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ABSTRACT

How should recipients of publicly-provided goods and services prove their identity in order to access these benefits? The core design challenge is managing the tradeoff between Type-II errors of inclusion (including corruption) against Type-I errors of exclusion whereby legitimate beneficiaries are denied benefits. We use a large-scale experiment randomized across 15 million beneficiaries to evaluate the effects of more stringent ID requirements based on biometric authentication on the delivery of India's largest social protection program (subsidized food) in the state of Jharkhand. By itself, requiring biometric authentication to transact did not reduce leakage, slightly increased transaction costs for the average beneficiary, and reduced benefits received by the subset of beneficiaries who had not previously registered an ID by 10.6%. An event study of subsequent reforms that made use of authenticated transaction data to determine allocations to the program shows that these coincided with large reductions in leakage, but also significant reductions in benefits received. Our results highlight that attempts to reduce corruption in welfare programs by making ID requirements more stringent can also generate non-trivial costs in terms of exclusion and inconvenience to genuine beneficiaries.

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1 Introduction

How should recipients of publicly provided goods and services prove their identity in order to access these benefits? From accessing welfare benefits to obtaining a driver's license to casting a vote, how stringent ID requirements should be is a perennially controversial question around the world. The core design issue is how to manage the tradeoff between Type-II errors of *inclusion* whereby benefits are granted to non-eligible or non-existent recipients against Type-I errors of *exclusion* whereby legitimate beneficiaries are denied benefits to which they are legally entitled. While there is a large literature on how to *target* people to be put on program beneficiary lists (Alatas et al., 2012; Niehaus et al., 2013; Alatas et al., 2016), there is much less evidence on the distinct question of how citizens should prove their *identity* at the point of receiving benefits.

This question is particularly salient in developing countries. Historically, states have invested in the ability to better identify their citizens as they develop (Scott, 1998). During the past two decades in particular "the number of national identification and similar programs has grown exponentially... to the point where almost all developing countries have at least one such program" (Gelb and Metz, 2018). Around two-thirds of these use biometric technology, reflecting the view that this provides more reliable authentication than alternatives, particularly in settings with low levels of literacy and numeracy.[1]

A leading case is India, where the government has issued unique identification ("Aadhaar") numbers linked to biometric records to over 1.24 billion people and is integrating Aadhaar-based biometric authentication into a range of applications. The extent to which Aadhaar should be mandated to receive welfare benefits has been controversial, contested all the way to India's Supreme Court. Proponents argue that this is necessary to prevent fraud, while critics argue that it denies people their legal entitlements and in doing so "undermines the right to life" (Khera, 2017). In a September 2018 ruling, the Court allowed the government to mandate Aadhaar for accessing social programs, making it all the more urgent to understand how doing so affects errors of inclusion and exclusion.

This paper reports results from the first (to our knowledge) experimental evaluation of introducing Aadhaar as a requirement to collect welfare benefits. Specifically, we examine how this introduction shifted the tradeoff between errors of inclusion and exclusion in the Public Distribution System (PDS), India's largest welfare program, accounting for roughly 1% of GDP. The PDS is the primary policy instrument for providing food security to the poor in India, which has the largest number of malnourished people in the world (FAO et al.,

[1]National biometric ID systems have recently been rolled out in Ghana, Kenya, Malawi, Senegal, Tanzania, and Uganda, amongst others. The World Bank has a dedicated initiative - ID4D - to help countries "realize identification systems fit for the digital age." (`https://id4d.worldbank.org/`).

2019). In principle, PDS ration card holders are entitled to purchase fixed monthly quantities of grain and other commodities at a highly-subsidized price from a government-run Fair Price Shop (FPS). In practice, the resulting dual-price system creates strong incentives for corrupt intermediaries to divert grains to the open market. The "leakage" of grains can happen both on the extensive margin through the creation of "excess" ration cards for non-existent (or unaware) beneficiaries to whom grain is allocated (and then pilfered), and on the intensive margin through "under-provision" of grain to legitimate beneficiaries.[2]

To evaluate the impact of introducing Aadhaar in the PDS, we worked with the government of the state of Jharkhand to randomize the order in which biometric authentication was introduced across 132 sub-districts in 10 districts in the state of Jharkhand. Our evaluation sample is representative by design of 15.1 million individuals in 3.3 million beneficiary households in 17 of Jharkhand's 24 districts, and representative on observables of the rest of the state. Further, the integration of Aadhaar into the PDS was implemented by the Government of Jharkhand (GoJH) as part of a full-scale deployment that was being rolled out across the country. Thus, our study design allows us to directly estimate the policy-relevant parameters of interest.

GoJH implemented this reform in two phases. In the first phase, electronic Point-of-Sale (ePOS) machines were installed in FPSs in treated areas while beneficiaries were required to obtain an Aadhaar number for at least one member of their household, link (or "seed") it to their PDS account, and then authenticate their identity by scanning the fingerprints of a seeded household member each time they transacted at a ration shop. This process of Aadhaar-based biometric authentication (ABBA) in turn generated a digital record of transactions for which beneficiaries had "signed" biometrically. The control group continued with the default of authentication based on presenting a paper ration card to collect benefits, and record-keeping on paper. ABBA by itself did not change the amounts disbursed by the government. However, it could have discouraged PDS dealers from diverting grains to the extent they anticipated being held responsible in the future for any grain not accounted for by an authenticated transaction.[3]

In the second phase ("reconciliation"), GoJH began adjusting downwards the amounts of grain it disbursed to each FPS to reflect the amounts they should still have in stock according to the electronic records of previously authenticated transactions from ePOS machines

[2]The most recent nation-wide estimates find that 42% of grain was diverted as of 2011-12 (Dreze and Khera, 2015). However, prior work has not (to the best of our knowledge) decomposed PDS leakage into the extensive and intensive margin channels.

[3]Dealers may have anticipated that they would be held responsible for diverted grain once reconciliation began (as explained below), or through less formal audits and investigations. In either case they would have an incentive to induce beneficiaries to "sign" biometrically for transactions, potentially giving beneficiaries additional leverage to obtain grain relative to the status quo.

(see details in Section 2.2). This contrasted with the status quo approach, which was to send each FPS the full amount of grain needed to satisfy the entitlements of all beneficiaries assigned to it. Reconciliation should therefore (weakly) reduce disbursals, with the key empirical question being the incidence of these reductions on beneficiaries and on leakage. GoJH launched reconciliation simultaneously in control and treatment areas, two (eleven) months after deploying ePOS devices in control (treatment) areas. We therefore present experimental estimates of the impact of requiring ABBA to collect benefits, and non-experimental estimates of the impact of reconciliation using a pre-specified event study framework (and also using as a placebo other PDS commodities not subject to reconciliation).

GoJH implemented ABBA quickly and thoroughly, and largely complied with the experimental design. Six months after treatment onset, 95% of beneficiary households in treated areas had at least one member with an Aadhaar number seeded to the PDS account, and 91% reported that transactions at their FPS were being authenticated, while only 6% of control households reported the same. The ITT estimates presented below can thus be reasonably interpreted as those of the reform.

Our main outcomes of interest are the value of goods disbursed by the government, value received by beneficiaries, and the difference between these (i.e. leakage). We measure these using comprehensive administrative data on disbursals of commodities to all ration shops matched to original survey data on commodity receipts and transaction costs collected in four rounds (one baseline and three follow-up rounds) from a panel of 3,840 PDS beneficiaries. The first follow-up was used to study the effects of ABBA, and the remaining to study reconciliation. At the time of the first follow-up, we estimate that the average leakage rate in the control group was Rs. 116 per ration card per month, or 20% of value disbursed, indicating that this remains a problem. On the other hand, we successfully located 97% of the beneficiaries we sampled, implying that at most 3% of accounts were fictitious and that leakage was taking place primarily through the intensive margin of under-provision rather than the extensive margin of fake beneficiaries.

We find that the impacts of ABBA by itself (without reconciliation) were small on average and, where significantly different from zero, negative for beneficiaries. As expected, ABBA by itself did not decrease (and if anything slightly increased) government spending. It did not substantially change the mean value received by beneficiaries or mean leakage. We also find no meaningful changes in measures of the quality of goods received, of their market prices, or of beneficiaries' food security. Beneficiaries, however, did incur 17% higher flow transaction costs to collect their benefits (a Rs. 7 increase on a base of Rs. 41), due in part to a doubling in the number of unsuccessful trips made to collect benefits.

While average benefits received may not have decreased, distributional effects are particularly important for evaluating more stringent ID requirements, as the most plausible downside is that they may exclude a vulnerable minority unable to meet the new standards. We find using survey data that treatment increased the probability that a real (i.e. not fictitious) beneficiary received no commodities at all in any given month by 2.4 percentage points ($p < 0.1$), implying that nearly 300,000 people lost access to benefits due to ABBA. Focusing on the 23% of households who did not have at least one member's Aadhaar number seeded to their PDS account at baseline, we find that exclusion errors increased significantly: the mean value of rice and wheat received fell by Rs. 49, or 10.6%, and the probability of receiving none of these commodities increased by 10 percentage points (a 50% increase on a base of 20%). This pattern of incidence is regressive, as unseeded households tend to be poorer and less educated than their seeded peers. Overall, these results are consistent with the critique that ABBA per se caused at least some "pain without gain" (Dreze et al., 2017).

A potential counterargument, however, is that authenticating transactions was a necessary first step towards reconciliation, which should reduce disbursals and (potentially) leakage. Indeed, the data suggest that reconciliation initially had a substantial impact. Focusing first on the control group, we find a 19% (Rs. 92 per ration card) reduction in the value of reconciled commodities disbursed by GoJH in July (the first month of reconciliation). Of the drop, we estimate that 22% represents a reduction in value received by beneficiaries (based on household-survey data). Thus, of the total reduction in disbursals, 78% represents a genuine reduction in leakage.

In treatment areas, effects of reconciliation were more pronounced, and the tradeoff between errors of exclusion and inclusion somewhat less advantageous. Disbursements in July fell by 37% (Rs. 182 per ration card), significantly more than in control areas. Of this 34% represented a drop in value received by beneficiaries and 66% a reduction in leakage. On the extensive margin, we estimate that an additional 1.6 million people did not receive PDS benefits in July. The larger effect in treated areas reflects the fact that dealers in these areas had been implementing ABBA for roughly nine months longer, and thus government records based on ABBA indicated that they should be holding more accumulated stock (even though in practice much of this had likely been diverted). Consistent with this pattern, dealers in treated areas reported a 72% lower expected future bribe price for FPS licenses, suggesting that they expected a substantial fall in the potential for extracting rents.[4]

A longer-run analogue to reconciliation is to delete unseeded ration cards entirely. We find that the rate of card deletions increased after the onset of ABBA and reconciliation.

[4] The combination of reductions in leakage for dealers, and reduction in benefits for users made reconciliation very unpopular, and GoJH had to relax and then entirely pause reconciliation after four months in the face of complaints from both dealers and beneficiaries before reintroducing it the following year. We discuss details and present estimates of impact over the entire 4-month period of reconciliation in Section 5.

Unseeded ration cards were much more likely than seeded ones to be deleted (36% vs 2%). In our survey sample, deleted cards included both non-existent ("ghost") households (12% of deletions) as well as real ones (88%). While these figures are purely descriptive and do not necessarily imply the exclusion of truly "deserving" beneficiaries, they do highlight another margin along which cutting leakage likely came at the cost of some additional exclusion.

Overall, biometric authentication in Jharkhand's PDS was not a free lunch: depending on how it was used, it either did not reduce leakage or did so at the cost of increased exclusion error. Considering (conservatively) the case of reconciliation in the control group, a planner would need to value marginal fiscal savings at at least 22% of the value placed on transfers to marginal households in order to prefer such a policy to the status quo.[5] In the conclusion, we discuss several practical ways to reduce the likelihood of exclusion errors while still achieving leakage reductions.

As with most impact evaluations, one limitation of our study is that we can evaluate ABBA and reconciliation only as implemented in this specific setting and point in time. Both measures of ABBA implementation and our estimated treatment effects appear stable during the window 6-8 months after rollout we measure, suggesting we capture a (relatively) steady state. Still, exclusion errors and transaction costs may attenuate in the longer run as Aadhaar-seeding becomes more widespread and connectivity improves. Similarly, reconciliation may not generate as much "pass through" of the pain of reduced stocks from dealers to beneficiaries in a longer-run steady state. Yet, while the longer-term effects may be different from what we report (especially if governments respond to our findings), the costs and benefits even of the transition phase of major reforms to social programs are an important consideration for policy, and our findings provide insight into these.

Our most important contribution is to provide the first experimental evidence on the trade-off between Type I and Type II errors from introducing stricter ID requirements for receiving welfare benefits. Further, we do so in the context of the largest welfare program (PDS) in the country with the largest biometric ID system in the world (India). Our results are directly relevant to policy discussions regarding the use of more stringent ID requirements to access public services in India and other countries.[6] More broadly, they add to the evidence base on how transaction costs affect the incidence of welfare benefits (e.g. Currie (2004) and more recently Alatas et al. (2016)). As predicted by Kleven and Kopczuk (2011), they illustrate

[5]In this specific case, the benefits of reduced disbursal may have been even lower as the savings were only notional, yielding an increased stock of grain in public warehouses as opposed to reduced spending. Over time, fiscal savings may be possible by reducing the amount of grain procured, but no such policy change has yet been announced (in part because procurement policy also aims to support farmers).

[6]They also provide a counterpoint, for example, to recent panel-data evidence that voter ID requirements have had surprisingly little effect on voter participation in the United States (Cantoni and Pons, 2019).

how the complexity of the process of obtaining benefits can affect their incidence; here, "complexity" does not appear to have been an effective screening device as the households excluded generally appear less well off on socioeconomic measures.

Second, our results illustrate the potential "shadow costs" of controlling corruption. It is an old idea that systems designed to control corruption may not only incur direct costs, such as hiring auditors, but also worsen decision-making on other margins – for example, by slowing it down, precluding the use of "soft information," or deflecting attention from other important matters (Klitgaard, 1988; Wilson, 1989). In addition to measuring the direct costs of an anti-corruption intervention, as empirical studies typically do, we also provide evidence that in our setting the indirect costs of excluding marginalized households from their legally entitled benefits were also considerable.[7]

Finally, our findings are directly relevant to research and policy discussions on using technology to improve governance and state capacity in developing countries. In prior work (Muralidharan et al., 2016), we found that introducing biometric payments in rural welfare programs in the state of Andhra Pradesh (AP) both reduced leakage *and* improved the payment experience. The impacts of ABBA and reconciliation in the PDS in Jharkhand were quite different, likely reflecting differences in both program and intervention design and in policy priorities. Relative to Jharkhand, the intervention in AP featured greater acceptance of manual overrides to prevent exclusion errors, greater emphasis on improving the beneficiary experience as opposed to achieving fiscal savings, and relocation of the point of service delivery closer to beneficiaries. The contrasting results caution against simplistic characterizations of the effects of new technologies such as biometric authentication, without reference to *details* of program and intervention design, and to the beneficiary experience.[8] More broadly, they highlight that discussions of external validity in program evaluation need to pay attention to differences in program and intervention "construct" as well as context.

The rest of the paper is organized as follows. Section 2 describes the context and intervention. Section 3 presents the research design including data collection and estimation strategy. Section 4 describes results of point-of-sale authentication, Section 5 describes results of reconciliation and ration card deletion, and Section 6 offers a concluding summary.

[7]In related work, Lichand and Fernandes (2019) find that the threat of audits reduced corruption but also displaced spending on services such as public health care in Brazilian municipalities, and that this worsened some local public health outcomes.

[8]There are close parallels with the literature on education technology, where the impacts of technology-based education interventions on learning outcomes have been found to vary widely as a function of design details (Muralidharan et al., 2019a).

2 Context and intervention

Malnutrition remains a serious problem today in India, which ranked 102 of 117 countries in the most recent Global Hunger Index Rankings (Grebmer et al., 2019) and had an estimated 38% of children stunted and 36% underweight as of 2015-2016 (UNICEF et al., 2017). The Public Distribution System (PDS) is the main program by which the Government of India aims to provide food security to the poor. Through a network of over 527,000 ration shops known as "Fair Price Shops" (FPS), it distributes subsidized wheat and rice to targeted households on a monthly basis, and other commodities such as sugar, salt, and kerosene on an occasional basis.[9] The quantity of subsidized goods each household can purchase is capped based on the category of ration card it holds and the size of the household. Under the National Food Security Act of 2013, the government has a mandate to include 75% (50%) of the rural (urban) population as beneficiaries. Individual states administer targeting and distribution within their boundaries. Overall, the PDS costs roughly 1% of GDP to operate.[10]

In part because it creates a dual-price system, distributing commodities at prices well below their market prices, the PDS has historically suffered from high levels of leakage and corruption. Commodities "leak" from the warehouses and trucking networks meant to deliver them to the FPS, or from the shops themselves. At the retail level, dealers have been known to adulterate commodities, over-charge for them, or provide beneficiaries with less grains than their legal entitlement.[11] Historically estimated leakage rates have been high; Dreze and Khera (2015) estimate that 42% of foodgrains nationwide and 44% in Jharkhand were diverted in 2011-2012, which is itself an improvement on the estimate of 73% by the Planning Commission in 2003 (The Programme Evaluation Organisation, 2005).

Various reforms meant to address these challenges are underway, including several grouped under the broad heading of "PDS computerization." We focus on one of the major components of computerization: the introduction of electronic point-of-sale (ePOS) devices to process and record transactions between dealers and beneficiaries. As we describe below, these devices enabled Aadhaar-based biometric authentication (ABBA) as well as the creation of a digital transaction ledger. Rollout of these devices was well underway elsewhere in

[9] Throughout the paper, we use the term "disbursal" to refer to commodities sent by the government to FPS dealers, and the term "distribution" to refer to commodities provided by FPS dealers to beneficiaries.

[10] The PDS is enabled in part by India's policy of a Minimum Support Price for essential commodities like rice and wheat combined with public procurement of these commodities from farmers. The resulting stocks of foodgrain with the government are then distributed to the poor through the PDS. In this way, Indian agriculture and food policy intervenes in both the production and distribution side of the market. For PDS expenditures, see `http://www.indiabudget.gov.in/ub2018-19/eb/stat7.pdf`. For GDP estimates, see `https://dbie.rbi.org.in/DBIE/dbie.rbi?site=statistics`. Both sources accessed on 5 March, 2018

[11] Under-provision can reflect both power asymmetry between dealers and beneficiaries (when dealers have the grain), as well as diversion before stocks even reach the village (when they may not have the grain).

India by the time GoJH began its deployment; as of July 2016 an estimated 23% of India's FPSs had received devices, rising to 54% by December 2017[12] with the rollout ongoing.[13]

ePOS devices perform biometric authentication using Aadhaar, India's landmark unique ID system. The Government of India launched Aadhaar in 2009 with the goal of issuing an identification number linked to biometric information for every resident of the country. As of June 2019, it had issued Aadhaar numbers to 1.24B people, or 91% of the country's population.[14] Investments in ID could be particularly important in India given its historically unusual situation as a country with a substantial welfare state at relatively low levels of per capita income and state capacity. Indeed, the government has touted Aadhaar as an enabling technology which will support reforms to the implementation of a wide range of government schemes – "a game changer for governance," as the Finance Minister at the time put it (Harris, 2013). Abraham et al. (2017) estimate that it was being applied to at least 558 use cases as of 2017. Government claims regarding the fiscal savings achieved by introducing Aadhaar have at times been met with skepticism (Khera, 2016), in part because they did not differentiate between real reductions in leakage and increased exclusion of legitimate beneficiaries. To our knowledge, however, there has been no experimental evidence to date on the impacts of an Aadhaar deployment in any welfare program.[15]

Jharkhand is a relatively challenging environment in which to roll out an ambitious reform such as ABBA. In terms of state capacity, it ranked 17th of 19 major states on the most recent Governance Performance Index (Mundle et al., 2012), well below 3rd-ranked Andhra Pradesh in which our previous evaluation of biometric authentication was set. As one concrete example, it had the highest rate of teacher absence among *all* Indian states in both 2003 and 2010 (Muralidharan et al., 2017). Jharkhand also rated relatively low in terms of key pieces of enabling infrastructure such as rural teledensity (40 telephone or mobile phone connections per 100 people in rural Jharkhand as of 31 October 2017, ranked 19 out of 19 reported states) and at the middle of the pack for Aadhaar penetration (93% penetration as of 31 December 2017, ranked 17th of 36 states).[16]

[12] For July 2016 statistics, see `http://164.100.47.190/loksabhaquestions/annex/9/AS26.pdf/`. For December 2017 statistics, see `http://pib.nic.in/PressReleseDetail.aspx?PRID=1512902`. Both sources accessed 5 March 2018.

[13] Other PDS computerization initiatives included digitization of beneficiary databases, computerization of supply-chain management, and creation of grievance redressal mechanisms and online transparency portals.

[14] For statistic on number of Aadhaar UIDs generated, see `https://uidai.gov.in/aadhaar_dashboard/india.php`. For total population statistics, see `https://data.worldbank.org/indicator/SP.POP.TOTL`.

[15] In addition to the tradeoffs we discuss here, implementing large-scale biometric ID schemes such as Aadhaar involves tradeoffs between state capacity and privacy. See Gelb and Metz (2018) for further discussion.

[16] For rural teledensity statistics, see `http://164.100.47.190/loksabhaquestions/annex/13/AU2751.pdf`, accessed March 5, 2018. For Aadhaar penetration statistics, see `https://uidai.gov.in/enrolment-update/ecosystem-partners/state-wise-aadhaar-saturation.html`, accessed January 31, 2018.

