Conspicuous Consumption and Peer Effects among the Poor: Evidence From a Field Experiment*

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Abstract

I use a randomised conditional cash transfer program from Indonesia to provide evidence on peer effects in consumption of poor households. I combine this with consumption visibility data from Indonesia to examine whether peer effects in consumption differ by a good's visibility. In line with a model of conspicuous consumption, I find that the expenditure share of visible (nonvisible) goods rises (falls) for untreated households in treated sub-districts, whose reference group visible consumption is exogenously increased. Finally, I provide evidence on the mechanisms underlying the estimated spillovers using data on social interactions and social punishment norms. In line with Veblen's (1899) claim that conspicuous consumption is more prevalent in societies with less social capital, I show that the peer effects in visible goods are larger in villages and for households with lower levels of social activities.

Keywords: Conspicuous Consumption, Peer Effects, Relative Concerns, Spillovers, Social Interactions, Social Norms.

JEL classification: D12, C21, I38

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1 Introduction

Why do the poor stay poor? One explanation for poverty traps is the prevalence of conspicuous consumption among the poor which hinders investments and savings (Neeman and Moav, 2012). In particular, spending on conspicuous goods to signal one's status could perpetuate poverty and prevent the poor from escaping a poverty trap. Banerjee and Duflo (2007) find that the poor spend surprisingly little on food, health and education and have very few savings. However, they spend substantial amounts on entertainment, feasts, clothing and tobacco (Banerjee and Duflo, 2007). An important question in this context is whether a household's conspicuous consumption is dependent on the household's peers' consumption patterns. Such reference-dependent preferences and resulting peer effects might constitute a key mechanism sustaining poverty traps for many poor households.

Moreover, peer effects in consumption affect optimal policy design. The design of a conditional cash transfer program, for example, could depend on the prevalence and size of peer effects for several reasons: first, to appropriately identify the effect of an aid program, correct identification of indirect treatment effects is essential (Angelucci and De Giorgi, 2009; Imbert and Papp, 2014). Second, the conditionalities imposed by conditional cash transfer programs could be guided by empirical findings on peer effects.

Rigorous evidence on peer effects in consumption, however, is extremely scarce and especially so in the developing world. No evidence exists that systematically investigates whether households have reference-dependent preferences only for certain kinds of goods that are easily observable and thus more likely to confer an individual a higher social status. Yet, the importance and policy implications of such status concerns and social comparison effects have been emphasized by economists for a long time (Duesenberry, 1949; Frank, 1985a,b). Little evidence exists that investigates the mechanisms underlying peer effects in consumption. Understanding the mechanisms, though, is important from a policy perspective (Deaton, 2010; Heckman and Smith, 1995).

To address this lack of evidence, I make use of two data sources: first, I use data on consumption visibility from Indonesia that I collected in August and September 2013 based on two consumption visibility surveys previously used in the US (Charles *et al.*, 2009; Heffetz, 2011). Heffetz (2011) introduced a measure of consumption visibility which tries to operationalise how visible consumption goods are. He provides empirical evidence that such a measure of consumption visibility is related to the elasticity of consumption goods with respect to income changes. He argues that only visible goods convey a signal about an individual's status. Second, I use data on a randomised conditional cash transfer program with poor households from Indonesia. I exploit the partial-population design (Moffitt, 2001) of the program. Specifically, a subset of the members of a peer group receives an exogenous shock in income for a duration of up to six years. This partial population experiment offers me the opportunity to compare untreated households in treated villages with counterfactual households from untreated villages. That is, I make use of an exogenous variation in some households' reference group consumption to provide evidence on peer effects in consumption.

Contribution. Field-experimental evidence on reference-dependent preferences is scarce. While there exists field-experimental evidence on neighbourhood effects (Kling *et al.*, 2007) and peer effects in domains, such as education (Bobonis and Finan, 2009) and savings decisions (Duflo and Saez, 2003), little work has been done on consumption. Moretti (2011) provides evidence on social learning and the importance of the social multiplier using box-office data on movie sales. Grinblatt *et al.* (2008) employ an IV strategy to estimate the peer effects of automobile sales. Bertrand and Morse (2013) provide evidence on trickle down-consumption, i.e. they show that the consumption behaviour of low-and middle income households is affected by the consumption behaviour of the rich. My study is most similar to Angelucci and De Giorgi (2009), who use a conditional cash transfer program from Mexico. They show that the overall food consumption of program ineligibles increases. They make the case that the most likely explanation for their results is that the poor households shared a part of their cash transfer. Boneva (2014) uses Progresa to estimate the impact of changes in the composition of neighbours' food consumption on a household's composition of food expenditures. As there were substantial changes in overall food expenditures of ineligibles, she proposes a structural methodology to separately identify compositional effects from overall effects. My paper is also closely related to Kuhn *et al.* (2011) who use the "Dutch Postcode lottery" as an exogenous variation in neighbours' income. They provide evidence on social effects in consumption. Specifically, they show that neighbours of recent lottery winners increase their expenditures on cars in comparison to counterfactual households from non-winning zip-codes.

My paper is different from the previous literature in three respects as it focuses on (i) how the consumption shares of households are affected by their peers, (ii) how consumption visibility affects peer effects and (iii) as it is the first to shed light on the social mechanisms underlying peer effects in consumption.

This paper makes several contributions to the literature:

(i) This paper is the first – based on an exogenous variation – to systematically investigate how peer effects in consumption differ by a good's visibility. I show that an increase in reference group visible consumption results in an increase (decrease) in an individual's visible (nonvisible) expenditure shares. Further, as the treated individuals are poorer than the untreated individuals, this paper is the first to provide evidence on downward-looking comparisons.

(ii) It sheds light on the mechanisms underlying the observed spillovers by the means of data on social interactions and social norms. The increases in visible expenditure and shares of visible goods of untreated households in treated villages are larger in areas with lower levels of social activities. This finding is related to the insight by Veblen (1899) that conspicuous consumption is higher in communities with lower social capital. Moreover, social punishment norms[3] in treatment areas are related to the size of the treatment effects. Visible expenditures for the treated households only increase in a significant manner in villages without social punishment norms.

(iii) To the best of my knowledge this is the first time that consumption visibility data was collected in a developing economy. Furthermore, this is the first time that the same individuals responded to the two visibility surveys by Heffetz (2011) and Charles *et al.* (2009) used in the literature. Thus, this is the first attempt to validate the surveys. I find that the results on consumption visibility from Indonesia are similar to the findings from the US. The most notable exception are expenditures on education which are perceived to be more visible in Indonesia.

The outcomes in many studies on peer effects can usually be generated by different interaction processes (Manski, 2000). In particular, all of the studies in the literature are plagued by the reflection problem (Manski, 1993), i.e. separate identification of endogenous effects is not possible, as it is confounded by contextual and correlated effects. Two features of my data allow me to address the reflection problem: first, I make use of an experimental variation, which helps me tackle the simultaneity problem. Second, I use unusually rich data on the supply-side and local prices to rule out explanations based on

[3]Social punishment norms measure whether households not contributing to community activities expect to be punished.

correlated effects à la Manski. Specifically, I use detailed data characterizing the supply-side and demonstrate that there is both balance and no effect of the treatment. Moreover, I employ several strategies to show that the results are not driven by local prices: First, I deflate the expenditure data by using district-poverty lines. Second, I show that there is no treatment heterogeneity by proxies of transport costs and that results are robust to including controls capturing transport costs. Third, I calculate the implied supply-elasticities if the treatment effects were completely driven by price effects and show that they are implausibly low.

Indonesia. Indonesia is an ideal setting to test for the relevance of peer effects in (conspicuous) consumption among poor households for at least two reasons: First, as Indonesia is a developing economy many poor households in rural areas live in the same village in which their parents lived. This implies that concerns of self-selection into similar neighbourhoods, i.e. positive sorting, are not a big concern. Second, even though poverty was reduced dramatically in Indonesia over the past decades, a large fraction of the rural population is still poor or highly vulnerable to fall back into poverty. While Indonesian economic growth has been strong in aggregate, 43 percent of Indonesians are still estimated to live on less than two US dollars per person per day in 2012.

This paper is structured as follows: first, section 2 delineates the implications of a model of conspicuous consumption. Section 3 presents the data. Then in section 4, I test whether the data is consistent with the model. Subsequently, section 5 examines potential social mechanisms underlying the peer effects. Then, section 6 critically assesses alternative explanations and the robustness of results. Section 7 concludes.

2 The theoretical framework

Veblen (1899) was one of the first to incorporate social status considerations into economic theory. He introduced the assumption that individuals compare themselves to one another on the basis of their economic achievements. He famously coined the term "conspicuous consumption", i.e. consumption as a signal for social status. Veblen's insights serve as a basis for a growing literature on relative concerns in economics and the implications of consumption externalities (Bagwell and Bernheim, 1996; Bowles *et al.*, 2012; Clark and Oswald, 1996; Duesenberry, 1949; Frank, 1985a; Glazer and Konrad, 1996). There are few papers that investigate the prevalence of conspicuous consumption based on observational data: Charles *et al.* (2009) show that Blacks and Hispanics devote larger shares of their expenditure bundles to visible goods (e.g. clothing, jewellery) than do comparable Whites. Kaus (2013) provides evidence on conspicuous consumption in a developing country. He investigates conspicuous consumption and race with South African data and delineates that the socially contingent share in visible consumption decreases with reference group income.

This paper focuses on models of conspicuous consumption, where individuals have relative and absolute concerns for visible goods and only absolute concerns for nonvisible goods. This distinction between visible and nonvisible goods is motivated by the observation that only visible goods reveal information of an individual's relative economic position to strangers. Heffetz (2011) provides evidence that income elasticities can be predicted from the visibility of consumer expenditures. He develops a survey-based measure of expenditure visibility, which ranks different expenditures by how noticeable they are to others. This measure explains up to one third of the observed variation in elasticities across consumption categories in U.S. data.

Hypotheses. According to models of conspicuous consumption a household's level and expenditure share of visible consumption is dependent on his peers' level of visible consumption. I outline a model of conspicuous consumption that formalises the following hypotheses in Appendix A.

The model has the following implications: An increase in peer group visible consumption will result in an increase of visible expenditures and a decrease of nonvisible expenditures holding total consumption constant. In other words, an increase in visible expenditures of one's reference group results in an increase (decrease) in a household's expenditure shares on visible (nonvisible) goods. I will test these hypotheses in this paper based on the conditional cash transfer program. I have two groups of households in both the treated and untreated villages: "very poor" households and "poor" households. The income and consumption of very poor households increases as a result of the cash transfer program. I make the assumption that untreated "poor" households compare themselves with treated "very poor" households. In the current literature on consumption peer effects there has been a focus on upward-looking comparisons. Yet, people might care about increases of visible expenditures of lower status households as they are averse to losing their relative position, i.e. their status. I test the following hypotheses:

(i) Visible and nonvisible consumption of treated households increases as a result of the cash transfer.

(ii) Visible (nonvisible) consumption of untreated households is increasing (decreasing) in the visible consumption of the treated households that constitute the household's reference group. Consequently, the share of visible (nonvisible) goods is predicted to rise (fall).

3 Data

In this section, I describe the conditional cash transfer dataset and explain how valid counterfactual households for treated and untreated households in treated villages are constructed. Finally, I present the consumption visibility data from Indonesia.

