Can Mistargeting Destroy Social Capital and Stimulate Crime? Evidence from a Cash Transfer Program in Indonesia

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I. Introduction

Developing countries are increasingly using cash transfers as a means of providing financial support to poorer households. However, these countries rarely have the detailed, verifiable, and legally enforceable databases that form part of the tax and welfare systems in industrialized nations. As a result, accurate targeting of such transfers in developing nations is very difficult.

Several recent papers have examined the problem of targeting (see, e.g., Coady, Grosh, and Hoddinott 2004; Elbers et al. 2007). The focuses of these papers have been the distributional consequences of undercoverage of eligible recipients (errors of exclusion) and leakage of funds to ineligible households (errors of inclusion). However, the downside of poor implementation of such programs extends beyond financial losses to the potential destruction of trust and social capital, which can, among other things, increase the prevalence of antisocial behavior like crime (Putnam 2000). The general media and sociological literature have discussed the possible drawbacks that can accompany the formalization of social security (e.g., see Berger and Neuhaus 1996), but there has been little attention paid to the social consequences of mistargeting in the economics literature. A recent notable exception is Alatas et al. (2011), which uses a randomized controlled trial to show that while statistical targeting methods such as proxy means testing can do a better job of identifying households with low per capita expenditure than community rankings, community rankings result in higher community satisfaction.

We explore the impact of a large-scale, nationwide antipoverty program in Indonesia that reportedly caused considerable social disharmony. The program, *Bantuan Langsung Tunai* (BLT), used a variant of proxy means testing

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to target eligible households (officially defined as households with per capita expenditures of less than Rp175,000). The BLT program aimed to compensate poor households for a sudden and large increase in fuel costs that resulted from the removal of fuel subsidies. Costing approximately 1 billion US dollars, this is one of the largest cash transfer programs in the developing world.[1] The poor targeting that resulted from its rapid implementation is well documented (Hastuti, Sumarto et al. 2006; Hastuti, Usman et al. 2006). Close to half a billion US dollars made its way to ineligible households. The social unrest that resulted was widely reported in the media and extended from protests across the nation to acts as extreme as the burning down and stoning of village heads' offices (Widjaja 2009). We hypothesize that the poor implementation of the program that saw many eligible households miss out on the payments and many ineligible households receive them reduced the level of trust within the community, had a deleterious effect on social capital, and led to an increase in antisocial and in some cases criminal behavior.

The aim of this article is to use nationally representative data to verify the anecdotal reports of social unrest and to isolate potential pathways. We find that some types of mistargeting are more harmful than others. Leakage (the share of ineligible households who received the funds) is a strong determinant of both increases in crime and decreases in social capital. In contrast, undercoverage (the share of eligible households who did not receive the payment) is not a significant determinant of crime and is not a predictor of changes in social capital. We test the impact on crime directly and find that as a result of poor targeting, crime increased by 0.1 percentage points, or approximately 4%. Our results withstand a range of robustness tests that examine possible alternative interpretations such as reverse causality and omitted third factors.

Using a smaller, supplementary data set with more detailed information on social capital than in the nationally representative data, we establish that social capital, measured by people's participation in community groups, was significantly adversely affected by the poor targeting. This is in line with qualitative reports from surveys of village heads that the BLT program made it harder to get households to work together for the betterment of the community (Hastuti, Usman et al. 2006). Finally, to close the causal chain and in support of our original hypothesis, we show that villages that experienced decreases in community participation were more likely to report declines in perceptions of safety.

[1] See World Bank (2006, 182). BLT translates as "direct cash assistance." We examine the 2005 BLT program. The program has since been implemented again in 2008–9 with better targeting and less social unrest (see Satriana 2009).

The remainder of this article is structured as follows. Section II discusses the conceptual framework. Section III describes the BLT program as it was implemented in October 2005. The data sources are described in Section IV and the empirical methodology in Section V. Section VI presents the main results, Section VII presents various robustness tests, and we explore the mechanisms by which the program increased crime in Section VIII. Section IX concludes.

II. Conceptual Framework

The aim of this article is to investigate the consequences of mistargeting and to test the hypothesis that the implementation of the BLT program and the distribution of payments caused significant dismay in the community and led to social disharmony. We focus on crime because it is an empirically tractable behavioral manifestation of such disharmony. We thus seek to identify whether, and to what extent, the introduction of the BLT program increased crime. We hypothesize that mistargeting of the BLT program could have led to increases in crime by invoking a sense of injustice that resulted in a deterioration in trust (social capital) among villagers.[2] This feeling of injustice could arise from the arbitrary nature of the allocation of the funds or may have stemmed directly from villagers observing elite capture of the program (e.g., village heads allocating the program to their friends).

There is ample support in the sociology and criminology literature that declines in social capital are associated with increases in crime. For example, Putnam (2000) argues that the presence or absence of networks of generalized trust and reciprocity within communities are an important determinant of a community's resilience or susceptibility to crime. Similarly, Bursik and Grasmick (1993) view weak social controls (as reflected in low social capital) as harming the ability of groups to organize and protect themselves, which induces mistrust and suspicion and leads to predatory crime. Related theories predict that where there is not a strong moral order and people behave egoistically and are willing to exploit others, social trust declines and crime flourishes (Rosenfeld and Messner 1998). These theories suggest that the crime that results from a decline in social capital is not necessarily targeted at those who caused the decline by acting "badly." For example, in the current context,

[2] Social capital can be broadly defined as the set of rules, norms, obligations, reciprocity, and trust embedded in social relations, social structures, and societies' institutional arrangements that enable members to achieve their individual and community objectives (Coleman 1990). Empirically "social capital" is hard to capture. Therefore our empirical results focus on participation in community groups.

those who misappropriate BLT funds might not necessarily be the target but rather the theories predict general increases in crime when social capital declines.[3]

Results from a survey conducted by the authors across the Indonesian province of East Java indicate the level of discontent with the program and the likelihood that social capital was adversely affected. Of the 160 community leaders surveyed, 40% said that the BLT caused problems in their village.[4] Twenty-nine percent of households said it caused anger toward community and village heads, 8% said it caused anger toward the government, and 8% said it caused anger toward BLT recipients.

Another possible route via which a targeted transfer program could have an impact on crime is its impact on inequality. Previous papers have illustrated that increases in inequality can lead to increases in crime (Bourguignon 1999; Fajnzylber, Lederman, and Loayza 2002). For example, Demombynes and Özler (2005), using data from South Africa, find a positive relationship between both mean household expenditure and inequality and property crime. BLT mistargeting caused arbitrary (and temporary) changes to the income distribution. Empirically, however, we find that the one-off payment received by BLT recipients had a small effect on inequality. The change in inequality the payments induced had no effect on crime. We examine the mechanism via which mistargeting led to increases in crime and explore the extent to which declines in social capital played a role in Section VIII.

III. The BLT Program

The Indonesian government reduced fuel subsidies on October 1, 2005. The fuel subsidies were expensive, caused the government budget to fluctuate with world oil prices, and largely benefited the well-off because they consume the most fuel. However, the price of kerosene, which many poor households use for cooking and lighting, rose by 185.7%.[5] To compensate the poor for these price rises, *Bantuan Langsung Tunai* (BLT) was introduced. All households

[3] The above mechanisms can be incorporated in the classical Beckerian model, where crime is a rational choice between legitimate and illegitimate sources of income and crime, if detected, is punished. In close-knit rural villages, "punishment" often takes the form of social isolation. Decreases in a community's stock of social capital reduces the effectiveness of this mechanism, and as in the standard model, less effective punishment results in increased crime.

[4] Note that this was a small module in a larger survey that was being conducted for a different purpose, and so the respondents were not primed to be thinking about the BLT. Further, in response to an earlier, open-ended question—"In your opinion, what is the biggest problem in this village?"—9% of the over 1,500 households surveyed volunteered BLT as constituting the main problem in their community.

[5] Gasoline prices increased by 87.5%, and diesel prices increased 104.8% (Widjaja 2009).

with a monthly per capita expenditure of less than Rp175,000 (US$17)—"eligible households"—were to receive Rp100,000 per month for 6 months.[6] This amounted to 22% of monthly household expenditure for these households on average and was paid in two 3-month lump sums. Approximately 18.6 million households (or approximately one-third of all Indonesian households) were to receive the payment (World Bank 2006).

From the outset the program was beset with problems, which stemmed from the short time period for program development and implementation, approximately 3 months. The greatest hurdle to overcome was the targeting of the nation's poor. No national database of household incomes or expenditures exists in Indonesia,[7] which is the case for most developing countries.

To deal with this lack of data, a proxy-means testing approach was developed. The procedure consists of a number of steps. First, data from the detailed annual national socioeconomic survey (SUSENAS) for the years 2002, 2003, and 2004 were combined and used to identify 14 variables that together had the greatest ability to predict household expenditure. This was done by estimating logistic regressions for each of the 377 districts (kabupaten/kota) in Indonesia. The list of these variables is presented table A1 in the online appendix. This process generated weights, which would later be used to calculate a value for the poverty index for each household.[8] A questionnaire was then constructed (Socioeconomic Data Collection on the Population 2005 = Pendataan Sosial Ekonomi Penduduk 2005: PSE05) to collect information on these variables from households.

Village heads in each of Indonesia's almost 70,000 villages were asked to provide a list of households that they considered to be poor. Enumerators from the Indonesian Statistical Agency (BPS) then went to the villages and used the new questionnaire to survey these poor households. While in the village the enumerator was also supposed to scout around and see whether she or he could identify any other poor households, which would then be surveyed. In

[6] This is slightly higher than the 2004 official poverty line of Rp110,000/capita/month. Many households in Indonesia are clustered around the poverty line. For this reason a cutoff point that included some of the "near poor" was chosen. No geographic targeting was used.

