

# **Automated Reminders Reduce Incarceration for Missed Court Dates: Evidence from a Text Message Experiment**

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**Millions of Americans must attend mandatory court dates every year. To boost appearance rates, jurisdictions nationwide are increasingly turning to automated reminders, but previous research offers mixed evidence on their effectiveness. In partnership with the Santa Clara County Public Defender Office, we randomly assigned 5,709 public defender clients to either receive automated text message reminders (treatment) or not receive reminders (control). We found that reminders reduced warrants issued for missed court dates by approximately 20%, with 12.1% of clients in control issued a warrant compared to 9.7% of clients in treatment. We further found that incarceration from missed court dates dropped by a similar amount, from 6.6% in control to 5.2% in treatment. This marks the first time that court reminders have been shown to reduce incarceration. The effectiveness of reminders bolsters the theory that lapses in memory or comprehension—rather than intentional noncompliance—drive missed court appearances.**

# 1 Introduction

In the United States, after a person is arrested and charged with a crime, they are either held in jail as their case proceeds, or they are released and asked to attend court of their own accord. While many released defendants do indeed attend court—as is legally required—some fail to do so. Non-appearance rates vary depending on jurisdiction and offense type, ranging from less than 10% to as high as 50% (1, 2). Failing to appear (FTA) at a required court date is a crime in nearly every state, and non-appearance can prompt judges to issue a warrant mandating the defendant’s arrest—hereafter called a “bench warrant”—at their next encounter with law enforcement (3). Judges often issue a bench warrant when a defendant does not attend a mandatory court date, though they can decline to do so if they believe the client has sufficient justification for not being present (e.g., being sick with COVID) (4). Missed court dates create inefficiency for the court system more broadly, increasing costs and exacerbating post-pandemic delays in U.S. courts (5).

Once arrested for a bench warrant, punishment can include time in jail. This pretrial incarceration can impose social and economic hardships on defendants and their families—including housing loss, family strain, and social stigma (6, 7)—even as evidence suggests that time in jail may not deter future court absences (8). Pretrial incarceration has also been found to increase recidivism and reduce employment and wealth (9–12). The consequences of missed court dates may fall particularly hard on racial minorities, given the disproportionate involvement of marginalized communities in the criminal legal system (7, 13, 14).

Many assume that missed court dates result from people intentionally “fleeing” their court obligations (15). But people may miss court simply due to forgetfulness or confusion about the court system (16), mirroring findings from other domains, including healthcare, finance, politics, and education (17–26). As a result, court date reminders are increasingly used to help people remember and plan for upcoming court obligations. Nearly half of all counties nationwide have implemented or plan to implement court date reminders via text message, phone call, mail, or some other method (27). Studies have shown that text message reminders can be effective for non-defendant participants in the criminal legal system (28, 29). For example, an experiment in Arkansas found that text message reminders reduced missed probation and parole appointments by over 40% (29). There are also several studies on the effectiveness of reminders sent to defendants via resource-intensive

methods like a letter or a telephone call (30–38). One such study found that postcard reminders reduced non-appearance rates by up to 34% in an experiment with misdemeanor defendants in Nebraska (38). (See (39) and (40) for reviews of the relevant literature.)

Yet research on the effects of automated *text message* reminders is limited, even though it is one of the most cost-effective ways of sending reminders, and is increasingly used by jurisdictions nationwide. The literature that does exist paints an incomplete picture of the efficacy of text message reminders to increase court appearance rates and decrease the negative consequences of missing court (Table 1). Two recent randomized controlled trials (RCTs) found significant and meaningful reductions in FTA rates from text message reminders (41, 42), while two other RCTs also found reductions in FTA rates, though the estimates were not statistically significant (2, 43). One of these studies additionally examined the impact of automated reminders on pretrial incarceration, finding no statistically significant effect of reminders on jail bookings (41). Another RCT estimated higher—but not statistically significant—warrant rates among people who received a text message reminder (44). Some of these studies likely lacked sufficient statistical power, but their presence in the literature may confuse or deter policymakers who are already hesitant to adopt behavioral nudges (45). In addition, we note that the two studies that found statistically significant impacts on FTA considered only municipal violations and misdemeanors, even though more serious felony-level cases often comprise a substantial proportion of a court’s caseload.

To help resolve whether text message reminders increase court appearance and reduce incarceration, we ran an RCT with clients at the Santa Clara County Public Defender Office (SCCPDO), headquartered in San Jose, California. Our experiment consists of 5,709 SCCPDO clients who were charged with felonies, misdemeanors, or supervision violations. Of these clients, 2,387 had court dates between May 17, 2022 and September 21, 2022, and 3,322 had court dates between October 14, 2022 and August 24, 2023. Clients were randomly assigned to treatment or control conditions with equal probability: 2,811 clients were assigned to the control condition, which meant they did not receive any automated reminders; and 2,898 clients were assigned to the treatment condition. To be eligible for inclusion in the experiment, clients must have had at least one court date in the timespans mentioned above, had a cellphone number available in SCCPDO’s case management system, and had never previously received an automated reminder from SCCPDO. In Figures 2 and S1, we show that covariate distributions were nearly identical across experiment arms, indicating

