

Supplementary Material for “Fines and Financial Wellbeing”

E Additional data information

E-1 Data sources

Citations data

I obtained administrative records of the universe of traffic citations issued in the state of Florida over the period 2010-2015 through a FOIA (*sunshine law*) request. A copy of each traffic ticket issued in Florida is sent to the county clerk, who then forwards the information along to the Florida Clerks and Comptroller’s Office (FCC). The FCC maintains the state’s Uniform Traffic Citation (UTC) database, which preserves an electronic record of each ticket transcribed from the paper citation written by the ticketing officer. Figure E-1 shows a sample UTC form and figure E-2 provides an example of a completed form.

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

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

Dispositions data

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

1 = *guilty*; 2 = *not guilty*; 3 = *dismissed*; 4 = *paid fine or civil penalty*; 6 = *estreated or forfeited bond*; 7 = *adjudication 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. See appendix C for an expanded discussion of the disposition verdicts.

Sanctions Information

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

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

Credit bureau Data

Access to monthly credit report data from January 2010 through December 2017 for cited drivers was granted by one of the three major credit bureaus through a data sharing agreement. The credit bureau data represent an aggregated snapshot of an individual's credit report taken on the final Tuesday of each month. The data include information reported by financial institutions, such as credit accounts and account balances, information reported by collections agencies, information culled from public records, and information computed directly by the credit bureau such as credit scores. The data also include an estimated income measure based on a proprietary model which predicts an individual's income, rounded to the nearest thousand, using information on the credit file. As shown in figure [E-4](#), estimated income is highly correlated with both zip code per-capita income and earnings in the payroll employment database where reported.

Payroll records

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

E-2 Matching and accessing credit bureau data

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

Two pieces of information are useful for interpreting the match rate. First, the data are transcribed from paper citations (e.g., figure E-2) and therefore contain transcription errors. Second, according to Brevoort et al. (2015), about eleven percent of adults, and as many as thirty percent in lower-income areas, have no credit record. Consistent with this finding, I find a strong relationship between neighborhood (zip code) income and the credit file match rate, as shown in figure E-3. Results from regressing a successful credit file match on available driver characteristics are shown in table E-1.

After matching the data, the credit bureau removed the citations data of all personally identifiable information such as driver names, addresses, birth dates, driver license numbers, and exact citation dates. They replaced DL numbers with a scrambled individual identifier (allowing me to track individuals who receive multiple citations) and the exact traffic stop date with the year and month. I was then allowed access, through a secure server hosted by the credit bureau, to the anonymized citations data and monthly credit reports, each with a scrambled individual identifier for linking across the two datasets.

Initial Sample

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

Aggregation

All variables are first computed using monthly data. I then aggregate the data to the person \times quarter level for two reasons. First, aggregating reduces the (already minimal) prevalence of missing values. For example, an individual may have a non-missing credit report in January 2010 but not February 2010 or March 2010. Quarterly aggregation uses the January credit report as the quarterly value. Second, the aggregation reduces the dimensions of the panel dataset to a more computationally manageable size.

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

E-3 Variable definitions

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

2. *Collections Balance.* Total collection amount (unpaid) for 3rd party collections (i.e. collections not being handled by original creditor) on file. Includes both public record and account level 3rd party collections information.
3. *Delinquencies.* Number of accounts on file with 90 days past due as the worst ever payment status.
4. *Derogatories.* Number of accounts on file with any of the following ever: repossession, charge off, foreclosure, bankruptcy, internal collection (collection being handled by original creditor and not a third party), defaulted student loan.
5. *New Collection.* I construct this variable by computing a first difference in the number of collections and defining an indicator for whether the first difference is greater than zero.
6. *New Delinquency.* An indicator for whether the pre-existing variable “Number of open accounts with current rate of 90 to 180 or more days past due (but not major derogatory) and *reported within one month*” is greater than zero.
7. *New Derogatory.* I construct this variable using the same method as collections from the stock derogatories measure.
8. *Any New Default Flag.* Equal to one if new collection, new delinquency, or new derogatory equals one. Zero otherwise.
9. *Any Revolving Account.* Equal to one if “number open revolving accounts on file” is greater than zero. Zero otherwise.
10. *Revolving Balances.* Sum of balances for all open revolving accounts on file with update within the last 3 months.
11. *Revolving Limits.* Total credit limit/high credit open revolving accounts with update within 3 months

All raw variables in the credit bureau database are pre-topcoded. Account-level counts, such as the number of delinquencies, are topcoded at 92. Balances are topcoded at \$9,999,992, which I typically further topcode at the 95th percentile.

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

E-4 Imputed variables

Baseline estimated income

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

In figure E-4, I plot the relationship between these income measures for the subset of individuals with observed payroll earnings at some point during the first year of the data. Here, zip code income is measured at each individual’s first traffic stop and both payroll 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 E-4, I construct my primary measure of baseline income using a weighted average of zip code income and credit bureau estimated income at baseline, with the weights taken from the regression of payroll earnings on zip code income and estimated income, again using only observations with observed payroll earnings. Hence, a literal interpretation of baseline predicted income is predicted payroll earnings based on zip code of residence and the credit bureau income model.

I estimate this regression only using baseline data and use this predicted income 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

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 and report estimates for this imputed limit in appendix F. As shown in panel (a) of figure A-13, the relationship is highly nonlinear in the raw data. I construct predicted borrowing limits by combining separate quartic polynomials estimated over the ranges 350-450, 450-775, and 775-850, imposing that the piece-wise function is continuous and weakly increasing over the range 350-850.

I impute a limit of zero for credit scores below 350 because the probability of having any revolving credit is approximately zero below 350 and impute an upper limit of \$80,000. Note that this upper limit only binds at credit scores above 838, which is outside the support of credit scores in the event study data. The solid line in panel (a) of figure A-13 illustrates the imputed borrowing limit. In the baseline cross-section, a regression of the true borrowing limit on the imputed borrowing limit, which can explain 16 percent of the variation in borrowing limits.

E-5 Computing

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

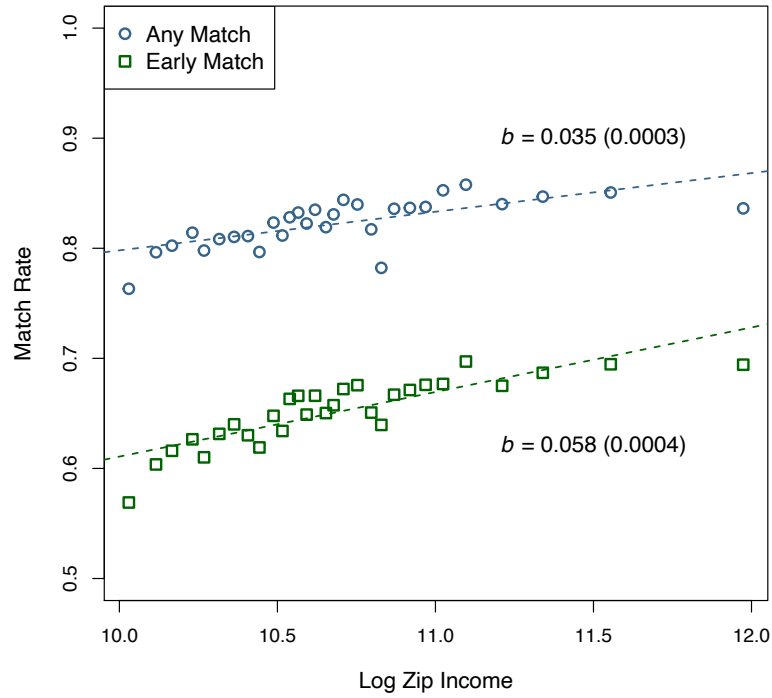
The eight-hour limit is also an issue for computing estimates using the [Borusyak et al. \(2024\)](#) method. I compute point estimates for their method manually following their two step imputation procedure, but existing packages to estimate standard errors (`didimputation` and `did2s`) cannot accommodate the size of the relevant panel. Standard errors could be bootstrapped, but a sufficiently large number of bootstrap iterations cannot be performed within the eight-hour time window. Hence, I do not report standard errors for estimates obtained via the [Borusyak et al. \(2024\)](#) approach.

[illegible]

[illegible]

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



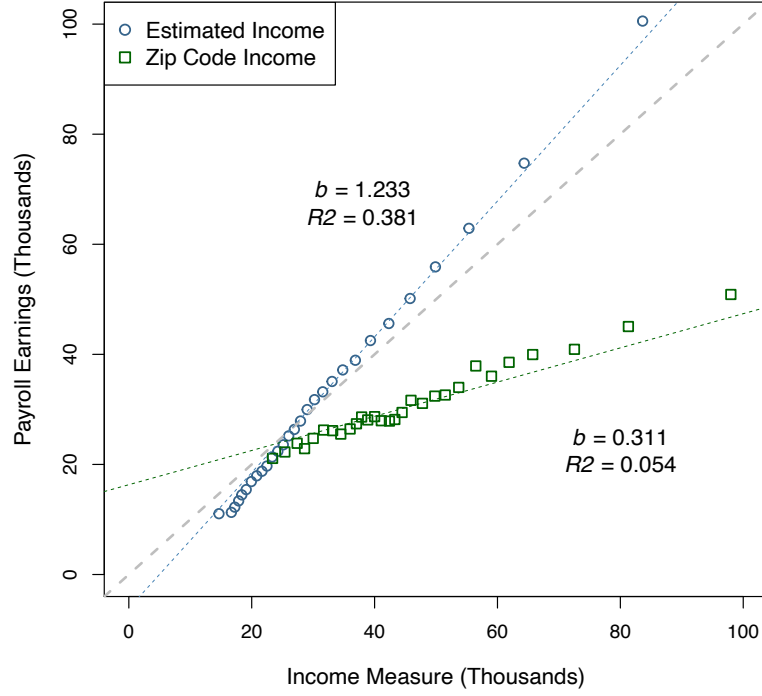
Notes: This figure plots the share of citations successfully matched to the credit file in each quantile bin of log zip code income. Blue circles (*any*) indicate whether the citation was matched at all. Green squares (*early*) indicate whether the citation was matched to a driver present on the credit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