[7] Past safety net programs used the National Family Planning Agency's database (*Badan Koordinasi Keluarga Berencana Nasional* [BKKBN]), which defines households as being "pre-prosperous" on the basis of four questions on whether the household members eat three times a day, have a change of clothes, live in a house with a dirt floor, and are able to observe their religious duties. This approach has met with mixed success (Pritchett, Sumarto, and Suryahadi 2002). See also Alatas et al. (2011) for a discussion of targeting methods in the context of Indonesia.

[8] Coady et al. (2004) provides a general discussion of proxy-means methodology. Badan Pusat Statistick (2005) gives a description of the methodology as it was followed in Indonesia.

practice this often did not happen.[9] Enumerators also often lived in the local area and claimed to know who was poor without further investigation. Hastuti, Usman et al. (2006) reports that 48% of households stated that the BPS enumerator did not ask them the full range of questions. They also found that some enumerators included people living close to them in the survey regardless of the households' standard of living.

Once the data had been collected they were transferred to the central statistical office in Jakarta, where the weights from the previous calculations were used to give each household a score. Households with a score above a certain cutoff point were deemed to be poor and so were to receive the BLT payment, while those with lower scores were deemed to be too well-off to be eligible. The data were then sent to the Indonesian Postal Service for the production of compensation cards. These cards were then distributed to the subdistrict statistical office, which disbursed them either directly or through community leaders. The card, which had the recipients name and address printed on it, had to be shown at the post office for receipt of benefits.

Distributing transfers on the basis of estimates of household expenditure undoubtedly led to substantial targeting error. Table 1 presents targeting statistics by quintiles of the per capita expenditure distribution and also according to the BLT criteria. The table is generated from the 2006 SUSENAS household survey data, which are the main data source for this article and which are discussed in detail below. The table shows that 46.5% of BLT recipients reported having per capita expenditures above the cutoff of Rp175,000 per month and that approximately 48.2% of the "poor" households did not receive the payment. In terms of quintiles of the distribution, higher percentages of those in the lower quintiles received the payment.[10]

Unhappiness with the targeting method caused severe social unrest. Table 2 from Widjaja (2009) shows the incidence of protests in response to the BLT. There were protests in 35% of the 566 villages surveyed. Respondents were asked about the cause of the unrest, and 90% responded that the protests were caused by the flawed targeting method.[11] Press reports cite instances in which

[9] The deadlines faced by enumerators simply did not provide enough time for this task to be carried out. The entire enumeration was scheduled to be undertaken between August 15 and September 15, 2005.

[10] Note that the official poverty line of Rp110,000 per capita expenditure falls in the lowest quintile, which has a cutoff of Rp120,986. The BLT mistargeting rate is very similar to that which results from the same targeting method in Alatas et al.'s (2011) targeting experiment in Indonesia. They find that 30% of households were mistargeted. That is, either eligible households did not receive the payment or ineligible households did. In our sample, 26% of households were mistargeted.

[11] Other responses were a lack of clarity about the distribution schedule (1%), a lack of clarity about the distribution location (2%), recipients not receiving the full sum (2%), lack of coordination between agencies in the distribution chain (4%), and complicated processes (1%).

TABLE 1
TARGETING PERFORMANCE

	% Receiving BLT (1)	BLT Households (Millions) (2)	% of BLT Recipients (3)
Poor	51.8	8.1	53.5
Nonpoor	17.7	7.1	46.5
By income quintile:			
Q1	57.0	6.3	41.2
Q2	35.8	4.0	26.5
Q3	24.6	2.7	17.8
Q4	14.9	1.6	10.6
Q5	5.2	.6	4.0
All	27.5	15.4	100

Note. Poor and nonpoor are defined to coincide with the eligibility criteria of a per capita expenditure of less than Rp175,000 per month.

TABLE 2
BLT AND SOCIAL UNREST

Type of Incident	% of Villages
Protests	34.6
Injured victims	14.9
Threats to village officials	11.8
Threats to BPS staff	4.4
Vandalism to public facilities	1.5
Conflict among citizens	1.4

Note. $N = 566$ villages. Calculated using data from the 2006 SUSENAS panel, which is conducted by the Indonesian Statistical Agency. Source: Widjaja (2009).

village and community leaders were the targets of violence and threats. There were many reports of village heads resigning and cases of village and subvillage heads offices and houses being burnt down and destroyed. Such violence was by no means isolated. In one of 10 villages studied in Hastuti, Usman et al. (2006), the village office was stoned.[12] This same study also reports that in several areas, the damage to the sociopolitical order of the local community was considered bigger than the advantage that was received by the poor. Further, in focus group sessions, community leaders voiced the concern that the program was counterproductive to other programs that relied on community empowerment. Almost all village officials said that they were negatively affected by the program, and in several villages it was reported that it became more difficult to request residents to engage in mutual assistance activities and that village tax levies were negatively affected.

Note, however, that the use of proxy-means as a targeting mechanism is not uncommon. Coady et al. (2004) provide a comparison of 49 targeted cash

[12] In subdistrict Cibeber in Cianjur, all village heads planned to resign if supplementary registrations were not approved because they feared for their safety.

programs in low- or medium-income countries, including several that use proxy-means testing. They conclude that it is one of the more accurate targeting mechanisms. Indeed the BLT targeting performance is not seriously worse than that in many programs that have not met with unrest. Table 3 presents a comparison of targeting performance of a number of cash transfer programs. The targeting performance of the BLT program is considerably worse than programs in Brazil, the Dominican Republic, Chile, and Nicaragua, but a greater proportion of the funds reached the poorest 20% of households than in Mexico's Progresa program, which is widely considered a role model for cash transfer programs and which met with no social unrest as far as we know. Hence, mistargeting may not of itself have led to social unrest.

Another likely factor was the poor socialization of the program. That is, the aim of the program and who it was intended to aid were not well communicated to the population. The factor that focus groups reported to be the most unsatisfactory was the lack of information on the criteria for selection of recipient households. Information about the program was only distributed to local governments once disbursement of funds had begun, and that information on the 14 variables used to establish eligibility was given only later as a means of resolving the complaints and tensions that had arisen as a result of disbursement. Hence, it seems that households knew very little or nothing about the eligibility criteria until well after disbursement and only when efforts to deal with the consequent disharmony had commenced.

The speed of implementation was a further contributing factor to these weaknesses in implementation. BLT was implemented simultaneously with a reduction in fuel subsidies. In contrast, Progresa accompanied a gradual elimination of food subsidies that began several years prior to its introduction and

TABLE 3
INTERNATIONAL COMPARISON OF TARGETING PERFORMANCE

		Percent of Transfers Going to:		
Country	Program	Poorest 10% of Households	Poorest 20% of Households	Poorest 40% of Households
Indonesia	BLT cash transfer	23.0	41.2	67.7
Brazil	Bolsa Familia conditional cash transfer	73.0	94.0	
Dominican Republic	Cash transfer		60.0	
Chile	SUF cash transfer		57.0	83.0
Nicaragua	RPS conditional cash transfer	32.6	55.0	80.9
Honduras	PRAF cash transfer	22.1	42.5	79.5
Mexico	Progresa conditional cash transfer	22.0	39.5	62.4

Sources. Coady, Grosh, and Hoddinott (2004). BLT figures are calculated by the authors using SUSENAS data.

that was completed 3 years thereafter. BLT was also implemented simultaneously across the entire nation, while Progresa was piloted prior to implementation and phased in gradually, initially to only a small number of poor, rural communities in 1997, expanding to include urban areas of up to a million people only in 2001 (Parker 2003).

For the BLT, there were also no enforcement mechanisms in place, such as auditing of subsamples of households to ensure that they had been correctly categorized. Also, initially in many regions there was no formal mechanism via which households could appeal or voice complaints. Hence, the results presented below should be interpreted as a cautionary tale of how things can go wrong. Hastuti, Usman et al. (2006) also note that the BLT program was implemented following recent increases in village and regional autonomy and that the "post-reformasi" climate resulted in village communities being unafraid of putting their opinions forth. This would not have been the case under the Suharto regime, and the recent increase in freedoms may have encouraged the extent of the public reaction.

IV. Data

This article draws on two main sources of data. The first source is the 2006 Indonesian Socioeconomic Census (Survei Sosial Ekonomi Nasional [SUSENAS]). These data cover a random sample of 277,202 households and over 1.1 million individuals (about 1 in 200 of the Indonesian population) drawn from 15,612 villages across the Indonesian archipelago. The SUSENAS is conducted annually and collects information on a large range of demographic and economic variables. The 2006 SUSENAS was conducted in July 2006. In addition to the normal range of questions, in 2006 households were asked whether they received BLT and if so, in which month they first received it. This enables us to identify recipient households. All individuals in the household were also asked whether they were a victim of crime in the last year, to which they answered yes or no.

The second source of data is the Indonesian Village Census (Potensi Desa [PODES]). The PODES is conducted every 3 years and collects a wide range of information from every village in Indonesia. It provides information on whether there were cases of 11 categories of crime in the previous year. The categories of crime are theft, looting, pillaging, assault, arson, rape, misuse of drugs, illegal drugs, murder, the sale of children, and other.[13] Respondents

[13] The PODES respondent is the village head or another village representative. We use all categories of crime so that the variable generated is consistent with the SUSENAS question on crime, which does not specify types. The results are, however, robust to including only categories of crime that we expect to be most strongly affected by the BLT, which are theft, looting, pillaging, violence, and arson.

