Does supplementary food program help children during the crisis?

Evidence from Indonesian Panel Data

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Abstract

This study evaluates the effectiveness of supplementary food program that was aimed to help the children to maintain their health when facing the 1997/98 economic crisis in Indonesia. To do so we apply difference-in-difference method for two different kinds of sample: unmatched and matched one. The results from unmatched (using pooled OLS and fixed-effect) and matched (average treatment effect based on propensity scores) tend to be consistent: during 1997/98 crisis, children who were exposed to the program have better nutritional status relative to those who were not. Yet with matching sample we manage to produce higher estimated program effect on nutritional status of the treated children. This result may suggest that the use of matched sample may even further eliminate the contamination of the program effect from unobserved heterogeneity. In addition, the results also raise the importance of properly pick the control groups for the treatment when it comes to the evaluation of public program using survey data.

1. Introduction

Studies show that economic crisis generally produce adverse impact on child and household welfare in developing countries. For example, looking at the impact of the 1988-1992 economic crisis in Peru, Paxson and Schady (2004) find evidence of an increase in infant mortality rate for children born during the crisis period. In addition, they also find that children who were exposed to the crisis were shorter than their age-cohort in 1996 and 2000. However, some studies focusing on Indonesia during 1997/98 East Asian financial crisis show different outcomes. For instance, using a nationally weighted representative sample, Strauss et al (2004) indicate that, instead of worsening, child health-for-age and weight-for-height in Indonesia appeared to have slightly improved during the period, 1997-2000. Block et al (2004), using a sample from rural Java, also indicate that child weight-for-age remained constant throughout the crisis.

Unfortunately there are not many studies have attempted to empirically investigate what explanations behind those ‘unexpected’ findings. One looks how mothers behave as nutrition buffer for their children when facing economic shocks resulting on increasing maternal waste in poorer household (see Block et al, 2004). But very few that have tried to assess the effectiveness of government program during crisis period using national level data.

This study seeks to evaluate the impact of the food supplementation program on child nutritional status in Indonesia which was launched to help maintaining child health during the 1997/98 economic crisis. It utilizes the two sets panel data of Indonesia Family Life Survey, 1997 and 2000, that cover periods before and after the crisis. This study designs a quasi-experimental setting and adopts difference-in-difference method where we seek to compare child nutritional status of the treatment and control groups. In addition, we adopt method of average treatment effect based on propensity scores to create a comparable treatment and control groups to deal with concern on bias that was potentially generated by ‘regular’ difference-in-difference.

2. Background: Crisis and Food Supplementary Program

Indonesia was firstly hit by the Asian economic crisis on mid 1997, but the major impacts had not been felt until January 1998. Afterward, the social and economic achievement in the last two decades appeared to shrink. Prior to the crisis, Indonesia socioeconomic development performance was one of the success stories in East Asia. In 1993 World Bank grouped Indonesia as one of the East Asian Miracle. The GDP per capita sharply increased from end of 1960s to mid of 1990s. It changed the Indonesian economic status, from one in low-income group to be the one of the middle income countries. In addition, the (headcount) poverty rate decreased from about 40 percent to about 18 percent in 1996. The other social indicators like fertility and infant mortality reflect similar success story (World Bank, 1993).

The 1997/98 Asian financial crisis quickly changed the face of Indonesia economy. It worsened the Indonesian Rupiah exchange rate from a level of Rp 2,500 in July 1997 to Rp 15,000 in July 1998. It is then followed by the dramatic increase in prices (particularly food price), causing the subsequent collapses of the domestic economy. The social impact of the crisis has been substantial, though some studies differ in the magnitude. Thomas et al (1999) report that the proportion of household lives below poverty line has risen by about 25%.

Education is also one of the sector where the effect of the crisis highly discernible. Frankenberg et al (1999) find that between 1997 and 1998, the percentage of 13-19 years old that were not enrolled in school has increased, in which percentage increase is observable more in urban rather than rural. Younger children were also less likely to be enrolled in school during 1998, especially those from poorer households (Frankenberg et al, 1999).

To mitigate the negative socioeconomic impact of the crisis, Indonesia government launched Social Safety Net (*Jaring Pengaman Sosial*, JPS). The JPS programs cover several areas including: food security, employment creation, education and health[1]. The supplementary food program (*Pemberian Makanan Tambahan*), under the area of JPS health, aimed to improve the nutritional status of targeted individuals particularly children in preschool age. The program was in fact an expansion from the one that had existed since before the 1997/98 crisis. We later on refer this post-crisis supplementary food program as ‘expanded program’.