I use the Program Keluarga Harapan (PKH) conditional cash transfer program dataset that was collected in Indonesia in 2007 and 2009 (Alatas, 2011). The aim of PKH was to improve health and education outcomes of poor households through the provision of a cash transfer conditional on participation in health and education services (Alatas, 2011). In particular, PKH delivers cash transfers to poor households only if a certain set of conditions is satisfied: specifically, these conditions are concerned with basic health provision for pregnant women or women with newborns or families with children of school age. The size of the intervention is substantial: households received amounts between between 60 and 220 US Dollars per year. Average yearly payments to households are 130 US Dollars, or about 12 percent of pre-PKH yearly household expenditure. The households are supposed to be part of the PKH program for up to six years. Rich data ranging from socio-economic and demographic characteristics such as schooling, health, nutrition to labour market outcomes and social activities was collected. Furthermore, detailed data on various consumption expenditures was collected.[4] The baseline survey was fielded between June and August 2007; the follow-up survey was conducted between October and December 2009. The attrition rate is 3.4 per cent.

3.1 Experimental design

The study is based on a partial-population design. Specifically, there are completely untreated villages as well as treated and untreated households in treated villages. This allows for the decomposition of total treatment effects into direct and indirect treatment

[4] Data was collected by an independent institution, the University of Gadjah Mada.

effects. Crucially, this set-up enables me to draw causal inferences, as I can make use of counterfactual households from untreated villages for both the treated households and untreated households in treated villages.

Randomisation at the sub-district level. This paper focuses on the rural subsample of the PKH program as I require a significant increase of visible expenditures of treated households to be able to test for peer effects in visible consumption. Such a significant increase in expenditures only occurs in the rural sub-districts. Focusing on the rural subsample is not as problematic from the perspective of inference as it may seem: first, the randomisation was stratified by rural areas (Duflo *et al.*, 2007). Second, in cluster-randomized designs, the impact evaluation has almost as much statistical power for the sub-group analysis as for analysis of the entire sample (Duflo *et al.*, 2007).[5]

PKH was randomly assigned in sub-districts that fulfilled certain supply-side as well as poverty criteria. As the focus of the program is poverty alleviation, upper income quintile districts were excluded from PKH eligibility based on an index considering poverty rates, malnutrition and schooling records. Out of eligible regions only those with sufficient health and education service institutions were chosen. Then out of the list of 85 eligible rural sub-districts 50 were randomly selected for treatment and 35 were chosen to be the control group. Eight villages were randomly drawn from each sub-district. Political pressures and a consequent unexpected expansion of the program on East Java resulted in deviations of the realised allocation from the original allocation in the treatment. In particular, 8 out of the 85 sub-districts that were designated to be part of the control group received PKH funds. Bias might result from this contamination as it is possible that those unobservables within contaminated sub-districts that induced a departure from the initial implementation plan, simultaneously affected household responses. This type of contamination can be dealt with by using the original assignment as an instrument for actual assignment. In section 6.4, I show that my results are robust to using this IV strategy.

In a first step, this section tests the implications of the randomisation for the demographic characteristics, consumption levels and asset holdings of households in treated and untreated sub-districts at baseline. The subscript i denotes the household, s denotes the sub-district and t denotes the time period. Let T_s denote the original allocation of the PKH program, where T_s takes value one if the sub-district was originally designated to

[5]This is true as the number of observations per cluster matters more than the number of clusters.

receive the program and zero if it was not designated to do so. Let A_s denote the actual assignment of the PKH program, taking value one if the program was implemented and value zero if it was not. Let Y_{is0} and X_{is0} be the baseline values of our outcome variables and covariates respectively. First, I consider whether baseline balance holds for the original assignment and then in a second step whether it holds for the actual assignment. I do so comparing the means and allowing the standard errors to be clustered at the sub-district level.

The variables of interest include the total, visible, nonvisible consumption expenditures, share spent on visible and nonvisible goods; demographic characteristics, such as age, education, employment sector, household size, social activities; and village level characteristics, such as the percentage of households without electricity, percentage of farmers, and population size. I find almost[6] no statistical differences between households in sub-districts originally and actually assigned to be treated vs. households in sub-districts originally and actually assigned to be untreated respectively, i.e. the randomisation seems to have worked well and the contamination does not seem to have systematically resulted in imbalances. Importantly all outcome variables of interest, i.e. consumption levels and shares are balanced at baseline. The baseline balance of treated and untreated sub-districts is evidenced in Table 1. Balance induced by the original treatment assignment is shown in Table 8 in Appendix B. Moreover, as can be seen in table 9 in Appendix B, I find barely[7] any statistical differences between contaminated and uncontaminated sub-districts.

[Insert Table 1]

3.2 Selection process at the household level

At the sub-district level the cash transfer program was offered to a list of eligible households that satisfied both certain demographic as well as certain economic requirements. A 2005 census from the national unconditional cash transfer program was used to construct the list of eligible households per village. Those targeted were classified as very poor by Statistics Indonesia. The classification was based on proxy-means tests to all poor

[6]In the original assignment the share of those with previous government aid is higher and the share of those households without schooling is higher. Both differences are significant at the 10 per cent level. Given that I tested for 16 differences in mean, it is not worrying that two variables are significantly different at the 10 percent level.

[7]I find that the share of those without schooling and of those working in the agricultural sector is lower in the contaminated sub districts.

households to identify program beneficiaries.

Identifying valid counterfactual households for the treated. The subsequent analysis to identify the causal effect of participating in the program will be reliant on propensity score matching as the allocation of the conditional cash transfer program was non-random within the villages and as there was no official categorization of households in untreated areas into "poor" and "very poor". I seek to identify households from untreated sub-districts that serve as valid counterfactuals for the treated households. Due to the large size of covariates and the resulting high-dimensionality of the covariate vector X_{ist}, I revert to a standard measure of how likely it is that a given individual receives the treatment, the propensity score (Rosenbaum and Rubin, 1983). I estimate the following logit model:

$$A_{is0} = \alpha_0 + \alpha_1 C_{is0} + \alpha_2 X_{is0} + \alpha_3 Z_{s0} + u_{is0} \tag{1}$$

I use those covariates that are currently predominantly used for the targeting purposes of poor households in Indonesia[8] and complement it with rich social and village-level covariates available in the dataset.
C_{is0} denotes baseline total expenditures, total expenditures squared and log food expenditures as well as dummies indicating asset holdings[9] X_{is0} is a vector of household-specific characteristics: in particular, the second polynomial of household size, the number of household members aged between zero and two, three and six as well as seven and fifteen respectively, education, number of club memberships, whether the household head works in the agricultural sector and age and age squared at baseline. Z_{s0} is a vector of variables characterizing sub-district level characteristics: specifically, the percentage of farmers, number of households receiving the raskin program (a subsidized rice program), population size, the number of social sanctions in the village[10], distance to the district capital and average village wage.[11]

The mean marginal effects[12] from this logit specification are displayed in Panel A, Table 2. Households with a TV, a refrigerator, a motorcycle and less children aged between

[8]Thanks to the targeting team at the World Bank and TNP2K for providing me with insights concerning the current practice of targeting.

[9]I include dummies on whether a household owns a TV, a refrigerator, a bicycle and a motorcycle respectively.

[10]Specifically, the number of sanctions in case households do not contribute to community activities.

[11]I conducted sensitivity analyses changing the covariates included in the propensity scores and find that my results are largely unaffected.

[12]The marginal effects at the mean are similar.

three and six are less likely to receive the program.

I drop all households with fitted propensity scores strictly outside the range of propensity scores of those receiving the PKH program (which is less than 10 percent of households). Thus, the remaining sample consists of households with propensity scores that have common support. Baseline balance in terms of observables is given for the recipients of the program and their counterfactual households. This leaves me with a sample of 2,022 households. Baseline balance of outcome variables and covariates for treated households and their counterfactuals is presented in Table 3.[13] Figure 1 depicts the propensity scores of treated and counterfactual households. It indicates that the counterfactuals are similar in terms of the propensity of receiving the treatment.

[Insert Figure 1]

Identifying valid counterfactual households for the untreated. I identify households in untreated sub-districts that serve as valid counterfactuals for the untreated individuals in treated sub-districts. First, I drop all treated households from the sample. To conduct the spillover analysis I compare the non-recipients in the treated villages ($A_s = 1$ and $pkh_{ist} = 0$) with a similar set of households in the untreated villages ($A_s = 0$). As before, I estimate a logit model based on the same set of observables as beforehand, based on the sample of all untreated households in treated villages and all households in untreated villages.

The mean marginal effects of this logit estimation are displayed Panel B of Table 2. They reveal that households that are more educated and have less children aged between zero and fifteen are more likely to be in a treated sub-district.

[Insert Table 2]

As before, to identify the causal effect, I exclude the individuals without common support, which constitute less than 10 percent of the sample. After the exclusion of households without common support baseline balance is restored for the outcome variables of interest and a large set of covariates. This can be seen in Table 3. This leaves me with a sample of 2,299 households. The kernel densities of the propensity scores of untreated households in treated sub-districts and their counterfactuals are displayed in Figure 2.

[13] While there is balance for most covariates, it should be noted that treated households are slightly younger and have received more government aid before than counterfactuals.

[Insert Figure 2]

[Insert Table 3]

3.3 Consumption visibility data

I collected consumption visibility data in Indonesia based on the surveys used by Heffetz (2011) and Charles *et al.* (2009) in August and September 2013. I did this for two reasons: first, I had to define some consumption categories as "visible" and others as "non-visible", and I wanted to inform this from the local context. Second, I wanted to investigate whether consumption visibility in a developing country is different from the western world. To the best of my knowledge this is the first time that this kind of consumption visibility data was collected in a developing economy. Furthermore, this is the first time that the same individuals responded to both visibility surveys used in the literature. Thus, this is the first attempt to validate the surveys used by Heffetz (2011) and Charles *et al.* (2009) are related. The surveys were translated into Bahasa Indonesia and the consumption categories were slightly adapted due to cultural idiosyncrasies of Indonesia. In particular, I included spending on weddings as well as spending to go on pilgrimage as additional categories in the consumption visibility survey. I collected 115 observations at two main sites by visiting people in their homes as well as by asking people on public places: in greater Jakarta and in villages around Yogyakarta. The mean duration of interviews was 35 minutes. The non-response rate was 14 per cent. Efforts were made to make the survey as representative as possible by interviewing individuals from different income classes, with different jobs, different gender and ethnicities. Yet, given the high ethnic, religious and cultural diversity in Indonesia the survey is only a first exploration to the issue of consumption visibility in Indonesia.

This paper uses Heffetz' survey-based measure of expenditure visibility which ranks expenditures by their consumption visibility, i.e. how noticeable these goods are to others. The visibility of a good is determined by sociocultural factors, such as norms, values, and customs. The question from Heffetz' survey was given as follows:

> *"Imagine that you meet a new person who lives in a household similar to yours. Imagine that their household is not different from other similar households, except that they like to, and do, spend more than average on [jewellery and watches]. Would you notice this about them, and if so, for how long would you have to have known them, to notice it? Would you notice it almost immediately upon meeting them for the first time, a short while after, a while after, only a long while after, or never?"*

Replies to this question were coded 1 (almost immediately) to 5 (never). The question was repeated 32 times for each respondent, with [jewellery and watches] in the example above replaced by each of 32 expenditure category titles, randomly ordered (Heffetz, 2011).

