Fines and Financial Wellbeing[*]

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Abstract

While survey evidence suggests the presence of widespread financial fragility in the United States, causal evidence on the implications of unanticipated, transitory income shocks for low-income households is scarce. I study the impact of fines for traffic violations on financial health using administrative data on traffic citations in Florida linked to high-frequency credit reports for cited drivers. Leveraging variation in the timing of traffic stops with difference-in-differences and event-study research designs, I find that fines are associated with increases in several measures of financial distress. Effects are concentrated among drivers with below-median incomes, for whom the probability of a new default increases by almost two percentage points (eight percent) and unpaid bills in collections increase by about $75 in the three to six quarters following a traffic stop. Relying on employment information from large employers covering about 15 percent of the sample, I also find evidence that fines induce employment instability for low-income individuals. Interpreted from the perspective of an accounting exercise, the results suggest that a typical low-income driver in the sample can cover no more than 40 percent of an unexpected $200 expense using savings or cash-on-hand.

JEL Codes: G51, I32, K42, H72

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 2006). Despite the prediction of canonical models that liquidity-constrained households anticipate income volatility by accumulating buffer stock savings (Deaton 1991, Carroll et al. 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 a 2017 survey (Board of Governors of the Federal Reserve System, 2018). Further, data from the consumer expenditure survey suggest that a typical household earning $20,000 per year has only $200 remaining each month after spending on essentials.[1]

While ethnographic studies (e.g., Shipler 2004, Desmond 2016) provide vivid accounts of disadvantaged individuals whose fortunes are altered by unplanned expenses, causal evidence on the impacts of typical 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 2017). Moreoever, the literature's reliance on policy variation generated by tax rebates or mortgage programs and on data from credit card companies and bankruptcy filings 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. This setting has several important advantages. First, traffic fines represent a common form of unplanned, everyday expense that can be observed and measured in data. Over forty million citations are issued annually for speed limit violations alone and standard fines are well within the range of typical monthly income fluctuations (Morduch & Schneider, 2016). Second, as shown in figure 1, policing activity disproportionately affects poor communities, allowing for the study of a large sample of low-income individuals. Third, driver license suspensions imposed for nonpayment incentivize high payment rates on average, increasing confidence that a traffic ticket represents a true expense shock.

Moreover, the relationship between low-level policing and financial wellbeing is independently interesting given current public debates over the unintended consequences of criminal justice policies in disadvantaged communities (e.g., Ang 2021). While a large literature has examined the public safety benefits of policing (e.g., Chalfin & McCrary 2017) in the spirit

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[1]See Goldstein and Vo, NPR Planet Money, 8/12/2012.

of deterrence models (Becker, 1968), the social costs of policing have historically received less attention. Prompted by the *Fergusion 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 numerous accounts of individuals suffering from cycles of debt and criminal justice involvement stemming from fine and fees. While compelling, such evidence is both anecdotal and correlational.

To estimate the impact of fines, I link administrative data on the universe of traffic citations issued in Florida over 2011-2015 to quarterly credit reports for cited drivers. The citations data provide near-complete coverage of the state's traffic offenders and my main analysis sample represents about 3.5 percent of Florida's driving-age population. Credit reports offer a detailed account of an individual's financial situation and include information on unpaid bills, borrowing account delinquencies, and other adverse financial events. Unpaid bills in collections represent a particularly useful outcome in my setting as they capture default on obligations such as medical and utility bills (Avery et al., 2003) and thus can provide a measure of financial distress even for the lowest-income drivers, many of whom have no active formal borrowing accounts.

Taking advantage of the high-frequency nature of the credit report data, I leverage variation in the timing of traffic stops for identification. My primary empirical approach is a matched difference-in-diffferences design that compares the evolution of outcomes for drivers around the time of a traffic stop with the simultaneous evolution for a matched control group of comparable individuals who receive citations two to four years later. Results are similar in a conventional event-study design using a larger sample of drivers.

Relative to their matched controls, cited drivers experience increases in several measures of financial distress, including unpaid bills in collections and borrowing account delinquencies, in the six quarters following a traffic stop. Effects are typically two to four times larger for individuals with below-median ($26,000) incomes at baseline. In this low-income sample, the probability of any default measure appearing on a credit report increases by almost two percentage points, or eight percent relative to the baseline mean, following a traffic stop. Fines averaging $190 are associated with a $75 increase in unpaid debt in collections for low-income drivers. Note that collections activity related to the traffic citation itself typically will not appear on a credit report, so the observed increases in collections most likely reflect default on other bills.

Relying on employment information supplied by a specialty credit-reporting agency, I also find some evidence that fines affect employment situations. The employment database covers large employers and around 17 percent of the sample is indicated as employed in

these records at baseline. Six quarters out from a traffic stop, low-income individuals are 1.5 percentage points (or about nine percent) less likely to be working in a job covered by the employment data. Several pieces of evidence suggest that these jobs are better than average, and I conservatively interepret the negative effect on the likelihood of appearing in the employment data as suggesting (i) increased likelihood of job transitions and (ii) a modest worsening in employment arrangements on average.

Examining hetereogeneity on dimensions other than income, I find that the largest impacts on financial distress accrue in the subsample of individuals with a history of financial instability. More financial distress at baseline predicts a higher treatment effect of a traffic fine on future financial distress. Low-income drivers with easy access to liquidity on credit cards appear to use their credit cards to cover a share of their traffic fines and suffer more modest increases in financial distress outcomes.

A natural concern is that the observed effects are explained by other, non-fine punishments associated with a traffic citation. Individuals opting not to pay their fines receive an automatic license suspension and most infractions result in "points" accruing on a driver's record. Points can cause small increases in car insurance premiums or trigger license suspensions for drivers with substantial offending histories. However, the available evidence suggest that simply paying the fine explains the vast majority of the estimated effects. Impacts are, if anything, larger for a sample of low-income drivers who pay their fines and impacts are quantitatively comparable, though less precise, for a subsample of individuals who can be identified as facing no punishment other than the fine itself.

I supplement the primary estimates with an empirical approach that isolates the impact of paying a harsher fine. This difference-in-differences approach compares individuals paying their fines after receiving speeding tickets for 9 miles per hour (MPH) and 10-14 MPH over the posted speed limit. Those ticketed at the higher speed pay a $75 higher fine but receive the same number of driver license points. The speeder design circumvents many of the identification challenges associated with the timing-only designs because it allows for comparisons of individuals who simultaneously commit very similar offenses. The primary disadvantage is a loss of precision due to a significantly reduced sample size. These results show that, for low-income drivers, a $75 fine increase is associated with a $30 increase in collections balances. As a fraction of the fine (36 percent), the effect is very simimlar to the matched difference-in-differences estimate (40 percent).

To interpret magnitudes, I use an accounting exercise that asks how drivers pay their fines. To cover a surprise $190 bill, individuals can tap into cash-on-hand or savings, borrow through formal or informal channels, reduce consumption, or miss other bill payments. For low-income drivers, the treatment effect on collections balances implies that at least $76

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is "borrowed" from other bills and an estimated effect on card usage suggests that $23 additional dollars are drawn from credit cards. Estimated impacts on distress, combined with a finding from Pattison (2020), suggest that $19 is covered by reducing consumption. Hence, the accounting exercise implies that a typical low-income individual covers less than forty percent of a $190 fine using cash-on-hand or savings. Moreover, some share of the remaining 40 is likely covered through informal borrowing or alternative subprime borrowing not measured in the credit report data. Those with above-median incomes are estimated to cover 81 percent of the fine through cash-on-hand or savings.

Viewed through the lens of this accounting exercise, the results appear to confirm the survey evidence highlighting the prevalence of financial fragility, or an inability to easily cope with unplanned negative shocks, amoung households in the United States. The fact that many individuals are meaningfully affected by small financial shocks highlights that policies providing insurance against income risk could have meaningful welfare effects.

The bulk of this paper is dedicated to empirically documenting the impact of fines and interpreting the results in a general framework that provides useful insights about the financial situations of low-income households. However, motivated by the specific context of my analysis, I also use a Becker (1968) deterrence-model framework to discuss the implications of my findings for criminal justice policy. In the model, the state trades off marginal benefits in the form of safety and revenue from fines against marginal costs in terms of labor and lost surplus to offenders when choosing a policing level. The inability of many drivers to cover fines with savings suggests that the utility cost of citing an additional driver exceeds revenue raised. Hence, one takeaway from the model is that marginal tickets with low deterrence benefts may be inefficient after taking into account the welfare costs to offenders.

Motivated by the heterogenous treatment effects by income, I also use the model to conceptually explore the implications of moving to an income-based fine scheme in a simple setup with two income groups. Starting from a flat fine schedule, the welfare gain from a small, progressive perturbation to the fine schedule is proportional to the difference in marginal utilities when fined for low- and high-income drivers. Making quantitative statements about this difference is beyond the scope of this paper, but the sizeable gap in the treatment effects of fines on financial wellbeing for low- and high-income drivers suggest that welfare gains from income-based fines could be meaningful.

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 discuss interpretation and policy implications in section 6 and conclude in section 7.

2   Setting and background

The context for this paper 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 citations are very common. Over 4.5 million individual Florida drivers received at least one traffic ticket between 2011 and 2015, with between 1.1 and 1.4 million licensed Floridians cited each year. As of the 2010 census, the population of Florida aged 18 or over 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 are ticketed each year.

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

Traffic citations specify an offense and fine to be paid. For reference, the most commmon single violation codes over 2011-2015 were speeding (28 percent), red light camera violations (7 percent), lacking insurance (7 percent), driver not seat-belted (7 percent), and careless driving (5 percent), which account for just over half of all citations over the period. Statutory fines vary widely across offense types, and to a lesser extent, across counties. For example, low-level equipment violations such as broken tail lights carry a fine of $110, while the fine for speeding 30+ miles per hour above the posted limit in a construction or school zone is $620. Punishments for certain criminal, rather than civil, traffic offenses can exceed $1,000 and may include jail time. As detailed in section 4, I focus my analysis on standard moving violations, with fines ranging from $123 to $273 ($\mu \approx \$190$).[2]

Many infractions also result in points on a driver's license. Insurance companies typically consider license points as a signal of driver risk when setting premiums, so individuals may face increases in car insurance costs following a citation. A rough, back of the envelope, calculation suggests that the typical speeding ticket could increase monthly car insurance premiums by about $10.[3] State law dictates that drivers accruing 12 points in 12 months (18 points in 18 months; 24 points in 36 months) have their licenses suspended for 30 days (6 months; one year). Typical moving violations are associated with three or four points, with the most severe offenses carrying six points. Points are generally not assessed for non-moving violations. A subset of violations also trigger an automatic suspension of driving privileges.

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[2]Table A-2 shows the distribution of offenses in the moving violations sample. Over 70 percent are speeding offenses. The remainder is made up primarily of failure to stop, yield, etc., offenses.

[3]See, e.g., Gorzelany in Forbes, 5/17/2012.

Paperwork offenders, such as those found lacking proof of auto insurance or an up-to-date registration, typically face an immediate DL suspension. Such individuals must visit the county clerk with the relevant paperwork (e.g., proof of insurance coverage) to regain their driving privileges.

Once a citation has been issued, a driver can either submit payment to the county clerk or request a court date to contest the ticket. For those contesting their ticket in court, a judge or hearing officer decides to either uphold the original charge, reduce the punishment, or dismiss the citation. A court fee averaging about $75 is required of those bringing their case to court. For individuals who do not request a court date, payment is due thirty 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, wipes the the citation from the driver's record, preventing the accrual of the associated license points on the individual's DL.[4] If the county clerk has not received payment 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 misdemeanor offense and typically results in a fine exceeding $300 and the possibility of jail time. Figure A-1 succintly illustrates the driver's potential decision tree and the corresponding outcomes for a standard moving violation.

If a citation remains unpaid after 90 days, the county clerk adds a late fee to the original amount owed and send the debt to a collections agency, who then solicits payment. Collections agencies are authorized by state law to add a 40 percent collection fee to the original debt. To the best my knowledge, collections activity originating wth unpaid ctations *will not appear on a driver's credit record.*[5]

An important takeaway from an examination of the institutional details is that the exact *treatment* offenders face can take many forms. Even holding the offense constant, the exact treatment for a given individual depends on ex-post decisions, and to a lesser extent driving

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[4]Individuals 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, costs $25 (but typically coupled with a $15 fine reduction), and is available online. Individuals can only complete traffic school once in any twelve-month period and five times total. About 20 percent of individuals in the subset of the main sample with valid traffic court disposition information participate in traffic school.

[5]Not all collections agencies report their activity to credit bureaus and reporting 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. While most signalled an ability to report to credit bureaus on their webpage, the two agencies repsondng to my inquiry indicated that they do not report traffic-citation-related collections.

history, neither of which are perfectly observed in the data. For reasons discussed further in section 4, I focus my analysis on moving violations not associated with mandatory court appearances or automatic license suspensions and primarily think of the treatment as the receipt of a bill for $190 (on average), where the punishment for nonpayment is a revocation of driving privileges. Treatment could entail time in court and court fees for ticket contesters and increases in insurance premiums for payers. I primarily estimate reduced-form, or intent-to-treat, effects, but rely on heterogeneity analyses, as well as a companion identification strategy (described in section 4.4), to study the importance of various treatment channels.

According to the Florida Clerks and Comptrollers (FCC), the threat of a license suspension provides a strong incentive for payment. They estimate that traffic fines are paid on-time in 90 percent of cases. Using data on traffic court dispositions available for about 80 percent of the citations in the main sample, I estimate a payment rate of at least 63 percent, and possibly as high as 94 percent depending on underlying assumptions, highlighting that the treatment is primarily a fine payment in the lion's share of cases.[6]

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 drawn from the FCC's Uniform Traffic Citation (UTC) database, which preserves an electronic record of each ticket, transcribed from the paper citation written by the ticketing officer. Each record includes the date and county of citation, as well as the violation code and information listed on the offender's driver license, such as driver license number, name, date of birth, and address.

3.2 Credit reports

Access to monthly credit reports from January 2010 through December 2017 was provided by one of the major credit bureaus.[7] 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

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[6]As discussed in the appendix E, estimating payment rates is complicated by missing and non-sensical traffic court disposition data for about 20 percent of the sample. Of those with a valid disposition, 63 percent indicate a paid citation or traffic court election (which requires accompanying payment). Another 20 percent have *witheld* dispositions, which could indicate a variety of scenarios in terms of payment and point status.

[7]My data-sharing agreement precludes me from disclosing the name of the credit bureau.

proprietary fuzzy linking algorithm, the driver information was matched with the credit file using name, data of birth, and home address on the citation. Worth noting is the fact that the credit bureau database includes previous addresses for individuals on file. Hence, the address at the time of traffic ticket need not be current to achieve a match.

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 the vast majority of cases, names and addresses were written by hand, undoubtedly leading to transcription errors. Hence, 82 percent is a reasonable match rate. The effective match rate is somewhat lower. About 3.4 million drivers appear on-file prior to any traffic stop and thus can be used for analysis. I typically require individuals to have a credit report as of January 2010, the first month of the credit bureau data, leaving about 2.7 million matched drivers.

Consistent with Brevoort et al. (2015), figure A-2 documents a strong, positive relationship between a driver's neighbohood income and the likelihood of a successful credit file match. Table A-2 examines other predictors of a match by taking all citations passed to the credit bureau and regressing a match indicator on driver characteristics available in the citations data. The regressions confirm a strong relationship between neighborhood income and a successful match, but also highlight differences across demographic groups. Female, white, and older drivers are more likely to be matched. Overall, the matching process appears to slightly erode the negative selection into the citations data. Individuals receiving traffic citations are less advantaged than average, as shown in figure 1, but among cited individuals, those matched to the credit file are positively selected. To the extent that treatment effects are larger for the most disadvantaged individuals, the selection induced by the credit file matching process ought to bias estimates towards zero.

After matching the data, the credit bureau removed the citations data of all personally identifiable information such as driver names, driver license numbers, addresses, birth dates, 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.[8] The data also include an estimated income measure, which is based on a proprietary model that predicts an individual's income,

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[8]The provided credit score is the VantageScore® 3.0.

rounded to the nearest thousand, using information in the credit file. I use the estimated income measure at baseline to split the sample into income groups.

I aggregate the credit report data from the individual × month level up to the individual × quarter level prior to the empirical analysis. The aggregation both reduces the prevalence of missing values and makes the dimensionality of the panel datasets more computationally manageable. For additional details, see appendix E.

3.3   Employment Information

The credit report provider also maintains a database of employment information covering a subset of employers. The employment data are quite thin, but include information on the number of jobs and annualized earnings for individuals in a given month. In terms of coverage, employers represented in the employment database are mostly larger businesses. Coverage appears more sparse in the citations sample than for the nation as a whole. The share of cited drivers appearing in the employment data hovers around 17 percent, while the credit bureau reports that around 30 percent of all individuals on the credit file are covered in the employment database.

In Appendix C, I estimate event studies of credit report outcomes and find that financial strain measures increase after an exit from the employment data. This excercise has two important benefits. First, increases in financial distress following a separation confirm that a spell in a job covered by the employment database represents both a "real" and "good" outcome on average. This makes sense given the existing evidence that large employers tend to pay higher wages and provide more generous benefits (e.g., Brown & Medoff 1989, Cardiff-Hicks et al. 2015). Second, these event studies provide a useful benchmark for the time path of treatment effects on credit report outcomes. The full impact of an employment data exit on unpaid bills accrues over four to six quarters.

3.4   Outcomes and interpretation

Credit report data provide a wealth of information on an individual's financial situation. One challenge in working with these data is to focus on a parsimonious set of outcomes with a relatively clear welfare interpretation. I focus my analysis on measures of financial strain (Dobbie et al., 2017), relying mainly on indications of default on financial obligations.

As a primary outcome, I examine collections activity on credit reports, which represent unpaid bills that have been sent out to third-party collections agencies who attempt to recover payment. As mentioned in section 2, to the best of my knowledge, unpaid traffic fines do not appear as collections on credit reports because collections agencies handling such debt

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do not report to credit bureaus. Collections and collections balances are an especially useful measure of financial distress in the current 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 third-party collections originate with credit accounts, with the majority associated with medical and utility bills. About 30 percent of individuals with below-median incomes have no active credit account at the time of their traffic stop. Unpaid bills in 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.

I also study borrowing account-based measures of default. I use the number of accounts ever at least 90 days past due as my primary measure of delinquency. Accounts are considered derogatory if a repossession, charge-off (where a creditor declares a debt unlikely to be repaid), foreclosure, bankrupcty, or internal collection has occurred. The credit bureau computes the number of accounts on file with any derogatory event to date and I use this as an additional measure of financial strain.

These outcomes are *stock* measures, capturing the number of relevant accounts on file at a given time. Collections and delinquencies, for example, typically remain on a credit report for seven years even if the individual has repaid their debt. I also construct a corresponding *flow* measure in the form of an indicator for any new financial distress incident, meaning that the individual accrued a new collection, delinquency, or derogatory on their credit report in a given quarter. For further details on variable construction, see appendix E.

Each distress outcome captures default on a financial obligation, suggesting a financially distressed position but not necessarily having a clear interpretation in terms of welfare. Morduch & Schneider (2016), for example, highlight missing and delaying bill payments as an important consumption-smoothing strategy for cash-strapped households. However, default can be associated with significant costs. Pattison (2020) documents that incidences of financial strain typically coincide with, rather than subsitute for, declines in consumption. Dynamic costs in terms of creditworthiness can also be severe. Collections, as well as delinquent and derogatory accounts, can reduce credit scores by as much as 30 points, with potentially sizable effects on future access to and costs of credit.[9] Liberman (2016) finds that such credit constraints can have significant welfare implications, estimating a typical

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[9]In a life-cycle consumption-savings problem (e.g., Ljungqvist & Sargent 2004), we could think of an individual as trading off consumption today, achieved through missing bills, against facing a tighter borrowing limit or higher interest rate in the future. The present discounted value of future utility flows is (weakly) decreasing in the future borrowing limit and interest rate. In the cross-section, a 15-point credit score decline is associated with a $3,000 reduction in borrowing limits.

willingness to pay of 11 percent of monthly income for a clean credit reputation.

Finally, I study effects of fines on the likelihood that an individual is employed in the employment records (*any job in the employment data*). Due to the low baseline coverage in the employment records (17 percent), this outcome should not be interpreted as signaling employment versus unemployment or nonemployment. Changes in whether an individual shows up as working in the employment records can be interpreted as indicative of job changes, however, and as discussed in section 3.3, the available evidence suggests that jobs covered in the employment database are better than average.

4   Empirical strategy

4.1   Matched difference-in-differences approach

The goal of the empirical analysis is to estimate the reduced form impacts of traffic tickets on financial outcomes. For my primary empirical approach, I use a matched difference-in-differences design that relies on a control group constructed from the sample of drivers matched to the credit file. Citations data linked to the credit file span from 2011 through 2015. I designate drivers receiving their first citation between January 2011 and June 2012 as the treatment group and drivers receiving their first citation after January 2014 as the control group. The six quarters between July 2012 and December 2013 are preserved as a follow-up period where the treated drivers have all received treatment (at least one traffic ticket) and control drivers have not.[10]

The delineation of treatment and control groups is meant to balance the need for variation in the timing of traffic stops within the treatment group, the desire for a longer follow-up period, and the preservation of a sufficiently large set of potential control drivers. Matching occurs as of January 2010, the first month of credit report data. Credit report information from January 2010 through December 2013 is then used in the analysis, guaranteeing that four quarters of data are available before and six quarters of data are available after the treatment group citation. Figure A-3 graphically depicts the timeline.

I use a parsimonious set of characteristics for the match and intentionally avoid matching on outcome variables. Treatment and control drivers are matched on gender, an indicator for white/nonwhite (where Hispanic is considered nonwhite), age, credit file age, commuting

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[10]Due to the sample selection criteria, some treatment drivers will accrue additional traffic stops relative to the control group during the follow-up period. In table B-3, I present estimates adjusted for additional follow-up citations in the treatment group using the recursive TOT method in Cellini et al. (2010), described briefly in section B. Adjusted estimates tend to be about 85 percent as large as those in the baseline specification.

zone of residence, and quartiles of credit score and estimated income. Gender, race, and commuting zone of residence are taken from the citations data and hence are measured at the time of citation, while age, credit file age, credit score, and estimated income are taken from the credit bureau data and are measured in January 2010.

Once all possible matching pairs are 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 as a placebo date, allowing for a comparision 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 the same age at the time of the focal stop for the treatment group. Hence, one could think of the identification strategy as leveraging variation in age at first citation, with treatment drivers first ticketed when slightly younger than control drivers.

The matched design then compares changes in outcomes of interest for treated and control drivers around the time of a treated driver's traffic stop. My primary estimating equation is a dynamic, non-parametric difference-in-differences specificaion:

$$y_{it} = \sum_\tau \left(\alpha_\tau + \theta_\tau \times Treat_i \times \alpha_\tau \right) + \phi_i + \kappa_t + \epsilon_{it} \qquad (1)$$

where τ indexes quarters relative to a citation (event time). Recall that for control drivers, τ is defined using the matched treatment driver's traffic stop date. The α_τ's are event time indicators, and ϕ_i and κ_t are individual and time (year $\times$ quarter) fixed effects. The θ_τ's, which measure treatment-control differences in each quarter relative to a citation, are the coefficients of interest.

Following the standard practice, I truncate the event-quarter indicators at $\tau = -5$ and $\tau = 7$ and and focus on coefficients between $\tau = -4$ and $\tau = 6$, where the sample is fully balanced. I normalize coefficients to $\tau = -1$ and consider $\tau = 0$ to be a partially treated quarter, given that it can include two months of post-citation data. Standard errors are clustered at the individual level.

An important aspect of equation 1 is the nonparametric time path of treatment effect estimates. Both the institutional features of credit reporting and the available empirical evidence would suggest that the impacts of fines on key outcomes will accrue somewhat slowly. A missed bill just a few days after a traffic stop could take several months to be sent out to a collections agency and several additional months for a report of the collections activity to appear on the credit file. For example, Dobkin et al. (2018) show that following

a hospital admission, individuals continue to accrue medical collections for about two years. In appendix C, I show that collections continue to increase for about one year after an individual separates from a job covered in the employment data.

In a few cases where I present results across many subsets of drivers, I estimate a parametric version of (1) to ensure that results are net of any small differences in pretrends between treatment and control groups in a given cut of the data:

$$y_{it} = \gamma_t \times Treat_i + \sum_{\tau=0}^{6} \left(\alpha_\tau + \theta_\tau \times Treat_i \times \alpha_\tau \right) + \phi_i + \kappa_t + \epsilon_{it} \qquad (2)$$

Here, the pre-period ($\tau < 0$) event time indicators and interactions are dropped in favor a linear trend, γ_t, interacted with treatment status. The θ_τ's represent the differential deviation from a linear pre-stop trend for treated drivers relative to the control group.

