The Equilibrium Effect of Information in Consumer Credit Markets: Public Records and Credit Redistribution*

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Abstract

Information in credit reports determines the availability and cost of credit and impacts many other areas of consumers' lives. Yet relatively little is known about what happens to consumers and the market equilibrium when the information contained in credit reports changes. We use the July 2017 nationwide purge of non-bankruptcy public records from credit reports to estimate the individual and equilibrium effects of information removal in consumer credit markets. To guide our analysis, we develop a model that implies that consumers who did not have a public record prior to the purge lost more distinguishing information in the purge if they were in a submarket with a higher share of consumers with a public record. Empirically, we find three different effects on the market: (1) Using the exogenous removal of non-bankruptcy public records during the purge, we find that the likelihood of having a credit card and an auto loan increased by 2.3 and 1.3 percentage points, respectively, for the 11.8 percent of low-credit-score consumers losing a public record. (2) For these consumers, there was also a 7.9 percent increase in existing credit limits and a 14.7 percent increase in credit card debt, with three quarters of this response being an endogenous dynamic response to credit expansion. (3) Among consumers who did not lose a public record, there was significant redistribution of credit from consumers in a submarket with a higher share of consumers with a public record to consumers in a submarket with a lower share.

JEL: G5; G2; D14; Keywords: Credit score; Public records; Market equilibrium; Pooling; Credit allocation; Credit use

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1 Introduction

Information in credit reports determines the availability and cost of credit for consumers. Moreover, credit report information is often used in employment and rental decisions and in setting mobile phone, utility, and insurance contracts, so a bad credit report can have a substantial negative impact on a consumer's life beyond formal credit. By allowing for a better allocation of scarce resources, including more information in credit reports that is predictive of credit risk may have positive social consequences. Yet there is a wide policy consensus that not all information that might predict risk should be included in credit reports or used in credit scoring models. For example, the Fair Credit Reporting Act requires consumer reporting companies (CRCs) to remove most negative information after seven years, whether or not it is still predictive of risk, and the Equal Credit Opportunity Act prohibits using race, gender, and marital status in making credit determinations, including in credit scoring. All the while, in recent years CRCs have increased the scope of their data offerings, for example, by providing data on payment histories from rentals, utilities, and mobile phone contracts and on performance histories from short-term credit products such as payday loans traditionally not included in credit reports.[1]

This paper advances the understanding of information provision in consumer credit markets by studying the nationwide purge of non-bankruptcy public records from credit records in July 2017. Because the change in information was extensive—11.8 percent of consumers with credit scores below 700 ("low-credit-score" consumers) had a non-bankruptcy public record—we are able to estimate both the direct effect of the purge on consumers who had a public record removed and the indirect or equilibrium effect of the purge on consumers who did not have a public record in the five years leading up to the purge. To guide our analysis, we develop an illustrative model that

[1] For example, in June of 2010, Experian purchased RentBureau, a specialty credit reporting agency focusing on the reporting of rental payment histories. Soon after, Experian started to incorporate positive rental payment data in consumer credit records. In October of 2017, Experian acquired Clarity, a specialty credit reporting agency focusing on alternative financial services, such as small-dollar loans, point-of-sale financing, and auto-title loans. Most recently, in March of 2019, Experian introduced its Boost product that allows consumers to elect to include their utility and telecom bill payment histories in their credit records. See https://www.experian.com/rentbureau/experian-rentbureau-press-releases.html, https://www.autoremarketing.com/financial-services/experian-acquires-clarity-services-launches-alternative-data-driven-solution, and https://www.experian.com/blogs/news/2019/03/18/experian-boost-launch.

implies that consumers who did not have a public record prior to the purge lost more distinguishing information in the purge if they were in a submarket with a higher share of consumers with a public record and so faced higher exposure to information reduction from the purge. To estimate these effects, we exploit the fact that submarkets' exposure to the purge varied significantly across the credit score distribution and across states. We find three different effects on the market. (1) Using the exogenous removal of non-bankruptcy public records during the purge, we find that the likelihood of having a credit card and an auto loan increased by 2.3 and 1.3 percentage points, respectively, for the 11.8 percent of low-credit-score consumers losing a public record. (2) For these consumers, there was also a 7.9 percent increase in existing credit limits and a 14.7 percent increase in credit card debt, with three quarters of this response being an endogenous dynamic response to credit expansion. (3) Among consumers who did not lose a public record, there was significant redistribution of credit from consumers in a submarket with a higher share of consumers with a public record to consumers in a submarket with a lower share. In other words, more information does not have unambiguously positive effects on consumers and changes in information can have substantial redistributive effects.

Traditionally, public records included in credit records were derived from court filings and included bankruptcies, civil judgments, and state and federal tax liens. In July 2017, the three nationwide CRCs removed all civil judgments from their credit records and about half of all tax liens. The following March, the CRCs removed the remaining tax liens. These changes to credit reporting were part of the CRCs' National Consumer Assistance Plan (NCAP) which was the result of a 2015 settlement between the CRCs and the state attorneys general led by New York. The settlement required the CRCs to take steps to improve the accuracy of their credit records. We focus specifically on the non-bankruptcy public records affected by the NCAP purge and refer to them for the rest of the paper as *public records* for convenience. While only 6.3 percent of the total population had a public record before the purge, 11.8 percent of the low-credit-score population (with a credit score of 700 or below) had one. Clarkberg and Kambara (2018) estimate that the modal credit score change among credit records that lost a civil judgment in the purge was a 15 point increase.[2]

We start by developing and analyzing an illustrative model of consumer credit markets with information provision that allows us to think about the addition and removal of informative signals. In the model, the removal of derogatory information leads to an increase in credit for those consumers who had the derogatory information removed. Yet the same removal leads to a reduction of credit for consumers who are otherwise ex-ante identical and who did not have the derogatory information prior to the removal. This pooling effect is stronger the more pervasive the derogatory information is.

In our empirical work, we start by examining the causal impact of the removal of public records information on the 11.8 percent of low-credit-score consumers with a public record. Because the timing of the removals considered—whether as a result of the NCAP purge or due to regular removal after seven or ten years from a credit record—is outside of the consumer's control and therefore exogenous, the resulting change in credit score, credit card limit, and various forms of debt has a causal interpretation. We show both reduced-form results and local-projection instrumental variables (LP-IV, Stock and Watson (2018)) results to capture the dynamic causal path. For those who had a public record removed, the LP-IV results imply that the likelihood of having a credit card increased by 2.3 percentage points over four quarters (over a baseline likelihood of 48.4 percent in June 2017). The existing credit card limit of these consumers increased by 4.7 percent, while credit card debt increased due to the exogenous increase in scores by 3.6 percent, so credit card utilization was slightly reduced after a year due to the exogenous change. Our reduced form results imply that the endogenous increase in credit card debt was significantly larger at 14.7 percent. This means that much of the debt response was an endogenous dynamic response. One plausible explanation for this is it is the expansion of actual credit that leads to increases in credit card debt, not just the relaxation of borrowing constraints resulting from the credit score

[2] In its own pre-purge analysis, Fair Isaac Corporation (2017) predicted that the removal would have "modest decline in risk prediction" and an increase in credit scores of less than 20 points for most of the affected population. Haughwout et al. (2018) examine the effect on affected consumers' credit scores of the 29% reduction in collection accounts reported in credit records that resulted from the NCAP. These authors and Clarkberg and Kambara (2018) both document a great deal of heterogeneity in the effects of NCAP on credit scores.

increase. In terms of the heterogeneity in responses, we show that the number of open credit cards, credit limits, and credit card debt all expand more for higher utilization consumers. Since the response of the credit limit and credit card debt is similar across utilization levels, credit utilization remains fairly stable. This finding suggests that credit card utilization is a good predictor of the the marginal propensity to increase debt from an increase in credit (which itself is closely related to the marginal propensity to consume, see Fulford and Schuh (2015)). Exogenous public records removal also caused an increase of 1.3 percentage points in the likelihood of having an auto loan (over a baseline likelihood of 31.4 percent in June 2017) and an 324 dollar increase in auto debt after a year.

For some credit records, the removal of a public record can lead to the loss of a credit score altogether by making the credit record unscorable. This, in turn, may reduce access to credit. About 11 percent of the credit records with a public record lost a credit score because of the NCAP purge. Yet 95% of such records only contained the public record that was removed. We show that the reduction in credit access from losing a credit score is minimal because, to the extent that these credit records represent actual consumers, they generally were not seeking credit before the purge.

Next, we examine the equilibrium impact of the reduction in information contained in credit records resulting from the NCAP purge. The equilibrium effects we are attempting to uncover are two-fold. First, the removal of public records does not just mean that consumers who used to have a public record no longer have one. It also means that consumers who did not have a public record can no longer use the lack of a public record to signal their higher creditworthiness, creating a *market-wide pooling effect* from the NCAP purge. Second, the removal of information from the market may lead to an *aggregate change in credit*. Whether the change is an expansion or a contraction depends, among other things, on the relative demand for credit among the groups of consumers being pooled (Liberman et al., 2018). Of course, identifying equilibrium effects is challenging because generally only one equilibrium is observed at a time. In our case, however, we rely on the fact that the pooling effect is stronger in submarkets with a higher share of consumers with a public record prior to the purge, which we refer to as a submarket's exposure to the purge.

Just as in our theoretical model, consumers who did not have a public record prior to the purge lost more distinguishing information in the purge if they were in a submarket with a higher share of consumers with a public record.

Using this insight, we estimate equilibrium effects using variation across the credit score distribution and across states in exposure to the purge. The results are inconclusive with regards to the aggregate change in credit for the low-credit-score population as a consequence of the purge, but they rule out the presence of a sizable aggregate change. Consistent with our model, the primary effect of the purge was to increase pooling within submarkets—there was a redistribution of credit away from consumers in high exposure submarkets to those in low exposure ones. Since exposure and credit score are negatively correlated, this also implies that, in a relative sense, the purge resulted in reallocation of credit from lower- to higher-score consumers.

Despite the importance of credit reports to many aspects of consumers' lives and the rapidly changing information environment, relatively little is known about the effect of information provision in consumer credit markets. Theory says that information must be central to solving the asymmetric information problem. Studies of individual lenders suggest that using information to help assess credit risk is crucial for profitability (Karlan and Zinman, 2009; Einav, Jenkin, and Levin, 2013; Dobbie and Skiba, 2013). To identify the equilibrium impact of changes in information, it is necessary to observe a market-level change, which is rare. Liberman et al. (2018) examine the removal of a default flag from a large fraction of the Chilean population. Using as a control group people who are not expected to be affected by the change according to the authors' custom credit scoring model, Liberman et al. (2018) conclude that the information removal reduced available credit in equilibrium.

This paper is also closely related to the small literature examining how long negative information should continue to be on credit reports. The welfare impacts must balance general equilibrium efficiency and moral hazard considerations and the relevance of past information for future default (Elul and Gottardi, 2015). Exploiting a change in policy in Sweden which shortened the period for which bankruptcy records are retained on credit records, Bos and Nakamura (2014) are able to examine the general equilibrium effects. They find that credit became tighter for a given credit score—consistent with credit scores becoming less informative—but that net access to credit increased, suggesting that the effects of changing the information on a credit report are intricate. An important difference between their study and ours is that they need to concern themselves with the potential moral hazard consequences of the shorter retention period which may increase the incidence of reported bankruptcies, while in our study of the removal of tax liens and civil judgments we are able to study the pure information effect. Our individual estimates of the effect of removing public records are also closely related to work that estimates the impact of the removal of bankruptcy flags from credit report (Musto (2004), Gross, Notowidigdo, and Wang (2016)).

A growing literature considers the individual impact of changes in credit and its impact on consumption and savings. Aydin (2018) and Gross, Notowidigdo, and Wang (2016) similarly exploit exogenous changes in credit and examine the changes in debt. They find similar sized effects. The estimate of a high marginal propensity to increase debt following an increase in credit line is surprisingly similar to the reduced form estimates of Fulford and Schuh (2017) and Gross and Souleles (2002). Leth-Petersen (2010) finds lower debt responses for Danish homeowners following an exogenous change in their liquidity.[3]

Finally, this paper is related to recent work that seeks to understand the equilibrium impact of information disclosure rules in labor markets. "Ban the box" policies that preclude employers from asking about past convictions may result in benefits to those with convictions, but also result in employers being more wary to hire from a population with a large fraction of convictions. Thus an unintended consequence of such policies is to hinder the employment prospect of young, low-skilled men without a criminal record (Agan and Starr (2018) and Doleac and Hansen (forthcoming)). The results we present suggest similar pooling effects in the context of credit provision. Relatedly, Bartik and Nelson (2019) study the ban of pre-employment credit checks and show that the effect of such bans in an employment context depends on the relative information content of credit checks for different groups, and not simply on the average signals that these checks provide

[3]A related extensive literature considers the consumption impact of expected and unexpected cash infusions. For example, see Johnson, Parker, and Souleles (2006), Agarwal, Liu, and Souleles (2007), and Hsieh (2003).

across the groups.