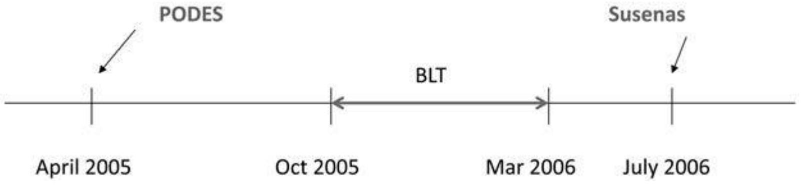

Figure 1. Timing of program and surveys. Color version available as an online enhancement

are also asked to designate the type of crime that occurred most often. The PODES was conducted pre-BLT in April 2005.

Figure 1 shows the timing of the surveys and the program. Note that the dates of the SUSENAS and PODES surveys allow us to closely examine the period of time over which the BLT would have had an impact on crime.

Both the PODES crime data and the SUSENAS crime data rely on self-reports from surveyed individuals. This type of data suffers less from underreporting biases that are evident in police crime statistics. Gibson and Bonggeun (2006), using the International Criminal Victimization Surveys of 140,000 respondents in 37 industrial, transition, and developing countries, compare crimes experienced by these respondents with those reported to the police. They find that rates reported to the police are significantly lower than actual rates reported in individual interviews. The SUSENAS is a household survey, so every household in the survey was asked about its experience of crime, alleviating the underreporting problem as well as the concern of selection based on who chooses to report crimes to the police.[14] A weakness of the SUSENAS crime measure, however, is that it does not disaggregate by type of crime. In addition, we know nothing from our data sources about who is committing the crimes.

Table 4 presents summary statistics of the crime variables. Of the households in our sample, 2.8% had a member who was a victim of crime in the year up to July 2006. Twenty-seven percent of the villages sampled in the SUSENAS have at least one household sampled that was a victim of crime.[15] Table 4 also presents summary statistics of our mistargeting measures. (Var-

[14] The 2009 SUSENAS asks whether respondents have been a victim of crime, and if so, whether it was reported to the police. It shows that only 16% of crimes were reported to police. This figure is higher, but still surprisingly low, for serious violent crimes. For example, only 58% of murders are reported to police.

[15] The International Crime Victims Survey (ICVS), a household survey that compares levels of victimization across countries, shows that crime rates in Asia are significantly lower than crime rates in Latin American and Africa. The crime rates in our sample are lower than in the ICVS but of the same order of magnitude. The ICVS reports that 5.0% of households had experienced a burglary in the previous 12 months, 5.6% personal theft, 0.6% robbery, and 2.6% assault. The ICVS figures are likely to be higher because it only interviews in large urban centers. It was conducted in 1996–97 and so may also be contaminated by the Asian Financial Crisis, which started in mid-1997.

TABLE 4
SUMMARY STATISTICS OF KEY VARIABLES

Variable	Mean	SD	Min	Max	N
Household level:					
crime$_{ivt}$	.028	.164	0	1	262,476
BLT-poor	.152	.359	0	1	262,476
BLT-nonpoor	.141	.348	0	1	262,476
No BLT-poor	.128	.334	0	1	262,476
In 2nd top decile (0/1)	.101	.301	0	1	262,476
In top decile (0/1)	.102	.302	0	1	262,476
Per capita expenditure	.309	.336	.017	79.038	262,476
Village level:					
Crime pre-BLT	.551	.497	0	1	14,815
BLT present	.878	.328	0	1	14,815
Leakage	.22	.248	0	1	14,815
Undercoverage	.358	.361	0	1	14,815
Proportion households in 2nd top decile	.093	.115	0	.813	14,815
Proportion households in top decile	.087	.161	0	1	14,815
Proportion households eligible	.298	.286	0	1	14,815
Gini coefficient	.207	.074	0	.810	14,815
Average per capita expenditure	.261	.152	.043	5.405	14,815
Rural	.677	.468	0	1	14,815
Village population	5.413	6.725	.042	78.986	14,815
Male share	.499	.033	.01	.975	14,815
Farm households	.625	.339	0	1	14,815
Farm laborers	.067	.107	0	.975	14,815
Poor letter	.071	.128	0	1	14,815
Hours to city	.423	.794	.017	13.5	14,815
One ethnic group	.732	.443	0	1	14,815
Security post	.841	.366	0	1	14,815
Civilian defense	.878	.327	0	1	14,815
Police post	.208	.406	0	1	14,815
Electric lights	.783	.412	0	1	14,815
Cook with fuel	.371	.257	0	1	14,806
Transport share	.041	.052	0	.61	14,760
Fuel share	.085	.046	0	.735	14,815
District leakage	.218	.134	.018	.943	14,815
District undercoverage	.48	.165	0	1	14,815
Change in Gini coefficient	−.004	.008	−.088	.147	14,815
Crime 2003	.577	.494	0	1	12,208
BPS leakage	.2	.259	0	1	14,815
BPS undercoverage	2.863	4.776	0	97	14,815

iable definitions are given in table A2 in the online appendix.) Our two main measures of mistargeting are leakage (error of inclusion) and undercoverage (error of exclusion). *Leakage* is defined as the proportion of ineligible households in the village that received the payment. *Undercoverage* is the proportion of eligible households in the village who missed out on receiving the payment. Eligible households in any village in the nation were supposed to receive the transfer. The table shows that 87.8% of the villages in our sample had at least one household that reported receiving the BLT payment (*BLT present = 1*). On average 22% of ineligible households in a village received the BLT payment, and 35.8% of eligible households missed out. Figures 2 and 3 show the distribution of the targeting variables. They show considerable variation across villages. In some villages there is no mistargeting, but in a large proportion there is substantial mistargeting. In some villages all the eligible households missed out on the payment and/or all of the ineligible received the payment.[16]

V. Empirical Methodology

The probability of household i in village v being a victim of crime is a function of both household and village characteristics. Household characteristics such as income, assets, and demographic structure reflect the household's susceptibility to crime. The income and demographic structure of other households in the village capture both the propensity of village residents to engage in crime and the relative attractiveness of household i as a victim. Institutional factors in the village, such as the presence of security posts and distance to police stations, also play a role.

We will thus model the probability of household i in village v in year t being a victim of crime, crime_{ivt}, in the following way:

$$\text{crime}_{ivt} = \alpha_0 + \alpha_1 X_{ivt} + \alpha_2 Y_{vt}^{\text{HH}} + \alpha_3 \text{INST}_{vt} + \alpha_4 \text{BLT}_v + \alpha_5 \text{crime}_{v,t-1} + \eta_v + \varepsilon_{ivt}, \tag{1}$$

where X_{ivt} are observed household-level variables that affect the household's susceptibility to crime, Y_{vt}^{HH} are observed village-level variables that reflect the characteristics of other households in the village v, INST_{vt} are observed variables reflecting village v's institutions that affect crime levels, η_v is unobservable village characteristics that affect crime, and ε_{ivt} is a random error term. In addition to these standard variables, we add a vector of variables, BLT_v, which reflects the presence and targeting of BLT within the village.

A concern with estimating an equation like (1) is that it is possible that there are unobservable variables that affect both crime and the implementation of BLT. One could imagine, for example, a village head who is adminis-

[16] In villages with no eligible households, undercoverage is set to 0. Similarly, in villages with no ineligible households, leakage is set to 0. Note that it is possible that post-BLT households could have thought that the information collected in the SUSENAS would be used to identify those who should be eligible for similar future programs. This would provide an incentive for systematic underreporting of household expenditure. We think systematic strategic underreporting is unlikely. However, if it did occur, this would lead us to underestimate the amount of leakage and overestimate the extent of undercoverage. The measurement error in both mistargeting measures would bias our estimates of the effect of mistargeting on crime toward zero.

