

Better methods of running subsidy programs: Evidence from the Karnataka schemes of 2023

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Abstract

Governments in developing countries spend heavily on subsidies, yet much of the money never reaches households: the macro–micro gap. We measure this gap in Karnataka, India, across a natural experiment. Karnataka has been delivering conditional Direct Benefit Transfers (DBT) for about a decade now. In July 2023 the state introduced new “guarantee” schemes, and directed much of its DBT allocation to a near-unconditional scheme. Because administrative records are endogenous to the state that produces them, we measure household receipts independently, from the CMIE Consumer Pyramids Household Survey. We compare robust population estimates from this survey against the state’s reported expenditure on DBTs in both periods. The gap averaged about 78% over the seven pre-2023 years and fell to about 17% after the shift. The paper contributes independently measured evidence on subsidy delivery in India and a replicable method for auditing welfare programs in the presence of longitudinal data.

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1 Introduction

A persistent gap separates government subsidy expenditure from what households actually receive. This macro–micro gap has long troubled policymakers and scholars (Deaton 2005; Meyer, Mok and Sullivan 2015). In 1985, Indian Prime Minister Rajiv Gandhi claimed that “of every rupee spent by the government, only 15 paise reached the intended beneficiary” (Press Trust of India 2017). Decades later, Finance Minister P. Chidambaram used a similar metaphor, arguing against sending more water down leaky pipes to describe the challenge of inefficient welfare delivery (Ahluwalia 2020). The scale of this leakage undermines the state’s ability to reduce poverty.

Over the past decade, India has attempted to fix these leaky pipes with a two-part reform. The state shifted social protection from in-kind subsidies toward cash transfers, and it built a new digital infrastructure to deliver them: Aadhaar-linked Direct Benefit Transfers, or DBT (Nilekani and Shah 2015). The DBT system was designed to reduce corruption and administrative friction by depositing cash directly into beneficiaries’ bank accounts. This brings fresh energy on the research question: how much of the money spent on these transfers now reaches households?

This paper measures the gap between the state’s actual expenditure and household receipts using a large-scale natural experiment in the state of Karnataka. For many years, Karnataka used DBT to deliver a range of conditional subsidies. While beneficiaries received cash directly in their bank account, they had to furnish multiple official documents to prove that they met every eligibility criteria. Administrators of these DBT schemes had high discretionary power. In July 2023, the state’s new government launched five large “guarantee” schemes and directed over sixty percent of the new DBT allocation to one of them: Gruha Lakshmi, a near-unconditional monthly cash transfer to the woman head of the household. The near-unconditional DBT had minimal eligibility criteria, which required beneficiaries to furnish significantly lower documents, and administrative discretion was much lower. This sharp policy change allows us to compare the performance of the two delivery models.

We audit the state’s performance using an independent, third-party household survey, the Centre for Monitoring Indian Economy’s Consumer Pyramids Household Survey (CPHS). Independent surveys are valuable because official administrative data on program delivery is endogenous to the political economy of the state (Sandefur and Glassman 2015). When a government’s performance is judged by the metrics it produces, those metrics can become unreliable, an instance of Goodhart’s or Campbell’s Law.

Our estimates reveal a large change in performance. We find the macro–micro gap (the share of the state’s actual DBT expenditure that did not reach households) averaged about 78% for the conditional DBT schemes before 2023. After the state concentrated its DBT expenditure in a near-unconditional scheme in July 2023, this gap fell to about 17%. The post-2023 average rests on two years that differ sharply. The gap all but vanished in the first year and reopened to 38% in the second, when actual DBT expenditure nearly doubled while reported receipts stayed flat. We read the fall with care. The CPHS measures income from government transfers the same way before and after 2023, so the fall reflects a change in delivery, not in what the survey captures. With one state and no control group, we cannot call design a proven cause, but the evidence points strongly to it. Section 7 returns to the problem.

This paper proceeds as follows. We motivate the need for independent measurement of subsidy delivery, review the literature on leakage and the macro–micro gap, describe the Karnataka 2023 natural experiment, explain the CPHS data and our population-estimation method, present the estimates of the macro–micro gap, and conclude with the policy implications for low-capacity states.

2 The macro–micro gap and subsidy reform

A large literature documents the macro–micro gap: a mismatch between aggregate economic data and household-level survey data that ought to measure the same flow. This gap appears in two distinct forms. The first is a measurement problem in national accounts versus poverty statistics. The second, which we study here, is a governance problem of fiscal leakage in subsidy programs.