2.1 The intervention

In August 2016, GoJH introduced ePOS machines in FPSs to authenticate beneficiaries when they came to collect their rations (Figure 2 provides the rollout timeline). Prior to the intervention, authentication in the Jharkhand PDS was relatively informal. Each beneficiary was assigned to a unique FPS and issued a ration card listing members of the household and displaying a photograph of the household head. To collect benefits, any one of these listed household members was required to appear in person with the ration card at the assigned FPS. Anecdotally it was not uncommon for neighbors or friends to collect benefits on their behalf, or for dealers to hold on to beneficiaries' ration cards themselves. Dealers were expected to record transactions both on ration cards and in their own ledgers; ledgers were typically not audited, and anecdotally there was wide variation in record-keeping practices.[17]

The reform modified authentication and record-keeping processes. The state gave each dealer an ePOS device configured to authenticate beneficiaries in one of three modes: online (81% of shops), offline (15%), and partially online (4%).[18] In *online mode*, the device required the operator to input a ration card number. It then displayed a list of all individuals who were both (i) listed as beneficiaries on the relevant ration card, and (ii) had an Aadhaar number linked ("seeded") to the card. The dealer selected the beneficiary present, and the device then prompted him/her to place a finger of choice on the device's scanner to be authenticated against the central Aadhaar database. If fingerprint authentication failed on three consecutive attempts, the beneficiary could opt to receive a one-time password texted to their mobile phone number as a fallback method of authentication.[19] In *offline mode*, the device simply captured and stored fingerprint information for the person collecting benefits but performed no authentication checks. However, transaction logs were meant to be synchronized with a server periodically (as explained below). In *partially online mode*, the device functioned as in online mode if it detected a network connection and in offline mode otherwise. Dealers did not have discretion to select modes (but could potentially have tried to force the device to operate in offline mode by disrupting connectivity).

The government varied the mode assigned to each FPS in an effort to balance the risks of inclusion and exclusion error: it sought to enforce relatively strict authentication requirements in areas where connectivity was strong enough to provide a reliable connection to the

[17] One common practice was to keep separate "official" and "unofficial" ledgers, where the unofficial ledgers recorded actual transactions while official clean ledgers would be produced in case of a government audit.

[18] Averages prior to August 2017; after this, the government ended the use of partially online mode, with 88% of FPSs operated in online mode and the remaining 12% offline.

[19] Some officials claimed that at least initially if neither method of authentication succeeded there was an "override" option available allowing the dealer to authenticate a beneficiary without using Aadhaar, but officially no such option was meant to exist.

central Aadhaar database, but not deny benefits to legitimate beneficiaries in areas where connectivity was weaker.[20] In our experimental design assignment to receive a machine was random but assignment to machine mode was not, so that the effects we report represent an average of mode-specific effects given the assignment policy described here.[21]

ePOS devices also enabled digital record-keeping. After authentication, the device would display any previously uncollected commodity balances to which the beneficiary was entitled, including the current months' entitlement and any uncollected balance from the previous month (but not balances from two or more months previous). After completing a transaction the dealer would record the amount of each commodity purchased in the device, which would print a paper receipt and also voice the transaction details in Hindi. Dealers were instructed to give the receipt to the recipient as well as recording the transaction in their ration card. In practice, recipients often reported not receiving receipts or that these faded quickly. In any case, the digital ledger maintained in the device became the source of truth for balance information from the government's perspective, though dealers were of course free to maintain their own parallel paper records if they wished.

The government accessed transaction data by synchronizing ("syncing") regularly with each device. Online devices synced their records with a central government server automatically in real time. Dealers using partially online and offline devices were instructed to sync data within 48 hours of a transaction, but did not face any obvious repercussions if they did not. Instead their binding constraint appeared to be monthly: devices would not authorize new transactions in a given month until the previous month's transactions had been synced.

The process of seeding Aadhaar numbers to ration cards was ongoing during the period we study. To seed their ration card, a household first needed to have at least one of the members listed on the ration card obtain an Aadhaar number, either at camps organized specially for this purpose or subsequently by applying at the local block or district office. It then needed to link this Aadhaar number to its ration card, again either at camps organized for this purpose during NFSA enrollment or by applying at the block or district office. As of May 2016 (three months prior to ABBA launch), 76.5% of ration cards in areas assigned to treatment and 79.9% of those in areas assigned to control had been seeded with at least one Aadhaar number. These figures had risen to 94.5% and 92.6% by October of 2016 and to 99.8% and 99.5% by May of 2018 (roughly one year after the period we examine experimentally). Note that the seeding process could itself have affected errors of inclusion

[20] In data collected by our survey team, the proportion of FPS at which no cellular signal could be detected was 5% for shops with online devices, 10% for shops with partially online devices, and 58% for shops with offline devices, which is consistent with government guidelines.

[21] We also report a non-experimental decomposition assuming that, had they been treated early, control FPSs would have been treated with the same machine mode to which they were subsequently assigned.

and exclusion if it affected decisions about the deletion of ration cards, which we examine in Section 5.4.

2.2 Reconciliation

Prior to the introduction of ABBA using ePOS devices, GoJH rarely (if ever) reconciled balances with FPS dealers. For example, if the grain needed to serve all PDS beneficiaries assigned to a given FPS was 100kg of rice per month, it was GoJH policy to ship 100kg of rice to that FPS each month *regardless* of how much rice it had distributed to beneficiaries in previous months. This reflected in part the simple fact that the government had no timely and reliable data on transactions at the shops.

By June of 2017, ePOS devices were actively in use for authentication in 93% of FPSs in our study area, including those in control blocks, where they were rolled out during April and May. Starting in July, GoJH introduced a second reform, reconciling its disbursements of rice and wheat, though not of sugar, salt or kerosene. The full formula used to determine disbursements under this regime is in Appendix C. To summarize, the government's new policy was to calculate (a) the amount each dealer would need to meet claims by beneficiaries against the current month's entitlements, as well as any outstanding claims on the preceding one month's entitlements, and (b) the amount the dealer should have in stock given the full history of deliveries and transactions (starting from the time the FPS first used an ePOS device), and then disburse the difference between these quantities.

From a dealer's perspective, this reform (if implemented by the book) had two effects. First, it had a retrospective effect, reducing the amount of rice and wheat received starting in July: dealers who had not distributed the full amounts disbursed to them in previous months (as recorded by the ePOS machines) received less. We would expect this effect to be larger for dealers in treatment blocks, since as of July they had been using devices for 11 months as opposed to 1-2 months for dealers in control blocks. As we discuss below, many dealers had "opening balances" at the onset of reconciliation equivalent to over a month of entitlement (based on the amount of grain they had received in previous months against which no authenticated transaction log existed). This implied that by rule they should have received no incremental grain at all, since they were supposed to be holding enough stocks of grains to make all program-required distributions in July. This was true regardless of the *actual* amount of stocks dealers held. In practice, many dealers' actual stock levels were below the official opening balances, either because they had diverted stock to the open market or grains had spoiled.

Second, the reform prospectively affected dealers' *marginal* incentives to report via the ePOS devices that they had distributed grain to ration card holders, since reporting less than full distribution would reduce the amount they received the next month. The legitimate incentive to do so was the commission of 1 rupee per kilogram of grain they received for distributing commodities. In addition, receiving more grains would also make it easy to divert some while still providing beneficiaries with at least some benefits.

From a beneficiary's perspective the consequences of reconciliation are unclear. On one hand, dealers might pass on some share of the reduction in grains disbursed to them, reducing in turn the amounts distributed to beneficiaries. On the other hand, dealers now needed beneficiaries to appear and scan their fingerprints to "sign for" transactions on which future grain disbursements were based; dealers may thus have distributed more grain to beneficiaries in order to incentivize them to do so. Note, however, that strictly speaking the reform created incentives for dealers to *report* that they had distributed grain, not to actually distribute it. In the words of an official quoted in the media: "Even if someone uses his thumb and gets 2 kg of rice instead of 5 kg, what can you do?" (Singh, 2019).

2.3 Summary

Overall, the reforms introduced by GoJH were representative of the way in which the Government of India has envisioned using Aadhaar to reform program administration. In particular, they made possession of an Aadhaar number effectively mandatory for the receipt of PDS benefits. ABBA and reconciliation likely made it more difficult for dealers to divert grains through "over-reporting" the number of beneficiaries (including making up fake or ghost beneficiaries), but its predicted impacts on their incentives for "under-payment" of benefits to genuine beneficiaries were ambiguous.

A priori one would thus expect it to have both strong potential to reduce errors of inclusion, and a high risk of generating additional errors of exclusion. Media criticism has argued that it has done exactly that, leading in some cases to preventable starvation deaths – "death by digital exclusion," as one headline put it (Singh, 2019). Yet, given the non-representative nature of these anecdotes, and strong views both in favor of and against the reform, understanding the impacts of making Aadhaar integration mandatory for receiving PDS benefits (with a well-identified research design in a representative sample) is a key open question for research and policy.

3 Research design

Our research design follows a pair of pre-specified and pre-registered analysis plans, one for the evaluation of Aadhaar-based authentication itself and another for the analysis of reconciliation.[22] Appendix D provides a comprehensive list of analysis reported in addition to what was pre-specified.

3.1 Randomization

To obtain policy-relevant estimates of impact, we sought to design an evaluation that was "at scale" in each of the three senses identified by Muralidharan and Niehaus (2017). These include conducting our study in a sample that is representative of the (larger) population of interest, studying the effects of implementation at large scale, and having large units of randomization to capture general equilibrium or other spillover effects such as changes in the market prices of subsidized commodities.[23]

We first sampled study districts. Of Jharkhand's 24 districts, we excluded 1 in which the intervention rollout had already begun and 6 in which a related reform (of Direct Benefit Transfers for kerosene) was being rolled out. From the remaining 17 districts, home to 24 million people and 15.1 million PDS beneficiaries, we randomly sampled 10 within which to randomize the rollout of the intervention.[24] This design ensures representativeness of the 17 districts in our frame. In practice our 10 study districts appear fairly comparable on major demographic and socio-economic indicators to all the 14 remaining districts of Jharkhand (Table 1). Our frame is thus arguably representative of the full population of 5.6 million PDS households and 26 million PDS beneficiaries in the state.

Finally, we assigned treatment to large units. We randomized the rollout at the level of the sub-district ("block"), which on average covers 73 FPSs and 96,000 people. Figure 1 maps treated and control blocks and illustrates their geographic balance and coverage of

[22] `https://www.socialscienceregistry.org/versions/39275/docs/version/document` and `https://www.socialscienceregistry.org/versions/39274/docs/version/document` respectively.

[23] Each of these three design choices helps to improve external validity. Conducting experimental evaluations in near-representative samples helps by reducing the risk of site-selection bias (Allcott, 2015). Evaluating a large-scale implementation helps because effect sizes have been shown to decline with size of implementation (Vivalt, forthcoming), Finally, randomizing large units into treatment and control status helps produce estimates that are inclusive of spillovers, which have been shown to be salient for policy in several studies including Cunha et al. (2018), Egger et al. (2019), and Muralidharan et al. (2020).

[24] We used stratified random sampling, stratifying on three variables related to geography and socio-economic status. We used these 3 binary variables to classify the 17 available districts into 8 (2x2x2) distinct categories. We then sampled half of the districts in each category, rounding down to the nearest integer and using probability proportional to size (measured as number of FPSs) sampling, and lastly sampled additional districts without stratification to reach our target of 10. Full details in the Pre-Analysis Plan: `https://www.socialscienceregistry.org/versions/39275/docs/version/document`.

the state. We allocated 132 blocks into a treatment arm of 87 blocks and a control arm of 45 blocks, reflecting the government's preference to delay treatment in as few blocks as possible.[25] Treatment and control blocks are similar in terms of demographic and program characteristics, as one would expect (Table 2, Panel A). Of 12 characteristics we examine, one is marginally significant at the 10% level.

The evaluation was conducted within the context of a full-scale rollout, as GoJH deployed ePOS devices to 36,000 ration shops covering the entire population of 26 million PDS beneficiaries in the state. This deployment involved a major effort by the government and was the stated top priority of the Department of Food and Civil Supplies for the year and (anecdotally) the single largest use to which they put staff time. We thus measure the effects of implementation at full scale by a bureaucratic machinery fully committed to the reform, which are the effects of interest for policy purposes.

Consistent with this commitment, we find that GoJH complied closely and quickly with the treatment assignment. By the time of our follow-up survey, households in treated blocks reported that 96% of dealers in treated blocks possessed an ePOS device and 91% were using it to process transactions (Table 2, Panel B). ePOS utilization was stable at 90-91% in treated blocks during January-March 2017, which increases our confidence that we are estimating steady state impacts and not transitional dynamics. In control blocks, on the other hand, 5% of dealers possessed a device and 6% were using it to process transactions, largely reflecting early rollout in one control block.[26] Overall these figures suggest that it is sensible to estimate intent-to-treat effects and to interpret them as fairly close approximations of the overall average treatment effect.

3.2 Sampling and Data Collection

Our data collection focused on measuring three core concepts: the value of commodities disbursed by the government, the value received by beneficiaries (both net of price paid), and the real transaction costs incurred by dealers and beneficiaries to implement/obtain this transfer of value. Leakage in this framework is simply the difference between value disbursed and value received.

[25] Within each district, we assigned blocks to treatment status as follows: We first divided blocks into rural and urban samples, then stratified them into groups of three by ordering them on the first principal component of three variables related to household size and benefit category. Within each group of 3 blocks we randomly assign 2 to treatment and 1 to control. Full details, including how we dealt with districts with residual strata of fewer than 3 blocks, are in the Pre-Analysis Plan: `https://www.socialscienceregistry.org/versions/39275/docs/version/document`.

[26] Of the 31 control households that report a dealer using an ePOS device, 24 are in one block. The remaining 7 are scattered across 6 other blocks and most likely reflect reporting errors.

To measure these quantities we begin with administrative records. These include information on monthly quantities of commodities disbursed to all FPSs, which we obtained from the National Informatics Centre (NIC),[27] and the administrative database of eligible PDS beneficiaries and their assignment to FPSs. We used the latter to draw samples of dealers and households to survey, and attempted to survey them four times – once at baseline and then at three subsequent follow-ups. We sampled as follows: from administrative records we drew a sample of 3 FPSs in each study block, for a total of 396 shops.[28] We successfully interviewed the dealers operating 367 (93%) of these shops at baseline, and 373 (94%) of them in the endline. Dealer surveys covered measures of the quantity of commodities received by the shop each month, their operating costs, the dealers' perceived value of FPS licenses and interest in continuing to operate a ration shop, and stated preferences for the reform as opposed to the status quo system. Enumerators also measured using our own equipment the strength of the four major cellular networks at the shop in order to capture connectivity.[29]

For each sampled ration shop we sampled 10 households from the government's list of PDS beneficiaries,[30] which had been created as part of a targeting exercise conducted in 2015 to comply with the National Food Security Act of 2013. This generated a target sample of 3,960 households. We attempted to interview these households for baseline and three follow-up surveys to create a household-level panel.[31] We ultimately identified and interviewed

[27]In some cases we were also able to obtain and digitize disbursement records directly from District Supply Officers, Market Supply Officers, Block Development Officers, and godowns run by the Food Corporation of India and the state of Jharkhand. These records generally correlated strongly (from 0.87 to 0.95 for various commodity $\times$ month pairs) but not perfectly with the NIC records. We use the NIC records to ensure representative coverage, but obtain qualitatively similar results if we use the hand-captured ones instead.

[28]The 3 shops were sampled using probability proportional to size (PPS) sampling, with "size" defined as the number of ration cards assigned to the shop.

[29]In follow-up surveys, we expanded the number of dealers surveyed, as a few (7.9%) of our sampled households had been re-assigned to new dealers in the normal course of operations during the 10 months since baseline. We report results for both the original and augmented dealer samples, as the reassignment rate of households is balanced across treatment and control, and the incremental dealers are not statistically distinguishable from the original ones on measured characteristics (Table A.1). Note also that the reassignment of households to other shops does not affect our ITT estimates because we follow the originally sampled households. It also does not affect the first-stage or the interpretation of our results because the reassignment was to other FPS in the same block, with the same treatment status (which is another advantage of randomizing at the block level)

[30]We define a household here as those individuals listed on a single ration card. We first sampled one village from the catchment area of each FPS using PPS sampling, with "size" defined as the number of ration cards in the village assigned to that FPS. We sampled ration cards using stratified random sampling, with strata including the method by which the household became eligible for the PDS and the benefit category to which the cardholder is entitled. Full details in the Pre-Analysis Plan, `https://www.socialscienceregistry.org/versions/39275/docs/version/document`.

[31]Because our frame is the universe of households previously deemed eligible for the program, our sample is not suited to examine errors of inclusion and exclusion in the process of determining *who is eligible* for the PDS, as in the extensive literature on poverty targeting. Our focus here is rather on studying changes in inclusion and exclusion resulting from increased stringency in verifying the *identity* of those previously

the corresponding household at least once in 97% of cases.[32] Overall, we estimate that at most 3% of beneficiaries were ghosts (see Figure A.1 for a more detailed categorization of households).[33] This is noteworthy as it suggests that the scope for eliminating leakage by removing ghosts (or non-existent households) from the beneficiary list was relatively limited in this setting.

We timed follow-up surveys and their associated recall periods to obtain continuous monthly data on beneficiaries' experiences with PDS from January through November of 2017. Figure 2 illustrates the recall window covered by each survey. We use data from follow-up 1, covering January through March, to measures the impacts of ABBA, and use data from all three follow-ups to examine the impacts of reconciliation. Topical coverage varied across surveys; follow-up 1 was most comprehensive, while follow-ups 2 and 3 measured a subset of outcomes (e.g. for households, the quantities of each commodity received). In particular, we did not measure market prices in follow-ups 2 and 3 and so do not examine price effects of reconciliation.

3.3 Estimation strategy: Aadhaar-based biometric authentication

To examine the impacts of ABBA we estimate intent-to-treat specifications of the form

$$Y_{hfbs}^{t} = \alpha + \beta Treated_{bs} + \gamma Y_{hfbs}^{0} + \delta_s + \epsilon_{hfbs}^{t} \tag{1}$$

where Y is an outcome measured for household h assigned to FPS f in block b of stratum s.[34] Regressors include an indicator T for whether that block was assigned to treatment, the baseline value Y_{hfbs}^{0} of the dependent variable, and a stratum fixed effect δ_s. Where we observe baseline values for multiple months we take their average. Where the baseline value is missing we set it equal to the overall mean value, and include an indicator for baseline missingness. When using survey data we weight specifications by (inverse) sampling probabilities to obtain results that are representative of the sample frame.[35] We use analogous specifications for outcomes measured at the level of the FPS or block. We pool observations for January-March 2017, following our pre-specified plan for dealing with the possibility of

deemed eligible for benefits.

[32] We successfully interviewed 3,410 (86%) of these households at baseline and 3,583 (90%), 3,618 (91%), and 3,562 (90%) at follow-ups 1, 2 and 3, respectively.

[33] Our procedure to classify a household as a ghost is stringent: the survey team makes three visits and attempts to locate the household as per the address in official records, and we only classify a household as a ghost household after three neighbors have certified that no such household exists.

[34] Because the randomization algorithm created 6 strata (3 urban and 3 rural) of size 1, we create a single fixed effect δ_s for each of these two groups.

[35] Variation in sampling probabilities was driven largely by field logistics constraints, e.g. the need to plan to interview a fixed number of households per village rather than a fixed proportion.

non-stationary treatment effects.[36]

Each regression table below reports the percent of the original sample for which data were non-missing and included in the estimation. In Tables A.2 and A.3 we examine missingness by treatment status and generally do not find evidence of imbalance, with 9% of differences significant at the 10% level. We impute zeros when calculating quantities and value received for verified "ghost" ration cards (which account for 1.6% of sampled households and do not differ across treatment and control groups). We report standard errors clustered by FPS. In most cases, we have a single well-defined summary measure of outcomes such as value disbursed or received. We adjust for multiple-hypothesis testing when reporting outcomes at the individual commodity level, reporting both standard p-values and q-values adjusted to control the false discovery rate.

3.4 Estimation strategy: reconciliation

GoJH introduced reconciliation in July 2017 across both treatment and control groups simultaneously. As mentioned in the Introduction, GoJH also suspended reconciliation in November 2017 (since it was very unpopular with both dealers and beneficiaries). We therefore examine the effects of reconciliation using time series variation in value disbursed and received using the following pre-specified model:

$$Y_{hfbst} = \alpha_{hfbs} + \gamma t + \beta_R R_t + \beta_{Rt} R_t (t - t^*) + P_t + \epsilon_{hfbst} \tag{2}$$

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). We estimate the model separately for treated and control blocks. To compare the two, we pool the data and interact all terms on the right-hand side in Equation 2 with an indicator for treatment.

This specification embodies several substantive assumptions. First, we assume the effect of reconciliation is identified once we control for a linear pre-trend. This is a strong assumption, but the best that is realistic with 6 months of pre-treatment data (and as it turns out yields an excellent fit). Second, because we include a distinct indicator for November we do not impose

[36] We pre-specified that we would (i) estimate models for each month individually, pooled models, and pooled models with a linear interaction between treatment and month, and then (ii) choose which specification to privilege based on the overall tendency of the trend terms to be significant predictors of primary outcomes. We generally do not observe any evidence of trends, and therefore privilege the pooled estimators. This is consistent with the fact discussed above that program implementation also appeared to have stabilized by the time of our follow-up. For completeness we report the other estimators in Appendix B.

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. Third, we model the potential for (linear) time variation in the treatment effect. This reduces power and increases the risk of overfitting if the treatment effect is in fact time-invariant, but seems appropriate given both that (a) theory suggests reconciliation should generate transitional dynamics, and (b) anecdotes suggest that the government granted many waivers to the reconciliation policy, and these may vary over time. Finally, we present results using time-series variation in value disbursed and received for both reconciled commodities (rice and wheat), and unreconciled ones (salt, sugar, and kerosene). The latter commodities provide a plausible contemporaneous placebo group to examine the effects of reconciliation.

