It's All in the Timing:
Cash Transfers and Consumption Smoothing in a Developing Country*

Samuel Bazzi† Sudarno Sumarto‡ Asep Suryahadi§

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Abstract

Cash transfer programs are increasingly common in both developed and developing countries. The consumption response to these programs is an important input to policy. We use quasi-experimental variation in a large-scale unconditional transfer program in Indonesia to test for consumption smoothing behavior among low-income households. Timely receipt of transfers yields no expenditure change relative to non-recipients. However, delayed receipt reduces expenditures by 7.5 percentage points. Proximity to banks moderates this negative shock. Ignoring heterogeneous timing leads to sizable underestimates of expenditure impacts. We reconcile these findings with models of consumption smoothing in which liquidity constraints imply asymmetric responses to positive and negative shocks. Our results have interesting parallels with research on fiscal interventions in rich countries and yield new implications for program evaluation.

Keywords: Cash Transfers, Marginal Propensity to Consume, Liquidity Constraints, Program Evaluation.

JEL Classifications: D91, D12, I38, O12.

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†*Corresponding Author*. Department of Economics, Boston University, 270 Bay State Rd., Boston, MA 02215. Email: sbazzi@bu.edu.

‡The SMERU Research Institute, Jl. Cikini Raya No. 10A, Jakarta 10330, Indonesia. Email: ssumarto@smeru.or.id.

§The SMERU Research Institute, Jl. Cikini Raya No. 10A, Jakarta 10330, Indonesia. Email: suryahadi@smeru.or.id.

1 Introduction

Cash transfer programs are a popular policy tool in developing countries. Beyond incentivizing human capital investment, cash transfers are increasingly viewed also as a potential vehicle for stimulating or sustaining household consumption, often in the process of introducing broader policy changes or coping with economic downturns.[1] Understanding how household consumption responds to transfer income is critical to public policy. A large body of work evaluates this question in developed countries through the lens of the life-cycle/permanent income hypothesis (PIH). These studies show how the individual and aggregate expenditure impacts depend on the timing and expected duration of transfers.[2] Yet, we have limited evidence on whether and how the effectiveness of fiscal interventions in low-income countries hinges on similar factors.

This paper aims to fill this gap by asking how household expenditures evolve over the course of a temporary cash transfer program with regularly scheduled disbursements in a developing country. We investigate this question using an unconditional cash transfer (UCT) program in Indonesia, which provided 19 million households with quarterly transfers of around 30 USD—roughly 15 percent of quarterly expenditures for the average recipient at baseline. The program was launched in the wake of fuel subsidy cutbacks and lasted for a period of one year. Several unique features of the program make it possible to test for different types of consumption smoothing behavior.[3] However, as we argue below, our approach and findings have general implications for the evaluation of transfer programs in other settings.

Using nationally representative household-level panel data, we examine the impact of the UCT program on consumption growth over two time horizons: (i) a short-term period spanning early 2005 to early 2006 during which time the program was introduced and widely publicized and all beneficiaries received at least one transfer, and (ii) a medium-term period spanning a few months after the program ended in early 2007. Our identification strategy relies on exogenous variation in the incidence, timing, and scale of transfer income.

First, many non-poor households received the UCT while many poor did not, making it possible to construct a credible counterfactual using a difference-in-difference reweighting approach (Abadie, 2005).[4] Given pervasive targeting errors, reweighting non-recipient households by their estimated odds of treatment effectively rebalances treatment and control households along baseline expenditure levels. Conceptually, this ensures that recipient and non-recipient households draw randomly from the same *ex ante* income (and wealth) distribution, which is essential for distinguishing between alternative explanations for observed consumption responses to transfers.

[1] For example, Coady et al. (2010) discuss the use of cash transfers to transition away from regressive fuel subsidies across a number of developing economies. Hur et al. (2010) discuss the use of cash transfers to low-income households in Asian countries during the global recession of 2008.

[2] Two recent surveys provide excellent background on the vast literature on the PIH (Jappelli and Pistaferri, 2010; Meghir and Pistaferri, 2011). Below, we discuss studies within this literature that are most relevant to our own work.

[3] Throughout the paper, we use consumption and expenditures synonymously.

[4] Although beneficiaries were targeted through a quasi-means testing process, we show that the reconstructed proxy means test (PMT) scores used to assign eligibility are too weakly correlated with treatment to justify a fuzzy regression-discontinuity design. Our augmented model for predicting program receipt captures substantial variation in treatment status across households and, in fact, outperforms the approximate PMT scores.

Second, due to arbitrary administrative delays, the second quarterly transfer was staggered across regions.[5] Given variation in local disbursement schedules, nearly 30 percent of all recipients in our sample were still awaiting their second transfer at the time of enumeration in early 2006. We show that the staggering process cannot be explained by any observable differences across regions in terms of remoteness, weather shocks, level of development, or political affiliation. The delayed disbursements are especially useful for distinguishing between consumption smoothing with and without credit constraints. If all households received the transfers on time *and* exhibit small consumption responses, we would not be able to distinguish between unconstrained consumption smoothing and credit-constrained, precautionary savings behavior.

Third, because all households received the same transfer amount per disbursement, we observe considerable variation in transfers *per capita*.[6] This allows us to test whether consumption is excessively sensitive to the magnitude of income changes, which would constitute a departure from a standard model of unconstrained consumption smoothing based on the PIH.

Our main empirical results suggest that timing and expectations matter and that credit constraints limit perfect consumption smoothing. On the one hand, recipient households still awaiting their second quarterly transfer in early 2006 report per capita expenditure growth rates that are roughly 7.5 percentage points lower on average than both (reweighted) control households and UCT beneficiaries that had already received the second transfer. This translates to a consumption loss of around USD 1.35 per person per month, which implies a marginal propensity to consume (MPC) out of transitory transfer income of 0.55. This is larger than what we should observe in a PIH-type model with perfect credit markets and is consistent income falling short of expectations, leaving households with limited cash-on-hand.

On the other hand, we find no mean differences in expenditure growth between control households and UCT beneficiaries that had received the two transfers by early 2006. Although the program was unforeseen as of early 2005, timely receipt of UCT disbursements between survey rounds had no economically significant effect on consumption growth. Moreover, by early 2007, several months after the final transfer was received by all beneficiaries, we find no differences in consumption growth across recipient groups or between recipients and non-recipients. These null effects are found for the long-difference between 2005 and 2007, a period spanning the life of the program, as well as between 2006 and 2007, a period within the life of the program.

Taken in isolation, these muted responses to cash transfers do not rule out consumption smoothing behavior consistent with standard permanent income models with perfect credit markets. However, given the negative consumption shock associated with delayed disbursement, we can be more confident that the null treatment effects are suggestive of some degree of precautionary savings behavior motivated by credit constraints.[7] As noted by Zeldes (1989), borrowing constraints can

[5]Indonesia's administrative divisions proceed from province to district to subdistrict to village. The staggering took place primarily across subdistricts, the level at which post offices were tasked with disbursing quarterly transfers to villages.

[6]De Janvry and Sadoulet (2006) make use of similar variation in treatment intensity imposed by the cap on total transfers in the *Progresa* program in Mexico, and Kaboski and Townsend (2011, 2012) analogously exploit fixed financial transfers across Thai villages that vary in population size.

[7]The precautionary savings motive may have been especially strong in this setting with high inflation and uncertainty about future subsidy reforms. The government cut fuel subsidies twice in 2005, and a sustained ban on rice imports

change consumption dynamics even if they never bind. Although it is difficult to separately identify precautionary savings from liquidity constraints (Deaton, 1991), our quasi-experimental variation allows us to highlight both types of behavior in a single population without having to split the sample *ex ante* between presumptively constrained and unconstrained households—a common practice criticized by Carroll and Kimball (2001).

Overall, then, the positive transitory shock associated with the arrival of the UCT program had a smaller expenditure impact than the negative shock associated with delayed disbursement of the second quarterly transfer. Like others looking at data from the United States, we infer that the asymmetric response to positive and negative shocks is consistent with consumption smoothing behavior in the presence of liquidity constraints (e.g., Altonji and Siow, 1987; Shea, 1995). In the absence of borrowing options, positive shocks encourage precautionary savings that dampen the expenditure response whereas negative shocks amplify that response. Our findings suggest some of the first transfer may have gone to precautionary savings but that those savings were an insufficient buffer against the negative shock imparted by the delayed arrival of the second transfer.

Several other findings support the cash-on-hand interpretation of our main results. First, the largest differential growth rates through early 2006 are found for food rather than non-food expenditures. This is consistent with the implications of liquidity constraints since food expenditures are reported over the prior week whereas non-food expenditures are reported over the prior month (or year). Second, the negative consumption impact of delayed disbursement is increasing over time with greater excess sensitivity observed for households enumerated in March than in February, 2006. Third, proximity to financial institutions largely offsets the negative shock associated with delayed disbursement. For beneficiaries residing in areas with more liquid local financial markets (and formal savings options), the delays did not lead to any significant change in mean expenditures. Fourth, we find a U-shaped relationship between the estimated treatment effects and household age—a pattern that is consistent with life cycle models of consumption smoothing in which credit constraints are binding most strongly for the young, and savings motives are weakest for the old. Finally, consumption growth is increasing in the size of transfers per capita with an implied MPC that complements the estimate based on the discrete treatment effects approach.

Moreover, our main findings are not an artifact of the data. The key null results for both the short- and medium-term are precisely estimated zeros, and we demonstrate that the sample sizes are sufficiently large to detect small treatment effects. We also show that spillovers to non-recipients via informal taxation and redistribution within villages cannot explain the results. The baseline results hold up to a battery of robustness checks including the use of a measure of non-food expenditures over the past year rather than the past month. This partially rules out an important alternative explanation that all of the transfer funds were spent immediately during a period missed by the survey instrument (i.e., households are perfect hand-to-mouth consumers).

This paper contributes to a growing literature that quasi-experimentally tests models of consumption and offers new evidence on how timing and expectations matter for understanding pol-

enacted in 2004 led to considerable inflation and volatility in prices of the main food staple. During this same period, public discussions were underway to cut fertilizer subsidies for agriculture.

icy impacts. We find several interesting parallels with the literature on fiscal interventions in high-income countries. For example, Hsieh (2003) shows that household expenditures in Alaska do not respond to regular cash transfers provided through the Alaska Permanent Fund but are excessively sensitive to tax refunds (similar to earlier findings in Souleles, 1999). In Spain, Browning and Collado (2001) find that consumption does not respond to regular semi-annual bonuses for full-time workers. In the United States, Johnson et al. (2006) show that households spent 20-40 percent of the tax rebates in 2001, and Parker et al. (forthcoming) find that 12-30 percent of stimulus payments in 2008 were spent primarily on nondurables. In both cases, households with low liquidity exhibited the largest response, and Sahm et al. (2012) show that the MPC was larger for those receiving a one-time payment rather than a flow from reduced withholdings. These findings are broadly consistent with those that we report for Indonesian households in a lower income setting where (i) consumption responds most strongly to unexpected negative changes in (the timing of) transfer income, (ii) the magnitude of transfers matter, (iii) the largest MPC is for food, and (iv) financial access facilitates consumption smoothing.

Our study clarifies how policy interventions can be used to test canonical theories of consumption in developing countries where previous tests have been based largely on weather volatility in agricultural areas. Many studies investigate the effects of transitory shocks to farmer income on consumption behavior in settings with limited formal insurance and weak credit markets (e.g., Kazianga and Udry, 2006; Paxson, 1992; Wolpin, 1982). Our key innovation is to examine such behavior among the general population of a large emerging market in response to an increasingly common policy intervention. Hence, we add to a small but growing body of work evaluating large-scale government interventions in developing countries through the lens of theory (e.g., Angelucci and Attanasio, 2013; Kaboski and Townsend, 2011). A closely related study by Gertler et al. (2012) finds that Mexican beneficiaries of *Progresa* have a MPC around 0.74, which is over 30 percent larger than the MPC reported in this paper. The full series of *Progresa* transfers last 7 or more years. Hence, these households may have been less inclined to save than Indonesian households given the (well-publicized) short-lived nature of the UCT program in 2005-6.

Furthermore, we directly contribute to the literature showing how explicitly accounting for timing and expectations can enrich our understanding of the mechanisms underlying the average treatment effects of cash transfer programs in developing countries. Bianchi and Bobba (forthcoming) show that *Progresa* increased entrepreneurship among Mexican beneficiaries in advance of actual transfer receipt. By exploiting the differential timing of transfers across households, they are able to show that the program increased entrepreneurship not only by relaxing liquidity constraints but also by encouraging risk-taking. Using a similar identification strategy, Edmonds (2006) finds that South African households reduce child labor and increase schooling in anticipation of future transfers from the Child Support Grant program, attributing the result to binding liquidity constraints. Beyond these reduced-form studies, Attanasio et al. (2012) develop a structural model in which control households' expectations over future transfers can influence the estimated treatment effects. Failing to account for such expectations can lead researchers to underestimate actual treatment effects. Attanasio et al. show that this was not the case in *Progresa*. However, if treatment households saved more as the transfers became routine, then the endline survey may still capture only part of the consumption (or other behavior) change. In our setting, failure to account for the timing of transfers leads to underestimates of the expenditure impacts by 30 percent.

Like ours, each of these three studies suggests large potential research gains to (re)optimizing survey and study design. Recent work has explored optimal program design along several dimensions (see, e.g., Baird et al., 2011; Barrera-Osorio et al., 2011; Filmer and Schady, 2011). Alongside these important innovations, our study suggests how to learn more from existing designs through well-timed surveys and better measurement. First, for programs like *Progresa* with eventual control group enrollment, we can estimate how much of the transfer is not spent by using the control group's marginal propensity to save subsequent to program announcement. Second, by measuring savings directly throughout the course of an intervention, we can estimate the expenditure impacts that go beyond the typical evaluation window. Our results indicate that evaluations of cash transfer programs in developing countries may continue to underestimate the total impact if these approaches are not brought more fully into future research designs.[8]

Going back to the title of the paper, it is not only the case that program impacts may depend on the timing of interventions but also that our interpretation of those impacts may hinge on the time(s) at which we observe households. McKenzie (2012) shows that more follow-up surveys conducted over relatively short intervals can dramatically increase the power to detect small treatment effects when outcomes of interest have low autocorrelation (e.g., expenditures). We show here how improved research design can add substantive value beyond these purely statistical gains. Overall, our findings strengthen the case for greater attention to pre-intervention (post-announcement) behavior change, differential treatment effects within the course of an intervention, and possible backloading of changes post-intervention via accumulated (or depleted) savings.

The paper proceeds as follows. Section 2 provides background on the empirical setting. Section 3 details our identification strategy. Section 4 presents the empirical results. Section 5 concludes.

2 Background and Data

In this section, we provide relevant background on Indonesia's unconditional cash transfer (UCT) program and describe our primary household panel data.

2.1 Indonesia's UCT Program

In the midst of escalating global oil and gas prices in 2005, the Government of Indonesia slashed fuel subsidies, raising regulated prices by a weighted average of 29 percent in February and then again by 114 percent in September. Although the subsidies were regressive, fuel products constitute

[8] Angelucci and De Giorgi (2009) offer another means of improving study design by extending the evaluation of program impacts to ineligibles within the same local economy who may benefit (or not) from increased liquidity among their neighbors. Using *Progresa* in Mexico, they argue that village-level randomization and sufficient sampling of ineligible households is necessary to identify these economically meaningful indirect treatment effects. Our focus on temporal coverage complements their focus on spatial and interhousehold coverage.

a small share of overall household expenditures among both rich and poor.[9] However, the increase in fuel prices triggered inflationary pressure as the CPI grew by 17.9 percent between February 2005 and February 2006.

With the fiscal savings generated by the subsidy cutbacks, the government launched a temporary UCT program beginning in late 2005. The stated goal of the program—first announced publicly in August 2005—was to provide four quarterly disbursements of 300,000 Rupiah (Rp) (around 30 USD) to the poorest 30 percent of households beginning on October 1st. The full program benefits accounted for around 15 percent of annual household expenditures for the average recipient at baseline. The transfers reached every village in the country and were provided to households via local post offices tasked with disbursing payments on scheduled dates for each village. Typically, such offices are located in subdistrict capitals and serve multiple villages.[10]

The targeting of beneficiaries proceeded in three stages. First, local government officials devised a large list of potential recipient households in August 2005 using a combination of own-discretion and community-based records from prior government programs. Second, using a minimalist survey instrument, the regional statistical bureaus enumerated households on the initial list as well as others from additional government sources.[11] Third, the Central Statistics Bureau (BPS) used the survey data to implement a proxy means test to generate the final list of eligible households by the end of September. Although the raw survey data and PMT scores are not available, the baseline *Susenas* data, which we describe next, include excellent proxies for most questions.

2.2 Data

We use three waves of nationally representative panel data from the National Socioeconomic Survey (known as *Susenas*) collected in February/March 2005, 2006 and 2007. After matching households across the 2005 and 2006 rounds, we obtain a balanced panel of 9,048 households. We also observe a subset of households ($N = 7,016$) again in early 2007.[12] The *Susenas* expenditure module provides a detailed account of food expenditures over the last week and durable expenditures over the last month and last year. As discussed in detail in Section 4.1, both the short- and medium-run panels have a large enough sample size to detect small expenditure impacts. This is important given that the transfers were relatively small and explicitly transitory.

