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 fines averaging $195 are associated with a $34 increase in unpaid bills in collections. Given additional evidence that fine payment explains this effect and that default is the "last resort" for households, I interpret this finding as suggesting rates of inability to meet unplanned expenses which are consistent with the survey evidence. 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, e.g., downstream license suspensions.

JEL Codes: G51, I32, K42

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. (2024) 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 some fraction of 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. Based on a back-of-the-envelope calculation using information in the ACS, this reduced likelihood of employment in the payroll records suggests about a \$300 decline in expected annual earnings. While fines both reduce the likelihood of transitions into the payroll records and increase the likelihood of transitions out, the impact on transitions into the payroll records is much more pronounced relative to counterfactual levels and trends. Hence, I interpret this result as evidence that fines reduce the rate at which individuals move into the subset of "good jobs" covered by the payroll records, rather than inducing transitions into non-employment, per se. I also find that impacts on payroll employment are wholly attributable to lower-income motorists and that those who are employed in payroll-covered jobs with above-median earnings at baseline experience no change in employment situations or 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 a total cumulative cost estimate of about \$315 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 larger 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 vehicle access (e.g., Baum 2009), job-finding (e.g., Bos et al. 2018; Bartik & Nelson 2021), or 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 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, I conceptualize the notion of financial fragility in terms of casual effects: a household is fragile if it must miss other bills to finance the payment of a traffic fine, and thus the relevant metric for comparison with the survey evidence is the share of households with positive treatment effects on collections balances. The event study estimates can identify bounds on this fraction under reasonable assumptions about the distribution of treatment effects. Based on this approach, a conservative bound implies that at least 11 percent ($se = 3$ percent), while a more data-driven estimate implies that at least 38 percent ($se = 11$ percent), of individuals borrow out of other financial obligations to cover an unexpected \$315 expense. 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 contribution to this literature is causal evidence 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 the 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 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 is associated with a \$93 (\$8) increase in annual (monthly) premiums. See appendix C for details.

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

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[1]As of 2022, a new Florida law requires that counties offer income-based payment plans for traffic citations. However, during my sample period (2011–2015), only two counties, Hillsborough and Pinellas, offered three-month payment plans for traffic fines (statute; news article). Figure F-2 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.

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 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. 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 provides a strong payment incentive. As such, I initially focus on estimating "intent-to-treat" effects, proceeding under the assumption that fines are paid in the majority of cases. Later in the paper, I leverage traffic court dispositions data to quantify the average total costs associated with citations and present heterogeneity analyses to assess the relative importance of channels other than fine payment in explaining the effects.

It is worthwhile to consider to what extent traffic fines are similar to and different from other expense shocks faced by households. Continuing to think of a traffic fine as a bill where the punishment for non-payment within thirty days is the revocation of driving privileges, potentially distinctive features of the setting are the short, non-negotiable due date and the swiftness and severity of punishment for non-payment. Default on medical bills, for example, is quite common (e.g., Dobkin et al. 2018; Kluender et al. 2024), and the consequences for such default will generally play out over a longer time horizon in the form of medical collections appearing on credit reports and, as a result, declines in access to credit.

Given the sanction for fine non-payment, a natural parallel is unexpected car repairs, which are the most common form of financial shock faced by households according to Pew Charitable Trusts (2015). Acute or debilitating car issues in particular impose a similar tradeoff for households, where one can either pay for repairs or stop driving. On the other hand, less urgent repairs highlight a contrast with traffic fines, as households can potentially defer paying for these repairs until a more financially convenient time.

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[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.

Other potential parallels could include surprise family expenses (e.g., funeral costs), surprise utility bills (e.g., due to unseasonably cold weather), necessary home repairs (particularly for renters, who may be charged by landlords and then face eviction in the case of non-payment), or unexpected medical prescriptions (where a co-pay is typically required at the pharmacy). Expense shocks where households can easily reduce their exposure after the fact, adjust the timing of payment, or where default consequences accrue slowly and/or uncertainly, potentially differ from traffic fines in ways such that we might expect somewhat different household responses.

To the extent that my analysis delivers the causal effect of unexpectedly *paying* a traffic fine, as much of evidence will ultimately suggest, the traffic fine setting may also generalize to a wide array of short-run, unexpected income fluctuations, such as service industry workers receiving fewer than expected tips or shifts in a given month (Morduch & Schneider, 2016).

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, information on charged violations, and information listed on the motorist's 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 E-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 E-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.

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 for 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.[4] Note that, in either case, we should still interpret the

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[4]Event studies in Dobkin et al. (2018) show that the impact of hospital admissions on medical

collections activity as attributable to the fine. The same logic also applies 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 Florida, online payment of traffic fines via credit card is permitted (see figure E-1), and hence we might expect to observe impacts on credit card balances directly attributable to fine payments. Alternatively, households may offset the payment of fines by borrowing on credit cards to finance other expenses. A 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 the default 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 incidences of financial distress typically coincide with, rather than substitute for, declines in consumption. Moreover, dynamic consequences of default in terms of creditworthiness can be severe. A typical default instance can reduce credit scores by as much as 30 points (see figure A-13), 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 consequences by studying impacts on credit scores and borrowing limits over a 3-year 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 earn-

collections materialize over 18-24 months. In appendix B, I show that the dynamic effects of separation from a payroll-covered job (described below) exhibit a similar pattern.

ings in the payroll records in a given quarter (year). To better understand what these data capture, appendix B 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 E-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 heterogeneity 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. 2024; 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.

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. The key threats to this identifying assumption are differing trends between treatment and comparison groups even prior to treatment and other, unobserved shocks which coincide with treatment timing. To test for potential violations of parallel trends prior to treatment, I adopt the strategy of Borusyak et al. (2024). 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. Figure A-2 compares the pre-stop trends in financial distress, estimated via the Borusyak et al. (2024) 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.[5]

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-5, 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

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[5]Consistent with the evidence presented in figure A-2, figure A-3 documents that the relationship between neighborhood poverty rates and citation rates depicted in figure 1 is much weaker, and in fact reversed, when examining the citation rate for speeding infractions.

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-5, 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 quite similar to the baseline results. Additional robustness exercises are discussed below in section 5.4.

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 non-missing 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-4 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. Interestingly, the analysis sample appears 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 C and section 6 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. (2024) 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. 2024).

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, about 17 percent of fine payments are financed through "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 ($N = 48,585$).[6] 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).

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[6]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.

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 mentioned in section 3.3, one reason to expect such a pattern is the impact of increased rates of default on access to credit, and correspondingly, panel (d) illustrates increases in credit card utilization, defined as total balances divided by total limits.

The fact that increases in utilization are comparable for both groups, but declines in balances are attributable to the group with available liquidity at baseline, implies that this liquid group, whose default responses are significantly attenuated, experiences more dramatic declines in credit limits (as confirmed in figure A-13). This result may appear surprising but can be rationalized by institutional features of credit markets. Specifically, as shown in panel (a) of figure A-13, the relationship between credit scores and credit limits is highly convex. The decline in credit limits associated with a ten point credit score decline is about \$2,550 for those with good credit scores (> 700), but only about \$140 for those with poor credit scores (< 500). Panels (b) and (c) of figure A-13 illustrate that, accordingly, the decline in credit limits following a typical default is much more dramatic for those with higher initial credit scores. Hence, the smaller increases in missed bills for this liquid group, whose baseline credit scores are 130 points higher, can generate comparatively large declines in credit limits. A similar pattern is seen in Dobkin et al. (2018), who compare the impacts of hospital visits for those with and without health insurance, finding larger impacts on default and credit scores for the uninsured, but more pronounced declines in credit card balances and limits for among the insured, who have significantly higher baseline credit scores.

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 E-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 ($N = 224,328$).[7]

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-

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[7]Event study estimates for credit card borrowing in the full sample, as well as estimates for all outcomes by baseline income and liquidity status are presented in supplementary appendix F.

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 figure 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 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 fines induce default on items such as utility bills suggests an inability to cover a \$200 expense via cash on hand for some fraction of households. 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 inter-

est 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. In figure A-12, I show that along 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).[8]

Panel (d) of figure 5 shows declines in the likelihood of working in a payroll-covered job ("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 finding indicates a reduced likelihood of working for large employers paying above-average wages, but does not necessarily capture employment versus non-employment.

To help interpret this estimated magnitude, appendix B-1 provides a simple, back-of-the-envelope calculation of the expected difference in annual earnings for those who are employed in the payroll records and those who are not based on information in the ACS. Taking into account both the above-average earnings in payroll-covered jobs and the fact that those working in payroll-covered jobs are employed for sure, whereas some fraction of those not in the payroll records are unemployed or out of the labor force, I estimate this difference to be about $25,000 on average. Hence, the 1.2 percentage point decline in the likelihood of payroll employment corresponds to an expected $300 decline in annual earnings.

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

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[8]The 0.6 percentage point decline in the likelihood of having a mortgage shown in figure A-12 is similar to what would be predicted given the 2.6 point decline in credit scores and the estimates in Dobbie et al. (2020), who find that that a 3.2 point increase in credit scores is associated with a 1 percentage point increase in homeownership.

job at baseline, defined as employed for all four quarters in 2010 ($N = 55,140$), 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 (-0.007 versus -0.005), but estimates are much less precise for the considerably smaller baseline employed sample.

A salient question given that the payroll records cover about 20-25 percent of total employment is the extent to which declines in the likelihood of working in a payroll-covered job reflect increased rates of non-employment. Note that while the estimates for the two groups are comparable in levels, percent effects (scaled by the means) are 5.5 times larger for those not in the payroll records at baseline. Hence, a reduction in the likelihood of transitions into the payroll records appears to be an important part of the overall story. Per the discussion in section 3.4, I view this as evidence of a reduced rate of transitions up the job ladder into a subset of "good jobs," but not necessarily evidence of elevated non-employment.

In appendix B-1, I conduct a back-of-the-envelope calculation which combines the estimated impacts by baseline payroll status with information on typical employment transitions in the CPS from Fujita et al. (2020) to estimate an implied impact on non-employment. This exercise suggests that, if transitions in and out of the payroll records follow the same pattern as the average transitions into and out of employment at a given employer in the CPS, the static ATT estimate (-0.006) and longer-run event study estimate (-0.012) imply 0.2 and 0.4 percentage point increases in the likelihood of non-employment, respectively.

In panels (c) and (d) of figure 6, 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 \$82 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 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.

As discussed in detail below in section 6.2, I find minimal evidence that the estimated impact on employment situations can be attributed to institutional features such as DL suspensions or other increases in driving costs, as opposed to the financial shock of a traffic fine, raising the question of why paying an unplanned expense may affect employment arrangements. As shown in figure 6, declines in the likelihood of working in a payroll-covered

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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.

Another possible mechanism is the impact of fines on car access or ownership. While the evidence discussed below in section 6.2 suggests a minimal role for license suspensions or insurance costs, changes in financial standing could affect households' ability to acquire or retain vehicles (Dobkin et al. 2018; Dobbie et al. 2017), and there is evidence that vehicle access can have important implications for employment outcomes (e.g., Raphael & Stoll 2001; Baum 2009; Johnson 2006). Table B-2, which presents results from a mediation-style analysis, provides some support for this hypothesis. Specifically, I divide the sample into forty groups based on demographics and baseline income, estimate group-specific impacts on payroll employment in the longer-run and several possible mediators in the medium-run, and explore the ability of the group-specific medium run effects to explain the group-specific long-run effects on payroll employment. Estimates suggest that larger declines in the likelihood of holding an auto loan in the medium term are a statistically significant predictor of larger declines in payroll employment in the longer-run.

One could alternatively ask to what extent the payroll employment effects can themselves explain the observed increases in financial distress. An important consideration here is dynamics: new defaults induced by changes in (payroll) employment status will typically take several quarters to accrue on a credit report, and by this logic alone, gradual changes in payroll employment status would appear quite unlikely to explain the initial increases in default. Another consideration is magnitudes. Based on the back-of-the-envelope calculation above, the longer-run effect on payroll employment implies a $25 decline in monthly earnings. Abstracting from dynamics, this earnings change would predict a $5 increase in collections debt based on the collections-earnings elasticity estimate in figure B-3.

5.4 Robustness

A central concern for the validity of the event study estimates is the possibility that traffic stops coincide with 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-5. To partially address this worry, figure A-6 reports event study estimates computed within auto purchase cohorts.

Specifically, for each individual i, I 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 and aggregate up the group-specific estimates, weighting by 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. This exercise is similar in spirit to the procedure of Freyaldenhoven et al. (2019), who suggest using changes in a relevant observable to purge trends in the outcome attributable to changes in unobservables around the event.

There are two complications faced by this approach. First, many individuals ($\sim$20 percent) never have an auto loan in the credit bureau data. And second, because auto purchases are related to a household's financial situation, which is the outcome of interest, one should not condition on the timing of purchases made after the traffic stop. Accordingly, I construct the auto purchase cohorts using the subgroups whose first auto purchases occur in 2010Q1 through 2012Q4 (to allow a sufficient post-event window for estimating the event studies) and whose traffic stop occurs after their auto purchase.

Figure A-6 presents event study estimates first using the baseline identification strategy but for this subsample with early auto purchases predating their traffic stops ($N = 291, 134$; 55 percent of the main sample) and then using the within-auto cohorts identification strategy for this subsample. Conditioning on the timing of auto purchases has minimal qualitative or quantitative impact on the pattern of results, with event study estimates and associated ATT's using the within-cohorts strategy nearly always within the 95 percent confidence interval of the estimates using the same sample but baseline event study approach.

Another concern for the validity of my approach, again potentially heightened by the slight increase in auto purchases prior to a traffic stop, is the possibility that deteriorating financial situations following a traffic stop could reflect mean reversion. While statistically insignificant pre-trends in a host of outcomes of interest and the similarity of the within-auto purchase cohort estimates to the baseline results (to the extent that auto purchases capture any improvements in financial situations leading up to a traffic stop) work against this hypothesis, I present two additional robustness checks in figures A-7 and A-8.

In figure A-7, I present estimates within *predicted* treatment cohorts. Specifically, I regress treatment timing on baseline characteristics as well as changes in auto loans and credit scores over the first year of the data. From this regression, I compute a predicted

cohort for each individual, divide individuals into deciles of this prediction, and estimate event studies which rely on comparisons only within these deciles. Were mean reversion an important concern, we should expect meaningful attenuation when conditioning on predicted treatment timing based on levels and changes in financial situations. Instead, figure A-7 shows that results from this approach are very similar to the baseline estimates.

Figure A-8 examines pre-stop trends over a longer time horizon. A salient issue here is that, while all individuals contribute to the identification of the estimates for the first four pre-stop quarters (because four quarters of pre-stop data are available for everyone), estimates beyond four quarters also reflect changes in the composition of cohorts contributing to identification. To assess pretrends over a longer horizon, then, I reweight each cohort to match the average baseline characteristics of the full sample, thus ensuring that the longer-run pre-period estimates are not driven by differences in characteristics across cohorts. These cohort-reweighted estimates reveal no pre-stop trends in all outcomes of interest for a full three years prior to the traffic stop, further reducing concerns about mean reversion. Importantly, treatment effect estimates (both dynamic event study estimates and static ATT estimates) are quite similar in the baseline and cohort-reweighted specifications.[9]

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

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[9]As additional robustness, figure A-9 shows that estimates obtained via the methods of Sun & Abraham (2021) or Borusyak et al. (2024) are nearly identical to the main estimates, figure A-10 shows that estimates are unchanged when relying instead on monthly data, and figure A-11 shows that results are not driven by trend breaks for the counterfactual control group. Supplementary appendix G presents a qualitatively similar pattern of results using a complementary instrumental variables strategy which does not exploit variation in traffic stop timing.

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 missing dispositions (1.8 percent) or disposition codes of guilty (6 percent), dismissed (8.7 percent), or "adjudication withheld" (24.6 percent), present significant interpretation challenges regarding the ultimate outcome of the citation. In particular, those with dismissed or withheld verdicts may have faced no sanctions, received partial penalty reductions (such as the waiving of license points), or pled down to lesser offenses. These complications, as well as my approach for constructing estimated payments in light of these challenges, are discussed in detail in appendix C.

Panels (a) and (b) of figure 7 present estimates of the total costs of citations which take into account the post-citation choices of motorists and the various complications associated with mapping dispositions into ultimate payment outcomes. As shown in panel (a), 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. This estimate reflects the fact that a meaningful share of the sample likely avoided the accrual of license points (and therefore insurance cost increases) via either traffic school or the traffic court system. Panel (b) illustrates the estimated cumulative costs, factoring in both the initial payment of fines and fees and 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 associated insurance cost increases, described in more detail in appendix C. Upper and lower bound estimates on combined fine and fee payments are $216 and $114, respectively. Estimated increases in insurance costs are more sensitive to assumptions, with cumulative insurance cost estimates ranging from $58 to $252. Combining upper and lower bounds on each yields a range of total cumulative cost estimates from $172 to $562, comparable to the $400 emergency expense considered in surveys, discussed in section 6.3.

Note that 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 facing no changes in insurance premiums.

Another consideration in quantifying the total costs associated with citations in my sample is additional traffic stops following the focal citation. Figure C-5, which presents event study estimates for cumulative fines paid, suggests that the average citation is associated with an additional $13 in traffic fines paid over the following six quarters, increasing my

estimate of the total costs faced by the average motorist to $315.

6.2   Fine payment versus other mechanisms

To shed 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 the sample is being split on endogenous, 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;[10] (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 I call the "possible suspension" subgroup. As discussed in appendix C, it is temping to view the "possible lenience" subgroup as a placebo group, 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 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

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[10]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.

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 remarkably 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).[11]

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 a meaningful share of the average payment of a $300 unplanned expense must be financed through default on other financial obligations. 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 and the finding from section 6.2 that default effects are comparable for the sample of 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

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[11]Figure F-1 provides additional evidence that initial fine payment, as opposed to downstream increases in auto insurance costs, is the primary driver of the effects by showing that results are similar for those cited near and far from their home county. Those cited near home are more likely to have dismissed/withheld verdicts instead of paid verdicts, translating to a small reduction in estimated paid fines but relatively large reductions in insurance cost increases.

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 default events (extensive margin) and on collections balances (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. The key to reconciling these two results is to note the very high counterfactual mean on the extensive margin: $\mu = 0.22$. In other words, default 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 is 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.

The average treatment effect I estimate can be expressed as $\hat{\Delta} = \sum_{\Delta=\underline{\Delta}}^{\bar{\Delta}} \Delta \pi_\Delta$, a weighted average of all possible treatment effects $\Delta \in [\underline{\Delta}, \bar{\Delta}]$, with weights π_Δ equal to the share of the sample with treatment effect Δ. I assume that treatment effects are bounded below by $\underline{\Delta} = 0$; in other words, no one is made *less* likely to default by an expense shock. Hence, the average treatment effect can be written as $\hat{\Delta} = \sum_{\Delta=0}^{\bar{\Delta}} \Delta \pi_\Delta = \sum_{\Delta>0}^{\bar{\Delta}} \Delta \pi_\Delta$ and the goal is to bound $\pi = \sum_{\Delta>0}^{\bar{\Delta}} \pi_\Delta$, the share with positive treatment effects. To see that a sharp lower bound on π is identified by $\bar{\Delta}$, the maximum treatment effect size, note that:

$$\underline{\pi} = \min_{\pi_\Delta} \sum_{\pi>0}^{\bar{\Delta}} \pi_\Delta \text{ s.t. } \sum_{\Delta=0}^{\bar{\Delta}} \pi_\Delta \Delta = \hat{\Delta}$$

yields $\underline{\pi} = \hat{\Delta}/\bar{\Delta}$, the ratio of the average and maximum treatment effect.

A useful starting point, then, is to assume $\bar{\Delta} = \$315$, the estimated average cumulative cost of a citation, including insurance premiums and future fines, as discussed above in section 6.1. In other words, no household is induced to default on more than \$315 by a \$315 expense shock. Based on the estimated ATT on collections balances ($= \$34$), this implies a lower bound on π given by $\underline{\pi} = 34/315 = 0.108$ ($se = 0.03$).

This lower bound is likely too conservative. On average, fines also induce default on credit lines, suggesting that defaults which lead to collections are not the only margin of adjustment. Further, there is no evidence for treatment effects as large as \$315 in the data; the estimated ATT for the low-income, illiquid group in figure 4 is \$48.62 ($se = \$9$). In appendix D, I

estimate an upper bound on treatment effects using an approach which first bins the data into cells based on age, gender, race, as well as baseline estimated income, credit score, collections debt, and available credit card liquidity. I then use heterogeneity in treatment effects based on these characteristics in one half of the data to predict heterogeneity in the other half, estimate effects by deciles of these predictions, and extrapolate to the implied estimate for the individual with the highest predicted treatment effect. This approach yields an estimated $\bar{\Delta} = \$89$ ($se = \$23$), with an associated $\underline{\pi} = 0.383$ ($se = 0.109$).

As robustness, I supplement this extrapolation-based approach with a companion strategy which estimates treatment effects for covariate groups and uses extreme quantiles of the deconvolved (i.e., net of estimation error) distribution of treatment effects as an estimate for $\bar{\Delta}$. This approach gives magnitudes ranging from $72 to $112 ($se = \$17$), with the largest of these estimates implying that $\underline{\pi} = 0.302$ ($se = 0.059$).[12]

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 A-14, which presents event study estimates where the outcome is an indicator for whether a motorist has accumulated any new default flag to date, implies that about 0.5 percent of the 39.4 percent of the sample at risk of being "pushed" into their first default by traffic fine are actually induced to default, suggesting that at least 38.9 percent of the sample is unaffected by fines.[13]

To summarize, conservative bounds suggest that between 11 and 61 percent of households are induced to default on other financial obligations by an unplanned, $315 expense. My preferred estimate, which relies on an extrapolation-based estimate of the maximum treatment effect, suggests that at least 38 percent ($se = 11$ percent) are induced to default. An alternative approach for estimating the maximum treatment effect yields similar conclusions.

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). As a counterpoint to the SHED, Bhutta & Dettling (2018) use the Survey of Consumer Finances (SCF) to estimate that about 24 percent of households could not pay a $400 expense using liquid savings.

Importantly, the composition of my sample of traffic offenders may differ from that

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[12]See appendix D for additional details on the estimation of $\bar{\Delta}$, including a graphical depiction of the extrapolation-based estimate, deconvolution-based estimates for various definitions of the covariate cells, and additional information on the computation of standard errors for $\bar{\Delta}$ and $\underline{\pi}$.

[13]This finding suggests an important dimension of heterogeneity in the data, further explored in panel (b) of figure A-14, which shows no effects of fines on collections balances for those with no default flags on their credit report at baseline (38 percent of the sample).

of a nationally representative survey. Hence, in table 3, I present estimated ATT's for collections balances and implied lower bounds on π when reweighting my sample to match the demographic, neighborhood income, and credit score distributions in Florida and U.S. Compared with my baseline ATT = \$34, reweighted estimates range from \$27.7 to \$30.1, suggesting that the inability to meet unplanned expenses is marginally more pronounced in my sample than in the broader population. Associated estimates of $\underline{\pi}$ are around 0.09 when using the conservative $\bar{\Delta} = \$315$, around 0.25 when using the largest deconvolution-based $\bar{\Delta} = \$112$, and around 0.32 when using the extrapolation based $\bar{\Delta} = \$89$.

These reweighted estimates of the lower bound affected share $(\underline{\pi})$, which suggest that at least 25 percent of households are induced to default by a traffic fine (when using the data-driven estimates of the treatment effect upper bound), are larger than would be expected based on the SHED (15 percent) and comparable to what would be expected based on the SCF (24 percent). Hence, I view my estimates as roughly consistent with the survey evidence but suggestive of higher rates of this notion 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 financial resiliency. 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.

6.4   Relevance beyond the United States

The discussion above implies a non-negligible prevalence of financial fragility, or the inability to cope with typical, unplanned expenses, in the United States. A natural follow up question is: to what extent is this also a salient issue elsewhere in the developed world?

The EU Statistics on Income and Living Conditions (EU-SILC) project conducts a yearly household survey which asks a comparable question to that in the SHED about whether a household could manage an unexpected expense using its own resources. The magnitude of the expense in the EU-SILC survey varies across countries and is defined as five percent of the country's median personal income, which is one month's income for a household at the poverty threshold. Hence, the magnitude of (hypothetical) unexpected expenses examined in the EU-SILC is significantly larger as a fraction of typical incomes than the \$400 figure in the SHED, which is about one percent of median personal income in the U.S.

Averaged across the E.U., the share of households reporting an inability to cover emergency expenses is 32 percent, less than the comparable figure in the U.S. based on the SHED (39 percent). Factoring in the stark differences in the magnitude of the expense shocks considered (\$987 in the EU-SILC versus \$400 in the SHED), it may be reasonable to conclude

that this notion of financial fragility is moderately to significantly less prevalent in the E.U.

One could also benchmark the size of the emergency expenses considered in the EU-SILC with the magnitude of the typical speeding fine. Average speeding fines in the E.U. are marginally higher than in the U.S. as a share of monthly incomes (8 percent versus 5 percent). However, speeding fines correspond to a significantly smaller share of the expense shock considered in the EU-SILC (14 percent) than in the SHED (37 percent). Combined with the higher self-reported rates of ability to cover larger expenses, then, a higher fraction of households are likely able to weather these fines in the E.U. than in the U.S. For further details, see table A-1, which reports the size of the expense shock, share of households reporting an inability to cover that shock, as well as information on typical speeding fines for 25 E.U. countries, as well as population-weighted averages.

Differences across European countries in the prevalence of self-reported financial fragility could be instructive in terms of understanding differences between the U.S. and the developed world more broadly. There is substantial heterogeneity in the share of households reporting fragility across countries, with rates exceeding 50 percent in Croatia, Greece, and Latvia and below 20 percent in Norway. Demertzis et al. (2020) note that potentially important determinants of differences across countries in this regard are differences in financial literacy and differences in culture, specifically as it relates to relying on families, friends, and communities for financial assistance (Lusardi et al., 2011).