2 Public records and credit reports

In this section, after briefly describing our main source of data, we discuss public records in consumer credit reporting, the National Consumer Assistance Plan (NCAP), and cross-state variation in the incidence of public records.

2.1 The CFPB Consumer Credit Panel

Our main data source is the Consumer Credit Panel (CCP) maintained by the CFPB, an anonymized 1 in 48 sample of credit records from one of the three national CRCs. We limit our analysis sample to all credit records in the CCP since 2012. Once a credit record enters the CCP—either through the initial pull or through each quarterly refreshes that add a sample of newly created credit records in order to ensure that the panel stays dynamically representative—it stays in the CCP unless the CRC merges it with a non-CCP record. The CCP contains approximately five million credit records each quarter.[4] In the CCP, we observe all trade line information reported to the CRC including account information for each credit card, auto loan, and mortgage. We do not generally observe the terms of these credit products, such as the interest rate, because that information is not reported to the CRCs. For this reason, we primarily focus on access to credit, rather than its cost.

2.2 Public records

Other than bankruptcy filings, there were three types of public records that appeared in credit records before the NCAP purge: civil judgments, federal tax liens, and state or local tax liens. Civil judgments are rulings against the consumer in a court of law in a non-criminal legal matter often requiring the payment of damages. The lawsuit may be brought by creditors over non-payment

[4] Unlike a similar data set maintained by the Federal Reserve System which samples on Social Security Number, the CFPB's CCP is sampled from the universe of all credit records and all newly created credit records.

of debts or for other damages. Civil judgments used to stay on a credit record for seven years from the filing date. Civil judgments that were paid remained on the record but were marked as "satisfied." If a civil judgment is vacated it can be removed from the credit record.[5] A tax lien is the government's claim against all or some of the consumer's assets based on failure to pay a tax debt on time. Unpaid tax liens can remain on a credit record indefinitely. In practice, however, the CRC providing the CCP removed unpaid tax liens after 10 years. Paid liens also remained on the credit record but were removed seven years after the date paid as required by the Fair Credit Reporting Act.[6] Federal tax liens could be "withdrawn" by the IRS and, once withdrawn, removed from the credit record before the seven year period, though this did not appear to be common in our data. Whether state tax liens could be withdrawn depended on the state, but the process was not generally considered easy.

In June 2017, just before the NCAP purge, around 6.3% of credit records had a public record: 4.3% had a civil judgment, whether satisfied or not, and 2.4% had a tax lien, whether paid or not. A little under 0.4% had both a civil judgment and a tax lien. For comparison, 4.2% of credit records contained a bankruptcy record at the time. While having a public record is not common in the overall population, it is quite common for those with low credit scores. Figure 1 shows the fraction of credit records with a public record by 10 point credit score bins in June 2017. Among credit records with credit scores of 700 or below, 11.8% had some type of public record. The most common were unpaid civil judgments.

For some credit records with a public record in June 2017, the public record was the only information on the credit record. The two spikes in the distribution in Figure 1 at 581-590 and 601-610 are accounted for by credit records with unpaid or paid public records and with no other information, thereby receiving the appropriate default credit scores (586 for unpaid and 601 for paid civil judgment or lien). 11.3% of credit records with a public record had a credit score in June 2017 but lost it by September 2017, compared to only 0.75% of credit records without a public record. Of credit records with a public record that lost a credit score, 94.6% had the default credit

[5] See https://www.creditsesame.com/blog/debt/how-long-does-a-judgement-stay-on-your-credit-report.

[6] See 15 U.S. Code §1681c - "Requirements relating to information contained in consumer reports."

score.

Next, we briefly describe the typical life cycle of a civil judgment or tax lien since how and why these public records appear and disappear from credit records matters for estimating their impact. After the creation of a civil judgment or a tax lien in court or tax records, an information furnisher recorded the effective date of the judgment or lien with the relevant identifying information and provided it to the CRC which would then associate it with a credit record. Often, but not always, the civil judgment or tax lien would get paid relatively quickly and the public record would then be reported as paid. For example, half of civil judgments that became paid before aging off did so in the first two years. Whether paid or unpaid, a public record would age off of the credit record after either 7 or 10 years, unless vacated or withdrawn, which occurred rarely.

We divide the reasons that public records leave or change on credit records into 1) conversion from unpaid to paid, 2) aging off, 3) removal by the NCAP purge, and 4) an unidentified reason.[7] We identify public records that convert from unpaid to paid by matching unpaid public records in one quarter with paid ones in the subsequent quarter. We identify public records that age off based on the source of public record, using either a 7 or 10 years window as appropriate. When linking public records over time, we are relatively conservative in our matching criteria, requiring the public record to match on the public record source, filing date, and amount owed across the two consecutive quarters.

Before the NCAP purge in July 2017, among credit records that had an unpaid public record in one quarter, 1.98% became paid in the next quarter. From quarter to quarter, 3.72% of unpaid public records aged off, while 1.62% were removed from the credit record for an unidentified

[7] Some data quality issues arise when working with public records data in the CCP. At times it seems that the same public record is recorded multiple times, possibly with slight differences. This can happen when multiple information furnishers submit the same public record. To address this issue and to allow us to match public records over time, we perform some cleaning of the data. Specifically, we identify and drop a small percentage of public records in our data as duplicates. There are several types of duplicates. First, some duplicates are the same as another record on every variable we observe. Second, for a given consumer, two records can be of the same source, for the same amount, with the same filing and legal status date, but with different payment statuses, in which case we treat the unpaid record as a duplicate. Third, we allow for fuzzy matching on legal status date, treating the earlier records as duplicates. Not all duplicates are necessarily identified by this procedure. For example, in some instances, a state and a federal tax lien have the same date and amount for the same credit record, suggesting that the source of one lien is possibly incorrectly recorded.

reason. Similarly, 6.97% of credit records with a paid public record before July 2017 were removed each quarter because they aged off the record while 1.2% were removed for an unidentified reason.

2.3 The National Consumer Assistance Plan

The NCAP resulted from a settlement in 2015 between the three national CRCs and 31 state attorneys general, led by the office of New York Attorney General Eric Schneiderman. The NCAP addressed several areas where information on credit reports was often in error, including collections, medical debts, and public records.[8] In the agreement, the CRCs agreed to improve the accuracy of credit records in a number of ways, to improve their dispute procedures, and to change how they report medical debt. The settlement established a working group to create standards for the accuracy of public records data. The agreed new standards required that, starting July 1, 2017, tax liens and civil judgments must include a person's name, address, and either Social Security number or a date of birth to be included in credit records. Moreover, to make sure that the information remained accurate, it had to be updated at least every 90 days. The rationale for these new standards was the view that previously lack of or inaccuracies in these fields led to public records being associated with the incorrect credit record, a point raised by consumer groups and the state attorneys general. While this paper does not address these inaccuracies and their effects directly, it is our expectation to address them in future work.

In its press release announcing the change, the Consumer Data Industry Association reported that all civil judgments and about half of reported tax liens would not meet the new standards. As shown in Figure 2, the NCAP purge in fact removed all civil judgments and about a half of all tax liens.[9] The three CRCs decided to remove all remaining tax liens in April 2018.[10]

[8] The executed settlement is available at: http://www.ag.ny.gov/pdfs/CRA%20Agreement%20Fully%20Executed%203.8.15.pdf. See the CFPB's report on medical debt here: http://files.consumerfinance.gov/f/201412_cfpb_reports_consumer-credit-medical-and-non-medical-collections.pdf.

[9] "New Public Record Credit Reporting Standards to Begin July 1, 2017 Civil Judgments and some Tax Liens to be removed from many credit reports" CDIA, June 28, 2017, https://www.cdiaonline.org/faqs-re-reporting-of-public-records-such-as-civil-judgments-and-tax-liens/.

[10] See AnnaMaria Andriotis, "Missed a Tax Payment? That May No Longer Count Against Your Credit Score," Wall Street Journal, March 22, 2018, https://www.wsj.com/articles/missed-a-tax-payment-that-may-no-longer-count-against-your-credit-score-1521716406.

2.4 Public records across states

Some states issue far more civil judgments and state tax liens than others. Figure 3 shows for each state the fraction of the low-credit-score population that has a civil judgment, state or federal tax lien. The federal tax lien fractions are fairly similar across states. The fraction with a civil judgment or a state tax lien varies substantially across states, however. In Indiana, the most public record intensive state, civil judgments and tax liens are both common. In other states, only civil judgments are common. Overall, the fraction of the low-credit-score population in a state with a public record varied from a low of 5 percent to a high of 35 percent.

Civil judgements and state tax liens are governed by state law which explains why their incidence varies substantially across states. For example, it is several times more expensive to bring civil court cases in some states than in others. States also vary in whether they facilitate filing with an electronic system and in the ease and cost of hiring local counsel. In addition, many states limit where a case can be filed and whether the entity bringing suit is required to have a local place of business. State consumer protections and approaches to debt collection litigation also vary significantly (Spector and Baddour, 2016, p.1436). While civil judgements occur for many reasons, an important reason for them is suits brought by debt collection firms that specialize in acquiring debts and attempting to collect on them. One way to collect these debts is to turn them into civil judgements which may allow wage garnishment. This often takes place through default judgements when the defendant does not appear in court (Stifler, 2017, p. 107). Debt collection firms are sensitive to the cost and difficulty of bringing a civil case when deciding how to attempt to collect, explaining some of the observed differences across states.

3 An illustrative model

Consider the following simple model of consumer credit markets with information provision motivated by a modern formulation of Jaffee and Russell (1976) and Vandell (1984). The model has two periods and a large number of consumers and firms. The model can be used to analyze the addition and removal of information in credit markets.

3.1 The consumer's problem

The consumer solves a two-period consumption smoothing problem given a deterministic income stream y_1 and y_2 in the two periods. In the first period, the consumer can borrow amount L at gross rate R. In the second period, the consumer repays the outstanding loan balance $B = RL$ unless the cost of default, Z, is lower than the outstanding balance. The cost of default is stochastic and is distributed according to F_i, where $i \in \{l, h\}$, where F_h first order stochastically dominates F_l. In other words, there are two types of consumers and the cost of default for type h consumers is higher in expectation than the cost of default for type l consumers. In this sense, lending to type l consumers is riskier.

For a given R, consumers of type i choose L to maximize

$$EU_i(c_1, c_2) \tag{1}$$

where

$$\begin{aligned} c_1 &= y_1 + L && (2) \\ c_2 &= y_2 - \min(Z, RL), && (3) \end{aligned}$$

where the second constraint incorporates the consumer' optimal default behavior. Writing out the expectation in terms of the distribution of the cost of default, the problem that a consumer of type i solves is:

$$\max_L \int_0^{RL} U(y_1 + L, y_2 - Z)dF_i(Z) + \int_{RL}^{\infty} U(y_1 + L, y_2 - RL)dF_i(Z). \tag{4}$$

The first-order condition of the consumer's problem then can be written as

$$\int_0^{RL} U_1(y_1{+}L, y_2{-}Z)dF_i(Z) + \int_{RL}^{\infty} U_1(y_1{+}L, y_2{-}RL) - RU_2(y_1{+}L, y_2{-}RL)dF_i(Z) = 0. \quad (5)$$

Simplifying and rewriting in terms of the balance $B = RL$, we get

$$\int_0^{\infty} LU_1(y_1 + L, y_2 - \min(Z, B))dF_i(Z) = \int_B^{\infty} BU_2(y_1 + L, y_2 - B)dF_i(Z). \quad (6)$$

Using the utility function of the consumer, we can define indifference curves in $\{L, B\}$ space. Under standard regularity conditions, the indifference curves are concave, increasing as we move down and to the right, with the indifference curve of low default cost consumers intersecting those of the high default cost consumers from above. This latter property reflects the fact that low default cost consumers are more willing to pay a higher interest rate given that they face a lower likelihood of repayment.

3.2 The firm's problem

Facing a pool of consumers within which a fraction $\tilde{p}$ have low default cost, the payoff of the firm in terms of loan size and repayment balance B can be written as

$$\Pi_{\tilde{p}}(L, B) = [\tilde{p}\,(1 - F_l(B)) + (1 - \tilde{p})\,(1 - F_h(B))]\,B - IL, \quad (7)$$

where I is the cost of funds for the firm. The firm's payoff is concave in B due to the fact that the probability of default increases in B.

Using the above payoff function, we can define the iso-profit function of the firms. This iso-profit function is convex, and, for well-behaved distribution functions F_l and F_h, will become backward bending for a sufficiently large B.

3.3 Information provision

The share of consumers who are low type in the economy as a whole is p. Firms receive independent signals about the consumers' type. For signal k, the probability that a high-type consumers receives the signal is σ_k and the probability that a low-type consumers receives the signal is $\rho_k\sigma_k$. σ_k can be thought of as the *pervasiveness* of signal k, i.e. a large σ_k means that a large fraction of the population receives the signal. The distance of ρ_k from 1 indicates the *informativeness* of the signal. We generally will focus on derogatory signals, i.e. signals such that $\rho_k > 1$, meaning that receiving the signal means that the consumer receiving the signal is more likely to be of the low type. Informativeness then increases in ρ_k (which is necessarily bounded above by $1/\sigma_k$).

