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Supplementary Materials for

Behavioral nudges reduce failure to appear for court

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Other Supplementary Material for this manuscript includes the following:
(available at science.sciencemag.org/cgi/content/full/science.abb6591/DC1)

MDAR Reproducibility Checklist (.pdf)

Materials and Methods

Summons form redesign: identification strategy

The new summons form was rolled out between March and July of 2016. In March 2016, NYPD stopped printing old forms and printed only new forms. Officers were instructed to use the new forms when they finished their pad of old forms. In order to speed the transition to new forms, in mid-June, the NYPD asked precincts to shred all old forms and to have only new forms in use by July 2016.

For each summons, we obtained the ID of the issuing officer, as well as serial numbers for the summons forms, which allows us to determine whether each form was an old or a new form. We can therefore determine the last date that an officer used an old form, and the first date they used the new form. Police officers only have one pad of forms at a given moment. While we do not observe the exact date that they received their pad of new forms, this occurred before they issued their first new form, and after the last date that they issued an old form or the date the new forms were introduced across NYPD (whichever comes later). We randomly choose the “switch date” to be a date between these two values. Note that we limit our sample to officers who issued at least one old and one new form, and so for whom we can define a “switch date”

We can then determine how many days before or after the switch date each summons was issued. The switch dates were staggered in time. We exploit this staggered roll-out of the new forms in a regression discontinuity design, where summons issuance date relative to the switch date is the running variable, and we can control for time trends. We compare outcomes for summonses issued just before and just after an officer started using the new form. Figure S1 plots the rate of new forms among summons issued, relative to the “switch date”. Mechanically, this rate is 0 before the switch. Once an officer uses a new form, on average, 97.6% are new forms.

We then test whether individuals who received a summons form just before their issuing officer changed to the new form are similar to those just after this change. Typically, this is meant to check that there is no manipulation of the running variable by people who would benefit from them. In our context, it is highly unlikely that potential offenders would be aware of the form changes, but we want to see whether there are changes in *police* behaviors because of the introduction of the new form. If police officers change their ticketing behaviors because of the introduction of the new summons form, differences in failure to appear could be caused by differences in underlying behaviors of defendants, and not by the changes in behaviors induced by the form, which would challenge the validity of our research design.

As a first test for manipulation, we look at the number of forms issued around the switch date. Figure S4 plots the number of forms issued relative to the switch date. This figure shows little evidence of sorting. To formally test this, we conduct a Frandsen test (*36*), which adapts the McCrary test for manipulation in the running variable to situations where the running variable is discrete—which is our case, since the running variable is days relative to the switch date. We cannot reject the null of no difference in number of cases (p=0.886), suggesting no manipulation. We also complement our analyses with a “donut RDD” approach, where we drop observations within 1 day of the switch date.

We find further evidence that there is no manipulation of the running variable as we see no notable difference in observables around the cutoff. If the RDD specification is valid, we would expect baseline outcomes to be smooth at the cutoff. Figure S2 shows that defendants are similar before and after the date at which officers started using new forms in terms of observable characteristics (gender, number of past failures to appear, number of past summonses). Figure S3 shows that, overall, defendants are also similar in terms of offenses. There is a discontinuous change in the number of summonses issued for public urination; but this represents a small share of offenses, and so the changes in overall case composition are small in magnitude. In order to synthesize different observables that may be correlated with failure to appear, we regress failure to appear on observables for 2015 (the year before the new forms were introduced). And, using the regression coefficients, we generate a predicted failure to appear for each defendant. We find that there is no discontinuity in predicted failure to appear before and after the threshold. The absence of changes in observable characteristics is confirmed in Table S2. The only statistically significant difference is for gender: There appear to be slightly fewer women. However, the difference is small. And since failure to appear rates are lower for women, if anything, this would cause a downward bias of the effect of the forms.

We estimate the effect of the new forms on failure to appear computing optimal bandwidths, bias-corrected point estimates using local linear functions, and valid confidence intervals (*11*, *12*). This approach allows us to obtain consistent estimates when we include covariates. In all of our estimates, we include controls for months, since failures to appear vary seasonally. Among defendants who received new forms, those who provided phone numbers (about 10% of the sample) were randomized to one of the text message treatment groups. In order to single out the effect of the new forms on failure to appear, we remove from our analyses people who got a text message. Since people who provided phone numbers are positively selected and we are dropping them from the study, this means that our estimates will be underestimates of the effects of the new forms.

Column 1 of Table S3 presents our main regression results. In the remainder of Table S3, we present robustness checks. In our main specifications, we include covariates that are correlated with failure to appear, since these could reduce the variance of the outcome and increase the precision of our estimates. The second and third columns remove controls for defendant observables and borough fixed effects, respectively. The point estimates do not change significantly, nor does the precision of our estimates, suggesting that in our setting including covariates did not substantially reduce the variance of our outcome of interest. In column 4, we remove summonses that were issued the day just before or just after the switch date (“donut RDD”). The point estimates are similar across specifications.

Lastly, Fig. 2 shows an upward trend in failure to appear before the switch date. This is most likely due to seasonality. Indeed, failure to appear rates increase in the spring and peak in the summer, when most of the new forms were introduced. To show this, we used data from 2015 to generate “placebo” switch dates. That is, we randomly assign a switch date to officers in 2015, which would have the same temporal distribution in terms of number of officers switching each day as in 2016. In Fig. S5, we see again the upward trend before the “switch date.” However, there is no change in trend after the placebo switch date (because there was no change in forms). That is, seasonality produces the upward trend observed in Fig. 2, but the drop in failures to appear is due to the introduction of the redesigned forms.

Text message reminders: Randomization strategy

Our text message randomized controlled trial is run among summons recipients who provided a phone number, which is non-mandatory. If summons recipients inquired about the reason for providing a phone number, officers were instructed to only tell them that they “might receive a text message.” Among defendants who provided phone numbers, those whose summonses passed the standard pre-hearing judicial reviews on technical and legal grounds were randomized into the treatment arms described in the paper (and reproduced below) by the Office of Courts Administration within their data system using a randomization procedure we have reviewed.

Table S4 presents the balance checks, comparing baseline means for individuals randomized into the control group, relative to any treatment group. The randomization was successful, and groups are similar on observables across treatment arms. We can thus determine the effect of the different text messages with the following OLS regressions:

$$FTA_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + \varepsilon_i$$

Where T_i captures treatment status (pooled treatment, or assignment to particular treatment arms) of individual i; and X_i is a matrix of individual covariates (gender, age, offense, dummies for past failure to appear and for past summons, and borough where the summons was issued). The main coefficient of interest is β_1.

Lab experiment 1 materials

The wording of the background information was: *Each year in New York City, police officers issue hundreds of thousands of tickets called "summonses" for a variety of offenses, such as carrying an open container of alcohol in public, public urination, disorderly conduct, park trespassing, smoking marijuana in public, and others. Most of these offenses require defendants to appear in court 60 to 90 days after receiving the summons form.*

Lab experiment 2 materials

The wording for the opening vignette was: *On September 1st, 2017, you were waiting in line to swipe your Metrocard to enter the subway when a man pushes in front of you and cuts you in line. You said, "Excuse me!" and he yelled back at you. You both began arguing loudly, and suddenly he shoved you, and you shoved him back. Within seconds, a police officer appeared and issued each of you a ticket (a.k.a. a summons) for Disorderly Conduct. Below is your copy of the ticket. Read over the ticket, and respond to the following questions.*

The questions that participants answered immediately after viewing their summons form are shown below:

Indicate how strongly the ticket (a.k.a. summons) makes you feel each of the following.

	Strongly Disagree	*Disagree*	*Neutral*	*Agree*	*Strongly Agree*
Angry	○	○	○	○	○
Confused	○	○	○	○	○

Indicate how strongly you agree with the following statements about the ticket.

	Strongly Disagree	*Disagree*	*Neutral*	*Agree*	*Strongly Agree*
The ticket is fair.	○	○	○	○	○
The ticket is reasonable.	○	○	○	○	○

The questions participants answered after the filler task are shown below:

Recall date: *When do you need to go to court?*

- *September 1st, 2017*
- *November 1st, 2017*
- *September 30th, 2017*
- *December 1st, 2017*
- *I don't know.*

Recall place: *Where do you have to go?*

- *Bronx Criminal Court*
- *Redhook Community Justice Center*
- *New York Criminal Court*
- *Kings Criminal Court*
- *Queens Criminal Court*
- *Midtown Community Center*
- *New York State Ticket Court*

Likelihood of consequences: *In your opinion, how likely do you think each outcome is to happen to you if you were to miss your court date altogether?*

	Highly Unlikely	*Somewhat Unlikely*	*Neutral*	*Somewhat Likely*	*Highly Likely*
The ticket will be dismissed.	○	○	○	○	○
I will be fined.	○	○	○	○	○
A warrant will be issued for my arrest.	○	○	○	○	○
Nothing; no one will notice	○	○	○	○	○
I will get something in the mail.	○	○	○	○	○

Offense*: What are you being charged with?*

- *Biking on a sidewalk*
- *Disorderly conduct*
- *Open container of alcohol*
- *Littering*
- *Speeding*

Getting more information: *What can you do to find more information and ask questions?*

- *Visit www.mysummons.nyc*
- *Call 646-760-3010*
- *Both the first and second options*
- *Call the precinct that issued the ticket.*
- *None of the above.*

Lab experiment 3 materials

The scenarios are shown below:

Showing up to court:

Imagine that someone is required to go to court in 60 days because they were issued a ticket for carrying an open container of alcohol. They are required to go during business hours and in total it could take a few hours to deal with (including travel time). This person does not want to go, but if they fail to show up then they face a penalty.