Table A demonstrates the timing of the program with respect to survey enumeration indicated

[9]Based on nationally representative household survey (*Susenas*) data from July 2004, households in the poorest (richest) decile of households allocate 3.7 (1.9) percent of total monthly expenditures to kerosene on average. The figures for gasoline and diesel are similarly low but relatively more important for rich than poor households.

[10]In our data, described below, households report an average cost of 75 cents to reach the post office. However, reported costs range from zero to 10 USD. In some villages, local officials would arrange group transport and accompany households to the post office. In others, local officials would deliver disbursements directly to beneficiaries in the village.

[11]The survey questions concerned: (1) floor type, (2) wall and roof type, (3) toilet facility, (4) electrical source, (5) cooking fuel source, (6) drinking water source, (7) frequency of meat consumption, (8) frequency of meal consumption, (9) frequency of purchase of new clothes, (10) access to public health facilities, (11) primary source of income, (12) educational attainment of household heads, (13) amount of savings and type of assets, and (14) floor width.

[12]The baseline survey contains 10,574 households, while the follow-up in 2006 contains 9,892 households. The 2007 survey meanwhile contains more than 55,000 households, a subset of which were interviewed in preceding years. See Appendix C for details on panel construction, attrition, and sampling design.

by years $t,\ t+1$, and $t+2$. The table makes clear that at the time of the baseline survey in early 2005, no households were aware of the forthcoming UCT program. Extensive media coverage and notification of beneficiaries proceeded from August of that year giving rise to a sharp break in expectations about future (transfer) income.

Table A: Timing Conventions

	Date	**Survey Timing**	**Program Timing**
t	February–March, 2005	*Susenas* "Baseline"	
	August–September, 2005		Program Announced
	October, 2005		Quarterly Disbursements Start
	January, 2006		2nd Disbursement Period Start
$t+1$	February–March, 2006	*Susenas* "Midline"	
	July, 2006		Final Disbursement Period
$t+2$	February–March, 2007	*Susenas* "Follow-up"	

Due to spatial variation in the timing of UCT disbursements, we observe three levels of treatment denoted by the number of disbursements $D \in \{0, 1, 2\}$ received by the time of *Susenas* enumeration in February and March 2006. There are 2,444 households in the treatment group ($D > 0$), 1,805 of whom had received two disbursements while 639 households were still awaiting the second disbursement.[13] Ninety percent of all recipients reported knowing the size of program transefers they were supposed to receive (by law) with each disbursement.

Although progressively targeted, the UCT benefits did not reach many (near-)poor households. Table 1 shows that recipient households are indeed poorer on average than non-recipients in early 2005 prior to the UCT program. However, there is strong evidence of potential (i) *leakage* of benefits as 37 percent of UCT recipients are in the top three national per-capita expenditure quintiles, and (ii) *undercoverage* as half of the lowest quintile did not receive any benefits. Figure 1 bears out these targeting results. Ultimately, only 50 (39) percent of poor (near-poor) households received any transfers despite being the nominal target population. We show next why these targeting errors make it possible to construct a credible group of counterfactual non-recipients in the absence of the PMT scores that were supposed to have dictated targeting.[14]

3 Empirical Strategy

In this section, we develop an empirical strategy for exploiting the multiple sources of variation in Indonesia's UCT program that allow us to identify consumption smoothing patterns. We are

[13] We are unable to observe the scheduled or actual dates of disbursement by village (or by household). However, as discussed below, this data constraint should not affect the credibility of our identification strategy or of the importance ascribed to timing and expectations in interpreting our results.

[14] While it is not possible to obtain the actual administrative PMT scores that would enable a regression discontinuity design, we can use the available questions in *Susenas* coupled with the district-specific coefficients for each qualifying criteria to construct a strong approximation to each household's actual PMT score. However, as we show in Appendix A, these reconstructed scores (i) fail to produce any fuzzy discontinuities around the stipulated threshold, and (ii) achieve less balance than our estimated propensity scores (see below) based on a richer set of household characteristics plausibly known to program enumerators and village officials at the time of beneficiary enrollment.

interested in the average treatment effect on the treated (ATT) of receiving d relative to $\widetilde{d}$ disbursements. Denoting this estimator by $\tau_{d\widetilde{d}} \equiv \mathbb{E}[\mathcal{C}_h(d) - \mathcal{C}_h(\widetilde{d}) \mid D_h = d]$ for some consumption outcome $\mathcal{C}_h$ for household h, we aim to identify the parameter vector $\boldsymbol{\tau} \equiv (\tau_{10}, \tau_{20}, \tau_{21})$ using the following difference-in-difference specification for the change in log household consumption per capita between year t and $t+1$,

$$\Delta \ln C_{h,t+1} = \kappa + \tau_{10}\mathbf{1}\{D_h > 0\} + \tau_{21}\mathbf{1}\{D_h = 2\} + \Delta\eta_{h,t+1}, \tag{1}$$

where $\tau_{20} \equiv \tau_{21} + \tau_{10}$, $\Delta\eta_{h,t+1}$ is the error term, and the constant κ is average growth among non-recipients. Note that τ_{10} identifies the mean consumption response among households awaiting their second quarterly disbursement at the time of enumeration in early 2006, and τ_{20} identifies the mean response among households that received two disbursements. In the remainder of this section, we take steps to ensure that the comparison of growth outcomes across groups is as close as possible to what one would observe if treatment status D had been assigned randomly.

3.1 Binary Treatment Effects

We pursue a reweighting approach to identify the binary treatment effect of the UCT program. In particular, the contribution of non-recipient households to the counterfactual is directly proportional to their estimated odds of treatment, $\widehat{\omega} = \widehat{P}/(1-\widehat{P})$, where $\widehat{P}$ is the household's predicted probability of receiving *any* UCT benefits. Using a logit specification, we estimate this propensity score as an additive function of all observable underlying components of the PMT scores including household size dummies and additional household characteristics that would have been known to local targeting agents at the time of eligibility designation. The full set of marginal effects estimates are reported in Table 2, which shows that the likelihood of receiving any UCT benefits is: (i) higher for female-headed households, those benefiting from the long-running Rice for the Poor program, and households whose primary income source is in low-skilled occupations such as construction; (ii) decreasing in the size of land owned, housing floor area, and education level of the household head; and (iii) sensitive to housing status, the type of drinking water used, and toilet disposal location. The UCT program ultimately reached every village in the country, and overall, our propensity score model explains around one quarter of the variation in treatment status.

Figure 2 demonstrates the considerable overlap in propensity scores for treatment ($D > 0$) and control ($D = 0$) households. We can use the $\widehat{\omega}$ terms as inverse probability weights (IPW) in order to rebalance recipient and non-recipient households along observable dimensions. Under the assumption that there are no time-varying unobservable determinants of consumption growth correlated with UCT receipt, we can then assign a causal interpretation to the conventional binary treatment effect (see Abadie, 2005). In our case, this conventional ATT estimate is simply $\tau^{\text{binary}} \equiv \tau_{10} + \pi\tau_{21}$, where π is the share of recipients with $D = 2$.

3.2 Multivalued Treatment Effects

In order to identify the causal multivalued treatment effects parameters, $\boldsymbol{\tau}$, in equation (1), we must validate the exogeneity of the staggered rollout of the second quarterly disbursement. Table 3 shows that the probability of receiving disbursement two conditional on receiving disbursement one, $\mathbb{P}(D = 2 \mid D > 0)$, is explained almost entirely by geographic fixed effects. In columns 1-6, we find that household-level characteristics explain considerable variation in the probability of receiving any disbursements, $P(D > 0)$, even with more than 600 subdistrict fixed effects. However, turning to columns 7-12, household-level characteristics explain very little variation in $\mathbb{P}(D = 2 \mid D > 0)$ after controlling for district or subdistrict fixed effects. The R-squared and F tests in Table 3 suggest that the staggering occurs largely across (sub)districts and is plausibly exogenous with respect to baseline household characteristics.

We go a step further in Table 4 to show that both fixed and time-varying geographic characteristics cannot explain the spatial variation in staggering. Although district population size and the presence of banks weakly explain some of the staggering process, relatively poorer or more remote regions do not receive the second disbursement any later than relatively wealthier, more central regions. Interestingly, distance to post offices has no predictive power. Nor does political affiliation of village officials with the central government in Jakarta. We also find that the actual date of survey enumeration in early 2006—days since the first household was enumerated on February 2—cannot explain the staggering process either. In other words, households waiting for their second disbursement at the time of enumeration were not simply residing in regions enumerated at earlier dates.[15] Villages that experienced any mudslides or earthquakes in the previous three years are slightly less likely to have received the second disbursement by enumeration in early 2006. Nevertheless, the significant results in Table 4 should be interpreted cautiously as we expect that at least a few coefficients (5-10 percent) would be precisely estimated even if there were no relationship with staggering. Overall, Table 4 suggests that the staggering occurred for largely unknown reasons correlated with geography and hence can be used to identify multivalued treatment effects.[16]

To sum up, there are several key assumptions that ensure causal identification of $\boldsymbol{\tau}$: (i) the usual assumptions required in a difference-in-difference setting with non-random treatment assignment, (ii) beneficiaries expect continued cash disbursements upon learning about or receiving the first disbursement of the UCT program, (iii) the unobservable determinants of the delayed second disbursement are uncorrelated with time-varying unobservable determinants of expenditure growth, $\Delta\eta_{h,t+1}$, and (iv) leisure and consumption are separable in the utility function. Although controversial (see Attanasio, 1999), assumption (iv) is a standard one in the PIH literature and allows us to focus exclusively on consumption outcomes. Assumption (iii) ostensibly justifies the use of geographic fixed effects as instrumental variables for $\mathbf{1}\{D_h = 2\}$. However, in order for district (or subdistrict) fixed effects to be valid instruments, it must be the case that these

[15] In fact, UCT recipients enumerated earlier were more likely to have received the second disbursement (see Figure 3).

[16] We do not consider other approaches to identifying multivalued treatment effects using the generalized propensity score (see Cattaneo, 2010; Imbens, 2000) since our multivalued treatment is plausibly exogenous. Augmenting the binary propensity score equation (see Appendix A) with the covariates in Table 4 does not affect any of the key results below. To retain the full sample size, we omit these covariates in the main specifications.

administrative boundaries only affect consumption growth through the delays in the UCT program. This seems implausible given that there are large differences in the rate of income convergence across regions of Indonesia that are bound up in these FE but are unrelated to administrative delays. We revisit the role of geographic fixed effects in Section 4.4 (and Appendix B.4).

Reweighting, Quasi-Random Staggering, and Balance. In Figure 4, we compare the distribution of baseline log household expenditures per capita across treatment levels. Given the exogeneity of the staggering process, it is not surprising that the distributions for treatment groups $D = 1$ and $D = 2$ are nearly identical and, in fact, statistically indistinguishable. Consistent with the summary statistics in Table 1, the control group is substantially richer at baseline than both treatment groups. However, once we reweight control households using $\widehat{\omega}$, the control group distribution shifts dramatically leftward and now effectively overlaps with the treatment groups' distributions. The slight imbalance in the extreme upper tail leads to a small albeit statistically significant mean difference across the treatment and control groups (at the 10% level). However, as demonstrated below, key results are robust to trimming upper tail of baseline log expenditures.[17] Other baseline covariates are effectively balanced after reweighting by $\widehat{\omega}$.[18]

Furthermore, even if targeting agents effectively selected those households with the lowest unobservable welfare levels at baseline, the selection bias should work against finding null or negative treatment effects since these households have the highest marginal utility of income. This argument also extends to the possibility of targeting on *ex ante* time-varying welfare shocks. If agents selected those households that just experienced negative shocks prior to the program, then it is likely that the observed effects of the UCT would be biased upwards given the same marginal utility argument and the strong mean-reverting properties of expenditures (typical autoregressive coefficients are less than 0.1).

Parallel Trends. We can also provide a partial test of the parallel trends assumptions underlying our identification strategy. Although we are unable to examine pre-program expenditure trends at the household level, we are able to do so at the district level using a representative estimate of average household expenditures from *Susenas* 2004 enumerated seven months prior to our baseline data in early 2005. We find that households with one disbursement by early 2006 are no more likely to reside in low-growth districts before the UCT program than are households with two or no disbursements.[19] This indirect evidence supports the assumptions underlying the difference-in-

[17] Regardless, this only poses a potential source of bias only insomuch as it cannot be explained by time-invariant determinants of consumption.

[18] Empirically, less than 10 percent of the covariates in Table 2 exhibit statistically significant mean differences across recipients and non-recipients after reweighting by $\widehat{\omega}$. These results are available upon request.

[19] In particular, we estimate the following equation by multinomial logit reweighting control households by $\widehat{\omega}$:

$$P(D_{hjt} = d) = \alpha_d \;+\; \beta_d \Delta \ln \overline{C}_{j,t-1} \;+\; \upsilon_{hjt},$$

where D_{hjt} is the number of UCT disbursements d received by household h residing in district j in early 2006, and $\Delta \ln \overline{C}_{j,t-1}$ is the log growth in average household expenditures per capita in district j between July 2004 and early 2005. Computing marginal effects (MFE), we find that $\widehat{\beta_1^{MFE}} = 0.015$ $(se_1 = 0.038)$ and $\widehat{\beta_2^{MFE}} = -0.027$ $(se_2 = 0.051)$.

difference identification and plausible exogeneity of the staggering process.

3.3 Variation in Transfers per Capita

Because all households received the same transfer amount per disbursement, we observe considerable variation in transfers *per capita*. This allows us to identify an auxiliary measure of the marginal propensity to consume (MPC) out of transfer income and to test further whether the size of shocks matters. Figure 5 plots the distribution of transfers per capita in early 2006 for all recipients. In order to exploit this intensive margin of treatment variation conditional on the number of disbursements d, we must address two potential concerns. First, if the UCT program caused changes in household size, then any observed effect of transfers per capita on expenditures may reflect this intermediate relationship. Second, local officials in some regions extracted a portion of the officially mandated 300,000 Rupiah disbursement per beneficiary.[20] If the incidence of informal taxes varied systematically depending on household size or other characteristics, then the estimated elasticity of expenditure growth with respect to transfers per capita might be biased. In Appendix B.1, we rule out both of these concerns, demonstrating that (i) program receipt had no effect on household size, and (ii) the probability of recipient household h being taxed is orthogonal to a large array of household characteristics.

4 Results: Cash Transfers and Consumption Smoothing

We turn now to the main empirical results in the paper. In addition to pure OLS, we consider four alternative reweighting estimators of equation (1). All are predicated on the *inverse probability weighting* (IPW) approach of reweighting control households by their estimated odds of treatment, $\widehat{\omega}$. The *double robust* estimator augments the IPW specification with the propensity score ($\widehat{P}$) or the covariates ($\mathbf{X}_h$) used to predict those scores. The *heterogeneous control function* estimator introduces a fifth-order polynomial in the propensity scores and allows it to vary across recipients and non-recipients.[21] We trim all households with estimated propensity scores $\widehat{P} > \tilde{p} = 0.91$, which is the optimal bound using the Crump et al. (2009) procedure.[22] In all specifications, we control for province fixed effects and cluster standard errors at the village level, using a block bootstrap

This suggests that, after reweighting to achieve balance, households in the two treatment groups are no more likely to reside in low (or high) growth districts before the UCT program than are households in the control group.

[20] Approximately 6.5 (8.5) percent of recipients were subject to these informal taxes at the time of obtaining their first (second) UCT disbursement. According to these recipients, most of the proceeds were intended for redistribution to non-recipients deemed deserving by local officials.

[21] Results are unchanged when using other polynomial orders. An excellent review of reweighting estimators can be found in Busso et al. (2009) and Imbens and Wooldridge (2009). We prefer these estimators to the more common matching procedures used in the evaluation literature because (i) reweighting estimators often have better finite sample properties than standard matching estimators in situations with considerable overlap in propensity scores as we have here, and (ii) these estimators make it computationally easy to recover *multivalued* treatment effects.

[22] Huber et al. (2013) propose a less biased, alternative trimming procedure. However, given our large sample size and the distribution of $\widehat{P}_{h|D=0}$, their approach results in no trimming at all. Our key results are nevertheless robust to dropping the Crump et al. (2009) approach and including the full sample.

whenever the generated propensity score is used.[23]

4.1 Main Results: Timing Matters

We begin by presenting estimates of equation (1) that are consistent with consumption smoothing behavior in the presence of borrowing constraints. The top panel in Table 5 reports results for consumption growth between 2005 and 2006. We find a consistent pattern of differential treatment effects across all reweighting specifications. First, recipients still awaiting their second disbursement at the time of enumeration in early 2006 have significantly lower growth relative to non-recipients *and* recipients with both disbursements. In the preferred control function specification in column 5, these single disbursement recipients have expenditure growth rates that are 0.076 log points lower than non-recipients. Second, consumption growth among recipients of two disbursements is statistically indistinguishable from the average of 0.11 among non-recipients. The results are largely insensitive to the estimator used with the exception that the OLS estimates of τ_{10} and τ_{21} are slightly lower.[24] However, pooling the two recipient groups and estimating a conventional binary treatment effect (τ^{binary}, see Section 3) would have understated by 30 percent the expenditure gains to receiving the full semi-annual disbursements by early 2006.

