

Speed Trap or Poverty Trap?

Fines, Fees, and Financial Wellbeing*

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Abstract

There is widespread financial fragility in the United States and anecdotal evidence that even small, unplanned shocks may have adverse effects on the financial situations of fragile households. However, causal evidence on the implications of unanticipated, transitory expense shocks for low-income individuals is scarce. I study the impact of fines for traffic violations on financial health, earnings and employment, and measures of borrowing and consumption using administrative data on traffic citations in Florida linked to high-frequency credit reports and payroll records. Leveraging variation in the timing of traffic stops with event study and difference-in-differences research designs, I find that following a traffic stop, individuals experience increases in financial distress, decreases in the probability of positive payroll earnings, and reductions in measures of consumption and borrowing. The effects are concentrated among the poorest quartile of drivers, where payroll employment falls by eight percent and the increases in financial strain induced by a \$175 fine are observationally similar to what would be predicted by a \$900 earnings decline. While adverse effects are partially caused by constraints on car usage due to driver license suspensions, an important mechanism appears to be a poverty trap whereby shocks have a compounding effect for the disadvantaged population. I conclude by estimating that the average traffic ticket is associated with at least a \$500 welfare loss and discussing this magnitude's implications for optimal policing.

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1 Introduction

The ability of households to cope with adverse shocks has important implications for taxation and social insurance policies (e.g., [Baily 1978](#), [Chetty 2006a](#)). Despite the prediction of canonical models that liquidity-constrained households anticipate income volatility by accumulating buffer stock savings ([Deaton 1991](#), [Carroll 1992](#), [Carroll 1997](#)), recent evidence has highlighted the lack of precautionary savings in the United States ([Beshears et al., 2018](#)). Half of all households accumulated no savings in 2010 ([Lusardi, 2011](#)) and forty percent of Americans indicated an inability to cover an emergency \$400 expense in 2017 ([Board of Governors of the Federal Reserve System, 2018](#)). The widespread dearth of rainy-day funds, termed financial fragility, has spurred concern among scholars and policymakers in recent years because fragile households may be particularly vulnerable to unexpected shocks ([Lusardi et al., 2011](#)).

While ethnographic studies such as [Shipler \(2005\)](#) and [Desmond \(2016\)](#) are rife with accounts of disadvantaged individuals whose fortunes are altered by unplanned expenses, causal evidence on the impacts of transitory, negative shocks on household finances is scarce. An important obstacle to such an empirical analysis is the lack of usable variation in small income shocks, especially for poor households. Existing studies have examined consumption responses to small positive shocks such as tax refunds (e.g., [Parker 2017](#)) or significant negative shocks such as hospital admissions ([Dobkin et al., 2018](#)) or job loss ([Stephens 2001](#), [Keys 2018](#)). The literature’s reliance on policy variation generated by tax rebates or mortgage programs and on credit card or bankruptcy filings data has left the bottom end of the income distribution relatively understudied.

In this paper, I examine the impacts of fines for traffic infractions on financial wellbeing. Over forty million traffic citations are issued each year for speed limit violations alone, making traffic fines a common unplanned expense for the driving population. Further, policing activity disproportionately affects poor communities, whose residents may have an especially limited capacity to absorb fines. As shown in [Figure 1](#), residents of the most disadvantaged zip codes receive traffic citations at nearly twice the rate of residents of rich zip codes.¹ While

¹The correlation between neighborhood income and ticketing rates is consistent with a wealth of evidence suggesting that low-income and nonwhite communities tend to be the most policed. For example, poorer cities employ more police officers per capita ([Figure A-1](#)) and rely more heavily on revenue from criminal justice fines and fees ([Figure A-2](#)).

most traffic fines are nominally small, typically between \$100 and \$400, they could induce financial distress in several ways. For individuals lacking financial slack, coping mechanisms such as forgoing basic needs, missing bills, or borrowing at high interest rates may impact future financial stability (e.g., [Skiba & Tobacman 2011](#)). Nonpayment of fines results in the revocation of driving privileges, which may jeopardize employment arrangements or put individuals at risk of a misdemeanor charge for driving without a valid license.

An analysis of the impacts of fines is particularly interesting given the current public concern regarding the unintended consequences of criminal justice policies (e.g., [Ang 2018](#)). While a large literature has examined the public safety benefits of policing ([Chalfin & McCrary, 2017](#)) in the spirit of deterrence models such as [Becker \(1968\)](#), the social costs of policing have historically received less attention. A host of recent events such as the 2014 riots in Ferguson, Missouri have vaulted the potential negative implications of policing to the forefront of public consciousness. Prompted by the *Ferguson Report*'s findings that a focus on revenue generation shaped the city's policing practices and that nonwhite and low-income citizens disproportionately received citations ([Department of Justice Civil Rights Division, 2015](#)), media outlets and advocates have offered accounts of individuals suffering cycles of debt and involvement with the criminal justice system stemming from fines and fees.² While compelling, such evidence is both anecdotal and correlational. To date, there has been no rigorous empirical analysis of the causal effects of fines on economic wellbeing.

To estimate the impacts of fines, I link administrative data on the universe of traffic citations issued in Florida over 2011–2015 to monthly credit reports and payroll records for ticketed drivers. The citations data provide nearly complete coverage of the state's traffic offenders and my analysis sample represents about five percent of Florida's driving-age population. Credit reports offer a detailed account of an individual's financial situation, including information on delinquencies, adverse financial events such as charge-offs and repossessions, and unpaid bills in collection. The payroll records report monthly earnings for individuals working at large employers. About sixteen percent of the analysis sample is employed in a payroll-covered job in the year prior to receiving a citation.

²For examples, see [Adams \(2015\)](#), [Lopez \(2016\)](#), [Grabar \(2017\)](#), or [Sanchez & Kambhampati \(2018\)](#). In 2015, John Oliver devoted a segment of his popular HBO show, *Last Week Tonight*, to municipal violations, providing several anecdotes and noting that "if you don't have enough money to pay a fine immediately, tickets can ruin your life." See <http://time.com/3754023/john-oliver-municipal-violations/>.

The high-frequency nature of the credit report and payroll data allows for the use of event study and difference-in-differences research designs that leverage variation in the timing of traffic stops for identification. My primary difference-in-differences approach compares the evolution of outcomes for drivers around the time of a traffic stop with a matched control group of comparable individuals who receive citations two to four years later. This empirical strategy relies on the identifying assumption that fined drivers would have trended similarly to control individuals in the absence of a traffic ticket, which I validate by showing that the two groups of drivers follow parallel pre-citation trends on a host of outcomes.

First, I examine the impact of traffic fines on several measures of financial distress. In the first year after a traffic stop, individuals experience a three percent increase in collections, a four percent increase in collections balances, and two percent increases in delinquencies and incidences of derogatory events. Collections activity related to an unpaid citation typically will not appear on a credit report, so the observed increases in collections most likely reflect increases in unpaid utility or medical bills (Avery et al., 2003). Estimated impacts persist, and in most cases continue to grow, two years out from the traffic stop date.

For the majority of strain outcomes, treatment effects are two to five times larger for the poorest quartile of drivers than for the richest quartile. While non-zero effect sizes for the richest subset of drivers may seem surprising, there is evidence of widespread hand-to-mouth behavior and binding liquidity constraints even among wealthy households (Chetty & Szeidl 2007, Kaplan et al. 2014). To help interpret the estimated magnitudes, I rely on the cross-sectional relationship between payroll earnings and financial strain outcomes to construct income-equivalent effect sizes — the change in income that would predict the observed change in distress. For low-income drivers, the two-year increase in financial strain is observationally similar to what would be predicted by a \$950, or five percent, drop in earnings.

Next, I study effects on payroll outcomes. Traffic citations could affect employment status through their impacts on financial distress, which may reduce labor supply (Dobbie & Song, 2015) or job-finding rates (Bartik & Nelson, 2017), or through their impacts on the costs of driving. Unpaid citations result in driver license suspensions, and many tickets result in driver license “points” which might increase auto insurance premiums. I find that one year (two years) out from a ticket date, individuals are about three (five) percent less likely to have any reported payroll earnings. Citations both reduce the likelihood of a transition into a payroll-covered job and increase the likelihood of a transition out of the payroll data.

As with the financial strain outcomes, employment effects are most pronounced for poor drivers. The estimated impact on payroll employment for the richest quartile of the sample is quite small, while the poorest quartile of drivers experience nearly a ten percent decline in the likelihood of positive reported earnings. For individuals remaining in the payroll data following a citation, there is no effect on earnings on average, but suggestive evidence of a two percent decline in earnings for low-income drivers.

I also examine the impact of traffic tickets on measures of borrowing and consumption. An unplanned expense may increase demand for credit, but financial distress or unemployment could restrict credit availability. I find small declines in the number of credit cards, credit card balances, and the likelihood of car and home ownership, proxied by the presence of an open auto loan and mortgage on a credit report, following a traffic stop. Reductions are more pronounced in the long-run than the short-run, suggesting that diminished access to credit following the accumulation of unpaid bills and delinquencies could be an important mechanism. The pattern of heterogeneity in the borrowing effects is less stark, likely because the poorest quartile of drivers exhibit tenuous borrowing at baseline.

After presenting the main results, I consider the relative importance of competing mechanisms in explaining the estimated effects. In particular, traffic tickets represent unplanned expense shocks but also can affect insurance costs or driving privileges. Using information on traffic ticket dispositions available for a subset of drivers, I show that treatment effects for those whose dispositions indicate payment, and therefore typically will not incur a suspended license, are similar to the sample-wide average effects. Impacts are smaller for individuals making payment and electing to attend an optional traffic school that suppresses points from accruing on the driver's license. On the one hand, the reduced treatment effects for school attendees suggest that the negative consequences of traffic tickets are in part due to license suspensions or increased insurance costs (individuals making payment can still face suspensions if payment is late or if they have accrued many past citations). On the other hand, impacts are still present for school attendees and the treatment effect disparities are largely eroded when accounting for observable differences between the two groups of drivers. Further, a separate analysis reveals that the causal effects of license suspensions are large, but not outsized compared to the main citation effects. On net, it appears that both the pure expense shock and potential effects on driving costs are important mechanisms.

I conclude by quantifying the welfare losses associated with traffic tickets and discussing

policy implications. Using back-of-the-envelope calculations and a standard willingness-to-pay framework, a conservative estimate of the welfare cost associated with the average ticket is about \$500. Intuitively, this quantity has a policy-relevant interpretation. To the extent that welfare costs are greater than the revenue raised and public safety produced by an additional traffic citation, there is deadweight loss associated with ticketing. Governments who do not consider the outsized welfare costs of citations will generally choose to over-police. I then use a simple Becker-style model to consider the welfare implications of moving to an income-based fine system.³ In a stylized environment where individuals earn either \$20,000 or \$40,000 per year and the multiplying welfare effects of fines for poor individuals are taken into account, a \$10 increase (decrease) in the fine for rich (poor) drivers yields a welfare benefit of between \$3 and \$10 dollars per citation. At current ticketing levels, this policy offers a total social benefit as high as \$20 million per year, eroding about one percent of the total welfare cost of annual citations in Florida ($\$500 \times 2$ million tickets).

My paper makes two important contributions. First, the empirical results highlight that many individuals are not fully insured against even small economic shocks. Faced with a \$175 traffic ticket, individuals accrue unpaid bills and delinquencies on their credit reports while also reducing consumption, suggesting an inability to cover the unexpected expense. While the increases in unpaid bills and declines in consumption are smaller than the fine itself for rich drivers, traffic tickets appear to have a multiplying effect on financial health for poor drivers, who exhibit increases in financial distress observationally similar to a \$950 income loss following a \$175 ticket. Results are even starker for individuals with unpaid bills at baseline, who experience the largest increases in distress and largest declines in employment and borrowing. This pattern of results is consistent with a poverty trap (e.g., [Banerjee & Duflo 2011](#), [Barrett et al. 2019](#)), whereby small shocks have minor consequences for financially stable individuals but deleterious effects for the already distressed population.

These findings have potentially important implications for social insurance programs as optimal policy formulas typically depend heavily on the ability of households to smooth across states of the world. Further, the empirical analysis contributes to a large literature studying how households are affected by economic shocks by providing some of the first

³Finland employs an income-based fine schedule for speeding. Countries such as Sweden and Denmark also use income-dependent fines in some form. See <https://www.theatlantic.com/business/archive/2015/03/finland-home-of-the-103000-speeding-ticket/387484/>.

causal evidence on the effects of small, negative shocks for low-income individuals.⁴

Second, this paper adds to the current public debate over the use of fines and fees in the criminal justice system. While scholarly work has found that increases in speeding tickets improve road safety (Makowsky & Stratmann 2011, DeAngelo & Hansen 2014, Luca 2015), critics have argued that the ability of police departments to raise municipal revenue through citations distorts policing incentives (Goldstein et al., 2018). Advocates and media outlets (e.g., Adams 2015, Lopez 2016, Grabar 2017) have argued that flat fine schedules and more intensive policing in low-income communities result in an unfair burden of fine systems on the poor. Others have called the harsh punishments imposed for nonpayment of fines an effective “criminalization of poverty” (Balko, 2018). My findings illustrate the outsized impacts of fines on the financial well-being of low-income individuals, a fact that has potentially important implications for both the optimal level of policing and the design of fine-and-fee systems.

The remainder of the paper is organized as follows. Section 2 explains the institutional details of traffic enforcement in Florida. I describe the data in Section 3 and the empirical strategy in Section 4. Results are presented in Section 5. I briefly discuss welfare and policy implications in Section 6 and conclude in Section 7.

2 Traffic Enforcement in Florida

The context of the present study is traffic enforcement in Florida. The vast majority of traffic laws, such as speed limits, are enforced with fines for violators. Patrolling police officers, or in some cases automated systems such as red light or toll cameras, issue citations to offenders. Traffic tickets are very common. Over 4.5 million individual Florida drivers received at least one traffic citation between 2011 and 2015, with between 1.1 and 1.4 million licensed Florida drivers cited each year. As of the 2010 census, the age 18 and over population of Florida was 14.8 million, implying that around thirty percent of the driving age population received a citation over 2011–2015 and about seven to ten percent of the driving age population receives a citation each year. As has been shown in other contexts, traffic enforcement appears to disproportionately affect low-income individuals. Figure 1 illustrates a clear correlation between the zip code ticketing rate (number of citations issued to zip code residents divided by the zip code population) and zip code per capita income, computed from the IRS public

⁴Beshears et al. (2018) provides a thorough and recent review of the literature.

use files.⁵ A ten percent decline in neighborhood per capita income is associated with a four percent increase in the citation rate.

Traffic citations specify the offense and a fine to be paid, which is determined by the violation code and the county of the offense. For reference, the most common single violation codes over 2011–2015 were speeding (20 percent), red light camera violations (8.5 percent), lacking proper insurance (7.5 percent), driver not seat-belted (6 percent), and failure to pay toll (6 percent), which account for nearly half of all citations over the period. Statutory fines vary widely across offense types and counties. For example, in Miami-Dade county, low-level equipment violations such as broken tail lights carry a fine of \$109, while the fine for speeding 30+ miles per hour above the posted limit in a construction or school zone is \$619. Punishments for very rare criminal, rather than civil, traffic offenses can exceed \$1,000 and in some instances may include jail time. Unfortunately, the citations database does not include a reliable measure of the statutory fine associated with each offense. Using an imputation procedure, I estimate that the average statutory fine faced by drivers in the main sample is about \$175, but this is likely an underestimate.

Citations can be associated with additional costs beyond the statutory fine. Traffic violations result in points on a driver’s license. Insurance companies typically consider driver-license points when setting premiums, so individuals may face increases in car insurance prices following a citation ([Gorzelany, 2012](#)). A rough back of the envelope calculation suggests the typical speeding ticket could increase monthly car insurance premiums by \$10. State law dictates that drivers accruing 12 points in 12 months (18 points in 18 months; 24 points in 36 months) have their driver license suspended for 30 days (3 months; one year). Most common offenses are associated with three points, but certain violations carry up to 6 points.⁶ Individuals cited for equipment violations such as broken taillights are ordered to make repairs or face the risk of quickly becoming repeat offenders.

Once a citation has been issued, a driver can either submit payment to the county clerk or request a court date to contest a ticket. For those contesting their ticket in court, a judge or hearing officer ultimately will decide to either uphold the original citation, reduce the punishment, or dismiss the charge. For individuals who do not request a court date, payment

⁵The IRS public use data are available from the IRS website at <https://www.irs.gov/statistics/soi-tax-stats-individual-income-tax-statistics-zip-code-data-soi>.

⁶See the FLDHSMV website at <https://www.flhsmv.gov/driver-licenses-id-cards/driver-license-suspensions-revocations/points-point-suspensions/>.

is due 30 days from the citation date. At the time of payment, a driver may also elect to attend traffic school. A voluntary traffic school election (and completion) coupled with an on-time fine payment prevents the license points associated with the citation from accruing on the individual's DL.⁷ If the county clerk has not received payment in-full within 30 days, the individual is considered delinquent and their license is suspended effective immediately. Knowingly driving with a suspended license is a low-level misdemeanor offense and typically results in a fine of \$300-500 with the possibility of jail time and punishments increasing drastically for second and third offenses.

If a citation remains unpaid after 90 days, county clerks add a late fee to the original amount owed and send the debt to a collections agency, who then solicit payment for the citation. Collections agencies are authorized by state law to, and therefore typically will, add a 40 percent collection fee to the original debt.⁸ Relevant for the empirical analysis is whether collections originating from unpaid citations will appear directly in the credit bureau data. Not all collections agencies report their activity to credit bureaus and reporting behavior varies across both agencies and clients. I compiled a list of collections agencies used by the five largest counties in Florida by examining county clerk webpages and contacted each one directly to inquire about their reporting behavior.⁹ While most signaled an ability to report to credit bureaus on their webpage, the two agencies that responded directly to my inquiry indicated that they did not report citation-related collections.

An important takeaway from a close examination of the institutional details is that a traffic ticket represents a possibly multi-faceted treatment. The exact treatment for a given individual may depend on driving history and ex post decisions, neither of which are perfectly observed in the data. We should primarily think of the treatment as receiving a bill for, on average, \$175, where the punishment for nonpayment is a revocation of driving privileges. However, the treatment could entail time in court for contesters and increases in

⁷Individuals seeking to prevent point accrual following standard non-criminal moving violations take the Basic Driver Improvement Course. The course is four hours of instruction, cannot be completed in one sitting, and typically costs about \$25.00. Many providers allow the course to be taken online. Individuals can only complete traffic school once in any twelve-month period and five times total.

⁸See [Adams \(2015\)](#) and corroborating evidence on the Miami-Dade County Clerk of Courts website at http://www.miami-dadeclerk.com/parking_collections.asp.

⁹Most counties use some combination of (1) Linebarger, Goggan, Blair and Sampson, LLP, (2) Penn Credit, and (3) AllianceOne, with some also using Law Enforcement Systems, Inc. and Municipal Services Bureau (MSB).

insurance premiums as well. I focus on estimating reduced form, or intent-to-treat, effects of traffic tickets, but rely on analysis of heterogeneous treatment effects and an independent examination of license suspensions to provide some insights about which components of treatment are particularly relevant.

3 Data

3.1 Traffic Citations

The Florida Clerks and Comptrollers Office (FCC) provided administrative records of all traffic citations issued in Florida from 2005 through 2015 in response to a sunshine law (FOIA) request. The records were culled from the Clerk’s Uniform Traffic Citation (UTC) database, which preserve an electronic record of each ticket transcribed from the paper citation written by the ticketing officer. Each record includes the data and county of the citation, as well as the violation code and information listed on the offender’s driver license, such as DL number, name, date of birth and address. My analysis makes use of subsets of citations issued in 2011–2015 due to the availability of credit report data, discussed below.

3.2 Credit Reports

Access to monthly credit reports from January 2010 through December 2017 was provided by a major credit bureau.¹⁰ I provided the credit bureau with a list of 4.5 million Florida residents issued a traffic citation between January 2011 and December 2015. Via a proprietary linking algorithm, the driver information was matched with the credit file using name, date of birth, and home address reported on the citation.¹¹ The linking process matched 3.7 million drivers for an 82 percent match rate. [Brevoort et al. \(2015\)](#) find that about eleven percent of adults, and as many as 30 percent in the lowest income areas, have no credit record. Additionally, in most cases, names and addresses were written by hand, undoubtedly leading to some mistakes in transcription. Hence, 82 percent is a reasonable match rate.

Consistent with [Brevoort et al. \(2015\)](#), the credit file match rate is higher for residents of the richest zip codes (~86 percent) than for the poorest zip codes (~78 percent), as shown

¹⁰My data sharing agreement precludes me from sharing the name of the Credit Bureau.

¹¹Note that the credit bureau preserves a list of previous addresses for individuals on file. Hence, the address at the time of the traffic ticket need not be current to achieve a match.

in Figure A-3. Table A-1 examines a more complete set of predictors of a credit file match. The regressions confirm a strong relationship between neighborhood income and a successful match, but also highlight differences across demographics groups. Female, white, and older drivers are more likely to be matched. We should think of the matching process as slightly eroding the negative selection into the citations data. Individuals receiving traffic tickets are more disadvantaged than average as shown in Figure 1, but among cited individuals, there appears to be positive selection in terms of being matched to the credit file. To the extent that the treatment effect is larger for the most disadvantaged individuals, the selection induced by the credit file matching process ought to bias estimates toward zero.

After matching the data, the credit bureau removed the citations data of all personally identifiable information such as driver names, addresses, birth dates, driver license numbers, and exact citation dates, preserving only the year and month of each citation. I was then allowed access, through a secure server, to the anonymized citations data and monthly credit reports each with a scrambled individual identifier for linking across the two datasets.

The credit bureau data represent a snapshot of an individual’s credit report taken on the last Tuesday of each month. The data include information reported by financial institutions, such as credit accounts and account balances, information reported by collections agencies, information culled from public records such as bankruptcy filings, and information computed directly by the credit bureau such as credit scores.¹² The data also include an estimated income measure, which is based on a proprietary model that predicts an individual’s income, rounded to the nearest thousand, using information in the credit file. Estimated income is highly, but not perfectly, correlated with payroll earnings (described below), as shown in Figure A-4. While I do not use estimated income as a primary outcome because I cannot replicate its computation, I make use of the measure both in constructing the matched control group (discussed below in Section 4) and in splitting the sample to examine heterogeneous effects by income.

Credit bureau data provide a wealth of information on an individual’s financial situation. The challenge in working with such data is to focus on a parsimonious set of outcomes with a relatively clear welfare interpretation. I focus my analysis on two types of outcomes –

¹²The provided credit score is the VantageScore[®] 3.0. For more information, see <https://www.vantagescore.com>. The innovation of the VantageScore 3.0, which is also an advantage for my analysis, is an improved ability to score individuals with thin credit files. The Credit Bureaus estimate that about 30M previously unscorable consumers can be assigned a VantageScore 3.0.

measures of financial strain and measures of credit usage. Following [Dobbie et al. \(2017\)](#), I use collections, delinquencies, and incidences of major derogatory events as measures of financial strain. Collections represent unpaid bills that have been sent out to third-party collections agencies. I use the number of accounts ever at least 90 days past due as my primary measure of delinquency, but also consider total balance currently past due, summed across all accounts. Major derogatory events are incidences of repossessions, charge-offs (where a creditor declares a debt unlikely to be paid), foreclosures, bankruptcies, or internal collections. The credit bureau computes the number of accounts on file with any major derogatory event to date, and I use this as an additional outcome measuring financial strain.

Collections and collections balances are an especially useful measure of stress in my context because unpaid bills need not be related to borrowing accounts. According to [Avery et al. \(2003\)](#) and [Federal Reserve Bank of New York \(2018\)](#), only a small fraction of collections are related to credit accounts, with the majority associated with medical and utility bills. Credit usage is sporadic among a sizable subset of cited drivers. Almost 20 percent of individuals in the primary sample have no open account at baseline. Collections can capture increases in financial strain even among individuals with tenuous credit usage, while individuals need to maintain open borrowing accounts in order to exhibit delinquency, for example, in the credit file.

My primary measures of credit usage are the number of open revolving accounts and revolving account balances. Revolving accounts are accounts that provide a borrowing limit and no set maturity date. The majority of revolving accounts are credit cards and store cards. I also examine whether individuals have any open auto loan or mortgage to study durable good consumption.