Consider a group of consumers about whom firms have received $k-1$ signals and whose ex-post probability of being of low type, given these signals, is $\hat{p}$ for all the consumers in the group. Assume that an additional derogatory signal, signal k, is received about these consumers. Then, using Bayes' rule, the probability of being low type among those receiving the signal, $\hat{p}^{+1}$, is

$$\hat{p}^{+1} = \frac{\rho_k \sigma_k \hat{p}}{\sigma_k (1-\hat{p}) + \rho_k \sigma_k \hat{p}} = \frac{\rho_k \hat{p}}{1-\hat{p}+\rho_k \hat{p}} > \hat{p}, \tag{8}$$

where the last inequality follows from $\rho_k > 1$. Not surprisingly, the probability of being a low type increases upon receiving a derogatory signal. The probability of being a low type among those not receiving signal k, $\hat{p}^{+0}$ can then be derived from Bayes' rule:

$$\hat{p}^{+0} = \frac{(1-\rho_k\sigma_k)\,\hat{p}}{(1-\sigma_k)\,(1-\hat{p}) + (1-\rho_k\sigma_k)\,\hat{p}} = \frac{(1-\rho_k\sigma_k)\,\hat{p}}{1-\sigma_k-(\rho_k-1)\,\sigma_k\hat{p}} < \hat{p}, \tag{9}$$

where the last inequality follows from the fact that $\hat{p}^{+0}$ is decreasing in ρ_k and $\rho_k > 1$. Not receiving the derogatory signal is positive news for these consumers so the probability of being a low type declines for them. Expressing as updating from $\hat{p}$,

$$\Delta^{+0} = \hat{p}^{+0} - \hat{p} = \frac{1-\rho_k}{\frac{1-\sigma_k}{\sigma_k} - (\rho_k - 1)\,\hat{p}}\,(1-\hat{p})\,\hat{p} < 0. \tag{10}$$

This expression implies that the amount of updating among those who did not receive the signal is an increasing function of σ_k, the fraction of consumers receiving the signal (i.e. the pervasiveness of the signal), and the informativeness of the signal ρ_k. Not surprisingly, updating is also greater the closer is $\hat{p}$ to 1/2.

We can similarly derive the effect of removing a derogatory signal, which is the empirically relevant case for us. To do so, consider a group of consumers about whom firms have received k signals (including signal k) and whose ex-post probability of being of low type, given these signals, is $\hat{p}$ for all consumers in the group. Assume that a derogatory signal k is removed from the information set. Then the above relationships can be inverted to derive the probability of being low type among those who used to receive signal k, $\hat{p}^{-1}$, is

$$\hat{p}^{-1} = \frac{\hat{p}}{\rho_k (1-\hat{p}) + \hat{p}} < \hat{p}, \tag{11}$$

where the last inequality follows from $\rho_k > 1$. The probability of being a low type declines upon removal of a derogatory signal. The probability of being a low type among those who did not use to receive signal k, $\hat{p}^{-0}$ can also be derived by inverting Bayes' rule to give

$$\hat{p}^{-0} = \frac{\hat{p}(1-\sigma_k)}{1-\rho_k\sigma_k + (\rho_k - 1)\sigma_k\hat{p}} = \frac{\hat{p}(1-\sigma_k)}{1-\rho_k\sigma_k(1-\hat{p}) - \sigma_k\hat{p}} > \hat{p} \tag{12}$$

where the last inequality follows from the fact that $\hat{p}^{-0}$ is increasing in ρ_k and $\rho_k > 1$. No longer being able to signal that they do not have derogatory signal k leads to an increase in the probability of being a low type for these consumers. Expressing as updating from $\hat{p}$,

$$\Delta^{-0} = \hat{p}^{-0} - \hat{p} = \frac{\hat{p}(1-\sigma_k)}{1-\rho_k\sigma_k(1-\hat{p}) - \sigma_k\hat{p}} - \hat{p} = \frac{\rho_k - 1}{\frac{1-\sigma_k}{\sigma_k} - (\rho_k - 1)(1-\hat{p})}(1-\hat{p})\hat{p} > 0. \tag{13}$$

Again, this expression implies that the amount of updating among those who used to not receive the signal is an increasing function of σ_k, the fraction of consumers receiving the signal (i.e. the pervasiveness of the signal) and the informativeness of the signal ρ_k.

3.4 Graphical analysis

Figure 4 shows the consumers' indifference and firms' zero profit curves in (L, B) space when there is full information about the consumers' type. In this setting, the firms' zero profit curve is conditional on the consumer's type. Such a zero profit curve starts at the origin, is initially increasing, and always lies above the ray with slope I. As the probability of default becomes sufficiently large, the zero profit curve becomes backward bending as in Figure 4. The zero profit curve for low types (Π_0^L) lies to the left of the zero profit curve for high types (Π_0^H) since the probability of default is higher for low types for a given level of B. In a full information equilibrium, firms offer contracts defined by the tangency of the indifference curves and the respective zero profit curves at C^H for high type consumers and C^L for low type consumers. These equilibria may survive as separating equilibria when firms do not have full information. This is not the case in Figure 4, however, since the low-type consumer has an incentive to imitate the high-type consumer since $U^L(C^H) > U^L(C^L)$.

Instead, when the type of the consumers is not known, there will arise a pooling equilibrium as in Figure 5. Here Π_0^A represents the zero profit curve of firms that face a pool of consumers made up of some low types and some high types, with the share of low types being $\tilde{p}$. The optimal pooling contract makes the high type as well off as possible given the average zero-profit curve: at point E the high-type's indifference curve is tangent to Π_0^A. Low types take up the pooling offer. Competing firms cannot offer another contract to high type consumers that would make them better off and they have no incentive to offer better contracts to low types. Both high types and low types are credit rationed in the sense that they would like to borrow more at the prevailing interest rate (the slope of the ray between the origin and point E), but they cannot. Low types are much more severely credit rationed than high types.

Figure 6 shows the effect of moving a consumer from a market where $\hat{p}$ fraction of the consumers are low type to one where $\hat{p}^{-0} > \hat{p}$ fraction are low type. This corresponds to the case of a consumer without a derogatory signal when that derogatory signal is removed from the market. Conditional on staying in a pooling equilibrium, the optimal contract moves from E to E', implying that the optimal amount of credit declines and the interest rate rises for these consumers.

The flip side of the change analyzed in Figure 6 are consumers whose derogatory information was removed which results in them moving from a market with $\hat{p}$ to a market with $\hat{p}^{-1} < \hat{p}$, resulting in a shift out of the zero-profit condition. Conditional on staying in a pooling equilibrium, the optimal amount of credit increases and the interest rate declines for these consumers. This is not shown in Figure 6 for ease of exposition.

4 The individual impact of having a public record removed

This section examines the impact of removing public records for consumers with such records. Our focus is on identifying the causal effect that an increase in perceived creditworthiness has on those directly affected by the removal of negative information. In the next section, we examine the equilibrium effects of information removal on the market as a whole.

4.1 Impact on scorable consumers with a public record

4.1.1 Empirical strategy

We focus on removals of public records resulting from aging off and from the NCAP purge in July 2017. Because the timing of these removals is outside of the consumer's control, we treat them as exogenous and estimate a causal impact.[11] Our general empirical strategy is to compare changes in credit scores and in other outcome variables for consumers who had a public record removed in a given quarter to changes for similar consumers who did not have a public record removed in the

[11] While the timing of aging off and of the NCAP purge is exogenous, these events may have been anticipated by at least some consumers. The length of time a public record stays on a consumer's credit report is public knowledge and the NCAP purge was reported on in the news in the months before it happened. To the extent that some consumers changed their behavior in anticipation (by, for example, taking on more debt), our estimates give a lower bound on the effect of unexpected removals. These anticipatory effects are likely not substantial given that after the NCAP purge the 2018 Credit Score Knowledge Survey by the Consumer Federation of America found that 63% of respondents (incorrectly) believed that civil judgments were used in credit scoring, and the share was 64% for tax liens. These shares are not much lower than the 79% of respondents who (correctly) believed that bankruptcies were used in credit scoring. See https://consumerfed.org/press_release/survey-shows-an-increasing-number-of-consumers-have-obtained-their-credit-scores-and-know-much-more-about-credit-scores.

same quarter. To make the groups more comparable, we restrict our sample to those credit records that had a public record some time since 2012.

We start with a reduced-form difference-in-difference specification that allows for the effect of public record removal to be distributed over several quarters. In particular, we pool all sources of public records and estimate versions of:

$$\Delta Y_{it} = \theta_t + f(a_{it-1}, S_{it-1}) + \sum_{p,r} \sum_{\tau=0}^{3} \left(\beta_{pr\tau} \Delta PR^{pr}_{it-\tau} + \beta^0_{pr\tau} \Delta I(PR^{pr}_{it-\tau} = 0) + \beta^a_{pr\tau} A^{pr}_{it-\tau} \right) + \varepsilon^r_{it} \tag{14}$$

where $\Delta Y_{it} = Y_{it} - Y_{it-1}$ is the quarterly difference in the outcome variable (credit score or some measure of credit or debt), θ_t are time indicators, $f(\cdot)$ is a third-order polynomial of age (a) and credit score (S). The terms in the summation capture the effect of removing different types of public records after τ quarters. ΔPR^{pr}_{it} measures the number of public records removed for consumer i at time t, where payment status is reflected by $p \in \{\text{Paid}, \text{Unpaid}\}$ and removal reason is reflected by $r \in \{$ Age off before NCAP, Other removal before NCAP, Remove in NCAP $\}$. The last two terms allow the number of public records going to zero ($\Delta I(PR^{pr}_{it-\tau} = 0)$) and the average age of public records removed (A^{pr}_{it}) to have a distinct effect.

The polynomial of age and credit score as of the previous quarter captures predictable changes in outcome variables by age and credit score. To illustrate why this is important, Figure 7 shows the change in credit score from June to September 2017 for those credit records that had an unpaid public record removed (and may have also had a paid one removed), for comparison, those that only had a paid public record removed, and those that had a public record at any time since 2012, but did not have one removed during that quarter. Credit scores tend to be increasing from quarter to quarter at low credit scores and decreasing, at a slower rate, for credit scores above 600. The change in credit scores for those who had both unpaid and paid public records removed are generally larger than the change for those who did not have a public record removed that quarter, but there are large predictable changes in credit score, particularly at the low end of the credit score distribution.

Even if the impact on credit score is immediate, the effects on other outcomes, such as credit limits and debt are likely to take some time to take effect. For example, a consumer with a higher credit score may be able to take out a larger auto loan or be more likely to succeed in a credit card application. Yet, because only a fraction of consumers are likely to apply for a new credit card or auto loan in a given quarter, a change in credit score may take several quarters to become a change in credit. Moreover, consumer behavior may affect credit scores after the initial change. For example, a consumer with an exogenous increase in credit score may have a higher credit card limit, and may subsequently borrow more, increasing credit utilization and reducing her credit score. Thus, the dynamic path of effects is possibly quite complex. In addition, interpreting the many coefficients in Equation (14) together is complicated.

Our second empirical approach allows for these dynamic causal effects in a way that combines information efficiently. We employ the local-projection instrumental variables (LP-IV) approach of Stock and Watson (2018). This method unites the local projection approach of Jordà (2005) for estimating dynamic effects (also known as impulse responses in the literature) at each horizon separately with the microeconometric tradition of using external instruments to estimate causal effects. The basic idea of LP-IV is that the exogenous removal of a consumer's public record is a good instrument for the change in credit score this quarter, and does not affect other credit report outcomes in the current and subsequent quarters other than through its effect on the credit score. Since our interest is in identifying the dynamic effect of external shocks and not in identifying structural impulse response functions, we can estimate the effects of interest using LP-IV without the need to specify a structural vector autoregression (SVAR). While SVAR-IV can provide more efficient estimates by estimating the dynamic effects jointly, its application can be challenging due to the possibility of misspecification and the need to assume invertibility in order to ensure consistency. Applying standard two-stage least square (2SLS) IV to estimate the dynamic effect at each horizon separately is straightforward and the combined effects give the dynamic causal effect.

For different possible horizons $\tau = 1, 2, 3, 4$, we seek to obtain causal estimates of the effect of a change in score:

$$Y_{it+\tau} - Y_{it} = \theta_t + f(a_{it}, S_{it}) + \beta_\tau (S_{it+1} - S_{it}) + \varepsilon^c_{it+\tau}. \tag{15}$$

Note that we estimate separate regressions for the first quarter effect ($\tau = 1$) and at each horizon τ, so the cumulative change over the year following the purge is captured by β_4.

There are two reasons for using an instrumental variable approach. First, as is the traditional rationale, there is an identification problem in Equation 15 resulting from the fact that changes in credit score are endogenous. The simplest example is that defaulting on some debt causes both credit score and credit limit to decline. Second, we are not primarily interested in the effect of credit score but use it only as an intermediary variable that allows us to combine and scale all of the external shocks in information coming from the various changes in public records. Instrumenting allows us to address the endogeneity issue, we make use of exogenous changes in public records that provide suitable instruments for estimating the causal effect of credit score changes. The key exogeneity assumption is that the public record changes are not correlated with other reasons for changes in credit for that consumer, which should be assured by the fact that the timing of the public records removal is exogenous.

