# Disaster Flags: Credit Reporting Relief from Natural Disasters

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**Abstract**

I study the use of 'disaster flags' applied to credit reports to provide relief to consumers affected by natural disasters. Between 2010 and 2020, 59 million consumers have a disaster flag on their US credit report. Flags tag a riskier subset of consumers' tradelines exposed to disasters and temporarily mask defaults on credit reports. Consumers with pre-disaster financial distress experience the largest, but temporary, VantageScore credit score increases from flags. Flags do not increase credit access. Counterfactual policies offering more relief by comprehensively masking all defaults during disasters appear proportionate with limited informational loss to lenders.

**Keywords:** Climate Finance, Consumer Financial Protection, Credit Information, Disaster Flags, FEMA, Household Finance, Natural Disaster, Social Insurance.

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# 1 Introduction

The United States is increasingly affected by more numerous and more economically damaging natural disasters such as hurricanes, tornadoes, wildfires and floods.[1] Such climate change poses many challenges for financial markets (e.g., Giglio et al., 2021). Climate change means some consumers are increasingly exposed to events causing them financial distress. Consumer financial distress in the form of temporarily missing one or more payments ('defaults') on their consumer credit report can have longer-term adverse effects including reduced credit access. This is because information on historical defaults from the last seven years remains on credit reports and are a key input to credit scores.

In this paper I consider the role for masking defaults in credit reports to provide relief to consumers exposed to natural disasters. Masking defaults may mean trading-offs heterogeneous impacts: improving credit access for masked defaulters who appear less risky to lenders but reducing credit access for non-defaulters who appear riskier having been pooled with masked defaulters. Crucially whether to mask defaults occurring during natural disasters ('disaster defaults') depends on how informative such data are at predicting future default. If disaster defaults are highly predictive then masking this information would be expected to be costly to lenders and reduce the market efficiency of lending. Whereas if disaster defaults offer limited predictive value then a social planner may want to mask them as it would indicate being adversely affected by natural disasters is more bad luck rather than informatively revealing a consumer's type and future behavior.

I research this topic by documenting and evaluating the existing, voluntary system of natural disaster credit report relief developed by the market and also considering counterfactual government social insurance policies – masking defaults during natural disasters – that could be implemented. I study how lenders respond to natural disasters by applying 'disaster flags' to their customers' credit reports. Disaster flags are designed to provide relief to consumers and protect their credit access following disasters such as hurricanes, forest fires, and COVID-19. Lenders voluntarily apply disaster flags. These flags temporarily mask a consumer's negative information (i.e. defaults) in the calculation of VantageScore credit scores. The only other study on this topic is a short Consumer Financial Protection Bureau report (Banko-Ferran and Ricks, 2018) that concludes "more

[1] Between 1980 to 2010, there were only two years (1998 and 2008) with at least ten disasters each resulting in damages of one billion dollars or more. In contrast, every year 2011 to 2022 except one (2014) witnessed at least ten disasters each causing damages of one billion dollars or more. In 2020, there were a record-breaking number of twenty-two billion-dollar disasters. Source: National Oceanic and Atmospheric Administration (NOAA) National Centers for Environmental Information (NCEI) U.S. Billion-Dollar Weather and Climate Disaster. Billion-dollar disasters are inflation-adjusted.

analysis is needed to better understand whether and how the furnishing of information on natural disasters affects consumer credit". My paper addresses this research gap.

I document five new facts on disaster flag's use from examining 23 years of monthly US consumer credit reporting data. First, disaster flags commonly appear on US credit reports. 59.2 million consumers had a disaster flag on their credit report between 2010 and 2020. This is over 3.5 times the number of consumers bankrupt over the same period. Second, the prevalence of disaster flags greatly increased over time. Disaster flags were introduced following 9/11, were rarely used until Hurricane Katrina in 2005, with use increasing tenfold in 2017 with Hurricanes Harvey and Irma. Third, there is broad geographic coverage of disaster flags. In the 2000s and early 2010s disaster flags were mainly used in the South East coastal areas but in the late 2010s have been used in other areas such as the West of the country that has been affected by wildfires, and were used across the entire US in response to the COVID-19 pandemic. Fourth, disaster flags typically only remain on a credit tradeline for a few months: 90% for less than six months. Fifth, disaster flags are typically only applied to a subset of consumers' credit tradelines.

Why would lenders voluntarily apply disaster flags? I evaluate this by examining the informational value of flagged defaults to understand how costly it is for lenders to voluntarily remove such information. I find consumers with disaster flags are adversely selected with ex-ante lower credit scores, more credit tradelines, and being more indebted. I construct credit scoring models to quantify how different the predictive value of a flagged default is from a non-flagged default. I find that flagged defaults are riskier than non-flagged defaults: but only when they have occurred in the last six months, otherwise they are similar. While a model predicting future credit default that masks flagged defaults as an input performs worse than a model without such masking, I interpret the difference between the two is economically small. It therefore appears lenders incur a seemingly small cost to voluntarily, temporarily applying such disaster flags to mask defaults.

What are the benefits to consumers of disaster flags? I use event study and difference-in-differences methodologies to study this. Disaster flags mask some defaults leading to an economically small 0.2% increase in average VantageScore credit scores. These average effect sizes are smaller than that found in research on the effects of removing bankruptcy flags from US consumer credit reports (e.g., Dobbie et al., 2020; Gross et al., 2020; Jansen et al., 2023). There is heterogeneity in the effects of disaster flags on credit scores. The effect is larger 11 to 18 points (1.9-3.3%), for the subgroup of consumers with low (subprime) credit scores or those with any defaults twelve months before the disaster flag was applied.

Even among consumers who received the largest, boosts to their credit scores, I do not find evidence of flags improving these consumers' real economic outcomes of credit access. Instead credit access significantly declines. Flags not improving credit access is partially because the credit score increases I find are temporary: dissipating within twelve months and turning negative for some consumers. Furthermore, although disaster flags temporarily affect VantageScore credit scores (observed in my data), they do not affect FICO credit scores (unobserved in my data), and therefore credit decisions taken using FICO scores would be unaffected. While a growing literature studies the effects of disasters and government aid on household finances (e.g., Billings et al., 2022; Gallagher and Hartley, 2017; Farrell and Greig, 2018; Deryugina et al., 2018; Bleemer and van der Klaauw, 2019; Gallagher et al., 2023), my paper contributes to studying the effects of a little-known but widely-used form of relief.

In the last part of the paper, I consider counterfactual, mandatory social insurance regimes automatically masking all defaults in credit reports for consumers exposed to natural disasters. Such place-based policies may be motivated by redistributive aims (e.g., Gaubert et al., 2021) given the large and persistent geographic inequalities in financial distress across the US (e.g., Keys et al., 2022). I construct counterfactuals by merging in government data on the timing and location of natural disasters. I find these would mask 7 to 18% defaults from credit reports. Yet there appears little trade-off from doing so. Masking disaster defaults would have a small reduction in credit risk predictive performance – especially small relative to the large quantity of information masked. To help interpret the magnitude, I benchmark this against a scenario where all defaults (i.e. irrespective of whether during a disaster or not) are masked: an alternative which more noticeably reduces predictive performance. The limited trade-off of masking disaster defaults could make it proportionate policy – in a similar way to how medical debts are increasingly not included in credit reports due to fairness concerns with limited predictive loss.[2]

These counterfactuals provide evidence to inform public policy discussions pertaining to the potential for providing credit reporting relief from natural disasters (e.g., Banko-Ferran and Ricks, 2018; National Consumer Law Center, 2019; Urban Institute, 2019; FinRegLab, 2020). The topic of understanding the implications of masking information from credit reports is important more broadly. It has received greater public attention in the wake of the COVID-19 pandemic which resulted in laws in US via the Coronavirus Aid, Relief, and Economic Security (CARES) Act preventing lenders from updating adverse information in credit reports. The UK and Canada also introduced regulations in 2020 preventing worsening status of consumers' credit reports during COVID-19. In this con-

[2] https://newsroom.transunion.com/equifax-experian-and-transunion-support-us-consumers-with-changes-to-medical-collection-debt-reporting/ https://vantagescore.com/major-credit-score-news-vantagescore-removes-medical-debt-collection-records-from-latest-scoring-models/ https://www.whitehouse.gov/briefing-room/statements-releases/2023/02/14/fact-sheet-new-data-show-8-2-million-fewer-americans-struggling-with-medical-

text, understanding the role of positive information sharing is increasingly important (e.g., Guttman-Kenney and Shahidinejad, 2023). Prior literature has studied the effects of changes in credit contract terms such as reductions of principal or monthly payments to alleviate consumer financial distress (e.g., Agarwal et al., 2017; Dobbie and Song, 2020; Ganong and Noel, 2020; Cherry et al., 2021). My study of credit report relief from natural disasters adds consideration of a new policy tool to prior literature. Disaster flags are a form of relief that do not change the contract terms, just how information on a contract is reported.

One can also consider my study of disaster flags in the context of the social insurance literature. One of the main roles of public policymaking is to provide social insurance: Providing insurance against adverse shocks such as becoming unemployed or suffering poor health (e.g., Feldstein, 2005; Chetty and Finkelstein, 2013) or distress from a natural disaster.[3] Deryugina (2017) shows the fiscal costs of social insurance payments (e.g., unemployment insurance, public medical payments) significantly outweigh direct disaster aid. It may be proportionate to use disaster flags to 'tag' (e.g., Akerlof, 1978) a group of consumers affected by natural disasters. By empirically studying a voluntary form of social insurance that redistributes credit scores I add to the previous literature on social insurance which finds less use of tags than theory would recommend (e.g., Mankiw et al., 2009; Weinzierl, 2012). The counterfactual policies I consider are feasible place-based policies targeted to regions and time periods suffering financial distress.

The paper proceeds as follows. I explain the data used in my paper in Section 2. Section 3 provides a motivating framework for studying the credit information of natural disasters. I then explain the institutional details of disaster flags in Section 4. Section 5 documents new facts of how disaster flags are used. Section 6 shows the characteristics of consumers with disaster flags and examines the informational value of masking defaults. Section 7 evaluates the consumer benefits of disaster flags. I conduct counterfactuals in Section 8 for how alternative social insurance regimes masking defaults during natural disasters. Finally, Section 9 concludes.

[3] See Hsu et al. (2018); Bornstein and Indarte (2023); Braxton et al. (2023) for studies of the connections between social insurance and household debt.

# 2 Data

## 2.1 Consumer Credit Reporting Data

This research utilizes a large, anonymized, representative sample of US consumer credit reporting data: the University of Chicago Booth School of Business TransUnion Consumer Credit Panel (BTCCP). The BTCCP is provided by TransUnion to the University of Chicago Booth School of Businesses Kilts Center for Marketing (TransUnion, 2023).[4] These data are a 10% sample of consumers with a TransUnion credit report in July 2000 supplemented with 10% of new entrants added each month to ensure the sample remains representative.

Data are at the individual tradeline account level (e.g., a particular mortgage or credit card account) at the monthly frequency from July 2000 to December 2022. Each month of data is a 'retro archive' that recreates the consumer's credit report as it would have appeared at that point-in-time and as lenders would take credit decisions on. Individual tradelines are tracked over time and linked to anonymized furnisher and anonymized consumer identifiers. From January 2009, these data contain more detailed data and so my research focuses on this period.

Each month these data also show a consumer's credit score: VantageScore 3.0. For each consumer, each month I observe their primary address geography with state, zipcode, and the census block group. Census block groups are units of geography typically containing 600 to 3,000 consumers and are more granular than census tracts. I keep observations for consumers with a birth date and restrict to where the birth year is before 1920 or after 2004 and when a consumer never has tradeline data in order to remove low-quality, fragmented credit records.

## 2.2 Natural Disasters Data

When a disaster occurs, it is declared as such by the US President under the Stafford Act. I use public government data on these declarations provided by the Federal Emergency Management Agency (FEMA)'s Disaster Declarations Summaries.[5] I restrict analysis to natural disasters (e.g., flooding, hurricanes, wildfires, severe storms, tornados): this excludes chemical, toxic substances, terrorist, or other disasters events. These data

[4] TransUnion is one of three US credit bureaus along with Equifax and Experian. The BTCCP sample has been used in prior published research (e.g., Kluender et al., 2021; Guttman-Kenney et al., 2022; Keys et al., 2022).

[5] https://www.fema.gov/openfema-data-page/disaster-declarations-summaries-v2

report the timing and location of all federally declared disasters. These data are merged to BTCCP by county, state, and date.

# 3 Motivating Framework

I provide a stylized framework of credit scoring that provides the motivation for this paper. The basis of most lending decisions in the US and other countries are credit scores (derived from credit reporting data) predicting the likelihood of future default (missed payment). A credit applicant's credit score determines whether their application is accepted and, if so, what contractual terms are offered (e.g., interest rate and amount of credit). A higher credit score represents a lower probability of default (lower credit risk). Equation 1 shows a simple example, where a credit score is predicting at time $t$, an outcome ($Y_{i,t+j}$), the likelihood of consumer $i$ defaulting on a credit agreement by $j$ periods in the future i.e. $Y_{i,t+j} \equiv \max_{s=1}^{j} D_{i,t+s}$ where $D = 1$ if default, $D = 0$ if otherwise. The credit score has some generic function $f(.)$ – historically this is typically a logistic – where I have partitioned the inputs into a default component ($D_{i,t}$), and a vector of all other non-default inputs ($X'_{i,t}$) such as product holdings, balances, credit card utilization.

$$Pr\Big(Y_{i,t+j} = 1\Big) = f\Big(X'_{i,t}\beta_1 + \theta_1 D_{i,t}\Big) \tag{1}$$

In such models, past defaults are a strong predictor of future defaults with $\theta_1 > 0$. Consumers with past defaults have lower credit scores resulting in lower access to credit and facing higher interest rates.

As credit scores are predictive models the relationships between input and output variables are not causal. Credit scoring models regard the predictive value of a default as being homogeneous irrespective of the underlying cause or heterogeneity by socio-economic characteristics – even though doing so masks variation that may improve prediction. This is due to a mixture of a lack of data and legal constraints limiting the predictive accuracy of credit scoring models. For examples, lenders have limited visibility of life events such as income shocks and, for equity reasons, cannot discriminate on the basis of protected characteristics such as gender and race.

One source of heterogeneity that is observable in data and is not a protected characteristic is whether defaults differ with natural disasters (e.g., wildfires, floods, hurricanes). Does the ability to predict future defaults vary depending on whether a default occurs during a natural disaster or not? Equation 2 allows for this by including an interaction term between the default term ($D_t$) and a binary variable for the geographical area $g$ and time $t$ experiencing a natural disaster ($N_{g,t}$).

$$Pr\Big(Y_{i,t+j} = 1\Big) = f\Big(X'_{i,t}\beta_2 + \theta_2 D_{i,t} + \pi(D_{i,t} \times N_{g,t})\Big) \tag{2}$$

Comparing measures of predictive performance (e.g., AUROC) between the models in Equations 1 and 2 shows whether allowing for a differential impact of natural disaster defaults to other defaults can improve credit risk prediction.

The value of the $\pi$ parameter in Equation 2 is informative of the marginal predictive value of defaults during natural disasters ('disaster defaults') compared to non-disaster defaults. It may be that $\pi < 0$ meaning that disaster defaults are lower risk than non-disaster defaults. This could be due to disasters being exogenous shocks to households, disasters disrupting communications making it difficult for households to make payments on-time, and households being better able to recover due to Federal assistance that may only arrive with a lag so be unable to prevent the original default. Conversely, disaster defaults may be higher risk ($\pi > 0$) than non-disaster defaults - possibly due to disasters causing longer-term damage to household resilience. If $\pi = 0$ and predictive performance does not improve then differentiating disaster defaults from non-disaster defaults does is not informative for improving credit risk prediction. If disaster defaults are uninformative noise then predictive performance may even be improved by masking such information.

Given this framework one can quantify how costly it would be for the credit industry to mask disaster defaults in credit reports. This adapts Equation 1 to Equation 3 where any disaster defaults are recorded as not in default. Comparing the predictive performance of these two models: if the difference is small the credit industry may voluntarily agree to mask disaster defaults, however, if it is large they would be reluctant to and then the government then has to decide the merits based on its social welfare function. While my study is of natural disaster defaults, this framework could be applied to evaluate other characteristics with richer data merged in e.g., defaults linked to life events such as divorce, income shocks, or expenditures shocks.

$$Pr\Big(Y_{i,t+j} = 1\Big) = f\Big(X'_{i,t}\beta_3 + \theta_3 \tilde{D}_{i,t}\Big), \text{ where } \tilde{D}_{i,t} \begin{cases} 0 \text{ if } N_{g,t} = 1 \\ D_{i,t} \text{ otherwise} \end{cases} \tag{3}$$

Depending on how strict credit reporting practices are, there may be heterogeneity in whether lenders report disaster defaults. Some lenders may choose to mask disaster defaults for a variety of reasons. For example, doing so may provide the lender with private information advantage over its competitors, help to increase customer retention, or it may view it as being their preferred approach for helping distressed consumers for non-profit reasons. It may also be that when borrowers default during a natural disaster, lenders learn private information on heterogeneous consumer circumstances and select a subset of disaster defaults to mask. With this framework in mind I study how firms currently mask information during natural disasters using 'disaster flags'.

## 4 What Are Disaster Flags?

Lenders may apply a 'disaster flag' to a tradeline on their consumer's credit report to show the consumer has been affected by 'natural or declared disasters'. These flags were introduced following September 11, 2001 terrorist attacks. Disaster flags aim to provide relief to consumers by protecting credit access following natural disasters such as hurricanes, forest fires, and COVID-19.

There are no governmental or regulatory requirements for lenders to use disaster flags. The industry body's guidance by the Consumer Data Industry Association (CDIA) is not prescriptive on lenders use of disaster flags.[6] Lenders have complete discretion over whether to apply disaster flags and, if so, which consumers and tradelines to apply it to (e.g., all or a subset in an area subject to a natural disaster), and how many months to keep flags on a consumer's credit report for. Disaster flags are a separate field to the reporting of defaults in credit reports. Discussions with industry participants indicate some lenders sometimes do not report new defaults during natural disasters, however, is unclear how common such unreported defaults are. While not the focus of this study, such non-reporting of defaults may explain why average effects of natural disasters on defaults observed in credit reporting data found in prior literature have been described as 'modest' (Gallagher and Hartley, 2017). Disaster flags may be applied instead of or in addition to changes in contract terms (e.g., deferring payments or offering forbearance) that may also get recorded in credit reports.[7]

Disaster flags mask negative information only on the flagged tradeline in the calcu-

[6]Credit Reporting Resource Guide (CRRG) FAQ 58 explains how these are recorded in credit reports with a comment code 'AW' added to the tradeline. In TransUnion data the comment (remark) code is technically named 'AND' instead of 'AW'.

[7]CRRG FAQ 44 and 45 explain how these 'accommodations' are recorded in credit reports by setting payments due equal to zero or adding codes to show that the payment is deferred or the agreement is in forbearance. The Consumer Data Industry Association defines a deferred payment as "A loan arrangement in which the borrower is allowed to start making payments at some specified time in the future." and forbearance as: "A period during repayment in which a borrower is permitted to temporarily postpone making regular monthly payments. The debt is not forgiven, but regular payments are suspended until a later time...The consumer may be making reduced payments, interest-only payments or no payments."

lation of VantageScore: a widely-used, mainstream credit score.[8] Flags only mask information when they are currently present on an tradeline: once a flag is removed, masked information is revealed. Disaster flags do not factor into the calculation of FICO credit scores i.e. they do not mask negative information.[9] Manual underwriters reviewing a consumers' credit report observe disaster flags and may consider these in their credit decision.