**Table 1: Inconsistent historical evidence on the efficacy of automated court date reminders.**

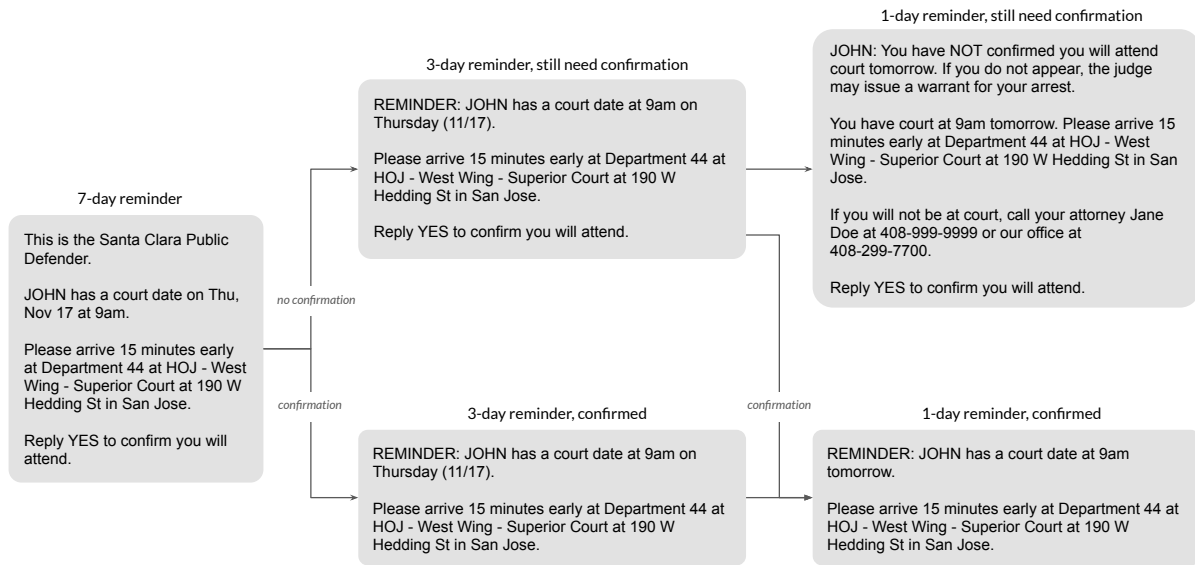
Past experiments have yielded mixed results on the effectiveness of text message court date reminders for improving appearance rates.

Study	Year	Outcome	Sample	Control	Est. effect	CI	Estimated rel. effect	P-val
Chivers & Barnes, 2018	2017	Warrant at court date	946 defendants	22.5%	+1.8pp	N/A	+ 8%	0.51
Lowenkamp et al., 2018	N/A	FTA at court date	10,228 defendants	13%	-2pp	N/A	-18%	0.07
Fishbane et al., 2019	2016–17	FTA/warrant at summons hearing	20,234 defendants	37.9%	-9.9pp	[-12 – -7.8pp]	-26%	<0.01
Emanuel & Ho, 2024	2018–19	FTA at arraignment	30,870 defendants	21%	-8.2pp	N/A	-39%	<0.01
Owens & Sloan, 2023	2021	FTA at court date	1,096 housed defendants	50%	-6pp	[-11.2 – +0.6pp]	-12%	0.08

that the randomization scheme worked as intended.

Clients in the treatment condition received reminders seven days, three days, and one day before each upcoming court date (see Figure 1 for a diagram of these reminders). The reminder schedule we used mirrors the timing of reminders in other studies, which sent reminders at various combinations of one, three, and seven days before a court date (34, 40–42, 44). Translated versions of these reminders were provided in Spanish and Vietnamese for the 22% of clients who had previously indicated a need for a translator in one of these languages (Figure S2).

Our study is the first to specifically examine the effect of reminders for clients of public defenders. Understanding the efficacy of reminders for this subpopulation is particularly important for ongoing policy debates, as policymakers may expect that public defenders can ensure court appearance for their own clients, obviating the need for reminders sent at additional cost to taxpayers. Indeed, SCCPDO clients appear at their court appointments the vast majority of the time. Yet there is still room for improvement, with about 10–15% of scheduled court dates for SCCPDO clients ending in a bench warrant for non-appearance. Given that people are often required to attend multiple court dates, nearly one-third of SCCPDO clients received at least one bench warrant for missing court over the course of 2022. Over half of these clients were only facing misdemeanor or lesser



**Figure 1: Message flow for clients in the treatment condition.** Clients were asked to confirm their attendance at each court date, with the timing of their confirmation determining their path through this flow. Reminders were also available in Spanish and Vietnamese for clients who preferred those languages (Figure S2).

charges, and one out of every four had no history of prior charges on file with SCCPDO. A single bench warrant for these clients thus has the potential to quickly ramp up an otherwise minimal brush with the criminal legal system, and underscores the importance of increasing appearance rates.

## 2 Results

We focus on how reminders affect two primary outcomes: (1) issuance of a bench warrant for failure-to-appear (FTA); and (2) remands to custody on a bench warrant. When measuring impacts on bench warrant issuance, we consider two timeframes: first, issuance at the client’s first scheduled court date after assignment, and second, issuance of at least one bench warrant at any point between assignment and the end of the experiment. When measuring impacts on remands to custody, we solely consider the second (longer) timeframe, since several events must transpire before a remand occurs: first, a client must miss court, then a judge must issue a bench warrant, and finally, sometime later, the defendant must have a chance encounter with law enforcement that results in an arrest for

**Table 2: The effect of text message reminders** on the issuance of bench warrants for non-appearance and on remanding to custody on a bench warrant. These effects were estimated using logistic regression as discussed in Section 4.2. Reported estimates are odds ratios (i.e., exponentiated logistic regression coefficients), with standard errors in parentheses calculated using the delta method. The single star indicates that the corresponding logistic regression coefficient estimates (on the log-odds scale) have a p-value between 0.01 and 0.05, and the double star indicates a p-value between 0.001 and 0.01. Coefficient estimates for other covariates are included in Table S2.

