Fines and Financial Wellbeing*

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Abstract

While survey evidence suggests widespread financial fragility in the U.S., causal evidence on the implications of typical, negative income shocks is scarce. I estimate the impact of speeding fines on household finances using administrative traffic citation records and a panel of credit reports. Event studies reveal that for every $195 in fines, unpaid bills in collections increase by $34. Given additional evidence that fine payment, rather than other non-fine sanctions, explains this effect and that default is the "last resort" for households, I interpret this finding as evidence of financial precarity. My estimates are consistent with at least 30 percent of households resorting to default on other bills to finance a typical, unplanned expense. I also find that fines are associated with longer-run declines in credit scores, borrowing limits, and the likelihood of appearing as employed in payroll records covering a subset of large, high-paying employers. This impact on employment situations appears attributable to the diminished financial position of households rather than, for example, downstream license suspensions.

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1 Introduction

The ability of households to cope with adverse shocks has important implications for taxation and social insurance policies (e.g., Baily 1978; Chetty 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 using liquid savings in a 2017 survey (FRBG, 2018).

This survey result in particular has received significant attention from journalists and policymakers. While some have cited the survey as another symbol of growing inequality or as motivation for an expanded social safety net, others have questioned the credibility of this statistic. Criticism has focused on the potentially misleading presentation of survey results (Reynolds, 2019), conflicting evidence from other data sources (e.g., Chen 2019; Nova 2019; Bhutta & Dettling 2018), and the belief that resilience against real world shocks may differ from self-reported ability to pay on a low-stakes survey (Strain, 2019).

An important obstacle in this debate is the lack of causal evidence on the impacts of typical, negative shocks on households. While ethnographies provide compelling accounts of families derailed by unplanned expenses (e.g., Shipler 2004; Desmond 2016), the lack of credible variation in small income shocks and data on the household finances of lower-income poulations have proven important obstacles to estimating causal effects. 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). Moreover, the literature's reliance on policy variation generated by tax rebates or mortgage programs and on data from credit cards and bankruptcy filings has left the bottom end of the income distribution relatively understudied.

In this paper, I explore whether households are able to absorb unplanned shocks by estimating the impacts of fines for traffic violations on household financial situations. This setting has several important advantages. First, traffic fines represent a common form of everyday, unplanned 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 poorer communities, allowing for the study of a large sample of low-income households. Third, driver license suspensions imposed for nonpayment incentivize high payment rates on average, increasing confidence that a traffic ticket represents a true expense, or transitory income, shock.

To estimate the impact of fines, I link administrative data on the universe of traffic citations issued in Florida over 2011-2015 to a quarterly panel of credit reports for cited drivers. The citations data provide near-complete coverage of the state's traffic offenders and my analysis sample represents about three percent of Florida's driving-age population. Credit reports offer a detailed account of an individual's financial situation and include information on defaults and borrowing. Unpaid bills in collections represent an especially useful outcome, 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 limited attachment to the formal financial sector.

Taking advantage of this unique panel of credit reports, I leverage staggered variation in the timing of traffic stops with an event study approach. To address the various identification concerns associated with two-way fixed effects DiD approaches raised in the recent econometrics literature (e.g., Roth et al. 2022), I estimate the event studies via the method of Callaway & Sant'Anna (2021), relying only on comparisons between individuals treated at a particular time and those treated in the future. To further mitigate concerns about violations of parallel trends, I focus my analysis on speeding violations rather than other types of infractions, such as equipment or paperwork violations, that may themselves signal changes in an individual's financial situation. In the sample of speeders, the pretrend test of Borusyak et al. (2022) consistently cannot reject the null of parallel trends.

Event study estimates reveal that traffic fines averaging \$195 increase unpaid bills in collections by about \$34 ($se =$ \$4). Given high payment rates on traffic fines and the fact that collections activity associated with traffic citations is very unlikely to be reported to credit bureaus, I interpret this event study estimate as a test of households' *ex ante* ability to cover unplanned expenses, in the spirit of Dobkin et al. (2018). Specifically, this finding implies that, on average, households resort to default on other financial obligations in order to finance the payment of a typical, unplanned expense.

This interpretation is bolstered by heterogeneity based on proxies for an individual's financial buffer available in the credit report data. Individuals with over \$200 in available credit card balances at baseline borrow about \$19 ($se =$ \$6) on credit cards and accrue about \$15 ($se =$ \$4) in collections debt. On the other hand, those without easy access to liquidity on credit cards accrue an additional \$44 ($se =$ \$7) in collections debt. Heterogeneity by both income and credit card liquidity suggests a clear hierarchy of sources for financing unplanned expenses: (i) cash-on-hand, (ii) borrowing on credit cards, (iii) delaying credit line payments, and (iv) default which ultimately leads to collections activity.

The observed increases in default following a traffic stop generate measurable, longer-term effects on access to credit. Three years out from the traffic stop, I estimate that credit scores and borrowing limits are 2.6 points ($se = 0.2$) and \$330 ($se =$ \$50) lower, respectively. I also find evidence for longer-run declines in home ownership, geographic mobility, and attachment to the formal financial sector, proxied by whether an individual has any open credit line.

Drawing on administrative payroll records from a subset of large employers covering 20-25 percent of total employment in Florida and paying above-average wages, I find that in the twelve quarters following a traffic stop, the likelihood that an individual appears as employed in these payroll records falls by 1.2 percentage points (se = 0.001), relative to a mean of 15 percent. Transitions in and out of the payroll records do not necessarily correspond to transitions in and out of employment, but the estimated effect on payroll employment implies, at the very least, an impact on employment stability. I find that fines both reduce the likelihood of transitions into these payroll records and increase the likelihood of transitions out, and that the effect on payroll employment is wholly attributable to lower-income motorists. Individuals employed in payroll-covered jobs with above median earnings at baseline experience no change in the likelihood of payroll employment and small, statistically insignificant changes in financial distress following a traffic stop.

A natural question is whether the observed impacts on financial distress and job stability can be explained by other, non-fine sanctions associated with traffic tickets, such as court fees, driver license (DL) points accrued on a driver's record and associated increases in car insurance costs, or DL suspensions imposed on non-payers. Based on imperfect data on the traffic court disposition associated with each citation, I estimate the average total financial costs of citations, taking into account the post-citation choices of motorists. My preferred estimate suggests that the average citation in my sample is associated with the payment of \$174 fines and fees initially and an \$18 increase in quarterly car insurance premiums (because many motorists avoid increases in premiums through the traffic court system), yielding to a total cumulative cost estimate of about \$300 over the six quarters following the citation.

I also present estimates for subgroups of individuals based on their traffic court disposition, with the caveat that this analysis splits the sample on the post-citation choices of motorists. I find that estimates for the subgroup of individuals who can be identified as paying their fines for sure are similar to, and if anything slightly larger than, estimates in the full sample. Estimates are also comparable for a subgroup who paid their fines but avoided increases in auto insurance costs altogether, while estimates are attenuated for a subgroup who likely received fine reductions in court. No more than eight percent of the sample faced a suspension for nonpayment, and estimates are modestly lager in this subgroup. While I cannot definitely rule out a role for DL suspensions or increases in insurance costs, the available evidence suggests that fine payment itself is the primary driver of the effects.

The fact that effects on employment arrangements appear unexplained by, e.g., driver license suspensions, raises a question about mechanisms. Impacts on payroll employment are consistently strongest in subsamples with the largest increases in financial distress, suggesting a role for the impacts of a diminished credit reputation on job-finding (e.g., Bos et al. 2018; Bartik & Nelson 2021) or, e.g., housing situations. My findings are also consistent with evidence that financial distress can reduce labor supply (e.g, Dobbie & Song 2015; Barr et al. 2023) and evidence that financial distress weakens decision-making (Schilbach et al. 2016; Mullainathan & Shafir 2013) and productivity (Kaur et al., 2021). Disentangling these potential mechanisms is an interesting avenue for future research.

My central contribution is evidence that, on average, households cannot easily absorb typical, unplanned expenses. I conclude by synthesizing the key lessons from this result and discussing them in the context of the relevant literatures. First, I consider how my findings can speak to the survey evidence on the prevalence of financial fragility (e.g., FRBG 2018). Under the assumption that default is the "last resort" for covering unplanned expenses, one can conceptualize the share of households for whom fines causally increase unpaid bills as the relevant metric. The event study estimates can identify bounds on this fraction under ad-hoc, but reasonable, assumptions about the distribution of treatment effects. Using this approach, I estimate that at least 10 percent, and more likely between 30 and 60 percent, of individuals borrow out of other financial obligations to cover an unexpected \$200 expense (or \$300, including downstream insurance costs). This paper is the first to provide evidence on the prevalence of this causal notion of financial fragility.

Next, I connect my findings to the vast literature on the consumption smoothing behavior of households (e.g., Stephens 2001; Parker 2017; Ganong et al. 2020; Golosov et al. 2022; Baker & Yannelis 2017; Gelman et al. 2020; Ganong & Noel 2019). My central contribution to this literature is causal evidence in a large sample that default is an important consumption smoothing strategy for liquidity-constrained households, even when facing "typical" income shocks (Morduch & Schneider, 2016). My documentation of households' tiered strategy for covering unplanned expanses is also a contribution to the literatures on low-income and behavioral household finance (e.g., Beshears et al. 2018; Gathergood et al. 2019).

My finding that a sizable fraction of households are affected by typical shocks has potentially important implications for the optimal coverage and generosity of social insurance. Specifically, the observed effects on default and the ensuing declines in creditworthiness imply that many households are not self-insured against usual income volatility. Abstracting away from the important logistical concerns, this finding implies that an expanded social safety net, which insures against a wider range of shocks faced by households, could carry social welfare gains (e.g., Mazumder & Miller 2016; Hu et al. 2019; Gallagher et al. 2019). Alternatively, the results may suggest a role for more aggressive policies to encourage self-insurance, such as expanded financial education or saving incentives programs (e.g., Klapper & Lusardi 2020, Lusardi et al. 2011).

Finally, my paper also adds to the nascent literature on the social costs of policing (e.g., Ang 2021) and a concurrent literature on the effects of legal financial obligations (LFO's) on offender outcomes (Kessler 2020; Pager et al. 2022; Giles 2022; Finlay et al. 2022; Lieberman et al. 2023). While a large literature has examined deterrence effects of fines (e.g., Makowsky & Stratmann 2011; DeAngelo & Hansen 2014; Traxler et al. 2018), interest in the potential negative effects of fines and fees on individuals has grown significantly in recent years. Finlay et al. (2022) note that an important distinction in the current research on LFO's appears to be whether fines are coupled with criminal convictions, with studies examining variation in fine amounts among those also convicted of felonies or misdemeanors tending to find null effects on life outcomes such as reoffending or employment. My paper, on the other hand, studies comparatively small fines which are not associated with convictions and documents impacts on household financial situations.

The rest of the paper proceeds as follows. Section 2 provides the relevant institutional background and section 3 describes the data. I lay out the empirical strategy in section 4 and present results in section 5. Section 6 interprets and contextualizes the findings and section 7 concludes.

2 Institutional background

2.1 Setting

The setting for this paper is traffic enforcement in Florida. Patrolling police officers, or in some cases automated systems such as red light or toll cameras, issue citations to offenders. Traffic citations are extremely common. Over 4.5 million individual Florida drivers received at least one traffic citation over 2011–2015, 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 30 percent of the driving age population was ticketed at least once over this five year period.

Traffic enforcement appears to disproportionately affect disadvantaged communities. Figure 1 plots the zip code citation rate, computed as the number of citations issued to residents of a zip code divided by the number of residents, against zip code characteristics. Residents of the poorest neighborhoods are cited about twice as often as residents of the lowest poverty neighborhoods. Residents of neighborhoods with the largest minority (Black or Hispanic) populations are cited four times more often than residents of the whitest communities.

2.2 Institutional details

Traffic citations specify an offense and fine to be paid. The most common 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. For example, minor equipment violations such as broken tail-lights carry a fine of \$110, while the fine for speeding 30+ miles per hour over the posted limit in a construction or school zone is \$620. Sanctions for certain criminal, rather than civil, traffic offenses can exceed \$1,000 and may include jail time. As discussed in section 4, I focus my analysis on speeding violations, with fines ranging from \$123 to \$273 ($\mu \approx \$195$).

Many offenses also result in "points" on a driver's license. 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). Speeding offenses are associated with

3-4 points, while points are generally not assessed for non-moving violations. In the main analysis, I focus on individuals facing their first citation in at least one year, minimizing the risk that individuals are in position to receive a points-based suspension.

Insurance companies will typically consider license points as a signal of driver risk when setting premiums, so individuals may face increases in auto insurance costs following a citation. I estimate that a typical speeding citation over this period is associated with an increase in annual (monthly) auto insurance premiums of $227 ($19). As described below, motorists can mitigate their point exposure through the traffic court system, and taking into account those choices, I estimate that the average citation in my analysis sample was associated with a $93 ($8) increase in annual (monthly) premiums. For more details on this calculation, see section 6.1 and appendix B-2.

Once a citation has been issued, a driver can either submit payment to the county clerk or request a court date to contest the charge. For those contesting their citation in court, a judge or hearing officer typically decides to (i) uphold the original charge, (ii) reduce the sanctions, or (iii) dismiss the citation. A court fee, averaging about $75, is required for those bringing their case to court, but may also be waived in some instances. For those not contesting the charge, payment is due thirty days from the citation date.[1] At the time of payment, a driver may also elect to attend traffic school. A voluntary traffic school election, coupled with an on-time payment, wipes the citation from the driver's record and thereby prevents the accrual of the associated license points on the individual's DL.[2] If the county clerk has not received payment in-full within 30 days, the individual is considered delinquent and their license is suspended, effective immediately. Knowingly driving with a suspended license is a misdemeanor offense and typically results in a fine exceeding $300, as well as potential jail time. Figure A-1 succinctly illustrates the driver's potential decision tree and corresponding outcomes for the case of a typical moving violation.

If a citation remains unpaid after 90 days, the county clerk adds a late fee to the original amount owed and sends 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. Note that, to the best of my knowledge, collections activity originating with unpaid

[1] As of 2022, a new Florida law requires that counties offer income-based payment plans for traffic citations. However, during my sample period (2005–2017), only two counties, Hillsborough and Pinellas, offered three-month payment plans for traffic fines (statute; news article). Figure G-1 offers suggestive evidence of smaller effects on unpaid bills in these counties during this period.

[2] 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 is 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 25 percent of individuals in the subset of the main sample with valid traffic court disposition information participate in traffic school.

citations *will not appear on a driver's credit report.*[3]

An important takeaway from a careful consideration of the institutional details is that the exact "treatment" a motorist faces can take many forms. Even holding the offense constant, a citation's outcome depends on an offender's ex-post decisions, and to some extent driving history, neither of which is perfectly observed in the data. For reasons discussed further in section 4, I focus my analysis on speeding violations, which are not associated with mandatory court appearances or automatic license suspensions, and think of the treatment as a bill for $195 (on average), where the punishment for nonpayment is a revocation of driving privileges. But treatment could also entail time in court and court fees for those contesting their citations, increases in car insurance premiums for payers, and license suspensions for non-payers. I initially focus on estimating "intent-to-treat" effects, but later discuss estimates of the "first stage," or the average total costs of citations accounting for the post-citation choices of motorists, as well as present heterogeneity analyses to study the relative importance of channels other than fine payment.

According to the Florida Clerks and Comptrollers, who estimate that over 90 percent of traffic fines are paid on time, the threat of license suspension is a strong incentive for payment. Using traffic court disposition information, I estimate a strict lower bound on the payment rate of 59 percent and cannot rule out a 100 percent payment rate (see appendix B-2 for additional discussion of the dispositions data). My analysis proceeds under the assumption that fines are paid in the majority of cases, but I leverage the disposition data to quantify total costs and to estimate impacts for subsets of individuals who surely paid their fines, whose sanctions may have been dismissed in court, and who avoided the accrual of driver license points via traffic school.[4]

3 Data

3.1 Citations data

The Florida Clerks and Comptrollers Office provided administrative records of all traffic citations issued in Florida from 2010–2015 from Florida's Uniform Traffic Citations (UTC) database. These records include the date and county of the citation as well as information on the charged violation. The UTC data also includes information listed on the motorist's

[3]The reporting of collections activity to credit bureaus 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 signaled an ability to report to credit bureaus on their webpage, the two agencies responding to my inquiry indicated that they do not report traffic citation-related collections.

[4]While some studies (e.g., Giles 2022) have documented very low payment rates for criminal fines, Dusek & Traxler (2022) find a payment rate above 75 percent for traffic fines and document fairly small compliance responses to fine increases.

driver license (DL): name, date of birth, address, race, and gender; as well as the driver license state and number.

3.2 Credit reports

Access to monthly credit reports from January 2010 through December 2017 was granted by one of the major credit bureaus. I provided the credit bureau with a list of 4.5 million Florida residents issued a traffic citation between January 2011 and December 2015. Using a proprietary fuzzy linking algorithm, the driver information was matched with the credit file using name, date of birth, and home address on the citation. About 3.7 million drivers were matched to the credit file, and I further require that individuals are on file as of January 2010, have a non-missing credit score as of that date, and are aged 18-59 as of that date for analysis. I sometimes refer to this sample of 2.6M individuals as the "drivers on file" or the "initial sample." For further information on the credit file match, see appendix F-2.

The credit bureau data represent a snapshot of an individual's credit report taken on the final Tuesday of each month. These data include information reported by financial institutions, such as credit accounts and balances, information reported by collections agencies, information culled from public records, and information computed directly by the credit bureau, such as credit scores (VantageScore® 3.0). As described in appendix F-4, I augment the credit report data by constructing an estimated income measure at baseline using an income estimate provided by the credit bureau, the average income in a motorist's home zip code, and payroll records for a subset of the sample (described below).

For the empirical analysis, I aggregate the credit report data from the individual × month level to the individual × quarter level. This aggregation makes the dimensionality of the panel datasets more computationally manageable, with the additional benefit of reducing the (already very rare) prevalence of missing values.

3.3 Outcomes and interpretation

While credit report data provide a wealth of information on an individual's financial situation, a challenge in working with these data is to focus on a parsimonious set of outcomes with a reasonably straightforward interpretation. My focus in terms of outcomes closely follows Dobkin et al. (2018).

As my primary outcome, I focus on collections activity on credit reports, which represents unpaid bills that have been sent to third-party collections agencies, who attempt to recover payment. To the best of my knowledge, as mentioned in section 2.2, unpaid traffic fines will not appear as collections on credit reports. Collections are an especially useful measure of financial distress in the current context because unpaid bills need not be related to credit lines. According to Avery et al. (2003) and FRBNY (2018), only a small fraction of third-party collections originate with credit accounts; the majority are associated with medical and utility bills. Hence, unpaid bills in collections can capture increases in financial strain even among those with tenuous credit usage, whereas individuals need to maintain open borrowing accounts in order to exhibit delinquency, for example, in the credit file.

Where relevant, and in the appendix, I also show results on other measures of default such as credit line delinquencies and derogatories (e.g., accounts with a charge-off). While these primary default measures are stocks, I additionally construct a binary flow measure that equals one if an individual has any new collection, delinquency, or derogatory appear on their credit report in a given quarter.

Importantly, when examining these primary default measures, we should expect to see effects (if any) materialize gradually over time, due both to how the outcomes are defined and to the credit reporting process. For a collection to appear on a credit report, a household needs to miss a bill, a creditor needs to send that default to a third-party collections agency, and that third-party collections agency needs to report that activity to a credit bureau. In many cases, creditors (e.g., utility providers) provide some temporary forbearance on late payments before sending the debt to a collector. Hence, collections activity appearing quickly following a traffic stop could correspond to bills which have already been missed but then transition into "late enough" for the creditor to send to a collector. On the other hand, a new missed bill immediately following a traffic stop should take several months to appear as a collection on a credit file.[5] Note that, in either case, we should still interpret the collections activity as attributable to the fine. The same logic applies also for delinquent or derogatory credit accounts, where fines may induce already delinquent accounts to pass the threshold for reporting or lead to new defaults which ultimately become 90-days delinquent (or sufficiently late to warrant a charge-off) and then show up on a credit report.

I also explore borrowing on credit cards. In a preview of the results, one complication associated with credit card borrowing as an outcome in this setting is that borrowing may be constrained by credit access, which will tend to be affected by changes in the financial distress measures I examine. A consistent feature of the estimates for credit card borrowing is a short-run increase followed by a long-run decline, and I show that this pattern can be explained largely by a reduction in borrowing limits in the medium-term.

I interpret effects on credit card and collections balances as the extent to which households borrow, either through formal channels or by "borrowing" out of other financial obligations in the case of collections, in order to cover a traffic fine. A challenge in relying on credit report data is that default outcomes in particular may not have a clear interpretation in terms of welfare. Morduch & Schneider (2016), for example, highlight missing bills and delaying bill payments as an important consumption-smoothing tactic for cash-strapped households.

However, default can be associated with significant costs. Pattison (2020) documents that

[5]Event studies in Dobkin et al. (2018) show that the impact of hospital admissions on medical collections materialize over 18-24 months. In appendix B-1, I show that the dynamic effects of separation from a payroll-covered job (described below) exhibit a similar pattern.

incidences of financial distress typically coincide with, rather than substitute for, declines in consumption. Moreover, dynamic consequences of default in terms creditworthiness can be severe. A typical default instance can reduce credit scores by as much as 30 points (see figure F-6), with implications for interest rates and borrowing limits, as well as apartment leasing or job-finding. Liberman (2016) finds that such credit constraints can have significant welfare implications, estimating a typical willingness to pay of 11 percent of monthly income for a clean credit reputation. To the extent possible, I directly examine these longer-term effects by estimating event studies where the credit score or borrowing limit is the outcome over a longer (three-year) time horizon.

3.4 Payroll records

Access to monthly payroll records for a subset of large employers was also provided by one of the major credit bureaus. The payroll records are quite thin and include no information on occupations or employers, but do provide earnings in each month for the subset of individuals working at a payroll-covered employer. I rely on these payroll records to explore whether unplanned shocks can impact employment arrangements.

In my analysis sample of cited drivers, about 12-15 (16-18) percent of motorists have earnings in the payroll records in a given quarter (year). To better understand what these data capture, appendix B-1 compares summary statistics from the payroll records with information on employment and earnings in the ACS microdata (Ruggles, 2023). Based on the ACS, the employment rate for a comparable sample of Floridians over this period was between 68 and 72 percent, suggesting that the payroll records cover about 20-25 percent of total employment in the state. For those in the payroll records, annualized earnings are about 25 percent higher than in the average job held by a demographically comparable sample in the ACS, consistent with existing evidence that larger firms pay higher wages (e.g., Brown & Medoff 1989; Cardiff-Hicks et al. 2015). I also find, via event studies, that transitions out of the payroll database are followed by increases in financial distress.

Hence, while the low coverage of the payroll records implies that transitions in and out of the payroll records do not necessarily correspond to transitions in and out of employment, the available evidence suggests that working in a payroll covered job captures something meaningful. At the very least, changes in the likelihood that a driver works in a payroll-covered job (which I term *payroll employment* when presenting the results) suggest an elevated rate of job transitions, which I interpret as evidence of employment instability.

Another benefit of the payroll records is that they provide a true income measure for a subset of the sample. As described in appendix F-4, I use the payroll information to construct an estimated income measure at baseline for the full sample and then use that measure to explore heterogeneoity by income. I also present results focusing only on the subset of motorists in the payroll records at baseline, splitting that sample by earnings.

4 Empirical strategy

4.1 Event study approach

I leverage the variation in the timing of traffic stops for identification with an event study approach. Specifically, letting i index individuals and t index calendar time (in quarters), I estimate equations of the form:

$$Y_{it} = \sum_{\tau} \alpha_\tau + \phi_i + \kappa_t + \epsilon_{it} \tag{1}$$

where $\tau = t - \tilde{t}_i$ indexes "event time," with $\tilde{t}_i$ denoting individual $i's$ treatment timing, which I refer to as a their "cohort" (Sun & Abraham, 2021).

Of course, a wave of recent econometric scholarship has documented the various empirical issues associated with estimating event study models with two-way fixed effects (TWFE) via OLS (e.g., Chaisemartin & D'Haultfoeuille 2020; Goodman-Bacon 2021; Sun & Abraham 2021; Callaway & Sant'Anna 2021; Borusyak et al. 2022; Roth et al. 2022). Some of the important concerns raised in this literature include the contamination of treatment effect estimates created by comparisons between currently treated and previously treated units and underidentification problems in fully dynamic specifications with no untreated group. To address these issues, I estimate the event studies using the method of Callaway & Sant'Anna (2021). Their approach is to construct estimates for each cohort and time period, based only on comparisons between each cohort and those treated in the future, and then aggregate these cohort $\times$ period effects into event study parameters.[6]

Estimated via the Callaway & Sant'Anna (2021) approach, the event study design relies only on comparisons between individuals treated in period t and those treated in future periods. Hence, identification relies on the following parallel trends assumption: in expectation, following a traffic fine, individuals would have trended similarly to those fined in the future, had they not been stopped at that date. To test for potential violations of parallel trends prior to treatment, I adopt the strategy of Borusyak et al. (2022). Specifically, I regress the outcome on a set of pre-treatment event time indicators, as well as individual and time fixed effects, using only the sample of not-yet-treated observations, and perform a joint significance test of the event-time indicators. I use the first four pre-treatment quarters as the time horizon for this pretrends test, because at least four quarters of pre-treatment data are observed for each cohort, and report p-values from this test.

There are two important identification concerns that bear mentioning here. First, many types of traffic infractions could signal changes in financial distress *ex ante*. For example, a citation for a broken tail-light or expired registration could be induced by a deteriorating financial situation. For this reason, I focus the event study analysis on speeding violations.

[6] As shown in figure A-7, estimates from the alternative methods of Sun & Abraham (2021) or Borusyak et al. (2022) are remarkably similar.

Figure A-2 compares the pre-stop trends in financial distress, estimated via the Borusyak et al. (2022) approach, for speeding violations and non-moving violations. For non-moving offenses, the majority of which are paperwork or equipment infractions, a strong pre-citation trend ($p < 0.001$) in financial distress is evident. On the other hand, there is no such trend for speeding violations ($p > 0.35$), suggesting that the precise timing of a speeding stop is unrelated to changes in an individual's financial situation.

Second, a traffic citation of any type could signal a change to an individual's driving patterns. There is some evidence to support this concern in the data. As shown in panel (a) of figure A-4, the likelihood that an individual has an open auto loan increases by about one percentage point in the six quarters prior to a traffic stop. On one hand, this is an important concern, suggesting that a car purchase, which could signal other changes in an individual's situation, sometimes directly precedes a traffic fine. On the other hand, pre-stop trends in the outcomes of interest are consistently zero and the presence of an auto loan on file is an imperfect indication that an individual is actively driving: less than half of the individuals in the event study sample hold an open auto loan in the quarter of their traffic stop. Moreover, as shown in panel (b) of figure A-4, the timing of an auto purchase tends to coincide with *improvements* in an individual's financial situation, as summarized by their credit score, suggesting that bias in the event study estimates could be towards zero.

Nonetheless, I take this concern seriously. As robustness, I estimate event studies for each car purchase timing group (e.g., individuals who first purchase cars in 2011Q2) and then aggregate up across the groups. These estimates leverage only the staggered timing in traffic stops within groups of individuals who purchase cars at the same time (and prior to their traffic stops) and are strikingly similar to the baseline results. As an additional robustness exercise, I describe and implement an alternative identification strategy which does not rely on variation in the timing of traffic stops in appendix D. This supplementary instrumental variables approach compares motorists who are stopped and cited at the same time but face different fine amounts, with variation in these fine amounts generated by heterogeneity across officers in ticket-writing practices (Goncalves & Mello 2021; Goncalves & Mello 2023). While this approach has the benefit of not relying on variation in timing, it also has several important downsides. First, standard errors are about three times larger in the IV approach. Second, imprecision and institutional features preclude the IV approach from speaking to heterogeneity on key dimensions. And finally, the IV approach is particularly poorly suited to the analysis of longer-term effects because the instrument generates changes in future traffic offending, as discussed in Goncalves & Mello (2023).

4.2 Sample construction

Motorists included in the event study sample are drawn from the initial sample of 2.6M individuals who, as of January 2010, are aged 18–59 and have a credit report with a nonmissing credit score. I start with all speeding citations attributable to this set of drivers and impose the following conditions: (i) speeding is the only violation on the citation; (ii) charged speed between 6 and 29 MPH over the limit (speeds below 6 are statutory warnings and speeds above 29 require a court appearance); (iii) motorist race is either white, Black, or Hispanic. I then select the first such stop for each individual and require that the driver has no other stops in the previous year ($N = 525,646$). Figure A-3 shows the distribution of treatment timing ("cohorts") in the event study sample as well as variation across cohorts in salient motorist characteristics.

Column 3 of table 1 reports average baseline characteristics for the event study sample. 45 percent of motorists are female, 59 percent are white, and the average age is 36. Column 4 of table 1 shows that the sample used in the supplementary IV analysis is similar in terms of baseline characteristics (described in appendix D). Interestingly, the analysis sample(s) appear positively selected relative to the full set of motorists on file: those in the event study study sample have credit scores which are 20 points higher and have about $300 less in collections debt at baseline.

In the event study sample, about 60 percent of motorists can be identified as having paid their fines for sure ("definitely paid") based on the traffic court disposition associated with the citation. Around 30 percent *may* have received reduced sanctions through the traffic court system ("possible lenience") and around 10 percent *may* have faced a license suspension ("possible suspension") due to nonpayment. See appendix B and section 6.1 for an expanded discussion of these definitions, as well as accompanying heterogeneity analyses.

5 Results

Figure 2 presents event study estimates for default outcomes. In each figure, I report the p-value from the Borusyak et al. (2022) pretrends test and the static ATT estimate (the cohort weighted average from Callaway & Sant'Anna 2021) Note that, in all cases, I cannot reject the null hypothesis of parallel trends ($p > 0.357$). To assist with interpreting magnitudes, I also report estimated post-treatment counterfactual means, which are constructed by regressing the outcome on individual and time effects using only the sample of not-yet-treated observations and averaging predictions from this regression over event time (e.g., Kleven et al. 2020, Borusyak et al. 2022).

As shown in panel (a), the probability of a new default flag appearing on an individual's credit report increases by about one percentage point in the year following a traffic stop, relative to the comparison group of motorists cited in the future. This effect represents about a five percent increase relative to a mean of 0.216. The smaller static ATT ($= 0.005$) implies a relative decline in the probability of new distress flags in the longer term.

Panel (b) presents event study estimates for collections balances, which I interpret as the extent to which households borrow out of other financial obligations to cover their fines.

Collections balances increase by about \$34 in the six quarters following a traffic stop. Scaled by the average statutory fine of \$195 (which may differ from the amount paid, as discussed below in section 6.1), this finding implies that, on average, households finance about 17 percent of their fines by "borrowing" out of other financial obligations such as utility or medical bills. Panel (c) of figure 2, along with table 2, which presents the corresponding regression estimates, also shows impacts on the number of credit lines at least 90 days past due, the number of derogatory credit lines, and the number of accounts in collections.

5.1 Heterogeneity by financial buffer proxies

Of course, we should expect the impacts of fines on default to vary by whether a household has a financial buffer to draw on. I first explore the role of access to liquidity in explaining treatment effect heterogeneity by splitting the sample into two groups based on their credit card situation at baseline: those with at least \$200 in available balances on credit cards ($N = 301,318$) and those without ($N = 224,228$), which includes both those without a credit card at baseline ($N = 175,643$) and those with maxed out credit cards at baseline ($N = 48,585$).[7] Note that, since this cut is defined at baseline, some individuals may "switch groups" between the baseline period and their treatment date.

As shown in panels (a) and (b) of figure 3, this proves to be an especially salient cut of the data. Estimated impacts of fines on the probability of new default and on collections balances are about three times larger (ATT = \$43.88) for those without access to liquidity than those with at least \$200 in available credit card balances at baseline (ATT = \$14.83).

As shown in panel (c), credit card balances increase by about \$19 (se = \$6) in the first quarter following a traffic stop for those with available credit card liquidity, suggesting that these households finance about 10 percent of a typical fine through credit card borrowing. In the longer-term, there is a pronounced decline in credit card balances for this group. As discussed in section 3.3, one reason to expect such a pattern is the impact of increased rates of default on access credit.

The fact that the long-run declines in card balances are attributable to the group with available liquidity at baseline highlights a subtle but important interpretation point stemming from two underlying features of the data and setting. First, as suggested by figure F-6, the credit score penalty associated with default appears more severe for individuals with higher initial credit scores. And second, as illustrated in figure F-5, the relationship between credit scores and borrowing limits is convex. For individuals with good (> 700) credit scores, a ten point credit score decline is associated with a \$2,550 decline in borrowing limits; the same credit score decline is associated with a \$880 decline in borrowing limits for

[7] To minimize concerns about mean reversion when constructing this sample split, I compute available balance on credit cards in each quarter, defined as the revolving limit minus the revolving balance, both summed across all revolving accounts, average across the first four quarters (2010Q1-2010Q4), and define an individual as having \$200 in liquidity if this average exceeds \$200.

an individual in the middle of the credit score distribution (500-700) and a \$140 decline for an individual with a deep subprime credit score (< 500). Hence, while the impacts of fines on default are dramatically attenuated for the liquid group, these small increases in missed bills can generate comparatively large declines in credit limits. Highlighting this point is panel (d) of figure 3, which illustrates that increases in credit card utilization, defined as total balances divided by total limits, are similar for both groups.

Easy access to credit card borrowing is, however, far from the only measure of a household's financial buffer or overall liquidity. In figure 4, I further split the sample by both available credit card liquidity and by baseline estimated income (cutting at the median $\approx$ \$31,000), using the income measure described in appendix F-4. This sample split results in four groups of motorists: higher income with credit card liquidity ($N = 232,230$), higher income without credit card liquidity ($N = 56,046$), lower income with credit card liquidity ($N = 69,088$) and lower income without credit card liquidity ($N = 224,328$).[8]

As shown in panel (a), estimated impacts on collections are most dramatic for the low-income, illiquid group (ATT = \$49). The next highest estimate is for higher-income, low-liquidity motorists (ATT= \$32), with smaller and comparable effects in the two subgroups with at least \$200 in available credit card balances (high income ATT = \$13; low income ATT = \$20). Interestingly, panel (b) illustrates a reversal of these patterns, at least with respect to the mitigating role of access to credit, when examining delinquencies on credit lines. Here, the largest effects are for the low-income but liquid subgroup, followed by low-income illiquid, high-income liquid, and high income illiquid.

As discussed in section 3.3, a potentially important consideration here is differential rates of formal borrowing: individuals must maintain open borrowing accounts in good standing in order to attain delinquencies on their credit reports. As of one quarter prior to the traffic stop, the shares of each group with at least one open credit line with the potential to transition into delinquency are: 87 percent (high income and liquid); 43 percent (high income and illiquid); 77 percent (low income and liquid) and 43 percent (low income and illiquid). Hence, these results highlight that delinquency impacts are larger for those with lower incomes and those with a greater potential for delinquency given *ex ante* borrowing.

Panel (c) of figure 4 illustrates, unsurprisingly, that the short-term increase in credit card balances seen in the prior figures is most pronounced for the subset of motorists with lower incomes and available balances on credit cards. For this group, the one-quarter event study estimate is $\beta = \$30$, or about 15 percent of a typical fine. The comparable estimate is about half the size ($\beta = \$16$) for the subset of high-income and liquid drivers and below \$10 for both illiquid subsamples. As in the previous figure, panel (d) shows estimates for revolving

[8] Event study estimates for credit card borrowing in the full sample are presented in table G-1 and figure G-3. Event studies for all outcomes estimated separately by estimated income and liquidity status are presented in figures G-2, G-3, G-6, G-7. Tables G-2 and G-3 report the regression estimates underlying figures 3 and 4, respectively.

utilization to confirm that longer-run declines in credit card borrowing can be explained by reduced credit access.

The patterns in figures 3 and 4 suggest a reasonably straightforward hierarchy of behavior with respect to baseline financial situation. Individuals with the largest buffer, proxied by income and available credit card balances, appear to cover the majority of the fine with cash on hand, as suggested by the low rate of credit card borrowing and minor impacts of default. Those who cannot cover the fine in cash first rely on credit card borrowing, as evidenced in particular by the borrowing patterns of the subset of low-income drivers with available credit card balances, followed by "borrowing" through delaying repayment on credit lines (i.e., delinquency). And finally, borrowing out of other financial obligations, which ultimately results in collections activity, is the "last resort" for covering unplanned expenses.

5.2 Longer-run effects

I interpret the results on default primarily as providing evidence on the *ex ante* financial situations of households: the fact that the average household finances 17 percent of a fine payment through default on items such as utility bills suggest an inability to cover a $200 expense via cash on hand, for example. This interpretation is bolstered by the above heterogeneity analyses, which reveals a clear hierarchy of payment sources.

However, increased default does not necessarily have a clear interpretation in terms of household wellbeing. If default allows consumption smoothing (Morduch & Schneider, 2016) at minimal costs, for example, then welfare could actually be increasing as collections balances accrue. As discussed in section 3.3, the welfare consequences of default are likely to play out in the longer term in the form of tighter borrowing constraints, higher interest rates, and other consequences of a diminished credit reputation or worsened financial standing, such as difficulty securing housing or employment.