In fact, this person ultimately does not show up for their appointment.

Paying an overdue bill:

Imagine that someone is required to go to an office in 60 days to pay an overdue bill to a collections agency. They are required to go during business hours and in total it could take a few hours to deal with (including travel time). This person does not want to go, but if they fail to show up then they face a penalty.

In fact, this person ultimately does not show up for their appointment.

Showing up for a doctor's appointment:

Imagine that someone is required to go to a doctor's appointment in 60 days for a mandatory exam and some tests. They are required to go during business hours and in total it could take a few hours to deal with (including travel time). This person does not want to go, but if they fail to show up then they face a penalty.

In fact, this person ultimately does not show up for their appointment.

Paperwork for an educational program:

Imagine that someone is required to go to an office in 60 days to submit some paperwork for enrollment in a mandatory educational program. They are required to go during business hours and in total it could take a few hours to deal with (including travel time). This person does not want to go, but if they fail to show up then they face a penalty.

In fact, this person ultimately does not show up for their appointment.

Vehicle emissions test:

Imagine that someone is required to get their car's emissions tested in 60 days because they are overdue for an inspection. They are required to go during business hours and in total it could take a few hours to deal with (including travel time). This person does not want to go, but if they fail to show up then they face a penalty.

In fact, this person ultimately does not show up for their appointment.

The questions are shown below:

How likely do you think it is that this person missed their appointment because they did not pay enough attention to the scheduled date or simply forgot? (1: very unlikely to 7: very likely)

How likely do you think it is that this person intentionally and deliberately decided to skip their appointment? (1: very unlikely to 7: very likely)

What do you think should be done to make sure that other people show up for their appointments? Pick one option from the choices below.

Increase the penalty for failing to show up
Send reminders to people about their appointments
Make sure that appointment dates are easy to notice on any paperwork

Lab experiment 4 materials

The wording of the background information was: *Each year in New York City, police officers issue hundreds of thousands of tickets called "summonses" for a variety of offenses, such as carrying an open container of alcohol in public, public urination, disorderly conduct, park trespassing, smoking marijuana in public, and others. Most of these offenses require defendants to appear in court 60 to 90 days after receiving the summons form. If defendants do not show up to court, then they are held in contempt of court, and a warrant is issued for their arrest. Defendants are warned about this consequence when they receive the summons form.*

In fact, every year, many people who receive summonses do not show up for their scheduled court appearance. For example, one study found that approximately 40% of people do not show up for their court date.

Lab experiment 5 materials and methods

Our recruitment script was:

Dear [ ],

Given your demanding schedules, we hope this will be an easy and worthwhile 5-10 minutes of your day. We are researchers at the University of Chicago, University of Pennsylvania, and ideas42. We have recently been evaluating policies to reduce failures to appear in court. As part of this work, we are conducting quick surveys with a small number of legal experts, like yourself. We would tremendously value your input if you are willing to participate in a short, 5-10 minute online survey. Your responses will be anonymous, and this study has been approved by the University of Chicago's Institutional Review Board. The link for the survey is below:

[LINK]

If you have any questions, please feel free to email us at our addresses below […]

We received 145 complete responses (with an additional 49 partial responses). Our analyses only focus on complete responses.

The wording of the questions in Part 2 was:

Do you think people who receive Form 1 or Form 2 will be more likely to remember the date of their scheduled court appearance?

Do you think people who receive Form 1 or Form 2 will be more likely to remember the location of their scheduled court appearance?

Do you think people who receive Form 1 or Form 2 will be more likely to show up for their scheduled court appearance?

For each question, there were three response options:

People who receive Form 1 will be more likely to…

People who receive Form 2 will be more likely to…

People who receive either form will be equally likely to…

A note on attrition in the sample: Overall, 75% of people who started the survey completed it. Attrition differed across treatment arms, with 81% of people randomized in the control group completing it, compared to 74% and 69% in the "mistake" and "intentional" conditions. This difference is likely driven by discouragement at the prospect of having to enter free text, and not by views on the reason for failing to appear in court, or perceptions of the court; so, it likely does not affect our findings.

Supplementary Text

Summonses in New York City

In New York City, summonses are given for the lowest level of offenses. Summonses are defined in Article 130 of the NY Criminal Procedure of Law. Technically, a criminal summons is a call to appear before a local court at a future date in connection to a minor penal law violation or a violation of the NYC Administrative Code. In New York City, summonses are a citywide process and can be issued to any violator within the five boroughs. Receiving a summons begins a process of filing a complaint with the Criminal Court which is followed by the arraignment of the defendant. At the time of arraignment, the judge assigned to the case determines the punishment or dismisses the case. Punishments can include fines, community service, or jail time.

More than 95% of summonses are issued to a defendant by an NYPD officer, but various municipal law enforcement officers, including individuals working for the Metropolitan Transportation Authority and the New York City Fire Department, can also issue summonses in connection with their job.

At the time of a summonses receipt, defendants are usually not arrested, detained or fingerprinted. The officer provides the defendant with a summons form that includes information detailing the offense as well as the date and location of their future court appearance. The next step is for a defendant to show up in court. As opposed to higher-level misdemeanors or felonies, people who receive summonses never have to post bail to avoid pre-trial detention.

Typically, actions that lead to a criminal summons can be thought of as "quality of life" offenses. For example, in our sample, 34% of summonses are for open container offenses, 10% are for park trespassing, and 8% are for public urination (Table S1). People can also get summonses for disorderly conduct (which includes engaging in fighting or in violent, tumultuous or threatening behavior). Marijuana possession of up to two ounces was formerly an arrestable offense, but now leads to a summons issuance. Any violation of the thousands of statutes within the NYC Administrative Code can lead to the issuance of a summons. Many urban areas have their own version of summonses. In recent years, there has been a decline in the number of summonses issued, but they are still very frequent: In 2015, 327,306 summonses were issued, and 256,488 were scheduled to be heard in court (*37*). Note that not all cases lead to a court date. They first have to pass a standard pre-hearing court review on technical and legal grounds. This can result in cases being thrown out on technical (incomplete form) or legal (situation described does not warrant the charge) grounds. There are about as many summonses issued in New York as there are arrests.

Data Sources

The key data source for our policy experiments is the New York State Office of Court Administration (OCA), which collects data on all summonses issued within New York City. The dataset includes information on all cases that passed legal review and for which a court date was scheduled. The data contains demographic information (gender, date of birth); information about the violation (type of offense, date of issuance, borough, precinct, arresting agency); and court outcomes (initial court date, whether a recipient appeared in court at the initial court date, and whether that recipient appeared in court at a later date). In our main sample, we obtained this data from January 2012 to September 12th, 2017. The court date for a summons is no later than 90 days after issuance, so by limiting our sample to summonses issued before June 14th, 2017, we have court-related outcomes for our full sample.

Table S1 presents summary statistics for our main period of study (January 2016 to June 2017). Summons recipients are 88% male and an average of 34.3 years old. About one in three summons recipients had already received a prior summons. The failure to appear rate before 2016 (the year that our interventions started) was 41%.

Discussion of external validity

In this section, we explore two questions of external validity. Would summons recipients who did not provide a phone number also benefit from text messages? And would defendants for more serious offenses benefit?

Only about 11% of summons recipients provided a phone number. Table S1 presents descriptive statistics on summons recipients overall, and on summons recipients who provided a phone number. Relative to people who did not provide a phone number, defendants who did are younger on average (32 years old vs. 34.5 years old), less likely to have gotten a summons for alcohol and more likely to have gotten a summons for marijuana, and had fewer past summonses and failures to appear. There appears to be positive selection into providing a phone number: The failure to appear rate of people who provided a phone number and were randomized to the control group is 37%, relative to 41% for defendants who did not provide a phone number. Still, failures to appear are clearly prevalent in both groups.