Identification in the matched difference-in-differences analysis comes from comparing changes around the traffic stop date for treatment drivers, who indeed receive a traffic citation at that date, and control drivers, who receive citations later. The identifying assumption is that treatment drivers would have trended similar to control drivers in the absence of the traffic stop. As in all difference-in-differences applications, the primary threats to this assumption are differing pre-treatment trends between treatment and control groups and unobserved shocks correlated with both treatment status and treatment timing. By plotting the coefficients from estimates of equation 1, I show that treatment and control groups follow nearly identical pre-treatment trends in the outcomes of interest.

There are two important identification concerns that bear mentioning here. First, several types of traffic infractions could signal changes in financial distress *ex ante*. For example, a citation for a broken taillight or expired registration could be induced by a deteriorating financial sitatuation. Therefore, I focus my analysis on moving violations. Figure A-4 plots trends in unpaid bills around the timing of traffic stops for moving and other violations. A strong pre-citation trend in collections is evident for non-moving violations, which are comprised mainly of paperwork and equipment infractions. On the other hand, there is no pre-citation trend when focusing on moving violations, suggesting that the precise timing of these stops is unrelated to changes in an individual's financial situation. An additional benefit of focusing on moving violation is a reduction in the punishment space. All moving violations carry a fine between \$123 and \$273 and three or four driver license points, and no moving violation in my main sample triggers an automatic suspension of driving privileges.

Second, a traffic citation of any type could indicate a change in an individual's driving patterns. There is some evidence to support such a concern in the data. As shown in panel (a) of figure A-5, the likelihoood that an indvidual has an open auto loan on file increases

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by 1-2 percentage points in the four quarters prior to a traffic stop. On one hand, this is an important concern suggesting that an auto purchase, which could be correlated with other changes in an individual's situation, sometimes directly precedes a traffic fine. On the other hand, the presence of an auto loan on file is an imperfect indication that an individual is actively driving. Less than thiry percent of drivers with below-median incomes hold an open auto loan in the quarter of their first traffic stop. Moreover, as shown in panel (b) of figure A-5, an auto purchase tends to coincide with improvements in an individual's financial situation, suggesting that any bias in the matched estimates could be towards zero.

I take several steps to address the concern that results are driven by changes in driving just prior to a traffic stop, explained in more detail in sections 4.4 and 5.2. First, I add an indvidual's auto loan history to the set of matching variables, yielding a difference-in-differences treatment effect estimate identified off comparisons between individuals with identical histories of auto purchases. Second, I estimate simple event studies with a second set of event-time indicators around the quarter of an individual's first auto purchase. These estimates rely on variation in the timing of traffic stops among individuals who purchase cars at the same time for identification. Finally, I rely on a supplemental identification strategy that compares individuals cited simultaneously but facing different fine amounts.

4.2 Construction of matched sample

Sample construction begins with 2,631,641 individuals with traffic stops over 2011-2015 that meet the following conditions: (a) successfully matched to the credit file; (b) present on the credit file in January 2010; (c) aged 18-59 as of January 2010; (d) nonmissing credit score and estimated income as of January 2010.

Candidate treatment drivers face their first observed traffic citation between January 2011 and June 2012 ($N = 1,252,284$). As discussed above, I further trim this list to include only those whose citation is for a moving violation ($N = 575,889$). I also require that the citation is for a single violation ($N = 357,282$). While restrictive, the single-violation requirement is important for the ability to accurately characterize the punishment faced by each treated driver in the sample.[11]

For candidate control drivers, I use all drivers with a first citation after January 2014

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[11]Citations for multiple violations are sometimes entered as separate citations in the database and sometimes only noted with a companion citation flag. In the latter case, the details of the companion citation cannot be inferred. Because exact dates of citations were required to be dropped prior to merging with the credit bureau data, implementing the single-violation restriction in practice requires dropping an individual if they have multiple tickets in the month of their first ticket. See appendix E for more details.

($N = 676,649$). I do not impose the same stop-category restrictions on the pool of controls for reasons both practical and conceptual. Practically, preserving a larger set of potential controls greatly increases the likelihood of finding a unique match for each treatment driver. Conceptually, one way to think about the matched difference-in-differences approach is to treat the citations data as if it ends in December 2013 and the pool of candidate controls as a provided sample of uncited drivers on the credit file. Using this thought experiment, it makes sense to use only baseline information on the controls and not future information on control drivers' traffic infractions.

Candidate controls and treatment drivers are matched exactly on gender, race (an indicator for white/nonwhite where Hispanic is considered nonwhite), age in years, credit file age in years, commuting zone of residence, and quartiles of estimated income and credit score as of January 2010 using an approach similar to the coarsened exact matching procedure in Iacus et al. (2012). Once all potential matches are generated, I use random draws to force a unique 1-to-1 match for each treatment driver. In total, a unique control match is found for 257,002 treatment drivers, or 71 percent of treatment group candidates.

Columns 1-2 of table 1 present baseline characteristics of matched treatment and control drivers. Drivers in the matched sample are 45 percent female and 55 percent nonwhite. The average age is about 35, and average credit scores and estimated incomes are about 625 and $35,000 respectively. The matching algorithm ensures that treatment and control drivers are identical in terms of baseline gender, race, and age, and similar in terms of baseline credit score and estimated income. Panels B through D illustrate that, at baseline, treatment and control drivers are also very similar on a host of credit report outcomes, including key outcomes of interest such as collections (2.18 and 2.24), collections balances ($1,590 and $1,606), and the likelihood of holding any job in the employment data (18 percent and 17 percent).

For reference, column 3 of table 1 shows baseline characteristics for the event study sample, which is all individuals meeting restrictions (a)-(d) above who receive a moving violation (and single-violation) traffic ticket at some point during 2011-2015 ($N = 983,206$). Comparing columns 1-2 with column 3 reveals that individuals in the matched sample are strikingly similar to the sample of moving violators more generally. In other words, the matching procedure does not appear to systematically bias selection into the analysis sample.[12]

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[12]However, it is useful to note that matches are acheived more often for drivers in higher income quartiles than driver in lower income quartiles. When running regressions separately for drivers above and below the median baseline income ($26,000) sample sizes are larger for the above-median income group because I use the initial quartiles used for matching when splitting the sample.

4.3 Comparison with event study design

One could also leverage variation in the timing of traffic stops with an event study approach. Specifically, one could estimate regressions of the form:

$$y_{it} = \sum_\tau \alpha_\tau + \phi_i + \kappa_t + \epsilon_{it} \tag{3}$$

where ϕ_i and κ_t are individual and time fixed effects. Here, τ indexes event time, or quarters since citatation. The α_τ's, the coefficients on the event time indicators, 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 individdiduals at the same calendar time but different event time. 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.

Relative to an event study approach, the matched difference-in-differences design has several important advantages. The matched approach is easier to interpret and basic results can be viewed using plots of the raw data. Credit report outcomes exhibit strong age profiles and, when treatment and control individuals are matched on age, age effects are nonparametrically differenced out even in a regression with individual and time fixed effects. The event study, on the other hand, forces the choice between including individual and age fixed effects. The matched design, with an explicit control group, is better suited to the analysis of longer-term effects (i.e., six quarters post citation), which is necessary here given the slowly-evolving outcomes of interest. Finally, the matched-pair difference-in-differences approach circumvents identification issues in event studies with no control group (e.g., Borusyak & Jaravel 2017) and in difference-in-differences designs leveraging only variation in treatment timing (e.g., Goodman-Bacon 2019) highlighted in recent econometric research.

On the other hand, there are also disadvantages of the matching approach. The matching procedure requires subjective choices by the researcher in terms of delineating treatment and control groups and selecting the variables for matching. Matching on age is necessary for comparability between treatment and control groups but imposes that treated individuals face fines when younger than control individuals, raising potential concerns over unobservable differences between the two groups. Finally, the maching process shrinks the dataset considerably by discarding later time periods and unmatched individuals.

I show results from simple event study estimates (equation 3) as robustness. The similarity between the primary matched estimates and event study estimates alleviates concerns that results are dependent on the details of the matching procedude or specific to the sample of successfully matched drivers. For these regressions, I use all individuals with a single-violation, moving-violation traffic stop over the period 2011-2015 ($N = 983,206$). For indi-

viduals with multiple such events, I use the timing of the first moving violation. Summary statistics for the event study sample are shown in column 3 of table 1.

4.4   Speeder design

To supplement the timing-based designs, I also show results from a different identification strategy that compares two individuals receiving citations at the same time but paying different fine amounts. In general, such a comparison can be problematic because large increases in fines are typically associated with much more severe infractions that could signal a meaningfully different type of individual. Further, violations that carry higher fine amounts often carry higher non-pecuniary punishments, such as additional driver license points, mandated court appearances, or automatic license suspensions, as well.

To circumvent these issues, I focus on individuals committing speeding offenses and leverage the details of the speeding fine schedule in Florida, which is shown graphically in panel (a) of figure 8. Fine amounts are a step function of the driver's charged speed, or miles per hour (MPH) over the posted speed limit. The fine amount jumps from \$123 to \$198 at 10 MPH, then \$198 to \$248 at 16 MPH, and so on. Drivers charged with speeding 14 MPH or below the posted limit receive three license points, while those speeding 15 MPH or above receive four license points. Hence, focusing on speeders charged at 9 MPH and 10-14 MPH allows for a comparison of individuals committing similar offenses and receiving the same number of license points, but facing fine amounts that differ by \$75. To operationalize this strategy, I estimate regressions identical to equation 1 where $Treat_i$ is an indicator for whether the driver was cited at 10-14 MPH over the posted limit and therefore faces a \$75 higher fine.

As shown in Goncalves & Mello (2021), officers commonly manipulate the charge speed distribution by *discounting* drivers down to 9 MPH. In other words, many drivers with a charged speed of exactly 9 MPH over the limit were observed driving a faster speed but were given a break on their fine by the ticketing officer. This feature of the setting has both advantages and disadvantages. An advantage is that a comparison of those cited at 9 MPH and 10-14 MPH is likely a comparison of individuals with very similar offenses and whose fine differences are the result of luck in the form of lenience by the citing officer. A disadvanage is that lenience to a particular driver could be correlated with driver characteristics. However, the estimating regression includes individual fixed effects and I verify that high- and low-fine drivers trend similarly prior to a traffic stop. Hence, the only remaining concern is whether lenience is correlated with a driver-level shock occuring coincidentally with the traffic stop.[13]

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[13]In principle, once could implement a strategy that uses across-officer variation in the propensity

To construct the speeder design sample, I identify all individuals in the inital sample (meeting the conditions at the start of section 4.2) with a single-violation, speeding-violation traffic ticket over 2011-2015. For individuals with multiple such incidents, I select the first one. I then limit the sample to individuals with charge speeds between 9 and 14 MPH over the posted limit. Finally, I focus on the subsample of these drivers who pay their fines in order to abstract away from other potential downstream outcomes and isolate the impact of paying an additional $75 in fines.[14]

Columns 4-5 report baseline characteristics for the treatment (10-14 MPH) and control (9 MPH) groups in the speeder sample. In terms of demographics, individuals in the speeder sample look reasonably similar to those in the matched and event study samples. The speeder sample does appear slightly, but systematically, more financially well off than the other samples. Speeders tend to have lower collections balances, fewer delinquent accounts, and are more likely to have open borrowing acccounts.

Comparing columns (4) and (5), there is a significant difference in racial composition between the 10-14 MPH (53 percent nonwhite) and 9 MPH (43 percent nonwhite) groups. This disparity is consistent with the finding in Goncalves & Mello (2021) that lenience is allocated by patrol officers in a racially-biased manner. The high-fine group also appears more disadvantaged on several financial outcomes, with lower credit scores (631 versus 648) and more unpaid bills (2.24 versus 1.82) at baseline. Fixed baseline differences are not necessarily problematic for the speeder regression, which includes individual fixed effects and focus on changes over time, but they are useful to keep in mind for interpretation.

To summarize, the speeder design is a useful complement to the matched difference-in-differences and event study designs because it allows for comparisons of two drivers stoppped at the same time. This strategy, therefore, does not suffer from the identification challenges associated with relying on variation in the timing of citations, such as a changes in driving frequency or car access just prior to the traffic stop. The speeder design also helps to isolate the impact of a pure expense shock, as both the treatment (10-14 MPH) and control (9 MPH) groups face the same number of license points. On the other hand, the speeder design yields less precise estimates due to a smaller sample size and estimates a slightly different (intensive versus extensive margin) type of effect.

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for lenience as an instrument for the fine faced by a driver (Goncalves & Mello, 2021). In practice, such a strategy is likely to be significantly underpowered for this particular application. The sample is already small ($N = 27,735$) and would have to be reduced even further because only Florida Highway Patrol officers can be consistently identified in the data (the current speeder sample includes speeding tickets isssued by any agency).

[14]I consider both payers and school attendees as payers. Traffic school attendance is not meaningfully different by treatment status in the speeder design. The rate of traffic school attendance in the high-fine (low-fine) group is 36.2 percent (35.3 percent).

5 Results

Panel (a) of Figure 2 plots the share of individuals in the matched sample with a new financial distress incident, defined as any new collection, delinquency, or derogatory appearing on a credit report in a given quarter, for treatment and control drivers around the time of the treatment group's traffic stop date. The two groups evolve similarly in the four quarters prior to the stop date, but a clear gap emerges following the traffic stop. Treatment drivers experience a slight increase beginning just after the stop date, with the treatment-control difference growing over the first 3-4 post-ticket quarters.

Panel (b) plots the regression analogue to panel (a). Specifically, the figure plots θ_τ's from estimates of equation (1), which are treatment-control differences over time, adjusted for individual and time fixed effects. I show results separately for drivers with baseline estimated incomes above (blue circles) and below (green squares) the median ($26,000). The figure reveals that the pattern observed in panel (a) is driven almost entirely by low-income drivers, who experience a sharp increase in the likelihood of a new financial distress event following a traffic stop. The effect peaks at nearly two percentage points in the three to four quarters after a citation.

Figure 3 presents identical plots to panel (b) of figure 2 for other outcomes of interest.[15] Panels (a) and (b) demonstrate that following a traffic stop, low-income treated drivers experience increases in unpaid bills in collections and collections balances relative to their control drivers. In both cases, there is little evidence of differential pre-citation trends for treatment and control groups and effects are substantially more pronounced for low-income than for higher-income drivers.

Panel (c) shows results for the sum of delinquent and derogatory accounts. Again, we observe increases in these account-based measures in financial distress measures following a traffic citation. The impact is larger for low-income drivers, but the income gradient is less pronounced than when examining collections. To interpret the differential effects across income groups here, it is important to note that an individual must maintain open credit accounts in order to obtain a delinquency or derogatory on her credit report. Differences between the lower- and higher-income subsamples in borrowing rates are sizeable. Among below-median income drivers at the timing of treatment, 70% have any open account and the average number of accounts is 1.7. The comparable numbers for the higher-income subsample are 95% and 6.7. Hence, we should expect less stark treatment effect differences when examining account-based measures, as low-income drivers have less scope for delinquency.

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[15]Raw data plot identical to panel (a) of figure 2 by income group and for other outcomes are presented in B-1 and B-2.

Finally, panel (d) shows the impact of traffic fines on any job in the employment data, an indicator for presence in the employment records in a given quarter. Following a traffic stop, treated individuals experience statistically significant declines in this employment measure relative to control drivers. The effect is negative and statistically significant, but quantitatively quite small, for higher-income drivers. Effects are substantially larger for low-income drivers. The graph suggests that, six quarters out from a traffic stop, low-income treated drivers are about 1.5 percentage points (or about eight percent) less likely to hold a job covered by the employment database than their matched controls.

Table 2 presents the estimated three- and six-quarter treatment effects and standard errors from the same set of regressions. As indicated in the column 2, low-income treatment drivers are 1.8 percentage points more likely than their matched controls to experience a new collection, delinquency, or derogatory in the third quarter after the treatment group's traffic stop. Scaled by the baseline control mean (0.22), this represents about an eight percent increase. Six quarters out, treatment-control differences persist at 1.4 percentage points, or about six percent. Estimated effects for higher-income drivers in columns 5-6, on the other hand, are an order of magnitude smaller and not statistically different from zero.

Figure A-6 shows impacts on any new distress, broken down by distress type. For low-income drivers, increases in the probability of a new collection, delinquency, or derogatory are all pronounced and statistically significant. Impacts on new collections are quantitatively largest. Interestingly, the sum of the coefficients at $\tau = 3$ (≈ 0.28) is larger than the impact on any new incident (0.018), implying that some individuals experience both a new collection and new delinquency or derogatory.

The second and third rows report estimates for unpaid bills in collections and associated collections balances. Recall that collections on credit reports are unlikely to be related to traffic fines and most likely represent unpaid utility or medical bills. Focusing on low-income drivers, the estimates reveal that six quarters out from the treatment group's traffic stop, treatment drivers have 0.12 more collections and $76 higher collections balances than their matched controls. Both effects are three percent increases relative to the baseline control mean. Given an average fine size of $190, the collections effect implies that about 40 percent of the fine amount ultimately turns up in the form of unpaid bills on a credit report. For higher-income drivers, the effect on collections is also statistically significant but about 75 percent smaller, and there is no statistically detectable effect on collections balances.

Moving onto the account-based measures of financial strain, low-income treatment drivers accrue 0.085 additional delinquencies and 0.045 additional derogatories relative to their matched controls in the six quarters following a traffic stop. Again, scaled by the mean, these effects are on the order of three to four percent. As mentioned in the discussion of

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figure 3, the differences in effect sizes across income groups are less stark here, a fact tht makes sense given the propensity for lower-income drivers to maintain significantly fewer open borrowing accounts. Impacts on delinquencies and derogatories are about 40-50 percent smaller in the higher-income group and represent two percent increases relative to the baseline control means.

Overall, the matched difference-in-differences estimates clearly suggest an increase in financial distress for cited drivers in the six quarters following a traffic fine. Impacts are particularly pronounced for lower-income drivers. While I defer a more thorough discussion of magnitudes to section 6, a few points bear brief mentioning here. First, a simple benchmark for the impacts on distress can be obtained by asking what income change would predict the observed effects. I answer this question in appendix C by estimating an elasticity of collections balances with respect to monthly earnings and backing out the implied earnings changes for each income group. For low-income drivers, the logged increase in collections balances would be predicted by a $236 decline in monthly income, about 25 percent larger than the typical fine. For higher-income drivers, the estimate is $-\$122$.

Second, one might question why effects are observed *at all* for higher-income drivers. It is important to remember, however, that the baseline (estimated) income threshold for the higher-income subsample is $26,000. Moreover, a small literature has documented that higher-income households are not immune to liquidity issues (e.g., Kaplan et al. 2014), especially because of sizeable consumption committments (e.g., Chetty & Szeidl 2007).

The final row of table 2 reports estimates for the employment measure. As shown in figure 3, low-income treatment drivers are about 1.5 percentage points less likely to hold a job covered by the employment database than their matched controls in the sixth quarter following a traffic stop. Relative to the control mean (0.17), this represents about a nine percent decline. Declines in this employment measure are also statistically significant, but substantially smaller, in the above median-income group. In this subsample, the reductions are about 75 percent smaller and represent a two percent decline relative to the mean.[16]

Keeping in mind that the emloyment records are not unemployment insurance records and include only partial coverage of jobs in the state, the estimated effect does not imply that traffic fines induce 1.5 percentage point increases in the probability of unemployment or nonemployment. Instead, a conservative interpretation is that citations increase employment instability (or the probability of a job change). Results in appendix C show that an

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[16]One natural question here is to what extent the covered-job effect can explain the increases in financial distress measures. In appendix C, I show estimates of the impact of separating from the employment data on financial outcomes. Scaling these estimates by the covered job treatment effect (-0.015) suggests that changes in financial outcomes caused by a separation could only explain about 1.6 percent of the impact of fines on collections and collections balances.

individual's financial situation tends to deteriorate after falling out of the employment data, suggesting that these job transitions are typically negative outcomes. Further, looking at table 1, average annual earnings for individuals working in jobs covered by the employment records at baseline is about $42,000 (= $3,5000 × 12). In the 2010 Current Population Survey, average salaries in Florida were about $32,000, suggesting that covered jobs are better than average.

Changes to employment situations following a traffic stop could be caused by a citation's potential effects on driving privileges. However, section 5.3 shows that impacts on the likelihood of holding a job in the employment records are present in the sample of fine payers who are very unlikely to face a driver license suspension. Another possible channel is the role of credit report information on job-finding (Bos et al. 2018, Bartik & Nelson 2017) or the effects of financial distress on labor supply (Dobbie & Song, 2015). Finally, a growing literature has highlighted the negative impacts of financial strain on productivity (Kaur et al., 2019) and decision-making more generally (Mullainathan & Shafir 2013, Schilbach et al. 2016).

5.1 Heterogeneity

Panel (a) of figure 4 examines heterogeneity on dimensions other than income, focusing on the effect of fines on unpaid bills.[17] The plot shows six-quarter treatment-control differences obtained from estimates of the parametric specification (equation 2) for each denoted subsample. In terms of demographics, the figure illustrates larger impacts of fines for younger and nonwhite drivers. The age gradient is starker for women, with women over 35 notably less affected than the other three gender × age groups.

The starkest differences are in the comparisons between subprime and prime drivers (those with credit scores below and above 600 at baseline), drivers with and without any prior financial strain (those with positive and zero collections balances at baseline), and drivers without and with any borrowing at baseline. These three cuts of the data are correlated; individuals with unpaid bills at baseline are likely to also have subprime credit scores and diminished access to borrowing. Differential impacts of fines across subsamples are in the expected direction in all three cases: those with subprime credit scores, unpaid bills, and no revolving debt at baseline are significantly more affected. In section 5.3 below, I further explore the comparisons between those with and without apparent attachment to formal borrowing at baseline.

Motivated by the disparities highlighted in panel (a) of figure 4, panel (b) plots six-quarter, parametric treatment effects estimated separately by quantile of baseline financial

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[17]Figure B-3 shows that heterogeneity patterns are similar for other outcomes.

strain, proxied by baseline collections balances. The quantiles are deciles, but the first six deciles are grouped together because the 55th percentile of baseline collections balances is zero. The figure illustrates a gradient in baseline financial distress. Those experiencing more financial distress *ex ante* suffer the lagest deteriorations in financial situations following a traffic fine.

While the analysis thus far has illustrated that effects largely accrue in the sample of individuals already suffering from financial problems, a separate group of interest is the (likely small) subset of individuals who appear financial stable but are pushed into distress by a traffic fine. To examine this subgroup, I first narrow the matched sample to the subset of individuals ($N = 167, 158$) with no collections, delinquencies, and derogatories at baseline and no new distress accrued through the date of their traffic stop, or the placebo date for the controls. I then estimate a matched difference-in-differences (equation 1) regression where the outcome is an indicator for whether an individual has experienced any new financial distress to date. As shown in panel (a) of figure 5, six quarters out from a traffic stop, treatment group drivers are 0.9 percentage points (b = 0.0088, se = 0.0018) more likely than the controls to have experienced an incident of financial distress.

Based on a potential outcomes interpretation (e.g., Imbens & Rubin 1997, Abadie 2003), the estimate implies that 0.9 percent of the clean-history sample are compliers – those induced into distress for the first time by a traffic fine. For reference, the coefficient on the event-time indicator for $\tau = 6$ is 0.085 (se = 0.002), suggesting that 8.5 percent of the clean-history sample are always-takers, or individuals who would have suffered distress regardless. The remaining 90.6 percent are never-takers, or individuals who are unaffected by fines.

Next, I examine treatment effect heterogeneity in the clean-history sample, focusing on an indicator for any new distress as the outcome of interest. Results are presented in panel (b) of figure 5. As expected, standard errors are large given the small outcome mean and smaller sample size. However, the figure strongly suggests that, among those with clean payment histories, young men are most likely to be affected by traffic fines. White drivers and residents of more affluent zip codes appear slightly less susceptible to fine effects than nonwhite drivers and residents of poorer zip codes. I do not show results by baseline estimated incomes or credit scores because the sample is selected on baseline default measures, which are inputs into baseline credit scores and estimated incomes.

5.2 Robustness

In this section, I probe the robustness of the matched difference-in-differences results, paying particular attention to the concerns raised in section 4.1. Given that results are significantly

stronger for drivers with below-median incomes, I focus on that subsample and show robustness results for the higher-income subsample in the appendix.