2.1 Two strands of the macro–micro gap literature

One strand of the literature studies the gap between household consumption measured in surveys and consumption measured in national accounts. Survey-based consumption means typically fall about 20% below national-accounts consumption aggregates and about 50% below GDP per capita (Prydz, Jolliffe and Serajuddin 2021). Earlier cross-country work found the gap concentrated in income rather than expenditure surveys: Ravallion (2003) could not reject the hypothesis that national-accounts consumption per capita is an unbiased estimate of the mean from expenditure surveys, while income-survey means averaged only two-thirds of national-accounts consumption. Independent data from satellite night-lights tends to corroborate the growth patterns seen in national accounts, not those in surveys (Pinkovskiy and Sala-i-Martin 2016). In India, this discrepancy between National Sample Survey (NSS) consumption data and national accounts data fueled the “Great Indian Poverty Debate” (Deaton and Kozel 2005).

A second strand of the literature studies fiscal leakage¹, the gap between public expenditure on a subsidy program and the benefits that households actually receive. This gap can be enormous. In Uganda, a public expenditure tracking survey found that schools received only 13% of the capitation grants budgeted for them by the central government (Reinikka and Svensson 2004). In India, estimates suggest that about 40% of the grain allocated to the Public Distribution System (PDS) was diverted before reaching households (Khera 2011; Drèze and Khera 2015). Research on India’s National Rural Employment Guarantee Scheme (MGNREGA) shows that e-governance reforms can substantially reduce such leakage (Muralidharan, Niehaus and Sukhtankar 2016; Banerjee et al. 2020). We explore this second strand: the gap between actual expenditure and household receipts in large-scale subsidy programs.

1. “Leakage” refers to the gap between the value of subsidy funds or goods disbursed by the government and the value actually received by intended beneficiaries. This gap arises from several channels: outright corruption or diversion of funds or commodities to the open market, fraud (including ghost or duplicate beneficiaries), targeting and identification errors (exclusion and inclusion errors), and legitimate administrative and transaction costs incurred in delivery. See Barnwal (2024), Banerjee et al. (2020), and Muralidharan, Niehaus and Sukhtankar (2016) to understand various channels of leakage in the Indian context.

2.2 Two directions of subsidy reform

Two broad reform agendas address fiscal leakage. The first moves social transfers toward direct electronic delivery. The second moves them toward unconditionality.

One direction of reform is the shift to Direct Benefit Transfers (DBT), which use biometric identification and electronic payments to reduce the role of intermediaries. In India, the Aadhaar biometric ID system underpins this shift. India’s Direct Benefit Transfer for LPG (DBTL) replaced an over-the-counter price subsidy for cooking gas with a transfer into beneficiaries’ bank accounts, and this cut diversion to the black market: subsidized household gas sales fell 17–19%, commercial gas sales rose 13–14%, and black-market prices fell 18–20% when the government repealed the policy (Barnwal 2024). In Andhra Pradesh, paying MGNREGA wages through biometric smartcards reduced leakage without causing exclusion, provided a no-fail protocol was in place (Muralidharan, Niehaus and Sukhtankar 2016). A study of e-governance reforms in Bihar’s MGNREGA estimated that pure leakage accounted for about 19–24% of total expenditure (Banerjee et al. 2020). This technology is not a panacea, however. In Jharkhand’s PDS, imposing Aadhaar-based authentication was “pain without gain” because the primary source of leakage was in the supply chain, not in identity fraud (Drèze et al. 2017; Muralidharan, Niehaus and Sukhtankar 2025).

The other major direction of reform is to remove complex conditions from transfers. In one systematic review of cash transfers and *schooling* outcomes, unconditional transfers raised enrolment nearly as much as conditional ones. The binary difference between the two was not statistically significant, though programmes that explicitly monitored and enforced conditions performed substantially better (Baird et al. 2014). Unconditional transfers also reduce the compliance costs of monitoring for poorer households, which can exclude the most marginalized. In low-capacity states, fine-grained targeting is often infeasible and can be regressive in practice (Coady, Grosh and Hoddinott 2004). Conditional transfers are transaction-intensive and create opportunities for discretion and rent-seeking. Removing conditions simplifies delivery, reduces official discretion, and makes programs more feasible for states with limited administrative capacity (Pritchett and Woolcock 2004).

The natural experiment that we examine isolates the gain from unconditional cash transfers. The state of Karnataka has used DBT for subsidy delivery for several years. In 2023, its new government kept the DBT payment rails but directed most of the new allocation to one near-unconditional scheme. We are then able to assess the impact of removing conditionality, holding the electronic delivery mechanism largely constant.