We use several strategies to estimate whether our results would generalize to other summons recipients as well (presented in Table S9). The first column presents our main estimates, and we compare this to estimates on various subsamples. First, we attempt to create a sample that is more similar to people who did not provide phone numbers using propensity score matching. We use nearest neighbor matching, matching on not providing a cell phone. We use the following covariates for matching: age, gender, offense, borough, racial composition of the census tract that a defendant lives in, and percent living under poverty in that census tract. We then re-run our main analyses weighing our OLS regression with the frequency with which an observation from a defendant who provided a cell phone number is used as a match. With this approach, the sample of people who provided phone numbers is closer, based on observables, to the sample of people who did not provide phone numbers. For example, in the original sample, the average age of people who provided a phone number is 32 years old compared to 34.3 years old for those who did not. In the weighted sample, the average age of people who provided a phone number is also 34.3 years old. Likewise, in the original sample, the average percent of Black residents in the census tract is 35% for people who did not provide a cell phone, and 30% for people who did. In the weighted sample, both are 35%. Results are presented in column 2 of Table S9. The point estimates are virtually unchanged in this weighted regression. There are limits to this analysis, since we can only match defendants based on observable characteristics. It is possible that people who did not provide phone numbers also differ based on unobservable characteristics, which could be correlated with responsiveness to text messages.

We use a second strategy to determine whether text messages would have similar effects for other summons recipients. While the analyses presented in this paper focus on 2016-June 2017 (as specified in our pre-analysis plan), defendants were still randomized until August of 2018, when all people who provided phone numbers received text messages. During that time period, 20% of defendants provided phone numbers – compared to 11% in the main sample of our study. In column 3, we look at the effect of receiving a text messages for this later period. We find that, if anything, the reduction in failures to appear is larger for this group: Defendants who received a text message have a 9.2 percentage point, or 25.5%, lower failure to appear rate (compared to an 8 percentage point, or 21%, reduction in failures to appear in our main sample). This suggests that as the phone number provision increases, the effectiveness does not decrease. Of course, there may be other differences in this time period. For example, fewer people were issued summonses, so perhaps the pool of defendants is not quite the same. But this again suggests that the people in our study were not unique in their receptiveness to text messages.

A second question regarding external validity pertains to whether non-summons recipients would also benefit from text messages. This is a more difficult question to answer, since we do not observe any misdemeanor or felony defendants in our study. However, when we look at more serious offenses, such as disorderly conduct (which includes fighting or engaging in violent, tumultuous or threatening behavior), we find similar estimates (column 4). Lastly, we look at marijuana offenses, which in many places are still misdemeanor offenses. Here again, we replicate our main results. These results suggest that, at least within our sample of summonsable offenses, the treatment effects are similar for more serious offenses. Note also that Emanuel and Ho (2020) find similar results in a study that looks at the effects of text messages for defendants who have to appear in traffic, criminal misdemeanor and municipal courts, suggesting that our results replicate in our contexts as well.

Cost-benefit calculation for the field studies

In order to calculate the costs and benefits of the intervention, we must determine how many summonses were issued and an estimate of how many warrants were avoided. We must also determine the costs of issuing warrants and of arrests. Before moving to the exact calculations, we should note that the estimation of costs is also challenging. There are no available estimates for the cost of arrests or the costs of issuing a warrant. So, we use estimates from the literature or, when relevant, we adapt previously used methods to the current context.

Avoided warrants. We use NYC Open Data to calculate the number of summonses issued between August 2016 and November 2019.[1] Summonses were issued less often, but there were still approximately 426,000 scheduled arraignments during this time period. Assuming a 41% failure to appear rate (the pre-intervention rate), this would result in 174,660 warrants. Assuming the new form reduces failures to appear by 13.2%, the new form would prevent 23,055 warrants. Assuming combination text reminders reduce failures to appear by 26% and 20% of defendants give their phone number (which is the rate of phone provision in 2018), texting would prevent another 7,883 warrants. Emanuel and Ho (2020) estimates that issuing each warrant costs $21 in judge and staff time. With 30,938 warrants avoided, just in court time, the intervention saved approximately $650,000 between August 2016 and December 2019.

Cost of arrests. To estimate the cost of each arrest, we estimate the time that it takes to process a case for police officers and pretrial officers, following the methodology of Fain et al (*24*). The average time between arrest and arraignment is 19 hours (*38*). Following Fain et al (*24*), we assume that an arrest actively takes 8 hours of police time, 1 hour of a district attorney's time, and 2 hours of a pretrial officer's time (the remaining time does not entail active personnel costs). To get estimates of salaries for police officers and assistant district attorneys, we use NYC public data.[2] The average wage between 2016 and 2019 for police officers was $42/hour; for district attorneys $60/hour. For pretrial officers, we use estimates from Ziprecruiter of $29/hour.[3] These estimates mean that each arrest would cost $454.

[1] https://data.cityofnewyork.us/Public-Safety/NYPD-Criminal-Court-Summons-Historic-/sv2w-rv3k
[2] https://data.cityofnewyork.us/City-Government/Citywide-Payroll-Data-Fiscal-Year-/k397-673e
[3] https://www.ziprecruiter.com/Salaries/How-Much-Does-a-Pretrial-Services-Officer-Make-an-Hour-in-New-York-City,NY

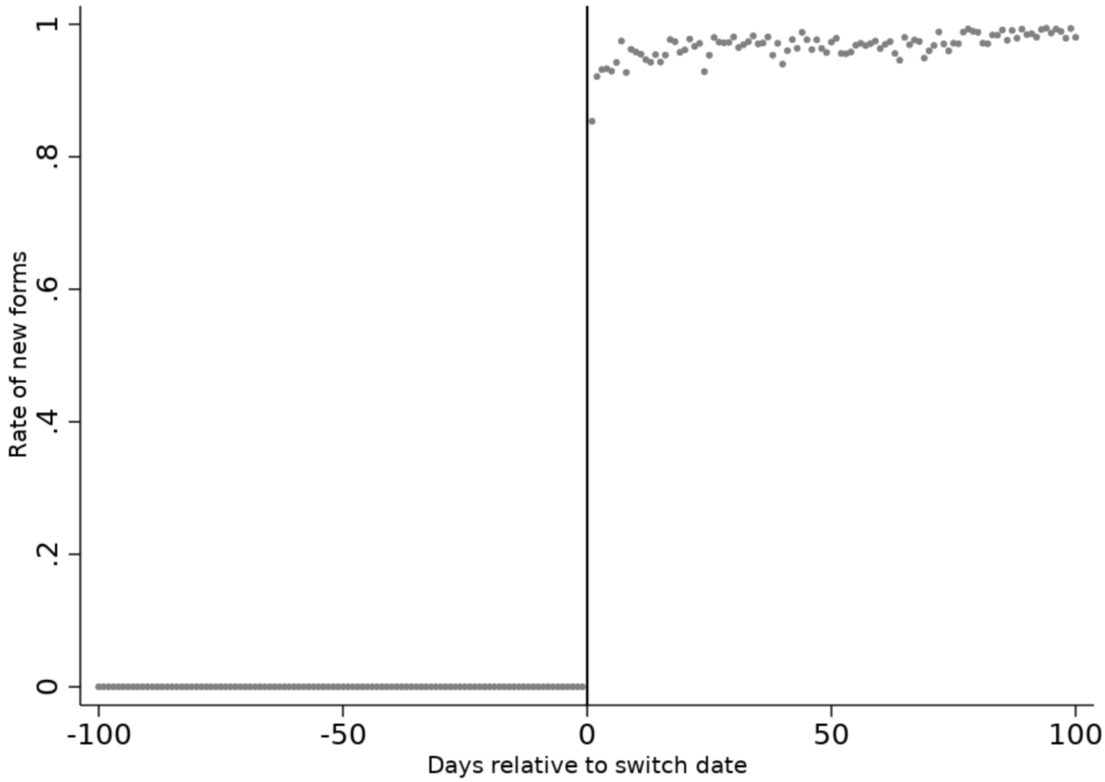
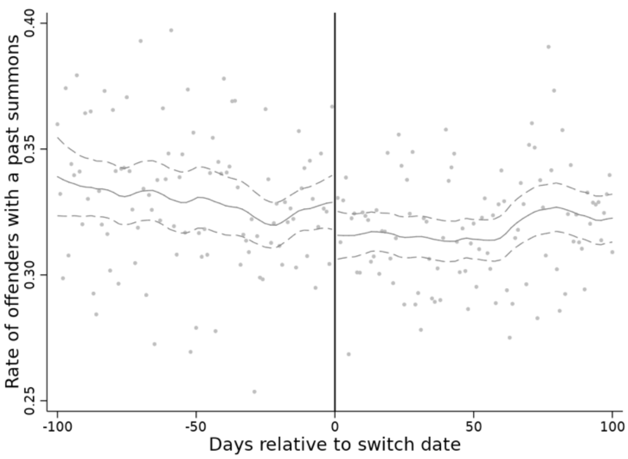
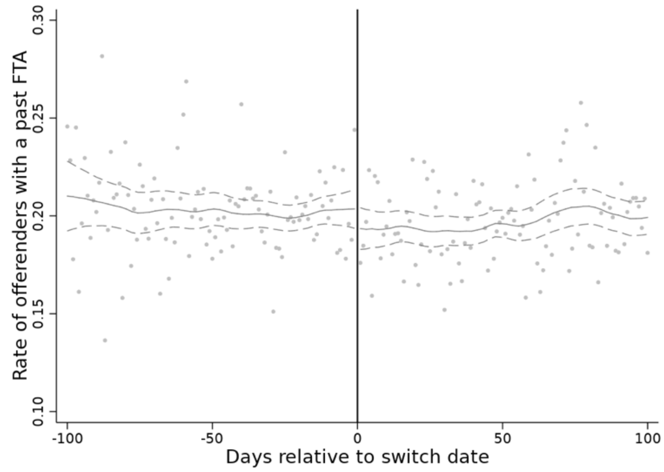