4 Results: Biometric authentication

4.1 Measuring value transfer

We measure value (V) as the sum across commodities c of quantity (Q) multiplied by the difference between the local market price (p^m) of that commodity and the statutory ration shop price (p^s).[37] Formally,

$$V_{ht} = \sum_c Q_{cht}(p^m_{ht} - p^s_{ht}) \tag{3}$$

In total, ration card holders are entitled to a meaningful monthly amount. The mean value of these entitlements evaluated using Equation 3 is Rs. 595 per month, equivalent to 14% of the national rural poverty line for an average household in our sample.[38] In practice, however, households receive less than their entitlement. The mean value received in the control group at follow-up was Rs. 463 per month, or 78% of the mean entitlement. This was largely not because the government failed to disburse commodities, as (according to its own records) it disbursed commodities worth an average of Rs. 579 per month, or 98% of mean value entitled. Rather, it reflects the fact that roughly 20% by value of the commodities the government did disburse did not reach beneficiaries.

[37]We find very little evidence of over-charging (below), and hence our results are essentially the same if we use actual as opposed to statutory ration shop prices. We obtained data on local market prices for equivalent commodities as those provided by the ration shops. Even if the prices reflected higher quality of market grains, that would not affect our leakage calculations because we use the same price to estimate both value disbursed and value received.

[38]An average household in our sample had 4.4 members, and the national rural expenditure poverty line was Rs. 972 / person / month (Commission, 2014). The poverty line had not been updated since 2014; if we adjust it upwards for changes in the rural consumer price index from 2014-2017, then the mean entitlement was 13% of the poverty line for an average household.

4.1.1 Value disbursed

Table 3 summarizes impacts on value transfer during January-March 2017, beginning in Panel A with value disbursed by the government. Note that we observe this outcome for the universe of FPSs in our study area and therefore use all of these data, with outcomes expressed per rationcard $\times$ month. We expect no meaningful changes to disbursements, as the government's policy during this period was to disburse to each FPS in each month the full amount to which households assigned to that shop were entitled. We find this is largely the case, though we do find some modest substitution away from wheat and towards rice which nets out to a small but significant increase in total value disbursed of Rs. 12 per ration-card month, or around 2%.[39] In any case, there is no evidence that ABBA saved the government money.

4.1.2 Value received

Panel B reports effects on value received by households using survey data. We see some directional evidence of the shift from wheat to rice noted above, but no significant change in overall value received. A 95% confidence interval for this effect is Rs. $[-25.2, 22.8]$, ruling out decreases greater than 4.3% and increases greater than 3.9% of value disbursed. Any effects on value received by the average household were thus small in economic terms.

If the intervention reduces quantities flowing into rural markets which in turn raises market prices, we might see no overall effect even though recipient welfare had changed. We examine this possibility in Panel B of Table A.4 reports, and see no significant changes in the mean quantity of any commodity received, though there appears to be a shift from wheat to rice as noted above. The market prices households faced for these commodities also did not change significantly, with the possible exception of a fall in the price of sugar which is marginally significant after adjusting for multiple testing (Table A.5, Panel A).[40]

Turning to quality of goods, PDS dealers are alleged to sometimes adulterate the goods they sell (e.g. by adding sand or stones to wheat) or sell spoiled goods (e.g. rotten grains). This may have increased due to the treatment if dealers try to substitute for loss of rents on the quantity margin. We test this in two ways. First, we asked respondents who had completed purchases whether they received adulterated or low-quality goods. Overall, few

[39]By default the government provided rice to rural blocks, but prior to the reform it made exceptions for those that expressed a desire for wheat, providing them with rice and wheat in 3:2 proportions. After the reform it appears to have reduced such exceptions.

[40]Interestingly, dealers do report facing lower prices, notably for rice (Table A.5, Panel B). We view these data cautiously as (i) unlike the household reports they are not based on actual transactions, and may not reflect the pricing that is relevant to the beneficiaries whose welfare we wish to examine, and (ii) only the effect on the price of rice survives the adjustment for multiple testing.

beneficiaries report experiencing these issues and rates are unaffected by treatment (Table 4). In the control group, reported adulteration rates range from 1% to 9% and none change significantly with treatment. Reported rates of quality issues are similarly low with the exception that 38% of control households report receiving low-quality salt; this rate is 6% lower among treated households, with the difference significant before but not after adjusting for multiple comparisons.

Second, we asked respondents what amount of money they would have been willing to accept in lieu of the bundle of goods they purchased at the FPS in each month. This metric has important limitations; it measures stated as opposed to revealed preferences, and requires asking subjects a series of questions which they often find confusing.[41] On the other hand, it has the advantage of capturing all aspects of both quantity and quality as perceived by the beneficiaries. When we replace our default measure of total value received with this alternative measure, we estimate an insignificant reduction in value received of Rs. 11 per month, equal to 1% of the control group mean. A 95% confidence interval for the treatment effect is $[-5.5\%, 3.6\%]$, letting us reject substantial changes in value received in either direction.

4.1.3 Leakage

Given that value disbursed increased slightly while value received was unchanged, we do not expect to find reductions in leakage. Panel C of Table 3 tests this directly. We use a Seemingly Unrelated Regressions framework with the ration card $\times$ month as the unit of analysis and with (i) value received as reported by the household, and (ii) value per ration card disbursed to the corresponding block as the dependent variables, and then report the difference between the estimated treatment effects on these variables.[42]

We estimate that leakage increased insignificantly by Rs. 14 per ration card $\times$ month. We can reject large decreases in leakage: a 95% confidence interval is $[-10, 38]$ which lets us reject changes in the share of value lost outside of $[-1.7\%, 6.6\%]$.

Because the value figures in Table 3 are based on the difference between market and statutory ration shop prices, they pick up leakage on the quantity margin (e.g. the diversion

[41] For households that purchased PDS commodities in a given month we elicited their stated willingness-to-accept (WTA) by asking for a series of values ranging from Rs. 100 to Rs. 2,000 whether they would have preferred to receive that amount of money to the opportunity to purchase the commodities they obtained. We define their WTA as the smallest amount of money for which they answered "yes." For households that did not purchase any PDS commodities in a given month we define WTA as zero. A high proportion (48%) of respondents answered "yes" for a range of values but then subsequently answered "no" for at least one higher value, likely reflecting confusion about the nature of exercise. We believe that the lowest "yes" coding is the most reasonable way to interpret the data, but clearly they should be treated with caution.

[42] This approach lets us take advantage of the fact that we observe the universe of disbursements while also exploiting potential efficiency gains due to covariance in the error terms in the two equations.

of food grains) but may not pick up leakage due to overcharging by the FPS dealer. We examine this separately in Panel D of Table A.5. Overall, we estimate that the average control group household overpaid by Rs. 8 for the bundle of commodities it purchased, representing a small share (less than 2%) of total value received. Treatment reduced overcharging by a statistically insignificant Rs. 2.6. This makes sense given that the intervention did not directly change marginal (dis)incentives for over-charging.

4.2 Transaction costs

Using household survey data, we estimate that the average control group household spent the monetary equivalent of Rs. 41, or 9% of mean value received, in order to collect its benefits in March 2017. We calculate this using information on the individual trips they took to the ration shop, whether each trip succeeded, the time each trip took, and any money costs incurred (e.g. bus fare), as well as information on the opportunity cost of time of the household member who made the trip. Treatment increased these transaction costs by a small but significant amount: Rs. 7, or around 1.5% of value received and 17% of the control mean (Table 5, Column 1). In Table A.6 we examine impacts on the variables that feed into our total cost measure; the cost increase appears to be due to (i) a significant increase in the number of trips that were unsuccessful in the sense that they did not result in any purchases, which doubled from 0.13 per household per month to 0.26, and (ii) an increase in the opportunity cost of time of the household member who collected benefits, consistent with the idea that the reform reduced households' flexibility to send whoever could be spared from other work.

Using dealer survey data, we reject economically meaningful treatment effects on dealer costs of storing and transporting grains (Table 5, Columns 2-3), consistent with the lack of impact on quantities. Finally, using official records, we calculate that the cost of ePOS deployment was Rs. 6.2 per ration card per month, which was a 5% increase on GoJH's base of cost of Rs. 144 per ration card per month operating the PDS.[43] Thus, overall transaction costs across the government, dealers, and beneficiaries increased by Rs. 13.6 per ration card per month, which represents a non-trivial 7.8% increase on a base of Rs. 175.

[43] The government paid around Rs. 1,600 per month per ePOS machine to an IT provider inclusive of equipment rental, maintenance, and training. The average FPS in our data has 257 households, yielding an incremental cost of Rs. 6.2 per ration card per month. While it is possible that some administrative costs associated with paper-based record keeping were reduced (including time taken to do so), these savings were not reported in any official spending records. Thus, we treat the costs of ePOS deployment as the change in administrative cost in treatment areas

4.3 Food security

Given the results above, we would not expect to see impacts on food security outcomes.[44] Table 6 confirms this. We examine two measures of a household's food security: a food consumption score that follows standard World Food Program methodology to calculate a nutrient-weighted sum of the number of times a household consumed items from each of a set of food groups in the last week, and a simple food diversity score defined as the number of groups from which the household consumed any items in the past week.[45] We see a tightly estimated null effect of treatment, with 95% confidence intervals expressed in control group standard deviations of $[-0.11\sigma, 0.12\sigma]$ and $[-0.11\sigma, 0.09\sigma]$ respectively.

4.4 Distributional and heterogeneous effects

The notion that stricter ID requirements should trade off reductions in errors of inclusion (including, broadly defined, leakage) against increases in errors of exclusion itself suggests that effects are likely to be heterogeneous: many may be unaffected, or affected only to the extent that transaction costs change, while the main risk is that some lose access to most or all of their benefits.

The distributional effects of treatment suggest this was the case. Figure 3 plots the CDFs of value received in the treatment and control groups separately; these track each other closely except for values close to zero, where there is more mass in the treatment group. The probability that a treated household received zero value is 2.4 percentage points higher than a control household (Table 7, Column 1), significant at the 10% level.

For a sharper test, we examine how impacts differed for the household we would expect to be most likely to lose access to benefits, namely the 23% of households that were "unseeded" at baseline: those that did not have at least one member whose Aadhaar number had been linked to their ration card. These households are generally poorer, less educated, and of lower caste than "seeded" households.[46]

Losses in value received are concentrated among unseeded households. Table 8 reports estimated treatment effects split by this variable. The reform lowered value received by

[44] Note that any treatment effects through indirect impacts on access to *other*, non-PDS benefits which also required Aadhaar is likely to be minimal since the difference in Aadhaar registration rates between treatment (96%) and control (92%) was only 4%.

[45] For more details on these methods including the weights for each food group, which are defined based on the group's nutrient density, see `http://documents.wfp.org/stellent/groups/public/documents/manual_guide_proced/wfp197216.pdf?_ga=1.115126021.300736218.1470519489`

[46] Figure A.2 plots the distributions of household income and mean years of schooling completed for the two most educated household members, separately by seeded status, and in both cases a Kolmogorov-Smirnov test rejects equality of distributions. Unseeded households are also 5% less likely to be upper caste ($p < 0.01$).

Rs. 49 per month for unseeded households, equivalent to 12.6% of the control group mean for this category. This is significantly different from zero as well as from the mean effect among seeded households. On the extensive margin, treatment lowered the probability that unseeded households received any benefit by 10 percentage points, also significantly higher than the (insignificant) impact on seeded ones. While we cannot of course identify specific households that counterfactually would not have been excluded, this decrease fully accounts in an accounting sense for the overall decrease in the fraction of households reporting receiving any benefits in a given month. Treatment effects on stated willingness to accept are also significantly lower for unseeded households, though not in this case significantly different from zero. Transaction costs, on the other hand, increase slightly more for seeded households, consistent with the idea that they are the ones able to continue transacting with the system, albeit at a higher cost (and that unseeded households may not have bothered making multiple trips).

Overall, the results suggest that the reform did cause a significant reduction in value received for the households least ready for the reform, likely driven by the total loss of benefits of a subset of these households. Multiplying the 2.4 percentage point increase in the likelihood that a household in a treated block received no benefits (Table 7, Column 1) by the total number of PDS beneficiaries in treated blocks (6.25 million), we estimate that around 150,000 beneficiaries were likely denied benefits in treated blocks alone. If we extrapolate to include the 7 non-study districts (that our study sample was representative of and which rolled out ePOS everywhere) in addition to treated study blocks, we estimate that 296,000 beneficiaries were denied benefits.

We also examine heterogeneity along several additional pre-specified dimensions, including (i) characteristics likely to matter for understanding the distributional and political consequences of the reform such as caste, education level, and income level, and (ii) characteristics of the location likely to predict heterogeneity in the implementation of the reform such as rural status, cellular network signal strength, and the device mode (online, partially online, or offline).[47] In general we find limited evidence of heterogeneity along these dimensions (Tables A.7, A.8, and A.9.). There is some evidence that wealthier and better-educated households receive differentially more value and that wealthier households incur larger increases in transaction costs.

[47]Note that machine mode was not randomly assigned; we identify heterogeneous effects by assuming that a control FPS would have been used the same machine type it was ultimately assigned once it received treatment. Further details are in the notes to Table A.9.

4.5 Stakeholder preferences and perceptions

Overall, views on the reform were sharply divided (Table 9). Fifty-three percent of households and 51% of dealers preferred the reform to the status quo method of authenticating. Even among unseeded households, which were most hurt as a group, 50% of households prefer the reform to the status quo. Views are quite polarized, with 89% (87%) of households (dealers) holding a strong as opposed to a weak view one way or the other. One interpretation is that respondents view the question as being as much about their aggregate policy preferences as about the direct effect of the policy on their wallets. Indeed, respondents often framed their responses in broader terms (for example, "I prefer the machine because finally the government is doing something about the PDS"). These responses highlight how even a reform that hurts a significant minority of citizens may still be politically viable if perceived as being done in the public interest (and may be supported by a majority of voters).

We also asked dealers their expectations about future business prospects (Table 10). Roughly the same share of treated dealers expect to continue running their FPS (Columns 1-2), but they predict that the going price to obtain a dealer's licenses in the first place will drop substantially, by 72% (Columns 3-4). Expected payments to renew a license turn out to be negligible and unaffected by treatment (Columns 5-6). We interpret this result cautiously given that it is a sensitive question and only a minority of dealers provided an answer. That said, it is intriguing that those who did answer expect the price of licenses to fall substantially, despite the fact that the reform requiring Aadhaar-based authentication to avail PDS by itself had not affected leakage. One possibility is that dealers in treated blocks anticipated that the government would soon begin using authenticated transaction data to reconcile commodity balances and that this would meaningfully reduce their ability to divert grain onto the open market. We turn to this next.

5 Reconciliation & ration card deletion

5.1 Policy implementation

GoJH began allocating its disbursals of rice and wheat based on reconciliation of authenticated transaction records from FPSs in July 2017. Anecdotally, reconciliation was only partially implemented. FPS dealers pressed for and were often granted adjustments or exceptions to offset its effects (we discuss reasons further below). In Appendix C we examine in more detail how strictly the government implemented its stated reconciliation formulae. Generally speaking, the policy had bite, but the government clearly made numerous exceptions and disbursed grain somewhat more leniently than a strict "by the book" implementation would have implied. The results that follow should thus be interpreted as the effect of the rules as actually implemented, net of various adjustments and exceptions which were made.

5.2 Effects of reconciliation on value transfer

Overall, the onset of reconciliation coincided with a drop in both disbursements and receipts of reconciled commodities (rice and wheat), but not of unreconciled ones (sugar, salt and kerosene). Figure A.3 illustrates this, plotting the evolution of value disbursed (Panel (a)) and received (Panel (b)) separately for reconciled and unreconciled commodities. It also overlays the raw data with the fit we obtain from estimating Equation 2 and 95% confidence bands around this fit.[48] For both series our pre-specified functional form fits the temporal patterns quite well.

For reconciled commodities, both value disbursed and received show little change until the onset of reconciliation, after which both drop sharply. They then rebound gradually until October, before GoJH suspended reconciliation in November (see more below). For unreconciled commodities – arguably a control or placebo group – both value disbursed and received drift slightly downwards over time without any substantial change during the period of reconciliation. If anything, the value of unreconciled commodities received during this period was high relative to trend.

The pattern we see for value of reconciled commodities disbursed from GoJH to FPSs is in line with the government's plans. GoJH's view was that dealers should have been holding grain stocks equivalent to the opening balances recorded on the ePOS machines at the start of the reconciliation period and would initially be able to meet obligations to beneficiaries by drawing these down, after which they would require fresh disbursements. As a corollary, disbursements from GoJH to FPSs would fall initially but then gradually rebound, which is exactly what we see in the data. As per the government's intentions, beneficiaries should not have been affected in this scenario as dealers would be able to fully supply grains in spite of the temporarily reduced flow of new disbursements from GoJH by drawing down their retained stocks.[49]

[48] We value commodities using market prices obtained from follow-up 1, as follow-ups 2 and 3 did not elicit updated market price data. The evolution of value metrics thus reflects the evolution of quantities.

[49] To illustrate, consider a dealer who distributes 85% of his allocation each month, but continues receiving a 100% grain allocation each month. If ePOS records were available for 8 months before reconciliation, the records would show an opening balance of $8 \times 15\% = 120\%$ of a month's supply and the government would not disburse any grains in month 1 of reconciliation, as the dealer's exiting stocks should be sufficient for all registered beneficiaries. If the dealer distributes 85% from those stocks in month 1, his opening balance for month 2 would be $120\% - 85\% = 35\%$, and the government would disburse 65% of a month's stock to bring the stock to 100%. By month 3, a new steady state would be reached in which the government disburses 85% each month as opposed to 100% and the dealer continues to distribute 85% to beneficiaries.

In the data, however, we see that a meaningful share of the drop in disbursements was passed through to beneficiaries in the form of lower value received. This likely reflects the fact that many dealers did not have as much grain stored as ePOS-based records indicated they did, since some of that grain had previously been diverted. As a result the drop in disbursements left them unable to meet their full obligations to beneficiaries. This likely explains why the government came under pressure to adjust its stock records and/or issue waivers to dealers, and why the data show that it adhered only loosely to the reconciliation algorithm it had defined (Appendix C).

In short, GoJH had hoped to achieve a "good" equilibrium in which the threat of reconciliation was enough to ensure that grain was not diverted, allowing it to cut rents from FPS dealers and other intermediaries without harming beneficiaries. However, once the grain had been diverted, the government was not able to implement reconciliation without causing some politically costly harm to beneficiaries, as dealers could and did pass on some of the pain of reduced disbursals to them. This made the threat of reconciliation costlier for GoJH to follow through on (and perhaps less credible in the first place).

In Table 11 we quantify these effects by treatment arm. Figure 4 provides the corresponding plots split by treatment arm. In the control group (which had ABBA for fewer months before the onset of reconciliation), we estimate that value disbursed fell by Rs. 92, or 19%, in the first month of reconciliation. Of this, an estimated 22% (Rs. 20) represents a reduction in value received by legitimate beneficiaries (Panel B), while the remaining 78% represents a reduction in leakage (Panel C). If we estimate Equation 2 with an indicator for receiving any grain as the dependent variable to examine the extensive margin, we estimate that the share of beneficiaries receiving no value increased by 4.3 percentage points. Averaging over the full 4-month period of reconciliation, we estimate that disbursals were Rs. 46 (9%) lower per month than they otherwise would have been, and that of this drop 34% was passed on as a reduction in benefits, with the remaining 66% representing a reduction in leakage.

The effects of reconciliation should have been more pronounced in treated areas, because dealers in treated blocks had ePOS devices for longer than those in control blocks (11 months vs 2), and had therefore accumulated greater recorded "opening balances" of undistributed grain by the beginning of July 2017. Figure 5 plots the distribution of total grain stocks at end of June for the treatment and control group separately, illustrating the difference. On average, the government held treated shops responsible for 7,716 kg of undistributed grain as opposed to 3,346 kg for control shops ($p < 0.0001$).

In line with this intuition, reconciliation in treated areas had larger estimated effects overall and generated a less advantageous tradeoff between errors of exclusion and inclusion. We estimate that value disbursed to treated blocks fell by Rs. 182, or 38%, in the first month of reconciliation. Of this, an estimated 34% (Rs. 62) represents a reduction in value received by legitimate beneficiaries (Panel B), while the remaining 66% represents a reduction in leakage (Panel C). In each case these figures are significantly larger than the corresponding figures for control areas.[50] On the extensive margin, the share of beneficiaries receiving no value increased by 13 percentage points. Multiplying this figure by the number of beneficiaries in treated areas for which our data are representative, we estimate that *1.6 million* more people did not receive PDS benefits during the first month of reconciliation. Averaging over the full 4-month period of reconciliation, we estimate that disbursals were Rs. 86 (17%) lower per month than they otherwise would have been, and that of this drop 49% was passed on as a reduction in benefits, with the remaining 51% representing a reduction in leakage.

As a result of these fairly dramatic reductions in benefits received, reconciliation triggered considerable negative press coverage and complaints from both beneficiaries and FPS dealers. Further, the larger reduction in benefits received in treated areas are more representative of the overall experience in the state because the full state was treated except for the control blocks, which account for only 13% of the state's total ration cards. Under pressure, GoJH granted numerous waivers or exemptions and, ultimately, decided to temporarily rescind reconciliation starting in November 2017. GoJH then restarted reconciliation in August 2018 (after our study period).

