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Food vs. Food Stamps: Evidence from an At-Scale Experiment in Indonesia*

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Abstract

Governments seeking to provide food assistance have a choice between providing in-kind food directly to beneficiaries, or providing vouchers that can be used to purchase food on the market. To understand the differences between these policies, the Government of Indonesia randomly phased in the transition from in-kind delivery of subsidized rice to approximately equivalent vouchers usable to buy rice and eggs across 105 districts comprising over 3.4 million beneficiary households. We find the transition led to substantial changes in the allocation of aid in practice. The vouchers provided concentrated assistance to targeted households, who received 45 percent more assistance in voucher areas than in in-kind districts. As a result, for households in the bottom 15 percent at baseline, poverty fell by 20 percent. Vouchers also allowed households to purchase higher-quality rice, and led to increased consumption of egg-based proteins. We find little effect on aggregate prices. Overall leakage from the program was not affected, but the administrative costs of benefits delivery substantially falls. The results suggest that the change from in-kind to vouchers led to substantial impacts on poverty through the way it changed how programs were implemented on the ground.

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I. INTRODUCTION

Targeted food programs, which aim to provide nutritional assistance to the poor, are one of the most common forms of social welfare programs in the world (The State of Safety Nets, 2019).[1] There are two broad approaches to these programs. The first approach is an in-kind program, which distributes a set amount of free or subsided foods to poor households. In developing countries, India's public distribution system and Egypt's Baladi Bread program fall into this category (Alderman, Gentilini and Yemtsov, 2018); similarly, the United States recently launched and distributed more than 100 million food boxes under the "Farmers to Families Food Box" program.[2] An alternative approach is a voucher program, which provides poor families with a voucher, or increasingly a pre-filled electronic debit card, that can be used to purchase particular food items at participating shops. One of the most famous of these programs is the United States' "Food Stamps" program, now known as the Supplemental Nutrition Assistance Program (SNAP), but these programs are very common in developing countries, such as, for example, Egypt's Tamween Ration Cards or Sri Lanka's Samurdhi program.

At some level, one might expect these programs to have similar outcomes: the government can distribute a free gallon of milk to a poor household or distribute a voucher with which a household can pick up a free gallon of milk, and in the end, a household will get a gallon of milk. However, there are a number of theoretical reasons, grounded in basic price-theory, that economists may expect these programs could lead to different outcomes. First, voucher programs typically offer more flexibility for beneficiaries than a physically delivered in-kind food basket. If a household would normally consume less of a particular type of food included in the in-kind bundle absent the transfer, in-kind transfers could constrain their ultimate consumption decisions. Vouchers, by contrast, could allow households to adjust the mix of items consumed, or even the specific brands or quality mix within those items, to best suit their needs. Thus, depending on the designs of two programs, one could observe different consumption choices, and ultimately nutritional outcomes (see for example, Hidrobo et al 2014; Cunha 2014; Aker 2017; Gentilini 2016).

[1] Acknowledging the importance of foods assistance worldwide, the Nobel Peace Prize of 2020 was awarded to the World Food Programme (WFP) "for its efforts to combat hunger, for its contribution to bettering conditions for peace in conflict-affected areas and for acting as a driving force in efforts to prevent the use of hunger as a weapon of war and conflict."

[2] See, for example, https://www.ams.usda.gov/selling-food-to-usda/farmers-to-families-food-box

Second, with in-kind food programs, the government intervenes in the market by supplying the requisite quantity of the goods in question, whereas with a voucher-based program, supply is left to the private market. If supply is elastic, this should not matter. However, with inelastic supply, this could make a difference, since in-kind programs may feature a de-facto positive supply shock, in addition to the positive demand shock common to both in-kind and voucher programs. In particular, one may imagine that food supplies may be less elastic in remote and isolated areas with limited transport capacity, in which case in-kind transfers could reduce prices relative to voucher programs if the government is sourcing the in-kind food elsewhere and bringing it in (Coate et al 1994; Cunha et al 2019). Third, in-kind programs could have important targeting properties through self-selection into the programs (Nichols and Zeckhauser 1982; Currie and Gahvari, 2008): often times the food provided in these programs is a type of food that richer households may not particularly need a lot of, or of much lower quality, so much so that richer households may choose not to avail themselves of these programs, ensuring that limited program resources get to the poor.[3] The flexibility of voucher-based programs may mean that this type of self-targeting is less effective.

However, in addition to these commonly cited reasons, these programs may differ for another important, but less frequently discussed reason: the relative ability to administer these programs on the ground as designed in theory could differ dramatically. In low state-capacity environments, these differences may be first-order. There are a number of important administrative differences. First, the logistical challenges of running voucher and in-kind programs are different. On the one hand, it may be easier and cheaper for governments to refill electronic vouchers each month rather than run a massive logistics operation, moving millions of tons of foods to the various parts of the country. On the other hand, administering debit cards raises its own set of logistical challenges: availability of cell-phones signals for debit card machines, challenges with PINs or other authentication mechanisms (Banerjee et al 2020; Muralidharan, et al, 2020; Muralidharan, 2016), or even challenges in ensuring that debit cards reach the correct households. Second, governments using vouchers may have more control over whether targeted beneficiaries actually

[3] There is also substantially larger literature in economics studying the difference between *cash*, which is completely flexible, and in-kind programs, as opposed to the question we focus on here of whether the in-kind assistance is actually directly delivered or delivered via vouchers. See Currie and Gahvari 2008 for a review; some more recent papers include Lieber and Lockwood (2019) on self-targeting within Medicaid in the United States; Hastings and Shapiro 2018 on mental accounting and stickiness of in-kind transfers studying SNAP in the United States; and Hidrobo et al (2014) examining cash versus in-kind in Ecuador.

get their full benefits, since a debit card is hard to subdivide, whereas in-kind food stocks can be divided by local government officials in whichever way that they choose in practice.[4] Finally, overall leakage may differ in voucher programs: it may be easier to monitor the money as it is directly transferred to the debit cards, whereas food can "fall off the truck" at various points in the distribution process. On the other hand, it may be easier to fraudulently claim digital payments than to abscond with tons of physical rice. In short, it is ultimately an empirical question as to which of these programs can be more effective in redistributing to the poor, and to ultimately affecting poverty rates.

To study the differences between in-kind food programs and electronic voucher programs in a setting with imperfect state capacity, we conducted a unique policy experiment in Indonesia in cooperation with the Indonesian government. Starting in 2017, Indonesia began a national reform to replace its largest anti-poverty program—"*Rastra*" (an abbreviation of *beRAS sejahTeRA*, or Rice for Welfare), an in-kind food program that delivered 10kg of free rice per month to 15 million targeted households nationwide—with a voucher program, named "BPNT" (*Bantuan Pangan Non-Tunai*, or Non-Cash Food Assistance), that aimed to provide the same targeted households with a debit card that allowed them to purchase a similar value of rice and eggs from any eligible private provider. The conversions were rolled out district by district, with the districts staggered into waves, such that the government could have the funds and logistical ability to switch over each set of districts. For the 2018 round of district conversions, 105 districts were deemed ready to convert, but funds were only budgeted to cover for a fraction of these districts. This allowed for random assignment, and 42 of these districts were randomly assigned to receive the program over three waves in 2018, while the remaining 63 districts were assigned to receive the program in 2019.

The enormous scale of this experiment – the 105 districts in the experiment have a combined population of 53 million, or about 1/5 of Indonesia's population, with over 3.4 million beneficiary households—allows us to study how in-kind transfers compare to voucher programs through the lens of both the price theory and administration in a real world setting at a large enough scale to incorporate general equilibrium effects (Muralidharan and Niehaus 2017). To study these

[4] There are, of course, many reasons why beneficiaries would not necessarily get their full voucher payment—they do not receive the debit card, they get held up by local officials or stores to process their voucher, or other non-poor community members or family members ask for a share given that the targeted household got a transfer—but the technological nature of the voucher may make it easier ensure that targeted households receive their entitlements.

issues, we use three primary datasets. First, we wrote a special module that was included in three waves of the national social-economic survey (*SUSENAS*), which we use as well as use its detailed consumption module. Second, we merged the SUSENAS survey with the administration targeting database at the household level to both identify which households were to be targeted by the programs, as well as to provide us with baseline characteristics of these households. Third, we merged in pre-data from the village census (*PODES*) to provide additional baseline data and allow us to measure heterogeneity by village characteristics.

We find that switching from in-kind transfers to vouchers led to a substantial change in the allocation of aid. Whereas in-kind food aid was often subdivided and distributed to many households in villages, vouchers were not. A similar amount of total aid was distributed in both voucher and in-kind districts, but conditional on receiving benefits, a household received a subsidy worth 83 percent more in voucher areas compared to in-kind areas. The share of households receiving a transfer fell in voucher areas, but disproportionately among better-off households: poorer households (at baseline) were 14 percent less likely to receive benefits in voucher areas, whereas wealthier households (at baseline) were 50 percent less likely to receive benefits in voucher areas. Combined, this resulted in a dramatic concentration of benefits among the poor: poorer households (at baseline) as a group received 45 percent more assistance in voucher areas than in in-kind districts. Interestingly, the better targeting in voucher areas occurred despite the fact that the food was rated as higher quality in the voucher program, and aid was more fungible, both reasons that the vouchers could have been more appealing to richer households. The administrative benefits of being able to better enforce the targeting of the electronic transfers thus swamped the self-targeting benefits of the poor quality of food in this context.

The increased concentration of benefits among the poor in voucher areas led to a large reduction in poverty, despite the fact that some poor households did lose benefits. For households in the bottom 15 percent at baseline, the share of households below the poverty line fell by 20 percent (4.3 percentage points). Thus, on net, by better concentrating benefits to targeted households, the voucher program reduced poverty.

In addition to these first-order changes from increased concentration of assistance, we also examine whether some of the more traditional price-theoretic mechanisms associated with the switch to vouchers are present in this context. First, we observe some changes in types of food consumed with the switch to vouchers. Almost all households consume more rice than what was

provided under the in-kind transfers – 97 percent of households in in-kind areas consume more than the 10kg rice / month, and in fact, the mean household consumes about 32 kg rice/month.[5] Given this, one would not expect a switch in the consumption bundle from a transition from the in-kind program to the voucher program, which also allowed households to purchase eggs, since households should have been unconstrained in their rice consumption and able to adjust on other margins. Yet, the switch to vouchers led eligible households to increase their *total* consumption of egg-based proteins, by about 4.5 percent. We do not observe changes in any other types of food, including other proteins such as beef and chicken, suggesting that this is unlikely to represent just an income effect. Instead, it appears that there is some type of stickiness from the types of food included in the voucher programs. This suggest that governments seeking to improve nutrition can potentially do so through the set of foods they chose to include or not include in vouchers, even if, in fact, basic price theory says that unconstrained households could undo these constraints through their other purchase decisions.[6]

One important concern is that voucher-based programs could lead to increases in prices relative to in-kind programs, particularly in remote areas (Cunha et al 2019). This was an important concern, especially given that, in some districts, the in-kind program represents on average about 6.2 percent of total rice consumption at baseline. On average, we do not find any change in rice prices associated with the transition. This is true both overall, as well as in districts where the transition represented a large negative supply shock, or in villages that are remote (as measured in four ways, looking by whether a village has an asphalt road, a road that is passable in all seasons, travel time from the village to the district's capital, and the time to the subdistrict's capital). Thus, on net, the types of concerns raised by Cunha et al (2019) in their study of Mexican mountain villages, whereby a switch from in-kind to vouchers could lead to substantial price effects, do not generally seem to pose a challenge in this context.

Finally, we examine the administrative costs of the programs. The administrative costs are low even for the in-kind program: the administrative costs are 4 percent of the benefits disbursed. But, the vouchers are even lower: between 0.75 to 2 percent of benefits disbursed, depending on

[5] The in-kind program actually provided a mean of only 3.9 kg of rice per month, since it was spread among so many more beneficiaries. More than 99 percent of eligible households consumed more than 3.9 kg of rice per month, so the in-kind program was likely binding for almost no one.

[6] This finding is reminiscent of the findings from Hastings and Shapiro (2019) that households have a higher marginal propensity to consume food out of SNAP benefits than out of cash. Our findings suggest that even the *types* of foods consumed can be influenced by what is included in vouchers, even when this is not binding.

what portion of the financial transaction infrastructure one apportions to the program relative to other transactions. The voucher program was thus substantially less costly to administer, despite the fact that it was more effective in delivering aid to targeted beneficiaries.

In short, switching from in-kind food delivery to vouchers made a real difference in the lives of the poor, reducing poverty among the poorest households by about 20 percent. This change was due to relative gains in program administration, as the vouchers resulted in a de facto allocation that was much more concentrated among the poor, consistent with the de jure program rules in both programs. The differences that occur due to differences in how these two programs were implemented in practice – i.e., the fact that the voucher-based program delivered concentrated assistance to targeted beneficiaries, rather than being distributed much more widely in local communities– swamped the differences in the programs that stemmed from more traditional price theoretic explanations, such as the ability for households to have more choice in spending or changes in aggregate prices.

The rest of the paper proceeds as follows. Section II describes the setting and research design. Section III presents our findings the effect of voucher versus in-kind subsidies on the delivery of assistance, consumption choices, prices and leakages. Section IV concludes.

II. SETTING, EXPERIMENTAL DESIGN, AND DATA

A. Setting

In 1998, during the Asian Financial Crisis, the Indonesian government launched a new program to provide a safety net to poor households in the form of heavily subsidized rice, delivered directly to poor beneficiary households through village and local governments.[7] With an annual budget of US1.5 billion dollars, the subsidized rice program, called "Rastra," aims to provide subsidized food assistance to 15 million beneficiary households per month.[8] While the program has changed slightly over time, in its current form, households are entitled to receive 10kg per month of free rice per month, with a value of about Rp. 100,000 (US$8) / month, depending on market price of

[7] Rastra is implemented by the lowest government administrative unit, known as a *desa* (village) in rural areas or *kelurahan* in urban areas. Both *desa* and *kelurahan* refer to the 4th tier of local government in rural and urban areas, respectively, below provinces, districts/cities, and subdistricts. We henceforth refer to these government units as "villages" throughout the paper, but it is important to note that the program operates identically in both rural and urban areas nationwide.

[8] The program was launched in 1998 under the name *Operasi Pasar Khusus* (OPK, or Special Market Operations). It was renamed *Raskin* (an abbreviation of b*eRAS misKIN*, or Rice for the Poor) in 2002, and renamed *Rastra* in 2015.

rice (~Rp 9,700 in our period). This is a substantial subsidy: it is about 6.5 percent of the poverty line for family of 4.

In theory, to be eligible for Rastra, targeting is conducted through proxy-means testing conducted by the central government, with the recipient list provided by the Ministry of Social Affairs. The proxy-means test was computed by conducting a census of more than 28 million households in 2015, which collected a variety of indicators that are predictive of the household's consumption level (assets, education, etc; see Alatas et al (2012) and Banerjee et al (2020) for more details).[9] The National Team for the Acceleration of Poverty Reduction (TNP2K) combines these indicators into a proxy means test score, and then computes percentiles of this score within the entire population; we use this percentile-normed score throughout the paper. In general, households with lower PMT scores are eligible for the program, with the precise cutoff varying by district and urban/rural status. Furthermore, villages are allowed some minimal changes to the recipient list through village meetings to replace households who have moved, died, or double-counted. Excluded households can also apply to the Ministry of Social Affairs if they believe they have been excluded from the proxy-means testing (Ministry of Social Affairs, 2018). According to the 2018 official rules, villages are not officially allowed to subdivide the rice – benefit recipients must receive the full 10kg.

Rice is procured by the government through the central logistics agency (BULOG), typically stored at district-level warehouses, and then delivered to villages (often in 50kg sacks). Local level officials are tasked with dividing the rice, and delivering it to beneficiaries on the list, either at the village office, at neighbor distribution points, or door-to-door.

In 2017, the government began their largest social assistance reform in nearly 20 years – removing Rastra and replacing it with a targeted voucher program. The government did this for a variety of reasons. First, while there is a robust targeting procedure, it was not often followed in practice, with many non-poor households receiving rice and many beneficiaries only receiving a fraction of their entitlement (Banerjee et al 2018). It was believed that the voucher program could reduce the improper inclusion of non-beneficiary households. Second, and related to the first point, Rastra was known for having high levels of overall leakages—a fair share of the rice went missing

[9] To comprise the sample, the government first lists everyone on the previous targeting list and asks local leaders to help update the sample list with poor and near-poor households (i.e. the goal is exclude richer households from the list). In 2015, this resulted in 28 million households being surveyed on household composition and assets variables.

