre-CARD-Act data, so that I can later, in Section 1.5, use the model as a tool to study the effects of introducing the CARD Act's price restrictions into this equilibrium. The model incorporates two features of the credit card market highlighted in the preceding section: lenders learn new information over time about both risk and

[40]Particularly relevant for credit card lending is the Federal Accounting Standards Board's release of FAS 166/167 in June 2009, which made securitization of credit card loans more costly for lenders. See Tian and Zhang (2016), who use a difference-in-differences strategy between securitizing and non-securitizing credit card lenders to estimate that these accounting changes led to a 40% reduction in loan balances by the most affected banks.

demand, and lenders respond to this information in the pre-CARD-Act regulatory regime by changing loan pricing. The model also has three other prominent features – heterogeneous price sensitivities among borrowers, adjustment costs for consumers who switch lenders or pay off their balances, and private information among lenders about borrowers. In subsection 1.4.1 I motivate these three model features and illustrate how these features are identified by the data. I then formally introduce the model in subsection 1.4.2, discuss estimation in subsection 1.4.3 and present model parameter estimates in subsection 1.4.4.

1.4.1 Credit Card Demand: Three Key Facts

Fact 1: Price Sensitivity of Demand

This subsection establishes that credit card borrowers are sensitive to price and illustrates how it is possible to identify heterogeneous price sensitivities in the data. This heterogeneity will play a key role when I later use the model to study the equilibrium effects of the CARD Act's price restrictions, because this heterogeneity affects which types of consumers change their borrowing behavior in response to different relative price changes.

I estimate these price sensitivities by exploiting a novel source of price variation in the credit card market: occasional, idiosyncratic repricing campaigns in the pre-CARD-Act data in which banks change interest rates on entire extant credit card portfolios simultaneously.[41] These campaigns come in two varieties. Occasionally, a credit card lender will reprice nearly all of its accounts at once, across all credit card types issued by that lender. In other cases, lenders will focus such repricing on all accounts in a single portfolio, such as a portfolio of airline credit cards. Such portfolios can be identified in the available data through clerical "tags" that lenders use to differentiate their accounts. It is plausible that these repricing campaigns are motivated by factors exogenous to consumer credit demand, such as changes to lenders' internal cost of funds, changes in individual portfolio managers' taste for risk, or a desire to shrink loan portfolios in advance of other institutional changes such as a merger or acquisition.

As an example of such repricing campaigns, Figure 10's left panel illustrates a campaign in which one lender, referred to as "Bank A," raised the APR on nearly all extant accounts by exactly 100 basis points in a month labeled as event time 0. The nine red lines show that all APR deciles of Bank A's accounts rose simultaneously, after a preceding period with minimal price change. This campaign occurred more than a year before the passage of the CARD Act, and occurred at a time when, as shown by the figure's dashed blue line, other lenders' pricing was on average unchanged.

This change in Bank A's pricing relative to its competitors facilitates a difference-in-difference analysis of borrower retention. The right panel of Figure 10 presents the standard difference-in-difference event-study plot for these two retention rates. Specifically, the right

[41] Firm-level price variation has also been used elsewhere in consumer finance research, for example by Cox (2017) in the context of student loan refinancing.

panel shows event-time-specific estimates from the equation,

$$\log Q_{jt} = \alpha_j + \alpha_t + \beta_j t + \alpha_{A,t} + \epsilon_{jt} \tag{1.4.1}$$

where Q_{jt} denotes retention rates among existing borrowers for lender j in month t, i.e. the share of borrowers who continue to borrow. The first two α terms in this equation implement a standard difference-in-differences design, while the $\alpha_{t,A}$ terms capture differences between Bank A and other, non-campaign banks. For sake of presentation, the β term is included to account for different time trends among the included banks, though as I show later this does not substantially affect the model parameters ultimately estimated off of this variation.

As can be seen in the right panel of Figure 10, the retention rate for Bank A's borrowers falls relative to other banks' borrowers immediately after the repricing campaign, with the greatest difference in the first month and a sustained but lesser gap in subsequent months. This pattern appears clearly despite strong seasonal effects on borrowing that occur during this time period, as retention rates peak annually in or around the month labeled as event time 0.

When estimating the demand side of the model, I use such price variation to estimate heterogeneous price sensitivities across different borrower types. This heterogeneity plays a key role in determining the equilibrium effects of the CARD Act's price restrictions, as it affects which types of borrowers – for example, high or low risk borrowers – are most likely to enter or exit the market in response to relative price changes.

Fact 2: Persistence and Adjustment Costs

The previous subsection showed that price elasticities of borrowing demand are nonzero; this subsection considers reasons why elasticities are also not infinite. In particular I posit two kinds of adjustment costs faced by credit card users and I show evidence for these costs in pre-CARD-Act data. These adjustment costs will play an important role when I use the model to study the CARD Act price restrictions' effects, as they affect both the intensity of competition between lenders for different borrower types, and also the degree to which different borrowers substitute toward accounts with promotional pricing if other prices rise.

I present evidence for these adjustment costs by showing persistence in two dimensions of consumer behavior. One dimension is borrowing choices: consumers who use a card for borrowing in one month are highly likely to continue borrowing in the next month, while consumers who do not borrow are highly likely to continue not borrowing. A second is firm choice: regardless of whether they are borrowing or not, consumers persist in holding a card from a given bank, despite sometimes strong incentives for switching to another bank's credit card. These two types of persistence suggest adjustment costs both in paying off balances and in switching to a new credit card issuer.

Table 5 presents evidence that consumers face some kind of adjustment cost when paying off credit card balances: throughout the FICO score distribution, consumers are substantially more likely to borrow on a credit card in a given month if they also borrowed in the preceding month (columns 1 and 3) than if they did not borrow in the preceding month (columns 2 and 4). In the first half of the table, columns (1) and (2) make this point in a subsample of consumers with a demonstrated preference for borrowing – those consumers who borrowed on their credit card at least once in the past six months. As an illustrative example, note that FICO 720 consumers in this subsample who were borrowers in the preceding month have an 87% chance of continuing to borrow in the current month, whereas their non-borrower counterparts in the preceding month have only a 9% chance of borrowing. Columns (3) and (4) then extend this analysis to the whole population of credit card holders, not just those who borrowed at some time in the past six months. There is strong persistence in this broader population too: to again consider the example of FICO 720 consumers, the probability of continuing to borrow is 70%, while the probability of new borrowing is only 2%. This persistence is suggestive of some kind of adjustment cost in paying off credit card balances, which I term a "liquidity cost" to reflect the opportunity cost of using other funds to repay a credit card balance.

I next show that borrowers often face strong incentives to switch credit cards but nevertheless switch cards infrequently. To illustrate these strong incentives to switch cards, Table 6 follows a format similar to Table 2, here showing *introductory* rates on newly originated accounts in the pre-CARD-Act period. Here prices are shown for newly originated accounts to which a borrower transferred a previous balance at a promotional interest rate. Discounts relative to mature accounts appear throughout the FICO score distribution. For example, among FICO 740 consumers, the average cost of borrowing is roughly 600 basis points lower on newly originated accounts with promotional balance transfers, relative to mature accounts. Next, in Figure 11 I examine how frequently borrowers switch cards in the presence of these price incentives. To estimate these switch rates, I calculate the total number of balance transfers with promotional rates per quarter in the pre-CARD-Act period, and I compare this flow to the stock of consumers borrowing on mature accounts at non-promotional rates.[42] The figure shows this rate, along with the total count of balance transfers, across a range of FICO groups. Even on a quarterly basis, only 16% of prime consumers and less than 5% of subprime consumers respond to the price incentives shown previously in Table 6 by transferring balances to a new credit card, indicating that many consumers face some kind of adjustment cost in setting up accounts with new issuers.

When estimating the model, I use these differences in switch rates and retention rates across borrower types to identify two corresponding sets of adjustment cost parameters – liquidity costs for paying off a balance, and set-up costs for opening a new account with a new lender. These adjustment cost parameters then determine which borrowers are most likely to substitute to promotional pricing and which borrowers are most likely to switch

[42]This ratio differs from the true balance transfer rate insofar as a single consumer may account for multiple balance transfers in the same quarter, for example when closing two cards and transferring both cards' balances to the same new card. It is impossible to quantify the number of such instances using the CCDB.

lenders when the CARD Act price restrictions are introduced.

Fact 3: Asymmetric Information

This subsection illustrates that lenders possess a substantial amount of private information about their ongoing borrowers. I also find that such private information was reflected in pre-CARD-Act loan pricing. These facts suggest that the CARD Act's pricing restrictions – which make it difficult for lenders to adjust prices when they acquire private information about borrowers over time – have different price effects across different consumers depending on these consumers' privately revealed types. Incorporating such private information in my model therefore becomes important in anticipation of using the model to study the Act.

I recover such private information from observed lender pricing in pre-CARD-Act data. This information is indeed private, because interest rates and fees in the credit card market are typically not observable to a lender's competitors.[43] Equilibrium pricing therefore reveals lenders' private information so long as distinct prices are assigned to distinct consumer types; I formalize some conditions sufficient for such pricing later.

To study the importance of this private information formally, I assign each borrower an index of private information corresponding to that borrower's location in the distribution of prices charged by their lender to other borrowers at their FICO score. I will develop this index in detail in section 1.4.3. This index has the properties that borrowers with the same index value and the same FICO score have the same expected default rate regardless of which lender they borrow from (despite different lenders pricing different risk levels differently); indexes are, by sign convention, increasing in risk; and, when indexes are discretized, they are discretized such that an equal share of borrowers in the market is assigned to each index.

I use these indexes in Table 7, where I present linear-probability estimates of default rates by quintile of this private default-risk index. In this analysis I control flexibly for 20-point bins of FICO score in order to measure the predictive power of private information *within* observably similar borrowers. Formally, I estimate these effects in the following equation,

$$\text{Default}_{i,t:t+12} = \alpha_{j(i),x(i)} + \alpha_t + \sum_{n=1}^{5} \beta_n 1_{\psi_{i,t}=n} + \epsilon_{it} \tag{1.4.2}$$

Here the dependent variable is an indicator for any instance of default by borrower i in the subsequent 12 months after period t, and the key coefficients β_n capture differences in default rates across five quintiles of the private information index, which I denote by ψ. Meanwhile the fixed effects for borrower i's firm j, FICO score x, and time period t help ensure that these risk comparisons are made within otherwise observably similar borrowers.

Estimates of β_n are presented in Table 7, first for all credit card borrowers in column (1), and then separately for prime and subprime borrowers in columns (2) and (3) respectively.

[43]The unobservability of competitors' prices stems from the issue I discussed when introducing the CCP credit report data, that credit reports contain no data on prices paid for each loan.

The first (lowest risk) quintile is omitted, so that all other coefficients are relative to this group. The table shows that private information has substantial predictive power for default risk, especially in the subprime market. Overall, the fifth quintile of private information has 9 percentage points higher probability of default than the lowest quintile, and in the subprime market this gap grows to 20 percentage points.

To help benchmark these estimates against median default rates at various FICO scores, Table 8 then presents default rates across the FICO score distribution at the top-quintile, bottom-quintile, and median of such private information. Strikingly, the top quintile among FICO 720 borrowers has roughly the same expected default rate as the median borrower with a FICO 680 score, while the bottom quintile among these FICO 720 borrowers has roughly the same expected default rate as a median borrower with a FICO 740 score. Further perspective on these gaps can come from the overall distribution of FICO scores among credit card holders: I find that moving from the first to the fifth quintile of privately-known default risk is, on average across all FICO scores, roughly equivalent to a 2 standard deviation (174 point) decrease in FICO score in the overall distribution of scores; likewise, one standard deviation of privately-known default risk is just as predictive of future risk as 0.74 standard deviations of borrower credit score.

These results highlight the importance of incorporating private information in the model in order to study the CARD Act price restrictions' effects. As the Act's restrictions limit lenders' ability to adjust loan pricing when they learn such private information over time, borrowers with different privately revealed types, and hence default risk, will experience different relative price effects and face different incentives to either continue or attrite from borrowing.

1.4.2 Model Exposition

This section presents my model of the credit card market. The backbone of the demand model is a finite mixture of consumer types, each of whom has logit demand over credit card lenders and over the choice of whether to use his credit card for borrowing or not. Precisely, in a market with J banks there are $2J + 1$ discrete choices available to each consumer each period: two choices per bank (i.e. borrowing, or holding a credit card from that bank without borrowing) and one outside good, which is the option to hold no credit card at all. Consumers choose at most one bank at any point in time, and with this bank consumers choose only whether or not to borrow – that is, I model only the extensive margin of borrowing, not the choice of how much to borrow.[44] Each type has different tastes for each choice.

[44]These two modeling decisions – that consumers single-home over banks and choose extensive rather than intensive-margin borrowing – are primarily made for sake of tractability. However, these decisions also do not depart much from realism in the credit card market. First, using CCP data, I find that a large majority of consumers hold only one "primary" credit card, where primary is defined as carrying the majority of a consumer's credit card balances. Depending on FICO score, this share ranges from at least 80% to over 90%. Hence a single-homing model can in many respects be thought of as a model of a consumer's choice of primary card. Additionally, a majority of deep subprime consumers and a large minority of prime consumers

I denote types by θ. I specify several taste parameters to be estimated for each type. First, each type enjoys a flow utility $d_{j\theta}$ from borrowing with bank j and a flow utility $n_{j\theta}$ from transacting (rather than borrowing) with bank j; meanwhile the utility of the outside good (holding no credit card at all) is normalized to zero. Additionally, in order to capture the adjustment costs documented earlier in this section, each type pays a setup cost $s_{j\theta}$ for opening a new account with bank j and a liquidity cost $l_{j\theta}$ or paying off a balance and transitioning to transacting (non-borrowing) status after borrowing with bank j in the past period. Additionally, types have heterogeneous marginal utilities of income γ_θ (i.e., the price coefficient in logit demand). The parameters $\{d_{j\theta}, n_{j\theta}, s_{j\theta}, l_{j\theta}, \gamma_\theta\}_{(\theta,j)\in\Theta\times J}$ are the key demand parameters to be estimated in the model, along with a probability distribution μ_θ over types.

This parameterization allows a type's preferences each period to depend on what bank he held a credit card from in the *previous* period, and also on whether he borrowed or not in the previous period. Because this is a model of industry-wide dynamics with differentiated firms, the total number of choice probabilities modeled is large ($|\Theta| \cdot (2J+1)^2$). I therefore use Table 9 to summarize which parameters enter different borrowers' flow utilities for each choice. The three rows of the table correspond to the consumer's circumstances at the end of the preceding period: a consumer either (i) has an open credit card from some bank j that he used for borrowing, (ii) has a credit card from j that he did not use for borrowing, or (iii) holds no credit card at all. The five columns of the table then correspond to the consumer's choice in the current period: a consumer either keeps his credit card from the same bank j (columns 1 and 2), or opens a new card with some other bank $j' \neq j$ (columns 3 and 4), or chooses the outside good of no credit card at all (column 5). When holding a credit card, a consumer chooses either to use it for borrowing (columns 1 and 3) or not (columns 2 and 4).

In reading the table, note that these banks j and j' can be any bank in the set of banks J, so there are $|J|$ distinct values of each parameter subscripted by j or j'. An important pattern to note in the table is that consumers only pay setup costs s when transitioning from some bank j to a new bank $j' \neq j$, and only pay liquidity costs l when transitioning from borrowing to transacting.

Meanwhile, as shown in the table, prices differ for consumers who are newly opening a credit card with a bank and consumers who held a credit card with that bank in the past period. These two prices are denoted $p^0_{\theta j}$ and $p^1_{\theta j}$. Allowing these prices to differ between new and mature accounts helps pin down consumers' switching costs across accounts when estimating the model, which then is helpful in predicting how consumers respond when such new-account discounts ("teaser" rates) change after I impose the CARD Act price

indeed hold only one credit card in CCP data. Second, there is some evidence that firms compete more on the extensive margin using price, and then use credit limits as their preferred instrument on the intensive margin (Trench et al. (2003), Agarwal et al. (2015b)). In fact, many credit limits are not disclosed until after a borrower has made the extensive margin choice of whether to open a credit card or not, whereas prices are advertised heavily to consumers considering a new card. Incorporating the intensive margin in the model would therefore seem to require including both prices and credit limits, which would expand the firms' strategy space to the point of intractability.

restrictions. Note also that these prices are one-dimensional, so in practice I use the fee-inclusive borrowing cost introduced in Section 1.2.2 when I estimate these prices in the data; these are also the appropriate marginal prices to use when modeling the extensive margin of borrowing.

The presence of adjustment costs makes the consumer's problem dynamic. Therefore the total expected payoff for a given choice is the sum of the relevant flow utility from Table 9 and also a discounted expectation of continuation values (plus also, given logit demand, the realization of an extreme value type-1 i.i.d. taste shock). To describe these continuation values, let $k \in \{\underline{\text{b}}\text{orrow}, \text{tra}\underline{\text{n}}\text{sact}\} \equiv \{\text{b,n}\}$ denote a consumer's choice of how to use his credit card and $j \in J$ again denote a consumer's choice of card.[45] I then write these continuation values as $V(\theta', j, k)$. Note that θ' is a consumer's type in the *next* period while j and k correspond to the current period. For example, a consumer i's total expected payoff for choosing to borrow ("b") with bank j in the current period after having also borrowed with bank j in the past period is,

$$\underbrace{d_{j\theta} - \gamma_\theta p^1_{\theta j}}_{\text{flow utility}} + \beta \underbrace{\mathbb{E}_\theta\left[V(\theta', j, b)\right]}_{\text{exp. cont. value}} + \epsilon_{ijb} \tag{1.4.3}$$

Integrating over taste shocks ϵ for each choice yields the standard Bellman equation for continuation values V,

$$V(\theta, j, k) = \log\left(\sum_{j',k'} \exp\left(v\left(j', k' | j, k, \theta\right)\right)\right) \tag{1.4.4}$$

where the lower-case v term denotes total expected payoffs for a given choice exclusive of taste shocks. The value of v depends on consumers' past-period and current-period choices as described previously in Table 9. For example, in the case of a consumer who chooses (as in equation 1.4.3) to borrow ("b") with bank j in the current period after having also borrowed with bank j in the past period, the value of v is,

$$v\left(j, b | j, b, \theta\right) = d_{j\theta} - \gamma_\theta p^1_{\theta j} + \beta \mathbb{E}_\theta\left[V(\theta', j, b)\right] \tag{1.4.5}$$

Besides determining flow utilities as above, consumer types θ additionally govern heterogeneity in default rates. Specifically each type defaults at exogenous rate $\delta(\theta)$ in periods when he chooses to borrow. Default occurs after all flow utilities are realized in that period. I later discuss how these default rates determine firms' costs, but here I emphasize how default rates also matter for consumer payoffs. In particular, a consumer who defaults has his credit card account "closed" and is reassigned to the outside good (holding no credit card at all) for purposes of computing adjustment costs in the next period. Hence default rates affect expected payoffs only through the expectation over future continuation values.

To tractably model expectations over continuation values, I follow the standard approach

[45] In this notation I also represent the outside good as $(j, k) = (0, 0)$.

in the dynamic discrete choice literature and suppose types evolve according to a Markov process,[46] with a transition matrix that I denote $T_{\theta\theta'}$. Transitions occur independently of default, consumer choices, and taste shocks. Hence, for consumers who use their credit card for borrowing, the expectation $\mathbb{E}_\theta$ can be decomposed as,

$$\mathbb{E}_\theta\left[V(\theta', j, b)\right] = \underbrace{(1-\delta(\theta))\,T_{\theta\theta'}(\theta)V(\theta', j, b)}_{\text{no default}} + \underbrace{\delta(\theta)T_{\theta\theta'}(\theta)V(\theta', 0, 0)}_{\text{default}} \tag{1.4.6}$$

where the θ argument in $T_{\theta\theta'}(\theta)$ selects the relevant row of the matrix $T_{\theta\theta'}$. In the second term on the right-hand-side, recall that I use $(j, k) = (0, 0)$ to denote the outside good.

In contrast, for consumers who do not choose to borrow (i.e., who choose $k = n$ or $k = 0$), the expectation $\mathbb{E}_\theta$ does not depend directly on default rates and takes the form,

$$\mathbb{E}_\theta\left[V(\theta', j, k)\right] = T_{\theta\theta'}(\theta)V(\theta', j, k) \tag{1.4.7}$$

The above exposition makes clear how the demand side of the model captures two of the three stylized facts I highlighted – price sensitivity and adjustment frictions. To capture the third stylized fact – the importance of private information – I now describe how the model parameterizes consumer types. Specifically I allow types θ to have two dimensions, one private component $\psi \in \Psi$ and one "public" component $x \in X$. The latter is public in the sense that it is observable to all firms in the market. Note that the public type x is best thought of as a FICO score, as FICO scores are expressly designed to be a composite of public information about a consumer, and are indeed observable to all firms in the market.[47] The joint of these two components is then a consumer's overall type, $\theta \equiv (x, \psi)$.

Two assumptions on borrower types will prove useful in estimating the model. One assumption, which is arguably the stronger of the two, is that borrower default rates depend only on types, and in particular do not depend on prices p_j^0 and p_j^1 or on bank j. This can be thought of as a "no moral hazard" assumption and I will refer to it as **Assumption 1:**

$$\delta = \delta(\theta)\ \forall j, p_j^0, p_j^1 \tag{1.4.8}$$

Several pieces of evidence are available in support of this assumption, including evidence on how observed default rates respond to two sources of plausibly exogenous price variation in the CCDB data, an argument using CCP data that examines how credit card price changes affect most consumers' overall cost of debt service (a budget constraint argument), and a review of related research in consumer finance suggesting little to no moral hazard channel through which prices affect default rates. This assumption also follows on other research that has used structural models of selection markets without moral hazard, for example Cohen and Einav (2007) and Einav et al. (2010).[48]

[46] See Rust (1994) for a review of this literature and a taxonomy of assumptions typically used to help make such models tractable.

[47] See Section 1.2.2 for further information on the contents and availabiltiy of credit report data.

[48] Additionally, I highlight in section 1.4.3 where the estimation procedure could be adapted, albeit at

Given this assumption, it is without loss of generality to order private types ψ by the default rates they induce. Essentially, private types become an index of residual default risk. I order private types ψ at each public type x such that default is increasing in ψ,

$$\psi' > \psi \implies \delta(x, \psi') > \delta(x, \psi) \; \forall \; x \tag{1.4.9}$$

A second assumption, which I view as the weaker of the two, is a "non-advantageous selection" assumption. This assumption is supported by extensive evidence showing the credit card market is not merely non-advantageously selected, but is indeed adversely selected (Ausubel (1999), Agarwal et al. (2010)).[49] Formally the assumption is that higher-risk private types do not have less demand for borrowing from any given lender than do lower-risk private types, at a given FICO score. I express this assumption in terms of resultant choice probabilities, which I term **Assumption 2:**

$$\psi' > \psi \implies \Pr(j, b | j, b, x, \psi') \geq \Pr(j, b | j, b, x, \psi) \; \forall \; x \in X, \; j \in J \tag{1.4.10}$$

Note that this assumption embeds some restrictions on the competitive environment, namely that one lender's relative quality advantage over competing lenders (as expressed in differences across j in demand parameters such as the flow utility from borrowing, $d_{j\theta}$) does not change so drastically with ψ, the private dimension of θ, such that lenders in fact face lower demand as private risk rises. That is, residual demand curves and not just aggregate demand curves are non-advantageously selected in the pre-CARD-Act equilibrium.[50]

The precise timing of the demand side of the model is as follows. First, borrower types θ are realized at the start of the period. Banks then post prices p^0 and p^1 for each type. Consumers choose a bank and a borrowing status, and enjoy flow utility from their choice. Default then arrives exogenously. Borrowers who default are forced into the outside good (no account with any bank) for purposes of determining their adjustment costs in the following period. Borrowers who do not default continue on to the next period with their chosen bank.

On the supply side of the model, a credit card lender's price-setting problem has two parts: what price of borrowing to offer on existing accounts, and what promotional or "teaser" price to offer for new customers. As in the consumer's choice problem, these two sets of prices are denoted $p^1_{\theta j}$ and $p^0_{\theta j}$ respectively, where subscripts denote bank j and consumer type θ.

Corresponding to these two types of prices, credit card lenders' costs can also readily be grouped into two types: acquisition costs related to originating a new account, which include underwriting costs, account set-up costs, and marketing expenses; and account maintenance

considerable computational expense, should this assumption fail.

[49] To clarify these terms, advantageous selection is the case where higher prices induce the composition of borrowers to become less risky; adverse selection is the more familiar opposite of this case. Non-advantageous selection includes adverse selection as well as the intermediate case where the composition of borrower risk is unchanged with price.

[50] While it would be preferable to express this assumption in terms of primitives, it appears quite cumbersome to do so, and the assumption as expressed in choice probabilities helps clarify the essential – and also most likely directly testable – content of the assumption.

and charge-off costs on existing accounts, which include day-to-day account management plus costs of default net of recoveries. I denote these costs $c^0_{\theta j}$ and $c^1_{\theta j}$ respectively.

My model focuses on the extensive margin of borrowing,[51] so lender flow profits for consumers who choose to borrow are the difference between the relevant price and cost: that is, flow profits for lender j are $p^1_{\theta j} - c^1_{\theta j}$ for existing borrowers and $p^0_{\theta j} - c^0_{\theta j}$ for borrowers opening a new account. I suppose acquisition costs must also be paid for new accounts even if consumers choose not to borrow, given that new-account costs are primarily driven by set-up and marketing expenses rather than default cost. This cost structure implies that expected discounted lifetime profits for a new consumer, Π^0, take the form,

$$\begin{aligned}\Pi^0(p_j, p_{-j}, \theta, k) =& \underbrace{\Pr^0_j(b|\theta, p, k)p^0_{\theta j} - c^0_{\theta j}}_{\text{flow profit}} + \\ & \underbrace{\Pr^0_j(b|\theta, p, k)\beta(1-\delta(\theta))T_{\theta\theta'}(\theta)\Pi^1(p_j, p_{-j}, \theta', b)}_{\text{exp. cont. profit} \mid \text{borrow}} + \\ & \underbrace{\Pr^0_j(n|\theta, p, k)\beta T_{\theta\theta'}(\theta)\Pi^1(p_j, p_{-j}, \theta', n)}_{\text{exp. cont. profit} \mid \text{not borrow}}\end{aligned} \tag{1.4.11}$$

Here the notation $Pr^0_j(b|\theta, p, k)$ denotes the probability of consumer type θ choosing to borrow conditional on having opened a new account with lender j in the current period, and conditional on having chosen $k \in \{\underline{\text{b}}\text{orrow, tra}\underline{\text{n}}\text{sact, out}\} \equiv \{\text{b,n,0}\}$ in the preceding period. Similarly $Pr^0_j(b|\theta, p,)$ denotes the probability of choosing to transact (i.e., hold a credit card without borrowing). The dependence on k is a result of consumers facing different adjustment costs depending on whether they borrowed in the previous period, and hence exhibiting different choice probabilities in the current period. As in the demand side of the model, $\delta(\theta)$ denotes borrower default probabilities, and the notation $T_{\theta\theta'}(\theta)$ selects the appropriate θ-specific row of the consumer type transition matrix. Also note that $p = (p_j, p_{-j})$ denotes the market price vector (including both existing-account prices and teaser prices). The final piece of new notation to introduce is $\Pi^1(p_j, p_{-j}, \theta', k)$, which is lenders' continuation profits on existing accounts, as a function of the consumer's choice $k \in \{\underline{\text{b}}\text{orrow, tra}\underline{\text{n}}\text{sact}\} \equiv \{\text{b,n}\}$ in the current period. These profits on existing accounts are defined further below.

Some intuition about issuers' dynamic incentives in the previous expression may be helpful. These continuation profits are the sum of two objects: first, the probability that a consumer chooses to borrow on a card, times the sum of both a one-period payoff and a discounted expected continuation value given that choice; and second, the probability that a consumer chooses to use a card only for transactional purposes (not for borrowing), times a corresponding payoff and continuation value. Accounts have higher continuation values the more likely these choices are, and the higher lenders' payoffs are given these choices. Account holders may also choose to close their account, which yields zero payoff and continuation

[51]See footnote 44.

value for the firm.[52]

Profits on existing accounts take a similar form to profits on new accounts,

$$
\begin{aligned}
\Pi^1(p_j, p_{-j}, \theta, k) = & \underbrace{\Pr_j^1(b|\theta, p, k)\left(p^1_{\theta j} - c^1_{\theta j}\right)}_{\text{flow profit}} + \\
& \underbrace{\Pr_j^1(b|\theta, p, k)\beta(1-\delta(\theta))T_{\theta\theta'}(\theta)\Pi^1(p_j, p_{-j}, \theta', b)}_{\text{exp. cont. profit } | \text{ borrow}} + \\
& \underbrace{\Pr_j^1(n|\theta, p, k)\beta T_{\theta\theta'}(\theta)\Pi^1(p_j, p_{-j}, \theta', n)}_{\text{exp. cont. profit } | \text{ not borrow}}
\end{aligned}
\tag{1.4.12}
$$

Here the primary difference between existing account profits and new account profits is that expected costs $c^1_{\theta j}$ are only paid if a consumer chooses to borrow, reflecting how existing-account costs primarily depend on loan default. Additionally, firms earn existing-account prices $p^1_{\theta j}$ and incur existing-account costs $c^1_{\theta j}$ rather than the new-account terms $p^0_{\theta j}$ and $c^0_{\theta j}$.

Notwithstanding the apparent similarity in these two profit functions, lenders' pricing problem on new accounts is starkly different from the pricing problem on existing accounts. This is because of the different types of information available to lenders on new and existing accounts. As discussed earlier, lenders' must make new account pricing decisions on the basis of "public" information available in credit reports, whereas pricing on existing accounts can depend on private information that a lender learns over the course of a lending relationship. I express these constraints in the following informational assumption: lenders observe only a borrower's public type x on a newly originated account, whereas lenders observe a consumer's full type $\theta = (x, \psi)$, including the private type ψ, on existing accounts.[53]

Given this informational assumption, I impose the natural restriction that lender pricing strategies on new accounts must be the same for all types θ that have the same public type x,

$$p^0_{\theta j} = p^0_{x(\theta)j} \ \forall\ \theta$$

where $x(\theta)$ selects the public component of types $\theta = (x, \psi)$. To be consistent with this restriction, I also suppose acquisition costs take the form $c^0_{\theta j} = c^0_{x(\theta)j} \ \forall\ \theta$.

In choosing prices p^0_{xj} a lender therefore takes into consideration its expectation of which private types ψ it acquires as new customers at any given price level, expressed below as a sum over types θ that share a given FICO score x, competing lenders j', and borrowers'

[52]Furthermore, lenders also lose any continuation value (but still receive flow profits) if an account used for borrowing goes into default at the end of the period; as described previously in the demand model, accounts in default are closed permanently at the end of the period.

[53]This assumption precludes borrowers behaving strategically in a way that prevents lenders from observing their true type, although it does allow for a signal-jamming behavior in which all consumers try to appear safer or more price sensitive than they truly are, so that lenders nonetheless infer their type.

past-period choices k,

$$\Pi^0(p_j, p_{-j}, x) = \sum_{j' \neq j} \sum_{\theta : x(\theta) = x} \sum_{k \in \{b,n,0\}} \mu_{j',\theta,k}(p) \Pr(j | p, j', k, \theta) \Pi^0(p_j, p_{-j}, \theta, k) \tag{1.4.13}$$

Here the weights $\mu_{j',\theta,k}$ are the share of consumers who are of type θ, who held a credit card from lender j' in the prior period (or held no card in the case of $j' = 0$), and who used that card for $k \in \{\underline{\text{b}}\text{orrow, tra}\underline{\text{n}}\text{sact, out}\} \equiv \{\text{b,n,0}\}$, as a function of the market price vector p.

Given the above expressions for $\Pi^0(p_j, p_{-j}, x)$ and $\Pi^1(p_j, p_{-j}, \theta, k)$, the lender's pricing problem can now be written as,

$$\max_{p_j} \sum_x \Pi^0(p_j, p_{-j}, x) + \sum_\theta \left[\mu_{j,\theta,b}(p)\Pi^1(p_j, p_{-j}, \theta, b) + \mu_{j,\theta,n}(p)\Pi^1(p_j, p_{-j}, \theta, n)\right] \tag{1.4.14}$$

In the following subsection I describe how I estimate the supply side of the model using the first-order conditions of this optimization problem. I also describe three distinct steps in estimating the demand side of the model: recovering borrower types θ and the probability distribution over types μ_θ; estimating the parameters γ_θ that govern consumers' price elasticities, conditional on types; and finally estimating all remaining demand parameters, conditional on both types and estimated elasticities.

1.4.3 Model Estimation

Demand Estimation: Borrower Private Types

The first step in demand estimation is recovering a type θ for each borrower in the data. To emphasize, rather than estimating a *parametric* mixture model of types, in which the key objects to be estimated would be parameters of the type mixture distribution, I instead recover a single type for each consumer in the data, and allow the distribution over types to remain flexible.

Recall types θ are the joint of public and private types, $\theta = (x, \psi)$. Finding borrowers' public types x is straightforward: I allow each borrower's public type to be a binned version of his FICO score. I make this choice because FICO scores are expressly designed to be a one-dimensional composite of all publicly available information predicting default, and because FICO scores are readily observable in the data. I use 20-point FICO score bins, which are a standard set of bins, or "breaks," the credit card industry uses to group borrowers for account management purposes. Additionally I pool all FICO scores of 599 or below into a single bin and all FICO scores of 780 or above into a single bin. This yields a total of 11 distinct public types x.

With these public types so defined, the remaining part of this exercise is to recover private types ψ. Empirically, my approach here builds on other literatures that seek to identify unobservable ex ante types from ex post outcomes, for example the public economics literature on annuities markets that estimates ex ante frailty using ex post mortality (Finkelstein and

Poterba (2004), Einav et al. (2010)). Here I use a similar outcome, loan default, to recover ex ante borrower types. Because borrower types change over time, and also because default is only observed at most once for each account, this exercise is more complex than simply estimating individual-level residual default risk after controlling for FICO. Rather, I develop an empirical strategy that recovers these private types from the observed pricing that each borrower faces in each period.

Here I make use of Assumptions 1 and 2 developed in the previous section. It can be shown that a straightforward implication of these two assumptions is that equilibrium prices $p^{\star}$ are increasing in private types ψ for all public types x and all lenders j,[54]

$$\psi' > \psi \implies p^{\star 1}_{j,x,\psi'} > p^{\star 1}_{j,x,\psi} \ \forall\ x, j \tag{1.4.15}$$

Recall also from equation 1.4.9 that default rates δ are also increasing in private types ψ for all FICO scores x and all banks j. So, default rates and prices $p^{\star 1}$ are increasing with respect to each other,

$$\hat{\delta}_{jx}\left(p^1_j\right) \nearrow p^1_j \tag{1.4.16}$$

where $\hat{\delta}_{jx}$ is the default rate as an indirect function of prices in equilibrium, among borrowers with FICO score x for lender j. Finally, using the inverse of δ implied by equation 1.4.9, private types can be recovered by inverting default rates observed at each price level,

$$(x, \psi) = \delta^{-1}_x(\hat{\delta}_{jx}((p^1_j(x, \psi))) \ \ \forall x \tag{1.4.17}$$

Note that equilibrium price schedules p^1_j are lender-specific, as are the indirect functions δ_{jx} relating these prices to realized default rates. However the inverse δ^{-1}_x maps default rates, which are *common* for all borrowers of a given type, back to types. So in estimating the model, $\hat{\delta}_{jx}$ is estimated separately by lender and by FICO score x, while δ^{-1}_x is estimated across all lenders – i.e., for the market as a whole – within each FICO group.

To do this inversion in practice, I first use isotonic regression to estimate $\hat{\delta}_{jx}$ for each lender j and FICO score group x. The default measure I use is delinquencies of 90+ days within the following two years, as this is the outcome FICO scores themselves are specified to predict. In a few cases where the fitted isotonic functions for a particular lender map onto a strict subset of the population distribution of default rates at a given FICO score, I use linear interpolation or extrapolation to extend the estimated function. This procedure results in $\hat{\delta}_{jx}$ being a consistent estimate of actual default rates at each price level, given Assumptions 1 and 2.

To define the inverse $\delta^{-1}_x(\cdot)$, I use the fact that private types ψ are an index of default risk (see equation 1.4.9), and I therefore specify $\delta^{-1}_x(\cdot)$ to return quantiles of the population distribution of estimated default rates, for a desired number of quantiles. In my baseline

[54]This result also makes use of an informational assumption I develop on the supply side of the model, that lenders observe ψ after having a relationship with a consumer in the preceding period.

estimation I take 5 such quantiles (i.e., quintiles). This yields 5 private types for each of the 11 public types, for a total of 55 consumer types θ. I then also bin each lender's pricing functions $p_j^1(x,\psi)$ to that lender's average price at each bin.

This process is illustrated for two actual lenders in the data in the three panels of Figure 12. As can be seen, a borrower of a given type shares a common default rate regardless of his current bank, while the price faced by each borrower is different depending on the bank he chooses. The raw data also show that the fit of the isotonic regressions is quite good – that is, true pricing functions do appear to be (nearly) monotone in default rates.

The consumer types estimated in this process make it straightforward to study the dynamics of how types change over time. In particular, the transition matrix $T_{\theta\theta'}$ can be estimated non-parametrically off of type-to-type transition rates for borrowers who are observed in two successive periods. This takes advantage of the independence of type transitions from borrower choices and default outcomes: type transitions do not depend on borrower choices or realized default, and borrowers do not choose entry or exit from the market in anticipation of type transitions, as these transitions are not yet realized at the time choices are made. The estimated transition matrix is illustrated as a contour plot in Figure 13. Here, the integer-labeled type indices correspond to the 11 different 20-point FICO score groups described earlier, while the sub-ticks within each integer index correspond to the 5 discrete private types ψ within each FICO group. As can be seen, types are strongly but not perfectly persistent, in both public and private dimensions. The rippling pattern evident in the plot shows the same phenomenon seen previously in Table 8, whereby borrowers of highly risky *private* types are more likely to be downgraded to a lower FICO score next period than other borrowers are.

Finally, after verifying that the estimated transition matrix $T_{\theta\theta'}$ is ergodic, this matrix can be used to recover the probability distribution over types μ_θ. Recovering this distribution is necessary even though ψ was taken to be quintiles of a default rate distribution. This is because these default rates are only observed for consumers who choose to *borrow*; hence, while there is a uniform distribution (within FICO score) of types among borrowers, the overall distribution of types may not be uniform, if different types have different probabilities of borrowing. To overcome this difficulty, I simply use the fact that type transition matrix $T_{\theta\theta'}$ operates independently of consumers' choices of whether to hold a credit card and whether to borrow, so ergodicity implies a unique steady state μ_θ that satisfies the equation $\mu_\theta = T_{\theta\theta'}\mu_\theta$.

Demand Estimation: Demand Elasticities

The next demand parameters to estimate are price elasticities of borrowing demand η_{ij}, across consumers i and credit card issuers j. In general demand elasticities change as prices change, so it is helpful to estimate primitives that determine these elasticities rather than merely estimate the elasticities themselves, as the latter only are local to a particular equilibrium. I therefore use the well-known relationship between demand elasticities and marginal utilities of income in logit demand,

$$\eta_{ij} = -\gamma_i p_{ij}(1 - Q_{ij}) \tag{1.4.18}$$

Here γ_i is consumer i's marginal utility of income, p_{ij} is consumer i's price of borrowing from lender j, and Q_{ij} is consumer i's probability of choosing to borrow from lender j. In particular I use η_{ij} to denote the elasticity of *continued* borrowing among current borrowers, so that the price on the right-hand side denotes lender j's pricing on mature credit card accounts, and Q_{ij} denotes a retention probability for current borrowers. Intuitively in this expression higher marginal utilities of income make borrowers more price elastic.

To derive an estimating equation for γ_i that uses price variation such as the repricing campaign illustrated previously in Figure 10, I first substitute for η_{ij} using the definition of an elasticity,

$$\mathrm{dlog}(Q_{ij}) = -\gamma_i p_{ij}(1 - Q_{ij})\mathrm{dlog}(p_{ij}) \tag{1.4.19}$$

I then draw on the form of borrower heterogeneity specified in section 1.4.2, and I take this equation from the level of individual consumers i to the level of consumer types θ. This leverages in particular the assumption that borrower types x and ψ capture all relevant borrower heterogeneity in the model (with $\theta = (x, \psi)$). This simply changes i subscripts to θ subscripts in the above, and substitutes observed type-level retention rates $Q_{\theta j}$ in lieu of of individual retention probabilities Q_{ij}.

Finally I use difference-in-differences in logs as empirical analogs of infinitesimal changes in logs,

$$\log Q_{\theta jt} = \alpha_{\theta j} + \alpha_t + \beta_j t - \gamma_\theta \log P_{\theta jt} + \epsilon_{\theta jt} \tag{1.4.20}$$

Here the fixed effects denoted by α implement difference-in-differences, and the term $P_{\theta jt}$ is a price term scaled as in equation 1.4.19 above, with scalars taken from the period immediately prior to a repricing denoted here by $t = 0$,[55]

$$\log P_{\theta jt} = (1 - Q_{\theta j0}) p_{\theta j0} \log(p_{\theta jt}) \tag{1.4.21}$$

Meanwhile the β term is included to account for different trends among the included banks; I explore robustness to excluding this term below. This equation differs from the earlier event-study version shown in equation 1.4.1 and Figure 10 only through the regressor $P_{\theta jt}$, which, following the above derivation, makes it possible to recover the primitives γ_θ rather than just a local elasticity.

I estimate γ_θ using both limited-information maximum likelihood and two-stage least squares, with instruments that isolate the type of repricing variation highlighted in Figure 10. Specifically, I instrument for the endogenous price term $P_{\theta jt}$ with a dummy instrument Z_{jt} equal to unity in all periods t following a repricing campaign by lender j. As is standard in

[55]These base-period values are chosen because they correspond to demand elasticities at the time of the repricing, as in equation (1.4.18).

a model that is fully interacted with consumer types θ, these instruments are also interacted with indicators for borrower types θ, so that there are $|\Theta|$ instruments corresponding to the $|\Theta|$ endogenous regressors $P_{\theta jt}$.[56] Note that these instrumental variables address two econometric issues, both the endogeneity of prices $p_{\theta j}$ with borrowers' marginal utilities γ_θ, and, in time period 0, the appearance of $Q_{\theta j0}$ on both the right- and left-hand sides. In summary, the first and second stage equations are then,

$$\log P_{\theta jt} = a_{\theta j} + a_t + b_j \times t + \pi_\theta Z_{jt} \times 1_\theta + e_{\theta jt} \tag{1.4.22}$$
$$\log Q_{\theta jt} = \alpha_{\theta j} + \alpha_t + \beta_j \times t - \gamma_\theta \log P_{\theta jt} + \epsilon_{\theta jt} \tag{1.4.23}$$

Given that $P_{\theta jt}$ contains the estimated quantity $Q_{\theta j0}$, I bootstrap to calculate standard errors.

Table 10 presents estimates corresponding to the repricing quasi-experiment shown in Figure 10. The first column shows OLS estimates of equation 1.4.23, while the second column then shows corresponding 2SLS estimates that use variation from the first-stage equation 1.4.22. Comparing these two estimates lends credence to the instrumental variables strategy: the OLS estimate of γ is substantially closer to 0 than is the 2SLS estimate, as would be expected if the instruments overcome the standard endogeneity problem whereby higher prices are charged to less price-sensitive borrowers in equilibrium.

The next column of the table then examines how estimates change with the exclusion of bank-specific time trends β_j, and the final column of the table explores heterogeneity in marginal utilities γ across borrower types. As can be seen, the inclusion of bank-specific trends changes the resulting estimates of γ slightly, with estimates falling from .106 to .0696 when these trends are excluded. Furthermore, heterogeneous estimates of γ across borrower types vary in a sensible way, with higher marginal utilities of income for lower-credit score borrowers who are, on average, lower income. Note that the estimates of γ I ultimately use in solving the model are presented in Figure 14, where I allow γ to vary flexibly across consumers' public types x.

For the estimates I present in this table, the set of instrumental variables I use are drawn from the repricing quasi-experiment illustrated previously in Figure 10. This particular quasi-experiment has the advantage that I have been able to verify important background details that help validate the exogeneity of this repricing campaign vis-a-vis existing borrowers' demand: documents from this lender's investor relations materials emphasize that the lender was seeking to consolidate its credit card portfolio at this time in advance of an upcoming merger or acquisition -- as would be rationalized by the bank's internal cost of capital changing in anticipation of such an acquisition. This merger or acquisition was not consummated until several quarters after the repricing event in question, so it likely did not substantially change the competitive environment in the event-time months immediately

[56] The high number of interaction terms motivates using limited-information maximum likelihood estimates in lieu of two-stage least squares estimates, to help overcome finite-sample bias in a setting with many instruments.

following the repricing quasi-experiment I use here. The lack of a detectable change in other competitors' pricing strategies at this time, as evidenced by the blue dashed line in Figure 10, also support the exogeneity of the repricing event.

Demand Estimation: Taste Parameters

Given the above estimates of each consumer's type θ and borrowers' price sensitivities corresponding to γ_θ, the remaining model parameters to be estimated are the flow utilities $d_{j\theta}$, $n_{j\theta}$, $s_{j\theta}$, and $l_{j\theta}$. Recall these terms are, respectively, flow utilities from borrowing, flow utilities from transacting (rather than borrowing), setup costs for opening an account with a new lender, and liquidity costs for paying off a balance in order to transition from borrowing to transacting. These are estimated by matching key moments of the data corresponding to the three key facts outlined in section 1.4.1, each moment being an observed probability that is matched to a corresponding likelihood predicted by the model. In particular, these moments are: borrowers' persistence in borrowing behavior; non-borrowing consumers' persistence in *non*-borrowing behavior; account closure rates for borrowers; and account opening rates for consumers not holding credit cards.