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

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

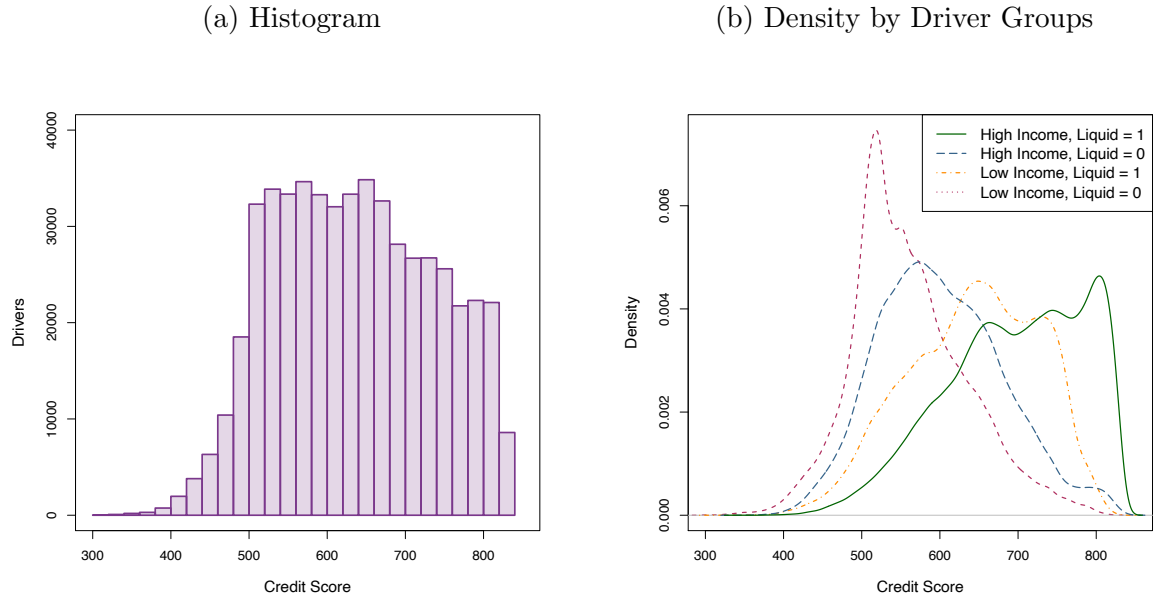
Notes: This table presents regressions estimated at the citation level. *Any Match* refers to whether the driver was matched to the credit file at any point. *Early Match* refers to whether the driver was matched and on the credit file as of January 2010. Ages 18-24 and white are the excluded age/race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

Figure E-4: Income measures



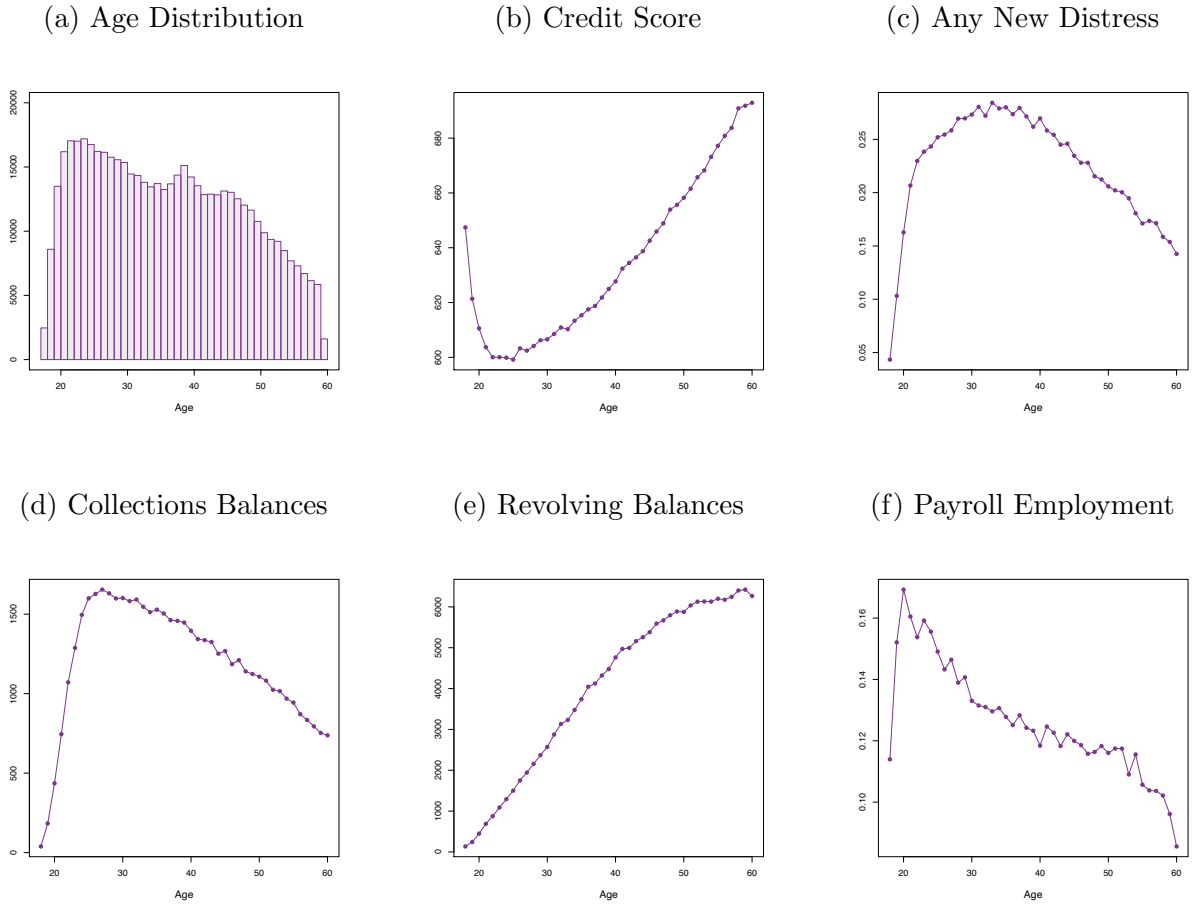
Notes: This figure illustrates the relationship between income measures using the subsample with positive payroll earnings at some point in 2010 ($N = 390,688$). The regression of payroll earnings on both income measures gives $R^2 = 0.388$ with coefficients on credit bureau estimated income and zip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

Figure E-5: Distribution of credit scores in event study sample



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 E-6: Age profiles in outcomes of interest



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

		Definitely Paid				
	(1)	(2)	(3)	(4)	(5)	(6)
	All	V=4/C	V=4	V=C	V=3/A	V=1/M
<i>Panel A: Demographics</i>						
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
<i>Panel B: Financial Distress</i>						
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
<i>Panel C: Credit Usage</i>						
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
<i>Panel D: Payroll Records</i>						
Any Payroll Earnings	0.13	0.13	0.13	0.14	0.12	0.13
Monthly Earnings	3319	3276	3073	3513	3491	2958
<i>Panel D: Citation Information</i>						
Fine Amount	195.53	184.55	183.49	185.92	220.45	172.07
DL Points	3.39	3.33	3.32	3.34	3.52	3.26
Individuals	525646	309598	174766	134832	175051	40997

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

F Additional results: event studies

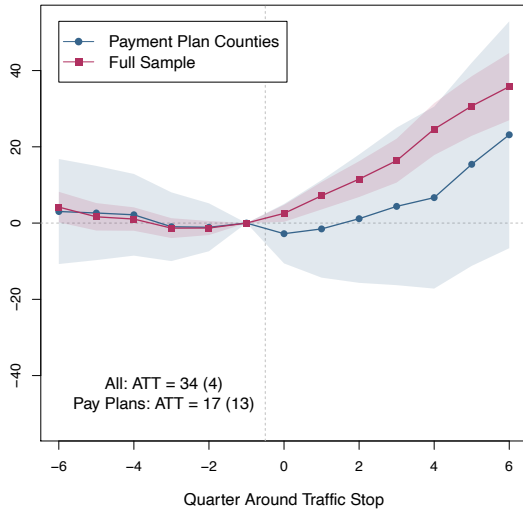
Figure F-1: Total costs and event study estimates by citation location



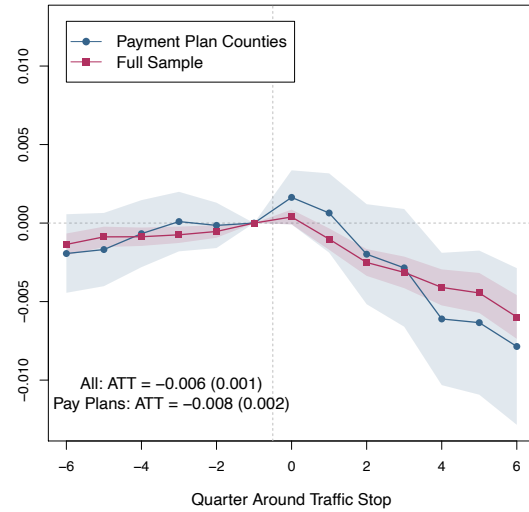
Notes: Panel (a) illustrates the distribution of traffic court disposition verdicts for citations issued to motorists who live in the county of their citation (solid blue bars; $N = 310,317$) and for motorists who live at least 150 miles away from the county of their citation (striped red bars; $N =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

Figure F-2: Event study estimates for counties with available payment plans

(a) Collections Balances



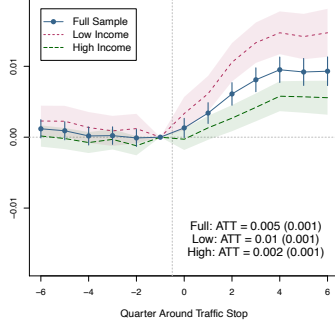
(b) Payroll Employment



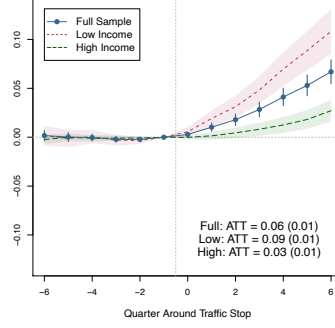
Notes: This figure reports event study estimates for the full sample and for the subset of motorists cited in Pinellas and Hillsborough counties ($N = 43,729$), which offered three month payment plans on traffic fines during the sample period.)