All fields in the credit report data are pre-topcoded. Fields measuring counts, for example the number of collections or number of revolving accounts, are topcoded at 92. Balances are topcoded at \$9,999,992. Credit report information can be missing either because an individual lacks a credit report in a given month or for reasons such as insufficient information to compute a field. For example, the field for number of open accounts may be missing because the credit bureau cannot ascertain whether certain accounts qualify as open. Balances corresponding to non-existent accounts, e.g. collections balances for a person-month with zero collections, are typically coded as missing. Both for simplicity and to be conservative, I impute missing fields as zero in the main specification. After imputing where account numbers

are zero, balances are frequently zero, and I therefore winsorize balance measures at the 99th percentile rather than taking logs.¹³

3.3 Payroll Data

The provider of the credit report data also maintains a database of payroll records that are shared directly with the credit bureau. The payroll data are relatively thin, but include information on the number of payroll accounts, i.e. number of jobs, and annualized earnings for individuals in a given month. In terms of coverage, employers reporting payroll information are mostly larger businesses, with about 85 percent of Fortune 500 companies covered in the payroll data. Coverage appears more sparse in the citations sample than for the nation as a whole. According to the credit bureau, around 30 percent of the individuals in the credit file are covered in the payroll data. In my main analysis sample of over 600,000 individuals, 16 percent are employed and 11 percent have nonmissing earnings at baseline in January 2010.

The primary outcome from the payroll data used in my analysis is employment, measured either as having an active account or having positive earnings in a given month. While non-presence in the payroll data does not indicate unemployment, transitions out of the payroll data indicate transitions into unemployment or to a new job. Further, there is reason to think that those covered in the payroll data represent relatively good and high-paying jobs. Existing research by [Cardiff-Hicks et al. \(2015\)](#) and [Brown & Medoff \(1989\)](#) has noted that large employers tend to pay higher wages and provide more generous benefits. For individuals covered in the payroll data at baseline, median earnings were over \$35,000. Median earnings in Florida in the 2010 American Community Survey were about \$27,000. Given the relatively young age distribution in the cited driver sample, and the fact that payroll earnings is a lower bound on total earnings, the evidence suggests that jobs covered in the payroll data are higher-paying than average.¹⁴

¹³For reference, the 99th percentile of collections balances is about \$35,000 while the maximum is about \$750,000. For revolving balances, the 99th percentile and maximum are about \$225,000 and \$9,500,000. In the appendix, I present results retaining missing values, with point estimates nearly identical to those shown in the main text.

¹⁴Appendix C presents further validation of the payroll employment measure. Specifically, I estimate the effect of separations from payroll-covered jobs occurring several months before a traffic stop on credit report outcomes using an event-study approach. I find that unpaid bills increase by about 5 percent and credit card balances decrease by about 5 percent in the year following a separation, suggesting a deterioration in financial health. See Appendix C for more detail.

4 Empirical Strategy

4.1 Event Study

The goal of the empirical analysis is to estimate the reduced form impacts of traffic tickets. Given that only cited drivers are matched to the credit file, the natural source of variation provided by the data is the timing of citations among ticketed drivers, which lends itself to an event study approach. Specifically, I estimate regressions of the form:

$$Y_{it} = \sum_{\tau} \alpha_{\tau} + f(\text{age}_{it}) + \phi_i + \kappa_t + \gamma_i(t) + \epsilon_{it} \quad (1)$$

where ϕ_i and κ_t are individual and time, i.e. year \times month, fixed effects. Here, τ indexes event time, or months since citation, and the coefficients on the event time indicators α_{τ} are the object of interest. Identification of the event-time effects relies on variation in the timing of traffic stops – deviations in y are compared for individuals at the same calendar time but different event time. To flexibly control for lifecycle dynamics in the credit bureau outcomes, I include a quartic in age. A causal interpretation of the post-event coefficients rests on the assumption that, among cited drivers, the precise timing of a traffic stop is as good as random.

Coefficients for $\tau < 0$ are typically viewed as a test of the identifying assumption. Pre-event trends may suggest that changes in y predict the timing of the event. Several of the outcomes under study exhibit a slight pre-trend but a trend break around the time of traffic stop, so I also include person-specific linear time trends, $\gamma_i(t)$ in my main estimates of equation (1).¹⁵ When linear trends are included, the α 's are identified off deviations from trend and the identifying assumption is that the traffic stop's timing is random conditional on a secular pre-event trend.

Estimates of equation (1) using all available data are computationally infeasible because I cannot invert a matrix larger than 60 million rows with the computing tools available for analyzing the credit report data. Therefore, I rely on a 25 percent random sample of drivers in the event study analysis. To construct the sample, I first identify the set of drivers who are present in the credit report data in January of the year prior to their first observed citation

¹⁵I show estimates without individual trends in the appendix. Results are qualitatively similar in all cases, with some outcomes displaying more of a trend-break than a simple increase or decrease around the time of a traffic stop.

in 2011–2015, then select individuals ages 18–64 as of that month. There are 2.8 million such drivers, and I draw a 25 percent random sample resulting in 710,486 individuals. I include each individual in the data for four years beginning in the aforementioned January, which reduces the dimensionality of the dataset but retains at least 12 months of pre-citation and 24 months of post-citation data for each driver and allows the generations (drivers with events in different years) to overlap, which aids in the separate identification of the time and event time effects.

Column 1 of Table 1 shows summary statistics for the event study sample, reported as of the base period. Cited drivers are, on average, 44 percent female, 38 years old, and 60 percent nonwhite, where Hispanics are considered nonwhite. While average estimated income is very close to the statewide average of \$32,000, the average credit score is 609, which is just above subprime and about 50 points lower than the statewide average of 662. The typical driver has 2.8 accounts and a \$2,169 balance in collections at baseline. About two percent of drivers have filed for bankruptcy in the past two years as of the base period.

Prior to a traffic stop, 80 percent of drivers maintain at least one open account, revealing that borrowing is somewhat tenuous among the sample of cited drivers. The typical driver maintains 2.82 open revolving accounts and a \$6,500 revolving balance. Of drivers in the event study sample, 34 percent have an auto loan and 25 percent have a mortgage at baseline. In terms of payroll data measures, 16 percent of drivers in the event study sample are indicated as having a job, while 11 percent have positive reported earnings. Among those with earnings, average monthly earnings were \$3,399, which corresponds to an annual salary of \$41,000.

4.2 Matched Difference-in-Differences

I supplement the event study approach with a difference-in-differences analysis. While the data do not provide an organic control group, I use a coarsened exact matching procedure [Iacus et al. \(2012\)](#) to construct one. The control group aids in the estimations of counterfactual trends and allows for a fully nonparametric differencing out of age or lifecycle effects.

Citations data linked to credit bureau data span from 2011 through 2015. I use drivers receiving their first citation in 2011 as the treatment group and drivers receiving their first citation in 2014–2015 as the control group. The period covering January 2012 through December 2013 is preserved as a follow-up period where the treatment drivers have all received

treatment (at least one traffic ticket) and control drivers have not. The delineation of treatment and control groups was meant to balance the desire to maintain a longer follow-up period with the need to retain sufficient mass in the control group. Matching occurs as of January 2010, the first month of credit report data. Credit report data from January 2010 through December 2013 is then used in the analysis, guaranteeing that 12 months of data are available before and 24 months of data are available following the treatment group citation. Figure 2 offers a graphical depiction of the timeline.

4.2.1 Matching Procedure

To be eligible for inclusion, individuals must be present in the credit file as of January 2010. I also require that individuals be between 18 and 64 years of age in January 2010. There are 818,000 eligible treatment drivers and 613,000 eligible control drivers, about 40% of the universe of drivers ever matched to the credit bureau data. I use a parsimonious set of characteristics for the match and intentionally avoid matching on outcome variables. Treatment and control drivers were matched using age bins (18-24, 25-29, 30-34, 35-39, 40-44, 45-49, and 50+), gender, race (measured as white or nonwhite where Hispanic is considered nonwhite), county of residence, and quintiles of credit score and estimated income. Gender, race, and county of residence are measured using the citations data and hence are measured at the time of citation, while age, credit score, and estimated income are taken from the credit bureau data and are measured in January 2010. Because credit score and estimated income are highly correlated with age, the quintiles are computed within age band.

I also use pre-citation growth rates in credit score and estimated income as matching variables. Specifically, I compute the January 2010–December 2010 change in credit score and estimated income for each driver, and match on within-age-bin quintiles of these growth rates. Note that neither estimated income nor credit score are primary outcomes in my analysis – matching on the first year growth rates in these variables does not ensure parallel pre-trends in focal outcomes across groups. Ultimately, it does aid slightly with ensuring pre-trend similarity, which is why I opt for including the growth rates in the list of matching variables. However, including the first-year growth rates in the set of matching variables is not at all necessary for obtaining the main results.¹⁶

¹⁶In Figure A-11, I plot outcome means for treatment and control drivers using all candidates and no matching, instead allocating placebo citation dates to control drivers randomly. The vast

Once all possible matching pairs have been identified, I ensure that control drivers are not associated with multiple treatment drivers and that each treatment driver is matched to one and only one control driver using random draws. Control drivers are then assigned the same traffic stop date as their matched treatment driver, allowing for a comparison of changes in outcomes around the exact time of a traffic stop for an individual receiving a citation at that date with her control driver, who is observably similar but does not receive a citation at that time. Note that, by construction, treated and control drivers are (approximately) the same age at the time of treatment. Hence, one can think of the identification strategy as leveraging variation in the age at first citation, with treatment drivers first ticketed when a few years younger than control drivers.

4.2.2 Characteristics of Matched Sample

Columns 2 and 3 of Table 1 present summary statistics for the matched sample as of January 2010.¹⁷ On average, individuals in the matched sample are observably quite similar to those in the event study sample. By construction, treatment and control drivers are similar in terms of demographics, credit score and estimated income. But as shown in Panels B-D, individuals are quite similar on most unmatched dimensions as well. Treatment and control drivers have similar numbers of collections and collections balances and nearly identical derogatory and delinquency rates. Treated and control drivers also maintain similar numbers of revolving accounts, own cars and homes at similar rates, and match very closely in terms of payroll data outcomes.

4.2.3 Estimation

The first step in the analysis of the matched sample is to plot average outcomes around the traffic stop date for treatment and control drivers. Recall that control drivers are assigned their matched treatment driver's citation date as a placebo date, which allows for the computation of event time (i.e. months since actual or hypothetical citation), for both group of

majority of main results remain in this no-matching approaching.

¹⁷Table A-2 compares means for matching candidates, all individuals meeting the sample inclusion criteria described above, and the individuals successfully matched. The primary takeaway from a comparison of means for candidates and matches is that control candidates are slightly less disadvantaged than treatment candidates. Accordingly, the matching procedure seems to drop the worst-off individuals from the set of treatment candidates and the best-off individuals from the set of control candidates.

drivers. The natural regression analogue to comparing changes over time in the raw data is

$$Y_{it} = \sum_{\tau=-12}^{24} [\theta_{\tau} \times Treat_i \times \alpha_{\tau} + \alpha_{\tau}] + \phi_i + \epsilon_{it} \quad (2)$$

where α_{τ} is a month relative to citation indicator and ϕ_i is an individual fixed effect. The θ_{τ} 's are the coefficients of interest, measuring treatment-control differences at each month relative to the citation.

For the estimation, I sample data between 12 months prior and 24 months following the treatment date. I further subset the data to include only every third month, centered at the month of the citation date, which greatly improves estimation speed. Finally, a key component of the empirical analysis will consider heterogeneous treatment effects across subsamples. For example, I compare the impact of citations for low versus high income individuals. While I confirm both in the raw data and with estimates of versions of equation (2) that treatment and control drivers trend similarly prior to the traffic stop on average, parallel trends may not be perfectly satisfied in every subsample. To ensure that differences in estimated effects across subsamples are not driven by variation in pre-treatment trends, my primary specification using the matched sample is a trend-adjusted version of equation (2):

$$Y_{it} = \sum_{\tau=0}^{24} [\theta_{\tau} \times Treat_i \times \alpha_{\tau} + \alpha_{\tau}] + \phi_i + \kappa_t + Treat_i \times \tau + \epsilon_{it} \quad (3)$$

Equation (3) is identical to equation (2) except that event-time and event-time-treatment interactions for $\tau < 0$ are dropped, while a treatment indicator interacted with a linear trend, $Treat_i \times \tau$ is added. I also add year and month fixed effects, represented by κ_t , to capture secular seasonality and time effects. The θ_{τ} coefficients are treatment-control differences in each post-ticket month after adjusting for differences in pre-treatment trends across the two groups. When presenting the main results, I report the θ 's for 12 and 24 months post-citation. I cluster standard errors at the matched pair-level.

4.2.4 Identification

Identification in the matched difference-in-differences analysis comes from comparing the changes around the traffic stop date for treatment drivers, who indeed receive a citation at that date, and control drivers, who receive citations a few years later. The identifying

assumption is that treatment drivers would have trended similarly to control drivers in the absence of a traffic stop. As with most applications of difference-in-differences, there are two primary threats to this assumption – different pre-treatment trends across treatment and control groups and unobserved shocks correlated with both treatment status and treatment timing. I verify that the two groups follow similar pre-treatment trends by examining the raw data and estimating non-parametric event study-style specifications in the spirit of equation (2) above. Further, to be conservative, I trend-adjust the regression estimates so that coefficients are identified off deviations from pre-treatment trends as in equation (3).

By construction, treated and control drivers are approximately the same age at the time of treatment. Hence, one can think of the identification strategy as leveraging differences in the age at first (observed) citation, with treatment drivers first ticketed when a few years younger than control drivers. Alternatively, one could think of the matching step as identifying candidates for a traffic stop at a specific time and the analysis as comparing candidates with stops that do and do not occur. In this framework, the empirical analysis parallels studies that compare, for example, accepted and denied applicants around the time of an application (e.g., [Cellini et al. 2010](#), [Mello 2018](#)). Lastly, the empirical design is similar to studies using individuals who receive treatment but outside the relevant time range as a control group, such as [Currie et al. \(2018\)](#).

4.3 Estimating Impacts of License Suspensions

A potentially important mechanism through which traffic tickets may impact individuals is through their impacts on driving privileges. Unpaid citations result in suspended driver licenses, and a lack of a valid driver’s license may jeopardize an individual’s employment arrangements. Additionally, the effects of license suspensions are of general interest, because state and local governments use DL suspensions as punishment for an array of infractions. For example, many states revoke driver licenses for individuals convicted of drug offenses.

While I cannot cleanly identify nonpayment of fines in the citations data, I estimate the effect of suspensions levied for accruing too many driver license points. The majority of citations carry three points and twelve points in twelve months results in a 30 day license suspension. Hence, I estimate the impacts of license suspensions using an event study approach around the time of a fourth citation in one year. I also sample individuals receiving

three, but not four, tickets in a one year period as a quasi-control group. The estimating equation is

$$Y_{it} = \sum_{\tau} \theta_{\tau} \times Treat_i \times \alpha_{\tau} + \sum_w \beta_w + \phi_i + \kappa_t + \epsilon_{it} \quad (4)$$

where τ indexes time around a license suspension and w indexes time around an initial ticket. The β_w 's are event time indicators corresponding to the initial citation date and the α_{τ} 's are event time indicators corresponding to the 4th citation date, all of which are set to zero for control drivers.

The final two columns of Table 1 presents summary statistics for the suspensions sample. There are 79,490 individuals who receive four tickets in the one year following their initial citation and 135,701 individuals who receive three but not four tickets over the same period. Treated and control drivers are comparable to each other in terms of demographics but are distinctly more likely to be male, more likely to be nonwhite, and are slightly younger on average than drivers in the event study and matched samples. In terms of credit bureau outcomes, the serial offenders used in the suspensions analysis are clearly more disadvantaged than the average cited driver.

5 Results

5.1 Financial Strain

Figure 3 plots event study estimates corresponding to equation (1) for the financial strain outcomes. In each case, I show the point estimates and 95 percent confidence bands for full sample (blue circles) and using only the poorest quartile of drivers in terms of baseline estimated income (red squares). The figures illustrate a consistent pattern, with all four strain outcomes increasing following a citation. For collections, collections balances, and delinquencies, the increase is more pronounced among poor drivers. The response is both gradual and slightly lagged, which makes sense given that an unpaid bill, for example, will take time to be sent to a collections agency and then appear on a credit report. [Dobkin et al. \(2018\)](#), who study collections around the time of a hospital admission using an event-study approach, find a quite similar time pattern.

The first four panels of Figure 6 plot the corresponding raw data for treated and control drivers in the matched sample. In the case of all four strain outcomes, treated drivers follow

nearly identical trends to control drivers prior to the traffic stop date, suggesting a successful matching procedure. However, trends diverge around the time of treatment, with treated drivers exhibiting relative increases in collections, collections balances, derogatories, and delinquencies following a traffic stop.

Table 2 plots the corresponding regression estimates. Each row corresponds to an outcome and column 1 reports the baseline mean. Columns 2-3 report the 12 and 24 month estimates from the event study approach, while columns 4-5 report the 12 and 24 month estimates from the matched difference-in-differences approach. Event study estimates imply that one year (two years) out from a traffic stop, individuals have about 0.09 (0.14) more reported collections, 0.04 (0.05) more derogatory accounts, and 0.01 (0.02) more delinquencies. Relative to the baseline means, the one (two) year effects are about three (five) percent for collections, two (three) percent for derogatories, and two (three) percent for delinquencies. In the fourth row, the outcome is an index that combines collections, derogatories, and delinquencies, with the point estimate implying that traffic stops increase strain by about 2-3 percent of a standard deviation.¹⁸ Balances past due and balances in collections also increase by about 2-5 percent. Estimates from the difference-in-differences approach are very similar in most cases.

Table 3 reports estimates separately for the poorest and richest quartile of drivers.¹⁹ As was apparent in Figure 3, the impact of traffic stops on collections is significantly larger for poor than for rich drivers, with the disparity present across both research designs. Estimated impacts on collections balances, for example, are 3-4 times larger for the poorest quartile of drivers (\$142) than for the richest (\$38). The two year impact on collections balances for poor drivers is over \$200, larger than the size of the typical fine, in both specifications.

When considering heterogeneous effects on the account-based measures of financial strain, we should keep in mind that richer drivers tend to have more accounts and higher balances

¹⁸The index is computed by standardizing each component, summing, and then standardizing again. For the event study sample, I standardize relative to the base period. In the matched difference-in-differences approach, I standardize relative to the control group in the base period.

¹⁹The quartiles are determined using baseline estimated income in the matched sample. I use the same thresholds when splitting the event study and license suspensions samples. Worth noting is the fact that the rich quartile of drivers are not particularly well-off due to the apparent negative selection into receiving a traffic ticket. Nearly 20% of the richest quartile of drivers has a subprime credit score at baseline. Median estimated income among the richest quartile, about \$53,000, is below the 75th percentile of personal income in Florida.

(see Table 4), and therefore may have more space for growth in outcomes such as delinquencies and adverse events. Still, I find larger impacts of traffic tickets on delinquencies for poor than rich drivers. Event study estimates suggest similar effect sizes on derogatory events for the richest and poorest quartiles, while the difference-in-difference estimates imply a larger impact for poor drivers.

5.2 Payroll Employment

Figure 4 plots coefficients from event study estimates where the dependent variable is an indicator for having positive payroll earnings in a given month. Recall that traffic tickets may impact employment arrangements either through their impacts on financial distress, which may reduce labor supply or job-finding rates, or through their impacts on driving costs. Results for the full sample (blue circles) show a flat pre-event trend and a drop in the likelihood of employment beginning in the first 2-3 months following a traffic stop and persisting a full two years later. Poorer drivers appear to be trending slightly upward prior to a traffic stop and experience a more dramatic drop following the date of a citation. The final panel of Figure 6 plots the corresponding raw data from the matched sample, which reveals a clear disparity between treatment and control drivers emerging only after the treatment group's traffic stop date.

Coefficients are reported in the first two rows of Table 4. For the full sample, regression estimates imply a half a percentage point decline in the likelihood of positive earnings in the payroll data, about a four percent decline relative to a baseline mean of twelve percent. Difference-in-differences estimates are nearly identical. Table 5 compares effects for the richest and poorest quartile of drivers and reveals that the impacts on employment are significantly more pronounced among poor individuals. For the poorest quartile of drivers, the one-year impact on employment is nearly a full percentage point (8 percent), while the effect for rich drivers is about 0.3 percentage points (2.5 percent). The effect size disparities between rich and poor drivers are even larger when considering the difference-in-differences specification. Difference-in-differences in estimates of the employment (positive earnings) effects for the richest quartile of drivers are not statistically different from zero.

Figure A-9 demonstrates that employment effects are driven both by an increase in the likelihood that a currently employed individual transitions out of the payroll data and a

decrease in the probability that an individual transitions into the payroll data. Specifically, I split the matched sample into individuals with and without payroll earnings as of 12 months prior to the citation date and plot employment probability over time. The figure shows that, relative to the control group, treated drivers in the payroll data at baseline become more likely to transition out following a traffic stop. In the same vein, treated drivers not in the payroll data at baseline become relatively less likely to transition into the payroll data post-treatment.

Table A-4, which presents difference in difference estimates for payroll earnings, suggests that traffic tickets have little impact on earnings for the average driver who remains in a covered job. Figure A-7 plots event study coefficients where log monthly earnings is the dependent variable. Consistent with the difference-in-difference estimates, there appears to be little impact on earnings in the full sample. The event study estimates suggest a 1-2 percent decline in earnings for the poorest quartile of drivers, however. Neither the difference-in-differences nor the event study estimates are precisely estimated.

5.3 Borrowing and Credit Usage

Event study estimates for the borrowing outcomes are plotted in Figure 5, while the raw means for the matched sample are shown in Panels E-H of Figure 6. While we would expect a surprise expense such as a traffic ticket to, if anything, increase financial strain, the predicted impact of such a shock on borrowing is, *ex ante*, ambiguous. On one hand, an unplanned expense may increase demand for credit. However, the impacts on financial duress discussed above may reduce access to credit through their impacts on credit scores or borrowing limits. While I estimate relatively small impacts of traffic stops on credit scores (about minus two points as shown in Table A-3), other studies have found that collections may result in reduced credit limits. Unfortunately, I do not observe borrowing limits in the credit report data. [Dobkin et al. \(2018\)](#) estimate that hospital admissions increased collections balances by \$122 and, correspondingly, that credit limits fell by \$500, despite also finding a small effect on credit scores (-1.6).

Both the event study and matched difference-in-differences approaches illustrate a reduction in number of open revolving accounts following a traffic stop. The event study estimates for revolving balances are noisy, but the raw means for matched treated and control drivers

suggest a relative decline in balances for treated drivers, although the response appears both delayed and muted. For auto loans, the pattern of results is a bit strange, but if anything, both the event study and matched difference-in-differences figures would suggest a decline in the likelihood of car ownership beginning 2-3 months following a citation. Both Panel D of Figure 5 and Panel H of Figure 6 suggest a decline in the likelihood of having a mortgage. The slightly lagged responses of revolving balances and durable consumption are consistent with the view that access to credit is affected by the increases in financial strain and reductions in employment documented above.

Regression estimates, presented in Table 4, show that traffic tickets induce about a 0.04 (1.5 percent) reduction in the number of open revolving accounts in the first year following a traffic stop. Using the matched difference in differences approach, I find one and two year effects on revolving balances of -\$91 and -\$218, with the two year estimate statistically significant and implying about a three percent decline at the mean. Event study estimates are smaller (\$30-\$50) and not statistically different from zero. Both strategies suggest statistically significant declines in car and home ownership. While one should note that pre-event trends in car ownership do not match perfectly for treated and matched control drivers, the trend-adjusted matched difference-in-difference estimate is sizable. The two year estimate, -0.044, represents about a thirteen percent reduction in the likelihood of having an open auto loan. Both strategies suggest 1-2 percent reductions in the probability of home ownership.

Examination of heterogeneous effects by driver income, shown in Table 5, yields mixed results. Estimated impacts of traffic tickets on revolving accounts are similar across the poor and rich subsamples (-0.042 and -0.038 in the difference-in-differences specification), but the similar point estimates imply quite different percent effects, -5 percent for poor drivers and -0.6 percent for rich drivers, given the different baseline means. Both event study and difference-in-differences approaches suggest a larger impact on auto loans for poor drivers, but the rich-poor disparity is larger when considering the event study estimates.

