Tripping through Hoops: The Effect of Violating Compulsory Government Procedures

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Abstract

Millions of Americans must navigate complex government procedures under the threat of punishment. Violating these requirements can lead to poverty traps or deepening legal system involvement. We use a field experiment to estimate the effect of failing to appear for court on subsequent legal contact. The treatments reduce failure to appear by 39 percent. Using treatment assignment to identify the causal impact of minor procedural violations, we find no effect on arrests. However, for lower-level cases, violations increase fines and fees paid by 60 percent or $80, equivalent to a high-interest loan, showing that minor procedural violations can be costly.

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Each year, millions of Americans are obligated to navigate compulsory government processes under threat of punishment, usually because they were accused of wrongdoing. For example, more than 42 million traffic cases come to courts each year (National Center for State Courts, 2018); 4.5 million adults in the US must comply with probation or parole procedures (Oudekerk and Kaeble, 2021); and parents of 37 percent of children must navigate investigations by child protective services at some point before the child is 18 (Kim et al., 2017). In such cases, individuals must comply with often complex and burdensome requirements—in effect, jump through a series of hoops.

Violations of these compulsory government procedures can lead to monetary sanctions, loss of freedom, or other serious consequences, even when these violations are minor. For example, failing to appear for a traffic hearing can result in a bench warrant, which may increase the likelihood of future stops and arrests (Needham, Mackall and Pettit, 2020). Sanctions for procedural violations can stack on top of the original offense, creating a cascading effect that leads to deeper and entrenched entanglements in the court system (e.g., Goffman, 2009, 2014; Kohler-Hausmann, 2018; Needham, Mackall and Pettit, 2020).

Although there is much evidence on the effects of government policies, there is little evidence on the effect of an individual violating compulsory government procedures. For example, while Dobbie, Goldin and Yang (2018) estimate the effect of pretrial detention on defendants, there is little evidence to quantify the impact of defendants jumping bail. It is difficult to estimate the effect of violation behavior itself because whether an individual commits a violation is often confounded by other factors. For example, estimating the impact of bail jumping is likely confounded by an unobserved propensity for violations.[1]

We examine the long-term consequences of violating compulsory government procedures in the context of failure to appear (FTA) for traffic, municipal, and criminal misdemeanor court hearings. This is a particularly instructive setting because failing to appear is common and potentially costly. Across the country, about 23 percent of felony cases where the defendant was released pre-trial resulted in a failure to appear (Cohen and Reaves, 2007).[2] For lower-level cases, such as traffic court, municipal violations or misdemeanors, the rates can be much higher. For low-level criminal, quality-of-life offenses

[1] Even with plausibly exogenous variation, researchers may not observe all potential and actual violations. For example, when examining the impact of receiving traffic tickets, Mello's (2021) estimates cannot be generalized to the impact of violating traffic laws because the sample included violations that were detected and received tickets, not the universe of traffic rule violators.

[2] Although our study focuses on less serious cases, there is a lack of national data on failure to appear for misdemeanor and non-criminal offenses.

in New York City, as many as 47 percent of defendants fail to appear for their hearings (Fishbane, Ouss and Shah, 2020). Failing to appear for a hearing carries harsh statutory consequences. These may include a warrant, a driver's license suspension, and additional fines and fees. Thus, a relatively minor violation like failure to appear could draw individuals into greater contact with the criminal justice system through arrests and new offenses (Crozier and Garrett, 2020; Needham, Mackall and Pettit, 2020; Huebner and Giuffre, 2022; Duda-Banwar and Burt, 2022). Additionally, fines and fees can cause financial distress, especially for people with low incomes (Harris, Evans and Beckett, 2010; Mello, 2021; Kessler, 2020). On the other hand, receiving warrants or additional fine and fee assessments may not lead to adverse court contact or financial distress if, for example, defendants can avoid police officers or refuse to pay their fines.

We conducted a randomized controlled trial to test the effect of nudges on violations and use the treatment assignment as an instrument to estimate the causal impact of a violation on downstream outcomes. We implemented the experiment from 2018 to 2019 in partnership with an anonymous court system that hears cases from both a county and city in a mid-sized metropolitan area. We estimated the effects of text-message-based reminders and offers of text-message-based assistance from court staff with over 30,000 defendants. One treatment arm provided relevant information about the case, the hearing time, and court accommodations such as the ability to reschedule the hearing or use a payment plan. The second treatment arm included both these reminders and the opportunity to text back and forth with court staff. The treatments had similar effects and reduced FTA rates by 39 percent from a base rate of 21 percent, a similar magnitude as effects found in other studies on court date reminders (Fishbane, Ouss and Shah, 2020; Bornstein et al., 2013).

Our analysis of downstream impacts considers two types of cases separately, based on the statutory consequence of an FTA. In the first set of cases, which include more serious traffic tickets, municipal violations, and misdemeanor cases, an FTA triggers a warrant for the individual's arrest. If these warrants cause more subsequent criminal justice involvement, such as additional court cases or loss of freedom, then the consequences are potentially very costly. We find that FTAs for these cases do not cause more involvement in the criminal justice system, as measured by new cases and jail bookings. Instead, we find that an FTA causes people to use an administrative procedure to clear their warrants that allows them to address the warrant at a time of their choosing. Take-up of this procedure may reflect defendants' need for flexibility. The importance of flexibility is also reflected in the fact that the interventions, which give information about how to resched-

ule a hearing, do in fact prompt an increase in rescheduling.

For the second set of cases, mostly low-level traffic cases, an FTA prompts an automatic conviction. When convicted, the (absent) defendant is sentenced to pay fines and fees, and often has additional fines and fees because of the FTA. Consequently, one worry is that an FTA may lead to financial distress. We find that an FTA causes a 60 percent ($80) increase in fines and fees paid—equivalent to a high-interest loan from the court. The increase in payments leaves low-income and liquidity-constrained individuals more vulnerable to financial shocks (Mello, 2021).

Our findings underscore the importance of measuring the realized consequences of violations rather than the stated, statutory consequences. For lower-level cases, we find that the increase in fines and fees paid *exceeds* the amount implied by the statutory consequences of FTA. However, for more serious cases FTA does not lead to deepening criminal justice contact as suggested by other work. Failing to accurately measure the realized consequences of violating government procedures leads policymakers to emphasize the wrong consequences when seeking to reform court processes. In this setting, much policy-making attention has focused on failures to appear potentially causing more arrests, but less on the direct financial consequences, which we find to be more significant (e.g., Donnella, 2017; Burness, 2019; Evans, 2019).

Moreover, our findings suggest that a long view of procedural requirements might be necessary. The majority of the defendants who initially violate the requirement to show up to court do end up complying by clearing their warrants or paying their fines and fees. That is, they end up jumping through the required hoops, even if they tripped on the first pass. And because of the stumble, they pay additional costs, either in time or money.

Our study complements the large body of research that explores how administrative burdens create barriers to accessing government benefits (Currie, 2006; Sunstein, 2018; Herd and Moynihan, 2019; Martin, Delaney and Doyle, 2022). Relatively few quantitative studies have explored the impacts of administrative burdens in the context of *compulsory* government processes.[3] Crucially, the existing literature focuses on areas where individuals are entitled to benefits, but are not required by law to navigate the administrative burdens. In our context of compulsory government procedures, on the other hand, individuals are required to navigate barriers under the threat of punishment. Furthermore, administrative burdens in the benefits context can theoretically be valuable for target-

[3]Administrative burden in punitive and compulsory contexts have been documented in sociology (e.g., Kohler-Hausmann, 2018).

ing benefits (Nichols and Zeckhauser, 1982). However, the burden of complying with administrative processes in compulsory settings do not advance efficiency or social welfare goals. Thus this work expands the scope of the administrative burdens literature in economics.

Our interventions highlight the administrative burdens imposed on defendants in complying with their court dates. Though the court we study attempted to make complying easier for defendants in numerous ways, the administrative difficulty of finding and utilizing these resources was substantial. The court allowed rescheduling, payment plans, and free child care during the hearing. Yet, without the interventions, very few people made use of either payment plans or rescheduling. That the information and assistance treatments increased usage suggests that these court-provided accommodations were previously too hard to access.

