Fines and Financial Wellbeing∗

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Abstract

While survey evidence suggests widespread financial fragility in the U.S., causal evidence on the implications of typical, negative income shocks is scarce. I estimate the impact of speeding fines on household finances using administrative traffic citation records and a panel of credit reports. Event studies reveal that for every $195 in fines, unpaid bills in collections increase by $34. Given additional evidence that fine payment explains this effect and that default is the “last resort” for households, I interpret this finding as evidence of financial precarity for the average motorist. My estimates are consistent with 30-60 percent of households resorting to default on other bills to finance an unplanned $200 fine payment. I also find that fines are associated with longer-run declines in credit scores, borrowing limits, and the likelihood of appearing as employed in payroll records covering a subset of large, high-paying employers. This impact on employment situations appears attributable to the diminished financial position of households rather than, for example, downstream license suspensions.

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1 Introduction

The ability of households to cope with adverse shocks has important implications for taxation and social insurance policies (e.g., Baily 1978; Chetty 2006). Despite the prediction of canonical models that liquidity-constrained households anticipate income volatility by accumulating buffer stock savings (Deaton 1991, Carroll et al. 1992; Carroll 1997), recent evidence has highlighted the lack of precautionary savings in the United States (Beshears et al., 2018). Half of all households accumulated no savings in 2010 (Lusardi, 2011) and forty percent of Americans indicated an inability to cover an emergency $400 expense in a 2018 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 data on bank account balances (e.g., Chen 2019; Nova 2019), and the belief that resiliency 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 populations 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 poor communities, allowing for the study of a large sample of low-income households. Third, driver license suspensions imposed for nonpayment incentivize high payment rates on average, increasing confidence that a traffic ticket represents a true expense, or transitory income, shock.

To estimate the impact of fines, I link administrative data on the universe of traffic citations issued in Florida over 2011-2015 to a quarterly panel of credit reports for cited
drivers. The citations data provide near-complete coverage of the state’s traffic offenders and my analysis sample represents about three percent of Florida’s driving-age population. Credit reports offer a detailed account of an individual’s financial situation and include information on defaults and borrowing. Unpaid bills in collections represent an especially useful outcome, as they capture default on obligations such as medical and utility bills (Avery et al., 2003) and thus can provide a measure of financial distress even for the lowest-income drivers, many of whom have limited attachment to the formal financial sector.

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

Event study estimates reveal that traffic fines averaging $195 increase unpaid bills in collections by about $34 (se = $4). Given high payment rates on traffic fines and the fact that collections activity associated with traffic citations is very unlikely to be reported to credit bureaus, this finding implies that, on average, households cover about 17 percent of a typical, unplanned expense through default on other financial obligations. In the spirit of Dobkin et al. (2018), I interpret this event study as a test for households’ ex ante ability to cover unplanned expenses, with the finding suggesting that the average household must resort to default in order to pay an unplanned $200 bill.

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

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

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

A natural question is whether the observed impacts on financial distress and job stability can be explained by other, non-fine sanctions associated with traffic tickets, such as driver license (DL) suspensions imposed on non-payers or DL “points” accrued on a motorist’s record. Relying on imperfect data on the traffic court disposition associated with each citation, 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, suggesting that simply paying the fine largely explains the findings.

The fact that effects on employment arrangements cannot be explained away by, e.g., driver license suspensions, raises a question about mechanisms. Impacts on payroll employment are consistently strongest in subsamples with the largest increases in financial distress, suggesting a role for the impacts of a diminished credit reputation on job-finding (e.g., Bos et al. 2018; Bartik & Nelson 2021) or, e.g., housing situations. My findings are also consistent with evidence that financial distress can reduce labor supply (e.g., Dobbie & Song 2015; Barr et al. 2023) and evidence that financial distress weakens decision-making (Schilbach et al. 2016; Mullainathan & Shafir 2013) and productivity (Kaur et al., 2021). Disentangling these potential mechanisms is an interesting avenue for future research.

To address the central identification concern associated with the event study approach, I present results from a supplementary identification strategy exploiting quasi-random variation in fine amounts generated by systemic differences in ticketing practices across patrol officers (Goncalves & Mello, 2022). This strategy compares motorists cited at the same time, circumventing the concern that the timing of traffic stops is nonrandom, but sacrifices statistical power and some ability to assess mechanisms or longer-run effects, because the instrument affects both traffic court decisions and future traffic offending. Although less precise, these instrumental variables results corroborate the headline result from the event study approach: a marginal $100 increase in fines is associated with about a $34 ($se = $12) increase in unpaid bills in collections.

Motivated by the large literature on the racial wealth gap (e.g., Derenoncourt et al. 2022) and the well-documented racial disparities in criminal justice outcomes, I explore heterogene-
ity by motorist race using both empirical approaches. Event study estimates for collections
debt are about 50 percent larger for Black and Hispanic individuals ($48, se = $10) than
for white individuals ($32, se = $6), while estimates for payroll employment are almost
twice as large for minorities (−0.009, se = 0.001) than for whites (−0.005, se = 0.001).
On average, minority households also borrow slightly more on credit cards following fines,
suggesting that these disparities cannot be explained solely by differential access to credit
markets across groups. In the IV design, which estimates effects of marginal fine increases
beyond the base speeding fine ($123), racial disparities are significantly more pronounced,
suggesting a particular low ability to cover larger fines among minority households.

My central contribution is evidence that, on average, households cannot easily absorb
typical, unplanned expenses. I conclude by synthesizing the key lessons from this result and
discussing them in the context of the relevant literatures. First, I consider how my findings
can speak to the survey evidence on the prevalence of financial fragility (e.g., FRBG 2018).
Under the assumption that default is the “last resort” for covering unplanned expenses, one
can conceptualize the share of households for whom fines causally increase unpaid bills as
the relevant metric. The event study estimates can identify bounds on this fraction under
ad-hoc, but reasonable, assumptions about the distribution of treatment effects. Using this
approach, I estimate that at least 13 percent, and more likely between 30 and 60 percent, of
individuals borrow out of other financial obligations to cover an unexpected $195 expense.
To my knowledge, this paper is the first to provide evidence on the prevalence of this causal
notion of financial fragility.

Next, I connect my findings to the vast literature on the consumption smoothing behavior
of households (e.g., Stephens 2001; Parker 2017; Ganong et al. 2020; Golosov et al. 2022;
Baker & Yannelis 2017; Gelman et al. 2020; Ganong & Noel 2019). My central contribution
to this literature is causal evidence in a large sample that default is an important consumption
smoothing strategy for liquidity-constrained households, even when facing “typical” income
shocks (Morduch & Schneider, 2016). My documentation of households’ tiered strategy for
covering unplanned expenses 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 surprisingly large fraction of households are at least moderately desta-
bilized 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
decreases in creditworthiness imply that many households are not self-insured against usual
income volatility. Abstracting away from the important logistical concerns, this finding im-
plies 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 ag-
gressive policies to encourage self-insurance, such as expanded financial education or saving
incentives programs (e.g., Klapper & Lusardi 2020, Lusardi et al. 2011).
Finally, my paper also adds to the nascent literature on the social costs of policing (e.g., Ang 2021) and a concurrent literature on the effects of legal financial obligations (LFO’s) on offender outcomes (Kessler 2020; Pager et al. 2022; Giles 2022; Finlay et al. 2022; Lieberman et al. 2023). While a large literature has examined deterrence effects of fines (e.g., Makowsky & Stratmann 2011; DeAngelo & Hansen 2014; Traxler et al. 2018), interest in the potential negative effects of fines and fees on individuals has grown significantly in recent years. Finlay et al. (2022) note that an important distinction in the current research on LFO’s appears to be whether fines are coupled with criminal convictions, with studies examining variation in fine amounts among those also convicted of felonies or misdemeanors tending to find null effects on life outcomes such as reoffending or employment. My paper, on the other hand, studies comparatively small fines which are not associated with convictions and documents impacts on household financial situations.

The rest of the paper proceeds as follows. Section 2 provides the relevant institutional background and section 3 describes the data. I lay out the empirical strategies in section 4, present results in section 5, and explore heterogeneity by motorist race in section 6. Section 7 interprets and contextualizes the findings and section 8 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. Insurance companies will typically consider license points as a signal of driver risk when setting premiums, so individuals may face increases in auto insurance costs following a citation. A rough, back of the envelope, calculation suggests that the typical speeding citation can increase monthly auto insurance premiums by about $10.\footnote{See, e.g., Gorzelany in Forbes, 5/17/2012.} 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.

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.\footnote{As of 2022, a new Florida law requires that counties offer income-based payment plans for traffic citations. However, during my sample period (2005–2017), only two counties, Hillsborough and Pinellas, offered three-month payment plans for traffic fines (statute; news article). Figure A-8 offers suggestive evidence of smaller effects on unpaid bills in these counties during this period.} 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.\footnote{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.} If the county clerk has not received payment in-full within 30 days, the individual is considered delinquent and their license is suspended, effective immediately. Knowingly driving with a suspended license is a misdemeanor offense and typically results in a fine exceeding $300, as well as potential jail time. Figure A-1 succinctly illustrates the driver’s potential decision tree and
corresponding outcomes for the case of a typical moving violation.

