San Diego Pothole Project: Pre-Analysis Plan

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1 Overview and research questions

This study evaluates whether (i) simple, low-cost information about the City of San Diego's "Get-It-Done" (GID) service request system and (ii) exposure to recent pothole repairs affect administrative service request behavior, beliefs, and attitudes about City services and pothole repairs. The study combines a street-segment-level randomized controlled trial of door hangers (DH) with quasi-experimental variation in pothole fixes (PF), along with a separate door-to-door survey (D2DS) used to measure beliefs and attitudes, and administrative outcomes for subsequent service uptake.

Key comparisons. The randomized variation is in DH saturation at the street-segment level (Full-DH, Partial-DH, Control-DH). Pothole fixes (PF) are *not* randomized, though we are familiar with the allocation mechanism and will attempt to exploit it for identification.

Main hypotheses.

1. DH may increase GID awareness and increase future service requests (pothole and/or non-pothole) by reducing informational/transaction costs.
2. Households on segments with timely pothole fixes (PF; main specification: within 2 days) have more favorable views of government than those experiencing long lags in pothole repairs.
3. Among segments with recent pothole service requests, the effect of DH may differ depending on whether those requests are fixed quickly (PF).
4. A small monetary reward conditional on a successfully repaired pothole report increases the probability that a D2DS respondent submits a GID pothole service request, and the submission probability is (weakly) monotone in the reward amount.

2 Pilot

We piloted DH and D2DS logistics in one neighborhood, Linda Vista. Linda Vista is excluded from the study sample and all analyses.

3 Sampling frame

3.1 Universe and segment/address construction

We start from all street segments in the City of San Diego. Street segments are a natural unit because they are the unit used by the City for road repair planning.

1. Assign residential addresses to segments if the address lies within 40 meters of the segment.
2. Require each segment to have at least two addresses within 40 meters.
3. Restrict to addresses in primarily single-family residential zoning categories (currently `RS-1-2` through `RS-1-14`, plus `RM-1-1`), and further restrict to addressable records without explicit unit numbers, to ensure physical accessibility for DH and D2DS (apartment buildings may not be accessible).

3.2 Bundling

We group eligible segments into compact, walkable *bundles* of approximately 80 addresses each. We drop bundles whose total segment length is above 3km, making them very hard complete in the allocated interviewer walk time. In the fielded sample, bundles hold 72–88 eligible addresses (median 83) and 3–17 segments (median 8); no fielded bundle has fewer than three segments. Let j index bundles, i index segments, and t index Saturdays (field weeks).

4 Field design, assignment, and sampling schedule

4.1 Door hangers (DH): segment-level randomized saturation

When a bundle is selected for DH distribution, each segment in that bundle is randomized into one of three DH saturation conditions:

- **Full-DH** with probability 1/4: all addresses on the segment receive a door hanger;
- **Partial-DH** with probability 1/4: exactly half of addresses on the segment receive a door hanger (simple random sample without replacement);
- **Control-DH** with probability 1/2: no addresses on the segment receive a door hanger.

For most administrative analyses, we define $DH = 1$ for Full-DH and Partial-DH segments and $DH = 0$ for Control-DH segments. We additionally pre-specify saturation/spillover comparisons using separate Full and Partial indicators (Section 9.5).

Implementation note 1: address-level assignment in the first five weeks. The segment-level assignment above was implemented from the field week of February 14, 2026 onward. For the first five DH weeks (January 10 through February 7, 2026) the field-file generator randomized at the *address* level within each bundle: each address was assigned with probability 1/2 to no hanger, 1/4 to a hanger, and 1/4 to a group of which a random half received a hanger, so that about 37.5% of the addresses on every segment received a hanger and there is no segment-level contrast. We retain these weeks in the sample. In specifications with bundle fixed effects they contribute no within-bundle DH variation and therefore do not inform the DH coefficients; they enter the realized-exposure specification (Section 9.6) and the specifications with council-district-by-week fixed effects. We report all DH estimates excluding these weeks as a robustness check.