To help illustrate how such moments identify the remaining model parameters, Figure 15 shows the example of how borrowers' persistence in borrowing behavior (i.e., lenders' retention rates among borrowers) identify flow utilities from borrowing, $d_{j\theta}$. The figure shows, for each FICO score group on the x-axis, the highest and lowest borrower retention rates across all lenders in solid lines; these lines are simply the upper and lower envelopes of retention rates in the market. The figure also shows in dashed lines the fee-inclusive prices[57] charged by the lenders in these upper and lower envelopes.[58] Reading across the FICO score distribution from low to high, note that at the bottom of the distribution the lender with the highest retention rate also charges a relatively high price of 45 percentage points annualized, relative to 20 percentage points for the lowest-retention lender; meanwhile in the middle of the FICO distribution, the price gap between high- and low-retention lenders converges to nearly zero, and at the top of the price distribution, the highest-retention lender instead charges lower prices than the lowest-retention lender. This pattern identifies differences in $d_{j\theta}$ for these high- and low-retention lenders across the FICO score distribution. At the bottom of the distribution, the gap in these two values of $d_{j\theta}$ must be large enough to rationalize borrowers having relatively high retention rates on a lender that is also relatively high priced; intuitively, this high-retention lender is a bank whose credit cards are particularly attractive to borrowers at the bottom of the FICO score distribution. In the middle of the FICO score distribution, the smaller price gap at comparable retention rates identifies a smaller

[57]Recall I use the *fee-inclusive* borrowing cost introduced in Section 1.2.2 when I estimate these prices in the data; these are also the appropriate marginal prices to use when modeling the *extensive* margin of borrowing.

[58]The figure is designed this way, using upper and lower envelopes rather than just showing two example lenders, so as to protect firms' confidentiality and avoid displaying the full price schedule for any single lender.

gap between these two values of $d_{j\theta}$, and at the top of the distribution, the reversal of the price gap points to this $d_{j\theta}$ gap being even smaller. In brief, the patterns in Figure 15 point to credit card product differentiation being a relatively important determinant of borrowing demand at the bottom of the credit score distribution, and a less important factor at higher credit scores.

Supply Estimation

The lender's maximization problem in equation 1.4.14 has tractable first-order conditions because many pricing decisions are made independently. This independence follows from lenders' lack of commitment power in the pre-CARD-Act regulatory regime, which implies a deviation in $p^1_{j\theta}$ only affects profits earned on existing accounts for consumers of type θ, and likewise a deviation in p^0_{jx} only affects profits earned on new accounts among consumers of public type x. Furthermore continuation profits are unaffected by these one-period deviations. The first-order condition for $p^1_{j\theta}$ at the equilibrium price vector $p^\star$ is thus, for a given θ,

$$\begin{aligned}\sum_{k\in\{b,n\}} \mathrm{Pr}^1_j(b|\theta,p^\star,k) = \sum_{k\in\{b,n\}} & \gamma_\theta \mu_{b,\theta,k}(p^\star)\mathrm{Pr}^1_j(b|\theta,p^\star,k)\left(\mathrm{Pr}^1_j(b|\theta,p^\star,k)\right)\times \\ & \left[p^1_{\theta j} - c^1_{\theta j} + \beta(1-\delta(\theta))T_{\theta\theta'}(\theta)\Pi^1(p_j,p_{-j},\theta',b)\right] \\ & - \gamma_\theta\mu_{n,\theta,k}(p^\star)\mathrm{Pr}^1_j(b|\theta,p^\star,k)\left(\mathrm{Pr}^1_j(n|\theta,p^\star,k)\right)\times \\ & \left[\beta(1-\delta(\theta))T_{\theta\theta'}(\theta)\Pi^1(p_j,p_{-j},\theta',b)\right]\end{aligned} \tag{1.4.24}$$

First-order conditions for prices on newly originated accounts p^0_{jx} are similarly, for a given x,

$$\begin{aligned}&\sum_{j'\neq j}\sum_{\theta:x(\theta)=x}\sum_{k\in\{b,n,0\}} \mu_{j',\theta,k}(p)\mathrm{Pr}(j|p,j',k,\theta) = \\ &\sum_{j'\neq j}\sum_{\theta:x(\theta)=x}\sum_{k\in\{b,n,0\}} \gamma_\theta\mu_{b,\theta,k}(p^\star)\mathrm{Pr}^0_j(b|\theta,p^\star,k)\left(1-\mathrm{Pr}^0_j(b|\theta,p^\star,k)\right)\times \\ &\qquad\left[p^1_{\theta j} - c^1_{\theta j} + \beta(1-\delta(\theta))T_{\theta\theta'}(\theta)\Pi^1(p_j,p_{-j},\theta',b)\right] \\ &\qquad - \gamma_\theta\mu_{b,\theta,k}(p^\star)\mathrm{Pr}^0_j(b|\theta,p^\star,k)\mathrm{Pr}^0_j(n|\theta,p^\star,k)\times \\ &\qquad\left[\beta T_{\theta\theta'}(\theta)\Pi^1(p_j,p_{-j},\theta',b)\right]\end{aligned} \tag{1.4.25}$$

The number of free supply parameters $\{c^0_{xj}, c^1_{\theta j}\}$ is equal to the number of prices set for all lenders j, and hence equal to the number of first-order conditions. The parameters are therefore just-identified and quickly converge in a procedure that minimizes squared violations of these FOCs.

1.4.4 Model Parameter Estimates

This subsection presents my estimates of model parameters. I emphasize two key results on the demand side of the model and two key results on the supply side.

First, on the demand side, my estimates of consumers' utility from borrowing (the parameters $d_{j\theta}$) correlate strongly with default rates across borrower types; this confirms a basic adverse selection property, that the highest-risk borrowers are also the borrowers with the greatest demand for credit. In Figure 16 I plot estimates of these flow utilities and also borrowers' average default rates, by type and by lender. The three panels of the figure correspond to three representative FICO scores, while the x-axis of each figure shows different borrower private types ψ. The evident pattern in these figures is that, across the FICO score distribution, borrower default rates are strongly correlated with demand for borrowing, with the highest-risk types also exhibiting the highest credit demand. The correlation between these two quantities across types θ ranges from .44 to as high as .88, depending on the lender. This correlation emerges mostly from the strong correlation between price and risk in the pre-CARD-Act data used to estimate the model, as these high demand parameters are revealed by consumers' willingness to borrow at those high rates.

Second, my estimates of the remaining demand parameters indicate that account set-up costs are a substantial friction for consumers, in particular limiting the degree to which many borrowers are able to refinance balances with competing lenders. Other parameters, including exit costs from borrowing and flow utilities from transacting, are only modestly important for determining consumer behavior. In Figure 17 I plot average account setup costs s, liquidity costs l, and utilities from transacting n across lenders and across borrower private types. The x-axis shows consumer FICO score groups, and the y-axis plots dollarized values of these utility parameters. For sake of comparability, these utilities are dollarized using the homogeneous marginal utilities of income (logit price coefficients) estimated in Table 10, not the heterogeneous marginal utilities presented in Figure 14. These estimates indicate that account setup costs are a substantial friction for consumers looking to switch credit cards or refinance their credit card balance with another lender, while exit costs from borrowing and demand for credit cards as transactional products are less important in driving consumer behavior. In particular, I estimate that for a FICO 700 borrower, the dollarized switch cost for setting up a new credit card account is roughly \$900, on par with the total pecuniary benefit from a typical new credit card's teaser interest rate spread over 4.5 years.[59]

On the supply side of the model, I first highlight that my estimated marginal cost parameters for borrowing correspond with default rates – the primary driver of lender costs – across consumer types. I also highlight how my estimates of lenders' costs for originating new accounts correspond to industry reports of average marketing, underwriting, and pro-

[59]This back-of-the-envelope calculation draws on the average annualized price gap between mature and new credit card accounts for FICO 700 borrowers shown in Table 6, and also the average credit card balance for FICO 700 borrowers of \$4000 dollars. These switch costs are several times larger than but still the same order of magnitude as the switch costs estimated in Shui and Ausubel (2004).

cessing costs associated with account origination. I present these results in Figures 18 and 19. Figure 18 follows a similar format to Figure 16 above: the panels of the figure correspond to three different FICO score groups, while the x-axis of each figure shows different borrower private types ψ. The striking pattern from the figure is that these costs are strongly correlated with default, but also that the cost estimates are not a consistent scalar multiple of default rates. On the one hand, this strong correlation indicates that the model first-order conditions are able to recover lender costs that closely follow the primary driver of actual costs, as reported in administrative data in the CCDB; this is an important validation for the model. On the other hand, the ratio between these estimated costs and data on default rates suggests a roughly 0% recovery rate on defaulted loans for the riskiest borrowers, and a closer to 100% recovery rate on defaulted loans for the safest borrowers. With industry average recovery rates around 10% and the majority of defaults generated by the riskiest borrowers, the scaling on these marginal costs therefore also appears consistent with industry benchmarks. Second, in Figure 19 I plot my estimates of lenders' costs for originating new accounts, separately by lender and across the FICO score distribution. The clear pattern in the plot is that lenders' acquisition costs are steadily increasing in FICO score; this is consistent with the extra incentives, for example airline miles, that lenders often use to encourage opening of new credit card accounts for higher FICO-score consumers. These estimates are roughly on par with industry estimates of the average cost of marketing, underwriting, and processing new accounts, which average roughly $200 per account.[60]

1.5 Equilibrium Effects of CARD Act Price Restrictions

I now use the model developed in the previous section as a tool to study the CARD Act's pricing restrictions. I impose the Act's restrictions in the model while otherwise leaving the pre-CARD-Act environment unchanged, and I analyze these restrictions' effects on pricing, borrowing choices, and total welfare after the model converges to a new equilibrium. This exercise is informative in three ways. First, this exercise makes it possible to analyze the mechanisms behind the effects of CARD-Act-like pricing regulation. Second, I use this exercise to assess the CARD Act pricing restrictions' effects across a range of consumer types, including borrowers who choose to exit the market after the restrictions take effect. Finally, this exercise helps identify the CARD Act pricing restrictions' effects in isolation from other non-price regulation included in the Act and other contemporaneous shocks to consumer credit markets.

[60]While industry contacts emphasize the high cost of new account acquisition, it is also plausible for the model to estimate these costs to be negative, especially on subprime accounts, reflecting fee revenue at the time of origination such as application fees that are not otherwise reflected in lender revenues in the model.

1.5.1 Modeling CARD Act Price Restrictions

I model the CARD Act price restrictions as a mandate that firms commit to a single long-run price on each credit card contract at the time of origination. Contracts also include a promotional or "teaser" rate for one period before the long-run price takes effect, as such teasers were an important carve-out still permitted under the Act. A credit card contract under the new restrictions therefore takes the form of a duple (p_j^0, p_j^1) for lender j, containing an initial teaser rate and a subsequent long-run rate.

The prices on a contract depend only on a consumer's public type (FICO score) at origination, x_0. In particular, a contract's long run price can no longer depend on private information ψ_t revealed to a lender over the course of an account-holding relationship, as these private types are unobservable at origination. A contract's long run price also can no longer depend on updated FICO scores x_t over time. That is,

$$\begin{aligned} \text{Pre-CARD-Act:} \quad & p_1^j = p_1^j(x_t, \psi_t) \\ \text{Post-CARD-Act:} \quad & p_1^j = p_1^j(x_0) \end{aligned} \tag{1.5.1}$$

However teaser rates continue to depend only on public types at origination, as they did in the pre-CARD-Act regime.

It is important to consider such teaser rates when imposing the CARD Act's pricing restrictions in the model, because the availability of these rates implies that the Act's price effects may differ substantially for consumers with different propensities to switch credit cards frequently. Consumer types who bear low setup costs (the demand parameter $s_{\theta j}$) on new accounts might serially transfer balances across cards to take advantage of promotional rates repeatedly, whereas consumers who bear higher setup costs are less likely to do so. Additionally, the Act may lead to less generous terms on new accounts by reducing the rents lenders are able to extract on these accounts in later periods (as in Petersen and Rajan (1995)), and including teaser rates p^0 when imposing the CARD Act's pricing restrictions in the model provides a means to study such effects.

To emphasize, the prices set at origination are only in effect for as long as a consumer keeps a given contract. Once the consumer closes a given credit card account and opens another, the new account's pricing reflects the consumer's public type at the time the new contract is originated. A basic intuition explains switching behavior in this environment: all else equal, a consumer becomes more likely to switch accounts as the gap increases between (1) his current contract's long-run price, $p_j^1(x_0)$, which was determined by his *past* public type at the time he originated this contract, and (2) a competing lender's teaser rate on a new contract, $p_{j'}^0(x_t)$, which is determined by the consumer's *current* public type.

I study an equilibrium where each firm can offer only one contract to each public type at origination. I make this restriction in part for sake of realism and in part for tractability. It is in practice rare for credit card lenders to offer a *menu* of contracts to the same borrower at the same point in time, and this restriction also avoids the difficulty of solving for an entire menu of contracts for each lender, and each public type, in an imperfectly competitive environment (Stole (2007)). As my model results later confirm, this "one contract per firm per origination credit score" specification still allows substantial price dispersion at each public type, as differentiated lenders post different price duples (p^0, p^1) to each public type.

Specifying the firm's problem in the presence of these repricing restrictions requires keeping track of the share of consumers of each type θ who hold a contract that they originated when they were of type x_0, where x_0 is potentially different from the current public type $x(\theta)$. This requires a slight update to the notation I used in the original model exposition. Previously I used $\mu_{j,\theta,k}(p)$ to denote the share of consumers of each type θ who hold a credit card with bank j, who use that card the purpose $k \in$ {borrow, transact} $\equiv$ {b,n}, in a market where banks offer the price vector p. I now additionally keep track of the share of consumers who make each of those choices *while* holding a contract they originated at public type x_0, which I denote $\mu_{j,\theta,x_0,k}(p)$. As before, this vector denotes the (unique) long-run distribution of consumers across contracts and choices for a given price vectpr p, where flows into a given component of $\mu_{j,\theta,x_0,k}(p)$ are equal to flows out of that component.

Bank j's expected discounted lifetime profits on a mature account can then be written in a form similar to equation 1.4.12,

$$\begin{aligned}\Pi^1(p_j, p_{-j}, \theta, x_0, k) = &\underbrace{\Pr_j^1(b|\theta, p, x_0, k)\left(p^1_{x_0 j} - c^1_{\theta j}\right)}_{\text{flow profit for type } \theta \text{, contract } x_0} + \\ &\underbrace{\Pr_j^1(b|\theta, p, x_0, k)\beta(1-\delta(\theta))T_{\theta\theta'}(\theta)\Pi^1(p_j, p_{-j}, \theta', x_0, b)}_{\text{exp. cont. profit | borrow}} + \\ &\underbrace{\Pr_j^1(n|\theta, p, x_0, k)\beta T_{\theta\theta'}(\theta)\Pi^1(p_j, p_{-j}, \theta', x_0, n)}_{\text{exp. cont. profit | not borrow}}\end{aligned} \tag{1.5.2}$$

Expected discounted lifetime profits on new accounts are defined analogously, by making slight revisions to equation 1.4.11 to show continuation profits' dependence on a consumer's origination type $x_0(\theta)$,

$$\begin{aligned}\Pi^0(p_j, p_{-j}, \theta, k) = &\underbrace{\Pr_j^0(b|\theta, p, k)p^0_{x_0(\theta),j} - c^0_{x_0(\theta),j}}_{\text{flow profit}} + \\ &\underbrace{\Pr_j^0(b|\theta, p, k)\beta(1-\delta(\theta))T_{\theta\theta'}(\theta)\Pi^1(p_j, p_{-j}, \theta', x_0(\theta), b)}_{\text{exp. cont. profit | borrow}} + \\ &\underbrace{\Pr_j^0(n|\theta, p, k)\beta T_{\theta\theta'}(\theta)\Pi^1(p_j, p_{-j}, \theta', x_0(\theta), n)}_{\text{exp. cont. profit | not borrow}}\end{aligned} \tag{1.5.3}$$

and likewise by revising equation 1.4.13 to reflect a sum over inflows from competitors' contracts originated at types x_0,

$$\Pi^0(p_j, p_{-j}, x) = \sum_{j' \neq j} \sum_{\theta : x(\theta) = x} \sum_{k \in \{b,n,0\}} \sum_{x_0} \mu_{j',\theta,x_0,k}(p) \Pr(j | p, j', \theta, x_0, k) \Pi^0(p_j, p_{-j}, \theta, k) \quad (1.5.4)$$

With this notation in hand, a lender's total expected discounted profits across both new and mature accounts under the restricted equilibrium can be written as,

$$\begin{aligned} \Pi_j(p_j, p_{-j}) = &\underbrace{\sum_x \Pi^0(p_j, p_{-j}, x)}_{\text{newly acquired accounts}} + \\ &\underbrace{\sum_\theta \sum_{x_0} \sum_{k \in \{b,n,0\}} \mu_{j,\theta,x_0,k,}(p) \times \Pi^1(p_j, p_{-j}, \theta, x_0, k)}_{\text{existing accounts}} \end{aligned} \quad (1.5.5)$$

I use successive lender best-replies that maximize this profit function to compute the new equilibrium, beginning this process at the pre-CARD-Act equilibrium price vector.[61] In practice, I find that a market equilibrium gets close to convergence after 5 to 8 iterations of updating lenders' best replies, while subsequent iterations are mostly needed to pin down prices on thinly traded contracts that few consumers choose in equilibrium. For market-level aggregate statistics, such as the average price paid at origination by a consumer of type x_0, model runs therefore exhibit substantial stability quite early in this iteration process.

1.5.2 Equilibrium Effects of CARD Act Price Restrictions

The estimated post-CARD-Act equilibrium reveals how the two forces of market power and adverse selection trade off in different parts of the credit card market. I find that market unraveling due to adverse selection after the Act is moderately severe among the most subprime of consumers, whereas the benefits of reduced markups are dominant at higher credit scores. Nevertheless, consumer surplus conditional on credit score rises at *all* credit scores, even in credit score segments where unraveling is relatively severe, reflecting the relative importance of price decreases for the riskiest and most inelastic of borrowers. Total

[61]These best replies serve both as a computational tool to iteratively find the new equilibrium, and as an equilibrium selection device. Similar to some other empirical work that has simulated a new market equilibrium under a new regulatory regime (e.g., Ryan (2012)), it is difficult to rule out the presence of multiple equilibria in my setting, which here can be seen most clearly from the fact that some bank j may find it profitable to originate contracts to a given FICO score if bank j' does not, whereas bank j' may find doing so profitable only if bank j does not. These two putative equilibria are also not unique up to the labeling of j and j', as each bank in the data faces different costs and provides different flow utilities for each consumer type. This process of successive best-replies from the pre-CARD-Act equilibrium seems most plausible as a device to select the post-CARD-Act equilibrium (as opposed to, for example, a starting price vector where all firms charge prices of zero). For evidence that firms indeed may converge on a new equilibrium gradually after a regulatory change by playing best replies to other firms' most recently observed pricing strategies, see Doraszelski et al. (2016).

surplus as well as consumer surplus rises in the highest credit score segments, where surplus lost due to adverse selection is lowest.

To illustrate these effects, Figures 20 and 21 respectively show contract prices and shares of consumers who borrow on credit cards, in pre-CARD-Act data and in the estimated post-CARD-Act equilibrium. The figures are divided into three panels for three representative FICO score segments in the deep subprime part of the market (FICO 580), in the near-prime segment (FICO 680, at the cusp between subprime and prime), and in the superprime segment (FICO 780). Each panel shows prices or borrowing shares across different private-information types within the relevant FICO score group.

Turning first to panel (a) of Figure 20, there is a shift from heterogeneous pricing (a separating equilibrium) across private-information types in pre-CARD-Act data, to nearly complete pooling in the estimated post-CARD-Act equilibrium. Under this pooled pricing, all private types are now estimated to pay a fee-inclusive cost of credit in excess of 50% annualized.[62] Only for the very riskiest and most inelastic of private types is this a lower rate than the average they paid in pre-CARD-Act data, and all other types face higher prices than they faced before.

These high prices are an equilibrium outcome driven in part by partial unraveling, whereby the safest private-information types exit from borrowing as prices rise, and the cost of lending to only the riskiest private-information types then drives prices higher still. Turning from panel (a) of Figure 20 to panel (a) of Figure 21, the data show these exit patterns sharply. In the pre-CARD-Act data, at least 30% of all private-information types used credit cards for borrowing; among all but the highest (riskiest) quintile, the shares who borrowed were roughly equal.[63] In contrast, in the estimated post-CARD-Act equilibrium the figure shows that the safest private-information types exit almost entirely from borrowing, and even the median private-information type has its borrowing share fall by over two-thirds. Meanwhile, the riskiest private-information types increase their borrowing share in response to the lower prices they face, as they are pooled with the safer of their peers who remain borrowers.

Turning to panels (b) and (c) of both figures, other credit score segments do not experience the same degree of unraveling as was seen among deep subprime consumers in panel (a). First in panel (b) of Figure 20, in the FICO 680 group nearly all private information types experience lower prices as a result of lower markups in the estimated post-CARD-Act equilibrium; only the safest quintile of private-information types face higher prices while being pooled with their riskier peers. Panel (b) of Figure 21 then shows how these relative price changes affect borrowing shares across types. While the very safest private types exit somewhat from borrowing, in response to the higher prices they face in the estimated

[62]These borrowing rates in fact track closely to some APRs seen among deep subprime credit cards in recent years, for example a 79.9% APR subprime credit card marketed in 2010 (Prater, 2010).

[63]In fact, this lack of correlation between prices and borrowing share is related to the near-zero correlation between borrowing probability and prices seen previously in the OLS estimates on pre-CARD-Act data in Table 10.

post-CARD-Act equilibrium, they do not exit to the same degree that analogously safe private types exited in panel (a). Meanwhile a greater share of all other private types borrow, reflecting these types' price decreases in the estimated post-CARD-Act equilibrium.

Panel (c) of both figures shows that the effects of reduced markups are even more pronounced at higher credit scores. In the example shown in Figure 20, in the FICO 780 group at the superprime end of the credit score distribution, all private-information types in fact face either reduced or nearly unchanged loan pricing. Correspondingly, in Figure 21, all private-information types in the FICO 780 group have greater borrowing shares in the estimated post-CARD-Act equilibrium.

While the price changes shown in Figure 20 give a sense of the long-run contract pricing faced by a consumer who *originates* a credit card at a given FICO score, some consumers hold contracts they originated in earlier periods when their credit scores differed. In particular, in the estimated post-CARD-Act equilibrium consumers can "lock in" relatively favorable rates by retaining a contract they originated at a higher credit score, as in equation (1.5.1). Given that borrowers are more likely to retain favorable contracts than unfavorable ones – the same adverse retention phenomenon documented previously in the reduced form results in Figure 6 – the average of all transacted prices among borrowers with a given credit score will generally be lower than contract prices for consumers who originated a contract *at* that FICO score. This phenomenon becomes clear in Figure 22, which shows average transacted prices on mature contracts at each FICO score, averaged across all private types, both in pre-CARD-Act data and in the estimated post-CARD-Act equilibrium. The average of transacted prices in the post-CARD-Act equilibrium is indeed lower than the contract prices shown in the previous Figure 20; for example, FICO 580 consumers' average prices are fully a third lower on average. Furthermore, the plot makes it clear that average *transacted* prices fall throughout the credit score distribution, reflecting both the attrition of borrowers who face price increases and the greater shares of borrowing among consumers who paid the highest prices conditional on their FICO score in pre-CARD-Act data.

Notwithstanding these decreases in transacted prices, the entry and exit patterns in Figure 21 suggest that the CARD Act's effects on consumer as well as total surplus could be ambiguous: quantities rise in some parts of the market and fall elsewhere.[64] In Figures 23 and 24, I show changes to consumer and total surplus resulting from the CARD Act price restrictions at each FICO group as well as in the market overall. As can be seen, consumer surplus rises across *all* FICO groups, reflecting the importance of markups on inelastic borrowers in determining pre-CARD-Act prices for many consumers. However, reflecting the relative importance of adverse selection among subprime consumers, the rise in subprime consumer surplus is mostly offset by a fall in lender profits on subprime accounts. In contrast, in the prime segment of the market both consumer and total surplus rise, reflecting

[64]In an adversely selected market with market power, equilibrium quantities are necessarily lower than efficient levels (Mahoney and Weyl (2014)), hence an increase in quantities *may* indicate a rise in total surplus. However, total surplus can still fall when quantities rise, depending on the composition of borrowers selecting into the market.

the relative importance of pre-CARD-Act markups rather than risk adjustment among these accounts.

These consumer surplus estimates also in part reflect the insurance value of the CARD Act's restrictions. Given that I estimated marginal utilities of income to generally rise as credit scores fall (see Figure 14), consumers prefer ex ante to shift high prices away from future states of the world where their credit scores are lower, and toward states of the world where their credit scores are higher.[65] This result also suggests analyzing the redistributive effects of the Act's pricing restrictions relative to other policies with more explicit redistributive goals (Hendren (2017)). To illustrate these redistributive effects more concretely, Figure 25 shows ZIP-imputed income for each consumer in the data as a function of that consumer's change in contract long-run prices under the CARD Act price restrictions.[66] Even though individual incomes can vary importantly from ZIP-code-level incomes, the figure suggests that the greatest price decreases as a result of the Act were incident on consumers who also had relatively low incomes, which emphasizes the value of exploring the Act's insurance value and redistributive effects in future work.

Despite the insurance value of the Act's restrictions, not all consumers with lower credit scores necessarily benefit. In part this was already seen in Figures 20 and 22, which show that although the average consumer who *holds* a credit card contract at low FICO scores benefits substantially from lower prices, consumers who wish to originate a *new* contract while holding a low credit score often face higher long-run prices on those contracts, especially if these borrowers are relatively safe private-information types. Figure 26 extends this finding to look at how lenders' total outlays on acquiring new accounts – including the cost of both promotional teaser rates and the direct costs paid for new account acquisition[67] – differ before and after implementing the Act's price restrictions. Consistent with the fall in long-run profitability among subprime accounts, but not among prime accounts, lender outlays for acquiring new subprime accounts fall modestly, while outlays to acquire new prime consumers increase substantially. Interestingly, this increase in outlays for new prime account acquisition matches recent trends in the post-CARD-Act credit market, where credit card issuers have invested heavily in new prime account acquisition (Kerr (2017)).

[65]This demand for insurance is in spite of a weak but opposite force that can be shown analytically to result from logit demand, whereby consumers prefer to shift high prices from states of the world where borrowing has a relatively choice probability (i.e., higher credit scores) to states of the world where borrowing has a higher choice probability.

[66]Whereas income is irregularly reported in the CCDB (and is drawn from credit card applications, where income is typically only self-reported), I use the availability of borrower ZIP code in the data to impute an average income at each ZIP code, using IRS Statistics on Income public data. I use IRS SOI data from the 2008 tax year, corresponding to the pre-CARD-Act equilibrium estimated in the model.

[67]Because the model captures the total outlay that credit card issuers invest in customer acquisition in two distinct parameters – both the acquisition cost of new accounts, and the teaser price provided to mature accounts – I present results on how the Act changes the sum of both parameters average across new accounts in different market segments. This sum in part reflects changing market shares across lenders with different acquisition costs for new accounts.

1.6 Conclusion

In this paper I study the consequences of restricting lenders from adjusting borrowers' interest rates in response to information acquired over the course of lending relationships. I focus on such restrictions in the 2009 CARD Act, which I find limited lenders' ability to adjust loan pricing in response to information about risk, but also in response to information about borrower demand characteristics. Building on reduced-form evidence, I develop and estimate a model that assesses how this policy caused partial market unraveling through unpriced risk, but also reduced lenders' rents on inelastic borrowers, and I use the model to study how this tradeoff affected pricing, borrowing choices, and total welfare in the market. Model estimates also uncover new facts about the credit card market, including the correlation between demand characteristics and risk, and the importance of lenders' private information in predicting borrower default. When I impose the CARD Act's price restrictions in the model, I find that the credit card market's new equilibrium involves partial unraveling, especially on subprime accounts, but sufficiently lower rents are extracted from most borrowers, such that consumer surplus rises and, in the prime credit card market, total surplus rises as well.

One important mechanism driving these results is that the CARD Act's price restrictions effectively provide price insurance for borrowers with deteriorating risk over time. Hence even though credit cards are not insurance products per se, they involve a tradeoff between insurance value and adverse selection similar to many insurance products. Handel et al. (2015) and Handel et al. (2016) evaluate this tradeoff empirically in a simulated health insurance exchange, and they find that the insurance value of restricting firms from pricing certain types of health information can be greater than the resulting welfare costs due to adverse selection. My results reach a similar conclusion in a very different setting, where I also consider issues of lender market power due, in part, to private information that lenders learn about consumers over time. Additionally, Handel et al. (2015) also find that restrictions on the pricing of health status lead to more severe unraveling than I estimate in the credit card market with CARD Act pricing restrictions, perhaps reflecting the nontrivial amount of risk-based pricing still allowed under the Act.

Promising areas for future work include studying the optimality of the CARD Act's price restrictions in a broader class of possible restrictions, potentially generalized through a tax on lenders' price changes that can be designed to balance the key forces I study here. Other alternative policies that can be evaluated in my modeling framework include a weaker version of the CARD Act's pricing restrictions that would allow lenders to adjust prices in response to changes in FICO score – but not other signals from borrowers – over time, and a stronger version of the Act's restrictions that would ban promotional teaser rates in addition to the Act's other price restrictions. In the credit card market more generally, my results also motivate additional analyses on what drives the dimension of consumer risk that appears through private-information types – for example, unanticipated income shocks versus permanently heterogeneous preferences – and how lenders differentially invest in screening such private information under different regulatory regimes.

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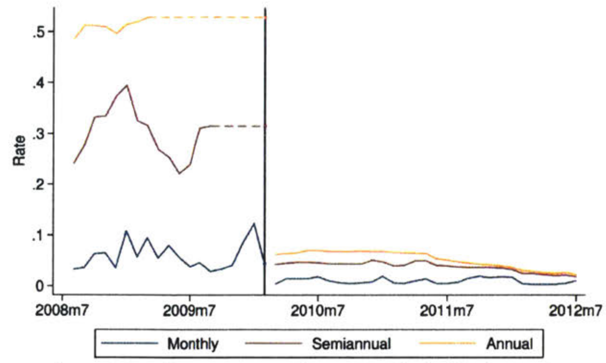
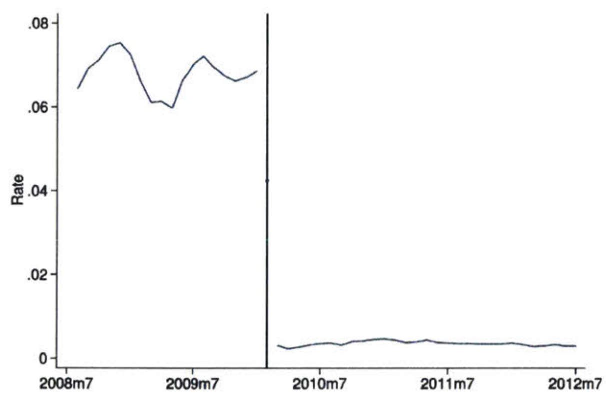
1.7 Figures

Figure 1: Direct Price Effects at CARD Act Implementation

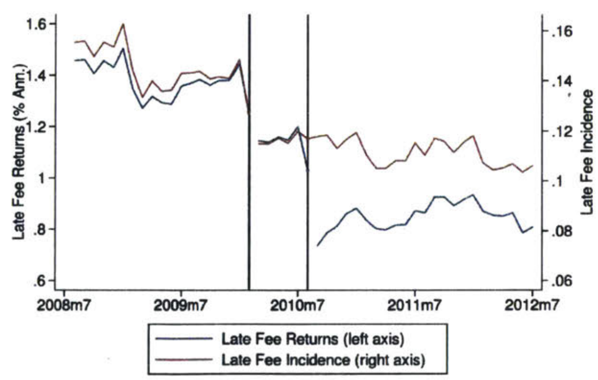
(a) Interest Rate Repricing



(b) Over-Limit Fees

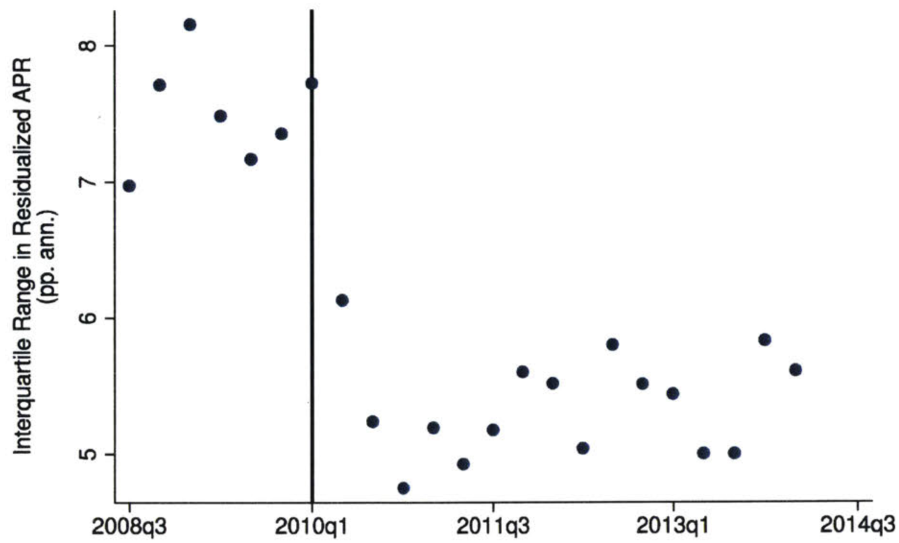


(c) Late Payment Fees



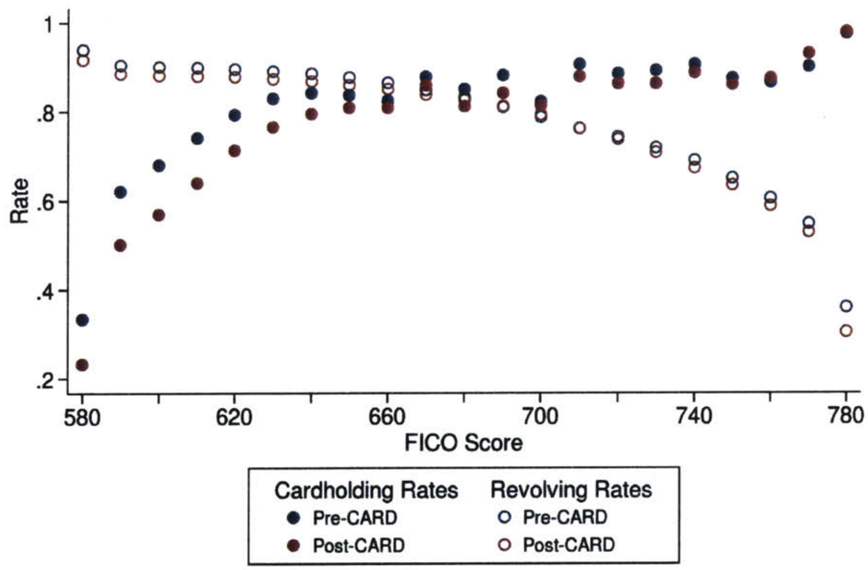
Notes: Panel (a) shows the incidence of interest rate increases on current borrowers over 1-month, 6-month, and 12-month horizons, excluding interest rate increases permitted by the CARD Act (i.e., increases coinciding with the expiration of a promotional rate, with changes in an index rate, or with delinquencies of 60 days or more). Dotted lines extrapolate from the most recent available datapoint when these horizons overlap with the implementation of the CARD Act's interest rate repricing restrictions in February 2010, which is marked by the vertical black line. Panel (b) shows the monthly incidence of over-limit fees on current borrowers, excluding any fees subsequently reversed. Th implementation date of the CARD Act's over-limit fee restrictions in February 2010 is marked by the vertical black line. Panel (c) shows annualized lender returns from late fees relative to total outstanding balances on borrowing accounts (left axis) and the average incidence of late fees across accounts (right axis). The vertical black lines show the CARD Act's implementation dates for restrictions on interest-rate increases and over-limit fees in February 2010 and for restrictions on late fee amounts in August 2010.

Figure 2: Interquartile Ranges in Credit Card APRs by Vintage



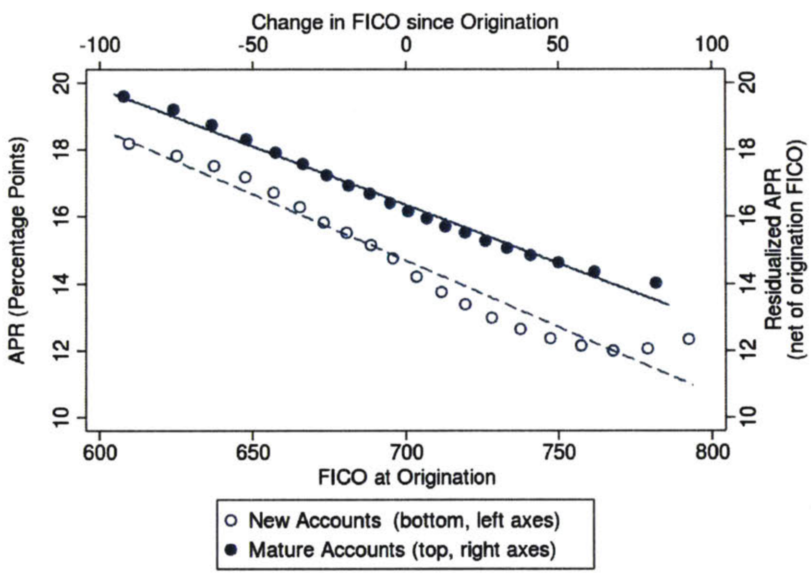
Notes: The figure shows the interquartile range (IQR) of annual percentage rates on borrowing accounts by origination cohort, after partialling out origination credit score and origination month. The date shown for each cohort is its age of maturity (18 months), by which point introductory promotional rates have typically expired. Credit score controls are 20-point bins, and the sample is restricted to include only accounts in the same credit score bin at the date observed as at origination. The vertical black line shows the date of implementation for the CARD Act's restrictions on interest rate increases, in February 2010.

Figure 3: Prevalence of Cardholding and Borrowing
Pre- and Post-CARD Act



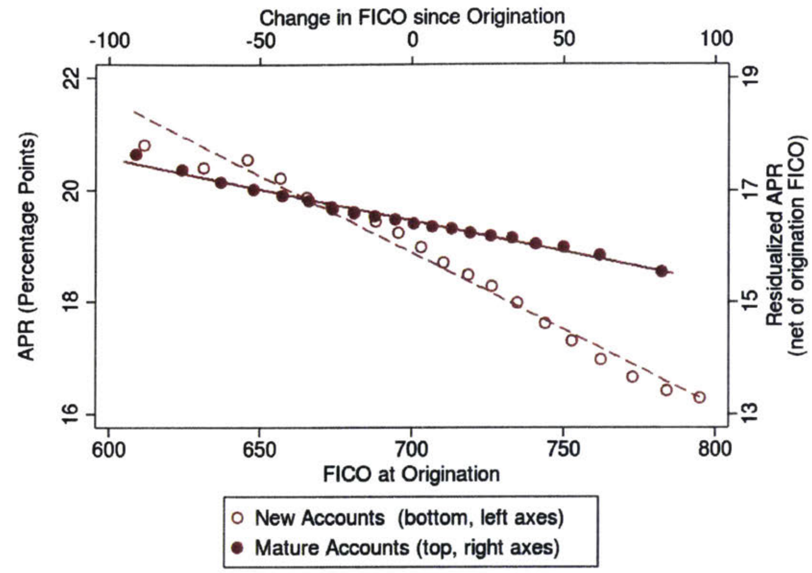
Notes: The figure shows the rate of credit card-holding among individuals in each credit score bin (CCP data) and the share of active credit card accounts used for borrowing (CCDB data), in pre- and post-CARD-Act periods (2008Q3 to 2009Q2, and 2011Q3 to 2014Q2, respectively). Borrowing is defined as not paying a balance in full for two successive billing cycles.

Figure 4: Pre-CARD-Act Price Gradients for Origination Risk and Emergent Risk



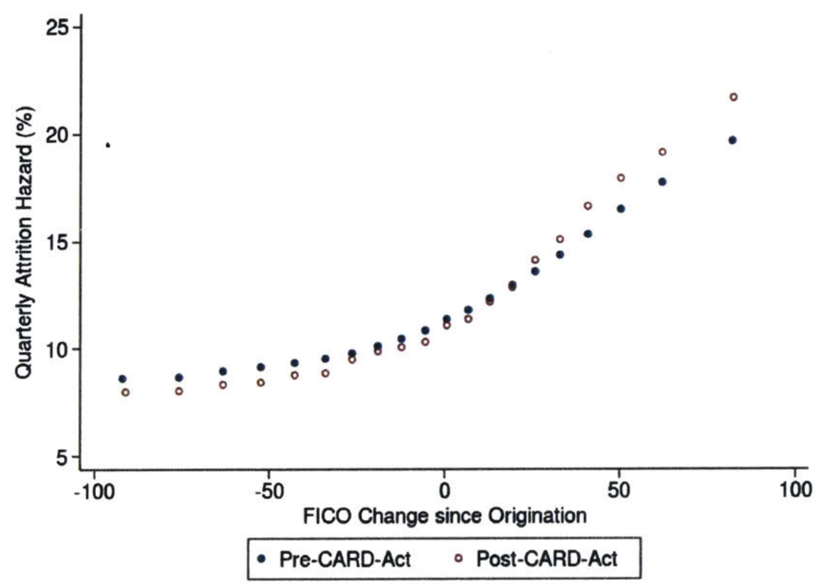
Notes: The figure shows two different gradients of risk in the pre-CARD-Act era (2008Q3 to 2009Q2) on two pairs of axes. On the left, bottom axes, the figure plots the average annual percentage rate (APR) on newly originated accounts across quantiles of the credit score distribution, together with a line of best fit. On the right, top axes, the figure plots the average current APR on mature accounts across quantiles of those accounts' change in credit score since origination, after partialling out origination credit score, together with a line of best fit. See equations 1.3.1 and 1.3.2 in the text.

Figure 5: Post-CARD-Act Price Gradients for Origination Risk and Emergent Risk



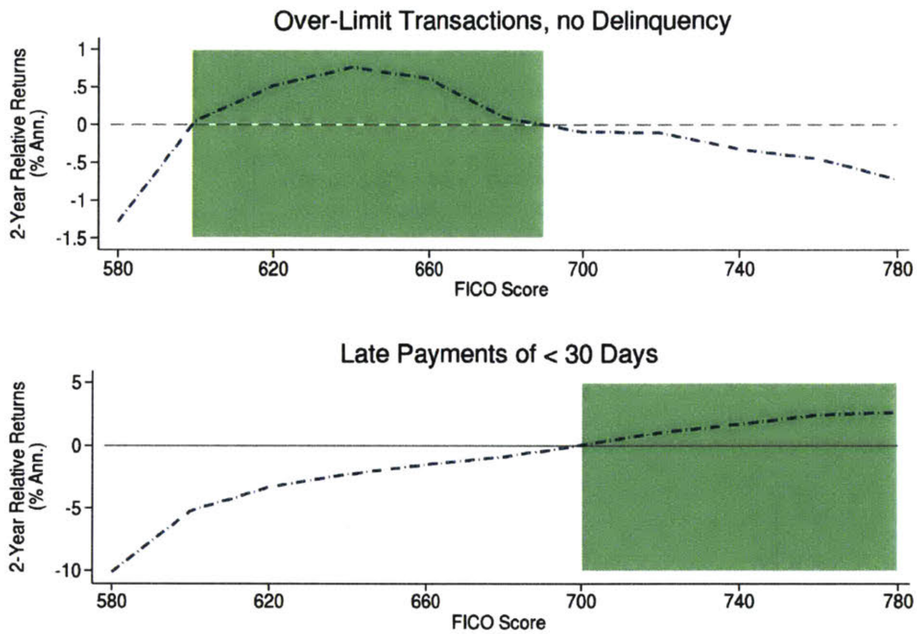
Notes: The figure presents the same price-risk gradients as in Figure 4 but in post-CARD-Act data (2011Q3 to 2014Q2). The two y-axes have the same axis scale, but the axis ranges are shifted to facilitate comparison of the two gradients. See notes to Figure 4 for further detail.

Figure 6: Adverse Retention in Response to Risk Mispricing



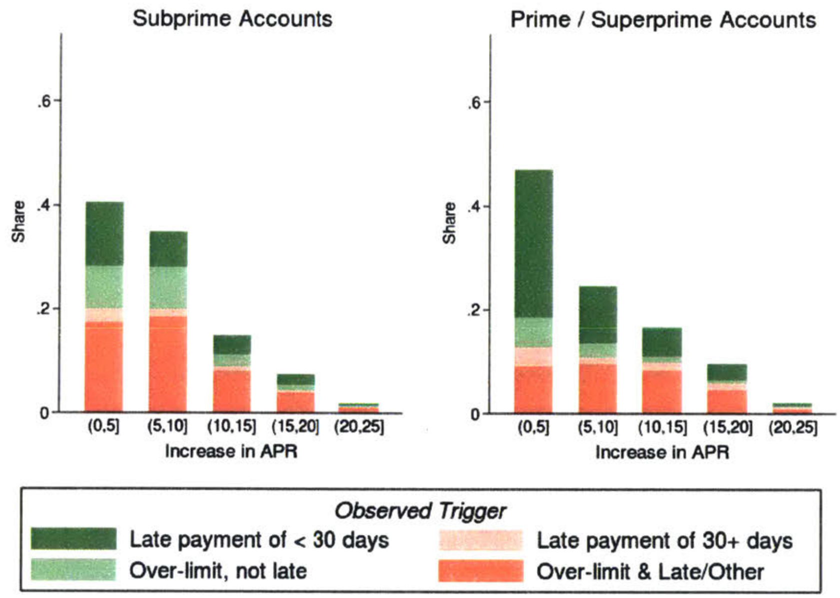
Notes: The figure plots quarterly attrition rates from borrowing (including both attrition through account closure and also attrition through paying off a credit card's balance) across quantiles of borrowing accounts' changes in FICO score since origination, separately in pre-CARD-Act data and post-CARD-Act data (2008Q3 to 2009Q2 and 2011Q3 to 2014Q2, respectively). See equation 1.3.3 in the text.