The pre-crisis government supplementary food program (later on referred to ‘regular program’) was solely distributed through village integrated health post (*Pos Pelayanan Kesehatan Terpadu*, Posyandu).[2] Posyandu typically provided child supplementary food program –along with other health services for children and pregnant women (mother)– once a month. It was monitored by sub-district public health clinic and funded by federal/local government budget as well as by a contribution from local community. It is usually run by local (women) activists.

In contrast, expanded program was distributed through some different providers such as village midwife, family welfare program (*Program Kesejahteraan Keluarga*, PKK), and some others. Thus while posyandu continued to run the regular supplementary food program once a month in a community after the crisis began, that community might also be benefited from expanded program provided by other providers. That explained the variability in number of program provisions and heterogeneity in quality of services within and across community after 1997. In addition, there are some other distinctions between the two programs. First, while the program that was distributed through posyandu –particularly before the crisis— tends to be ‘universally-targeted’ program, the ‘expanded’ program under JPS health after 1997 was targeted more to the poor areas. Some local authorities designed the criteria and then selected program receivers based on

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[1] Food security includes sales of subsidized rice, employment creation area covers labor intensive program and village block grant, education consists of scholarship to targeted child and school block grant, and health area contains medical services, family planning, food supplementation and midwife services.

it. Second, posyandu in some communities may charge the users of the services it provided including for supplementary food program, while expanded program was provided free of charge.

3. Review of Literature

Early literatures on the impact evaluation of social program and public services particularly that addressed child health and nutritional status have found some mixed evidence. For instance, while Rosenzweig and Schultz (1982), Thomas, Strauss and Henriques (1991), Thomas, Lavy and Strauss (1992) find positive association between the availability of health infrastructure and child health indicators, some other studies like Rosenzweig and Wolpin (1982) and Hosain (1989) suggest that health infrastructures are negatively associated with child health outcomes [See Strauss and Thomas (1995) for further review on these studies].

These mixed results from the early studies may partly be explained by the fact that most of those studies assumed that there exists no correlation between program availability and unobserved heterogeneity that might rule the program placement. Rosenzweig and Wolpin (1986) show that some programs placement may be ruled by some (unobserved) criteria that were correlated with the outcomes being studied. In this case, the negative association between public health infrastructure and child health outcomes, thus, may reflect more on successful targeting. On the other hand, ignoring this program placement issue will likely produce bias in the program's effect estimator.

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[2] IFLS data reveals that Posyandu has existed in some communities and run the public health programs since late 1980s.

Some more recent studies on this topic apparently have taken into account the endogenous program placement issue. With the advantage of the access to panel data, they designed a quasi-natural experiment setting and compare the parameters of interest between the so-called treatment and control groups using difference-in-difference method. This procedure may not clean all the potential sources of bias, yet it clearly eliminates the main one --time-invariant unobserved heterogeneity-- that may affect such program availability. Some of them (see Pitt, Rosenzweig and Gibbons 1993, Frankenberg 1995, Frankenberg and Thomas 2001, Chaudhury 2003, Quisumbing 2003, Behrman et al 2004, Frankenberg et al 2005, Alderman et al 2005), generally find positive estimated program effect from program participation or exposure.

However, growing literature on program evaluation points to two potentially important sources of bias when adopting difference-in-difference method to evaluate program effect using non-experimental/survey data [see Ravallion (2005) for literature review on program evaluation]. First, difference-in-difference method, particularly when applied using non-experimental data, does not fully guarantee that the comparability between the characteristics of 'treatment' and 'control' groups. If the two groups in fact have 'large' differences in characteristics, then the comparison between the two groups may not be unbiased. Here, isolating the treatment and control groups within the same geographical or any administrative area may not be sufficient to eliminate the bias. Second, difference-in-difference generates biased impact estimates if the expected outcome changes are a function of initial conditions that also influence the categorizing of treatment and control groups.