If a citation remains unpaid after 90 days, the county clerk adds a late fee to the original amount owed and sends the debt to a collections agency, who then solicits payment. Collections agencies are authorized by state law to add a 40 percent collection fee to the original debt. Note that, to the best of my knowledge, collections activity originating with unpaid citations will not appear on a driver’s credit report.4

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

According to the Florida Clerks and Comptrollers, who estimate that 90 percent of traffic fines are paid on time, the threat of license suspension is a strong incentive for payment. Using traffic court disposition information for a subset of citations, I estimate a lower bound on the payment rate of 59 percent and cannot rule out a 100 percent payment rate (see appendix C for additional discussion of the dispositions data). My analysis proceeds under the assumption that fines are paid in the majority of cases, but I leverage the disposition data to estimate effects for subsets of individuals who surely paid their fines, whose sanctions may have been dismissed in court, and who avoided the accrual of DL points via traffic school.5

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)

4The 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.

5While some studies (e.g., Giles 2022) have documented very low payment rates for criminal fines, Dusek & Traxler (2022) find a payment rate above 75 percent for traffic fines and document fairly small compliance responses to fine increases.
database. These records include the date and county of the citation as well as information on the charged violation. The UTC data also includes information listed on the motorist’s 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 C-1.1.

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 C-2, I augment the credit report data by constructing an estimated income measure at baseline using an income estimate provided by the credit bureau, the average income in a motorist’s home zip code, and payroll records for a subset of the sample (described below).

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

3.3 Outcomes and interpretation

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

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

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

Importantly, when examining these primary default measures, we should expect to see
effects (if any) materialize gradually over time, due both to how the outcomes are defined
and to the credit reporting process. For a collection to appear on a credit report, a household
needs to miss a bill, a creditor needs to send that default to a third-party collections agency,
and that third-party collections agency needs to report that activity to a credit bureau.
In many cases, creditors (e.g., utility providers) provide some temporary forbearance on
late payments before sending the debt to a collector. Hence, collections activity appearing
quickly following a traffic stop could correspond to bills which have already been missed
but then transition into “late enough” for the creditor to send to a collector. On the other
hand, a new missed bill immediately following a traffic stop should take several months to
appear as a collection on a credit file.\footnote{Event studies in Dobkin et al. (2018) show that
the impact of hospital admissions on medical collections materialize over 18-24 months. In
appendix D, I show that the dynamic effects of separation from a payroll-covered job (described
below) exhibit a similar pattern.} Note that, in either case, we should still interpret the
collections activity as attributable to the fine. The same logic applies also for delinquency or
derogatory credit accounts, where fines may induce already delinquent accounts to pass the
threshold for reporting or lead to new defaults which ultimately become 90-days delinquent
or sufficiently late to warrant a charge-off and then show up on a credit report.

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

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

However, default can be associated with significant costs. Pattison (2020) documents that incidences of financial strain typically coincide with, rather than substitute for, declines in consumption. Moreover, dynamic consequences of default in terms creditworthiness can be severe. A typical default instance can reduce credit scores by as much as 30 points (see figure C-6), with implications for interest rates and borrowing limits, as well as apartment leasing or job-finding. Liberman (2016) finds that such credit constraints can have significant welfare implications, estimating a typical willingness to pay of 11 percent of monthly income for a clean credit reputation. To the extent possible, I directly examine these longer-term effects by estimating event studies where the credit score or borrowing limit is the outcome over a longer (three-year) time horizon.

3.4 Payroll records

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

In my analysis sample of cited drivers, about 12-15 (16-18) percent of motorists have earnings in the payroll records in a given quarter (year). To better understand what these data capture, appendix D 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 C, I use the payroll information to construct an estimated income measure at baseline for the full sample and then use that measure to
explore heterogeneous effects of fines 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:

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

(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 their “cohort” (Sun & Abraham, 2021).

Of course, a wave of recent econometric scholarship has documented the various empirical issues associated with estimating event study models with two-way fixed effects (TWFE) via OLS (e.g., Chaisemartin & D’Haultfoeuille 2020; Goodman-Bacon 2021; Sun & Abraham 2021; Callaway & Sant’Anna 2021; Borusyak et al. 2022; Roth et al. 2022). Some of the important concerns raised in this literature include the contamination of treatment effect estimates created by comparisons between currently treated and previously treated units and underidentification problems in fully dynamic specifications with no untreated group. To address these issues, I estimate the event studies using the method of Callaway & Sant’Anna (2021). Their approach is to construct estimates for each cohort and time period, based only on comparisons between each cohort and those treated in the future, and then aggregate these cohort $\times$ period effects into event study parameters.\(^7\)

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

\(^7\)As shown in figure A-7, estimates from the alternative methods of Sun & Abraham (2021) or Borusyak et al. (2022) are remarkably similar.
There are two important identification concerns that bear mentioning here. First, many types of traffic infractions could signal changes in financial distress \textit{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. (2022) approach, for speeding violations and non-moving violations. For non-moving offenses, the majority of which are paperwork or equipment infractions, a strong pre-citation trend ($p < 0.001$) in financial distress is evident. On the other hand, there is no such trend for speeding violations ($p > 0.35$), suggesting that the precise timing of a speeding stop is unrelated to changes in an individual’s financial situation.

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

Nonetheless, I take this concern seriously. As robustness, I estimate event studies for each car purchase timing group (e.g., individuals who first purchase cars in 2011Q2) and then aggregate up across the groups. These estimates leverage only the staggered timing in traffic stops within groups of individuals who purchase cars at the same time (and prior to their traffic stops) and are strikingly similar to the baseline results. I also rely on a companion identification strategy, described in more detail below, that compares individuals cited at the same time but facing different fine amounts.

### 4.2 Instrumental variables approach

I supplement the event study approach with a secondary identification strategy that leverages quasi-random variation in fine amounts generated by differences across officers in ticket-writing practices. In Florida, statutory fines for speeding violations depend only on an offender’s speed relative to the posted limit and increase discretely at various speed thresholds. As shown in panel (a) of figure 8, over one third of all citations are issued for exactly nine MPH over the limit, just below a $75 increase in fine amount. This stark bunching suggests the systematic manipulation of speeds by officers as a form of lenience (Anbarci & Lee 2014;
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:

\[ Z_{ij} = 1 - \left( \frac{1}{N_j - 1} \sum_{k \neq i} 1[speed_{kj} = 9] \right) \equiv \text{stringency} \tag{2} \]

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:

\[ \Delta Y_{ij\tau} = \theta \text{Fine}_{ij} + \gamma X_i + \psi_s + u_{ij\tau} \tag{3} \]

by 2SLS, using \( Z_{ij} \) as an instrument for the fine amount. Here, \( \Delta Y_{ij\tau} \) is the change in outcome \( Y \) for driver \( i \) stopped by officer \( j \) between one quarter prior to the traffic stop and \( \tau \) quarters after the traffic stop. The \( \psi_s \)'s are beat-shift fixed effects at the level of county \( \times \) agency \( \times 1[\text{highway}] \times \) year \( \times \) month \( \times 1[\text{weekend}] \times \) 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 (Clyn et al., 2022)

Validity of this IV approach requires the usual LATE assumptions (e.g., Imbens & Angrist 1994). Papers using comparable examiner designs for identification (e.g., Kling 2006; Maestas et al. 2013; Dahl et al. 2014; Dobbie & Song 2015) typically appeal to institutional features, such as the randomized rotation of criminal case assignments across courtrooms, as evidence for instrument exogeneity. In the traffic citation setting, there is no institutional randomization of patrol officers to motorists, highlighting the important concern that officers with different bunching propensities may have differently selected samples. Figure B-2, however, shows that, after conditioning on beat-shifts, stringency is uncorrelated with an officer’s citation frequency and uncorrelated with a motorist’s financial situation, as summarized by their credit score.\(^9\) Moreover, equation 3 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 (3), which replace the outcome with \((Y_r - Y_{-1}) - (Y_{-1} - Y_{-4})\), i.e., the change in \( Y \) following the stop minus the change in \( Y \) preceding the stop.

\(^8\)Figure B-1 illustrates significant dispersion across officers in the propensity to bunch drivers, net of beat-shift effects, motorist characteristics, and estimation error, as well as the correlation in estimated officer bunching propensity in two random partitions of the data. See Goncalves & Mello (2022) for further discussion.

\(^9\)Table B-1 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 (2022).
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 B-2 illustrates that the first stage estimates are comparable across subgroups of motorists.

Relative to the staggered timing design, the main advantage of the instrumental variables approach is the ability to compare two drivers stopped at the same time, alleviating the core identification concerns associated with the event study. On the other hand, there are several complications associated with the IV approach. As shown in Goncalves & Mello (2022), 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 B-3) precludes the IV approach from separating the effects of fine payment and other potential mechanisms. Specific deterrence effects associated with stringency should bias the instrumental variables estimates towards zero, as motorists facing lower fines are more likely to accrue additional fines in the future, a feature that makes the stringency approach especially poorly suited to the estimation of longer run effects. And finally, as one might expect, the instrumental variables estimates are substantially less precise.

Also worth noting is the fact that the instrumental variables approach identifies a different parameter than the event study design. First, the IV estimates correspond to a pure intensive margin effect: the officer instrument generates variation in fine amounts among individuals fined at the same time and in the same area. Everyone in the IV sample faces a fine of at least $123, which is the fine for speeding 9 MPH over the limit. And second, the IV estimates recover a local average treatment effect (LATE) for the subsample of compliers, or individuals whose fine amounts are actually affected by officer assignment. In this setting, the existence of patrol officers who always (never) bunch drivers rules out the existence of never (always) takers (Goncalves & Mello, 2022), but the LATE may still differ from the ATE due to the underlying LATE weights (e.g., Frandsen et al. 2019).