There are potential parallels between the addition of a disaster flag to the removal of a bankruptcy flag 7 to 10 years after bankruptcy studied in many prior papers (e.g., Dobbie et al., 2020; Gross et al., 2020; Jansen et al., 2023). Both of these mask information in credit reports resulting in the pooling of consumers with different credit risks. One may even consider 'disaster flags' as a type of temporary, low-cost bankruptcy providing a non-governmental form of social insurance for consumers affected by disasters.

# 5 Disaster Flag Facts

I document five new facts describing the use of disaster flags in US consumer credit reports over twenty years. I observe disaster flags in my credit reporting data (BTCCP). Each tradeline, each month these data show whether a disaster flag was applied. This monthly, tradeline level view is crucial. Disaster flags would not be visible in variables aggregated to the consumer-level. Quarterly (or annual) tradeline-level data, would not observe disaster flags applied intra-quarter unless flags were still present on a tradeline at the end of a quarter.

## 5.1 FACT 1. 59.2 million consumers had a disaster flag on their credit report between 2010 and 2020.

Across my entire dataset covering July 2000 - December 2022, 67.6 million consumers had a disaster flag on their credit report. This is a disaster flag on at least one open tradeline on their consumer credit report for at least one month. Between 2010 and 2020, 59.2 million consumers had a disaster flag.[10] To help to evaluate how 'big' such numbers are: this

[8] https://vantagescore.com/newsletter/did-you-know-credit-reporting-and-natural-disaster
https://vantagescore.com/2022-market-adoption-study/

[9] https://www.fico.com/en/covid-19-credit-reporting-impact-US/

[10] If closed tradelines are included this is 70.2 mn (2000 - 2022), 67.0 mn (2012 - 2022), 61.6 mn (2010 - 2020) consumers. This only open tradelines with positive balances are included this is 62.8 mn (2000 - 2022), 59.8 mn (2012 - 2022), 54.6 mn (2010 - 2020) consumers.

is over 3.5 times the number of US consumers becoming bankrupt over 2010 to 2020.[11] The large number of consumers with disaster flags on their credit reports makes this an important practice to understand.

## 5.2 FACT 2. A level shift in disaster flag use in 2017 with Hurricanes Harvey and Irma.

Disaster flags were very rarely used until 2005 when Hurricane Katrina hit. Figure 1, Panel A displays disaster flag use has greatly increased over time. The growth is so large in 2017 that I separately present the 2000-2017 period in Panel B with a ten times smaller scale to be able to see this earlier period. The growth over time is consistent with Banko-Ferran and Ricks (2018) which examined Hurricane Harvey and found very few tradelines in Texas already had disaster flags in the months just before it hit.

Flags are applied across all mainstream credit types (e.g., auto loans, credit cards, mortgages, and student loans) and across all lender types (banks, non-bank finance companies, and credit unions) – more details are in Online Appendix Table A1 and Figures A1 and A2. Flags are typically applied to tradelines without deferments: except for student loans and during COVID-19 when Federally-mandated payment deferments occurred more broadly. Deferments cover when deferments are listed on the tradeline or when tradelines have positive balances but zero payments due. Between 2009 and 2022, 15% of all tradelines (excluding student loans) that have disaster flags are also deferred at the same time (Online Appendix Figures A3 and A4). During the pre-COVID-19 period, 2009 to 2019 then only 6% are also deferred. Between 2009 and 2022, 75% of student loans with disaster flags are also deferred and between 2009 and 2019, this is 93%. While the focus of my study is of relief from natural disasters, there is little research evaluating the effectiveness of relief in credit reports beyond the COVID-19 pandemic (e.g., Cherry et al., 2021; Kim et al., 2022) and further investigation of this topic can help to inform lenders' practices and policymaking.

## 5.3 FACT 3. Broad geographic usage of flags.

In the 2000s and most of the 2010s credit report disaster flags were mainly used in the South East coastal areas which are more prone to hurricanes. Figure 2 displays the fraction of consumers in US counties with a credit report who had any disaster flag for each

[11] Credit reports contained 161 million bankruptcies with filing dates 2010 - 2020 based on chapter 7 or chapter 13 filings, dismissals, or discharges observed. For 2012 - 2022, it is over five times the number who became bankrupt: 64.4 million consumers had disaster flags compared to 12.6 becoming bankrupt.

year 2015 to 2022. This pattern is still visible during 2016-2019 in Panels B to E of Figure 2. There were also some lower incidence pockets of usage elsewhere in the country in other months such as areas of the Northwest affected by California wildfires.

Panel F displays how disaster flags were used by lenders across the country in response to COVID-19 and other natural disasters in 2020. Coverage is broad based across counties, however, there is noticeable regional variation in the intensity of usage.

### 5.4 FACT 4. The majority of flags only remain on a credit tradeline for a few months.

Figure 3, Panel A shows the persistence of disaster flags remaining on a tradeline over time since a flag was first applied. It is at the discretion of lenders how long to keep disaster flags on credit report tradelines for. I observe, disaster flags typically only remain on a credit tradeline for up to three months and rarely more than six months. 33% of flagged tradelines are only flagged for one month, 74% for three months or less, 90% for six months or less, and 95% for twelve months or less. These results are broadly similar across lender types (Online Appendix Figure A5), and over time (Online Appendix Figure A6). Figure 3, Panel A, shows flags on auto loans are likely to remain on those tradelines slightly longer than occurs credit cards, mortgages, or student loans. This short duration limits the potential relief disaster flags can provide to consumers given consumers may experience disruption from disasters over a much longer time.

### 5.5 FACT 5. Flags are usually only applied to subset of consumers' credit tradelines

Among consumers with flags, typically only a third of their tradelines on their credit report are flagged: 34% across 2009 to 2022 and 32% as of December 2022. Disaster flags are only attached to the individual tradeline accounts they are listed on. This means a consumer's entire portfolio only has disaster flags on it if all lenders add disaster flags to all their tradelines. I find this is a rare event: only 11% of consumers with at least one disaster flag on one tradeline have disaster flags on *all* of their open tradelines. It is the same both across 2009 to 2022 and as of December 2022. As only a subset of a consumers' tradelines are flagged, this limits the potential relief disaster flags can provide to consumers.

Figure 4 shows the intensive margin of flag use by the number of tradelines held: the fraction of a consumers' tradelines flagged (Panel A) and the share of consumers with all tradelines flagged (Panel B). Panel A shows the fraction of tradelines flagged decreases with the number of tradelines held. Panel B shows it is extremely rare (2% or less) for consumers with three or more tradelines to have flags on all of their tradelines. This indicates some frictions exist in the use of disaster flags although there is a slight trend of increasing intensity of use over time (Online Appendix Figure A7).

# 6 Informational Costs of Disaster Flags Masking Defaults

This section describes selection of consumers with disaster flags (6.1), then builds predictive models (6.2) applying the earlier conceptual framework (from Section 3) to quantify the information value contained in flagged defaults (6.3).

## 6.1 Describing Selection

What are the characteristics of consumers who have disaster flags? It is ambiguous whether selection into disaster flags will be advantageous or adverse and therefore I study this empirically. I examine this in Table 1 by comparing (I) consumers with disaster flags, to (II) consumers without disaster flags in the same geographical region (a combination of census block group and zipcode), and to (III) consumers unflagged across the US.

I find evidence of adverse selection of consumers with disaster flags. Consumers with disaster flags are ex-ante riskier with lower credit scores, with more defaults, and higher indebtedness than 'unflagged' consumers without disaster flags. This selection holds both when comparing to unflagged consumers in the same geographical region and to unflagged consumers across the US.

## 6.2 Predictive Methodology

I take my motivational framework (Section 3) to my data to evaluate the credit risk costs of disaster flags masking defaults in credit reports. I do so by building a series of predictive models. My outcome ($Y_{t+24}$) is any *new* default (90+ days past due) in the next 24 months. I predict this outcome using data to October 2017 to ensure there is a large number of disaster defaults in my data. The model is trained on two-thirds of data and tested out-of-sample on the remaining third. $X_{i,t}$ contains non-default variables (e.g., balances, number of products, debts in collections, length of credit report, bankruptcy) that predict default. $D_{i,t}$ are the default variables: I use a variety of these – measuring the number of defaults in the last 6, 12, 24, 36, and 84 months – to capture how the informativeness of historical defaults data for predicting new defaults may change over time. 84 months is the maximum duration defaults remain on credit reports for.

I construct my own credit scores in order to ensure I can vary the input data these depend on. I use a logistic regression rather than machine learning methods as the former is how most credit scoring models are historically constructed and because I am interested in not only the predictive performance but comparing the coefficients on default parameters.

My baseline predictive model (4) is a traditional credit score including default information. My second model (5) adds an interaction term for defaults flagged by disaster flags. Comparing the coefficients between models informs of informativeness of this variable. The final model (6), adjusts the input data to mask all defaults covered by disaster flags: reclassifying such 'flagged defaults' as not in default. Comparing the predictive performance of model 4 to model 6 shows the information costs of masking defaults.

$$Pr(Y_{i,t+24} = 1) = f\Big(X'_{i,t}\beta_1 + \theta_1 D_{i,t}\Big) \tag{4}$$

$$Pr(Y_{i,t+24} = 1) = f\Big(X'_{i,t}\beta_2 + \theta_2 D_{i,t} + \pi(D_{i,t} \times FLAG_{i,t})\Big) \tag{5}$$

$$Pr(Y_{i,t+24} = 1) = f\Big(X'_{i,t}\beta_3 + \theta_3 \tilde{D}_{i,t}\Big), \text{ where } \tilde{D}_t \begin{cases} 0 \text{ if } FLAG_{i,t} = 1 \\ D_{i,t} \text{ otherwise} \end{cases} \tag{6}$$

## 6.3 Predictive Results

Figure 5 shows the average marginal effects from Equation 5: $\theta_2$ in black and $\pi$ in red. I find $\theta_2 > 0$ which means defaults in the past increase the risk of a consumer defaulting in the future. For flagged defaults in the last six months, $\pi > 0$ and much larger than $\theta_2$: showing that recent flagged defaults are economically and statistically significantly higher risk. For flagged defaults further back in time, $\pi$ is typically statistically insignificant from zero.

I interpret this as flagged defaults quickly lose their information value at predicting future default. This interpretation is corroborated by comparing the out-of-sample predictive performance measured by AUROC (shown in Table 2) from the baseline credit risk model (0.8790) to one masking flagged defaults (0.8786) . Masking flagged defaults therefore reduces predictive performance by approximately 0.05%. Such an economically small cost of reduced prediction can explain why lenders voluntarily, temporarily apply disaster flags.

# 7 Consumer Benefits of Disaster Flags

What are the benefits to consumers of having disaster flags on their credit report? I study this using an event study design with the methodology explained in Subsection 7.1). I then show descriptive results from this for defaults (7.2), credit scores (7.3), and credit access (7.4). Finally, I use a difference-in-differences methodology (7.5) and present results on credit access 7.6.

## 7.1 Event Study Methodology

My event study methodology exploits the timing of disaster flags being applied being quasi-random as a function of the timing and geography of natural disasters. This descriptive methodology evaluates how much consumers' finances have changed relative to their pre-disaster levels. I take the first time a consumer has a disaster flagged applied to their credit report. I exclude consumers where the first time they are flagged only occurs for a student loan since these commonly, contemporaneously have payments deferred. I keep cohorts January 2010 to December 2018 to ensure I observe sufficient pre and post periods of each cohort and to exclude cohorts affected by COVID-19 disruptions. I retain consumers with open tradelines with positive balances and credit scores observed twelve months before first being flagged as a group of active consumers.

This produces a dataset of 2.8 million consumers representative of 28 million consumers. For all of these consumers I construct a balanced panel of 25 months showing twelve months pre and post flags being first applied. This time window is driven by my descriptive evidence given disaster flags only remain on credit reports for a short period of time, any economic effects are expected to be observed within twelve months.

I show results across flagged consumers and heterogeneous effects based on two measures of pre-disaster financial distress. The first heterogeneous measure is whether a consumer's credit score twelve months before first being flagged was a low score 'subprime' (300 - 600) or high score 'non-subprime' (601 - 850). 11.4% of consumers in my sample are subprime. The second heterogeneous measure is whether a consumer has any defaults (30+ days past due) on open tradelines with positive outstanding balances on their credit report twelve months before first being flagged. 5.5% of consumers in my sample have any defaults. These two measures are highly correlated: 76.3% of consumers with any defaults have subprime credit scores, 37.1% of consumers with subprime credit scores have any defaults.

This heterogeneity is motivated by prior research showing effects of natural disasters on consumers' finances vary by pre-disaster financial distress (e.g., Billings et al., 2022).[12] The second measure is also motivated by the institutional details of flags where one would expect the potential gains from using disaster flags to be largest for those with defaults that flags mask.

To assist with interpreting these charts, I add linear time trends to credit score event studies which may be a reasonable counterfactual over a short time horizon for how consumers' credit scores would have evolved without a natural disaster or disaster flag occurring (see Dobbie et al., 2020; Gross et al., 2020, for examples using similar approaches). Linear time trends are calculated from OLS regressions on data t-12 to t-1.

## 7.2 Masking Defaults

I examine the mechanism through which disaster flags can affect credit scores and credit access: masking defaults. The first stage of my event study analysis is presented in Figure 6 examines the prevalence of defaults on credit reports. The black line on Figure 6, Panel A shows the fraction of consumers with any defaults before flag masking in event time. This trends slightly up over time. The orange line masks defaults that occur on tradeline months where flags also appear. Flag masking immediately reduces the fraction of consumers with any defaults appearing on their credit report by 1.5 percentage points. However, it does not go to zero but remains at 5 percent: showing these consumers have defaults on other tradelines without flags and so remain unmasked. The two default series quickly converge within twelve months showing that any potential benefit of flags masking defaults is temporary.

These small, average results are largely driven by the consumers experiencing pre-disaster financial distress. Panels B and C repeat this for heterogeneous cuts of the data by pre-disaster financial distress where t-1 values of defaults are normalized to 0. This shows the temporary effects are concentrated among consumers with pre-disaster financial distress: subprime credit scores or those with any defaults. Flags masks defaults for approximately ten percent of pre-disaster subprime consumers and fifteen percent of consumers with any pre-disaster defaults. But masking of defaults is still only temporary for these groups. There is no discernible difference in defaults before or after masking among consumers without pre-disaster financial distress.

This evidence indicates that any positive effects of flags on credit scores and credit access would be expected to be concentrated on consumers experiencing pre-disaster financial distress and only occur with a few months of the flag being first applied.

[12]Cookson et al. (2023) shows informal crowdfunding via social networks after wildfires are regressive and exacerbates inequality.

## 7.3 Credit Score

How does applying disaster flags affect credit scores? Figure 7, Panel A finds an average increases to credit scores of 2 points in the month the flag was applied and by 1.5 points after twelve months. This is an increase of 0.2 percent relative to the t-1 baseline mean (717) and is little different from that predicted by a linear pre-trend. This average change in credit scores is too small to generate economically meaningful differences in credit access. This change is economically small relative to the approximately 15 points average increase from removing bankruptcy flag from credit reports (e.g., Dobbie et al., 2020; Gross et al., 2020; Jansen et al., 2023). We might not expect the effects of disaster flags to be as large as the effect of removing bankruptcy flags as (i) bankruptcy is an extreme form of financial distress; and (ii) the average positive effect of disaster flags is expected to be a dilution of a larger positive effect from the subset of consumers with defaults.

Panels B and C show how the most financially distressed consumers – those with subprime credit scores or any defaults – receive the largest increases to their credit scores from disaster flags (results for all credit score segments in Online Appendix Figure A9). These panels normalize credit scores for each subgroup relative to their t-1 baseline mean. Financially distressed consumers experience increases of 11 points for subprime consumers (relative to the t-1 baseline mean of 571) and 16 points for consumers with defaults (relative to the t-1 baseline mean of 584). Such effect sizes are similar in magnitude to the effects of bankruptcy flag removal which, in turn, had real effects. However, such increases in credit scores appear short-lived. Within twelve months credit scores become *lower* than that predicted by a linear pre-trend or relative to a control in a differences-in-difference approach. Credit scores of consumers without pre-disaster financial distress are effectively flat: increasing by 0.8 to 0.9 points on baselines of 737 and 726 respectively for Non-Subprime and No Defaults groups.

It is ambiguous whether such temporary increases to credit scores will translate into improved credit access for financially distressed consumers experiencing non-trivial credit score increases. In general, an increase in credit scores would be expected to increase credit access. Our results only apply to VantageScore credit scores which consider disaster flags in their calculation. While I do not observe FICO credit scores, it is reasonable to assume the effects on these would be zero since they do not consider disaster flags in their calculation.[13] This means credit decisions by lenders who use FICO credit scores would be unaffected by disaster flags unless lenders also undertook manual underwriting examining a consumer's raw credit report. Effects through VantageScore are only likely to

[13] https://www.fico.com/en/covid-19-credit-reporting-impact-US/

occur if a financially-distressed consumer applies for credit within a few months of the disaster flag being added during the short time they experience a temporary boost to their score.

## 7.4 Credit Access

I do not find disaster flags improve credit access for consumers who experience pre-disaster financial distress who receive the largest boosts to their credit scores.

I examine the extensive margin of credit access using new account openings. New account openings often have lags of several months before they are recorded on a consumer's credit report (Gross et al., 2021; Gibbs et al., 2023). To address this, I use the variable recording an account's opening date to create a new time series of account openings (rather than the credit report archive date) and record zeros for months where no accounts are opened.

Figure 8, Panel A shows account openings decline over time and there is no sign of improvement following disaster flags being applied. If anything, there are slight *decreases* in credit access with new account openings falling below their linear pre-trend.

Figure 8, Panels B and C show similar conclusions for the consumers experiencing pre-disaster financial distress: there is no sign of improved credit access with estimates remaining around zero relative to the t-1 baseline mean of 0.11. If anything, there is possibly a slight reduction relative to a linear pre-trend.

## 7.5 Difference-in-Differences Methodology

I estimate the causal effects of adding disaster flags to a credit report on consumers using a stacked difference-in-differences empirical design (Cengiz et al., 2019; Deshpande and Li, 2019). Dube et al. (2023) show this stacked approach is equivalent to using a local projections estimator and it corrects for potential bias of negative weighting arising in designs with staggered, heterogeneous, or dynamic treatments. This stacked difference-in-differences approach, differences out the contemporaneous effects of the natural disaster to leave only the effects of the disaster flag. This methodology exploits the timing of disaster flags being applied as a quasi-random function of natural disasters.