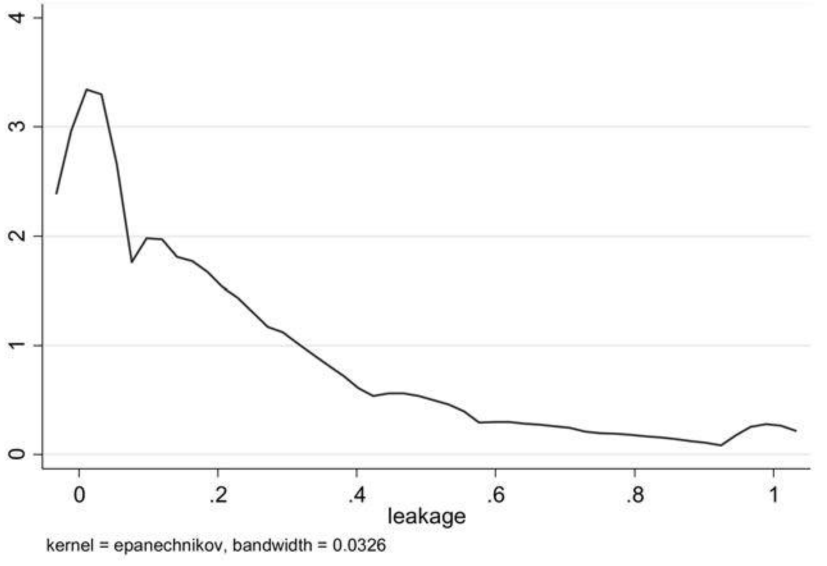

Figure 2. Kernel density estimate of village leakage rates

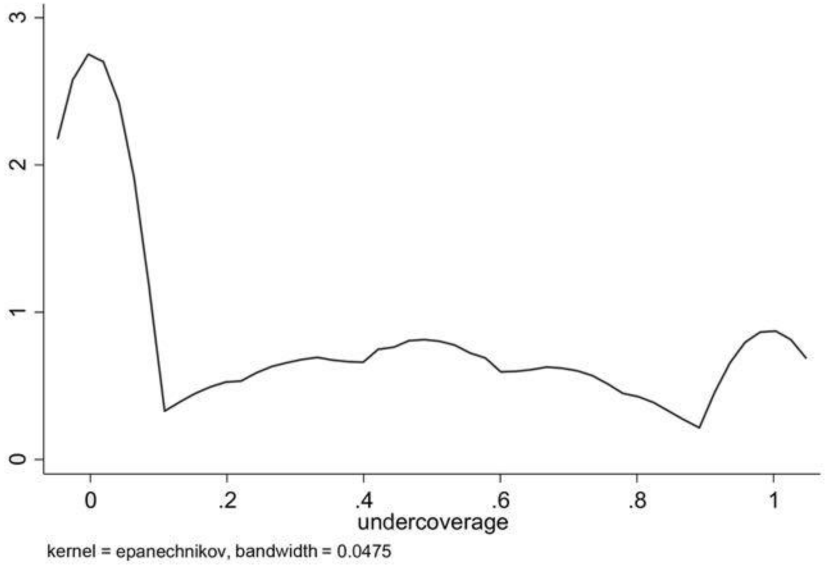

Figure 3. Kernel density estimate of village undercoverage rates

tratively incompetent. The consequent disorganization may result in crime being high and BLT being poorly administered, but no causal relationship may exist between the two. The coefficient on the BLT variable will then be biased. However, if the unobserved variable is not time-varying, we can control for this heterogeneity by including an indicator of past village crime prevalence as a right-hand-side control variable, $\text{crime}_{v,t-1}$. We use the PODES data to construct an indicator of whether or not there was crime in the village prior to the introduction of BLT.[17]

VI. Empirical Results

We first present results that examine the relationship between crime and a range of variables that the literature suggests may be determinants of crime. These results establish that crime is correlated with various village and household characteristics in the expected ways. We then move on to discuss the relationship between crime and the BLT variables.

Column 1 of table 5 presents a parsimonious model that shows that crime is positively associated with both mean per capita expenditure in the village and village inequality as measured by the Gini coefficient, consistent with Demombynes and Özler (2005) for South Africa. Rural villages have no more or less crime than urban villages, but the time it takes to get to a local center is positively and significantly related to the crime rate. Being an hour farther away increases the probability of being a victim of crime by 0.3 percentage points (10.7%). This may reflect distance from law enforcement authorities and is consistent with the findings of Fafchamps and Moser (2003) for Madagascar. We investigate the impact of law enforcement on crime in more detail below. Having fewer men in the village is also positively correlated with crime. Crime also increases with village population. Living in a village with an extra 1,000 people increases the probability of being a victim of crime by 0.2 percentage points (7.4%).[18]

[17] The SUSENAS samples from different villages each year, and so previous SUSENAS samples cannot be used for this purpose. Also, prior to 2006, the most recent year in which the crime question was asked in the SUSENAS was 2000. Note that the time period we are examining is short (15 months between the PODES questionnaire and the SUSENAS questionnaire). This increases the likelihood that any unobservables that affect crime prevalence and mistargeting are likely to be time-invariant.

[18] Controlling for the share of households that are farm households controls for the effects of increasing rice prices. Rice prices went up uniformly across Indonesia between 2005 and 2006. This led to farmer's income increasing over this time and could have led to greater "leakage" to what were now richer farm households. The rice price increases also reduced the real incomes of nonfarm households, and so the increased inequality could have given rise to increased crime. Thus higher rice prices could result in a noncausal association between crime and leakage of BLT funds. Controlling for the share of farm households and the share of farm laborers in the village will, however, capture this effect.

TABLE 5

HOUSEHOLD-LEVEL CRIME REGRESSIONS ($CRIME_{ivt}$)

Variable	(1)	(2)	(3)	(4)	(5)
Household level:					
BLT-poor			−.0004 (.001)	−.0004 (.001)	−.0009 (.001)
BLT-nonpoor			−.005 (.001)***	−.005 (.001)***	−.005 (.001)***
No BLT-poor			−.001 (.001)	−.001 (.001)	−.0007 (.001)
In 2nd top decile (0/1)		.0008 (.001)	.0003 (.001)	.0003 (.001)	.0002 (.001)
In top decile (0/1)		.005 (.002)***	.004 (.002)**	.004 (.002)**	.004 (.002)**
Per capita expenditure	.004 (.002)	.001 (.008)	.003 (.008)	.003 (.008)	−.004 (.007)
Village level:					
Crime pre-BLT				.005 (.001)***	.003 (.001)***
BLT present			.006 (.002)***	.006 (.002)***	.007 (.002)***
Leakage			.01 (.003)***	.01 (.003)***	.01 (.003)***
Undercoverage			−.001 (.002)	−.001 (.002)	.0009 (.002)
Proportion households in 2nd top decile		−.003 (.006)	−.0009 (.006)	−.0008 (.006)	.01 (.006)*
Proportion households in top decile		.02 (.007)***	.02 (.007)***	.02 (.007)***	.02 (.007)***
Proportion households eligible		.002 (.002)	.002 (.003)	.002 (.003)	−.009 (.003)***
Gini coefficient	.06 (.007)***	.05 (.008)***	.04 (.008)***	.04 (.008)***	.04 (.008)***
Average per capita expenditure	.01 (.006)**	.001 (.008)	.003 (.008)	.003 (.008)	−.004 (.007)
Rural	−.0009 (.001)	−.001 (.001)	−.001 (.001)	−.0009 (.001)	−.0002 (.002)
Village population	.0002 (.0001)***	.0002 (.0001)**	.0002 (.0001)***	.0002 (.0001)**	−.0002 (.0001)*
Male share	−.02 (.01)*	−.03 (.01)*	−.03 (.01)*	−.03 (.01)**	−.02 (.01)
Farm households	.0004 (.002)	.0009 (.002)	−.0003 (.002)	.0003 (.002)	−.001 (.002)
Farm laborers	−.004 (.004)	−.003 (.004)	−.002 (.004)	−.004 (.004)	−.002 (.005)
Poor letter	−.0004 (.004)	−.0006 (.004)	−.002 (.004)	−.002 (.004)	−.006 (.004)
Hours to city	.003 (.0009)***	.002 (.0009)***	.002 (.0009)**	.002 (.0009)**	.002 (.0009)**
One ethnic group	.001 (.001)	.001 (.001)	.002 (.001)	.001 (.001)	.001 (.001)
Fixed effects	No	No	No	No	District
R^2	.002	.002	.003	.003	.01

Note. $N = 262{,}476$. Results are from OLS regressions in which the dependent variable is an indicator of the household being a victim of crime between July 2005 and 2006. All standard errors (in parentheses) are clustered at the village level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

While one's own level of expenditure is not linearly associated with the probability of being a victim of crime, column 2 of table 5 shows that very wealthy households (those in the top decile of the national per capita expenditure distribution) are 0.5 percentage points (or 18%) more likely to be a victim of crime. This is consistent with Anderson (2008), which finds that richer households are more lucrative targets in South Africa. Living in a village with more wealthy people (a greater proportion of households in the highest decile of the national per capita expenditure distribution) also increases one's probability of being a victim of crime, regardless of one's own living standards.[19] The results in columns 1 and 2 of table 5 thus indicate that our crime results are consistent with previous research in this area.

Column 3 in table 5 adds variables reflecting the presence of the BLT program and its targeting accuracy in the village. BLT-present equals 1 if anyone in the sample from the village reported receiving the BLT. This variable is statistically significant at the .01 level and indicates that living in a village in which the BLT has been active increases the probability of being a victim of crime by 0.6 percentage points, or 21.4%. Note that this result is not being driven by these villages being poorer than others and so being more adversely affected by the fuel price increases. This can be seen by the coefficient on the proportion of village households that are "poor" (proportion households eligible), where we define poor to correspond with eligibility for the BLT (i.e., those households with per capita expenditure less than Rp175,000).[20] This variable is not significant in column 3 and is negatively correlated with crime in later specifications. In fact, our results suggest that richer villages experience more crime.

Column 3 also shows that mistargeting is significantly associated with crime levels. The variable leakage is statistically significant at the .01 level and indicates that for every additional 10% of nonpoor households that receive the payment the probability of being a victim of crime increases by 0.1 percentage points, or 4.4 percent. In contrast, the variable undercoverage is not statistically significant.[21]

[19] We also ran specifications that included controls for all deciles of the income distribution, but only the top decile was significantly different from the others.

[20] Per capita expenditure is calculated as pre-BLT expenditure. The SUSENAS questionnaire does not directly ask about the full amount of the payments received. It does, however, ask households when they received their first BLT payment. We use the date at which the household first received BLT to calculate how many payments the household is likely to have received by the time of the survey. (One payment of Rp300,000 is assumed if the first was received within 3 months of the survey date, two payments if received prior to this.) We then subtract this estimate of the total amount received from total expenditure and then divide through by the number of household members. The results are, however, very similar to those that use actual per capita expenditure.

[21] We also examine whether crime was more likely to increase if the leakage was to more wealthy groups rather than to those just above the program's cutoff. We find no evidence of crime reacting more strongly to leakages to the very well off.

Column 3 of table 5 also includes indicator variables at the household level that show whether the household was eligible and received the BLT (*BLT-poor*); whether the household was not eligible but received the BLT (*BLT-nonpoor*), and whether the household was eligible but did not receive the BLT (*No BLT-poor*). Thus the omitted category is ineligible households that did not receive the BLT. These household variables indicate that although there is more crime in villages where there is more leakage, those nonpoor households that received the BLT payment are actually 18% less likely than other households to be a victim of crime. This is consistent with the BLT resulting in a general increase in crime in the village and these households' connections providing them with protection of some sort, as well as access to the payment for which they were ineligible. This thus suggests some degree of elite capture. We will return to this below.