Timeframe	First court date		Any court date			
	Bench warrant		Remand to custody			
Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Observations	5,709 clients		5,709 clients		5,709 clients	
Obs. (control)	2,811 clients		2,811 clients		2,811 clients	
Obs. (treatment)	2,898 clients		2,898 clients		2,898 clients	
Rate (control)	12.1%		20.7%		6.6%	
Rate (treatment)	9.7%		17.6%		5.2%	
Difference	-2.5pp		-3.1pp		-1.4pp	
Est. treat. effect	0.748**	0.775**	0.797**	0.818**	0.768*	0.775*
Std. Error	(0.068)	(0.066)	(0.058)	(0.055)	(0.094)	(0.088)
Covar. adjustment	Yes	No	Yes	No	Yes	No

the open warrant.

In the control condition, 12.1% of clients received a bench warrant at their first scheduled court date after assignment, compared to 9.7% for clients in the treatment condition. This 2.5pp difference (95% CI: 0.9pp–4.1pp) corresponds to a 20.4% reduction in bench warrant rates. Between assignment and the end of the experiment, 20.7% of clients in the control condition received at least one bench warrant, compared to 17.6% of clients in the treatment condition, a 3.1pp difference (95% CI: 1.1pp–5.1pp), corresponding to a 15.0% reduction in the issuance of bench warrants.

These reductions persist in subsequent rates of incarceration. 6.6% of clients in the control con-

dition were remanded on a bench warrant after assignment to our experiment, compared to 5.2% of clients in the treatment condition, a 1.4pp difference (95% CI: 0.2pp–2.6pp) corresponding to a relative reduction of 21.4%. Impacts on both bench warrants and remands to custody are qualitatively similar for clients with both misdemeanor- and felony-level cases, suggesting that both groups benefit from automated reminders (Table S3).

To improve the precision of our results, we also estimate the impact of text message reminders via logistic regression models corresponding to each of our outcomes of interest, adjusting for available pre-treatment covariates, as described in Section 4.2. With these models, we estimate effects that are comparable to those seen with the raw, unadjusted rates (Table 2).

### 3 Discussion

Our experiment demonstrates that automated reminders increase court attendance and reduce pre-trial incarceration among public defender clients. The effectiveness of these simple behavioral nudges suggests that forgetfulness or lack of comprehension is an important factor explaining missed court dates. Our results highlight the value of addressing cognitive and informational barriers to court attendance instead of relying solely on punitive measures to ensure attendance. With an average marginal cost of roughly 60¢ per defendant per case, automated text message reminders can be an effective and relatively inexpensive way to help people attend court and avoid incarceration.

More broadly, arrests stemming from missed court dates are a significant contributor to incarceration (46). Reminders can help reduce pressures on overcrowded and underfunded jails across the United States (47), including in the face of court mandates to reduce incarcerated populations. For example, the Supreme Court of the United States ordered California to reduce the size of its prison population because overcrowding rendered prison conditions unconstitutional (see *Brown v. Plata*, 2011, no. 09-12330).

Much remains unanswered about how to design behavioral nudges to be most effective at preventing bench warrants. For example, the optimal timing and frequency of text message reminders is unclear. It may be more effective to remind clients about court obligations over a week in advance or to do so more frequently in the week before. The reminders we used also only briefly mentioned the possible consequences of missing court. Other content—a stronger focus on the consequences,

or a focus on possible supports—may be more effective at preventing bench warrants. In addition, court date reminders may not help clients who are struggling with more fundamental barriers to court attendance, such as lack of transportation or childcare, or work obligations. Other behavioral nudges, like transportation or financial assistance, might further address these barriers and could complement court date reminders (48). Alongside defendants, a wide array of court participants (including witnesses and police officers) may also struggle to attend court, and may benefit from reminders like the ones we describe here (49). Finally, many public defender clients may lack a reliable and persistent cellphone number altogether, preventing them from benefiting from these types of interventions.

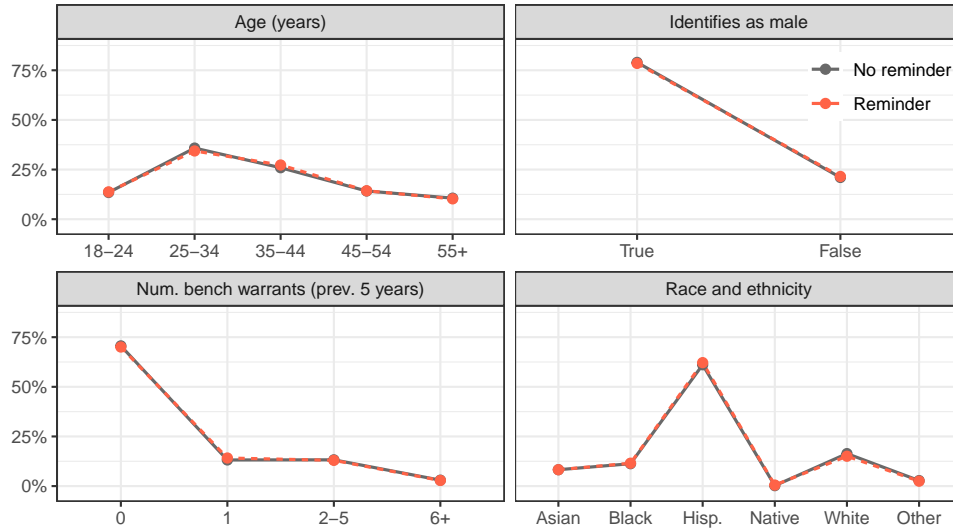
In addition to behavioral nudges, policymakers might consider alternative pathways to reducing pretrial incarceration. For example, judges could issue a bench warrant for non-appearance only in the most egregious circumstances, such as when there is clear evidence a defendant is unwilling to cooperate with the judicial process. Some counties in California are working to improve appearance rates and other outcomes by pairing defendants with case managers that help to address underlying challenges, like housing instability and substance use, that their clients may be facing. And courts may consider alternative means to case resolution that do not require the hassle of physical appearance in court, e.g., participation in ongoing proceedings via asynchronous online portals for less serious cases (50). While our work demonstrates the promise of behavioral nudges for reducing incarceration, this approach is but one step in more broadly reforming the criminal legal system.