In terms of magnitudes, $\tau_{10} \approx -0.075$ implies an average consumption loss of around Rp 13,500 (roughly, USD 1.35) per capita per month. This seems plausible given that one UCT disbursement provided around Rp 75,000 per capita per quarter or Rp 25,000 per capita per month. The estimates therefore imply that recipient households spend nearly 55 percent of the transfers. This implied marginal propensity to consume (MPC) is larger than what we would expect if households behaved according to the permanent income hypothesis with perfect credit markets.

Retaining the same specifications and moving ahead to early 2007, the bottom panel of Table 5 shows that the differential treatment effects dissipate over the two-year time horizon by which time all recipients had obtained the full four quarterly disbursements. Both groups of treatment households are statistically indistinguishable from control households.[25] This same conclusion holds when looking at annual consumption growth between early 2006 and early 2007, a period beginning at a time when households would have expected the program to cease by the end of the year.

To be sure, the sample sizes are sufficiently large to identify small impacts as the data allow for minimum detectable effects (MDE) of around 0.05 standard deviations. This calculation is based on (i) a sample of 9,048 households, (ii) baseline variance of log expenditures of 0.56, and (iii) one-third

[23]More specifically, we (i) draw all households from a random sample of the 629 villages, (ii) recover $\widehat{P}$ and $\widehat{\omega}$ using a logit specification, and (iii) estimate $\boldsymbol{\tau}$ in equation (1) using the given reweighting estimator and clustering standard errors by village. With over 600 villages, there is little concern about small cluster bias (see Cameron et al., 2008). The key qualitative conclusions of the paper remain unchanged when clustering at higher administrative levels.

[24]Nevertheless, the OLS estimates are statistically indistinguishable from the reweighting estimates. One explanation could be that selection bias is limited after taking first-differences and hence may be largely confined to the cross-section. However, in each of the columns 3-5, the selection terms are (jointly) statistically significantly different from zero. Thus, it may be the case that the biases due to nonrandom selection of households with low η_{ht} (accurately targeted on unobservables) and high η_{ht} (mistargeted on unobservables) offset each other on average given the mean-reverting properties of expenditures.

[25]These estimates are not an artifact of the attrition of households between 2006 and 2007 survey rounds (see Section 2.2). Key results remain largely unaffected when reweighting the sample to account for the probability of attrition.

of the population receiving the treatment. The implied MDE compares favorably to the standard deviation of observed log expenditure per capita growth (roughly 0.4). Similarly small MDEs are found for the smaller sample over 2005–2007. Given that we use monthly expenditures and each quarterly disbursement comprises nearly 45 percent of average monthly expenditures in recipient households at baseline, we could observe treatment effects as large as one standard deviation of growth if households consumed all of the transfers within the enumeration period.

If disbursement delays cause sizable reductions in expenditures, then the impact of the delayed disbursement "shock" should be increasing in the length of the delay as cash-in-hand constraints become more binding. Table 6 provides indirect evidence of this mechanism by showing that the drop in expenditures among recipients still waiting for the second disbursement is larger for households enumerated in March than for those in February 2006.[26] In fact, average expenditure growth among those enumerated in February 2006 is indistinguishable from non-recipients and those that already received a second disbursement. The similarity of τ_{10}^{March} with τ_{10} in Table 5 suggests that the average negative expenditure impact of realized transfers falling short of expectations is driven by those households experiencing the longest delay. Before further reconciling the estimates of $\boldsymbol{\tau}$ in these tables to consumption smoothing models in the literature, we briefly discuss a few results that shed additional light on the timing of consumption.

Decomposing Expenditure Growth. Table 7 shows that the differential treatment effects are driven largely by changes in expenditures on food rather than non-food items. Using the preferred control function estimator, we cannot reject the null hypothesis that all three groups $D \in \{0, 1, 2\}$ have identical non-food expenditure growth over both the short- and medium-term periods. Over the short-term period, the key parameters τ_{21} and τ_{10} are nearly halved when restricting attention to non-food expenditures. However, the opposite is true for food expenditures, where we find that these parameters are amplified and statistically significant at the one percent level.[27] Although the differences in coefficients between columns 1 and 2 are not statistically significant, these results suggest that the expenditure growth differential between recipient groups can be attributed primarily to differences in food expenditures over the week prior to survey enumeration in early 2006.

In Table 8, we further disaggregate food and non-food expenditure items and find the same general patterns.[28] For most expenditure subcategories, (i) recipients still awaiting their second disbursement have lower expenditure growth than recipients of two disbursements and non-

[26] Around one quarter of households were enumerated in February with a proportional share of recipients still awaiting the second quarterly disbursement.

[27] Shea (1994) shows using aggregate data from the U.S. that the PIH predictions hold for food but not durables expenditures. However, our findings seem to be consistent with the finding in Souleles (1999) that nondurable consumption is excessively sensitive to predictable tax rebates among liquidity constrained households in the U.S., whereas unconstrained households exhibit less sensitivity. In a developing country context, Angelucci and Attanasio (2013) show that the *Oportunidades* cash transfers led to larger increases in food than non-food expenditures among poor households in urban Mexico. In the Online Appendix, we find that the effects of the UCT do not differ in urban relative to rural areas of Indonesia where households grow more of the food they consume.

[28] In keeping with the specification for aggregate expenditure growth, we restrict the estimates for each commodity group to those households with non-zero expenditures in both periods. In unreported results, we find little evidence of any differential treatment effects along the extensive margin in terms of switching in or out of nonzero expenditures across the categories of goods in Table 8 (results available upon request).

recipients, and (ii) the second disbursement closes the gap between recipient and non-recipient expenditure growth. However, we find no effect of the transfers on purchases of grain including rice and tubers. Given the storability of grains, it is likely that such purchases are less sensitive to the cash-in-hand mechanism underlying the treatment effects for other, more perishable foods. In other words, grains are the least likely to have a one week shelf-life that would have led to their entry in the expenditure survey module. For reasons we discuss next, these expenditure decompositions provide further evidence that timing matters.

Reconciling Treatment Effects with Theory. At first glance, the pattern of coefficients in Tables 5-8 seem to be at odds with a large literature documenting sizable consumption gains from transfer programs in low-income settings. However, as we discuss now, these baseline findings can be explained by appealing to the literature on consumption smoothing and in particular the possibility that households may respond asymmetrically to positive and negative income shocks.

The treatment effects reported in Table 5 are consistent with two consequences of borrowing constraints: hand-to-mouth consumption and precautionary savings.[29] The estimate of $\tau_{10} < 0$ is consistent with credit constrained households being unable to smooth consumption while waiting for the delayed second disbursement. These households must consume their (depleted) cash-on-hand while waiting for the transfer to arrive. The large implied MPC > 0.5 supports such an interpretation, which is bolstered by the finding that the largest consumption growth differences are observed for (i) households experiencing the longest administrative delays, and (ii) (perishable) food rather than non-food items since the former is reported over the week immediately prior to enumeration whereas the latter is reported over the month prior to enumeration.

Since $\tau_{10} < 0$, which is consistent with binding credit constraints, it follows that $\tau_{20} \approx 0$ is consistent with precautionary savings behavior (motivated by credit constraints). This claim hinges on the two groups of beneficiaries being identical at baseline. Otherwise, that $\tau_{20} \approx 0$ might also be consistent with the absence of borrowing constraints *or* hand-to-mouth consumption behavior being undetectable given the low frequency data constraints (i.e., all of the transfer was spent on goods outside the recall window). We provide evidence against this latter concern in Section 4.4 below. This same logic suggests that all beneficiaries must have engaged in precautionary savings out of the first disbursement in late 2005. Hence, $\tau_{10} < 0$ implies that those precautionary savings were not substantial enough to buffer against the (long, unexpected) delay in the second disbursement.

A consumption smoothing framework with borrowing constraints can also explain why the differential treatment effects in Table 5 dissipate by 2007 (i.e., $\tau_{20} = \tau_{10} = \tau_{21} \approx 0$). The shock associated with delayed disbursements no longer holds as all recipients received the four quarterly transfers as expected by the end of 2006 ahead of enumeration in early 2007. Recall that the full set of quarterly disbursements still constitutes a positive shock over the two-year time horizon between early 2005 and early 2007. Hence, it seems plausible that these null results are explained in part by

[29] The latter is possible in the absence of borrowing constraints (e.g., with prudence), but given the salience of credit market imperfections in developing countries like Indonesia, it is helpful to make an assumption of quadratic preferences wherein households self-insure against future income uncertainty through precautionary savings.

households undertaking precautionary savings ahead of the anticipated end of the program in late 2006.

The evidence from the short-term period suggests that Indonesian households exhibit a MPC out of transitory income transfers that is larger than would be predicted in a standard PIH-type model of consumption smoothing without borrowing constraints (and precautionary savings). Real interest rates were around zero at this time as high inflation due to the fuel subsidy cutbacks offset the otherwise high nominal interest rates prevailing in financial markets. Several explanations for excess sensitivity have been proposed in the literature on consumption smoothing. We argued above that the leading explanation in our setting is that borrowing constraints made it difficult to smooth negative shocks and also induced a strong precautionary savings motive in the face of uncertainty about future policy reform and inflation. We turn now to examine additional ways in which credit constraints might affect our results and interpretation.

4.2 Financial Institutions Moderate the Expenditure Response to Transfers

There are several reasons why credit constraints can lead to departures from perfect consumption smoothing. In this section, we show how the expenditure response to the UCT program may depend on financial access. Although we do not observe a meaningful, direct measure of financial access (or savings) in our household level data, proximity to banks therefore serves as a reasonably exogenous proxy for the liquidity of local credit markets and availability of savings instruments which might facilitate consumption smoothing.[30] We investigate this possible interaction with the program by augmenting equation (1) with an indicator for bank presence and its interaction with treatment indicators. This test is akin to an approach pursued in the literature since Zeldes (1989) whereby one splits the sample on the basis of some exogenous determinant of liquidity in order to identify departures from the standard consumption Euler equation without borrowing constraints.

For every village in the *Susenas* data, we identify a “bank nearby” if there are any banking institutions operating within the village’s subdistrict in early 2005 (see Appendix C). Around 75 (80) percent of treatment (control) households reside close to at least one banking institution. This measure includes, among others, *Bank Rakyat Indonesia*, which has been hailed as the world’s largest microfinance *and* microsavings institution (Robinson, 2002). Gertler et al. (2009) provide evidence that the location choices of many banking institutions is exogenous with respect to spatial differences in wages prevailing across Indonesia.

Overall, the results in Table 9 imply that the presence of financial institutions dampens the expenditure response to cash transfers. First, the negative short-term expenditure shock from experiencing a late second disbursement is almost entirely offset by proximity to banks. The (precisely estimated) null coefficient τ_{10}^{banked} suggests that expenditure growth for single disbursement recip-

[30]There is a measure of household-level credit access in *Susenas*, but it is limited to a very specific type of credit, use of which is too limited for econometric purposes (less than 1.5 percent of households report any use in 2005). However, it stands to reason that a non-trivial fraction of our sample households do have access to some sort of credit and savings instruments. According to axuiliary data from the fourth wave of the Indonesian Family Life Survey from 2007, around 15 (17) percent of former UCT recipient (non-recipient) households report having access to credit in the last year. Moreover, 12 (30) percent of former UCT recipient (non-recipient) households report access to savings accounts.

ients near banks is statistically indistinguishable from non-recipients. At the same time, the difference between recipient groups also dissipates among those UCT beneficiaries located near banks. Second, proximity to banks seems to have no differential effect on UCT recipients that obtained the second disbursement early in 2006 (compare τ_{20}^{banked} and $\tau_{20}^{unbanked}$). This is intuitive since, on average, these households did not experience any surprise shocks in response to which they would have needed to draw on local financial markets. Last, by early 2007, we observe that UCT recipients residing near banks have slightly higher expenditure growth than recipients residing farther away from banks ($\tau_{d0}^{banked} > \tau_{d0}^{unbanked}$ for $d = 1, 2$). These differences suggest that local economic vitality and expenditure growth trends may be correlated with bank presence, the cross-sectional findings in Gertler et al. (2009) notwithstanding. Hence, this final result in Table 9 could be explained by banked and unbanked recipient households returning to their differential pre-program expenditure trends several months after receiving the final UCT disbursement.[31]

4.3 Age Heterogeneity in the Expenditure Response to Transfers

In this section, we consider two interesting implications of the life cycle or finite horizon version of the permanent income hypothesis that are (indirectly) testable in our setting. First, in a setup with or without borrowing constraints, the consumption response to transitory income shocks should be increasing in age as individuals near retirement at the end of the life cycle. Second, in a setup with borrowing constraints, younger households with limited savings should exhibit larger responses to transitory income shocks. Blundell et al. (2008) and Kaplan and Violante (2010) provide evidence supporting this U shaped age profile of the MPC out of income shocks. To capture these potential nonlinearities, we augment equation (1) with a quadratic in the age of the household head and the interactions of those terms with the two treatment indicators.

The key results of this exercise are summarized succinctly in Figure 6, which plots the marginal effects, $\boldsymbol{\tau}$, across ages in the data. Although the estimates are imprecise (see Appendix B.2), we find an age profile of treatment effects that is consistent with the hypotehesized convex relationship. In particular, the consumption responses implied by τ_{10} and τ_{20} are largest for (i) the youngest Indonesian households for whom cash-in-hand constraints are most likely binding, and (ii) the oldest households with the lowest annuity value of savings.

4.4 Robustness, Spillovers, and Rainfall

In this section we show that the main empirical results (i) hold up to a number of robustness checks, (ii) cannot be explained by systematic spillovers to the control group via local redistribution, and (iii) are consistent with household responses to other transitory income shocks due to rainfall.

[31] We also explore whether household members working abroad may be another vehicle for smoothing consumption *ex post*. Yang and Choi (2007), for example, find that remittances can act as insurance against negative rainfall shocks in the rural Philippines. In our setting, households still awaiting their second quarterly disbursement may have been able to draw on remittances from family members abroad to smooth the negative income shock. In results available upon request, we take a similar approach as in Table 9 (replacing bank nearby with an indicator for migrant abroad) and find some evidence for this mechanism, but the results are not particularly robust.

Robustness Checks. In Table 10, we illustrate a battery of robustness checks using the control function estimates for the period from 2005–2006 as a baseline.[32]

Timing of the Midline Survey. Our identification strategy relies on differences in the disbursement schedule across households enumerated at roughly the same point in time in early 2006. Using a coarse indicator for later enumeration, we saw in Table 6 that the negative shock experienced by recipients with delayed disbursements is larger for those enumerated in March than in February 2006. We further ensure that differential enumeration dates are not driving our results by including exhaustive dummies for the 65 distinct days of enumeration across the country. Doing so in row 2 of Table 10 leaves the results unchanged from the baseline estimates in row 1.

Alternative Geographic Fixed Effects Specification. Row 3 shows that results are robust to including district fixed effects. However, as expected based on the results in Table 3, including subdistrict (village) fixed effects in row 4 (5) removes nearly all of the exogenous variation in the staggering of the second quarterly transfer and pushes the estimates closer to a simple binary treatment effects specification. These specifications serve more as a confirmation of the identifying variation than as a robustness check per se.

However, if one is willing to assume that geography only affects consumption growth through its effect on the timing of transfers, then (sub)district FE can also be used as instrumental variables (IV) for the arrival of the second disbursement. We show in Appendix B.4 that this alternative identification strategy leads to qualitatively similar albeit less precise results. The IV estimates are, however, larger by around 50 percent—an increase that is consistent with attenuation bias in the OLS estimates due to our inability to measure the actual date that each beneficiary received (or was scheduled to receive) the first and second transfer disbursements (see Souleles, 1999, for a related argument in the context of tax refunds in the U.S.). Because the (sub)district FE capture most of the variation in the timing, the IV estimates can reduce (some of) the measurement error.[33]

Trimming Extreme Expenditures. In rows 6 and 7, the key qualitative results do not change if we trim (i) the top and bottom percentile of $\Delta \ln C_{ht}$, or (ii) the top and bottom percentile of $\ln C_{h,t-1}$.

Regional Differences in Inflation. By including province fixed effects, we remove trend differences across regions in terms of inflation and hence of the passthrough from fuel price increases to other consumer goods. We take two additional steps in rows 8 and 9 to ensure that local price differences are not driving our results. First, we deflate nominal expenditures using the nearest official regional CPI measures (see Appendix C). Second, we deflate using the price of the goods basket used to

[32] More detailed tables for all of the robustness checks can be found in the Online Appendix. In unreported results, we also consider alternative estimators for the binary treatment effect of receiving any UCT benefits including nearest-neighbor matching (Abadie and Imbens, 2005), local linear matching (Heckman et al., 1998), and inverse probability tilting (IPT) (Graham et al., 2012). In all cases, the main qualitative and quantitative findings remain unchanged from those binary treatment effect estimates recoverable from Table 5. Moreover, as we show in the Online Appendix, there does not appear to be heterogeneity in the multivalued treatment effects across the distribution of expenditure growth (i.e., the effects at the mean are statistically indistinguishable from the effects at other quantiles).

