The Effects of Direct Elections on Program Implementation: Evidence from India's Village Councils

Priyadarshi Amar* Rikhil Bhavnani⁺ Amit Jadhav[‡]

August 2023

Scholars in the field of democracy have long engaged in debates regarding the merits of direct versus indirect elections. They have argued that direct elections lead to enhanced representation (Rogers, 1926; Riker, 1955), the adoption of moderate policies (Bernhard and Sala, 2006), greater responsiveness towards voters (Crook and Hibbing, 1997), and increased participation in legislative activities (Meinke, 2008). On the other hand, indirect elections tend to result in policies that prioritize party interests (Bullock III and Brady, 1983) and foster accountability towards political intermediaries (Micozzi, 2013; Gailmard and Jenkins, 2009). These contentions mirror the discourse surrounding presidential systems, which involve directly elected executives, as opposed to parliamentary systems with indirectly elected government heads (Lijphart, 1992; Przeworski et al., 1996; Cheibub, 2007; Horowitz, 1990; Linz, 1990).

The empirical literature on the merits of direct elections has not arrived at a consensus on the effects of direct elections. For example, Crook and Hibbing (1997) find that direct elections enhance responsiveness towards voters. On the other hand, Rogers (2012) finds no significant evidence to support this claim. Most empirical studies do not provide credible causally identified effects, possibly since electoral systems rarely change, and when they do, it is typically due to endogenous political processes. Recently, a nascent literature has begun to provide causal estimates, though it predominantly concentrates on legislative behavior (Micozzi, 2013; Gailmard and Jenkins, 2009), even as government heads don't just legislate but also execute policies.

We extend this debate by examining whether the shift from an indirect to a direct election system for village government heads affects the implementation of government programs in Maharashtra, a large state in western India with a population of 113 million. As is the case for many local governments across the world, Maharashtra's village governments have little sway over the creation of policies but have substantial sway on policy implementation and identification of beneficiaries. We therefore aim to estimate the causal effect

^{*}University of Wisconsin-Madison, pamar@wisc.edu

[†]University of Wisconsin-Madison, bhavnani@wisc.edu

[‡]University of Wisconsin-Madison, asjadhav2@wisc.edu

of direct election on the implementation of two of country's flagship programs in rural areas, on toilet building and housing construction. Performance in the implementation of government initiatives is crucial for elected representatives in decentralized government structures for several reasons. Incumbents can use it to garner support for their re-election bids (De Janvry et al., 2012). Additionally, good performance can help them move up the ladder within a political party and compete for higher electoral offices (Micozzi, 2013).

Why would direct elections affect program implementation? Existing theoretical models have examined differences between direct and indirect election models in terms of political selection, accountability, and re-election incentives (Persson and Tabellini, 2003; Micozzi, 2013; Gaebler and Roesel, 2019). Indirectly elected heads have a strong incentive to gratify the council members who elect them, whereas directly elected heads are motivated to be accountable to the preferences of the larger electorate. In this context, if the broader electorate wants what the government if offering, then directly elected representatives are strongly motivated to ensure efficient implementation. Furthermore, direct elections improve representation, allowing constituents—rather than just council members—to choose the head, thereby enhancing program implementation. On the other hand, elections are expensive; hence, direct elections may exclude the poor and decrease descriptive representation and political competition (Austen-Smith, 1987; Avis et al., 2022; Coate, 2004), which could potentially exacerbate program implementation (Gottlieb and Kosec, 2019).

Our identification strategy hinges on the variation in the timing of village council elections across the state. Village government elections follow a predetermined schedule and are held every five years. In Maharashtra village councils, similar to local governments in numerous other contexts, variations in election timing result are due to alterations in the boundaries, the establishment of new villages, the death or resignation of council members, and motions of no confidence, among other factors (Fukumoto and Horiuchi, 2011). Village heads were usually indirectly elected from among council members directly elected from smaller constituencies known as wards. However, between July 2017 and March 2020, and Auguts 2022 onwards, they were directly elected by voters. The timeline below illustrates the shifts in voting systems in Maharashtra's village councils.



1 Empirics

To examine performance in program implementation, we focus on two of the current government's flagship welfare schemes: the rural sanitation and house building programs. Since 2014, the BJP government has emphasized the public provision of private goods, including sanitation facilities (toilets) and housing. Village councils, particularly the village head, bear sole responsibility for identifying beneficiaries and, in conjunction with the local bureaucracy, facilitating the provision under these schemes.