Figure F-3: Event study estimates for distress outcomes by baseline income

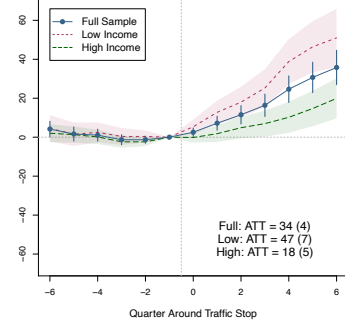
(a) Any New Distress



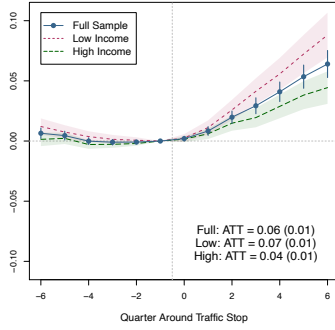
(b) Collections



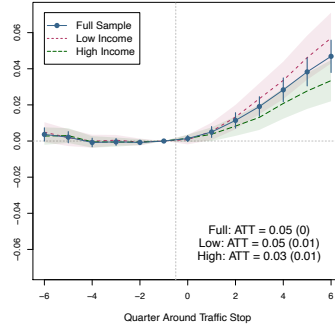
(c) Collections Balances



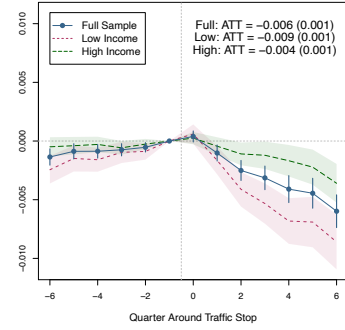
(d) Delinquencies



(e) Derogatories



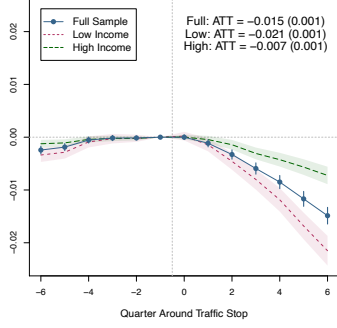
(f) Payroll Employment



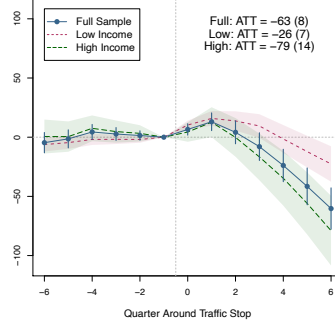
Notes: Each panel reports event study estimates for the full sample as well as estimates from separate event studies for motorists with above ($N = 288,276$) and below ($N = 237,730$) median estimated income at baseline.

Figure F-4: Event study estimates for credit card outcomes by baseline income

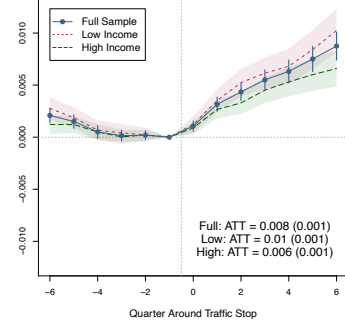
(a) Any Revolving Account



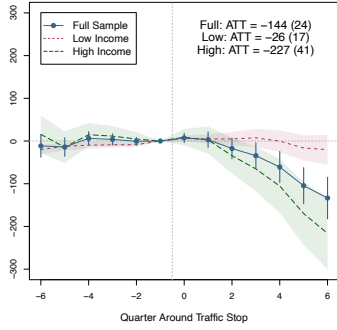
(b) Revolving Balances



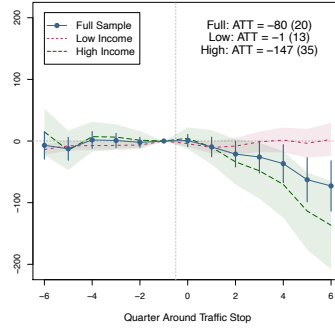
(c) Revolving Utilization



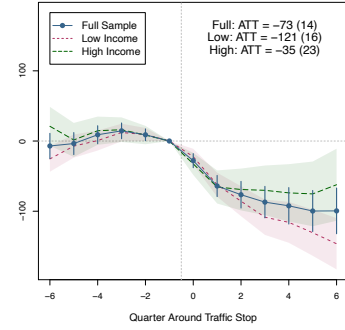
(d) Revolving Limits



(e) Available Balances

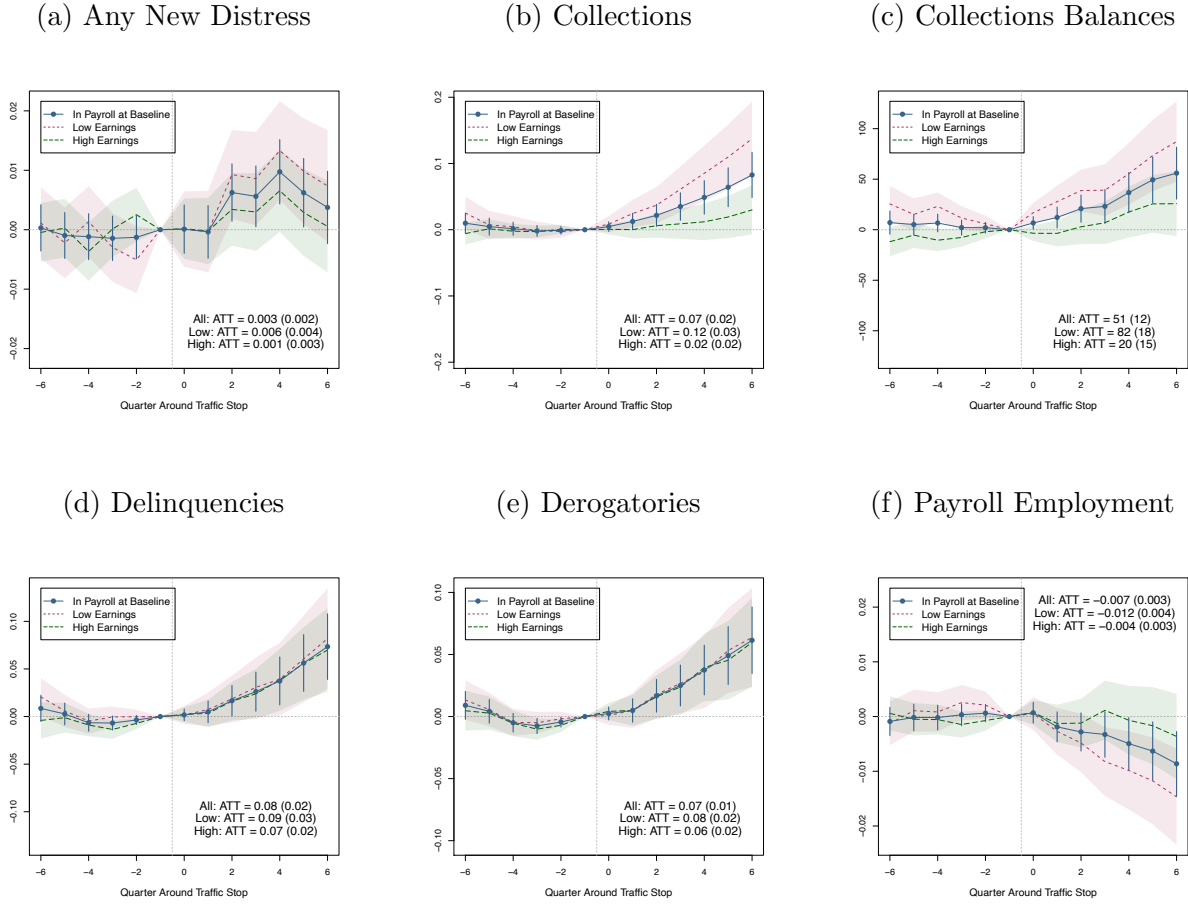


(f) Imputed Limits



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

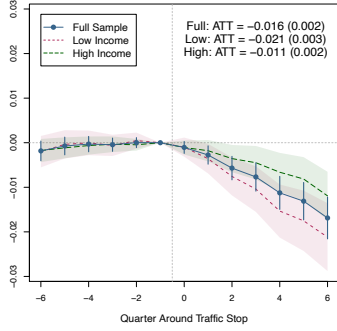
Figure F-5: Event study estimates for distress outcomes for subset in payroll records at baseline



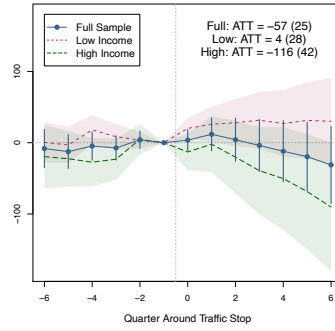
Notes: Each panel reports event study estimates for the full sample of motorists who are in the payroll records at baseline ($N = 55,140$) as well as estimates from separate event studies for motorists who are in the payroll records at baseline and have above ($N = 27,570$) and below ($N = 27,570$) median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