This questionnaire tries to elicit the speed with which they would notice an exogenous shock to the tastes of another household. The shock, in turn, ignites the household's deviation from the equilibrium behaviour. I use the same procedure as Heffetz (2011) to create the ranking of goods, i.e. I assign five values from 0 to 1 to the five response options (0, 0.25, 0.5, 0.75 and 1). I calculate the mean value for each category over all respondents.[14] The resulting range of visibility index is 0 (least visible) to 1 (most visible).

Moreover, I make use of a consumption visibility survey developed by Charles *et al.* (2009). It investigates the familiarity an individual needs to determine someone's above average consumption rather than the length of time it would take him to observe this:

> *"Consider a person who lives in a household and community roughly similar to yours. How closely would you have to interact with this person in order to observe that they consistently spend more than average on each of the following categories?"*

Their answers ranged from 1 (indicating that higher than average spending could be observed if the respondent did not interact socially with the person at all) to 5 (indicating that spending would never be observed). I use the same transformation of the raw data as in Charles *et al.* (2009) to create the consumption visibility index. For each consumption category I calculate the proportion of respondents reporting that they are able to observe above-average spending on an item even if relatively unfamiliar with the consumer.[15]

The results from my dataset (shown in Table 4) are similar to the results from the literature. Heffetz (2011) used data from a nationally representative sample from the US, whereas Charles *et al.* (2009) make use of a sample of business students from the University of Chicago. The correlation of the two rankings (indices) in my dataset is 0.86 (0.84). This validation exercise seems to indicate that survey instruments seem to measure a similar underlying latent variable. In particular, the most visible goods are mostly durable and non-durable goods for both of my rankings: clothes, appliances, jewellery, cars, tobacco and 'food out'. The only visible service good is education. The least visible goods tend to be services like insurance policies, legal and accounting fees, telephone charges, utilities, bills, vehicle insurance and alcohol. Yet, there are some important differences

[14] The rankings are robust to using non-linear transformations of the data.

[15] The results from the ranking are robust to using a linear transformation as in Heffetz (2011).

in comparison to Charles *et al.* (2009) and Heffetz (2011): education is perceived to be much more visible in Indonesia. Alcohol is deemed to be less visible by individuals in Indonesia as compared to the U.S., which most likely results from the influences of Islam. Expenditures on furniture and technological devices for recreation are also deemed less visible in Indonesia. These results and rankings are similar in both of my visibility rankings. I also conducted the same analyses based on the sub-sample of observations from rural areas close to Yogyakarta. By and large results are similar, but there are some notable exceptions: specifically, expenditures for charity, mobile phone expenses as well as expenditures on tobacco and pilgrimage are deemed more visible by the rural sub-sample. Individuals in rural areas consider education, cars and jewellery less visible.

The perceived visibility in consumption has potentially important implications for household responses to income shocks. Heffetz (2011) provided evidence that consumption visibility explains up to one third of the consumption elasticity of goods with respect to income shocks. The finding that education is perceived to be a more visible good than in the western world may have important policy implications in Indonesia.

Definition of visible and nonvisible goods. My Indonesian consumption visibility indices indicated that clothing, jewellery, vehicles, education, tobacco and food out are visible goods. Only visible luxuries are hypothesised to be a signal for status. My dataset does not contain any information concerning expenditures on jewellery, vehicles and 'food out'. Therefore, I define the dependent variable as the log of expenditures on clothing and tobacco. The nonvisible goods considered are given by expenditures on miscellaneous goods[16], food at home and alcohol expenses. While alcohol is a highly visible consumption good in the western world, the influence of Islam makes it a highly nonvisible good - in particular in rural Java. My definition of visible and nonvisible categories are similar to the one's used by Charles *et al.* (2009) and Kaus (2013) with the exception of alcohol expenditures. A number of consumption categories are not included in either the visible consumption aggregate or the nonvisible consumption aggregate. First, both education and health expenditures are not included due to the conditionalities of the program which might mechanically induce increases in the consumption goods. Second, expenditures on utilities are excluded due to large supply-side constraints and supply-driven consumption externalities. Third, expenditures on feasts are not considered because of direct effects of an individual's expenditures on feasts on other individuals' utility. Lastly, expenditures

[16] Soap, transport, reading materials, ID/driving license fee, recreation, telephone card, stamps.

on housing-related expenditures are not considered as it is partially reliant on an estimate of rent.

[Insert Table 4 here]

4 Direct and indirect treatment effects

In this section, I test the main implications of the model of conspicuous consumption. I focus on the effect of the conditional cash transfer program on visible and nonvisible expenditures and expenditure shares. I identify the following two effects:

(i) Direct Treatment Effect: I evaluate how receiving the program affects a household's consumption behaviour. To appropriately identify the causal effect of the treatment, I compare the consumption behaviour of households who received the treatment against the consumption behaviour of valid counterfactual households from untreated villages.

(ii) Indirect Treatment Effect: I compare PKH non-recipients' consumption behaviour in treated villages with the consumption behaviour of valid counterfactual households in untreated villages.

4.1 Identification assumptions

Due to the availability of the experimental variation the identifying assumptions needed to assess the causal effect of the program are relatively weak:

(i) Unconfoundedness requires that conditional on observables, X_{ist}, treatment status, pkh_{ist}, is independent of potential outcomes:

$$(Y_{is0}, Y_{is1}) \perp pkh_{ist} | X_{ist}. \tag{2}$$

(ii) The common support assumption necessitates that for all values of the covariates in the vector of independent variables, X_{ist}, I can find households belonging to both the treatment and the control group: This assumption is pivotal for all methods relying on conditional unconfoundedness. In practice, this assumption is satisfied, as I exclude all households without common support after the propensity score estimation.

(iii) The stable unit treatment value assumption (SUTVA) requires that potential outcomes for household i are independent of the treatment status of any other individual k. Any potential externalities from treated to control households are thus explicitly ruled out. For the SUTVA to hold, it is crucial that the counterfactual households are taken from untreated villages. The SUTVA is not violated as I use households from untreated sub-districts as counterfactuals as long as there are no between-cluster spillovers. I test for the prevalence of such spillovers by using a measure of distance to the closest treated sub-district. I find no evidence for such spillovers. These results are available upon request.

(iv) The common trend assumption imposes a common trend for the comparison and the treatment group, i.e. there are no time-varying unobservables that differentially affect the treated and untreated sub-districts. The prime example of such time-varying unobservables in this context are local prices and local supply-side conditions. To control for time-varying unobservables, I include district-trends in the section 6.4.

4.2 Methodology

As evidenced in Table 3, there is baseline balance in terms of outcome variables. In my main specification of interest I use a first-differenced specification which is equivalent to a fixed effects estimator in the case of $t = 2$. Since there is partial treatment contamination, I use the more conservative fixed-effects specification to control for time-invariant locality-differences, such as differences in institutions and local state capacity and time-invariant household-specific differences.[17] Moreover, I control for the month of interview, M_m to take account of seasonal variation of consumption.[18] In particular, my main estimating equation is given by:

$$\triangle C_{ist} = \theta_1 A_s \times Post + \theta_2 M_m + \triangle\varepsilon_{ist}. \tag{3}$$

The main object of interest, the treatment effect, is given by θ_1. One might be concerned that unobservable sub-district level shocks result in correlations of consumption within villages or sub-districts. In addition, one could hypothesize that such shocks are also correlated across time periods. To account for this spatial correlation and the serial correlation of shocks in sub-districts over time, I cluster standard errors at the sub-district level (Angrist and Pischke, 2008). All results are robust to clustering at the village level, which is a less conservative way of clustering the standard errors. In section 6.4, I discuss alternative specifications of my main equation of interest and demonstrate robustness of results.

[17] Results are robust to not including household fixed effects and are available upon request.

[18] Exclusion of the month of interview fixed effects barely changes the coefficient estimates and slightly increases the standard errors. The results are available upon request.

4.3 Results

I test two implications of the model of conspicuous consumption:

Hypothesis 1: Treated households" total expenditure increase and, in particular, their expenditures on visible goods.

Hypothesis 2: Given that treated households increase their expenditures on visible goods, untreated households in treated villages increase their visible consumption expenditures, but lower their nonvisible expenditures holding constant total expenditures. More precisely, the share of visible (nonvisible) expenditures is hypothesised to rise (fall).

4.3.1 The direct treatment effect

The model's predictions are borne out in the data. These results are displayed in Table 5. In line with the hypotheses, total expenditures increase by ca. 8 USD (a 10 per cent (=exp(0.091)) increase), which is slightly lower than the amount that the average household receives in a month from the conditional cash transfer program. This implies that households seem to spend almost the entire cash transfer. This finding is in line with a life-cycle model of consumption with credit-constrained households. It is not compatible with a classical life-cycle model without credit-constraints, in which individuals would be expected to smooth their consumption inter-temporally. Furthermore, I find significant increases in log visible expenditures by 22 per cent (=exp(0.200)) respectively and marginally significant increases in visible expenditures by 1.5 USD which are noisily measured.[19] There is a statistically insignificant increase of nonvisible expenditures by five per cent (=exp(0.042)) and a statistically significant decrease in the expenditure share of nonvisible goods by 2.5 percentage points.

4.3.2 The indirect treatment effect

I compare PKH non-recipients' consumption behaviour in treated villages with the consumption behaviour of counterfactuals in untreated villages. The results on the indirect treatment effect can be found in Table 5. Substantial compositional changes in the consumption of untreated households occur: visible expenditures increase by 3.32 USD or by 30 per cent (=exp(0.275)) and the share of visible expenditure increases in a statistically significant manner by two percentage points. I find a statistically insignificant decrease in expenditures on nonvisible goods by 1.34 USD and a statistically significant decrease

[19] Less than 11 per cent households reported 0 spendings on visible goods at either the baseline or follow-up. These observations are missing in the analyses based on logs. This unlikely induces severe biases, as results from logs and levels are in line.

in nonvisible expenditure shares by three percentage points. The results strongly bear out the testable implications of the model of conspicuous consumption. In other words, I find that households whose reference group consumption on visible goods increases, increase their expenditures on visible goods. Yet, I find no evidence of significant increases in overall expenditures of the untreated households in treated sub-districts. Log total expenditures increase in a statistically insignificant manner by less than 4 per cent (=exp(0.034)).

[Insert Table 5 here]

5 Mechanisms

In this section, I investigate the mechanisms underlying the estimated spillover effects. First, I make use of baseline data on social interactions, village size, punishment norms, log expenditures and the propensity score to investigate treatment heterogeneity of responses to the receipt of the cash program and to shocks of peer group consumption.

5.1 Methodology

To investigate treatment heterogeneity I interact the baseline social activities, propensity score, total expenditures and log population size with a treatment-post-indicator. In addition, I also interact the treatment with an indicator on whether there are social punishment norms. In particular, the village head is asked whether households are punished when they do not contribute to community projects. Social activity data is based on a rich set of household variables indicating attendance of social organizations, such as religious study groups, neighbourhood associations, and women's groups, but also on many self-help organizations. I use social activities as a proxy for the degree of anonymous interactions in a given village. The degree of social activities is operationalised by the average number of social organisations in a given village attended by the poor households in my sample as well as by a household-level measure of social activities.