Implementation note 2: fixed random seed. The planning script drew the segment arms with a fixed pseudo-random seed, so the same sequence of arms was applied, by position in the weekly list (sampled bundles in order, four pothole-conditional bundles before two unconditional ones, and segments in file order within each bundle), in each of the 24 weeks from February 14 to August 15; the August 22 plan, regenerated after two interviewers withdrew, used a different draw. Conditional on which bundles were sampled (itself random, without replacement), the arm of a segment is therefore a deterministic function of its list position. List position is unrelated to the pothole conditions that select bundles and to any field decision, so we treat assignment as as-good-as-random conditional on bundle-by-week. We pre-specify (i) balance tests regressing each segment covariate (length, PCI, eligible addresses, road class, pre-period service requests) on the two arm indicators with bundle-by-week fixed effects, and (ii) a robustness specification that adds within-bundle position-rank fixed effects to every DH regression. Because the sequence is fixed, the mix of arms differs between the conditional bundles (list positions 1–4) and the unconditional bundles (positions 5–6); this difference is absorbed by the bundle fixed effects. The balance tests are reported with the results.

The door hanger is the City's own Get It Done hanger (form PW/PS-793), in English or Spanish (Figure 1); it lists the services that can be requested and carries the web address, the app-store links and a QR code.

4.2 Fieldwork schedule

Six interviewers worked every Saturday from January 10 through August 22, 2026, except May 23 and July 4 (holidays) and June 20 (cancelled after the plan had been generated; its bundles were returned to the pool), yielding 30 DH field weeks and 29 D2DS field weeks. Fielding was extended by two weeks beyond the August 8 end date given at registration. June 6 ran with five interviewers and August 22 with four; on May 9 and July 18 one interviewer's DH and D2DS bundles were not fielded. Three of the 24 week-1 DH bundles were not visited and one bundle not on the plan was; sampling status follows the field record.

Figure 1: The door hanger (English and Spanish versions).

4.3 Week 1 (January 10, 2026): DH only

On the first field day, we sample 24 bundles (4 per interviewer) for DH distribution only. Of these 24 bundles, 4 are sampled from bundles with at least one pothole patched in the preceding week (conditional), and 20 are sampled pothole-unconditionally from all available bundles. D2DS does not occur in Week 1. This ensures that, starting in Week 2, D2DS can be conducted in areas where DH may have been delivered in the prior week, and provides a sufficient pool of previously-sampled DH bundles (including the 4 conditional bundles) for D2DS reuse.

4.4 Weeks 2+ (January 17, 2026 onward): DH and D2DS

Each Saturday $t \geq 2$:

- **DH bundles:** sample one DH bundle per interviewer (6 total), from bundles not previously sampled for DH, with composition:
  - four out of six DH bundles have at least one pothole patched on one of their segments during the seven days prior to the Saturday field day ("this week"). This bundle selection criterion ensures sufficient variation in exposure to recent pothole patches and (non-random) time-to-fix, while recognizing that selected bundles will contain a mix of segments: some with pothole patches, some with no pothole activity, and some with pothole reports not yet serviced. In case there are less than 4 bundles with pothole patches from this week, we sample unconditionally to fill in the remaining bundles.

  - the remaining two out of six DH bundles are sampled pothole-unconditionally. We do this so as to not skew the sample entirely towards pothole-ridden bundles.
- **D2DS bundles:** sample one D2DS bundle per interviewer (6 total), from bundles not previously sampled for D2DS, with composition:
  - four of the six D2DS bundles correspond to the four DH bundles with at least one pothole patched drawn last week;
  - the remaining two D2DS bundles are drawn from bundles that have not received any DH but have had potholes patched in the *preceding* week. We focus D2DS on pothole-ridden bundles so as to maximize the sample of pothole-affected households in the survey.
  - in either case, if there are not enough pothole-conditional bundles, we again sample pothole-unconditionally to fill in the remaining bundles.

When the preceding Saturday was not fielded (May 30, June 27 and July 11), the D2DS bundles were drawn from the most recent DH week, 14 days earlier. We pool the 7- and 14-day lags; the lag is constant within a bundle-week and is absorbed by the bundle fixed effects.

4.5 D2DS pothole-report incentive (RI)

A subset of the D2DS instrument tests whether a small monetary reward for *successfully* reporting a pothole through the City's GID platform increases the probability that a household engages with the system.