Furthermore, our estimates provide evidence on a missing link in the theory of deterrence and violation behavior. The probability of punishment and the expected utility of violations are major parameters in deterrence models (see Becker, 1968; Nagin, 2013; Chalfin and McCrary, 2017), but they are difficult to estimate. Even studies that assess the accuracy of perceptions of sanctions benchmark those perceptions with biased measures of statutory risk rather than experienced sanctions (e.g., Apel, 2013). We can use estimates of experienced sanctions to assess models of violation behavior. Interpreted in a rational actor model, our estimates for low-level cases suggest that defendants were willing to pay $80 to violate their ticket requirements. However, the cost of complying is likely less than $80, especially for defendants with better access to credit. This supports the hypothesis that many violations are unintentional rather than deliberate (Fishbane, Ouss and Shah, 2020). The results for more serious cases are consistent with both rational actor and behavioral models of compliance. We find that an FTA does not increase effective arrest risk. Nevertheless, rational defendants may be deterred by the cost of clearing a warrant and/or costly behaviors to avoid arrest (Flannery and Kretschmar, 2012; Duda-Banwar and Burt, 2022). On the other hand, that 79 percent of defendants comply at baseline suggest that there could be behavioral defendants who overestimate the risk of arrest from an FTA.

The rest of the paper is organized as follows. Section 1 introduces the study site, interventions and experimental design. Section 2 describes the study sample and take up. Section 3 discusses the treatment effects on FTA and subsequent court contact and explores treatment effect heterogeneity. Section 4 estimates the causal effects of failing to appear on defendants. Section 5 concludes.

1. STUDY DESIGN

1.1. STUDY SITE AND CONTEXT

Our study takes place in a county court's traffic, municipal and criminal misdemeanor cases. The county has over 700,000 residents.[4] Eighty percent of cases heard are traffic cases, which include speeding and driving with an expired license.[5] Municipal violations (6 percent of all cases) include offenses such as trespassing, loitering, and marijuana possession. Criminal misdemeanor cases (14 percent of all cases) include intentional property damage, driving under the influence, and low-level assault.

Defendants have been issued a ticket and instructed to return to court for a hearing. Defendants cannot choose the hearing date and time when they first receive the ticket. For some lower-level cases (e.g., speeding), a defendant can plead guilty and pay online or by mail, thereby obviating the need to show up at the first hearing. This is not an option for more serious cases (e.g., driving without insurance). Figure 1 shows the case resolution options for defendants in our sample. About 34 percent of traffic cases in our sample and 5 percent of municipal and criminal cases allow defendants to prepay.[6]

Hearings can have several outcomes. The judge can dismiss or amend any of the case's charges or the consequences.[7] If the case is not dismissed, a defendant may plead guilty or not guilty. If they plead not guilty, a new court date is set for the case to be heard. If a defendant pleads guilty, their fines and fees are due immediately. If fines and fees surpass $99—which is true for 93 percent of defendants who were found guilty of at least one charge—defendants can apply for a payment plan, which costs $25.

A defendant has failed to appear if they do not (a) show up at the hearing, (b) reschedule[8] the hearing beforehand, or (c) plead guilty ahead of time and pay fines/fees in ad-

[4]The population of defendants is not representative of the county as a whole. In the court at large, 37 percent of defendants during our time period are female. Relative to the proportion of the county as a whole, Black individuals are 79 percent overrepresented in our sample, and Hispanic individuals are underrepresented by 74 percent. The median age of our sample is 31 years old, a few years younger than the median age in the county (US Census Bureau, 2017). The average income in defendants' zipcodes is 10 percent higher than the median income in the county.

[5]These statistics include all cases that occurred during our sample period, regardless of whether they were eligible for our study. As such, the numbers do not match the descriptive statistics of our sample in Table 1.

[6]A handful of cases that can be paid ahead by statute were, during our study period, accidentally designated as unable to prepay due to a glitch in the court's computer system. To pay online for these cases, defendants must contact the court to override the glitch.

[7]For example, defendants charged with driving without insurance may bring proof of newly purchased insurance, prompting the judge to reduce the fine.

[8]Defendants can FTA on the rescheduled hearing, which leads to the same outcomes as failing to appear

vance of their court date.

The statutory consequences of an FTA vary by case type (see Figure 1). For 65% of our sample, an FTA leads to a bench warrant for the defendant's arrest. In this jurisdiction, a bench warrant is generally not actively pursued in this district, but will show up on the individual's record.[9] A little over half of the cases where an FTA leads to a bench warrant are more serious traffic offenses and the remaining are misdemeanor or municipal violations. In the vast majority of these cases (97 percent), the case will not reach a conclusion until the defendant attends court. For about 35% of cases in our sample, an FTA results in an automatic conviction, also known as a default judgment. With an automatic conviction, the defendant is found guilty and sentenced in their absence. Cases where an FTA leads to an automatic conviction are generally less serious traffic violations. An FTA for these cases will also trigger a driver's license suspension. About two-thirds of these cases can be resolved by pleading guilty and paying the fine before the court date.

FTAs can also result in additional fines and fees: in this jurisdiction there is a $30 FTA fine, a $25-50 warrant-issuing fee, and a $100 warrant cancellation fee, on top of any fines/fees the original infraction incurs. Showing up to court incurs a fee of $26, even when attendance is mandatory. Failure to pay fines and fees within 3 days of a sentence incurs an "outstanding judgment warrant," which does not lead to arrest, but places a hold on a person's driver's license until fines and fees are paid. Failure to pay after 30 days incurs a $50 failure to pay fee.

The court provides several accommodations that attempt to make the process easier for defendants, but these accommodations are unlikely to be known to defendants. A case's first hearing can be rescheduled once to another date within two weeks of the first assigned date. Defendants may not know that they can use a payment plan to relieve liquidity constraints. Finally, defendants may be unaware that the court provides free childcare during the period of one's hearing from 7:30 am to 5 pm, Mondays through Fridays.

1.2. INTERVENTIONS

Under normal circumstances, a defendant receives no reminder about their hearing. We introduce an informational nudge treatment and a treatment that provides both an

on the original hearing.

[9] For example, if a driver with a bench warrant is stopped for speeding, the police officer will see the warrant when querying the driver's information in the police database. However, officers are not monitoring for license plates associated with a suspended driver license.

informational nudge and an opportunity to text back and forth with court staff. In both treatment arms, the defendant receives text message reminders 14, 7 and 1 day in advance of their court date with information about the case itself (including the case number, the time and location of the court date, and whether it can be paid online), information about the consequences of not showing up (a warrant, a driver's license suspension, or a fine, as applicable), and court services they can access (such as payment plans, court date rescheduling, or free childcare). Appendix Table A.1 displays a set of example texts.

Texts on each day are divided into two messages to accommodate character limits. Per federal regulations, defendants had the option of opting out of text message reminders at any point. Thus, defendants receive up to 3 doses with two messages each, for a total of up to 6 text messages per case.

In one treatment arm defendants also receive an invitation to text back with questions to a court staff-person. We call this treatment the "personalized assistance" arm. Court staff monitor the messaging software for incoming messages and respond to defendant texts similarly to how they respond to defendants' phone and in-person queries, both answering questions and directly handling paperwork to, for example, reschedule hearings.[10]

Notably, the personalized assistance treatment uses scalable, government-administered assistance in navigating administrative processes. Many nudge-to-navigation assistance programs that increase the engagement with government processes do so by channeling individuals toward non-profits or other services for assistance. For example, low-income individuals are encouraged to have volunteers, non-profit staff, or for-profit staff help fill in their taxes (e.g., Linos et al., 2020; Goldin et al., 2022), assist in benefits applications (Finkelstein and Notowidigdo, 2019), and complete FAFSA information (Bettinger et al., 2012). In contrast, the navigation assistance used in this study relies only on existing government staff, is low-cost, and is easy to scale-up. In particular, by allowing defendants to reschedule without having to submit a confusing legal form, the assistance eased defendants' access to the court's flexibility.

