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Achmad Maulana
Elan Satriawan
Abror Tegar Pradana

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THE NATIONAL TEAM FOR THE ACCELERATION OF POVERTY REDUCTION
Office of the Vice President's Secretariat
Jl. Kebon Sirih Raya No.14, Jakarta Pusat, 10110

THE EFFECT OF EDUCATIONAL EXPANSION ON HOUSEHOLD LABOR ALLOCATION AND EARNING: EVIDENCE FROM RURAL INDONESIA[1]

Achmad Maulana^ Elan Satriawan* Abror Tegar Pradana§

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Abstract

In this study, we here examine one of the mostly studied education project in developing countries, the school construction program (INPRES) in Indonesia, on household labor allocation and earning. The effect of (expanding) education on earning has been well-documented in the literature but the results are mixed when it comes to mechanics on how education affects earnings. In addition to revisit its impact on earning, we therefore estimate program effect on allocation of rural household labor supply to establish potential mechanics on how such education project increase earning. In doing so, this study employs the *difference- in-difference* and *instrumental variable* methods to utilize the variation resulted from the program. The results show that household labor allocation to non-agriculture sector increased, especially in household where its member(s) born in districts that was exposed to more intensed school construction during the INPRES program period. We also find that the positive effects of higher average of years of education on household earning, indicating a positive return to schooling among these sampled rural households.

JEL codes: I26; J22; Q12

Keywords: Return to Education; Labor Supply; Farm Household

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^Prospera & Universitas Padjadjaran, Email: achmad.maulana@fe.unpad.ac.id

*National Team for Acceleration of Poverty Reduction (TNP2K) & Universitas Gadjah Mada, Email: esatriawan@ugm.ac.id

§Universitas Gadjah Mada

1. Introduction

For decades, many developing countries have spent a large sum amount of money to build educational infrastructure to increase school enrollment and educational attainment. While the effects of such huge investment on economic growth and labor market outcome are well studied (See Bils and Klenow (2000) and Duflo (2001)), the results are not yet conclusive when it comes to the mechanic or transmission on how education affected those outcomes (see Bahadur & Maja (2012), Jolliffe (2004), Corral & Reardon (2001), Fafchamps & Quisumbing (1999) and Escobal (2001)). In theory, individuals or households with more education are better equipped to decide how much they should supply their labor time and to what sector in order to optimize their welfare –when marginal value of time of working in different sectors are equal or marginal value of time of additional working equal to marginal value of extra leisure. But whether more education links to more earning through better allocation of labor supply, which reflect improved household technical or allocative efficiencies[2], is an open empirical question.

In this study, we aim to investigate the impact of a large school construction program in Indonesia, known as *Sekolah Dasar INPRES* program, on rural households' labor supply and earning. A crucial challenge of this study is to discern whether the increase in education due to the INPRES program helps household to allocate their labor to a more profitable sector and whether after controlling this the households are economically better off. To address this challenge, we combine the data on INPRES program intensity with other sources of variation at the household level. First, an individual in Indonesia can officially enter the labor market when they are 15 years old, although, on average, they wait a bit longer to form a household.[3] Hence, the program effect on household labor allocation towards non-agriculture sector and household total earning that are driven by the change in education of husband and their spouse start taking place later in time. On the other hand, the effects on household labor allocation and household total earning that are driven by the change in education household members, other than husband and wife, start immediately. With this information, we then can capture the dynamics of these two groups.

To that end, we use the simple majority rule in household. If a household demographic structure is dominated by the cohorts that have maximum exposure to the INPRES program, then regardless of their household member status in the household then increase

[2]For discussions on the effect of schooling on economic growth and labor productivity see Bils and Klenow (2000) and Duflo (2001).

[3]Breierova and Duflo (2004) finds that in SUPAS 1995 wife's age at first marriage was about 18 years old while the age difference with their husband was four years old.

in education via this household may have bigger effect than household whose demographic structure is not dominated by the cohorts that have the maximum exposure to the INPRES program. We follow Duflo's (2001) cohort classification to identify household that has exposure to the INPRES program and extend it to 15 years old. Our main identification strategy combines the number of INPRES schools constructed with the household demographic structure and their respective information on district of birth and year of birth.

We implement *difference-in-difference* strategy and *instrumental variable* estimations to estimate the impact of INPRES school construction on the average years of education, labor allocation, and total earning in the households. Specifically, we compare two groups of households with different demographic structure: household whose members aged 15 to 27 and household whose members aged 33-38. We control for the possibility of preexisting trend by including number of children and district enrollment before the INPRES program was implemented. In addition to that, we also control for number of household members per cohort which capture changes overtime that affects all households in a similar way.

The main result of this study is that rural households with (simple) majority of its members exposed to the INPRES program are more likely to allocate their human resources toward non- agriculture sector and earn more in total from the labor market. Furthermore, consistent with Duflo (2001), we find that the effects are stronger for households that have household head with age between 21 and 23 years old (born between 1972 to 1974). This paper thus documents that household with additional aggregate years of education tends to move away resources from agriculture sector. While this finding is justifiable and reflect rational behavior of farmers, it limits the effect educated labor to agricultural activity.