[33] However, as discussed in the Appendix, it is also possible that some of the increase in the coefficient estimates is attributable to weak identification or to a violation of the exclusion restrictions if poorer (or richer) districts exhibit slower consumption growth for other reasons.

construct the district-specific poverty lines. Neither approach materially affects our estimates of $\boldsymbol{\tau}$.[34]

Alternative "per capita" Formulations. Some authors argue that when looking at household expenditure outcomes, one should account for the fact that children require less consumption than adults to attain equivalent levels of welfare (see Deaton, 1997; Olken, 2006). To the extent that household composition differs across treatment and control groups (see Table 2), this could impact our results. Ultimately, though, this adjustment is irrelevant as the baseline estimates in row 1 are indistinguishable from those in rows 10 and 11 where we treat children aged 0-9 years old as 0.5 adult-equivalents for total and food expenditures, respectively.

Durable Goods Expenditures Beyond the Last Month. In the baseline regressions, we measure durable goods expenditures in the last month. In so doing, we may have missed important purchases using UCT funds prior to the survey enumeration period in early 2006. In other words, the UCT may have led to an increase in expenditures several months prior to enumeration and perhaps immediately after UCT receipt in October–December 2005. Hence our comparison of durable goods purchases in the early months of 2005 and 2006 might understate the large positive effects of the UCT had we compared those purchases going back over the full year prior to enumeration. This does not seem to be the case. Using a (pro-rated) measure of annual non-food expenditures in row 12 leaves our key parameter estimates unchanged. This provides additional evidence of asymmetric expenditure responses to positive and negative shocks. That is, the positive income shock associated with the first disbursement leads to a smaller expenditure impact than the negative shock associated with the delayed second disbursement.

Participation in Other Social Programs. Several other previously operative social programs continued alongside the UCT. Receipt of such programs might confound our estimates of $\boldsymbol{\tau}$ parameters if, for example, the UCT disbursement schedule was timed so as to reach those households lacking other programs first. In row 13, we control for participation in other programs—a rice subsidy scheme, scholarships for poor students, and subsidized health insurance for the poor—and the results remain similar to the baseline.

Idiosyncratic Health Shocks. In row 14, we show that the results are unchanged when conditioning on changes in the incidence of health shocks within the household between 2005 and 2006. This is reassuring given that health shocks are potentially important time-varying omitted variables correlated with both treatment assignment and expenditures.

Local Natural Disasters. In row 15, we control for the incidence of local natural disasters from 2003-5 and find no systematic departures from the baseline estimates of $\boldsymbol{\tau}$. This is reassuring given the slight correlation found in Table 4 between late disbursement and these other shocks to (income and hence) expenditures arising from natural disasters.

[34]In results available upon request, we show that the UCT program led to a statistically significant albeit economically trivial effect on local prices.

Other Covariates of Staggering in Table 4. We go a step further in row 16 and control for *all* covariates used to explain staggering in Table 4. Doing so leaves the main baseline findings largely unchanged. However τ_{21} falls slightly, suggesting that regional characteristics correlated with disbursement timing may explain some of the differential expenditure growth across recipients.

Program Enrollment and Systematic Underreporting of Expenditures. In the midst of public scrutiny over perceived program leakage and undercoverage, it is possible that UCT recipients and particularly those still awaiting their second disbursement systematically underreported their expenditures.[35] This would lead to non-classical measurement error and could bias the treatment effects downward if recipients perceived their ongoing participation as being contingent upon reported welfare levels. We (partially) test for this by controlling for whether the household was assigned to the initial list by the village head (potentially more prone to patronage) or by a regional government official outside the village (less prone to patronage). The drop in magnitude and significance of τ_{10} in row 17 of Table 10 relative to Table 5 provides some indirect evidence in support of this mechanism. Conditional on official program enumerator visits, there are no longer statistically significant differences between control households and recipients of a single transfer.

Ruling Out Spillover Effects. We can also provide indirect evidence that potential spillovers to control households do not explain the expenditure impacts reported in Table 5. We proceed in three steps. First, we identify villages in which UCT recipients report any informal taxes and redistribution during disbursement round one or two.[36] Second, we assign all control households in these villages to treatment group $D = 1$ ($D = 2$) if any informal taxes are observed for disbursement one (two). Third, we re-estimate the propensity scores and finally the key parameters in Table 5. We find that τ_{10} remains largely unchanged while τ_{21} and τ_{20} fall relative to the baseline estimates (see Appendix B.3). This is the opposite of what we should observe if spillovers were contaminating the control group and hence raising their expenditures and systematically biasing our baseline treatment effects downward. Of course, these results should be interpreted with caution as not all non-recipients benefited from informal redistribution of UCT taxes within given villages and our test implicitly assigns equal transfers across households.

Other Transitory Income Shocks. Following others in the development literature beginning with Wolpin (1982), we examine the expenditure response to rainfall shocks as another test for excess sensitivity to changes in transitory income.[37] The key message from Table 12 is that these shocks are associated with higher growth in household expenditures per capita among rural households engaged in agriculture. In column 3, we find that a 10 percent deviation of rainfall from its long-

[35]The first few months of the UCT program in 2005 generated considerable public controversy surrounding the allocation of benefits and widespread perception of mistargeting (see Cameron and Shah, 2012).

[36]Out of 538 villages with any UCT recipients, we find that 17 (16) report informal taxes during the first (second) disbursement period, and 23 report informal taxes in both periods. Among those taxed, the median amount also increased from 20,000 Rp to 50,000 Rp. The portion allocated to supposed local redistribution increased from 40 percent at the first disbursement to 62 percent at the second disbursement.

[37]The transitory rainfall shock in year t is defined as the log rainfall level in village v's district over the province-specific growing season minus the log mean rainfall level for that district over the forty years/seasons prior to t.

run mean yields roughly a 1.8 percent increase in expenditures per capita. We find in column 4 a similarly positive elasticity for land-owning households, and in column 5 the elasticity is increasing in land-holding size—though only the interaction terms are significant in both cases.[38]

4.5 Intensive Margin Treatment Effects: Transfer Size Matters

Having found that the PIH under borrowing constraints can explain the consumption response to the UCT program, we now provide evidence that the size of the transfers matters as well. In particular, we the fact that the transfer size per disbursement was fixed across households of varying size. This allows us to estimate the following equation:

$$\begin{aligned} \Delta \ln C_{h,t+1} &= \alpha + \tau_{10}\mathbf{1}\{D_h > 0\} + \tau_{21}\mathbf{1}\{D_h = 2\} \\ &\quad + \psi \, transfers_h/capita_h + \sum_{j=2}^{13} \beta_j \mathbf{1}\{HHsize_h = j\} + \Delta\eta_{h,t+1}, \end{aligned} \tag{2}$$

where $transfers$ is the total amount of UCT funds (in 100,000s of Rupiah) received by enumeration in early 2006, and $capita$ and $HHsize$ are household size. After removing (i) the multivalued treatment effects through reweighting and the disbursement indicators, and (ii) the independent effects of household size through β_j terms, all that remains is information on the scale or intensity of UCT benefits. Under the assumptions verified in Section 3 as well as $\mathbb{E}[HHsize_{ht}\Delta\eta_{ht}] = 0$ (after reweighting), ψ identifies the marginal effect of an additional unit of transfer income per capita.

In Table 11, we find robust positive estimates of ψ. Columns 1-3 impose $\beta_j = 0$ for all j, and column 4 permits $\beta_j \neq 0 \; \forall j$ to allow for unconditional scale effects in the growth . The point estimates of 0.04-0.065 for total expenditures per capita suggest that an increase in household transfers per capita by Rp 33,000 raises consumption by Rp 9,000 per person per month. The implied MPC is slightly lower than that found in Section 4.1 based on the estimated consumption loss due to the delayed disbursement. This lower MPC is consistent with the transfer size being fairly predictable (and well publicized) and consumption being more sensitive to unexpected income losses than (un)expected income gains. Coupled with the evidence in Section 4.2, these results provide additional evidence in support of a consumption smoothing framework with credit constraints and precautionary savings.

5 Conclusion

This paper has demonstrated the importance of incorporating timing and expectations into the evaluation of the expenditure response to cash transfers in a developing country context. We investigate the effects of a large scale, temporary UCT program on consumption smoothing behavior in Indonesia. On average, beneficiary households that received the full two transfers as expected by early 2006 do not differ from comparable non-beneficiaries in terms of per capita expenditure

[38] Although rainfall shocks only affect the transitory income and consumption of certain segments of the (rural) population, we do not heterogeneous effects along these same dimensions (see the Online Appendix).

growth. However, beneficiaries still awaiting their second transfer report significantly lower expenditure growth especially in areas with limited financial access. These growth differentials dissipate by early 2007, several months after the final transfer was received by all beneficiaries. Using the fact that the transfer amount per disbursement was fixed across households, we also identify an auxiliary marginal propensity to consume out of transfer income that is consistent with the short-run differential treatment effects.

More broadly, our paper offers new insights into the expenditure impacts of large-scale fiscal interventions in developing countries. Unlike numerous programs in Latin America and elsewhere, the UCT in Indonesia was not explicitly designed as a transformative poverty alleviation program. Rather, the government used the program as a means of transitioning away from regressive fuel subsidies. Similar subsidy reforms have either recently been implemented or are being considered in a number of developing countries (Coady et al., 2010). Our results from Indonesia suggest that the aggregate effects of cash transfers in such contexts may hinge on perceived program duration as well as the timing of the transfers with respect to subsidy cutbacks. Accurate microeconomic estimates of the effectiveness of fiscal stimulus programs are an important input to public policy. In order to understand the full, policy-relevant impact of these (and other) programs, evaluators must explicitly test for behavior change at various times before, during, and after the actual interventions.

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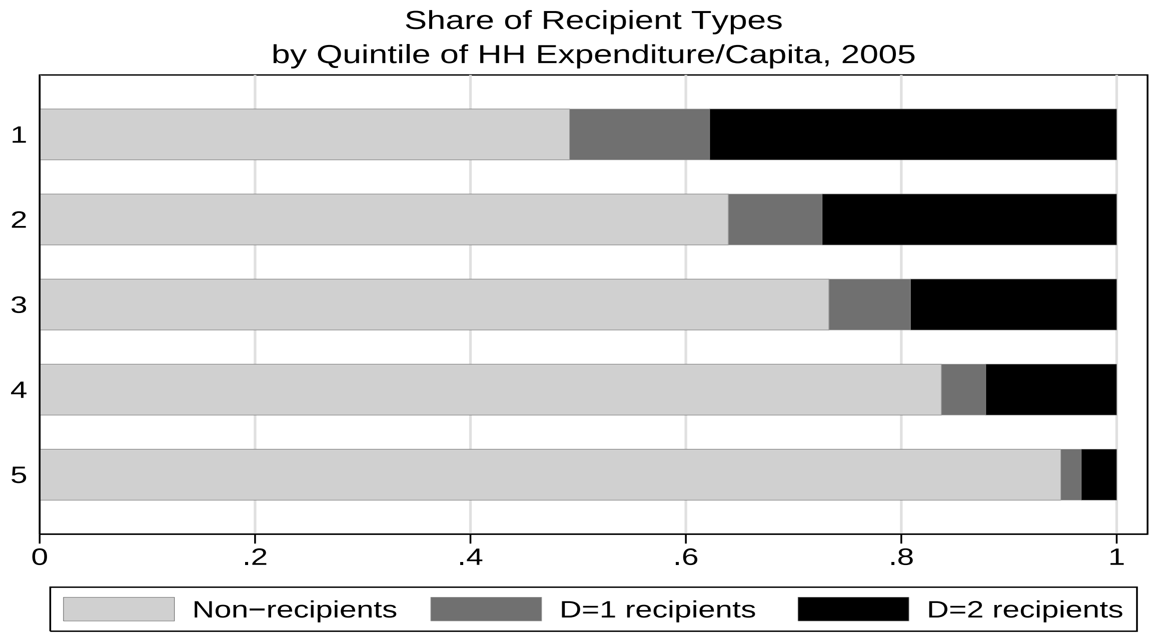
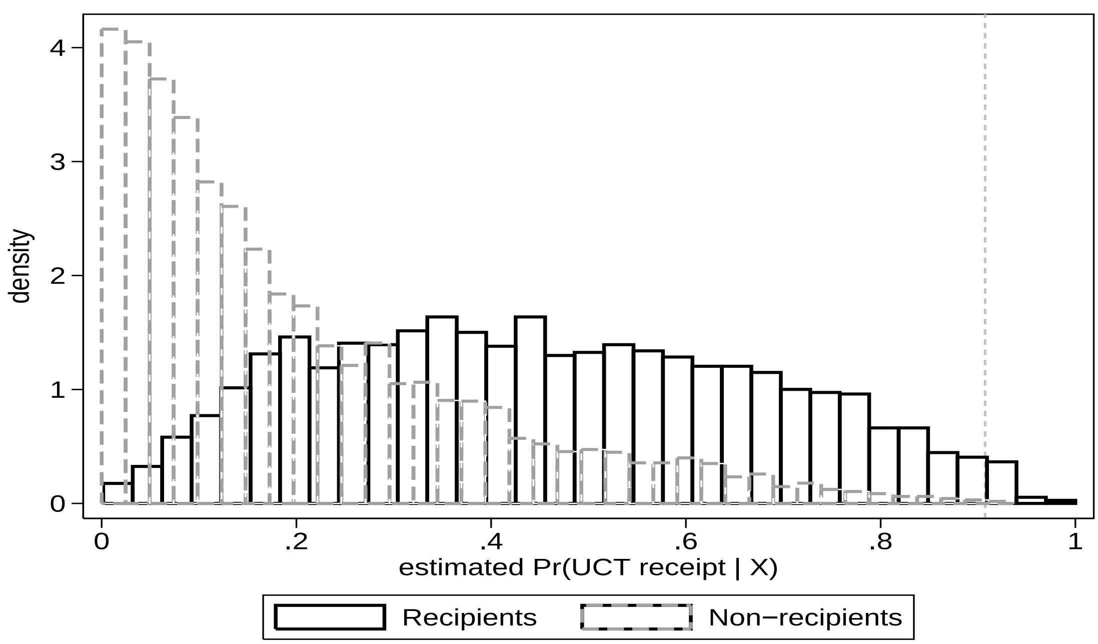
Figures

Figure 1: Treatment Level by Baseline Expenditure Decile



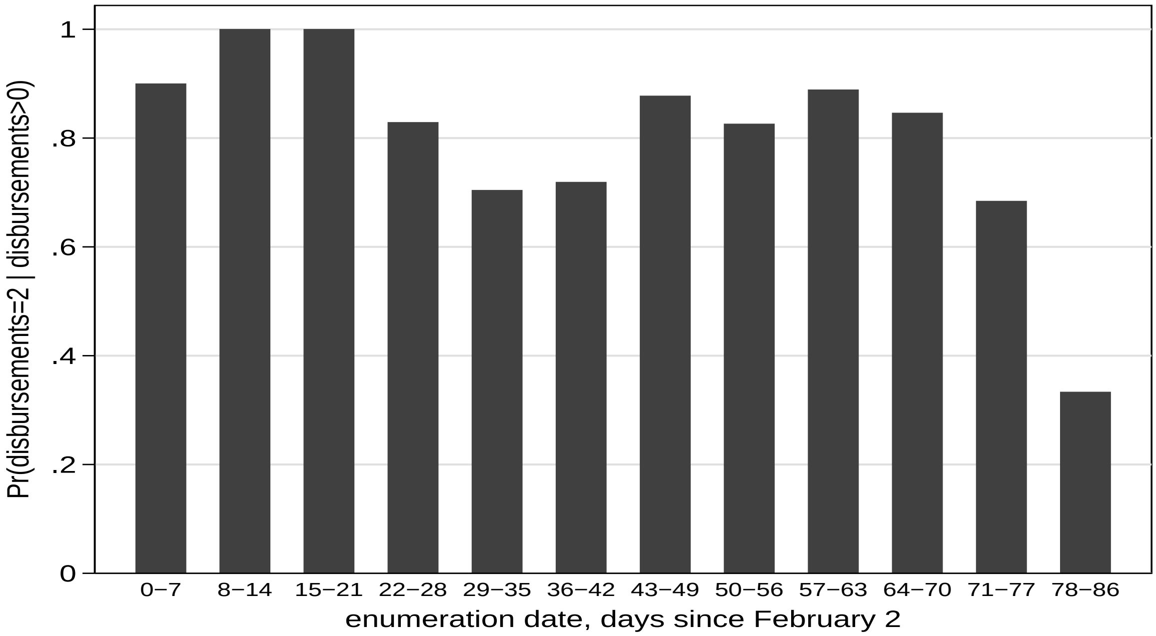
Notes: D denotes the number of UCT disbursements d received by enumeration in early 2006 as reported in a module attached to *Susenas* 2006. The quintile of household expenditures per capita is based on data reported in *Susenas* 2005.