In figure 5, I present event study estimates for credit scores and borrowing limits over a three-year time horizon. This figure presents estimates from both the baseline Callaway & Sant'Anna (2021) approach and an approach based on Sun & Abraham (2021) which compares only those cited in 2011–2012 to those cited in 2015Q4. I present the Sun & Abraham (2021) results, which hold the "control group" constant, to confirm that longer-run patterns are not driven only by compositional changes in the DiD comparisons.

As shown in panels (a) and (b), traffic fines are associated with a 2.6 point decline in credit scores and a 1.6 percentage point increase in the likelihood of having a subprime credit score, with both effects persisting for three years following a traffic stop. Coinciding with the credit score declines, I find that borrowing limits fall by about $330 over three years.[9] In

[9] One complication with interpreting the estimated impact on borrowing limits is that individuals must maintain open borrowing accounts in order to observe their credit limits in the data. One could alternatively replace the observed credit limit (which equals zero for those with no open lines)

figure A-8, I show that coinciding with these longer-run declines in creditworthiness are lower rates of home ownership (proxied by mortgages), geographic mobility (proxied by whether an individual's address was updated on the credit file), and attachment to the formal financial sector (proxied by whether an individual has any open credit line).

Panel (d) of figure 5 shows declines in the likelihood of working in a payroll-covered ("payroll employment") beginning in the first quarter following a traffic stop and persisting in the medium-to-long term. Three years out, the estimated decline in the payroll employment rate is about 1.2 percentage points, or 8 percent relative to a mean of 15 percent. As discussed in section 3.4, this measure does not necessarily capture employment versus unemployment. However, the observed decline suggests an increased rate of job transitions, which I interpret as employment instability, and reduced likelihood of working for a subset of employers that pay above average wages.

5.3 Additional payroll employment results

To further unpack the impact on employment arrangements documented in panel (d) of figure 5, I first split the sample according to baseline payroll employment status. Specifically, I estimate effects separately for individuals who are consistently employed in a payroll-covered job at baseline ($N = 55,140$) and the rest of the sample ($N = 470,506$).

Panels (a) and (b) of figure 6 illustrate that the impacts of fines on collections balances and payroll employment are present and comparable in both of these subsamples. In particular, panel (b) implies that traffic fines both reduce the likelihood of transitions into the payroll records for those not in the payroll-covered jobs at baseline and accelerate transitions out of the payroll records for those who are "employed" at baseline. The point estimate associated with the latter effect is slightly larger than (-0.007 versus -0.005), but estimates are much less precise for the considerably smaller baseline employed sample. Moreover, in proportion to the means, the impact for the baseline unemployed group is substantially larger.

In panels (c) and (d), I show results using only the subsample in the payroll records at baseline and split motorists at the median of annualized payroll earnings ($\approx \$34,000$). Both panels reveal stark heterogeneity by baseline income. For "employed" motorists with below median earnings, collections balances increase by \$84 and the likelihood of working in a payroll-covered job declines by 1.2 percentage points. For those with above median earnings, the comparable estimates are \$20 and 0.4 percentage points, with neither effect statistically distinguishable from zero. One could view panels (c) and (d) as a reassuring placebo test for the validity of the event study approach: individuals with stable employment

with an imputed credit limit based on (i) the credit score and (ii) the nonlinear cross-sectional relationship between credit scores and borrowing limits. One can view this measure either as a way to rescale the credit score estimate into a more useful magnitude or as capturing the credit limit an individual is eligible for, based on their credit score, were they to apply for new revolving credit. I find that imputed limits also fall by over \$100 in the six quarters following a traffic stop.

and above-median earnings at baseline experience no detectable change in either collections debt or the likelihood of working in a payroll-covered job following a traffic stop.

A natural question is whether the estimated impact on employment arrangements can be attributed to institutional features such as DL suspensions or other increases in driving costs rather than the financial shock of a traffic fine. I discuss this question in much more detail below in section 6, but to preview briefly, the available evidence suggests that the lion's share of the documented impact on payroll employment (as well as the other outcomes I study) can be explained by fine payment. These findings raise the question of why paying an unplanned expanse may affect employment arrangements.

As shown in figure 6, declines in the likelihood of working in a payroll-covered job are concentrated among lower-income motorists, who also see the largest increases in financial distress. Hence, the hypothesis that weaker financial standing and a diminished credit reputation induces employment instability or reduces the ability to obtain or hold good jobs is at least consistent with the evidence. In particular, a lower credit score could affect job-finding directly (e.g., Bartik & Nelson 2021; Bos et al. 2018) or indirectly through a compromised ability to secure new housing, for example.

While this channel is a reasonably compelling explanation for reduced transitions into the payroll records, it has less bite as an explanation for the increased separation rate documented in panel (d) of figure 6. This pattern is, however, consistent with the result of Dobbie & Song (2015) that financial distress reduces labor supply and the finding in Barr et al. (2023) that cash transfers in a particularly financially-constrained sample increase labor supply. My results are also consistent with a growing body of work documenting the psychological costs of financial distress (e.g., Mullainathan & Shafir 2013; Schilbach et al. 2016), including lower productivity (Kaur et al., 2021). Disentangling these potential explanations is beyond the scope of this paper but presents an interesting avenue for future research.

One could alternatively ask to what extent the payroll employment effects can themselves explain the observed increases in financial distress. In trying to answer this question, there are two important considerations. The first is dynamics: as discussed in the section 3.3 and shown in figure B-2, new defaults induced by changes in (payroll) employment status will typically take several quarters to accrue on a credit report. By this logic alone, the gradual changes in payroll employment status would appear quite unlikely to explain the initial increases in default in the first six quarters following a traffic stop.

The second consideration is magnitudes. As shown in table 1, average monthly earnings in the payroll records are \$3319. Hence, the full-sample ATT estimate for payroll employment (-0.006) implies a \$20 decline in monthly earnings. Abstracting from dynamics, this earnings change would predict a \$4 increase in collections debt (about 11% of the overall effect = \$34) based on the collections-earnings elasticity estimate in figure B-3. Note that, on the one hand, this calculation likely overstates the contribution of changes in payroll employment, as it assumes zero earnings for those not in the payroll records. One the other hand, this calculation could understate the role of employment changes in explaining increased default rates if the the effects on working at payroll-covered employers generalize to other employment margins, which unfortunately cannot be tested.

5.4 Robustness

A central concern for the validity of the event study estimates is the possibility that traffic stops are preceded by significant life changes that, for example, result in increased driving and also predict declines in financial situation. Bolstering this concern is the finding that car purchases, proxied with the presence of an open auto loan on the credit file, increase over the six quarters leading up to a traffic stop, as shown in figure A-4. To partially address this concern, figure A-6 reports event study estimates computed within auto purchase cohorts.

Specifically, for each individual i, I first compute the first quarter in which I observe them as having an open auto loan on the credit file $\tilde{z}_i$. I then estimate event studies separately for each $\tilde{z}$ group, using only individuals whose traffic stop occurs after their auto purchase, and aggregate up these group-specific estimates, weighting by their sample shares. These estimates leverage staggered variation in the timing of traffic stops only within groups of individuals who purchase cars at the same time. Note that this exercise is very similar in spirit to the procedure of Freyaldenhoven et al. (2019), who suggest using changes in a relevant observable to net out trends in the outcome potentially attributable to changes in unobservables around the timing of an event.

Using this approach, I find estimates for payroll employment and credit card borrowing that are nearly identical to the baseline event study estimates. Estimated effects for default measures are, if anything, larger when conditioning on the timing of auto purchases, which is sensible in light of the finding from figure A-4 that the timing of car purchases tends to coincide with an improving financial situation.

In figure A-7, I also show that the choice of method for estimating the event studies has no bearing on the empirical conclusions. Estimates based on either the Borusyak et al. (2022) or Sun & Abraham (2021) approach are nearly identical the baseline estimates using the Callaway & Sant'Anna (2021) method. Because the Sun & Abraham (2021) approach uses only the final cohort as the "control" group and thereby relies on fewer comparisons, standard errors about 75 percent larger. Hence, the Callaway & Sant'Anna (2021) estimates are preferable on precision grounds. As detailed in appendix F-5, computing constraints prevent the computation of standard errors for the Borusyak et al. (2022) approach.

In appendix D, I present results from the complementary instrumental variables design. This approach compares two motorists cited at the same time and in the same area but ticketed by officers with varying propensities to issue harsh (versus lenient) fines. Estimates from this approach suggest that a \$125 increase in statutory fines is associated with a \$43 (se = \$15) increase in collections balances over the following six quarters. Note that as a fraction of the relevant first stage (= 0.34), this is larger than the event study estimate (= \$34/\$195 = 0.17). The IV design also yields a qualitatively similar pattern of results for credit card borrowing and payroll employment, but estimates for these outcomes are too imprecise to draw firm conclusions.

6 Discussion

6.1 Quantifying the shock

In terms of interpreting the empirical findings, a critical question is how sizable of a financial shock is the average traffic citation in practice. As discussed in section 2, the ultimate sanctions faced and paid by each motorist depends both on the statutory sanctions associated with an offense and on post-citation choices made by motorists, such as whether to contest a citation in traffic court. One could think of the task at hand as estimating a "first stage" by which the event study estimates should be scaled.

Using data on traffic court dispositions associated with each citation, I focus on quantifying three types of costs: fine payments, court fees, and increases in auto insurance premiums triggered by the accrual of points on a motorist's driver license. The central challenge I face is that the disposition records only provide definitive information on a citation's outcome in a subset of cases. In the event study sample ($N = 525,646$), 33.2 percent and 25.7 percent of citations have dispositions indicating fine payment and traffic school, respectively. For these citations, statutory fines were paid in full and there were no associated court fees. Those with paid dispositions accrue the DL points associated with their offense, while those with school dispositions do not accrue license points.

The remaining 41 percent of citations in the sample, with disposition codes of guilty (6 percent), dismissed (8.7 percent), "adjudication withheld" (24.6 percent), or a missing disposition (1.8 percent), present significant interpretation challenges regarding the ultimate outcome of the citation. Missing dispositions could indicate non-payment or an issue with the underlying data. Guilty dispositions most likely indicate that an individual contested in court but "lost" and ultimately paid a fine, but may alternatively signal non-payment and thus license suspension. Those with dismissed or withheld dispositions attended a traffic court hearing and likely received lenience at that hearing, but exactly the form of lenience is unknown. For example, those with dismissed verdicts may have "pled down" and then paid the fine for a lesser offense. Those attending traffic court would have been subject to a statutory \$75 court fee, which may or may not have been waived at the hearing. The Florida Clerk of Courts office has indicated that, in their estimation, the vast majority of individuals with dismissed or withheld dispositions likely avoided the accrual of license points but a large share of this group ultimately paid (likely reduced) fines.

My baseline estimate of paid fines combines the knowledge that those with paid or school verdicts (59 percent) paid the full statutory fine with an assumption that those with missing or guilty verdicts (8 percent) paid the full statutory fine, while those with dismissed and withheld verdicts (33 percent) paid half the statutory fine, giving an estimate of average fine payments = $158.65. I assume that $75 court fees were paid by those with guilty verdicts, waived for those with dismissed verdicts, and paid for half the citations with a withheld verdict, giving an estimate of $15.07 in court fees paid.

To compute costs of citations stemming from increases in auto insurance premiums, I assume that those with paid, guilty, and missing verdicts (41 percent) accrue statutory points. Those participating in traffic school (26 percent) do not accrue points, and I further assume that those with withheld and dismissed verdicts (33 percent) do not accrue points, based on information provided by the Florida Clerks. I then map accrued points to insurance premium increases based on information from law firm and personal finance webpages. I estimate that the average annual premium is $2,014.56 and that premiums increase by 11 and 12 percent following a 3-point (speeds 6-14 MPH over the limit) and 4-point (speeds 15-29 MPH over the limit) citation, respectively.[10] To summarize, I estimate that 41 percent of citations are associated with increases in auto insurance premiums, with an average increase in annual premiums for that subset of $227, giving a sample-wide average estimate of $93 (or about $23 per quarter). Insurance premiums are only updated to reflect accrued DL points once a contract is renewed, and I assume that contract updates are distributed evenly throughout the year in my calculations.[11]

Panel (a) of figure 7 illustrates these estimates graphically. Factoring in the distribution of statutory sanctions and the post-citation behavior of motorists, I estimate that in the quarter of the citation, the average payment of fines and court fees equals $174. Insurance premium increases phase in over the first three quarters and reach a maximum of $23 per quarter. Panel (b) illustrates the estimated cumulative costs, factoring in both the initial payment of fines and fees and the additional insurance payments over time. My preferred estimate is that, after six quarters, the average motorist has paid $302 in costs associated with their traffic citation.

Shaded regions in panels (a) and (b) report "confidence bands" that illustrate the sensitivity of these estimates to various assumptions about the outcomes of citations with court-related verdicts and insurance cost increases, described in more detail in appendix B-2. Upper and lower bound estimates on combined fine and court fee payments are $114 and

[10] Note that an internet search will turn up a range of estimates of the average annual car insurance premiums in Florida that are around $3,000. Importantly, these are *current estimates as of 2023*. The Bureau of Labor Statistics estimates that average auto insurance premiums increased by 58 percent between 2013 (the mid-point of my sample) and 2023. Based on this inflation rate, my estimate of $2,015 is equivalent to $3,183 in 2023. The 11 and 12 percent increase estimates are taken from Gorzelany in Forbes, 5/17/2012.

[11] For a more thorough discussion of estimates of the average paid fines, average paid court fees, and average increases in insurance premiums, see appendix B-2.

$216 respectively. If one assumes that all motorists with dismissed and withheld verdicts accrue points *and* that insurance premiums increase by 18 and 16 percent (instead of 12 and 11 percent), estimated increases in annual insurance premiums increase to $252. Combining this "worst case" scenario with the upper bound on fine payments gives an upper bound on cumulative costs over six quarters equal to $562. On the other hand, assuming the lower bound on fine and fee payments and a lower rate of insurance increases (which may be the case given that the majority of individuals in my sample are first-time offenders) gives a lower bound estimate of total costs equalling $175. To summarize, I interpret the average traffic fine as associated with an up-front payment of about $175 in fines and fees and then additional auto insurance payments of about $125 over the next six quarters. The range of total cumulative cost estimates ($175–$562) is comparable to the typical $400 "emergency expense" considered in surveys, discussed below in section 6.3.

Increases in insurance costs persist for three years following a citation, after which insurance companies no longer consider the offense when pricing. My middle-ground estimate of total costs paid over three years is $443, but over this longer time horizon, I cannot rule out total cumulative costs as large as $940 (with $724 in total insurance payments). On one hand, these more significant long-term costs could play a role in generating the long-run effects presented in figure 5. On the other hand, the analysis below provides suggestive evidence that effects are similar even for those with no changes in insurance premiums.

6.2 Fine payment versus other mechanisms

To shed some light on the relative importance of fine payment *per se*, as opposed to other institutional features such as traffic court involvement, the accrual of license points, or license suspensions imposed on non-payers, in explaining the observed effects, figure 8 presents event studies for subsamples based on the traffic court disposition associated with the citation. While I view this exercise as descriptively useful, it is important to note that these results should be interpreted with caution, as the sample is being split on the endogeneous, post-citation choices made by motorists.

In panels (a) and (b), I show event study estimates for collections balances and payroll employment for the full sample, as well as for three subgroups: (i) those with dispositions indicating fine payment;[12] (ii) those with dispositions indicating dismissal or withheld adjudication, which I call the "possible lenience" subgroup; and (iii) those with missing or guilty dispositions, which a call the "possible suspension" subgroup. As discussed above, there are important interpretation challenges associated with both of the latter two categories. In particular, it is temping to view the "possible lenience" subgroup as a placebo group,

[12] To keep the benchmark results the same throughout figure 8, the payer group in panels (a) and (b) is the group with verdict = 4, which is a subgroup of the "definitely paid" sample, as that sample also includes those who attend traffic school. Estimates for the entire "definitely paid" sample are similar to those for this group, as can be seen from panels (c) and (d) of figure 8.

but there are several important caveats to this interpretation. This subgroup of individuals must have attended traffic court during a workday and would have faced a $75 court fee, which may or may not have been waived regardless of the outcome of the court hearing. Moreover, individuals who receive only partial reductions in penalties via traffic court, such as a reduced charge or a waiving of license points, will show up in the disposition records as having their case dismissed or adjudication withheld. Hence, a sizable share of this group almost surely faced some form of (albeit reduced) sanctions.

Aligning well with these institutional caveats, panels (a) and (b) of figure 8 show that estimated impacts for this "possible lenience" group are significantly attenuated relative to the the estimates for fine payers, about half the size in each case, but non-zero. For the subgroup of motorists who may have faced DL suspensions due to nonpayment, the increase in collections balances is significantly more pronounced relative to the the increase for fine payers. For payroll employment, however, this dimension of heterogeneity is less stark; while the point estimates are consistently more negative for the possible suspension group, the estimated overall ATT is similar in both subsamples.

Arguably the most important takeaway from panels (a) and (b) of figure 8, however, is the fact that estimates for the subgroup who can be identified as paying their fines for sure are similar, and if anything slightly larger, than estimates in the full sample. This finding suggests fine payments (and insurance costs), as opposed to license suspensions, as the primary driver of the financial distress and employment instability effects in the full sample. Importantly, this result also highlights that the main collections balances estimate cannot be explained by collections originating with unpaid citations, since those with paid fines would not be subject to collections activity associated with their citation.

In panels (c) and (d) of figure 8, I assess the relative importance of driver license points, which are accrued by fine payers and can affect future car insurance premiums, in explaining the results. Specifically, I compare effects for those with "paid" (same as above) and "traffic school" disposition verdicts. Traffic school attendees are required to pay their fines but, in return for completing a four hour course, do not accrue the driver license points associated with the citation. Hence, comparing effects for these two groups can shed light on the relative importance of accruing DL points. For both collections balances and payroll employment, estimates appear similar, but slightly smaller, for those attending traffic school.

For collections balances, these estimates indicate a modest downward trend in collections balances prior to the citation, which perhaps make sense: those with the wherewithal to opt for traffic school are also those with improving financial situations. Therefore, I also present estimates for the traffic school group, reweighting these motorists to match the characteristics of the payer sample based on baseline age, gender, race, and quartiles of credit score and estimate income. This reweighting eliminates the downward pretrend for the traffic school group and slightly increases the treatment effect estimates. With reweighting, the effects for payers (ATT = $43) and school attendees (ATT = $40) are remarkable similar, suggesting a minimal role for DL points in explaining increased financial distress. Reweighting also reduces, but does not quite close, the gap in the payroll employment estimates for payers (ATT $= -0.008$) and school attendees (ATT $= -0.006$).

In appendix figure A-9, I present results from an additional exercise which compares estimates for motorists who are cited in their county of residence and those who are cited further away. The idea of this exercise is that individuals must attend court to receive a penalty reduction and that penalty reductions have a disproportionate affect on future insurance costs. Local residents are about 20 percentage points more likely to have dismissed or withheld verdicts instead of paid verdicts, translating to a small reduction in estimated paid fines ($\approx$ \$9) but a relatively large reduction in insurance cost increases from citations ($\approx$ \$46). With the caveat that estimates are imprecise for the much smaller "distant" group, I find similar effects for both groups of motorists, which further suggests that the initial fine payment, as opposed to downstream increases in auto insurance premiums, is the primary driver of the effects that I document.

6.3 Financial fragility in surveys

The central contribution of my analysis is the finding that typical, unplanned expense shocks can have important implications for household financial situations. I interpret the headline event study results on collections balances as evidence that, on average, households must default on other financial obligations to finance an unexpected \$175 payment (or an unexpected \$300 payment over six quarters after factoring in insurance costs). This interpretation is supported by the analysis in section 5.1, which suggests that default ultimately leading to collections activity is the "last resort" for households, as well as the finding from section 6.2 that default effects are comparable for the sample of fine-payers.

Motivated by the much-cited survey evidence on the share of households self-reporting an inability to cover emergency expenses (e.g., FRBG 2018), a related question of interest is about the *distribution* of ability to cover emergency expenses. In other words, based on my estimates, what share of individuals cannot cover traffic fines without defaulting on other obligations and how does that share compare with prominent survey estimates? Writing the treatment effect of fines on financial distress for individual i as $\Delta_i = Y_i(1) - Y_i(0)$, this amounts to quantifying $\pi = Pr(\Delta_i > 0)$.

A natural first step here is to address a lingering question about the event study results. At first glance, the estimated impacts on the probability of new distress events (i.e., on the extensive margin) and on collections debt balances (i.e., the intensive margin) may seem inconsistent. The former suggests "small" effects of fines on the share of households with any financial distress while the latter suggests larger effects on default amounts. They key to reconciling these two results is to note the remarkably high counterfactual mean on the extensive margin: $\mu = 0.22$. In other words, financial distress is quite common in this sample and many households would have missed a bill, regardless of whether they faced traffic fines. However, the unexpected fines induce *additional* default on the intensive margin for those who would have defaulted to some extent.

Hence, an accurate characterization of the fraction of households affected by fines requires consideration of both the extensive and intensive margins. The share of households induced to borrow from other financial obligations depends on the full distribution of treated and untreated potential outcomes and therefore unidentified by the event studies (Borusyak, 2015). However, one can place ad-hoc bounds on this notion of π by making assumptions about the distribution of treatment effects. For example, under the assumption that $\Delta_i \in \{0, \bar{\Delta}\}$, π is identified by $\hat{\theta} = \pi\bar{\Delta}$, where $\hat{\theta}$ is the average treatment effect estimate.

A useful starting point, then, is to set $\bar{\Delta} = \$302$; i.e., households are either unaffected by fines or are induced to miss the entire estimated average cost of a citation (\$302 is the estimated average cumulative cost, including insurance premium increases, as discussed above in section 6.1). Taking the lower 95 percent confidence bound of the ATT estimate for collections balances, $\hat{\theta} = \$29.99$, then, yields an estimated $\pi \geq 0.099$. Importantly, this is a *sharp* lower bound for π as long as the distribution of treatment effects is bounded below by zero and above by \$302, because a larger π can always be rationalized by allowing probability mass on other values of $\Delta_i \in (0, 195.53)$.

Of course, this lower bound is likely too conservative. On average, fines also induce default on credit lines, suggesting that defaults which ultimately lead to collections are not the only margin of adjustment. Further, there is no evidence in the data for treatment effects as large as $\bar{\Delta} = \$302$. Estimating the collections balance treatment effect using only the bottom five percent of the baseline estimated income distribution yields $\hat{\theta} = 52.18$ (22.98). Using the upper 95 percent confidence bound of this estimate as the upper limit for treatment effects, $\bar{\Delta} = 97.22$, gives a lower bound estimate of $\pi \geq 0.298$.

One can also estimate an upper bound on π by examining the share of individuals who are induced into their first ever default by fines. Panel (a) of figure 9 presents event study estimates where the outcome variable is an indicator for whether a motorist has accumulated any new default flag to date. For reference, just over 60 percent of the sample has already accrued a default flag as of one quarter prior to their traffic stop, while just over 20 percent of the sample never accrues a default flag over 2010–2015. Six quarters out from the traffic stop, the event study estimate is $\hat{\beta} = 0.005$ ($se = 0.0006$). In other words, of the 39.4 percent of the sample that has yet to accrue a default flag as of one quarter prior to treatment, about 1.3 percent are induced into default by fines.

Panel (a) of figure 9 reveals an important dimension of heterogeneity in the data: among individuals who are not prone to default in general, the impact of fines appears minimal. Another way to see this point is to estimate effects on collections debt for individuals with (62 percent) and without (38 percent) at least one default flag on their credit report at baseline. As shown in panel (b) of figure 9, estimated effects of fines are approximately zero for those with clean credit reports at baseline, whereas effects are about one third larger for the any default subsample than for the full sample.

Based on the following logic, the estimate in figure 9, panel (a), provides an informative upper bound on π. As of one quarter prior to treatment, 39.4 percent of the sample is at risk of being "pushed" into their first default to date by a traffic fine and 0.5 percent of the sample is induced to default. Hence, at least 38.9 percent of the sample is unaffected by fines, meaning that at most 61.1 percent is affected. To summarize, I find that $\pi \geq 0.1$ and my best estimate is $\pi \in [0.298, 0.611]$. In other words, between 30 and 60 percent of individuals are induced to borrow from other financial obligations to finance a $302 expense.

How do these bounds compare to existing evidence on this notion of financial fragility? While the most heavily-cited statistic is the finding from the 2017 Survey of Household Economics and Decisionmaking (SHED) that only 60 percent of households would cover an emergency $400 expense with cash (FRBG, 2018), the same survey includes another question which is more directly comparable to my analyses. Specifically, 15 percent of respondents indicated that they would miss other monthly bills if faced with an emergency $400 expense (Reynolds 2019; Strain 2019). Importantly, the demographic composition of my sample of traffic offenders in Florida may differ from that of a nationally representative survey, as suggested by figure 1. In appendix C, I show that, after reweighting SHED respondents to match the age and race distribution in the event study sample, the estimated share indicating that they would miss bills increases to 25 percent.

As a counterpoint to the SHED, which asks respondents to judge how they would be affected by an emergency $400 expense, Bhutta & Dettling (2018) use the Survey of Consumer Finances (SCF) to estimate that about 76 percent of households could pay a $400 expense using liquid savings. Reweighting the SCF sample to match the age and race distribution in the event study sample, this estimate falls to 72 percent, and further reweighting to match the baseline rate of delinquency in my sample reduces their estimate to 69 percent

To summarize, my preferred lower bound estimate that about 30 percent of households are induced to default by a traffic fine is larger than would be expected based on the SHED (25 percent) and comparable to what would be expected from the SCF (31 percent), after adjusting for differences in the demographic makeup of the survey samples. Hence, I view my estimates as consistent with the existing survey evidence, but also potentially suggestive of higher rates of financial fragility.

Note that we may not necessarily expect the causal evidence I present to align with the survey evidence. In the SHED, household expectations may differ from reality, and one could read my findings as suggesting that households overestimate their ability to cope with unplanned expenses. The Bhutta & Dettling (2018) estimate from the SCF could overestimate the share of households that would actually be affected by a $400 expense if some households have easy access to other sources of liquidity, such as loans from family. On the other hand, Chen (2019) has pointed out that their estimate is sensitive to various computational and modeling assumptions. As discussed in appendix C, a reweighted version of the lower bound estimate from Bhutta & Dettling (2018), which equals the share of households with at least $400 plus one month's worth of expenses, is just 48 percent.

6.4 Consumption smoothing

A vast literature in economics has examined the consumption responses to income fluctuations (e.g., Stephens 2001; Parker 2017; Ganong et al. 2020; Golosov et al. 2022; Baker & Yannelis 2017; Gelman et al. 2020; Ganong & Noel 2019). A contribution of my paper to this literature is compelling evidence on the consumption smoothing strategies of household when faced with typical negative income shocks. The evidence presented in section 5.1 implies a "pecking order," with households first drawing on cash-on-hand, followed by borrowing on credit cards, followed by delinquency on credit cards, and finally default on other obligations which ultimately results in collections. More broadly, the evidence suggests that default is an important consumption smoothing strategy for liquidity-constrained households. While surveys and ethnographies have suggested the prevalence of this behavior (e.g., Morduch & Schneider 2016), my analyses confirm this by showing that typical shocks causally induce default using a large panel of individuals.

In appendix E, I document larger effects of fines on default for Black and Hispanic motorists. My findings on these racial disparities in default risk also speak to an emergent literature on consumption smoothing differences across racial groups. In concurrent work, Ganong et al. (2020) study the differences in consumption responses to typical income shocks, leveraging bank account data and an identification strategy relying on firm-level pay fluctuations, for white, Black, and Hispanic individuals. They find that Black and Hispanic households reduce their consumption by 20-50 percent more than white households in response to transitory, negative income shocks, and accordingly argue that the welfare costs of income volatility are considerably higher for minority households.

Evaluated at the median household incomes by race in Florida, the estimates in Ganong et al. (2020) predict a $38 decline in consumption for white households and a $52 decline in consumption for minority households.[13] My results imply that, in order to prevent these declines from being more dramatic, white households borrow about $32 and minority households borrow about $48 from other financial obligations. Hence, my findings suggest that Ganong et al. (2020) may underestimate the racial gap in the welfare costs of income volatil-

[13]Median monthly household incomes for white and minority households in Florida as of the 2010 census were $4193 and $3206, respectively. The average fine, $195, thus represents a 4.5 (6) percent decline in monthly income for white (minority) households. The consumption elasticity estimates in Ganong et al. (2020) are 0.2 for white households and 0.265 for Black and Hispanic households (taking a simple average of the reported estimates for each racial group). Hence, predicted consumption declines are $0.2*0.045*4193 = 37.74$ for white households and $0.265*0.06*3206 = 51.98$ for minority households.

ity by illustrating that minority households additionally must borrow more intensely out of other financial obligations, with associated further declines in welfare through reduced access to future borrowing, when faced with negative shocks.

6.5 Social insurance

From the perspective of a policymaker, the consumption smoothing behavior of households is particularly relevant for the optimal generosity of social insurance programs. In the canonical model (e.g., Baily 1978; Chetty 2006), the optimal social insurance benefit solves:

$$\frac{u'_b - u'_g}{u'_g} = \frac{\epsilon_{p,b}}{1-p}$$

where u'_g and u'_b are marginal utilities in the "good" and "bad" states, p is the probability of the bad state occurring, and $\epsilon_{p,b}$ is the moral hazard elasticity. Holding moral hazard effects fixed, more generous benefits are socially desirable as the gap between marginal utilities in the good and bad states becomes more pronounced. If households can self-insure ($u'_b = u'_g$), there is no need for social insurance.

The empirical findings speak to the difference in marginal utilities associated with typical, negative income shocks in a dynamic sense: shocks induce default, which in turn reduce welfare in the future through tighter borrowing limits, higher interest rates, and employment instability, as discussed in section 5.2. While quantifying the implied differences in marginal utilities is beyond the scope of this paper, the results suggest the important lesson that many households are not self-insured against even typical income fluctuations. Hence, an expanded social safety net which insures against a larger set of usual shocks could yield social welfare gains, abstracting away from the obvious logistical and moral hazard concerns. Note that my findings mesh well with a nascent literature documenting the role of existing social insurance programs such as medicaid in insuring households against financial distress (e.g., Mazumder & Miller 2016; Gallagher et al. 2019).

Or alternatively, the results may imply a need for policy interventions with the goal of preventing households from reaching the point where they are unable to self-insure against typical income fluctuations, such as financial education or savings incentives (e.g., Klapper & Lusardi 2020, Lusardi et al. 2011). The relative social welfare gains from an expanded social insurance system versus expanded financial education, for example, would depend on the moral hazard costs of insuring against a wider range of shocks and on the relative effectiveness of, e.g., the financial literacy program.

7 Conclusion

Motivated both by a growing body of evidence suggesting the inability of low-income households to cope with unexpected expenses and the observation that the incidence of policing falls largely on disadvantaged communities, this paper studies the effect of fines for speeding violations on financial wellbeing. To estimate causal effects, I link administrative traffic citation records to a panel of credit reports for cited motorists and rely on the staggered timing of traffic stops for identification.

I find that traffic fines averaging \$195 and associated with payments of \$302 over the following six quarters are associated with increases in unpaid bills in collections of about \$34. I interpret this as evidence that, on average, households must borrow \$12 out of other financial obligations such as utility or medical bills to cover \$100 in unplanned expenses. This interpretation is supported by two additional results. First, the effect of fines on unpaid bills appears attributable to fine payment itself, rather than other institutional explanations such as traffic court involvement, driver license points, or driver license suspensions. And second, heterogeneity analysis by proxies for an individual's financial buffer reveal that default which ultimately leads to collections activity is the "last resort" for households, preceded by paying with cash on hand, formal borrowing, and delaying credit line payments as sources for financing unplanned expenses.

In turn, increased default leads to measurable longer-run effects on access to credit. I find that three years out from a traffic stop, credit scores and borrowing limits are 2.6 percentage points and \$330 lower, respectively. I also find evidence that this worsening financial position is associated with a 1.2 percentage point (eight percent) decline in the likelihood of appearing as employed in a database of payroll records from large employers, who cover about a quarter of total employment in Florida and pay 25 percent higher wages than the average job. I interpret this result as suggesting employment instability as well as a slightly diminished ability to obtain or hold "good" jobs.

My finding that fines are associated with increased financial distress, declines in credit reputation, and reduced employment stability suggests that, on average, households cannot easily absorb typical, but unplanned, expense shocks. Based on reasonable assumptions about the distribution of treatment effects, the results suggest that between 30 and 60 percent of individuals in the sample are induced to borrow from other financial obligations when faced with a \$300 emergency expense, consistent with recent survey evidence on the prevalence of financial fragility in the United States.

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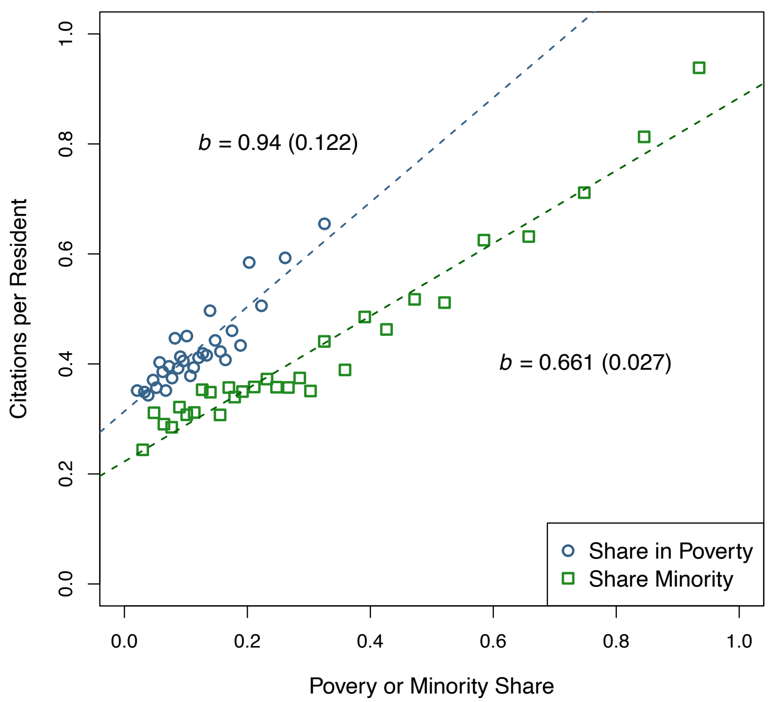
Figure 1: Citation rates by neighborhood characteristics



*N*otes: This figure plots binned means of the neighborhood ticketing rate (total citations 2011–2015 issued to zip code residents divided by the number of residents) against binned means of neighborhood characteristics. $N = 908$ zip codes.

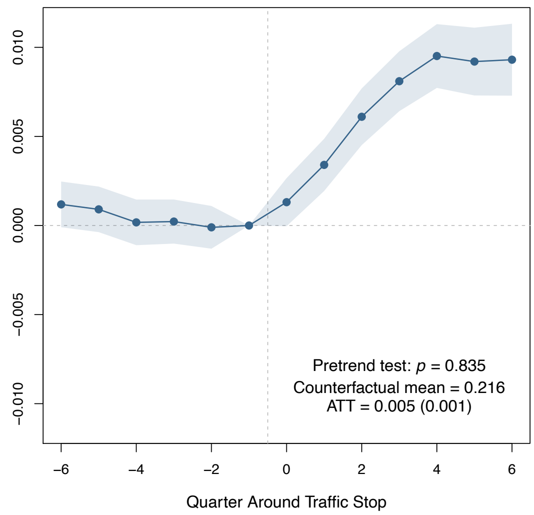
Table 1: Summary statistics at baseline

	(1) Florida	(2) Drivers on File	(3) Event Study	(4) IV
Panel A: Demographics				
Female	0.51	0.44	0.45	0.41
Race = White	0.53	0.4	0.59	0.57
Race = Black	0.17	0.17	0.2	0.2
Race = Hispanic	0.27	0.22	0.22	0.23
Age	40.3	36.81	36.37	35.44
Credit File Age	–	13.02	13.2	12.73
Credit Score	662	604	624	618
Estimated Income	32000	35137	39524	38528
Zip Income	52872	51481	55023	54700
Panel B: Financial Distress				
Collections		2.83	2.24	2.33
Collections Balances		1636	1299	1360
Delinquencies		2.21	1.99	2.06
Derogatories		1.62	1.43	1.48
Panel C: Credit Usage				
Any Revolving		0.66	0.73	0.71
Any Auto Loan		0.36	0.41	0.41
Any Mortgage		0.28	0.33	0.32
Revolving Balances		4023	4950	4729
Revolving Limit		12177	15367	14279
Panel D: Payroll Records				
Any Payroll Earnings		0.12	0.13	0.13
Monthly Earnings		2975	3319	3284
Panel D: Citation Information				
Fine Amount		171.85	195.53	197.62
DL Points		1.74	3.39	3.43
Definitely Paid		0.465	0.589	0.592
Possible Lenience		0.401	0.333	0.304
Possible Suspension		0.134	0.078	0.104
Individuals	14800000	2631641	525646	362854

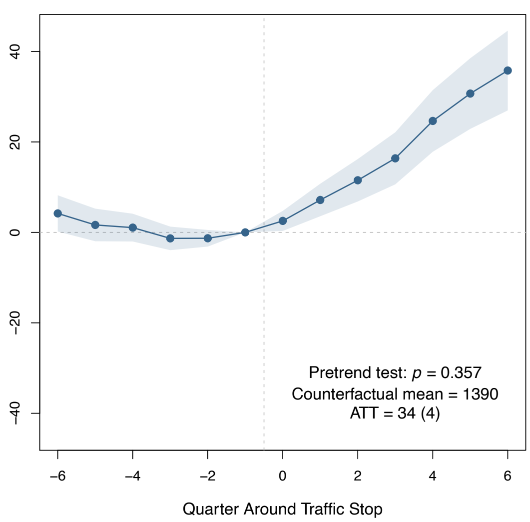
*N*otes: This table reports summary statistics as of 2010Q1 across samples. Column 1 reports statewide means computed from the ACS or provided by the credit bureau. Column 2 reports means for the "initial sample" of drivers who are (i) matched to the credit file, (ii) present on the credit file as of 2010Q1, and (iii) aged 18-59 and have a credit score as of that date. Column 3 reports means for the event study sample and column 4 reports means for the IV sample. See text for additional details on sample construction.