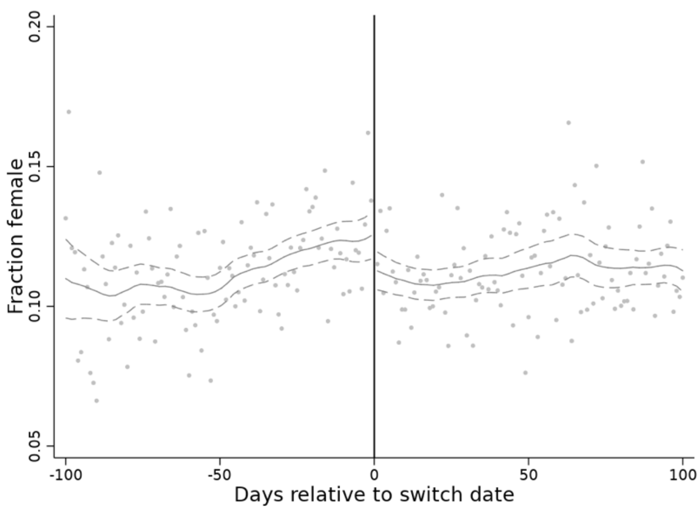
Fig. S1.
Use of old and new summons, relative to the switch date. On the X-axis is the day that a summons was issued, relative to the switch date (a randomly chosen date between when the officer issues their last old form and their first new form). On the Y-axis is the percent of new forms issued for each day. *Data source: New York Office of Court Administration.*



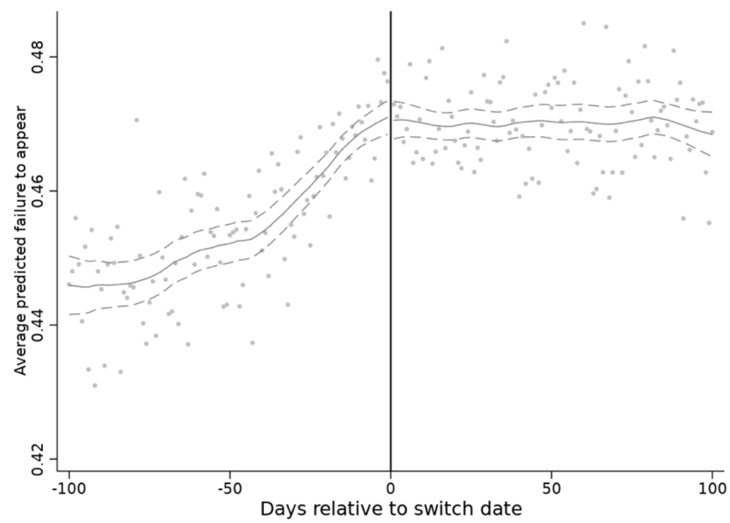
(a) Offenders with a past summons



(b) Offenders with a past failure to appear



(c) Female offenders



(d) Predicted failure to appear

Fig. S2

Observable baseline defendant characteristics, relative to the switch date of the issuing officer. The graph includes scatter plots of daily averages of the baseline variables of interest, and local-polynomial regression lines, fitted separately before and after the switch date. In panel d, “predicted failure to appear” is computed by running a linear regression of failure to appear on observables (offense, age, gender, past failure to appear, past summonses, day of the week), and using the regression coefficients to compute the predicted failure to appear based on observables. The dashed lines represent the 95% confidence interval. *Data source: New York Office of Court Administration*

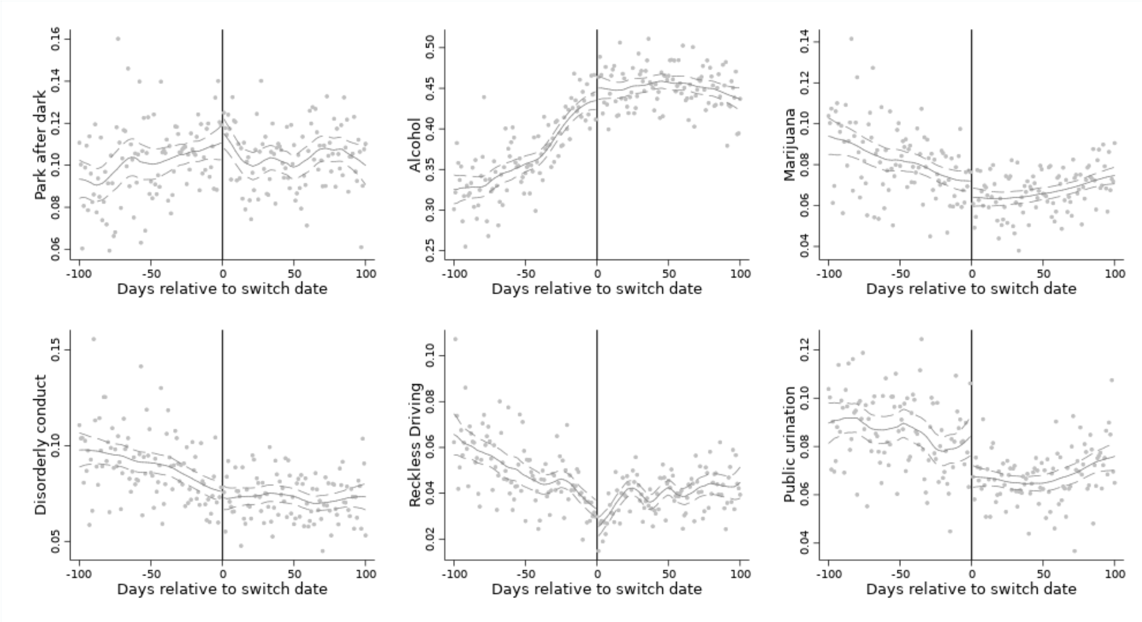


Fig. S3

Offenses, before and after the switch date. Graphs include scatter plots of daily averages of the baseline variables of interest and local-polynomial regression lines. The dashed lines represent the 95% confidence interval. *Data source: New York Office of Court Administration.*

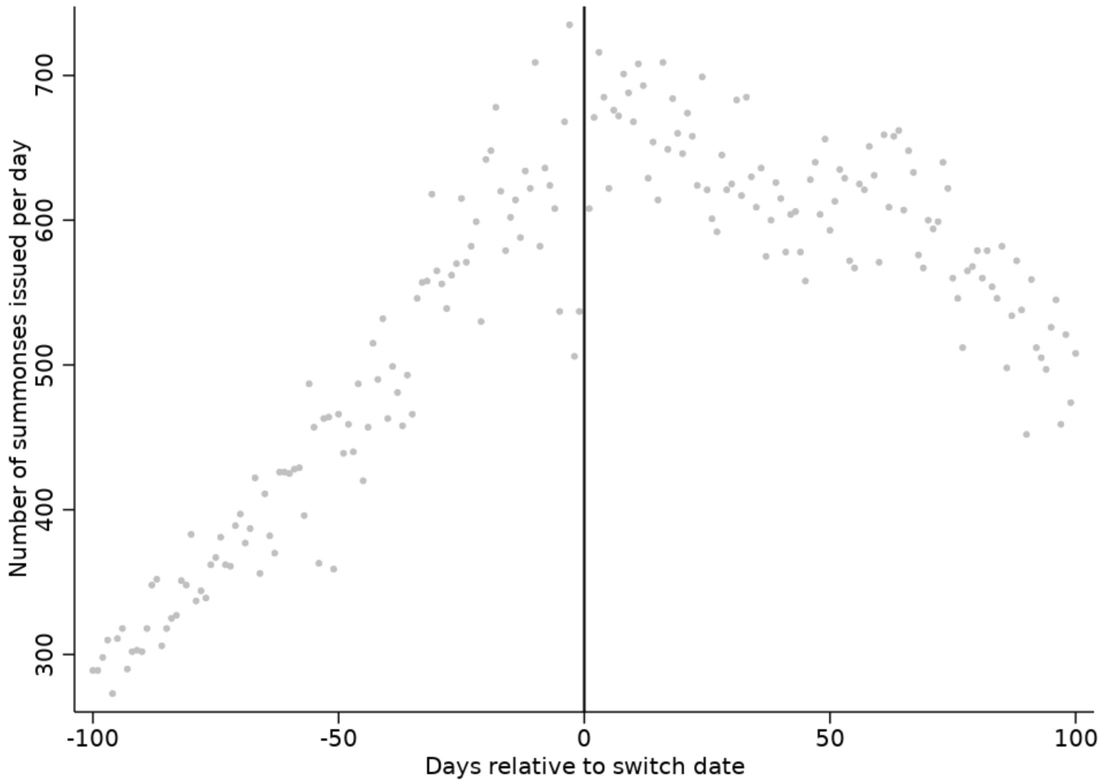


Fig. S4

Number of forms issued, relative to the switch date of the issuing officer. The graph plots daily averages of the number of forms issued. *Data source: New York Office of Court Administration.*