Figure 2: Overlap in Estimated Propensity Scores ($\widehat{P}$)



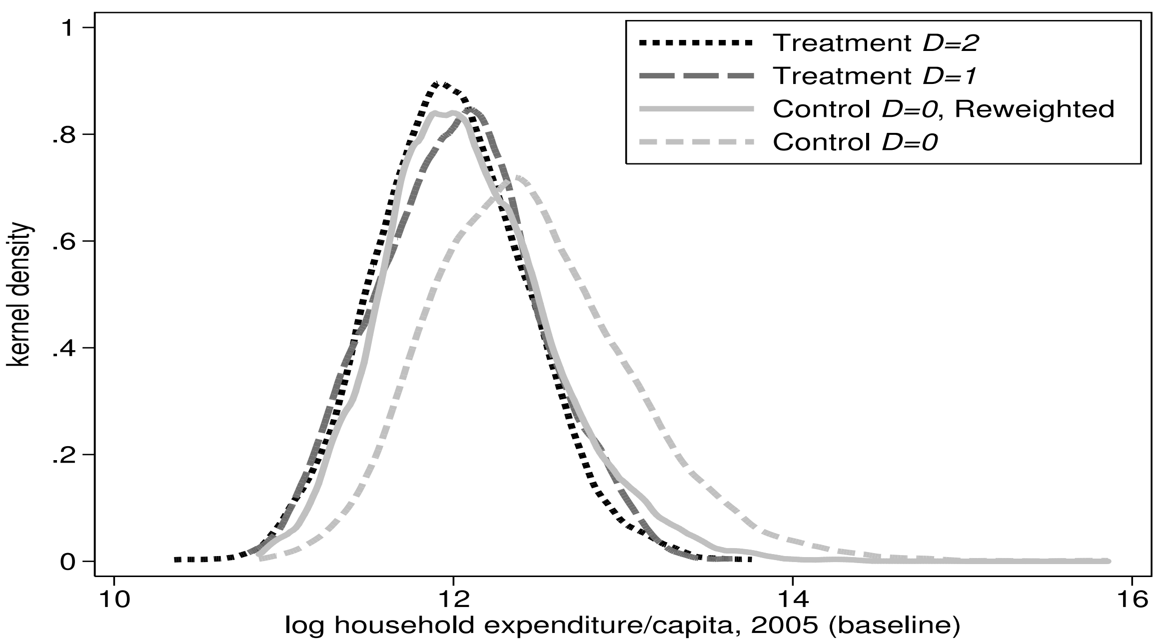
Notes: Propensity scores obtained from flexible logit regressions (see Table 2). Observations to the left of the dashed vertical line fall within the Crump et al. (2009) optimal overlap region.

Figure 3: Staggered Disbursements and Survey Enumeration Date



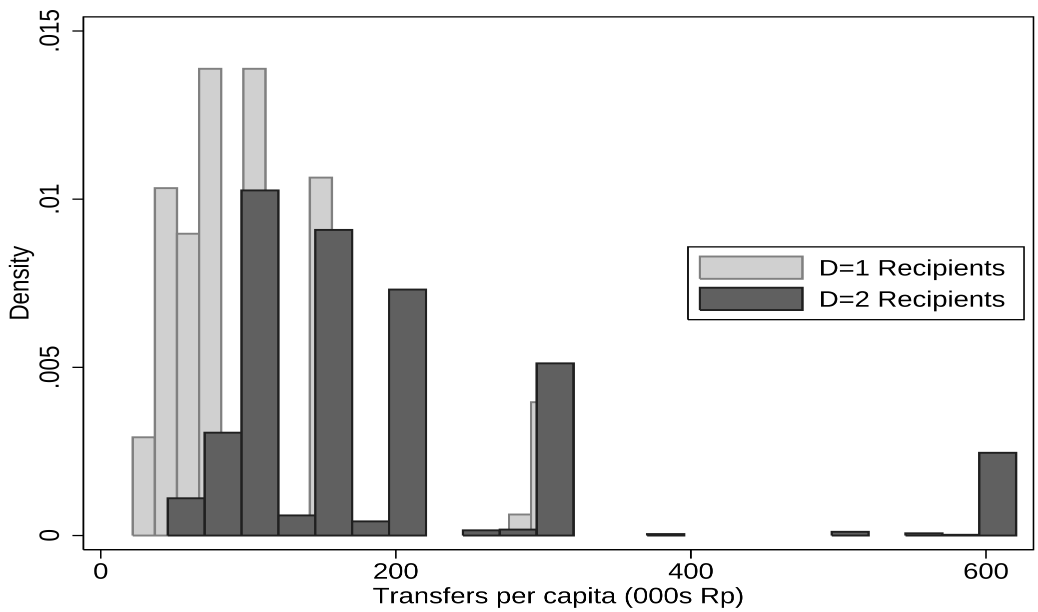
Notes: The bars represent the share of UCT beneficiary households with two disbursements at the time of being visited by *Susenas* enumerators in early 2006. The vast majority of enumeration dates fall between February 15 and March 25.

Figure 4: Baseline Expenditure Distributions by Treatment Status



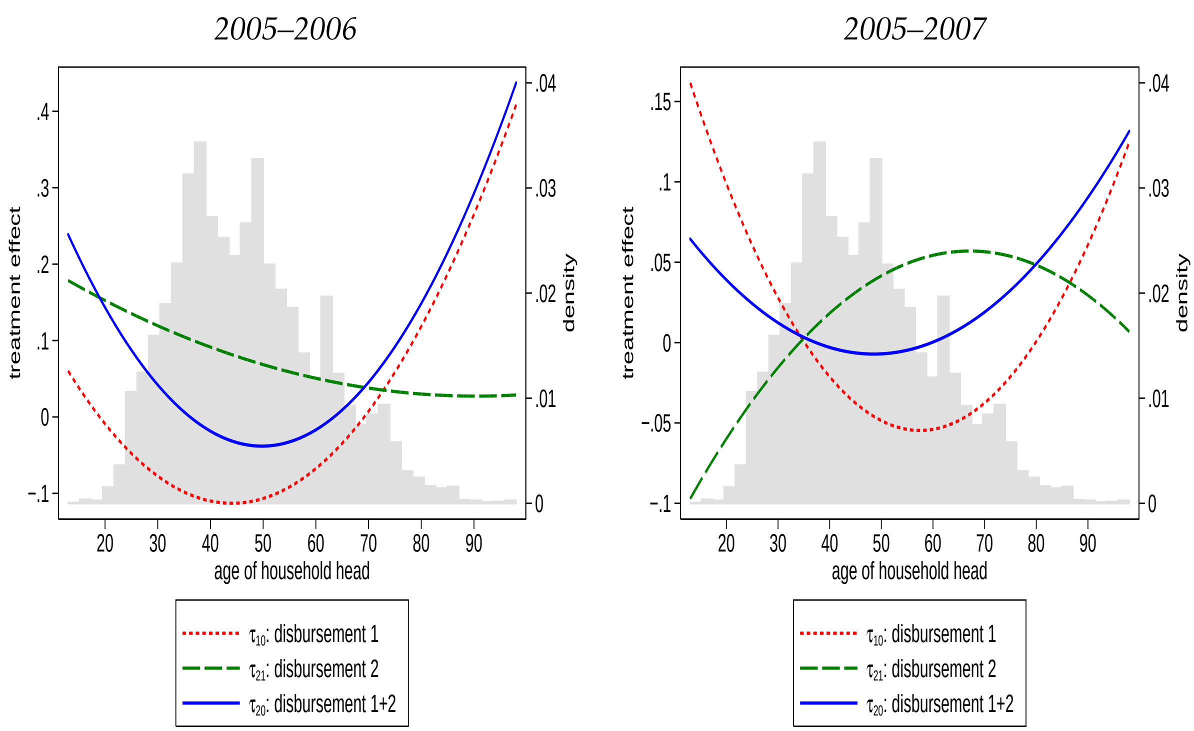
Notes: D denotes the number of UCT disbursements d received by early 2006. All distributions estimated using Epanechnikov kernel and a rule-of-thumb bandwidth. The "Control (Reweighted)" observations are adjusted using inverse probability weights (IPW) based on normalized estimated odds of treatment $\widehat{\omega} = \widehat{P}/(1 - \widehat{P})$.

Figure 5: Distribution of Transfers per Capita through early 2006



Notes: D denotes the number of UCT disbursements d received. The transfer amount reported per disbursement is also reported *Susenas* 2006.

Figure 6: Age-Specific Treatment Effects



Notes: Each curve represents the treatment effects coefficients estimated across household head ages. The marginal effects are recovered from a regression allowing the τ parameters to vary with a quadratic in age.

Tables

Table 1: Expenditure statistics, 2005 and 2006

	2005					2006				
	Mean	**SD**	**Min**	**Median**	**Max**	**Mean**	**SD**	**Min**	**Median**	**Max**
	Non-recipients ($N = 6604$)									
Expenditure/capita (000s Rp)	315	292	52	243	7702	356	300	31	272	4891
Food expenditure/capita (000s Rp)	162	93	30	138	2790	182	104	20	155	1141
Non-food expenditure/capita (000s Rp)	153	234	8	94	7071	174	228	0	108	4236
Education expenditure/capita (000s Rp)	11	60	0	2	2269	8	41	0	0	1660
Health expenditure/capita (000s Rp)	11	67	0	2	2607	10	62	0	2	3137
Below poverty line	0.10	0.30	0	0	1	0.11	0.31	0	0	1
Quintile (nat'l) expenditure/capita	3.23	1.38	1	3	5	3.28	1.37	1	3	5
Quintile (intra-province) expenditure/capita	3.21	1.39	1	3	5	3.26	1.38	1	3	5
	$D = 1$ ***Recipients*** ($N = 639$)									
Expenditure/capita (000s Rp)	185	93	49	165	843	195	118	41	170	1817
Food expenditure/capita (000s Rp)	121	60	32	110	761	123	62	30	110	422
Non-food expenditure/capita (000s Rp)	65	49	9	52	423	72	80	9	56	1581
Education expenditure/capita (000s Rp)	2	6	0	0	220	2	4	0	0	48
Health expenditure/capita (000s Rp)	10	83	0	1	1832	4	11	0	1	150
Below poverty line	0.25	0.43	0	0	1	0.34	0.47	0	0	1
Quintile (nat'l) expenditure/capita	2.25	1.21	1	2	5	2.14	1.18	1	2	5
Quintile (intra-province) expenditure/capita	2.36	1.27	1	2	5	2.27	1.25	1	2	5
	$D = 2$ ***Recipients*** ($N = 1805$)									
Expenditure/capita (000s Rp)	178	90	31	159	945	192	92	37	172	908
Food expenditure/capita (000s Rp)	115	54	17	104	645	124	57	23	112	484
Non-food expenditure/capita (000s Rp)	63	50	9	50	576	68	51	0	55	682
Education expenditure/capita (000s Rp)	3	8	0	0.4	220	2	5	0	0	68
Health expenditure/capita (000s Rp)	5	11	0	2	178	5	22	0	1	751
Below poverty line	0.28	0.45	0	0	1	0.31	0.46	0	0	1
Quintile (nat'l) expenditure/capita	2.12	1.16	1	2	5	2.11	1.12	1	2	5
Quintile (intra-province) expenditure/capita	2.28	1.25	1	2	5	2.27	1.22	1	2	5
	Attritors ($N = 771$)									
Expenditure/capita (000s Rp)	323	272	54	252	2927					
Food expenditure/capita (000s Rp)	180	119	38	150	1073					
Non-food expenditure/capita (000s Rp)	142	197	10	86	2497					
Education expenditure/capita (000s Rp)	7	27	0	0.4	563					
Health expenditure/capita (000s Rp)	13	55	0	2	750					
Below poverty line	0.14	0.35	0	0	1					
Quintile (nat'l) expenditure/capita	3.23	1.41	1	3	5					
Quintile (intra-province) expenditure/capita	3.24	1.42	1	3	5					

Notes: See Appendix C for details on the panel construction. $D = d$ recipients obtained d UCT disbursements by enumeration in early 2006. Attritors are those households which could be identified in the 2005 baseline survey but not in the subsequent rounds. *Rp* stands for *Rupiah*. The exchange rate fluctuated between $9,500$ Rp and $10,500$ Rp to the dollar between October 2005 and September 2006. All expenditure variables are household per capita expressed in *Rupiah* per month. The underlying food expenditure items are recorded for the week prior to enumeration and scaled up to the monthly level by the factor $30/7$. The underlying non-food expenditure items are recorded for the year prior to enumeration and scaled down to the monthly level by the factor $1/12$. *Below poverty line* is an indicator for whether or not the household's total expenditures per capita fell below the district rural or urban poverty line in the given year. Per capita expenditure quintiles are computed separately within the full national sample and within the 31 provinces in which sample households reside. The 2005 quintiles are calculated including attritors.

Table 2: Marginal Effects in the Propensity Score Model, $Pr(disbursements_h > 0|\mathbf{X}_h)$

	coefficient	**(std. error)**		**coefficient**	**(std. error)**
Urban Area	-0.138	(0.091)	*Housing status (reference = other)*		
HH Head Female	0.609	(0.110)***	Own house	-0.126	(0.102)
Land owned (hectares)	-0.112	(0.036)***	Lease house	-0.172	(0.242)
Land owned^2 (hectares)	0.001	(0.000)***	Rent house	-0.454	(0.252)*
HH ever participate Rice for the Poor	0.889	(0.075)***	Free house	-0.021	(0.207)
# children in school	-0.101	(0.063)	Official house	-0.937	(0.297)***
# children in school ^{2}	0.024	(0.017)			
Indicators for Household Size$\in \{2, \ldots, 12\}$	—	[0.222]	*Roof type (reference = other)*		
Floor area	-0.006	(0.002)***	Concrete roof	-0.697	(0.473)
			Tile roof	-0.378	(0.341)
Household composition (reference = share adult males, 10+ years)			Shingle roof	-0.121	(0.368)
Share Female Children, 0-9 yrs	0.334	(0.289)	Iron roof	-0.185	(0.303)
Share Male Children, 0-9 yrs	0.491	(0.266)*	Asbestos roof	-0.012	(0.464)
Share Adult Females, 10+ yrs	-0.090	(0.193)	Fiber/Thatch roof	0.021	(0.302)
Primary household income source (reference = other)			*Floor type (reference = other)*		
Trade/Retail	-0.155	(0.102)	Brick wall	-0.205	(0.259)
Financial/Real Estate	-0.575	(0.186)***	Wood wall	0.218	(0.282)
Agriculture	0.103	(0.075)	Bamboo wall	0.542	(0.287)*
Mining	-0.120	(0.120)	Cement/Tile/Plaster floor	-0.006	(0.473)
Manufacturing	0.198	(0.117)*	Wood/Reed/Bamboo floor	0.218	(0.484)
Electricity/Gas/Water	0.422	(0.669)	Earthen floor	0.592	(0.481)
Construction	0.307	(0.094)***			
			Souce of drinking water (reference = other)		
Household head education level (reference = no education)			Bottled water	-0.987	(0.474)**
Primary	-0.271	(0.115)**	Pump water	-1.092	(0.252)***
Junior secondary	-0.558	(0.162)***	Tap water	-0.473	(0.295)
Senior secondary	-1.089	(0.139)***	Protected well water	-0.740	(0.249)***
Higher	-2.388	(0.511)***	Unprotected well water	-0.820	(0.265)***
			Protected spring water	-1.072	(0.280)***
Toilet facilities (reference = other)			Unprotected spring water	-1.024	(0.304)***
Own toilet	-0.254	(0.176)	River water	-0.840	(0.322)***
Shared toilet	-0.043	(0.166)	Rain water	-0.462	(0.376)
Public toilet	-0.031	(0.199)	Buy drinking water	-0.151	(0.179)
Source of light (reference = other)			*Toilet disposal location (reference = other)*		
PLN electricity	-0.450	(0.642)	Septic tank	-0.321	(0.150)**
Non-PLN electricity	-0.681	(0.763)	Pond/Rice field	-0.114	(0.225)
Pump lantern	0.352	(0.702)	Lake, river, sea	-0.106	(0.169)
Oil lamp	0.028	(0.639)	Beach	-0.105	(0.149)
Constant	1.216	(0.895)			
Pseudo-R^2	0.23				

Notes: Significance levels: $*$ 10% $**$ 5% $***$ 1%. This table reports average marginal effects for the estimates of the propensity score model in equation (A.1) based on a logit specification. The sample includes the 9,048 households comprising the balanced constructed from *Susenas* 2005 and 2006. Standard errors are clustered by village. All variables are as reported in February-March 2005. The coefficients on the indicators for household size are suppressed, but the p-value $= 0.222$ for the joint significance of those indicators is reported in brackets. The regression also controls for province fixed effects. *PLN* is the state-run electricity firm.