(Olken 2006), and there was a belief that the switch to vouchers could reduce this leakage. Third, the quality of Rastra rice was very low--dusty, full of rocks (Banerjee et al, 2019)—and while this could have self-targeting properties, there was a belief that the move to vouchers could improve program satisfaction by increasing rice quality. Finally, Indonesia has high levels of stunting, even conditional on income levels (Cahyadi et al forthcoming), and there was a belief that the vouchers could increase food diversity by allowing households to also purchase eggs.[10]

The new voucher program was called Bantuan Pangan Non-Tunai (BPNT), or "Non-Cash Food Aid." In principle, program eligibility was the same as Rastra. Households received a monthly voucher of Rp 110,000 per month onto a debit card that was issued to the female adult in the household. The amount was chosen to be approximately equal to the value of the subsidized Rastra rice.[11]

Households could use the voucher for purchases of rice or eggs at a network for eligible small shops, i.e. shops which were registered as remote agents for the state-owned bank chosen to implement the program in the district.[12] The bank provided each agent with a debit-card reader machine (EDC machine), which connected to the bank's network over a cellular connection to debit the amount from the household's voucher account. The voucher could not be cashed out; it could be saved for future months, but that was not always encouraged and rare in practice. The government required banks to increase the number of agents, with a goal of 1 agent per 250 beneficiaries in every village, with a minimum of 2 shops in every village. While the agent network did increase, and almost all villages had at least 1 shop, not all villages met the government mandated standards (Banerjee et al, 2020). Agents were allowed to source rice and eggs from the

[10] In fact, in 2020 (subsequent to the period we study here), the BPNT voucher program later morphed into a program called Program Sembako, which allowed households to buy a much wider a variety foods, including carbohydrates (rice, but also locally-prefererd carbohydraes such as corn or cassava), animal proteins (eggs, and also beef, chicken, and fish), vegetable proteins (nuts, tofu, and tempeh), as well as fruits and vegetables.

[11] The Rastra rice was valued at 10kg * the market price of rice, which fluctuates around Rp 10,000. Since Raskin rice is often delivered in neighborhoods or door-to-door, whereas BPNT is at one or two shops in the village, the BPNT amount was slightly higher to compensate for slightly higher transportation costs borne by households. Given the fluctuating price of rice, it is possible that food transfers provide some insurance against a price increase (see, for example, Gadenne et all 2020), but in this setting, the government can easily adjust the BPNT value to compensate for such macroeconomic shocks and, in fact, did exactly this in the recent COVID pandemic in which the benefit of the BPNT was increased by 25% (which is outside of the time-frame that we study).

[12] The debit cards were issued and administered by one of the 4 state-owned banks, Mandiri, BRI, BNI, and BTN. The government assigned one bank to administer the program in each district. As such, they could only be redeemed at agents of the designated bank.

private market as they saw fit, thought in practice there was pressure in some areas to source rice from the government logistics agency.

B. Sample

The conversion from Rastra to BPNT was rolled out over approximately four years, as the government had budget targets in place for the yearly conversion processes. The program was first rolled out to 44 cities as a pilot in 2017. For the second phase, in 2018, the government first chose a number of districts (e.g. primarily in East Java) to definitely be converted in 2018. It then ranked all remaining districts by a readiness indicator; based on this indicator 105 districts were potentially ready to be converted next. However, given the government's budget—they planned to convert the program for about 8.3 million beneficiary households total, including the ones that were definitely chosen—they could not convert all 105 districts to BPNT in 2018.

Therefore, the government randomized which of the 105 districts would be treated in 2018, and which ones would subsequently be treated in 2019 (see Appendix Figure 1). These 105 districts—spread across Indonesia's diverse landscape—comprise our sample (see Appendix Figure 2), encompassing about a fifth of Indonesia's population, with a total population size of 53 million individuals and about 3.4 million targeted beneficiary households. Given the scope, as well as the fact that these were real programs conducted by the government, this sample gives us a unique opportunity to study both the general equilibrium effects and administration outcomes of moving from an in-kind to a cash transfer program.

C. Randomization Process and Compliance

We randomized at the district level, as it is the level of administration for both the in-kind and voucher programs. Forty-two districts were randomly chosen to be converted in 2018, while the remaining 63 were scheduled to converted in 2019 (see Appendix Figure 1). The treatment districts were additionally randomized to be converted in three waves: 10 districts in May 2018, 18 in October 2018, and 14 in November 2018 (see Appendix Figure 3).

Note two aspects of the randomization. First, we grouped districts by geography, and then stratified our sample based on this geographic grouping. Second, the government added a constraint that it wanted the randomization results to come as close as possible to exhausting the budget of 8.3 million households (including the districts that were chosen for sure, as well as

randomly chosen districts). To accomplish this, in setting up the geographic strata, we reserved the 20 districts with the smallest number of beneficiaries and put them in a special stratum. For this last stratum, we randomized order of districts, and assigned districts to treatment until assigning one more district (in randomized order) would have exceeded the 8.3 million total budget. Therefore, while the probability a given district is treated is random, the treatment probability in the holdout stratum is different than in other strata; to account for this, we include strata fixed effects in all regressions. We also show below robustness results dropping this holdout stratum entirely; results are very similar.

In practice, the randomization was followed almost exactly. All of the districts randomly chosen to receive vouchers were converted to vouchers in 2018. Only 3 of the 63 districts randomized to be converted later were in fact converted in 2018; given this small number, we study the reduced form effect of being assigned to the vouchers (i.e., the intent to treat).

D. Data

In order to evaluate the experimental switch from in-kind transfers to the vouchers program, we worked with the Government of Indonesia to design a special module to be added to the SUSENAS, the Indonesian national household survey. The SUSENAS is a national representative households survey, completed twice annually—a large wave in March covering about 300,000 households and a smaller wave in September covering about 75,000 households—by the Indonesian Census Bureau (BPS). It is a repeated cross-section of households.

We use two waves of the SUSENAS for our main analysis: March 2018 and March 2019. We focus on the March 2019 SUSENAS as our endline, as all of the districts randomly assigned to vouchers had been converted before March 2019, whereas the districts randomly assigned to later conversion had not (see timeline in Appendix Figure 3). The special module collected questions on whether households received each program, how much they received from each program, prices and quantities of subsidized rice and eggs, the quality of rice, and so forth. In addition to the social protection module, we also analyzed the separate, detailed consumption module normally administered in the survey, which not only allowed us to look at total household consumption of different commodities (e.g. rice, eggs, cigarettes, etc), but also the household poverty measures. Note that these two modules –the social protection module, which covers social assistance, and the consumption module, which covers consumption from all sources – are distinct

sections of the survey. For all variables, we construct district * rural/urban averages of the same variables in the March 2018 SUSENAS, which we merge in at that level as potential baseline control variables.

The second data set we use is administrative data from the unified targeting database (UDB). As described above, this database was constructed for targeting purposes in 2015, and forms the basis of eligibility for both the in-kind and voucher program. The government merged UDB variables into the SUSENAS data using each individual's national identification number (NIK), which is collected in both datasets; we then analyzed the de-identified version of this merged data. The merge is important for two reasons. First, it allows us to independently identify who in the SUSENAS is likely to be eligible for the program, since targeting was done by the Ministry of Social Affairs using UDB data. Second, as the SUSENAS is a repeated cross-section, we do not have household level baseline variables, which are useful to examine heterogenous treatment effects and to absorb residual variation. The UDB variables, since they were collected in 2015, give us baseline measures of household well-being (PMT percentile scores) for heterogeneity analysis, as well as baseline family composition and assets that can be used as potential control variables. In particular, the PMT percentile score is the predicted percentile of the consumption distribution the household would be in, based on the household's assets, composition, etc. Finally, we merged the 2018 village census (PODES) to provide village-level baseline control variables, as well as to be able to explore heterogeneity by baseline village characteristics.[13]

E. Experimental Validity

We examine potential differences between the in-kind and voucher districts in using baseline data from the 2018 SUSENAS. We chose the variables for the randomization check in January 2019, covering per capita consumption, caloric consumption and basic assets. In general, looking at the differences in Table 1 across the variables considered, we find that treatment and control groups

[13] The 2018 PODES was enumerated from May 2 – May 31, 2018. Since BPNT distribution in the first set of units did not occur until the end of the month (starting May 25, 2018), and since PODES captures long-moving variables such as population, infrastructure, and so on, we consider the 2018 PODES as baseline variables. (See https://www.tribunnews.com/bisnis/2018/04/22/bantuan-pangan-non-tunai-tahap-2-cair-bulan-mei-2018-penerimanya-nambah-2-juta). We present robustness results below that drop these as well as other control variables; results are qualitatively similar.

appear balanced at baseline. Only 1 of the 11 variables is statistically significant, which is what one would expect by chance, and the randomized inference p-value of a joint F test is 0.384).

III. Results

Section III.A outlines the estimation strategy. We then begin by exploring the impact of switching from the in-kind to voucher program on the delivery of assistance and poverty in Section III.B. Section III.C explores the impact on consumption choices, while Section III.D. explores the impact on overall rice prices. Finally, section E explores the impact on leakages and administrative program costs.

A. Estimation

We estimate the impact of the experimental switch from in-kind assistance to vouchers using Equation 1:

$$y_{hvds} = \beta_0 + \beta_1 Voucher_{ds} + \boldsymbol{X'_{hvds}\gamma} + \alpha_s + \epsilon_{hvds} \quad (1)$$

where y_{hvds} is the relevant outcome variable; $Voucher_{ds}$ is the randomization into the voucher conversion in 2018; $\boldsymbol{X'_{hvds}}$ are control variables selected using a double LASSO (Belloni et al, 2014); and α_s are strata fixed effects. The baseline controls fed into the LASSO include household covaries from the UDB, village level covariates from the village census and district*urban/rural averages of variables from the March 2018 SUSENAS. A full list of the control variables used as input into the LASSO is shown in Appendix B. We estimate the intent to treat using the original randomization results (only 3 of our control districts were treated during the study period, so treatment-on-treatment models would be very similar). Standard errors are clustered by district, as that is the level of randomization; we report randomization inference p-values in all tables (Young, 2019).

Following our pre-analysis plan, we differentiate results based on households that are likely to be eligible subsidized assistance, and households unlikely to be eligible. To determine this, we use the baseline proxy-means test (PMT) score each household received in 2015 in the unified database (recall, as described above, that the PMT score is the household's predicted percentile in the national per-capita consumption distribution based on household assets and household

composition). The Ministry of Social Affairs drew up the final list of eligible beneficiaries in each district using this dataset as a basis. Since targeted was aimed at approximately households in the bottom 30 percent of the population, following our pre-analysis plan we divide households into two groups: those with a PMT score of 30 or below, and those who had either a PMT score above 30 or above or those who were not included in the UDB.[14] We also examine several additional cuts of the PMT score distribution, and in particular those who with a PMT score of 15 or below, i.e. below the approximate poverty line.

While imperfect, the 2015 PMT score cutoff of 30 has strong predictive power both for consumption/poverty levels and for program eligibility. Appendix Table 2 provides summary statistics from the March 2018 SUSENAS data (i.e. the baseline household survey), separately for the two main groups we analyze: those with PMT scores <= 30 (i.e. the approximate target group), and those with PMT > 30 / those with no PMT score. The data reveal that households with PMT scores <= 30 are substantially poorer on virtually all dimensions. In particular, for households with PMT scores <= 30, average per-capita consumption is Rp. 689,000 / month; for those with PMT score >= 30, average per-capita consumption is Rp. 1,116,000 / month – i.e., 62 percent higher. Appendix Figure 4 plots this relationship as a bin-scatter, showing average baseline per-capita consumption levels for households by PMT scores (in bins of 5). There is a very strong, nearly linear relationship between PMT scores and mean baseline consumption levels. This is true within the set of households who have PMT scores – households with a PMT score from 0-5 have mean consumption of about Rp. 590,000 per month, compared to Rp. 940,000 or more for those with PMT scores 40 and above. Households who were not included in the PMT process (shown as PMT = 100 on the graph) have even higher average consumption, averaging almost Rp. 1,200,000 per capita.

Appendix Table 2 also analyzes the relationship between PMT scores and a number of other variables, including some not included in the PMT formula (i.e. poverty rate, own-flat screen television) and variables included in the PMT formula (e.g. low-quality wall, floor, or roof

[14] A full pre-analysis plan can be found at https://www.socialscienceregistry.org/trials/4675. We pre-specified the regression in (1), as well as pre-specified the separation by baseline PMT score <= 30 or not. The primary outcome variables specified in the pre-analysis plan are total subsidy received, self-reported quality of rice, and food insecurity indicators. The 'populated analysis plan' (Banerjee et al 2020) for the primary outcome variables can be found in Appendix C.

material, ownership of refrigerators). Households with low PMT scores appear poorer on both sets of variables.

The PMT cutoff that we use is also strongly associated with program eligibility: the households in the PMT <= 30 are substantially more likely to have been officially eligible for the Rastra program in 2017 (75 percent eligible for those with PMT <= 30, compared to only 5.7 percent for those with PMT > 30), and to have received the Rastra program in 2018 (71 percent for those with PMT <= 30, compared to only 33.4 percent for those with PMT > 30). Given these results, we interpret the split based on household pre-period PMT score as indicative of both official program eligibility and overall poverty levels.

B. Delivery of Assistance and Poverty

Who gets assistance, and how much?

We begin by showing the impacts on the receipt of subsidized assistance. First, we examine the average monthly value of assistance that households receive from either the voucher or in-kind program, in Rupiah, averaged over the 4 months prior to the survey.[15] For BPNT, we take the average reported receipt in Rupiah; if the monetary value is missing in the survey but quantities are available, we take the amount of rice and eggs reported, multiplied by the market rate of the item in each rural or urban area of a given island/island group.[16] We value rice received in-kind at the market rate in each rural or urban area of the given island group, measured from the SUSENAS consumption module. Note that households who received nothing from either program are included with a value of zero.

Figure 1, Panel A begins by showing the treatment effect on value of the total subsidy received non-parametrically. We break the households into bins based on household pre-period PMT scores (i.e. PMT scores 0-5, 6-10, and so on, with those who do not have PMT scores – typically the wealthy – assigned the maximum score of 100).

Switching from an in-kind program to vouchers greatly increased the ability of the government to target aid to eligible households. Poorer households, as measured by having lower baseline PMT scores, experience substantial gains in the total amount of subsidy received. The

[15] Note that 1 USD averaged Rp. 14,000 during the period of this study.

[16] By island groups, we refer to the main regions of Indonesia, i.e. these units are even larger than provinces, and much larger than the districts which are the unit of analysis here. The island groups we use in our data are Sumatra, Java, Kalimantan, Sulawesi, Maluku, Papua, and Bali/Southeastern Indonesia.

gains are most pronounced for those with PMT scores of 20 or below, which corresponds to the bottom 18 percent of the population. Households in the middle of the distribution received about the same amount of subsidy on average. By contrasts, households at the top – i.e. those without PMT scores (shown as PMT = 100, which are largely households that local communities saw as too wealthy to be measured as possibly poor), saw the amount of subsidy they receive fall considerably.

Table 1 presents the regression results from estimating equation (1) for total subsidy, shown for all households (column 1), households with PMT scores <= 30 (i.e. targeted beneficiaries), and PMT score > 30 (i.e. those not targeted).[17] The key result is that the voucher program reallocated aid towards those with low PMT scores, i.e. the poorer, targeted group.[18] Specifically, as shown in Column 2, targeted households received about 45 percent more in subsidy in voucher districts (Rp 13,234 additional per month on average, compared to Rp. 29,200 per month in in-kind areas; $p < 0.001$). Conversely, the vouchers led to a reduction in subsidy received for those above 30. Those with PMT scores > 30 already received substantially less in in-kind areas – only Rp. 9,161 on average – and this fell by 28 percent in voucher areas ($p = 0.001$). Overall, the mean amount received in in-kind areas across all households was slightly higher in voucher areas, by about 9% (column 1, $p=0.087$).[19]

This change came about because households who received the transfer received substantially more in voucher areas, i.e. the aid was substantially more concentrated to the poor in voucher areas. On the one hand, Figure 1, Panel B shows that the probability of receiving any aid is lower for *all* groups in voucher areas compared to in-kind areas, regardless of their PMT score. This difference in the probability of receiving any aid is smallest for those with the smallest PMT scores, but more substantial for those with PMT scores above 30. Table 1, columns 4-6, show the

[17] Table 1 examines outcomes in the March 2019 SUSENAS. The September SUSENAS also included part of the new social protection module. It was a smaller wave in terms of both sample size and included questions (e.g. the quality question was not included). Perhaps more importantly, only 10 of our 43 treatment districts were randomized to be treated by the time of the survey, and so we would expect to have less statistical power. Nonetheless, if we run our analysis only in September (defining as treated the 10 districts randomly selected to be treated prior to September), or in the pooled September and March data, we find very similar results. See Appendix Table 3 and 4.

