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# Essays in Household and Labor Finance

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# Essays in Household and Labor Finance

by

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# Table of Contents

# List of Figures

# List of Tables

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Dedicated to my parents and Professor Radhakrishnan Gopalan

ABSTRACT OF THE DISSERTATION

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My dissertation focuses on two broad questions. First, what is the role of financial regulation in alleviating financial distress of households? Second, how do labor markets shape individual financial outcomes?

In Chapter 1, I document a positive impact of foreclosure delays — a widely used measure of foreclosure prevention — on labor income. I present novel evidence showing that foreclosure delays can foster sustained economic recovery through local labor markets by allowing borrowers to re-evaluate their employment decisions and address the root causes of their financial distress. To conduct this study, I leverage a temporary rule introduced by the Consumer Financial Protection Bureau in June 2021. This change prohibited mortgage servicers from initiating foreclosures between September and December 2021 on loans that missed their fifth mortgage payment on or after March 2020. I uncover a noteworthy increase in income, resulting from a four months foreclosure delay and attribute this to increased job mobility. The granularity of the dataset allows me to establish temporary liquidity and housing stability as potential mechanisms. This research carries substantial policy implications, underscoring the potential effectiveness of well-designed, short-term borrower protection policies. These policies can yield significant real-world effects and promote financial stability.

In Chapter 2, I investigate how firm monopsony power in local labor markets impacts households' ability to withstand adverse shocks. Rising labor market concentration and firms' influence on wages can hinder households' ability to adjust income during financial difficulties, exacerbating their distress. Using gasoline price fluctuations following the Russia-Ukraine conflict as a natural experiment, I find that individuals without access to public transportation, and facing longer commutes to work, experience higher credit card defaults and revolving debt burdens. High-monopsony employers limit individuals' capacity to negotiate better wages, particularly affecting financially constrained households. Overall, this study highlights local labor markets' role in transmitting macroeconomic shocks through household balance sheets.

In Chapter 3 with Naser Hamdi and Professor Ankit Kalda we examine whether labor markets can effectively discipline financial misconduct. Our findings indicate that finance professionals who experience involuntary separation for misconduct tend to earn higher income than those laid off for no-fault reasons. We attribute this to assortative matching in the finance labor market, where firms prone to misconduct also hire such individuals, offering them a wage premium. Our results are unique to the finance sector. The absence of a disciplining mechanism in the finance sector is likely due to the nature of financial products which rely on future cash flows, making misconduct harder to detect.

# Chapter 1

# Time on your Side: Labor Market Effects of Foreclosure Delays

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## Abstract

I study the effect of foreclosure delays on labor income and employment outcomes, exploiting a temporary CFPB rule that restricted servicers from initiating foreclosures. Using detailed employee-employer matched administrative data linked with individual credit profiles in the U.S., I employ a difference-in-differences design and compare borrowers who were 120+ days delinquent one month before versus one month after the cutoff eligibility date of the rule. I estimate a 2.5 percent increase in income for borrowers eligible for up to four months of foreclosure delays. The higher income is attributed to an increased probability of job switching. Temporary liquidity and extended period of housing stability explain my findings. Furthermore, these delays lead to a persistent decrease in the probability of default and foreclosure over the year following the policy. Overall, my research suggests temporary delays, when implemented during the early stages of the foreclosure process, can empower borrowers to achieve financial stability by fundamentally reshaping their income prospects through the labor markets.

*Keywords:* Foreclosure Delays, Labor Income, Job Mobility, Liquidity, Credit, Default, COVID-19

## 1.1 Introduction

Do policies designed to prevent foreclosures, such as foreclosure delays, yield substantial economic benefits? Since the aftermath of the Great Recession, delays in the foreclosure process, whether mandated by law or government intervention, have been utilized to mitigate the impact of foreclosures—a situation known to have far-reaching consequences on financially struggling homeowners and neighborhoods (Clauretie and Herzog, 1990, C. W. Calomiris et al., 2008, Campbell et al., 2011, Mian et al., 2015, Gupta, 2019, R. Diamond et al., 2020, Piskorski and Seru, 2021). Despite being a widely used policy instrument for foreclosure prevention, little consensus exists over the efficacy of foreclosure delays. Whereas some studies suggest that they result in a higher rate of recovery and loan modifications (Maturana, 2017, Collins and Urban, 2018, Gabriel et al., 2021, Sandler, 2023, Padi et al., 2023), others offer evidence that points to a recurrence of homeowner delinquencies and only modest effects on consumption and wealth (C. Calomiris and Higgins, 2011, Cordell et al., 2015, Zhu and Pace, 2015, Gerardi et al., 2015, Kim, 2019). These contrasting views have led some to conclude that the benefits of foreclosure delays have been overstated. This debate primarily revolves around the advantages associated with debt restructuring while overlooking the substantial potential for recovery through local labor markets. A report by the US Government Accountability Office (2011) highlights the possibility that foreclosure delays could provide borrowers with extra time to secure additional income, thereby affecting their ability to address the root causes of financial distress.

In this paper, I present new systematic evidence for this prospective benefit of foreclosure delays, revealing a pathway to sustained economic recovery through labor markets. Specifically, I ask, Can early stage foreclosure delays, that is, delays in foreclosure filing, enhance the income and employment prospects of distressed borrowers? I exploit a temporary rule delaying foreclosure filing and leverage detailed employee-employer matched administrative data linked with individual credit profiles to answer this question.

Conceptually, there are two opposing forces influencing the impact of foreclosure delays on labor income. On the one hand, there is moral hazard stemming from an increased likelihood of income-contingent loan modifications during delays, which might diminish the incentives for borrowers to actively pursue higher-wage employment. On the other hand, the supplementary resources—both in terms of time and liquidity—that foreclosure delays offer can strengthen borrowers' capacity to engage in job search, leading to an increase in income.

My central finding is that delaying foreclosure filing increases labor income, primarily driven by increased job mobility. Temporary liquidity and extended period of housing stability from foreclosure delays enhances job search ability by acting as a private source of self-insurance against search-and-matching frictions in the labor market. Consistent with this, my estimated effects are concentrated among ex-ante liquidity-constrained borrowers, especially those facing labor market slackness. Furthermore, I note larger effects for borrowers in non-judicial states and those with high loan-to-value ratios in states permitting recourse on non-mortgage assets, proxying for susceptibility to housing insecurity. I discuss the detailed theoretical underpinnings of this mechanism in the conceptual framework section below. Finally, I document long-term benefits of delaying foreclosure filing. I provide evidence of a persistent decrease in the likelihood of both mortgage and non-mortgage defaults and a reduction in the probability of foreclosure occurring over a year following the implementation of the policy.

Identifying the causal effect of foreclosure delays on labor market outcomes is empirically challenging. This challenge arises from the difficulty in identifying appropriate counterfactuals given the broad coverage of foreclosure prevention policies and the need for granular borrower-level income and employment data. My paper overcomes these challenges by combining a novel administrative payroll dataset with credit bureau records within the framework of a quasi-natural experiment. The quasi-random variation in foreclosure delays stems from a temporary extension of foreclosure filing introduced by the Consumer Financial Protection Bureau (CFPB), which applied retroactively.

The CFPB policy was announced in June 2021 and went into effect between September and December 2021. It mandated servicers to complete a detailed list of procedural safeguards before issuing foreclosure notices to eligible mortgages. Eligibility was based on delinquency status around an arbitrary cut-off date of March 1, 2020, more than a year before the policy announcement. The retroactive eligibility limits strategic selection into eligible groups because individuals could not have anticipated this rule in 2020. Specifically, this policy delayed foreclosure filing by up to four months for borrowers who missed their fifth mortgage payment (i.e., entered 120+ days of delinquency) in March 2020 (treated group). By contrast, borrowers for whom the transition to 120+ days delinquency happened in February 2020 (control group) were not granted the delays. I employ a difference-in-differences (DiD) research design that compares the labor income of the treated group with the control group before and after the policy implementation. I condition on individual fixed effects to control for all time-invariant heterogeneity due to differences in preferences, zip-code by year-month fixed effects to control for all time-varying geographic variation, and industry by year-month fixed effects to control for industry-specific shocks.

I begin by evaluating the first-stage relevance of the policy. The probability of foreclosure filing for the loans subject to the policy reduced by as much as 3 percentage points compared with the control group during the policy's effective period. This reduction constitutes a substantial 52 percent decrease when compared with the average likelihood of foreclosure filing within the sample, indicating a noteworthy adherence to the policy directives.

The paper's first result identifies the effect of foreclosure delays on labor income. My DiD estimate indicates the income for treated borrowers increased by 2.5 percent relative to the control group of borrowers. Specifically, delaying foreclosure filings by four months boosts borrowers' annual income by $1600 following the implementation of the CFPB policy.

The causal interpretation of the treatment effect relies on the assumption that, in the absence of delays in foreclosure filings induced by the CFPB rule, labor income for individuals in the treatment and control groups would have evolved according to parallel trends. I provide evidence for this assumption by analyzing dynamic treatment effects and find no indication of pre-existing trends and a significant increase in income by up to 5 percent over the one-and-a-half years following the policy. Furthermore, I conduct a falsification test by exploiting variation within a group of eligible loans for which the policy did not bind, namely, loans that were already in some stage of foreclosure or were current as of the month before policy implementation. I find no significant differences in labor income between the pseudo treated and control groups after policy implementation within this group.

I address any residual concerns related to potential systematic differences between the treatment and control groups attributable to COVID-19-induced distress, which could threaten my identification. In my research design, I compare loans that missed their fifth mortgage payment in March 2020 with those in February 2020. This approach ensures the treated group differs from the control group in only one missed payment. In fact, this transition from 120 days of delinquency to 120+ days past due is marginal compared with entering a 90-day delinquency, which both groups had experienced before the onset of pandemic-related events. Furthermore, mortgage contracts typically adhere to a standardized payment structure, specifically requiring payments at the start of each month, with a 15-day grace period before borrowers are classified as delinquent.[1] Because lockdowns in the US began after March 19, [2] the ability to pay for the mortgage within the due date, that is, before March 15, should remain unaffected by pandemic-related events. I demonstrate that layoffs followed a consistent seasonal pattern until March 18, after which a notable surge occurred, coinciding with the timing of the lockdown. I also show the likelihood of transitioning from 120 to 120+ days of delinquency in March 2020 is similar across states that experienced lockdown earlier versus later. Finally, I present evidence indicating no meaningful differences between the treated and control groups across a wide set of observable characteristics.

My baseline finding which indicates a positive effect of foreclosure delays on labor income is inconsistent with moral hazard, and rather aligns with the job search mechanism. Subsequently, my second set of findings furnishes supporting evidence for the job transition as a primary driving force. Specifically, my analysis reveals a 18% higher relative likelihood of individuals changing their job compared to the sample mean and an increased likelihood of individuals working in a different zip code. Furthermore, the relative income increase from unemployment to employment transition is 8.7% and is larger compared to the income increase attributable

[1] Borrowers have discretion on when to close the mortgage, but the mortgage payment is typically due on the first day of each month starting the second month after the loan's closing date. Some lenders provide the flexibility of due date changes, but borrowers owe more once the account matures to cover any interest accrued because of the change. Moreover, this flexibility is subject to several conditions, e.g., the borrower must have made the first mortgage payment, the intended due date cannot be more than 15 days from your existing mortgage payment due date, this change is permitted only once during the loan's duration, and the borrower must be current at the time of requesting a due date change. Source: Source: Experian, Quicken, Lending Club, US Bank

[2] https://en.wikipedia.org/wiki/COVID-19_lockdowns

to on-the-job transition. Additionally, a decomposition of gross income into components such as commissions, hourly wage rates, and the number of hours worked reveals a relative increase in hourly wage rates, more hours worked, and heightened commissions for the treatment group over a year and a half following delays. Moreover, individuals subject to the treatment exhibit longer tenure following foreclosure delays, a higher likelihood of working in full-time positions, and a reduced likelihood of holding multiple jobs. I find limited evidence for changes in individual income within the same employer. Considering insignificant income variation among non-switchers, these findings likely indicate an enhanced job-match quality subsequent to job switch.

Underlying job mobility is the enhanced ability to search, which is faciliated by resources from foreclosure delays such as liquidity and extended time to stay in the house. In my third set of results, I test the merit of this resource channel by examining heterogeneity based on differential exposure to liquidity constraints, search-and-matching frictions and housing insecurity. In the cross-section, I observe that liquidity-constrained individuals — as measured by above-median credit utilization, debt-to-income ratio, and credit score below 620 — experience a larger relative increase in labor income. The treatment effects are further concentrated amongst liquidity constrained individuals in slack labor markets. Additionally, the unemployed, who face relatively more severe labor market frictions compared to employed experience larger relative income increases following foreclosure delays. Lastly, conditional on finding a job, within employed, the treated take longer to make a job switch and within unemployed, the treated experience a longer unemployment duration relative to the control group respectively. These findings align with the concept that liquidity empowers individuals to extend their job search by facilitating costly job search efforts and acting as a form of self-insurance. Moreover, the effects are larger for borrowers in power-of-sale states versus judicial states and high loan-to-value (LTV) borrowers in recourse states vs low LTV borrowers in non-recourse states. The findings underscore the added benefit of prolonged housing stability in the context of job search.

The paper's final result evaluates the long-term impacts of foreclosure delays on borrowers' credit performance. I observe a sustained decline in the probability of mortgage and non-mortgage defaults and foreclosure incidents over a year following the introduction of the CFPB policy. This observation suggests pre-foreclosure delays empower borrowers to achieve lasting financial stability by fundamentally reshaping their income prospects through the labor markets. This finding has important implications in light of concerns that many debt-relief measures may still leave individuals susceptible to delinquency and financial irresponsibility. Nevertheless, given that these measures come with expenses such as direct costs for servicers and indirect costs such as housing market inefficiencies and tighter credit conditions for new borrowers,[3], further analysis is required to determine if the benefits from foreclosure delays can outweigh these costs.

**Conceptual Framework:** I rely on different existing theories of job search to inform my empirical examination of the underlying mechanisms explaining my results.

Temporary liquidity from foreclosure delays comes in the form an implicit credit line that opens up during delays where mortgagors may default on their mortgage payment without facing imminent foreclosure or eviction. This liquidity may enhance individuals' ability to navigate labor market frictions.[4] Theories of job search which incorporate liquidity and financial frictions inform this mechanism. A broad class of search models accounting for factors like dependence on unemployment benefits (Mortensen, 1977, P. A. Diamond, 1981, Acemoglu and Shimer, 2000, Chetty, 2008) and credit access (K. Herkenhoff et al., 2016, Braxton et al., 2020) demonstrate that additional liquidity leads to increases in reservation wages, unemployment durations, and subsequent re-employment wages. Another class of models, not mutually exclusive, incorporates on-the-job search, expanding the scope of analysis beyond the unemployed to also consider employed individuals. These models show that job switching is often triggered by low or volatile earnings and heightened unemployment risks (Pissarides, 1994, Akerlof et al., 1988, Christensen et al., 2005, Pinheiro and Visschers, 2015, Gregor, 2015, Jung and Kuhn, 2019). Furthermore, within this class of models, financial market incompleteness has been shown to affect labor income by preventing workers' ability to switch to more productive occupations (Hawkins and Mustre-del-Rio, 2016; Cubas and Silos, 2020).

Besides liquidity, the extended period of housing stability and avoidance of time intensive foreclosure and eviction proceedings allow individuals to allocate more time to improving

[3] I perform back-of-the envelope calculations to quantify the direct costs to services based on a report by Goodman, 2016, which outlines the penalties imposed by FHA and GSEs for delaying foreclosure filings beyond 180 days from the initial default. The penalty for each loan per month is determined by multiplying the remaining balance at the time of default by the note rate divided by 12 and then multiplying this amount by the number of delayed months. As of August 2021, there were 720,000 mortgage accounts in default that had not yet undergone foreclosure. The average outstanding balance for these accounts was $110,000. I further assume an average note rate of 3% because most of the loans in the sample originated between 2010 and 2019. Consequently, the calculation is as follows: 720,000 x (0.03/12) x 110,000 x 4 = $800 million.

[4] In search models, the "job offer arrival rate" is modeled as a function of search effort or waiting time.

employment prospects. This assertion aligns with existing literature on housing insecurity, which documents the negative effects of eviction of low-income renters (Desmond, 2012; Desmond and Shollenberger, 2015; Desmond and Gershenson, 2016) and foreclosure-related evictions (Collinson et al., 2022, R. Diamond et al., 2020) on employment outcomes.

Finally, an extension in foreclosure filing may serve as a crucial respite for borrowers, offering temporary relief from the overwhelming stress and anxiety associated with impending foreclosure (Currie and Tekin, 2011; Osypuk et al., 2012; Houle, 2014; Allen et al., 2015; Tsai, 2015; Bernal-Solano et al., 2019). To the extent that reduced financial stress frees up cognitive resources for productive work (Sergeyev et al., 2023), this may empower borrowers to approach their financial predicament with a clearer mindset. This clarity could lead to optimized employment choices, such as improving job match or increasing work hours, ultimately resulting in higher earnings.

The rest of the paper is structured as follows. Section 2 discusses the related literature, and section 3 presents the institutional details. Section 4 describes the data employed, sample construction, and my empirical strategy. Section 5 presents the main results of the paper. Section 6 examines the underlying mechanisms, section 7 discusses policy implications, and section 8 concludes.

## 1.2 Related Literature

The contribution of this paper is three-fold. Firstly, my paper assesses the impact of foreclosure delays on borrower income and reveals a new channel, mediated by labor markets, through which foreclosure prevention policies like delays can lead to sustained economic benefits. In doing so, it broadens our understanding of the effects of such delays on a borrower's overall financial profile, going beyond the typical focus on the liabilities side of borrowers' balance sheets. To the best of my knowledge, my research is one of the first papers to provide a systematic empirical analysis of the effect of foreclosure delays on labor market outcomes. A closely related study by K. F. Herkenhoff and Ohanian, 2019 examines the impact of extended foreclosure timelines on job search for unemployed in a quantitative job search model and uses aggregate data for unemployed mortgagors to show that foreclosure delays enhance job match quality by providing people additional time to search for high paying jobs. My paper differs in several significant ways. I investigate the broader labor market implications of foreclosure delays, considering both employed and unemployed individuals. Moreover, the rich microdata on credit and employment allows me to delve into detailed mechanisms by which foreclosure delays affect job search and subsequent labor income. My research specifically focuses on evaluating the consequences of foreclosure delays introduced at the filing stage, diverging from their study, which investigates the effects of a general increase in the foreclosure timeline owing to various regulatory changes in the years following the Great Recession.

Second, my paper contributes to the broad literature examining the causes and consequences of delays in the foreclosure process. Varying regulations across states, such as right-to-cure law or the judicial-review right (Gerardi et al., 2013) and documentation issues (Allen et al., 2015) have been shown to lengthen foreclosure timelines. Foreclosure delays induced by these regulatory changes have proven costly (C. Calomiris and Higgins, 2011, Gerardi et al., 2013, Cordell et al., 2015,Dagher and Sun, 2016, Cordell and Lambie-Hanson, 2016) with implications on borrower credit outcomes (Ghent and Kudlyak, 2011, Collins and Urban, 2014, Zhu and Pace, 2015, Chan et al., 2016, Kim, 2019, (Calem et al., 2017, Sandler, 2023, Padi et al., 2023), credit supply (Jones, 1993, Pence, 2006,Ghent and Kudlyak, 2011, Curtis, 2014, Dagher and Sun, 2016, Zhao et al., 2019) and house prices (Gerardi et al., 2015, Gabriel et al., 2021). Unlike previous studies that heavily rely on variations between states to identify foreclosure delays, the individual-level cross-sectional variation in foreclosure timelines induced by the CFPB policy in my setting allows me to tackle numerous underlying endogeneity concerns by enabling me to utilize variations within a state and within lenders to identify foreclosure delays.

Third, my study speaks to the growing literature evaluating borrower protection policies, including studies on bankruptcy protection (Dobbie and Song, 2015, Dobbie et al., 2017, Auclert et al., 2019, Di Maggio et al., 2019), debt waiver (Mukherjee et al., 2018, Piskorski and Seru, 2021) and forbearance and debt restructuring (Mayer et al., 2014, Agarwal et al., 2017, Ganong and Noel, 2020, Cespedes et al., 2021, Cherry et al., 2021, Aydin, 2021, Fiorin et al., 2023, Dinerstein et al., 2023, Lourie et al., 2023) and moratoriums (O'Malley, 2021, Dinerstein et al., 2023). The large real effects I document from a temporary four-month debt relief in the form of an extension of foreclosure timeline suggests that well-designed borrower protection policies, even temporary in nature have the potential to be cost-effective instruments for promoting financial stability.

## 1.3 Institutional Details

### 1.3.1 Foreclosure Process

The Dodd-Frank Act grants the Consumer Financial Protection Bureau (CFPB) the authority to oversee and enforce compliance with the Real Estate Settlement Procedures Act (RESPA) and its rules. In January 2013, the CFPB introduced Mortgage Servicing Rules, incorporated into Regulation X, to implement RESPA.

According to Regulation X, a borrower's mortgage loan enters default if it becomes more than 90 days delinquent; however, servicers cannot initiate foreclosure until the delinquency exceeds 120 days. This period from the first missed payment until 120+ days of delinquency is referred to as the pre-foreclosure period. During this phase, when a borrower defaults, the lender sends a notification known as a "breach letter" or "notice of default" before accelerating the loan and moving toward foreclosure. Regulation X provides guidelines for early intervention, maintaining communication with borrowers, and pursuing loss mitigation actions during this period. Once the delinquency surpasses 120 days, the lender sends a "Notice of Intention to Foreclose (NOI)." The subsequent steps vary depending on whether the mortgage originated in a judicial or non-judicial foreclosure state. In non-judicial states, the lender may combine the NOI with a notice of sale or a publicly posted/published announcement indicating the property's intended sale unless missed payments are resolved. Failure to reinstate the loan by a specific deadline leads to auctions, deed transfers, and eviction. For judicial foreclosure, the lender sends the NOI at least 30 days before filing a complaint in the Office of Foreclosure. The borrower has 35 days to respond and 60 days to request mediation. In the absence of a response or case of a non-contesting answer, the lender asks the court to Enter default, followed by the court permitting foreclosure. The property then undergoes a Sheriff's Sale Process (public auction), and eviction follows. The stages in the foreclosure process are depicted in Figure 1.1.

### 1.3.2 Details of the CFPB Temporary Rule

This section describes the temporary amendments to mortgage servicing rules under Regulation X announced by CFPB, which introduced delays in foreclosure filing.

With the onset of the Covid-19 pandemic and the various protections granted to borrowers within the scope of the CARES Act, March $27^{th}$ 2020 (e.g., forbearance program of up to 180 days and foreclosure moratoria), any new foreclosure actions were halted, and ongoing foreclosure proceedings were suspended until a specified date. Over subsequent months, extensions were granted to these provisions until their final expiration in July 2021.[5] Since the forbearance program/moratoriums did not necessarily pause delinquency, borrowers could have been delinquent for longer than 120 days at expiration of the CARES act protection without having faced foreclosure action during this period.[6] As a result, once a borrower's forbearance program or the moratorium ended, the servicer could accelerate the foreclosure process unless further extensions were granted to these protective measures.

Recognizing this foreclosure risk faced by millions of borrowers upon the expiration of the federal moratoria, on June 28, 2021, the CFPB issued amendments to the federal mortgage servicing regulations, which became effective starting August $31^{st}$ 2021. These amendments aimed to reinforce the ongoing economic recovery and protect mortgage borrowers as the federal foreclosure moratoria were phased out. The rule established temporary special safeguards to ensure that borrowers had sufficient time before foreclosure to explore their options, such as loan modifications and selling their homes.[7] In particular, during the protected period effective from August 31, 2021, to December 31, 2021, for "eligible" mortgages, servicers were prohibited from initiating foreclosure proceedings under state law unless they fulfilled specific procedural safeguards in addition to those mandated under Regulation X (Details in section A.1). These procedural safeguards applied to mortgages secured by the borrower's primary residence that became more than 120 days delinquent on or after March 1, 2020. Mortgages that entered 120+ days of delinquency before March 1, 2020, were not considered eligible. Starting in January 2022, servicers could resume foreclosure actions on all mortgages without the requirement to adhere to these comprehensive procedural safeguards. Because the procedural safeguards closely resembled the mitigation actions recommended before foreclosure filing, this temporary rule essentially prolonged the pre-foreclosure timeframe

[5] See the following for details. https://www.nhlp.org/wp-content/uploads/2020.04.10-NHLP-Homeowner-Relief-Info-Sheet-Update2.pdf

[6] See for details https://www.federalregister.gov/documents/2021/06/30/2021-13964/protections-for-borrowers-affected-by-the-covid-19-emergency-under-the-real-estate-settlementfootnote-62-p34853

[7] See CFPB Amendment to Mortgage Servicing Rules for the exact rule. These extensions weren't automatically granted to eligible borrowers; instead, they needed to contact their servicer and choose to participate proactively.

by as much as four months for qualifying mortgages, allowing foreclosure proceedings to commence for other cases. The timeline is summarized in Figure 1.2.

## 1.4 Data

My empirical analysis uses anonymized proprietary data on individual credit profiles and administrative payroll records from Equifax Inc..

### 1.4.1 Credit Data

The anonymized credit data contains information on the credit histories of all individuals (with a credit history) in the U.S. between 2010Q1-2023Q1. This data includes anonymous information on historical credit scores along with disaggregated individual credit-account level information such as account type (e.g., home loan, auto loan, student loan, and credit cards, etc.), borrower location, account age, total borrowing, account balance, monthly scheduled payment, any missed or late payments, defaults, foreclosures, and bankruptcy filings. It contains over 260 million consumer credit files and over a billion credit trades, i.e., information about single loans, and is updated monthly.

### 1.4.2 Payroll Data

The employment data contains anonymized information reported by employers who subscribe to the income verification services. Employers may face income and employment verification requests for their employees from social service agencies, property managers, mortgage/auto lenders, credit card issuers, pre-employment screeners, and ACA (Affordable Cares Act) verifications. While the requests from state and federal agencies mandate compliance, employers also want to fulfill other requests to support employees during significant life events. The employers use the services of Equifax Inc. to streamline and automate the process to ensure faster and more secure verifications and free up HR departments from this time-consuming and complex task, mainly because many different parties at different points in time can request these verifications. The data covers over 5,000 employers who report all employees' information on a payroll-to-payroll basis. It contains anonymized employee information on income, job locations, job tenures, type of jobs, and industry, among other details. The data covers over 100 million employees between 2010Q1 and 2023Q1 and is representative of the U.S. labor force along several dimensions, including median personal incomes and median employee tenure. In addition, the data closely tracks aggregate U.S. private sector payroll growth, hiring, and separations as shown in Gopalan et al., 2021. Additionally, Kalda, 2020a shows that the credit profiles of employees in the data are similar to those of the U.S. population.

### 1.4.3 Sample Construction and Summary Statistics

There were 720,000 active[8] mortgage accounts in default and not subject to foreclosure as of August 2021. From this population, the treatment group constitutes 13,000 loans, which had transitioned to 120+ days delinquency in March 2020, indicating that they were reported as only 120 days delinquent as of February 2020. The control group consists of 10,000 loans that entered 120+ delinquency in February 2020

I merge these borrowers' income and employment records using the payroll data described above. This merging process enables me to construct a panel dataset at the individual-employer-month level. By linking the relevant information, I can analyze the income and employment dynamics of the borrowers in question. I can identify employment records, including labor income, employer information, and industry classification, for approximately 12,000 loans out of 23,000. My sample period spans from September 2020 to April 2023.

Table 1.1 summarizes the main variables in my analysis. The median individual in my sample originates a $151,304 mortgage and has an outstanding balance of $129,459. Additionally, the median individual in my sample has seven active revolving credit accounts with overall credit utilization of 64%, measured as the ratio of the outstanding balance on revolving credit and their credit limit. The median credit score in the sample is fairly low at 554. These measures indicate that the median borrower in the sample is extremely credit constrained. The loan-to-value ratio (LTV) is imputed using origination amount, outstanding balance and zip code level house price index from Corelogic, assuming an origination LTV of 0.8. The median sample LTV of 0.46 and mean is 0.44, both indicating positive home equity. The

[8] These were also active as of January 2020

average annual labor income in my sample is \$61,815, with median income of \$54,575. I provide summary statistics of the full sample in Table A1. The two sample are very similar.

Note that my sample consists of individuals who fell into 120+ days of mortgage delinquency between February and March 2020 and remain delinquent, yet have not faced foreclosure as of August 2021. In Figure A1, the distribution of loans is illustrated across various underlying reasons for survival from March 2020 to August 2021. Considering that forbearance protection was available for federally backed loans under the COVID-19 CARES Act during this timeframe, nearly 50% of loans survive due to forbearance or federal moratoriums. Additionally, 35% of the loans able able to self-cure either completely or partially at least once and another 15% receive modification but eventually re-default.

I further investigate the underlying predictors of mortgage delinquency for these loans from June 2019 to August 2021. [9] Table A2 indicates that the likelihood of delinquency increases with the probability of unemployment and the presence of outstanding medical, child care, and utility-related debt (see Columns (1)-(2)). Alternatively, the growth in these expenses also positively influences the incidence of mortgage default. Interestingly, the coefficient on Loan-to-Value (LTV) is insignificant, suggesting that strategic motives are not the primary driver of default in my sample. This aligns with two facts presented earlier: a significant fraction of loans attempt to cure without assistance, and the median and average borrower has positive equity in their home.

### 1.4.4 Empirical Strategy

My paper estimates the effect of foreclosure delays on labor income. I exploit the cross-sectional and time-series variation due to the temporary amendments to the foreclosure filing process announced by the CFPB. The CFPB policy was implemented on August $31^{st}$ and delayed foreclosure filing by up to four months with retroactive eligibility. Delays in

[9] I estimate the following OLS regression:

$$y_{i,t} = \beta_1 \times Unemployed_{i,t} + \beta_2 \times Utilization_{i,t} + \beta_3 \times LTV_{i,t} + \sum \beta_k Expenses_{k,i,t} + \theta_i + \gamma_{z,t} + \epsilon_{i,t}$$

where $y_{i,t}$ is a dummy coded as 1 if individual $i$ has a delinquent mortgage in year-month $t$ and 0 otherwise. $Expenses_k$ measures the incidence of $k = \{medical,\ child\ support,\ utility\}$ debt, represented as an indicator coded as 1 in case of a positive outstanding balance in year-month $t$ or the growth from $t-1$ to $t$. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ for zipcode x year-month fixed effects.

foreclosure filing were applicable for individuals who entered 120+ days delinquent on or after an arbitrary cut-off date of March 2020. However, the policy did not grant foreclosure delays to loans for which the transition to 120+ days of delinquency happened before this cut-off date. My research design considers a tight one-month window around this arbitrary cut-off date. In particular, loans that missed their fifth payment (i.e., became 120+ days delinquent) in March 2020 constitute my treated group, and those that missed their fifth payment in February 2020 form my control group.

My baseline specification is a difference-in-differences research design, estimated on an individual-month panel:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h} \times Post_t^{Sep2021} + \theta_i + \gamma_{z,t} + \delta_{h,t} + \epsilon_{i,z,h,t} \tag{1.1}$$

where $y_{i,z,h,t}$ represents log earnings or dollar earnings for individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+DPD in March 2020 and 0 if the loan became 120+DPD in February 2020. $Post^{Sep2021}$ is an indicator variable equal to 1 for September 2021 and after and 0 for the months before. Individual fixed effects ($\theta_i$) address the concern that a direct comparison of the treatment and the control group may pose an empirical challenge if the two groups differ. Individual fixed effects allow to control for all time-invariant heterogeneity due to differences in preferences, skills, and other unobserved traits. Another issue with a direct comparison is that the labor income is a function of the local economic development and idiosyncratic shocks that may affect certain industries. Zipcode $\times$ month fixed effects ($\gamma_{z,t}$) and industry x month fixed effects ($\delta_{h,t}$) allow me to non-parametrically control for all time-varying granular differences arising from geography and industry that may determine labor earnings. Zipcode and industry are measured as of February 2020. Since individuals employed in the same industry may be subject to similar economic shocks, I cluster the standard errors at the 6-digit NAICS code throughout my analysis, allowing the errors to be correlated for all individuals within the same industry.

A potential concern with Equation 1.1 is that the systematic differences between the treatment and control groups due to COVID-19-induced policies and distress could drive the estimate of $\beta_{DD}$. In my research design, I compare loans that missed their fifth mortgage payment in March 2020 with those in February 2020. This approach ensures that the treated group differs from the control group by only one payment, shifting from 120 to 120+ days delinquent.

This distinction is marginal compared to entering a 90-day delinquency, which both groups had experienced prior to the onset of pandemic-related events. I further argue that mortgage contracts adhere to a standardized payment structure, specifically requiring payments at the start of each month with a 15-day grace period before borrowers are classified as delinquent. The decision on whether to skip a mortgage payment in March should remain unaffected by pandemic-related events, notably since lockdowns began only after March 19. Figure 1.3 demonstrates that layoffs followed a consistent seasonal pattern until March 18, after which a notable surge occurred. I also show that the likelihood to transition from 120 to 120+ days delinquency in March 2020 is similar across states that experienced lockdown in March versus later (See Table A3). Furthermore, in Table 1.2, I present evidence indicating no statistically significant differences between the treated and control groups concerning observable loan and borrower characteristics prior to the intervention, except for loan origination amount and origination term. However, I include these attributes as non-parametric controls in my baseline estimates to validate that these distinctions do not introduce bias into my findings.

The estimate of interest is the coefficient of the interaction term of $Treated_{i,z,h}$ and $Post_t$ given by $\beta_{DD}$. $\beta_{DD}$ is the estimate of the treatment effect capturing the treatment group's response to the policy relative to the control group. Specifically, $\beta_{DD}$ is a within ZIP code and within industry estimator comparing the average difference in the treatment and the control groups operating in the same zip code and within the same industry after controlling for all observed and unobserved time-invariant heterogeneity across individuals.

The causal interpretation of $\beta_{DD}$ relies crucially on two assumptions. First, the treatment group was subject to delays in foreclosure filing, whereas the control group was not. I verify the first-stage relevance assumption by examining the time series distribution of the cumulative share of foreclosure filing in my sample for the treated and control loans when the CFPB policy was effective. I further formally examine the differential likelihood of foreclosure filing between the treated and control group of mortgages by implementing Equation 1.1, replacing $y$ with an indicator coded as 1 if the loan contains a flag for foreclosure filing in calendar month $t$ and 0 otherwise. I further include lender x year-month fixed effects ($\delta_{j,t}$) accounting for any time-varying lender characteristics and control for time-varying origination cohort-specific effects using $\omega_{c,t}$.

Second, without a policy change, the outcomes for mortgagors in the treatment and control groups would have evolved according to parallel trends. I investigate the parallel trends assumption by estimating a dynamic specification as in Equation 1.2 to analyze the log earnings for the treatment and control groups before the policy.

$$y_{i,z,h,t} = \sum_{\substack{k=Sep'20 \\ k\neq Aug'21}}^{Apr'23} \beta_k \times Treated_{i,z,h} \times D_k + \theta_i + \gamma_{z,t} + \delta_{h,t} + \epsilon_{i,z,h,t} \tag{1.2}$$

where $D_k$ is an indicator that equals one for observations corresponding to individual $i$ when the observation belongs to month $k$. All other variables are the same as defined earlier. The omitted baseline period is August 2021. An added advantage of the dynamic specification is that it allows us to evaluate the evolution of the treatment effect after the policy.

Another assumption is the stability of the treatment and the control unit over time. The stability assumption is mechanically satisfied in my setting due to the institutional feature of the policy. The policy was announced in June 2021 and fixed eligibility based on delinquency status as of March 2020, i.e., approximately a year before. Therefore, the policy design makes eligibility an immutable characteristic and ensures the stability of my treatment and control units over time.

## 1.5 Results

This section presents the results of the effect of foreclosure delays under the CFPB policy on labor earnings and job mobility.

### 1.5.1 First-Stage Relevance of the Policy

I begin my analysis by evaluating the first-stage relevance of the policy. The treatment group, which experienced up to four additional months of foreclosure delays, experienced a smaller likelihood of foreclosure filing when the policy was effective vis-a-vis the control group. Panel a of Figure 1.4 plots the time series distribution of the cumulative share of foreclosure filings in my sample for the treated and control loans separately. During the policy's effective period, between September 2021 and December 2021, there was a notable difference in foreclosure filing activity between the treated and control groups. The cumulative share of foreclosures increased at a steeper rate for the control group compared to the treated group, indicating that the policy slowed down foreclosure filings for the treated mortgages relative to the control group. However, following the expiry of the policy, foreclosure filings for the treated loans started to catch up. Note that the eventual foreclosures/repossession did not fully catch up for the treated group relative to the control group, as shown in parallel trends observed between the two groups in Panel b of Figure 1.4.

The estimates of the dynamic treatment coefficients are shown in panel a Figure 1.5. Consistent with the time series plots, the Figure 1.5 shows a decline in the likelihood of foreclosure filing by up to 3 percent, representing a nearly 52 percent decrease relative to the average probability of foreclosure filing in the sample, i.e., 5.6%. This reduction in foreclosure filing likelihood is statistically and economically significant, indicating meaningful compliance with the policy guidelines.

### 1.5.2 Second Stage: Effect of the CFPB Policy on Labor Earnings

This section examines the effect of foreclosure delays on labor earnings under the amended CFPB rule. I estimate the differential change in earnings between the treated and control groups of mortgagors around policy implementation following different combinations of the empirical specification in Equation 1.1. The findings are summarized in Table 1.3 where column (1) is the least saturated specification with only individual and month fixed effects. The difference-in-differences estimator shows that treated individuals, on average, experienced between 2.5 percentage points (pp) growth in annual earnings following a four-month delay in foreclosure filing. Measured in dollar terms, this growth corresponds to an average increase in annual earnings of $1,600, as indicated in column (1) Panel B. [10] I gradually saturate the model by including additional fixed effects in columns (2) through (4). For example, in column (2), I add zip x month fixed effects where the zip is the zip code of residence of the mortgagors before treatment and captures any changes in local economic conditions over time. Column (3) further controls for industry-specific differences varying over time using industry-by-month fixed effects. Finally, wage quartiles x month and credit score quartiles by month fixed effects in column (4) allow comparing individuals with similar income and credit

[10] Figure A2 illustrates the evolution of income for both treated and control individuals during the period surrounding the policy implementation. The raw plot suggests that the relative increase in income is not a result of income decline in the control group; instead, it stems from the treated group experiencing a relative income increase compared to the control group.

quality. Finally column (5) includes non paramteric controls for loan size and loan term in the form of loan and term deciles interacted with month to control for any pre-existing differences betwen treated and control group based on these attributes as indicated in Table 1.2. My estimates for the treatment effect are robust across these different specifications, suggesting that differences across the treatment and control groups are less likely to be systematically correlated with the treatment and labor earnings.

Figure 1.6 shows the coefficients $\beta_k$ from Equation 1.2, which capture the differential response of labor earnings for treated mortgagors relative to the control ones in the months around CFPB policy, along with the 95% confidence intervals. While there are no significant pre-trends in income before treatment, there is a notable increase in income for the treated group in the subsequent months. This increase becomes particularly evident from February 2022 onwards. It persists, resulting in the treated group experiencing income levels that are approximately 5 percent or equivalently up to $3,000 higher than the control group one year after the implementation of the policy. The rationale behind the lagged response is discussed in section subsection 1.6.1.

#### Robustness

This section examines several potential concerns related to the robustness of the findings presented in Table 1.3 and Figure 1.6.

**Sample Attrition**: My sample consists of individuals who seek employment within firms that subscribe to the verification services of the data provider. This creates two potential issues. First, there could be a selection of who gets hired within this set of employers that can bias my findings. Furthermore, if the control individuals are more likely to transition to employers outside of my data coverage following treatment, my interpretation of the relative increase in earnings may be misleading. I address these issues by examining dropout rates from my sample across the treated and control mortgagors. Specifically, I re-estimate Equation 1.1, replacing the outcome variable with an indicator called $Inactive$, which is coded as 1 if an individual is not actively employed at time period $t$ and 0 otherwise. As summarized in Table A4, the treated individuals do not exhibit any differential likelihood of sample attrition relative to the control group in the months following policy implementation.

**Falsification Test:** Differences between the treated and control group of loans other than foreclosure delays induced by the CFPB policy could be driving my results. To alleviate this concern, I exploit variation between treated and control within loans for which the policy did not bind. These include mortgages that met the eligibility criteria but were either already in some stage of foreclosure before the policy or had recovered from delinquency as of August 2021, i.e., the month prior to policy implementation. If my relative income increase was explained by factors other than foreclosure delays, then we should expect to see differential income for the treated relative to the control following policy within the group of non-binding loans. First, I confirm the absence of any first-stage effects of the policy in terms of differential likelihood of foreclosure filing within the non-binding group of loans as shown in Figure A3. Then, I re-estimate Equation 1.1 for the non-binding group of loans. Table A5 shows that the treatment effect is both statistically and economically insignificant. The corresponding dynamic treatment effects are shown in Figure A4. I also estimate a triple difference-in-differences (DiD) specification as in **??**, where the coefficient of the triple interaction measures the differential treatment effect for the binding loans relative to the non-binding loans. This coefficient shown in Table A6 resembles my baseline treatment effect. Taken together, these results provide direct evidence for the assumption that the treatment and control groups would have evolved according to the parallel trends in the absence of the policy.

**Confounding Effects due to forbearance or modification:** Mortgages within my dataset may not have undergone foreclosure proceedings as of August 2021, as they could have been subject to a forbearance arrangement. Given that forbearance temporarily suspends mortgage payments, it's plausible that those subject to this treatment experienced a relative increase in their income due to the liquidity provided by forbearance, rather than as a result of foreclosure delays. However, it's worth noting that as of August 2021, less than 2% of individuals in my sample were enrolled in a forbearance plan, primarily because most federal protections that offered forbearance had expired in July 2021, predating the CFPB policy. Importantly, my baseline estimates remain robust even when excluding loans that were under forbearance as of August 2021, as demonstrated in Table A7. Similarly, loans in my sample may have been subject to modification in the form of payment or principal reductions. To the extent that individuals could have utilised the resulting liquidity towards enhancing labor outcomes, this may confound my estimates. My baseline estimates are robust to the exclusion of these loans as well as depicted in Table A8.

**Other:** Table A9 shows that my estimates are robust to alternate choices of stand error clustering, for e.g., cluster standard errors at the individual and zip code level respectively. My estimates are also robust to alternate specifications of the dependent variable. I use normalized labor income as the dependent variable where I divide monthly income by the average monthly earnings from September 2020 to April 2023. The results are presented in Table A10.

## 1.6 Mechanism

In this section, I explore the mechanisms of how foreclosure delays lead to higher earnings. I start by examining the job search channel and show that foreclosure delays increase job mobility and transition from unemployment to employment and positively alter the nature of job match. Next, I investigate temporary liquidity and additional time period of housing stability as a potential pathways in facilitating higher earnings.

### 1.6.1 Sources of Increase in Labor Earnings

There are three potential sources of increase in labor income: 1) individuals may experience increase in income in their current employment attributable to enhancements in labor productivity; or foreclosure delays may facilitate 2) job-to-job transitions and 3) transition from unemployment to employment. I explore each of these possibilities in detail.

**Within Employer Changes:** The higher earnings could stem from changes in income within the same employer, possibly due to improvements in labor productivity subsequent to foreclosure delays. I isolate within employer changes in earnings by replacing individual fixed effects by employer interacted with individual fixed effects in my baseline specification Equation 1.1. The treatment effect here is identified from variation in earnings for individuals who are employed with the same firm as their pre-policy employment for atleast some time during the months following the CFPB policy. Column (5) in Table 1.3 shows that with the inclusion of employer x individual fixed effects, the treatment effect is no longer statistically different than zero. Within firm, changes explain little variation in labor earnings following CFPB policy.

**Job Mobility:** The delayed reaction of labor earnings, as illustrated in Figure 1.6, suggests job-mobility to a potential reasons behind the relative earnings increase. This delay may be linked to the time required for job searches. I investigate the possibility of job mobility by examining two proxies, namely, the likelihood to change employer and the likelihood to change employment zip code respectively. In Panel A of Table 1.4, I present the effects of foreclosure delays on the likelihood of changing job. This variable is coded as 100 if individuals change their job from the month before and 0 otherwise. I find that individuals who are subject to foreclosure delays under the policy are 0.31 percentage points more likely to change their employment. In terms of economic magnitude, this represents a 18% increase relative to the mean likelihood of switching job in my sample for employed (1.772%). Similarly, Panel B shows the changes in the likelihood of changing employment zip code. This variable is coded as 100 if an individual works in a different employment zip code relative to their employment in the previous month. I find consistent evidence of increasing job mobility along this dimension as well.

Moreover, when individuals undergo a change in employment, those subjected to delays, conditional upon making the switch, take 6% more time or 0.58 months longer—a proxy for extended job search duration.

**Unemployment to Employment:** I re-estimate the income effects for the sub-group of individuals who were unemployed as of August 2021 and compare the magnitudes to those who had a job as of this date. As shown in Columns (1)-(2) of Table 1.5, the relative earnings increase for treated individuals following foreclosure delays is 8.7%. Note that the wage change is relative to the earnings in the pre-unemployment job. This increase is larger is compared to the earnings increase for those employed. Furthermore, considering unemployment duration as a proxy for search duration, I find that treated individuals have a greater unemployment duration i.e., between 0.37-1.61 months or 4-19% relative to the control group, depending on how long these individuals had been in unemployment.

**Nature of Job Transition:** I analyze the impact of foreclosure delays resulting from the CFPB policy on various components of gross income. Specifically, for hourly wage workers, I observe both their hourly wage rate and the number of hours worked. The findings, presented in Table 1.6, reveal several noteworthy outcomes. Treated individuals experience a 1.7% increase in their hourly wage rate (column (6)), and there is a positive and significant effect on hours worked (column (5)). Additionally, there is a 0.182% rise in the share of commission as a fraction of total compensation, representing a 25% increase relative to the average commission share. Moreover, individuals subject to the treatment exhibit longer tenure following foreclosure delays, a higher likelihood of working in full-time positions, and a reduced likelihood of holding multiple jobs. Considering that individuals with more than one job tend to earn less on average, as indicated in Table A11, these findings suggest that foreclosure delays enable individuals to transition to more stable employment. Overall, as there is insufficient variation in earnings from non-movers, as illustrated in subsection 1.6.1, these results signify long-term effects of foreclosure delays on the quality of job matches.

### 1.6.2 Temporary Liquidity and Extended Period of Housing Stability

In the context of imperfect credit markets, the financial expenses linked to job search, coupled with search-and-matching frictions in labor markets, render job-seeking a costly and time-consuming endeavor. Consequently, the supplementary liquidity stemming from delayed foreclosure may serve as a form of self-insurance and bolster individuals' capacity to cover the costs associated with job search. Moreover, beyond the aspect of liquidity, the heightened housing and financial stability arising from an extended stay in the residence and the mitigation of the prolonged foreclosure process may independently empower individuals to devote more time to job search activities. This, in turn, holds the potential for a substantial improvement in their income. Empirically differentiating between these two channels poses a challenge. Consequently, I discuss evidence consistent with both these channels.

I begin by presenting evidence supporting the notion of temporary liquidity creation resulting from foreclosure delays. These delays afford borrowers the opportunity to reside in their homes on partial or no mortgage payments, shielding them from the immediate threat of foreclosure or eviction. This implicit line of credit, extending from the lender to the borrower, generates temporary liquidity. Essentially, it enables individuals to bolster their current liquid asset position by borrowing against future payments. In my sample, borrowers granted foreclosure delays exhibit a 7% higher likelihood of skipping mortgage payments without facing foreclosure during the four months when the CFPB rule was effective relative to those exempt from the policy. This, coupled with the observation that the relative likelihood of defaulting on non-mortgage and mortgage debt in the subsequent months following the policy is lower, aligns with the consumption smoothing motive of default documented in Baker and Yannelis, 2015 and Gelman et al., 2015 and is consistent with the limited roles for strategic default found in Guiso et al., 2013.

Subsequently, considering that individuals with ex-ante liquidity constraints stand to gain the most from additional liquidity, I investigate heterogeneity in my baseline results by employing various proxies to assess the pre-policy liquidity constraints. The summarized findings are presented in Table 1.7. I observe that the relative increase in labor earnings is more pronounced among individuals with above-median credit utilization (Column (1)), above-median mortgage payments as a fraction of their monthly income (Column (3)), above-median debt-to-income ratio (DTI) (Column (5)), and a credit score lower than 620 (Column (7)). [11]

Furthermore, recognizing that additional liquidity could be particularly crucial for individuals navigating greater labor market frictions, I conduct an analysis to investigate this aspect. Specifically, I explore heterogeneity based on ex-ante labor market tightness. To measure this, I utilize vacancy and separations statistics from the Bureau of Labor Statistics (BLS) and construct an industry-level measure of tightness. I divide my sample based on median level of tightness, with below (above) median group referred “Slack" (“Tight") respectively. The results are summarized in Table 1.8. Columns (1) - (2) indicate that the relative increase in income is more pronounced among individuals working in industries with slack labor market conditions compared to those in tight markets. Moreover, in columns (3) - (6), I narrow down the analysis to borrowers facing ex-ante slack conditions and observe that the effects are concentrated among the liquidity-constrained subgroup of individuals, across various proxies for liquidity such as debt-to-income and credit utilization. Furthermore, considering that the unemployed face more frictions than the employed, implying a relatively lower job offer arrival rate, I hypothesize that the income effects should be more substantial for the unemployed compared to the employed. As already discussed, the evidence in Table 1.5 is consistent with this hypothesis.