Mechanism. At the end of the interview the respondent is offered a printed business card containing: (i) a study-issued five-character card ID, (ii) a QR code linking to a study-run landing page (`sdpotholes.org`), and (iii) a printed reward amount, or no amount. To claim the reward, the respondent submits a pothole service request through the City's GID platform and then enters the City Service Request ID and an email address at the landing page. The study team sends the submitted email addresses to the City's GID product team, which returns the associated case numbers, request types and status. If a pothole case is returned for the email and it is closed within 60 days of submission, a gift card of the printed amount is emailed to the respondent. Submissions outside the 60-day window or without a matching City case are ineligible for payment. The reward is conditional on a repair rather than on a submission to discourage fabricated requests; the design was proposed to the City in this form on January 14, 2026 and approved on January 23, 2026, on the condition that the City bears no cost. The incentive arm was added after registration and entered the survey instrument on February 21, 2026; interviews completed before that date did not include the module, so the RI sample consists of interviews completed from February 21 onward. Figure 2 shows the cards.

Randomization. The reward amount was pre-assigned to every eligible address in the sampling frame before fielding, independently across addresses: with probability 1/2 a \$0 *placebo*, and with probability 1/2 a positive amount drawn uniformly from $\{\$5, \$10, \$15, \$20\}$ (so each positive amount

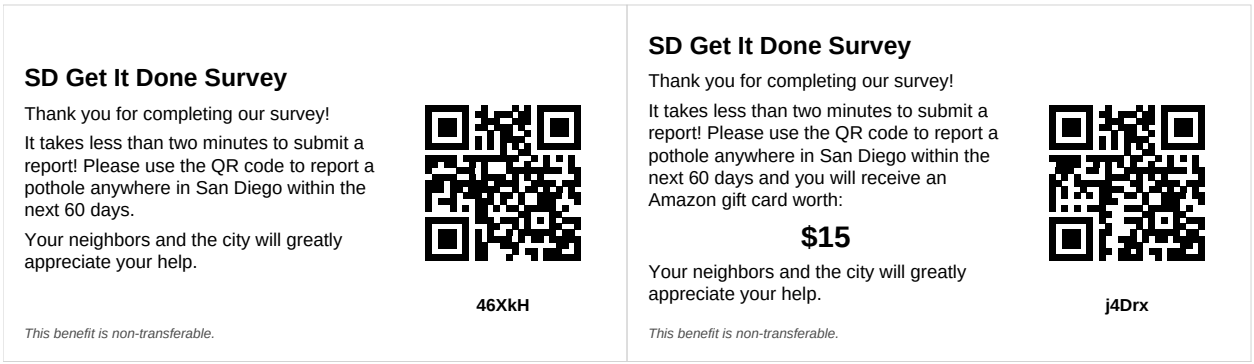

Figure 2: Incentive cards: a \$0 (placebo) card and a \$15 card. The five-character code under the QR code is the card ID recorded by the interviewer.

has marginal probability 1/8). The assignment was loaded into the survey software, which displayed the amount to the interviewer at the incentive module; the interviewer handed a pre-printed card of that amount and recorded its ID. Cards with no amount are otherwise identical, advertise the GID submission process and route to the same landing page, but do not pay out. Let $RI_h \in \{0, 5, 10, 15, 20\}$ denote the amount assigned to respondent h's address, and let $POS_h \equiv \mathbf{1}\{RI_h > 0\}$; both are defined by the assignment, so respondents who declined the card remain in the analysis (intention to treat).

Unit of randomization and identification. RI is randomized at the address level within D2DS bundles. Identification of the RI effect comes from within-bundle, across-respondent variation in RI_h, holding fixed the DH and bundle-week environment.

5 Non-randomized pothole fixes (PF)

Pothole fixes are not randomized. We exploit quasi-idiosyncratic variation in time-to-fix that arises from SDTD's FIFO logic and routing constraints, conditional on pavement condition index (PCI) categories and other pre-specified controls.