[18] Our results are remarkably robust to specification choices, such as dropping the controls selected via LASSO (Appendix Table 5), dropping the holdout sample (Appendix Table 6), or winsorizing the subsidy outcome (Appendix Table 7)

[19] This likely represents the fact that, due to fluctuations in the market price of rice, at the time of our survey the market price of rice was only Rp. 9,700/kg nationwide, so 10kg of in-kind rice would be valued at Rp. 97,000, compared to Rp. 110,000 for the voucher. We explore more systematically whether this result represents a reduction in leakage from the program in Section D below, and find that most likely it does not.

results in a regression format. The probability of receiving any assistance falls 16 percent (10.5 percentage points; p< 0.001) in voucher areas for those with PMT scores <= 30; for those with PMT scores > 30, the probability of receiving assistance falls by 49 percent (14.5 percentage points; p<0.001).[20] Thus, while everyone across the income distribution has a lower probability of receiving the voucher program than the in-kind program, the reductions were much more pronounced for those that were not targeted.

On the other hand, those who do receive assistance receive substantially more in assistance in voucher areas. Column 7 of Table 1 shows that, conditional on receiving assistance of some form, recipients in voucher areas received 84 percent more in voucher areas than in in-kind areas on average over the previous 4 months (Rp 30,957 more; p<0.001). In particular, conditional on receiving anything, recipients in voucher areas are substantially more likely to receive the full amount they are entitled to. This pattern can be seen in Figure 2, which plots histograms of the average amount received in the 4 months prior to the survey in both in-kind and voucher districts. Panel A in Figure 2 shows all respondents with PMT score <= 30; Panel B shows only respondents with PMT score <= 30 who received some transfers. What is remarkable are the 4 regularly spaced spikes in Panel B. These correspond to households who received the full Rp. 110,000 transfer in the previous 4 months, 3 of the previous 4 months, and so on. By contrast, recipients in in-kind areas not only tend to receive substantially less on average, but they also do not show the clear bunching we see in the voucher areas. In fact, in February 2019, 97 % of BPNT recipients who reported their transfer amount received the full Rp.110,000, compared to only 18% of Rastra recipients who reported receiving the full 10kg. Similar patterns can be seen in Panel A, which shows all respondents.

Combined, these results show a clear pattern. In in-kind districts, assistance is subdivided into much smaller amounts, so that the typical beneficiary receives only about 1/3 of the intended transfer size. The additional funds are used to give many more households assistance, despite the fact that this is explicitly against the stated rules. In voucher areas, this practice is substantially less prevalent. Somewhat fewer households receive assistance, but those that receive assistance

[20] The fact that households with low PMT scores were slightly less likely to receive assistance under the voucher program may be in part a consequence of the fact that the PMT lists we use here for analysis are not the exact final lists used for program distribution. As described above, the Ministry of Social Affairs made some revisions to the initial targeting lists we use here before sharing them with local governments, but we were unable to obtain this data. It is possible then that some of the households we observe not receiving the program were in fact ineligible households according to the final Ministry of Social Affairs list.

receive substantially more, and usually the full amount they were entitled to. In short, the voucher program exhibited dramatically higher fealty to program design, and as a result, transferred substantially more to those in the lowest percentiles of PMT scores.

One potential concern with this analysis is that the PMT scores do not fully capture true consumption, and that the re-targeting that communities are doing in the in-kind program is towards poor households excluded by the PMT bin (Alatas et al 2012). To examine this issue, Appendix Figure 5 graphs the subsidy received by per capita consumption percentile bins, rather than PMT bins. Note that since this measure is contemporaneous consumption (unlike our PMT data, which is measured several years prior), we subtract out the value of subsidy received from either the in-kind or voucher programs prior to calculating this graph. Appendix Figure 5 shows the same pattern as our main results – the voucher program increases the amount of subsidy received by the poorest households – particularly so for those in the bottom decile, but also for those in the 20th and 30th percentiles, and does so by reducing the amount of subsidy received by wealthier households, particularly those in the 65th – 90th percentiles of the per-capita consumption distribution.

To further probe this issue, we look within the voucher areas, and compare households who did and did not receive the program. We find that households who receive the BPNT voucher are substantially poorer than those who do not. Remarkably, this is true even controlling flexibly for the PMT score – households who receive BPNT are about 19 percent poorer than households who do not, even holding PMT scores constant. This suggests that, to the extent there are local deviations from the raw PMT scores in allocating vouchers, they go in the direction of including poorer households.[21]

There are several important differences in the mechanisms through which the two programs were administered that may explain these differences. First, there is an important difference in the divisibility of what was given out to beneficiaries. In the in-kind program areas, sacks of rice were brought to local government warehouses, where they were then packaged into individual sized portions and delivered to beneficiaries. This manual process affords little ability for central

[21] These results are shown in Appendix Table 8. Columns 1-3 present results with no controls, with district fixed effects, and controlling for dummies for each fine PMT score level (i.e., a dummy for each integer of PMT scores, i.e. 1, 2, 3, etc). Columns 4-6 present the same results, but with raw per-capita consumption as measured at endline (i.e. not making any adjustment for program receipt). Panel A considers the entire sample and Panel B restricts to households with PMT scores < 30.

government oversight. By contrast, the voucher program used individualized debit cards, with the intended recipient's name pre-printed on the card (see Appendix Figure 6). A debit card is not easily subdivided – the number of cards was fixed, and each card received the full transfer each month electronically, so local officials could not easily reallocate a part of their value to other beneficiaries.

Second, the administration of the program was switched from government officials to banks and bank agents. We collected data on the types of actors that were involved in the distribution of both programs through a phone survey to district governments. The results, in Appendix Table 9, show that that the distribution points for the in-kind program in 88% of districts are government-run, while in 99% of districts, the voucher program distribution sites are private bank agents.[22] Private agents may be less sensitive to political pressures than government officials to distribute aid to those other than non-beneficiaries. Moreover, the designation of private vendors as agents was run as part of a bank's remote-bank-agent program. Those programs are governed by banking regulations that also regulates other debit cards, which require banks to verify that debit cards are issued to the person who is listed on the account (i.e. know-your-customer rules), and require bank agents to verify that this person is indeed the person redeeming the card. The move from a program administered directly by local governments, to a program effectively administered by banks, may be a second reason for increased fidelity to distribution rules. It is worth noting that the involvement of banks in issuing and managing debit cards is not unique to Indonesia; JP Morgan Chase, for example, issues and manages SNAP electronic benefit debit cards for US food assistance in 24 states, while other private debit card issuers manage them in all other states.[23]

Quality and targeting

In addition to how *much* assistance people received, there is also a question of the *quality* assistance people received. The poor quality of rice under the in-kind program was a frequent complaint about the Raskin program, as well as other government-delivered programs more

[22] Unfortunately, the SUSENAS did not ask recipients for the food subsidy program where they received their rice (due to the fact that it was all being distributed through the government). However, the results for location of where voucher recipients redeemed the voucher for food is confirmed in the SUSENAS data, where only 11 percent of BPNT recipients stated that they received it at the government offices.

[23] Data as of 20132; see http://www.g-a-i.org/wp-content/uploads/2012/10/GAI-Report-ProfitsfromPoverty-FINAL.pdf

broadly (Jacoby, 1997). Even though most rice consumed in developed economies like the United States is of high quality, in Indonesia, rice has many quality grades, and lower-quality rice often contains small stones, is off-color, has broken kernels, or smells bad, all of which are complaints that have surfaced about the Raskin program (Banerjee et al, 2019).

The SUSENAS household survey module asked respondents about the quality of rice received, on a 3-point Likert scale, which we transformed to percentiles between 0 and 1. The question was asked identically for both the in-kind and voucher program recipients. On this scale, column 8 in Table 1 shows that households rated the voucher rice substantially higher quality, by about 37 percent. This suggests that the quality-adjusted amount of subsidy received by targeted beneficiaries in voucher areas is even higher than the 45 percent increase discussed above. Putting a monetary value on this quality improvement is challenging, but a back-of-the-envelope calculation suggests that the quality-adjusted values of subsidy received by targeted households in voucher areas may have further increased the amount of the transfer received by about 20 percent.[24]

The quality improvements in the voucher program are likely related to the fact that sourcing of the rice was more flexible. In the in-kind program, all rice was sourced and distributed by BULOG, the government logistics agency. By contrast, in the voucher program, agents were able to source rice where they see fit – which resulted in a very different sourcing pattern. Specifically, a survey of agents conducted by the World Bank found that in the voucher program, 7.2 percent of agents reported sourcing their rice only from the government (BULOG), 27 percent report a mix of BULOG and the private market, and 65 percent reporting sourcing it entirely from the private market.

The improvement in the quality of the assistance makes the improved targeting even more remarkable. A common argument in favor of providing lower-quality food assistance is that the low quality serves as a screening device, so that it can be better targeted to the poor (Nichols and Zeckhauser, 1982; Besley and Kanbur, 1988). What is remarkable here, however, is that targeting

[24] To compute the quality adjusted price, we take the point estimate from the subsidized rice quality results and transform this into a z-score. That is, the quality improvements in Table 1 translate to an increase of 0.71 standard deviations on the quality measure. We then calculate the rice price distribution paid by residents within each district-urban area, which may capture heterogeneity in rice quality. Finally, we assign a monetary value of the quality improvements equal to that number of standard deviations of the rice price (i.e. if the standard deviation of rice price is Rp. 1,000/kg, and the quality measure in Table 1 represents 0.25 standard deviations, we would assign a price equivalent of Rp. 250/kg). On average, this suggests that the quality improvements in Table 1 would be would be worth an additional Rp. 1060 per kg. Appendix Table 10 shows that, when we used a quality-adjusted price compared to a fixed price, the benefits of the voucher program for households with PMT <= 30 are about 20 percent higher.

improved despite the food being substantially higher quality. In this case, the improved administrative process of the voucher program swamped whatever targeting benefits may have been accruing from lower-quality in-kind rice.

Impacts on Poverty

The previous analysis showed that the vouchers channeled additional resources to the poorest households, we next ask whether this effect was large enough to affect household poverty rates.

To examine this, for each household, we compute whether they are below the official Government of Indonesia poverty line, which is set separately for rural and urban households in each province. Since richer households are largely above the poverty line to begin with, one might expect stronger results for those households who have lower PMT scores.

We therefore begin in Figure 3 by presenting the results non-parametrically, using the same bin-scatters as above. As discussed before, baseline PMT score is strongly predictive of poverty status, in both voucher and in-kind areas. About 27 percent of households with PMT scores 0-5 are below the poverty line in in-kind areas. This falls to 21 percent for households with PMT scores 5-10, about 16 percent for households in the next few PMT bins, and further at higher levels.

Figure 3 shows substantial reductions in poverty associated with the move from the in-kind program to the voucher program. This is particularly apparent for the poorest households – poverty rates fall by about 6.5 percentage points for households in the poorest group (baseline PMT score 0-5), and about 4 percentage points for households in the next-poorest group (baseline PMT score 5-10). This occurs both because more of these households were close to the poverty line to begin with, and also because – as shown above – the impact of the transfer was the largest for these groups.[25]

Table 2 shows the results for poverty in regression form. We show results for the full sample (column 1), for households with PMT scores <= 30 at baseline (column 2), and for households with PMT scores > 30 at baseline (column 3). In the remaining columns, we zoom in further on the poor, restricting to households with PMT <= 25, <= 25, and so on.

[25] Appendix Tables 11 and 12 further break down the impact on total subsidy received and rice quality by subgroup as we zoom in on poorer households, as defined by baseline PMT status. As shown in Appendix Table 11, Households with PMT scores 0-5 saw their mean subsidy increase by Rp. 19400 in voucher areas compared to control ($p<0.001$), compared to Rp 13,324 for the entire group with PMT <= 30.

The results confirm substantial reductions in poverty for those households with low PMT scores. For the full sample of all households with PMT scores <= 30 at baseline, the share in poverty in voucher areas fell by 12.7 percent (2.3 percentage point reduction from 18 percent in in-kind areas, p = 0.134; column 2). As we focus our attention on households with lower PMT scores, we see (as expected) higher poverty rates in the in-kind areas, and substantially larger reductions in poverty in the voucher areas. For example, for households with PMT scores <= 10 at baseline – which approximately corresponds to the bottom 10 percent of the population (similar to the overall poverty rate in the entire sample), we find a reduction in poverty of 22 percent (5.2 percentage point reduction from a mean of 23.7 percent in voucher areas, p=0.02). For the very poorest – those with PMT scores 0-5 at baseline, we see a 24 percent reduction in the share of households in poverty (6.5 percentage points, p=0.012).[26] Appendix Table 15 shows similar patterns in the voucher program for both the poverty gap and the squared poverty gap.

In combination, the results here suggest that the improved targeting of assistance in the voucher program had a substantial impact: substantially more assistance to targeted households, particularly for the very poor, which in turn led to a substantial reduction in the share of these households below the poverty line.

B. Consumption choices

We now turn to understanding how the move from in-kind subsidies to vouchers affected the patterns of consumption choices. Recall that the in-kind subsidy program only provided rice, where the voucher program allowed for more flexibility by allowing households to also purchase eggs. The government made this change as it wanted to encourage protein consumption in addition to starches. Does this increased flexibility translate into changes in the patterns of consumption?

Before we turn to the experimental results, we first examine consumption patterns in the in-kind areas. Theory would predict that if households are already consuming more rice than they

[26] Since poverty is measured by per-capita consumption being below the relevant poverty line, these results are, mechanically, coming from increases in consumption for households near the poverty line. We also examine changes in *mean* consumption levels for each of these groups in Appendix Table 13, both in logs (Panel A) and levels (Panel B). While we do not see any changes in mean consumption level for either the entire PMT <= 30 or PMT > 30 samples, we do see increases in consumption when we zoom in on the poorest households. Households with PMT <= 10 have 3.8 percent higher mean per-capita consumption (p=0.104); households with PMT < 5 have 5.4 percent higher mean per-capita consumption (p=0.034). We find qualitatively similar results examining changes in CRRA consumption (assuming a coefficient of relative risk-aversion of 3), see Appendix Table 13 Panel C. We find no observable impact on the food insecurity index (Appendix Table 14).

would receive in the transfer, this would imply they were unconstrained by the fact that the in-kind transfer was limited to rice. As such, one would expect that the switch from the in-kind transfer to the more flexible voucher (where they can also choose to purchase eggs) would not affect consumption patterns. That is, under a standard consumption model, if a household was consuming more than 10kg of rice per month, it was doing so not because of the in-kind transfer of 10kg rice per month, but because it's optimal consumption of rice was above 10kg. As such, one would expect households to consume the same consumption bundle regardless of whether they received the transfers of rice in-kind of the more flexible vouchers.

Figure 4, Panel A plots out rice consumption in the in-kind areas, and shows that almost everyone is consuming more than 10 kgs of rice. Examining households with PMT score below than 30, we find similar patterns; in fact, only 3.3% of these households consume less than 10kgs of rice. Moreover, in practice the in-kind program did not distribute 10kg to beneficiaries – as discussed above, the typical beneficiary received only 3.9 kg of rice – and over 99 percent of households with PMT scores < 30 consumed more than this amount of rice. These patterns are similar among the poorest of the poor: for example, among households with PMT score of less than 5, we find that households consume about 35kgs of rice on average (Appendix Table 16). Thus, we would expect that the switch from in-kind to more flexible vouchers would not mechanically affect consumption patterns, as virtually all households are unconstrained even under the in-kind program.

Turning to our experimental results, we examine the effect of the conversion to the voucher program on both consumption of both subsidized rice and eggs (Table 3, Panel A), and *total* consumption of rice and eggs (Table 3, Panel B). Note that the total consumption measures come from a completely separate module in the SUSENAS which ask about consumption of the food product in question (as well as over 200 other consumption items) over the previous week from all sources. Meanwhile, information on the subsidized consumption come from questions about the Rastra and BPNT programs specifically. For eggs, we report the total egg protein consumption in grams, which is the Indonesian government's aggregated estimate totaling all different types of eggs consumed (e.g. farmed chicken eggs, free-range chicken eggs, duck eggs, quail eggs).

Looking first at the bundle of subsidized food households receive, we see no change in consumption of subsidized rice, either all households (Panel A, column 1) or those with PMT below 30 (Panel A, Column 2). We do, however, observe that targeted voucher households

consume more of their subsidy in the form of eggs. This is, of course, virtually zero in in-kind areas, but increases to about 33 grams per month of eggs protein for households with PMT scores <= 30 ($p < 0.001$), and by a much smaller amount but still positive for households with PMT scores > 30.

The key question is what happens to the patterns of *total* consumption, which we explore in Panel B. We find no impact on total rice consumption for either of these samples.[27] However, we do observe that eligible voucher households consume more total egg protein. Specifically, for households with PMT scores <= 30, we observe an increase in total egg protein of about 4.3 percent (9.3 grams, p-value 0.10). Combined with the estimates in Panel A, this implies that about 28 percent of the increase in subsidized egg protein consumed represented an increase in net consumption.