Additionally, if income increase is due to extended period of housing security, the grace time from foreclosure delays should be marginally more valuable for borrowers in power-of-sale states. In these states, foreclosure completion timelines are shorter due to the absence of judicial intervention. I test heterogeneity in my baseline findings across judicial and

[11] I present supplementary evidence. If larger mortgage payments result in more significant temporary savings from missing them, liquidity creation should be more pronounced for individuals with higher ex-ante monthly mortgage payments. In line with this, I observe a larger relative increase in income for individuals with above-median monthly mortgage payments (refer to Table A12) and for the subgroup of individuals who missed mortgage payments during the four-month effective policy period (see Table A13). Given that missing mortgage payments is an endogenous decision, this finding is suggestive and must be interpreted with limitations.

power-of-sale states. The results in Table 1.9 support this conjecture. Additionally, I stratify the sample based on ex-ante home equity and cross-state variation in creditor laws such as recourse debt, no debt collection and wage garnishment restrictions. [12] As depicted in Table 1.10, I find that the relative increase in earnings is more substantial in the subgroup of high LTV borrowers in states with pro-creditor laws compared to the other extreme of low LTV borrowers in states with borrower-friendly laws, supporting my hypothesis.

## 1.7 Discussion

My research uncovers the positive impact of foreclosure delays on labor earnings. However, when contemplating policy proposals aimed at providing debt relief, a common concern is the perception of these policies as transfers from lenders to borrowers. Foreclosure delays come with various associated costs. These costs encompass direct expenses, such as penalties imposed on servicers by entities like GSEs and FHA (Goodman, 2016). When borrowers fail to make their monthly payments, servicers are also obligated to cover property taxes and hazard insurance premiums. Additionally, they may encounter liquidity challenges as they continue to advance principal and interest payments to investors for delinquent loans, a process that only ceases with the official commencement of the foreclosure process and is recoverable only at the end of liquidation (Cordell et al., 2015). Indirect costs also come into play, including housing market inefficiencies and the potential for lenders to miss out on housing returns, especially in a thriving market (C. Calomiris and Higgins, 2011). These costs can become particularly significant if these borrowers remain at risk of financial difficulties in the future. Indeed, the advantages of assisting financially distressed borrowers may diminish if there is an expectation that these individuals will continue to be prone to delinquency and financial irresponsibility.

[12] Recourse mortgages provide lenders with legal recourse to pursue additional actions, such as wage garnishments or levying the borrower's bank account, to recover outstanding amounts even after collateral has been seized and the home has been sold. Similarly, states vary in laws affecting the ability of third-party debt collectors to recover delinquent debts. I utilize the index of state debt collection restrictions from Fedaseyeu, 2020 to measure the extent of restrictions on debt collectors, where a lower value of this index indicates fewer restrictions. Finally, following Lefgren and McIntyre, 2009 and Kalda, 2020b, I classify borrowers in my sample based on whether they reside in states with severe, medium and no restrictions on wage garnishment, that is, states that impose different caps to wage garnishment, potentially lower than the 25% of disposable income that is the federal maximum.

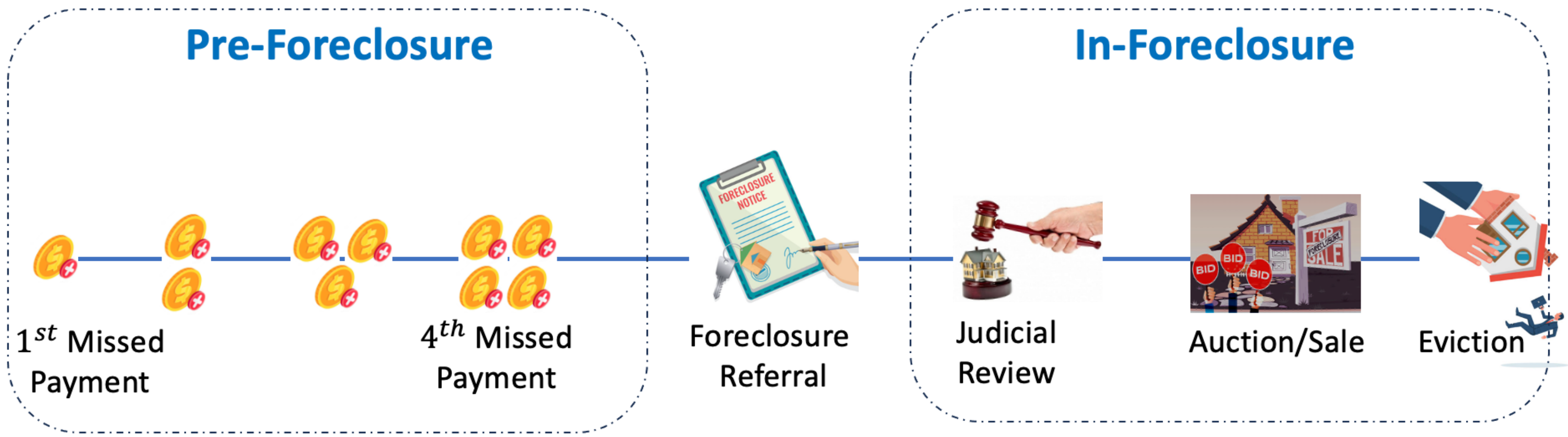
Nonetheless, my analysis demonstrates that foreclosure delays in the form of foreclosure filing extensions mandated by the CFPB policy provide borrowers with the opportunity to address the root causes of financial distress i.e., additional time and resources to increase their earnings, yielding long-term benefits. As seen in both Panel (a) and Panel (b) of Figure 1.7, there is a sustained reduction in the likelihood of default on mortgage and non-mortgage debt over a year following the implementation of the CFPB policy. Moreover, the relative likelihood of foreclosure also decreases significantly after the CFPB policy, as depicted in Panel (b) of Figure 1.5. It's important to note that the decline in mortgage defaults may be partially attributed to an increased rate of mortgage modifications, in addition to the improved financial position of borrowers. However, as revealed in Table A14, the increase in the probability of mortgage modifications in the post-policy period, while statistically significant, demonstrates limited economic significance compared to the mean modifications within the sample. These findings imply that the observed economic recovery, characterized by decreased delinquencies and actual foreclosures, can be attributed to the fundamental resolution of borrower distress, primarily driven by a relative increase in labor earnings. To the extent that prior research that examines in-foreclosure delays from judicial review fails to find positive effects, my research underscores that foreclosure delays if introduced during the early stages of the foreclosure process may be a crucial dimension to consider when assessing the merits of foreclosure delays. Nevertheless, while I document long-term benefits from foreclosure delays, further analysis is needed to assess whether these benefits can outweigh the costs. This provides a direction for future research.

Finally, there could be concerns regarding the external validity of my findings. The introduction of foreclosure delays through the CFPB policy coincided with a period of economic recovery following the pandemic, marked by tight labor markets. These concurrent factors may have amplified the income and employment responses I observe. While the extent of the observed effect may fluctuate in a different time period, the economic mechanisms I've described are likely to persist, suggesting that the link between foreclosure delays and income remains relevant.

## 1.8 Conclusion

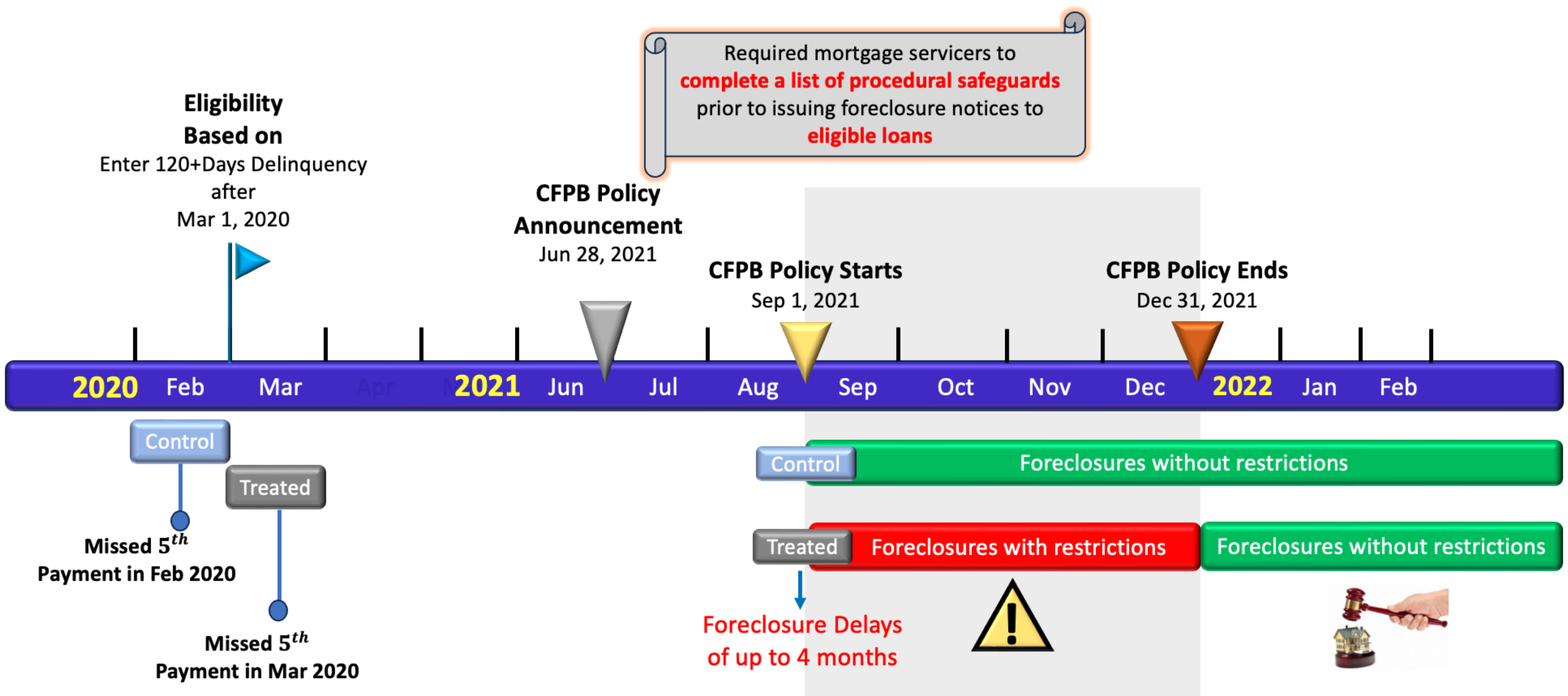
In this paper, I provide empirical evidence on the significant real effects of temporary foreclosure delays on borrowers' income and labor market dynamics. By taking advantage of the June 2021 CFPB amendment to the foreclosure initiation process as a source of exogenous variation to delays in foreclosure filings, my analysis reveals a substantial increase in labor earnings for borrowers eligible for up to four months of foreclosure delays. I document the role of job mobility in achieving earnings increase. I find evidence consistent with the results being driven by temporary liquidity and additional period of housing security. Finally, I provide evidence for a persistent decline in the default and foreclosure likelihood. Overall, I present novel evidence showing that delays can foster sustained economic recovery through local labor markets, allowing borrowers to re-evaluate their employment decisions and address the root causes of their financial distress. These results are particularly important in light of the debate surrounding the effectiveness of foreclosure prevention policies. My results underscore the potential for foreclosure policies that offer borrowers more time to address their financial distress to not only benefit individual homeowners but also to contribute to economic stability.

Figure 1.1: Foreclosure Process



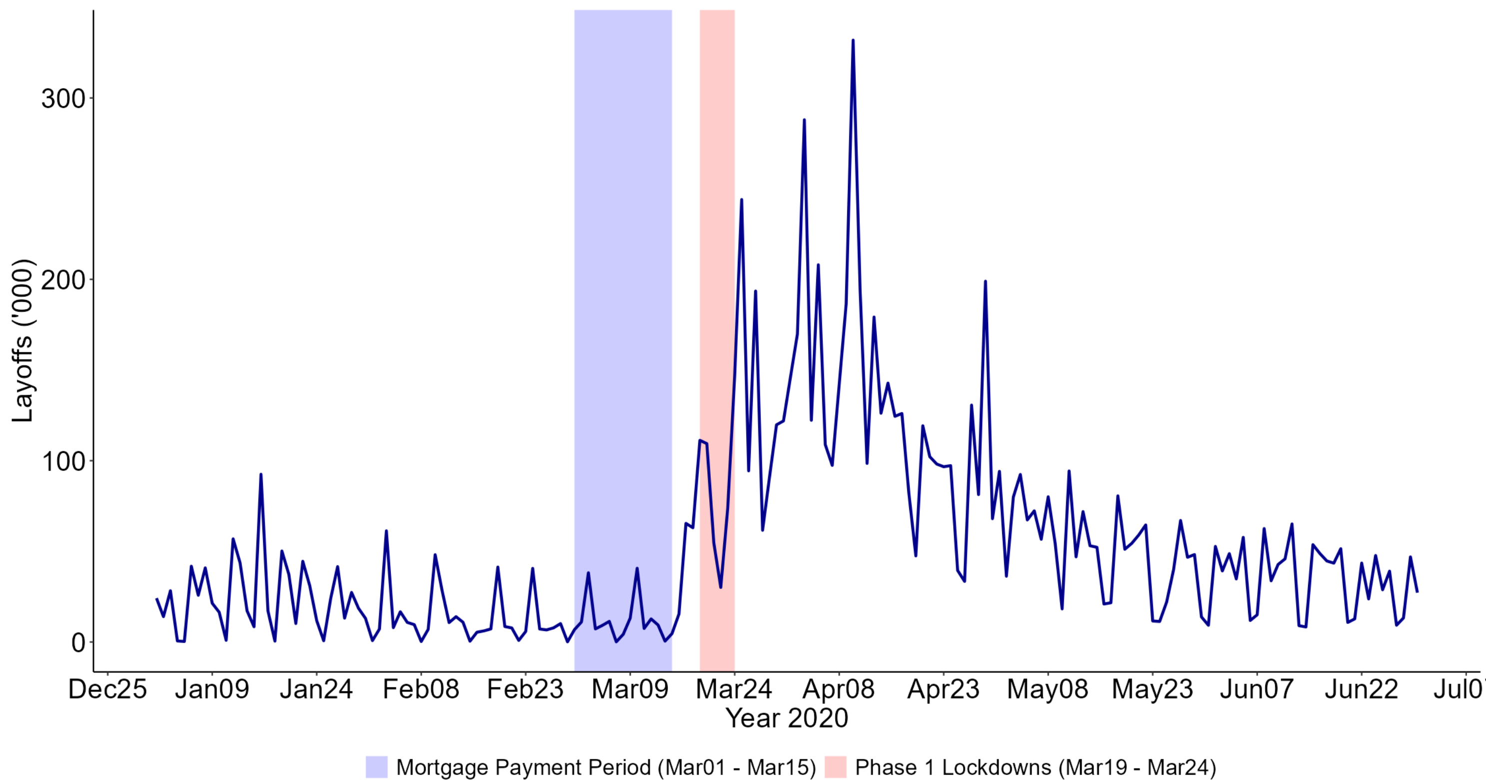

*Notes*: This diagram illustrates the key stages in the foreclosure process, which can be broadly categorized into two phases. The pre-foreclosure phase initiates with the first missed payment and concludes with foreclosure referral or filing. During this phase, the servicer is required to wait until 120 days have passed before they can legally initiate the foreclosure proceedings. On the other hand, the in-foreclosure phase begins with the foreclosure filing and concludes with the eviction of the homeowner. In states where judicial execution of foreclosures is required, there is a step for judicial review within the in-foreclosure proceedings. In contrast, in non-judicial or power-of-sale states, the servicer can proceed directly to the property sale stage without the need for judicial review.

Figure 1.2: Policy Timeline



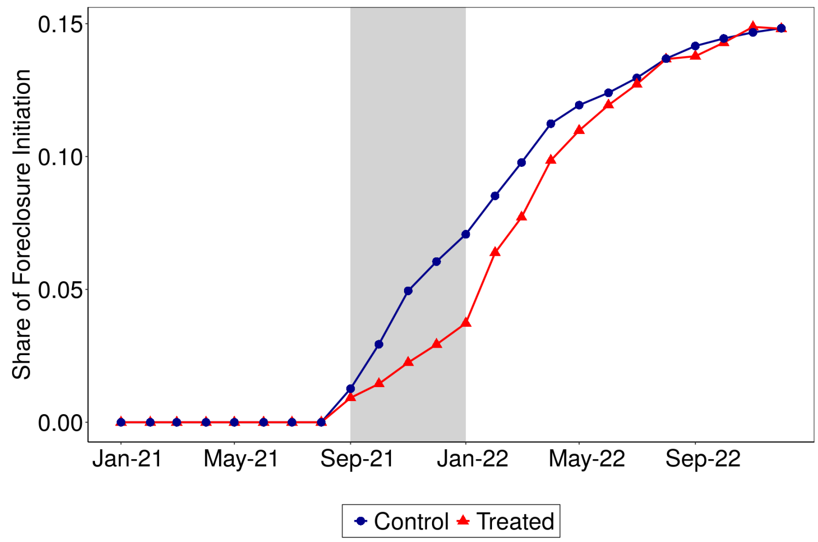

*Notes*: This diagram spresents a timeline of events related to the Consumer Financial Protection Bureau (CFPB) policy. The CFPB announced amendments to foreclosure proceedings on June 28, 2021. These changes were implemented and took effect between September and December 2021. Eligibility for these changes was determined based on the delinquency status of the mortgages as of March 1, 2020. The treated group in this context comprises mortgages that missed their fifth payment, meaning they transitioned from being 120 days past due to 120+ days past due in March 2020. In contrast, the control group includes mortgages for which this transition occurred in February 2020.

Figure 1.3: Time Series of Layoffs

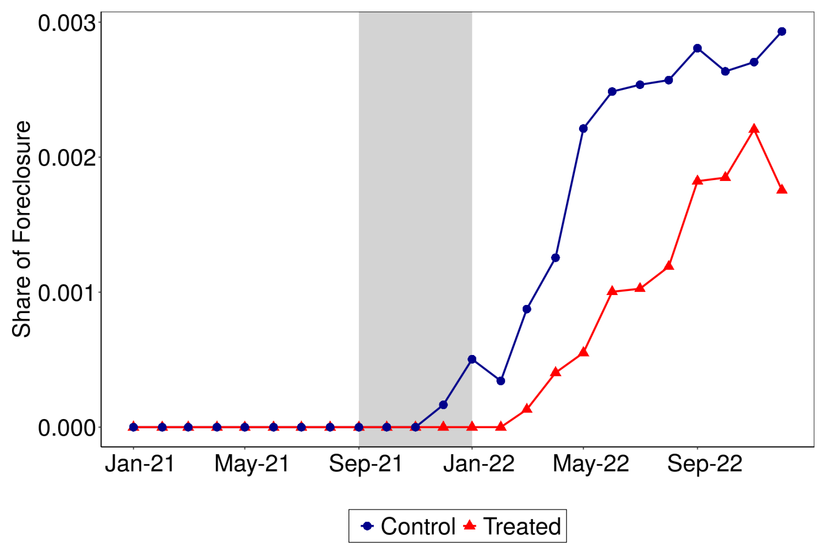



*Notes*: This figure displays the time series of layoffs in 2020, utilizing daily separations data from Equifax Inc. The purple shaded area represents the days falling between March 1st and the $15^{th}$. This period coincides with the typical due dates for mortgage payments, including the grace period allowed by servicers. The pink shaded region corresponds to the time span between March $19^{th}$ and the $24^{th}$. This period marks the announcement of the first round of lockdowns in some states within the United States.

Figure 1.4: First Stage Relevance of Policy: Share of Foreclosure Referral and Foreclosure



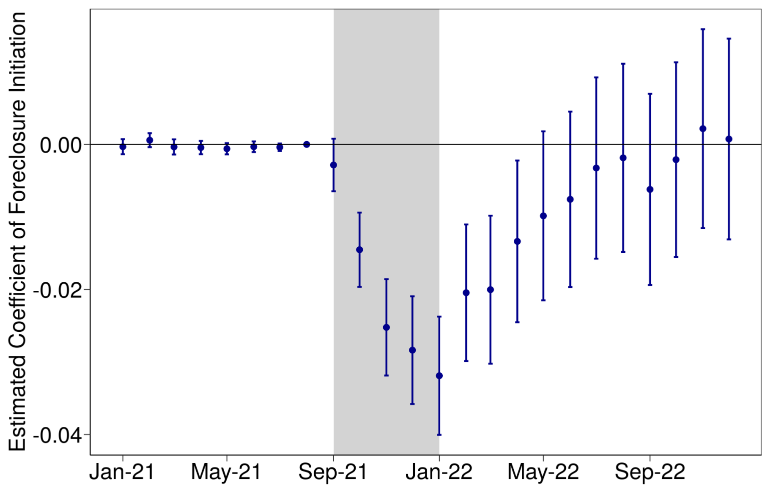

(a) Foreclosure Referral



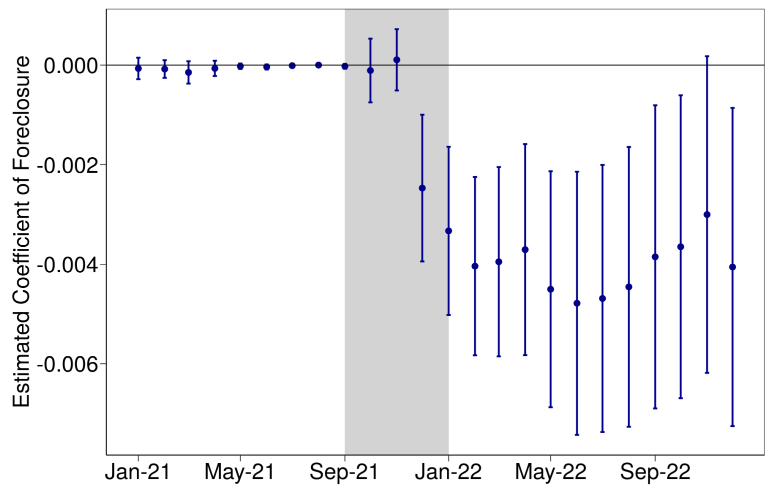

(b) Foreclosure

*Notes*: The figure presents the evaluation of the first-stage relevance of the policy, showing the time series of cumulative share of foreclosure referrals in panel (a) and foreclosure in panel (b) between January 2021 and December 2022 for my sample. The red line denoted as '*Treated*' plots the trends for mortgages that entered 120+ days of delinquency in March 2020. The blue line plots the trends for the *control* group, i.e., mortgages that transitioned to 120+ days delinquency in February 2020.

Figure 1.5: First Stage Relevance of Policy: Effect of CFPB Policy on Foreclosure Referral and Foreclosures




(a) Foreclosure Referral



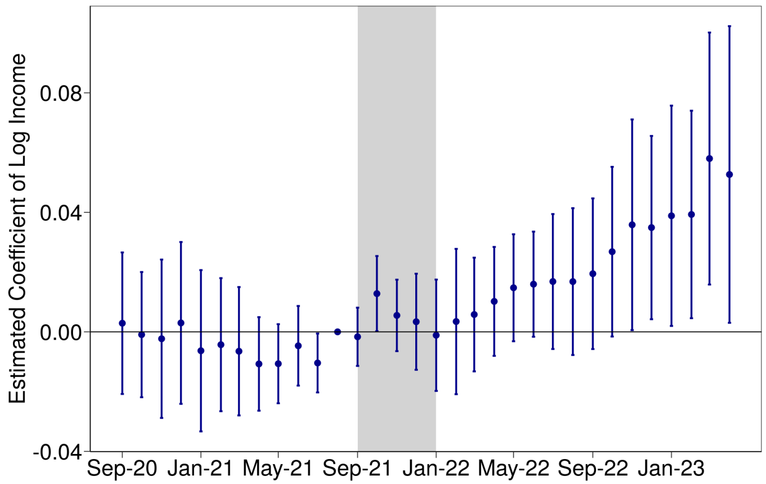

(b) Foreclosure

*Notes*: This figure plots the $\beta_k$ coefficients along with the corresponding 95% confidence intervals from

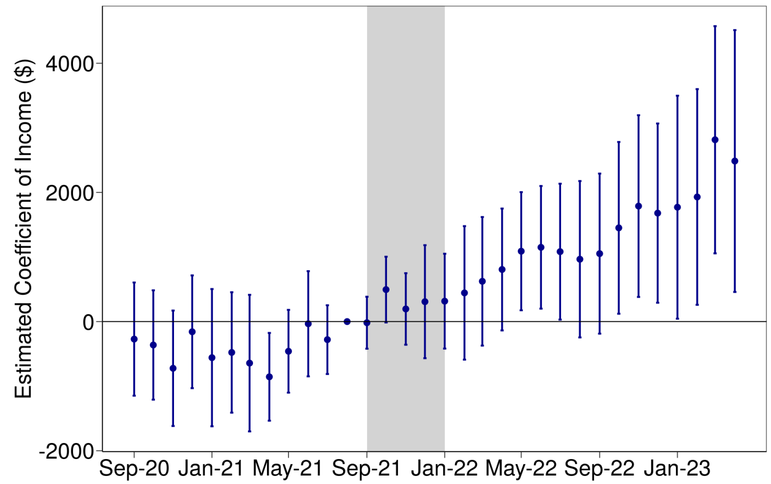
$$y_{i,j,z,c,s,t} = \sum_{\substack{k=Jan'21 \\ k\neq Aug'21}}^{Dec'22} \beta_k \times Treated_{i,j,z,c,s} \times D_k \ + \theta_i + \gamma_{z,t} + \delta_{j,t} + \omega_{c,t} + \phi_{s,t} + \epsilon_{i,j,z,c,s,t}$$

which captures the differential change in the likelihood of foreclosure referrals (panel a) and foreclosures (panel b) in the months around the treatment between the treated and control group of mortgagors. $y$ is an indicator variable coded as 1 if individual $i$'s loan, serviced by lender $j$, originated in year-month $c$ in zipcode $z$ with credit score $s$ contains a flag for foreclosure in calender month $t$ and 0 otherwise. $Treated$ is a binary variable that takes a value of 1 if the individual's mortgage loan became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $\theta_i$ denotes loan fixed effects, $\gamma_{z,t}$ and $\delta_{j,t}$ indicates zipcode x year-month and lender x year-month fixed effects respectively. $\omega_{c,t}$ refers to origination cohort specific effects and $\phi_{s,t}$ represents credit score quartiles-time effects. Score, lender, industry, and zip code in the fixed effects are measured in the period prior to treatment. The sample time period is between September 2020 to December 2022. Standard errors are robust to heteroskedasticity and are clustered at the loan level.

Figure 1.6: Dynamic Treatment Effects: Effect of CFPB Policy on Labor Income




(a) Log Income



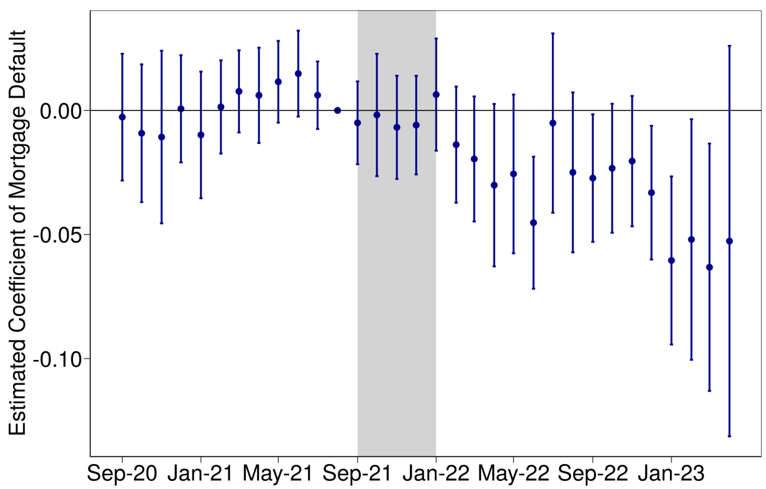

(b) Dollar Income

*Notes*: This figure plots the $\beta_k$ coefficients along with the corresponding 95% confidence intervals from the following equation:

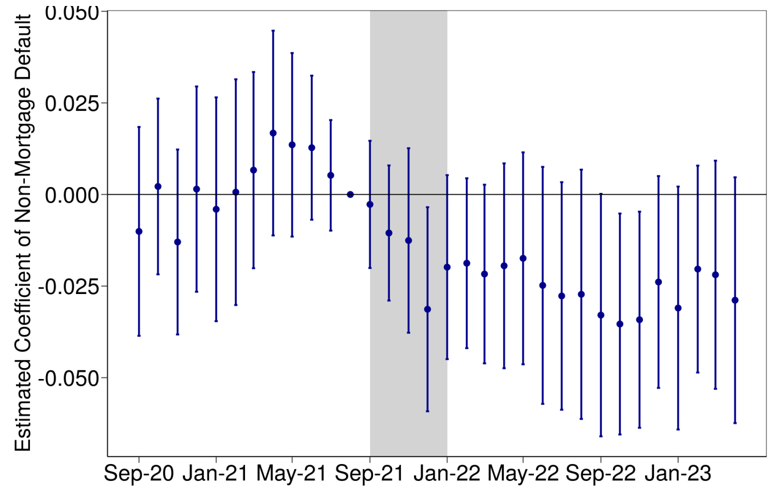
$$y_{i,z,h,t} = \sum_{\substack{k=Sep'20 \\ k\neq Aug'21}}^{Apr'23} \beta_k \times Treated_{i,z,h,t} \times D_k + \theta_i + \gamma_{z,t} + \delta_{h,t} + \epsilon_{i,z,h,t}$$

which captures the differential change in log earnings in the months around the treatment between the treated and control group of mortgagors. $y_{i,z,h,t}$ represents log earnings or dollar earnings for individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $D_k$ is an indicator that equals one for observations corresponding to individual $i$ when the observation belongs to month $k$. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects and $\delta_{h,t}$ indicates industry x year-month fixed effects. Zipcode and industry are measured in the period prior to treatment. The sample time period is between September 2020 to April 2023. Standard errors are robust to heteroskedasticity and are clustered at the six-digit NAICS code level.

Figure 1.7: Dynamic Treatment Effects: Effect of CFPB Policy on Likelihood of Default




(a) Mortgage Default




(b) Non Mortgage Default

*Notes*: This figure plots the $\beta_k$ coefficients along with the corresponding 95% confidence intervals from the following equation:

$$y_{i,z,h,t} = \sum_{\substack{k=Sep'20 \\ k \neq Aug'21}}^{Apr'23} \beta_k \times Treated_{i,z,h,t} \times D_k + \theta_i + \gamma_{z,t} + \delta_{h,t} + \epsilon_{i,z,h,t}$$

which captures the differential change in the likelihood of default in the months around the treatment between the treated and control group of mortgagors. $y_{i,z,h,t}$ represents log earnings or dollar earnings for individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+DPD in March 2020 and 0 if the loan became 120+DPD in February 2020. $D_k$ is an indicator that equals one for observations corresponding to individual $i$ when the observation belongs to month $k$. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects and $\delta_{h,t}$ indicates industry x year-month fixed effects. Zipcode and industry are measured in the period prior to treatment. The sample time period is between September 2020 to April 2023. Standard errors are robust to heteroskedasticity and are clustered at the six-digit NAICS code level.

Table 1.1: Descriptive Statistics

This table summarizes the main variables in my analysis. The sample period ranges from September 2020 to March 2023.

| Statistic | N | Mean | St. Dev. | p25 | Median | p75 |
|---|---|---|---|---|---|---|
| Loan Amount ($) | 164,000 | 179,074 | 139,986 | 89,250 | 151,304 | 235,653 |
| Loan Term | 164,000 | 363 | 114 | 360 | 360 | 360 |
| Loan Balance ($) | 164,000 | 154,014 | 141,977 | 53,968 | 129,459 | 217,734 |
| Loan Payment ($) | 164,000 | 840 | 1,423 | 0 | 336 | 1336 |
| Credit Score | 164,000 | 557 | 73 | 511 | 554 | 599 |
| Credit Utilisation (%) | 164,000 | 57.63 | 40.08 | 15.57 | 64.11 | 100 |
| # of Credit Cards | 164,000 | 4.5 | 6 | 0 | 3 | 7 |
| Total Debt Payment ($) | 164,000 | 1,191 | 1,677 | 0 | 801 | 1,820 |
| Modification (%) | 164,000 | 19.41 | 39.55 | 0 | 0 | 0 |
| Term Modifications (%) | 164,000 | 5.67 | 23.13 | 0 | 0 | 0 |
| Balance Modifications (%) | 164,000 | 5.62 | 23.03 | 0 | 0 | 0 |
| Delinquency Non-Mortgage (%) | 164,000 | 17 | 37.56 | 0 | 0 | 0 |
| Annual Income ($) | 164,000 | 61,815 | 37,009 | 35,206 | 54,575 | 81,376 |
| % Commission | 164,000 | 0.73 | 5.4 | 0 | 0 | 0 |
| Hourly Wage ($) | 109,077 | 24.52 | 12.36 | 16.50 | 21 | 28.93 |
| Hours Worked | 115,077 | 51.31 | 25.06 | 40 | 40 | 76 |
| Change Employer (%) | 164,000 | 2.245 | 14.81 | 0 | 0 | 0 |
| Change Work-zip (%) | 164,000 | 0.81 | 8.93 | 0 | 0 | 0 |
| Imputed LTV | 66,100 | 0.44 | 0.18 | 0.35 | 0.46 | 0.56 |

Table 1.2: Systematic Differences across Treatment and Control

This table compares the key metrics across treatment and control groups for my sample. *Treated* are the group of mortgagors whose loans became 120+days delinquent in March 2020 and *Control* are those whose loans became 120+days delinquent in February 2020. For comparison of the treatment and control groups I use the data for February 2020.

| | Treated<br>(1) | Control<br>(2) | Difference<br>(1) - (2) | t-stat<br>(3) |
|---|---|---|---|---|
| Annual Earnings ($) | 62,967 | 63,036 | -69.17 | -0.041 |
| Commission Share (%) | 0.654 | 0.806 | -0.152 | -0.991 |
| Hourly wage ($) | 23.623 | 23.431 | 0.192 | 0.488 |
| Hours Worked | 52.540 | 51.611 | 0.929 | 1.140 |
| Change Employer (%) | 1.429 | 1.487 | -0.059 | -0.176 |
| Change Work Zip (%) | 0.700 | 0.634 | 0.067 | 0.282 |
| Credit Score | 551 | 549 | 2 | 0.900 |
| Debt-to-Income (DTI) | 0.454 | 0.463 | -0.009 | -0.202 |
| Origination Term | 364 | 350 | 14*** | 4.807 |
| Origination Amount ($) | 182,795 | 164,181 | 18,614*** | 5.366 |

Table 1.3: Foreclosure Delays and Labor Income

This table reports the effect of CFPB's amendment to mortgage servicing guidelines regarding foreclosure initiation on individual labor earnings, estimated on an individual-month panel:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h} \times Post_t^{Sep2021} + \theta_i + \gamma_{z,t} + \delta_{h,t} + \eta_{w,t} + \phi_{s,t} + \alpha_{a,t} + \Gamma_{d,t} + \epsilon_{i,z,h,t}$$

where $y$ represents log earnings or dollar earnings for individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $Post^{Sep2021}$ is an indicator that equals one for months September 2021 onwards and 0 otherwise. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects, $\delta_{h,t}$ indicates industry x year-month fixed effects, $\eta_{w,t}$ indicates wage quartile bins, credit score quartile bins-time effects are given by $\phi_{s,t}$, loan size and loan term interacted with month fixed effects are denoted as $\alpha_{a,t}$ and $\Gamma_{d,t}$ respectively. Zipcode, industry, wage bins and credit score are measured as of February 2020. Employer FE in column (6) corresponds to contemporaneous employer. The sample time period is between September 2020 to April 2023. Standard errors are robust to heteroskedasticity and are clustered at the six-digit NAICS code level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Panel A: Log Income | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $Post^{Sep2021} \times Treated$ | 0.025** | 0.023** | 0.020** | 0.021*** | 0.022*** | -0.001 |
| | (0.009) | (0.008) | (0.008) | (0.008) | (0.007) | (0.007) |
| N | 164,000 | 164,000 | 164,000 | 163,610 | 149,392 | 164,000 |
| $R^2$ | 0.850 | 0.871 | 0.874 | 0.875 | 0.880 | 0.954 |
| | Panel B: Dollar Income | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $Post^{Sep2021} \times Treated$ | 1,657.9*** | 1,515.9*** | 1,393.0*** | 1,398.0*** | 1,394.2*** | 337.9 |
| | (458.4) | (426.8) | (422.7) | (413.8) | (404.6) | (345.1) |
| N | 164,000 | 164,000 | 164,000 | 163,610 | 149,392 | 164,000 |
| $R^2$ | 0.895 | 0.912 | 0.914 | 0.915 | 0.918 | 0.961 |
| Individual FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Month FE | Yes | No | No | No | No | No |
| Zipcode × *Month FE* | No | Yes | Yes | Yes | Yes | Yes |
| Industry × *Month FE* | No | No | Yes | Yes | Yes | Yes |
| Wagebin × *Month FE* | No | No | No | Yes | Yes | No |
| Scorebin × *Month FE* | No | No | No | Yes | Yes | No |
| Origination Amount × *Month FE* | No | No | No | No | Yes | No |
| Origination Term × *Month FE* | No | No | No | No | Yes | No |
| Individual FE × *Employer FE* | No | No | No | No | No | Yes |

Table 1.4: Foreclosure Delays and Job Mobility

This table reports the effect of CFPB's amendment to mortgage servicing guidelines regarding foreclosure initiation on the likelihood of switching job, estimated on an individual-month panel:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h} \times Post_t^{Sep2021} + \theta_i + \gamma_{z,t} + \delta_{h,t} + \epsilon_{i,z,h,t}$$

where $y$ represents an indicator coded as 100 if an individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$ switches employer relative to $t-1$ in Panel A and changes employment zip code in Panel B respectively. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $Post^{Sep2021}$ is an indicator that equals one for months September 2021 onwards and 0 otherwise. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects, $\delta_{h,t}$ indicates industry x year-month fixed effects. Zipcode and industry are measured prior to treatment. The sample time period is between September 2020 to April 2023. The sample consists of individuals emploed as of August 2021. Standard errors are robust to heteroskedasticity and are clustered at the six-digit NAICS code level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Panel A: Change Employer | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| $Post^{Sep2021} \times Treated$ | 0.306* | 0.292** | 0.268* |
| | (0.163) | (0.139) | (0.154) |
| N | 135,105 | 135,105 | 135,105 |
| $R^2$ | 0.085 | 0.264 | 0.274 |
| | Panel B: Change Employment Zip code | | |
| | (1) | (2) | (3) |
| $Post^{Sep2021} \times Treated$ | 0.218** | 0.285*** | 0.231** |
| | (0.109) | (0.102) | (0.101) |
| N | 135,105 | 135,105 | 135,105 |
| $R^2$ | 0.061 | 0.238 | 0.266 |
| Individual FE | Yes | Yes | Yes |
| Month FE | Yes | No | No |
| Zipcode $\times$ $Month\ FE$ | No | Yes | Yes |
| Industry $\times$ $Month\ FE$ | No | No | Yes |

Table 1.5: Heterogeneity by Employment Status

This table reports the effect of CFPB's amendment to mortgage servicing guidelines regarding foreclosure proceeding on individual earnings by employment status as of August 2021, estimated on an individual-month panel:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h,t} \times Post_t + \theta_i + \gamma_{z,t} + \delta_{h,t} + \eta_{w,t} + \phi_{s,t} + \epsilon_{i,z,h,t}$$

where $y$ represents log earnings or dollar earnings for individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+DPD in March 2020 and 0 if the loan became 120+DPD in February 2020. $Post$ is an indicator that equals one for months September 2021 onwards and 0 otherwise. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects, $\delta_{h,t}$ indicates industry x year-month fixed effects. Zipcode, industry are measured prior to treatment. Columns (1)-(2) report the treatment effects for individuals employed as of August 2021; columns (3)-(4) corresponds to the sample of individuals with no active employment as of 2021 August (i.e., unemployed). The sample time period is between September 2020 to April 2023 and is restricted to loans not subject to foreclosure and reported as in default as of August 2021. Standard errors are robust to heteroskedasticity and are clustered at the six-digit NAICS code level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Unemployed | | Employed | |
|---|---|---|---|---|
| | Log Income | Dollar Income | Log Income | Dollar Income |
| | (1) | (2) | (3) | (4) |
| $Post \times Treated$ | 0.087** | 3431.7* | 0.015* | 1105.7** |
| | (0.039) | (1741.7) | (0.006) | (341.3) |
| N | 28,895 | 28,895 | 135,105 | 135,105 |
| $R^2$ | 0.867 | 0.914 | 0.958 | 0.967 |
| Individual FE | Yes | Yes | Yes | Yes |
| Zipcode × *Month FE* | Yes | Yes | Yes | Yes |
| Industry × *Month FE* | Yes | Yes | Yes | Yes |

Table 1.6: Foreclosure Delays and Job Match Quality

This table reports the effect of CFPB's amendment to mortgage servicing guidelines regarding foreclosure initiation on individual's variable compensation, estimated on an individual-month panel:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h} \times Post_t^{Sep2021} + \theta_i + \gamma_{z,t} + \delta_{h,t} + \epsilon_{i,z,h,t}$$

where $y_{i,z,h,t}$ measures for individual $i$ residing in zipcode $z$ employed in industry $h$ in year-month $t$, hours worked in Column (1), log hourly wage in Column (2) and commission as a percentage of total compensation in Column (3). $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $Post^{Sep2021}$ is an indicator variable equal to 1 for period September 2021 and after and 0 for the months before that. The coefficient $\beta_{DD}$ represents the change in the outcome variable in the months around treatment conditional on $\theta_i$ i.e., individual fixed effects, $\gamma_{z,t}$ for zipcode x year-month fixed effects, $\delta_{h,t}$ indicating industry x year-month fixed effects. Columns (4)-(2) are restricted to hourly wage workers only. The sample time period is between September 2020 to April 2023. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the six-digit NAICS code level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | % of Commission | Log Tenure | Full-Time | $\#Jobs_{>1}$ | Hours Worked | Log Hourly Wage |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $Post^{Sep2021} \times Treated$ | 0.182* | 0.021** | 0.024*** | -0.011* | 1.30*** | 0.017*** |
| | (0.102) | (0.010) | (0.008) | (0.009) | (0.403) | (0.006) |
| N | 164,000 | 164,000 | 164,000 | 164,000 | 109,093 | 115,077 |
| $R^2$ | 0.829 | 0.838 | 0.796 | 0.681 | 0.801 | 0.768 |
| Individual FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Zipcode $\times$ $Month\ FE$ | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry $\times$ $Month\ FE$ | Yes | Yes | Yes | Yes | Yes | Yes |

Table 1.7: Heterogeneity by Liquidity Measures

This table reports the effect of CFPB's amendment to mortgage servicing guidelines regarding foreclosure proceeding on individual earnings by ex-ante liquidity, estimate on an individual-month panel:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h} \times Post_t^{Sep2021} + \theta_i + \gamma_{z,t} + \delta_{h,t} + \epsilon_{i,z,h,t}$$

where $y_{i,z,h,t}$ represents log earnings for individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $Post^{Sep2021}$ is an indicator variable equal to 1 for period September 2021 and after and 0 for the months before that. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects, $\delta_{h,t}$ indicates industry x year-month fixed effects. Regression coefficients for sub-samples based on above (below) median credit utilisation are shown in Columns (1)-(2), median mortgage payments as a fraction of income, measured as of August 2021 in columns (3)-(4) and median debt-to-income ratio in columns (5)-(6). Finally columns (7)-(8) report coefficients by credit score below and above 620. The sample time period is between September 2020 to April 2023. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the six-digit NAICS code level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Income | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Credit Utilisation | | Payment/Income | | Debt/Income | | Credit Score | |
| | Above Median | Below Median | Above Median | Below Median | Above Median | Below Median | <620 | ≥620 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| $Post^{Sep2021} \times Treated$ | 0.044*** | 0.004 | 0.024** | 0.010 | 0.021** | 0.005 | 0.020** | -0.009 |
| | (0.013) | (0.015) | (0.010) | (0.020) | (0.011) | (0.011) | (0.008) | (0.025) |
| N | 83,167 | 77,655 | 92,550 | 52,499 | 76,392 | 68,658 | 136,635 | 26,975 |
| $R^2$ | 0.886 | 0.893 | 0.877 | 0.885 | 0.892 | 0.891 | 0.875 | 0.929 |
| Individual FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Zipcode × $Month\ FE$ | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry× $Month\ FE$ | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Table 1.8: Heterogeneity by Labor Market Tightness

This table reports the heterogeneous effect of CFPB's amendment to mortgage servicing guidelines regarding foreclosure proceeding on individual earnings by labor market tightness:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h} \times Post_t + \theta_i + \gamma_{z,t} + \delta_{h,t} + \epsilon_{i,z,h,t}$$

where $y$ represents log earnings for individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+DPD in March 2020 and 0 if the loan became 120+DPD in February 2020. $Post$ is an indicator that equals one for months September 2021 onwards and 0 otherwise. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects, $\delta_{h,t}$ indicates industry x year-month fixed effects. Zipcode and industry are measured prior to treatment. Labor market tightness is computed at the industry level as the ratio of vacancies by unemployment using data from Bureau of Labor Statistics. $Tight$ ($Slack$) corresponds to the subsample of individuals in industries with above (below) median value of tightness. Columns (1)-(2) include the entire sample. Columns (3)-(6) condition upon ex-ante industries facing slack labor markets and then split based on median value of proxies for how liquidity constrained individuals are. The sample time period is between September 2020 to April 2023 and is restricted to loans not subject to foreclosure and reported as in default as of August 2021. Standard errors are robust to heteroskedasticity and are clustered at the six-digit NAICS code level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | | | |
|---|---|---|---|---|---|---|
| | Slack | Tight | Conditional upon slackness | | | |
| | | | Debt/Income | | Credit Utilisation | |
| | | | Above Median | Below Median | Above Median | Below Median |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $Post \times Treated$ | 0.032** | 0.019 | 0.044*** | 0.013 | 0.037* | 0.015 |
| | (0.012) | (0.014) | (0.012) | (0.031) | (0.021) | (0.025) |
| N | 82,911 | 81,051 | 47,200 | 35,711 | 43,236 | 39,675 |
| $R^2$ | 0.890 | 0.888 | 0.892 | 0.924 | 0.905 | 0.909 |
| Individual FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Zipcode $\times$ $Month\ FE$ | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry $\times$ $Month\ FE$ | Yes | Yes | Yes | Yes | Yes | Yes |

Table 1.9: Heterogeneity by Judicial Foreclosure Laws

This table reports the heterogeneous effect of CFPB's amendment to mortgage servicing guidelines regarding foreclosure proceeding on individual earnings by whether mortgages were originated in judicial states:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h} \times Post_t + \theta_i + \gamma_{z,t} + \delta_{h,t} + \epsilon_{i,z,h,t}$$

where $y$ represents log earnings for individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+DPD in March 2020 and 0 if the loan became 120+DPD in February 2020. $Post$ is an indicator that equals one for months September 2021 onwards and 0 otherwise. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects, $\delta_{h,t}$ indicates industry x year-month fixed effects. Zipcode and industry are measured prior to treatment. The sample time period is between September 2020 to April 2023 and is restricted to loans not subject to foreclosure and reported as in default as of August 2021. Standard errors are robust to heteroskedasticity and are clustered at the six-digit NAICS code level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | |
|---|---|---|
| | Non-Judicial<br>(1) | Judicial<br>(2) |
| $Post \times Treated$ | 0.030** | 0.018 |
| | (0.013) | (0.012) |
| N | 86,832 | 77,168 |
| $R^2$ | 0.851 | 0.847 |
| Individual FE | Yes | Yes |
| Zipcode $\times$ $Month\ FE$ | Yes | Yes |

Table 1.10: Heterogeneity by Home Equity and State Laws

This table reports heterogeneity in the effect of CFPB's amendment regarding foreclosure initiation on individual earnings by a combination of state creditor laws and home equity, estimated on an individual-month panel:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h} \times Post_t^{Sep2021} + \theta_i + \gamma_{z,t} + \delta_{h,t} + \epsilon_{i,z,h,t}$$

where $y$ measures log earnings for individual $i$ residing in zipcode $z$ employed in industry $h$ changes employer in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $Post^{Sep2021}$ is an indicator variable equal to 1 for period September 2021 and after and 0 for the months before that. Column (1) ((2)) corresponds to above (below) median LTV individuals across all panels. Panel A-C reports results for sub-samples based on recourse laws, restrictions on debt collectors and wage garnishments respectively. The sample time period is between September 2020 to April 2023. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the six-digit NAICS code level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Income | |
|---|---|---|
| | (1) | (2) |
| | High LTV | Low LTV |
| Panel A: Recourse | Yes | No |
| $Post^{Sep2021} \times Treated$ | 0.029** | -0.031 |
| | (0.016) | (0.027) |
| N | 44,367 | 15,076 |
| $R^2$ | 0.886 | 0.906 |
| Panel B: Debt Collection Restrictions | Lax | Strict |
| $Post^{Sep2021} \times Treated$ | 0.037** | -0.006 |
| | (0.018) | (0.018) |
| N | 37,923 | 30,329 |
| $R^2$ | 0.907 | 0.913 |
| Panel C: Wage Garnishment Restrictions | Lax | Strict |
| $Post^{Sep2021} \times Treated$ | 0.023* | -0.017 |
| | (0.017) | (0.028) |
| N | 50,558 | 22,325 |
| $R^2$ | 0.892 | 0.906 |
| Individual FE | Yes | Yes |
| Zipcode $\times$ $Month\ FE$ | Yes | Yes |
| Industry$\times$ $Month\ FE$ | Yes | Yes |

Table 1.11: Foreclosure Delays and Default

This table reports the effect of of CFPB's amendment to mortgage servicing guidelines regarding foreclosure proceedings on loan performance, estimated on an individual-month panel:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h,t} \times Post_t^{Sep2021} + \theta_i + \gamma_{z,t} + \delta_{h,t} + \epsilon_{i,z,h,t}$$

where $y$ is an indicator variable coded as 1 if individual $i$ residing in zipcode $z$ employed in industry $h$ is reported as 60+ days delinquency on mortgage debt (column 1) and non mortgage debt (column 2). $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $Post$ is an indicator variable equal to 1 for period September 2021 and after and 0 for the months before that. The coefficient $\beta_{DD}$ represents the change in the outcome variable in the months around treatment, conditional on $\theta_i$ i.e., individual fixed effects, $\gamma_{z,t}$ for zipcode x year-month fixed effects and $\delta_{h,t}$ indicating industry x year-month fixed effects. The sample time period is between September 2020 to April 2023. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the six-digit NAICS code level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Default | |
|---|---|---|
| | Mortgage Debt | Non-Mortgage Debt |
| | (1) | (2) |
| Post$^{Sep2021}\times$ $Treated$ | -0.024** | -0.017** |
| | (0.010) | (0.008) |
| N | 157,951 | 125,825 |
| $R^2$ | 0.631 | 0.690 |
| Individual FE | Yes | Yes |
| Zip FE $\times$ $Month\ FE$ | Yes | Yes |
| Industry FE $\times$ $Month\ FE$ | Yes | Yes |

# Chapter 2

# Households' Ability to Weather Adverse Shocks: Role of Firm Monopsonies

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## Abstract

Using new employer-employee matched data, I investigate the role of firm monopsony power on individual's ability to weather adverse shocks. I document that plausibly exogenous variation in gasoline prices following the Russia-Ukraine conflict amplifies household financial distress by increasing credit card default and revolving debt burden for individuals with limited access to public transportation and longer commutes to work. These effects are pronounced for lowest-income individuals. Employer monopsony power in local labor markets deteriorates workers' ability to negotiate better wages to mitigate the impact of the shock on their financial situation. To this extent, I find that price shocks lead to greater financial fragility for individuals employed in firms with a high employment market share and voluntary quit rate within a commuting zone. These effects manifest from lack of adjusment of overall compensation and in particular variable pay around the price shock. Overall, my results highlight the importance of firms in pass-through of macro-shocks on financial instability through household balance-sheets.