5.4 Interpreting Magnitudes

The estimates for credit report outcomes suggest a consistent pattern of results, with traffic tickets appearing to increase financial strain and reduce credit usage among cited drivers. However, it is difficult to interpret the estimated magnitudes given that many of the credit

report measures are not what we would consider *real* outcomes. I use two approaches to aid in the interpretation of the results, detailed below.

5.4.1 Benchmarking to Other Studies

The most similar study to mine is [Dobkin et al. \(2018\)](#), who examine the impact of hospital admissions on credit report outcomes using an event study approach. Table 6 allows for a comparison of effect sizes between my paper and [Dobkin et al. \(2018\)](#) (referred to as DFKN in the table). Panel A highlights that the hospital admissions sample is older and more advantaged than the cited driver sample. However, the financial shock accompanying a hospital admission is also more severe. For the nonelderly insured population, the authors estimate that an average hospital admission increases out-of-pocket medical expenditures by about \$3,300.

As shown in Panel B, estimated 12 month effects of traffic tickets and hospital admissions on collections (0.075, 0.11) and collections balances (\$94, \$122) are quantitatively similar. Given that the average individual in the hospital sample has fewer collections, however, the percent effects are larger in [Dobkin et al. \(2018\)](#). As shown in Panel B, hospital admissions are associated with a slightly larger decline in revolving balances, -\$293 (-2.5 percent), than are traffic tickets, -\$91 (-1.3 percent). On net, the estimated impacts on financial wellbeing appear relatively similar across the two contexts, which perhaps makes sense when considering the larger shock but more advantaged sample in the [Dobkin et al. \(2018\)](#) study.

For context, I also present estimated effects from two other studies in Table 6. Note that both [Herbst \(2018\)](#) and [Dobbie et al. \(2017\)](#) study positive shocks, and hence the effects are opposite-signed. [Herbst \(2018\)](#) finds similar effects to mine of income-driven student loan repayment plans on the number of revolving accounts but larger effects on balances. Unsurprisingly, [Dobbie et al. \(2017\)](#) find significantly larger impacts of Chapter 13 bankruptcy protection on financial health outcomes.

5.4.2 Benchmarking to Earnings Changes

An alternative method for benchmarking magnitudes is to ask what change in income would predict the observed increases in financial strain. To approximate this thought experiment, I take a cross-section of individuals from the matched sample as of three months prior to the traffic stop date with positive payroll earnings. I then fit annualized payroll earnings to a

quartic in each financial strain measure.²⁰ Using the estimated quartic coefficients combined with the treatment effect estimate, I compute the income change predicted by the estimated financial strain effects. Specifically, for strain outcome z , I compute

$$\hat{\Delta}(z) = \frac{\partial y}{\partial z}(\hat{\beta}, \bar{z}) \times \hat{\theta}_z.$$

In words, $\hat{\Delta}$ is the derivative of income with respect to z , a function of the quartic coefficients $\hat{\beta}$ and evaluated at the sample mean of z , scaled by the estimated treatment effect of citations on z from Table 2. In addition to the individual account measures, I compute the income metric for the strain index, which can be interpreted as the income change implied by the joint changes in the strain outcomes.

The results are presented in Table 7. Columns 1 and 2 show income losses implying the difference-in-differences strain coefficients as of 12 and 24 months post citation for the full sample, while columns 3-6 repeat the analysis for the poorest and richest quartile of the sample corresponding to the main result tables. In each case, I evaluate at the relevant baseline mean shown in Table 2 and Table 3. Below the computed dollar values, I show the implied percentage change in income, evaluated at the relevant sample mean, in brackets.

Row 1 indicates that the sample-wide, one-year impact on collections, 0.075, is about what would be predicted by a \$360 reduction in annual outcome. For poor drivers, the income-equivalent effect is much larger. The 12 and 24 month increases in collections are associated with predicted income changes of \$663 and \$951, respectively. In other words, a poor individual's long-run post-citation increase in collections is observationally similar to about a 5.5 percent income loss. The estimated treatment effects on derogatories and delinquencies are notably smaller, and therefore the income-equivalent effects are smaller as well. The income loss predicting the observed increase in the strain index similar to that predicting the collections effect alone.

It is also useful to benchmark the treatment effects against the estimated impacts separations from payroll-covered jobs, which are presented in Appendix C. The effect of a traffic ticket on collections (0.075) is about two-thirds as large as the effect of a job separation (0.114), while the ticketing effect on revolving balances is (-\$95) is about one third as large

²⁰A flexible functional form is important for fitting the data well. The observed relationship between, for example, number of collections, and earnings is highly nonlinear, with a steep gradient at low values of collections and a much flatter gradient at high values.

as the separation effect (-\$280). Job separations increase delinquencies by 0.2, or about twice as much as traffic fines. The estimated impacts of job separations and traffic tickets on derogatories and collections balances are similar, while the citation effect on number of credit cards is about 40 percent larger than the separation effect.

5.5 Heterogeneity

As discussed above, the impacts of traffic tickets on financial strain and employment differed meaningfully for high- and low-income drivers. In this section, I consider heterogeneity along other dimensions. To be parsimonious, I first consider only impacts on the financial strain index and employment using the matched difference-in-differences framework.

Figure 7 plots one year difference-in-differences estimates for the strain index across subsets of drivers. Impacts are larger for younger (under 35) than for older (over 35) drivers and appear similar for women and men. Treatment effect estimates are similar for subprime and prime individuals, but are more pronounced for individuals with low credit usage, measured either as having a below median revolving balance or having any durable account at baseline. The most striking cut of the data is along the dimension of baseline collections. Traffic tickets have no effect on strain for drivers with a collections balance below \$150 at baseline, suggesting that the entire effect is driven by individuals who already have unpaid bills.

Figure 8 is identical to Figure 7 except that the dependent variable is employment. The pattern of heterogeneity is similar – subsamples with a large strain effects also tend to have larger employment effects and vice versa. Treatment effects on employment are larger for younger individuals and especially pronounced for young women, and are larger for individuals with higher collections, lower credit scores, and less borrowing at baseline.

Motivated by the striking difference in strain impacts across individuals with high and low initial collections, I present one year difference-in-differences estimates by baseline credit score and collections for all outcomes in Table 8. Note that below the standard errors, I report the relevant baseline control mean in brackets. As mentioned previously, one caveat with interpreting differences in effects on borrowing-related outcomes across subsamples is that credit usage may also differ across samples. Subprime individuals maintain about one quarter as many revolving accounts, for example, and individuals who do not maintain open accounts cannot experience increases in delinquencies or decreases in balances.

As shown in columns 2-3, impacts on collections and employment are about two times as large for subprime than for prime individuals. The disparate collections effect makes sense – credit is more readily available for prime individuals, and such individuals may be able to cover the unexpected nuisance fine through borrowing.

The effect size gaps are even larger when comparing individuals with high and low baseline collections in columns 4-5. Point estimates for collections and adverse financial events are, in fact, negative for individuals with little to no balance under collection at baseline. Individuals with above median baseline collections balances experience a four percent increase in collections, a six percent increase in collections balances, and a four percent increase in derogatory accounts in the one year following a traffic stop. Reductions in revolving accounts and revolving balances are also driven entirely by individuals with unpaid bills at baseline. The effect of a traffic ticket on payroll employment is about two and a half times larger for the high collections sample.

Overall, there is a clear pattern to the heterogeneity of the results. For individuals exhibiting financial stability at baseline, e.g. those without unpaid bills and those with high credit scores, traffic citations have minimal impacts. Drivers showing signs of instability, such as high collections balance, low credit scores, and low borrowing, experience significant increases in measures of financial strain and the largest drops in employment. This set of results suggests the presence of a poverty trap where small shocks have deleterious effects on already distressed individuals but are negligible for the non-distressed population.

The notion of the poverty or financial distress strap is shown empirically in Figure 9. The figure shows one year (blue circles) and two year (red squares) difference-in-differences estimates of the impact of a traffic stop on the financial strain index, estimated separately by quantiles of strain at baseline. The one year treatment effects are clearly increasing in baseline strain. Through much of the distribution, the gradient in the two year effects is even stronger. In other words, the effect of a small shock is larger for more distressed individuals and, further, effect sizes increase more over time for such individuals.

5.6 Mechanisms

As detailed in the discussion of the institutional background, a traffic citation represents a potentially multi-faceted treatment, with the exact treatment faced by any given individual

depending on post-citation decisions such as whether the individual chooses not to pay or opts to contest the ticket. While it is useful to note that the Florida Clerk’s office’s records indicate that over 90% of citations are paid on time, the data from the Florida Clerks include some information on the court disposition associated with each citation that is potentially useful for disentangling the relative important of the aspects of traffic fines beyond the pure expense shock in explaining the estimated effects. ²¹

Specifically, I examine treatment effects for individuals whose dispositions indicate payment and those whose dispositions indicate a traffic school election. Individuals making on-time payment may opt to participate in traffic school, which consists of four hours of instruction and costs \$25, but suppresses the points associated with the citation from accruing on the driver’s record. Because she makes an on-time payment and faces no increase in license points, a driver opting for traffic school almost surely will not suffer a license suspension or an increase in car insurance premiums.²²

Comparing payers and school-attendees to the sample as a whole and to each other should help isolate the impacts of various components of the treatment. The typical payer will not incur a license suspension, but those with significant driving histories or making late payments may. Payers will incur increased license points possibly leading to increased auto insurance costs. Traffic schools attenders will not bear any burden of license suspensions or increased license points. However, a traffic school election signals a savviness of institutions, an ability to come up with an extra \$25, and the flexibility to participate in four hours of instruction. As shown in the footer of Table 9, school participants are older, richer, and have higher credit scores. While I cannot account for unobservable differences between the two groups of drivers, I do present estimates using the reweighting scheme from DiNardo et al. (1996) to account for observable differences across the subsamples at baseline. Specifically, I group the data into cells according the baseline age, income, and credit score bands used in the matching step. I then reweight the cells in the payer and school subsamples to match the distribution in the full sample.

²¹There are important caveats to consider regarding the court dispositions data. Traffic citations are resolved through the ticketing county’s court system. The individual county clerks then share disposition information with the Florida Clerk of Courts. However, many of the disposition-related fields are not required to be shared with the state clerk. Futher, information on dispositions reflect current status. A disposition indicating payment may not reflect on-time payment, for example.

²²Moreover, due to concerns over the quality of the dispositions data raised earlier, traffic school attendees are the only subsample who are guaranteed to have made on-time payment.

Table 9 shows the one-year difference-in-differences estimates for the financial strain index and employment by disposition. Columns 2-3 indicate that the impact of a citation on strain is nearly two times larger for payers than for individuals opting for traffic school. Similarly, employment effects are about 50 percent larger for payers, with the point estimate not statistically significant in the traffic school sample. While neither difference is statistically significant, the pattern of results is consistent with the hypothesis that the potential costs of tickets beyond the pure financial shock, particularly those associated with increased driver license points, are an important driver of the results.²³

However, columns 4-5 reveal that the treatment effect disparities are much smaller after reweighting the data to account for observable differences between the two samples. While the point estimates still suggest that those attending traffic school experience slightly smaller increases in financial strain, the narrowing of the treatment effect disparities suggests that differences in the type of individuals opting for traffic school are quite important in explaining the treatment effect disparities. On net, the evidence provides some support for the view that license suspensions and increased insurance costs are relevant for explaining the impact of tickets but also highlights that individuals making on-time payment still experience a worsening in financial standing. The small effects for school attendees using the unweighted data supports the view that the type of individual choosing traffic school is less susceptible to the harm caused by a citation.

5.7 Effects of License Suspensions

I supplement the comparison of effects across disposition types with a direct analysis of license suspensions due to the accrual of driver license points using the empirical approach described in section 4.3. Figure 10 plots event study coefficients around the time of an individual's fourth citation in twelve months, with proxies for the timing of a 30-day points-based license suspension. Recall that the regressions also include indicators for months since initial citation (and use individuals accruing three but not four tickets in twelve months to help in the estimation of these coefficients), so the estimates should be interpreted as additive

²³Another piece of evidence consistent with this view is presented in Figure A-12 in the appendix, which plots estimated impacts of citations by quantiles of the imputed fine amount and shows minimal treatment effect gradients with respect to fine size. If effects are due only to financial shocks, we might expect larger impacts associated with larger fines.

to the effects of an initial traffic stop.

Panels A and B document sharp increases in collections following a license suspension. Quantitatively, the impact appears more pronounced than for the average traffic stop, and strikingly, the increase in collections is nearly as large for the typical driver as for the poorest individuals.²⁴ Panel C documents a fall in borrowing, measured with revolving balances, coinciding with the timing of a license suspension. Finally, Panel D illustrates an immediate and sustained drop in the likelihood of having positive payroll earnings following the revocation of driving privileges. The short-run fall in employment appears more pronounced for the poorest quartile of drivers. Figure A-14 plots event study coefficients for other outcomes.

I present the coefficient estimates in Table 10. One year out from a license suspension, individuals have about 0.15 (4 percent) more collections and \$140 (5 percent) higher collections balances. Both effects are larger than the one-year event study estimates focused on an initial citation. Incidences of adverse financial events increase by about 4 percent. I also find evidence of a slight increase in bankruptcy, measured by the presence of any public records bankruptcy filing in the past 24 months on a credit report, following a suspension. Revolving balances and employment probability are about 2.5 percent and 3 percent lower one year out from a suspension. As shown in Panel B, estimated effects are slightly larger for the poorest subset of drivers. Recall from Table 1 that the license suspension sample is more disadvantaged, and therefore the poor driver group is more representative of the sample as a whole than in the analysis of initial citations. On average, one year impacts are 10-25 percent larger for poorer drivers. Poor drivers experience larger drops in employment (about 5 percent), but no increase in bankruptcy filings, likely due to the fact that bankruptcy is quite rare among individuals without much borrowing.

The analysis of license suspensions is not only independently interesting, but also can provide insights about the mechanisms underlying the main results. Some of the estimated effect of citations alone is almost certainly due to suspensions imposed on individuals electing non-payment or those with poor driving records at baseline, both of which are difficult to observe directly in the data. The fact that the effects of suspensions on wellbeing are substantial lends credence to this view. On the other hand, the suspension effects are not

²⁴Note that I use a baseline estimated income of \$21,000 as the threshold for the poorest subset in the suspensions analysis for comparability between citation effects and suspensions effects among poor drivers. \$21,000 is the 37th percentile of baseline estimated income in the suspensions sample.

enormous relative to the main estimates, implying that the treatment effects of citations cannot be due only to ensuing license suspension effects. The observation that citations increase distress even among individuals who participate in traffic school, discussed above, also supports this claim.

6 Estimating Welfare Effects

6.1 Framework

Thus far, we have considered an array of evidence illustrating declines in wellbeing in the two years following a traffic stop. To quantify welfare losses, I adapt a common approach for valuing policies (e.g. [Finkelstein et al. 2015](#), [Deshpande 2016](#)) to the dynamic nature of the treatment effects. The approach approximates the following thought experiment – at the time of the traffic stop, how much would an individual be willing to pay to avoid the ensuing utility loss? Specifically, assume individuals have utility over consumption $u(c)$ and discount the future at rate β . Let D be an indicator for whether an individual receives a traffic ticket at $t = 0$. The parameter of interest is V from the following equation:

$$u(c_0 - V) + \sum_{t=1}^T \beta^t u(c_t | D = 0) = u(c_0) + \sum_{t=1}^T \beta^t u(c_t | D = 1). \quad (5)$$

The left hand side is the utility value of the consumption path for an unticketed driver except that the individual pays V at time zero. The right hand side is the consumption path for a ticketed driver. V is the foregone consumption at $t = 0$ that makes an individual indifferent between the ticketed and unticketed consumption streams, which we can interpret as willingness to pay to avoid the negative downstream consequences of a traffic ticket.

Solving equation (5) requires a function form assumption on $u(\cdot)$, as well as estimates of the two consumption streams. A common form for $u(\cdot)$ is a constant relative risk aversion (CRRA) utility function,

$$u(c) = \frac{c^{1-\gamma}}{1-\gamma}$$

I take $\gamma = 1$, implying a logarithmic utility function, the mean estimate in [Chetty \(2006b\)](#), as a benchmark. Note that, in this framework, increasing the curvature in the utility function will typically reduce estimates of V by increasing the pain associated with the one-time

payment at low levels of c .

For simplicity, I consider one- and two-year effects and assume individual's discount the future at rate 0.96. To estimate the consumption paths, I take a proxy measure y and use means over (event) time as the untreated consumption path. I then add the month specific treatment effects from estimates of (3) to the means to obtain the treated consumption stream:

$$[c_t|D_0 = 0] = \mu_{yt}, \quad [c_t|D_0 = 1] = \mu_{yt} + \hat{\theta}_t.$$

where μ_{yt} is the mean of y for the control group at time t .

6.2 Estimating Consumption

Even in credit report data, consumption is not observed. The most straightforward approach to estimating consumption is to assume no savings and proxy consumption with income. While income is not observed directly for individuals without payroll earnings, I can approximate income changes using either the credit bureau's estimated income measure or using the employment treatment effects combined with an assumption about the earnings loss associated with employment transitions. For the average driver, difference-in-differences estimates imply one- and two-year reductions in estimated income of \$189 and \$385 (see Table A-3). At the mean estimated income of \$33,000, and assuming log utility and $\beta = 0.96$, these effects imply $V = \$534$.

Alternatively, using the estimated employment impacts does not rely on a measure of unknown origin, but requires two important assumptions. First, one needs an assumption about how the employment estimates should be scaled. Difference in differences estimates suggest declines in employment by between one half and three quarters of a percentage point, but rely on an employment measure with low coverage. Scaling by the coverage and assuming a population-wide employment rate of about 90 percent, the estimates imply one and two year employment declines of 2.7 and 4.5 percentage points. Second, one needs an assumption on earnings losses occurring from employment transitions. A comparison of payroll earnings with ACS data in the matched sample implies payroll covered jobs pay about \$8,000 more annually. Hence, a back-of-the envelope calculation suggests one and two year income losses of \$216 ($\$8,000 \times .027$) and \$360, yielding a very similar estimate of V .

These welfare calculations ought to be considered conservative. I consider two year im-

pacts because that is the time horizon that can be reliably studied in the data. In nearly all cases, treatment effects persist for the full two years. To the extent that the effects persist or grow in the long-run, an estimate of V based on a two-year window is understated. Moreover, the impacts on outcomes such as collections and adverse financial events may have long-run impacts on access to credit, diminishing an individual's ability to smooth consumption in the future and resulting in additional utility losses. Finally, the computation of V does not consider welfare effects associated with the reductions in durable consumption.

6.3 Discussion

While simplistic, the above welfare calculation is useful for considering the policy implications of my findings more generally. Before discussing the relevance of the results for policing, however, it is important to note the argument of [Atkinson & Stiglitz \(1976\)](#) that welfare losses induced through commodity taxation, in this case the taxation of traffic infractions, ought to be remedied with redistribution through the income tax system. Moreover, the finding that many low-income households are not insured against expense shocks suggests that social programs offering insurance against expenditure or income risk may yield large benefits.

To consider the implications of my findings for criminal justice policy, note that existing evidence and standard economic theory would suggest that local governments have two goals when issuing traffic tickets — promoting safety and raising revenue. For example, [DeAngelo & Hansen \(2014\)](#), [Makowsky & Stratmann \(2011\)](#), and [Luca \(2015\)](#) show that increases in traffic citations reduce car accidents. [Baicker & Jacobson \(2007\)](#), [Makowsky & Stratmann \(2009\)](#), and [Garrett & Wagner \(2009\)](#) find evidence of a revenue-raising motive in policing decisions. Standard models for analyzing criminal justice policy typically build on [Becker \(1968\)](#), and I present a formal Becker-style model in [Appendix B](#).

The intuition of the model is that an increase in the traffic ticketing rate deters dangerous behavior by increasing the probability that an individual is audited and sanctioned, but is costly in terms of policing effort and reduces the welfare of offenders. Increased ticketing also raises government revenue. Optimal policing will set marginal benefits equal to marginal

costs:

$$\underbrace{-h'(p)}_{\text{marginal safety benefit}} + \underbrace{r}_{\text{marginal revenue benefit}} = \underbrace{c'(p)}_{\text{marginal cost of policing}} - \underbrace{V'(p)}_{\text{marginal welfare loss}}$$

To the extent that a citation represents a lump-sum transfer from an individual to the government, there are no efficiency implications. An optimizing government should then trade off the marginal labor costs and marginal deterrence benefits when deciding the optimal policing intensity.

However, welfare losses associated with citations exceeding the size of the transfer can be considered deadweight loss and ought to be weighed against the deterrence benefits when optimizing ticketing intensity. The welfare metric introduced above, which is an individual's willingness to pay to avoid future utility losses, embodies the notion of deadweight loss well.²⁵ I find that the typical ticket, which imposes a fine of about \$175, induces welfare losses slightly larger than \$500, implying that the safety benefit associated with a marginal citation must exceed \$300 for its issuance to be optimal. More generally, if governments set optimal enforcement without taking into account the compounding welfare effects of fines, they will tend to over-police. Quantifying deterrence benefits is beyond the scope of this paper, but one could speculate that citations issued for minor offenses such as broken taillights or marginal citations at already high rates of ticketing are unlikely to provide much return in terms of safety.

The heterogeneous welfare consequences of fines across driver-income levels also highlight the potential inefficiency of the flat traffic fine schedule. Appendix B considers in detail the implications of an income-based fine schedule. In particular, I present a stylized environment with two types of individuals, low-income (y_L) and high-income (y_H) and consider the effects of moving from a one-size-fits-all fine f_0 to a scheme that charges high-income drivers $f_0 + \Delta$ and low-income drivers $f_0 - \Delta$, where Δ is positive, small, and satisfies an additional simplifying assumption detailed in the appendix. The welfare effects of such a policy change

²⁵Note that, as shown in Appendix B, for the class of marginal criminals, the welfare cost associated with writing one more citation is $\frac{1}{1-p}[u(y-f) - u(y)]$, which for small p is approximately the utility cost associated with being sanctioned, i.e. the quantity V estimated above.

are proportional to the difference in the marginal utilities for poor and rich drivers:

$$\Delta \times \underbrace{\left[\frac{\partial u}{\partial c}(y_L - f_0) - \frac{\partial u}{\partial c}(y_H - f_0) \right]}_{\text{difference in marginal utilities}} \times \underbrace{p[1 - G(x^*)]}_{\text{number of tickets}} \quad (6)$$

which is positive when $u(\cdot)$ is concave. The empirical estimates suggest that the difference in marginal utilities is potentially large. Under various assumptions about the compounding utility consequences of fines for poor drivers and values of γ , and taking into account that about two million citations are issued in Florida annually, the welfare gains to setting $\Delta = \$10$ are between \$6 and \$21 million per year. Using the result in section 6.3, the total utility cost of traffic tickets is about \$2 billion, implying that the simple \$10 fine perturbation could erode the welfare costs of enforcement by as much as one percent.

7 Conclusion

Motivated both by the observation that the incidence of policing falls largely on disadvantaged communities and by a growing body of evidence suggesting that many low-income individuals may be unable to cope with unexpected expenses, this paper studies the effect of fines for traffic violations on financial wellbeing. To estimate causal effects, I link administrative traffic citation records to high frequency credit report and payroll data and leverage variation in the timing of traffic stops for identification.

The empirical analysis reveals that following the receipt of a traffic fine, individuals fare worse than would otherwise be predicted on a host of credit report outcomes. Citations increase unpaid bills, delinquencies, and adverse financial events, with the increases most pronounced for the poorest quartile of drivers. For the average driver, the short-run increases in measures of financial strain are about what would be predicted by a \$285 income loss. For the poorest drivers, the two-year increases in financial distress are observationally similar to an \$800-900, or about 5 percent, income reduction. I also find evidence of a decline in borrowing, measured by revolving accounts and balances, as well as the presence of home and auto loans on credit reports, following a traffic stop.

Traffic tickets reduce the likelihood that an individual appears as having any earnings in payroll data covering large employers by about 0.5 percentage points, or almost 5 percent relative to the mean. The employment effects are, again, most pronounced among the poor-

est drivers. Poor drivers experience an 8 percent drop in the probability of having payroll earnings in the one year following a traffic stop.