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

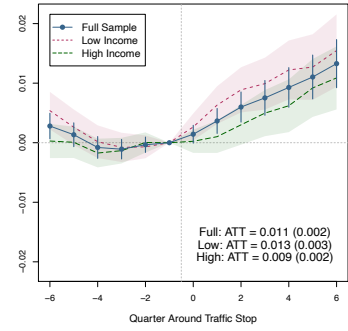
(a) Any Revolving Account



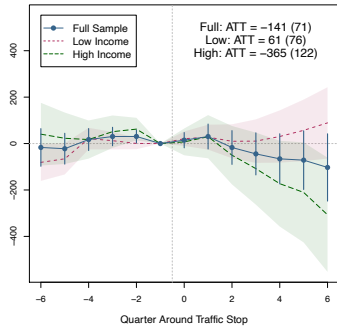
(b) Revolving Balances



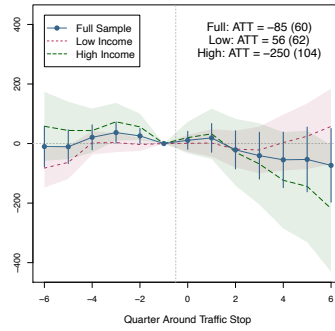
(c) Revolving Utilization



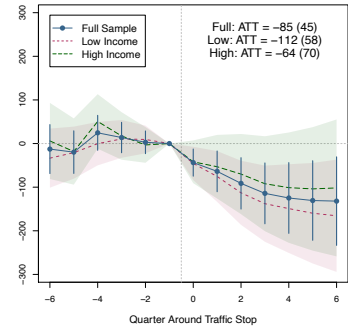
(d) Revolving Limits



(e) Available Balances



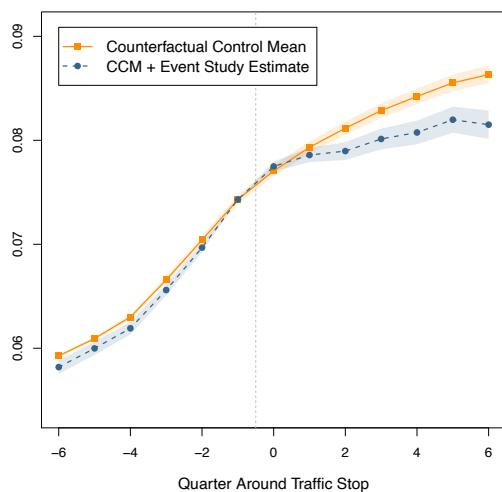
(f) Imputed Limits



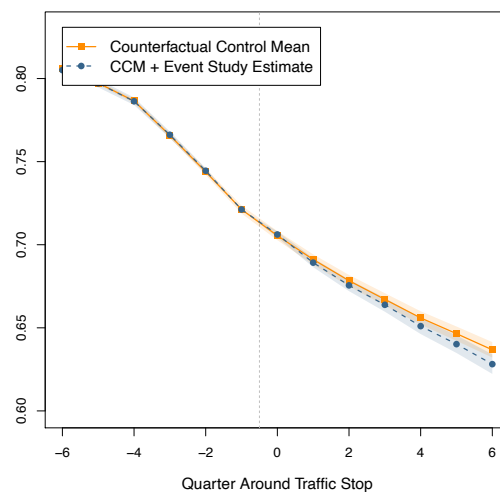
Notes: Each panel reports event study estimates for the full sample of motorists who are in the payroll records at baseline ($N = 55,140$) as well as estimates from separate event studies for motorists who are in the payroll records at baseline and have above ($N = 27,570$) and below ($N = 27,570$) median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

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

(a) Not in payroll records at baseline



(b) In payroll records at baseline



Notes: This figure reports the time path of estimated counterfactual means (orange squares) and the estimated counterfactual means plus the event study estimates (blue circles) where the outcome is employment in the payroll records and sample is split by whether the individual is employed in the payroll records at baseline (same sample split as panels (a) and (b) of figure 6).

Figure F-8: Event study estimates for distress outcomes by baseline credit card liquidity

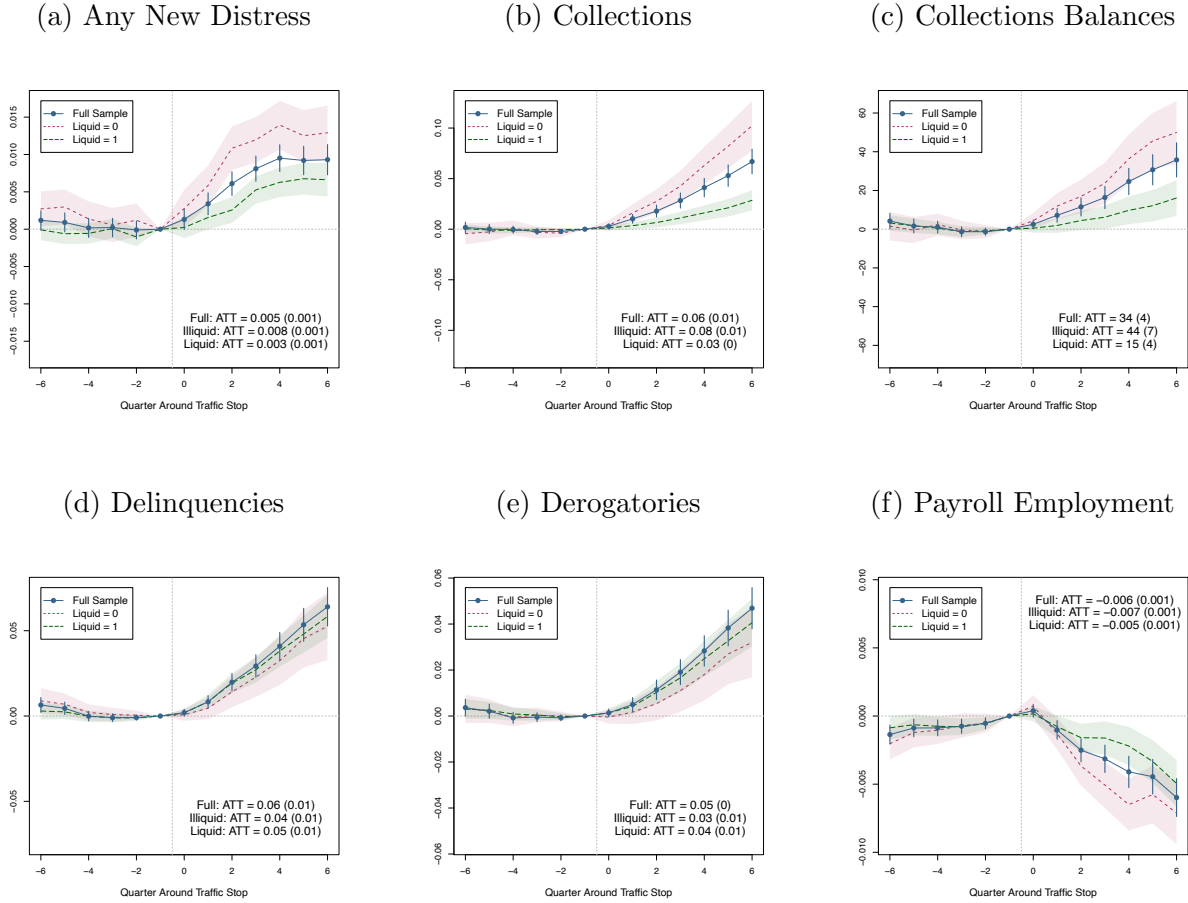
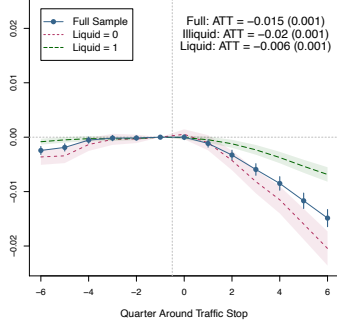
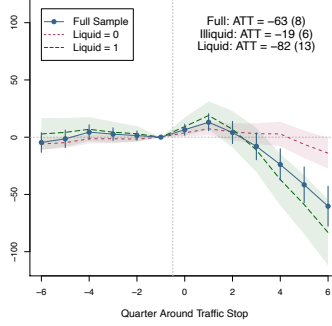


Figure F-9: Event study estimates for credit card outcomes by baseline credit card liquidity