Appendix Table 17 replicates the egg consumption results, by different cuts of the PMT score, i.e. different poverty levels. Total egg protein consumption increases the most for poor; for example those with PMT score below 10 experience a 8.3 percent increase ($p= 0.056$) while those with PMT score below 5% experience a 11.51 percent ($p = 0.035$). The 'stickiness' of the transfer increases as we look at poorer households – for households with PMT <= 10, the change in total egg consumption is about 45 percent of the change in subsidized egg protein. For the very poorest households – those with PMT < 5 – fully 59 percent of the increase in subsidized egg protein represents a change in net consumption. These results imply that flexibility with the vouchers -- combined with the labelling for eggs -- may substantially affect real consumption decisions.

An important question is whether the change in egg consumption represents an income effect for the set of households who received the larger subsidy, as opposed to some specific 'stickiness' coming from the set of goods included in the voucher. To examine this, we explore consumption of a variety other food items, as well as "temptation goods" such as cigarette and alcohol (Appendix Table 18). We do not observe systematic increases in other food consumption (p-value of the joint test=0.232); if anything, there is a small reduction in salt consumption. We find no observable change in cigarette consumption nor alcohol consumption. This suggests the results we find on egg consumption really are coming from the fact that eggs, and only eggs, were

[27] Appendix Table 16 replicates the rice consumption results by different cuts of PMT score, i.e. different poverty scores. We find no observable difference in rice consumption even in the poorest groups (i.e. PMT score less than 10 or less than 5).

added to the set of goods that the transfer could be used on, rather than a more general income effect.

C. Prices

One benefit of this unique policy experiment is that given that the unit of randomization was at a high geographic level – districts, with an average population of about 500,000 – we can measure the general equilibrium effects of the switch to vouchers on overall prices in the district. Table 4 presents these results. We obtain rice prices from the SUSENAS consumption module; we focus on prices reported by households not in the UDB and thus not eligible for the subsidy programs.

In column 1, we begin by looking at the overall impact of the voucher program on the price of rice. Standard price theory predicts that reducing the subsidized rice supply and instead increasing demand for private rice in shops could increase the price of rice more broadly, though of course the magnitude of this would depend on the elasticity of supply of ice. While the coefficient is indeed positive, it is small in magnitude and not significant (p-value of 0.296).

We, then turn to three forms of heterogeneity as laid out by theory. First, theory would predict that the price effect would be smaller in rice producing districts, i.e. areas that had a large supply of locally available rice; as shown in Column 2, however, we find no observable differential effect of the voucher program based on rice production. Second, theory may predict that areas where there the subsidized rice consumption is a large share of total rice consumed in the district, prices may adjust more. However, in column 3, we find no observable differential effect of treatment in districts with a larger share of baseline subsidized rice.[28]

Finally, theory would predict that areas that are more remote may have larger price adjustments, as they are more isolated markets and households have less options available to them to purchase products. We explore three measures of village level remoteness: whether your village has access to an asphalt road in Column 4, whether the road is always passable in Column 5, whether your village is above median in the distribution of time to the district capital in Column 6, and whether your village is above median in the distribution of time to subdistrict capital in Column 7. While the signs of the coefficients go in the predicted directions, we do not observe

[28] We also examine these heterogeneity measures using alternative specifications, either examining continuous measures (Appendix Table 19) or whether the area falls above the 75th percentile (Appendix Table 20). We observe similar conclusions.

impact (the p-value joint test across all four distance measures * treatment is 0.691).[29] One possible reason why we do not observe substantial price effects here (in contrast to Cunha et al (2019)) is that rice markets in Indonesia are reasonably well integrated, leading to elastic supply of rice at the local level.

D. Leakage and Administrative Program Costs

Finally, we explore the impact of the program on both leakage rates and the administrative costs of running the program.

A key criticism of the administration of in-kind programs is that complexity of the food distribution makes it hard to monitor, and so much of the food can "fall of the truck" (Banerjee, et al 2018). In Table 5, we examine whether the vouchers reduced such leakage, by comparing the actual subsidy received by households as measured by the SUSENAS survey with the intended subsidy for each district.

We compute the fraction of subsidy received by households in three ways. In column 1, we calculate subsidy as the sum of reported in-kind and voucher values received. In column 2, we compute value the voucher subsidy based on the amount of eggs or rice purchased times the market price of rice or eggs. If local agents were inflating prices charged, so that voucher households were receiving less 'real' assistance than the nominal amount of subsidy would suggest, this would not be counted as leakage in column 1, but would show up as a form of leakage in column 2. We do this in case the prices paid at local agents are inflated. Finally, in column 3, we adjust the market price of rice in BPNT to reflect the higher quality rice bought to allow for quality weighted comparisons.[30]

In all cases, we compare this to the value of the intended subsidy received. We compute intended subsidy for the in-kind districts by multiplying the official number of targeted beneficiaries by 10kg of rice and then by the official procurement price of the Rastra Rice. To compute the comparable measure for the voucher districts, we multiplied the number of official targeted beneficiaries by the Rp 110,000 payment.

[29] As Appendix Table 20 shows, we observe a positive and significant effect if your village falls above the 75th percentile of the distribution of time to district capital (p-value of 0.029), suggesting that there may be some effect on price in areas that are further isolated.

[30] To compute the quality adjusted price, we take the point estimate from the subsidized rice quality results and transform this into a z-score. Then we take the same z-score in the rice price distribution of each district-urban area.

Overall leakage by this measure is high, but since it is possible that the level of this metric is affected by aggregate reporting issues (Olken 2007), what is more relevant is whether this leakage metric *changes* in response to the switch, holding measurement constant. We find that it does not. As shown in Column 1, only about 59 percent of the overall intended subsidy reaches targeted households, and the conversion to the voucher program has no impact on the intended subsidy. If anything, as shown in Column 2, allowing prices to adjust by area, the voucher program led to a decrease in total subsidy received, significant at the 5 percent level. Finally, adjusting for quality of rice received, we also observed negative, but insignificant impacts of the conversion on leakages.[31]

We also examine the impact of the switch of from in-kind transfers to vouchers on the administrative costs of running the program. We start by estimating the program costs for delivery of in-kind benefits. We obtain the operating costs for the in-kind program from BULOG's annual report. Note, however, the that national government only facilitates delivery to the district or subdistrict capital, and local governments are required to cover the costs of their own pick-up and delivery; we obtain an estimate of these costs through a survey of the distributers (Banerjee, et al, 2019). These costs are summarized in Appendix Table 22. On net, the administrative program costs for the in-kind delivery are about 4.1 percent of the total benefits.

The administrative costs of the voucher program are much less than the in-kind program. There are two main costs. First, there is a cost of printing the debit card (Rp. 12,500 per person), which we assume lasts three years since targeting is conducted every three years. Second, there is the monthly rental cost of the EDC machines for the agents. Assuming that the agents only process vouchers –and did not use the machines for other financial transactions—Appendix Table 22 shows that the total administrative costs would be 2.16 percent of the benefits dispersed, or about half the size of the administrative costs incurred for in-kind benefits. Note, however, that a large fraction of the agents existed prior to the voucher program, and paid rent on the machines for other transitions, and so the marginal cost for them of processing the vouchers is minimal; in fact, Banerjee et al (2020) experimentally shows that about 77 percent of agents already existed prior to the program. Under the assumption that all existing agents would have had the machines in any

[31] It is possible that overall leakages could fall in districts that were better prepared to convert from in-kind to vouchers (e.g. had a more established agent network, etc). In Appendix Table 21, we therefore estimate the effect of treatment on leakages by whether the district scored higher or lower on the baseline readiness index. We do not find observable evidence of heterogeneity by baseline readiness.

case, but all new agents use the machines only for the voucher program, the administrative costs would be 0.74 percent of the benefits disbursed or about 18 percent of administrative costs for the in-kind benefits.

V. Conclusion

In this paper, we examine the results from an at-scale experiment to study the switch from an in-kind social assistance program to a similar electronic voucher-based program. We find that, while the programs are broadly similar, they led to dramatically different differences in outcomes through the way the programs were administered in practice. Specifically, households with low baseline predicted poverty scores – i.e. those households who were the intended beneficiaries of both programs – received 45 percent more assistance in voucher areas compared to in-kind areas. By contrast, benefits in the in-kind program were diffused much more broadly throughout the population.

The voucher program's substantially higher concentration of benefits among the poor led to a reduction in poverty. For those households in the bottom 15 percent at baseline, the poverty rate fell by 20 percent. This occurred despite the fact that the administrative costs of operating the voucher-based program were less than half that of the in-kind program.

We also find evidence that the flexibility of the voucher program led to an increase in consumption of egg-based proteins. This occurred despite the fact that households in the in-kind program were unconstrained; that is, since virtually all households consume more rice anyway than they receive in the program, basic price-theory suggests that these households could have used the transfer amounts for rice and substituted other purchases towards eggs had they chosen to do so. The fact that the vouchers allowed only rice and eggs, and it was specifically egg protein consumption that increased, suggests that there is 'stickiness' in the consumption bundles allowed in the voucher program. This type of stickiness is reminiscent of the findings of Hastings and Shapiro 2018, who find the related result that the overall marginal propensity to spend SNAP benefits on food is higher than the marginal propensity to spend cash transfers on food. We find no changes in aggregate prices or in leakage.

On net, the results here suggest an important additional dimension for how to think about vouchers vs. in-kind programs. While the economics literature has focused primarily on outcomes grounded in price-theory – such as changes in the consumption bundle caused by either the

mechanical budget constraint or mental accounting, or aggregate price changes– our results here suggest that the differences in how these programs are *administered* may be a far more important determinant of their relative effectiveness, at least in settings with imperfect administrative capacity.

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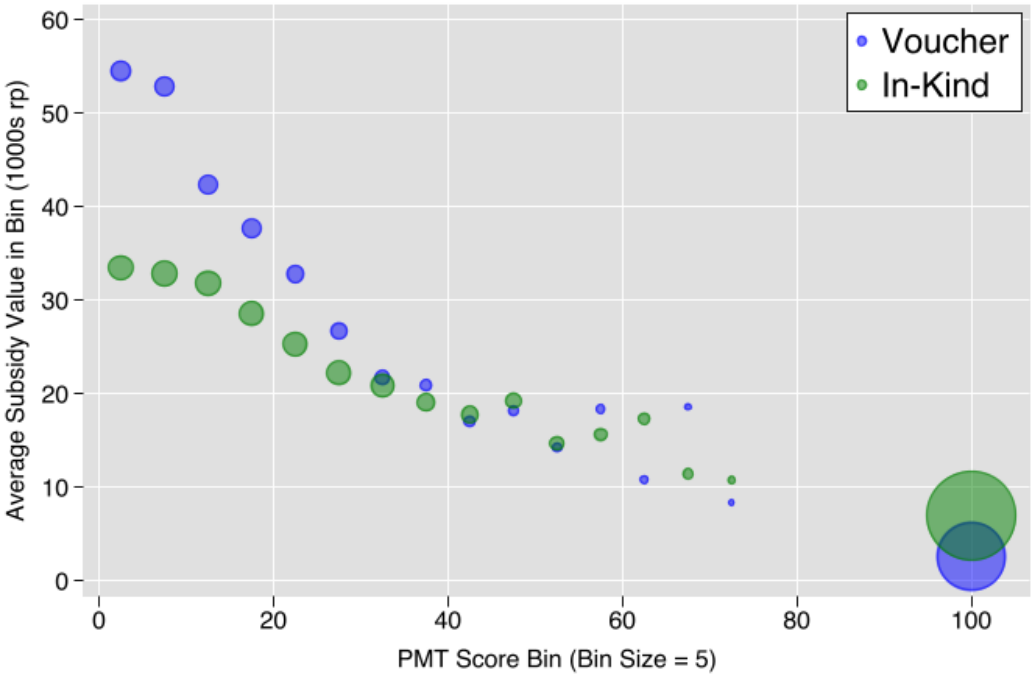
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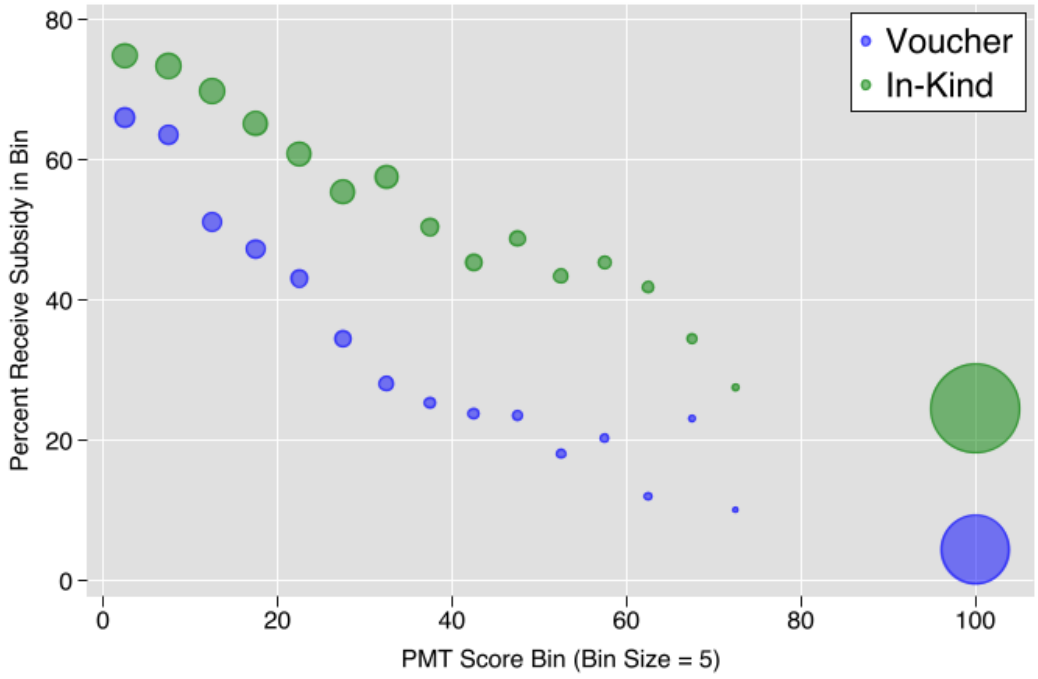
1 Tables and Figures

Figure 1: Relationship between PMT Score and Subsidy Received

Panel A: Total Subsidy (1000s rp)