The inclusion of the BLT variables does not affect the significance of the other variables described above such as inequality, size of village, proportion of wealthy households, and share of men. Column 4 of table 5 includes the prevalence of crime in the village prior to the implementation of the BLT as an explanatory variable and so controls for unobservable characteristics of the village that may be correlated with crime. As expected, crime is positively correlated across time. However, including past crime prevalence does not affect the coefficients on the BLT variables, suggesting that unobserved heterogeneity is not likely to be driving our results. Finally, column 5 includes district fixed effects. These control for any unobserved differences across districts that might affect changes in crime, such as conditions in regional labor markets and cultural and political differences across regions. The key results are unchanged.[22]

Another way to control for unobservables correlated with both community crime and community implementation of BLT would be to estimate equation (1) in differences. This would, however, require a panel of households, whereas the SUSENAS is a repeated cross section. Because we only have information on household crime victimization at one point in time, we cannot difference the household-level regressions in table 5. We can, however, estimate a first-differenced village-level equation. That is, we construct a village-level variable from the SUSENAS household-level data. This variable equals 1 if any household sampled in the village reported a crime and equals 0 otherwise. We then subtract the baseline village crime variable constructed from the PODES data (Crime pre-BLT) from this. This is not ideal as the pre-BLT and post-BLT measures of crime differ; the PODES variable is a village-level indicator of whether there was crime in the village, and the SUSENAS variable

[22] Results with subdistrict effects are similar and available upon request.

is calculated from crime reports from a sample of households in the village (stratified by expenditure). This means that the differenced variable will understate increases in crime. As long as the enumeration areas sampled in the SUSENAS are representative of the extent of crime in the villages, the results will not be biased. The working paper version of this article provides more detail on this point (Cameron and Shah 2012). The results from estimating this differenced equation are presented in table A3 in the online appendix. They are qualitatively similar to those presented in table 5.

VII. Robustness Tests

While we control for a large number of village-level characteristics above, we still worry that unobservables are biasing the main results. In this section, we explore various hypotheses that might bias our results and test the robustness of the results by estimating different specifications.

A. Security Arrangements

The extent of security in a village is an obvious potential determinant of community crime. Variables reflecting security were not included in the previous regressions because of concern about their potential endogeneity. Column 1 of table 6 adds variables that reflect whether the village built a security post in the last 12 months, whether there is a civilian defense organization—platoons formed in the village responsible for matters concerning security and order—or a police post in the village. Having a civilian defense force and a police post are associated with a lower probability of being a victim of crime, consistent with previous findings that increased security measures lower crime rates (Levitt 1997; Di Tella and Schargrodsky 2004); however, none of the three variables are statistically significant. Importantly, their inclusion does not affect the coefficients on the other variables in the regression.

B. Fuel-Related Variables

Another possible concern with our results is that this period was a time of considerable change. The BLT was introduced to offset the negative welfare effects of soaring fuel prices. Might it not be these high fuel prices that are driving the crime increase? First, note that if it was the case that poor families were resorting to crime to deal with the increase in fuel prices, then we would expect to see the poverty indicators showing a positive relationship with changes in crime. This is not the case. Nevertheless, to examine this issue more closely, column 2 in table 6 adds some further variables that control for the extent to which fuel price increases an impact upon the village. These are the percentage of households in the village that use electric lights (and hence not fuel), the percentage of households that use oil as their cooking fuel, the average share of transport costs in household expenditure, and the average share of fuel costs in household expenditure. Transport expenditure share is positive and significant and indicates that a greater reliance on fuel is associated with higher crime prevalence. The shares of expenditure spent on fuel and cooking with fuel are also significant but negatively signed. The key finding, however, is that inclusion of these variables does not substantively affect the coefficients on the BLT variables.

C. Choice of Geographic Unit

The anecdotal evidence suggests that the program gave rise to tensions within communities. For this reason, we have focused on the impact of mistargeting within a village on crime within that village. These communities comprise around 200–300 households. We are thus examining the effect of mistargeting in a relatively small geographic area on crime in that area. To examine the impact of our choice of geographic unit, we now construct the targeting variables at the district level. Column 3 of table 6 reports the results. The coefficients on the district-level variables are consistent with our earlier results—greater leakage of funds to the nonpoor across the district is associated with more crime. When both community-level and district-level leakage variables are included, as in column 4, the district-level variables remain significant and leakage within the community is significant. Thus, mistargeting in other communities also increases the village's crime rate. For example, mistargeting in villages close to Village A may cause some residents of those villages to turn to crime, some of which is conducted in Village A.

D. Village Administrator Characteristics

A lingering concern is that the BLT variables might be proxying for something about the village that is also correlated with changes in crime. One likely candidate is village administration. As mentioned above, in places where the BLT is poorly administered, there may be other administrative problems that cause crime. If these factors are non-time-varying, then we control for them by controlling for the prevalence of crime in the village prior to the program. However, it is possible that the quality of village administration only matters in times of rapid change and crisis (so its effect is time-varying), and so it may be in poorly administered villages that we observe higher crime prevalence even when we control for baseline crime prevalence. The PODES provides information on the age, gender, and educational attainment of village heads, village secretaries,

TABLE 6

ROBUSTNESS TESTS, CRIME$_{ivt}$

Variable	(1)	(2)	(3)	(4)	(5)	(6)
Household level:						
BLT-poor	−.0009	−.0003	−.0004	−.0006	−.003	−.009
	(.001)	(.001)	(.001)	(.001)	(.001)**	(.002)***
BLT-nonpoor	−.005	−.004	−.003	−.005	−.005	−.004
	(.001)***	(.001)***	(.001)***	(.001)***	(.001)***	(.002)**
No BLT-poor	−.0007	−.0003	−.001	−.001	−.001	.002
	(.001)	(.001)	(.001)	(.001)	(.001)	(.002)
In 2nd top decile (0/1)	.0002	−.0005	.0005	.0002	.0002	.002
	(.001)	(.001)	(.001)	(.001)	(.001)	(.002)
In top decile (0/1)	.004	.002	.004	.004	.004	.008
	(.002)**	(.002)	(.002)***	(.002)**	(.002)**	(.002)***
Village level:						
Crime pre-BLT	.003	.003	.005	.005	.003	
	(.001)***	(.001)***	(.001)***	(.001)***	(.001)**	
BLT present	.007	.007	.008	.006	.008	−.009
	(.002)***	(.002)***	(.001)***	(.002)***	(.002)***	(.01)
Leakage	.01	.009		.01	.01	.0005
	(.003)***	(.003)***		(.003)***	(.003)***	(.006)
Undercoverage	.0009	.001		−.0006	.0001	−.004
	(.002)	(.002)		(.002)	(.002)	(.004)
District leakage			.02	.01		
			(.007)***	(.007)*		
District undercoverage			.0001	.0009		
			(.005)	(.005)		
Crime 2003					.002	
					(.001)	
Crime 1998						.004
						(.002)**
Proportion households eligible	−.009	−.009	−.0006	.0005	−.007	.03
	(.003)***	(.003)***	(.003)	(.003)	(.003)**	(.06)
Rural	−.0004	−.0008	−.0008	−.001	−.0005	.006
	(.002)	(.002)	(.001)	(.001)	(.002)	(.002)**
Proportion households in 2nd top decile	.01	.01	−.002	−.001	.01	.06
	(.006)*	(.006)*	(.006)	(.006)	(.006)*	(.06)
Proportion households in top decile	.02	.02	.02	.02	.02	.04
	(.007)***	(.007)***	(.007)***	(.007)***	(.007)***	(.06)
Gini coefficient	.04	.04	.04	.04	.05	.03
	(.008)***	(.008)***	(.008)***	(.008)***	(.009)***	(.01)*
Average per capita expenditure	−.004	−.004	.003	.003	−.006	.05
	(.007)	(.007)	(.008)	(.008)	(.008)	(.03)
Village population	−.0002	−.0002	.0002	.0002	−.0002	−.0000427
	(.0001)	(.0001)*	(.0001)**	(.0001)**	(.0001)*	(.0002)
Male share	−.02	−.02	−.03	−.03	.003	.03
	(.01)	(.01)	(.01)**	(.01)**	(.02)	(.02)
Hours to city	.002	.002	.002	.002	.001	−.0008
	(.0009)**	(.0009)**	(.0009)**	(.0009)**	(.0009)	(.001)
One ethnic group	.001	.002	.0009	.001	.0002	
	(.001)	(.001)	(.001)	(.001)	(.001)	
Security post	.00002	.0001				
	(.001)	(.001)				

TABLE 6 (*Continued*)

Variable	(1)	(2)	(3)	(4)	(5)	(6)
Civilian defense	−.002	−.002				
	(.002)	(.002)				
Police post	−.001	−.001				
	(.001)	(.001)				
Electric lights		−.0002				
		(.001)				
Cook with fuel		−.003				
		(.002)*				
Transport share		.03				
		(.008)***				
Fuel share		−.05				
		(.009)***				
Fixed effects	District	District	No	No	District	District
R^2	.01	.01	.003	.003	.01	.02
N	262,476	262,267	262,476	262,476	214,823	119,177

Note. Results are from OLS regressions in which the dependent variable is an indicator of the household being a victim of crime between July 2005 and 2006 (cols. 1–5) and in the 1999 calendar year (col. 6). All specifications also control for farm households, farm laborers, and poor letter. Standard errors (in parentheses) allow for clustering at the village level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

the head of the village community organization (Lembaga Pemberdayaan Masyarakat Desa, LPMD), and the head of the Village Legislative Body (Badan Perwakilan Desa, BPD). Column 1 in table 7 shows that these variables are not jointly significant in a regression in which the dependent variable is whether or not the village received the BLT program. This is as expected because village choice was determined by the Indonesian Statistical Agency (BPS). However, columns 2 and 3 show that these variables are jointly significant explanators of both types of targeting error within the village, although not in a uniform way. Column 4 shows that these variables are not a significant determinant of village crime and that their inclusion in the crime regression does not affect the magnitude and statistical significance of our main targeting variables.