5.2 Social and economic mechanisms

Direct Treatment Effect. As evidenced in Table 6, I find that the response to the treatment of the treated is more pronounced for households with lower baseline expenditure, with a higher propensity to receive the program and in villages without social punishment norms. The response of total expenditure and, in particular, visible expenditures and expenditure shares is only statistically significant in villages without such social punishment norms. I make the assumption that the social punishment norms constitute a proxy for the degree of social sharing constraints. There are two potential mechanisms with similar predictions through which social punishment norms could affect the treated household's expenditures on visible goods:

(i) I assume that only village heads and treated households knew about the conditional cash transfer program to avoid envy among non-recipients. Then, if the sharing constraints (which can be thought of as taxes) only arise as a result of increases in visible expenditures (as they reveal that households received the cash transfer), I expect to find smaller increases in visible expenditures in villages with social punishment norms and larger increases in villages without social punishment norms.

(ii) Social punishment norms could alternatively proxy for the punishment of economic actions that impose negative externalities on others. Visible consumption of a household results in negative externalities on others (if the assumption of relative concerns is true). Therefore, I expect a lower increase in visible expenditures (which are revealing of the cash transfer) in villages with punishment norms, whereas I predict stronger increases when there are no punishment norms.

Indirect Treatment Effect. As can be seen in Table 6, I find that the indirect effects are larger for log visible expenditures, total expenditures and nonvisible expenditures for households with initially lower total expenditure. I find no treatment heterogeneity by baseline expenditure for visible expenditure shares, but I find that the share of nonvisible expenditures decreases by less for originally poorer households. The spillover effects on visible expenditure shares are lower in villages with more social activities and for households with more social activities. This finding is related to the hypothesis by Veblen (1899) that in societies with more anonymous interactions, conspicuous consumption will be more prevalent. In other words, individuals do not want to signal their status to their neighbours and friends (who already possess a large amount of information on the household's relative economic position), but to strangers (without information about the household's economic position). In other words, in villages with a high level of social activities and high social capital I find no significant spillovers in visible expenditures and visible expenditure shares. Similarly, households with many social activities already reveal a lot of economic information through social interactions with others, which in turn lowers the pay-off of conspicuous consumption. Moreover, these results on treatment heterogeneity seem to rule out an explanation of the patterns in the data by sharing of visible goods. Specifically, if we assume that sharing is more prevalent in places with more social activities and more "social capital" then we would expect the spillover effects to be larger. Yet, this is the exact opposite of what I observe in the data.

[Insert Table 6 here]

Robustness. The results are robust to including all interaction terms in the regression specification at once.[20] These results are reported in Table 10, Appendix B. Also, the results are robust to using a sample-split method, where households are grouped into below or above median baseline values for the above six interaction-terms. My results on social punishment norms are also robust to using household-level data on punishment norms rather than village-level data. These results are available upon request.

6 Alternative explanations and robustness

This section examines alternative explanations of the results. I consider the potential of income effects, non-linear Engel curves and local price effects to explain the patterns in the data. Finally, I demonstrate robustness of results by estimating different model specifications.

6.1 Income effects

Total expenditures of the untreated households did not increase in a significant manner. Yet, the point estimates indicate a statistically insignificant increase of total expenditures by 4 percentage points. Could this small increase in total expenditures of the untreated explain their change in expenditure shares? Visible expenditures are luxuries. Therefore, it would be possible that the increase in visible expenditure shares of the untreated households could be explained by the increase in total expenditures, i.e. by a "luxury-effect". In particular, the increased overall expenditures could explain the increase in visible expenditures if the estimated elasticity of visible expenditures with respect to total expenditures is sufficiently high.

I use the treatment effect on log expenditures and log visible consumption to calculate the implied elasticities of visible expenditures with respect to total expenditures. I find that

[20] As village and household level social activities are highly correlated, only the latter are included. Results are similar if we include village-level social activities rather than household-level activities.

for the treated the implied elasticity (standard errors in brackets) is given by 1.79 (.61). The implied elasticity of visible expenditures (standard error in brackets) with respect to total expenditures for the untreated households is given by 9.82 (10.82). To rationalise these results, I would have to make very strong assumptions about the preferences of treated and untreated households. It seems rather implausible that while the expenditure shares are quite similar for the treated and untreated (and their counterfactuals) at baseline, the responses to income shocks are fundamentally different.

6.2 Non-linear Engel curves

A further possible rationalisation of the data are non-linear Engel curves of visible expenditure shares with a hump-shaped form. In particular, Engel curves of visible expenditure shares that decrease at a certain total expenditure level. Specifically, the treated households increase their total expenditures by approximately 10 per cent, while not significantly changing their visible expenditure shares. On the other hand, the untreated households in treated villages increase their total expenditures by less than 4 per cent, while increasing their visible expenditure shares substantially. Figure 3 depicts how a non-linear hump-shaped Engel curve could explain the patterns in the data. In particular, let the baseline mean log expenditures of treated and untreated household be denoted by BT and BU respectively. Similarly, the follow-up mean log expenditures of the untreated and treated are denoted by FU and FT respectively. Figure 3 shows that the treated households were slightly poorer at baseline than the untreated households. Yet, the treated households have higher levels of expenditures than the untreated households post-treatment. The graph describes the increase in the share of visible expenditures of the untreated and the zero effect of the treated.

[Insert Figure 3]

While the change in log total expenditures for the treated $(FT - BT)$ is approximately twice as large as that of the untreated $(FU - BU)$, visible expenditure shares for the latter increase and remain constant for the former. This illustrates that the data could – in theory – be rationalised by non-linear Engel curves without relative concerns.

Estimating the Engel curves. I find no evidence for hump-shaped patterns in the visible expenditure shares in either the treated or the untreated villages. The Engel curves for the untreated and treated households and their counterfactuals are estimated non-parametrically by local polynomial kernel regressions and are displayed in figures 4 and 5.

Due to the randomisation, there should be no preference heterogeneity between untreated and treated households in treated villages and their counterfactuals. I focus on the change in visible expenditures regressed non-parametrically on the change in total expenditures. I find no evidence of hump-shaped Engel curves for neither treated households and their counterfactuals (as displayed in figure 4) nor untreated households in treated villages and their counterfactuals (as evidenced in Figure 5). These figures emphasize that the Engel curves are well-approximated linearly. Moreover, I also find no evidence of hump-shaped Engel curves when regressing levels in visible expenditure shares non-parametrically on levels in total expenditures. These latter results are omitted for brevity's sake, but are available upon request.

[Insert Figures 4 and 5]

6.3 Goods market and local price effects

There are two goods market mechanisms affecting the untreated households: first, the income of untreated households could increase as they might increase sales to the treated. Second, the cash transfer program might result in local price increases. Thus, what seems like a substantial increase in expenditures on a certain good would just reflect differential price effects between treatment and control sub-districts. This seems unlikely for three reasons:

First, there is evidence that conditional cash transfer programs do not result in significant general equilibrium effects in the form of prices and wages (Angelucci and De Giorgi, 2009; Fiszbein *et al.*, 2009). Even though such large differential price effects for visible goods seem rather unlikely, I demonstrate that the implied supply elasticities needed for the price effects to explain a large fraction of the indirect treatment effects are implausible. I engage in the following thought experiment: how low would the supply elasticity have to be to explain the indirect treatment effects through prices? I calculate the implied supply elasticity for visible expenditures based on (a) the observed change in demand for visible expenditures (given by the direct treatment effect) and (b) the observed visible expenditure changes from the untreated households (given by the indirect treatment effect). If I assume that untreated households have the same expenditure bundles as at baseline, but that the results are completely driven by price effects, then the implied supply elasticity (with the standard error[21] in brackets) for visible expenditures is:

$$\text{supply elasticity} = \frac{\text{change in demand}}{\triangle\text{price}} = 0.07\ (0.02). \tag{4}$$

As the visible consumption of about 10 per cent of the population increases by 22 per cent, overall demand for visible goods in the village increases by at most 2.2 per cent, while visible expenditures of untreated households increase by ca. 30 per cent. The implied supply elasticity of .07 seems implausibly low.

Second, markets are fairly integrated: As there are treated and untreated sub-districts in all districts, then if one store serves treated and control sub-districts any price effect caused by the program will equally affect all sub-districts in the districts, with no differential effect in treatment sub-districts. To test whether there is any relationship between a proxy for market integration, I include variables that capture the distance, travel cost and duration of a journey to the closest market and the capital in the sub-district. If markets and sub-district capitals are distant, one could hypothesise that local price effects are likely to be larger. If there is a systematic relationship between the size of the spillover effect and the distance to markets, then local prices might be driving the results. I interact the treatment-time interaction with the distance measures. To identify the true effect of distance, I also control for the observables used in the propensity score interacted with a *Post* indicator as distance to markets may be correlated with a variety of other variables. Thus, I flexibly control for differential trends resulting from baseline differences in terms of observables. I find that the main results decrease in magnitude but are robust and remain statistically significant. Moreover, I find no evidence that proxies for market integration are able to explain the results. Results using these additional controls are displayed in Table 7 Panel B. Moreover, in Panel F in Table 6, I show that there is no treatment heterogeneity by distance to the district-capital which is used as a proxy for market integration. Yet, if effects were driven by price effect then treatment effects would be expected to be increasing in the distance to the district-capital.

Fourth, to account for local price effects, I deflate prices by the district-specific poverty lines constructed by the Indonesian statistical agency (BPS). District poverty lines evaluate the cost of a food basket and a non-food basket that is deemed to correspond to meeting basic food and non-food needs. As my sample consists of poor households these poverty lines are a good approximation to local price movements that are relevant for

[21] To recover an estimate of the supply elasticity (and its standard error), I use the estimates of the visible expenditure increases for the treated and untreated.

the poor; but clearly they are more suited to reflect price movements in food than price movements in conspicuous goods. Panel I in Table 7 shows that my results are robust to deflating expenditures by district-level poverty lines. Importantly, once prices are deflated I find no more evidence at all of an increase in total expenditures of the untreated. Yet, the effect on visible expenditure shares and log-visible expenditures remains.

6.4 Changes in the Supply Side

To rule out that supply-side changes drive the results, I use data from the census of villages (PODES) from 2005 prior to the treatment and after the treatment 2008 to show that the supply environment was not significantly affected by the receipt of the cash transfer program. In particular, I use data on the number of markets, shopping areas, agricultural production kiosks, small industry firms, farming businesses and mini markets in the village as well as information on the distance to the closest shopping area and market. I find neither baseline differences in market environments between treated and untreated sub-districts nor any significant treatment effects. These results are displayed in Table 12 in Appendix B.