3 Institutional Background and the Natural Experiment

The government of Karnataka implemented at least 14 significant direct benefit transfer (DBT) schemes between fiscal years 2016–17 and 2024–25. We detail these schemes in Table A.1. A common feature of this pre-2023 landscape was reliance on multiple eligibility gates. Beneficiaries had to produce income certificates, caste documents, or proof of employment to qualify for transfers.

In July 2023, the state government announced five large subsidy schemes, branded as

“guarantees”. This announcement created a sharp discontinuity in social protection policy. Three of the five guarantees involved a direct cash transfer: Gruha Lakshmi, a transfer to the woman head of household; Yuvanidhi, a transfer to unemployed graduates; and Annabhagya, a cash transfer to ration-card holders to complement grain distribution. The allocation was not spread evenly across the three. Gruha Lakshmi alone accounted for over 60% of the state’s DBT expenditure in each post-2023 year (Table A.1), and it was near-unconditional.

3.1 Delivery mechanism: old versus new

The 2023 policy shift changed the mechanism for delivering cash transfers. Before 2023, transfers were conditional. A household had to prove its eligibility, and a government official had to verify compliance. This verification step gave officials high discretion. It created a channel for leakage through both exclusion and inclusion errors: officials could deny transfers to genuinely eligible households or extract side-payments to admit ineligible ones.

Gruha Lakshmi, which dominates the post-2023 DBT expenditure, uses a different mechanism with near-unconditional eligibility. The process involves a simple portal registration, de-duplication using the national biometric ID (Aadhaar), and direct credit to a bank account. This shorter chain removes the compliance-verification step and minimizes the role of official discretion. The primary remaining channel for leakage is identity or de-duplication error, not official rent-seeking.

The crucial difference is that a near-unconditional scheme, Gruha Lakshmi², now dominates the DBT expenditure. In a state with limited administrative capacity, a delivery process with high transaction intensity and official discretion is prone to leakage (Pritchett and Woolcock 2004). By simplifying eligibility and automating delivery, Gruha Lakshmi tests whether a less conditional approach can reduce such leakage.

Figure 1 provides descriptive evidence consistent with this change in mechanism. The figure plots the fraction of households in the CPHS survey data that report receiving income from government cash transfers. After the introduction of the 2023 guarantee schemes, this fraction rises sharply. The increase suggests that the new schemes, with their broader eligibility and direct bank transfers, reached a much larger share of the population than the conditional schemes that came before.

We now turn to our research question. *Did concentrating DBT expenditure in a near-unconditional cash transfer reduce the gap between the government’s actual expenditure and the income households reported receiving?* The remainder of this paper answers this question.

2. Under this scheme women recognised as heads of households on the household ration cards received a monthly transfer of Rs. 2000. The scheme excluded households where their husbands were income tax payers or filed Goods and Services Tax returns. We classify this scheme as near-unconditional due to its simple and minimal eligibility criteria.

Figure 1 Fraction of CPHS households in Karnataka that received income from government cash transfers, monthly data. The share rises sharply after the July 2023 guarantee schemes.



4 Data

4.1 Why independent survey measurement

Administrative records of welfare delivery are endogenous to the political economy of the state that produces them. When a measure becomes a target, it ceases to be a good measure—a phenomenon known as Goodhart’s Law. Similarly, Campbell’s Law holds that the more a quantitative social indicator is used for decision-making, the more it is subject to corruption pressures (Best 2021). Low state capability thus affects not only program implementation but also the administrative data used to measure it. Sandefur and Glassman (2015) document this directly: comparing administrative statistics against independent household surveys across a large sample of African countries, they find that official figures systematically overstate progress wherever funding is tied to reported performance. The correct response to this endogeneity is to use an independent, third-party survey. We use the Consumer Pyramids Household Survey (CPHS) for this purpose.

4.2 The CPHS data

The CPHS, conducted by the Centre for Monitoring Indian Economy (CMIE), is a long-running, high-frequency panel survey of Indian households. It began in 2014 and surveys a nationally representative sample of approximately 175,000 households in three waves each year. The survey’s breadth and quality have made it an important data source for research on the Indian economy, spanning public health (Du et al. 2022; Malani 2022; Kaicker 2025; I. Patnaik et al. 2023), labour economics (Abraham and Kesar 2025; Dev and Unni 2024; Jha and Basole 2023; Vyas 2018), household finance (Pandey, Patnaik and Sane 2019), and measurement methods (Patnaik, Sane and Shah 2019; A. Patnaik et al. 2021; Sun et al. 2026).