## **4 Materials and Methods**

### **4.1 Experimental Design**

For clients in the treatment condition, we sent an introductory text message prior to the first reminder explaining the reminder program and explaining how to opt out, if desired. Of the 2,898 clients in the treatment arm, 107 opted out of receiving text message reminders, of which a majority (57 clients) opted out by noting that we had the wrong number. After a reminder, clients were prompted to confirm their attendance by responding with “yes” or similar affirmations. For example, our application recognized many possible confirmations, including “OK”, “Confirmed”, “I’ll be there”,





**Figure 2: Distributions of select covariates split between treatment and control populations.**

Nearly half of participants were under the age of 34, most identified as male, a majority identified as Hispanic, and most had no record of a bench warrant within the past five years. In addition, distributions are nearly identical between conditions, confirming that our assignment mechanism randomly assigned clients to the two conditions as intended. See Figure S1 for an expanded version.

a thumbs-up emoji, and confirmations in Spanish (like *Sí* or *Gracias*) and Vietnamese (like *Đi* or *Được*). If they confirmed, we did not prompt for confirmation on subsequent reminders. Ultimately, 51% of clients in the treatment arm confirmed their attendance, and among these clients, 2.9% received a bench warrant at their first court date; in comparison, a bench warrant was issued for 16.8% of clients who did not confirm their attendance (Table S4). This difference could be explained by the act of confirming, self-selection, or a combination thereof. In any case, our study is not designed to determine whether confirmations affect appearance in court, and we do not consider confirmation behavior in our analysis in order to avoid post-treatment bias.

In our original pre-registration for this experiment (available at [https://aspredicted.org/SMY\\_N1R](https://aspredicted.org/SMY_N1R)), we proposed comparing two message variants: a variant drafted by the public defender versus a simpler variant that we thought would be easier for clients to understand. However, we later concluded that the two message variants were not meaningfully comparable (e.g., because they differed in length) and so terminated that experiment without analyzing any of the resulting data. Simultaneously, we ran the experiment described in this paper, comparing the simpler variant

against no messages. For transparency, we have now analyzed the data corresponding to our two-variant experiment, finding that bench warrant rates were lower among clients receiving the simpler variant compared to the longer one (12.3% vs. 13.4%, respectively), although the difference was not statistically significant. We are currently running a new experiment that is better designed to compare message templates, pre-registered at [https://aspredicted.org/FKC\\_XYY](https://aspredicted.org/FKC_XYY).

## 4.2 Statistical Analysis

Bench warrants are rarely proactively enforced. In theory, clients may resolve an open bench warrant by independently reaching out to the court or their attorney. In practice, however, clients with open bench warrants are typically arrested during their next, unrelated encounter with law enforcement (e.g., as a result of a traffic stop). In either case, the client then appears at a “bench warrant hearing.” At these hearings, judges may choose to release the client back into the community if they believe the client will appear at future court dates, or—alternatively—may remand the client to jail pending bail, later release, or case resolution. As a result, a bench warrant does not always result in incarceration, given the dynamics of client initiative, chance contact with law enforcement, and judicial discretion. In particular, only half of SCCPDO clients with bench warrants issued between 2019 and 2021 were remanded to jail within two years of their missed court date.

We are specifically interested in how court date reminders impact these remands to jail. To measure this phenomenon, we code a client’s outcome as “incarcerated” if they were remanded at a bench warrant hearing where no new charges were brought, and code the outcome as “not incarcerated” for all other clients. This metric directly corresponds to the target of our intervention—incarceration solely attributable to missed court dates (discussed in further detail in Section S2). Our findings are qualitatively similar if we redefine the outcome to indicate whether a client was remanded at any type of bench warrant hearing, regardless of whether they were arrested on new charges.

In Table 2, we estimate the impact of text message reminders using logistic regression models of the following form:

$$\Pr(Y_i = 1) = \text{logit}^{-1}(\alpha + \beta T_i + \gamma^T X_i), \quad (1)$$

where  $Y_i$  indicates our outcome of interest (e.g., issuance of a bench warrant),  $T_i$  indicates whether

the client was in the treatment condition, and  $X_i$  is a vector representing a variety of observable features of the client, case, and first scheduled court date. In particular,  $X_i$  includes: demographic information (the client's age, race, whether the client identifies as male, whether the client prefers a language interpreter, whether the client's attorney indicated a possible mental health issue for the client, whether a home address is on file for the client, and the distance between the client's home address and the courthouse where their appearance is scheduled, coded as zero if there is no address on file); client history (the number of bench warrants for non-appearance known to SCCPDO in the previous five years, the inverse number of court dates known to SCCPDO in the previous five years, the product of these two covariates, representing the client's bench warrant rate for failing to appear over the last five years, whether the client was "new", i.e., whether the earliest court date known to the public defender was in the preceding year, the number of previous cases with the public defender's office, the number of previous convictions or guilty pleas with the public defender office (including *nolo contendere* pleas), and the number of years since the client's phone records were updated); case information (whether the most serious charge was classified as a felony, misdemeanor, or supervision violation, and indicators for which of 19 high-level charge categories were present, e.g., burglary or robbery); and court date information (the courthouse where the court date was scheduled, the day of week, the month, and a number indicating the court date was the  $n$ -th scheduled appointment on a case).

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# **Supplementary Materials for Automated Reminders Reduce Incarceration for Missed Court Dates: Evidence from a Text Message Experiment**

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## **S1 Treatment assignment**

Starting in 2021, as we developed the software necessary to conduct this experiment, we sent court date reminders to some SCCPDO clients; these clients were not eligible for inclusion in our experiment, though they received similar reminders to those described here.

