

Pre-analysis plan II: Examining the Impact of Reconciliation in the Public Distribution System (PDS) in Jharkhand

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1 Context and intervention

A previous pre-analysis plan, “Evaluation of the Impact of the Introduction of Electronic Point of Sale (ePOS) Machines in the Public Distribution System (PDS) in Jharkhand,”¹ specified procedures for analyzing data from the first endline survey conducted as part of this evaluation as well as related administrative records. This analysis was focused on estimating the effect of the introduction of biometric point-of-sale (POS) devices into the PDS. The government experimentally staggered the introduction of these devices, with “treated” FPS receiving them in August - November 2016 while control FPS received them in May - June 2017, allowing for experimental estimation of impacts.

In the principle, the rollout of ePOS devices enabled the government to alter its monthly disbursements to FPS by reconciling records of past disbursement with the electronic records of past offtake by beneficiaries captured in the POS device. For example, an FPS expected to disburse 100kg of rice per month that uploaded 60kg worth of authenticated transactions from its POS device could be shipped an incremental 60kg the next month, rather than the full 100kg. The government did not roll this practice out along with the devices themselves, but instead chose to wait until they had been universally deployed before commencing with reconciliation. This pre-analysis plan specifies methods for examining the impact of this subsequent policy change.

Reconciliation was introduced into the Jharkhand PDS as follows. Prior to July of 2017 the government did not communicate with FPS dealers or program participants about the possibility of reconciliation, expecting that this would be unpopular and reduce acceptance of the ePOS devices themselves. Of course, dealers may have anticipated reconciliation nonetheless.

In July of 2017 the government began adjusting disbursements of wheat and rice (but not other commodities) downward for the first time, reflecting cumulative records of commodities not disbursed since the deployment of ePOS devices in each FPS (a period of 8-11 months for treated blocks and 1-2 months for control blocks). The precise formula specified is provided below; in essence, it reduced disbursements by the amounts of grain that beneficiaries had not collected from their entitlements, excepting the previous month’s entitlement. This meant that older entitlements were not honored, and might also mean that current entitlements could not be honored if grain had in fact been diverted and was no longer sitting in the ration shop.

In practice, we heard that many shops were given “exemptions” as taken literally this policy would have meant no commodities available in July, since past surpluses had not

¹<https://www.socialscicenter.org/trials/1620/history/24690>

been stored but diverted in many shops. Reconciliation continued in August, September and October, allegedly with fewer exemptions granted. Resistance to the policy continued, however, and in November the government reverted to disbursing full monthly allotments of all commodities to each FPS with no reconciliation conducted.

2 Research questions

The primary research questions are to understand (a) how the rollout of the reconciliation policy was implemented in practice, and (b) how this in turn affected the value disbursed by the government and the value received by beneficiaries (and thus the difference between these quantities, i.e. leakage). Impacts on the costs of delivering this value, one of the primary outcome concepts in the previous analysis plan, are also relevant but less central here since reconciliation did not directly affect the checkout process.

We expect impacts to vary over time, both because compliance with the policy is said to have varied over time as described above and also because the effects of full compliance in the policy should in theory exhibit transitional dynamics, as characterized in the conceptual framework below. A secondary goal is thus to distinguish transitional dynamics from steady-state impacts.

3 Data sources

We use three primary sources of data: government records of disbursements of commodities to the FPSs, government records of transactions conducted on ePOS devices at each FPS, and data from original surveys we conducted of ration card holders and FPS dealers. The following chart summarizes the time periods covered by the various data sources we use and their relationships with the rollout of the ePOS and reconciliation interventions.

	2017											
	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
Interventions												
ePOS (T blocks)												
ePOS (C blocks)					*							
Reconciliation												
Data coverage												
Survey	EL1			EL2				EL3				
Transactions (T blocks)												
Transactions (C blocks)					*							
Disbursements	Manual			Digital / NIC								

* Roll-out began in a very few places at the end of May

Three points deserve highlighting. First, our survey data come from three separate endline rounds (EL1, EL2 and EL3) all conducted in-person and each covering a distinct recall period and collectively giving us continuous coverage from January through November of 2017. Of these, the second endline had the longest recall period, asking respondents about outcomes as many as 5-6 months previously. Second, we observe ePOS transaction data only in FPS where ePOS devices had been rolled out, and thus observe these data for fewer months in blocks assigned to control in the ePOS evaluation than for those assigned to treatment. Finally, our preferred source of disbursement data changes over time. Prior to April 2017 the only available data were kept at individual block offices in a variety of formats, sometimes on paper; we therefore collected these data and manually entered them. Starting in April 2017 the National Informatics Commission began systematically collecting these data and storing them digitally, and we therefore rely on their records.