I keep consumers that first received a disaster flag between January 2010 and December 2018. This stacked difference-in-differences empirical design stacks data from each flag event study and, for each event, constructs a clean control group of 'unflagged' consumers who are never flagged between July 2000 and January 2020. The control group is constructed using variables calculated twelve months prior to the date the flagged group is first flagged. The clean control of unflagged consumers are in the same combination of geographic area (same census block group $\times$ zipcode), pre-disaster credit score group, and any pre-disaster defaults to the flagged consumer. Within that combination I keep unflagged consumers who are the nearest neighbor in Euclidian distance by standardized credit score, credit card limit, number of trades, outstanding balances, and outstanding mortgage balances. I keep cases where flagged consumers have controls that are close matches (Euclidean distance less than or equal to one) which retains 67% of flagged consumers. This leaves me with a dataset of cohorts of consumers where each flagged consumer is matched with one unflagged consumer. This results in a dataset of 3.77 million consumers representative of 37.7 million consumers. For each of these consumers I take twelve months of observations before and twelve months twelve months after the flagged event to create a balanced panel of observations: 25 months per consumer stacked into a single dataset.

I estimate regression shown in Equation 7 for individual consumer $i$ in cohort $c$ at time $t$. This regression includes fixed effects for each cohort-by-calendar-year-month ($\gamma_{c,t}$) and for each consumer ($\gamma_i$). $Flag_i$ is an indicator taking a value of 1 if a consumer is in the flagged group and a value of 0 if a consumer is in the unflagged control group. Standard errors are clustered at the cohort-level.

$$Y_{i,c,t} = \sum_{\tau \neq -1} \delta_\tau \Big( FLAG_i \times D^\tau_{c,t} \Big) + \gamma_i + \gamma_{c,t} + \varepsilon_{i,c,t} \tag{7}$$

The parameters of interest are $\delta_\tau$ which is the interaction on event time dummies ($D^\tau_{c,t}$) and the $Flag_i$ indicator. Under the assumption of common trends $\delta_\tau$ estimates the effect of disaster flags, among those selected in with suitable controls, on outcomes ($Y_{i,c,t}$) after $\tau$ months.

## 7.6 Difference-in-Differences Results

In line with my event study results, I find disaster flags cause a 1.94 point (s.e. 0.20) average increase to Vantagescore credit scores (relative to a baseline of 723) that dissipate to being insignificant from zero within four months (Online Appendix Figure A10, Panel A). After twelve months the effects are insignificantly different from zero (-0.38, s.e. 0.28).

The average, temporary positive effect of credit scores are driven by consumers experiencing pre-disaster financial distress. Figure 9, Panel A shows at t = 0, consumers with any pre-disaster defaults experience a 3.3% estimated increase in credit score of 18.29 points (s.e. 1.19, baseline mean 556). As credit scores are non-linear predictors of default risk, an 18 point increase for a consumer with a score of 550 is expected to be more valuable than a similarly sized increase for consumers with higher scores. Consumers without pre-disaster defaults experience an increase of 1.31 points (s.e. 0.16) which is less than 0.2% of the baseline mean (730). Effects for both groups dissipate within twelve months to be insignificant from zero. After twelve months the estimated effects for those with any pre-disaster defaults is -1.64 (s.e. 0.96) and for those without pre-disaster defaults is -0.34 (s.e. 0.27). Online Appendix Figure A11, Panel A shows results by credit score group: positive effects are concentrated among consumers with subprime credit scores peaking at t = 0 at 10.86 points (s.e. 1.20), an increase of 1.9% relative to baseline These increases among subprime consumers are short-lived and turn significantly negative within twelve months.

I find no effects of flags increasing credit access. I find no positive effect of flags increasing credit access even for the consumers who received the largest temporary boosts to their credit scores: those with pre-disaster defaults. There is no significant effect on whether a consumer opened any new credit card account each month. Figure 9, Panel B shows credit access declines for both consumers with and without pre-disaster defaults. After twelve months the estimated effects for those with any pre-disaster defaults is significantly negative -0.0042 (s.e. 0.0011): a decline of 23% relative to the baseline mean. The effect for those without pre-disaster defaults is also significantly negative -0.0086 (s.e. 0.0013): a decline of 18% relative to the baseline mean. Results are not specific to new credit cards: examining the number of new accounts opened across all credit types shows the same pattern of results (Online Appendix Figure A12).

There does not appear to be an effect on the intensive margin: Figure 9, Panel B shows no positive effect on the value of new credit card limits. On this margin, consumers with pre-disaster defaults experience no significant difference (estimate -$5.26, s.e. $7.53) relative to their t-1 mean of $66.92 ($2,828 conditional on any new credit card being opened). Whereas consumers with pre-disaster defaults experience a significant decrease of 25% (estimate -$107.30, s.e. $18.20) relative to their t-1 mean of $434.70 ($11,749 conditional on any new credit card being opened).

Results on credit access are consistent when segmenting by credit score (Online Appendix Figure A11, Panels B and C) and when averaging across all consumers (Online Appendix Figure A10, Panels B and C).

Flags not improving credit access may be because credit score boosts are not for a long enough period of time for consumers to realize the potential benefits. Furthermore, although disaster flags temporarily affect VantageScore credit scores (observed in my data), they do not affect FICO credit scores (unobserved in my data), and therefore credit decisions taken using FICO scores would be unaffected. Credit cards are a domain where any positive effects of disaster flags on credit access are most likely to show up as VantageScore is commonly used for credit cards whereas FICO is predominately used for mortgages. This motivates considering a counterfactual system to preserve the credit access of consumers affected by disasters by pooling them with consumers unaffected by disasters.

# 8 Informational Losses From Counterfactuals Masking Disaster Defaults

The existing voluntary regime of disaster flags appears to have limited costs to lenders or benefits to consumers' credit access. What are the feasible alternatives? I quantify the informational losses from counterfactual government policies automatically requiring masking of all defaults during disasters ('disaster defaults'). These counterfactuals are designed to apply equitably to all consumers subject to a disaster: removing selection. Doing so pools consumers affected by disasters with those unaffected by disasters.[14] They are also designed to provide more permanent relief by masking defaults for longer periods of time than disaster flags typically do. Such counterfactual policies have been proposed by consumer organizations (e.g., National Consumer Law Center, 2019; Urban Institute, 2019; FinRegLab, 2020).

If such counterfactual policies were required by law they would affect the underlying credit reporting data which all credit scores (e.g., FICO, VantageScore) and manual underwriters rely on and therefore would be expected to have downstream impacts on consumers' credit access. While I cannot estimate supply responses, the prior research of Cortés and Strahan (2017) studying supply responses to natural disasters more generally may be indicative of lenders being willing to meet increased local credit demand. I consider a larger predictive loss from masking disaster defaults indicates lenders are more likely to restrict credit supply whereas lenders may be expected to more easily absorb a small predictive loss.

To evaluate this I first examine the coefficients ($\phi$) on the interaction term between defaults ($D_{i,t}$) and natural disasters ($FEMA_{g,t}$) in the regression specified in Equation 8. I define defaults as exposed to a natural disaster if the consumer ($i$) resides in county $g$

[14] Practically implementing this would mean disaster defaults are masked before they appear on credit reports that disaster defaults cannot be observed by lenders in their credit decisions. This could either be required of firms furnishing data to credit reporting agencies or required of the credit reporting agencies before they release data to be used by lenders.

where a disaster was declared in the last six months (i.e. $FEMA_{g,t} = 1$ if exposed to a disaster in the last six months, 0 otherwise). This interaction term informs about whether disaster defaults are different from other defaults in informing of a consumer's future risk.

$$Pr(Y_{i,t+24} = 1) = f\Big(X'_{i,t}\beta_4 + \theta_4 D_{i,t} + \phi(D_{i,t} \times FEMA_{g,t})\Big) \tag{8}$$

Figure 5 shows the coefficients from this regression. All the coefficients on disaster defaults are lower than those on all defaults. The disaster defaults terms are generally insignificant from zero but positive over the longest window: 84 months. I interpret this as recent disaster defaults are not especially informative of a consumer's future risk of default. This leads me to evaluate policies masking disaster defaults.

I consider two feasible government policies automatically masking all disaster defaults. Both of these tag consumers based on whether they reside in an area when it was affected by a natural disaster: as this is based on historical location it limits the potential for consumers moving to an area to gain relief. By tagging all consumers in an area affected by natural disasters this is designed to be a more equitable approach than disaster flags where there is adverse selection and unequal potential access to relief based on idiosyncratic lender policies. I tag defaults that occur in disaster areas and disaster time periods over the last 84 months.

The first policy ('Temporary Masking') automatically masks all defaults that occur for consumers residing in a county FEMA reports to be affected by a natural disaster. It masks defaults for six months from the date of disaster (see Equation 9). This is designed to provide temporary relief for consumers – especially those more financially distressed pre-disaster who are most likely to experience disaster defaults. Such an approach would ensure that any defaults during that six month period only affect subsequent credit access if they are present after six months. This limits the ability of temporary adverse shocks to propagate to have long-term impacts. Six months is chosen as a time window long enough to capture temporary financial distress and for consumers to be able to access relief. It is longer than most disaster flags are currently applied for: which appears too short to deliver real benefits. It provides time for consumers to be able to apply for and receive Federal social insurance and disaster aid as well as contacting their creditors to adjust payments if required. It is also short enough to limit potential moral hazard of consumers strategically defaulting. Although empirically moral hazard appears less of a concern than life events (e.g., Low, 2023; Ganong and Noel, 2023) with consumers also being motivated to repay by non-financial concerns (e.g., Bursztyn et al., 2019). More broadly, applying such an automatic flagging approach could be considered more equitable – removing frictions for lenders and consumers – and easier to understand than the existing, ad hoc approach taken by lenders.

$$Pr(Y_{i,t+24} = 1) = f\left(X'_{i,t}\beta_5 + \theta_5\tilde{D}_{i,t}\right), \text{ where } \tilde{D}_{i,t} \begin{cases} 0 \text{ if } \left(D_{i,t} \times FEMA_{g,t}\right) > 0 \\ D_{i,t} \text{ otherwise} \end{cases} \tag{9}$$

The second policy ('Permanent Masking') is designed to provide an upper bound on the amount of disaster defaults masked. Some defaults caused by disasters could take longer than six months to be occur. Or some disaster defaults may be persistently reported in my historical data for more than six months but in a counterfactual world may not be. To address these issues, if a tradeline ($d$) experiences a new default during the six month window my second measure masks defaults not only during that six months but for subsequent tradeline months (see Equation 10).

$$Pr(Y_{i,t+24} = 1) = f\left(X'_{i,t}\beta_6 + \theta_6\tilde{D}_{i,t}\right) \text{ , where} \tag{10}$$

$$\tilde{D}_{i,t} : max \sum_{d=1}^{D} D_{d,i,t} = \begin{cases} 0 \text{ if } \left(\sum_{j=0}^{t} \mathbf{1}\{\Delta D_{d,i,j} = 1\} \times FEMA_{g,j}\right) > 0 \\ D_{d,i,t} \text{ otherwise} \end{cases}$$

These two measures respectively remove 6.7% and 18.4% of all default data present in US credit reports in the seven years of data to October 2017. This is an economically large removal of data from credit reports. Two to four percent of consumers go from some defaults to no defaults in their seven year credit history under these two measures.

I evaluate predictive performance comparing these two models that mask disaster defaults (Equations 9 and 10) benchmarking relative to two extremes: a baseline credit scoring model that uses all defaults data to represent the status quo (Equation 4) and a counterfactual model without any default data (Equation 11) which is also a more intrusive policy one could consider. Across these regressions I use the same baseline outcome of new defaults without masking as this is the outcome that would ultimately impact lenders.

$$Pr(Y_{i,t+24} = 1) = f\left(X'_{i,t}\beta_7\right) \tag{11}$$

Masking disaster defaults reduces the credit risk predictive performance as measured by AUROC from 0.8790 in the baseline to 0.8777 (temporary masking) and 0.8764 (permanent masking) as shown in Table 2. Figure 10 presents the ROC curves where the difference from masking disaster defaults is barely noticeable. These effects of masking disaster defaults can be benchmarked against a counterfactual policy masking all defaults (i.e. disaster and non-disaster defaults) that significantly reduces predictive performance with AUROC declining to 0.8641 and a clear gap in the ROC curve relative to the baseline. I therefore conclude that counterfactual policies masking disaster defaults offer little trade-offs. Such policies would only slightly reduce lenders' abilities to predict future defaults by 0.15 to 0.30%. This reduction in predictive performance appears especially small relative to the quantity of disaster defaults masked (6.7 to 18.4%) and therefore appears a proportionate policy to help provide credit reporting relief for consumers from natural disasters.

# 9 Concluding Discussion

This research provides new evidence of the widespread use of 'disaster flags' on credit reports that are intended to provide relief to consumers affected by natural disasters. This research advances understanding of the informativeness of past defaults in predicting consumers' future behaviors. My consideration of counterfactual policies masking defaults during disasters can help inform policy discussions on how to design credit information markets and alleviate financial distress from natural disasters.

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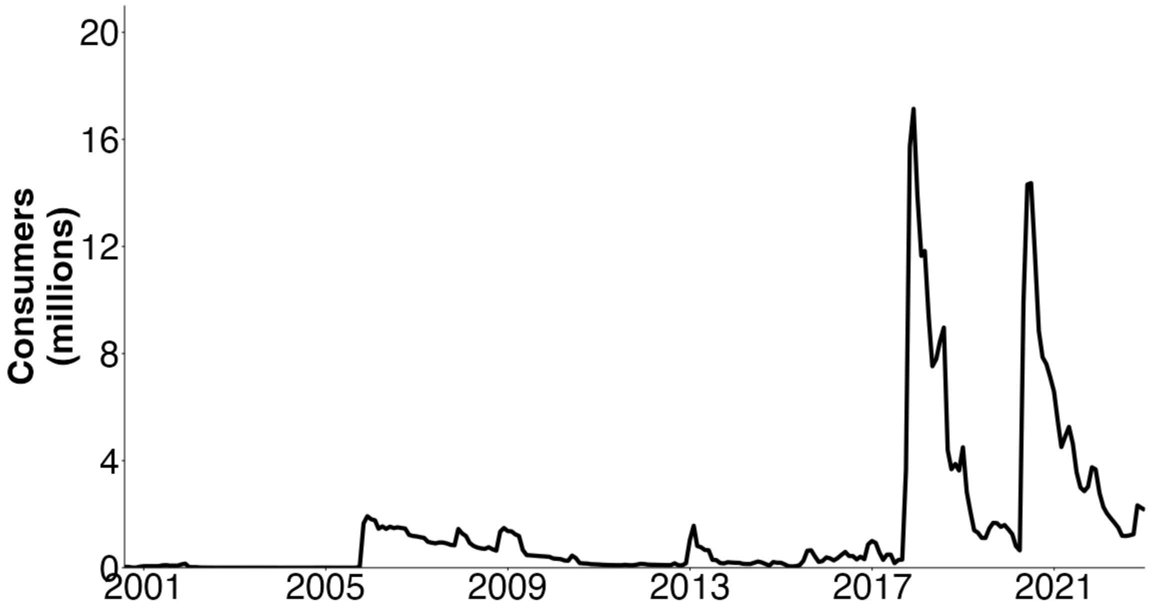
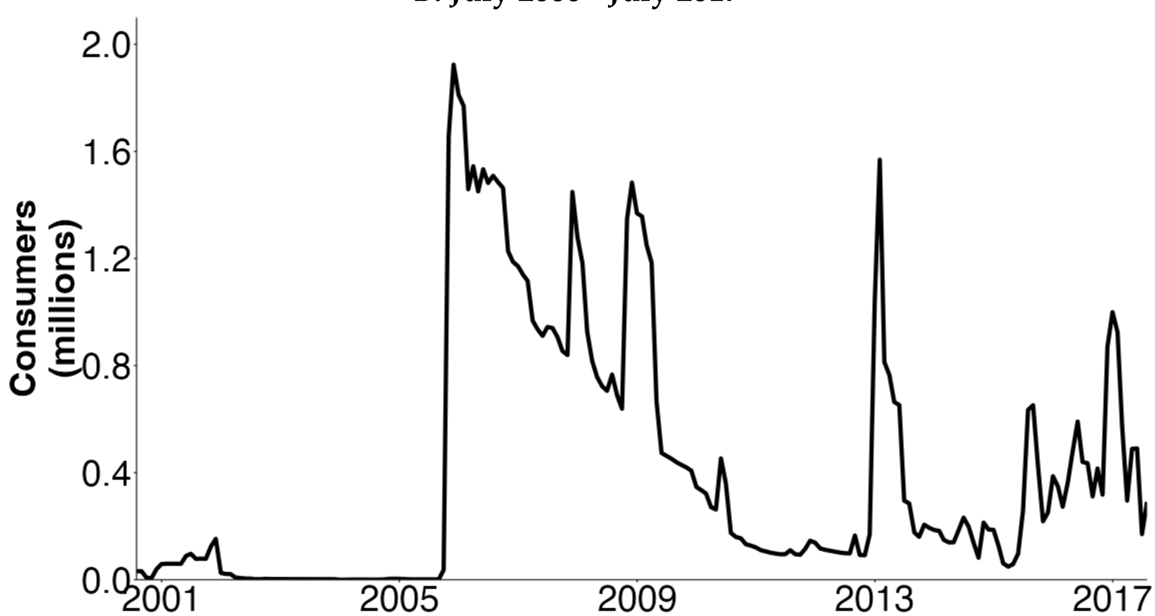
# 10 Figures and Tables

**Figure 1: Consumers with any credit report disaster flag**

**A. July 2000 - December 2022**



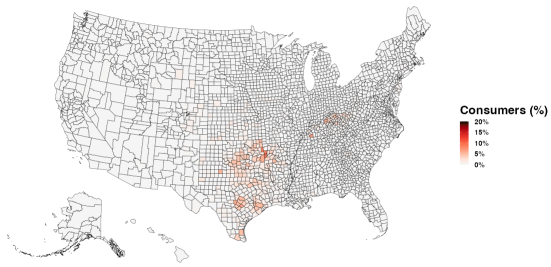

**B. July 2000 - July 2017**



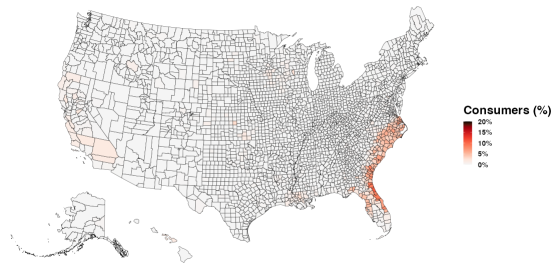

*Notes: TransUnion data. Consumers with a credit report disaster flag on at least one open tradeline in their credit report. Numbers extrapolated to population estimates from 10% sample.*

