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Heterogeneity in the Returns to Investment in Poor Villages

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Abstract

Under Indonesia's anti-poverty program, IDT, the government provided selected poor villages with grants of the same value, regardless of population size. Exploiting the variation in per household grant value that is caused by this program design, I estimate the returns to public grants, which are designated for investment loans. Results show that the returns are heterogeneous. Villages with pre-existing market facilities demonstrate increases in male labor supply, per capita income (PCI) and per capita expenditure (PCE). However, villages not accessible by land exhibit few changes in labor supply or PCI and yet an increase in PCE, particularly on festivals. These results suggest that the returns to investment capital are limited without a basic economic infrastructure.

JEL Codes: D1, H3, J2, O1
Keywords: poverty, labor supply, investment, IDT, Indonesia

1 Introduction

Access to credit and start-up capital has long been seen as an important means of escaping poverty. Theory suggests that, without such access, a vicious circle can arise which keeps the poor trapped in poverty, through a limited choice of occupations and investment opportunities (Banerjee and Neumann (1993) and Aghion and Bolton (1997)). This gives rise to policies that provide the poor with credit or start-up grants; however, the effectiveness of these investment-oriented public funds remains controversial. On one hand, subsidized credit may be leaked to the non-poor and the low cost of credit may induce risky investments.[1] On the other hand, some credit programs are reported to avoid these problems and enhance poverty alleviation and economic growth.[2] These positive effects are also found in recent empirical studies that utilize exogenous policy changes (Banerjee and Duflo (2004), Burgess and Pande (2005), Burgess and Pande (2003));[3] however, such evidence is still relatively scarce. Literature on the impact of microfinance also provides mixed evidence, with recent studies that control for endogeneity issues suggesting muted or little effect compared to more descriptive evidence indicating larger benefits (Armendariz and Morduck (2005)).

This paper contributes to these strands of literature by providing a new set of evidence based on a unique identification strategy; it shows that the returns to public capital for

[1] For example, Hoff and Lyon (1995) theoretically illustrate this possibility. Adams and Vogel (1986) provide numerous descriptive and analytical studies on various institutional aspects of rural subsidized credit programs. Braverman and Guasch (1986) also discuss the importance of politics and institutional mechanisms involved in loan management.

[2] Armendariz (1999) and Adams and Vogel (1986) point to specific features needed for public financial services to achieve these positive outcomes.

[3] Also, Binswanger and Khandker (1995) and Binswanger, Khandker, and Rosenzweig (1993) find a large, positive effect of credit on investment and a small effect on output in India's agricultural sector, using prices and agro-climate conditions as instruments.

business loans vary by the initial local condition. The identification strategy exploits a quasi-experimental program in Indonesia, *Inpres Desa Tertinggal* (IDT, 1994-1997). Under this program, the government provided selected poor villages with the same value of lump-sum grants, *regardless of the population size*, as a fund for investment loans. This creates the variation in the grant value per household (or the value divided by the number of households in 1993). Using this variation, I estimate the effect of the marginal increase in the public investment capital on labor supply, household per capita expenditure (PCE), and per capita income (PCI), conditional on grant receipt. The utilization of the variation among targeted villages allows me to purge a possible bias due to endogenous program placement. Though village size can be correlated with the outcomes, I address this issue by first controlling for village fixed effects, and second by using the estimated correlation between village size and the outcomes prior to program implementation as a proxy for the relationship that would have been realized without the program. I also examine how the change in the correlation evolves after the termination of the grants. A similar approach is used by Banerjee and Duflo (2004) and Burgess and Pande (2005), where policy introduction and termination are exploited with panel data straddling these changes.[4]

Results show that IDT had a limited overall impact. However, the heterogeneity analysis reveals that villages with pre-existing market facilities show particularly strong positive impacts on labor supply, PCI and PCE. For example, the annual rate of return for PCE is higher

[4]Banerjee and Duflo (2004) utilize prioritized access to subsidized loans that was legislated, and later abolished, to identify the returns to investment for medium-scale firms in India. Burgess and Pande (2005) use the fact that, between 1977 and 1990, if banks wanted to build a new branch in locations that already had a bank, they were obliged to establish four branches in locations with no pre-existing bank. They estimate the impact of the number of banks in such "unbanked" locations.

by 34 percentage points - a substantial difference compared to the average rate of 2 percent. In contrast, villages without land access show an immediate increase in PCE, particularly on cultural festivals, with no significant change in PCI or labor supply outcomes. These findings suggest that program resources are used more productively in villages with a local market infrastructure and greater access to outside their communities.

The findings are also relevant to the literature on firms' returns to capital in developing countries. In their recent literature survey, Banerjee and Duflo (2005) suggest that the returns to investment are likely to be highly heterogeneous within an economy. However, there is little direct empirical evidence of such heterogeneity.[5] Though several studies have examined IDT, no study has investigated the heterogeneity in the impact of the program by the initial conditions. Also, no study has exploited the variation in the grant value per household at the village level.[6] This identification strategy not only imposes less stringent assumptions but also allows for the inclusion of the poorest villages - and it is for these villages that an understanding of the impact of the anti-poverty program is most relevant. This contrasts with other studies on IDT which use matching methods, thus excluding the poorest villages.[7] Finally, this paper provides the first evidence on the impact on a new set of outcomes such

[5]Exceptions include the study by De Mel, McKenzie, and Woodruff (2007), who use a field experiment to estimate the returns to capital in Sri Lanka.

[6]At the province level, places receiving a larger per capita grant achieved greater within-province equality after the introduction of IDT (Akita and Szeto (2000)).

[7]Since the government intentionally selected poor villages for grants, when this selection is not controlled, the average household PCE is lower in districts where a larger fraction of villages receives grants (Daimon (2001)). In order to control for this, Molyneaux and Gertler (1999) apply the propensity-score-matching and village fixed effects model. Results, however, show no significant difference between villages with and without grants in changes in a number of outcomes, including labor supply and household expenditure measures. Using a different matching method that utilizes the government's selection rules, Alatas (2000) finds that treated villages in rural areas have a higher level of household PCE and higher proportions of spouses at work. However, once province-level fixed effects are incorporated, no effect is found for household expenditures, and the results for labor supply are not reported.

as per capita income and employment status by sector and occupation. The rest of the paper is organized as follows: Sections 2 and 3 describe the data and more details of IDT, followed by the illustration of the identification strategy. Section 5 shows the empirical results on the overall impact of IDT. I further discuss the issues and results on the heterogeneity in the impact in Section 6. Finally, Section 7 concludes.

2 Data

The following three datasets are combined for this study: 1993-99 *Survei Sosial Ekonomi Nasional* (SUSENAS, National Socio-Economic Household Survey), 1993 *Potensial Desa* (PODES, Village Potential Survey), and the administrative data on IDT. The SUSENAS is a nationally representative, cross-sectional household survey. It provides the information on labor supply, household income and expenditures, which is aggregated at the village-level for the analysis. The PODES contains the data on the number of households and various village characteristics in a pre-program period. The administrative data indicate which villages were funded under IDT.[8]

This paper focuses on rural villages that were selected for funding in the initial year (analysis sample), for which the significant impact of IDT is found.[9] Other funded communities (urban communities and rural villages that were selected for funding in later years) show few program effects, most likely due to the smaller sample size, smaller per household grant, and

[8]The proportion of SUSENAS villages that are matched with the two other datasets averages 88%. Details on the construction of the sample are available on the author's website, http://econrsss.anu.edu.au/Staff/yamauchi/pdf/ReturnsAppendices2.pdf.

[9]Rural villages received about 95% of IDT grants; among these villages, the analysis sample received 82% of the grants (based on the IDT data).

shorter exposure to the program.[10] In the following sections, therefore, I discuss the results for villages in the analysis sample.

3 Indonesia's Poverty Alleviation Program: IDT

3.1 Scope, Objectives, and Per Household Grant Value

After periods of rapid economic growth, Indonesia's progress in poverty alleviation slowed down in the early 1990s. The government launched IDT in order to enhance the employment opportunities and welfare of the poor in villages that were "left-behind" during the growth periods (Badan Pusat Statistik (BPS) (1994)). The program covered over 20,000 out of approximately 65,000 villages for three fiscal years, 1994/95 - 1996/97. With each of the selected villages receiving Rp.20 million (20 million rupiah, approximately US$8932[11]) per annum, the government expenditure for this program totalled more than Rp.1.2 trillion (US$536 million). Forty-two percent of households in the analysis sample participated in the program at least once by the end of the three-year program period. In each year, 16-25 % of households received a loan, which averaged Rp.354,562 - nine times their monthly per capita expenditure (PCE) in 1996.[12]

[10]The absence of significant impacts of IDT for communities that were funded from the second year is consistent with Alatas (2000). She finds no systematic pattern in the impacts for these communities across provinces.

[11]Based on the average exchange rate in 1995, Rp.2239 per US dollar (Indonesian Financial Statistics, Bank Indonesia). The government also provided a subset of villages (about 27%) with infrastructure such as roads, bridge, water, and sanitation facilities. Given the lack of information on villages that received these infrastructure grants, the effects of grants for loans and infrastructure cannot be separately identified. However, the infrastructure grant is unlikely to be driving the results. See Appendix 1.