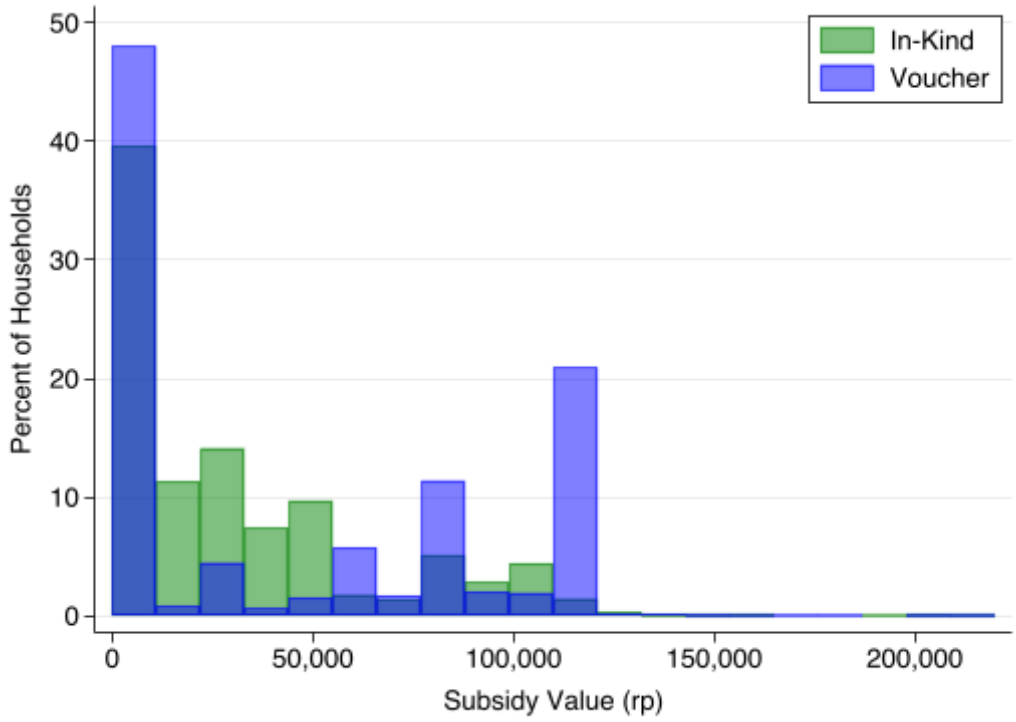
Panel B: Subsidy Receipt



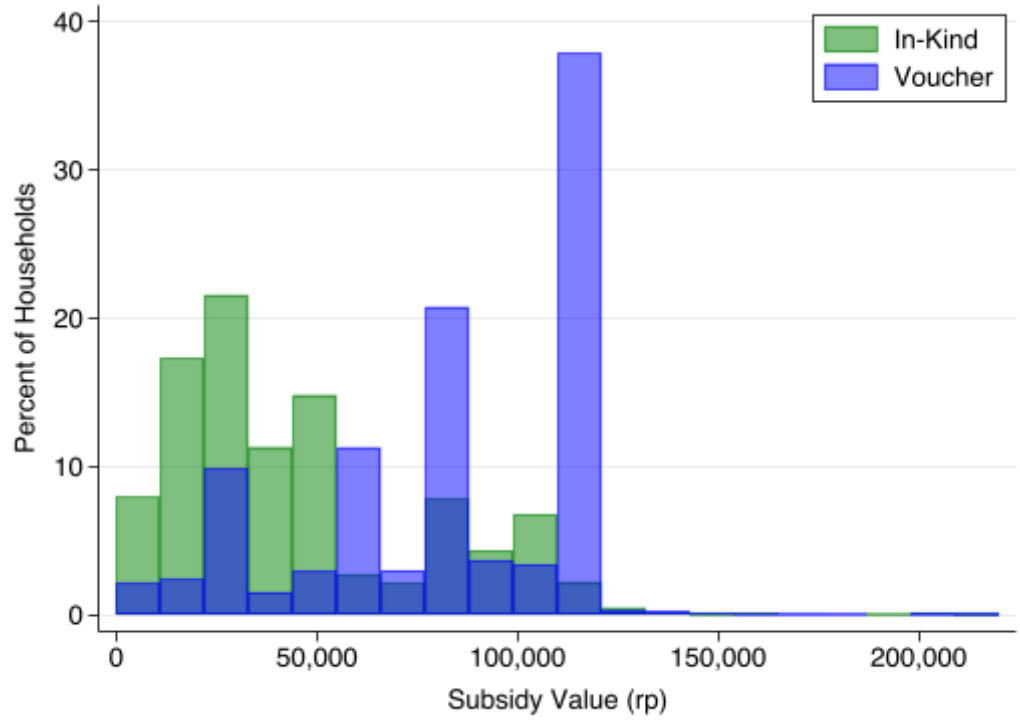
Note: These figures graph the relationship between a household's PMT score and, respectively, their total subsidy received (Panel A) and an indicator variable for received subsidy (Panel B), by treatment. PMT scores are binned in groups of 5, with those who have no PMT score grouped with those with a score of 100. Markers are scaled by the number of households in each bin. Data on outcomes are from the Match 2019 SUSENAS, while PMT data are from the Unified Targeting Data Base.

Figure 3: Concentration of BPNT Subsidy

Panel A: Households with PMT score <= 30

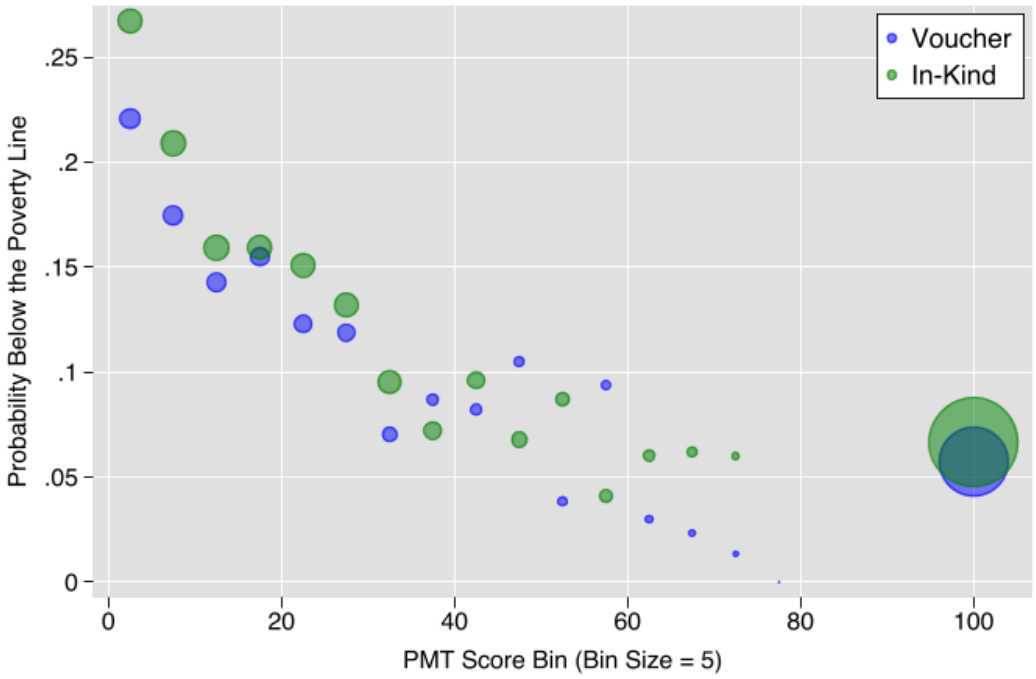


Panel B: Recipient households with PMT score <= 30



Note: These figures graph the distribution of subsidy received, by treatment and PMT score cutoff. The data are from March 2019 SUSENAS. The sample size in Panel A is 16,316, while it is 9,850 in Panel B.

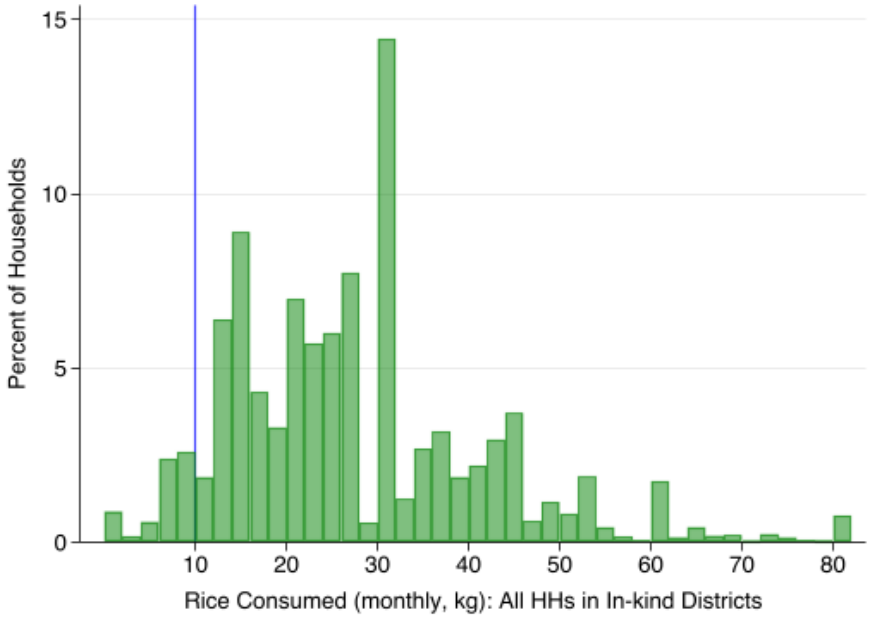
Figure 4: Relationship Between PMT Score and Poverty Status



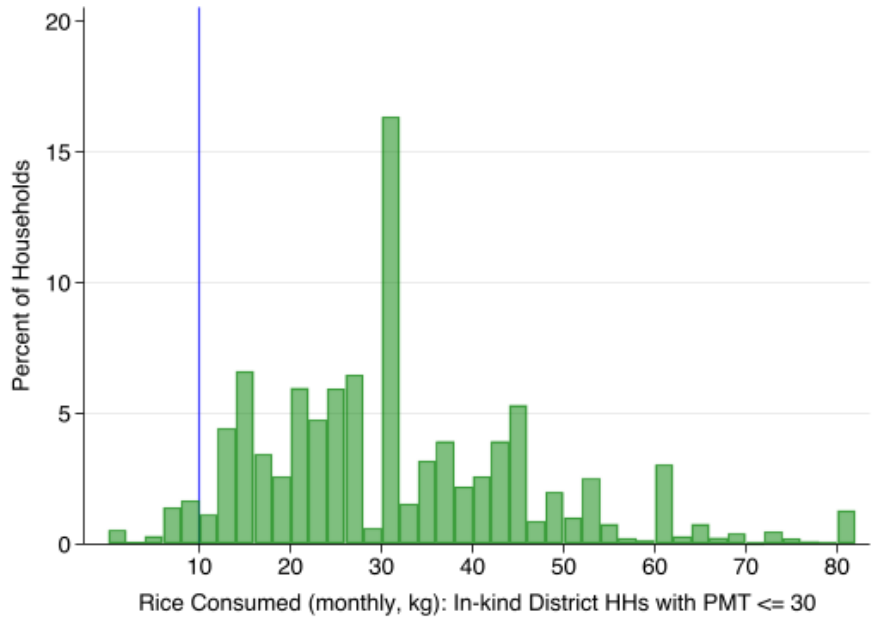
Note: This graph provides the relationship between whether the household is below the poverty line and PMT score, by treatment status. PMT scores are binned in groups of 5, with those who have no PMT score grouped with those with a score of 100. Markers are scaled by the number of households in each bin. Data on outcomes are from the Match 2019 SUSENAS, while PMT data are from the Unified Targeting Data Base.

Figure 5: Total Monthly Rice Consumption (kg)

Panel A: All households in in-kind areas



Panel B: Households in in-kind areas with PMT score <= 30



Note: This figure graphs the distribution of total rice consumed in kilograms in the in-kind districts for all households (Panel A) and those who have a PMT score that is below or equal to 30. The data are from March 2019 SUSENAS. The sample size in Panel A is 39,612, while it is 9,763 in Panel B.

Table 1: Experimental Difference between Voucher and In-kind Districts on Subsidy Outcomes

	Total Subsidy (rp)			Receive Subsidy			Recipients Only	
							Total Subsidy (rp)	Rice Quality
	All	PMT <= 30	PMT > 30	All	PMT <= 30	PMT > 30	All	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Voucher	1304.749	13234.952	-2598.768	-0.134	-0.105	-0.145	30957.183	0.203
	(617.738)	(1915.934)	(564.894)	(0.019)	(0.021)	(0.020)	(3164.224)	(0.020)
	[0.087]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Observations	66494	16327	49566	66496	16329	49566	19355	19260
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	14456.314	29200.535	9161.727	0.393	0.669	0.293	36918.120	0.630

Note: This table experimentally estimates the difference in subsidy outcomes for voucher versus the in-kind districts. Total Subsidy (rp) is the sum of Rastra and BPNT subsidy values, while received subsidy is an indicator variable for receiving any amount of subsidy. In Columns 2 and 3 we present the results on total subsidy disaggregated by whether the household is targeted (a PMT score below or equal to 30) or not targeted (score above 30); we do the same in Columns 3 and 4 for received subsidy. The quality of rice measure is standardized between 0 and 1, where 1 is the highest quality. For continuous outcome variables, we drop any value greater than 12 standard deviations from the mean. The outcome data come from March 2019 SUSENAS; the PMT data come from the Unified Targeting database. We used a double LASSO to choose the control variables (all potential variables in the lasso are listed in Appendix B). Standard errors are clustered at the district (kabupaten) level and displayed in parentheses. Randomization inference p-values are from 1,000 permutations of the treatment assignments.

Table 2: Experimental Difference between Voucher and In-kind Districts Being Below the Poverty Line

	All	PMT <= 30	PMT <= 25	PMT <= 20	PMT <= 15	PMT <= 10	PMT <= 5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Voucher	-0.010	-0.023	-0.025	-0.034	-0.043	-0.052	-0.065
	(0.008)	(0.015)	(0.016)	(0.017)	(0.018)	(0.020)	(0.024)
	[0.199]	[0.134]	[0.166]	[0.078]	[0.028]	[0.020]	[0.012]
Observations	66496	16329	13707	11072	8307	5529	2788
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	0.098	0.180	0.189	0.198	0.210	0.237	0.267

Note: This table explores the impact of being below the consumption poverty line, by PMT groupings. The outcome variable, below poverty line, is an indicator for whether a household is below the poverty line in its province by urban/rural area, as measured by per capita consumption. The outcome data come from March 2019 SUSENAS; the PMT data come from the Unified Targeting database. We used a double LASSO to choose the control variables (all potential variables in the lasso are listed in Appendix B). Standard errors are clustered at the district (kabupaten) level and displayed in parentheses. Randomization inference p-values are from 1,000 permutations of the treatment assignments.

Table 3: Experimental Difference between Voucher and In-kind Districts on Food Consumption

Panel A: Subsidized Food Consumption

	Subsidized Rice (kg)			Subsidized Egg Protein (g)		
	All	PMT <= 30	PMT > 30	All	PMT <= 30	PMT > 30
	(1)	(2)	(3)	(4)	(5)	(6)
Voucher	-0.300	0.062	-0.425	10.932	32.719	3.362
	(0.066)	(0.205)	(0.059)	(1.534)	(4.648)	(0.463)
	[0.002]	[0.773]	[0.000]	[0.000]	[0.000]	[0.000]
Observations	66495	16328	49566	66423	16270	49552
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	1.494	2.987	0.957	0.140	0.484	0.015

Panel B: Total Food Consumption

	Total Rice (kg)			Total Egg Protein (g)		
	All	PMT <= 30	PMT > 30	All	PMT <= 30	PMT > 30
	(1)	(2)	(3)	(4)	(5)	(6)
Voucher	-0.077	-0.397	0.074	2.658	9.519	-0.562
	(0.305)	(0.471)	(0.294)	(3.592)	(4.764)	(4.037)
	[0.838]	[0.501]	[0.852]	[0.486]	[0.090]	[0.902]
Observations	66229	16293	49335	66216	16291	49324
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	27.742	31.643	26.302	227.323	214.040	231.896

Note: This table examines consumption of rice (Columns 1 -3) and egg protein from all types of eggs (Columns 4 -6). Panel A is any consumption from Rastra or BPNT, while Panel B is total consumption. For continuous outcome variables, we drop any value greater than 12 standard deviations from the mean. The outcome data come from March 2019 SUSENAS; the PMT data come from the Unified Targeting database. We used a double LASSO to choose the control variables (all potential variables in the lasso are listed in Appendix B). Standard errors are clustered at the district (kabupaten) level and displayed in parentheses. Randomization inference p-values are from 1,000 permutations of the treatment assignments.

Table 4: Experimental Difference between Voucher and In-kind Districts on Price

				Measures of Isolation			
	Main Effect Only (1)	Above Med. Rice Area (2)	Above Med. Supply Shock (3)	Asphalt Road (4)	Road Always Passable (5)	Above Med. Time to District Capital (6)	Above Med. Time to Subdistrict Capital (7)
Voucher	140.536	47.892	77.643	162.536	207.561	58.970	157.657
	(135.230)	(209.150)	(180.112)	(118.115)	(136.254)	(150.040)	(138.702)
	[0.296]	[0.816]	[0.673]	[0.184]	[0.121]	[0.689]	[0.268]
Voucher × [Variable]		342.196	180.141	-114.831	-121.072	155.648	54.750
		(276.173)	(267.890)	(166.264)	(103.486)	(119.427)	(80.871)
		[0.250]	[0.541]	[0.537]	[0.242]	[0.226]	[0.546]
Observations	32343	31185	32343	32343	32343	32334	32334
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Main Effect Included	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	9478.508	9458.047	9478.508	9478.508	9478.508	9478.508	9478.508
[Variable] Mean		0.406	0.540	0.270	0.531	0.489	0.466

Note: This table examines the impact of the vouchers on market rice prices. Data are from the March 2019 SUSENAS, taken from households that are not in the Unified Data Base. Measures of isolation data come from the 2018 PODES. Data on rice cultivation area per capita come from the 2003 Agricultural Census. Rice Area per capita is calculated as the total area under rice cultivation in the district divided by the population. Above median supply shock indicates whether the district has above median subsidized rice as a fraction of total rice consumption in the district. Asphalt road indicates whether the roads leading to the main population center in the village are paved. Road always passable indicates whether these roads are passable the whole year. Above median time to district capital indicates whether the village's distance to the nearest district capital is above the median. Above median time to subdistrict capital indicates the same, but for the nearest subdistrict capital. Standard errors are clustered at the district (kabupaten) level and displayed in parentheses. Randomization inference p-values are from 1,000 permutations of the treatment assignments. The randomized inference p-value for the joint test for the supply variables (columns 2 and 3) is 0.587; The randomized inference p-value for the joint test for the distance variables (columns 4 to 7) is 0.691.