**Figure 2: Fraction of consumers in a county with any credit report disaster flag**

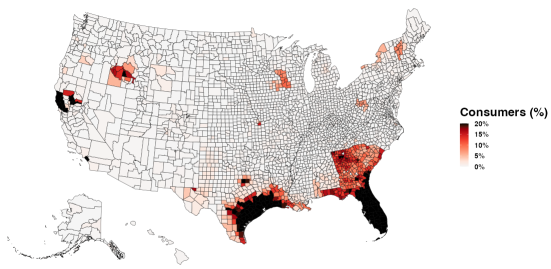
**A. 2015 (August)**



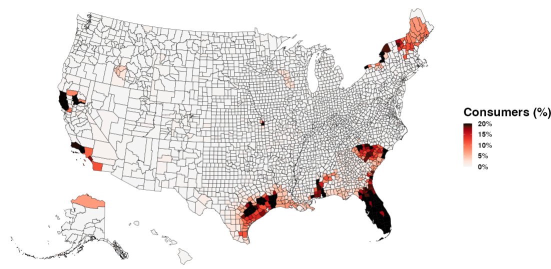

**B. 2016 (December)**



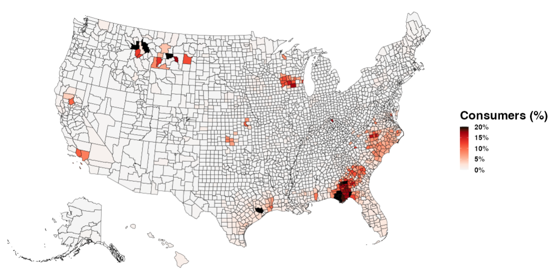

**C. 2017 (November)**



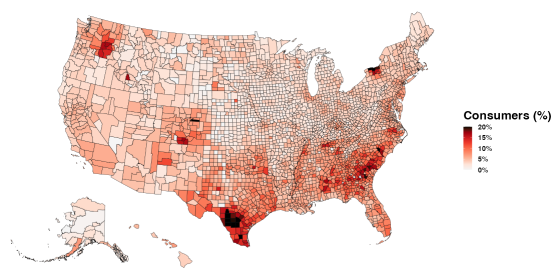

**D. 2018 (January)**



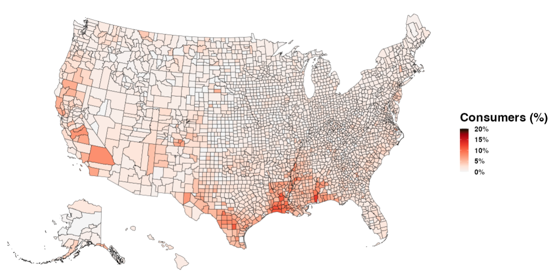

**E. 2019 (January)**



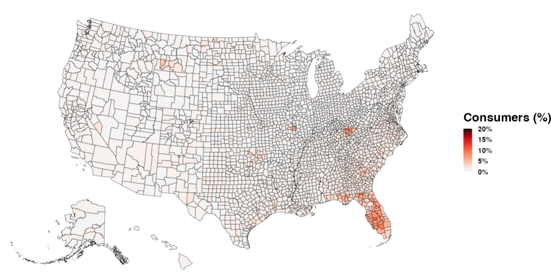

**F. 2020 (June)**




**G. 2021 (January)**

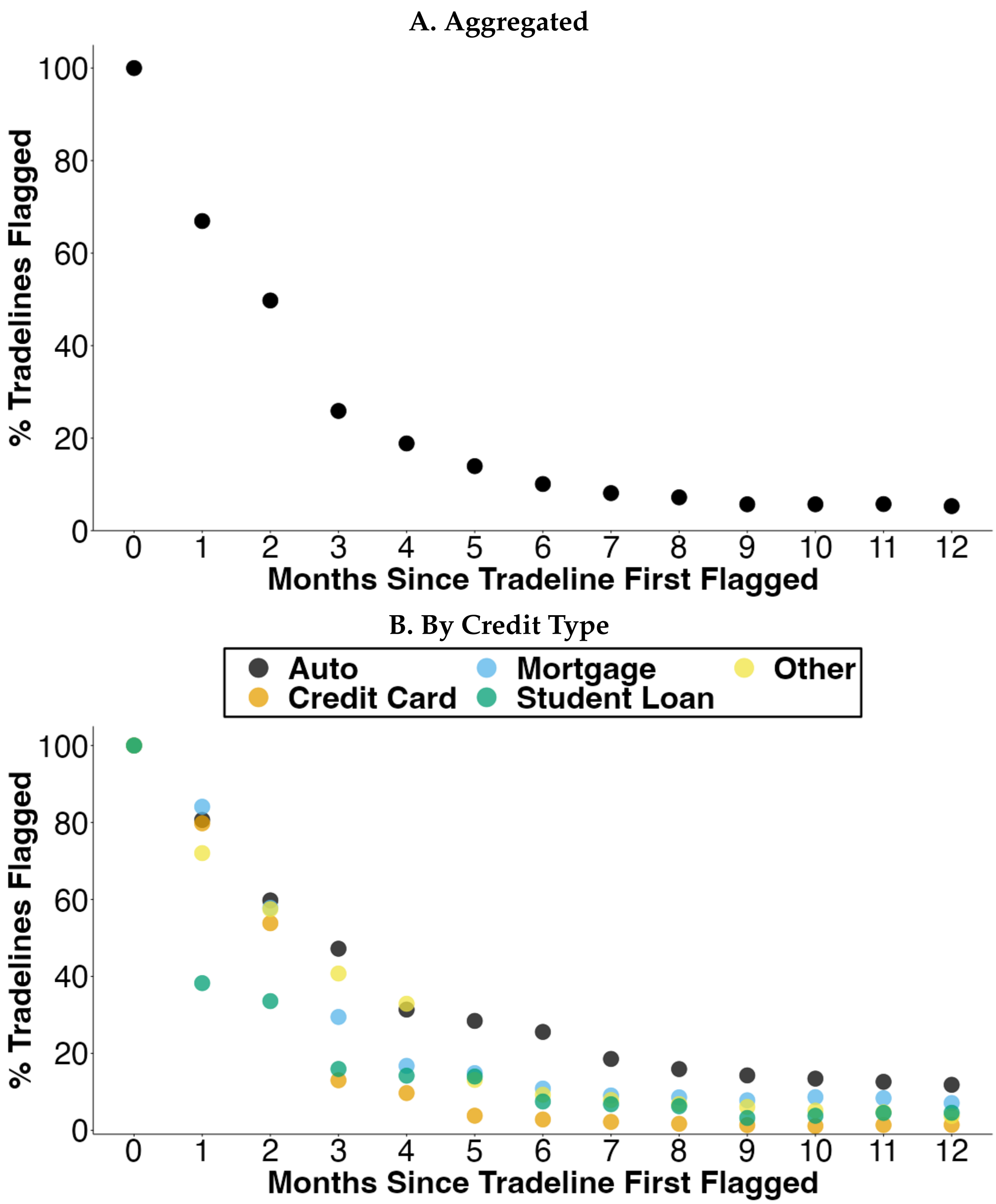



**H. 2022 (October)**




*Notes: TransUnion data. Denominator is number of consumers with an open tradeline with a positive balance on their credit report in a county that month. Numerator is the subset of these consumers with a credit report disaster flag on at least one of these tradelines that month. Values in each county are top-coded at 20%. Months shown are those with the highest number of consumers with disaster flags in each year.*

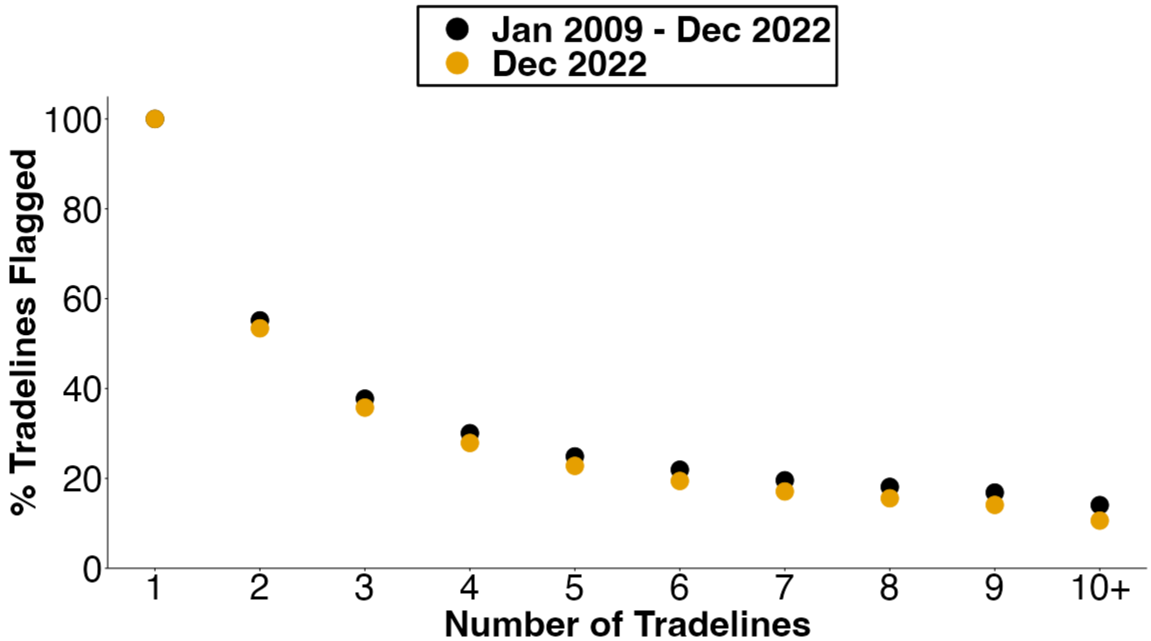
**Figure 3: Persistence of disaster flags on credit report tradelines: aggregated (Panel A) and by credit type (Panel B)**



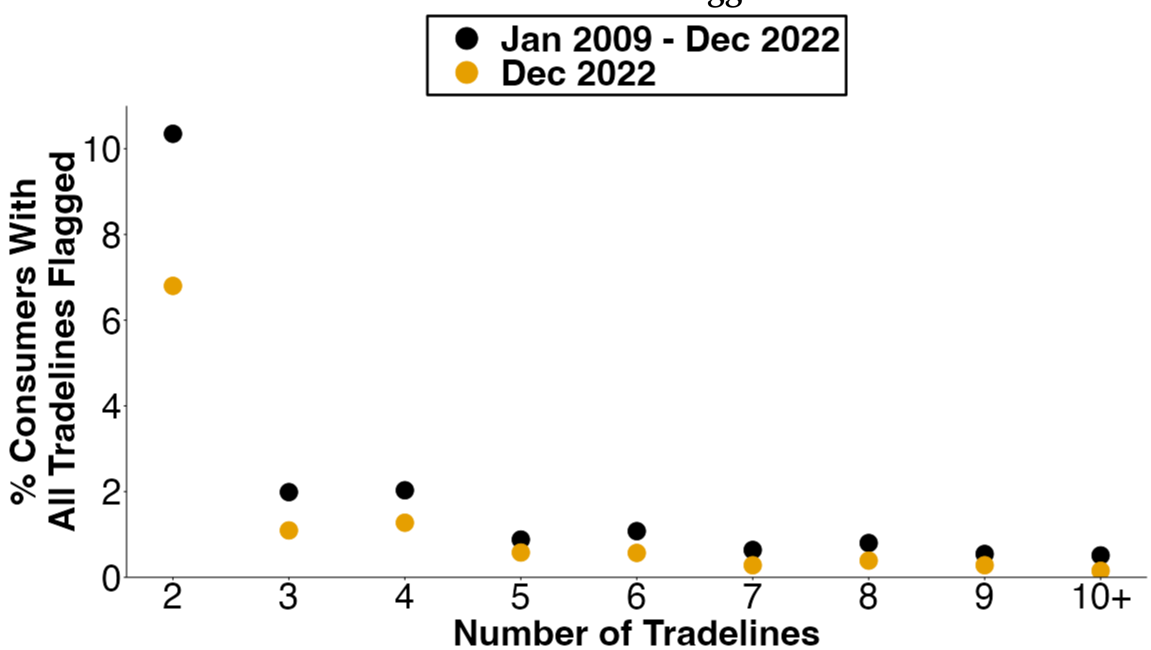

*Notes: TransUnion data. This takes open credit report tradelines with positive balance that first have a disaster flag added between February 2009 to December 2021. Plots the fraction of these with disaster flags still present 1 to 12 months later. Panel A shows for all disaster flags. Panel B splits by credit type where 'other' contains retail cards and unsecured loans.*

**Figure 4: Intensive Margin: Among consumers with disaster flags, mean fraction of tradelines flagged (Panel A) and fraction with all tradelines flagged (Panel B), split by number of tradelines**

A. % Tradelines Flagged



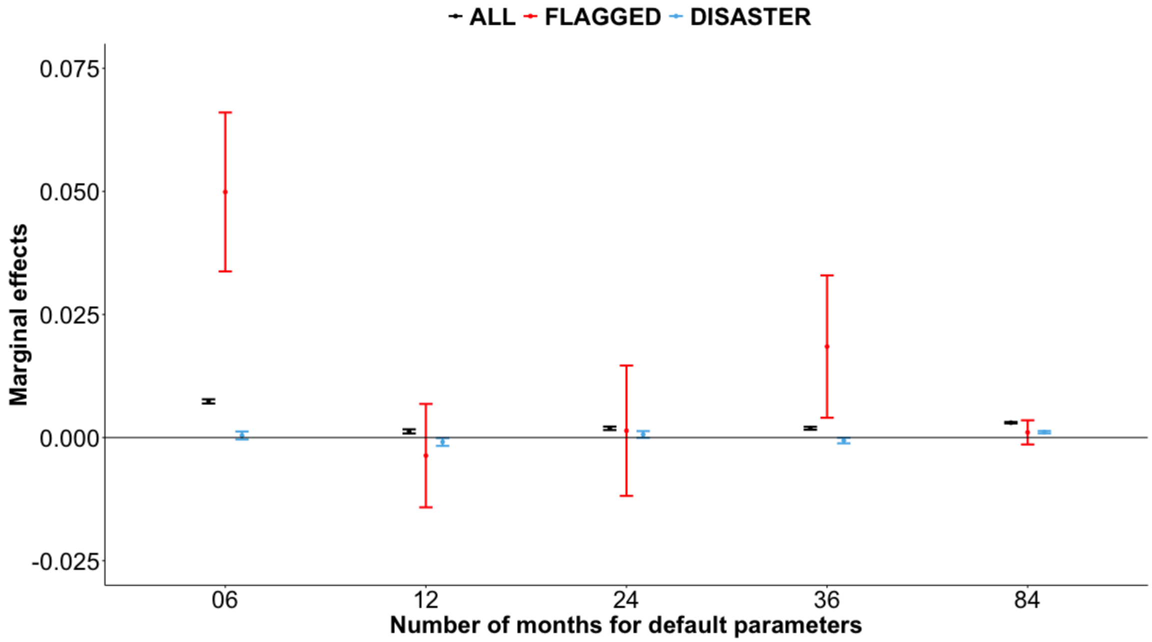

B. % All Tradelines Flagged



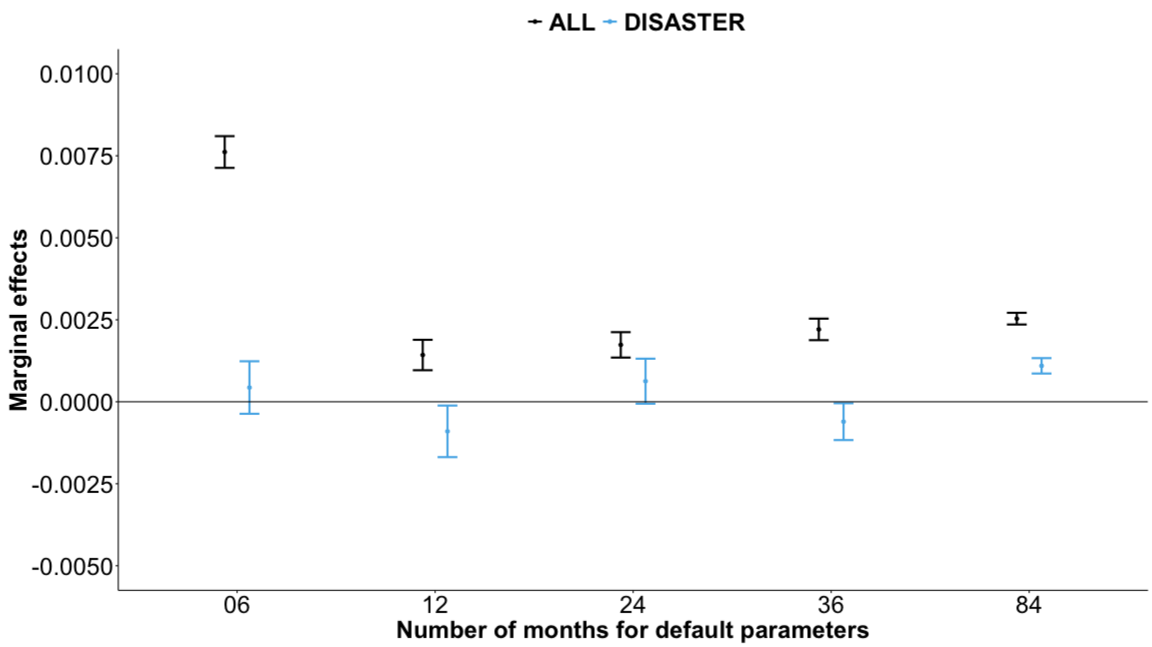

*Notes: TransUnion data. Both Panels A and B restrict to consumer months where the consumer has a credit report disaster flag on at least one open tradeline with a positive balance in their credit report. Panel A shows for these consumers, the mean number of tradelines with a credit report disaster flag. Panel B shows the fraction of these consumers where all their tradelines have credit report disaster flags. X axes on both panels plots number of open trades with a positive balance a consumer has on their credit report. Statistics shown combining observations Jan 2009 - Dec 2022 (black) and also for December 2022 only.*