5.3 Clean-slate reconciliation

This episode highlights the general importance of managing the introduction of potentially disruptive new policies carefully, and raises the question of whether there are ways GoJH could have better managed this particular transition. Recall that from the point of view of dealers the reconciliation reform had both an incentive effect (increasing their incentive to record authenticated transactions, and thus potentially increasing beneficiaries' bargaining power) and a resource constraint effect (reducing available grains to distribute to the extent the dealers stocks were less than the recorded opening balances). The former effect should tend to help beneficiaries, while the latter seems more likely to hurt them. If this logic is correct, it suggests that reconciliation might have impacted beneficiaries more (less) adversely if the resource constraint was tighter (looser). Thus, one alternative would have been to introduce a "clean-slate" reconciliation, using the same algorithm to determine the amount shipped each month but setting the *initial* values of stock for which dealers were held accountable to zero as opposed to the cumulative opening balances from prior months.

[50] The *proportion* of the drop in value disbursed that represents a reduction in benefits received is higher in treated than in control areas, by 34% to 22%, (though statistically we do not reject the null that these proportions are the same, $p = 0.18$).

We can use our experimental variation to extrapolate and predict the likely results of such a policy. Specifically, we estimate Equation 2 fully interacted with the FPS-level stock balance as of the beginning of July 2017, expressed per ration card (the "opening balance"). Since opening balances are endogenous, but vary systematically with assignment to early v.s. late treatment, we instrument for balance (and its interactions) with assignment to treatment (and its interactions). This instrument is valid if assignment to treatment altered the effects of reconciliation only through its effects on opening balances. We then interpret the point estimates of the main effect of reconciliation, with opening balances set to zero, as estimates of the initial effects of a clean slate reconciliation.

The estimates from this procedure suggest that a clean-slate reconciliation would have weakly *increased* value received by beneficiaries (Table A.10). Column 4 examines effects on value received; setting the opening balance to zero, reconciliation would have increased value received (insignificantly) by Rs. 7.3. The interaction of reconciliation with opening balance is significantly negative, implying that it was the opening balances for which dealers were held accountable rather than the reconciliation policy per se which led to the drop in benefits received. Turning to value disbursed, the estimates suggest that value disbursed would have declined by Rs. 59 per ration card (Column 1), substantially less than the drop we see in our data although still greater than zero.

Overall, this exercise suggests the government could have achieved more modest reductions in leakage without adversely affecting beneficiaries. It should be interpreted with caution as it necessarily involves extrapolation: the mean opening balance in the control group, while only 32% of that in the treatment group, was not zero, so that we do not directly observe the consequences of reconciliation at a zero balance. That said, the results are consistent with the basic intuition that the incentive effects of reconciliation should benefit recipients, while the resource constraint effects may not.

5.4 Ration card deletion

We close by examining the deletion of ration cards, another margin along which the government traded off errors of inclusion and exclusion during the rollout of Aadhaar. Some card deletion was normal, due for example to the death of beneficiaries, dissolution of beneficiary households, or discovery of fraud. From 2014-2016, for example, GoJH reported deleting 2% of the total number of ration cards.[51] From October 2016 to May 2018 we observe 6% of ration cards removed from the database for the 10 districts we study, suggesting that the overall rate of deletion was higher after the rollout of ABBA in the PDS.

[51] Data from `https://data.gov.in/resources/stateut-and-year-wise-number-deleted-ration-cards-2014-2018-ministry-consumer-affairs-food`, accessed 12 July 2019.

Our survey data allows us to match these administrative actions with ground reality on households whose cards were deleted (non-experimentally). The top panel of Table 12 examines the universe of ration cards in our 10 study districts. We define a card as deleted if it appeared in the list we obtained in October 2016, but not the list we obtained in May 2018. We define a card as unseeded if no household member had seeded an Aadhaar number to that card as of October 2016. We observe that unseeded cards were substantially more likely to be deleted during this period, with 36% of unseeded cards $(80,085/213,089)$ deleted compared to 2% $(64,076/2,236,521)$ of seeded cards.

Two key questions are whether and how effectively this removal of unseeded cards differentially eliminated fraudulent accounts. In the bottom panel we restrict the analysis to our sampled ration cards, for which we can categorize beneficiaries as ghosts or not based on the results of our survey. We estimate that unseeded cards are disproportionately likely to belong to ghosts, who make up 9.5% (21/232) of unseeded cards, as opposed to just 0.8% (32/3616) of seeded cards ($p < 0.001$ from a two-way Pearson χ^2 test). Deleting unseeded cards does thus differentially target ghost accounts. However, because ghosts make up such a small share of the overall population, we estimate that the great majority (88%, or $(90 + 97)/213$) of the deleted cards, and even of the deleted unseeded cards (84%, or $90/(15 + 90)$), belonged to real households. One would thus need to cost inclusion errors at more than five times the cost of exclusion errors for these terms to be advantageous.

To be clear, our data do not allow us to estimate a causal effect of ABBA on card deletions. Further, deletions (even of real households) do not by themselves imply exclusion of deserving beneficiaries. For example, a card might remain unseeded because a (relatively) wealthy holder did not think it worth the trouble to seed it, and deleting this card would be desirable.[52] At the same time, data from our field surveys show that a considerable number of deleted cards corresponded to real households. This illustrates another way in which attempts to reduce leakage may come at the cost of exclusion error of genuine households.

6 Conclusion

Navigating the tradeoffs between errors of inclusion and exclusion in social programming is a common challenge facing governments worldwide. This is especially so in developing countries such as India where state capacity is limited and lives may literally be at stake. In this paper we examine the effect on inclusion and exclusion error of stricter identification requirements, and do so in the context of Aadhaar, the world's largest biometric ID system.

[52] In our data, cards held by the better-off among the unseeded are deleted somewhat more often, but this pattern is not statistically significant.

On its own, Aadhaar-based authentication of transactions in the PDS had no measurable benefit; it slightly increased mean transaction costs for beneficiaries, excluded a minority who did not have IDs "seeded" to their ration cards at baseline, and did not reduce leakage. When paired with the new reconciliation protocols, ABBA facilitated a meaningful reduction in leakage but at the cost of concurrent reductions in mean value received by legitimate beneficiaries, as well as a considerable increase in the fraction who were denied benefits. These results illustrate how the costs of controlling corruption may include indirect "collateral damage" beyond the direct costs of intervening. They also highlight how intermediaries can pass on the pain of anti-corruption measures to more vulnerable beneficiaries, and the importance of considering and studying the incidence of anti-corruption measures.

Juxtaposed with prior evidence, our results also demonstrate the range of impacts that "biometric authentication" can have depending on the specific ways it is used. The comparison of these results with our own prior work (reported in Muralidharan et al. (2016)) on the impacts of biometric authentication in a public employment program and a pension program in the state of Andhra Pradesh (AP), is especially illustrative. A key point to note is that *both* programs reduced leakage. However, in the case of AP, the money saved from reduced leakage was passed on to beneficiaries, while there were no savings to the government. In contrast, in the case of Jharkhand, the reduced leakage in the PDS led to reduced disbursals from the government, but did not improve the beneficiary experience in any way (and worsened it in some ways). In other words, the technology of biometric authentication "worked" in both settings in terms of reducing leakage. But the question of how the benefits of this leakage reduction should be shared between the government and beneficiaries is ultimately a design question and also a political one.

Overall, the reforms in AP focused more on improving the beneficiary experience and less on fiscal savings (as seen by their generous manual override provision, which reflected a strong priority on preventing exclusion errors). In contrast, the reforms in Jharkhand (implementing the policy decision of the Government of India) focused more on reducing fraud and generating fiscal savings. The differences in results across the studies are consistent with this difference in emphasis.[53] Taken together, our results suggest that "biometric authenti-

[53] This difference in emphasis is also consistent with patterns of political priorities and messaging around the world. The government in AP was run by a center-left party, and correspondingly placed greater emphasis on preventing exclusion. The government in Jharkhand was of a center-right party (and was implementing a policy priority of a center-right party at the central government) and correspondingly focused more on reducing fraud. Both sets of emphases are legitimate political choices, and our data on beneficiary opinions on ABBA suggests that political messaging around the importance of "fraud reduction" can be appealing to a majority of voters even if a significant minority is hurt. Thus, our results may also be seen as illustrating the limits of electoral democracy as an instrument for protecting the vulnerable and the importance of institutions like Courts for securing the rights of such groups even under democratically elected governments.

cation" per se may not be a particularly stable or helpful construct for policy analysis: the design details of how the technology is used and how it alleviates (or aggravates) existing constraints matter a lot.

Another key lesson to note is the importance of credible evaluations of major welfare reforms. In the AP case, it would have been easy to think that there was no impact on leakage because there was no change in government expenditure on welfare programs. It was only with the matched data between administrative records and household surveys (and the existence of a control group) that we could see that leakage had fallen sharply and that more benefits were reaching people. Conversely, in the case of the PDS in Jharkhand, it would have been easy to interpret the reduction in disbursals as evidence of reduced leakage (and indeed, officials often made this claim). However, it was the matched data using household surveys that clarified that at least some of the reductions in disbursal were coming at the cost of exclusion errors.

Our results suggest that it is important to both build in procedures to guard against exclusion error at the program design phase, as well as monitor this during the implementation phase. Some design choices that may improve the terms of the trade-off between leakage reduction and exclusion errors include: (i) authenticating beneficiary lists (say annually) rather than transactions,[54] (ii) creating alternative methods of authentication or override mechanisms,[55] and (iii) conducting reconciliation on a clean-slate basis. Our estimates suggest that the last option might have been effective at reducing leakage without increasing exclusion errors.

However, regardless of design safeguards, policy making can also benefit from improving real-time visibility on the last-mile beneficiary experience with welfare programs to enable rapid course correction of policies that may be hurting vulnerable populations. One promising way of doing this may be to use outbound call centers to call representative samples of beneficiaries regularly and measure if they are receiving their benefits. Recent evidence suggests that such an approach may be a scalable way of measuring and improving last-mile service delivery (Muralidharan et al., 2019b).

[54] Gelb and Metz (2018) similarly argue that "point-of-service biometric authentication for every service can... be overkill" and that "programs also need to allow for people who cannot register or authenticate themselves in the regular way..." On the other hand, authenticating every interaction may be valuable in cases where physical attendance is an important margin. Bossuroy et al. (2019) find for example that requiring (non-Aadhaar) biometric authentication in health clinics increased adherence, and reduced over-reporting of adherence, to a tuberculosis treatment regimen. It may also be important for the portability of benefits, whereby beneficiaries (especially internal migrant workers) can access their benefits outside their home location.

[55] For example, Aadil et al. (2019) find few reports of exclusion from PDS benefits due to ABBA in Krishna district in Andhra Pradesh, which they attribute to the availability of several override mechanisms.

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Table 1: Representativeness within Jharkhand

	Study district	Non-study district	Difference	p-value
	(1)	(2)	(3)	(4)
Panel A: 2011 Census				
Population in 2011	1,267,604	1,450,864	-183,260	0.50
Population growth, 2001-2011	0.23	0.24	-0.02	0.56
Population density	451	459	-8	0.94
% Literate	0.62	0.66	-0.04	0.22
Panel B: Beneficiary List				
Ration cards per FPS	308	293	15	0.45
Beneficiaries per FPS	981	1,041	-60	0.33
% FPS rural	0.92	0.89	0.03	0.39
% AAY beneficiares	0.18	0.18	0	0.92
Number of blocks	13.20	12	1.20	0.58
Panel C: NSS 68				
% With salary income	0.11	0.16	-0.04	0.25
Monthly per capita consumption	1,097	1,298	-201	0.10
Consumption value food	4,050	3,518	532	0.34
Consumption value fuel/light	506	462	44	0.16
N	10	14		

This table compares the 10 districts studied with the remaining 14 districts in Jharkhand using data from the 2011 and 2001 Censuses (Panel A), the PDS beneficiary list prior to baseline (Panel B), and the 68th Round of the National Sample Survey (NSS 68) (Panel C). Column 3 reports the raw difference in means between columns 1 and 2. Column 4 reports the p-value from a test of equality of means. “Population density” is in population per square mile. “Ration cards per FPS” is the ratio of PDS ration cards to the number of FPSs. “Beneficiaries per FPS” is the ratio of PDS ration cards to the number of FPSs. “% FPS rural” is the share of FPSs located in areas classified as rural. “% AAY beneficiaries” is the percentage of PDS beneficiaries covered by the more generous Antyodaya Anna Yojana (AAY) scheme. “% With salary income” is the share of the population that reports earning a salaried income. “Monthly per capita consumption” is household monthly per capita consumption in Rs. “Consumption value food” is household monthly expenditure on food in Rs. “Consumption value fuel/light” is the household monthly expenditure on fuel and lighting in Rs. Statistical significance is denoted as: $^{*}p < .10,^{**}p < .05,^{***}p < .01$.

Table 2: Baseline balance and program implementation

	Treatment	Control	Regression-adjusted difference	p-value
	(1)	(2)	(3)	(4)
Panel A: Baseline Characteristics				
Priority households	13080	12292	345	.82
AAY households	2922	2576	125	.68
Aadhaar numbers seeded per rationcard	2.4	2.4	.046	.58
Rice disbursed per priority household	23	20	7.5	.13
Rice disbursed per AAY household	35	32	11	.27
Number of FPS	73	71	-2.1	.8
Median household size	4.4	4.3	.069	.42
% of rationcard holders identified via SECC	.71	.68	.023	.27
% of rationcard holders identified by application	.16	.16	-.0014	.93
% of rationcard holders without eligibility info	.13	.16	-.022	.22
Whether at least one Aadhaar seeded	.77	.8	-.025*	.082
Missing whether any Aaadhaar seeded	.096	.16	-.024	.19
Panel B: Program implementation				
Dealer has an ePOS machine at endline survey	.96	.05	.91***	0.00
Dealer used ePOS in January 2017	.91	.06	.85***	0.00
Dealer used ePOS in February 2017	.91	.06	.85***	0.00
Dealer used ePOS in March 2017	.91	.05	.85***	0.00

This table compares treatment to control blocks within study districts on baseline characteristics (Panel A) which should be balanced due to randomization, and measures of program implementation (Panel B) which should not. Column 3 reports the regression-adjusted difference in means after conditioning on strata fixed effects, and column 4 reports the p-value from a test that this quantity equals zero. "Priority households" is the number of ration cards assigned to households under the priority households scheme; "AAY households" is the number of ration cards assigned to households under the Antyodaya Anna Yojana (AAY) scheme. "Aadhaar numbers seeded per ration card" is the average number of verified Aadhaar numbers seeded per ration card. "Rice disbursed per priority household" is kilograms of rice disbursed per PHH ration card. "Rice disbursed per AAY household" is kilograms of rice disbursed per AAY ration card. "Number of FPS" is the total number of FPSs. "Median household size" is the block median number of household members listed on ration cards. "% of ration card holders identified via SECC" is the share of ration card holders whose eligibility was established using data from the Socio Economic Caste Census. "% of ration card holders identified by application' is the share of ration card holders whose eligibility was determined by local authorities after submitting applications. "% of ration card holders without eligibility info" is the share of ration card holders for which we do not observe how they became eligible. "At least one Aadhaar number seeded" is an indicator equal to one if the household had at least one Aadhaar number seeded to its ration card at baseline. "Missing Aadhaar seeding status" is an indicator equal to one if we do not observe the count of Aadhaar numbers seeded to the ration card at baseline. Estimates in Panel B are weighted by inverse sampling probabilities. "Dealer has an ePOS machine at endline" is an indicator equal to one for endline survey respondents who reported that their FPS dealer had an ePOS machine. "Dealer used an ePOS machine in Month X 2017" is an indicator equal to one for endline survey respondents who reported that their FPS dealer used or attempted to use an ePOS machine in the corresponding month. Statistical significance is denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 3: Effects on value disbursed, value received, and leakage

	Total	Rice	Wheat	Sugar	Salt	Kerosene
	(1)	(2)	(3)	(4)	(5)	(6)
Mean entitlement	595	492	18	19	9	57
Panel A: Value disbursed						
Treatment	12**	35***	-27**	.093	.028	-.25
	(4.9)	(12)	(12)	(.15)	(.045)	(.18)
		[0.05]	[0.16]	[1.00]	[1.00]	[0.68]
Control mean	579	417	72	26	9.4	55
Observations	26,611	26,611	26,611	26,611	26,611	26,611
% of frame	99	99	99	99	99	99
Panel B: Value received						
Treatment	-1.2	17	-15	.55	.51	-.56
	(12)	(10)	(11)	(1.6)	(.58)	(1.1)
		[0.96]	[0.96]	[1.00]	[1.00]	[1.00]
Control mean	463	348	54	14	7.2	40
Observations	10,396	10,557	10,654	10,670	10,726	10,618
% of sample	88	89	90	90	90	89
Panel C: Leakage						
Treatment	14	18	-11	-.46	-.48	.56
	(12)	(12)	(7.2)	(1.6)	(.56)	(1.1)
		[0.72]	[0.72]	[1.00]	[1.00]	[1.00]
Control mean	116	68	19	12	2.1	15
Observations	10,396	10,557	10,654	10,670	10,726	10,618
% of sample	88	89	90	90	90	89

This table reports estimated treatment effects on the value of commodities disbursed by the government (Panel A), received by recipients (Panel B), and the difference (Panel C) in endline one (January - March). The unit of measurement is rupees per ration card-month throughout. In Panel A the unit of observation is FPS $\times$ month and we use the universe of FPSs; in Panels B and C the observation is the ration card $\times$ month and a representative sample of ration card holders in Panels B and C. The dependent variable in columns 2-6 is the relevant quantity of the commodity multiplied by the difference between the median market price of that commodity in control blocks in the same district, and the statutory PDS price for that commodity. The dependent variables in column 1 is the sum of the values in columns 2-6. In Panel C, estimated effects are the difference between estimated effects on block-level mean value disbursed per ration card and value received per ration card, estimated within a Seemingly Unrelated Regression framework. All specifications include strata fixed effects and the baseline value of the dependent variable. Standard errors clustered at the block level are reported in parentheses, with statistical significance denoted as: $^{*}p < .10,^{**}p < .05,^{***}p < .01$. q values adjusted to control the false discovery rate across the five commodities are reported in brackets.

Table 4: Effects on quality of ration received

	Rice	Wheat	Sugar	Salt	Kerosene
	(1)	(2)	(3)	(4)	(5)
Panel A: Adulteration					
Treatment	.0097	.0074	.01	-.0036	-.0019
	(.012)	(.027)	(.0079)	(.0076)	(.003)
	[0.88]	[1.00]	[0.88]	[1.00]	[1.00]
Control mean	.087	.068	.028	.033	.0095
Observations	3,308	971	2,305	2,381	3,205
% of sample	84	25	58	60	81
Panel B: Low quality					
Treatment	-.0014	.045	.014	-.058**	-.0008
	(.0068)	(.031)	(.0088)	(.029)	(.0026)
	[1.00]	[0.58]	[0.58]	[0.52]	[1.00]
Control mean	.036	.069	.056	.38	.0076
Observations	3,329	975	2,319	2,402	3,228
% of sample	84	25	59	61	82

This table reports estimated treatment effects on the quality of commodities received by beneficiaries in endline one (January - March). The unit of analysis is the ration card in both panels. The dependent variable in Panel A is an indicator equal to one if the respondent reported receiving adulterated commodities at least once in the past three months for each of the five commodities. The dependent variable in Panel B is an indicator equal to one if the respondent reported that the overall quality of commodities received over the past three months was "very bad" or "bad" (as opposed to "OK" or "good') for each of the five commodities. In both panels, observation of the outcome is conditional on the ration card holder purchasing a positive quantity of the commodity during January-March 2017. All regressions include strata fixed effects and the baseline value of the dependent variable. Standard errors clustered at the block level are reported in parentheses, with statistical significance denoted as: $^{*}p < .10,^{**}p < .05,^{***}p < .01$. q values adjusted to control the false discovery rate across the five commodities are reported in brackets.

Table 5: Effects on transaction costs

	Beneficiary costs	Dealer costs		Government costs
	(1)	(2)	(3)	(4)
Treatment	6.9^{*}	.51	.65	$6.2^{\dagger}$
	(3.8)	(.95)	(.63)	–
Adjusted R^2	.09	.10	.28	–
Reference group mean	41	6.8	5.9	144
Observations	3,538	441	367	–
% of sample	89	–	93	–
Sample	–	Full	Restricted	–

This table reports estimated treatment effects on measures of transaction costs incurred transferring PDS commodities. In column 1 the unit of analysis is the ration card and the dependent variable is the total cost incurred in March by the household holding that ration card in purchasing or attempting to purchase PDS commodities, including time and money costs (see text for details). In columns 2 and 3 the unit of analysis is the FPS and the dependent variable is the total cost incurred by the dealer to transport and store PDS commodities in an average month in January - March divided by the number of ration cards assigned to that dealer. In column 2 the sample includes all dealers surveyed, including those to whom sampled households switched between baseline and endline; in column 3 it includes only dealers drawn in the original sample. Column 4 reports the mean administrative cost per ration card $\times$ month incurred by the state government to administer the PDS, but does not report an estimated treatment effect as cost data disaggregated by block are not available. All specifications include strata fixed effects, and regressions in columns 1-3 include the baseline value of the outcome variable. Standard errors clustered at the block level are reported in parentheses with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

† Estimated separately using vendor costs. We do not calculate if this is significantly different from that in the control group.