Another potential determinant of differences across countries could be the generosity of the social safety net. Figure A-15 shows that a simple proxy for the generosity of a country's tax and transfer system, the difference in pre- and post-tax poverty rates, can explain about 25 percent of the variation in self-reported financial fragility. Hence, overall rates of redistribution could partially explain differences in financial fragility between the U.S. and Europe. Note that this conclusion meshes well with an emerging literature documenting the role of social insurance programs in the U.S. in insuring households against financial distress (e.g., Mazumder & Miller 2016; Gallagher et al. 2019).

6.5   Consumption smoothing and social insurance

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.

From the perspective of a policymaker, the consumption smoothing behavior of households is particularly relevant for the optimal generosity of social insurance programs. In canonical models (e.g., Baily 1978; Chetty 2006), more generous social insurance benefits are socially desirable as the difference in a household's marginal utility of consumption between the "bad" and "good" states increases, holding constant moral hazard effects. If households can self-insure, there is no need for social insurance.

My empirical findings speak to the differences in marginal utilities associated with typical, negative income shocks in a dynamic sense: shocks induce default, which in turn reduce future welfare 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.

Alternatively, the results may imply a need for policy interventions to encourage self-insurance, 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 $315 over the following six quarters are associated with increases in unpaid bills in collections of about $34. I interpret this as evidence that some fraction of households must default on other financial obligations in order to cover unplanned expense, which that interpretation supported by two additional pieces of evidence. 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 suspensions, or the accrual of driver license points. 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 unexpected expenses.

In turn, increased default leads to measurable longer-run effects on access to credit, with credit scores and borrowing limits falling by 2.6 points and \$330, respectively, over the three years following a traffic stop. 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. I interpret this finding as suggestive of a diminished ability to obtain or hold "good jobs."

The evidence that fines are associated with increased financial distress, declines in credit reputation, and changes in employment situations suggests that many households cannot easily absorb typical, but unplanned, expense shocks. A conservative estimate implies that at least 11 percent ($se = 3$ percent), while my preferred approach implies that at least 38 percent ($se = 11$ percent), of households are induced to borrow from other financial obligations when faced with an emergency \$315 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
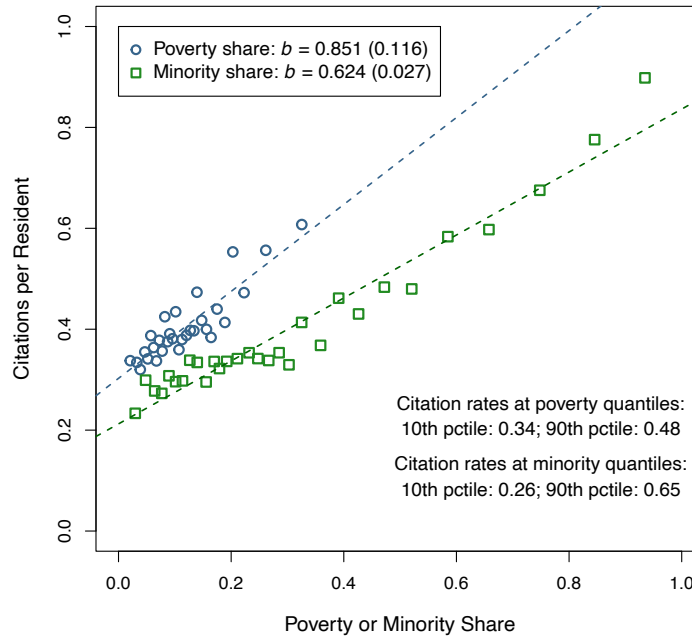
Notes: This figure plots binned means of the neighborhood ticketing rate (total citations 2011–2015 issued to zip code residents divided by the number of residents) against binned means of neighborhood characteristics. $N = 908$ zip codes.

Table 1: Summary statistics at baseline

	(1) Florida	(2) Drivers on File	(3) Event Study
Panel A: Demographics			
Female	0.51	0.44	0.45
Race = White	0.55	0.4	0.59
Race = Black	0.16	0.17	0.2
Race = Hispanic	0.24	0.22	0.22
Age	39.3	36.81	36.37
Credit File Age	–	13.02	13.2
Credit Score	662	604	624
Estimated Income	32000	35137	39524
Zip Income	52874	51481	55023
Panel B: Financial Distress			
Collections		2.83	2.24
Collections Balances		1636	1299
Delinquencies		2.21	1.99
Derogatories		1.62	1.43
Panel C: Credit Usage			
Any Revolving		0.66	0.73
Any Auto Loan		0.36	0.41
Any Mortgage		0.28	0.33
Revolving Balances		4023	4950
Revolving Limit		12177	15367
Panel D: Payroll Records			
Any Payroll Earnings		0.12	0.13
Monthly Earnings		2975	3319
Panel E: Citation Information			
Fine Amount		171.85	195.53
DL Points		1.74	3.39
Definitely Paid		0.465	0.589
Possible Lenience		0.401	0.333
Possible Suspension		0.134	0.078
Individuals	14800000	2631641	525646

Notes: 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. See text for additional details on sample construction.

Figure 2: Event study estimates for default outcomes

(a) Any New Default
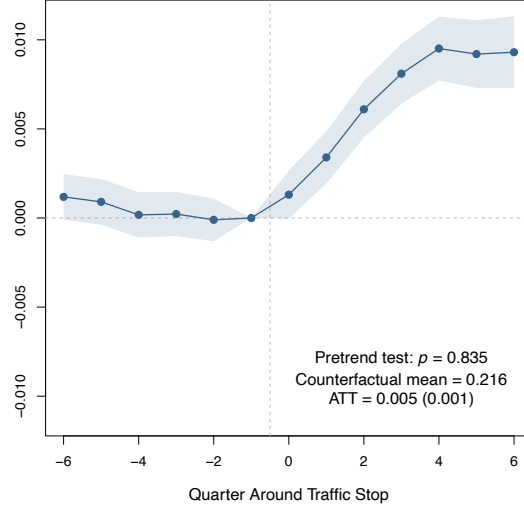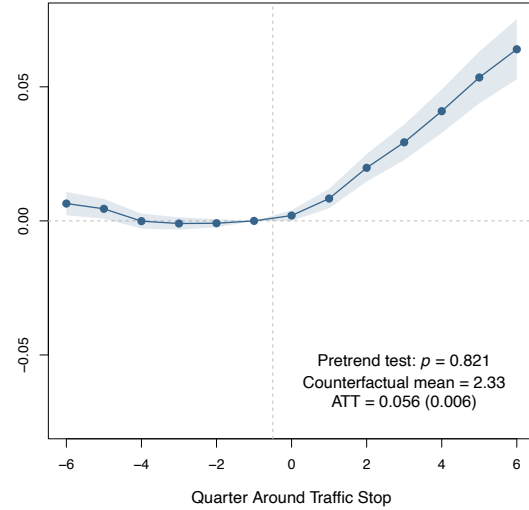
(b) Collections Balances

(c) Credit Line Delinquencies

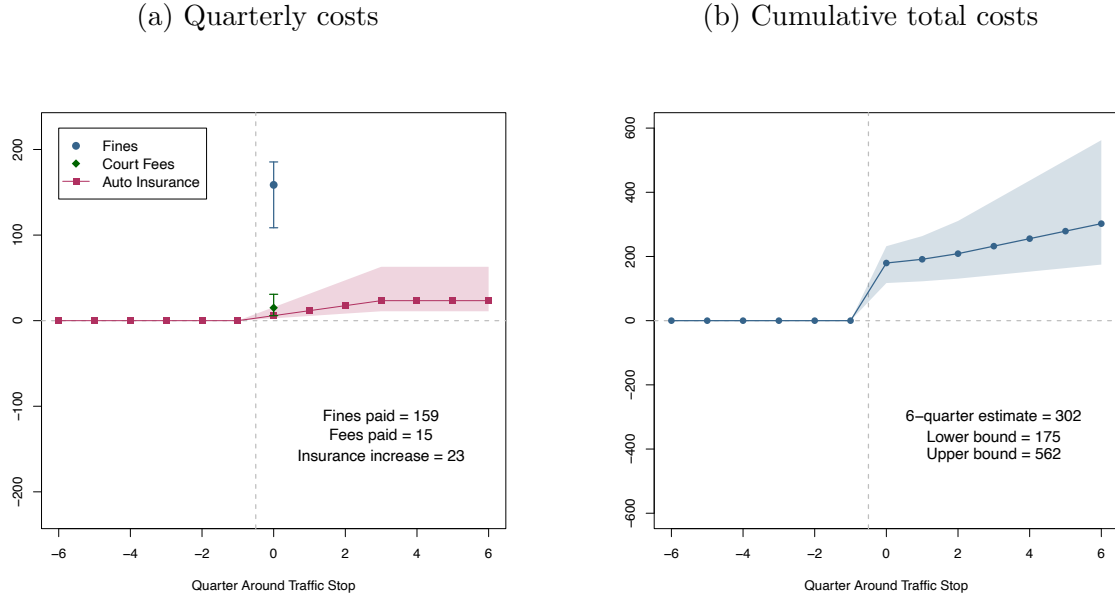*Notes*: Each figure reports event study estimates, obtained via the Callaway & Sant'Anna (2021) approach, as well as 95 percent confidence intervals based on design-based standard errors from Roth & Sant'Anna (2022), for the denoted outcome. Sample is the full event study sample ($N = 525,646$). Figures also report the p-value from the Borusyak et al. (2024) 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		Credit Lines	
		(2) Number	(3) Balances	(4) Delinquencies	(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					
	$p = 0.835$	$p = 0.525$	$p = 0.357$	$p = 0.821$	$p = 0.176$

Notes: 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. (2024). The sample is the full event study sample ($N = 525,646$).

Figure 3: Event study estimates by baseline credit access

(a) Any New Default
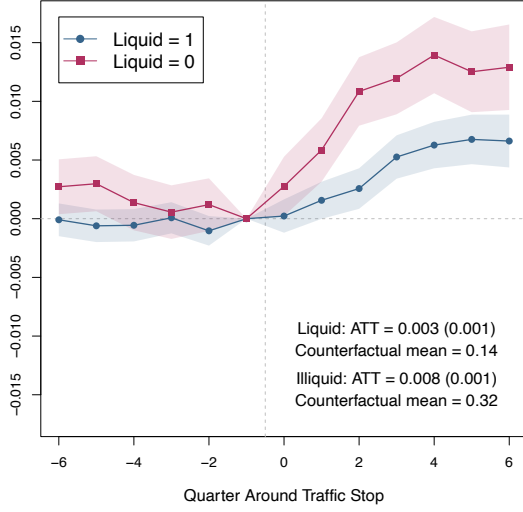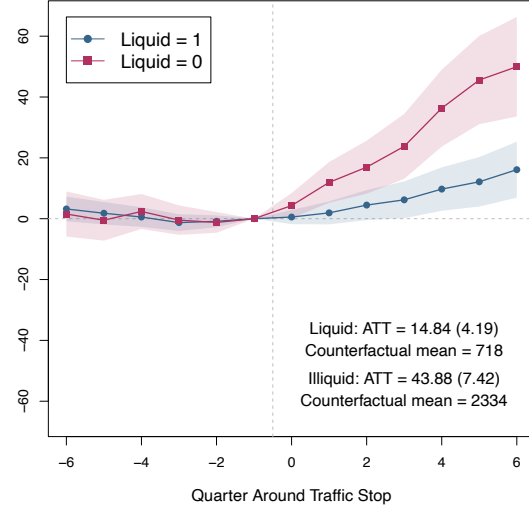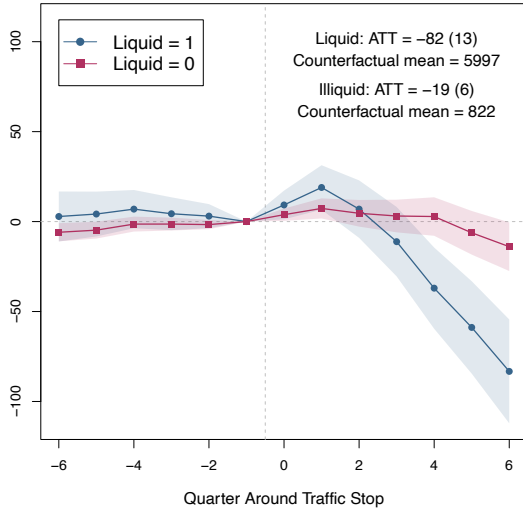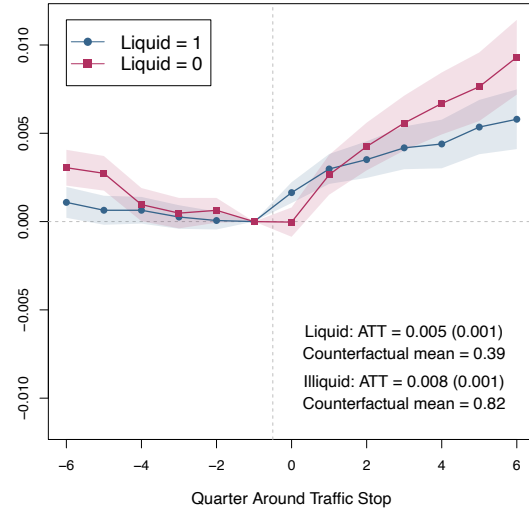
(b) Collections Balances

(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
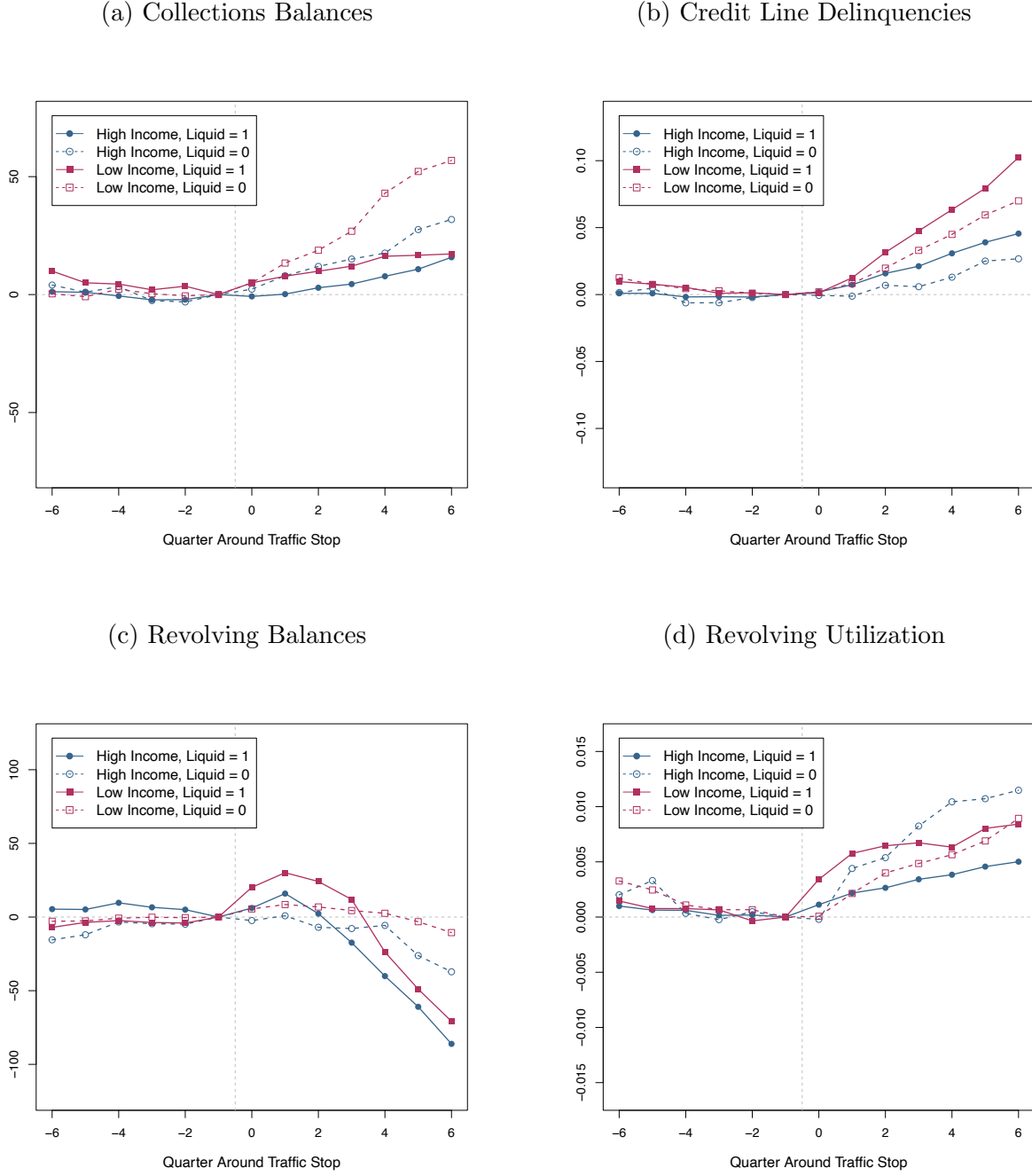
(a) Collections Balances

(b) Credit Line Delinquencies

(c) Revolving Balances

(d) Revolving Utilization

Notes: 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
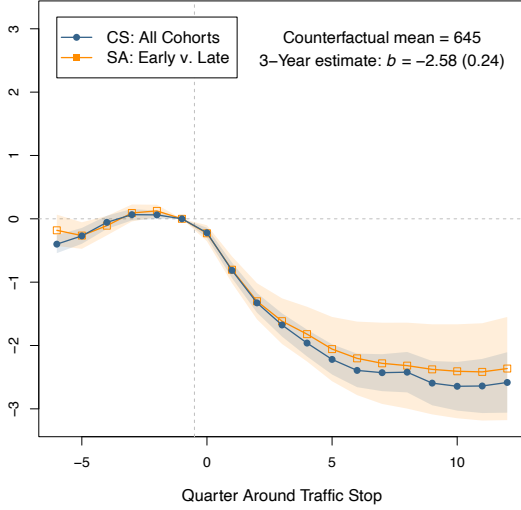
(b) Subprime
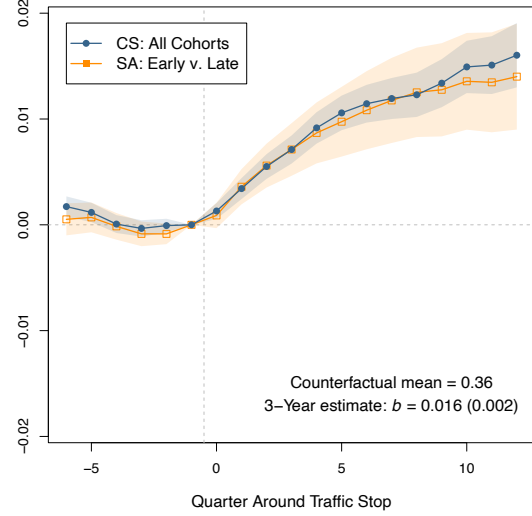
(c) Borrowing Limit
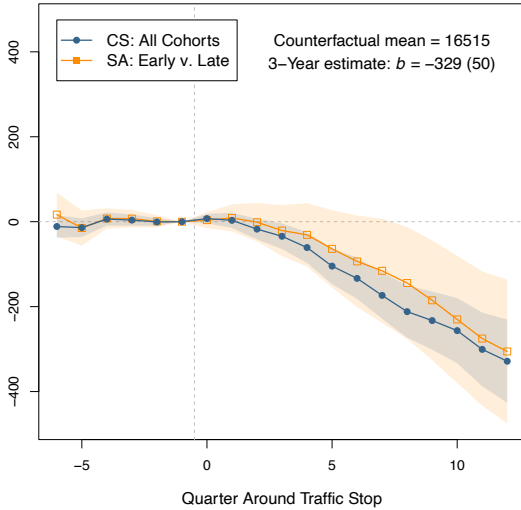
(d) Payroll Employment
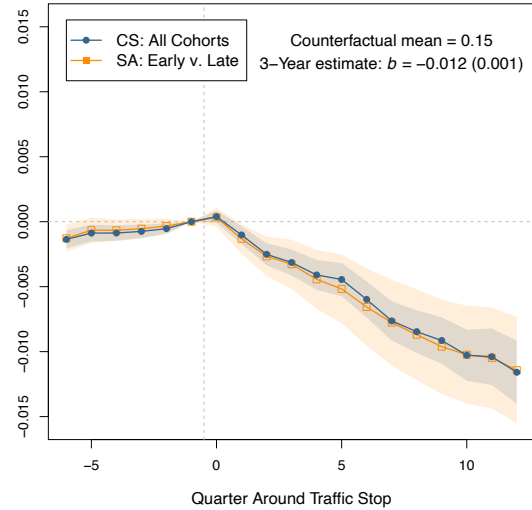
*N*otes: This figure reports event study estimates obtained from the primary Callaway & Sant'Anna (2021) specification (blue circles) as well as from a specification based on Sun & Abraham (2021) which compares only those cited in 2011–2012 ("early" cohorts) to those cited in 2015Q4 ("late" cohort). 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.

Figure 6: Event study estimates by baseline payroll status

(a) Collections Balances

(b) Payroll Employment

(c) Collections Balances

(d) Payroll Employment

Figure 7: Estimated total costs of citations

(a) Quarterly costs

(b) Cumulative total costs

Notes: 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. Panel (b) reports cumulative total cost estimates. Confidence bands reflect the sensitivity of the estimates to various assumptions. For additional details on the underlying calculations, see appendix C

Figure 8: Event study estimates by traffic court disposition

(a) Collections Balances
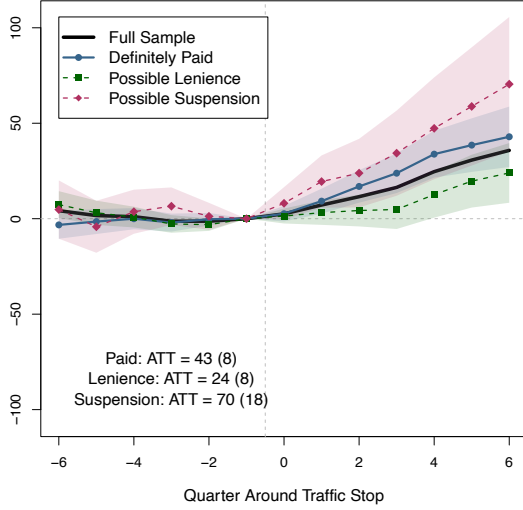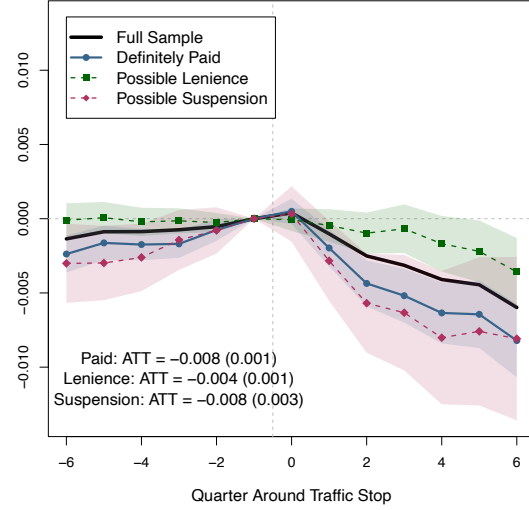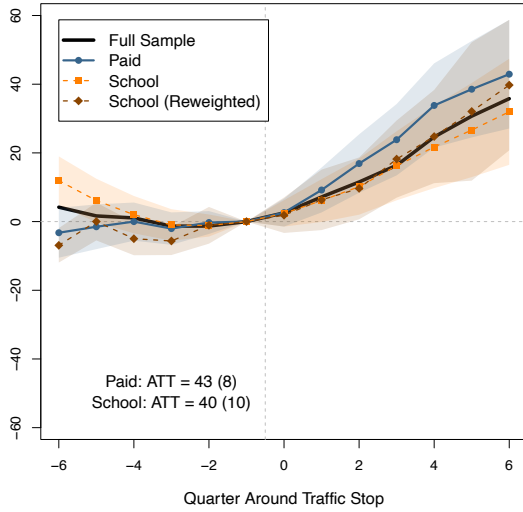
(b) Payroll Employment

(c) Collections Balances

(d) Payroll Employment
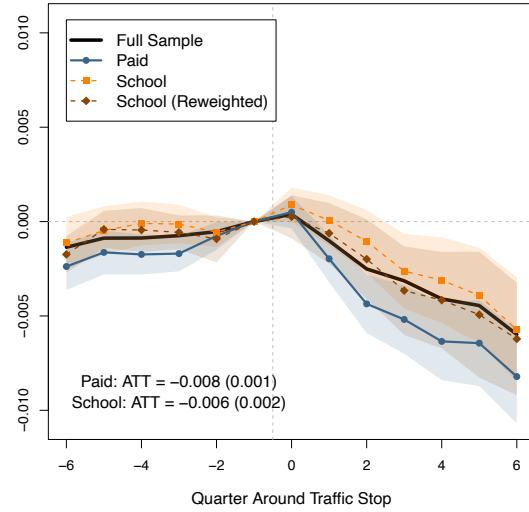
Notes: Each figure reports event study estimates, obtained via the Callaway & Sant'Anna (2021) approach, as well as 95 percent confidence intervals based on design-based standard errors from Roth & Sant'Anna (2022), for collections balances or payroll employment, estimated separately by traffic court 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 C for additional details.