*Keywords:* Russia-Ukraine war, price shock,gasoline price, oil price, public transit, commuting, monopsony power, financial stability, credit card debt, default

"*If inflation and price instability prevail, so will financial instability*" —Schwartz, 1998

## 2.1 Introduction

Income adjustment is one the primary channels through which households can maintain their purchasing power in times of adverse shocks. The alternative is to take on unsecured debt which is costly and supposedly worse than being able to bargain higher wages if possible[13]. There is ample research highlighting the role of access to credit in households' ability to mitigate financial distress or lack thereof making them fare relatively worse (Fos et al., 2019, Morse, 2011). However, little is known about the role of local labor markets in alleviating household financial distress. With the increasing shift in US labor markets towards becoming more concentrated and deteriorating worker bargaining capacity due to role of firms in wage setting, firm monopsony power may infact exacerbate household financial distress. Specifically, does the ability to bargain higher wages matter in mitigating the effect of adverse shocks for households?

A significant adverse shock crippling households is that of rising prices, especially when it comes to essential goods and services. A sudden and unexpected increase in gasoline prices for example — a non-discretionary expense constituting a substantial fraction of consumption basket for many individuals — may leave individuals less cash to afford other expenses such as debt repayment obligations. Without commensurate wage adjustment or benefits, this can lead to delinquencies on unsecured credit[14]. In this paper, I employ anonymized detailed credit and payroll data on US households from Equifax Inc. to investigate how inflation induced by energy price shocks impacts individual financial distress as measured by their default likelihood on unsecured debt. In doing so, I assess the extent to which firm

[13]United States works very much on a fee-based system for delinquencies such that once individuals' get into distress, they very much end up in a cycle of debt.

[14]A study by researchers as University of Sydney shows that consumers are nearly four times as likely to default on their credit card, compared with their personal loans. Furthermore, Andersson et al., 2013 finds that consumers are eight times more likely to prioritize payments on mortgage debt over credit card payments during crisis

monoposony power affects the pass-through of inflation through household financial distress to aggregate financial instability.

The main mechanism through which rising consumer prices can affect consumer default is through nominal wage adjustment. To the extent that rise in energy price may disproportionately affect the poorer segment of the income distribution and that minimum wage-low skilled jobs generally see wage adjustment during inflation, there should be no effect of inflation on household distress. However, in markets that are imperfectly competitive, a small number of employers may negotiate with workers, leading to sluggish wage changes. This situation can disproportionately disadvantage individuals employed with firms that have substantial monopsony power, as their inability to negotiate wages will result in a steeper purchasing power deterioration in the face of rising consumer prices. Consequently, they may need to allocate a larger portion of their budget to non-discretionary spending, which could reduce their ability to pay off debts and lead to financial distress. Thus, differential worker bargaining power in the face of firm monoposony power in the labor market may play a significant role in determining individual purchasing power during inflationary periods.

Measuring the impact of inflation on household financial choices poses a considerable empirical challenge because of the extensive diversity in household spending habits and the fact that consumer prices are closely linked to household decision-making[15]. To address these obstacles, my research design exploits inflationary pressures arising from plausibly exogenous energy price shocks to isolate the cause-and-effect relationship between rising prices and the individual propensity to default. In particular, my empirical methodology estimates a triple difference-in-differences specification. The first difference captures changes in household financial distress following the Russia-Ukraine conflict, which led to a steep rise in crude oil prices and a subsequent increase in gasoline prices. The second difference uses variation due to zipcode level local coverage of public transportation systems. Access to an extensive public transit network should technically reduce the dependency on personal vehicles for daily commuting, leaving people in such areas less exposed to high gasoline prices. [16]. Finally, the third

[15]In the United States, the Consumer Price Inflation (CPI) Index measures the average change over time in the prices that urban consumers pay for a range of goods and services. It encompasses over 200 categories of expenditure items, grouped into eight major categories (including food and beverages, housing, apparel, energy transportation, medical care, recreation, education and communication, and other goods and services)

[16]According to the American Public Transportation Association, 45% of Americans have no access to public transportation. A study by the Urban Institute shows that buses and trains travel a modest 145 feet per resident per day in the typical American urban area, but they run almost five times as much per resident in the New York City region, where transit is frequent and convenient. In the New York City region, the

difference exploits variation from individuals' work commuting distance within a zipcode. Specifically, those who travel farther to work are likely to be more exposed to gas price changes, resulting in significant changes to their monthly expenditures. This addresses concerns regarding the potential correlation between availability of public transportation in a region and the level of economic growth and financial well-being of households in that area[17]. Combining these three sources of variation, my empirical strategy relies on the identifying assumption that in the absence of the Russian-Ukraine war (and the associated shock to gasoline prices), the financial health of individuals with high work-commuting distance in regions with limited public transport access would follow similar trends as those commuting shorter distance within the same region.

I begin my analysis by focusing on changes in credit card performance around the gas price shock as a measure of financial distress. I find a statistically significant increase in the likelihood of default, ranging from 0.654% to 0.778%, depending on whether default transition is defined as 90+DPD or 60+DPD, respectively for individuals with long work commutes in low transit zip codes, relative to those with shorter commutes in the same neighborhoods. These estimates are economically significant, ranging between 7% and 11% relative to their respective sample averages.

Furthermore, I find that the credit card loan balance for individuals with longer commutes in areas with limited access to public transit increased by an average of $120 per month following the gas price shock, which translates into an annual increase in outstanding debt of $1,440. Based on these findings, it appears that the financial well-being of individuals suffered in terms of their credit card loan performance. This suggests that the source of deterioration in loan performance is the propensity to borrowing more and accumulate larger outstanding balances to fund both increased gasoline consumption and other expenses from increasing gas prices. To contextualize this result, it's worth noting that during the period around the war, the median gas price increased by as much as $2 for a typical US zipcode. Given that the median US household commutes 25,000 vehicle miles per year, with a gasoline consumption of about 961 gallons per year and 80 gallons per month (assuming a mileage of 26 miles per gallon), the median household would have had to spend an additional $160 per

average urban resident takes 224 transit trips annually. In the Cincinnati region, where buses are infrequent and don't serve many neighborhoods, the average resident takes fewer than 11 transit trips a year.

[17]Relying solely on access to public transportation as a measure of exposure to gasoline price changes may underestimate the true level of exposure, as some individuals with access may still opt to use personal vehicles.

month on gas after the war-induced price hike. This would have doubled the share of their monthly budget spent on gas constituting a significant proportion of their monthly income.

To understand the implications of my findings on the overall US credit card debt, I conduct some back-of-the-envelope calculations. Prior to the shock, in January 2022, the credit card balance in the US was around $1 trillion aggregated over 200 million people. Using my classification of more vulnerable individuals - those in low public transit zip codes with long average commuting distances - there were approximately 10,000 such zip codes with a combined credit card debt of around $126 billion at the time of the shock. Based on my model's default estimates (between 0.654% and 0.778%), a gasoline price shock could cause between $824 million and $980 million of credit card balance to default over the following year. These figures suggest that inflation could have significant economic implications for financial stability through the household channel.

In the cross-section, I find that within the group more exposed to gas price changes, the probability of default is higher for lower-income households, with a 2.3 percentage point increase for the bottom income tercile (annual earnings below $18,000) and a 1.52 percentage point increase for the middle income group (annual earnings between $40,000 and $80,000), relative to the top income tercile. This is consistent with the fact that lower and middle-income households devote a relatively larger fraction of their annual expenditure to motor fuel and vehicles compared to higher-income households[18].

One potential concern in my study is that individuals with greater exposure to gas price shocks, such as those living in low transit zipcodes and having a long work commuting distance, may not be randomly assigned and therefore could differ systematically. If this were the case, any unobserved variation correlated with the decision to live in a certain region with a particular degree of transit network and commute a specific distance to work would have to affect the more exposed group differently and systematically than the less exposed group around the gasoline price shock. Although it may be difficult to argue such unobservables, I take several steps to address these concerns. First, I include a robust set of fixed effects in my analysis to control for time-invariant individual characteristics and local time-varying economic shocks. These include individual fixed effects and zip x time fixed effects, respectively. Additionally, I incorporate non-parametric controls for individual income

[18]Source: Consumer Expenditure Survey 2019 shows that lower and higher-income households tend to spend differently across various categories, such as food, energy, education, health, etc.

and credit score in the form of deciles for each variable. Finally, I show that the likelihood of delinquency evolves similarly for those with greater exposure to the gasoline price shock and those with less exposure in the six months prior to the war. These results provide evidence in support of the parallel trends assumption, which is essential to my identification.

An additional concern may arise regarding the interpretation of my findings in relation to the reduced-form model that I am estimating. Specifically, a crucial assumption in my analysis is that the Russian-Ukraine war mainly affects individual behavior through its impact on gasoline prices. Although my estimation relies on cross-sectional differences in exposure to oil price shocks, I bolster this assumption by presenting evidence on several additional fronts. First, I demonstrate that my primary results are mainly concentrated among individuals residing in regions with above median gasoline prices increases around the war. Additionally, I show that the effects are more pronounced for individuals living in areas with above median levels of pre-war fuel oil demand, vehicle trips per household and ownership of two or more vehicles respectively. These cross-sectional tests provide suggest that the effects I observe can be ascribed to the inflation in consumer prices resulting from the changes in oil prices induced by the war.

My results so far document that rising gasoline prices as a proxy for consumer price inflation increase individual default. Based on theory, these effects likely manifest through the erosion of purchasing power as wages do not rise enough or are slow to adjust. To the extent that local labor market monopsonies are key in firm wage setting, I find that individuals more exposed to gasoline price shock fair worse in terms of their credit performance if they happen to be employed with firms that enjoy large employment market share in their respective local labor market. I measure firm monopsony power across different definitions of what may constitute a 'local labor market' i.e. county, commuting zone or commuting zone-industry and show that the likelihood of default is between 0.8-1.8 percentage points higher when the exposed group of individuals are employed with relatively large employers in the labor market. Furthermore, my results indicate that individuals employed in firms with high monopsony power experience a smaller annualized wage adjustment post-war, ranging from 3.6% to 22.8%. Overall, these findings suggest that the combination of growing labor market monopsonies and declining worker bargaining power exacerbates the negative effects of inflation on household financial health, leading to greater financial fragility.

## 2.2 Related Literature

My paper contributes several strands of literature. A literature talks about wage compression due to increasing presence of firm monopsony power. Studies document the role of local-level labor market monopsonies in influencing firm wage-setting(Benmelech et al., 2022, Azar et al., 2022, Rinz et al., 2018, Manning, 2013). Further studies show that inflation hurts those the most that are unable to switch jobs (Autor et al., 2022) due to compression of wages emanating from firm monopsony power. However the fact that it could be critical for debt repayment and something that the lenders must account for (i.e., local labor market) in assessing the creditworthiness of borrowers is an unexplored territory. Lenders consider income, but how individual bargaining power can be vital in their ability to deal with financial distress and the role of local labor markets in that is an important heterogeneity to examine. My paper suggests that firm monopsony power can affect workers' bargaining power during price shocks, which in turn can impact their ability to negotiate better wages and mitigate the impact of the shock on their financial situation.

The second strand of related literatures studies the effects of negative shocks on consumer debt and delinquency responses. Research by Low, 2022 demonstrates that a range of typically unobserved liquidity shocks, such as health shocks, divorce, increases in required mortgage payments, and other expense shocks, collectively trigger nearly all defaults. Other papers document how income risks (Madeira, 2018, Luzzetti and Neumuller, 2016, Mitman et al., 2015, Nakajima and Ríos-Rull, 2014, Farrell et al., 2018) and labor market shocks(Gerardi et al., 2018, Athreya et al., 2015) increase foreclosures, and consumer debt default during recessions. However, much of the existing research either explores the effects of idiosyncratic shocks such as job loss or macro-shocks related to house price volatility on individual financial distress. In contrast, my paper examines the ability of individuals to mitigate the effects of another extremely important macro-shock i.e., oil price shock and highlights that heterogeneity in the response for similarly liquidity constrained households but with very different wage negotiation avenues.

Another related body of research examines how firms transmit idiosyncratic shocks to their employees, with studies examining the effects of firm-specific productivity shocks (Souchier, 2022, B. Friedrich et al., 2019, Kline et al., 2019), trade shocks (B. U. Friedrich, 2022), and other unforeseen shocks (Garin, Silvério, et al., 2019) on workers' employment stability and compensation. Complementing this literature, there are also studies that investigate how such uninsurable wage risks influence individual financial decisions, including consumption and debt repayment (Di Maggio et al., 2022), and portfolio choices (Fagereng et al., 2018). My paper contributes to this literature by highlighting the broader risk-sharing function of firms i.e., around shocks that may directly affect their employees. By leveraging credit report data that is enhanced with comprehensive wage information, my study investigates the direct influence of oil price shocks on household debt repayment and default probabilities. Specifically, I analyze the extent to which firms' market power affects wage bargaining ability during these macro-shocks.

Finally, my paper speaks to the broader literature on the effect of inflation and inflation expectations which refers to a sustained increase in price on household decisions. Household decisions affected by inflation are consumption (Angelico and Di Giacomo, 2019, borrowing (Zhang, 2022, Malmendier and Nagel, 2016) and savings (Vellekoop and Wiederholt, 2019)

The rest of the paper is organized as follows: Section 2 describes the data, presents summary statistics and illustrates the empirical methodology. Section 3 summarizes the main results, section 4 discusses the mechanism and section 6 provides a brief discussion. Finally, section 7 performs some robustness checks and section 8 concludes.

## 2.3 Data and Empirical Strategy

This section describes the data used in the analyses, discusses my sample, and details my empirical strategy.

### 2.3.1 Data

I obtain consumer credit data from Equifax Inc., which includes details on account types, balances, credit limits, and any missed or delayed payments. I select a random one percent sample of active credit card accounts i.e, an account reported by the creditor within 3 months of the date that the credit records were drawn each quarter during my sample period– September, 2021 to January, 2023. I consolidate the anonymized account-level information into individual-month level, by combining balances, credit limits, and credit performance for all active credit card accounts held by a particular individual during a specific calendar month.

In addition to the credit card data, I augment my analysis with anonymized monthly earnings, job type, job title, employer name, location, and industry information. This data is sourced from the payroll data provided by Equifax Inc., containing anonymized details on employees across more than 5,000 firms in the United States, spanning from 2010 to the present.

Finally, I collect publicly available data from AllTransitTM[19] to obtain a metric of public transit accessibility. This score considers several metrics, such as the number of connections to other routes, the number of jobs accessible within a 30-minute transit ride, and the number of workers who use transit to commute, to rank cities and assign them a score from 1 to 10. I incorporate this transit score to divide individuals in my data into low and high transit accessibility regions.

### 2.3.2 Sample and Summary Statistics

To create my primary sample, I combine these three datasets, resulting in an individual-month panel with credit and employment information covering the period between September 2021 and January 2023, as well as public transit information measured as of December 2021. After merging, my final sample comprises over 650,000 individuals.

In Table 2.1, I present an overview of the primary variables used in my analysis. Across my sample, the likihood of 60+day or 90+ day past due (DPD) statuses are 12% and 11%, respectively. The transit score ranges from 0 to 10, with a median score of 4.7 available for approximately 27,000 U.S. zip codes. Figure 2.1 displays the geographic variation in public transit access based on the CNT-developed transit measure, as described earlier. The bottom $10^{th}$ percentile of my sample has a transit score of 0. There is significant heterogeneity in public transportation coverage, both across and within states, in my sample. In my sample, the median individual works within their residential zip code, and only 30% of my sample reports a commuting distance (calculated as the distance between their home and work zip

[19] https://alltransit.cnt.org/about/. The Center for Neighborhood Technology (CNT) compiles this data using publicly accessible General Feed Specification (GTFS) data for over 677 agencies, and from schedules and route maps for the remaining 225 agencies without GTFS. CNT calculates an *All Transit Performance Score* by analyzing transit stops, routes, and frequency in regions across the United States with scheduled bus, rail, or ferry service and over 100,000 residents.

codes) greater than zero. It is important to note that individuals who report the same zip code for both residence and workplace may still travel a non-zero distance to work. However, my commuting distance measure calculates the distance in miles between the centroids of two zip codes. Thus, within the same zip code, the calculated distance is mechanically equal to zero. While this measurement error is present, it is unlikely to be correlated with factors that differentially affect those more versus less exposed to gas price shocks in my sample around the war conflict. Lastly, I summarize individual characteristics. The median person in my sample has an outstanding credit card balance of $2,215, and holds three unsecured lines of credit with a total credit limit of $15,500. This credit limit is approximately three times their monthly income of $4,600.

### 2.3.3 Empirical Strategy

The goal of the analysis is to test the extent to which ability to bargain wages — affected by employer monopsony power — exacerbates household financial distress.

I first identify how adverse shocks affect financial distress i.e., default likelihood. I exploit the variation in gasoline prices from the outbreak of the Russia-Ukraine war on February 24, 2022[20]. This event was followed by severe sanctions imposed by several countries, including the U.S., that banned Russian oil, natural gas, and coal imports. In the U.S., crude oil prices increased sharply by 18% relative to the month before and 22% relative to the quarter before, and further rose by another 15% by March 2022 [21]. When oil prices shoot upward, gas prices rise with them[22]. Figure 2.2 shows a 28% jump in the retail gasoline prices after February 2022 from an average level of $3.38 per gallon in the six months before the event to an average level of $4.33 per gallon in the six months following the event. Taking into account the considerable variation in gas prices at the regional level, for a median US zipcode, this increase was as high as $2 in absolute terms.

[20]See https://www.reuters.com/world/europe/events-leading-up-russias-invasion-ukraine-2022-02-28/

[21]These statistics are generated using data from U.S. Energy Information Administration, Crude Oil Prices: West Texas Intermediate (WTI) - Cushing, Oklahoma [DCOILWTICO], retrieved from FRED, Federal Reserve Bank of St. Louis; https://fred.stlouisfed.org/series/DCOILWTICO, December 3, 2022.

[22]https://fredblog.stlouisfed.org/2022/06/oil-and-gas-prices-move-together-like-rockets-and-feathers/?utm_source=series_page&utm_medium=related_content&utm_term=related_resources&utm_campaign=fredblog

I further model cross-sectional variation in exposure to energy price shock along two dimensions which allows me to assign individuals to groups exposed more versus less to the increased gasoline price. First, I consider the heterogeneity in exposure at the regional level from differences in access to a public transport network and, thus, the scope of using transit as an alternative to personal vehicles to commute to work and for daily chores when needed. In this regard, I measure public transit connectivity at the zip code level using the All Performance Transit score data described in section 2.3. I compare regions with a transit score of 0 with non-zero transit score regions to construct my second source of variation. This manner of cutting my sample ensures that I consider the zip codes with the worst public transit network as the group with greater exposure and compare it to the less exposed counterparts. One potential concern could be that the areas with the worst transit network are not randomly assigned and may be systematically different than others. In particular, there could be confounding effects from regional trends such as local economic growth, etc. which might correlate with household financial well-being.

To alleviate these concerns, I explore another dimension in the data as a source of within zip code variation to determine exposure to gas price shock. This dimension varies at the individual level and is based on how far individuals travel for work. Looking within a zip code, and hence within a certain degree of public transportation network, changes in gasoline prices should have first-order effects for those with high work commuting distance. Their dependence on personal vehicles for commuting must be substantially higher, affecting them significantly when gas prices increase. Only 30% of individuals in my sample work outside their residence zip code, with about 20% traveling over 8 miles to their workplace. To capture the individuals who are the most exposed to gas price shocks, I compare individuals traveling over 8 miles (one-way) to those working and living in the same zip code, i.e., a commuting distance equal to zero, as my third source of variation.

I combine these three sources of variation in a triple difference-in-differences empirical strategy as specified in the following regression specification:

$$\begin{aligned} y_{i,z,t} = \theta_i + \theta_{z,t} + \Gamma \times X_{i,t-1} + \beta \times Commuting\ Far_i \times Low\ Transit_z \times Post_t + \\ \alpha \times Commuting\ Far_i \times Post_t\ + \gamma \times Commuting\ Far_i \times Low\ Transit_z\ + \epsilon_{i,z,t} \end{aligned} \tag{2.1}$$

where $i, z, t$ denote individual, zip code and calender month; $Post_t$ is a dummy variable and equals one if year-month $t$ is within one year after the Russia-Ukraine war outbreak i.e., February, 2022 onwards; $Commuting\ Far_i$ is a dummy equal to 1 if the distance between residence and work is greater than 8 miles, measured as of the month prior to the event and 0 for commuting distance equal to zero otherwise; $Low\ Transit_z$ is a dummy variable and equals one if individual resides in a zipcode $z$ with a public transit score equal to zero and 0 otherwise. My specification includes a robust set of fixed effects which control for time-in-varying individual characteristics (individual fixed effects $\theta_i$) and zip-time fixed effects $\theta_{z,t}$ for local time-varying economic shocks. I further include non-parametric controls ($X_{i,t-1}$) for quartiles of wage bin $\times$ calender month and quartiles of credit score bin $\times$ calender month, measured prior to the shock. For my regression estimates to have a causal interpretation, my empirical strategy requires the identifying assumption that in the absence of the Russian-Ukraine war (and the resulting gasoline price shock), the outcome variables of interest for far commuters in the limited public transport access regions would follow similar trends as near commuters.

## 2.4 Are Energy Price Shocks Consequential for Household Financial Distress?

This section presents my main result, which documents the extent of household financial distress caused by energy price shocks.

### 2.4.1 Deteriorating Credit Performance

I first investigate the extent to which price shocks in the form of sudden increase in gasoline prices could be detrimental to household financial health. Deteriorating financial health of households could be reflected in bad loan performance or increased debt burden. I examine each of these separately. Using Equation 2.1, I estimate changes in the likelihood of default on credit card debt around the Russia-Ukraine war conflict for individuals with higher exposure to gas price shocks compared to those with less exposure as my primary indicator of financial distress. I consider the likelihood of individuals credit card accounts being reported as 60+DPD or 90+DPD respectively.

Table 2.2 presents my main findings, with Columns (1) through (4) showing estimates for the effect of gas price shock on the likelihood of default (measured as 60+DPD) for different specifications of Equation 2.1. Column (1) only includes individual fixed effects and region x month fixed effects. The coefficient of the triple interaction term shows that individuals who are far commuters in low transit areas are 0.634 percentage points more likely to be reported as 60+ days past due after gas price shock compared to their less exposed counterparts. This increase represents approximately 5% relative to the unconditional mean of accounts reported as 60+DPD. Columns (2) to (3) incrementally add fixed effects for income quartiles interacted with calender month and credit score quartiles interacted with calender month, respectively. Column (4) further introduces employer times month fixed effects to effectively take out any employer specific time-varying attributes. While still significant, the magnitude of the effect decreases to represent 3.2% relative to average. Columsn (5) through (8) repeat the analysis above using the 90+DPD as the measure of default. The likelihood of default in response to gas price shock is between 0.40-0.57 percentage points higher for far commuters in low transit areas. This increase represents roughly 3.6-5.2% of the average default propensity i.e., 90+DPD.

### 2.4.2 Increased Debt Burden

Based on my previous findings, it appears that the financial well-being of individuals suffered in terms of their credit card loan performance. It is possible that this negative impact on loan performance is due to those who were more heavily impacted by the rise in gas prices borrowing more and accumulating larger outstanding balances. This additional debt could have been used to fund both increased gasoline consumption and other expenses. Unfortunately, due to limitations in the available data, I cannot evaluate changes in spending for each consumption category individually. However, I can investigate the overall effects on credit card expenditure. To do so, I use the same model as before (Equation 2.1), but this time I use credit card balance, number of active credit cards, and credit limit as additional outcome variables. I hypothesize that there will be a disproportionate increase in consumption expenditure, as indicated by a higher aggregate balance outstanding on the credit card and increased credit demand at the intensive margin, and an increased number of active credit cards on the extensive margin in the months following the shock.

In Table 2.3, I present my results, which indicate that the credit card loan balance for far commuters in low transit areas increases by $126 in the months following the gas price inflation shock (Column 2). During the period around the war, the gasoline price increased by as much as $2 in absolute terms for a median US zipcode. Considering that the median US household commutes 25,000 vehicle miles per year, with a gasoline consumption of about 961 gallons per year and 80 gallons per month (assuming a mileage of 26 miles per gallon), the median household would have had to spend an additional $160 per month on gas after the war-induced price hike. This would have doubled the share of their monthly budget spent on gas from 3% (i.e., a median monthly spending of $200) to 6%, a significant amount of their monthly income. The estimated increase in my model aligns closely enough with the median US household. This month-on-month increase results in an annual increase in outstanding debt of $1,440. Additionally, my analysis shows that the group exposed to the gas price shock opens additional unsecured credit lines and obtains a higher credit limit post-shock. These results suggest that household consumption expenditure increased in response to the gas price inflation. Taken together, these findings provide suggestive evidence that household budgets were squeezed following gas price shock, resulting in increased revolving debt burden.

### 2.4.3 Vulnerability of Liquidity Constrained Households

Previous research has indicated that lower income households tend to experience higher levels of inflation compared to higher income households due to differences in spending patterns across various consumption categories such as food, energy, education, and health (Kaplan and Schulhofer-Wohl, 2017, Jaravel and Sager, 2019; Argente et al., 2020). In fact, according to the 2019 Consumer Expenditure Survey, lower and middle-income households spent a significantly larger proportion of their total annual expenditure on motor fuel and vehicles compared to the higher income group.

I explore this heterogeneity among income groups in Table 2.4, using a cost of living adjusted income to assign individuals into lower-middle-upper groups. Lowest income tercile is the group earning less than $18,000 annually; middle income individuals earning between $18,000 to $80,000 a year and anyone making above $80,000 dollars falls in the upper income bracket. The results are consistent with previous research, where I find that amongst those most exposed to gasoline price shock, the default likelihood is between 0.736-0.882 percentage points higher for the lowest income group relative to the highest income category.

## 2.5 Does Employer Market Power Amplify Household Distress around Price Shock?

Employer monopsony affects workers' bargaining power during price shocks, and this in turn could affect their ability to negotiate better wages to mitigate the impact of the shock on their financial situation.

Standard search-and-matching models, such as those developed by Diamond, Mortensen, and Pissarides (Pissarides, 2000), suggest that nominal wage adjustment depends on labor market tightness, firm monopsony power, and workers' bargaining power. Recent research has shown that the rising levels of market concentration in the US (e.g., Abraham and Barkai, 2022; Grullon et al., 2019; Autor et al., 2017) have made it increasingly challenging to assume perfectly competitive labor markets as an accurate representation of wage-setting processes. Rather than acting as price-takers in the face of an infinitely elastic labor supply curve, firms have some degree of wage-setting ability, often bargaining with workers over the surplus created by employment[23].

A possible explanation for firms' ability to set wages below competitive levels is their market power within labor markets. In such imperfectly competitive markets, a small number of employers can negotiate with workers, leading to wage rates lower than those in perfectly competitive markets (Manning, 2003, Prager and Schmitt, 2021, Ashenfelter et al., 2021, Benmelech et al., 2022) [24]. High market concentration in labor markets may disadvantage individuals in the face of unexpected price shocks by hurting their ability to negotiate higher wages or benefits with their employer. As a result, individuals may have to allocate a larger portion of their budget to non-discretionary spending, which reduces their ability to pay off debts, exacerbating financial distress.

[23] See Krueger, 2018 for an overview of empirical evidence concerning worker bargaining power and applications to monetary policy.

[24] Other factors that can give firms bargaining power and wage-setting ability include search costs, switching costs between jobs, and firm-specific human capital, as well as heterogeneous firm characteristics that generate match-specific capital (Flinn, 2006).

### 2.5.1 Worse Off Working for a High Monoposony Employer?

I first examine whether individuals employed with firms having high monoposony power face greater financial distress around price-shocks compared to those employed with smaller firms due to the potential for employers to wield monopsony power in wage negotiations. The underlying identification assumption is that the difference in default likelihood for high verus low monopsony power employeed workers in the non exposure (less exposed) group acts as a counterfactual for what high versus low monopsonmy power employees bargaining power differentials for the exposed group would have been in the absence of adverse oil price shock.

To the extent that job searches are often localized (Manning and Petrongolo, 2017), I examine heterogeneity by monoposony power in relatively small geographic areas. To measure the local employer monopsony power, I use their market share in the labor market, which I define at the county and commuting zone level respectively. Defining the geographic boundaries of a local labor market is not straightforward; hence, I calculate the employment share of a firm in a particular county (commuting zone) by year as a proxy for market share as: $s_{fmt} = \frac{emp_{fmt}}{\sum_{f=1}^{N} emp_{fmt}}$, where $emp_{fmt}$ represents total employment of firm $f$ in labor market $m$ (which could be either county or commuting zone) in year $t$. After obtaining the market share measures for each firm, I categorize individuals in my sample into different subgroups depending on whether their employer has a high or low market share. To be more precise, I determine high versus low market power by comparing the employer's market share to the distribution of market shares. Employers with a market share in the top tercile of the distribution are classified as having high market power, while those in the bottom tercile are classified as having low market power.

In Table 2.5, the findings of my research are presented. Panel A displays the results for the various measures of employer market share, which is calculated at the county level. Across both measures of default, there is a positive and statistically significant difference in the coefficient of the triple interaction term. This implies that individuals commuting longer distances in low transit zip codes have a 0.881 to 1.475 higher probability of 60+ or 90+ days delinquency if they work for firms with a greater degree of monopsony power as opposed to those with low market power. In Panel B, I repeat the analysis for an alternative definition of local labor market i.e., defined at the commuting zone level. The results are consistent with the previous findings. These differences are both statistically and economically significant and represent between 8%-11% of the sample average.

The degree of monopsony power can also be measured using the quit elasticity, which indicates how likely a worker is to leave a job in response to a small wage change. Workers may be reluctant or unable to switch jobs, leading to monopsony power. A low quit elasticity suggests high monopsony power. To measure monopsony power, I use the average quit percentage of employees in a firm in the six months prior to a shock. This measure is based on UI management data from Equifax Inc. that provides anonymized information on the date, employer, and reason for separation. I calculate the fraction of voluntary separations labeled as 'quit' in my data as a proportion of active employees in the previous month. I divide my sample into sub-samples based on the median value of the quit rate distribution and find that default likelihood is significantly higher among individuals employed with firms having a below-median quit rate, according to a cross-sectional test presented in Table 2.6. These results are consistent with my findings corresponding to the other measures of monopsony power.

## 2.6 Mechanism

### 2.6.1 Firm Monopsony Wields Wage Negotiation

I proceed to demonstrate the wage adjustment disadvantage that individuals working for firms with a high degree of monopsony power face following a gas price shock. To achieve this, I estimate the following regression specification on an individual-employer-month panel:

$$log(income)_{i,f,m,t} = \theta_i + \theta_f + \theta_{z,t} + \beta \times Post_t \times High_{f,m} + \epsilon_{i,f,m,t} \tag{2.2}$$

where i, f, m, z, t correspond to individual, employer, local labor market (which could be either the county, commuting zone or commuting zone-industry), work zip code and calender month respectively. $High_{f,m}$ is a binary variable that takes a value of 1 if the employer f in market m had a market share in the top 30 percent of the market share distribution, and 0 if the market share was in the bottom 30 percent of the distribution. The coefficient $\beta$ represents the change in log income for individuals employed in high market power firms around the gas price shock relative to low market power firm employees, after controlling for any time-invariant individual characteristics ($\theta_i$), time-invariant firm attributes ($\theta_f$), and time-varying local economic changes ($\theta_{z,t}$).

In Table 2.8, I present the findings. The coefficient of $Post \times High$ is negative and statistically significant for all three measures of market share. These results suggest that individuals employed by firms with high monopsony power experienced a decrease in earnings of between 0.3 to 1.9 percentage points following a gas price shock when compared to those employed by firms with low monopsony power. The annualized income differentials illustrate a significant impact, with a range of 3.6-22.8 percentage points lower earnings.

I perform a similar exercise to demonstrate the wage adjustment disadvantage that individuals working for firms with a high degree of monopsony power face as measured by quit rate, following a gas price shock. Table B9 shows that the coefficient of $Post \times Quit\ Rate_{<Median}$ is negative and statistically significant suggesting that individuals employed by firms with high monopsony power, as measured by low quit rate experienced a decrease in monthly earnings of 1.4 percentage points following a gas price shock when compared to those employed by firms with low monopsony power which amounts to 16.8 percentage points annual income differential. Taken together, these results suggest that the difference in earnings between high and low monopsony power employers is substantial.

### 2.6.2 Price Shocks Pass-through from Firms to Workers?

An alternate mechanism could be that oil shocks can affect employers directly, which can in turn impact their employees. For instance, a rise in oil prices can increase the cost of production for firms that rely heavily on oil and oil-based products. This can lead to reduced profits and cash flow for the firm, potentially resulting in cost-cutting measures such as reduced wages, benefits, or even layoffs. As a result, employees may experience financial distress due to reduced income or unemployment. Additionally, higher oil prices may also increase the cost of transportation for firms, leading to reduced competitiveness and potential business closures, which can also impact employees.

I formally test this channel by creating sub-samples based on whether individuals are employed in industries input-intensive in oil and oil based products. For this, I use the input-output accounts data published by the Bureau of Economic Analysis at 6 digits NAICS industry classification. In particular, I specifically consider industries that heavily rely on imported oil. These include but are not limited to sub-industries within machine manufacturing (NAICS 333, 336, 325). As shown in Table Table 2.9, I donot find statistically and economically significant differences in the default likelihood across the two sub-samples for both my measures of default. This further supports my finding that firm monopsony power and the resulting differences in individuals wage negotiation is key in their ability to weather the negative effects of price shocks on the extent of financial distress.

## 2.7 Robustness

A potential concern in my setting could be that individuals who are more exposed to gas price shocks due to living in low transit areas and having longer work commutes may not be randomly assigned and could differ systematically from those who are less exposed. In order for this to threaten my identification strategy, there must be unobserved factors that are correlated with the choice of living in an area with a certain transit network and having a certain commute distance that affect the more exposed group compared to the less exposed group differentially and systematically so around the gas price shock. Although it may be difficult to argue such unobservables, I do the following to address these concerns. I saturate my specification with a robust set of fixed effects which control for time-in-varying individual characteristics (individual fixed effects $\theta_i$) and zip-time fixed effects $\theta_{zt}$ for local time-varying economic shocks. I further include non-parametric controls for individual income and credit score in the form of deciles for each of these variables measured prior to the shock ($X_{it-1}$).

Furthermore, I conduct a formal test to examine if there were any pre-existing trends in the likelihood of delinquency. To do so, I estimate the dynamic version of the equation presented in Equation 2.1, which can be expressed as follows:

$$
\begin{aligned}
y_{izt} = \theta_i + \theta_{zt} + X_{it-1} + \sum_{\substack{k=-6 \\ k \neq -1}}^{12} \beta_k \times Commuting\ Far_i \times Low\ Transit_z\ + \\
\alpha_k \times \sum_{\substack{k=-6 \\ k \neq -1}}^{12} \beta_k \times Commuting\ Far_i\ + \gamma \times Commuting\ Far_i \times Low\ Transit_z\ + \epsilon_{izt}
\end{aligned}
\tag{2.3}
$$

The omitted time period is the month before the shock i.e., k=-1. The coefficient of interest, denoted by $\beta_k$, captures the differential in the response of delinquency in the months around the gas price shock between low transit far commuters and low transit near commuters as compared to the divergence in the response of high transit far commuters and high transit near commuters. Figure 2.5 illustrates these coefficients along with the corresponding 95% confidence intervals. The three panels in Figure 2.5, namely (a), (b), and (c), correspond to the likelihood of 30DPD, 60+DPD, and 90+DPD, respectively. The plot indicates that, while the likelihood of delinquency changes similarly for both groups in the six months before the war conflict, there is a gradual deterioration of loan performance for the more exposed group thereafter. This provides evidence supporting the parallel trends assumption, which is crucial for the identification of my study.

Another issue that requires addressing is the interpretation of my results concerning the reduced form specification I am estimating. Specifically, the assumption that the Russian-Ukraine war is affecting individual behavior primarily through its impact on oil prices is critical in my analysis. My primary method of identifying the groups more versus less exposed to gas price changes based on transit access and commuting distance should indicate that there are no other factors outside of oil price changes around the shock that could be differentially influencing the credit performance of these groups. However, to further support this assumption, I perform a battery of tests to provide evidence across additional dimensions. First, I use the 2012-2016 American Community Survey 5-year estimate tract data to append information on the average fuel oil consumption per household, the percentage of households with 2 or more vehicles and the average number of trips taken per household in a year. Once appended to my primary sample, I create sub-samples based on the median of the distribution of each of these three variables respectively. I show that the baseline effects are stronger for individuals who reside in areas with above median levels of ownership of 2 or more vehicles (refer Table B5) per household, fuel oil consumption (refer Table B6) and vehicle trips taken (refer Table B4) respectively.

In my initial analysis, I establish the categories of *Low Transit* and *Commuting Far* using specific cutoff values for the All Transit Performance score and commuting distance. However, the determination of these thresholds is somewhat subjective. To ensure the reliability of my results, I vary the cutoff values along both dimensions. For "Low Transit", I maintain the same definition as in my original specification, but adjust the cutoff for commuting distance. As only a small proportion of my sample work outside their residential zip code, I use p70 (0 miles) and p75 (4 miles) as additional thresholds to define *Commuting Far*, in addition to my original cutoff of p80 (8 miles or more). Please note that a value of 1 is assigned to

*Commuting Far* if the commuting distance exceeds the threshold, whereas a value of 0 is assigned if the work and residence zip codes are the same. The coefficients for all three cutoffs are presented in Figure B1, and I find it reassuring that as the commuting distance increases, the impact of gas price inflation on delinquency rates also increases.

I further refine my analysis by keeping the definition of *Commuting Far* the same as in my initial analysis, but varying the cutoffs along the transit score dimension. In particular, I focus on the bottom 10% of my sample, which has a transit score of 0, and compare it with two other variations: the top 10% of the transit score distribution and the sample with transit score above and below the median. I find that my results are statistically significant for both of these alternate definitions of *Low transit*, as depicted in **??**. However, I do not use the bottom 10% versus top 10% comparison as my baseline due to concerns about external validity, which could arise from inherent differences between metropolitan and rural areas.

In considering shocks to gasoline prices to understand the effect of inflation on individual financial health, my approach captures the effects of rising prices only from one single commodity in the overall household consumption basket. However, I argue that energy goods and services, a category of which gasoline is a major component, accounts for roughly 7.5% of the overall Consumer Price Index (CPI) and its salience in the economy in general makes it of first order relevance for households financial decision making. Further, oil supply is often considered one of two of the primary drivers of inflation in U.S. history, the other being and monetary policy. Next,Moreover, energy price increases are fundamentally different from increases in the prices of other goods since energy prices experience sharp and sustained increases at times that are not typical of other goods and services, matter more because the energy demand is comparatively inelastic and, fluctuations are determined by forces plausibly exogenous to the U.S. economy (Kilian, 2008). Nonetheless, my estimates from this reduced-form approach may only represent a conservative estimate of the overall price effect on the outcome variables of interest.

## 2.8 Discussion

During the second and third quarters of 2022, household debt in the United States surged to an all-time high of $16.15 trillion, accompanied by a $46 billion increase in credit card balances. Transition rates into early delinquency for credit cards and auto loans rose by 0.6

and 0.4 percentage points, respectively, in 2022 Q4, following similarly sized increases in the previous two quarters. In the same quarter, delinquency transition rates for mortgages increased by 0.15 percentage points as well[25]. Many people believe that these developments coincide with a period of high inflation, suggesting that individuals accumulate debt and fall behind on payments to cope with soaring prices[26].

Using my estimates, I conduct calculations to provide a broader economic perspective on the impact of gas price inflation on credit card debt. As of January 2022, credit card debt in the United States stood at 1 trillion dollars, and approximately 200 million individuals hold credit cards. Based on my transit score and definition of far commuters, I classify nearly 10,000 zip codes as having significant exposure to gas price inflation caused by the war. The total credit card debt held by individuals in these areas is approximately 126 billion dollars. Using my default estimates of 0.654% to 0.778%, I predict that gas price inflation is likely to push between 824 million dollars and 980 million dollars of credit card balances into default within one year of the inflationary shock. These numbers suggest a reasonably significant effect of prolonged inflation on the fraction of non-performing loans. When individuals default on their credit card debts, it can lead to a cascade of negative effects, including damage to their credit score, collection calls and legal action, and potential bankruptcy. This not only affects individuals but also financial institutions that hold these debts, which can cause ripple effects throughout the broader economy.

## 2.9 Conclusion

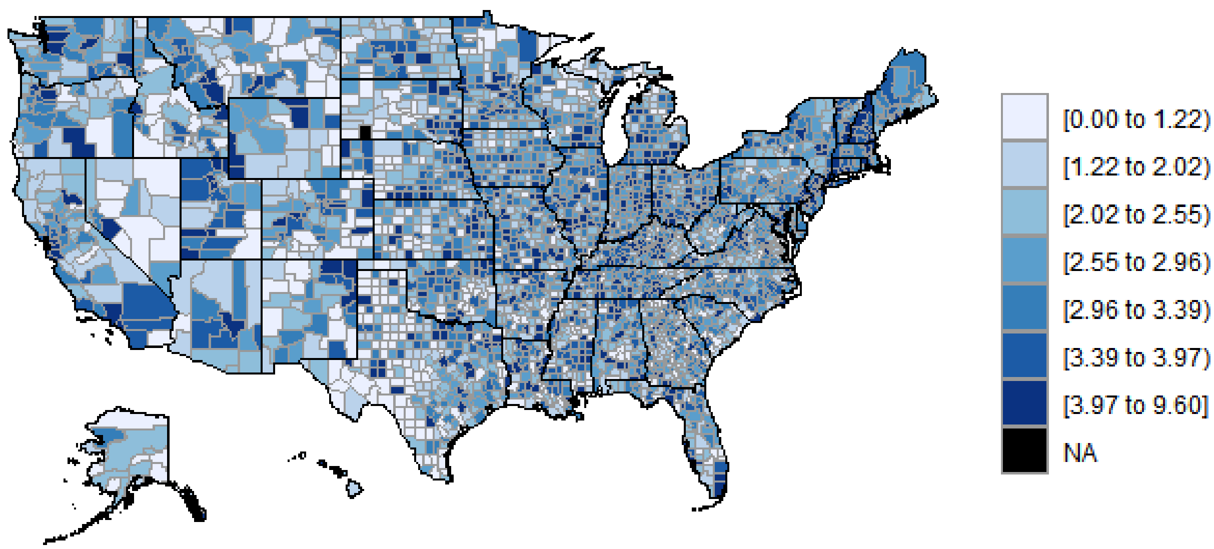
To summarize, in this paper, I explore the impact of unexpected inflation on financial stability through its effects on household debt defaults. While the impact of inflation on government, firms, and financial markets is well-known, its effect on households is not fully understood. Using anonymized detailed credit and payroll data on US households, the paper examines how inflation affects the likelihood of defaulting on debt, and the role of monopsony power in determining the pass-through of inflation to financial distress. The paper uses energy price shocks as a measure of inflationary pressure and the availability of public transportation

[25] Source: New York Fed Quarterly Household Debt Report

[26] According to the Clever Real Estate — Credit Card Debt Survey conducted in August 2022, about a third of respondents (31%) said they missed credit card payments to buy food or groceries, while 29% did so to cover utilities and 26% said they skipped payments to prioritize other forms of debt.

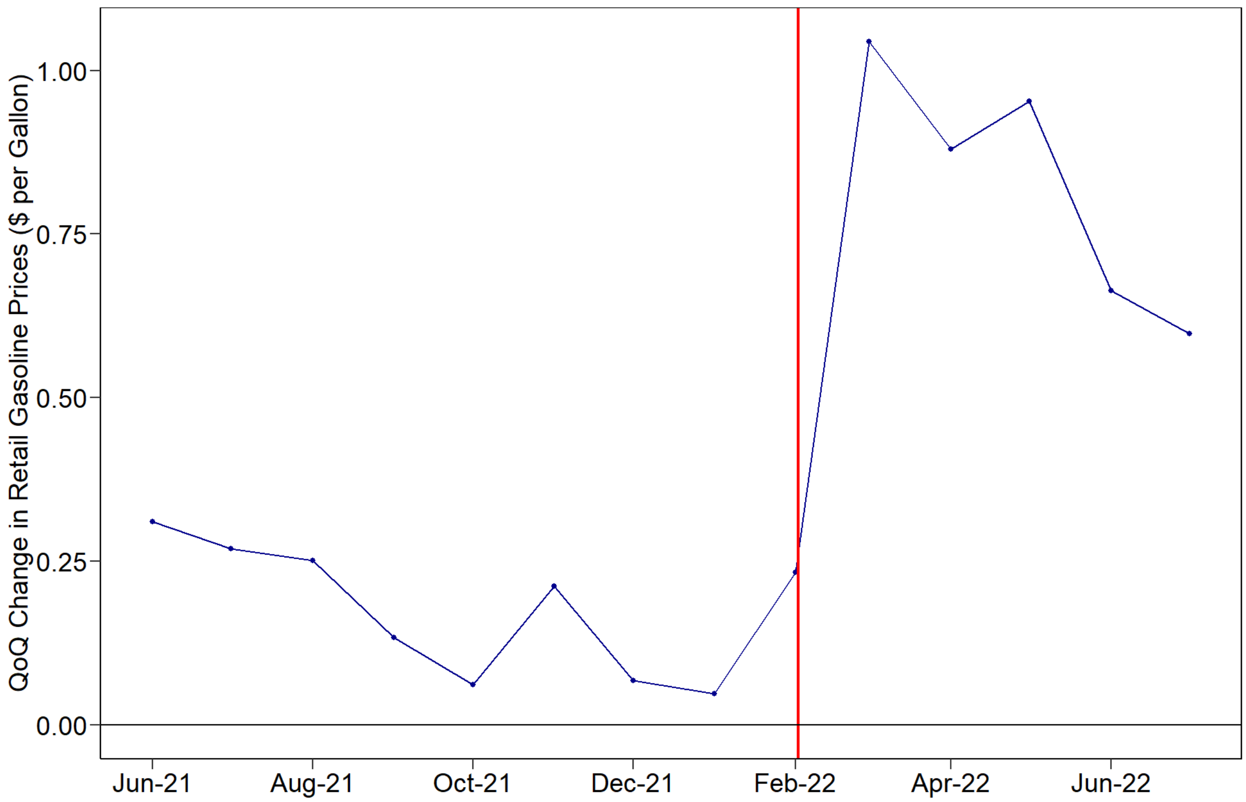
systems in addition to workplace commuting distance as a measure of heterogeneity in exposure to gasoline price changes. The results highlight the importance of labor market concentration and worker bargaining power in determining the impact of inflation on financial stability. From one perspective, concentrated labor markets and firms with large monopsony power are helping in curbing the wage-price spiral in an already inflationary environment, more so triggered by rising oil prices. So, by preventing wage price spiral, its preventing future rising commodity prices that could further distress households. An opposite perspective is that employers are key in consumption smoothing for individuals facing liquidity shocks since wage adjustment is important to maintain purchasing power. Thus, the lack of ability to negotiate wages emanating from high monopsony power is increase the propensity of bad household debt in the economy. To the extent that the role of market concentration in containing wage-price spiral in an inflationary environment happens at the cost of increasing the level of bad-debt, a complete welfare analysis is required to understand the overall impact on financial stability, which gives direction for future research.