As discussed in section 4.1, one important threat to the validity of the matched difference-in-differences design is the possibility that the timing of a traffic stop coincides with changes in driving behavior such as an increase in driving frequency, and that changes in driving predict changes in an individual's financial situation. Figure B-7 highlights this concern by illustrating a differential pre-citation trend in the car ownership for treatment group individuals. Relative to their matched control, the probability that a low-income individual has an open auto loan on their credit report increases by about two percentage points in the four quarters leading up a traffic stop.

To illustrate that differential changes in car ownership cannot explain the results, I replicate the matched difference-in-differences analysis but add auto loan histories (up to the citation quarter) to the set of matching criteria. For example, consider a treatment group driver cited during 2011Q2. I define such a driver's auto loan history as whether that individual had an open auto loan in each of the six quarters from 2010Q1 to 2011Q2. If the driver takes out an auto loan in November 2011 and retains it thereafter, her auto history is {0,0,0,1,1,1}. The matching algorithm ensures that such an individual is matched to a control driver with an indentical sequence of $\mathbf{1}$[any auto loan] over the first six quarters. When adding the auto sequences to the matching criteria, the number of sample drivers shrinks to 321,980 (160,990 each in the treatment and control groups). Summary statistics for this sample as well as further details on the matching algorithm are presented in appendix B.

Another question about the matched differences-in-differences design is whether anything specific about the matching procedure, such as the construction of the matching criteria or focusing on a subset of six treatment quarters, is important in generating the results. To answer this question, I show results from simple event studies (equation 3) using all single-violation, moving violation citations over 2011-2015.

I also show results from event study regressions where indicators for quarters since an auto purchase (i.e., car purchase event-time) are added. For each driver, I proxy the timing of an auto purchase with the first quarter in which an individual has an open auto loan on their credit report. With these indicators included in the regression, the event study design leverages variation in the timing of traffic stops within subsets of individuals who purchased cars at the same time for identification.

Figure 6 summarizes the robustness results, focusing on the sample of below-median income drivers.[18] The green solid line shows the primary matched difference-in-differences

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[18]Event study estimates for both income groups are presented in figure A-7. Matched difference-in-differences estimates when matching on auto histories for both income groups are presented in

estimates. The blue diamonds show the corresponding estimates when using the sample of drivers matched on auto loan histories. The purple x marks show results from the simple event studies and the orange + signs show event study results when the auto purchase event-time indicators are added.

For financial distress measures, the figure highlights the similarity in results across the four different empirical approaches. For collections (b) and collections balances (c), estimated impacts are, if anything, larger when requiring a match on car ownership history and close to the main DD estimates in either event study specification. Examining panel (d), matching on auto loan histories has minimal impact on the estimated employment effect relative to the main DD estimate. However, event study estimates for the employment measure are quite attenuated relative to either difference-in-differences specification. Worth noting here is one reason to especially prefer the difference-in-differences framework when examining employment data outcomes: the control group accounts for secular, quarterly changes in the employment database's coverage, which are documented in figure E-4.

Table 3 presents the corresponding point estimates and standard errors, with each column corresponding to one of the four specifications. The point estimates confirm the graphical evidence in figure 6. Estimated impacts on any new financial distress (0.16-0.2), collections (0.085-0.131), and collections balances ($64-$93) are reasonably similar across the four different approaches. In both the matched differences-in-differences and event study designs, adjusting for the timing of auto purchases systematically but slightly reduces the estimated impact on the account-based measures of distress. As mentioned in the discussion of figure 6, event study estimates for likelihood of working in a job covered by the employment database are attenuated relative to the difference-in-differences estimates.

To summarize, the robustness analysis illustrates that neither a differential trend in car ownership around the timing of traffic stop nor specifics of the matching process can explain the primary results. My baseline estimates for the focal financial distress outcomes are largely unchanged when nonparametrically adjusting for car purchase patterns via matching and when using a very straightforward event study design covering a larger sample of drivers.

5.3 Mechanisms

As detailed in section 2, 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 important to note that the Florida Clerk's office's records indicate that over 90% of

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B-5. Figure A-8 presents the same information as figure 6 for the above-median income group.

citations are paid on time, the data provided by the Florida Clerks includes information on the associatead traffic court dispositions for a subset of citations.[19] The court disposition information is potentially useful for disentagling the relative importance of the financial shock itself as opposed to impacts on driving costs via license points or license suspensions.

Specifically, I focus on low-income drivers and use the matched difference-in-differences setup to examine treatment effects for individuals whose dispositions indicate a fine payment and individuals whose dispositions indicate a traffic school election. Drivers making payment will not face a nonpayment suspension but accrue driver license points. Those 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 on-time payment and faces no increase in license points, a driver opting for traffic school will not face a license suspension or an increase in future car insurance costs.

Those with a payment (but not traffic school) disposition are also very unlikely to face a license suspension following their citation. Treatment drivers are ticketed during the period January 2011 through June 2012. By construction, these individuals have at least one year of clean driving history prior to their focal traffic stop (no citations in 2010). All treatment group citations carry a penalty of 3-4 license points. Thus, for a paid treatment group citation to trigger a points-based license suspension, an individual would need to either (a) be ticketed in the first two quarters of 2011 *and* have accrued at least 14 license points during the final two quarters of 2009 or (b) have accrued at least 20 license points over the period between three years before their traffic stop and the start of 2010.

Motivated by the institutional features, I show results for payers and school attendees and examine the difference between the two groups. However, this comparison is certainly imperfect. A traffic school election signals a savviness of institutions, an ability to come up with an extra few dollars, and the flexibility to participate in four hours of instruction. In the data, those attending traffic school not only appear more advantaged at baseline but, perhaps unsurprisingly, exhibit differential improvements in their financial situations in the months leading up to a traffic stop, as showm in figure B-6. Hence, I use the parametric difference-in-differences specification (equation 2) and estimate six-quarter effects relative to a differential linear trend for treatment and control groups, but caution the reader to interpret the comparisons between the payer and traffic school groups as useful but descriptive.

The results are presented in table 4. Comparing columns 1 and 2 reveals that, if anything, impacts of citations on unpaid bills (0.17 v. 0.11), collections balances ($104 v. $55), and employment in a covered job (-0.23 v. -0.2) are larger in the payer sample than the sample

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[19]For additional details on the various issues with the dispositions data, see appendix E.

as whole. While the ultimate outcome of the citation is difficult to infer in the non-payer, non-school segment of the sample, this result strongly suggests that actually paying the fine, rather than a license suspension, explains the observed deteriorations in financial health.

Comparing columns 2-3, effects appear smaller for school attendees than for payers. In the school sample, the impact on collections (0.099) is marginally significant, while effects on collections balances ($56) and the employment measure (-0.015) are statistically insignificant. The magnitudes, however, are similar to those in the full sample in all three cases. Further, as shown in column 3, the differential effect of opting for traffic school (relative to paying) is statistically indistinguishable from zero.

Overall, this analysis points to the fine itself, rather than complications associated with fine nonpayment such as driver license suspensions, as the source of the financial distress effects in the full sample. Estimated impacts are larger when focusing only on the subset of individuals who can be cleanly identified as having paid their fines. Impacts are quantitatively similar to, but less precise than, the main estimates when focusing on a positively-selected subgroup of individuals who pay their fines and receive no driver license points following the citation. In this sample, we can completely rule out suspensions or changes in insurance premiums as other causes of financial distress.[20]

To futher unpack potential mechanisms, I also examine the role of access to liquidity in explaining heterogeneity in the treatment effects. Again focusing on low-income drivers, I split individuals into two categories based on their baseline credit card situation. The first group is individuals with an open credit card and at least $200 in available balances (23% of the sample), defined as the credit limit (summed across all open revolving accounts) minus the current revolving balance (summed across all open revolving accounts). The second group is all others, which includes individuals with no open revolving account or less than $200 in available balances.

Panel (a) of figure 7 plots matched estimates where the outcome is revolving utilization, defined as the total revolving balance divided by the total revolving limit. Utilization is coded as zero for person-quarters with no open revolving account and is topcoded at one. The figure illustrates a differential increase in utilization following a traffic stop for treatment drivers in the credit-access group.

As indicated in table 5, as of 3 quarters out from the stop date, utilization has increased by about 0.9 percentage points. Scaled by the average credit limit among low-income, control drivers with open credit cards ($3,999), the utilization increase translates to a $36 dollar

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[20]In section 5.4, I show results from the speeder design which compares individuals facing the same number of license points but differing fine amounts. These results also suggest that paying the fine itself can explain the lion's share of a ticket's impact on unpaid bills.

increase in borrowing. In other words, individuals in the credit-access example appear to cover about $36 of the typical $190 fine (19 percent) using credit cards.

Panel (b) of figure 7 plots estimates for collections for the credit-access and other samples. The figure shows a smaller increase in collections in the credit-access sample, which suggests that an ability to rely on credit card borrowing reduces the risk of missing other bills following a fine. Impacts on collections are still present for borrowers, however. Table 5 explicitly compares impacts on financial distress measures across the credit-access and other samples. For tose with available card balances at baseline, fines induce 0.09 (0.45 v. 0.134) fewer missed bills and $59 ($26 v. $85) less debt in collections. The impact on job transitions is slightly attenuated in the card-access sample (-0.012 v -0.016), but the difference is not statistically significant.

Taken together, the results suggest that liquidity constraints play an important role. Low-income individuals with available balances on credit cards use those balances to cover a share of the fine and miss fewer bills as a result. However, such individuals still accrue collections on their credit reports, suggesting that easy access to formal borrowing does not provide full insurance against the financial consequences of a fine.

5.4 Speeder Results

Panel (a) of figure 8 illustrates the key points of the speeder identification strategy, as described in section 4.4. The black dotted line plots the fine amount as a function of the driver's speed relative to the speed limit. The fine amount jumps discretely at 10, 16, and 21 MPH over the posted limit. As illustrated by the blue dots and green circles, a citation for 14 or fewer MPH over the posted limit carries three license points while a citation for 15 or more carries four license points. The histogram of ticketed speeds shows substantial excess mass at 9 MPH over the limit, suggesting manipulation of the charged speed by the ticketing officer (Goncalves & Mello, 2021). As discussed earlier in section 4.4, this manipulation poses both advantages and disadvantages for the speeder design.

Panel (b) shows raw means of collections for treated (10-14 MPH) and control (9 MPH) drivers around the timing of a speeding stop. The two groups evolve similarly prior to the traffic stop date, but those paying the higher fine experience a clear increase in collections relative to the control group following a speeding stop. Panel (c) illustrates the regression analogue, with regressions estimated separately for drivers above and below the median income at baseline. The figure indicates a differential increase in unpaid bills for drivers paying the additional fine, with the impact more pronounced for low-income drivers. The story is simimlar when examining collections balances in panel (d).

Table 6 presents the corresponding coefficients and standard errors. As shown in column 3, low-income drivers paying a $75 higher fine have accrued 0.07 additional collections on their credit report as of six quarters following a speeding stop. For higher income drivers, the comparable estimate is 0.033 and also statistically significant. A higher fine is associated with a marginally significant $27 increase in collections balances for low-income drivers and a statistically insignificant $15 increase for higher-income drivers. Identical figures for other outcomes are shown in figure A-9.

Focusing on collections balances, the results are remarkably similar to the main matched difference-in-differences estimates when taking into account the different fine amounts. For low-income speeders, $27 out of a $75 fine increase (36 percent) shows up on a credit report in the form of unpaid bills in collections. The main matched estimate from 2 implies that out of a $190 fine, $76 (40 percent) show up as unpaid bills in collections.

There is no evidence that the higher fine affects the likelihood of any new distress incident. This is somewhat unsurprising given that the control group is also fined. The treatment in the speeder design is on the intensive margin and the effects are observed on the intensive margin. Low-income individuals are not necessarily more likely to miss any bill (as opposed to missing none), but tend to miss more bills for larger amounts. Impacts on the account-based distress measures, as well as on employment transitions, are all statistically indistinguishable from zero in the speeder design.

The speeder results contribute to the paper in two important ways. First, by comparing two individuals stopped at the same time, the speeder design avoids many of the potential shortcomings associated with the matched difference-in-differences and event study approaches, which rely explicitly on the timing of traffic stops for identification. For unpaid bills, the similarity between the matched and speeder estimates after appropriately scaling for treatment dosage bolsters confidence in the robustness of the main set of results. Second, the speeder design provides another opportunity to explore mechanisms. Here, the comparison is between two individuals *paying* different fine amounts but facing the same non-fine punishments. Hence, the speeder results further support the hypothesis advanced in 5.3 that paying the fine itself, rather than consequences of license points, explains the observed deteriorations in financial situtations.

6 Discussion

6.1 Interpreting Magnitudes

Given that the bulk of the evidence points to the expense shock itself as driving the impacts of fines on financial distress, an accounting exercise can provide a useful framework for interpreting magnitudes. Consider an individual facing an unexpected bill for $190. Such an individual can cover the expense from four sources. First, they can draw down cash-on-hand or savings. Second, they can reduce consumption outlays (or, in other words, borrow from funds earmarked for consumption). Third, they can borrow through formal or informal credit channels. And finally, they can miss other bills (or, in other words, borrow from funds earmarked for other bills such as a utility bill).

The third and fourth channels are observable in the data. Focusing on low-income drivers, table 2 indicates that a $190 fine induces a $76 increase in unpaid bills in collections. As shown in figure B-7, individuals experience a 0.48 percentage point increase in revolving utilization. Scaled by the baseline average revolving limit for low-income borrowers ($3,999), this estimate implies a $19 increase in credit card borrowing.

Consumption is not observed in the credit report data. However, existing research can shed light on possible consumption effects. Specifically, Pattison (2020) finds that a typical default is associated with a six percent decline in consumption. Summing up the estimated effects on collections (0.118), delinquencies (0.085), and derogatories (0.048), which are all default measures, implies that each fine causes 0.251 incidences of default. Thus, the expected decline in consumption is 1.5 percent ($0.25 \times 0.06 = 0.015$). Scaling by average monthly earnings in the low-income sample at baseline ($1,532) yields an estimated $23 decline in consumption.[21]

Hence, for the low-income sample, missed bills, credit card borrowing, and reduced consumption can account for $118 (= $76 + $19 + $23), or 63 percent of the typical $190 fine amount. Making the very strong assumption that drivers do not borrow informally or through alternative subprime channels, this accounting exercise suggests that the typical low-income individual covers about 37 percent of a $190 fine out of cash-on-hand.

The comparable estimate for higher-income drivers is 82 percent. In the the above-median income sample, fines increase collections balances by $7 and incidences of default by 0.1 (= 0.034 + 0.04 + 0.029). Again using the estimate from Pattison (2020), the latter impact implies a 0.6 percent decline in consumption, or $28 when scaling by baseline average monthly

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[21]Alternatively, one could use a monthly consumption-income elasticity estimate, treating the $190 fine as an income shock. Following this approach and using the elasticity (0.23) from Ganong et al. (2020) gives an estimate of a $43 decline in consumption.

earnings in the high-income group ($4,752). As illustrated in figure B-7, there is no treatment effect of fines on credit card borrowing in the higher-income subsample. Hence, missed bills, formal borrowing, and reduced consumption account for $35 (or 18 percent of $190).

Table 7 performs the same accounting excercise under more and less conservative assumptions. Column 1 (4) reports the same estimates as those discussed above for below-median (above-median) income drivers. In column 2 (5), I use a more conservative approach. Specifically, I use the lower 95 percent confidence interval for the collections balances effect as the estimate of money borrowed from other bills and the lower 95% confidence interval for the utilization effect to compute funds from credit cards. For funds paid out of consumption, I estimate the impact of fines on the sum of collections and delinquencies (to prevent double counting) and use the lower 95 percent confidence interval, and then scale using the Pattison (2020) estimate. The very conservative approach yields implied fine shares covered from cash of 62 percent for low-income drivers and 87 percent for higher-income drivers.

In column 3 (6), I show a less conservative calculation aimed towards a more accurate representation of funds attributable to missed bills. Focusing only on collections balances, which corresponds primarily to missed utility or medical bills, in the prior calculations yields a conservative estimate because individuals also accrue delinquencies, suggesting that they additionally "borrow" out of funds earmarked for credit line payments. I account for this in column 3 by summing (i) the collections balances effect and (ii) the delinquency effect, scaled by the ratio of the collections balances and collections effect to estimate a dollar value associated with each delinquency. I also use a direct estimated effect on credit card balances in the column 3, rather than an implied effect from the utilization regressions. The less conservative approach suggests that, for low-income drivers, $133 of the $190 is covered via missed bills and yields an implied cash share of 5 percent. For higher-income drivers, the estimates are $33 covered via missing bills and a 67 percent cash share.

To further interpret these calculations, one can briefly consider an individual's decisions from the perspective of a lifecycle consumption-savings model (e.g, Ljungqvist & Sargent 2004). In such a setup, covering an expense shock from cash-on-hand is likely preferred in most cases. Cutting current consumption is costly in terms of welfare, especially in the low-income sample, where the marginal utility of consumption is likely quite high. Through the credit scoring channel, missing bills can have significant impacts on future access to liquidity, which is valued highly by individuals based on Liberman (2016). Finally, money "borrowed" from cash-on-hand can be "repaid" interest-free, as opposed to the 10-30% interest rates charged by most credit cards.

From this point of view, one could interpret the results as suggestive evidence on the size of the financial buffers available to drivers in the sample. Through this lens, the estimates

suggest a precarious financial situation for the typical low-income individual, who can cover only $71 out of an unexpected $190 expense using cash-on-hand under reasonably conservative assumptions. Unsurprisingly, higher-income indivduals are significantly more prepared to weather such an expense and appear to cover $155 using a cash buffer.

Hence, the results appear quite consistent with the survey evidence (e.g., Lusardi 2011, Board of Governors of the Federal Reserve System 2018) suggesting the prevalence of financial precarity. Moreover, the finding that many low-income households are vulnerable to real financial consequences from seemingly small shocks suggests that social insurance programs offering insurance against expenditure or income risk may yield large benefits. This argument is consistent with a relatively new but growing literature documenting the beneficial effects of public health insurance coverage on household finances (Finkelstein et al. 2012, Mazumder & Miller 2016, Gallagher et al. 2019, Hu et al. 2019).

6.2 Implications for policing

To consider the implications of my findings for ciminal 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), Luca (2015) show that increases in traffic citation activity reduce auto accidents. Baicker & Jacobson (2007), Makowsky & Stratmann (2009), and Garrett & Wagner (2006) find evidence for 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 that follows closely the formulation from Burlando & Motta (2016) in appendix D.

In the model, 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. Increasing ticketing also raises government revenue. The social planner chooses the optimal policing intensity p^* to set marginal social benefits equal to marginal social costs:

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

Or alternatively, $-h'(p) = c'(p) + (-V'(p) - r)$. If a traffic fine represents a lump-sum transfer from an individual to the government $(-V' = r)$, the government trades off deterrence and policing costs. Marginal welfare costs exceeding revenue raised are a form of deadweight loss and need to be weighed against marginal deterrence benefits.

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In appendix D, I show that for a small increase in p starting from a small baseline, $-V'(p) \approx u_{nf} - u_f$, or the welfare difference between the unfined (nf) and fined states (f). Converting the treatment effects into welfare cost estimates is difficult, especially given that a non-neglible share of the total welfare effects will play out dynamically through impacts of default on future credit worthiness. However, the fact that individuals are unable to cover fines using savings strongly suggests that welfare costs exceed the fine size. Specifically, using the framework in section 6.1, money "borrowed" from other sources must be repaid with interest, while funds "borrowed" from consumption many be more costly in utility-terms than the corresponding revenue raised, especially for lower-income individuals. The issuance of citations with low safety benefits, such as citations issued for low-level infrations or issued at already high levels of ticketing, may not be efficient after accounting for the welfare costs to citizens.

The heterogeneous effects of fines across driver-income levels also highlight the potential inefficiency of a flat traffic fine schedule. In appendix D, I consider the implications of an income-based fine schedule.[22] 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 and small. The welfare effects of such a policy change are proportional to the difference in the marginal utilities for poorer and richer drivers when fined:

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

which is positive when $u(\cdot)$ is concave. Moreover, this difference is likely to be large when considering the gap between above- and below-median income drivers in the treatment effects of fines on financial distress and employment stability.

Finally, my results speak to the potential social costs of racially-biased policing. A large literature has documented racial disparities in criminal justice outcomes, including traffic stops (e.g., Pierson et al. 2020), with some evidence (e.g., Horrace & Rohlin 2016) that officers are racially biased when choosing which drivers to stop and ticket. Goncalves & Mello (2021) find that officers exhibit racial bias when choosing which drivers receive reduced fines during speeding stops. The meaningful impacts of fines on financial situations highlight that the welfare costs of fines issued due to racial bias could be substantial.

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[22] A caveat to the consideration of an income-based fine system is the Atkinson & Stiglitz (1976) result that welfare losses induced through commodity taxation, in this case the taxation of traffic infractions, ought to be remedied with redisribution through the income tax system.

7   Conclusion

Motivated both by the observation that the incidence of policing falls largely on disadvanted 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 reports and leverage variation in the timing of traffic stops for identification.

The empirical analyses reveal that following a traffic stop, individuals fare worse than would otherwise be predicted on a host of financial outcomes. For drivers with below-median incomes at baseline, fines averaging $190 increase the likelihood of default on a financial obligation by almost two percentage points and increase debt in collections by about $75. I also find that fines reduce the likelihood that low-income individuals are recorded as holding a job in employment records covering large employers, suggesting that fines can reduce employment stability. On all dimensions, impacts are signficantly attenuated for higher-income drivers.

While traffic citations can also lead to court costs, increases in car insurance premiums, and even driver license suspensions, fine payments alone appear to explain the vast majority of the estimated effects. I find that impacts are similar for a subset of the sample than can be identified as paying their fines and similar in a companion identification strategy that compares drivers cited at the same time and receiving identical non-fine punishments but paying different fine amounts.

To interpret magnitudes, I combine treatment effect estimates with evidence from the literature in an accounting exercise that asks how individuals pay their traffic fines. Under a fairly conservative set of assumptions, the results suggest that low-income drivers cover at most $72 out of a $190 fine (37 percent) using cash-on-hand. Higher-income individuals are estimated to cover 82 percent of a fine using a cash buffer.

The results are consistent wth a literature using surveys to document the prevalence of financial fragility among households in the United States and imply that many individuals are not self-insured against even small expense shocks. Faced with a $190 traffic fine, individuals accrue collections and delinquencies on their credit reports, highlighting an inability to draw on a financial buffer to cover the unplanned expense.

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Figure 1: Ticketing Frequency and Neighborhood Per Capita Income in Florida
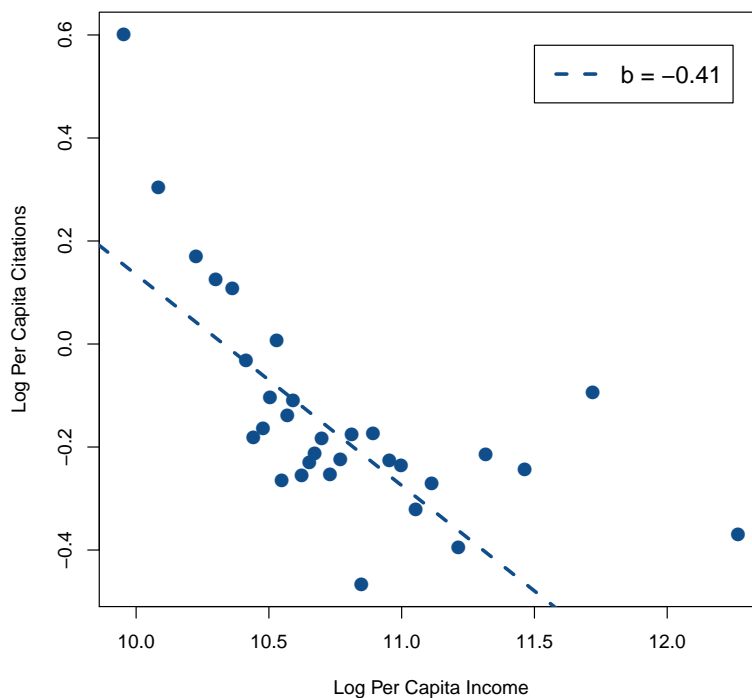
Notes: Figure plots binned means of log zip code ticketing frequency (2011-2015) against binned means of log zip code per capita income in 2010 (N=918). Zip code income data taken from the IRS. Number of citations for zip code residents and adjusted gross income are scaled by the number of tax returns 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).