[12]A household is defined as a participant if it answers that one of the household members belongs to *pokmas*, a group of individuals eligible for IDT loans, and the household has actually received a loan. The loan size is the annual cumulative amount of credit. The participation rate and average loan size are likely to be the

Importantly, both the participation rate and average loan size are positively associated with the key policy variable of this paper, the value of per household grant. The nonparametric estimates indicate that villages with fewer households achieve higher proportion of participating households (left panel, Fig.1) and larger average loan size among participants (right panel). These relationships suggest that, if the program has any causal effects, smaller villages are likely to demonstrate disproportionate changes in the outcomes of interest.

3.2 Two-Stage Selection of Beneficiaries

IDT funds were *grants* from the government to selected poor villages, but were expected to be used as rotating *loans* for poor households within the villages. In the first year, a village was considered poor, and thus selected for funding, if it received a relatively low value within the province for a living-standard indicator called village score, which was created by the government. Those villages selected in the first year generally continued to receive grants in the second and third years of the program.[13] The selected villages were directed by the government to choose relatively poor households as eligible for IDT loans, using their own local knowledge and criteria. Though this process is unknown to researchers, empirical evidence indicates that eligible and participating households were in fact poorer within the selected

upper bounds for the proportion of households that directly received funds and for the average value of the funds. This is because, even if a household with a *pokmas* member has not received a loan, it can be recorded as participating if the *pokmas* has received IDT funds as a unit. In this case, the loan size is reported as the value of funds that the *pokmas* has received divided by the total number of the *pokmas* members.

[13]Funding was terminated for a small proportion of villages with a very small number of households, based on the concern about the inequality in the value of per household grant. This funding history is taken into account in the computation of the grant value for these villages. In the regression analysis, the same set of coefficients is applied for these villages as for villages that were fully funded for three years. Relaxing this assumption does not substantively change the results. See Appendix 2 for a more detailed description of the selection procedure.

villages. Their loan size were however smaller, therefore the average value of program funds given to poorer and wealthier households did not vary much (Yamauchi (2007)).

Selected households received the grant directly, and became mainly responsible for loan management. These households first formed groups called *Pokmas* (community groups) and made a group loan proposal.[14] Once it was approved by the subdistrict office, the grant was given through a local branch of a state-owned commercial bank to the *pokmas* treasury, who then extended loans to the members (BPS, 1994). Lending conditions such as the interest rate, terms of repayment, and sanctions against defaults are also unknown to researchers. A predominant proportion (78%) of participants reported that they invested these loans in the agricultural sector (animal husbandry, crop cultivation, fishery or other agricultural activities), while the rest invested in trading, small-scale manufacturing, and services.[15] The rate of repayment was not high; on average, only 20% of these households repaid the loan.[16] This suggests that, in some cases, IDT functioned almost as grants even *within* villages.

3.3 Misappropriation and Returns to Investment

Due to the implementation process, and for other reasons, it is difficult to theoretically predict the effects of IDT on labor supply and household PCE and PCI. First, for the program to increase all of these outcomes, participants must invest in productive activities that increase

[14]These households were helped by officials who were recruited and assigned by the government and local volunteers such as teachers and NGO workers (OECF, 1999).

[15]Based on the self-reported usage of IDT loans (1998 SUSENAS).

[16]Fifty-three percent of the sample villages had at least one sample household participating in IDT in 1996. For these villages, I calculated the proportion of participating households that repaid by the beginning of 1998. No participating household repaid in 70% of the villages, and in the rest of the villages, the repayment rate averages 65%.

labor demand. However, such investment may not take place if the participants' expected returns are low or if the borrowers repayments are not strictly enforced. For example, participants might rather misappropriate the program funds for short-run consumption, immediately increasing household expenditure, but possibly reducing labor supply. Further, even if participants decide to invest, it is uncertain whether it increases or decreases labor demand, depending on whether a purchased input is a complement or substitute for labor.

Even if we assume that IDT induces investments and increase labor demand, in order to increase PCI and PCE, investments also need to be profitable. If investments fail or yield only small returns,[17] the change in household PCI and PCE may be limited. Finally, if household income is significantly increased, demand for leisure may offset the demand for labor input, diluting the effect on labor supply. In order to address these theoretical ambiguities, this paper empirically explores the impact of IDT by utilizing the following identification strategy.

4 Identification Strategy

4.1 Per Household Grant Value and Village Size

In order to control for the government's selection of poor villages, I focus on the analysis sample, consisting of villages that are all selected for funding under the common criteria. Using this sample, I estimate the effect of a marginal increase in the value of grant per household, conditional on grant receipt. This estimate is relevant because, given the anti-poverty objective of the program, the population of interest is these selected poor villages,

[17]For example, the death of livestock and a lack of knowledge on tools needed for fishing are reported as cases where investments have failed (Kimura (1999)).

rather than all the communities in the country. This type of geographic targeting is often used in developing countries.

The per household grant value is defined as the cumulative value of grants received by the village divided by the number of households as of 1993. If a village received a grant for a full three years, then the cumulative, nominal value is Rp.20 million, Rp.40 million, and Rp.60 million for 1995, 1996, and 1997, respectively. The value remains Rp.60 million in the post-program period of 1998 and 1999. A cumulative, rather than contemporary, grant size is used to measure the maximum value of money that could be used for rotating loans. The use of the 1993 number of households is likely to purge a possible bias in per household grant value that arises from possible migration motivated by the program.[18] Since households, not individuals, were considered as a unit to receive a loan, I use the grant value per household, instead of per capita.[19]

Because the variation in per household grant value arises from the variation in village size, if small villages are inherently different from large villages, then my estimates can falsely attribute the inherent differences to the effects of IDT.[20] I control for a possible time-invariant

[18]A number of factors suggest that such migration was unlikely to have happened. First, in order to be eligible for IDT loans, households needed not only to be residents (Kimura (1999)) but also to create a business proposal in cooperation with *pokmas* members. Second, villages with fewer households tended to be poorer. Perhaps reflecting these factors, between 1993 and 1996, the number of households decreased more, if anything, in villages that initially had a smaller number of households. This does not preclude a possible increase in the number of poor migrating households and an offsetting decrease in the number of wealthy migrating households; however, even if this is the case, the use of the number of households as of 1993 suppresses a bias stemming from population movements induced by the program.

[19]Officially, a family is a unit to receive a loan. A family usually consists of a household head, his/her spouse and children. There are therefore some households where the head's parents make the second family. However, the average household size is 4.3 and the proportion of members aged 56 and over is 6.5%. This suggests that a household is a reasonable approximation for a unit to receive an IDT loan.

[20]In fact, the non-parametric estimates for the 1993 cross-sectional correlation (not shown) suggest that smaller villages tend to spend less on non-food items and have a larger number of men aged 20-60 self-employed and engaged in agriculture. A similar pattern is indicated for women. These differences could be due to unobserved factors, some of which are time-invariant (for example, soil quality, at least for the short

factor by incorporating village fixed effects; in addition, I take into account a time-variant factor by using the estimated correlation between village size and outcome variables in the pre-program period, 1993-1994. That is, I assume that the correlation between village size and unobserved time-variant factors remained unchanged before and after the start of the program, and use the estimated 1993-94 correlation as a proxy for what the post-program correlation would have been, had there not been IDT grants. Specifically, I assign the value of Rp.1000 divided by the 1993 number of households to the variable of per household grant for this period.[21] All these values are adjusted for inflation and expressed in terms of 1995 prices. As a result, the average per household grant value increases significantly with the start of the program in 1995, keeps increasing until 1997, and then decreases afterwards (Appendix 3). The increments between 1995 and 1997 are small due to the termination of funding for villages with very small numbers of households.

My identification strategy can be graphically illustrated. For example, Fig.2 shows the nonparametric estimates for the relationships between the natural log of per household grant value and the change in the proportion of men who are at work.[22] The relationship for the pre-program period (1993-1994) indicates that smaller villages are experiencing a slightly larger decline in the proportion. In contrast, the relationship for the period encompassing the program launch (1993-1996) indicates that villages with larger values of per household grant

or medium term) while others are time-variant (for example, industry mix, human capital, and quality of infrastructure).

[21]Rp.1000 is an arbitrarily chosen value; however, due to the natural log specification in the regression equation (see Eqs.1-3), the value for the enumerator in the variable does not significantly affect the results; it only shifts the intercept.

[22]The logarithm is used because the per household grant, or the number of households, is skewed and also non-parametric estimates such as those shown in Fig.2 indicate a linear relationship for many outcome variables. This assumption is relaxed later in the heterogeneity analysis.

show a smaller decline or a larger increase. This change in the relationship suggests that the larger grant value per household is attributable to increased work opportunities. If this was due to some underlying trend specific to smaller villages, it should appear in the relationship for the pre-program period; however, the results for that period demonstrate no such tendency.

4.2 Econometric Specifications: Overall Impact

Following the graphical illustration, I specify the following village fixed effects model for the period of 1993-96:

$$Y_{jt} = \alpha_0 + \alpha_{96}T_{96} + \delta_0 lnF_{jt} + \delta_{96}[lnF_{jt} * T_{96}] + \mu_j + u_{jt} \ \ (t = 1993 \text{ and } 1996) \qquad (1)$$

The outcome variable, Y_{jt}, is for instance the proportion of men in village j in year t who are at work in the week previous to the survey interview.[23] It is assumed to be a function of the year dummy variable, T_{96}, village fixed effects, μ_j, the natural log of the value of per household grant, lnF_{jt}, and its interaction with the year dummy. The parameter of interest is δ_{96}. This corresponds to the steepness of the relationship for the period of 1993-96 in Fig. 2. The estimate is net of both the 1993-1996 change that is common to all the funded villages, α_{96}, and any time-invariant, village-level, unobserved factors. This pair-wise estimation can be conducted for years 1993 and 1994, 1993 and 1995, ..., 1993 and 1999 using villages that were observed both in 1993 (the baseline year) and one of the later years.[24]

[23]Each observation (village) is weighted according to the accuracy of the dependent variable; that is, weights are proportional to the number of individuals or households observed in the village.