Figure 2.1: Geographic Distribution of Public Transit Score



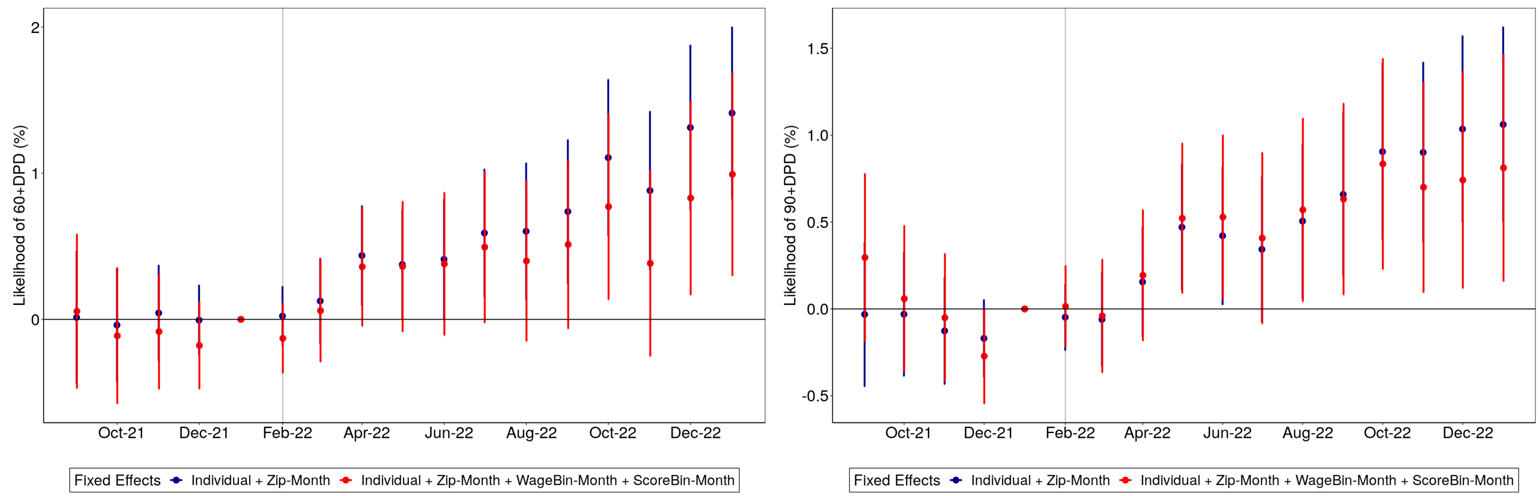

*Notes:* This figure presents the geographic distribution of public transit score measured using publicly available data from AllTransitTM. Transit score varies from 0-10 and is increasing in the degree of public transit accessibility within a city.

Figure 2.2: Time series trends in Retail Gasoline Prices




*Notes:* This figure presents the quarterly change in national retail gasoline price between June 2021 and June 2022 i.e., 12 months around the Russia-Ukraine war conflict.

Figure 2.3: Dynamics of Credit Card Performance around Gasoline Price Shock




(a) Likelihood of 60+DPD

(b) Likelihood of 90+DPD

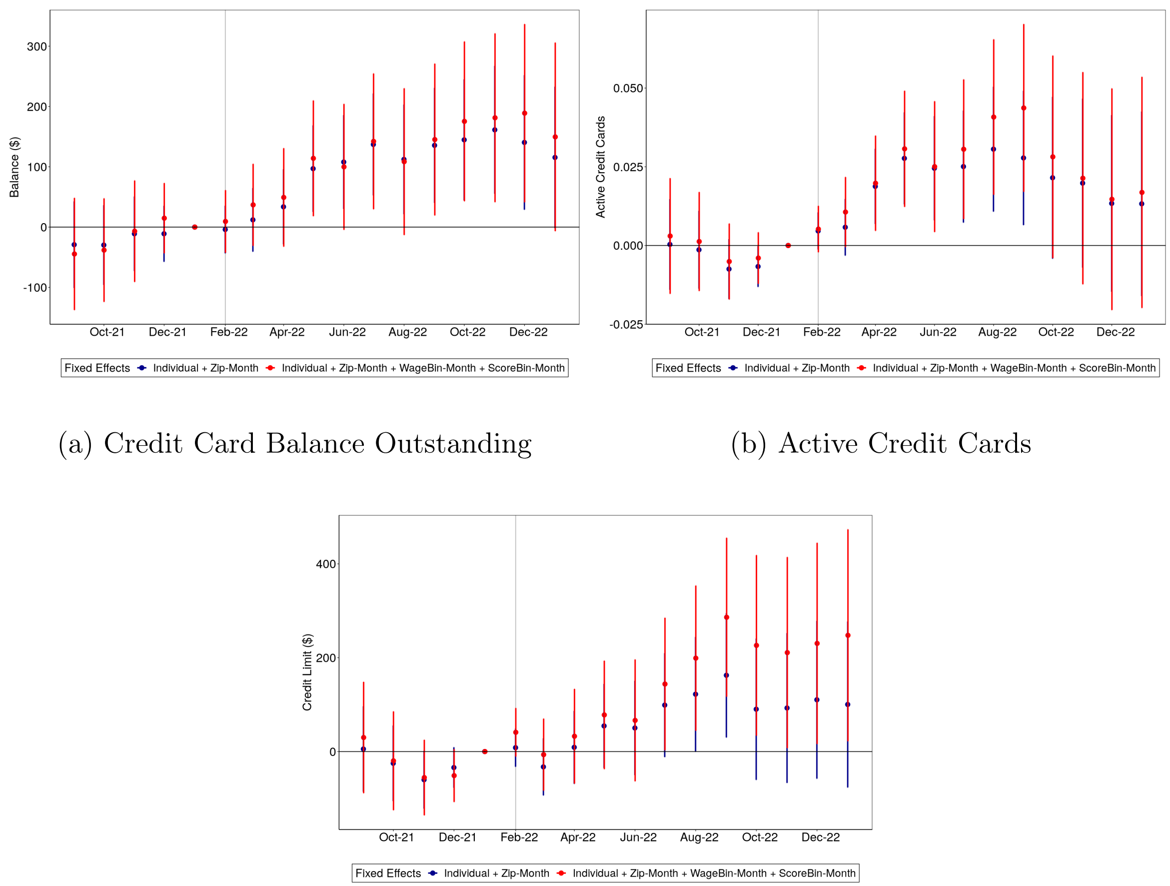
*Notes*: This figure plots the $\beta_k$ coefficients along with the corresponding 95% confidence intervals from Equation 2.3

$$Y_{izt} = \theta_i + \theta_{zt} + X_{it-1} + \sum_{\substack{t=Sep21 \\ t\neq Jan21}}^{Jan22} \beta_t \times 1_t \times Commuting\ Far_i \times Low\ Transit_z +$$

$$\sum_{\substack{t=Sep21 \\ t\neq Jan21}}^{Jan22} \alpha_t \times 1_t \times Commuting\ Far_i\ + \gamma \times Commuting\ Far_i \times Low\ Transit_z\ + \epsilon_{izt}$$

which captures the differential response of delinquency in the months around the gas price shock between low transit-far commuters and low transit-near commuters as compared to the divergence in the response of high transit-far commuters and high-transit near commuters. Panel (a) and (b) correspond to the likelihood of 60+DPD, and 90+DPD, respectively. Each panel plots two variations of Equation 2.3 i.e., in blue the specification with just individual ($\theta_i$) and zip-month fixed effects ($\theta_{zt}$) and subsequently adds wage quartiles-month and score quartiles-month fixed effects ($X_{it-1}$) in red respectively. Standard errors are clustered at the individual level.

Figure 2.4: Dynamics of Credit Card Usage around Gasoline Price Shock




(a) Credit Card Balance Outstanding

(b) Active Credit Cards

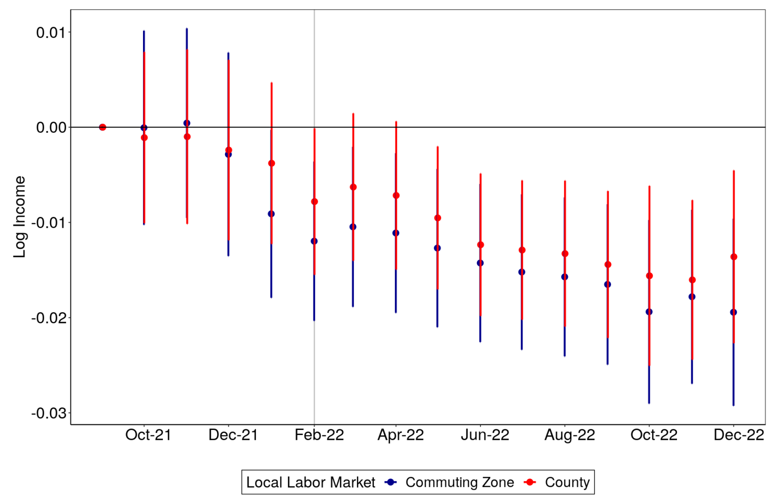
(c) Credit Limit

*Notes*: This figure plots the $\beta_t$ coefficients from Equation 2.3

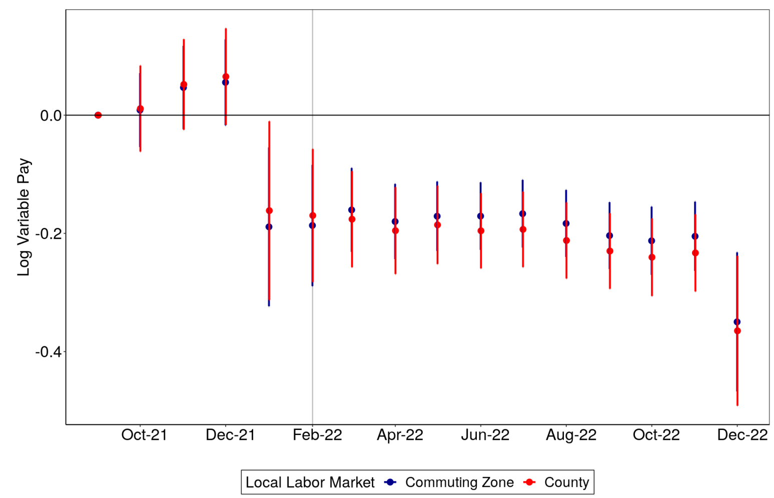
$$Y_{izt} = \theta_i + \theta_{zt} + X_{it-1} + \sum_{\substack{t=Sep21 \\ t \neq Jan21}}^{Jan22} \beta_t \times 1_t \times Commuting\ Far_i \times Low\ Transit_z + \sum_{\substack{t=Sep21 \\ t \neq Jan21}}^{Jan22} \alpha_t \times 1_t \times Commuting\ Far_i + \gamma \times Commuting\ Far_i \times Low\ Transit_z + \epsilon_{izt}$$

for three different measures of credit demand used as outcome variables. Panel (a) plots the differential trend in outstanding balance on credit card in the months around the gas price shock between low transit-far commuters and low transit-near commuters as compared to the divergence in the response of high transit-far commuters and high-transit near commuters. Panels (b) and (c) examine similar differential dynamic trends for number of newly opened revolving credit accounts and the aggregate credit limit respectively. Each panel plots two variations of Equation 2.3 i.e., in blue the specification with just individual and zip-month fixed effects and subsequently adds wage quartiles-month and score quartiles-month fixed effects in red respectively. 95% confidence intervals are included for each monthly point estimate with standard errors clustered at individual level.

Figure 2.5: Dynamics of Wage Adjustment by Employer Market Share




(a) Log Total Income




(b) Log Variable Pay

*Notes*: This figure plots the $\beta_k$ coefficients along with the corresponding 95% confidence intervals from the following Figure 2.5 estimated on an individual-employer-month panel:

$$Y_{ifmt} = \theta_i + \theta_f + \theta_{zt} + \theta_{kt} + \sum_{\substack{t=Sep21 \\ t\neq Sep21}}^{Dec22} \beta_t \times 1_t \times High_{fm} \ + \epsilon_{ifmt}$$

where i, f, m, z, k, t correspond to individual, employer, local labor market (which could be either the county (line in red) or commuting zone (line in blue)), work zip code, industry and calender month respectively. $High_{fm}$ is a binary variable that takes a value of 1 if the employer f in market m had a market share in the top tercile of the market share distribution, and 0 if the market share was in the bottom tercile percent of the distribution. The coefficient $\beta$ represents the change in log overall income in Panel (a) and log variable pay in Panel (b) for individuals employed in high market power firms around the gas price shock relative to workers in low market power firms, after controlling for any time-invariant individual characteristics ($\theta_i$), time-invariant firm attributes ($\theta_f$), and time-varying local economic changes ($\theta_{zt}$) and time-varying changes in the employer's industry ($\theta_{kt}$). 95% confidence intervals are included for each monthly point estimate with standard errors clustered at firm-industry-time level.

Table 2.1: Summary Statistics

This table reports summary statistics for the variables used in my analysis. Sample period is between September 2022 - January 2023. 30DPD, 60+DPD and 90+DPD have been defined as an absorbing state. Public Transit Score ranges from 0 to 10. Commuting Distance is measured in miles.

| | N | Mean | St. Dev. | p20 | Median | p80 |
|---|---|---|---|---|---|---|
| Dependent Variables | | | | | | |
| 30DPD | 10,520,002 | 0.16 | 0.30 | 0 | 0 | 0 |
| 60+DPD | 10,520,002 | 0.13 | 0.24 | 0 | 0 | 0 |
| 90+DPD | 10,520,002 | 0.11 | 0.32 | 0 | 0 | 0 |
| Credit Card Balance ($) | 10,518,227 | 5,614 | 9,481 | 319 | 2,215 | 8,439 |
| Credit Limit($) | 10,425,089 | 26,289 | 31,247 | 3,000 | 15,500 | 44,100 |
| # of Cards | 10,520,002 | 4 | 4 | 1 | 3 | 6 |
| Independent Variables | | | | | | |
| Public Transit Score | 9,900,389 | 4.34 | 2.84 | 1.1 | 4.7 | 7.1 |
| Commuting Distance | 10,520,002 | 7.39 | 16.05 | 0 | 0 | 7.91 |
| Credit Score | 10,520,002 | 704 | 97 | 623 | 725 | 795 |
| Monthly Income($) | 8,262,081 | 6,374 | 6,312 | 2,260 | 4,590 | 8,933 |

Table 2.2: Credit Card Performance and Gasoline Price Shock

This table reports the results of the OLS regression specified in Equation 2.1. Sample period is between September 2022 - January 2023. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. $LowTransit$ is a dummy and takes a value equal to 1 if the transit score is in the bottom 10 percentile (p10) and 0 otherwise. $Commuting\ Far$ is a dummy which is 1 for distance travelled to work beyond 8 miles(p80) one way and 0 if distance between home and work zip is 0. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | 60+DPD | | | | 90+DPD | | | |
|---|---|---|---|---|---|---|---|---|
| Variables | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post × $Low\ Transit$ × $Commuting\ Far$ | 0.634*** | 0.622*** | 0.497** | 0.422* | 0.571*** | 0.584** | 0.471** | 0.401* |
| | (0.199) | (0.238) | (0.225) | (0.233) | (0.190) | (0.227) | (0.214) | (0.223) |
| Post × $Commuting\ Far$ | 1.748*** | 1.419*** | 0.394 | 0.691*** | 2.083*** | 1.380*** | 0.445*** | 0.714*** |
| | (0.069) | (0.079) | (0.075) | (0.077) | (0.065) | (0.074) | (0.070) | (0.073) |
| Low Transit× $Commuting\ Far$ | 1.432 | 2.321** | 2.270* | 9.941*** | 1.242** | 1.942*** | 1.860** | 6.624* |
| | (0.668) | (0.764) | (0.793) | (4.621) | (0.629) | (0.717) | (0.743) | (3.559) |
| Individual FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Zipcode ×$Month\ FE$ | Y | Y | Y | Y | Y | Y | Y | Y |
| Wage Bin ×$Month\ FE$ | N | Y | Y | Y | N | Y | Y | Y |
| Score Bin ×$Month\ FE$ | N | N | Y | Y | N | N | Y | Y |
| Employer FE ×$Month\ FE$ | N | N | N | Y | N | N | N | Y |
| N | 9,278,346 | 7,358,616 | 7,348,056 | 7,348,056 | 9,278,346 | 7,358,616 | 7,348,056 | 7,348,056 |
| $AdjR^2$ | 0.836 | 0.838 | 0.704 | 0.846 | 0.841 | 0.844 | 0.853 | 0.852 |

Table 2.3: Credit Usage and Gasoline Price Shock

This table reports the results of the OLS regression specified in Equation 2.1. Sample period is between September 2022 - January 2023. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. *LowTransit* is a dummy and takes a value equal to 1 if the transit score is in the bottom 10 percentile (p10) and 0 otherwise. *Commuting Far* is a dummy which is 1 for distance travelled to work beyond 8(p80) miles one way and 0 if distance between home and work zip is 0. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Credit Limit | Balance | # Active Cards |
|---|---|---|---|
| Variables | (1) | (2) | (3) |
| Post × *Low Transit* × *Commuting Far* | 127.397** | 125.602*** | 0.021** |
| | (62.028) | (40.095) | (0.010) |
| Post × *Commuting Far* | -94.292*** | 70.433*** | -0.046*** |
| | (23.711) | (14.833) | (0.003) |
| Low Transit× *Commuting Far* | -378.062 | -259.408* | -0.070* |
| | (232.153) | (154.688) | (0.038) |
| Individual FE | Y | Y | Y |
| Zipcode ×*Month FE* | Y | Y | Y |
| Wage Bin ×*Month FE* | Y | Y | Y |
| Score Bin ×*Month FE* | Y | Y | Y |
| N | 7,302,949 | 7,346,972 | 7,348,056 |
| $AdjR^2$ | 0.978 | 0.877 | 0.968 |

Table 2.4: Heterogeneity by Income

This table reports the heterogeneity in the consumer default by income terciles. Sample period is between September 2022 - January 2023. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. *Low Transit* is a dummy and takes a value equal to 1 if the transit score is in the bottom 10 percentile (p10) and 0 otherwise. *Commuting Far* is a dummy which is 1 for distance travelled to work beyond 8 miles(p80) one way and 0 if distance between home and work zip is 0. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | 60+DPD | | | 90+DPD | | |
|---|---|---|---|---|---|---|
| | By Tercile | | | By Tercile | | |
| | Bottom | Middle | Upper | Bottom | Middle | Upper |
| Variables | (1) | (2) | (3) | (4) | (5) | (6) |
| Post × *Low Transit* × *Commuting Far* | 1.010** | 0.157 | 0.128 | 0.865* | 0.256 | 0.129 |
| | (0.466) | (0.408) | (0.291) | (0.455) | (0.385) | (0.251) |
| Post × *Commuting Far* | 0.642*** | 0.729*** | 0.118 | 0.692*** | 0.688*** | 0.213*** |
| | (0.156) | (0.136) | (0.087) | (0.148) | (0.126) | (0.076) |
| Low Transit× *Commuting Far* | 1.982 | -1.097 | 0.650 | 0.591 | -2.189 | 1.231 |
| | (2.196) | (1.795) | (1.354) | (2.093) | (1.644) | (1.232) |
| Individual FE | Y | Y | Y | Y | Y | Y |
| Zipcode ×*Month FE* | Y | Y | Y | Y | Y | Y |
| Score Bin ×*Month FE* | Y | Y | Y | Y | Y | Y |
| N | 2,416,049 | 2,446,932 | 2,485,075 | 2,416,049 | 2,446,932 | 2,485,075 |
| $Adj R^2$ | 0.838 | 0.849 | 0.859 | 0.842 | 0.857 | 0.878 |

Table 2.5: Heterogeneity by Employer Market Share

This table reports the heterogeneity in the consumer default around the gas price shock by employer market power. Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. *LowTransit* is a dummy and takes a value equal to 1 if the transit score is in the bottom 10 percentile (p10) and 0 otherwise. *Commuting Far* is a dummy which is 1 for distance travelled to work beyond 8 miles(p80) one way and 0 if distance between home and work zip is 0. High (Low) is coded as 1 if the individual's employer is in the top (bottom) 30 percent of the distribution of firm market share by county. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | 60+DPD | | | 90+DPD | | |
|---|---|---|---|---|---|---|
| | High | Low | Diff | High | Low | Diff |
| Variables | (1) | (2) | (1) - (2) | (4) | (5) | (4) - (5) |
| **Panel A: County** | | | | | | |
| Post × *Low Transit* × *Commuting Far* | 1.50*** | 0.025 | 1.475*** | 1.58*** | 0.160 | 1.42*** |
| | (0.481) | (0.389) | | (0.470) | (0.371) | |
| Post × *Commuting Far* | 1.79*** | 1.41*** | | 1.73*** | 1.42*** | |
| | (0.171) | (0.119) | | (0.161) | (0.112) | |
| Low Transit× *Commuting Far* | 4.42** | 1.18 | | 2.05 | 0.497 | |
| | (2.13) | (1.41) | | (2.01) | (1.33) | |
| N | 2,153,959 | 2,879,734 | | 2,153,959 | 2,879,734 | |
| Adj $R^2$ | 0.871 | 0.869 | | 0.874 | 0.875 | |
| **Panel B: Commuting Zone** | | | | | | |
| Post × *Low Transit* × *Commuting Far* | 1.26** | 0.301 | 0.959* | 1.29** | 0.409 | 0.881* |
| | (0.516) | (0.378) | | (0.504) | (0.359) | |
| Post × *Commuting Far* | 2.15*** | 1.36*** | | 2.08*** | 1.31*** | |
| | (0.176) | (0.116) | | (0.167) | (0.109) | |
| Low Transit× *Commuting Far* | 3.85* | 1.18 | | 2.74 | 0.953 | |
| | (2.14) | (1.48) | | (2.07) | (1.39) | |
| N | 2,269,960 | 2,777,928 | | 2,269,960 | 2,777,928 | |
| Adj $R^2$ | 0.869 | 0.869 | | 0.873 | 0.875 | |
| Individual FE | Y | Y | | Y | Y | |
| Zipcode ×*Month FE* | Y | Y | | Y | Y | |
| Wage Bin ×*Month FE* | Y | Y | | Y | Y | |

Table 2.6: Heterogeneity by Quit Rate for Employers

This table reports the heterogeneity in the consumer default around the gas price shock by the extent of quit rate for employers calculated as the average rate of voluntary separations in the 6 months prior to the shock. Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. $LowTransit$ is a dummy and takes a value equal to 1 if the transit score is in the bottom 10 percentile (p10) and 0 otherwise. $Commuting\ Far$ is a dummy which is 1 for distance travelled to work beyond 8 miles(p80) one way and 0 if distance between home and work zip is 0. Above (Below) is coded as 1 if the individual is employed with a firm which has an above (below) median quit rate. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | 60+DPD | | | 90+DPD | | |
|---|---|---|---|---|---|---|
| | By Median | | | By Median | | |
| | Above | Below | Diff | Above | Below | Diff |
| Variables | (1) | (2) | (2) - (1) | (4) | (5) | (5) - (4) |
| Post × $Low\ Transit$ × $Commuting\ Far$ | -1.023 | 2.663*** | 3.686*** | -0.956 | 2.257** | 3.214*** |
| | (0.786) | (1.023) | | (0.765) | (0.927) | |
| Post × $Commuting\ Far$ | 0.977*** | 0.916*** | | 1.105*** | 0.808*** | |
| | (0.267) | (0.291) | | (0.252) | (0.273) | |
| Low Transit× $Commuting\ Far$ | 10.836** | 3.278 | | 12.502* | 3.718 | |
| | (5.455) | (5.917) | | (5.163) | (6.010) | |
| N | 979,154 | 734,449 | | 979,154 | 734,449 | |
| $Adj R^2$ | 0.838 | 0.851 | | 0.842 | 0.858 | |
| Individual FE | Y | Y | | Y | Y | |
| Zipcode ×$Month\ FE$ | Y | Y | | Y | Y | |
| Wage Bin ×$Month\ FE$ | Y | Y | | Y | Y | |
| Score Bin ×$Month\ FE$ | Y | Y | | Y | Y | |

Table 2.7: Heterogeneity by Job-to-Job Separation Rate

This table reports the heterogeneity in the consumer default around the gas price shock based job-to-job separation rate at the zipcode level as a measure of local labor market tightness. Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. $LowTransit$ is a dummy and takes a value equal to 1 if the transit score is in the bottom 10 percentile (p10) and 0 otherwise. $Commuting\ Far$ is a dummy which is 1 for distance travelled to work beyond 8 miles(p80) one way and 0 if distance between home and work zip is 0. Slack is coded as 1 if the individual works in a zipcode with job-to-job separation rate in the Tight is when the job-to-job separation rate at the work zip is in the top thirty percent. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | 60+DPD | | | 90+DPD | | |
|---|---|---|---|---|---|---|
| | Slack | Tight | Diff | Slack | Tight | Diff |
| Variables | (1) | (2) | (1) - (2) | (4) | (5) | (4) - (5) |
| Post × *Low Transit* × *Commuting Far* | 1.09** | -0.848 | 1.938*** | 0.964** | -0.367 | 1.331*** |
| | (0.459) | (0.553) | | (0.427) | (0.550) | |
| Post × *Commuting Far* | -0.036 | 1.43*** | | 0.007 | 1.43*** | |
| | (0.137) | (0.192) | | (0.127) | (0.183) | |
| Low Transit× *Commuting Far* | -1.24 | 2.09 | | -1.03 | 1.23 | |
| | (1.67) | (1.81) | | (1.51) | (1.69) | |
| N | 2,230,146 | 2,171,370 | | 2,230,146 | 2,171,370 | |
| Adj $R^2$ | 0.882 | 0.865 | | 0.891 | 0.868 | |
| Individual FE | Y | Y | | Y | Y | |
| Zipcode ×*Month FE* | Y | Y | | Y | Y | |
| Wage Bin ×*Month FE* | Y | Y | | Y | Y | |
| Score Bin ×*Month FE* | Y | Y | | Y | Y | |

Table 2.8: Income Adjustment around Gasoline Price Shock by Employer Market Share

This table reports the results of the OLS regression specified in Figure 2.5. The regression is estimated on a individual-employer-month panel. Sample period is between September 2022 - January 2023. The main dependent variable is logarithm of average firm level income measured at each calender month. High is coded as 1 if the individual's employer is in the top 30 percent of the distribution of firm market share by county in column (1), commuting zone in column (2) and commuting zone-industry in column (3) respectively. Low is 1 if the individual's employer is in the bottom 30 percent of the distribution of firm market share by county (commuting zone). The Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. Employer Zipcode is the zip of firm location. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | County | | Commuting Zone | |
|---|---|---|---|---|
| | Log Income | Log VariablePay | Log Income | Log VariablePay |
| Variables | (1) | (2) | (3) | (4) |
| Post $\times High$ | -0.003** | -0.076*** | -0.005*** | -0.046*** |
| | (0.001) | (0.007) | (0.001) | (0.007) |
| Individual FE | Y | Y | Y | Y |
| Employer FE | Y | Y | Y | Y |
| Employer-Zipcode $\times Month\ FE$ | Y | Y | Y | Y |
| N | 5,520,983 | 5,520,498 | 5,510,405 | 5,509,934 |
| $Adj R^2$ | 0.935 | 0.883 | 0.935 | 0.883 |

Table 2.9: Heterogeneity by Oil Input Intensive Industries

This table reports the heterogeneity in the consumer default around the gas price shock by the extent of quit rate for employers calculated as the average rate of voluntary separations in the 6 months prior to the shock. Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. *LowTransit* is a dummy and takes a value equal to 1 if the transit score is in the bottom 10 percentile (p10) and 0 otherwise. *Commuting Far* is a dummy which is 1 for distance travelled to work beyond 8 miles(p80) one way and 0 if distance between home and work zip is 0. Above (Below) is coded as 1 if the individual is employed with a firm which has an above (below) median quit rate. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | 60+DPD | | 90+DPD | |
|---|---|---|---|---|
| | Oil-Intensive | Oil Non-Intensive | Oil-Intensive | Oil Non-Intensive |
| Variables | (1) | (2) | (3) | (4) |
| Post × *Low Transit* × *Commuting Far* | 0.409 | 0.610** | 0.392 | 0.570** |
| | (0.733) | (0.245) | (0.668) | (0.235) |
| Post × *Commuting Far* | 0.749*** | 0.359*** | 0.592*** | 0.430*** |
| | (0.231) | (0.081) | (0.216) | (0.076) |
| Low Transit× *Commuting Far* | 15.6*** | 1.13 | 13.6*** | 0.913 |
| | (4.52) | (0.867) | (4.17) | (0.813) |
| N | 883,896 | 6,464,160 | 883,896 | 6,464,160 |
| $AdjR^2$ | 0.904 | 0.865 | 0.909 | 0.871 |
| Individual FE | Y | Y | Y | Y |
| Zipcode ×*Month FE* | Y | Y | Y | Y |
| Wage Bin ×*Month FE* | Y | Y | Y | Y |
| Score Bin ×*Month FE* | Y | Y | Y | Y |

# Chapter 3

# External Labor Market Punishment in Finance

Naser Hamdi, Ankit Kalda, Avantika Pal [27]

## Abstract

We document that finance employees involuntarily separated for misconduct earn 2.8% to 8.6% higher income than similar employees laid-off for no fault. Our results are most consistent with assortative matching in the finance labor market — firms more likely to engage in misconduct are also more likely to hire employees separated for misconduct and pay a wage premium for them. Finance is unique in that these patterns are reversed for all other sectors. One hypothesis explaining our findings is that most products and services in finance are based on future cash flows which makes it potentially easier to camouflage such behavior.

*Keywords:* Misconduct, punishment, finance sector, layoffs, earnings, employer-employee match

[27]Hamdi is a Equifax Inc. Kalda is at Indiana University. Pal is at Washington University in St. Louis.

## 3.1 Introduction

Fraud, bad faith dealings, and misconduct remain prevalent in the finance sector. For example, Egan et al., 2019 document that up to 15% of financial advisors in large firms have a misconduct record. Such activities likely contribute towards the low public trust in finance professionals and financial institutions guiso2008.[28] Given these potential costs for firm and industry reputation, it is not clear why these misbehaviors remain commonplace. One plausibility may be that the benefits of such misbehaviors outweigh the costs for both firms and individuals perpetrating them. This can happen for example if employees involved in misconduct do not bear high personal costs in terms of their career outcomes. In this paper, we evaluate this potential and study the extent of *external* labor market punishment for misconduct in the finance sector and contrast this to consequences in non-finance sectors.

The external labor market can compound or undo, either partially or completely, the internal punishment. To evaluate this hypothesis, we focus on employees involuntarily separated for misconduct and examine how their income evolves post separation from the firm. Ours is the first paper to examine income trajectories for employees involved in misconduct. We are also the first to study a large sample of employees representing the entire finance and insurance sector (NAICS 52) and compare the outcomes for employees in other sectors.[29]

Evaluating income progression for those involuntarily separated for misconduct is empirically challenging because these employees experience separation from their employers, which by itself has been shown to affect income irrespective of employees' involvement in misconduct. A large literature documents that employees laid-off for no fault of their own (e.g., due to lack of work) experience significant decline in income [e.g.,][]jacobson1993,couch2001, jacobson2005, von2009,schmieder2010, couch2010. This can happen for a number of non-exclusive reasons including loss of firm-specific human capital topel1991, neal1995, kletzer1998, loss of favorable employer-employee matches and firm rents krueger1988,abowd1999, bronars1997, card2013, lachowska2020, song2019, moore2019, demotion in the job ladder jarosch2021, krolikowski2017,

[28]Sapienza and Zingales maintain a financial trust index which has fluctuated between 20-35% between 2008 to present. See here for more details: http://www.financialtrustindex.org/about.htm

[29]In contrast the literature focuses on employees with specific roles. For example, among others, dimmock2012, Parsons et al., 2018, Egan et al., 2019 evaluate misconduct among financial advisers; Ellul et al., 2020 document under-performance for asset managers; Griffin et al., 2019 focus on employees involved in residential mortgage-backed security securitization; Gao et al., 2020 examine loan officers who structured poorly performing corporate loans.

and changes in occupation huckfeldt2022 among others. Hence, without controlling for this "separations effect", we may wrongly attribute entire changes in income to misconduct. We overcome this issue by using income response for those laid-off for no fault (i.e., no fault layoffs) as a benchmark in our setting.

Ex-ante, it is not obvious how income would evolve for those separated for misconduct. If the external labor markets punish employees with misconduct background or there are reputational costs to hiring them, these employees may receive lower income than those separated for no fault. Alternatively, if there are information frictions and potential employers find it difficult to discern across employees, there would be no differences in income. Lastly, if misconduct is correlated with certain desirable traits (e.g., higher risk appetite, 'ends justify the means' attitude), employees separated for misconduct may experience higher income relative to those laid-off for no fault.

In order to examine these possibilities, we leverage anonymized proprietary data from Equifax Inc. which offers unemployment management, and verification services (e.g., employment) among other products. Employers that subscribe to these services report information on all separations in their workforce and reasons for the separation, along with employment information including wages, job tenure, and employment location among others. The data covers over 20% of all separations in the US during our sample period from 2011 to 2018. Our definition of misconduct comes from the reason of separation reported by the employer wherein misconduct and no fault layoffs are reported as involuntary separations categories.

Since our analysis introduces new data, we begin by replicating the results in the literature that estimate the changes in income around no-fault layoffs (i.e., the separations effect) to help establish the validity of the sample. We follow Jacobson et al., 1993 (JLS, henceforth) — the standard in the literature — and estimate a difference-in-differences specification that compares employees laid-off for no fault to those who continue to be employed before and after separation. This specification includes individual fixed effects that account for static individual level differences, industry (6-digit NAICS) and wage-bin ($1,000 bins) time effects that control for time varying differences across industry and income levels respectively. We find patterns and magnitudes similar to those documented in the literature [e.g.,][]couch2001, jacobson2005,couch2011, moore2019.

To evaluate the income evolution following misconduct related involuntary separations, we examine employees separated from firms in the finance and insurance sector defined by the

NAICS code of 52 and non-finance firms separately. We estimate a difference-in-differences specification similar to JLS and compare income for employees involuntarily separated for misconduct to those who continue to remain employed. To account for the separations effect, we compare this estimate to a similar coefficient for no fault layoffs. This yields estimates akin to a triple differences model. Other specifications further include firm-by-3-digit zipcode and tenure time effects that account for all time-varying differences at the separated firm-region combinations (e.g., economic conditions, regulation faced by firms etc.), and across employees with different tenure (in years) respectively. Across both specifications, we find that employees involuntarily separated for misconduct earn 2.5% to 8.6% *higher* income relative to those laid-off for no fault in the finance sector.[30] In sharp contrast, we find that those involuntarily separated from firms in non-finance sectors for misconduct earn 4.4% to 8.1% *lower* income post separation than those experiencing no fault layoffs.

A concern in our setting is that differences across employees separated for misconduct versus no fault or non-separated employees may bias our results. We take a number of steps to help address this concern. Our baseline specification saturates the model with multiple fixed effects to non-parametrically account for a number of factors including different levels of time-varying skills (captured by income and job tenure), inherent capabilities (through individual fixed effects), firm regulation, and differences in regional and industry job market conditions (firm-by-3-digit zip and industry time effects). The absence of differential pre-trends in income across employees involuntarily separated for misconduct and those who continue to remain employed provides some reassurance that our specification appropriately controls for differences across employees. We also re-estimate our baseline results with a collapsed triple interaction specification that imputes fixed effects for the entire sample simultaneously instead of separately for different sub-samples. That we find our estimates to be unchanged further helps reassure that differences across employees are less likely to explain our results. This specification also allows us to include separation cohort time effects thereby comparing misconduct and no-fault employees separated during the same year-month. Yet another piece of evidence that helps address this concern is that we find similar results when we conduct our analyses with a matched sample.

Firms may choose to layoff less productive employees when letting go of only a few employees and this may lead to differences between those separated for layoff and misconduct. To

[30] Even though both types of employees experience income declines in absolute terms post separation, the differences arise from relatively lower income declines for those separated for misconduct.

address this concern, we re-estimate the triple interaction using no fault layoffs that were part of a mass layoff where firm's choice is less likely to be a factor. We find results similar to our baseline with this sample less subject to firm discretion. Finally, we rely on heterogeneity of findings across different dimensions. For example, our results are driven by those rehired within the finance sector, concentrated amongst finance related jobs within the finance sector and firms more likely to engage in misconduct themselves.

One mechanism that can help explain the potentially surprising patterns that we observe for the finance sector is assortative matching in the labor markets. Firms more likely to engage in misconduct themselves may value employees with a misconduct background either because they have demonstrated a willingness to engage in certain behaviors or misconduct is correlated with traits valued by such firms (e.g., higher risk taking, 'ends justify the means' attitude). We test this hypothesis by studying transitions made by employees separated for misconduct between firms with different likelihoods to commit misconduct or 'cut corners'. We use the number of fraud related complaints filed by consumers against firms, firms' timely response rates, and instances of corporate misconduct as different proxies that capture this firm behavior. For the finance sector, employees separated for misconduct are both more likely to make transitions from firms with low- to high-likelihood of corporate misconduct, and experience longer tenures post such transitions. Further, the income results are concentrated/stronger for employees separated from firms with lower propensity to commit misconduct who get rehired in firms with higher propensity to do so. Both higher likelihood of transitions of employees separated for misconduct from firms with low- to high-likelihood of corporate misconduct and larger income changes for such moves are even more stark for repeat offenders. Overall, these results are consistent with assortative matching in the labor market where employers with higher propensity to engage in misconduct pay a wage premium for employees with a misconduct background. We do not detect any such heterogeneity for non-finance sectors.

A natural question at this point is whether finance is the only sector where employees separated for misconduct experience higher wage changes than those separated for no fault or are there other sectors where similar patterns hold. To address this question, we re-estimate our main triple interaction specification for all major sectors in the economy (defined by 2-digit NAICS codes)[31] and find that though there is significant heterogeneity across different sectors, this pattern only holds for the finance sector. While several factors may contribute

[31] With the exception of public administration and agriculture that are not covered in the data.

towards making finance unique, one feature that we propose is that most products and services offered in the sector are based on future cash flows. This likely makes it difficult for consumers or other stakeholders to disentangle “bad luck” from a deliberate risky or unethical transaction in case of losses, thereby reducing the expected costs of engaging in such behavior for both firms and employees. We conduct a number of analyses to evaluate this hypothesis. First, we examine heterogeneity in our findings based on whether employees’ jobs are related to financial products and services, and find our results to be concentrated for finance-related jobs and absent for non-finance jobs within the finance sector. Second, firms more likely to engage in such behavior may cater towards more unsophisticated consumers who may be easier to be ensnared egan2019. Consistent with this argument, we find our income results to be concentrated or stronger amongst hiring firm establishments more likely to cater to unsophisticated consumers, i.e., those located in areas with lower education levels and higher proportion of elderly population, especially for finance- and sales-related jobs within finance. In contrast, we find no such heterogeneity for non-finance sectors or non-finance jobs within the finance sector.

Systematic differences in inherent culture between the finance sector and others may also contribute towards our findings. It is well known that finance is unique in that it is a high-skill and high-wage sector where returns to talent have increased more relative to other sectors [e.g.,][]philippon2012, Celerier2019. Similarly, finance may be unique in some aspect positively correlated with the propensity to engage in risky or potentially unethical/fraudulent behavior [e.g.,][]Gill2022.

In addition, we evaluate a number of alternative mechanisms that may contribute to our findings including differences across sectors in regulation, type of misconduct, job search intensities, and scapegoating behavior among others but do not find evidence supporting these hypotheses. We discuss these tests in section 3.4 and robustness in section 3.5.

Our study relates to a growing literature that examines the extent of misconduct and fraud within the finance sector [e.g.,][]dimmock2012, griffin2016, gurun2016, mian2017, gurun2018, parsons2018, dimmock2021, tookes2021, Celerier2023, and how financial institutions and labor markets discipline finance employees for both poor performance and misconduct/fraud [e.g.,][]chevalier1999, egan2019, griffin2019, ellul2020, gao2020. While Ellul et al., 2020 document that asset managers working for funds liquidated following persistently poor performance experience demotions and declines in imputed compensation, Gao et al., 2020 show that banks discipline loan officers involved in originating corporate loans that end up performing poorly. Griffin et al., 2019 document that employees involved in residential mortgage-backed security (RMBS) securitization prior to the great recession did not experience differential job retention, promotion, and external job opportunities relative to similar non-RMBS employees.

Egan et al., 2019 document the widespread nature of misconduct among financial advisors and that significant fraction of advisors who turnover following a misconduct get rehired within the industry, albeit in less reputable firms that on average pay lower compensation to their employees. Our paper contributes to this literature in two distinct ways. First, using data spanning the entire finance and insurance sector we show the prevelance of 'misconduct wage premium' in finance. Second, our results highlight that the finance sector is unique in exhibiting this premium. One potential explanation consistent with our results is that difficulty in guaging misconduct for product and services based on future cash flows reduces the expected costs of engaging in such behavior for both firms and employees.

## 3.2 Data and Empirical Strategy

This section describes the data used in the analyses, discusses our sample, and details our empirical strategy.

### 3.2.1 Data

The data comes from Equifax Inc. and contains anonymized employment information across two dimensions: job separation events and employment characteristics.

Job separation data are disseminated to Equifax Inc. by self-reporting employers who subscribe to Unemployment Insurance (UI) management services provided by the company. When a UI claim is filed, government agencies reach out to the ex-employer to acquire information on the terms of separation in order to verify UI eligibility.[32] Many states require employers to respond to all such government requests to facilitate the efficient administration

[32]Most states require that claimants must have separated from the employer involuntarily due to no fault of their own.

of UI claims. In order to adhere to such requirements, participating employers subscribe to the UI management services from Equifax Inc. which manages all such inquiries on their behalf. As a result, participating employers report data related to all incidences of job separation to the company. The job separations data includes over 20% of all separations reported in the Bureau of Labor Statistics (BLS)'s Job Openings and Labor Turnover Survey (JOLTS) data over our sample period.[33] Using this anonymized data, for each job separation, we are able to observe the date of the job separation and the reason for the separation.

The employment data contains anonymized information reported by employers who subscribe to the verification services. They report information on monthly earnings, job locations, job tenures, type of jobs, and industry of employees among other firm level details. The data contains over 5000 employers who report information on all their employees on a payroll-to-payroll basis. The data covers over 100 million employees and is representative of the U.S. labor force along several dimensions, including median personal incomes and median employee tenures. In addition, the data closely tracks aggregate U.S. private sector payroll growth, hiring, and separations. While most industries are represented in the correct proportions, the share of employment in the retail trade industry is significantly higher in the data than in the population. The average firm in the data is also significantly larger than the average firm in the U.S. population. Kalda, 2020a shows that the credit profiles of employees in the data are similar to those of the U.S. population. Both datasets cover periods from 2010 through 2021.

### 3.2.2 Sample and Summary Statistics

Since evaluating our hypotheses requires that we observe both income and job separation reasons, we begin with a sample of employees separated from firms that subscribe to both UI management and verification services. We confine the separations event to be between 2011 and 2018. This allows us to use one year of data both prior to the first and following last separations and to avoid the pandemic period. We then further restrict the separations to be involuntary and fall under two main categories — no fault layoffs and misconduct. Finally, we require the employees to be re-employed within the firms that subscribe to the verification services within 12 months following separation so that we can observe income in the post-separation period. This results in a sample of 455k separations comprising 54k and 401k from finance and non-finance sectors respectively.

[33]The JOLTS program from BLS provides data on job openings, hires, and separations.

The no fault layoffs comprise separations reported under the sub-categories like lack of work, position eliminated, location shut down etc. Misconduct separations include over 25 sub-categories/specific reasons including violation of company policy, removal of company property or funds, gross misconduct among others. Table C1 reports top 10 reasons that account for over 90% of all separations along with their contributions for both the finance and non-finance sectors separately.

Figure 3.1 shows the composition of the separations in our sample (including all sectors) that comprises of 48.1% no fault separations and 51.9% misconduct related separations. No fault separations comprise 60.9% of all separations in the finance sector with misconduct separations comprising 39.1%. The composition is skewed less towards no fault separations in non-finance sectors as they account for 46.4% of all separations while misconduct contributes the remaining. This difference between finance and non-finance is driven by retail trade and accommodation & food that are the only sectors with higher misconduct separations relative to no fault. Other sectors seem to have relatively similar distribution. These patterns emerge from figure 3.2 where we report this distribution between misconduct and no fault separations across sectors in the economy defined by 2-digit NAICS code. Figure 3.3 plots how this composition across finance and non-finance evolves through time in the sample. The plot shows that there are several thousand separations every month throughout our sample period. The total number of separations increases over time because the number of employers subscribing to the UI management services increases over time. However, the distribution of the separations owing to no fault and misconduct remains stable.

To help account for the separations effect, we include up to 20 random employees for each separation company-separation month combination working in the same firm but who do not get separated involuntarily for at least 12 months. This gives us our final sample. Table 3.1 reports summary statistics for pre-separation annual income across six different groups: separated and non-separated employees across all sectors, finance and non-finace sectors.[34] The separated employees are then further categorized into the two reasons of separation — no fault and misconduct. Sections A and B report these stats for the entire sample. The average annual income among separated employees in our sample is just over $70k. Those separated for no fault have higher income than those separated for misconduct. The next sections report similar stats for finance (sections C and D) and non-finance (sections E and F) sectors

[34] For non-separated employees, we use the separation date for their separated counterparts that dictated their inclusion in the sample to calculate pre-separation earnings.

separately. While earnings in finance are higher across all groups, the differences between misconduct and the no fault layoffs are similar across sectors. Income for those separated for misconduct is on average \$49.3k and \$46.6k lower than their no fault counterparts in finance and non-finance sectors respectively. Overall, there are differences in the type of employees separated for misconduct versus no-fault but these differences don't seem to vary systematically across sectors. We take several steps in our analyses to help ensure that these differences do not explain our findings.

### 3.2.3 Sample Validation and Empirical Methodology

Given the filters imposed on our sample, it may not be representative of the US workforce. Not only are the employees in the sample employed in firms that subscribe to both UI management and verification services prior to separation, they also get rehired within firms that subscribe to verification services within 12 months of separation. Since employers that subscribe to these services tend to be larger, employees in the sample may not be representative if those working for larger firms are systematically different. Similarly those re-employed within 12 months at firms subscribing to verification services may be inherently different than those who are not.

To help address this selection, we first replicate results in the literature for no-fault layoffs. We follow JLS — the standard in the literature — and estimate a difference-in-differences specification that compares employees laidoff for no fault to those who continue to be employed before and after separation. Specifically, we estimate the following model:

$$y_{i,j,w,t} = \beta \times Separation_{i,j,w} \times Post_t + \alpha_i + \gamma_{j,t}(\gamma_t) + \delta_{w,t} + \epsilon_{i,j,w,t} \tag{3.1}$$

where $y$ measures log earnings for employee $i$ in industry $j$ with income in wage bin $w$ at year-month $t$. The industry $j$ is defined at the 6-digit NAICS code level and wage bins $w$ are of \$1,000 width. *Separation* is a dummy variable that takes a value of one for employees who get separated at some point in our sample, and *Post* is a dummy variable that takes a value of one during the months following separation. $\alpha_i$ denotes individual fixed effects that control for time-invariant individual level differences, $\gamma_{j,t}$ indicates industry x year-month fixed effects and accounts for all time varying changes at 6-digit NAICS level, and $\delta_{w,t}$ represents wage-bin ($1,000 bins) time effects that control for all time varying differences across employees with different income levels. Robust standard errors are clustered at the employee level.

Table 3.2 reports estimates for this analyses. Instead of only showing the results with the fixed effects included the JLS model, we report three different specifications to evaluate the robustness of the findings. While column (1) includes only individual and year-month fixed effects, column (2) includes individual and industry x year-month fixed effects and column (3) in addition adds wage bin x year-month fixed effects. Across all specifications, we find that employees laid-off for no fault experience significant income declines relative to those who continue to be employed. The estimates of the results are close to each other varying between 28.5% and 29.4%. These magnitudes are similar to those documented in the literature as most papers find a decline between 25% and 30% [e.g.,][]couch2001, jacobson2005,couch2011, moore2019.

To assess the time dynamics of the effects on income and explicitly test whether trends between the separated and non-separated groups are parallel before separation, we estimate the dynamic version of Equation 3.1 given as follows:

$$y_{i,j,w,t} = \sum_{\substack{k=-8 \\ k\neq-4}}^{16} \beta_k \times Separation_{i,j,w} \times D_k + \alpha_i + \gamma_{j,t}(\gamma_t) + \delta_{w,t} + \epsilon_{i,j,w,t} \tag{3.2}$$

where $D_k$ is an indicator that equals one $k$ quarters to or from separation. All other variables are same as defined earlier. The omitted baseline category is fourth quarter prior to separation (i.e., quarter one year prior to separation). The coefficient of interests are $\beta_k$ where each of these coefficients captures the differential response of income for separated employee relative to the non-separated ones. Figure 3.4 plots estimates for this analysis along with 99% confidence intervals. We find that while income trends similarly between laid-off employees and those who continue to remain employed prior to separation, the decline occurs sharply at the quarter of separation and remains persistent with income for separated employees being 23% lower four years following separation. Both the absence of pre-trends and long-lasting effects with the magnitude four year post separation are similar to the patterns documented in the literature [e.g.,][]moore2019.