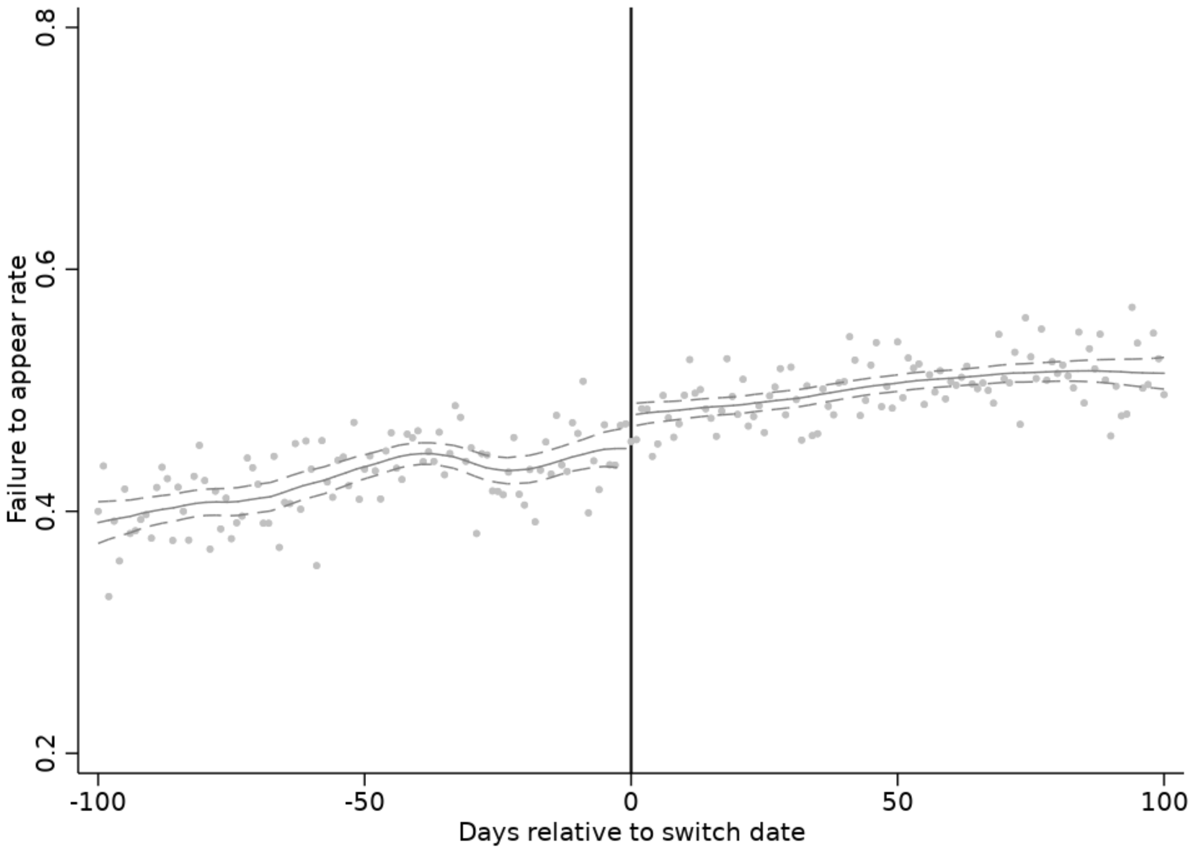


Fig. S5

Failure to appear rate, relative to the “placebo switch date” of the issuing officer. The graph includes scatter plots of daily averages of failure to appear, and local-polynomial regression lines, fitted separately before and after the switch date. The dashed lines represent the 95% confidence interval. *Data source: New York Office of Court Administration.*

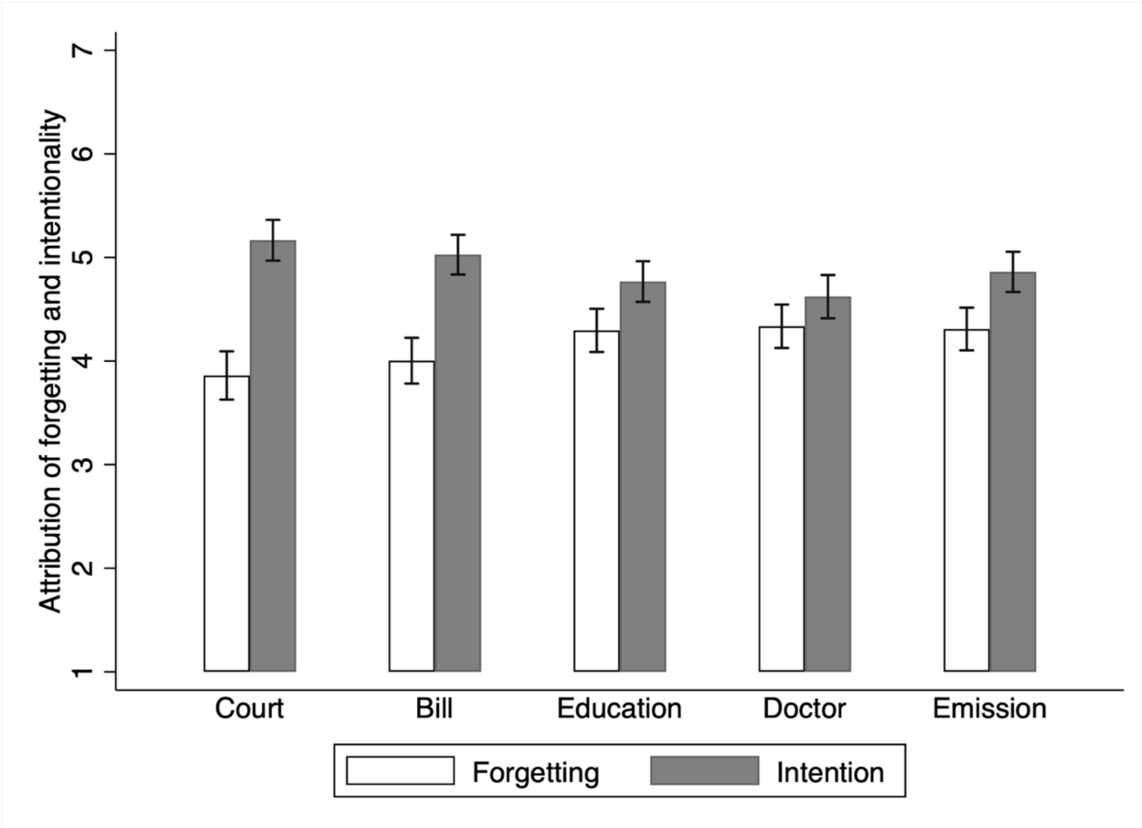


Fig. S6

Participants' attributions of forgetting and intentionality across five scenarios from lab experiment 3. Errors bars represent 95% confidence intervals.