Figure 7: Pre-CARD Act Price Elasticity Signals



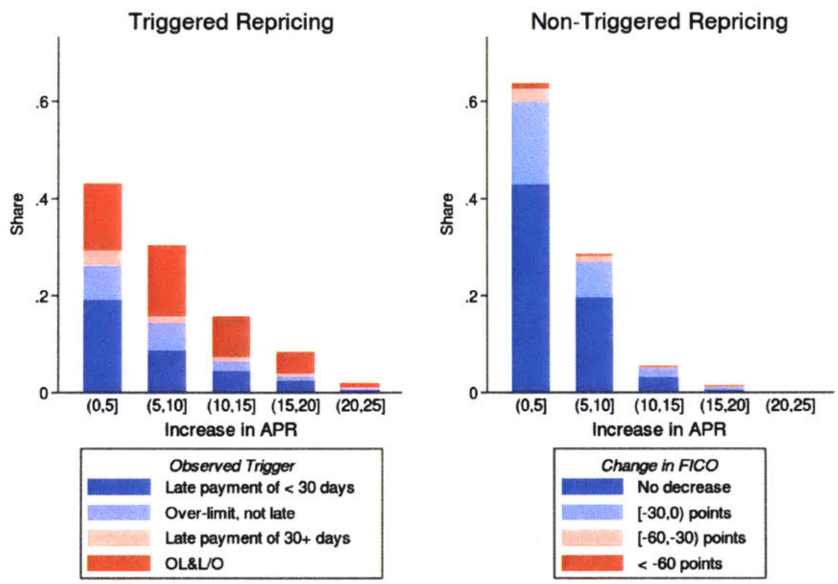
Notes: The figure highlights two commonly used triggers for interest rate increases that I identify as price elasticity signals (see equation 1.3.5 in the text): over-limit transactions not coinciding with delinquency, and late payments of less than thirty days. The plotted line shows the change in lenders' expected returns after observing the relevant signal on an account, relative to expected returns on accounts that send no particular signal (behavior "0" in equation 1.3.5), as a function of accounts' credit score. Green shading emphasizes the credit score segments where behaviors are identified as price elasticity signals.

Figure 8: Causes of Triggered Repricing



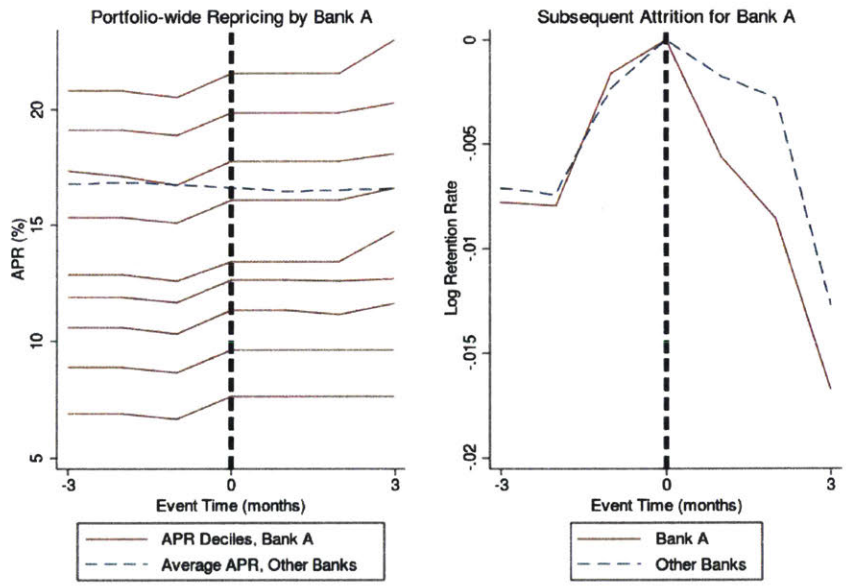
Notes: The figure shows a decomposition of interest rate increases in pre-CARD-Act data (2008Q3 - 2009Q2) across various standard triggers that may coincide with an interest rate increase. This decomposition is shown separately for subprime and prime accounts (left and right panels) and separately by the size of the APR increase (grouped across the x-axes). Color shading emphasizes which triggers are behaviors that predict higher vs. lower lender returns, with the darkest green showing the highest future returns and the darkest red showing the most negative future returns on average across accounts. See Figure 8 and Table 3 for evidence on which signals predict higher and lower future returns.

Figure 9: Causes of Discretionary Repricing



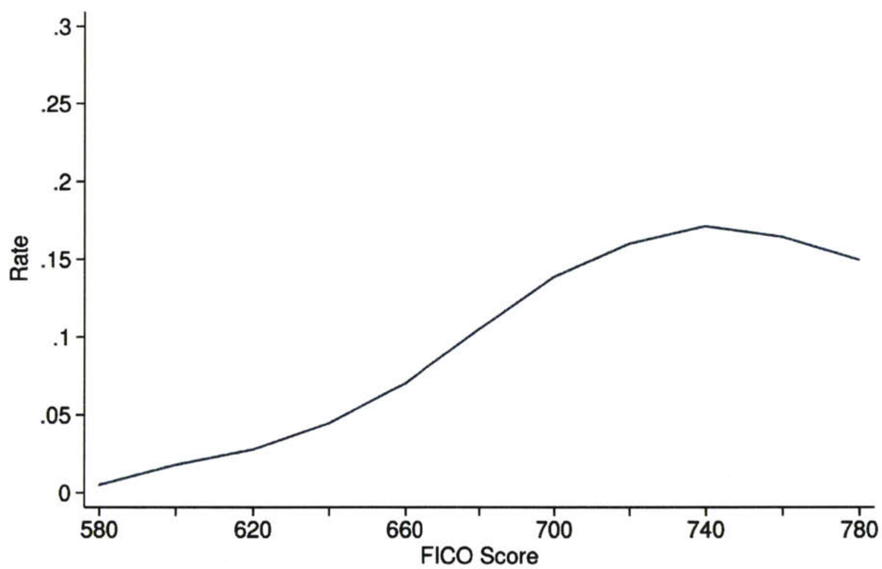
Notes: The figure presents evidence on how credit scores are evolving at the time of non-triggered interest rate increases. Whereas some interest rate increases are "triggered" by a behavior typically delineated in credit card contracts as a justification for raising interest rates, such as over-limit transactions or late payments, lenders can also impose "non-triggered" interest rate increases through contract provisions that allow interest rates to be increased "at any time, for any reason" in the absence of any particular trigger. These interest rate increases are shown separately by size, in percentage points, across the x-axis. The shading illustrates the one-quarter change in credit score preceding the interest rate increase, with darker green indicating declining risk for lenders and darker red shades indicating rising risk.

Figure 10: Example of Repricing Quasi-Experiment



Notes: The figure plots an example of a repricing quasi-experiment (left panel) and subsequent attrition from borrowing (right panel) from the pre-CARD-Act data. In the left panel, the solid red lines plot deciles of the distribution of annual percentage rates (APRs) on mature, borrowing accounts for one lender in the data, denoted Bank A. All deciles of this distribution rise by 100 basis points in the month labeled event time 0, emphasizing how this repricing campaign affects (nearly) all accounts in the portfolio.The dotted blue line shows the average APR for all other lenders' mature, borrowing accounts. In the right panel, log monthly attrition rates from borrowing are shown relative to their value in event time 0 for Bank A and for all other banks. Here attrition includes attrition through paying off a balance, through refinancing with another lender, or through closing a card. See equation 1.4.1 in the text.

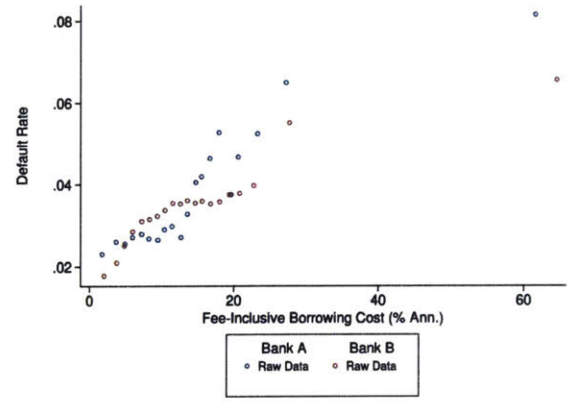
Figure 11: Prevalence of Balance Transfer Activity by FICO Score
Pre-CARD Act



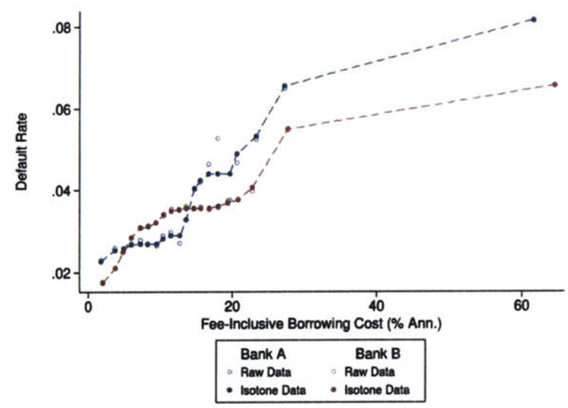
Notes: The figure shows the rate of balance transfers by credit score, calculated as the ratio of incoming balance transfers at promotional rates or on newly originated accounts, to the number of mature borrowing accounts without promotional rates in effect. Borrowing is defined as not paying a balance in full for two subsequent billing cycles.

Figure 12: Recovering Private-Information Types from Equilibrium Pricing

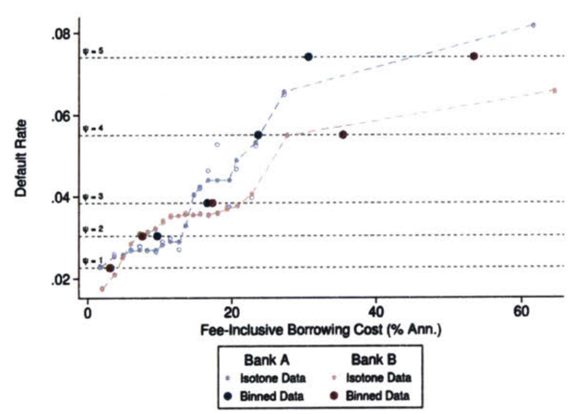
(a) Step 1: Inverse Pricing Functions for Ex-Post Default



(b) Step 2: Isotonic Inverse Pricing Functions

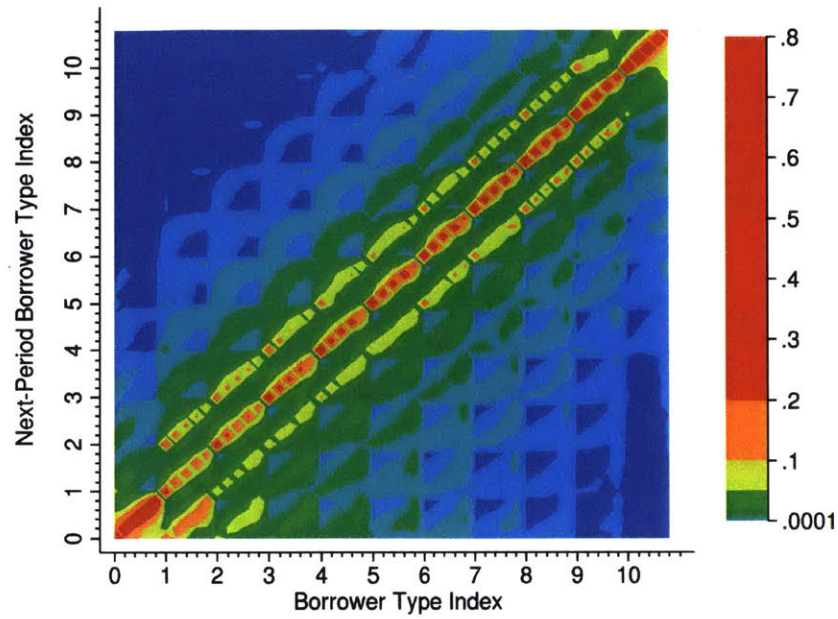


(c) Step 3: Discretizing Private Types ψ from Pricing Functions



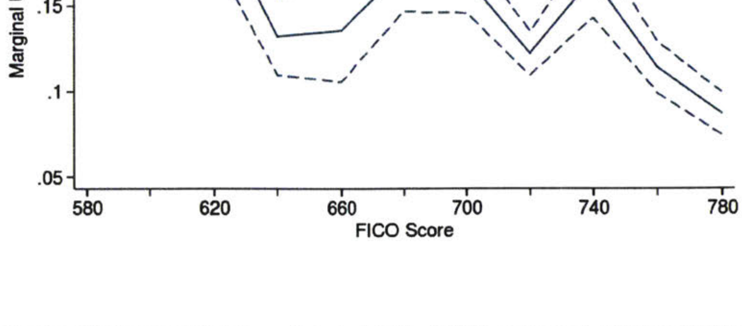
Notes: The figure illustrates the process of recovering private-information types from observed equilibrium pricing in pre-CARD-Act data, as described in equations 1.4.16 and 1.4.17 in the text. This example is taken from the market segment defined by the credit score range 720-739. Panel (a) shows raw data on observed default rates at quantiles of price levels on two different banks, labeled Bank A and Bank B. Default is defined as delinquencies of 90+ days at any time over the subsequent 2 years. Panel (b) shows isotonic regression estimates of the relationship between default and equilibrium pricing, together with the raw data from panel (a) for sake of comparison. Panel (c) then shows how borrowers at different quantiles of the *population* distribution of default rates within this credit score range are grouped into discrete private-information types ψ that share a common default rate, but face different prices depending on their choice of lender.

Figure 13: Transition Rates Among Public and Private Types



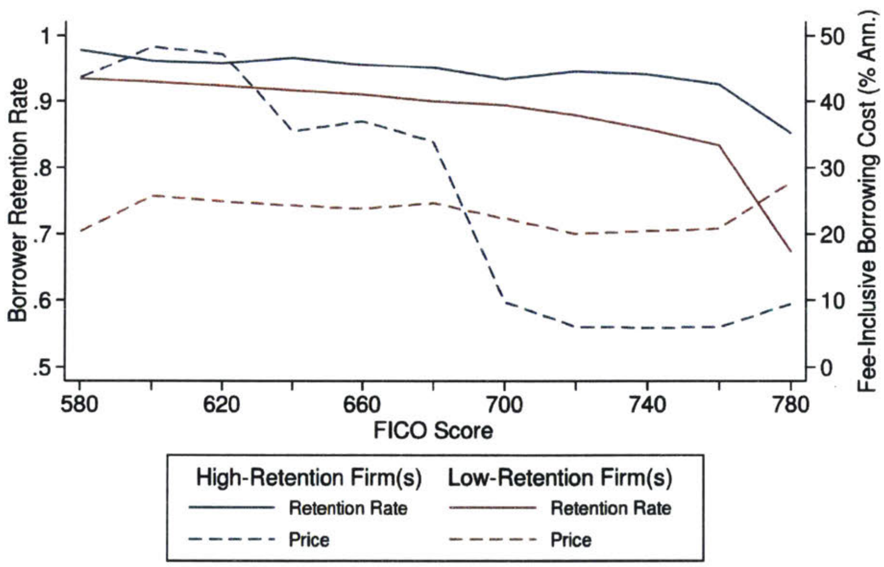
Notes: The figure displays a contour plot of period-to-period transition probabilities among consumer types. These probabilities are estimated quarterly among borrowers observed for two subsequent quarters, using the joint of public and private types recovered through the process illustrated in Figure 12. The integer values of the index correspond to the public dimension of types, in order of increasing credit score; for example the range [0,1) corresponds to the 580-599 FICO score group, the range [1,2) corresponds to the 600-619 FICO score group, and so-on. Within integers, the sub-ticks correspond to the five private-information types recovered at each FICO score level, in order of increasing risk.

Figure 14: Heterogeneity in Price Coefficients



Notes: The figure displays estimates of heterogeneous price coefficients (marginal utilities of income γ_x) across FICO score, estimated via equation 1.4.23. Dotted lines display 95% confidence bands.

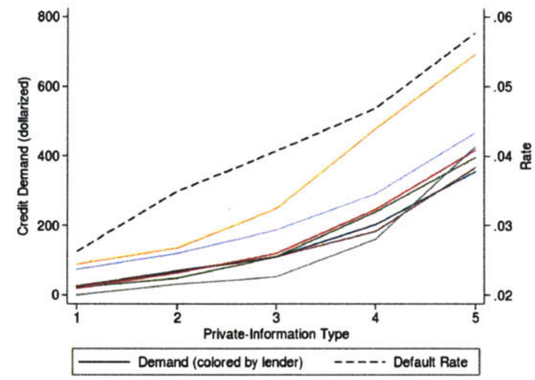
Figure 15: Identification of Demand Parameters



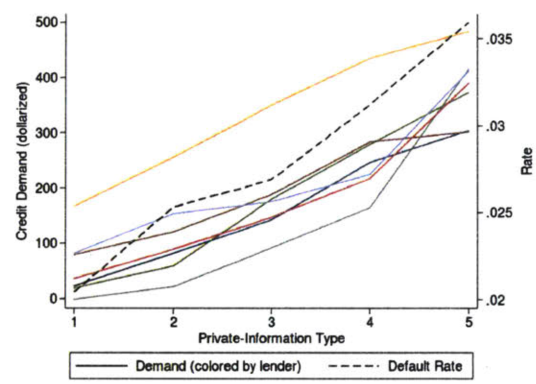
Notes: The figure shows borrower retention rates for the highest-retention and lowest-retention credit card issuers at each 20-point credit score group. Hence the retention lines are upper and lower envelopes across the market, not the set of retention rates for any single firm. For each firm included in these envelopes, corresponding prices are shown in the dotted lines. Results are shown for the median private-information type in each FICO score group.

Figure 16: Borrowing Demand and Default Rates by Consumer Type

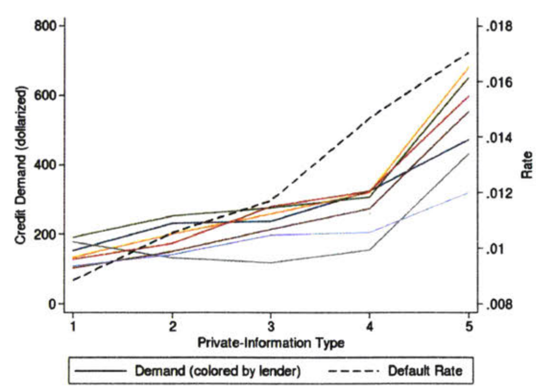
(a) FICO 620-639 Consumers



(b) FICO 660-679 Consumers

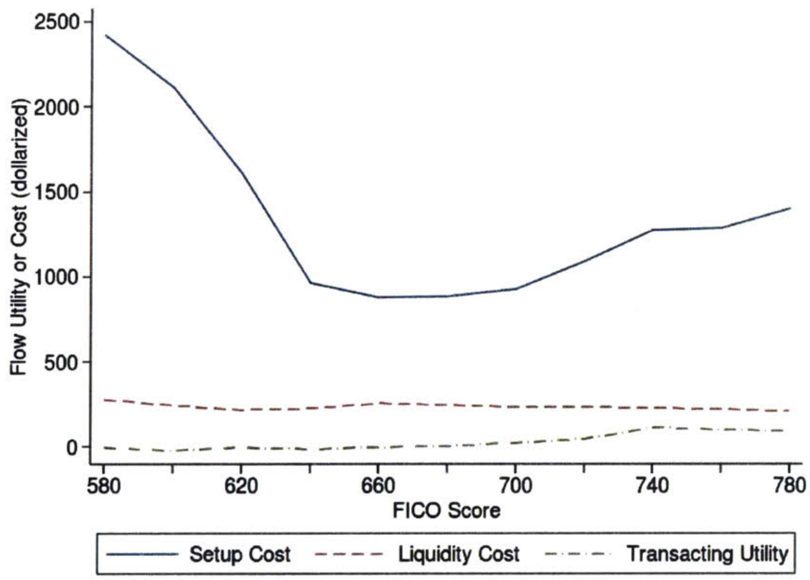


(c) FICO 720-739 Consumers



Notes: The figure shows estimates of consumer types' flow utilities from borrowing, together with these types' default rates. Consumer types $\theta = (x, \psi)$ are shown separately by private-information type ψ (across the x-axes) and by public type x, i.e., credit score group (three selected groups are shown separately in the three panels). Flow utilities (the parameter $d_{\theta j}$) are plotted separately by lender j in solid lines. These flow utilities are dollarized using each type's marginal utility of income (the price coefficient γ_θ) and using average borrowed balances for that credit score group. Default rates measure the probability of being 90+ days delinquent at a quarterly horizon.

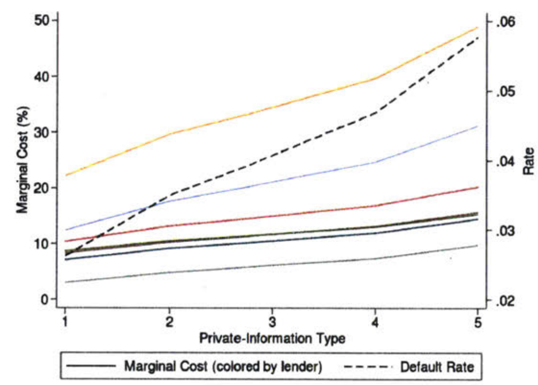
Figure 17: Setup Costs, Exit Costs and Transacting Demand



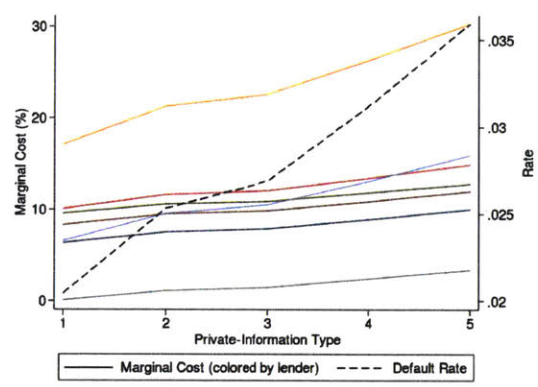
Notes: The figure shows estimates of flow utilities from transacting, liquidity costs to paying off a balance, and set-up costs for opening a new account, separately by 20-point bin of credit score. Parameters that are estimated separately by lender and by private-information type are averaged within credit-score group, using pre-CARD-Act market share weighting by lender and the probability distribution μ_θ across private types. Parameters are dollarized using a population-average marginal utility of income, estimated in column (1) of Table 10, and using average borrowed balances for each credit score group.

Figure 18: Marginal Costs and Default Rates by Consumer Type

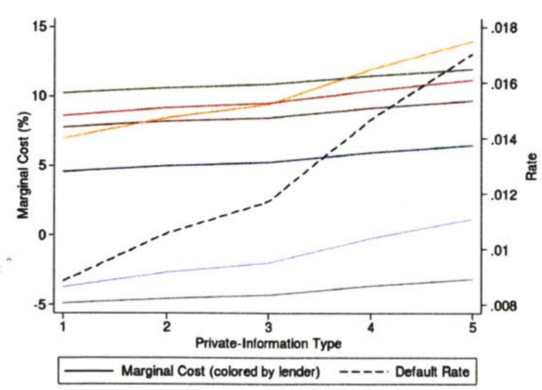
(a) FICO 620-639 Consumers



(b) FICO 660-679 Consumers

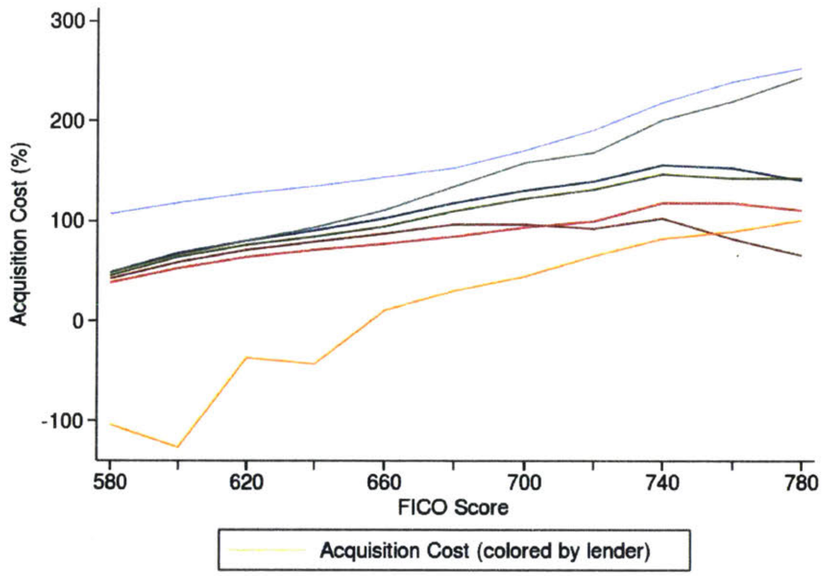


(c) FICO 720-739 Consumers



Notes: The figure shows estimates of firms' marginal cost of lending to each consumer type in three selected credit score groups, together with these types' default rates. Marginal costs are expressed as an annualized percentage of average borrowed balances, and default rates measure the probability of being 90+ days delinquent at a quarterly horizon.

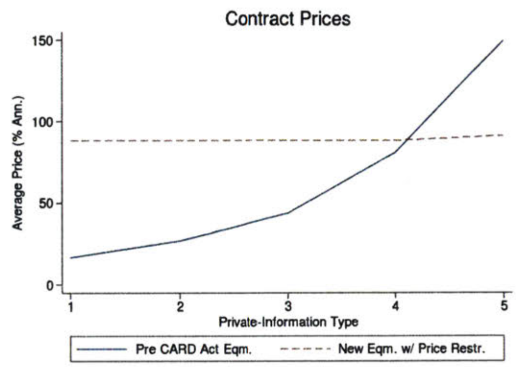
Figure 19: Consumer Acquisition Costs



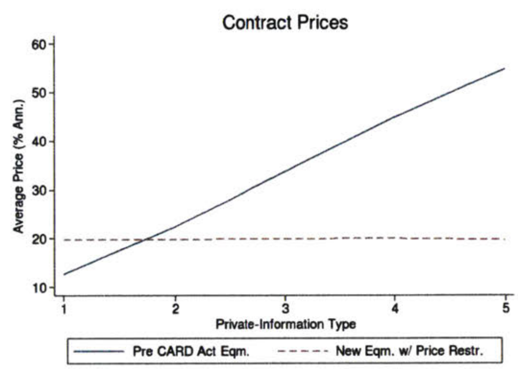
Notes: The figure shows estimates of firms' per-account acquisition cost for consumers in each 20-point credit score group, expressed as an annualized percentage of average borrowed balances in that credit score group. Occasional estimates of negative acquisition costs may reflect fee revenue at the time of account origination, such as application fees, as discussed in footnote 60.

Figure 20: Equilibrium Changes in Contract Pricing with CARD-Act Pricing Restrictions

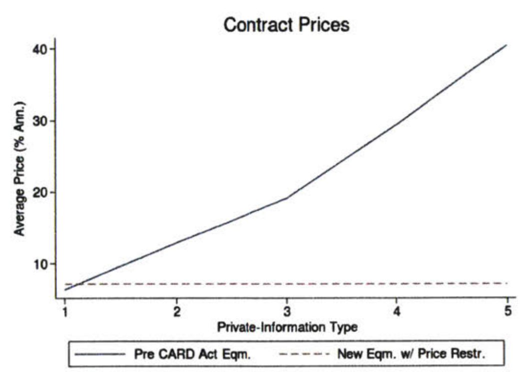
(a) FICO 580-599 Consumers



(b) FICO 680-699 Consumers



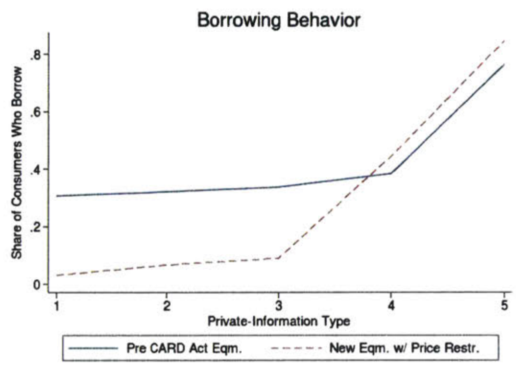
(c) FICO 780+ Consumers



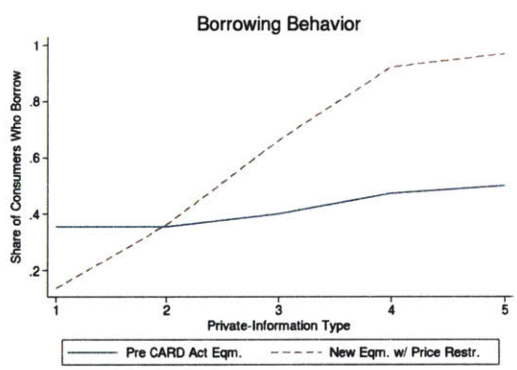
Notes: The figure shows observed average contract prices for each consumer type in three selected credit score groups in the pre-CARD-Act equilibrium, together with model results for these types' equilibrium contract prices after imposing the CARD Act price restrictions. The prices shown are annualized, account-level averages at a quarterly frequency inclusive of both interest charges and fees, normalized by the amount borrowed. This price measure is described in Section 1.2.2 of the text.

Figure 21: Equilibrium Entry/Exit from Borrowing with CARD-Act Pricing Restrictions

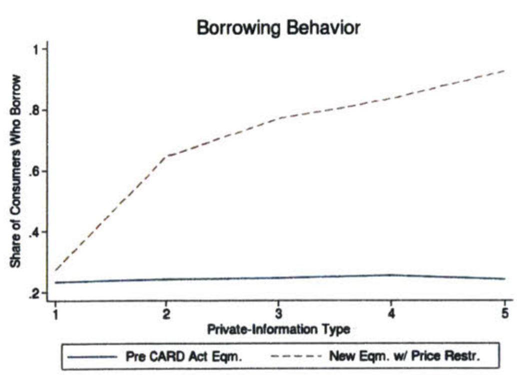
(a) FICO 580-599 Consumers



(b) FICO 680-699 Consumers

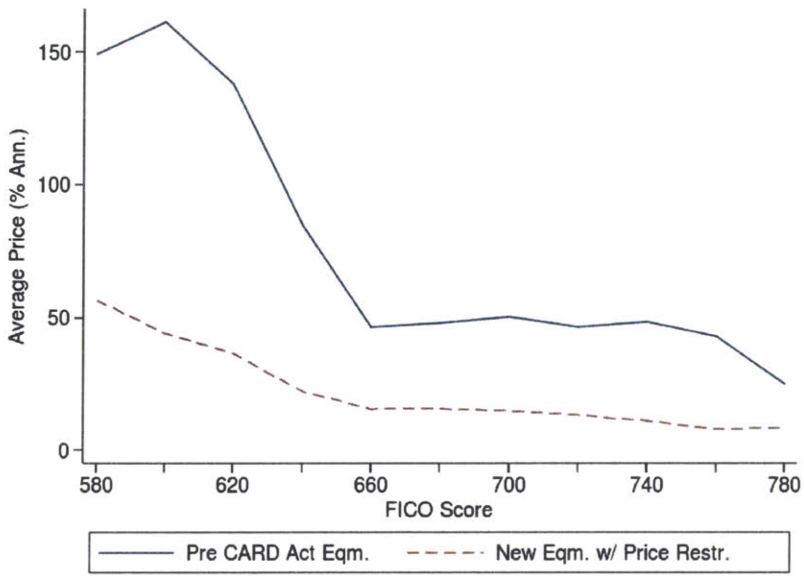


(c) FICO 780+ Consumers



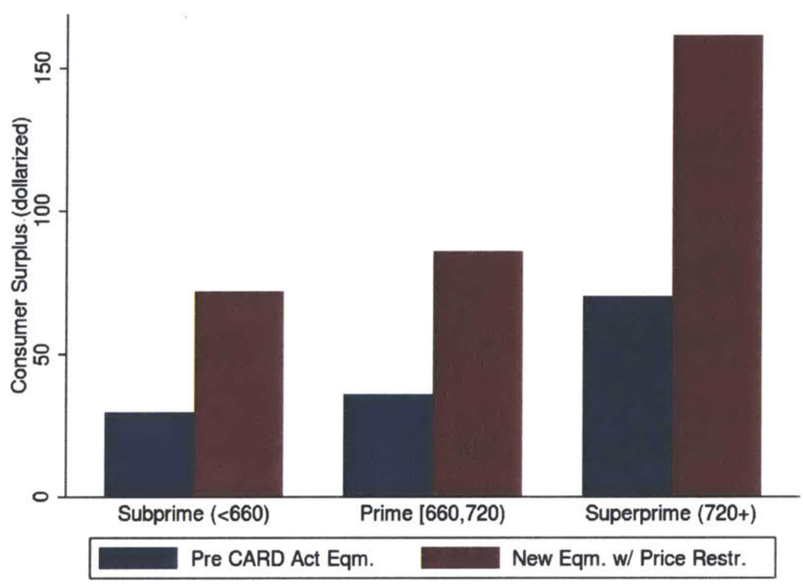
Notes: The figure shows the share of consumers who use a credit card for borrowing among various consumer types. Shares range from 0 to 1. Shares for the new equilibrium with price restrictions reflect the effect of CARD Act price restrictions when implemented in the model, holding constant other parameter estimates from the *pre*-CARD-Act equilibrium. Private-information types are shown across the x-axis of each panel and the three panels show three selected public information (credit-score) groups.

Figure 22: Changes in Transacted Contract Prices



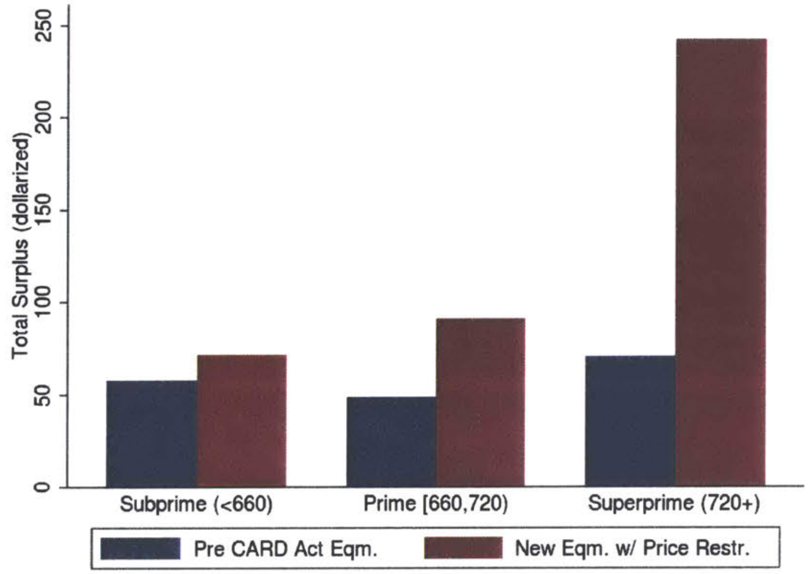
Notes: The figure shows changes in *transacted* long-run contract prices across FICO scores on the x-axis. Consumers who exit the market are therefore not counted in the new equilibrium with price restrictions. Prices shown are weighted averages across private types and across lenders.

Figure 23: Changes in Consumer Surplus



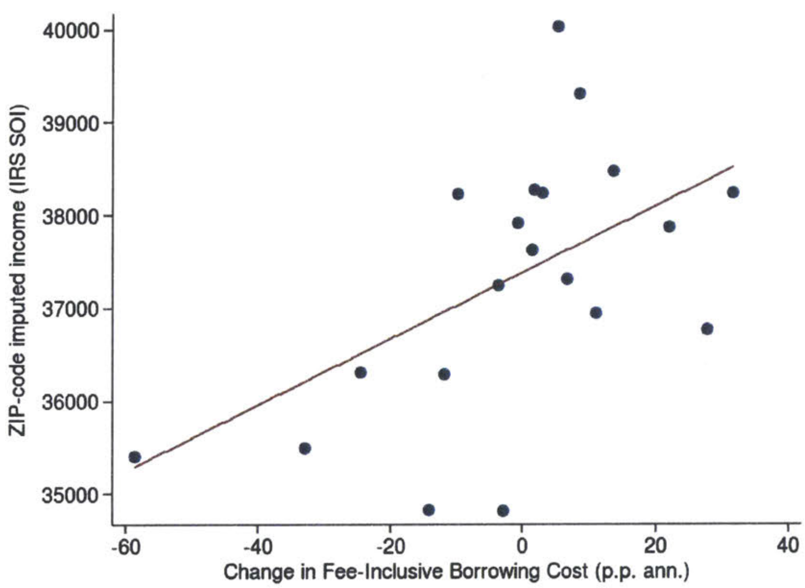
Notes: The figure shows estimated per-person consumer surplus (including both borrowers and non-borrowers) in the pre-CARD-Act equilibrium and also in the new equilibrium found in the model after imposing the CARD Act price restrictions. Surplus is dollarized using each type's marginal utility of income (the price coefficient γ_θ) and using average borrowed balances for a type's credit score group. Per-person surplus numbers are averaged to coarser credit-score groups using the type probability distribution μ_θ.

Figure 24: Changes in Total Surplus



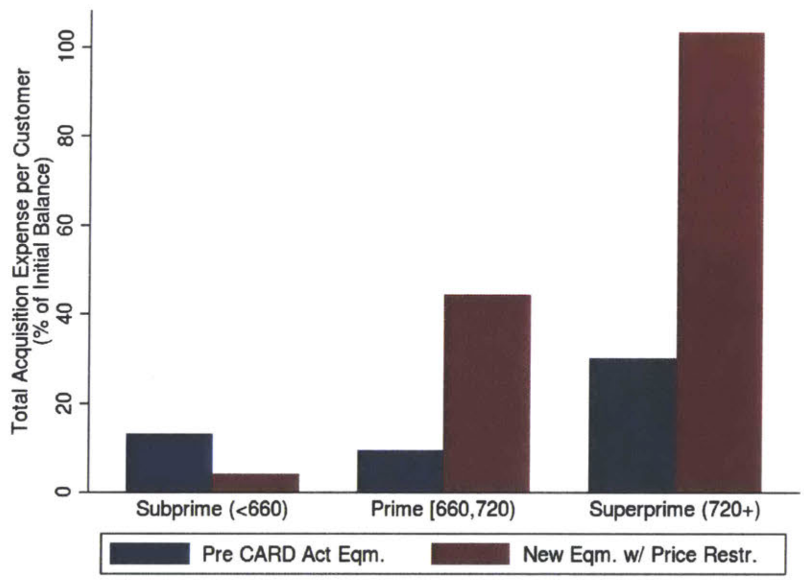
Notes: The figure shows estimated per-person total surplus (including both borrowers and non-borrowers' consumer surplus as well as firm profits) in the pre-CARD-Act equilibrium and also in the new equilibrium found in the model after imposing the CARD Act price restrictions. Consumer surplus is dollarized using each type's marginal utility of income (the price coefficient γ_θ) and using average borrowed balances for a type's credit score group. Per-person surplus numbers are averaged to coarser credit-score groups using the type probability distribution μ_θ.

Figure 25: Incidence of CARD Act Price Changes across Income



Notes: The figure plots annual incomes imputed at the ZIP-code level using IRS Statistics of Income data against the predicted change in the contract price of borrowing. This price change is from the pre-CARD-Act equilibrium to the new equilibrium found in the model after imposing the CARD Act price restrictions. See Figure 20 for further discussion of this price measure.

Figure 26: Changes in Total Outlay for New Account Acquisition



The figure shows firms' total per-account outlay for new account acquisition in the pre-CARD-Act equilibrium and also in the new equilibrium found in the model after imposing the CARD Act price restrictions. Outlay is defined as account acquisition costs (a model parameter) minus introductory prices (p^0) offered on new accounts (a variable chosen by firms in the model). Outlay is averaged across firms using equilibrium market share, so changes in outlay reflect both changing market shares across firms with different acquisition costs and also changes in introductory prices.

1.8 Tables

Table 1: Observed Price Changes from Pre- to Post-CARD Act

Panel A: Changes in Interest Charges (% Ann.)					
FICO	P10	P25	Mean	P75	P90
580 - 599	2.46	-0.03	-2.52	-4.62	-2.83
600 - 619	2.16	0.89	-1.54	-4.32	-2.28
620 - 639	2.66	1.70	-0.75	-3.66	-1.91
640 - 659	3.03	2.49	0.12	-2.69	-2.11
660 - 679	3.01	2.95	0.88	-1.06	-2.15
680 - 699	2.67	3.15	1.38	0.05	-1.50
700 - 719	1.44	3.22	1.59	0.99	-0.49
720 - 739	0.44	3.18	1.56	1.33	0.44
740 - 759	-0.99	2.68	1.45	1.44	0.28
760 - 779	-2.55	1.91	1.07	1.44	-0.04
780 - 799	-2.54	-0.02	0.82	1.41	1.07

Panel B: Changes in Fee-Inclusive Charges (% Ann.)					
FICO	P10	P25	Mean	P75	P90
580 - 599	3.14	-0.06	-6.10	-7.39	-10.60
600 - 619	2.27	0.83	-3.43	-5.61	-6.31
620 - 639	2.76	1.64	-2.22	-4.87	-4.71
640 - 659	3.21	2.50	-0.90	-3.41	-3.49
660 - 679	3.14	3.04	0.20	-1.70	-2.86
680 - 699	2.78	3.25	0.90	-0.23	-2.37
700 - 719	1.50	3.23	1.25	0.36	-1.32
720 - 739	0.63	3.27	1.31	1.20	-0.35
740 - 759	-0.88	2.73	1.25	1.24	0.06
760 - 779	-2.35	1.97	0.88	1.30	-0.23
780 - 799	-2.74	0.10	0.68	1.42	0.76

Notes: The table shows percentage point changes in two price measures across the FICO score distrubtion from before the CARD Act to after (2008Q3 to 2009Q2 and 2011Q3 to 2014Q2 respectively). The first price measure, shown in Panel A, is an account's annualized percentage interest charges, defined as annualized monthly interest charges divided by borrowed balances. The second price measure, shown in Panel B, adds fee charges to the numerator of the first price measure.

Table 2: Pre-CARD Act Price Distribution on Mature Accounts

FICO Group	Cum. Months of Borrowing	Share within FICO Group	Interest Charges (% Ann.) P25	Interest Charges (% Ann.) Mean	Interest Charges (% Ann.) P75	Fee-Inclusive Charges (% Ann.) P25	Fee-Inclusive Charges (% Ann.) Mean	Fee-Inclusive Charges (% Ann.) P75
620 - 639	0	1.81%	.	.	.	.	.	.
	1-2	2.13%	10.23	17.90	25.73	11.03	25.75	29.03
	3-5	4.10%	8.31	16.14	24.91	8.86	21.35	27.98
	6-11	20.79%	9.50	16.73	25.12	9.98	21.19	27.92
	12	71.16%	11.62	18.29	26.00	12.18	21.15	27.99
680 - 699	0	5.33%	.	.	.	.	.	.
	1-2	4.23%	4.78	12.89	19.34	4.94	16.14	21.21
	3-5	6.33%	2.87	11.28	17.79	2.96	13.34	19.21
	6-11	23.33%	4.33	12.02	18.13	4.61	13.58	19.34
	12	60.77%	8.35	14.36	19.46	8.57	15.36	20.38
740 - 759	0	15.86%	.	.	.	.	.	.
	1-2	8.01%	2.11	9.56	14.61	2.16	11.52	15.65
	3-5	9.27%	1.23	8.56	13.41	1.68	9.84	14.29
	6-11	24.03%	3.10	9.32	13.66	3.17	10.24	14.36
	12	42.83%	6.13	11.07	14.50	6.20	11.59	14.98
800 - 819	0	44.68%	.	.	.	.	.	.
	1-2	14.22%	0.00	7.59	12.71	0.00	9.82	13.41
	3-5	10.97%	0.27	8.26	12.86	0.47	9.66	13.40
	6-11	16.24%	3.79	8.82	12.72	3.90	9.69	13.15
	12	13.89%	5.46	9.71	12.72	5.51	10.15	13.01

Notes: The table shows price quartiles and means at selected FICO score groups and across accounts with different cumulative months of borrowing over the course of the year in the pre-CARD-Act period (2008Q3 to 2009Q2). This sample includes only mature accounts (observed at 18 or more months since origination). The two price measures shown are, first, an account's annualized percentage interest charges, defined as annualized monthly interest charges divided by borrowed balances, and second, a price measure that adds fees charged to the numerator of the first price

Table 3: Lender Returns after Borrower Risk Signals

FICO Group	Baseline (% Ann.)	Over-Limit and Delinquent	Late by 90+ Days	Late by 60-89 Days	Late by 30 - 59 Days	FICO Drop of 60+ Points	FICO Drop of 30-59 Points
580 - 599	0.89	-36.65	-40.77	-34.25	-27.34	-12.65	-6.26
600 - 619	2.99	-25.36	-41.97	-35.77	-25.69	-9.55	-6.17
620 - 639	3.30	-21.90	-43.67	-37.73	-24.20	-7.96	-5.82
640 - 659	3.69	-19.95	-45.26	-38.92	-23.20	-6.16	-5.30
660 - 679	4.35	-19.04	-47.23	-40.17	-23.39	-5.22	-4.59
680 - 699	5.09	-18.70	-48.28	-42.01	-23.00	-4.21	-3.66
700 - 719	6.02	-17.94	-49.06	-42.51	-22.06	-3.87	-2.89
720 - 739	6.99	-16.88	-51.65	-44.83	-19.53	-3.87	-2.37
740 - 759	7.92	-16.07	-53.05	-45.12	-16.27	-3.18	-2.10
760 - 779	8.82	-15.78	-52.16	-43.26	-11.98	-2.79	-1.81
780 - 799	9.24	-17.47	-50.81	-42.11	-8.19	-2.31	-1.43

Notes: The table shows baseline annual percent returns on accounts in each FICO score group (column 1) in the pre-CARD-Act period (2008Q3 to 2009Q2), and differences from these baseline returns that are predicted in the pre-CARD-act period by the risk signals in each column. Returns are calculated by dividing finance revenue less default cost by borrowed balances.