## 3.3 Main Results

In this section, we evaluate changes in income following involuntary separations owing to misconduct relative to the separations effect in the finance sector, and contrast them to analogous relative changes in non-finance sectors.

### 3.3.1 Income following Misconduct Separations

Our baseline analysis estimates income following misconduct separations and no fault layoffs separately using difference-in-differences equations and compares the two coefficients. This yields estimates similar to a triple interactions specification and allows us to account for the separations effect. Specifically, we estimate the JLS specification described in Equation 3.1 that compares income for employees involuntarily separated for misconduct (or no fault) to those who continue to remain employed. We also employ a more stringent specification that includes additional fixed effects namely firm x 3-digit zipcode x year-month fixed effects that allow us to compare employees separated from the same firm residing in the same geographic region thereby controlling for all time-varying differences at the separated firm-3 digit zipcode combination (e.g., economic conditions, firm level time varying policies, local job market characteristics that affect employees working in certain industries or firms etc.), and tenure x year-month fixed effects that account for time varying differences across employees with different tenure in years. We conduct this analyses separately for the finance and insurance sector defined by the NAICS code of 52 and non-finance sectors that include all other industries.

Table 3.3 reports estimates for these analyses where the differences in two difference-in-differences coefficients (i.e., those associated with misconduct separations and no fault layoffs) reported in the bottom row are the coefficients of interest. While columns (1) and (2) focus on the finance and insurance sector, the final two columns report estimates for the non-finance sectors. Column (1) reports results for the specification with the same fixed effects as JLS while column (2) reports estimates for the more stringent specification. Across both specifications, we find that employees involuntarily separated for misconduct earn higher income relative to those laid off for no fault in the finance sector. The magnitudes correspond to 7.7% and 3.9% higher incomes respectively. This occurs because employees separated for misconduct experience 12.4% (=0.039/0.314) to 25.5% (=0.077/0.301) lower income declines when compared to their no fault layoff counterparts. In sharp contrast, estimates for non-finance sectors reported in columns (3) and (4) show that those fired for misconduct earn 4.8% to 8.1% lower income post separation than those experiencing no fault layoffs.

Analogous to the results in table 3.3, we evaluate pre-trends and long-run dynamics in income between employees separated for different reasons — misconduct and no fault — and their counter parts who continue to remain employed, across both finance and non-finance sectors. We estimate equation 3.2 separately for these four sub-samples. While figure 3.5 plots the estimates for misconduct separations, figure 3.6 shows the coefficients for no fault ones. Panels (a) and (b) of figure 3.5 show these results for finance and non-finance sectors respectively. Across both sectors we find that while income trends similarly between employees separated for misconduct and those who continue to remain employed for two years prior to separation, the decline occurs sharply at the quarter of separation and remains persistent with income for separated employees being significantly lower four years following separation. We find similar patterns for employees separated for no fault from both the finance and non-finance sectors in panels (a) and (b) of figure 3.6 respectively.

### 3.3.2 Is Finance Unique?

A natural question at this point is whether finance is the only sector where employees separated for misconduct experience higher changes in wages than those separated for no fault or are there other sectors where we may find similar patterns. We examine this by re-estimating our main triple interaction coefficient for all major sectors in the economy (i.e., all industries with different 2-digit NAICS codes). Figure 3.7 plots these estimates for all sectors in the economy except Agriculture and Public Administration as there are either no or very few employers from these two sectors covered in our sample.[35] Though there is significant heterogeneity across different sectors, those involuntarily separated for misconduct earn higher income relative to those separated for no fault layoffs only within the finance and insurance sector.

[35] A list of all sectors based on 2-digit NAICS code is available through census using this link: https://www.census.gov/programs-surveys/economic-census/year/2022/guidance/understanding-naics.html

### 3.3.3 Do differences across Misconduct Employees and others drive our results?

A concern in our setting is that differences across employees involuntarily separated for misconduct versus no fault or non-separated employees may drive our results. For example, as discussed in section 3.2.2, the mean pre-separation income for misconduct employees is lower than those experiencing no fault layoffs and non-separated employees. Alternatively, higher performing employees or those with better outside options may be more likely to commit misconduct. To account for such differences across employees, our baseline specification saturates the difference-in-differences model with multiple fixed effects to non-parametrically account for a number of factors including time varying differences in industry, income, job tenure, and firm-location combinations, and time invariant differences across individuals. For example, among other non-observables these include differences in time-varying skills and income, inherent capabilities, regulations faced by pre-separation employers, and differences in regional and industry job market conditions employees experience. The absence of pre-trends in income across employees separated for misconduct (or no fault) and those who continue to remain employed provides some reassurance that our specification properly accounts for pre-separation differences.

In addition, we re-estimate our baseline results with a collapsed triple interaction specification that allows for the estimation of fixed effects for the entire sample simultaneously instead of separately across different sub-samples. If covariates included in our fixed effects are correlated to the reason of separation in ways that affect our estimates, the collapsed triple interaction coefficients are likely to yield different results from the baseline. This is because while the baseline allowed the fixed effects to interact with the separation reason, the collapsed regression does not. Specifically, we estimate the following model:

$$\begin{aligned} y_{i,f,j,z,w,\tau,t,c} = {} & \beta \times Misconduct_{i,f,j,z,w,\tau,c} \times Separation_{i,f,j,z,w,\tau,c} \times Post_t + \Gamma \\ & \times Separation_{i,f,j,z,w,\tau,c} \times Post_t + \alpha_i + \delta_{w,t} + \gamma_{f,z,\tau,r} + \theta_{c,r} + \epsilon_{i,j,w,t} \end{aligned} \tag{3.3}$$

where $y$ measures log earnings for employee $i$ working for firm $f$ in industry $j$ with income in wage bin $w$ and tenure as $\tau$ years and residing at the 3-digit zipcode $z$ at year-month $t$ who got separated with separation cohort $c$. The industry $j$ is defined at the 6-digit NAICS code level and wage bins $w$ are at \$1,000 width. *Misconduct* is an indicator variable that equals one for employees who involuntarily separated for misconduct. $Separation$ is a dummy variable that takes a value of one for employees who get separated at some point in our sample either for misconduct or no fault, and $Post$ is a dummy variable that takes a value of one during the months following separation. $\alpha_i$ denotes individual fixed effects that control for time-invariant individual level differences, $\delta_{w,t}$ represents wage-bin (\$1,000 bins) time effects that control for all time varying differences across employees with different income levels, $\gamma_{f,z,\tau,r}$ represents firm x 3-digit zipcode x tenure x year fixed effects that account for any time varying differences at firm-location-employee tenure levels, and $\theta_{c,r}$ denote separation cohort x year fixed effects that control for time varying differences across employees separated at different year-months. Robust standard errors are clustered at the employee level.

Table 3.4 reports the estimates for these results separately for the finance and non finance sectors. While the first column reports results for the specification that uses same fixed effects as the JLS model, the second column reports estimates using Equation 3.3. The estimates in column (1) show that employees separated for misconduct earn 7.5% higher income than those laid off for no fault. The estimate reduces with the more stringent specification to 2.8%. In contrast, in non-finance sectors those separated for misconduct earn 4.4% to 6.9% lower income relative to those experiencing no fault layoffs. Though we use even more stringent fixed effects that are estimated for the entire sample simultaneously instead of separately across different sub-samples, we find our estimates to be very similar as before both qualitatively and quantitatively. This further helps reassure that differences between employees experiencing misconduct separation and others are less likely to explain our findings.

While we rely on fixed effects to account for differences across different types of employees in our baseline analyses, we also re-estimate our results for matched samples. The use of fixed effects allows us to flexibly control for a number of dimensions non-parametrically. Using these many dimensions in a matching technique (e.g., propensity score) may lead to inefficient matches if the number of observations within each bin is not large enough. We construct two different matched samples to evaluate this plausibility in our setting where the first sample matches on pre-separation income and the second on pre-separation income within the same separating firm, tenure, and location (measured as 3-digit zip code). Table C2 compares pre-separation income for different types of employees for the two matched samples. While differences in income reduce significantly across employees for the sample that matches only on income, the improvement is limited for the other sample. This is because of insufficient number of similar observations within granular bins across separation types.[36] Nonetheless, we re-estimate the collapsed triple interactions regressions for both matched samples and report the results in Table C3. Across both types of matched samples, we find results similar to our baseline.

One plausible reason through which differences between those separated for misconduct versus no fault arise is that firms may choose to let go less productive employees when they are forced to layoff employees. Conditional on laying off if firms in finance sector are more likely to let go worse performing employees than other sectors, the market may recognize it and penalize them more than those separated for misconduct. We address this concern by using no fault separations that were part of mass layoffs as firm discretion is less likely to play a role when firms let go of significant portions of their workforce [e.g.,][]jacobson1993, moore2019, braxton2022a, braxton2022b. Following the literature, we define a mass layoff to have occurred when a firm involuntarily separates at least 20% of its employees for no fault between two consecutive quarters. We then re-estimate our triple interactions specification using two types of involuntary separations: misconduct and mass layoffs. Table 3.5 reports estimates for this analysis where we find similar results to our baseline even with this sample less subject to firm discretion.[37]

Another reason why employees separated for misconduct may be different from those separated for no fault could be that both perform different types of jobs. If employees working in certain types of jobs are more prone to engage in misconduct, it could lead to inherent differences across both types. To address this concern, we re-estimate our baseline coefficients after further controlling for the type of job within the same firm. Specifically, we add firm-job title time effects to our baseline model and report the results from both the baseline (columns (1) and (3)), and the augmented model (columns (2) and (4)) in Table C5. Including the baseline helps facilitate comparisons across both the model and estimates. We find the results to be very similar to that of our baseline suggesting that heterogeneity in the type of jobs likely doesn't affect our results.

[36]For example, our sample contains 21,114 employees who were separated for misconduct in finance. Hence, those within the same firm, tenure, and location are likely to be limited.

[37]We also redo our validation exercise using the JLS specification to estimate the effect of separation owing to mass layoffs across all industries on income. Similar to before, we find results consistent with JLS as reported in Table C4.

Finally, we rely on the heterogeneity in our findings to help address that any remaining differences not controlled for in the analyses so far may be driving our results. First, since we find contrasting results in finance and non-finance sectors, differences across employees separated for misconduct and no fault cannot explain our findings unless they systematically vary across sectors. Second, we conduct a number of heterogeneity analyses even within sectors and find our results to be concentrated within certain sub-subsamples. We examine the role of both the firms and jobs employees get separated from and rehired at in our setting. We first evaluate heterogeneity based on whether or not employees get rehired within the sector they get separated from. Table C22 reports estimates for these analyses where we report results for those separated from finance and whether or not they get rehired within finance in columns (1) and (2) respectively. Columns (3) and (4) report analogous estimates for those separated from non-finance sectors and whether or not they get rehired within the same sector. It is worth noting that the counterfactuals (i.e., employees separated for no fault) also either stay within or depart from the same sector and hence account for any systematic differences between employees who stay within their previously employed sector versus those who leave, and the nature of the move itself. For instance, if changing industries implies higher income losses, the counterfactual would account for the differential. That we find our results to be concentrated (stronger) among employees rehired within their previous sector for finance (non-finance) suggests that differences across employees separated for misconduct and no fault can only explain our findings if they vary both across sectors and within sectors based on rehiring industries. We also find similar or stronger heterogeneity along a number of dimensions as detailed in sections 3.4 and 3.5.

Taken together, the results suggest that differences in employees separated for misconduct and no-fault or non-separated employees are less likely to explain our findings.

## 3.4 Mechanisms

In this section we investigate what drives the differential income for employees separated for misconduct versus no fault.

### 3.4.1 Assortative matching in labor markets

Our results show that finance employees involuntarily separated for misconduct experience higher income changes than those separated for no fault. One explaination for this potentially surprising finding could be assortative matching in the labor markets where firms more likely to engage in misconduct may be more likely to hire employees with misconduct background either because these individuals have shown a disposition towards such activities themselves or misconduct background is correlated with certain attributes valued by such firms (e.g., higher risk taking). If firms vary in their involvement in such activies, they may match with employees with different tendencies towards such behaviors. For example, if firms are more likely to adopt high pressure sales tactics they may be more inclined to hire sales employees willing to sell products that may not be in the best interest of the consumer. To the extent firms value such matches, they may be willing to pay a wage premium for them.

We evaluate this hypothesis using three different measures that capture firms' likelihood to engage in misconduct themselves. While the first two come from the complaints data maintained by the consumer financial protection bureau (i.e., CFPB), the third comes from Violation Tracker that captures all instances of corporate misconduct. The CFPB was established as a watchdog of financial services industry in 2010 as part of the Dodd-Frank Act. Among other things the bureau provides an avenue for consumers not satisfied with the services they receive to lodge complaints against financial institutions. They can do this in a number of different ways including the bureau's online system, email, postal mail, fax, phone, or through a referral from other agencies. These complaints are typically major serious allegations or issues that could not be resolved between the consumer and the firm begley2021. This is further elaborated by the fact that the CFPB uses these complaints and their resolutions as an input in its enforcement decisions, and has fined almost $10 billion to firms since its inception. In their complaints, individuals provide information on the products and detailed accounts of events that led them to file a complaint along with the firm's name. We focus on fraud-related complaints. In particular, we manually examine the description of issues reported in the data and focus on keywords such as misleading, crime, privacy, fraud, wrong amongst others to classify complaints as being fraudulent. Some examples of the categories of complaints classified as fraud in our analysis include account opened as a result of fraud, fraudulent loan, attempt to collect wrong amount, high pressure sales tactics, overcharged, didn't receive services advertised, confusing or misleading advertising, etc. We then aggregate this data to capture the total number of complaints received against a particular firm over our sample period.

Using this aggregated data we create two different proxy measures for the likelihood that the firm is involved in potentially fraudulent related activities. First, we use the fraction of fraud related complaints of the total number of complaints against the firm. We use the proportion to account for the size and the type of clientelle that firms cater towards. For example, consumers for different types of products may have different tendencies to complaint. Second, we use the timely response rates for firms. When the CFPB receives complaints, it sends them over to the firms giving them an opportunity to reach out to the consumer and resolve the issue within a given time frame. Timely response rate captures the tendency of the firms to resolve consumer complaints or issues. We split the firms into above and below median levels based on both measures and find the tendency to be persistent over time. Figure 3.8 shows this graphically where we plot the averages across the two groups based on both measures and find the differences to exist from 2012 through 2022.

To further account for the plausibility that complaints can be driven by both the size of the firm and the type of products or services firms provide (e.g., some may be more consumer facing than others; different products may affect consumers differently etc.), we control for both firm size and 6-digit NAICS code for the hiring firm in analyses that use these measures. We first examine whether our results vary for employees rehired within finance by firms with different levels of complaints or non-timely response rates[38]. Table C6 reports results for this analysis. Columns (1) & (2) report results for employees rehired by firms with above & below median levels of fraud related complaints respectively. We find stronger results for employees rehired by firms with higher levels of complaints. Among employees rehired by such firms, those separated for misconduct earn 6 percentage points (pp) higher income relative to their no fault counterparts. In contrast, among employees rehired by firms with below median levels of complaints those separated for misconduct experience 5.4% higher relative income. Columns (3) and (4) report results for employees rehired by firms with above & below median levels of non-timely response rates and find similar results. While misconduct employees rehired by firms with higher levels of non-timely responses earn 7.8% higher income relative to their no fault counterparts this difference amounts to 5.5% for those rehired by firms with lower levels of non-timely responses.

[38]We use the complement of timely response rate (i.e., 1-timely response rate) to make it consistent across the two measures that higher values represent undesirable characteristics of firms.

The next part of our analysis hypothesizes that assortative matching should lead to asymmetric results depending on which type of firms employees are separated from and who rehires them. Conditional on employees being separated from firms with below median levels of complaints and rehired by those with above median levels of complaints, the matching improves for those separated for misconduct but not necessarily for those separated for no fault. However, this differential does not exist for employees with reverse job switches. We test this plausibility and report the results in Table 3.7. Consistent with the hypothesis, we find that our results are concentrated for the pool of employees separated from employers with below median levels of complaints (or non-timely response rates) who moved to employers with above median complaints (or non-timely response rates). Columns (2) and (4) report estimates for this group where we find that those separated for misconduct earn 9.1% and 9.9% higher relative income respectively. In contrast, among those who switch jobs from employers with above median to below median levels of complaints or non-timely response rates earnings are statistically indistinguishable between those separated for misconduct and no fault. Similar patterns emerge for match rates and tenure with the new employer. Among those separated from firms with below median levels of complaints, 49% of those separated for misconduct and rehired within finance were rehired by employers with above median levels of complaints. In contrast, 33% of those separated for no fault made a transition from firms with below to above median levels of complaints.[39] Following switching from employers with less to more complaints, tenure for employees with misconduct background are 10%-15% higher than those with opposite moves.

Since the CFPB complaints database only covers firms within the finance sector, our first two measures based on this data don't allow us to examine the assortative matching hypothesis for non-finance sectors. To overcome this issue, we augment our analysis with another measure based on data from Violation Tracker, a comprehensive database on corporate misconduct. This data are collected from more than 400 agencies and contain over 560,000 civil and criminal cases brought against firms since 2000, the beginning of the coverage period. Since different agencies have oversight over different sectors, the advantage that this data offers is that it covers multiple sectors from our sample including finance and insurance. Violation Tracker removes violations in which the penalty or settlement is lower than $5,000, hence keeping only more serious violations. Heese et al., 2022 provide a more detailed overview of the data. We aggregate this data to capture the total penalty levied on firms during our

[39]The remaining 18% move to firms not covered in the CFPB complaints database.

sample period. Using this aggregated data we classify firms that have received penalties as those more likely to engage in corporate misconduct versus those that did not receive any fine greater than $5,000.

We use this measure and test whether our results are stronger for employees separated from firms less likely to engage in misconduct who get rehired within firms more likely to do so relative to those who make the opposite moves. Conditional on employees being separated from firms that did not receive any penalties and rehired by those who did, the matching improves for those separated for misconduct but not necessarily for those separated for no fault. However, this differential does not exist for employees with reverse job switches. We test this plausibility and report the results in Table 3.8. Columns (1)-(2) report results for the finance sector and columns (3)-(4) report them for the non-finance sectors. Consistent with the analysis with other two measures, we find our results to be stronger for employees who move from zero to non-zero penalty firms relative to those making opposite moves in the finance sector. However, in sharp contrast, we do not find any heterogeneity across these moves for the non-finance sectors.

The assortative matching relates to both firms and employees being of certain type. While employers in our tests are categorized into different types based on three different measures, employees are categorized based on whether they are separated for misconduct. Another measure that likely identifies the type of employees even more closely is whether an employee is a 'repeat offender.' Those separated for misconduct on more than one occassion during our sample period between 2011-2018 are potentially even more likely to engage in such activities at the new job. We find that 18% of all misconduct employees are repeat offenders in our sample. We find three different results consistent with assortative matching. First, repeat offenders are significantly more likely to move from firms with low- to high-fraud related complaints than to make opposite moves. Among those separated for misconduct and moving from low- to high-fraud firms, 23% are repeat offenders compared to less than 13% of those making reverse moves. Second, we find stronger results for repeat offenders relative to one-time offenders as reported in Table C7. Finally, we redo our analysis for assortative matching and find stronger results for repeat offenders than those separated for misconduct only once. Table C8 reports these results where the difference between columns (1) and (3) to be starker than that between columns (2) and (4).

Overall, our results are consistent with assortative matching in the finance labor market where employers with higher propensity to engage in misconduct pay a wage premium for employees with a misconduct background. However, these patterns do not show up for non-finance sectors.

**What makes finance unique?**

While several factors may contribute towards making finance unique, one feature that we propose is that most products and services offered in the sector are based on future cash flows. This likely makes it difficult for consumers or other stakeholders to disentangle "bad luck" from a deliberate risky or unethical transaction in case of losses, thereby reducing the expected costs of engaging in such behavior for both firms and employees. This may in turn increase expected profits for firms to engage in such behavior in finance and induce assortative matching in the labor markets as they pursue these agenda.

We conduct a number of analyses to evaluate this hypothesis. Even within the finance sector employees working in jobs directly related to financial products and services may have greater opportunity to exploit this feature relative to other employees. To test this differential, we use job titles and categorize jobs within the finance sector into finance-related and non-finance jobs. We manually classify the most common 500 job titles into three categories — finance, non-finance, and ambiguous. We adopt a cautious approach and include titles that are not clearly finance or non-finance into the ambiguous category. While some examples of finance related jobs include banker, loan officer, financial advisor, etc., those in non-finance jobs include software engineer, application developer, customer service representative etc. Using this categorization, we re-estimate our findings across employees separated from finance-related versus non-finance jobs within the finance sector. Table 3.9 reports estimates for this analysis. We find our results to be concentrated for employees separated from finance-related jobs and absent for non-finance jobs within the sector.

Firms more likely to engage in misconduct, especially owing to the products and services being based on future cash flows, may cater towards more unsophisticated consumers who are easier to be ensnared egan2019. This implies a segmentation in the finance sector with a section of firms more likely to engage in misconduct and cater to unsophisticated consumers but not in other sectors. We examine this differential based on whether rehiring establishments are more or less likely to cater to unsophisticated consumers determined by the demographics of population residing nearby. We follow the extant literature and use education levels and elderly age as proxies for consumer sophistication, and estimate heterogeneity in our findings based on the percentage of college educated and elderly population residing near rehiring establishments. Since our earlier estimates show that finance jobs within the finance sector drive our baseline results, we evaluate these heterogeneity separately for finance job profiles and non-finance jobs within the finance sector. Table 3.10 reports these results where we find that for finance job profiles (panel A), our results are concentrated among zip codes with lower levels of college educated population and stronger in areas with higher levels of elderly population (defined as 65 years and older in age). In sharp contrast, there are no significant differences in income changes for employees separated from non-finance jobs based on either variable (panel B). Since these tests focus on the type of consumers, one would expect stronger heterogeneity for employees involved in consumer-facing jobs. We examine this plausibility based on whether employees are directly involved in sales and report results in Table C9. Supporting the idea that employees interacting with consumers should be more affected we find this heterogeneity to be stronger for sales professionals (panel A) relative to non-professionals (panel B). Further, we detect no heterogeneity based on consumer sophistication measures for non-finance sectors as reported in Table 3.11. Taken together, these results are consistent with a segmentation in the finance sector where firms more likely to cater towards unsophisticated consumers are also more likely to pay a wage premium for employees separated for misconduct, especially those in finance- and sales-relatd jobs. However, we do not find such patterns in non-finance sectors.

Another plausibility consistent with our findings may be that the inherent culture in the finance sector may be systematically different than other sectors. For instance, the literature has shown that finance is unique in a different aspect: it is a high-skill and high-wage sector and returns to talent in finance have substantially increased over the years relative to other sectors [e.g.,][]philippon2012, Celerier2019. Similarly, it can help explain our results if the sector is also unique in a characteristic (e.g., subscribing to "success at all costs" mentality) that bolsters the net returns to risky or potentially unethical/fraudulent behavior [e.g.,][]Gill2022.

### 3.4.2 Differences in regulation

Regulation may affect how firms react to employee misconduct. Higher regulatory costs in the finance sector may incentivize firms to let go employees even for minor offences and errors of judgement. The labor markets may recognize this and undo part of this 'abnormally strict' internal punishment subsequently. This may be less likely for other sectors with less stringent regulation. We evaluate this hypothesis in our setting using heterogeneity at both intra- and inter-sector levels.

At intra-sector level, we estimate the heterogeneity in our findings based on severity of regulation faced by different sub-sectors within the finance and insurance sector. The RegData provides information that helps quantify the size and scope of regulations affecting different sub-sectors. Based on the 3-digit classification, the sub-sector that houses firms in credit intermediation and related activities receives the most amount of regulatory scrutiny with over 60,000 regulatory restrictions imposed on the sector as of 2016.[40] We estimate the heterogeneity in our findings across firms in the credit intermediation sector and those operating in other sub-sectors. Table 3.12 reports the results for this analysis where column (1) reports results for employees separated from the credit intermediation sector while column (2) reports estimates for all other employees. We find similar results across the both groups.

Further, at the inter-sector levels our estimates do not seem to systematically vary with the extent of regulation. For instance, finance and insurance, health care, and utilities are amongst the most regulated sectors in the economy. Yet estimates plotted in Figure 3.7 show that while banking and insurance, and utilities are on the opposite ends of the spectrum, health care is statistically indistinguishable from less regulated sectors like retail trade, waste management etc. Overall, we find no evidence supporting the hypothesis that differences in regulation across sectors may explain our findings.

### 3.4.3 Job search

Another plausible mechanism consistent with our findings may be that workers separated for misconduct in finance search longer for jobs which allows them to find higher paying jobs.

[40]This link provides more information on the heterogeneity of regulation across sub-sectors: https://www.mercatus.org/publications/regulation/regulatory-accumulation-financial-sector

This can especially be true for high income employees who potentially have more resources to help smooth consumption while searching longer. We evaluate this plausibility by examining the time it takes for employees across different separation categories in our sample to find re-employment. Table C10 reports average time to re-employment measured in months by different income categories for the finance sector. For employees in bottom 90% of the income distribution, those separated for misconduct take 11.1% less time to find re-employment relative to those separated for no fault. Specifically, the former on average take 4.8 months relative to 5.4 months for the latter. Similar patterns hold even for high income employees belonging to the top decile of income distribution.

Not only do employees separated for misconduct in finance find jobs quicker, they are also more likely to find jobs within the finance sector. Table C11 reports industry departure rates from the finance sector for employees involuntarily separated for misconduct versus no fault. Panel A (B) reports the percentage of employees departing the sector at some point within two (four) years following separation. Across both horizons, we find that employees separated for misconduct are less likely to leave the finance sector than those separated for no fault. Taken together, these estimates are inconsistent with the hypothesis that differences in job search across employees separated for misconduct and no fault in finance explain our findings.

### 3.4.4 Types of misconduct across sectors

Our results can potentially be explained by differences in the type of misconduct across different sectors. Independent of the opportunities to camouflage misconduct related activities, if misconduct in finance comprises of activities positively correlated to firm profits (e.g., high pressure sales tactics) and those in non-finance sectors negatively correlate to profits (e.g., using incorrect parts during production), rehiring firms may react to employees engaged in them differently.

To help evaluate this mechanism, we first examine whether there are differences in the type of reported misconduct across sectors. Table C12 reports this distribution for the top 10 misconduct reasons across finance and non-finance sectors further split by firm size. The distributions show that the ordering of misconduct reasons from most to least common remains fairly stable across the four sub-samples. Further, the contribution of each reason is also comparable across sub-samples except for improper conduct, misconduct related performance, and gross misconduct. We also plot the distribution of top three misconduct reasons — company policy violation, improper conduct, and misconduct related performance — across sectors in figure AC1. The ordering of the three reasons and share seems fairly similar across sectors with the exception of the infomration sector. Next, we examine heterogeneity in our findings by the type of misconduct reason reported by splitting the sample into those separated for violation of company policy versus all others.[41] Table C13 reports our baseline results for these sub-samples where we find similar results across different types of misconduct.

While these results suggest that different types of misconduct likely do not explain our findings, we conduct a couple of more analyses to further help establish this. We re-estimate our findings within the same job type but across different sectors: sales professionals. To the extent that employees involved in sales jobs are likely to be involved in similar type of misconduct (e.g., adopting aggressive sales strategies like lying to clients, overselling etc.), the issue is less severe for this sub-sample. Table C14 reports results for these estimations where we find patterns similar to our baseline. Finally, we repeat this analysis for sales professionals separated for the same reported misconduct — company policy violations — across sectors, a sub-sample even more likley to be involved in similar type of misconduct. Table 3.13 reports these estimates where again we find results similar to our baseline: while those separated for misconduct earn higher income relative to no fault separations in finance, opposite occurs for other sectors.

### 3.4.5 Differences in scapegoating

Finance firms and executives may be systematically more likely to be the perpetrators of such activity instead of rank-and-file employees, but may blame it on their employees and let them go to create scapegoats. Other firms within the sector may know this, and not attach a discount to prospective hires with a history of 'misconduct.'

We conduct a couple of tests to evaluate this alternative. If finance firms are more likely to engage in scapegoating, they are likely to have higher incidence of unjust dismissals and retaliation from separated employees in the form of lawsuits. We test whether finance has higher labor related lawsuits using data from the federal judicial center and report results in

[41] We do not have enough observations in most of these sub-categories to estimate our triple interaction coefficients separately for them.

Table C15. However, we find that the finance sector does not have a high proportion of labor related lawsuits among the Fama-French 12 industries. Notwithstanding this result, we also examine the heterogeneity in our findings based on the seniority of the separated employees in their pre-separation firms. If scapegoating plays a role in explaining our results, one would expect to find stronger results for junior employees. We classify employees previously employed with job titles that earn above (below) median income as seniors (juniors) and estimate our triple interactions separately for these groups. Table C16 reports these results where inconsistent with the scapegoating hypothesis we find similar results across all types of employees.

### 3.4.6 Job performance & selection into misconduct

Yet another alternative mechanism may be that separation owing to misconduct affects job performance by incentivizing employees to work harder in their new jobs. However, this is unlikely to explain our findings unless it varies systematically across finance and non-finance sectors. Nonetheless we re-estimate our baseline analysis to evaluate rehiring earnings for employees separated for misconduct versus no fault. Employees' job performance post getting rehired is less likely to affect their income at the time of rehiring. Table C17 reports these estimates where we find results consistent with our baseline suggesting that changes in job performance are unlikely to explain our entire result.

The final alternative that we consider is that different types of employees may select into misconduct across different types of sectors in a way that the differences between misconduct and no-fault employees systematically vary by sectors. For instance, those with better outside options relative to no fault counterparts or relatively more willing to lie may be likely to engage in misconduct in finance but not in other sectors. While consistent with our baseline findings, this argument cannot explain the heterogeneity in our findings across numerous dimensions including which firms employees get separated from and where do they get rehired (good vs bad firms), characteristics of hiring firm establishments, repeat vs one time offenders, and the type of jobs among others. Hence it is unlikely to drive our results by itself.

## 3.5 Other Robustness

While presenting our results we have discussed at length one of the main concerns in our analysis: differences in employees separated for misconduct versus no fault or non-separated employees and the steps we took to help address this concern. In this section, we describe some other potential limitations and the robustness of our findings.

### 3.5.1 Misconduct measure

Our measure of misconduct comes from the employer reported reason of separation where it is explicitly stated as misconduct. In addition, the employers also report a more detailed description and classify misconduct separations into over 25 sub-categories including violation of company policy, improper conduct, and gross misconduct among others. Table C1 reports top 10 reasons that account for over 90% of all separations along with their contributions for both the finance and non-finance sectors separately. While our data and setting have several strengths and are rich along a number of dimensions, one limitation is that these sub-categories describing the reasons for misconduct might not be very informative given that the distribution of our sample is skewed towards less informative sub-categories like violation of company policy and improper conduct. This restricts our ability to observe the exact type of misconduct covered in our sample.

We overcome this limitation by examining indirect evidence through different sub-sample analyses also discussed in earlier sections. First, our heterogeneity estimation based on whether employees work in finance-related versus non-finance jobs within the finance sector helps us evaluate the extent to which financial misconduct and offences are captured by our measure. That we find our results to be concentrated among employees working in finance-related jobs suggests that our measure is able to capture financial misconduct along with other types. Second, we estimate the heterogeneity in our findings across different types of reported reasons by splitting the sample into those separated for violation of company policy versus all others and find similar results across different types of misconduct. Third, we re-estimate our findings within the same job type separated for same reported reason but across different sectors: sales professionals separated for company policy violation. We find results similar to our baseline for this sub-sample.

We augment this analysis with externally available public data to further shed light on what our misconduct measure captures. We use data from the Financial Industry Regulatory Authority's (FINRA) BrokerCheck database that includes employment history and any disclosures filed, including information about customer disputes, whether these are successful or not, disciplinary events, and other financial matters. Several papers have used this data before including Egan et al., 2019, Kleiner et al., 2021, and Egan et al., 2022 among others. We obtain this information for our sample period between 2010-2018. While this data only covers financial advisors, it includes more granular information on the type of misconuduct through the disclosures that are reported as 23 categories. For a sub-sample of employees who work as financial advisors in our sample, we are able to merge this data and obtain this measure of misconduct. Our merge yields 1,826 advisors that were separated for misconduct in our sample and successfully merged with the BrokerCheck data. We then examine the disclosures on these advisors' record from 12 months prior to their separation. We find that 99.4% (i.e., 1,816) of them have disclosures within this duration. While over 40% of these disclosures are related to consumer dispute; criminal, employment separation after allegations, financial, judgment and regulatory constitute the other main contributing categories. Figure AC2 plots the distribution of disclosure categories within the matched sample which includes 9 of the 23 reported categories. These 9 also include the categories used in Egan et al., 2019 to define misconduct, thereby further providing reassurance that our measure is able to capture financial misconduct. We also re-estimate our baseline analysis using the matched financial advisors as employees separated for misconduct, and all observations from the no fault and non-separated employees included in our baseline sample. Table C18 reports estimates for this analysis. Though we are not able to include all fixed effects as our baseline owing to the small number of observations for misconduct employees in this sub-sample, we find results similar to the baseline estimates for the finance sector.

While there are some reported reasons that should not be preferrable for potential employers (e.g., removal of company property, unauthorised use of company credit card etc.), we do not have enough number of separations in these sub-categories to examine heterogeneity based on whether or not the misconduct separation reason is potentially unacceptable for hiring employers. However, we split the sample based on top three reasons versus all others and compare average industry departure rates. Since these analyses do not include the fixed effects as our baseline specifications, they are less demanding in terms of the number of separations required for estimation. Table C19 reports these results where we find the industry departure rates to be significantly higher following misconduct separations which include the potentially undesirable categories, i.e., outside of the top-3 categories.

Overall, the results in this section in combination with the ones discussed in section 3.4.4 suggest that our measure captures wrongdoing/misconduct including financial misconduct for the finance sector, and our inability to granularly capture the exact type of misconduct likely doesn't affect our interpretations.

### 3.5.2 Job finding rates

Our sample consists of employees who get rehired within firms that subscribe to verification services within twelve months of separation. This creates two potential issues. First, there could be selection in who gets rehired that can bias our findings. Our replication and sample validation exercise discussed in Section 3.2.3 helps address this concern. Second, if those separated for misconduct are much less likely to find a job, our interpretation of lack of external punishment in finance based on the income results from our sample may be misleading. We overcome this second issue by examining drop out rates from our sample by reason of separation across sectors.

Since we cannot directly measure the job finding rates as employees may drop out of our sample either because they did not find a job post separation or found one at a firm not covered in our data, we measure the drop out rates and report them in Table C20. We find that across all sectors those separated for misconduct are about 14% less likely to drop out of our sample relative to no fault separations. This large difference seems to be driven by the finance sector as there is considerable heterogeneity in the difference in drop out rates between misconduct and no fault across sectors. While those separated for misconduct are 15% less likely to drop out relative to no fault counterparts in finance, they are only 4.2% less likely to drop out in non-finance sectors. Taken together with the earlier results that those separated for misconduct find jobs faster, these findings further support our interpretation of lack of external punishment in finance.

### 3.5.3 Sample attrition and missing income

In addition to dropping out of our sample at the time of separation, employees are also allowed to drop out at any point they become self-employed, unemployed, or switch to an employer not included in our data.[42] To examine the implications of missing earnings for our results, we follow the approach used in Graham et al., 2023 who address a similar issue. Specifically, we use imputed income for employees who drop out in two different approaches. First, for all three employee groups — separated for misconduct, no fault layoff, and non-separated — we replace missing earnings with the first percentile value of the earnings distribution in our sample. Similar to Graham et al., 2023, this approach essentially assumes that individuals who disappear from the our sample are unemployed. Alternatively, we replace missing earnings with the maximum monthly earnings in the last year the individual appeared in the our sample. This approach effectively assumes that those who disappear move to work in firms not covered in our data or become self-employed and earn the same wages as before. Table C21 reports results for these analyses where across both imputations we find results consistent with our baseline.

### 3.5.4 Robustness to different sub-samples, clustering, and outliers

Our final set of analyses evaluate robustness of our findings to different sub-samples, clustering, and outliters. We begin by re-estimating our baseline results separately for employees rehired by firms located in the five regions within the U.S. Tables C23 and C24 report these results for the finance and non-finance sectors respectively. For finance, we find that employees separated for misconduct earn higher income relative to no fault separations across all regions except the Southwest because we do not have many misconduct separations within finance in that region. The estimates for the other four regions range from 3.8% to 6.5%. In contrast, for non-finance we find that employees separated for misconduct earn lower income than those separated for no fault across all five regions with estimates ranging from 7.1% to 10.8%.

Employees separated from same firm may have correlated income and different observations within employers may not be independent. We address this concern by re-estimating our baseline coefficients while clustering at the separating firm level instead of employee level.

[42] Figures AC3 and AC4 show income and tenure distribution of employees who drop out at different points in time from our sample, and for those who never drop out.

Table C25 reports these estimates where we find our results to be robust to this alternate clustering.

Yet another concern in our setting may be that a few outliers may drive our results. This may be especially problematic for the results in the finance sector where a small number of employees separated for misconduct may perform exceptionally well post separation and be responsible for the findings. While the use of log earnings reduces the influence of outliers, we also re-estimate our baseline coefficients by dropping employees belonging to both top and bottom 5% of income. We find our results to be robust to excluding these outliers and report the results in Table C26.

## 3.6 Conclusion

Though misconduct in the finance sector potentially contributes towards the low public trust in finance professionals and financial institutions, it remains prevalent. One plausible reason why it persists is that perpetrators of such behavior do not bear sufficiently high personal costs, especially in terms of their labor market outcomes. Using detailed data on job separations and income, we study the extent of *external* labor market punishment for misconduct in the finance and insurance sector (NAICS 52) and contrast this to consequences in non-finance sectors.

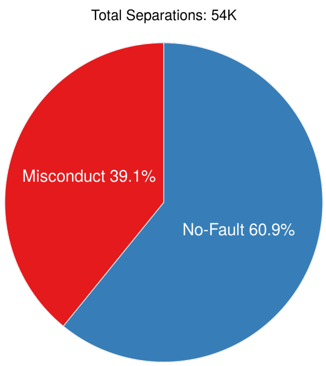
We focus on employees involuntarily separated for misconduct and examine how their income evolves post separation from the firm. Because these employees get separated from the firm, examining only their income pre- and post-separation can be misleading as separation itself affects income irrespective of involvement in misconduct. Our data allows us to overcome this empirical challenge by using income response for those laid-off for no fault (i.e., no fault layoffs) as a benchmark in our setting.

We find that finance employees involuntarily separated for misconduct earn 2.8% to 8.6% higher income than those laid-off for no fault post separation. These patterns are less likely to be driven by differences across workers involuntarily separated for misconduct vs no fault. In sharp contrast to finance, non-finance employees separated for misconduct experience 4.4% to 8.1% lower income than their no fault counterparts. Even amongst employees separated from the finance sector, results are concentrated amongst those who get rehired within finance and are absent for those rehired in other sectors. The patterns are most consistent with assortative matching in the finance labor market. Our results are concentrated/stronger among employees separated from firms less likely to engage in misconduct but who get rehired by employers more likely to do so. Those separated for misconduct are more likely to be rehired within firms more likely to engage in misconduct and once matched with such firms employees stay 10-15% longer relative to when matched with the other type of firms. These heterogeneity are starker for repeat offenders. We do not detect any such heterogeneity in non-finance sectors.

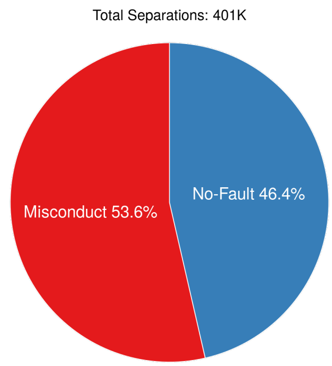
One feature that makes finance unique is that most products transacted upon in the sector are based on future cash flows which makes it more difficult for consumers or other stakeholders to disentangle bad luck from deliberate risky or unethical transaction in case of losses, thereby reducing the costs of engaging in such behavior for both firms and employees. We find our results to be most consistent with this argument. Other potential explanations considered like differences in regulation across sectors, longer job search by finance employees separated for misconduct, differences in type of misconduct and scapegoating behavior, as well as differential selection into misconduct are less likely to explain our findings.

Figure 3.1: Separations Composition

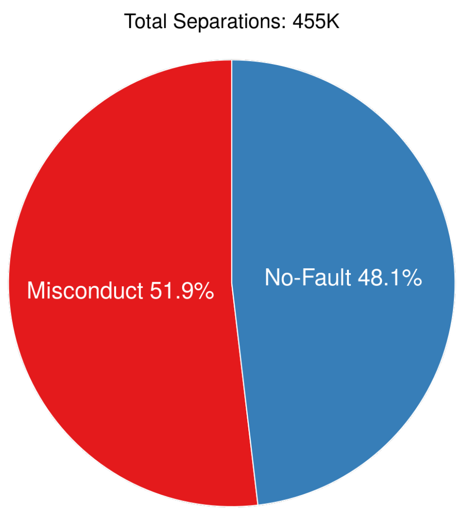
(a) Finance




(b) Non-Finance

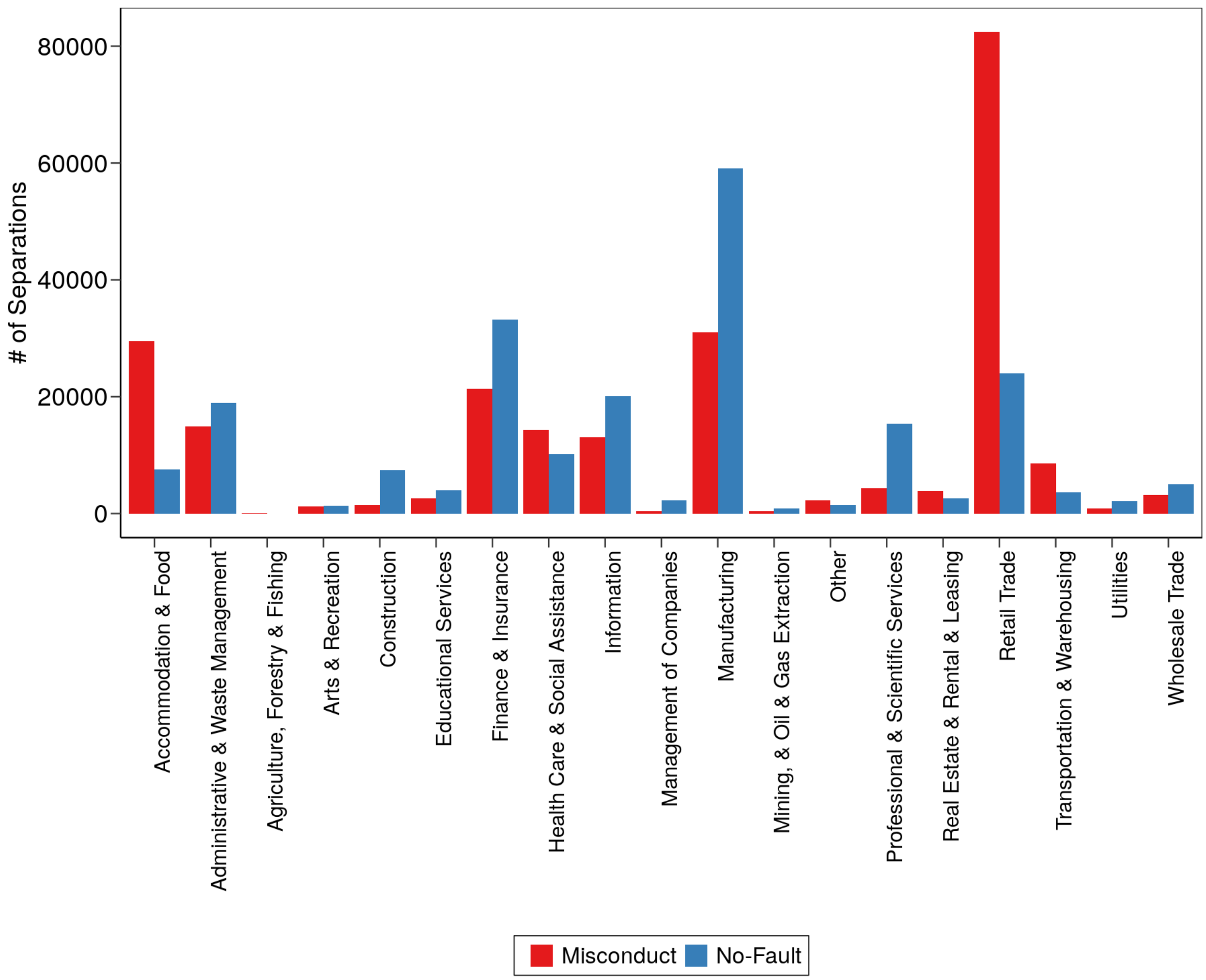



(c) All Professions



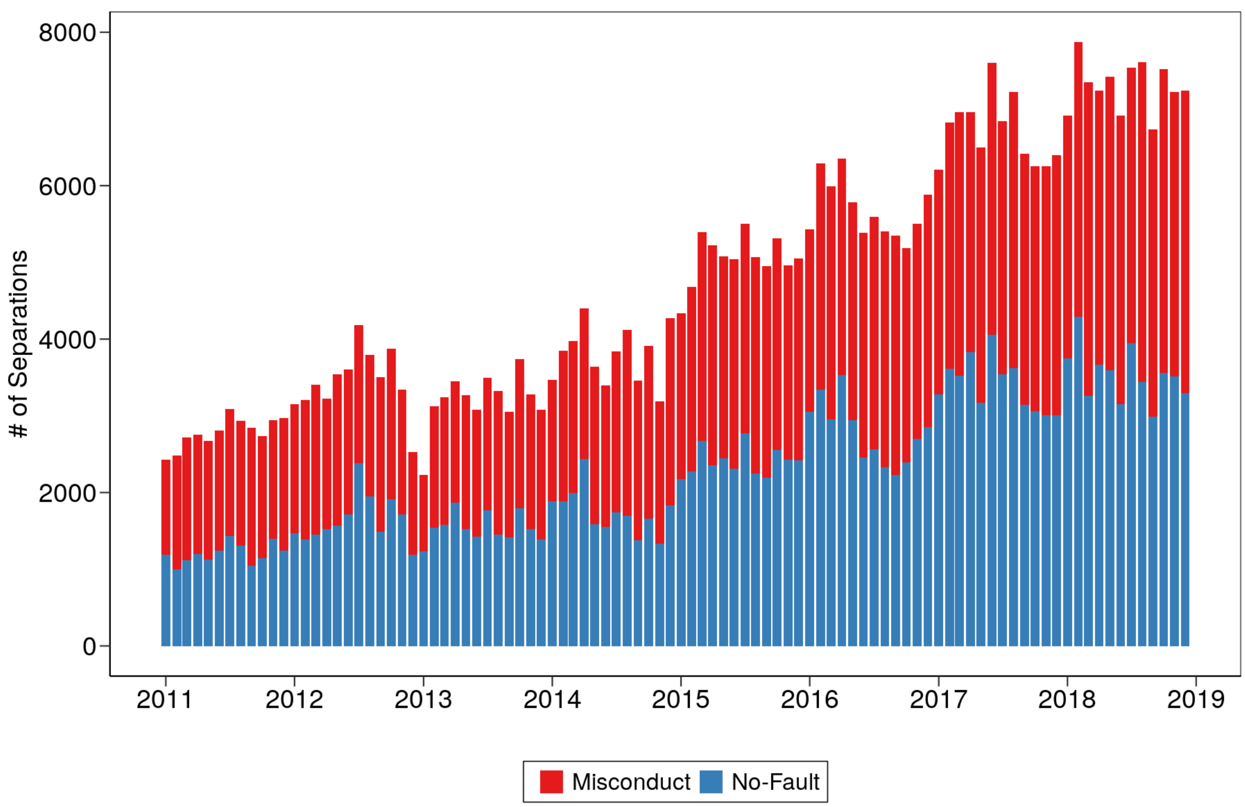

*Notes:* This figure shows the distribution of separations by separation type. Panel (a) and (b) plots the distribution for finance and non-finance sectors respectively and panel (c) plots the distribution for all professions.