**Figure 5: Average marginal effects of coefficients predicting future default**

**A. All Defaults (black), Disaster Flag Defaults (red), and FEMA Disaster Defaults (blue)**

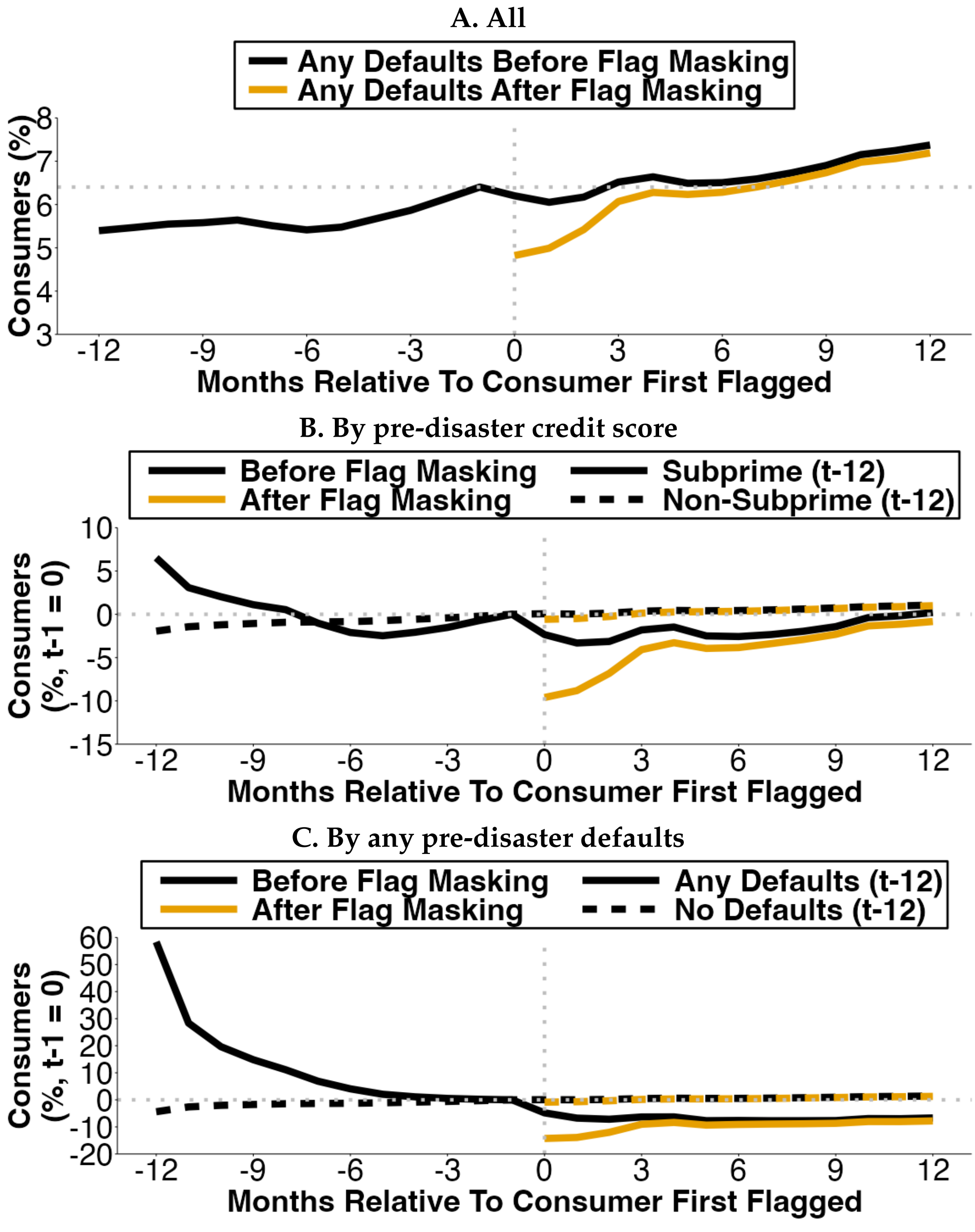



**B. All Defaults (black) and FEMA Disaster Defaults (blue)**



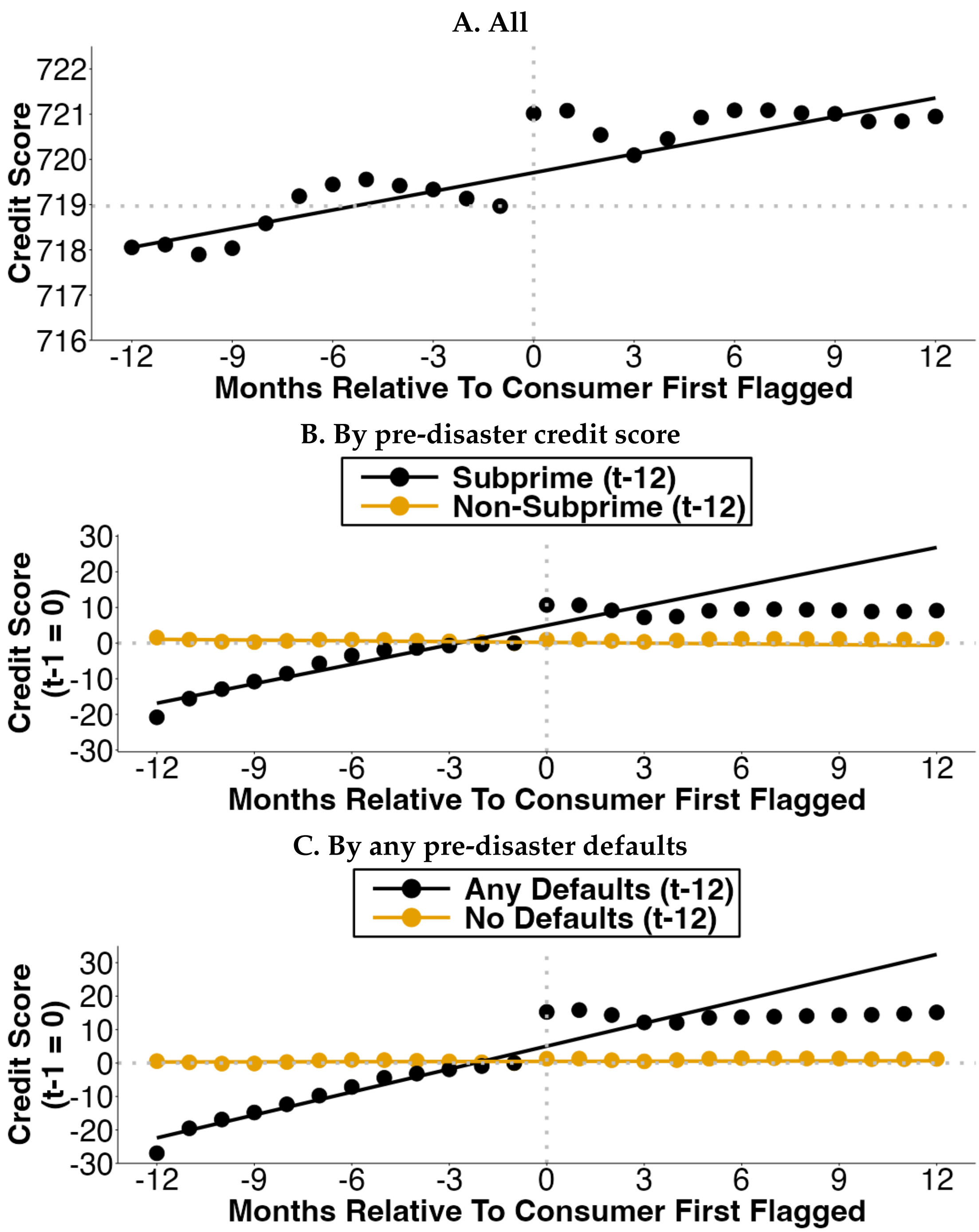

*Notes: TransUnion data. X axis show coefficients on parameters with any defaults in last 6, 12, 24, 36, and 84 months respectively. Y axis shows average marginal effects on coefficients from logistic regressions predicting any new defaults in the next 24 months. Black are $\theta$ coefficients on default term ($D_t$) from Equation 4, red are $\pi$ coefficients on default term ($D_t$) interacted with disaster flag indicator ($FLAG_t$) from Equation 5, and blue are $\phi$ coefficients on default term ($D_t$) interacted with FEMA indicator ($FEMA_t$) from Equation 8.*

**Figure 6: Event study of percent of consumers with defaults on credit reports before (black) and after (orange) flag masking for: (A) all flagged consumers, (B) by pre-disaster credit score, (C) by any pre-disaster defaults**



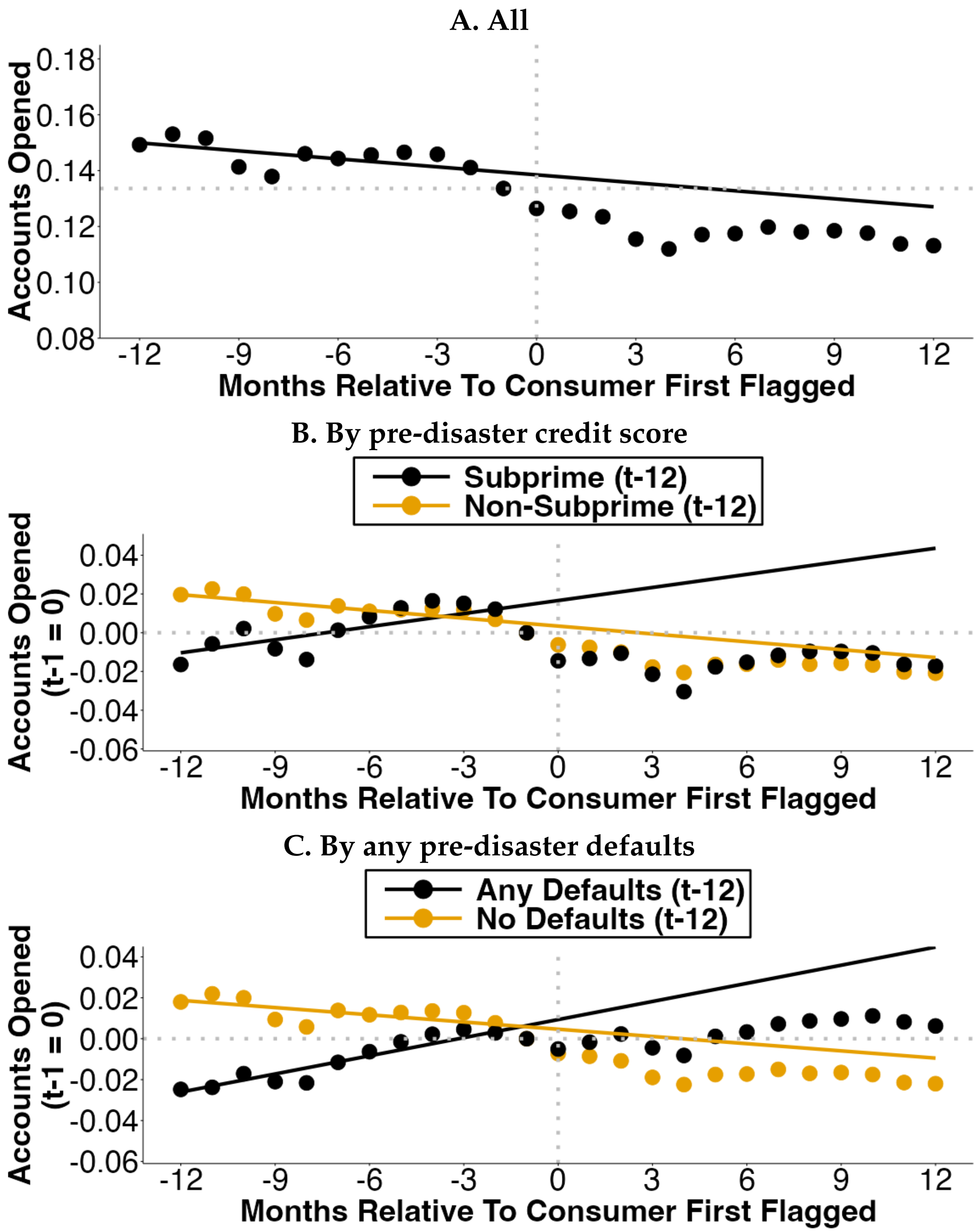

*Notes: TransUnion data. Unconditional means from a balanced panel constructed of consumers with disaster flags first applied January 2010 to December 2018. X axis shows months since consumer first flagged. Y axis shows fraction of consumers with any defaults before (black) and after (orange) tradeline months where defaults masked by flags. Panel B splits by whether consumer consumer has subprime credit score (300 - 600) twelve months prior to first being flagged (t-12). Panel C splits by whether consumer consumer has any defaults twelve months prior to first being flagged (t-12). Panels B and C normalize each series to t-1 = 0.*

**Figure 7: Event study of VantageScore credit scores relative to linear pre-flag time-trends for: (A) all flagged consumers, (B) by pre-disaster credit score, and (C) by any pre-disaster defaults**




*Notes: TransUnion data. Dots are unconditional means from a balanced panel constructed of consumers with disaster flags first applied January 2010 to December 2018. Lines are linear time trend from OLS regressions on data t-12 to t-1. X axis shows months since consumer first flagged. Y axis shows VantageScore credit score. Panel B splits by whether consumer consumer has subprime credit score (300 - 600) twelve months prior to first being flagged (t-12). Panel C splits by whether consumer consumer has any defaults twelve months prior to first being flagged (t-12). Panels B and C normalize each credit score to t-1 = 0.*

**Figure 8: Event study of new account openings relative to linear pre-flag time-trends for: (A) all flagged consumers, (B) by pre-disaster credit score, and (C) by any pre-disaster defaults**

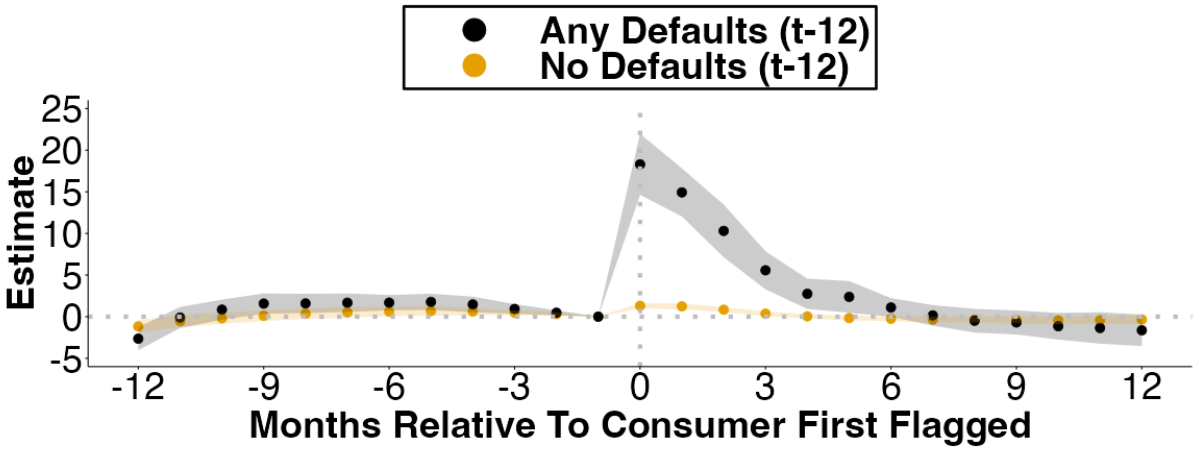



*Notes: TransUnion data. Dots are unconditional means from a balanced panel constructed of consumers with disaster flags first applied January 2010 to December 2018. Lines are linear time trend from OLS regressions on data t-12 to t-1. X axis shows months since consumer first flagged. Y axis shows new account openings. Panel B splits by whether consumer consumer has subprime credit score (300 - 600) twelve months prior to first being flagged (t-12). Panel C splits by whether consumer consumer has any defaults twelve months prior to first being flagged (t-12). Panels B and C normalize each credit score to t-1 = 0.*

**Figure 9: Difference-in-differences estimates of effects on credit access by any pre-disaster defaults**

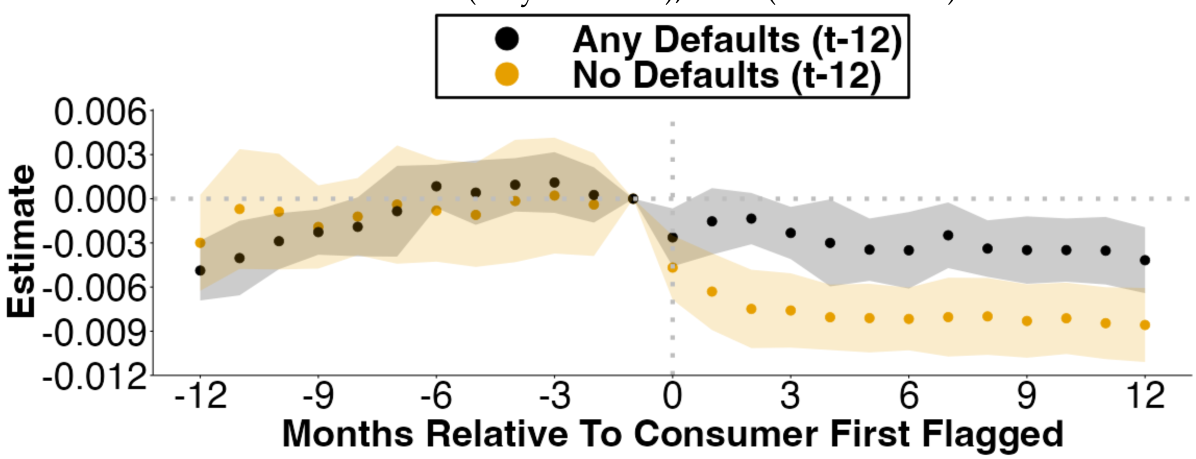
**A. Credit score**

t-1 means: 556 (Any Defaults), 730 (No Defaults)



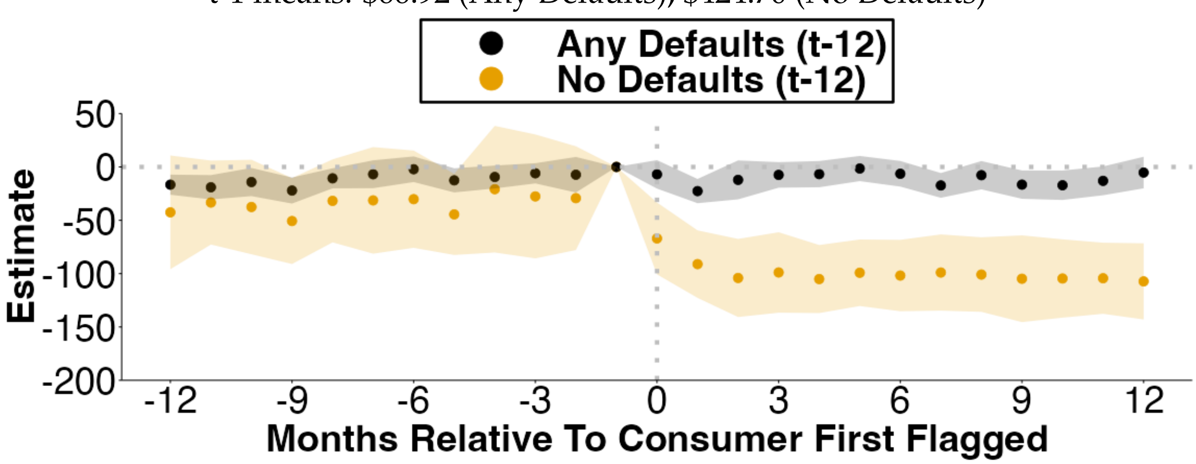

**B. Any new credit card openings**

t-1 means: 2.4% (Any Defaults), 3.7% (No Defaults)