Figure 2: Event study estimates for default outcomes

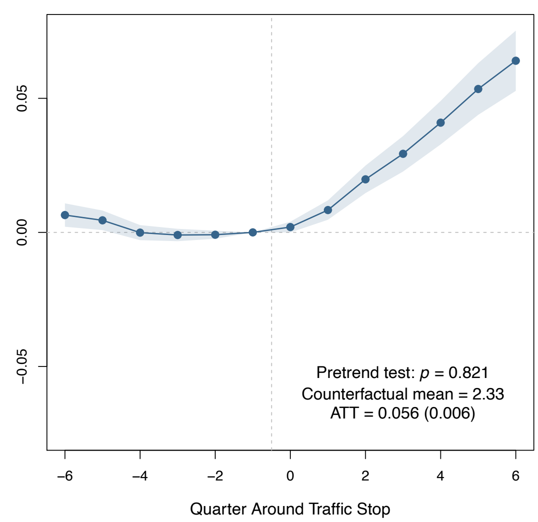
(a) Any New Default



(b) Collections Balances



(c) Credit Line Delinquencies



*N*otes: Each figure reports event study estimates, obtained via the Callaway & Sant'Anna (2021) approach, as well as 95 percent confidence intervals based on design-based standard errors from Roth & Sant'Anna (2022), for the denoted outcome. Sample is the full event study sample ($N = 525,646$). Figures also report the p-value from the Borusyak et al. (2022) pretend test, the estimated counterfactual mean at $\tau = 6$, and the static ATT estimate.

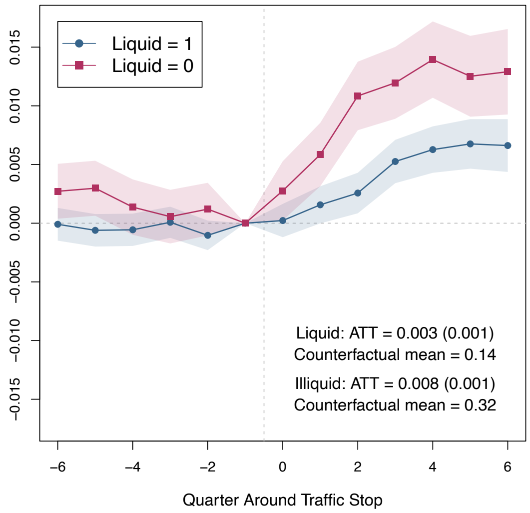
Table 2: Event study estimates for default outcomes

	(1) Any New Default	Collections (2) Number	Collections (3) Balances	Credit Lines (4) Delinquencies	Credit Lines (5) Derogatories
Event Study Estimates					
$\tau = 1$	0.003	0.01	7.17	0.008	0.005
	(0.001)	(0.002)	(1.84)	(0.002)	(0.002)
$\tau = 4$	0.01	0.041	24.64	0.041	0.028
	(0.001)	(0.005)	(3.48)	(0.004)	(0.003)
$\tau = 6$	0.009	0.067	35.79	0.064	0.047
	(0.001)	(0.006)	(4.5)	(0.006)	(0.005)
ATT	0.005	0.06	33.91	0.056	0.045
	(0.001)	(0.006)	(4.04)	(0.006)	(0.004)
Counterfactual Means					
$\tau = 1$	0.22	2.36	1395	2.31	1.57
$\tau = 6$	0.22	2.36	1392	2.32	1.55
Tests for Parallel Trends					
	$p = 0.835$	$p = 0.525$	$p = 0.357$	$p = 0.821$	$p = 0.176$

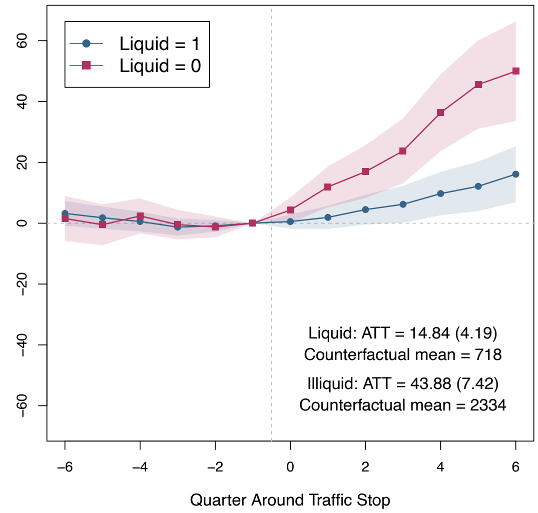
*N*otes: This table reports event study estimates for one, four, and six quarters post traffic stop, as well as the static ATT estimate, all obtained via the Callaway & Sant'Anna (2021) approach. Design-based standard errors from Roth & Sant'Anna (2022) in parentheses. The lower panels report counterfactual means for $\tau = 1$ and $\tau = 6$, estimated using the method described in the text, and results of the pretrends test from Borusyak et al. (2022). The sample is the full event study sample ($N = 525,646$).

Figure 3: Event study estimates by baseline credit access

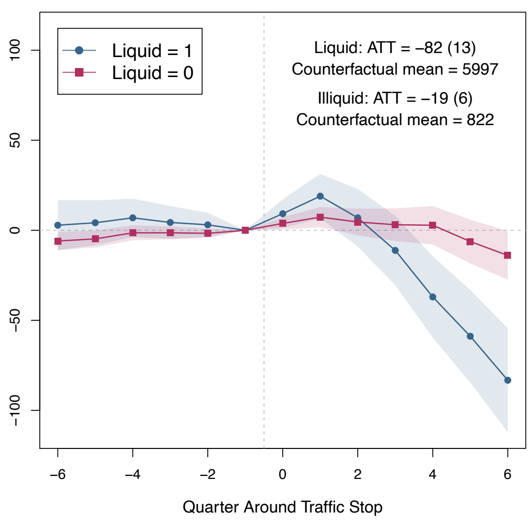
(a) Any New Distress



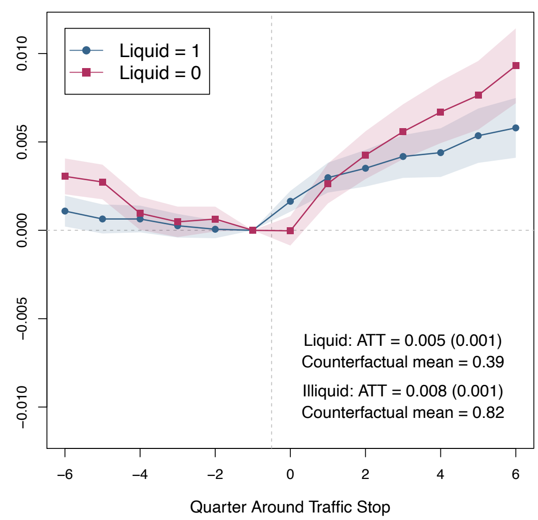
(b) Collections Balances



(c) Revolving Balances



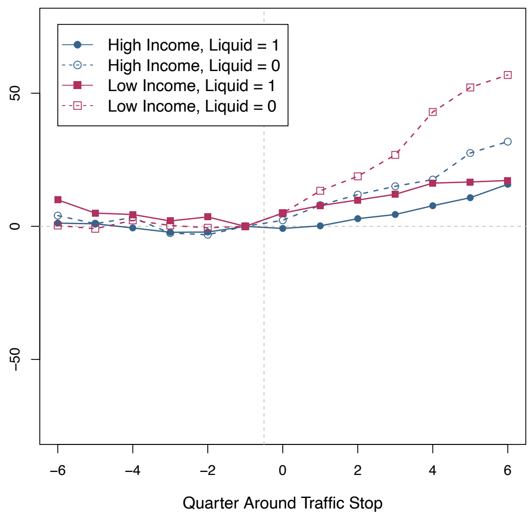
(d) Revolving Utilization



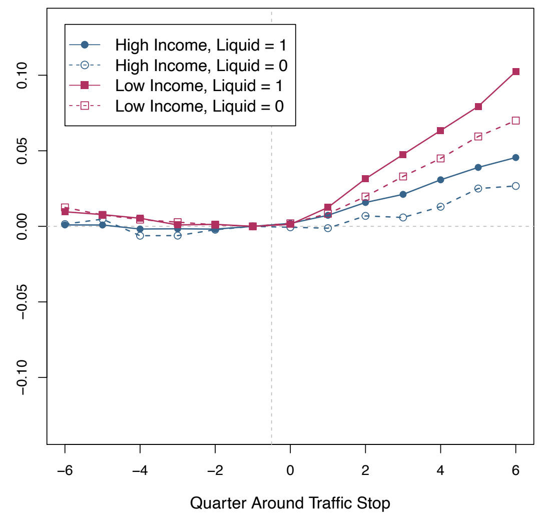
*N*otes: Each figure reports event study estimates, obtained via the Callaway & Sant'Anna (2021) approach, as well as 95 percent confidence intervals based on design-based standard errors from Roth & Sant'Anna (2022), for the denoted outcome. Event studies are estimated separately for subgroups of motorists based on baseline credit card situation. *Liquid*$=1$ is the subset of individuals with at least \$200 in available credit card borrowing at baseline ($N = 301,318$) and *Liquid*$=0$ is the subset of individuals with less than \$200 available at baseline, which includes those with no open credit cards at baseline ($N = 224,328$).

Figure 4: Event study estimates by baseline credit access and estimated income

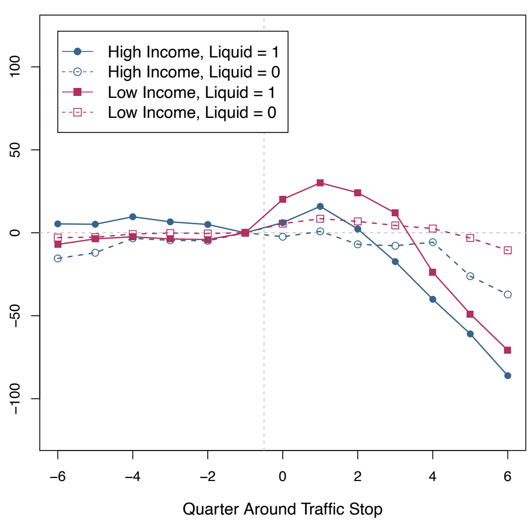
(a) Collections Balances



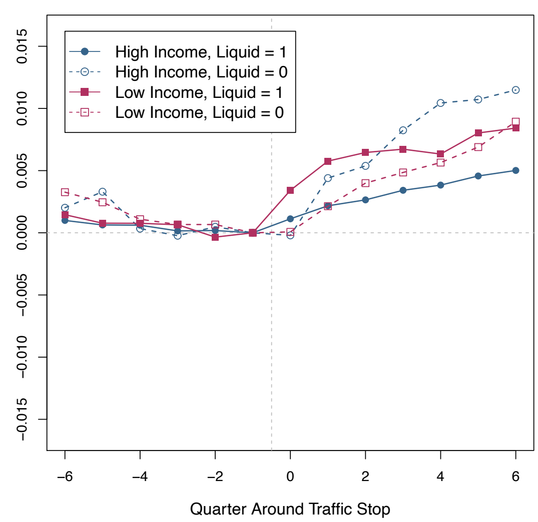
(b) Credit Line Delinquencies



(c) Revolving Balances



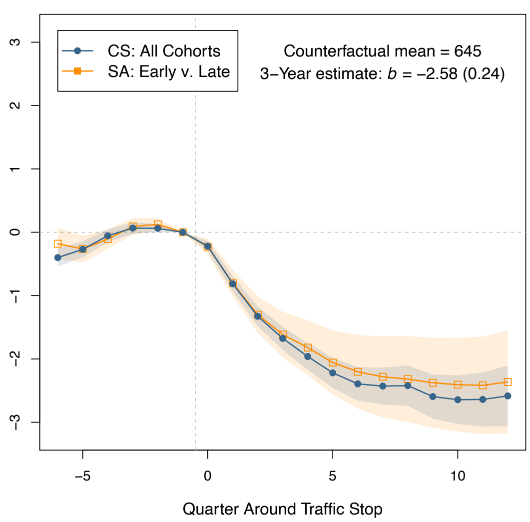
(d) Revolving Utilization



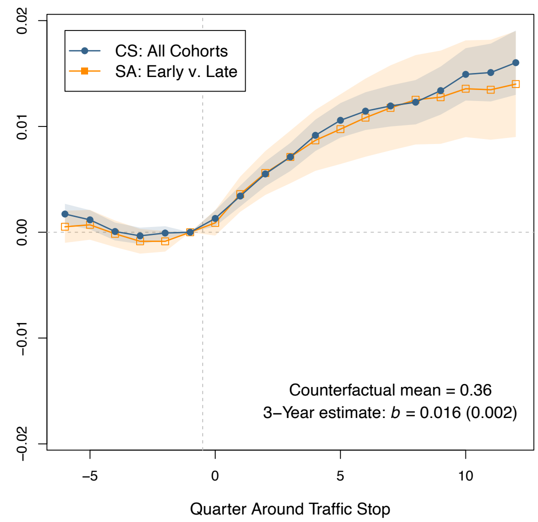
*N*otes: Each figure reports event study estimates, obtained via the Callaway & Sant'Anna (2021) approach for the denoted outcome. Event studies are estimated separately for subgroups of motorists based on baseline credit card situation and estimated income. *High income* is defined as being above the median baseline estimated income and *liquid* is defined as in figure 3. Sample sizes are $N = 232,230$ (high income, liquid$=1$), $N = 56,046$ (high income, liquid$=0$), $N = 69,088$ (low income, liquid$=1$), $N = 224,328$ (low income, liquid$=0$).

Figure 5: Event study estimates for long-run outcomes

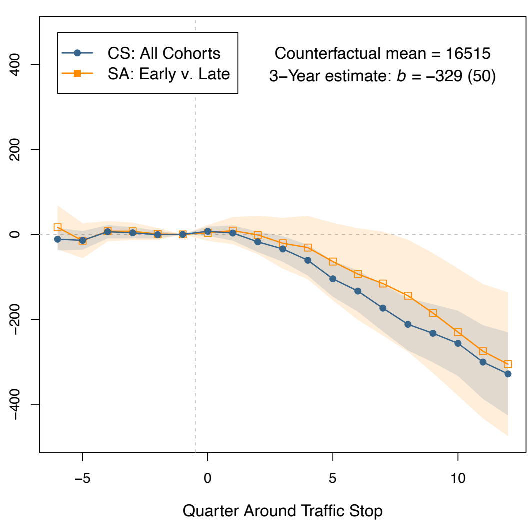
(a) Credit Score



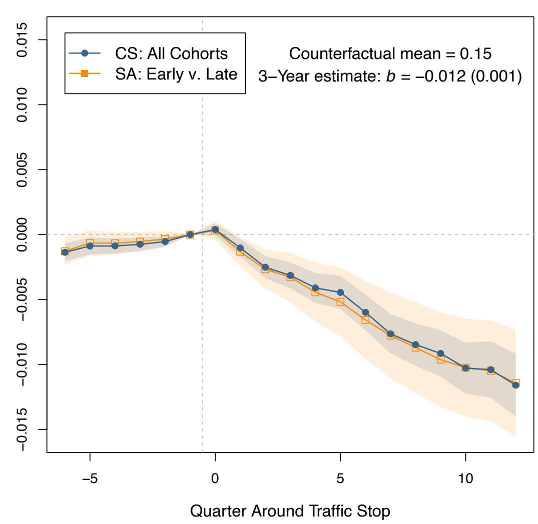
(b) Subprime



(c) Borrowing Limit



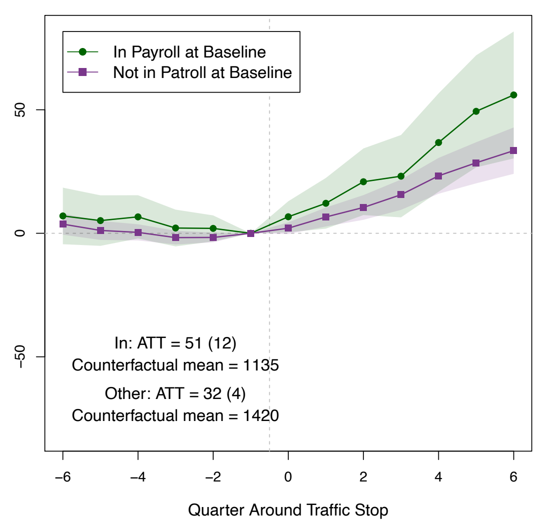
(d) Payroll Employment



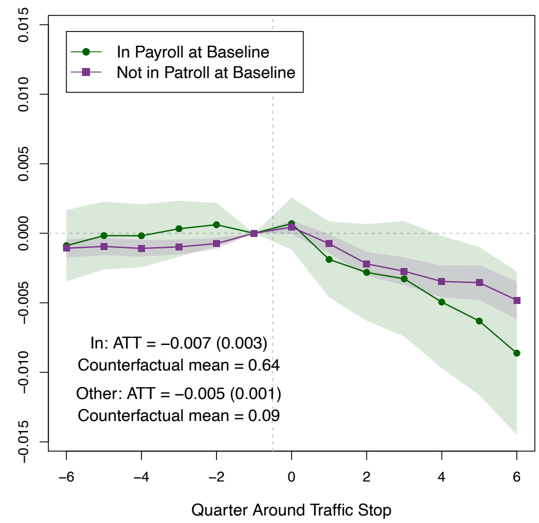
*N*otes: This figure reports event study estimates obtained from the primary Callaway & Sant'Anna (2021) specification (blue circles) as well as from a specification based on Sun & Abraham (2021) which compares only those cited in 2011–2012 ("early" cohorts) to those cited in 2015Q4 ("late" cohort). Subprime = $\mathbf{1}$[credit score $<$ 600]. Borrowing limit is the sum of the observed limits across all revolving accounts and thus equals zero for individuals with no revolving lines. Imputed borrowing limit is imputed based on the credit score, as described in appendix B.

Figure 6: Event study estimates by baseline payroll status

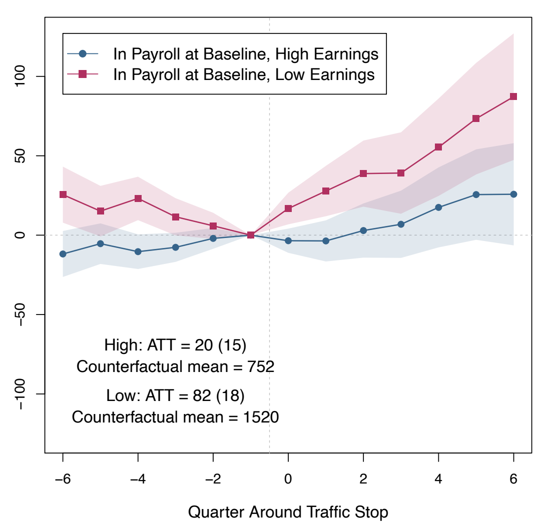
(a) Collections Balances



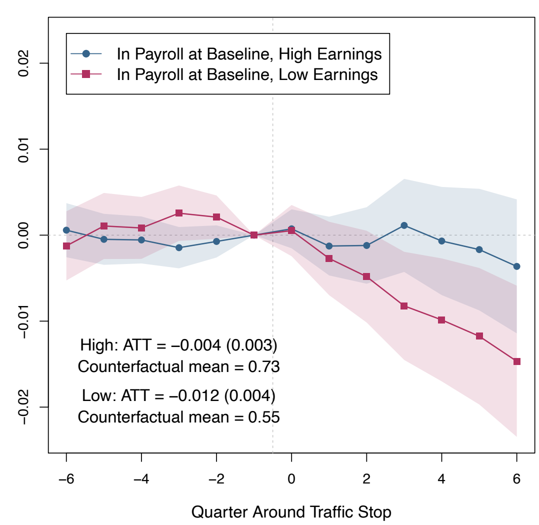
(b) Payroll Employment



(c) Collections Balances



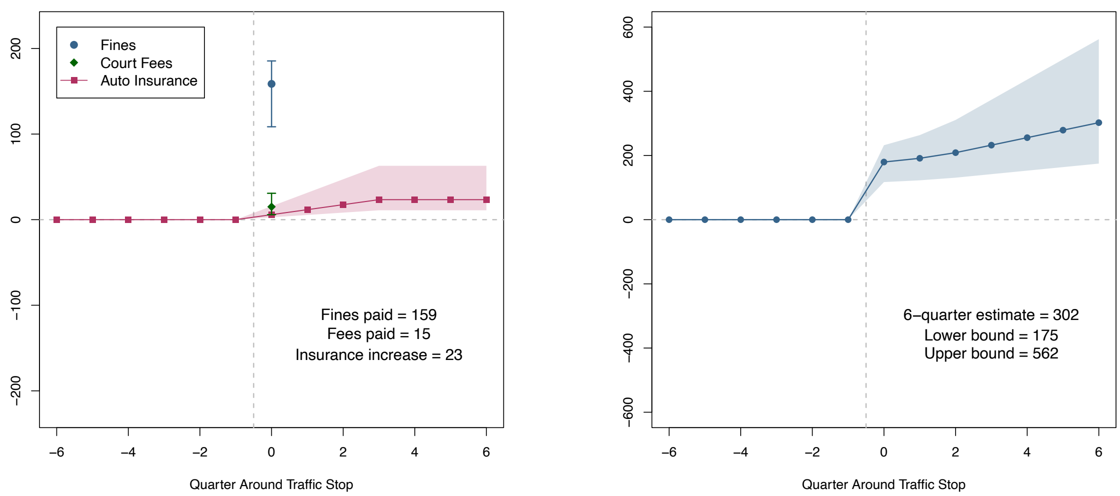
(d) Payroll Employment



Notes: Each figure reports event study estimates, obtained via the Callaway & Sant'Anna (2021) approach, as well as 95 percent confidence intervals based on design-based standard errors from Roth & Sant'Anna (2022), for collections balances or payroll employment, estimated separately by baseline payroll employment status. In panels (a) and (b), the sample is split by whether a motorist was "employed" at baseline, defined as being in the payroll records for all four quarters of 2010 (N employed = 55,140, N other = 470,506). In panels (c) and (d), the sample is the baseline "employed" sample and is split at the median of earnings in 2010, which is $34,198 ($N$ low $= 27,570$; N high $= 27,570$).

Figure 7: Estimated total costs of citations

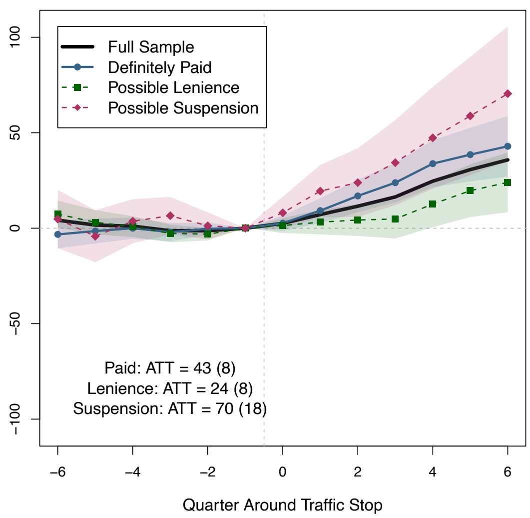
(a) Quarterly costs (b) Cumulative total costs



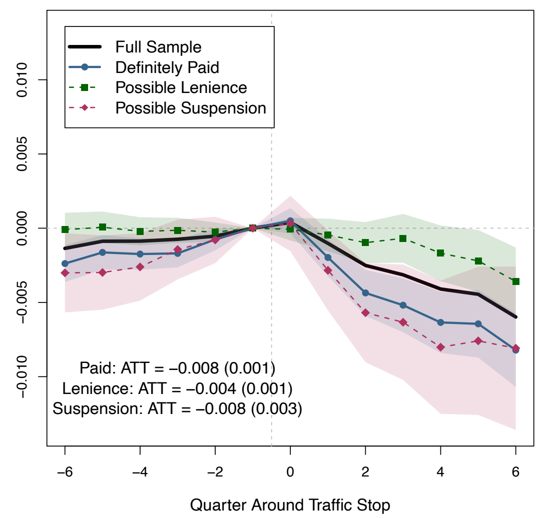
*N*otes: This figure reports estimated average total costs of citations in the event study sample ($N = 525,646$), taking into account statutory sanctions and the post-citation choices of motorists based on the traffic court dispositions data. Panel (a) reports the per-quarter cost estimates, with estimated fine payments shown in blue circles, estimated court fees paid shown in green diamonds, and car insurance *increases* shown in red squares. Confidence bands reflect the range of estimates based on various assumptions, as discussed in the text and in appendix B-2

Figure 8: Event study estimates by traffic court disposition

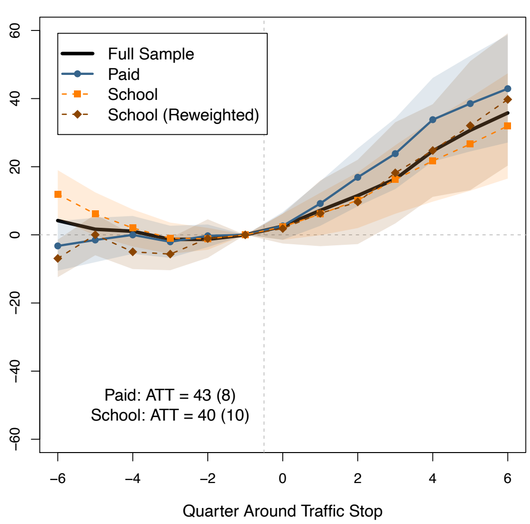
(a) Collections Balances



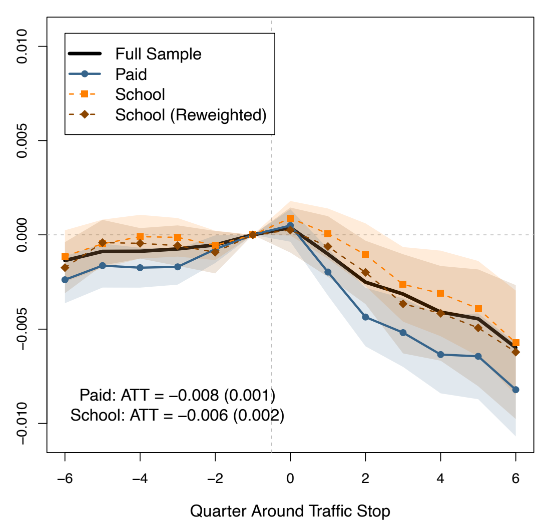
(b) Payroll Employment



(c) Collections Balances



(d) Payroll Employment

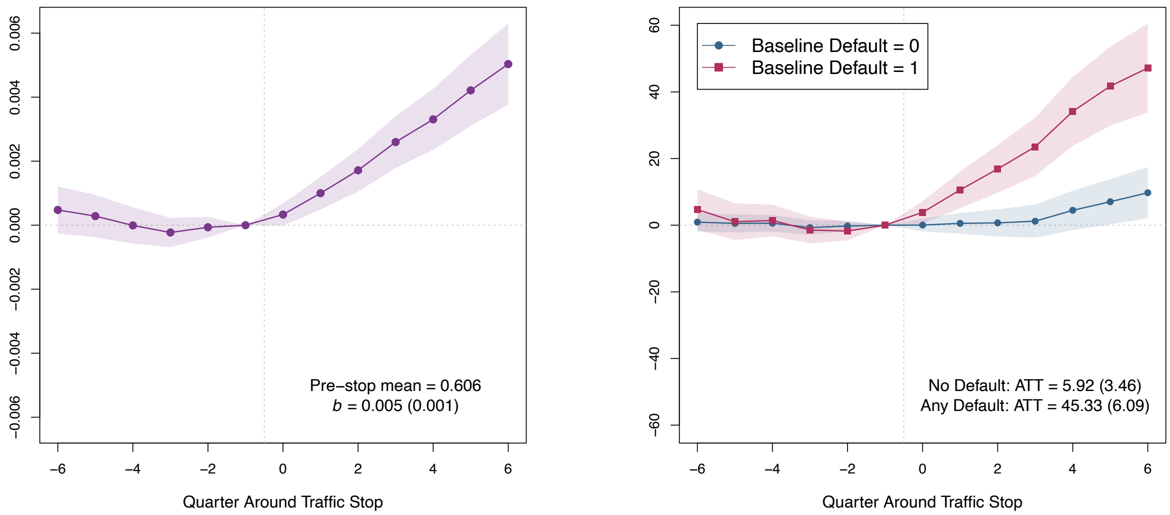


Notes: Each figure reports event study estimates, obtained via the Callaway & Sant'Anna (2021) approach, as well as 95 percent confidence intervals based on design-based standard errors from Roth & Sant'Anna (2022), for collections balances or payroll employment, estimated separately by traffic court disposition. In all panels, solid black line in the estimate for the dull sample ($N = 525,646$) and solid blue line with circles is the subgroup whose dispositions indicate fine payment ($N = 174,766$; note that this excludes the traffic school group and is therefore a subset of the "definitely paid" sample). In panels (a) and (b), green squares report estimates for those who *may* have received punishment reductions ($N = 175,051$) and red diamonds report estimates for those who *may* have received a license suspension for nonpayment ($N = 40,997$). In panels (c) and (d), orange squares report estimates for those who elected traffic school ($N = 134,832$) and brown diamonds report estimates for the same subsample after reweighting to match the distribution of baseline characteristics (age, gender, race, and quartile bins of credit score and estimated income) in the benchmark payer subsample. See main text and appendix B for additional details.

Figure 9: Event study estimates on extensive and intensive margins

(a) Any Default to Date

(b) Collections by Baseline Default

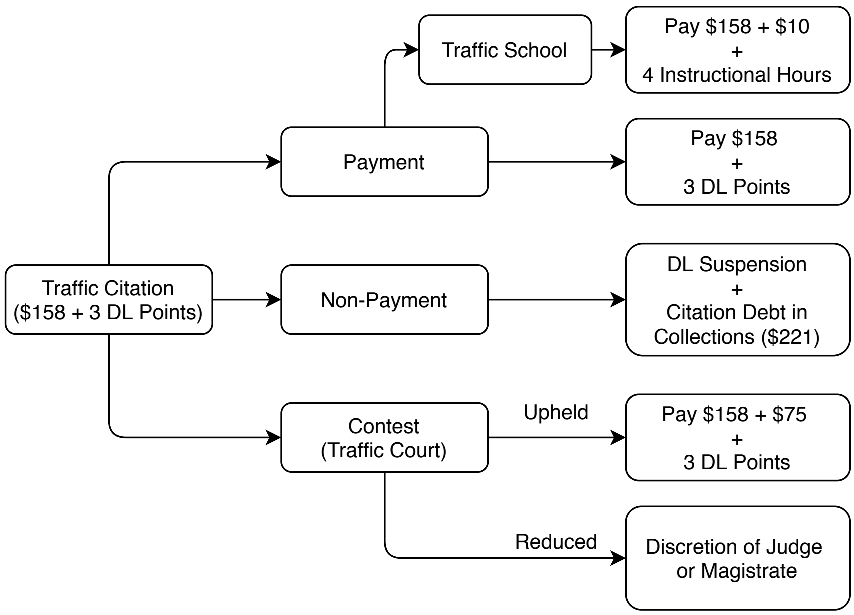


*N*otes: Each figure reports event study estimates, obtained via the Callaway & Sant'Anna (2021) approach, as well as 95 percent confidence intervals based on design-based standard errors from Roth & Sant'Anna (2022), for the denoted outcome. In panel (a), the outcome is an indicator for whether an individual has accumulated any new default since the start of the sample for the full event study sample ($N = 525,646$); once this variable has switched to one, it remains one forever. Figure reports the mean at $t = -1$ and the event study estimate at $\tau = 6$. In panel (b), the outcome is collections balances and the sample is split by whether an individual has any default flag (collection, delinquency, or derogatory) on their credit report at baseline (no default: $N = 201,070$; any default: $N = 324,576$).

ONLINE APPENDIX

A Appendix Figures and Tables

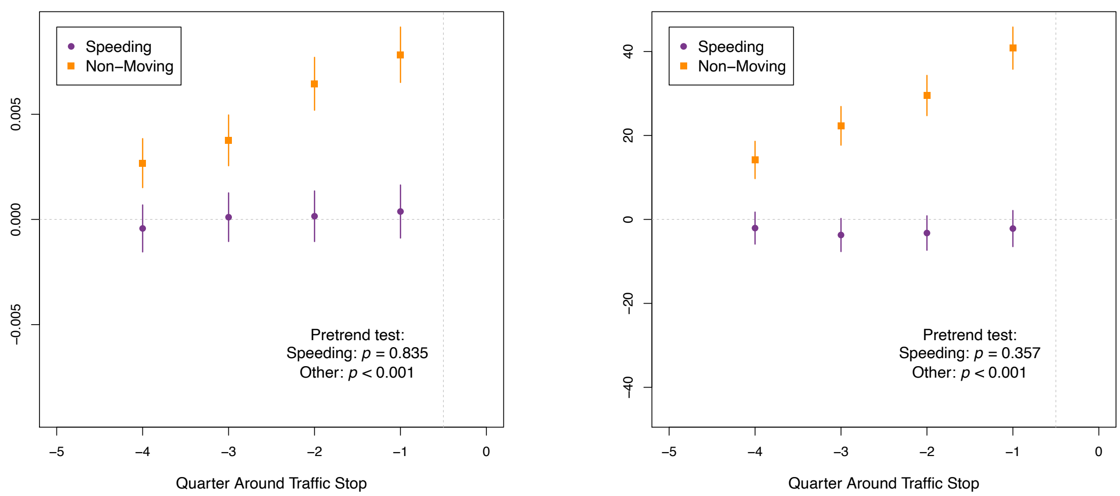
Figure A-1: Potential outcomes associated with standard moving violation



Notes: This figure provides a flow chart summarizing driver choices and the associated outcome(s) for each choice. The $10 surcharge for traffic school attendees represents the typical net surcharge, $25 for the course minus a $15 fine reduction. The citations debt in collections ($221) for non-payers assumes a 40 percent collections fee, the maximum allowed by law. Note that such collections activity, to the best of my knowledge, will not appear on the credit reports used in the empirical analysis. The $75 surcharge for contesters is the standard court fee.

Figure A-2: Pretrends by violation type

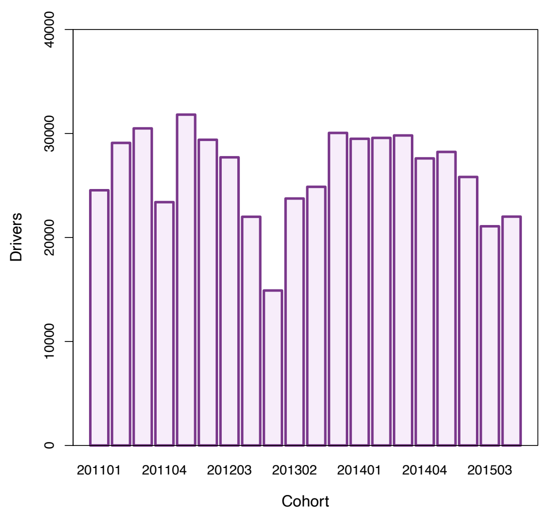
(a) Any New Distress (b) Collections Balances



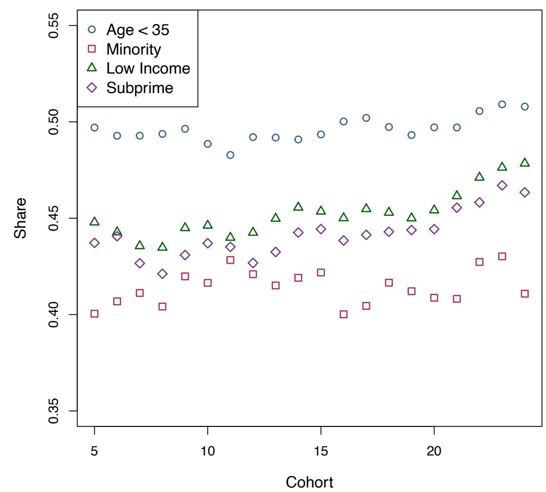
Notes: This figure reports results from the parallel trends test of Borusyak et al. (2022) for the primary event study sample of speeders ($N = 525,646$) and a sample, constructed in an identical way, of individuals who commit non-moving traffic violations, such as paperwork and equipment infractions ($N = 625,097$).