[10]The personalized assistance intervention is in line with emerging court practices. As part of the broader attempt to make courts more accessible, criminal justice practitioners have explored the provision of personalized assistance through texting to encourage defendants to show up. Indeed, several services have emerged to help courts administer these two-way texting programs, such as eCourtDate, Uptrust, and Stanford's Court Messaging Project.

1.3. Experimental Design and Methodology

Between March 7, 2018 and July 15, 2019, the court randomized 31,372 eligible individuals into a control group or one of two treatment groups: an informational intervention or an intervention composed of the same information with an offer to text back and forth with a court clerk.

The randomization algorithm ran each morning, to check for cases that met the following criteria:

- The hearing is in 14, 7, or 1 day.
- The hearing has not already been resolved by paying online, pleading guilty by mail, rescheduling, or other pre-hearing action.
- The person has not already received a treatment assignment. Previously randomized individuals (as identified by name and date of birth match) receive the same treatment assignment.
- The person's address is not listed as "transient" or "homeless."[11]
- The person has a phone number in the database.[12]

This list of eligible cases was then sorted by random number and treatment was assigned in a rotating manner, resulting in a 50-25-25 proportion split between the control, information and personalized assistance treatments. The treatment assignment was stratified at the courthouse-day level. Criminal and municipal cases, which are seen in the same courthouse, were randomized together while traffic cases were randomized separately.

If a given defendant had multiple cases during the study period, they received the same treatment assignment in subsequent cases, but only the first case was included in our analysis sample. Only 3,551 cases in our time period—10 percent of cases—were subsequent cases for defendants.

[11] A pre-study sample of all cases heard during a two-month interval suggests that no more than 6 percent of cases had homeless or transient defendants.

[12] Defendants are not legally obligated to share their phone numbers with police officers or court personnel. About 40 percent of cases that were not resolved two weeks prior to the hearing had phone numbers on file. In New York City, only 13 percent of defendants have available phone numbers on file (Fishbane, Ouss and Shah, 2020). Defendants without phones but with address information were randomly assigned to a postcard reminder or control condition. Postcards reduce FTA by 5 percentage points off a baseline 24 percent FTA rate, an effect that is statistically significant at the 1 percent level. We compare the characteristics of defendants with and without phone numbers in the court system for the full sample and each of our analysis samples in Tables A.3, A.4, and A.5. We report reweighted analyses of the treatment effects on court date outcomes in Appendix Table A.6.

1.4. DATA AND METHODS

Court data include defendant characteristics, case characteristics, treatment assignment, case dispositions, case events (e.g., hearings, bond setting, etc.), fines and fees assessed, and fines and fees paid. It also includes a defendant's subsequent traffic, municipal, and criminal misdemeanor cases, but does not include felony cases. The case event data include events with their corresponding dates, allowing us to follow cases over time. The fine and fee assessment and payment data come from a snapshot on August 6, 2019 — three weeks after the conclusion of our randomization. The fine and fee analyses pool cases with different follow-up periods ranging from 3 to 74 weeks post-hearing and an average follow-up period of 40 weeks. We used jail booking data from the Sheriff's Office, which included data on date of booking and bookings for all reasons, including felony charges.

A major credit bureau provided credit report data for February, June, and October 2018. We provided name, date of birth, and home address information for individuals in our study sample to the credit bureau to match to each of their monthly data files. The linking process resulted in a 64 percent of the sample having credit bureau data at baseline. There was a higher match rate for the automatic conviction sample (72 percent versus 59 percent). We used the month of the citation or the most recent month before the citation with available data for the individual's baseline data. We use data on the individual's credit score (VantageScore® 4.0) as a proxy for access to credit.

We start by estimating the how the treatments affect failure to appear rates:

$$F_i = \beta_0 + \beta_1 T_i + \beta_2 \mathbf{X}_i + \beta_3 \mathbf{S_i} + \epsilon_i \tag{1}$$

where F_i is the outcome of interest, such as failing to appear on the first hearing; T_i is the treatment assignment; X_i represents a vector of baseline individual characteristics including age as a quartic, race, sex, average income in their zipcode of residence, case characteristics, such as the court division, whether the case can be paid before the court date, whether an FTA triggers a warrant or an automatic conviction, the number of charges faced and the time of day of the hearing, and finally a vector of criminal history binary variables such as a prior conviction, prior incarceration and prior FTA; and S_i is a vector of indicators for courthouse-day strata. We include X_i to improve precision. Our analysis excludes 502 individuals missing zipcode information and therefore missing income

data.[13] Standard errors are robust to heteroskedasticity.

Next, we estimate the reduced form relationship between an FTA and downstream criminal justice contact and financial status:

$$Y_i = \gamma_0 + \gamma_1 T_i + \gamma_2 \mathbf{X}_i + \gamma_3 \mathbf{S_i} + u_i \tag{2}$$

where Y_i includes fines and fees charged and paid, having a warrant on one's case, being booked into jail, having a bond return court date scheduled, and having a new case within our study period.

To estimate the causal effect of an FTA on fines and fees as well as subsequent criminal justice involvement, we use a two-stage least squares framework.[14] We use treatment assignment as an instrument for FTA. The first stage is given by Equation 1 where F_i is a binary indicator for whether the defendant failed to appear on the randomized hearing.

The second stage analyzes the effects of an FTA on outcomes including fines and fees paid as well as subsequent criminal justice involvement (Y_i):

$$Y_i = \sigma_0 + \sigma_1 \hat{F}_i + \sigma_2 \mathbf{X}_i + \sigma_3 \mathbf{S_i} + v_i \tag{3}$$

where $\hat{F}_i$ is the predicted FTA from Equation 1.

2. Descriptive Statistics & Take Up

Individuals with cases in our sample tend to be male (63 percent), White (71 percent) or Black (17 percent), fairly young (on average, 35 years old), and living in zipcodes with an average income of over $66,000 per year (see Table 1 and Table A.2). Traffic court sees 68 percent of cases, with the remainder split between municipal and criminal courts (9 and 22 percent, respectively). Only 25 percent of cases may be paid ahead of time. On average, each case has 2.28 charges on it. Almost half of the individuals with cases in our sample had a prior case and 21 percent have previously failed to appear. Comparing the automatic conviction sample to the warrant sample, defendants in automatic conviction cases are less likely to be male, more likely to be White, less likely to be Black, are slightly older, live in richer zip codes, and have prior adverse contact with the court system less frequently.

[13] Our results are robust to including people missing zip code data and excluding covariate controls. We present these results in Appendix B.

[14] The two-stage least squares analysis was not initially included in our preanalysis plan, though it was included in a grant application from the preceding year.

Though a handful of criminal history and case characteristic variables are out of balance within our analysis samples, they are not jointly significant, suggesting that there was not systematic bias in the randomization process. Appendix Table A.2 shows descriptive statistics by intervention.

The SMS software suggests that the delivery rate of the treatments was high. Of treated cases, only 3.27 percent had an undeliverable message or number. An additional 0.92 percent of treated cases had a person reply saying it was the wrong number. Only 2.3 percent of treated defendants opted out of receiving additional reminders.

Selection into the study sample based on phone number availability resulted in a sample that was *less* advantaged than defendants as a whole. Police officers were more likely to ask for phone numbers in criminal and municipal cases, resulting in a 70% phone availability rate for criminal and municipal cases and a 35% availability rate for traffic cases. Defendants with phone numbers were more likely to be Black, more likely to have a prior case and FTA, and had lower credit scores (see Appendix Table A.3). The focal cases in the study were also less likely to allow defendants to pay fines ahead of the court date and had more charges. Similar patterns of selection exist within the automatic conviction and warrant analysis samples, but the differences are not economically large (see Appendix Tables A.4 and A.5, respectively). We report reweighted analyses of the treatment effects on failure to appear and related court date outcomes in Table A.6. The estimated treatment effects are almost identical to those from the main specification.

3. INTERVENTION RESULTS

The treatments were highly effective in reducing FTA rates. We provide evidence on the results, pooling both treatments before discussing the (lack of) differences between the two interventions' effects on FTA. We then discuss heterogeneous treatment effects by defendant characteristics. Finally, we discuss the effects of the pooled treatments on downstream criminal justice outcomes.