Our analysis uses the CPHS sample for the state of Karnataka. Table 1 reports the number of households approached and the response rates for Karnataka and all of India from financial year (FY) 2016–17 to 2024–25. In a typical year, the Karnataka sample comprises 9,000–10,500 households approached.³ As Table 1 shows, response rates in Karnataka fell during the COVID-19 pandemic, from 86% in 2018–19 to about 46% in 2020–21, mirroring the all-India pattern. Rates have partially recovered since. This drop in response rates is a caveat for our analysis and motivates the use of robust estimation methods.

Table 1 Households approached per financial year and response rates, CPHS.

FY	Karnataka		India	
	Approached (N)	Responded (%)	Approached (N)	Responded (%)
2016–17	9024	86.23	160263	83.11
2017–18	9426	84.73	165370	82.50
2018–19	9682	86.39	172867	85.30
2019–20	9717	83.05	174405	82.42
2020–21	9941	46.17	174970	60.13
2021–22	10549	57.66	178453	70.54
2022–23	10549	74.50	178677	73.16
2023–24	10138	67.62	173325	61.02
2024–25	8852	52.55	156413	68.21

4.3 The transfer variable and its distribution

Our primary outcome variable is monthly household income from government transfers, captured by the CPHS variable `INC_OF_HH_FRM_GOVT_TRF`. This variable captures money received through Direct Benefit Transfers (DBTs). It excludes salaries and pensions received by government employees and payments from the Mahatma Gandhi National Rural Employment Guarantee Act (MGNREGA).

Table 2 presents summary statistics for this variable for the pooled sample of Karnataka households from FY2016–17 to FY2024–25. The distribution of transfer income is extremely sparse and right-skewed. The median household reported zero income from government transfers in every pre-guarantee year except FY2018–19, when the median was Rs 100. Mean monthly transfer income was low in the pre-guarantee period, ranging from Rs 19 to Rs 150 per household.

Following the introduction of the guarantee schemes, this mean jumped to Rs 1,113 in 2023–24 and Rs 1,418 in 2024–25. The median also rose sharply from zero to Rs 510 in 2023–24 and Rs 2,000 in 2024–25. The presence of a few households reporting very large receipts each year, shown in the final column of Table 2, is addressed using outlier-robust estimation techniques.

3. The number of households approached per financial year is estimated as the mean monthly sample size for that year multiplied by four, corresponding to the four-month recall period of each survey wave. The response rate is the mean of the monthly response rates for the financial year.

Table 2 Summary statistics for monthly household income from government transfers, Karnataka, pooled FY sample, nominal Rupees. N is the number of monthly household observations; $N_{>10000}$ counts observations that report income from government transfers above Rs 10,000.

FY	Min	Q1	Mean	Median	Q3	Max	N	$N_{>10000}$
2016–17	0	0	19	0	0	22500	96350	22
2017–18	0	0	101	0	180	18000	93473	7
2018–19	0	0	143	100	250	50220	101075	5
2019–20	0	0	131	0	240	12000	81238	5
2020–21	0	0	151	0	0	94000	60203	17
2021–22	0	0	137	0	0	35000	84150	8
2022–23	0	0	89	0	0	40000	93271	18
2023–24	0	0	1113	510	2340	20000	72550	5
2024–25	0	0	1418	2000	2510	20000	58260	2

5 Methodology

Our goal is to estimate the total value of government transfers received by all households in Karnataka for each financial year. We construct this population total from the CPHS microdata. The standard approach is to scale each household’s reported transfer income by its survey weight and sum these weighted values to a state-level aggregate. This method, however, is fragile for survey data on transfers.

Two features of the CPHS transfer data make a simple weighted sum unreliable. The first is sparsity. Most households in any given year report receiving zero transfer income; the median was zero in every pre-guarantee year except FY2018–19, where it was Rs 100. The second problem is outliers. A few households report very large receipts, perhaps due to lump-sum payments of arrears or data entry errors. When multiplied by their large survey weights, these few observations can exert an outsized influence on the population total.

To address the influence of outliers, we compute four different population estimates. We use one as our main estimate and the other three as robustness checks.