### 4.3 Sample construction

Individuals included in the event study and IV analysis 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. For the event study, I start with all speeding citations attributable to this set of drivers and impose the following conditions: (i) speeding is the only violation on the citation; (ii) charged speed between 6 and 29 MPH over the limit (speeds below 6 are statutory warnings and speeds above 29 require a court appearance); (iii) motorist race is either white, Black, or Hispanic. I then select the first such stop for each individual and require that the driver has no other stops in the previous year ($N = 525,646$). Figure A-3
shows the distribution of treatment timing ("cohorts") in the event study sample as well as how characteristics vary across cohorts.

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 B-7 shows that results are similar when additionally imposing these restrictions.

Columns 3–4 in table 1 report average baseline characteristics for the event study and IV samples, respectively. In the event study sample, 45 percent of motorists are female, 59 percent are white, and the average age is 36. The event study and IV samples are very similar in terms of both demographics and baseline financial outcomes. Interestingly, the analysis samples appear positively selected relative to the full set of motorists on file: those in the analysis sample(s) have credit scores which are 15-20 points higher and have about $300 less in collections debt at baseline.

In the analysis samples, 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 5.1.4 for an expanded discussion of these definitions, as well as accompanying heterogeneity analyses.

5 Results

5.1 Event study results

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

As shown in panel (a), the probability of a new default flag appearing on an individual’s credit report increases by about one percentage point in the year following a traffic stop, relative to the comparison group of motorists cited in the future. This effect represents about a five percent increase relative to a mean of 0.216. The smaller static ATTT (= 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. Given an average fine of $195, this finding implies that, on average, households finance about 17 percent of their fines by “borrowing” out of other financial obligations such as utility or medical bills. Panel (c) of figure 2, along with table 2, which presents the corresponding regression estimates, also shows impacts on the number of credit lines at least 90 days past due, the number of derogatory credit lines, and the number of accounts in collections.

5.1.1 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$). Note that, since this cut is defined at baseline, some individuals may “switch groups” between the baseline period and their treatment date.

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

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

\footnote{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.}
The fact that the long-run declines in card balances are attributable to the group with available liquidity at baseline highlights a subtle but important interpretation point stemming from two underlying features of the data and setting. First, as suggested by figure C-6, the credit score penalty associated with default appears more severe for individuals with higher initial credit scores. And second, as illustrated in figure C-5, the relationship between credit scores and borrowing limits is convex. For individuals with good (> 700) credit scores, a ten point credit score decline is associated with a $2,550 decline in borrowing limits; the same credit score decline is associated with a $880 decline in borrowing limits for an individual in the middle of the credit score distribution (500-700) and a $140 decline for an individual with a deep subprime credit score (< 500). Hence, while the impacts of fines on default are dramatically attenuated for the liquid group, these small increases in missed bills can generate comparatively large declines in credit limits. Highlighting this point is panel (d) of figure 3, which illustrates that increases in credit card utilization, defined as total balances divided by total limits, are similar for both groups.

Easy access to credit card borrowing is, however, far from the only measure of a household’s financial buffer or overall liquidity. In figure 4, I further split the sample by both available credit card liquidity and by baseline estimated income (cutting at the median \( \approx $31,000 \)), using the income measure described in appendix C-2. 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 \( (N = 224,328) \).11

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

As discussed in section 3.3, a potentially important consideration here is differential rates of formal borrowing: individuals must maintain open borrowing accounts in good standing in order to attain delinquencies on their credit reports. As of one quarter prior to the traffic stop, the shares of each group with at least one open credit line with the potential to transition into delinquency are: 87 percent (high income and liquid); 43 percent (high income and illiquid); 77 percent (low income and liquid) and 43 percent (low income and liquidity status are presented in figures A-9, A-10, A-13, A-14. Tables A-2 and A-3 report the regression estimates underlying figures 3 and 4, respectively.

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11 Event study estimates for credit card borrowing in the full sample are presented in table A-1 and figure A-10. Event studies for all outcomes estimated separately by estimated income and liquidity status are presented in figures A-9, A-10, A-13, A-14. Tables A-2 and A-3 report the regression estimates underlying figures 3 and 4, respectively.
illiquid). Hence, these results highlight that delinquency impacts are larger for those with lower incomes and those with a greater potential for delinquency given \textit{ex ante} borrowing.

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

\subsection{5.1.2 Longer-run effects}

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

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

In figure 5, I present event study estimates for credit scores and borrowing limits over a three-year time horizon. This figure presents estimates from both the baseline Callaway & Sant’Anna (2021) approach and an approach based on Sun & Abraham (2021) which compares only those cited in 2011–2012 to those cited in 2015Q4. I present the Sun & Abraham (2021) results, which hold the “control group” constant, to confirm that longer-run patterns are not driven only by compositional changes in the DiD comparisons.
As shown in panels (a) and (b), traffic fines are associated with a 2.6 point decline in credit scores and a 1.6 percentage point increase in the likelihood of having a subprime credit score, with both effects persisting for three years following a traffic stop. Coinciding with the credit score declines, I find that borrowing limits fall by about $330 over three years. In figure A-15, I show that coinciding with these longer-run declines in creditworthiness are lower rates of home ownership (proxied by mortgages), geographic mobility (proxied by whether an individual’s address was updated on the credit file), and attachment to the formal financial sector (proxied by whether an individual has any open credit line).

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

5.1.3 Additional payroll employment results

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

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

In panels (c) and (d), I show results using only the subsample in the payroll records

\footnote{One complication with interpreting the estimated impact on borrowing limits is that individuals must maintain open borrowing accounts in order to observe their credit limits in the data. One could alternatively replace the observed credit limit (which equals zero for those with no open lines) with an imputed credit limit based on (i) the credit score and (ii) the nonlinear cross-sectional relationship between credit scores and borrowing limits. One can view this measure either as a way to rescale the credit score estimate into a more useful magnitude or as capturing the credit limit an individual is eligible for, based on their credit score, were they to apply for new revolving credit. I find that imputed limits also fall by over $100 in the six quarters following a traffic stop.}
at baseline and split motorists at the median of annualized payroll earnings ($≈$ $34,000). Both panels reveal stark heterogeneity by baseline income. For “employed” motorists with below median earnings, collections balances increase by $84 and the likelihood of working in a payroll-covered job declines by 1.2 percentage points. For those with above median earnings, the comparable estimates are $20 and 0.4 percentage points, with neither effect statistically distinguishable from zero. One could view panels (c) and (d) as a reassuring placebo test for the validity of the event study approach: individuals with stable employment 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.

5.1.4 Fine payment versus other mechanisms

To assess the relative importance of fine payment per se, as opposed to other institutional features such as traffic court involvement, the accrual of license points, and license suspensions imposed on non-payers, in explaining the observed effects, figure 7 presents event studies for subsamples based on the traffic court disposition associated with the citation.

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;13 (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, there are severe interpretation challenges associated with both of the latter two categories. In particular, it is tempting 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 waivering 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 7 show that estimated impacts for this “possible lenience” group are significantly attenuated relative to the the estimates for fine payers, about half the size in each case, but non-zero. For the subgroup of motorists who may have faced DL suspensions due to nonpayment, the increase in collections balances is significantly more pronounced relative to the the increase for fine payers. For payroll employment, however, this dimension of heterogeneity is less stark; while

13To keep the benchmark results the same throughout figure 7, 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 7.
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 7, 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, as opposed to license suspensions, as the primary driver of the financial distress and employment instability effects in the full sample. Importantly, this result also highlights that the main collections balances estimate cannot be explained by collections originating with unpaid citations, since those with paid fines would not be subject to collections activity associated with their citation.

In panels (c) and (d) of figure 7, I assess the relative importance of driver license points, which are accrued by fine payers and can affect future car insurance premiums, in explaining the results. Specifically, I compare effects for those with “paid” (same as above) and “traffic school” disposition verdicts. Traffic school attendees are required to pay their fines but, in return for completing a four hour course, do not accrue the driver license points associated with the citation. Hence, comparing effects for these two groups can shed light on the relative importance of accruing DL points. For both collections balances and payroll employment, estimates appear similar, but slightly smaller, for those attending traffic school.

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

5.1.5 Further discussion of payroll employment results

While figure 7 potentially suggests some role for institutional features such as DL points and associated increased driving costs in explaining the estimated impacts of citations on the payroll employment measure, it also highlights that fine payment itself appears to explain the lion’s share of the effect. A natural question warranting further discussion is why paying an unplanned expense might affect employment arrangements.

As shown in figure 6 and figure A-9, declines in the likelihood of working in a payroll-covered job are concentrated among lower-income motorists, who also see the largest increases in financial distress. Hence, the hypothesis that weaker financial standing and a diminished
credit reputation induces employment instability or reduces the ability to obtain or hold good jobs is at least consistent with the evidence. In particular, a lower credit score could affect job-finding directly (e.g., Bartik & Nelson 2021; Bos et al. 2018) or indirectly through a compromised ability to secure new housing, for example.

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

One could alternatively ask to what extent the payroll employment effects can themselves explain the observed increases in financial distress. In trying to answer this question, there are two important considerations. The first is dynamics: as discussed in the section 3.3 and shown in figure D-3, new defaults induced by changes in (payroll) employment status will typically take several quarters to accrue on a credit report. By this logic alone, the gradual changes in payroll employment status would appear quite unlikely to explain the initial increases in default in the first six quarters following a traffic stop.