3.1. INTERVENTION EFFECTS ON FAILURE TO APPEAR

3.1.1. POOLED RESULTS

The treatments were highly effective in reducing failure to appear in both samples (see Figure 2). Among defendants with cases in which an FTA results in a warrant, treatments reduce FTA rates by 6.2 percentage points, a 29 percent decrease from a 21.2 percent mean. Since virtually none of these cases can be resolved by paying in advance, the increase in

legal compliance only stems from two methods: an increase in appearing in person (3.5 percentage points or 5 percent) and rescheduling hearings (3.6 percentage points or 88 percent).

In the sample where an FTA results in an automatic conviction, the treatments reduced failure to appear by 8.8 percentage points, a 42 percent decrease from a 20.9 percent mean. The increase in legal compliance came from several sources: an increase in appearing in person (1.9 percentage point or 11 percent), paying and pleading guilty in advance (4.3 percentage point or 8 percent), and rescheduling hearings (2.1 percentage point or 72 percent). There are also smaller but statistically significant increases in defendants' use of payment plans and in getting their cases dismissed.

These treatments are cost-effective, scalable interventions that can improve defendants' welfare without changing legislative policy. The informational nudge messages cost $0.10 per defendant in software and messaging costs compared to $0.78 per defendant for the personalized assistance nudge, which incurred additional messaging and staff costs.[15] In comparison, a summons redesign would have negligible marginal costs (Fishbane, Ouss and Shah, 2020) and postcard-based reminders (e.g., Bornstein et al., 2013) would cost around $0.46 per defendant for postage and printing. Since it takes about 12.5 treated defendants to eliminate one FTA using the text message interventions, the cost of a single reduction is $1.25 for informational nudges and $9.75 for informational nudges with personalized assistance.

3.1.2. Differences Between the Two Treatments

Offering text conversations with court clerks does not enhance the effect of text message reminders on FTA, since the treatment arms experienced similar FTA rates in all case types. Each treatment arm reduced FTA rates by 8.3 to 8.6 percentage points, around a 40 percent reduction from a control mean of 21.2 percent. Appendix Figure A.1 shows case outcomes by treatment status.

However, the personalized assistance add-on is more effective at encouraging take-up of court accommodations. Personalized assistance increases the probability of rescheduling by 4.1 percentage points over a control mean of 3.7 percent, while the informational nudge boosts rescheduling by 1.8 percentage points. Personalized assistance increases payment plan usage by 0.9 percentage points off a control mean of 6.9 percent. Both actions require submitting information to the court and navigating legalese, which speaking

[15] Cost estimates for Fishbane, Ouss and Shah (2020)'s text reminder program are even lower due to fewer messages per dose and do not include software costs.

to court staff over text could make easier, similar to the benefit take-up context (Goldin et al., 2022; Finkelstein and Notowidigdo, 2019; Bettinger et al., 2012). In contrast, the informational nudge is slightly more effective at increasing appearances at the hearing, causing a 4.5 percentage point increase over a control mean of 47 percent compared to a 1.9 percentage point effect from the personalized assistance arm.

Personalized assistance increased the take-up of court accommodations, but did not further decrease FTA, which suggests that the barriers to FTA could not be overcome by the flexibility afforded by available accommodations. Resolving one's arraignment often requires showing up in person or paying substantial fines and fees. Court clerks in the study site estimated that awaiting one's hearing and the hearing itself may take four hours plus transit time. The flexibility gained from the available accommodations does not change the reality that showing up to court may be costly for people who would get fired for missing work or for whom paying a $200 fine would lead to financial distress. However, that defendants increase their use of rescheduling and payment plan opportunities is consistent with defendants needing more flexibility than the court initially allowed.

3.1.3. HETEROGENEOUS TREATMENT EFFECTS

We estimate treatment effects by defendant characteristics to understand the distributional effects of nudging in the criminal legal system.[16] Appendix Figure A.2 displays treatment and interaction effects with different defendant characteristics. In the automatic conviction sample, receiving a nudge is more effective for female defendants, which has also been seen in a benefit take-up context (Finkelstein and Notowidigdo, 2019). The treatment effect for Black defendants is not statistically significantly different from that of White defendants, consistent with other studies on nudges to increase appearance (Rogers and Feller, 2018; Fishbane, Ouss and Shah, 2020). However, due to a higher baseline FTA rate for Black defendants, the relative difference in FTA rates between White and Black defendants become larger under the treatment condition. The FTA reduction for Hispanic defendants is smaller than for White defendants, but the two coefficients are not statistically significantly different. Baseline FTA rates and treatment effects for defendants living in zip codes with average incomes above and below the sample median are similar, which is in contrast to other studies finding text reminders were more effective for people with lower socioeconomic status or living in lower income areas (Castleman

[16]We consider this analysis exploratory as the study was not sufficiently powered to detect a range of economically significant differences in treatment effects, especially for subgroups with smaller sample sizes, such as defendants who were Hispanic or categorized as "Other race."

and Page, 2017; Fishbane, Ouss and Shah, 2020). Finally, the treatment is more effective for defendants with a prior FTA by 3.4 percentage points, in contrast to prior findings on the lack of heterogeneous treatment effects of a nudge for school absences by prior absences (Rogers and Feller, 2018). The difference is almost large enough to close the FTA disparity between those with and without prior FTAs (5.9 percentage points). In the warrant sample, we do not find statistically significant differences in treatment effects by defendant characteristics. However, we see larger reductions in FTA for Black defendants compared to White defendants and for those with a prior FTA.

A concern with nudges delivered through low-cost outreach is that differences in access to technology or rates of correct contact information may exacerbate existing disparities. Although we cannot observe who received the text message reminders, we can analyze differential engagement with the personalized assistance treatment to assess access to the nudges. We find that defendants who initiated a conversation with court staff were more likely to be female, Black, older, and have a prior FTA (Appendix Table A.7). The cases of conversation initiators were more likely to be ineligible to be paid ahead of the court date and more likely to have a warrant as a consequence of FTA. These findings suggest that the offer of text message-based support can reach harder-to-reach groups and have the potential to decrease disparities by race and prior court contact. However, the assistance offered or language used must adequately address the barriers faced by those groups to deliver on that potential.

3.2. Intervention Effects on Downstream Criminal Justice Outcomes

The treatments can potentially reduce future criminal justice contact by preventing the consequences of an FTA. To assess whether this is the case, we consider downstream outcomes, including fines and fees charged and paid as well as warrants, jail bookings, whether the defendant bonded out, and whether they were the defendant in a new case.

3.2.1. Warrant Sample

In cases where an FTA prompts a warrant, the amount of fines and fees levied are substantial. On average, defendants are charged $270 and ultimately pay about $117 (43 percent). *A priori,* it is unclear whether preventing an FTA is likely to increase or reduce fines and fees. Since fines and fees associated with a conviction cannot be levied until the defendant appears and the case is decided, the treatments may actually increase realized fines and fees charged by spurring case dispositions. On the other hand, preventing FTA

would decrease fines and fees associated with an FTA and could encourage more lenient sentencing.

Empirically, we find that the treatments have no average impact on fines and fees charged or paid (Table 3(A)). It is possible that combination of increasing dispositions, but decreasing FTAs may create a zero effect on net.

Defendants with cases where an FTA results in a warrant have high rates of future criminal system contact. Almost 30 percent of cases in the control group end up having a warrant for the case. Over 14 percent have a jail booking within a year, and just over 17 percent feature a defendant going through a bond process. Only 3.5 percent of defendants end up with a new case within a year. We would expect the interventions to decrease warrants issued on the case by reducing FTA for this court date, but may not prevent other causes for warrants or arrests, such as new charges.

Empirically, we find that the interventions decrease the probability of a warrant being issued by 16 percent (a reduction of 4.7 percentage points), but affect neither jail bookings nor new cases (see Table 3). The lack of an effect on jail bookings despite a reduction in warrants is consistent with the fact that FTA warrants are not proactively pursued in this jurisdiction. That is, police officers in this jurisdiction do not spend time specifically searching for people with outstanding FTA warrants, but may initiate an arrest if a person with an FTA warrant is encountered.