Figure A-3: Event study cohorts

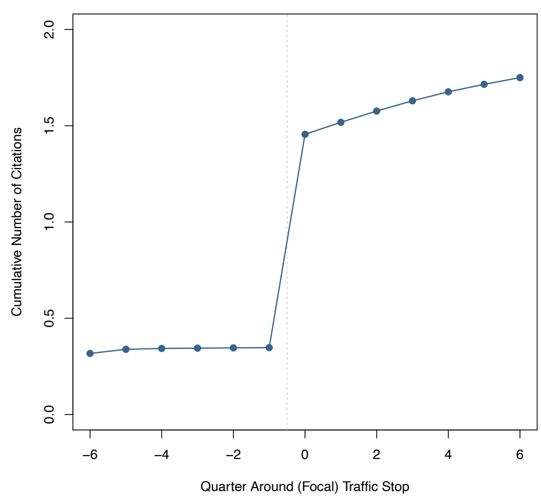
(a) Distribution of Timing



(b) Characteristics by Timing



(c) Cumulative Stops in Event Time

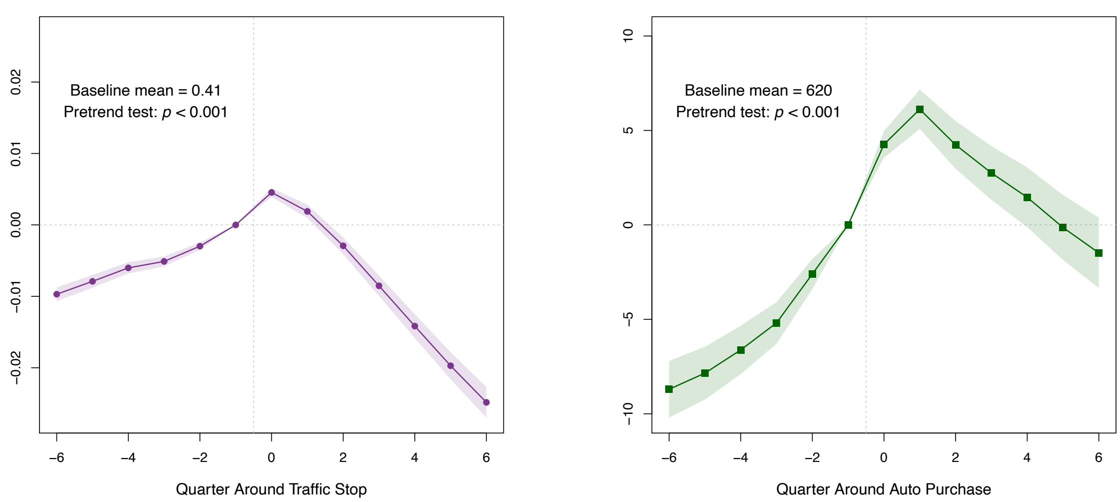


*N*otes: Panel (a) plots the distribution of treatment timing ("cohort") for event study sample ($N = 525,646$). Panel (b) illustrates characteristics of each cohort. Panel (c) shows how the cumulative number of traffic stops for each driver varies in event time.

Figure A-4: Trends in car ownership around traffic stops

(a) Any Car Loan Around Traffic Stop

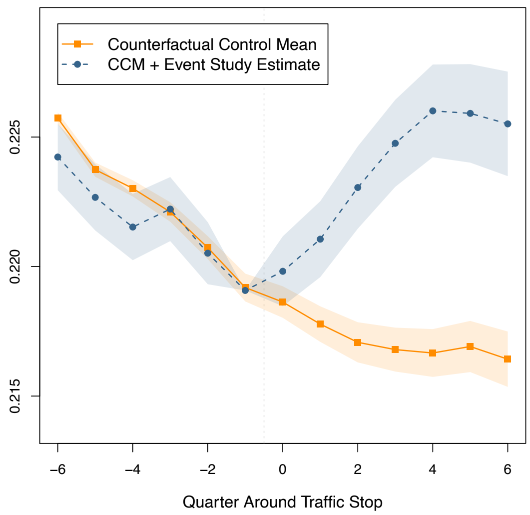
(b) Credit Score Around Car Purchase



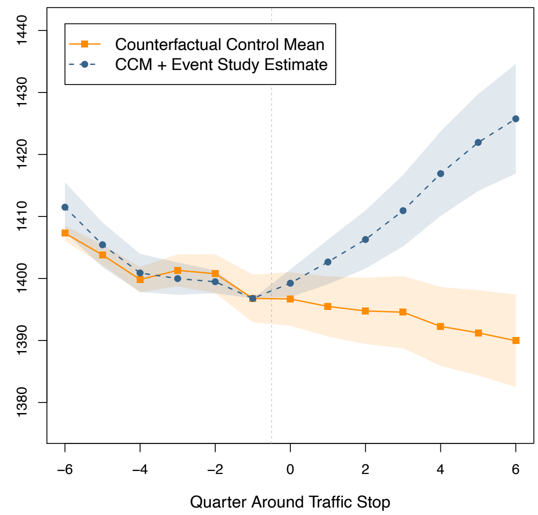
*N*otes: Panel (a) reports event study estimates using the event study sample ($N = 525,646$) where the outcome of interest is the presence of an open auto loan on the credit file (baseline $\mu = 0.412$; at the time of traffic stop, $\mu = 0.475$). Panel (b) reports event study estimates around the time of a car purchase where the outcome of interest is the credit score, using only the final cohort of the event study sample ($N = 22,006$) and examining only auto purchases prior to that date.

Figure A-5: Event study estimates relative to counterfactual control means

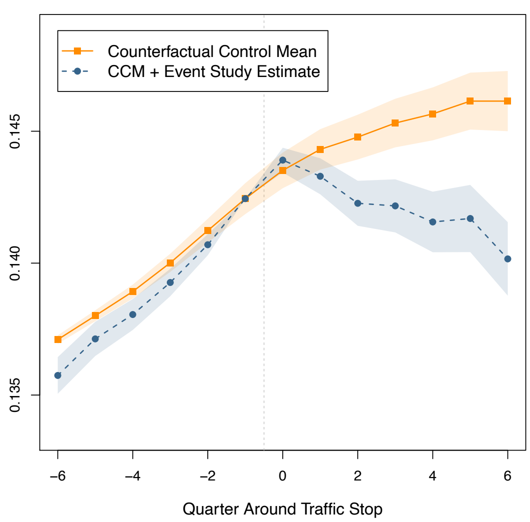
(a) Any New Distress



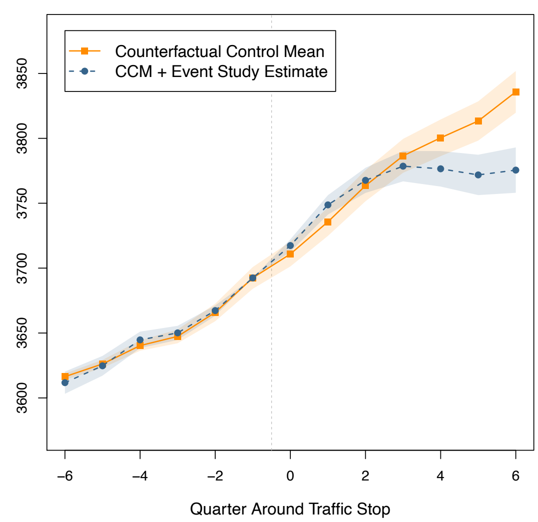
(b) Collections Balances



(c) Payroll Employment



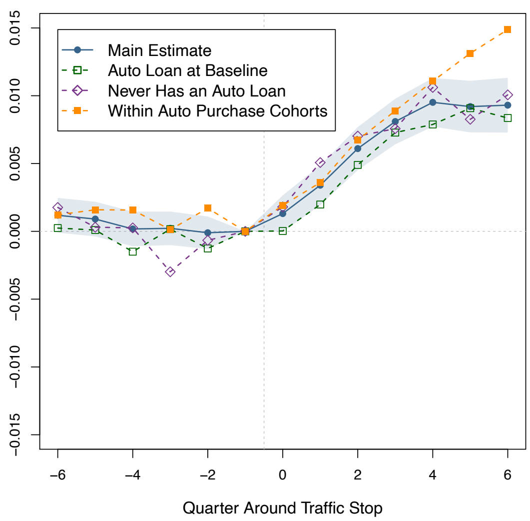
(d) Revolving Balances



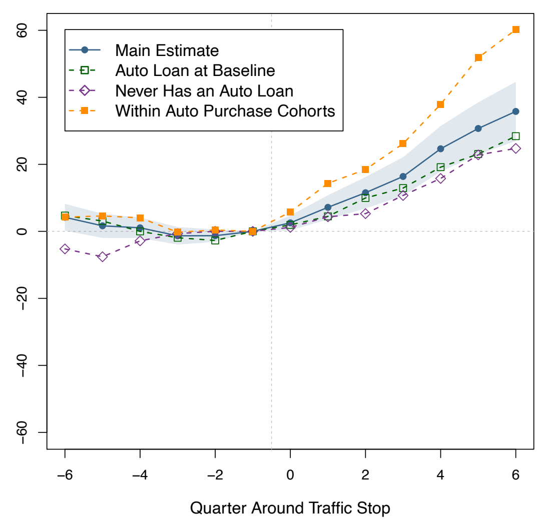
*N*otes: This figure reports the time path of estimated counterfactual means (orange squares) and the estimated counterfactual means plus the event-study estimates (blue circles) using the full event study sample ($N = 525,646$). Counterfactual means are estimated using the method described in the text. 95 percent confidence bands for the estimated counterfactual means are obtained via a Bayesian bootstrap clustered at the motorist-level.

Figure A-6: Event study estimates conditional on car purchase timing

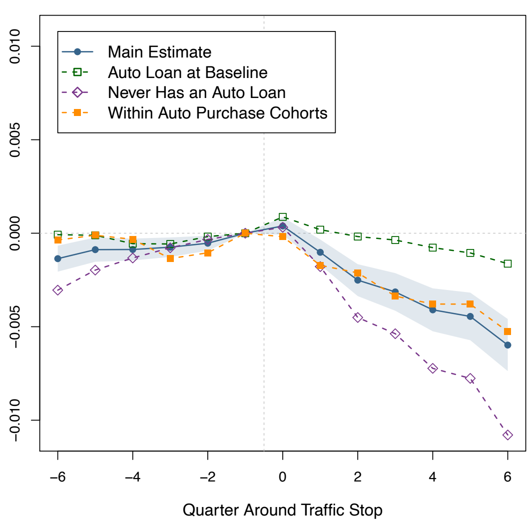
(a) Any New Distress



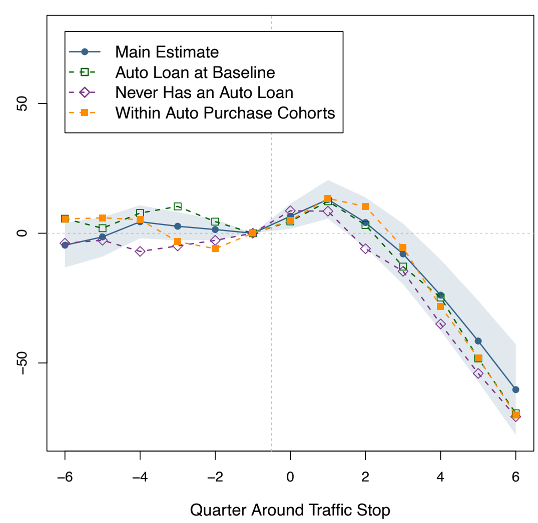
(b) Collections Balances



(c) Payroll Employment

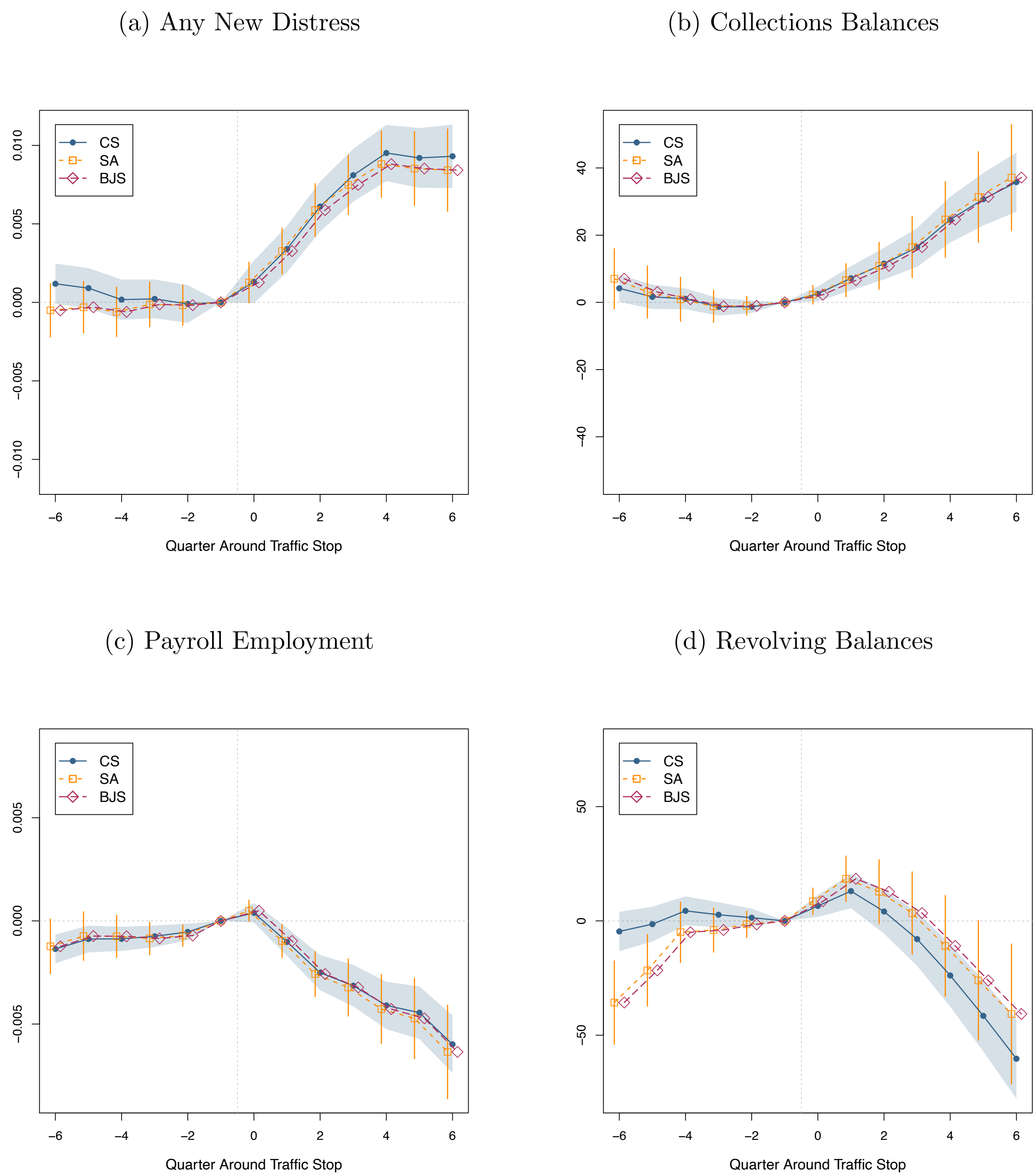


(d) Revolving Balances



Notes: This figure reports event study estimated which condition on the timing of an individual's auto first purchase. In each panel, the solid blue circles report the main estimate (same as reported in the main text) for the full event study sample ($N = 525,646$). Hollow green squares and hollow purple diamonds report estimates using only the subsets of individuals who already have an auto loan at baseline ($N = 216,625$) and who never have auto loans during the sample period ($N = 110,540$). Solid orange squares report estimates *within* auto timing groups for those who first purchase cars in or after 2010Q2 and purchase cars prior to their traffic stop ($N = 111,059$), obtained by estimating event studies separately for each auto purchase cohort and then aggregating up, weighting by sample shares.

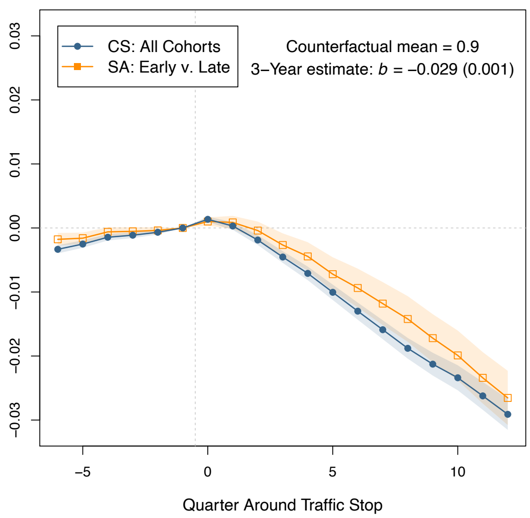
Figure A-7: Event study estimates via alternative methods



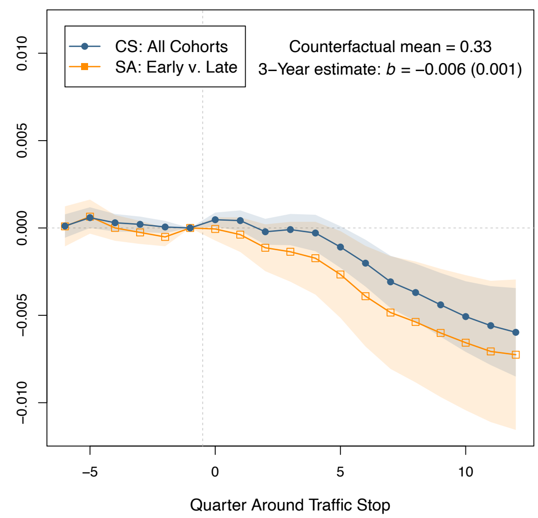
*N*otes: Each figure plots event study estimates obtained via the approaches of Callaway & Sant'Anna (2021) (same as baseline; blue circles), Sun & Abraham (2021) (orange squares), and Borusyak et al. (2022) (purple diamonds).

Figure A-8: Event study estimates for other long-run outcomes

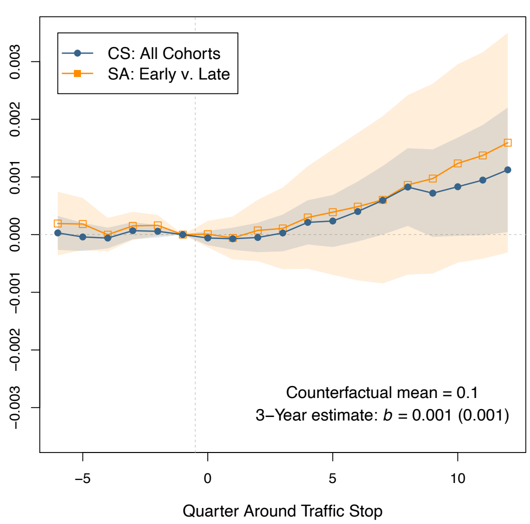
(a) Any Open Credit Account



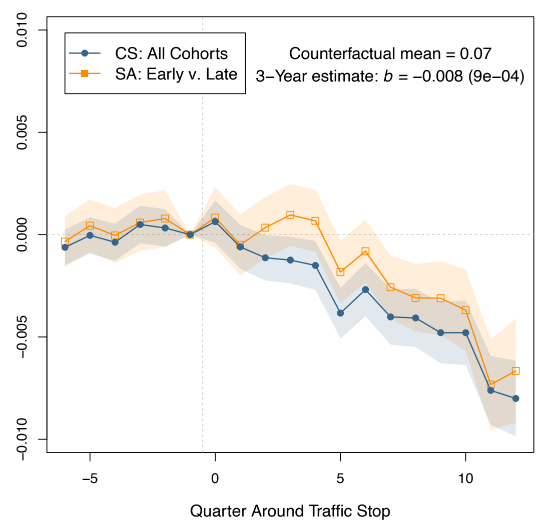
(b) Any Mortgage



(c) Any Bankruptcy to Date



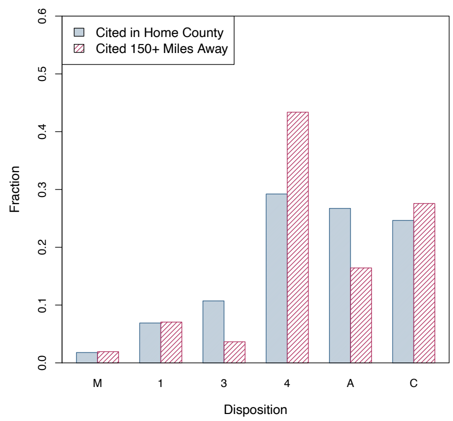
(d) New Address



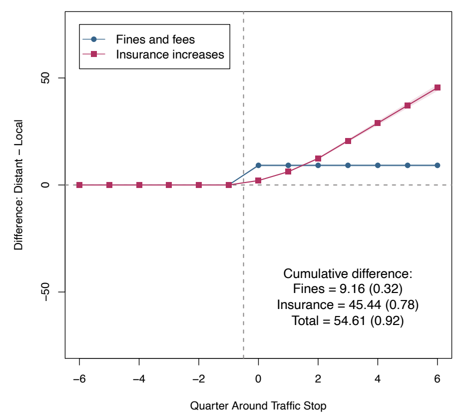
*N*otes: This figure reports event study estimates obtained from the primary Callaway & Sant'Anna (2021) specification (blue circles) as well as from a specification based on Sun & Abraham (2021) which compares only those cited in 2011–2012 ("early" cohorts) to those cited in 2015Q4 ("late" cohort). Imputed borrowing limit is imputed based on the credit score, as described in appendix B. New address is an indicator for whether the address on the credit file was updated in a given quarter; information on new addresses is redacted in the credit file.

Figure A-9: Total costs and event study estimates by citation location

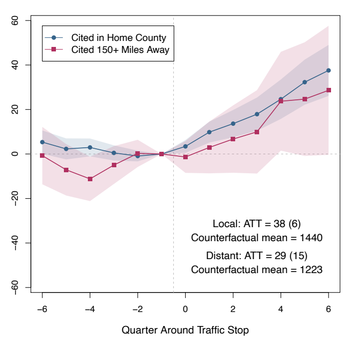
(a) Verdicts by location



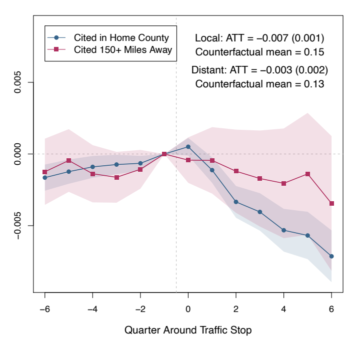
(b) Differences in total costs



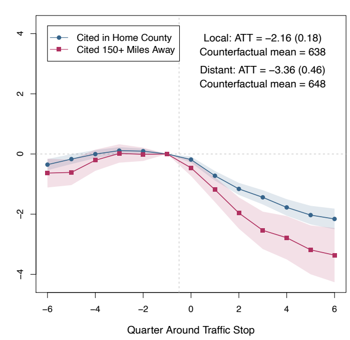
(c) Collections Balances



(d) Payroll Employment



(e) Credit Score



*N*otes: Panel (a) illustrates the distribution of traffic court disposition verdicts for citations issued to motorists who live in the county of their citation (solid blue bars; $N = 310,317$) and for motorists who live at least 150 miles away from the county of their citation (striped red bars; $N = 46,058$), where the estimated distances are based on the centroids of the the motorist's county of residence and county where the citation occurred. Panel (b) illustrates the *differences* in estimated total costs of citations over time (total costs for those who live 150+ miles away − total costs for those cited in their county of residence). Panels (c)-(d) plot event study estimates, estimated separately for these two groups of motorists.

B Data

B-1 Additional information on payroll records

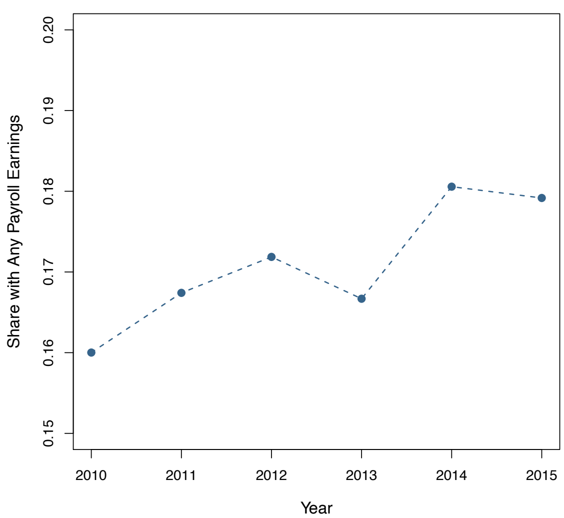
Table B-1: Comparison with ACS, 2010

	(1) Share Employed	(2) Annual Earnings
Payroll Data	0.16	46453
ACS: Comparable	0.682	40430
ACS: Reweighted	0.686	37122

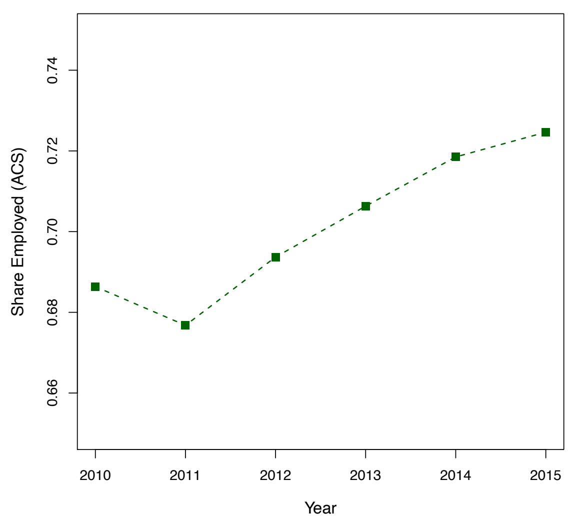
*N*otes: This table compares employment rates and earnings from the payroll data and from ACS data in 2010, using the event study sample. "Employment" in the payroll data is defined as having any payroll earnings at some point in 2010. Annual earnings are averages for only those with positive earnings in each dataset. The second row presents means from a comparable subsample (Florida residents aged 18-59) of the 1% ACS microdata sample (Ruggles, 2023). The third row reweights the demographics (age, gender, race) of this comparable subsample to match the characteristics of the event study sample.

Figure B-1: Comparison with ACS employment rates over time

(a) Share with Payroll Earnings



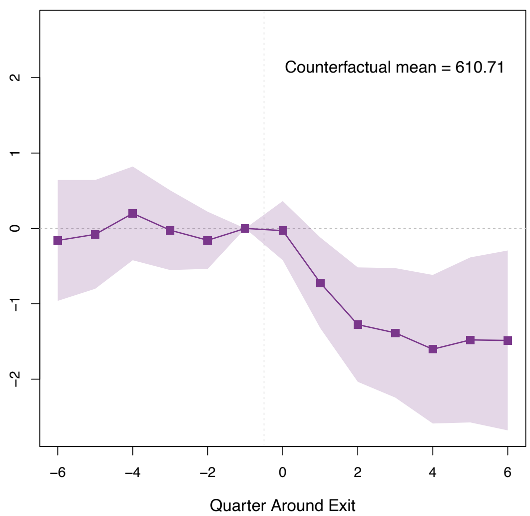
(b) ACS Employment Rates



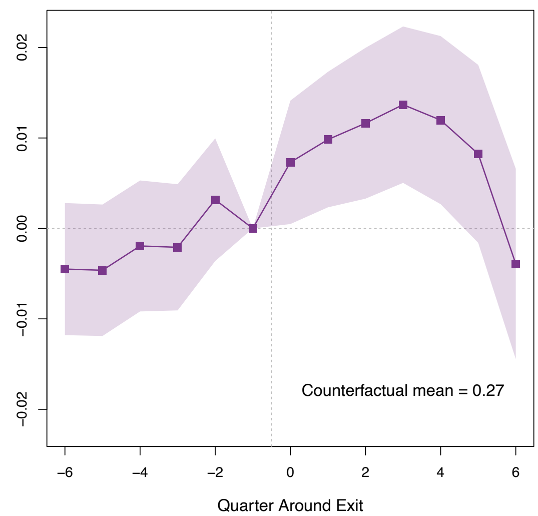
*N*otes: Panel (a) plots the share of the event study sample with any payroll earnings at some point during each year and panel (b) plots ACS employment rates, which are computed using the 1% microdata samples (Ruggles, 2023), reweighting the ACS sample based on age, gender, and race to match the characteristics of the event study sample.

Figure B-2: Event study estimates, payroll data exits

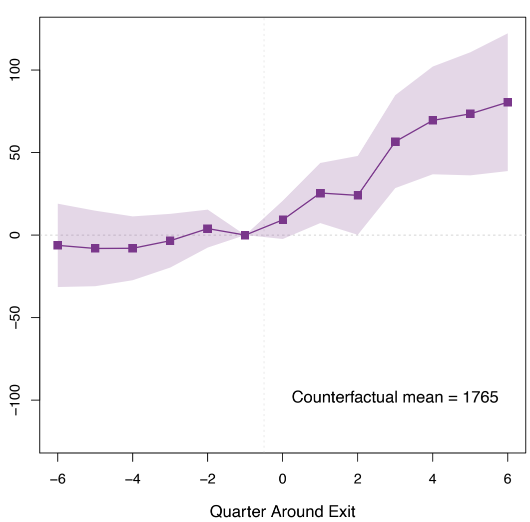
(a) Credit Score



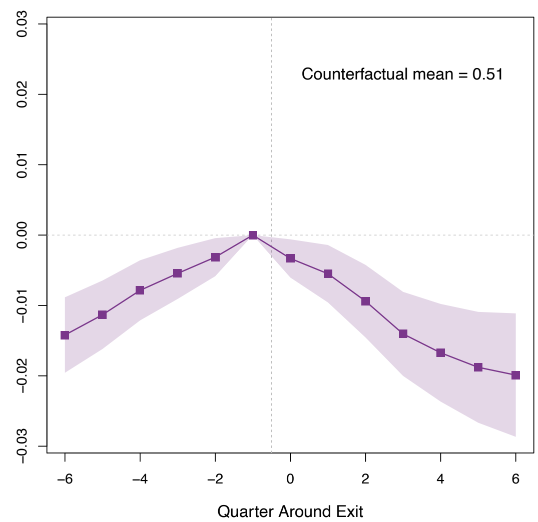
(b) Any New Default



(c) Collections Balances

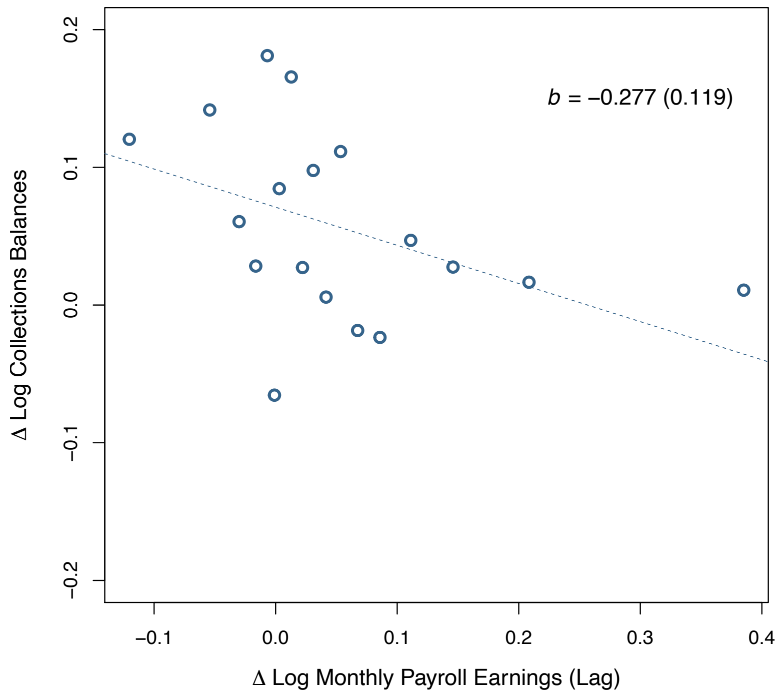


(d) Any Durable Loan



*N*otes: This figure presents event study estimates of the effect of "separating" from a job in the payroll records. To construct the sample, I first take the subset of all drivers on file that receive their first citation in 2015 and use only data from pre-2015. I define the event as transitioning from having positive payroll earnings to having zero payroll earnings after at least four consecutive quarters with positive payroll earnings; there are 19,998 individuals with an event. As a control group, I use individuals who have a spell of at least four consecutive quarters with positive payroll earnings that ends sometime after 2014 ($N = 66,640$).

Figure B-3: Collections-earnings elasticity



Notes: This figure uses the subset of the event study sample that is continuously employed in the payroll records over the first two years (2010Q1 through 2011Q4; $N = 18,512$) and plots the relationship between the log change in collections balances from 2010Q4 to 2011Q4 against the log change in monthly earnings from 2010Q1 to 2011Q1.

B-2 Traffic court dispositions and estimating the first stage

Table B-2 below shows the distribution of dispositions in the event study sample:

Table B-2: Distribution of traffic court dispositions

Disposition	N	Fraction
Missing	9,653	0.018
1 = *guilty*	31,344	0.0596
3 = *dismissed*	45,772	0.087
4 = *paid fine*	174,766	0.332
A = *adjudication withheld*	129,279	0.246
C = *traffic school*	134,832	0.257
Total	525,646	

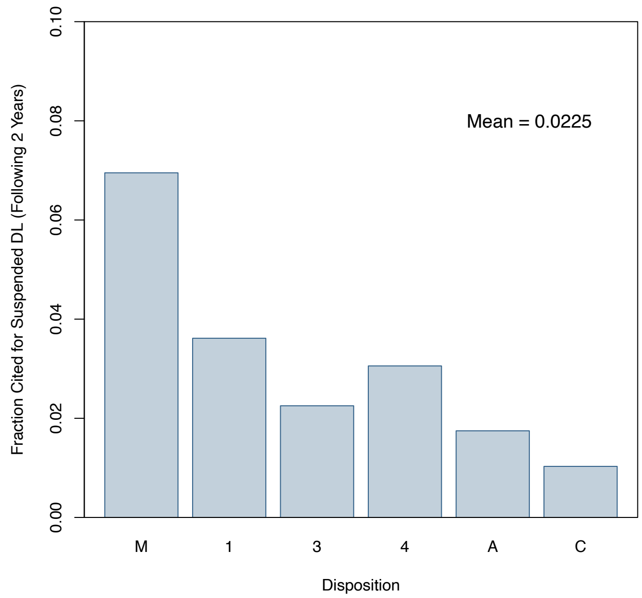
and table F-3 below provides summary statistics by disposition group. As highlighted in conversations with Beth Allman at the Florida Clerk of Courts, several of these disposition verdicts are remarkably hard to interpret in practice. The two verdicts with the most straightforward interpretation are 4 and C, which both indicate a paid fine (traffic school election requires fine payment). Hence, based on the disposition information, a very conservative lower bound on the fraction of citations where the fine was paid is 59 percent. Those with paid verdicts (33 percent) accrue the drive license statutory points associated with their offense, while those who elect school (25.7 percent) accrue no points.

The remaining dispositions all have associated complications. A disposition = 3 almost surely indicates that that the individual attended a traffic court hearing and received some leniency from the judge or hearing officer. However, this verdict could mean that all sanctions were dismissed, that only license points were dismissed, or that the charge was reduced to a lesser offense with a lower fine, which was then paid. Also, this disposition does not necessarily mean that the requisite $75 court fee was waived. The exact same issues are present when the disposition = A. Officials at the Florida Clerk of Courts have indicated that, in their estimation, a sizable share of citations with verdicts = $3/A$ were likely associated with paid fines but waived license points, or with paid fines and accrual of points associated with a lesser charge than the original citation. And importantly, attending traffic court could certainly be disruptive in its own right.

A disposition verdict = 1 could indicate that an individual attended court but "lost" and ultimately paid a fine plus a court fee, or that the individual never paid their fine and faced a license suspension. A missing disposition could mean non-payment and no interaction with the court system or could reflect an issue with the underlying data. Figure B-4 provides suggestive evidence that a missing verdict (1.8 percent of the sample) is associated with nonpayment by illustrating that the share of motorists who are cited for driving with a suspended license at some point in the future is more than twice as large in the group of citations with a missing verdict than in any other group of citations. I assume throughout that those with guilty and missing verdicts accrue the statutory license points associated with their offense.

Motivated by the background information provided by the Florida Clerks, in the analyses splitting the sample based on disposition records, I mainly group citations into three

Figure B-4: Future DL suspension offenses by disposition verdict



*N*otes: This figure plots the share of citations in the event study sample ($N = 525,646$) where the motorist is cited for a driving with a suspended driver license in the following two years, by the disposition verdict associated with the citation in the event study sample.

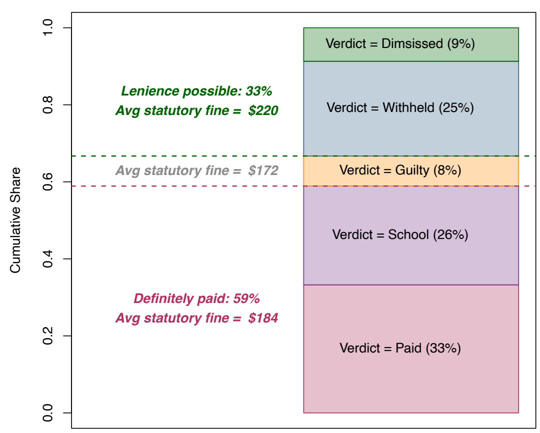
groups: (1) paid citations (disposition $= 4/C$), I refer to this group as "definitely paid"; (ii) citations where penalties were likely reduced (disposition $= 3/A$), I refer to this group as the "possible lenience" group; (3) citations where penalties were likely increased (disposition = 1 or missing), I refer to this group as the "possible suspension" group. I also compare effects for those with dispositions $= 4$ and $= C$ as a way to assess the relative importance of license points in explaining estimated effects, since both groups pay their fines but those with 4's will accrue license points while those C will not.