Table 4: Fee Revenue Shares by Signal Type

FICO Group	Late by <30 Days	Over-Limit not Delinquent	Over-Limit and Delinquent	Late by 30+ Days	FICO Drop of 30+ Points
580 - 599	11.49	9.85	72.42	6.15	0.10
600 - 619	27.11	18.20	47.57	6.78	0.35
620 - 639	32.15	20.33	41.04	6.01	0.47
640 - 659	38.71	20.63	34.25	5.76	0.64
660 - 679	47.20	19.00	27.18	5.70	0.92
680 - 699	56.19	16.38	20.38	5.88	1.18
700 - 719	64.78	13.51	13.98	6.25	1.47
720 - 739	71.26	11.02	9.60	6.59	1.53
740 - 759	77.00	8.40	6.34	7.06	1.19
760 - 779	82.71	5.13	3.62	7.80	0.74
780 - 799	85.03	2.63	2.11	9.97	0.26

Notes: The table shows the share of fee revenue in each FICO score group generated by the fee categories in each column in the pre-CARD-Act period (2008Q3 to 2009Q2). Late fees are shown separately by delinquency status and by whether they coincided with an over-limit fee.

Table 5: Persistence in Consumer Revolving Behavior

FICO Group	Recent Borrowers		All Accounts	
	Transactor	Borrower	Transactor	Borrower
580	0.16	0.85	0.05	0.84
600	0.14	0.89	0.05	0.80
620	0.13	0.89	0.05	0.79
640	0.12	0.89	0.04	0.81
660	0.12	0.89	0.03	0.77
680	0.11	0.88	0.03	0.79
700	0.10	0.88	0.02	0.75
720	0.09	0.87	0.02	0.72
740	0.08	0.87	0.02	0.70
760	0.08	0.86	0.01	0.65
780	0.08	0.82	0.01	0.49

Notes: The table shows probabilities of next-quarter borrowing in the pre-CARD-Act period (2008Q3-2009Q2) for consumers who are either transactors or borrowers in the current period. The first two columns restrict the sample to consumers who have borrowed at least once in the past 6 months (recent borrowers), and the latter two columns extend these results to the full sample of active credit-card holders.

Table 6: Pre-CARD Act Price Distribution on New Accounts

FICO Group	Cum. Months of Borrowing	Share within FICO Group	Interest Charges (% Ann.)			Fee-Inclusive Charges (% Ann.)		
			P25	Mean	P75	P25	Mean	P75
620 - 639	0	2.03%	.	.	.	.	.	.
	1-2	2.49%	0.00	11.45	18.86	0.00	20.36	24.58
	3-5	4.90%	0.00	10.99	17.85	0.00	17.40	21.16
	6-11	38.15%	0.84	11.78	18.24	1.94	17.52	22.01
	12	52.43%	4.77	12.41	18.15	5.52	16.76	21.83
680 - 699	0	6.47%	.	.	.	.	.	.
	1-2	5.44%	0.00	6.90	12.62	0.00	10.17	14.56
	3-5	8.27%	0.00	6.58	12.35	0.00	8.87	13.88
	6-11	38.84%	0.00	6.81	12.44	0.00	8.64	13.62
	12	40.98%	0.00	8.16	12.88	0.00	9.57	13.91
740 - 759	0	15.00%	.	.	.	.	.	.
	1-2	8.83%	0.00	3.94	8.14	0.00	5.38	8.74
	3-5	11.36%	0.00	3.49	5.93	0.00	4.48	6.82
	6-11	36.24%	0.00	3.46	6.34	0.00	4.25	7.10
	12	28.58%	0.00	5.36	9.47	0.00	6.03	9.86
800 - 819	0	28.17%	.	.	.	.	.	.
	1-2	14.11%	0.00	3.37	5.08	0.00	4.97	6.48
	3-5	14.66%	0.00	2.74	0.61	0.00	3.67	2.12
	6-11	29.25%	0.00	2.38	1.54	0.00	2.89	2.87
	12	13.81%	0.00	3.62	7.17	0.00	4.12	7.56

Notes: The table shows price quartiles and means at selected FICO score groups and across accounts with different cumulative months of borrowing over the course of the year in the pre-CARD-Act period (2008Q3 to 2009Q2). This sample includes only young accounts (observed at 12 or fewer months since origination). The two price measures shown are, first, an account's annualized percentage interest charges, defined as annualized monthly interest charges divided by borrowed balances, and second, a price measure that adds fees charged to the numerator of the first price measure. Borrowing is defined as not repaying a balance in full at the end of a given month.

Table 7: Default Rates by Private-Information Type
(relative to lowest quintile)

	(1)	(2)	(3)
Dependent Variable		One-Year Default Rate	
Sample	All Accounts	Subprime	Prime
Estimator	OLS	OLS	OLS
2nd Quintile	0.0317***	0.0902***	0.00176***
	(0.0000460)	(0.000116)	(0.0000310)
3rd Quintile	0.0585***	0.147***	0.00502***
	(0.0000503)	(0.000118)	(0.0000355)
4th Quintile	0.0780***	0.191***	0.0129***
	(0.0000535)	(0.000131)	(0.0000367)
5th Quintile	0.0904***	0.198***	0.0257***
	(0.0000627)	(0.000150)	(0.0000437)
Quarter FEs	YES	YES	YES
Bank x FICO FEs	YES	YES	YES
Observations	243734158	88264172	155469986

Notes: The table shows regression estimates for a model using private information types as well as public types (FICO scores) to predict 1-year default. Private information types are presented as quintiles of the distribution of lender private information; estimates are relative to the lowest quintile of the private information distribution.

Table 8: Average Default Rates by Public and Private-Information Type

FICO Group	One-Year Default Rate by Quintile of Private-Information Type (%) 1st	2nd	3rd	4th	5th
580 - 599	14.92	31.14	39.14	45.75	45.73
600 - 619	5.93	9.37	13.75	16.78	20.47
620 - 639	5.02	7.12	10.23	12.35	15.47
640 - 659	4.18	5.25	7.17	9.20	11.54
660 - 679	3.34	4.08	5.13	6.80	8.75
680 - 699	2.66	3.08	3.41	4.58	6.72
700 - 719	1.80	1.97	2.21	3.40	4.76
720 - 739	1.05	1.29	1.59	2.18	3.27
740 - 759	0.64	0.76	0.99	1.40	2.45
760 - 779	0.42	0.48	0.64	0.90	1.77
780 - 799	0.29	0.30	0.43	0.58	1.22

Notes: The table shows one-year default rates by private information types (quintiles of the private information distribution) in the pre-CARD-Act period (2008Q3 to 2009Q2), for the FICO score group in each row. Default is defined as any instance of delinquency of over 90 days. Private information types are constructed to be weakly increasing in default risk, but the relative predictivenss of private vs. public information (FICO scores) remains flexible.

Table 9: Demand Model: Consumers' One-Period Payoffs by State

Current Period:	**Same Bank j**		**New Bank j'**		**No Credit Card**
Prior Period:	**Borrower**	**Non-Borrower**	**Borrower**	**Non-Borrower**	**with Any Bank**
Borrower, on Credit Card with Bank j	$d_{\theta j} - \gamma_\theta p_{1\theta j}$	$n_{\theta j} - l_{\theta j}$	$d_{\theta j'} - s_{\theta j'} - \gamma_\theta p_{0\theta j'}$	$n_{\theta j'} - s_{\theta j'} - l_{\theta j}$	$-l_{\theta j}$
Non-Borrower, on Credit Card with Bank j	$d_{\theta j} - \gamma_\theta p_{1\theta j}$	$n_{\theta j}$	$d_{\theta j'} - s_{\theta j'} - \gamma_\theta p_{0\theta j'}$	$n_{\theta j'} - s_{\theta j'}$	0
No Credit Card with Any Bank	*(choice not available)*	*(choice not available)*	$d_{\theta j'} - s_{\theta j'} - \gamma_\theta p_{0\theta j'}$	$n_{\theta j'} - s_{\theta j'}$	0

Notes: The table shows a consumer's one-period flow payoffs depending on the consumer's circumstances at the end of the previous period (by row) and the consumer's choice in the current period (by column). The parameters shown include the flow utility from borrowing, $d_{\theta j}$, and the flow utility from holding a credit card without borrowing, $n_{\theta j}$, as well as disutility from price (marginal utilities of income), γ_θ, and two adjustment costs, including setup costs for opening new accounts, $s_{\theta j'}$, and liquidity costs for paying off existing balances, $l_{\theta j}$. The subscripts j and j' can refer to any bank in the set of banks J, while subscripts θ refer to consumer types.

Table 10: Demand Model: Marginal Utilities of Income

	(1)	(2)	(3)	(4)
Dependent Variable	Log(Retention Rate)		Log(Retention Rate)	
Estimator	OLS	2SLS	2SLS	2SLS
Gamma	-0.0000339***	-0.106***	.	-0.0696***
	(0.0000118)	(0.0129)		(0.00664)
Gamma | Subprime			-0.187***	
			(0.0281)	
Gamma | Prime			-0.141***	
			(0.0108)	
Gamma | Superprime			-0.104***	
			(0.0104)	
Bank-Specific Trends	YES	YES	YES	NO
Observations	60638012	60638012	60638012	60638012
1st-Stage F-Statistic		54.26	47.759	51.31
Clusters	550	550	550	550

Notes: The table shows estimates of price coefficients (marginal utilities of income) estimated via OLS and 2SLS using quasi-experimental lender repricing. Subprime, prime, and superprime accounts in column (3) are defined as FICO scores less than 660, from 660 to 719, and 720 or above respectively. 2SLS estimators use a total of 55 instruments from repricing event dummies interacted with consumer types.

Chapter 2

Credit Reports as Résumés: The Incidence of Pre-Employment Credit Screening*

2.1 Introduction

From social media posts to credit reports and criminal background checks, there is increasingly easy access to information about individuals' private lives. The uses of this information have also grown, as car insurance underwriters check credit scores, not just driving records, and prospective employers check Facebook profiles, not just résumés. In this paper we study a leading example of this information boom: the use of credit reports in labor markets. Pre-employment credit screening (or "PECS" for short) is a popular screening tool among employers, used by perhaps 60% of large firms to screen (some) job applicants in 2010 (Society for Human Resource Management (2012)). However, PECS has also been controversial for its perceived costs, especially for minorities. Policy-makers, often directly citing these disparate impact concerns, have now banned or restricted PECS in eleven states, New York City, and Chicago since 2007.

We develop a statistical discrimination model that builds on Phelps (1972) and Autor and Scarborough (2008) to guide our thinking about PECS bans and restrictions on the usage of information in labor markets more broadly. The model illustrates that a ban on PECS can plausibly either raise or lower both minority hiring rates, and average match quality of minority hires, even though minorities in the US are disproportionately likely to

*This chapter is co-authored with Alexander Bartik. This paper has benefited from conversations with David Autor, Kenneth Brevoort, Amy Finkelstein, Peter Ganong, Michael Greenstone, Danielle Li, Pascal Noel, Jonathan Parker, James Poterba, Paul Rothstein, Antoinette Schoar, and Danny Shoag, and seminar participants at the CFPB and MIT. Joyce Hahn (US Census Bureau) provided generous advice on the LEHD J2J data. The views expressed herein are those of the authors and do not necessarily reflect those of the Consumer Financial Protection Bureau or the United States. The first version of this paper was released in March 2016.

have low credit scores.[1] The key mechanism highlighted by the model is that credit report information may be differentially informative *relative* to other available screening tools for different groups. In particular the use of PECS may benefit minorities by allowing some job-applicants to, intuitively speaking, "stand out from the competition," or more formally, move an employer's posterior beliefs about match quality above a low or diffuse prior (Autor and Scarborough (2008)).

We investigate the implications of this model by studying the impact of PECS bans on different racial groups. Using geographic, temporal, and job-level variation in these bans, we study labor market outcomes in two distinct datasets - the Current Population Survey (CPS), and administrative data aggregated from state unemployment insurance records. We estimate that PECS bans in fact have sizable, negative effects on labor market outcomes for blacks. Our estimates suggest that black job-finding hazards declined by 7 to 16 percent after a PECS ban, while new black hires became 3 percentage points more likely to experience involuntary separation shortly after being hired. Our estimates for Hispanics and non-minorities are less conclusive, and are imprecise enough to be consistent with large or moderate positive or negative effects. Interpreted in light of our model, these findings suggest that in the absence of PECS firms deferred more to their priors in evaluating new applicants' match quality. More broadly, regulation of the information used in hiring may have surprising consequences depending on the alternative match-quality signals towards which firms substitute.

Our empirical work begins with a standard, state-time difference-in-differences analysis of job-finding and separation rates, exploiting temporal and geographic variation in recent PECS bans. We then corroborate our results using job-level variation in which individuals are covered by or exempted from PECS bans. Next, we also estimate demanding triple-difference models to explore the robustness of our results to less restrictive identifying assumptions. Finally, we also estimate "event-study" (or here, "event-time") style models where we fully interact PECS ban indicators with time dummies, allowing us to explore both the evolution of PECS bans' impacts over time, and to test our estimators' various parallel-trends identifying assumptions.

In the CPS, using our preferred state-time specification we find that PECS bans decreased black job-finding hazards by 15.6 log-points. When we corroborate this result using job-level variation, we find that this estimate grows in magnitude, to 20.9 log-points. The difference in these estimates is remarkably close to what would be predicted by the share of black workers actually covered by PECS bans, given various bans' carve-outs for some sectors or occupations. Our event-time analyses also show no evidence of divergent pre-trends for blacks in PECS-ban states or jobs, supporting our causal interpretation of these results.

Our corresponding CPS estimates for Hispanics and whites are less conclusive. For Hispanics, our estimates of PECS bans' effects on job-finding hazards are positive, but

[1] In the mid-2000s for example, over 50% of blacks were in the bottom quintile of the credit score distribution, and roughly 50% of Hispanics were in the bottom tertile (Avery et al. (2009))

imprecise. For whites, our estimates are more precisely estimated near zero, but exhibit some sensitivity to how we control for possible divergent pre-trends. So, while we are most confident in our results for blacks' labor market outcomes, it may be that PECS also has labor market effects for other groups.

Next we further probe these questions in an administrative dataset, the Longitudinal Employer Household Dynamics Job-to-Job data (LEHD J2J). The US Census Bureau builds this dataset by aggregating wage and employment data from state unemployment-insurance records. Relative to the CPS, the LEHD J2J data have the advantages of not relying on self-reported employment status, and being built from a (near) universe rather than a CPS sample that, in some less populous states, can be relatively small. We find qualitatively similar, although smaller, results for black job-finding rates in the LEHD J2J, with our preferred specification suggesting that PECS bans decrease black job-finding hazards by 7.4 log-points.

Similar to the CPS estimates, the LEHD J2J estimates for whites are small in magnitude and sensitive to how we control for possibly divergent pre-trends. Our estimates for Hispanics in the LEHD J2J diverge from those in the CPS, with the estimated effect on job-finding for Hispanics being negative in the LEHD J2J, but positive in the CPS. These inconsistencies again make it difficult to make any conclusive statement about the effects of PECS bans for these two groups.

We next investigate a second prediction of our statistical discrimination model, that these negative effects of PECS bans on black applicants' hiring rates should coincide with lower average match quality among the set of black applicants who get hired. That is more high-match-quality applicants are rejected and more low-match-quality applicants are hired. We use separation rates among new hires as an observable proxy for their match quality. Using state and temporal variation, we find that the share of new black hires involuntarily separating from their jobs increases by 2.7 percentage points. This estimate rises to 4.4 percentage points if we also exploit the variation in which jobs are covered by PECS bans. We perform placebo checks using a sample of long-tenure black employees, who were presumably screened before PECS bans took effect, and find no effect of PECS bans in this sample, suggesting that our results are not due to general declines in black labor market outcomes in states or occupations were PECS usage is banned.

These findings have important implications for both labor and credit markets. First, they show that restrictions on the information used in job-applicant screening can have large effects on labor market outcomes. Indeed, the estimated effect of PECS ban on job-finding is as large in magnitude as an 8 to 18 percent reduction in unemployment benefits (Meyer (1990)) or a 3 to 9% rise in wages (Lichter et al. (2014)).[2] Second, the results suggest that credit reports contain economically relevant information in at least one large non-credit market, the labor market, and so changes in credit reporting practices or regulation may have effects that extend far beyond credit markets themselves.

[2]See Section 2.8 for further interpretation of the magnitudes of our estimates.

A contemporaneous paper, Ballance et al. (2017), studied the impacts of PECS bans on census tracts with different average credit scores. In subsequent work, Friedberg et al. (2016) expanded upon this work, and studied the direct effects of PECS bans on individuals with poor credit health, and Cortes et al. (2017) investigated the market-wide effects of PECS bans in labor markets with low average credit scores. These studies paint the picture that PECS bans have disproportionately helped individuals with poor credit, although they may have had more adverse market-wide consequences. Our study instead emphasizes that not all groups with poor credit necessarily benefit from PECS bans, and that these bans can have surprisingly adverse effects depending on how *informative* PECS was as a signal of job-applicant match quality for the given group. Our paper also relates to a broader literature on the usage of information in labor markets, including more specific types of credit information (Bos et al. (forthcoming), Herkenhoff et al. (2016), and Dobbie et al. (2017)), drug testing (Wozniak (2015)), and criminal records (Holzer et al. (2006), Agan and Starr (2016), Doleac and Hansen (2017), and Shoag and Beuger (2016)).

Our paper makes three novel contributions to this literature. First, we provide the first evidence that PECS bans have negative effects on average labor market outcomes for minorities, despite their lower average credit scores, arguably contrary to the bans' intended effects. Second, we validate these findings using two datasets, a longitudinal dataset of individuals (the Current Population Survey) where we are able to exploit variation in PECS ban coverage across occupations and industries as well as states, and administrative data aggregated from unemployment records, where we exploit state-level variation in PECS bans. Third, we explain these findings using a statistical discrimination framework that builds on Phelps (1972) and Autor and Scarborough (2008), which shows that PECS bans can hurt minority workers - despite their lower credit scores - if credit scores are relatively more informative about match quality for minorities. We investigate one implication of this model, that the match quality of minority new hires should decline after PECS bans are implemented, and find that separation rates for black new hires rise, consistent with the decline in match quality predicted by our model.[3]

The remainder of this paper is organized as follows. In the next section, we discuss the growing literature on the usage of non-labor market information in labor markets. We then provide background on the use of PECS and describe the eight state PECS bans we study.[4] We then develop a statistical discrimination model in Section 2.4 that guides the interpretation of our results. Section 2.5 introduces our data and presents basic summary statistics. In Section 2.6, we flesh out our empirical strategy for estimating the causal effect of PECS bans. We present our results in Section 2.7. We provide some interpretation of our

[3]Herkenhoff et al. (2016) find a related result in studying the removal of past bankruptcy flags from credit reports, finding that individuals hired after their bankruptcy flag is removed have lower earnings than observably similar individuals who did not have a bankruptcy. They perform a back of the envelope calculation which suggests that this difference corresponds to roughly 3% lower match quality among individuals who have declared bankruptcy.

[4]Although there have been 13 state and local PECS bans, in practice we only study eight; the remaining five have been enacted too recently to provide sufficient post-ban data to study.

results in Section 2.8, and then Section 2.9 concludes.

2.2 Literature

As the costs of storing and accessing information about potential employees has fallen, employer usage of this information during the hiring process has risen. Policymakers and researchers have taken a keen interest in the broader implications of this growth in the types and amount of information used during the hiring process. This growth has led to bans and proposed bans on the usage of criminal records, credit information, and drug-testing in the hiring process and a growing body of empirical and theoretical research on these policies.

Most closely related to our paper are one contemporaneous paper, Ballance et al. (2017), and two more recent papers, Cortes et al. (2017), and Friedberg et al. (2016). Whereas we focus on the theory and empirics of how PECS bans can potentially harm groups that have poor credit, these three papers focus on the direct effect of PECS on individuals or geographic areas with bad credit scores or who are in financial distress.[5] These papers also differ from ours in that they do not investigate in a detailed way how the effects of PECS bans vary by occupation or the implication of theory that PECS bans should reduce the match quality of new hires. Instead, these papers focus on the complementary questions of how the effect of PECS bans vary by credit status or proxies for credit status, and whether firms substitute towards alternative signals of match quality.

Friedberg et al. (2016) use the Survey of Income and Program Participation (SIPP) and state-level variation in PECS bans to study the impacts of PECS bans on job-seekers who have had recent trouble paying their bills, which they use as a proxy for poor credit. They find that PECS bans increase job finding hazards by roughly 25 percent for these individuals, consistent with PECS bans allowing these individuals to avoid being removed from consideration for jobs because of bad credit.

Using the Longitudinal Origin Destination Employment Statistics (LODES) and tract-level variation in average credit scores, Ballance et al. (2017) find evidence consistent with Friedberg et al. (2016), estimating that employment in tracts with average credit scores below 620 rises roughly 6 percent after PECS bans are enacted. They then exploit data on the text of job-postings to investigate whether firms have substituted towards alternative signals of match quality, and find evidence that firms increased education and experience required in job-postings when they were unable to rely on credit information.

[5] Friedberg et al. (2016) and Ballance et al. (2017) briefly also explore effects by race. Friedberg et al. (2016)'s point estimates for blacks are actually positive, but their standard errors large enough to be consistent with very large positive or negative effects on blacks. Ballance et al. (2017) use the ACS to study the effect of the bans on the overall black employment rate, rather than transitions from unemployment to employment, and they do not investigate effects on whites or Hispanics. However, despite these differences, a back of the envelope calculation suggests that Ballance et al. (2017)'s estimate that PECS bans reduce employment by 1.9 percentage points for blacks are quite similar to our range of 0.5 to 1.3 percentage points. See Section 2.8 for more details on interpreting our estimates.

Cortes et al. (2017) investigate the effect of PECS bans at a more aggregate level - the county - and find that job *vacancies* decline in counties with lower average credit scores. Combined with the findings from Friedberg et al. (2016) and Ballance et al. (2017), these findings suggest that although for individuals with low credit scores, PECS bans have raised hiring rates, they have reduced aggregate job-posting. Intriguingly, Cortes et al. (2017) find evidence that PECS bans feed back into additional financial distress and worsening credit outcomes in counties with low average credit scores.

Several related literatures have also studied PECS as a screening tool. In organizational psychology, Bernerth et al. (2012) find that higher credit scores are positively correlated with worker "conscientiousness" and negatively correlated with worker "agreeableness," whereas Bryan and Palmer (2012) find no correlation between adverse credit histories and employee appraisal ratings. In management science, Weaver (2015) finds that his imputed values of credit scores are uncorrelated with match quality.

Three recent studies on the removal of adverse information from credit reports are closely related to, and complementary to, our own. Bos et al. (forthcoming) study an administrative change in Sweden that removed bankruptcy and default information from some borrowers' credit reports, and find that this change led to higher employment rates for affected individuals. In two related studies, Herkenhoff et al. (2016) and Dobbie et al. (2017) both study the effect of the removal of bankruptcy flags on labor market outcomes using labor market data linked with credit records. Consistent with our findings, they both find that removal of bankruptcy from credit records has modest effects on flows into employment.[6] Both authors also find that employment rises particularly in retail and services, consistent with survey evidence (Society for Human Resource Management (2012)) that these types of jobs may be particularly likely to rely on credit checks during the hiring process. Herkenhoff et al. (2016) also explores transitions into self-employment, finding that bankruptcy flag removal increases the probability of self-employment and earnings from self-employment.[7]

All three of these papers provide important new evidence on the relationship between labor and credit markets. However, the settings and variation in these papers differ from our own. The identifying variation in Bos et al. (forthcoming) affected both credit and labor markets, as it prevented lenders as well as employers from viewing past default information. Their study also focused on Swedish pawnshop borrowers who previously defaulted on their loans, which is both a much different institutional context and likely a more credit-challenged

[6]The authors interpret the magnitude of these results differently, with Herkenhoff et al. (2016) viewing the estimates as sizable and Dobbie et al. (2017) viewing them as small. In practice, the magnitudes they estimate are similar, with Herkenhoff et al. (2016) finding that removal of bankruptcy flags increases the probability of transitioning to formal employment by .3 pp (roughly 7 percent), while Dobbie et al. (2017) find that removing bankruptcy flags reduces the probability of being non-employed by .4 pp, or around 3.6 percent.

[7]Dobbie et al. (2017) briefly explore the effects of PECS bans on workers who filed for bankruptcy 4-6 years ago. However, they estimate effects on employment levels, rather than flows out of unemployment, and do not disaggregate these results by race. More broadly, it's unclear how much weight employers put on bankruptcy filings 4-6 years ago, making the results difficult to generalize to the overall population.

group than the population of US job-seekers as a whole.

Herkenhoff et al. (2016) and Dobbie et al. (2017) study the removal of bankruptcy flags, which usually occurs 7 to 10 years after bankruptcy was declared. However, survey research suggests that only some employers check information as far back as seven years.[8] Furthermore, bankruptcy is only one type of adverse credit information and the effects of availability of other types of adverse credit information may differ. We thus view these three studies as informative about different types of information in different populations than our own.

Our results are also related to other research in economics on the relationship between minority employment and the use of criminal background checks or drug tests: Holzer et al. (2006) find that employers' use of criminal background checks predicts higher black-male employment, despite higher levels of criminal history among black males. Finlay (2009) finds that labor market outcomes declined for ex-offenders once criminal records became widely available online. Similarly, Wozniak (2015) finds that restrictions on drug testing during employment screening are associated with reduced black employment.[9]

More recently, a similar movement to the one that has led to PECS bans has resulted in a number of cities and states restricting the use of criminal record checks during the hiring process for private-sector firms. These "Ban-The Box" (BTB) policies have been passed in a number of states and large cities. Using a clever resume-audit design to measure discrimination and a difference-in-differences strategy exploiting New Jersey and New York's BTB policies, Agan and Starr (2016) find that BTB increases job application callback rates among blacks with criminal records, but decreases them substantially among blacks without criminal records. On net, the BTB policies hurt the average callback rates of blacks, with the primary beneficiary of the policies being whites with criminal records.

Doleac and Hansen (2017) confirm that the differences in callback rates found in Agan and Starr (2016) have translated into changes in actual hiring, finding that BTB policies reduce employment by 3.4 percentage points for low-skilled black men. Using the Longitudinal Origin Destination Employment Series (LODES), Shoag and Beuger (2016) reach somewhat contradictory findings, finding that employment actual rose by 4% for residents of neighborhoods with high crime-rates. Broadly, estimates of the effect of restrictions on the use of criminal records and drug-testing parallel our own result that PECS bans have negative labor market consequences for blacks despite their low average credit scores.

Finally, our work is also related to a growing literature on the optimal accessibility and content of credit report data (Musto (2004); Bos and Nakamura (2014); Kovbasyuk and Spagnolo (2015)). While this literature has focused on credit markets per se, work is increasingly needed on non-credit markets where the use of credit report data is common, including labor markets, apartment rental markets, utilities, and cable/telecom contracts.

[8]In the Society for Human Resource Management (2012) report, roughly 25% of firms say they look at information from 7 years ago or later.

[9]Wozniak (2015) notes two important facts: (1) the prevalence of illegal drug use is broadly similar among blacks and whites, and (2), nevertheless, a literature in sociology and economics finds that drug use is commonly perceived to be more prevalent among blacks.

2.3 Background and Institutions

In this section, we discuss the usage of credit information in the hiring process, outline the policy concerns with this practice, and detail the bans that several state and local governments have enacted in response.

While evidence on employer use of PECS is limited, an industry survey suggests that perhaps 60% of firms used PECS to screen job applicants in 2010; roughly a quarter of these firms used PECS for all job applicants (Society for Human Resource Management (2012)). For over half of these firms, the primary reason for using PECS was to prevent theft, and correspondingly these firms report using PECS for nearly all jobs (91%) that involve handling cash or other fiduciary responsibility (Society for Human Resource Management (2012)).

PECS also has a non-trivial effect on hiring decisions. Household survey evidence suggests that 10% of low- and middle-income[10] job seekers recall being told they were denied a job on the basis of information in their credit report (Traub (2013a)). As Traub (2013b) argues, the true PECS-related rejection rate may be higher if firms do not always comply with the Fair Credit Reporting Act's requirement of sending adverse-action letters that report the use of credit report data in adverse hiring decisions, or if applicants do not recall receiving these letters.

Restrictions of the use of PECS have typically been motivated by two concerns. First, PECS is seen as having an inequitable effect on traditionally disadvantaged job applicants. US Sen. Elizabeth Warren, for example, has claimed that "credit reports in the hiring process are disproportionately used to disqualify people of color from open positions" (Office of Senator Elizabeth Warren (2013)). The EEOC has pursued a series of PECS-related Civil Rights Act suits against employers, asserting that PECS "tend[s] to impact more adversely on females and minorities" (Crawford (2010)). Second, PECS is seen as contributing to labor market hysteresis, as PECS may increase the persistence of unemployment shocks for individuals with poor credit histories.

On net, this policy debate has seen thirteen new PECS bans enacted over the past eight years, while more than a dozen other states have seen related legislation proposed but not enacted (Morton (2014)). Washington was the first state to enact a PECS ban, in April 2007. Hawaii, Oregon and Illinois then followed suit in 2009-2010 and were joined by Maryland, Connecticut, California, Vermont, Colorado and Nevada in 2011-2013. Delaware restricted PECS for public employers in 2014. Chicago enacted city-level restrictions on PECS in 2012 to eliminate some of the exemptions in Illinois's ban, and New York City joined Chicago in mid-2015, enacting what is arguably the US's strongest PECS ban. Table 1 lists all states and large cities that have enacted PECS bans, along with the dates the laws were signed and went into effect.

These bans vary in strength because of the exemptions they grant to certain jobs. The bans variously grant exemptions to jobs that involve access to payroll information, jobs

[10]The survey defined middle-income as up to 120% of county-level median income.

in high-level management, jobs that involve supervising other staff, and jobs in dozens of other industries or categories such as law enforcement, gaming, space research, banking, or insurance.[11,12] In Table 2 we summarize the full breadth of this heterogeneity in PECS bans' exemptions. We collected this heterogeneity by referring to statute texts, various state agencies' interpretations of statutory terms such as "banking activities," and guidance from human-resources law firm Littler Mendelson that summarizes relevant case law (Gordon and Kauffman (2010), Rubin and Nelson (2010), Rubin and Kim (2010), Fliegel and Mora (2011), Fliegel and Simmons (2011), Fliegel et al. (2011), Fliegel and Mora (2012), Fliegel et al. (2013)). To use these exemptions empirically, we then translate each law's exemptions into the Census industry and occupation codes that will classify jobs in our data, a process that we describe in more detail in Section 2.5 below.

The results of this process indicate how PECS bans' coverage varies across states. Among jobs ever covered by a PECS ban, we estimate that 48.8% are granted exemptions from a PECS ban in at least one state.[13] And among states that enact PECS bans, we find that the share of workers actually covered by a ban ranges from 41.5%, in Connecticut, to 79.7%, in Hawaii.

2.4 Conceptual Framework

In this section we develop a framework for understanding PECS's possible effects on minority labor market outcomes[14]. We develop a statistical discrimination model that builds on Phelps (1972) and especially on Autor and Scarborough (2008), and we illustrate in the model how PECS's effects on minority labor market outcomes can be positive even if minorities' credit reports are on average less favorable than non-minorities'.[15] The key mechanism in the model

[11]The bans also differ in their enforcement mechanisms. The enforcement mechanism in Illinois, for example, relies on private litigation by job applicants; in contrast the Connecticut law tasks the state Department of Labor with enforcement. These differing enforcement mechanisms also raise the question of how vigorously different regulators or plaintiff bars have chosen to enforce these laws. From our conversations with state regulators and reading of the professional literature in human resources, we conclude that enforcement has not been particularly vigorous in most states, but that some employers have nonetheless been eager to comply with bans to avoid being in non-compliance. Indeed, Phillips and Schein (2015) reported that, as of their writing, state courts had seen no cases on the state-level bans enacted by 2012, which could be consistent with strong compliance with these laws. Because it is difficult to categorize which of these laws' enforcement mechanisms are stronger than others, our analysis focuses on the between- and within-state heterogeneity in PECS bans' exemptions.

[12]Evidence is consistent with at least a large share of firms complying with the bans. For example, Ballance et al. (2017) find, using Equifax credit data, that employer related credit checks per unemployed person decline 7 to 11 percent in the three years after credit bans are passed (see Figure 3 in Ballance et al. (2017)).

[13]To compute this statistic, we weight by the number of workers employed in each job; the corresponding unweighted statistic is 40.1%.

[14]This model does not incorporate ideas related to hystersis. This is an important topic for future work

[15]Discrimination models are generally divisible into statistical discrimination models and taste-based discrimination models, as reviewed in Bertrand and Mullainathan (2004). It may be compelling to develop a taste-based rather than statistical model of discrimination through PECS, but such a model is difficult to

is that PECS may send, moreso for minorities than for non-minorities, a high-precision signal relative to the precision of other available screening tools (such as interviews). Intuitively, if employers have trouble screening minority job applicants, then it can be difficult for minority applicants to send strong enough positive signals to stand out from the rest of the applicant pool; the availability of additional screening tools such as PECS can help counteract this effect.

Formally we follow Autor and Scarborough (2008) and suppose employers screen job applicants from two observably distinct groups, a minority (B) and a majority (W). We let minority match qualities be normally distributed with mean μ_0^B and inverse variance h_0^B, and majority match qualities likewise be normally distributed, with (potentially different) mean and inverse variance μ_0^W and h_0^W. Match qualities are unobserved, and firms have rational priors over match qualities based on job applicants' membership in group B or W.

Continuing to follow Autor and Scarborough (2008), we suppose that in the absence of PECS, employers' available screening technologies send noisy and unbiased signals $\eta = y + \epsilon$ of each job applicant's match quality, distributed as

$$\eta^r | y^r \sim N\left(y^r, \frac{1}{h_\epsilon^r}\right) \tag{2.4.1}$$

for $r \in \{B, W\}$. Note h_ϵ^r is the inverse variance of the noise term ϵ for race r, and so also the inverse conditional variance of the signal η^r. By Bayes' rule, firms assess the match quality of a given applicant, who is from group r and who has signal η, to be distributed as

$$\mu_1(\eta, r) \sim N\left(\frac{h_0^r \mu_0^r + h_\epsilon^r \eta}{h_0^r + h_\epsilon^r}, \frac{1}{h_0^r + h_\epsilon^r}\right) \tag{2.4.2}$$

and so the unconditional distribution of such posteriors μ_1 (i.e. integrating over the distribution of η) is

$$\mu_1(r) \sim N\left(\mu_0^r, \frac{h_\epsilon^r}{h_0^r\left(h_0^r + h_\epsilon^r\right)}\right) \tag{2.4.3}$$

Now suppose firms additionally use PECS as a screening technology. We suppose PECS sends a noisy unbiased signal $s = y + u$, where the noise u is independent of other screening technologies' noise ϵ, and is normally distributed with inverse variance h_s^r.[16] The unconditional distribution of firms' revised posteriors μ_2 (i.e. integrating over both signals η and s)

reconcile with our empirical results.

[16]Corbae and Glover (2017) presents a related model that microfounds the correlation between credit reports and match quality by assuming that individuals with low wealth and credit constraints are less likely to invest in education and more likely to have poor credit reports. If firms cannot perfectly observe education, this will lead firms to use credit reports as a proxy for education. Corbae and Glover (2017) then embeds this idea into an equilibrium search framework. They abstract from the issues of race that we focus on here.

is therefore,

$$\mu_2(r) \sim N\left(\mu_0^r,\ \frac{h_\epsilon^r + h_s^r}{h_0^r\left(h_0^r + h_\epsilon^r + h_s^r\right)}\right) \tag{2.4.4}$$

We assume firms are seeking to fill an exogenous number of positions M by hiring applicants with the highest expected match quality. Optimal firm strategies are then to hire all applicants with posterior match quality above some cutoff κ (see Autor and Scarborough (2008) for simple primitive conditions guaranteeing these strategies are optimal). Therefore to study minority job-finding we are interested in what share of posteriors $\mu_2(r)$ are above κ for each race r, given the distributions of posteriors that arise from a given set of screening tools. As we define formally below, we refer to the share of race r's posteriors above κ as a hiring rate for race r.

We now examine how the introduction of PECS affects these hiring hazards. While PECS unambiguously makes the tails of the distribution of posteriors thicker,[17] the hiring cutoff κ must also increase in response, in order for markets to clear. So, as we now show formally, the hiring rate for a given race r can either rise or fall in response to the introduction of PECS.

To keep the key intuitions of the model clear, we now proceed by assuming $\mu_0^B = \mu_0^W$ and $h_0^B = h_0^W$; that is, both groups' unobserved match qualities are identically distributed. Thus the key difference between the two groups is in the precision of signals sent by PECS and by non-PECS screening technologies: $h_s^B \neq h_s^W$ and $h_\epsilon^B \neq h_\epsilon^W$. This stands in contrast to Autor and Scarborough (2008), who focus on differences in means μ_0. Meanwhile our emphasis on differences in signal precision follows in the vein of Phelps (1972), who illustrated how screening precision can affect hiring outcomes.[18] Results available upon request show that our findings are robust to the more general case where $\mu_0^B \neq \mu_0^W$ and $h_0^B \neq h_0^W$.

If we normalize the number of majority applicants to 1 and let m^B be the number of

[17]That is to say, posteriors about each applicant become more precise, and therefore the *population* distribution of posteriors, i.e. the distribution of posterior means, becomes more diffuse. This can be seen by comparing expressions 2.4.3 and 2.4.4.

[18]See especially Phelps (1972)'s Case II and its extension.

minority applicants, then the hiring cutoff κ is defined implicitly by,

$$M = \quad \lambda^W + m_B \lambda^W \tag{2.4.5}$$

$$\lambda^W = \quad 1 - \Phi\left[\frac{\kappa - \mu_0}{\left(\left(h_\eta^W + h_s^W\right) \Big/ \left(h_0\left(h_0 + h_\eta^W + h_s^W\right)\right)\right)^{1/2}}\right] \tag{2.4.6}$$

$$\lambda^B = \quad 1 - \Phi\left[\frac{\kappa - \mu_0}{\left(\left(h_\eta^B + h_s^B\right) \Big/ \left(h_0\left(h_0 + h_\eta^B + h_s^B\right)\right)\right)^{1/2}}\right] \tag{2.4.7}$$

where λ^W and λ^B are race-specific hiring rates. Note that the expressions for λ^W and λ^B follow immediately from 2.4.4 and from firms' cutoff strategies in κ.

To study the effect of PECS, we parameterize the availability of PECS using $t \in [0, 1]$, and we replace the noise of the PECS signal h_s^W with the term th_s^W in expressions 2.4.6 and 2.4.7 above. The case of a complete PECS ban corresponds to $t = 0$, and the case where PECS is available corresponds to $t = 1$. We are then interested in the total derivative $d\lambda^B/dt$ evaluated at various values of t,

$$\frac{d\lambda^B}{dt} = \frac{\partial \lambda^B}{\partial t} + \frac{\partial \lambda^B}{\partial \kappa}\frac{\partial \kappa}{\partial t} \tag{2.4.8}$$

Applying the implicit function theorem to 2.4.5 yields,

$$\frac{d\lambda^B}{dt} = \frac{\partial \lambda^B}{\partial t} + \frac{\partial \lambda^B}{\partial \kappa}\left[\frac{-\frac{\partial \lambda^W}{\partial t} - m^B \frac{\partial \lambda^B}{\partial t}}{\frac{\partial \lambda^W}{\partial \kappa} + m^B \frac{\partial \lambda^B}{\partial \kappa}}\right] \tag{2.4.9}$$

By introducing a common denominator $\frac{\partial \lambda^W}{\partial \kappa} + m^B \frac{\partial \lambda^B}{\partial \kappa}$ which we note to be negative, we conclude

$$\operatorname{sign}\left(\frac{d\lambda^B}{dt}\right) = -\operatorname{sign}\left(\frac{\partial \lambda^B}{\partial t}\frac{\partial \lambda^W}{\partial \kappa} - \frac{\partial \lambda^B}{\partial \kappa}\frac{\partial \lambda^W}{\partial t}\right) \tag{2.4.10}$$

After differentiation and some manipulation, this expression can be evaluated at $t = 0$ to show,

$$\frac{d\lambda^B}{dt} > 0 \iff \frac{h_s^B}{h_s^W} > \frac{h_\epsilon^B}{h_\epsilon^W}\left(\frac{h_0 + h_\epsilon^B}{h_0 + h_\epsilon^W}\right) \tag{2.4.11}$$

That is to say, the minority hiring rate λ^B is increasing in the availability of PECS, t, if and only if PECS sends relatively high-information signals for minorities. Importantly, minorities do not need PECS to send more precise signals in absolute terms; rather the ratio of PECS' precision for minorities relative to non-minorities, h_s^B/h_s^W, must be greater than the corresponding ratio of precisions for non-PECS screening tools, $h_\epsilon^B/h_\epsilon^W$, times a scalar. (Note also that this scalar can readily be interpreted as the ratio of the precision of conditional posteriors in the absence of PECS, as in Equation 2.4.2.)

Figure 1 illustrate this result graphically. In Figure 1 Panel A, we show equilibrium hiring rates in the absence of PECS: one group, labeled "blue," has noisier signals under non-PECS screening technology and therefore less diffuse posteriors than another group, labeled "green." Correspondingly the blue group has a lower hiring rate, given the equilibrium hiring cutoff κ. Figure 1 Panel B then illustrates how posterior beliefs of applicants' match qualities shift after the introduction of PECS, in the case where PECS sends more precise signals for the previously disadvantaged blue group. Finally, Figure 1 Panel C illustrates how the hiring cutoff κ must then shift in order for markets to clear in response to the new information provided by PECS, and how this shift affects hiring rates for each group.

Intuitively, the mechanism illustrated in Figure 1 Panel B illustrates the effect of the term $\frac{\partial \lambda^B}{\partial t}$ in Equation 2.4.8, while the mechanism illustrated in Figure 1 Panel C illustrates the term $\frac{\partial \lambda^B}{\partial \kappa}\frac{\partial \kappa}{\partial t}$. In the simplified case illustrated graphically, we see that the group for which PECS sends relatively precise signals is indeed the group that benefits from the introduction of PECS.

We now turn to our empirical work to study these effects of PECS in practice, beginning with an introduction to our two datasets.

2.5 Data Sources and Summary Statistics

In this section we describe our data and the construction of several variables that play a central role in our analysis. Our primary dataset is the Current Population Survey (CPS), which we use to measure job-finding, separation rates, and overall employment. We supplement our CPS measures of job-finding by using publicly available administrative data collected from the near-universe of state unemployment records: the Job-to-Job (J2J) Flows data from the US Census Bureau, which are compiled as part of the Longitudinal Employer-Household Dynamics (LEHD) program and are currently in beta release.

2.5.1 CPS

We use the panel dimension of the 2003-2015 Current Population Survey's (CPS) micro-data (US Census Bureau (2015a)). The Bureau of Labor Statistics uses the CPS to measure cross-sectional unemployment and labor-force participation, while the panel dimension is used for estimating gross flows in and out of unemployment, employment, and non-participation (e.g., as in Shimer (2012)). Monthly sample sizes are about 100,000 adults, each of whom stays in the sample for four consecutive months, then leaves for eight months, and then re-enters for a final four months. The panel has a rotating structure so that roughly one-eighth of the sample is in each of the eight months.

We adjust the panel structure of the raw CPS micro-data only slightly. We correct household identifiers for occasional erroneous matches between months.[19] Due to recent improvements in the CPS micro-data (see Drew et al. (2014) for a discussion) this procedure does not rely on the more intricate matching process often used on CPS data from the 1990s and earlier (Madrian and Lefgen (1999)). We also remove military members and any children aged eighteen or younger from our panel. Finally, in order to have more clearly interpretable flow estimates, we remove individuals on temporary layoff from the population we refer to as "unemployed" (Katz and Meyer (1990)).

When measuring an individual's race, we make the ad hoc classification choice that multi-racial individuals are "black" whenever they identify partly as black, and otherwise are "Hispanic" whenever they identify as Hispanic. We group all other race groups into a non-minority category that we refer to as "white." Thus we reach three mutually exclusive, collectively exhaustive categories which we refer to together as "race."