4. Conceptual Framework

A simple framework can be illustrated to model the differential impact of heterogeneous program intensity and quality on child nutritional status. Following some previous works like Pitt and Rosenzweig (1984) and Thomas, Lavy and Strauss (1996), we extend the conventional single-person agricultural household model (Singh, Squire and Strauss, 1984) by incorporating a health production sector and multiple household members and apply for the general household model. The model assumes that preferences of household members are inter-temporally separable and in current period it maximizes a quasi-concave utility function over some goods, services and (child) health/nutritional status:

$$\max_{x,h} U(x_t, l_t, h_t, z_{ht}, \mu_t) \qquad (1)$$

where **x** is n x T vector of consumptions of goods and services, while ***l*** and ***h*** are, respectively, 1xT vectors of leisure, and health/nutritional status. $\boldsymbol{z_{ht}}$ is a vector of household characteristic and μ is unobserved heterogeneity in preferences. The maximization problem is subject to current period time and budget constraint and a health production technology. The budget constraint defines that total consumption of goods and services can not exceed total of labor and non-labor income:

$$\mathbf{p_t x_t} = \mathbf{w}_t (T_t - l_t) + y_t \qquad (2)$$

where **p** is 1xn vector of prices, ***w*** is JxT vector of wages for each household member, ***T*** is vectors of maximum hours each of household member can work, while y is household non-labor income. While technological constraint is characterized by following health production function (for each household member including children):

$$h_{it} = f_t(h_{it-1}, N_{it}, z_{it}, z_{ht}, z_{ct}, v_{it}) \qquad (3)$$

in which, household member health is a function of previous period health/nutritional status, h_{-1}, individual health inputs vector $\boldsymbol{N}_{it}$, vector of individual characteristics $\boldsymbol{z}_{it}$, vector of household characteristics $\boldsymbol{z}_{ht}$ and vector of community characteristics, $\boldsymbol{z}_{ct}$. The central in this model is that current availability of the nutritional program may enter this production function through health inputs. So health inputs, ***N***, which is a function of nutritional program that may include health information, vitamins, vaccines, etc, can be analytically written as: $N_{it} = N(PROG_{ct})$. Then solving optimization problem in equation (1) conditional on equation (2) and (3) and taking into account health inputs function yields a reduced-form for household member health outcomes function for each individual, including children, within the household:

$$h_{it} = h(p, y, z_i, z_h, z_c, PROG_{ct}, \xi_{it}) \qquad (4)$$

5. Identification Strategy and Empirical Specifications

We will estimate the reduced-form of child nutritional status (equation 4) for each age group in two different years to measure the effect of supplementary food program on nutritional status of children between 6 and 48 months.[3] In doing so we will compare the nutritional outcomes of children between 6 and 48 months olds who were exposed to the program with older children who were not (suppose to be) exposed to the program but residing in the same community. The idea behind this strategy is because we expect that children between 6 and 48 months in 2000 were fully exposed to the expanded program so they would be benefited most. Therefore we will estimate the following equation:

[3] The estimation of reduced-form relationship provides a consistent framework within which to examine the impact of changes in market prices, endowment and policies on the health- and nutrition-related consumption of different type of individuals (Behrman and Deolalikar, 1988).

$$h_{ivt} = \alpha + \beta PMT_v * I48_t + \gamma' X_{ivt} + \mu_v + \xi_{ivt} \qquad (5)$$

where h_{it} is nutritional outcomes for each child *i* in community *v* and for time-specific *t* [t=0 (1997), 1(2000)], PMT_v is indicator whether community gained the supplementary food program in 1997 (regular) and 2000 (regular and expanded), and $I48_t$ is binary indicator for group of children between 6 and 48 months in 1997 and 2000. We also control some (exogenous) individual, household as well as community time-specific characteristics to increase the efficiency of the estimation. The rest of the covariates are μ_v which is community fixed-effect that control for constant unobserved heterogeneity in community level that may correlate with program placement, and ξ_{ivt} that is time-varying unobserved heterogeneity. The interaction between PMT_v and $I48_t$ in this specification thus defines our treatment groups: children between 6 and 48 months old who were exposed to the program. Accordingly parameter β represents the effect of the program on nutritional status of the children in that age group in 1997 and 2000.