The findings offer several important takeaways. First, consistent with a growing literature documenting widespread financial fragility among U.S. households, the results imply that many individuals are not insured against even small financial shocks. When faced with a \$175 traffic fine, individuals accrue collections and delinquencies on their credit reports, suggesting an inability to cover the unexpected expense. Second, individuals exhibiting minimal distress at baseline are largely unaffected by nuisance fines, while those already facing several unpaid bills experience the most significant declines in financial wellbeing. This pattern of results is consistent with a poverty trap, whereby already distressed individuals are derailed by a new expense. Third, both the pure financial shock component of a traffic citation and the ensuing increases in driving costs, either through increases in insurance premiums or the revocation of driving privileges, appear to be important mechanisms. And fourth, a conservative estimate of the welfare loss associated with the average traffic ticket is more than two times the size of the revenue raised, suggesting that policies to reduce citations with low public safety benefits could be welfare enhancing.

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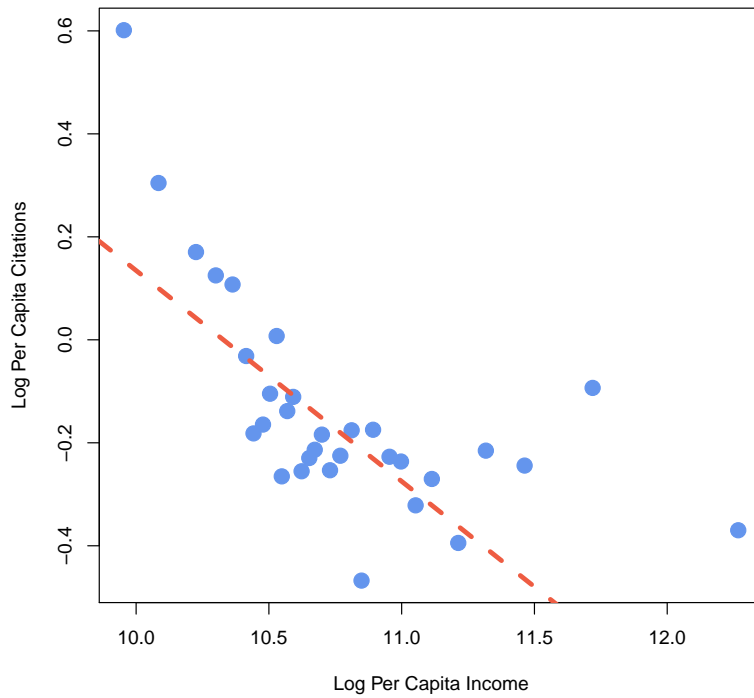
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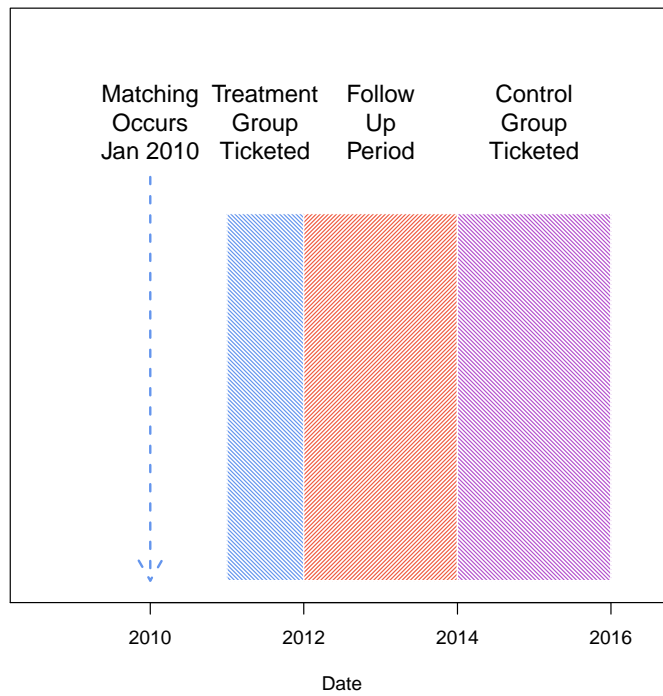
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Figure 1: Ticketing Frequency and Neighborhood Per Capita Income in Florida



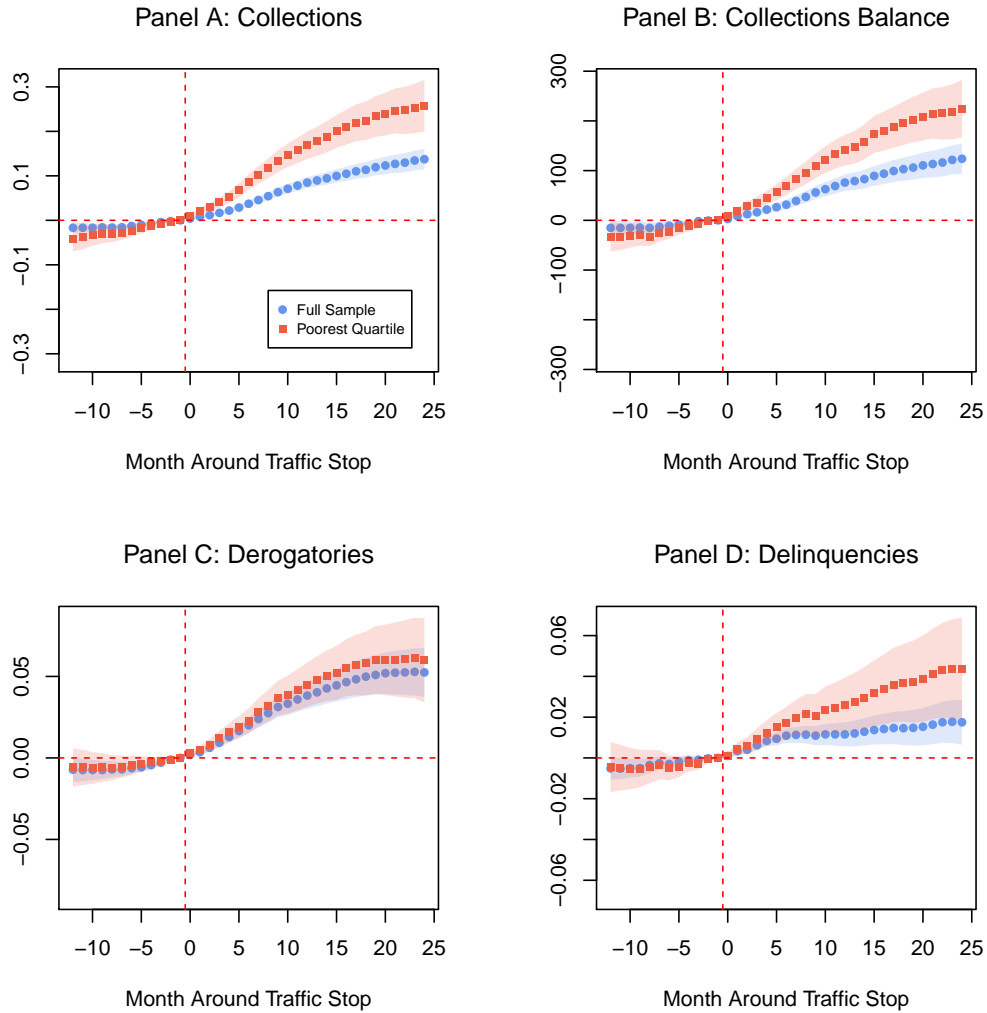
Notes: Figure plots binned means of log zip code ticketing frequency (2011-2015) against binned means of log zip code per capita income in 2010 ($N=918$). Zip code income data taken from the IRS. Number of citations for zip code residents and adjusted gross income are scaled by the number of tax returns in the IRS data to convert to per capita measures. Coefficient (standard error) from linear fit weighted by number of zip code residents is -0.41 (.07).

Figure 2: Timeline for Matching



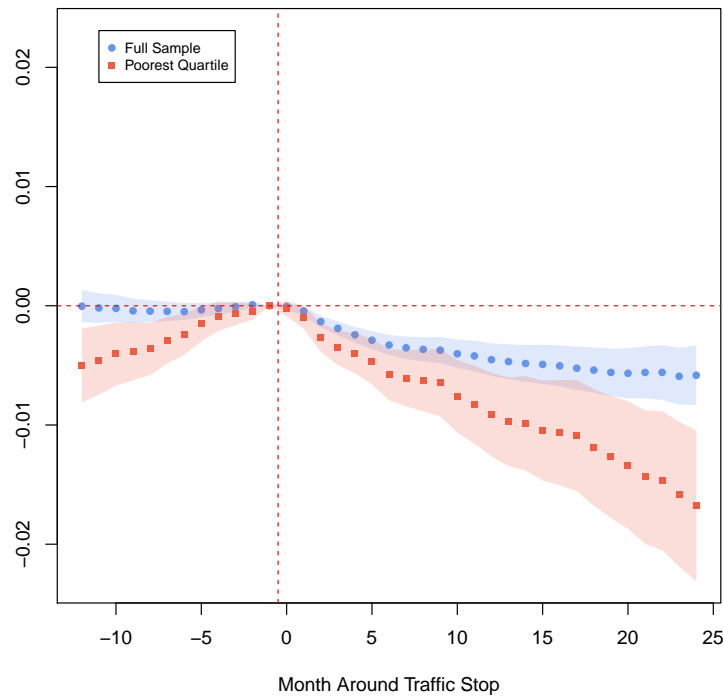
Notes: Credit bureau data range from January 2010 through December 2017. Citations data matched to credit reports range from January 2011 through December 2015. Matching uses credit report data from January 2010 and growth rates from January 2010 to January 2011. Treated drivers receive their first citation in 2011. Control individuals receive their first citation between January 2014 and December 2016. Subsamples of credit reports from January 2010 through December 2013 are used in the matched difference-in-differences analysis.

Figure 3: Event Study Estimates for Financial Strain Outcomes



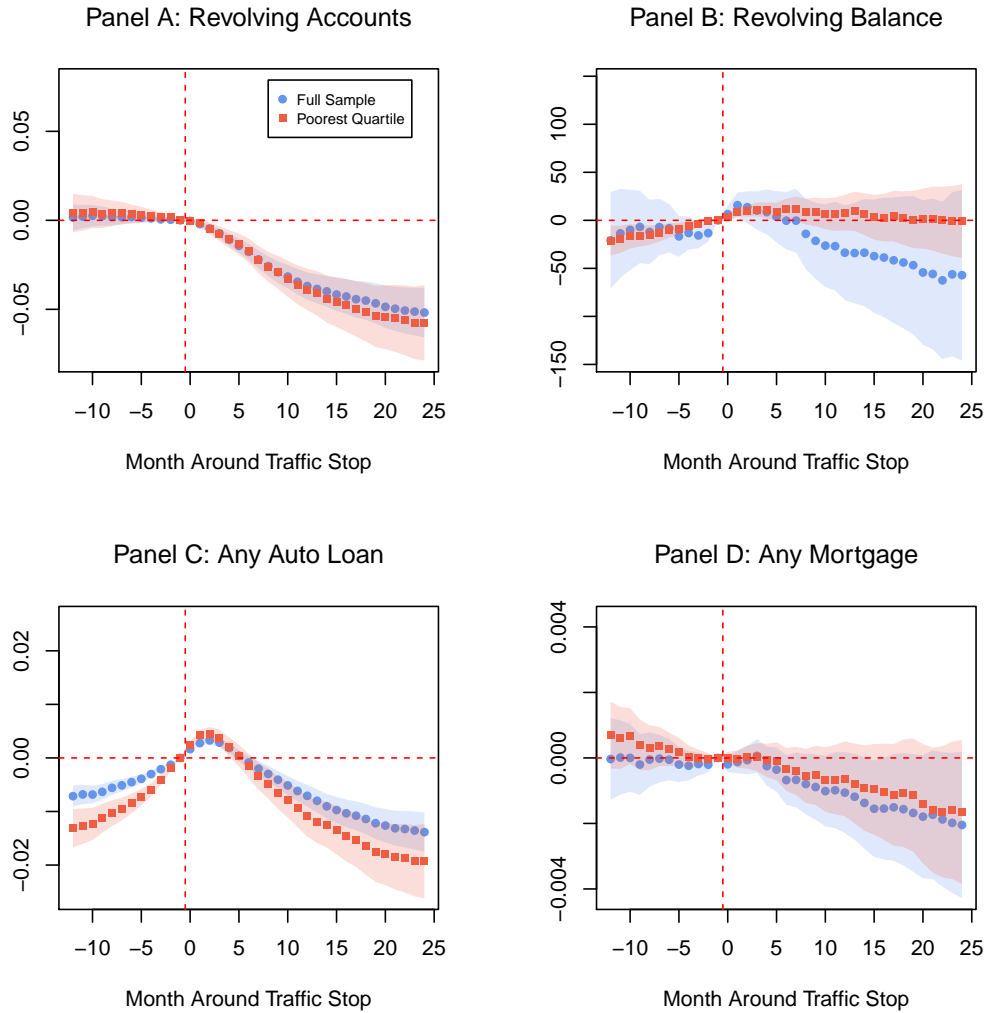
Notes: Figure plots event study estimates (with 95% confidence bands) for financial strain outcomes using the event sample (710,486 individuals). Blue circles correspond to estimates using the full sample, while red squares correspond to estimates using the poorest quartile of drivers (estimated income < \$21,000). Coefficients are normalized to $t = -1$. All regressions include individual fixed effects, time fixed effects, individual trends, and control for a quartic in age. Confidence bands constructed from standard errors clustered at the individual level.

Figure 4: Event Study Estimates for (Payroll) Employment



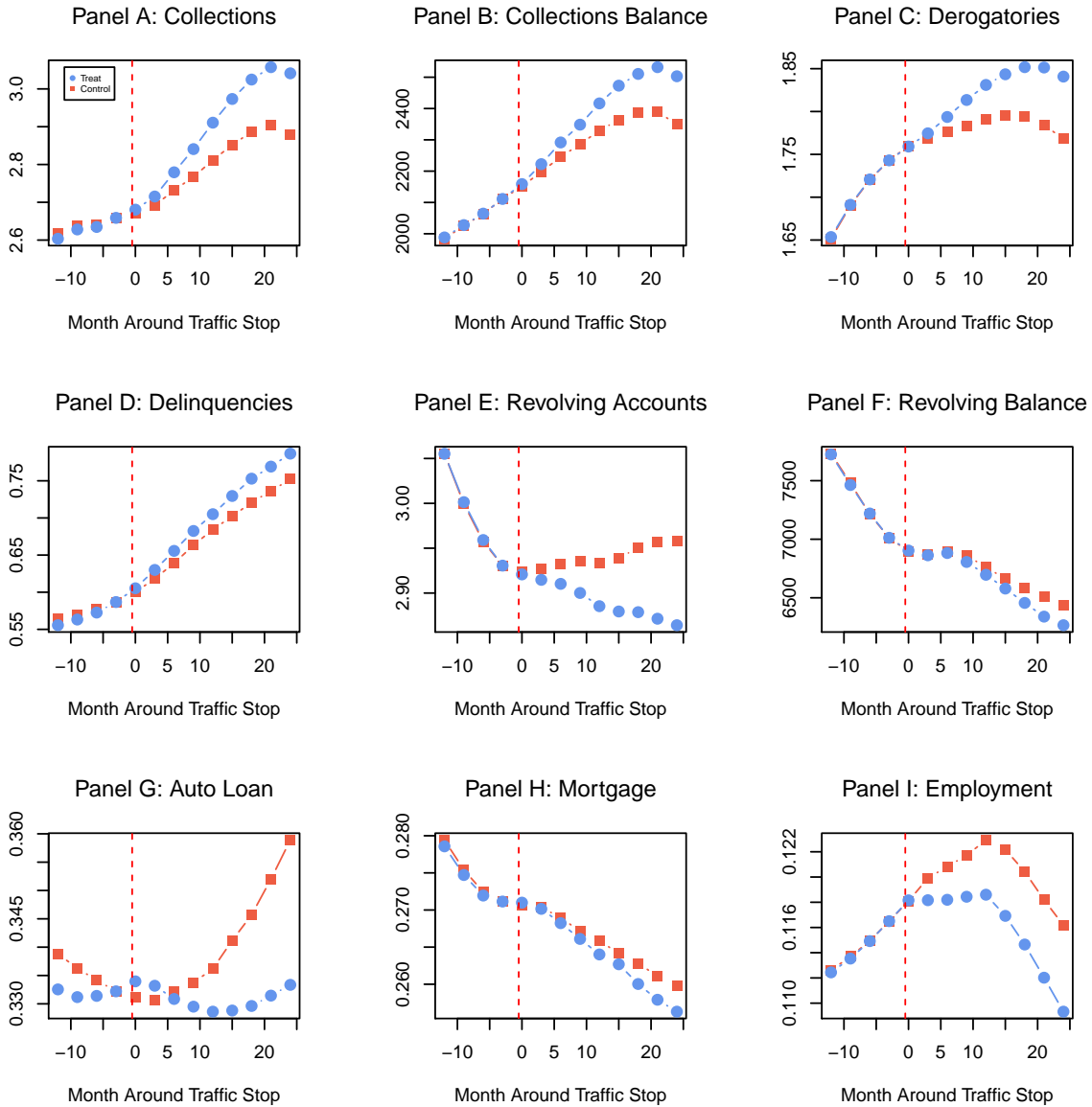
Notes: Dependent variable is an indicator for positive payroll earnings ($\mu = 11\%$). Figure plots event study estimates (with 95% confidence bands) using the event sample (710,486 individuals). Blue circles correspond to estimates using the full sample, while red squares correspond to estimates using the poorest quartile of drivers (estimated income $< \$21,000$). Coefficients are normalized to $t = -1$. All regressions include individual fixed effects, time fixed effects, individual trends, and control for a quartic in age. Confidence bands constructed from standard errors clustered at the individual level.

Figure 5: Event Study Estimates for Borrowing Outcomes



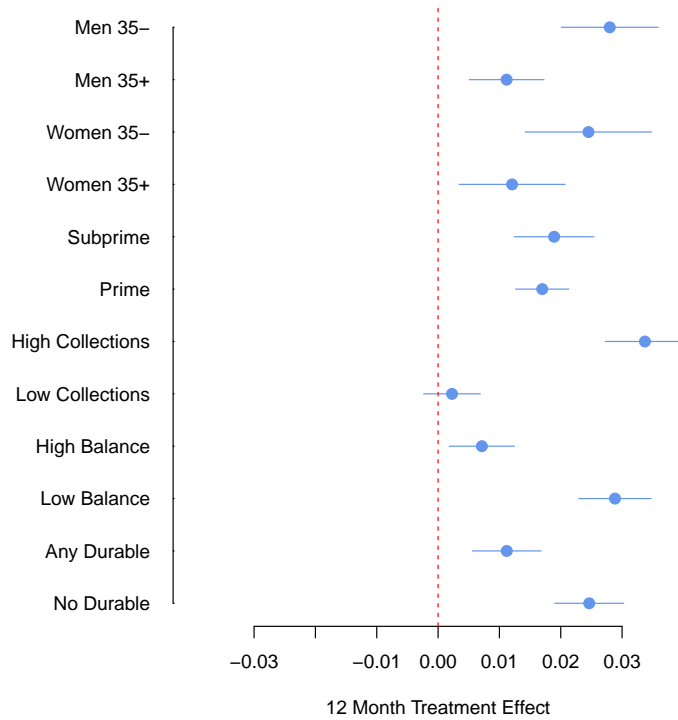
Notes: Figure plots event study estimates (with 95% confidence bands) for borrowing outcomes using the event sample (710,486 individuals). Blue circles correspond to estimates using the full sample, while red squares correspond to estimates using the poorest quartile of drivers (estimated income < \$21,000). Coefficients are normalized to $t = -1$. All regressions include individual fixed effects, time fixed effects, individual trends, and control for a quartic in age. Confidence bands constructed from standard errors clustered at the individual level.

Figure 6: Outcomes Around Traffic Stop for Matched DD Sample (Raw Data)



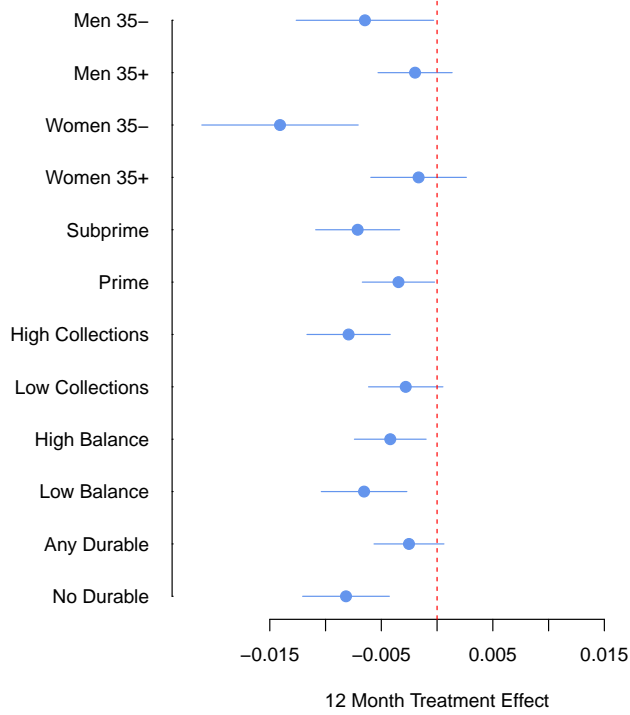
Notes: Figure plots averages of denoted outcome for the matched treatment ($N = 333,232$) and control ($N = 333,232$) groups around the traffic stop date. Blue circles correspond to the treatment group and red squares correspond to the control group. Treatment group means are normalized to the control group at $t = -3$.

Figure 7: Impacts on Financial Strain by Baseline Characteristics



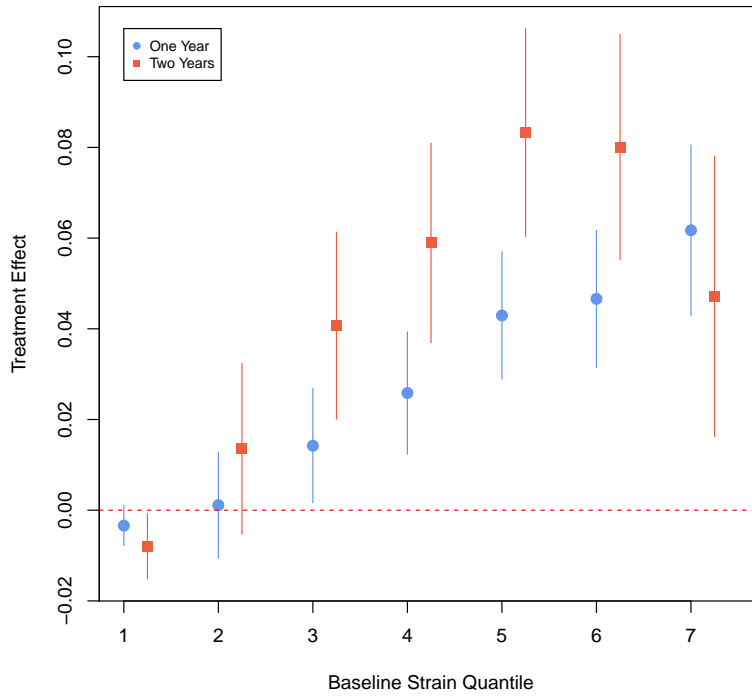
Notes: Figure plots estimated 12 month impacts (and 95% confidence intervals) of traffic tickets on the financial strain index for the denoted subsamples. The financial strain index is a standardized index summing collections, delinquencies, and derogatory accounts. See text for additional details. Estimates obtained via difference-in-differences regressions (equation 3). Each coefficient is from a separate regression. *Subprime/Prime* refers to credit scores below and above 600. *High/Low Collections* refers to individuals with above/below median collections balances. *High/Low Balance* refers to individuals with above/below median revolving balances. *Any Durable* refers to individuals with an open auto loan or mortgage.