In the first phase of the experiment (i.e., for clients with initial court dates between May 17, 2022 and September 21, 2022), clients in the treatment condition received an introductory text message up to seven days before their first court date reminder. Occasionally, however, court dates once eligible for reminders may have become ineligible in this interim period after the introductory message was sent (e.g., because the attorney indicated they would appear on the client's behalf, or because the recipient may have opted out of text message reminders immediately after their introductory message). As a result, 85 of the 2,898 clients in the treatment condition did not receive a reminder for their initially scheduled court date. Nevertheless, we include in the treatment condition all clients who received an introductory message, regardless of whether or not a reminder was actually sent, as the introductory text message could itself impact behavior. In the second phase of the experiment (i.e., for clients with initial court dates between October 14, 2022 and August 24, 2023), we adjusted our protocol to address this issue, sending the introductory message and the first court date reminder at the same time. This change ensures that all clients in the treatment condition did in fact receive at least one reminder.

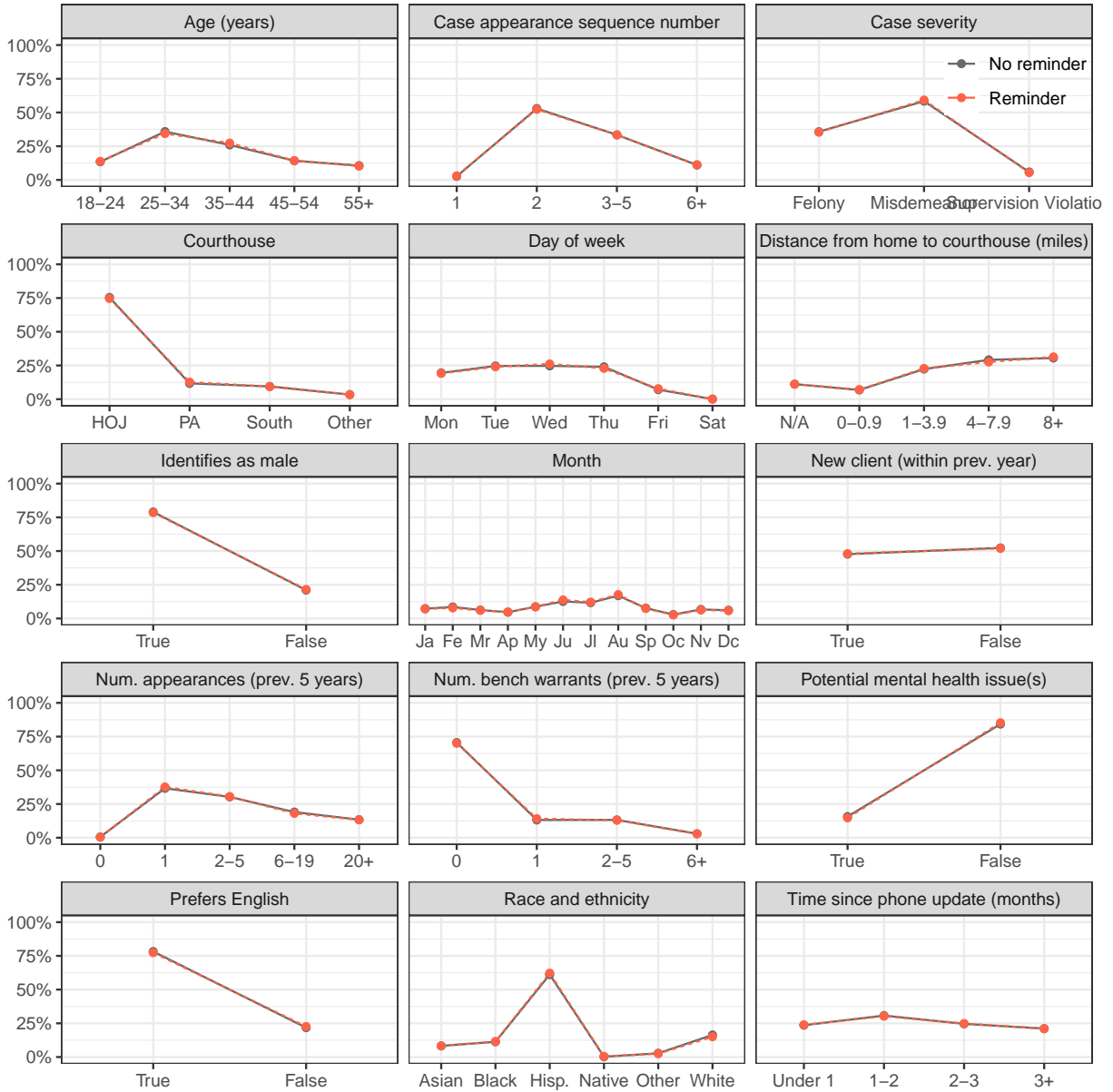
At the end of the first phase of the experiment, all clients in the first phase were transitioned to

receive text messages reminders for any future court dates, regardless of whether they were initially assigned to treatment or control. As a result, our estimate of the effect of reminders on long-term outcomes is likely conservative, since some clients in the control condition received reminders for part of the observation window. This pattern does not affect our estimate of reminders on the issuance of bench warrants at the first court date, since that outcome is measured before any transitioning occurred. No clients in the second phase of the experiment were transitioned, i.e., clients in the control condition in the second phase did not receive reminders during the observation period.