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

5.1.6 Robustness

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

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

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

In figure A-7, I also show that the choice of method for estimating the event studies has no bearing on the empirical conclusions. Estimates based on either the Borusyak et al. (2022) or Sun & Abraham (2021) approach are nearly identical the baseline estimates using the Callaway & Sant’Anna (2021) method. Because the Sun & Abraham (2021) approach uses only the final cohort as the “control” group and thereby relies on fewer comparisons, standard errors about 75 percent larger. Hence, the Callaway & Sant’Anna (2021) estimates are preferable on precision grounds. As detailed in section C-3, computing constraints prevent the computation of standard errors for the Borusyak et al. (2022) approach.

Below, I discuss results from the instrumental variables approach, which compares individuals cited at the same time, this circumventing the central identification challenge associated with the event study approach.

### 5.2 Instrumental variables results

Panel (a) of figure 8 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;
Although the standard error is large (11.7), the estimate is statistically significant at conventional levels.\textsuperscript{14}

Figure B-4 in the appendix plots the corresponding estimates over all (feasible) time horizons for the full set of outcomes. As in the event study analysis, slight increases in the 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 3, which reports IV estimates in different specifications.

Columns 1-2 of table 3 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 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 8, panel D of table 3 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.

\textsuperscript{14}Figure B-5 illustrates that this reduced form relationship is more pronounced for lower-income motorists ($\beta = \$44.3$, se = 14.5) than for higher-income motorists ($\beta = \$16.3$, se = 13.13).
5.2.1 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 B-4). Hence, the estimates in both approaches may suggest similar overall impacts on financial distress. Unfortunately, this hypothesis is not directly testable without a dollar metric for delinquency, which the data do not include.

Finally, differences across the two approaches may be due to a positive correlation between treatment effects and LATE weights. In this setting, every motorist is a complier for at least some value of the instrument; this fact follows from the presence of officers who always and never bunch drivers, which rules out the existence of always and never takers, given the LATE assumptions. However, the weights underlying the LATE will vary across drivers based on their compliance across the stringency distribution (Frandsen et al., 2019).

5.2.2 Robustness

In the appendix, I subject the primary officer IV estimates to several robustness checks. First, to assuage lingering concerns that results are driven by differentially selected samples across officers of varying stringencies, I present 2SLS estimates after trimming the data of officers with relatively selected samples based on motorist characteristics, following Feigenberg & Miller (2022) and Goncalves & Mello (2022). As shown in figure B-6, the IV estimates are very similar, and if anything slightly larger, even after dropping up to 50 percent of the officers in the data based on disparities between and observed and predicted sample
composition, given beat and shift assignments, motorist composition.

In table B-3, I show that IV estimates are qualitatively and quantitatively similar when using alternative versions of the stringency instrument, including estimated officer fixed effects after applying empirical Bayes shrinkage and a binary version of the instrument that compares bunching and non-bunching officers. Figure B-7 illustrates similar estimates when restricting the IV sample to the first in-sample stop for each motorist (i.e., so that each driver appears in the data only once) and requiring that each motorist has faced no other citations in the previous year, resulting in a sample of 272,866 motorists.

6 Heterogeneity by motorist race

An exploration of heterogeneity by motorist race is warranted both by the question and the context of this paper. One of the most salient dimensions of inequality in the present day United States is the racial wealth gap: current estimates suggest that the median Black household holds about one sixth the liquid wealth of the median white household, with comparable gaps between white and Hispanic households (e.g., Derenoncourt et al. 2022). Moreover, the shocks considered in this paper are a feature of the criminal justice system. A large literature in economics has documented the sizable racial disparities in criminal justice outcomes: relative to observably similar whites, racial minorities are more likely to be stopped by the police (Coviello & Persico, 2013), issued a harsh traffic fine (Goncalves & Mello, 2021), convicted of a crime (Anwar et al., 2012), denied bail (Arnold et al., 2018), and issued a lengthy prison sentence (Rehavi & Starr, 2014).

Figure 9 presents event study estimates separately for white and Black or Hispanic (“minority”) drivers, restricting to the subset with dispositions indicating fine payment to abstract away from differences in interactions with the traffic court system across racial groups, documented in table C-3. As indicated in panel (a), the increase in unpaid bills in collections following a traffic stop is more pronounced for minority motorists than for white motorists. Although I cannot reject that the effect is equal across the two groups, the marked difference in point estimates is worth highlighting: the ATT estimate for minority motorists is about 50 percent larger ($48, se = $9.6 versus $31, se = $5.9). Further, as shown in panel (c), differential default rates cannot be explained by differential substitution between credit card borrowing and default across the two groups. If anything, minority motorists borrow slightly more on credit cards while also accruing additional collections balances, a pattern which is consistent with lower levels of cash-on-hand or liquid savings for minority households. Panel (d) of figure 9 illustrates a considerably more pronounced decline in the likelihood of holding a payroll-covered job for minority motorists following a traffic stop.

Figure 10 explores heterogeneity by motorist race using the officer IV design. Note that, to simplify exposition (i.e., to minimize the need to check pre-stop trends and differences in pre-stop trends across race in each specification), I use the DiD formulation, which uses
the difference in pre and post-stop changes as the outcome, when presenting the IV results by race. Panel (a) of figure 10 illustrates a stark disparity in the reduced form relationship between the officer stringency instrument and growth in collections balances over the three quarters following a traffic stop. The estimated reduced form coefficient ($\$53, se = $22$) is about three times larger for minority motorists than for whites ($\$15, se = $17$) and the difference is statistically significant at the ten percent level. Importantly, this disparity cannot be explained by differences in the first stage across groups; the first stage coefficient is about $1.40$ larger for white motorists.

A particularly useful feature of the IV approach for examining racial disparities is that it can easily accommodate motorist controls. Panel (b) reports the same reduced form estimates after conditioning on age, gender, baseline estimated income, credit score, and available balances on credit cards. Specifically, these estimates are obtained from a joint regression that does not allow the influence of beat-shift effects or motorist controls to vary by race. This approach is specifically chosen to quantify the extent to which differences in panel (a) can be explained by differences in salient observable characteristics across the two groups of motorists. Adjusting for racial differences in observable characteristics reduces the estimated racial gap by about 25% and renders the difference statistically insignificant ($p = 0.19$). Still, the point estimate remains about 2.5 times larger for minority estimates and the corroborating visual evidence in figure 10 is quite striking.\[15\]

Table 4 expands on this analysis by examining the sensitivity of the estimated racial disparities to the inclusion of different sets of controls. Focusing first on the shorter-run IV estimates in columns (1)-(3), which correspond to the plots in figure 10, panels A-C demonstrate that adjusting for differences in demographics and baseline estimated income across motorist groups has minimal impact on the estimated disparity. The 2SLS estimates for white and minority estimates are stable in the range of 0.135 and 0.412, respectively, while tests of the difference yield $p$-values below 0.09.

Panel D, which adds controls for motorist credit scores and available balances on credit cards, has the most significant impact on the estimated disparity, increasing the estimate for white motorists to 0.161 and reducing the estimate for minority motorists to 0.37 ($p = 0.19$). Additionally adding controls for the presence of open auto loans or mortgages on the credit file has minimal effect on either estimate or the disparity, as shown in panel E.

Columns 4-6 report the identical set of estimates for six quarters out from the traffic stop, which offer several important takeaways. Firstly, the growth in collections balances induced by marginal fine increases is significantly more persistent for Black and Hispanic individuals than for white individuals. Estimates over the six-quarter time horizon are consistently about 50 percent larger than those for the three-quarter time horizon for minority drivers,\[15\]

\[15\]In figure 10, I show that the racial gap in estimated treatment effects is similar, and if anything larger, when using a version of the officer stringency instrument that is computed only with driver racial groups.
while the longer-run estimates are only about 15 percent larger for white drivers. Second, the sensitivity of the longer-term estimates to controls mirrors the pattern in columns 1-3; adjusting for differences in access to credit reduces the estimated racial gap in treatment effects by about 25 percent. And finally, the differences in IV estimates across driver race are consistently distinguishable from zero in a statistical sense, at least at the ten percent level ($p \leq 0.053$), when examining the longer time horizon

Focusing on columns 4-6 in panel E, which includes the full set of motorist controls, the 2SLS estimates suggest that, as of six quarters out from the traffic stop, about 18 percent of the marginal fine increase induced by officer stringency has appeared as unpaid bills in collections on the typical white driver’s credit report. This estimate is not statistically distinguishable from zero. On the other hand, the comparable estimate for Black and Hispanic drivers is a striking 57 percent ($p < 0.05$). In other words, this estimate implies that, on average, minority motorists borrow an additional $60 out of other financial obligations in order to meet an additional $100 in fines.

One question raised by the analyses in figure 10 and table 4 is why the racial disparities appear more dramatic when using the IV approach than when examining event study estimates. Note that any of the points laid out in section 5.2.1 also apply here. In particular, one compelling argument is that differences in savings or liquid wealth across racial groups result in differential convexity in the relationship between fine amounts and default. In other words, as shocks become larger, the race gap in default becomes larger, because the race gap in the presence of additional savings to draw on becomes larger.

A perhaps more interesting question raised by the IV results is why the racial disparities persist even after adjusting for differences in the financial situations of white and minority households. Of course, an important drawback of the credit bureau data is that it lacks measures of cash-on-hand, checking account balances, or liquid savings more generally. Worth noting here is the result of Ganong et al. (2020), who find that the racial gap in consumption responses to income shocks can be explained away by differences in liquid savings across racial groups. Hence, one possibility is that, even after conditioning on estimated income, credit score, available credit card balances, and durable loans, there remains important variation in liquid savings across racial groups which explains the observed differences in accrued collections debt following a traffic fine.