[24]For periods 1993-1994 and 1993-1995, the coefficient δ_0 cannot be estimated because the variable F_{jt} does not change between 1993 and 1994 and its values for 1993 and 1995 are collinear with each other. The effect of the variable in these periods is absorbed in the village fixed effects.

Another key parameter, δ_{94}, is estimated by applying Eq.(1) to the 1993 and 1994 data. This is equivalent to the steepness of the relationship for the pre-program period in Fig.2. By comparing this benchmark estimate with δ for 1995-1999,[25] I test whether the effects found for the periods of 1993-95, ..., 1993-1999 are due to the program or due to unobserved time-variant factors that are specific to smaller villages. In order to conduct this test parsimoniously, I also estimate the following village fixed effects model that pools villages that are used for the estimation of Eq.(1) for the periods of 1993 and 1994, ..., 1993 and 1999:

$$Y_{jt} = \alpha_0 + \Sigma_{s=94}^{99}(\alpha_s T_s + \delta_s[lnF_{jt} * T_s]) + \mu_j + u_{jt} \ \ (t = 1993 - 1999) \tag{2}$$

All the included villages are observed in 1993; thus, δ's are identified by the change before and after the program's introduction (not by the change after 1994) in the correlation between the outcome and the log of per household grant value.[26] Note that, in general, the estimated impact includes the direct effect for participants and possible indirect effect for non-participants because the outcome variables are the aggregated figures for all the residents in the sample villages. This overall impact is an important parameter of interest, given that IDT is a public investment in poor villages.[27]

[25]The comparison between δ_{94} and δ for later years could still produce a spurious result if the trend in the outcome changes specifically for smaller or larger villages between 1994 and 1995 due to reasons unrelated to IDT.

[26]Because of the collinearity mentioned in footnote 23, lnF_{jt} is not included in Eq.(2). However, the estimates for δ for 1996-1999 indicate substantively consistent results between Eq.(1), which controls for lnF_{jt}, and Eq.(2), which does not. The weights are applied in a manner consistent with Eq.(1). The error term permits any arbitrary correlation across time within a village in order to robustly estimate the standard errors (Bertrand, Duflo, and Mullainathan (2004)).

[27]The household-level impact is of interest as well. However, since the data is repeated cross-section, it does not provide the information on the outcomes before and after the program at that level. With the number of households per village being mostly 16 or 32, matching of households across years is unlikely to be reliable.

5 Results on Overall Effects

5.1 Occupational and Sectoral Distribution of Labor

Overall, the results indicate that men shifted into the agricultural sector after the introduction of IDT, which is consistent with the predominant share of agricultural projects reported by participants. Table 1[A] shows the results of estimating Eq.(2) for a number of male labor supply measures (see Appendix 4 for the definitions). The estimates for δ's in Column 2 demonstrate that the change between 1993 and 1994 in the proportion of men who were at work in the agricultural sector did not vary between villages of different sizes. In contrast, the proportion significantly increased between 1993 and 1995/96/97 particularly in villages with a larger value of per household grant. The effect then dissipated once villages stopped receiving additional funding in 1998 and 1999. The estimates for the year dummies indicate that the positive effect on agricultural employment is partly due to the relatively large decline in the proportion of agricultural workers in villages with a larger per household grant.[28] Altogether, the lack of a significant relationship in the pre-program period (1993-1994), coupled with the subsequent positive correlations during the program period (1993-1996/97), suggests that the changes in the outcome reflect the causal effect of IDT. The estimated coefficients however suggest a very limited effect. For example, when evaluated at the mean per household grant value (Rp.167,900), the 1997 coefficient (0.038) indicates that an additional Rp.1000 increases

[28]The predicted change in the outcome is $Y_{jt}|_{t=s} - Y_{jt}|_{t=1993} = \alpha_s + \delta_s lnF_{jt}$. For instance, for 1996, the value of lnF_{jt} ranges from Rp.2.20-7.65. The change associated with the minimum and maximum values of lnF_{jt} are -0.100 and 0.042. These figures indicate that, between 1993 and 1996, the proportion of agricultural male workers decreased by 10% in the village with the largest per household grant, but increased by 4.2% in the village with the smallest per household grant.

the outcome by 0.02 percentage point.[29] In other words, a 10% increase in the grant value (Rp.16,790 or US$7.5) will lead to a 0.38 percentage-point increase-0.5% of the 1993 mean proportion of 76% (See Table 1[B] for the 1993 summary statistics of the outcome variables).

This small, positive effect on men's agricultural employment reflects the reallocation of workers from the non-agricultural sector. Though the effect on the overall proportion of men at work is statistically significant in 1996 and 1997 (Column 1), the 1997 marginal effect of Rp.1000 is just 0.004 percentage points. Further disaggregating workers by occupation, I find that the increase in agricultural workers was driven by the increases in the proportions of wage workers and self-employed workers in the sector (Columns 3 and 4). In contrast, the proportions of wage and self-employed workers in the non-agricultural sector declined in these periods (Columns 6 and 7). One interpretation of these results is that IDT induced workers to start up their own agricultural businesses (and thus move into the self-employment category); these new and existing self-employed workers also increased demand for hired labor.[30] While workers reallocate themselves across sectors, their average number of work hours does not change very much (Columns 9 and 10).

These findings are robust. The results based on the pair-wise estimation specified in Eq.(1) suggest a qualitatively consistent pattern of changes (Appendix Table 1).[31] In addition, the results are not altered even if I take into account the trends in the outcome variables that are specific to smaller villages during the pre-program period. The difference in coefficient δ_{94}

[29] Based on $\partial Y/\partial F|_{s=1997} = \delta_{97}/F$. A unit of variable F is Rp.1000 in 1995 prices.

[30] The overall impact on the proportion of wage workers includes a possible increase in the local wage rate, which can negatively affect the net buyers of labor. It is beyond the scope of this paper to disentangle the direct effect and indirect, general equilibrium effect.

[31] All the Appendix Tables are available on the author's website, http://econrsss.anu.edu.au/Staff/yamauchi/pdf/ReturnsAppendices2.pdf

and each of the coefficients for the subsequent years is depicted in Table 1[B] with the p-value in the parenthesis. Since δ_{94} is in general not significantly different from zero, the results in Table 1[B] mirror, or if anything strengthen, those in Table 1[A].[32]

5.2 Household Income and Expenditure

Though IDT slightly shifted male workers into the agricultural sector (which could have become more productive due to the injection of IDT capital), this change was accompanied by very limited reduction in poverty. For example, household PCI (see Appendix 5 for their definitions) does not show a significant difference in the change between 1993 and 1996 across villages with varying values of per household grant (Column 1, Table 2). If anything, the results for disaggregated income components indicate a significantly negative effect on agricultural PCI (Column 3). The results for earned PCI indicate a positive but insignificant effect (Column 2). Similarly, the results for PCE (See Appendix 6 for the definitions) do not show a significant impact on total, food or non-food PCE (Table 3). Only when the non-food expenditure is disaggregated, do the results show the positive effects on clothing and festivals. However, the effects were found only in the last stages of the program period (Columns 7 and 10); and then, analogous to the effect on male labor supply, the expenditure effects faded away in 1999.[33]

[32]Compared to men, the effect for women is even more limited, suggesting that the impact of IDT on labor market outcomes was concentrated among men. Women show a positive effect on the 1993-96 change in the proportion of non-agricultural, self-employed workers (Appendix Table 2). This increase in the proportion of non-agricultural, self-employed female workers is coupled by the offsetting negative effect on the proportion of non-agricultural wage workers. These two changes suggest that IDT reallocates female workers into the self-employment sector.

[33]The average food expenditure shows a significantly negative effect in 1999. This is not due to a decline in the level of food expenditure; rather, the increase in the level was smaller in villages with larger values of per household grants. This might be related to the currency crisis in 1998; for example, even if the quantity of

These results suggest that IDT brought about limited increases in agricultural employment and consumption. Also, the impact of IDT was rather temporary, as opposed to the expectation that the program funds would serve as a rotating fund enhancing sustainable regional development.[34] The analysis thus far however has assumed homogeneous effects across funded villages, even though their economic conditions vary substantially. Some conditions may induce investments while others could make villages more susceptible to misappropriation. The next section sheds more light on this issue by examining types of villages that exhibit patterns of impact that are associated with misappropriation versus productive use of program funds.[35]

6 Heterogeneity in the Impact of IDT

6.1 Heterogeneity by Initial Local Conditions

One source of heterogeneity in the impact of IDT is differential initial conditions. Particularly, attributes such as the availability of market facilities, access to the outside of the village, and the existence of other sources of credit are likely to affect the impact of IDT by influencing the profitability of investment and the quality of program implementation. For example, the expected returns from entering or expanding firm/farm businesses are likely to be higher if a village has some marketplaces where entrepreneurs can purchase inputs and sell outputs. Such

food consumed did not change, if the villages that received a larger amount of funds per household experienced lower rates of increase in food prices, their food expenditure (which was not adjusted for the regional price difference) would appear smaller. Inflation can be controlled only across years and not across years and villages.