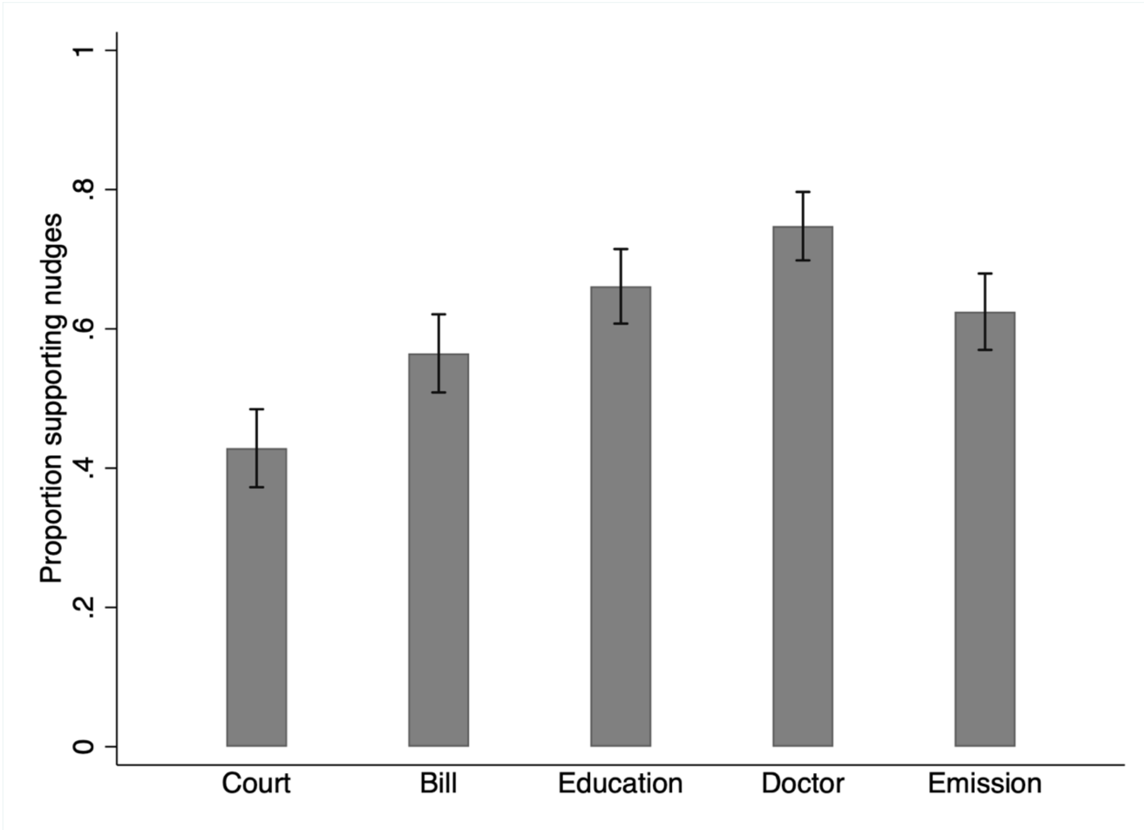


Fig. S7

Proportion of participants supporting nudges to reduce failures across five scenarios from lab experiment 3. Errors bars represent 95% confidence intervals.

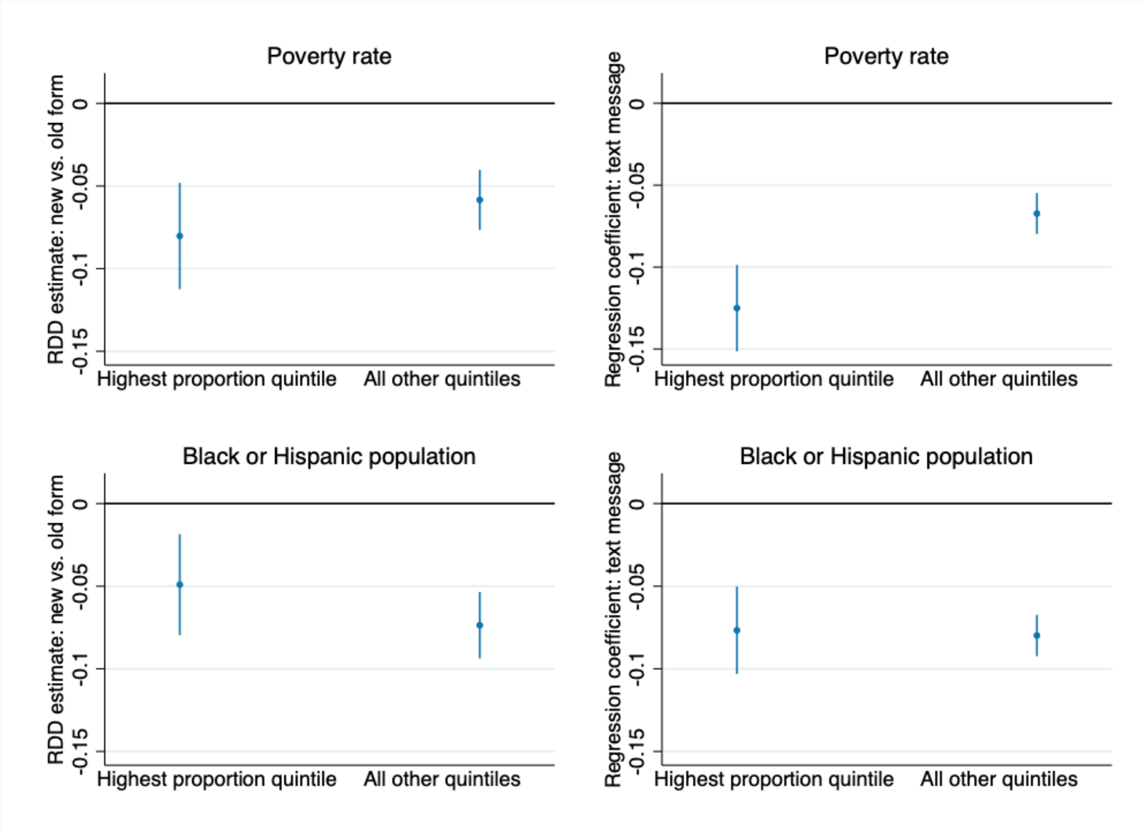
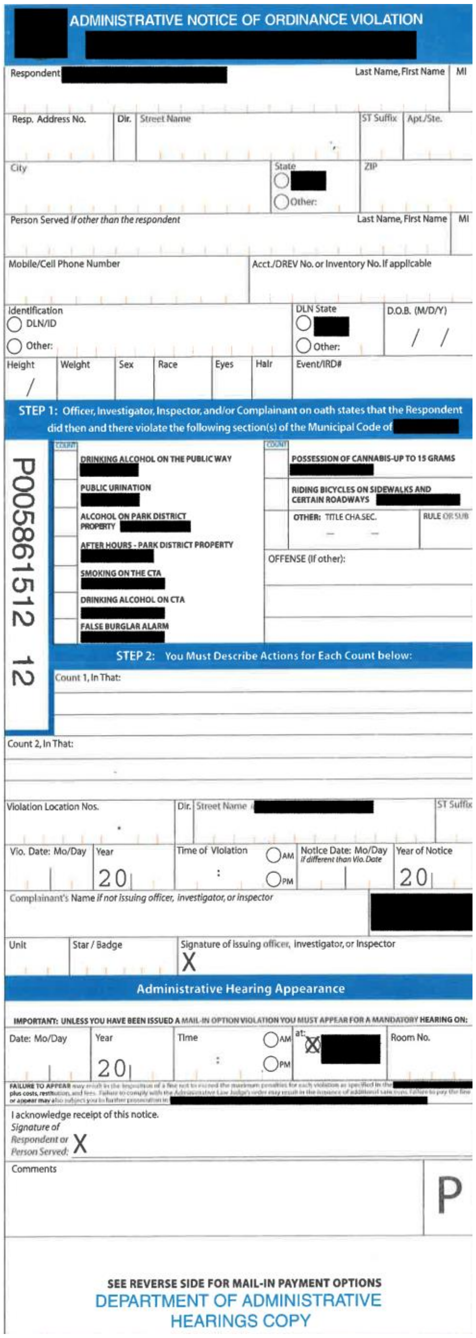


Fig. S8

Top left: Heterogeneity in treatment effect for the form redesign study by poverty rate in the census tract.
Top right: Heterogeneity in treatment effect for the text message study by poverty rate in the census tract.
Bottom left: Heterogeneity in treatment effect for the form redesign study by proportion of Black and Hispanic residents in the census tract.
Bottom right: Heterogeneity in treatment effect for the text message study by proportion of Black and Hispanic residents in the census tract.


[redacted] ADMINISTRATIVE NOTICE OF ORDINANCE VIOLATION
[redacted]

Respondent [redacted] Last Name, First Name MI

Resp. Address No. | Dir. | Street Name | ST Suffix | Apt./Ste.

City | State [redacted] Other: | ZIP

Person Served *if other than the respondent* Last Name, First Name MI

Mobile/Cell Phone Number | Acct./DREV No. or Inventory No. if applicable

Identification DLN/ID Other: | DLN State [redacted] Other: | D.O.B. (M/D/Y) / /

Height / | Weight | Sex | Race | Eyes | Hair | Event/IRD#

STEP 1: Officer, Investigator, Inspector, and/or Complainant on oath states that the Respondent did then and there violate the following section(s) of the Municipal Code of [redacted]

P005861512 12

COUNT | DRINKING ALCOHOL ON THE PUBLIC WAY [redacted]
PUBLIC URINATION [redacted]
ALCOHOL ON PARK DISTRICT PROPERTY [redacted]
AFTER HOURS - PARK DISTRICT PROPERTY [redacted]
SMOKING ON THE CTA [redacted]
DRINKING ALCOHOL ON CTA [redacted]
FALSE BURGLAR ALARM [redacted]

COUNT | POSSESSION OF CANNABIS-UP TO 15 GRAMS [redacted]
RIDING BICYCLES ON SIDEWALKS AND CERTAIN ROADWAYS [redacted]
OTHER: TITLE CHA.SEC. | RULE OR SUB
OFFENSE (If other):

STEP 2: You Must Describe Actions for Each Count below:

Count 1, In That:

Count 2, In That:

Violation Location Nos. | Dir. | Street Name [redacted] | ST Suffix

Vio. Date: Mo/Day | Year 20 | Time of Violation : AM PM | Notice Date: Mo/Day *If different than Vio. Date* | Year of Notice 20

Complainant's Name *if not issuing officer, investigator, or inspector* [redacted]

Unit | Star / Badge | Signature of issuing officer, investigator, or inspector X

Administrative Hearing Appearance

IMPORTANT: UNLESS YOU HAVE BEEN ISSUED A MAIL-IN OPTION VIOLATION YOU MUST APPEAR FOR A MANDATORY HEARING ON:

Date: Mo/Day | Year 20 | Time : AM PM | at: [redacted] | Room No.