Table 1: Summary Statistics for Analysis Samples

	Matched			Speeder	
	(1) Treat	(2) Control	(3) Event Study	(4) 10-14 MPH	(5) 9 MPH
Panel A: Demographics					
Female	0.45	0.45	0.44	0.47	0.48
Nonwhite	0.55	0.55	0.55	0.53	0.43
Age	35.47	35.47	36.55	35.61	35.41
Credit File Age	12.89	12.89	13.13	13.3	13.32
Credit Score	627	626	621	631	648
Estimated Income	35250	34621	34548	35905	37720
Panel B: Financial Distress					
Collections	2.18	2.24	2.34	2.24	1.82
Collections Balances	1590	1606	1722	1482	1177
Delinquencies	1.89	1.87	2	1.78	1.58
Derogatories	1.37	1.36	1.45	1.28	1.14
Panel C: Credit Usage					
Any Account	0.87	0.85	0.86	0.88	0.89
Revolving Accounts	3.44	3.34	3.34	3.36	3.69
Revolving Balances	5908	5625	5753	5920	6297
Any Auto Loan	0.4	0.38	0.39	0.41	0.42
Any Mortgage	0.32	0.32	0.33	0.35	0.38
Panel D: Employment Data					
Any Job	0.18	0.17	0.17	0.18	0.18
Positive Earnings	0.13	0.12	0.12	0.13	0.14
Monthly Earnings	3519	3477	3483	3640	3719
Panel E: Citation Information					
Fine Amount	189.59	173.61	186.49	198	123
DL Points	3.37	1.94	3.35	3	3
Individuals	257002	257002	983206	12200	15535

Notes: This table reports summary statistics as of January 2010 for the analysis samples. Columns 1-2 report means for treatment and control drivers in the matched difference-in-differences sample. Column 3 reports means for the event-study sample (all individuals with a moving violation, single-violation traffic stop over 2011-2015). Columns 4-5 report means for the speeder difference-in-differences sample. See text for additional details on sample construction. As of the 2010 ACS, Florida as a whole was 51% female, 41% nonwhite, and the average age was 40.3. Statewide average credit scores and estimated incomes were 662 and $32,000 as of January 2010.

Figure 2: Impact of Fines on Probability of New Financial Distress

(a) Raw Data

(b) DD Estimates by Income
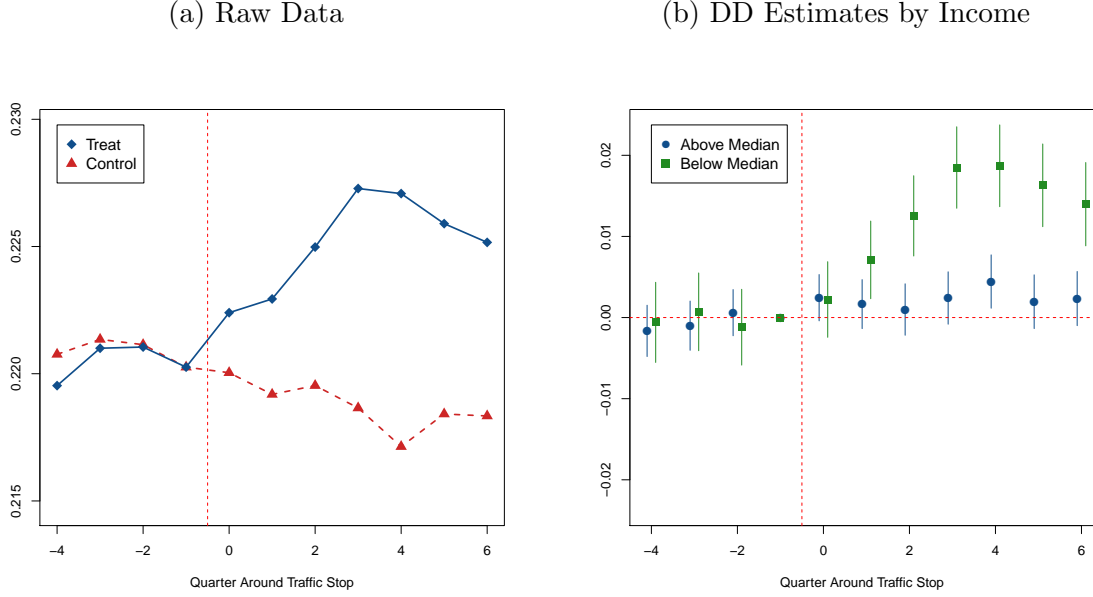
*N*otes: Panel (a) plots the share of treatment (blue diamonds) and control (red triangle) drivers with a new financial distress distress incident in each quarter relative to the treatment driver's traffic stop date using the matched DD sample. A new financial distress incident is defined as a new collection, delinquency, or derogatory in a given quarter. Means are normalized to be equal at $\tau = -1$ and adjusted for age effe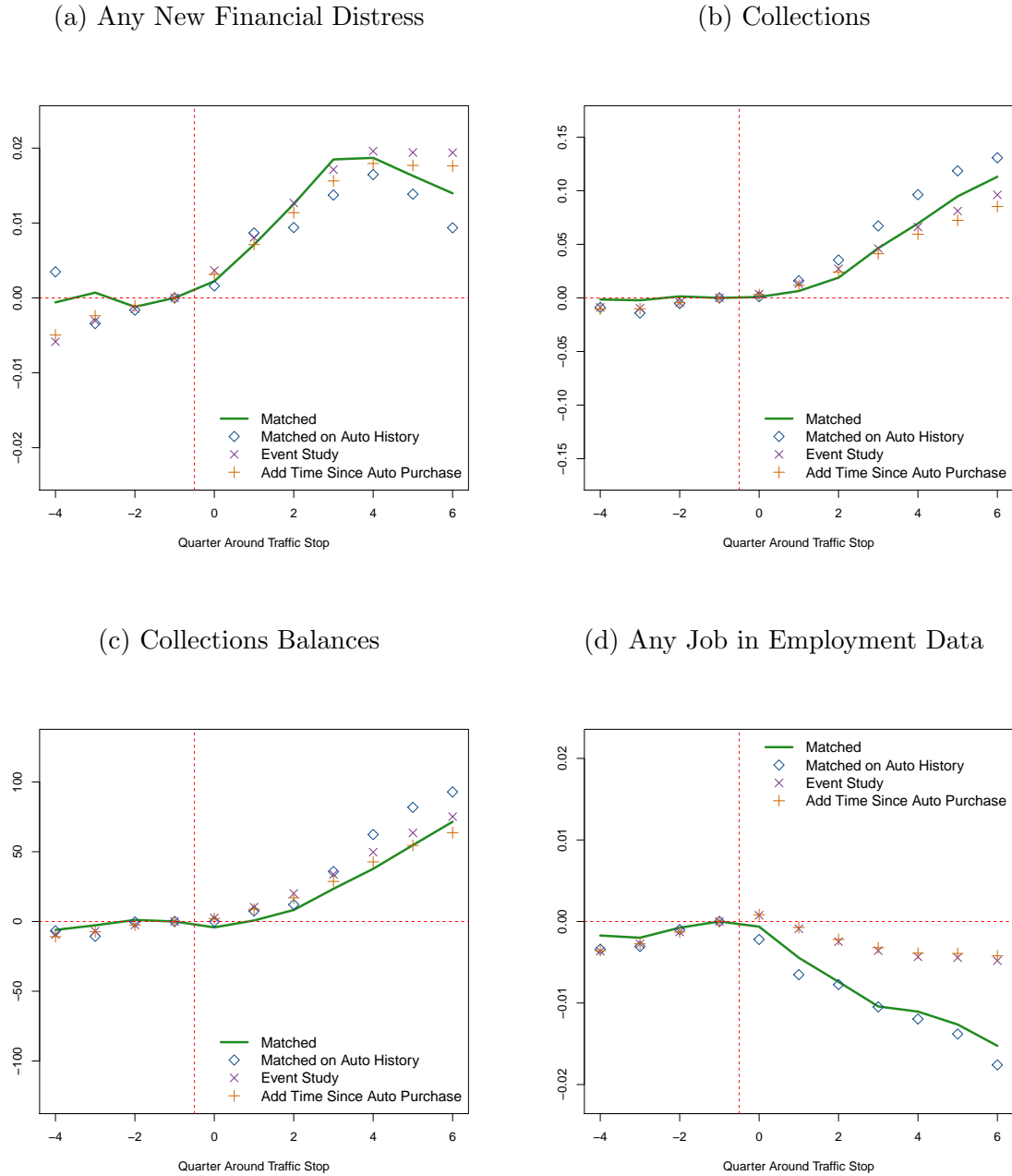cts. Panel (b) plots coefficients (and 95% confidence bands) on interactions between a treatment indicator and event time indicators from matched DD regressions (equation 1). Coefficients are normalized to $\tau = -1$. Regressions also include individual, time, and event time fixed effects. Regressions are estimated separately for for drivers above (blue circles) and below (green squares) median baseline income ($26,000). Confidence intervals are constructed from standard errors clustered at the driver level.

Figure 3: Impact of Fines on Financial Distress Outcomes by Driver Income
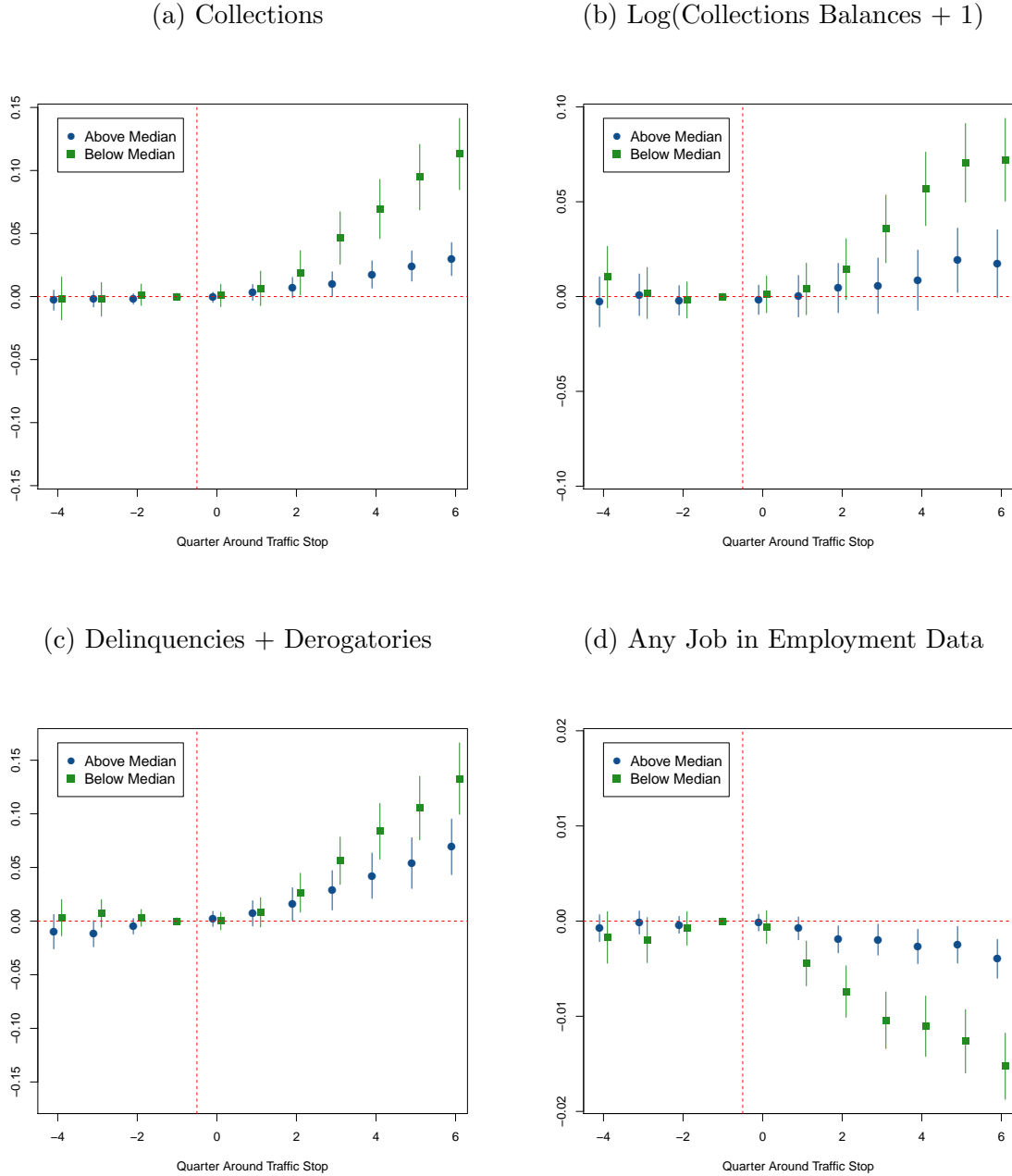
(a) Collections

(b) Log(Collections Balances + 1)

(c) Delinquencies + Derogatories

(d) Any Job in Employment Data

Notes: Each figure plots coefficients (and 95% confidence bands) on interactions between a treatment indicator and event time indicators from matched DD regressions (equation 1). Coefficients are normalized to $\tau = -1$. All regressions include individual, time, and event time fixed effects. Regressions are estimated separately for drivers above (blue circles) and below (green squares) the median baseline esimated income ($26,000). Confidence intervals are constructed from standard errors clustered at the individual level.

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Table 2: Impact of Fines on Financial Distress Outcomes

	Below Median Income			Above Median Income		
	(1) Control Mean	(2) 3 Quarters	(3) 6 Quarters	(4) Control Mean	(5) 3 Quarters	(6) 6 Quarters
Any New Distress	0.22	0.018*** (0.003)	0.014*** (0.003)	0.146	0.002 (0.002)	0.002 (0.002)
Collections	3.681	0.048*** (0.011)	0.118*** (0.015)	1.361	0.012** (0.005)	0.034*** (0.007)
Collections Balances	2363	26** (11)	76*** (15)	965	-1 (8)	7 (11)
Delinquencies	2.063	0.039*** (0.007)	0.085*** (0.011)	1.831	0.016*** (0.005)	0.04*** (0.008)
Derogatories	1.579	0.017*** (0.005)	0.048*** (0.008)	1.286	0.013*** (0.005)	0.029*** (0.006)
Any Job in Employment Data	0.172	-0.01*** (0.002)	-0.015*** (0.002)	0.17	-0.002** (0.001)	-0.004*** (0.001)
Individuals			198664			315340
Observations			3178624			5045440

Notes: Table reports coefficients on interactions between a treatment indicator and event time indicators for $\tau = 3$ and $\tau = 6$ from matched DD regressions (equation 1). All regressions include individual, time, and event time fixed effects. Regressions are estimated separately for drivers above (columns 1-3) and below (columns 4-6) the median baseline estimated income ($26,000). Means (columns 1 and 4) are control group means at baseline for the indicated income group. Standard errors are clustered at the individual level.

Figure 4: Heterogeneous Effects of Fines on Collections
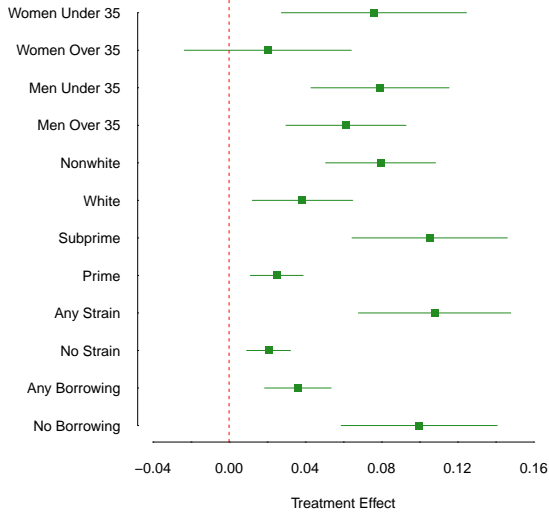
(a) Impacts by Salient Groups   (b) Impacts by Baseline Financial Strain
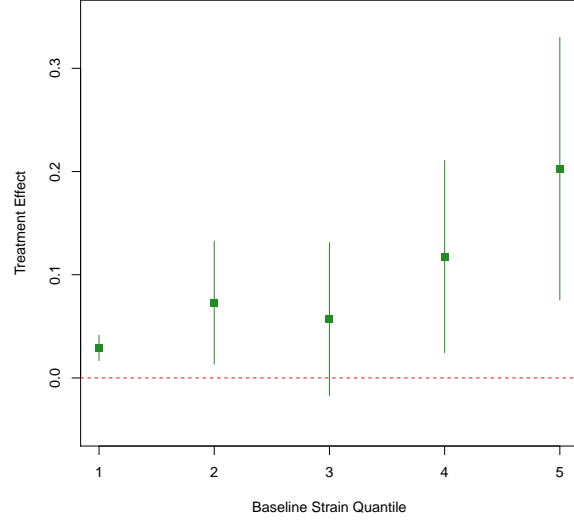
Notes: Panel (a) plots 6-quarter ($\tau = 6$) estimated treatment effects (and 95% confidence bands) where the outcome of interest is collections. Each coefficient is from a separate regression estimated using only the denoted subgroup and using the parametric matched DD estimating equation (equation 2). Subprime indicates individuals with credit scores below 600 at baseline. Panel (b) plots identical coefficients estimated separately by quantile of baseline collections balances, where baseline collections balances proxies for baseline financial strain. The quantiles are deciles where the first six deciles are grouped together as one quantile (because the 55th percentile of baseline collections balances is zero).

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Figure 5: Impacts of Fines in Sample with Clean Payment History

(a) DD: Any Distress Incident to Date   (b) Impacts by Salient Groups
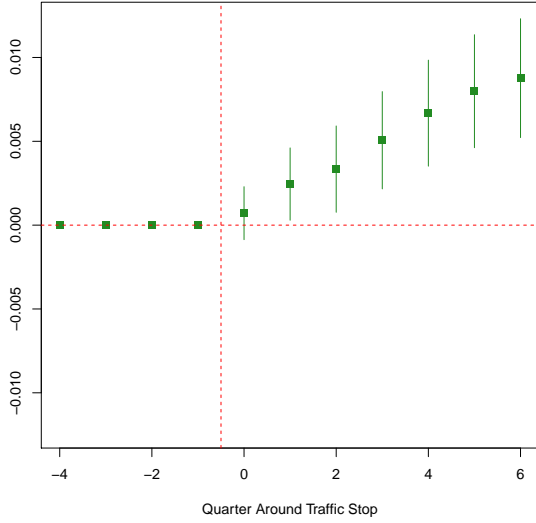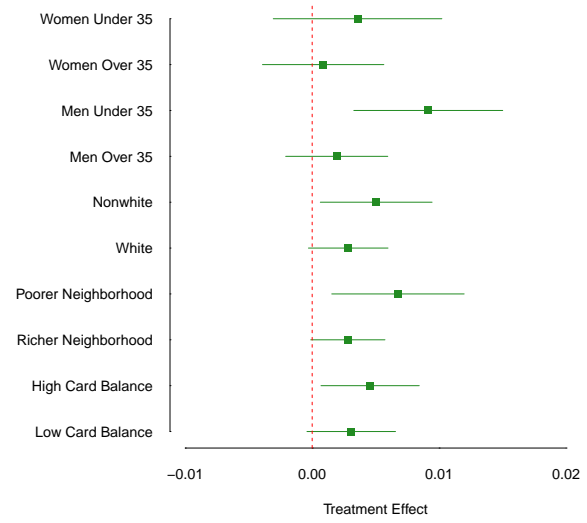
Notes: Panel (a) plots coefficients (and 95% confidence bands) on interactions between a treatment indicator and event time indicators from matched DD regressions (equation 1) where the outcome of interest is whether an individual has ever had a new financial distress incident appear on a credit report. The sample is the set of individuals with clean payment histories through the quarter prior to treatment (167,158 individuals). Panel (b) plots 4-quarter ($\tau = 4$) estimated treatment effects (and 95% confidence bands) where the outcome of interest is any new financial distress. Each coefficient is from a separate regression estimated using only the denoted subgroup and using the parametric matched DD estimating equation (equation 2). Richer/poorer neighborhoods are zip codes with above/below median per-capita income and high/low card balances refer to revolving balanes above and below the median.

Figure 6: Impact of Fines for Low-Income Drivers in Alternate Specifications

(a) Any New Financial Distress

(b) Collections

(c) Collections Balances

(d) Any Job in Employment Data

Notes: This figure compares estimates across four empirical specifications, focusing on below-median income drivers. The green solid line indicates the main matched DD estimate (same as figures 2 and 3). The blue diamonds denote matched DD estimates when also matching on auto loan histories (see text for additional details). The red x marks denote event study estimates (equation 3). The orange + marks denote event study estimates that also control for quarters since an auto purchase.

Table 3: Impacts of Fines For Low-Income Drivers Across Specifications

	Matched DD		Event Study	
	(1) Base	(2) + Auto History	(3) Base	(4) + Auto Event
Any New Distress	0.019***	0.016***	0.02***	0.018***
	(0.003)	(0.003)	(0.001)	(0.001)
Collections	0.118***	0.131***	0.096***	0.085***
	(0.015)	(0.018)	(0.008)	(0.008)
Collections Balances	76***	93***	75***	64***
	(15)	(18)	(8)	(8)
Delinquences	0.085***	0.041***	0.076***	0.052***
	(0.011)	(0.01)	(0.006)	(0.006)
Derogatories	0.048***	0.013	0.051***	0.033***
	(0.008)	(0.009)	(0.005)	(0.005)
Any Job in Employment Data	-0.015***	-0.018***	-0.005***	-0.004***
	(0.002)	(0.002)	(0.001)	(0.001)
Individuals	198664	134922	382463	382463
Observations	3178624	2158752	12238816	12238816

Notes: This table presents six-quarter ($\tau = 6$) effect estimates across four empirical specifications, focusing on below-median income drivers (estimates shown are 4-quarter estimates for any new distress). Column 1 reports estimates from the main matched DD specification. Column 2 reports estimates from matched DD estimates where drivers are also matched on auto loan histories (see main text for details). Column 3 reports estimates from event study specifications and Column 4 reports estimates from event study specifications where quarters since auto purchase is included in the regression. Standard errors are clustered at the individual level. Auto loan history matched DD estimates for all outcomes and both income groups are presented in figure B-5. Event study estimates for all outcomes and both income groups are presented in figure A-7.

Table 4: Impacts for Low-Income Fine Payers

	(1) All	By Disposition		
		(2) Paid	(3) School	(4) Difference
Collections	0.107***	0.173***	0.099*	-0.074
	(0.025)	(0.047)	(0.057)	(0.074)
Control Mean	3.65	4.312	2.905	-
Collections Balances	55*	104**	56	-48
	(29)	(50)	(64)	(81)
Control Mean	2663	2663	1833	-
Any Job in Employment Data	-0.02***	-0.023***	-0.015	0.007
	(0.004)	(0.007)	(0.011)	(0.013)
Control Mean	0.175	0.176	0.175	-
Individuals	198664	72190	31108	
Observations	3178624	1155040	497728	

Notes: This table presents six-quarter ($\tau = 6$), parametric matched DD (equation 2) estimates for below-median income drivers. Column 1 presents estimates for the full sample of below-median income drivers (same as Table 2). Column 2 presents estimates for the subset of drivers that can be designated as paying their fines. Column 3 presents estimates for the subset of drivers that can be designated as electing traffic school, which also requires on-time fine payment. Column 4 displays the difference in effects between payers and school attendees. Graphical versions of the estimates are presented in figure B-6.