Table 3 presents summary statistics for the CPS data. Columns (1) and (2) respectively show statistics for states that do and do not ban PECS. Columns (3) and (4) then focus on states with PECS bans, and respectively show statistics for jobs covered by and exempted by those bans. Statistics are presented separately for blacks, Hispanics, and whites in three different panels, A through C. We see that labor market characteristics such as employment rates and labor force participation rates are broadly similar within race across states, although employment rates are slightly higher in states without PECS bans. For both blacks and whites, wages are also higher in PECS ban states, as is the share of workers with a four-year a college degree.

Whereas states with PECS bans have higher average wages and more high-education workers, these states tend to target PECS bans at relatively low-wage and low-education jobs. We see in columns (3) and (4) that blacks, for example, earn roughly 37% higher wages in jobs exempted from PECS bans, and are twice as likely to hold a college degree when working in exempted jobs. This reflects the patterns seen previously in Table 2, where PECS exemptions generally are granted for white-collar jobs such as jobs in management and finance.

Finally, in Table 4 we present a similar table of CPS summary statistics where we focus on three outcomes related to the dependent variables used in our analysis: job-finding hazards, involuntary separation rates for new hires, and involuntary separation rates for long-tenure workers. We discuss these statistics when we turn to our analysis of these outcomes in Section 2.7.

[19]More precisely, when one household is replaced by another due to mid-panel attrition, we generate new identifiers for the replacement households in cases where the new and old identifiers coincide. This affects less than 0.07% of households in the data.

2.5.2 LEHD J2J Data

The CPS provides rich longitudinal information on individual job-finding hazards and separation rates. However, our estimates, although reasonably precise, are somewhat noisy. Furthermore, data in the CPS is self-reported and this may result in further uncertainty. We address these concerns by analyzing the Job-to-Job (J2J) Flows data released as part of the Longitudinal Employer-Household Dynamics (LEHD) program (US Census Bureau (2015b)). This is a publicly available administrative data aggregated from Unemployment Insurance (UI) records from all 51 states and Washington, DC.[20] We refer to these data as the LEHD J2J data.

The LEHD J2J reports three different measures of transitions to new jobs, depending on the duration of unemployment spells between jobs. These three measures correspond to spells that last two or more quarters ("transitions from persistent unemployment"), spells that last roughly one quarter ("adjacent-quarter transitions"), and spells that last less than one quarter (including spells of zero length, i.e. job changes without any time off from work). Because of the coarse nature of the quarterly data none of these three categories contains exclusively voluntary or involuntary job changes.

In our analysis we focus on the intermediate category, adjacent-quarter flows. We note that this category includes spells of involuntary unemployment but also short voluntary breaks between jobs (Hyatt et al. (2015)). The choice to use this category strikes a balance between trying to focus on involuntary unemployment, which would be impossible in the shortest-duration category, and avoiding duration-dependence problems that would arise in using the longest-duration category.[21]

The Census releases the LEHD J2J data separately by worker race categories. The census generates these race data by merging the Unemployment Insurance data using Social Security Numbers (SSNs) with the Decennial Census short form, which contains detailed race information, and the Social Security Administration (SSA) Personal Characteristics File (PCF), which contains more limited information on race.[22] Roughly 95% of observations in the LEHD J2J are matched to either the Census short form or the SSA PCF file. Race is imputed for the remaining 5% of observations (Abowd and McKinney (2009)). Following our procedure in the CPS data, we then aggregate race into three major categories: black, Hispanic, and a non-minority category referred to as "white."

[20]Note that although the LEHD compiles data from all 51 states, all states are not yet included in the LEHD J2J data. Data from Massachusetts and Kansas are excluded, which causes data for several other states to be suppressed in cases where there are high migration rates to or from Massachusetts or Kansas. This leads to the exclusion of Missouri, Connecticut, Rhode Island, Maine, New Hampshire, and Vermont. The data from these suppressed states is still included in the flows data for other states. For example, if a person separated from a job in New York and took a job in Connecticut, this would be recorded as a job-to-job flow for New York, even though Connecticut's own flows data are suppressed.

[21]These duration dependence problems arise because the same individual appears in the unemployed pool in multiple quarters, yet the length to-date of the spell in each case is unobserved. See Section 2.6 for more discussion of how we account for duration dependence in the CPS data.

[22]Most importantly, the PCF does not contain information on Hispanic origin.

The Census releases both raw and seasonally adjusted versions of the LEHD J2J. We use the seasonally adjusted time-series.

Table 5 reports summary statistics on separation rates and adjacent-quarter job-finding in the LEHD J2J between the first quarter of 2005 and the second quarter of 2014 (the years for which we have data from all states). Columns (1) and (2) report averages for states that have not banned and have banned PECS respectively. Panel A then reports the average separation rate (the fraction of workers that separate from their main job within a quarter), while Panel B reports the average adjacent-quarter job-finding rate, which measures the share of workers separating from their job in a given quarter that find a new job by the end of the next quarter.

A few features of Table 5 are notable. Starting with Panel A, notice that separation rates are fairly high: between 6 and 10 percent of workers separate from their employer and do not have new employment at the start of the subsequent quarter. Separation rates are also around forty to fifty percent higher for blacks and Hispanics than they are for whites. Conversely, in Panel B we see that adjacent-quarter job-finding rates are quite similar for different racial groups, with blacks, whites, and Hispanics all having job finding rates between .216 and .238, depending on the state category. Comparing columns, we see that states that ban PECS have lower separation rates and job-finding rates for all racial groups, but the differences are small in magnitude.

2.5.3 Encoding Job-Level Variation

As we introduced in Section 2.3 and Table 2, PECS bans typically include a substantial number of job-specific exemptions. Our goal is to use this rich, job-level variation to complement our baseline state-level analyses. To do so, we first need to categorize which individuals' jobs in our data are covered by or exempted from each law.

We identify jobs in our data using US Census 4-digit industry codes and 4-digit occupation codes, the most precise classifiers available in the CPS.[23] We then encode each of these occupations and industries as either covered by, or exempted from, each PECS ban, based on the legal sources detailed in Section 2.3 and, when necessary, our judgment.[24] Finally, consistent with the PECS ban statutes, we code a job as exempt whenever either its industry or occupation is coded as exempt.

[23] These 4-digit codes represent a relatively fine partition of industries and occupations: for example, industry code 9070 is for "drycleaning and laundry services," while occupation code 4420 is for "ushers, lobby attendants and ticket takers."

[24] In some cases the correct encoding of industries' and occupations' exempt status is clear. For example, we encode occupation code 3850, "police and sheriff's patrol officers," as exempt in states that grant an exemption for law enforcement occupations. Other cases are more ambiguous, particularly when exemptions are granted to specific job features (e.g., "unsupervised access to marketable assets"). In these cases we use our judgment and explore robustness to alternative classification schemes. In general, if we misclassify jobs in any of these ambiguous cases, our empirical estimates of PECS bans' effects will be biased toward finding no effect (i.e., attenuation bias), as we discuss more in Section 2.6 below.

We next use this classification of jobs' exempt status to measure individuals' exposure to PECS bans. For employed individuals this is straightforward: in a PECS-ban state after the enactment of a ban, an individual is exposed to the ban whenever his current job is not exempt.

For unemployed individuals, we develop two measures of exposure to PECS bans, both based on an individual's most recent job. Our first measure, which we refer to as "past job" exposure, simply supposes an individual is exposed to a ban whenever his most recent job is not exempted by that ban. The limitation of this measure is that many job-seekers search for jobs in occupations and industries other than those of their most recent job. Therefore our second measure, which we refer to as "expected job" exposure, uses an estimate of each unemployed individual's probability of searching for work in a non-exempt job, conditional on her most recent job. We construct this measure by assuming these search probabilities are proportional to observed job-to-job transition rates (via unemployment) in the absence of PECS bans, and then using our ban-specific measures of jobs' exempt status.[25]

For all three of these measures of individuals' exposure to PECS bans, i.e., current (C), past (P), and expected (E) job exposure, let $T^{l}_{j(i),s(i)}$ stand for the exposure of individual i in job j and state s after the enactment of state s's PECS ban, for $l \in \{C, P, E\}$. For example, $T^{E}_{j(i),s(i)} \in [0, 1]$ stands for unemployed individual i's expected-job exposure when formerly employed in job j, i.e. $T^{E}_{j(i),s(i)} = p_{j,s} = \Pi_j t_s$, using the notation developed in Footnote 25.

2.6 Empirical Strategy

In this section, we discuss the variation we exploit to study the effect of PECS bans and how we use this variation empirically.

The core of our empirical strategy is difference-in-differences. We first develop this strategy using between-state variation in the timing of PECS bans, and we then show how to extend this strategy to use job-level variation in which workers or job-seekers are exposed to each ban. We discuss and test, by means of event-time plots, the parallel-trends identifying assumptions underlying this strategy. We also estimate demanding triple-difference models of the effect of PECS bans, to explore the robustness of our results to less restrictive identifying assumptions. Given our focus on how PECS bans affect minority labor market outcomes, we also allow all of our estimates of PECS bans' effects to vary by race.

We next discuss in detail how this empirical strategy can be used to study our main outcome of interest, job-finding rates. Because job-finding rates are well known to exhibit

[25]To describe our "expected job" exposure more formally, let t_s be a vector of job-specific treatment dummies indicating which jobs are treated by a PECS ban in state s (i.e., zeros in t_s correspond to exempted jobs). We estimate job-to-job transition probabilities (via unemployment) in all untreated states and months, collect these probabilities in the Markov matrix Π, and pre-multiply t_s by Π to obtain a state-specific vector of job treatment probabilities p_s for the unemployed, $p_s = \Pi \times t_s$. Intuitively, each component j of p_s is a measure of the probability that an unemployed worker formerly employed in job j will transition into employment in a job that is treated in state s, conditional on transitioning into some employment. We then assume search probabilities are equal to these estimated transition probabilities. See also footnote 30.

duration dependence, we formally estimate a hazard model of job-finding. We show how a discrete-time, semi-parametric proportional hazards model can be used tractably in conjunction with our difference-in-differences strategy. Finally, we develop an estimator for this model on each of our two datasets.

2.6.1 State-level Variation

Our baseline specifications take advantage of geographic variation in which states enacted PECS bans and temporal variation in when those bans were enacted. Letting y_{ist} be a labor market outcome for an individual i living in state $s(i)$ in time t, $D_{s(i)}$ be an indicator for state s(i) having ever put into effect a PECS ban, and $P_{s(i),t}$ be an indicator for whether a state had implemented a PECS ban by time t, we estimate a difference-in-differences model of the effect of PECS on labor market outcomes:

$$y_{it} = \alpha_{s(i)} + \gamma_t + \delta D_{s(i)} \times P_{s(i),t} + \epsilon_{it} \tag{2.6.1}$$

The parameter of interest in Equation 2.6.1, δ, will identify the causal effect of PECS on outcome y_{it} under the assumption that states enacting PECS bans would have had, in the absence of a PECS ban, parallel trends in y_{it} relative to states not enacting PECS bans.

Given the policy concerns about PECS' disparate impacts on minorities we are interested in whether PECS differentially affects different racial groups. To capture these heterogeneous effects of PECS, we fully interact all of the right-hand-side variables in Equation 2.6.1 with race[26] categories, leading to the workhorse specification we use in much of our analysis:

$$y_{it} = \alpha_{s(i),r(i)} + \gamma_{t,r(i)} + \sum_{r} \delta_r 1_{r=r(i)} \times D_{s(i)} \times P_{s(i),t} + \epsilon_{it} \tag{2.6.2}$$

Equation 2.6.2 is equivalent to running Equation 2.6.1 separately by race.[27] The parameters of interest, the race specific interactions δ_r, will identify the causal effect of PECS on labor market outcomes for each race r under the slightly weaker identifying assumption that different races would have had, in the absence of a PECS ban, similar trends in states enacting PECS bans as in states not enacting PECS bans.

Finally, in our most demanding specifications, we add state-time fixed effects to Equation 2.6.2, leading to the triple-differences specification:

$$y_{it} = \alpha_{s(i),r(i)} + \gamma_{t,r(i)} + \xi_{s(i),t} + \sum_{r \neq W} \delta_r 1_{r=r(i)} \times D_{s(i)} \times P_{s(i),t} + \epsilon_{it} \tag{2.6.3}$$

Note that we can no longer include all race-treatment interactions, because one is absorbed

[26]Recall from Section 2.5 that we defined three mutually exclusive and collectively exhaustive race categories, $R = \{\text{white}, \text{black}, \text{Hispanic}\}$.

[27]In practice, as we discuss below, we cluster our standard errors at the state level, meaning that our variance-covariance matrix differs from running Equation 2.6.1 separately by race.

by the state-time fixed effect, so we instead sum over all non-white races (i.e. $\sum_{r \neq W}$). Equation 2.6.3 will identify the causal effect of PECS bans on the relative outcomes of blacks or Hispanics (compared to whites) under the assumption that PECS-ban states and non-ban states would have had, in the absence of a PECS ban, the same trends in the *difference* between blacks' and whites' (or between Hispanics' and whites') labor market outcomes. It is important to keep in mind that Equation 2.6.3 only identifies the effect of PECS on blacks or Hispanics relative to whites. To interpret the triple-difference estimates as the total effect of PECS on minority job finding, one needs to add the additional assumption that PECS does not impact the job-finding hazard for whites.

In an effort to investigate the validity of our parallel trends assumptions and explore how treatment effects change over time, we also estimate event-time models where we fully interact our treatment dummies with event time, i.e., the number of time periods since a given ban took effect. Formally, let t_0^s be the time period when PECS is banned in state s and define $\kappa_{st} = t - t_0^s + 1$. Fully interacting the vector of event-time indicators, κ, with a PECS ban indicator then leads to the event-time study specifications below. For example, the event-time specification corresponding to Equation 2.6.2 is:

$$y_{it} = \quad \alpha_{s(i),r(i)} + \gamma_{t,r(i)} + \sum_k \sum_r \delta_r^k 1_{r=r(i)} \times D_{s(i)} \times 1_{\kappa_{st}=k} + \epsilon_{it} \tag{2.6.4}$$

2.6.2 Job-level Variation

The specifications in Section 2.6.1 above do not exploit the substantial job-level variation available in different states' PECS bans.

To leverage this job-level variation we use the treatment measure $T^l_{j(i),s(i)}$, as we constructed in Section 2.5.3. Recall that $T^l_{j(i),s(i)}$ is a measure of how state s's PECS ban covers an individual i with job j, where we use the notation $l \in \{C, P, E\}$ to stand for a PECS ban's coverage of either a current job (C), past job (P), or expected job (E).[28] Our baseline specification relying on job variation is then:

$$y_{it} = \quad \alpha_{s(i),j(i)} + \gamma_t + \delta D_{s(i)} \times P_{s(i),t} \times T^l_{j(i),s(i)} + \epsilon_{it} \tag{2.6.5}$$

The identifying assumption in this baseline specification is that treated jobs (i.e. a job covered by a law in a treated state) are on parallel trends with non-treated jobs (both exempted jobs within PECS-ban states and all jobs in non-PECS-ban states).

Note that this specification may produce high-variance estimates in datasets of moderate size, given the large number of state-job fixed effects $\alpha_{s(i),j(i)}$ to be estimated. (In our CPS data, for example, there are 473,398 such fixed effects, i.e. 473,398 non-empty state $\times$ industry $\times$ occupation cells, to be estimated on 13,077,449 panel observations.) In practice we therefore form groups of jobs according to each job's treatment status. We choose the smallest possible number of groups such that all jobs in a given group are either all treated

[28]See Section 2.5.3 for more details.

or all not treated by a PECS ban in any given state at any given time.[29] These are simply the standard fixed effects to include in a difference-in-differences strategy, recognizing that our state-job variation is truly at the state and job-*group* level. Throughout our empirical work we therefore allow $j(i)$ to stand for job group rather than job.[30]

As in Section 2.6.1 above, we are interested in how PECS bans differentially impact different racial groups. Consequently, in practice we interact Equation 2.6.5 with a full set of race dummies, leading to the empirical specification:

$$y_{it} = \quad \alpha_{s(i),j(i),r(i)} + \gamma_{t,r(i)} + \sum_r \delta_r D_{s(i)} \times P_{s(i),t} \times T^l_{j(i),s(i)} + \epsilon_{it} \tag{2.6.6}$$

Like before, interacting all regressors with race yields the same coefficient estimates as separately estimating the effect of PECS by race, and so our identifying assumption only needs to hold separately for each race. Specifically, for example, estimating the parameter δ_B in Equation 2.6.6 requires that outcomes for blacks in treated jobs are on parallel trends to outcomes for blacks in untreated jobs (both within PECS states and in other states).

As in Section 2.6.1 above, we also estimate event-time versions of our job-level specifications to determine the validity of our parallel trends assumptions, and explore the path of treatment effects over time.

2.6.3 Duration Dependence

Our main outcome of interest is job-finding, i.e., how PECS bans affect unemployed individuals' probability of re-employment. It is well known that job-finding probabilities exhibit duration dependence, so we formally estimate a hazard model to account for how a PECS ban may affect both the probability of job-finding at a given unemployment duration and the composition of durations among the pool of unemployed. In particular, we specify a semi-parametric proportional hazards model of job-finding as in Han and Hausman (1990) or Meyer (1990), and show how it can incorporate our difference-in-differences strategy.

To begin, we model $\lambda_{i,t}(\tau)$, the probability of finding a job for person i, at time t, after being unemployed for a length of time τ, given individual characteristics X_i and an arbitrary set of fixed effects W_i, as:

$$\lambda_{i,t}(\tau) = \quad \lambda_0(\tau) \exp\left(W_i + \sum_r \beta_r 1_{r=r(i)} \times D_{s(i)} \times P_{s(i),t} + X_i'\beta_{x,r(i)}\right) \tag{2.6.7}$$

Note that the λ_0 term is fully non-parametric in τ, as in Cox (1972), while the proportional hazards assumption appears through the exponentiated term's non-dependence on τ. The

[29]In set-theoretic terms, that is to say our set of job groups is the meet of the job partitions generated by the PECS bans we study.

[30]For clarity, we emphasize that this also is true of the $j(i)$ notation used in our estimation of the transition probabilities that determine $T^E_{j(i),s(i)}$; see also footnote 25.

choice of the exponential functional form is standard, in order to model the hazard rate $\lambda_{i,t}(\tau)$ as nonnegative.

In order to bring this expression to the data we need to transform this continuous time hazard, $\lambda_{i,t}(\tau)$, into a discrete time hazard, $\lambda^d_{i,t}(\tau)$, defined as the probability of job-finding between $\tau - 1$ and τ conditional on being unemployed at time $\tau - 1$. Formally, $\lambda^d_{i,t}(\tau)$ is defined as:

$$\lambda^d_{i,t}(\tau) = \frac{\exp\left(\int_0^\tau \lambda_{i,t}(s)ds\right) - \exp\left(\int_0^{\tau-1} \lambda_{i,t}(s)ds\right)}{1 - \exp\left(\int_0^{\tau-1} \lambda_{i,t}(s)ds\right)} \tag{2.6.8}$$

Following Han and Hausman (1990) and Meyer (1990), we can work from the definitions in 2.6.7 and 2.6.8 to write the discrete time hazard in complementary-log-log form as:

$$\ln(-\ln(1 - \lambda^d_{i,t}(\tau))) = \alpha_\tau + W_i + \sum_r 1_{r=r(i)} \times D_{s(i)} \times P_{s(i),t} + X_i'\beta_{x,r(i)} \tag{2.6.9}$$

$$\alpha_\tau = \ln \int_{\tau-1}^{\tau} \lambda_0(s)ds \tag{2.6.10}$$

If we replace the arbitrary fixed effects W_i with those from the difference-in-differences specifications described in Sections 2.6.1 and 2.6.2, we can then rely on our earlier identifying assumptions to identify the parameters of interest, β_r. For example, our workhorse state-time difference-in-difference model in Equation 2.6.2 can be written in complementary-log-log form as

$$\begin{aligned}\ln(-\ln(1 - \lambda^d_{i,t}(\tau))) = \quad & \alpha_\tau + \alpha_{s(i),r(i)} + \gamma_{t,r(i)} + X_i'\beta_{x,r(i)} \\ & + \sum_r \delta_r 1_{r=r(i)} \times D_{s(i)} \times P_{s(i),t}\end{aligned} \tag{2.6.11}$$

This equation inherits the state-time difference-in-difference strategy's basic identifying assumption of parallel trends between PECS-ban states and non-PECS-ban states. In particular, we assume parallel trends in the complementary-log-log of discrete-time hazards. It can be shown in the derivation of Equation 2.6.9 that this assumption is equivalent to the (arguably more interpretable) assumption of parallel trends in log *continuous* time hazards. Given the nonnegativity of hazard rates, we view this log form as the most natural parallel trends assumption to make.

Note that we can also interact our treatment dummies with dummies for event time, i.e. κ_{st}, to generate event-time versions of any of these difference-in-difference hazard model specifications, analogous to Equation 2.6.4. For example, the event-time version of Equation

2.6.11 is,

$$\ln(-\ln(1-\lambda^d_{i,t}(\tau))) = \alpha_\tau + \alpha_{s(i),r(i)} + \gamma_{t,r(i)} + X_i'\beta_{x,r(i)} + \sum_k \sum_r \delta^k_r 1_{r=r(i)} \times D_{s(i)} \times P_{s(i),t} \times 1_{\kappa_{st}=k} \tag{2.6.12}$$

The interpretation of exponentiated event-time coefficients δ^k_r is then as event-time-specific hazard ratios; for example, $\exp(\delta^k_r)$ is the multiplicative effect of a PECS ban for race r in the k^{th} period after the implementation of a ban.

When it comes to estimation, we take two approaches depending on the aggregation level of our data. For individual-level data like the CPS, the parameters of Equation 2.6.11 can be estimated via maximum-likelihood (as detailed in Meyer (1990)). For aggregated data like the LEHD J2J, we use OLS where we plug in population-average job-finding rates for $\lambda^d_{i,t}(\tau)$ on the left-hand-side of Equation 2.6.11. As we detail in the next subsection, care needs to be taken with how this OLS estimator behaves in finite samples.

2.6.4 Hazard Model Estimation on Aggregate Data

We consider the problem of how to estimate the parameters of discrete-time hazard models such as Equation 2.6.11 when only population-average job-finding rates are observed, as in our LEHD J2J data, rather than individual job-finding outcomes, as in the CPS. Let $B_{s,r,t}(\tau)$ be the observed number of individuals finding a job, and $N_{s,r,t}(\tau)$ be the number of job-seekers, at unemployment duration τ for race r in state s time period t. Likewise define $b_{s,r,t}(\tau) = B_{s,r,t}(\tau)/N_{s,r,t}(\tau)$. We begin by noting that $B_{s,r,t}(\tau) \sim \text{Bin}(N_{s,r,t}(\tau), \lambda^d_{s,r,t}(\tau))$, where $\lambda^d_{s,r,t}(\tau)$ is as specified in Equation 2.6.9. For large $N_{s,r,t}(\tau)$, we therefore know $b_{s,r,t}(\tau) \xrightarrow{p} \lambda^d_{s,r,t}(\tau)$. So by the continuous mapping theorem, which shows that functions of random variable's limit to the function of the random variables limit, we can consistently estimate models such as Equation 2.6.11 on aggregate data, simply by plugging in $b_{s,r,t}(\tau)$ for $\lambda^d_{s,r,t}(\tau)$.

However in practice the number of unemployed individuals, $N_{s,r,t}(\tau)$, is of course finite. Because we model a nonlinear function of $\lambda^d_{s,r,t}(\tau)$ on the left-hand-side of Equation 2.6.11, our OLS estimator will exhibit some finite sample bias when we plug in $b_{s,r,t}(\tau)$ for $\lambda^d_{s,r,t}(\tau)$.

We use numerical integration to investigate the size of this bias. Specifically, we calculate as a function of $N_{s,r,t}(\tau)$ and $\lambda^d_{s,r,t}(\tau)$ the size of the expected bias ϵ in the dependent variable[31]:

$$\epsilon = \mathbb{E}\Big[\ln(-\ln(1-b))\Big] - \lambda^d_{s,r,t}(\tau) \tag{2.6.13}$$

$$b \sim \text{Bin}(N_{s,r,t}(\tau), \lambda^d_{s,r,t}(\tau)) \tag{2.6.14}$$

[31]Where Bin refers to the Binomial distribution

We find that ϵ is less than 0.1% of $\lambda^d_{s,r,t}(\tau)$ when $N_{s,r,t}(\tau) > 500$. So in practice when we estimate the parameters of models such as Equation 2.6.11 on the LEHD J2J, we exclude states in which any race group ever has fewer than 500 unemployed individuals at the unemployment duration that we study. This leads to the exclusion of data points from Idaho, Wyoming, Montana, North Dakota, and South Dakota.

For clarity we also reiterate that, as discussed in Section 2.5.2, we only estimate job-finding models in the LEHD J2J on a single length of unemployment duration τ, the intermediate category of "adjacent-quarter flows." Because the data are available at a quarterly frequency and all individuals in this duration category are newly unemployed in the past quarter, this is consistent with $B_{s,r,t}(\tau)$ being distributed $\text{Bin}(N_{s,r,t}(\tau), \lambda^d_{s,r,t}(\tau))$.

2.7 Results

In this section, we present our main results on the labor market effect of PECS bans. We focus on the two outcomes highlighted by the model in Section 2.4: job-finding rates and match quality for new hires, as indicated by new hires' subsequent rates of involuntary separation. For both analyses we also explore the validity of our identifying assumptions, and we probe the robustness of our results to relaxations of these identifying assumptions.

2.7.1 Job-Finding Rates

We begin our empirical work by asking how a PECS ban affects unemployed workers' job-finding rates. This question has the advantage of focusing on the most direct mechanism by which PECS affects labor market outcomes: as a screen for job applicants. This question has also been central in PECS-related policy debates.

As a first pass at this question, we estimate our basic proportional hazards model of job-finding described in Equation 2.6.11, using CPS data. Recall that our coefficients of interest in this specification are the race-specific treatment effects δ_r. Because this specification uses state-time variation, these coefficients capture the difference in differences in log hazard rates for race r in PECS-ban states relative to race r in non-ban states. As a concrete example, an estimate of $\delta_r = 0.01$ would indicate that race r's job-finding hazards are higher by a factor of one log point in PECS-ban states after the implementation of a PECS ban, relative to any contemporaneous change in race r's job-finding hazards in non-ban states.

To illustrate what patterns in the data generate our estimates of δ_r, we first show an event-time plot of how these log hazard ratios vary with event time κ. Specifically, we plot estimates of δ^k_r from estimating Equation 2.6.12, the event-time version of Equation 2.6.11, for years $k = -3, -2, \dots 2, 3$.[32] These estimates are shown in Figure 2. Note that the omitted

[32]Note that while our CPS data are at the monthly level, we aggregate CPS event time to the yearly level to improve precision. Concretely, for example, the dummy for $k = 1$ absorbs the 12 months immediately after the implementation of a PECS ban (inclusive of the month of implementation).

event time is $k = 0$, so all estimates are relative to the 12 months immediately prior to the implementation of a PECS ban.

Panel A of the figure shows estimates of δ_r^k for blacks ($r = B$). We see that black job-finding hazards in PECS-ban states are on parallel trends with non-ban states in the three years prior to implementing PECS bans, as indicated by δ_B^k estimates close to zero for $k \leq 0$. This helps validate our difference-in-differences strategy's identifying assumption. Then immediately after the implementation of a PECS ban, black job-finding hazards fall by 13 log points, and fall by an additional 7 log points in the second year after a PECS ban's implementation (for a total of 20 log points). Black job-finding hazards remain 21 log points lower in the third year after implementation. These event-time estimates help illustrate what generates the main coefficient of interest for blacks, δ_B, in our baseline hazard model in Equation 2.6.11: δ_B will be equal to a weighted average of δ_B^k for $k > 0$ minus a weighted average of δ_B^k for $k \leq 0$.[33]

Panels B and C of Figure 2 then show analogous estimates for Hispanics and whites, respectively. These plots also exhibit parallel trends in the three years prior to the implementation of PECS bans, albeit with perhaps a slight negative pre-trend for whites. On net we see little evidence of an effect in the three post-years for these groups. However, these estimates are somewhat imprecise, and especially for Hispanics we are unable to reject large positive or moderately negative effects. Indeed, for both Hispanics and whites the statistically significant uptick in job-finding in the first year after PECS bans' implementation may indicate an initial positive effect for these groups that later dissipates.

Having illustrated these patterns in our data, we now turn to our baseline estimates of δ_r. Table 6 reports these estimates in three rows for blacks, Hispanics, and whites respectively. Column (1) presents results from a specification corresponding to our event-time plots in Figure 2. Column (2) then adds our set of standard covariates: education groups; age groups; gender and marital status; urbanicity; and interactions between month-of-year and Census division (to capture possible seasonal effects).[34]

As expected given the patterns in Figure 2, Table 6's estimate for blacks in column (1) is roughly -16 log points. This estimate is essentially unchanged after adding covariates in column (2). And while the 95% confidence interval for this estimate is somewhat wide, we reject a null of no effect of PECS bans for blacks.

Our estimated effects for Hispanics and whites are smaller and statistically indistinguishable from zero, as expected given the patterns in the event-time plots. Our standard errors for whites are smaller than for Hispanics, in part reflecting the larger sample sizes available, and for whites we can reject a positive effect on job-finding of 5 log points or more.[35]

[33]These weights are approximately all equal, but the weights vary slightly with relative changes in PECS-ban states' black populations across years.

[34]Specifically, our age categories are 18-29, 30-39, 40-49, 50-61, and 62+; education categories are less than high school, GED, high school diploma (not GED), some college, and college or more; marital status is an indicator for married; and the definition of urbanicity is taken from the CPS documentation.

[35]We note, however, that our estimates in our later analysis using job-level variation are noisier, and do not allow us to similarly reject a a positive effect on job-finding of 5 log points or more; see Table 7.

With these baseline state-level estimates in hand, we next turn to using job-level variation to estimate the effects of PECS bans on job-finding. This primarily serves as a validation of our state-level results: if the effects we documented in Table 6 are indeed attributable to PECS bans, then we should see these effects in particular for jobs exposed to these bans. To do this, we extend the baseline proportional hazards model in Equation 2.6.9 to use our measures $T^{l}_{j(i),s(i)}$ of job-specific exposure to PECS bans, as defined in Section 2.5.3. Accordingly, when we substitute for Equation 2.6.9's generic fixed effects W_i, we use the *job*-level fixed effects developed in our baseline *job*-level difference-in-difference specification, Equation 2.6.6. Results for estimating several versions of this specification are shown in Table 7, while corresponding event-time plots are shown in Figure 3.

The first two columns of Table 7 report our estimates when we use our expected-job exposure measure, $T^{E}_{j(i),s(i)}$. These two columns respectively exclude and then include individual-level covariates X_i. The next two columns then repeat this analysis using our past-job exposure measure, $T^{P}_{j(i),s(i)}$. Estimates for blacks are larger than our state-level estimates from Table 6, indicating that the patterns observed in our state-level analysis are indeed driven by jobs covered by PECS bans. Indeed, if one scales up the point-estimates from our state-level analysis Table 6 by the share of unemployed blacks who previously worked in covered jobs in PECS ban states (as reported in the summary statistics in Table 3), the resulting estimate is .238, not far from our job-level estimate of .209 in Column (1) of 7. This similarity provides strong evidence that our results are driven by individual exposure to PECS bans rather than alternative explanations.

Event-time plots corresponding to column (1) of Table 7 are shown in Figure 3. Estimates for blacks are shown in Panel A and are remarkably similar to those in Figure 2: little or no apparent pre-trend, and a persistent drop in job-finding hazards of about 20 log points after the enactment of a PECS ban. The plots for Hispanics and whites in Panels B and C are also similar to those shown previously in Figure 2.

On net, we view our job-level results as corroboration that our earlier state-level results are attributable to PECS bans.

2.7.2 Job Separation Rates

In this section, we explore how a PECS ban affects newly hired workers' rates of subsequent involuntary separation, which is a readily available measure of new hires' match quality. The answer to this question provides information about how PECS is used as screening tool, and can shed light on some possible efficiency gains or costs from the use of PECS. The effect of PECS on new-hire match quality has also played a central role in policy debates about PECS.

Our dependent variable throughout this section is an indicator for any subsequent involuntary separation after being newly hired.[36] Given the rotating-panel structure of our

[36]This choice may be particularly apt in the case of PECS, as PECS is seen as a screen for behaviors such as theft (Society for Human Resource Management (2012)) that are likely to lead to involuntary termination.

CPS data (as discussed in Section 2.5.1), we observe involuntary separation for new hires at horizons ranging from 1 to 14 months, making this a short- to medium-run measure of separation. Individual observations are assigned to a time period t based on their *hire* date, and each newly hired individual only appears once in our estimating sample, at the point of hire. Accordingly our empirical strategy does not need to account for any dependence of separation rates on the duration of employment, and we use linear probability models estimated via OLS.

Similar to our job-finding specifications, we begin our analysis using difference-in-differences models fully interacted with race, as in Equation 2.6.2. Figure 4 reports event-time versions of this baseline model. Starting with the event-time analysis for blacks in Panel A, we see that separation rates for new hires fluctuated prior to the enactment of PECS bans, but were on a downward trend in the two years immediately preceding the ban. Black separation rates then increase immediately after PECS bans go into effect, although this increase is eroded by a decline in the third year.

The increase in black new hires' separation rates stands in contrast to the patterns seen for Hispanics and whites in Panels B and C of the figure. Unlike black new hires, Hispanic new hires exhibit a strong negative pre-trend, which makes the near-zero point estimates in post years somewhat difficult to interpret. As we discuss below in section 2.7.3, we explore possible strategies to control for this pre-trend, although we ultimately find that a PECS ban's effect on Hispanic new hires' match quality is inconclusive.

Meanwhile we see a slight but precisely estimated decrease in white new hires' separation rates in Panel C: after exhibiting parallel trends in pre-years, separation rates fall by roughly 1 percentage point in the first two years after a PECS ban's implementation, and then by 4 percentage points in the third year.

The first column of Table 8 reports estimates of Equation 2.6.2 corresponding to these event-time plots. The results confirm our visual inspection of Figure 4, with Column (1) showing a precisely estimated 2.7 percentage-point rise in involuntary separation rates for black new hires, and a similar in magnitude decline in separation rates for white new hires. Given the evidence seen in Figure 4 of strong pre-trends for Hispanics, we defer our interpretation of the point estimates for Hispanics until we later control for pre-trends more explicitly. Column (2) shows that adding our standard, individual-level covariates X_i has little impact on the results.

In Columns (3) and (4) of the table, we then take advantage of job-level variation based on whether new hires' jobs are covered by, or exempted from, their states' PECS bans. We estimate our baseline job-level difference-in-difference model, Equation 2.6.6, using our current-job exposure measure $T^C_{j(i),s(i)}$ (i.e, the job into which new hires are newly hired). As with our earlier job-finding results, the use of job-level variation also increases the magnitude of the estimated impact on black separation rates, which rises from 2.7 percentage points in Columns (1) and (2) to 4.1 percentage points in Columns (3) and (4). However, also note that the use of job-level variation leads to little quantitative change in the estimated coefficients for whites and Hispanics. We revisit this fact later in this section.

One potential concern with the results in Table 8 is that we could spuriously estimate a large effect of PECS bans if these bans coincide by chance with other labor market disruptions. To address this concern, we re-estimate the four separation regressions from Table 8 on a placebo sample: long-tenure employees rather than new hires. Unfortunately the CPS does not report employment tenure, so we define this "long-tenure" sample as all individuals in our panel who are never observed as unemployed in any preceding sample month. As compared to the sample of new hires, this sample is less likely to have been hired when a PECS ban was in effect, but arguably is equally exposed to broader labor-market disruptions, such as plant closings and sectoral change.

Table 9 reports results of these placebo regressions. These results do not suggest that simultaneous labor market shocks are biasing our results for blacks, as none of the black estimates in the placebo sample is statistically significant at even the 10 percent level, and all estimates for blacks have a different sign than before. However, note that our placebo results for Hispanics and whites are qualitatively similar to our results on the new-hires sample. The Hispanic and white placebo results are also comparable in magnitude to results in the new-hires sample, when considered relative to the mean of the dependent variable in each sample (see Table 4).

We thus have two reasons for confidence in our results that PECS have decreased new black hires' match quality, and also two reasons for caution in our corresponding estimates for Hispanics and whites. First, for blacks the estimated effect on new hires' separation rates in Table 8 is higher when we use job-level variation than when we use state-level variation, indicating that the state-level results are indeed driven by jobs covered by PECS bans. However, as we noted, the same is not true of Hispanics and whites. Second, these effects for blacks do not appear in a placebo sample of long-tenure employees, while they do appear in the placebo sample for Hispanics and whites. We conclude that the job-separation patterns documented for Hispanics and whites are not necessarily attributable to PECS bans. In principle, it is possible that the effect seen for all white workers, regardless of tenure or job exemption status, is an indirect, equilibrium outcome resulting from PECS' direct effects on blacks, but such general equilibrium analyses are not our focus in this paper, nor is our empirical strategy well suited to investigate them.

2.7.3 Robustness

We now revisit our earlier job-finding and job-separation results and explore their robustness to other identifying assumptions, more demanding specifications, and in the case of job-finding, estimation on a supplementary dataset built from administrative data.

We begin by studying job-finding in our supplementary dataset, the LEHD J2J data described earlier in Section 2.5.2. Although the LEHD J2J does not provide the rich individual-level demographics or information on spell length as the CPS does, the fact that it is aggregated from administrative data on the near-universe of state unemployment-insurance records makes it a valuable source of additional information. We follow the empirical strategy detailed in Section 2.6.4, where we estimate a difference-in-differences model for the complementary-log-log of observed job-finding rates for newly unemployed job-seekers at the state-race-time level.

Figure 5 reports estimates from event-time versions of Equation 2.6.12 using the LEHD J2J, where we plug in observed job-finding rates $b_{s,r,t}(\tau)$ for $\lambda^{d}_{s,r,t}(\tau)$ and estimate via OLS.[37] Unlike Figure 2, where we showed analogous event-time plots for our CPS data, all three panels in Figure 5 suggest the existence of downward relative trends in PECS states prior to PECS bans' implementation. After PECS bans went into effect, we see that the decline in black job-finding rates accelerated, whereas job-finding rates for both Hispanics and whites rise for a period and then fall slightly.

The existence of a downward trend in black job-finding rates in the LEHD J2J, but no trend in the CPS, is somewhat puzzling. One possibility is that, because the LEHD J2J includes some individuals who made job to job transitions not via unemployment, but voluntarily took time off from work between jobs, the downward trend for the LEHD J2J could be driven by these on-the-job switchers. These individuals are not ever counted as unemployed in our CPS analysis.

The divergent pre-trends in states enacting PECS bans in the LEHD J2J data suggest that we explore the robustness of these results to alternative identifying assumptions. To begin, Table 10, Column (1) shows coefficient estimates that correspond to the LEHD J2J event-time plots in Figure 5. Column (2) adds controls for linear trends at the state-race level to explore sensitivity to the baseline difference-in-difference estimator's parallel trends assumption. Column (3) adds state-time fixed effects to Column (1), creating a more-demanding triple-difference estimator as in Equation 2.6.3. Finally, Column (4) adds state-race linear trends to Column (3). Recall that the triple-difference estimator estimates the effect of PECS bans on the *difference* between black and white (or between Hispanic and white) job-finding rates. These triple-difference specifications require the weaker identifying assumption that the difference between black and white (or between Hispanic and white) job-finding rates would have exhibited common trends between states banning and not banning PECS in the absence of PECS bans.[38]

The results in Column (2) of the table change markedly, all becoming more positive and less precise relative to the estimates in Column (1). This is to be expected, given the large pre-trends for all three races apparent in the LEHD J2J figures. When we add state-time fixed effects in Column (3), the estimates become more similar to our baseline estimates in Column (1), with the estimated effect for blacks being around -4.4 log-points, although this estimate is imprecise. Finally, when we add state-race linear trends in Column (4), we see little change in the point-estimates for blacks relative to Column (3), while the Hispanic estimate increases in magnitude. This suggests the presence of small or no pre-trends in

[37]This plug-in estimator was described in more detail in Section 2.6.4.

[38]We re-emphasize that these estimates only estimate the impact of PECS on blacks or Hispanics *relative* to the impact for whites. Only under the further restriction that PECS not impact job-finding rates for whites do these estimates represent the total impact of PECS for blacks or Hispanics.

Column (3) for blacks, but possible pre-trends for Hispanics.

We graphically validate the absence of pre-trends for blacks in the specification from Column (3), using event-time estimates corresponding to this triple-difference estimator. These estimates are shown in Figure 6. Looking at Panel A, which shows the evolution of black job-finding hazards compared to white-job finding hazards in PECS-ban states compared to non-ban states, we confirm there was little discernible trend prior to PECS bans' implementation. After PECS bans are introduced, black job-finding rates steadily decline relative to white job-finding rates. Turning to Panel B, we see that there may be a slight downward trend in job-finding rates for Hispanics that accelerates slightly after PECS bans were introduced. This explains the sensitivity of the estimates for Hispanics in Table 10 to the inclusion of state-race linear trends.

Overall, these results lend additional credence to our baseline estimates that PECS bans have a significant negative effect on job-finding rates for blacks. Three of the four estimates of the effect of PECS on black job-finding in the LEHD J2J are negative and economically large, although they are consistently smaller in magnitude than our baseline estimates from the CPS, varying from one-half to one-third of our CPS estimates. Furthermore, the most demanding specifications in Column (4) are both economically large and precisely estimated. However, these results also illustrate the continuing difficulty of estimating the effect of PECS bans on Hispanics. Because our estimates for Hispanics are volatile across specifications and datasets, we conclude that the effect of PECS bans on Hispanic labor market outcomes should be left for later work, in different data or with a different identification strategy.

For completeness, we also replicate the four columns of our LEHD J2J robustness table in our CPS data. These results are shown in Table 11. Column (1) repeats our baseline analysis of CPS job-finding from Column (1) of Table 6. Column (2) then adds, as our LEHD J2J results, state-race linear trends to our baseline CPS specification in Equation 2.6.11. We see that adding state-race linear trends leaves the point estimate of the effect of PECS on job-finding unchanged, although it does substantially increase the standard errors. The estimate for Hispanics increases slightly, while the estimate for whites increases notably. Moving to Column (3), where we add state-time fixed effects Column (1), turning the specification into a triple-difference estimator, we see that the estimated effect of PECS on blacks is reduced and made noisier, but is still economically large. The estimated effect for Hispanics is indistinguishable from the estimate in Column (1). Figure 7 reports the event studies corresponding to Column (3), showing relatively little pre-trend for blacks and a steady decline in job-finding rates after PECS bans go into effect. Conversely, Hispanic job-finding rates jump up in the first two years after a PECS ban goes into effect, but then decline to around their previous levels. Finally, in Column (4) we add state-race specific linear trends to the triple-difference specification in Column (3). This change roughly triples the estimated effect for blacks to over 35 log-points, while the estimate for Hispanics is now near 0. The patterns in this table are broadly similar to those we observe in the LEHD J2J, further validating our headline CPS results on job-finding.

Finally, in Table 12 we perform the same robustness analysis for the effect of PECS on separation rates for new hires. This analysis is particularly important in light of the pre-trends evident for blacks and Hispanics in Figure 4. Beginning with Column (2), we see that adding state-race linear trends nearly doubles the magnitude of the point estimates for blacks (although it remains imprecisely estimated), while the estimated magnitudes for Hispanics and whites are sharply reduced, suggesting that the estimates for them are driven by pre-existing trends. This finding is confirmed when we turn to Column (3), which shows that adding state-time effects to Column (1) increases the estimate for blacks considerably compared to our baseline specification in Column (1), while it decreases the estimate for Hispanics to less than half its previous size. Adding state-race linear trends in Column (4) further reduces the Hispanic estimate, while increasing the estimate for blacks by another fifty percent. These changes should not be surprising given the substantial pre-trends visible for Hispanics in the event-time version of Column (3), which is shown in Figure 8. Overall, Table 12 confirms our finding that PECS bans are associated with increases in separation rates for black new hires, while casting doubt on any relationship between PECS bans and separation rates for new hires for whites and Hispanics. This is the same conclusion reached in our earlier analysis of a placebo sample and of job-level variation, in Section 2.7.2.

2.8 Discussion

We have documented that PECS bans were associated with a marked decrease in job-finding hazards for blacks, and also a decrease in black new hires' match quality as measured by subsequent separation rates. We found little evidence of an impact for whites, and inconsistent and imprecise estimates for Hispanics. In this section, we discuss how economically large our estimated effects for blacks are, and we compare these estimates to estimates of other, previously studied policies' effects on job-finding hazards.

In our preferred specification using state-time variation, Equation 2.6.11, we estimate that PECS reduced the job-finding hazard for blacks by 15.6 and 7.4 log-points in the CPS and LEHD J2J respectively. To get a sense for the magnitude of these effects in absolute terms, we compare them to the baseline job-finding rates for blacks in the LEHD J2J data, as reported in Table 5.[39] This comparison implies our estimates translate into 3.7 and 1.7 percentage-point reductions in the probability of black job-seekers finding a new job within a quarter of job-loss.

We now attempt to gage how these magnitudes compare to those of other policies studied in the literature. Meyer (1990)'s study of unemployment insurance finds that a 10 percent increase in the size of unemployment benefits results in a roughly 8.8 percent decline in the job-finding hazard. Interpreting our results in light of this finding, the effect of PECS

[39]We estimate similar baseline job-finding rates in the CPS. However, given the CPS sample structure, estimating these baseline rates in the CPS requires formally estimating a hazard model, and *baseline* hazards are only estimated consistently in our specified model under strong conditions. Among these strong conditions is the unrealistic requirement that no relevant regressors are omitted from the model, even if those regressors are independent of our primary regressor of interest, the indicator for PECS ban coverage (Lancaster (1979)).

hiring bans on black job-finding rates are equivalent to about an 18 percent reduction in unemployment benefits using our estimates from the CPS, and an 8 percent reduction in unemployment benefits using our estimates from the LEHD J2J. In more recent work using data from the Austrian unemployment insurance system, Card et al. (2007) find that eligibility for two months of severance pay results in a roughly 12 percent reduction in the job-finding rate, while extending unemployment insurance benefits from 20 to 30 weeks reduces job finding hazards by 6 to 9 percent. These estimates are similar in magnitude to our estimates of the impact of PECS. Hence we find that our estimates of the effect of PECS bans on black job-finding hazards are comparable in magnitude to quite large interventions.