FAILURE TO APPEAR [redacted]

I acknowledge receipt of this notice.
Signature of Respondent or Person Served: X

Comments | P

SEE REVERSE SIDE FOR MAIL-IN PAYMENT OPTIONS
DEPARTMENT OF ADMINISTRATIVE HEARINGS COPY

Fig. S9

The equivalent of a summons form from another major U.S. city. Many of the features from the old New York City summons form are present in this form as well, such as placing court information below all other information. Note: Some identifying information about the city has been masked.

Table S1.
Summary statistics on summons recipients for summonses issued in New York City between January 1st, 2016 and June 14th, 2017. The "defendant zip code" characteristics are statistics taken from the 2015 American Community Survey for the zip code listed on the defendant's home address. Median income is in 2018 dollars. "Predicted failure to appear" is computed by running a linear regression of failure to appear on observables (offense, age, gender, past failure to appear, past summonses, day of the week). *Data source for all other outcomes: New York Office of Court Administration.*

		Mean of each variable				
		Overall	New summonses	New summonses without phone numbers	New summonses with phone numbers	Part of the text message experiment
	Age	34.31	34.25	34.52	32.05	32.00
	Female	0.12	0.12	0.11	0.14	0.14
Offense	Park Trespassing	0.10	0.10	0.10	0.12	0.13
	Alcohol	0.34	0.37	0.37	0.32	0.32
	Marijuana	0.09	0.09	0.09	0.15	0.15
	Disorderly conduct	0.08	0.08	0.08	0.08	0.08
	Motor vehicle	0.06	0.06	0.06	0.04	0.04
	Public Urination	0.08	0.08	0.08	0.08	0.08
Borough	Bronx	0.24	0.24	0.24	0.25	0.25
	Brooklyn	0.29	0.30	0.30	0.30	0.30
	Manhattan	0.20	0.19	0.19	0.22	0.22
	Queens	0.22	0.22	0.23	0.19	0.19
	Staten Island	0.05	0.05	0.05	0.04	0.04
Defendant zip code	White	0.35	0.35	0.35	0.37	0.37
	Black	0.33	0.33	0.33	0.30	0.30
	Hispanic	0.34	0.34	0.34	0.34	0.34
	Median income	49,328	49,229	49,219	49,305	49,307
	Below poverty	0.21	0.21	0.21	0.21	0.21
	Nb. past summonses	1.32	1.30	1.36	0.85	0.85
	Nb. past failures to appear	0.57	0.56	0.59	0.38	0.38
	Any past summons	0.32	0.32	0.33	0.30	0.30
	Any past failure to appear	0.19	0.19	0.19	0.17	0.17
	Predicted failure to appear	0.45	0.45	0.45	0.45	0.44
	Sample size	323,922	211,791	188,548	23,243	20,234

Table S2.
Comparing observable characteristics of recipients of old and new summonses. Note: Each column represents a separate regression. We calculate bandwidths following (*11*) for failure to appear as the outcome variable. *Data source: New York Office of Court Administration.*

Outcome:	Past Summons	Past failure to appear	Female	Age	Predicted failure to appear
	(1)	(2)	(3)	(4)	(5)
After cutoff date	-0.010	-0.010	-0.015***	-0.388	-0.003
	(0.008)	(0.006)	(0.005)	(0.252)	(0.002)
Average value for the old forms	0.33	0.20	0.11	34.12	0.46
Effective RD Observations	64,792	84,747	72,585	48,652	53,207
Bandwidth for estimation	54	71	60	39	44
Bandwidth for bias	86	112	101	77	75
Standard errors in parentheses					
*** p<0.01, ** p<0.05, * p<0.1					

Table S3.

Regression discontinuity design estimates of the effect of new forms on failures to appear in court. Note: Each column represents a separate regression, the outcome being a dummy equal to one if a defendant failed to appear in court on their scheduled court date. Column 1 includes controls for gender, age, and type of offense. In column 4, we drop observations on the day before or after the switch date. We compute bandwidths and estimates following (*11*). *Data source: New York Office of Court Administration*

	Outcome = Failure to Appear to Court on time			
	Full sample			Donut (drop 1 day pre / post)
	(1)	(2)	(3)	(4)
RDD estimate	-0.062***	-0.066***	-0.067***	-0.062***
	(0.010)	(0.010)	(0.011)	(0.011)
Failure to appear rate for the old forms in the estimation bandwidth	0.47			
Effective RD Observations	49,757	45,650	44,482	45,575
Bandwidth for estimation	40	37	36	38
Bandwidth for bias	73	69	67	70
Controls:				
Month and day of week	YES	YES	YES	YES
Borough	YES	YES	NO	YES
Offender observables	YES	NO	NO	NO
Standard errors in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

Table S4.
Balance tests for the text message experiment: characteristics of the control group vs. pooled treatments. The "defendant zip code" characteristics are statistics taken from the 2015 American Community Survey for the zip code listed on the defendant's home address. Median income is in 2018 dollars. "Predicted failure to appear" is computed by running a linear regression of failure to appear on observables (offense, age, gender, past failure to appear, past summonses, day of the week). *Data source for all other outcomes: New York Office of Court Administration.*

		Overall mean	Control mean	Pooled Treatment mean	Difference: Treatment-Control	P-value of Difference
	Age	32.00	31.83	32.10	0.28	0.12
	Female	0.14	0.14	0.14	0.00	0.48
Offense	Park Trespassing	0.12	0.12	0.12	0.00	0.68
	Alcohol	0.32	0.32	0.32	0.01	0.43
	Marijuana	0.15	0.15	0.15	0.00	0.47
	Disorderly conduct	0.08	0.08	0.08	0.00	0.24
	Motor vehicle	0.04	0.04	0.04	0.00	0.80
	Public Urination	0.08	0.08	0.08	0.00	0.98
Borough	Bronx	0.25	0.26	0.25	-0.01	0.24
	Brooklyn	0.30	0.30	0.30	0.00	0.60
	Manhattan	0.22	0.22	0.22	0.00	0.93
	Queens	0.19	0.18	0.19	0.01	0.27
	Staten Island	0.04	0.05	0.04	0.00	0.50
Defendant zip	White	0.37	.037	0.37	0.00	0.76
	Black	0.30	0.30	0.30	0.00	0.38
	Hispanic	0.34	0.34	0.34	0.00	0.21
	Median income	49,307	49,146	49,402	255	0.43
	Below poverty	0.21	0.21	0.21	0.00	0.26
	Nb. past summons	0.85	0.89	0.83	-0.06	0.08
	Nb. past failures to appear	0.38	0.40	0.38	-0.02	0.29
	Any past summons	0.30	0.29	0.30	0.01	0.43
	Any past failure to appear	0.17	0.17	0.17	0.00	0.72
	Predicted failure to appear	0.44	0.44	0.44	0.00	0.64
	Sample size	20,234	7,522	12,712		

Table S5.
Effect of text messages on failures to appear in court. Note: Each column represents a separate regression, the outcome being a dummy equal to one if a defendant failed to appear in court on their scheduled court date. Control variables are: gender, age, number of past summonses, number of past failures to appear, and type of offense. *Data source: New York Office of Court Administration.*

	Outcome = Failure to Appear to Court on time					
	Pooled Treatment vs. Control		All individual treatment arms		Consequences vs. Plan-making	
	(1)	(2)	(3)	(4)	(5)	(6)
Received any message	-0.0796***	-0.0804***				
	(0.0068)	(0.0068)				
Consequences			-0.0886***	-0.0899***		
			(0.0085)	(0.0084)		
Plan Making			-0.0605***	-0.0616***		
			(0.0085)	(0.0084)		
Combination			-0.0990***	-0.0981***		
			(0.0107)	(0.0106)		
Consequences vs. Plan making					-0.0281***	-0.0281***
					(0.0091)	(0.0090)
Failure to appear rate: control (col 1-4) and plan-making (col 5-6)	.379				.319	
Observations	20,234	19,878	20,234	19,878	10,160	9,993
Controls for observables, borough, month, precinct	NO	YES	NO	YES	NO	YES
Standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

Table S6.
Characteristics of participants in lab experiment 2 and of people who were issued a summons in a time window of 30 days before or after their issuing police officer switched forms. *Data source: New York Office of Court Administration and participant responses.*

		Summons recipient sample	MTurk sample
	Has had a summons	1	0.04
	Age	34.00	38.83
	Female	0.12	0.60
Borough	Bronx	0.28	0.10
	Brooklyn	0.28	0.30
	Manhattan	0.18	0.30
	Queens	0.21	0.22
	Staten Island	0.05	0.07
	Sample size	34,815	725

Table S7.
Lab experiment 2: Recall of information from old and new summons forms. The "New Form" variable is a dummy equal to one if a respondent saw a redesigned summons form. In columns 1 and 2, the outcome is a dummy equal to one if a person responded that a warrant was "likely or very likely." In columns 3 and 4, the outcome is a dummy equal to one if a person accurately recalled the court date. In columns 5 and 6, the outcome is a dummy equal to one if a person accurately recalled the court location. Even columns include controls for whether a person got a summons in the past, age, gender, and borough of residence.