B-2.1 Estimating average fine payments

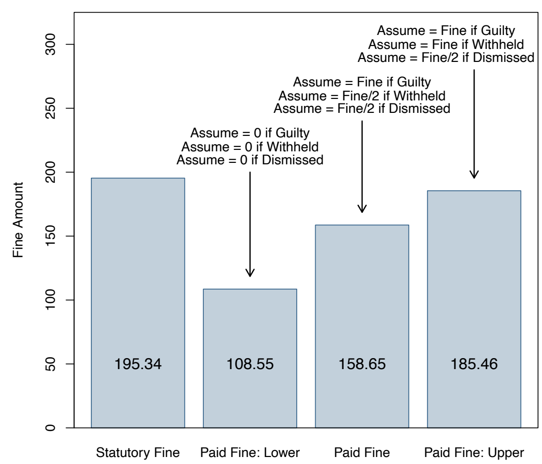
Per the above discussion of disposition records, a simple lower bound estimate on average fine payments would assume that 59 percent of the sample pays their fines in full, while the remainder of the sample pays no fines. This gives a lower bound estimate on the average fine payment $= 0.59 \times E(\text{fine}|\text{verdict} \in \{4, C\}) = \$108$. However, based on the information provided by the Florida Clerk of Courts, this estimate is likely too low, as guilty verdicts were likely associated with fine payment, while some significant share of those with verdicts $= 3/A$ paid fines which may have been reduced. My preferred estimate of average fine payment assumes that those with guilty (and missing) verdicts pay a full fine, while those with dismissed and withheld verdicts pay 1/2 of their full fine, giving an estimate of \$159. As an upper bound, I assume that everyone with a withheld verdict pays a full fine but those with dismissed verdicts pay a half fine, yielding an upper bound estimate of \$185. Note that assuming full fine payment for those with missing dispositions may be incorrect (as figure B-4 suggests that this verdict may correspond to non-payment) but has minimal effect on the overall estimates, as only 1.8 percent of the sample has a missing disposition.

Figure B-5: Estimating paid fines

(a) Sample shares by type



(b) Range of estimates



*N*otes: Panel (a) illustrates the distribution of dispositions, characterized by fine payment status. For ease of exposition, I pool those with missing verdicts (1.84 percent of the sample) together with those with guilty verdicts (5.96 percent of the sample). Panel (b) depicts the sample average estimated fine payment under various assumptions. Estimates may differ slightly from those reported in the text due to rounding.

B-2.2 Estimating average court fees

Individuals choosing to contest their citation in traffic court face a \$75 court fee As discussed above, disposition codes 1 (*guilty*), 3 (*dismissed*), and A (*withheld*) are the codes that suggest a motorist attended traffic court. Pooling together missing verdicts with guilty verdicts, then, an upper bound on the share of individuals who contested their citation is 41%. An upper bound estimate on average court fees paid is thus $0.41 \times \$75 = \$30.83$.

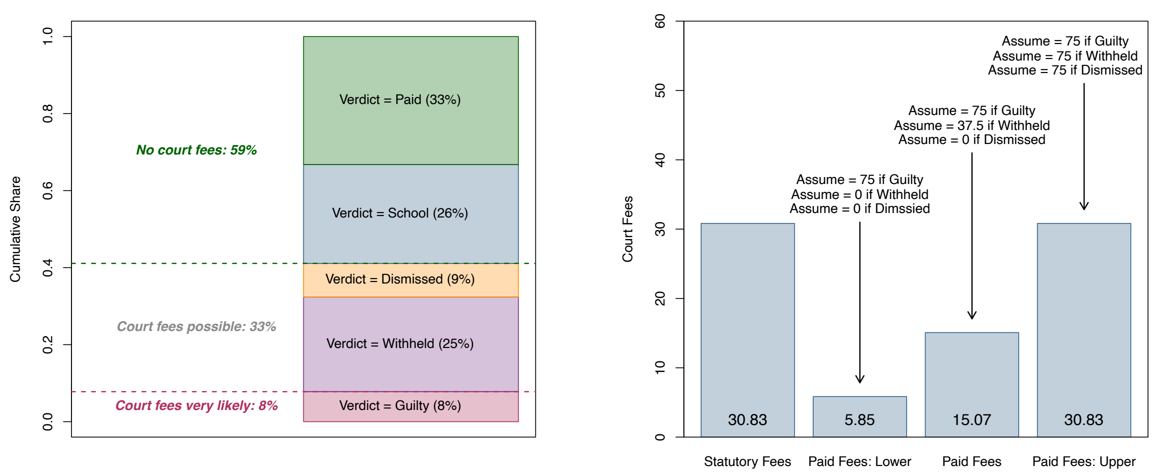
Of course, the court fee could have been waived in some instances. To construct a lower bound estimate on paid court fees, I assume that the court fee was waived for dispositions 3 (*dismissed*) and A (*withheld*) but paid by those with guilty verdicts (again, I pool missing verdicts with guilty verdicts). Hence, the lower bound estimate of paid court fees is $0.08 \times \$75 = \$5.85$.

My preferred estimate lies between these two estimates and assumes that some fraction of those with dismissed and withheld verdicts paid court fees and some fraction did not. In particular, I assume that those with verdict = 3 (*dismissed*) had their court fees waived, while half of those with verdict = A (*withheld*) had their court fees waived. This gives an overall estimate of paid court fees = $(0.25 \times \$75 \times 0.5) + (0.08 \times \$75) = \$15.07$.

Figure B-6: Estimating paid court fees

(a) Sample shares by type

(b) Range of estimates



*N*otes: Panel (a) illustrates the distribution of dispositions, characterized by court fee status. For ease of exposition, I pool those with missing verdicts (1.84 percent of the sample) together with those with guilty verdicts (5.96 percent of the sample). Panel (b) depicts the sample average estimated court fee payment under various assumptions.

B-2.3 Estimating insurance cost increases

Estimating increases in auto insurance costs is more difficult for several reasons, one of which is that there is an additional layer of assumptions required. First, I need to assume the driver license points accrued based on the disposition verdict, but then points need to be mapped into increases in auto insurance costs.

As with the above cases, there are a few subsets of the data where allocating DL points is straightforward. Those who attend traffic school (verdict = C, 25 percent) do not accrue DL points. Those who pay their fines but do not attend school (verdict = 4, 33 percent) and those who do not receive any lenience in court (verdict = 1 or missing, 8 percent) accrue the statutory points associated with their offense, which is either 3 or 4 points depending on the speed at which they were charged. Those who likely received some lenience in court (verdict = 3, 9 percent and verdict = A, 25 percent) are very likely to have had their DL points waived in court (per conversations with the Florida Clerk of Courts).

My preferred estimate of accrued points, then, assumes that those with verdicts = 3/A accrue no points. Again, those with verdict = C accrue no points for sure. Hence, this preferred estimate implies that 42 percent of the sample accrues DL points (with 29 percent accruing 3 DL points and 13 percent accruing 4 DL points). As an upper bound, I can alternatively assume that those with verdicts = 3/A accrue full DL points, which increases the estimated share of motorists accruing DL points to 74 percent (with 44 percent accruing 3 DL points and 30 percent accruing 4 DL points).

The next step is to map accrued DL points into increases in auto insurance premiums. To start, I use $3,183 as a estimate of the average annual car insurance premiums in Florida *as of 2023*.[14] According to the Bureau of Labor Statistics, the nationwide average cost of car insurance increased by 58 percent between 2013 and 2023.[15] Hence, I use $2,014.56 as my estimate of the average annual premium for my sample of citations issued over 2011-2015 (monthly premium = $167.88; quarterly premium = $503.64).

I then rely on estimates of the percent change in insurance premiums following a speeding ticket from personal finance and law firm webpages. My preferred estimate, taken from Forbes magazine, assumes that motorists with a 3-point and 4-point speeding citation experience 11 and 12 percent increases in premiums, respectively.[16] As an upper bound estimate, I replace 11 and 12 percent with 16 and 18 percent, which matches the increase reported in the same article from which I take the average premium estimate. As a lower bound estimate, I replace these estimates with 5 and 6 percent, to reflect the fact that many of the personal finance websites indicate that many insurance carriers will not increase premiums for first time offenders (and most of my sample are first time offenders).

Putting these two steps together, my preferred estimates imply that 29 percent of the sample accrues 3 DL points and faces an 11 percent increase in annual insurance premiums, while 13 percent of the sample accrues 4 DL points and faces a 12 percent increase in insurance premiums. Using the estimated average insurance premium from above, this implies an overall sample average estimate of increase auto insurance premiums =

[14] This estimate is from bankrate as of July 2023. Estimates vary widely across sources, ranging from $2,412 (according to insurify) to $3,605 (according to nerdwallet).

[15] See `https://data.bls.gov/timeseries/CUUR0000SETE?output_view=data`

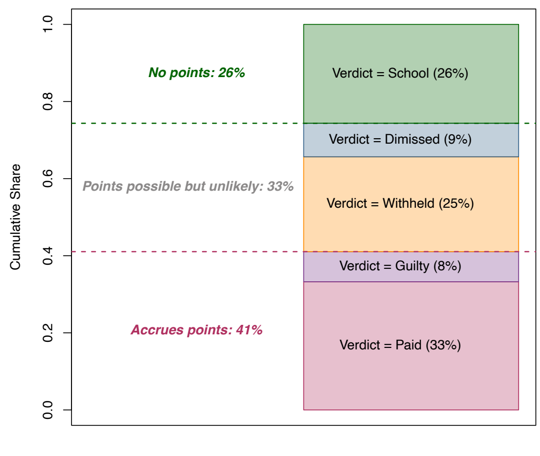
[16] See Gorzelany in Forbes, 5/17/2012.

$(0.29 \times 0.11 \times \$3,183) + (0.13 \times 0.12 \times \$3,183) = \$93$. If one holds the shares with 3 and 4 points fixed but uses the upper bound on percent increases in premiums, this number increases to \$137, while if one uses the less conservative upper assumption for the share accruing points *and* the higher estimate for premium increases, this number increases to \$252. Note that these are increases in annual premiums; assuming quarterly payments, the preferred estimate implies an increase in quarterly premiums of \$23 per quarter.

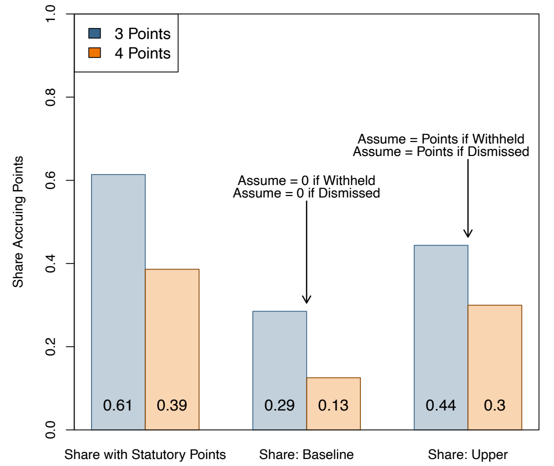
An additional complication in terms of estimating increases in car insurance costs originating with traffic citations arises from the fact that auto insurance premiums do not adjust in real time. Rather, premium increases associated with accrued driver license points will occur at the next policy renewal date. To incorporate this institutional feature into my estimates, I make the simple assumption that policy renewal dates are evenly distributed throughout the year: of those facing insurance cost increases, 25 percent face them in the quarter of their citation, 25 percent face them in the following quarter, 25 percent face them one quarter later, and all have seen insurance premium increase as of three quarters following the citation (hence the dynamic pattern shown in figure 7).

Figure B-7: Estimating insurance cost increases

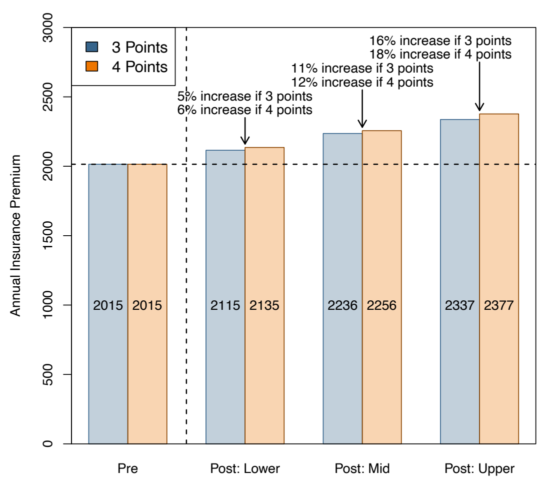
(a) Sample shares by type



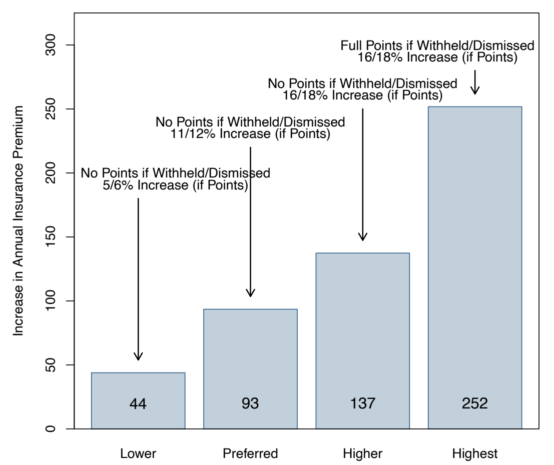
(b) Range of estimates: accrued DL points



(c) Range of estimates: insurance increases



(d) Range of estimates: combined



*N*otes: Panel (a) illustrates the distribution of dispositions, characterized by DL points status. For ease of exposition, I pool those with missing verdicts (1.84 percent of the sample) together with those with guilty verdicts (5.96 percent of the sample). Panel (b) plots the range of estimates of the share of citations that accrue 3 and 4 points, under varying assumptions. Panel (c) plots the range of estimates of annual insurance premiums, before and after a citation, for those accruing 3 and 4 points, under varying assumptions. Panel (d) combines the range of estimates in panels (b) and (c) into a range of estimates of average increases in annual insurance premiums.

C Comparison with survey estimates

Panel (a) of figure C-1 presents key responses from the public version of the 2018 Survey of Household Economics and Decisionmaking (FRBG, 2018). 46 percent of respondents indicated that they would cover a $400 emergency expense with cash, 71 percent indicated they would cover it with cash or pay it on a credit card and then pay it off at the next statement. This is the question that the heavily-cited 40 percent statistic is based on; my estimate of the share of families who could not cover an emergency $400 expense using the public data is about 30 percent, smaller than that reported in the official report. The share of households who indicated that they could still pay all their bills that month if they faced an emergency $400 expense is 86 percent.

I then explore how these shares change after considering differences between the shed sample and my sample of cited motorists. Restricting to a comparable sample (ages 18–60; white, Black, or Hipsanic) reduces each estimate slightly and then reweighting the SHED respondents to match the age and race distribution in my event study sample further reduces each estimate. After reweighting, only about 75 percent indicate that they would still be able to cover their monthly bills after facing an emergency expense.

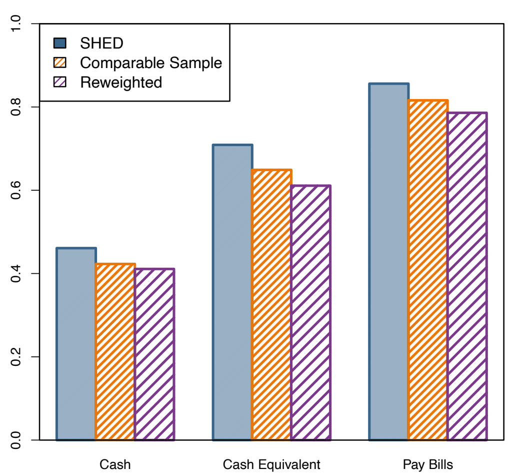
Bhutta & Dettling (2018) argue that 76 percent of American families have the funds to cover an emergency $400 expense, and this finding is often cited as evidence that the 40 percent statistic from FRBG (2018) overstates the prevalence of the inability to cover unplanned expenses. Important to note, however, is the finding from Chen (2019) that the SCF gives a similar answer to the SHED survey if one subtracts credit card debt from the liquidity measure in the SCF.

Using the 2016 SCF, Bhutta & Dettling (2018) divide households into three groups: (1) those with less than $400 in liquidity; (2) those whose liquidity $\geq$ $400 but $<$ $400 plus monthly expenses; (3) those whose liquidity $\geq$ $400 plus monthly expenses. Those in groups (1) and (3) cannot and can, respectively, cover an emergency $400 expense, while those those in group (2) may fall into either category. Their approach is to estimate the share of respondents in group (2) based on information on saving behavior and the relationship between saving behavior and liquidity.

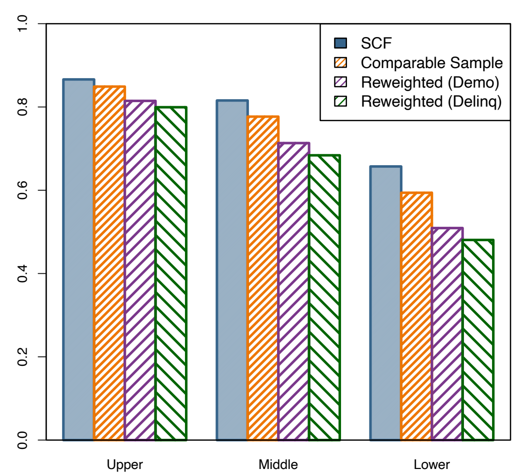
I replicate the analysis in Bhutta & Dettling (2018) in panel (b) of figure C-1 but cannot match their finding exactly, likely due to a mismatch in estimated monthly expenses. My baseline version of their estimate is that 81.6 percent can cover an emergency $400 expense. Restricting to a demographically comparable sample and then reweighing to match the demographic distribution of the sample further reduces the estimate to 71.3 percent. The SCF also includes information on a household's delinquency. Reweighting the SCF sample to match the delinquency rate at baseline in the event study sample further reduces the estimated share of families that can cover a $400 expense to 68.4 percent.

Figure C-1: Share of households that can cover an emergency $400 expense in surveys

(a) SHED



(b) SCF



*N*otes: Panel (a) plots the share of respondents in the 2018 Survey of Household Economics and Decisionmaking (SHED) public dataset indicating that they could pay a $400 emergency in cash, could pay with cash equivalent (pay in cash or pay with a credit card and pay off the credit card at the end of the month), and the share of households that indicated that they could still pay all their other bills that month after facing an emergency $400 expense. Solid blue bars report raw estimated from the SHED. Orange striped bars report estimates from the subsample of the SHED which is demographically comparable to the event study sample (ages 18–60; white, Black, or Hispanic). Purple striped bars report estimates that reweight the SHED sample to match the age and race distribution in the event study sample. Panel (b) reports the estimated share of families in the 2016 Survey of Consumer Finances (SCF) who can cover a $400 emergency expense based on the approach from Bhutta & Dettling (2018). The first set of bars reports upper bound estimates (those with at least $400 in liquid assets); the second set of bars reports their preferred estimates; the third set of bars reports the lower bound estimates (those with at least $400 plus a month's worth of expenses). Solid blue bar reports the raw data estimates from the 2016 Survey of Consumer Finances (SCF) public data extract. Orange dashed bars report estimates which restrict the SCF sample to a demographically comparable sample to the event study sample (ages 18–60; white, Black, or Hispanic). Purple dashed bars report estimates which reweight the SCF sample to match the age and race distribution in the citations sample. Green dashed bars report estimates with reweight the SCF sample to match both the age and race distribution and the rate of delinquency in the citations sample.

D Instrumental variables approach

D-1 Empirical strategy

I supplement the event study approach with a secondary identification strategy that leverages quasi-random variation in fine amounts generated by differences across officers in ticket-writing practices. In Florida, statutory fines for speeding violations depend only on an offender's speed relative to the posted limit and increase discretely at various speed thresholds. As shown in panel (a) of figure D-1, over one third of all citations are issued for exactly nine MPH over the limit, just below a \$75 increase in fine amount. This stark bunching suggests the systematic manipulation of speeds by officers as a form of lenience (Anbarci & Lee 2014; Goncalves & Mello 2021; Goncalves & Mello 2023).[17]

I leverage the systemic variation across officers in the propensity to bunch drivers below the fine increase by computing the following instrument, which I call officer stringency:

$$Z_{ij} = 1 - \left(\frac{1}{N_j - 1} \sum_{k \neq i} \mathbf{1}[speed_{kj} = 9] \right) \equiv \text{stringency} \tag{D-1}$$

In words, Z_{ij} is the fraction of officer j's citations to motorists other than i which are not bunched at nine MPH. I then estimate regressions of the form:

$$\Delta Y_{ijs\tau} = \theta Fine_{ij} + \gamma X_i + \psi_s + u_{ijs} \tag{D-2}$$

by 2SLS, using Z_{ij} as an instrument for the fine amount. Here, $\Delta Y_{ijs\tau}$ is the change in outcome Y for driver i stopped by officer j between one quarter prior to the traffic stop and τ quarters after the traffic stop. The ψ_s's are beat-shift fixed effects at the level of county $\times$ agency $\times$ $\mathbf{1}$[highway] $\times$ year $\times$ month $\times$ $\mathbf{1}$[weekend] $\times$ shift, which adjust for differences in driver and officer composition across patrol assignments. Estimated using a cross-section, this specification permits the inclusion of motorist-level controls, X_i. Standard errors are clustered at the beat-shift level (Chyn et al., 2022).

Validity of this IV approach requires the usual LATE assumptions (e.g., Imbens & Angrist 1994). Papers using comparable examiner designs for identification (e.g., Kling 2006; Maestas et al. 2013; Dahl et al. 2014; Dobbie & Song 2015) typically appeal to institutional features, such as the randomized rotation of criminal case assignments across courtrooms, as evidence for instrument exogeneity. In the traffic citation setting, there is no institutional randomization of patrol officers to motorists, highlighting the important concern that officers with different bunching propensities may have differently selected samples. Figure H-2, however, shows that, after conditioning on beat-shifts, stringency is uncorrelated with an officer's citation frequency and uncorrelated with a motorist's financial situation, as summarized by their credit score.[18] Moreover, equation D-2 is specified in differences, so exogeneity

[17] Figure H-1 illustrates significant dispersion across officers in the propensity to bunch drivers, net of beat-shift effects, motorist characteristics, and estimation error, as well as the correlation in estimated officer bunching propensity in two random partitions of the data. See Goncalves & Mello (2023) for further discussion.

[18] Table H-1 shows the relationship between other motorist characteristics and officer stringency,

only requires that stringency is unrelated to (potential) trends in financial outcomes. As a validity check, I show that the stringency instrument cannot predict pre-stop changes in outcomes and also estimate more conservative DiD, or trend-break, versions of (D-2), which replace the outcome with $(Y_\tau - Y_{-1}) - (Y_{-1} - Y_{-4})$, i.e., the change in Y following the stop minus the change in Y preceding the stop.

This IV approach also requires exclusion and monotonicity assumptions. Exclusion requires that stringency only influences changes in outcomes through fine amounts. As shown in Frandsen et al. (2019), 2SLS estimates in examiner designs recover the desired LATE under an average monotonicity assumption which states that counterfactual reassignment to a more stringent officer increases fine amounts in expectation. Table H-2 illustrates that the first stage estimates are comparable across subgroups of motorists.

Relative to the staggered timing design, the main advantage of the instrumental variables approach is the ability to compare two drivers stopped at the same time, alleviating the core identification concerns associated with the event study. On the other hand, there are several complications associated with the IV approach. As shown in Goncalves & Mello (2023), officer stringency generates variation in both traffic court behavior and future traffic offending. The fact that stringency increases the likelihood that a motorist contests a citation in court (see figure H-3) precludes the IV approach from separating the effects of fine payment and other potential mechanisms. Specific deterrence effects associated with stringency should bias the instrumental variables estimates towards zero, as motorists facing lower fines are more likely to accrue additional fines in the future, a feature that makes the stringency approach especially poorly suited to the estimation of longer run effects. And finally, as one might expect, the instrumental variables estimates are substantially less precise.

Also worth noting is the fact that the instrumental variables approach identifies a different parameter than the event study design. First, the IV estimates correspond to a pure intensive margin effect: the officer instrument generates variation in fine amounts among individuals fined at the same time and in the same area. Everyone in the IV sample faces a fine of at least $123, which is the fine for speeding 9 MPH over the limit. And second, the IV estimates recover a local average treatment effect (LATE) for the subsample of compliers.

D-2 Sample construction

To compute the officer stringency instrument, I use the full sample of speeding citations for speeds 9–29 MPH over the posted limit where speeding is the only violation, regardless of whether the driver is matched to the credit file, imposing the following restrictions: (i) citations issued by Florida Highway Patrol (FHP) or county sheriffs; (ii) the officer is identifiable; (iii) the officer issues at least 50 citations. I focus on FHP or sheriff citations because the officer identity is not consistently recorded on citations issued by municipal police. The instrument can be computed for 2,265 officers and 761,355 total speeding citations.

The IV sample is then the intersection of this set of speeding citations for which the instrument can be computed and the set of speeding citations attributable to the initial sample of matched individuals, again restricting to white, Black, or Hispanic motorists (N

conditional on beat-shift effects (joint $F = 2.6$). For an expanded discussion of instrument validity in this setting, see Goncalves & Mello (2023).

citations = $362,854$, N individuals = 332,933). To maximize the IV sample size, I do not impose the clean year restriction and allow motorists to appear multiple times. Figure H-7 shows that results are similar when additionally imposing these restrictions.

D-3 Results

Panel (a) of figure D-1 illustrates the idea underlying the officer IV approach, which is that officers tend to bunch apprehended speeders below a \$75 increase in fine at 10 MPH over the limit. Panel (b) illustrates the first stage relationship between officer stringency, or the propensity *not to bunch* drivers, and fine amounts, conditional on beat-shift fixed effects. The first stage slope estimate, $\beta = \$124$, approximately corresponds to the expected fine increase associated with being reassigned from the most lenient to the most stringent officer. The first stage is linear, precisely estimated, and statistically strong ($F \approx 70,000$).

Panel (c) illustrates the reduced form relationship between officer stringency and changes over time in collections balances, both residualized of beat-shift effects. While officer stringency has no ability to predict changes between four quarters and one quarter prior to the traffic stop (red squares; $\beta = -0.41$; $se = 9.9$), a relationship between stringency and the change between one quarter prior and three quarters after is apparent (blue circles; $\beta = 29.83$). Although the standard error is large (11.7), the estimate is statistically significant at conventional levels.[19]

Figure H-4 in the appendix plots the corresponding estimates over all (feasible) time horizons for the full set of outcomes. As in the event study analysis, slight increases in credit card balances and declines in the likelihood of holding a payroll-covered job are suggested but imprecisely estimated. Estimates for the remainder of financial distress outcomes are both very small in magnitude and too imprecise to draw firm conclusions; hence, I focus primarily on collections balances when presenting IV estimates but also show results for credit card balances in table D-1, which reports IV estimates in different specifications.

Columns 1-2 of table D-1 report estimates when including controls for motorist age, gender, race, neighborhood income, and credit score, while columns 3-4 show that all estimates are both qualitatively and quantitatively similar when omitting motorist controls. Panel B of the table shows the relationship between the instrument and the pre-stop change, while panels C, D, and E show estimates for the post-stop change over different time horizons ($\tau = 1,\ \tau = 3,\ \tau = 6$). For each of the post-stop time horizons, I also report the more conservative DiD version of the 2SLS estimate which replaces the outcome with the difference in the post- and pre-stop changes: $(Y_{i,\tau} - Y_{i,-1}) - (Y_{i,-1} - Y_{i,-4})$ for $\tau \in \{1, 3, 6\}$.

As shown in panel B, the officer instrument cannot predict pre-stop changes in collections or credit card balances. The point estimate in panel C suggests that the stringency instrument predicts a \$24 ($se = 26$) increase in credit card balances in the first quarter after a traffic stop, with a corresponding 2SLS estimate of 0.198. Recall that the 2SLS estimates will rescale the reduced form estimates for the change in balances by the fine amount; hence these IV estimates are directly interpretable as the share of the marginal fine borrowed. While these estimates are not statistically distinguishable from zero, the pattern of short-

[19] Figure H-5 illustrates that this reduced form relationship is more pronounced for lower-income motorists ($\beta = \$44.3,\ se = 14.5$) than for higher-income motorists ($\beta = \$16.3,\ se = 13.13$).

run increases in credit card borrowing which do not persist (as shown in panels D and E) is remarkably consistent with the corresponding event study estimates.

Also consistent with the event study estimates, the stringency instrument predicts meaningful increases in collections balances over longer time horizons. Corresponding to figure D-1, panel D of table D-1 implies that 24 percent of the marginal fine increases generates by the stringency instrument have appeared as collections balances on a motorist's credit report as of three quarters after the traffic stop. Six quarters out, the corresponding estimate grows to about 34 percent. Thus, the IV estimates support the basic conclusion of the event study analysis that fines induce default on other financial obligations, or in other words, that individuals borrow from other financial obligations in order to cover the fine.

D-3.1 Comparison with event study results

The IV estimates imply that three (six) quarters out from a traffic stop, 24 (34) percent of the additional fine amount has appeared as unpaid collections debt. The comparable estimates for the event study design based on the average fine amount ($195.53) are 13 and 18 percent. Hence, adjusted for the relevant fine amounts, the IV estimates appear about 85 percent larger, with the caveat that the IV estimates are not sufficiently precise to rule out that the two strategies give identical estimates.

A particularly plausible rationale for the different estimates is some convexity in the relationship between fine amounts and default. The event study approach yields the average default amount associated with a $195 fine, while the IV estimate gives the effect of an *additional* $124 in fines beyond the $123 fine associated with the most lenient speeding charge. If households default on a lower share of the first hundred dollars in fines than the second hundred dollars in fines, which seems like a reasonable hypothesis, we would indeed expect larger estimates from the IV approach. The same logic could also be applied to rationalize the larger, albeit imprecise, short-run effect on credit card borrowing in the IV design (~ 20 percent) than in the event study design (~ 7 percent).

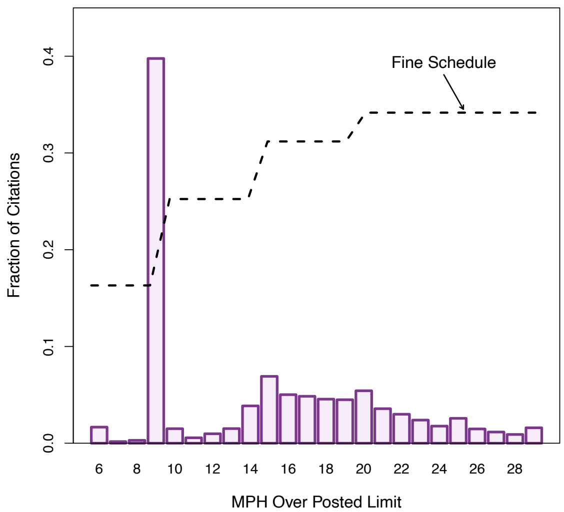
Alternatively, the effects on default in the two empirical designs may be more similar than is suggested by comparing the collections balances estimates. While event study estimates suggest increases in other measures of default, there is no evidence of impacts on the number of delinquent or derogatory accounts using the IV approach (see figure H-4). Hence, the estimates in both approaches may suggest similar overall impacts on financial distress. Unfortunately, this hypothesis is not directly testable without a dollar metric for delinquency, which the data do not include. Differences between IV and event study estimates could alternatively be due to a correlation between LATE weights and treatment effects.

D-3.2 Robustness

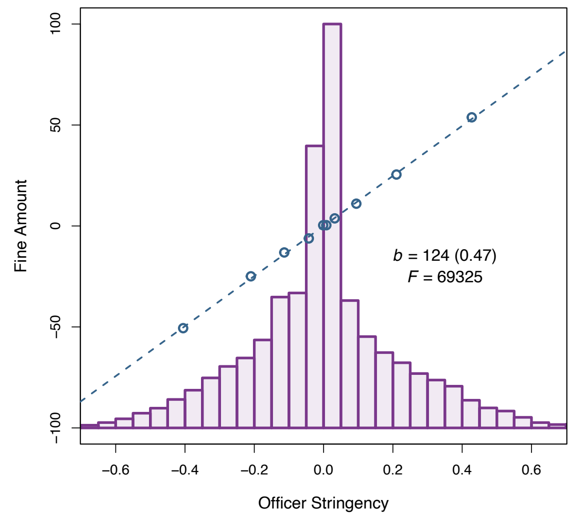
In supplementary appendix H, I show that IV estimates are qualitatively and quantitatively similar when using alternative definitions of the stringency instrument and imposing alternate sample restrictions. I also show that results are not sensitive to trimming officers from the sample based on their estimated degree of sample selection.

Figure D-1: Instrumental variables approach

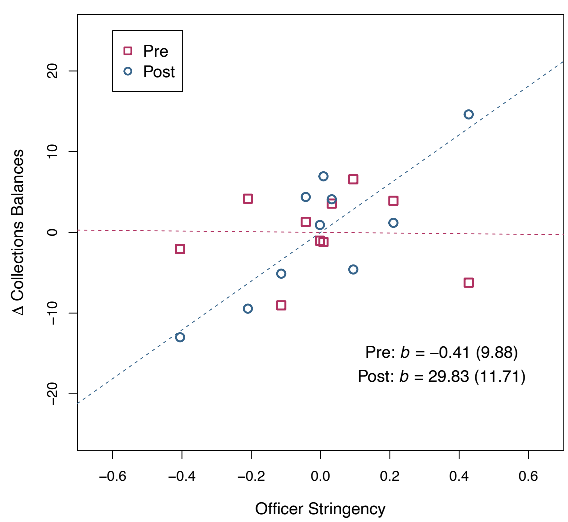
(a) Histogram of Charged Speeds



(b) First Stage



(c) Reduced Form



*N*otes: Panel (a) shows the the distribution of charged speeds relative to the posted limit on all speeding tickets issued by the Florida Highway Patrol or county sheriff departments. Panel (b) illustrates the relationship between the fine amount and the officer stringency instrument, both residualized of beat-shift fixed effects, using the IV sample ($N = 362,854$). Panel (c) plots the relationship between the officer stringency instrument and the change over time in collections balances, both residualized of beat-shift fixed effects and motorist controls, again using the IV sample. Red squares denote the *pre-stop* change between $\tau = -4$ and $\tau = -1$ and blue circles plot the *post-stop* change between $\tau = -1$ and $\tau = 3$. Figure reports the corresponding regression estimates and standard errors clustered at the beat-shift level.

Table D-1: Officer IV Results

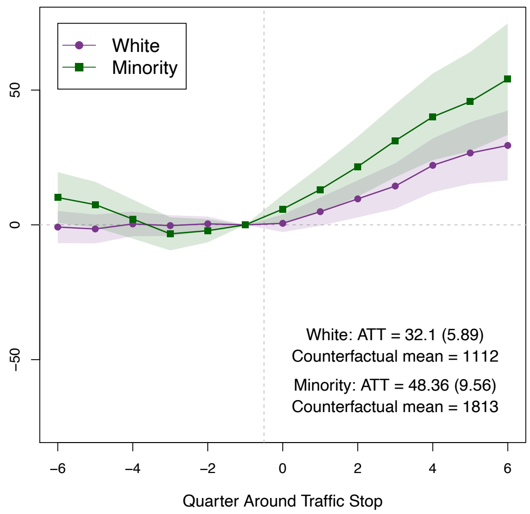
	With Controls		Without Controls	
	(1) Collections	(2) Revolving	(3) Collections	(4) Revolving
Panel A: First Stage				
Fine Amount	124.01		124.17	
	(0.47)		(124.17)	
Panel B: Δ -4 to -1				
Reduced Form	-0.41	-3.51	-3.87	5.02
	(9.88)	(30.15)	(9.91)	(30.27)
2SLS	-0.003	-0.028	-0.031	0.04
	(0.08)	(0.243)	(0.08)	(0.244)
Panel C: Δ -1 to 1				
Reduced Form	7.32	24.6	5.79	28.43
	(8.51)	(25.55)	(8.51)	(25.72)
2SLS	0.059	0.198	0.047	0.229
	(0.069)	(0.206)	(0.069)	(0.207)
2SLS DiD	0.062	0.227	0.078	0.189
	(0.108)	(0.329)	(0.108)	(0.329)
Panel D: Δ -1 to 3				
Reduced Form	29.83	6.54	27.35	12.97
	(11.71)	(35.29)	(11.7)	(35.68)
2SLS	0.241	0.053	0.22	0.104
	(0.094)	(0.285)	(0.094)	(0.287)
2SLS DiD	0.244	0.081	0.251	0.064
	(0.128)	(0.394)	(0.128)	(0.395)
Panel E: Δ -1 to 6				
Reduced Form	42.6	-5.14	39.39	6.43
	(15.06)	(44.36)	(15.06)	(44.76)
2SLS	0.344	-0.041	0.317	0.052
	(0.121)	(0.358)	(0.121)	(0.36)
2SLS DiD	0.347	-0.013	0.348	0.011
	(0.152)	(0.459)	(0.152)	(0.46)

*N*otes: This table reports estimates from the officer IV design for collections and revolving balances, with and without motorist controls. All regressions include beat-shift fixed effects and standard errors are clustered at the beat-shift level. Panel (a) reports the first-stage relationship between the officer instrument and the fine amount. Panel (b) reports reduced form and 2SLS estimates where the outcome is the *pre-stop* change between $\tau = -4$ and $\tau = -1$. Panels (c)-(e) report estimates for the *post-stop* change over different time horizons ($\tau = 1$, $\tau = 3$ and $\tau = 6$), relative to $\tau = -1$. 2SLS DiD estimates replace the change $Y_\tau - Y_{-1}$ with the pre-period adjusted change, $(Y_\tau - Y_{-1}) - (Y_{-1} - Y_{-4})$ as the outcome. Estimates for additional time horizons and additional outcomes are presented in figure H-4. Sample is the IV sample, $N = 362,854$.