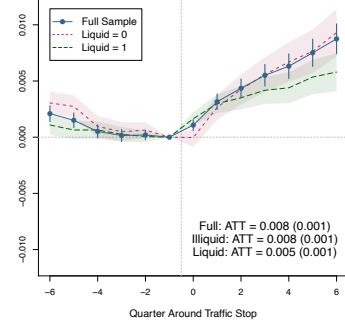
(a) Any Revolving Account



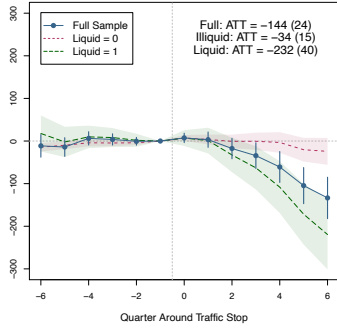
(b) Revolving Balances



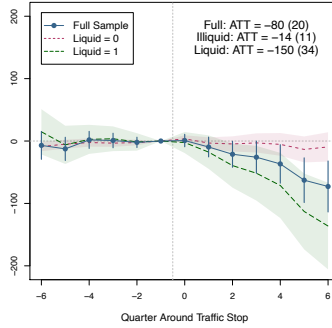
(c) Revolving Utilization



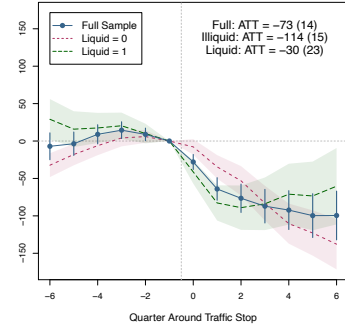
(d) Revolving Limits



(e) Available Balances

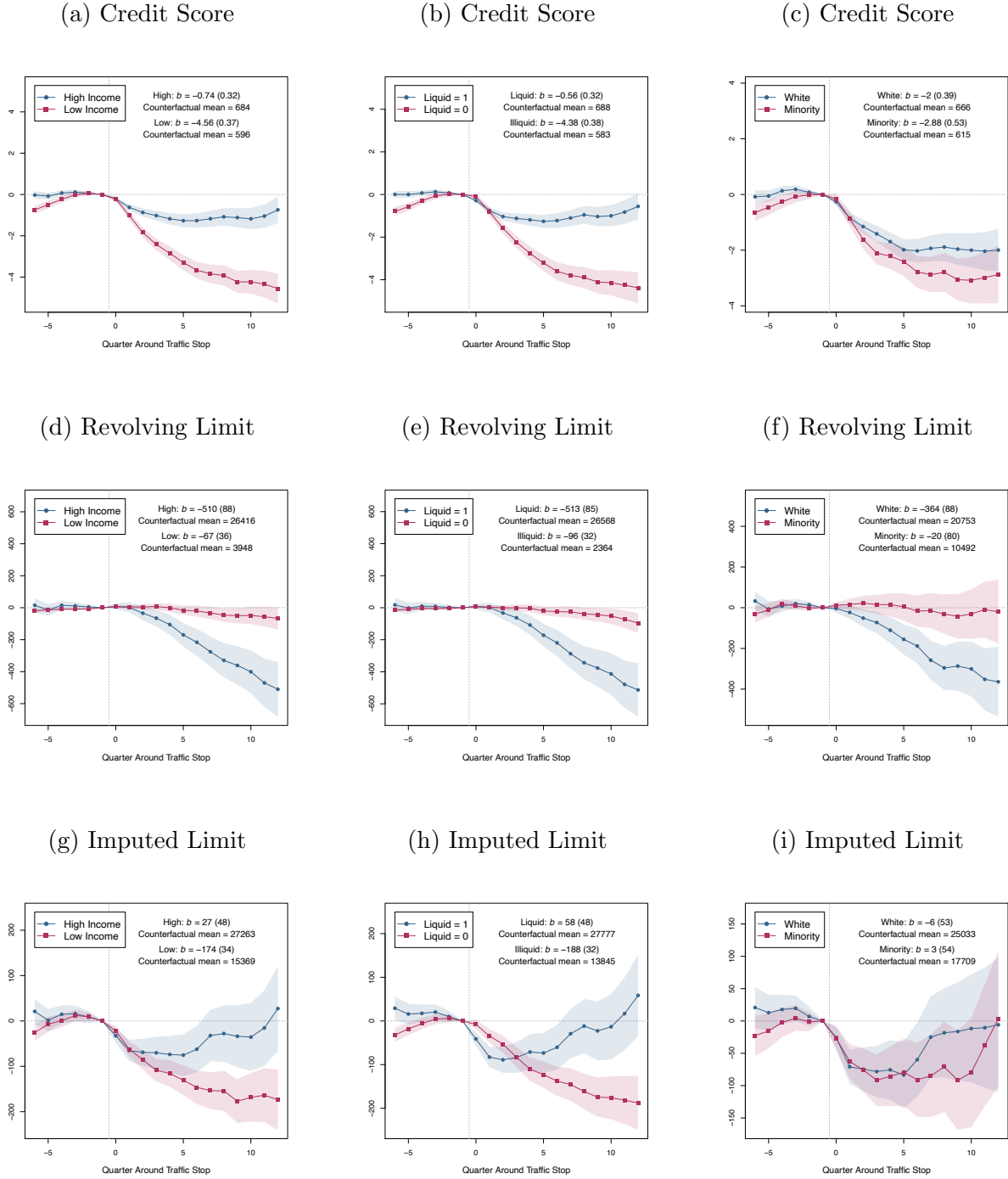


(f) Imputed Limits



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

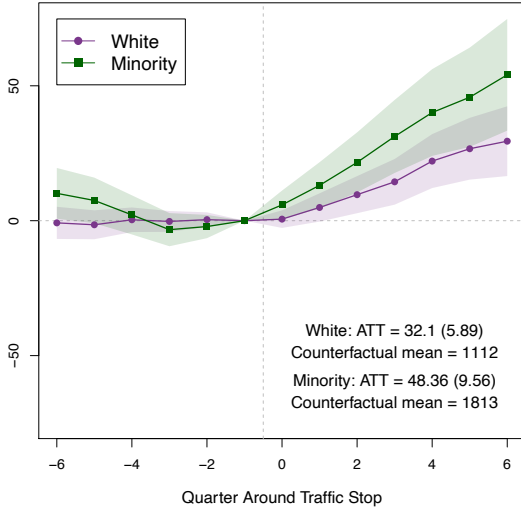
Figure F-10: Heterogeneity for long-run outcomes



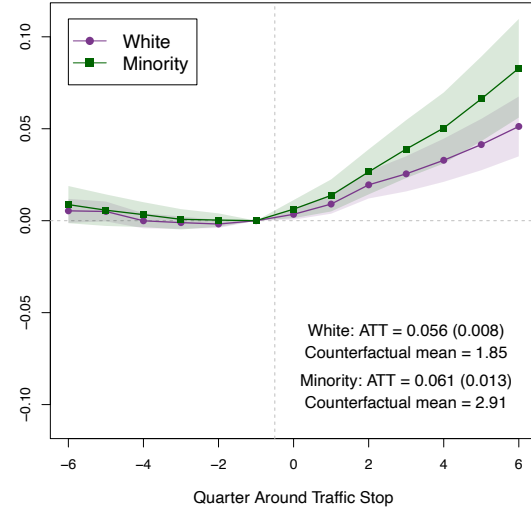
Notes: This figure reports heterogeneity in the estimates for longer-run outcomes by baseline estimated income, baseline credit card situation, and by motorist race.

Figure F-11: Event study estimates by motorist race

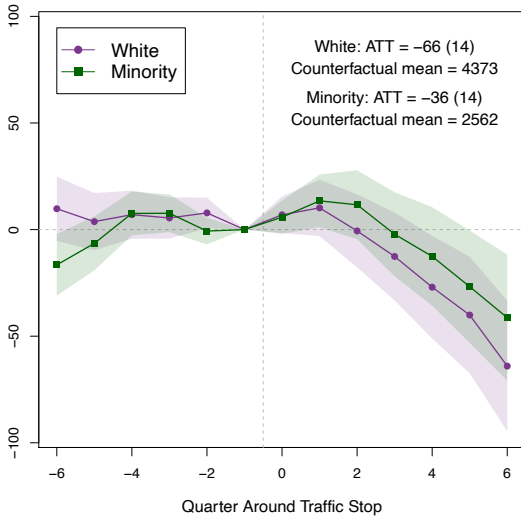
(a) Collections Balances



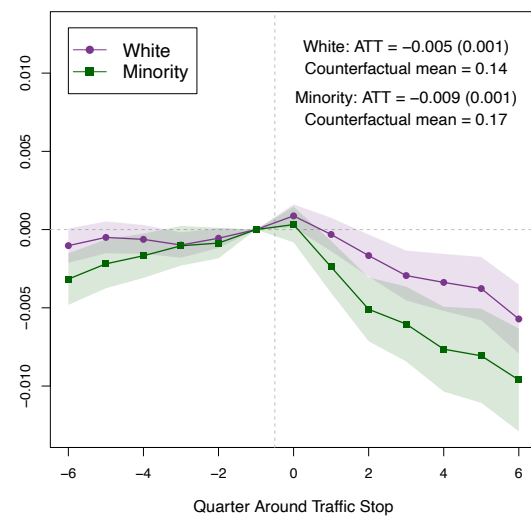
(b) Credit Line Delinquencies



(c) Revolving Balances

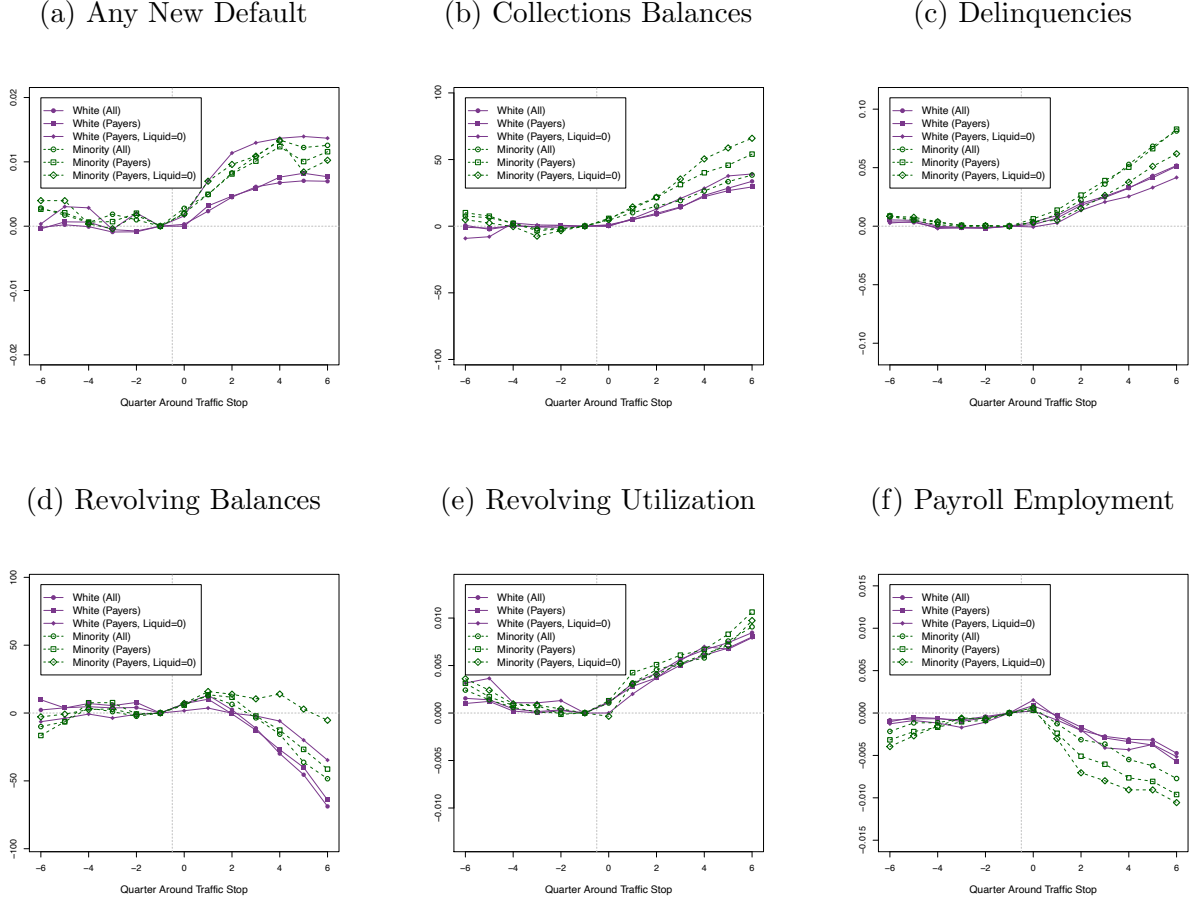


(d) Payroll Employment



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

Figure F-12: Event study estimates by race for subgroups



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

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

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

Notes: This table reports event study estimates for one, four, and six quarters post traffic stop, as well as the static ATT estimate, all obtained via the [Callaway & Sant’Anna \(2021\)](#) approach. Design-based standard errors from [Roth & Sant’Anna \(2022\)](#) in parentheses. The lower panels report estimated counterfactual means for $\tau = 1$ and $\tau = 6$, estimated using the method described in the text, and results of the pretrends test from [Borusyak et al. \(2024\)](#). The sample is the full event study sample ($N = 525,646$) and the average fine is \$195.53.

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

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

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

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

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

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

G Instrumental variables approach

G-1 Empirical strategy

I supplement the event study approach with a secondary identification strategy that leverages quasi-random variation in fine amounts generated by differences across officers in ticket-writing practices. In Florida, statutory fines for speeding violations depend only on an offender’s speed relative to the limit and increase discretely at various speed thresholds. As shown in panel (a) of figure G-1, over one third of all citations are issued for exactly nine MPH over the limit, just below a \$75 increase in fine. This bunching suggests the systematic manipulation of speeds by officers as a form of lenience (Goncalves & Mello, 2023).¹⁸

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

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

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

$$\Delta Y_{ijs\tau} = \theta \text{Fine}_{ij} + \gamma X_i + \psi_s + u_{ijs} \quad (\text{G-2})$$

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

Validity of this IV approach requires the usual LATE assumptions (e.g., Imbens & Angrist 1994). Papers using comparable examiner designs for identification (see Chyn et al. 2022 for a summary) typically appeal to institutional features, such as the randomized rotation of criminal case assignments across courtrooms, as evidence for instrument exogeneity. In the traffic citation setting, there is no institutional randomization of patrol officers to motorists, highlighting the important concern that officers with different bunching propensities may have differently selected samples. Figure G-3, however, shows that, after conditioning on beat-shifts, stringency is uncorrelated with an officer’s citation frequency and uncorrelated with a motorist’s financial situation, as summarized by their credit score.¹⁹ Moreover, equation G-2

¹⁸Figure G-2 illustrates significant dispersion across officers in the propensity to bunch drivers, net of beat-shift effects, motorist characteristics, and estimation error, as well as the correlation in estimated officer bunching propensity in two random partitions of the data. See Goncalves & Mello (2023) for further discussion.