Given that PECS bans primarily affect labor demand, we can also compare the estimated effect of PECS to demand-side factors. To do so, we need to convert our estimates of the effects of PECS on job-finding hazards to estimates of the effect of PECS on black *employment rates.* A complete analysis of this question would require using the baseline hazard estimates and data on the number and characteristics of individuals entering employment between 2007 and 2015 to compute counterfactual employment rates. Instead, we perform a back of the envelope calculation. We assume that the baseline hazard was equal to the mean job-finding rate for PECS states in the sample. We can then combine that figure with the mean job-separation rate for a given race to compute the change in the steady-state employment rate for the given group caused by PECS bans.[40] Using this approach, we estimate that PECS reduced the steady-state black employment rate by 0.5 to 1.3 percentage points in states banning PECS. Combining these calculations with the elasticity of labor demand estimate of $-.246$ from Lichter et al. (2014)'s meta-analysis, the effects of PECS bans are equivalent to the employment declines resulting from a 3 to 9% increase in wages for black workers. This is a modest, but non-trivial wage change.

Is it reasonable that restrictions on the use of information like PECS in the hiring process can have such a large impact on job-finding rates? Other evidence from the literature suggests yes. Studying the effect of the usage of credit information in hiring in Sweden, Bos et al. (forthcoming) find that the removal of information on past defaults from credit reports results in a 2-3 percentage point increase in employment rates for affected individuals in the year after the past default information removal. In related work, Wozniak (2015) finds that laws discouraging or encouraging the use of drug-testing in the hiring process have a 7 to 30 percent effect of black employment levels in affected industries. In work most closely resembling ours in empirical strategy, time-period, and policy, Doleac and Hansen (2017) find that Ban-the-Box polices reduce the employment of low-skilled black workers by 3.5 percentage points.[41] All three of these papers suggest that regulations of information used in the hiring process can have economically large impacts on employment outcomes.

[40] The steady-state employment rate is equal to $lf_r \times \frac{f_r}{f_r+s_r}$ where f_r is the job-finding race for race r, s_r is the job-separation rate for race r, and lf_r is the labor-force participation rate for race r.

[41] Given that criminal records may be a more important labor market signal than credit scores, it seems reasonable that Doleac and Hansen (2017)'s estimated effects of criminal record bans are 3-7 times as large as our estimates.

2.9 Conclusions

In this paper, we estimate difference-in-difference models of the effect of PECS bans on labor market outcomes by race. Under the assumption that different racial groups would have common trends in different states in the absence of PECS bans, our estimates suggest two broad sets of findings. First, using both the CPS and LEHD J2J data, we find that PECS bans lead to reductions in black job-finding hazards. These estimates vary with the exact specification, but are large in magnitude, with our preferred estimate implying that PECS bans decrease black job-finding hazards in PECS ban states by 16 log-points, or roughly 3.7 percentage points at a quarterly horizon. We find less conclusive evidence for whites and Hispanics.

Second, we find that separation rates for newly hired blacks rise after PECS bans are enacted, while Hispanic and white separation rates appear to change little. This finding is consistent with PECS bans reducing the match quality of newly hired black job applicants – that is, more high-match-quality applicants are rejected and more low-match-quality applicants are hired. However, the presence of pre-trends in black separation rates prior to PECS bans' enactment makes us more cautious about this conclusion than our conclusions about black job-finding rates. Whereas results from our attempts to control for these pre-trends are consistent with PECS having a causal effect on black separation rates, these controls also make our estimates imprecise.

We submit both of these main results to a range of robustness tests. Using rich, job-level variation generated by these bans' exemptions for various occupations and industries, we find that our estimated effects are more pronounced for covered rather than exempted jobs, consistent with our results being attributable to PECS bans. We also find that our separation results for blacks do not appear in a placebo sample of long-tenure workers, who were more likely to have been hired before PECS bans were implemented. We also use event-time plots to test our difference-in-difference estimators' key identifying assumptions. We find evidence in the CPS that black job-finding rates were indeed on parallel trends between PECS-ban states and non-ban states prior to the passage of PECS bans. On the other hand, when we find evidence of non-negligible pre-trends, as we do for black job-finding rates in our LEHD J2J data, we show our results are generally robust to various strategies that control for these pre-trends, including a demanding triple-difference estimator.

Broadly, our results suggest that PECS bans are associated with worsening labor market outcomes for unemployed blacks, and no consistent pattern for whites or Hispanics. While a range of labor market phenomena may contribute to these results, we show how to make sense of these results in a statistical discrimination model in which credit report data, moreso for blacks than for other groups, send a high-precision signal relative to the precision of employers' priors. This conceptual framework sheds light both on the role of PECS in labor market screening, and on the "soft" information available in credit report data more generally. Along with Ballance et al. (2017) and Bos et al. (forthcoming), these are some of the first results to show that credit report data may contain information valuable not just in credit markets, but in non-credit markets as well.

However, the imprecision and inconsistency of our estimates for whites and Hispanics does not rule out the possibility that PECS bans substantially affected average labor market outcomes for individuals in these groups as well. The wide confidence intervals for blacks also include both quite large and modest magnitudes. Furthermore, these aggregate results may disguise substantially heterogeneous effects within race. In particular, even if PECS bans decrease average job-finding rates for blacks, they may have actually *increased* job-finding rates for blacks with particularly unfavorable credit reports. Friedberg et al. (2016) finds evidence that PECS bans help individuals with low credit scores more broadly, but there is not yet evidence on how PECS bans affected blacks with low credit scores.[42]

More broadly, our results do not provide any information on how PECS affects an important subpopulation of interest: highly indebted households that are more likely to become delinquent on debts after job loss, and therefore may have more protracted unemployment spells when employers screen using PECS. The ability of PECS to thus prevent a particular kind of long-term unemployment is a promising area for future research.

[42]In the related setting of Ban-the-Box policies, Agan and Starr (2016) find precisely this pattern: overall blacks are hurt, but blacks with criminal records are helped.

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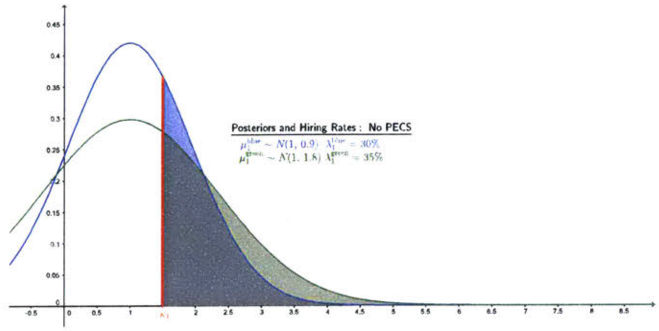
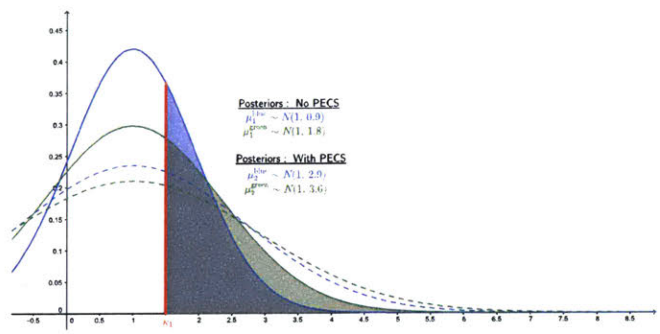
Figure 2-1: Illustrating the Effects of Pre-Employment Credit Screening (PECS)

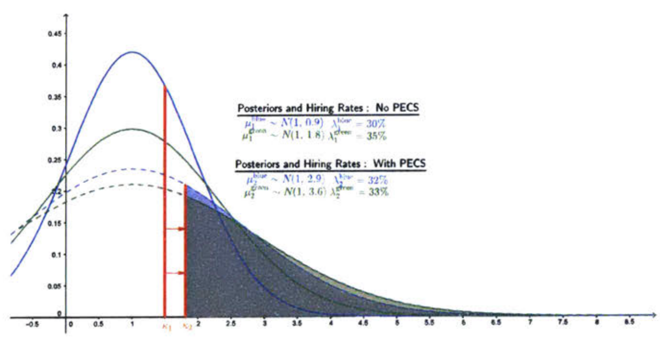
(a) Hiring without PECS



(b) Change in Employer Posteriors Post-PECS Introduction



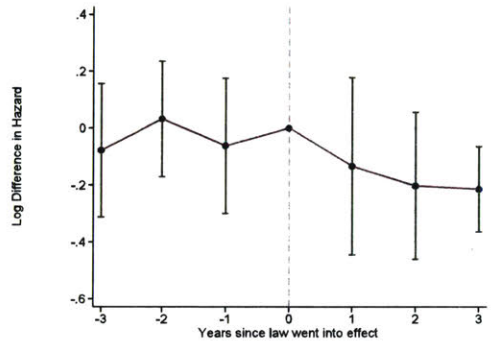
(c) Change in Hiring Threshold Post-PECS Introduction



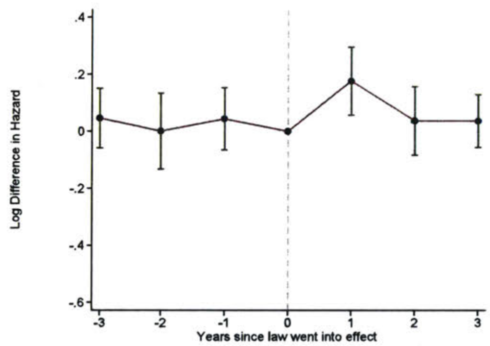
Notes: This figure graphically illustrates the statistical discrimination model described in Section 2.3. We apply the model to the parameter values reported in the figure for two groups of job-seekers: "blue" workers and "green" workers, who have identically distributed productivities but who differ in how precisely they are screened by employers' available screening tools. Panel A shows the distribution of an employer's posteriors for green and blue workers when employers do not have access to Pre-Employment Credit Screening (PECS), and the resulting hiring threshold κ_1. The greater variance of the population distribution of posteriors μ_1 for green workers reflects the greater precision of non-PECS screening tools for this group. Panel B then shows the change in the population distribution of posteriors when employers gain access to PECS. The greater increase in the variance of these posteriors, μ_2, for the blue group than for the green reflects that PECS is a relatively precise screening tool for this group. Panel C then shows how the employer's hiring threshold shifts to ensure market-clearing for both groups after the introduction of PECS, and how this leads to an increase (resp. decrease) in the hiring rate for the group for which PECS provided relatively precise (resp. imprecise) signals.

Figure 2-2: Event-Time Analysis of the Effect of PECS on Job-Finding: CPS State-Race FE, Time-Race FE

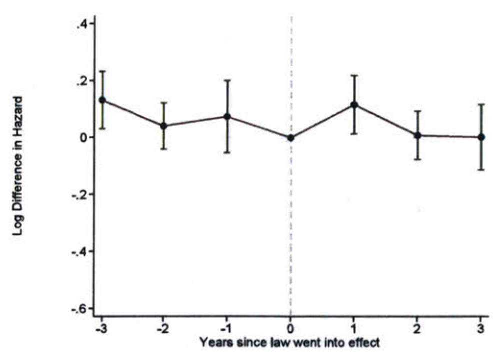
(a) Blacks



(b) Hispanics



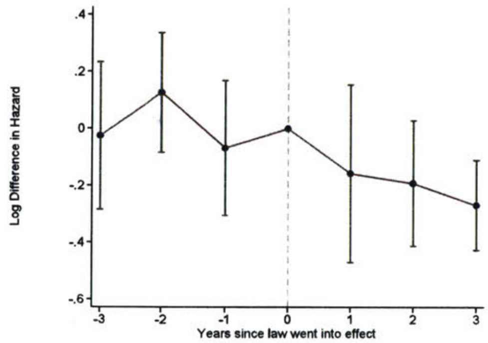
(c) Whites



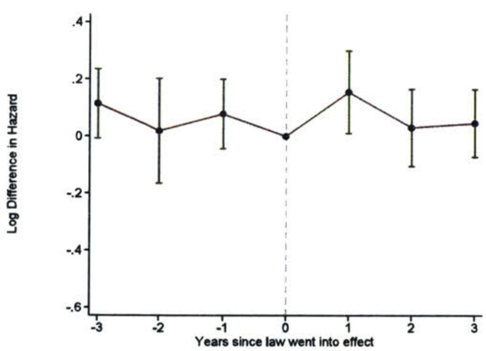
Notes: **This figure shows the results of an event-time analysis of the difference in job-finding for newly unemployed individuals between states banning and not banning Pre-Employment Credit Screening (PECS) before and after the PECS bans went into effect. Each panel shows results for a different race. The reported coefficients come from estimating via MLE a version of the proportional hazards model in Equation 2.5.12 where we interact an indicator for being covered by a PECS ban, $D_{s(i),t}$, with indicators for event time, κ_{st}. Event time is defined as the calendar year and month, t, minus the year and month that a PECS ban took effect in state s. To improve precision we pool twelve months of event-time dummies into year dummies. The model also includes time-race and state-race fixed effects. The sample is restricted to balanced event years common to all PECS-ban states. Microdata on individual unemployment and job-finding come from the Current Population Survey (US Census Bureau (2015a)). Error bars show 95% confidence intervals generated from standard errors clustered at the state level.**

Figure 2-3: Event-Time Analysis of the Effect of PECS on Job-Finding State-Job-Race FE, Time-Race FE

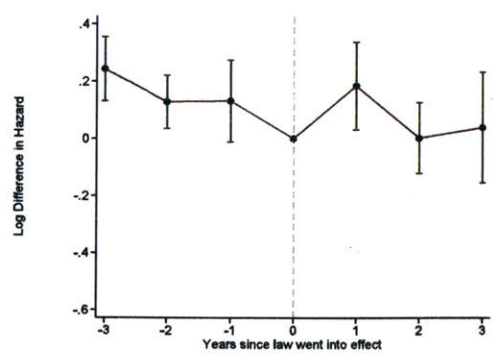
(a) Blacks



(b) Hispanics



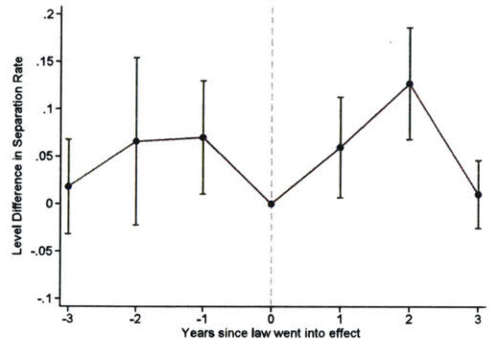
(c) Whites



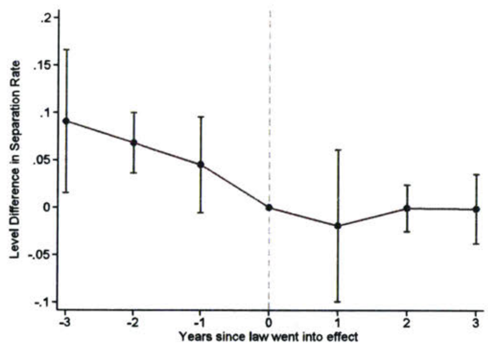
Notes: **This figure shows the results of an event-time analysis of the difference in job-finding for newly unemployed individuals between states banning and not banning Pre-Employment Credit Screening (PECS) before and after the PECS bans went into effect. Each panel shows results for a different race. The reported coefficients come from estimating with MLE a version of the proportional hazards model in Equation 2.5.12, where we use state-job-race fixed effects in lieu of state-race fixed effects, and where we interact an indicator for being covered by a PECS ban with the expected probability of being in a PECS covered job (given the unemployed workers previous job $j(i)$), $D_{s(i),t} \times \Pi_{j(i),s(i)}$, with indicators for event time, κ_{st}. Event time is defined as the calendar year and month, t, minus the year and month that a PECS ban took effect in state s. To improve precision we pool twelve months of event-time dummies into year dummies. The model also includes time-race and state-job-race fixed effects. The sample is restricted to balanced event years common to all PECS-ban states. Microdata on individual unemployment and job-finding come from the Current Population Survey (US Census Bureau (2015a)). Error bars show 95% confidence intervals generated from standard errors clustered at the state level.**

Figure 2-4: Event-Time Analysis of the Effect of PECS on Separations: New Hires State-Race FE, Time-Race FE

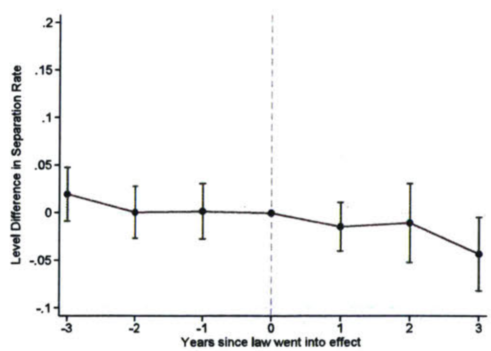
(a) Blacks



(b) Hispanics



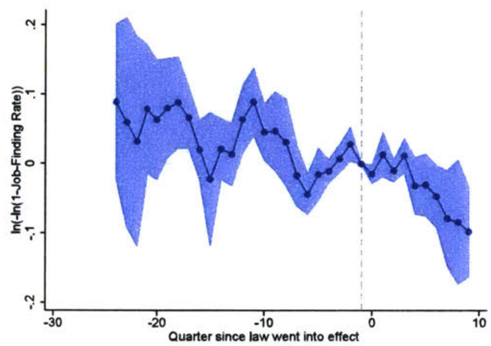
(c) Whites



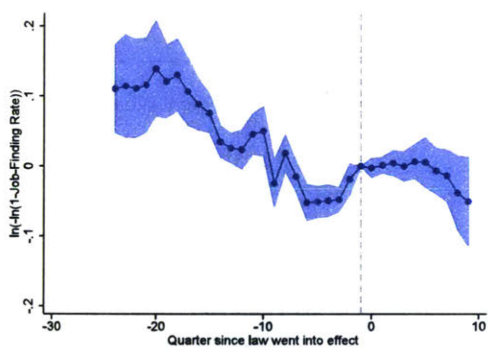
Notes: This figure shows the results of an event-time analysis of the difference in involuntary separation rates for workers newly hired out of unemployment between states banning and not banning Pre-Employment Credit Screening (PECS) before and after the PECS bans went into effect. Each panel shows results for a different race. The reported coefficients come from estimating a modified linear probability model of Equation 2.5.2 where we interact an indicator for being covered by a PECS ban, $D_{s(i),t}$, with indicators for event time, κ_{st}. Event time is defined as the calendar year and month, t, minus the year and month that a PECS ban took effect in state s. To improve precision we pool twelve months of event-time dummies into year dummies. The model also includes time-race, state-race, and state-time fixed effects. The sample is restricted to balanced event years common to all PECS-ban states. Microdata on individual unemployment and involuntary separation rates for new hires come from the Current Population Survey (US Census Bureau (2015a)). Error bars show 95% confidence intervals generated from standard errors clustered at the state level.

Figure 2-5: Event-Time Analysis of the Effect of PECS on Job-Finding: LEHD J2J State-Race FE, Time-Race FE

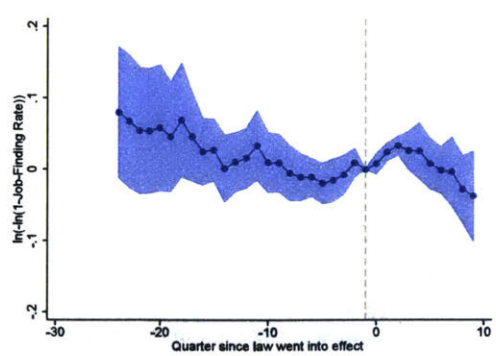
(a) Blacks



(b) Hispanics



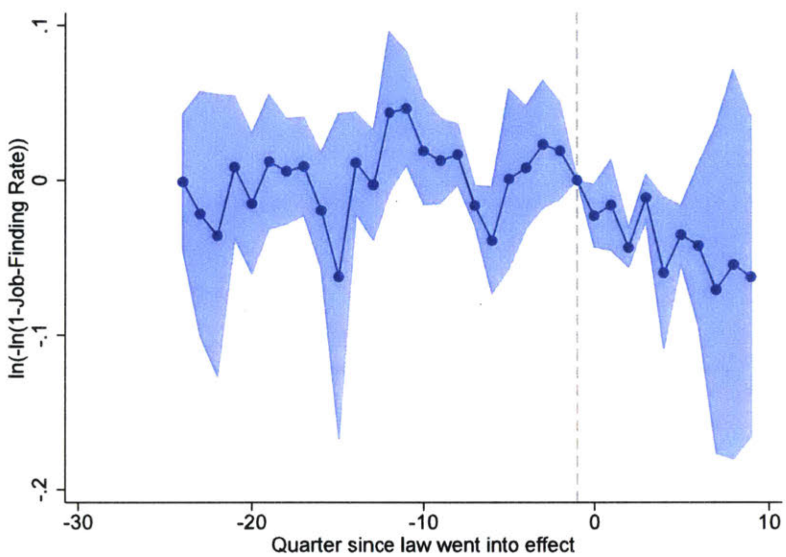
(c) Whites



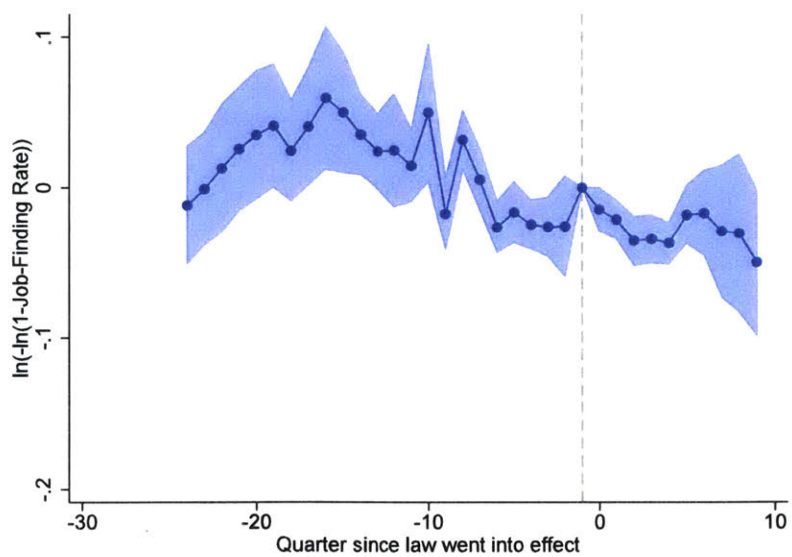
Notes: **This figure shows the results of an event-time analysis of the difference in the complementary log-log of the average job-finding rate (i.e. $\ln(-\ln(1-\text{job-finding rate}))$) between states banning and not banning Pre-Employment Credit Screening (PECS) before and after the PECS bans went into effect. The reported coefficients come from estimating a version of Equation 2.5.2 where we interact an indicator for being covered by a PECS ban, $D_{s(i),t}$, with indicators for event time, κ_{st}. Event time is defined as the calendar year-quarter, t, minus the year-quarter that a PECS ban took effect in state s. The model also includes year-quarter-race and state-race fixed effects. Regressions are weighted by the number of individuals of a given race who separated from their jobs in state s in year-quarter t. The sample is restricted to balanced event years common to all PECS-ban states. Data on job-finding rates for workers who separate from their main jobs come from the Longitudinal Employer-Household Dynamics Job-to-Job Flows data (LEHD J2J) (US Census Bureau (2015b)). Error bars show 95% confidence intervals generated from standard errors clustered at the state level.**

Figure 2-6: Event-Time Analysis of the Effect of PECS on Job-Finding: LEHD J2J State-Race FE, Time-Race FE, Time-State FE

(a) Blacks



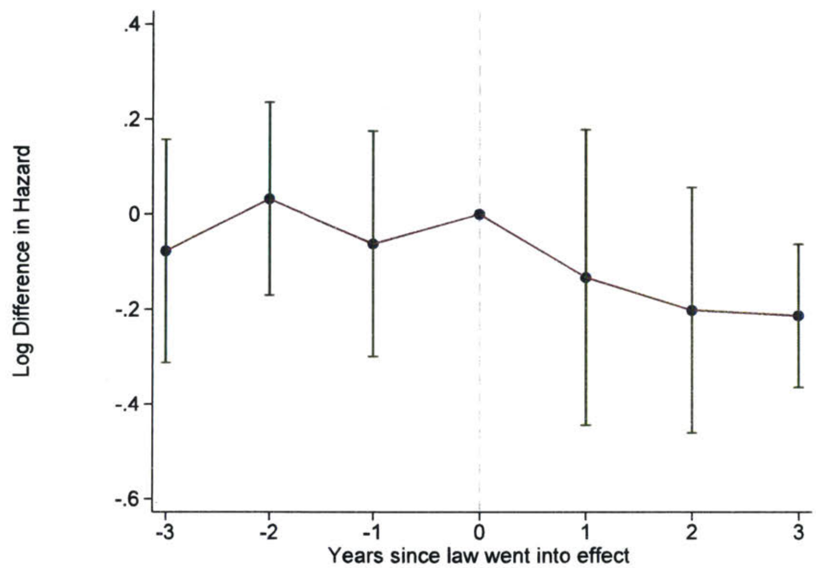
(b) Hispanics



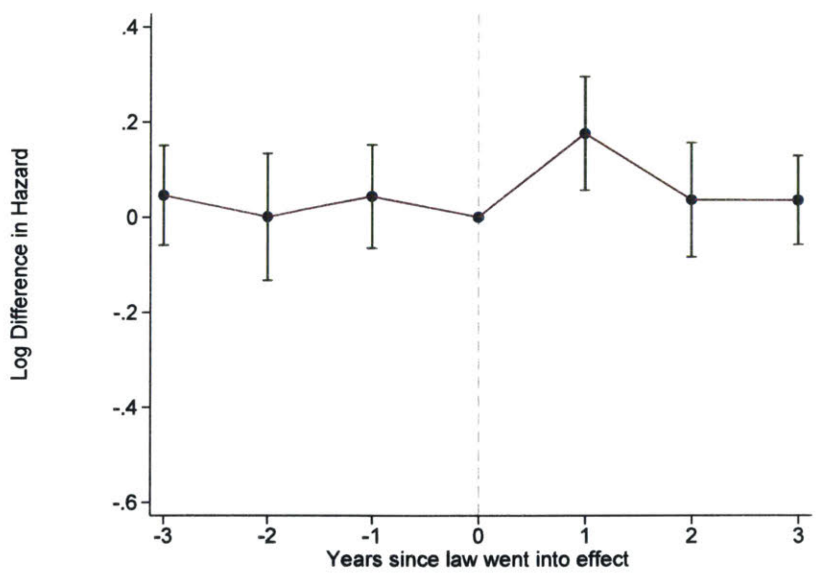
Notes: This figure shows the results of an event-time analysis of the difference in the complementary log-log of the average job-finding rate (i.e. $\ln(-\ln(1-$ job-finding rate$)))$ between states banning and not banning Pre-Employment Credit Screening (PECS) before and after the PECS bans went into effect. The reported coefficients come from estimating a version of Equation 2.5.3 where we interact an indicator for being covered by a PECS ban, $D_{s(i),t}$, with indicators for event time, κ_{st}. Event time is defined as the calendar year-quarter, t, minus the year-quarter that a PECS ban took effect in state s. The model also includes year-quarter-race, state-race, and year-quarter-state fixed effects. The inclusion of state-quarter-year fixed effects means the reported coefficients should be interpreted as the difference between PECS-banning states and others states in the *difference* in the job-finding rate between blacks or Hispanics and whites. Regressions are by the number of individuals of a given race who separated from their jobs in state s in year-quarter t. The sample is restricted to balanced event years common to all PECS-ban states. Data on job-finding rates for workers who separate from their main jobs come from the Longitudinal Employer-Household Dynamics Job-to-Job Flows data (LEHD J2J) (US Census Bureau (2015b)). Error bars show 95% confidence intervals generated from standard errors clustered at the state-race level.

Figure 2-7: Event-Time Analysis of the Effect of PECS on Job-Finding: CPS State-Race FE, Time-Race FE, Time-State FE

(a) Blacks



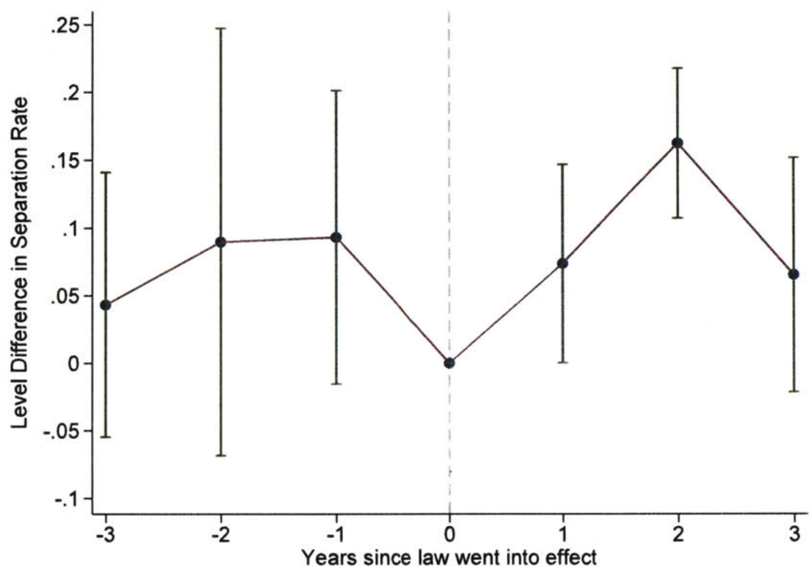
(b) Hispanics



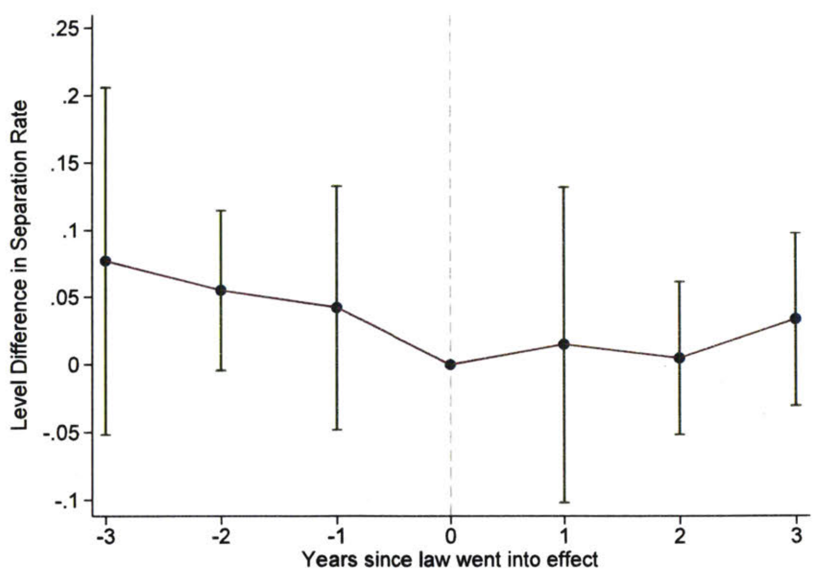
Notes: This figure shows the results of an event-time analysis of the difference in job-finding for newly unemployed black and Hispanic individuals between states banning and not banning Pre-Employment Credit Screening (PECS) before and after the PECS bans went into effect. Each panel shows results for a different race. The reported coefficients come from estimating with MLE a version of the proportional hazards model in Equation 2.5.12 where we interact an indicator for being covered by a PECS ban, $D_{s(i),t}$, with indicators for event time, κ_{st}, and where we add state-time fixed effects to the original specification. Event time is defined as the calendar year and month, t, minus the year and month that a PECS ban took effect in state s. To improve precision we pool twelve months of event-time dummies into year dummies. In addition to state-time fixed effects, the model also includes time-race and state-race fixed effects. The inclusion of state-time fixed effects means the reported coefficients should be interpreted as the difference between PECS-banning states and others states in the *difference* in the job-finding hazard between blacks or Hispanics and whites. The sample is restricted to balanced event years common to all PECS-ban states. Microdata on individual unemployment and job-finding come from the Current Population Survey (US Census Bureau (2015a)). Error bars show 95% confidence intervals generated from standard errors clustered at the state level.

Figure 2-8: Event-Time Analysis of the Effect of PECS on Separations: New Hires State-Race FE, Time-Race FE, Time-State FE

(a) Blacks



(b) Hispanics



Notes: This figure shows the results of an event-time analysis of the difference in involuntary separation rates for workers newly hired out of unemployment between states banning and not banning Pre-Employment Credit Screening (PECS) before and after the PECS bans went into effect. Each panel shows results for a different race. The reported coefficients come from estimating a modified linear probability model of Equation 2.5.3 where we interact an indicator for being covered by a PECS ban, $D_{s(i),t}$, with indicators for event time, κ_{st}. Event time is defined as the calendar year and month, t, minus the year and month that a PECS ban took effect in state s. To improve precision we pool twelve months of event-time dummies into year dummies. The model also includes time-race and state-race fixed effects. The inclusion of state-time fixed effects means the reported coefficients should be interpreted as the difference between PECS-banning states and others states in the *difference* in the separation rate between blacks or Hispanics and whites. The sample is restricted to balanced event years common to all PECS-ban states. Microdata on individual unemployment and involuntary separation rates for new hires come from the Current Population Survey (US Census Bureau (2015a)). Error bars show 95% confidence intervals generated from standard errors clustered at the state level.

2.10 Tables

Table 2.1: PECS Bans

State/City (1)	Date Passed (2)	Date Took Effect (3)
Washington	4/18/07	7/22/07
Hawaii	7/15/09	7/15/09
Oregon	3/29/10	7/10/10
Illinois	8/10/10	1/1/11
Maryland	4/12/11	10/1/11
Connecticut	7/13/11	10/1/11
California	10/10/11	1/1/12
Chicago	3/14/12	4/1/12
Vermont	5/17/12	7/1/12
Colorado	4/19/13	7/1/13
Nevada	5/25/13	10/1/13
Delaware	5/18/14	5/8/14
New York City	5/6/15	9/3/15

Table 2.2: PECS Bans: Exempted Jobs and Industries

		HI (1)	OR (2)	IL (3)	MD (4)	CT (5)	CA (6)	CHI (7)
Panel A. Exempted Jobs / Job Duties								
Management	Set the direction of a business or business unit	X		X	X	X	X	X
	Access to high-level trade secrets			X	X	X	X	X
	Access to corporate financial info			X				
	Access to payroll info					X		
	Provide administrative support for executives							
	Direct employees using independent judgment	X						
Legal	Law enforcement		X	X			X	X
Confidentiality	Access to clients' financial info (non-retail)		X	X	X	X	X	X
	Access to clients' personal confidential info			X		X		
Fiduciary	Signatory power / custody of corporate accounts			X	X	X	X	X
	Unsupervised access to marketable assets			X		X		X
	Unsupervised access to cash			X		X	X	
Miscellaneous	Control over digital security systems							
	Airport security		X					
Panel B. Exempted Industries								
Finance	Banking and related activities	X	X	X	X	X	X	X
	Savings institutions, including credit unions	X	X	X	X	X	X	X
	Securities, commodities, funds, trusts, etc.				X	X	X	
	Insurance carriers and related activities			X		X	X	X
Law Enforcement	Law Enforcement and Corrections			X				X
	Department of Natural Resources			X				
Miscellaneous	Gaming							
	Space Research							
	National Security							
	Debt Collection			X				X
	Other state and local agencies			X				

Notes: Marketable assets are e.g. museum/library collections, pharmaceuticals, and exclude furniture and equipment Table excludes Washington, which passed similar legislation on 4/18/07, taking e

Table 2.3: Summary Statistics: CPS

	PECS-Ban States	Non-PECS-Ban States	Covered Jobs (within PECS-ban states)	Exempted Jobs
	(1)	(2)	(3)	(4)
Panel A: Blacks				
Black Share of State Adult Population	9.29%	13.32%	.	.
Black Labor-Force Participation Rate	66.21%	66.08%	.	.
Black Employment Rate	87.04%	88.92%	.	.
Share of Black Labor Force Covered by Ban	55.72%	0%	.	.
Share of Black Unemployed Covered by Ban	65.33%	0%	.	.
Average Weekly Wage ($)	771.98	633.09	660.57	902.76
Share of Workers with 4-Year College Degree	30.41%	22.71%	20.98%	41.63%
Panel B: Hispanics				
Hisp. Share of State Adult Population	20.78%	10.71%	.	.
Hisp. Labor-Force Participation Rate	71.78%	70.21%	.	.
Hisp. Employment Rate	89.21%	92.89%	.	.
Share of Hisp. Labor Force Covered by Ban	71.79%	0%	.	.
Share of Hisp. Unemployed Covered by Ban	76.77%	0%	.	.
Average Weekly Wage ($)	635.95	605.35	559.87	825.87
Share of Workers with 4-Year College Degree	13.62%	16.32%	9.23%	24.40%
Panel C: Whites				
White Share of State Adult Population	69.93%	75.97%	.	.
White Labor-Force Participation Rate	67.10%	66.11%	.	.
White Employment Rate	93.28%	95.00%	.	.
Share of White Labor Force Covered by Ban	52.67%	0%	.	.
Share of White Unemployed Covered by Ban	61.95%	0%	.	.
Average Weekly Wage ($)	979.55	837.47	837.87	1,136.74
Share of Workers with 4-Year College Degree	44.75%	36.19%	35.81%	54.41%

Notes: This table shows summary statistics by race from the CPS for years 2003-2015. Columns (1) and (2) respectively show statistics for PECS-ban states and non-ban states. Columns (3) and (4) then compare covered vs. exempted jobs within PECS-ban states. The share of unemployed workers covered by a PECS ban is determined by whether an unemployed worker's most recent job was covered by or exempted from his home state's PECS ban.

Table 2.4: Summary Statistics: CPS Dependent Variables

	PECS-Ban States	Non-PECS-Ban States	Covered Jobs (within PECS-ban states)	Exempted Jobs
	(1)	(2)	(3)	(4)
Panel A: Blacks				
Job-Finding Rate (out of Unemployment)	0.127	0.148	0.187	0.145
Involuntary Separation Rate, New Hires	0.113	0.096	0.111	0.128
Involuntary Sep. Rate, Long-Tenure Workers	0.025	0.020	0.025	0.026
Panel B: Hispanics				
Job-Finding Rate (out of Unemployment)	0.179	0.224	0.253	0.190
Involuntary Separation Rate, New Hires	0.103	0.079	0.100	0.133
Involuntary Sep. Rate, Long-Tenure Workers	0.021	0.017	0.022	0.020
Panel C: Whites				
Job-Finding Rate (out of Unemployment)	0.148	0.188	0.203	0.165
Involuntary Separation Rate, New Hires	0.087	0.070	0.079	0.109
Involuntary Sep. Rate, Long-Tenure Workers	0.016	0.012	0.016	0.015

Notes: This table shows job-finding rates, separation rates for recent hires, and separation rates for long-tenure workers, by race in the CPS for years 2003-2015. Recent hires are defined as individuals observed with previous unemployed-to-employed transitions in up to 15 months of history in the CPS panel. Long-tenure workers are defined as individuals observed as employed at all prior available dates in the CPS panel. Columns (1) and (2) respectively show statistics for states with and without PECS bans. Columns (3) and (4) then compare covered vs. exempted jobs within PECS-ban states.

Table 2.5: Summary Statistics: LEHD J2J

	Non-Ban States	PECS Ban States
	(1)	(2)
Panel A: Separation Rate		
Blacks	0.101	0.094
Hispanics	0.099	0.085
Whites	0.072	0.064
Panel B: Adjacent Quarter Job-Finding Rate		
Blacks	0.238	0.222
Hispanics	0.223	0.217
Whites	0.224	0.216

Notes: This table shows separation and job-finding rates by race in the LEHD J2J Flows data, broken down by states with and without PECS bans. Three states with PECS bans after 2012 (Colorado, Nevada, and Delaware), New England states, Kansas and Missouri are not included in this table due to data limitations. The data begin in 2005Q1 and end in 2014Q2. The separation rate is computed as the number of separations divided by beginning of quarter employment (i.e. "ensep" / "mainb"). Adjacent quarter job-finding rate is computed as the number of people who separate to adjacent quarter employment dividied by total separations (i.e. . "aqsep" / "ensep").

Table 2.6: Impact of PECS on Job-Finding: State-Level Variation

	(1)	(2)
1(Black)*1(Treated by Ban)	-0.156**	-0.158**
	(0.0666)	(0.0655)
1(Hispanic)*1(Treated by Ban)	0.0727	0.0642
	(0.0558)	(0.0568)
1(Non-Hispanic white)*1(Treated by Ban)	-0.0130	-0.0126
	(0.0310)	(0.0309)
N	298,386	298,386
States	51	51
Ban States	8	8
Time-Race Fixed Effects	Y	Y
State-Race Fixed Effects	Y	Y
Demographic Controls	N	Y

Notes: This table reports MLE estimates of race-specific log differences in job-finding hazard rates following a PECS ban, using a state-time difference-in-differences strategy (Equation 5.11 in the text). Data are from the CPS for years 2003-2015. Column (1) includes the state-race and time-race fixed effects that implement difference-in-differences, while Column (2) adds demographic controls fully interacted with race, which include binned education, binned age, gender and marital status, urbanicity, and interactions between month-of-year and Census division. Standard errors clustered at the state level are shown in parentheses.

Table 2.7: Impact of PECS on Job-Finding: Job-Level Variation

	(1)	(2)	(3)	(4)
1(Black)*1(Treated by Ban)	-0.209***	-0.209***	-0.173***	-0.171***
	(0.0714)	(0.0709)	(0.0624)	(0.0622)
1(Hispanic)*1(Treated by Ban)	0.0383	0.0356	-0.00653	-0.00311
	(0.0642)	(0.0636)	(0.0538)	(0.0524)
1(Non-Hispanic white)*1(Treated by Ban)	-0.0352	-0.0376	-0.0199	-0.0252
	(0.0529)	(0.0531)	(0.0495)	(0.0502)
N	293,336	293,336	293,344	293,344
States	51	51	51	51
Ban States	8	8	8	8
Treatment Level	Expected Job	Expected Job	Past Job	Past Job
State-PastJob-Race Fixed Effects	Y	Y	Y	Y
Time-Race Fixed Effects	Y	Y	Y	Y
Demographic Controls	N	Y	N	Y

Notes: This table reports MLE estimates of race-specific log differences in job-finding hazard rates following a PECS ban, using a state-job-time difference-in-differences strategy (Equation 5.11 in the text). Data are from the CPS for years 2003-2015. Columns (1) and (3) include the state-job-race and time-race fixed effects that implement difference-in-differences, while Columns (2) and (4) add demographic controls fully interacted with race, which include binned education, binned age, gender and marital status, urbanicity, and interactions between month-of-year and Census division. In the first two columns, a job-seeker's exposure to a PECS ban is determined by whether his expected next job (as defined in Footnote 15 in the text) is covered by or exempted from a PECS ban, while in the final two columns, exposure is determined by whether a job-seeker's most recent job was covered or exempted. Data from Washington are excluded due to uncertainty about which jobs are exempted from Washington's ban. Sample sizes differ slightly between columns using expected-job variation ((1) and (2)) and columns using past-job variation ((3) and (4)) because not all jobs are observed in the pre-ban sample on which search probabilities are estimated. (See Footnote 15 in the text.) Standard errors clustered at the state level are shown in parentheses.

Table 2.8: Impact of PECS on Separation Rates for New Hires

	(1)	(2)	(1)	(2)
1(Black)*1(Treated by Ban)	0.0272	0.0276*	0.0442*	0.0434**
	(0.0162)	(0.0158)	(0.0243)	(0.0213)
1(Hispanic)*1(Treated by Ban)	-0.0544***	-0.0549***	-0.0494***	-0.0494***
	(0.0163)	(0.0159)	(0.0180)	(0.0182)
1(Non-Hispanic white)*1(Treated by Ban)	-0.0268***	-0.0262***	-0.0240***	-0.0242***
	(0.00879)	(0.00907)	(0.00825)	(0.00830)
N	46,201	46,201	45,446	45,446
States	51	51	51	51
Ban States	8	8	8	8
Treatment Level	State	State	New Job	New Job
Time-Race Fixed Effects	Y	Y	Y	Y
State-Race Fixed Effects	Y	Y	N	N
State-NewJob-Race Fixed Effects	N	N	Y	Y
Demographic Controls	N	Y	N	Y

Notes: This table reports linear probability model estimates of race-specific differences in separation rates for newly hired workers following a PECS ban, using various difference-in-difference strategies. Columns (1) and (2) use state-time difference-in-differences, while Columns (3) and (4) use state-job-time difference-in-differences. Data are from the CPS for years 2003-2015. Columns (1) and (3) include the state-(job-)race and time-race fixed effects that implement difference-in-differences, while Columns (2) and (4) add demographic controls fully interacted with race, which include binned education, binned age, gender and marital status, urbanicity, and interactions between month-of-year and Census division. In the state-job-time

Table 2.9: Impact of PECS on Separation Rates for Long-Term Employees

	(1)	(2)	(3)	(4)
1(Black)*1(Treated by Ban)	-0.00370	-0.00377	-0.00381	-0.00399
	(0.00236)	(0.00238)	(0.00514)	(0.00514)
1(Hispanic)*1(Treated by Ban)	-0.00399**	-0.00405**	-0.00511**	-0.00509**
	(0.00169)	(0.00167)	(0.00217)	(0.00217)
1(Non-Hispanic white)*1(Treated by Ban)	-0.00282***	-0.00271***	-0.00392***	-0.00383***
	(0.000674)	(0.000686)	(0.000848)	(0.000854)
N	3,889,607	3,889,607	3,824,278	3,824,278
States	51	51	51	51
Ban States	8	8	8	8
Treatment Level	State	State	Current Job	Current Job
Time-Race Fixed Effects	Y	Y	Y	Y
State-Race Fixed Effects	Y	Y	N	N
State-NewJob-Race Fixed Effects	N	N	Y	Y
Demographic Controls	N	Y	N	Y

Notes: This table re-estimates the difference-in-difference models from Table 7 on a placebo sample of long-tenure workers. Long-tenure workers are defined as individuals observed as employed at all prior available dates in the CPS panel. As in Table 7, Columns (1) and (3) include the state-(job-)race and time-race fixed effects that implement difference-in-differences, while Columns (2) and (4) add demographic controls. Data from Washington are excluded from Columns (3) and (4) due to uncertainty about which jobs are exempted from Washington's ban. Standard errors clustered at the state level are shown in parentheses.