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Table 3: Reweighted ATT estimates and implied affected share estimates

| | (1) Baseline | Demographics | | + Zip Income | | Credit Score |
		(2) FL	(3) US	(4) FL	(5) US	(6) US
ATT Estimate	33.91	27.74	29.09	27.7	30.1	28.39
	(4.04)	(3.51)	(2.95)	(4.57)	(4.31)	(3.52)
Lower Bound Affected Share	0.108	0.088	0.092	0.088	0.096	0.09
(Using estimated $\bar{\Delta} = 315$)	(0.03)	(0.025)	(0.025)	(0.026)	(0.028)	(0.025)
Lower Bound Affected Share	0.302	0.247	0.259	0.247	0.268	0.253
(Using estimated $\bar{\Delta} = 112$)	(0.059)	(0.049)	(0.048)	(0.056)	(0.056)	(0.05)
Lower Bound Affected Share	0.383	0.313	0.329	0.313	0.34	0.321
(Using estimated $\bar{\Delta} = 89$)	(0.109)	(0.09)	(0.091)	(0.096)	(0.1)	(0.092)

Notes: This table reports ATT estimates for collections balances and associated lower bound affected share estimates (referred to as $\underline{\pi}$ in section 6.3) for the event study sample. Column 1 reports the baseline estimates, while columns 2-6 report estimates when reweighting the sample to match the distribution of baseline characteristics in Florida and the United States. Columns 2-3 reweight the sample to match the age × gender × race sample in the 2010 ACS microdata. Columns 3-4 reweight the sample to match the age, race, gender and zip code income distribution by estimating the ATT separately for age × gender × race groups, reweighting to match the distribution of zip code income in the Florida and the U.S. for that group, and then aggregating up these estimates, weighting by each group's sample share in Florida and the U.S. Column 6 reports estimates when reweighting only to match the U.S. credit score distribution. Rows 2-4 report report the estimated lower bound on the affected share, $\underline{\pi}$, defined as the ATT/$\bar{\Delta}$, using different estimates of the treatment effect upper bound $\bar{\Delta}$ as described in the text and in appendix D. Standard errors for the $\underline{\pi}$ estimates are computed via the delta method.

ONLINE APPENDIX for "Fines and Financial Wellbeing"

A   Appendix Figures and Tables

Figure A-1: Potential outcomes associated with standard moving violation
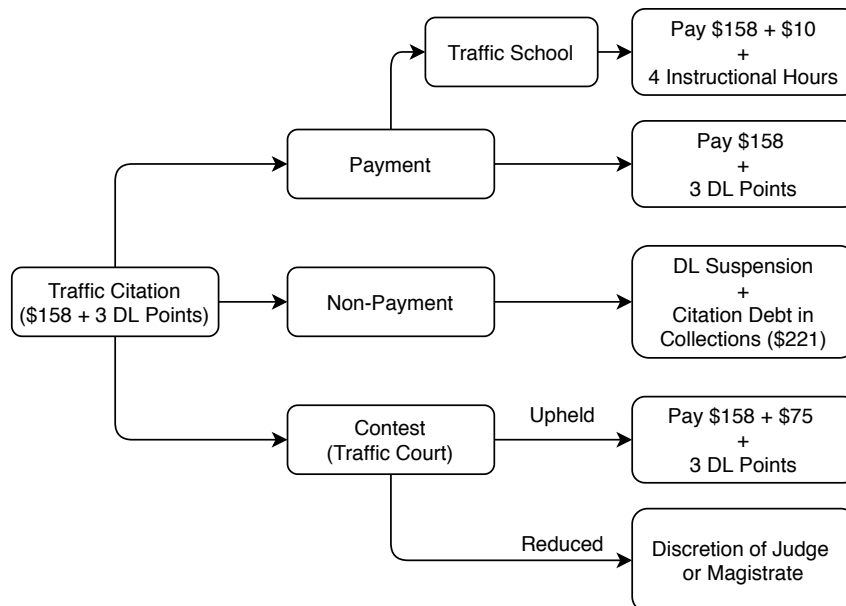
*N*otes: This figure provides a flow chart summarizing driver choices and the associated outcome(s) for each choice. The $10 surcharge for traffic school attendees represents the typical net surcharge, $25 for the course minus a $15 fine reduction. The citations debt in collections ($221) for non-payers 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 Default

(b) Collections Balances
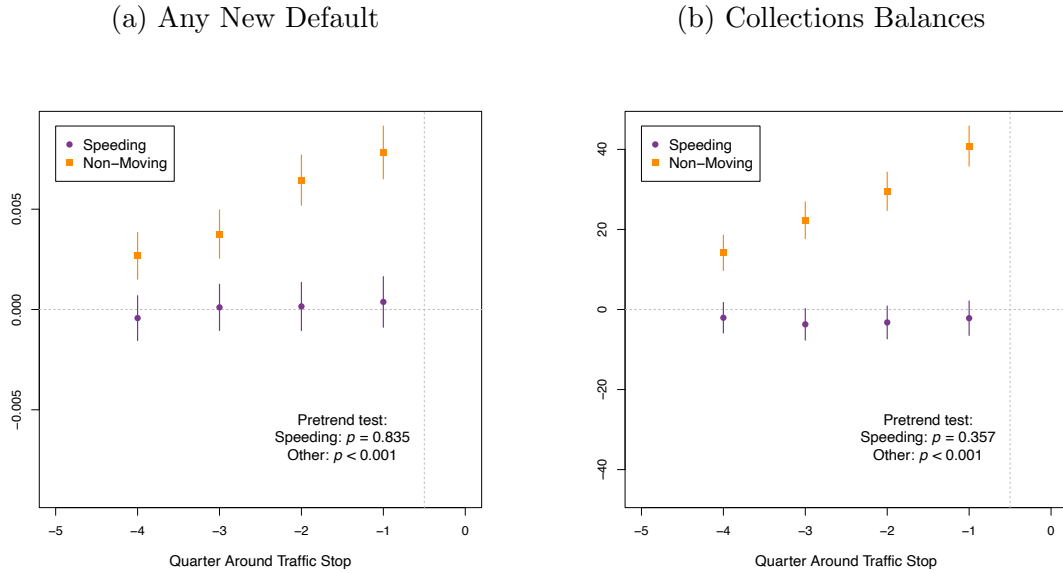
*N*otes: This figure reports results from the parallel trends test of Borusyak et al. (2024) for the primary event study sample of speeders ($N = 525,646$) and a sample, constructed in an identical way, of individuals who commit non-moving traffic violations, such as paperwork and equipment infractions ($N = 625,097$).

Figure A-3: Citation rates by neighborhood characteristics
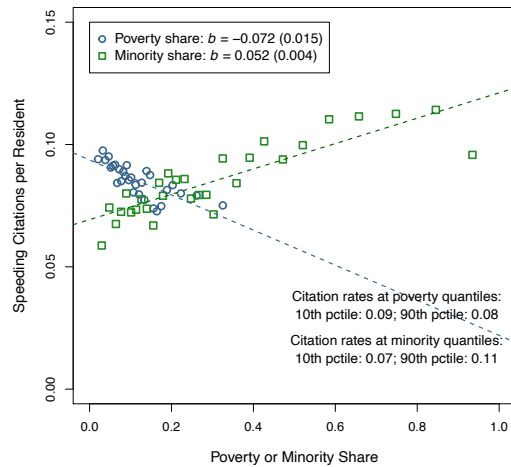
*N*otes: Same as figure 1 except using citations for speeding infractions rather than all citations.

Figure A-4: Event study cohorts

(a) Distribution of Timing

(b) Characteristics by Timing

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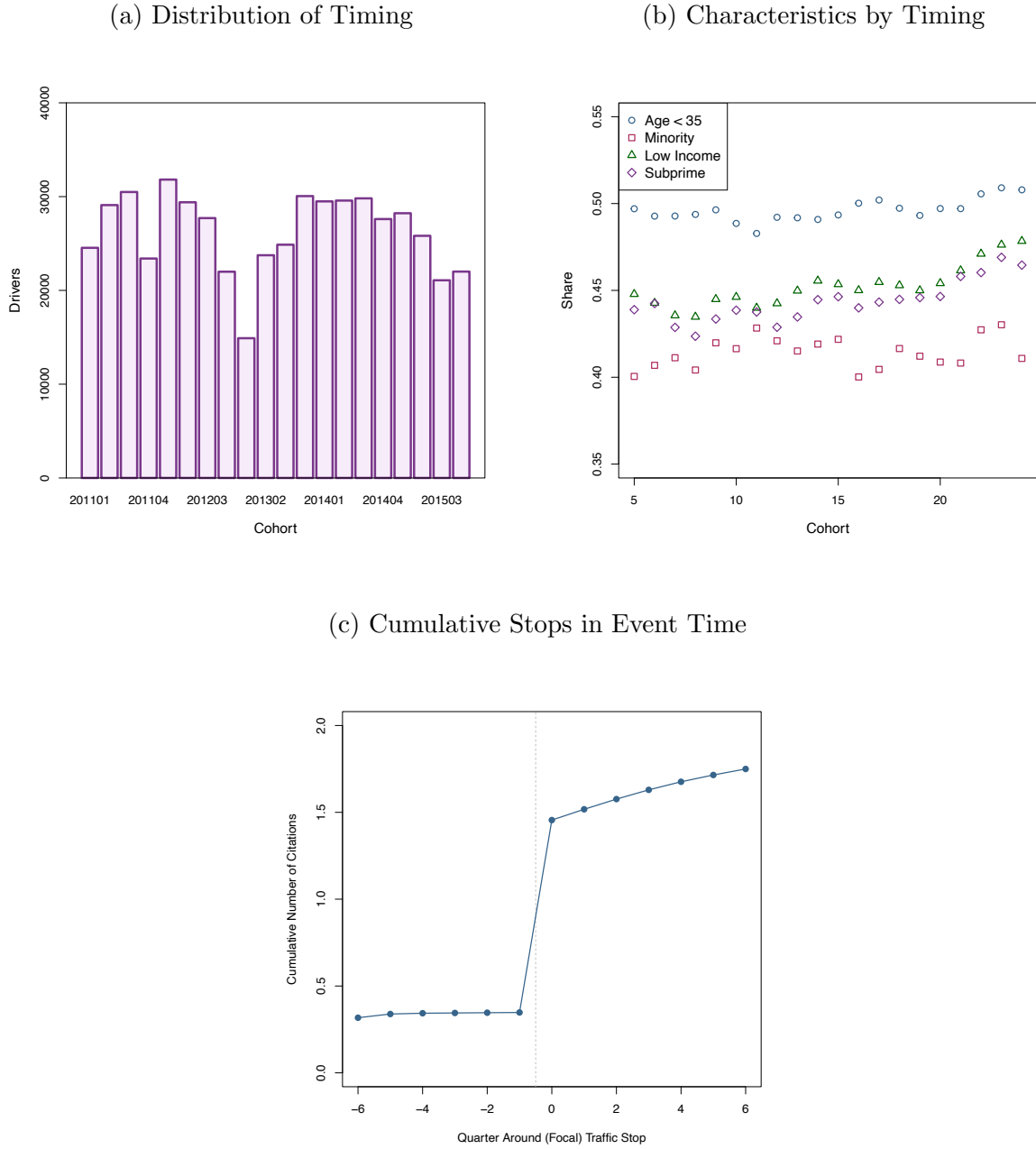
Notes: Panel (a) plots the distribution of treatment timing ("cohorts") for the event study sample ($N = 525,646$). Panel (b) illustrates characteristics of each cohort. Panel (c) shows how the cumulative number of traffic stops varies in event time.

Figure A-5: Trends in car ownership around traffic stops

(a) Any Car Loan Around Traffic Stop
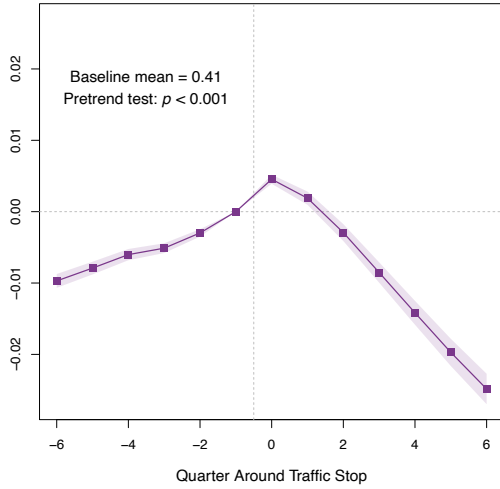
(b) Credit Score Around Car Purchase
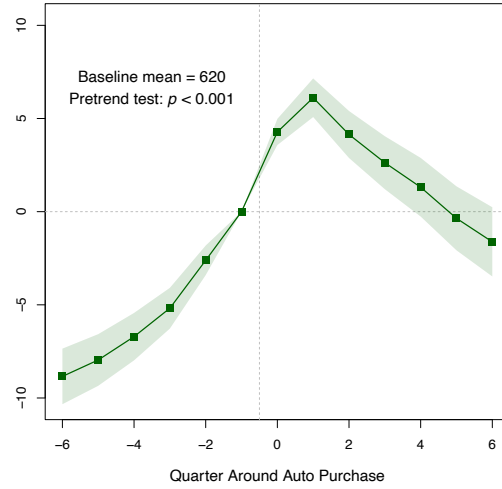
Notes: Panel (a) reports event study estimates using the event study sample ($N = 525,646$) where the outcome of interest is the presence of an open auto loan on the credit file (baseline $\mu = 0.412$; at the time of traffic stop, $\mu = 0.475$). Panel (b) reports event study estimates around the time 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-6: Event study estimates within auto purchase cohorts

(a) Any New Default

Main: ATT = 0.005 (0.001)
Subsample: ATT = 0.004 (0.001)
Within Cohorts: ATT = 0.007 (0.001)

(b) Collections Balances

Main: ATT = 34 (4)
Subsample: ATT = 41 (5)
Within Cohorts: ATT = 32 (5)

(c) Delinquencies

Main: ATT = 0.056 (0.006)
Subsample: ATT = 0.076 (0.007)
Within Cohorts: ATT = 0.062 (0.007)

(d) Revolving Balances

Main: ATT = −63 (8)
Subsample: ATT = −109 (12)
Within Cohorts: ATT = −73 (12)

(e) Credit Score

Main: ATT = −1.86 (0.12)
Subsample: ATT = −2.01 (0.15)
Within Cohorts: ATT = −1.68 (0.15)

(f) Subprime

Main: ATT = 0.01 (0.001)
Subsample: ATT = 0.011 (0.001)
Within Cohorts: ATT = 0.009 (0.001)

(g) Borrowing Limit

Main: ATT = −144 (24)
Subsample: ATT = −266 (35)
Within Cohorts: ATT = −188 (35)

(h) Payroll Employment

Main: ATT = −0.006 (0.001)
Subsample: ATT = −0.003 (0.001)
Within Cohorts: ATT = −0.003 (0.001)

Notes: Each figure reports the baseline event study estimates (blue circles), event study estimates which restrict to a sample who first purchase cars between 2010Q1 and 2012Q4 and whose traffic stop occurs after that date (red diamonds; $N = 291,134$), and event study estimates using this sample but which are estimated within auto purchase cohorts (orange squares). These estimates are computed by estimating event studies separately for each auto purchase cohort and aggregating up the group-level estimates, weighting by sample shares.

Figure A-7: Event study estimates within predicted cohorts
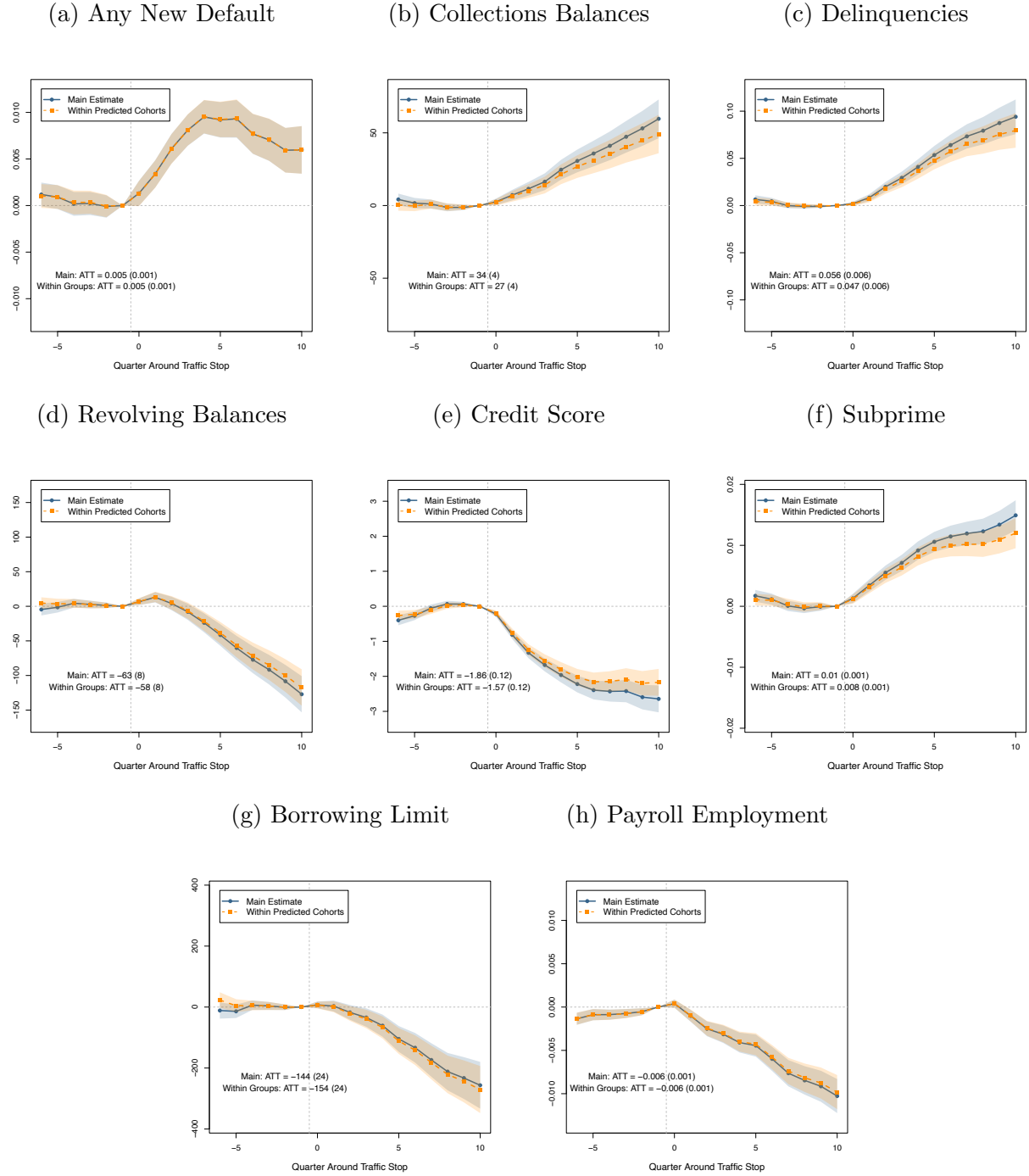
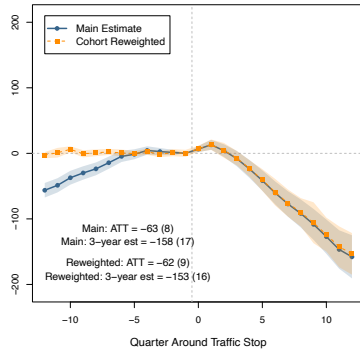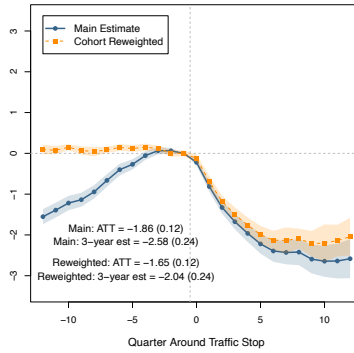(a) Any New Default   (b) Collections Balances   (c) Delinquencies
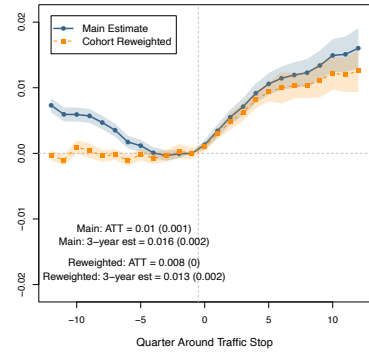

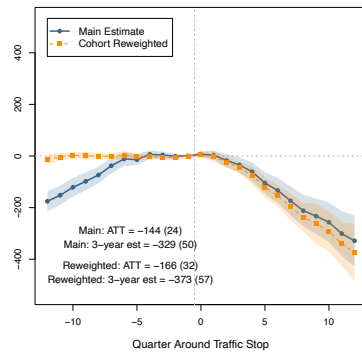(d) Revolving Balances   (e) Credit Score   (f) Subprime
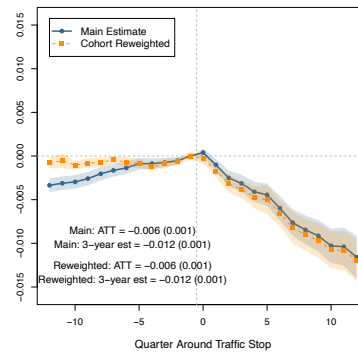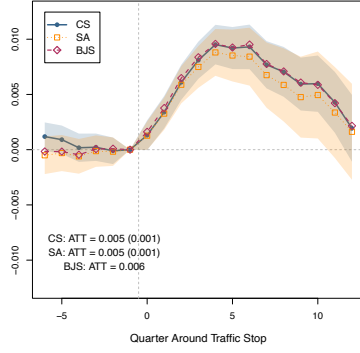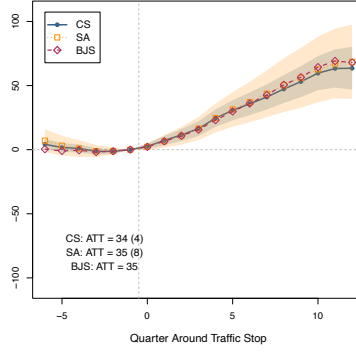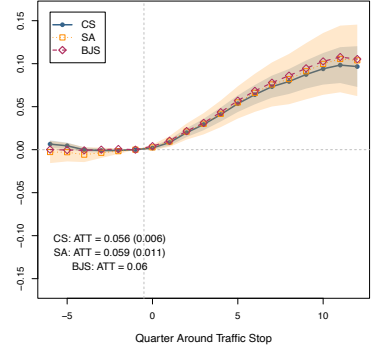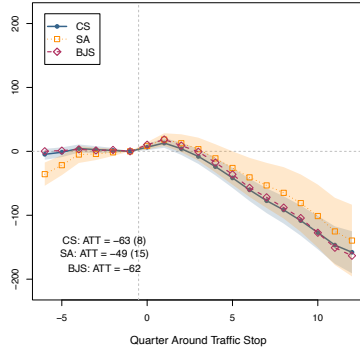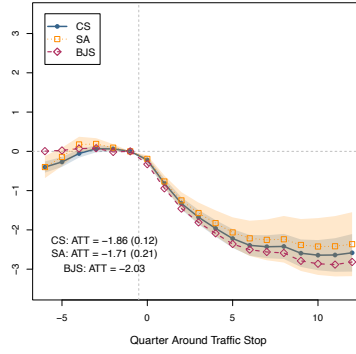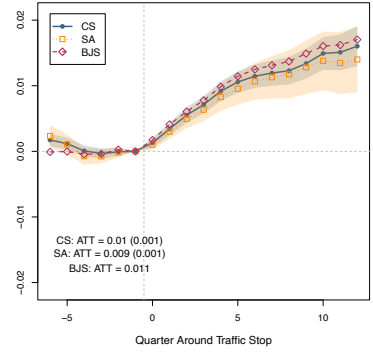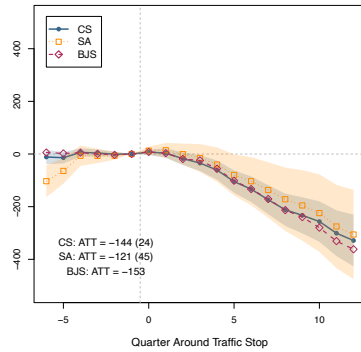

(g) Borrowing Limit   (h) Payroll Employment

Notes: Each figure reports the baseline event study estimate (blue circles) as well as event study estimates which are estimated within predicted treatment timing groups ("cohorts"). To predict cohorts, I regress the observed cohort on gender, race, a quadratic in baseline age, log baseline estimated income, the interaction of indicators for whether an individual has an auto loan at baseline (2010Q1) and in 2010Q4, and the interaction of indicators for deciles of baseline credit score and deciles of the change in credit score between 2010Q1 and 2010Q4. I compute predicted values from this regression, bin individuals into deciles, and estimate event studies separately for each decile and aggregate up the estimates, weighting by sample shares.

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Figure A-8: Event study estimates with cohort-level reweighting

(a) Any New Default
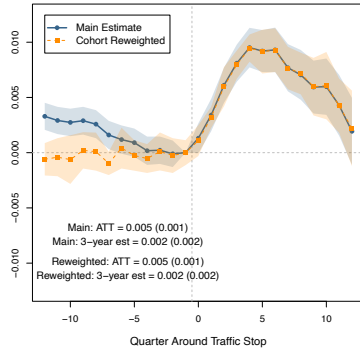
(b) Collections Balances
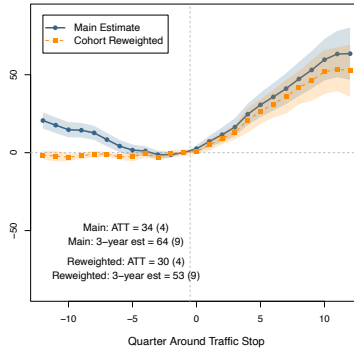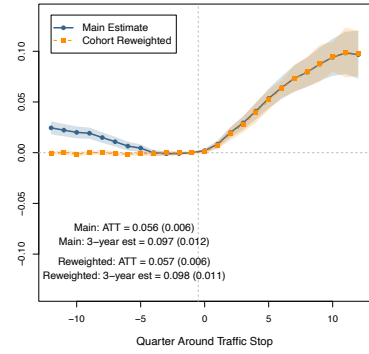
(c) Delinquencies

(d) Revolving Balances

(e) Credit Score

(f) Subprime

(g) Borrowing Limit

(h) Payroll Employment

Notes: Each figure reports the baseline event study estimate (blue circles) as well as event study estimates which reweight each "cohort" (treatment timing group) to match the baseline characteristics (age quntiles × gender × race × above/below median estimated income × above/below median credit score) of the overall sample.