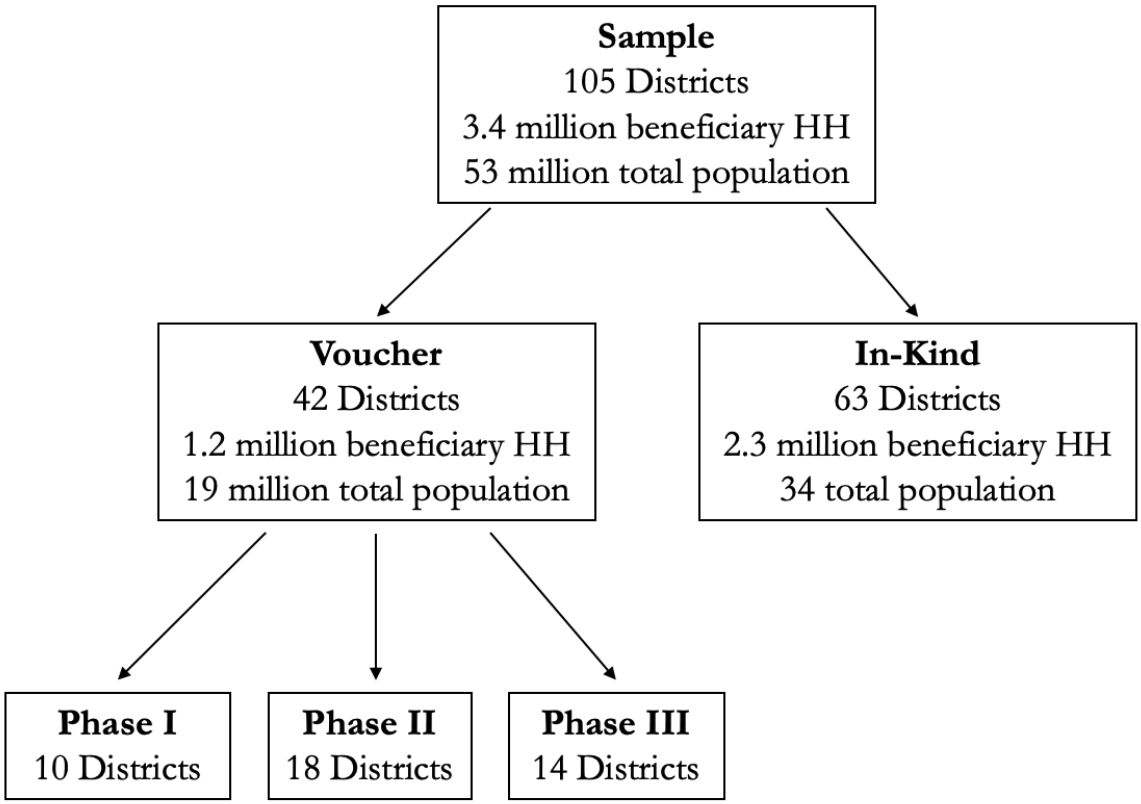
Table 5: Experimental Difference between Voucher and In-kind Districts on Leakage

	Subsidy Received / Intended Subsidy (1)	Subsidy Received (Market Prices) / Intended Subsidy (2)	Subsidy Received (Quality-Adjusted) / Intended Subsidy (3)
Voucher	-0.020	-0.059	-0.032
	(0.031)	(0.029)	(0.032)
	[0.533]	[0.055]	[0.351]
Observations	105	105	105
Stratum FE	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes
DV Mean (Control)	0.587	0.586	0.587

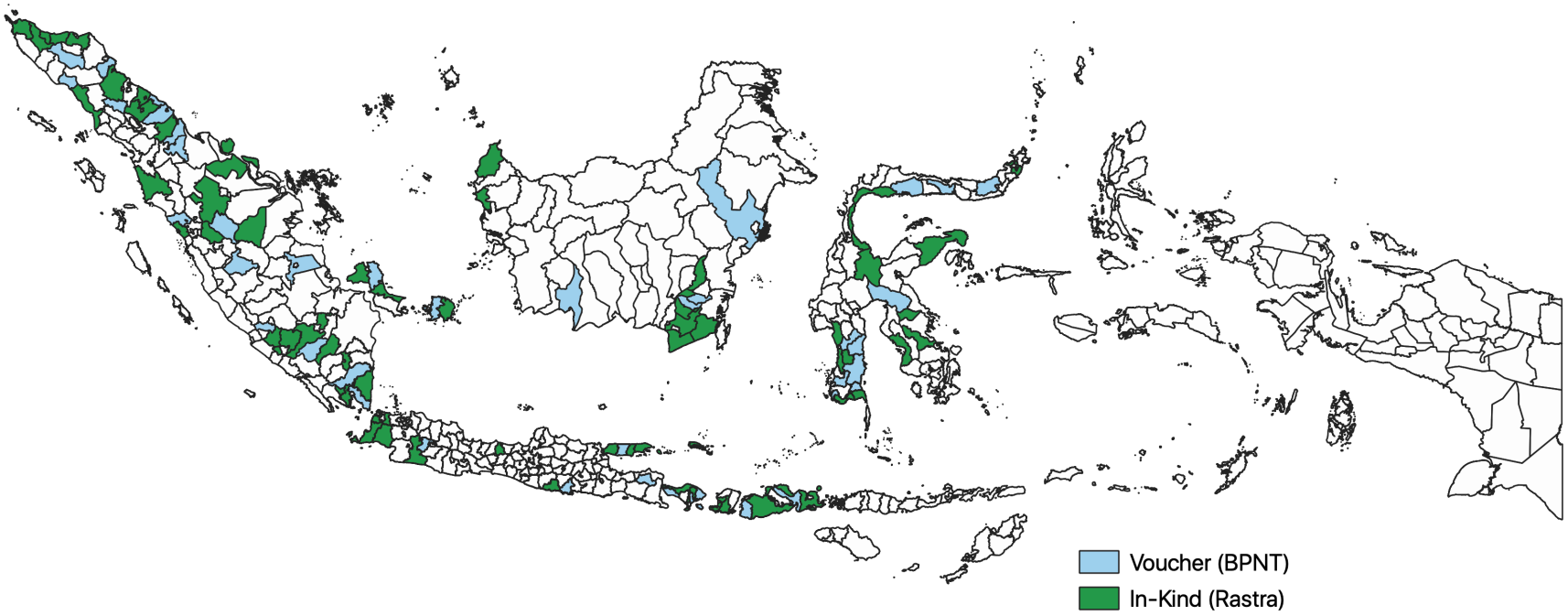
Note: In this table, we examine the subsidy received relative to the intended subsidy to disburse, by treatment status. The data come from the March 2019 SUSENAS and from administrative data. To compute the intended subsidy in an in-kind district, we multiple the number of beneficiaries in the district by the 10kg rice disbursement and the official procurement price of Rastra rice; in the voucher districts, we multiple the BPNT beneficiaries in the district by the disbursement amount (Rph 110,000). We calculate subsidy received in three ways: in Column 1, it is the sum of the value of any program received; in Column 2, we adjust the voucher disbursement by the market price of rice in the area; in Column 3, we adjust the voucher disbursement by the market price of rice of higher quality rice. We used a double LASSO to choose the control variables (all potential variables in the lasso are listed in Appendix B). Standard errors are clustered at the district (kabupaten) level and displayed in parentheses. Randomization inference p-values are from 1,000 permutations of the treatment assignments.

A Appendix

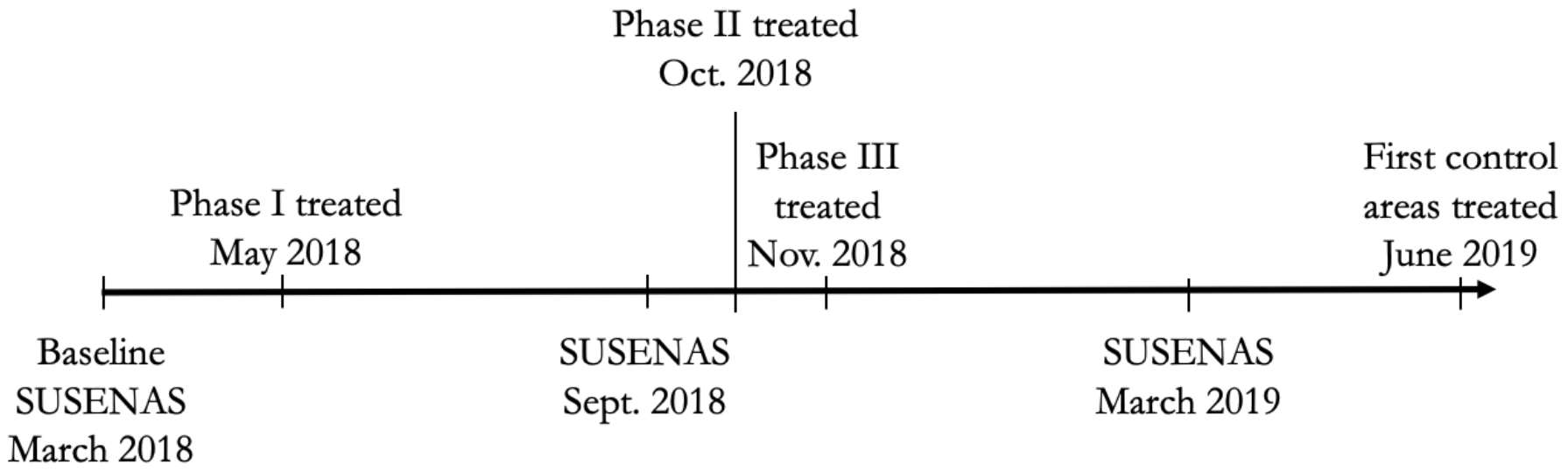
Appendix Figure 1: Randomization Design



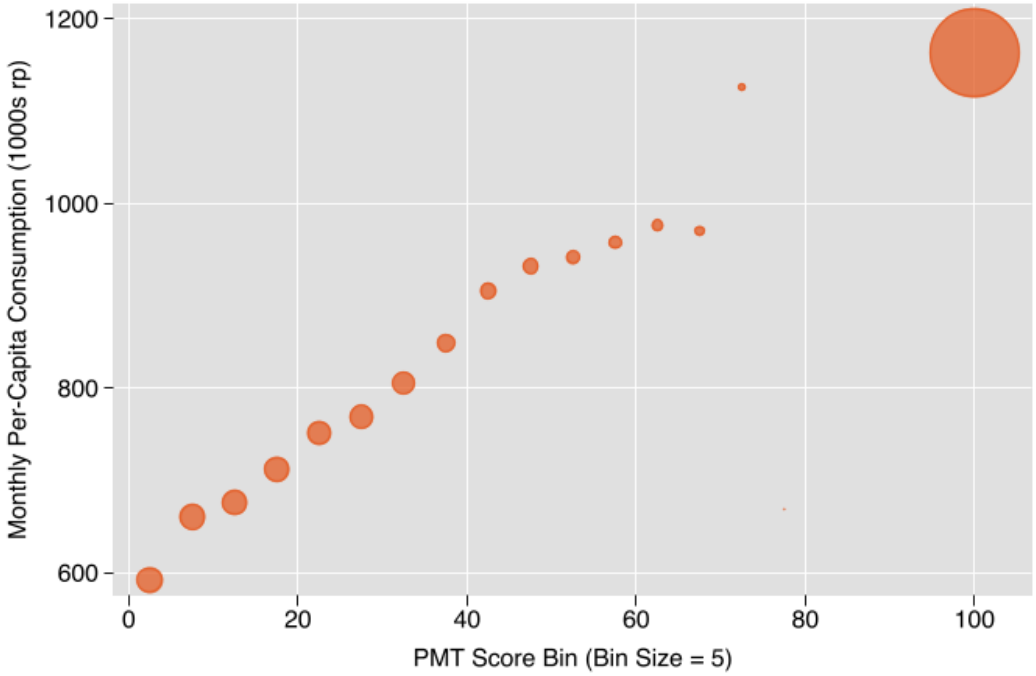
Appendix Figure 2: Map of Experimental Districts



Appendix Figure 3: Timeline



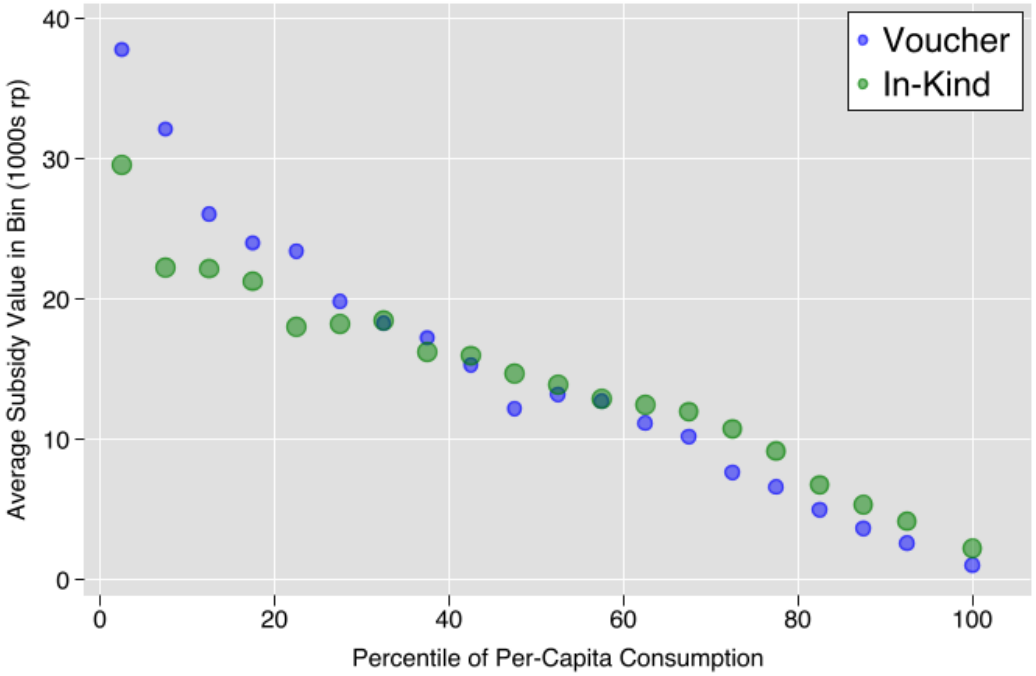
Appendix Figure 4: Relationship between PMT score at Baseline and Per Capita Consumption



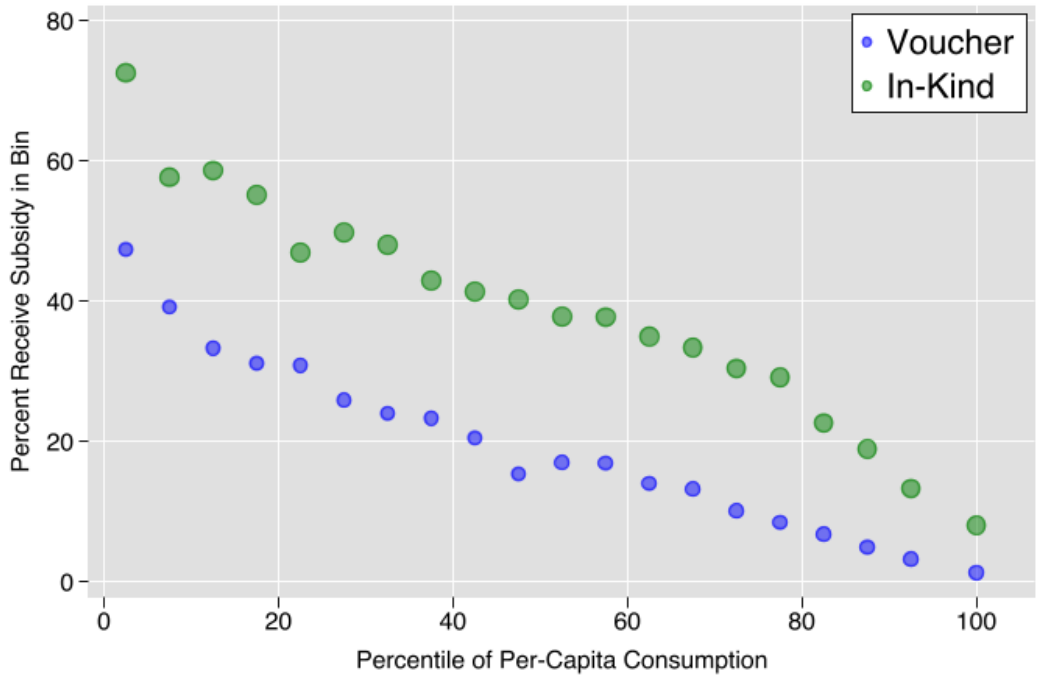
Note: This graph provides the relationship between per capita consumption in 1000s of Rph and PMT score at baseline. PMT scores are binned in groups of 5, with those who have no PMT score are grouped with those with a score of 100. Markers scaled by number of households in each bin. Data on outcomes are from the Match 2018 SUSENAS, while PMT data are from the Unified Targeting Data Base.