Figure 8: Impacts on Employment by Baseline Characteristics



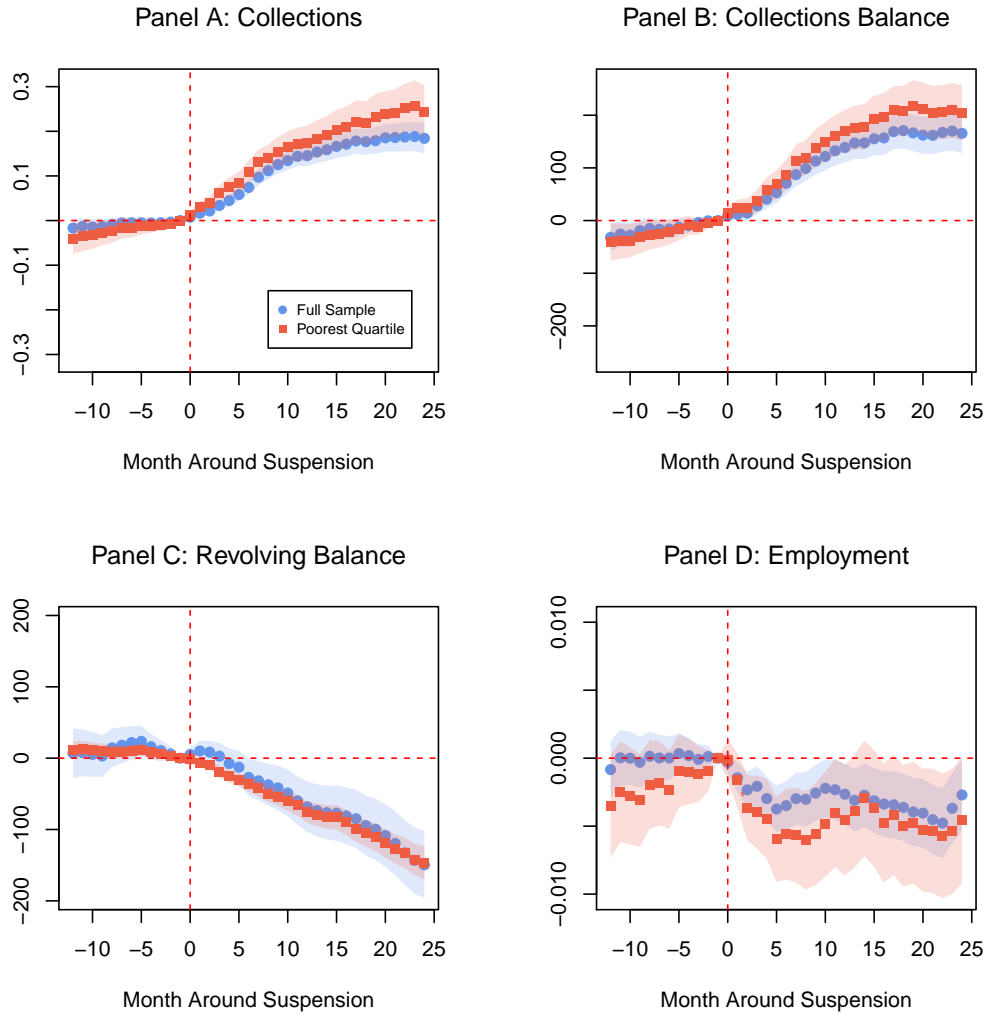
Notes: Figure plots estimated 12 month impacts (and 95% confidence intervals) of traffic tickets on the employment for the denoted subsamples. Employment is an indicator for having a payroll-covered job ($\mu = 0.16$). Results using the alternate employment measure (positive payroll earnings) are nearly identical. Estimates obtained via difference-in-differences regressions (equation 3). Each coefficient is from a separate regression. *Subprime/Prime* refers to credit scores below and above 600. *High/Low Collections* refers to individuals with above/below median collections balances. *High/Low Balance* refers to individuals with above/below median revolving balances. *Any Durable* refers to individuals with an open auto loan or mortgage.

Figure 9: Treatment Effects on Strain by Baseline Financial Distress



Notes: Figure plots estimated 12 and 24 month impacts (and 95% confidence intervals) of traffic tickets on financial strain (an index capturing collections, derogatories, and delinquencies) by quantiles of baseline strain. Estimates obtained via difference-in-differences regressions (equation 3). Quantiles are deciles except that there is excess mass at the lower bound. Hence, quantiles 2-7 correspond to the 5th-10th deciles, while quantile 1 is the bottom 40% who are approximately at the lower bound (i.e. individuals without collections, etc.). Regressions estimated separately by quantile group.

Figure 10: License Suspension Event Studies



Notes: Figure plots coefficients and 95% confidence intervals on indicators for month relative to a point-based license suspension (see text for additional details). Blue squares correspond to estimates for the full sample and red squares correspond to estimates for the poorest quartile of drivers (estimated income < \$21,000). All regressions also include month relative to initial citation indicators, a quartic in driver age, and individual and time fixed effects.

Table 1: Summary Statistics

	(1) Event Study	Matched		Suspensions	
		(2) Treat	(3) Control	(4) Treat	(5) Control
<i>Panel A: Demographics</i>					
Female	0.44	0.43	0.43	0.36	0.38
Nonwhite	0.61	0.61	0.61	0.72	0.69
Age	38.35	37.94	37.97	33.86	35.14
Credit File Age	14.12	13.54	13.37	11.77	12.57
Credit Score	609	608	609	555	573
Estimated Income	31859	32901	32827	23678	25942
<i>Panel B: Financial Strain</i>					
Collections	2.81	2.75	2.58	4.02	3.64
Collections Balance	2169	1998	1898	3182	2874
Derogatory Accounts	1.65	1.57	1.58	1.97	1.88
Delinquent Accounts	0.62	0.56	0.56	0.85	0.79
Past Due Balance	4296	3750	3657	5276	5091
Prior Bankruptcy	0.02	0.02	0.02	0.01	0.02
<i>Panel C: Credit Usage</i>					
Any Account	0.8	0.81	0.81	0.66	0.71
Revolving Accounts	2.82	3.15	3.19	1.27	1.71
Revolving Balance	6471	8663	8485	1884	2722
Any Auto Loan	0.34	0.36	0.35	0.29	0.3
Any Mortgage	0.25	0.28	0.28	0.11	0.14
<i>Panel D: Payroll Data</i>					
Employed	0.16	0.16	0.16	0.15	0.16
Positive Earnings	0.11	0.11	0.11	0.11	0.11
Monthly Earnings	3399	3422	3566	2507	2778
Individuals	710486	333232	333232	79490	135701

Notes: Column 1 reports means for the event study sample (a random 25% sample of drivers). Columns 2-3 report means for treated and control drivers in the matched sample. See Table A-2 for summary statistics for all matching *candidates*. Columns 5-6 report means for the driver license suspensions sample. See text for further details on sample construction. Summary statistics are reported as of the base period for each sample (January of the year prior to citation for the event study sample, January 2010 for the matched sample, and 12 months prior to the initial citation for the suspensions sample). As of the 2010 ACS, Florida as a whole was 51% female, 41% nonwhite, and the average age was 40.3. Statewide averages in January 2010 were 662 (credit score) and \$32,000 (estimated income).

Table 2: Impact of Citations on Financial Strain

	(1) Mean	Event Study		Matched DD	
		(2) 12 Months	(3) 24 Months	(4) 12 Months	(5) 24 Months
Collections	2.66	0.085*** (0.006)	0.137*** (0.012)	0.075*** (0.009)	0.117*** (0.015)
Derogatories	1.74	0.038*** (0.004)	0.052*** (0.008)	0.044*** (0.006)	0.078*** (0.01)
Delinquencies	0.59	0.012*** (0.003)	0.017*** (0.006)	0.008 (0.005)	0.011 (0.008)
Index	0	0.019*** (0.001)	0.028*** (0.003)	0.018*** (0.002)	0.029*** (0.003)
Collections Balance	2111.26	75.941*** (8.455)	123.815*** (15.652)	94.069*** (13.842)	166.995*** (22.453)
Past Due Balance	4457.96	61.783** (28.575)	111.996** (52.139)	138.917*** (46.125)	138.496* (75.675)

Notes: Mean in Column 1 is the control mean from the matched sample as of 3 months prior to citation. Columns 2-3 report 12 and 24 month estimates from event studies (corresponding to Figure 3). Number of individuals (observations) for event study regressions is 710,486 (34,103,328). Columns 3-4 report 12 and 24 month estimates from matched difference-in-differences regressions (corresponding to Figure 6). Number of individuals (observations) for DD regressions is 666,464 (8,664,032). Index refers to the financial strain index, a standardized sum of collections, delinquencies, and derogatory accounts. In the event study regressions, standard errors are clustered at the individual level. In the DD regressions, standard errors are clustered at the matched-pair level.

Table 3: Impacts of Citations on Financial Strain by Driver Income

	(1) Mean	Event Study		Matched DD	
		(2) 12 Months	(3) 24 Months	(4) 12 Months	(5) 24 Months
<i>Panel A: Bottom Income Quartile (<\$21,000)</i>					
Collections	3.91	0.169*** (0.016)	0.257*** (0.03)	0.134*** (0.023)	0.192*** (0.038)
Derogatories	1.31	0.045*** (0.007)	0.06*** (0.013)	0.052*** (0.009)	0.098*** (0.015)
Delinquencies	0.45	0.026*** (0.007)	0.043*** (0.013)	0.021** (0.009)	0.044*** (0.015)
Index	0.02	0.033*** (0.003)	0.05*** (0.005)	0.03*** (0.004)	0.052*** (0.007)
Collections Balance	2657.4	141.196*** (15.816)	224.426*** (29.479)	125.108*** (26.102)	203.051*** (42.108)
Past Due Balance	1478.92	1.58 (22.782)	-9.023 (44.076)	94.495*** (34.687)	215.643*** (56.844)
<i>Panel B: Top Income Quartile (>\$41,000)</i>					
Collections	0.45	0.033*** (0.006)	0.063*** (0.011)	0.023*** (0.008)	0.031** (0.013)
Derogatories	0.68	0.05*** (0.007)	0.071*** (0.012)	0.028** (0.012)	0.036* (0.019)
Delinquencies	0.36	0 (0.004)	-0.003 (0.008)	0.014* (0.009)	0.014 (0.014)
Index	-0.49	0.012*** (0.002)	0.018*** (0.003)	0.012*** (0.003)	0.014*** (0.005)
Collections Balance	518.85	38.83*** (13.519)	70.924*** (25.716)	45.129** (21.434)	78.459** (34.675)
Past Due Balance	4430.08	347.832*** (70.682)	576.437*** (130.903)	235.609** (113.636)	87.908 (185.353)

Notes: Number of individuals are as follows – event study, poorest quartile ($N = 172, 582$), event study, richest quartile ($N = 169, 643$), matched DD, poorest quartile ($N = 163, 100$), matched DD, richest quartile ($N = 158, 618$). See notes to Table 2 for additional details.

Table 4: Impact of Citations on Employment and Borrowing

	(1) Mean	Event Study		Matched DD	
		(2) 12 Months	(3) 24 Months	(4) 12 Months	(5) 24 Months
Employment	0.16	-0.004*** (0.001)	-0.004*** (0.001)	-0.005*** (0.001)	-0.008*** (0.002)
Any Earnings	0.12	-0.005*** (0.001)	-0.006*** (0.001)	-0.005*** (0.001)	-0.007*** (0.002)
Revolving Accounts	2.93	-0.037*** (0.004)	-0.052*** (0.007)	-0.049*** (0.006)	-0.096*** (0.01)
Revolving Balance	7012.45	-33.691 (25.17)	-57.093 (45.378)	-90.98 (57.02)	-217.874** (91.57)
Any Auto Loan	0.33	-0.007*** (0.001)	-0.014*** (0.002)	-0.018*** (0.002)	-0.044*** (0.003)
Any Mortgage	0.27	-0.001* (0.001)	-0.002* (0.001)	-0.003*** (0.001)	-0.006*** (0.002)

Notes: Mean in Column 1 is the control mean from the matched sample as of 3 months prior to citation. Columns 2-3 report 12 and 24 month estimates from event studies (corresponding to Figure 4 and Figure 5). Number of individuals (observations) for event study regressions is 710,486 (34,103,328). Columns 3-4 report 12 and 24 month estimates from matched difference-in-differences regressions (corresponding to Figure 6). Number of individuals (observations) for DD regressions is 666,464 (8,664,032). In the event study regressions, standard errors are clustered at the individual level. In the DD regressions, standard errors are clustered at the matched-pair level.

Table 5: Impacts of Citations on Employment and Borrowing by Driver Income

	(1) Mean	Event Study		Matched DD	
		(2) 12 Months	(3) 24 Months	(4) 12 Months	(5) 24 Months
<i>Panel A: Bottom Income Quartile (<\$21,000)</i>					
Employment	0.16	-0.009*** (0.002)	-0.01*** (0.003)	-0.011*** (0.003)	-0.015*** (0.005)
Any Earnings	0.11	-0.009*** (0.002)	-0.012*** (0.003)	-0.012*** (0.003)	-0.019*** (0.005)
Revolving Accounts	0.86	-0.039*** (0.006)	-0.058*** (0.011)	-0.042*** (0.008)	-0.099*** (0.013)
Revolving Balance	412.5	7.97 (9.476)	-0.741 (19.552)	-30.313** (13.203)	-78.118*** (20.726)
Any Auto Loan	0.13	-0.011*** (0.002)	-0.019*** (0.004)	-0.02*** (0.003)	-0.054*** (0.005)
Any Mortgage	0.02	-0.001 (0.001)	-0.002 (0.001)	0 (0.001)	-0.003* (0.002)
<i>Panel B: Top Income Quartile (>\$41,000)</i>					
Employment	0.15	-0.003** (0.001)	-0.004* (0.002)	-0.003* (0.002)	-0.004 (0.003)
Any Earnings	0.12	-0.003*** (0.001)	-0.006*** (0.002)	-0.003 (0.002)	-0.003 (0.003)
Revolving Accounts	6.07	-0.029*** (0.009)	-0.032* (0.017)	-0.039** (0.016)	-0.059** (0.026)
Revolving Balance	22918.09	-120.313 (96.974)	-182.541 (175.894)	-117.946 (223.667)	-312.501 (358.718)
Any Auto Loan	0.49	-0.002 (0.002)	-0.004 (0.004)	-0.018*** (0.004)	-0.035*** (0.006)
Any Mortgage	0.7	-0.003 (0.002)	-0.005 (0.003)	-0.008** (0.003)	-0.017*** (0.005)

Notes: Number of individuals are as follows – event study, poorest quartile ($N = 172, 582$), event study, richest quartile ($N = 169, 643$), matched DD, poorest quartile ($N = 163, 100$), matched DD, richest quartile ($N = 158, 618$). See notes to Table 4 for additional details.

Table 6: Treatment Effects Across Studies

	(1) This Paper	Studies		
		(2) DFKN	(3) Herbst	(4) DGY
<i>Panel A: Sample Means</i>				
Income	39,000	47,000	–	–
Credit Score	607	731	589	581
Age	37	49	43	45
<i>Panel B: Financial Strain Effects</i>				
Collections	.075 [2.8%]	.11 [12%]	–	-0.15 [-25%]
Collections Balance	94 [4.5%]	122 [10%]	–	-1,315 [-31%]
<i>Panel C: Borrowing Effects</i>				
Revolving Accounts	-.049 [-1.7%]	–	.07 [2.3%]	–
Revolving Balance	-91 [-1.3%]	-293 [-2.5%]	2,400 [17%]	-920 [-36%]
Any Auto Loan	-.18 [-5%]	–	0 [0%]	.02 [11%]
Any Mortgage	-.003 [-1%]	–	.02 [10%]	.132 [36%]

Notes: DFKN refers to [Dobkin et al. \(2018\)](#) who study the impact of hospital admissions. The typical admission results in \$3,275 in out-of-pocket spending. Reported estimates from DFKN correspond to the 12 month effects for the non-elderly insured population. Herbst refers to [Herbst \(2018\)](#) who studies the impact of income-driven student loan repayment, which reduces student debt minimum monthly payments by \$140 per month on average. Reported estimates from [Herbst \(2018\)](#) refer to the first year DD estimates. DGY refers to [Dobbie et al. \(2017\)](#), who study the impact of Chapter 13 bankruptcy protection. Effects sizes scaled by relevant baseline (i.e. percent effects) are shown in brackets. This list is to provide context about the range of estimates in other studies using credit report data and is not meant to be exhaustive.

Table 7: Income Changes Predicting Financial Strain Impacts

	Full Sample		Bottom Quartile		Top Quartile	
	(1) 12 Months	(2) 24 Months	(3) 12 Months	(4) 24 Months	(5) 12 Months	(6) 24 Months
Collections	-361 [-1.05%]	-564 [-1.64%]	-663 [-3.74%]	-951 [-5.36%]	-144 [-0.25%]	-195 [-0.34%]
Derogatories	-250 [-0.73%]	-443 [-1.29%]	-324 [-1.83%]	-611 [-3.45%]	-208 [-0.36%]	-267 [-0.47%]
Delinquencies	-38 [-0.11%]	-52 [-0.15%]	-106 [-0.6%]	-222 [-1.25%]	-73 [-0.13%]	-73 [-0.13%]
Index	-285 [-0.83%]	-459 [-1.34%]	-465 [-2.62%]	-806 [-4.55%]	-281 [-0.49%]	-328 [-0.57%]

Notes: Table reports an income-based metric for the matched DD treatment effects on financial strain. Specifically, for each outcome (e.g. collections), sample (e.g. poorest quartile), and time (e.g. 12 or 24 months), I estimate the income loss that would predict the treatment effect on the relevant financial strain using the estimates from the matched DD approach. See text for further details. In brackets, I report the income change as a percentage of the mean income in each subsample.

Table 8: Heterogeneous Impacts by Baseline Financial Situation

	Baseline Credit Score			Baseline Collections	
	(1) Full Sample	(2) Subprime	(3) Prime	(4) High	(5) Low
Collections	0.075*** (0.009) [2.66]	0.1*** (0.016) [4.48]	0.047*** (0.007) [0.68]	0.161*** (0.017) [4.29]	-0.012* (0.007) [1.03]
Collections Balance	94.069*** (13.842) [2111.26]	133.211*** (24.582) [3583.76]	51.357*** (10.878) [504.43]	200.431*** (23.939) [3323.54]	-12.213 (13.908) [899.89]
Derogatories	0.044*** (0.006) [1.74]	0.046*** (0.01) [2.81]	0.04*** (0.007) [0.58]	0.108*** (0.009) [2.53]	-0.021*** (0.008) [0.96]
Delinquencies	0.008 (0.005) [0.59]	0.001 (0.008) [0.86]	0.015*** (0.006) [0.29]	-0.004 (0.007) [0.77]	0.02*** (0.006) [0.4]
Revolving Accounts	-0.049*** (0.006) [2.93]	-0.04*** (0.006) [1.16]	-0.06*** (0.01) [4.86]	-0.091*** (0.007) [1.53]	-0.008 (0.01) [4.33]
Revolving Balance	-90.98 (57.02) [7012.45]	-72.554 (54.52) [2149.66]	-111.088*** (103.34) [12318.82]	-282.195*** (48.507) [2747.69]	100.092 (103.178) [11274.03]
Auto Loan	-0.018*** (0.002) [0.33]	-0.021*** (0.002) [0.24]	-0.014*** (0.003) [0.44]	-0.018*** (0.002) [0.26]	-0.018*** (0.003) [0.41]
Mortgage	-0.003*** (0.001) [0.27]	-0.002 (0.001) [0.13]	-0.004*** (0.002) [0.42]	-0.002 (0.001) [0.15]	-0.004** (0.002) [0.4]
Employment	-0.005*** (0.001) [0.16]	-0.007*** (0.002) [0.16]	-0.003*** (0.002) [0.16]	-0.008*** (0.002) [0.16]	-0.003* (0.002) [0.16]
Individuals	666464	347768	318696	333356	333108

Notes: Table reports 12 month matched difference-in-differences estimates across subsamples. Subprime refers to individuals with credit scores below 600 at baseline. High collections refers to individuals with an above median (\sim \$150) collections balance at baseline.

Table 9: Treatment Effects of Payers and Traffic School Attendees

	(1) Full Sample	Unweighted		Reweighted	
		(2) Paid	(3) School	(4) Paid	(5) School
Strain	0.018*** (0.002)	0.0197*** (0.004)	0.0108* (0.006)	0.0195*** (0.004)	0.0159** (0.008)
P-Value	-	-	0.21	-	0.67
Control Mean	0	0.05	-0.16	0.01	0.01
Employment	-0.0054*** (0.001)	-0.0078*** (0.002)	-0.0054 (0.004)	-0.0074*** (0.002)	-0.0071 (0.005)
P-Value	-	-	0.62	-	0.96
Control Mean	0.16	0.16	0.16	0.16	0.16
Individuals	666464	198986	60288	198986	60288
N	8664032	2586818	783744	2586818	783744
Age	37.95	37.5	39.74	37.96	37.96
Income	32.86	31.86	37.94	32.83	32.96
Credit Score	609	601	644	608	609

Notes: Table reports 12 month matched difference-in-differences estimates for individuals with dispositions indicating a straight-pay and individuals with dispositions indicating a traffic school election. Columns 2-3 present unweighted estimates. Column 3-4 present estimates DFL reweighting the payer and school subsamples to replicate the baseline age \times baseline income \times baseline credit score distribution of the full sample. P-values are for tests of equality between coefficients in columns 2-3 and columns 3-4.

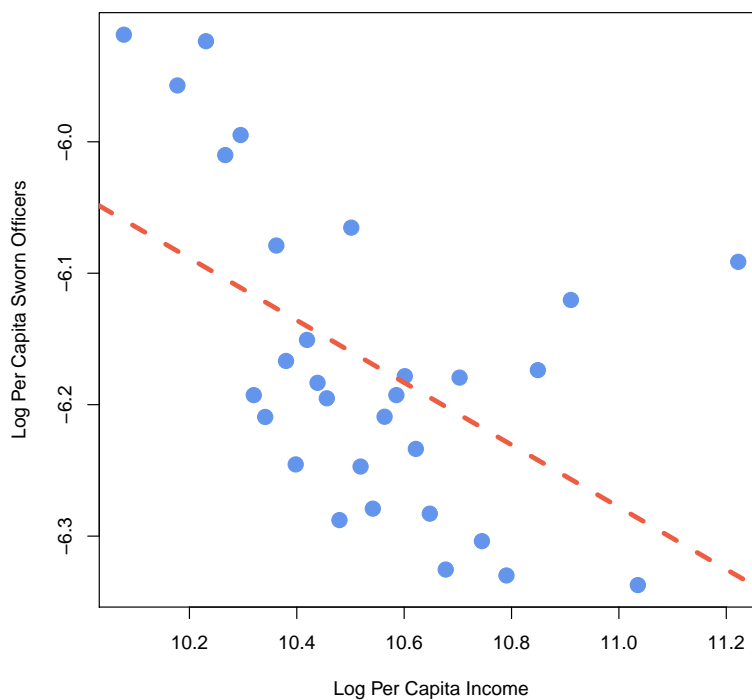
Table 10: Event Study Estimates of Impact of License Suspensions

	Event Study Estimates				
	(1) Mean	(2) 3 Months	(3) 6 Months	(4) 12 Months	(5) 24 Months
<i>Panel A: Full Sample</i>					
Collections	3.63	0.034*** (0.006)	0.074*** (0.008)	0.145*** (0.012)	0.184*** (0.017)
Collections Balance	2860.01	27.95*** (8.136)	70.848*** (10.814)	138.717*** (14.71)	165.685*** (19.26)
Derogatories	1.88	0.023*** (0.003)	0.044*** (0.005)	0.078*** (0.007)	0.078*** (0.011)
Bankruptcy	0.02	0 (0)	0.001** (0)	0.001* (0)	0.001 (0.001)
Revolving Balance	2732.85	2.786 (9.633)	-26.905** (13.193)	-68.114*** (18.046)	-149.406*** (24.102)
Employment	0.11	-0.002*** (0.001)	-0.003*** (0.001)	-0.003** (0.001)	-0.003** (0.001)
<i>Panel B: Bottom Income Quartile (<\$21,000)</i>					
Collections	3.79	0.061*** (0.01)	0.108*** (0.014)	0.173*** (0.02)	0.244*** (0.029)
Collections Balance	2539.17	37.257*** (11.016)	87.269*** (14.954)	169.645*** (20.662)	204.426*** (26.994)
Derogatories	0.85	0.032*** (0.004)	0.05*** (0.006)	0.097*** (0.009)	0.132*** (0.014)
Bankruptcy	0	0 (0)	0 (0)	0 (0)	0 (0)
Revolving Balance	280.81	-18.692*** (4.324)	-36.706*** (5.455)	-75.562*** (7.794)	-146.456*** (11.843)
Employment	0.11	-0.004*** (0.001)	-0.006*** (0.002)	-0.005** (0.002)	-0.005* (0.002)

Notes: Table reports event study estimates around the time of a license suspension using the suspensions sample (215,191 individuals, 79,490 treated). Regressions also includes months since initial citation effects, individual fixed effects, and time effects. Standard errors clustered at the individual level. Employment refers to positive payroll earnings.