In addition to these primary data sources we also expect to make use of several ancillary sources including the statutory commodity prices and entitlement amounts for 2017 and the full list of beneficiaries.

4 Empirical methods

The analysis proceeds in two steps. We first examine the extent to which the government adhered to the reconciliation policy it had specified, and characterize any correlates of deviations from this policy; one can think of this as helping to characterize the “first stage”

impact that the rollout of the de jure policy had on de facto behavior. We then examine the impact this had on ultimate outcomes.

4.1 Adherence

To examine adherence, we characterize the joint distribution of the target quantity D_{cft} of commodity c that we calculate the government should have disbursed to a given Fair Price Shop f in a given month t with the actual quantity \hat{D}_{cft} it disbursed. Dropping commodity and ration shop indices for simplicity, the target amount for reconciled commodities (i.e. wheat and rice) is given by

$$D_t = \max(0, E_t - S_{t-1}) \quad (1)$$

$$S_t = S_{t-1} + \hat{D}_t - O_t - C_{t+1} \quad (2)$$

$$C_{i,t} = \begin{cases} E_{i,t-1} - (O_{i,t-1} - C_{i,t-1}) & O_{i,t-1} > C_{i,t-1} \\ E_{i,t-1} & O_{i,t-1} \leq C_{i,t-1} \end{cases} \quad (3)$$

where

- D_t is the amount that should have been disbursed at the beginning of period t according to policy
- $E_t = \sum_i E_{i,t}$ is the total amount entitled to recipients assigned the FPS in period t
- S_t is the “effective stock” the government believes the FPS should hold at the end of period t available to meet the next period’s obligations. This is equal to the stock the FPS should physically be holding if no diversion took place (i.e. $S_{t-1} + \hat{D}_t - O_t$), less the amount required to meet carryover commitments C_{t+1} in the next period
- $C_t = \sum_i C_{i,t}$ is the total carryover commitment owed to recipients in period t because they did not collect their entitlement in period $t - 1$. Note that this is defined in such a way that offtake is interpreted as first accruing against carryover commitments from the previous period until these have been exhausted, and then accruing against current period commitments.
- $O_t = \sum_i O_{i,t}$ is the total offtake of the commodity by recipients from the FPS in period t

For commodities not subject to reconciliation (salt, sugar and kerosene), on the other hand, these relationships are simply

$$D_t = E_t \quad (4)$$

We will report various plots and summary statistics of the joint distribution of (D_t, \hat{D}_t) . We will also test for systematic heterogeneity over time, commodities and fair price shops by estimating

$$\hat{D}_{cft} - D_{cft} = \alpha + \delta_t + \gamma_c + \rho_f + \epsilon_{cft} \quad (5)$$

and

$$|\hat{D}_{cft} - D_{cft}| = \alpha + \delta_t + \gamma_c + \rho_f + \epsilon_{cft} \quad (6)$$

and reporting F -tests of the null that the time, commodity, and FPS indicators are jointly zero, respectively. Finally, we will examine whether certain types of FPS are systematically more likely to receive allocations that deviate from their statutory level by running regressions as above but including the same FPS-level characteristics that we used to examine heterogeneity in the earlier pre-analysis plan: an indicator for above/below median cellular network signal strength, an indicator for being in an urban location, and indicators for machine operating mode (online, partially online, or offline).

4.2 Impacts

We estimate impacts on a subset of the outcomes defined in the earlier pre-analysis plan for the ePOS evaluation, excluding some which were either confusing to recipients to elicit or seemed unlikely to be affected by reconciliation.² We make one modification to outcome definitions: for outcomes that are aggregates over the commodities, we calculate these separately for wheat and rice (which were subject to reconciliation) and for sugar, salt and kerosene (which were not).