6.5 Robustness

To ensure robustness of results, I estimate several variants of my baseline equation: First, I include the fourth polynomial of the predicted propensity score interacted with a *Post* indicator to account for the propensity of receiving the program. Angrist and Pischke (2008) stresses that the only covariate researchers need to control for is the probability of treatment itself. I include the fourth polynomial as this allows for more flexibility in terms of the underlying selection model. Merely including the propensity score would impose a quite strong linearity assumption in terms of the underlying selection process. As the propensity score is a predicted regressor, I bootstrap the standard errors (with 1,000 replications) and cluster them at the sub-district level. The inclusion of the propensity score in my main specification controls for differential trends by the propensity of receiving the program and living in a treated village. I find that my main results are robust. These results can be found in Panel C. Second, as there may be concerns that 85 clusters may be too small and may result in biases, I cluster standard errors at the village level, which is less conservative in terms of flexibility of correlated error terms, but more efficient as the number of clusters is larger. I find that the standard errors barely change and become slightly lower. This can be seen in Panel D of Table 7.[22] Third, as shown in panel G, I include district-specific time-trends to take account of time-varying unobservables at the district level. The inclusion of district-specific trends increases the size of the treatment effects substantially. Fourth, as is shown in Panel H of Table 7, I exclude obvious outliers from the sample (the bottom one and top one per cent of the expenditure distribution). In particular, I drop outliers to reduce the effect of possible measurement error. I find that the results barely change, i.e. outliers do not drive the results. Fifth, the results are robust to using the visible and nonvisible consumption in levels, logs, per capita levels, per capita logs. Sixth, the main results for the untreated are mostly driven by expenditures on clothing expenditures. As clothing is the most visible good in my consumption visibility survey this result is supportive of the visibility mechanism from my model. This is evidenced in Table 11 in Appendix B. Finally, the results on expenditure shares are not merely driven by food consumption. As a further robustness check, I define expenditure shares of visible and nonvisible goods (excluding food) in terms of overall non-food consumption. The patterns in the data are the same as beforehand. In particular, visible expenditure shares increase significantly, while nonvisible expenditure shares decrease (though not significantly). This shows that food consumption is important for the magnitude of the effects on nonvisible expenditure shares, but that the share of other nonvisible goods (miscellaneous goods and alcohol) move into the same direction. These latter results are available on request.

LATE and ITT. We might be concerned that – even though the treatment contamination did not result in baseline imbalances – there may be systematic differences between treated and untreated sub-districts in terms of unobservables. In our case, however, such concerns are less severe as the contamination of treatment resulted from political pressures and not from direct choices of households to take up or not to take up the program. To mitigate any concerns that the treatment effects are biased due to differences in unobservables, I instrument the actual assignment of the treatment with the original assignment of the treatment, i.e. I identify the local average treatment effect, LATE (Imbens and Angrist, 1994). With a randomly assigned treatment, LATE is the effect of the treatment on those who comply with the offer but are not treated otherwise (Angrist and Pischke, 2008). As a further check, I use the original treatment assignment to estimate the intent-to-treat effect (Angrist and Pischke, 2008). I find that results are robust and remain statistically and economically significant. These ITT and IV results can be found

[22] Moreover, clustering the standard errors at the district-level does not change the results. These results are available upon request.

in Panel E and F of Table 7 respectively.

[Insert Table 7 here]

7 Conclusion

This paper contributes to the literature on conspicuous consumption and on peer effects in consumption more generally. This paper is the first – based on an exogenous variation – to systematically investigate how peer effects in consumption differ by a good's visibility.

I show that the implications of the model of conspicuous consumption are strongly borne out in the data: first, treated households increase their total expenditures and, in particular, their visible expenditures. Second, I find substantial and robust evidence on peer effects: the untreated households in treated villages increase their visible expenditures and increase (decrease) expenditure shares on visible (nonvisible) goods. Moreover, I shed light on the social mechanisms underlying the tendency to consume visible goods. In villages with more social interactions there is almost no increase in visible expenditures of the untreated. On the other hand, in villages with low social activities there is a strong increase in visible expenditures for the untreated. Interestingly, this evidence is consistent with the idea by Veblen (1899) that conspicuous consumption is higher in environments with more "anonymous interactions" and lower "social capital". Furthermore, I find treatment heterogeneity by social punishment norms. Treated households in villages with social punishment norms do not increase their expenditures and expenditure shares on visible goods, whereas treated households in villages without social punishments norms increase their expenditures on visible goods. Finally, I rule out alternative mechanisms that could drive the results. Specifically, I provide evidence that neither local price effects nor non-linear Engel curves or income effects explain the patterns observed in the data.

This evidence highlights the need to explicitly account for social status in models of consumption behaviour. The effects on treatment heterogeneity accentuate the importance of the social context in shaping economic behaviour. Finally, my findings emphasise the need to consider indirect treatment effects in the cost-benefit analysis of conditional cash transfer programs.

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Main tables

Table 1: Baseline characteristics of households in treated and untreated sub-districts

	Treated sub-districts	Untreated sub-districts	P-value: Equality
Average Consumption			
Total Expenditures	95.67	98.31	0.607
Visible Expenditures	10.29	10.91	0.439
Nonvisible Expenditures	57.62	60.62	0.295
Food Expenditures	55.34	58.21	0.282
Share of Visible Goods	.109	.112	0.637
Share of Nonvisible Goods	.671	.666	0.620
Average Demographics (Household Head)			
Household Size	5.041	5.14	0.465
Social Activities	1.51	1.55	0.802
Age	43.64	43.89	0.670
Share: No Schooling	.312	.286	0.479
Share: Main Profession: Agriculture	.600	.575	0.582
Share: Previous Government Aid	.936	.917	0.194
Average Village Characteristics			
Percentage of households without electricity	.023	.020	0.688
Percentage of farmers	68.60	67.22	0.636
Percentage Islam	92.39	95.22	0.517
Population Size	4,813	4,691	0.815
Distance district capital	22.37	21.95	0.872
Number of Sanctions	0.656	0.628	0.840
N	1840	1480	3399

The expenditures are monthly household-level expenditures in USD (1 USD = 10000 IRP) at 2007 prices. To test the equality between baseline characteristics standard errors were clustered at the village level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Logit estimation underlying the propensity score

Log Total Expenditures	0.274	-1.038
	(0.917)	(0.669)
Log Total Expenditures squared	-0.012	0.037
	(0.033)	(0.023)
Log Food Expenditure	-0.023	-0.035
	(0.053)	(0.052)
Population Size	0.000	0.000
	(0.000)	(0.000)
Distance to District Capital	-0.000	-0.001
	(0.003)	(0.003)
Average Wave Village	0.001	-0.000
	(0.004)	(0.004)
Base Percentage Islam	-0.002	-0.002
	(0.002)	(0.002)
Household Size	0.053	0.057
	(0.035)	(0.035)
Household Size Squared	-0.004	-0.003
	(0.003)	(0.003)
Household Head: Age	-0.003	-0.009
	(0.006)	(0.006)
Household Head: Age Squared	0.000	0.000
	(0.000)	(0.000)
Number of Sanctions	0.039	-0.031
	(0.032)	(0.035)
Social Activities	-0.007	-0.013
	(0.018)	(0.017)
Percentage of Farmers	0.001	0.001
	(0.001)	(0.001)
Number of Families Receiving Raskin	0.026	0.033
	(0.023)	(0.024)
No Schooling	0.035	0.022
	(0.040)	(0.037)
Junior High School	-0.005	0.032
	(0.040)	(0.037)
Secondary School	0.060	0.117**
	(0.056)	(0.047)
University	0.115	0.451*
	(0.241)	(0.242)
TV	-0.088**	-0.023
	(0.037)	(0.035)
Refrigerator	-0.230*	0.049
	(0.123)	(0.060)
Bicycle	0.043	0.095
	(0.059)	(0.061)
Motorcycle	-0.094**	0.049
	(0.046)	(0.037)
Main Occupation: Agriculture	0.003	0.016
	(0.039)	(0.037)
Number of children aged between zero and two	0.010	-0.051**
	(0.022)	(0.023)
Number of children aged between three and six	0.036*	-0.064***
	(0.020)	(0.020)
Number of children aged between seven and fifteen	0.006	-0.068***
	(0.018)	(0.017)
Observations	2,096	2,373

Mean marginal effects; Standard errors clustered at the sub-district level in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Baseline characteristics of treated households and their counterfactuals and untreated households in treated villages and their counterfactuals

	Treated Households			Untreated Households		
Consumption	Treated Village	Untreated Village	p-Value	Treated Village	Untreated Village	p-Value
Total Expenditures	89.49	96.76	.152	100.76	99.02	.785
Visible Expenditures	10.69	11.14	.607	10.59	11.27	.459
Nonvisible Expenditures	57.40	60.66	.283	57.86	60.89	.326
Food Expenditures	55.02	58.27	.260	55.55	58.47	.321
Share of Visible Goods	0.12	0.12	.861	0.11	0.12	.423
Share of Nonvisible Goods	0.68	0.67	.339	0.66	0.66	.949
Demographics (Household Head)						
Household Size	5.12	5.17	.760	5.02	5.18	.284
Social Activities	1.52	1.58	.742	1.53	1.58	.758
Age	42.82	43.90	.085*	44.21	43.92	.659
Share: No Schooling	0.32	0.29	.417	0.30	0.29	.730
Share: Main Profession: Agriculture	0.96	0.92	.681	0.92	0.92	.655
Share: Previous Government Aid	0.958	0.916	0.008***	0.918	0.915	0.998
N	726	1370	2096	997	1376	2374

The expenditures are monthly household expenditures in USD at 2007 prices. Standard errors underlying the p-value are clustered at the sub-district level.
$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Consumption Visibility rankings based on my Indonesian sample

Consumption Categories	Charles et al. (2009) Index	Charles et al. (2009) Rank	Heffetz (2011) Index	Heffetz (2011) Rank
Clothing (Clo)	.526	1	.656	1
Education (Edu)	.385	2	.562	2
Jewellery (Jwl)	.380	3	.535	5
Nonformal Education(Nonf Edu)	.361	4	.533	6
Food Out (FdO)	.350	5	.548	3
Tobacco (Cig)	.312	6	.533	7
Food at home (FdH)	.308	7	.523	8
Automobiles (Car)	.298	8	.548	3
Tvs etc. (Ot1)	.283	9	.460	13
Car Maintenance (CMn)	.280	10	.433	17
Barbershop (Brb)	.279	11	.488	9
Wedding (Wed)	.273	12	.470	12
Charity (Cha)	.265	13	.473	11
Housing (Hom)	.256	14	.342	27
Medical Expenditures (Med)	.256	14	.406	20
Furniture (Fur)	.247	16	.457	15
Mobile (Cel)	.247	16	.432	18
Pilgrimage (Pil)	.245	18	.368	26
Books (Bks)	.236	19	.477	10
Alcohol Out (AlO)	.221	20	.379	24
Technological Devices (Ot2)	.219	21	.387	23
Petrol, Gas (Gas)	.219	21	.453	16
Utilities (Utl)	.203	23	.393	21
Public Transport (Bus)	.201	24	.459	14
Plane Tickets (Air)	.176	25	.391	22
Home Insurance (HIn)	.166	26	.227	31
Laundry (Lry)	.146	27	.407	19
Alcohol at Home (AlH)	.141	28	.315	28
Car Insurance (CIn)	.140	29	.276	29
Legal Fees, etc (Fee)	.140	29	.227	30
Phone Expenses (Tel)	.090	31	.370	25
Life Insurance (LIn)	.087	32	.212	32
N	115	115	115	115

In brackets next to the good categories is the abbreviation used by Heffetz (2011). A detailed description of the good categories can be found in Appendix C.

