# Strengthening Local Governance through Ward Committees: Experimental Evidence on Service Delivery

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## Introduction

The constitutional mandate for ward level meetings in India primarily comes from the 74th Constitutional Amendment Act, 1992. Ward committee meetings are crucial in India because they bring governance closer to ordinary citizens, enable direct public participation, and help address local issues efficiently and transparently. Municipal service delivery (water supply, waste management, road repairs, street lighting) is a critical determinant of urban quality of life. In many cities, ward committees exist on paper but function irregularly and without structure.

## Research Question

Does the implementation of regular, structured ward committee meetings improve the timeliness, quality, and citizen satisfaction with municipal service delivery?

## Program

Instead of irregular, ad-hoc, or poorly documented ward meetings, the treatment delivers a specific, repeatable package that makes meetings predictable, inclusive, and accountable — so issues are logged, owners assigned, and follow-up tracked.

**Regularity predictability**

Meetings scheduled once a month (e.g., first Saturday, 5–6:30 PM).

Venue and time fixed in advance; citizens notified via SMS/posters 7 days and 24 hours before.

**Structured agenda facilitation**

Standardized, time-boxed agenda: review of last month's action items, municipal updates, citizen issue queue, assignment of owners/deadlines.

**Citizen attendance encouragement design**

To increase participation and generate variation in meeting attendance, an encouragement strategy will be implemented within treatment wards

## Unit of Randomization and Experimental Arms

**Population of Interest:** All households and residents within the municipal wards included in the study area.

**Unit of Randomisation:** Ward-level (cluster) randomization. Wards are the unit of assignment because the intervention (regular, structured ward meetings) operates at the ward institutional level

**Treatment Group:** Wards receive the full Regular, Structured Ward Meetings package

**Control Group:** The control group does not receive the RSWM package during the trial period.

## Estimation Strategy

In the proposed randomized controlled trial (RCT):

- Wards ($j$) are randomly assigned to either the treatment arm (regular, structured ward meetings) or the control arm (status quo).
- $Z_j$ denotes the random assignment of ward $j$ to treatment or control.
- For household $i$ in ward $j$, $D_{ij}$ indicates the actual treatment status, i.e., whether the household participates in or holds the meetings.
- $Y_{ij}$ denotes the outcome of interest for household $i$ in ward $j$, such as timeliness of service delivery, quality of services, or citizen satisfaction.

This setup captures both cluster-level randomization and household-level outcomes, allowing estimation of causal effects while accounting for potential variation in household-level compliance. **ATE under perfect compliance.** If $D_{ij} = Z_j$, then treatment status is as good as randomized and the Average Treatment Effect (ATE) could be estimated consistently by:

$$Y_{ij} = \alpha + \beta D_i j + \varepsilon_{ij},$$

where $\beta$ captures the causal effect of actual participation.

**Compliance problem.** In practice, some treatment wards may not hold meetings, some citizens may not attend, and some control wards may still organize informal meetings. Thus $D_{ij} \neq Z_j$ for some units.

**Intent-to-Treat (ITT).** We therefore estimate the effect of assignment:

$$Y_{ij} = \alpha + \beta Z_i + \varepsilon_{ij},$$

which identifies the policy-relevant impact of being offered structured ward meetings, regardless of compliance.

**Local Average Treatment Effect (LATE).** To recover the causal effect of actual participation, we instrument $D_{ij}$ with $Z_i$.

$$\text{Stage 2: } Y_i = \beta_0 + \beta_{\text{LATE}} D_i + \beta_2 X_i + \epsilon_{ij}$$

i.e. the effect of attending meetings for those units who attend only because they were encouraged.Thus, while the ATE is not point-identified due to noncompliance, both ITT and LATE are estimable under the proposed design. My preferred estimate is ITT as the design will be able to hold only the following assumptions (i) random assignment of $Z_i$, and not (ii) exclusion (assignment affects $Y_i$ only via $D_{ij}$), and (iii) monotonicity (no defiers).

## Heterogeneity Analysis

Let $Y_{ij}$ denote the outcome of household $i$ in ward $j$ (e.g., timeliness, quality, or satisfaction), $D_{ij}$ denote actual participation (endogenous), $Z_j$ be the randomized assignment of ward $j$ to treatment, and $Align_j$ be an indicator equal to 1 if the ward councillor is from the same party as the municipal ruling party, and 0 otherwise. ($Align_j = 1$).

$$Y_{ij} = \alpha + \beta D_{ij} + \delta(D_{ij} \times Align_j) + \varepsilon_{ij} \quad (1)$$

A positive $\delta$ would indicate that the intervention is more effective in wards with aligned councillors, whereas a negative $\delta$ would suggest smaller effects relative to opposition wards. I would expect heterogeneity along dimensions such as household phone ownership or literacy (since access to information and meeting notifications may differ), gender of the household head (female- versus male-headed households may face distinct constraints and opportunities in participation), and household economic status measured by income or asset terciles (with low- and high-income groups potentially experiencing different benefits from improved service delivery).

## Potential Threats to Validity

**Potential Spillovers**: Residents of treated wards tell neighbours in control wards about meeting content or file complaints that prompt municipal action affecting nearby control wards. Household-level encouragement (if targeted) creates peer effects within communities crossing ward borders.

**Minimizing spillover:** To minimize spillovers, we will designate geographic buffer zones by excluding wards that directly border treated wards from the treatment–control comparison. In addition, we will randomize at the level of ward clusters (groups of non-adjacent wards), rather than single wards, to ensure treated and control areas are spatially separated.

**Attrition**: Treated households may be more engaged and therefore more likely to complete endline surveys (differential attrition). Alternatively, time costs from participation may increase drop-out among busy households.Attrition could be correlated with outcomes (non-random), biasing ITT and LATE.

**Reducing Attrition**: To address the risk of differential attrition, we implement Lee2009 bounds. This method equalizes observation rates across treatment and control by trimming outcomes from the group with lower attrition until rates match. We then compute treatment effects under the assumption that attrition removed either the highest or lowest outcome individuals, producing a conservative interval (Lee bound) for the treatment effect. If attrition is minimal, the bounds will be tight and close to the standard ITT estimate; if attrition is differential, the bounds provide a transparent range of plausible effects.

## Potential ethical concerns

**Control wards are denied an intervention that could improve services.** Control wards retain all existing services and channels (no reduction). Offer a delayed roll-out / waitlist: after the evaluation period (e.g., 18 months), control wards may receive the intervention package or training if results and agreements permit.

**Surveys or follow-ups may disproportionately burden certain groups or lead to differential dropout.** Keep household survey short (10–15 minutes for core module); offer modest, non-coercive token compensation for time (e.g., a pack of sanitary items).

**Intervention may be perceived as politically biased or trigger local tensions.** Ensure intervention materials emphasize civic process and are non-partisan.