Exploratory note on instruments (not pre-registered as main analysis). We may explore instrumental-variable approaches for PF timing (e.g., leave-segment-out measures of crew workload or nearby fix intensity). These IV approaches are not part of the main pre-registered estimands. An IV design is hard to anticipate because of the nature of sampling, which is a function of the frequency and distribution of potholes.

6 Data sources and units of observation

6.1 Administrative data (GID)

We use City administrative data from the GID platform. Because service requests are difficult to assign to individual addresses, the primary administrative unit of observation is the street segment-week. We may attempt such a linking, but it would likely be subject to considerable noise.

6.2 Pothole repair data for sampling

For bundle sampling, we use pothole repair data from the City's internal notification activities system (`notification_activities.csv`), filtered to records with activity code "POTHOLE PATCHED (EA)". These are repair work orders initiated by the City, with dates corresponding to when repair work was notified or initiated (`NOTIFICATION_DATE`), rather than citizen service request dates. We will also use the publicly available service request data.

Bundles are selected based on whether at least one segment within the bundle has at least one pothole patched during the relevant time window. Importantly, bundles selected through this criterion contain a heterogeneous mix of segments: some segments have pothole patches, some have no pothole-related activity, and some may have pothole reports (either citizen-initiated or City-identified) that have not yet been serviced by the field date. This selection strategy ensures exposure to recent pothole repair activity while maintaining representativeness across segments with varying pothole conditions.

6.3 GIS data and segment covariates

Segment length, the 2023 Pavement Condition Index (PCI, with the City's seven-category rating) and functional road class (residential local street, cul-de-sac, alley, collector, major) come from the City's street segment file.

6.4 Survey data (D2DS)

D2DS outcomes are measured at the respondent (address) level. In regressions, D2DS outcomes are analyzed with fixed effects at the bundle-by-week level (each bundle is surveyed once, so these coincide with bundle fixed effects) and controls at the segment level. The door-hanger disposition file records, for every address visited, whether a hanger was left, so each respondent's own-door treatment status is observed; Section 9.5 uses it.

7 Outcomes

7.1 Primary administrative outcomes (segment-week)

For each segment i and week t, we construct:

- pothole service requests (count and indicator),

- non-pothole service requests (count and indicator),
- any service requests (count and indicator).

We also construct cumulative outcomes for each segment over horizons of 1 week, 1 month, and 3 months following the segment's DH week.

7.2 Primary D2DS outcomes

We pre-specify the following City Service Trust/Quality Index: the average of standardized survey items $G1$ (overall satisfaction), $G2$ (tax dollars used efficiently), and $G3$ (tax dollars used fairly across neighborhoods), coded so higher values indicate more positive perceptions.

All remaining D2DS items are analyzed as primary outcomes as well, coded as follows ("declined to answer" is coded missing throughout):

- $G1$–$G3$ on their 1–5 scales, plus the index above.
- GID awareness and use: $K1$ (aware = 1); indicators for having ever requested any service, and a pothole repair, through GID (zero for the unaware); an indicator for naming a door hanger as the source of awareness (the manipulation check for DH).
- Pothole salience: $P1$–$P4$ as yes/no indicators; $P5$ on its 0–10 scale.
- Policy preferences: $P6$ ($5 per month) and $P7$ ($50 per month) as yes/no indicators; agree indicators for the four $P8$ statements on why San Diego has so many potholes (available from February 21, 2026): "City road maintenance is poorly managed", "The City does not prioritize road maintenance", "The City lacks sufficient funding to maintain roads", "Weather and heavier rains damage roads more quickly".
- Response-time beliefs: $R1$ (unreported) and $R2$ (reported) as ordinal codes (one day, one week, one month, one year, never) and as indicators for "within one week".
- Neighbor beliefs: $N1$–$N3$ on their 0–10 scales.

Pothole service requests induced by DH are an outcome under hypothesis 1, and any repairs they trigger are part of the DH effect. In the static specification below, $PF_i(2)$ is measured on requests created in the seven days preceding the segment's DH Saturday (the window that governs conditional bundle sampling), so it is determined before DH and is not itself an outcome of DH.