1. **Base estimate.** This is the simple weighted total described above, using all observations in a financial year scaled by their household weights.
2. **Main estimate: 1% winsorised weighted total.** This is our preferred estimate, used throughout the results section. We first scale each observation by its household weight, then winsorise the top 1% of these weighted observations. This procedure bounds the influence of extreme values while retaining their contribution to the total. Our results are not sensitive to this 1% threshold; the other robust estimators, which use different approaches to limit outliers, yield similar totals.
3. **Huber M-estimator.** As a first robustness check, we use a Huber M-estimator to limit the influence of extreme transfer values. This robust estimator down-weights observations in the tails of the skewed distribution (Hulliger 1995). We use a tuning constant of $k = 5$.
4. **Non-zero, winsorised estimate.** As a second robustness check, we drop all households reporting zero transfer income. We then generate a winsorised population

estimate from the remaining observations, setting the upper bound at 0.95.

Further details on the software used to create these estimates are in Appendix B. Figure 2 in the next section shows that all four estimators track each other closely over the study period, giving us confidence in the stability of our main approach.

We define the macro-micro gap by comparing our CPHS-based estimate of household transfer receipts against official government expenditure. The macro-level data come from the Karnataka State Budget Expenditure Volumes, which report actual expenditure on all Direct Benefit Transfer (DBT) schemes. These figures are actual, released expenditure, not budget estimates. We define underpayment as the difference between actual DBT expenditure and our survey-based estimate of receipts.

$$\text{Underpayment} = \text{DBT actuals} - \text{CPHS transfer estimate} \quad (1)$$

We express this gap as a percentage of actual DBT expenditure.

$$\text{Underpayment (\%)} = \frac{\text{Underpayment}}{\text{DBT actuals}} \times 100 \quad (2)$$

A positive value for this underpayment percentage indicates that households received less in total than the state actually spent.

6 Results

The gap between actual social expenditure and household receipts fell sharply after Karnataka concentrated its DBT expenditure in a near-unconditional guarantee scheme in mid-2023. Before the policy shift, we estimate that households received only about one-fifth of the state’s actual direct benefit transfer (DBT) expenditure. After the shift, receipts matched actual expenditure in the first year and fell back to about three-fifths of it in the second.

Transfers received by households rose sharply. This is expected, since Karnataka’s DBT expenditure more than doubled from 2022-23 to 2023-24. Figure 2 shows our population estimates of income from government transfers in Karnataka from fiscal year (FY) 2016–17 to 2024–25. The vertical line marks the introduction of the guarantee schemes in July 2023. Before this date, estimated receipts were low and flat. After this date, they jumped sharply. The four different estimators—our main 1% winsorised estimator, a base estimator, a robust Huber estimator, and a non-zero winsorised estimator—all track each other closely.

Table 3 compares the state’s actual DBT expenditure with our CPHS-based estimate of household receipts.⁴ In the pre-period, from FY2016–17 to FY2022–23, the underpayment was consistently large, averaging 77.9% over the seven years, with a standard deviation of 10.3 percentage points. The gap ranged from a low of 67.0% in FY2020–21 to a high of 98.3% in FY2016–17.

The post-period had a significantly lower underpayment, averaging to 17.0%. This average is based on two financial years with varying underpayment percentage (-4.1%

4. DBT actuals are the total actual expenditure on all DBT schemes reported in the Karnataka State Budget Expenditure Volumes, not budget estimates. CPHS transfer receipts are the 1% winsorised, weighted population total from our calculations (the main estimate).

Figure 2 Robust population estimates of income from government transfers in Karnataka using CPHS (Rs. Crores). The four estimators track each other closely. The dashed vertical line marks the introduction of the July 2023 guarantee schemes. Source: CMIE CPHS; authors' calculations.