Our first stage allows for public records of different source, payment status, and removal reason to affect credit reports differently. For public records of source $s \in \{$ Civil judgement, Federal tax lien, State tax lien $\}$, of payment status p, and removal reason r, we estimate:

$$\Delta S_{it} = \theta_t + f(a_{it-1}, S_{it-1}) + \sum_{s,p,r} \left(\gamma_{spr} \Delta PR^{spr}_{it} + \gamma^0_{spr} \Delta I(PR^{spr}_{it} = 0) + \gamma^a_{spr} A^{spr}_{it} \right) + \varepsilon^i_{it} \tag{16}$$

where PR^{spr}_{it} is the number of public records of source s, payment status p, and removal reason r, and $I(PR^{spr}_{it} = 0)$ is an indicator for whether person i has zero public records at time t of type spr. We drop the s, p, r combinations that do not have variation. For example, because all civil judgments were removed in July 2017, removals in March do not include these types.

It is worth noting that estimating the dynamic effects using standard 2SLS with lagged credit scores as the instrumented variables without the local projection would not result in correct estimates of the desired effects. In such a specification all lags of public records variables would enter as instruments for all lags of the credit score. Thus the estimated coefficient on the contemporaneous credit score change, for example, would not be the contemporaneous effect, but some combination of the contemporaneous effect from external changes in public records this quarter and the dynamic effects from changes in public records in previous quarters.

4.1.2 Results

We begin by showing the reduced-form effects of having unpaid public records removed for different exogenous reasons as in Equation (14), pooling all sources of unpaid public records together and focusing on the low-credit-score population. We show the cumulative marginal effect of the NCAP purge of unpaid public records using the average number of unpaid public records removed (1.39), the fraction that go to zero unpaid public records (0.93), and the average age (3.24 years) in the NCAP purge. We show cumulative coefficients scaled this way to facilitate comparison with the LP-IV results later on and to give a natural interpretation to the marginal effects for different removal reasons. For example, the average age of the public records removed in the purge was much less than the average age of public records that age of at 7 or 10 years. Standard errors are clustered at the credit record level. To reduce the influence of outliers and reporting errors, we winsorize the top and bottom 1% of changes in the non-indicator outcome variables.

Figure 8 shows the results for credit (risk) score, whether the consumer has an open credit card, the number of open credit cards the consumer has, log credit card limit, log credit card debt,[12] and credit card utilization, while Figure 9 shows the results for whether the consumer has an auto loan, the amount of auto debt, whether the consumer has a mortgage, and the amount of mortgage debt. The sample includes only consumers with a public record since 2012, so the identifying variation is comparing consumers who had a public record removed in a given quarter to consumers who did not have one removed that quarter but had a public record some time since 2012.

[12]By construction, this considers the intensive margin since these measures do not include observations with zero limit or zero debt.

Having unpaid public records removed in the NCAP purge has an immediate effect of raising the credit score by close to seven points with the effect size moderating to under four points after a year. This effect is smaller than the modal effect of 15 points reported by Clarkberg and Kambara (2018), since we take into account the underlying dynamics of credit scores observed in Figure 7 by including the polynomial in past credit score and the reported effects are only for unpaid public records (though the effects for paid public records are smaller, so adding those would account for a small part of the difference). The contemporaneous effect of the NCAP purge is very similar to the effect of aging off prior to July 2017. The result that the public record removal has not just a contemporaneous effect but a dynamic effect on credit scores underscores the importance of focusing below on dynamic causal effects by using LP-IV.

Following the NCAP purge of unpaid public records, the likelihood of having an open credit card increases by 0.5 percentage points on impact and this effect grows to 1.8 percentage points after a year. Given that the share of consumers with an open credit card was 48.4 percent in June 2017 in the population of interest, this is a sizable effect. As panel 3 shows, the number of open credit cards increased by 0.09 cards over a year, representing a 4.7 percent increase over the average number (1.98) among these consumers in June 2017. Turning to the intensive margin, credit card limits increased by about 1.4 percent on impact and this effect grew to 7.9 percent after a year, implying a substantial increase in credit limit (panel 4 of Figure 8). The effect is larger for aging off before July 2017. This difference could suggest both more of an anticipatory effect in case of an individual removal. Equilibrium effects could also explain some of the difference. In response to the NCAP purge, credit card debt increases notably more than credit limits (14.7 percent vs. 7.9 percent after one year, see panel 5), so utilization (panel 6) is increasing by 1.85 percentage points (over a base of 49.9 percent in June 2017).

The results in Figure 9 suggest that removing unpaid public records increased the fraction of people with an auto loan by 0.36 percentage points on a base of 31.4 percent in June 2017. The amount of auto debt increased by 209 dollars, representing 3.2 percent of the average auto debt of 6,632 dollars held by the relevant population in June 2017. The impact on mortgages is smaller (even negative on the extensive margin) and economically small, especially for the NCAP purge. Given the relatively small changes in credit score and that the population with a public record tends to be low credit score and therefore less likely to have a mortgage in the first place (with a probability of 17.6 percent in June 2017), it is not a surprise that there is a less pronounced effect on mortgages. Moreover, public records remained public after the purge, even if they were no longer included in credit records. Mortgage lenders in particular may still look up public record information, even if it is not worth the cost to do so for smaller loans.[13]

Next we show the LP-IV results estimated according to Equation (15) allowing for the full heterogeneity of public records by source, payment status, and removal reason to affect outcomes through their impact on the credit score. Figures 10 and 11 and Table 1 show the cumulative dynamic effects of an exogenous increase in credit score by 6.54 point for the affected population of low-credit-score consumers. We scale the exogenous increase by 6.54 points—the average effect of removing an unpaid public record during the NCAP purge—to make the results comparable to the reduced form results. We show the coefficients from each horizon in Table 1. The table also shows the summary statistics for the first stage of the three quarter ahead impact which is very strong in all cases.

The results in Figure 10, 11, and Table 1 indicate a substantial increase the likelihood of having an open credit card and the number of open credit cards, similar in size to the reduced form estimate. The estimated increase in the credit card limit is somewhat smaller than the reduced form estimate (4.7 percent versus 7.9 percent after a year.) The dynamic causal estimate of the change in credit card debt is significantly smaller than the reduced form estimate (3.6 versus 14.7 percent after a year). One possible explanation for the difference is that it is the increase in credit limit itself that induces these consumers with public records at some point (and low credit scores on average) to carry more credit card debt above and beyond any increase in debt that comes directly from the higher credit score relaxing the consumer's shadow credit constraint. Correspondingly, the change in utilization is negative or zero, in stark contrast with the increase shown with the reduced form

[13]For example, see https://www.creditkarma.com/advice/i/credit-score-increase-removal-tax-liens, accessed June 10, 2019.

estimates. These differences demonstrate the benefit of using the LP-IV estimates.

Exogenous increases in credit score lead to increases in the likelihood of having an auto loan and auto debt in Figure 11 and Table 1. Scaled by the same six point increase in credit score, we get a 1.3 percentage point increase in the likelihood of having an auto loan and an 324 dollar increase in auto debt. For the affected population, exogenous changes in credit score coming from the removal of public records again seem to have a tightly estimated zero impact on the likelihood of having a mortgage and on mortgage debt.

Table 1 reports the relevance tests for the instruments and over-identification tests. The exogenous removal of public records produces highly relevant instruments, as was clear from the first panel of Figure 8 which shows results closely related to the first stage for a subset of the public records. The various LP-IV estimates tend to fail the overidentification test suggesting that restricting the different types of public records to have a common impact is binding (Parente and Silva, 2012). This is not too surprising given the reduced form results that indicate that the effects of the NCAP purge and of the aging off of public records are somewhat different. Given the large sample sizes, this result suggests that there may be some benefit in exploring the heterogeneity of responses across public records of different source, payment status, and removal reason. Moreover, it is also possible that results differ by individual characteristics, a possibility we will explore next.

4.2 Different responses across credit utilization

Recent work (Aydin, 2018; Fulford and Schuh, 2017; Parker, 2017; Carroll, Slacalek, and Tokuoka, 2017) has emphasized the importance of heterogeneity of the marginal propensity to consume for understanding the distribution of consumption, savings, and debt. To explore this heterogeneity, we estimate Equation (15) limiting the sample to consumers within each of eleven credit utilization bins in the quarter before the public record removal, including a separate bin for zero utilization.

Figure 12 shows the heterogeneous LP-IV treatment effects across prior utilization for changes in the number of open credit cards, log credit limits, log credit card debt, and utilization. Four quarters out, all levels of prior credit utilization saw some increases in the number of open credit cards, but the increase for consumers with a higher utilization was more significant. This is also true for changes in credit limits and credit card debt which were larger for higher utilization consumers.

The last panel of Figure 12 shows the change in credit utilization following the exogenous increase in credit. Despite a fairly large change in credit limits at all levels of utilization, utilization falls slightly at all levels of prior utilization. Even as credit limits increase from quarter to quarter following the public records removal, utilization stays more stable.

These results imply that credit utilization approximates the marginal propensity to increase debt from an increase in credit. For example, consumers using 70 percent of their credit increased their debt by almost 70 percent of the increase in their credit limits, so their utilization stayed similar. The marginal propensity to increase debt from an increase in credit is closely linked to the marginal propensity to consume out of an increase in income (Fulford and Schuh, 2015). These results suggest that even without a natural experiment, credit utilization is directly informative about the marginal propensity to consume.

4.3 Impact on those who become unscorable

The purging of public records caused 11.3% of credit records with a public record to become unscorable. It is possible that some newly unscorable records were credit fragments that were not matched to the existing credit record of the affected consumer.[14] Nonetheless, to the extent that the newly unscorable records represented consumers with thin files, losing a credit score may have made it more difficult for these consumers to obtain credit.

For credit records that lost a credit score due to the purging of public records, the purge did not impact access to credit because these records were not using credit before the purge: 94.6% of them had the default public record credit score in June 2017 which indicates that there was no scorable information in the credit record other than the public record. Figure 13 shows various credit record characteristics (whether there is a credit score and age associated with the credit record, whether a

[14] As discussed above, an important rationale for purging public records was that they did not contain sufficient and accurate information to correctly match to the affected consumer's credit record.

credit card is associated with the credit record and the mean number of inquiries) by whether the credit record lost a credit score between June and September 2017 among credit records that ever had a public record since 2012. By definition, among records that lost a credit score, none had a credit score in September 2017. Only a small fraction had a credit score 12 months later. These credit records were generally very thin; less than 20% had an age reported before June 2017, and the public record was the source of the age for about half of these credit records judging by the age dropping off the credit record after the purge. Almost no credit record losing a credit score had a credit card. Moreover, there were almost no credit inquiries (measured as the average number of inquiries in the previous quarter) either before or after the purge.

Put together, the evidence suggests that while one effect of removing information from a credit record is that some credit records become unscorable, the apparent consequences of the public records removal for the affected credit records were minimal. Credit records—not necessarily consumers—for whom the only information the CRC had at the time of the purge was a public record had no demonstrated history of using credit and, judging by inquiries, almost no demand for credit.

5 The equilibrium effects of public record removal

This section examines the equilibrium impact of removing public records for those who did not have one at the time of the NCAP purge and had not had one since at least June 2012.[15] Many credit effects may take several quarters to fully take effect. For example, lenders may not reduce credit limits directly, but may not automatically increase them for people who lose a valuable signal in the NCAP purge, or may be less willing to extend new credit when someone applies for it. These impacts will take several quarters to accumulate, so we switch to yearly differences to better capture these accumulated effects. While the quarterly dynamics are interesting, our variation is at

[15]It is possible that, to the extent that lenders pool credit record data over time, they may treat some credit records that lost a public record shortly before the NCAP purge as if they were included in the NCAP purge. Such effects will not be present for credit records that have not had a public record in the five years prior to the NCAP purge.

the market-time level, so quarterly effects are not necessarily well identified.

5.1 Empirical strategy

We approach estimating equilibrium effects two ways: first we ask if there was an overall reduction in credit access following NCAP. A market-wide equilibrium effect might occur if firms reduce credit provision overall because the consumer credit market has become more risky. The estimated market-wide effect will capture total changes in credit, but will not identify redistribution across credit scores if firms reduce credit for newly more risky groups and increase credit for relatively safer groups. Second, we examine whether there was redistribution of credit from people whose credit scores became much less informative to those whose credit scores were less impacted.

Our approach to identifying overall changes in credit access is simple, but rests on strong assumptions: we examine whether credit became less available for consumers with scores below 700 compared to consumers above 700 in the year following NCAP:

$$\Delta Y_{it} = \theta_t + \beta I(\text{Below } 700_{it}) \times I(\text{July } 2018) + \beta_0 I(\text{Below } 700_{it}) + X_{it}\gamma + \varepsilon^o_{it}, \quad (17)$$

where $I(\cdot)$ are indicators and X_{it} include state and year effects. The time effects (θ_t) capture overall changes in credit, β_0 captures whether these overall changes are on average larger or smaller for consumers below 700, and β_1 captures whether the change in the year following NCAP was larger or smaller than the average below-700 change.