Defendants may also reduce their time at risk by addressing their warrant before being arrested. The negative treatment effect on "bond return" dates provides some evidence of this. Although bond return dates are typically associated with a release from custody, not all bond return dates are associated with detention, as evidenced by a higher rate of bond return court dates than jail bookings in the control group. Discussions with court and jail staff revealed that defendants who are not detained can initiate "walkthrough bonds." In the walkthrough bond process, defendants with open warrants voluntarily travel to jail and fill out paperwork to resolve their warrant and set a new court date without being detained. In this process, the rescheduled court date is labeled a "bond return" date similar to hearings in the traditional bond process. We cannot connect a bond event to a specific jail booking, but we find that most of the reductions in bond return court dates are driven by reductions in court dates that are not preceded by a jail booking, suggesting that these bond return court dates were for walkthrough bonds (Appendix Table A.8).

3.2.2. AUTOMATIC CONVICTION SAMPLE

Cases in which an FTA results in an automatic conviction generally come with substantial fines and fees. Defendants in the control group are charged an average of $171 and end up paying $152 (88 percent of the total owed). Since these fines and fees include those that are levied for failing to appear, the interventions can reduce the amount charged by preventing further violations or leading to a more lenient sentence or case dismissal. Indeed, the text message interventions reduce fines and fees charged by 3.7 percent (see Column (1) in Table 3(B)). This may not translate into tangible cost reductions for defendants if those who fail to appear continue to violate court orders by refusing to pay. However, we find that the interventions also reduce the amount *paid* by 4.6 percent, suggesting that defendants, including those who FTA, are responsive to changes in the size of court debt (see Column (2) in Table 3(B)).

It is possible for FTAs can lead to more court contact because an FTA-triggered driver's license suspension may lead an individual to drive without a current license, which is a traffic violation. However, we do not expect the interventions to have large impacts on future adverse court contact as there is very little criminal justice involvement among defendants with automatic conviction cases. In the control sample, almost no cases result in a warrant, only 2 percent experience a jail booking within one year, 0.1 percent have a bond return court date, which are typically scheduled after obtaining a bond, and only 2.7 percent have a new case within a year.

The text message interventions have little or no effect on other downstream contact with the criminal justice system. The treatments increase the percent of defendants with warrants issued on their case by 0.1 percentage points. The treatments also increase the percent of defendants with bond return court dates of 0.2 percentage points, which could either indicate that the defendant has bonded out of custody or obtained a non-custodial bond that closes a warrant. Appendix Table A.8 shows that both types of court dates increase, but the effect on court dates without a preceding jail booking (suggesting a non-custodial bond) is statistically significant.

4. CAUSAL EFFECTS OF FAILURE TO APPEAR

We estimate the causal effects of an FTA on adverse court contact to understand the *de facto*, experienced consequences rather than the *de jure*, statutory consequences of a common legal violation. These estimates quantify the costs a rational actor might consider when weighing whether to comply or FTA.

Since FTAs and the consequences of an FTA can be related to unobserved variables, such as a defendant's underlying propensity to be arrested, estimating an unbiased effect of an FTA is difficult. We use random assignment to treatment as an instrument for FTA in a two-stage least squares (2SLS) analysis to address omitted variable bias. The 2SLS estimates scale the effects of the treatment on subsequent criminal justice system contact by the effect of the treatment on FTA. If the intervention only impacts outcomes through reductions in FTA, then our analysis represents the causal impact of an FTA on fines and fees paid or criminal justice outcomes for those who are influenced to comply by the treatments.

4.1. WARRANT SAMPLE

For cases where an FTA triggers a warrant, an FTA does not increase involuntary court contact, though there is an increase in costly voluntary engagement with the court. An FTA increases the warrant rate by 66 percentage points (186 percent). This increase in warrants issued is in part a mechanical effect as the law requires that an FTA on such cases triggers a warrant. But the positive effect shows that many defendants would not have otherwise incurred a warrant on the case, which could occur for missing subsequent hearings.

We explore whether these warrants "trap" individuals into ever more contact with the criminal justice system. Warrants issued as a result of an FTA show up if a police officer looks up an individual, if for example, the defendant got a subsequent speeding ticket. However, warrants are generally not actively pursued by police, and they may be deactivated if a defendant either pays $100 to cancel the warrant online or travels to jail to obtain a walkthrough bond, both of which reschedule the initial court date. It is possible that a warrant does not increase involuntary court contact if either police do not pursue the warrant or individuals resolve the warrant on their own. Defendants may also engage in behavior unrelated to the focal case that results in a jail booking or other criminal legal contact. As such, it is an open question as to whether an increase in warrants is likely to cause an increase in court contact.

We use three measures to assess the downstream impacts of FTA warrants. We first measure jail bookings, which capture whether a defendant was arrested and put in jail for any reason, including the FTA warrant. We next measure bond return court dates, which indicate that an individual has either bonded out of detention or obtained a non-custodial walkthrough bond. Finally, we measure whether the defendant has new court cases after the one studied in the experiment.

We find that an FTA does not increase jail bookings (see Figure 3).[17] Rather, we find an increase in bonds. As noted in Section 3.2, we interpret this to mean that defendants are using walkthrough bonds, where they show up to jail to resolve their warrant and set a new court date. Furthermore, we do not see an increase in the number of new cases that defendants have within the following year.

It may still be the case that the increased arrest risk from an outstanding warrant is sizeable, leading defendants to travel to jail to resolve their warrant. However, this voluntary interaction can be completed at the defendant's convenience, whereas defendants do not have a choice of date and time for initial arraignments. Given the use of walkthrough bonds and pre-FTA rescheduling, defendants may place a premium on flexibility.

The lack of new cases and jail bookings refute a model in which an FTA traps defendants into subsequent adverse legal system contact in which the defendant experiences, in the worst case scenario, severe losses of freedom and, in the best case scenario, a seemingly never-ending mountain of additional court requirements to satisfy. Our findings suggest both a muted legal system response (i.e., lack of fulfillment of warrants) and the existence of options to escape potential traps (i.e., non-detention-based warrant resolution) in this jurisdiction.

4.2. Automatic Conviction Sample

For cases that result in an automatic conviction, an FTA increases the fines and fees paid by 60 percent — $79.95 from an average payment of $133.78 (see Figure 4). The increase in payments is accompanied by a similar increase in the amount that individuals are charged.[18] We note that since we cannot measure the effect of FTA on driver's license suspensions, and a person must pay $95 to reinstate a suspended license, we likely understate the financial costs of an FTA. Since defendants pay the increased amount more over a longer time horizon, the result is very similar to taking a high-interest loan from the court.

We analyze time to payment to directly compare the increase in fines and fees paid to loan interest rates. Appendix Figure A.3 plots the average percent of fines and fees paid among control cases where the defendant failed to appear by the age of the case (in weeks) as a proxy for payments over time. Most payments occur by 16 weeks after the court date. Using this figure to calculate a lower bound, an FTA represents an average APR of 196 percent. This is much higher than the interest rate of a payday loan, which is

[17] Appendix Table A.10 displays the results in a table.
[18] Appendix Table A.11 displays the results in a table.

capped below 50 APR in the study site's region,[19] and a credit card, with average interest rates around 14 APR during the study period (Board of Governors of the Federal Reserve System, 2022). The high effective interest rate compared to other forms of credit suggests that many FTAs are mistake rather than the result of an intentional decision.

The estimated elasticity of fines and fees paid with respect to those charged is above 1 (1.16; 95% CI: [1.13, 1.22]).[20] A point estimate greater than one shows that this municipality actually receives more money when they charge a defendant more after an FTA. This large elasticity helps explain why some municipalities use fines and fees to raise revenue (Maciag, 2019; Makowsky and Stratmann, 2009), potentially leading to a regressive form of taxation through law enforcement (Makowsky, 2019).[21]

We use credit bureau data to understand how financial consequences of failure to appear affect people with differing access to credit. We divide our sample into those who have high credit scores (a VantageScore rated as "fair" or higher), low credit scores (a VantageScore rated as "poor" or lower), and those who did not match to the credit bureau data. Those who are unmatched to the credit data are either unmatched because of false negative data linkages or lack a credit history. Appendix Table A.9 displays descriptive statistics for each credit data sample.