To examine impacts we have available only time-series variation in the policy rollout. We therefore estimate

$$Y_{hfbst} = \alpha_{hfs} + \gamma t + \beta_R R_t + \beta_{Rt} R_t(t - t^*) + P_t + \epsilon_{hfbst} \quad (7)$$

where R_t is an indicator equal to one if disbursements for month t were calculated using the reconciliation formula (i.e. for July through October), t^* is the first month of reconciliation (i.e. July), and P_t is an indicator for the one post-reconciliation month in our data (i.e. November).³ We will report standard errors clustered by FPS and within outcome families will report both standard p -values and q -values adjusted to control the false discovery rate. This specification embodies the following major assumptions:

²Specifically, we drop V2 (households stated willingness to accept in lieu of benefits received per month), C1 (total commodity delivery cost per household per month), F1-3 (food security outcomes), D1 (average monthly profit FPS dealer profit), and S2a-e (median local market commodity prices).

³If available we may also include time-varying controls X_{hfbst} .

- We assume treatment is identified once we control for a linear pre-trend. This is obviously a strong assumption, but the best that is realistic without experimental variation and with only 6 months of pre-treatment data. That said, in earlier work evaluating the impact of ePOS itself we found little evidence of any trends in outcomes for the first 3 months, which suggests that a simple linear trend may if anything be more than sufficient.
- We treat November as a distinct treatment, and in particular do not impose that outcomes immediately revert to what they would have been absent the intervention. While the latter assumption would significantly improve power if true, we find it implausible.
- We model the potential for (linear) time variation in the treatment effect. This reduces power if the treatment effect is in fact time-invariant, but seems inarguably necessary given (a) anecdotes that many waivers were granted in the early months of treatment, suggesting that compliance likely varied over time, and (b) theory which suggests that we should expect to see transitional dynamics even if compliance were constant over time.
- We do not specify a dynamic model including lagged outcomes. In principle there are good reasons to expect autocorrelation (for example, people picking up allotments they did not collect in the previous month), but we view the identification challenges inherent in estimating a lagged dependent variable model in such a short panel as prohibitive.

4.2.1 Heterogeneity

We will also estimate effect heterogeneity by accumulated balance B at the time of reconciliation onset, since in theory the initial (and potentially long-term) effects should depend on this. We estimate the specification above with additional terms that interact the R and P terms with B_{t^*} . Since B is endogenous, we instrument for it using assignment to T/C in the initial experiment; the intuition for this instrument is that blocks assigned to early treatment had a longer period to accumulate a balance than those assigned to late treatment (i.e. control). In these specifications we will additionally include stratum fixed effects and their interaction with B , and will cluster standard errors at the block level.

In addition to heterogeneity by balance, we will examine heterogeneity along the dimensions listed above: an indicator for above/below median cellular network signal strength, an indicator for being in an urban location, and indicators for machine operating mode (online, partially online, or offline). We expect these comparisons to be underpowered, however.

4.2.2 Addressing data limitations

We expected that some of the ration cards we initially sampled would no longer be active by the time of our second and third endline surveys, as some rationcards are deactivated and (sometimes) replaced over time. By default, if a household reports that the sampled ration card had been replaced at a given time t we use the values they report for the replacement card. As a robustness check we will also examine whether the onset of reconciliation affected the rate of card replacement, and if so will also report results using only the originally sampled cards.

We anticipate that we may observe a ragged panel for some outcomes. In this case we will estimated effects in the ragged panel by default, but will also show results in a balanced panel if we see evidence that selection in or out of the sample is differential by baseline values of the outcome.

We expect recall quality of our survey data will vary. To assess the sensitivity of our results to recall issues, we will include as a robustness check a specification in which we condition on the length of the recall period by including the number of days between survey date and reference date as a control variable.

We anticipate that some households may collect benefits to which they are entitled in month t in subsequent month $t + 1$. We will address this following the approach described in the first endline pre-analysis plan for the ePOS evaluation, making adjustments if we extensive delayed collection and otherwise ignoring it. Note that if anything we expect to see less delayed collection than previously since ePOS devices would only allow dealers to disburse rations for the current or immediately preceding month.