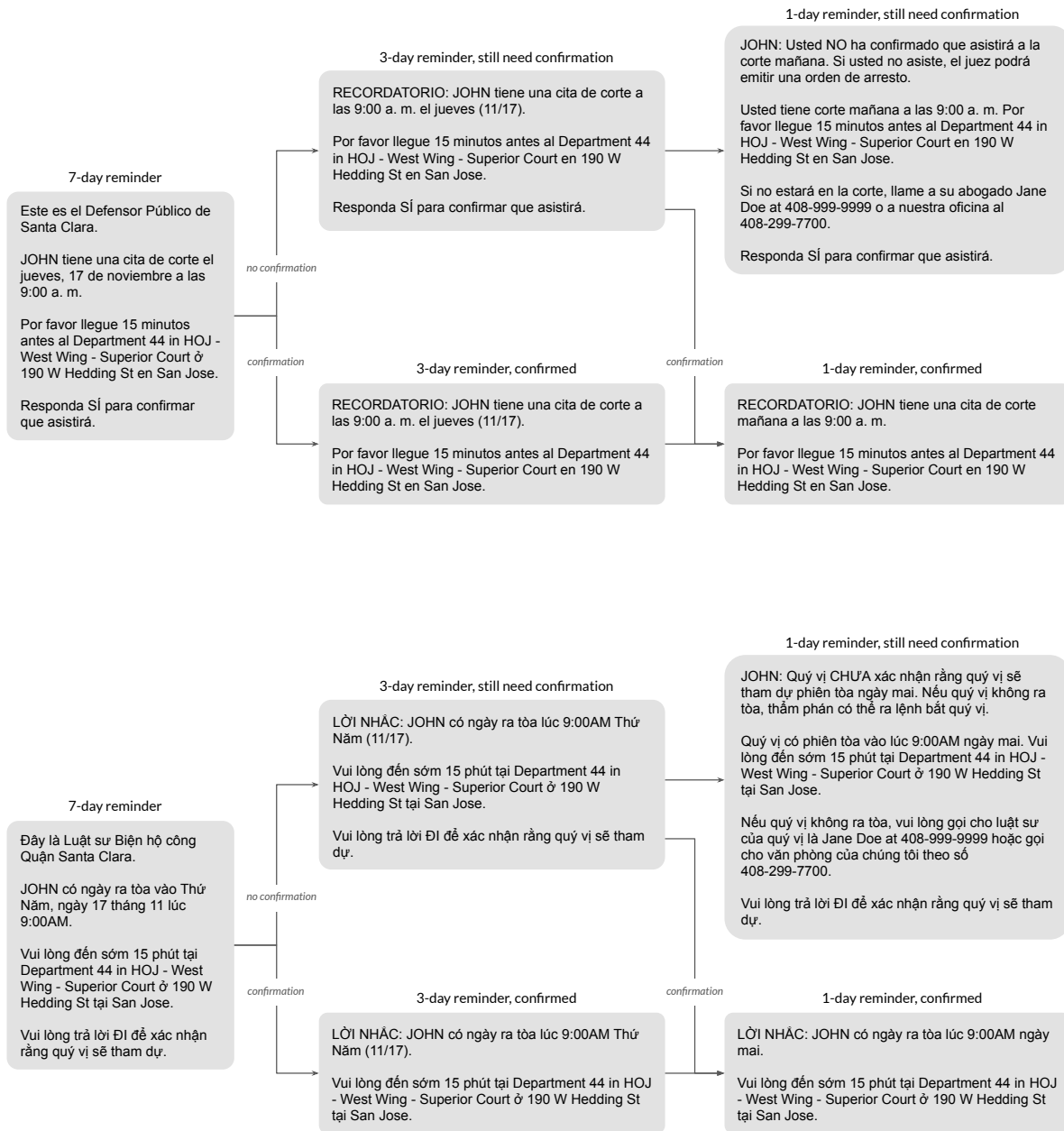
To confirm that our assignment procedure indeed randomly assigned clients to treatment or control, we examined balance plots (Table S1 and Figure S1). Across a wide range of covariates, we see that the distributions are nearly identical between the two conditions, as expected.

## **S2 Verifying custody status**

To verify that a client who was remanded to custody was, in fact, held in the county jail, we worked with SCCPDO to manually query the custody status for a sample of 41 clients with court dates between June 5 and June 7, 2023. Of these 41 clients, 19 were remanded to custody at their court date. As of June 9, 2023, 16 of these 19 clients remanded to custody were verified to be in jail. Of the remaining 22 clients (who were not remanded to custody), 21 were verified not to be in jail. The small discrepancy between remands and incarceration is likely due to events that transpired between the court date and the custody check; for example, clients may have been released after paying bail, or may have been incarcerated on a different case not represented by SCCPDO. These results suggest that the vast majority of clients remanded to custody spend at least several days in jail.



**Figure S1:** Covariate distributions for the treatment and control conditions were nearly identical, confirming that our assignment mechanism correctly randomly assigned clients to the two conditions. Statistics in this figure are drawn from the last two columns in Table S1.



**Figure S2:** Reminder flows in Spanish (top) and Vietnamese (bottom).

	All	Cell on File	Experiment	Control	Treatment
<b>Age (years)</b>					
18-24	12%	13%	14%	13%	14%
25-34	33%	34%	35%	36%	34%
35-44	27%	28%	27%	26%	27%
45-54	15%	15%	14%	14%	14%
55+	13%	10%	10%	11%	10%
<b>Any bench warrant remand (without new charges) during experiment</b>					
True	3%	11%	6%	7%	5%
False	97%	89%	94%	93%	95%
<b>Any bench warrant remand during experiment</b>					
True	3%	13%	7%	8%	6%
False	97%	87%	93%	92%	94%
<b>Any remand during experiment</b>					
True	26%	34%	23%	24%	22%
False	74%	66%	77%	76%	78%
<b>Case appearance sequence number</b>					
1	5%	36%	3%	3%	3%
2	30%	16%	53%	53%	52%
3-5	32%	22%	33%	33%	33%
6+	33%	25%	11%	11%	11%
<b>Case severity</b>					
Felony	35%	37%	36%	36%	35%
Misdemeanor	49%	57%	59%	58%	59%
Supervision Violation	16%	6%	6%	6%	5%
<b>Courthouse</b>					
Hall Of Justice	67%	70%	75%	75%	75%