Another possibility is that, even conditional on liquidity, there are important differences in the availability of resources to cover emergency expenses across racial groups. For example, one implication of the existing research on the racial wealth gap (e.g., Derenoncourt et al. 2022) is that liquid savings tend to be lower in minority communities more generally, meaning that Black and Hispanic households may have less access to informal loans from family or friends when faced with expense shocks, thus inducing a higher rate of borrowing out of other financial obligations for these groups.

 Nonetheless, the unconditional racial disparities are consequential in light of the relevant
evidence on racial bias in traffic enforcement. Pierson et al. (2020) find that Black and Hispanic motorists are more likely to be stopped than comparable white drivers and event study estimates suggests the consequences of those stops in terms of default are about 50 percent more severe for minority households. Goncalves & Mello (2021) find that Black and Hispanic motorists are about six percentage points more likely to face harsh fines than white motorists when ticketed for speeding and the IV results imply that the consequences of those harsher fines are significantly larger for racial minorities. Hence, these findings can speak to the broader social costs of racially biased policing.

7 Discussion

7.1 Financial fragility in surveys

The central contribution of my analysis is the finding that typical, unplanned expense shocks can have important implications for household financial situations. I interpret the headline event study results on collections balances as evidence that, on average, households must default on other financial obligations to finance an unexpected $200 payment. This interpretation is supported by the fact that default effects are comparable for the subsample of fine-payers, as shown in section 5.1.4, and the analysis in section 5.1.1, which suggests that default ultimately leading to collections activity is the “last resort” for households.

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

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

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

A useful starting point, then, is to set $\Delta = 195.53$; i.e., households are either unaffected by fines or are induced to miss an entire fine’s worth of bills. Taking the lower 95 percent confidence bound of the ATT estimate for collections balances, $\hat{\theta} = 29.99$, then, yields an estimated $\pi \geq 0.133$. Importantly, this is a sharp lower bound for $\pi$ as long as the distribution of treatment effects is bounded below by zero and above by $91.53$, because a larger $\pi$ can always be rationalized by allowing probability mass on other values of $\Delta_i \in (0, 195.53)$.

Of course, this lower bound may be overly conservative. On average, fines also induce default on credit lines, suggesting that defaults which ultimately lead to collections are not the only margin of adjustment. Further, there is no evidence in the data for treatment effects as large as $\Delta = 195.53$. Estimating the collections balance treatment effect using only the bottom five percent of the baseline estimated income distribution yields $\hat{\theta} = 52.18$ (22.98). Using the upper 95 percent confidence bound of this estimate as the upper limit for treatment effects, $\tilde{\Delta} = 9.22$, gives a lower estimate of $\pi \geq 0.298$.

One can also estimate an upper bound on $\pi$ by examining the share of individuals who are induced into their first default by fines. Panel (a) of figure 11 presents event study estimates where the outcome variable is an indicator for whether a motorist has accumulated any new default flag to date. For reference, just over 60 percent of the sample has already accrued a default flag as of one quarter prior to their traffic stop, while just over 20 percent of the sample never accrues a default flag over 2010–2015. Six quarters out from the traffic stop, the event study estimate is $\hat{\beta} = 0.005$ ($se = 0.0006$). In other words, of the 39.4 percent of the sample that has yet to accrue a default flag as of one quarter prior to treatment, about 1.3 percent are induced into default by fines.

Panel (a) of figure 11 reveals an important dimension of heterogeneity in the data: among individuals who are not prone to default in general, the impact of fines appears minimal. Another way to see this point is to estimate effects on collections debt for individuals with (62 percent) and without (38 percent) at least one default flag on their credit report at baseline. As shown in panel (b) of figure 11, estimated effects of fines are approximately zero for those with clean credit reports at baseline, whereas effects are about one third larger for the any default subsample than for the full sample.

Based on the following logic, the estimate in figure 11, panel (a), provides an informative upper bound on $\pi$. As of one quarter prior to treatment, 39.4 percent of the sample is at risk of being “pushed” into their first default to date by a traffic fine and 0.005 percent of the sample is induced to default. Hence, at least 38.9 percent of the sample is unaffected by fines, meaning that at most 61.1 percent is affected. To summarize, I find that $\pi \geq 0.13$.
and my best estimate is $\pi \in [0.298, 0.611]$. In other words, between 30 and 60 percent of individuals are induced to borrow from other financial obligations when faced with a $195 traffic fine. My estimates, therefore, cannot rule out the oft-cited statistic that 40 percent of households cannot absorb a “typical” unplanned expense shock.

Of course, following the same logic as above, heterogeneity in the treatment effect estimates implies significant heterogeneity in the prevalence of financial precarity across groups. The comparable lower bounds for lower-income individuals, or those without easy access to credit card borrowing, are in the range 35-40 percent. The stark heterogeneity in the IV estimates by motorist race suggest that, particularly for larger shocks, inability to absorb unplanned expenses is likely much more prevalent in minority communities.

### 7.2 Consumption smoothing

A vast literature in economics has examined the consumption responses to income fluctuations (e.g., Stephens 2001; Parker 2017; Ganong et al. 2020; Golosov et al. 2022; Baker & Yannelis 2017; Gelman et al. 2020; Ganong & Noel 2019). A contribution of my paper to this literature is compelling evidence on the consumption smoothing strategies of household when faced with typical negative income shocks. The evidence presented in section 5.1.1 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.

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

Evaluated at the median household incomes by race in Florida, the estimates in Ganong et al. (2020) predict a $38 decline in consumption for white households and a $52 decline in consumption for minority households.\(^{16}\) My results imply that, in order to prevent these

\(^{16}\)Median monthly household incomes for white and minority households in Florida as of the 2010 census were $4193 and $3206, respectively. The average fine, $195, thus represents a 4.5 (6) percent decline in monthly income for white (minority) households. The consumption elasticity
declines from being more dramatic, white households borrow about $32 and minority households borrow about $48 from other financial obligations. Hence, my findings suggest that Ganong et al. (2020) may underestimate the racial gap in the welfare costs of income volatility by illustrating that minority households additionally must borrow more intensely out of other financial obligations, with associated further declines in welfare through reduced access to future borrowing, when faced with negative shocks.

7.3 Social insurance

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

$$\frac{u_b' - u_g'}{u_g'} = \frac{\epsilon_{p,b}}{1 - p}$$

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

The empirical findings speak to the difference in marginal utilities associated with typical, negative income shocks in a dynamic sense: shocks induce default, which in turn reduce welfare in the future through tighter borrowing limits, higher interest rates, and employment instability, as discussed in section 5.1.2. While quantifying the implied differences in marginal utilities is beyond the scope of this paper, the results suggest the important lesson that many households are not self-insured against even typical income fluctuations. Hence, an expanded social safety net which insures against a larger set of usual shocks could yield social welfare gains, abstracting away from the obvious logistical and moral hazard concerns. Note that my findings mesh well with a nascent literature documenting the role of existing social insurance programs such as medicaid in insuring households against financial distress (e.g., Mazumder & Miller 2016; Gallagher et al. 2019).

Or alternatively, the results may imply a need for policy interventions with the goal of preventing households from reaching the point where they are unable to self-insure against typical income fluctuations, such as financial education or savings incentives (e.g., Klapper & Lusardi 2020, Lusardi et al. 2011). The relative social welfare gains from an expanded social insurance system versus expanded financial education, for example, would depend estimates in Ganong et al. (2020) are 0.2 for white households and 0.265 for Black and Hispanic households (taking a simple average of the reported estimates for each racial group). Hence, predicted consumption declines are $0.2 \times 0.045 \times 4193 = 37.74$ for white households and $0.265 \times 0.06 \times 3206 = 51.98$ for minority households.

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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.

8 Conclusion

Motivated both by a growing body of evidence suggesting the inability of low-income house-
holds to cope with unexpected expenses and the observation that the incidence of policing
falls largely on disadvantaged communities, this paper studies the effect of speeding fines on
financial wellbeing. To estimate causal effects, I link administrative traffic citation records
to a panel of credit reports for cited drivers and rely on the staggered timing of traffic stops,
as well as variation in fine amounts generated by police behavior, for identification.

I find that traffic fines averaging $195 are associated with increases in unpaid bills in col-
cections of about $34. I interpret this as evidence that, on average, households must borrow
$17 out of other financial obligations such as utility or medical bills to cover a $100 unplanned
expense. This interpretation is supported by two additional results. First, the effect of fines
on unpaid bills is attributable to fine payment itself, rather than other institutional expla-
nations such as traffic court involvement, driver license points, or driver license suspensions.
And second, heterogeneity analysis by proxies for an individual’s financial buffer reveal that
default which ultimately leads to collections activity is the “last resort” for households, pre-
ceded by paying with cash on hand, formal borrowing, and delaying credit line payments as
sources for financing unplanned expenses.

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

My finding that fines are associated with increased financial distress, declines in credit
reputation, and reduced employment stability suggests that, on average, households cannot
easily absorb typical, but unplanned, expense shocks. Impacts of fines are considerably larger
for the lowest-income motorists and Black and Hispanic motorists, suggesting particular low
ability to cope with unplanned expenses in these subgroups. Based on reasonable assump-
tions about the distribution of treatment effects, the results suggest that between 30 and 60
percent of individuals in the sample are induced to borrow from other financial obligations
when faced with an emergency $195 expense, consistent with recent survey evidence on the
prevalence of financial fragility.
References


Strain, M. (2019). Americans may be strapped, but the go-to statistic is fake. *Bloomberg*.