Figure A-9: Event study estimates via alternative estimation methods

(a) Any New Default

(b) Collections Balances

(c) Delinquencies

(d) Revolving Balances
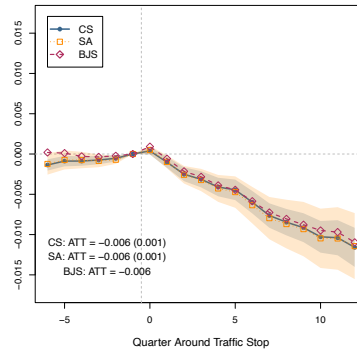

(e) Credit Score

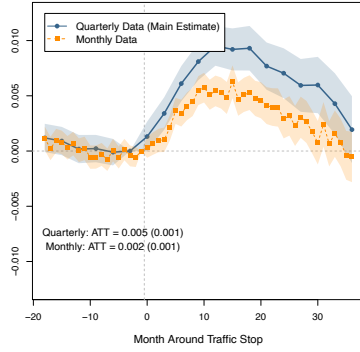### (f) Subprime

(g) Borrowing Limit
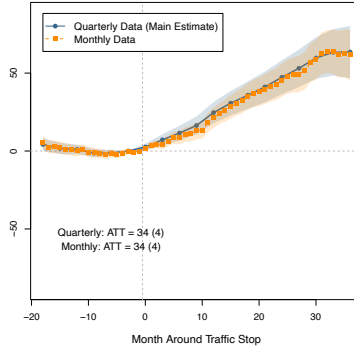

(h) Payroll Employment

Notes: 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. (2024) (red diamonds). As described appendix E-5, computing constraints prevent the calculation of standard errors for the Borusyak et al. (2024) approach.

Figure A-10: Event study estimates using monthly data

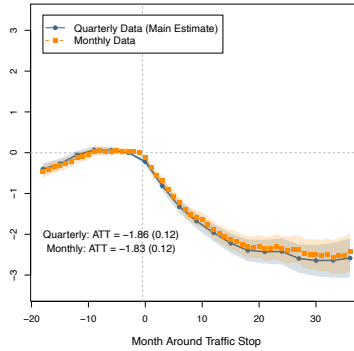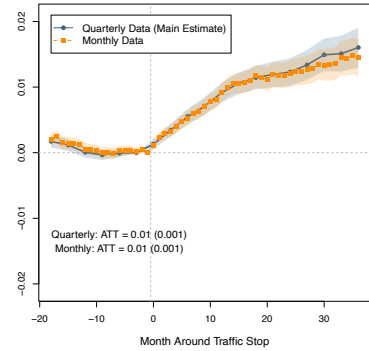### (a) Any New Default

(b) Collections Balances

(c) Delinquencies

(d) Revolving Balances

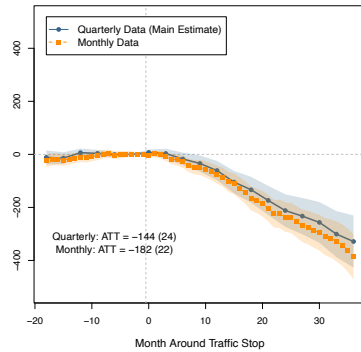
(e) Credit Score

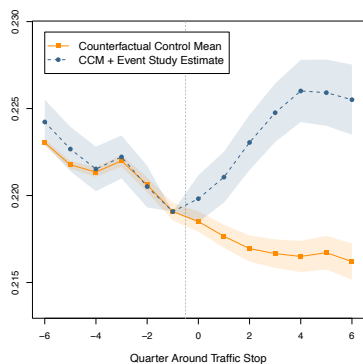### (f) Subprime
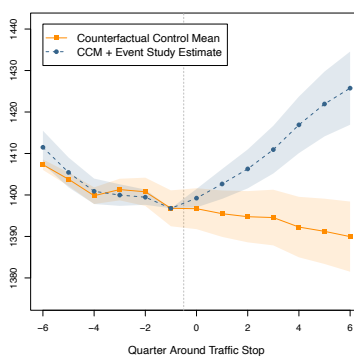

(g) Borrowing Limit

(h) Payroll Employment

Notes: Each figure plots the baseline event study estimate (blue circles) which rely on data aggregated to the quarterly level as well event study estimates based instead on monthly data (orange squares). Note that because *any new default* is a flow measure defined as the maximum over the months in a given quarter, we should expect the quarterly event study estimates to be about three times larger than the monthly estimates. The average event study estimate over the first six post-stop quarters using the quarterly and monthly data are 0.0067 and 0.0031, respectively.

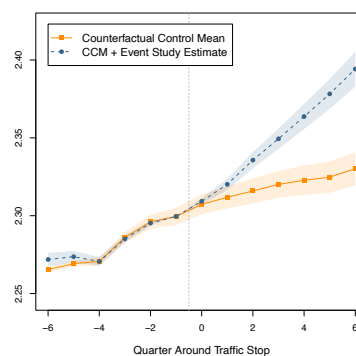Figure A-11: Event study estimates relative to counterfactual control means
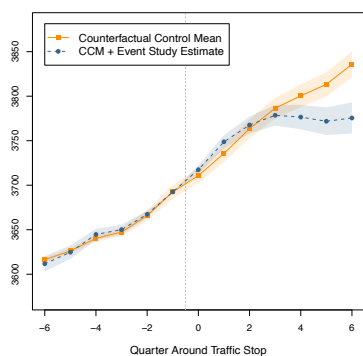
(a) Any New Default

(b) Collections Balances

(c) Delinquencies

(d) Revolving Balances

(e) Credit Score
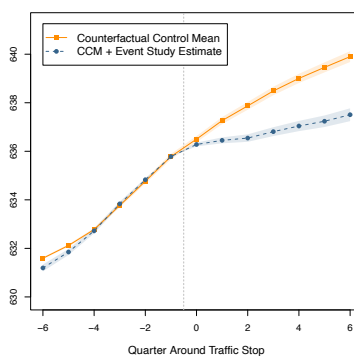
(f) Subprime
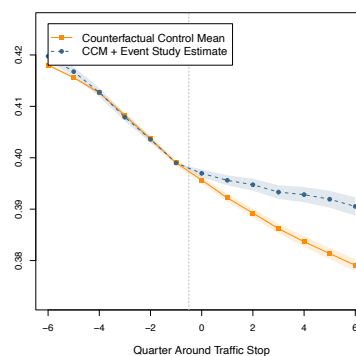
(g) Borrowing Limit
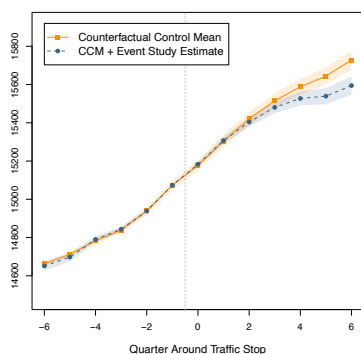
(h) Payroll Employment
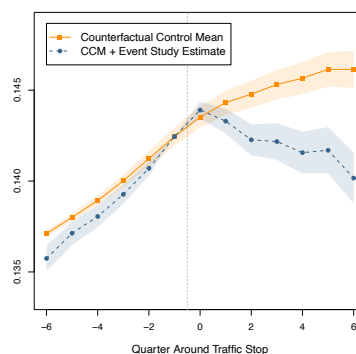
Notes: Each figure plots the time path of estimated counterfactual means (orange squares) and the estimated counterfactual means plus the event study estimates (blue circles) using the full event study sample ($N = 525,646$). Counterfactual means are estimated using the method described in the text and 95 percent confidence bands for the estimated counterfactual means are obtained via a Bayesian bootstrap clustered at the motorist-level.

Figure A-12: Event study estimates for other long-run outcomes

(a) Any Open Credit Account
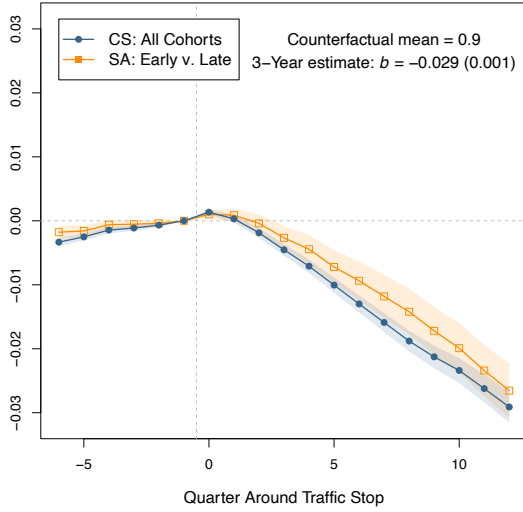
(b) Any Mortgage
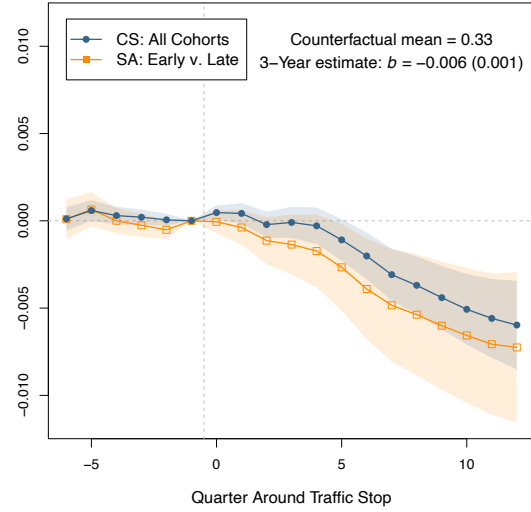
(c) Any Bankruptcy to Date
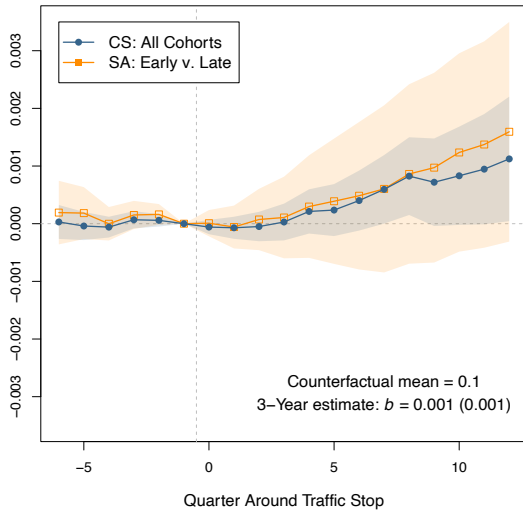
(d) New Address
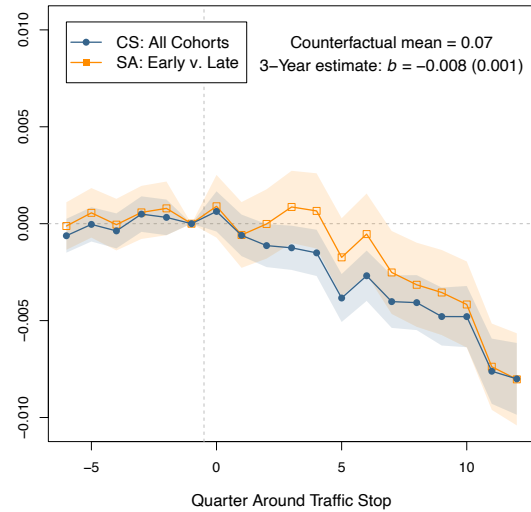
*N*otes: This figure reports event study estimates obtained from the primary Callaway & Sant'Anna (2021) specification (blue circles) as well as from a specification based on Sun & Abraham (2021) which compares only those cited in 2011–2012 ("early" cohorts) to those cited in 2015Q4 ("late" cohort). 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-13: Understanding heterogeneous effects on credit card outcomes

(a) Credit scores and limits   (b) Default and credit scores   (c) Default and credit limits
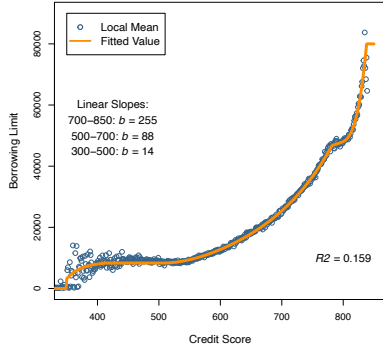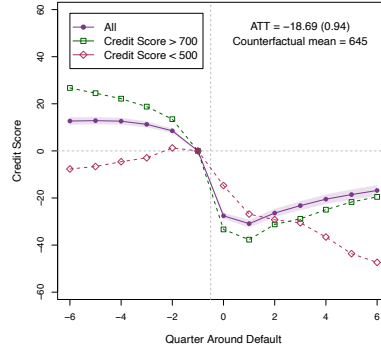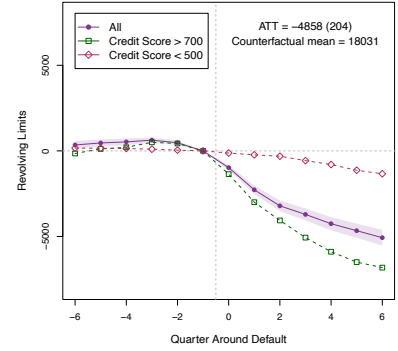
(d) Fines and credit scores   (e) Fines and credit limits
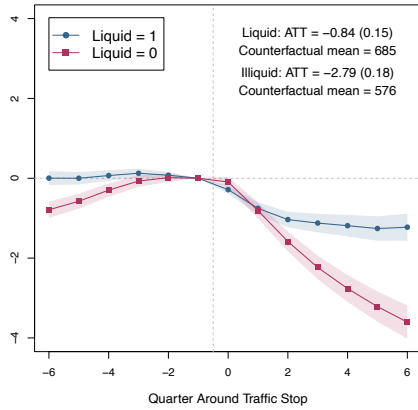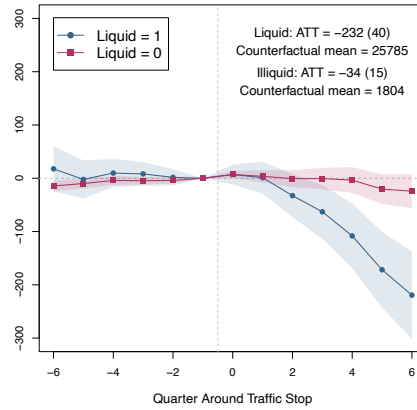
Notes: Panel (a) plots the cross-sectional relationship between credit scores and revolving limits at baseline and reports linear slope estimates over ranges of the credit scores. Panels (b) and (c) report event study estimates around the time of first default for credit scores and revolving limits. These estimates use the subset of the event study sample whose first traffic stop occurs in the final possible quarter and have no default flag on their credit reports at baseline and treats an individual's first default as the event of interest. Estimates are shown for the full sample, as well as those with high (> 700) and low (< 500) credit scores at baseline. Panels (d) and (e) report estimates which are identical to figure 3 in the main text for credit scores and revolving limits.

Figure A-14: Event study estimates on extensive and intensive margins

(a) Any Default to Date

(b) Collections by Baseline Default
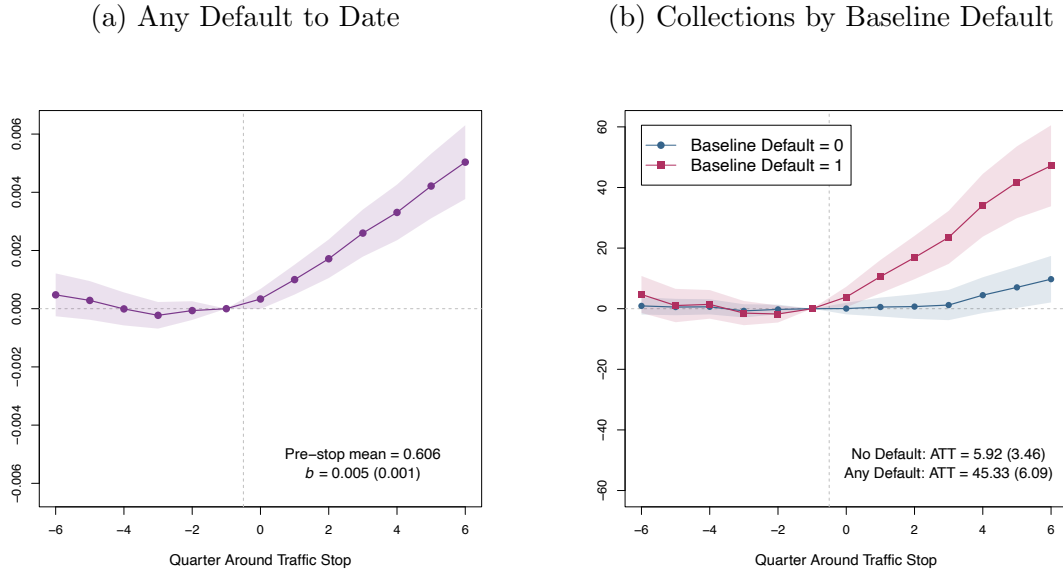
*N*otes: Each figure reports event study estimates for the denoted outcome. In panel (a), the outcome is an indicator for whether an individual has accumulated any new default since the start of the sample for the full event study sample ($N = 525,646$); once this variable has switched to one, it remains 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: $N = 324,576$).

Figure A-15: Transfers and financial fragility in the European Union
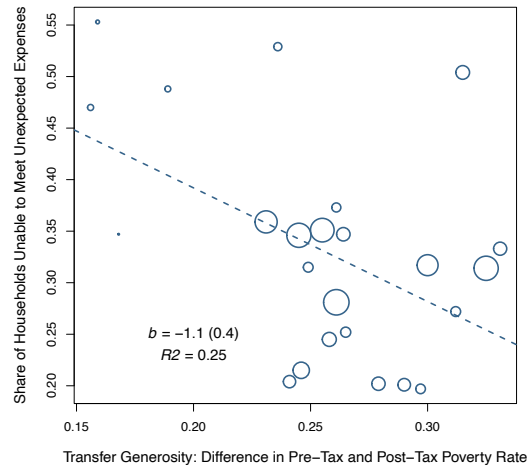
*N*otes: This figure illustrates the relationship between redistribution and financial fragility. Each circle is an EU country using the same list of countries shown in table A-1, with the size of the circle proportional to the country's population. The horizontal axis is a summary measure of transfer generosity, computed as the country's poverty rate before taxes minus the country's poverty rate after taxes (so a higher value indicates that a higher share of the population is moved above the poverty line by the transfer system). The vertical axis is the share of households indicating an inability to cover unexpected expenses from the EU-SILC. Another way to note this relationship is to regress the fragile share on both the pre- and post-tax poverty rates. In this regression, the coefficient on the pre-tax poverty rate is insignificant, while the coefficient on the post-tax poverty rate is 10 times larger and statistically significant.

Table A-1: Financial fragility in the U.S. and the E.U.

| | Median Income (1) | Poverty Rate (2) | Fragility Survey | | Speeding Fines | | |
| | | | Expense Shock (3) | Fragile Share (4) | Fine (5) | Fine as share of: | |
						Monthly Income (6)	Expense Shock (7)
Florida	36629	0.14	–	–	198	0.06	0.5
United States	40430	0.13	400	0.41	131	0.04	0.33
European Union (25 Countries)							
Albania	2163	0.23	101	0.47	22	0.12	0.21
Austria	27266	0.14	1283	0.2	54	0.02	0.04
Belgium	25652	0.16	1207	0.24	166	0.08	0.14
Croatia	7212	0.19	310	0.53	109	0.18	0.35
Denmark	32603	0.13	1552	0.25	328	0.12	0.21
Estonia	11397	0.22	468	0.35	325	0.34	0.69
Finland	26581	0.12	1281	0.27	310	0.14	0.24
France	24064	0.13	1176	0.31	97	0.05	0.08
Germany	24598	0.16	1152	0.28	81	0.04	0.07
Greece	8516	0.18	406	0.5	108	0.15	0.27
Hungary	5874	0.13	258	0.33	87	0.18	0.34
Ireland	26988	0.15	1221	0.37	97	0.04	0.08
Italy	18242	0.2	880	0.35	155	0.1	0.18
Latvia	7942	0.23	345	0.55	115	0.17	0.33
Lithuania	7467	0.23	306	0.49	62	0.1	0.2
Luxembourg	37333	0.17	1778	0.2	53	0.02	0.03
Netherlands	26009	0.13	1231	0.22	212	0.1	0.17
Norway	42711	0.13	2143	0.2	770	0.22	0.36
Poland	7120	0.15	319	0.32	52	0.09	0.16
Portugal	10122	0.17	476	0.35	195	0.23	0.41
Slovakia	8081	0.12	376	0.32	65	0.1	0.17
Spain	16012	0.22	741	0.36	108	0.08	0.15
Sweden	27660	0.16	1363	0.2	349	0.15	0.26
Switzerland	46583	0.15	2396	0.2	482	0.12	0.2
United Kingdom	23246	0.19	1145	0.35	103	0.05	0.09
Average	20601	0.17	987	0.32	131	0.08	0.14

*N*otes: All data is from 2018. EU sample is 25 countries for which I was able to obtain information on typical speeding fines and with non-missing information on the share of households reporting inability to cover emergency expenses. Columns 1 and 2 report median incomes and poverty rates. Columns 3 and 4 report results from financial fragility survey (SHED for the US; EU-SILC for the EU); column 3 reports the size of the unexpected expense and column 4 reports the share of households unable to meet that expense. Column 5 reports the average speeding fine, and columns 6-7 report the average speeding fine as fraction of median monthly income and the expense shock from the fragility survey, respectively. Median incomes, expense shocks, and speeding fines are reported in USD. Data on the U.S. is drawn from FRED and the U.S. Census Bureau. Data for the E.U. is from Eurostat and from speedingeurope.com.