Table 3: Idiosyncratic vs. Spatial Variation in Staggering

Fixed Effects	Province	District	Subdistrict	Province	District	Subdistrict
$\mathbf{X}_{h,t-1}$ controls	No	No	No	Yes	Yes	Yes
Dependent variable	$Pr(disbursements > 0)$					
	(1)	(2)	(3)	(4)	(5)	(6)
$H_0: \boldsymbol{\beta}_X = \mathbf{0}$						
F statistic	—	—	—	31.35	29.03	28.69
[p-value]	—	—	—	$[< 0.001]$	$[< 0.001]$	$[< 0.001]$
R^2	0.053	0.171	0.240	0.241	0.331	0.389
Dependent variable	$Pr(disbursements = 2 \mid disbursements > 0)$					
	(7)	(8)	(9)	(10)	(11)	(12)
$H_0: \boldsymbol{\beta}_X = \mathbf{0}$						
F statistic	—	—	—	3.01	1.81	0.90
[p-value]	—	—	—	$[< 0.001]$	$[< 0.001]$	[0.706]
R^2	0.266	0.810	0.895	0.327	0.821	0.898

Notes: $disbursements$ denotes the number of disbursements received by *Susenas* enumeration in early 2006. Linear probability regressions for $Pr(disbursements_{hv} = 2 \mid disbursements_{hv} > 0)$ and $Pr(disbursements > 0)$ are based on the sample of recipient and all households, respectively. The F tests correspond to a test of the null the hypothesis that all household-specific variables included in the $\mathbf{X}$ vector have no relationship with these probabilities. The R^2 are inclusive of the geographic fixed effects. There are 30 provinces, 339 districts, and 619 subdistricts.

Table 4: Staggering is Orthogonal to Observable Differences Across Regions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
days since Feb. 2	-0.002	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
log distance to subdistrict capital		0.057	0.048	0.043	0.043	0.045	0.038	0.035
		(0.039)	(0.040)	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)
log distance to district capital		-0.040	-0.030	-0.024	-0.026	-0.026	-0.021	-0.020
		(0.037)	(0.037)	(0.037)	(0.037)	(0.037)	(0.034)	(0.033)
log distance to Jakarta		0.018	-0.024	-0.021	-0.024	-0.019	-0.019	-0.009
		(0.032)	(0.041)	(0.040)	(0.040)	(0.044)	(0.043)	(0.040)
urban village		0.038	0.053	0.031	0.037	0.041	0.029	0.037
		(0.052)	(0.051)	(0.054)	(0.058)	(0.058)	(0.052)	(0.051)
village road paved		0.029	0.024	0.009	0.012	0.013	0.022	0.023
		(0.056)	(0.055)	(0.053)	(0.053)	(0.053)	(0.051)	(0.050)
village accessible only by water		-0.101	-0.103	-0.104	-0.101	-0.106	-0.117	-0.131
		(0.083)	(0.083)	(0.080)	(0.080)	(0.080)	(0.077)	(0.075)*
post office in village		-0.025	-0.065	-0.065	-0.064	-0.061	-0.060	-0.073
		(0.086)	(0.091)	(0.094)	(0.093)	(0.091)	(0.080)	(0.081)
log distance to post office		-0.016	-0.031	-0.009	-0.011	-0.010	-0.023	-0.030
		(0.034)	(0.035)	(0.037)	(0.037)	(0.037)	(0.034)	(0.035)
log district population, 2005			-0.083	-0.087	-0.086	-0.090	-0.097	-0.108
			(0.047)*	(0.046)*	(0.046)*	(0.047)*	(0.046)**	(0.045)**
bank in subdistrict				0.101	0.101	0.098	0.076	0.062
				(0.058)*	(0.057)*	(0.058)*	(0.057)	(0.055)
log mean HH exp./capita in district, 2005					-0.044	-0.049	-0.057	-0.058
					(0.087)	(0.088)	(0.090)	(0.086)
rainfall shock, 2005						0.076	0.081	0.117
						(0.168)	(0.156)	(0.154)
any mudslide, 2003-5							-0.177	-0.189
							(0.093)*	(0.093)**
any flood, 2003-5							-0.004	0.007
							(0.056)	(0.055)
any earthquake, 2003-5							-0.248	-0.240
							(0.122)**	(0.131)*
any fire 2003-5							-0.062	-0.064
							(0.079)	(0.079)
any other disaster, 2003-5							-0.001	0.018
							(0.090)	(0.093)
President's party 1st in village								0.137
								(0.090)
President's party 2nd in village								0.079
								(0.086)
President's party 3rd in village								0.075
								(0.095)
President's party 4th in village								-0.029
								(0.105)
Number of Households	2,349	2,349	2,349	2,349	2,349	2,349	2,349	2,349
R^2	0.001	0.020	0.035	0.047	0.048	0.049	0.091	0.105
$H_0:\ \boldsymbol{\beta}_X = \mathbf{0}$ [p-value]	0.470	0.591	0.120	0.024	0.017	0.026	0.001	0.0002

Notes: Significance levels: $*$ 10% $**$ 5% $***$ 1%. Linear probability regressions based on the sample of recipient households using the following specification: $Pr(disbursements_{hv} = 2 \mid disbursements_{hv} > 0) = \boldsymbol{\gamma}\mathbf{Z}_v + \upsilon_{hv}$, where $\mathbf{Z}_v$ is a vector of characteristics associated with the village v within which household h resides. Standard errors are clustered at the district level in all specifications. Distances to (sub)district capitals and post offices are based on actual travel distance; distance to Jakarta is great-circle. Further background on each of the variables can be found in Appendix C.

Table 5: Baseline Estimates of Multi-valued Treatment Effects, Short- and Medium-Term

Estimator	OLS	IPW	Double Robust		Control Function
			$(\widehat{P}_h)$	$(\mathbf{X}_h)$	
	(1)	(2)	(3)	(4)	(5)
			Short-Term: 2005-2006		
τ_{10}: disbursement 1	-0.062	-0.091	-0.091	-0.090	-0.076
	(0.027)**	(0.032)***	(0.030)***	(0.031)***	(0.030)**
τ_{21}: disbursement 2	0.049	0.074	0.077	0.072	0.076
	(0.030)*	(0.035)**	(0.032)**	(0.032)**	(0.034)**
$\tau_{20} \equiv \tau_{21} + \tau_{10}$: disbursements 1+2	-0.013	-0.017	-0.014	-0.018	0.000
	(0.014)	(0.020)	(0.020)	(0.018)	(0.019)
Reweighted	No	Yes	Yes	Yes	Yes
Propensity Score Control(s)	No	No	Yes	No	Yes
$\mathbf{X}_h$ Controls	No	No	No	Yes	No
Number of Households	9,011	9,011	9,011	9,011	9,011
R^2	0.045	0.100	0.104	0.181	0.119
			Medium-Term: 2005-2007		
τ_{10}: disbursement 1	-0.034	-0.056	-0.066	-0.044	-0.027
	(0.040)	(0.038)	(0.038)*	(0.032)	(0.037)
τ_{21}: disbursement 2	0.026	0.028	0.032	0.009	0.028
	(0.045)	(0.043)	(0.043)	(0.035)	(0.042)
$\tau_{20} \equiv \tau_{21} + \tau_{10}$: disbursements 1+2	-0.008	-0.028	-0.034	-0.034	0.001
	(0.020)	(0.023)	(0.024)	(0.023)	(0.023)
Reweighted	No	Yes	Yes	Yes	Yes
Propensity Score Control(s)	No	No	Yes	No	Yes
$\mathbf{X}_h$ Controls	No	No	No	Yes	No
Number of Households	6,992	6,992	6,992	6,992	6,992
R^2	0.044	0.055	0.062	0.144	0.068

Notes: Significance levels: $*$ 10% $**$ 5% $***$ 1%. The dependent variable in all specifications is Δlog total monthly household expenditures per capita between 2005 and 2006/2007. In the top panel, the constant term in columns 1 and 2 (i.e., average non-recipient log expenditure growth, or κ in equation (1)) equals 0.107 and 0.109, respectively. In the bottom panel, the constant term equals 0.113 and 0.153 in columns 1 and 2, respectively. Columns 2-5 are estimated by weighted least squares where the weights for treatment households equal one and the weights for control households are given by the normalized estimated odds of treatment $\widehat{\omega} = \widehat{P}/(1-\widehat{P})$, where the normalization is over the entire sample for the given time horizon. Using the Crump et al. (2009) procedure, we trim 37 households in the upper tail of the estimated propensity scores, $\widehat{P}$, and do the same in the OLS regressions for comparability. Column 3 controls linearly for the propensity score and column 5 for a fifth-order polynomial in the propensity score allowing it to vary by treatment and control. Column 4 controls for all covariates $\mathbf{X}_h$ used to estimate the propensity score. The coefficients on these additional terms in columns 3-5 are suppressed for presentational purposes. All columns include province fixed effects. Standard errors are clustered by village and computed over the entire two-step process using a block bootstrap with 1000 repetitions.

Table 6: Heterogeneity by Time of Survey and Length of Delay

Estimator	OLS	IPW	Double Robust		Control
			$(\widehat{P}_h)$	$(\mathbf{X}_h)$	Function
	(1)	(2)	(3)	(4)	(5)
enumerated in March	0.043	0.045	0.050	0.038	0.053
	(0.024)*	(0.041)	(0.040)	(0.035)	(0.041)
τ_{10}^{f}: disbursement 1	0.003	-0.042	-0.041	-0.026	-0.029
	(0.058)	(0.064)	(0.061)	(0.063)	(0.062)
τ_{10}^{m}: disbursement 1 $\times$ enumerated in March	-0.085	-0.064	-0.065	-0.083	-0.059
	(0.066)	(0.072)	(0.071)	(0.071)	(0.070)
τ_{21}^{f}: disbursement 2	-0.004	0.046	0.052	0.014	0.056
	(0.064)	(0.071)	(0.070)	(0.067)	(0.070)
τ_{21}^{m}: disbursement 2 $\times$ enumerated in March	0.071	0.037	0.033	0.076	0.027
	(0.072)	(0.076)	(0.075)	(0.075)	(0.074)
$\tau_{10}^{March} \equiv \tau_{10}^{f} + \tau_{10}^{m}$	-0.082	-0.105	-0.106	-0.109	-0.088
	(0.030)***	(0.037)***	(0.036)***	(0.033)***	(0.034)***
$\tau_{21}^{March} \equiv \tau_{21}^{f} + \tau_{21}^{m}$	0.067	0.083	0.085	0.090	0.083
	0.033)**	(0.037)**	(0.037)**	(0.034)***	(0.036)**
$\tau_{20}^{March} \equiv \tau_{21}^{f} + \tau_{10}^{f} + \tau_{21}^{m} + \tau_{10}^{m}$	-0.015	-0.022	-0.021	-0.019	-0.005
	(0.015)	(0.021)	(0.020)	(0.019)	(0.020)
$\tau_{20}^{February} \equiv \tau_{10}^{f} + \tau_{21}^{f}$	-0.001	0.004	0.011	-0.012	0.027
	(0.032)	(0.049)	(0.048)	(0.043)	(0.050)
Reweighted	No	Yes	Yes	Yes	Yes
Propensity Score Control(s)	No	No	Yes	No	Yes
$\mathbf{X}_h$ Controls	No	No	No	Yes	No
Number of Households	9,011	9,011	9,011	9,011	9,011

Notes: Significance levels: $*$ 10% $**$ 5% $***$ 1%. Significance levels: $*$ 10% $**$ 5% $***$ 1%. The dependent variable in all specifications is Δlog household expenditures per capita between 2005 and 2006. The variable *enumerated in March* is an indicator for whether the household was enumerated in March (relative to February) 2006. All columns are estimated by weighted least squares where the weights for treatment households equal one and the weights for control households are given by the normalized estimated odds of treatment $\widehat{\omega} = \widehat{P}/(1-\widehat{P})$. See Table 5 for further details on each of the estimators. All columns include province fixed effects. Standard errors are clustered by village and computed using a block bootstrap with 1000 repetitions.

Table 7: Multi-valued Treatment Effects by Expenditure Group

Growth Horizon	2005-2006			2005-2007		
Expenditure Group	**total**	**food**	**non-food**	**total**	**food**	**non-food**
	(1)	(2)	(3)	(4)	(5)	(6)
τ_{10}: disbursement 1	-0.076	-0.094	-0.046	-0.017	-0.030	0.031
	(0.031)**	(0.031)***	(0.045)	(0.038)	(0.039)	(0.056)
τ_{21}: disbursement 2	0.076	0.097	0.037	0.023	0.068	-0.053
	(0.034)**	(0.034)***	(0.048)	(0.043)	(0.041)*	(0.060)
$\tau_{20} \equiv \tau_{21} + \tau_{10}$: disbursements 1+2	0.000	0.003	-0.010	0.006	0.038	-0.022
	(0.019)	(0.019)	(0.025)	(0.026)	(0.026)	(0.039)
Number of Households	9,011	9,011	9,009	6,992	6,992	6,992

Notes: Significance levels: $*$ 10% $**$ 5% $***$ 1%. The dependent variable in all specifications is Δlog household expenditures per capita on the given commodity group between 2005 and 2006/2007. All columns are estimated by weighted least squares where the weights for treatment households equal one and the weights for control households are given by the normalized estimated odds of treatment $\widehat{\omega} = \widehat{P}/(1-\widehat{P})$. All columns include a 5th order polynomial in the propensity scores that is allowed to vary by treatment and control. All columns include province fixed effects. Standard errors are clustered by village and computed using a block bootstrap with 1000 repetitions.

Table 8: Multi-valued Treatment Effects by Expenditure Subgroup, 2005-2006

Expenditure Group	**grains**	**fish/meat/dairy**	**fruit/nuts/veg.**	**other food**	**outside prep.**
	(1)	(2)	(3)	(4)	(5)
τ_{10}: disbursement 1	-0.025	-0.173	-0.120	-0.084	-0.218
	(0.045)	(0.063)***	(0.043)***	(0.040)**	(0.071)***
τ_{21}: disbursement 2	0.055	0.125	0.122	0.085	0.160
	(0.054)	(0.068)*	(0.051)**	(0.045)*	(0.079)**
$\tau_{20} \equiv \tau_{21} + \tau_{10}$: disbursements 1+2	0.031	-0.048	0.003	0.000	-0.059
	(0.034)	(0.035)	(0.030)	(0.023)	(0.041)
Number of Households	8,789	8,334	8,844	8,879	7,649
Expenditure Group	**housing**	**transport/comm.**	**appliances**	**debt/taxes**	**educ./health**
	(6)	(7)	(8)	(9)	(10)
τ_{10}: disbursement 1	0.012	-0.285	0.006	0.083	-0.149
	(0.052)	(0.118)**	(0.065)	(0.122)	(0.109)
τ_{21}: disbursement 2	-0.031	0.259	0.119	-0.149	0.191
	(0.056)	(0.132)**	(0.074)	(0.135)	(0.125)
$\tau_{20} \equiv \tau_{21} + \tau_{10}$: disbursements 1+2	-0.020	-0.026	0.125	-0.066	0.042
	(0.025)	(0.061)	(0.046)***	(0.059)	(0.065)
Number of Households	9,002	5,478	8,898	5,995	6,504

Notes: Significance levels: $*$ 10% $**$ 5% $***$ 1%. The dependent variable in all specifications is Δlog household expenditures per capita on the given commodity group between 2005 and 2006/2007. All columns are estimated by weighted least squares where the weights for treatment households equal one and the weights for control households are given by the normalized estimated odds of treatment $\widehat{\omega} = \widehat{P}/(1-\widehat{P})$. All columns include a 5th order polynomial in the propensity scores that is allowed to vary by treatment and control. All columns include province fixed effects. Standard errors are clustered by village and computed using a block bootstrap with 1000 repetitions.