[34]The other possible form of welfare-improvement induced by IDT is increased savings. The available data, however, do not allow a thorough examination on the impact on savings. See Appendix 7 for details.

[35]Part of the mismanagement of the program can be the failure to target beneficiaries. This issue is addressed in Yamauchi (2007).

economic infrastructure is therefore likely to induce investment rather than misappropriation. Also, limited access to outside of the village might also imply both lower expected returns to investment and little pressure from the upper-level government to monitor participants' investments. Both of these are likely to discourage investment. Implications of the availability of credit institutions are more ambiguous.[36] It may be associated with an enhanced program implementation (such as the selection and grouping of poor households) if credit institutions have more advanced social networks. On the other hand, the access to other sources of credit could make IDT funds less attractive, as these have the government's guidelines attached to them. Finally, the effect of grant per household may not be log-linear. Villages with a very large value may have a disproportionate effect; or alternatively, the effect could be weaker in these villages if abundant program funds induce loose screening of loan application.

These possibilities are tested by employing the following fixed effects model, which allows the coefficients of the time dummies, T's, and the interaction terms, $[lnF_{jt} * T]$'s, to vary across villages with different characteristics, Z_j (See Appendix 8 for detailed definitions):

$$Y_{jt} = \alpha_0 + \Sigma_{s=94}^{99}(\alpha_s T_s + \beta_s[T_s * Z_j] + \delta_s[lnF_{jt} * T_s] + \theta_s[lnF_{jt} * T_s * Z_j]) + \mu_j + u_{jt} \qquad (3)$$

An intuitive interpretation of this model can be illustrated for the case where a village

[36]Though Indonesia had relatively developed formal financial services when IDT started, not many villages in the sample had access to such services. For example, though a state-owned bank, *Bank Rakyat Indonesia* (BRI), had more than 1.8 million loans outstanding in 1990, these loans were mainly extended to the non-poor in urban areas (Snodgrass and Patten (1991); Patten and Rosengard (1991)). There were banks with a focus on rural areas and poorer households, such as village banks and *Badan Kredit Kecamatan* (BKK) in Central Jakarta (Riedinger (1994); Patten and Rosengard (1991); Morduch (1999)). However, only 14% of IDT villages had some banking facilities in 1993. Other credit sources were also limited; 11% had cooperatives in 1993 and 36% received a public credit program in 1992 (Appendix 8).

characteristic, Z_j^k, is a dummy variable indicating the availability of market facilities. While the benchmark year effects are estimated by α_s, villages with market facilities can take their own year effects, $\alpha_s + \beta_s^k$. Similarly, while δ_s estimates the benchmark effects of the value of grant per household, the coefficients for these villages can deviate from δ_s by taking $\delta_s + \theta_s^k$. The parameters β_s^k and θ_s^k are identified by the variation in the value of grant per household among villages with market facilities. Then, the change in the effect of the per household grant value between 1994 and year s, $(\delta_s + \theta_s^k) - (\delta_{94} + \theta_{94}^k)$ (s = 1995-1999), indicates the effect of IDT for these villages. In other words, $(\theta_s^k - \theta_{94}^k)$ shows how the effect differs for villages with characteristic Z_j^k compared to benchmark villages that do not have any of the characteristics indicated by the set of Z_j's - that is, villages that are characterized by initially having land access to outside of the village and a relatively small size (the 1993 number of households was fewer than the median) but no general-purpose market, no agricultural production input market, no cooperative, and no public credit program.

6.2 Results on Heterogeneous Effects

Once the impact is allowed to be heterogeneous, the results reveal that IDT not only increased male labor supply but also alleviated poverty in villages where a general-purpose market infrastructure had been in place when IDT started. It becomes also clear that isolated villages exhibit stronger evidence indicating the limited productive use of grants.

6.2.1 Market Matters

First, the results in Table 4 indicate that the effect on the change in PCE between 1993 and 1996 is larger in villages with market facilities compared to the benchmark effect. That is, while the benchmark estimate suggests that a 10% increase in per household grant value increases PCE by Rp.67.4, the increase is Rp.868.7 among villages with general-purpose market facilities. These figures represent 0.17% and 2.15% of the 1993 mean PCE of Rp.40,380. The estimates are net of the pre-program correlation between the change in PCE and village size. Also, the deviations in 1997 and 1998 from the benchmark effects are not very small, with the p-values 16% (1997) and 14% (1998). The total impacts (the sum of the benchmark effect and the deviation) for these villages are significantly positive for the whole program period of 1993-1996/97/98. This positive effect on total PCE mainly reflects the increased expenditures on necessities such as food (1993-1996/97/98) and clothing (1993-1996) (Appendix Table 4[A] and Appendix Table 5[B]).

Second, among these villages with market facilities, places with a larger grant per household demonstrate a greater change in PCI between 1993 and 1996 (Column 1, Table 5). Evaluated at the mean grant value, a 10% increase in the grant value will increase PCI by Rp.6,020.6 (12% of the 1993 mean PCI), which is Rp.2,304.9 greater than suggested by the benchmark estimate. This income effect is chiefly driven by the increase in agricultural income (Column 3). Third, men in these villages significantly increased their average number of work hours between 1993 and 1996/97 compared to the benchmark effect (Table 6). A 10% increase in per household grant value will raise the average weekly number of work hours by 0.47-0.79 (1-2%

of the 1993 mean work hours), which is 0.36-0.45 hours longer than implied by the benchmark estimates. The results for the other labor supply measures (not shown) suggest that the proportions of men at work and men engaged in the agricultural sector are also stronger in these villages, but the magnitudes of the deviations are not statistically significant. Thus the main deviation in the effect on labor supply is the impact on the intensive margin.

Altogether, the results for villages with pre-existing general market facilities suggest that IDT-induced investment increased work opportunities and household income, eventually bringing about a positive impact on household consumption. This finding is related to Binswanger et al. (1993) and Binswanger and Khandker (1995), who show that the availability of a regulated market and commercial banks contributes to the growth in agricultural output. My results suggest that market infrastructure and access to investment capital have interaction effects on labor supply, income and expenditure. Possible explanations for the interaction effects are that the presence of market facilities reduces the transaction costs and raises the expected returns to investment, thereby also inducing compliance. Villages with market facilities might also have better roads as well as educated village heads and population; however, the impact of IDT does not significantly vary by these village attributes. Therefore, for simplicity, these are not included in the regressions (See Appendix 8).

6.2.2 Misappropriation in island villages?

The other major finding is that, for villages that had no land access, the results are indicative of limited productive use, or possibly, misappropriation. That is, there is a large and immediate increase in PCE, accompanied by no effect on labor supply or PCI. Villages where the main

inter-village transportation is by sea or by air show a significantly positive deviation in the impact on the change in household PCE between 1993 and 1996/97/98 (Table 4). The effects suggest that a 10% increase in per household grant value will increase PCE by Rp.1,001.7 on average, which amounts to 2.5% of the initial average PCE. This positive effect on total PCE reflects the increase in expenditures on food, housing, and festivals (Appendix Tables 4[A], 4[B] and 5[A]-5[C]). Particularly, the results on the festival expenditure emerge immediately in the first year of the program period, while the results on the other expenditures arise in the later years. Despite this positive deviation in the effect on consumption, these villages do not exhibit a significant deviation in the effect on labor supply outcomes. If anything, the effect on the average number of work hours among men indicates a significantly negative deviation between 1993 and 1998 (Table 6).[37] Perhaps reflecting this, the impact on household PCI for these villages is no different from, or slightly smaller than, the benchmark effect (Table 5).

These results are likely to be taken as an indication that a larger proportion of IDT funds were not invested in these island villages. Though it is possible that part of the expenditure on food and housing was used for businesses (See Appendix 6), even if this is the case, there was no measurable deviation in the impact on employment. A possible reason for island villages to be associated with these responses is low expected returns from investments as well as a loose enforcement of compliance with the government's guideline to invest in productive activities. Note that the results control for the heterogeneity of the effect of IDT by village size. Thus, the results are not driven because island villages have fewer households.[38]

[37]It shows a positive deviation in 1998 when the benchmark effect becomes negative. However, the timing and the decline in the benchmark effect do not seem to suggest that this positive effect is due to IDT

[38]Villages with a greater-than-median 1993 number of households indicate a negative deviation in the effect on PCE in 1997 and 1998, thus indicating the concentrated poverty alleviation effect in smaller villages. This

6.2.3 Some Help from Earlier Credit Programs

The last finding is that the results for villages that had received other public credit programs before the start of IDT indicate a poverty reduction effect. That is, positive deviations are found for these villages in the impact on the proportion of agricultural workers in 1995 (Appendix Table 6) and in the impact on agricultural PCI, though this income effect is not large enough to affect the total PCI (Table 5). This might be because some of the previous programs provided credit to people engaged in agriculture and to people who were willing to use proposed production technology (see Appendix 8). These programs might have equipped workers in these villages with higher production skills, which complemented IDT capital.