Figure 3.2: Distribution of Separations by Industry



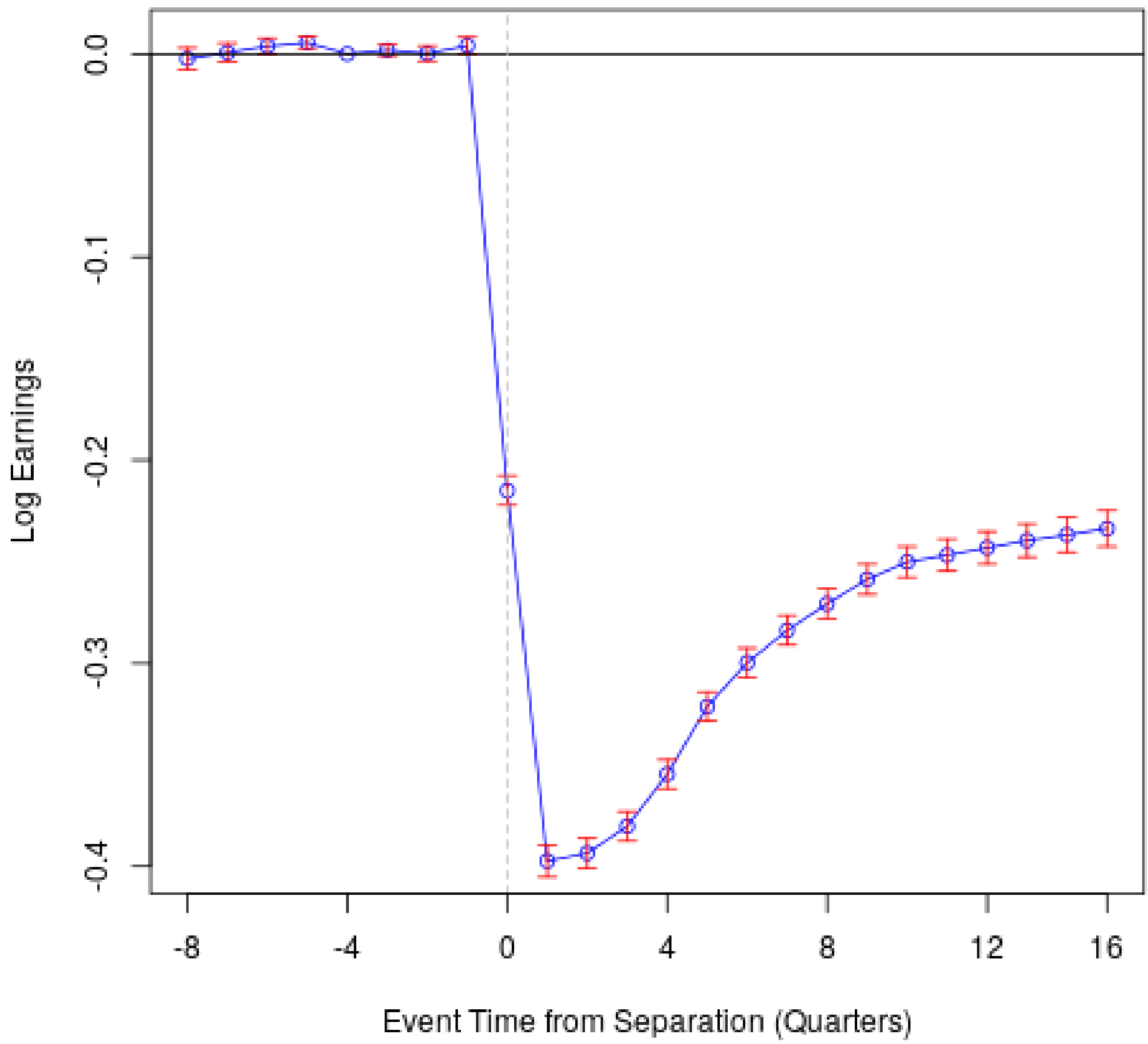

*Notes:* This figure plots the distribution of separations across different separation types by sectors in the economy.

Figure 3.3: Separations Composition over Time



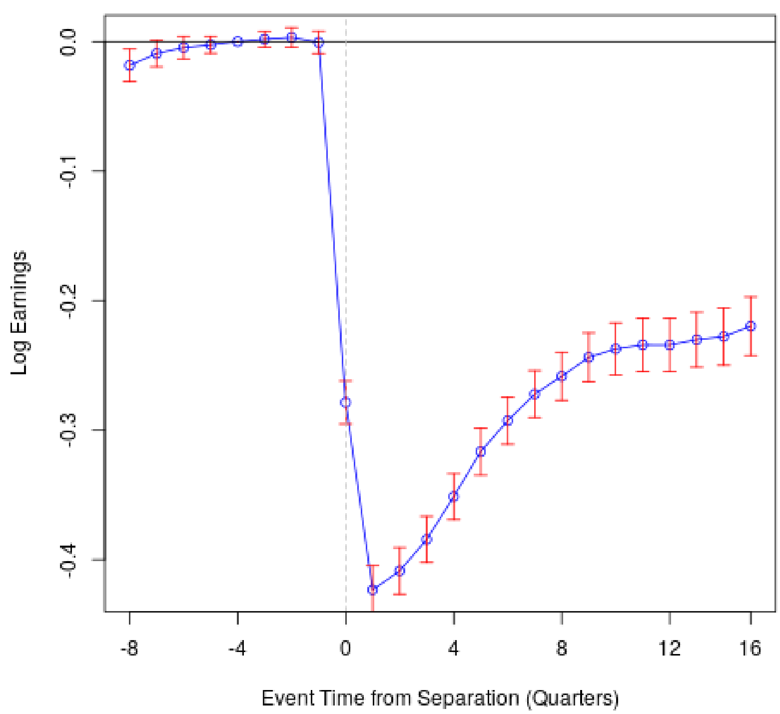

*Notes:* This figure plots the time-series of the distribution of separations from Jan 2011 through Dec 2018 by different separation types.

Figure 3.4: Income dynamics around No fault Layoffs: All Industries

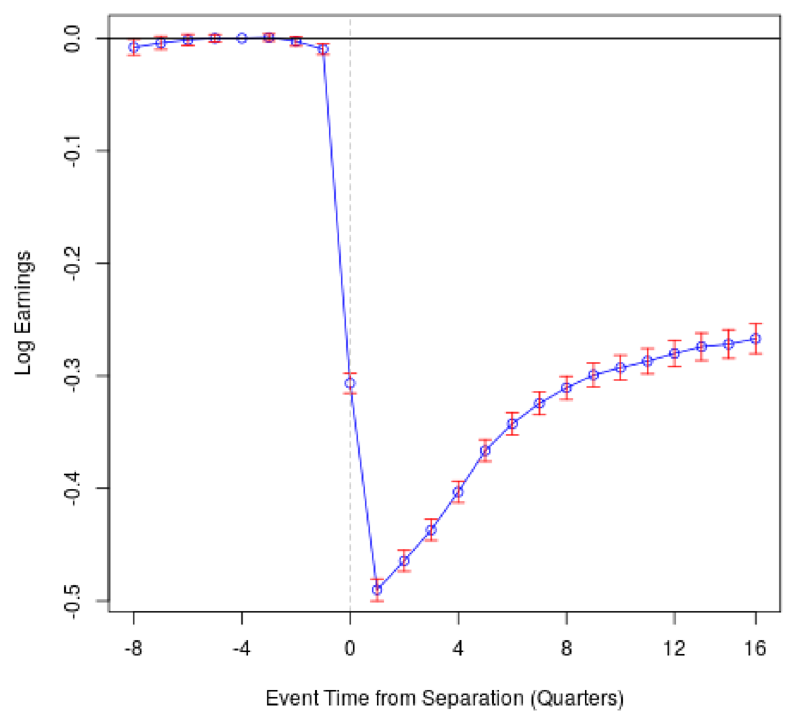



*Notes:* This figure plots the coefficients for the association between earnings and no fault layoffs in event-time around separation estimated for employees across all sectors following equation 3.2. The vertical bars correspond to 99% confidence levels.

Figure 3.5: Income dynamics around Misconduct Separations



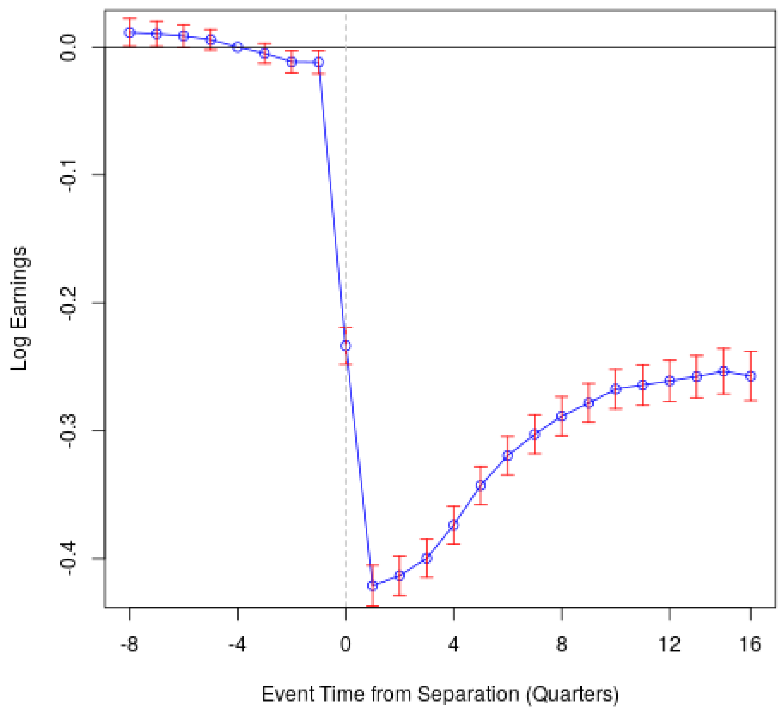

(a) Finance sector



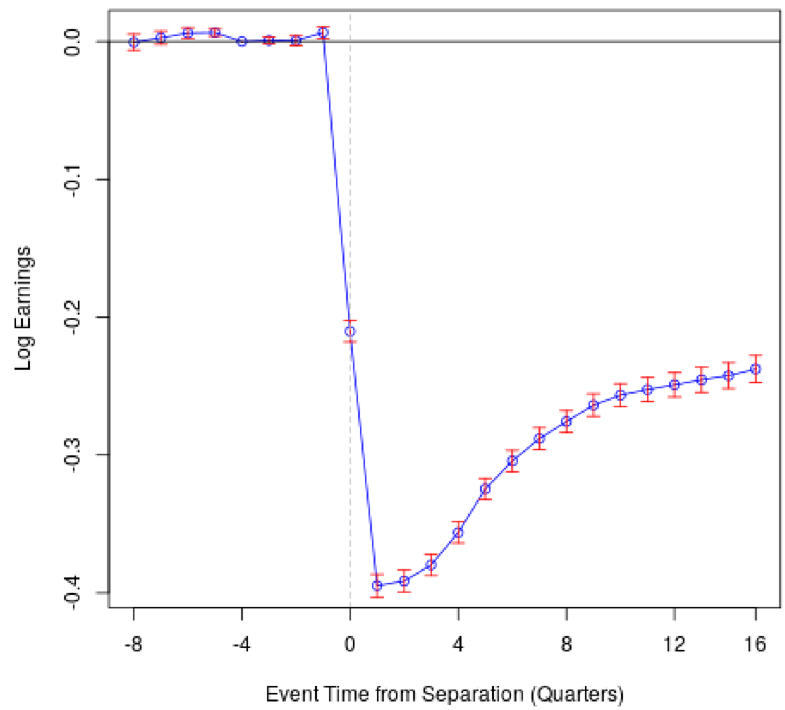

(b) Non-finance sector

*Notes:* This figure plots the coefficients for the association between earnings and involuntary separations owing to misconduct in event-time around separation following equation 3.2. While Panel (a) plots the coefficients for employees separated from the finance sector panel (b) plots them for those separated from other sectors. The vertical bars correspond to 99% confidence levels.

Figure 3.6: Income dynamics around No fault Layoffs



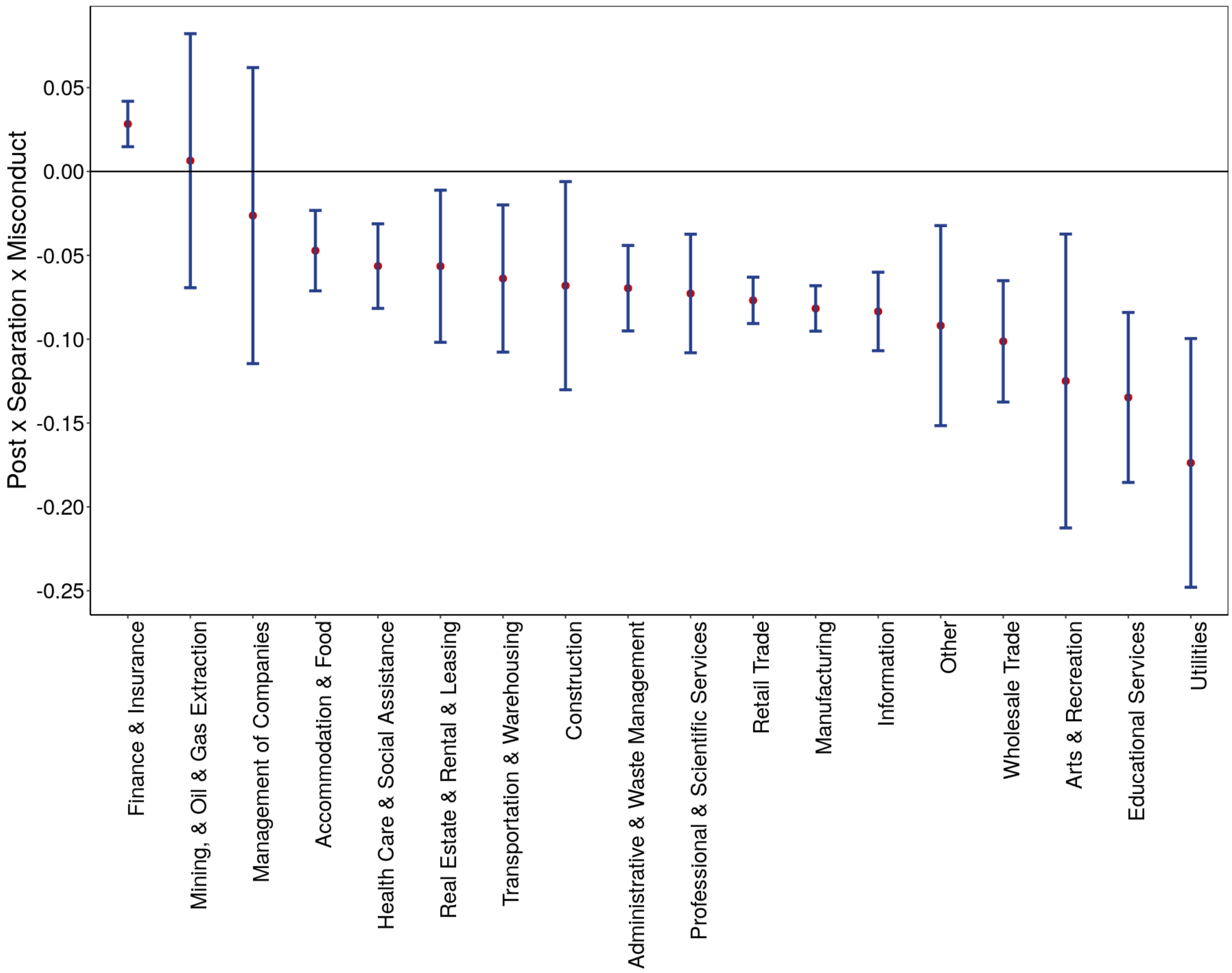

(a) Finance sector




(b) Non-finance sector

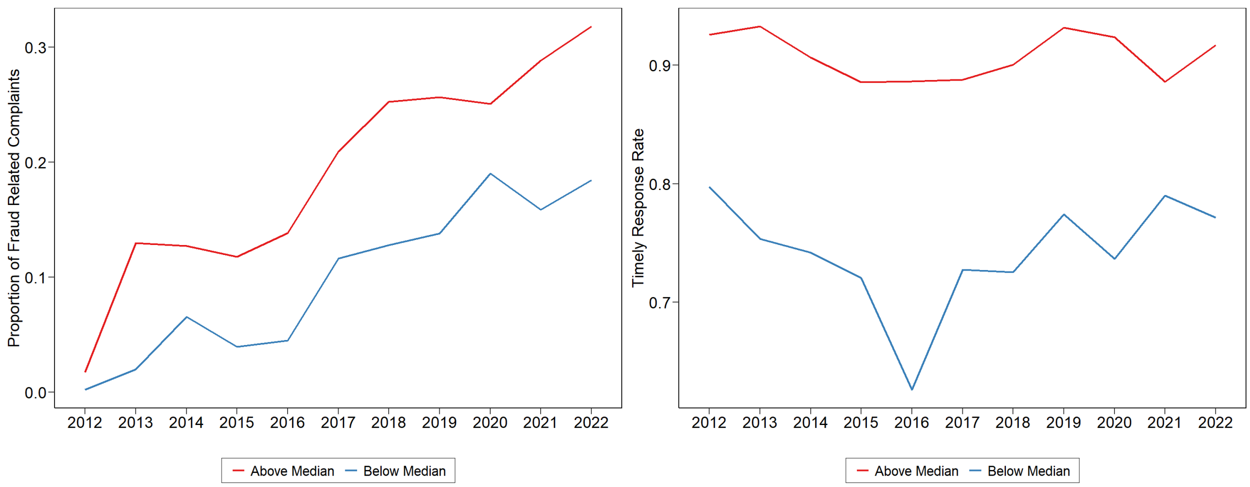
*Notes:* This figure plots the coefficients for the association between earnings and no fault layoffs in event-time around separation following equation 3.2. While Panel (a) plots the coefficients for employees separated from the finance sector panel (b) plots them for those separated from other sectors. The vertical bars correspond to 99% confidence levels.

Figure 3.7: Heterogeneity across Sectors




*Notes:* This figure plots the association between earnings and involuntary separations owing to misconduct estimated using the triple interactions from Equation 3.3. The vertical bars correspond to 99% confidence levels.

Figure 3.8: Persistent difference across types of firms in Finance




*Notes:* This figure plots the average proportion of fraud related complaints (left panel) and timely response rate to these complaints (right panel) over 2012 through 2022. Red (blue) color represents firms with above (below) median levels of fraud related proportion of complaints or timely response rates.

Table 3.1: Summary of Pre-Separation Annual Income (in '000 Dollars)

This table summarizes annual earnings for employees in our sample. *Separated* comprises of employees who were involuntarily separated either for misconduct or no fault. *Remain employed* refers to employees who are not involuntarily separated until atleast one year from the respective sample separation dates. Annual earnings are reported in thousands of dollars and are measured as of the month prior to separation. Finance sector corresponds to the NAICS code of 52.

| | Mean | Std. Dev. | p25 | Median | p75 |
|---|---|---|---|---|---|
| *A. All Industries: Separated* | | | | | |
| All Workers | 70.1 | 64.7 | 32.1 | 49.7 | 82.3 |
| No fault | 99.9 | 82.0 | 46.2 | 74.7 | 123.6 |
| Misconduct | 52.2 | 41.2 | 28.5 | 42.0 | 61.6 |
| *B. All Industries: Remain employed* | | | | | |
| All Workers | 87.3 | 78.5 | 41.1 | 64.4 | 104.7 |
| *C. Finance: Separated* | | | | | |
| All Workers | 86.1 | 81.9 | 40.7 | 58.4 | 96.6 |
| No fault | 111.0 | 98.01 | 50.9 | 77.5 | 130.4 |
| Misconduct | 61.7 | 52.0 | 36.3 | 47.5 | 67.5 |
| *D. Finance: Remain employed* | | | | | |
| All Workers | 105.3 | 94.5 | 50.3 | 76.2 | 121.3 |
| *E. Non-Finance: Separated* | | | | | |
| All Workers | 67.9 | 61.8 | 30.9 | 48.5 | 80.4 |
| No fault | 97.8 | 78.5 | 45.3 | 74.2 | 122.4 |
| Misconduct | 51.2 | 39.8 | 27.7 | 41.3 | 60.9 |
| *F. Non-Finance: Remain employed* | | | | | |
| All Workers | 77.4 | 66.0 | 36.2 | 58.2 | 95.0 |

Table 3.2: Income following No fault Layoffs: All Industries

This table reports the results of the OLS regressions specified in Equation 3.1. The sample comprises employees from all sectors laid off for no fault and their corresponding non-separated counterparts. *Layoff* is an indicator equal to 1 if a worker was laid off between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Industry* refers to the 6 digit NAICS code for the separated firm, and *Wage Bins* are constructed at $1,000 width for pre-separation income. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Layoff × *Post* | -0.294*** | -0.287*** | -0.285*** |
| | (0.002) | (0.002) | (0.002) |
| Individual FE | Y | Y | Y |
| Month FE | Y | N | N |
| Industry × $Month\ FE$ | N | Y | Y |
| Wage Bin × $Month\ FE$ | N | N | Y |
| N | 62,618,513 | 62,618,513 | 62,618,513 |
| $Adj.R^2$ | 0.842 | 0.844 | 0.845 |

Table 3.3: Income following Misconduct Separation

This table reports the results of the OLS regressions specified in Equation 3.1. The sample comprises employees involuntarily separated for misconduct and their corresponding non-separated counterparts. Columns (1)-(2) report the estimates for employees separated from the finance sector defined by the NAICS code of 52. Columns (3)-(4) report the estimates for employees separated from all other sectors. While the second last row reports the baseline separation effect for no fault separations, the bottom line shows the difference between income changes following misconduct vs no fault separations. *Misconduct* (*Layoff*) is an indicator equal to 1 if an employee was separated for misconduct (no fault) between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Industry* refers to the 6 digit NAICS code for the separated firm, *Wage Bins* are constructed at \$1,000 width for pre-separation income, *Firm* represents the separated firm, *Location* corresponds to the 3-Digit Zipcode, and *Tenure* is measured as of the month prior to separation. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | Finance | | Non-Finance | |
| | (1) | (2) | (3) | (4) |
| Misconduct × *Post* | -0.224*** | -0.275*** | -0.330*** | -0.379*** |
| | (0.005) | (0.005) | (0.002) | (0.002) |
| Individual FE | Y | Y | Y | Y |
| Wage Bin × $Month\ FE$ | Y | Y | Y | Y |
| Industry × $Month\ FE$ | Y | N | Y | N |
| Firm × *Location* × $Year\ FE$ | N | Y | N | Y |
| Tenure × $Year\ FE$ | N | Y | N | Y |
| N | 19,279,776 | 19,279,776 | 43,521,289 | 43,521,289 |
| $Adj.R^2$ | 0.884 | 0.897 | 0.817 | 0.838 |
| Layoff × *Post* | -0.301 | -0.314 | -0.282 | -0.298 |
| **Difference** | **0.077*** ** | **0.039*** ** | **-0.048*** ** | **-0.081*** ** |

Table 3.4: Income following Misconduct Separation: Collapsed

This table reports results of the OLS regressions specified in Equation 3.3. The sample comprises of employees involuntarily separated either for no fault or misconduct and their corresponding non-separated counterparts. Columns (1)-(2) report the estimates for employees separated from the finance sector defined as all firms in the NAICS code of 52. Columns (3)-(4) report the estimates for employees separated from all other sectors. *Misconduct* is an indicator equal to 1 if an employee was separated for misconduct between 2011 to 2018 and 0 otherwise. *Separated* is a dummy equal to 1 if the employee was separated between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Industry* refers to the 6 digit NAICS code for the separated firm, *Wage Bins* are constructed at $1,000 width for pre-separation income, *Firm* represents the separated firm, *Location* corresponds to the 3-Digit Zipcode, *Tenure* is measured as of the month prior to separation, and *Separation Cohort* refers to the year of separation. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | Finance | | Non-Finance | |
| | (1) | (2) | (3) | (4) |
| Misconduct$\times$ *Separated* $\times$ *Post* | 0.075*** | 0.028*** | -0.044*** | -0.069*** |
| | (0.007) | (0.007) | (0.003) | (0.003) |
| Separated $\times$ *Post* | -0.303*** | -0.320*** | -0.290*** | -0.310*** |
| | (0.004) | (0.004) | (0.002) | (0.002) |
| Individual FE | Y | Y | Y | Y |
| Wage Bin $\times$ *Month FE* | Y | Y | Y | Y |
| Industry $\times$ *Month FE* | Y | N | Y | N |
| Firm $\times$ *Location* $\times$ *Tenure* $\times$ *Year FE* | N | Y | N | Y |
| Separation Cohort $\times$ *Year FE* | N | Y | N | Y |
| N | 21,152,903 | 21,152,903 | 52,471,961 | 52,471,961 |
| *Adj.*$R^2$ | 0.880 | 0.896 | 0.817 | 0.849 |

Table 3.5: Income following Separation: Mass Layoffs as Counterfactual

This table reports results of the OLS regressions specified in Equation 3.3. The sample comprises of employees involuntarily separated either for misconduct or in no fault mass layoff. Columns (1)-(2) report the estimates for employees separated from the finance sector defined as all firms in the NAICS code of 52. Columns (3)-(4) report the estimates for employees separated from all other sectors. *Misconduct* is an indicator equal to 1 if an employee was separated for misconduct between 2011 to 2018 and 0 otherwise. *Separated* is a dummy equal to 1 if the employee was separated between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Industry* refers to the 6 digit NAICS code for the separated firm, *Wage Bins* are constructed at $1,000 width for pre-separation income, *Firm* represents the separated firm, *Location* corresponds to the 3-Digit Zipcode, *Tenure* is measured as of the month prior to separation, and *Separation Cohort* refers to the year of separation. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | Finance | | Non-Finance | |
| | (1) | (2) | (3) | (4) |
| Misconduct$\times$ $Separated \times$ $Post$ | 0.105*** | 0.075** | -0.054*** | -0.071*** |
| | (0.024) | (0.025) | (0.009) | (0.008) |
| Separated $\times$ $Post$ | -0.329*** | -0.367*** | -0.276*** | -0.314*** |
| | (0.023) | (0.025) | (0.009) | (0.008) |
| Individual FE | Y | Y | Y | Y |
| Wage Bin $\times$ $Month\ FE$ | Y | Y | Y | Y |
| Industry $\times$ $Month\ FE$ | Y | N | Y | N |
| Firm $\times$ $Location \times$ $Tenure \times$ $Year\ FE$ | N | Y | N | Y |
| Separation Cohort $\times$ $Year\ FE$ | N | Y | N | Y |
| N | 19,328,073 | 19,328,073 | 44,210,510 | 44,210,510 |
| $Adj.R^2$ | 0.884 | 0.901 | 0.817 | 0.852 |

Table 3.6: Income following Separation: Stay vs Depart

This table reports heterogeneity in log earnings following separation for different sub-samples. While column (1) reports the estimates for employees separated from finance who are rehired within the finance sector post-separation, column (2) reports them for employees rehired outside of the sector following separation. Similary, columns (3)-(4) correspond to subsamples of those who stay and depart from their separated non-finance sector respectively. *Misconduct* is an indicator equal to 1 if an employee was separated for misconduct between 2011 to 2018 and 0 otherwise. *Separated* is a dummy equal to 1 if the employee was separated between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Industry* refers to the 6 digit NAICS code for the separated firm, *Wage Bins* are constructed at $1,000 width for pre-separation income, *Firm* represents the separated firm, *Location* corresponds to the 3-Digit Zipcode, *Tenure* is measured as of the month prior to separation, and *Separation Cohort* refers to the year of separation. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | Finance | | Non-Finance | |
| | Stay | Depart | Stay | Depart |
| | (1) | (2) | (3) | (4) |
| Misconduct × *Separated* × *Post* | 0.037*** | 0.018 | -0.099*** | -0.045*** |
| | (0.008) | (0.011) | (0.006) | (0.005) |
| Separated × *Post* | -0.205*** | -0.427*** | -0.257*** | -0.347*** |
| | (0.004) | (0.007) | (0.005) | (0.003) |
| Individual FE | Y | Y | Y | Y |
| Wage Bin × *Month FE* | Y | Y | Y | Y |
| Firm × *Location* × *Tenure* × *Year FE* | Y | Y | Y | Y |
| Separation Cohort × *Year FE* | Y | Y | Y | Y |
| N | 19,563,489 | 19,578,473 | 47,552,330 | 51,101,910 |
| $Adj.R^2$ | 0.901 | 0.899 | 0.853 | 0.847 |

Table 3.7: Assortative Matching between Employer and Employees: CFPB Complaints

This table reports heterogeneity in our findings based on the types of separated and rehiring employers within the finance sector. The different types of employers are measured as proportion of fraud-related complaints received against or non-timely response rates to these complaints made by these employers. While column (1) reports the estimates for sub-sample of employees separated from employers with above median levels of fraudulent complaints who get rehired by firms with below median levels of complaints, column (2) reports results for opposite moves. Similarly column (3) reports the estimates for those separated from employers with above median levels of non-timely response rates who get rehired in firms with below median rates and column (4) reports results for the opposite moves. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | Fraud Related Complaints | | Non-Timely Response | |
| | Above to Below Median (1) | Below to Above Median (2) | Above to Below Median (3) | Below to Above Median (4) |
| Misconduct × *Separated* × *Post* | 0.048 | 0.091*** | 0.020 | 0.099*** |
| | (0.029) | (0.031) | (0.022) | (0.022) |
| Separated × *Post* | -0.182*** | -0.215*** | -0.162*** | -0.210*** |
| | (0.017) | (0.017) | (0.015) | (0.012) |
| Individual FE | Y | Y | Y | Y |
| Wage Bin × *Month FE* | Y | Y | Y | Y |
| Firm × *Location* × *Tenure* × *Year FE* | Y | Y | Y | Y |
| Separation Cohort × *Year FE* | Y | Y | Y | Y |
| Hiring Firm Size × *Hiring Firm Industry* | Y | Y | Y | Y |
| N | 5,862,987 | 4,175,508 | 4,575,474 | 5,578,247 |
| $Adj.R^2$ | 0.904 | 0.901 | 0.909 | 0.897 |

Table 3.8: Assortative Matching between Employer and Employees: Violations

This table reports heterogeneity in our findings based on the types of separated and rehiring employers. The different types of employers are measured as levels of penalty for corporate violations. While column (1) reports the estimates for sub-sample of finance employees separated from employers with zero penalty who get rehired by firms with positive penalty, column (2) reports results for opposite moves. Columns (3)-(4) report analogous estimates for the non-finance sectors. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | Finance | | Non-Finance | |
| | Zero Penalty to Non-Zero Penalty (1) | Non-Zero Penalty to Zero Penalty (2) | Zero Penalty to Non-Zero Penalty (3) | Non-Zero Penalty to Zero Penalty (4) |
| Misconduct × *Separated* × *Post* | 0.032** | 0.019* | -0.066*** | -0.068*** |
| | (0.015) | (0.011) | (0.008) | (0.006) |
| Separated × *Post* | -0.289*** | -0.315*** | -0.301*** | -0.343*** |
| | (0.010) | (0.007) | (0.006) | (0.004) |
| Individual FE | Y | Y | Y | Y |
| Wage Bin × *Month FE* | Y | Y | Y | Y |
| Firm × *Location* × *Tenure* × *Year FE* | Y | Y | Y | Y |
| Separation Cohort × *Year FE* | Y | Y | Y | Y |
| N | 18,569,070 | 19,160,344 | 36,347,842 | 39,537,471 |
| $Adj.R^2$ | 0.904 | 0.902 | 0.865 | 0.861 |

Table 3.9: Heterogeneity by Type of Job Profile

This table reports heterogeneity in our findings based on the pre-separation job profile within the finance sector. While Column (1) reports the estimates for sub-sample of employees with finance-related pre-separation jobs Column (2) reports them for employees with non-finance job profiles . Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | |
|---|---|---|
| | Finance Job Profile | Non-Finance Job Profile |
| | (1) | (2) |
| Misconduct × *Separated* × *Post* | 0.061*** | 0.031 |
| | (0.013) | (0.019) |
| Separated × *Post* | -0.227*** | -0.153*** |
| | (0.008) | (0.013) |
| Individual FE | Y | Y |
| Wage Bin × *Month FE* | Y | Y |
| Firm × *Location* × *Tenure* × *Year FE* | Y | Y |
| Separation Cohort × *Year FE* | Y | Y |
| N | 8,632,563 | 3,999,109 |
| $Adj.R^2$ | 0.909 | 0.906 |

Table 3.10: Heterogeneity by Geographic Makeup of Hiring Firm Location: Finance

This table reports heterogeneity in our findings based on the geographic makeup of the hiring firm zipcodes. Columns (1) and (2) report the estimates for sub-sample of employees rehired by establishments in zipcodes above and below median levels of % population with college education respectively. Similarly columns (3) and (4) report the estimates for those rehired by establishments in zipcodes with above and below median levels of % population 65 years and older. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | %college | | %65 or older | |
| | Above Median (1) | Below Median (2) | Above Median (3) | Below Median (4) |
| **Panel A: Finance Job Profiles** | | | | |
| Misconduct × *Separated* × *Post* | -0.0003 | 0.123*** | 0.152*** | 0.119** |
| | (0.055) | (0.046) | (0.052) | (0.056) |
| Separated × *Post* | -0.237*** | -0.221*** | -0.266*** | -0.151*** |
| | (0.028) | (0.030) | (0.026) | (0.037) |
| N | 8,195,671 | 8,194,824 | 8,197,564 | 8,196,441 |
| $Adj.R^2$ | 0.910 | 0.909 | 0.910 | 0.909 |
| **Panel B: Non-Finance Job Profiles** | | | | |
| Misconduct × *Separated* × *Post* | 0.0007 | 0.010 | -0.035 | 0.001 |
| | (0.087) | (0.070) | (0.076) | (0.067) |
| Separated × *Post* | -0.127** | -0.120** | -0.147*** | -0.060 |
| | (0.061) | (0.051) | (0.037) | (0.041) |
| N | 3,864,187 | 3,864,685 | 3,864,938 | 3,862,485 |
| $Adj.R^2$ | 0.907 | 0.907 | 0.907 | 0.907 |
| Individual FE | Y | Y | Y | Y |
| Wage Bin × *Month FE* | Y | Y | Y | Y |
| Firm × *Location* × *Tenure* × *Year FE* | Y | Y | Y | Y |
| Separation Cohort × *Year FE* | Y | Y | Y | Y |

Table 3.11: Heterogeneity by Geographic Makeup of Hiring Firm Location: Non-Finance

This table reports heterogeneity in our findings based on the geographic makeup of the hiring firm zipcodes. Columns (1) and (2) report the estimates for sub-sample of employees rehired by establishments in zipcodes above and below median levels of % population with college education respectively. Similarly Columns (3) and (4) report the estimates for those rehired by establishments in zipcodes with above and below median levels of % population 65 years and older. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | %college | | %65 or older | |
| | Above Median | Below Median | Above Median | Below Median |
| | (1) | (2) | (3) | (4) |
| Misconduct × *Separated* × *Post* | -0.047*** | -0.043*** | -0.043*** | -0.056*** |
| | (0.013) | (0.013) | (0.013) | (0.013) |
| Separated × *Post* | -0.284*** | -0.304*** | -0.309*** | -0.277*** |
| | (0.008) | (0.010) | (0.010) | (0.009) |
| N | 34,751,633 | 34,577,836 | 34,874,183 | 34,528,699 |
| $Adj.R^2$ | 0.862 | 0.862 | 0.862 | 0.862 |
| Individual FE | Y | Y | Y | Y |
| Wage Bin × *Month FE* | Y | Y | Y | Y |
| Firm × *Location* × *Tenure* × *Year FE* | Y | Y | Y | Y |
| Separation Cohort × *Year FE* | Y | Y | Y | Y |

Table 3.12: Heterogeneity by Extent of Regulation

This table reports heterogeneity in our findings based on the strictness of regulation faced by separated employers. While column (1) reports the estimates for sub-sample of employees separated from employers in heavily regulated sub-sectors within finance column (2) reports it for employers in less regulated sub-sectors. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | |
|---|---|---|
| | More Regulated | Less Regulated |
| | (1) | (2) |
| Misconduct × *Separated* × *Post* | 0.025*** | 0.029* |
| | (0.006) | (0.012) |
| Separated × *Post* | -0.303*** | -0.382*** |
| | (0.004) | (0.006) |
| Individual FE | Y | Y |
| Wage Bin × *Month FE* | Y | Y |
| Firm × *Location* × *Tenure* × *Year FE* | Y | Y |
| Separation Cohort × *Year FE* | Y | Y |
| N | 14,226,556 | 7,703,891 |
| $Adj.R^2$ | 0.898 | 0.890 |

Table 3.13: Income following Company Policy Violation Separation for Sales Professionals

This table reports log earnings following separation for sales professionals across finance and non-finance sectors. While column (1) reports the estimates for employees separated from the finance sector column (2) reports them other sectors. *Misconduct* is an indicator equal to 1 if an employee was separated for misconduct attributable to violation of company policy between 2011 to 2018 and 0 otherwise. *Separated* is a dummy equal to 1 if the employee was separated between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Wage Bins* are constructed at $1,000 width for pre-separation income, *Firm* represents the separated firm, *Location* corresponds to the 3-Digit Zipcode, *Tenure* is measured as of the month prior to separation, and *Separation Cohort* refers to the year of separation. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | |
|---|---|---|
| | Finance | Non-Finance |
| | (1) | (2) |
| Misconduct × *Separated* × *Post* | 0.070* | -0.063*** |
| | (0.041) | (0.019) |
| Separated × *Post* | -0.293*** | -0.264*** |
| | (0.025) | (0.013) |
| Individual FE | Y | Y |
| Wage Bin × $Month\ FE$ | Y | Y |
| Firm × *Location* × $Year\ FE$ | Y | Y |
| Separation Cohort × $Year\ FE$ | Y | Y |
| N | 1,642,767 | 3,473,614 |
| $Adj.R^2$ | 0.949 | 0.945 |

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# Appendix A

# Time on your Side: Labor Maket Effects of Foreclosure Delays

## A.1 CFPB Amendment of mortgage servicing rules: Details

### Foreclosure Protection under CARES Act: March 2020-July 2021

On March 27, 2020, the President signed into law the CARES Act, which includes a foreclosure moratorium for certain loans on single-family properties.

- Who is protected: Borrowers with "federally backed mortgage loans" and tenants living in a property with such a loan. A "federally backed mortgage loan" is a loan owned, insured or guaranteed by one of the following entities: the Department of Housing and Urban Development (HUD)2; the Department of Veterans Affairs, the Department of Agriculture, Fannie Mae or Freddie Mac.
- What it does: Prevents mortgage servicers from initiating a judicial or non-judicial foreclosure, seeking a court order for a foreclosure judgment or order of sale, holding a foreclosure sale or executing a foreclosure-related eviction.

## Procedural Safeguards prescribed by CFPB for Amendment of Regulation X

From August 31, 2021 through December 31, 2021, unless an exception applies, before referring certain 120-day delinquent accounts for foreclosure the servicer must make sure at least one of the temporary procedural safeguards has been met.

1. The borrower was evaluated based on a complete loss mitigation application and existing foreclosure protection conditions are met. To meet this safeguard, the servicer must confirm that:

    - The borrower submitted a complete loss mitigation application, and the servicer evaluated the application.
    - The borrower remained delinquent since submission of the loss mitigation application.
    - The foreclosure protection conditions in the existing Mortgage Servicing Rules discussed above, are met, such that a servicer is permitted by the Rules to make a foreclosure referral.

2. The property is abandoned. To meet this safeguard, applicable state or local law must consider the property securing the mortgage abandoned when referred to foreclosure.

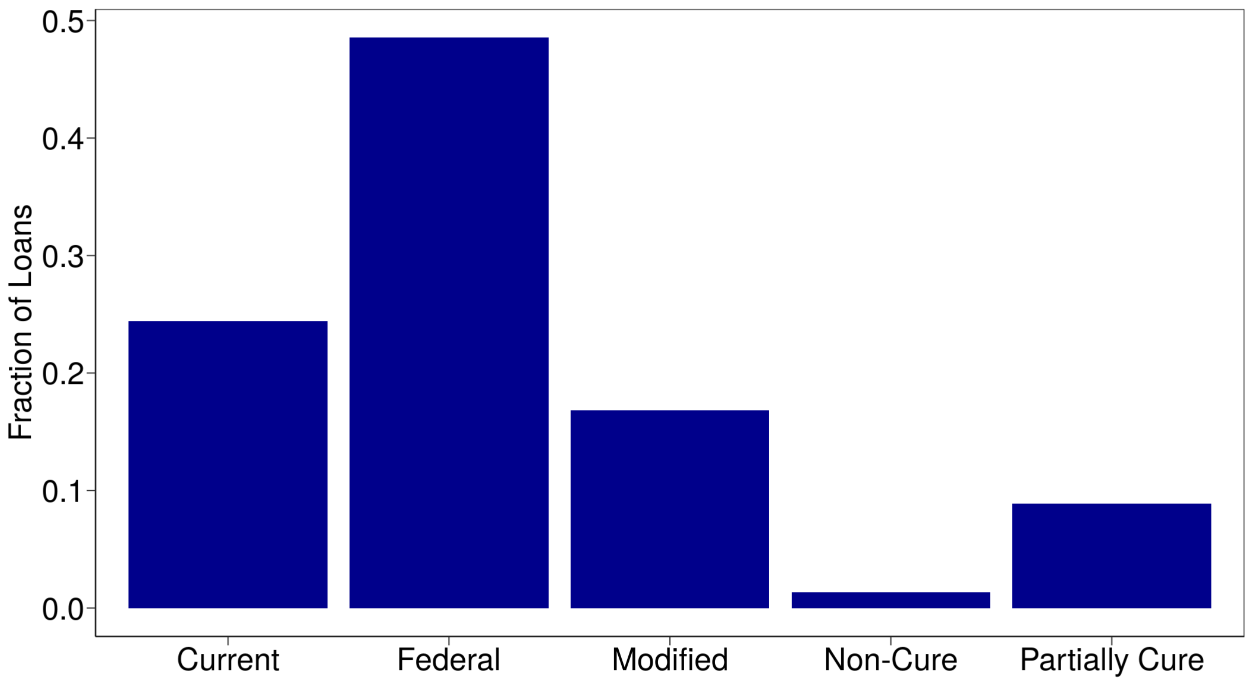
3. The borrower is unresponsive to servicer outreach. To meet this safeguard, the servicer must not have received any communications from the borrower in the 90 days prior to the foreclosure referral and the servicer must confirm:

    - It has complied with the early intervention live contact requirements in the Mortgage Servicing Rules during that 90-day period.
    - It has provided the early intervention 45-day written notice required by the Mortgage Servicing Rules. The servicer must have sent the notice at least 10 but no more than 45 days before foreclosure referral.
    - It has complied with all loss mitigation notice requirements in the Mortgage Servicing Rules during that 90-day period, such as the notice of an incomplete loss mitigation application.

- The borrower's forbearance program, if applicable, ended at least 30 days before foreclosure referral.

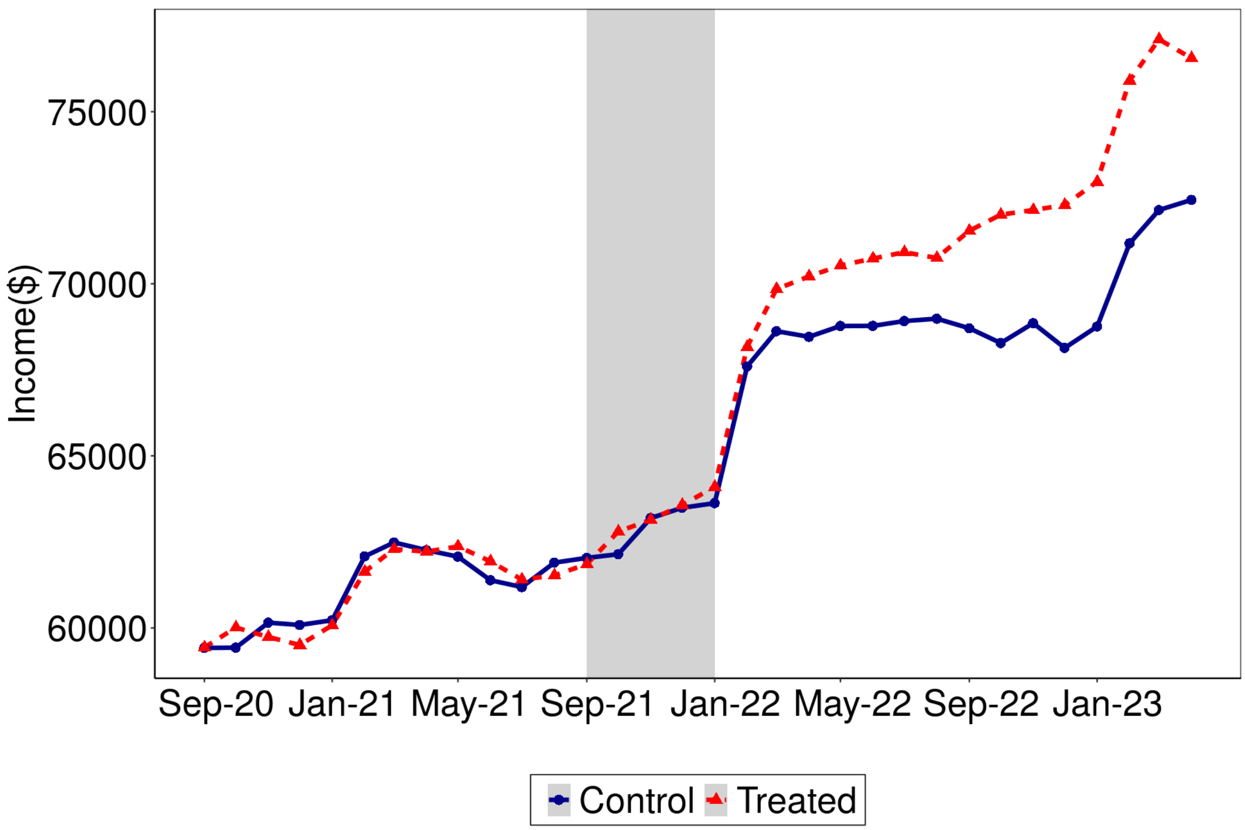
## A.2 Additional Tables and Figures

Figure A1: Reasons for survival



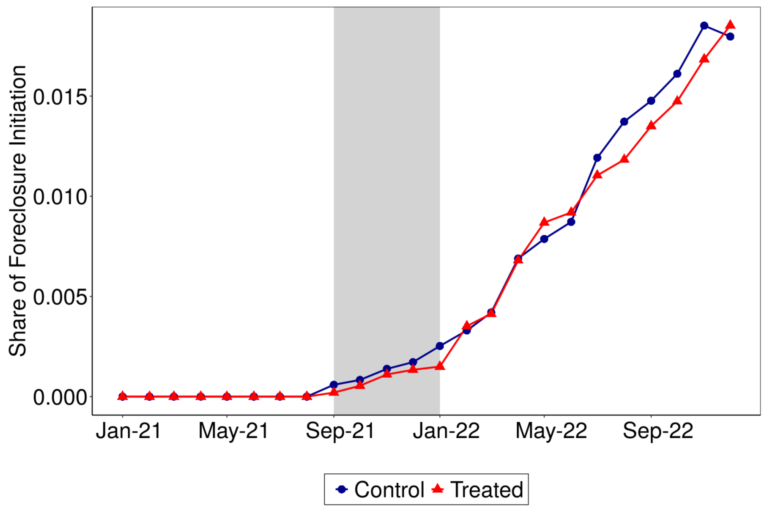

*Notes*: This figure displays the distribution of underling reasons for survival of loans that became 120+days delinquent in February or March 2020 but were reported as in default and not foreclosed upon as of August 2021.

Figure A2: Time Series of Labor Income

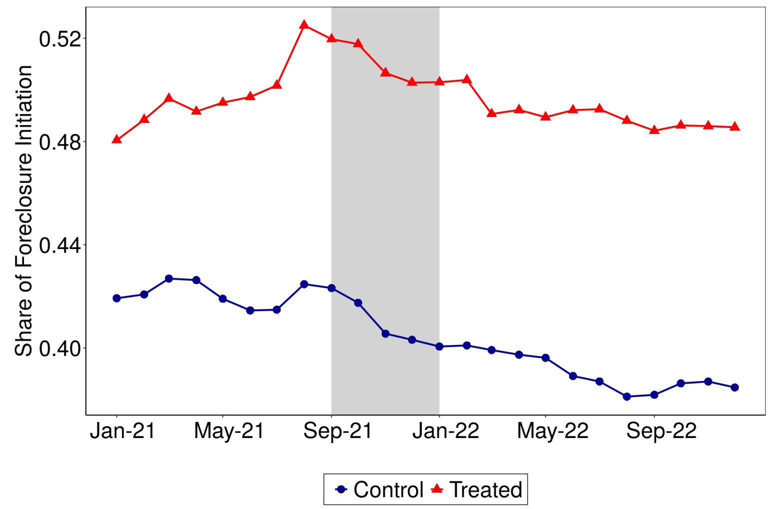



*Notes*: This figure displays the time series of labor income, separately for the treated and control group.