B 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

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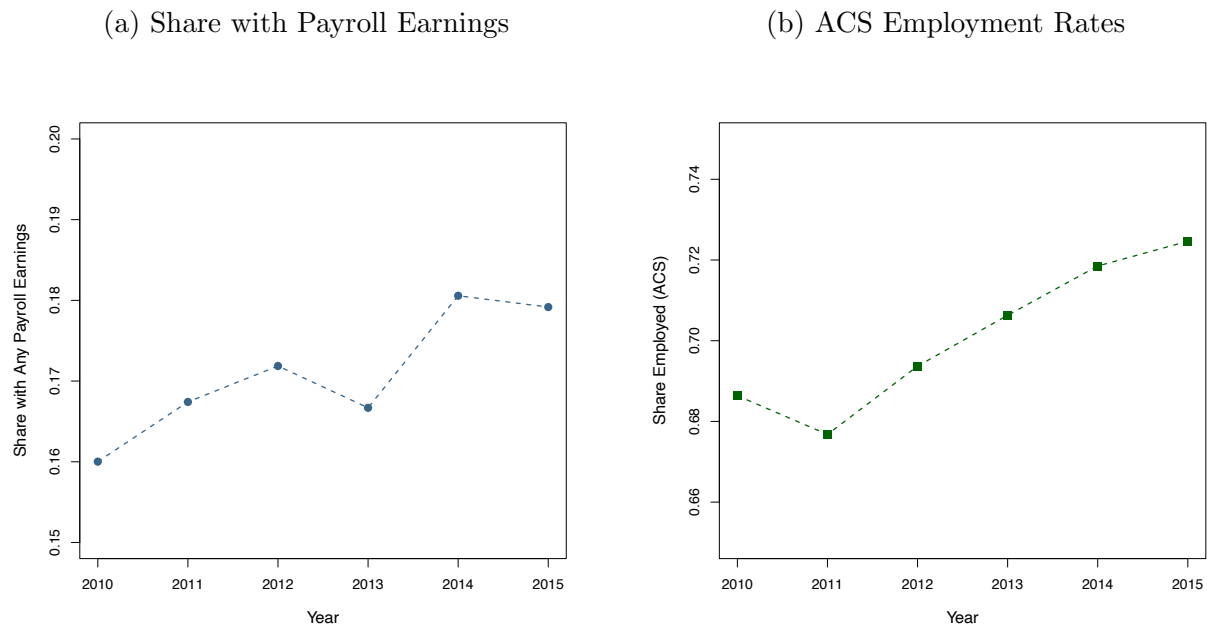
Notes: Panel (a) plots the share of the event study sample with any payroll earnings at some point during each year and panel (b) plots ACS employment rates, which are computed using the 1% microdata samples (Ruggles, 2023), reweighting the ACS sample based on age, gender, and race to match the characteristics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

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

(a) Any New Default
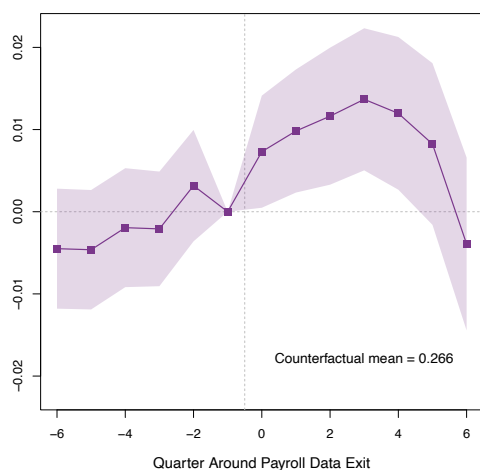
(b) Collections Balances
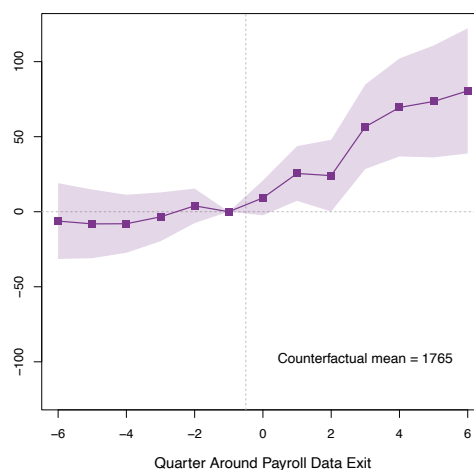
(c) Credit Line Delinquencies
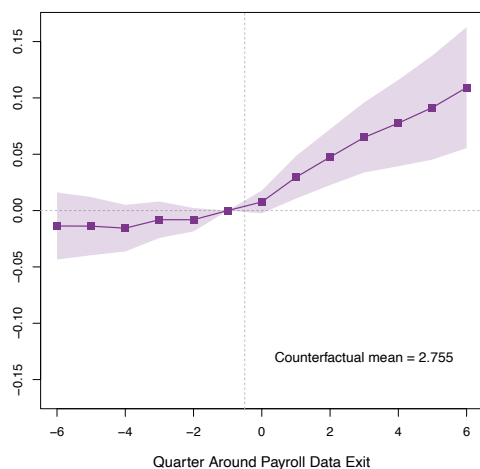
(d) Credit Score
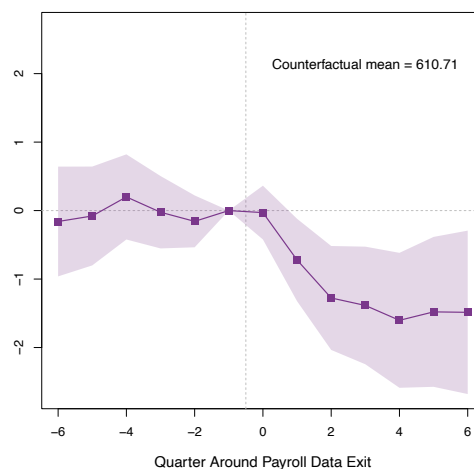
Notes: This figure presents event study estimates of the effect of "separating" from a job in the payroll records. To construct the sample, I first take the subset of all drivers on file that receive their first citation in 2015 and use only data from pre-2015. I define the event as transitioning from 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
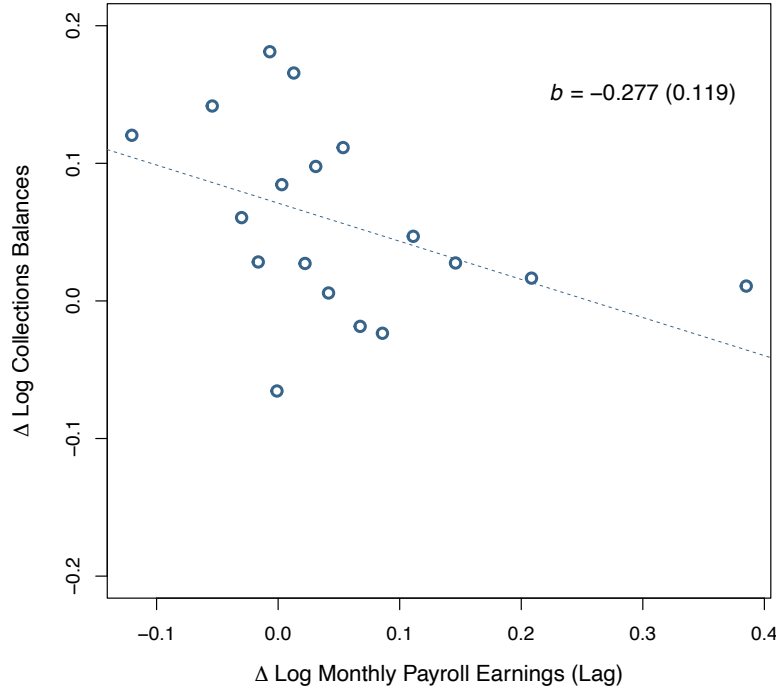
Notes: This figure uses the subset of the event study sample that is continuously employed in the payroll records over the first two years of the data (2010Q1 through 2011Q4; $N = 18,512$). The horizontal axis is the log change in an individual's payroll earnings between 2010Q1 and 2011Q1, computed as $\log(\text{earnings}_{i,2011Q1}) - \log(\text{earnings}_{i,2010Q1})$. The vertical axis is the log change in an individual's collections balances between 2010Q4 and 2011Q4, computed as $\log(\text{balance}_{i,2011Q4} + 1) - \log(\text{balance}_{i,2010Q4} + 1)$, where the ones are added to retain those with zero collections balances in either period. The figure depicts means for twenty quantile bins of the log change in earnings and reports the coefficient and robust standard error from the corresponding linear regression.

B-1   Back of the envelope calculations

Earnings benchmark for impacts of fines on payroll employment

Based on table B-1, the average annual earnings for employed individuals in my sample of traffic offenders is \$37,122. Average earnings for those employed in the payroll records is \$45,453. The implied share of employed individuals who are employed in the payroll records as opposed to not in the payroll records is $p = 0.16/0.68 = 0.23$. Hence, the average annual earnings for those who are employed outside the payroll records, $\bar{y}_{np}$, is given by:

$$37,122 = (1 - 0.23) \times \bar{y}_{np} + 0.23 \times 46,453$$

which implies that $\bar{y}_{np} = \$34,334$.

Individuals employed in the payroll records are employed for sure, whereas some share of individuals who are not in the payroll records are unemployed. Based on table B-1, the

overall employment rate in my sample is expected to be 0.68. If 16 percent are employed in the payroll records, the employment rate of those not in the payroll records e_{np} is given by:

$$0.68 = 0.16 \times 1 \ + (1 - 0.16) \times e_{np}$$

which implies that $e_{np} = 0.62$.

Putting the above two calculations together, I can compute the expected annual earnings for an individual who is not employed in the payroll records as $0.62 \times \$34,334 = \$21,287$. Hence, the expected effect of being employed in the payroll records on annual earnings is given by $\$46,453 - \$21,287 = \$25,166$. The estimated effect on the probability of payroll employment reported in figure 5 is -0.012, so the implied effect on annual earnings is given by $-0.012 \times \$25,166 = -\$302$.

Estimated impact of fines on non-employment

Figure 6 reports ATT estimates on payroll employment for the subsamples who are employed in the payroll records at baseline (10.5%) and not employed in the payroll records at baseline (89.5%). Focusing first on the impacts for those in the payroll records (ATT $= -0.007$), I use a CPS benchmark to quantify the expected share of transitions out of the payroll records which are to non-employment as opposed to employment outside the payroll records. Fujita et al. (2020) show that, during my sample period, the probability of a transition from employment at employer j in month t to employment at employer j' in month $t + 1$ is 2.25%. The probability of a transition from employment in month t to non-employment (unemployment or not in the labor force) in month $t + 1$ is 3.8%. So the share of transitions from employment which are to non-employment and employment at another employer are 60% and 40%, respectively. If transitions out of the payroll records follow the same pattern as the average such transition in the CPS, then, the implied impact of fines on employment for those employed in the payroll records at baseline is $0.6 \times -0.007 = -0.0042$.

Next, focusing on the impacts for those not in the payroll records (ATT $= -0.005$), the key question for quantifying the implied effect on non-employment is the fraction of these abated transitions into the payroll records which are transitions from non-employment as opposed to transitions from employment. Following from the above discussion, the estimated employment rate among those not in the payroll records in a given period is 0.62. Assuming the abated transitions are drawn at random, then, 62% of the prevented transitions are transitions from employment elsewhere to employment in the payroll records and 38% of the prevented transitions are transitions from non-employment into the payroll records. So the implied effect of fines on employment for those not in the payroll records at baseline is given by $0.38 \times -0.005 = -0.0019$.

The implied overall effect on non-employment, then, is the weighted average of the implied effect for the two groups, weighted by the groups' sample shares, which is $(0.895 \times -0.0019) + (0.105 \times -0.0042) = -0.002$, or a 0.2 percentage point increase in non-employment.

The above analysis is based on the static ATT estimates reported in figure 6. To obtain an analogous estimate which corresponds to the larger "longer-run" 12-quarter estimate, which implies a 1.2 percentage point reduction in the likelihood of payroll employment, I repeat the above calculation replacing the ATT estimate for each group with the 12-quarter estimate for the groups that are employed and not employed in the payroll records at baseline. These

estimates are -0.012 ($se = 0.005$) and -0.0093 ($se = 0.001$), respectively. So, for those employed in the payroll records at baseline, the implied longer-run impact on employment is $0.6 \times -0.012 = -0.0072$. The figure for those not employed in the payroll records at baseline is $0.38 \times -0.0093 = -0.0035$. Hence, the implied longer-run effect on the employment versus non-employment margin is $(0.895 \times -0.0035) + (0.105 \times -0.0072) = -0.0039$, or an implied 0.4 percentage point increase in non-employment.

B-2 Additional evidence on mechanisms

Table B-2: Relationship between group-level impacts on medium-term credit report outcomes and longer-term impacts on payroll employment

	Standardized group-level treatment effect at $\tau = 12$:					
	(1) Employment	(2) Employment	(3) Employment	(4) Employment	(5) Employment	(6) Employment
Standardized group-level treatment effect at $\tau = 6$:						
Collections	-0.603					-0.475
	(0.248)					(0.3)
Credit Score		0.517				0.021
		(0.217)				(0.274)
Any Mortgage			-0.892			-0.953
			(0.4)			(0.404)
New Address				-0.16		-0.065
				(0.215)		(0.204)
Any Auto*					0.43	0.331
					(0.202)	(0.204)
N	40	40	40	40	40	40

Notes: This table reports the relationship between medium-term impacts on credit bureau outcomes and longer-term impacts on payroll employment across demographic groups. I divide the sample into 40 cells based on quintiles of baseline age, gender, race (white or nonwhite), and baseline estimated income (above or below median) and estimate event studies separately for each cell. I then regress the event study estimate for payroll employment at $\tau = 12$ on the event study estimate for the denoted outcome y at $\tau = 6$. For comparability, I rescale each event study estimate into standard deviation units by dividing by the outcome standard deviation at baseline. Each regression is weighted by the inverse variance of the estimated effect on payroll employment.
*Due to the pretrend in auto loans shown in figure A-5, I adjust the six quarter event study estimate for auto loans by subtracting the estimate at six quarters *prior* to the citation. In the full sample, this approach yields an estimated 1.5 percentage point decline in car ownership. Note that this decline is similar to what would be predicted by the estimated six-quarter decline in credit scores (-2.4). Specifically, Dobbie et al. (2020) and Dobbie et al. (2017) find that chapter 13 bankruptcy protection is associated with a 9.8 point increase in credit scores and a 4.6 percentage point increase in the likelihood of having an auto loan.

C Traffic court dispositions and estimating the first stage

C-1 Discussion of dispositions data

Table C-1 below shows the distribution of dispositions in the event study sample:

Table C-1: Distribution of traffic court dispositions

| Disposition	N	Fraction
Missing	9,653	0.018
1 = *guilty*	31,344	0.06
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	

Table E-2 in the supplementary appendix 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 C-1 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.

Figure C-1: Future DL suspension offenses by disposition verdict
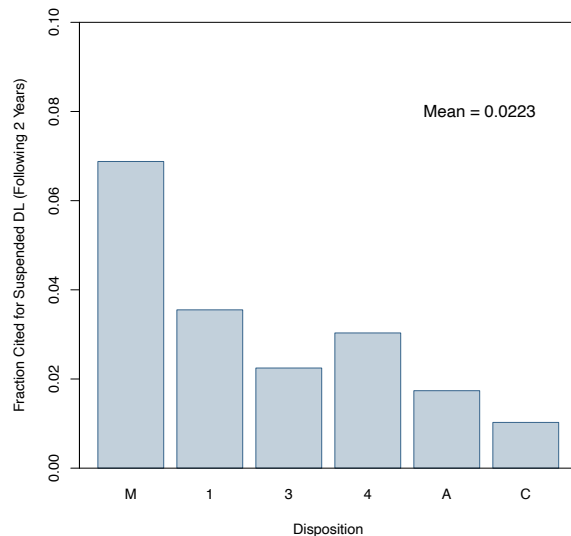
Notes: This figure plots the share of citations in the event study sample ($N = 525,646$) where the motorist is cited for a driving with a suspended driver license in the following two years, by the disposition verdict associated with the citation in the event study sample.

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 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.

C-2   Estimating citation costs

C-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 C-1 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 C-2: Estimating paid fines

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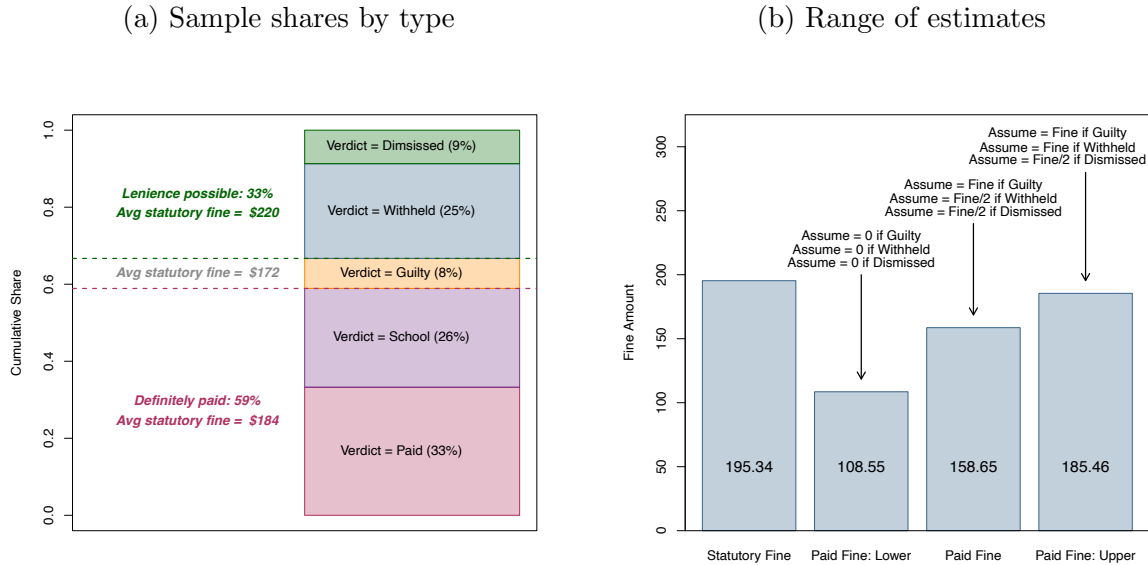
Notes: Panel (a) illustrates the distribution of dispositions, characterized by fine payment status. For ease of exposition, I pool those with missing verdicts (1.84 percent of the sample) together with those with guilty verdicts (5.96 percent of the sample). Panel (b) depicts the sample average estimated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

C-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,

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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 C-3: Estimating paid court fees

(a) Sample shares by type

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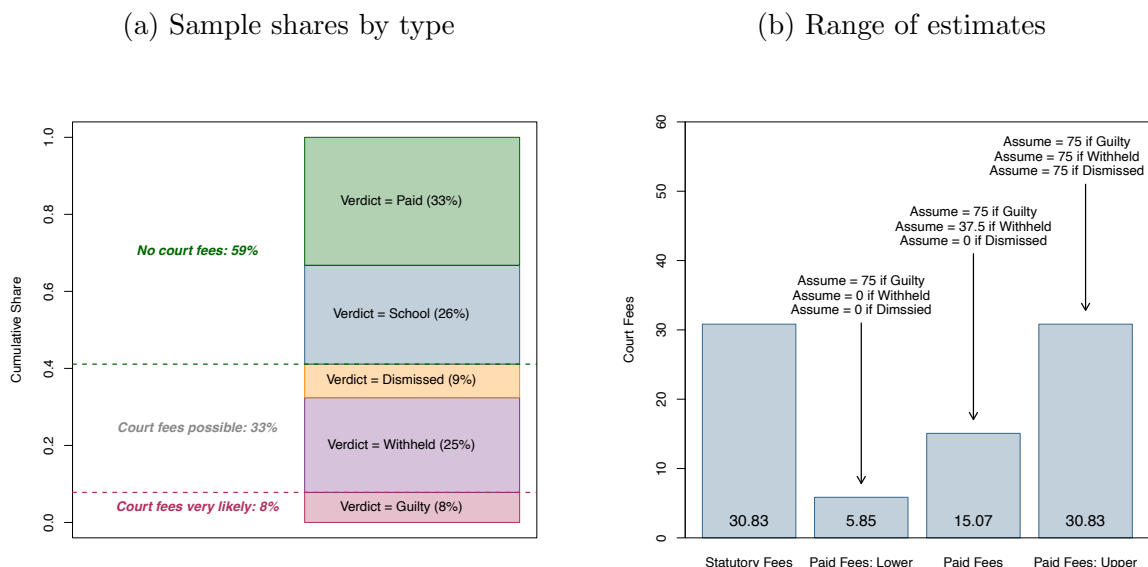
*N*otes: Panel (a) illustrates the distribution of dispositions, characterized by court fee status. For ease of exposition, I pool those with missing verdicts (1.84 percent of the sample) together with those with guilty verdicts (5.96 percent of the sample). Panel (b) depicts the sample average estimated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

C-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 = $(0.29 \times 0.11 \times \$3,183) + (0.13 \times 0.12 \times \$3,183) = \$93$. 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).

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[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.

Figure C-4: Estimating insurance cost increases

(a) Sample shares by type
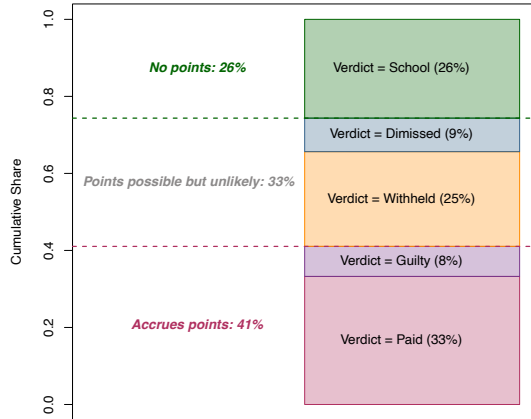
(b) Range of estimates: accrued DL points
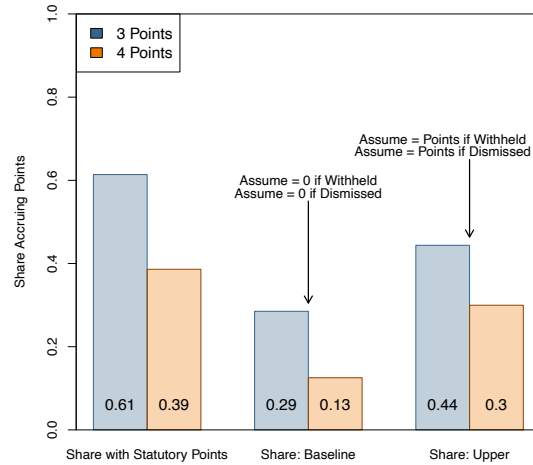
(c) Range of estimates: insurance increases
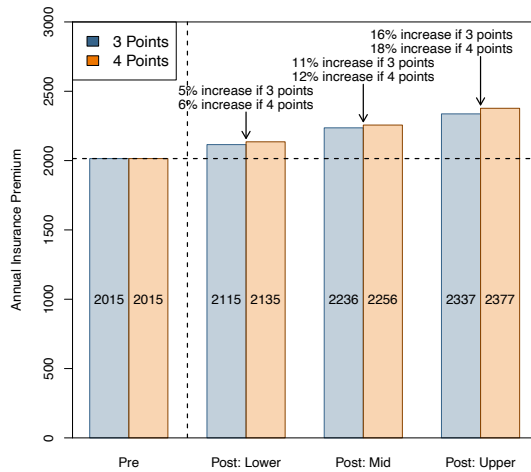
(d) Range of estimates: combined
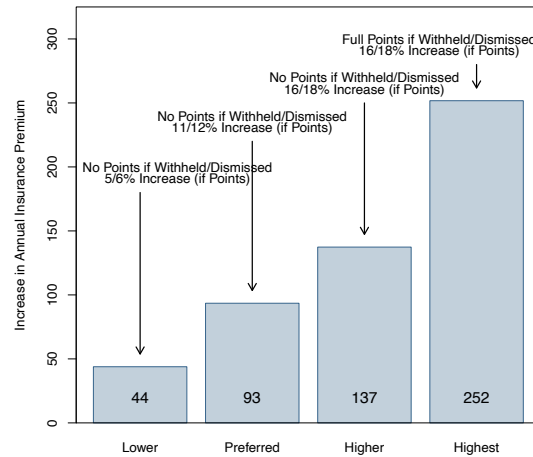
Notes: Panel (a) illustrates the distribution of dispositions, characterized by DL points status. For ease of exposition, I pool those with missing verdicts (1.84 percent of the sample) together with those with guilty verdicts (5.96 percent of the sample). Panel (b) plots the range of estimates of the 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-2.4 Accounting for follow-up traffic citations

Figure C-5: Event study estimates for cumulative traffic stops

(a) Cumulative Stops   (b) Cumulative Paid Fines
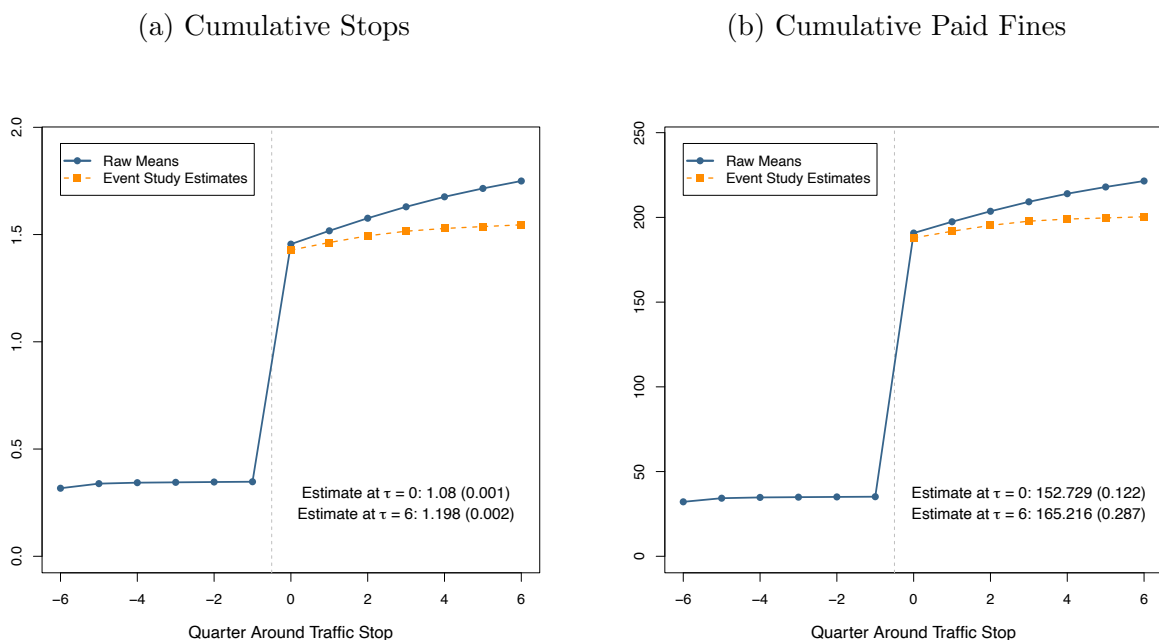
Notes: This figure reports raw means in event time (blue circles) and event study estimates (orange squares) where the outcome is the cumulative number of traffic stops (left panel) and cumulative fines paid (right panel), where fines paid are computed using the same method to adjust for post-citation choices of motorists described in appendix C. Each panel reports the event study estimate at $\tau = 0$ and $\tau = 6$, and I use the difference in these two estimates to adjust the total cost of citations estimate for future citations.

D   Estimating the affected share lower bound

D-1   Standard error for conservative bound

The conservative lower bound is given by $\underline{\pi} = 33.91/315 = 0.108$. The numerator, which is the sample average treatment effect, is measured with error as summarized by its standard error. The denominator, which is my estimate of the total costs of citations, is also estimated as described in appendix C.

   To compute a standard error, I assume that the upper bound estimate of total citation costs reported in figure 7 corresponds to the 99.9 upper confidence bound of the total cost estimate. Adding \$13 to both the mean and the upper bound to account for future citations, then, the implied standard error on the \$315 estimate is given by $315 + 3.291\sigma = 572$, which implies that $\sigma = 79$. I then compute a delta method standard error for $\underline{\pi}$ given by:

$$Var(\underline{\pi}) = Var(\theta/\bar{\Delta}) = (1/\bar{\Delta})^2 \cdot Var(\theta) + (\theta/\bar{\Delta}^2)^2 \cdot Var(\bar{\Delta})$$

Note that I use the same delta method approach for computing standard errors on $\underline{\pi}$ based on the estimated upper bounds described below.

D-2   Extrapolation approach for estimating $\bar{\Delta}$

Let $z_i = Pr(\Delta_{j \neq i} < \Delta_i)$, in other words, individual i's percentile in the treatment effect distribution. The goal is to estimate $\bar{\Delta} = E(\Delta_i | z_i = 1)$.

   Note that even if z_i were observed, this quantity is not generally estimable because treatment effects cannot be estimated for a single individual. However, we could estimate this quantity by assuming that $\Delta_i = f(z_i) + \epsilon_i$, with $f(\cdot)$ weakly increasing and strictly continuous, estimating the function $f(\cdot)$, e.g., by estimating treatment effects for discrete bins of z_i, and then computing $E(f(1))$. In other words, we could extrapolate the relationship between z_i and treatment effects to the implied effect when $z_i = 1$.