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

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

This IV approach also requires exclusion and monotonicity assumptions. Exclusion requires that stringency only influences changes in outcomes through fine amounts. As shown in Frandsen et al. (2019), 2SLS estimates in examiner designs recover the desired LATE under an average monotonicity assumption which states that counterfactual reassignment to a more stringent officer increases fine amounts in expectation. Table G-4 illustrates that the first stage estimates are comparable across subgroups of motorists.

Relative to the staggered timing design, the main advantage of the instrumental variables approach is the ability to compare two drivers stopped at the same time, alleviating the core identification concerns associated with the event study. On the other hand, there are several complications associated with the IV approach. As shown in Goncalves & Mello (2023), officer stringency generates variation in both traffic court behavior and future traffic offending. The fact that stringency increases the likelihood that a motorist contests a citation in court (see figure G-4) precludes the IV approach from separating the effects of fine payment and other potential mechanisms. Specific deterrence effects associated with stringency should bias the instrumental variables estimates towards zero, as motorists facing lower fines are more likely to accrue additional fines in the future, a feature that makes the stringency approach especially poorly suited to the estimation of longer run effects. And finally, as one might expect, the instrumental variables estimates are substantially less precise.

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

G-2 Sample construction

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

The IV sample is then the intersection of this set of speeding citations for which the instrument can be computed and the set of speeding citations attributable to the initial sample of matched individuals, again restricting to white, Black, or Hispanic motorists (N citations = 362,854, N individuals = 332,933). To maximize the IV sample size, I do not impose the clean year restriction and allow motorists to appear multiple times. Figure G-8 shows that results are similar when additionally imposing these restrictions. Table G-1

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

G-3 Results

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

Panel (c) illustrates the reduced form relationship between officer stringency and changes over time in collections balances, both residualized of beat-shift effects. While officer stringency has no ability to predict changes between four quarters and one quarter prior to the traffic stop (red squares; $\beta = -0.41$; $se = 9.9$), a relationship between stringency and the change between one quarter prior and three quarters after is apparent (blue circles; $\beta = 29.83$). Although the standard error is large (11.7), the estimate is statistically significant at conventional levels.²⁰

Figure G-5 in the appendix plots the corresponding estimates over all (feasible) time horizons for the full set of outcomes. As in the event study analysis, slight increases in credit card balances and declines in the likelihood of holding a payroll-covered job are suggested but imprecisely estimated. Estimates for the remainder of financial distress outcomes are both very small in magnitude and too imprecise to draw firm conclusions; hence, I focus primarily on collections balances when presenting IV estimates but also show results for credit card balances in table G-2, which reports IV estimates in different specifications.

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

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

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

remarkably consistent with the corresponding event study estimates.

Also consistent with the event study estimates, the stringency instrument predicts meaningful increases in collections balances over longer time horizons. Corresponding to figure G-1, panel D of table G-2 implies that 24 percent of the marginal fine increases generated by the stringency instrument have appeared as collections balances on a motorist's credit report as of three quarters after the traffic stop. Six quarters out, the corresponding estimate grows to about 34 percent. Thus, the IV estimates support the basic conclusion of the event study analysis that fines induce default on other financial obligations, or in other words, that individuals borrow from other financial obligations in order to cover the fine.

G-3.1 Comparison with event study results

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

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

Alternatively, the effects on default in the two empirical designs may be more similar than is suggested by comparing the collections balances estimates. While event study estimates suggest increases in other measures of default, there is no evidence of impacts on the number of delinquent or derogatory accounts using the IV approach (see figure G-5). Hence, the estimates in both approaches may suggest similar overall impacts on financial distress. Unfortunately, this hypothesis is not directly testable without a dollar metric for delinquency, which the data do not include. Differences between IV and event study estimates could alternatively be due to a correlation between LATE weights and treatment effects.

G-3.2 Robustness

In supplementary appendix G-4, I show that IV estimates are qualitatively and quantitatively similar when using alternative definitions of the stringency instrument and imposing alternate sample restrictions. I also show that results are not sensitive to trimming officers from the sample based on their estimated degree of sample selection.

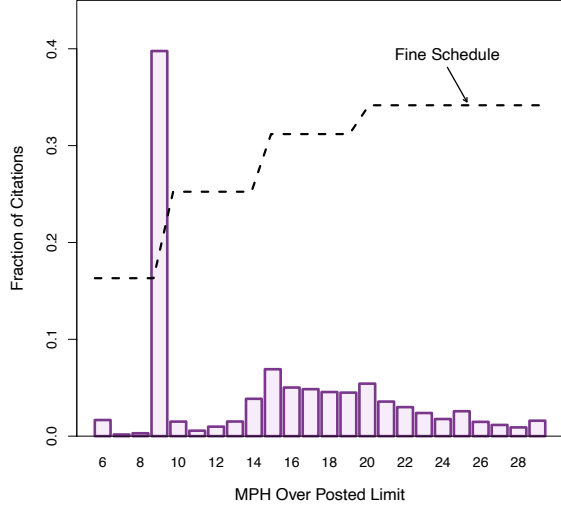
Table G-1: Summary statistics at baseline

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

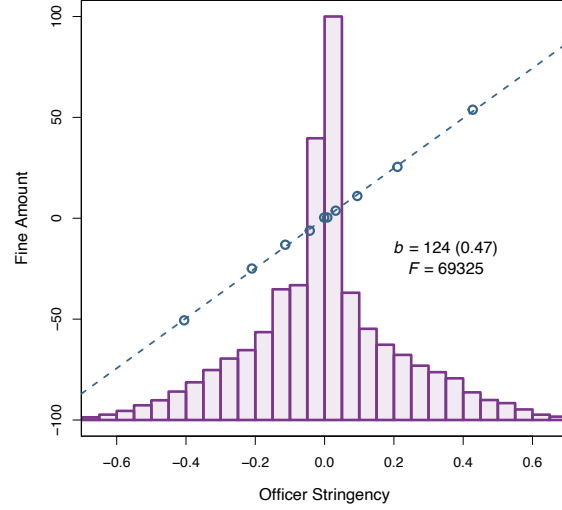
Notes: This table reports summary statistics as of 2010Q1 across samples. Column 1 reports statewide means computed from the ACS or provided by the credit bureau. Column 2 reports means for the “initial sample” of drivers who are (i) matched to the credit file, (ii) present on the credit file as of 2010Q1,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

Figure G-1: Instrumental variables approach

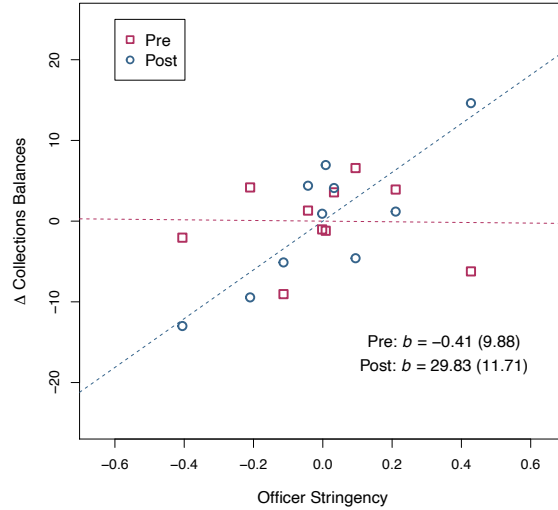
(a) Histogram of Charged Speeds



(b) First Stage



(c) Reduced Form



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

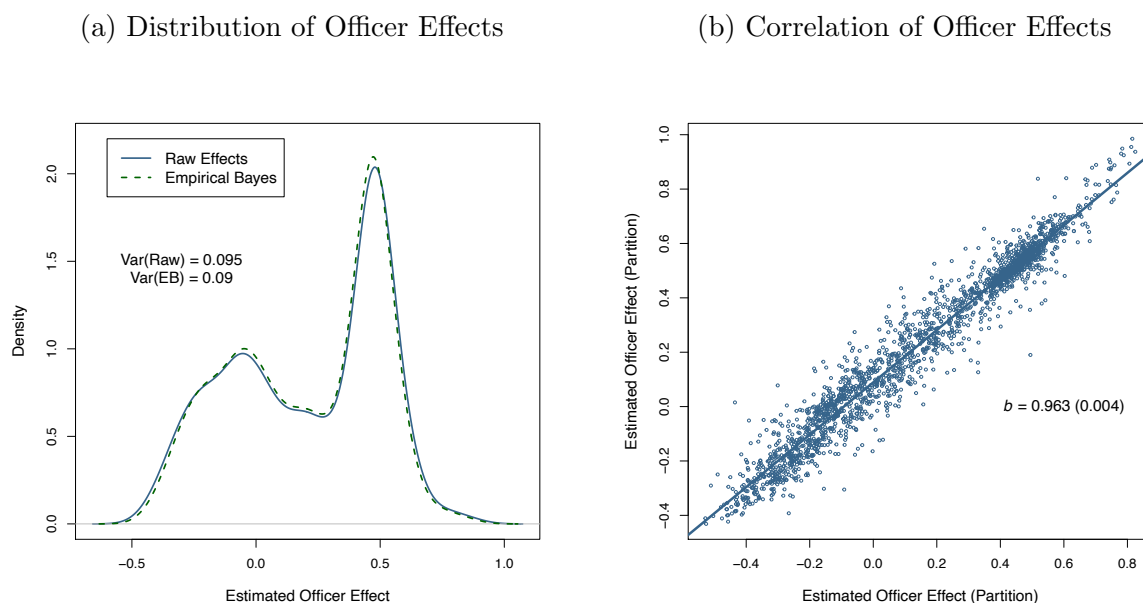
Table G-2: Officer IV Results

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

Notes: This table reports estimates from the officer IV design for collections and revolving balances, with and without motorist controls. All regressions include beat-shift fixed effects and standard errors are clustered at the beat-shift level. Panel (a) reports the first-stage relationship between the officer instrument and the fine amount. Panel (b) reports reduced form and 2SLS estimates where the outcome is the *pre-stop* change between $\tau = -4$ and $\tau = -1$. Panels (c)-(e) report estimates for the *post-stop* change over different time horizons ($\tau = 1$, $\tau = 3$ and $\tau = 6$), relative to $\tau = -1$. 2SLS DiD estimates replace the change $Y_\tau - Y_{-1}$ with the pre-period adjusted change, $(Y_\tau - Y_{-1}) - (Y_{-1} - Y_{-4})$ as the outcome. Estimates for additional time horizons and additional outcomes are presented in figure G-5. Sample is the IV sample, $N = 362,854$.