Figure 7: Impact of Fines for Low-Income Drivers by Baseline Credit Card Status
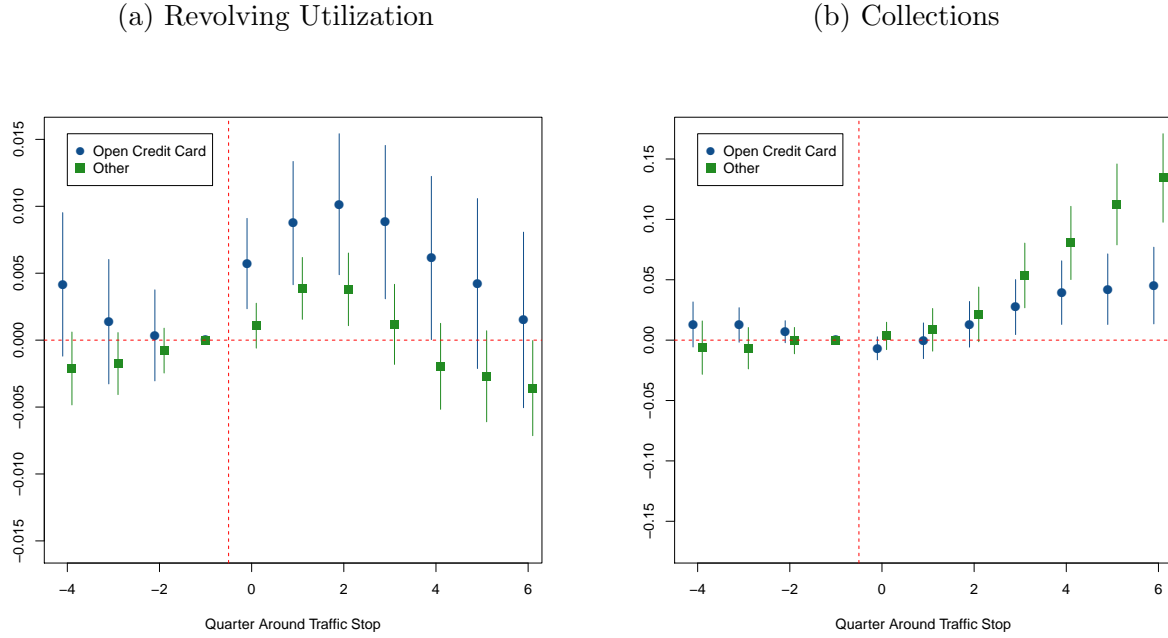
(a) Revolving Utilization

(b) Collections

*N*otes: This figure plots coefficients (and 95% confidence bands) on interactions between a treatment indicator and event time indicators from matched DD regresions (equation 1) for below-median income drivers. All regressions include individual, time, and event time fixd effects. Regresions are estimated separately for groups of drivers based on baseline credit card status. The blue dots (*open credit card*) denote estimates for drivers who had an open credit card with at least $200 in available credit at baseline. The green dots denote estimates for all others. *Revolving utilization* is computed as the across-account sum of revolving account balances divided by the across-account sum of revolving credit limits. Utilization is set to zero when there are no open revolving accounts and takes a maximum value of one. Confidence bands are constructed from standard errors clustered at the individual level.

Table 5: Impacts for Low-Income Drivers by Baseline Credit Card Status

| | | By Baseline Credit Card Status | | |
| | (1) | (2) | (3) | (4) |
	All	Open	Other	Difference
Revolving Utilization	0.002	0.009***	0.001	0.008**
	(0.001)	(0.003)	(0.002)	(0.003)
Control Mean	0.191	0.401	0.131	-
Collections	0.118***	0.045***	0.134***	-0.089***
	(0.015)	(0.016)	(0.019)	(0.025)
Control Mean	0.86	0.86	4.455	-
Collections Balances	76***	26	85***	-59**
	(15)	(18)	(18)	(25)
Control Mean	2871	512	2871	-
Any Job in Employment Data	-0.015***	-0.012***	-0.016***	0.003
	(0.002)	(0.004)	(0.002)	(0.004)
Control Mean	0.175	0.181	0.169	-
Individuals	198664	46625	152039	
Observations	3178624	746000	2432624	

Notes: This table corresponds to figure 7 and reports six-quarter (four-quarter in the case of utilization) matched DD estimates (equation 1) for below-median income drivers. Column 1 reports results for the full-sample. Column 2 reports results for the subset of drivers who had an open credit card with at least $200 in available credit at baseline. Column 3 reports results for all other drivers. Column 4 reports the difference between columns 3 and 4. At baseline, the average credit score in the open-card and other samples are 650 (prime) 544 (subprime). The average borrowing limit in the open-card sample is $3,999. Standard errors are clustered at the driver level.

Figure 8: Speeder Difference-in-Differences Design

(a) Speeding Punishment Schedule
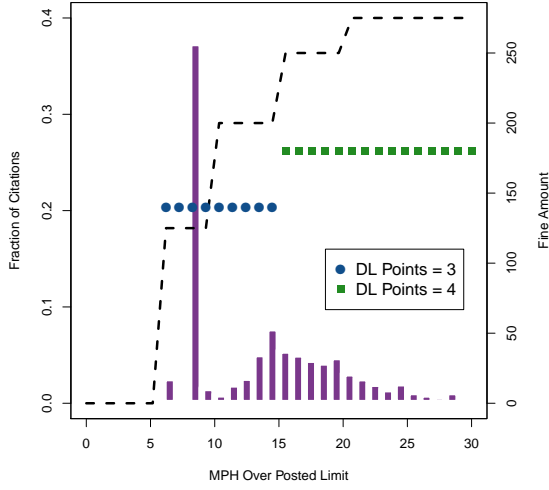
(b) Raw Data: Collections
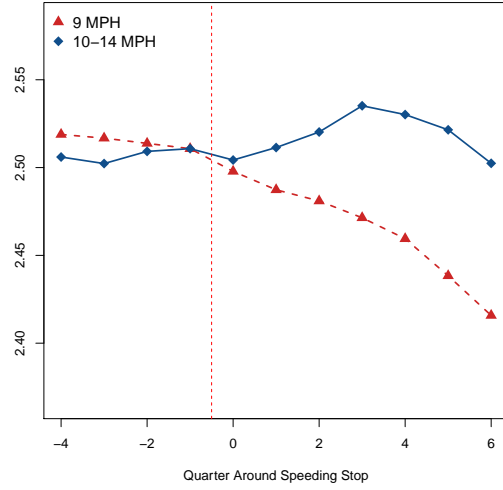
(c) DD Estimates: Collections
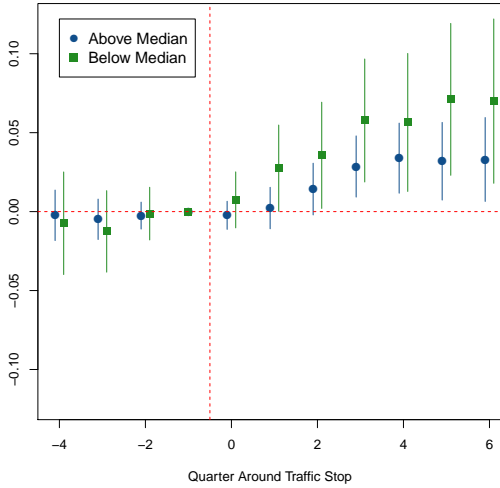
(d) DD Estimates: Collections Balances
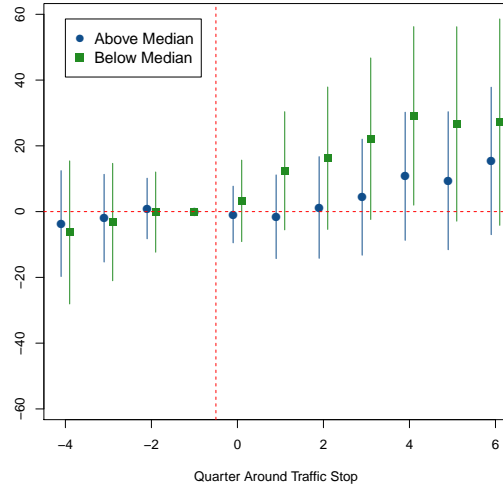
Notes: Panel (a) plots a histogram of charged speeds on speeding tickets (purple bars) as well as the fine (dotted black line) and license point (blue dots and green squares) schedule as a function of the charged speeds. Panel (b) plots average collections around the time of a traffic stop for drivers with charged speeds of 10-14 MPH (blue diamonds) and 9 MPH (red triangles) over the posted limit. Panels (c) and (d) plot coefficients and 95% confidence bands on indicators between a treatment (10-14 MPH) indicator and event time indicators using the speeder dsample. Regressions also include individual, time, and event time fixed effects.

Table 6: Impact of Additional $75 Fine, Speeder Design

	Below Median Income			Above Median Income		
	(1) Control Mean	(2) 3 Quarters	(3) 6 Quarters	(4) Control Mean	(5) 3 Quarters	(6) 6 Quarters
Any New Distress	0.238	-0.003 (0.004)	-0.007 (0.005)	0.123	0.003 (0.003)	-0.001 (0.003)
Collections	3.991	0.058*** (0.02)	0.07*** (0.027)	0.92	0.029*** (0.01)	0.033** (0.014)
Collections Balances	1980	22* (13)	27* (16)	474	4 (9)	15 (11)
Delinquencies	2.458	0.012 (0.014)	0.016 (0.021)	1.392	0.001 (0.011)	-0.008 (0.016)
Derogatories	1.887	0.003 (0.011)	0.006 (0.016)	0.911	0.002 (0.01)	-0.001 (0.013)
Any Job (Employment Data)	0.189	0.003 (0.003)	0.002 (0.003)	0.168	-0.001 (0.002)	0.001 (0.002)
Individuals		98545			101044	
Observations		3153440			3233408	

Notes: Table reports coefficients on interactions between a treatment (charged speed of 10-14 MPH) indicator and event time indicators for $\tau = 3$ and $\tau = 6$ from speeder DD regressions (equation 1). All regressions include individual, time, and event time fixed effects. Regressions are estimated separately for drivers above (columns 1-3) and below (columns 4-6) the median baseline estimated income ($26,000). Means (columns 1 and 4) are control group means at baseline for the indicated income group. Standard errors are clustered at the individual level.

Table 7: Accounting for Fines

	Below Median Income			Above Median Income		
	(1) Baseline	(2) Conservative	(3) Other	(4) Baseline	(5) Conservative	(6) Other
Amount Covered By:						
Missed Bills	76	47	131	7	0	33
Formal Borrowing	19	10	27	0	0	0
Reduced Consumption	23	15	23	29	24	29
Total	118	72	181	36	24	62
Remainder	72	118	9	154	166	128
Implied Cash Share	0.38	0.62	0.05	0.81	0.87	0.67

Notes: This table corresponds to the accounting exercise in section 6.1 and reports estimated funding sources for covering a \$190 traffic fine. Columns 1-3 (4-6) report estimates for the below-median (above-median) income sample. For funds from missed bills, the baseline strategy uses the six-quarter treatment effect estimate for collections balances.The conservative strategy uses the lower 95% CI of that estimate. The other strategy uses (i) the point estimate for collections balances plus (ii) the point estimate for delinquencies scaled by the ratio of collections to collections balances to impute a dollar value. For funds from formal borrowing, I use the two-quarter treatment effect estimate for revolving utilization. The conservative approach uses lower 95% CI and the other approach uses a directly estimated balance effect. For funds from consumption, the baseline strategy adds treatment effect estimates for collections, delinquencies, and derogatories and scales by the Pattison (2020) estimate. The conservative strategy uses the lower 95% CI of a treatment effect estimate on the sum of collections and delinquencies, scaled in the same way.

A Appendix Figures and Tables

Figure A-1: Potential outcomes associated with a standard moving violation
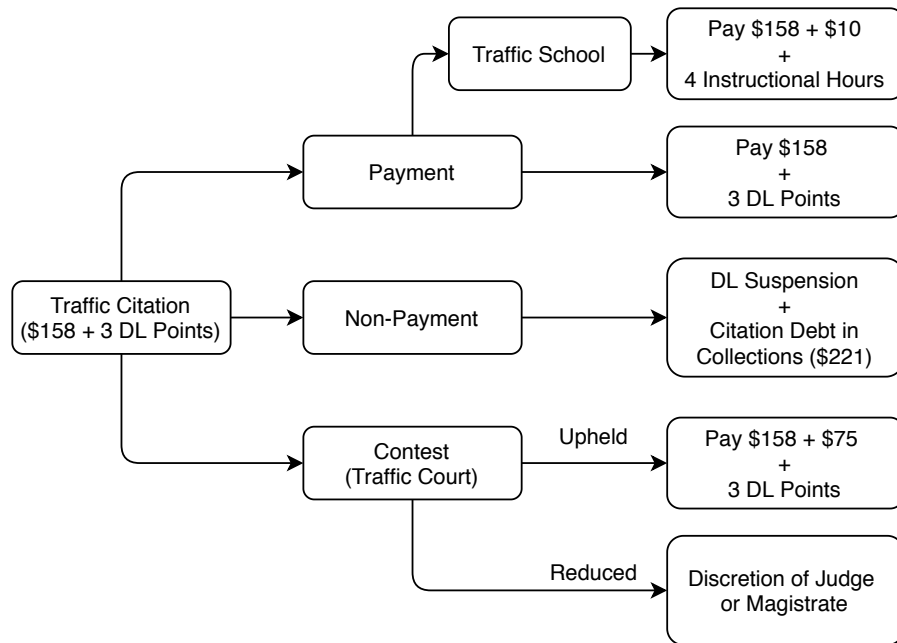
Notes: This figure provides a flow chart summarizing driver choices and the associated *treatment* stemming from each choice. The $10 surcharge for traffic school attendees represents the typical net surchage, $25 for the course minus a $15 fine reduction. The citations debt in collections ($221) for nonpayers assumes a 40 percent collection fee, the maximum allowed by law. Note that such collections activity typically will not appear on the credit reports used in the empirical analysis. The $75 surcharge for contestors is a standard court fee.

Figure A-2: Credit File Match Rate by Zip Code Income
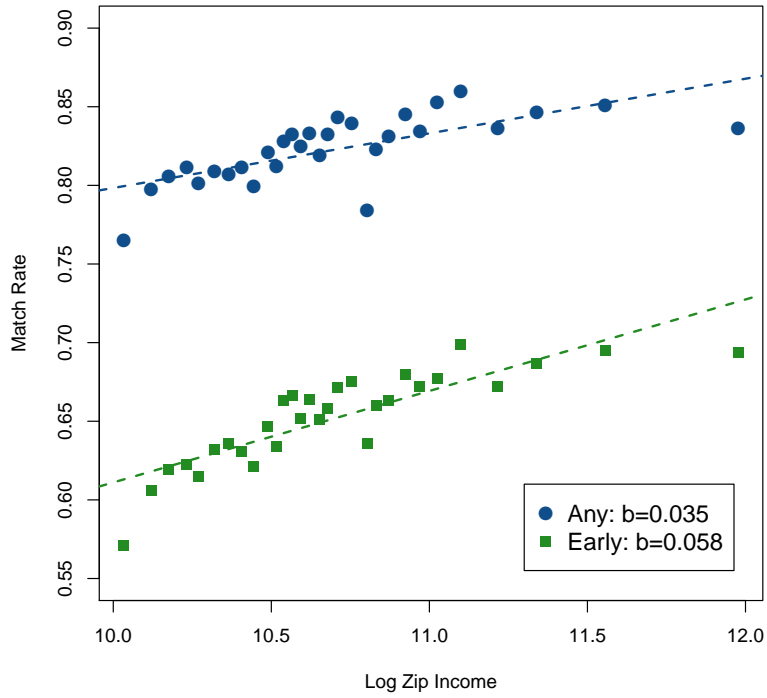
*N*otes: This figure plots the share of citations successfully matched to the credit file in each bin of log zip code income. Blue circles (*any*) indicate whether the citation was matched at all. Green squares (*early*) indicate whether the citation was matched to a driver present on the credit file as of January 2010. Dashed lines indicator linear fits (coefficients reported in the figure legend).

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

	Any Match		Early Match	
	(1)	(2)	(3)	(4)
Female	0.044***	0.043***	0.06***	0.059***
	(0.001)	(0.001)	(0.002)	(0.002)
Age <18	-0.154***	-0.152***	-0.534***	-0.533***
	(0.005)	(0.004)	(0.007)	(0.006)
Age 25-34	0.07***	0.069***	0.2***	0.2***
	(0.002)	(0.002)	(0.006)	(0.006)
Age 35-44	0.098***	0.097***	0.241***	0.24***
	(0.004)	(0.004)	(0.008)	(0.008)
Age 45-54	0.107***	0.106***	0.256***	0.255***
	(0.004)	(0.004)	(0.008)	(0.008)
Age 55+	0.121***	0.121***	0.276***	0.276***
	(0.006)	(0.007)	(0.011)	(0.011)
Black	-0.017***	-0.02***	-0.02***	-0.024***
	(0.005)	(0.002)	(0.004)	(0.002)
Hispanic	-0.028***	-0.035***	-0.041***	-0.048***
	(0.006)	(0.005)	(0.006)	(0.006)
Other/Unknown	0.002	-0.006	-0.002***	-0.009
	(0.007)	(0.007)	(0.008)	(0.008)
Log Zip Income	0.025***	0.03***	0.028***	0.034***
	(0.005)	(0.003)	(0.005)	(0.002)
Mean	0.823	0.823	0.652	0.652
R2	0.022	0.026	0.09	0.094
County FE	No	Yes	No	Yes
Time FE	No	Yes	No	Yes
N	8851688	8851688	8851688	8851688

Notes: This table presents regressions estimated at the citation level. *Any Match* refers to whether the driver was matched to the credit file at any point. *Early Match* refers to whether the driver was matched and on the credit file as of January 2010. Ages 18-24 and white are the excluded age/race categories. County and time fixed effects are for the county and year × month of the traffic stop. Standard errors are clustered at the county level.

Figure A-3: Timeline for Matched Difference-in-Differences Analysis
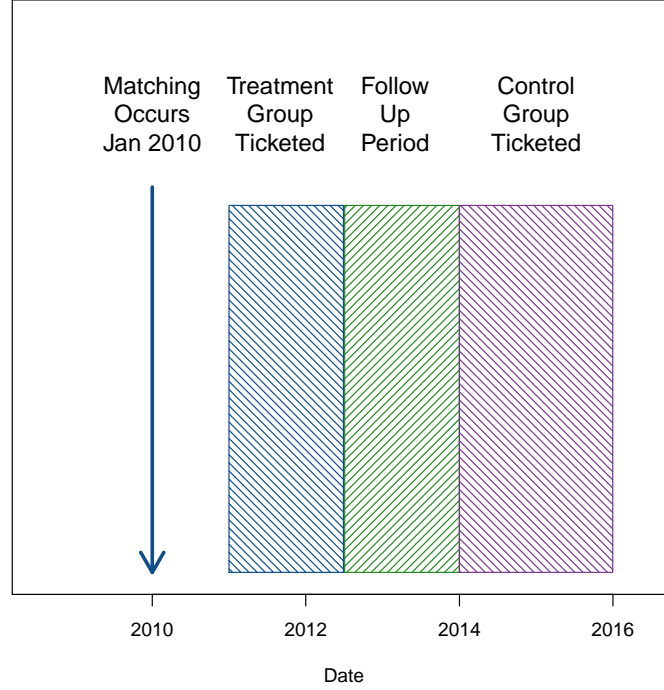
Notes: This figure illustrates the timeline for the matched difference-in-differences strategy. Treatment group individuals are those receiving their first citation during 2011Q1-2012Q2. Control group individuals are those receiving their first citation during 2014Q1-2015Q4. The period 2012Q3-2013Q4 is the follow-up period, where the treatment group has received treatment and the control group has not. Only data from 2010Q1 and 2013Q4 are used in the estimation. Treatment and control individuals are matched based on characteristics as of January 2010, the first period of credit report data.

Figure A-4: Trends in Collections Around Traffic Stops by Violation Type
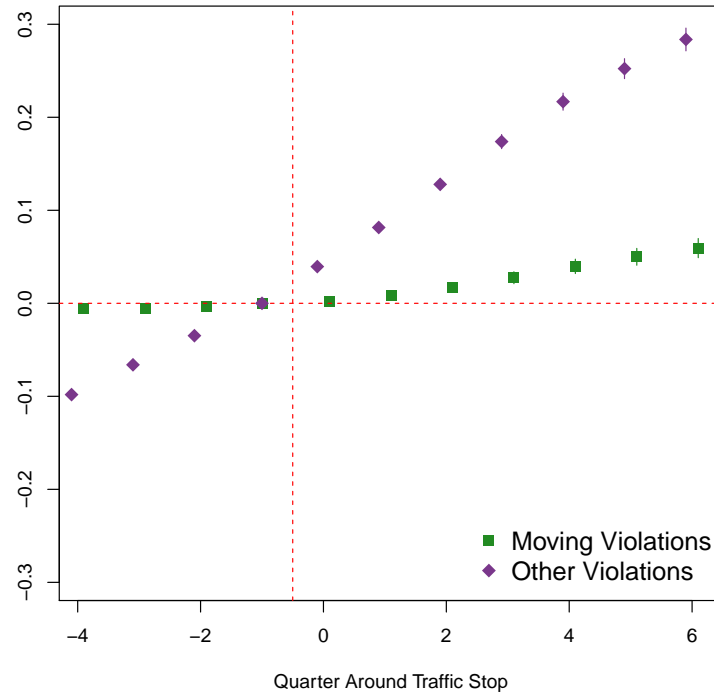
Notes: This figure plots event study estimates (equation 3) around the time of a moving violation (green squares) and non-moving violation (purple diamonds) traffic stop. The outcome of interest is the number of collections on file. The moving violations sample is the event study sample (983,206 individuals). The non-moving violations sample is comprised of 1,114,083 individuals with a single-violation non-moving-violation traffic stop over the period 2011-2015.

Figure A-5: Trends in Car Ownership Around Traffic Stops

(a) Any Auto Loan Around Traffic Stop   (b) Credit Score Around Auto Purchases
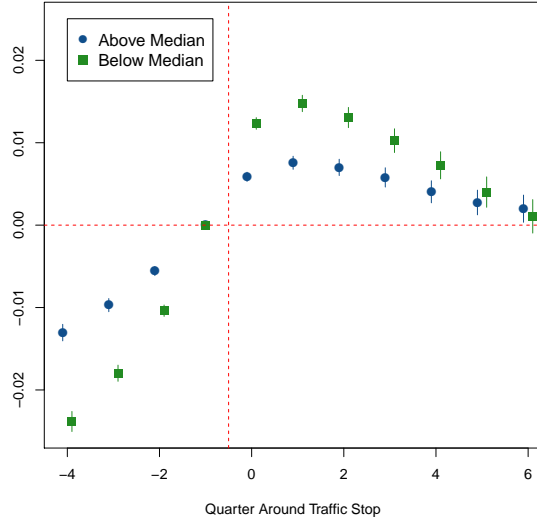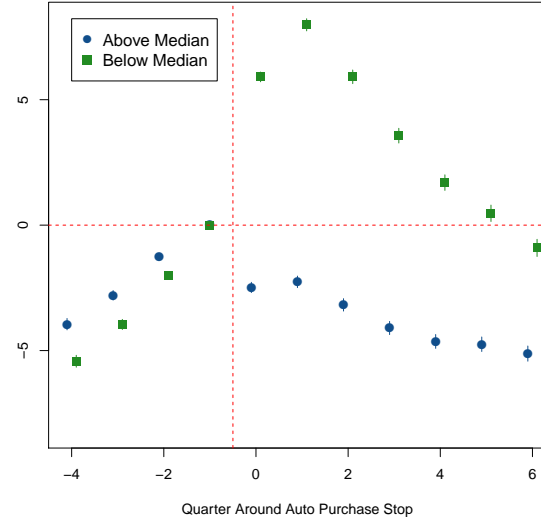
*N*otes: Panel (a) plots event study estimates (equation 3) using the event study sample where the outcome of interest is the presence of an open auto loan on the credit file. Panel (b) plots coefficients on quarter around the timing of an auto purchase from event study regressions (also including quarter around traffic stop indicators) where the outcome of interest is the credit score. Error bars are 95% confidence intervals constructed from standard errors clustered at the individual level.