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

<table>
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<th>(1) Florida Drivers on File</th>
<th>Event Study</th>
<th>IV</th>
</tr>
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<tbody>
<tr>
<td><strong>Panel A: Demographics</strong></td>
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<td>Female</td>
<td>0.51</td>
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<td><strong>Panel B: Financial Distress</strong></td>
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<tr>
<td>Any Payroll Earnings</td>
<td>0.12</td>
<td>0.13</td>
<td>0.13</td>
</tr>
<tr>
<td>Monthly Earnings</td>
<td>2975</td>
<td>3319</td>
<td>3284</td>
</tr>
<tr>
<td><strong>Panel D: Citation Information</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fine Amount</td>
<td>171.85</td>
<td>195.53</td>
<td>197.62</td>
</tr>
<tr>
<td>DL Points</td>
<td>1.74</td>
<td>3.39</td>
<td>3.43</td>
</tr>
<tr>
<td>Definitely Paid</td>
<td>0.465</td>
<td>0.589</td>
<td>0.592</td>
</tr>
<tr>
<td>Possible Lenience</td>
<td>0.401</td>
<td>0.333</td>
<td>0.304</td>
</tr>
<tr>
<td>Possible Suspension</td>
<td>0.134</td>
<td>0.078</td>
<td>0.104</td>
</tr>
<tr>
<td>Individuals</td>
<td>14800000</td>
<td>2631641</td>
<td>525646</td>
</tr>
</tbody>
</table>

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 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. (2022) pretend test, the estimated counterfactual mean at τ = 6, and the static ATT estimate.
Table 2: Event study estimates for default outcomes

<table>
<thead>
<tr>
<th></th>
<th>Collections</th>
<th>Credit Lines</th>
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</thead>
<tbody>
<tr>
<td></td>
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<td>(2)</td>
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<tr>
<td>Any New Default</td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td><strong>Event Study Estimates</strong></td>
<td>**(1)</td>
<td>**(2)</td>
</tr>
<tr>
<td>$\tau = 1$</td>
<td>0.003</td>
<td>0.01</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>$\tau = 4$</td>
<td>0.01</td>
<td>0.041</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>$\tau = 6$</td>
<td>0.009</td>
<td>0.067</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>AT&amp;T</td>
<td>0.005</td>
<td>0.06</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.006)</td>
</tr>
</tbody>
</table>

**Counterfactual Means**

|                  | **(1)                          | **(2)                          | **(3)                          | **(4)                          | **(5)                          |
| $\tau = 1$       | 0.22                         | 2.36                         | 1395                         | 2.31                         | 1.57                         |
| $\tau = 6$       | 0.22                         | 2.36                         | 1392                         | 2.32                         | 1.55                         |

**Tests for Parallel Trends**

\[ p = 0.835 \quad p = 0.525 \quad p = 0.357 \quad p = 0.821 \quad p = 0.176 \]

Notes: This table reports event study estimates for one, four, and six quarters post traffic stop, as well as the static AT&T estimate, all obtained via the Callaway & Sant’Anna (2021) approach. Design-based standard errors from Roth & Sant’Anna (2022) in parentheses. The lower panels report counterfactual means for $\tau = 1$ and $\tau = 6$, estimated using the method described in the text, and results of the pretrends test from Borusyak et al. (2022). The sample is the full event study sample ($N = 525,646$).
Figure 3: Event study estimates by baseline credit access

(a) Any New Distress

(b) Collections Balances

(c) Revolving Balances

(d) Revolving Utilization

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 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).

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Figure 5: Event study estimates for long-run outcomes

(a) Credit Score

(b) Subprime

(c) Borrowing Limit

(d) Payroll Employment

Notes: 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 = 1[credit score < 600]. Borrowing limit is the sum of the observed limits across all revolving accounts and thus equals zero for individuals with no revolving lines. Imputed borrowing limit is imputed based on the credit score, as described in appendix C.
Figure 6: Event study estimates by baseline payroll status

(a) Collections Balances

(b) Payroll Employment

(c) Collections Balances

(d) Payroll Employment

Notes: Each figure reports event study estimates, obtained via the Callaway & Sant’Anna (2021) approach, as well as 95 percent confidence intervals based on design-based standard errors from Roth & Sant’Anna (2022), for collections balances or payroll employment, estimated separately by baseline payroll employment status. In panels (a) and (b), the sample is split by whether a motorist was “employed” at baseline, defined as being in the payroll records for all four quarters of 2010 (N employed = 55,140, N other = 470,506). In panels (c) and (d), the sample is the baseline “employed” sample and is split at the median of earnings in 2010, which is $34,198 (N low = 27,570; N high = 27,570).
Figure 7: 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 full 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 $\textit{may}$ have received punishment reductions ($N = 175,051$) and red diamonds report estimates for those who $\textit{may}$ 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.
Figure 8: Instrumental variables approach

(a) Histogram of Charged Speeds

(b) First Stage

(c) Reduced Form

Notes: Panel (a) shows 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 3: Officer IV Results

<table>
<thead>
<tr>
<th></th>
<th>With Controls</th>
<th>Without Controls</th>
</tr>
</thead>
<tbody>
<tr>
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<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td><strong>Collections</strong></td>
<td></td>
<td></td>
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<tr>
<td><strong>Revolving</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Panel A: First Stage</strong></td>
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<td></td>
</tr>
<tr>
<td>Fine Amount</td>
<td>124.01</td>
<td>(0.47)</td>
</tr>
<tr>
<td><strong>Panel B: Δ −4 to −1</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Reduced Form</td>
<td>-0.41</td>
<td>-3.51</td>
</tr>
<tr>
<td></td>
<td>(9.88)</td>
<td>(30.15)</td>
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<tr>
<td>2SLS</td>
<td>-0.003</td>
<td>-0.028</td>
</tr>
<tr>
<td></td>
<td>(0.08)</td>
<td>(0.243)</td>
</tr>
<tr>
<td><strong>Panel C: Δ −1 to 1</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Reduced Form</td>
<td>7.32</td>
<td>24.6</td>
</tr>
<tr>
<td></td>
<td>(8.51)</td>
<td>(25.55)</td>
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<tr>
<td>2SLS</td>
<td>0.059</td>
<td>0.198</td>
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<tr>
<td></td>
<td>(0.069)</td>
<td>(0.206)</td>
</tr>
<tr>
<td>2SLS DiD</td>
<td>0.062</td>
<td>0.227</td>
</tr>
<tr>
<td></td>
<td>(0.108)</td>
<td>(0.329)</td>
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<tr>
<td><strong>Panel D: Δ −1 to 3</strong></td>
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<td>Reduced Form</td>
<td>29.83</td>
<td>6.54</td>
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<td></td>
<td>(11.71)</td>
<td>(35.29)</td>
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<td>2SLS</td>
<td>0.241</td>
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<tr>
<td></td>
<td>(0.094)</td>
<td>(0.285)</td>
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<td>2SLS DiD</td>
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<td></td>
<td>(0.128)</td>
<td>(0.394)</td>
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<td><strong>Panel E: Δ −1 to 6</strong></td>
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<td>Reduced Form</td>
<td>42.6</td>
<td>-5.14</td>
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<td></td>
<td>(15.06)</td>
<td>(44.36)</td>
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<td>2SLS</td>
<td>0.344</td>
<td>-0.041</td>
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<tr>
<td></td>
<td>(0.121)</td>
<td>(0.358)</td>
</tr>
<tr>
<td>2SLS DiD</td>
<td>0.347</td>
<td>-0.013</td>
</tr>
<tr>
<td></td>
<td>(0.152)</td>
<td>(0.459)</td>
</tr>
</tbody>
</table>

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 B-4. Sample is the IV sample, $N = 362,854$. 

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Figure 9: Event study estimates by race

(a) Collections Balances

(b) Credit Line Delinquencies

(c) Revolving Balances

(d) Payroll Employment

Notes: Each figure reports event study estimates, obtained via the Callaway & Sant’Anna (2021) approach, as well as 95 percent confidence intervals based on design-based standard errors from Roth & Sant’Anna (2022), for the denoted outcome. Event studies are estimated separately for white (N = 195,373) and Black or Hispanic (114,225) motorists with dispositions indicating a paid fine or traffic school election (the “definitely paid” subset).
Figure 10: Officer IV reduced form estimates by race

(a) Without controls

White: $b = 15.85$ (17.28)
Minority: $b = 52.68$ (21.18)
Diff = 36.83 (21.38)

(b) With controls

White: $b = 19.08$ (17.29)
Minority: $b = 47.35$ (21.18)
Diff = 28.27 (21.384)

Notes: 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 4: Officer IV results by race

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

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, \((Y_\tau - Y_{-1}) - (Y_{-1} - Y_{-3})\), 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.
Figure 11: Event study estimates on extensive and intensive margins

(a) Any Default to Date

(b) Collections by Baseline Default

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. 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: $N = 324,576$).
A Appendix Figures and Tables

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

Notes: This figure provides a flow chart summarizing driver choices and the associated outcome(s) for each choice. The $10 surcharge for traffic school attendees represents the typical net surcharge, $25 for the course minus a $15 fine reduction. The citations debt in collections ($221) for non-payers assumes a 40 percent collections fee, the maximum allowed by law. Note that such collections activity, to the best of my knowledge, will not appear on the credit reports used in the empirical analysis. The $75 surcharge for contesters is the standard court fee.
Figure A-2: Pretrends by violation type