Failure to appear increases fines and fees paid for all three samples. The second and third sets of bars in Figure 4(B) show the results for those with high and low scores, respectively. Among those with comparatively good credit, the increase in fines and fees paid is \$84.48 (57 percent). For those with worse credit, the increase is smaller \$51.84 (39 percent).[22] The difference likely reflects lower ability to pay for those with worse credit. To contextualize this result, a payment plan with the court costs \$25, less than half of the \$52 increase in payment experienced by people with low access to credit. Finally, among those who are unmatched in the credit data, fines and fees increase by \$75.94 (67 percent). Thus when they FTA, even those with liquidity constraints or an inability to pay out of pocket experience an interest rate much higher than that of a local payday loan.

[19]This locality is rare in having a low cap on the APR associated with a payday loan. Payday loans elsewhere can have APRs as high as 400 percent.

[20]Because 0's are included in both fines and fees charged and paid, we cannot use the log-log specification to calculate the elasticity or its confidence interval. Rather, we use the classic definition ($\frac{\delta y/y}{\delta x/x}$) and Monte-Carlo simulations to estimate the confidence interval.

[21]Indeed, 22 states collect at least 40 percent of police expenditures in revenue from fines and forfeits (Maciag, 2019).

[22]The increase in fines and fees paid for those with low credit scores represents an APR of 109.

4.3. Characterizing Compliers

We examine the characteristics of defendants for whom treatment prevents FTA ("compliers") to understand the scope of our treatments and the generalizability of our estimates of the effects of FTA. Table 2 compares covariate means for compliers to the means for people who FTA in the control group.[23] We choose control group members who FTA as the comparison group because concerns over the effects of FTA are motivated by the experiences of people who have failed to appear.

In the sample of cases where an FTA results in a warrant, compliers are substantially less likely to have a prior case or a prior FTA. As such, when compliers with these cases avoid an FTA, they are more likely to be preserving clear records. Among the automatic conviction sample, compliers are more likely to be female and White, less likely to be Hispanic, and may come from a higher income zipcode. Compliers may also be less likely to be Black in both samples, differences that are statistically significant at the 10 percent level. If the effects of FTA are worse for male, lower income, and/or Black defendants or those with prior violations, we may understate the effects of FTA for those who experience FTA absent any intervention.

5. Discussion

This paper estimates the causal effect of violating compulsory government procedures—in effect, failing to jump through mandatory hoops. Using a randomized controlled trial, we estimate whether failure to appear for court impacts subsequent legal contact and financial payments.

For cases where an FTA prompts a warrant, we find that violating this mandatory government procedure does not trap people into deepening criminal justice contact. We find no increase in subsequent cases or jail bookings. But we do find defendants travel to jail to clear their warrants, an option that prevents warrants from leading to legal system traps. In these cases, it may be preferable to answer to a warrant on their own time rather than appear in court for many hours on a specified date. Indeed, our interventions substantially increase the use of rescheduling for these cases, emphasizing the importance of flexibility to defendants.

For cases in which an FTA prompts an automatic conviction, the causal effect of an FTA is equivalent to taking out a high-interest loan from the court. An FTA causes a 60 percent ($80) increase in fines and fees paid, much of it coinciding with an increase in the

[23] We calculate complier means using the method developed in Marbach and Hangartner (2020).

fines and fees levied on defendants. One may have expected defendants who failed to appear to continue to skirt requirements by declining to pay their charged fines and fees. Instead, we find that defendants end up paying more, but later. As a lower bound, the effective APR is 196, compared to the roughly 40 percent APR cap on payday loans in this locality. Given the difficulty low-income people have with paying traffic citations (Mello, 2021), the court may be effectively charging people a high fee for additional time to pay.

Overall, we find that the causal effects of an FTA are both larger and smaller than might be expected. Failing to appear for a low-level violation hearing causes a substantial increase in fines and fees paid, larger than the statutory FTA fine. On the other hand, an FTA does not increase involuntary court contact, even in cases where an FTA increases warrants. While we do not see cascading criminal justice involvement in a jurisdiction that volunteered to implement customer service-minded interventions, it is possible that outcomes would be worse in jurisdictions that more actively pursue arrests for FTA or charge interest explicitly (U.S. Department of Justice, 2015; Harris, 2016). Even so, failure to appear comes with a large penalty for low-income people. Our estimates of fines and fees paid may even be an underestimate, since we do not observe driver's license reinstatement fees, which can only be paid after clearing one's outstanding tickets.

Under the standard deterrence model, increasing the probability or the magnitude of the consequence for violations should increase compliance. However, our causal estimates suggest that in cases where an FTA causes an automatic conviction, the expected cost of violating is already high compared to reasonable alternatives. The fact that even people with good credit violated and consequently paid higher fines and fees—several times the interest rate of a payday loan—suggests that a large portion of violation behavior is due to mistakes, consistent with the hypothesis put forward by Fishbane, Ouss and Shah (2020).

In cases where an FTA causes a warrant, the threatened cost of a violation—namely an arrest—is not realized. These results are less conclusive about competing models of violation behavior. The fact that 79 percent of the population nevertheless complies with the procedure suggests they overestimate the probability of arrest (in a biased beliefs model of the world), have an extremely high cost of arrest (in a rational actor model), or are deterred by the time required to clear a warrant or remain "on the run" (again in a rational actor model).

Finally, contrary to mainstream beliefs, we find that many of the defendants who initially FTA ultimately fulfill the government's procedural requirements, though sanctions

make complying on the second try more costly. In effect, defendants trip rather than jump through government hoops. In both types of cases we study, it would be more efficient to ease compliance costs rather than increase sanctions. Simplifying or adding flexibility to compulsory government processes would facilitate compliance. The interventions in our experiment demonstrate highly effective ways of encouraging compliance with government procedures without increasing the probability or magnitude of sanctions.

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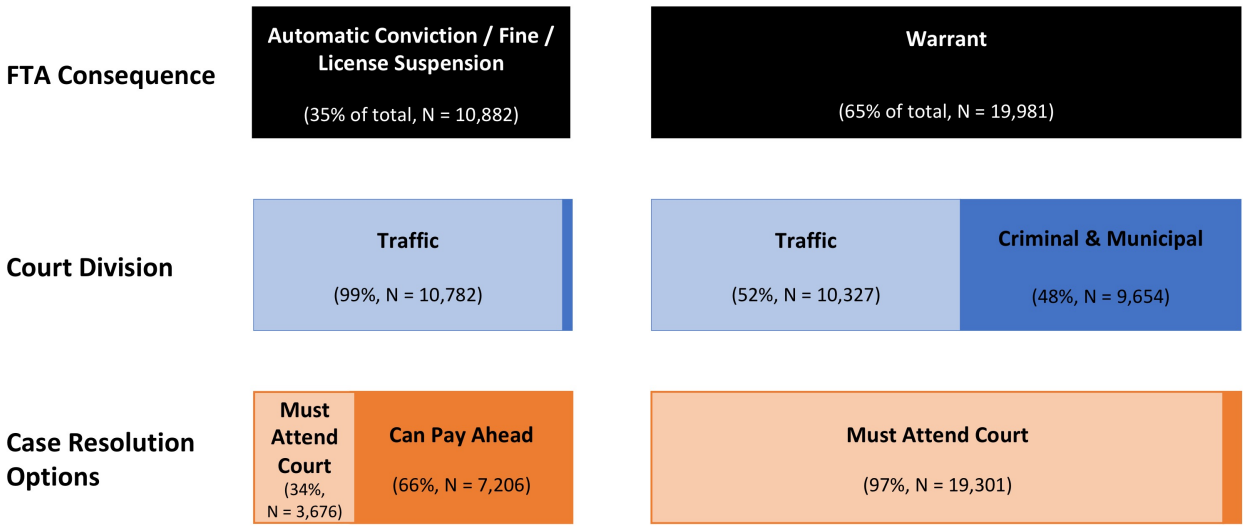

Figure 1: **Court Division, FTA Consequences and Case Resolution Options of Cases**

Notes: This figure depicts the 30,870 traffic, municipal, and misdemeanor cases in our study. We depict the consequences of failing to appear for a hearing (black bars), the court division that the case is in (blue bars), and whether or not a defendant must show up in order to resolve their case (orange bars). The exact charges on the case determine both whether a case results in an automatic conviction or a warrant, and also if the case can be paid ahead of time or the defendant must attend court in order to handle the case. Percentages in the black bars are of the total sample of cases. Percentages in the blue and orange bars show the percent of the cases shown in the black bar.