Appendix Figure 5: Relationship between Percentile of Consumption and Subsidy Received

Panel A: Total Subsidy (1000s rp)



Panel B: Subsidy Receipt



Notes: This figure replicates Figure 1, plotting bins of per-capita consumption on the x-axis instead of bins of PMT score. Data are from the March 2019 SUSENAS. See Figure 1 for details.

Appendix Figure 6: Picture of BPNT Card



Appendix Table 1: Baseline Balance Check

Variable	Control Mean (1)	Treated Mean (2)	Difference (3)
Log Monthly Per-Capita Consumption	13.658	13.678	0.007
			(0.040)
			[0.845]
Daily Per-Capita Calorie Consumption	2265.816	2275.096	-15.246
			(31.605)
			[0.631]
Receive Rastra	0.410	0.330	-0.044
			(0.026)
			[0.144]
HH Has 5.5kg LPG Gas	0.095	0.117	0.036
			(0.016)
			[0.038]
HH Owns Refrigerator	0.552	0.570	0.018
			(0.024)
			[0.515]
HH Has Air Conditioning	0.048	0.051	0.003
			(0.008)
			[0.702]
HH Has Landline	0.006	0.009	0.003
			(0.002)
			[0.210]
HH Owns Computer	0.148	0.162	0.013
			(0.011)
			[0.271]
HH Owns Car	0.095	0.111	0.008
			(0.008)
			[0.284]
HH Owns Flatscreen TV	0.093	0.104	0.010
			(0.010)
			[0.277]
HH Owns Land	0.791	0.747	-0.035
			(0.028)
			[0.125]
N	105	105	105
F-statistic of joint orthogonality test			1.426
Conventional p-value			0.178
Randomization inference p-value			0.384

Note: This table provides a baseline balance check. All data come from the March 2018 SUSENAS, with district-level means computed using SUSENAS household weights. In Column 3, we compute the difference in means conditional on strata fixed effects. Robust standard errors are in parentheses. Randomization inference p-values—in brackets—are from 1,000 permutations of the treatment assignments.

Appendix Table 2: Baseline Summary Statistics, by PMT score group

Variable	PMT <= 30 (1)	PMT > 30 (2)	p-value of difference (3)
Per Capita Consumption (rp 1000s)	689.613 (459.172)	1116.125 (872.613)	0.000
Poor Wall Material	0.150 (0.357)	0.070 (0.255)	0.000
Poor Floor Material	0.736 (0.441)	0.519 (0.500)	0.000
Poor Roof Material	0.029 (0.168)	0.019 (0.137)	0.000
Own Flat-Screen TV	0.024 (0.152)	0.116 (0.320)	0.000
Own Computer	0.036 (0.187)	0.178 (0.383)	0.000
Own Refrigerator	0.321 (0.467)	0.594 (0.491)	0.000
HH Head Higest Education Level is Post-Secondary	0.010 (0.098)	0.085 (0.280)	0.000
HH Head Highest Education Level is High School	0.099 (0.298)	0.250 (0.433)	0.000
HH Head Highest Education Level is Junior High	0.145 (0.352)	0.169 (0.375)	0.000
HH Head Highest Education Level is Primary	0.647 (0.478)	0.446 (0.497)	0.000
HH Head Does Not Have Primary School Education	0.100 (0.300)	0.050 (0.218)	0.000
# HH Members	4.197 (1.726)	3.729 (1.617)	0.000
# Children in HH	1.506 (1.202)	1.217 (1.095)	0.000
Below Poverty Line	0.195 (0.396)	0.066 (0.249)	0.000
HH Receives Rastra (Self-Report)	0.714 (0.452)	0.334 (0.472)	0.000
HH Eligible for Rastra in 2017 (UDB)	0.754 (0.431)	0.057 (0.232)	0.000
N	16348	48381	

Note: Standard deviations in parentheses. Data come from the March 2018 SUSENAS, with the exception of the official indicator for whether the household is eligible for Raskin in 2017 that comes from the Unified Data Base. All means and standard deviations for SUSENAS variables are calculated using the SUSENAS household weights.

Appendix Table 3: Replication of Table 1 for September 2018

	Total Subsidy (rp)			Receive Subsidy			Recipients Only: Total Subsidy (rp)
	All	PMT <= 30	PMT > 30	All	PMT <= 30	PMT > 30	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Voucher	1645.693	13311.554	-2789.336	-0.151	-0.151	-0.139	38418.390
	(1526.190)	(3102.830)	(1080.264)	(0.030)	(0.033)	(0.029)	(5878.164)
	[0.232]	[0.002]	[0.050]	[0.000]	[0.000]	[0.000]	[0.000]
Observations	16428	3998	12294	16432	3999	12296	5765
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	16597.507	34568.698	10284.263	0.406	0.726	0.294	41098.141

Note: This table replicates Table 1 for the September 2018 SUSENAS. See Table 1 for details.

Appendix Table 4: Replication of Table 1 for Pooled SUSENAS

	Total Subsidy (rp)			Receive Subsidy			Recipients Only: Total Subsidy (rp)
	All	PMT <= 30	PMT > 30	All	PMT <= 30	PMT > 30	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Voucher	1296.333	13788.275	-2293.981	-0.136	-0.106	-0.141	31924.802
	(617.504)	(1762.912)	(551.095)	(0.019)	(0.020)	(0.020)	(3020.496)
	[0.076]	[0.000]	[0.002]	[0.000]	[0.000]	[0.000]	[0.000]
Observations	82922	20325	61860	82928	20328	61862	25120
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	15709.936	32319.518	9821.281	0.401	0.702	0.294	39399.904

Note: This table replicates Table 1, pooling the September 2018 and March 2019 SUSENAS. See Table 1 for details.

Appendix Table 5: Replication of Table 1, including only the Strata FE and the Baseline Dependent Variable

	Total Subsidy (rp)			Receive Subsidy			Recipients Only: Total Subsidy (rp)	Recipients Only: Rice Quality
	All	PMT <= 30	PMT > 30	All	PMT <= 30	PMT > 30	All	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Voucher	1330.235	13834.341	-3495.576	-0.127	-0.103	-0.149	35432.229	0.234
	(742.828)	(1766.575)	(613.037)	(0.022)	(0.027)	(0.022)	(3479.809)	(0.020)
	[0.145]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]
Observations	66494	16327	49566	66496	16329	49566	19355	19260
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mar. 2018 DV	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	No	No	No	No	No	No	No	No
DV Mean (Control)	14456.314	29200.535	9161.727	0.393	0.669	0.293	36918.120	0.630

Note: This table replicates Table 1 without the LASSO chosen control variables. We only include the strata fixed effects and the baseline of the dependent variable from the March 2018 SUSENAS. As the SUSENAS is a repeated cross-section, we use the average of the dependent variable at the district urban-rural level for the baseline value. See Table 1 for additional details.

Appendix Table 6: Replication of Table 1, dropping the HOLDOUT sample

	Total Subsidy (rp)			Receive Subsidy			Recipients Only	
							Total Subsidy (rp)	Rice Quality
	All	PMT <= 30	PMT > 30	All	PMT <= 30	PMT > 30	All	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Voucher	1412.521	13551.140	-2968.675	-0.148	-0.108	-0.162	31857.163	0.208
	(702.016)	(2066.531)	(627.763)	(0.020)	(0.023)	(0.021)	(3451.003)	(0.020)
	[0.106]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Observations	54857	14142	40171	54859	14144	40171	17505	17451
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	14563.986	29068.994	9237.732	0.400	0.671	0.300	36491.540	0.629

Note: This table replicates Table 1 dropping the HOLDOUT sample. See Table 1 for additional details.

Appendix Table 7: Replication of Table 1, winsorized at the 0.5th and 99.5th percentile

	Total Subsidy (rp)			Recipients Only: Total Subsidy (rp)
	All	PMT <= 30	PMT > 30	All
	(1)	(2)	(3)	(4)
Voucher	1364.387	13341.041	-2581.077	30944.225
	(614.135)	(1912.012)	(550.537)	(3127.384)
	[0.076]	[0.000]	[0.001]	[0.000]
Observations	66496	16329	49566	19356
Stratum FE	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes
DV Mean (Control)	14302.455	28916.967	9049.591	36635.563

Note: This table replicates Table 1 for the continuous outcome variables, winsorizing them at the 0.5th and 99.5th percentile. See Table 1 for additional details.

Appendix Table 8: Consumption by BPNT Receipt in Voucher Districts

	Per-Capita Consumption Minus Subsidy			Total Per-Capita Consumption		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All						
Receive BPNT	-497415.3	-435521.9	-211337.8	-472847.5	-410540.5	-186209.8
	(23628.3)	(27180.7)	(16929.8)	(23442.9)	(26654.3)	(16470.4)
Observations	25918	25918	25918	25918	25918	25918
DV Mean (Non-BPNT HHs)	1149537	1149537	1149537	1149783	1149783	1149783
District FE	No	Yes	Yes	No	Yes	Yes
PMT Score FE	No	No	Yes	No	No	Yes
Panel B: PMT <= 30						
Receive BPNT	-169492.5	-169746.9	-156905.9	-146347.2	-145898.0	-132667.0
	(16060.9)	(15881.2)	(16225.1)	(15892.6)	(15532.2)	(15899.1)
Observations	6402	6402	6402	6402	6402	6402
DV Mean (Non-BPNT HHs)	790535	790535	790535	791415	791415	791415
District FE	No	Yes	Yes	No	Yes	Yes
PMT Score FE	No	No	Yes	No	No	Yes

Note: Columns 1-3 present results for monthly consumption per-capita minus BPNT value received per-capita, while columns 4-6 present results for monthly consumption per-capita including BPNT value received. Standard errors are displayed in parentheses and clustered at the kabupaten level.

Appendix Table 9: Distribution points for the In-Kind and Voucher Districts

Government Only (1)	Agent Only (2)	Both (3)	Other (4)	N (5)
Panel A: In-Kind Subsidy Districts				
87.8%	0.3%	1.9%	10.1%	52
Panel B: Voucher Districts				
0.0%	99.4%	0.0%	0.6%	55

Note: This table tabulates government officials' answers about the Rastra/BPNT distribution point in their district. Multiple officials are interviewed in some districts, in which case responses are averaged at the district level. Note that at the time of this survey, May-July 2019, some districts were implementing both Rastra and BPNT simultaneously. Responses from 102 of the 105 experimental districts are reported.

Appendix Table 10: Alternate Versions of Total Subsidy, by PMT Groupings

	PMT > 30	PMT <= 30	PMT <= 25	PMT <= 20	PMT <= 15	PMT <= 10	PMT <= 5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Total Subsidy (Fixed Price)							
Voucher	-3495.328	9140.029	9958.923	11167.982	12432.262	14456.854	13885.509
	(562.984)	(1844.989)	(2068.022)	(2256.397)	(2533.854)	(2724.979)	(3629.553)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.008]
Observations	49566	16328	13706	11071	8306	5529	2788
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	9268.476	29230.967	30559.439	31791.101	32826.879	33211.277	33670.687
Panel B: Quality-Adjusted Total Subsidy							
Voucher	-2782.928	11241.006	12207.459	13323.233	14630.190	17014.057	16167.532
	(569.605)	(1932.797)	(2166.490)	(2312.472)	(2580.014)	(2808.868)	(3632.105)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]
Observations	49566	16328	13706	11071	8306	5529	2788
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	9157.160	29099.132	30431.608	31661.954	32642.663	33003.982	33369.103

Note: This table replicates Table 2, but with fixed price total subsidy and quality-adjusted total subsidy as the outcome variables. Total subsidy at fixed prices is calculated by multiplying subsidized rice and eggs by a fixed market price. The fixed price is the average market price paid by non-subsidy recipients in the March 2019 SUSENAS (Rph 9943 per kg of rice and Rph 1513 per egg). Quality-adjusted total subsidy uses an adjusted market price for BPNT rice to account for the higher reported quality of BPNT rice. See Table 1 for additional details on the outcome variables and Table 2 for additional details on the specifications.

Appendix Table 11: Total Subsidy, by PMT Groupings

	PMT > 30	PMT <= 30	PMT <= 25	PMT <= 20	PMT <= 15	PMT <= 10	PMT <= 5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Voucher	-2598.768	13234.952	14334.674	15659.489	17332.398	19803.957	19400.529
	(564.894)	(1915.934)	(2121.507)	(2268.394)	(2513.651)	(2760.619)	(3603.554)
	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
N	49566	16327	13705	11070	8305	5528	2788
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	9162	29201	30525	31741	32736	33185	33513

Note: This table replicates Table 2, but with total subsidy as the outcome variable. See Table 2 for additional details.

Appendix Table 12: Subsidy Outcomes for Recipients, by PMT Groupings

	PMT > 30	PMT <= 30	PMT <= 25	PMT <= 20	PMT <= 15	PMT <= 10	PMT <= 5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Total Subsidy							
Voucher	28800.126	32652.711	31998.378	32082.856	32534.959	32378.285	32575.512
	(3683.851)	(3053.076)	(3335.427)	(3522.212)	(3910.914)	(4234.021)	(5109.684)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Observations	9131	9862	8634	7230	5642	3874	1975
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	31243.307	43899.509	44481.664	45061.521	45402.485	44736.016	44722.672
Panel B: Rice Quality							
Voucher	0.189	0.209	0.213	0.227	0.228	0.211	0.207
	(0.025)	(0.019)	(0.019)	(0.018)	(0.018)	(0.020)	(0.024)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Observations	9108	9790	8570	7181	5596	3842	1957
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	0.614	0.649	0.649	0.652	0.652	0.659	0.660

Note: This table replicates Table 2, but with subsidy outcomes for those who received either program as the outcome variable. See Table 1 for additional details on the outcome variables and Table 2 for additional details on the specifications.

Appendix Table 13: Per Capita Consumption by PMT Groupings

	PMT > 30	PMT <= 30	PMT <= 25	PMT <= 20	PMT <= 15	PMT <= 10	PMT <= 5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Log Per Capita Consumption							
Voucher	0.007	0.005	0.006	0.013	0.030	0.038	0.054
	(0.017)	(0.016)	(0.019)	(0.019)	(0.020)	(0.022)	(0.024)
	[0.664]	[0.758]	[0.773]	[0.523]	[0.166]	[0.104]	[0.034]
Observations	49566	16329	13707	11072	8307	5529	2788
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	13.765	13.391	13.371	13.353	13.327	13.292	13.248
Panel B: Per Capita Consumption (rp)							
Voucher	14183.682	-2559.178	6118.680	9306.161	21423.936	21629.454	28989.661
	(18316.990)	(15351.067)	(14958.885)	(14914.466)	(15292.427)	(17040.968)	(16441.619)
	[0.479]	[0.881]	[0.675]	[0.538]	[0.178]	[0.223]	[0.109]
Observations	49538	16329	13707	11072	8307	5529	2788
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	1136251	742584	727377	713850	694961	674623	646553
Panel C: CRRA Utility (Relative Risk Aversion Coefficient of 3)							
Voucher	0.030	0.063	0.072	0.103	0.151	0.197	0.227
	(0.043)	(0.070)	(0.076)	(0.079)	(0.081)	(0.092)	(0.112)
	[0.404]	[0.387]	[0.386]	[0.239]	[0.105]	[0.060]	[0.030]
Observations	49566	16324	13702	11067	8303	5526	2785
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	-1.006	-1.795	-1.860	-1.916	-2.004	-2.161	-2.327

Note: This table replicates Table 2, but with per capita consumption and constant relative risk aversion (CRRA) utility as the outcome variable. Consumption is converted to millions of rupiah before calculating utility. See Table 2 for additional details on the specifications.

Appendix Table 14: Food Insecurity Indicators by PMT Groupings

	PMT > 30	PMT <= 30	PMT <= 25	PMT <= 20	PMT <= 15	PMT <= 10	PMT <= 5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Voucher	-0.001	0.004	0.002	0.006	0.003	0.005	-0.003
	(0.005)	(0.010)	(0.010)	(0.011)	(0.012)	(0.013)	(0.016)
	[0.938]	[0.713]	[0.883]	[0.620]	[0.771]	[0.711]	[0.829]
Observations	49410	16254	13641	11021	8271	5505	2776
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	0.062	0.102	0.102	0.102	0.103	0.101	0.105

Note: This table replicates Table 2, but with an index of food insecurity indicators as the outcome variable. See Table 2 for additional details on the specifications.