Another concern one may have about the role of village administration in generating our results is the potential for misreporting of crime by the village head in the PODES survey. If corrupt village heads systematically underreport crime in their village (e.g., to cover their tracks if they are involved in organized crime) and also influence the targeting of BLT payments to benefit their cronies, then it will appear that crime is higher in villages where there is greater leakage and possibly also where there is greater undercoverage. While we think such a mechanism is unlikely, and contrary to the structure of such crime in Indonesia (which is usually payment for protection and so

TABLE 7

VILLAGE ADMINISTRATION ROBUSTNESS TESTS

Variable	BLT Present (1)	Leakage (2)	Undercoverage (3)	$crime_{ivt}$ (4)	$crime_{ivt}$ (5)
Household level:					
BLT-poor				−.0009	−.0009
				(.001)	(.001)
BLT-nonpoor				−.005	−.005
				(.001)***	(.001)***
No BLT-poor				−.0007	−.0007
				(.001)	(.001)
In 2nd top decile (0/1)				.0002	.0002
				(.001)	(.001)
In top decile (0/1)				.004	.004
				(.002)**	(.002)**
Village level:					
Crime pre-BLT				.003	.004
				(.001)***	(.001)***
Crime pre-BLT × only village head reports					−.002
					(.001)
BLT present				.008	.007
				(.002)***	(.002)***
Leakage				.01	.01
				(.003)***	(.003)***
Undercoverage				.0009	.0009
				(.002)	(.002)
Proportion households eligible	.02	−.11	.25	−.009	−.009
	(.01)	(.01)***	(.01)***	(.003)***	(.003)***
Rural	.006	.01	−.004	−.00001	−.0002
	(.009)	(.006)	(.009)	(.002)	(.002)
Proportion households in 2nd top decile	−.15	−.18	−.49	.01	.01
	(.03)***	(.02)***	(.03)***	(.006)*	(.006)*
Proportion households in top decile	−.43	−.16	−.38	.02	.02
	(.03)***	(.02)***	(.03)***	(.007)***	(.007)***
Gini coefficient	.58	−.02	.39	.04	.04
	(.04)***	(.03)	(.04)***	(.008)***	(.008)***
Average per capita expenditure	−.17	−.05	−.05	−.004	−.004
	(.03)***	(.02)*	(.04)	(.007)	(.007)
Village population	−.001	−.003	.003	−.0002	−.0002
	(.0006)**	(.0004)***	(.0007)***	(.0001)*	(.0001)*
Male share	−.04	−.07	.07	−.02	−.02
	(.08)	(.06)	(.09)	(.01)	(.01)
Farm households	.04	.08	−.03	−.0006	−.001
	(.01)***	(.01)***	(.01)*	(.002)	(.002)
Farm laborers	−.01	.03	−.03	−.001	−.002
	(.03)	(.02)	(.03)	(.005)	(.005)
Poor letter	.04	.06	−.006	−.006	−.006
	(.02)*	(.01)***	(.02)	(.004)	(.004)
Hours to city	.005	.02	−.01	.002	.002
	(.004)	(.003)***	(.004)***	(.0009)**	(.0009)**
One ethnic group	.002	−.02	.003	.001	.001
	(.006)	(.005)***	(.007)	(.001)	(.001)
Village administration:					
Village has a head	.004	.07	−.05	−.004	
	(.04)	(.03)***	(.04)	(.007)	

TABLE 7 (*Continued*)

Variable	BLT Present (1)	Leakage (2)	Undercoverage (3)	$crime_{ivt}$ (4)	$crime_{ivt}$ (5)
Head's age	−.0002	−.0004	.00003	.0001	
	(.0003)	(.0002)	(.0003)	(.0000581)*	
Head male	−.0008	.02	−.008	.003	
	(.01)	(.01)	(.02)	(.003)	
Head finished primary or secondary school	−.03	−.03	.04	−.002	
	(.02)	(.02)	(.03)	(.004)	
Head tertiary educated	−.03	−.03	.05	.0002	
	(.02)	(.02)	(.03)**	(.004)	
Village has a secretary	.06	.06	−.06	.008	
	(.04)*	(.03)**	(.04)	(.007)	
Secretary's age	−.0007	−.0005	.0008	−.00003	
	(.0003)**	(.0002)**	(.0003)**	(.0000596)	
Secretary male	.02	.008	−.02	−.001	
	(.01)	(.007)	(.01)	(.002)	
Secretary finished primary or secondary school	−.03	−.04	.03	−.01	
	(.03)	(.02)*	(.03)	(.005)*	
Secretary tertiary educated	−.04	−.04	.05	−.01	
	(.03)	(.02)*	(.03)	(.005)**	
Village has a BPD	−.004	.006	−.01	.006	
	(.04)	(.03)	(.04)	(.008)	
BPD head's age	.0002	−.00007	.0003	.00005	
	(.0003)	(.0002)	(.0003)	(.000062)	
BPD head male	−.03	−.02	.04	−.006	
	(.03)	(.02)	(.03)	(.006)	
BPD head finished primary or secondary school	.03	.02	−.04	−.002	
	(.02)	(.02)	(.02)	(.004)	
BPD head tertiary educated	.03	.02	−.03	−.001	
	(.02)	(.02)	(.03)	(.004)	
Village has LPMD	−.0005	.04	−.06	.02	
	(.05)	(.03)	(.05)	(.01)	
LPMD head's age	.0001	−.0003	.0006	.00008	
	(.0003)	(.0002)	(.0003)*	(.0000618)	
LPMD head male	−.04	−.007	.005	−.02	
	(.04)	(.03)	(.04)	(.01)	
LPMD head finished primary or secondary school	.03	−.009	.01	−.002	
	(.02)*	(.01)	(.02)	(.005)	
PMD head tertiary educated	.03	−.01	.02	−.002	
	(.02)	(.01)	(.02)	(.005)	
Constant	.87	.23	.29	.02	.02
	(.05)***	(.04)***	(.05)***	(.009)**	(.008)**
p-value from test of joint significance of village administration variables	.30	.01***	.05**	.29	
Fixed effects	District	District	District	District	District
R^2	.23	.33	.25	.01	.01
N	14,815	14,815	14,815	262,476	262,476

Note. Results are from OLS regressions. LPMD = village community organization. Standard errors (in parentheses) allow for clustering at the village level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

paradoxically results in fewer crime events), we examine whether such misreporting might be driving our results. We do this by using information in the PODES on who was the survey respondent. In 53% of villages, the village head is the only respondent, but in 47% of villages, at least one other respondent was involved (most commonly the village doctor, a teacher, a representative of the village legislative body, or a religious leader). In villages with more than one respondent, it is less likely that the village head would have been able to underreport village crime. Column 5 in table 7 reestimates our preferred specification (table 5, col. 5) allowing for a different coefficient on pre-BLT crime when the village head was the only respondent. This variable is insignificant, and its inclusion does not affect the coefficients on the targeting variables. Hence, village heads' misreporting does not seem to be driving the relationship between mistargeting and village crime.

E. Preprogram Trends in Crime

As a further check that the presence and mistargeting of BLT is not proxying for something correlated with changes in crime, we compare pre-BLT trends in crime in villages that did and did not have BLT recipients. We also compare the pre-BLT crime trend where there was a lot of leakage and undercoverage with crime in villages where there was less. Figure 4 presents graphs of trends in incidence of crime (the share of villages in which crime was reported in the PODES data) prior to the program—from 2002 to 2005. There is no evidence that prior to the program crime was increasing more in villages that ended up having households that received the BLT. In fact, crime decreased by slightly more in these villages prior to the program. Similarly, the prevalence of crime decreased by slightly more in villages that, once the program started, had higher leakage (above the median). Villages that later had higher undercoverage were also, if anything, experiencing a greater decline in crime than other villages prior to the program. Note, however, that all of these differences are very small and that none of the differences in the preprogram trend in crime are statistically significant (all *p*-values are greater than 0.27).[23]

We do two things to more formally examine this issue. First, we add observations of pre-BLT crime in the 2003 PODES as independent variables in the regressions. Column 5 in table 6 adds an indicator for whether there was a report of crime in the village in the 2003 PODES. The control for village

[23] Differences in the preprogram level of crime likely reflect differences in socioeconomic status, which affect both program eligibility and crime, which are controlled for in the regressions.