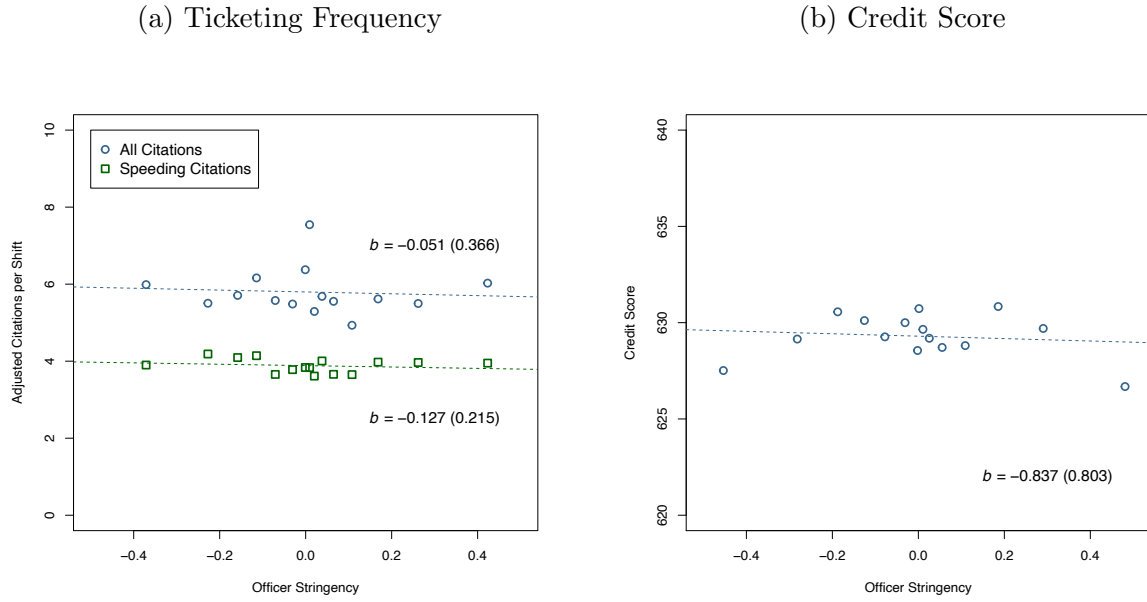
G-4 Instrumental variables: additional results

Figure G-2: Evidence of officer behavior



Notes: Panel (a) plots the distribution of estimated officer fixed effects from a regression of $\mathbf{1}[\text{harsh fine}]$, 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 G-3: Instrument validity



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

Table G-3: Randomization test

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

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

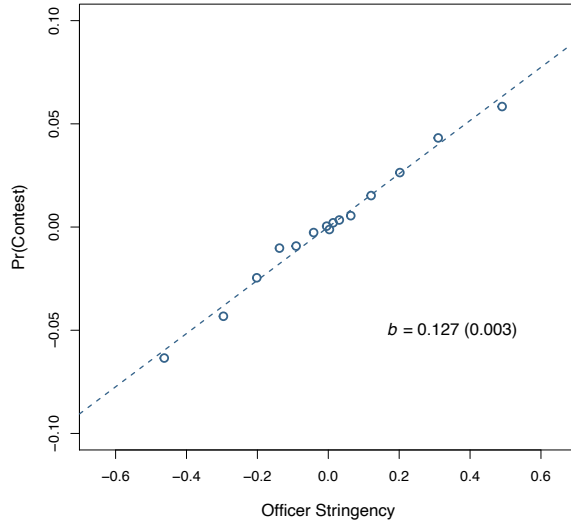
Table G-4: First stage estimates across subsamples

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

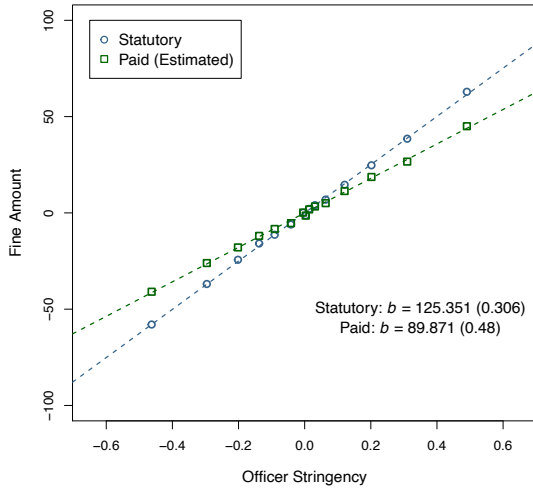
Notes: This table reports first stage estimates across subsamples. Each coefficient is from a separate regression of the fine amount on the stringency instrument and beat-shift effects using only the denoted subgroup of drivers, where the subgroups are the groups for which the denoted indicator variable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