This approach rests on very strong assumptions and so should be treated cautiously. First, it assumes that, to the extent that other changes in consumer credit access occurred in the year following NCAP, they are captured by changes in the credit to above-700 consumers and a below-700 effect. Second, it is possible that there was some redistribution from below 700 to above 700. Public records before NCAP were concentrated in the population below 700 (see figure 7), so it is here that lenders might reduce overall credit availability, but if lenders then increased credit availability to the above-700 consumers, this approach may capture redistribution rather than a change in overall availability.

We identify the market equilibrium redistribution effects of information removal by using variation in the exposure to the NCAP purge by state and broad score bin. In a market with a high fraction of credit records that lost a public record as a result of the NCAP purge, the model suggests it was more valuable for credit records without a public record to be able to signal that they did not have a pubic record. For these credit records, the NCAP purge resulted in the loss of a more valuable signal than for credit records without a public record in a market that had a low fraction of credit records that lost a public record.

To capture these exposure effects, we estimate versions of:

$$\Delta Y_{it} = \theta_t + \beta \text{PR removal exposure}_{it} \times I(July2018) + \beta_1 \text{PR removal exposure}_{it} + X_{it}\gamma + \varepsilon^e_{itm}, \tag{18}$$

where PR removal exposure$_{it}$ is the extent of exposure of person i to public record removed in the NCAP purge between June and September 2017 and X_{it} are a set of controls to absorb predictable changes in credit. These controls are potentially important because there may be predictable changes in credit that are state and score-bin dependent, so correlated with public record removal exposure.

This analysis is similar in spirit to Liberman et al. (2018). A distinction is that, unlike in their work, we can use an already existing scoring model to study the effect of exposure, rather than create a new scoring model to predict who might look similar to lenders after the purge and we observe differences in exposure across states. Using differences across exposure by score bins and states, the identifying variation is the difference between low-exposure state-bins and high-exposure state-bins. The estimated coefficients capture the combined effect of potential reductions in credit to high-exposure groups and increases in credit to low-exposure groups, so capture pooling effects, rather than aggregate changes in credit in equation 17.

5.2 Results

Table 2 shows the overall credit estimates from equation 17 for those who do not have a public record. The results are somewhat mixed. All estimates hold the average difference between above- and below-700 score consumers without a public record constant (the coefficient on I(Below 700 at $t-1$)). Below-700 consumers consumers were less likely to have an open credit card, but the number of cards increased in the year following NCAP. At the same time, the log credit limits for were lower than the average difference.

As discussed above, these results depend on strong assumptions to capture the causal effect of NCAP removal: that the changes in credit availability of the above-700 consumers capture any non-NCAP market effects and there was little to no redistribution from below to above 700. The inconsistent results, particularly between log limits and dollar values suggest that, rather than a strong overall reduction in credit, the primary effect of NCAP may have been distributional.

Table 3 show the results from equation 18 examining whether there was redistribution from consumers in states and credit score bins where more information was lost to consumers with less information loss. Across states and scores, the dynamics of score and credit changes are complicated. To control for these dynamics, Panel A includes county effects, age effects, 50-point score bin effects, and allows a one year and two year lag of score to be different across 50-point credit score bins. Panel B shows the results for a narrower set of controls, only including state, age, and 50-point score bin effects. We cluster standard errors at the score-bin state level.

The public records exposure variable measures the share of consumers who lose a public record from June to September 2017 in the state and 50-score bin a year ago. Interacted with a dummy for the year following the NCAP removal (June 2017 to June 2018), this variable captures whether credit availability changed more for people whose scores became less informative because they are in a state-score bin where more information was removed. We normalize the public records exposure variable into standard deviation units (z-score) based on the variance across 50-point score bins and states in the share of public records removed. To allow for any remaining correlation between credit changes across states and score bins that is also correlated with public record exposure, we also include the public record exposure variable not interacted with a dummy. Doing so absorbs any pre- and post-trend with exposure, so the public record exposure following NCAP captures only changes across exposure groups in that year that is different from average.

The results in Table 3 Panel A and suggest a meaningfully large redistribution effect. In the year following NCAP removal, a one standard deviation increase in the fraction of public records removed, decreases log credit limits by 0.9 percent, increases the fraction who lose more than 20 percent of their credit limit by 0.03 percent, decreases the likelihood of having an open card or auto loan slightly.

The impact of public records removal in the year following NCAP is larger in Panel B with narrower controls than in Panel A. Panel A includes one and two year lags of scores interacted with the lagged score bins, allowing the dynamics of score to differ across the score distribution. It also includes county dummy effects. Including these controls in Panel A, the public record exposure variable not following NCAP is much smaller or insignificant than in Panel B, suggesting that the controls are helping to absorb predictable variation that is different across states and score bins.

6 Conclusion

We show that removing information in consumer credit markets produces complex effects at the individual and market equilibrium level. In particular, while we show that individuals benefit from removing derogatory information, for the affected population credit card debt increases at the same rate as credit limits. Comparing parts of the credit score distribution with more or fewer public records removed, we show that the pooling effect coming from removing information has mostly redistributive effects. The evidence that access to credit declined overall is weak and inconsistent. Following the removal of public records, there is evidence of redistribution of credit from people did not have public record but after removal look similar to people who did have a public record. These redistributive effects tend to reallocate credit towards higher credit scores. These results are potentially consistent with a model of pooling as lenders cannot discriminate along risk as precisely as before the NCAP purge.

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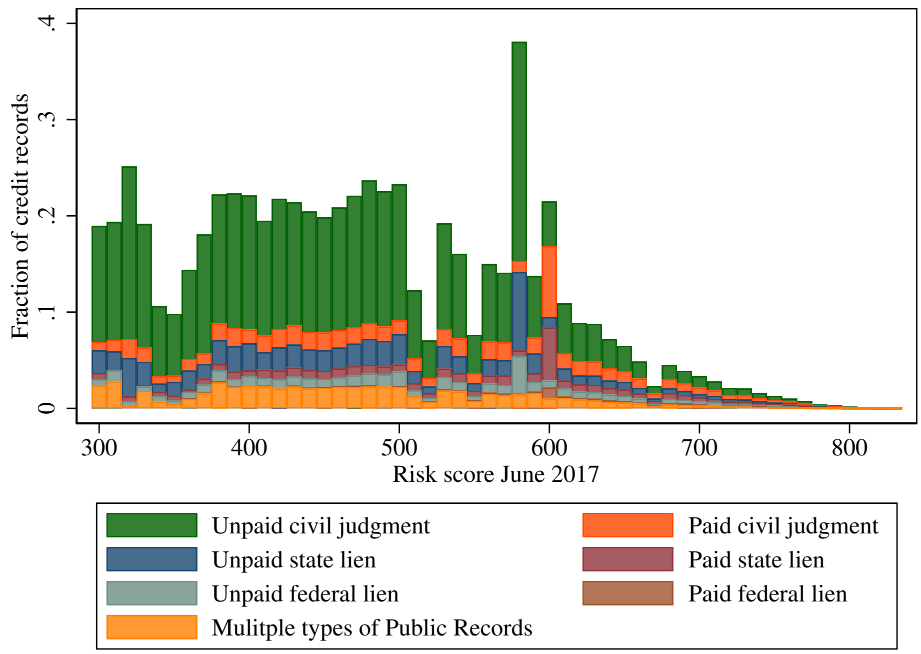
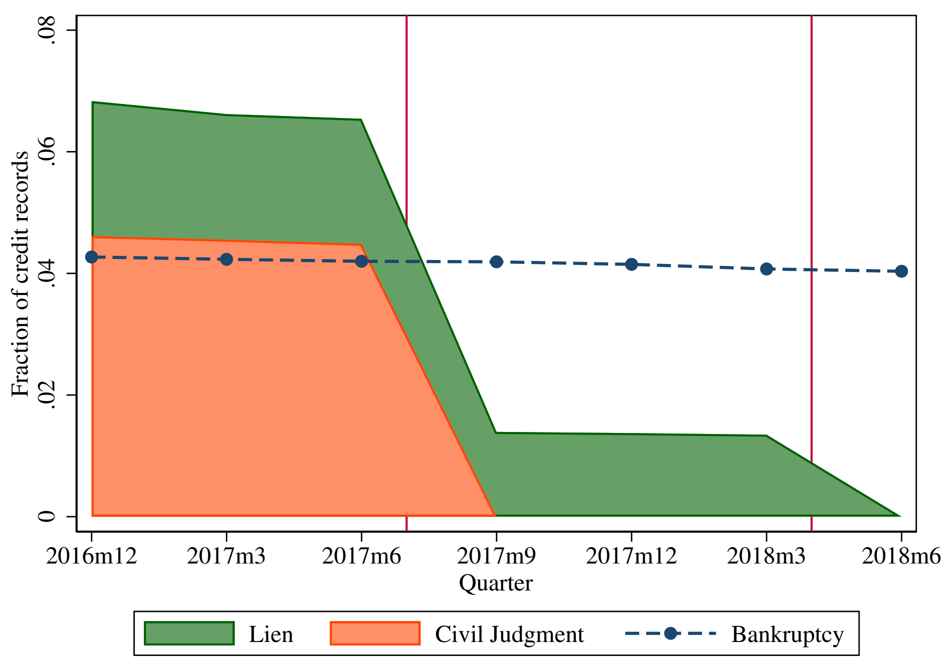
Figure 1: Fraction with a public record in June 2017 by credit score bins



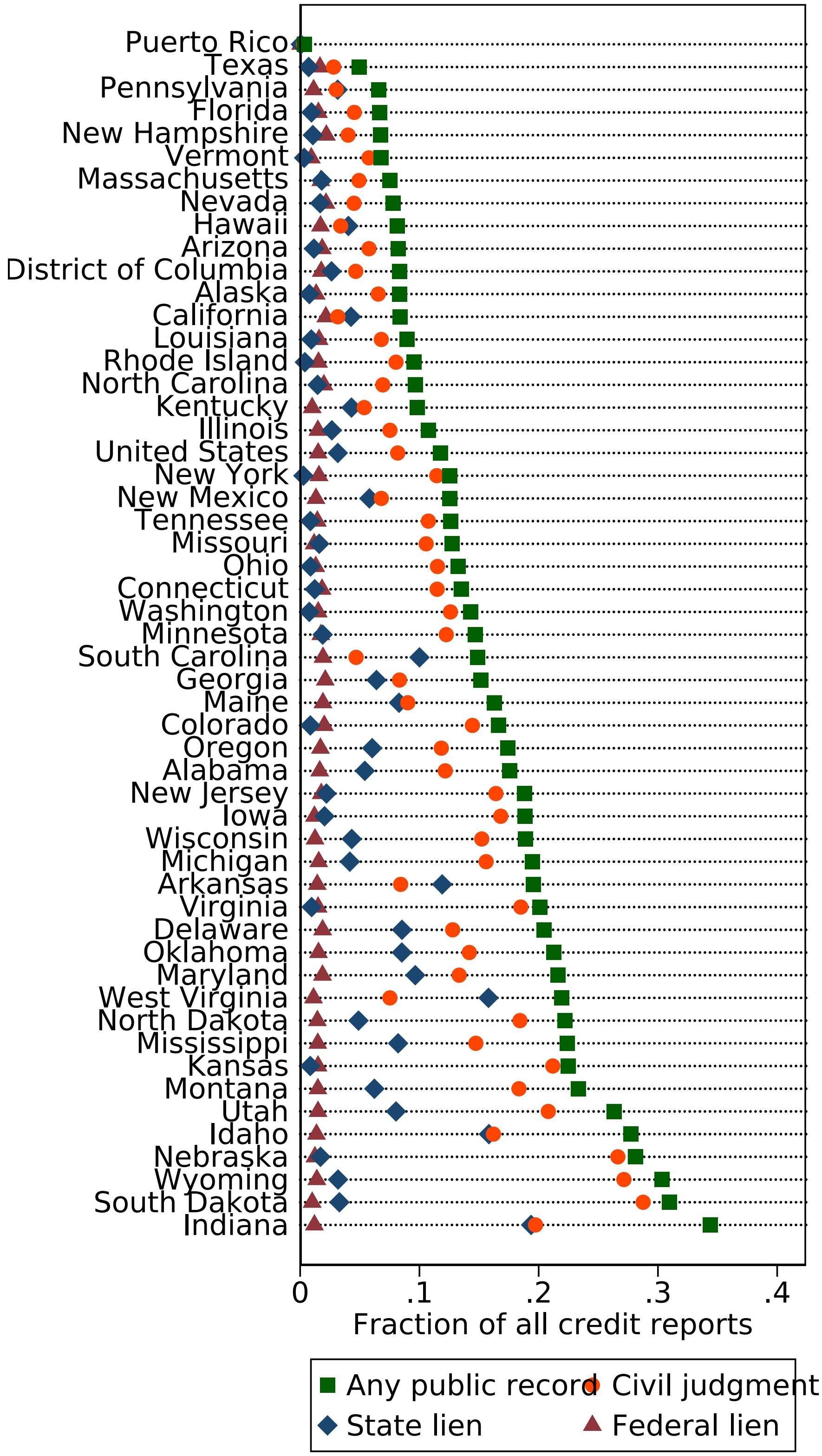
Source: Authors' calculations from CCP.