Figure A3: Share of Foreclosure Filings: Non Binding Loans



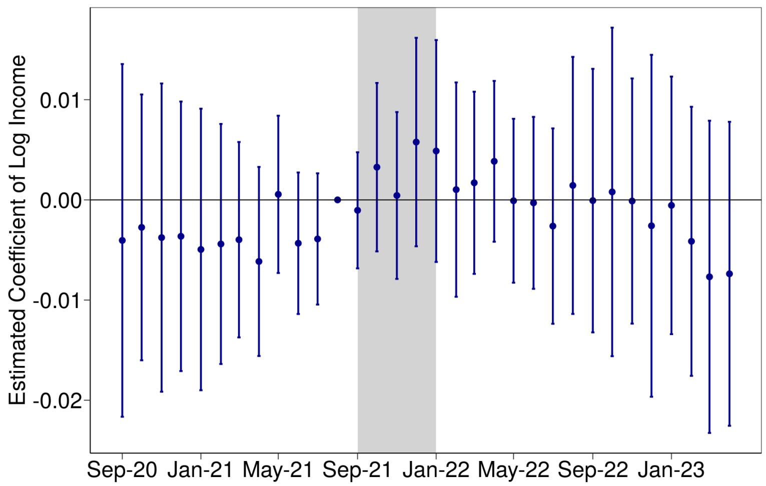

(a) Foreclosure Process not Started and Current



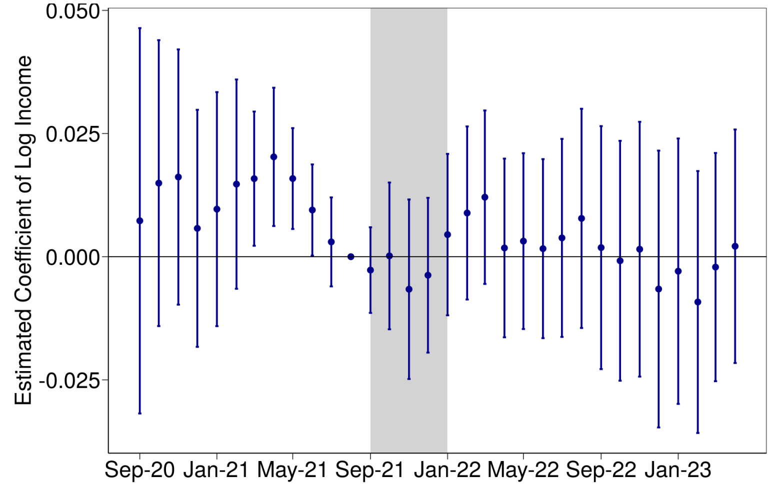

(b) Foreclosure Process Started

*Notes*: This figure presents the time-series for the cumulative share of foreclosure filing between January 2021 and December 2022. The red line denoting '*Treated*' plots the cumulative share of mortgages that entered 120+days of delinquency in March 2020 for which foreclosure was initiated. Similarly the blue line plots the cumulative share of foreclosures by month for the control group i.e., mortgages that transitioned to 120+days of delinquency in February 2020. Panel (a) restricts the sample to loans reported as current as of August 2021 and Panel (b) presents the trends for mortgages subject to foreclosure proceedings on or before August 2021. These correspond to loans for which the CFPB policy did not bind.

Figure A4: Dynamic Treatment Effects: Effect of the Policy on Labor Income for Non Binding loans




(a) Foreclosure Process not Started and Current




(b) Foreclosure Process Started

*Notes*: This figure plots the $\beta_k$ coefficients along with the corresponding 95% confidence intervals from the following equation:

$$y_{i,z,h,t} = \sum_{\substack{k=Sep'20 \\ k\neq Aug'21}}^{Apr'23} \beta_k \times Treated_{i,z,h} \times D_k + \theta_i + \gamma_{z,t} + \delta_{h,t} + \epsilon_{i,z,h,t}$$

which captures the differential change in log earnings in the months around the treatment between the treated and control group of mortgagors. $y_{i,z,h,t}$ represents log earnings for individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $D_k$ is an indicator that equals one for observations corresponding to individual $i$ when the observation belongs to year-month $k$. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects and $\delta_{h,t}$ indicates industry x year-month fixed effects. Zipcode and industry are measured in the period prior to treatment. The sample time period is between September 2020 to April 2023 and is restricted to loans reported as current as of August 2021 in panel (a) and subject to foreclosure on or before August 2021 in panel (b). Standard errors are robust to heteroskedasticity and are clustered at the six-digit NAICS code level.

Table A1: Summary Statistics: All Loans

This table summarizes the entire data consisting of all loans whether foreclosed or not in the period prior to August 2021. The sample period ranges from September 2020 to March 2023.

| Statistic | N | Mean | St. Dev. | p25 | Median | p75 |
|---|---|---|---|---|---|---|
| Origination Amount | 798,765 | 185,803 | 132,453 | 100,152 | 158,822 | 241,147 |
| Origination Term | 798,765 | 381 | 108 | 360 | 360 | 360 |
| Loan Balance | 798,765 | 155,560 | 132,491 | 68,201 | 133,271 | 215,713 |
| Loan Payment | 798,765 | 774 | 2,418 | 0 | 0 | 1268 |
| Credit Score | 798,765 | 588 | 73 | 540 | 586 | 637 |
| Total Debt Payment | 798,765 | 1,163 | 2,582 | 0 | 727 | 1,813 |
| Utilisation (%) | 798,765 | 56.87 | 38.66 | 19.00 | 60.55 | 100 |
| Modification (%) | 798,765 | 23.74 | 42.55 | 0 | 0 | 0 |
| Term Modifications (%) | 798,765 | 5.15 | 22.1 | 0 | 0 | 0 |
| Balance Modifications (%) | 798,765 | 4.97 | 21.73 | 0 | 0 | 0 |
| Delinquency Non-Mortgage (%) | 798,765 | 15.24 | 35.94 | 0 | 0 | 0 |
| Annual Income ($) | 798,765 | 64,234 | 37,971 | 36,611 | 57,000 | 85,000 |
| % Commission | 798,765 | 0.73 | 5.18 | 0 | 0 | 0 |
| Hourly Wage | 515,171 | 24.86 | 12.49 | 16.71 | 21.42 | 29.2 |
| Hours Worked | 544,818 | 51.17 | 25.25 | 40 | 40 | 77 |
| Change Job (%) | 798,765 | 0.81 | 8.97 | 0 | 0 | 0 |

Table A2: Borrower & Loan Characteristics and Mortgage Delinquency

This table reports the predictors of mortgage delinquency for borrowers that became 120+ days delinquent between Feb-Mar 2020 and were in default and not foreclosed as of August 2021. The following OLS regression is estimated on an individual-month panel between June 2019 to August 2021.

$$y_{i,t} = \beta_1 \times Unemployment_{i,t} + \beta_2 \times Credit\ Utilization_{i,t} + \beta_3 \times LTV_{i,t} + \sum \beta_k Expenses_{k,i,t} + \theta_i + \gamma_{z,t} + \epsilon_{i,t}$$

where $y_{i,t}$ is a dummy coded as 1 if individual $i$ has a delinquent mortgage in year-month $t$ and 0 otherwise. LTV is imputed using zip code house price index from CoreLogic. $Expenses_k$ includes indicators for outstanding medical, child support and utility debt/expenses in columns (1-2) and the growth in the medical, utility and child support expense from $t-1$ to $t$ in columns (3-4). $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ for zipcode x year-month fixed effects.

| | Mortgage Delinquency = {0,1} | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Unemployment | 0.013*** | 0.012*** | 0.013*** | 0.012*** |
| | (0.003) | (0.003) | (0.003) | (0.003) |
| Credit Utilization | 0.036** | 0.017* | 0.036** | 0.017* |
| | (0.016) | (0.010) | (0.017) | (0.010) |
| LTV | | 0.048 | | 0.048 |
| | | (0.032) | | (0.032) |
| Outstanding Medical Debt | 0.082*** | 0.050*** | | |
| | (0.014) | (0.014) | | |
| Outstanding Child Support | 0.027** | 0.036*** | | |
| | (0.013) | (0.013) | | |
| Outstanding Utilty Debt | 0.085*** | 0.069*** | | |
| | (0.007) | (0.008) | | |
| Medical Debt Growth | | | 0.012* | 0.012* |
| | | | (0.007) | (0.006) |
| Child Support Expense Growth | | | 0.0006* | 0.0007** |
| | | | (0.0003) | (0.0003) |
| Utility Expense Growth | | | 0.014 | 0.017 |
| | | | (0.011) | (0.012) |
| Individual FE | Yes | Yes | Yes | Yes |
| Zip FE $\times MonthFE$ | Yes | Yes | Yes | Yes |
| N | 487,727 | 453,782 | 487,859 | 453,910 |
| $R^2$ | 0.460 | 0.455 | 0.460 | 0.455 |

Table A3: Transition Rate from 120 to 120+ Days Delinquency in March 2020

This table reports the percentage of loans that transitioned from 120 to 120+ delinquency in March 2020 for different sample of states based on the timing of lockdown imposition. The first row corresponds to sample of loans in states which were the earliest to go into lockdown i.e., between March 15 and March 21, 2020. Similarly, the second and thir row correspond to loans in states where lockdown was imposed between March 22-March 31 2020 and April 2020 onwards respectively. *None* in row 4 represents states where no official stay home orders were passed by state authorities.

| Lockdown Begins | % Loans |
|---|---|
| March 15 - March 21 | 32.45% |
| March 22 - March 31 | 33.44% |
| April onwards | 31.97% |
| None | 33.38% |

Table A4: Foreclosure Delays and Sample Attrition

This table estimates the change in the likelihood of attrition around the CFPB amendment estimated on a balanced individual-month panel:

$$y_{i,z,h,t} = \beta \times Treated_{i,z,h} \times Post_t^{Sep2021} + \theta_i + \gamma_{z,t} + \delta_{h,t} + \eta_{w,t} + \phi_{s,t} + \epsilon_{i,z,h,t}$$

where $y$ is an indicator variable coded as 1 if individual $i$, residing in zipcode $z$ employed in industry $h$ drops out of the sample in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $Post^{Sep2021}$ is an indicator that equals one for months September 2021 onwards and 0 otherwise. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects, $\delta_{h,t}$ indicates industry x year-month fixed effects, $\eta_{w,t}$ indicates wage quartile bins-and credit score quartile bins-time effects are given by $\phi_{s,t}$. Zipcode, industry, wage bins and credit score are measured prior to treatment. The sample time period is between September 2020 to April 2023. Standard errors are robust to heteroskedasticity and are clustered at the six-digit NAICS code level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Inactive | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Post^{Sep2021} \times Treated$ | -0.003 | 0.002 | 0.001 | 0.003 |
| | (0.008) | (0.011) | (0.009) | (0.009) |
| N | 285,192 | 285,192 | 285,192 | 284,340 |
| $R^2$ | 0.519 | 0.569 | 0.589 | 0.596 |
| Individual FE | Yes | Yes | Yes | Yes |
| Month FE | Yes | No | No | No |
| Zipcode $\times$ *Month FE* | No | Yes | Yes | Yes |
| Industry $\times$ *Month FE* | No | No | Yes | Yes |
| Wagebin $\times$ *Month FE* | No | No | No | Yes |
| Scorebin $\times$ *Month FE* | No | No | No | Yes |

Table A5: Foreclosure Delays and Labor Income: Non Binding Loans

This table reports the effect of CFPB's amendment to mortgage servicing guidelines regarding foreclosure proceeding on individual earnings, estimated on an individual-month panel:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h} \times Post_t^{Sep2021} + \theta_i + \gamma_{z,t} + \delta_{h,t} + \eta_{w,t} + \phi_{s,t} + \epsilon_{i,z,h,t}$$

where $y$ represents log earnings for individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $Post^{Sep2021}$ is an indicator that equals one for months September 2021 onwards and 0 otherwise. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects, $\delta_{h,t}$ indicates industry x year-month fixed effects, $\eta_{w,t}$ indicates wage quartile bins-and credit score quartile bins-time effects are given by $\phi_{s,t}$. Zipcode, industry, wage bins and credit score are measured prior to treatment. Panels A-B report estimates for loans not foreclosed but current as of August 2021 and foreclosed as of August 2021 respectively. The sample time period is between September 2020 to April 2023. Standard errors are robust to heteroskedasticity and are clustered at the six-digit NAICS code level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Income | | | |
|---|---|---|---|---|
| | Panel A: Current and Not Foreclosed as of August 2021 | | | |
| | (1) | (2) | (3) | (4) |
| $Post^{Sep2021} \times Treated$ | 0.006 | 0.005 | 0.005 | 0.003 |
| | (0.005) | (0.005) | (0.005) | (0.006) |
| N | 469,152 | 469,152 | 469,152 | 435,665 |
| $R^2$ | 0.839 | 0.847 | 0.849 | 0.851 |
| | Panel B: Foreclosed as of August 2021 | | | |
| | (1) | (2) | (3) | (4) |
| $Post^{Sep2021} \times Treated$ | 0.001 | -0.006 | -0.009 | -0.010 |
| | (0.008) | (0.008) | (0.008) | (0.010) |
| N | 165,613 | 165,613 | 165,613 | 156,873 |
| $R^2$ | 0.855 | 0.877 | 0.880 | 0.882 |
| Individual FE | Yes | Yes | Yes | Yes |
| Month FE | Yes | No | No | No |
| Zipcode $\times$ $Month\ FE$ | No | Yes | Yes | Yes |
| Industry $\times$ $Month\ FE$ | No | No | Yes | Yes |
| Wagebin $\times$ $Month\ FE$ | No | No | No | Yes |
| Scorebin $\times$ $Month\ FE$ | No | No | No | Yes |

Table A6: Foreclosure Delays and Labor Income: Collapsed

This table reports the effect of CFPB's amendment to mortgage servicing guidelines regarding foreclosure proceedings on individual earnings relative to the loans for which the policy was non-binding, estimated on an individual-month panel:
$y_{i,z,h,t} = \beta_1 \times Treated_{i,z,h} \times Post_t^{Sep2021} \times Binding_{i,z,h} + \beta_2 \times Treated_{i,z,h} \times Post_t^{Sep2021} + \theta_i + \gamma_{z,t} + \delta_{h,t} + \epsilon_{i,z,h,t}$
where $y$ represents log earnings for individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$. *Treated* is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. *Binding* is 1 if a loan is not foreclosed and in default as of August 2021 and 0 for loans reported as current for which foreclosure process was not started as of August 2021 in Column (1); mortgages subject to foreclosure proceedings as of August 2021 in column (2) respectively. $Post^{Sep2021}$ is an indicator that equals one for months September 2021 onwards and 0 otherwise. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects, $\delta_{h,t}$ indicates industry x year-month fixed effects, $\eta_{w,t}$ indicates wage quartile bins-and credit score quartile bins-time effects are given by $\phi_{s,t}$. Zipcode, industry are measured prior to treatment. The sample time period is between September 2020 to April 2023. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the industry level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Income | |
|---|---|---|
| | Foreclosure Not Started & Current | Foreclosure Started |
| | (1) | (2) |
| $Post^{Sep2021} \times Treated \times Binding$ | 0.020** | 0.025** |
| | (0.009) | (0.012) |
| $Post^{Sep2021} \times Treated$ | 0.005 | -0.005 |
| | (0.005) | (0.008) |
| $Post^{Sep2021} \times Binding$ | -0.003 | -0.005 |
| | (0.006) | (0.009) |
| N | 633,152 | 329,613 |
| $R^2$ | 0.850 | 0.867 |
| Individual FE | Yes | Yes |
| Zipcode $\times$ $Month\ FE$ | Yes | Yes |
| Industry $\times$ $Month\ FE$ | Yes | Yes |

Table A7: Foreclosure Delays and Labor Income: Excluding Loans in Forbearance as of August 2021

This table reports the effect of CFPB's amendment to mortgage servicing guidelines regarding foreclosure initiation on individual labor earnings, estimated on an individual-month panel:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h} \times Post_t^{Sep2021} + \theta_i + \gamma_{z,t} + \delta_{h,t} + \eta_{w,t} + \phi_{s,t} + \epsilon_{i,z,h,t}$$

where $y$ represents log earnings for individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $Post^{Sep2021}$ is an indicator that equals one for months September 2021 onwards and 0 otherwise. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects, $\delta_{h,t}$ indicates industry x year-month fixed effects, $\eta_{w,t}$ indicates wage quartile bins-and credit score quartile bins-time effects are given by $\phi_{s,t}$. Zipcode, industry, wage bins and credit score are measured prior to treatment. The sample excludes loans which were in forbearance as of August 2021. The sample time period is between September 2020 to April 2023. Standard errors are robust to heteroskedasticity and are clustered at the six-digit NAICS code level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Income | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Post^{Sep2021} \times Treated$ | 0.023** | 0.019** | 0.017** | 0.019*** |
| | (0.009) | (0.009) | (0.008) | (0.008) |
| N | 159,637 | 159,637 | 159,637 | 159,247 |
| $R^2$ | 0.852 | 0.874 | 0.877 | 0.878 |
| Individual FE | Yes | Yes | Yes | Yes |
| Month FE | Yes | No | No | No |
| Zipcode $\times$ $Month\ FE$ | No | Yes | Yes | Yes |
| Industry $\times$ $Month\ FE$ | No | No | Yes | Yes |
| Wagebin $\times$ $Month\ FE$ | No | No | No | Yes |
| Scorebin $\times$ $Month\ FE$ | No | No | No | Yes |

Table A8: Foreclosure Delays and Labor Income: Excluding Modified Loans

This table reports the effect of CFPB's amendment to mortgage servicing guidelines regarding foreclosure initiation on individual labor earnings, estimated on an individual-month panel:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h} \times Post_t^{Sep2021} + \theta_i + \gamma_{z,t} + \delta_{h,t} + \eta_{w,t} + \phi_{s,t} + \epsilon_{i,z,h,t}$$

where $y$ represents log earnings for individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $Post^{Sep2021}$ is an indicator that equals one for months September 2021 onwards and 0 otherwise. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects, $\delta_{h,t}$ indicates industry x year-month fixed effects, $\eta_{w,t}$ indicates wage quartile bins-and credit score quartile bins-time effects are given by $\phi_{s,t}$. Zipcode, industry, wage bins and credit score are measured prior to treatment. The sample excludes loans which were in forbearance as of August 2021. The sample time period is between September 2020 to April 2023. Standard errors are robust to heteroskedasticity and are clustered at the six-digit NAICS code level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Income | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Post^{Sep2021} \times Treated$ | 0.029** | 0.023** | 0.019* | 0.019* |
| | (0.110) | (0.010) | (0.009) | (0.010) |
| N | 109,991 | 109,991 | 109,991 | 109,677 |
| $R^2$ | 0.844 | 0.875 | 0.880 | 0.881 |
| Individual FE | Yes | Yes | Yes | Yes |
| Month FE | Yes | No | No | No |
| Zipcode $\times$ $Month\ FE$ | No | Yes | Yes | Yes |
| Industry $\times$ $Month\ FE$ | No | No | Yes | Yes |
| Wagebin $\times$ $Month\ FE$ | No | No | No | Yes |
| Scorebin $\times$ $Month\ FE$ | No | No | No | Yes |

Table A9: Foreclosure Delays and Labor Income: Alternate Clustering

This table reports the effect of CFPB's amendment to mortgage servicing guidelines regarding foreclosure initiation on individual labor earnings, estimated on an individual-month panel:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h} \times Post_t^{Sep2021} + \theta_i + \gamma_{z,t} + \delta_{h,t} + \eta_{w,t} + \phi_{s,t} + \alpha_{a,t} + \Gamma_{d,t} + \epsilon_{i,z,h,t}$$

where $y$ represents log income for individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $Post^{Sep2021}$ is an indicator that equals one for months September 2021 onwards and 0 otherwise. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects, $\delta_{h,t}$ indicates industry x year-month fixed effects, $\eta_{w,t}$ indicates wage quartile bins, credit score quartile bins-time effects are given by $\phi_{s,t}$, loan size and loan term interacted with month fixed effects are denoted as $\alpha_{a,t}$ and $\Gamma_{d,t}$ respectively. Standard errors are robust to heteroskedasticity and are clustered at the individual level in Column 1 and zip code level in Column (2). Zipcode, industry, wage bins and credit score are measured as of February 2020. The sample time period is between September 2020 to April 2023. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Income | |
|---|---|---|
| | (1) | (2) |
| $Post^{Sep2021} \times Treated$ | 0.020** | 0.020*** |
| | (0.010) | (0.011) |
| N | 164,000 | 164,000 |
| $R^2$ | 0.874 | 0.874 |
| Individual FE | Yes | Yes |
| Zipcode × $Month\ FE$ | Yes | Yes |
| Industry × $Month\ FE$ | Yes | Yes |

Table A10: Foreclosure Delays and Normalized Labor Income

This table reports the effect of CFPB's amendment to mortgage servicing guidelines regarding foreclosure initiation on individual labor earnings, estimated on an individual-month panel:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h} \times Post_t^{Sep2021} + \theta_i + \gamma_{z,t} + \delta_{h,t} + \eta_{w,t} + \phi_{s,t} + \alpha_{a,t} + \Gamma_{d,t} + \epsilon_{i,z,h,t}$$

where $y$ represents normalized earnings for individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $Post^{Sep2021}$ is an indicator that equals one for months September 2021 onwards and 0 otherwise. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects, $\delta_{h,t}$ indicates industry x year-month fixed effects, $\eta_{w,t}$ indicates wage quartile bins, credit score quartile bins-time effects are given by $\phi_{s,t}$, loan size and loan term interacted with month fixed effects are denoted as $\alpha_{a,t}$ and $\Gamma_{d,t}$ respectively. Zipcode, industry, wage bins and credit score are measured as of February 2020. The sample time period is between September 2020 to April 2023. Standard errors are robust to heteroskedasticity and are clustered at the six-digit NAICS code level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Normalised Income | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Post^{Sep2021} \times Treated$ | 0.027** | 0.025*** | 0.022** | 0.023*** |
| | (0.009) | (0.008) | (0.008) | (0.008) |
| N | 164,000 | 164,000 | 164,000 | 163,610 |
| $R^2$ | 0.895 | 0.912 | 0.914 | 0.915 |
| Individual FE | Yes | Yes | Yes | Yes |
| Month FE | Yes | No | No | No |
| Zipcode $\times$ *Month FE* | No | Yes | Yes | Yes |
| Industry $\times$ *Month FE* | No | No | Yes | Yes |
| Wagebin $\times$ *Month FE* | No | No | No | Yes |
| Scorebin $\times$ *Month FE* | No | No | No | Yes |

Table A11: Income by Number of Jobs

This table reports the summary of income for individuals with multiple jobs versus single job.

| Statistic | Mean | St. Dev. | p25 | Median | p75 |
|---|---|---|---|---|---|
| Jobs_>1 | 47,659 | 38,541 | 14,354 | 36,777 | 67,196 |
| Jobs_=1 | 65,543 | 37,615 | 38,282 | 58,234 | 85,915 |

Table A12: Heterogeneity by Monthly Mortgage Payment

This table reports the heterogeneous effect of CFPB's amendment to mortgage servicing guidelines regarding foreclosure proceeding on individual earnings by size of monthly mortgage payments, estimated on an individual-month panel:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h} \times Post_t^{Sep2021} + \theta_i + \gamma_{z,t} + \delta_{h,t} + \epsilon_{i,z,h,t}$$

where $y$ represents log earnings for individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $Post^{Sep2021}$ is an indicator that equals one for months September 2021 onwards and 0 otherwise. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects, $\delta_{h,t}$ indicates industry x year-month fixed effects. Zipcode and industry are measured prior to treatment. The sample is split based on the median size of monthly mortgage payment. The sample time period is between September 2020 to April 2023. Standard errors are robust to heteroskedasticity and are clustered at the six-digit NAICS code level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Income | |
|---|---|---|
| | Mortgage Payment$_{>=Median}$ | Mortgage Payment$_{<Median}$ |
| | (1) | (2) |
| $Post^{Sep2021} \times Treated$ | 0.036** | 0.010 |
| | (0.018) | (0.014) |
| N | 78,740 | 85,693 |
| $R^2$ | 0.888 | 0.882 |
| Individual FE | Yes | Yes |
| Zipcode $\times$ $Month\ FE$ | Yes | Yes |
| Industry $\times$ $Month\ FE$ | Yes | Yes |

Table A13: Heterogeneity by Missed Mortgage Payments

This table reports the heterogeneous effect of CFPB's amendment to mortgage servicing guidelines regarding foreclosure proceeding on individual earnings by whether individuals missed their mortgage payments during the policy effective period, estimated on an individual-month panel:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h} \times Post_t^{Sep2021} + \theta_i + \gamma_{z,t} + \delta_{h,t} + \epsilon_{i,z,h,t}$$

where $y$ represents log earnings for individual $i$, residing in zipcode $z$ employed in industry $h$ in year-month $t$. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $Post^{Sep2021}$ is an indicator that equals one for months September 2021 onwards and 0 otherwise. $\theta_i$ denotes individual fixed effects, $\gamma_{z,t}$ is zipcode x year-month fixed effects, $\delta_{h,t}$ indicates industry x year-month fixed effects. Zipcode and industry are measured prior to treatment. The sample is split based on whether individuals missed mortgage payments between September-December 2021. The sample time period is between September 2020 to April 2023. Standard errors are robust to heteroskedasticity and are clustered at the six-digit NAICS code level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Income | |
|---|---|---|
| | Missed Mortgage Payments | Not Missed Mortgage Payments |
| | (1) | (2) |
| $Post^{Sep2021} \times Treated$ | 0.023** | -0.016 |
| | (0.009) | (0.018) |
| N | 111,325 | 57,271 |
| $R^2$ | 0.877 | 0.906 |
| Individual FE | Yes | Yes |
| Zipcode $\times$ $Month\ FE$ | Yes | Yes |
| Industry $\times$ $Month\ FE$ | Yes | Yes |

Table A14: Foreclosure Delays and Mortgage Modification

This table reports the effect of of CFPB's amendment to mortgage servicing guidelines regarding foreclosure proceedings on the likelihood of mortgage modification, estimated on an individual-month panel:

$$y_{i,z,h,t} = \beta_{DD} \times Treated_{i,z,h,t} \times Post_t^{Sep2021} + \theta_i + \gamma_{z,t} + \delta_{h,t} + \epsilon_{i,z,h,t}$$

where $y$ is an indicator variable coded as 1 if individual $i$ residing in zipcode $z$ employed in industry $h$ contains a flag for modification in calender month $t$ and 0 otherwise. $Treated$ is a binary variable that takes a value of 1 if the mortgage loan associated with the individual became 120+days delinquent in March 2020 and 0 if the loan became 120+days delinquent in February 2020. $Post^{Sep2021}$ is an indicator variable equal to 1 for period September 2021 and after and 0 for the months before that. The coefficient $\beta_{DD}$ represents the change in the outcome variable in the months around treatment, conditional on $\theta_i$ i.e., individual fixed effects, $\gamma_{z,t}$ for zipcode x year-month fixed effects and $\delta_{h,t}$ indicating industry x year-month fixed effects. The sample time period is between September 2020 to April 2023. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the six-digit NAICS code level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

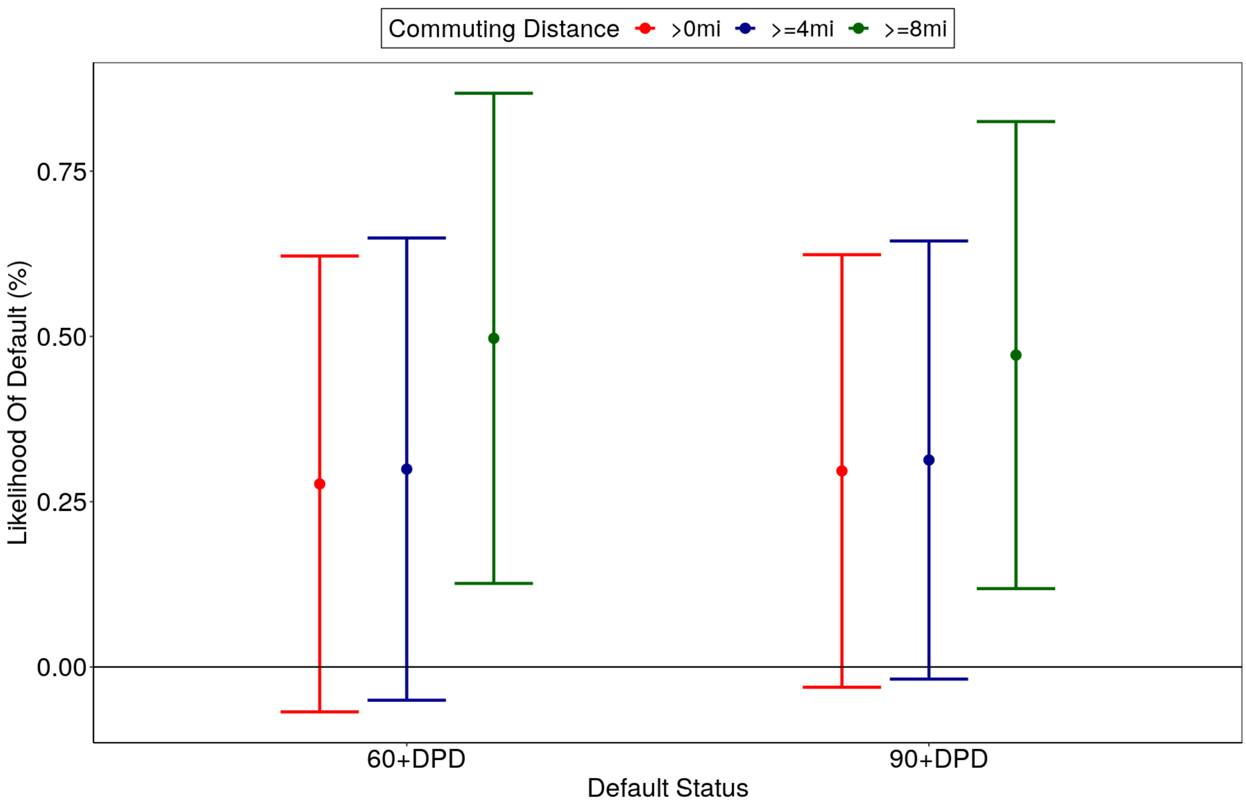
| | Modification | | |
|---|---|---|---|
| | Overall | Maturity Extension | Balance Reduction |
| | (1) | (2) | (3) |
| Post^Sep2021$\times$ $Treated$ | 0.003 | 0.005** | 0.004* |
| | (0.010) | (0.002) | (0.003) |
| N | 155,557 | 155,557 | 155,557 |
| $R^2$ | 0.790 | 0.575 | 0.652 |
| Individual FE | Yes | Yes | Yes |
| Zipcode FE $\times$ $Month\ FE$ | Yes | Yes | Yes |
| Industry FE $\times$ $Month\ FE$ | Yes | Yes | Yes |

# Appendix B

# Households' Ability to Weather Adverse Shocks: Role of Firm Monopsonies

## B.1 Additional Figures and Tables

Figure B1: Alternative Definitions of Commuting Far




*Notes:* This figure plots the $\beta_k$ coefficients along with the corresponding 95confidence intervals from Figure 2.5.

Table B1: Credit Card Default and Gas Price Shock: DiD

This table reports the difference-difference results from equation xx and shows heterogeneity by access to public transit. Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. $LowTransit$ is a dummy and takes a value equal to 1 if the transit score is in the bottom 10 percentile (p10) and 0 otherwise. $Commuting\ Far$ is a dummy which is 1 for distance travelled to work beyond 8 miles(p80) one way and 0 if distance between home and work zip is 0. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | 60+DPD | | | 90+DPD | | |
|---|---|---|---|---|---|---|
| | Low Transit | High Transit | Diff | Low Transit | High Transit | Diff |
| Variables | (1) | (2) | (1) - (2) | (4) | (5) | (4) - (5) |
| Post × $Commuting\ Far$ | 1.09*** | 0.468*** | 0.622*** | 1.09*** | 0.524*** | 0.566*** |
| | (0.225) | (0.075) | | (0.215) | (0.071) | |
| N | 923,851 | 6,424,205 | | 923,851 | 6,424,205 | |
| $AdjR^2$ | 0.881 | 0.863 | | 0.886 | 0.868 | |
| Individual FE | Y | Y | | Y | Y | |
| Zipcode ×$Month\ FE$ | Y | Y | | Y | Y | |
| Wage Bin ×$Month\ FE$ | Y | Y | | Y | Y | |
| Score Bin ×$Month\ FE$ | Y | Y | | Y | Y | |

Table B2: Heterogeneity by Homeownership

This table reports the heterogeneity in the consumer default around the gas price shock based individual's homeownership status.Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. $LowTransit$ is a dummy and takes a value equal to 1 if the transit score is in the bottom 10 percentile (p10) and 0 otherwise. $Commuting\ Far$ is a dummy which is 1 for distance travelled to work beyond 8 miles(p80) one way and 0 if distance between home and work zip is 0. Homeowner(NotHomeowner) is coded as 1 if the individual had (didnot have) a mortgage at anytime prior to the shock. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | 60+DPD | | | 90+DPD | | |
|---|---|---|---|---|---|---|
| | Homeowner | Not Homeowner | Diff | Homeowner | Not Homeowner | Diff |
| Variables | (1) | (2) | (1) - (2) | (4) | (5) | (4) - (5) |
| Post × $Low\ Transit$ × $Commuting\ Far$ | 0.255 | 1.012*** | -0.757** | 0.591** | 0.806** | -0.215 |
| | (0.315) | (0.349) | | (0.289) | (0.331) | |
| Post × $Commuting\ Far$ | 0.468*** | 1.060*** | | 0.556*** | 1.573*** | |
| | (0.103) | (0.107) | | (0.089) | (0.101) | |
| Low Transit× $Commuting\ Far$ | 1.183 | 1.002 | | 1.744* | -0.061 | |
| | (0.914) | (0.947) | | (1.013) | (1.715) | |
| N | 2,963,044 | 4,385,012 | | 2,963,044 | 4,385,012 | |
| $AdjR^2$ | 0.682 | 0.681 | | 0.859 | 0.838 | |
| Individual FE | Y | Y | | Y | Y | |
| Zipcode ×$Month\ FE$ | Y | Y | | Y | Y | |
| Income Deciles FE | Y | Y | | Y | Y | |
| Credit Score Deciles FE | Y | Y | | Y | Y | |

Table B3: Heterogeneity by Non-Compete Enforceability

This table reports the heterogeneity in the consumer default around the gas price shock based on non-compete enforceability in the state of individual's residence and the industry. Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. $LowTransit$ is a dummy and takes a value equal to 1 if the transit score is in the bottom 10 percentile (p10) and 0 otherwise. $Commuting\ Far$ is a dummy which is 1 for distance travelled to work beyond 8 miles(p80) one way and 0 if distance between home and work zip is 0. Above (Below) is coded as 1 if the individual is residing in a state with above median levels of non-compete enforceability in addition to being employed in Wholesale trade (NAICS 42), business services which include Information (NAICS 51) Professional, Scientific, and Technical Services (NAICS 54) and Healthcare (NAICS 62) and 0 otherwise. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | 60+DPD | | | 90+DPD | | |
|---|---|---|---|---|---|---|
| | Above | Below | Diff | Above | Below | Diff |
| Variables | (1) | (2) | (1) - (2) | (4) | (5) | (4) - (5) |
| (7) | (8) | (7) - (8) | | | | |
| Post × *Low Transit* × *Commuting Far* | 2.576** | 0.572 | 2.004** | 1.647* | 0.517*** | 1.13 |
| | (1.045) | (0.208) | | (0.918) | (0.196) | |
| Post × *Commuting Far* | 1.911*** | 1.752*** | | 2.289*** | 2.025*** | |
| | (0.288) | (0.067) | | (0.113) | (0.104) | |
| Low Transit× *Commuting Far* | 1.281 | 0.939 | | 1.259 | 1.163* | |
| | (4.029) | (0.597) | | (3.640) | (0.662) | |
| N | 549,867 | 8,721,844 | | 549,867 | 8,721,844 | |
| $AdjR^2$ | 0.691 | 0.682 | | 0.851 | 0.841 | |
| Individual FE | Y | Y | | Y | Y | |
| Zipcode ×*Month FE* | Y | Y | | Y | Y | |

Table B4: Heterogeneity by Vehicle Trips per Household

This table reports the heterogeneity in the consumer default around the gas price shock by ex-ante vehicle trips per household. Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. $LowTransit$ is a dummy and takes a value equal to 1 if the transit score is in the bottom 10 percentile (p10) and 0 otherwise. $Commuting\ Far$ is a dummy which is 1 for distance travelled to work beyond 8 miles(p80) one way and 0 if distance between home and work zip is 0. Above (Below) is coded as 1 if the individual is residing in zip code with above (below) median vehicle trips per household. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | 60+DPD | | | 90+DPD | | |
|---|---|---|---|---|---|---|
| | Above | Below | Diff | Above | Below | Diff |
| Variables | (1) | (2) | (1) - (2) | (4) | (5) | (4) - (5) |
| Post × *Low Transit* × *Commuting Far* | 0.756** | 0.104 | 0.642*** | 0.857*** | -0.029 | 0.886** |
| | (0.340) | (0.310) | | (0.322) | (0.297) | |
| Post × *Commuting Far* | 0.294*** | 0.629*** | | 0.399*** | 0.612*** | |
| | (0.101) | (0.116) | | (0.094) | (0.109) | |
| Low Transit× *Commuting Far* | 1.89 | 1.36 | | 1.24 | 1.54 | |
| | (1.24) | (1.17) | | (1.18) | (1.10) | |
| N | 3,695,798 | 3,618,287 | | 3,695,798 | 3,618,287 | |
| Adj $R^2$ | 0.866 | 0.867 | | 0.873 | 0.872 | |
| Individual FE | Y | Y | | Y | Y | |
| Zipcode ×*Month FE* | Y | Y | | Y | Y | |
| Wage Bin ×*Month FE* | Y | Y | | Y | Y | |
| Score Bin ×*Month FE* | Y | Y | | Y | Y | |

Table B5: Heterogeneity by 2+ Vehicles Ownership

This table reports the heterogeneity in the consumer default around the gas price shock by ex-ante ownership of two or more vehicles per household. Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. $LowTransit$ is a dummy and takes a value equal to 1 if the transit score is in the bottom 10 percentile (p10) and 0 otherwise. $Commuting\ Far$ is a dummy which is 1 for distance travelled to work beyond 8 miles(p80) one way and 0 if distance between home and work zip is 0. Above (Below) is coded as 1 if the individual is residing in zip code with above (below) median of percentage households with 2+ vehicles per household. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | 60+DPD | | | 90+DPD | | |
|---|---|---|---|---|---|---|
| | Above | Below | Diff | Above | Below | Diff |
| Variables | (1) | (2) | (1) - (2) | (4) | (5) | (4) - (5) |
| Post × *Low Transit* × *Commuting Far* | 0.887*** | 0.028 | 0.859** | 0.880*** | -0.062 | 0.942*** |
| | (0.309) | (0.341) | | (0.293) | (0.328) | |
| Post × *Commuting Far* | 0.275*** | 0.632*** | | 0.415*** | 0.597*** | |
| | (0.101) | (0.115) | | (0.095) | (0.108) | |
| Low Transit× *Commuting Far* | 1.15 | 2.27* | | 0.795 | 1.87* | |
| | (1.24) | (1.18) | | (1.17) | (1.11) | |
| N 3,710,893 | 3,603,192 | | 3,710,893 | 3,603,192 | | |
| Adj $R^2$ | 0.867 | 0.867 | | 0.874 | 0.871 | |
| Individual FE | Y | Y | | Y | Y | |
| Zipcode ×*Month FE* | Y | Y | | Y | Y | |
| Wage Bin ×*Month FE* | Y | Y | | Y | Y | |
| Score Bin ×*Month FE* | Y | Y | | Y | Y | |

Table B6: Heterogeneity by Fuel Oil Consumption

This table reports the heterogeneity in the consumer default around the gas price shock by ex-ante fuel oil consumption.Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. $LowTransit$ is a dummy and takes a value equal to 1 if the transit score is in the bottom 10 percentile (p10) and 0 otherwise. $Commuting\ Far$ is a dummy which is 1 for distance travelled to work beyond 8 miles(p80) one way and 0 if distance between home and work zip is 0. Above (Below) is coded as 1 if the individual is residing in zip code with above (below) median fuel oil consumption per household. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | 60+DPD | | | 90+DPD | | |
|---|---|---|---|---|---|---|
| | Above | Below | Diff | Above | Below | Diff |
| Variables | (1) | (2) | (1) - (2) | (4) | (5) | (4) - (5) |
| Post × $Low\ Transit$ × $Commuting\ Far$ | 1.206*** | 0.281 | 0.925** | 0.992*** | 0.459 | 0.533 |
| | (0.332) | (0.389) | | (0.312) | (0.368) | |
| Post × $Commuting\ Far$ | 1.740*** | 1.885*** | | 1.591*** | 1.787*** | |
| | (0.092) | (0.106) | | (0.113) | (0.104) | |
| Low Transit× $Commuting\ Far$ | -0.892 | 1.482 | | 1.074 | 1.301 | |
| | (1.052) | (1.001) | | (1.100) | (1.204) | |
| N | 3,471,528 | 3,528,469 | | 3,471,528 | 3,528,469 | |
| $Adj R^2$ | 0.684 | 0.684 | | 0.844 | 0.846 | |
| Individual FE | Y | Y | | Y | Y | |
| Zipcode ×$Month\ FE$ | Y | Y | | Y | Y | |
| Income Deciles FE | Y | Y | | Y | Y | |
| Credit Score Deciles FE | Y | Y | | Y | Y | |

Table B7: Heterogeneity by Gasoline Price Change

This table reports the heterogeneity in the consumer default around the gas price shock by the extent of change in local gas prices around the shock. Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. $LowTransit$ is a dummy and takes a value equal to 1 if the transit score is in the bottom 10 percentile (p10) and 0 otherwise. $Commuting\ Far$ is a dummy which is 1 for distance travelled to work beyond 8 miles(p80) one way and 0 if distance between home and work zip is 0. Above (Below) is coded as 1 if the individual is residing in county with above (below) median gasoline price change around the shock. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | 60+DPD | | | 90+DPD | | |
|---|---|---|---|---|---|---|
| | Above | Below | Diff | Above | Below | Diff |
| Variables | (1) | (2) | (1) - (2) | (4) | (5) | (4) - (5) |
| Post × *Low Transit* × *Commuting Far* | 1.058** | 0.587 | 0.471 | 1.004** | 0.229 | 0.775* |
| | (0.464) | (0.403) | | (0.428) | (0.378) | |
| Post × *Commuting Far* | 1.936*** | 2.262*** | | 1.968*** | 2.292*** | |
| | (0.106) | (0.115) | | (0.099) | (0.108) | |
| Low Transit× *Commuting Far* | 0.947 | -1.351 | | -0.938 | 1.311 | |
| | (1.308) | (1.288) | | (1.459) | (1.430) | |
| N | 3,382,804 | 3,333,195 | | 3,382,804 | 3,333,195 | |
| $AdjR^2$ | 0.679 | 0.684 | | 0.841 | 0.843 | |
| Individual FE | Y | Y | | Y | Y | |
| Zipcode ×*Month FE* | Y | Y | | Y | Y | |

Table B8: Heterogeneity by Remote Work

This table reports the heterogeneity in the consumer default around the gas price shock by the possibility to work from home. Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. $LowTransit$ is a dummy and takes a value equal to 1 if the transit score is in the bottom 10 percentile (p10) and 0 otherwise. $Commuting\ Far$ is a dummy which is 1 for distance travelled to work beyond 8 miles(p80) one way and 0 if distance between home and work zip is 0. Below (Above) is coded as 1 if the individual is residing in zipcode with below (above) median share of individuals employed in skilled scalable services (SSS) i.e., characterised by Information (NAICS 51), Finance and Insurance (NAICS 52), Professional Services (NAICS 54), and Management of Companies (NAICS 55). Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | 60+DPD | | | 90+DPD | | |
|---|---|---|---|---|---|---|
| | Below | Above | Diff | Below | Above | Diff |
| Variables | (1) | (2) | (1) - (2) | (4) | (5) | (4) - (5) |
| Post × $Low\ Transit$ × $Commuting\ Far$ | 0.953*** | 0.515 | 0.438 | 0.846*** | 0.256 | 0.59* |
| | (0.330) | (0.362) | | (0.313) | (0.334) | |
| Post × $Commuting\ Far$ | 1.790*** | 1.701*** | | 1.968*** | 2.292*** | |
| | (0.118) | (0.095) | | (0.099) | (0.108) | |
| Low Transit× $Commuting\ Far$ | 0.113 | 1.279 | | 3.453 | 1.324 | |
| | (1.424) | (0.949) | | (1.448) | (1.103) | |
| N | 3,617,626 | 3,744,935 | | 3,617,626 | 3,744,935 | |
| $AdjR^2$ | 0.686 | 0.677 | | 0.842 | 0.848 | |
| Individual FE | Y | Y | | Y | Y | |
| Zipcode ×$Month\ FE$ | Y | Y | | Y | Y | |
| Income Deciles FE | Y | Y | | Y | Y | |
| Credit Score Deciles FE | Y | Y | | Y | Y | |

Table B9: Income Adjustment around Gasoline Price Shock by Employer Quit Rates

This table reports the results of the OLS regression specified in Equation 2.5. The regression is estimated on a firm-month panel. Sample period is between September 2022 - January 2023. The main dependent variable is logarithm of average firm level income measured at each calender month. $QuitRate_{<Median}$ is a dummy coded as 1 for firms with below median quit rate and 0 for above median quit rate. The Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. Zipcode is the zip of firm location. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Income |
|---|---|
| Variables | (1) |
| Post × Quit Rate_<Median | -0.014*** |
| | (0.003) |
| Firm FE | Y |
| Zipcode $\times Month\ FE$ | Y |
| N | 1,896,359 |
| $Adj R^2$ | 0.440 |

Table B10: Income Adjustment around Gasoline Price Shock by Job-to-Job Separation Rate

This table reports the results of the OLS regression specified in Equation 2.5. The regression is estimated on a individual-firm-zip-month panel. Sample period is between September 2022 - January 2023. The main dependent variable is logarithm of average firm level income measured at each calender month. Slack is coded as 1 if the individual works in a zipcode with job-to-job separation rate in the Tight is when the job-to-job separation rate at the work zip is in the top thirty percent. The Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. Zipcode is the zip of firm location. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Income |
|---|---|
| Variables | (1) |
| Post × Slack | -0.004* |
| | (0.002) |
| Individual FE | Y |
| Employer FE | Y |
| Month FE | Y |
| Work-Zipcode | Y |
| N | 3,718,705 |
| $AdjR^2$ | 0.930 |

Table B11: Income Adjustment around Gasoline Price Shock by Employer Market Share

This table reports the results of the OLS regression specified in Figure 2.5. The regression is estimated on a individual-employer-month panel. Sample period is between September 2022 - January 2023. The main dependent variable is logarithm of average firm level income measured at each calender month. High is coded as 1 if the individual's employer is in the top 30 percent of the distribution of firm market share by county in column (1), commuting zone in column (2) and commuting zone-industry in column (3) respectively. Low is 1 if the individual's employer is in the bottom 30 percent of the distribution of firm market share by county (commuting zone). The Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. Post is a dummy equal to 1 for months February 2022 and beyond and 0 otherwise. Employer Zipcode is the zip of firm location. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

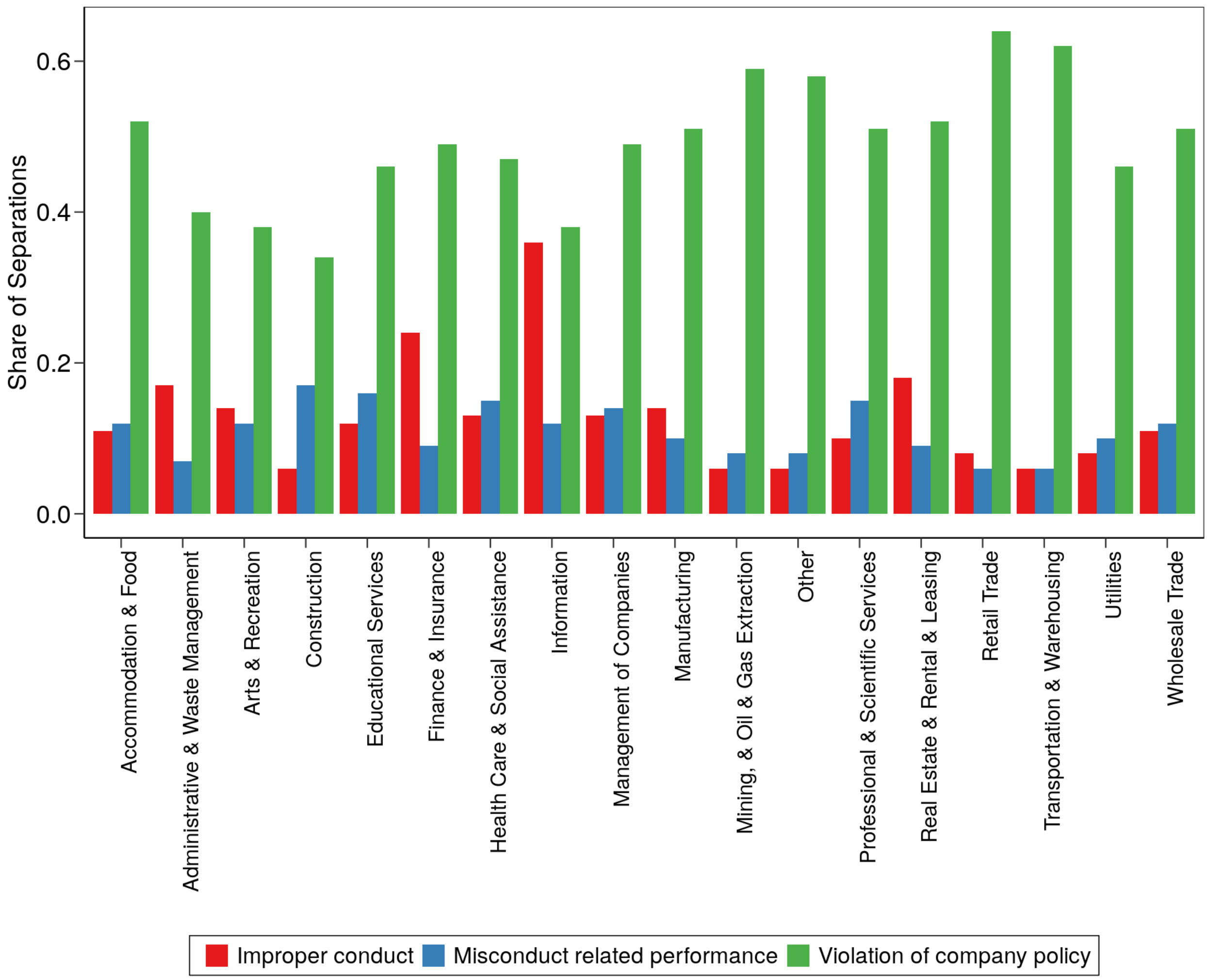
| | County | | Commuting Zone | |
|---|---|---|---|---|
| | Log Income | Log VariablePay | Log Income | Log VariablePay |
| Variables | (1) | (2) | (3) | (4) |
| Post $\times High$ | -0.010*** | -0.203*** | -0.012*** | -0.178*** |
| | (0.002) | (0.020) | (0.002) | (0.018) |
| Individual FE | Y | Y | Y | Y |
| Employer FE | Y | Y | Y | Y |
| Employer-Zipcode $\times Month\ FE$ | Y | Y | Y | Y |
| Industry $\times Month\ FE$ | Y | Y | Y | Y |
| N | 2,634,821 | 2,634,336 | 2,627,672 | 2,627,201 |
| $AdjR^2$ | 0.940 | 0.832 | 0.939 | 0.832 |

# Appendix C

# External Labor Market Punishment in Finance

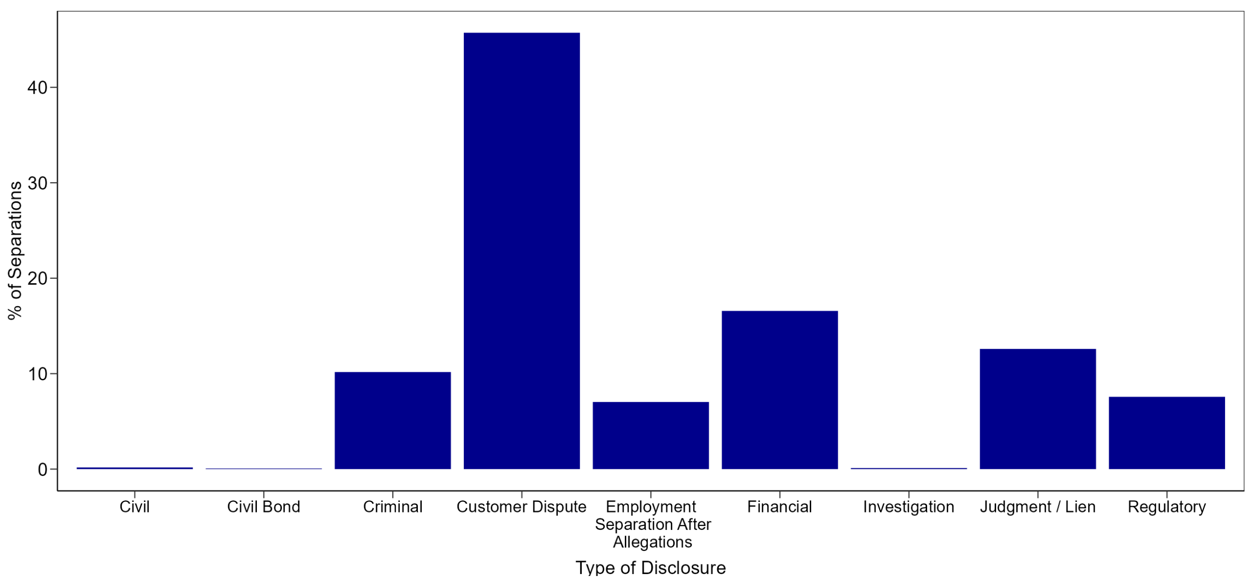
## C.1 Additional Figures and Tables

Figure C1: Distribution of Misconduct Separations by Industry



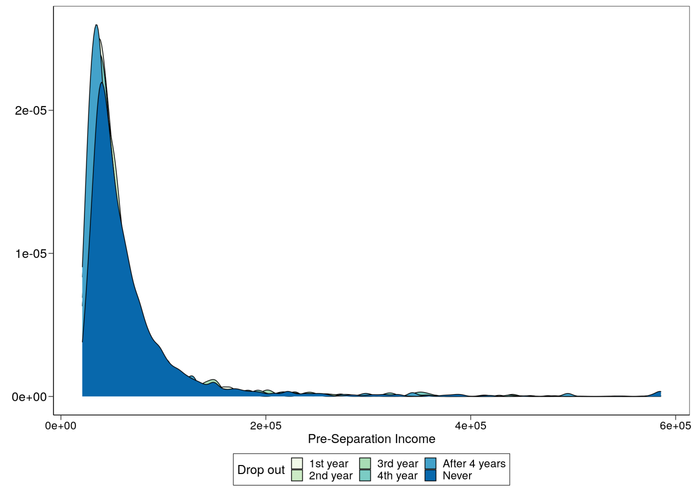

*Notes:* This figure plots the distribution of top three sub categories of misconduct separations by sectors in the economy.