Figure G-4: Officer stringency and citation outcomes

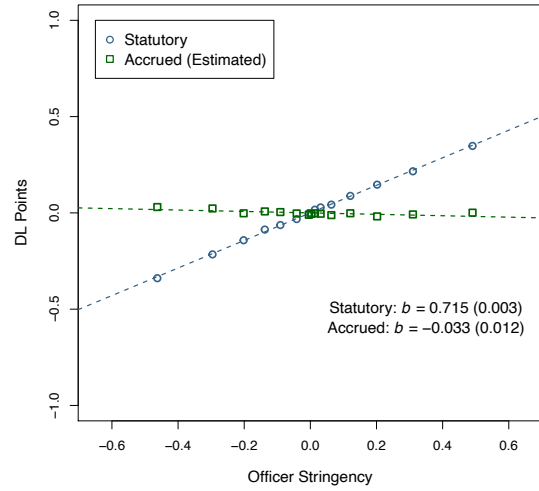
(a) Contested in Traffic Court



(b) Fine Amount

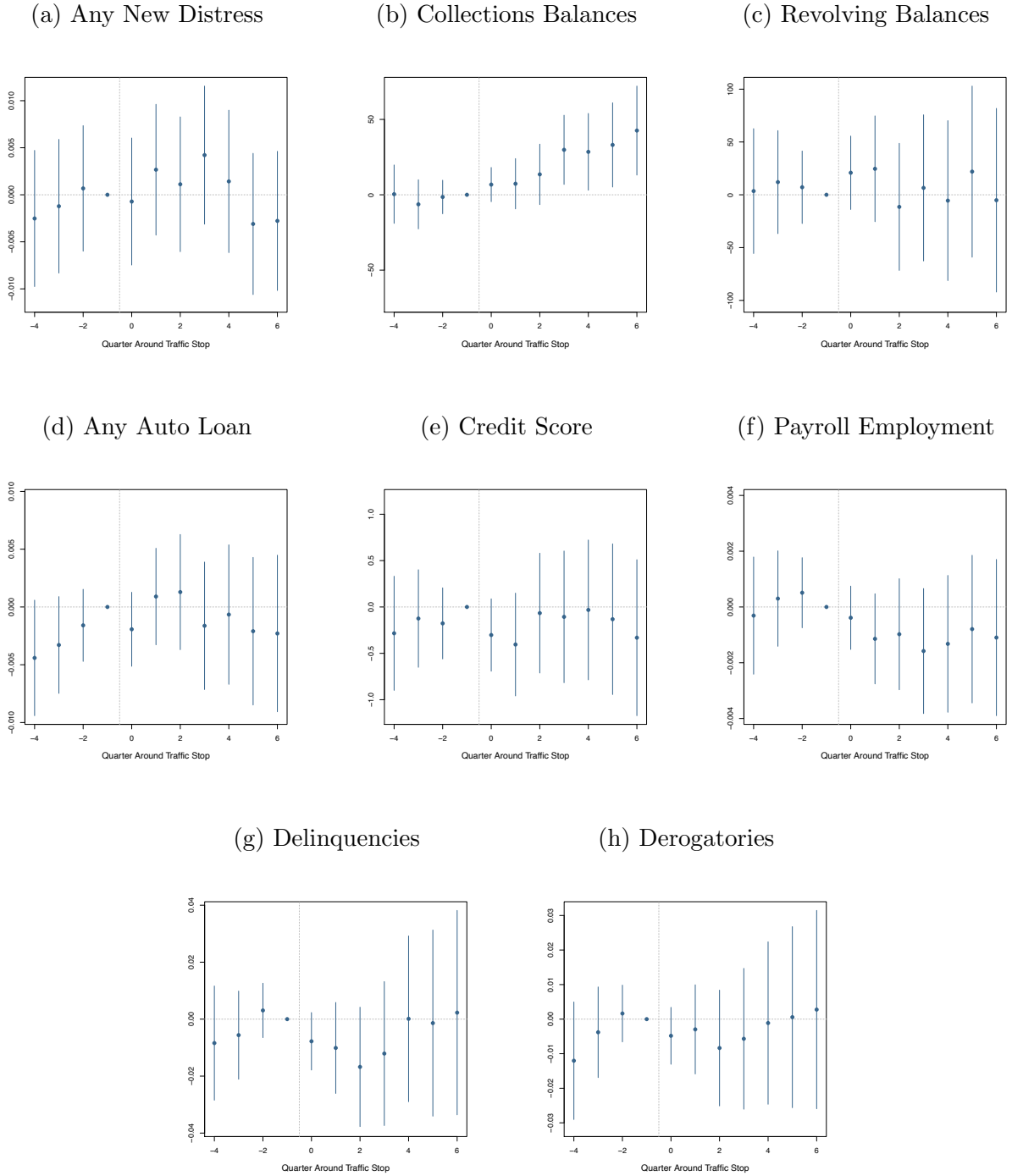


(c) DL Points



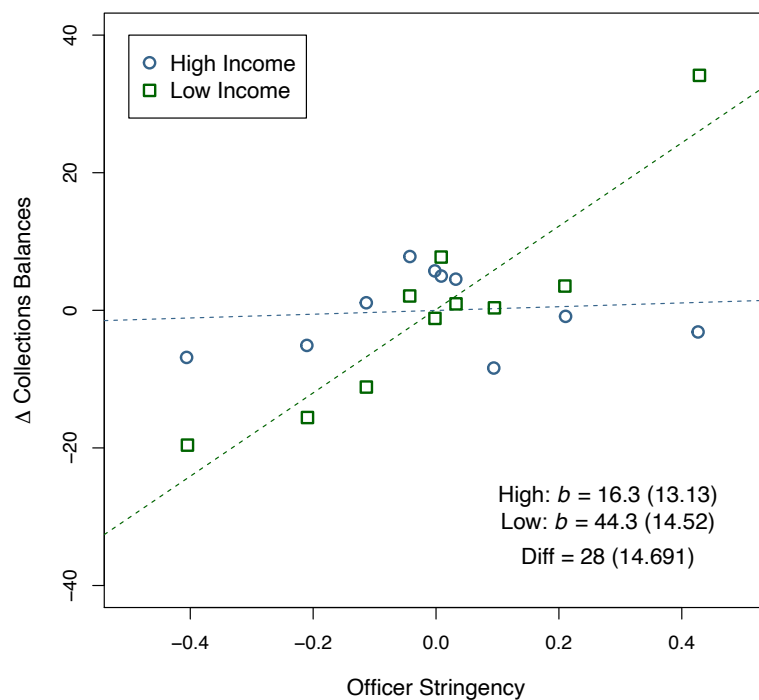
Notes: Each figure reports the relationship between citation outcome and the officer stringency instrument, both residualized of beat-shift fixed effects. Whether a citation is contested in court, as well as the paid fines and accrued points (as opposed to statutory) measures, are approximated based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

Figure G-5: Dynamic reduced form estimates



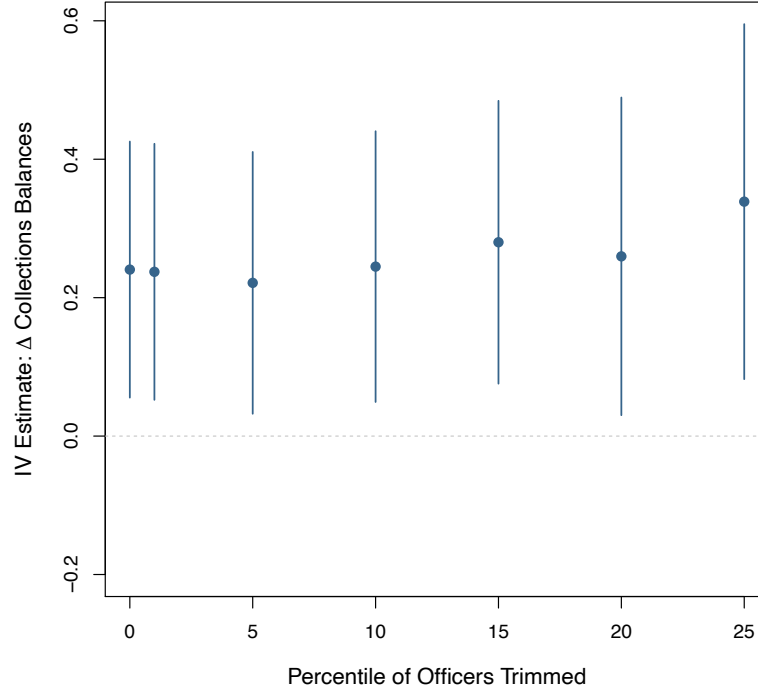
Notes: Each panel reports coefficients and 95 percent confidence bands from separate regressions of $Y_{\tau} - Y_{-1}$ (i.e., the change in Y relative to $\tau = -1$, where τ indexes event time) on the officer stringency instrument. All regressions include beat-shift fixed effects and motorist controls.

Figure G-6: Reduced form estimates by motorist income



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

Figure G-7: Robustness of IV estimates to sample selection



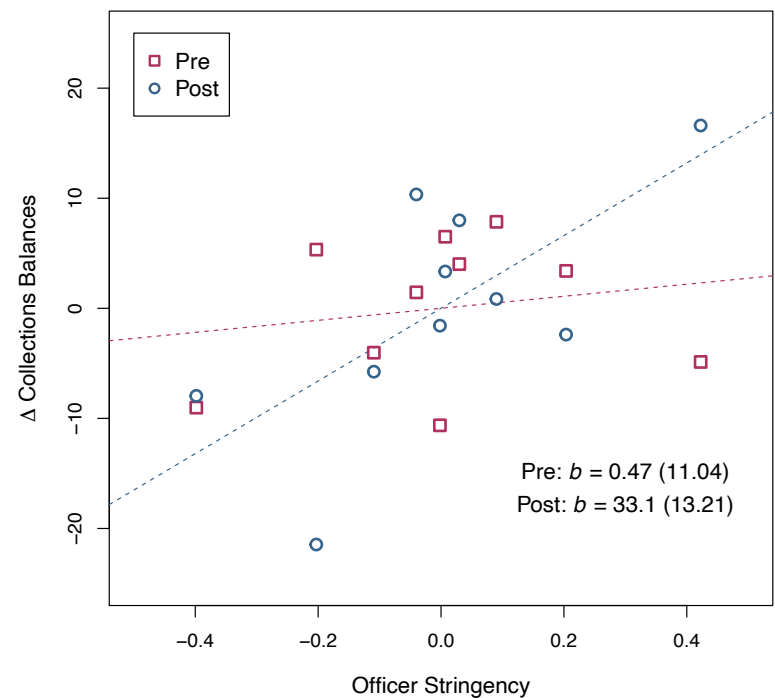
Notes: This figure reports IV estimates for the one-year change in collections balances when trimming the sample of officers with selected samples. First, a covariate index \hat{Y} is constructed by regressing Y on motorist demographics using only the sample of lenient officers. Then, I construct residuals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

Table G-5: IV Results with alternative instruments

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

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

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

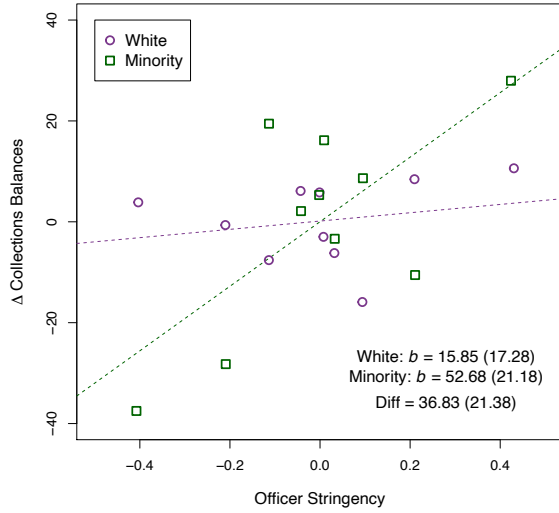


Notes: Same as figure G-1 using a subsample of the IV sample that requires only one citation per motorist (the first in-sample citation per motorist) and requires that each motorist has not received a citation in the previous year ($N = 272,866$).

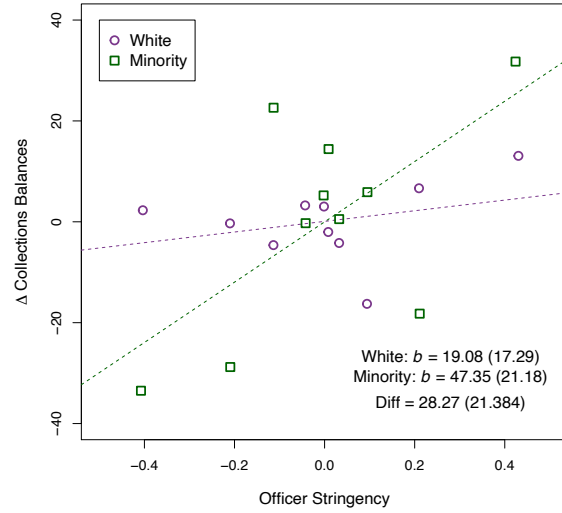
G-5 IV estimates by motorist race

Figure G-9: Officer IV reduced form estimates by motorist race

(a) Without controls



(b) With controls



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.

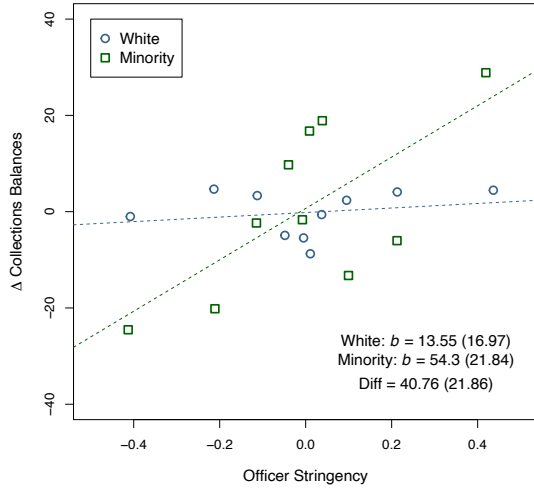
Table G-6: Officer IV results by motorist race

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

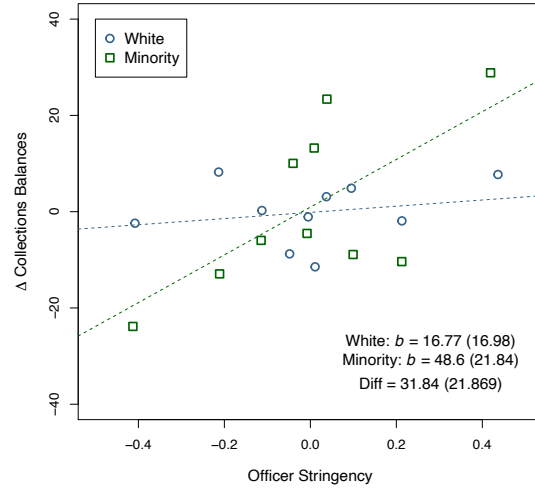
Notes: This table reports 2SLS IV estimates from the officer IV design by motorist race. All regressions include beat-shift fixed effects and standard errors are clustered at the beat-shift level. Dependent variable is the DiD in collections balances, $(Y_\tau - Y_{-1}) - (Y_{-1} - Y_{-4})$, for $\tau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

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

(a) Without controls



(b) With controls



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