   Of course, z_i is not observed, so I implement a procedure to estimate z_i, the idea of which is to use variation in treatment effects across covariate groups in one half of the data (i.e., a holdout sample) to predict treatment effect heterogeneity in the other half. The full procedure for estimating $\bar{\Delta}$ is as follows:

1. Define covariate cells X_j at the level of above/below median age, gender, race (white or nonwhite) and above/below median baseline estimated income, baseline credit score, baseline collections balances, and baseline available balances on credit cards. These are important dimensions of heterogeneity highlighted in figures 4 and A-14.

2. Within covariate cells, randomly divide the data into two partitions.

3. Using only partition 1, estimate a treatment effect for each covariate cell, $\hat{\theta}_j^1$.

4. Using only partition 1, regress cell-level treatment effects $\hat{\theta}_j^1$ on cell-level average covariates $\bar{X}_j$ (same as those used to construct the cells), weighting by the inverse variance of the estimated $\hat{\theta}_j^1$.

5. Using the regression coefficients from step (4), construct predicted treatment effects for each individual in partition 2. Individual i's percentile in the distribution of predicted treatment effects is the estimate of z_i. Note that this step generates continuous variation in predicted treatment effects beyond the discrete cell-level variation, with the continuous variation generated by the regression estimates in step 4.

6. In partition 2, estimate a treatment effect by decile of predicted treatment effects.

7. Repeat steps (3)-(6) swapping partitions (i.e., now use heterogeneity in partition 2 to predict treatment effect heterogeneity in partition 1). So now we have decile-specific treatment effect estimates for each partition.

8. Average the decile-specific treatment effect estimates from the two partitions to obtain overall decile-specific estimates.

9. Assuming that each decile specific estimate is the estimate for the midpoint of the decile, fit a quadratic of the decile-specific estimates to the percentiles and compute the expected value at the 100th percentile, which is an estimate of $\bar{\Delta}$.

To compute a standard error, I bootstrap the entire procedure, clustering at the individual-level. To compute a standard error on the implied $\underline{\pi}$, I use the delta method as above. Results from this procedure are depicted below in figure D-1:

Figure D-1: Extrapolation-based estimate of $\bar{\Delta}$
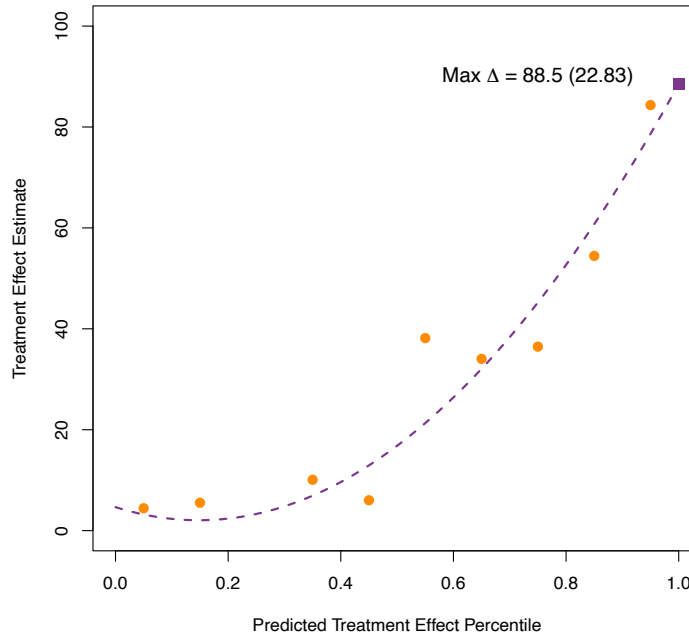
Notes: This figure depicts the extrapolation-based approach for estimating $\bar{\Delta}$, the treatment effect upper bound. Orange circles depict treatment effects estimated separately by decile of predicted treatment effects (where the predictions are computed using opposite partitions of the data). Purple line depicts a quadratic fit, and the purple square depicts the extrapolated value at the 100th percentile (reported in the figure).

D-3 Deconvolution approach for estimating $\bar{\Delta}$

Suppose that the treatment effect for individual i can be written as $\Delta_i = f(X_i) + \nu_i$, with $E(\nu_i | X_i = 0)$. In this case, we can think of the goal as to estimate $\bar{\Delta} \equiv \max\{f(X_i)\}$.[17]

Further, suppose that covariates X_i take only a finite set of discrete values, that is, $X_i \in \{X_1, X_2, ..., X_N\}$. This assumption suggests a non-parametric approach for approximating the maximum treatment effect $\bar{\Delta}$ by estimating treatment effects $\hat{\theta}_j$ for each X_j and then computing $\max\{\hat{\theta}_j\}$.

However, an issue confronted by this approach is that the estimated $\hat{\theta}_j$'s will tend to be over-dispersed due to estimation error. Specifically, each $\hat{\theta}_j$ can be written as $\hat{\theta}_j = \theta_j + \epsilon_j$ and this estimation error leads to bias such that $\max\{\hat{\theta}_j\} \neq \max\{\theta_j\}$. To address this issue, I follow Aaronson et al. (2007) and estimate the true variance of θ_j by subtracting the average squared standard error of the individual $\hat{\theta}_j$'s from the variance of the estimated θ_j's:

$$\hat{\sigma}^2 = Var(\hat{\theta}) - \frac{1}{N}\sum_j Var(\hat{\theta}_j)$$

Based on this estimated true variance of θ, I estimate the upper bound on treatment effects using extreme quantiles of the implied (deconvolved) distribution of θ, assuming that these treatment effects follow a normal distribution:

$$\bar{\Delta} = \bar{\theta} + \alpha \cdot \sqrt{\hat{\sigma}^2}$$

where $\alpha = 2.576$ for the 99th percentile or 3.291 for the 99.9th percentile.

In table D-1 below, I report these upper quantile estimates using different sets of baseline observables to create the covariate cells. I also report bootstrapped standard errors for two specifications, one which partitions the sample using age quintiles, gender, race, and above/below median estimated income and credit score (2nd row) and one which uses age terciles, gender, above/below median estimate income and credit score, and quintiles of baseline credit score (7th row).

In the final column of table D-1, I also report the maximum estimate after performing an empirical bayes shrinkage procedure on the estimated $\hat{\theta}_j$'s:

$$\theta_j^{EB} = \lambda_j \hat{\theta}_j + (1 - \lambda_j)\bar{\theta} \quad \text{where } \lambda_j = \frac{\hat{\sigma}^2}{\hat{\sigma}^2 + \hat{\sigma}_j^2}$$

with $\hat{\sigma}^2$ the same estimated variance as above and σ_j^2 is the squared standard error of the estimated θ_j (e.g., Morris 1983; Koedel et al. 2015)

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[17]Note that if we write $\Delta_i = f(X_i) + \nu_i$, the distribution of treatment effects is unbounded whenever the distribution of ν_i is unbounded. This exercise abstracts from this issue by targeting $\max\{f(X_i)\}$, which is an unbiased estimate of the realized maximum in any given finite sample.

Table D-1: Estimating the distribution of treatment effects across covariate cells

	(1) $E(\hat{\theta}_j)$	(2) $Var(\hat{\theta}_j)$	(3) $\widehat{Var(\theta)}$	(4) p99	(5) p99.9	(6) $\max\{\theta_j^{EB}\}$
Demographics x Income (40 Cells)	33	888	230	72	83	49
Demographics x Income x Credit Score (80 Cells)	31	1711	403	83 (18)	97 (23)	50
Demographics x Income x Credit Score x Liquidity (144 Cells)	30	2818	437	84	99	46
Deomgraphics x Income x Credit Score x Collections (116 Cells)	31	2382	324	77	90	47
Demographics x Income x Credit Score x Liquidity x Collections (204 Cells)	30	3816	162	63	72	37
Demographics x Collections Quintiles (60 Cells)	28	1381	506	86	102	53
Demographics x Income x Collections Quintiles (120 Cells)	31	2407	595	94	112	57
Demographics x Income x Credit Score x Collections Quintiles (240 Cells)	32	4473	590	95 (14)	112 (17)	56

Notes: In rows 1-5, demographics refers to cells at the level of age quintiles × gender × race (defined as white or nonwhite). In rows 6-8, demographics refers to cells at the level of age terciles × gender × race (again defined as white or nonwhite). Unless otherwise noted, cells based on non-demographic characteristics refer to binary splits of the sample at the within-demographic group median of the variable of interest at baseline (e.g., above or below median estimated income).

E   Additional data information

E-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 E-1 shows a sample UTC form and figure E-2 provides an example of a completed form.

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

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

Dispositions data

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

1 = guilty; 2 = not guilty; 3 = dismissed; 4 = paid fine or civil penalty; 6 = estreated or forfeited bond; 7 = adjudication witheld (criminal); 8 = nolle prosequi; 9 = adjudged delinquent (juvenile); A = adjudication 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 C 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 E-4, estimated income is highly correlated with both zip code per-capita income and earnings in the payroll 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 B.

E-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 E-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 E-3. Results from regressing a successful credit file match on available driver characteristics are shown in table E-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 (∼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 non-missing credit score and non-missing 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 × quarter level for two reasons. First, aggregating reduces the (already minimal) prevalence of missing values. For example, an individual may have a non-missing 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.

When aggregating continuous variables (e.g, number of collections on file) to the person-quarter level, I take the average of the non-missing 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 non-missing values and impute zero if all values are missing.

E-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.

E-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 E-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 earn-
ings 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 E-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 mea-
sure 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
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 re-
volving accounts and report estimates for this imputed limit in appendix F. As shown in
panel (a) of figure A-13, 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 panel (a) of figure A-13 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.

E-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. (2024) 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. (2024) approach.

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

XXXXXXE

FLORIDA UNIFORM TRAFFIC CITATION

COUNTY OF ___ ☐ (1) FHP ☐ (2) P.D. ☐ (3) S.O. ☐ (4) OTHER
AGENCY NAME ___
CITY OF (IF APPLICABLE) ___
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 THIS 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.

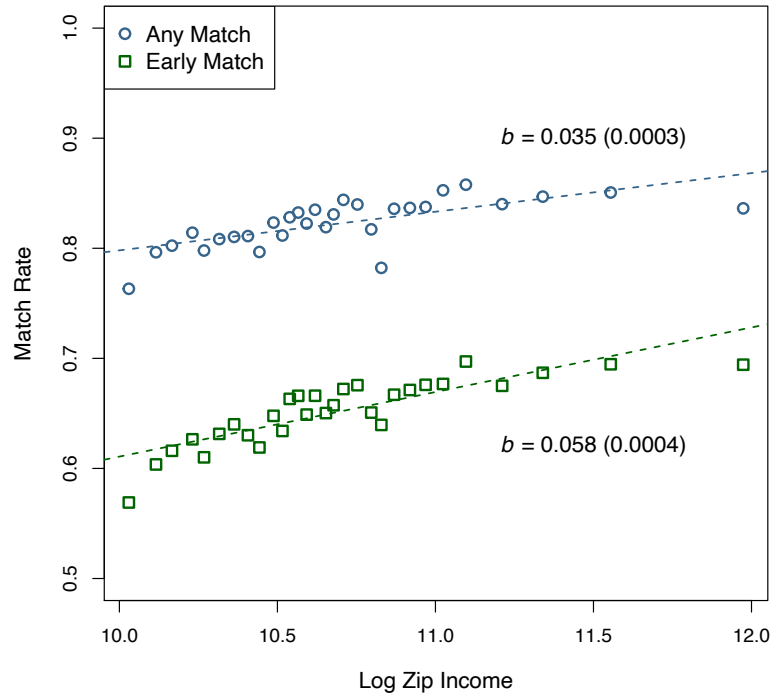**FAULTY EQUIPMENT AFFIDAVIT OF COMPLIANCE**
(Law Enforcement Use Only)

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#)

Figure E-2: Example of completed UTC form

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

Notes: 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 E-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 E-4: Income measures
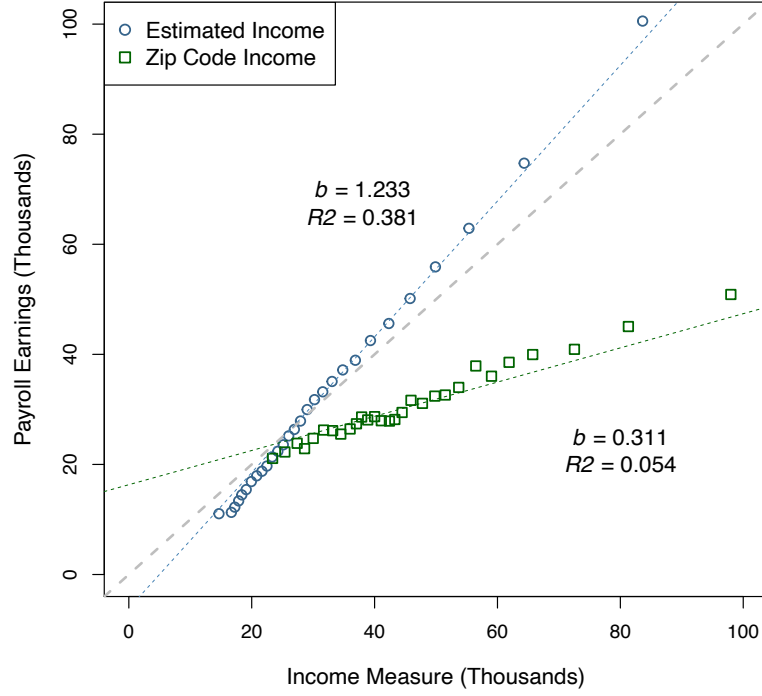
Notes: This figure illustrates the relationship between income measures using the subsample with positive payroll earnings at some point in 2010 ($N = 390,688$). The regression of payroll earnings on both income measures gives $R^2 = 0.388$ with coefficients on credit bureau estimated income and zip code income of 1.191 (0.004) and 0.112 (0.002), respectively.
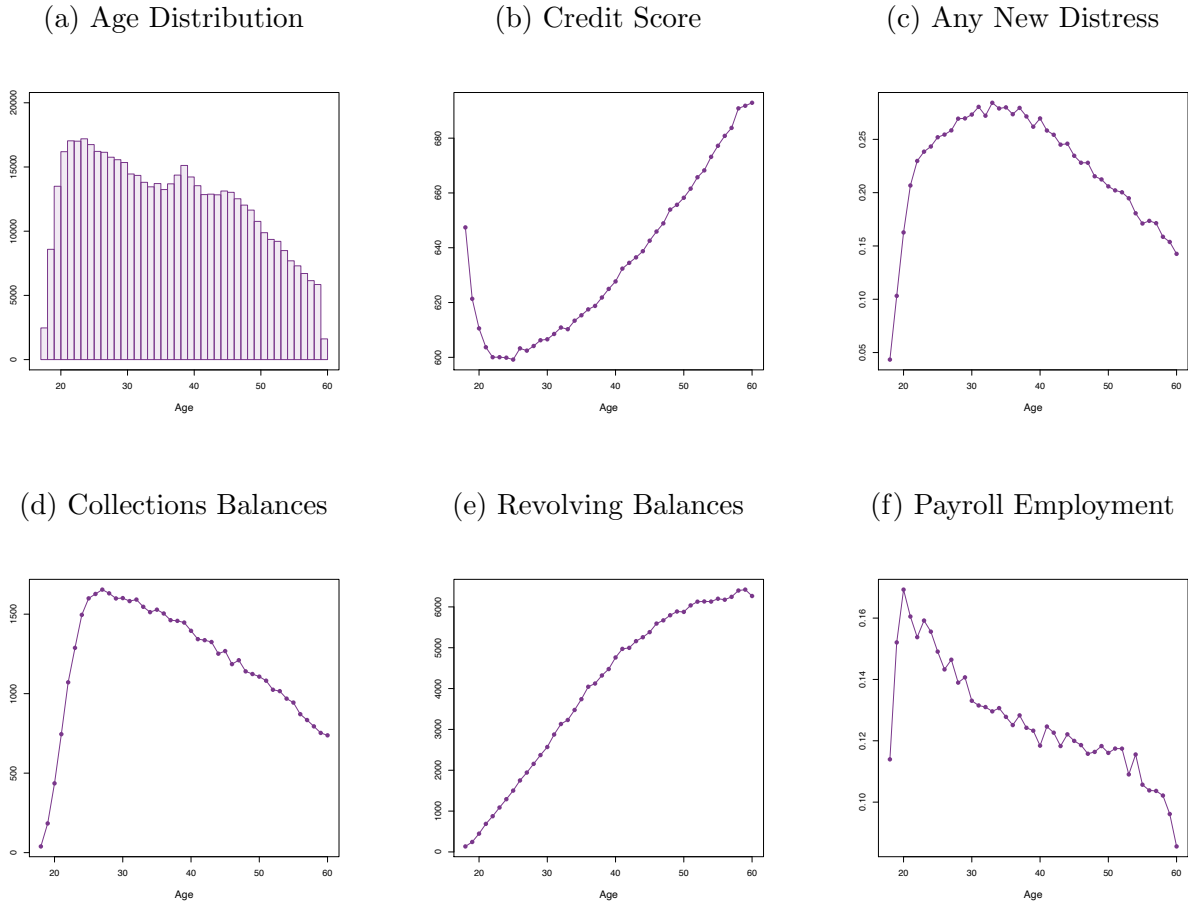
Figure E-5: Distribution of credit scores in event study sample

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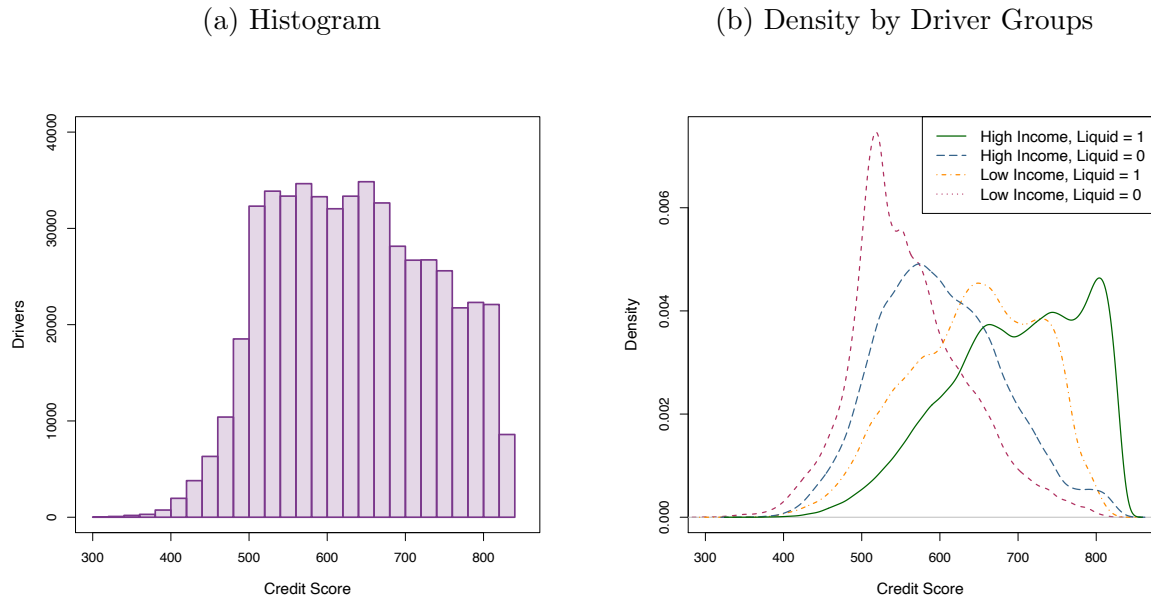
*N*otes: Panel (a) plots the distribution of credit scores in the event study sample as of one year prior to each individual's traffic stop. Panel (b) illustrates kernel density plots of these credit scores broken down by baseline estimated income and baseline liquidity status.

Figure E-6: Age profiles in outcomes of interest

(a) Age Distribution

(b) Credit Score

(c) Any New Distress

(d) Collections Balances

(e) Revolving Balances

(f) Payroll Employment

*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 E-2: Summary Statistics at Baseline by Traffic Court Disposition

	(1) All	Definitely Paid (2) V=4/C	(3) V=4	(4) V=C	(5) V=3/A	(6) 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

Notes: 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.

F   Additional results: event studies

Figure F-1: 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

Notes: 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.

Figure F-2: Event study estimates for counties with available payment plans
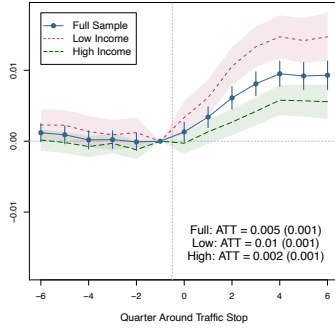
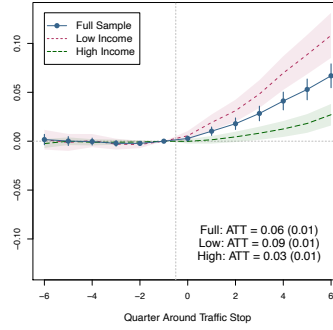(a) Collections Balances
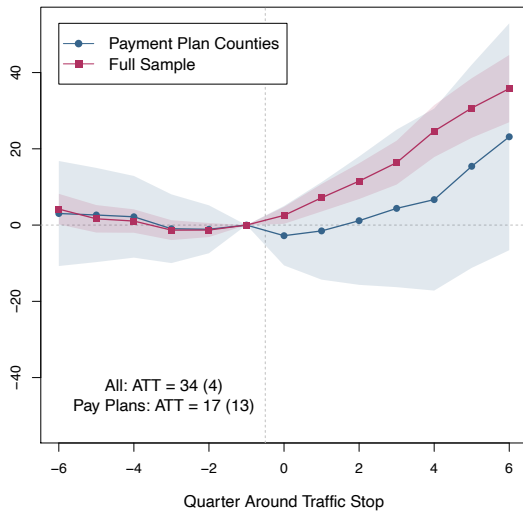
(b) Payroll Employment
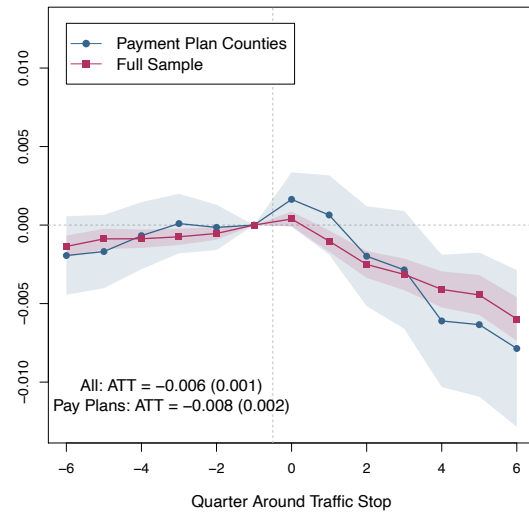
Notes: This figure reports event study estimates for the full sample and for the subset of motorists cited in Pinellas and Hillsborough counties ($N = 43,729$), which offered three month payment plans on traffic fines during the sample period.)

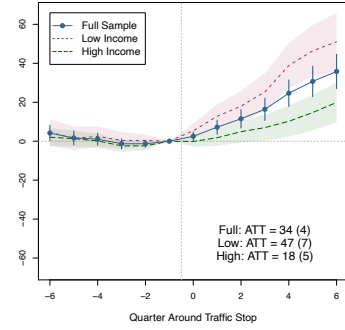Figure F-3: Event study estimates for distress outcomes by baseline income
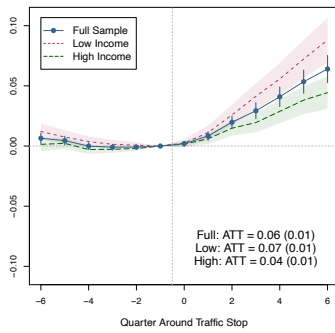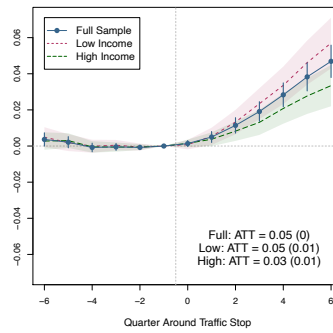
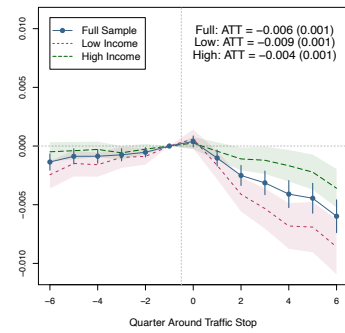(a) Any New Distress

(b) Collections

(c) Collections Balances
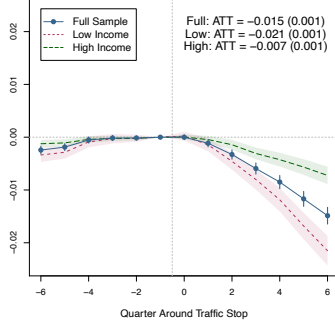

(d) Delinquencies
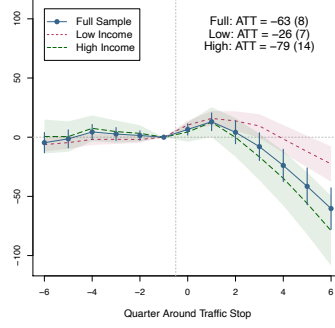

(e) Derogatories

(f) Payroll Employment

Notes: 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 F-4: Event study estimates for credit card outcomes by baseline income
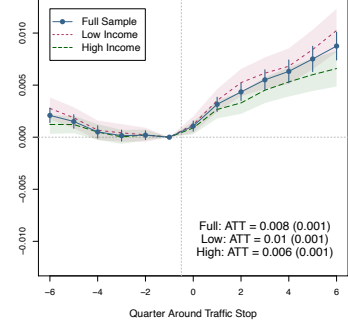
(a) Any Revolving Account

(b) Revolving Balances

(c) Revolving Utilization

(d) Revolving Limits
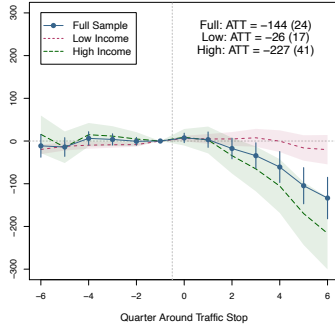
(e) Available Balances

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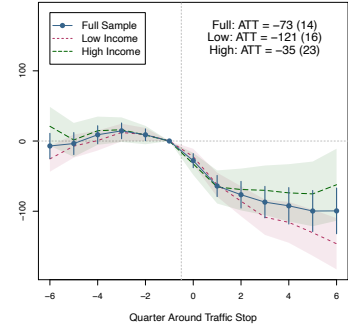
Notes: Each panel reports event study estimates for the full sample as well as estimates from separate event studies for motorists with above ($N = 288,276$) and below ($N = 237,730$) median estimated income at baseline.