Table A-2: Moving Violations in Main Sample

| | | Frequency | | Punishments | |
| | | (1) | (2) | (3) | (4) |
Code	Description	N	Fraction	Points	Fine
575	Unlawful Speed (6-9 Mph)	269850	0.274	3	123
575	Unlawful Speed (15-19 Mph)	180634	0.184	4	248
575	Unlawful Speed (20-29 Mph)	126349	0.129	4	273
532	Traffic Control Device - Fail To Obey	91962	0.094	3	158
575	Unlawful Speed (10-14 Mph)	82649	0.084	3	198
537	Stop Sign - Fail To Stop	42722	0.043	3	158
586	Unlawful Speed - Work Zone	29945	0.03	3	256
545	Traffic Signal - Fail To Stop	24253	0.025	4	256
511	Fail To Yield To Emergency/Etc Vehicle	17217	0.018	3	158
573	Unlawful Speed	15602	0.016	3	256
455	Careless Driving	15086	0.015	3	158
593	Improper Turn	14291	0.015	3	158
332	Fail To Use Designated Lane	9069	0.009	3	158
543	Traffic Signal - Fail To Stop	7071	0.007	4	256
271	Following Too Closely	5777	0.006	3	158
331	Improper Change Of Lane	4590	0.005	3	158
411	Improper Passing - No Passing Zone	4409	0.004	3	158
516	Traffic Signal - Fail To Obey	3995	0.004	3	158
632	Wrong Side Of Roadway	2574	0.003	3	158
415	School Bus - Fail To Stop	2538	0.003	4	263
336	Traffic Control Device - Avoid	2482	0.003	3	158
513	Fail To Yield	2244	0.002	3	158
533	Traffic Sign - Avoid	2191	0.002	3	158
514	Traffic Signal - Fail To Yield	2061	0.002	3	158
551	Improper Turn - Improper Signal	2045	0.002	3	158
412	Improper Passing	1780	0.002	3	158
437	Special Hazard - Fail To Use Due Care	1727	0.002	3	158
403	Refuse To Obey Traffic Laws	1673	0.002	3	158
512	Stop Sign - Fail To Yield	1652	0.002	3	158
572	Unlawful Speed - Too Fast For Conditions	1634	0.002	3	158
392	Improper Backing	1551	0.002	3	158
413	Improper Change Of Lane	1498	0.002	3	158
631	Wrong Direction	1435	0.001	3	158
546	Traffic Signal - Fail To Yield	1238	0.001	3	158
396	Blocking Intersection Or Crosswalk	1163	0.001	3	158
587	Fail To Use Due Care	824	0.001	3	158
535	Safety Zone - Driving Through	790	0.001	3	158
417	Improper Passing - Cutting In	756	0.001	3	158
585	Impeding Traffic	570	0.001	3	158
540	R/R - Fail To Obey Traffic Signal	551	0.001	3	198
334	Driving On Sidewalk/Bicycle Path	392	0	3	158
398	Improper Stopping/Standing/Parking - Roadways	364	0	3	158
422	Improper Passing - Intersection	319	0	3	158
420	Improper Passing - Other	220	0	3	158
589	Excessive Speed	208	0	4	256
526	Improper Passing - Cutting In	183	0	3	158
580	Special Hazard - Too Fast For Conditions	179	0	3	158
548	Traffic Signal - Fail To Yield	174	0	3	158
416	Improper Passing - Fail To Yield	169	0	3	158
633	Wrong Direction	120	0	3	158
538	Inoperative Traffic Light - Fail To Stop	115	0	3	158
574	Unlawful Speed - Below Minimum	76	0	3	158
418	Improper Passing - Hill	52	0	3	158
414	School Bus - Improper Pass	48	0	4	263
423	Improper Passing - Curve	48	0	3	158
421	Improper Passing - Obstructed View	35	0	3	158
660	Left Lane Impeding Traffic	33	0	3	158
419	Improper Passing - Intersection	20	0	3	158
456	Improper Passing - Due Care	2	0	3	158
523	Unlawful Speed - Special Zone	1	0	3	256

Notes: This table reports the distribution of violation codes in the event-study sample.

Table A-3: Summary Statisics Across Samples

	(1) Florida	(2) Drivers Matched to Credit File	(3) Initial Sample	(4) Event Study
Panel A: Demographics				
Female	0.51	0.43	0.44	0.44
Nonwhite	0.41	0.61	0.6	0.55
Age	40.3	37.16	36.73	36.55
Credit File Age	–	11.5	12.95	13.13
Credit Score	662	614	603	621
Estimated Income	32000	27490	31664	34548
Panel B: Financial Distress				
Collections		2.14	2.81	2.32
Collections Balances		1534	1994	1655
Delinquencies		1.68	2.19	1.98
Derogatories		1.24	1.61	1.44
Panel C: Credit Usage				
Any Account		0.7	0.8	0.85
Revolving Accounts		2.65	2.96	3.38
Revolving Balances		3930	4567	5442
Any Auto Loan		0.28	0.35	0.38
Any Mortgage		0.23	0.27	0.32
Individuals	14,800,000	3683016	2631641	983206

Notes: This table reports summary statistics as of January 2010 across samples. Column 1 reports statewide means computed from the 2010 ACS or provided by the credit bureau. Column 2 reports means for all drivers ever matched to the credit bureau. Column 3 reports means for drivers in the initial sample (present on credit file, aged 18-59, nonmissing credit score, nonmissing esimated income as of January 2010). Column 3 reports means for the event study sample (all drivers in the initial sample with a single-violation moving-violation traffic stop). Minor discrepancies between means in this table and table 1 are because this table uses monthly data whereas table 1 uses aggregated quarterly data.

Figure A-6: Impact of Fines on New Distress Incidents by Incident Type
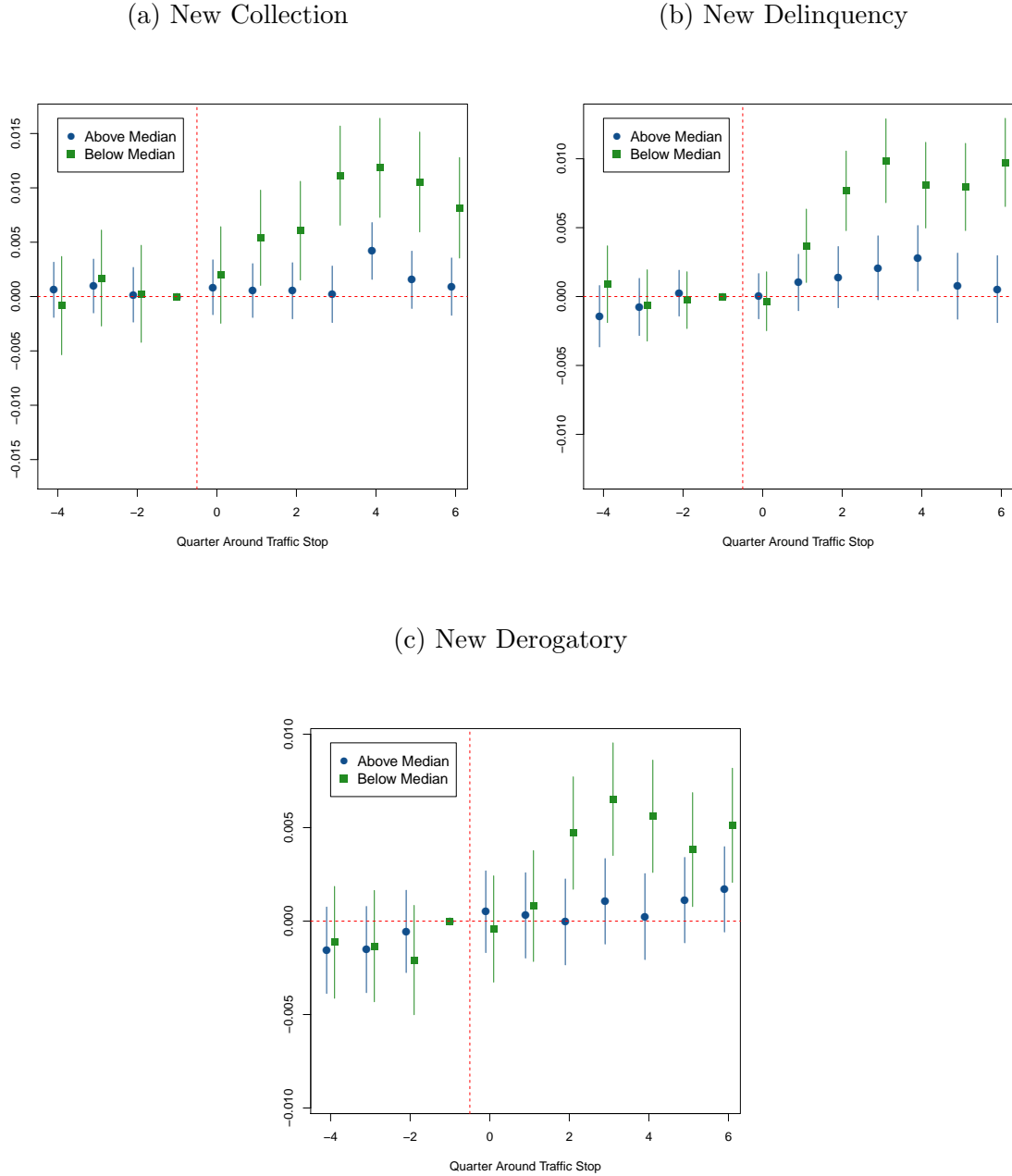
(a) New Collection

(b) New Delinquency

(c) New Derogatory

Notes: This figure plots coefficients (and 95% confidence bands) on interactions between a treatment indicator and event time indicators from matched DD regressions (equation 1). Coefficients are normalized to $\tau = -1$. All regressions include individual, time, and event time fixed effects. Regressions are estimated separately for drivers above (blue circles) and below (green squares) the median baseline esimated income ($26,000). Confidence intervals are constructed from standard errors clustered at the individual level.

Figure A-7: Event Study Estimates By Income

(a) Any New Distress
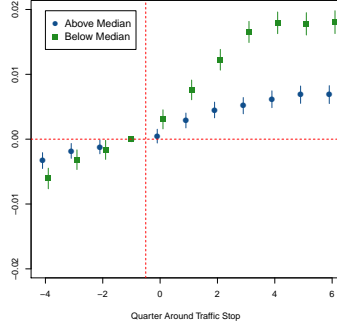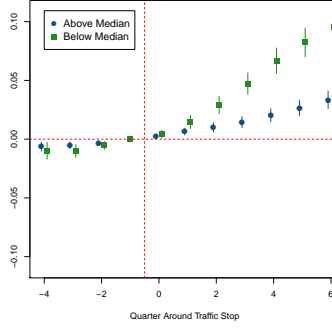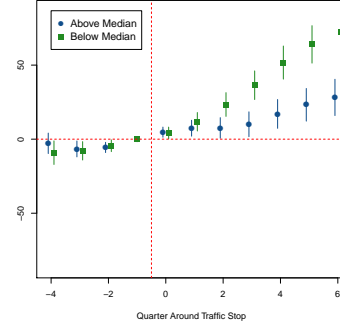
(b) Collections

(c) Collections Balances

(d) Delinqs + Derogs
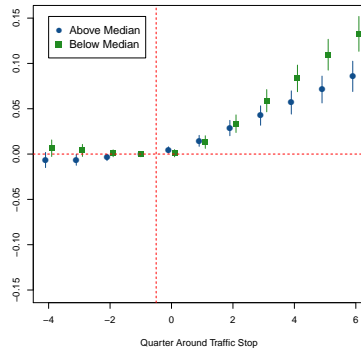
(e) Any Job in Emp Data
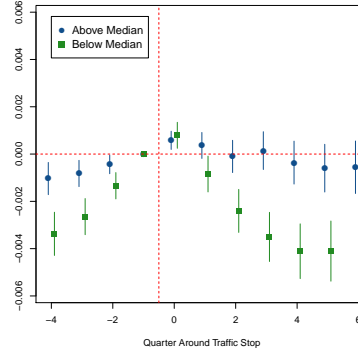
Notes: Each figure plots coefficients and 95% confidence bands on event time indicators from event study regressions (equation 3). Regressions also include individual and time fixed effects. Regressions are estimated separeately for drivers above (blue circles) and below (green circles) the baseline estimated income. Error bars are constructed from standard errors clustered at the individual level.

Figure A-8: Impact of Fines for Higher-Income Drivers in Alternate Specifications

(a) Any New Financial Distress

(b) Collections

(c) Collections Balances

(d) Any Job in Emp Data
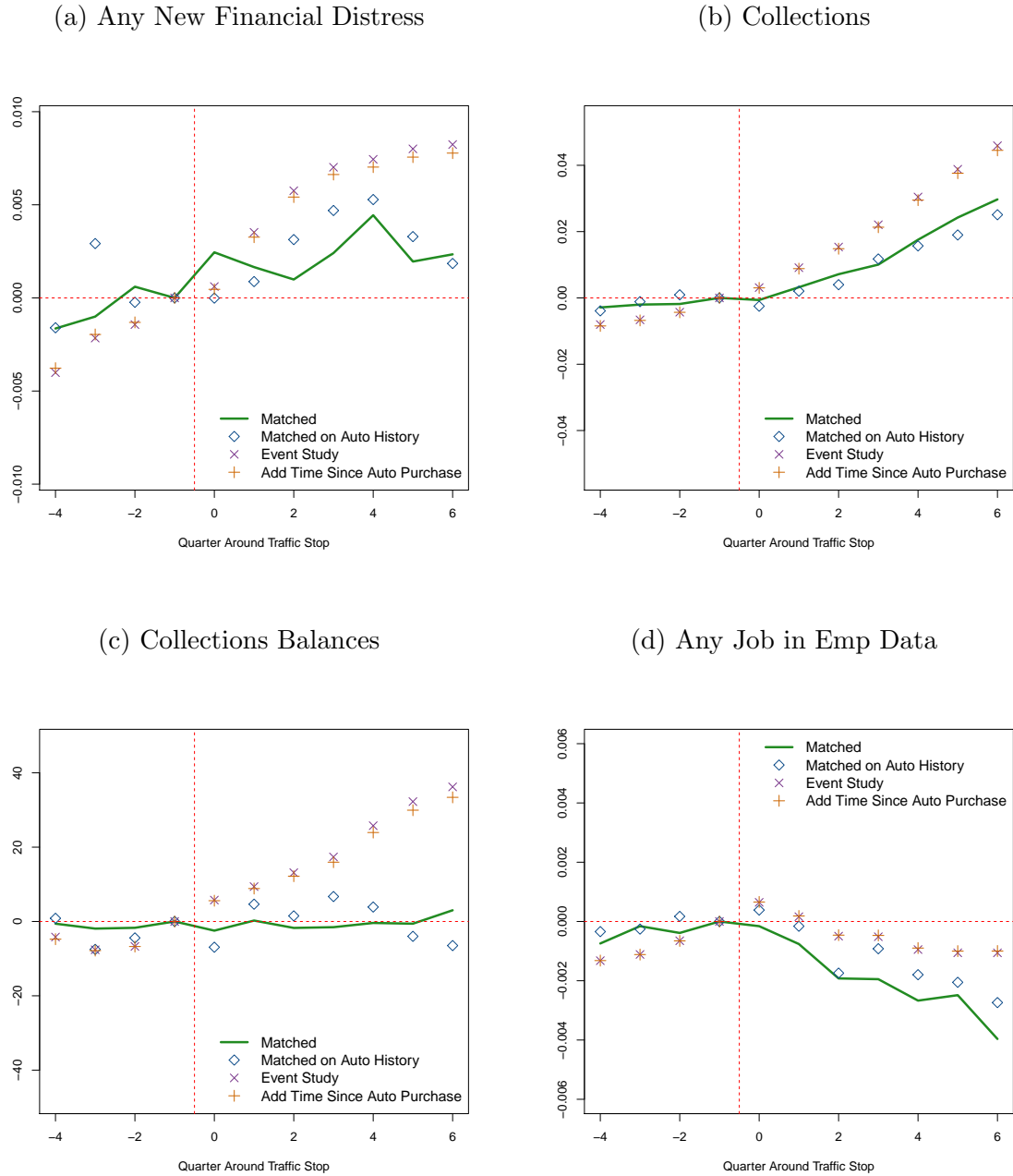
Notes: This figure compares estimates across four empirical specifications, focusing on above-median income drivers. The green solid line indicates the main matched DD estimate (same as figures 2 and 3). The blue diamonds denote matched DD estimates when also matching on auto loan histories (see text for additional details). The red x marks denote event study estimates (equation 3). The orange + marks denote event study estimates that also control for quarters since an auto purchase.

Figure A-9: Speeder DD Estimates by Driver Income for Other Outcomes
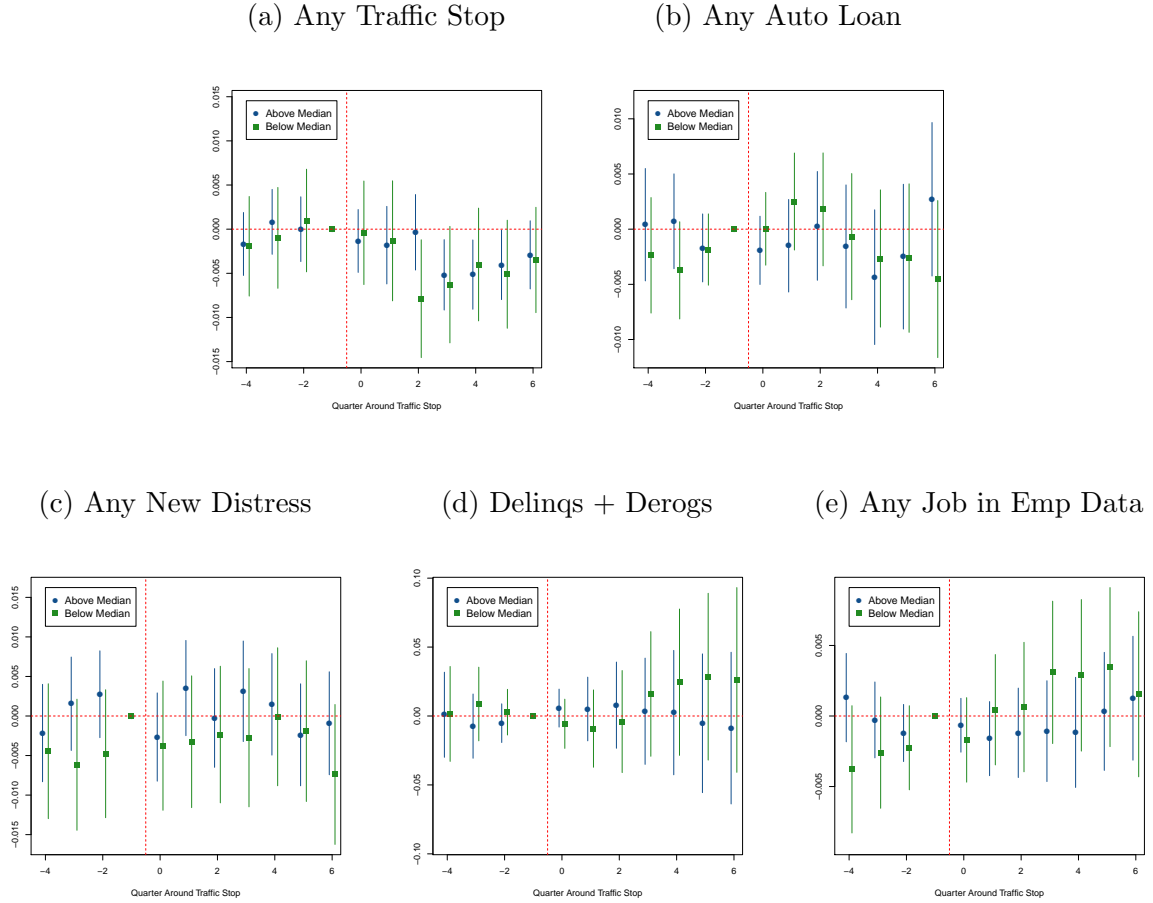
(a) Any Traffic Stop

(b) Any Auto Loan

(c) Any New Distress

(d) Delinqs + Derogs

(e) Any Job in Emp Data

Notes: Each figure plots coefficients and 95% confidence bands on indicators between a treatment (10-14 MPH) indicator and event time indicators using the speeder design sample (drivers with speeding tickets for speeds 9-14 MPH over the limit who pay their fines). Regressions also include individual, time, and event time fixed effects. Regressions are estimated separataely for drivers above and below the median baseline income. Confidence intervals are constructed from standard errors clustered at the individual level.

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B Matching Appendix

Matching algorithm

The algorithm for matching treatment and control drivers works as follows:

1. Define a set of matching characteristics.

2. Store all exact matches on the set of matching characteristics.

3. For each control driver, randomly select one treatment driver from the list of matches. The list of matches is now unique in the control driver identity.

4. For each treatment driver, randomly select one control driver from the list of matches. The list of matches is now unique in both the treatment and control driver identify (i.e. 1-to-1 match).

Matching algorithm when including auto loan histories

The matching algorithm requires a slight modification when matching on auto loan histories. Treatment candidates are ticketed in six possible quarters (2011Q1-2012Q2). When incorporating the auto loan history into the match, the matching algorithm works as follows:

1. Select all treatment candidates cited in a given quarter (e.g., 2011Q1).

2. Compute the relevant auto loan history for those treatment candidates. For example, if the treatment quarter is 2011Q1, I construct an auto history over the period 2010Q1-2011Q1 (5 quarters). If the treatment quarter is 2012Q2, I construct an auto history over the period 2010Q1-2012Q2 (10 quarters).

3. Select *all* control candidates.

4. Compute same auto loan history (e.g., first 5 quarters) for all control candidates.

5. Find all treatment control matches that match exactly on the original matching criteria plus the computed auto loan history.

6. Store the set of all matches for the given treatment quarter. Loop over all treatment quarters.

7. Append all matches.

8. Randomly select only one match per control candidate.

9. Ramdonly select only one match per treatment candidate.

Adjusting estimates for follow-up citations

I use the strategy in Cellini et al. (2010) for converting ITT into TOT estimates to adjust my matched difference-in-differences estimates for potential impacts of follow-up citations for the treatment group. The intuition of the strategy is as follows. At $\tau = 0$, everyone in the treatment group has been treated once and no one in the control group has been treated. Hence, the treatment-control difference θ_0 (estimated using equation 1) at $\tau = 0$ is both an ITT and TOT estimate. At $\tau = 1$, some share of the treatment group may have been treated twice, meaning the treatment-control difference θ_1 is too big. Individuals who are treated again at $\tau = 1$ are in the 0th quarter since their second treatment, so one can use the estimated effect of the original effect at time zero, θ_0 to rescale the estimate. Specifically, the TOT estimate at $\tau = 1$ is $\theta_1 - \pi_1\theta_0$, where π_1 is the share of the treatment group treated again at $\tau = 1$. One can then iterate this forward to the end of the follow-up period ($\tau = 6$):

$$\theta_0^{TOT} = \theta_0^{ITT}$$

$$\theta_1^{TOT} = \theta_1^{ITT} - \pi_1\theta_0^{TOT}$$
$$\theta_2^{TOT} = \theta_2^{ITT} - \pi_1\theta_1^{TOT} - \pi_2\theta_0^{TOT}$$

and so on. I use the θ_τ's from the main matched estimates as the θ_τ^{ITT}'s and estimate the π_τ's directly by estimating equation 1 where the outcome is whether the individual receives a traffic ticket in a given quarter. The share of the treatment group receiving a ticket is shown in figure B-4.

Results from this rescaling exercise are presented in table B-3, where I show six-quarter ITT and recursive TOT estimates for each outcome, separately by income group. The size of the rescaling depends on the dynamic path of the treatment effects and thus varies across outcomes. But in general, the recursive TOT estimates are between 8 and 15 percent smaller than the original ITT estimates.

Table B-1: Summary Statistics for Matching Samples

	Candidates		Matched		Matched on Auto History	
	(1) Treat	(2) Control	(3) Treat	(4) Control	(5) Treat	(6) Control
Panel A: Demographics						
Female	0.45	0.47	0.45	0.45	0.45	0.45
Nonwhite	0.57	0.53	0.55	0.55	0.55	0.55
Age	36.74	37.39	35.47	35.47	34.91	34.91
Credit File Age	13.28	13.31	12.89	12.89	12.55	12.55
Credit Score	626	613	627	626	624	624
Estimated Income	35259	32768	35250	34621	34484	33893
Panel B: Financial Distress						
Collections	2.23	2.63	2.18	2.24	2.24	2.29
Collections Balances	1650	1926	1590	1606	1630	1631
Delinquencies	1.94	2.07	1.89	1.87	1.83	1.82
Derogatories	1.4	1.53	1.37	1.36	1.33	1.33
Panel C: Credit Usage						
Any Account	0.87	0.82	0.87	0.85	0.84	0.82
Revolving Accounts	3.47	3.15	3.44	3.34	3.31	3.23
Revolving Balances	6000	5166	5908	5625	5625	5348
Any Auto Loan	0.41	0.36	0.4	0.38	0.35	0.35
Any Mortgage	0.34	0.3	0.32	0.32	0.31	0.3
Panel D: Employment Data						
Any Job	0.17	0.16	0.18	0.17	0.18	0.17
Positive Earnings	0.13	0.12	0.13	0.12	0.13	0.12
Monthly Earnings	3533	3322	3519	3477	3458	3379
Panel E: Citation Information						
Fine Amount	187.41	173.32	189.59	173.61	190.46	173.85
DL Points	3.35	1.9	3.37	1.94	3.37	1.93
Individuals	352873	662794	257002	257002	160990	160990

Notes: This table reports summary statistics as of January 2010 for the matching samples. Columns 1-2 report means for the treatment and control *candidates*. Columns 3-4 report means for treatment and control drivers in the matched DD sample. Columns 5-6 report means for treatment and control drivers in the sampled matched on auto loan histories.