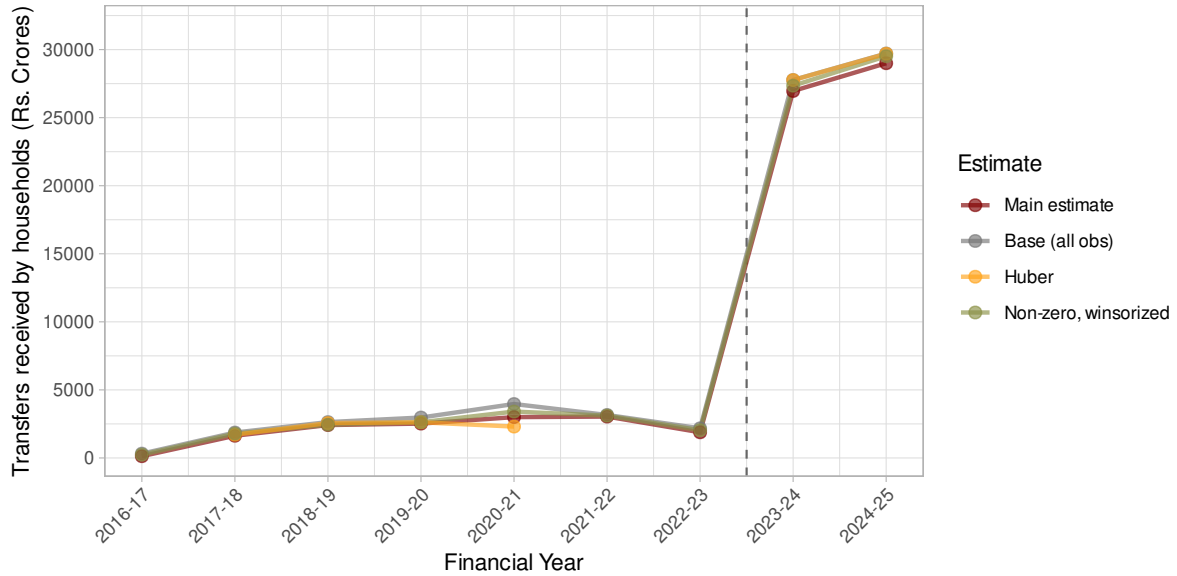
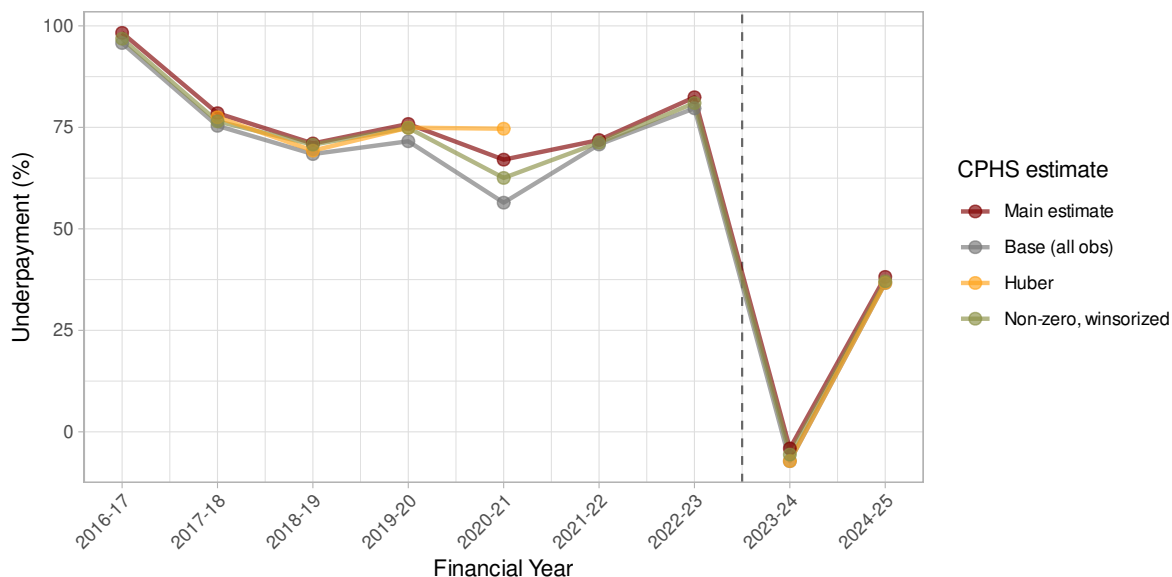


Table 3 Karnataka DBT actual expenditure versus CPHS-estimated household receipts, Rs. Crores. Underpayment = DBT actuals – CPHS transfer; Underpayment (%) = Underpayment / DBT actuals $\times 100$. Pre-period (2016–17 to 2022–23) average underpayment is 77.9% ($n = 7$, SD 10.3 percentage points); post-period (2023–24 to 2024–25) average is 17.0% ($n = 2$, SD 29.9 percentage points), and the two post-period years differ sharply.