Figure 2: Fraction of credit records with a public record over time



Source: Authors' calculations from CCP.

Figure 3: Fraction of credit records with a public record by state among credit records with a credit score of 700 or below



Source: Authors' calculations from CCP.

Figure 4: Graphical analysis of theoretical model: full information zero profit and indifference curves

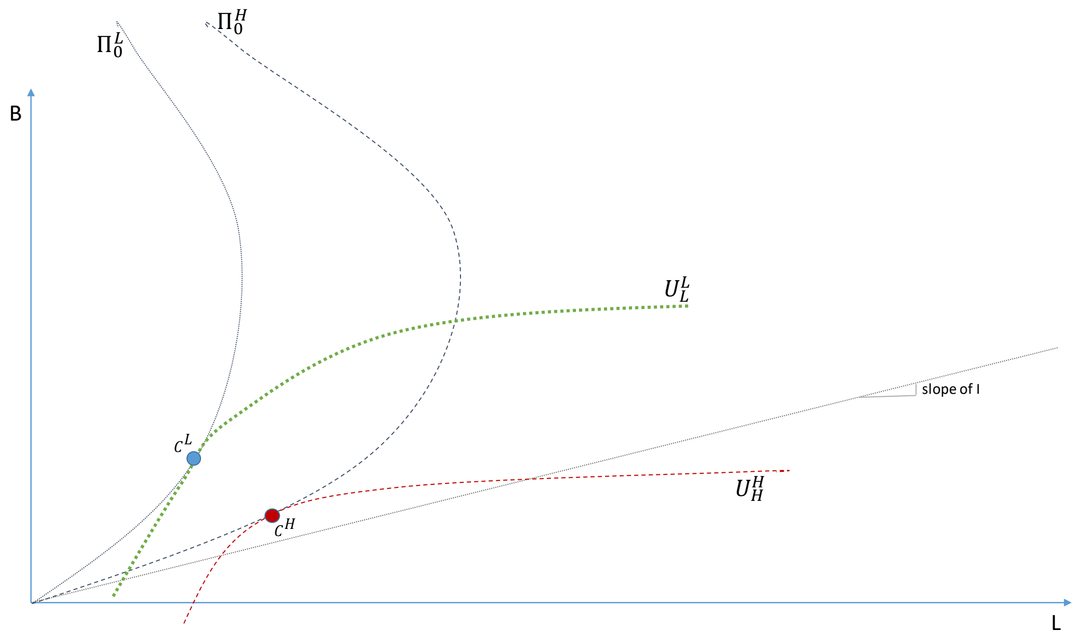


Figure 5: Graphical analysis of theoretical model: pooling equilibrium

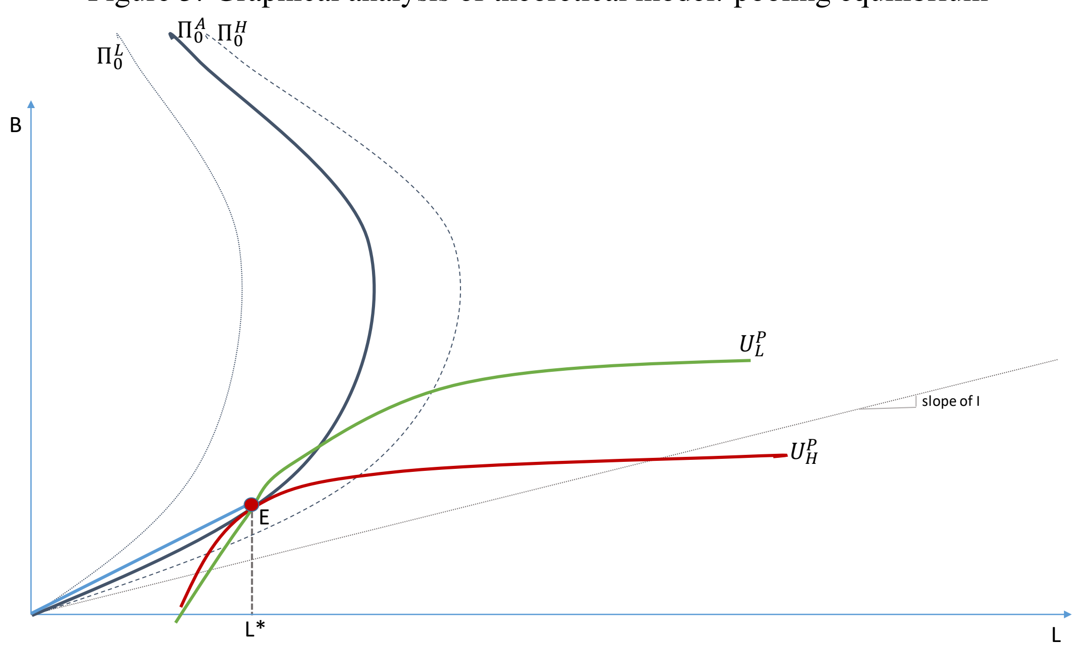


Figure 6: Graphical analysis of theoretical model: pooling equilibrium with increase in probability of being low-type

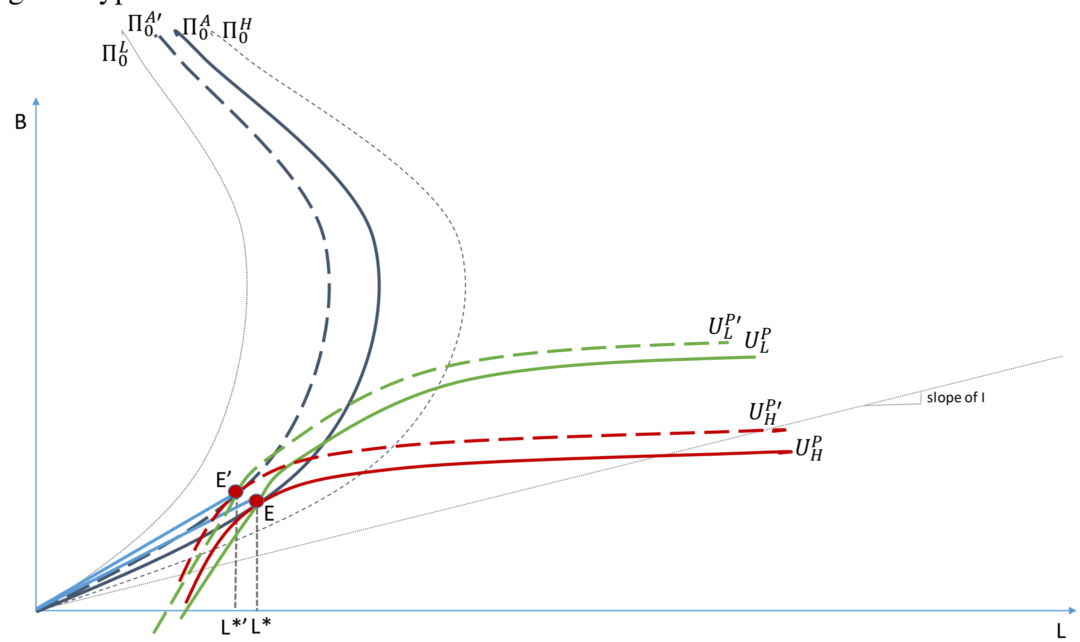
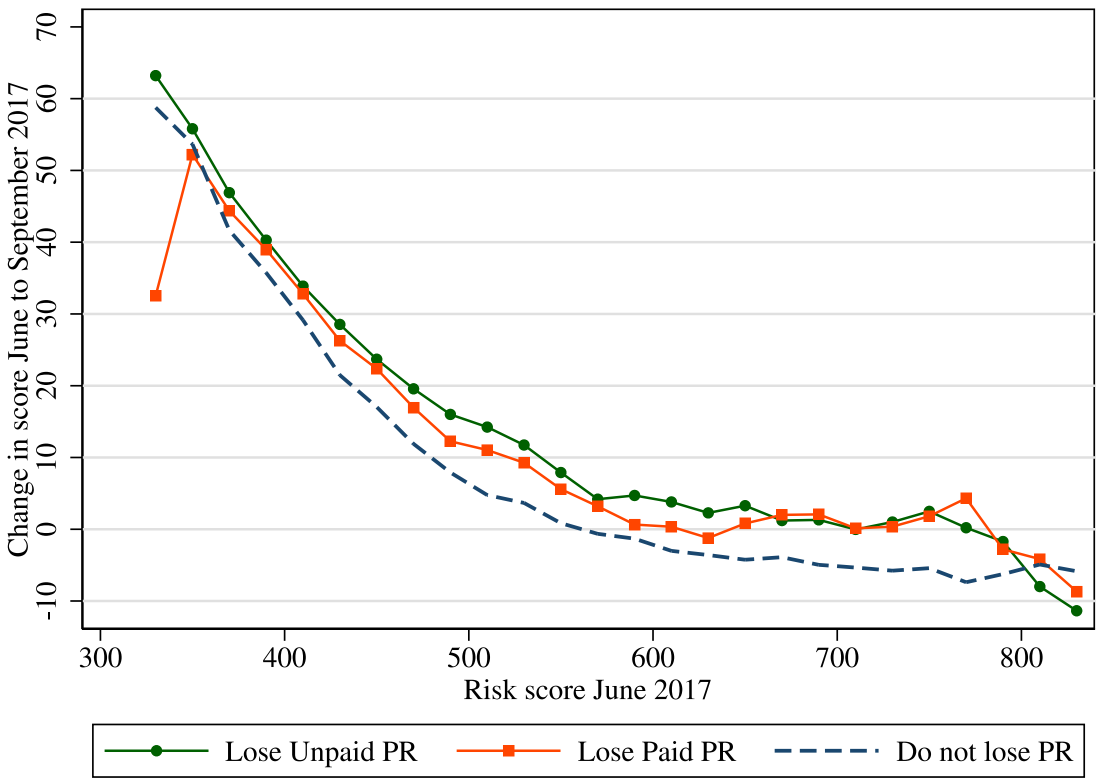
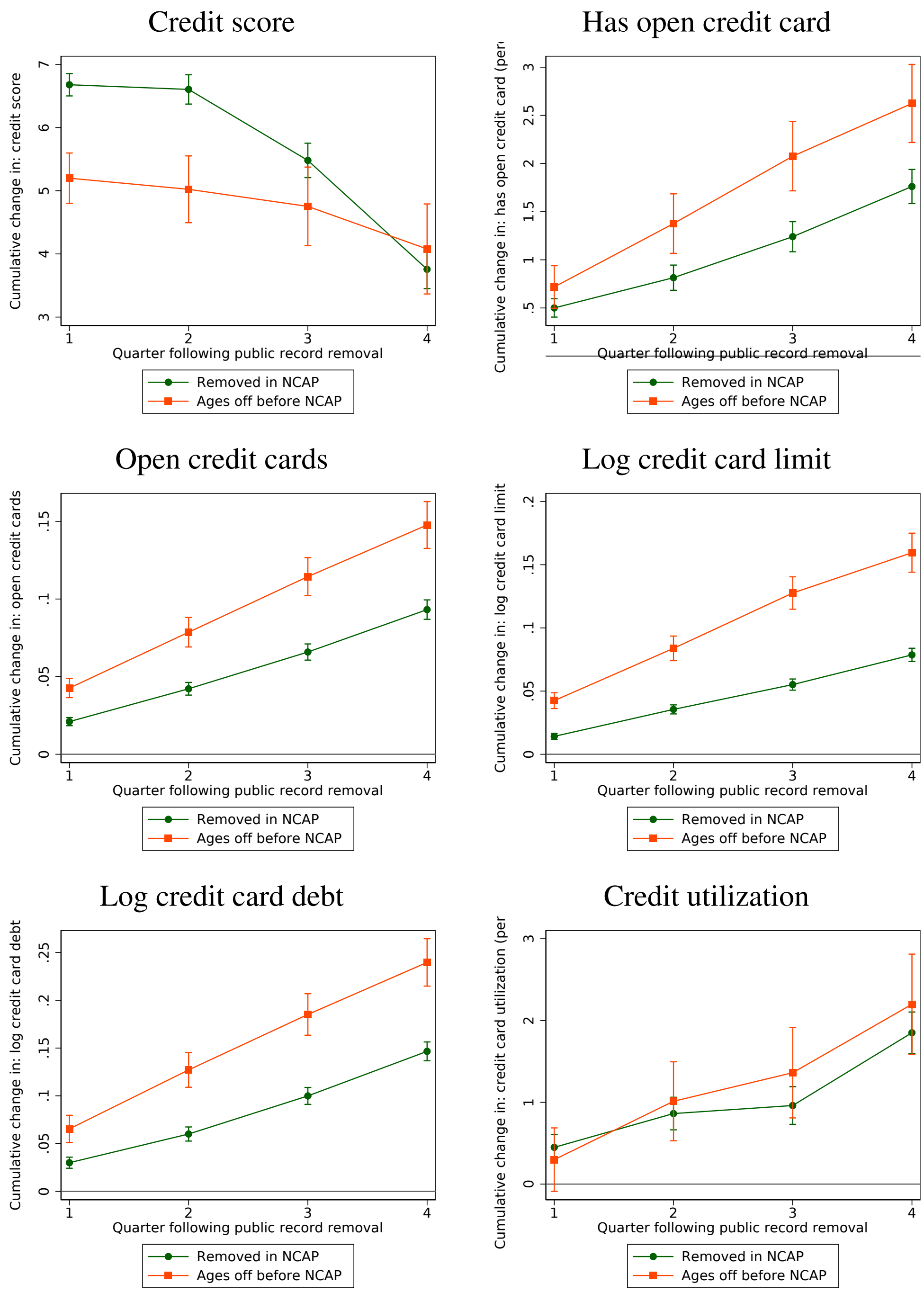