Figure C2: Distribution of Misconduct Separations by Disclosure Type

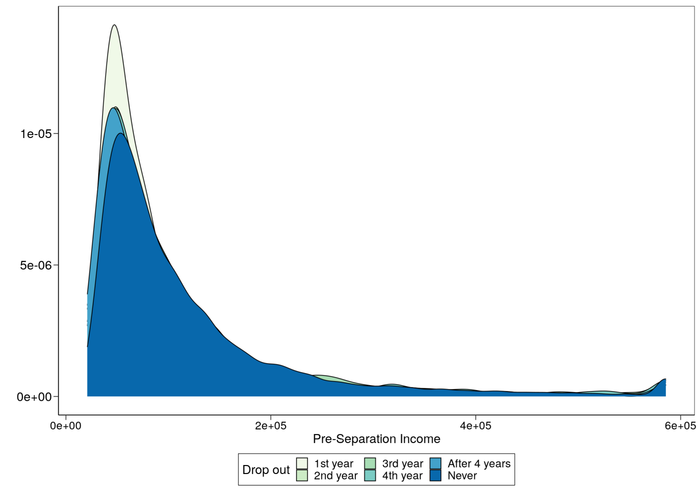



*Notes:* This figure plots the distribution of misconduct separations by disclosure type for matched sample of financial advisors receiving misconduct disclosure in the FINRA disclosure database.

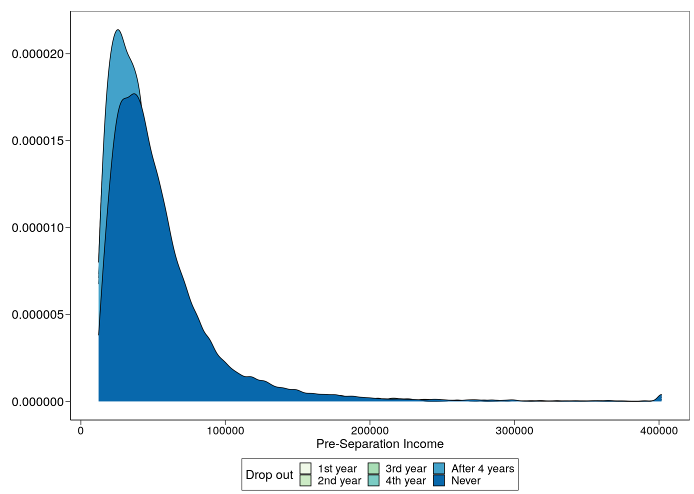
Figure C3: Distribution of Pre-Separations Income by Sample Attrition



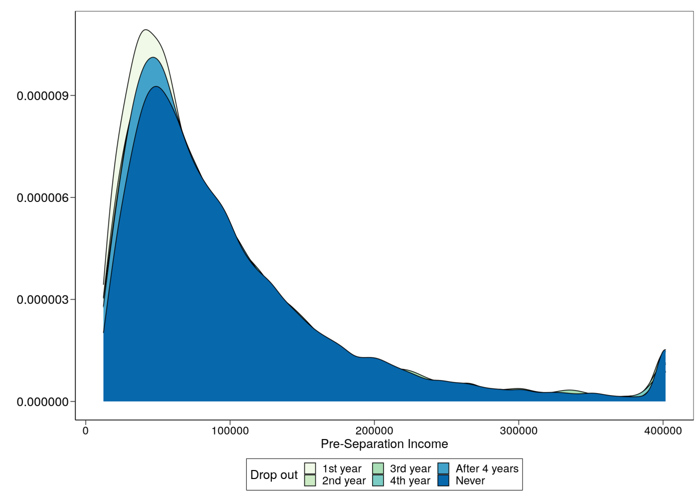

(a) Finance: Misconduct




(b) Finance: No Fault



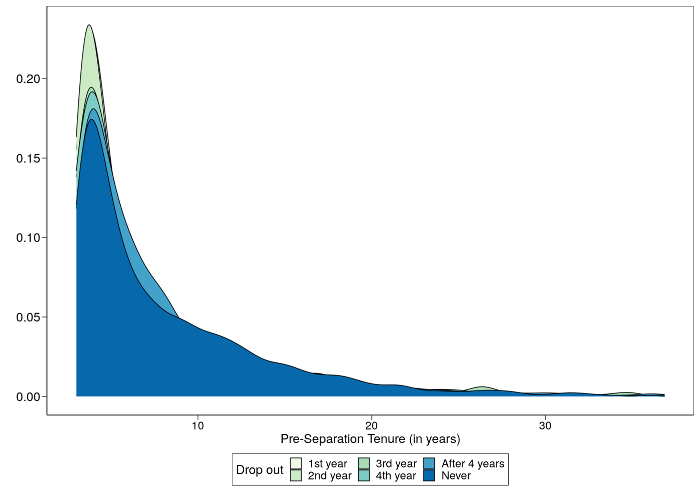

(c) Non-Finance: Misconduct



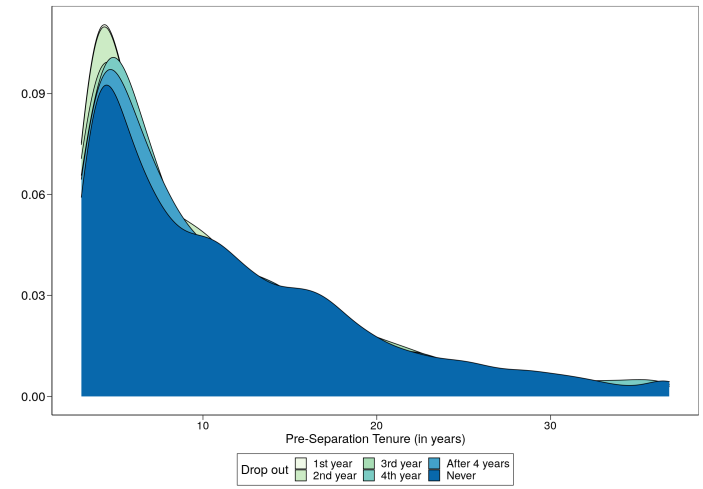

(d) Non-Finance: No Fault

*Notes:* This figure plots the distribution of pre-separation income by separation reason. Drop out $1^{st}$ year corresponds to individuals who drop out of the sample within one year of separation. Similarly definition applies to others. *Never* is for individuals who do not drop out.

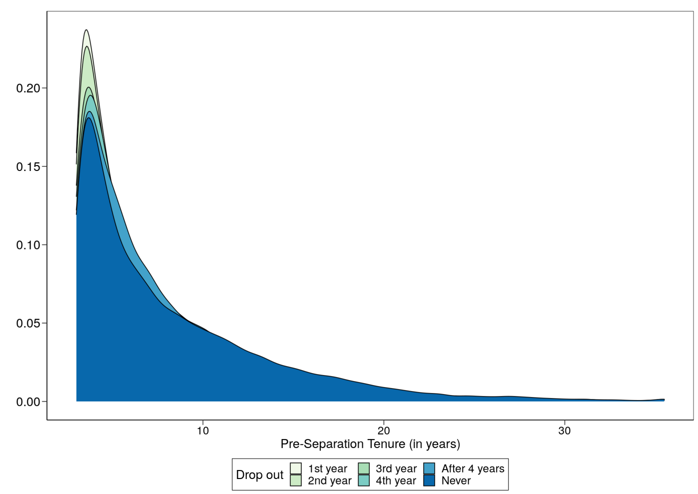
Figure C4: Distribution of Pre-Separations Tenure by Sample Attrition



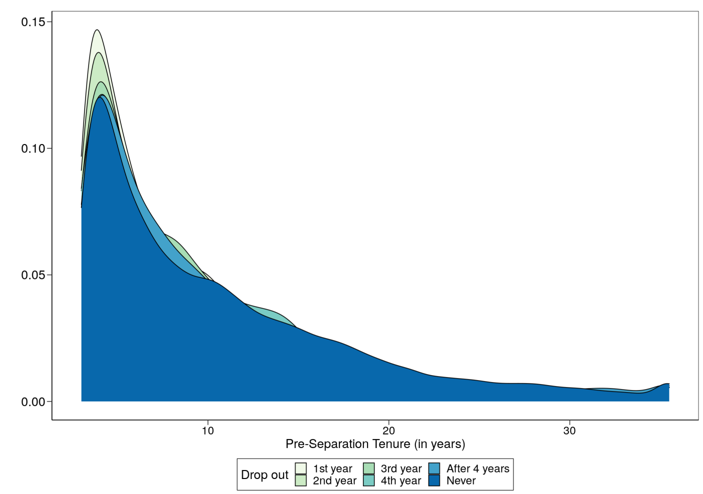

(a) Finance: Misconduct




(b) Finance: No Fault Compensation




(c) Non-Finance: Misconduct




(d) Non-Finance: No Fault

*Notes:* This figure plots the distribution of pre-separation tenure by separation reason. Drop out $1^{st}$ year corresponds to individuals who drop out of the sample within one year of separation. Similarly definition applies to others. *Never* is for individuals who do not drop out.

Table C1: Misconduct Firing: Top 10 Separation Reasons

This table summarizes the distribution of misconduct separations across the top 10 sub-categories. Each row reports the proportion of misconduct firings attributable to a certain sub-category. While Column (1) reports the distribution within the finance sector, Column (2) does so for the non-finance sectors.

| | Proportion of Separations | |
|---|---|---|
| | Finance | Non-Finance |
| | (1) | (2) |
| Violation of Company Policy | 0.49 | 0.54 |
| Improper Conduct | 0.24 | 0.11 |
| Misconduct Related Performance | 0.09 | 0.09 |
| Gross Misconduct | 0.03 | 0.04 |
| Removal of Company Property or Funds | 0.01 | 0.03 |
| Falsification of Records | 0.02 | 0.02 |
| Violation of Safety Rules | 0.00 | 0.02 |
| Insubordination | 0.01 | 0.02 |
| Falsification | 0.02 | 0.02 |
| Failure to Report | 0.01 | 0.02 |
| Total | 0.92 | 0.92 |

Table C2: Matched Sample: Mean Income

This table summarizes the sample means for pre-separation income by separation types and across sectors for two types of matching. While columns (1) and (3) report means for the sample using pre-separation income as the matching variable, columns (2) and (4) report the means for the one using pre-separation income within the same separating firm, tenure, and location (measured by 3-digit zip code).

| | Finance | | Non-Finance | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Matching variables*: | Income | Income w/in firm tenure & location | Income | Income w/in firm, tenure & location |
| | Pre-Separation Income ($) | | | |
| Misconduct | 63,357 | 61,989 | 60,570 | 57,487 |
| No-Fault | 66,804 | 101,590 | 71,896 | 89,850 |
| Non-Separated | 62,883 | 80,275 | 79,011 | 69,913 |
| | Pre-Separation Log Income | | | |
| Misconduct | 11.05 | 11.03 | 10.83 | 10.95 |
| No-Fault | 11.07 | 11.52 | 11.09 | 11.40 |
| Non-Separated | 11.05 | 11.29 | 11.27 | 11.15 |

Table C3: Income following Misconduct Separation: Matched Sample

This table reports results of the OLS regressions specified in Equation 3.3. In Panel A, sample comprises of employees involuntarily separated either for no fault or misconduct and their corresponding non-separated counterparts, matched on pre-separation income. In Panel B, the the matching is performed within a separation firm, location and tenure bucket. *Misconduct* is an indicator equal to 1 if an employee was separated for misconduct between 2011 to 2018 and 0 otherwise. *Separated* is a dummy equal to 1 if the employee was separated between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Industry* refers to the 6 digit NAICS code for the separated firm, *Wage Bins* are constructed at $1,000 width for pre-separation income, *Firm* represents the separated firm, *Location* corresponds to the 3-Digit Zipcode, *Tenure* is measured as of the month prior to separation, and *Separation Cohort* refers to the year of separation. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | Finance | | Non-Finance | |
| **Panel A: Matched on Income** | | | | |
| | (1) | (2) | (3) | (4) |
| Misconduct× *Separated* × *Post* | 0.028* | 0.031** | -0.031*** | -0.061*** |
| | (0.008) | (0.008) | (0.003) | (0.003) |
| Separated × *Post* | -0.256*** | -0.250*** | -0.315*** | -0.313*** |
| | (0.006) | (0.006) | (0.002) | (0.002) |
| N | 9,327,648 | 9,327,648 | 44,015,882 | 44,015,882 |
| $Adj.R^2$ | 0.828 | 0.828 | 0.821 | 0.852 |
| **Panel B: Matched on Income within Separation Firm, Tenure, & Location** | | | | |
| Misconduct× *Separated* × *Post* | 0.042*** | 0.041*** | -0.055*** | -0.063*** |
| | (0.008) | (0.008) | (0.005) | (0.004) |
| Separated × *Post* | -0.302** | -0.302*** | -0.287*** | -0.313*** |
| | (0.005) | (0.005) | (0.004) | (0.003) |
| N | 6,462,636 | 6,462,636 | 24,870,188 | 24,870,188 |
| $Adj.R^2$ | 0.862 | 0.862 | 0.815 | 0.838 |
| Individual FE | Y | Y | Y | Y |
| Wage Bin × *Month FE* | Y | Y | Y | Y |
| Industry × *Month FE* | Y | N | Y | N |
| Firm × *Location* × *Tenure* × *Year FE* | N | Y | N | Y |
| Separation Cohort × *Year FE* | N | Y | N | Y |

Table C4: Income following No fault Mass-Layoffs: All Industries

This table reports the results of the OLS regressions specified in Equation 3.1. The sample comprises employees from all sectors laid off for no fault and their corresponding non-separated counterparts. *Layoff* is an indicator equal to 1 if a worker was laid off as part of a mass layoff between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Industry* refers to the 6 digit NAICS code for the separated firm, and *Wage Bins* are constructed at \$1,000 width for pre-separation income. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | |
|---|---|---|---|
| Variables | (1) | (2) | (3) |
| Layoff × *Post* | -0.225*** | -0.230*** | -0.225*** |
| | (0.010) | (0.010) | (0.010) |
| Individual FE | Y | Y | Y |
| Month FE | Y | N | N |
| Industry × $Month\ FE$ | N | Y | Y |
| Wage Bin × $Month\ FE$ | N | N | Y |
| N | 7,174,790 | 7,174,790 | 7,174,790 |
| $Adj.R^2$ | 0.854 | 0.858 | 0.858 |

Table C5: Income Following Separation: Controlling for type of job

This table reports log earnings following separation. Column (1)-(2) reports the estimates for individuals separated from the finance sector, and columns (3)-(4) report them for separations from the non-finance sector. *Misconduct* is an indicator equal to 1 if an employee was separated for misconduct between 2011 to 2018 and 0 otherwise. *Separated* is a dummy equal to 1 if the employee was separated between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Wage Bins* are constructed at $1,000 width for pre-separation income, *Firm* represents the separated firm, *Location* corresponds to the 3-Digit Zipcode, *Tenure* is measured as of the month prior to separation, and *Separation Cohort* refers to the year of separation. *Job-Title* is measured pre-separation. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | Finance | | Non-Finance | |
| | (1) | (2) | (3) | (4) |
| Misconduct × *Separated* × *Post* | 0.028*** | 0.021*** | -0.069*** | -0.066*** |
| | (0.007) | (0.007) | (0.003) | (0.004) |
| Separated × *Post* | -0.320*** | -0.322*** | -0.310*** | -0.319*** |
| | (0.004) | (0.005) | (0.002) | (0.004) |
| Individual FE | Y | Y | Y | Y |
| Wage Bin × *Industry* × *Month FE* | Y | Y | Y | Y |
| Firm × *Location* × *Tenure* × *Year FE* | Y | Y | Y | Y |
| Separation Cohort × *Year FE* | Y | Y | Y | Y |
| Firm × *JobTitle* × *Year FE* | N | Y | N | Y |
| N | 21,152,903 | 20,242,834 | 52,471,961 | 48,007,728 |
| $Adj.R^2$ | 0.896 | 0.917 | 0.849 | 0.866 |

Table C6: Heterogeneity by Complaints

This table reports heterogeneity in our findings based on different levels of consumer complaints received against or non-timely response rates of the rehiring employers to these complaints. Columns (1) and (2) report the estimates for sub-sample of employees rehired by employers with above and below median levels of fraudulent complaints respectively. Similarly columns (3) and (4) report the estimates for those rehired by employers with above and below median levels of non-timely response rates. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | Fraud Related Complaints | | Non-Timely Response | |
| | Above Median (1) | Below Median (2) | Above Median (3) | Below Median (4) |
| Misconduct × *Separated* × *Post* | 0.060*** | 0.054** | 0.078*** | 0.055*** |
| | (0.013) | (0.023) | (0.017) | (0.014) |
| Separated × *Post* | -0.198*** | -0.250*** | -0.231*** | -0.201*** |
| | (0.009) | (0.014) | (0.010) | (0.011) |
| Individual FE | Y | Y | Y | Y |
| Wage Bin × *Month FE* | Y | Y | Y | Y |
| Firm × *Location* × *Tenure* × *Year FE* | Y | Y | Y | Y |
| Separation Cohort × *Year FE* | Y | Y | Y | Y |
| Hiring Firm Size × *Hiring Firm Industry* | Y | Y | Y | Y |
| N | 6,656,028 | 4,348,275 | 5,124,947 | 5,879,356 |
| $Adj.R^2$ | 0.901 | 0.899 | 0.904 | 0.898 |

Table C7: Heterogeneity by Repeat Offenders

This table reports heterogeneity by repeat versus non repeat miscount offenders. While column (1) reports the estimates for repeat offenders i.e., employees separated more than once for misconduct from the finance sector, column (2) reports them for one-time offenders. Similarly Columns (3)-(4) show this heterogeneity for the non-finance sector. *Misconduct* is an indicator equal to 1 if an employee was separated for misconduct between 2011 to 2018 and 0 otherwise. *Separated* is a dummy equal to 1 if the employee was separated between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Wage Bins* are constructed at $1,000 width for pre-separation income, *Firm* represents the separated firm, *Location* corresponds to the 3-Digit Zipcode, *Tenure* is measured as of the month prior to separation, and *Separation Cohort* refers to the year of separation. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | Finance | | Non-Finance | |
| | Repeat Offenders | Onetime Offenders | Repeat Offenders | Onetime Offenders |
| | (1) | (2) | | |
| Misconduct × *Separated* × *Post* | 0.038*** | 0.030*** | -0.073*** | -0.065*** |
| | (0.014) | (0.007) | (0.006) | (0.003) |
| Separated × *Post* | -0.319*** | -0.320*** | -0.308*** | -0.311*** |
| | (0.004) | (0.004) | (0.003) | (0.003) |
| Individual FE | Y | Y | | |
| Wage Bin × *Month FE* | Y | Y | Y | Y |
| Firm × *Location* × *Year FE* | Y | Y | Y | Y |
| Separation Cohort × *Year FE* | Y | Y | Y | Y |
| N | 20,286,503 | 20,854,483 | 45,115,144 | 49,929,933 |
| $Adj.R^2$ | 0.898 | 0.898 | 0.857 | 0.855 |

Table C8: Heterogeneity in Assortative Matching by Repeat Offenders

This table reports heterogeneity in our findings based on types of separated and rehiring employers separately for repeat vs one time misconduct offenders. The different types of employers are measured as proportion of fraud-related complaints received against firms. While columns (1)-(2) report the estimates for sub-sample of employees separated from employers with above median levels of fraudulent complaints who get rehired by firms with below median levels of complaints, columns (3)-(4) report results for opposite moves. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | Fraud Related Complaints | | | |
| | Above to Below Median | | Below to Above Median | |
| | Repeat-Offenders | Onetime-Offenders | Repeat-Offenders | Onetime-Offenders |
| | (1) | (2) | (3) | (4) |
| Misconduct$\times$ $Separated \times$ $Post$ | -0.037 | 0.041 | 0.119** | 0.074** |
| | (0.075) | (0.031) | (0.055) | (0.034) |
| Separated $\times$ $Post$ | -0.186*** | -0.186*** | -0.213*** | -0.214*** |
| | (0.012) | (0.012) | (0.017) | (0.017) |
| Individual FE | Y | Y | Y | Y |
| Wage Bin $\times$ $Month\ FE$ | Y | Y | Y | Y |
| Firm $\times$ $Location \times$ $Tenure \times$ $Year\ FE$ | Y | Y | Y | Y |
| Separation Cohort $\times$ $Year\ FE$ | Y | Y | Y | Y |
| N | 6,815,984 | 6,843,078 | 4,851,089 | 4,865,561 |
| $Adj.R^2$ | 0.906 | 0.906 | 0.902 | 0.901 |

Table C9: Heterogeneity by Geographic Makeup of Hiring Firm Location: Finance Sales vs Non-Sales Professionals

This table reports heterogeneity in our findings based on the geographic makeup of the hiring firm zipcodes. The sample consists of only finance job profiles within the finance industry. Panel A reports estimates for sales professionals and panel B reports estimates for non-sales professionals respectively. Columns (1) and (2) report the estimates for sub-sample of employees rehired by establishments in zipcodes with above and below median levels of % population with college education respectively. Similarly columns (3) and (4) report the estimates for those rehired by employers in zipcodes with above and below median levels of % population 65 years and older. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | %college | | %65 or older | |
| | Above Median (1) | Below Median (2) | Above Median (3) | Below Median (4) |
| **Panel A: Sales Professionals** | | | | |
| Misconduct × *Separated* × *Post* | -0.006 | 0.122*** | 0.160*** | 0.104** |
| | (0.060) | (0.046) | (0.054) | (0.057) |
| Separated × *Post* | -0.226*** | -0.231*** | -0.268*** | -0.150*** |
| | (0.030) | (0.029) | (0.037) | (0.041) |
| N | 8,193,364 | 8,196,187 | 8,196,637 | 8,195,475 |
| $Adj.R^2$ | 0.911 | 0.911 | 0.911 | 0.911 |
| **Panel B: Non-Sales Professionals** | | | | |
| Misconduct × *Separated* × *Post* | 0.091 | 0.089 | 0.132** | 0.108 |
| | (0.072) | (0.083) | (0.077) | (0.095) |
| Separated × *Post* | -0.260*** | -0.112** | -0.214*** | -0.065 |
| | (0.047) | (0.065) | (0.041) | (0.078) |
| N | 8,181,222 | 8,178,702 | 8,181,910 | 8,181,826 |
| $Adj.R^2$ | 0.911 | 0.911 | 0.911 | 0.911 |
| Individual FE | Y | Y | Y | Y |
| Wage Bin × *Month FE* | Y | Y | Y | Y |
| Firm × *Location* × *Tenure* × *Year FE* | Y | Y | Y | Y |
| Separation Cohort × *Year FE* | Y | Y | Y | Y |

Table C10: Time to Re-employment

This table reports the average time (in months) for separated employees to be rehired after separation (conditional on being rehired). This statistic is reported separately for individuals belonging to the top 10% and bottom 90% of income distribution respectively. The sample is restricted to finance professionals only.

| | Time to Re-employment (in Months) | |
|---|---|---|
| | Top 10% | Bottom 90% |
| Misconduct | 4.5 | 4.8 |
| Layoff | 5.1 | 5.4 |

Table C11: Industry Departure Rates

This table reports departure rates from the finance sector defined as the share of employees who find employment outside the finance industry following separation. Departure rate is measured over either a two year (Panel A) or a 4 year (Panel B) horizon following separation. Industry is either classified using 6-Digit NAICS or 2-Digit NAICS code.

| | Misconduct | Layoff | Misconduct vs Layoff |
|---|---|---|---|
| | (1) | (2) | (1) - (2) |
| | *A. Within 2 Years of Separation* | | |
| 6-Digit | 69.4% | 74.9% | -5.5%*** |
| 2-Digit | 59.0% | 60.0% | -1.0%** |
| | *B. Within 4 Years of Separation* | | |
| 6-Digit | 70.6% | 75.6% | -5.0%*** |
| 2-Digit | 60.0% | 60.7% | -0.7%* |

Table C12: Misconduct Firing: Top 10 Separation Reasons

This table summarizes the distribution of misconduct separations across the top 10 sub-categories across finance and non-finance sectors by separating firm size. Each row reports the mean proportion of misconduct firings attributable to a certain sub-category. While columns (1)-(2) report the distribution within the finance sector columns (3)-(4) do so for the non-finance sectors.

| | Proportion of Separations | | | |
|---|---|---|---|---|
| | Finance | | Non-Finance | |
| | Below Median Size | Above Median Size | Below Median Size | Above Median Size |
| | (1) | (2) | (3) | (4) |
| Violation of Company Policy | 0.50 | 0.49 | 0.50 | 0.56 |
| Improper Conduct | 0.17 | 0.25 | 0.10 | 0.13 |
| Misconduct Related Performance | 0.11 | 0.08 | 0.12 | 0.07 |
| Gross Misconduct | 0.07 | 0.02 | 0.04 | 0.04 |
| Removal of Company Property or Funds | 0.01 | 0.01 | 0.02 | 0.03 |
| Falsification of Records | 0.03 | 0.02 | 0.03 | 0.02 |
| Violation of Safety Rules | 0.00 | 0.00 | 0.03 | 0.02 |
| Insubordination | 0.01 | 0.00 | 0.03 | 0.02 |
| Falsification | 0.03 | 0.02 | 0.02 | 0.02 |
| Failure to Report | 0.01 | 0.00 | 0.02 | 0.01 |
| Total | 0.92 | 0.92 | 0.91 | 0.92 |

Table C13: Heterogeneity by Type of Misconduct

This table reports heterogeneity in our findings by the type of misconduct. While columns (1) and (3) report the estimates for the sub-sample of employees separated for violation of company policies, columns (2) and (4) report the estimates for sub-sample of employees separated for all other misconduct reasons. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | Finance | | Non-Finance | |
| | Violation of Company Policy | Other Reasons | Violation of Company Policy | Other Reasons |
| | (1) | (2) | (3) | (4) |
| Misconduct × *Separated* × *Post* | 0.046*** | 0.052*** | -0.075*** | -0.059*** |
| | (0.011) | (0.011) | (0.004) | (0.003) |
| Separated × *Post* | -0.199*** | -0.199*** | -0.309*** | -0.309*** |
| | (0.005) | (0.005) | (0.003) | (0.003) |
| Individual FE | Y | Y | Y | Y |
| Wage Bin × *Month FE* | Y | Y | Y | Y |
| Firm × *Location* × *Tenure* × *Year FE* | Y | Y | Y | Y |
| Separation Cohort × *Year FE* | Y | Y | Y | Y |
| N | 19,268,640 | 19,280,184 | 48,036,954 | 47,008,123 |
| $Adj.R^2$ | 0.901 | 0.901 | 0.853 | 0.852 |

Table C14: Income following Misconduct Separation for Sales Professionals

This table reports heterogeneity in log earnings following separation for sales professionals across finance and non-finance sectors. While column (1) reports the estimates for employees separated from the finance sector column (2) reports the results for other sectors. *Misconduct* is an indicator equal to 1 if an employee was separated for misconduct between 2011 to 2018 and 0 otherwise. *Separated* is a dummy equal to 1 if the employee was separated between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Wage Bins* are constructed at $1,000 width for pre-separation income, *Firm* represents the separated firm, *Location* corresponds to the 3-Digit Zipcode, *Tenure* is measured as of the month prior to separation, and *Separation Cohort* refers to the year of separation. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | |
|---|---|---|
| | Finance | Non-Finance |
| | (1) | (2) |
| Misconduct × *Separated* × *Post* | 0.056* | -0.071*** |
| | (0.030) | (0.017) |
| Separated × *Post* | -0.350*** | -0.344*** |
| | (0.023) | (0.014) |
| Individual FE | Y | Y |
| Wage Bin × $Month\ FE$ | Y | Y |
| Firm × *Location* × *Tenure* × $Year\ FE$ | Y | Y |
| Separation Cohort × $Year\ FE$ | Y | Y |
| N | 1,727,408 | 3,918,612 |
| $Adj.R^2$ | 0.957 | 0.956 |

Table C15: Distribution of Labor Related Lawsuits by Industry

This table reports the percentage of labor related lawsuits in each Fama-French 12 industry between 2015 and 2019.

| Industry | Mean % Labor Lawsuits |
|---|---|
| Consumer Non-durables | 3.82 |
| Consumer Durables | 0.92 |
| Manufacturing | 3.71 |
| Oil, Gas & Coal | 2.98 |
| Chemicals | 2.31 |
| Business Equipment | 6.18 |
| Telecommunications | 9.47 |
| Utilities | 0.00 |
| Wholesale & retail trade | 5.97 |
| Healthcare | 0.11 |
| Finance | 2.17 |
| Other | 2.96 |

Table C16: Heterogeneity by Seniority in Rank

This table reports results of the OLS regressions specified in Equation 3.3. The sample comprises of employees involuntarily separated either for misconduct or no fault. Columns (1)-(2) report the estimates for employees separated from the finance sector defined as all firms in the NAICS code of 52. Columns (3)-(4) report the estimates for employees separated from all other sectors. Junior (Senior) refers to employee rank in the separated firm. *Misconduct* is an indicator equal to 1 if an employee was separated for misconduct between 2011 to 2018 and 0 otherwise. *Separated* is a dummy equal to 1 if the employee was separated between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Industry* refers to the 6 digit NAICS code for the separated firm, *Wage Bins* are constructed at \$1,000 width for pre-separation income, *Firm* represents the separated firm, *Location* corresponds to the 3-Digit Zipcode, *Tenure* is measured as of the month prior to separation, and *Separation Cohort* refers to the year of separation. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | Finance | | Non-Finance | |
| | Junior | Senior | Junior | Senior |
| | (1) | (2) | (3) | (4) |
| Misconduct× *Separated* × *Post* | 0.034*** | 0.032** | -0.031*** | -0.047*** |
| | (0.006) | (0.010) | (0.004) | (0.004) |
| Separated × *Post* | -0.320*** | -0.226*** | -0.333*** | -0.176*** |
| | (0.004) | (0.004) | (0.004) | (0.002) |
| Individual FE | Y | Y | Y | Y |
| Wage Bin × *Month FE* | Y | Y | Y | Y |
| Firm × *Location* × *Tenure* × *Year FE* | Y | Y | Y | Y |
| Separation Cohort × *Year FE* | Y | Y | Y | Y |
| N | 10,831,300 | 11,099,147 | 25,544,132 | 26,927,829 |
| $Adj.R^2$ | 0.760 | 0.889 | 0.739 | 0.870 |

Table C17: Income following Separation: Rehiring Income

This table reports log of rehiring earnings following separation. While Column (1) reports the estimates for employees separated from the finance sector Column (2) reports them other sectors. *Misconduct* is an indicator equal to 1 if an employee was separated for misconduct between 2011 to 2018 and 0 otherwise. *Separated* is a dummy equal to 1 if the employee was separated between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Wage Bins* are constructed at $1,000 width for pre-separation income, *Firm* represents the separated firm, *Location* corresponds to the 3-Digit Zipcode, *Tenure* is measured as of the month prior to separation, and *Separation Cohort* refers to the year of separation. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | |
|---|---|---|
| | Finance | Non-Finance |
| | (1) | (2) |
| Misconduct × *Separated* × *Post* | 0.021*** | -0.082*** |
| | (0.007) | (0.003) |
| Separated × *Post* | -0.402*** | -0.416*** |
| | (0.005) | (0.003) |
| Individual FE | Y | Y |
| Wage Bin × *Month FE* | Y | Y |
| Firm × *Location* × *Year FE* | Y | Y |
| Separation Cohort × *Year FE* | Y | Y |
| N | 12,442,898 | 32,299,656 |
| $Adj.R^2$ | 0.912 | 0.862 |

Table C18: Income Following Separation: Financial Advisors

This table reports log earnings following separation. The sample for misconduct related separations is restricted to financial advisors that received misconduct disclosures in the FINRA BrokerCheck database. *Misconduct* is an indicator equal to 1 if an employee was separated for misconduct between 2011 to 2018 and 0 otherwise. *Separated* is a dummy equal to 1 if the employee was separated between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Wage Bins* are constructed at $1,000 width for pre-separation income, *Firm* represents the separated firm, *Location* corresponds to the 3-Digit Zipcode, *Industry* refers to the 6 digit *NAICS* code, as of the month prior to separation. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Misconduct × *Separated* × *Post* | 0.032 | 0.038* | 0.036* | 0.036* |
| | (0.021) | (0.021) | (0.021) | (0.021) |
| Separated × *Post* | -0.303*** | -0.301*** | -0.301*** | -0.299*** |
| | (0.004) | (0.004) | (0.004) | (0.004) |
| Individual FE | Y | Y | Y | Y |
| Industry × *Month* | Y | N | Y | N |
| Wage Bin × *Month FE* | N | Y | Y | Y |
| Firm × *Location* × *Year FE* | N | N | N | Y |
| N | 20,093,747 | 20,093,747 | 20,093,747 | 20,093,747 |
| $Adj.R^2$ | 0.881 | 0.881 | 0.882 | 0.892 |

Table C19: Industry Departure Rates by Misconduct Reasons

This table reports departure rates from the finance sector defined as the share of employees who find employment outside the finance sector following separation. Departure rate is measured over a two year horizon following separation in Panel A and over a 4 year in Panel B. Industry is either classified using 6-Digit NAICS or 2-Digit NAICS code.

| | Top-3 Misconduct Reasons | Others |
|---|---|---|
| | (1) | (2) |
| | *A. Within 2 Years of Separation* | |
| 6-Digit | 68.5% | 73.2% |
| 2-Digit | 57.8% | 64.2% |
| | *B. Within 4 Years of Separation* | |
| 6-Digit | 69.9% | 73.7% |
| 2-Digit | 59.0% | 64.5% |

Table C20: Sample Drop-out Rate

This table summarizes the drop-out rates in our sample by separation types and across sectors.

| | Drop out rate | | |
|---|---|---|---|
| | Overall | Finance | Non-Finance |
| Misconduct | 55.1% | 51.1% | 65.3% |
| No-Fault | 69.0% | 66.16% | 69.55% |

Table C21: Income following Separation: Imputed Missing Income

This table reports results of the OLS regressions specified in Equation 3.3. The sample comprises of employees involuntarily separated either for misconduct or no fault. Columns (1)-(2) report the estimates for employees separated from the finance sector defined as all firms in the NAICS code of 52. Columns (3)-(4) report the estimates for employees separated from all other sectors. *Misconduct* is an indicator equal to 1 if an employee was separated for misconduct between 2011 to 2018 and 0 otherwise. *Separated* is a dummy equal to 1 if the employee was separated between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Industry* refers to the 6 digit NAICS code for the separated firm, *Wage Bins* are constructed at $1,000 width for pre-separation income, *Firm* represents the separated firm, *Location* corresponds to the 3-Digit Zipcode, *Tenure* is measured as of the month prior to separation, and *Separation Cohort* refers to the year of separation. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | |
|---|---|---|---|---|
| | Finance | | Non-Finance | |
| | Bottom percentile | Previous Income | Bottom percentile | Previous Income |
| | (1) | (2) | (3) | (4) |
| Misconduct× *Separated* × *Post* | 0.038*** | 0.015** | -0.100*** | -0.056*** |
| | (0.010) | (0.007) | (0.004) | (0.003) |
| Separated × *Post* | -0.809*** | -0.281*** | -0.875*** | -0.214*** |
| | (0.007) | (0.004) | (0.004) | (0.002) |
| Individual FE | Y | Y | Y | Y |
| Wage Bin × *Month FE* | Y | Y | Y | Y |
| Firm × *Location* × *Tenure* × *Year FE* | Y | Y | Y | Y |
| Separation Cohort × *Year FE* | Y | Y | Y | Y |
| N | 24,068,892 | 23,991,207 | 62,977,558 | 61,696,994 |
| $Adj.R^2$ | 0.727 | 0.884 | 0.661 | 0.833 |

Table C22: Income following Separation: Heterogeneity by Hiring Firm Industry in Finance

This table reports heterogeneity in log earnings following separation by the hiring firm industry within the finance industry. *Misconduct* is a dummy equal to 1 if a worker was fired for misconduct between 2011 to 2018 and 0 otherwise. *Separated* is a dummy equal to 1 if the individual was separated between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months after misconduct firing and 0 otherwise. *Year* & *Month* refer to the calender year-month. *Wage Bins* are constructed as at \$1,000 width and *Tenure* is constructed as deciles from the distribution of tenure, also measured in the month prior to separation. *Location* corresponds to the 3-Digit Zipcode, *Firm* is the separation firm. *Separation Cohort* refers to the year of separation. Reported standard errors in parentheses are heteroscedasticity-robust and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | | |
|---|---|---|---|---|---|
| | Monetary Authorities | Credit Intermediation | Brokerage & Securities | Insurance | Other Investment |
| | (1) | (2) | (3) | (4) | (5) |
| Misconduct × *Separated* × *Post* | | 0.057*** | 0.043* | 0.021 | -0.004 |
| | | (0.011) | (0.026) | (0.017) | (0.100) |
| Separated × *Post* | | -0.190*** | -0.230*** | -0.203*** | -0.143* |
| | | (0.007) | (0.012) | (0.009) | (0.080) |
| N | | 18,820,963 | 18,309,301 | 18,444,998 | 18,128,694 |
| $Adj.R^2$ | | 0.904 | 0.904 | 0.904 | 0.905 |
| Individual FE | Y | Y | Y | Y | Y |
| Wage Bin × *Month FE* | Y | Y | Y | Y | Y |
| Firm × *Location* × *Tenure* × *Year FE* | Y | Y | Y | Y | Y |
| Separation Cohort × *Year FE* | Y | Y | Y | Y | Y |

Table C23: Heterogeneity by Hiring Firm Region: Finance Separations

This table reports heterogeneity in log earnings following separation for the different sub-samples based on the region where hiring firms are located. The sample is restricted to the finance sector. *Misconduct* is an indicator equal to 1 if an employee was separated for misconduct between 2011 to 2018 and 0 otherwise. *Separated* is a dummy equal to 1 if the employee was separated between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Industry* refers to the 6 digit NAICS code for the separated firm, *Wage Bins* are constructed at $1,000 width for pre-separation income, *Firm* represents the separated firm, *Location* corresponds to the 3-Digit Zipcode, *Tenure* is measured as of the month prior to separation, and *Separation Cohort* refers to the year of separation. Reported standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | | | | |
|---|---|---|---|---|---|
| | Midwest | Northeast | Southeast | Southwest | West |
| | (1) | (2) | (3) | (4) | (5) |
| Misconduct × *Separated* × *Post* | 0.047*** | 0.065*** | 0.038** | -0.006 | 0.046** |
| | (0.016) | (0.014) | (0.016) | (0.030) | (0.012) |
| Separated × *Post* | -0.391*** | -0.296*** | -0.355*** | -0.354*** | -0.334*** |
| | (0.011) | (0.009) | (0.012) | (0.023) | (0.014) |
| Individual FE | Y | Y | Y | Y | Y |
| Wage Bin × *Month FE* | Y | Y | Y | Y | Y |
| Firm × *Location* × *Tenure* × *Year FE* | Y | Y | Y | Y | Y |
| Separation Cohort × *Year FE* | Y | Y | Y | Y | Y |
| N | 5,483,758 | 5,375,957 | 3,264,080 | 1,575,368 | 2,455,024 |
| $Adj.R^2$ | 0.902 | 0.904 | 0.899 | 0.878 | 0.895 |

Table C24: Heterogeneity by Hiring Firm Region: Non-Finance Separations

This table reports heterogeneity in log earnings following separation for the different sub-samples based on the region where hiring firms are located. The sample is restricted to the non-finance sectors. *Misconduct* is an indicator equal to 1 if an employee was separated for misconduct between 2011 to 2018 and 0 otherwise. *Separated* is a dummy equal to 1 if the employee was separated between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Industry* refers to the 6 digit NAICS code for the separated firm, *Wage Bins* are constructed at $1,000 width for pre-separation income, *Firm* represents the separated firm, *Location* corresponds to the 3-Digit Zipcode, *Tenure* is measured as of the month prior to separation, and *Separation Cohort* refers to the year of separation. Reported standard errors are reported in parentheses and clustered at the individual level.

| | Log Earnings | | | | |
|---|---|---|---|---|---|
| | Midwest | Northeast | Southeast | Southwest | West |
| | (1) | (2) | (3) | (4) | (5) |
| Misconduct × $Separated \times\ Post$ | -0.072*** | -0.083*** | -0.071*** | -0.108*** | -0.096** |
| | (0.008) | (0.007) | (0.008) | (0.010) | (0.009) |
| Separated × $Post$ | -0.329*** | -0.285*** | -0.371*** | -0.282*** | -0.293*** |
| | (0.008) | (0.006) | (0.007) | (0.009) | (0.008) |
| Individual FE | Y | Y | Y | Y | Y |
| Wage Bin × $Month\ FE$ | Y | Y | Y | Y | Y |
| Firm × $Location \times\ Tenure \times\ Year\ FE$ | Y | Y | Y | Y | Y |
| Separation Cohort × $Year\ FE$ | Y | Y | Y | Y | Y |
| N | 12,218,374 | 10,999,279 | 10,127,418 | 6,028,041 | 7,625,545 |
| $Adj.R^2$ | 0.854 | 0.868 | 0.844 | 0.860 | 0.871 |

Table C25: Income Following Separation: Cluster at Separation Firm Level

This table reports results of the OLS regressions specified in Equation 3.3. The sample comprises of employees involuntarily separated either for no fault or misconduct and their corresponding non-separated counterparts. Columns (1)-(2) report the estimates for employees separated from the finance sector defined as all firms in the NAICS code of 52. Columns (3)-(4) report the estimates for employees separated from all other sectors. *Misconduct* is an indicator equal to 1 if an employee was separated for misconduct between 2011 to 2018 and 0 otherwise. *Separated* is a dummy equal to 1 if the employee was separated between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Industry* refers to the 6 digit NAICS code for the separated firm, *Wage Bins* are constructed at $1,000 width for pre-separation income, *Firm* represents the separated firm, *Location* corresponds to the 3-Digit Zipcode, *Tenure* is measured as of the month prior to separation, and *Separation Cohort* refers to the year of separation. Robust standard errors are reported in parentheses and clustered at the separating firm level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | |
|---|---|---|
| | Finance (1) | Non-Finance (2) |
| Misconduct × *Separated* × *Post* | 0.028** | -0.069*** |
| | (0.011) | (0.007) |
| Separated × *Post* | -0.320*** | -0.310*** |
| | (0.012) | (0.007) |
| Individual FE | Y | Y |
| Wage Bin × *Month FE* | Y | Y |
| Firm × *Location* × *Tenure* × *Year FE* | Y | Y |
| Separation Cohort × *Year FE* | Y | Y |
| N | 21,152,903 | 52,471,961 |
| $Adj.R^2$ | 0.897 | 0.849 |

Table C26: Income following Separation: Excluding Outliers

This table reports log earnings following separation. While column (1) reports the estimates for employees separated from the finance sector column (2) reports them other sectors. *Misconduct* is an indicator equal to 1 if an employee was separated for misconduct between 2011 to 2018 and 0 otherwise. *Separated* is a dummy equal to 1 if the employee was separated between 2011 to 2018 and 0 otherwise. *Post* is a dummy equal to 1 for months following separation and 0 otherwise. *Month* refers to the calender year-month, *Wage Bins* are constructed at $1,000 width for pre-separation income, *Firm* represents the separated firm, *Location* corresponds to the 3-Digit Zipcode, *Tenure* is constructed as deciles from the distribution of tenure as of the month prior to separation, and *Separation Cohort* refers to the year of separation. The sample excludes individuals in the top and bottom 5% of pre-separation income. Robust standard errors are reported in parentheses and clustered at the individual level. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

| | Log Earnings | |
|---|---|---|
| | Finance | Non-Finance |
| | (1) | (2) |
| Misconduct × *Separated* × *Post* | 0.021*** | -0.067*** |
| | (0.007) | (0.003) |
| Separated × *Post* | -0.402*** | -0.306*** |
| | (0.005) | (0.003) |
| Individual FE | Y | Y |
| Wage Bin × *Month FE* | Y | Y |
| Firm × *Location* × *Year FE* | Y | Y |
| Separation Cohort × *Year FE* | Y | Y |
| N | 12,442,898 | 39,853,346 |
| $Adj.R^2$ | 0.912 | 0.811 |

Table C27: Re-employment Income

This table reports the mean and median re-employment income for separated employees. This statistic is reported separately for finance and non-finance separations respectively.

| | Re-employment Income ($) | | | |
|---|---|---|---|---|
| | Mean | | Median | |
| | Finance | Non Finance | Finance | Non Finance |
| Misconduct | 48,566 | 36,415 | 37,500 | 27,895 |
| No fault | 84,468 | 76,416 | 68,000 | 63,915 |