Table 5: Main Results: Direct and Indirect Treatment Effect

Panel A: Direct Treatment Effect

	Total Expenditure	Total log Expenditure	Total Visible Expenditure	Log Visible Expenditure	Share Visible Expenditure	Nonvisible Expenditure	Log Nonvisible Expenditure	Share Nonvisible Expenditure
Treatment	8.21*	0.091**	1.51	0.200**	0.002	2.54	0.042	-0.025
Effect	(4.81)	(0.039)	(1.10)	(0.080)	(0.007)	(2.59)	(0.035)	(0.016)
Number of households	2022	2022	2022	1806	2022	2022	2022	2022

Panel B: Indirect Treatment Effect (Peer Effect)

	Total Expenditure	Total log Expenditure	Total Visible Expenditure	Log Visible Expenditure	Share Visible Expenditure	Nonvisible Expenditure	Log Nonvisible Expenditure	Share Nonvisible Expenditure
Treatment	-.44	0.034	3.32**	0.275***	0.020**	-1.34	-0.014	-0.030**
Effect	(6.92)	(0.043)	(1.39)	(0.086)	(0.008)	(2.51)	(0.037)	(0.015)
Number of households	2299	2299	2299	2018	2299	2299	2299	2299
Household Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes
Month controls	yes	yes	yes	yes	yes	yes	yes	yes

Standard errors clustered at the sub-district level in parentheses. The expenditures are monthly household-level expenditures in USD at 2007 prices. All specifications include a time trend. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Treatment Heterogeneity

	Direct Treatment Effect on the treated					Indirect Treatment Effect on the untreated				
	Total Log Expenditure	Log Visible Expenditure	Log Nonvisible Expenditure	Expenditure Share Visible	Expenditure Share Nonvisible	Total Log Expenditure	Log Visible Expenditure	Log Nonvisible Expenditure	Expenditure Share Visible	Expenditure Share Nonvisible
Panel A:										
Treatment × Post	0.028	0.087	0.016	-0.005	-0.005	0.046	0.230**	-0.010	0.018*	-0.029
	(0.047)	(0.095)	(0.041)	(0.008)	(0.018)	(0.046)	(0.096)	(0.041)	(0.010)	(0.018)
Treatment × Post × No Social Sanction	0.131***	0.234**	0.054	0.014*	-0.042**	-0.024	0.085	-0.009	0.004	-0.002
	(0.045)	(0.095)	(0.048)	(0.008)	(0.017)	(0.044)	(0.074)	(0.035)	(0.009)	(0.016)
Panel B:										
Treatment × Post	0.159***	0.230**	0.070	0.000	-0.047**	0.073	0.400***	-0.020	0.031***	-0.058***
	(0.051)	(0.106)	(0.044)	(0.009)	(0.020)	(0.055)	(0.103)	(0.044)	(0.009)	(0.017)
Treatment × Post × Household Social Activities	-0.045***	-0.020	-0.019	0.001	0.014**	-0.025	-0.081**	0.004	-0.007***	0.018***
	(0.017)	(0.045)	(0.013)	(0.003)	(0.007)	(0.017)	(0.034)	(0.012)	(0.003)	(0.006)
Panel C:										
Treatment × Post	0.131**	0.164	0.046	-0.002	-0.044*	0.085	0.391***	-0.022	0.033***	-0.068***
	(0.061)	(0.108)	(0.051)	(0.009)	(0.023)	(0.071)	(0.129)	(0.053)	(0.012)	(0.022)
Treatment × Post × Village-level social activities	-0.026	0.024	-0.003	0.002	0.012	-0.033	-0.076	0.005	-0.009*	0.025**
	(0.022)	(0.048)	(0.017)	(0.004)	(0.009)	(0.027)	(0.055)	(0.019)	(0.004)	(0.011)
Panel D:										
Treatment × Post	0.390*	0.424	0.328	-0.030	-0.006	0.217	0.146	0.140	-0.049	0.034
	(0.223)	(0.608)	(0.218)	(0.039)	(0.090)	(0.263)	(0.632)	(0.207)	(0.047)	(0.109)
Treatment × Post × Log Population Size	-0.036	-0.027	-0.035	0.004	-0.002	-0.022	0.016	-0.019	0.008	-0.008
	(0.027)	(0.072)	(0.026)	(0.004)	(0.011)	(0.032)	(0.078)	(0.026)	(0.006)	(0.013)
Panel E:										
Treatment × Post	-0.250**	-0.038	-0.212***	-0.002	0.016	-0.002	0.181	-0.205**	0.023	-0.066*
	(0.095)	(0.179)	(0.075)	(0.014)	(0.028)	(0.118)	(0.208)	(0.096)	(0.020)	(0.033)
Treatment × Post × Propensity Score	0.904***	0.629	0.673***	0.009	-0.110*	0.082	0.213	0.431**	-0.008	0.080
	(0.232)	(0.399)	(0.177)	(0.033)	(0.065)	(0.281)	(0.466)	(0.214)	(0.048)	(0.076)
Panel F:										
Treatment × Post	9.486***	10.274***	6.045***	0.059	-1.834***	9.913***	7.877***	4.212***	0.046	-2.362***
	(0.614)	(1.716)	(0.517)	(0.121)	(0.247)	(0.532)	(1.371)	(0.632)	(0.113)	(0.184)
Treatment × Post × Log Expenditure	-0.691***	-0.741***	-0.442***	-0.004	0.133***	-0.725***	-0.557***	-0.310***	-0.002	0.171***
	(0.045)	(0.126)	(0.038)	(0.009)	(0.018)	(0.039)	(0.100)	(0.046)	(0.008)	(0.014)
Panel G:										
Treatment × Post	0.067	0.130	0.026	-0.006	-0.015	0.052	0.159	-0.010	0.010	-0.029
	(0.054)	(0.134)	(0.048)	(0.011)	(0.023)	(0.075)	(0.156)	(0.057)	(0.015)	(0.027)
Treatment × Post × Distance to district capital	0.001	0.003	0.001	0.000	-0.000	-0.001	0.005	-0.000	0.000	-0.000
	(0.002)	(0.005)	(0.002)	(0.000)	(0.001)	(0.003)	(0.006)	(0.002)	(0.001)	(0.001)
Household Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Month FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Number of households	2022	1806	2022	2022	2022	2299	2018	2299	2299	2299

Standard errors are clustered at the sub-district level. Baseline covariates are interacted with the treatment-post indicator. All specifications include a time trend. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Robustness of direct and indirect treatment effects

	Direct Treatment Effect on the treated					Indirect Treatment Effect on the untreated				
	Total Log Expenditure	Log Visible Expenditure	Log Nonvisible Expenditure	Expenditure Share Visible	Expenditure Share Nonvisible	Total Log Expenditure	Log Visible Expenditure	Log Nonvisible Expenditure	Expenditure Share Visible	Expenditure Share Nonvisible
Panel A: Main specification	0.091**	0.200**	0.042	0.002	-0.025	0.034	0.275***	-0.014	0.020**	-0.030**
	(0.039)	(0.080)	(0.035)	(0.007)	(0.016)	(0.043)	(0.086)	(0.037)	(0.008)	(0.015)
Panel B: baseline controls	0.086**	0.183**	0.043	0.001	-0.020	0.037	0.239***	-0.010	0.018**	-0.029**
	(0.037)	(0.078)	(0.034)	(0.007)	(0.016)	(0.043)	(0.084)	(0.037)	(0.008)	(0.014)
Panel C: propensity score	0.044	0.148*	0.003	0.001	-0.022	0.034	0.275***	-0.014	0.020**	-0.030**
	(0.039)	(0.085)	(0.035)	(0.007)	(0.016)	(0.042)	(0.090)	(0.036)	(0.008)	(0.015)
Panel D: cluster at the village-level	0.091***	0.200***	0.042	0.002	-0.025**	0.034	0.275***	-0.014	0.020***	-0.030***
	(0.032)	(0.068)	(0.030)	(0.006)	(0.011)	(0.032)	(0.067)	(0.028)	(0.006)	(0.011)
Panel E: intent-to-treat (ITT)	0.079**	0.188**	0.042	-0.000	-0.019	0.010	0.205***	-0.023	0.014**	-0.023
	(0.039)	(0.075)	(0.035)	(0.006)	(0.016)	(0.043)	(0.075)	(0.037)	(0.007)	(0.015)
Panel F: LATE	0.082**	0.195***	0.044	-0.000	-0.019*	0.011	0.230***	-0.026	0.016**	-0.026**
	(0.033)	(0.069)	(0.032)	(0.006)	(0.012)	(0.038)	(0.076)	(0.032)	(0.007)	(0.013)
Panel G: with District Trends	0.114***	0.193**	0.051	0.001	-0.030**	0.001	0.280***	-0.031	0.024***	-0.023*
	(0.038)	(0.079)	(0.032)	(0.006)	(0.014)	(0.043)	(0.089)	(0.033)	(0.007)	(0.013)
Panel H: without outliers	0.094**	0.200**	0.038	0.001	-0.027*	0.041	0.272***	-0.007	0.019**	-0.032**
	(0.039)	(0.079)	(0.035)	(0.007)	(0.016)	(0.041)	(0.085)	(0.037)	(0.008)	(0.015)
Panel I: Deflated by	0.059	0.166**	0.010	0.002	- 0.025	0.006	0.245***	-0.042	0.020**	-0.030**
district poverty lines	(0.050)	(0.083)	(0.045)	(0.007)	(0.016)	(0.051)	(0.090)	(0.044)	(0.008)	(0.015)
Household Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Month FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Number of households	2022	1806	2022	2022	2022	2299	2018	2299	2299	2299

All specifications include a time trend. Standard errors are clustered at the sub-district level in all Panels except D and F, where they are clustered at the village-level. In Panel B all baseline covariates used in the propensity score matching are interacted with a *Post* indicator. In Panel C the vector of propensity scores is interacted with a *Post* indicator. Standard errors for this specification are bootstrapped as the propensity scores are predicted values. In Panel E, I identify the intent-to treat effect by using the original treatment assignment. In Panel F, I identify the local average treatment effect, i.e. I instrument the actual assignment of the treatment with the original treatment assignment. In this specification I cluster standard errors at the village level to have a sufficient number of clusters available to compute the cluster-robust covariance matrix. The cluster-robust Angrist-Pischke first stage F-stat is above 3000 for both the direct and indirect treatment effect with an associated p-value of 0.000 for both, i.e. no weak instrument problems are present. In panel G, include district-specific time trends. In panel H I remove the top and bottom 1 per cent of the expenditure distribution. In panel I I deflate the expenditure data by district-specific poverty lines. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Main Figures

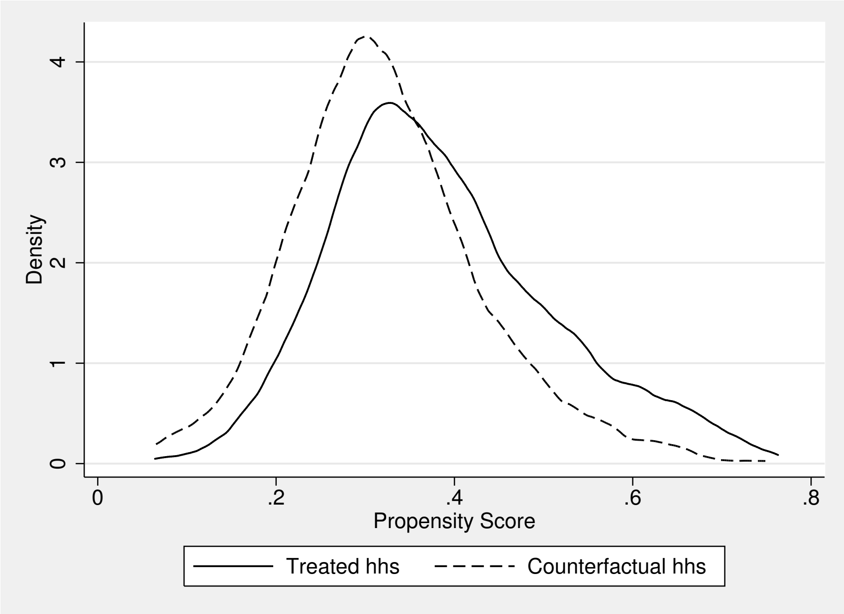

Figure 1: Kernel density of propensity score of treated households and counterfactual households.