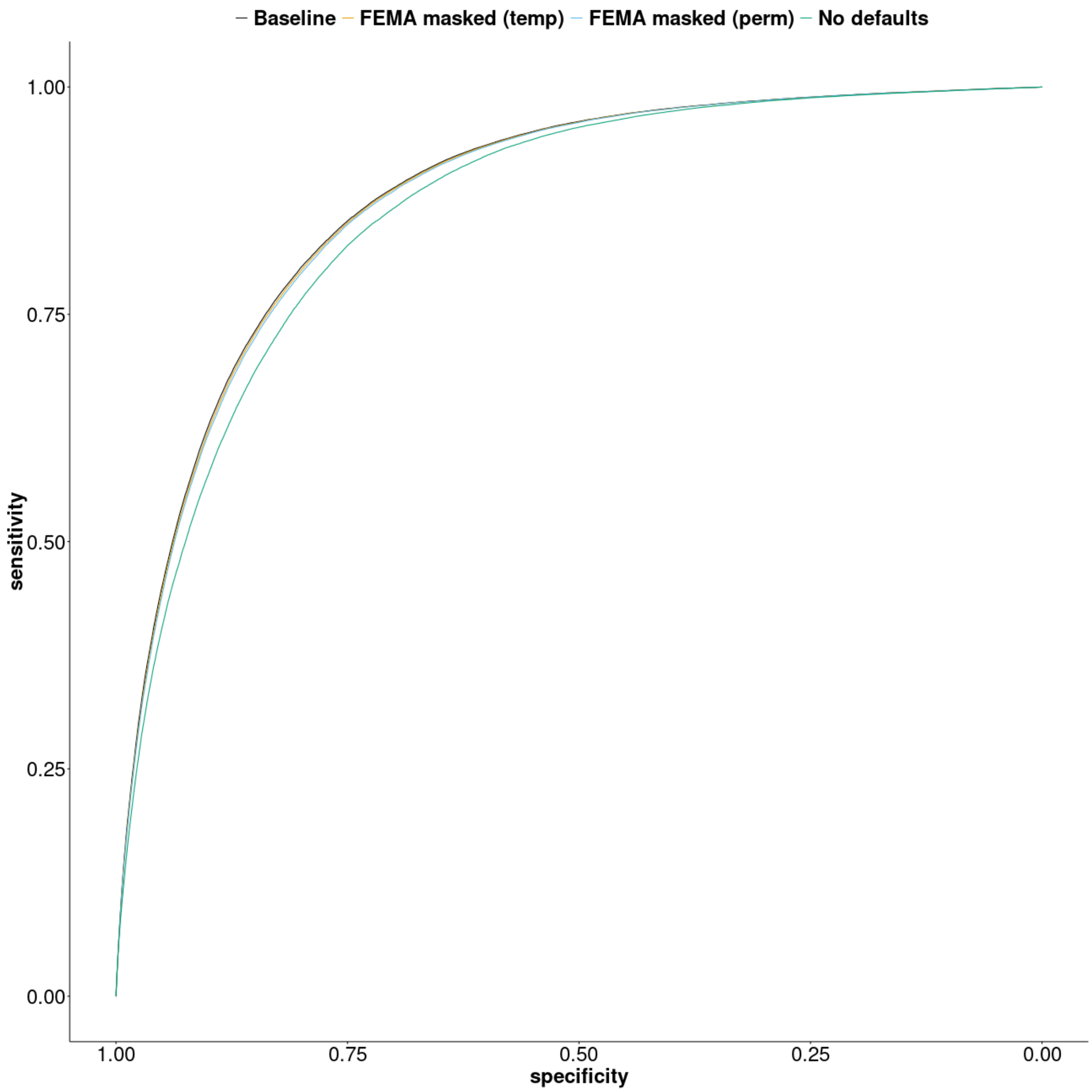

**C. Value of new credit card limits**

t-1 means: $66.92 (Any Defaults), $424.70 (No Defaults)




*Notes: TransUnion data. Plots show estimates of Equation 7's $\delta_\tau$ from stacked difference-in-differences regression with 95% confidence intervals. Standard errors are clustered at cohort-level.*

**Figure 10: Receiver operating characteristic (ROC) curves showing predictive performance of models comparing baseline model predicting default (black) to temporarily masking FEMA defaults (yellow), permanently (blue) masking FEMA defaults, and masking all defaults (green)**




*Notes: TransUnion data. ROC curves from out-of-sample prediction from models predicting any new default in next 24 months using data to October 2017. Black is baseline model that includes defaults and non-default information, red is model masking defaults occurring on tradeline months with credit report disaster flags, blue is model masking defaults occurring in six months of FEMA disaster, and green is model masking all defaults.*

**Table 1: Summarizing consumers with (I) disaster flags compared to (II) unflagged in same census block group $\times$ zipcode (CBGZIP) and (III) unflagged in US**

| | (I) Flagged | (II) Unflagged in CBGZIP | (III) Unflagged in US |
|---|---|---|---|
| **Credit Score** | 696 | 704 | 704 |
| **Age (years)** | 50.59 | 49.44 | 49.45 |
| **Accounts (#)** | 7.84 | 5.47 | 5.30 |
| **Any 30+ defaults (%)** | 0.09 | 0.07 | 0.08 |
| **30+ defaults (#)** | 0.17 | 0.12 | 0.13 |
| **Any Balance (%)** | 0.97 | 0.89 | 0.89 |
| **Any Auto (%)** | 0.53 | 0.38 | 0.34 |
| **Any Credit Card (%)** | 0.80 | 0.71 | 0.68 |
| **Any Mortgage (%)** | 0.45 | 0.31 | 0.33 |
| **Balances ($)** | 141,177 | 88,402 | 86,143 |
| **Mortgage Balances ($)** | 233,733 | 205,812 | 188,589 |
| **Non-Mortgage Balances ($)** | 29,347 | 19,132 | 16,273 |
| **Auto Balances ($)** | 21,868 | 19,202 | 16,986 |
| **Credit Card Balances ($)** | 7,985 | 5,866 | 5,533 |
| **Credit Card Limits ($)** | 32,741 | 26,788 | 24,939 |

*Notes: TransUnion data. Table summarizes data for consumers using characteristics twelve months prior. 'Flagged' shows characteristics of consumers with any disaster flags. 'Unflagged CBGZIP' shows consumers with no disaster flags in the same census block group $\times$ zipcode where any other consumers had disaster flags. 'Unflagged US' shows consumers with no disaster flags.*

**Table 2: Credit risk prediction performance of models varying masking of defaults**

| Model | AUROC | Change from Baseline |
|---|---|---|
| **(1) Baseline** | 0.8790 | - |
| **(2) Disaster Flag Defaults Masked** | 0.8786 | -0.05% |
| **(3) FEMA Defaults Masked (Temporary)** | 0.8777 | -0.15% |
| **(4) FEMA Defaults Masked (Permanent)** | 0.8764 | -0.30% |
| **(5) All Defaults Masked** | 0.8641 | -1.70% |

*Notes: TransUnion data. Table shows predictive performance as measured by Area under the receiver operating characteristic (AUROC) from logistic regressions predicting any new default 90+ days past due over the next 24 months using data to October 2017. AUROCs are calculated using out-of-sample data to data models were trained on. Models (1) to (5) use non-default data (e.g. number and type of credit accounts, balances, limits, utilization, bankruptcy, duration of credit history) as predictors. Models (1) to (4) also include default data as predictors. Model (1) includes all default data as predictors as in Equation 4)and Model (5) has no default data as predictors as in Equation 11. Model (2) reclassifies defaults on tradeline months with disaster flags as non-defaults as in Equation 6. Defaults of a consumer residing in a FEMA natural disaster that occur within six months of the natural disaster are reclassified as non-defaults for those six months (6.7% of total) in Model (3) as in Equation 9. Defaults by a consumer residing in a FEMA natural disaster area that newly occur within six months of the natural disaster are reclassified in Model (4) as non-defaults for all subsequent tradeline months (18.4% of total) as in Equation 10.*

# 11 Online Appendix to *"Disaster Flags: Credit Reporting Relief from Natural Disasters"*

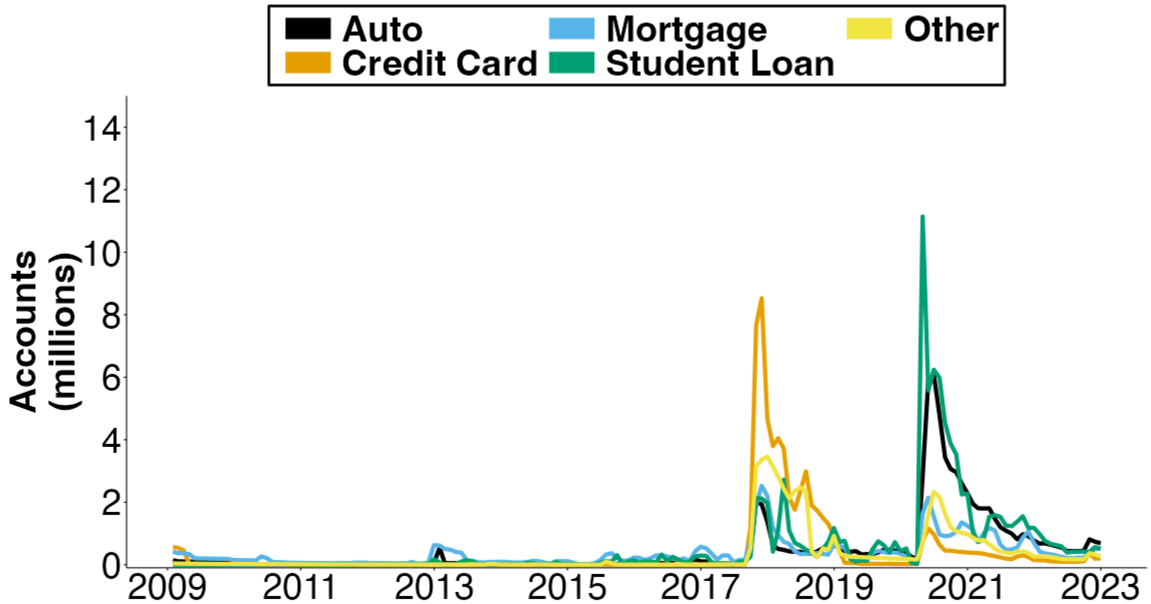
**Table A1: Summarizing tradeline months with disaster flags by credit type**

| **Credit Type** | **2009 - 2019 (%)** | **2009 - 2022 (%)** | **Dec 2022 (%)** |
|---|---|---|---|
| **Auto** | 12.26 | 22.09 | 31.60 |
| **Credit Card** | 31.24 | 18.03 | 9.22 |
| **Mortgage** | 20.76 | 17.35 | 23.81 |
| **Student Loan** | 15.44 | 26.51 | 23.46 |
| **Other** | 20.29 | 16.02 | 11.91 |

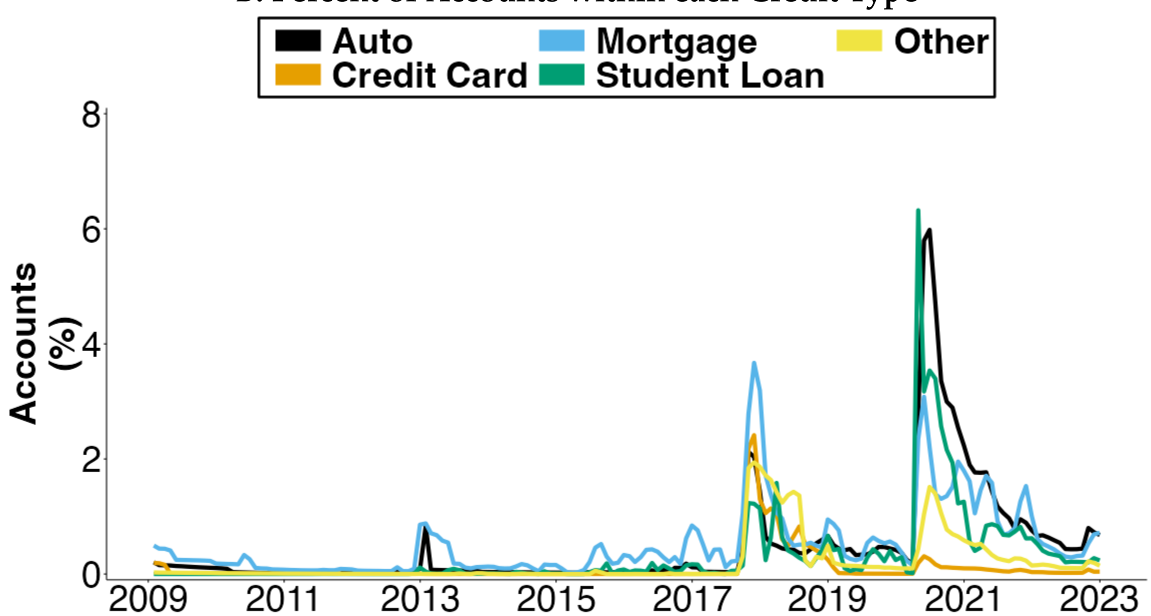
*Notes: TransUnion data. Displays each credit type's share of all disaster flagged trade months over (i) January 2009 to December 2019 (ii) January 2009 to December 2022 (iii) December 2022. Other contains retail cards and unsecured loans.*

**Figure A1: Trades with credit report disaster flag by credit type, 2000 - 2022**

**A. Number of Accounts**



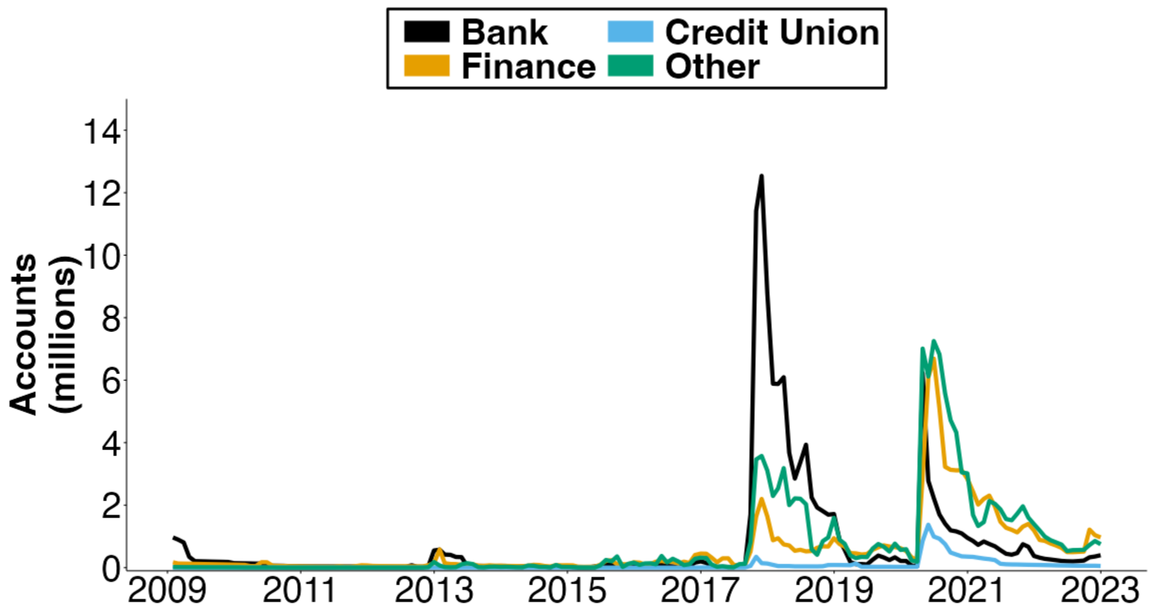

**B. Percent of Accounts within each Credit Type**



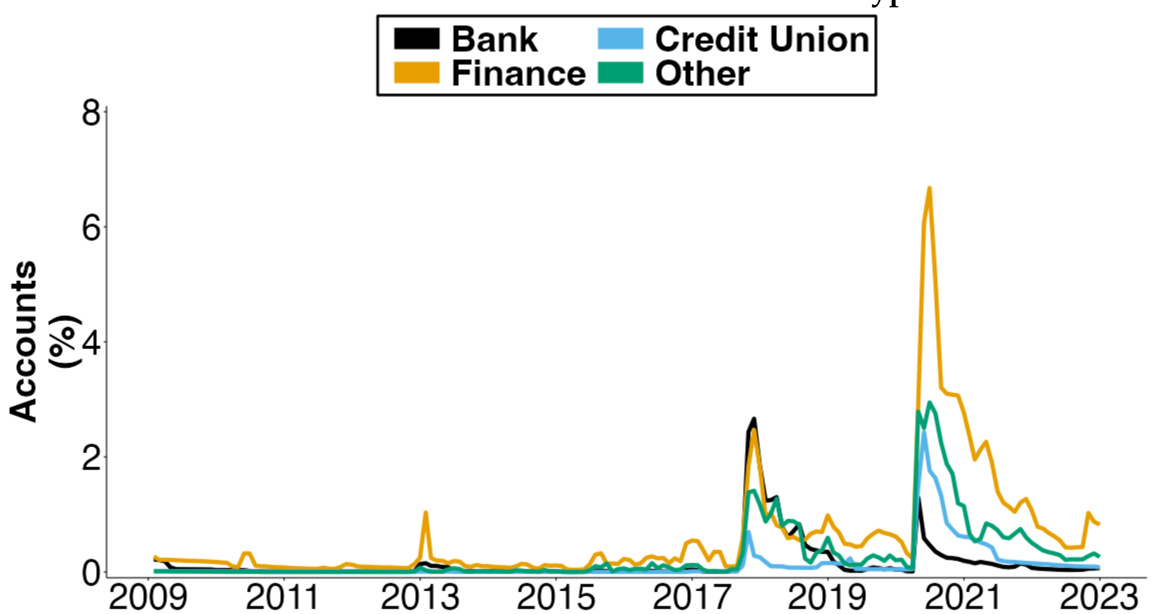

*Notes: TransUnion data. Open tradelines with a positive balance in their credit report with a credit report disaster flag. Numbers extrapolated to population estimates from 10% sample. Other contains retail cards and unsecured loans.*

**Figure A2: Trades with credit report disaster flag by lender type, 2000 - 2022**

**A. Number of Accounts**

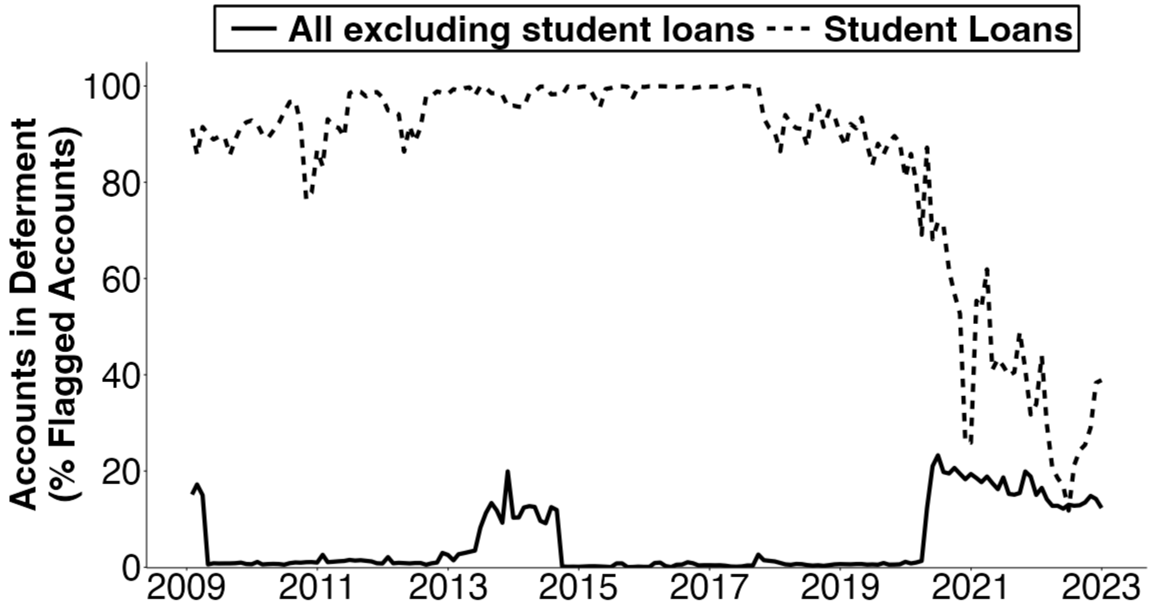



**B. Percent of Accounts within each Lender Type**



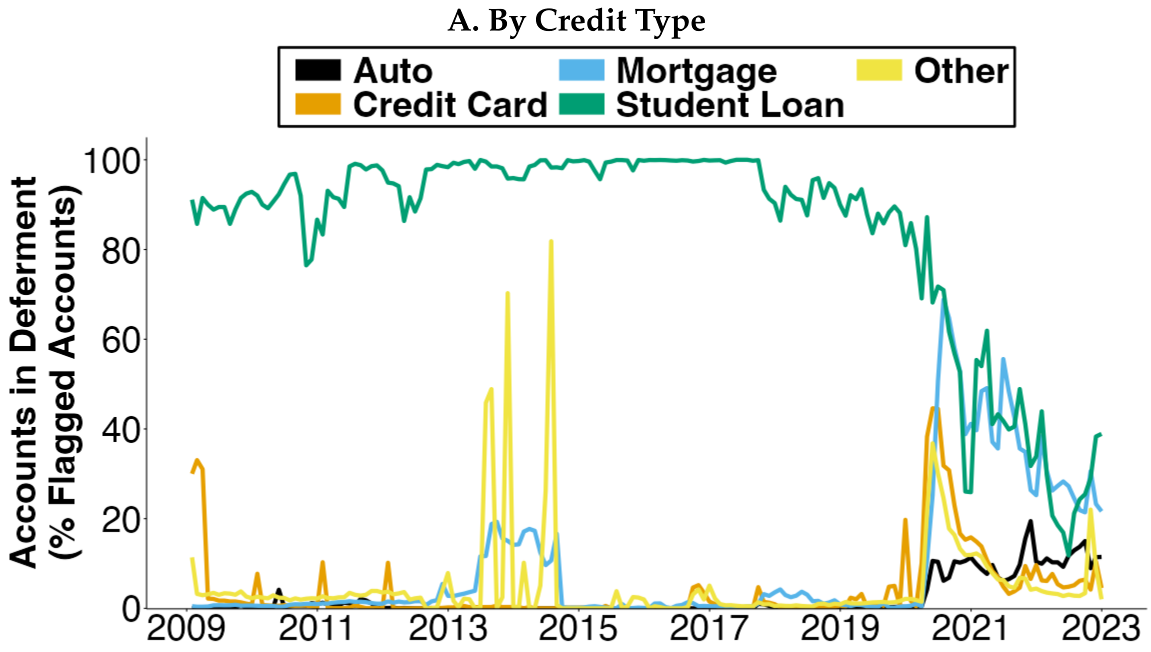

*Notes: TransUnion data. Open tradelines with a positive balance in their credit report with a credit report disaster flag. Numbers extrapolated to population estimates from 10% sample.*