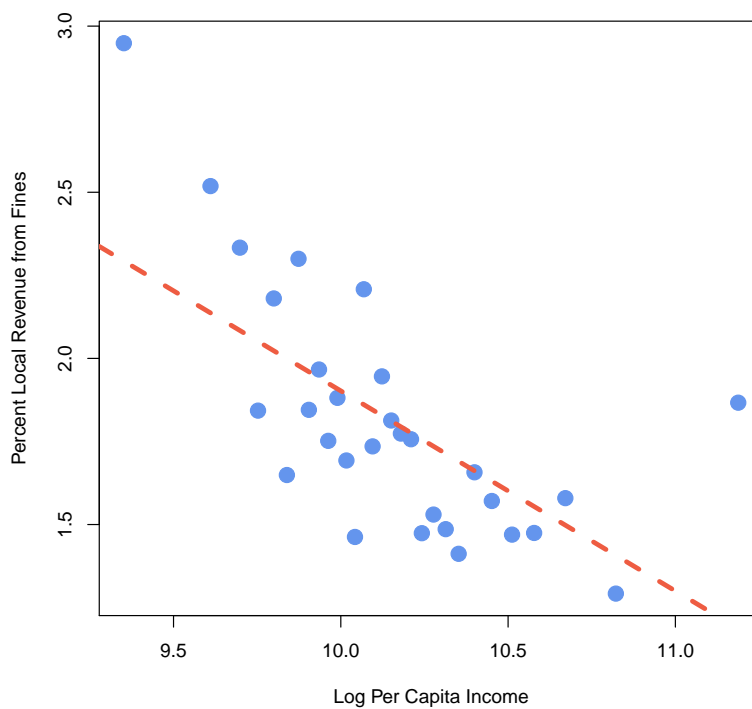
A Appendix Figures and Tables

Figure A-1: Local Policing Intensity and Per Capita Income in the U.S.



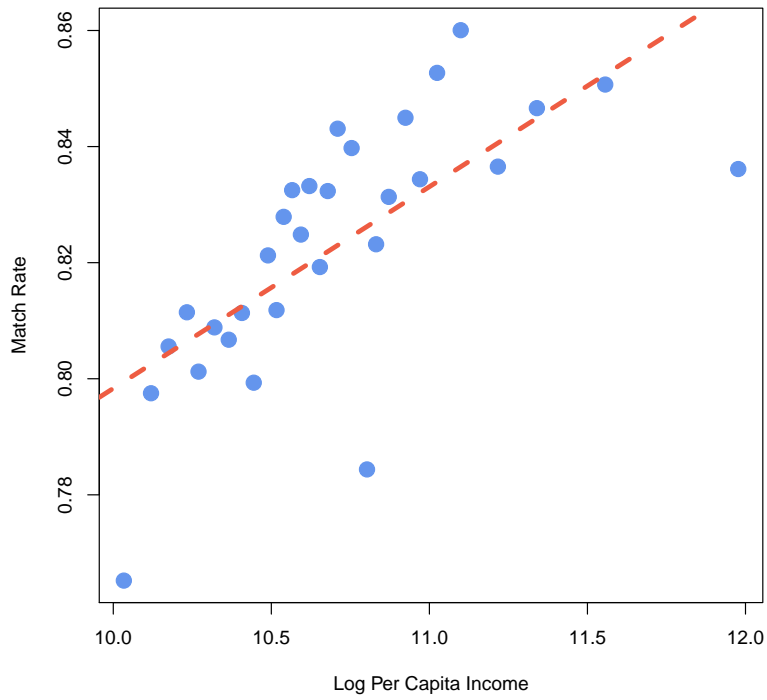
Notes: Figure plots binned means of log sworn officers per capita against binned means of log local per capita income using a 2010 cross-section from the sample of municipal police departments in [Mello \(2018\)](#) ($N = 4,327$). Dashed line is a linear fit. Coefficient (standard error) from linear fit is -0.24 (.03).

Figure A-2: Reliance on Fines and Fees and Per Capita Income in the U.S.



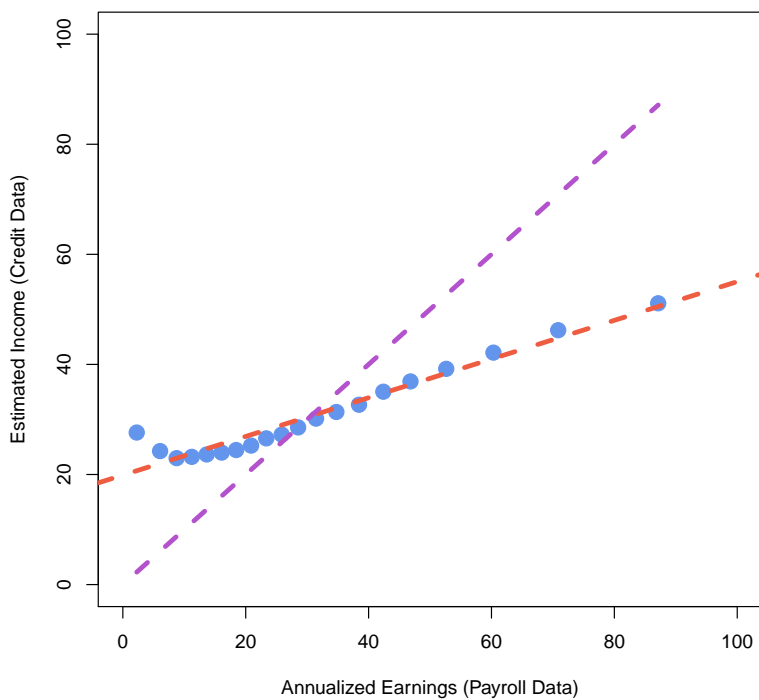
Notes: Figure plots local means of the fraction of local revenue generated from fines and fees against local means of log per capita income using the data from [Sances & You \(2017\)](#) ($N = 9,142$). Dashed line is a linear fit. Coefficient (standard error) from the implied regression is -0.6 (0.09).

Figure A-3: Credit File Match Rate by Zip Code Per Capita Income



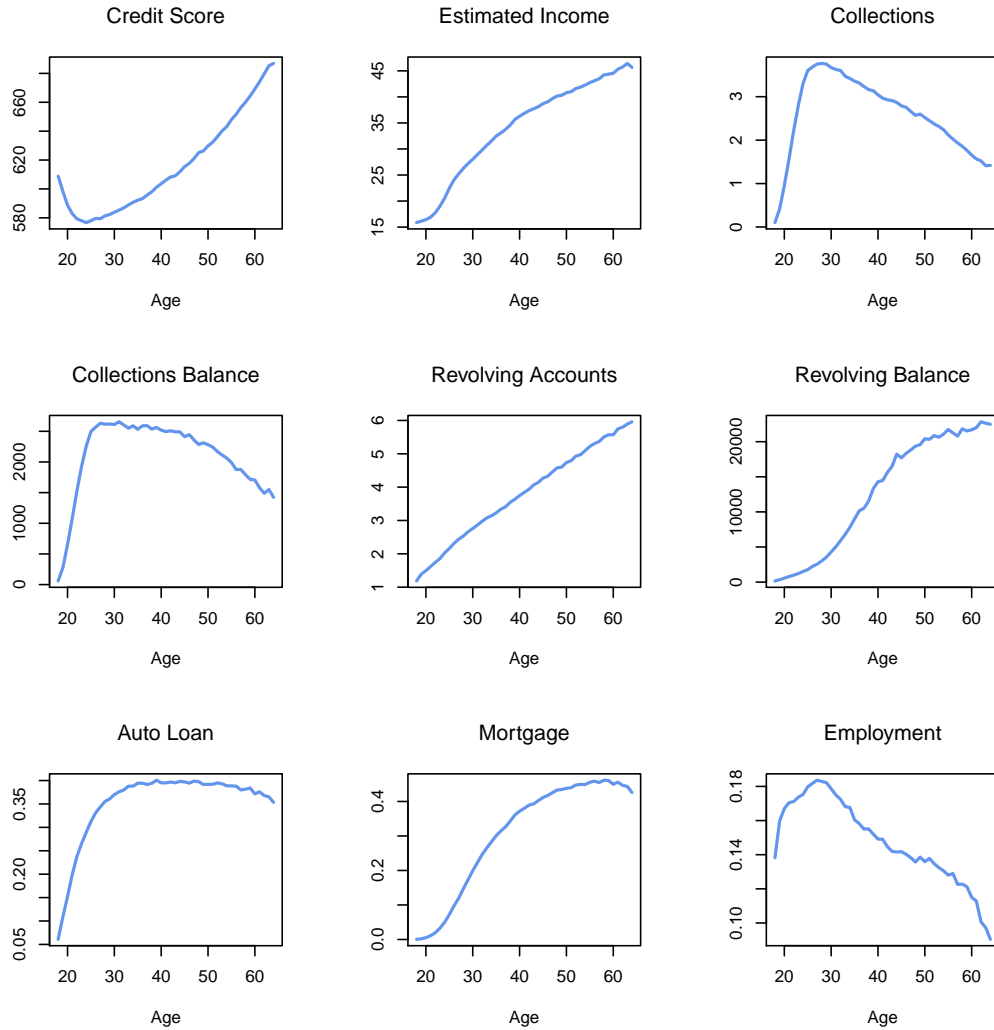
Notes: Figure plots local means of the match rate (fraction of citations matched to the credit file) against the log per capita income of the driver's home zip code computed from the IRS public use files. Sample is the universe of citations sent to credit bureau ($N = 8,851,688$). Dashed line is a linear fit. Coefficient (standard error) from the implied regression is 0.035 (0.003).

Figure A-4: Correlation Between Estimated Income and Payroll Earnings



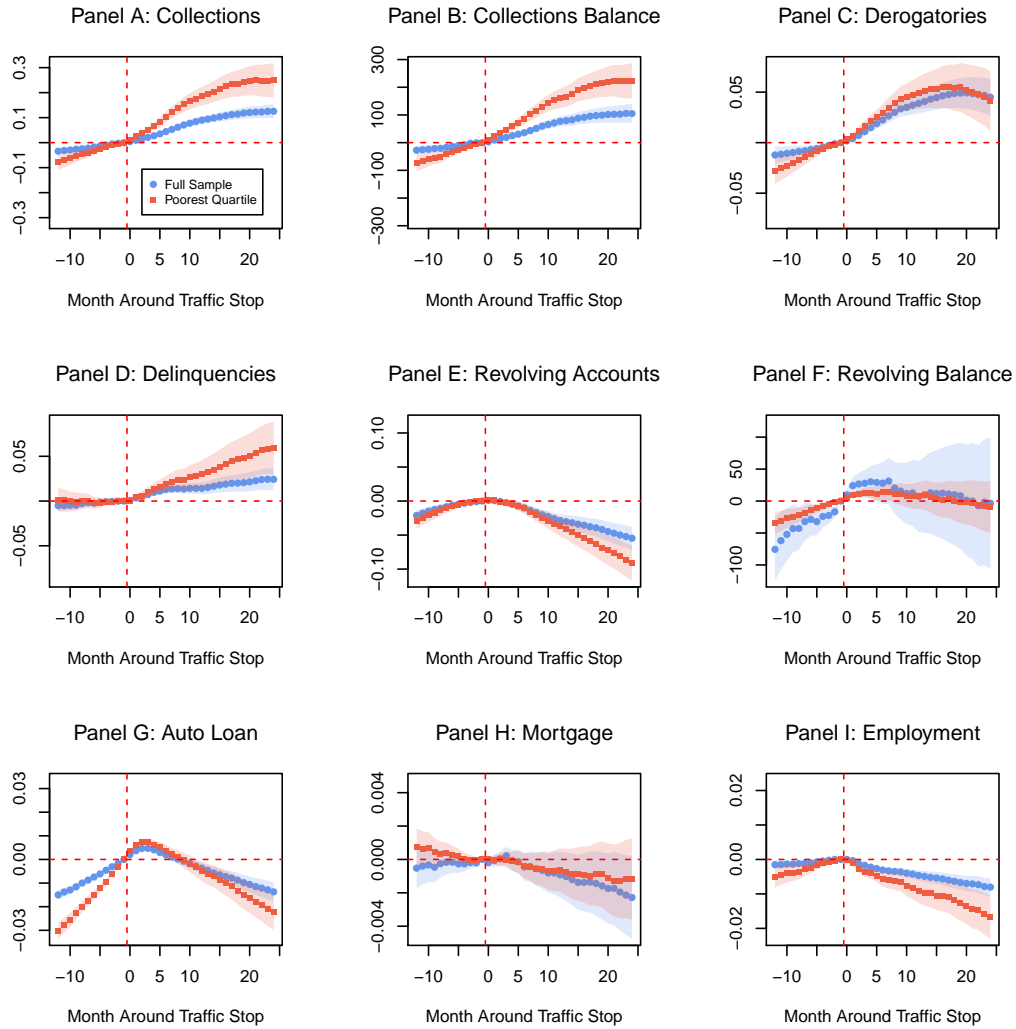
Notes: Figure plots local means of estimated income against annual earnings in the payroll data as of January 2010. Sample is individuals in the matched sample with positive payroll earnings at that date ($N = 69,548$). Red dashed line is a linear fit. Coefficient (standard error) from the implied regression is 0.3515 (0.0023). Purple dashed line is the 45-degree line.

Figure A-5: Age Profiles for Select Outcomes



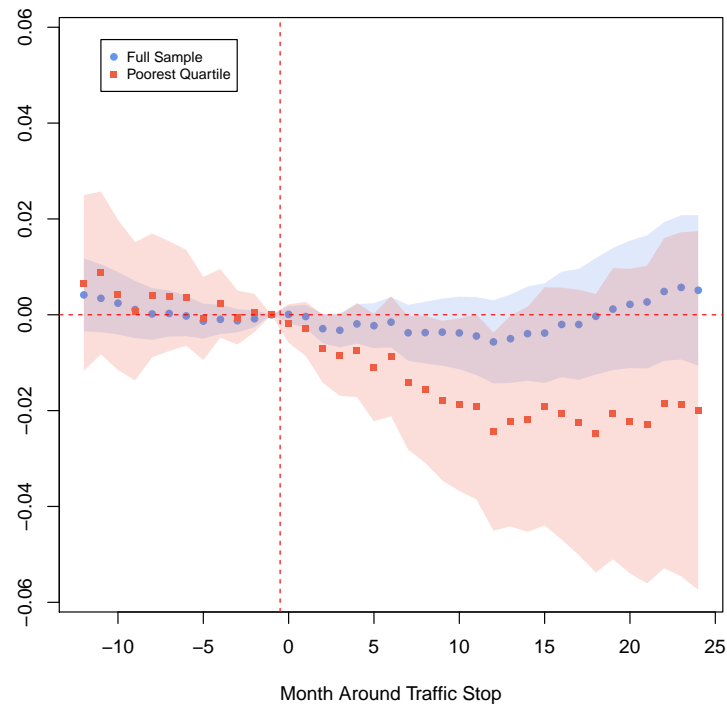
Notes: Figure plots the cross-sectional age profiles in January 2010 for selected outcomes using cited drivers present in the credit report data as of that date using ages 18-64 ($N = 2,720,749$).

Figure A-6: Event Study Estimates without Individual Trends



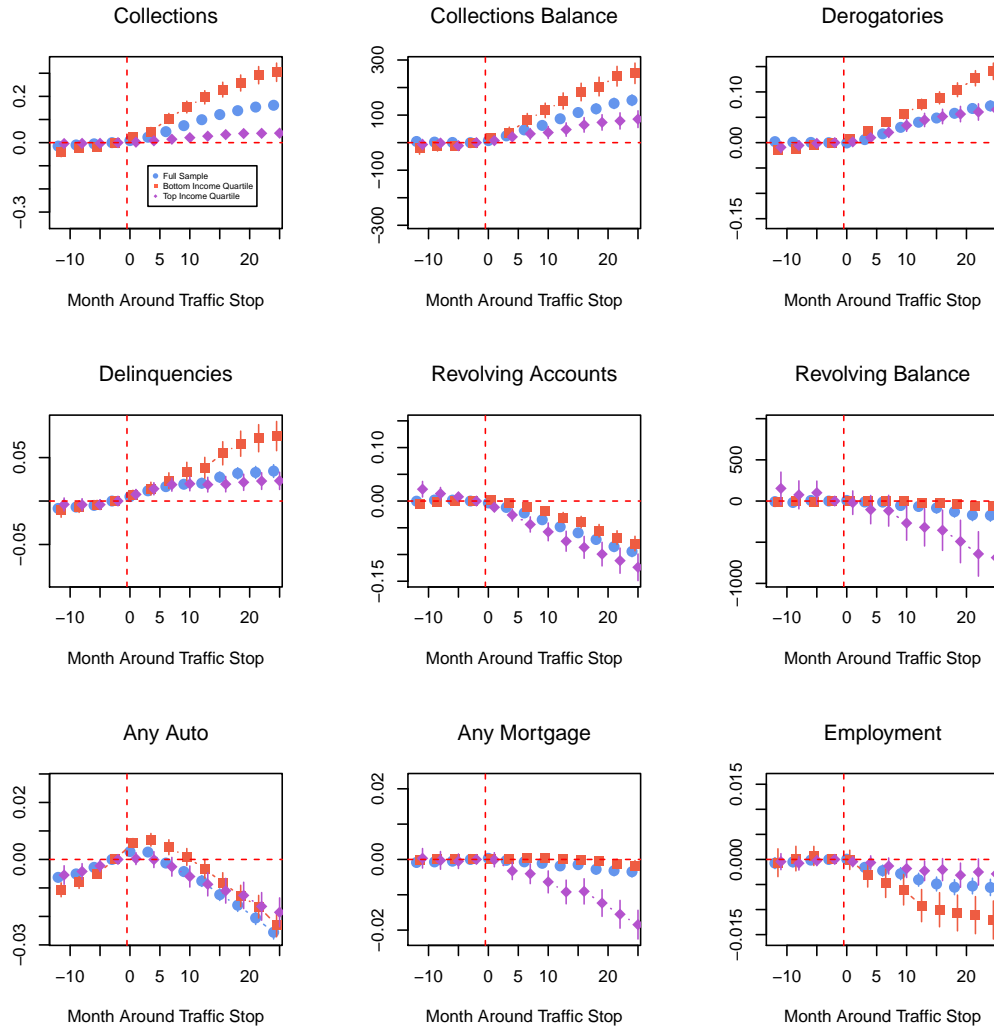
Notes: Figure plots coefficients (with 95% confidence bands) from event study regressions. Coefficients are normalized to $t = -1$. Blue circles correspond to estimates using the full sample and Red squares correspond to estimates using the poorest quartile. Identical to Figure 3, Figure 4, and Figure 5 except that regressions do not include individual trends.

Figure A-7: Event Study Estimates for Monthly Earnings



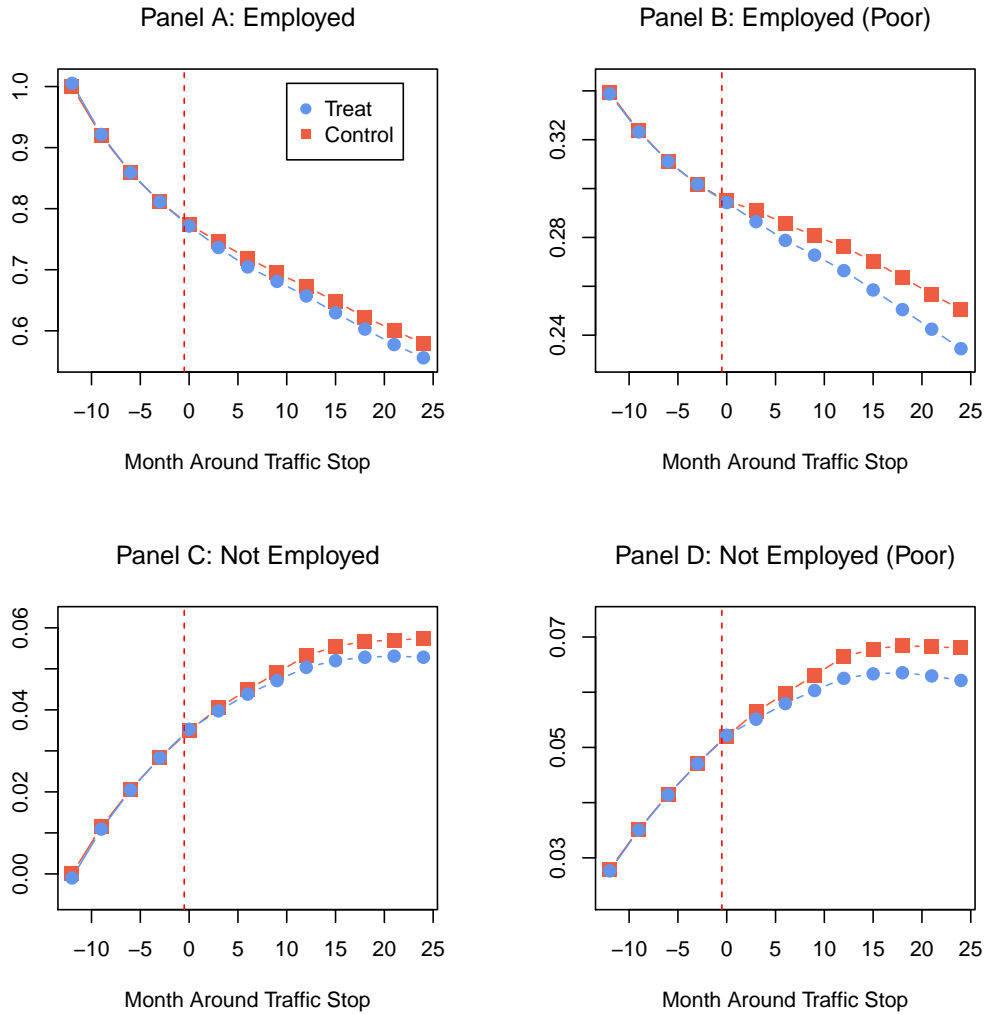
Notes: Dependent variable is a log monthly earnings from the payroll data. Figure plots event study estimates (with 95% confidence bands) using individuals from the event sample ever having positive earnings ($N = 191,054$). Coefficients are normalized to $t = -1$. All regressions include individual fixed effects and time effects.

Figure A-8: Fully Non-Parametric Matched Difference-in-Differences Estimates



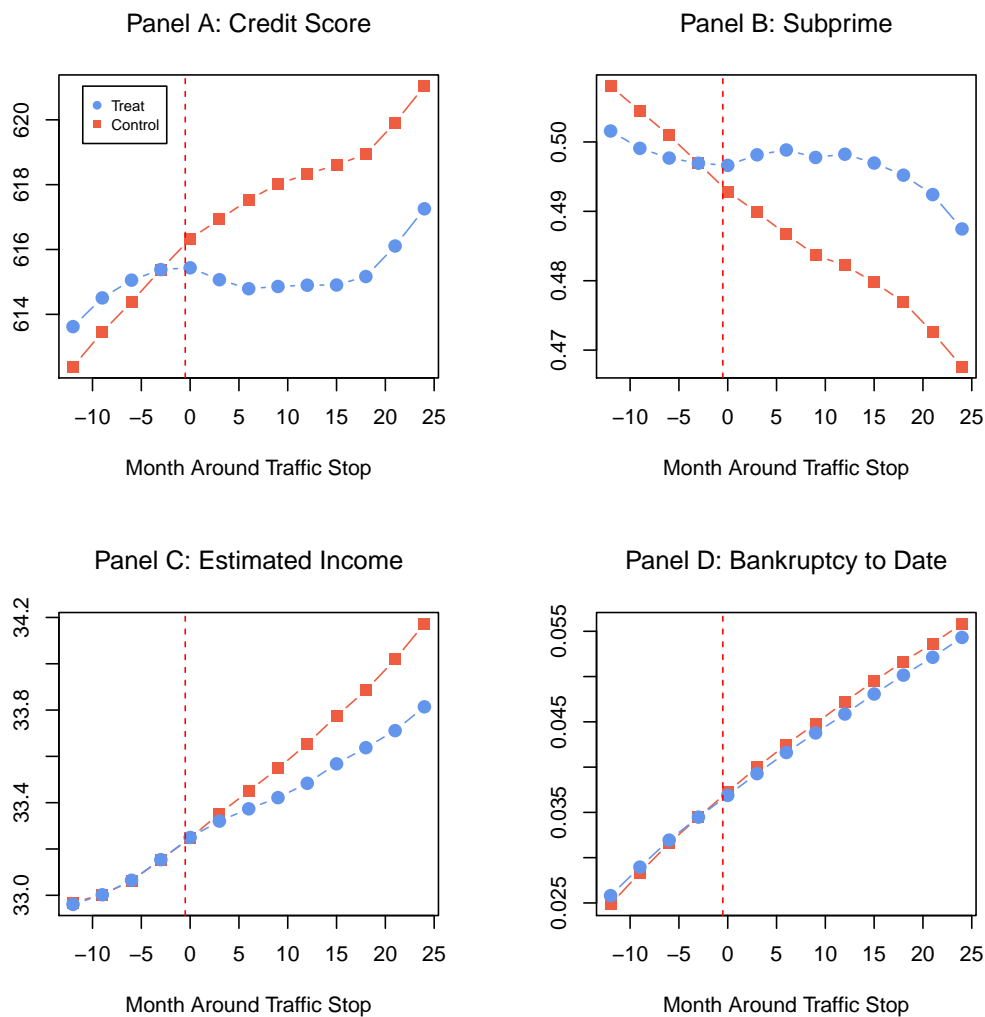
Notes: Figure plots coefficients (95% confidence intervals) on interactions between a treatment indicator and event time indicators, normalized to equal zero at $t = 3$, corresponding to equation (2). All regressions include event time fixed effects, individual fixed effects, and year and month fixed effects. Standard errors are clustered at the matched pair-level. Blue circles are estimates using the full matched sample, red squares are estimates using the poorest quartile of drivers, and purple diamonds are estimates using the richest quartile of the sample. Each series (outcome \times sample) is from a separate regression.