Table 9: Heterogeneity by Proximity to Financial Institutions

Growth Horizon	2005-2006			2005-2007		
Estimator	OLS	IPW	Control Fn.	OLS	IPW	Control Fn.
	(1)	(2)	(3)	(4)	(5)	(6)
bank nearby	0.001	-0.027	-0.013	0.003	0.022	0.049
	(0.026)	(0.047)	(0.045)	(0.033)	(0.044)	(0.042)
τ_{10}^{u}: disbursement 1	-0.133	-0.165	-0.140	-0.142	-0.144	-0.100
	(0.047)***	(0.061)***	(0.054)**	(0.060)**	(0.063)**	(0.058)*
τ_{10}^{b}: disbursement 1 $\times$ bank nearby	0.104	0.108	0.104	0.153	0.126	0.112
	(0.058)*	(0.069)	(0.064)	(0.077)**	(0.079)	(0.074)
τ_{21}^{u}: disbursement 2	0.102	0.106	0.118	0.067	0.052	0.058
	(0.054)*	(0.057)*	(0.057)**	(0.067)	(0.071)	(0.066)
τ_{21}^{b}: disbursement 2 $\times$ bank nearby	-0.087	-0.065	-0.078	-0.066	-0.042	-0.051
	(0.067)	(0.072)	(0.070)	(0.086)	(0.089)	(0.085)
$\tau_{10}^{banked} \equiv \tau_{10}^{u} + \tau_{10}^{b}$	-0.027	-0.057	-0.036	0.012	-0.018	0.012
	(0.032)	(0.037)	(0.035)	(0.049)	(0.047)	(0.045)
$\tau_{21}^{banked} \equiv \tau_{21}^{u} + \tau_{21}^{b}$	0.015	0.041	0.040	0.002	0.009	0.007
	(0.035)	(0.042)	(0.039)	(0.055)	(0.054)	(0.051)
$\tau_{20}^{banked} \equiv \tau_{21}^{u} + \tau_{10}^{u} + \tau_{21}^{b} + \tau_{10}^{b}$	-0.014	-0.016	0.004	0.014	-0.009	0.019
	0.015	(0.019)	(0.020)	(0.023)	(0.026)	(0.027)
$\tau_{20}^{unbanked} \equiv \tau_{10}^{u} + \tau_{21}^{u}$	-0.030	-0.058	-0.023	-0.074	-0.092	-0.042
	0.030	(0.042)	(0.039)	(0.043)*	(0.050)*	(0.049)
Reweighted	No	Yes	Yes	No	Yes	Yes
Propensity Score Controls	No	No	Yes	No	No	Yes
Number of Households	8,923	8,923	8,923	6,966	6,966	6,966

Notes: Significance levels: $*$ 10% $**$ 5% $***$ 1%. The dependent variable in all specifications is Δlog household expenditures per capita between 2005 and 2006/2007. The variable, *bank nearby*, equals one if there are any banking institutions located in the given village's subdistrict as reported in *Podes* 2005 (see Appendix C). Columns 2-3 and 5-6 are estimated by weighted least squares where the weights for treatment households equal one and the weights for control households are given by the normalized estimated odds of treatment $\widehat{\omega} = \widehat{P}/(1-\widehat{P})$. Columns 3 and 6 additionally include a 5th order polynomial in the propensity scores that is allowed to vary by treatment and control. All columns include province fixed effects. Standard errors are clustered by village and computed using a block bootstrap with 1000 repetitions.

Table 10: Robustness Checks

		τ_{10} disbursement 1	τ_{21} disbursement 2	$\tau_{20} \equiv \tau_{10} + \tau_{21}$ disbursements 1+2	No. of Households
1.	baseline	-0.076 (0.031)**	0.076 (0.034)**	0.000 (0.019)	9,011
2.	controls for day of survey enumeration	-0.070 (0.027)**	0.071 (0.031)**	0.001 (0.017)	9,011
3.	district fixed effects	-0.059 (0.024)**	0.057 (0.027)**	-0.002 (0.016)	9,011
4.	subdistrict fixed effects	-0.038 (0.022)*	0.037 (0.024)	-0.002 (0.014)	9,011
5.	village fixed effects	-0.039 (0.021)*	0.038 (0.024)	-0.001 (0.015)	9,011
6.	$\ln C_{ht}$ trimmed at 1st and 99th percentile	-0.085 (0.031)***	0.074 (0.034)**	-0.011 (0.018)	8,833
7.	$\Delta \ln C_{h,t+1}$ trimmed at 1st and 99th percentile	-0.052 (0.024)**	0.050 (0.027)*	-0.002 (0.016)	8,832
8.	$\Delta \ln C_{h,t+1}$ deflated by regional poverty line	-0.057 (0.033)*	0.066 (0.036)*	0.009 (0.019)	8,851
9.	$\Delta \ln C_{h,t+1}$ deflated by regional CPI	-0.070 (0.029)**	0.069 (0.032)**	-0.001 (0.017)	8,923
10.	$\Delta \ln C_{h,t+1}$ adjusted for adult-equivalence (total)	-0.070 (0.030)**	0.071 (0.033)**	0.002 (0.018)	9,011
11.	$\Delta \ln C_{h,t+1}$ adjusted for adult-equivalence (food)	-0.072 (0.028)**	0.072 (0.032)**	0.001 (0.018)	9,011
12.	$\Delta \ln C_{h,t+1}$ prorated annual durable expenditures	-0.092 (0.034)***	0.083 (0.038)**	-0.009 (0.020)	9,011
13.	controls for other social program receipt	-0.056 (0.031)*	0.073 (0.032)**	0.017 (0.025)	9,011
14.	controls for change in health shocks/capita	-0.078 (0.030)***	0.078 (0.033)**	0.001 (0.018)	9,011
15.	controls for natural disasters, 2003-5	-0.084 (0.031)***	0.080 (0.033)**	-0.003 (0.018)	8,785
16.	controls for staggering covariates, Table 4	-0.074 (0.029)**	0.051 (0.032)	-0.024 (0.029)	8,687
17.	controls for pre-program enumerator visit	-0.045 (0.038)	0.075 (0.032)**	0.030 (0.029)	9,011

Notes: Significance levels: $*$ 10% $**$ 5% $***$ 1%. Each row corresponds to a separate regression with log household expenditure per capita growth, $\Delta \ln C$, as the dependent variable. The estimates are obtained by weighted least squares where the weights for treatment households equal one and the weights for control households are given by the normalized estimated odds of treatment $\widehat{\omega} = \widehat{P}/(1 - \widehat{P})$. All rows include a 5th order polynomial in the propensity scores that is allowed to vary by treatment and control. All rows include province fixed effects. Standard errors are clustered by village (except in rows 3 and 4, which cluster by the given geographic FE) and computed using a block bootstrap with 1000 repetitions. See Section 4.1 for details on the specifications and the Online Appendix for results using other estimators.

Table 11: Intensive Margin Treatment Effects by Expenditure Group

	(1)	(2)	(3)	(4)
	$\Delta \ln total\ expenditures/capita$			
transfers per capita (000,000s Rupiah)	0.045	0.044	0.037	0.064
	(0.008)***	(0.008)***	(0.009)***	(0.012)***
	$\Delta \ln food\ expenditures/capita$			
transfers per capita (000,000s Rupiah)	0.045	0.044	0.039	0.063
	(0.009)***	(0.009)***	(0.010)***	(0.013)***
	$\Delta \ln nonfood\ expenditures/capita$			
transfers per capita (000,000s Rupiah)	0.046	0.045	0.036	0.070
	(0.011)***	(0.012)***	(0.012)***	(0.019)***
Treatment Indicators	Yes	Yes	Yes	Yes
Reweighted	No	Yes	Yes	Yes
Propensity Score Polynomial	No	No	Yes	Yes
Household Size Indicators	No	No	No	Yes
Number of Households	9,011	9,011	9,011	9,011

Notes: Significance levels: $*$ 10% $**$ 5% $***$ 1%. Each cell corresponds to a separate regression. Transfers are rescaled to 100,000s of Rupiah (approximately 10 USD). Columns 2-4 are estimated by weighted least squares where the weights for treatment households equal one and the weights for control households are given by the normalized estimated odds of treatment $\widehat{\omega} = \widehat{P}/(1-\widehat{P})$. All include the treatment indicators and province fixed effects. Columns 3-4 include a 5th order polynomial in the propensity scores that is allowed to vary by treatment and control. Column 4 includes indicators for household size. Standard errors are clustered by village and computed using a block bootstrap with 1000 repetitions.

Table 12: (Agricultural) Household Expenditures Respond to Transitory Rainfall Shocks

	(1)	(2)	(3)	(4)	(5)
rainfall shock	0.030	-0.049	-0.043	-0.055	-0.029
	(0.071)	(0.095)	(0.073)	(0.074)	(0.068)
rural village		-0.005			
		(0.019)			
rural village × rainfall shock		0.125			
		(0.109)			
agriculture primary income			0.018		
			(0.014)		
agricultural primary income × rainfall shock			0.220		
			(0.088)**		
own any agricultural land				-0.001	
				(0.014)	
own any agricultural land × rainfall shock				0.170	
				(0.087)*	
own agricultural land (Ha)					0.001
					(0.003)
own agricultural land (Ha) × rainfall shock					0.063
					(0.029)**
rainfall shock + rainfall shock × covariate		0.077	0.177	0.114	
		(0.084)	(0.095)*	(0.091)	
Number of Households	8,923	8,923	8,923	8,923	8,923
R^2	0.042	0.043	0.045	0.044	0.045

Notes: Significance levels: $*$ 10% $**$ 5% $***$ 1%. The dependent variable in all specifications is Δlog total monthly household expenditures per capita between 2005 and 2006. The rainfall shock is the log deviation of the seasonal rainfall level in the district from the long-run (1952-2004) district mean. Standard errors clustered by district. All columns include province fixed effects. The interaction terms are as observed at baseline.

Appendix

A Propensity Scores and Reconstructed Quasi-PMT Scores

To estimate the probability that household h receives any UCT disbursements, D, we consider the following specification, which roughly approximates information on household h available to enumerators and local officials in mid-2005,[1]

$$Pr(disbursements_h > 0) = F\left(\boldsymbol{\beta}\mathbf{X}_h^{\text{fam}} + \boldsymbol{\gamma}\mathbf{X}_h^{\text{house}} + \boldsymbol{\alpha}\mathbf{X}_h^{\text{head}} + \boldsymbol{\delta}\mathbf{X}_h^{\text{welfare}} + \zeta_h > 0\right), \tag{A.1}$$

All right-hand variables are observed in at baseline in early 2005: $\mathbf{X}_h^{\text{fam}}$ is a vector of demographic variables including household age structure, gender breakdown; $\mathbf{X}_h^{\text{house}}$ includes variables pertaining to the quality of the physical structures in which household h lives; $\mathbf{X}_h^{\text{head}}$ includes characteristics of the head of the household, $\mathbf{X}_h^{\text{welfare}}$ includes indicators for employment among household members, prior participation in government welfare programs, and amount of land owned; F is the logistic CDF;[2] and ζ_h captures all variables unobservable to the econometrician but possibly observable to program administrators. We also control for province fixed effects to subsume some of the regional differences in targeting infrastructure (among other things). A full elaboration of the coefficient estimates was reported in Table 2.[3] Given our large set of dummy variables, there is little advantage to estimating equation (A.1) nonparametrically.[4]

As discussed in Section 2.2, although we made every effort to reconstruct the underlying PMT scores using available data, the resulting scores were not discriminating enough to allow for a fuzzy regression-discontinuity research design. After transforming applicable questions in *Susenas* 2005 into the corresponding variable-specific eligibility criteria, we apply the district-specific PMT coefficients corresponding to the given variables to produce a measure $\widetilde{P}_h$, which reflects a data-constrained approximation to the actual PMT scores based on the original eligibility survey.[5] According to program guidelines, households with PMT scores above the 70th percentile should qualify for benefits. We take this rule to our estimates $\widetilde{P}_h$ in search of a potential discontinuity. Unfortunately, as seen in Figure A.1, no such discontinuity can be found—perhaps unsurprisingly given the evidence on leakage and undercoverage. Moreover, the actual probability of UCT receipt looks quite similar across the distribution of the estimated propensity scores $\widehat{P}_h$. Yet, if we predict the probability of program receipt using $\widetilde{P}_h$ as the only regressor—effectively fixing $(\beta, \gamma, \delta, \alpha)$ in equation A.1 at the district-specific PMT coefficients—and accordingly reweight households in the control group, the balance at baseline is much worse than when using our arguably more flexible approach in equation (A.1). This can be seen by comparing the effect of reweighting the control group in Figure A.2, which uses $\widetilde{P}_h$, and Figure 4 discussed in the paper, which uses our estimated propensity scores. This balance differential is intuitive because our propensity score model is based on a richer set of variables plausibly in the information set of local officials engaged in community-based alongside or possibly in defiance of official targeting.

[1]Moreover, we pursue an additive specification in keeping with the weighting procedure used to construct the original PMT scores.

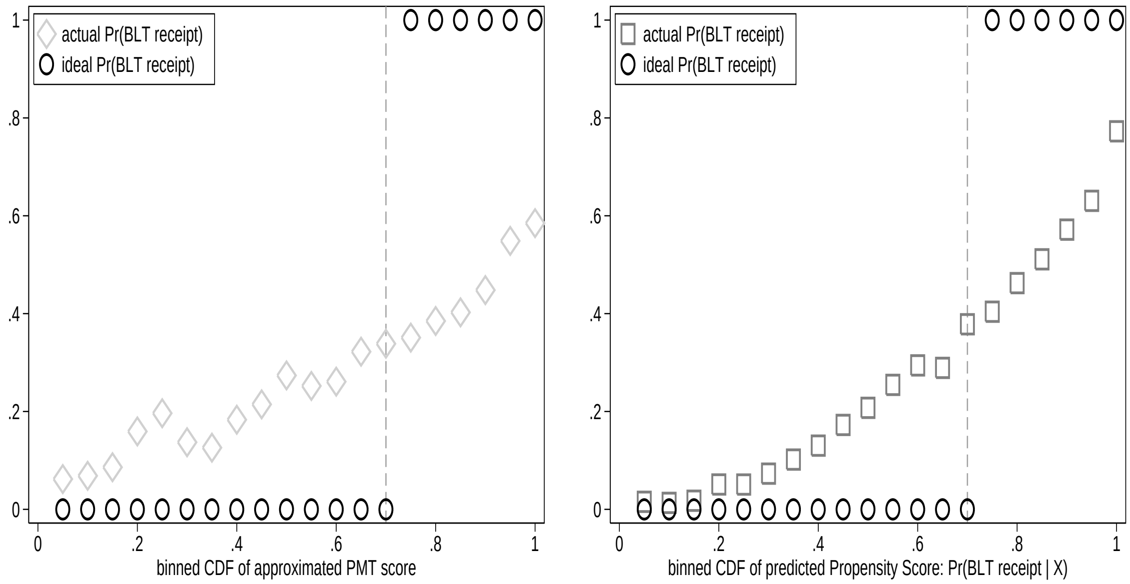
[2]In unreported results, we find that a probit estimator yields identical results.

[3]The official eligibility survey grouped several response categories to questions in *Susenas* concerning household characteristics. Whether one leaves the individual responses as separate indicators (in a fully saturated sense) or groups them according to the rubric in the original survey does not matter for our key qualitative findings.

[4]Doing so using the Klein and Spady (1993) estimator yields an estimated propensity score that has a 0.97 correlation with the computationally simpler parametric logit.

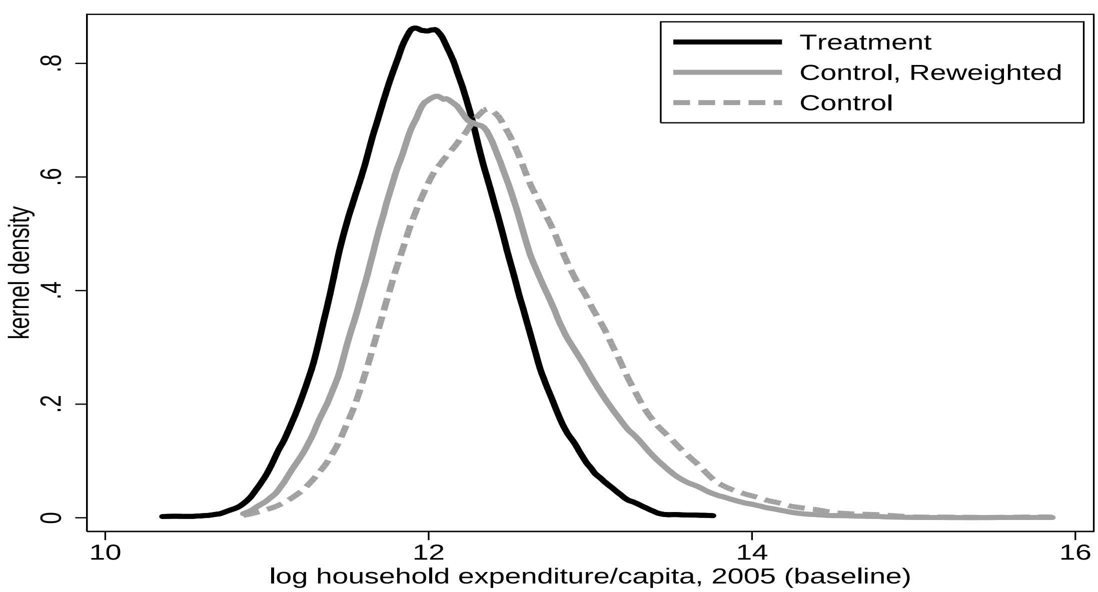
[5]Prior to this, we rescale the coefficients to ensure that they sum to 1 after dropping the questions not available in *Susenas*.

Figure A.1: Comparing Propensity Score Estimates and Approximated PMT Scores



Notes: LEFT—The circles capture the share of UCT (BLT) recipients within the given bin where the bins are 0.05 width slices of the CDF of the quasi-PMT scores approximated using the procedures described in the text. The dashed vertical line constitutes the 30% threshold above which households were (in theory) supposed to receive the program. RIGHT—The circles capture the share of UCT (BLT) recipients within the given bin where the bins are 0.05 width slices of the CDF of the propensity scores obtained from estimating a binary version of equation (A.1) by maximum likelihood where ζ_h is logistic distributed.

Figure A.2: Baseline Expenditure Distributions by Treatment Status



Notes: All distributions estimated using Epanechnikov kernel and a rule-of-thumb bandwidth. The "Control (Reweighted)" observations are adjusted using inverse probability weights (IPW) based on normalized estimated odds of treatment $\widetilde{\omega} = \widetilde{P}/(1 - \widetilde{P})$, where $\widetilde{P}_h$ is as described in Appendix A.