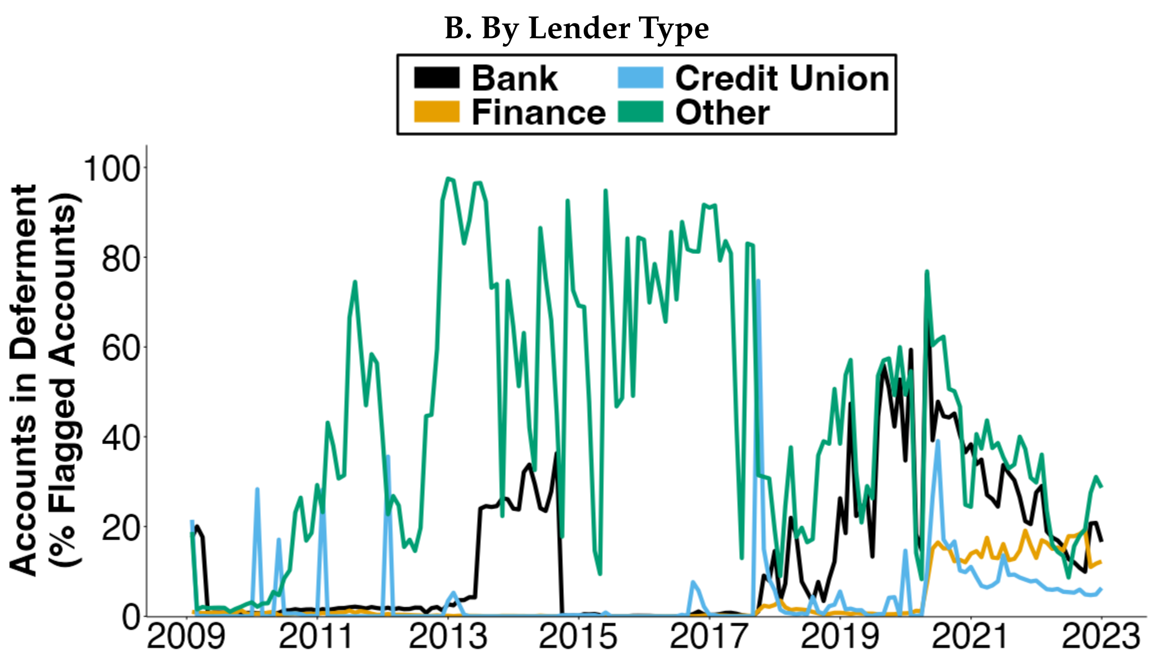
**Figure A3: Trades with credit report disaster flag that also had deferments, 2000 - 2022**




*Notes: TransUnion data. Open tradelines with a positive balance in their credit report with a credit report disaster flag. Solid line shows fraction of flagged tradelines excluding student loans that also have deferments. Dashed line shows fraction of flagged student loan tradelines that also have deferments: accounts listed with deferments and tradelines with positive balances but zero payments due.*

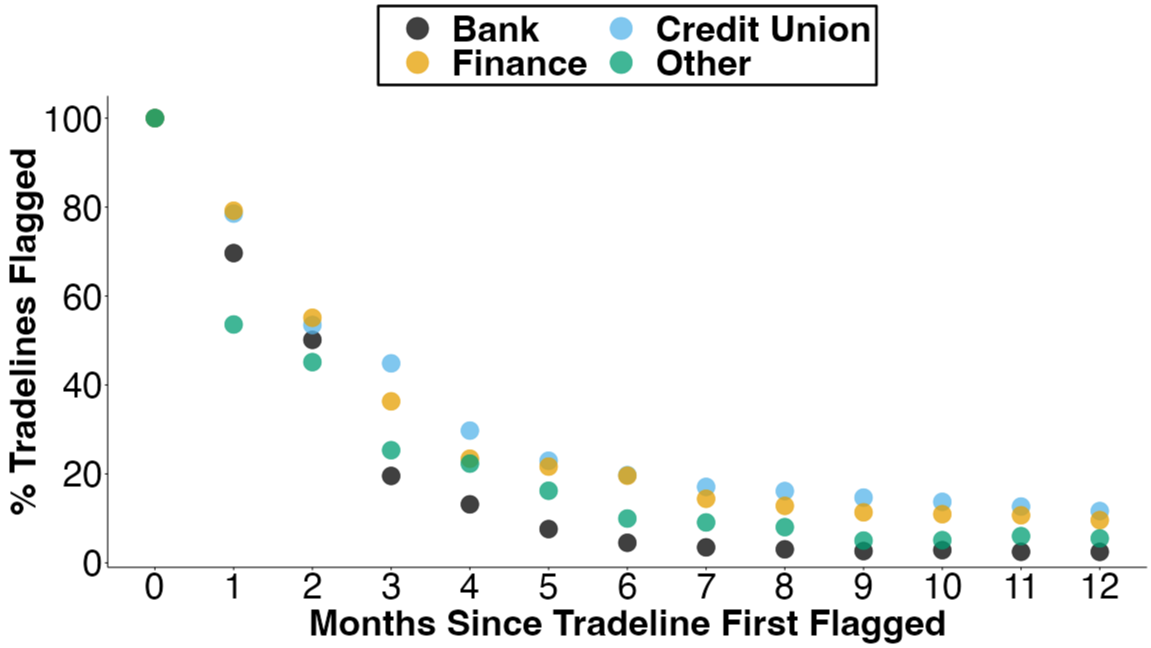
**Figure A4: Trades with credit report disaster flag that also had deferments, 2000 - 2022, by credit type (Panel A) and lender type (Panel B)**



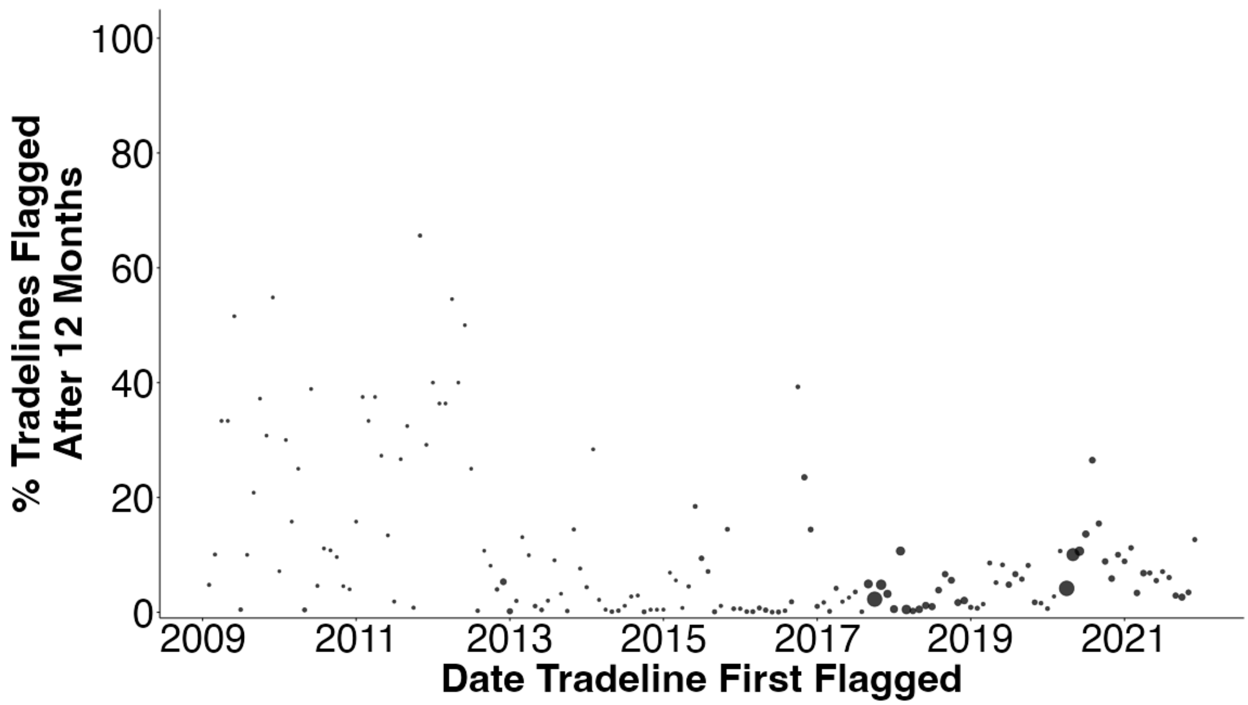

*Notes: TransUnion data. Open tradelines with a positive balance in their credit report with a credit report disaster flag. Lines show fractions of flagged tradelines that also have deferments: accounts listed with deferments and tradelines with positive balances but zero payments due.*

**Figure A5: Duration of disaster flags remaining on a credit report tradeline, by lender type**




*Notes: TransUnion data. This takes open credit report tradelines with positive balance that first have a disaster flag added between February 2009 to December 2021. Plots the fraction of these with disaster flags still present 1 to 12 months later. Colors are lender types.*

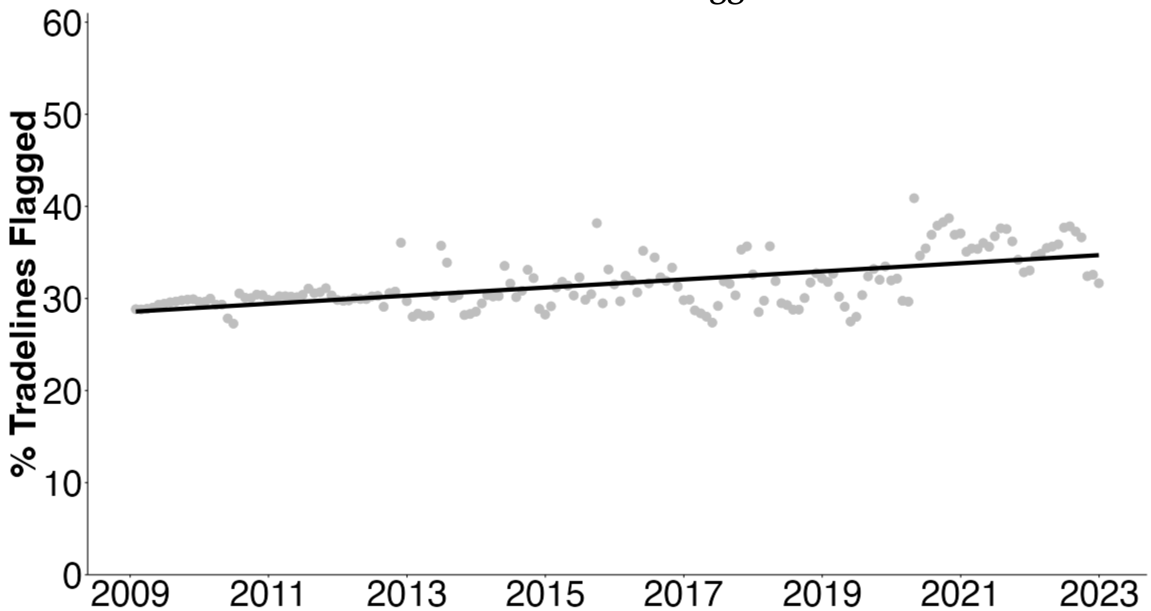
**Figure A6: Fraction of disaster flags remaining on a credit report tradeline after 12 months, by cohort**



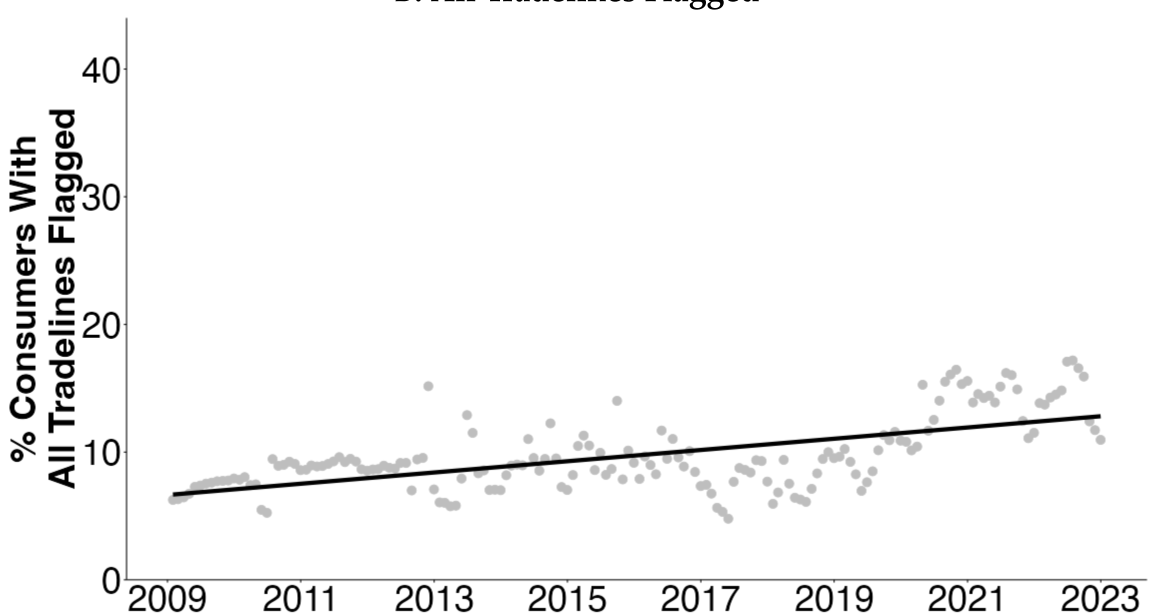

*Notes: TransUnion data. This takes open credit report tradelines with positive balance that first have a disaster flag added between February 2009 to December 2021. Plots the fraction of these with disaster flags still present 12 months later for each cohort. X axis is cohort date when disaster flag first added to tradeline. Size of dot is proportional to initial disaster flag cohort size.*

**Figure A7: Intensive Margin: Among consumers with disaster flags, mean fraction of tradelines flagged (Panel A) and fraction with all tradelines flagged (Panel B) over time**