6.3 The Rates of Returns

The results for the heterogeneity analysis imply that the rate of return to IDT varies by the initial local conditions. Based on the preferred specification in Eq.(3), the average rates of return for PCI and PCE are 27% and 2% per annum, respectively (Columns 1 and 2, Table 7).[39] The rate for PCI is somewhat lower than the range indicated by previous studies on the firms. For example, based on a field experiment conducted in Sri Lanka, the average rate of return to investment is 5.7% per month, or 84% per annum (McKenzie and Woodruff, 2007). The return to business loans is estimated to be 73% in India (Banerjee and Duflo, 2004). The rate of return to IDT is lower possibly because the household-level enterprises funded under

effect is obscured in the previous analysis because the log-linear functional form is imposed.

[39]The effect on PCI can be seen as gross returns to the program because the wage costs for self-employed and unpaid family workers are not subtracted (loan repayments are taken into account). The return measured by PCE does not indicate profitability, but signifies the ultimate impact on the level of well-being or poverty, which is relevant given the aim of the program.

the program are smaller than the firms examined in previous studies. Also, possible corruption or an adherence to the guidelines on targeting the poor may have resulted in an allocation of IDT funds to unprofitable investments. In addition, as the results suggest, some of IDT funds may have been misappropriated for consumption.

The heterogeneity in the returns to IDT can be seen in the marginal change in the rate of return associated with one of the village characteristics indicators, Z_k:

$$12 * [\theta_s^k / F_{jt}] * 100 = 12 * [(\delta_s + \theta_s^k + \theta_s^{-k} * Z^{-k}) - (\delta_s + \theta_s^{-k} * Z^{-k}) / F_{jt}] * 100$$

where θ_s^k and θ_s^{-k} are the coefficients for Z^k and other village characteristics indicators, Z^{-k}. This marginal change is evaluated at the average per household grant value among villages that have the specific characteristic (that is, $Z^k = 1$). The results demonstrate that having market facilities is associated with 34 percentage-point and 300 percentage-point increases in the rates of return for PCE and PCI, respectively. On the other hand, having no land access is associated with negative returns for PCI and high returns for PCE (particularly for non-food PCE). Though the rates for PCI have to be interpreted with caution as they are based on a small sample, the estimates for PCE, which are based on a larger sample, consistently indicate the advantage associated with having market facilities. These results cannot be taken as the causal effect of market infrastructure,[40] but they provide direct evidence that the returns to public investment varies with the initial local conditions.

[40]For instance, villages with market facilities may have more entrepreneurial households or better networks of traders, which are likely to raise the rate of return yet not observed.

7 Conclusions

Access to credit and start-up funds has long been argued as an important means of escaping poverty. However, many studies report limitations of public credit programs because of the difficulties in preventing recipients from misappropriating or investing in unprofitable projects. This paper sheds light on this issue by providing new evidence on the effect of public grants for investment loans, using Indonesia's large-scale anti-poverty program, IDT. Under this program, the same value lump-sum grants were provided to targeted poor villages as a fund for business loans. Utilizing the identification strategy that exploits the variation in the value of per household grant, I have shown that the returns to the program capital vary depending on the initial conditions of villages where programs are placed.

The concentrated positive impacts are found on labor supply as well as household PCI and PCE for villages with pre-existing market facilities. On the other hand, isolated villages with no land access exhibit an increase in PCE (particularly on festivals in the early stages of the program period), without a significant change in labor supply or PCI. A possible explanation for this heterogeneity is that, on one hand, pre-existing market facilities complement the program capital by reducing transaction costs, thereby inducing investment. On the other hand, isolated villages might have a weak incentive to monitor participants as well as low expected returns from investment.

These results provide robust reduced-form evidence, which suggests that returns to investment are limited without a basic economic infrastructure. While investment capital could be utilized in villages with market facilities and access to outside the villages, other types of pro-

grams might be more suitable for less endowed villages. This is probably well understood by private banks, which tend to locate branches in areas with better infrastructure (Binswanger et al. (1993)). This case study provides one example where public resources are allocated with possibly too much emphasis on outreach and equity and too little attention to returns. Although the results do not compare the impact of IDT with any other programs, the negative returns in isolated villages are unlikely to justify the investment of public funds. The findings therefore in turn underscore the importance of tailoring public assistance according to the availability of local infrastructure and economic opportunities.

While the findings of this paper contributes to advancing our knowledge of heterogeneity in the returns to investment capital, they also give rise to further questions. First, the heterogeneity shown in this paper is likely to be larger than a variation in returns to bank/microcredit branches. This is because these branches come with staff and rules to screen loan applications, monitor investment, and enforce repayment, all of which are likely to reduce the heterogeneity in the returns to investment. Further evidence on heterogeneity in the returns to these branches is likely to shed more light on the interaction between local conditions and investment potential. Second, the absence of exogenous shocks that change the availability of economic infrastructure precludes the possibility of investigating the mechanisms through which local economic infrastructure (or unobserved factors associated with it) changes returns to investment capital. It is also outside the scope of the paper to address the question of what types of public assistance is effective in villages without a basic economic infrastructure. Addressing these questions is likely to be fruitful future research.

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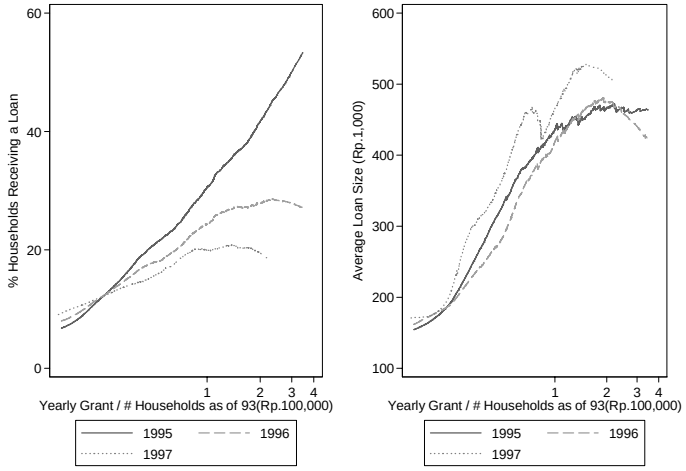
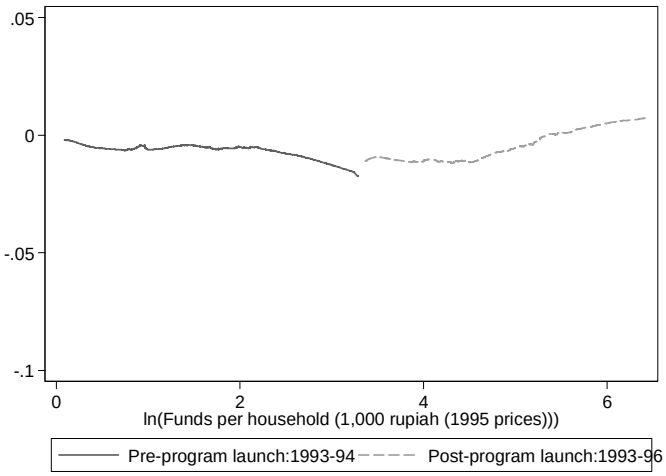
Figure 1: The Proportion of Participating Households, Average Loan Size, and the Value of Yearly Grant per Household in Rural Indonesia: 1995-1997



Sources: 1996-1997 SUSENAS, 1993 PODES, and IDT data.
Notes: The proportion of participating households is the number of households within a village that received a loan at least once in the specified year. The average loan size reflects the sum of all the loans for households that received more than one loan within the year. The values are in terms of 1995 prices. For simplicity, these participation measures are plotted against the fixed yearly grant of Rp.20 million per household. I use the cumulative value of grants per household received by the villages for the regression analysis. The nonparametric estimates are obtained using STATA's "lowess" procedure, with the bandwidth of 0.5, and the estimates except for the bottom and top 5 percentiles are depicted.

Figure 2: The Changes in the Proportion of Men Aged 20-60 Who Are at Work in Rural Indonesia: Before and After the Launch of IDT



Sources: 1993, 1994, and 1996 SUSENAS, 1993 PODES, and IDT data.
Notes: Since IDT started in 1995, the relationship for 1993-1994 indicates the changes before the introduction of the program. The relationship for 1993-1996 indicates the changes before and after the launch of the program. The value of funds per household is the cumulative amount of money received by the village (see section 4.1). The proportion of men who are at work is the village share of men who spent most of the time in the previous week on working for earnings (see Appendix 4).