Figure A-9: Employment Effects by Baseline Employment Status



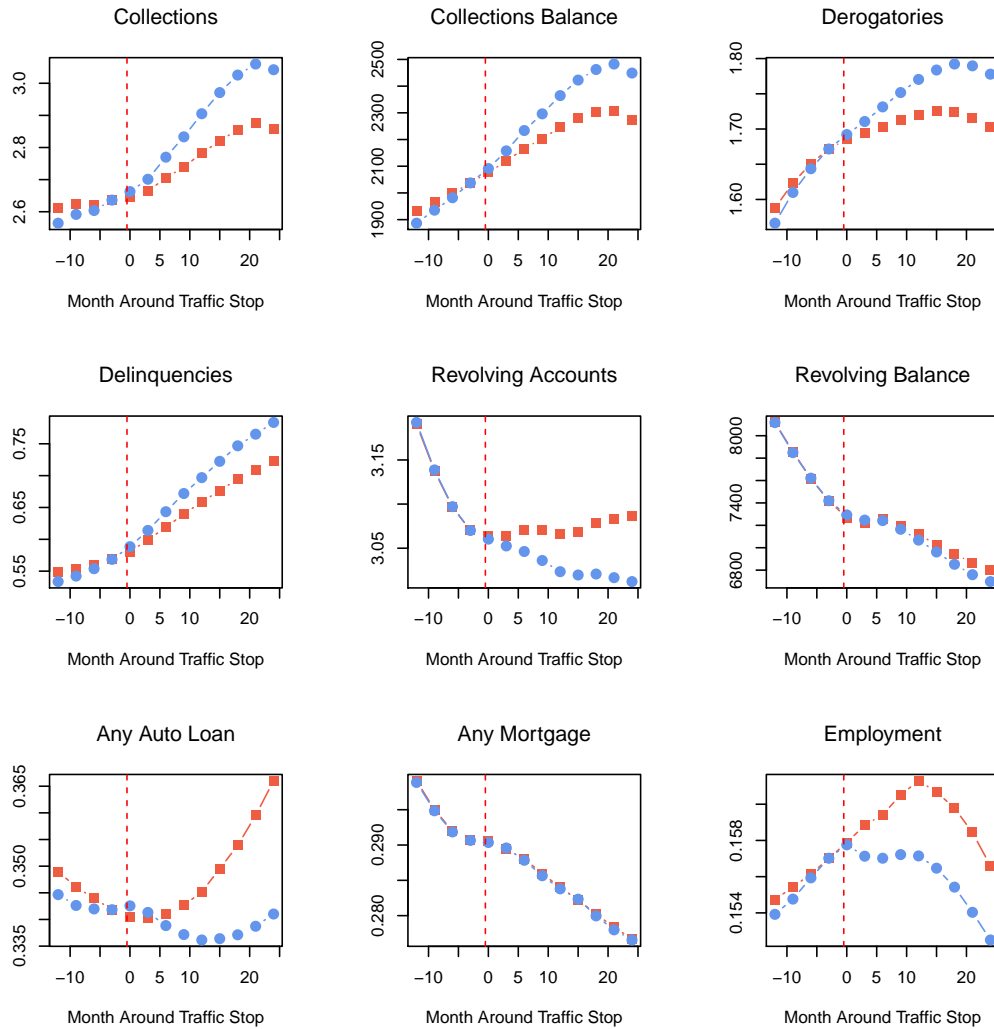
Notes: Figure plots mean employment rates (covered by payroll data) around the time of a traffic stop for the treatment and control groups (analogous to Figure 6), splitting the sample by baseline employment status. Blue dots denote the treatment group and red dots denote the control group. Treatment group means normalized to control group at $t = -3$. Panels A and C plot means for the full sample, while Panels B and D plot means for the poorest quartile of drivers.

Figure A-10: Means Around Traffic Stop Date for Other Outcomes (Raw Data)



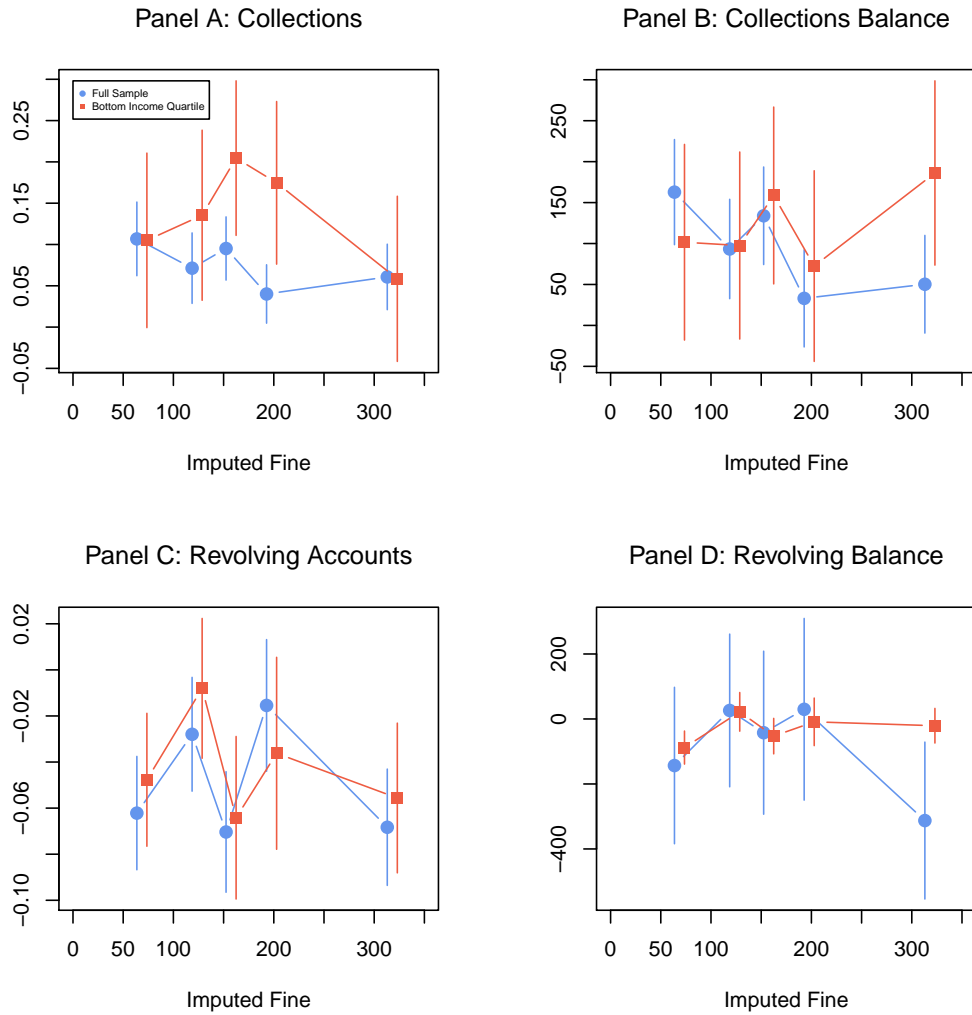
Notes: Figure plots means around the time of a traffic stop for the treatment and control groups (analogous to Figure 6). Blue dots denote the treatment group and red dots denote the control group. Treatment group means normalized to control group at $t = -3$. Dependent variable in Panel B is an indicator for having a subprime (< 600) credit score. Estimated income (Panel C) is annualized and in thousands. Dependent variable in Panel D is an indicator for any bankruptcy to date, computed using an indicator variable for the presence of a public records bankruptcy filing in the past 24 months.

Figure A-11: Outcome Means Using All Match Candidates



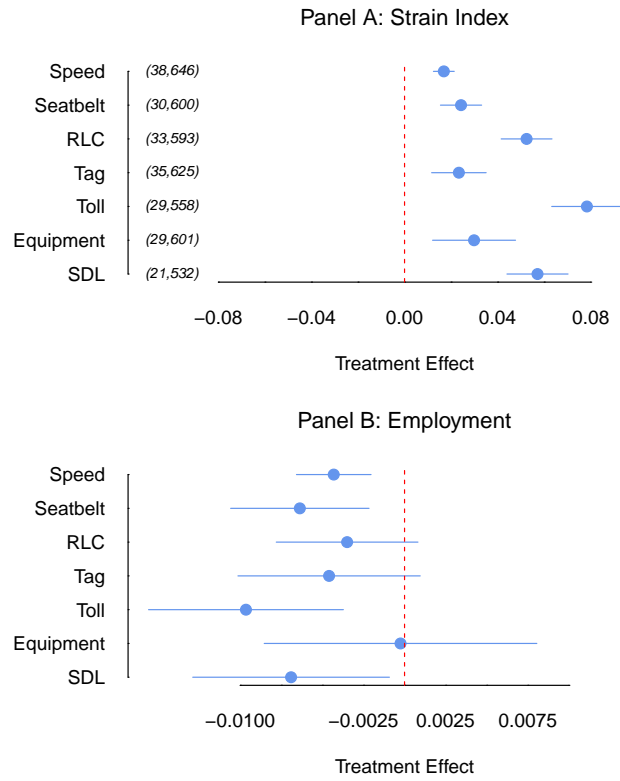
Notes: Figure plots means of outcomes for treatment and control groups using all match candidates ($N=1,430,723$). Blue dots denote the treatment group and red squares denote the control group. Treatment groups means normalized to equal control group means at $t = -3$. Placebo traffic stop dates are assigned to the control group randomly to replicate the distribution of traffic stop dates in the treatment group.

Figure A-12: Imputed Fine Gradients



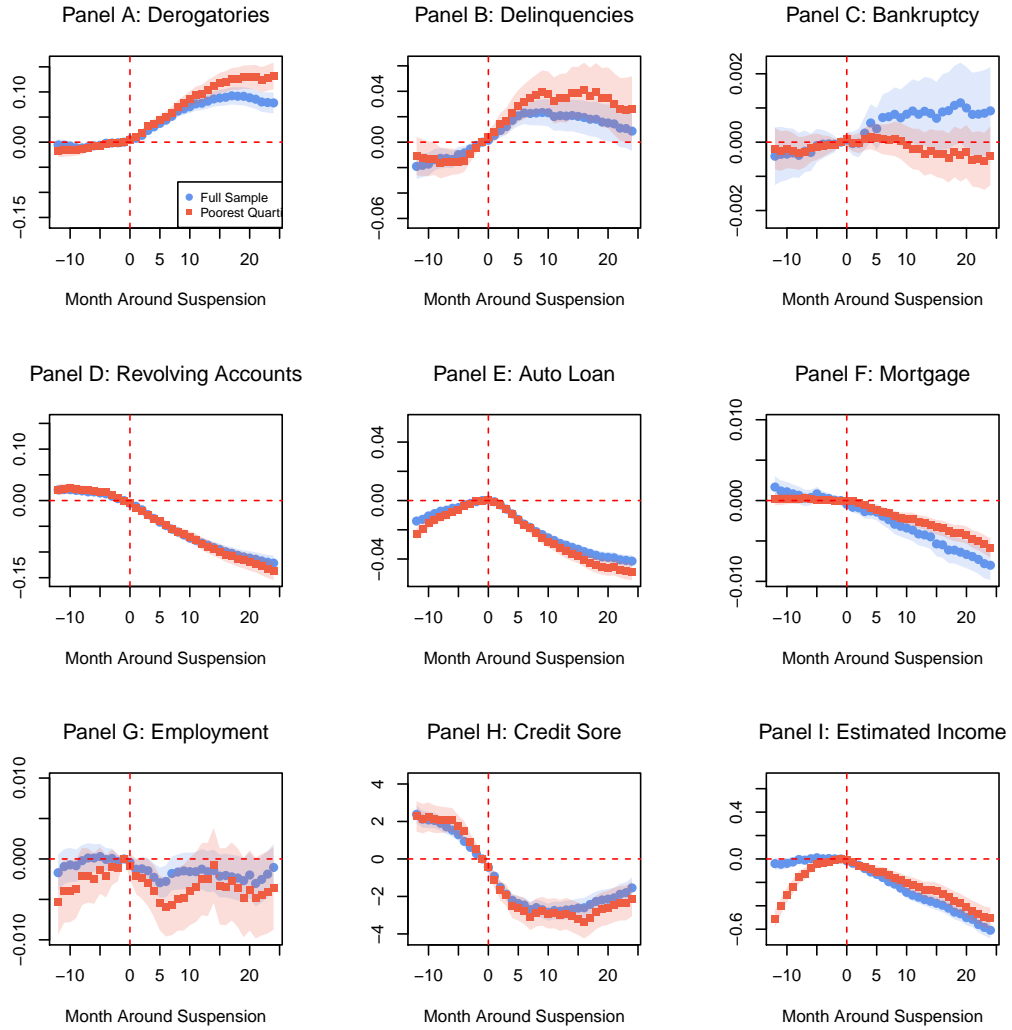
Notes: Figure plots 12 month matched difference-in-differences estimates (and 95% confidence intervals) separately by quintile of imputed fine for the treatment group's citation. Blue circles correspond to estimates using the full sample, while red squares correspond to estimates using only the bottom income quartile.

Figure A-13: Effects by Common Violation Types



Notes: Figure plots 12 month matched difference-in-differences estimates by violation type for common violation categories. *SDL* refers to driving with a suspended license. Numbers in parentheses are average estimated income (in thousands) and credit score at baseline for the relevant sample.

Figure A-14: License Suspension Event Studies for Other Outcomes



Notes: Figure plots coefficients and 95% confidence intervals on indicators for month relative to a point-based license suspension (same as Figure 10). All regressions also include month relative to initial citation indicators, a quartic in driver age, and individual and time fixed effects.

Table A-1: Credit File Match Rate by Driver Characteristics

	Any Match		Current Match	
	(1)	(2)	(3)	(4)
Female	0.044*** (0.001)	0.043*** (0.001)	0.060*** (0.002)	0.059*** (0.002)
Age <18	-0.154*** (0.005)	-0.152*** (0.004)	-0.534*** (0.007)	-0.533*** (0.006)
Age 25-34	0.070*** (0.002)	0.069*** (0.002)	0.200*** (0.006)	0.200*** (0.006)
Age 35-44	0.098*** (0.004)	0.097*** (0.004)	0.241*** (0.008)	0.240*** (0.008)
Age 45-54	0.107*** (0.004)	0.106*** (0.004)	0.256*** (0.008)	0.255*** (0.008)
Age 55+	0.121*** (0.006)	0.121*** (0.007)	0.276*** (0.011)	0.276*** (0.011)
Black	-0.017*** (0.005)	-0.020*** (0.002)	-0.020*** (0.004)	-0.024*** (0.002)
Hispanic	-0.028*** (0.006)	-0.035*** (0.005)	-0.041*** (0.006)	-0.048*** (0.006)
Other/Unknown	0.002 (0.007)	-0.006 (0.007)	-0.002 (0.008)	-0.009 (0.008)
Log Zip Income	0.025*** (0.005)	0.030*** (0.003)	0.028*** (0.005)	0.034*** (0.002)
Mean	0.82	0.82	0.75	0.75
County FE	No	Yes	No	Yes
Time FE	No	Yes	No	Yes
R2	0.022	0.026	0.09	0.094
N	8,851,688	8,851,688	8,851,688	8,851,688

Notes: Regression is estimated at the citation level. *Any Match* refers to whether the driver was matched to the credit file at any point. *Current Match* refers to whether the driver was matched to the credit file at the time of citation. Ages 18-24 and white are the excluded age/race categories. County fixed effects refer to county of the traffic stop. Time fixed effects are for the month (year \times month) of the traffic stop.

Table A-2: Summary Statistics for Matching Candidates and Matches

	Candidates		Matches		(5) Statewide
	(1) Treat	(2) Control	(3) Treat	(4) Control	
<i>Panel A: Demographics</i>					
Female	0.4	0.45	0.43	0.43	0.51
Nonwhite	0.65	0.53	0.61	0.61	0.41
Age	36.96	38.65	37.94	37.97	40.3
Credit File Age	13	13.94	13.54	13.37	-
Credit Score	597	616	608	609	662
Estimated Income	31323	33788	32901	32827	32000
<i>Panel B: Financial Strain</i>					
Collections	2.95	2.58	2.75	2.58	-
Collections Balance	2139	1846	1998	1898	-
Derogatory Accounts	1.68	1.53	1.57	1.58	-
Delinquent Accounts	0.6	0.54	0.56	0.56	-
Past Due Balance	4092	3479	3750	3657	-
Prior Bankruptcy	0.02	0.02	0.02	0.02	-
<i>Panel C: Credit Usage</i>					
Any Account	0.8	0.82	0.81	0.81	-
Revolving Accounts	2.85	3.32	3.15	3.19	-
Revolving Balance	7802	8929	8663	8485	-
Any Auto Loan	0.35	0.36	0.36	0.35	-
Any Mortgage	0.26	0.3	0.28	0.28	-
<i>Panel D: Payroll Data</i>					
Employed	0.16	0.15	0.16	0.16	-
Positive Earnings	0.11	0.11	0.11	0.11	-
Monthly Earnings	3203	3612	3422	3566	-
Individuals	817775	612948	333232	333232	-

Notes: *Candidates* refers to individuals eligible for the matching procedure. *Matches* refers to individuals successfully matched. Benchmark values for demographic characteristics computed from the 2010 ACS. Benchmark values for credit score and estimated income were provided by the credit bureau.

Table A-3: Difference in Difference Estimates for Other Outcomes

	(1)	(2)	(3)	(4)
	Credit Score	Subprime	Estimated Income	Bankruptcy
<i>Panel A: Full Sample</i>				
12 Months Post	-1.452*** (0.25)	0.006*** (0.002)	-186.988*** (34.353)	0 (0)
24 Months Post	-0.106 (0.39)	0.001 (0.003)	-385.384*** (54.384)	0.001 (0.001)
Control Mean	615.38	0.5	33154.02	0.03
Individuals	666464	666464	666464	666464
N	8641126	8641126	8664025	8664032
<i>Panel B: Bottom Income Quartile (<\$21,000)</i>				
12 Months Post	-2.33*** (0.532)	0.01*** (0.003)	6.055 (38.949)	0 (0)
24 Months Post	-2.3*** (0.825)	0.01* (0.005)	-203.058*** (61.563)	0 (0.001)
Control Mean	556.62	0.73	16517.7	0.01
Individuals	163100	163100	163100	163100
N	2108433	2108433	2120295	2120300
<i>Panel C: Top Income Quartile (>\$41,000)</i>				
12 Months Post	-0.909* (0.49)	0.002 (0.003)	-507.786*** (114.526)	0.001 (0.001)
24 Months Post	0.835 (0.767)	-0.007 (0.005)	-871.389*** (181.841)	0.003 (0.002)
Control Mean	711.46	0.15	57946.12	0.02
Individuals	158618	158618	158618	158618
N	2060092	2060092	2062034	2062034

Notes: Table presents matched differences-in-differences estimates (same as columns 4-5 in Table 3) for other outcomes. Dependent variable in column 2 is an indicator for having a subprime (< 600) credit score. Dependent variable in column 4 is any bankruptcy to date, constructed from a variable indicating the presence of a public records bankruptcy filing in the past 24 months. Note that credit score and estimated income variables are not imputed and hence missing person-months are dropped.

Table A-4: Difference-in-Differences Estimates for Employment and Earnings

	Full Sample		Positive Earnings	
	(1) Employed	(2) Any Earnings	(3) Earnings	(4) Log Earnings
<i>Panel A: Full Sample</i>				
12 Months Post	-0.005*** (0.001)	-0.005*** (0.001)	-126.103 (78.933)	-0.004 (0.009)
24 Months Post	-0.008*** (0.002)	-0.007*** (0.002)	-112.557 (131.556)	-0.007 (0.014)
Control Mean	0.16	0.12	3697	7.74
Individuals	666464	666464	146141	146141
N	8664032	8664032	1015962	1015962
<i>Panel B: Bottom Income Quartile (<\$21,000)</i>				
12 Months Post	-0.011*** (0.003)	-0.012*** (0.003)	-52.391* (30.16)	-0.027 (0.022)
24 Months Post	-0.015*** (0.005)	-0.019*** (0.005)	-3.251 (69.609)	-0.041 (0.035)
Control Mean	0.16	0.11	1611	7.1
Individuals	163100	163100	44652	44652
N	2120300	2120300	244051	244051
<i>Panel C: Top Income Quartile (>\$41,000)</i>				
12 Months Post	-0.003* (0.002)	-0.003 (0.002)	-279.688 (192.022)	0.004 (0.015)
24 Months Post	-0.004 (0.003)	-0.003 (0.003)	-425.991 (309.955)	-0.002 (0.024)
Control Mean	0.15	0.12	6926	8.39
Individuals	158618	158618	27284	27284
N	2062034	2062034	241762	241762

Notes: Table presents matched differences-in-differences estimates (same as columns 4-5 in Table 3) for employment and earnings.

Table A-5: Sensitivity of 12 Month Effects to Imputation

	Data Type	
	(1) Imputed	(2) Not Imputed
<i>Panel A: Financial Strain</i>		
Collections	0.075*** (0.009)	0.079*** (0.009)
Derogatories	0.044*** (0.006)	0.047*** (0.006)
Collections Balance	94*** (14)	94*** (14)
Past Due Balance	139*** (46)	148*** (48)
<i>Panel B: Credit Usage</i>		
Revolving Accounts	-0.049*** (0.006)	-0.051*** (0.007)
Revolving Balance	-91 (57)	-58 (109)
Any Auto Loan	-0.018*** (0.002)	-0.022*** (0.002)
Any Mortgage	-0.003*** (0.001)	-0.003 (0.002)

Notes: Table presents 12 month matched difference-in-differences estimates (standard errors in parentheses) with and without data imputation. Column 1 reports estimates identical to those in Table 2 and Table 4.

B Becker-Style Model

B-1 Model Environment

The model is based on the canonical model of the economics of crime in [Becker \(1968\)](#) and follows closely the formulation in [Burlando & Motta \(2016\)](#). Society is comprised of a unit mass of individuals indexed by their endowed income y and taste for crime x . I assume that income is exogenous, and to start, homogenous in the population. Taste for crime x is distributed according to the cumulative distribution function $G(\cdot)$. Individuals have strictly concave utility over consumption $u(c)$ and receive utility x from (successfully) committing crime.