Table 1: Descriptive Statistics and Covariate Balance by Type of Failure to Appear Consequence

	Automatic Conviction		Failure to Appear Warrant	
	Control (1)	Treatment (2)	Control (3)	Treatment (4)
(A) Demographics and Income				
Female	0.395	0.401	0.344	0.352
	(0.489)	[0.636]	(0.475)	[0.561]
White	0.730	0.742	0.687	0.693
	(0.444)	[0.211]	(0.464)	[0.948]
Black	0.150	0.141	0.195	0.186
	(0.358)	[0.157]	(0.396)	[0.425]
Hispanic	0.077	0.069	0.080	0.082
	(0.267)	[0.136]	(0.272)	[0.420]
Other Race	0.032	0.036	0.033	0.035
	(0.175)	[0.301]	(0.179)	[0.324]
Age	35.632	35.732	34.465	34.915
	(13.909)	[0.867]	(13.638)	[0.092]
Avg Inc. of Zip Code	70.287	71.594	63.664	63.786
	(36.841)	[0.382]	(33.334)	[0.899]
(B) Case Characteristics				
Municipal	0.007	0.011	0.137	0.145
	(0.086)	[0.487]	(0.343)	[0.351]
Traffic	0.993	0.989	0.520	0.514
	(0.086)	[0.419]	(0.500)	[0.310]
Criminal	0.000	0.000	0.343	0.341
	(0.000)	[0.319]	(0.475)	[0.351]
Can Pay Ahead	0.661	0.663	0.032	0.032
	(0.473)	[0.681]	(0.176)	[0.115]
Num Charges on Case	1.707	1.797	2.586	2.542
	(0.494)	[0.000]	(1.485)	[0.503]
(C) Prior Court Contact				
Prior Case	0.404	0.426	0.548	0.536
	(0.491)	[0.007]	(0.498)	[0.544]
Prior FTA	0.102	0.108	0.273	0.258
	(0.303)	[0.329]	(0.446)	[0.198]
Observations	5092	5797	9517	10464

Notes: This table displays the average characteristics of individuals assigned to the control group and any text intervention group by the type of failure to appear consequence for the case. Standard deviations are shown in parentheses. P-values of the difference between treatment and control arms are shown in square brackets. The differences are estimated with a regression of the characteristic on treatment and controlling for randomization strata.

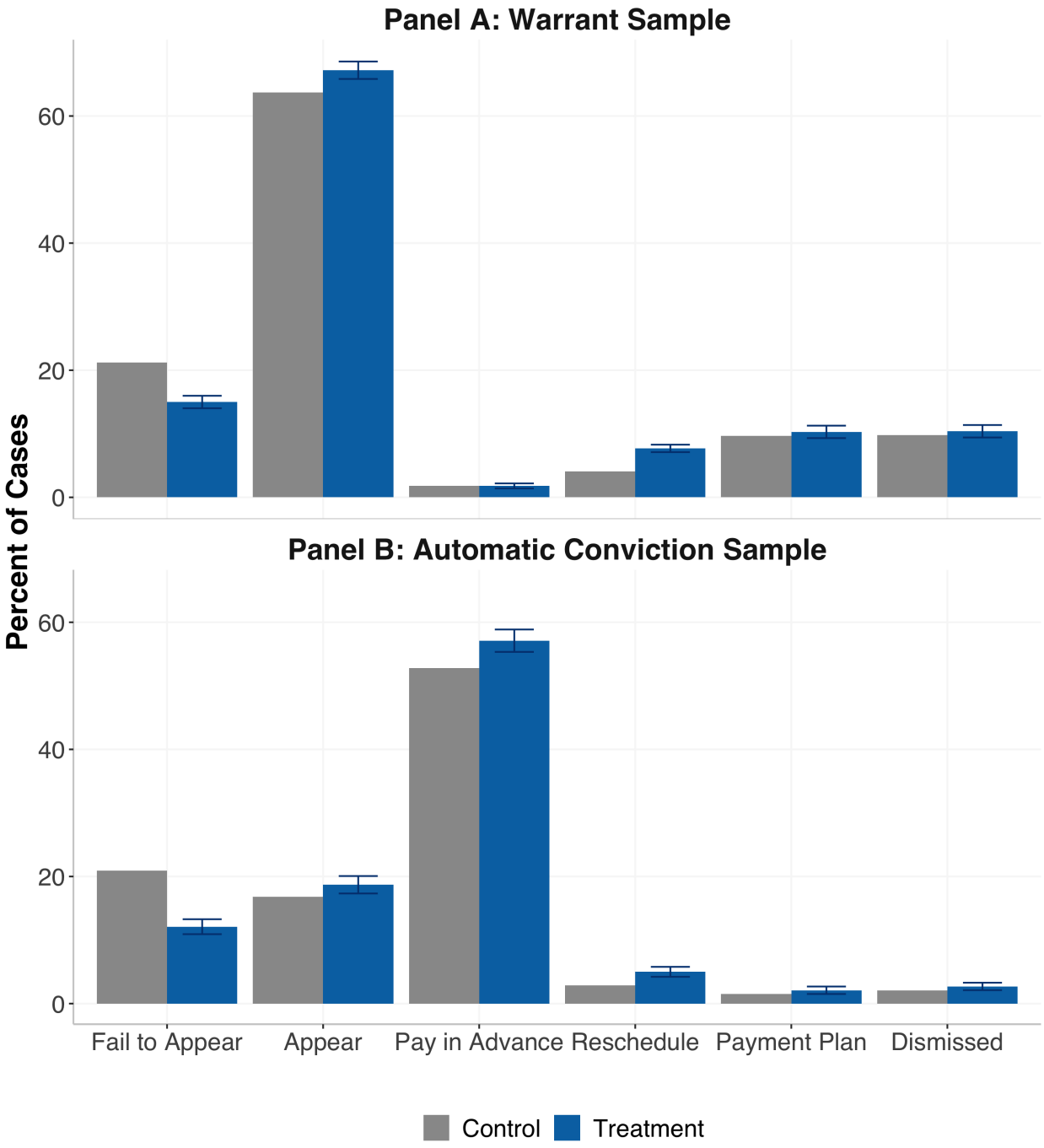

Figure 2: **Effects of any Text Message Treatment on Case Outcomes**

Note: We plot the effects of the treatments relative to the control group on case resolutions. Panel (A) shows how cases resolve for the warrant sample (N=19,981) and Panel (B) shows how cases resolve for the automatic conviction sample (N=10,889). Payment plan and dismissal outcomes are not mutually exclusive with the rest of the outcomes. For example, a person may appear for court and also use a payment plan. Each bar represents an estimate from Equation 1, controlling for the variables shown in Table 1 as well as court time and court date and court building strata. Whiskers show 95 percent confidence intervals.