7.3 Primary D2DS outcome: GID submission (RI sample)

For each D2DS respondent h in the RI sample (Section 4.5), define $Submit_h$ as an indicator equal to 1 if the respondent's card ID appears at `sdpotholes.org` paired with a GID Service Request ID within 60 days of card issuance, and 0 otherwise. As secondary outcomes we construct $SubmitMatched_h$, equal to 1 only if the City returns at least one pothole case for the submitted email, and $Repaired_h$, equal to 1 only if that case is closed within 60 days. Respondents who declined the card or gave no email are coded 0 on all three.

8 Key explanatory variables

8.1 Door-hanger event time

Let t_i^* denote the Saturday in which segment i's bundle is sampled for DH (the segment's "DH week"). For segments in DH-sampled bundles (including Control-DH segments), define event time $s = t - t_i^*$ and an indicator DH_{is} for being assigned DH in that DH week.

The event-time window is $s \in \{-4, -3, -2, 0, 1, \ldots, 12\}$, omitting $s = -1$ as the reference period.

8.2 Pothole service request (PSR)

Define PSR_{it} as an indicator equal to 1 if segment i has at least one pothole service request created during the seven days preceding Saturday t (the "request week" window), and 0 otherwise.

8.3 Pothole fix within x days

For segments with at least one pothole service request in the relevant request window, define $PF_i(x)$ as an indicator equal to 1 if at least one such request is fixed within x days of request creation, and 0 otherwise. A request is considered "fixed" if it can be matched to a pothole patch record in the City's internal notification activities data (`POTHOLE PATCHED (EA)`). The number of days to fix is calculated from the public service request creation date to the corresponding repair completion date. In the main static specification below, we set $x = 2$. As sensitivity analyses, we will re-estimate the same specification for each $x \in \{1, \ldots, 7\}$. In cross-sectional analyses involving pothole fixes, PF_i (without an argument) denotes whether at least one such request has been fixed by the time outcomes are measured.

9 Estimation

9.1 Main administrative event study: DH dynamics

Our baseline specification is an event-study style regression of an administrative outcome:

$$Y_{it} = \alpha_i + \gamma_{j(i),t} + \sum_{s \in \{-4,-3,-2,0,1,\ldots,12\}} \beta_s \, DH_{is} + \varepsilon_{it}, \tag{1}$$

where α_i are segment fixed effects and $\gamma_{j(i),t}$ are bundle-by-week fixed effects. Coefficients β_s trace the dynamic effect of DH relative to the omitted reference period $s = -1$. No fielded bundle has fewer than three segments, so no bundle drops out of this specification; the distribution of segments per bundle and of the within-bundle share of DH segments are reported as descriptive statistics. Administrative outcomes will be pulled through November 2026, three months after the last DH week, so that the three-month horizon is available for every cohort.

9.2 DH event-study robustness with external controls

As a robustness check, we will also estimate a stacked event study in which each DH-treated cohort is compared only to control segments in the same PCI category drawn from bundles that are never sampled for DH during the study period and are outside the treated segment's bundle. This specification is intended to reduce the scope for within-bundle spillovers.

We do not pre-specify a separate PF event study. PF enters the pre-specified analysis only through the static cross-sectional specifications below.

9.3 Static cross-sectional specification (admin and D2DS)

For administrative outcomes aggregated over horizons (1 week, 1 month, 3 months) and for D2DS outcomes, we estimate:

$$Y_i = \delta_{j(i)} + \beta_1 DH_i + \beta_2 PF_i(2) + \beta_3 DH_i \times PF_i(2) + X_i'\mu + \varepsilon_i, \tag{2}$$

where $\delta_{j(i)}$ are bundle fixed effects and X_i is a vector of pre-specified controls. For administrative outcomes, Y_i is the cumulative count over a post-DH horizon (1 week, 1 month, or 3 months) measured from the segment's DH week. For D2DS outcomes, DH_i indicates whether the segment was assigned DH on the fielded Saturday preceding the D2DS survey. These regressions are estimated on the sample of segments with at least one pothole service request in the relevant request window (for D2DS, the seven days preceding the DH Saturday), so that $PF_i(2)$ is well-defined; we also report β_1 from the same specification without the PF terms on the full sample. As sensitivity analyses, we will re-estimate the same specification for each $x \in \{1, \ldots, 7\}$, replacing $PF_i(2)$ with $PF_i(x)$.