FY	DBT actuals	CPHS transfer	Underpayment	Underpayment (%)
2016–17	7505.6	129.1	7376.4	98.3
2017–18	7578.4	1629.4	5948.9	78.5
2018–19	8340.0	2415.8	5924.2	71.0
2019–20	10444.4	2523.6	7920.8	75.8
2020–21	9099.0	2998.2	6100.8	67.0
2021–22	10807.2	3039.1	7768.1	71.9
2022–23	10722.8	1884.8	8838.0	82.4
2023–24	25897.1	26959.5	-1062.4	-4.1
2024–25	46901.8	28999.5	17902.2	38.2

for FY2023–24 and 38.2% for FY2024–25). It is possible that underpayments reduced in the post-period due to households having better recall. Survey respondents have better recall for income from transfer that they receive every month, as opposed to income from sporadic lumpsum transfers, which they were receiving in the pre-period. Moreover, politicians are incentivised to do subsidy delivery through large concentrated schemes, to have better recall from their voters. That said, subsidy schemes coming out of election promises also put positive pressure on the bureaucracy for timely and efficient delivery. Both explanations can lead to reduced underpayment in the post-period. Figure 3 plots the underspend per financial year from Table 3, and adds to it by considering all four CPHS estimates to calculate the underpayment.

Figure 3 Annual macro–micro gap: Karnataka DBT underpayment (%). Pre-period average 77.9% ($n = 7$, SD 10.3 percentage points); post-period average 17.0% ($n = 2$, SD 29.9 percentage points). The two post-period years, -4.1% and 38.2% , differ sharply. Source: Karnataka State Budget; CMIE CPHS; authors’ calculations.



The two years post-2023 disagree, and the disagreement matters. Between them actual DBT expenditure nearly doubled, from Rs 25,897 crore to Rs 46,902 crore, while estimated receipts stayed flat at about Rs 27,000 to Rs 29,000 crore. Delivery may not have kept pace: FY2024–25 carried a full year of Gruha Lakshmi and added Annabhagya (DBT). Or new kinds of corruption emerged that increased the leakage. We cannot separate the two explanations. Both post-period years still sit far below the pre-period range of 67.0% to 98.3%.

The composition of the DBT basket changed along with its design. Gruha Lakshmi, which pays Rs 2,000 a month to the woman head of the household, alone accounts for 65.5% of DBT expenditure in FY2023–24 and 63.1% in FY2024–25 (Table A.1). No scheme of that kind existed before 2023. The old basket ran instead on lumpy, one-off payments: housing grants, sanitation, and producer incentives. Section 7 takes up what this change of composition does to our reading of the gap.

6.1 Why a residual gap persists

The post-period gap narrowed sharply, but it did not close. Two phenomena likely explain much of the residual gap: targeting errors and household under-reporting.

First, even well-designed cash transfer programs suffer targeting errors: some eligible households are excluded, some ineligible ones included. Thomas, Uday and Zaveri (2020), studying a different DBT scheme in India, document village-level inclusion and exclusion errors of 9–20% even in a scheme with careful preparation. Administrative spending totals will never match the receipts of the correctly targeted population.

Second, household surveys consistently find under-reporting of transfer income due to errors in household reporting (Meyer, Mok and Sullivan 2015). Respondents may forget small or irregular payments received months earlier, or they may be reluctant to report government benefits. Such measurement error depresses our estimate of total receipts, creating an apparent gap even if delivery were perfect.

7 Discussion and conclusion

We draw four conclusions from our analysis. Our first conclusion is methodological. Administrative data on welfare program delivery are not neutral records of fact; they are endogenous to the political incentives of the state. To measure leakage accurately, these official accounts must be juxtaposed against independent data. We show that high-frequency household surveys, such as the CMIE Consumer Pyramids Household Survey allow a near real-time measure of the gap between actual expenditure and household receipts.

Second, we diagnose the pre-2023 gap in Karnataka. The state had delivered cash through direct benefit transfer (DBT) systems for years before 2023. Yet we estimate that the macro–micro gap averaged about 78% of actual social security spending. The payment “rails” were in place, and still most of the money did not reach households. This shows the need for deep reforms of subsidy programs and their implementation: the mere addition of DBT did not suffice. The likely culprit is the complex eligibility rules and conditions attached to the dozens of small, pre-existing schemes, which open opportunities for diversion.

Third, the macro–micro gap contracted sharply after the 2023 guarantee schemes arrived, falling to about 17% on average. The fall was large but uneven. The gap all but vanished in the first year and reopened in the second. The new basket ran mostly through Gruha Lakshmi, a near-unconditional monthly transfer, and shed the conditions that had fragmented the old system. The *design* of a cash transfer, and not merely the existence of a DBT mechanism, governs how much money reaches households.

The fall reflects delivery, not measurement. We measure transfer income with the same CPHS variable, by the same method, before and after 2023. The instrument did not change; the schemes did. A sharp fall in the measured gap therefore reflects a change in what households received, not a change in what the survey captures. Two limitations remain. The CPHS pools all transfer income into one variable, so we cannot isolate a gap for Gruha Lakshmi alone, and Karnataka has no control group. We read the fall as strong evidence that near-unconditional design cut leakage.