Table 1[A]: Impact of per household grant value on the occupational and sectoral distribution of men aged 20-60 in rural Indonesia: 1994-1999

	The proportion of men are:								Hours of work	
		Engaged in agriculture				Engaged in non-agriculture				
	At work	Total	Self-employed	Wage worker	Unpaid family worker	Self-employed	Wage worker	Unpaid family worker	All	Conditional on being at work
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ln[Grants per household] *										
1{1994}	-0.005	-0.006	0.003	-0.003	-0.007	-0.002	0	0.002	-0.339	-0.263
	[0.005]	[0.007]	[0.009]	[0.004]	[0.005]	[0.004]	[0.004]	[0.001]*	[0.331]	[0.307]
1{1995}	0.004	0.016	0.005	0.005	0.006	-0.007	-0.008	0.003	0.158	-0.033
	[0.003]	[0.008]**	[0.010]	[0.005]	[0.008]	[0.005]	[0.006]	[0.002]**	[0.408]	[0.399]
1{1996}	0.008	0.026	0.023	0.009	-0.006	-0.005	-0.015	0.003	0.193	-0.115
	[0.003]**	[0.010]**	[0.011]**	[0.005]	[0.007]	[0.006]	[0.008]**	[0.001]**	[0.459]	[0.466]
1{1997}	0.007	0.038	0.02	0.013	0.005	-0.012	-0.022	0.001	-0.315	-0.561
	[0.004]*	[0.010]***	[0.012]	[0.005]***	[0.008]	[0.006]*	[0.008]***	[0.002]	[0.576]	[0.571]
1{1998}	0.004	0.018	0.03	0.005	-0.012	0	-0.017	0.003	-0.095	-0.27
	[0.005]	[0.011]*	[0.012]**	[0.006]	[0.008]	[0.006]	[0.008]**	[0.001]**	[0.574]	[0.558]
1{1999}	0.004	0.014	0.025	0	-0.008	-0.002	-0.008	0.002	-0.727	-0.957
	[0.004]	[0.013]	[0.014]*	[0.005]	[0.008]	[0.009]	[0.008]	[0.001]	[0.560]	[0.557]*
1{1994}	0	0.008	0.009	-0.002	0.003	0.005	-0.007	-0.004	0.77	0.834
	[0.006]	[0.012]	[0.014]	[0.008]	[0.008]	[0.007]	[0.008]	[0.002]	[0.596]	[0.584]
1{1995}	-0.02	-0.093	-0.036	-0.026	-0.029	0.047	0.04	-0.015	-0.184	0.597
	[0.013]	[0.036]***	[0.041]	[0.024]	[0.031]	[0.022]**	[0.026]	[0.007]**	[1.825]	[1.769]
1{1996}	-0.044	-0.157	-0.116	-0.049	0.01	0.047	0.08	-0.016	-2.229	-0.573
	[0.017]**	[0.050]***	[0.056]**	[0.028]*	[0.032]	[0.029]	[0.037]**	[0.007]**	[2.224]	[2.264]
1{1997}	-0.044	-0.211	-0.102	-0.072	-0.038	0.07	0.115	-0.008	-0.103	1.303
	[0.022]**	[0.054]***	[0.060]*	[0.027]***	[0.040]	[0.034]**	[0.042]***	[0.009]	[2.964]	[2.929]
1{1998}	-0.037	-0.098	-0.127	-0.024	0.025	-0.005	0.084	-0.019	-1.128	0.295
	[0.026]	[0.055]*	[0.062]**	[0.031]	[0.037]	[0.033]	[0.042]**	[0.008]**	[2.793]	[2.719]
1{1999}	-0.034	-0.068	-0.099	0.006	0.01	0.01	0.026	-0.009	1.655	3.122
	[0.017]*	[0.055]	[0.060]*	[0.026]	[0.033]	[0.037]	[0.036]	[0.007]	[2.472]	[2.469]
Constant	0.969	0.775	0.606	0.045	0.121	0.08	0.105	0.006	35.854	37.055
	[0.001]***	[0.004]***	[0.004]***	[0.002]***	[0.003]***	[0.002]***	[0.003]***	[0.001]***	[0.176]***	[0.178]***
Number of villages	5043	5043	5043	5043	5043	5043	5043	5043	5043	5043
Unique number of villages	1811	1811	1811	1811	1811	1811	1811	1811	1811	1811
F-stat	3.64	3.34	2.06	2.4	2.99	3.17	1.81	1.41	3.77	3.01

Sources: 1993-1999 SUSENAS, 1993 PODES, and IDT data.

Notes:

- Coefficients α_s and δ_s (s = 1994-1999) in Eq.(2) are shown.
- Robust p values in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.
- All the equations also include village fixed effects and the error term is allowed to be correlated across time within a village.
- See Appendix 4 for the definition of the outcome variables.

Table 1[B]: Difference in the coefficient of ln[Grants per household] * year dummy between 1994 and the subsequent years for men aged 20-60 in rural Indonesia: 1995-1999

	The proportion of men are:								Hours of work	
		Engaged in agriculture				Engaged in non-agriculture				
	At work	Total	Self-employed	Wage worker	Unpaid family worker	Self-employed	Wage worker	Unpaid family worker	All	Conditional on being at work
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Mean (1993)	0.97	0.76	0.60	0.06	0.10	0.10	0.10	0.01	37.52	36.39
SD (1993)	0.05	0.22	0.25	0.13	0.13	0.13	0.14	0.02	9.22	9.14
1995	0.009	0.022	0.002	0.008	0.012	-0.005	-0.008	0.001	0.497	0.23
	(0.05)	(0.02)	(0.88)	(0.16)	(0.10)	(0.34)	(0.20)	(0.49)	(0.30)	(0.60)
1996	0.012	0.032	0.02	0.012	0.001	-0.003	-0.016	0.001	0.532	0.148
	(0.03)	(0.01)	(0.13)	(0.03)	(0.92)	(0.62)	(0.05)	(0.55)	(0.31)	(0.77)
1997	0.012	0.044	0.016	0.016	0.012	-0.01	-0.022	-0.001	0.024	-0.298
	(0.04)	(0.00)	(0.22)	(0.00)	(0.16)	(0.14)	(0.01)	(0.49)	(0.97)	(0.61)
1998	0.009	0.024	0.027	0.008	-0.005	0.003	-0.017	0.001	0.244	-0.007
	(0.24)	(0.05)	(0.05)	(0.21)	(0.56)	(0.70)	(0.04)	(0.56)	(0.71)	(0.99)
1999	0.009	0.02	0.021	0.003	-0.001	0	-0.009	-0.001	-0.388	-0.694
	(0.15)	(0.14)	(0.13)	(0.62)	(0.89)	(0.98)	(0.30)	(0.71)	(0.53)	(0.23)

Sources: 1993-1999 SUSENAS, 1993 PODES, and IDT data.

Note:The difference in the coefficients δ_s (s = 1995-1999) and δ_{94} (estimated using Eq.(2)) is shown together with the p-value for the test of whether the difference is different from zero.

Table 2: Impact of per household grant value on real household per capita income in rural Indonesia: 1996

Monthly household per capita income	Total	Earned	Agriculture	Net transfer
(1,000 rupiah, 1995 prices)	(1)	(2)	(3)	(4)
Mean (1993)	51.91	20.75	18.05	0.54
SD (1993)	29.35	13.98	26.92	7.59
ln[Grants per household] * 1{1996}	-2.934	3.324	-5.114	-0.675
	[5.239]	[2.719]	[2.503]**	[1.828]
1{1996}	15.299	-21.134	27.831	4.739
	[23.579]	[13.887]	[11.293]**	[7.581]
Constant	52.265	17.432	21.983	0.417
	[2.101]***	[1.529]***	[1.181]***	[0.900]
Number of villages	94	94	94	94
Unique number of villages	47	47	47	47
F-stat	0.31	3	4.54	0.66

Sources: 1993-1999 SUSENAS, 1993 PODES, and IDT data.
Notes:
- Coefficients α_s and δ_s (s = 1996) in Eq.(1) are shown.
- Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.
- This set of estimation results relies on a smaller number of villages because the information on income is available for only a subgroup of households in the SUSENAS.
- Also, this information is available only in 1993 and 1996; therefore the trend in the pre-program period cannot be controlled.
- All the equations also include village fixed effects and the error term is allowed to be correlated across time within a village. See Appendix 5 for the definition of the outcome variables.

Table 3: Difference in the coefficient of ln[Grants per household] * year dummy between 1994 and the subsequent years for household PCE (rural Indonesia): 1995-1999

Monthly household per capita expenditure	Total	Food	Non-food	Housing	Education	Health	Clothing	Durables	Festivals
(1,000 rupiah, 1995 prices)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(10)
Mean (1993)	40.38	28.16	12.22	5.75	0.85	0.59	1.90	1.44	1.62
SD (1993)	14.92	10.70	6.67	3.40	1.20	0.97	1.29	2.24	2.46
1995	-0.303	-0.295	-0.008	0.125	-0.032	-0.091	0.034	-0.017	0.016
	(0.69)	(0.58)	(0.98)	(0.56)	(0.42)	(0.03)	(0.63)	(0.85)	(0.79)
1996	-0.274	0.023	-0.297	-0.15	-0.035	0.003	0.026	0.031	-0.188
	(0.78)	(0.97)	(0.53)	(0.49)	(0.30)	(0.93)	(0.77)	(0.76)	(0.49)
1997	1.172	0.577	0.595	0.247	-0.001	-0.092	0.11	-0.049	0.246
	(0.18)	(0.36)	(0.13)	(0.24)	(0.98)	(0.20)	(0.13)	(0.67)	(0.03)
1998	0.333	-0.153	0.486	0.302	-0.086	-0.089	0.191	-0.066	0.221
	(0.79)	(0.87)	(0.40)	(0.34)	(0.09)	(0.08)	(0.03)	(0.67)	(0.06)
1999	-1.532	-1.980	0.448	0.116	0.01	-0.039	0.061	0.243	0.048
	(0.10)	(0.01)	(0.28)	(0.57)	(0.78)	(0.42)	(0.49)	(0.20)	(0.62)

Sources: 1993-1999 SUSENAS, 1993 PODES, and IDT data.

Notes:

- The difference in the coefficients δ_s (s = 1995-1999) and δ_{94} (estimated using Eq.(2)) is shown together with the p-value for the test of whether the difference is different from zero.
- See Appendix 6 for the definition of the outcome variables.