Table B-2: Summary Statistics for Matched Sample by Income Group

	Below Median Income		Above Median Income	
	(1)	(2)	(3)	(4)
	Treat	Control	Treat	Control
Panel A: Demographics				
Female	0.47	0.47	0.43	0.43
Nonwhite	0.66	0.66	0.48	0.48
Age	27.38	27.38	40.56	40.56
Credit File Age	6.69	6.69	16.8	16.8
Credit Score	568	567	663	663
Estimated Income	18420	18131	45852	45010
Panel B: Financial Distress				
Collections	3.55	3.65	1.31	1.34
Collections Balances	2464	2495	1040	1046
Delinquencies	1.97	1.94	1.84	1.83
Derogatories	1.49	1.49	1.29	1.29
Panel C: Credit Usage				
Any Account	0.71	0.67	0.97	0.96
Revolving Accounts	1.12	1.03	4.9	4.8
Revolving Balances	625	567	9236	8812
Any Auto Loan	0.21	0.17	0.53	0.51
Any Mortgage	0.02	0.02	0.51	0.51
Panel D: Employment Data				
Any Job	0.19	0.17	0.17	0.17
Positive Earnings	0.14	0.12	0.13	0.13
Monthly Earnings	1570	1488	4854	4650
Panel E: Citation Information				
Fine Amount	190.65	172.84	188.91	174.08
DL Points	3.38	1.77	3.36	2.05
Individuals	99332	99332	157670	157670

Notes: This table reports summary statistics as of January 2010 for the matched samples. Columns 1-2 (3-4) report means for the treatment and control drivers in the below (above) median income group.

Figure B-1: Raw Data Plots for Below-Median Income Drivers in Matched Sample
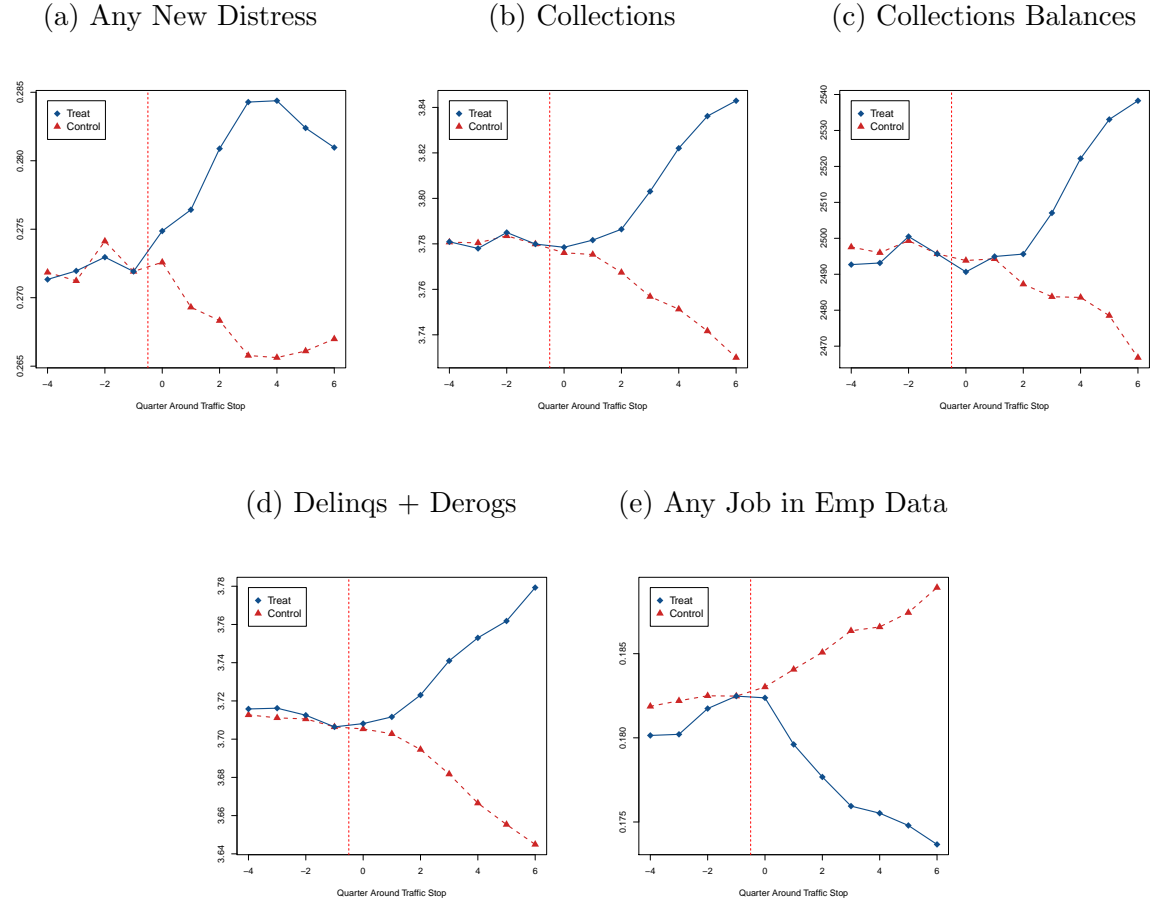
(a) Any New Distress


(b) Collections


(c) Collections Balances

(d) Delinqs + Derogs

(e) Any Job in Emp Data

Notes: Each figure plots averages for treatment (blue diamonds) and control (red triangles) around the time of the treatment group's traffic stop. The sample is below-median income drivers in the matched DD sample. Means are normalized to $\tau = -1$ and adjusted for age effects.

Figure B-2: Raw Data Plots for Above-Median Income Drivers in Matched Sample
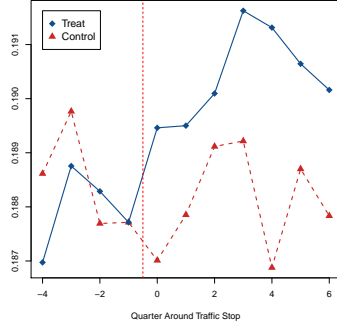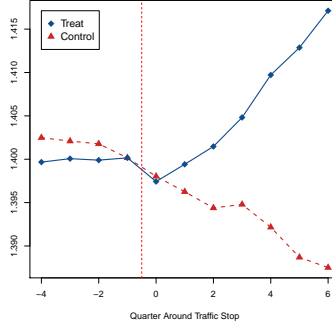
(a) Any New Distress

(b) Collections

(c) Collections Balances
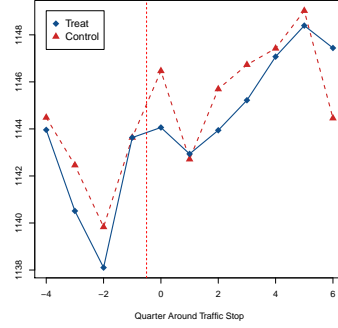
(d) Delinqs + Derogs
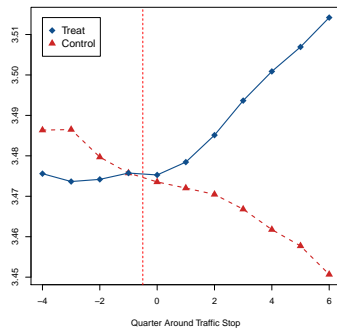
(e) Any Job in Emp Data
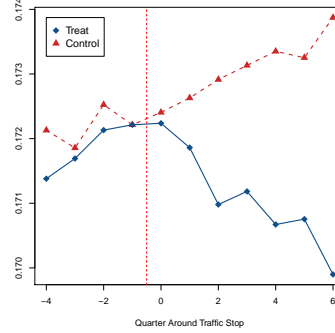
Notes: Each figure plots averages for treatment (blue diamonds) and control (red triangles) around the time of the treatment group's traffic stop. The sample is above-median income drivers in the matched DD sample. Means are normalized to $\tau = -1$ and adjusted for age effects.

Figure B-3: Heterogeneity Results for Other Outcomes

(a) Collections
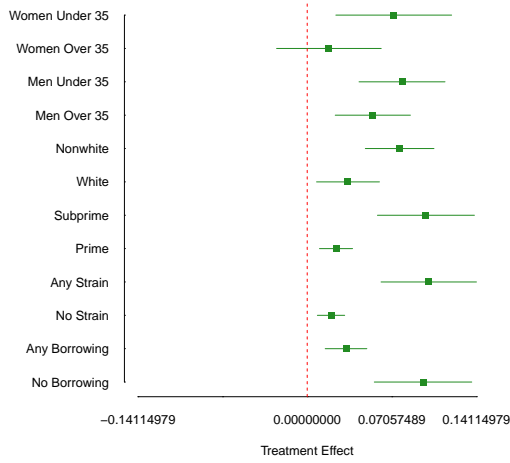
(b) Collections Balances
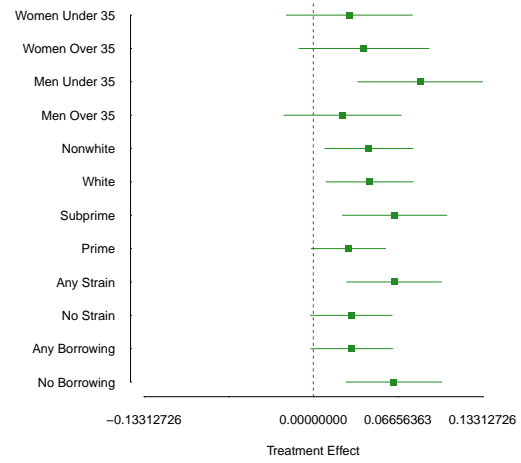
(c) Delinquencies + Derogatories
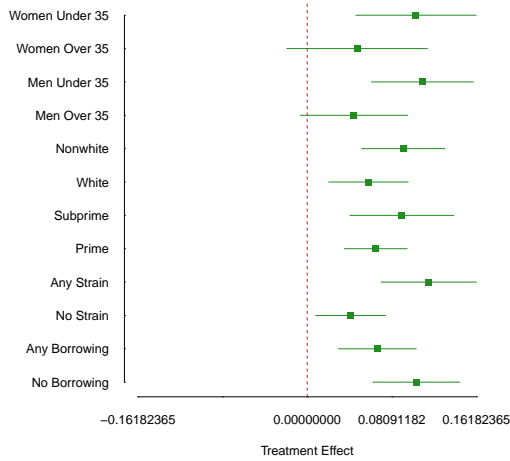
(d) Any Job in Emp Data
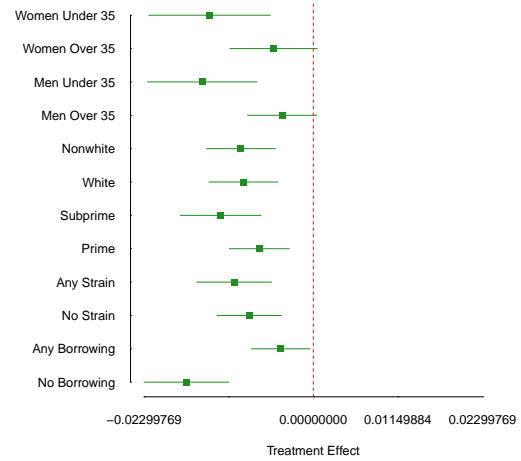
Notes: This figure plots treatment effects on the noted outcome by the identified group. For more details, see notes to figure 4.

Figure B-4: Share of Treated Matched DD Drivers with Traffic Stops in Follow-Up Quarter
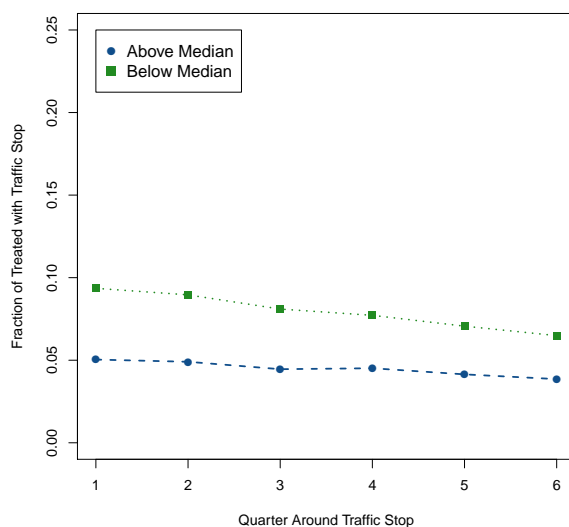
*N*otes: This figure plots the fraction of treatment drivers in the matched DD sample with a traffic ticket in each follow-up quarter. After regression adjusting, these fractions are used as the π's to construct the recursive TOT estimates.

Table B-3: Adjusting Matched DD Estimates for Follow-Up Citations

	Below Median Income			Above Median Income		
	(1) Matched DD	(2) Recursive TOT	(3) Difference	(4) Matched DD	(5) Recursive TOT	(6) Difference
Any New Distress	0.018	0.016	0.002	0.002	0.002	0
Collections	0.113	0.094	0.019	0.03	0.027	0.003
Collections Balances	71.48	61.6	9.88	3.02	3.32	-0.3
Delinquencies	0.087	0.071	0.016	0.037	0.033	0.004
Derogatories	0.048	0.04	0.008	0.027	0.024	0.003
Any Job in Employment Data	-0.015	-0.011	-0.004	-0.004	-0.004	0

Notes: The table reports 6-quarter ($\tau = 6$) effect estimates. Columns 1-3 report estimates for below-median income drivers and Colums 4-6 report estiamtes for above-median income drivers. Column 1 (4) reports the baseline matched DD estimates (samee as table 2). Column 2 (5) reports estimates adjusted for follow-up citations using the recursive TOT method outline in appendix B.

Figure B-5: Impact of Fines by Income for Sample Matched on Auto Histories

(a) Any New Distress   (b) Collections   (c) Collections Balances
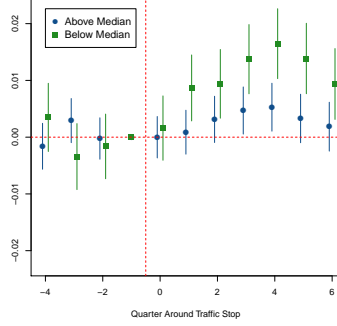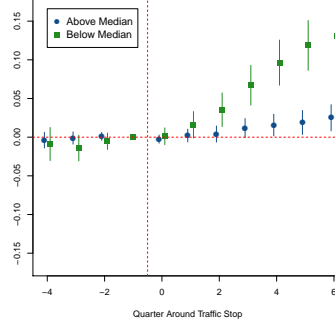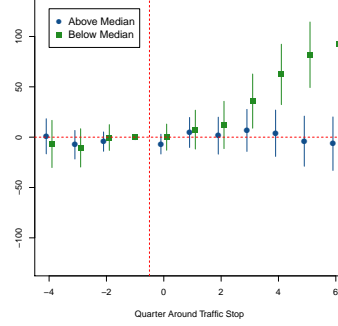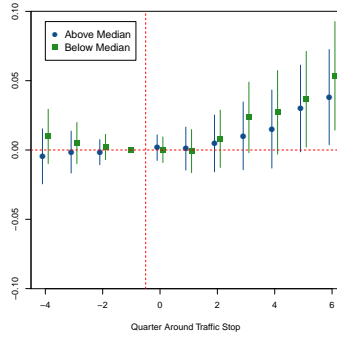
(d) Delinqs + Derogs   (e) Any Job in Emp Data
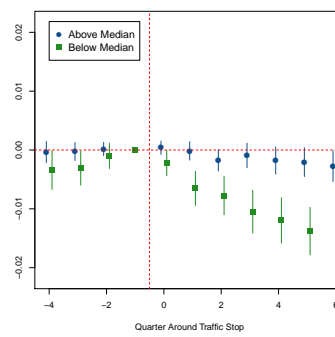
*N*otes: Each figure plots coefficients and 95% confidence bands on indicators between a treatment indicator and event time indicators using the matched DD design where drivers are matched on auto loan histories. Regressions also include individual, time, and event time fixed effects. Regressions are estimated separataely for drivers above and below the median baseline income. Confidence intervals are constructed from standard errors clustered at the individual level.

Figure B-6: Impacts of Fines for Low-Income Payers and School Attendees on Collections

(a) Payers

(b) School Attendees
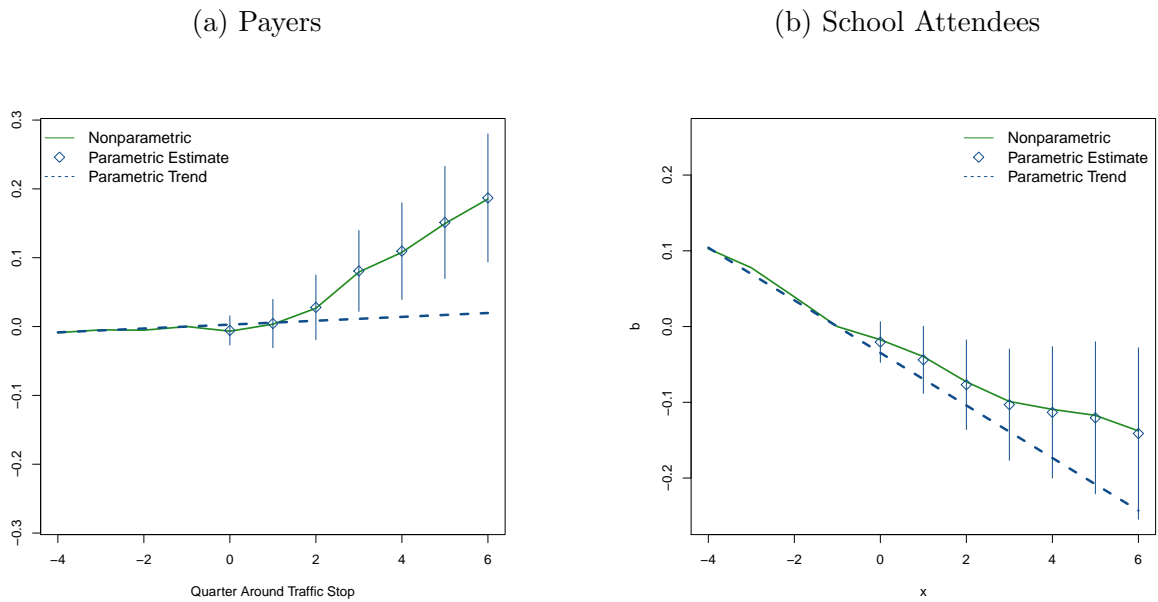
Notes: This figure corresponds to table 4 and plots matched difference-in-differences estimates of the impact of fines on collections by citation disposition. The green line plots the nonparametric estimate. The blue dashed line indicates the parametric pre-trend estimate, and the blue diamonds indicate the parametric estimate (relative to the estimated pre-trend).

Figure B-7: Impact of Fines by Income on Borrowing Outcomes

(a) Any Non-Auto Trade
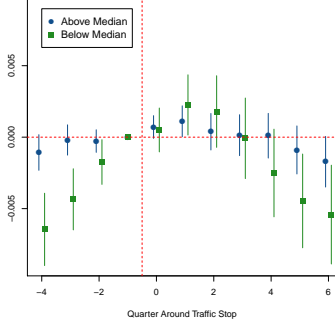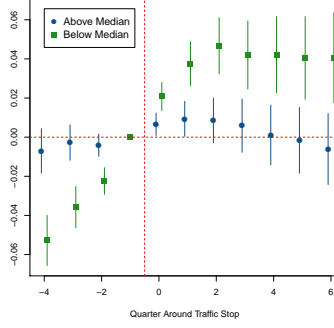
(b) Non-Auto Trades

(c) Revolving Utilization
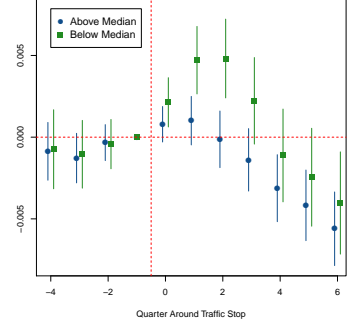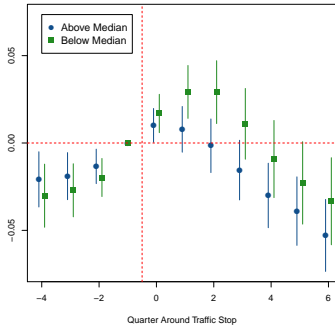
(d) Log(Rev Balances + 1)
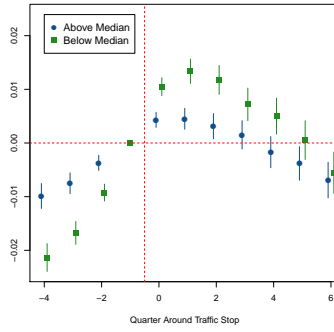
(e) Any Auto Loan
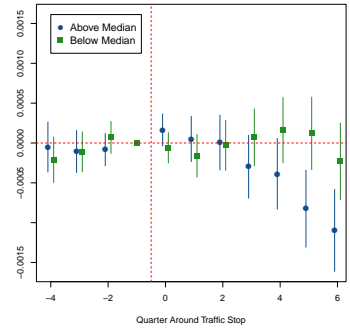
(f) Any Mortgage

*N*otes: See notes to figure 3.

Figure B-8: Impact of Fines by Income on Borrowing Outcomes Sample Matched on Auto Histories

(a) Any Non-Auto Trade
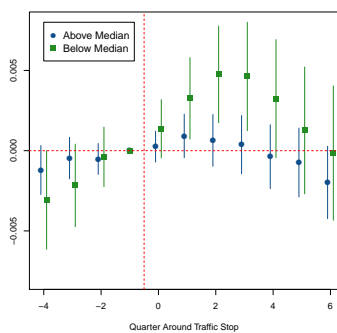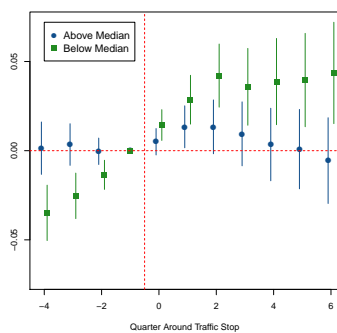
(b) Non-Auto Trades

(c) Revolving Utilization
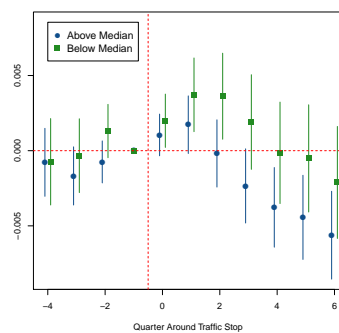
(d) Log(Rev Balances + 1)
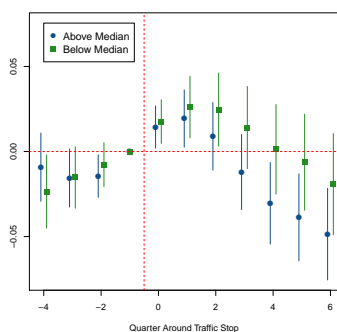
(e) Any Auto Loan
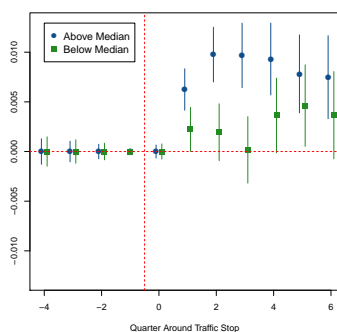
(f) Any Mortgage
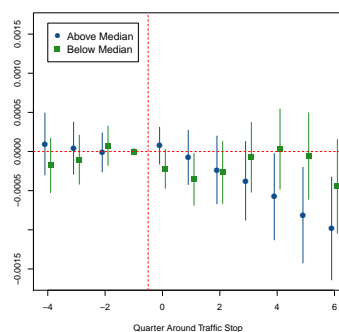
*N*otes: See notes to figure B-5.

C   Employment Database Analysis

In this section, I estimate the impact of a separation from a job covered by the employment records on credit report outcomes. The exercise serves two distinct purposes. First, it provides a test of the hypothesis that an indicator for working in the employment records is a meaningful and positive outcome. Second, to the extent that a separation impacts credit report ouctomes, these estimates are useful as a benchmark for the expected time path of traffic fine effects on the same set of outcomes outcomes.

To isolate the impacts of separations unrelated to traffic citations, I sample from the set of individuals in the initial sample who receive their first traffic ticket after January 2014. I analyize data from 2010-2013.

I then identify individuals with a separation from the employment data during in 2011 or 2012, measured as a transition from having at least one covered job to having zero covered jobs in adjacent quarters. Requiring that the separation occurs in the 2011-2012 period allows a balanced 4-quarter 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 24,784 individuals meeting all the above requirements. To help estimate time and age effects, I include individuals meeting the same criteria but whose spells in the employment data begin after 2013 as a quasi-control group. There are 47,943 such individuals.

Table C-1 presents summary statistics for the separations sample. To estimate impacts of separations, I use an event study approach:

$$y_{it\tau} = \sum_\tau \theta_\tau + \kappa_t + \epsilon_{it}$$

Here, the θ_τ's are quarter around separation indicators. I cluster standard errors at the individual-level.