The finding still carries a policy implication for low-capacity states. Conditional cash transfers (CCTs) are susceptible to leakage. They are transaction-intensive and require high levels of administrative discretion to verify eligibility and compliance, creating multiple points for rent-seeking, especially where state capacity is weak (Pritchett and Woolcock 2004). The 2023 shift put most DBT expenditure into a near-unconditional scheme. Reducing conditionality reduced discretion and, on this account, leakage. Our results are consistent with that simplification, though they cannot establish it on their own. This evidence aligns with one systematic review of cash transfers and schooling outcomes, which found the average difference between CCTs and UCTs statistically insignificant and highly context-dependent, though it also found that where conditions are actively monitored and enforced, conditional transfers did outperform unconditional ones (Baird et al. 2014).

Our study has limitations. The aggregate expenditure data covers all DBT schemes, while the 2023 policy shock was concentrated in three large guarantee schemes. Furthermore, CPHS response rates declined during the COVID-19 pandemic, though our estimators are robust to this. The post-2023 period rests on only two years, and the two point estimates disagree. A residual gap remains, likely reflecting a combination of targeting errors and some under-reporting of receipts by households (Meyer, Mok and Sullivan 2015). Our conclusion is therefore one of substantially reduced leakage, not its elimination.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

The CMIE Consumer Pyramids Household Survey microdata used in this study is proprietary and is available from the Centre for Monitoring Indian Economy under a commercial licence; the authors cannot redistribute it. The Karnataka DBT expenditure figures are drawn from the publicly available Karnataka State Budget Expenditure Volumes.

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A Top DBT scheme actuals by financial year

Table A.1 reports the largest direct benefit transfer (DBT) schemes in Karnataka by actual expenditure, in Rs. Crores, for FY2016–17 to FY2024–25. Schemes with variant names across years are consolidated; “–” indicates zero expenditure for that financial year. The actual expenditure totals in Table 3 correspond to the “Total” row.

Table A.1 Top DBT scheme actuals by FY (Rs. Crores).

Scheme	16–17	17–18	18–19	19–20	20–21	21–22	22–23	23–24	24–25
Gruha Lakshmi	–	–	–	–	–	–	–	16,964	29,608
New Social Security (Sandhya Suraksha) (NSAP)	1,395	1,474	2,201	3,383	3,469	3,603	4,001	4,348	4,346
Incentive to Milk Producers	994	1,157	1,460	1,261	1,186	1,403	1,217	1,181	1,693
Monthly Fin. Assist., Physically Challenged (NSAP)	829	873	1,060	1,172	1,172	1,120	1,266	1,443	1,572
Ashraya-Basava Vasathi	1,252	1,281	654	1,461	600	1,600	265	575	550
DBT under Annabhagya Scheme	–	–	–	–	–	–	–	–	7,858
Dr. B.R. Ambedkar Nivas Scheme	823	812	1,340	1,077	750	590	570	450	420
Pradhan Mantri Kisan Samman Yojane	–	–	–	1,487	1,876	2,027	960	–	–
Swachha Bharath Mission	912	1,680	970	–	–	–	–	–	–
PMAY-Urban	–	–	–	–	–	368	864	625	6
PMAY-Grameena	963	–	361	310	–	–	409	–	463
Mukhya Mantri Raitha Vidyanidhi Programme	–	–	–	–	–	–	599	–	2
Dr. BR Ambedkar Housing + PMAY(U)	–	–	–	–	–	–	435	132	161
Bhagya Lakshmi	338	302	294	294	47	97	137	179	224
Total	7,506	7,578	8,340	10,444	9,099	10,807	10,723	25,897	46,902

B Statistical packages used for population estimation

We compute the four population estimators in R as follows.

1. **Base (all observations).** Using the `survey` package (Lumley 2026): each observation in a financial year is scaled by its household weight and summed to the year total.
2. **Main estimate.** Using `survey` (Lumley 2026) with 1% winsorisation: after weighting, the top 1% of the weighted observations are winsorised before the year total is computed.
3. **Huber.** Using `robsurvey::svytotal_huber` (Hulliger and Schoch 2026): the influence of extreme transfer values is limited with a tuning constant of $k = 5$, accounting for the skewed distribution of the transfer variable.
4. **Non-zero, winsorised.** Using `robsurvey::svytotal_winsorized` (Hulliger and Schoch 2026): observations with zero transfer income are dropped, and a winsorised population estimate is generated with the upper bound set to 0.95.