Table 6: Effects on food security

	Dietary diversity score	Food consumption score
	(1)	(2)
Treatment	-.011	.08
	(.061)	(1)
Adjusted R^2	.05	.10
Control mean	5.7	43
Observations	3,578	3,578
% of sample	90	90

This table reports estimated treatment effects on measures of food security in March. The unit of observation is the ration card. The dependent variable in column 1 is the sum of a series of indicators each equal to one if the household has consumed any items from within a major food group during the previous week. The dependent variable in column 2 is a weighted sum of the number of times the household consumed items from each major food group in the past week, with weights based on the group's nutrient density.The major food groups are: main staples, pulses, vegetables, fruit, meat and fish, milk, sugar, oil, and condiments. The definition of food groups and their weights can be found from the World Food Programme. All regressions include strata fixed effects. Standard errors clustered at the block level are reported in parentheses. Statistical significance is denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 7: Effects on the extensive margin of value received

	Any Commodity	Rice	Wheat	Sugar	Salt	Kerosene
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-.024*	-.025	-.064	-.011	.0049	-.013
	(.014)	(.016)	(.047)	(.019)	(.018)	(.018)
		[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Adjusted R^2	0.10	0.10	0.32	0.05	0.04	0.10
Control mean	.85	.83	.28	.28	.29	.75
Observations	10,396	10,557	10,654	10,670	10,726	10,618
% of sample	88	89	90	90	90	89

This table reports estimated treatment effect on the extensive margin of the values received by beneficiaries per month in endline one (January - March). The unit of analysis is the ration card-month. The dependent variable in columns 2-6 is an indicator equal to one if the ration card holder received a positive quantity of the commodity in a given month. The dependent variable in column 1 is an indicator of whether the household received a positive quantity of any commodity in a given month. All regressions include strata fixed effects and the baseline value of the dependent variable. Standard errors clustered at the block level are reported in parentheses, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$. q values adjusted to control the false discovery rate across the five commodities are reported in brackets.

Table 8: Heterogeneous effects by Aadhaar seeding

	At least one Aadhaar seeded?		
	(1) No *(N=797)*	(2) Yes *(N=2645)*	(3) Δ
Value received (market prices)	-49***	.054	49***
	(18)	(14)	(15)
Value received>0	-.1***	-.023	.079***
	(.024)	(.015)	(.022)
Value received (WTA)	-31	37*	68*
	(36)	(22)	(35)
Transaction costs	6.8	8.9**	2.1
	(6.6)	(4.3)	(6.9)

This table reports estimated differential treatment effects by Aadhaar seeding status in endline one (January - March). Column 1 (2) reports estimated treatment effects for households that did not (did) have at least one member with an Aadhaar number seeded to their ration cards at baseline. Column 3 reports the difference between these effects. Each row represents a different primary outcome; all estimates are derived from a single underlying regression that interacts treatment with an indicator equal to one for households with one or more Aadhaar numbers seeded. All specifications include strata fixed effects and the baseline value of the dependent variable when available. Standard errors clustered at the block level are reported in parentheses with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table 9: Perceptions of the ePOS intervention

	Households	Dealers
Overall, do you prefer ePOS to the old system of manual transactions?	*(N=2182)*	*(N=288)*
Strongly disagree	.44	.45
Weakly disagree	.03	.04
Weakly agree	.08	.09
Strongly agree	.45	.42
Did not know/answer	.01	–
Reasons for preference of manual transactions:	*(N=1023)*	*(N=150)*
It is cheaper to run FPS operations	–	1
Manual transactions faster	.66	.75
Manual transactions easier to understand	.44	.62
There are no problems with network or software	.28	.74
Anyone can collect rations on my behalf	.43	.06
Could give ration to those who did not have ration cards	–	.03
Dealer to verify amounts purchased	.01	–
It is more profitable	–	.01
Other	.01	.02
Reasons for preference of ePOS enabled transactions:	*(N=1165)*	*(N=137)*
ePOS transactions are faster	.54	.60
There is a lower chance of fraud by the FPS dealer	.56	–
The official transaction is equal to what I receive	.14	–
I know my exact ration entitlement and payment amounts	.15	–
Nobody else can collect ration in my name	.38	–
Ration balance carry forward if I don't collect	.06	.13
I receive physical receipts after ePOS transactions	.18	–
I receive text messages after ePOS transactions	.02	–
The dealer calls me to buy ration as he cannot hide supply	.07	–
Better relationship with beneficiaries	–	.64
Beneficiaries are more informed	–	.35
Nobody can steal ration from beneficiary	–	.48
Other	–	.02

This table reports summary statistics of households' and FPS dealers' stated preferences for and perceptions of the ePOS intervention in March. The sample is restricted to households and dealers in treated blocks. In Panel B the sample is further restricted to respondents who said they strongly or weakly disagreed in Panel A, while in Panel C it is restricted to those who strongly or weakly agreed. Estimates are weighted by inverse sampling probabilities. Some values are missing because the list of options provided to households and dealers differed.

Table 10: Effects on dealer expectations

	Intends to continue running FPS?		Expected bribes to obtain license?		Expected bribes to renew license?	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	.054	.033	-56,816**	-58,393*	-111	-83
	(.055)	(.059)	(28,561)	(33,370)	(123)	(147)
Adjusted R^2	.093	.13	.31	.27	.053	.035
Control mean	.73	.71	76,590	81,188	565	555
Observations	437	366	150	127	370	307
% of sample		92		32		78
Sample	Full	Restricted	Full	Restricted	Full	Restricted

This table reports estimated treatment effects on measures of FPS dealers' expectations in March about the future. The unit of analysis is the FPS. The dependent variable in columns 1-2 is an indicator equal to 1 if the dealer responded "yes" when asked whether they intended to continue running an FPS for the next two years and to 0 if they responded "maybe" or "no." The dependent variable in columns 3-4 is the dealer's estimate of the additional money (excluding official fees) someone would have to pay to obtain a new license to operate a FPS. The dependent variable in columns 5-6 is the dealer's estimate of the additional money (excluding official fees) an existing FPS dealer would have to pay to renew his or her license. In columns 1, 3 and 5 the sample includes all dealers surveyed, including those to whom sampled households switched between baseline and endline; in columns 2, 4 and 6 it includes only dealers drawn in the original sample. All specifications include strata fixed effects. Standard errors clustered at the block level are reported in parentheses with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table 11: Effects of reconciliation

	Reconciled			Unreconciled		
	Treatment	Control	Difference	Treatment	Control	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Mean entitlement	512	504	8	71	68	3
Panel A: Value disbursed						
Reconciliation	-182***	-92***	-90***	-2.1***	.24	-2.3***
	(2.3)	(2.9)	(3.8)	(.24)	(.39)	(.46)
Reconciliation * Month	64***	31***	33***	8.2***	7.9***	.3
	(1)	(1.2)	(1.6)	(.11)	(.15)	(.19)
January 2017 mean	496	488		86	83	
Observations	66,404	31,350		66,404	31,350	
% of frame	96	96		96	96	
Panel B: Value received						
Reconciliation	-62***	-20**	-43***	19	-4.1**	23
	(7.4)	(7.9)	(11)	(32)	(1.7)	(32)
Reconciliation * Month	13***	2.9	10*	-8.3	2.2**	-10
	(3.9)	(4.2)	(5.7)	(8.8)	(.84)	(8.9)
January 2017 mean	406	408		66	60	
Observations	25,469	13,447		25,349	13,334	
% of sample	89	91		88	90	
Panel C: Leakage						
Reconciliation	-121***	-72***	-49***	-21	4.7***	-25
	(9.1)	(10)	(14)	(32)	(1.8)	(32)
Reconciliation * Month	51***	28***	23***	17*	5.5***	11
	(4.7)	(5.4)	(7.1)	(9)	(.93)	(9)
January 2017 mean	90	81		20	23	
Observations	25,469	13,447		25,349	13,334	
% of sample	89	91		88	90	

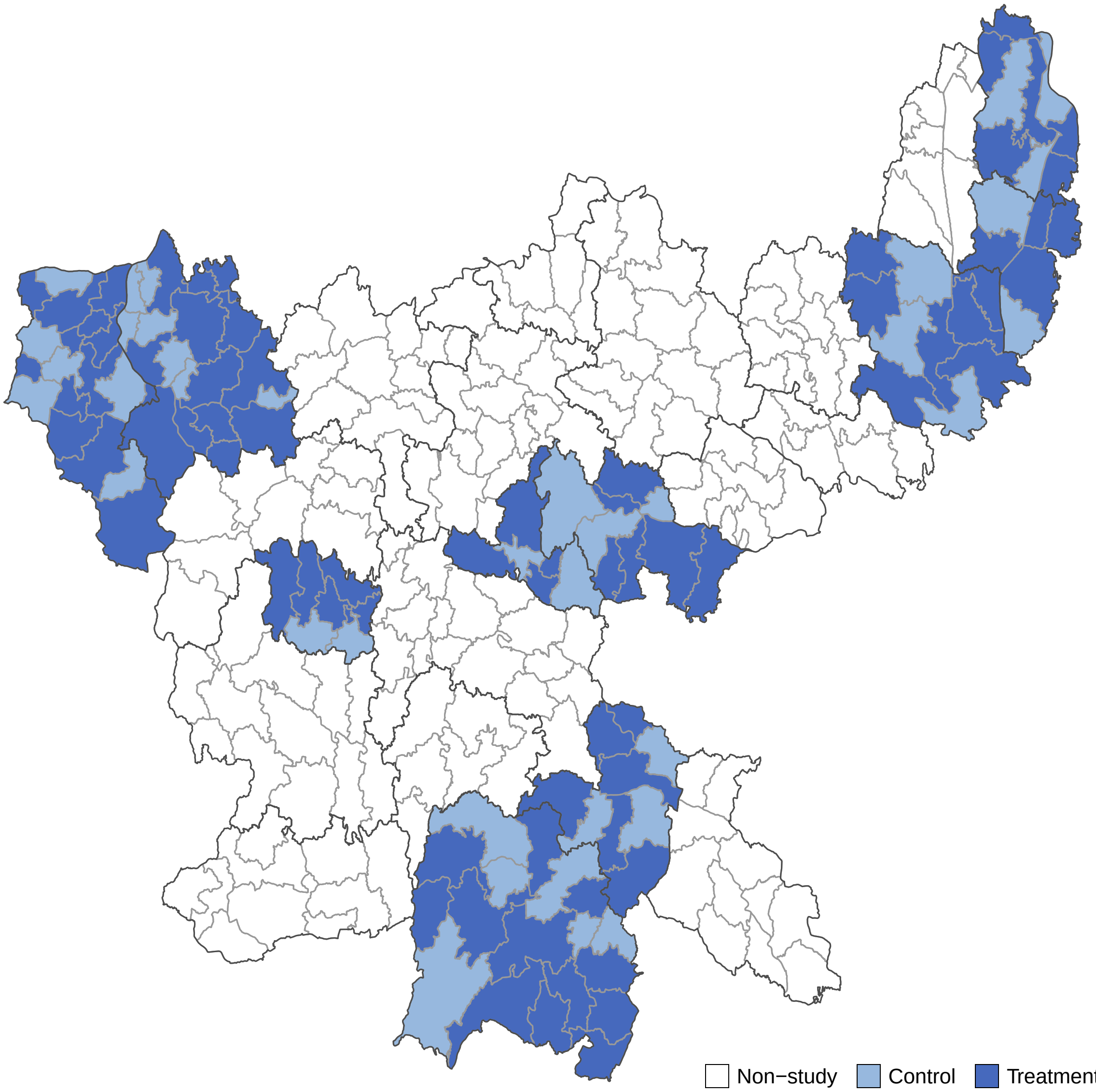
This table reports estimates of the effect of reconciliation on measures of the value disbursed by the government (Panel A), received by recipients (Panel B), and the difference (Panel C) separately for treatment and control areas using data from all three endlines. The unit of analysis is the FPS-month in Panel A and the ration card-month in Panels B and C, but all figures are per ration card-month. Observation counts vary by panel because we use the universe of FPSs to estimate effects on disbursements in Panel A, and a representative sample of ration card holders in Panels B and C, but all samples are representative. The dependent variable in columns 1 and 2 is the sum of values for rice and wheat, and the dependent variable in columns 4 and 5 is the sum of values for sugar, salt, and kerosene. Per-commodity values are defined in the notes to Table 3 above. Columns 3 and 6 test the difference between columns 1 ,2 and 4,5, respectively. Standard errors clustered at the FPS level are reported in parentheses, with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table 12: Decomposition of ration card deletions

	Deleted	Non-deleted	Total	%
	(1)	(2)	(3)	(4)
Admin data				
Unseeded	80,085	133,004	213,089	8.7%
Seeded	64,076	2,172,445	2,236,521	93.1%
Total	144,161	2,305,449	2,449,610	100%
% of overall total	5.9%	94.1%	100%	
Survey data				
Unseeded and ghost	15	6	21	.5%
Unseeded and not ghost	90	142	232	5.9%
Seeded and ghost	11	21	32	.8%
Seeded and not ghost	97	3519	3616	92.7%
Total	213	3688	3901	100%
% of overall total	5.5%	94.5%	100%	

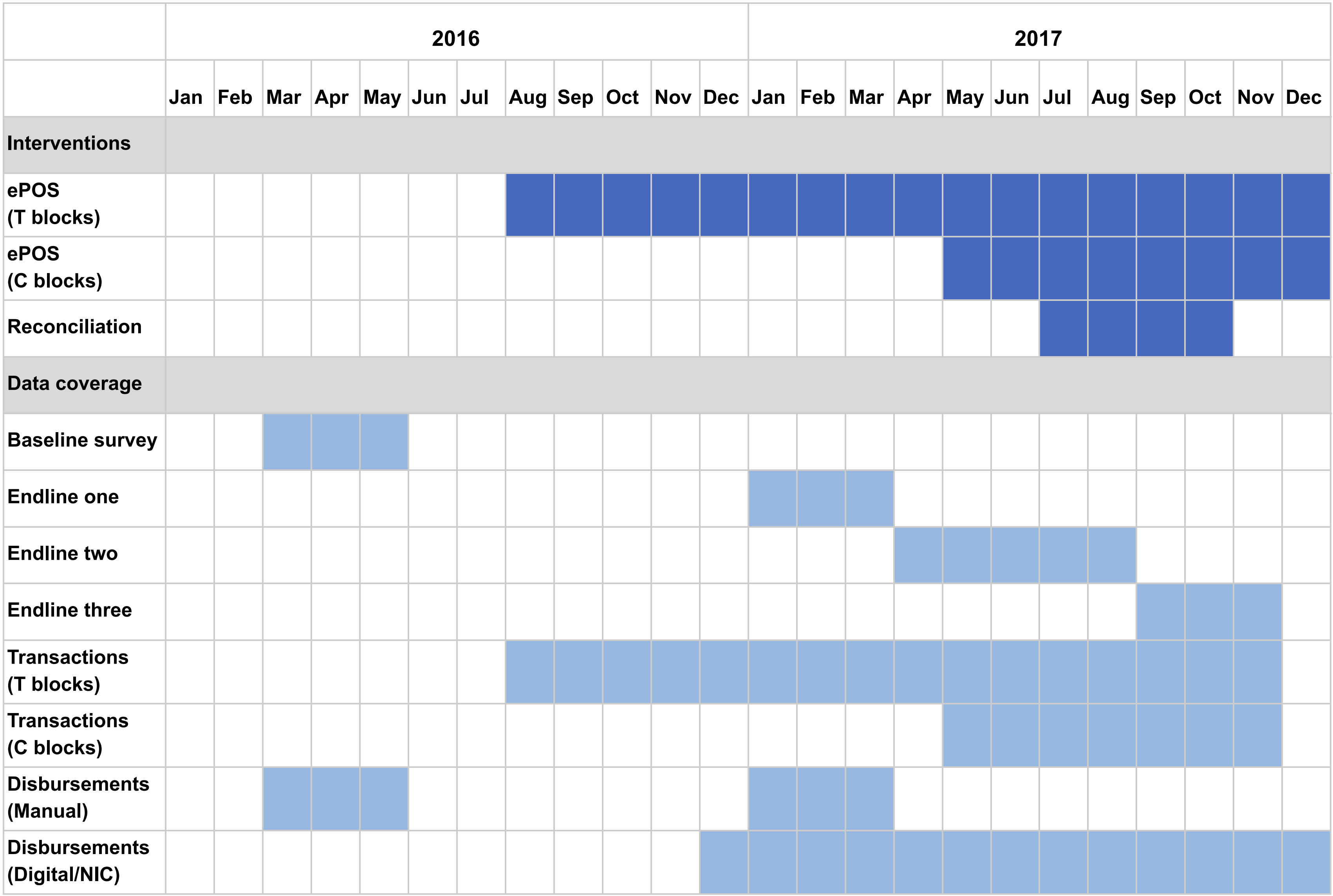
This table reports the decomposition of ration card deletions by Aadhaar seeding status. The top panel shows the results from the universe of ration cards in our 10 study districts, and the bottom panel shows results from our sampled ration cards, for which we show counts adjusted by sampling probability and categorized beneficiaries as based on survey results. A ration card is if it was present in the beneficiary list in October 2016 but absent in May 2018, and is if still present in May 2018. A ration card is if it did not have any Aadhaar number seeded to the card in October 2016, and if it did.

Figure 1: Blockwise treatment assignment



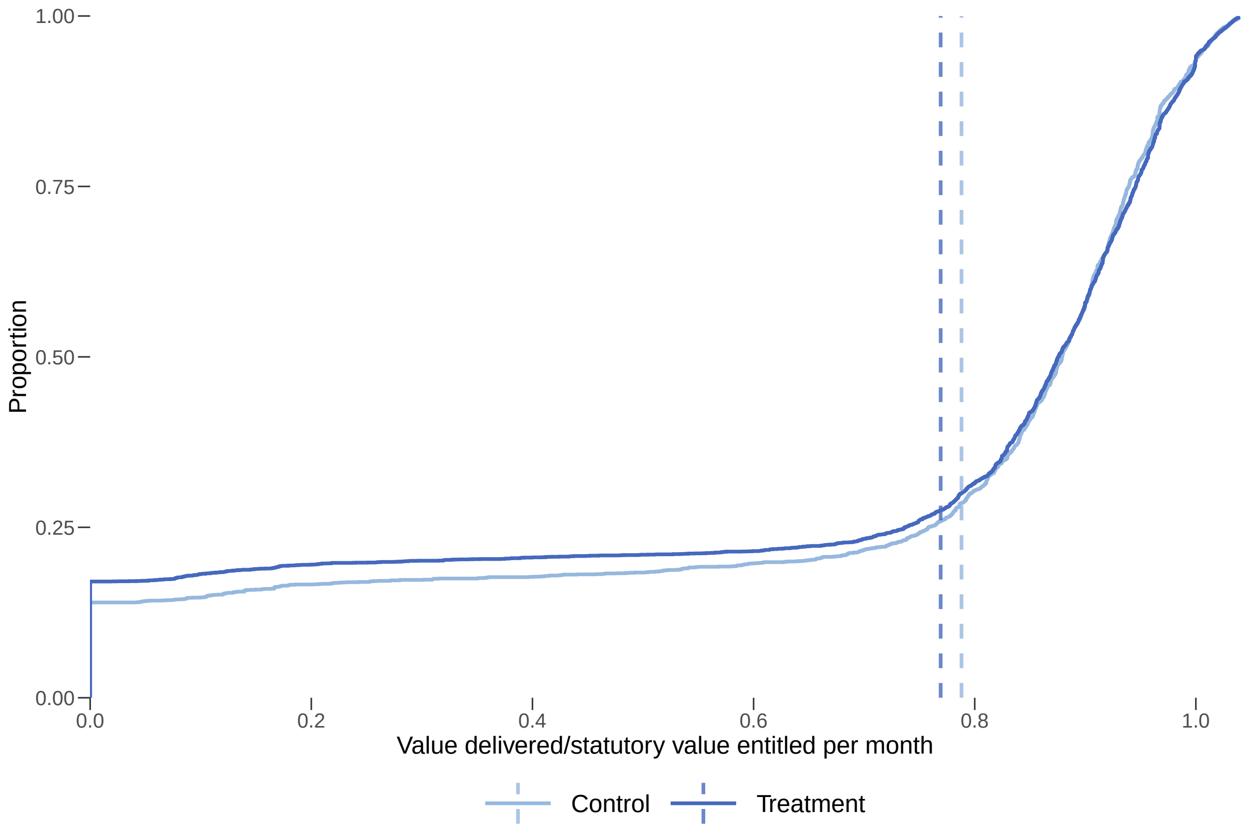
This figure shows the assignment of districts within Jharkhand to study (10) and non-study (14) status, and the assignment of blocks within these districts to treatment and control. Note that four of the census blocks depicted here are further sub-divided for the purposes of PDS administration into an urban and a rural "PDS block;" in these cases we give the entire census block the color corresponding to the treatment status of its larger, rural PDS block.

Figure 2: Intervention and data collection timeline



This figure plots the evolution of the interventions delivered by the Government of Jharkhand (top panel) and the coverage of the various data sources we use for analysis (bottom panel). Transaction data coverage in control areas in May 2017 is partial, as the rollout of ePOS devices in control areas began but did not finish in that month.