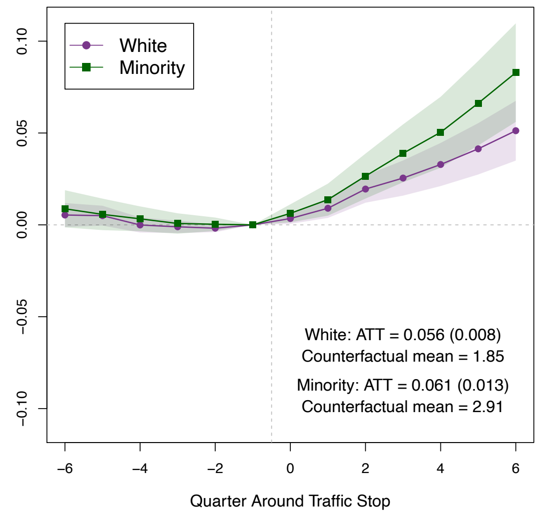
E Heterogeneity by motorist race

Figure E-1: Event study estimates by race

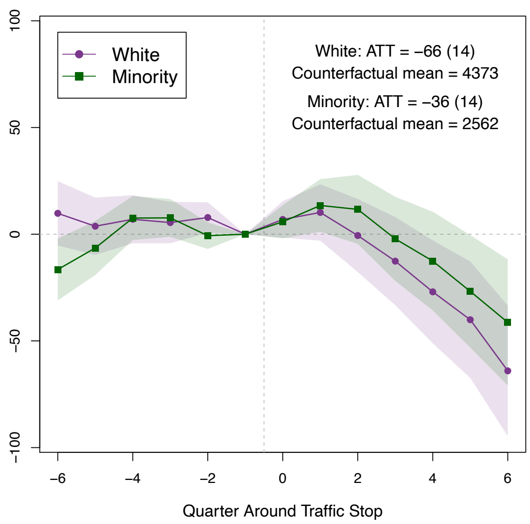
(a) Collections Balances



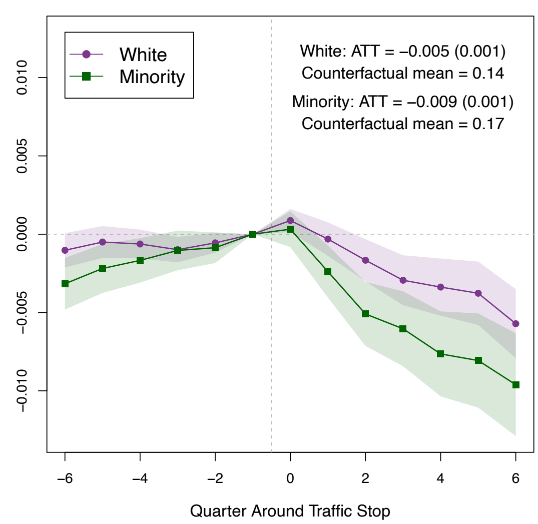
(b) Credit Line Delinquencies



(c) Revolving Balances



(d) Payroll Employment

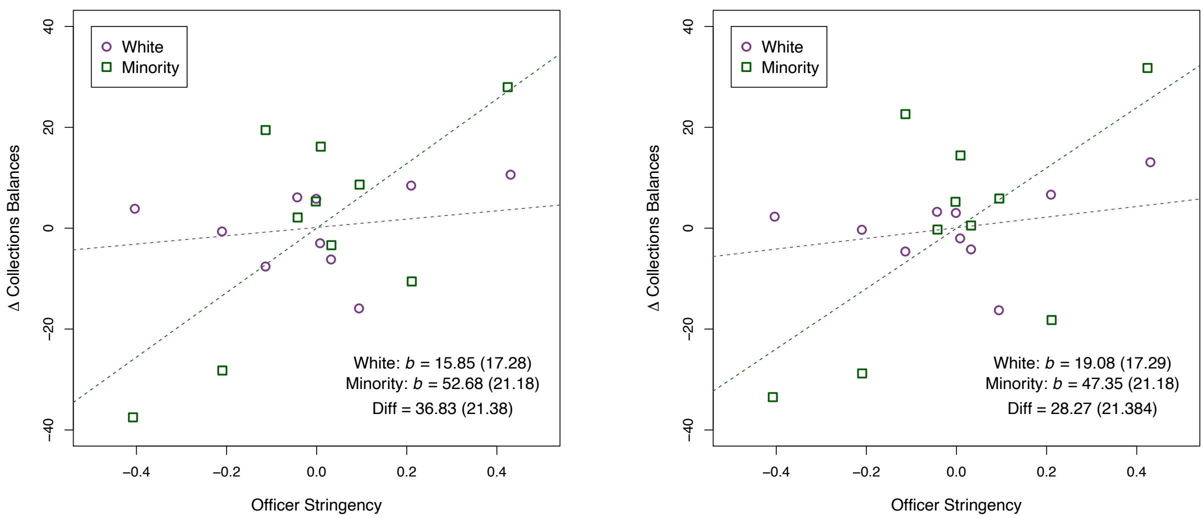


Notes: Each figure reports event study estimates, obtained via the Callaway & Sant'Anna (2021) approach, as well as 95 percent confidence intervals based on design-based standard errors from Roth & Sant'Anna (2022), for the denoted outcome. Event studies are estimated separately for white ($N = 195,373$) and Black or Hispanic $(114,225)$ motorists with dispositions indicating a paid fine or traffic school election (the "definitely paid" subset). See figure G-9 for estimates by various subgroups and by race.

Figure E-2: Officer IV reduced form estimates by race

(a) Without controls

(b) With controls



*N*otes: This figure reports heterogeneity in the relationship between the officer stringency instrument and the DiD in collections balances, $(Y_3 - Y_{-1}) - (Y_{-1} - Y_{-4})$ where the subscripts index event time, both residualized of beat-shift fixed effects, by motorist race. The first stage estimate for white motorists is $\beta_{FS} = 124.69$ (0.497) and the first stage estimate for minority motorists is $\beta_{FS} = 123.22$ (0.51). Panel (a) reports estimates without controls and panel (b) reports estimates that include controls for age, age squared, gender, baseline estimated income, credit score, and available credit card balances. Each figure reports the corresponding regression estimates for white and minority motorists, as well as the difference, with standard errors clustered at the beat-shift level.

Table E-1: Officer IV results by race

	$\tau = 3$			$\tau = 6$		
	(1) White	(2) Minority	(3) p-val	(4) White	(5) Minority	(6) p-val
Panel A: No Controls						
	0.134	0.414	0.082	0.153	0.628	0.012
	(0.137)	(0.167)		(0.163)	(0.197)	
Panel B: Demographics						
	0.137	0.412	0.088	0.162	0.626	0.014
	(0.137)	(0.167)		(0.163)	(0.198)	
Panel C: Add Income						
	0.136	0.412	0.087	0.157	0.625	0.013
	(0.137)	(0.167)		(0.163)	(0.198)	
Panel D: Add Credit Access						
	0.161	0.371	0.192	0.196	0.563	0.053
	(0.137)	(0.167)		(0.162)	(0.197)	
Panel E: Add Durables						
	0.154	0.373	0.176	0.178	0.569	0.039
	(0.137)	(0.167)		(0.162)	(0.197)	

*N*otes: This table reports 2SLS IV estimates from the officer IV design by motorist race. All regressions include beat-shift fixed effects and standard errors are clustered at the beat-shift level. Dependent variable is the DiD in collections balances, $(Y_\tau - Y_{-1}) - (Y_{-1} - Y_{-4})$, for $\tau = 3$ (columns 1-3) and $\tau = 6$ (columns 4-6). Each panel successively adds motorist controls. Demographics include age, age squared, and gender. Panel C adds baseline estimated income. Panel D adds credit score and available balance on credit cards. Panel E adds indicators for any open auto loan or mortgage. Columns (3) and (6) report the p-value from a test of equality for the white and Minority estimates in columns 1-2 and columns 4-5, respectively.

F Additional data information

F-1 Data sources

Citations data

I obtained administrative records of the universe of traffic citations issued in the state of Florida over the period 2010-2015 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 F-1 shows a sample UTC form and figure F-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 withheld by judge*; B = *other*; C=*adjudication withheld by clerk (school election)*; D = *adjudication withheld by clerk (plea nolo and proof of compliance)*; E = *set aside or vacated by court*.

In the event study sample ($N = 525,646$ citations), 1.8 percent have no associated disposition, 80.9 percent have one associated disposition, and the remaining 17.4 percent have multiple dispositions records (some of which may be duplicated). When a there are multiple disposition records, I use the first valid entry as the disposition verdict. See appendix section B-2 for an expanded discussion of the disposition verdicts.

Sanctions Information

The UTC database does not include reliable measures of sanctions. 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 appearance), 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 proprietary model which predicts an individual's income, rounded to the nearest thousand, using information on the credit file. As shown in figure F-4, estimated income is highly correlated with both zip code per-capita income and earnings in the employment database where reported.

Payroll records

Access to payroll records covering a subset of large employers was also provided by one of the three major credit bureaus. 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. Additional information on the payroll records is provided in appendix section B-1.

F-2 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 F-2) and therefore contain transcription errors. Second, according to Brevoort et al. (2015), about eleven percent of adults, and as many as thirty percent in lower-income areas, 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 F-3. Results from regressing a successful credit file match on available driver characteristics are shown in table F-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 access, through a secure server hosted by the credit bureau, to the anonymized citations data and monthly credit reports, 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 ($\sim$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.

Aggregation

All variables are first computed using monthly data. I then aggregate the data to the person $\times$ 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 2010-2015 panel of 525,646 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. If the variable is still missing (less than 0.5 percent of the data in all cases), I impute zero. 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.

F-3 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 Default Flag.* Equal to one if new collection, new delinquency, or new derogatory equals one. Zero otherwise.

9. *Any Revolving Account.* Equal to one if "number open revolving accounts on file" is greater than zero. Zero otherwise.

10. *Revolving Balances.* Sum of balances for all open revolving accounts on file with update within the last 3 months.

11. *Revolving Limits.* Total credit limit/high credit open revolving accounts with update within 3 months

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 95th percentile.

Credit bureau variables can be missing in a given month because an individual lacks a credit report or for other reasons related to reporting issues or data quality. In most cases, this is due either to the fact that there is a balance or number of accounts on file but no associated update date, or vice versa, i.e., there is an update date but no information on balances. If key inputs are missing for this reason, computed variables such as credit scores will typically also be missing. Again, this is true for less than 0.5% of all person-quarters in the data. There are also missing codes for no relevant account on file. I impute zeroes for all missing codes, which is a conservative choice.

F-4 Imputed variables

Baseline estimated income

The data include three separate income measures: (i) per-capita income in the individual's zip code of residence, computed from the IRS Statistics of Income (SOI) files and based on the zip code reported on a driver's DL in the citations data; (ii) credit bureau estimated income, which is estimated based on credit file attributes according to a proprietary model; (iii) annualized payroll earnings, available only for the subset of individuals with an active entry in the payroll database (∼15 percent of the data).

In figure F-4, I plot the relationship between these income measures for the subset of individuals with observed payroll earnings at some point during the first year of the data. Here, zip code income is measured at each individual's first traffic stop and both payroll earnings and credit bureau estimated income are averaged over the first year of the data. While all three measures are highly correlated, credit bureau estimated income has substantially more ability to predict cross-sectional variation in payroll earnings ($R^2 = 0.38$) than does zip code income ($R^2 = 0.054$). Based on figure F-4, I construct my primary measure of baseline income using a weighted average of zip code income and credit bureau estimated income at baseline, with the weights taken from the regression of payroll earnings on zip code income and estimated income, again using only observations with observed payroll earnings. Hence, a literal interpretation of baseline predicted income is predicted payroll earnings based on zip code of residence and the credit bureau income model.

I estimate this regression only using baseline data and use this predicted income measure only to split the sample based on baseline income. If a contemporaneous, rather than baseline, income measure is desired (e.g., for heterogeneity in the IV estimates), I use the zip code income measured in the citations data.

Imputed borrowing limits

As highlighted in the text, one complication with interpreting results based on the borrowing limit measure in the data is the fact that borrowing limits are only reported for individuals with open revolving accounts. Hence, I also construct an imputed borrowing limit based on the cross-sectional relationship between credit scores and borrowing limits at baseline for individuals with revolving accounts. As shown in figure F-5, the relationship is highly nonlinear in the raw data. I construct predicted borrowing limits by combining separate quartic polynomials estimated over the ranges 350-450, 450-775, and 775-850, imposing that the piece-wise function is continuous and weakly increasing over the range 350-850.

I impute a limit of zero for credit scores below 350 because the probability of having any revolving credit is approximately zero below 350 and impute an upper limit of $80,000. Note that this upper limit only binds at credit scores above 838, which is outside the support of credit scores in the event study data. The solid line in figure F-5 illustrates the imputed borrowing limit. In the baseline cross-section, a regression of the true borrowing limit on the imputed borrowing limit, which can explain 16 percent of the variation in borrowing limits.

F-5 Computing

All data analysis was conducted in Rstudio workbench server, accessed through a citrix terminal operated by the credit bureau. On the credit bureau system, an Rstudio server session automatically terminates after eight hours regardless of jobs in progress. The command `att_gt` from the `did` package, which computes the parallel trends test from Callaway & Sant'Anna (2021), cannot be completed in eight hours using the full event-study sample ($N = 525,646$). To obtain event study estimates and standard errors, I use the `staggered` package, which automatically normalizes estimates to $\tau = -1$ and computes analytical uniform confidence bounds based on the design-based standard errors in Roth & Sant'Anna (2022) instead of the default bootstrapped standard errors in the `did` package. I also use the `staggered` package to estimate event studies via the method in Sun & Abraham (2021).

The eight-hour limit is also an issue for computing estimates using the Borusyak et al. (2022) method. I compute point estimates for their method manually following their two step imputation procedure, but existing packages to estimate standard errors (`didimputation` and `did2s`) cannot accommodate the size of the relevant panel. Standard errors could be bootstrapped, but a sufficiently large number of bootstrap iterations cannot be performed within the eight-hour time window. Hence, I do not report standard errors for estimates obtained via the Borusyak et al. (2022) approach.

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

XXXXXXE

FLORIDA UNIFORM TRAFFIC CITATION

COUNTY OF

CITY OF (IF APPLICABLE)

☐ (1) FHP ☐ (2) P.D. ☐ (3) S.O. ☐ (4) OTHER

AGENCY NAME____________________________

AGENCY #_______________________________

IN THE COURT DESIGNATED BELOW THE UNDERSIGNED CERTIFIES THAT HE/SHE HAS JUST AND REASONABLE GROUNDS TO BELIEVE AND DOES BELIEVE THAT ON

SUMMONS
(VIOLATOR'S COPY)

DAY OF WEEK | MONTH | DAY | YEAR | ☐ A.M. ☐ P.M.

NAME (PRINT) FIRST | MIDDLE | LAST

STREET | IF DIFFERENT THAN ONE ON DRIVER LICENSE "X" HERE

CITY | STATE | ZIP CODE

TELEPHONE NUMBER | DATE OF BIRTH MO | DAY | YEAR | RACE | SEX | HGT

DRIVER LICENSE NUMBER | STATE | CLASS | CDL LICENSE ☐ YES ☐ NO | YR. LICENSE EXP. | COMMERCIAL VEHICLE ☐ YES ☐ NO

YR. VEHICLE | MAKE | STYLE | COLOR | PLACARDED HAZ. MATERIAL ☐ YES ☐ NO

VEHICLE LICENSE NO. | TRAILER TAG NO. | STATE | YEAR TAG EXPIRES | ≥ 16 PASSENGERS ☐ YES ☐ NO

UPON A PUBLIC STREET OR HIGHWAY, OR OTHER LOCATION, NAMELY | MOTORCYCLE ☐ YES ☐ NO | COMPANION CITATION(S) ☐ YES ☐ NO

FT__________ MILES__________ ☐ N ☐ S ☐ E ☐ W OF NODE ______

DID UNLAWFULLY COMMIT THE FOLLOWING OFFENSE. CHECK ONLY ONE OFFENSE EACH CITATION.

☐ UNLAWFUL SPEED ________ MPH SPEED APPLICABLE ________ MPH
(☐ INTERSTATE ☐ SCHOOL ZONE ☐ CONSTRUCTION WORKERS PRESENT)
SPEED MEASUREMENT DEVICE:

☐ CARELESS DRIVING
☐ VIOLATION OF TRAFFIC CONTROL DEVICE
☐ FAILURE TO STOP AT A TRAFFIC SIGNAL
☐ IMPROPER LANE CHANGE OR COURSE
☐ NO PROOF OF INSURANCE
☐ VIOLATION OF RIGHT-OF-WAY
☐ CHILD RESTRAINT
☐ SAFETY BELT VIOLATION
☐ IMPROPER OR UNSAFE EQUIPMENT
☐ EXPIRED TAG SIX (6) MONTHS OR LESS
☐ EXPIRED TAG MORE THAN SIX (6) MONTHS
☐ IMPROPER PASSING
☐ EXPIRED DRIVER LICENSE SIX (6) MONTHS OR LESS
☐ EXPIRED DRIVER LICENSE MORE THAN SIX (6) MONTHS
☐ NO VALID DRIVER LICENSE
☐ DRIVING WHILE LICENSE SUSPENDED OR REVOKED
☐ DRIVING UNDER THE INFLUENCE
☐ Passenger Under 18 Yrs
BAL ________

OTHER VIOLATIONS OR COMMENTS PERTAINING TO OFFENSE: | RE-EXAM ☐ YES ☐ NO | DL SEIZED ☐ YES ☐ NO

☐ AGGRESSIVE DRIVING | IN VIOLATION OF | SECTION | SUB-SECTION

CRASH ☐ YES ☐ NO | PROPERTY DAMAGE ☐ YES $ ________ ☐ NO | INJURY TO ANOTHER ☐ YES ☐ NO | SERIOUS BODILY INJURY TO ANOTHER ☐ YES ☐ NO | FATAL ☐ YES ☐ NO

☐ CRIMINAL VIOLATION. COURT APPEARANCE REQUIRED. AS INDICATED BELOW.
☐ INFRACTION. COURT APPEARANCE REQUIRED. AS INDICATED BELOW.
☐ INFRACTION WHICH DOES NOT REQUIRE APPEARANCE IN COURT.

XXXXXXE

CIVIL PENALTY IS $__________

COURT INFORMATION
DATE | TIME
COURT
LOCATION

Additional Comments: This is sample text showing proof of concept of additional comment proposal for the Uniform Traffic Citation.This field can hold up to 255 Alphanumeric characters. This text will show under the Court information as positioned in the current UTC print out.

ARREST DELIVERED TO________________________ DATE__________

I AGREE AND PROMISE TO COMPLY AND ANSWER TO THE CHARGES AND INSTRUCTIONS SPECIFIED IN THIS CITATION. WILLFUL REFUSAL TO ACCEPT AND SIGN THE CITATION MAY RESULT IN ARREST. I UNDERSTAND MY SIGNATURE IS NOT AN ADMISSION OF GUILT OR WAIVER OF RIGHTS. IF YOU NEED REASONABLE FACILITY ACCOMMODATIONS TO COMPLY WITH THIS CITATION, CONTACT THE CLERK OF THE COURT.

X SIGNATURE OF VIOLATOR (SIGNATURE IS REQUIRED IF INFRACTION REQUIRES APPEARANCE IN COURT)

Rank - Name Of Officer | Badge No. | ID No. | Troop/Unit

☐ I CERTIFY THIS CITATION WAS DELIVERED TO THE PERSON CITED ABOVE AND CERTIFY THE CHARGE ABOVE

Additional Officer:

Rank - Name Of Officer | Badge No. | ID No. | Troop/Unit

HSMV 75901 (Rev. 06/19)

IMPORTANT INSTRUCTIONS REGARDING A NON-CRIMINAL TRAFFIC INFRACTION NOT REQUIRING A COURT APPEARANCE

If you were charged with a civil infraction, you must complete one of the following options **within 30 calendar days** of the date of this citation. If you fail to comply **within 30 calendar days**, your driving privilege will be suspended until you comply. You will then be subject to additional penalties. Please see the front of the citation for the contact information for the Clerk of Court in the county where this violation occurred.

Option 1: You may pay the civil penalty listed on the front of the citation to the Clerk of Court. You must enclose this citation if you mail payment, which may be a money order or a cashier's check. The clerk _____does _____does not accept personal checks. You may pay this citation on-line at **www.payflclerk.com**. Payment of the civil penalty is considered a conviction and points will be assessed, if applicable. Proof of compliance in the form of a driver license or registration certificate, whichever is applicable, is required in addition to payment if you were cited for driver license expired less than six months, expired tag less than six months, failure to display a valid driver license or failure to display a valid registration. You **will** be required to complete a driver improvement course if you are convicted of running a red light or passing a school bus. Your driving privilege will be suspended if you are convicted of not providing proof of insurance. Accumulation of points may increase the cost of your insurance.

Option 2: If you were cited for expired driver license, failure to display a valid driver license, expired tag, failure to possess a valid registration, or no proof of insurance, you may show proof to the Clerk of Court that you had a valid driver license, tag/registration, or insurance, whichever is applicable, at the time of the offense. The charge will be dismissed upon payment of a dismissal fee.

Option 3: If you **do not** hold a commercial driver license and you were cited for driver license expired 6 months or less, expired tag 6 months or less, failure to display a valid driver license, failure to possess a valid registration, no proof of insurance, or driving while license suspended [see s. 322.34(10)(a), F.S.], you may elect to show proof of compliance to the Clerk of Court in the form of a valid driver license, registration or proof of insurance, whichever is applicable. You may only make one such election per 12 month period and no more than three elections in a lifetime. You must pay court costs and adjudication will be withheld.

Option 4: If you **do not** hold a commercial driver license, you may be eligible to elect to complete a Florida driver improvement course. You must contact the Clerk of Court to make this election. You may make only one such election per 12 month period and not more than 5 elections in your lifetime. Please visit www.flhsmv.gov for a list of approved courses and to determine your eligibility for this election. Adjudication will be withheld and points will not be assessed. You must pay a civil penalty and court costs. This option is not available for certain traffic offenses, including driver license, tag, and registration violations. Completion of a driver improvement course is required if you are cited for running a red light/traffic control device, even if you do not make this election.

Option 5: You may elect a court hearing by contacting the Clerk of Court. If you request a hearing and the County Judge/Magistrate/Hearing Officer determines that you have committed the offense, the County Judge/Magistrate/Hearing Officer may impose a penalty of up to $500 (or $1,000 if a fatality occurred) and/or require completion of a driver improvement course. Points may be assessed. If it is determined that no infraction has been committed, no cost or penalties shall be imposed.

Option 6: If you were cited with a non-criminal violation of operating a motor vehicle in an unsafe condition (s. 316.610 F.S.) or not properly equipped (s. 316.610, F.S. or s. 316.2935, F.S.), you may have the defect corrected, then contact your local county or city law enforcement agency to have the correction certified below. You must pay the local law enforcement agency [$_______] for this service. You may then mail or present this affidavit of compliance along with [$_______] to the Clerk of Court within 30 calendar days of the date of this citation. No points will be assessed. This option does not apply to a commercial motor vehicle or a transit bus owned by a governmental entity.

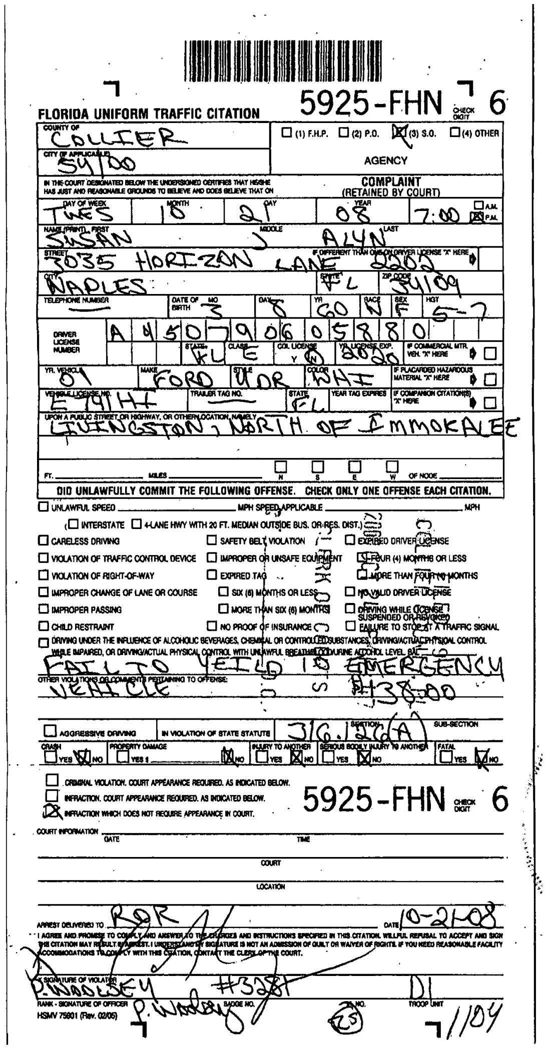
FAULTY EQUIPMENT AFFIDAVIT OF COMPLIANCE
(Law Enforcement Use Only)

I certify that the defective equipment described herein has been corrected and complies with the requirements of the Florida traffic laws.
Date: ______________________ ASSIGNED DHSMV AGENCY #______________

Signed: ____
(Name, Title, ID#)

Source: https://www.flhsmv.gov/courts-enforcement/utc/forms-and-resources/.

Figure F-2: Example of completed UTC form


FLORIDA UNIFORM TRAFFIC CITATION 5925-FHN CHECK DIGIT 6

COUNTY OF COLLIER

☐ (1) F.H.P. ☐ (2) P.D. ☒ (3) S.O. ☐ (4) OTHER

CITY OF APPLICABLE 34100

AGENCY

IN THIS COURT DESIGNATED BELOW THE UNDERSIGNED CERTIFIES THAT HE/SHE HAS JUST AND REASONABLE GROUNDS TO BELIEVE AND DOES BELIEVE THAT ON

COMPLAINT (RETAINED BY COURT)

DAY OF WEEK TUES MONTH 10 DAY 21 YEAR 08 7:00 ☐ A.M. ☒ P.M.

NAME (PRINT) FIRST SUSAN MIDDLE J LAST ALYN

STREET 3035 HORIZON LANE

CITY NAPLES STATE FL ZIP CODE 34109

TELEPHONE NUMBER DATE OF BIRTH MO 3 DAY 8 YR 60 RACE W SEX F HGT 5-7

DRIVER LICENSE NUMBER A 450 790 06 0580

STATE FL CLASS E CDL LICENSE Y N YR LICENSE EXP. 2010

YR. VEHICLE 01 MAKE FORD STYLE 4DR COLOR WHI

VEHICLE LICENSE NO. E 791 HT TRAILER TAG NO. STATE FL YEAR TAG EXPIRES

UPON A PUBLIC STREET OR HIGHWAY, OR OTHER LOCATION, NAMELY LIVINGSTON, NORTH OF IMMOKALEE

DID UNLAWFULLY COMMIT THE FOLLOWING OFFENSE. CHECK ONLY ONE OFFENSE EACH CITATION.

☐ UNLAWFUL SPEED MPH SPEED APPLICABLE MPH

(☐ INTERSTATE ☐ 4-LANE HWY WITH 20 FT. MEDIAN OUTSIDE BUS. OR RES. DIST.)

☐ CARELESS DRIVING ☐ SAFETY BELT VIOLATION ☐ EXPIRED DRIVER LICENSE

☐ VIOLATION OF TRAFFIC CONTROL DEVICE ☐ IMPROPER OR UNSAFE EQUIPMENT ☐ FOUR (4) MONTHS OR LESS

☐ VIOLATION OF RIGHT-OF-WAY ☐ EXPIRED TAG ☐ MORE THAN FOUR (4) MONTHS

☐ IMPROPER CHANGE OF LANE OR COURSE ☐ SIX (6) MONTHS OR LESS ☐ NO VALID DRIVER LICENSE

☐ IMPROPER PASSING ☐ MORE THAN SIX (6) MONTHS ☐ DRIVING WHILE LICENSE SUSPENDED OR REVOKED

☐ CHILD RESTRAINT ☐ NO PROOF OF INSURANCE ☐ FAILURE TO STOP AT A TRAFFIC SIGNAL

☐ DRIVING UNDER THE INFLUENCE OF ALCOHOLIC BEVERAGES, CHEMICAL OR CONTROLLED SUBSTANCES, DRIVING/ACTUAL PHYSICAL CONTROL WHILE IMPAIRED, OR DRIVING/ACTUAL PHYSICAL CONTROL WITH UNLAWFUL BREATH/BLOOD ALCOHOL LEVEL BAL

FAIL TO YEILD TO EMERGENCY

OTHER VIOLATIONS OR COMMENTS PERTAINING TO OFFENSE: VEHICLE $138.00

☐ AGGRESSIVE DRIVING IN VIOLATION OF STATE STATUTE 316.126(A) SECTION SUB-SECTION

CRASH ☐ YES ☒ NO PROPERTY DAMAGE ☐ YES $ ☒ NO INJURY TO ANOTHER ☐ YES ☒ NO SERIOUS BODILY INJURY TO ANOTHER ☐ YES ☒ NO FATAL ☐ YES ☒ NO

☐ CRIMINAL VIOLATION. COURT APPEARANCE REQUIRED. AS INDICATED BELOW.

☐ INFRACTION. COURT APPEARANCE REQUIRED. AS INDICATED BELOW.

☒ INFRACTION WHICH DOES NOT REQUIRE APPEARANCE IN COURT.

5925-FHN CHECK DIGIT 6

COURT INFORMATION DATE TIME

COURT

LOCATION

ARREST DELIVERED TO ROR DATE 10-21-08

I AGREE AND PROMISE TO COMPLY AND ANSWER TO THE CHARGES AND INSTRUCTIONS SPECIFIED IN THIS CITATION. WILLFUL REFUSAL TO ACCEPT AND SIGN THE CITATION MAY RESULT IN ARREST. I UNDERSTAND MY SIGNATURE IS NOT AN ADMISSION OF GUILT OR WAIVER OF RIGHTS. IF YOU NEED REASONABLE FACILITY ACCOMMODATIONS TO COMPLY WITH THIS CITATION, CONTACT THE CLERK OF THE COURT.

SIGNATURE OF VIOLATOR

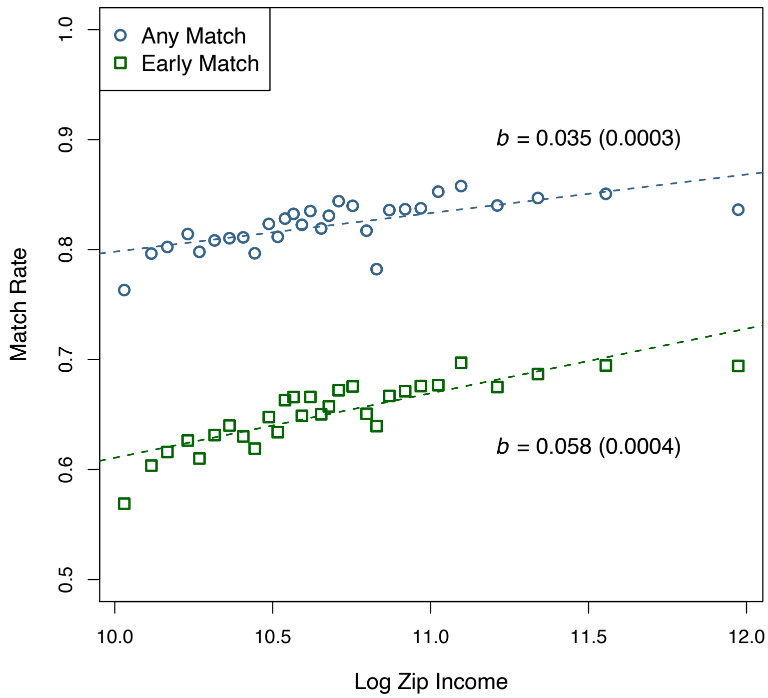
P. WOOLSEY #3281 D1

RANK - SIGNATURE OF OFFICER BADGE NO. ID NO. TROOP UNIT

HSMV 75901 (Rev. 02/05)

Source: https://www.muckrock.com/foi/collier-county-35/bogus-traffic-ticket-collier-county-19486/.

Figure F-3: Credit file match rate by zip code income



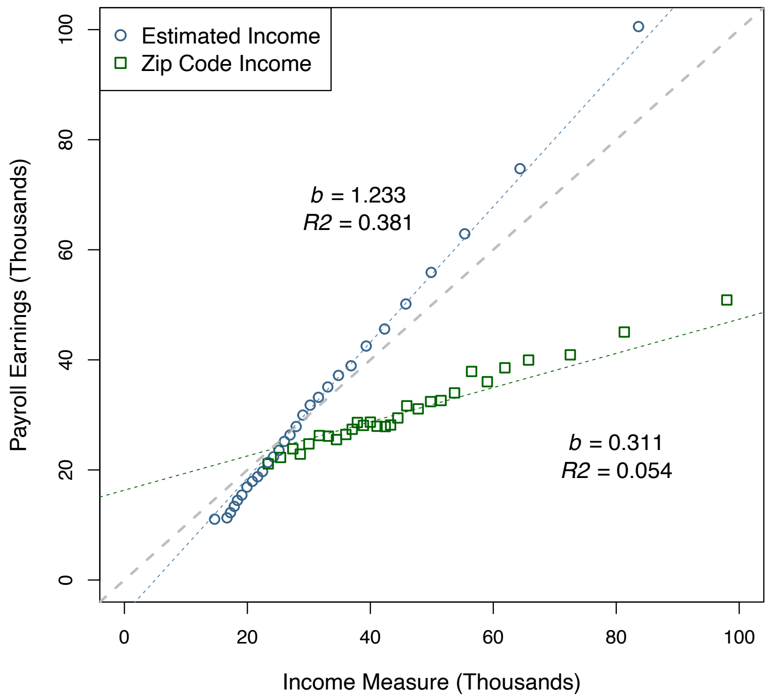
*N*otes: This figure plots the share of citations successfully matched to the credit file in each quantile bin of log zip code income. Blue circles (*any*) indicate whether the citation was matched at all. Green squares (*early*) indicate whether the citation was matched to a driver present on the credit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

Table F-1: Credit file match rate by driver characteristics

	Any Match		Early Match	
	(1)	(2)	(3)	(4)
Female	0.0440	0.0432	0.0644	0.0642
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Age <18	-0.0698	-0.0690	-0.4701	-0.4712
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Age 25-34	0.0286	0.0281	0.0718	0.0709
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Age 35-44	0.0372	0.0369	0.0950	0.0954
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Age 45-54	0.0516	0.0521	0.1222	0.1266
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Age 55+	-0.2236	-0.2214	-0.7080	-0.7062
	(0.0016)	(0.0016)	(0.0005)	(0.0006)
Race = Black	-0.0170	-0.0199	-0.0327	-0.0338
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Race = Hispanic	-0.0277	-0.0351	-0.0657	-0.0692
	(0.0003)	(0.0004)	(0.0004)	(0.0004)
Race = Other	0.0020	-0.0065	0.0031	-0.0263
	(0.0004)	(0.0004)	(0.0004)	(0.0005)
Log Zip Income	0.0246	0.0301	0.0316	0.0357
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Mean	0.823	0.823	0.652	0.652
County FE	No	Yes	No	Yes
Time FE	No	Yes	No	Yes
R2	0.022	0.026	0.245	0.259
N	8851688	8851688	8851688	8851688

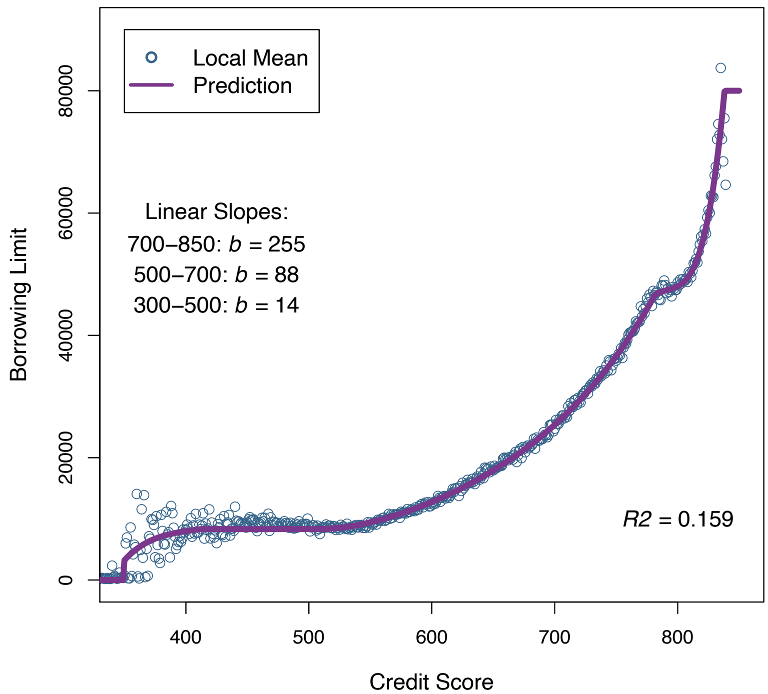
*N*otes: This table presents regressions estimated at the citation level. *Any Match* refers to whether the driver was matched to the credit file at any point. *Early Match* refers to whether the driver was matched and on the credit file as of January 2010. Ages 18-24 and white are the excluded age/race 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 F-4: Income measures



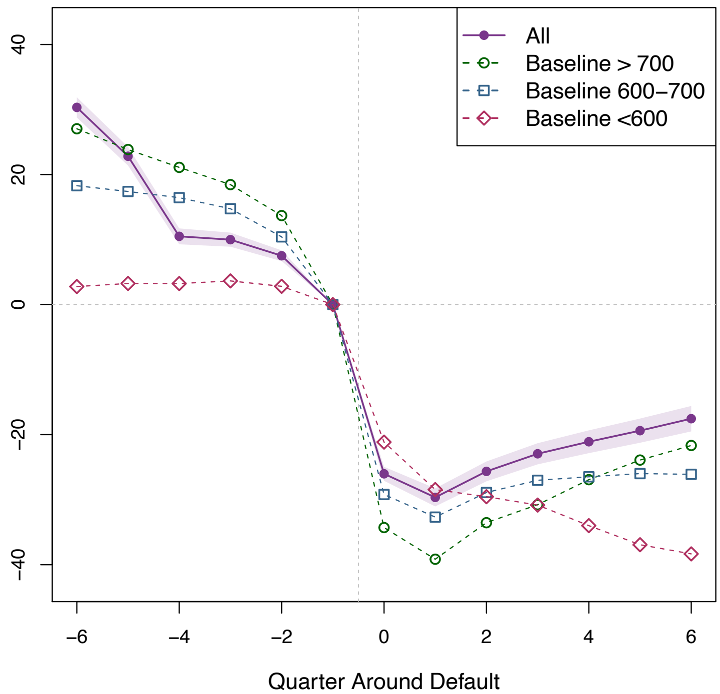
*N*otes: This figure illustrates the relationship between income measures using the subsample with positive payroll earnings at some point in 2010 ($N = 390,688$). The regression of payroll earnings on both income measures gives $R^2 = 0.388$ with coefficients on credit bureau estimated income and zip code income of 1.191 (0.004) and 0.112 (0.002), respectively.