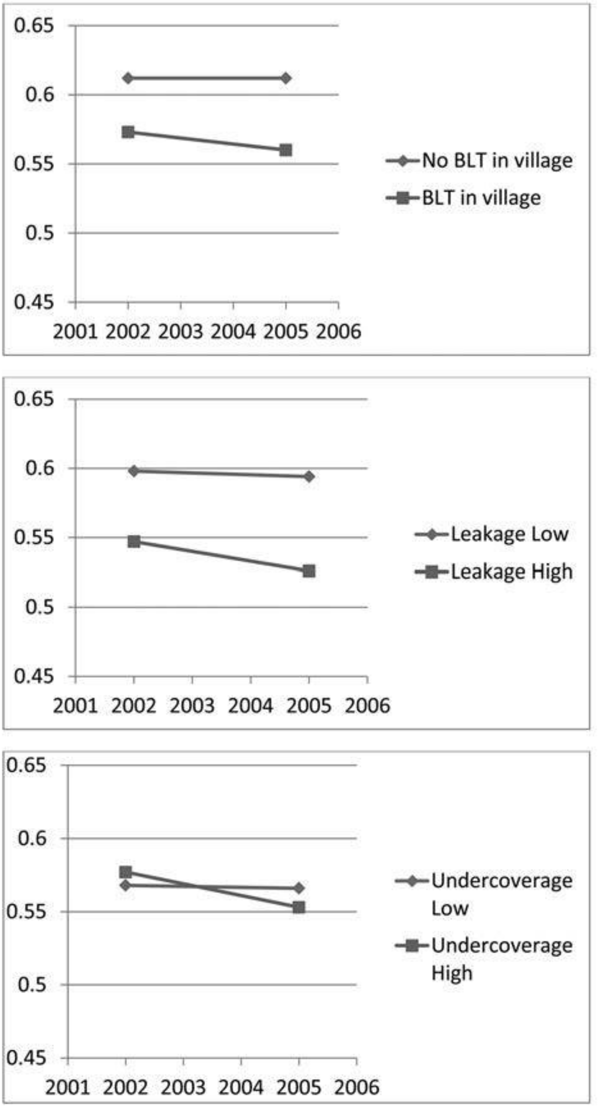

Figure 4. Comparison of trends in crime rates (share of villages experiencing crime). Source: PODES 2005 and PODES 2002. Color version available as an online enhancement.

crime in 2003 is positive (reflecting correlation with the crime rate immediately prior to the program) but statistically insignificant. The coefficient on leakage does not change. It remains positive and statistically significant.[24]

[24] Note that adding this variable reduces the sample size as village codes change over time and not all of the villages in 2005 can be merged back to the 2003 data.

The final test we conduct to rule out the possibility that BLT is proxying for something unobserved that is correlated with crime is a falsification test. The 2000 SUSENAS asks households about crime in the 1999 calendar year. We regress whether the household was a victim of crime in 1999 on a vector of control variables for 1999 similar to that used earlier and predicted measures of BLT receipt and targeting. We use crime as reported in the 2000 PODES (for the period October 1998–October 1999) as our control for baseline crime.[25]

We use BPS's administrative data to predict BLT receipt and targeting. We have access to the weights that BPS generated and that were used to assign households as being eligible or ineligible. These weights differ across districts. Using these BPS weights and information about households in the SUSENAS sample, we are thus able to construct a BPS score for each household.[26] We use this score to predict receipt of BLT for each household in our sample. We then recreate our targeting measures. That is, we compare the predicted receipt of BLT payment with the program's expenditure threshold (converted to Rp2,000) and then calculate a measure of undercoverage and leakage for each village and an indicator of whether the program would have been active in the village.[27] We then estimate a crime regression equivalent to column 5 in table 5 using these data.

Column 6 in table 6 reports the results from this exercise. While some variables—the Gini coefficient and being in the top expenditure decile—are statistically significant and similar in magnitude to the results in table 5, the predicted presence of the BLT program and village-level targeting variables are not significant (all p-values are greater than .36).[28]

[25] This is denoted Crime 1998 in table 6. By controlling for crime prevalence as reported in the 2000 PODES, we are effectively examining changes in crime between October and December 1998 and October and December 1999.

[26] We are unable to perfectly replicate the BPS score because the SUSENAS does not cover all of the variables used to construct the score. Specifically, the SUSENAS does not have variables indicating the frequency of meat/chicken/milk purchases per week; meal frequency; frequency of new clothes purchases; access to health clinics; type of fuel used; and assets. Except for fuel use and assets we are able to use expenditure on these categories to construct proxies for the missing variables. We reweight the weights to ignore the asset and fuel variables. The resulting index thus closely resembles but is not identical to the BPS ranking of households. Approximately 30% of our sample received the BLT. Here we designate the 30% of the sample with the highest calculated BPS score as BPS recipients.

[27] When we do similar calculations using the 2006 SUSENAS, these variables are strongly predictive of actual undercoverage and leakage ($p < .000$ in both cases) and can explain 19% and 24% of actual leakage and undercoverage, respectively. The 2000 cutoff used is the per capita expenditure at the same percentile of the distribution as Rp175,000 in 2005.

[28] Note that predicted receipt of BLT is negatively associated with being a victim of crime (BLT-poor and BLT-nonpoor). This just reflects that these households get a low proxy means score as a result of having few easily observable assets and so are an unlikely target of crime.

There are a number of other falsification tests that can be conducted. We examine whether the predicted mistargeting in 2000 is a determinant of the change in crime between 2000 and 2002 (as captured in the SUSENAS 2000 data and the PODES 2002 data, respectively). It is not. We also examine whether actual mistargeting in 2005 predicts the change in crime between 2002 and 2005 (as captured in the PODES 2002 and PODES 2005). We find that it does not. (These results are available on request.)

VIII. Understanding the Pathways

The results above demonstrate that leakage of BLT funds to the nonpoor is associated with increased crime. We now examine potential pathways through which this effect might operate. We examine whether there is evidence of social capital (community participation) decreasing as a result of the BLT, and find this to be the case. We also examine whether mistargeting from the proxy-means testing methodology or mistargeting as a result of village level interference with the allocation of funds is driving the increases in crime.

A. Social Capital

We have hypothesized that, consistent with sociological theory, social capital is an avenue via which mistargeting may affect crime. Mistargeting may cause feelings of mistrust in the community and so diminish the ability of the community to work together. This may make the community more susceptible to crime both by inducing criminal acts and by reducing the community's effectiveness at combatting crime. The SUSENAS and PODES data do not provide detailed information on social capital. To examine the impact of the program on social capital we use data from the Indonesian Family Life Survey (IFLS). The IFLS is a longitudinal household survey that is representative of 83% of the Indonesian population (13 out of Indonesia's 33 provinces). It consists of four waves of data, collected in 1993, 1997, 2000, and 2007.[29] The fourth round (IFLS4) is thus conducted about 2 years after the implementation of the 2005 BLT. IFLS4 covers approximately 13,000 household and asks individual respondents whether they were a victim of crime in the previous 12 months.

In column 1 of table 8, we establish that the same relationship between the level of crime and BLT mistargeting that was found in the SUSENAS data is also evident in the IFLS data. That is, the greater the proportion of ineligible households that received the BLT payment, the higher is crime in the village. Unfortunately, previous waves of the IFLS do not collect crime data so

[29] See http://www.rand.org/labor/FLS/IFLS/ for more details.

TABLE 8

INDONESIAN FAMILY LIFE SURVEY SOCIAL CAPITAL RESULTS

Variable	Crime (0/1) (1)	Participation in 2007 (2)	Female Participation in 2007 (3)	Male Participation in 2007 (4)	Change in Community Safety (5)
Change in community participation					−.2
					(.12)*
Leakage	.11	−.43	−.75	−.11	
	(.03)***	(.21)**	(.2)***	(.33)	
Undercoverage	.11	−.35	−.32	−.43	
	(.06)*	(.43)	(.47)	(.59)	
Individual and household characteristics:					
Participation in 2000		.16	.16	.13	
		(.01)***	(.01)***	(.01)***	
BLT-poor	−.02	−.23	−.3	−.13	
	(.01)*	(.05)***	(.05)***	(.07)*	
BLT-nonpoor	−.03	−.06	−.12	.04	
	(.009)***	(.03)*	(.04)***	(.05)	
No BLT-poor	−.01	−.11	−.2	−.01	
	(.01)	(.04)***	(.05)***	(.07)	
BLT present	.01	.008	−.12	.05	
	(.03)	(.2)	(.3)	(.2)	
Age		−.004	−.003	−.004	
		(.001)***	(.001)*	(.002)**	
Married		.45	.46	.41	
		(.03)***	(.04)***	(.05)***	
Primary school education		.06	.06	.01	
		(.03)*	(.04)	(.05)	
Lower secondary education		.23	.36	.05	
		(.05)***	(.07)***	(.06)	
Upper secondary education		.21	.36	.05	
		(.05)***	(.07)***	(.06)	
Tertiary education		.33	.52	.09	
		(.09)***	(.12)***	(.13)	
Log of per capita income		.02	.02	.03	
		(.002)***	(.003)***	(.003)***	
Constant	.02	−.27	.35	−1.38	1.46
	(.06)	(.72)	(.97)	(.77)*	(.77)*
Fixed effects	District	District	District	District	
R^2	.05	.32	.38	.29	.56
N	13,957	13,957	7,610	6,347	310

Note. Results are from OLS regressions. Controls were also included for the share of eligible households in the village, whether the household was in the top or second top decile of the national income distribution, the percentage of village households in each of these deciles, the Gini coefficient of per capita income in the village, distance from the city in minutes, and the village population. Standard errors (in parentheses) are clustered in cols. 1–4.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

we cannot control for the preprogram level of crime. Undercoverage of eligible households is also marginally significant (at the 10% level) in this specification.

Both the 2000 and 2007 waves of the IFLS collect detailed information on individuals' participation in community groups. This is an often used proxy for community social capital. We use these data to examine the impact of program mistargeting on community participation, controlling for the level of preprogram participation. Specifically, we construct a measure of how many groups the individual participated in during the previous 12 months, for each of the two waves of data. The number of groups individuals participate in ranges from 0 to 18, with a mean of 1.5 in 2007.[30]

Column 2 of table 8 shows that the extent of community involvement is negatively affected by the extent of leakage of BLT funds. Columns 3 and 4 disaggregate by gender and show that this finding is driven by women's responses to leakage. An additional 10% of nonpoor households receiving BLT reduces the mean number of groups a woman participates in by 0.075 (6% at the mean). Men's participation is unaffected by leakage. BLT undercoverage is negatively signed but not statistically significant for either gender.[31] Women's participation being more responsive to the misallocation of resources is consistent with findings in the experimental economics literature that women are more concerned with the welfare of others and fairness than men (Eckel and Grossman 1998).