However β in equation (5) capture both regular and expanded supplementary food program as implied by the way we define PMT_v variable. Our main goal here is also in fact to identify separately the effect of post-crisis (expanded) program. To do so, we divide our treatment group in equation (5) into two: (i) children between 6 and 48 months old in 1997 who were exposed to the regular program, and (ii) children from the same age group who were exposed to both regular and expanded program in 2000. With this setting, we will be able to identify the net effect of post-crisis (expanded) supplementary food program on targeted child's nutritional status. We thus modify equation (5) into:

$$h_{ivt} = \alpha + \beta_0 PMT_v * I48_{i0} + \beta_0 PMT_v * I48_{i1} + \gamma' X_{ivt} + \mu_v + \xi_{ivt} \qquad (6)$$

We will estimate equation (6) for all sampled children (up to 108 months). $I48_{i0}$ and $I48_{i1}$ are respectively group of children who were between 6 and 48 months old in 1997 and 2000. Accordingly β_{i0} represents the 'regular' program's effect on nutritional status of children who were between 6-48 months in 1997, while β_{i1} reflects both 'regular' and 'expanded' programs' effect on the same age group of children in 2000. The fact that the 'expanded' program was started only after the crisis implies that the program has no effect to any targeted children (6-48 months old) in 1997. This fact along with our specification thus allows us to correctly measure the expanded supplementary food program effect as β_{i1}-β_{i0}.

There is however one potential problem: non-comparability between the so-called treatment and control group. In our case that appears when the treatment group (children in age group of 6-48 months in 2000) and its controls have large different characteristics. This occurs since we could not ensure randomness when evaluating public program using non-experimental data. Ignoring this issue may lead to the bias in program effect estimator.

In order to deal with this potential problem, our strategy is to combine propensity score matching and difference-in-difference methods. We adopt method of average treatment effect for the treated (ATT) based on propensity scores and use STATA program as composed by Becker and Ichino (2002). Ravallion (2005) asserts that combining the two is the most flexible method of cleaning out initial heterogeneity prior to differencing. Our procedure is to use propensity score in selecting a comparable control group of children (non-participants) for the treatment group (participants) which thus will form more matched sample.[4] This is done basically by pooling all households where the all sampled children reside and running a probit regression of whether they were exposed to the program in both years on all possible household characteristics. We then re-estimate equation 6 using this matched (and usually smaller) sample of children.

6. Data

We use the data from two waves of Indonesia family life survey (IFLS), 1997 and 2000, which covers period before and after the crisis. During the 2000 survey, about 43,000 individuals in 10,450 households, distributed in 313 villages, were interviewed. One advantage of this survey is the unique structure of the survey that link household and village-facilities surveys enabling us to control for many household as well as village characteristics. Another advantage of using this panel data is the non-substantial attrition rate. Thomas and Frankenberg (2000) show that attrition rate in the 1997 wave in comparison to the baseline survey in 1993 is 6%, while Strauss et al (2004) indicate that the rate in 2000 is only about 5 percent. These are very low rates implying that we may not to worry about the estimation problem created by the attrition.

Our sample here is all children aged up to 8 years in 1997 and 2000. We define program exposed children as those who were in between 6 and 48 months and live in community with program in 1997 and 2000. As explained before, the programs consist of two parts: regular (started on pre-crisis and continue after the crisis) and expanded (start right after the crisis). Table 1 below show the proportion of the children between 6 and

[4] For further discussion on Average Treatment Effect and Propensity Score Matching see Wooldridge (2002), while for the empirical example of combining propensity scores and difference-in-difference please see, one of them, Ravallion and Chen (2005).

49 months who were exposed to both programs in 1997 and 2000 is almost 30 percent of the total sample. If we break that number into those who were expose to the both program in 2000 and to only the regular in 1997, the proportion for each group are about 18 and 12 percent.

As a proxy for child nutrition status, we use anthropometric indicators, particularly, child height-for-age. This measure has been suggested as less problematic indicators of health compared to the other health measures (see Strauss and Thomas (1995) for a discussion on various health measures)[5]. Child height-for-age reflects all health events and inputs since birth which may not be sensitive to abrupt shocks yet may well respond to these shocks over time. The average of the child-for-age z-score for our sample is -1.65.[6]

Furthermore, it is important to notice that the program is not the only path that affects the child nutritional status. Some individuals, household as well as community characteristics may also determine child nutritional status. Therefore we control some covariates that likely affect child health. In individual and household level, we control for some covariates that may reflect child health endowment (father and mother's height), access to affordable health services and household health environment (access to septic-tanked toilet and closed (piped) water sources) in addition to some typical household demographic characteristics. Table 1 displays mean and standard deviation for those variables.