Figure F-5: Event study estimates for distress outcomes for subset in payroll records at baseline

(a) Any New Distress

(b) Collections

(c) Collections Balances

(d) Delinquencies

(e) Derogatories

(f) Payroll Employment

Notes: 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 F-6: Event study estimates for credit card outcomes for subset in payroll records at baseline

(a) Any Revolving Account
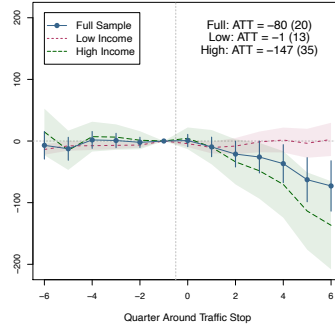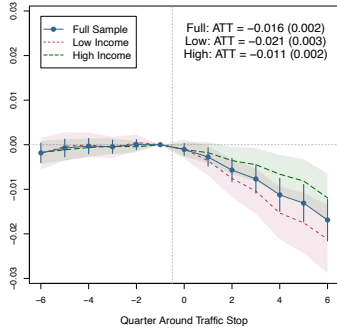
(b) Revolving Balances

(c) Revolving Utilization
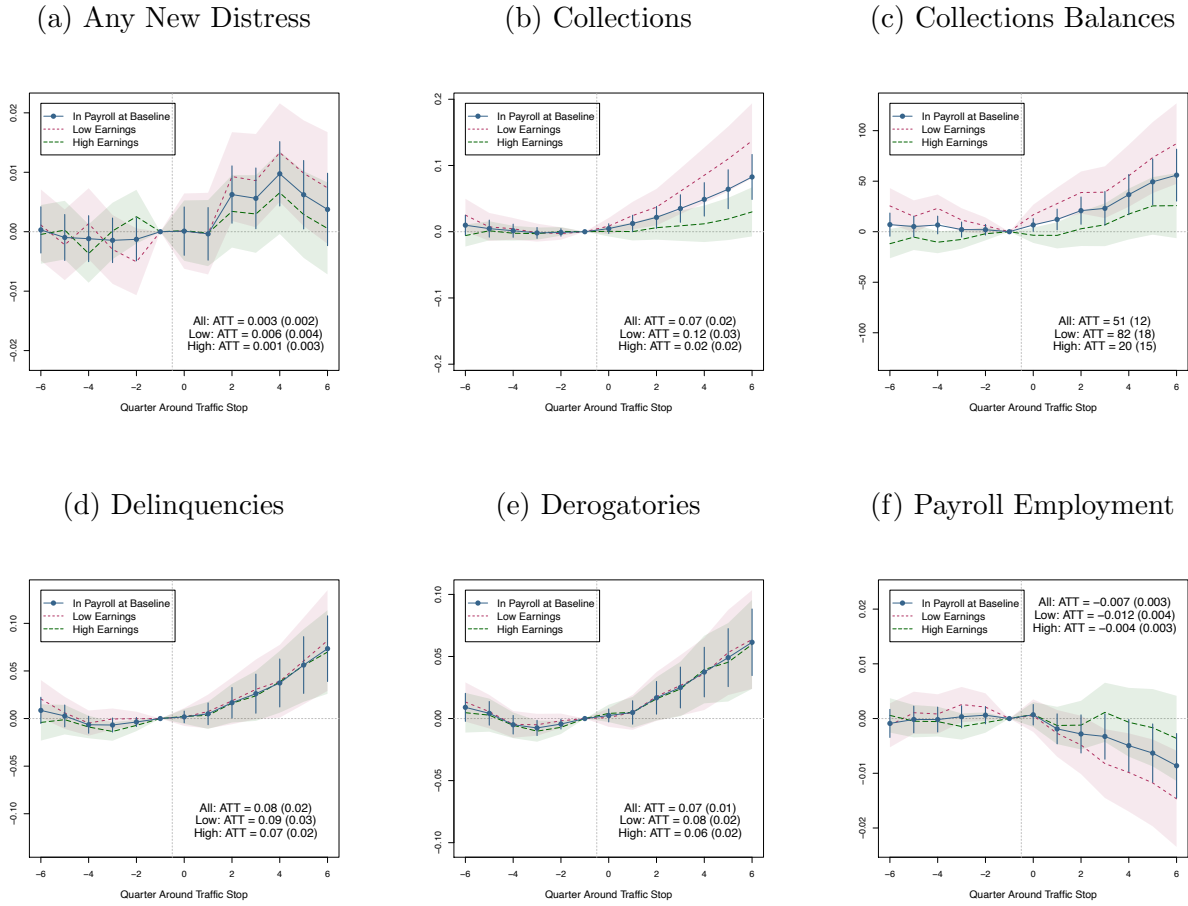
(d) Revolving Limits
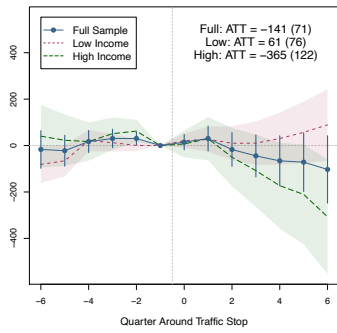
(e) Available Balances
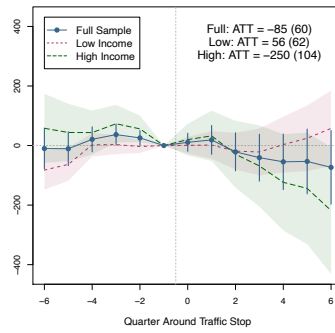
(f) Imputed Limits

Notes: 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.

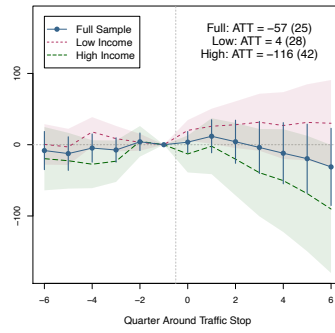Figure F-7: Event study estimates relative to counterfactual control means for payroll employment, by baseline payroll employment status

(a) Not in payroll records at baseline
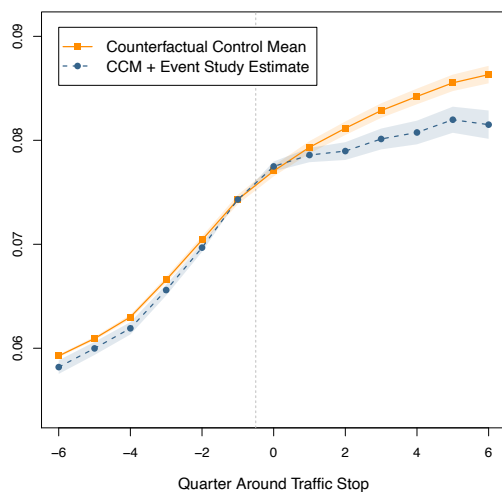
(b) In payroll records at baseline
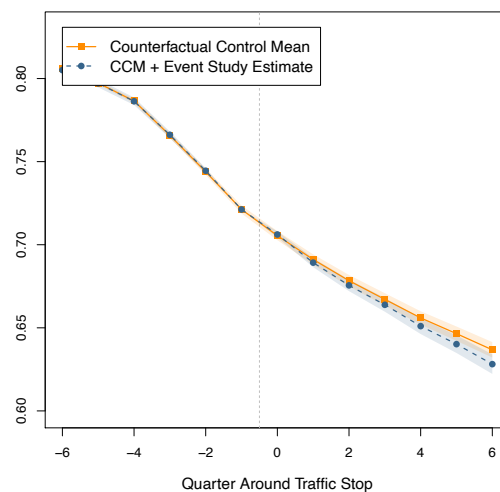
Notes: This figure reports the time path of estimated counterfactual means (orange squares) and the estimated counterfactual means plus the event study estimates (blue circles) where the outcome is employment in the payroll records and sample is split by whether the individual is employed in the payroll records at baseline (same sample split as panels (a) and (b) of figure 6).

Figure F-8: 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

Notes: 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 F-9: Event study estimates for credit card outcomes by baseline credit card liquidity
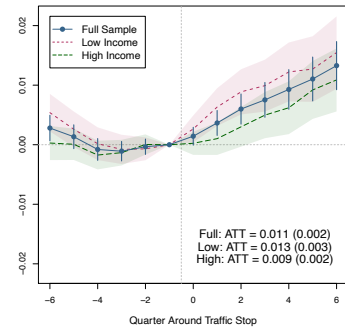
(a) Any Revolving Account
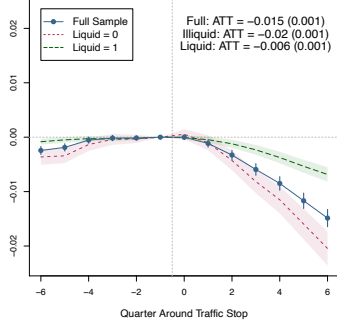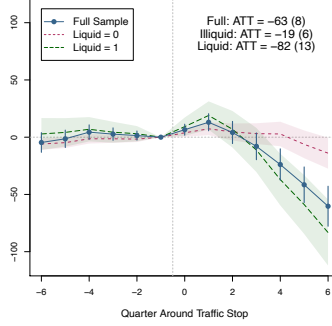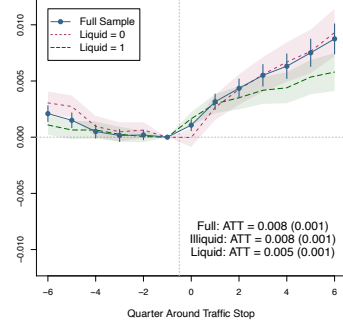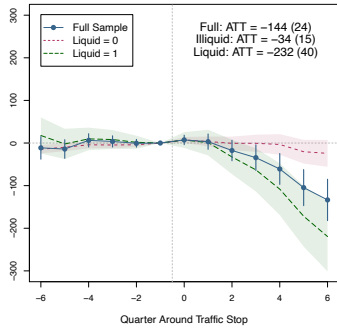
(b) Revolving Balances

(c) Revolving Utilization
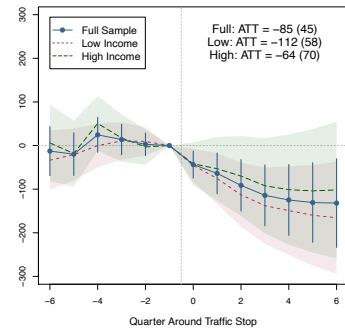
(d) Revolving Limits

(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 F-10: Heterogeneity for long-run outcomes
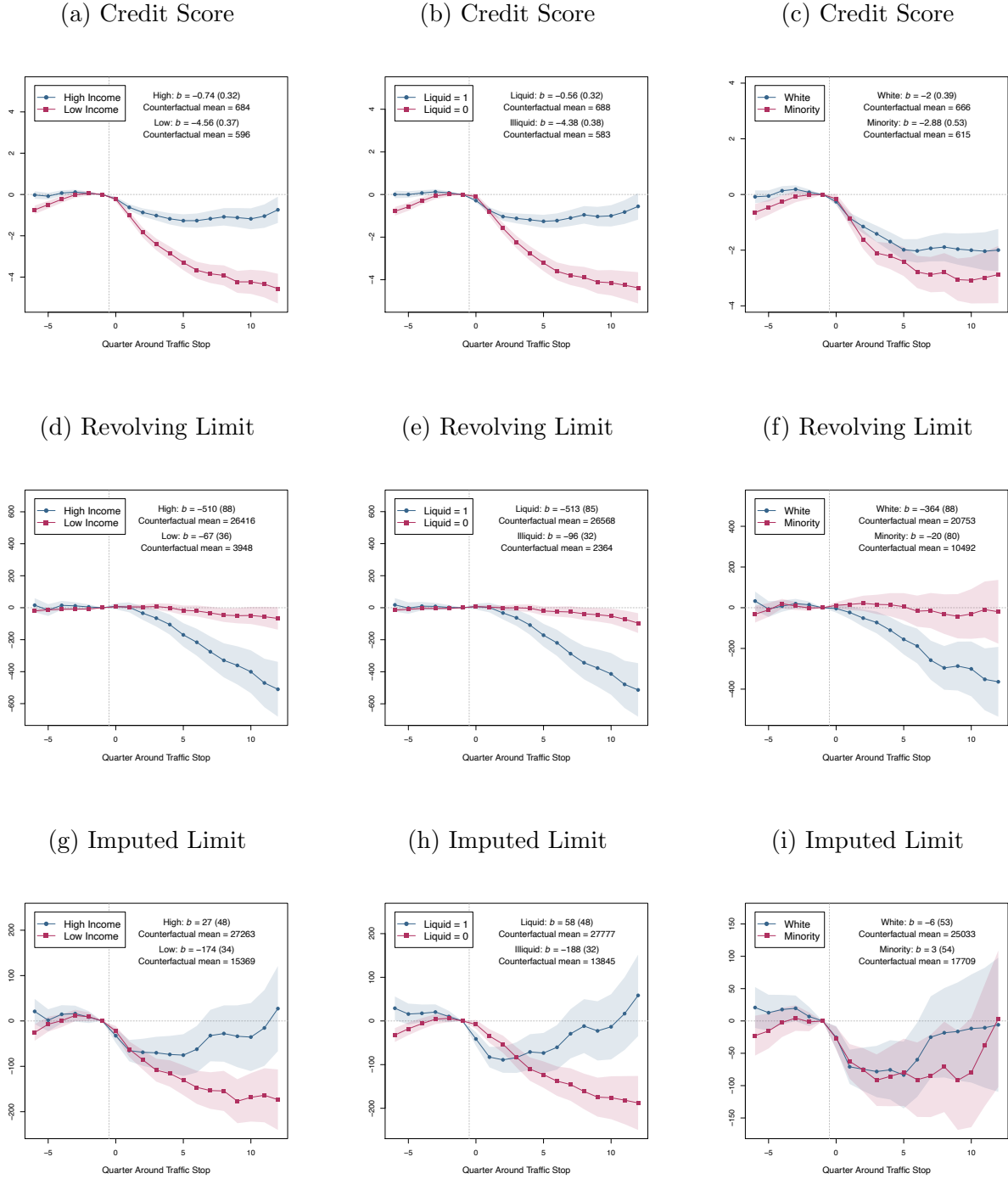
Notes: This figure reports heterogeneity in the estimates for longer-run outcomes by baseline estimated income, baseline credit card situation, and by motorist race.

99

Figure F-11: Event study estimates by motorist race

(a) Collections Balances
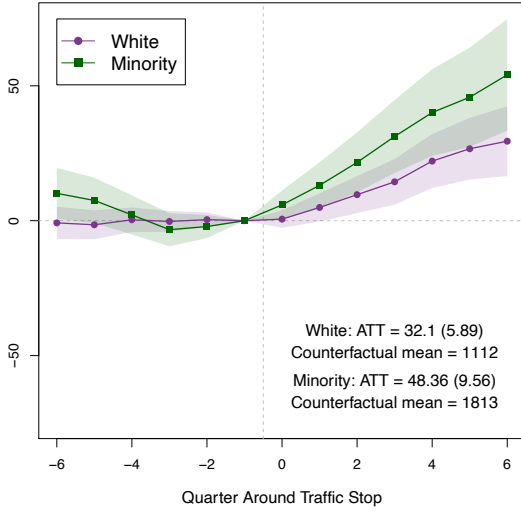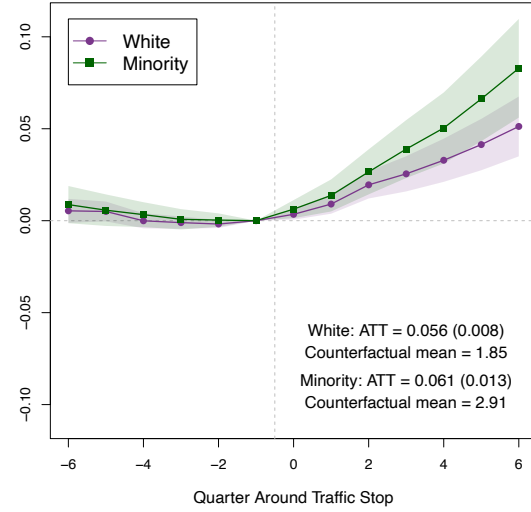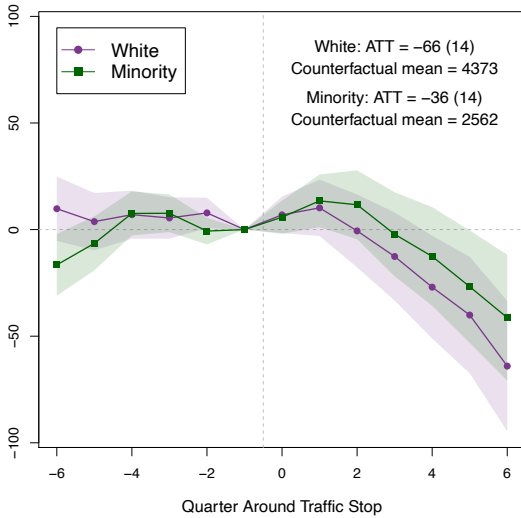
White: ATT = 32.1 (5.89)
Counterfactual mean = 1112

Minority: ATT = 48.36 (9.56)
Counterfactual mean = 1813

(b) Credit Line Delinquencies

White: ATT = 0.056 (0.008)
Counterfactual mean = 1.85

Minority: ATT = 0.061 (0.013)
Counterfactual mean = 2.91

(c) Revolving Balances

White: ATT = −66 (14)
Counterfactual mean = 4373

Minority: ATT = −36 (14)
Counterfactual mean = 2562

(d) Payroll Employment

White: ATT = −0.005 (0.001)
Counterfactual mean = 0.14

Minority: ATT = −0.009 (0.001)
Counterfactual mean = 0.17
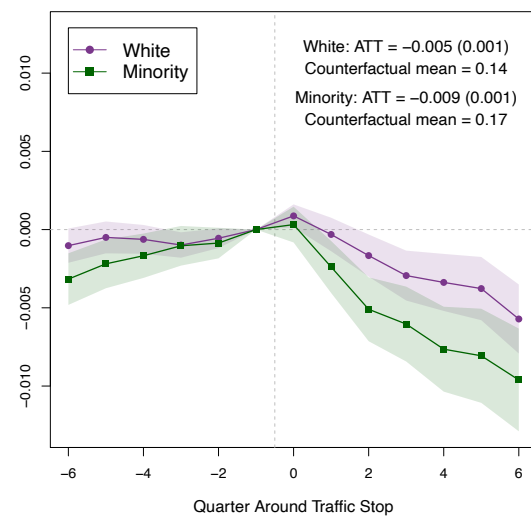
Notes: Each figure reports event study estimates, obtained via the Callaway & Sant'Anna (2021) approach, as well as 95 percent confidence intervals based on design-based standard errors from Roth & Sant'Anna (2022), for the denoted outcome. Event studies are estimated separately for white ($N = 195,373$) and Black or Hispanic ($114,225$) motorists with dispositions indicating a paid fine or traffic school election (the "definitely paid" subset). See figure F-12 for estimates by various subgroups and by race.

Figure F-12: 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
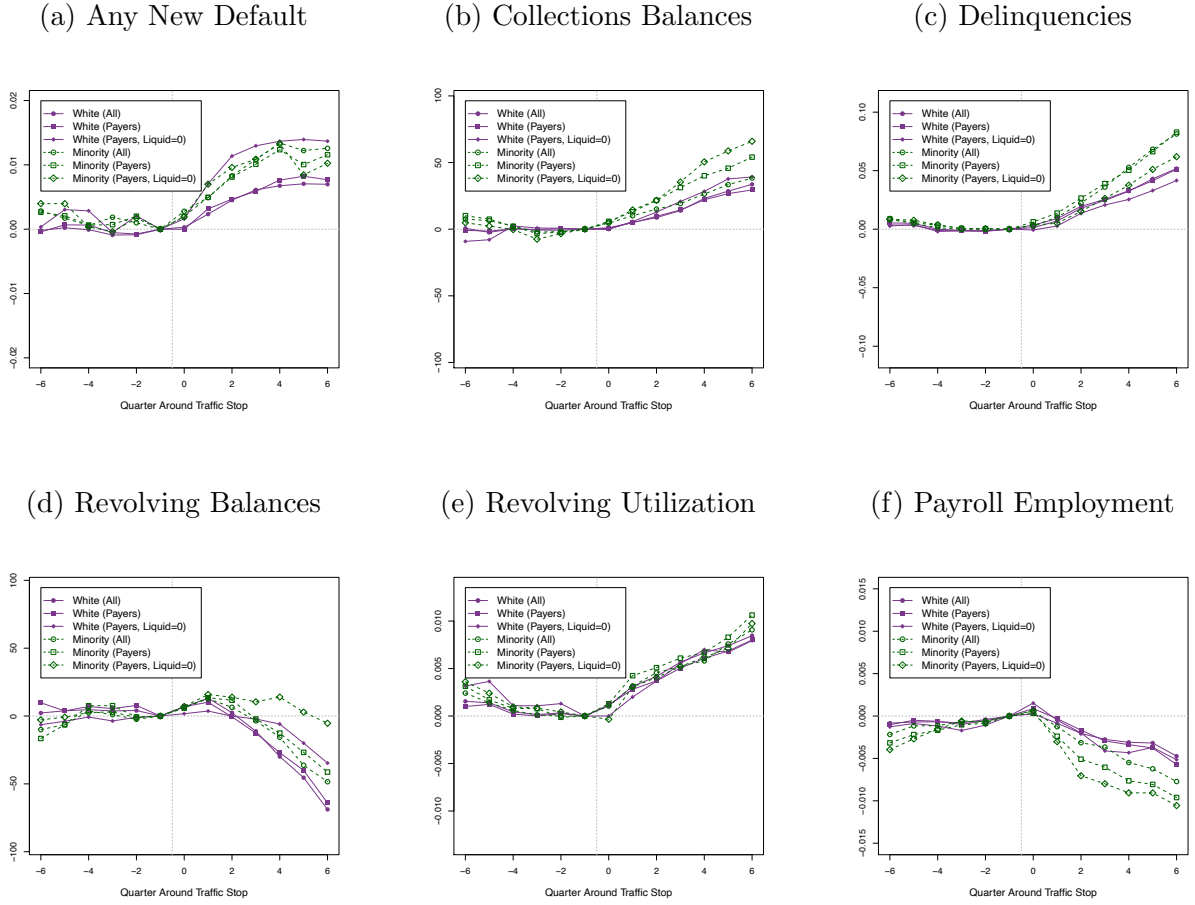
Notes: Same as figure F-11 except additionally showing results for the full sample (i.e., those with any court disposition) (N white $= N = 308,116$; N Black or Hispanic $= 217,530$) and for the subset who both pay their fines and with less than $200 in available balances on credit cards (N white $= 105,529$; N Black or Hispanic $= 118,799$).

Table F-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				
	$p = 0.136$	$p = 0.393$	$p = 0.742$	$p = 0.367$

Notes: 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. (2024). The sample is the full event study sample ($N = 525,646$) and the average fine is \$195.53.

Table F-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	$p = 0.364$	$p = 0.178$	$p = 0.412$	$p = 0.365$	$p = 0.507$	$p = 0.197$

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

Table F-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 F-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.

G Instrumental variables approach

G-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 limit and increase discretely at various speed thresholds. As shown in panel (a) of figure G-1, over one third of all citations are issued for exactly nine MPH over the limit, just below a \$75 increase in fine. This bunching suggests the systematic manipulation of speeds by officers as a form of lenience (Goncalves & Mello, 2023).[18]

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:

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

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:

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

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 (see Chyn et al. 2022 for a summary) 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 G-3, 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.[19] Moreover, equation G-2

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[18]Figure G-2 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.

[19]Table G-3 shows the relationship between other motorist characteristics and officer stringency, conditional on beat-shift effects (joint $F = 2.6$). For an expanded discussion of instrument validity in this setting, see Goncalves & Mello (2023).

is specified in differences, so exogeneity 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 (G-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 G-4 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 G-4) 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.

G-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 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 G-8 shows that results are similar when additionally imposing these restrictions. Table G-1

illustrates that the IV sample is quite similar to the event study sample in terms of baseline characteristics.

G-3   Results

Panel (a) of figure G-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.[20]

Figure G-5 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 G-2, which reports IV estimates in different specifications.

Columns 1-2 of table G-2 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-run increases in credit card borrowing which do not persist (as shown in panels D and E) is

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[20]Figure G-6 illustrates that this reduced form relationship is more pronounced for lower-income motorists ($\beta = \$44.3$, $se = 14.5$) than for higher-income motorists ($\beta = \$16.3$, $se = 13.13$).

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 G-1, panel D of table G-2 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.

G-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 G-5). 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.

G-3.2 Robustness

In supplementary appendix G-4, 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.