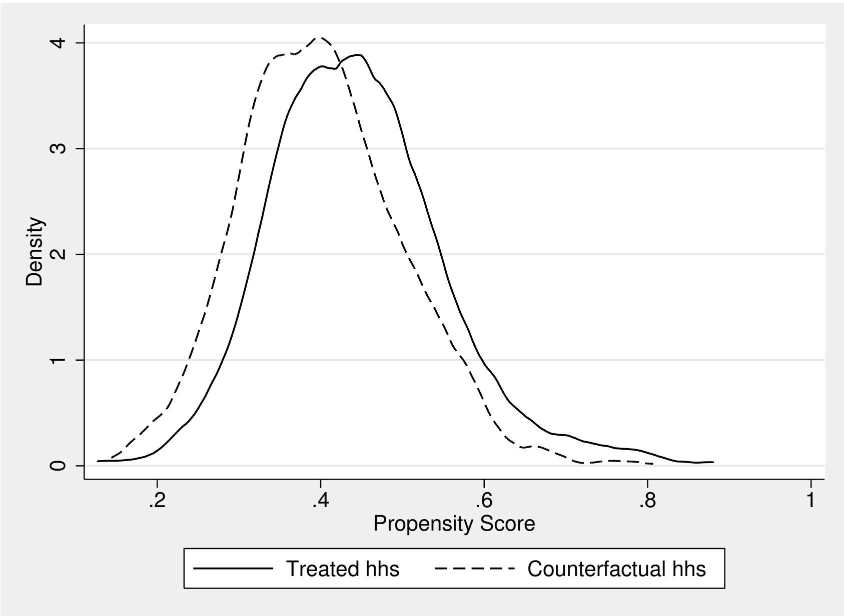

Figure 2: Kernel density of the propensity score of untreated households in treated sub-districts and their counterfactuals.

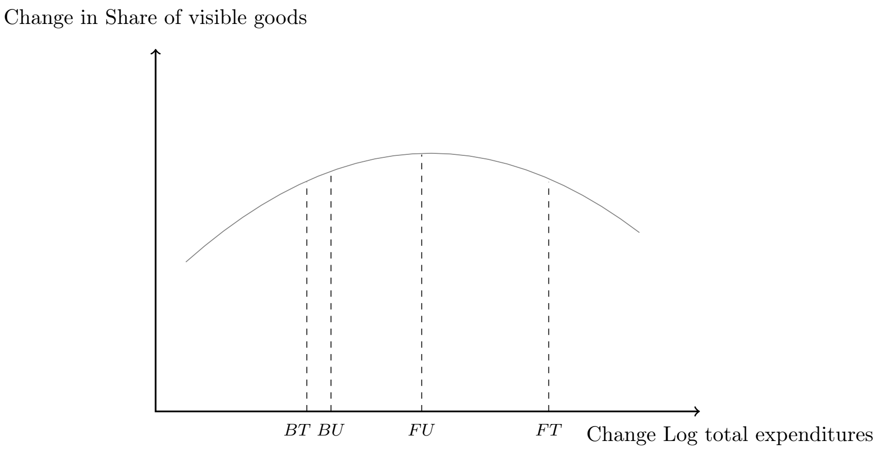

Figure 3: Non-linear Engel curve of visible expenditure shares

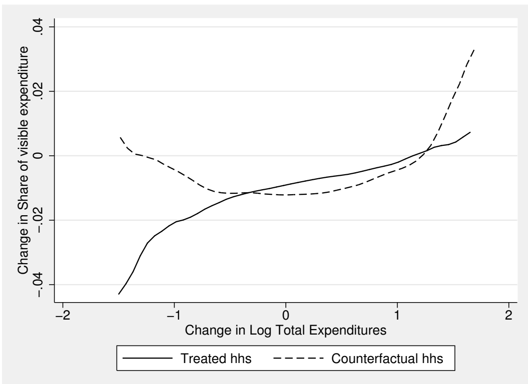

Figure 4: Non-linear Engel curve of changes in visible expenditure shares: Treated vs. untreated households

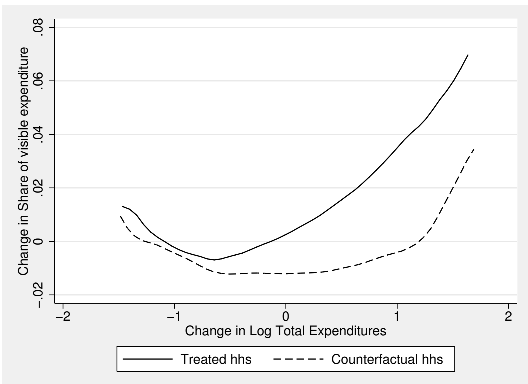

Figure 5: Non-linear Engel curve of changes in visible expenditure shares: Untreated households in treated sub-districts vs. untreated counterfactual households

Appendix A: Model

Assumption 1 (Player Utility). *There are two identical players*[23]*, player i and player j, who each noncooperatively maximize their utility function subject to their budget constraint:*

$$U_i = \theta \ln(n_i) + (1-\theta)\ln(v_i + \alpha(v_i - v_j)), \textit{where } \alpha > 0 \;\; \textit{s.t.} \;\;\; n_i + v_i p_v = y_i. \tag{5}$$

Remark. Both players care about nonvisible goods only in absolute terms, whereas they care about visible goods both in absolute and relative terms. The price of nonvisible goods is normalised to 1, the price of visible goods is given by p_v, and an individual's income is given by y_i. Individual j's utility function and budget constraint are identical to i's utility function and budget constraint. I do not assume homogeneity in income, i.e. agents are allowed to have different levels of income. For now, I do not allow for heterogeneity in comparison concerns and assume that α is the same for both players. As the relative concerns parameter is positive, $\alpha > 0$, an increase in player $j's$ level of visible consumption decreases player i's utility and vice versa. Further, I assume that individuals care about both visible and nonvisible goods, i.e. $\theta \in (0,1)$. Crucially, I make the following two restrictions on the players' levels of visible consumption to ensure non-negativity of the argument of the log:

$$(1+\alpha)v_i \geq \alpha v_j; \tag{6}$$

$$(1+\alpha)v_j \geq \alpha v_i. \tag{7}$$

As will be shown subsequently, these conditions are satisfied in equilibrium.

Assumption 2 (Information Structure). *The players' rationality and their preferences are all common knowledge.*

The solution concept used is Nash equilibrium. Solving the model's constrained optimization problem yields the following best response function for player i:

Proposition 1 (Best Response Functions). *Player i's best response is given by:*

$$v_i(v_j) = \left(\frac{\theta\alpha}{1+\alpha}\right) v_j + (y_i/p_v)(1-\theta). \tag{8}$$

Remark. Individual i's level of visible consumption is increasing in $j's$ level of visible consumption, i.e. v_i and v_j are strategic complements. The effect of an increase in j's level of visible consumption can be expressed succinctly as follows:

$$\frac{\partial v_i}{\partial v_j} = \frac{\theta\alpha}{1+\alpha} > 0 \text{ given } \alpha, \theta > 0. \tag{9}$$

[23]By players I mean households – which for simplicity's sake – shall be conceived of as a single decision unit. In other words, I abstract from intrahousehold bargaining considerations.

The intuition for this result is as follows: an increase in j's visible consumption increases i's marginal utility from consuming v_i. Therefore, v_i is increasing in v_j and vice versa. The magnitude of effects will depend positively on the magnitude of relative concerns, α, as well as the relative weight θ put on nonvisible consumption. This may strike as counter-intuitive at first sight. But a high θ implies that the relative share of visible goods is relatively small, which in turn implies that the marginal utility of visible consumption increases more substantially given an increase in v_j.

For equilibrium characterization, I assume that $y_i = y_j = y$ to simplify the calculations. Exploiting the resulting symmetry of the problem, I solve for the unique Nash equilibrium value of visible consumption given by:

$$v_i^* = v_j^* = \left(\frac{(1+\alpha)(1-\theta)}{1+\alpha(1-\theta)}\right)(y/p_v). \tag{10}$$

Crucially, the non-negativity constraints are satisfied as $v_i^* = v_j^*$. Plugging the equilibrium value into the budget constraint yields the equilibrium value of nonvisible consumption:

$$n_i^* = n_j^* = \frac{\theta y}{1+\alpha(1-\theta)}. \tag{11}$$

Comparative statics

I analyse how i's equilibrium level of conspicuous consumption is affected by (i) i's relative concerns α, (ii) i's own income y_i as well as (iii) j's income, y_j.

Proposition 2 (Relative Concerns). *An individual's visible consumption is increasing in the parameter measuring the magnitude of relative concerns, α, whereas an individual's nonvisible consumption is decreasing in α:*

$$\frac{\partial v_i^*}{\partial \alpha} = \frac{(y_i/p_v)(1-\theta)\theta}{[1+\alpha(1-\theta)]^2} > 0; \tag{12}$$

$$\frac{\partial n_i^*}{\partial \alpha} = -\frac{(1-\theta)\theta y_i}{[1+\alpha(1-\theta)]^2} < 0. \tag{13}$$

Remark. Changes in relative concerns α affect the slope of the best-response function. The higher the parameter capturing relative concerns, the steeper the best response function. In other words, the higher an individual's relative concerns, the more responsive the individual is to changes in visible consumption by the other player and the higher the individual's income share devoted to visible goods in equilibrium. i's visible consumption is increasing in the relative concerns of j.

Proposition 3 (Own Income). *Both visible and nonvisible consumption are increasing in own income:*

$$\frac{\partial v_j^*}{\partial y_j} = (1/p_v)(1-\theta) > 0; \tag{14}$$

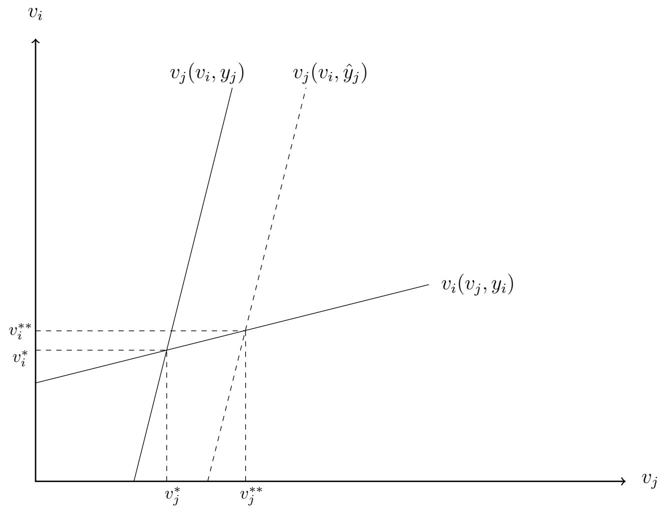

Figure 6: Comparative Statics: i's best response to a rise in $j's$ income from y_j to $\hat{y}_j$

$$\frac{\partial n_j^*}{\partial y_j} = \frac{\theta}{1+\alpha(1-\theta)} > 0. \tag{15}$$

Remark. The larger $(1-\theta)$, the larger the response of visible consumption to own income. The larger the price of visible goods p_v, the lower an individual's response in the face of income changes.