(a) Any New Distress

(b) Collections Balances

Notes: This figure reports results from the parallel trends test of Borusyak et al. (2022) for the primary event study sample of speeders ($N = 525,646$) and a sample, constructed in an identical way, of individuals who commit non-moving traffic violations, such as paperwork and equipment infractions ($N = 625,097$).
Figure A-3: Event study cohorts

(a) Distribution of Timing

(b) Characteristics by Timing

(c) Cumulative Stops in Event Time

Notes: Panel (a) plots the distribution of treatment timing (“cohort”) for event study sample ($N = 525,646$). Panel (b) illustrates characteristics of each cohort. Panel (c) shows how the cumulative number of traffic stops for each driver varies in event time.
Figure A-4: Trends in car ownership around traffic stops

(a) Any Car Loan Around Traffic Stop

(b) Credit Score Around Car Purchase

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-5: Event study estimates relative to counterfactual control means

(a) Any New Distress

(b) Collections Balances

(c) Payroll Employment

(d) Revolving Balances

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) using the full event study sample ($N = 525,646$). Counterfactual means are estimated using the method described in the text. 95 percent confidence bands for the estimated counterfactual means are obtained via a Bayesian bootstrap clustered at the motorist-level.
Figure A-6: Event study estimates conditional on car purchase timing

(a) Any New Distress

(b) Collections Balances

(c) Payroll Employment

(d) Revolving Balances

Notes: This figure reports event study estimated which condition on the timing of an individual’s auto first purchase. In each panel, the solid blue circles report the main estimate (same as reported in the main text) for the full event study sample \(N = 525,646\). Hollow green squares and hollow purple diamonds report estimates using only the subsets of individuals who already have an auto loan at baseline \(N = 216,625\) and who never have auto loans during the sample period \(N = 110,540\). Solid orange squares report estimates within auto timing groups for those who first purchase cars in or after 2010Q2 and purchase cars prior to their traffic stop \(N = 111,059\), obtained by estimating event studies separately for each auto purchase cohort and then aggregating up, weighting by sample shares.
Figure A-7: Event study estimates via alternative methods

(a) Any New Distress

(b) Collections Balances

(c) Payroll Employment

(d) Revolving Balances

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. (2022) (purple diamonds).
Figure A-8: 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 A-9: 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 A-10: 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 A-11: 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 A-12: 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 A-13: 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 A-14: 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

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 A-15: Event study estimates for other long-run outcomes

(a) Any Open Credit Account

(b) Any Mortgage

(c) Any Bankruptcy to Date

(d) New Address

Notes: This figure reports event study estimates obtained from the primary Callaway & Sant’Anna (2021) specification (blue circles) as well as from a specification based on Sun & Abraham (2021) which compares only those cited in 2011–2012 (“early” cohorts) to those cited in 2015Q4 (“late” cohort). Imputed borrowing limit is imputed based on the credit score, as described in appendix C. 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-16: Heterogeneity for long-run outcomes

(a) Credit Score  
(b) Credit Score  
(c) Credit Score  
(d) Revolving Limit  
(e) Revolving Limit  
(f) Revolving Limit  
(g) Imputed Limit  
(h) Imputed Limit  
(i) Imputed Limit  

Notes: This figure reports heterogeneity in the estimates for longer-run outcomes by baseline estimated income, baseline credit card situation, and by motorist race.
Figure A-17: 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 9 except additionally showing results for the full sample (i.e., those with any court disposition) \( N \text{ white } = N = 308,116; N \text{ Black or Hispanic } = 217,530 \) and for the subset who both pay their fines and with less than $200 in available balances on credit cards \( N \text{ white } = 105,529; N \text{ Black or Hispanic } = 118,799 \).
Table A-1: Event study estimates for credit card outcomes

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<tbody>
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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. (2022). The sample is the full event study sample ($N = 525,646$) and the average fine is $195.53$. 

71
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Notes: Same as tables 2 and A-1, broken down by motorist credit card situation at baseline (top two panels) and motorist estimated income at baseline (bottom two panels).
Table A-3: Event study estimates by baseline income and liquidity

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Notes: Same as tables 2 and A-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.
B Additional IV Results

Figure B-1: Evidence of officer behavior

(a) Distribution of Officer Effects  
(b) Correlation of Officer Effects

Notes: Panel (a) plots the distribution of estimated officer fixed effects from a regression of  \( 1[\text{harsh fine}] \), 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 B-2: 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 B-1: Randomization test

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<td>-0.010200266</td>
<td>-0.000608807</td>
<td>0.002252310</td>
</tr>
<tr>
<td>Speeding Past Year</td>
<td>0.027481035</td>
<td>0.003004149</td>
<td>0.004030380</td>
</tr>
<tr>
<td>Other Past Year</td>
<td>0.020618536</td>
<td>0.001886688</td>
<td>0.003126596</td>
</tr>
<tr>
<td>Credit Score</td>
<td>-0.0000050305</td>
<td>0.000001815</td>
<td>-0.00000260</td>
</tr>
<tr>
<td>Any Auto Loan</td>
<td>-0.001412710</td>
<td>0.001369150</td>
<td>-0.000021948</td>
</tr>
<tr>
<td>Collections Balance</td>
<td>0.000001222</td>
<td>-0.000000105</td>
<td>-0.000000246</td>
</tr>
<tr>
<td>Revolving Balance</td>
<td>0.000000087</td>
<td>0.000000075</td>
<td>0.000000102</td>
</tr>
<tr>
<td></td>
<td>(0.002042597)</td>
<td>(0.001310563)</td>
<td>(0.001747316)</td>
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<tr>
<td></td>
<td>(0.000556643)</td>
<td>(0.000373237)</td>
<td>(0.000467060)</td>
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<td></td>
<td>(0.000006526)</td>
<td>(0.000004369)</td>
<td>(0.000005478)</td>
</tr>
<tr>
<td></td>
<td>(0.002760112)</td>
<td>(0.002099292)</td>
<td>(0.00260463)</td>
</tr>
<tr>
<td></td>
<td>(0.002918837)</td>
<td>(0.002463199)</td>
<td>(0.003805099)</td>
</tr>
<tr>
<td></td>
<td>(0.003390758)</td>
<td>(0.003023269)</td>
<td>(0.004072405)</td>
</tr>
<tr>
<td></td>
<td>(0.003105808)</td>
<td>(0.001680398)</td>
<td>(0.002169710)</td>
</tr>
<tr>
<td></td>
<td>(0.002215740)</td>
<td>(0.001323286)</td>
<td>(0.001867001)</td>
</tr>
<tr>
<td></td>
<td>(0.000009803)</td>
<td>(0.000006746)</td>
<td>(0.000008515)</td>
</tr>
<tr>
<td></td>
<td>(0.001401109)</td>
<td>(0.000002545)</td>
<td>(0.001261897)</td>
</tr>
<tr>
<td></td>
<td>(0.000000323)</td>
<td>(0.000000202)</td>
<td>(0.000000269)</td>
</tr>
<tr>
<td></td>
<td>(0.000000083)</td>
<td>(0.000000052)</td>
<td>(0.000000067)</td>
</tr>
<tr>
<td>Joint test</td>
<td>25.27</td>
<td>2.64</td>
<td>1.79</td>
</tr>
<tr>
<td>p-val: All</td>
<td>&lt;0.001</td>
<td>0.02</td>
<td>0.044</td>
</tr>
<tr>
<td>p-val: Demographics</td>
<td>&lt;0.001</td>
<td>0.01</td>
<td>0.039</td>
</tr>
<tr>
<td>p-val: Credit Bureau</td>
<td>&lt;0.001</td>
<td>0.28</td>
<td>0.484</td>
</tr>
</tbody>
</table>

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 dependent 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>
<thead>
<tr>
<th>Subgroup</th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Female</td>
<td>124.18</td>
<td>124.26</td>
</tr>
<tr>
<td></td>
<td>(0.561)</td>
<td>(0.651)</td>
</tr>
<tr>
<td>Age &gt; 35</td>
<td>123.99</td>
<td>124.26</td>
</tr>
<tr>
<td></td>
<td>(0.62)</td>
<td>(0.583)</td>
</tr>
<tr>
<td>Minority</td>
<td>123.37</td>
<td>124.26</td>
</tr>
<tr>
<td></td>
<td>(0.579)</td>
<td>(0.642)</td>
</tr>
<tr>
<td>Past Offense</td>
<td>124.29</td>
<td>124.26</td>
</tr>
<tr>
<td></td>
<td>(0.514)</td>
<td>(0.868)</td>
</tr>
<tr>
<td>High Income</td>
<td>123.23</td>
<td>124.26</td>
</tr>
<tr>
<td></td>
<td>(0.599)</td>
<td>(0.6)</td>
</tr>
<tr>
<td>High Credit Score</td>
<td>123.32</td>
<td>124.26</td>
</tr>
<tr>
<td></td>
<td>(0.621)</td>
<td>(0.586)</td>
</tr>
</tbody>
</table>

Notes: 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 B-3: 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 B-4: Dynamic reduced form estimates

(a) Any New Distress
(b) Collections Balances
(c) Revolving Balances
(d) Any Auto Loan
(e) Credit Score
(f) Payroll Employment
(g) Delinquencies
(h) Derogatories

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 $\tau$ indexes event time) on the officer stringency instrument. All regressions include beat-shift fixed effects and motorist controls.
Figure B-5: Reduced form estimates by motorist income

Notes: Same as figure 10, 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 B-6: Robustness of IV estimates to sample selection