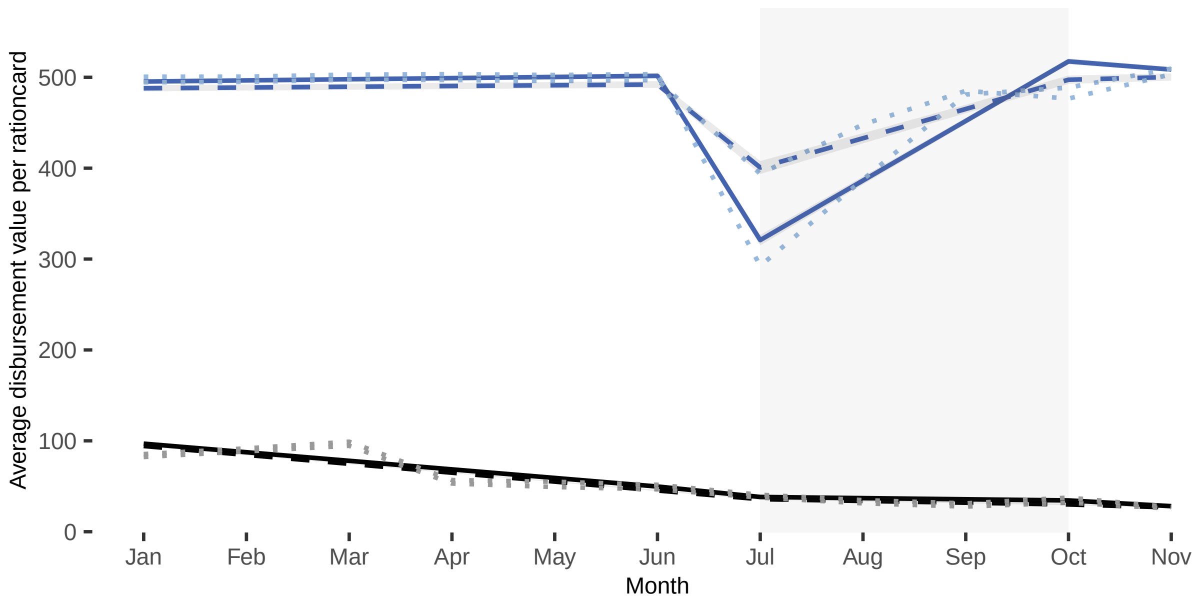
Figure 3: Value received as a proportion of entitlement



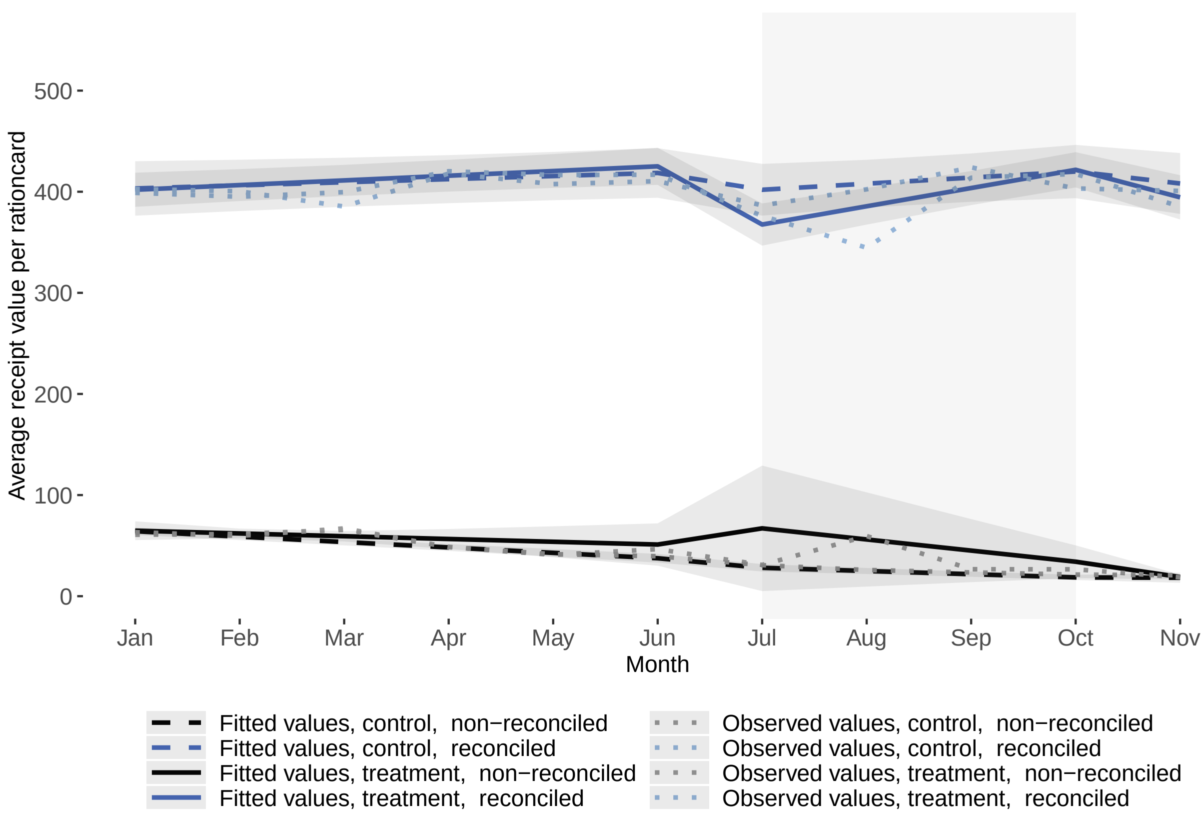
This figure plots the empirical cumulative distribution, separately for households in treatment and control blocks, of value received divided by value entitled per month, pooling the months of January, February, and March 2017. To improve legibility we right-censor the distributions at the 90th percentile.

Figure 4: Effects of reconciliation on value disbursed and received, by treatment

(a) Value disbursed

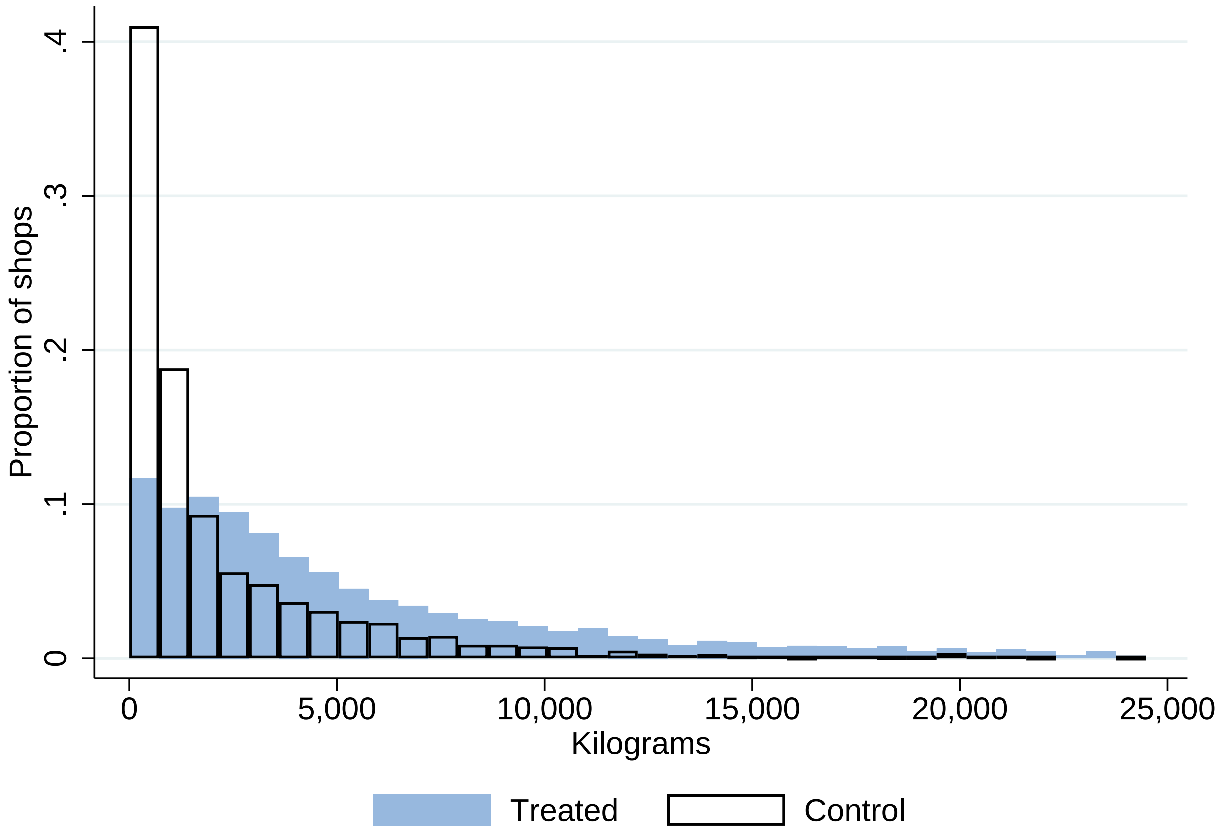


(b) Value received



This figure plots the evolution of the average value of commodities disbursed (Panel A) and received (Panel B) by treatment status from January to November of 2017. The unit is the ration card-month. Value disbursed and value received are as described in Figure A.3. Dotted lines represent the raw data, while solid lines and dashed lines represent fitted values obtained by estimating Equation2 for treatment and control, respectively. The shaded bands represent 95% confidence intervals for the fitted values. Values are shown separately for commodities that were (blue) and were not (black) separately subject to reconciliation. The shaded region from July to November indicates the period of reconciliation.

Figure 5: Recorded grain stock as of June 2017, by treatment status



This figure shows the distribution of grain (i.e. rice and wheat) in kilograms held by FPSs at the end of June 2017 according to government records, separately for shops in treated and control blocks. The unit of observation is the FPS. To increase legibility the distributions are right-censored at the 95th percentile.

A Supplemental exhibits

Table A.1: Comparison of dealer samples

	Original sample	Additional sample	Regression-adjusted difference	p-value
Treatment	.67	.77	-.081*	.07
Age	44	43	1.5	.33
Years of education	9.8	10	-.22	.74
Has an FPS dealer in family	.13	.27	-.12	.18
Years as FPS dealer	14	14	-.094	.95
Has other income sources	.79	.73	.066	.32
Runs FPS out of own home	.61	.73	-.16*	.06
Days open per month	19	20	-1.4*	.10
Hours open per day	6.7	6.7	-.073	.80
Days mandated to be open per month	23	24	-.58	.56
Hours selling PDS commodities per day	6.5	7.3	-.78	.50
Hours mandated to be open per day	6.9	7	.01	.97
Number of total ration cards	269	291	-20	.51
Number of PH ration cards	225	249	-21	.46
Number of AAY ration cards	44	42	.7	.89
Number of villages	2	2.5	-.38	.12

This table compares the PDS dealers originally sampled at baseline ("original sample") with those added at the first endline as a result of ration card re-assignment across ration shops ("additional sample"). Columns 1 and 2 report the means of each variable for the respective groups. Column 3 reports the coefficient from a regression of the given variable on an indicator for being in the original sample, controlling for strata fixed effects, and column 4 reports the p-value for a test that this coefficient is zero. Estimates are weighted by inverse sampling probabilities. Statistical significance is denoted as: $^{*}p < .10,^{**}p < .05,^{***}p < .01$

Table A.2: Outcome missingness by treatment status: all households

	Treatment	Control	difference	p-value
HH classified as ghost	.013	.025	-.0059	.15
Quantity rice purchased in January	.032	.039	-.0039	.66
Quantity rice purchased in February	.035	.037	-.00042	.96
Quantity rice purchased in March	.034	.041	-.0071	.41
Quantity wheat purchased in January	.026	.025	.0031	.69
Quantity wheat purchased in February	.028	.025	.0034	.67
Quantity wheat purchased in March	.025	.024	.0016	.82
Quantity sugar purchased in January	.021	.029	-.0078	.18
Quantity sugar purchased in February	.02	.028	-.0076	.19
Quantity sugar purchased in March	.024	.026	-.0017	.75
Quantity salt purchased in January	.016	.024	-.0073	.17
Quantity salt purchased in February	.015	.023	-.0081	.11
Quantity salt purchased in March	.017	.02	-.0033	.51
Quantity kerosene purchased in January	.025	.038	-.0084	.35
Quantity kerosene purchased in February	.025	.036	-.0081	.38
Quantity kerosene purchased in March	.026	.038	-.0081	.33
Value rice purchased in January	.032	.039	-.0039	.66
Value rice purchased in February	.035	.037	-.00042	.96
Value rice purchased in March	.034	.041	-.0071	.41
Value wheat purchased in January	.026	.025	.0031	.69
Value wheat purchased in February	.028	.025	.0034	.67
Value wheat purchased in March	.025	.024	.0016	.82
Value sugar purchased in January	.021	.029	-.0078	.18
Value sugar purchased in February	.02	.028	-.0076	.19
Value sugar purchased in March	.024	.026	-.0017	.75
Value salt purchased in January	.016	.024	-.0073	.17
Value salt purchased in February	.015	.023	-.0081	.11
Value salt purchased in March	.017	.02	-.0033	.51
Value kerosene purchased in January	.025	.038	-.0084	.35
Value kerosene purchased in February	.025	.036	-.0081	.38
Value kerosene purchased in March	.026	.038	-.0081	.33
Total value purchased in January	.049	.054	-.0019	.84
Total value purchased in February	.045	.054	-.0069	.47
Total value purchased in March	.049	.054	-.0047	.62

This table reports the rate at which various household outcomes measured in endline one (January - March) are not observed, by treatment status. We include all surveyed households and all households categorized as "ghosts." Columns 1 and 2 report the mean of each outcome among treatment and control households, respectively. Column 3 reports the simple differnece between these, and Column 4 reports the p-value on a test of the null that this difference is equal to zero. Estimates are weighted by inverse sampling probabilities.

Table A.3: Outcome missingness by treatment status: surveyed households

	Treatment	Control	difference	p-value
Sampled rationcard is inactive	.019	.013	.0089	.097*
Household does not know of sampled rationcard	.054	.073	-.015	.18
Willingness to accept in January	.12	.12	.0036	.78
Willingness to accept in February	.14	.12	.025	.11
Willingness to accept in March	.14	.12	.022	.2
Rice was low quality	.071	.06	.021	.043**
Wheat was low quality	.77	.66	.08	.15
Sugar was low quality	.36	.3	.038	.22
Salt was low quality	.33	.34	.0021	.95
Kerosene was low quality	.1	.094	.023	.097*
Rice was adulterated	.077	.067	.019	.095*
Wheat was adulterated	.77	.66	.081	.15
Sugar was adulterated	.36	.3	.039	.21
Salt was adulterated	.34	.35	.00093	.98
Kerosene was adulterated	.1	.11	.016	.27
Access cost in January	.11	.13	-.015	.27
Access cost in February	.11	.13	-.015	.31
Access cost in March	.0085	.016	-.0057	.13
Total access cost in March	.0085	.016	-.0057	.13
Overcharge on rice in January	.045	.062	-.0099	.37
Overcharge on rice in February	.047	.063	-.01	.35
Overcharge on rice in March	.045	.069	-.02	.074*
Overcharge on wheat in January	.035	.045	-.0041	.69
Overcharge on wheat in February	.037	.05	-.0071	.51
Overcharge on wheat in March	.034	.048	-.0081	.41
Overcharge on sugar in January	.041	.052	-.0074	.34
Overcharge on sugar in February	.04	.056	-.017	.099*
Overcharge on sugar in March	.042	.045	-.0024	.74
Overcharge on salt in January	.032	.037	-.0037	.58
Overcharge on salt in February	.028	.036	-.0092	.2
Overcharge on salt in March	.031	.036	-.0053	.35
Overcharge on kerosene in January	.044	.058	-.0085	.48

Overcharge on kerosene in February	.044	.058	-.0092	.47
Overcharge on kerosene in March	.051	.059	-.0047	.7
Total overcharge in January	.094	.11	-.004	.79
Total overcharge in February	.089	.12	-.024	.12
Total overcharge in March	.097	.12	-.021	.17
FPS-level market price of rice in March	.027	.039	-.011	.62
FPS-level market price of wheat in March	.38	.47	-.098	.064*
FPS-level market price of sugar in March	.027	.045	-.019	.38
FPS-level market price of salt in March	.0051	0	.0056	.18
FPS-level market price of kerosene in March	.36	.42	-.078	.16
HH-level market price of rice in March	.53	.53	-.025	.22
HH-level market price of wheat in March	.87	.87	-.021	.16
HH-level market price of sugar in March	.45	.49	-.037	.2
HH-level market price of salt in March	.3	.3	-.005	.81
HH-level market price of kerosene in March	.87	.89	-.018	.19

This table reports the rate at which various household outcomes measured in endline one (January - March) are not observed, by treatment status. We include only surveyed households. Columns 1 and 2 report the mean of each outcome among treatment and control households, respectively. Column 3 reports the simple differnece between these, and Column 4 reports the *p*-value on a test of the null that this difference is equal to zero. Estimates are weighted by inverse sampling probabilities.

Table A.4: Effects on quantity disbursed, quantity received, and leakage

	Rice	Wheat	Sugar	Salt	Kerosene
	(1)	(2)	(3)	(4)	(5)
Mean entitlement	24	1	1	1	2
Panel A: Quantity disbursed					
Treatment	1.457**	-1.072*	0.007	0.003	0.000
	(0.568)	(0.559)	(0.008)	(0.005)	(0.000)
	[0.13]	[0.33]	[1.00]	[1.00]	[0.38]
Control mean	20	3.4	1.3	1	2.4
Observations	26,611	26,611	26,611	26,611	26,611
Panel B: Quantity received					
Treatment	.76	-.58	.026	.056	-.034
	(.5)	(.48)	(.08)	(.065)	(.048)
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Control mean	17	2.6	.72	.81	1.8
Observations	10,557	10,654	10,670	10,726	10,618
Panel C: Leakage					
Treatment	.68	-.5	-.019	-.053	.034
	(.57)	(.32)	(.081)	(.063)	(.047)
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Control mean	3.4	.86	.61	.23	.68
Observations	10,557	10,654	10,670	10,726	10,618

This table reports estimated treatment effects on the quantity of commodities disbursed by the government (Panel A), received by recipients (Panel B), and the difference (Panel C) in endline one (January - March). The unit of analysis is the FPS-month in Panel A and the ration card-month in Panels B and C. Observation counts vary by panel because we use the universe of FPSs to estimate effects on disbursements in Panel A, and a representative sample of ration card holders in Panels B and C, but all samples are representative. In Panel C, estimated effects are the difference between estimated effects on quantity disbursed per ration card and quantity received per ration card with block-level mean imputation in a Seemingly Unrelated Regression framework. All specifications include strata fixed effects and the baseline value of the dependent variable. Standard errors clustered at the block level are reported in parentheses with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$. q values adjusted to control the false discovery rate across the five commodities are reported in brackets.

Table A.5: Effects on market prices and overcharges

	Total	Rice	Wheat	Sugar	Salt	Kerosene
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Local market prices—reported by households						
Treatment		.043	5.6	-1.1***	.17	1.4
		(.29)	(5.3)	(.39)	(.16)	(1.8)
		[1.00]	[1.00]	[0.09]	[1.00]	[1.00]
Control mean	.	23	22	43	10	43
Observations	.	383	248	382	392	229
Panel B: Local market prices—reported by dealers (Rs)						
Treatment		-1.3***	.021	-1.3*	-.31	-1.7*
		(.46)	(.92)	(.69)	(.54)	(.94)
		[0.06]	[1.00]	[0.25]	[1.00]	[0.25]
Control mean	.	19	17	38	8.5	38
Observations	.	344	109	283	282	251
Panel C: Statutory prices						
Treatment		–	–	–	–	.027
						(.03)
Control mean		–	–	–	–	18
Observations	.	.	.	.	.	396
Panel D: Overcharges						
Treatment	-2.6	.069	-.13**	-2.1	.016	-.66
	(1.9)	(.24)	(.056)	(1.7)	(.035)	(.51)
		[1.00]	[0.21]	[0.84]	[1.00]	[0.84]
Control mean	8.2	1.1	.22	.91	.17	6
Observations	9,623	10,183	10,317	10,260	10,375	10,185

This table reports estimated treatment effects on the market prices reported by beneficiaries (Panel A), market prices reported by FPS dealers (Panel B), statutory prices (Panel C), and total overcharges (Panel D) in endline one (January - March). The unit of analysis is the FPS for Panels A and B, the block-month for Panel C, and the ration card-month for Panel D. Prices are in rupees per kilogram except for kerosene, which is priced in rupees per liter. Observation counts vary in panels A and B as we observe outcomes only when at least one household purchased the commodity and when the dealer reported the commodity is sold in the private market, respectively. In Panels A-C the dependent variables are the median market price reported by beneficiaries assigned at baseline to the given FPS, the local market price reported by FPS dealers (Panel B), and the statutory PDS price, respectively. We do not report effects on statutory prices for goods other than kerosene as these did not vary. In Panel D the dependent variable is the amount beneficiaries report paying above what they should have paid for the quantity they received, by commodity in columns 2-6 and in total in column 1. All regressions include strata fixed effects; those in Panels C and D also include the baseline value of the dependent variable. Standard errors clustered at the block level are reported in parentheses with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$. q values adjusted to control the false discovery rate across the five commodities are reported in brackets.

Table A.6: Effects on determinants of beneficiary transaction costs

	Total Cost		Opportunity cost	Unsuccessful trip count	Unsuccessful trip length	Successful trip count	Successful trip length	Transport cost
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	7.2*	6.9*	.87*	.13***	.14	.016	.15	-.81
	(3.8)	(3.8)	(.5)	(.024)	(.32)	(.054)	(.092)	(.52)
Baseline lag		1.6**						
		(.79)						
Adjusted R^2	.06	.09	.08	.10	.01	.06	.06	.00
Control mean	41	41	11	.13	1.2	1.5	2.3	1.6
Observations	3,538	3,538	3,066	3,565	449	3,565	3,062	3,538
% of sample	90	90	78	91	11	91	77	90

This table reports estimated treatment effects on the costs incurred by beneficiaries to access PDS rations in March 2017. The unit of analysis is the ration card. The dependent variable in columns 1-2 is the total estimated cost as reported in Table 5, and the remaining columns show impacts on its components. The dependent variable in column 3 is the weighted mean opportunity cost in rupees per hour of household members, weighted by the number of trips each household member made to their FPS in March. The dependent variables in columns 4 and 5 are the number of unsuccessful trips made to the ration shop (defined as trips that did not result in the purchase of positive quantities of any rationed commodity) and the average time in hours spent on these trips. Note that our survey asked about both the number of unsuccessful trips made by each individual on the household roster and for the total number of unsuccessful trips taken. When the latter exceeds the sum of the former we attribute the stated total number of trips to household members in proportion to their stated individual number of trips; the results are not sensitive to alternatives.; tThe dependent variables in columns 6 and 7 are the analogous quantities for successful trips. Finally, the dependent variable in column 8 is the average monetary cost in rupees of any transport fees paid to make these trips (e.g. bus fare). Thus, the total cost in column1 equals to unit opportunity cost * (total time spent on unsuccessful trips + total time spent on successful trips) + transporation cost * (number of unsuccessful trips + number of successful trips) = column 3 * (column 4 * column 5 + column 6 * column 7) + column 8 * (column 4 + column 6). All regressions include strata fixed effects. Standard errors clustered at the block level are reported in parentheses, with statistical significance denoted as: $^{*}p < .10, ^{**} p < .05, ^{***} p < .01$.