9.4 Heterogeneity

We pre-specify two primary heterogeneity dimensions, both defined at the Census block-group level using the most recent ACS 5-year estimates and matched to each segment via its centroid:

- **Below-median household income:** $LowInc_i \equiv \mathbf{1}\{MedHHInc_{bg(i)} < m_Y\}$, where m_Y is the median of $MedHHInc_{bg(i)}$ taken over the universe of segments eligible for the study (sampling frame, Section 3).
- **Below-median share white:** $LowWhite_i \equiv \mathbf{1}\{ShareWhite_{bg(i)} < m_W\}$, where m_W is the median of $ShareWhite_{bg(i)}$ over the same eligible-segment population.

Both cut points are fixed ex ante from the eligibility frame, before unblinding, and are applied identically to the administrative and D2DS samples. For each primary outcome, we re-estimate equation (2) interacting DH_i, $PF_i(2)$, and $DH_i \times PF_i(2)$ with $LowInc_i$, and (in a separate regression) with $LowWhite_i$.

Beyond these two pre-specified cuts, we may explore additional heterogeneity dimensions; any such analyses will be clearly labeled as exploratory.

9.5 Within-segment saturation / spillover analysis (D2DS sample)

To assess within-segment spillovers under partial saturation, we use the D2DS sample and estimate:

$$Y_h = \delta_{j(h)} + \theta_F \, \mathbf{1}\{FullDH_{ih}\} + \theta_P \, \mathbf{1}\{PartialDH_{ih}\} + X'_{i(h)}\mu + \varepsilon_h, \tag{3}$$

where h indexes D2DS respondents and the right-hand-side saturation indicators are defined at the segment level (control omitted). We will also report the linear combination $\theta_P - \frac{1}{2}\theta_F$ as a simple diagnostic for departures from proportional scaling with saturation, consistent with within-segment spillovers or nonlinearities.

Because the disposition file records whether a hanger was left at each respondent's own door, we also estimate, on the same sample,

$$Y_h = \delta_{j(h)} + \theta_F \, \mathbf{1}\{FullDH_{ih}\} + \theta_P \, \mathbf{1}\{PartialDH_{ih}\} + \theta_O \, \mathbf{1}\{PartialDH_{ih}\} \times Own_h + X'_{i(h)}\mu + \varepsilon_h, \tag{4}$$

where Own_h indicates that respondent h's own address received a hanger. Within Partial segments, θ_P is the effect on untreated neighbors of treating half the segment (a within-segment spillover) and $\theta_P + \theta_O$ the effect on treated households.

9.6 Realized-exposure specification (all weeks)

To use the address-level weeks and the variation in delivery within arms, we estimate

$$Y_h = \delta_{j(h)} + \beta \, Share_{i(h)} + X'_{i(h)}\mu + \varepsilon_h, \tag{5}$$

where $Share_i$ is the share of segment i's eligible addresses at which a hanger was left on the DH Saturday (zero on Control segments and in bundles without DH). This specification is secondary to equations (2) and (3).

9.7 Reporting-cost incentive (RI) regressions

On the sample of D2DS respondents offered an RI card, we estimate three pre-specified regressions of $Submit_h$ on reward indicators with bundle-by-week fixed effects $\delta_{j(h),t}$ and the same control vector $X_{i(h)}$ used in the static D2DS specification.

The primary specification is a binary contrast,

$$Submit_h \;=\; \delta_{j(h),t} \;+\; \pi^{POS} \, POS_h \;+\; X'_{i(h)}\mu \;+\; \varepsilon_h, \tag{6}$$

where $POS_h = \mathbf{1}\{RI_h > 0\}$. By construction, POS_h has marginal probability 1/2, so this contrast carries the most power. The second specification replaces POS_h with four amount indicators $\mathbf{1}\{RI_h = a\}$, $a \in \{5, 10, 15, 20\}$, and reports the joint test that the four coefficients are equal. The third is linear in the amount, $\pi^{RI} \, RI_h$, estimated on the full RI sample and on the positive-card

sample, which tests the weak monotonicity in hypothesis 4. The same three specifications are estimated for $SubmitMatched_h$ and $Repaired_h$.