[5] In IFLS, height is measured by trained health workers with regularly calibrated health equipments. Accordingly we believe that measurement error is negligible in this case.
[6] For detail comparison and discussion about child health in Indonesia using anthropometric measures in 1997 and 2000 see Strauss et al (2004).

7. Results and Analysis

We divide this part into two sections. First section discusses the program effect resulted from employing pooling OLS and fixed-effect using unmatched sample. Then in second section we employ propensity score to create a more comparable (matched) sample of treatment and control group, and calculate and discuss the program effect on child's nutritional status using average treatment effect method.

In all models we use similar covariates as we discussed in detail previously. All covariates, particularly those that significant for child health, appear –as we expected— to have positive sign. The only puzzle is for mother height where it appears to influence child nutritional status strongly yet in the opposite direction. We tried some different specifications, but those do not change the results much.

7.1. Unmatched sample

Table 2 contains the regression result of equation 5 and 6 using unmatched sample. Model 1 and model 2 utilize pooling OLS while model 3 and 4 use fixed-effect estimation. Although all models consistently show the positive impact of the supplementary food program in nutritional status of children between 6 and 48 months, the magnitudes of the program effects are different.

Model 1 shows that children between 6 and 48 months who were exposed to both program in 1997 and 2000 have better nutritional status than their control --groups who were not exposed to the program. The total estimated program effect is 0.46. This is relatively big impact for child nutritional status particularly when the average of z-score height for all sampled children is -1.65. One should notice though here that we compare the treatment (children between 6 and 48 months in 1997 and 2000) with all other children regardless the community where they reside. In this case, we may contrast the treatment children with those who live in different village that never been exposed to the program. In this case, the unobserved heterogeneity, particularly in village level, may contaminate the difference in estimated program effect.

Using fixed effect in model 3, the program effect magnitude decreases slightly – estimated program effect is 0.43. This slight change is likely due to applying village fixed-effect. By doing so, we limit the comparison only between treated and control children who live in the same village and thus control the unobserved heterogeneity in village level. Consequently we believe that model 3 produces relatively more accurate program effect than model 1 does.

While model 1 and 3 show the impact of both regular and expanded supplementary food program, our main goal in this paper is also to identify the impact of expanded program on child nutritional status. As discussed before, we then divide the treatment group in model 1 and 3 into two: children who were exposed only to regular program in 1997 and those who were exposed to both programs in 2000. Using this strategy we thus are able to separate the net effect of expanded supplementary food program that is launched particularly to cushion the negative impact of the crisis on child health from regular program that have been running prior to the crisis.

We design model 2 and 4 to capture the net effect of the expanded program on child nutritional status. Similarly model 2 and 4 show consistent results but are slightly different in program effect magnitude. Employing pooled OLS, model 2 produces positive program effect for nutritional status of children between 6 and 48 months who were exposed in 1997 and 2000. The estimated effects of the program for the children who were exposed to both program in 2000 is higher than the same age group of children who were exposed only to regular program in 1997. The estimated program effect for each groups are 0.57 and 0.31. Therefore the estimated net effect of expanded food supplementary program on nutritional status of children who were between 6 and 48 months and exposed to the program in 2000 is 0.266.

Model 4 use village fixed effect to estimate the program effect on children in age between 6 and 48 months who were exposed to program in 1997 and 2000. It produces positive program effect on nutritional status of treated children in 1997 and 2000. Consistent with model 2, the estimated program effect for treated children in 2000 is higher than those who were in 1997 (0.54 compared to 0.27). This is justifiable since treated children in 2000 were exposed to both regular and expanded program, while those in 1997 were only exposed to regular one. To calculate net expanded program effect for treated children in 2000, we just need to difference the two estimated program effects in model 4. Therefore the estimated effect of ‘emergency’ supplementary food program for treated children in 2000 is 0.271.

So far we have shown that different models and specifications have consistently produced positive effect on nutritional status of children who were exposed to the program in 1997 and 2000. Yet these results also suggest that program placement effect may not take place. If this is true, there are two possible explanations. First, related to empirical specifications, all controls may capture all program placement effect. Second – this is more likely to occur— the program, particularly the regular one, appears to be a ‘universally targeted’ one. In addition, although the expanded/emergency one was designed to be targeted to poor areas, our data show that since 1998 the expanded program has even reached more than 80 percent communities in our sampled areas.