Figure 7: Change in credit score between June and September 2017



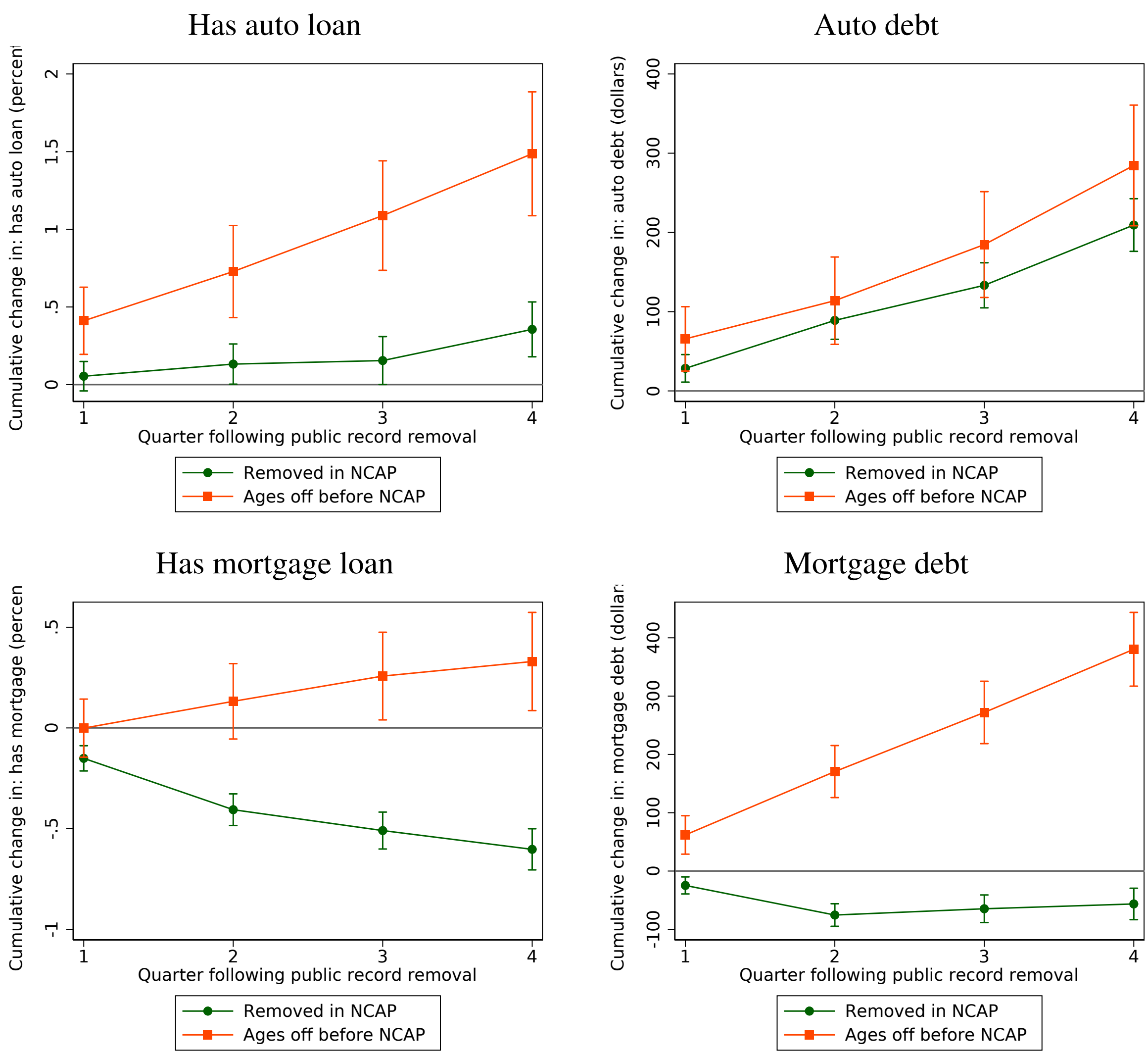
Notes: The "Do not lose PR" group includes credit records that had a civil judgment or tax lien since 2012, but did not lose one between June and September 2017. Source: Authors' calculations from CCP.

Figure 8: Reduced form effect of unpaid public record removal



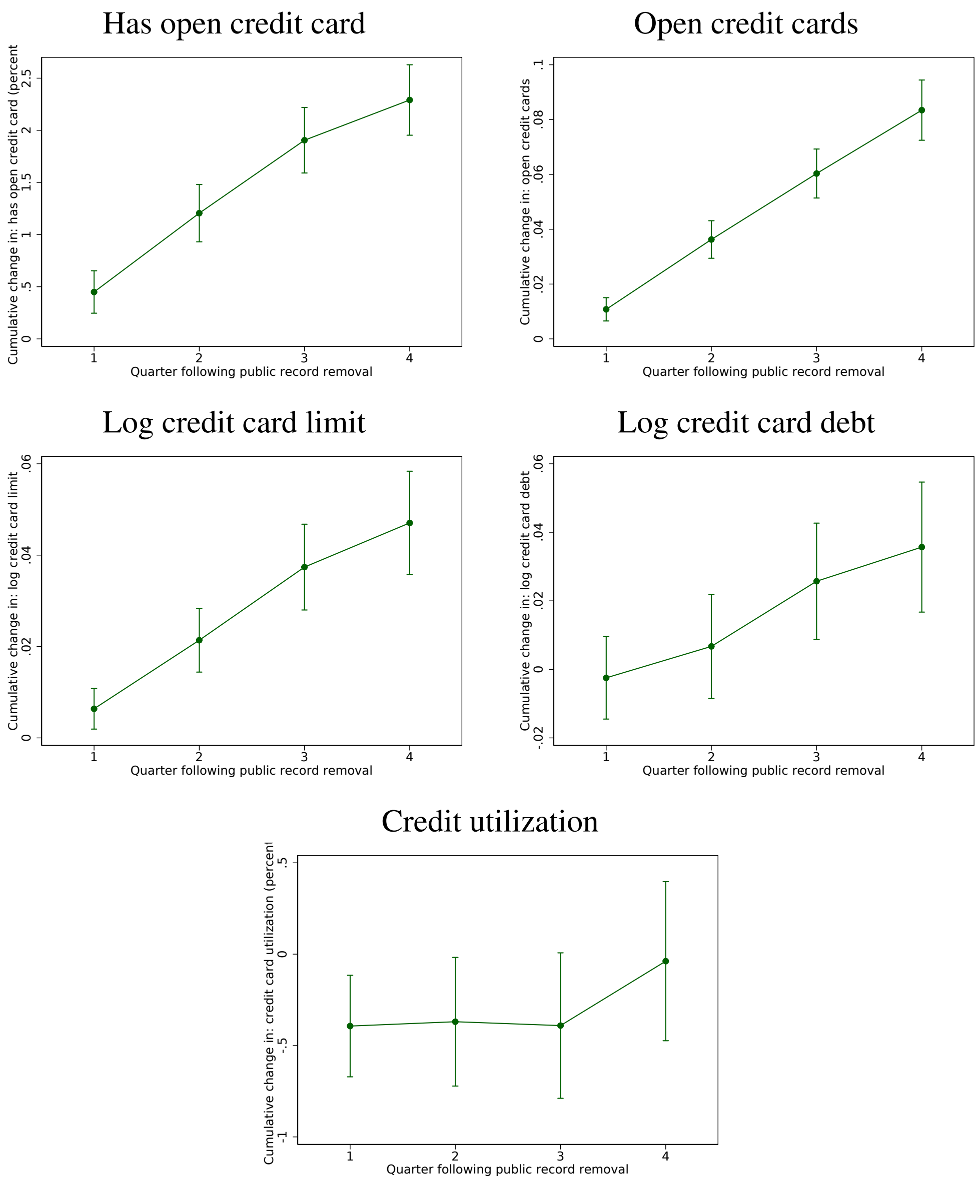
Notes: Each panel shows the cumulative marginal effect of the exogenous removal of unpaid public records on the selected outcome variable estimated using Equation (14). To standardize the effect size, the margin for each removal reason is calculated using the average number of unpaid public records removed (1.39), the fraction that go to zero unpaid public records (0.93), and the average age (3.24 years) in the NCAP purge. Each estimate includes date effects, a cubic in lag credit score, a cubic in age, and the removal of paid public records, the effects of which are not shown. The sample includes only credit records with a public record since 2012. Bars represent 95% confidence intervals. Standard errors are clustered at the credit record level. To reduce the influence of outliers and reporting errors, we winsorize the top and bottom 1% of changes in the non-indicator outcome variables. Source: Authors' calculations from CCP.

Figure 9: Reduced form effect of unpaid public records removal



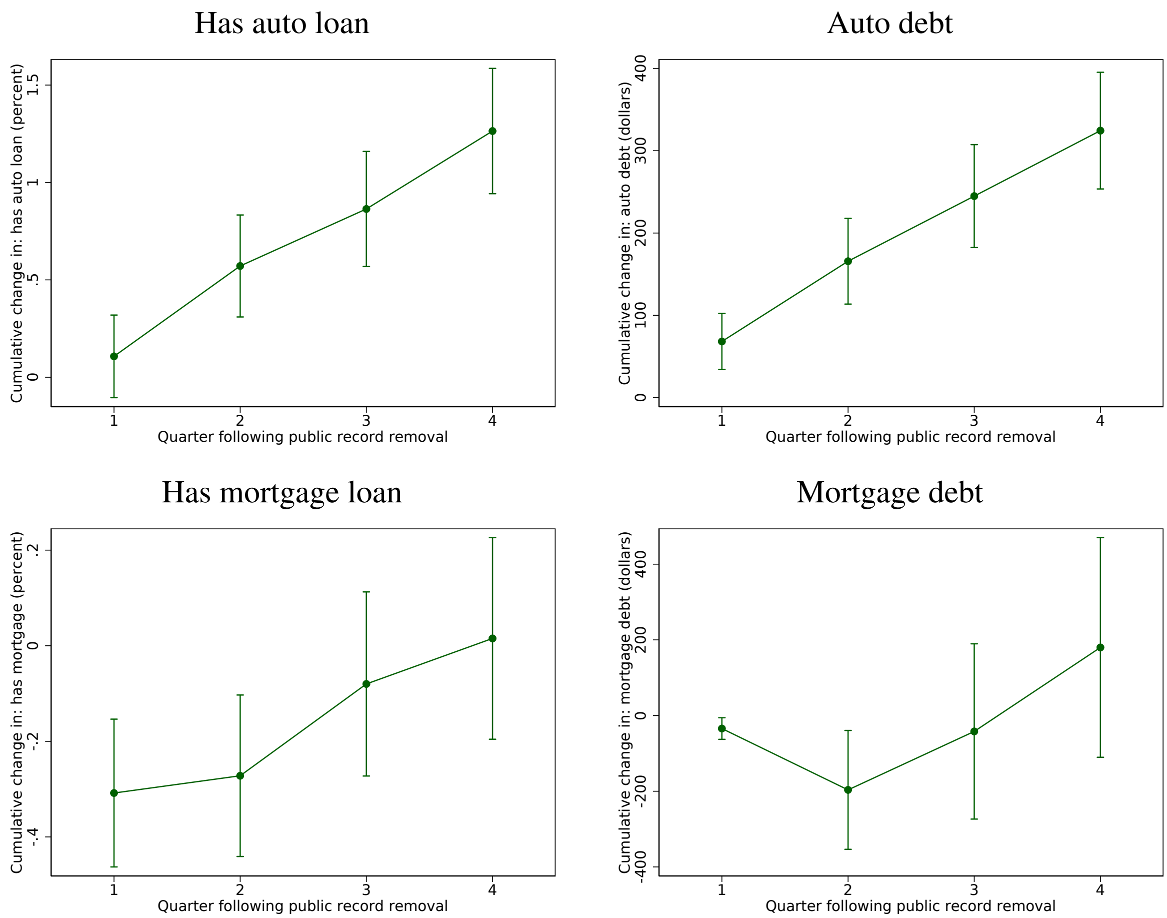
Notes: Each panel shows the cumulative marginal effects of the exogenous removal of unpaid public records on the selected outcome variable estimated using Equation (14). To standardize the effect size, the margin for each removal reason is calculated using the average number of unpaid public records removed (1.39), the fraction that go to zero unpaid public records (0.93), and the average age (3.24 years) in the NCAP purge. Each estimate includes date effects, a cubic in lag credit score, a cubic in age, and the removal of paid public records, the effects of which are not shown. The sample includes only credit records with a public record since 2012. Bars represent 95% confidence intervals. Standard errors are clustered at the credit record level. To reduce the influence of outliers and reporting errors, we winsorize the top and bottom 1% of changes in the non-indicator outcome variables. Source: Authors' calculations from CCP.