Table 4 Heterogeneity in the effect of per household grant value by pre-existing village characteristics (rural Indonesia): 1995-1999

Outcome = Monthly household per capita expenditure (Rp.1000, 1995 prices)	Subsequent year t =				
	1995	1996	1997	1998	1999
	(1)	(2)	(3)	(4)	(5)
$(\delta_s - \delta_{94})$: Benchmark effect	-0.972	0.674	3.911	3.632	-1.997
	(0.59)	(0.75)	(0.12)	(0.23)	(0.45)
$(\theta_s - \theta_{94})$: Deviation from the benchmark for villages that:					
Had at least one cooperative as of 1993	1.947	0.407	-2.474	-2.34	-0.545
	(0.29)	(0.91)	(0.33)	(0.59)	(0.86)
Received at least one public credit program in 1992	2.157	-0.482	0.681	0.581	0.653
	(0.33)	(0.86)	(0.75)	(0.83)	(0.79)
Were accessible via sea or air, but not land as of 1993	-1.885	4.647	6.647	10.538	-1.037
	(0.42)	(0.10)	(0.04)	(0.03)	(0.74)
Had at least one agriculture-related market in 1993	-2.433	0.246	1.81	-1.725	3.379
	(0.24)	(0.94)	(0.40)	(0.55)	(0.30)
Had at least one market facility in 1993	1.661	8.013	3.686	7.902	4.055
	(0.51)	(0.02)	(0.16)	(0.14)	(0.24)
Had more than the median number of households in 1993	3.876	-5.062	-6.301	-6.247	0.772
	(0.30)	(0.23)	(0.10)	(0.17)	(0.87)

Sources: 1993-1999 SUSENAS, 1993 PODES, and IDT data.
Notes:
- The difference in the coefficients $\delta_s - \delta_{94}$ and $\theta_s - \theta_{94}$ (s = 1995-1999) are shown together with the p-value for the test of whether the difference is different from zero.
- Each of the estimates is obtained by estimating Eq.(3).

Table 5 Heterogeneity in the effect of per household grant value by pre-existing village characteristics (rural Indonesia): 1996

Outcome = Monthly household per capita income (Rp.1000, 1995 prices)	Total	Earned Income	Agricultural Income	Net Transfer
	(1)	(2)	(3)	(4)
ln[Grants per household] * 1{year = t}	37.157	17.937	-1.771	0.789
	[26.460]	[9.673]*	[7.550]	[10.878]
ln[Grants per household] * 1{year = t} *				
Had at least one cooperative as of 1993	11.512	9.293	13.959	-13.185
	[16.823]	[8.249]	[12.706]	[8.025]
Received at least one public credit program in 1992	1.451	-2.715	2.743	1.669
	[2.662]	[1.962]	[1.438]*	[1.223]
Were accessible via sea or air, but not land as of 1993	-10.549	-1.442	3.852	-8.362
	[14.519]	[5.953]	[4.143]	[5.110]
Had at least one agriculture-related market in 1993	-10.963	-12.133	5.255	-5.376
	[11.811]	[8.516]	[3.891]	[3.380]
Had at least one market facility in 1993	23.049	4.191	12.035	4.584
	[11.557]*	[5.855]	[5.831]**	[4.812]
Had more than the median number of households in 1993	-48.368	-3.042	-17.037	-3.005
	[28.200]*	[13.103]	[8.999]*	[11.071]
Number of Villages	94	94	94	94
Unique number of villages	47	47	47	47
F-stat	0.85	37.19	11.93	2.3

Sources: 1993-1999 SUSENAS, 1993 PODES, and IDT data.
Notes:
- The coefficients δ_{96} and θ_{96} are shown, where the model is $Y_{jt} = \alpha_0 + \alpha_{96}T_{96} + \beta_{96}[T_{96} * Z_j] + \delta_{96}[lnF_j * T_{96}] + \theta_{96}[lnF_j * T_{96} * Z_j] + \mu_j + u_{jt}$ (t = 1993 and 1996).
- Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6 Heterogeneity in the effect of per household grant value by pre-existing village characteristics (rural Indonesia): 1995-1999

Outcome = Hours of work among all men aged 20-60 years	Subsequent year t =				
	1995	1996	1997	1998	1999
	(1)	(2)	(3)	(4)	(5)
$(\delta_s - \delta_{94})$: Benchmark effect	0.235	3.413	1.172	1.422	0.428
	(0.83)	(0.02)	(0.50)	(0.51)	(0.82)
$(\theta_s - \theta_{94})$: Deviation from the benchmark for villages that:					
Had at least one cooperative as of 1993	1.404	2.25	1.365	1.079	-0.931
	(0.23)	(0.10)	(0.39)	(0.43)	(0.53)
Received at least one public credit program in 1992	-2.391	-0.036	0.291	-3.331	-0.29
	(0.13)	(0.98)	(0.82)	(0.02)	(0.84)
Were accessible via sea or air, but not land as of 1993	-0.98	-0.792	1.98	-4.605	0.865
	(0.44)	(0.58)	(0.26)	(0.02)	(0.59)
Had at least one agriculture-related market in 1993	-0.768	-0.564	0.468	0.677	0.959
	(0.57)	(0.73)	(0.78)	(0.70)	(0.59)
Had at least one market facility in 1993	1.839	4.52	3.574	0.97	2.62
	(0.18)	(0.00)	(0.02)	(0.53)	(0.08)
Had more than the median number of households in 1993	3.434	-4.609	-5.528	1.139	-1.527
	(0.13)	(0.05)	(0.02)	(0.68)	(0.57)

Sources: 1993-1999 SUSENAS, 1993 PODES, and IDT data.
Notes:
- The difference in the coefficients $\delta_s - \delta_{94}$ and $\theta_s - \theta_{94}$ (s = 1995-1999) are shown together with the p-value for the test of whether the difference is different from zero.
- Each of the estimates is obtained by estimating Eq.(3).

Table 7 Rate of return to per household grant value: 1995-1999

Outcome variable	Rate of return to per household grant (%)						
	Overall Average	Marginal change associated with the availability of pre-existing:					
		co-operative	public credit program	no land access	agricultural market	general market	less-than median village size
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1996 PCI	26.5	119.7	22.0	-40.8	-178.6	300.7	-1089.2
1996 PCI in agriculture	-113.9	145.1	41.6	14.9	85.6	157.0	-383.6
1997 PCE	1.6	-42.9	1.8	39.9	2.8	33.6	-115
1997 PCE on non-food items	1.4	-6.1	13.7	19	-4.3	1.5	-34.5
1997 Hours of work	-2.2	0.4	1.1	1.5	-0.8	2.8	-5.2

Sources: 1993-1999 SUSENAS, 1993 PODES, and IDT data.
Notes:
- Column 1 shows the mean of the rate of return based on the coefficients estimated using Eq.(3). Specifically, the rate is computed as $(12*[\delta_s+\theta_s*Z_j])/F_{jt}*100$, where δ_s is the coefficient of the per household grant value, θ_s is the coefficient of the per household grant value for villages that have characteristics Z_j. F_{jt} is the grant value. I multiply the coefficient by 12 to convert monthly PCI or PCE into an annual value. The coefficients for 1996 are used for PCI, and those for 1997 are used for PCE.
- Columns 2-7 depict the change in the rate associated with the change in one of Z_j, say, Z_j^k from $Z_j^k = 0$ to $Z_j^k = 1$, holding other factors constant. That is, it is computed as: $(12*\theta_s^k)/F_{jt}*100$. I report this marginal change in the rate of return evaluated at the average per household grant value among villages with $Z_j^k = 1$.

Appendices

Appendix 1: Infrastructure component of IDT

It is possible that the infrastructure grants given to a subset of villages increased wage employment and enhanced investments by raising the expected returns. However, empirical results suggest that, if anything, these effects are not driving the main findings of the paper. For example, in Java, villages with lower village scores were more likely to be selected for infrastructure grants - the value of such grants was the same across funded villages (The World Bank (1995) and The World Bank (1996)). However, when villages with lower village scores are allowed to have differential effects of per household grant value, no significant difference is found in the impact on PCI or PCE. In fact, only the effect on the proportion of men engaged in non-agricultural wage work is significantly larger in these villages in 1996, possibly reflecting the participation of men in the construction of infrastructure. In off-Java areas, villages with higher village scores were more likely to be selected for infrastructure grants, and they were given various values of funds depending on the needs of each village (Overseas Economic Cooperation Fund (1999) and Japan Bank for International Cooperation (2001)). The value of these grants can still be correlated with the per household value of the grant for loans if villages with fewer households were more likely to receive the infrastructure grants. However, the effect on PCE does not vary by village score. Also, though the effect on PCI is stronger in those villages with higher village scores, this income effect is driven by an increase in earned income. These results are different from the main findings for villages with market facilities, which is driven mainly by an increase in agricultural income. Thus, though there might have been a locality-specific additional effect of grants for infrastructure, it is unlikely to be the driving factor of the heterogeneity in the effect of per household value of grant for loans for the whole of Indonesia.