B Further Empirical Results

This appendix provides tables and discussion of additional empirical results mentioned in the paper.

B.1 Validating the Exogeneity of Variation in Transfers per Capita

Table B.1 shows that the UCT program had no effect on household size over the period from early 2005 to early 2006, and Table B.2 shows that the size of the informal tax levied on UCT recipients is uncorrelated with household characteristics. The results in these tables support the claim in the paper that the residual variation in transfers per capita is exogenous with respect to a number of other factors potentially associated with expenditure growth over the period under study.

B.2 Heterogeneity by Age of Household Head

In Table B.3, we allow the estimated treatment effects $\boldsymbol{\tau}$ to vary by the age of the household head. We do this in columns 1 and 3 by interacting the treatment indicators with a quadratic in age and in columns 2 and 4 by interacting those indicators with three bins comprising roughly the bottom three quintiles of the age distribution. The marginal effects reported in Figure 6 correspond to the quadratic interactions in columns 1 and 3. While we observe an interesting nonlinear age profile in the treatment effects consistent with life cycle behavior, the estimates underlying Figure 6 are somewhat imprecise.

B.3 Ruling Out Spillover Effects via Local Informal Redistribution

In Table B.4, we consider the effects of informal redistribution of the UCT benefits within villages using the approach described in Section 4.4. As noted there, the results withstand this test, which reassigns all control households to the given treatment group within a village if there are any reports of redistribution.

B.4 Using Geographic Fixed Effects as Instruments for Delayed Disbursement

In Table B.5, we use geographic fixed effects as instruments for the arrival of the delayed second disbursement by early 2006. As discussed in the paper, this requires the strong and arguably implausible assumption that geographic FE only explain variation in consumption growth across households through their effect on the timing of cash transfers. Nevertheless, in doing so, we find results that are largely consistent with the reduced form approach to geography pursued in the main analysis and preceding specification checks. In particular, we find that the differential treatment effects remain qualitatively unchanged. However, the coefficients increase by about 50 percent in magnitude at the expense of some precision. The increase in coefficients suggests that the reduced form estimates in the paper may be downward biased due to our inability to observe actual dates of transfer receipt (or scheduled receipt for those still awaiting the second disbursement). However, underidentification tests suggest that the instruments may be collectively weak, which might lead to upwardly biased second stage estimates. Overall, though, the instrumental variables estimates in this table support the main reduced form estimates in the paper.

Tables

Table B.1: UCT Benefits Had No Effect on Household Size

Estimator	OLS	IPW	Double Robust ($\widehat{P}_h$)	Double Robust ($\mathbf{X}_h$)	Control Function
	(1)	(2)	(3)	(4)	(5)
τ_{10}: disbursement 1	-0.014	0.069	0.069	0.058	0.095
	(0.059)	(0.127)	(0.129)	(0.116)	(0.117)
τ_{21}: disbursement 2	0.014	-0.090	-0.090	-0.045	-0.115
	(0.065)	(0.133)	(0.135)	(0.116)	(0.127)
$\tau_{20} \equiv \tau_{21} + \tau_{10}$: disbursements 1+2	0.0002	-0.021	-0.022	0.013	-0.020
	(0.030)	(0.078)	(0.079)	(0.075)	(0.080)
Reweighted	No	Yes	Yes	Yes	Yes
Propensity Score Control(s)	No	No	Yes	No	Yes
$\mathbf{X}_h$ Controls	No	No	No	Yes	No
Number of Households	9,011	9,011	9,011	9,011	9,011
R^2	0.006	0.011	0.011	0.131	0.016

Notes: All columns estimated by linear probability regressions with Δ log household size between 2005 and 2006 on the left hand side. Columns 2-5 are estimated by weighted least squares where the weights for treatment households equal one and the weights for control households are given by the normalized $\widehat{\omega} = \widehat{P}/(1 - \widehat{P})$. All columns include province fixed effects. Standard errors are clustered by village and computed using a block bootstrap with 1000 repetitions.

Table B.2: Idiosyncratic vs. Spatial Variation in the "Tax" on UCT Recipients

	(1)	(2)	(3)	(4)
household size, $t-1$	-0.001	0.001	0.003	-0.000
	(0.004)	(0.002)	(0.005)	(0.002)
Fixed Effects (FE)	Province	Subdistrict	Province	Subdistrict
$\mathbf{X}_{h,t-1}$ controls	No	No	Yes	Yes
p-value joint statistical significance			[0.52]	[0.99]
Number of Households	2,410	2,410	2,410	2,410
R^2	0.113	0.822	0.187	0.827

Notes: All columns are estimated by linear probability regressions of the following specification: $Pr(transfer_h < full\ amount \mid disbursements > 0) = \boldsymbol{\beta}\mathbf{X}_{h,t-1} + \theta_{FE} + e_h$, where $\mathbf{X}_{h,t-1}$ includes all the baseline household characteristics used to estimate propensity scores. Standard errors clustered by village.

Table B.3: Heterogeneity by the Age of the Household Head

Growth Horizon	2005-2006		2005-2007	
	(1)	(2)	(3)	(4)
disbursement 1	0.235	0.014	0.336	-0.006
	(0.231)	(0.042)	(0.258)	(0.056)
disbursement 1 × age of HH head	-0.016		-0.014	
	(0.009)*		(0.011)	
disbursement 1 × age of HH head squared	0.000		0.000	
	(0.000)**		(0.000)	
disbursement 1 × age of HH head $\in [13, 36]$		-0.086		0.056
		(0.058)		(0.076)
disbursement 1 × age of HH head $\in [37, 46]$		-0.116		-0.013
		(0.055)**		(0.077)
disbursement 1 × age of HH head $\in [47, 56]$		-0.183		-0.067
		(0.062)***		(0.074)
disbursement 2	0.234	0.025	-0.189	0.060
	(0.212)	(0.039)	(0.264)	(0.053)
disbursement 2 × age of HH head	-0.005		0.007	
	(0.008)		(0.011)	
disbursement 2 × age of HH head squared	0.000		-0.000	
	(0.000)		(0.000)	
disbursement 2 × age of HH head $\in [13, 36]$		0.082		-0.081
		(0.050)		(0.069)
disbursement 2 × age of HH head $\in [37, 46]$		0.069		-0.020
		(0.048)		(0.069)
disbursement 2 × age of HH head $\in [47, 56]$		0.075		-0.067
		(0.049)		(0.067)
disbursement 1+2	0.469	0.039	0.147	0.054
	(0.191)**	(0.037)	(0.189)	(0.044)
disbursement 1+2 × age of HH head	-0.020		-0.007	
	(0.008)***		(0.008)	
disbursement 1+2 × age of HH head squared	0.000		0.000	
	(0.000)***		(0.000)	
disbursement 1+2 × age of HH head $\in [13, 36]$		-0.004		-0.025
		(0.049)		(0.062)
disbursement 1+2 × age of HH head $\in [37, 46]$		-0.047		-0.033
		(0.041)		(0.058)
disbursement 1+2 × age of HH head $\in [47, 56]$		-0.108		-0.133
		(0.050)**		(0.056)**
Number of Households	9,011	9,011	6,992	6,992

Notes: Significance levels: $*$ 10% $**$ 5% $***$ 1%. The dependent variable in all specifications is Δlog household expenditures per capita between 2005 and 2006/2007. All columns are estimated by weighted least squares where the weights for treatment households equal one and the weights for control households are given by the normalized $\widehat{\omega} = \widehat{P}/(1 - \widehat{P})$. All columns include a 5th order polynomial in the propensity scores that is allowed to vary by treatment and control. All columns include province fixed effects. Standard errors are clustered by village and computed using a block bootstrap with 1000 repetitions.

Table B.4: Reassigning Control Households to Treatment Groups

Estimator	OLS	IPW	Double Robust		Control Function
			$(\widehat{P}_h)$	$(\mathbf{X}_h)$	
	(1)	(2)	(3)	(4)	(5)
			Short-Term: 2005-2006		
τ_{10}: disbursement 1	-0.051	-0.077	-0.076	-0.081	-0.047
	(0.030)*	(0.035)**	(0.033)**	(0.033)**	(0.032)
τ_{21}: disbursement 2	0.022	0.036	0.038	0.042	0.034
	(0.032)	(0.036)	(0.036)	(0.033)	(0.035)
$\tau_{20} \equiv \tau_{21} + \tau_{10}$: disbursements 1+2	-0.030	-0.040	-0.038	-0.039	-0.013
	(0.015)**	(0.020)**	(0.020)*	(0.019)**	(0.018)
Reweighted	No	Yes	Yes	Yes	Yes
Propensity Score Control(s)	No	No	Yes	No	Yes
$\mathbf{X}_h$ Controls	No	No	No	Yes	No
Number of Households	9,007	9,007	9,007	9,007	9,007
R^2	0.045	0.079	0.083	0.153	0.099
			Medium-Term: 2005-2007		
τ_{10}: disbursement 1	0.005	-0.012	-0.016	-0.003	0.016
	(0.040)	(0.039)	(0.039)	(0.036)	(0.040)
τ_{21}: disbursement 2	-0.019	-0.016	-0.015	-0.022	-0.020
	(0.043)	(0.043)	(0.041)	(0.037)	(0.043)
$\tau_{20} \equiv \tau_{21} + \tau_{10}$: disbursements 1+2	-0.014	-0.028	-0.031	-0.026	-0.005
	(0.020)	(0.022)	(0.024)	(0.022)	(0.022)
Reweighted	No	Yes	Yes	Yes	Yes
Propensity Score Control(s)	No	No	Yes	No	Yes
$\mathbf{X}_h$ Controls	No	No	No	Yes	No
Number of Households	6,994	6,994	6,994	6,994	6,994
R^2	0.044	0.059	0.062	0.144	0.074

Notes: Significance levels: $*$ 10% $**$ 5% $***$ 1%. This table reproduces the baseline estimates after first reassigning all control households ($D = 0$) to one of the two treatment levels ($D = 1, 2$) if recipients in their village report being informally taxed during disbursement rounds one and/or two for the purposes of redistribution to non-recipients. As in Table 5, Columns 2-5 are estimated by weighted least squares where the weights for treatment households equal one and the weights for control households are given by the normalized estimated odds of treatment $\widehat{\omega} = \widehat{P}/(1 - \widehat{P})$, where the normalization is over the entire sample for the given time horizon. Using the Crump et al. (2009) procedure, we trim 41 households in the upper tail of the estimated propensity scores, $\widehat{P}$, and do the same in the OLS regressions for comparability. Column 3 controls linearly for the propensity score and column 5 for a fifth-order polynomial in the propensity score allowing it to vary by treatment and control. Column 4 controls for all covariates $\mathbf{X}_h$ used to estimate the propensity score. The coefficients on these additional terms in columns 3-5 are suppressed for presentational purposes. All columns include province fixed effects. Standard errors are clustered by village and computed over the entire two-step process using a block bootstrap with 1000 repetitions.

Table B.5: Using Geographic Fixed Effects as Instruments for Staggering

Estimator	OLS	IPW	Double Robust		Control Function
			$(\widehat{P}_h)$	$(\mathbf{X}_h)$	
	(1)	(2)	(3)	(4)	(5)
	Panel A: District FE as Instruments				
τ_{10}: disbursement 1	-0.072	-0.132	-0.143	-0.129	-0.119
	(0.067)	(0.080)*	(0.084)*	(0.073)*	(0.077)
τ_{21}: disbursement 2	0.063	0.130	0.147	0.125	0.134
	(0.087)	(0.107)	(0.111)	(0.096)	(0.101)
$\tau_{20} \equiv \tau_{21} + \tau_{10}$: disbursements 1+2	-0.01	-0.002	0.005	-0.004	0.015
	(0.025)	(0.036)	(0.033)	(0.031)	(0.032)
Number of Households	9,011	9,011	9,011	9,011	9,011
R^2	0.045	0.099	0.102	0.181	0.118
Overidentification Test (p-value)	0.506	0.373	0.363	0.213	0.335
First-stage F Statistic	11.34	10.28	10.27	10.01	10.36
Underidentification Test (p-value)	0.850	0.692	0.861	0.627	0.561
	Panel B: Subdistrict FE as Instruments				
τ_{10}: disbursement 1	-0.099	-0.152	-0.153	-0.145	-0.130
	(0.058)*	(0.080)*	(0.076)**	(0.069)**	(0.067)*
τ_{21}: disbursement 2	0.099	0.157	0.161	0.146	0.149
	(0.074)	(0.103)	(0.097)*	(0.088)*	(0.088)*
$\tau_{20} \equiv \tau_{21} + \tau_{10}$: disbursements 1+2	-0.0002	0.005	0.008	0.001	0.019
	(0.021)	(0.032)	(0.030)	(0.026)	(0.027)
Number of Households	9,011	9,011	9,011	9,011	9,011
R^2	0.044	0.098	0.102	0.180	0.118
Overidentification Test (p-value)	0.493	0.434	0.433	0.406	0.442
First-stage F Statistic	8.012	7.737	7.728	7.506	7.742
Underidentification Test (p-value)	0.988	0.984	0.973	0.765	0.437

Notes: Significance levels: $*$ 10% $**$ 5% $***$ 1%. The dependent variable in all specifications is Δlog total monthly household expenditures per capita between 2005 and 2006. Panel A instruments for the receipt of disbursement 2 using 297 district fixed effects, and Panel A uses 589 subdistrict fixed effects instead. All columns control for province fixed effects in the second stage. Standard errors are clustered by village and computed using a block bootstrap with 300 repetitions. The overidentification test is a Hansen test of the null hypothesis that the exclusion restrictions are valid. The underidentification test is a Kleibergen-Paap Lagrange multiplier test of the null hypothesis that the system of equations is underidentified. See the notes to Table 5 in the paper for further details on the estimators and specifications.

C Data Construction

Merging Across Survey Waves. The main panel dataset is constructed by merging household identifiers across the three waves of *Susenas* enumerated in early 2005, 2006, and 2007. We first construct a balanced two-year panel between 2005 and 2006 by matching along (i) province-district-subdistrict-village-sampling ID-household ID and (ii) household head names in the 2005 and 2006 *Susenas* panels. While a traditional merge along strict geographic identifiers provides a balanced panel of $9,797$ households, significant discrepancies in household characteristics (including first names of household members) across waves indicate that survey administrators did not ensure the time-consistency of household presence in the physical location of prior enumeration. A name-matching algorithm provided by Robert Sparrow generated an initial balanced panel of almost 8500 households, and through further manual inspection, we added an additional 548 households. We then merged this balanced two-year panel of 9,048 households with the subset of all households enumerated in early 2007 also reporting enumeration in 2005 (in a specific survey question verified by enumerators). This process generated a balanced three-year panel comprising 7,014 households. A number of households are lost between 2006 and 2007 as a result of a change in the sampling rather than direct attrition by households.

Attrition. The data structure pose a nonstandard attrition problem. Although attritors appear much more similar to non-recipients than recipients (see Table 1), we do not know which attritors between 2005 and 2006 actually received the UCT. We observe recipient status among the 2,034 attritors between 2006 and 2007, and reassuringly the ratio of recipients to non-recipients remains essentially unchanged across years. Although inter-survey attrition is potentially a non-negligible problem, we ignore its consequences in the main results presented below. Nevertheless, all results are robust to reweighting the sample so as to account for the probability of attrition as a function of all observable characteristics used to predict treatment. The lack of contrast between attrition probability-weighted and unweighted estimates provides evidence against attrition being a source of misspecification that might confound identification of the causal average partial effect of interest (see Solon et al., 2013).[1]

Variable Construction. We use the *Susenas* datasets to construct all of the expenditure outcomes, baseline controls (see Table 2), health shocks and participation in other social programs (see Table 10), and information on UCT benefits.

The following variables were constructed based on data recorded in the Village Potential or *Podes* administrative census from April 2005: bank presence in the subdistrict, log travel distance to the (sub)district capital, log travel distance to the post office, village road paved, village accessibility by land, the incidence of natural disasters by type, and the ranking of the President's political party in the 2004 local village election. We are unable to match the data in *Podes* to *Susenas* villages for a small number of households in Papua. However, the main baseline results in Table 5 are robust to dropping these households.

Data on rainfall shocks were obtained from NOAA/GPCP sources. The total amount of rainfall during the given growing/harvest season where (i) seasons are 12 month intervals beginning with the first month of the province-specific wet season in a given year, and (ii) rainfall at the village level is based on rainfall levels recorded interpolated down to 0.5 degree (latitude/longitude) pixels between rainfall stations.

We obtain data on the regional CPI and the district-specific poverty lines (see Table 10) from the Central Bureau of Statistics. The former measures were mapped to villages based on the great circle distance from the centroid of the village's district to the centroid of the city in which the CPI measure was taken.

The (log) distance to Jakarta was computed as the great circle distance from the centroid of each district to the centroid of Jakarta

[1] A similar argument can be made for why we prefer not use the sampling weights provided in *Susenas*. Although we do not use those weights in the paper, doing so leaves key results are unchanged, and the lack of contrast between weighted and unweighted estimates provides evidence against the sort of misspecification that weighting might help correct in our context.