**A. Mean Tradelines Flagged**

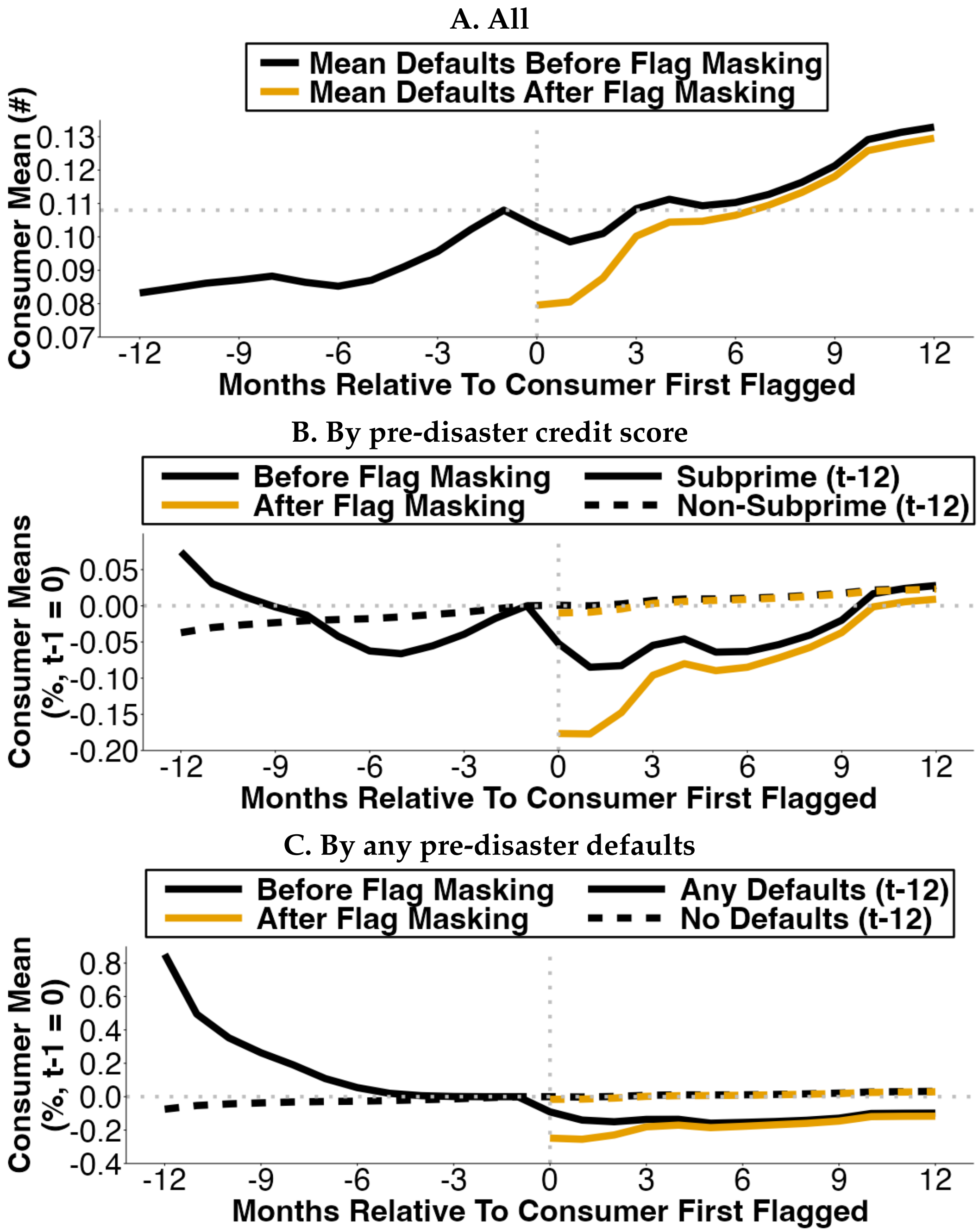



**B. All Tradelines Flagged**



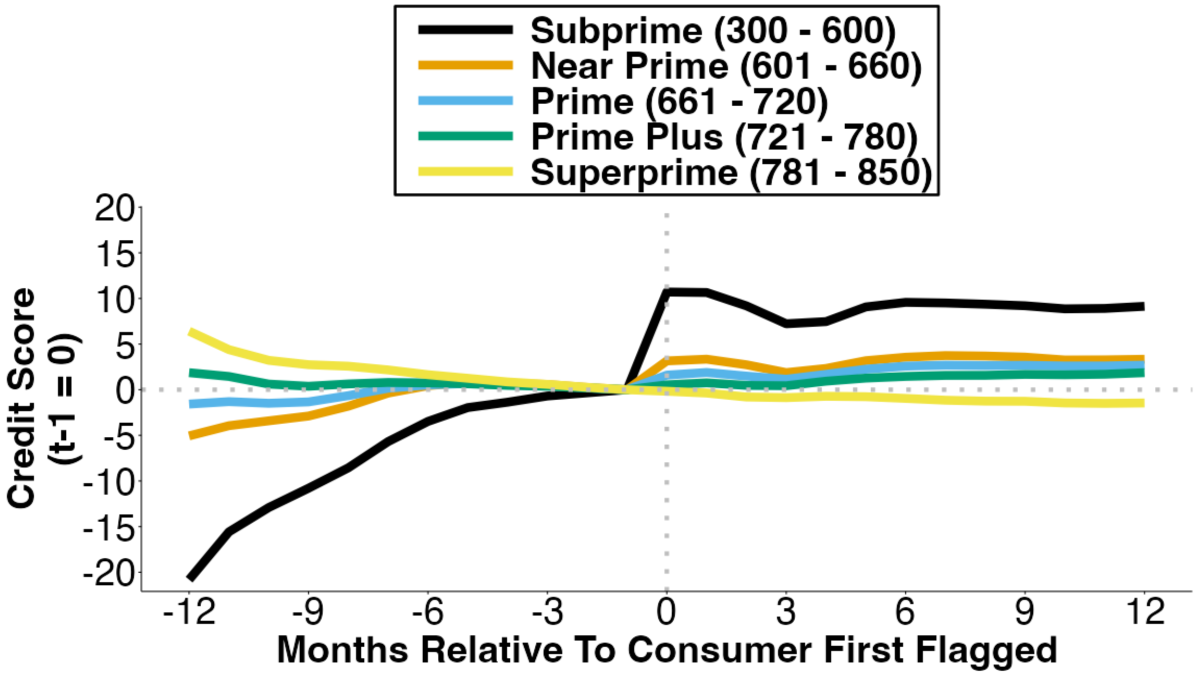

*Notes: TransUnion data. Both Panels A and B restrict to consumer months where the consumer has a credit report disaster flag on at least one open tradeline with a positive balance in their credit report. Panel A shows for these consumers, the mean of tradelines with a credit report disaster flag. Panel B shows the fraction of these consumers where all their tradelines have a credit report disaster flag. Linear time trends added in both Panels.*

**Figure A8: Event study of mean number of defaults on credit reports before (black) and after (orange) flag masking for all flagged consumers (Panel A), by pre-disaster credit score (Panel B), and by any pre-disaster defaults (Panel C)**




*Notes: TransUnion data. Unconditional means from a balanced panel constructed of consumers with disaster flags first applied January 2010 to December 2018. X axis shows months since consumer first flagged. Y axis is mean number of defaults on consumers' credit reports before (black) and after (orange) tradeline months where defaults are masked by flags. Panel B splits by whether consumer consumer has subprime credit score (300 - 600) twelve months prior to first being flagged (t-12). Panel C splits by whether consumer consumer has any defaults twelve months prior to first being flagged (t-12). Panels B and C normalize each series to t-1 = 0.*

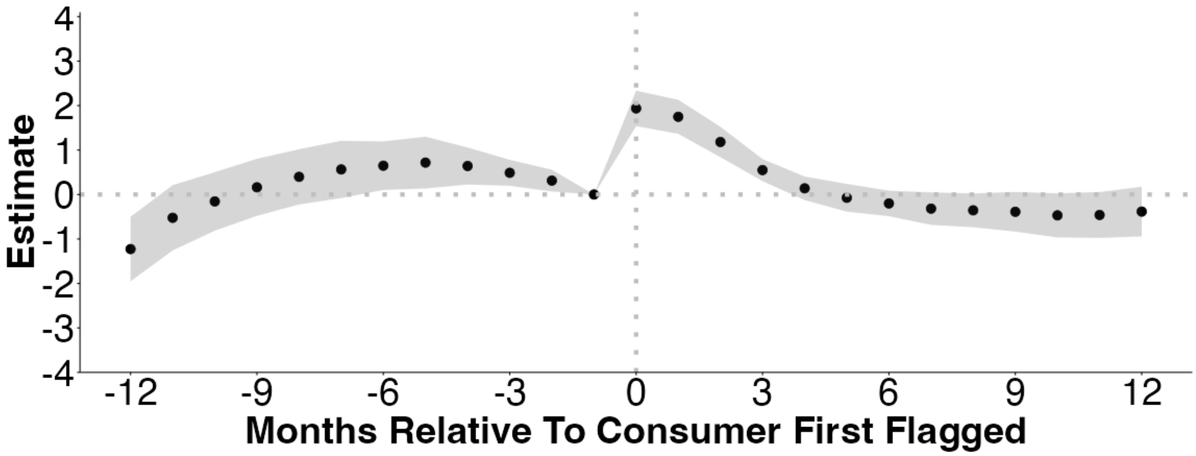
**Figure A9: Event study of VantageScore credit scores by pre-disaster credit score (t-12)**



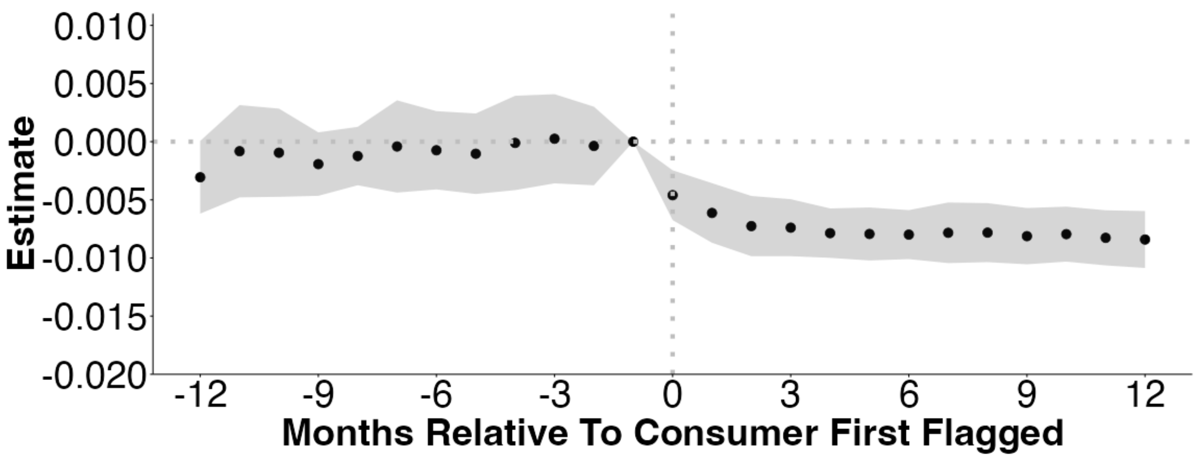

*Notes: TransUnion data. Unconditional means from a balanced panel constructed of consumers with disaster flags first applied January 2010 to December 2018. X axis shows months since consumer first flagged. Y axis is fraction of consumers with any defaults before (black) and after (orange) tradeline months where defaults are masked by flags. Splits by consumer's credit score twelve months prior to first being flagged (t-12). Each credit score normalized to t-1 = 0.*

**Figure A10: Difference-in-differences estimates of effects on credit access**

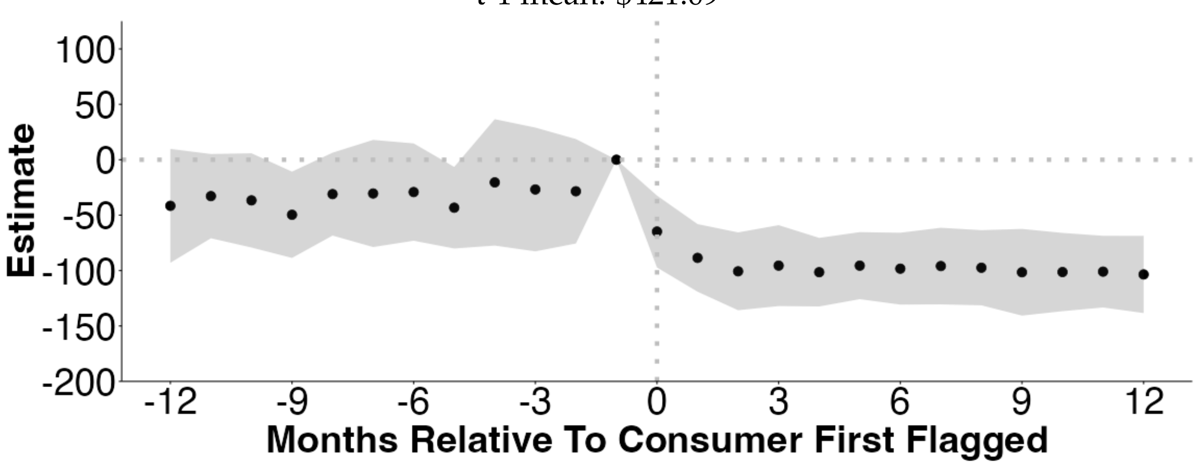
**A. Credit score**
t-1 mean: 723




**B. Any new credit card openings**
t-1 mean: 3.7%




**C. Value of new credit card limits**
t-1 mean: $421.09

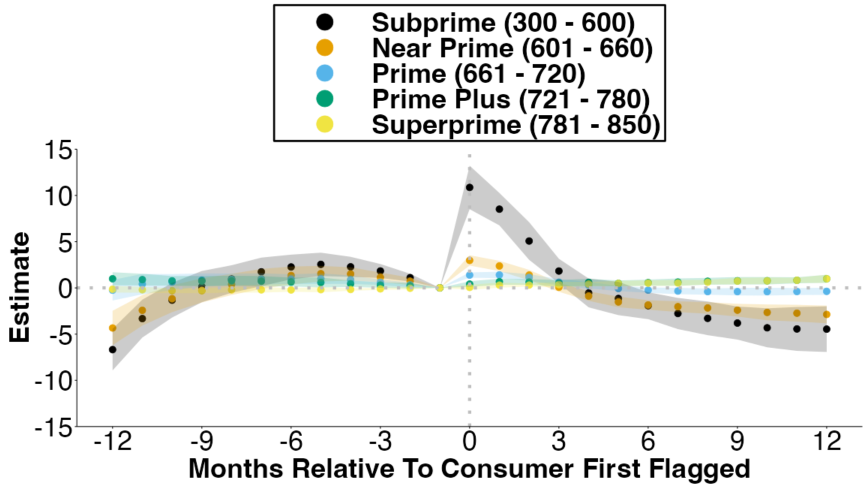



*Notes: TransUnion data. Plots show estimates of Equation 7's $\delta_\tau$ from stacked difference-in-differences regression with 95% confidence intervals. Standard errors are clustered at cohort-level.*

**Figure A11: Difference-in-differences estimates of effects on credit access by pre-disaster credit score**

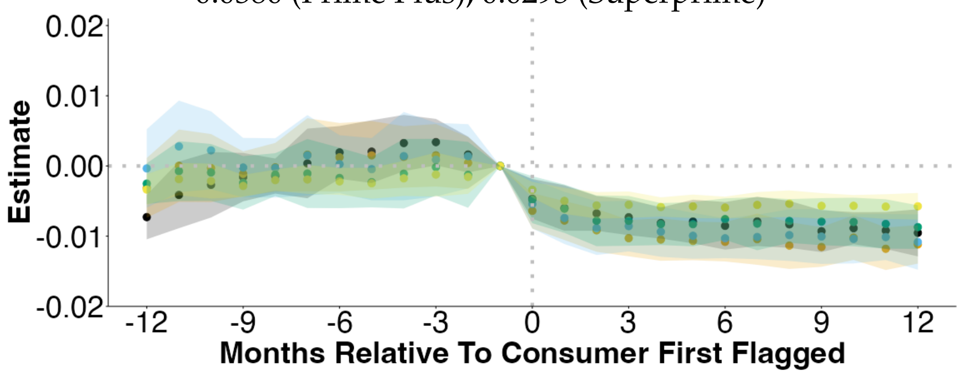
**A. Credit score**

t-1 means: 569 (Subprime), 636 (Near Prime), 690 (Prime)
t-1 means: 747 (Prime Plus), 804 (Superprime)



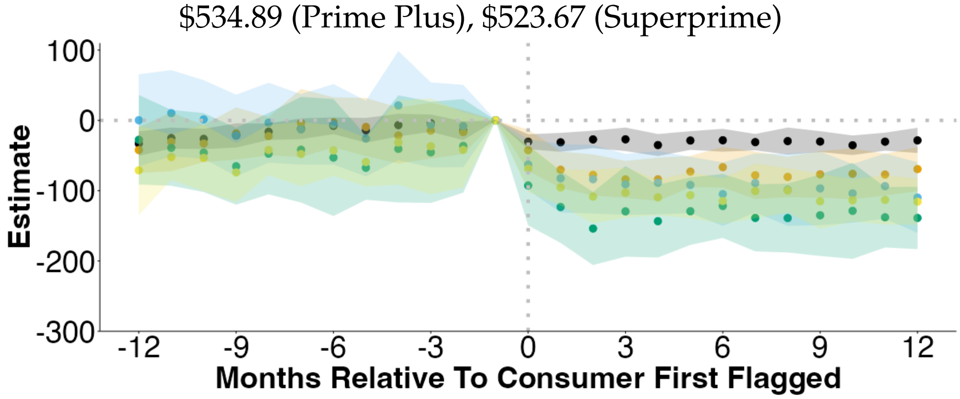

**B. Any new credit card openings**

t-1 means: 0.0375 (Subprime), 0.0439 (Near Prime), 0.0433 (Prime)
0.0380 (Prime Plus), 0.0293 (Superprime)



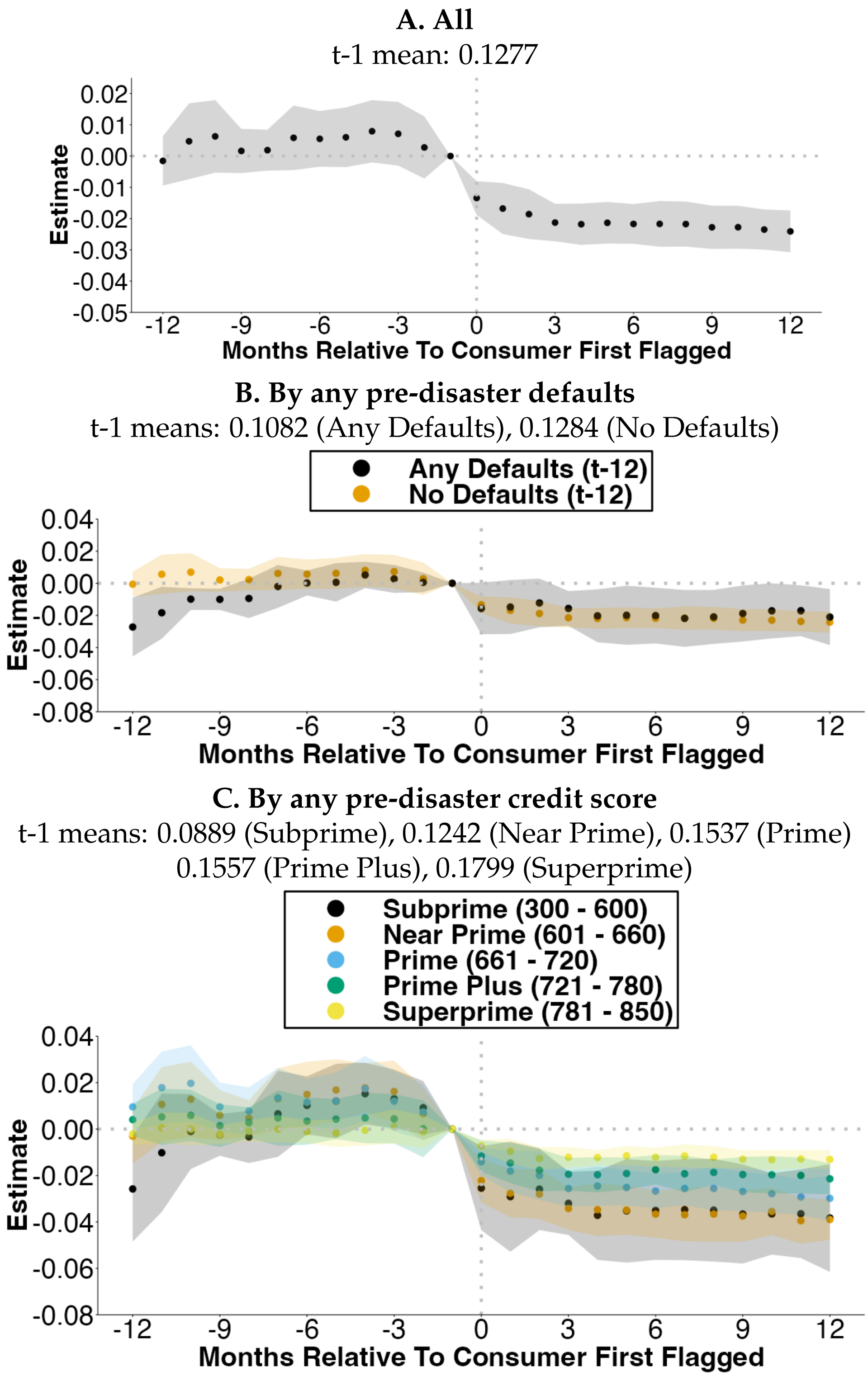

**C. Value of new credit card limits**

t-1 means: $108.45 (Subprime), $259.77 (Near Prime), $390.73 (Prime)
$534.89 (Prime Plus), $523.67 (Superprime)




*Notes: TransUnion data. Plots show estimates of Equation 7's $\delta_\tau$ from stacked difference-in-differences regression with 95% confidence intervals. Standard errors are clustered at cohort-level.*

**Figure A12: Difference-in-differences estimates of effects on the number of new account openings across credit types**




*Notes: TransUnion data. Plots show estimates of Equation 7's $\delta_\tau$ from stacked difference-in-differences regression with 95% confidence intervals. Standard errors are clustered at cohort-level.*