Outcome:	Warrant is likely if failure to appear		Recalls Date		Recalls Place	
	(1)	(2)	(3)	(4)	(5)	(6)
New Form	0.105***	0.110***	0.194***	0.197***	0.205***	0.208***
	(0.037)	(0.037)	(0.033)	(0.033)	(0.035)	(0.035)
Mean with old form	0.41		0.19		0.26	
Controls	No	Yes	No	Yes	No	Yes
Sample Size	725					
	Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1					

Table S8.
Additional outcomes from lab experiment 2. In column 1, the outcome is a dummy equal to one if a person responded "yes" to the question: "Would you show up for your court date for this summons?" In columns 2 and 3, the outcomes are dummies equal to one if a person agreed or strongly agreed with the statement "the ticket makes you feel" angry or confused. In columns 4 and 5, the outcomes are dummies equal to one if a person agreed or strongly agreed with the statement "The ticket is fair" or "The ticket is reasonable." In the columns 6 and 7, the outcomes are dummies equal to one if a person accurately recalled the offense or how to get more information on their ticket.

	Reaction to ticket					Recall	
Outcome:	Likely to show up to court	Angry	Confused	Fair	Reasonable	Correct offense	More information
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
New Form	0.041	0.072**	-0.032	0.038	0.031	0.004	0.022
	(0.027)	(0.031)	(0.037)	(0.036)	(0.037)	(0.015)	(0.037)
Mean with old form	0.82	0.72	0.54	0.38	0.41	0.95	0.51
Sample Size	725						
Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1							

Table S9.
Effect of text messages on failures to appear in court: tests for external validity. Note: Each column represents a separate regression, the outcome being a dummy equal to one if a defendant failed to appear in court on their scheduled court date. Column 1 presents our baseline estimates of the effect of receiving any text message on failure to appear. Columns 2-5 re-estimate this for different samples. In column 2, the sample is built using propensity score matching to get a group of people who provided phone numbers that is closer to people who did not provide phone numbers. In column 3, we look at the effect of text messages between June 2017 and August 2018, when 20% of defendants were providing phone numbers (against 11% in our main study sample). Columns 4 and 5 limit the sample to defendants who received a summons because of disorderly conduct (which includes engages in fighting or in violent, tumultuous or threatening behavior) or marijuana possession. *Data source: New York Office of Court Administration and 2015 American Community Survey.*

	Outcome = Failure to Appear to Court on time				
	Baseline	Matched sample	June 2017-August 2018	Disorderly conduct	Marijuana
	(1)	(2)	(3)	(4)	(5)
Received any message	-0.080***	-0.11***	-0.092***	-0.088***	-0.098***
	(0.007)	(0.020)	(0.008)	(0.024)	(0.017)
Failure to appear rate in the control group	0.38	0.41	0.36	0.34	0.33
Observations	20,234	2,406	15,033	1,543	3,009

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table S10.
Regression discontinuity design estimates of the effect of new forms on failures to appear in court, by quintile of poverty within the census tract. Note: Each column represents a separate regression, the outcome being a dummy equal to one if a defendant failed to appear in court on their scheduled court date. For each summons recipient, we compute the percent living below poverty within their census tract. For each quintile, we compute the effect of the form redesign. Estimations include controls for month, day of the week, borough gender, age, and type of offense, and percent Black or Hispanic in the census tract. We compute bandwidths and estimates following Calonico et al. (2014). *Data source: New York Office of Court Administration and 2015 American Community Survey.*

	Outcome = Failure to Appear to Court on time				
	Fraction below poverty in the census tract				
	0-0.07	0.07-0.14	0.14-0.22	0.22-0.3	0.3-1
	(1)	(2)	(3)	(4)	(5)
RDD estimate	-0.071***	-0.028	-0.052***	-0.060***	-0.080***
	(0.018)	(0.018)	(0.019)	(0.018)	(0.019)
Failure to appear rate for the old forms in the estimation bandwidth	0.37	0.41	0.47	0.50	0.53
Effective RD Observations	12,365	13,265	12,609	13,389	12,716
Bandwidth for estimation	57	62	56	60	53
Bandwidth for bias	103	98	87	93	87

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table S11.
Effect of text messages on failures to appear in court, by quintile of poverty within the census tract. Note: Each column represents a separate regression, the outcome being a dummy equal to one if a defendant failed to appear in court on their scheduled court date. For each summons recipient, we compute the percent living below poverty within their census tract. For each quintile, we compute the effect of receiving text messages. The regressions include controls for gender, age, number of past summonses, number of past failures to appear, type of offense and percent Black or Hispanic in the census tract. *Data source: New York Office of Court Administration and 2015 American Community Survey.*

	Outcome = Failure to Appear to Court on time				
	Fraction below poverty in the census tract				
	0-0.07	0.07-0.14	0.14-0.23	0.22-0.32	0.32-1
	(1)	(2)	(3)	(4)	(5)
Received any message	-0.078***	-0.073***	-0.056***	-0.064***	-0.125***
	(0.014)	(0.015)	(0.016)	(0.016)	(0.016)
Failure to appear rate in the control group	0.30	0.33	0.37	0.40	0.46
Observations	3,812	3,872	3,756	3,731	3,746
Standard errors in parentheses					
*** p<0.01, ** p<0.05, * p<0.1					

Table S12.
Regression discontinuity design estimates of the effect of new forms on failures to appear in court, by quintile of percent of Black or Hispanic residents within the census tract. Note: Each column represents a separate regression, the outcome being a dummy equal to one if a defendant failed to appear in court on their scheduled court date. For each summons recipient, we compute the percent of Black or Hispanic residents within their census tract. For each quintile, we compute the effect of the form redesign. Estimations include controls for month, day of the week, borough gender, age, and type of offense, and percent living below poverty in the census tract. We compute bandwidths and estimates following Calonico et al. (2014). *Data source: New York Office of Court Administration and 2010 Census.*

	Outcome = Failure to Appear to Court on time				
	Fraction of Black or Hispanic residents in the census tract				
	0-0.31	0.31-0.73	0.73-0.88	0.88-0.95	0.95-1
	(1)	(2)	(3)	(4)	(5)
RDD estimate	-0.023	-0.060***	-0.058***	-0.110***	-0.050***
	(0.014)	(0.019)	(0.020)	(0.021)	(0.019)
Failure to appear rate for the old forms in the estimation bandwidth	0.34	0.42	0.50	0.50	0.53
Effective RD Observations	19,698	11,974	11,282	10,150	13,426
Bandwidth for estimation	95	54	52	44	56
Bandwidth for bias	149	87	85	83	88
Standard errors in parentheses					
*** p<0.01, ** p<0.05, * p<0.1					

Table S13.
Effect of text messages on failures to appear in court, by quintile of percent of Black or Hispanic residents within the census tract. Note: Each column represents a separate regression, the outcome being a dummy equal to one if a defendant failed to appear in court on their scheduled court date. For each summons recipient, we compute the percent Black or Hispanic within their census tract. For each summons recipient, we compute the percent of Black or Hispanic residents within their census tract. For each quintile, we compute the effect of receiving text messages. The regressions include controls for gender, age, number of past summonses, number of past failures to appear, type of offense and percent below poverty in the census tract. *Data source: New York Office of Court Administration and 2010 Census.*

	Outcome = Failure to Appear to Court on time				
	Fraction of Black or Hispanic residents in the census tract				
	0-0.23	0.23-0.67	0.67-0.87	0.87-95	0.95-1
	(1)	(2)	(3)	(4)	(5)
Received any message	-0.069***	-0.087***	-0.075***	-0.089***	-0.077***
	(0.014)	(0.015)	(0.016)	(0.016)	(0.016)
Failure to appear rate in the control group	0.29	0.33	0.39	0.43	0.44
Observations	3,805	3,801	3,757	3,788	3,766
Standard errors in parentheses					
*** p<0.01, ** p<0.05, * p<0.1					

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