Figure F-5: Credit scores and borrowing limits



*N*otes: This figure illustrates the cross-sectional relationship between credit scores and revolving credit limits at baseline using the subset of the initial sample with an open revolving account ($N = 1,623,184$). Local means correspond to each integer value of the credit score and the solid line illustrates fitted values used for imputation, described in the data appendix. Reported R^2 is from a regression of true borrowing limits on predicted borrowing limits using the fitted values. Regression coefficients are linear slopes over the denoted ranges.

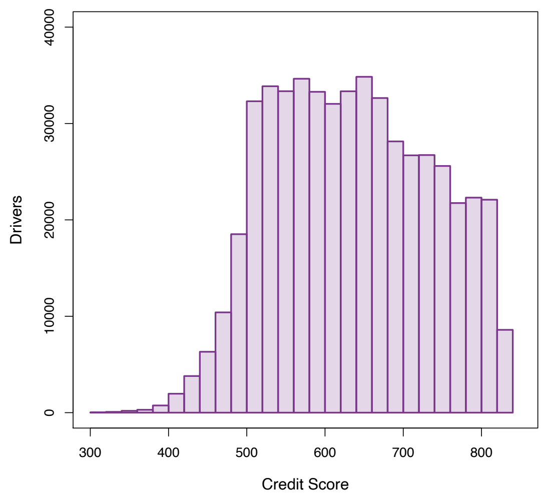
Figure F-6: Default and credit scoring



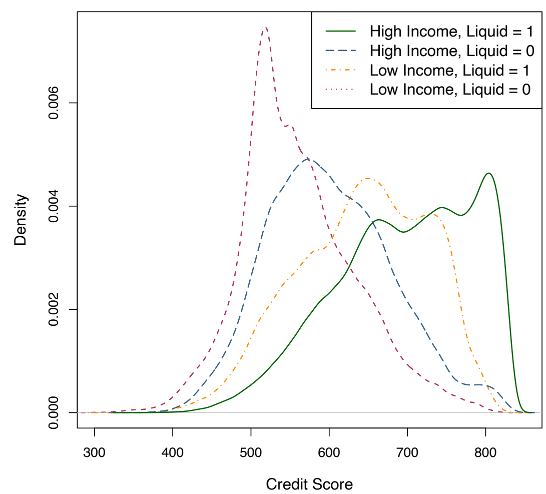
*N*otes: This figure reports event study estimates for credit scores around the time of first default incident (new collection, new delinquency, or new derogatory flag) observed in the data, using only the final cohort of the event study sample ($N = 22,006$), for the full sample as well as broken down by baseline credit scores.

Figure F-7: Distribution of credit scores in event study sample

(a) Histogram

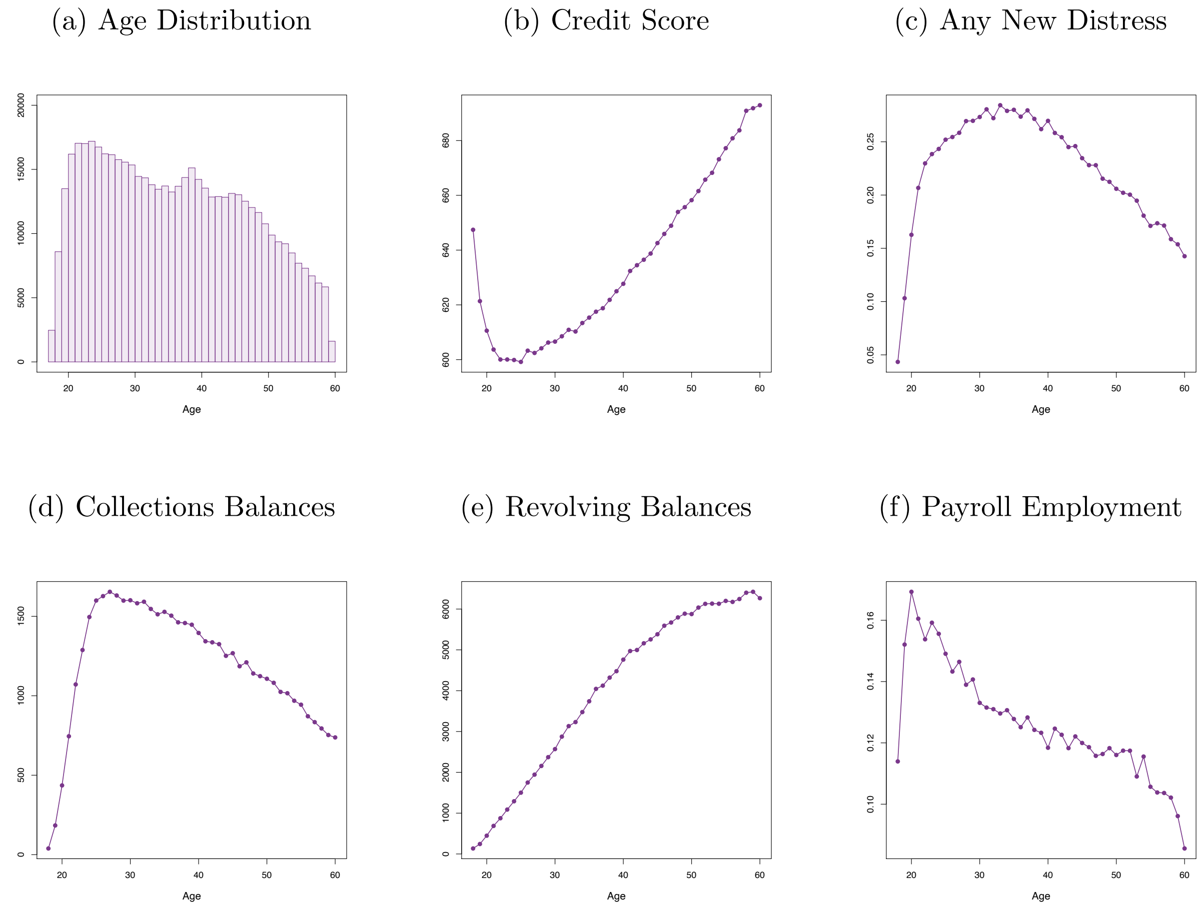


(b) Density by Driver Groups



*N*otes: Panel (a) plots the distribution of credit scores in the event study sample as of one year prior to each individual's traffic stop. Panel (b) illustrates kernel density plots of these credit scores broken down by baseline estimated income and baseline liquidity status.

Figure F-8: Age profiles in outcomes of interest



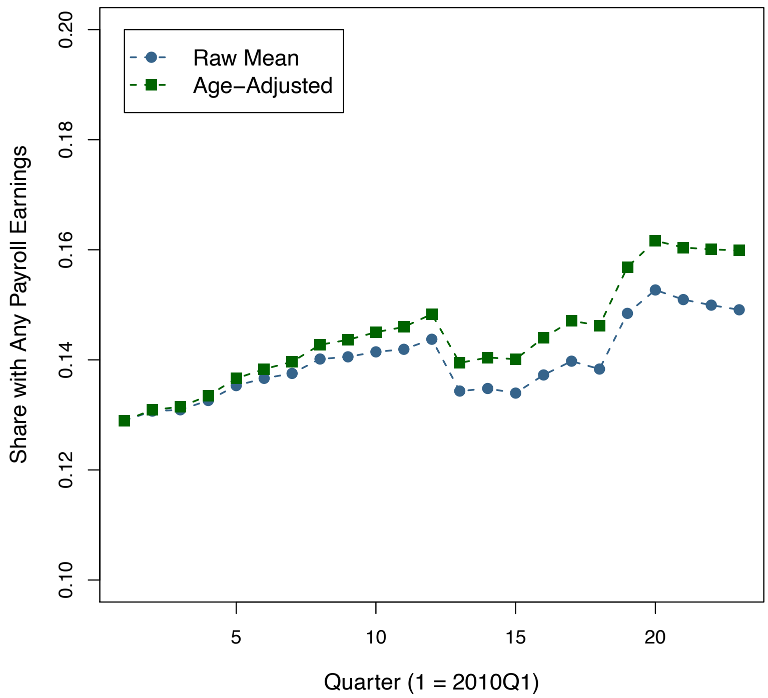
*N*otes: Panel (a) plots the distribution of ages in the event study sample as of 2010Q1. Panels (b)-(f) report average outcomes by age as of 2010Q1 for the event study sample.

Table F-2: Predictors of payroll employment

	Baseline		$\tau = -1$	
	(1)	(2)	(3)	(4)
Age (10s)	-0.0412	-0.0419	-0.0551	-0.0554
	(0.0031)	(0.0031)	(0.0033)	(0.0033)
Age Squared	0.0033	0.0035	0.0043	0.0045
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Female	0.0076	0.0071	0.0073	0.0066
	(0.0009)	(0.0009)	(0.0010)	(0.0010)
Race = Minority	0.0251	0.0230	0.0274	0.0271
	(0.0010)	(0.0011)	(0.0011)	(0.0011)
Log Zip Income	-0.0149	-0.0136	-0.0175	-0.0156
	(0.0011)	(0.0012)	(0.0012)	(0.0012)
Credit Score (100s)	0.0035	0.0031	0.0023	0.0021
	(0.0005)	(0.0005)	(0.0006)	(0.0006)
Credit Lines	0.0015	0.0015	0.0017	0.0018
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Any Mortgage	0.0166	0.0156	0.0180	0.0167
	(0.0012)	(0.0012)	(0.0012)	(0.0012)
Any Auto Loan	0.0181	0.0179	0.0178	0.0174
	(0.0010)	(0.0010)	(0.0010)	(0.0010)
Collections Balances ($1000s)	-0.0014	-0.0012	-0.0016	-0.0014
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Revolving Balances ($1000s)	-0.0004	-0.0004	-0.0006	-0.0006
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Mean	0.129	0.129	0.143	0.143
County FE	No	Yes	No	Yes
R2	0.006	0.01	0.009	0.014
N	525646	525646	525646	525646

Notes: This table reports results from regressions using the event study sample where the outcome is an indicator for whether the motorist has any payroll earnings. In columns (1)-(2), the regression is estimated at baseline (2010Q1) and in columns (3)-(4), the regression is estimates as of one quarter prior to the traffic stop. County FE are fixed effects for the motorist's county of residence at the time of the traffic stop. Robust standard errors in parentheses.

Figure F-9: Payroll data coverage over time



Notes: This figure plots the share of the event study sample employed in the payroll data over time, using only not-yet-treated observations to net out treatment effects. Blue circles report raw means for not-yet-treated observations and green squares report age-adjusted payroll employment rates obtained from a regression of a payroll employment indicator on age and time fixed effects, again using only not-yet-treated observations.

Table F-3: Summary Statistics at Baseline by Traffic Court Disposition

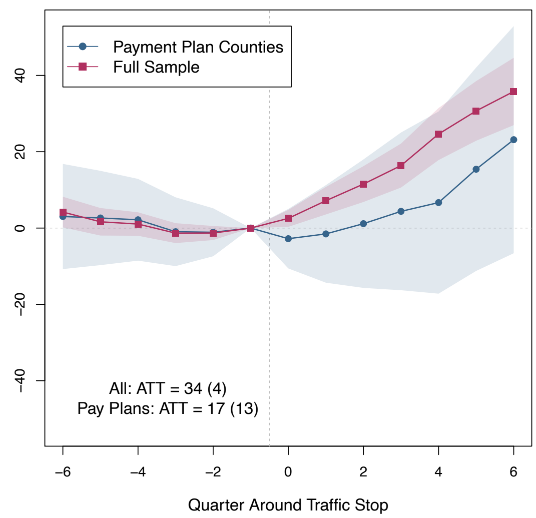
		Definitely Paid				
	(1)	(2)	(3)	(4)	(5)	(6)
	All	V=4/C	V=4	V=C	V=3/A	V=1/M
Panel A: Demographics						
Female	0.45	0.47	0.45	0.5	0.43	0.44
Race = White	0.59	0.63	0.62	0.64	0.51	0.57
Race = Black	0.2	0.18	0.2	0.15	0.21	0.29
Race = Hispanic	0.22	0.19	0.18	0.21	0.28	0.14
Age	36.37	36.2	35.64	36.93	37.01	34.88
Credit File Age	13.2	13.25	13	13.58	13.24	12.69
Credit Score	624	625	610	645	628	602
Estimated Income	39524	38973	36529	42141	41456	35439
Zip Income	55023	53485	51978	55439	58234	52925
Panel B: Financial Distress						
Collections	2.24	2.37	2.82	1.78	1.85	2.96
Collections Balances	1299	1304	1539	1000	1210	1640
Delinquencies	1.99	1.9	2.05	1.7	2.1	2.15
Derogatories	1.43	1.37	1.49	1.21	1.52	1.57
Panel C: Credit Usage						
Any Revolving	0.73	0.72	0.67	0.79	0.77	0.64
Any Auto Loan	0.41	0.41	0.39	0.42	0.43	0.39
Any Mortgage	0.33	0.33	0.3	0.37	0.35	0.28
Revolving Balances	4950	4729	4144	5488	5592	3876
Revolving Limit	15367	14658	12372	17621	17591	11228
Panel D: Payroll Records						
Any Payroll Earnings	0.13	0.13	0.13	0.14	0.12	0.13
Monthly Earnings	3319	3276	3073	3513	3491	2958
Panel D: Citation Information						
Fine Amount	195.53	184.55	183.49	185.92	220.45	172.07
DL Points	3.39	3.33	3.32	3.34	3.52	3.26
Individuals	525646	309598	174766	134832	175051	40997

*N*otes: This table reports summary statistics as of 2010Q1 for subsets of the event study sample by traffic court disposition. Column 2 corresponds to those with disposition verdicts = $4/C$ (paid or traffic school), which is the *definitely* paid group. Columns 3 and 4 report means for these two subsets individually. Columns 5 and 6 report means for the possible lenience (verdict = $3/A$) and possible suspension (verdict = 1 or missing) subgroups.

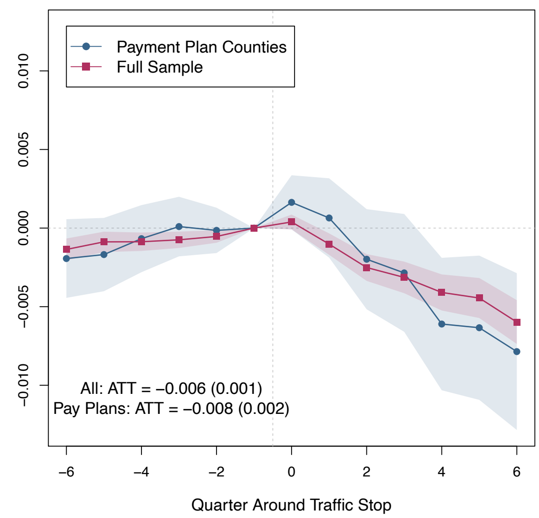
G Additional results: event studies

Figure G-1: Event study estimates for counties with available payment plans

(a) Collections Balances



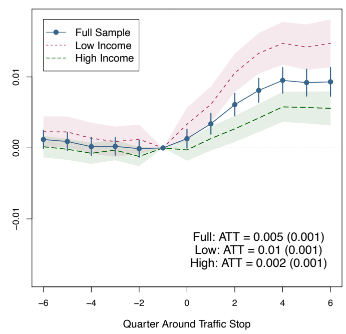
(b) Payroll Employment



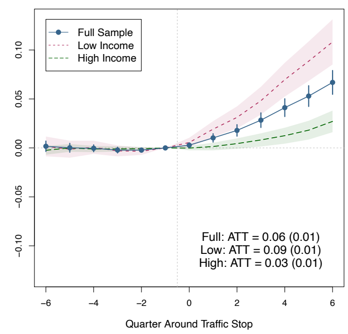
*N*otes: This figure reports event study estimates for the full sample and for the subset of motorists cited in Pinellas and Hillsborough counties ($N = 43,729$), which offered three month payment plans on traffic fines during the sample period.)

Figure G-2: Event study estimates for distress outcomes by baseline income

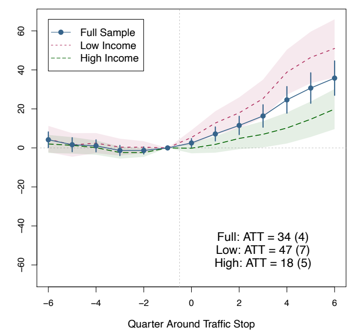
(a) Any New Distress



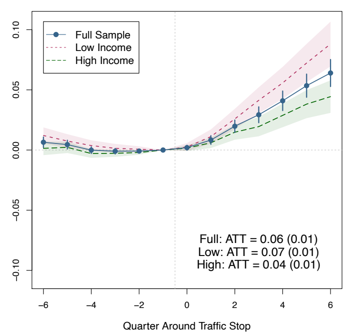
(b) Collections



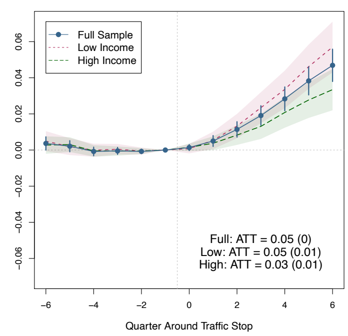
(c) Collections Balances



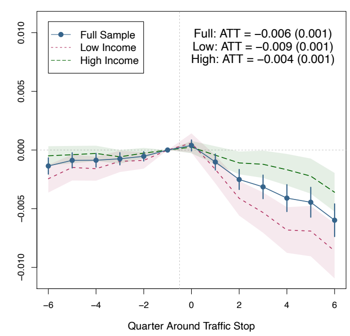
(d) Delinquencies



(e) Derogatories



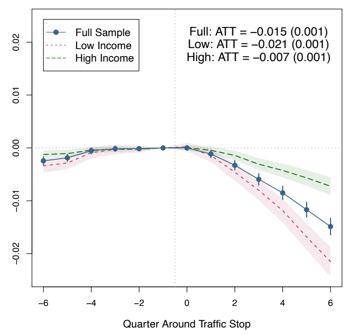
(f) Payroll Employment



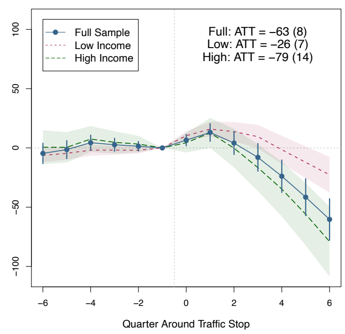
*N*otes: Each panel reports event study estimates for the full sample as well as estimates from separate event studies for motorists with above ($N = 288,276$) and below ($N = 237,730$) median estimated income at baseline.

Figure G-3: Event study estimates for credit card outcomes by baseline income

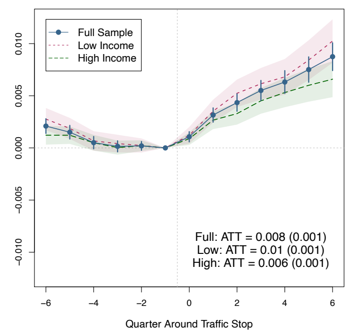
(a) Any Revolving Account



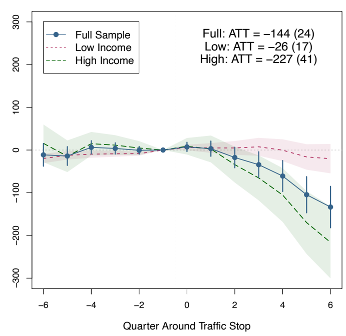
(b) Revolving Balances



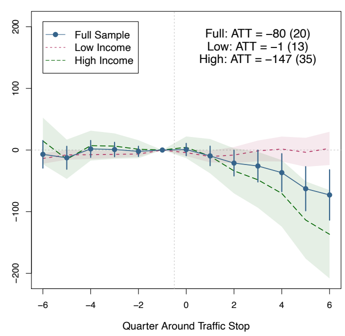
(c) Revolving Utilization



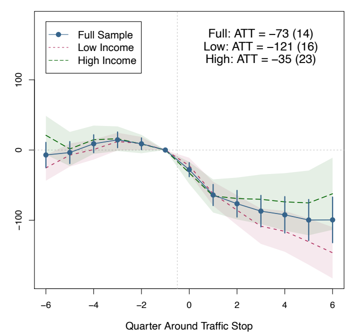
(d) Revolving Limits



(e) Available Balances

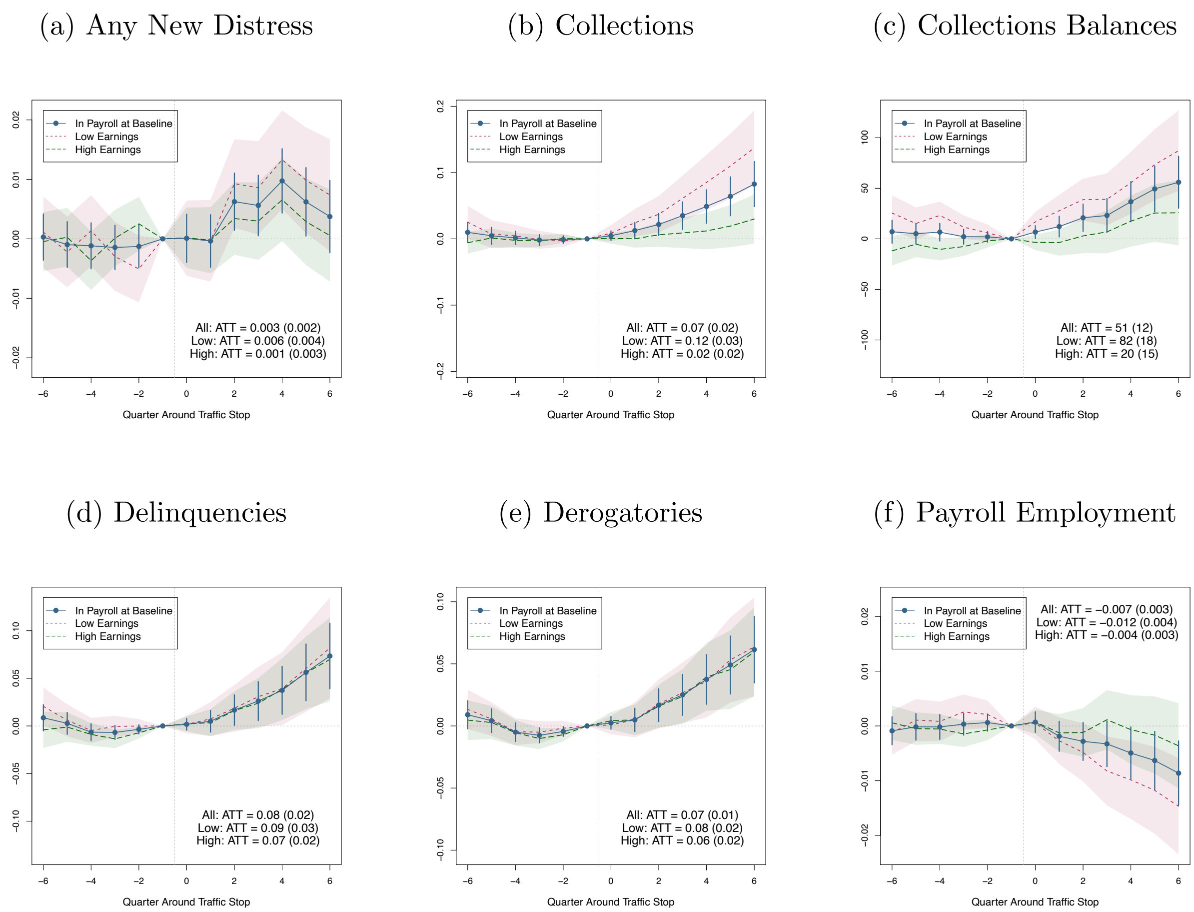


(f) Imputed Limits



*N*otes: Each panel reports event study estimates for the full sample as well as estimates from separate event studies for motorists with above ($N = 288,276$) and below ($N = 237,730$) median estimated income at baseline.

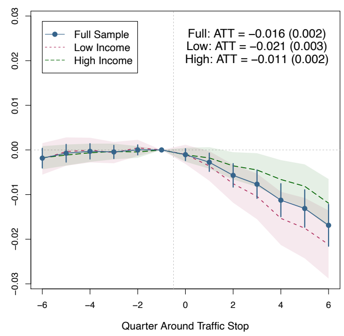
Figure G-4: Event study estimates for distress outcomes for subset in payroll records at baseline



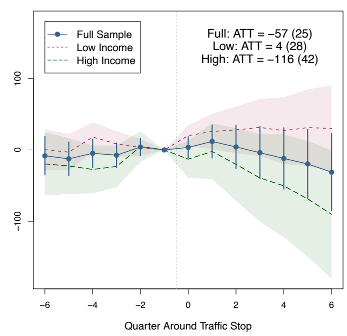
*N*otes: Each panel reports event study estimates for the full sample of motorists who are in the payroll records at baseline ($N = 55,140$) as well as estimates from separate event studies for motorists who are in the payroll records at baseline and have above ($N = 27,570$) and below ($N = 27,570$) median payroll earnings.

Figure G-5: Event study estimates for credit card outcomes for subset in payroll records at baseline

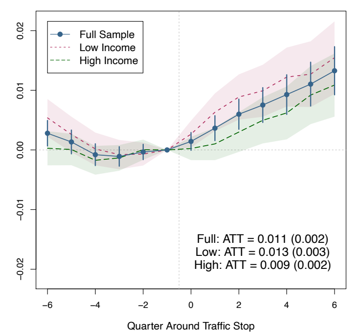
(a) Any Revolving Account



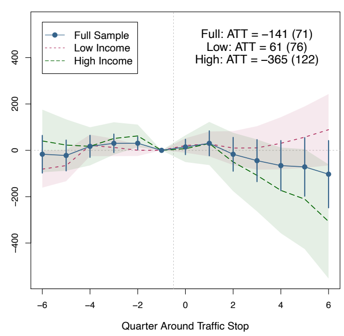
(b) Revolving Balances



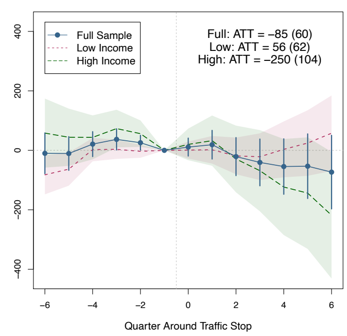
(c) Revolving Utilization



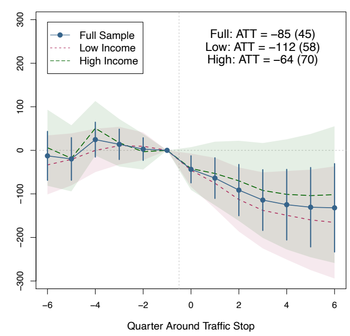
(d) Revolving Limits



(e) Available Balances



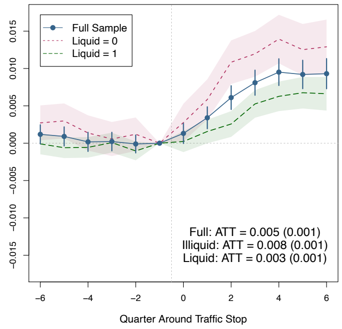
(f) Imputed Limits



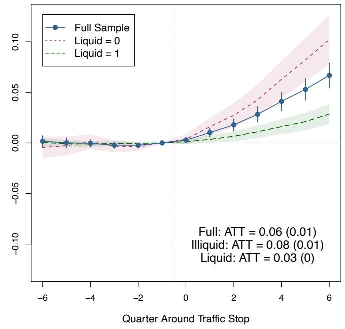
*N*otes: Each panel reports event study estimates for the full sample of motorists who are in the payroll records at baseline ($N = 55,140$) as well as estimates from separate event studies for motorists who are in the payroll records at baseline and have above ($N = 27,570$) and below ($N = 27,570$) median payroll earnings.

Figure G-6: Event study estimates for distress outcomes by baseline credit card liquidity

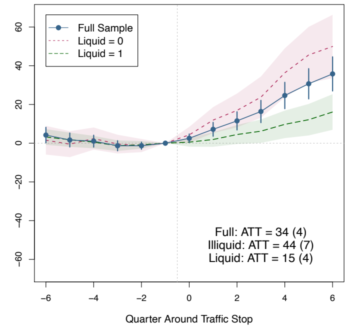
(a) Any New Distress



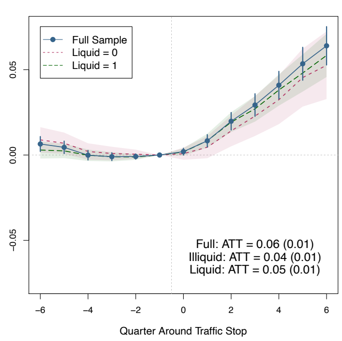
(b) Collections



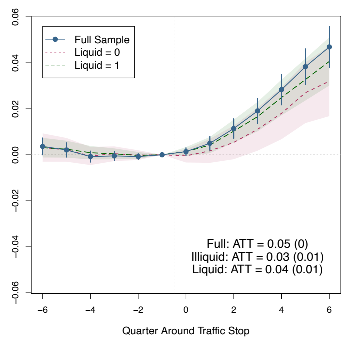
(c) Collections Balances



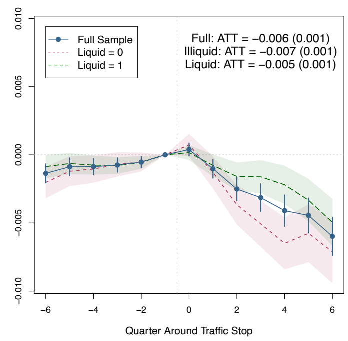
(d) Delinquencies



(e) Derogatories



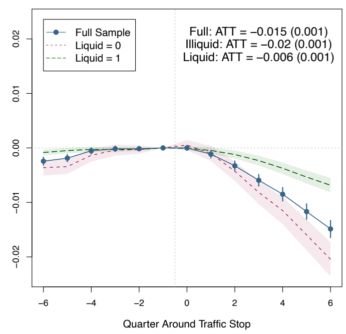
(f) Payroll Employment



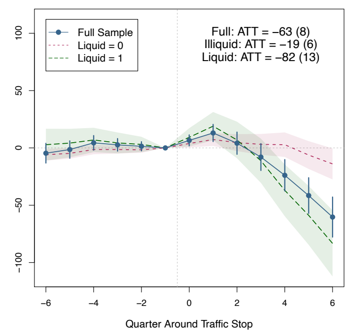
*N*otes: Each panel reports event study estimates for the full sample as well as estimates from separate event studies for subgroups based on baseline credit card liquidity. *Liquid*= 1 is the subset of individuals with at least \$200 in available credit card borrowing at baseline ($N = 301,318$) and *Liquid*= 0 is the subset of individuals with less than \$200 available at baseline, which includes those with no open credit cards at baseline ($N = 224,328$).

Figure G-7: Event study estimates for credit card outcomes by baseline credit card liquidity

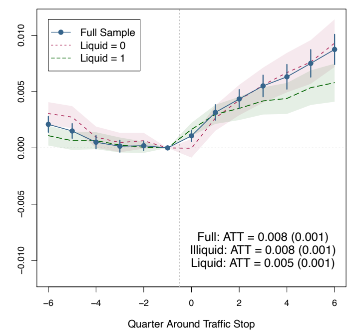
(a) Any Revolving Account



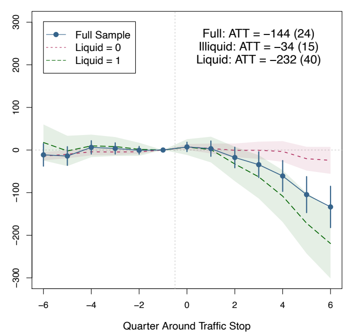
(b) Revolving Balances



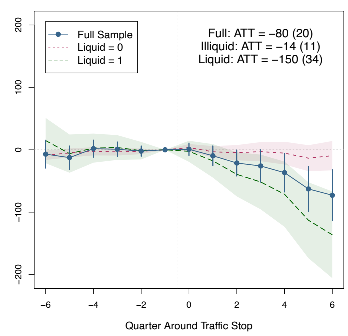
(c) Revolving Utilization



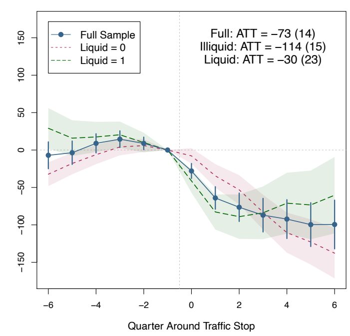
(d) Revolving Limits



(e) Available Balances

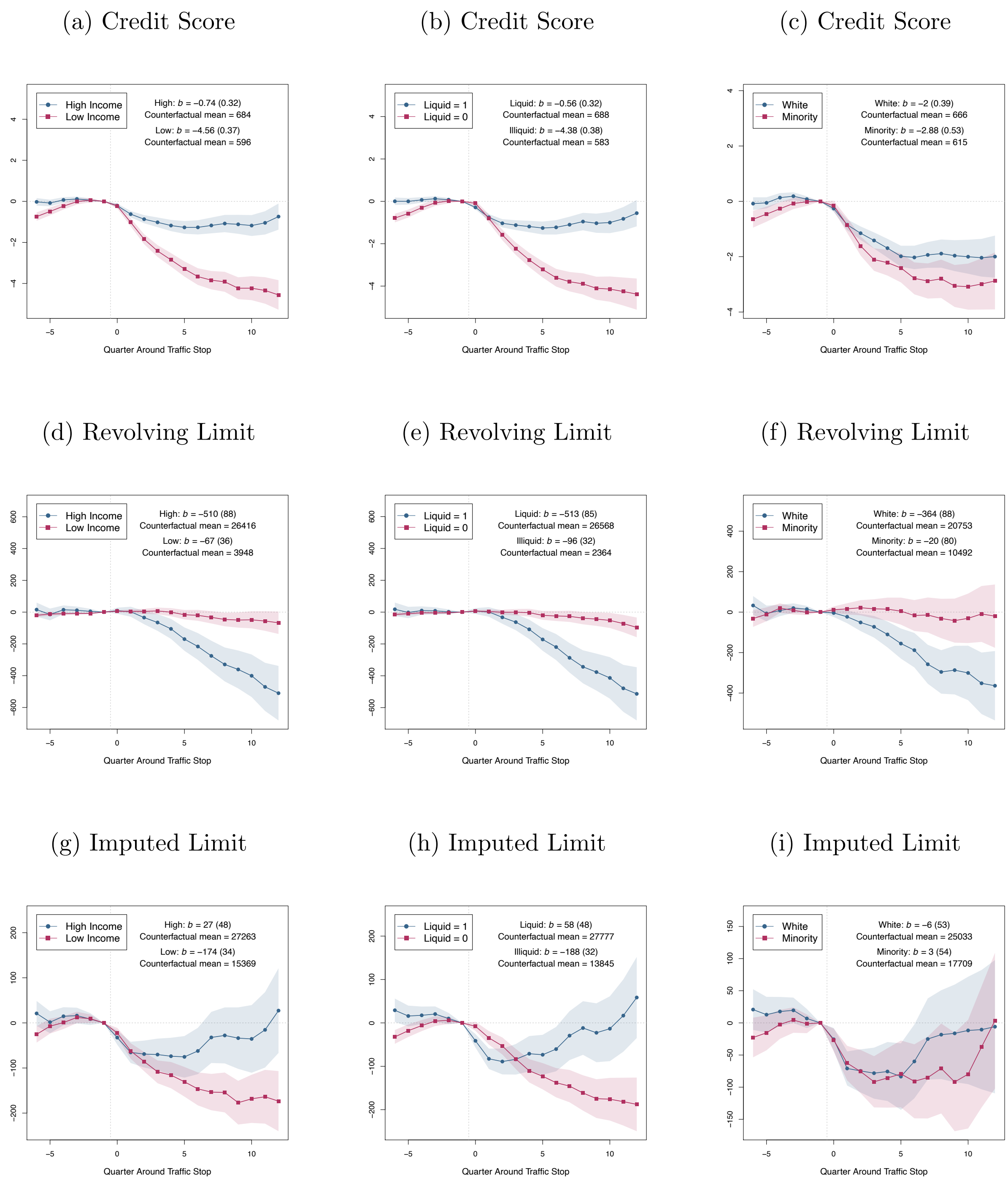


(f) Imputed Limits



*N*otes: Each panel reports event study estimates for the full sample as well as estimates from separate event studies for subgroups based on baseline credit card liquidity. *Liquid*$=1$ is the subset of individuals with at least \$200 in available credit card borrowing at baseline ($N = 301,318$) and *Liquid*$=0$ is the subset of individuals with less than \$200 available at baseline, which includes those with no open credit cards at baseline ($N = 224,328$).