Table 2.10: Robustness of Impact of PECS on Job-Finding: LEHD J2J

	DD		DDD	
	(1)	(2)	(3)	(4)
1(Black)*1(Treated by Ban)	-0.074**	-0.006	-0.044	-0.052***
	(0.032)	(0.030)	(0.028)	(0.018)
1(Hispanic)*1(Treated by Ban)	-0.053***	0.078**	-0.043***	-0.014
	(0.015)	(0.034)	(0.013)	(0.013)
1(Non-Hispanic white)*1(Treated by Ban)	-0.017	0.046	.	.
	(0.015)	(0.032)	.	.
N	5,448	5,448	5,448	5,448
States	38	38	38	38
Ban States	6	6	6	6
Time-Race Fixed Effects	Y	Y	Y	Y
State-Race Fixed Effects	Y	Y	Y	Y
State-Time Fixed Effects	N	N	Y	Y
State-Race Linear Trends	N	Y	N	Y

Notes: This table reports OLS estimates of difference-in-differences and triple-difference models for the complementary-log-log of race-specil aggregate job-finding rates (i.e. ln(-ln(1-job-finding))). Data are from the LEHD J2J. Column (1) includes the state-race and time-race fixed effects th implement difference-in-differences (Equation (XX) in the text), while Column (2) adds controls for linear trends at the state-race level. Column (3) adı state-time effects to the specification from Column (1), implementing a triple-difference estimator. Column (4) then augments the triple-differen model with linear trends at the state-race level. Three states with PECS bans after 2012 (Colorado, Nevada, and Delaware), New England states, Kans and Missouri are not included in this table due to data limitations. Standard errors clustered at the state level are shown in parentheses.

Table 2.11: Robustness of Impact of PECS on Job-Finding: CPS

	DD		DDD	
	(1)	(2)	(3)	(4)
1(Black)*1(Treated by Ban)	-0.156**	-0.160	-0.115	-0.358**
	(0.0666)	(0.177)	(0.100)	(0.180)
1(Hispanic)*1(Treated by Ban)	0.0727	0.107	0.0746	-0.00716
	(0.0558)	(0.0676)	(0.0618)	(0.0953)
1(Non-Hispanic white)*1(Treated by Ban)	-0.0130	0.127***	.	.
	(0.0310)	(0.0397)	.	.
N	298,386	298,386	298,386	298,386
States	51	51	51	51
Ban States	8	8	8	8
Time-Race Fixed Effects	Y	Y	Y	Y
State-Race Fixed Effects	Y	Y	Y	Y
State-Time Fixed Effects	N	N	Y	Y
State-Race Linear Trends	N	Y	N	Y

Notes: This table reports MLE estimates of race-specific log differences in job-finding hazard rates following a PECS ban, using a variety of difference-i differences and triple-difference models. Data are from the CPS for years 2003-2015. For convenience, Column (1) is a repeat of the state-time differen in-differences results in Column (1) of Table 5. Column (2) adds controls for linear trends at the state-race level. Column (3) adds state-time effects to tl specification from Column (1), implementing a triple-difference estimator. Column (4) then augments the triple-difference model with linear trends the state-race level. Standard errors clustered at the state level are shown in parentheses.

Table 2.12: Robustness of Impact of PECS on Separation: CPS

	DD		DDD	
	(1)	(2)	(3)	(4)
1(Black)*1(Treated by Ban)	0.0272	0.0491	0.0429**	0.0660*
	(0.0162)	(0.0314)	(0.0166)	(0.0371)
1(Hispanic)*1(Treated by Ban)	-0.0544***	-0.0179	-0.0214	0.00700
	(0.0163)	(0.0425)	(0.0199)	(0.0629)
1(Non-Hispanic white)*1(Treated by Ban)	-0.0268***	-0.00368	.	.
	(0.00879)	(0.0145)	.	.
N	46,201	46,201	46,201	46,201
States	51	51	51	51
Ban States	8	8	8	8
Time-Race Fixed Effects	Y	Y	Y	Y
State-Race Fixed Effects	Y	Y	Y	Y
State-Time Fixed Effects	N	N	Y	Y
State-Race Linear Trends	N	Y	N	Y

Notes: This table reports linear probability model estimates of race-specific differences in separation rates for newly hired workers following a PECS ba using a variety of difference-in-differences and triple-difference strategies. Data are from the CPS for years 2003-2015. For convenience, Column (1) is repeat of the state-time difference-in-differences results in Column (1) of Table 5. Column (2) adds controls for linear trends at the state-race leve Column (3) adds state-time effects to the specification from Column (1), implementing a triple-difference estimator. Column (4) then augments the tripl difference model with linear trends at the state-race level. Standard errors clustered at the state level are shown in parentheses. New hires are define as individuals observed with previous unemployed-to-employed transitions in up to 15 months of history in the CPS panel. Standard errors clustered the state level are shown in parentheses.

Chapter 3

Tax Refund Expectations and Financial Behavior*

3.1 Introduction

Income uncertainty is thought to play a central role in household finances. While pre-tax income volatility is often emphasized as a source of this uncertainty, households may also have substantial uncertainty about their income *tax*. For low-income individuals, tax-linked transfer payments, including payments from the Earned Income Tax Credit (EITC), comprise a substantial portion of annual income.[1] Quantifying and understanding uncertainty about income taxes is therefore critical for understanding the role of transfer payments through the tax system in household finances, the potential consequences of changes to the tax system, and the effects of income uncertainty on consumption and financial decisions more broadly.

In this paper, we study what low- and moderate-income households do and do not know about their income tax refunds before they file taxes. We then examine how financial behavior responds to expectations of future tax refunds, refund uncertainty, and surprises in realized tax refund amounts. We do so using a unique combination of (1) administrative tax records, (2) a linked panel of consumer credit reports, and surveys to measure both (3) expectations of tax refunds before tax filing and (4) consumption behavior after tax refund receipt. One key innovation in our setting is our direct measurement of taxpayers' beliefs about the probability distribution over their own future tax refund amounts. These expectations data allow us to study the amount of, and the effects of, income tax uncertainty on consumption without making strong assumptions about the sources of taxpayers' uncertainty.

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[1]For instance, in our sample, the mean refund totals nearly eight percent of annual income (roughly one month of earnings).

We start by showing that taxpayers have correct mean expectations about their refund on average, but also face substantial uncertainty. This self-reported uncertainty accurately tracks "true" uncertainty, as measured by the difference ("surprise") between realized and mean expected tax refund amounts. We examine sources of refund uncertainty. Surprises are driven by changes in income and family structure in ways that are consistent with households misunderstanding how marginal tax rates change at different parts of the earned-income tax credit schedule. Nevertheless, we also find that much of this uncertainty is not explained by observables or by changes in household circumstances.

We then show that household consumption and borrowing behavior depends on expectations about tax refund amounts, refund uncertainty, and refund surprises. Households borrow a moderate amount out of their expected tax refunds: for each dollar of expected refund, households repay roughly 15 cents in debt shortly after tax refund receipt. Households also exhibit precautionary behavior in borrowing out of future tax refunds, as these borrowing and repayment patterns out of expected tax refunds are less pronounced for households that report being more uncertain about their refunds ex-ante. To our knowledge, this is some of the first evidence of precautionary behavior (prudence) among a low-income population in the US. This finding contrasts with prior work which has interpreted the combination of high income volatility and low savings rates as evidence against the existence of precautionary behavior among low-income households (Carroll et al., 2003).

Finally, we examine the link between tax refund surprises and debt, and find that surprises in tax refund amounts are not used to repay debt. In fact, we find that larger refund surprises lead to *increases* in overall debt, an effect that is entirely driven by higher balances on installment loans such as auto loans. This pattern implies a medium-run marginal propensity to consume (MPC) out of windfall income above one. One explanation for this stark finding is that refund surprises may be used to relax collateral constraints for newly financed durable purchases. We find suggestive evidence from a follow-up survey on durable consumption choices to corroborate this interpretation.

There are two primary advantages to our empirical approach. First, we obtain rich data on household balance sheets before and after the resolution of tax-related income uncertainty: administrative data on all reported income and nearly all financial liabilities, as well as survey-based measures of real and financial assets. Such data are particularly difficult to assemble for lower-income populations. Second, we directly elicit individuals' uncertainty about the component of future income risk driven by tax refund uncertainty. This stands in contrast to much of the existing literature that looks for evidence of precautionary behavior in response to uncertainty; we know of one notable exception (Jappelli and Pistaferri, 2000).

One substantial caveat to our approach is that we, like most researchers who use U.S.-based data, have relatively poor data on households' real and financial assets. All of our asset measures are survey-based, whereas we have administrative data on income and debt liabilities. Perhaps reassuringly, the low-income population in the United States has elsewhere been shown to hold low levels of financial and real assets, a finding that we corroborate in our survey measures. A second important caveat is that, while we analyze differences in financial behavior within groups that are at similar stages in the life-cycle (age, income, and family structure), there nevertheless may be important unobservable differences across individuals within these groups – for example, in unobservable labor income risk – that we cannot control for and that are correlated with tax-relevant uncertainty.

Related Literature This paper contributes to at least three distinct literatures.

First, we contribute to a large empirical literature in macroeconomics on household consumption, savings, and borrowing decisions. This work studies how households respond to income uncertainty ex-ante, and how households react to income surprises ex-post. A robust theoretical literature predicts that households will save precautionarily – maintaining a "buffer stock" – in the presence of future income uncertainty (Kimball, 1990; Deaton, 1991; Carroll, 1996), and calibration exercises suggest that the role of precautionary motives in saving over the life-cycle is substantial (Carroll and Samwick, 1998). However, other empirical work has found limited evidence for precautionary behavior (Dynan, 1993), especially among low-income households (Carroll et al., 2003). This latter result stems from substantial labor income uncertainty faced by low income households coupled with low observed savings rates. Much of the empirical work testing for precautionary motives uses labor income uncertainty implied by income processes imputed using observables such as age and occupation (Skinner, 1988; Dynan, 1993; Carroll and Samwick, 1998). One notable exception uses self-reported uncertainty measured through a survey, as we do (Jappelli and Pistaferri, 2000). We believe we are the first paper to link such survey-based measures of uncertainty to administrative data on income and borrowing.

Another vein of empirical macroeconomics research studies how consumers respond to windfall income surprises. Most closely related to our study of tax refund surprises is a set of papers analyzing responses to tax rebates (Johnson et al., 2006; Agarwal et al., 2007; Parker et al., 2013; Broda and Parker, 2014; Baugh et al., 2018). These papers find, as we do, high marginal propensities to consume (MPCs) out of such windfall income. Of particular note is Parker et al. (2013), which finds that up to 60 percent of tax rebate payments are used to purchase durables, and especially vehicles, within 3 months of rebate receipt. These findings are consistent with our result that positive refund surprises are used to finance durable purchases.

Second, we contribute to a growing literature on the limits of taxpayers' understanding of the tax code and on the consequences of tax complexity for individuals and firms. Several recent papers have shown that individuals and firms fail to take full advantage of the credits and refunds for which they are eligible. Part of this is likely due to hassle costs: individuals may rationally choose not to invest the time or money required to optimize their tax benefits (Benzarti, 2017). Among low-income EITC filers, like those in our sample, part of this failure to optimize may be due to lack of information about the tax system (Aghion et al., 2017; Chetty et al., 2013; Zwick, 2018). Prior research has shown that many individuals are unaware of EITC program rules and that lack of information has real consequences for earnings behavior (Chetty and Saez, 2013; Chetty et al., 2013; Romich and Weisner, 2000;

Smeeding et al., 2000). We contribute to this literature by directly quantifying the amount of uncertainty faced by our population of low-income tax filers, and by linking this uncertainty to actual consumption decisions.

Third, we contribute to a diverse literature on the measurement and reliability of subjective expectations data (Manski, 2004). Following the pioneering work of Engelberg et al. (2009), we elicit not just point forecasts or mean expectations, but individuals' subjective probability distributions over future events. These methods have previously shown success in measuring inflation expectations (Armantier et al., 2016), income expectations among college students (Zafar, 2011), and income expectations in a developing country (Delavande et al., 2011). Elicited expectations have been shown to affect financial behavior in lab settings (Wiswall and Zafar, 2014). Our contribution is to link probabilistic income expectations with a panel of administrative data to study how financial behavior responds to such expectations in a "real world" (non-lab) setting, and to demonstrate success of these survey questions even in a low-income, relatively low-education U.S. population.

The rest of the paper proceeds as follows. The next section describes the empirical setting and data. Section 3.3 describes how we translate our survey measures of beliefs into probabilistic distributions and compares these distributions to actual refund amounts. Section 3.4 shows how refund expectations, uncertainty, and surprises translate into consumption and borrowing decisions. Section 3.5 concludes.

3.2 Data and Empirical Setting

In this section we describe our data and empirical setting. We first provide institutional background on the setting, a clinic that provides free income tax preparation services in Boston. We then describe our administrative (tax and credit) and survey (expectations, assets, and consumption) data sources. We conclude by describing the characteristics of our sample.

3.2.1 Boston Tax Sites

Our data come from a Volunteer Income Tax Assistance (VITA) tax preparation center operated by the Boston Tax Help Coalition and the Boston Office for Financial Empowerment (OFE). The City of Boston runs over 30 free tax preparation centers, which annually serve more than 13,000 clients. Our data come from one of the largest of these centers, Dorchester House.

Boston residents are eligible to receive these free tax preparation services if they worked in the prior year, earned less than $54,000, and do not own their own business. Eligible individuals who come to the tax site ("clients") typically go through three separate stations. First, they complete an intake survey, which includes questions on demographics, use of city services, savings behavior, and credit usage. Next, clients are offered a free "financial check-up" from a trained volunteer referred to as a "financial guide." The financial guides offer the client a free credit report and provide information on other city services for which the individual may be eligible. Finally, the client is sent to a tax preparer who electronically prepares and submits the individual's tax return.

We partnered with the Boston OFE to field a survey of clients' expectations about their tax refund (detailed in Section 3.2.3) at the second of these three stations, together with the financial guide. This survey came before clients filed taxes, and so measures their pre-filing uncertainty about their tax refund. At this stage, clients also provided consent for their tax, credit, and survey information to be used for research purposes. Figure 3.6.1 describes the flow of clients through Dorchester House in more detail.

Two operational features of the financial check-up stage deserve mention. First, because of financial guide shortages and constrained tax site operating hours, many tax filers skipped the financial check-up during busy periods. As a result, we obtained consent from only 60 percent of tax filers. However, among clients who completed the financial counseling session our consent rate was 96 percent. Therefore, we do not believe that tax filer consent was a major source of selection into our research sample.

Second, the OFE implemented a separate randomized controlled trial as part of the financial check-up wherein clients were randomly assigned to a more or less detailed check-up. Those assigned the more detailed check-up were given an in-depth explanation of their credit report, as well as financial advice and referrals to a variety of services provided by the City of Boston and state and federal organizations. Those assigned to the less detailed check-up also received their credit report, but no detailed financial advice or referrals. In our analysis of consumption responses in Section 3.4, we control for treatment status at the financial check-up stage.[2]

3.2.2 Administrative Tax and Credit Data

We obtain administrative tax returns for consenting clients who filed their taxes at Dorchester House. These data include information on income, family structure (filing status and number of dependents), and refund amount. For individuals who previously used the city's tax preparation services, we are able to link these returns to those from earlier years. We have two years of returns for 69 percent of the tax filers in the credit and expectations survey sample.

We merge these administrative tax records with a short panel of consumer credit reports for clients who provided consent during the financial check-up. We have four reports for each individual in our sample: one that was pulled when they visited the tax site, and three that

[2] An OFE analysis of the randomized controlled trial finds balance across treatment assignment on a range of taxpayer characteristics. The report can be accessed at https://owd.boston.gov/wp-content/uploads/2017/07/DES-89-Financial-Check-up-Evaluation-2017-Web.pdf?utm_source=Office+of+Workforce+Development&utm_campaign=699487e955-EMAIL_CAMPAIGN_2017_07_24&utm_medium=email&utm_term=0_f071f9ca69-699487e955-226050949

were pulled one, two, and six months later. The one and two month credit reports measure changes in debt levels soon after tax filing. For clients who receive their refund by direct deposit, both the first and second-month follow-up credit reports show loan balances after tax refund receipt; for clients who receive their tax refund by paper check, the first of these two credit reports likely show balances from prior to refund receipt. The six month report allows us to observe longer-run deleveraging and new loan originations (e.g. auto loans) that may not have been reported in time for the one and two month follow-ups.

3.2.3 Expectations and Consumption Surveys

We supplement these administrative data sets with three distinct surveys. These surveys provide information on taxpayer demographics and assets, refund expectations, and consumption before and after refund receipt. Our first source of survey data is the demographics and assets survey individuals completed when they arrived at Dorchester House. From this survey we obtain information on a client's gender and level of education (high school degree or some college), and on a client's savings behavior. The response rate for this survey was high: of the 1,186 individuals who filed taxes at Dorchester House during the spring 2016 season, 995 completed the survey. A copy of the survey is provided in Appendix .2.1.

We obtain information on tax filers' expectations and uncertainty about their refunds from a short four-question survey. Tax filers completed this survey after they had been paired with a financial guide, but before they filed their taxes and learned their actual refund amount. We elicited beliefs in two ways. First, we directly asked each filer how much they expected their refund to be, and their qualitative certainty that the refund would fall within $500 of this amount. Second, we provided individuals with a set of six bins, and asked them the probability that their refund would fall within each bin. A copy of the survey is provided in Appendix .2.2. We discuss how we translate the answers to these questions into probabilistic beliefs in Section 3.3.

Finally, we merge these expectations with data from a second consumption survey designed to measure saving and consumption behavior before and after refund receipt. While we obtain substantial information on consumption from the panel of credit reports – for example, the presence of new auto loans or pay-down of debt – these reports do not contain information on durable purchases or on the timing of purchases relative to refund receipt. They also contain no information on savings. The response rate to our consumption survey was 46 percent (291 out of 625 filers in our sample), which is high compared to similar phone-based surveys.[3] The consent language and questions are provided in Appendix .2.3.

[3]For example, Allcott and Kessler (2018) obtain an 18 percent response rate to a phone survey of energy usage, a higher rate than they expected.

3.2.4 Descriptive Statistics

Table 3.1 presents descriptive statistics from our sample of low-income Boston tax filers. Column 1 includes all 995 tax payers who visited Dorchester House and completed the intake survey during the spring 2016 tax season. The average adjusted gross income (AGI) in this sample is $21,603, and the mean refund size is $1,765. Thirty-eight percent of filers receive the Earned Income Tax Credit (EITC). Most filers are unmarried, but only 28 percent file as a single head of household, and 34 percent have dependents (including married filers). Eighty percent of filers have a high school degree, but only 15 percent have attended college. The average age is 41 years old.

The remaining columns restrict the sample to the subsets for which we have credit reports, the expectations and consumption surveys, and tax returns from the prior year (2015). Column 2 reports statistics from the 714 filers in the credit report sample. Tax filers in this sample are highly leveraged with very low savings rates. The average filer has roughly $9000 in installment debt, $1700 in credit card debt, and $500 in savings. Average savings is less than one third of the average refund amount, and less than 5 percent of average debt. The mean FICO score for those with credit reports is 664, below the 2016 U.S. average of 700. The credit report sample is similar to the asset survey sample in terms of age, gender, education, family status, AGI, and refund amounts.

These economic and demographic variables remain stable across columns 3-5, suggesting that attrition across surveys is largely unrelated to tax status or demographic characteristics that could bias our results. Column 3 restricts to the 625 tax filers in the credit sample who completed the expectations survey. The vast majority (557) of these filers also completed the asset survey. Column 4 restricts to the much smaller sample of 291 filers who completed the follow-up consumption survey. Despite a 46 percent response rate, households that did and did not respond are nearly identical in terms of their average characteristics. Column 5 restricts to tax filers with expectations and credit reports who filed their taxes with the City of Boston in the previous year (2015).

Table 3.1 shows that refund amounts are large relative to income, savings, and debt levels. The mean refund of $1,776 is approximately eight percent of the average individual's adjusted gross income and is triple the average individual's savings at tax filing. In addition, we show in section 3.3 that tax filers face a large degree of uncertainty about their refunds. This suggests that tax refunds and uncertainty about them can have important implications for financial behavior in this population.

3.3 Tax Refund Expectations and Realizations

We surveyed Dorchester House tax filers to elicit their beliefs about the tax refund they would receive after filing. Since consumption responses to refund amounts may depend on both mean expectations and uncertainty, our survey elicited both aspects of filers' beliefs through a probabilistic survey question. This section describes the belief survey; explains how we converted survey responses to smooth belief distributions; and compares beliefs to realized refund amounts and surprises. Although tax filers reported substantial uncertainty about their refunds, their mean expectations were, on average, correct. In addition, filers reporting greater uncertainty saw larger refund surprises. This suggests that most tax filers had an accurate sense of the refund amount they could expect to receive, and also had an accurate sense of the uncertainty they faced. However, there is evidence of (mean) inaccurate expectations particularly among tax filers whose incomes or family status changed relative to past years, suggesting that tax filers are imperfectly aware of after-credit tax rates at different points in the income tax schedule.

3.3.1 Belief Elicitation Survey

The survey was administered at the beginning of the financial counseling session at Dorchester House, which took place prior to the tax preparation session. We view this as the ideal time to survey program participants on their refund expectations: tax filers had not yet received any information about their refunds. However, tax filers had collected their tax documents, come to the tax site, and filled out a detailed economic and demographic survey for use during the tax preparation session.

The final question in this survey elicited probabilistic refund expectations. Respondents were asked the percent chance that their refund would fall in each of six bins: negative (they would have to pay taxes), \$0-\$500, \$500-\$1,000, \$1,000-\$2,500, \$2,500-\$5,000, over \$5,000. We asked for points in a cumulative density function rather than moments such as the mean and standard deviation because subjective probabilities are easier to understand and calculate. In addition, probabilistic survey questions can provide richer information about beliefs. We would have ideally constructed bins around each filer's point estimate to obtain comparable uncertainty measures across households. The need to conduct the survey quickly made this approach too difficult to implement, so we used fixed intervals. Nevertheless, we show in the next section that the fitted distributions accurately capture both expected refund amounts and uncertainty.

Appendix Table A1 describes features of the elicited belief distributions. The first column presents statistics for all tax filers in our main analysis sample, and the remaining columns disaggregate those statistics into subgroups. Forty percent of respondents put nonzero probability on three or more bins, while 60 percent did so on only one or two bins.

3.3.2 Fitting Belief Distributions

To summarize beliefs and to quantify both mean expectations and uncertainty, we convert each probabilistic elicitation into a smooth probability distribution following Engelberg et al. (2009) (hereafter EMW). Our goal is to use all information available in respondents' subjective probabilities and to smooth between points of the cumulative density function in a reasonable way. We fit a distribution which depends on the number of bins on which the respondent placed positive probability. Single bin reports are fit with a scalene triangle; the support is the full bin, and the mode is the point estimate. In this case, we depart from EMW by using additional information from the respondent's point estimate, fitting a scalene triangle rather than an isosceles triangle. Meanwhile, two-bin reports are fit with an isosceles triangle with the widest possible support that is consistent with the probabilities for each bin. These sets of assumptions uniquely pin down a distribution for one- and two-bin responses. For three or more bins, we follow EMW in fitting a beta distribution to the reported quantiles. Triangle and beta distributions are appropriate for our setting because they have finite support, and because beta distributions can match a wide range of distributional shapes that might be implied by probabilistic survey questions.[4] The maximum refund amount was a little below $20,000, and the lowest refund amount was about -$500. We take these two values as the endpoints of the support of the highest (over $5,000) and lowest (negative) bins.

The triangle distributions are exactly identified and fit using analytical formulas. To fit the beta distributions, we follow EMW and minimize the sum of squared differences between the reported cumulative probabilities at each point in the distribution's support and those of a beta distribution with the same support. Let $\mathcal{X}$ denote the support points of the response to the probabilistic survey question. Let Z denote a beta-distributed random variable governed by parameters (α, β) and normalized to have support on $\mathcal{X}$. Finally, let p_x denote the reported cumulative probability at each point $x \in \mathcal{X}$. We find the $(\hat{\alpha}_i, \hat{\beta}_i)$ for the elicited distribution from each individual i which solves

$$\min_{\alpha,\beta} \sum_{x \in \mathcal{X}_i} [p_{x,i} - P(Z \leq x \mid \alpha, \beta)]^2$$

The fitted distributions reveal large variation in the expected refund amounts and uncertainty across tax filers. Appendix Table A1 shows that the average mean expectation is $1,970, and the average standard deviation is $740. The average coefficient of variation is 0.50 – so refund uncertainty is, on average, large relative to the expected amount. These averages mask an enormous amount of variation across tax filers in their refund expectations. The standard deviation across tax filers of their mean expectations is $2,850, and the standard deviation of subjective uncertainty (where uncertainty is measured using the standard deviation of each tax filer's fitted distribution) is $1,019.

It is illustrative to compare self-reported measures of qualitative and quantitative uncertainty as a validity check on these survey responses . Table 3.2 summarizes the coefficients of variation of respondents' belief distributions depending on whether they were "very sure," "somewhat sure," or "not sure at all" about whether their refund would be within $500 of their point estimate. The most uncertain individuals have much larger coefficients of variation. Two-way t-tests of equal means strongly reject equal quantitative uncertainty for any two qualitative responses. The next subsection provides additional evidence that the

[4]We also depart from EMW by not constraining the estimated beta densities to be single-peaked.

quantitative measures of uncertainty meaningfully capture tax filers' subjective beliefs.

3.3.3 Beliefs and Realizations

Our unique institutional setting allows us to compare applicants' refund expectations to what they actually received. This comparison shows not only that applicants have correct mean expectations, but also that they understand the degree of uncertainty they face, at least on average.

Figure 3.6.2 compares mean expectations from survey responses to actual refund amounts. Mean expectations closely track realized amounts. The slope of the regression line is close to one, though beliefs are slightly attenuated: those with the most extreme realizations had slightly less extreme expectations. The strongly linear relationship between expected and actual refund amounts does not imply that tax filers faced little uncertainty, or that any individual had unbiased beliefs. Rather, it shows that beliefs tracked realized refund amounts on average, suggesting that the probabilistic survey question does contain meaningful quantitative information.

Figure 3.6.3 performs a similar exercise for the degree of self-reported uncertainty. It compares the magnitude of each tax filer's refund "surprise"–the difference between the realized and expected refund amounts–to the fitted standard deviation of their belief distribution. There is a clear linear relationship between subjective uncertainty and realized absolute errors.[5] Thus, tax filers face substantial refund uncertainty, and furthermore they seem to be aware of the degree of uncertainty that they face. The next section investigates the determinants of refund uncertainty and shows that some but not all of the variation in refund uncertainty across individuals can be explained by observed characteristics.

3.3.4 Predictors of Refund Uncertainty and Surprises

In this section we investigate the predictors of refund uncertainty and the magnitude and direction of refund surprises. We find that current income and family structure are highly predictive of refund uncertainty, while demographic variables such as age, gender, and education are less predictive. Filers whose income or family structure changed from previous years are also more uncertain, but tax filers whose situation did not change still report substantial uncertainty. Furthermore, the characteristics that predict greater uncertainty are also associated with larger surprises, suggesting that these relationships reflect real differences in uncertainty across tax filers. Even after controlling for demographic characteristics and changes in tax situation there is substantial variation in both uncertainty and surprises.

To analyze determinants of subjective refund uncertainty, we first regress three of measures of refund uncertainty on a range of economic and demographic characteristics. Our

[5]Note that the slope of the line should not necessarily be one – a standard deviation is the square root of the expected squared error, not the expected absolute error – and the conditional expectation function need not be linear.

specifications take the form

$$y_i = X_i\beta + \epsilon_i \tag{3.3.1}$$

where y_i is a measure of uncertainty, and X_i includes sociodemographic variables capturing age, education, and gender; demographic variables including marital status and number of dependents; and dummy variables for each quartile of adjusted gross income (AGI). Results from a series of these regressions are shown in Table 3.3. Columns 1-3 use the standard deviation of the household's parametric belief distribution as the measure of uncertainty, y_i; columns 4-6 use the absolute value of the refund surprise (refund amount - mean expectation); and columns 7-9 use the size of the refund surprise.

Column 1 shows that the number of dependents and income quartile are quite predictive of refund uncertainty, as measured by the standard deviation of beliefs. Tax filers in the third and fourth income quartiles report greater uncertainty, as do households with more dependents. These differences are large: for example, an additional dependent is associated with \$449 more in refund uncertainty (as measured by the standard deviation of an individual's fitted belief distribution), and filers in the third AGI quartile report over \$600 greater uncertainty than filers in the first quartile. In contrast, the demographic variables capturing age, education, and gender are less predictive of uncertainty.[6] These patterns are consistent with a model in which cognitive limitations and total experience with the tax system are less important determinants of refund uncertainty than economic characteristics that directly determine tax liabilities.

Columns 2 and 3 add several variables related to changes in financial and family status: whether the filer received unemployment insurance payments in the past year; whether their filing status changed, e.g. from single to married; the (absolute or level) change in their AGI; and the (absolute or level) change in the number of dependents. The sample size in these columns is lower, reflecting the fact that we only observe these changes for filers who filed at a Boston tax site in previous years. Column 2 controls for indicators for, and magnitudes of, these year-to-year changes, while column 3 replaces absolute changes with level (signed) changes.

Our results in these two columns provide mixed evidence on whether changes in financial and family status contribute to refund uncertainty. In column 2, the magnitude of change in AGI is positively related to uncertainty but not statistically significant. Households that experienced an increase in the number of dependents actually report significantly lower refund uncertainty, but there is no significant correlation between uncertainty and the *absolute* change in number of dependents. These results however are noisy enough to be consistent with taxpayer uncertainty being partially driven by changes in financial or family situations, which may result from how after-credit tax rates depend directly on family size and structure as well as income. In both columns 2 and 3, the coefficient estimates on third quartile of

[6]The coefficient on the indicator for age ¿ 50 is negative and marginally significant, but this pattern disappears after adding controls for change in filing status.

AGI and number of dependents remain statistically significant and of similar magnitudes as seen previously in column 1.

A natural question is whether the tax filers who reported greater uncertainty actually saw higher variance in their refund surprises. Columns 4-6 of Table 3.3 repeat the regressions in columns 1-3 with the absolute value of the tax filer's refund surprise as the dependent variable. Almost all variables which significantly predict refund uncertainty in columns 1-3 significantly predict absolute errors in the corresponding specification in columns 4-6, with the same sign and similar magnitudes. Number of dependents and income quartile remain the main predictors of refund surprise magnitudes, while demographic variables are less predictive. In addition, changes in AGI and changes in the number of dependents are significantly predictive of surprise sizes. Column 5 shows that tax filers with larger AGI changes saw larger surprises (30 dollars of uncertainty per 1,000 dollar change in AGI), as did filers with changes in number of dependents (1,000 dollars of additional uncertainty). In addition, surprise size is predicted by the direction of these changes. Households with increases in AGI and dependents actually saw smaller surprises. Taken together, these results suggest that different household types accurately assess the uncertainty they face as a result of unchanging family characteristics, but they may not fully update their beliefs or their subjective uncertainty based on changes in income and family structure.

Finally, columns 7-9 of Table 3.3 regress the surprise amount, rather than the magnitude, on the same sets of predictors to investigate whether certain types of households systematically over- or under-estimate the size of their tax refunds beliefs. Consistent with the finding in section 3.3.3 that expected refund amounts track realizations, most of the coefficient estimates on current tax filer characteristics are statistically insignificant. In particular, current AGI and number of dependents, which were predictive of refund uncertainty and surprise magnitudes, do not systematically predict the direction of mistakes. Though the coefficients on the second and third income quartiles are marginally significant in column 7, they become insignificant after controlling for the change covariates.

That said, there is one demographic variable which significantly predicts bias: married tax filers systematically overestimate their refund amounts by $1,000 relative to unmarried filers. Additionally, we find in columns 9 that changes in financial or family status are predictive of under- or over-estimates: filers whose AGI rose had lower surprises, while filers with an increase in their number of dependents saw higher surprises. In particular, tax filers whose incomes rose overestimated their refund amounts by 3 cents per each dollar of change in AGI, whereas filers underestimated their tax refund by more than 700 dollars for each additional dependent relative to the previous year. This is consistent with tax filers underestimating the slope of their refund with respect to characteristics: for example, the EITC claw-back rate as incomes rise and the generosity of EITC or child-tax credit benefits for additional dependents.

The above discussion yields several takeaways regarding tax refund expectations. First, the relationship between uncertainty and variables directly relevant to a household's tax liability, but not sociodemographic variables, suggests that uncertainty about how financial characteristics map to tax liabilities is common across a range of households with varied levels of sophistication and experience. Second, the types of households who report greater uncertainty also see larger refund surprises. Third, current taxpayer characteristics do not predict the direction of mistakes, with the exception of marital status; however, tax filers do not fully update about how changes in income and family structure affect their tax liabilities. A final observation is that even this broad set of tax filer attributes fails to explain all of the variation in uncertainty. The R-squared in column 3 – the highest across specifications – is 0.358, leaving substantial unexplained variation.

3.4 Borrowing and Consumption Responses to Tax Refunds

In this section we study how individuals' borrowing and consumption behavior around the time of tax filing responds to their expectations about, and actual realizations of, their tax refunds. In our sample of low-income tax filers, we find that roughly 15 cents per dollar of expected tax refund is used to repay revolving debt after tax refund receipt. In contrast, the unexpected ("surprise") component of tax refunds has a precisely estimated near-zero effect on revolving debt repayment. These results are consistent with individuals borrowing out of their expected tax refunds to smooth consumption over the course of the year, while also having a high propensity to consume out of windfall income in the form of tax refund surprises. Furthermore, we find that individuals exhibit precautionary behavior in their willingness to borrow out of expected tax refunds, as post-refund debt repayment is significantly more pronounced for individuals who report being more certain of their refund amount ex-ante.

Further exploring the effects of tax refund surprises, we find that surprises have a significantly positive effect on installment debt balances: unexpectedly high refunds lead to higher installment debt levels. This result is consistent with tax refunds partly being used to fund down payments for newly financed durable purchases, such as new auto loans. We use our follow-up survey of consumption behavior to corroborate this possible mechanism, finding that individuals with higher tax refund surprises indeed more frequently report that they bought a new car or initiated home repairs after tax filing. Summing the effects of refund surprises across both installment balances and revolving balances, we estimate that an additional dollar of refund surprise leads to an additional 40 cents of debt. While noisy, this estimate is significantly greater than zero and suggests a mechanism whereby medium-run MPCs can lie above 1 when windfall income is used to relax collateral constraints for new borrowing.

Finally, we test whether ex-ante uncertainty about tax refunds affects individuals' propensity to consume out of tax refund surprises. If individuals behave precautionarily and if the consumption function is concave in cash on hand, as predicted under a broad set of conditions (Zeldes, 1989; Carroll and Kimball, 1996), then individuals with more ex-ante uncertainty should have lower propensities to consume out of tax refund surprises. Our results are consistent with this prediction in sign, but underpowered, and we fail to reject the null that MPCs out of surprises are the same for different levels of ex-ante uncertainty.

3.4.1 Revolving Debt Repayment

We begin by examining financial behavior around tax filing by studying revolving debt balances.[7] These loans are sensible to examine first, as their balances are most readily adjustable over a short time horizon; we defer until section 3.4.2 a discussion of less easily adjustable installment debt.[8]

Using the linked panel of credit report data, we calculate the change in each tax filer's revolving debt balance between the credit report drawn just prior to tax filing and credit reports at subsequent two-month and six-month horizons. These provide short- and medium-run measures of responses to tax refunds. We then regress these two-month and six-month changes on three features of tax filers' beliefs and realizations of tax refunds: (1) their expectation of their tax refund amount, (2) their uncertainty about their tax refund amount as measured by the standard deviation of their elicited subjective probability distribution over refund amounts, and (3) the surprise in their realized tax refund relative to their expected refund. Debt changes are signed so that a negative change is a decrease in debt levels. Refund surprise is defined as realized tax refund minus expected tax refund; thus, a positive surprise is "good news" for the tax filer. We estimate regressions of the form

$$\Delta b_i = \alpha + \beta_1 \mu_i + \beta_2 \text{surprise}_i + \beta_3 \sigma_i + \gamma Z_i + \eta_i \tag{3.4.1}$$

where i indexes a tax filer; Δb_i denotes change in balances; μ_i is i's mean expected refund; σ_i is their subjective standard deviation; and surprise_i is their refund surprise. The vector Z_i controls for a range of interacted tax filer characteristics because household debt paths may differ at different stages of the lifecycle. We include fully interacted fixed effects for age group, income quartile, marital status, and whether an individual has dependents.[9] These interaction terms aim to absorb differences in levering or deleveraging over time that are due to differences between, for example, a young unmarried parent in the middle of our sample's income distribution, and a married elder at the bottom of our sample's income distribution. All residual variation is within a set of individuals who have similar lifecycle circumstances.[10] We also add controls for whether an individual received their refund by

[7] Revolving debt includes all loans with a flexible repayment schedule and an open line of credit that can be used flexibly over time and over purchases, including credit cards, retail store cards, and home equity lines of credit (HELOCs).

[8] Installment debt includes all loans with a fixed repayment schedule. These loans are often used to fund one-time purchases, including car loans, student loans, and mortgages.

[9] We group individuals into three age bins based on whether they are younger than 25, between 26 and 50, or over 50.

[10] The most notable omission from these lifecycle controls is arguably the variability of individuals' labor income. In future work, additional data cleaning of individuals' self-reported industry and/or occupation,

direct deposit or by paper check, and for an individual's treatment status in the randomized trial being conducted simultaneously at the tax site as discussed in section 3.2.1.

Table 3.4 reports estimates from specifications without and with lifecycle controls at a two-month horizon (columns 1 and 2) and a six-month horizon (columns 3 and 4). Column 1 indicates that for every dollar of expected tax refund, our sample repays roughly 15 cents in revolving balances after refund receipt. These estimates remain stable and significantly different from zero across both horizons and with the inclusion of lifecycle controls. These results quantify how revolving lines of credit are used to transfer a moderate share of expected tax refunds forward in time to fund earlier consumption.

Turning to the second row of the table, we see that surprises in tax refunds are *not* used to repay revolving debt. After including lifecycle controls (columns 2 and 4), we can reject more than 13 cents of refund surprise being put toward revolving debt repayment at a two-month horizon, and 21 cents at a six-month horizon. Considering the low savings levels in our sample (see section 3.2.4) this suggests that households have a marginal propensity to consume (MPC) of close to one out of cash on hand.[11] This is consistent with existing evidence of high MPCs from windfall income among low-income consumers (Jappelli and Pistaferri, 2014). We further explore such propensities to consume in section 3.4.2.

In the final row of the table we test for the presence of precautionary behavior in borrowing out of tax refunds. If tax filers are less certain of their tax refund amount ex-ante, they may borrow less of their expected refund before filing. We find that revolving balances are repaid less after refund receipt for more uncertain tax filers; for every dollar of standard deviation in a tax filer's subjective beliefs about their tax refund amount ex-ante, we estimate 35 to 40 cents less is used to repay debt ex-post. This pattern is consistent with uncertain tax filers precautionarily taking on less debt prior to filing their taxes. The estimated relationship between uncertainty and debt changes remains stable across both horizons and with the inclusion of lifecycle controls, although standard errors become larger at the six-month horizon. In dollar terms (results not shown), we estimate that having above-median refund uncertainty predicts roughly $275 less repaid toward revolving debt after refund receipt.

3.4.2 Installment Debt Repayment and Durable Consumption

We now turn our attention from revolving debt balances, such as credit card borrowing, to non-mortgage installment debt such as auto loans, retail loans, and student loans. We conduct the same analyses as in table 3.4 for installment debt instead of revolving debt, again estimating equation 3.4.1 with and without taxpayer controls at two-month and six-month horizons. We present these results in table 3.5.

together with our data on age, income level, and history of unemployment insurance receipt, will make it possible to impute a measure of labor income uncertainty using data such as the Panel Study of Income Dynamics (PSID) or Current Population Survey (CPS) for long-run or short-run income risk, respectively.

[11] Note that a coefficient of -1 on refund surprise would indicate an MPC of zero; tax filers would then be spending their entire refund surprise to pay down debt rather than changing consumption.

We find that higher (more positive) tax refund surprises lead to relative *increases* in installment debt. The effect of surprises on installment debt is near zero at a two-month horizon, but at a six-month horizon each additional dollar of tax refund surprise leads to an additional 65 cent increase in installment debt. This estimated effect remains stable as lifecycle controls are added in column 4. We reject a zero effect on installment debt changes in columns 3 and 4 with 95% and 90% confidence, respectively.

Other coefficient estimates for installment debt are noisier than for revolving debt. While we estimate that an even larger share of expected refunds are used for installment debt repayment than for revolving debt repayment, the estimates are not significantly different from zero or from the revolving debt estimates at reasonable confidence levels.[12] Similarly, the effect of uncertainty on borrowing behavior for installment debt cannot be distinguished from zero or from the estimated effects for revolving debt.

The result that positive refund surprises lead to rising installment balances is intriguing. We corroborate this relationship visually in figure 3.6.4, using a binned scatter plot to show conditional means of the dependent variable across bins of refund surprise after partialling out the controls in column 4 of table 3.5. The visual evidence strongly confirms the relationship between refund surprises and changes in installment debt. Across large and small, positive and negative surprises, an approximately linear relationship holds between surprises and installment debt changes, confirming that this pattern is not driven by non-linearity or outliers.

Given that installment debt is less adjustable over short horizons than revolving debt - it has a fixed repayment schedule, and taking out additional debt typically must coincide with a new purchase - this result suggests that positive tax refund surprises may be used to fund down payments on newly financed durable purchases. Conversely, a negative tax refund surprise may make an anticipated durable purchase no longer possible due to collateral constraints. This mechanism is illustrative of a case where medium-run MPCs out of windfall income can in fact be above 1, when such income is used to relax collateral constraints for new durable financing.

To investigate the durable purchases explanation, table 3.7 examines the relationship between tax refund surprises and self-reported durable purchases in the follow-up durable consumption survey we conducted approximately two months after tax filing. See section 3.2 for a survey description. This table follows a similar format to tables 3.4 and 3.5. While the coefficient estimates are not statistically significant at conventional levels, they are consistent ($p = 0.263$) with the interpretationthat higher refund surprises lead to a greater likelihood of new durable purchases after refund receipt.

To conclude this section, we pool both revolving and installment debt balances together and estimate the effects of tax refund expectations, uncertainty, and surprises on overall non-

[12]Greater rates of borrowing out of tax refunds through installment rather than revolving debt would be consistent with optimal consumption smoothing behavior when installment debt interest rates are lower than revolving debt interest rates (and when installment borrowing is sufficiently fungible to substitute for revolving debt).

mortgage debt balances. We estimate the same specifications as in tables 3.4 and 3.5, but with the total change in installment and revolving debt balances as the dependent variable.[13] Estimates for these specifications are shown in table 3.6. A moderate amount of each dollar of expected refund is used to repay debt shortly after refund receipt, suggesting that individuals use both revolving and installment debt to smooth consumption by borrowing out of refunds ex-ante. We also find evidence that individuals behave precautionarily in borrowing out of tax refunds, as this borrowing is less pronounced for individuals with more uncertainty ex-ante. However, unlike in our specification using only revolving debt, these differences are not statistically significant. Finally, we again find that more positive refund surprises lead to significantly higher total debt at a six-month horizon. Our preferred estimate when including lifecycle controls in column 4 is an additional 38 cents of total debt for each additional dollar of refund surprise. The modest amount of deleveraging on revolving debt does not overwhelm the larger, positive effect on installment debt balances. On net, positive surprises appear to lead to higher indebtedness in the medium term.

3.4.3 Further Tests of Precautionary Behavior

In this final subsection, we study the relationship between ex-ante uncertainty about tax refunds and individuals' later consumption out of tax refund surprises. This provides a further test of precautionary behavior. Here, we test a central prediction of the buffer stock consumption-savings model: the consumption function should be concave in cash on hand (Carroll and Kimball, 1996; Zeldes, 1989).