7.2. Matched sample

In contrast to the previous section, here we will create a new sample in which the treatment and control group have more comparable (matched) characteristics before evaluating the net effect of expanded supplementary food program. We use STATA program provided by Becker and Munich (2002) to estimate the average treatment effect of treated based on propensity scores. The method starts by pooling all households where the sampled children reside and estimate Probit model of whether children exposed to the expanded program on pre-intervention (1997) household characteristics. We use all households and village characteristics in model 1-4 in previous section. One should notice here that we do not place causal interpretation in this Probit model but instead only an association. Table 3 displays the results of this Probit model estimation.

Based on this estimation, the program will estimate the propensity score and provide a range of common support and then identify number of blocks where the mean propensity score for treated and controls is not different. Then the program will test the balancing property and in our case end up with 6 blocks of propensity scores with intervals of roughly 0.05 propensity score units. We then test the equality of the mean value of treated and controls and could not reject the equality of all observables in all cases.[7]

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[7] See Becker and Ichino (2002) for detail procedures of program in estimating average treatment effect using propensity scores.

In conducting average treatment effect for treated children we use three different methods: two from neighbor matching (random draw and equal weight) and one from radius matching. The first two will pick the controls for treated from the nearest neighbor (in term of propensity scores). The difference between the two is while the first method will randomly draw in picking the controls, the later will put equal weight on the backward and forward matches. Radius matching instead will pick the controls with difference in estimated propensity scores between treated and controls falling within any certain radius (see Becker and Ichino, 2002).

The result of average treatment effect for the treated (ATT) children is displayed in table 4. The way we define treatment should allow us to compare the ATT result with results from model 2 and 4. With all three average treatment methods, we consistently produce higher effect on nutritional status of children between 6 and 48 months who were exposed to the expanded program in 2000. Two neighbor matching methods produce higher program effect than radius matching method --0.495 and 0.513 compared to 0.413.

One should notice that in calculating average treatment on treated here we use smaller but matched sample. With this method, the unobserved heterogeneity that may contaminate the estimated program effect may even be further eliminated. In this matched sample thus, the outcome of interest of treated children from poor household for example will be compared with those of the controls from poor household too. This likely explains why when using matched sample, the program effect for treated children are higher than using unmatched sample.

8. Summary and Conclusions

In this paper we evaluate supplementary food program that was launched to soften the negative impact of 1997/98 crisis on child health/nutritional status in Indonesia. We use data from Indonesia Family Life Survey from waves that covers period prior to and after the crisis. We design the quasi-experimental setting with this data where we seek difference between treatment and control group using unmatched and matched sample.

Using unmatched sample, we apply pooled OLS and village fixed-effect. The results from OLS and fixed-effect consistently show positive total estimated program effect on child nutritional status. Similar effect can also be seen for nutritional program that only exist after the crisis begin. With these results, we conclude that government food supplementary program appears to be successful in maintaining child nutritional status in Indonesia during the 1997/98 crisis.

In contrast with theory though, these results may also suggest that program placement issue may not take place. But the nature of the programs –both are ‘almost’ universally targeted program—may explain why the program placement effect was not there. Future studies may want to test directly whether program placement effect matter as well as investigate the determinant of program targeting in village level.

Finally, using matched sample produced by propensity score method, we find that the program effect, particularly the expanded one is also positive and higher compared to when we do not. This result may suggest that using matched (though smaller) sample may eliminate the contamination on the program effect from unobserved heterogeneity. In addition the results also raise the importance of properly pick the control groups for the treatment when using survey data to evaluate the effectiveness of public program.

Table 1. Descriptive Statistics

Variable	Mean	Std. Dev.
Child Health		
height-for-age (z-score)	-1.65	1.37
Program Exposure on treatment		
Both program in 1997 & 2000 (dummy)	.297	.457
Both program in 2000 (dummy)	.176	.381
Regular program in 1997 (dummy)	.121	.326
Health endowment & environment		
Father's height (cm)	126.5	16.6
Mother height (cm)	121.8	17.2
HH access to health card (dummy)	0.16	0.37
HH access to closed-source water (dummy)	0.85	0.36
Access to private and septic-tanked toilet (dummy)	0.59	0.49
Household characteristics		
Household head is male (dummy)	0.88	0.32
Mother's education (years)	.869	1.872
# of male adults	1.39	0.92
Household access to electricity	0.87	0.34
Per-capita expenditure (real, ln)	11.92	0.72
Household main business is farm (dummy)	0.41	0.49
Head of household main activity (categorical)	1.52	1.36
Village characteristics		
Distance to district capital (km)	22.13	29.38
Village is urban (dummy)	0.44	0.50