Table 2: Covariate Means for Compliers and Potential Compliers

	Automatic Conviction		Failure to Appear Warrant	
	Compliers	Potential Compliers	Compliers	Potential Compliers
	(1)	(2)	(3)	(4)
Female	0.452	0.384	0.365	0.303
	(0.039)	[0.96]	(0.052)	[0.917]
White	0.804	0.726	0.644	0.653
	(0.036)	[0.989]	(0.052)	[0.445]
Black	0.118	0.174	0.207	0.268
	(0.029)	[0.024]	(0.045)	[0.077]
Hispanic	0.028	0.063	0.090	0.046
	(0.021)	[0.029]	(0.03)	[0.928]
Other Race	0.034	0.025	0.063	0.032
	(0.015)	[0.704]	(0.02)	[0.95]
Age	33.292	33.703	35.856	34.528
	(1.092)	[0.347]	(1.566)	[0.815]
Avg. Inc. of Zip Code	75.321	71.024	64.69	63.194
	(3.029)	[0.934]	(3.732)	[0.683]
Prior Case	0.461	0.400	0.516	0.663
	(0.041)	[0.936]	(0.056)	[0.004]
Prior FTA	0.137	0.142	0.219	0.445
	(0.025)	[0.446]	(0.048)	[0.000]
Observations	1374	2267	1324	4271

Note: This table reports means for compliers and potential compliers. Potential compliers are individuals in the control group who failed to appear. Bootstrapped standard errors, based on 1000 repetitions, are reported in parentheses. P-values from a t-test of whether the complier mean is greater than that of potential compliers and allowing for unequal variances are reported in brackets.

Table 3: The Reduced Form Impact of Any Text Message Treatment on Criminal Justice Contact

	Fines and Fees Paid (1)	Fines and Fees Charged (2)	Warrant on Case (3)	Jail Booking by 1 Yr (4)	Bonded Out by 1 Yr (5)	New Case by 1 Yr (6)
(A) Failure to Appear Warrant Sample						
Treatment	0.680	1.915	-0.047	0.005	-0.022	-0.001
	(1.869)	(3.799)	(0.006)	(0.008)	(0.007)	(0.004)
Control Mean	117.976	270.774	0.290	0.145	0.172	0.035
Observations	19981	19981	19981	7448	7448	7448
(B) Automatic Conviction Sample						
Treatment	-7.062	-6.403	0.001	-0.000	0.002	0.002
	(1.185)	(1.287)	(0.000)	(0.005)	(0.001)	(0.005)
Control Mean	152.238	171.466	-0.000	0.021	0.001	0.027
Observations	10889	10889	10889	3621	3621	3621

Notes: This table shows the reduced form impact of the interventions on subsequent court contact for our two analysis samples: those in which an FTA results in a warrant and those in which an FTA results in an automatic conviction. Estimates come from Equation 2, controling for the covariates in Table 1, time of day of the hearing, and randomization strata. Standard errors are displayed in parentheses and are robust to heteroskedasticity.

Figure 3: **The Impact of Failure to Appear on Subsequent Court Contact for the Warrant Sample**

Note: This figure shows the effect of a failure to appear on future court contact. The sample is restricted to cases where a failure to appear leads to a bench warrant and that have a full 12 months of follow-up data available to us. The darker bars, labeled "Did Not Fail to Appear," show the compiler mean, namely means for those who were assigned to treatment and for whom treatment prevents a failure to appear. The lighter bars, labeled "Failed to Appear," shows the causal effect estimated using two-stage least squares in which assignment to the interventions is an instrument for failure to appear, where the effect is added to the complier mean. Whiskers show 95 percent confidence intervals constructed using standard errors that are robust to heteroskedasticity. Regressions control for covariates in Table 1, time of day of the hearing, and randomization strata. The numbers underlying this figure can be found in Appendix Table A.10.

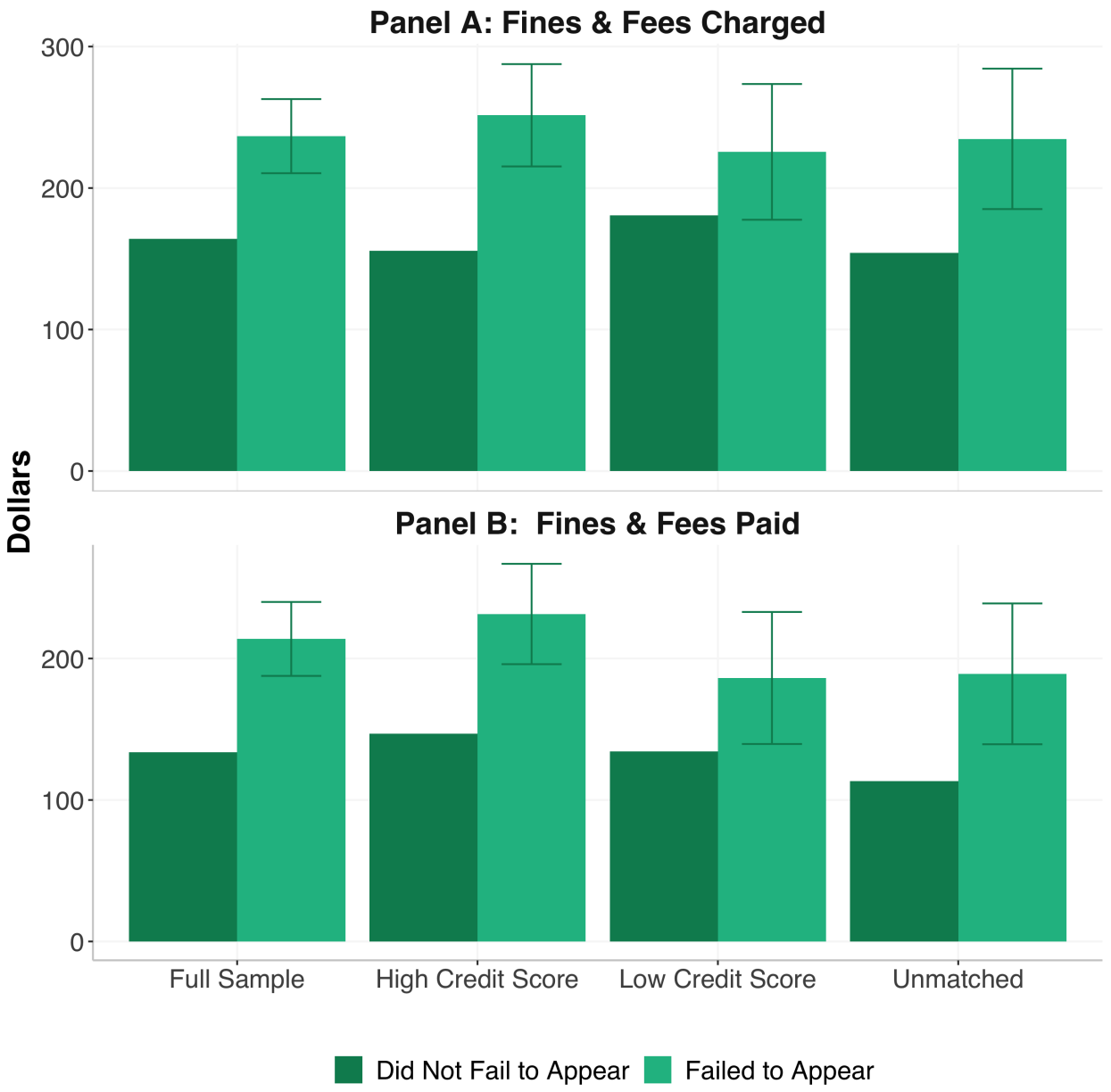

Figure 4: **The Impact of Failure to Appear on Fines and Fees for the Automatic Conviction Sample**

Note: This figure shows the effect of a failure to appear on fine and fees charged and paid on the case. The sample is restricted to cases where a failure to appear leads to an automatic conviction and sentence. The darker bars, labeled "Did Not Fail to Appear," show the compiler mean, namely means for those who were assigned to treatment and for whom treatment prevents a failure to appear. The lighter bar, labeled "Failed to Appear," shows the causal effect estimated from Equation 3, a two-stage least squares approach in which assignment to the interventions is an instrument for failure to appear, where the effect is added to the complier mean. Whiskers show 95 percent confidence intervals constructed using standard errors that are robust to heteroskedasticity. The high credit score sample is defined as those with VantageScores categorized by the credit agency as "Fair" or higher. The low credit score sample is defined as those with VantageScores categorized as "Poor" or lower. Regressions control for covariates in Table 1, time of day of the hearing, and randomization strata. The numbers underlying this figure can be found in Appendix Table A.11.