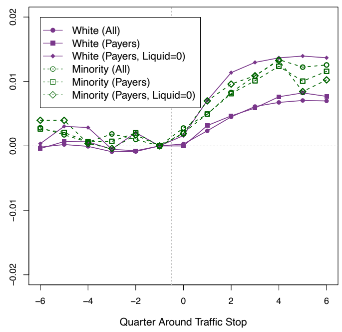
Figure G-8: Heterogeneity for long-run outcomes



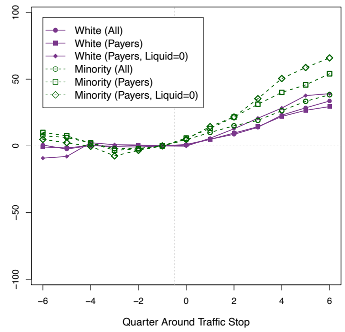
*N*otes: This figure reports heterogeneity in the estimates for longer-run outcomes by baseline estimated income, baseline credit card situation, and by motorist race.

Figure G-9: Event study estimates by race for subgroups

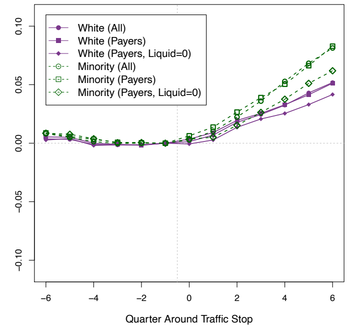
(a) Any New Default



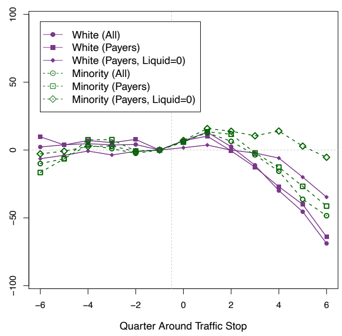
(b) Collections Balances



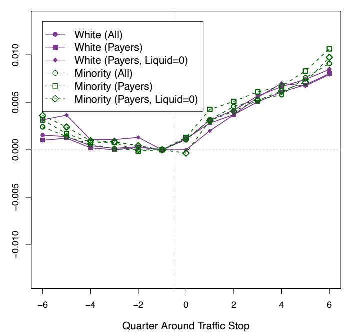
(c) Delinquencies



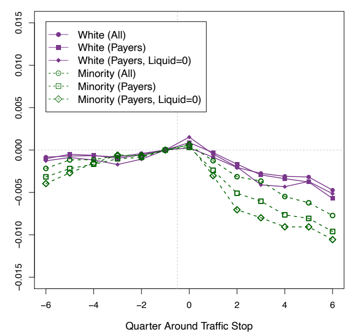
(d) Revolving Balances



(e) Revolving Utilization



(f) Payroll Employment



*N*otes: Same as figure E-1 except additionally showing results for the full sample (i.e., those with any court disposition) (N white $= N = 308,116$; N Black or Hispanic $= 217,530$) and for the subset who both pay their fines and with less than \$200 in available balances on credit cards (N white $= 105,529$; N Black or Hispanic $= 118,799$).

Table G-1: Event study estimates for credit card outcomes

	(1) Any Card	(2) Balances	(3) Limits	(4) Utilization
Event Study Estimates				
$\tau = 1$	-0.0011	13.04	3.19	0.0032
	(0.0003)	(3.82)	(9.18)	(0.0003)
$\tau = 4$	-0.0085	-23.88	-60.91	0.0063
	(0.0006)	(6.98)	(18.49)	(0.0006)
$\tau = 6$	-0.0149	-60.3	-133.5	0.0087
	(0.0008)	(8.92)	(24.77)	(0.0007)
ATT	-0.0148	-63.26	-143.53	0.0083
	(0.0007)	(7.99)	(23.55)	(0.0006)
Counterfactual Means				
$\tau = 1$	0.74	3736	15304	0.58
$\tau = 6$	0.75	3800	15588	0.57
Tests for Parallel Trends				
	$p = 0.136$	$p = 0.393$	$p = 0.742$	$p = 0.367$

*N*otes: This table reports event study estimates for one, four, and six quarters post traffic stop, as well as the static ATT estimate, all obtained via the Callaway & Sant'Anna (2021) approach. Design-based standard errors from Roth & Sant'Anna (2022) in parentheses. The lower panels report estimated counterfactual means for $\tau = 1$ and $\tau = 6$, estimated using the method described in the text, and results of the pretrends test from Borusyak et al. (2022). The sample is the full event study sample ($N = 525,646$) and the average fine is $195.53.

Table G-2: Event study estimates by baseline income and liquidity

	(1) Any New	(2) Collections	(3) Delinquencies	(4) Card Balances	(5) Card Utilization	(6) Payroll
Liquid = 0						
$\tau = 1$	0.006	11.94	0	7.32	0.003	-0.0013
	(0.001)	(3.42)	(0.003)	(2.853)	(0.001)	(0.0006)
$\tau = 6$	0.013	49.95	0.05	-13.95	0.009	-0.0071
	(0.001)	(3.42)	(0.003)	(2.853)	(0.001)	(0.0006)
ATT	0.008	43.88	0.04	-19.23	0.008	-0.0072
	(0.001)	(7.42)	(0.01)	(6.223)	(0.001)	(0.001)
μ	0.32	2334	3.17	822	0.82	0.15
Pretrends	$p = 0.759$	$p = 0.923$	$p = 0.83$	$p = 0.004$	$p < 0.001$	$p = 0.447$
Liquid = 1						
$\tau = 1$	0.002	1.91	0.01	18.96	0.003	-0.0008
	(0.001)	(1.93)	(0.002)	(6.313)	(0)	(0.0004)
$\tau = 6$	0.007	16.11	0.06	-83.28	0.006	-0.0049
	(0.001)	(1.93)	(0.002)	(6.313)	(0)	(0.0004)
ATT	0.003	14.84	0.05	-81.94	0.005	-0.0049
	(0.001)	(4.19)	(0.006)	(13.306)	(0.001)	(0.0008)
μ	0.14	718	1.74	5997	0.39	0.15
Pretrends	$p = 0.372$	$p = 0.244$	$p = 0.837$	$p = 0.389$	$p = 0.938$	$p = 0.098$
Low Income						
$\tau = 1$	0.006	12.75	0.01	15.87	0.004	-0.0016
	(0.001)	(3.1)	(0.003)	(3.172)	(0.001)	(0.0006)
$\tau = 6$	0.015	51	0.09	-22.63	0.01	-0.0086
	(0.001)	(3.1)	(0.003)	(3.172)	(0.001)	(0.0006)
ATT	0.01	47.4	0.07	-25.51	0.01	-0.0086
	(0.001)	(6.8)	(0.009)	(6.769)	(0.001)	(0.001)
μ	0.29	2098	2.68	1261	0.74	0.16
Pretrends	$p = 0.621$	$p = 0.818$	$p = 0.959$	$p = 0.002$	$p = 0.46$	$p = 0.067$
High Income						
$\tau = 1$	0.001	1.8	0.01	12.68	0.003	-0.0004
	(0.001)	(2.16)	(0.002)	(6.455)	(0)	(0.0004)
$\tau = 6$	0.006	19.91	0.04	-78.99	0.007	-0.0036
	(0.001)	(2.16)	(0.002)	(6.455)	(0)	(0.0004)
ATT	0.002	18.23	0.04	-78.85	0.006	-0.0036
	(0.001)	(4.68)	(0.007)	(13.602)	(0.001)	(0.0007)
μ	0.15	824	2.05	5886	0.43	0.14
Pretrends	$p = 0.364$	$p = 0.178$	$p = 0.412$	$p = 0.365$	$p = 0.507$	$p = 0.197$

Notes: Same as tables 2 and G-1, broken down by motorist credit card situation at baseline (top two panels) and motorist estimated income at baseline (bottom two panels).

Table G-3: Event study estimates by baseline income and liquidity

	(1) Any New	(2) Collections	(3) Delinquencies	(4) Card Balances	(5) Card Utilization	(6) Payroll
Low Income, Liquid $= 0$						
$\tau = 1$	0.007	13.35	0.01	8.49	0.002	-0.0015
	(0.002)	(3.97)	(0.004)	(2.69)	(0.001)	(0.0007)
$\tau = 6$	0.016	56.86	0.07	-10.56	0.009	-0.008
	(0.002)	(3.97)	(0.004)	(2.69)	(0.001)	(0.0007)
ATT	0.011	48.62	0.06	-19.37	0.008	-0.0082
	(0.002)	(8.58)	(0.011)	(5.894)	(0.001)	(0.0012)
μ	0.33	2503	2.94	613	0.84	0.15
Pretrends	$p = 0.34$	$p = 0.995$	$p = 0.857$	$p = 0.087$	$p < 0.001$	$p = 0.615$
Low Income, Liquid $= 1$						
$\tau = 1$	0.004	7.79	0.01	30.1	0.006	-0.0019
	(0.002)	(4.43)	(0.005)	(8.717)	(0.001)	(0.0011)
$\tau = 6$	0.012	17.23	0.1	-70.77	0.008	-0.0098
	(0.002)	(4.43)	(0.005)	(8.717)	(0.001)	(0.0011)
ATT	0.007	19.97	0.08	-62.67	0.007	-0.0092
	(0.002)	(9.99)	(0.015)	(18.698)	(0.002)	(0.0019)
μ	0.19	1174	2.11	2813	0.52	0.17
Pretrends	$p = 0.168$	$p = 0.146$	$p = 0.722$	$p = 0.224$	$p = 0.859$	$p = 0.036$
High Income, Liquid $= 0$						
$\tau = 1$	0.003	7.94	0	0.85	0.004	-0.0004
	(0.003)	(6.77)	(0.007)	(8.109)	(0.001)	(0.001)
$\tau = 6$	0.007	31.82	0.03	-37.17	0.011	-0.004
	(0.003)	(6.77)	(0.007)	(8.109)	(0.001)	(0.001)
ATT	0.003	32.47	0.04	-34.83	0.011	-0.0039
	(0.003)	(14.74)	(0.02)	(17.934)	(0.002)	(0.0018)
μ	0.28	1840	3.82	1447	0.75	0.13
Pretrends	$p = 0.553$	$p = 0.71$	$p = 0.997$	$p = 0.12$	$p = 0.316$	$p = 0.758$
High Income, Liquid $= 1$						
$\tau = 1$	0.001	0.19	0.01	15.93	0.002	-0.0004
	(0.001)	(2.13)	(0.002)	(7.767)	(0)	(0.0004)
$\tau = 6$	0.005	15.81	0.05	-86.09	0.005	-0.0035
	(0.001)	(2.13)	(0.002)	(7.767)	(0)	(0.0004)
ATT	0.002	13.39	0.04	-86.04	0.004	-0.0036
	(0.001)	(4.54)	(0.007)	(16.286)	(0.001)	(0.0008)
μ	0.12	583	1.63	6942	0.36	0.14
Pretrends	$p = 0.656$	$p = 0.346$	$p = 0.337$	$p = 0.418$	$p = 0.814$	$p = 0.142$

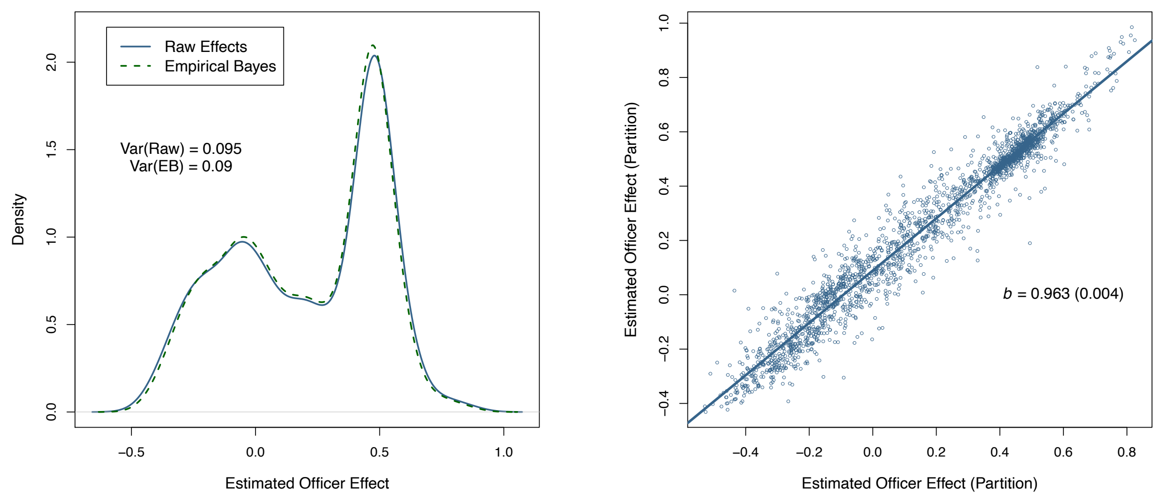
*N*otes: Same as tables 2 and G-1, broken down by the combination of motorist credit card situation at baseline and motorist estimated income at baseline. These estimates correspond to those plotted in figure 4.

H Additional results: Instrumental variables

Figure H-1: Evidence of officer behavior

(a) Distribution of Officer Effects

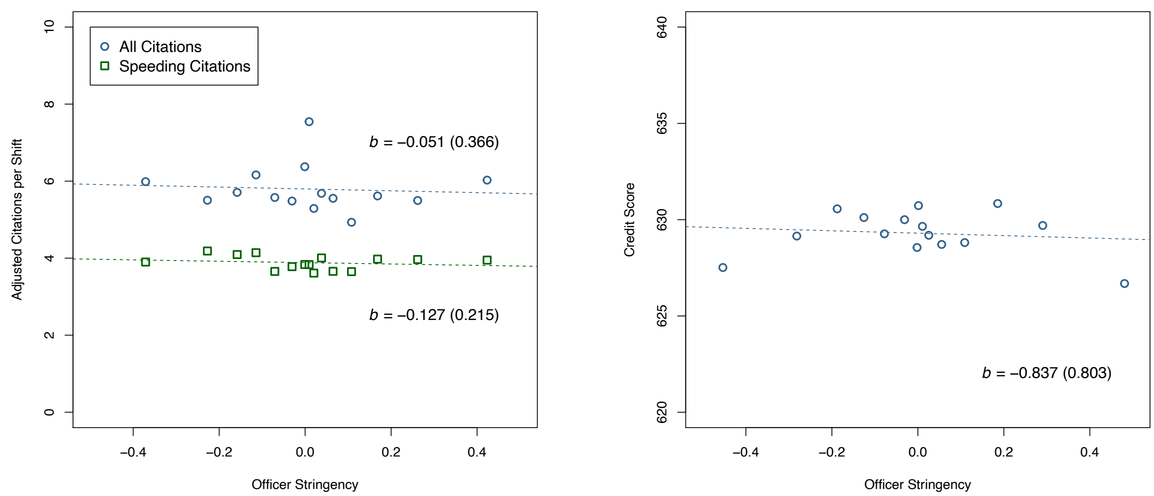
(b) Correlation of Officer Effects



*N*otes: Panel (a) plots the distribution of estimated officer fixed effects from a regression of **1**[harsh fine], where harsh fine indicates a charged speed > 9, on motorist covariates and beat-shift fixed effects. Solid blue line shows the distribution of raw estimated effects and dashed green line shows the distribution after applying empirical Bayes shrinkage. Panel (b) shows the correlation between officer effects estimated in two random partitions of the data.

Figure H-2: Instrument validity

(a) Ticketing Frequency (b) Credit Score



*N*otes: Panel (a) illustrates the relationship between the officer stringency instrument, residualized of beat-shift fixed effects and an officer's average number of citations per shift, adjusted for beat-shift effects. Panel (b) illustrates the relationship between the officer stringency instrument and the stopped motorist's credit score in the quarter prior to the stop, both residualized of beat-shift fixed effects.

Table H-1: Randomization test

	(1) 1[Harsh Fine]	(2) Stringency	(3) 1[Stringent]
Female	-0.024094240	-0.003927328	-0.003901145
	(0.002042597)	(0.001310563)	(0.001747316)
Age	-0.001522373	0.000928583	0.000711303
	(0.000556643)	(0.000373237)	(0.000467660)
Age Squared	0.000009005	-0.000012324	-0.000010421
	(0.000006526)	(0.000004369)	(0.000005478)
Minority	0.026224638	0.005691016	0.001744268
	(0.002760112)	(0.002009292)	(0.002600463)
Log Zip Income	0.004088861	0.000306912	-0.004300029
	(0.002918837)	(0.002463199)	(0.003805009)
County Resident	-0.010200266	-0.000608807	0.002252310
	(0.003390758)	(0.003023269)	(0.004072405)
Speeding Past Year	0.027481035	0.003004149	0.004030380
	(0.003105808)	(0.001680398)	(0.002169710)
Other Past Year	0.020618536	0.001886688	0.003126596
	(0.002215740)	(0.001323286)	(0.001867001)
Credit Score	-0.000050305	0.000001815	-0.000000260
	(0.000009803)	(0.000006746)	(0.000008515)
Any Auto Loan	-0.001412710	0.001369150	-0.000021948
	(0.001401109)	(0.000902545)	(0.001261897)
Collections Balance	0.000001222	-0.000000105	-0.000000246
	(0.000000323)	(0.000000202)	(0.000000269)
Revolving Balance	0.000000087	0.000000075	0.000000102
	(0.000000083)	(0.000000052)	(0.000000067)
Joint test	25.27	2.64	1.79
p-val: All	<0.001	0.002	0.044
p-val: Demographics	<0.001	0.001	0.039
p-val: Credit Bureau	<0.001	0.28	0.484

*N*otes: All regressions include beat-shift fixed effects. In column (1), the dependent variable is whether the driver is charged with a sped greater than 9 MPH over the posted limit. In columns (2) and (3), the dependant variable is the stringency instrument and an indicator for whether the citing officer is stringent (see data appendix for additional details). Credit bureau information is measured as of one quarter prior to the stop. Table footer reports the F-statistic and p-value from a joint test of all driver characteristics as well as for two subsets of driver characteristics (demographics and credit bureau information).

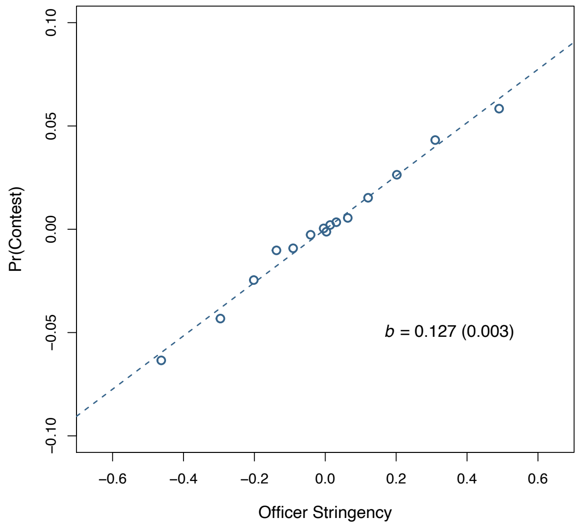
Table H-2: First stage estimates across subsamples

	Subgroup	
	(1) = 0	(2) = 1
Female	124.18	124.26
	(0.561)	(0.651)
Age > 35	123.99	124.26
	(0.62)	(0.583)
Minority	123.37	124.26
	(0.579)	(0.642)
Past Offense	124.29	124.26
	(0.514)	(0.868)
High Income	123.23	124.26
	(0.599)	(0.6)
High Credit Score	123.32	124.26
	(0.621)	(0.586)

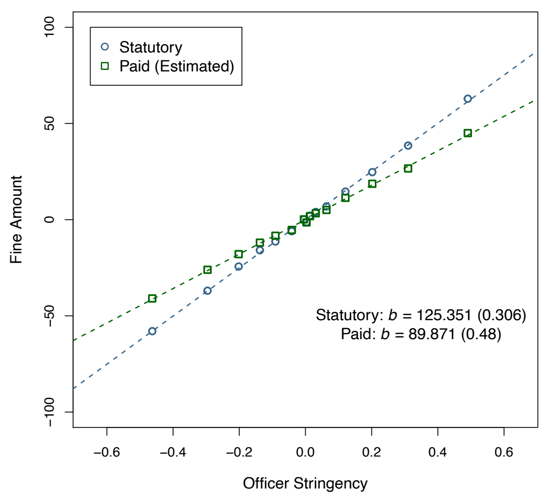
*N*otes: This table reports first stage estimates across subsamples. Each coefficient is from a separate regression of the fine amount on the stringency instrument and beat-shift effects using only the denoted subgroup of drivers, where the subgroups are the groups for which the denoted indicator variable = 0 (column 1) and = 1 (column 2). Standard errors clustered at the beat-shift level in parentheses.

Figure H-3: Officer stringency and citation outcomes

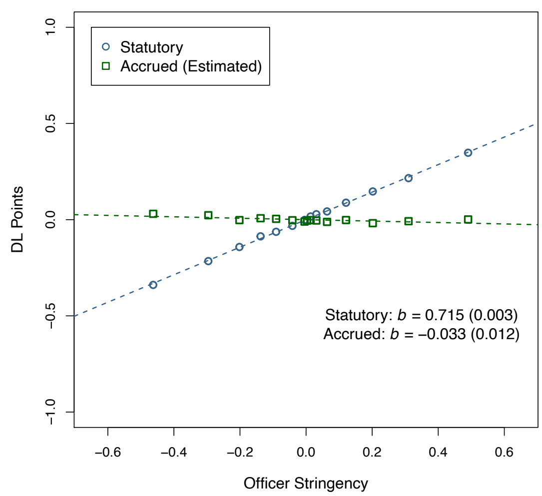
(a) Contested in Traffic Court



(b) Fine Amount



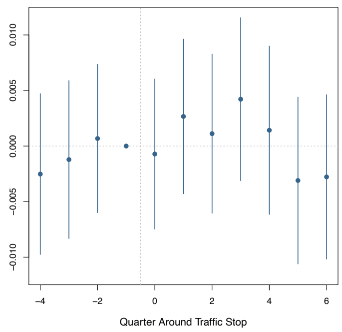
(c) DL Points



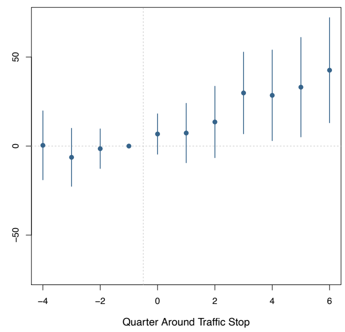
*N*otes: Each figure reports the relationship between citation outcome and the officer stringency instrument, both residualized of beat-shift fixed effects. Whether a citation is contested in court, as well as the paid fines and accrued points (as opposed to statutory) measures, are approximated based on disposition verdicts. See the data appendix for further details.

Figure H-4: Dynamic reduced form estimates

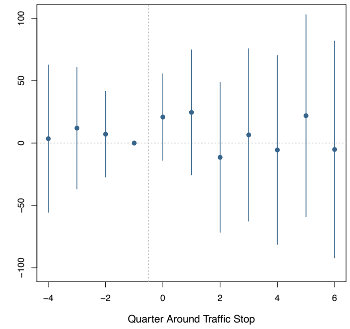
(a) Any New Distress



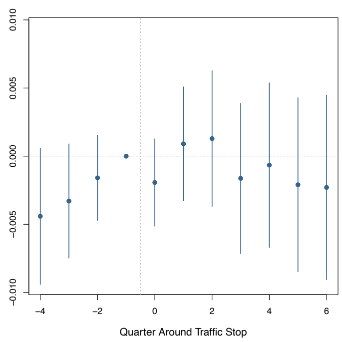
(b) Collections Balances



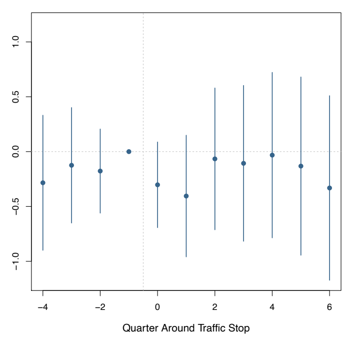
(c) Revolving Balances



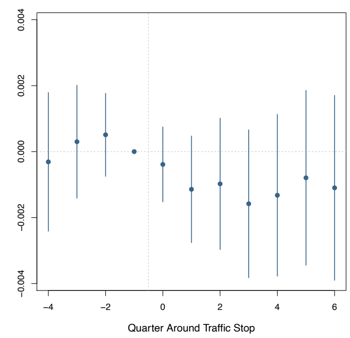
(d) Any Auto Loan



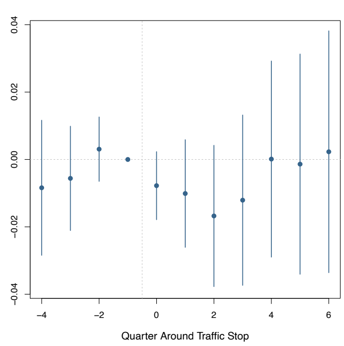
(e) Credit Score



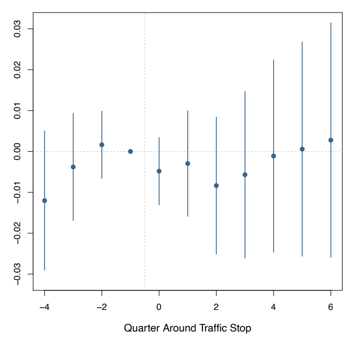
(f) Payroll Employment



(g) Delinquencies

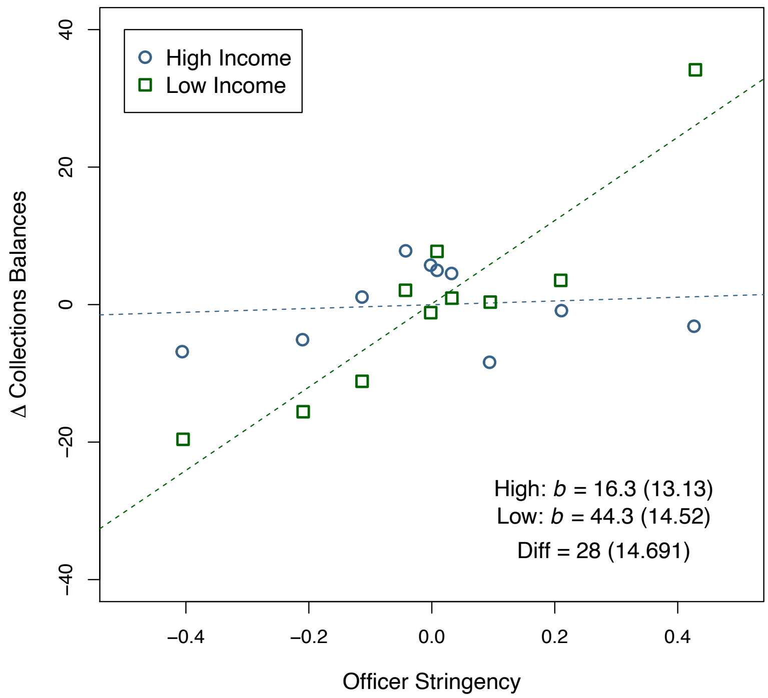


(h) Derogatories



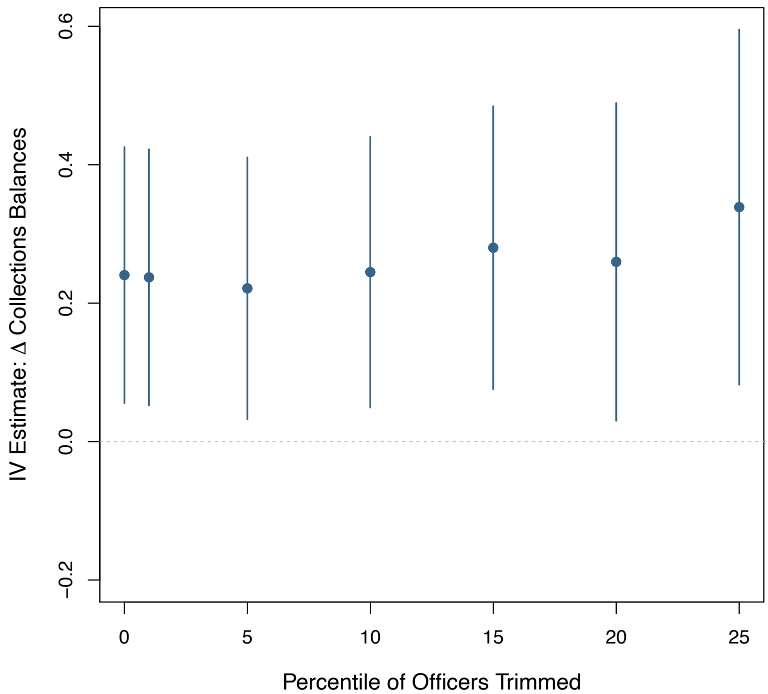
*N*otes: Each panel reports coefficients and 95 percent confidence bands from separate regressions of $Y_\tau - Y_{-1}$ (i.e., the change in Y relative to $\tau = -1$, where τ indexes event time) on the officer stringency instrument. All regressions include beat-shift fixed effects and motorist controls.

Figure H-5: Reduced form estimates by motorist income



Notes: Same as figure E-2, illustrating the post-stop change in collections balances separately for motorists with above (FS = \$124.76, se = 0.49) and below median (FS = \$123.2, se = 0.5) zip code incomes.

Figure H-6: Robustness of IV estimates to sample selection



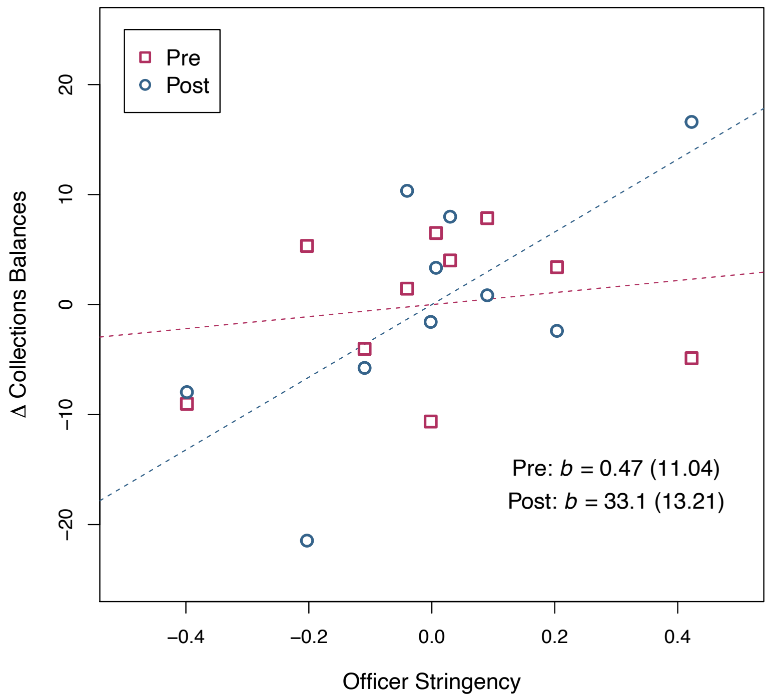
Notes: This figure reports IV estimates for the one-year change in collections balances when trimming the sample of officers with selected samples. First, a covariate index $\hat{Y}$ is constructed by regressing Y on motorist demographics using only the sample of lenient officers. Then, I construct residuals $\tilde{Y}$ from a regression of $\hat{Y}$ on beat-shift fixed effects using all speeding stops. Finally, I average $\tilde{Y}$ across officers and rank officers based on these averages. I re-estimate the 2SLS regressions dropping officers in the top or bottom p percent of the distribution of average $\tilde{Y}$. The estimate for $p = 0$ corresponds to that reported in table D-1.

Table H-3: IV Results with alternative instruments

	Collections Balances			Revolving Balances		
	(1)	(2)	(3)	(4)	(5)	(6)
	$\tau = 1$	$\tau = 3$	$\tau = 6$	$\tau = 1$	$\tau = 3$	$\tau = 6$
Leave-out (Baseline)	0.062	0.244	0.347	0.227	0.081	-0.013
	(0.108)	(0.128)	(0.152)	(0.329)	(0.394)	(0.459)
Leave-out (Residualized)	0.07	0.253	0.329	0.36	0.143	-0.026
	(0.109)	(0.13)	(0.154)	(0.338)	(0.402)	(0.468)
Officer Effects	0.073	0.261	0.358	0.459	0.231	-0.152
	(0.144)	(0.169)	(0.201)	(0.453)	(0.536)	(0.617)
Officer Effects (Shrunken)	0.063	0.22	0.331	0.289	0.099	-0.291
	(0.133)	(0.157)	(0.186)	(0.411)	(0.49)	(0.565)
Binary	0.061	0.362	0.467	0.049	0.259	-0.227
	(0.167)	(0.198)	(0.232)	(0.497)	(0.591)	(0.681)

*N*otes: This table reports DiD IV estimates over different time horizons using alternative versions of the officer stringency instrument. Each coefficient reports the 2SLS estimate where the outcome is $(Y_\tau - Y_{-1}) - (Y_{-1} - Y_{-4})$ and the fine amount is instrumented with a version of the stringency instrument, Z. In the first row, Z is the baseline leave-out mean. In the second row, Z is the leave-out mean after residualizing of beat-shift fixed effects. In the third row, Z is the estimated officer fixed effect, where the officer effects are estimated in two partitions of the data and the officer effect in the opposite partition is used (to avoid the reflection problem). In the fourth row, the same fixed effect estimates are used after applying Empirical Bayes shrinkage. The final row uses a binary version of the instrument (whether the officer is a buncher v. not).

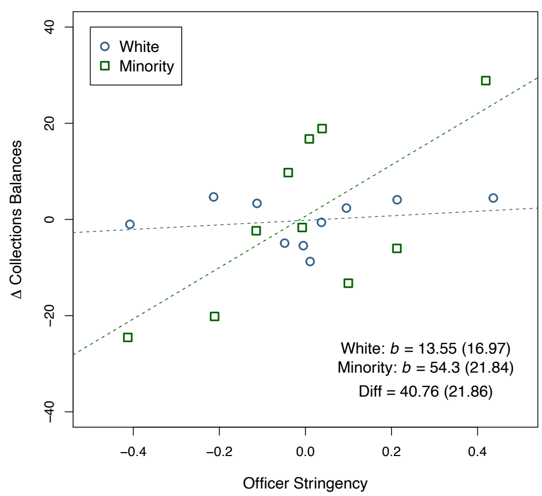
Figure H-7: Reduced form estimates for motorists without past citations



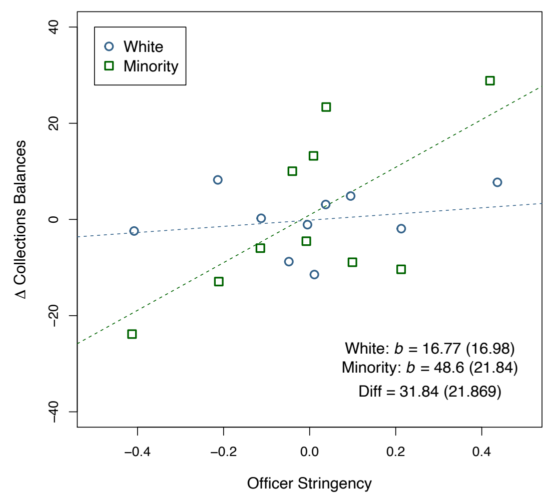
*N*otes: Same as figure D-1 using a subsample of the IV sample that requires only one citation per motorist (the first in-sample citation per motorist) and requires that each motorist has not received a citation in the previous year ($N = 272,866$).

Figure H-8: Reduced form estimates by race using within-race instrument

(a) Without controls



(b) With controls



Notes: Same as figure E-2 except using a stringency instrument that is recomputed within racial groups. The first stage estimate for white motorists is $\beta_{FS} = 121.95$ (0.48) and the first stage estimate for minority motorists is $\beta_{FS} = 125.11$ (0.53).