The logic of our test is that conditional on other characteristics, refund uncertainty is an instrument for initial debt levels. Suppose two tax filers have identical characteristics, mean refund expectations, and refund surprises, but have different levels of refund uncertainty at filing. With a precautionary savings motive, the filer with greater uncertainty should enter tax season with lower debt or higher assets to maintain a buffer against lower-than-expected tax refund realizations. Since the two filers are identical after refund uncertainty is realized, they should have the same consumption function, but the measured MPC[14] will be evaluated at different wealth levels. As a result, we should estimate a negative interaction between refund surprise and uncertainty after controlling for mean expectations and mean tax filer characteristics. We implement this test by adding an interaction between refund surprise and the subjective standard deviation of ex-ante expectations to equation 3.4.1:

$$\Delta b_i = \alpha + \beta_1 \mu_i + \beta_2 \text{surprise}_i + \beta_3 \sigma_i + \beta_4 \text{surprise}_i \times \sigma_i + \gamma Z_i + \eta_i \qquad (3.4.2)$$

[13] Individuals are included in this regression sample if they ever have either revolving loans or installment loans over our panel horizon, so the sample differs from that in our analyses of revolving debt and installment debt alone.

[14] Precisely, we measure changes in debt balances, and invoke the low levels of saving (in either real or financial assets) in our sample to translate from changes in debt balances to consumption. See also section 3.2.

Estimates from these specifications are shown in Table 3.8. Each pair of columns shows changes in debt at two-month and six-month horizons, and the three pairs of columns respectively show results for revolving debt, non-mortgage installment debt, and both debt categories together. The estimates are noisy, but we generally estimate negative signs on the interaction term that are consistent with precautionary behavior having induced lower MPCs. We conclude that this test for precautionary behavior is underpowered in our setting.

3.5 Conclusion

This paper uses a rich dataset linking administrative tax and credit data to surveys on taxpayer expectations and consumption behavior to shed new insight on low- and moderate-income households' choices to pay down debt, save, and consume. We showed that simple questions about an individual's expected tax refund can be used to generate rich probabilistic distributions that are informative about both mean expectations and uncertainty. We then showed that, in our sample of low-income filers, individuals face substantial uncertainty about the size of their tax refund. This is true despite the fact that annual refunds make up a substantial part of individuals' annual income and is true even for individuals whose tax situation has not changed since they last filed. Finally, we showed that refund expectations and surprises influence household financial decisions after tax filing. Filers use roughly 15 cents per dollar of expected tax refund to repay revolving debt after tax refund receipt. In contrast, refund surprises are not used to pay down debt, but rather, lead to higher borrowing through installment credit such as auto loans, consistent with MPCs potentially lying above one due to relaxed collateral constraints for financed durables. Post-refund debt repayment is most pronounced for less uncertain individuals, suggesting precautionary behavior.

There are two key limitations to our work. First, because of the small size of our sample, we are unable to generate precise estimates of the impact of uncertainty on consumption decisions. In particular, while our results are in line with buffer-stock consumption theory's predictions about how uncertainty should affect ex-ante borrowing out of expected refunds and ex-post propensities to consume out of surprises, our estimates remain somewhat noisy, especially in our tests for heterogeneous MPCs. Second, because we focus on individuals who take advantage of the city of Boston's free tax preparation services, our results may be specific to this population of low-income filers. The fact that these individuals sought out city services suggests that they may already be in a distressed financial situation. As a result they might be more responsive to income surprises than other individuals with similar incomes. However, the fact that they were aware of the city's services and were able to gather their paperwork and, in many cases, file their taxes months ahead of the deadline, suggests that they may be more conscientious on average. This would suggest that they may be less uncertain than the average taxpayer with their demographic characteristics.

One possible direction for future work would be to study how uncertainty and expectations evolve over time. While we were only able to collect one year of expectations data, it would be interesting to examine whether individuals' beliefs about their refunds become more precise if they file similar returns for several years. A second possibility would be to consider a broader sample of tax filers, who may have higher incomes or who may not not take advantage of free government-provided tax filing services. Finally, given the recent changes to the U.S. tax code, a complementary question is how taxpayers update their beliefs about their tax liabilities when the tax code itself – in addition to their own financial status – has changed.

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6 Figures and Tables

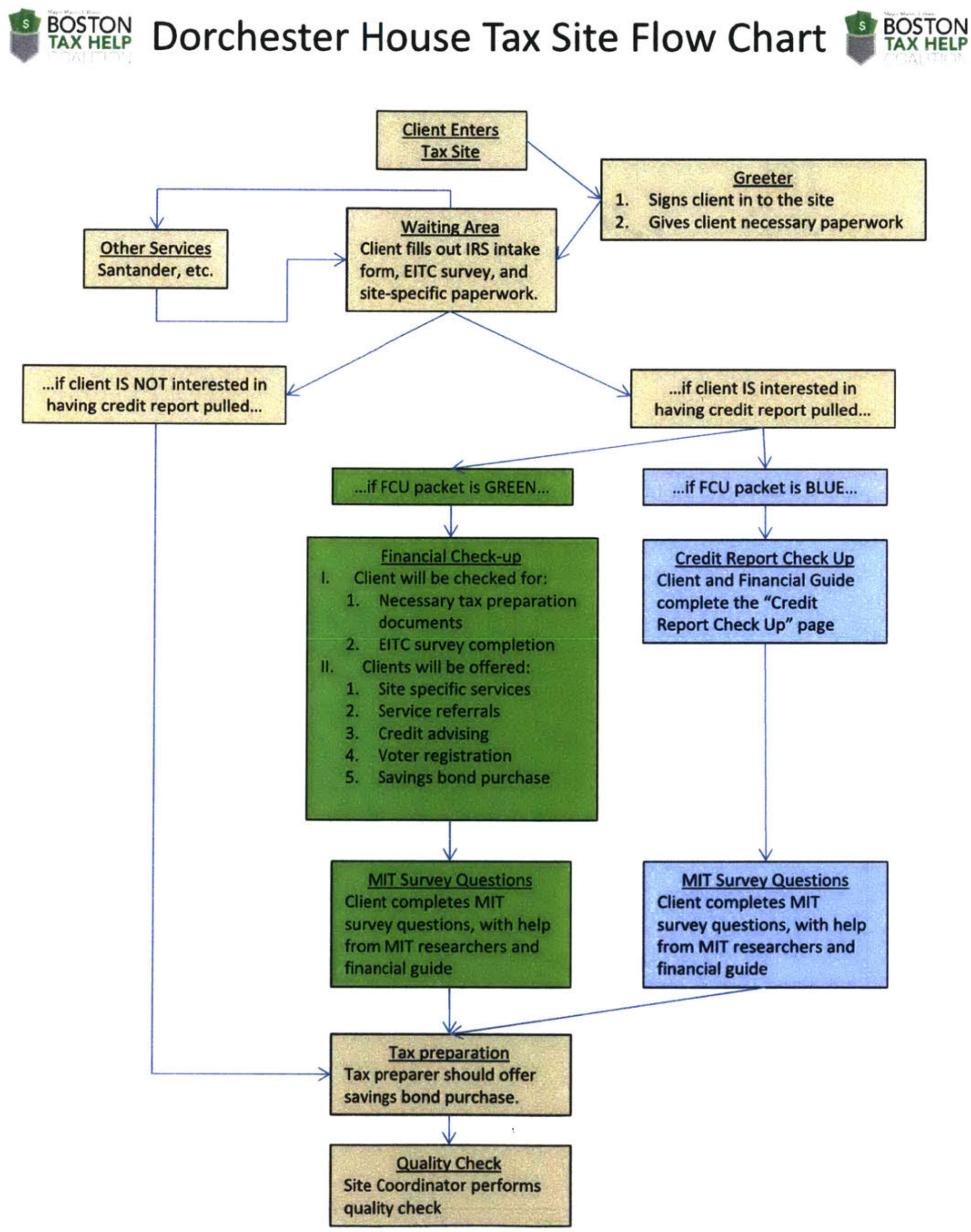

Figure 1: Dorchester House Tax Site Flow

Note: This figure shows the steps a Dorchester House tax client would go through upon arriving at the center.

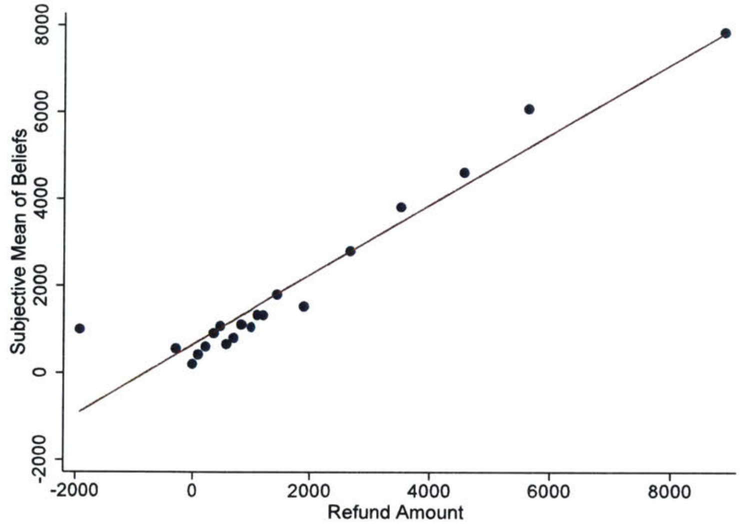

Figure 2: Expected Versus Actual Refunds
Note: This figure plots a binscatter of mean expectations against actual refund amounts. The expected refunds are the means of the distributions calculated using the procedure described in Section 3.

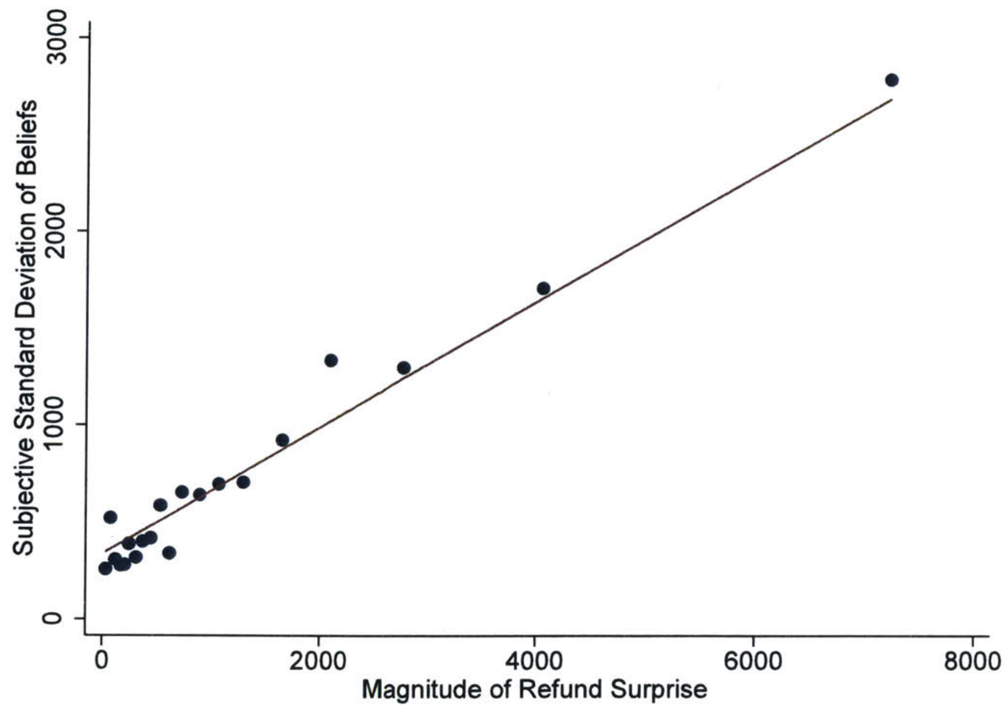

Figure 3: Refund Uncertainty and Refund Surprises
Note: This figure plots the size of the refund "surprise" (actual refund - mean expectation) against the standard deviation of beliefs.

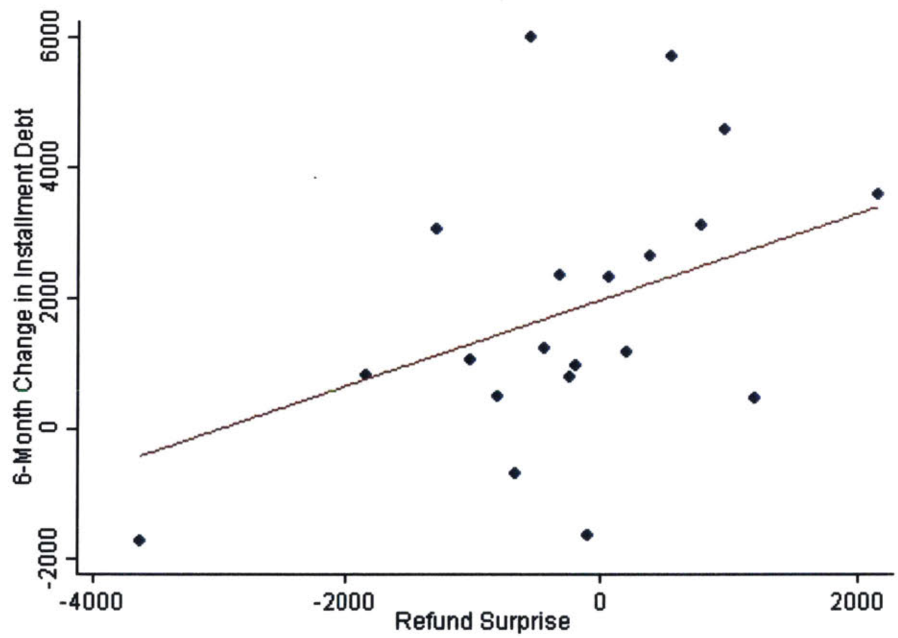

Figure 4: Refund Surprises and Changes in Installment Debt
Note: This figure plots a binscatter of 6-month changes in installment balalnces against tax refund surprises. Surprises are defined as realization minus expectation, such that a positive surprise is "good news" for the tax filer. These data are plotted after partialling out the other controls included in column (4) of Table 5.

Table 1: Descriptive Statistics

	Asset Survey Sample	Credit Report Sample	Expectations and Credit Reports	Consumption, Expectations, and Credit Reports	Past Year Taxes, Expectations, and Credit Reports
	(1)	(2)	(3)	(4)	(5)
Female	0.64	0.62	0.62	0.61	0.63
Age	40.91	40.92	40.71	40.50	42.15
	(15.66)	(15.69)	(15.85)	(16.24)	(15.62)
Adjusted Gross Income ($)	$21,603	$21,535	$21,572	$21,092	$23,784
	($15,962)	($15,924)	($15,988)	($16,152)	($15,938)
Has Dependents	34%	33%	33%	32%	36%
Filing Status					
Married	10%	9%	8%	9%	6%
Single Head of Household	29%	28%	28%	28%	32%
Filed Schedule C	6%	6%	7%	8%	6%
Refund Size	$1,765	$1,677	$1,635	$1,605	$1,849
	($2,406)	($2,413)	($2,384)	($2,484)	($2,446)
Received EITC	38%	37%	35%	33%	36%
EITC Refund (If >0)	$1,769	$1,724	$1,760	$1,777	$1,940
	($1,686)	($1,644)	($1,671)	($1,749)	($1,724)
Chose Direct Deposit	59%	60%	59%	56%	62%
Total Savings Balance	$518	$525	$524	$603	$540
	($562)	($568)	($572)	($599)	($578)
High School or Above	80%	83%	83%	84%	85%
Some College or More	15%	16%	15%	15%	17%
FICO Score		664	664	671	668
		(86)	(87)	(87)	(87)
Credit Card Balances ($)		$1,732	$1,672	$1,548	$1,840
		($4,888)	($4,845)	($3,994)	($5,493)
Installment Balances ($)		$9,205	$9,171	$9,306	$10,396
(non-mortgage)		($22,500)	($22,046)	($22,264)	($24,202)
Has Mortgage		4%	4%	5%	5%
Observations	995	714	625	291	424
Obs. with Asset Survey	995	626	557	253	383

Note: This table provides descriptive statistics on our population of low-income tax filers. The first column includes all individuals who visited Dorchester House and responded to the demographics and asset survey. The second column restricts the sample to the population for whom we have both initial and follow-up credit reports. The third column includes individuals who have both credit reports and completed expectations surveys. The fourth column includes individuals with credit reports, completed expectations surveys, and consumption surveys. The fifth column includes individuals who additionally could be matched to the preceding year's tax return by virtue of being return clients. In each column, gender, savings balance, and education are provided for the subset of individuals in that column who also completed the asset survey.

Table 2: Comparison of Qualitative and Quantitative Uncertainty

Qualitative Uncertainty	Quantitative Uncertainty				
		Coefficient of Variation		p-Value from t-test for Equality of Means	
	N	Mean	S.D.	Not Sure	Somewhat Sure
Not Sure at All	149	0.79	3.73		
Somewhat Sure	254	0.45	0.65	0.00038	
Very Sure	215	0.38	0.66	0.00002	0.00000

Note: This table compares the coefficient of variation calculated for the parametric belief distributions to the qualitative uncertainty responses. The sample includes all individuals who responded to the expectations survey.

Table 3: Correlates of Refund Uncertainty and Surprises

	Elicited Standard Deviation of Refund Amount			Magnitude of Refund Surprise			Refund Surprise		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
25 or Younger	62.90	-93.53	-66.94	19.34	304.2	485.8	297.0	398.8	487.4
	(108.2)	(161.9)	(160.3)	(209.0)	(317.4)	(315.8)	(271.9)	(434.9)	(430.3)
Older than 50	-171.2*	-134.7	-130.1	-166.6	136.2	79.31	308.7	222.2	143.8
	(98.69)	(126.3)	(123.8)	(190.3)	(247.9)	(244.2)	(247.6)	(339.6)	(332.7)
Any College	-24.99	-33.90	-2.697	101.3	143.9	151.1	204.2	172.0	33.05
	(83.72)	(113.3)	(112.2)	(161.9)	(222.9)	(221.8)	(210.6)	(305.4)	(302.2)
Female	-79.07	-89.00	-77.56	-51.55	-149.0	-162.2	157.5	229.0	204.9
	(87.97)	(124.4)	(121.4)	(170.4)	(244.4)	(239.7)	(221.6)	(334.9)	(326.5)
Has Dependent	347.8**	545.5**	516.7**	676.8**	1162.6***	823.1**	-172.1	-466.4	-504.0
	(168.0)	(219.0)	(211.2)	(323.5)	(428.8)	(415.7)	(420.8)	(587.5)	(566.4)
Number of Dependents	448.7***	321.0**	377.9***	494.9***	296.4	663.2***	312.2	565.7*	455.8
	(90.97)	(126.7)	(118.1)	(175.1)	(248.0)	(232.4)	(227.7)	(339.8)	(316.7)
Married	143.1	309.1	390.6	184.2	198.3	227.0	-967.3**	-1383.6**	-1598.6**
	(159.8)	(261.7)	(254.7)	(312.1)	(512.2)	(500.9)	(406.0)	(701.7)	(682.5)
AGI in 2nd Quartile	88.16	67.49	53.96	330.5	281.1	407.0	558.0*	696.2	891.5*
	(113.1)	(171.1)	(170.2)	(218.5)	(334.9)	(334.8)	(284.3)	(458.9)	(456.2)
AGI in 3rd Quartile	608.8***	489.7***	480.2***	1000.8***	925.2***	1031.3***	-686.8**	-422.3	-112.2
	(115.2)	(168.4)	(169.8)	(222.6)	(330.4)	(334.8)	(289.5)	(452.7)	(456.2)
AGI in 4th Quartile	289.4**	174.5	187.6	501.4**	93.67	304.4	-460.5	154.8	485.7
	(118.3)	(174.4)	(178.1)	(228.2)	(341.2)	(350.3)	(296.8)	(467.5)	(477.3)
Received UI in Past Year		27.56	28.98		-336.2	-278.9		752.2	758.6
		(221.5)	(216.7)		(433.5)	(426.2)		(593.9)	(580.7)
Change in Filing Status		-121.1	-228.1		-723.6	54.14		-43.27	69.35
		(266.0)	(187.6)		(520.7)	(369.1)		(713.4)	(502.9)
Magnitude of Change in AGI ($1,000)		8.298			28.48**			-6.706	
		(6.555)			(12.83)			(17.58)	
Any Change in Number of Dependents		-130.7			1011.5**			173.3	
		(235.8)			(461.5)			(632.3)	
Change in AGI ($1,000)			-1.726			-22.53**			-28.01**
			(5.206)			(10.24)			(13.95)
Change in Number of Dependents			-296.3***			-517.3***			736.2***
			(92.90)			(182.8)			(249.0)
Constant	256.3**	292.9*	280.2	326.7	124.1	154.7	-592.7**	-873.7*	-899.1*
	(114.5)	(177.3)	(172.1)	(220.6)	(347.4)	(338.8)	(286.9)	(476.0)	(461.6)
N	463	268	268	460	267	267	460	267	267
R-squared	0.348	0.333	0.358	0.222	0.281	0.301	0.072	0.083	0.118

Note: This table reports estimates from ordinary least squares regressions of refund uncertainty and surprises on tax filer characteristics. The sample in columns 1, 4, and 7 is all Dorchester House tax filers who completed the assets and beliefs surveys. The sample in the remaining columns is the subset of those tax filers who could be linked to the previous year's tax return by virtue of being a repeat client. The Elicited Standard Deviation of Refund Amount (columns 1-3) is the standard deviation of the parametric belief distribution fit to each tax filer's probabilistic survey question response. * $p < .1$ ** $p < 0.05$ *** $p < 0.01$

Table 4: Impact of Refund Surprise on Revolving Debt Balances

	Dependent Variable: Change in Revolving Debt ($)			
	2-Month Follow-Up		6-Month Follow-Up	
	(1)	(2)	(3)	(4)
Mean Expectation ($)	-0.150***	-0.103*	-0.145**	-0.142*
	(0.0457)	(0.0569)	(0.0671)	(0.0826)
Surprise ($)	-0.0677*	-0.0393	-0.0831	-0.0831
	(0.0370)	(0.0462)	(0.0546)	(0.0674)
S.D. of Beliefs	0.395**	0.378**	0.377*	0.358
	(0.154)	(0.168)	(0.226)	(0.243)
Controls for Taxpayer Characteristics		X		X
R-Squared	0.042	0.198	0.021	0.204
N	302	302	301	301

Note: This table reports estimates from ordinary least squares regressions of changes in revolving debt balances on tax filer expectations, refund surprises, and characteristics. The sample in all columns is individuals with non-missing data on demographics as measured in the asset survey, expectations, and an open revolving loan observed at any point during the sample period. Balances of zero are assigned to loans reported as closed with no balance. In columns 1 and 2, the dependent variable is the change in revolving debt between the week of tax filing and the two-month credit report follow-up. Column 1 controls for the mean and standard deviation of each tax filer's parametric belief distribution, fit as described in Section 3.2, as well as their refund surprise. Column 2 adds controls for tax filer characteristics. Columns 3 and 4 repeat these specifications for the six-month change in revolving debt. Tax filer characteristics are fully interacted bins of age less than 25, 25-50, and over 50; adjusted gross income (AGI) quartile; marital status; and an indicator for any dependents. Standard errors are in parentheses. * $p < .1$ ** $p < 0.05$ *** $p < 0.01$

Table 5: Impact of Refund Surprise on Installment Debt Balances

	Dependent Variable: Change in Installment Debt ($)			
	2-Month Follow-Up		6-Month Follow-Up	
	(1)	(2)	(3)	(4)
Mean Expectation ($)	-0.128	-0.233	-0.0231	-0.303
	(0.150)	(0.196)	(0.325)	(0.416)
Surprise ($)	0.0599	0.0318	0.676**	0.659*
	(0.125)	(0.158)	(0.272)	(0.338)
S.D. of Beliefs	-0.353	-0.146	-0.539	-0.374
	(0.478)	(0.527)	(1.036)	(1.115)
Controls for Taxpayer Characteristics		X		X
R-Squared	0.036	0.218	0.043	0.260
N	216	216	215	215

Note: This table reports estimates from ordinary least squares regressions of changes in installment debt balances on tax filer expectations, refund surprises, and characteristics. The sample in all columns is individuals with non-missing data on demographics as measured in the asset survey, expectations, and an open non-mortgage installment loan observed at any point during the sample period. Balances of zero are assigned to loans reported as closed with no balance.. In columns 1 and 2, the dependent variable is the change in installment debt between the week of tax filing and the two-month credit report follow-up. Column 1 controls for the mean and standard deviation of each tax filer's parametric belief distribution, fit as described in Section 3.2, as well as their refund surprise. Column 2 adds controls for tax filer characteristics. Columns 3 and 4 repeat these specifications for the six-month change in installment debt. Tax filer characteristics are fully interacted bins of age less than 25, 25-50, and over 50; adjusted gross income (AGI) quartile; marital status; and an indicator for any dependents. Standard errors are in parentheses. * $p < .1$ ** $p < 0.05$ *** $p < 0.01$

Table 6: Impact of Refund Surprise on Non-Mortgage Debt Balances

	Dependent Variable: Change in All Non-Mort. Debt ($)			
	2-Month Follow-Up		6-Month Follow-Up	
	(1)	(2)	(3)	(4)
Mean Expectation ($)	-0.163**	-0.106	-0.223	-0.348
	(0.0772)	(0.0960)	(0.206)	(0.255)
Surprise ($)	-0.0194	0.0303	0.384**	0.382*
	(0.0641)	(0.0813)	(0.172)	(0.217)
S.D. of Beliefs	0.183	0.165	0.673	0.789
	(0.256)	(0.272)	(0.683)	(0.723)
Controls for Taxpayer Characteristics		X		X
R-squared	0.024	0.158	0.021	0.167
N	352	352	351	351

Note: This table reports estimates from ordinary least squares regressions of changes in total non-mortgage debt balances on tax filer expectations, refund surprises, and characteristics. The sample in all columns is individuals with non-missing data on demographics as measured in the asset survey, expectations, and an open revolving or non-mortgage installment loan observed at any point during the sample period. Balances of zero are assigned to loans reported as closed with no balance.. In columns 1 and 2, the dependent variable is the change in debt between the week of tax filing and the two-month credit report follow-up. Column 1 controls for the mean and standard deviation of each tax filer's parametric belief distribution, fit as described in Section 3.2, as well as their refund surprise. Column 2 adds controls for tax filer characteristics. Columns 3 and 4 repeat these specifications for the six-month change in debt. Tax filer characteristics are fully interacted bins of age less than 25, 25-50, and over 50; adjusted gross income (AGI) quartile; marital status; and an indicator for any dependents. Standard errors are in parentheses. * $p < .1$ ** $p < 0.05$ *** $p < 0.01$

Table 7: Impact of Refund Surprise on Durable Purchases

	Dependent Variable: Survey-Reported Durables Purchase	
	2-Month Follow-Up	
	(1)	(2)
Mean Expectation ($)	0.0000143	0.0000197
	(0.0000138)	(0.0000176)
Surprise ($)	0.00000182	0.00000873
	(0.0000118)	(0.0000148)
S.D. of Beliefs	-0.0000228	-0.0000398
	(0.0000442)	(0.0000476)
Controls for Taxpayer Characteristics		X
R-squared	0.003	0.127
N	443	443

Note: This table reports estimates from ordinary least squares regressions of an indicator for durable purchases between tax filing and the two-month follow-up consumption survey. Controls are tax filer expectations, refund surprises, and characteristics. The sample in all columns isindividuals with non-missing data on demographics as measured in the asset survey, expectations, and a response to the follow-up consumption survey conducted approximately two months after tax refund receipt. Column 1 controls for the mean and standard deviation of each tax filer's parametric belief distribution, fit as described in Section 3.2, as well as their refund surprise. Column 2 adds controls for tax filer characteristics. Tax filer characteristics are fully interacted bins of age less than 25, 25-50, and over 50; adjusted gross income (AGI) quartile; marital status; and an indicator for any dependents. Standard errors are in parentheses. * $p < .1$ ** $p < 0.05$ *** $p < 0.01$

Table 8: Testing Concavity of the Consumption Function

Dependent Variable	Change in Revolving Debt		Change in Installment Debt		Change in All Non-Mort. Debt	
Horizon	2-Mo.	6-Mo.	2-Mo.	6-Mo.	2-Mo.	6-Mo.
	(1)	(2)	(3)	(4)	(5)	(6)
Surprise ($)	-0.0697	-0.112	0.0839	0.922**	0.0291	0.541**
	(0.0569)	(0.0833)	(0.197)	(0.422)	(0.102)	(0.272)
Surprise * S.D. of Beliefs	0.0000395	0.0000376	-0.0000639	-0.000319	0.00000151	-0.000190
	(0.0000431)	(0.0000628)	(0.000144)	(0.000305)	(0.0000735)	(0.000196)
S.D. of Beliefs	0.409**	0.387	-0.197	-0.627	0.166	0.622
	(0.172)	(0.249)	(0.540)	(1.141)	(0.280)	(0.743)
Controls for Taxpayer Characteristics	X	X	X	X	X	X
Controls for Mean Expectation	X	X	X	X	X	X
R-squared	0.200	0.205	0.219	0.264	0.158	0.170
N	302	301	216	215	352	351

Note: This table reports estimates from ordinary least squares regressions of changes in debt balances on tax filer expectations, refund surprises, and tax filer characteristics. The sample in all columns is individuals with non-missing data on demographics as measured in the asset survey, expectations, and an open loan (either revolving loan, installment loan, or either type of loan, depending on the column) observed at any point during the sample period. Balances of zero are assigned to loans reported as closed with no balance. In columns 1 and 2, the dependent variable is change in two- and six-month revolving debt balances, respectively. Both columns control for the mean and standard deviation of each tax filer's parametric belief distribution, fit as described in Section 3.2, as well as their refund surprise, an interaction between the surprise and belief standard deviation, and tax filer characteristics. Columns 3 and 4 repeat these specifications for changes in installment debt balances, and columns 5 and 6 do the same for all non-mortgage debt. Tax filer characteristics are fully interacted bins of age less than 25, 25-50, and over 50; adjusted gross income (AGI) quartile; marital status; and an indicator for any dependents. Standard errors are in parentheses. * $p < .1$ ** $p < 0.05$ *** $p < 0.01$

Table A1: Elicited Beliefs by Tax Filer Group

	Features of Probabilistic Survey Question Responses								
	Full Sample	Has Dependents		Marital Status		Adjusted Gross Income (AGI)		Education	
		Yes	No	Married	Single	Above $20,000	Below $20,000	Some College	No College
Number of Bins with Positive Probability									
1 Bin	22.2%	24.0%	21.4%	28.3%	21.7%	24.7%	20.1%	21.2%	23.0%
2 Bin	38.8%	39.9%	38.3%	32.6%	39.3%	34.4%	42.5%	37.3%	39.9%
3 Bin	20.3%	14.2%	23.2%	13.0%	20.9%	18.1%	22.1%	18.7%	21.5%
4 Bin	11.8%	12.0%	11.7%	13.0%	11.7%	13.9%	10.1%	14.5%	9.8%
5 Bin	5.5%	8.2%	4.2%	8.7%	5.2%	6.9%	4.2%	6.6%	4.6%
6 Bin	1.4%	1.6%	1.3%	4.3%	1.2%	1.9%	1.0%	1.7%	1.2%
Qualitative Uncertainty									
Very Sure	34.4%	30.6%	36.3%	48.0%	33.2%	29.9%	38.4%	32.4%	35.8%
Somewhat Sure	40.6%	47.6%	37.2%	34.0%	41.2%	43.2%	38.4%	40.2%	41.0%
Not Sure at All	23.8%	21.4%	25.1%	18.0%	24.3%	25.5%	22.4%	25.9%	22.4%
Quantitative Responses									
Point Estimate	1,758	3,466	921	2,336	1,708	2,377	1,202	1,753	1,762
Minimum	-364	1,071	-1,048	-304	-369	-75	-607	-527	-244
Maximum	5,922	10,885	3,557	7,783	5,758	7,851	4,300	6,344	5,610
Features of Parametric Distribution									
Mean	1,970	4,211	902	2,891	1,889	2,817	1,258	1,995	1,952
Median	2,073	4,225	1,047	2,768	2,011	2,889	1,386	2,089	2,061
Std. Dev.	740	1,475	390	1,052	713	1,023	502	803	693
Coefficient of Variation	0.50	0.33	0.58	0.35	0.51	0.37	0.61	0.57	0.45

Notes: This table reports responses to the beliefs survey. All statistics are means within each group. The last panel contains statistics based on the parametric distributions fit to the probabilistic survey question described in Section 3.

Table A2: Parametric Belief Distributions

	Features of Elicitations under Alternative Parametric Assumptions and Sample Restrictions											
	Baseline		Uniform		Lower Bound		Upper Bound		Omitting Only Top/Bottom Bin		Omitting 50-50 Reports	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Mean	1,970	2,850	2,312	3,297	1,098	1,418	3,527	5,212	1,566	2,047	2,061	2,992
Median	2,073	3,369	2,066	3,171	996	1,456	3,179	5,413	1,713	2,874	2,179	3,554
Std. Dev.	740	1,019	2,312	2,976					631	885	799	1,075
Minimum	548	1,261	525	1,251					353	816	553	1,315
Maximum	5,247	6,765	5,922	7,333					4,641	6,111	5,698	7,093

Notes: This table reports features of parametric belief distributions under alternative assumptions. Statistics are aggregated across all tax filers in the main analysis sample. The first pair of columns contains statistics based on the parametric distributions fit to the probabilistic survey question described in Section 3. Uniform assumes a uniform distribution within each bin with nonzero probability. Lower (Upper) bound calculates the lowest (highest) value of each tax filer's subjective mean and median expectation that is consistent with their subjective probabilities. The last two pairs of columns implement sample restrictions using the baseline parametric assumptions.

B Survey Appendix

B.1 On-Site Survey

PLEASE DO NOT WRITE IN THIS AREA

02403

Mayor Martin J. Walsh
BOSTON TAX HELP COALITION

2015 BOSTON TAX HELP SURVEY

The overall purpose of this survey is to inform how the services and resources available in the community can be improved. Your answers will be added with the answers we receive from other surveys. We will never share your personal information with anyone without your permission.

FEDERAL RESERVE BANK OF BOSTON™

• USE A NO. 2 PENCIL ONLY

- Make solid marks that fill the oval completely
- Erase cleanly any marks you wish to change

1. GENDER? ○ Female ○ Male

2. ARE YOU A VETERAN? ○ Yes ○ No

3. ARE YOU REGISTERED TO VOTE? ○ Yes ○ No

4. HOW WOULD YOU DESCRIBE YOUR RACE OR ETHNICITY? (please mark all that apply)
○ Asian/Pacific Islander ○ Hispanic/Latino ○ White ○ African
○ Black/African American ○ Native American ○ Caribbean ○ Other ______

5. HOW FLUENT ARE YOU IN ENGLISH?
Please rate from 0 (beginner) to 5 (native speaker): **Beginner** ⓪ ① ② ③ ④ ⑤ **Native Speaker**

6. WHAT IS THE HIGHEST LEVEL OF EDUCATION YOU HAVE COMPLETED? (please mark one)
○ 0–8 grades ○ High School grad or GED ○ Associate's Degree ○ Some graduate school
○ 9–12 grades (no diploma) ○ Some college ○ Bachelor's Degree ○ Graduate degree

7. DO YOU WANT TO CONTINUE YOUR EDUCATION OR VOCATIONAL TRAINING? (please mark all that apply)
○ Yes, but I don't have enough time
○ Yes, but I am not sure investing in additional education or training will pay off for me
○ Yes, but I don't have enough money to cover all the cost
○ No, I am all set

8. LAST YEAR, WHAT BENEFITS DID YOU OR YOUR FAMILY RECEIVE? (please mark all that apply)
○ WIC ○ SNAP/food stamps ○ TAFDC ○ Child Care Vouchers ○ SSI ○ SSDI ○ None

9. HOW MANY MONTHS WERE YOU EMPLOYED LAST YEAR?
Full-time: ○ 0 ○ 1–3 ○ 4–6 ○ 7–9 ○ 10–12
Part-time: ○ 0 ○ 1–3 ○ 4–6 ○ 7–9 ○ 10–12

10. WHICH OF THE FOLLOWING BEST DESCRIBES YOUR HOUSING STATUS? (please mark one)
○ Rent, no subsidy ○ Rent, public housing ○ Living with family or friends ○ Homeless, in shelter ○ Own a home with a mortgage
○ Rent, Section 8 ○ Rent, other subsidy ○ Assisted Living Facility ○ Homeless, no shelter ○ Own a home with no mortgage

11. DID YOU LOSE YOUR HOUSING THROUGH EVICTION OR FORECLOSURE IN THE LAST YEAR? (please mark all that apply)
○ Yes, foreclosure ○ Yes, eviction ○ No

12. DID YOU FILE TAXES LAST YEAR (2014)? ○ Yes ○ No **If No, Then Skip To 15.**

13. Did you receive a refund for the 2014 tax year?
○ Yes, and it was more than $1,000
○ Yes, and it was less than $1,000
○ No **If No, Then Skip To 15.**
○ Don't Remember

14. If you received a refund, were you able to use it as you intended?
○ Yes, pretty much as I had planned
○ No, I had planned to save more of my refund
○ No, I had planned to use it for something else but ended up spending most of it on bills
○ I did not have any specific plans for the refund
○ I don't remember

***15. If you get a tax refund this year, what do you plan to do with the money?* (please mark all that apply)**
○ Buy groceries
○ Pay child expenses (including K-12 school fees)
○ Buy a car
○ Car repairs/expenses
○ Save for emergencies
○ Pay bills
○ Pay medical bills
○ Pay moving expenses
○ Pay down debt
○ Go on vacation
○ Home improvement
○ Pay for college
○ Save for college
○ Save for retirement
○ Other ______

16. DO YOU HAVE A BANK ACCOUNT?
○ Checking & Saving
○ Just checking
○ Just savings
○ No, but I am interested in one **If No, Then Skip To 21.**
○ No, not interested **If No, Then Skip To 21.**

***17. If you have bank account(s), with whom do you bank?* (please mark all that apply)**
○ Bank of America
○ Citibank
○ Citizens
○ City of Boston Credit Union
○ East Boston Savings/Mt. Washington
○ Eastern
○ TD Bank
○ One United
○ Santander
○ Cathay
○ Mass State Employees Credit Union
○ Metro Credit Union
○ Capital One 360
○ Century Bank
○ Webster First Financial
○ Other ______

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PLEASE CONTINUE ON BACK

18. *Did you pay an overdraft fee in the past 12 months?*

O Yes, once O Yes, more than once O No

19. *If you have bank account(s), how much money do you regularly keep in it (them) all together?*

O $0 O $1 - $100 O $101 - $500 O $501 - $1,000 O More than $1,000

20. *On a monthly basis, how much money do you regularly put into savings?*

O $0 O Between $1 - $50 O Between $51 - $100 O More than $100

If you have a bank account, Then Skip To 22.

21. *If you do not have bank account(s), why not?* (please mark all that apply)

O Overdraft fees
O Other fees
O I don't trust them
O Goes against my religious beliefs
O I don't think I can get an account
O They don't speak my language
O Locations or hours are not good for me
O Isn't really worth it
O Other ________________

22. IN THE EVENT OF A FINANCIAL HARDSHIP, DO YOU KNOW ANYONE LIKELY TO LOAN YOU $200 OR MORE IF YOU ASKED? (please mark one)

O No, Nobody
O Yes, Maybe 1 or 2 people
O Yes, Maybe 3 or 4 people
O Yes, Maybe 5 or more people

23. *In 2014, did you have a friend or relative that borrowed $200 or more from you because of a financial hardship?*

O No
O Yes, and I expect to get paid back
O Yes, and I do not expect to get paid back

24. DO YOU HAVE ONE OR MORE CREDIT CARDS?

O Yes, one credit card
O Yes, more than one credit card
O No, but I want a credit card **If no, Then Skip To 28.**
O No, I don't want a credit card **If no, Then Skip To 28.**

25. *How much do you usually pay each month on your highest balance card?*

O The full balance O More than the minimum payment O The minimum payment O Less than the minimum payment

26. *What is your current outstanding balance on all credit cards?*

O Less than $1,000 O $1,000 - $5,000 O $5,000 - $10,000 O More than $10,000 O Unsure

27. *What is the interest rate you are charged on your credit card with the highest balance? If you are unsure, please approximate.*

O Less than 10% O 10% - 14.9% O 15% - 19.9% O More than 20% O Don't Know (Can't even guess)

28. DID YOU EXPERIENCE ANY OF THE FOLLOWING IN THE LAST YEAR (2014) WITH CREDIT CARD(S)? (please mark all that apply)

O Missed a credit card payment
O Went over the credit card limit
O Credit Limit decreased
O Credit Limit increased
O Interest Rate changed
O Offered a credit card to pay a medical expense
O I don't know
O Did not have a credit card in the last year (2014)

29. *Did you use a credit card in the last year (2014) for any of the following reasons because you had no access to cash to pay for them?* (please mark all that apply)

O Medical Bills O Prescriptions O Groceries O Utilities O Phone O Did not have a credit card in the last year (2014)

30. DO YOU KNOW WHAT A CREDIT SCORE IS?

O No O Yes, I know but it is not important for me O Yes, I know and it is important for me

31. How would you rate your credit score?

O Very Bad
O Bad
O Fair
O Good
O Very Good

32. How easy do you think it is to improve your credit score?

O Very Easy
O Easy
O Neutral
O Hard
O Very Hard

33. How often do you get your credit report?

O Never **If never, Then Skip To 35.**
O Less than once per year
O Once per year
O More than once per year

34. How do you get your credit report?

O Request it on-line through AnnualCreditReport.com
O Request it by mail through AnnualCreditReport.com
O Access it online for a fee
O A different method
O Through the tax sites

35. DURING 2014, DID YOU DO ANY OF THE FOLLOWING? (please mark all that apply)

O Attend a credit counseling session
O Experience a financial crisis
O Filed a credit report complaint
O Lose a job
O Open a checking account
O Open a retirement account
O Open a savings account
O Pay a bill over the internet
O Purchase a Money Order
O Purchase a US Savings Bond
O Received a court order for debt collection
O Received a notice for debt you don't owe
O Received threatening phone calls for debt collection
O Take a class on personal finances
O Take a loan from a payday lender
O Take out a loan from a bank
O Use a Check Casher (Western Union, PL$, MoneyGram, etc.)
O Use Direct Deposit (paycheck, gov't check, etc.)
O Used a pre-paid debit card with fees
O Used mobile banking on a smartphone

36. WOULD YOU LIKE HELP WITH ANY OF THE FOLLOWING? (please mark all that apply)

O Emergency/short term loans
O Foreclosure help/prevention
O Small business loans
O Credit advising
O Low cost-prepaid cards
O Helping me work with my bank
O Budgeting help
O Debt Management
O Continuing education
O Employment

Thank you for your participation

B.2 Expectations Survey

The expectations survey consisted of four questions, printed below. The survey was administered by the financial guides at Dorchester House. Along with the answers to these four questions, financial guides recorded each individual's tax client number so that the survey could be linked to the other data we collected.

1) If you get a tax refund this year, how much do you think it will be? Please choose an amount:

$____________

(Financial Guide volunteer: please write $500 above this number, and $500 below this number, in the two blank lines in the question below)

2) How sure are you that your refund will be between $_______ and $_______? Please circle one:

NOT SURE AT ALL   SOMEWHAT SURE   VERY SURE

3) Suppose you want to make some extra money by working more hours next week. Do you think you could you get your manager/supervisor to schedule you for more hours?

YES

NO

I AM NOT WORKING RIGHT NOW

I AM NOT PAID HOURLY

4) We have one final question about your tax refund. Below we show six possible amounts that your refund could be (for example, "between $1000 and $2500"). For each of the six possibilities, please say what is the "percent chance" that you think your refund could be that amount:

Could my refund be...	**(Please Enter % Chance for Each)**
Over $5000	%
Between $2500 and $5000	%
Between $1000 and $2500	%
Between $500 and $1000	%
Between $0 and $500	%
Negative: I will owe taxes	%

B.3 Follow-Up Survey

The follow-up survey was conducted via phone by a Dorchester House volunteer. After introducing herself and reading the consent statement, the volunteer went through a pre-specified script and coded the answers into a spreadsheet. Individuals who completed the survey were mailed a $10 gift card.

Consent Statement: The survey information will be stored securely at the City of Boston's Office of Financial Empowerment, will be kept confidential, and will only be accessed by OFE employees. The information will also used as part of ongoing research with researchers at MIT. All survey questions are voluntary and you can stop the survey at any time. Participation will not affect your eligibility for city services. The survey should take about 4 minutes. To thank you for your participation, you will be given a $10 gift card at the end of the survey.

Questions

1. Have you made any of the following large purchases in 2016?

   (a) Car or motorcycle
   (b) Large household appliance, for example a dishwasher, refrigerator, or clothes dryer
   (c) A major repair to your home or the place you live
   (d) Television or computer
   (e) Car repairs
   (f) Wedding, funeral, or party expenses
   (g) [Repeat for each of the items purchased:]

      i. About when was it that you purchased? How certain are you of this date?
      ii. How much did it cost?
      iii. How did you pay for it? (cash/check/credit...)

2. Have you faced any unexpected expensive life events, such as job loss, job change, or medical bills, in 2016?

   (a) [Repeat for each event:]

      i. About when did — happen? How certain are you of this date?
      ii. If applicable: how much did the expense cost, and how did you pay for it?

3. About what time did you receive your tax refund this year?

   (a) How long was it after you first came to Dorchester House to file taxes?

4. Did you use your tax refund to put more money in a savings or checking account?
5. OK, now I have just one last question about the things we've discussed so far.

   (a) [Repeat for each large purchase or life event in Questions 1 and 2:]

      i. Do you recall if — was before or after you got your tax refund?
      ii. Do you recall if that was before or after you came to Dorchester House to file taxes?