Appendix Table 15: Alternative Poverty Metrics, by PMT Groupings

	PMT > 30	PMT <= 30	PMT <= 25	PMT <= 20	PMT <= 15	PMT <= 10	PMT <= 5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Poverty Gap							
Voucher	-0.0015	-0.0053	-0.0062	-0.0074	-0.0104	-0.0122	-0.0132
	(0.0012)	(0.0033)	(0.0035)	(0.0037)	(0.0040)	(0.0046)	(0.0057)
	[0.207]	[0.129]	[0.111]	[0.072]	[0.017]	[0.018]	[0.023]
Observations	49566	16329	13707	11072	8307	5529	2788
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	0.0108	0.0305	0.0324	0.0337	0.0359	0.0411	0.0463
Panel B: Poverty Gap Squared							
Voucher	-0.0003	-0.0012	-0.0014	-0.0017	-0.0026	-0.0032	-0.0037
	(0.0003)	(0.0008)	(0.0008)	(0.0009)	(0.0009)	(0.0012)	(0.0015)
	[0.337]	[0.146]	[0.129]	[0.098]	[0.015]	[0.011]	[0.017]
Observations	49537	16285	13665	11034	8276	5503	2775
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	0.0023	0.0066	0.0071	0.0073	0.0077	0.0090	0.0101

Note: This table replicates Table 2, but with alternative poverty metrics as the outcome variable. See Table 2 for additional details on the specifications.

Appendix Table 16: Rice Consumption, by PMT Groupings

	PMT > 30	PMT <= 30	PMT <= 25	PMT <= 20	PMT <= 15	PMT <= 10	PMT <= 5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Subsidized Rice (kg)							
Voucher	-0.425	0.062	0.079	0.113	0.182	0.322	0.174
	(0.059)	(0.205)	(0.227)	(0.247)	(0.286)	(0.313)	(0.411)
	[0.000]	[0.773]	[0.746]	[0.690]	[0.582]	[0.408]	[0.722]
Observations	49566	16328	13706	11071	8306	5529	2788
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	0.957	2.987	3.124	3.250	3.357	3.396	3.446
Panel B: Total Rice (kg)							
Voucher	0.074	-0.397	-0.236	-0.393	-0.396	-0.581	-1.231
	(0.294)	(0.471)	(0.486)	(0.531)	(0.551)	(0.587)	(0.812)
	[0.852]	[0.501]	[0.706]	[0.549]	[0.601]	[0.469]	[0.274]
Observations	49335	16293	13680	11050	8293	5520	2785
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	26.302	31.643	31.932	32.322	32.932	33.938	35.769

Note: This table replicates Table 2, but with rice consumption as the outcome variable. See Table 2 for additional details on the specifications.

Appendix Table 17: Egg Protein, by PMT Groupings

	PMT > 30	PMT <= 30	PMT <= 25	PMT <= 20	PMT <= 15	PMT <= 10	PMT <= 5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Subsidized Egg Protein (g)							
Voucher	3.362	32.719	35.442	39.039	37.959	39.869	42.353
	(0.463)	(4.648)	(5.122)	(5.876)	(6.301)	(6.958)	(8.160)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Observations	49552	16270	13655	11030	8271	5503	2774
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	0.015	0.484	0.506	0.567	0.528	0.634	0.524
Panel B: Total Egg Protein (g)							
Voucher	-0.562	9.519	10.167	11.774	14.736	17.844	25.548
	(4.037)	(4.764)	(5.184)	(5.797)	(6.187)	(7.615)	(9.157)
	[0.902]	[0.090]	[0.091]	[0.080]	[0.050]	[0.056]	[0.035]
Observations	49324	16291	13678	11048	8292	5520	2785
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	231.896	214.040	214.640	215.116	215.049	215.213	222.012

Note: This table replicates Table 2, but with egg protein consumption as the outcome variable. See Table 2 for additional details on the specifications.

Appendix Table 18: Experimental Difference between Voucher and In-kind Districts on Food Consumption and Temptation Goods, for Targeted Households

	Sugar (oz) (1)	Cooking Oil (l) (2)	Beef (kg) (3)	Chicken (kg) (4)	Milk (rp) (5)	Corn (kg) (6)	Salt (g) (7)	Liquor (l) (8)	Cigarettes (rp) (9)
Voucher	0.022	0.002	-0.003	-0.020	-177.160	0.041	-6.936	-0.003	298.558
	(0.145)	(0.017)	(0.002)	(0.011)	(380.599)	(0.018)	(3.454)	(0.004)	(523.391)
	[0.884]	[0.903]	[0.207]	[0.144]	[0.720]	[0.061]	[0.088]	[0.638]	[0.586]
Observations	16293	16292	16288	16292	16291	16288	16293	16271	16292
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	5.930	0.814	0.012	0.299	6199.458	0.348	106.643	0.019	17084.338

Note: This table examines the difference between voucher and in-kind districts for various food items and temptation goods for targeted households (PMT <= 30). For continuous outcome variables, we drop any value greater than 12 standard deviations from the mean. The outcome data come from March 2019 SUSENAS; the PMT data come from the Unified Targeting database. We used a double LASSO to choose the control variables (all potential variables in the lasso are listed in Appendix B). Standard errors are clustered at the district (kabupaten) level and displayed in parentheses. Randomization inference p-values are from 1,000 permutations of the treatment assignments. Joint test significance: F-statistic = 1.823, randomized inference p-value = 0.232.

Appendix Table 19: Experimental Difference between Voucher and In-kind Districts on Price (Continuous shock and distance variables)

				Measures of Isolation	
	Main Effect Only	Rice Area	Supply Shock	Time to District Capital	Time to Subdistrict Capital
	(1)	(2)	(3)	(4)	(5)
Voucher	140.536	135.926	-383.354	55.803	186.976
	(135.230)	(247.287)	(361.300)	(149.422)	(143.098)
	[0.296]	[0.576]	[0.266]	[0.698]	[0.194]
Voucher × [Variable]		0.200	10969.497	1.615	-0.431
		(0.552)	(7945.179)	(1.050)	(1.734)
		[0.770]	[0.113]	[0.178]	[0.815]
Observations	32343	31185	32343	32334	32334
Stratum FE	Yes	Yes	Yes	Yes	Yes
Main Effect Included	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	9478.508	9458.047	9478.508	9478.508	9478.508
[Variable] Mean		260.940	0.049	51.708	15.811

Note: This table replicates Table 4, but uses continuous variables for the shock and distance variables. The randomized inference p-value for the joint test for the supply variables (columns 2 and 3) is 0.617; the randomized inference p-value for the joint test for the distance variables (columns 4 and 5 from this table and columns 4 and 5 from Table 4) is 0.608.

Appendix Table 20: Experimental Difference between Voucher and In-kind Districts on Price (Above 75th percentile shock and distance variables)

				Measures of Isolation	
	Main Effect Only (1)	Above 75th Pct. Rice Area (2)	Above 75th Pct. Supply Shock (3)	Above 75th Pct. Time to District Capital (4)	Above 75th Pct. Time to Subdistrict Capital (5)
Voucher	140.536	217.400	69.657	56.963	140.708
	(135.230)	(181.223)	(138.748)	(142.097)	(143.234)
	[0.296]	[0.211]	[0.623]	[0.655]	[0.329]
Voucher × [Variable]		-141.611	554.537	338.125	-0.200
		(231.521)	(487.630)	(140.447)	(98.789)
		[0.598]	[0.145]	[0.029]	[0.997]
Observations	32343	31185	32343	32334	32334
Stratum FE	Yes	Yes	Yes	Yes	Yes
Main Effect Included	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	9478.508	9458.047	9478.508	9478.508	9478.508
[Variable] Mean		0.145	0.238	0.236	0.257

Note: This table replicates Table 4, but uses indicator variables for above 75th percentile for the shock and distance variables. The randomized inference p-value for the joint test for the supply variables (columns 2 and 3) is 0.659; the randomized inference p-value for the joint test for the distance variables (columns 4 and 5 from this table and columns 4 and 5 from Table 4) is 0.200.

Appendix Table 21: Differential effect of readiness on leakage

	Subsidy Received / Intended Subsidy (1)	Subsidy Received (Market Prices) / Intended Subsidy (2)	Subsidy Received (Quality-Adjusted) / Intended Subsidy (3)
Voucher	-0.015	-0.053	-0.029
	(0.051)	(0.047)	(0.052)
	[0.787]	[0.284]	[0.580]
Voucher × Above	-0.021	-0.022	-0.015
Med. Readiness Index	(0.059)	(0.056)	(0.061)
	[0.747]	[0.758]	[0.840]
Observations	105	105	105
Stratum FE	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes
DV Mean (Control)	0.587	0.586	0.587

Note: This table examines the differential effect of the voucher as compared to the in-kind transfers by baseline readiness index. The baseline readiness index comes from government administrative data, and was used to decide which districts would be included in the experimental sample. See Table 5 for additional details.

Appendix Table 22: Administrative Cost Calculations

Panel A: In-kind Program

Item	Details	Total annual costs
Program Benefits	5.6 million beneficiaries × Rp. 100,000 per beneficiary per month	Rp. 6.72 trillion
Annual BULOG operating costs	Rp. 120.2 billion	Rp. 120.2 billion
Local operating costs	5.6 million beneficiaries × 10/kg month × 12 months × Rp. 233 / kg	Rp. 156.6 billion
Total operating costs		Rp. 276.6 billion
Costs as a share of benefits		4.1%

Panel B: Voucher Program, assuming all agent costs charged to program

Item	Details	Total annual costs
Program Benefits	10 million beneficiaries × Rp. 110,000 per beneficiary per month	Rp. 13.2 trillion
Card-printing	10 million beneficiaries × Rp. 12,500, assumed to last 3 years	Rp. 41.6 billion
Agents EDC machine (online)	59,315 total agents × 61% online × 12 months × Rp. 130,000 / month	Rp. 56.1 billion
Agents EDC machine (offline capable)	59,315 total agents × 39% online × 12 months × Rp. 671,000 / month	Rp. 187.7 billion
Total operating costs		Rp. 285.5 billion
Costs as a share of benefits		2.1%

Panel C: Voucher Program, assuming 77% of agents were pre-existing, so charging only 23% of agent costs charged to program

Item	Details	Total annual costs
Program Benefits	10 million beneficiaries × Rp. 110,000 per beneficiary per month	Rp. 13.2 trillion
Card-printing	10 million beneficiaries × Rp. 12,500, assumed to last 3 years	Rp. 41.6 billion
Agents EDC machine (online)	59,315 total agents × 61% online × 12 months × 23% × Rp. 130,000 / month	Rp. 12.9 billion
Agents EDC machine (offline capable)	59,315 total agents × 39% online × 12 months × 23% × Rp. 671,000 / month	Rp. 43.2 billion
Total operating costs		Rp. 97.7 billion
Operating costs as a share of benefits		0.7%

Note: Administrative costs from BULOG are from the 2018 BULOG Annual Report. Local in-kind operating costs from Banerjee et al (2019). Costs of EDC machines obtained from XX.

Appendix Table 23: Local Politics (Village Level)

	Protest	Corruption	New Village Head
Voucher	0.003	-0.000	0.012
	(0.003)	(0.001)	(0.020)
	[0.351]	[0.895]	[0.510]
Observations	20818	20818	20387
Stratum FE	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes
DV Mean (Control)	0.009	0.004	0.228

Note: This table explores the impact of the transition to the voucher on village-level politics. The dependent variable in column 1 is a dummy for whether the village experienced a protest in the past year, in column 2 it is a dummy for any incident of criminal corruption reported in the past year, and in column 3 it is a dummy for the installment of a new village head during the year. Observations are at the village level. Baseline versions (PODES 2018) of the outcomes and a set of PODES 2018 covariates are included as LASSO controls. Standard errors are clustered at the district (kabupaten) level and displayed in parentheses. Randomization inference p-values are from 1,000 permutations of the treatment assignments.

B Appendix B

Full List of Variables Used as Inputs for LASSO

Dataset	Level	Variables
March 2016-2018 SUSENAS	District by Urban/ Rural Level	# Household Members, House Floor Area, Per-Capita Expenditure, Home Ownership/Lease Status, Roof Material Type, Wall Material Type, Floor Material Type, Restroom Access Categories, Toilet Type, Restroom Access × Toilet Type, Asset Ownership Variables (Cooking Gas, Refrigerator, Air Conditioning, Water Heater, Landline, Personal Computer, Gold/Jewelry 10g+, Motorcycle, Boat, Motor Boat, Car), Waste Disposal Location, Drinking Water Source, Drinking Water Purchase Type, Water Source Ownership Status, Cooking Water Source, Washing Water Source, Public Water Source, Clean Drinking Water, Clean Cooking Water, Clean Washing Water, # Male Household Members, # Female Household Members, # Children in Household, # Cell Phones Owned, Any Cell Phone Owned, # Household Members Use Computer, Any Household Member Uses Computer, # Household Members Use Internet from Source (Any, Home, Outside Home, Office, School, Public, Vehicle), Any Household Member Uses Internet from Source (Any, Home, Outside Home, Office, School, Public, Vehicle), Internet Use Reasons, Electricity Subscription Status, Cooking Gas Types
UDB	Household	Household in UDB, PKH Enrollment Status, BPNT Enrollment Status, KKS Card in 2016, KKS Card in 2017, PBI Enrollment Status, Household in Dapodik, Household in UDB in 2015, # Cows Owned, # Water Buffalo Owned, # Horses Owned, # Pigs Owned, # Goats Owned, Proxy-Means Test (PMT) Percentile Score, Home Ownership/Rental Status, Land Ownership Status, House Floor Area, Floor Material Type, Wall Material Type, Roof Material Type, Drinking Water Source, Drinking Water Subscription Status, Electricity Subscription Status, Electricity Wattage Categories, Cooking Fuel Categories, Restroom Facilities Ownership Status, Toilet Types, Waste Disposal Location, Floor Condition, Wall Condition, Roof Condition, Household Assets (Gas Tank, Air Conditioner, Landline, Water Heater, Gold/Jewelry 10g+, Bicycle, Motorcycle, Car, Boat, Motor Boat, Ship, Refrigerator), Household Assets (Self-Reported), Business Ownership, Self-Reported KKS Card, Self-Reported KIS Card, Self-Reported PKH Enrollment, Self-Reported Jamsostek Status, Self-Reported KIP Card, Self-Reported BPJS Mandiri Enrollment, Self-Reported Other Health Insurance, Self-Reported Rastra Enrollment, Land/Estate Ownership, Land/Estate Area, Second Home Ownership, Self-Reported KUR Enrollment, # Household Members (Categories), # Family Members (Categories), # Rooms in Home (Categories), Head of Household Marital Staus, # Hours Head of Household Works, Head of Household Age, Male Head of Household, Head of Household Education Level Completed, Head of Household Highest Education Level Reached, Head of Household Highest Degree Received, Head of Household Works, Head of Household Employment Sector, Head of Household Employment Status, Pregnant Woman in Household, Disability in Household, Chronic Disease in Household, Student in Household, Worker in Household
PODES	Village	Main Agricultural Crop in Village Categories, Road to Village Center Type, Road Passability Categories, Distance to Subdistrict Capital, Travel Time to Subdistrict Capital, Travel Cost to Subdistrict Capital, Cell Signal Strength Categories, Mobile Internet Types, Bank Agent in Village, Distance to Nearest Bank Agent if None in Village, Ease of Reaching Nearest Bank Agent if None in Village

Missing observations for variables in the PODES and UDB are set to 0, and indicator variables are included to indicate missing status. Variables from the March 2016-2018 SUSENAS are averaged at the district by urban/rural level using household weights.

C Appendix C

Appendix C Table 1: Total Subsidy (rp)

	September 2018			March 2019			Pooled		
	All	PMT <= 30	PMT > 30	All	PMT <= 30	PMT > 30	All	PMT <= 30	PMT > 30
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Voucher	1645.693	13311.554	-2789.336	1304.749	13234.952	-2598.768	1296.333	13788.275	-2293.981
	(1526.190)	(3102.830)	(1080.264)	(617.738)	(1915.934)	(564.894)	(617.504)	(1762.912)	(551.095)
	[0.232]	[0.002]	[0.050]	[0.087]	[0.000]	[0.001]	[0.076]	[0.000]	[0.002]
Observations	16428	3998	12294	66494	16327	49566	82922	20325	61860
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
DV Mean (Control)	16597.507	34568.698	10284.263	14456.314	29200.535	9161.727	15709.936	32319.518	9821.281

Note: This table populates the pre-specified analysis plan table 1. See Table 1 for additional details on the variable and specifications.

Appendix C Table 2: Subsidized Rice Quality

	All (1)
Voucher	0.203 (0.020) [0.000]
Observations	19260
Stratum FE	Yes
Double Lasso	Yes
DV Mean (Control)	0.630

Note: This table populates the pre-specified analysis plan table 2. See Table 1 for additional details on the variable and specifications.

Appendix C Table 3: Food Insecurity Indicators

	All (1)	PMT <= 30 (2)	PMT > 30 (3)
Voucher	0.001 (0.006) [0.905]	0.004 (0.010) [0.713]	-0.001 (0.005) [0.938]
Observations	66258	16254	49410
Stratum FE	Yes	Yes	Yes
Double Lasso	Yes	Yes	Yes
DV Mean (Control)	0.072	0.102	0.062

Note: This table populates the pre-specified analysis plan table 3. See Table 3 for additional details on the variable and specifications.