1.1 Data

We rely on rich micro-datasets focusing on publicly provided private goods under these schemes. Firstly, we analyze data related to toilet construction as a part of the rural sanitation program known as Swachh Bharat Mission-Gramin (literally translating to Clean India Mission-Rural), initiated by the Government of India on October 2, 2014. The central aim of this initiative was to eradicate open defecation, a fairly common practice in rural India, and consequently, improve solid waste management. Accordingly, approximately 110 million households received financial support for constructing toilets under this program. The program primarily targeted households below poverty line and marginalized groups, such as Scheduled Tribes (STs). Our dependent variable is the number of toilets constructed in a village for a specific financial year. The histogram in Figure 1 shows all non-zero observations and that fall below the 99th percentile. The final dataset spans 24,567 village councils between 2013 and 2023.





Secondly, we utilize data on households benefiting from housing assistance under the Prime Minister's Rural housing scheme (also known as Pradhan Mantri Gram Awaas Yo-

jana or PMGAY). This program aims to provide durable permanent houses for families living in inadequately constructed dwellings made from materials such as bamboo, mud, grass, reed, stones, thatch, straw, leaves, and unfired bricks. Our dependent variable is the number of beneficiaries (overall and by social group) in a village for a specific financial year (See the histogram of the main PMGAY outcomes in Figure 2).¹ The final dataset on beneficiaries covers 22,572 village councils from 2012 to 2021.



Figure 2: Distribution of no. of beneficiares under PMGAY

We combine these datasets with village council election data sourced from the State Election Commission (SEC) of Maharashtra. The start date of the electoral term is used to determine whether the village head was elected directly or indirectly. Figure 3 shows variation in start date of the term of village councils.

1.2 Empirical Strategy

To estimate the causal effect of direct elections, we use the Callaway and Sant'anna estimator (hereafter, CS estimator) for staggered treatment adoption. Similar to other designs, this approach addresses empirical challenges posed by staggered designs with heterogeneity in treatment effects over time. The traditional Two-Way Fixed Effects Difference-in-Differences (TWFE DID) estimator may yield biased estimates if treatment effects vary over time, as early-treated observations function as controls even though their previous

¹Histograms of all non-zero observations and observations less than the 99th percentile.



Figure 3: Histogram of term start dates of village councils

treatment status should alter their trends. To avoid such biases, the CS estimator considers all treated units in a given year as a group and compares them with never-treated units to compute the average treatment effect on the treated for each group.

To implement this approach, a two-step estimation procedure is proposed. It calculates the group-time average treatment effects on the treated in the following manner (simplified version):

$$AT\hat{T}(g,t) = \frac{1}{n_g} \sum_{i=1}^n \mathbf{1}\{G_i = g\}\}(Y_{it} - Y_{ig-1}) - \frac{1}{n_U} \sum_{i=1}^n \mathbf{1}\{U_i = 1\}(Y_{it} - Y_{ig-1})$$
(1)

where Y_{it} represents the outcome of interest for unit *i* in period *t*, G_i indicates the group to which a treated unit belongs, which is determined by the year the unit becomes treated, with n_g being the number of observations in that group. The indicator $U_i = 1$ denotes never-treated units, and n_U is the number of observations in the never-treated group. Y_{ig-1} corresponds to the outcome for unit *i* in period g - 1, the year before the first group is treated. In practice, we use the recommended (default) doubly-robust CS estimator, which combines regression and propensity score methods to obtain precise and stable estimates (Callaway and Sant'Anna, 2021).

Using ATT(g, t) as a building block, the CS estimator then aggregates them to compute

the overall average treatment effects of the treated in the following manner:

$$A\hat{T}T^{O} = \sum_{g \in G} \sum_{t=g}^{t=\tau} w^{O}(g,t) AT\hat{T}(g,t)$$
⁽²⁾

and dynamic treatment effects for *e* periods since the treatment in the following manner:

$$A\hat{T}T^{ES} = \sum_{g \in G} w^{ES}(g, e) AT\hat{T}(g, t)$$
(3)

where $w^O(g,t)$, $w^{ES}(g,e)$ are the weights used in aggregation for computing $A\hat{T}T^O$ and $A\hat{T}T^{ES}$, respectively. A key identifying assumption is that the trends between treated and control units are parallel conditional on pre-treatment covariates. We test the assumption by comparing the changes in treated and control units in the pre-treatment period.

2 Results Overview

Figure 4 shows the coefficients of the CS DID event study estimates of direct elections on toilet construction. The results suggest that the direct election of village heads increases toilet construction under the Rural Sanitation Program by an average of 29%. We find substantial effects across the state but it is higher in scheduled areas, regions identified by a high share of the Scheduled Tribes (ST) population—a historically disadvantaged minority group and one of the primary target groups of the program. Additionally, all event-study estimates do not reveal any pre-trends.