Appendix 2: The rules used to select poor villages under IDT (1994-1997)

In the first year, the government computed a village score based on measures of socio-economic infrastructure and living standards in the village. It also calculated two threshold values of the score to determine which villages were poor: a standard deviation (SD) indicator and a range indicator. The SD indicator is the provincial average minus one provincial standard deviation; the range indicator is the average minus the provincial range multiplied by 0.6. If the minimum and maximum of these two indicators are L and H and a village has a score below L, then the village is selected for a grant; if the village score is higher than H, then the village is not selected. If a score is between the two thresholds, the selection status of the village depends on the field officer's evaluation. That is, if the officer thinks that the village is poor, then the village is selected. Different sets of measures were used for urban and rural areas in computing the village score, and thus the selection of poor communities was also separately conducted (Badan Pusat Statistik (BPS) (1994)). There were few errors in this classification (Alatas (2000)).

In the second year, the government modified both the set of variables and the formula used to compute the village score. Also, neither the field officer's subjective evaluation nor the two threshold values were used any longer. Villages that were not funded in the first year, yet received relatively lower values for the new village score, were considered for funding from the second year. The number of additional poor villages was determined based on the budgetary constraint. Villages that were treated in the first year were dropped if they had fewer than 50 families based on concern about the inequality in the per household grant value (Badan Pusat Statistik (BPS) (1995)). The information on the selection rule for the third year is relatively limited. The PODES and the administrative dataset on IDT suggest that a similar rule was used to restrict very small villages from continued funding. In addition to her matching analysis that uses the first year rules, Alatas (2000) conducts another set of regression discontinuity analysis using the second year rules, which indicates varying effects for different provinces.

Appendix 3: Per household grant value (Rp.1000, 1995 prices)

Year	Villages	Mean	SD
1993	1811	7.0	11.4
1994	822	6.7	10
1995	499	103.7	161
1996	507	154.5	180
1997	509	167.9	132
1998	426	145.4	122
1999	469	94.3	86

Sources: 1993-1999 SUSENAS, 1993 PODES, and IDT data.
Notes: The value of per household grant is defined as the cumulative value of grants received by the village divided by the number of households as of 1993. If a village received a grant for a full three years, then the cumulative, nominal value is 20 million, 40 million, and 60 million rupiah for 1995, 1996, and 1997, respectively. The nominal value remains 60 million rupiah in the post-program period of 1998 and 1999. The value is defined as one thousand rupiah divided by the 1993 number of households in the pre-program period of 1993-1994. The number of villages is larger in 1993 because villages that were surveyed in 1993 and one of the later years are included for the analysis.

Appendix 4: The definition of labor supply variables

Individuals at Work	Individuals are defined to be at work if they either (1) spent the majority of their time in the previous week to earn income, (2) did not spend the majority of their time but spent at least one hour to earn income, or (3) did not spend even one hour to earn income, but were temporarily on vacation from regular work.
Agricultural Workers	Individuals who are at work (workers) report the sector and occupation of the job on which they spend most of the time in the previous week if they fall into category (1) or (2). Individuals who fall into category (3) answer about their usual jobs. If the sector of the main job is agriculture, it includes crop cultivation, animal husbandry, forestry, fishery, hunting, and agricultural services (development of soil, rental of farming machines, and health services for husbandry).
Self-Employed Workers	These workers operate agricultural and non-agricultural businesses and bear the risk of possibly volatile income. They could be working on their own (e.g., vendors at markets and train stations) or hiring family or non-permanent, casual workers (e.g., farmers who work on their land helped by household members and vendors helped by people who are given wages based on the days worked). Self-employed workers who hire permanent workers are categorized as employers, who are not included in this group nor separately examined as the number is very small.
Wage Workers	These workers are employed by private/government institutions and receive a salary or wages in cash or in goods. They could be permanent or casual workers and engaged in agricultural or non-agricultural sector. Unpaid Family Workers These are not individuals who take care of household tasks such as cooking, cleaning and child-rearing; rather, these workers assist household members, relatives, or neighbors, without receiving salary or wages. Examples include individuals who help their parents or spouses in the fields and who help neighbors running a small store without receiving wages.
Unpaid Family Workers	These are not individuals who take care of household tasks such as cooking, cleaning and child-rearing; rather, these workers assist household members, relatives, or neighbors, without receiving salary or wages. Examples include individuals who help their parents or spouses in the fields and who help neighbors running a small store without receiving wages.

Appendix 5: The definition of household income

All these income variables include households that report no income from a specific category.

Earned income	Salary received from the main and side jobs, and includes fringe benefit and salary in kind. The values are in terms of 1995 prices.
Agricultural income	Income accrued through the production of food and non-food crops as well as from subsectors such as animal husbandry, poultry, fishery, and forestry. Further disaggregated items can be examined in the 1993 data but not in the 1996 data. In terms of 1995 prices.
Net transfer	Inflow minus outflow of income associated with (a) gift-giving and (b) asset / financial transactions. Gifts include grant and inheritance (inflow) and contributions and dispersed gifts (outflow). Asset transactions refer to the selling and purchasing of valuables and real estate. Financial transactions include withdrawn deposit and received loans as well as loan repayments and making a deposit. Though IDT loans and repayments are likely to be included in this item, no association is found between net transfer and per household grant value (Column 4, Table 2). This might be an indication that transfers were not accurately reported. Alternatively, it might be an indication that, in smaller villages, IDT funds are more likely to be used collectively by *pokmas* without being distributed to individual members.[41] In terms of 1995 prices.

Appendix 6: Definition of household expenditure

Food expenditure	This does not include items for household businesses, and refers only to what is consumed by household members. The values are expressed in terms of 1995 prices. Monthly food expenditure is defined as the expenditure in the previous week * 30 / 7.
Non-food expenditure	This also includes only items that serve for the use of household members. Monthly non-food expenditure is defined as the expenditure in the previous 12 months divided by 12. The values are in terms of 1995 prices. Spending for housing includes repairs, maintenance, and utilities. Festival-related outlay includes expenses for wedding parties, religious rituals, and other celebrations. It is possible that some investments in businesses that use the residence for production or transactions (for instance, households that sell snacks using part of their residence as a store) may have been counted as household expenditure.

Appendix 7: The impact on savings and livestock

The other possible form of welfare-improvement induced by IDT is increased savings. However, the 1993-1996 change in the average amount of out-disbursement, which includes outflow of money associated with asset / financial transactions, does not indicate any significant difference between villages with different values of per household grants. Another possible form of savings and a source of consumption smoothing is livestock (Rosenzweig and Wolpin (1993)). However, the lack of data on livestock before the program introduction precludes an analysis that is comparable to the analysis for employment, income and consumption outcomes. Available evidence suggests that a larger number of pigs per household was sold in villages with a larger per household grant between the beginning of 1997 (the last year of the program period) and that of 1998 (Appendix Table 3). No significant difference is found in the pattern of transactions of cows, buffalos, horses, goats, sheep, chickens and ducks. It is possible that pigs were purchased during the program period, and then sold later during the currency crisis (between January of 1997 and January of 1998) to shelter household expenditure from a possible loss in income. However, the pattern of transactions during the program period cannot be tested. Also, without the knowledge of whether and how much the size of the income loss that would have been realized without any response differed between small and large villages, it is difficult to assess whether the loss is associated with the sales of pigs.

Appendix 8[A]: Definition and summary statistics for village characteristics

Availability of sources of credit other than IDT	Two dummy variables are used. One indicates a village where at least one cooperative was operating as of 1993; the other indicates a village that received some public credit programs in 1992. Public credit programs include Farmers Enterprise Credit, Community Sugar Cane Intensification Credit (TRI), Candak kulak credit, and other credit provided by foundations with a legal status. These do not include usurers.
General / agricultural market facility	Two dummy variables are used indicating a village that had at least one general-purpose market facility and at least one facility related to agricultural production. A general-purpose market includes a permanent or semi permanent building that is used as a market, a non-permanent building made of materials such as bamboo and leaves, and a group of at least 10 shops. An agriculture-related market facility includes a production tools kiosk where agricultural inputs such as fertilizer and seedlings are sold, warehouses where agricultural products are stored, and a fish market.
Transportation / access	A dummy variable indicating a village with no land access is used.
Village size	A dummy variable is included indicating a village where the number of households as of 1993 was greater than the median.
Other village characteristics	The following pre-existing characteristics have been tested for a possible heterogeneous effect of the program; however, they do not show any evidence for heterogeneity, thus are not included. The first is the characteristics of the village head, that is, whether the village head was older than the median age and whether the head completed secondary education. The second is the educational attainment of the population, as measured by the proportion of adults aged 20-60 who completed secondary education and the gender gap in the proportion. Third, the existence of a bank (one type of credit sources) does not show systematic evidence for heterogeneity in the program effects. Fourth, additional information on road conditions does not indicate heterogeneous effects such as dummy variables distinguishing, among villages with land access, places where inter-village roads are hardened, places where inter-village roads are made of asphalt, and places where inter-village roads are soil or pebbles.

Appendix 8[B]: Summary statistics for the village characteristics included in the analysis

Village Characteristics	Mean	SD
1{Village had at least one cooperative as of 1993}	0.108	0.311
1{Village received at least one public credit program in 1992}	0.360	0.480
1{Village was accessible via sea or air as of 1993}	0.130	0.337
1{Village had at least one market facility as of 1993}	0.149	0.356
1{Village had at least one agriculture-related market as of 1993}	0.260	0.439
1{Village size was less than the median as of 1993}	0.506	0.500
Number of villages	1811	

Sources: 1993 SUSENAS, 1993 PODES, and IDT data.