10 Controls

We pre-specify two control sets.

Primary (minimal) controls. Segment length and PCI-category fixed effects.

Secondary (augmented) controls (robustness). In addition to the primary controls: seasonally-adjusted historical pothole service requests (last four years), functional road class, continuous PCI, and within-bundle position-rank fixed effects (Section 4.1, implementation note 2). We may also merge Census neighborhood characteristics to segments as further controls.

No household-level controls pre-specified. All controls are defined at the segment level; the $X_{i(h)}$ vector in the D2DS and RI specifications is the segment-level control vector indexed by the respondent's segment. We do not pre-specify household-level covariates as controls. We may attempt to merge parcel-level characteristics (such as hedonics or prices) as controls.

11 Inference

Event studies. Standard errors are clustered at the segment level to account for serial correlation in the segment-week panel.

Cross-sectional regressions. For our primary segment-level cross-sectional regressions, we report heteroskedasticity-robust standard errors. For D2DS individual-level regressions, including the RI regressions, we report standard errors clustered at the segment level.

Multiple inference. We do not plan to adjust standard errors or p-values for multiple testing. In settings with many estimates (event-time coefficients; PF(x) sensitivity), we will present full coefficient paths/curves and emphasize pre-trends diagnostics (event study) and sensitivity plots (PF(x)) rather than selecting individual coefficients ex post. Since our set of survey outcomes is relatively small, we prefer to be transparent about the full set of results across all survey responses and interpret inference accordingly.

12 Secondary specifications

1. **Across-segment spillovers within bundles:** re-estimate key specifications replacing bundle-by-week fixed effects with council-district-by-week fixed effects.

2. **D2DS effects on administrative outcomes:** estimate event studies on D2DS event-time indicators and add a D2DS indicator (and interactions) to the static administrative specification.
3. **Heterogeneity:** explore heterogeneous effects by PCI category (good/fair/poor/etc.).
4. **Design robustness:** re-estimate every DH specification (a) excluding the five address-level weeks (January 10 to February 7, 2026) and (b) adding within-bundle position-rank fixed effects.

13 Deviations from the registered design

The trial was registered on January 10, 2026 and published on January 12, 2026 (AEARCTR-0017630). The following differences between the registered design and the realized field design are documented above and summarized here.

1. Fielding ran through August 22, 2026 rather than August 8 (30 DH weeks and 29 D2DS weeks; June 20 cancelled).
2. Door-hanger assignment was at the address level within bundle for the five DH weeks January 10 to February 7, 2026, and at the segment level (Full/Partial/Control) from February 14 onward.
3. The segment-level arms were drawn with a fixed random seed, so the same arm sequence was applied by list position in every week from February 14 to August 15; August 22 used a fresh draw. Balance tests and a position-rank robustness check are pre-specified.
4. D2DS bundles surveyed on May 30, June 27 and July 11 received door hangers 14 rather than 7 days earlier.
5. The reporting-incentive (RI) arm was added after registration (City approval January 23, 2026) and entered the instrument on February 21, 2026; the reward amount was pre-assigned per address rather than by shuffled cards, and verification runs through the City's GID product team rather than the internal repair file. The $P8$ battery was added on the same date.
6. Three week-1 DH bundles were not visited and one off-plan bundle was; on May 9 and July 18 one interviewer's bundles were not fielded; June 6 and August 22 ran with five and four interviewers.

14 Ethics / IRB

This study has been reviewed and determined exempt by the University of Southern California IRB (Study ID UP-25-00768; exemption determination under 45 CFR §46.104(d)(2); approval dated Oct 7, 2025) and by MIT COUHES (E-7213; exempt categories 2 and 3; submission dated Oct 10, 2025).

15 Registration and deviations

The trial is registered as AEARCTR-0017630 (registered January 10, 2026; published January 12, 2026). This version of the plan, dated September 16, 2026, updates the May 15, 2026 version. It was completed after fielding closed on August 22, 2026 and after descriptive checks of data completeness, response rates, survey item levels and covariate balance across arms (Section 4.1), but before any treatment effect on any outcome was estimated. Any further deviations will be documented and clearly labeled in dissemination materials.