Table G-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

Notes: 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.
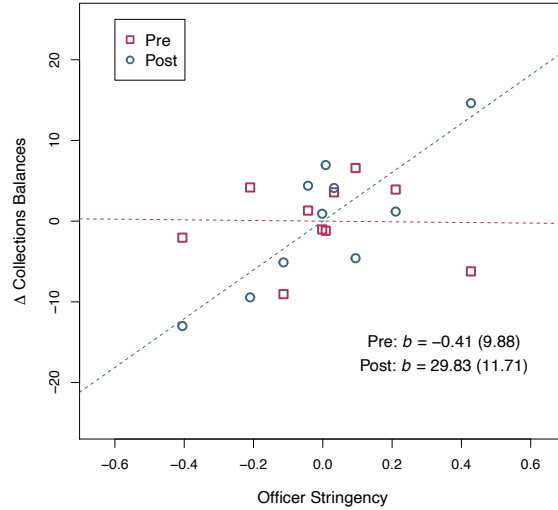
Figure G-1: Instrumental variables approach

(a) Histogram of Charged Speeds
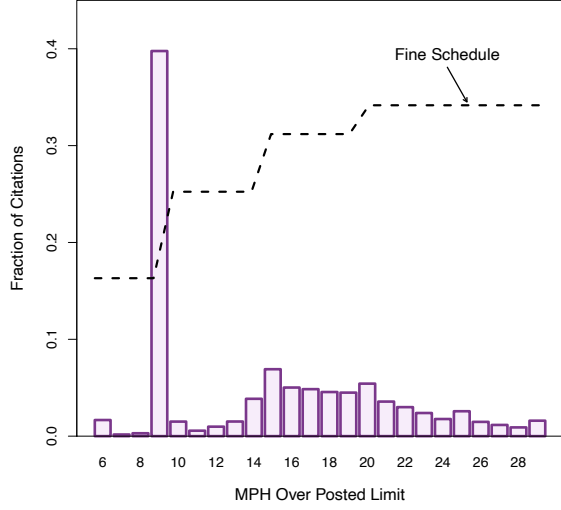
(b) First Stage
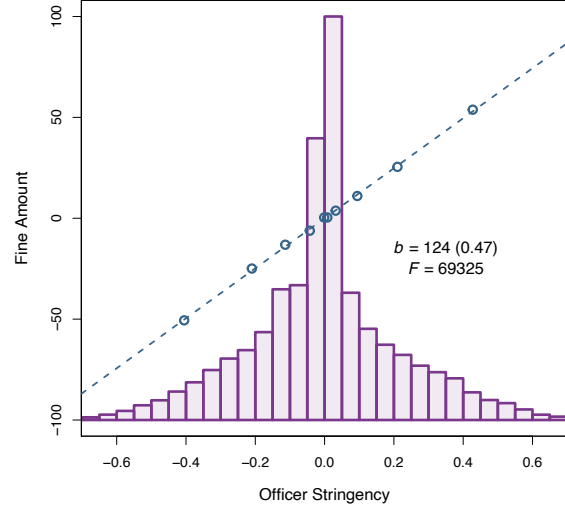
(c) Reduced Form

Notes: 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 G-2: 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)

Notes: 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 G-5. Sample is the IV sample, $N = 362,854$.

G-4 Instrumental variables: additional results

Figure G-2: Evidence of officer behavior

(a) Distribution of Officer Effects

(b) Correlation of Officer Effects
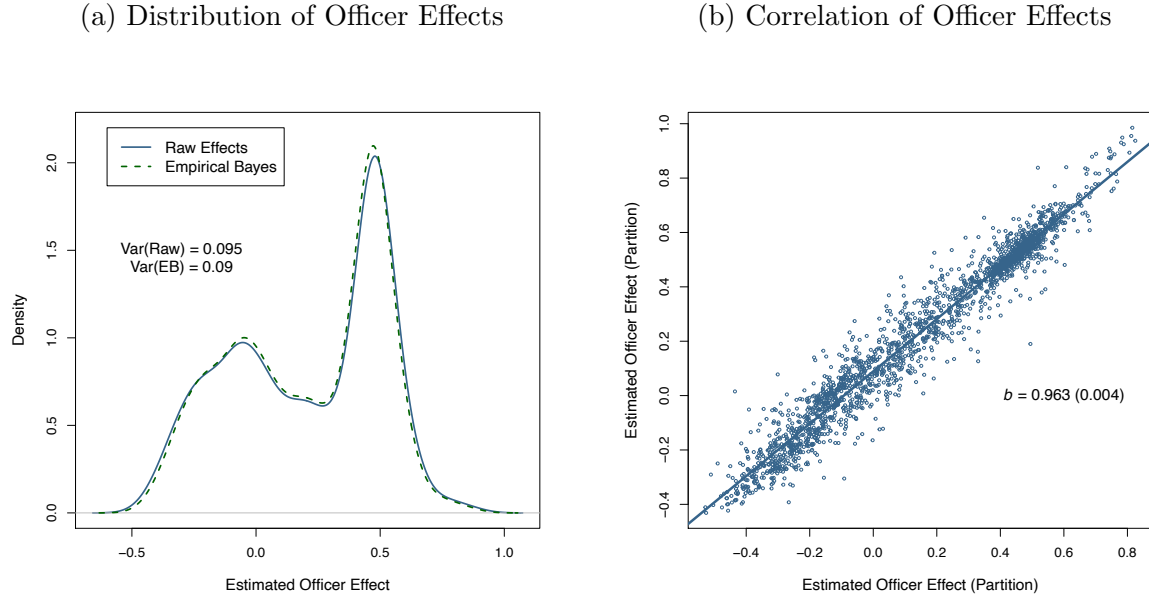
*N*otes: Panel (a) plots the distribution of estimated officer fixed effects from a regression of **1**[harsh fine], where harsh fine indicates a charged speed > 9, on motorist covariates and beat-shift fixed effects. Solid blue line shows the distribution of raw estimated effects and dashed green line 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 G-3: Instrument validity

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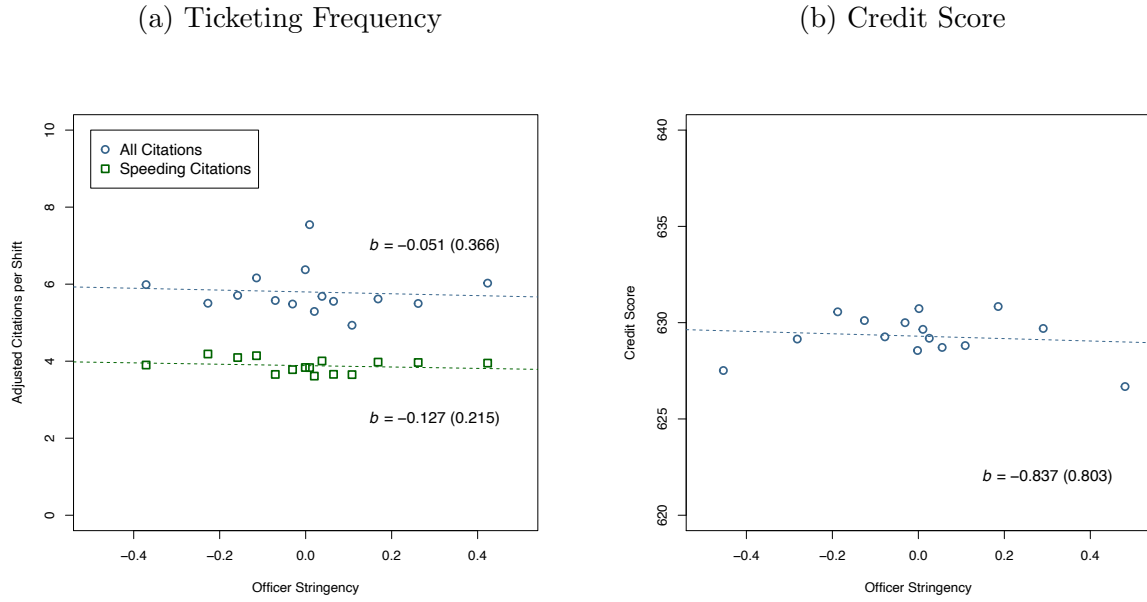
Notes: Panel (a) illustrates the relationship between the officer stringency instrument, residualized of beat-shift fixed effects and an officer's average number of citations per shift, adjusted for beat-shift effects. Panel (b) illustrates the relationship between the officer stringency instrument and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

Table G-3: 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

Notes: 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 G-4: First stage estimates across subsamples

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

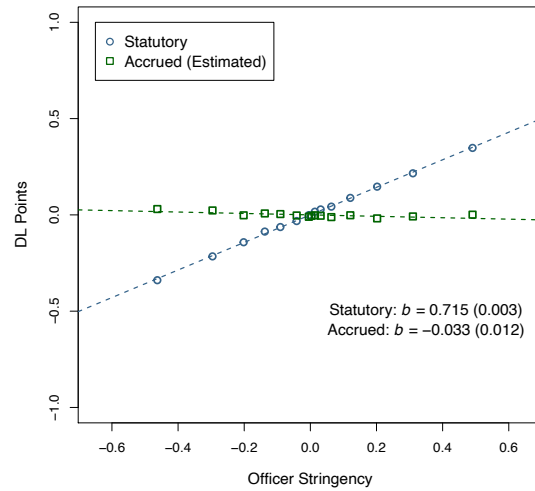Figure G-4: Officer stringency and citation outcomes
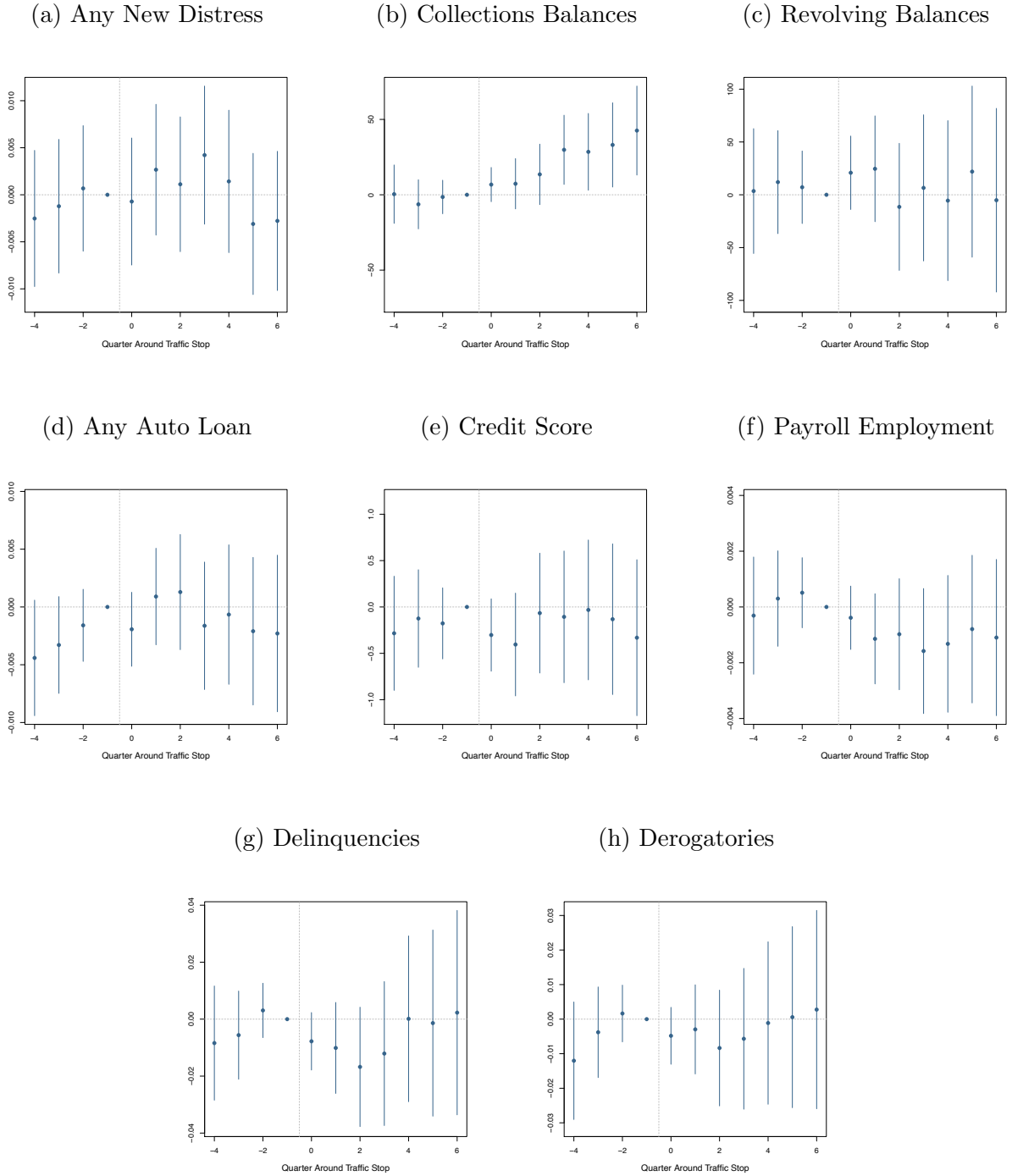
(a) Contested in Traffic Court
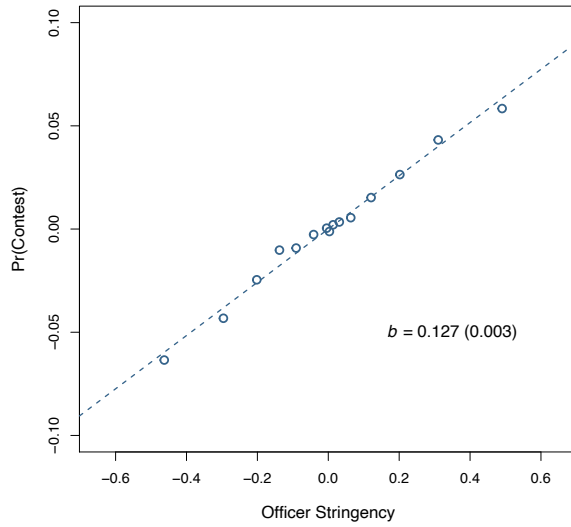

(b) Fine Amount
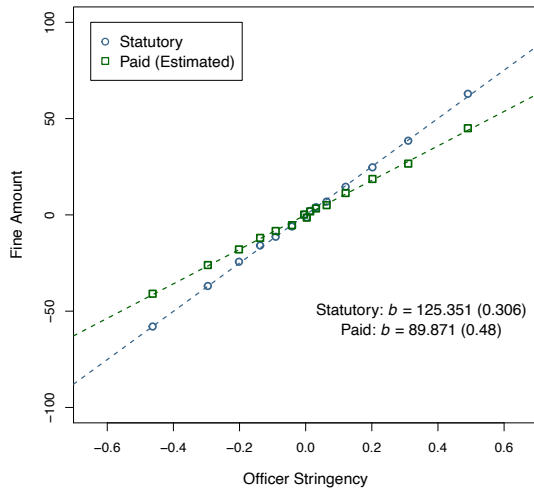
(c) DL Points

Notes: 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 G-5: 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

Notes: 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 G-6: Reduced form estimates by motorist income
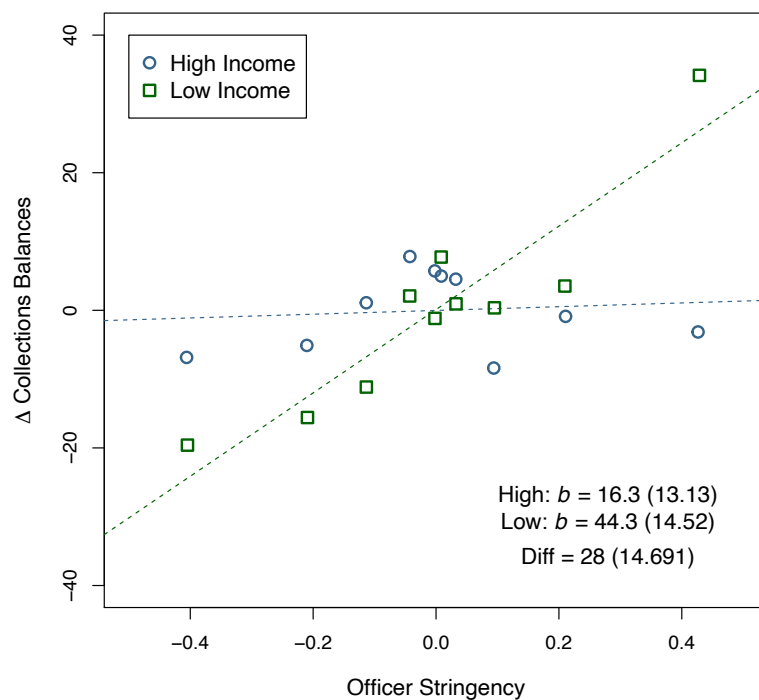
Notes: Same as figure G-9, illustrating the post-stop change in collections balances separately for motorists with above (FS = $124.76, se = 0.49) and below median (FS = $123.2, se = 0.5) zip code incomes.

Figure G-7: Robustness of IV estimates to sample selection
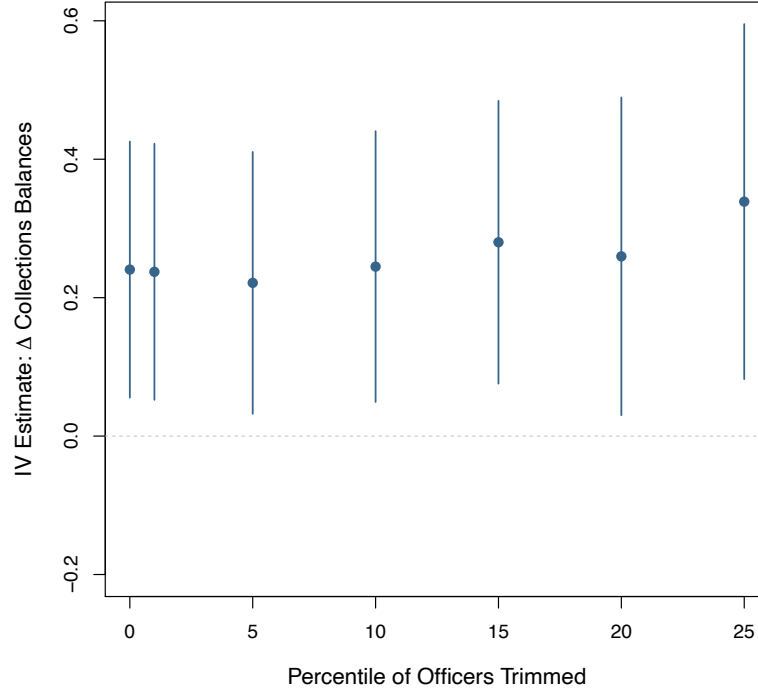
*N*otes: This figure reports IV estimates for the one-year change in collections balances when trimming the sample of officers with selected samples. First, a covariate index $\hat{Y}$ is constructed by regressing Y on motorist demographics using only the sample of lenient officers. Then, I construct 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 G-2.

Table G-5: IV Results with alternative instruments

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

Notes: 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 G-8: Reduced form estimates for motorists without past citations

Pre: *b* = 0.47 (11.04)
Post: *b* = 33.1 (13.21)
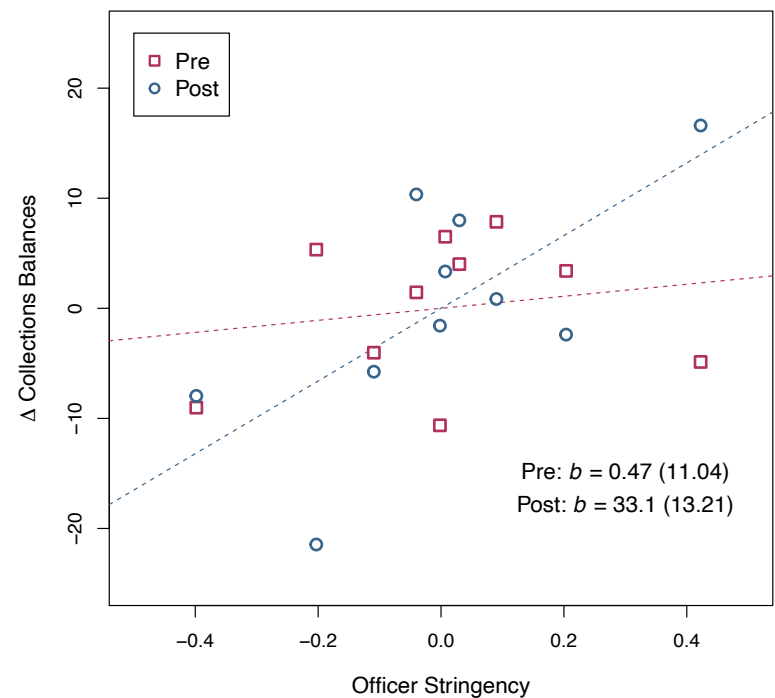
*N*otes: Same as figure G-1 using a subsample of the IV sample that requires only one citation per motorist (the first in-sample citation per motorist) and requires that each motorist has not received a citation in the previous year ($N = 272,866$).
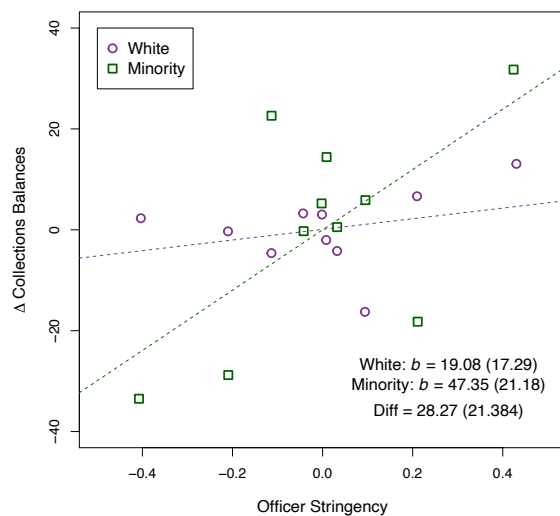
G-5   IV estimates by motorist race

Figure G-9: Officer IV reduced form estimates by motorist race
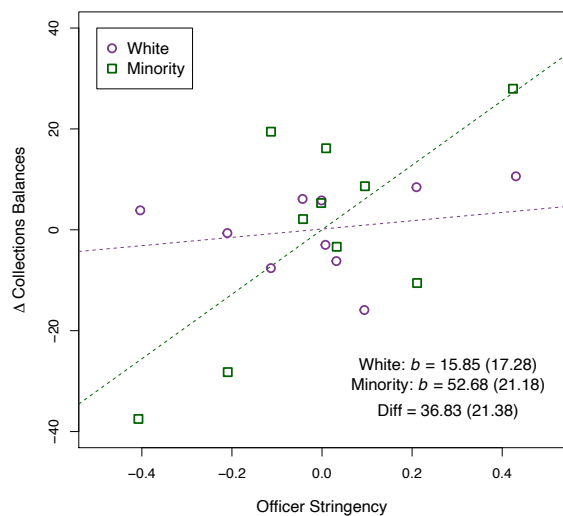
(a) Without controls   (b) With controls

*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 G-6: Officer IV results by motorist race

	$\tau = 3$			$\tau = 6$	
(1)	(2)	(3)	(4)	(5)	(6)
White	Minority	p-val	White	Minority	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)	

Notes: 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.

Figure G-10: Reduced form estimates by motorist race using within-race instrument

(a) Without controls

(b) With controls
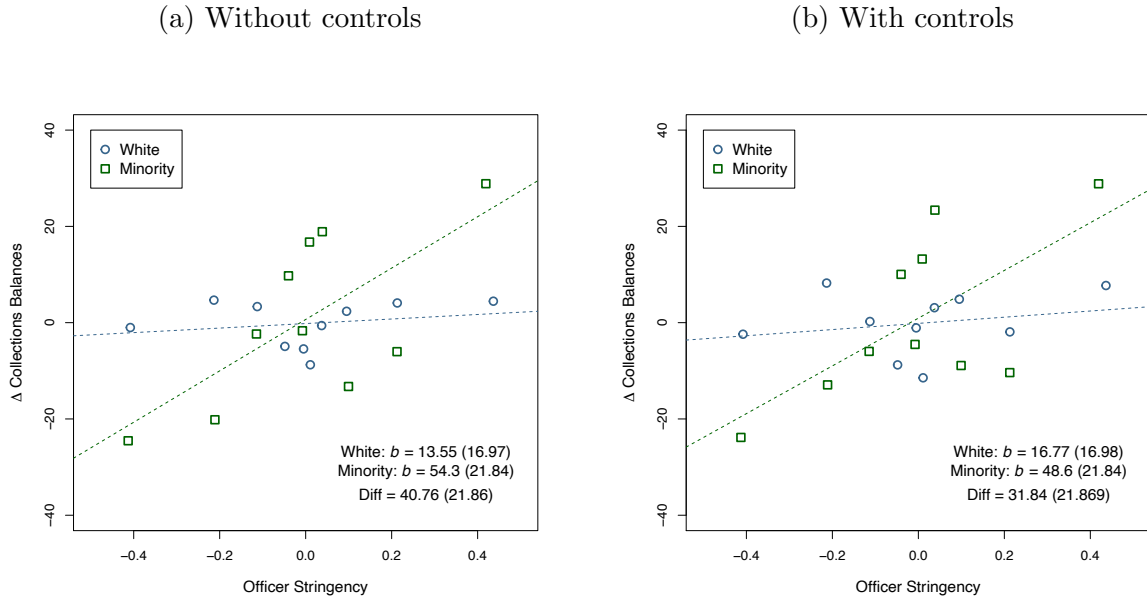
Notes: Same as figure G-9 except using a stringency instrument that is recomputed within racial groups. The first stage estimate for white motorists is $\beta_{FS} = 121.95$ (0.48) and the first stage estimate for minority motorists is $\beta_{FS} = 125.11$ (0.53).