Proposition 4 (Effect of an Increase in j's Income on i's Consumption)**.** *The visible consumption of i is increasing in j's income and vice versa, whereas i's nonvisible consumption is decreasing in j's income:*

$$\frac{\partial v_i(v_j(y_j))}{\partial y_j} = \frac{\partial v_i}{\partial v_j}\frac{\partial v_j}{\partial y_j} = \left(\frac{\theta\alpha}{1+\alpha}\right)(1/p_v)(1-\theta) > 0; \tag{16}$$

$$\frac{\partial n_i(v_i(v_j(y_j)))}{y_j} = \frac{\partial n_i}{\partial v_i}\frac{\partial v_i}{\partial v_j}\frac{\partial v_j}{\partial y_j} = -\left(\frac{\theta\alpha}{1+\alpha}\right)(1-\theta) < 0. \tag{17}$$

Remark. As described in Figure 6, the increase in y_j is reflected in an outward shift of j's best response curve, which results in an upward movement along i's best response curve. Figure 6 shows that the best response behaviour of i will take into account the effect of j's income change on j's level of visible consumption, which in turn has repercussions for i's optimal level of visible consumption. As j's income increase results in an increase of j's visible consumption, i's visible spending will increase, whereas i's nonvisible spending will decrease. The impact of an increase in j's income has a larger effect on j's visible consumption v_j than on i's level of visible consumption, as

$$\frac{\partial v_i}{\partial v_j} = \frac{\theta\alpha}{1+\alpha} < 1. \tag{18}$$

Appendix B: Additional tables

Table 8: Baseline characteristics of households in treated and untreated sub-districts in the original treatment assignment

	Treated sub-districts	Untreated sub-districts	P-value: Equality
Average Consumption			
Total Expenditures	96.79	96.91	.982
Visible Expenditures	10.35	10.75	.611
Nonvisible Expenditures	57.86	59.90	.470
Food Expenditure	55.75	57.40	.540
Share of Visible Goods	0.11	0.11	.515
Share of Nonvisible Goods	0.67	0.67	.595
Average Demographics (Household Head)			
Household Size	5.06	5.11	.709
Social Activities	1.49	1.56	.643
Age	43.77	43.74	.950
Share: No Schooling	0.33	0.27	.091*
Share: Agriculture	0.62	0.56	.174
Share: Previous Government Aid	0.94	0.92	.059*
Average Village Characteristics			
Percent without Electricity	0.02	0.02	.635
Percentage Islam	93.55	93.74	.966
Percentage of Farmers	70.27	66.04	.141
Population Size	4,942	4,604	.538
N	1560	1839	3399

The expenditures are monthly household-level expenditures in USD at 2007 prices.

To test the equality between baseline characteristics standard errors were clustered at the village level.

Table 9: Baseline characteristics of contaminated vs. uncontaminated subdistrics.

	Contaminated sub-districts	Uncontaminated sub-districts	
Average Consumption			
Total Expenditures	90.26	97.54	.441
Visible Expenditures	10.04	10.62	.656
Nonvisible Expenditures	56.50	59.22	.597
Food Expenditures	53.330	56.99	.406
Share of Visible Goods	0.11	0.11	.809
Share of Nonvisible Goods	0.67	0.67	.916
Average Demographics (Household Head)			
Household Size	4.96	5.10	.286
Social Activities	1.61	1.52	.655
Age	42.98	43.83	.521
Share: No Schooling	0.20	0.31	.009***
Share:Agriculture	0.49	0.60	.036**
Share: Previous Government Aid	0.91	0.93	.335
Average Village Characteristics			
Percent without Electricity	0.02	0.02	.872
Percentage Islam	86.72	94.38	.491
Percentage of Farmers	60.45	68.77	.114
Population Size	4190.67	4818.61	.235
Number of households	320	3079	

The expenditures are monthly household-level expenditures in Indonesian Rupiah (IRP) at 2007 prices.

Table 10: Treatment Heterogeneity

	Direct Treatment Effect on the treated					Indirect Treatment Effect on the untreated				
	Total Log Expenditure	Log Visible Expenditure	Log Nonvisible Expenditure	Expenditure Share Visible	Expenditure Share Nonvisible	Total Log Expenditure	Log Visible Expenditure	Log Nonvisible Expenditure	Expenditure Share Visible	Expenditure Share Nonvisible
Treatment × Post	9.004***	10.020***	5.645***	0.021	-1.807***	10.192***	7.885***	4.056***	0.043	-2.478***
	(0.651)	(1.724)	(0.555)	(0.125)	(0.265)	(0.555)	(1.354)	(0.653)	(0.100)	(0.179)
Treatment × Post interacted with:										
Basline log population size	-0.000	-0.000	-0.000**	0.000	-0.000	-0.000	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
No Sanction	0.066	0.182	0.035	0.018**	-0.018	0.039	0.080	-0.035	0.013	-0.051***
	(0.050)	(0.119)	(0.042)	(0.009)	(0.018)	(0.042)	(0.136)	(0.040)	(0.013)	(0.016)
Household Social Acitivites	-0.006	0.027	0.003	0.003	0.005	-0.001	-0.059*	0.014	-0.006**	0.011**
	(0.015)	(0.047)	(0.011)	(0.004)	(0.007)	(0.013)	(0.031)	(0.012)	(0.003)	(0.005)
Propensity Score	0.318*	0.185	0.311*	0.026	0.011	-0.419*	-0.276	0.297	-0.037	0.252***
	(0.164)	(0.359)	(0.161)	(0.032)	(0.060)	(0.233)	(0.487)	(0.218)	(0.050)	(0.071)
Log Expenditure	-0.664***	-0.733***	-0.419***	-0.003	0.131***	-0.734***	-0.549***	-0.307***	-0.001	0.173***
	(0.047)	(0.123)	(0.039)	(0.009)	(0.019)	(0.039)	(0.099)	(0.046)	(0.008)	(0.013)
Household Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
District Trends	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
N	2022	1806	2022	2022	2022	2299	2018	2299	2299	2299
R^2	0.132	0.045	0.077	0.014	0.055	0.190	0.051	0.057	0.030	0.129

Standard errors are clustered at the sub-district level. All specifications include a time trend. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Visible and Novisible Consumption: Disaggregated

	Clothing	Log Clothing	Tobacco	Log tobacco	Food	Log Food	Miscellaneous	Log miscellaneous
				Panel A: Direct Treament Effect				
Treatment	13508.305	0.359**	1557.753	-0.032	31191.884	0.050	-5854.006	-0.181
	(8193.896)	(0.136)	(5725.834)	(0.059)	(23782.667)	(0.035)	(6425.012)	(0.129)
N	2022	1069	2022	1429	2022	2022	2021	1967
				Panel B: Indirect Treament Effect (Peer Effect)				
Treatment	22894.952**	0.326**	10765.638*	0.098	-14249.576	-0.016	1257.205	0.002
	(11191.611)	(0.149)	(5926.333)	(0.061)	(22360.567)	(0.036)	(6210.438)	(0.121)
N	2297	1179	2298	1589	2299	2299	2298	2242
Household Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes
Month Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes

Standard errors clustered at the subdistrict level are in parentheses. All specifications include a time trend. The specifications with logs exclude all observations with zeros. It is reassuring that results based on logs and levels are qualitatively similar. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Supply-Side Characteristics at the Village Level

	(1) Deflator	(2) Production Kiosk	(3) Small Industry	(4) Shopping Area	(5) Distance to Shopping	(6) Market	(7) Distance to Market	(8) Number of Minimarkets
Treatment	-0.000	-0.234	6.690	-0.000	0.491	0.029	0.293	-0.032
	(0.000)	(0.193)	(12.365)	(0.020)	(0.698)	(0.028)	(0.554)	(0.034)
Treatment × Post	0.045	-2.302	39.629	-0.362	-7.863	0.253	-3.659	0.030
	(0.034)	(2.936)	(117.456)	(0.304)	(14.597)	(0.368)	(5.684)	(0.368)
Number of Villages	1310	1278	1278	1278	1278	1278	1278	1278
R^2	0.652	0.489	0.218	0.487	0.329	0.381	0.614	0.286
Time dummy	yes	yes	yes	yes	yes	yes	yes	yes

Standard errors clustered at the sub-district level in parentheses. All specifications include a time trend. For a small number of villages (32 out of 1310) I was not able to match the supply-side data.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix C: Consumption visibility survey

The consumption categories. The following consumption caegories (taken from Heffetz (2011) and extended by three new categories were used in my survey. The version of this survey in Bahasa Indonesia that I used to collect this data is available upon request. Also the original filled in surveys are available upon request.

Consumption Categories taken from Heffetz:

FdH: Food and nonalcoholic beverages at grocery, specialty, and convenience stores;
FdO: Dining out at restaurants, drive-throughs, etc., excluding alcohol; including food at school;
Cig: Tobacco products like cigarettes, cigars, and pipe tobacco;
AlH: Alcoholic beverages for home use;
AlO: Alcoholic beverages at restaurants, bars, cafeterias, cafe's, etc.;
Clo: Clothing and shoes, not including underwear, undergarments, and nightwear;
Lry: Laundry and dry cleaning; Jwl: Jewelry and watches;
Brb: Barbershops, beauty parlors, hair dressers, health clubs, etc.;
Hom: Rent, or mortgage, or purchase, of their housing;
Htl: Lodging away from home on trips and housing for someone away at school;
Fur: Home furnishings and household items, like furniture, appliances, tools, and linen;
Utl: Home utilities such as electricity, gas, and water; garbage collection;
Tel: Home telephone services, not including mobile phones;
Cel: Mobile phone services;
HIn: Homeowner's insurance, fire insurance, and property insurance;
Med: Medical care, including health insurance, drugs, dentists, doctors, hospitals, etc.;
Fee: Legal fees, accounting fees, and occupational expenses like tools and licenses;
LIn: Life insurance, endowment, annuities, and other death benefits insurance;
Car: The purchase of new and used motor vehicles such as cars, trucks, and vans;
CMn: Vehicle maintenance, mechanical and electrical repair and replacement;

Gas: Gasoline and diesel fuel for motor vehicles;
CIn: Vehicle insurance, like insurance for cars, trucks, and vans;
Bus: Public transportation, both local and long distance, like buses and trains;
Air: Airline fares for out-of-town trips;
Bks: Books, including school books, newspapers and magazines, toys, games, and hobbies;
Ot1: Computers, games, TVs, video, audio, musical and sports equipment, tapes, CDs;
Ot2: Cable TV, pets and veterinarians, sports, country clubs, movies, and concerts;
Education: Education, from nursery to college, like tuition and other school expenses;
CHA: Contributions to churches or other religious organizations, and other charities;
Added Consumption Categories
Pil: Pilgrimage, e.g. expenses for going to Haji;
Wed: Weddings and Feasts.
Dropped Consumption Categories:
Und: Underwear, undergarments, nightwear, and sleeping garments;
Htl: Lodging away from home on trips and housing for someone away at school.