Figure 10: Dynamic causal estimates of increases in credit score from removing public records among population with a public record



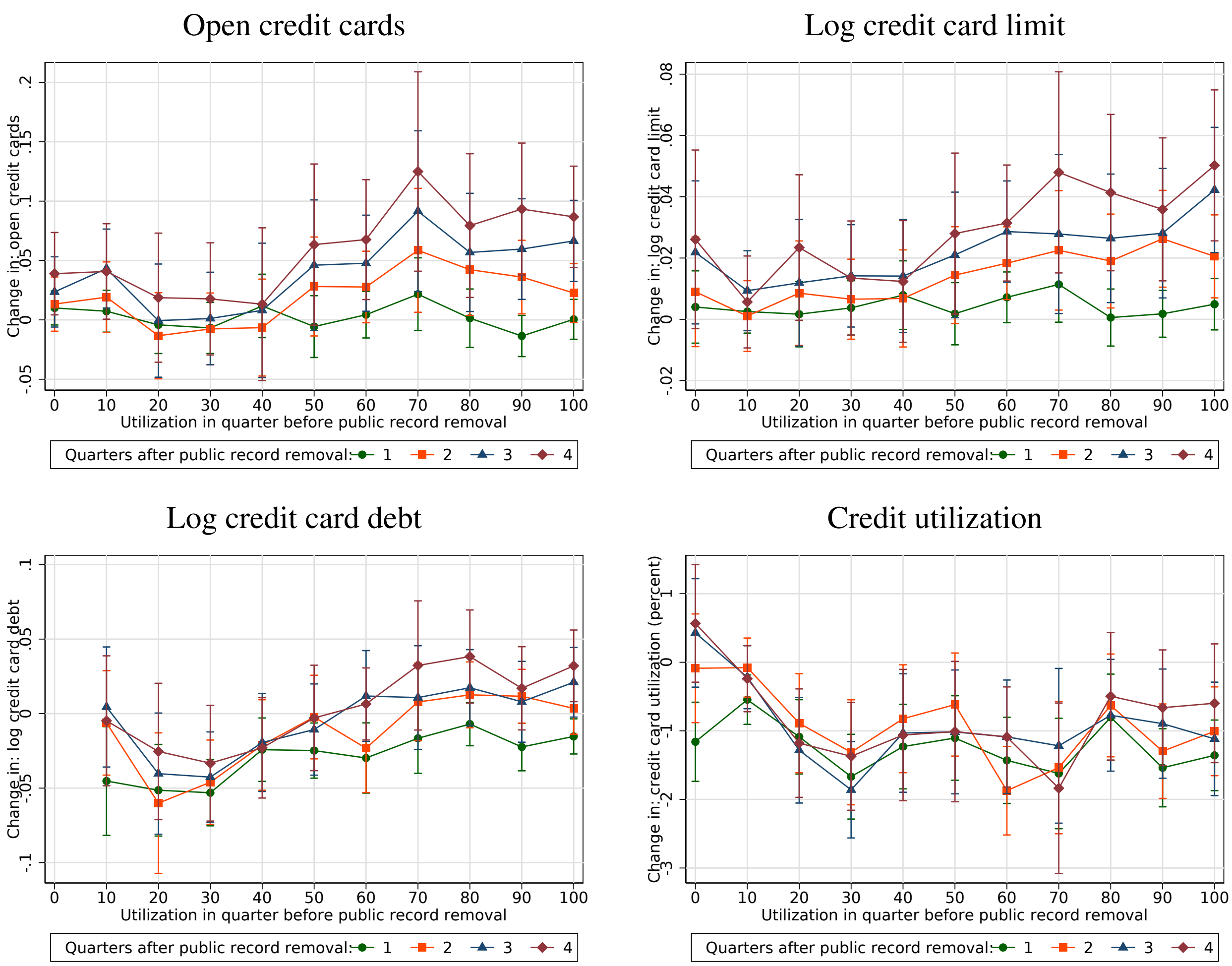
Notes: Each panel shows the LP-IV coefficients for cumulative change in the selected outcome variable from an exogenous change in credit score in Equation (15). For sake of comparability, a 6.54 point change in credit score is considered, which is the change in credit score estimated in the first stage. Each estimate includes date effects, a cubic in lag credit score, and a cubic in age. The sample includes only consumers with a public record since 2012. Bars represent 95% confidence intervals. Robust standard errors are clustered at the credit record level. To reduce the influence of outliers and reporting errors, we winsorize the top and bottom 1% of changes in the non-indicator outcome variables at each horizon. Source: Authors' calculations from CCP.

Figure 11: Dynamic causal estimates of increases in credit score from removing public records among population with a public record



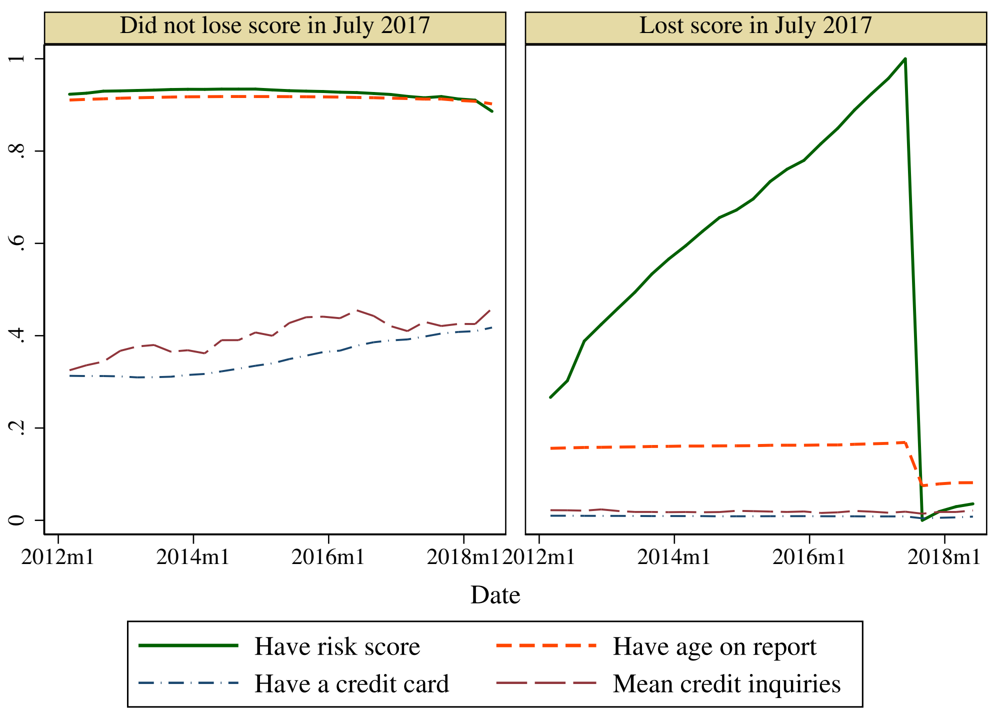
Notes: Each panel shows the LP-IV coefficients for cumulative change in the selected outcome variable from an exogenous change in credit score in Equation (15). For sake of comparability, a 6.54 point change in credit score is considered, which is the change in credit score estimated in the first stage. The sample includes only consumers with a public record since 2012. Bars represent 95% confidence intervals. Robust standard errors are clustered at the credit record level. To reduce the influence of outliers and reporting errors, we winsorize the top and bottom 1% of changes in the non-indicator outcome variables at each horizon. Source: Authors' calculations from CCP.

Figure 12: Heterogeneous effects by credit utilization



Notes: Source: Authors' calculations from CCP.

Figure 13: Fraction of credit records having a credit score, age, and credit card on the record, and mean credit inquiries by whether lost credit score between June and September 2017



Notes: Source: Authors' calculations from CCP.

Table 1: Dynamic causal impacts of public record removal

TO BE ADDED

Table 2: Equilibrium effects of public record removal: overall time effects

	Change from the previous year: Has open credit card	Open credit cards	log CC limit	Has auto loan	Auto debt	Has mortgage	Mortgage debt
I(Year after NCAP)	-0.00621***	0.0229***	-0.0180***	0.000371	-102.6***	0.00942***	1,095***
X I(Below 700 at t-1)	(0.000287)	(0.00149)	(0.000774)	(0.000550)	(10.47)	(0.000342)	(68.51)
I(Below 700 at t-1)	0.0215***	0.0694***	0.0618***	0.00120***	-42.97***	-0.0131***	-4,072***
	(0.000122)	(0.000629)	(0.000326)	(0.000233)	(4.435)	(0.000145)	(29.01)
I(July 2015)	-0.000346*	-0.0736***	0.00784***	0.00349***	26.70***	-0.0127***	-2,208***
	(0.000187)	(0.000966)	(0.000489)	(0.000358)	(6.811)	(0.000223)	(44.56)
I(July 2016)	0.00187***	-0.0565***	0.0188***	0.00599***	55.40***	-0.00831***	-760.5***
	(0.000187)	(0.000969)	(0.000488)	(0.000359)	(6.826)	(0.000223)	(44.66)
I(July 2017)	-0.00561***	-0.0786***	0.00570***	-0.00160***	-116.0***	-0.00843***	-277.2***
	(0.000186)	(0.000964)	(0.000485)	(0.000357)	(6.795)	(0.000222)	(44.46)
I(July 2018)	-0.00480***	-0.159***	-0.0124***	-0.00922***	-186.4***	-0.0160***	-1,245***
	(0.000234)	(0.00121)	(0.000555)	(0.000448)	(8.525)	(0.000279)	(55.77)
I(July 2019)	-0.00858***	-0.146***	-0.0177***	-0.0162***	-312.5***	-0.00923***	-753.9***
	(0.000186)	(0.000963)	(0.000483)	(0.000357)	(6.787)	(0.000222)	(44.40)
Observations	18,535,994	18,505,459	13,916,948	18,505,459	18,505,459	18,505,459	18,505,459
R-squared	0.003	0.009	0.035	0.002	0.001	0.005	0.005
State effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: I(Below 700) is the an indicator for the individual having a score below 700 one earier. Limit, number of card, and debt variables are winsorizd at 0.5% and 99.5%. Omitted date is July 2014. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' calculations from the CCP.

Table 3: Equilibrium effects of public record removal: Redistribution across state 50-point bins

	Change from the previous year						
	Has open credit card	Number credit cards	log CC limit	Has auto loan	Auto debt	Has mortgage	Mortgage debt
	Panel A: Wide controls						
State-Score bin PR exposure	-0.00129***	-0.000382	-0.00891***	-0.000886**	-11.22	-7.69e-05	-150.4***
X I(Year after NCAP)	(0.000322)	(0.00142)	(0.00130)	(0.000362)	(7.476)	(0.000243)	(51.93)
State-Score bin PR exposure	-0.00256***	-0.00751***	-0.00978***	-0.000509	3.345	-0.00112***	71.55
	(0.000493)	(0.00253)	(0.00149)	(0.000377)	(9.265)	(0.000355)	(81.44)
Observations	14,911,442	14,889,624	11,315,428	14,889,624	14,889,624	14,889,624	14,889,624
R-squared	0.025	0.039	0.073	0.004	0.003	0.007	0.008
One and two year lag score	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Date effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	Panel B: Narrow controls						
State-Score bin PR exposure	-0.00222***	-0.00206	-0.0129***	-0.00160***	-17.03**	0.000305	-115.5**
X I(Year after NCAP)	(0.000375)	(0.00172)	(0.00168)	(0.000358)	(8.327)	(0.000242)	(52.75)
State-Score bin PR exposure	-0.00904***	-0.107***	-0.0628***	-0.00821***	-175.2***	-0.00856***	-1,563***
	(0.00108)	(0.00946)	(0.00661)	(0.000746)	(13.79)	(0.000687)	(134.8)
Observations	18,535,994	18,505,459	13,916,948	18,505,459	18,505,459	18,505,459	18,505,459
R-squared	0.004	0.013	0.039	0.002	0.001	0.005	0.006
One and two year lag score	No	No	No	No	No	No	No
Date effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County effects	No	No	No	No	No	No	No
Age effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: State-Score bin PR exposure is the z-score normalized change in each 50 score bin in each state in the fraction the population with a public record between June and September 2017. Individuals may change bins over time as their scores change. Limit, number of card, and debt variables are winsorizd to the at 0.5% and 99.5%. The one and two year lag score is interacted with score bins so dynamics may vary by score. Panel B includes state effects. Standard errors are clustered at the state-50 score bin level, *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' calculations from the CCP.