Palo Alto	12%	10%	12%	12%	13%
South County	7%	8%	9%	9%	9%
Other	14%	11%	3%	3%	3%
<b>Distance from home to courthouse (miles)</b>					
0-0.9	6%	7%	7%	7%	7%
1-3.9	19%	22%	23%	22%	23%
4-7.9	24%	28%	28%	29%	28%
8+	26%	29%	31%	31%	31%
N/A	25%	14%	11%	11%	11%
<b>Identifies as male</b>					
True	80%	80%	79%	79%	78%
False	20%	20%	21%	21%	22%
<b>New client (within prev. year)</b>					
True	34%	38%	48%	48%	48%
False	66%	62%	52%	52%	52%
<b>Num. appearances (prev. 5 years)</b>					
0	4%	23%	0.5%	0.6%	0.4%
1	20%	11%	37%	37%	38%
2-5	26%	21%	30%	30%	31%
6-19	28%	24%	19%	19%	18%
20+	23%	22%	13%	13%	13%
<b>Num. bench warrants (prev. 5 years)</b>					
0	61%	64%	70%	71%	70%
1	15%	13%	14%	13%	14%
2-5	19%	17%	13%	13%	13%
6+	5%	5%	3%	3%	3%
<b>Potential mental health issue(s)</b>					
True	16%	18%	15%	16%	15%

False	84%	82%	85%	84%	85%
<b>Prefers English</b>					
True	82%	80%	78%	78%	77%
False	18%	20%	22%	22%	23%
<b>Race and ethnicity</b>					
Asian	8%	8%	8%	8%	8%
Black	11%	12%	11%	11%	12%
Hispanic	55%	60%	62%	61%	62%
Native	0.3%	0.4%	0.4%	0%	0.5%
White	17%	17%	16%	16%	15%
Other	8%	3%	3%	3%	2%
<b>Total clients</b>	<b>21,343</b>	<b>14,063</b>	<b>5,709</b>	<b>2,811</b>	<b>2,898</b>

**Table S1:** Population characteristics for five subsets of SCCPDO clients. The first population, “all” clients, was created by considering all SCCPDO clients with a reminder-eligible court date during the experiment window and measuring client and case characteristics at the first reminder-eligible court date for each client within this window. The second population, “cell on file”, was constructed by considering all SCCPDO clients who had a cellphone number on file and a reminder-eligible court date during the experiment window, and measuring attributes at the first reminder-eligible court date for each client within the experiment window. The third population, “experiment” clients, represents all clients in the experiment population at their first observed court date. Clients in the “cell on file” population who are not in the “experiment” population were not eligible for our experiment because they received reminders before our experiment began. The fourth and fifth populations, “treatment” clients and “control” clients, further break down the “experiment” population by the client’s random assignment.

Timeframe	First Court Date	Any Court Date	
Outcome	Bench Warrant		Incarceration
Model	(1)	(3)	(5)
<b>Intercept</b>			
-	-2.54*** (0.73)	-1.17* (0.58)	-2.66** (0.83)
<b>Treatment</b>			
Reminders	-0.29** (0.09)	-0.23** (0.07)	-0.26* (0.12)
<b>Client race/ethnicity</b>			
Asian	-0.26 (0.19)	-0.05 (0.16)	0.15 (0.26)
Black	-0.35* (0.17)	-0.07 (0.14)	0.12 (0.22)
Hispanic	-0.23 (0.13)	-0.21* (0.11)	-0.05 (0.17)
Native	0.57 (0.59)	0.06 (0.54)	0.93 (0.78)
Other	-0.61 (0.36)	-0.12 (0.25)	-0.17 (0.46)
<b>Client information</b>			
Is not male	-0.18 (0.12)	-0.07 (0.09)	-0.18 (0.17)
Age	0.00 (0.00)	0.00 (0.00)	-0.01 (0.01)
Mental health	-0.03 (0.13)	-0.05 (0.10)	0.22 (0.15)
New client	-0.07 (0.13)	0.10 (0.11)	0.05 (0.21)
Prefers english	-0.05 (0.13)	0.06 (0.10)	0.19 (0.18)
Years since phone added	0.54*** (0.11)	0.36*** (0.09)	-0.06 (0.14)
Home address recorded	-0.61*** (0.12)	-0.59*** (0.10)	-0.61*** (0.15)
Miles from home to court	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
<b>Client history (5 yr counts)</b>			
Cases	0.02* (0.01)	0.02* (0.01)	0.02 (0.01)
Convictions	0.00 (0.00)	0.00 (0.00)	0.00 (0.01)
1/court dates	-0.05 (0.37)	-0.55 (0.31)	-2.83*** (0.63)
Bench warrants	0.01 (0.03)	0.04 (0.03)	0.07 (0.03)