Source: Author's calculation

Table 2. Estimates of the effect of presence of suplementary food program on child height-for-age z-scores, Indonesia 1997-2000 (Unmatched sampled)

Panel A	**Pooled OLS**		**Fixed-effect**	
	Model 1	**Model 2**	**Model 3**	**Model 4**
Children exposed to all program	0.461**		0.432**	
	(0.036)		(0.045)	
Children exposed to expanded program	--	0.572**	--	0.543**
		(0.045)		(0.059)
Children exposed to regular program	--	0.306**	--	0.268**
		(0.053)		(0.065)
Mother's education	0.014*	0.013	0.013	0.012
	(.008)	(.008)	0.009	0.009
Mother's height	-0.013**	-0.017**	-0.013**	-0.016**
	(0.002)	(.002)	(0.002)	(0.002)
Father's height	0.008**	0.012**	0.008**	0.012**
	(0.002)	(0.002)	(0.002)	(0.002)
Male household head	-0.068	-0.069	-0.017	-0.017
	(0.061)	(0.061)	0.069	(0.069)
# of male adults	0.033*	0.033**	0.022	0.022
	(0.017)	(0.017)	0.017	(0.017)
Percapita expenditure (ln)	0.226**	0.230**	0.204**	0.208**
	(0.022)	(.022)	(0.023)	(0.023)
Access to free health services	0.131**	0.128	0.098**	0.094**
	(0.039)	(0.039)	(0.044)	(0.044)
Access to electricity	0.032	0.033	0.032	0.032
	(0.048)	(0.048)	(0.062)	(0.062)
Access to closed water sources	0.100**	0.095**	0.010	0.007
	(0.044)	(0.044)	0.056	(0.056)
Access to private & septic-tanked toilet	0.167**	0.169**	0.146**	0.146**
	(0.032)	(0.032)	(0.034)	(0.034)
farm household	0.028	0.025	0.065*	0.063*
	(0.035)	(0.035)	(0.037)	(0.037)
household head main activity	0.010	0.009	0.008	0.008
	(0.012)	(0.012)	0.012	(0.012)
Distance from district capital	0.000	0.000	0.002**	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
Urban	0.264**	0.264**	0.289**	0.288**
	(0.037)	(0.037)	(0.058)	(0.058)
Constant	-4.148**	-4.302**	-3.927**	-4.093**
	(0.295)	(0.297)	(0.313)	(0.314)
Panel B				
Unmatched Difference-in-Difference, (treatment-control)		0.266**		0.271**
		(0.068)		(0.086)

Note: n=8323. Standard errors are reported in parentheses (robust standard errors for FE estimation). FE estimation is using village fixed-effect. **: significant at 5 percent, *: significant at 10 percent.

Table 3. Probit estimation of children being exposed by expanded supplementary program

	Coefficients	z-statistics
Male household head	-0.011	-0.22
	(0.051)	
# of male adults	0.010	0.6
	(0.017)	
Percapita expenditure (ln)	-0.110	-4.53
	(0.024)	
Access to free health services	0.139	3.43
	(0.041)	
Access to electricity	0.167	3.18
	(0.052)	
Access to closed water sources	0.114	2.34
	(0.049)	
Access to private & septic-tanked toilet	-0.002	-0.06
	(0.04)	
farm household	0.060	1.61
	(0.037)	
household head main activity	0.032	2.83
	(0.011)	
Distance from district capital	0.001	1.3
	(0.001)	
Urban	0.120	3.07
	(0.039)	
Constant	-0.138	-0.48
	(0.290)	

Note: dependent variable is binary of targeted group (children 6-48 months) being exposed by the program in 2000. Standard errors are in parentheses.

Table 4. Average treatment effect on treated (ATT) of supplementary food program on child health, Indonesia 1997-2000 (Matched difference-in-difference)

# of treatment	# of control	Matched DID/ATT	Stdr. error	t-stat
Nearest neighbor matching (random draw)				
1312	968	0.494**	0.068	7.314
Nearest neighbor matching (equal weight)				
1312	968	0.513**	0.068	7.573
Radius matching				
1005	4146	0.413**	0.060	6.930

Source: Author's calculation. **: significant at 5 percent.

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