Table A.7: Heterogeneous effects by household characteristics

	HH is upper caste?			HH above median education level?			HH above median annual income?		
	(1) No *(N=1875)*	(2) Yes *(N=1705)*	(3) Δ	(4) No *(N=2083)*	(5) Yes *(N=1500)*	(6) Δ	(7) No *(N=1634)*	(8) Yes *(N=1608)*	(9) Δ
Value received (market prices)	-1.4	-5	-3.6	-21	23*	44***	-15	11	26*
	(15)	(12)	(13)	(13)	(14)	(13)	(14)	(15)	(14)
Value received (WTA)	55**	-14	-69***	38	-.027	-38	26	30	4.1
	(26)	(24)	(25)	(25)	(27)	(29)	(26)	(27)	(27)
Transaction costs	4.5	9.6**	5.1	7.9**	5.4	-2.5	2	14***	12***
	(4.4)	(4.7)	(5)	(3.9)	(4.5)	(3.7)	(3.9)	(4.6)	(3.8)

This table reports differential estimated treatment effects along dimensions of household characteristics for ration card-level outcomes in endline one (January - March). Each row represents a different primary outcome, and each column grouping represents a different dimension of heterogeneity. Within each column group, the first column reports the average treatment effect on households that do not satisfy the stated condition, the second column reports the average effect on those that do, and the third column reports the difference in these two effects, all estimated from a single underlying regression that interacts treatment with an indicator for the stated condition. The indicator "HH is upper caste" is equal to one if the household does not belong to Scheduled Caste or Scheduled Tribe. The indicator "HH above median education level" is equal to one if the average number of years of schooling of the two highest-educated household members is above the sample median. The indicator "HH above median annual income" is equal to one if the household's annual income (as predicted from assets and household characteristics at baseline) is above the sample median. All regressions include strata fixed effects and baseline value of the outcome variable when available. Standard errors clustered at the block level are reported in parentheses, with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table A.8: Heterogeneous effects by location characteristics

	FPS in urban area?			Network strength above median?		
	(1)	(2)	(3)	(4)	(5)	(6)
	No *(N=3129)*	Yes *(N=513)*	Δ	No *(N=1565)*	Yes *(N=1444)*	Δ
Value received (market prices)	-.74	-5.9	-5.2	-3.1	-4.2	-1.1
	(13)	(19)	(23)	(13)	(16)	(18)
Value received (WTA)	30	-79	-109**	9.2	4.4	-4.8
	(23)	(48)	(53)	(29)	(29)	(37)
Transaction costs	7.1*	4.3	-2.8	5.9	1.1	-4.8
	(3.9)	(14)	(14)	(5)	(5.7)	(6.5)

This table reports differential estimated treatment effects along dimensions of location characteristics for ration card-level outcomes in endline one (January - March). Each row represents a different primary outcome, and each column grouping represents a different dimension of heterogeneity. Within each column group, the first column reports the average treatment effect on households that do not satisfy the stated condition, the second column reports the average effect on those that do, and the third column reports the difference in these two effects, all estimated from a single underlying regression that interacts treatment with an indicator for the stated condition. The indicator "FPS in urban area?" is equal to one if the household's FPS belongs to block that is administratively classified as urban. The indicator "Network strength above median?" is equal to one if the measured signal strength of the Airtel network (which was the most common SIM card type installed in ePOS machines) at the household's assigned FPS is above the sample median. All regressions include strata fixed effects and baseline value of the outcome variable when available. Standard errors clustered at the block level are reported in parentheses, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table A.9: Heterogeneous effect by machine mode

	Value received (mkt prices)	Value received (WTA)	Transaction costs
	(1)	(2)	(3)
Treatment*Online	1.8	1	3.1
	(15)	(30)	(5.2)
Treatment*Offline	-1.6	-57	9.2
	(22)	(52)	(9.5)
Treatment*Partial	-37	-52	19*
	(29)	(104)	(10)
Adjusted R^2	.20	.06	.08
Control mean	463	1028	41
Observations	9733	9787	3337
% of sample	82	84	86

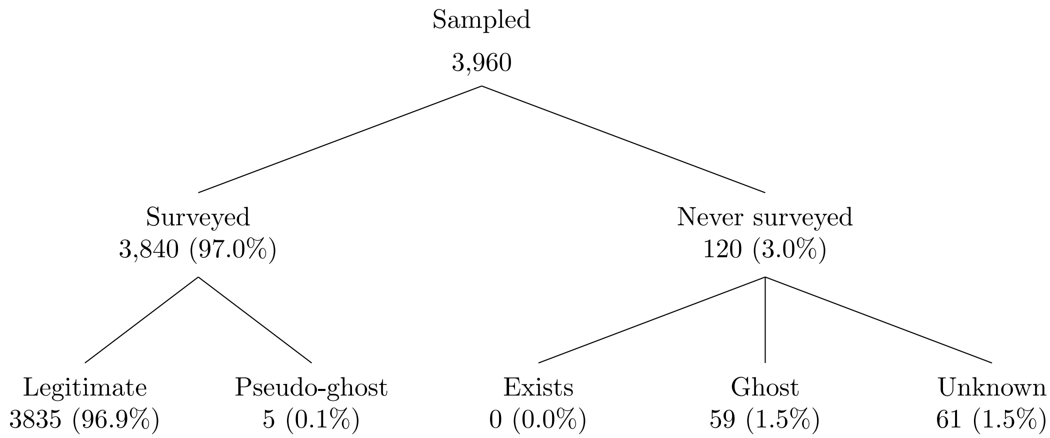
This table reports differential estimated treatment effects by machine mode assigned to dealers per month for ration card-level outcomes in endline one (January - March). We assign counterfactual machine modes to control FPS's by assuming they could have received machines operating in the same mode as it was ultimately assigned once treated. This assumption appears reasonable in the sense that the distribution of machine types 6-8 months after the reform was implemented appear similar in both treatment and control areas, with the one exception that the government ended the use of partially online mode in August 2017 and so we impute fewer partially online machines in control. We define the mode in which a machine operated from transaction data as the modal transaction type conducted by that machine and during that month. On average the modal transaction type accounts for 99% of the transactions in a given machine $\times$ month cell. The unit of analysis for columns 1 and 2 is the ration card-month and for column 3 the ration card. The dependent variable in column 1 is the sum of the values for each commodity, defined as the quantity multiplied by the difference between the median market price of that commodity in control blocks in the same district, and the statutory PDS price for that commodity. The dependent variable in column 2 is the household reported willingness to accept (WTA), constructed as the smallest value X for which the respondent reported that they would have preferred in cash to the commodities received. The WTA for ration cards that did not receive any ration is set to zero. The dependent variable in column 3 is the total cost incurred in March by the household holding that ration card in purchasing or attempting to purchase PDS commodities, including time and money costs. All regressions include strata fixed effects, and columns 1 and 3 include the baseline value of the outcome variable. Standard errors clustered at the block level are reported in parentheses, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table A.10: Heterogeneous effects of reconciliation on value disbursed and received by FPS balance

	Value disbursed			Value received		
	(1) Total	(2) Rice	(3) Wheat	(4) Total	(5) Rice	(6) Wheat
Reconciliation	-59***	-35***	-19***	7.3	3.7	-2.1
	(3.9)	(3.3)	(2.6)	(13)	(11)	(18)
Reconciliation*Balance	-.21***	-.24***	.03	-.1***	-.1***	-.014
	(.0089)	(.0092)	(.056)	(.028)	(.026)	(.61)
Reconciliation*Month	18***	14***	3.8***	-4	-3.2	3
	(2.1)	(1.9)	(1)	(8.6)	(6.6)	(16)
Reconciliation*Month*Balance	.078***	.083***	.022	.027	.027*	-.072
	(.0045)	(.0045)	(.021)	(.017)	(.015)	(.6)
January 2017 mean	493	439	54	407	363	44
Observations	92489	95581	93258	35518	36269	36046
% of frame/sample	91	94	92	82	83	83

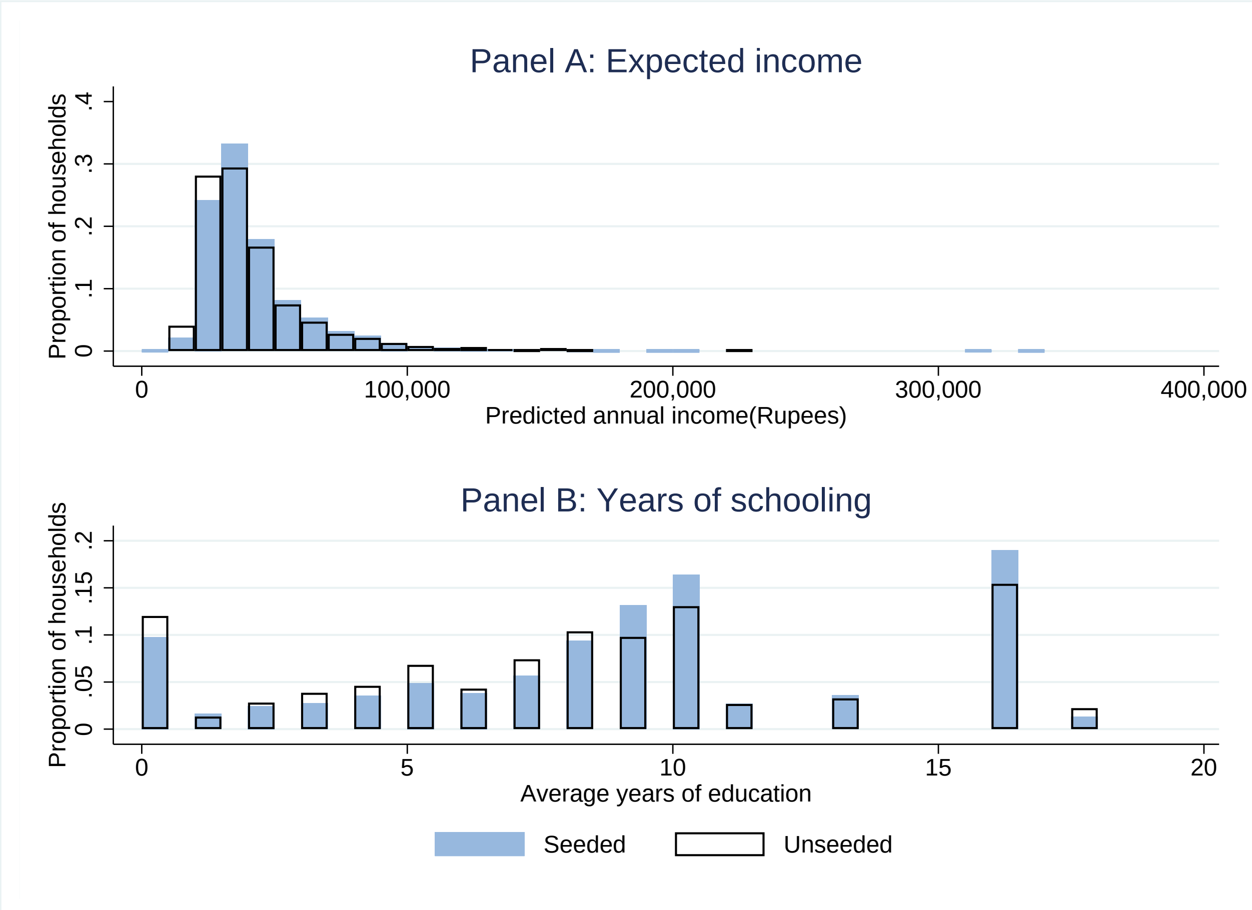
This table reports differential effects of accumulated stock balance at the time of reconciliation onset (July 2017) on value disbursed by dealers (columns 1 - 3) and received by beneficiaries (columns 4 -6). The unit of analysis is the FPS-month for columns 1-3 and ration card - month for columns 4-6. Observation counts vary because we use the universe of FPSs to estimate effects on disbursements in columns 1-3, and a representative sample of ration card holders in columns 4-6, but both samples are representative. The dependent variable in columns 2, 3, 5, and 6 is the per-commodity value disbursed and received as defined in the notes to Table 3 above. The dependent variable in column 1 is the sum of the values from columns 2 and 3, and the dependent variable in column 4 is the sum of values in columns 5 and 6. We calculate average balance per ration card as the balance per FPS at the beginning of July 2017, provided by NIC, divided by ration card counts per FPS, and we instrument for this FPS-level average balance per ration card using the block's initial ePOS treatment assignment. Standard errors clustered at the block level are reported in parentheses, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$. All regressions include strata fixed effects and their interactions with stock balance.

Figure A.1: Household classification results



This figure classifies the households listed in the ration cards we originally sampled and attempted to survey. "Surveyed" households are those we were able to locate and survey at least once across baseline and the three endline surveys, as opposed to "never surveyed" households. Among the former "legitimate" are those that knew of the ration card based on which we sampled them, and "quasi-ghosts" are those that were unaware of the existence of this ration card across all surveys. Among the latter, households "exist" if we were able to locate the household but not survey it; are a "ghost" if we could not locate it after an exhaustive search and confirmed with multiple neighbors that it did not exist; and as "unknown" otherwise.

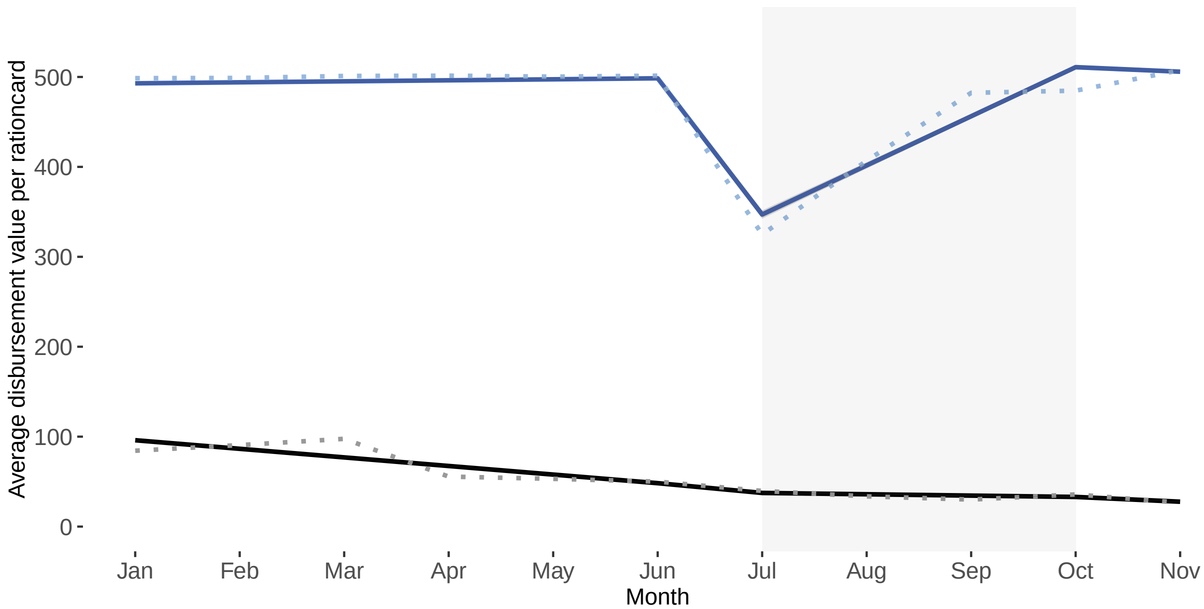
Figure A.2: Household expected income and years of schooling by seeding status



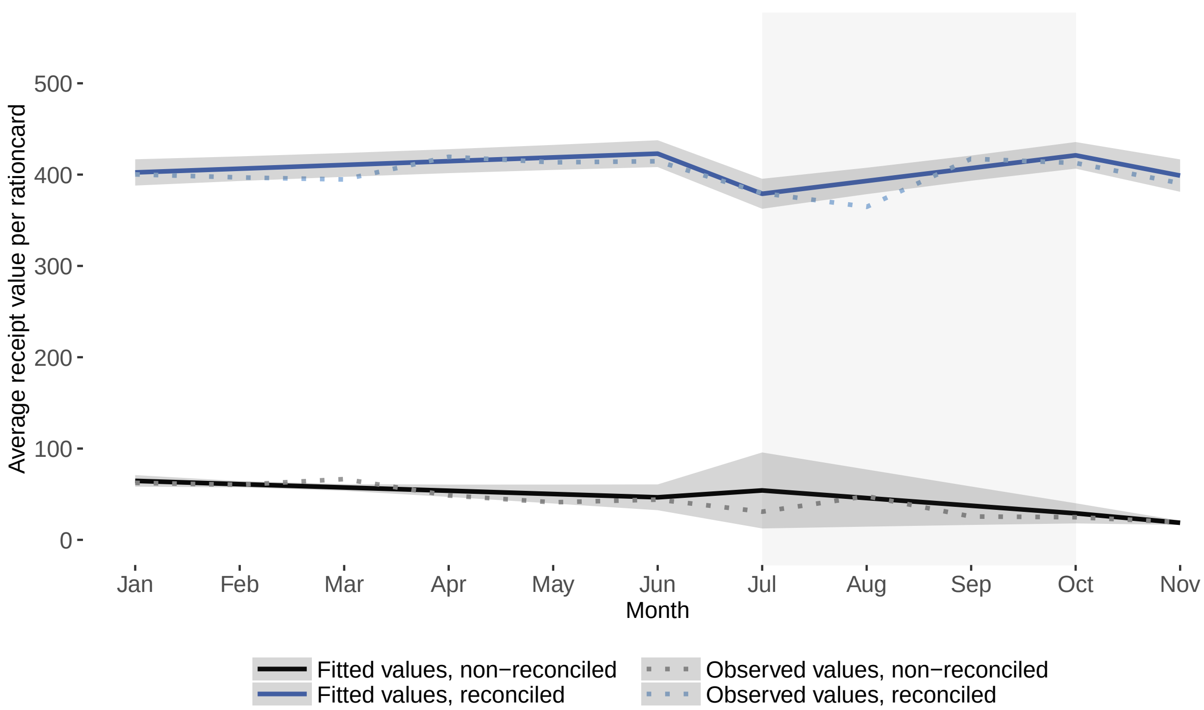
This figure shows the distribution of measures of household income (Panel A) and education (Panel B) by whether the ration card that household was attached to had at least one Aadhaar number seeded at baseline. In Panel A, the outcome is the linear prediction of annual income based on assets and household characteristics at baseline. In Panel B, the outcome is the average years in education of the two most educated members in the household.

Figure A.3: Effects of reconciliation on value disbursed and received

(a) Value disbursed



(b) Value received



This figures plots the evolution of the average value of commodities disbursed (Panel A) and received (Panel B) from January to November of 2017. The unit is the ration card-month. Value disbursed is calculated from administration data and value received from our series of endline surveys, using market price data as described in the notes to Table 3. Dashed lines represent the raw data, while solid lines represent fitted values obtained by estimating Equation 2. The shaded bands around the latter represent 95% confidence intervals for the fitted values. Values are shown separately for commodities that were (blue) and were not (black) separately subject to reconciliation. The shaded region from July to November indicates the period during which reconciliation was in effect.

B Temporal heterogeneity

Table B.1: Effects on value received using alternative specifications

	Total	Rice	Wheat	Sugar	Salt	Kerosene
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: March only						
Treatment	9.7	31***	-19*	.049	1.1	.94
	(13)	(11)	(10)	(2.1)	(1.2)	(1.8)
		[0.09]	[0.45]	[1.00]	[1.00]	[1.00]
Control mean	456.71	337.81	55.57	15.80	8.56	37.83
Observations	3,460	3,517	3,553	3,551	3,575	3,533
Panel B: Pooled data with linear trend						
Treatment	-12	-1.1	-9	3	-.88	-1.9
	(18)	(16)	(12)	(5.5)	(1.8)	(1.8)
		[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Month	-5.7	-8.3*	1.5	1.9	1.2*	-1.9***
	(5)	(4.5)	(1.6)	(1.4)	(.68)	(.47)
Treatment X Month	5.2	9	-3*	-1.2	.69	.66
	(6.7)	(5.6)	(1.8)	(2.3)	(.89)	(.86)
Control mean	463.30	348.18	53.73	13.80	7.25	39.64
Observations	10,396	10,557	10,654	10,670	10,726	10,618
Panel C: Pooled data with no baseline lag						
Treatment	.85	14	-13	.38	.62	-.33
	(14)	(12)	(11)	(1.5)	(.58)	(1.1)
		[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Control mean	463.30	348.18	53.73	13.80	7.25	39.64
Observations	10,396	10,557	10,654	10,670	10,726	10,618

This table reports alternative specifications for Panel B in Table 3 by reporting models with March data only (Panel A), pooled data with a linear trend (Panel B), and pooled data without baseline lag (Panel C). All regressions include strata fixed effects, and regressions in panels A and B include the baseline value of outcome variable. Standard errors clustered at the block level are reported in parentheses, with statistical significance is denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$. q values adjusted to control the false discovery rate across the five commodities are reported in brackets.