Furthermore, Figure 5 presents the coefficients of the CS DID event study estimates of direct elections on the number of beneficiaries under PMAGY. The results do not show a statistically significant overall effect; however, we observe a significant increase in benefits for religious minorities. Additionally, there is a significant increase in benefits within scheduled areas as depicted in Figure 6. The event study estimates do not indicate any pre-trends in most pre-periods.

Next, we intend to explore mechanisms such as selection, by examining the characteristics of the village head, and accountability, by comparing village councils with and without temporary term limits due to the rotation of quotas. Additionally, we plan to examine heterogeneities in the relationship between direct election and program implementation based on the population of targeted groups like religious minorities and STs, as well as income concentration.



Figure 4: CS DID event-study: Toilet Construction under Rural Sanitation Program



Figure 5: CS DID event-study: Housing Benefits under PMAGY



Figure 6: CS DID event-study: Housing Benefits under PMAGY in Scheduled Area

References

- Austen-Smith, David, "Interest groups, campaign contributions, and probabilistic voting," *Public choice*, 1987, 54 (2), 123–139.
- Avis, Eric, Claudio Ferraz, Frederico Finan, and Carlos Varjão, "Money and politics: The effects of campaign spending limits on political entry and competition," *American Economic Journal: Applied Economics*, 2022, 14 (4), 167–199.
- Bernhard, William and Brian R Sala, "The remaking of an American Senate: The 17th amendment and ideological responsiveness," *The Journal of Politics*, 2006, 68 (2), 345–357.
- **Callaway, Brantly and Pedro HC Sant'Anna**, "Difference-in-differences with multiple time periods," *Journal of econometrics*, 2021, 225 (2), 200–230.
- Cheibub, José Antonio, Presidentialism, parliamentarism, and democracy, Cambridge University Press, 2007.
- **Coate, Stephen**, "Political competition with campaign contributions and informative advertising," *Journal of the European Economic Association*, 2004, 2 (5), 772–804.
- Crook, Sara Brandes and John R Hibbing, "A not-so-distant mirror: the 17th amendment and congressional change," *American Political Science Review*, 1997, 91 (4), 845–853.
- **Fukumoto, Kentaro and Yusaku Horiuchi**, "Making outsiders' votes count: Detecting electoral fraud through a natural experiment," *American Political Science Review*, 2011, 105 (3), 586–603.
- Gaebler, Stefanie and Felix Roesel, "Do direct elections matter? Quasi-experimental evidence from Germany," *International Tax and Public Finance*, 2019, *26*, 1416–1445.
- **Gailmard, Sean and Jeffery A Jenkins**, "Agency problems, the 17th Amendment, and representation in the Senate," *American Journal of Political Science*, 2009, 53 (2), 324–342.
- **Gottlieb, Jessica and Katrina Kosec**, "The countervailing effects of competition on public goods provision: When bargaining inefficiencies lead to bad outcomes," *American Political Science Review*, 2019, *113* (1), 88–107.
- Horowitz, Donald L, "Comparing democratic systems," J. Democracy, 1990, 1, 73.
- **III, Charles S Bullock and David W Brady**, "Party, constituency, and roll-call voting in the US Senate," *Legislative Studies Quarterly*, 1983, pp. 29–43.
- Janvry, Alain De, Frederico Finan, and Elisabeth Sadoulet, "Local electoral incentives and decentralized program performance," *Review of Economics and Statistics*, 2012, 94 (3), 672–685.
- Lijphart, Arend, "Democratization and constitutional choices in Czecho-Slovakia, Hungary and Poland 1989-91," *Journal of theoretical politics*, 1992, 4 (2), 207–223.

- Linz, Juan J, "Presidents vs. parliaments: The virtues of parliamentarism," Journal of Democracy, 1990, 1 (4), 84–91.
- **Meinke**, **Scott R**, "Institutional change and the electoral connection in the Senate: Revisiting the effects of direct election," *Political Research Quarterly*, 2008, *61* (3), 445–457.
- **Micozzi, Juan Pablo**, "Does electoral accountability make a difference? Direct elections, career ambition, and legislative performance in the Argentine Senate," *The Journal of Politics*, 2013, 75 (1), 137–149.
- **Persson, Torsten and Guido Tabellini**, "The economic effects of constitutions: what do the data say," 2003.
- Przeworski, Adam, Michael Alvarez, José Antonio Cheibub, and Fernando Limongi, "What makes democracies endure?," J. Democracy, 1996, 7, 39.
- **Riker, William H**, "The senate and American federalism," *American Political Science Review*, 1955, 49 (2), 452–469.
- Rogers, Lindsay, The American Senate, AA Knopf, 1926.
- **Rogers, Steven**, "The responsiveness of direct and indirect elections," *Legislative Studies Quarterly*, 2012, 37 (4), 509–532.