Although the IFLS does not provide longitudinal data on the crime rate, IFLS4 does ask a community leader to compare village safety in 2007 to village safety in 2000. The answer is given on a 5-point scale from Much Safer (1) to Much Less Safe (5). We are thus able to examine whether declines in social capital are associated with a decline in the perception of community safety which will reflect the extent of crime in the village. Column 5 in table 8 reports the results. Although the sample size is small, with only one observation for each of the 310 villages, we find that people do perceive the community to be less safe ($p = .09$).[32]

[30] Many community groups are predominantly for men or women only. Our measures for both genders reflect participation in community rotating savings associations (arisan), community meetings, community cooperatives, voluntary labor programs, and programs to improve the neighborhood. In addition, for women there are women's associations and child-weighing posts. For men, there are neighborhood security groups, water supply committees, and garbage disposal committees.

[31] The household-level variables BLT-poor and No BLT-poor are also significant. For both men and women, these two variables are not significantly different from one another and so jointly indicate that poorer households are less likely to participate in 2007, controlling for the level of participation in 2000.

[32] The IFLS data also collected information on perceptions of corruption, which allow us to directly examine whether the mistargeting measures are proxying for corrupt village administration. In par-

The results above show that leakage of funds to the nonpoor is associated with decreases in community participation and a decrease in the perception of safety in the villages. So far we have not said much about who is perpetrating the crimes. It could be that the mistargeting within the community causes people to turn on one another within that community. It is also possible that by weakening social capital, the community becomes more vulnerable to preexisting criminality, be it from within or from outside the village. That the change in women's community participation is responsive to the leakage of BLT funds to ineligible households but men's is not allows us to hypothesize on this point. It suggests that the mistargeting of the program may not have increased the propensity of individuals to engage in crime (as men are the main perpetrators of crime) but rather may have made the community more susceptible to existing levels of criminality by weakening social cohesion and hence the community's ability to protect itself. For example, it may have made people less likely to look out for one another and share information on recent incidences of crime within the village.[33]

B. Do People Resent Elite Capture or Mistargeting Generally?

The BPS administrative data can also be used to shed light on what is the root cause of people's dissatisfaction with the program. We generate mistargeting rates for 2006 arising from the proxy-means methodology (as described above for 2000). Table 9 compares actual rates of mistargeting with those that would have arisen anyway from the proxy-means methodology. The rates of mistargeting are remarkably similar. The proxy-means approach is predicted to

ticular, it asked individual respondents to rank governance in this village on a 4-point scale from Very Good (1) to Very Bad (4). Respondents were then asked how this compared to 2000 ($1 =$ Much Better to $4 =$ Much Worse) and whether there were currently any cases of corruption involving the village office. We construct means of these responses at the village level and add these as explanatory variables in the IFLS crime regression. None of the measures are close to being statistically significant, and their inclusion does not affect the coefficient on the BLT variables. Results are available on request.

[33] We also empirically examined whether the BLT affects crime via its effect on inequality. In most villages the change in inequality associated with the program, as measured by the Gini coefficient, was small. We examine this by calculating the Gini coefficient using household expenditures as reported in the SUSENAS. We then recalculate the Gini coefficients, subtracting off the amount of the BLT transfer from household expenditure in recipient households. The SUSENAS data show that the BLT resulted in increases in inequality in 16% of villages and decreases in inequality in 72% villages. In results that are available from us upon request, we add controls for the change in the community Gini coefficient associated with the BLT program to the crime regressions. The variable is not statistically significant ($p = .71$) and does not affect the coefficients on the other variables. We also construct a measure of the change in the rank of households within the village (when ranked by per capita expenditure before and after the program—calculated as the sum of the absolute difference of the change in rank, normalized by the village sample size). This variable is also not statistically significant. Therefore, we conclude that the increases in crime are not a result of increases in inequality due to BLT.

TABLE 9
TARGETING PERFORMANCE

	% Receiving BLT (Actual) (1)	% Receiving BLT (Proxy Means Calculation) (2)	% of BLT Recipients (Actual) (3)	% of BLT Recipents (Proxy Means Calculation) (4)
Poor	50.0	52.6	55.0	57.8
Nonpoor	16.6	15.6	45.0	42.2
By income quintile:				
Q1	55.1	59.1	43.0	46.1
Q2	33.5	32.5	26.7	25.9
Q3	22.5	20.4	17.2	15.6
Q4	13.2	12.1	9.7	8.8
Q5	4.5	5.0	3.4	3.7
All	26.4	26.3		

Note. Poor and nonpoor are defined to coincide with the eligibility criteria of per capita expenditure of less than Rp175,000 per month.

deliver slightly better targeting at the lower end of the distribution. Table 10 presents results with the mistargeting that can be attributed to the proxy-means approach and the remaining mistargeting (attributed to interference at the village level) as control variables in crime regressions. Both types of mistargeting are insignificant for undercoverage as expected. For leakage, however, both are significantly and positively associated with higher crime prevalence. The coefficient on the non-BPS leakage is 50% larger than the coefficient on the leakage arising from the proxy-means methodology (although not significantly so). This suggests that people are upset by mistargeting per se (irrespective of its cause), while being suggestive of people being more upset by interference at the local level. This is consistent with Widjaja (2009), which reports that protests took place because households did not receive what they felt entitled to (as a result of failing the proxy-means test) and also because in some communities the amount received was less than the full amount, with the blame for this most commonly being laid on the community leader.

IX. Discussion and Conclusion

The findings presented above suggest that a poorly administered targeted program can significantly disturb the social fabric within a community to the extent that people disengage from the community. This makes it more susceptible to crime. Crime has its own immediate costs, but there are also other intangible consequences of such disruption. Most worrying perhaps is the impact that this has on social cohesion and the willingness and ability to work together for the betterment of the community in the future. This is one facet

TABLE 10

IS IT CENTRALIZED MISTARGETING OR LOCAL MISTARGETING?

Variable	$crime_{ivt}$
Household level:	
BLT-poor	−.001
	(.001)
BLT-nonpoor	−.005
	(.001)***
No BLT-poor	−.0007
	(.001)
In 2nd top decile (0/1)	.0002
	(.001)
In top decile (0/1)	.004
	(.002)**
Village level:	
Crime pre-BLT	.003
	(.001)***
BLT present	.007
	(.002)***
BPS leakage	.007
	(.003)**
Non-BPS leakage	.01
	(.003)***
BPS undercoverage	.002
	(.002)
Non-BPS undercoverage	.0005
	(.002)
Proportion households eligible	−.009
	(.003)***
Rural	−.0001
	(.002)
Proportion households in 2nd top decile	.01
	(.006)*
Proportion households in top decile	.02
	(.007)***
Gini coefficient	.04
	(.008)***
Average per capita expenditure	−.004
	(.007)
Village population	−.0002
	(.0001)*
Male share	−.02
	(.01)
Farm households	−.0008
	(.002)
Farm laborers	−.002
	(.005)
Poor letter	−.006
	(.004)
Hours to city	.002
	(.0009)**
One ethnic group	.001
	(.001)
Fixed effects	District
R^2	.01

Note. N = 262,476. Results are from OLS regressions. Standard errors (in parentheses) allow for clustering at the village level. Note that the mere existence of the program in a village remains strongly positively associated with higher crime prevalence. This is consistent with a story where people resent a targeted program (even if precisely targeted) and resent a mistargeted program even more. For example, Gelbach and Pritchett (1995) show that under certain circumstances, any type of targeting will be a political loser relative to a universal transfer. In a country like Indonesia, with many poor households clustered close to the program cutoff point and a culture of sharing, attempts at targeting may be unpopular even if conducted fairly and transparently.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

of the BLT program that village heads made explicit in group discussions and that is backed up by our findings using the IFLS data.

The results strongly suggest that leakage of payments to the nonpoor fans the flames of social unrest. In contrast, eligible households missing out on the payment is neither associated with increases in crime nor decreases in community participation. It is well established within the social psychology literature that "sins of commission" are judged more harshly and invoke a stronger emotional response than "sins of omission" (e.g., see Spranca, Minsk, and Baron 1991; Ritov and Baron 1992; Baron and Ritov 1994). A sin of commission is one in which a person acts in a harmful way. In contrast, an act of omission is one where by omitting to act, a person harms someone. In this framework leakage of payments to the nonpoor can be viewed as a sin of commission because an action was taken to allocate the money to the nonpoor. Undercoverage of the poor, however, is a sin of omission: the poor are harmed by no action being taken to allocate the money to them. Kahneman and Miller (1986) argue that individuals perceive outcomes as being worse when they can easily imagine that a better outcome could have occurred. When an action has occurred it is easy to imagine the result of inaction, so it invokes a stronger emotional reaction. When harm is caused by inaction, imagining the result of action is more difficult, so the response is not so strong as in the case of undercoverage. Note also that undercoverage preserves the status quo. Experiments show that people have a strong preference for the status quo (Ritov and Baron 1992). This "status quo bias" may further dampen the emotional response to the poor missing out on the program. For both these reasons, communities may judge leakage of funds to better-off households more harshly than the nonallocation of funds to the poor. Consequently, the emotional reaction to leakage is stronger and more crime results.

In conclusion, our findings suggest that the negative effects of poorly administering a targeted transfer program could extend well beyond the monetary value of leaked funds. This study underscores the importance of targeting programs in a way that is acceptable to the affected communities. Program acceptance may be enhanced by improving targeting accuracy and by transparent communication of this mechanism and the program's aims to the general population. Recent work (Alatas et al. 2011; Mendoza and Prydz 2011) finds that involving the community in the targeting process substantially improves community satisfaction. It is however quite possible that the negative societal consequences associated with "sharp targeting" in a context where many households are observationally equivalent on the ground may outweigh the budgetary benefits of targeting. This is an area for further research.

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