Table B.2: Effects on quantities received using alternative specifications

	Rice	Wheat	Sugar	Salt	Kerosene
	(1)	(2)	(3)	(4)	(5)
Panel A: March only					
Treatment	1.5***	-.75	.0084	.13	.034
	(.55)	(.47)	(.11)	(.13)	(.083)
	[0.10]	[0.64]	[1.00]	[1.00]	[1.00]
Control mean	16.28	2.68	0.82	0.95	1.71
Observations	3,517	3,553	3,551	3,575	3,533
Panel B: Pooled data with linear trend					
Treatment	-.15	-.29	.14	-.097	-.1
	(.77)	(.53)	(.27)	(.2)	(.08)
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Month	-.42*	.071	.1	.14*	-.04*
	(.22)	(.08)	(.071)	(.075)	(.021)
Treatment X Month	.45	-.14	-.059	.077	.035
	(.28)	(.089)	(.12)	(.099)	(.039)
Control mean	16.81	2.59	0.72	0.81	1.76
Observations	10,557	10,654	10,670	10,726	10,618
Panel C: Pooled data with no baseline lag					
Treatment	.53	-.5	.018	.069	-.019
	(.6)	(.52)	(.076)	(.064)	(.051)
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Control mean	16.81	2.59	0.72	0.81	1.76
Observations	10,557	10,654	10,670	10,726	10,618

This table reports alternative specifications for Panel B of Table A.4 by reporting models with March data only (Panel A), pooled data with a linear trend (Panel B), and pooled data without baseline lag (Panel C). All regressions include strata fixed effects, and regressions in panels A and B include the baseline value of outcome variable. Standard errors clustered at the block level are reported in parentheses, with statistical significance is denoted as: $^{*}p < .10,^{**}p < .05,^{***}p < .01$. q values adjusted to control the false discovery rate across the five commodities are reported in brackets.

Table B.3: Effects on overcharges using alternative specifications

	Total	Rice	Wheat	Sugar	Salt	Kerosene
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: March only						
Treatment	.7	.033	-.11*	.88***	-.12	-1.4
	(.61)	(.25)	(.063)	(.31)	(.078)	(1.4)
		[1.00]	[0.53]	[0.06]	[0.53]	[0.91]
Control mean	7.74	1.12	0.22	0.94	0.32	5.18
Observations	3,184	3,391	3,438	3,418	3,447	3,377
Panel B: Pooled data with linear trend						
Treatment	-7.3	.14	-.15***	-6.6	.25**	.11
	(4.8)	(.28)	(.056)	(4.5)	(.11)	(.95)
		[1.00]	[0.11]	[0.55]	[0.18]	[1.00]
Month	-.63**	.011	.0071	.16	.13**	-1***
	(.27)	(.032)	(.02)	(.16)	(.055)	(.19)
Treatment X Month	2.4	-.038	.0067	2.3	-.12*	-.38
	(1.7)	(.062)	(.022)	(1.6)	(.062)	(.59)
Control mean	8.22	1.08	0.22	0.91	0.17	5.95
Observations	9,623	10,183	10,317	10,260	10,375	10,185
Panel C: Pooled data with no baseline lag						
Treatment	-2.2	.11	-.13**	-1.8	.019	-.59
	(1.8)	(.29)	(.056)	(1.5)	(.035)	(.49)
		[1.00]	[0.23]	[0.92]	[1.00]	[0.92]
Control mean	8.22	1.08	0.22	0.91	0.17	5.95
Observations	9,623	10,183	10,317	10,260	10,375	10,185

This table reports alternative specifications for Panel D of Table A.5 by reporting models with March data only (Panel A), pooled data with a linear trend (Panel B), and pooled data without baseline lag (Panel C). All regressions include strata fixed effects, and regressions in panels A and B include the baseline value of outcome variable. Standard errors clustered at the block level are reported in parentheses, with statistical significance is denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$. q values adjusted to control the false discovery rate across the five commodities are reported in brackets.

Table B.4: Effects on willingness to accept using alternative specifications

	January	February	March	Pooled	
	(1)	(2)	(3)	(4)	(5)
Panel A: All households					
Treatment	-7.6	-23	-1.8	-11	-19
	(27)	(28)	(29)	(26)	(29)
Month					-22***
					(5.9)
Treatment X Month					4
					(7.2)
Control mean	1,045	1,041	1,000	1,028	1,028
Observations	3,395	3,522	3,520	10,437	10,437
Panel B: Excludes HHs who did not purchase ration in a given month					
Treatment	23	11	32	22	9.6
	(21)	(24)	(22)	(21)	(26)
Month					-26***
					(7.5)
Treatment X Month					6.3
					(8.9)
Control mean	1,163	1,157	1,111	1,143	1,143
Observations	3,165	3,122	3,102	9,389	9,389

This table reports a robustness check to Panel B of Table 3 by measuring value as the amount a household is willingness to accept in lieu of ration. We report estimates from both the sample that includes (Panel A) and excludes (Panel B) households that did not purchase any ration in a given month. The unit of analysis is the ration card - month. The dependent variable is the household reported willingness to accept (WTA), constructed as the smallest value X for which the respondent reported that they would have preferred in cash to the commodities received. The WTA for ration cards that did not receive any ration is set to zero. All regressions include strata fixed effects. Standard errors clustered at the block level are reported in parentheses, with statistical significance is denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

C Reconciliation protocol and implementation

As provided to us, the protocol governing the disbursements of reconciled commodities (i.e. wheat and rice) which the government of Jharkhand introduced was given by

$$
\begin{aligned}
D_t &= max(0, E_t + C_t - S_{t-1}) && (1)\\
S_t &= S_{t-1} + \hat{D}_t - O_t && (2)\\
C_{i,t} &= \begin{cases} E_{i,t-1} - (O_{i,t-1} - C_{i,t-1}) & O_{i,t-1} > C_{i,t-1} \\ E_{i,t-1} & O_{i,t-1} \leq C_{i,t-1} \end{cases} && (3)
\end{aligned}
$$

Equation 1 defines the amount D_t to disburse at the beginning of period t as a function of the amount $E_t = \sum_i E_{i,t}$ to which recipients assigned to the FPS in period t were entitled, the amount $C_t = \sum_i C_{i,t}$ is the total carryover commitment owed to recipients in period t because they did not collect their entitlement in period $t-1$, and the amount of stock S_{t-1} the government believes the FPS should have been holding at the end of the preceding month. Equation 2 defines the law of motion for stock, which increases with disbursements D_t and decreases with offtake $O_t = \sum_i O_{i,t}$ by beneficiaries. 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.

C.1 Adherence

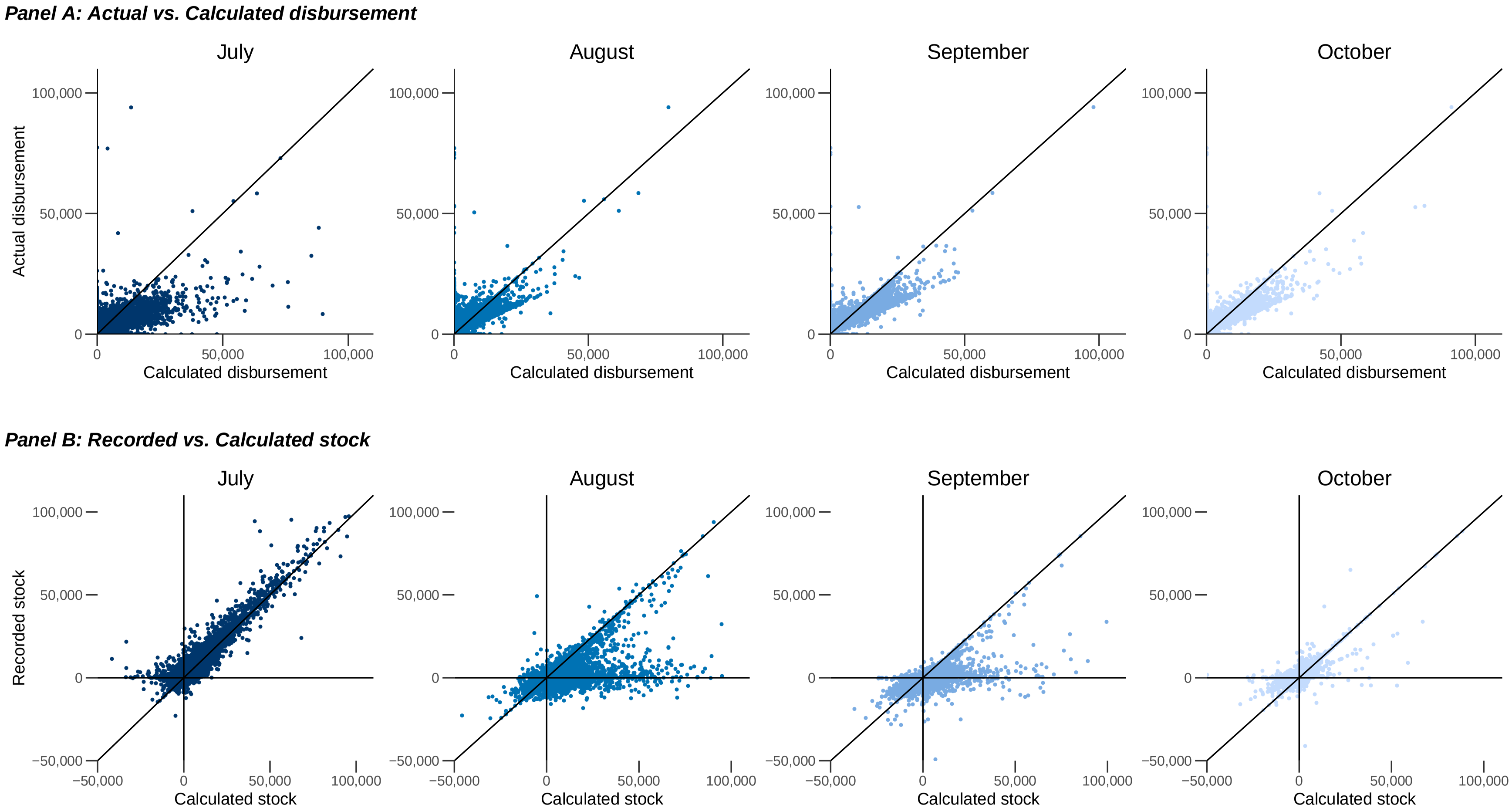
We next examine how closely the government adhered to this protocol during the four-month period of reconciliation. To do so we examine two sources of data: measures of the aggregates S_t, E_t, C_t, D_t and O_t which the National Informatics Commission maintained for each FPS and month, and transaction-level data directly from the ePOS devices themselves which record $O_{i,t}$ for each household and month (also provided by the National Informatics Commission).

According to its own records, the government was fairly flexible in its implementation of reconciliation. Figure C.1 illustrates this, showing scatterplots of the left- and right-hand sides of Equations 1 and 2 for the months July-October 2017 and for wheat and rice pooled. For both relationships, the actual quantities on record are positively associated with those we obtain by mechanically re-calculating them using the reconciliation formulae, but with substantial noise. It is particularly striking the extent to which the government held dealers accountable for less stock than implied by formula in August and September, and that in many cases they recorded a *negative* closing stock, something that should not have been possible if dealers accurately reported transactions.

Indeed, there is some evidence that the government did not take transaction data on offtake by beneficiaries at face value in all cases. Panel A of Figure C.2 plots the relationship between offtake as recorded in the official aggregates and our own independent measure of offtake calculated from the raw transaction data. In the majority of cases the two coincide exactly (85%) or are within 1% of each other (92%), but in other cases the transaction data imply higher offtake than the government aggregates acknowledge. This suggests the government may have suspected that the transaction data were over-stating actual offtake, as for example would be the case if FPS dealers asked beneficiaries to "sign" for more grain than they actually received. Indeed, Panel B – which restricts focus to our sampled ration cards – illustrates that beneficiaries generally report receiving less grain than the transaction data say they did.

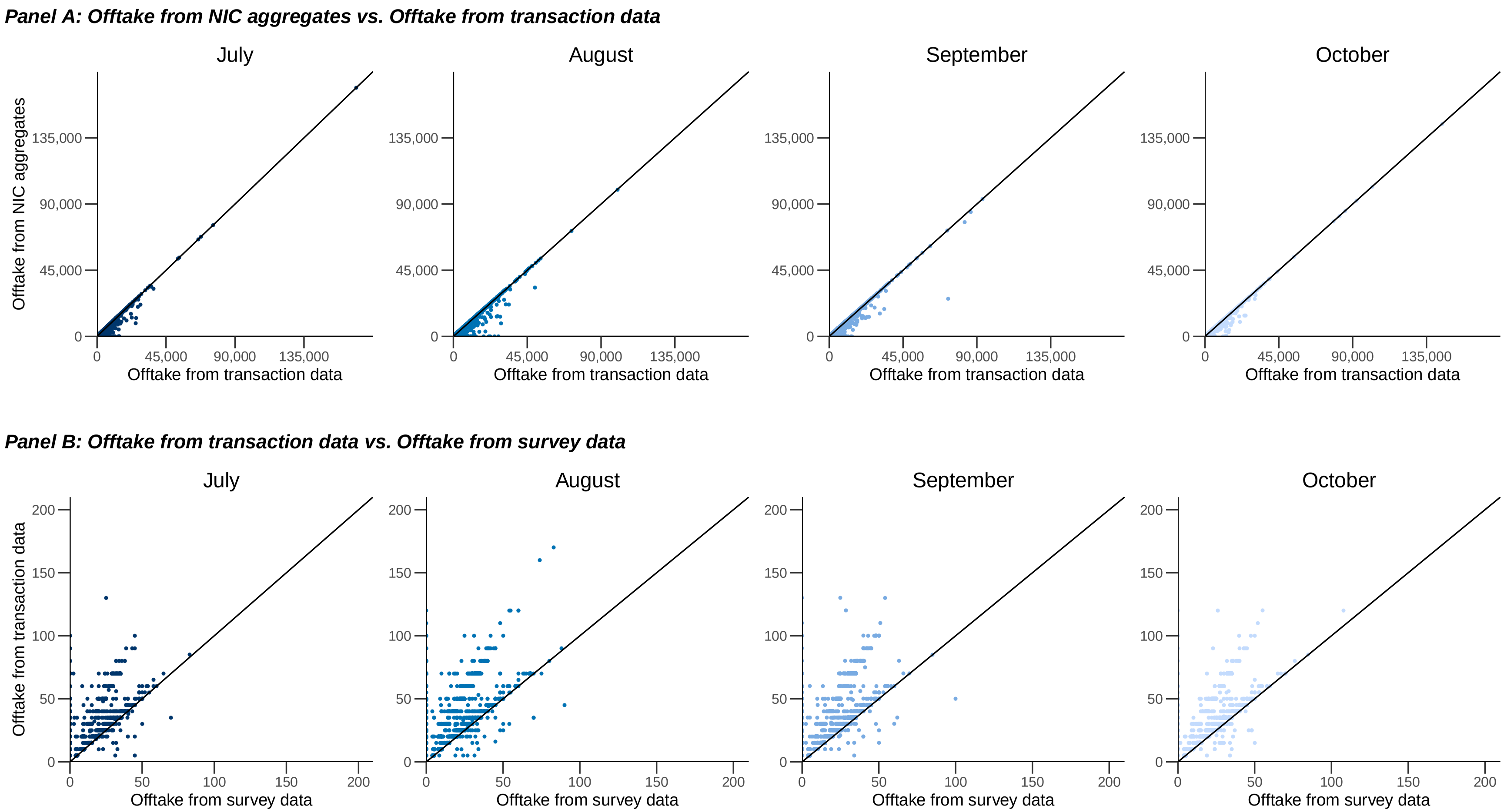
Overall, reconciliation as actually implemented was somewhat less punitive than reconciliation strictly by the book would have been. We illustrate this in Figure C.3, where we plot the evolution of actual grain disbursements per FPS against the counterfactual time series obtained by (i) recalculating disbursements assuming that the government implemented reconciliation strictly, while (ii) leaving unchanged the reported offtake of grain. If the government had implemented Equations 1 and 2 and disallowed negative values for stock, we estimate that grain disbursements would have been lower than actually observed in all four months. If it had instead allowed stock to take negative values, disbursements would initially have fallen more than they did but eventually rebounded to be even higher than entitlements, reflecting the implausibly large disbursements some ration shops were reporting and the resulting negative stock values they accumulated.

Figure C.1: Adherence of disbursement and stock to reconciliation policy, by month



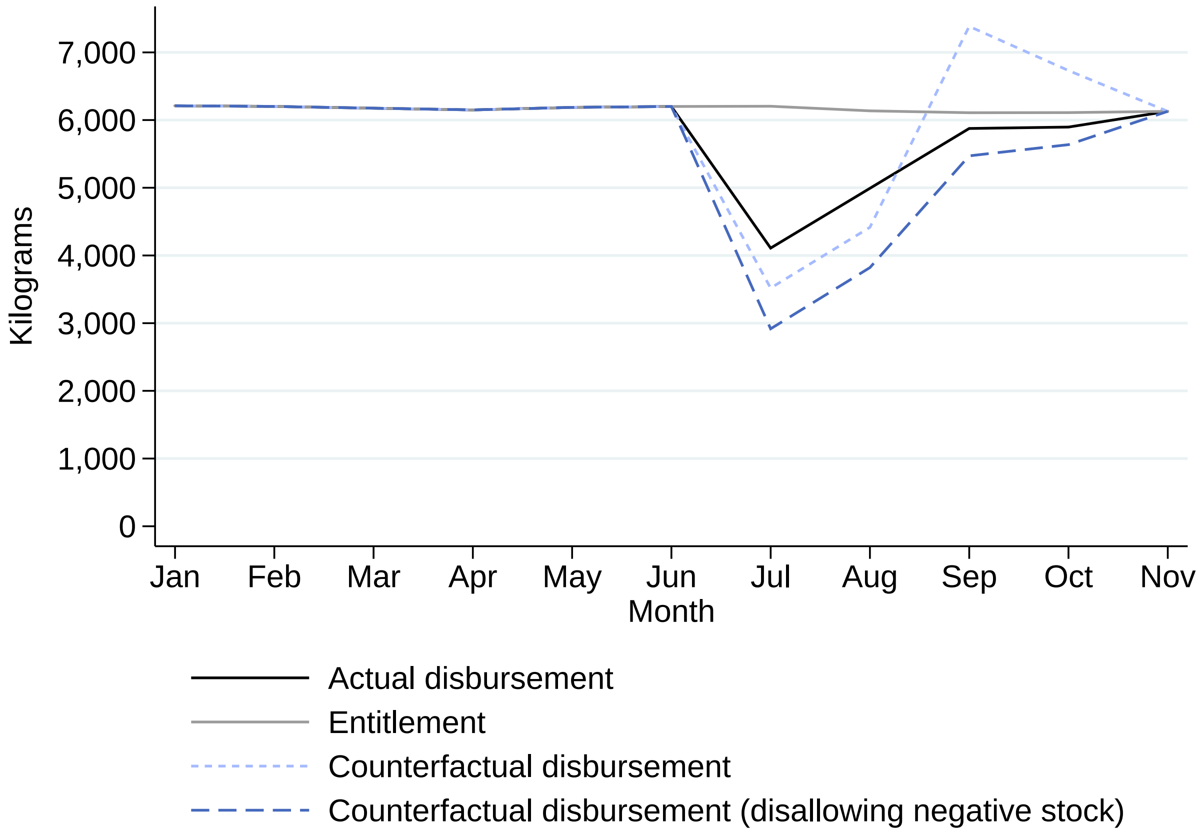
This figure shows the scatterplots of the left- and right-hand sides of Equations 1 and 2for the months July-October 2017 using aggregate measures obtained from NIC data in kilograms. Panel A has actual disbursements on the y-axis and uncensored calculated disbursements on the x-axis, and the Panel B recorded stock on y-axis and calculated stock on x-axis.

Figure C.2: Comparison of offtake from three data sources, by month



This figure compares offtake from three data sources: NIC aggregates provided by the government, transaction data from ePOS records, and survey data from beneficiaries in kilograms. Panel A plots the relationship between FPS-level offtake as recorded in the official aggregates and our own independent measure of offtake calculated from the raw transaction data. Panel B plots the relationship between ration card-level offtake as recorded in transaction data and reported by beneficiaries.

Figure C.3: Adherence to reconciliation protocol



This figure plots the evolution of actual grain disbursements per FPS against the counterfactual time series obtained by recalculating disbursements assuming that the government implemented reconciliation strictly and while leaving unchanged the reported offtake of grain. "Counterfactual disbursement (disallowing negative stock)" censors negative stock in the calculation.

D List of additional analysis conducted

The following pieces of analysis reported in the paper are additional to those pre-specified:

- Table 1, which reports the representativeness of our study area with Jharkhand;
- Panel B of Table 2, which compares measures of program implementation in treated and control areas;
- Table 7, which examines impacts on the extensive margin of value received;
- Table 8, which reports heterogeneous effects by Aadhaar seeding;
- Figure 3, which plots the distribution of value received in treated and control areas;
- Figure A.2, which compares household income and education levels between seeded and un-seeded households;
- Table A.1, which compares the dealers we originally sampled to those we added to the sample as households were re-assigned to them;
- Figure 4, which plots the evolution of value disbursed and received in treated and control areas;
- Figure 5, which plots the distribution of grain stocks as of June 2017 by treatment status.