Bench warrants/court dates	2.87*** (0.45)	3.07*** (0.39)	2.99*** (0.61)
<b>Day of week</b>			
Monday	0.64** (0.23)	0.32 (0.17)	0.25 (0.27)
Tuesday	0.45* (0.22)	0.27 (0.16)	0.13 (0.26)
Wednesday	0.64** (0.22)	0.32* (0.16)	0.33 (0.25)
Thursday	0.48* (0.22)	0.24 (0.16)	0.05 (0.27)
<b>Month</b>			
February	-0.12 (0.24)	0.00 (0.18)	0.06 (0.36)
March	0.10 (0.25)	0.04 (0.19)	-0.25 (0.42)
April	0.29 (0.26)	-0.12 (0.21)	-0.25 (0.46)
May	0.14 (0.23)	-0.16 (0.18)	0.35 (0.33)
June	0.21 (0.21)	-0.09 (0.16)	0.63* (0.30)
July	-0.10 (0.22)	-0.50** (0.17)	0.45 (0.31)
August	0.05 (0.21)	-0.90*** (0.17)	0.27 (0.30)
September	0.37 (0.23)	-0.68*** (0.20)	0.28 (0.35)
October	0.40 (0.30)	0.42 (0.23)	0.77 (0.39)
November	0.07 (0.24)	0.21 (0.18)	0.38 (0.35)
December	0.06 (0.25)	-0.03 (0.19)	-0.11 (0.39)
<b>Court date info</b>			
Appearance number	-0.01 (0.01)	-0.01 (0.01)	-0.03 (0.01)
<b>Case severity</b>			
Felony	-0.22 (0.23)	-0.07 (0.18)	0.01 (0.24)
Misdemeanor	0.24 (0.22)	0.30 (0.18)	0.02 (0.23)
Prcls violation	1.68 (1.27)	0.05 (0.66)	0.24 (0.89)
Probation violation	1.73 (1.26)	0.06 (0.61)	0.64 (0.85)
<b>Courthouse</b>			
Hall Of Justice	0.65 (0.61)	0.31 (0.49)	0.16 (0.65)
Family Court	-1.02 (0.75)	0.24 (0.53)	0.54 (0.70)
Palo Alto	0.19 (0.62)	-0.25 (0.50)	0.02 (0.67)

San Jose Municipal	-0.04 (0.46)	0.42 (0.34)	-0.17 (0.62)
Morgan Hill	0.16 (0.63)	-0.35 (0.51)	0.23 (0.68)
Other	0.40 (0.37)	0.28 (0.33)	0.81 (0.44)
<b>Charges</b>			
Assault	-0.34** (0.12)	-0.19 (0.10)	0.32* (0.15)
Burglary	0.22 (0.25)	0.27 (0.20)	0.37 (0.27)
Disorderly	-0.15 (0.48)	-0.09 (0.39)	0.99* (0.45)
Driving	-0.45*** (0.13)	-0.22* (0.11)	-0.61** (0.20)
Drugs	0.54*** (0.13)	0.56*** (0.12)	0.44* (0.18)
Forgery	-0.43 (0.59)	-0.06 (0.47)	-1.83 (1.12)
Fraud	0.56** (0.18)	0.40* (0.16)	-0.10 (0.28)
Homicide	-1.51 (1.01)	-1.09 (0.60)	-0.66 (1.03)
Kidnapping	-1.17 (0.60)	-0.87* (0.39)	-0.44 (0.55)
Larceny	0.39* (0.16)	0.53*** (0.13)	0.33 (0.20)
Larceny (vehicular)	0.86*** (0.20)	0.89*** (0.18)	0.79** (0.25)
Probation/parole	-2.20 (1.27)	-0.28 (0.61)	-0.15 (0.85)
Robbery	0.01 (0.36)	-0.13 (0.27)	-0.47 (0.44)
Sex offenses	-1.12* (0.47)	-1.39*** (0.40)	-1.26 (0.73)
Stolen property	0.61* (0.24)	0.75*** (0.22)	0.01 (0.32)
Trespassing	0.58* (0.26)	0.27 (0.23)	0.48 (0.32)
Weapons	-0.17 (0.18)	-0.08 (0.15)	0.08 (0.23)
Vandalism	0.27 (0.18)	0.30* (0.14)	0.31 (0.20)
Other	-0.14 (0.12)	-0.04 (0.10)	0.30* (0.14)

**Table S2:** Logistic regression coefficient estimates for the three covariate-adjusted models described in Section 2. Model numbers correspond to those listed in Table 2. Coefficient estimates are on the log-odds scale, with standard errors in parentheses. A single star indicates that the corresponding logistic regression coefficient estimate has a p-value between 0.01 and 0.05, a double star indicates a p-value between 0.001 and 0.01, and a triple star indicates p-values under 0.001.

Timeframe	First Court Date	Any Court Date	
Outcome	Bench Warrant	Incarceration	
Model	(7)	(8)	(9)
<b>Intercept</b>			
Intercept	-1.96*** (0.07)	-1.35*** (0.06)	-2.87*** (0.10)
<b>Treatment</b>			
Reminders	-0.25* (0.11)	-0.20* (0.08)	-0.19 (0.15)
<b>Case severity</b>			
Felony	-0.06 (0.12)	0.03 (0.10)	0.53*** (0.15)
<b>Interaction</b>			
Reminders * Felony	-0.01 (0.18)	-0.01 (0.14)	-0.14 (0.23)

**Table S3:** Logistic regression coefficient estimates for three alternate models that include an interaction between the treatment and case severity. Reminder impacts on bench warrant rates appear qualitatively similar for both felony-level and misdemeanor-level clients. The model estimate of reminders' impact on incarceration rates for felony-level clients is negative but highly uncertain, a result of the fact that this experiment is not powered to determine whether reminders are more effective for clients with felony charges compared to those with less serious cases. Coefficient estimates are on the log-odds scale, with standard errors in parentheses. A single star indicates that the corresponding logistic regression coefficient estimate has a p-value between 0.01 and 0.05, a double star indicates a p-value between 0.001 and 0.01, and a triple star indicates p-values under 0.001.

Confirmed?	Proportion of Clients	Bench Warrant Rate
Yes	51%	2.9%
No	49%	16.8%

**Table S4:** Proportion of clients in treatment arm who confirmed and did not confirm, and their corresponding bench warrant rates at the first observed court date. Note that confirmation behavior is a response to the reminder, and as such the act of confirmation cannot be interpreted causally.