Results are presented in figure C-1. Panel (a) indicates that a separation is associated with a \$2,170 decline in monthly earnings in the employment records. Panel (b) shows 0.018 percent spike in the probability of a new financial distress incident two quarters following a separation. Panel (c) shows increase in collections (0.13) and collections balances (9 percent) accruing in the 4-5 quarters after a separation.

Income-Equivalent Effects

I also use the jobs information from the employment records to estimate an earnings-collections elasticity. Here, I focus on the subset of individuals in the initial sample with their first traffic ticket after January 2014 who are continuously employed in the employment databse over 2010-2013. The characteristics of the sample are shown in column 3 of table C-1.

I estimate an elasticity using the regression:

$$\Delta \log(c_{it}) = \epsilon \cdot \Delta \log(y_{it-1}) + \nu_{it}$$

The coefficient $\hat{\epsilon}$ is the elasticity of collections balances with respect to monthly earnings.

My estimate of ϵ is -0.3 (0.1) and is presented graphically in figure C-2. This elasticity can be used to compute the monthly income change that would predict the observed increases in collections balances. Specifically, the implied percentage change in monthly income is $\hat{\beta}/\hat{\epsilon}$. Table C-2, shows this calculation for individuals above and below median the median baseline income. For low-income drivers, the observed impact of collections a fine on collections is the same as would be predicted by a \$236 reduction in monthly income. For high-income drivers, the observed impact on collections the same as would be predicted by a \$122 drop in monthly earnings.

Table C-1: Summary Statistics for Employment Database Analysis

	Separations		
	(1)	(2)	(3)
	Separations	Control	Continuous
Panel A: Demographics			
Female	0.47	0.5	0.49
Nonwhite	0.53	0.57	0.56
Age	35.39	35.06	39.53
Credit File Age	12.47	12.32	15.27
Credit Score	604	599	637
Estimated Income	30215	29468	37598
Monthly Earnings	2549	3393	4567
Panel B: Financial Strain			
Collections	2.73	2.91	1.96
Collections Balance	3400	3272	1291
Derogatory Accounts	1.63	1.6	1.42
Delinquent Accounts	2.22	2.19	1.99
Past Due Balance	3196	3057	3025
Prior Bankruptcy	0.07	0.07	0.08
Panel C: Credit Usage			
Any Account	0.83	0.79	0.91
Revolving Accounts	2.78	2.61	4.07
Revolving Balance	5754	5430	9398
Any Auto Loan	0.36	0.34	0.46
Any Mortgage	0.25	0.24	0.42
Individuals	24784	47943	37893

Notes: The table reports summary statistics for the sample to study employment database outcomes. Columnn 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 employment data after January 2014). Column 3 reports means for the set of individuals continuously in the employment data over the period 2010Q1-2013Q4.

Figure C-1: Estimating Impacts of Separating from Employment Records

(a) Monthy Earnings (Employment Data)   (b) Any New Financial Distress
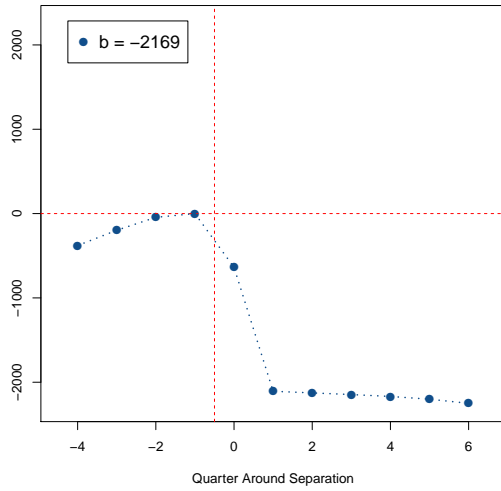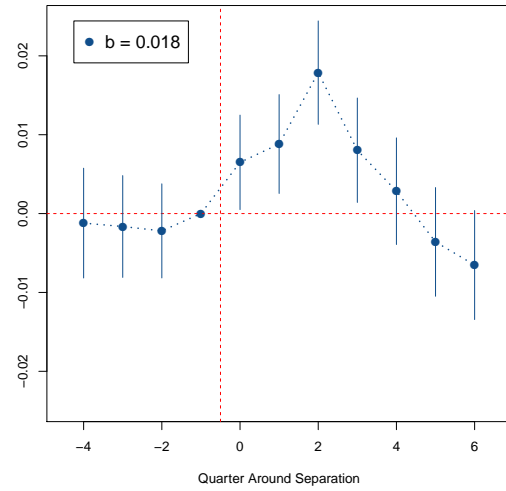
(c) Collections
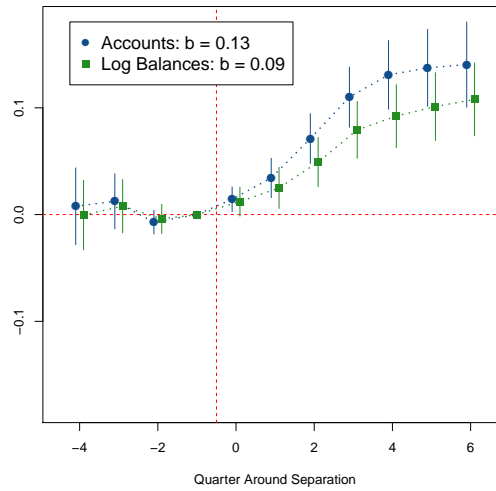
*N*otes: Each figure plots event study coefficients around the time of separation from the employment records.

Figure C-2: Collections Balances-Earnings Elasticity Estimate
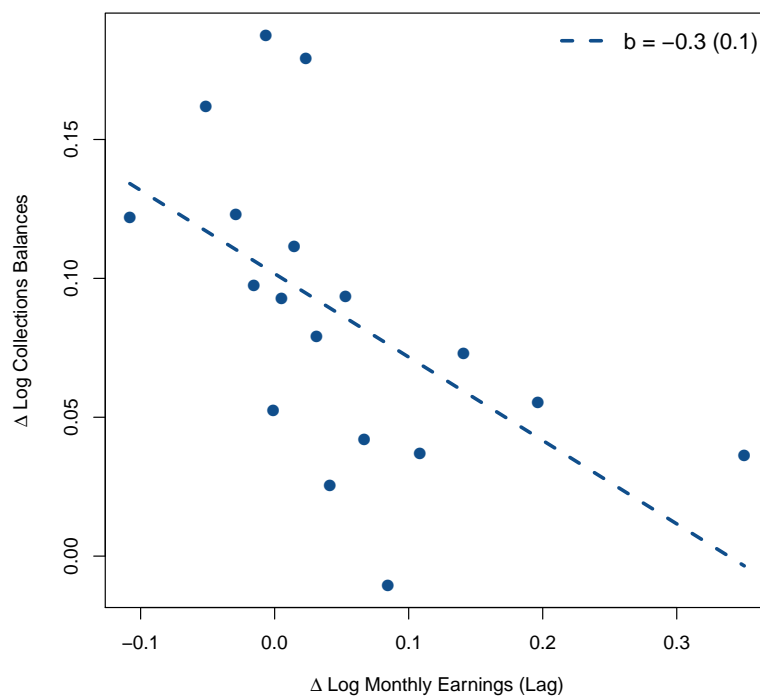
*N*otes: This figure plots log changes in collections balances between 2012Q1 and 2011Q1 (y-axis) against log changes in monthly earnings in the employment records between 2010Q1 and 2011Q1 using the continuously employed sample in column 3 of table C-1.

Table C-2: Income-Equivalent Effect Sizes for Collections Balances

	(1) Below Median	(2) Above Median
Fine Effect on Log Collections Balances	0.057*** (0.01)	0.009 (0.008)
Collections Balances-Income Elasticity	-0.371** (0.171)	-0.277** (0.13)
Implied Percent Change in Income	-0.154	-0.032
Average Monthly Income	1535	3821
Implied Change in Income	-236.39	-122.272

Notes: Table illustrates the calculation of income-equivalent effect size estimates for the treatment effects on collections balances. Columns 1-2 report estimates for below-median and above-median income drivers. The first row reports the estimated four-quarter, matched DD treatment effect on log(collections balances +1), identical to panel (b) of figure 3. The second row reports the collections balances-income elasticity estimated using the method described in the text. The third row reports the implied percent change in income based on the first two rows, the fourth row reports average montly earnings, and the fifth row reports the object of interest, the implied change in monthly income.

D Theoretical Appendix

D-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

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

Equation D.1 determines a threshold value of x as a function of y and θ:

$$x^*(y, p, f) = \frac{p}{1 - p}\left[u(y) - u(y - f)\right] \tag{D.2}$$

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

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

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

D-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

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

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 in-framarginal criminals. Hence, given tht the first two terms are zero, the only welfare impacts of a small change in enforcement are the effects on inframarginal criminals:

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

The following discussion below makes use of this result.

D-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.[23]

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

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

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

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[23]Standard 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

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

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 D.7. With a revenue benefit, the government is willing to allow a larger welfare loss to citizens when optimizing.

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

$$\frac{\partial V}{\partial p} = \int_{x^*}^{\infty} \left[u(y-f) - u(y) - x \right] g(x) dx \tag{D.8}$$

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

$$\frac{1}{1-p} \left[u(y-f) - u(y) \right] \tag{D.9}$$

Assuming p is small, then, optimal enforcement sets

$$u(y) - u(y-f) = -h'(p) - c'(p) \tag{D.10}$$

We can think of the left-hand side of D.10 as related to the quantity estimated in the data, the welfare cost of punishing an individual.

D-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:

$$x^*(y_H, p, f_0 + \Delta) = x^*(y_L, p, f_0) \tag{D.11}$$

The relevance of this assumption is as follows.[24] Recall from section D-2 that, for small Δ,

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[24]To 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, D.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 D.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

$$\frac{\partial}{\partial f} \left\{ pu(y - f) + (1 - p) \left[u(y) + x \right] \right\} = -p \frac{\partial u}{\partial c}(y - f) < 0 \tag{D.12}$$

Substituting D.12 into D.5 gives the following expression for the net welfare change associated with the change in the fine scheme:

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

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 D.11, which ensures that the limits of integration are equal, this expression can be rewritten as

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

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.

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

$$\frac{\partial C}{\partial f} = -g(x^*) \times \frac{p}{1 - p} \times \frac{\partial u}{\partial c}(y - f) \tag{D.15}$$

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

$$\Delta \frac{p}{1-p} \left[g(x^*(y_L, p, f_0)) \frac{\partial u}{\partial c}(y_L - f_0) - g(x^*(y_H, p, f_0)) \frac{\partial u}{\partial c}(y_H - f_0) \right] \tag{D.16}$$

The first term inside the brackets represents in 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 D.16 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.

E   Data Appendix

E-1   Data Sources

Citations Data

I obtained adminstrative records of the universe of traffic citations issued in the state of Florida over the period 2005-2015 through a FOIA (*sunshine law*) request. A copy of each traffic ticket issued in Florida is sent to the county clerk, who then forwards the information along to the Florida Clerks and Comptroller's Office (FCC). The FCC maintains the state's Uniform Traffic Citation (UTC) database, which preserves an electronic record of each ticket transcribed from the paper citation written by the ticketing officer. Figure E-1 shows a sample UTC form and figure E-2 provides an example of a completed form.

The UTC data include information about the cited individual and the offense. The individual information is taken from the driver license and includes DL number, name, date of birth, and address. Offense characteristics include the date, county, violation code (~ 260 codes), an indicator for the presence of a secondary violation, and an indicator for whether the offense involved a traffic accident.

The data also include the offender's gender and race as coded by the ticketing officer. Race is occasionally but inconsistently coded as Hispanic. For example, less than five percent of citations issued in Miami-Dade county, where Hispanics make up over fifty percent of the population, are issued to Hispanics. I follow Goncalves & Mello (2021) and recode the race information to Hispanic based on surname. I also match the citation of residence denoted on the citation to zip-code per capita income available from the IRS.

Dispositions Data

Traffic court dispositions associated with the citations from the *TCATS* database were also shared by the Florida Clerk of Courts. Citations were matched to disposition information using county codes and alphanumeric citation identifiers (which are unique within counties). Some citations have no associated disposition in the *TCATS* database, while others have multiple associated entries. Disposition verdicts can take on the following values:

1 = *guilty*; 2 = *not guilty*; 3 = *dismissed*; 4 = *paid fine or civil penalty*; 6 = *estreated or forfeited bond*; 7 = *adjudication witheld (criminal)*; 8 = *nolle prosequi*; 9 = *adjudged delinquent (juvenile)*; A = *adjudication witheld by judge*; B = *other*; C=*adjudication witheld by clerk (school election)*; D = *adjudication witheld by clerk (plea nolo and proof of compliance)*; E = *set aside or vacated by court*.

In practice, the verdicts 1 (6.5 percent), 3 (32 percent), 4 (32 percent), A (15 percent), and C (10 percent) account for 96 percent of citations with a disposition. Moreover, as confirmed in a phone conversation with Beth Allman at the Florida Clerk of Courts on July 24, 2018, several of the violation codes are difficult to interpret. In particular, it is very difficult in practice to infer the precise outcome of tickets with disposition codes *1*, *3*, *A*, or those with multiple dispositions in the TCATS database.

My primary interest in the disposition information is to focus on subsets of the data where I can say for certain that the citation is paid. I define a citation as paid if the citation has a *single* disposition verdict and that verdict equals *4* or *C*. I include *C* in the paid definition because a traffic school election requires on-time payment. Individuals participating in traffic school pay their citations and enroll in a four-hour course (available in-person and online) costing $25 (school electors receive a $15 discount on their fines). In exchange for completion, the citation is wiped from the driver's record. In section 5.3, I compare citations with dispositions *4* and *C*.

Punishment Information

The UTC database does not include reliable measures of citation punishments. I use a combination of information available in Appendix C of the Uniform Traffic Citation Manual (link) and the fine distribution schedules (link) to characterize citation punishments.

Appendix C of the UTC manual maps violations codes to classifications (e.g., moving; non-moving; criminal), disposition options (e.g., mailable fine; mandatory court apperance), associated DL points, and base fine amounts. The base fine amounts do not correspond to the amount payable and due, however, as they exclude the various fees and surcharges. I use the information in the distribution schedules to convert base fines to effective fines. For the case of moving violations (the focus of the main empirical analysis), this exercise amounts to adding $98 to the base fine amount.

Credit Bureau Data

Access to monthly credit report data from January 2010 through December 2017 for cited drivers was granted by one of the three major credit bureaus through a data sharing agreement. The credit bureau data represent an aggregated snapshot of an individual's credit report taken on the final 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, and information computed directly by the credit bureau such as credit scores. The data also include an estimated income measure based on a propietary model which predicts an individual's income, rounded to the nearest thousad, using information on the credit file. As shown in figure E-3, estimated income is highly correlated with both zip code per-capita income and earnings in the employment database where reported.

Employment Information

Access to employment information covering a subset of large employers was also provided by the credit bureau. The provided data are quite thin and include the number of jobs and total earnings in a given a month. No information on occupation or location is present. In terms of coverage, employers represented in the employment records tend to be larger businesses. As shown in figure E-4, coverage exhibits secular variability over-time and grows somewhat over the sample period.

Matching and Accessing Credit Bureau Data

I provided the credit bureau with a list of 4.5 million Florida residents (individuals with a valid Florida driver license and a Florida zip code) issued a traffic citation between January 2011 and December 2015. The credit bureau use a proprietary fuzzy matching algorithm to link individuals to the credit file using name, date, of birth, and home address reported on the citation. Importantly, the credit bureau maintains a list of previous addresses for individuals on file, meaning that the address I provided need not to be an individual's current one to obtain a successful match. The linking process matched 3.7 million drivers for an 82 percent match rate (as discussed below, the effective match rate is lower because of individuals who first appear on file *after* their traffic citation).

Two pieces of information are useful for interpreting the match rate. First, the data are transcribed from paper citations (e.g., figure E-1) and therefore contain transcription errors. Second, according to Brevoort et al. (2015) , about eleven percent of adults, and as many as thirty percent in low-income area, have no credit record. Consistent with this finding, I find a strong relationship between neighborhood (zip-code) income and the credit file match rate, as shown in figure A-2. Results from regressing a successful credit file match on available driver characteristics are shown in table A-1.

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. They replaced DL numbers with a scrambled individual identifier (allowing me to track individuals who receive multiple citations) and the exact traffic stop date with the year and month. I was then allowed acess, through a secure server hosted by the credit bureau, to the anonymized citations data and monthly credit reports (and employment records), each with a scrambled individual identifier for linking across the two datasets.

Initial Sample

Of the 3,684,650 cited drivers matched to the credit file, I first drop 1,634 (~0.4 percent) individuals with fragmented credit files, leaving 3,683,016 drivers. I also drop 240,959 drivers with no available credit report data prior to a traffic stop, leaving 3,442,057. For simplicity, I further require that drivers appear on the credit file in January 2010 (the first possible month), leaving 2,994,894 drivers. I also require that individuals have a nonmissing credit score and nonmissing estimated income as of that date, leaving 2,966,055 individuals, and focus on individuals aged 18–59 as of that date, leaving 2,631,641 individuals. Analysis samples are constructed from this group of individuals.

Single Citation Restriction

In the analysis samples, I focus on citations for moving infractions that are also for a single violation. A single traffic stop can result in citations for multiple offenses, which can appear in the UTC database in multiple ways (e.g., companion violation flag; companion violation field; multiple entries with the same date). In the interest of having a cleanly-defined treatment in terms of an offense, associated fine, and associated DL points, I focus on stops resulting in a single citation and not associated with a traffic accident. A *single citation* stop meets

the following restrictions: (a) no crash; (b) no companion citation or violation noted; (c) the only stop for a driver in a given month. Exclusion (c) is restrictive but necessary because, once matched, I can only observed the year × month of the citation and cannot distinguish between citations issued on the same day and on different days within the same month.

Variable Definitions

1. *Collections.* Number of 3rd party collections (collections not being handled by original creditor) on file. Includes both public record and account level 3rd party collections information.

2. *Collections Balance.* Total collection amount (unpaid) for 3rd party collections (i.e. collections not being handled by original creditor) on file. Includes both public record and account level 3rd party collections information.

3. *Delinquencies.* Number of accounts on file with 90 days past due as the worst ever payment status.

4. *Derogatories.* Number of accounts on file with any of the following ever – repossession, charge off, foreclosure, bankruptcy, internal collection (collection being handled by original creditor and not a third party), defaulted student loan.

5. *New Collection.* I construct this variable by computing a first difference in the number of collections and defining an indicator for whether the first difference is greater than zero.

6. *New Delinquency.* An indicator for whether the pre-existing variable "Number of open accounts with current rate of 90 to 180 or more days past due (but not major derogatory) and *reported within one month*" is greater than zero.

7. *New Derogatory.* I construct this variable using the same method as collections from the stock derogatories measure.

8. *Any New Financial Distress.* Equal to one if new collection, new delinquency, or new derogatory equals one. Zero otherwise.

9. *Any Job in Employment Data.* Equal to one if an individual is reported to have at least one active job in the employment records (covering a subset of large employers) in a given month.

All raw variables in the credit bureau database are pre-topcoded. Account-level counts, such as the number of delinquencies, are topcoded at 92. Balances are topcoded at $9,999,992, which I typically further topcode at the 99th percentile.

Credit bureau information can be missing in a given month because an individual lacks a credit report or for reasons which are noted in the data. The missing codes fall into four categories, but not all codes are relevant for every attribute – (1) no available date or amount, (2) accounts are in an exclusion category, (3) no relevant account on file, (4) no accounts on file. I retain the missing values where the codes are (1)-(2) but impute zeroes for codes (3)-(4) that denote a lack of account.

Aggregation

All variables are first computed using monthly data. I then aggregate the data to the person × quarter level for two reasons. First, aggregating reduces the (already minimal) prevalence of missing values. For example, an individual may have a nonmissing credit report in January 2010 but not February 2010 or March 2010. Quarterly aggregation uses the January credit report as the quarterly value. Second, the aggregation reduces the dimensions of the panel dataset to a more computationally manageable size. The event study regressions, which use a 2010-2017 panel of 983,206 individuals, cannot be estimated on monthly data using the computing tools available for analyzing the credit report data due to the dimensionality of the matrix that needs inverting. These regressions are computationally manageable when the data are collapsed to the person-quarter level.

When aggregating continuous variables (e.g, number of collections on file) to the person-quarter level, I take the average of the nonmissing values within the person-quarter. For binary variables (e.g., any new financial distress), I take the maximum of the nonmissing values and impute zero if all values are missing.

Figure E-1: Florida Uniform Traffic Citation (UTC) Form

*S*ource: https://www.flhsmv.gov/courts-enforcement/utc/forms-and-resources/.

Figure E-2: Example of Completed UTC Form

Figure E-3: Comparison of Income Measures
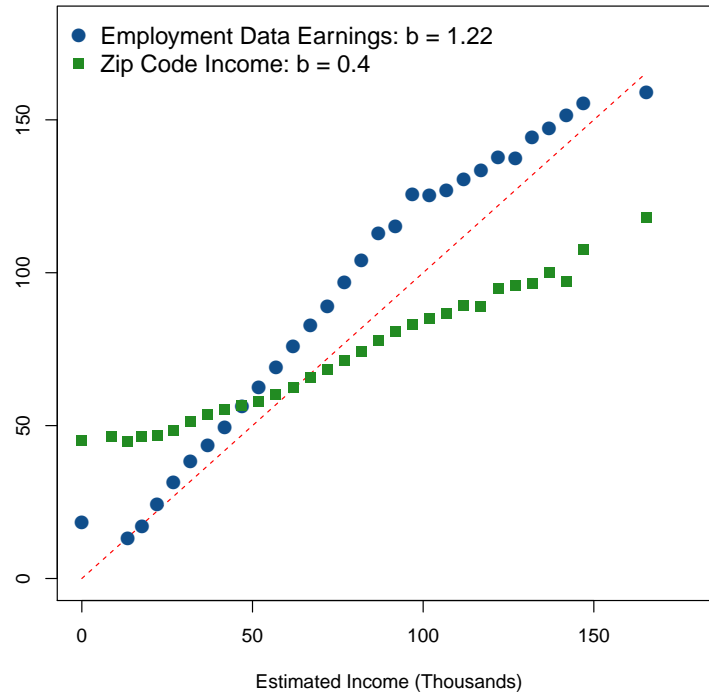
Notes: This figure plots binned means of annualized earnings from the employment records (blue circles) and zip code income (green squares) against the credit bureaus estimated income measure as of January 2010. Dotted line denotes the 45-degree line. The underlying data are a cross-section of the initial sample of 2,631,641 (293,641 with employment-data earnings). The coefficient (standard error) from a regression of zip code income on employment-data earnings is 0.19 (0.001).

Figure E-4: Employment Database Coverage Over Time
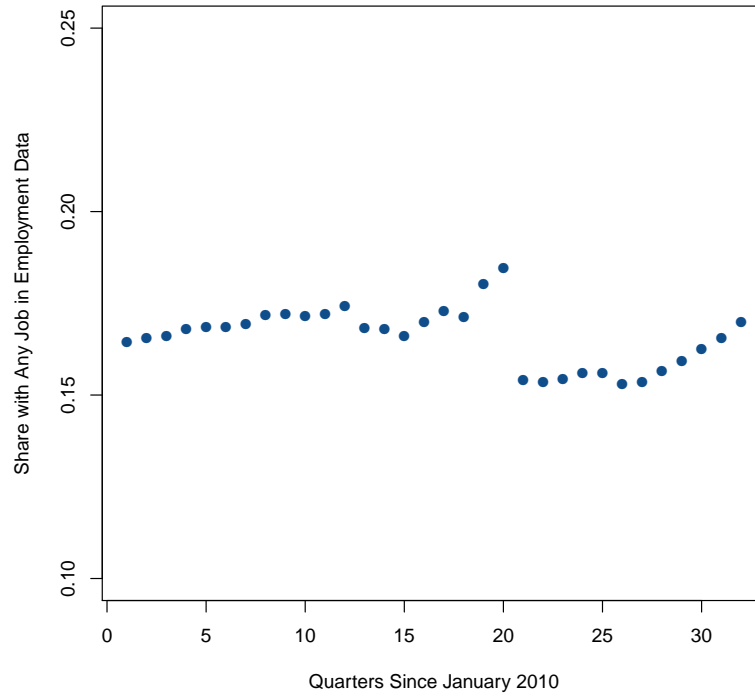
*N*otes: This figure plots plots the share of the initial sample ($N = 2,631,641$) holding any job in the employment database in each quarter 2010Q1 through 2017Q4. Note that the sizeable decline in coverage occurs after the conclusion of data used in the main matched difference-in-differences analysis.