Each criminal act causes harm to society. Hence, the government tries to curb crime through an enforcement scheme $\theta = (p, f)$, where p represents the probability a citizen is audited and f denotes the fine paid by an individual found to be engaging in crime. Taking the enforcement scheme as given, individuals choose whether to engage in crime to maximize expected utility. Hence, individuals choose crime if

$$\underbrace{pu(y - f) + (1 - p)[u(y) + x]}_{\text{expected utility for criminals}} > \underbrace{u(y)}_{\text{utility for abstainers}} \quad (\text{B.1})$$

Equation [B.1](#) determines a threshold value of x as a function of y and θ :

$$x^*(y, p, f) = \frac{p}{1 - p} [u(y) - u(y - f)] \quad (\text{B.2})$$

Individuals with $x > x^*$ engage in crime, while those with $x \leq x^*$ abstain. Given y and θ , the amount of crime is $1 - G(x^*(y, \theta))$. One can think of this expression as a demand curve, mapping the (expected) price of crime to the quantity of offenses.

It is also useful to note that given y and θ , total welfare of citizens can be expressed as

$$V(y, \theta) = \int_0^{x^*} u(y)g(x)dx + \int_{x^*}^{\infty} \left\{ pu(y - f) + (1 - p)[u(y) + x] \right\} g(x)dx \quad (\text{B.3})$$

which is the utility of abstainers and criminals integrated over the distribution of x .

B-2 Enforcement and Welfare

Before turning to policy discussion, it is useful to note that policy analysis in this Becker-style model will require an understanding of the relationship between welfare V and the enforcement scheme θ . In particular, one needs to differentiate V with respect to the policy parameters p and f . Taking y as given and beginning at the enforcement scheme $\theta_0 = (p_0, f_0)$, consider a small change in one of the policy parameters moving to θ_1 .

With respect to a policy change, there are three distinct types of citizens. First, there is a group of never-takers. Never-takers are individuals who abstain from crime regardless of the enforcement scheme, i.e. individuals with $x \leq x_1$. If the policy change is, for example, an increase in p , then $x_1 = x^*(y, p_0, f_0)$. Second, there is a group or always-takers. Always-takers are citizens who choose crime regardless of the enforcement scheme, i.e. individuals with

$x > x_2$, where $x_2 = x^*(y, p_1, f_0)$ for an increase in p . Finally, there is a group of compliers. Compliers are individuals with $x \in (x_1, x_2]$, and therefore whose behavior is altered by the policy change. For an increase in p , compliers are individuals who choose crime under θ_0 but abstain under θ_1 . Hence, the welfare change associated with a small policy change can be expressed as

$$\int_0^{x_1} \left[\frac{\partial u}{\partial \theta} \Big|_{x \leq x_1} \right] g(x) dx + \int_{x_1}^{x_2} \left[\frac{\partial u}{\partial \theta} \Big|_{x \in (x_1, x_2]} \right] g(x) dx + \int_{x_2}^{\infty} \left[\frac{\partial u}{\partial \theta} \Big|_{x > x_2} \right] g(x) dx \quad (\text{B.4})$$

The first term is the change in utility for the never-takers. Because such individuals abstain regardless, they receive $u(y)$ under either θ . There is no welfare change for never-takers, meaning the first term is zero.

The second term is the welfare change for the compliers. Such individuals were marginal to abstaining under θ_0 and choose to abstain under θ_1 . By the envelope theorem, there is no welfare change for compliers. The second term is zero.

The third term is the welfare change for the always-takers. Policy parameters do impact the expected payoff associated with crime, thereby affecting the expected utility of the inframarginal criminals. Hence, given that the first two terms are zero, the only welfare impacts of a small change in enforcement are the effects on inframarginal criminals:

$$\frac{\partial V}{\partial \theta} = \int_{x_2}^{\infty} \frac{\partial}{\partial \theta} \left\{ pu(y - f) + (1 - p)[u(y) + x] \right\} g(x) dx \quad (\text{B.5})$$

The following discussion below makes use of this result.

B-3 Optimal Enforcement

The government chooses an enforcement scheme to maximize the welfare of citizens, net of the social costs of crime and the costs of enforcement. For simplicity, assume the government takes the fine f as given and chooses only p . This assumption captures the fact that, in many cases, fines are set at the state or county-level but policing intensity is chosen locally.²⁶

To begin with a reduced-form version of the planner's problem, let $h(p)$ represent the social cost of crime as a function of p and let $c(p)$ denote the cost of policing. One could think of this formulation as expressing that only the government cares about crime or that victimization costs are evenly distributed throughout the population. The government's problem is

$$\max_p V(p) - h(p) - c(p) \quad (\text{B.6})$$

Under standard regularity conditions, the solution is characterized by the first-order condi-

²⁶Standard Becker-style models typically assume that increasing the number of searches is costly but increasing the charged fine is not, which leads to the prediction of much higher fines than are generally observed in cases such as traffic enforcement. Assuming the government takes the fine as given is isomorphic to assuming there is maximum acceptable fine amount \bar{f} , reflecting fairness concerns for example, because optimization will always dictate $f = \bar{f}$.

tion

$$\underbrace{-h'(p)}_{\text{marginal safety benefit}} = \underbrace{c'(p)}_{\text{marginal cost of policing}} - \underbrace{V'(p)}_{\text{marginal welfare loss}} \quad (\text{B.7})$$

In words, the government tickets until the marginal safety benefit equals the marginal cost of writing tickets and the marginal lost surplus to citizens. It is worth noting that if the government also faces a revenue-raising motive when issuing citations, this would enter the first-order condition as a constant on the left-hand side of B.7. With a revenue benefit, the government is willing to allow a larger welfare loss to citizens when optimizing.

Using B.5, the marginal welfare loss associated with increasing p , $V'(p)$ is

$$\frac{\partial V}{\partial p} = \int_{x^*}^{\infty} [u(y-f) - u(y) - x]g(x)dx \quad (\text{B.8})$$

This expression depends on the utility losses associated with punishment and the benefits to criminal behavior. To obtain a more tractable expression, one can think of a small increase in p as writing one more traffic ticket. Moreover, assume that the marginal person ticketed was close to the margin of criminal behavior. Hence, we can substitute the indifference x^* condition into the derivative of the expected utility of criminals to get the marginal welfare loss associated with one more ticket is

$$\frac{1}{1-p} [u(y-f) - u(y)] \quad (\text{B.9})$$

Assuming p is small, then, optimal enforcement sets

$$u(y) - u(y-f) = -h'(p) - c'(p) \quad (\text{B.10})$$

We can think of the left-hand side of B.10 as a reduced-form expression of the quantity estimated in the data, the welfare cost of punishing an individual.

B-4 Income-Based Fines

Now suppose that society is comprised of types of individuals, those with high incomes y_H and those with low incomes y_L , where $y_H > y_L$. Assume taste for crime x is distributed identically across the two types of individuals. I examine the effect of moving from an initial enforcement scheme $\theta_0 = (p_0, f_0)$ to a small perturbation in the fines for the two types. Specifically, I consider an increase in the fine for rich individuals to $f_H = f_0 + \Delta$ and a decrease in the fine for rich individuals to $f_L = f_0 - \Delta$, where $\Delta > 0$.

To simplify the exposition, let Δ satisfy the following condition:

$$x^*(y_H, p, f_0 + \Delta) = x^*(y_L, p, f_0) \quad (\text{B.11})$$

The relevance of this assumption is as follows.²⁷ Recall from section B-2 that, for small Δ ,

²⁷To see that such a Δ exists, note that by the definition x^* , Δ solves $u(y_L) - u(y_L - f_0) = u(y_H) - u(y_H - f_0 - \Delta)$. The properties of $u(\cdot)$ dictate that $u(y_L) - u(y_L - f_0) > u(y_H) - u(y_H - f_0)$ and that $u(y_H) - u(y_H - f_0 - \Delta)$ is increasing in Δ .

we need only consider the utility implications for the always-takers when evaluating welfare effects. When moving from f_0 to $f_0 + \Delta$, the always-takers among the rich are those with $x > x^*(y_H, p, f_0 + \Delta)$, or those who engage in crime when f is either f_0 or $f + \Delta$. When moving from f_0 to $f_0 - \Delta$, the always-takers among the poor are those with $x > x^*(y_L, p, f_0)$. Hence, [B.11](#) ensures that the distribution of x 's among the rich and poor always-takers are identical, allowing for a simpler expression of welfare effect that abstracts from compositional changes.

Equation [B.5](#) shows that the derivative of the expected utility of criminals with respect to the relevant enforcement parameter is a key object in evaluating welfare effects. With respect to fine changes, this quantity is

$$\frac{\partial}{\partial f} \{pu(y - f) + (1 - p)[u(y) + x]\} = -p \frac{\partial u}{\partial c}(y - f) < 0 \quad (\text{B.12})$$

Substituting [B.12](#) into [B.5](#) gives the following expression for the net welfare change associated with the change in the fine scheme:

$$\int_{x^*(y_L, f_0, p)}^{\infty} \Delta p \frac{\partial u}{\partial c}(y_L - f_0) g(x) dx + \int_{x^*(y_H, f_0 + \Delta, p)}^{\infty} -\Delta p \frac{\partial u}{\partial c}(y_H - f_0) g(x) dx \quad (\text{B.13})$$

where the first term is the welfare change among poor always-takers and the second term is the welfare change among rich always-takers. Using assumption [B.11](#), which ensures that the limits of integration are equal, this expression can be rewritten as

$$\Delta \times \underbrace{\left[\frac{\partial u}{\partial c}(y_L - f_0) - \frac{\partial u}{\partial c}(y_H - f_0) \right]}_{\text{difference in marginal utilities}} \times \underbrace{p[1 - G(x^*)]}_{\text{number of tickets}} \quad (\text{B.14})$$

The first and last components are positive by assumption and definition. Strict concavity of $u(\cdot)$ ensures that the difference in marginal utilities is positive, and therefore, that the welfare change is positive.

To obtain a money metric for the welfare changes, I rescale by marginal utility at the low income level ([Chetty, 2006a](#)).²⁸ For a CRRA utility function with risk aversion γ , the money-metric welfare change is

$$\frac{(y_L - f_0)^{-\gamma} - (y_H - f_0)^{-\gamma}}{y_L^{-\gamma}} \times \Delta \times p[1 - G(x^*)] \quad (\text{B.15})$$

To relate this expression to the paper's empirical exercise, let $y_L = \$20,000$, $y_H = \$40,000$, and $f_0 = \$200$. One of the main insights offered by the empirical analysis is the fact that fines have outsized effects on the utility of poor drivers. To incorporate this finding into the welfare analysis in a reduced-form way, let e capture the *excess burden* of fines on poor drivers. We can think of this quantity as the "effective" fine size, corresponding to the welfare cost estimates in Section 6. Taking this heterogeneity into account, the change-in-welfare

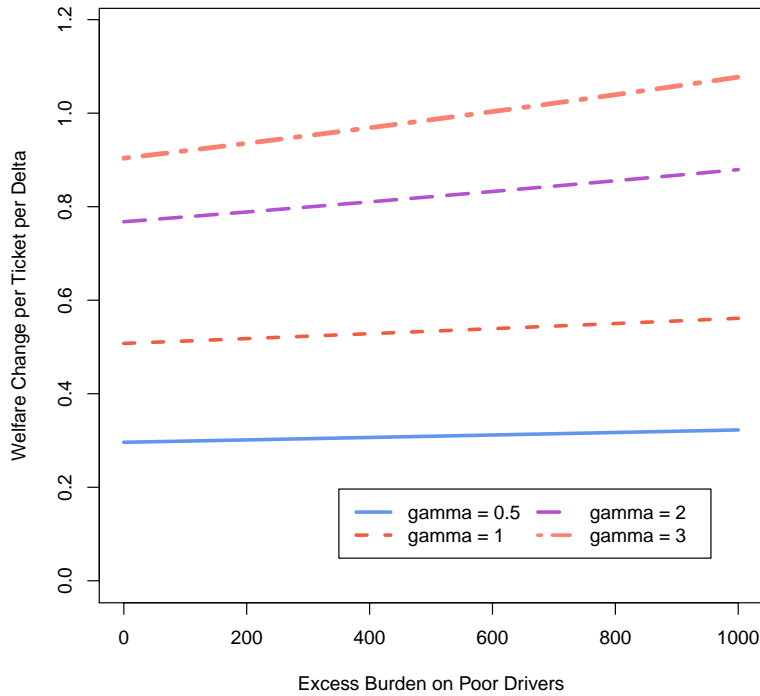
²⁸The unit of the welfare change is change in utils. Multiplying by one over the marginal utility scales by the price of a util, i.e. converts the change in utility into dollar units.

expression becomes

$$\frac{(y_L - f_0 - e)^{-\gamma} - (y_H - f_0)^{-\gamma}}{y_L^{-\gamma}} \times \Delta \times p[1 - G(x^*)] \quad (\text{B.16})$$

Figure B-1 plots the first term of B.16, which is the per-dollar (of fine change), per-citation change in utility, as a function of the excess burden e and for different values of risk aversion. Unsurprisingly, welfare effects depend heavily on γ , which governs the curvature of the utility function. For low-levels of risk aversion and without excess burden on poor drivers, the welfare benefit of a \$10 fine perturbation is about \$3 per citation. For $\gamma = 1$, benefits are between \$5 and \$5.60 depending on the excess burden. At higher levels of risk aversion, both baseline benefits and the dependence of benefits on the excess burden increase considerably. When $\gamma = 3$ and $e = \$1000$, per ticket welfare effects of a \$10 fine perturbation are about \$10.10. At current ticketing rates, the total utility benefit associated with such a policy is between \$6 and \$21 million.

Figure B-1: Welfare Effects by Risk Aversion and Excess Burden



Notes: The figure plots the money metric per-dollar per-ticket welfare change (first term of B.16), i.e. the per-ticket welfare increase from a \$1 fine perturbation, as a function of the excess welfare burden of fines on poor drivers, e , for different values of risk aversion γ .

Impacts on Crime

Of course, the net social welfare implications of the policy change also depends on the policy's effects on crime and/or revenue from fines. Note that for a given y and enforcement regime θ , the amount of crime is $C = 1 - G(x^*(y, \theta))$. Hence, crime changes with f according to

$$\frac{\partial C}{\partial f} = -g(x^*) \times \frac{p}{1-p} \times \frac{\partial u}{\partial c}(y - f) \quad (\text{B.17})$$

where the expression beginning with $\frac{p}{1-p}$ follows from differentiating x^* with respect to f .

The income-based fine regime increases (decreases) the price of crime for the rich (poor), thus decreasing crime among rich individuals but increasing crime among poor individuals. The net effect of the policy on crime can be expressed as

$$\Delta \frac{p}{1-p} \left[g(x^*(y_L, p, f_0)) \frac{\partial u}{\partial c}(y_L - f_0) - g(x^*(y_H, p, f_0)) \frac{\partial u}{\partial c}(y_H - f_0) \right] \quad (\text{B.18})$$

The first term inside the brackets represents the increase in crime for the poor and the second term represents the decline for the rich. While concavity of $u(\cdot)$ ensures that $u'(y_L - f_0) > u'(y_H - f_0)$ and $x_0^*(y_L) > x_0^*(y_H)$, the sign of [B.18](#) depends on the functional form of $g(\cdot)$, or more specifically the shape of the distribution of crime tastes in the range of the cutoff values. If x has a strictly decreasing probability distribution function (an exponential distribution, for example), the policy increases crime. If $g(\cdot)$ is increasing in the range of the initial x^* values, the policy could reduce crime.

An important point to note is that the above analysis of welfare changes relies on a specific magnitude of Δ to simplify the exposition. However, one could also have chosen an alternate fine scheme specifically to hold crime constant. The redistributive welfare benefits would still be present under such an alternative policy, but one would also need to consider changes in the composition of criminals and the associated welfare implications.

C Effects of Payroll-Job Separations

In this section, I estimate the impact of a separation from a payroll-covered job on credit report outcomes. This exercise serves two distinct purposes. First, it provides a test of the hypothesis that payroll employment is a meaningful and positive outcome. Second, to the extent that a separation impacts credit report outcomes, the estimates can be used as a benchmark to help interpret the magnitudes of the estimated traffic ticket effects.

C-1 Sample Construction

To isolate the impacts of separations unrelated to traffic citations, I sample from the set of individuals who receive their first traffic ticket after January 2014 and analyze data from January 2010 through 2013. I drop individuals included in the matched difference-in-differences sample, require that individuals are present in the credit file in January 2010, and require that individuals are between 18 and 60 years of age as of that date.

I then identify individuals with a separation from the payroll data during in 2011 or 2012, measured as a transition from having at least one covered job to having zero covered jobs in adjacent months. Requiring that the separation occurs in the 2011-2012 period allows a balanced 12-month period before and after the separation for analysis and allows for the computation of a crude tenure measure. That is, using the one-year pre-period, I can at least distinguish between spells of, e.g., three months and spells of longer than twelve months. There are 26,718 individuals meeting all the above requirements. To help estimate time and age effects, I include individuals meeting the same criteria but whose payroll employment spells begin after 2013 as a quasi-control group. There are 38,345 such individuals.

Table C-1 presents summary statistics for the separations sample. The treatment (separations) and control groups are quite similar on most dimensions. Compared with the event study and matched difference-in-differences samples, this group of drivers is a higher fraction female and slightly younger, but otherwise similar on most dimensions.

C-2 Estimation

To estimate the impacts of separations, I use an event-study approach. Specifically, I estimate regressions of the following form:

$$Y_{it\tau} = \sum_{\tau} \theta_{\tau} + \phi_i + \kappa_t + \gamma_i(t) + \epsilon_{it} \quad (\text{C.1})$$

Here, the θ_{τ} 's are month around separation indicators and ϕ_i and κ_t are individual and time fixed effects. I group event-time values larger than +/-13 into +/-13. I also include individual-specific linear trends $\gamma_i(t)$ in the regressions. Finally, I control for a quartic in driver age and include a set of job tenure indicators, which are indicators for number of months since the payroll employment spell began, topcoded at twelve because this is the longest look-back period allowed for the universe of separations. Event-time and tenure indicators are set to zero for the control group. I cluster standard errors at the individual-level.

C-3 Results

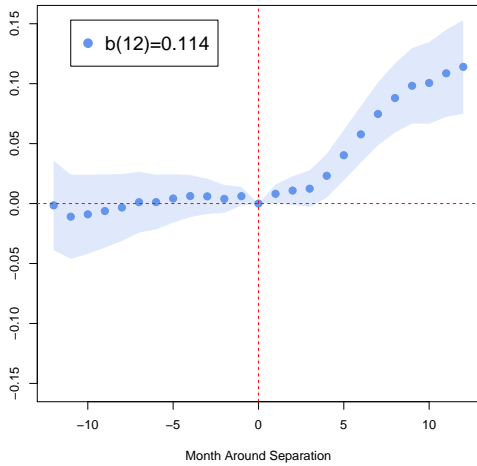
Table C-1: Summary Statistics for Job Separations Sample

	(1)	(2)
	Separations	Control
<i>Panel A: Demographics</i>		
Female	0.51	0.52
Nonwhite	0.47	0.49
Age	34.7	35.15
Credit File Age	11.85	12.13
Credit Score	600	599
Estimated Income	28746	28972
<i>Panel B: Financial Strain</i>		
Collections	3.01	3.03
Collections Balance	1991	2010
Derogatory Accounts	1.59	1.56
Delinquent Accounts	0.53	0.55
Past Due Balance	2793	2881
Prior Bankruptcy	0.02	0.02
<i>Panel C: Credit Usage</i>		
Any Account	0.78	0.77
Revolving Accounts	2.63	2.63
Revolving Balance	5302	5376
Any Auto Loan	0.32	0.32
Any Mortgage	0.23	0.24
Individuals	26718	38345

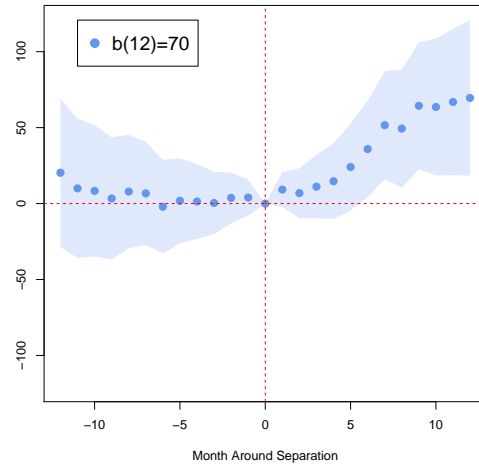
Notes: The table reports summary statistics for the event-study analysis of payroll job separations. Column 1 reports means as of January 2010 for individuals with a separation in 2011-2012 and column 2 reports means as of January 2010 for control individuals (those with a spell in the payroll data after January 2014). See notes to Table 1 for further details.

Figure C-1: Effect of Payroll Separations on Financial Strain

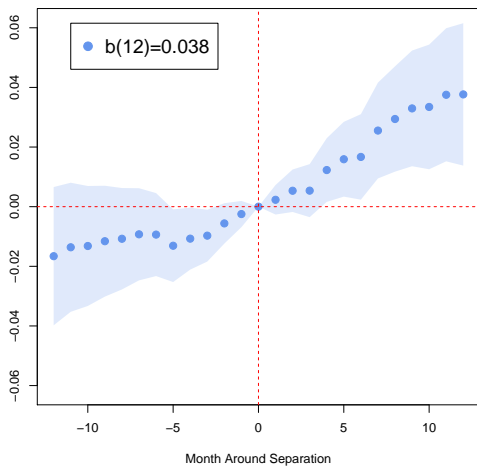
(a) Collections



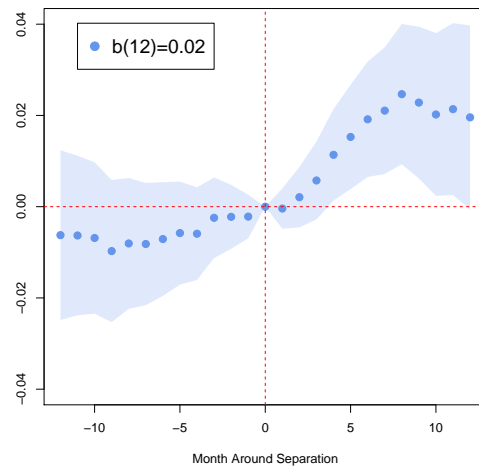
(b) Collections Balance



(c) Derogatories



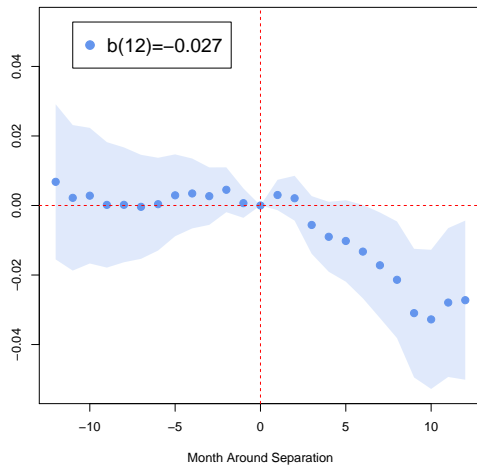
(d) Delinquencies



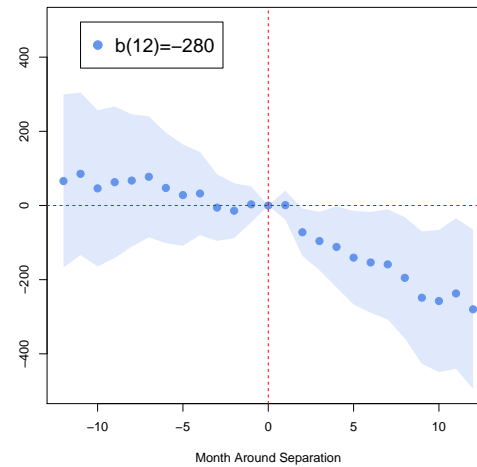
Notes: Each figure plots coefficients and 95% confidence intervals on month around payroll separation indicators. Regressions also include individual and time fixed effects, payroll tenure fixed effects, a quartic in age, and individual-specific linear trends. Standard errors are clustered at the individual level. The average separation corresponds to a \$1,600 decline in monthly payroll earnings. Legend reports the 12-month estimate.

Figure C-2: Effect of Payroll Separations on Credit Cards

(a) Revolving Accounts



(b) Revolving Balance



Notes: Each figure plots coefficients and 95% confidence intervals on month around payroll separation indicators. Regressions also include individual and time fixed effects, payroll tenure fixed effects, a quartic in age, and individual-specific linear trends. Standard errors are clustered at the individual level. The average separation corresponds to a \$1,600 decline in monthly payroll earnings. Legend reports the 12-month estimate.