Notes: This figure reports IV estimates for the one-year change in collections balances when trimming the sample of officers with selected samples. First, a covariate index $\hat{Y}$ is constructed by regressing $Y$ on motorist demographics using only the sample of lenient officers. Then, I construct residuals $\tilde{Y}$ from a regression of $\hat{Y}$ on beat-shift fixed effects using all speeding stops. Finally, I average $\tilde{Y}$ across officers and rank officers based on these averages. I re-estimate the 2SLS regressions dropping officers in the top or bottom $p$ percent of the distribution of average $\tilde{Y}$. The estimate for $p = 0$ corresponds to that reported in table 3.
Table B-3: IV Results with alternative instruments

<table>
<thead>
<tr>
<th></th>
<th>Collections Balances</th>
<th>Revolving Balances</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td></td>
<td>τ = 1</td>
<td>τ = 3</td>
</tr>
<tr>
<td>Leave-out (Baseline)</td>
<td>0.062</td>
<td>0.244</td>
</tr>
<tr>
<td></td>
<td>(0.108)</td>
<td>(0.128)</td>
</tr>
<tr>
<td>Leave-out (Residualized)</td>
<td>0.07</td>
<td>0.253</td>
</tr>
<tr>
<td></td>
<td>(0.109)</td>
<td>(0.13)</td>
</tr>
<tr>
<td>Officer Effects</td>
<td>0.073</td>
<td>0.261</td>
</tr>
<tr>
<td></td>
<td>(0.144)</td>
<td>(0.169)</td>
</tr>
<tr>
<td>Officer Effects (Shrunken)</td>
<td>0.063</td>
<td>0.22</td>
</tr>
<tr>
<td></td>
<td>(0.133)</td>
<td>(0.157)</td>
</tr>
<tr>
<td>Binary</td>
<td>0.061</td>
<td>0.362</td>
</tr>
<tr>
<td></td>
<td>(0.167)</td>
<td>(0.198)</td>
</tr>
</tbody>
</table>

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 \((Y_\tau - Y_{\tau-1}) - (Y_{\tau-1} - Y_{\tau-4})\) 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 B-7: Reduced form estimates for motorists without past citations

Notes: Same as figure 8 using a subsample of the IV sample that requires only one citation per motorist (the first in-sample citation per motorist) and requires that each motorist has not received a citation in the previous year ($N = 272,866$).
Figure B-8: Racial differences using within-race instrument

(a) Without controls

(b) With controls

Notes: Same as figure 10 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 $\beta_{FS} = 125.11 \ (0.53)$. 
C  Data appendix

C-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 C-1 shows a sample UTC form and figure C-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 withheld (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 there are multiple disposition records, I use the first valid entry as the disposition verdict. Table C-1 below shows the distribution of dispositions in the event study sample.
Table C-1: Distribution of dispositions in event study sample

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

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 (recall that traffic school election requires a paid fine). Hence, based on the disposition information, a very conservative lower bound on the fraction of citations that are paid is 59 percent.

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.

Motivated by the background information provided by the Florida Clerks, in the analyses relying on the 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 leniency” 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.
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 C-4, estimated income is highly correlated with both zip code per-capita income and earnings in the employment database where reported.

Payroll records

Access to payroll records covering a subset of large employers was also provided by one of the three major credit bureaus. The provided data are quite thin and include the number of jobs and total earnings in a given a month. No information on occupation or location is present. In terms of coverage, employers represented in the employment records tend to be larger businesses.

C-1.1 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 C-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 C-3. Results from regressing a successful credit file match on available driver characteristics are shown in table C-2.

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 2,994,954 drivers. I also require that individuals have a nonmissing credit score and nonmissing estimated income as of that date, leaving 2,966,055 individuals, and focus on individuals aged 18–59 as of that date, leaving 2,631,641 individuals. Analysis samples are constructed from this group of individuals.

**Aggregation**

All variables are first computed using monthly data. I then aggregate the data to the person × quarter level for two reasons. First, aggregating reduces the (already minimal) prevalence of missing values. For example, an individual may have a nonmissing credit report in January 2010 but not February 2010 or March 2010. Quarterly aggregation uses the January credit report as the quarterly value. Second, the aggregation reduces the dimensions of the panel dataset to a more computationally manageable size. The event study regressions, which use a 2010-2015 panel of 525,646 individuals, cannot be estimated on monthly data using the computing tools available for analyzing the credit report data due to the dimensionality of the matrix that needs inverting. These regressions are computationally manageable when the data are collapsed to the person-quarter level.

When aggregating continuous variables (e.g., number of collections on file) to the person-quarter level, I take the average of the nonmissing values within the person-quarter. If the variable is still missing (less than 0.5 percent of the data in all cases), I impute zero. For binary variables (e.g., any new financial distress), I take the maximum of the nonmissing values and impute zero if all values are missing.
C-1.2 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.


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.

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

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

**Imputed borrowing limits**

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

I impute a limit of zero for credit scores below 350 because the probability of having any revolving credit is approximately zero below 350 and impute an upper limit of $80,000. Note that this upper limit only binds at credit scores above 838, which is outside the support of credit scores in the event study data. The solid line in figure C-5 illustrates the imputed borrowing limit. In the baseline cross-section, a regression of the true borrowing limit on the imputed borrowing limit, which can explain 16 percent of the variation in borrowing limits.
C-3 Computing

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

The eight-hour limit is also an issue for computing estimates using the Borusyak et al. (2022) method. I compute point estimates for their method manually following their two step imputation procedure, but existing packages to estimate standard errors (`didimputation` and `did2s`) cannot accommodate the size of the relevant panel. Standard errors could be bootstrapped, but a sufficiently large number of bootstrap iterations cannot be performed within the eight-hour time window. Hence, I do not report standard errors for estimates obtained via the Borusyak et al. (2022) approach.
Figure C-1: Florida Uniform Traffic Citation (UTC) Form

Source: https://www.flhsmv.gov/courts-enforcement/utc/forms-and-resources/.
Figure C-2: Example of completed UTC form

Figure C-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 indicate linear fits (coefficients reported in the figure legend).
### Table C-2: Credit file match rate by driver characteristics

<table>
<thead>
<tr>
<th></th>
<th>Any Match</th>
<th>Early Match</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
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<td>0.0432</td>
<td>0.0644</td>
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<td>-0.0690</td>
<td>-0.4701</td>
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<td>0.0281</td>
<td>0.0718</td>
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<td>Age 45-54</td>
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<td>0.1222</td>
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<td>-0.0327</td>
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<tr>
<td>Race = Hispanic</td>
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<td>-0.0657</td>
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<tr>
<td>Log Zip Income</td>
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<td>0.652</td>
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<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Time FE</td>
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<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>R2</td>
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<td>0.026</td>
<td>0.245</td>
<td>0.259</td>
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<td>8851688</td>
<td>8851688</td>
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</tr>
</tbody>
</table>

Notes: This table presents regressions estimated at the citation level. *Any Match* refers to whether the driver was matched to the credit file at any point. *Early Match* refers to whether the driver was matched and on the credit file as of January 2010. Ages 18-24 and white are the excluded age/race categories. County and time fixed effects are for the county and year × month of the traffic stop. Standard errors are clustered at the county level.
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 C-5: Credit scores and borrowing limits

Notes: This figure illustrates the cross-sectional relationship between credit scores and revolving credit limits at baseline using the subset of the initial sample with an open revolving account ($N = 1,623,184$). Local means correspond to each integer value of the credit score and the solid line illustrates fitted values used for imputation, described in the data appendix. Reported $R^2$ is from a regression of true borrowing limits on predicted borrowing limits using the fitted values. Regression coefficients are linear slopes over the denoted ranges.
Figure C-6: Default and credit scoring

Notes: This figure reports event study estimates for credit scores around the time of first default incident (new collection, new delinquency, or new derogatory flag) observed in the data, using only the final cohort of the event study sample \( (N = 22,006) \), for the full sample as well as broken down by baseline credit scores.
Figure C-7: Distribution of credit scores in event study sample

(a) Histogram

(b) Density by Driver Groups

Notes: 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 C-8: 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

Notes: 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 C-3: Summary Statistics at Baseline by Traffic Court Disposition

<table>
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<th>Definitely Paid</th>
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<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>All</td>
<td>-</td>
</tr>
<tr>
<td>V=4/C</td>
<td>-</td>
</tr>
<tr>
<td>V=4</td>
<td>-</td>
</tr>
<tr>
<td>V=C</td>
<td>-</td>
</tr>
<tr>
<td>V=3/A</td>
<td>-</td>
</tr>
<tr>
<td>V=1/M</td>
<td>-</td>
</tr>
</tbody>
</table>

**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.
### D Payroll records appendix

<table>
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<td>(2)</td>
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<tr>
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<tr>
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<td>(0.0031)</td>
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<td>0.0035</td>
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<tr>
<td></td>
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<td>(0.0009)</td>
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</table>

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

Notes: This figure plots the share of the event study sample employed in the payroll data over time, using only not-yet-treated observations to net out treatment effects. Blue circles report raw means for not-yet-treated observations and green squares report age-adjusted payroll employment rates obtained from a regression of a payroll employment indicator on age and time fixed effects, again using only not-yet-treated observations.
Table D-2: Comparison with ACS, 2010

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<tr>
<td>ACS: Reweighted</td>
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<td>37122</td>
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</table>

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 D-2: 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 D-3: Event study estimates, payroll data exits

(a) Credit Score

Counterfactual mean = 610.71

(b) Any New Default

Counterfactual mean = 0.27

(c) Collections Balances

Counterfactual mean = 1765

(d) Any Durable Loan

Counterfactual mean = 0.51

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 D-4: Collections-earnings elasticity

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

\[ b = -0.277 (0.119) \]