The rest of the paper is organized as followed. We highlight previous works on impact of such large education investment project and related topics. Afterward we provide brief description about the INPRES program, followed by explanation of the data. We then discuss about empirical strategy to estimate the impact of program alongside with addressing the estimation issues. The discussions on results comes after and then we close it with summary the main findings and policy implications.

2. Previous Works

We know that education may affect farmers' earning throughout number of channels. First, a more educated farmer usually uses more of technical skills when cultivating their plot, increases used of technical efficiency of production (Lockheed et al. (1980)). Output vis-a-vis with earning is increasing in education. Second, farmer with higher education tends to exploit augment skills in allocating resources in the most profitable manner (Nelson & Phelps (1966), Schultz (1975), and Rosenzweig (1982)). Fane (1975) finds that farmers with more years of schooling are much closer to pre-constructed measure of minimum cost, which he claims as finding to support that education improves allocative efficiency. Which one matter more of these two channels? Huffman (1974) further finds that education matters more for increasing allocative efficiency than technical efficiency while Wu (1977) argues that education matters more for technical efficiency than allocative efficiency. Third, education can help farmer to reduce their effort to process new information associated with new technology (Rosenzweig (1982)).

This paper relates to several group of works in the literature. First, it connects to the literature that has study the economic effects of education on household labor allocation between agriculture and non-agriculture sectors. This literature has typically documented on the high return to education on agriculture sector. In Ghana, Jolliffe (2004) finds that increasing stock of human capital at the household level has a detrimental effect on supply of labor to agriculture sector while the adverse effect is found for the effect of additional year of education on labor supply to non-agriculture sector. This finding also holds either for male or female members of households as documented by Fafchamps & Quisumbing (1999). Similarly, Escobal (2001) also finds that households with more education have greater incentive to allocate their working hours to non-farm employments. Our contribution to this literature is by focusing on how investment in educational infrastructure in developing country that led to increasing human capital stock have particular impact on rural household labor supply allocation towards rural non-agricultural sector. Since a large fraction of labor force in rural areas in developing supplies their labor to agriculture, understanding in what ways we could reallocate their labor supply toward non- agriculture sector have a large potential to improve the well-being of the poor, which most of them reside in rural.

Second, this paper is related to the literature that documents the impact of education of household on their total earning. This literature has typically found on its positive impact on household welfare, as measured by income or earning derived from employment or profit generated from business activity (Bahadur & Maja (2012), Jolliffe (2004), Corral & Reardon (2001), Fafchamps & Quisumbing (1999) and Escobal (2001)). However, many of these works relied only OLS estimation when evaluating the impact of education on earnings. Arguably education is endogenous and thus ignoring it leads to bias in estimated parameter of education. To address the issue, we use instrumental variable (IV) approach in estimating the impact of education on earning. We find the use of IV estimation leads to bigger estimates than OLS – consistent with findings from industrialized countries, but different with Duflo (2001) findings that used male sample who work for wage in Indonesia.[4]

Third, this paper also relates with the literature in empirical estimation of return to schooling on rural economy. Jamison & Lau (1982) find that productivity increases by an average 8.7% as a result from farmers completing four years of elementary school. On subsequent years, a consen- sus on the effect of schooling to farm productivity is yet to be reached (see discussion on Taylor & Yunez-Naude (2000)). This paper finds that household with more years of education is likely to move their resource from agriculture, indicating a small return from agricultural sector. Figure A.2 depicts a rough estimate of return to schooling between different sector in the economy and area.[5]As we can see return to education on rural agricultural sector is the lowest one, along with rural manufacture and rural transportation sectors. Thus, finding from this study and many others who find a limited impact of schooling on rural agricultural productivity raise a question on the appropriateness of education policy for rural development.

Finally, this paper relates to the set of papers that have analyzed the impact of the School Construction Program (INPRES) in Indonesia. See, for example, Duflo (2001), Duflo (2004), Breierova & Duflo (2004), Somanathan (2008), Ashraf et al. (2016), Maulana (2016), and Martinez-Bravo (2017). In addition to revisiting program impact on household earning, we add to this literature the evaluation of program impact on rural household labor supply allocation as potential mechanism on how education increases earnings. We argue that increased years of schooling among farmers –due to INPRES Program—would improve their ability in in allocating better their labor supply toward jobs and sectors that pay more.

3. The Sekolah Dasar INPRES Program

In 1973, Government of Indonesia (GOI) started a school construction program through Presidential Instruction (Instruksi Presiden, which is called INPRES program afterward). The program was financed by the unexpected windfall in central

[4]See Card (1995) and Card (1999) for discussion on the difference between OLS and IV estimates when used to estimate the causal impact of education on earning.
[5]We exactly replicate Duflo's (2001) sampling frame to estimate this figure by sector and residency.

government revenue from international oil price hike that time. The main objective of the program was to increase enrollment rate to primary education. The program also recruited teachers and provided complementary school infrastructures such as textbooks, libraries, etc. The first installment of the program was targeted to build 6,000 new schools nationwide in area where enrollment rate to primary education was low. The World Bank hailed the INPRES program as one the most successful cases of large school system expansion on record (World Bank (1990)) as it succeeded in increasing primary education enrollment rate from only 47 percent in 1971 to 92 percent in 1987.

The program was intended to target children who live in area where schools were scarce. A separate budget was allocated to rehabilitate of existing school buildings. The distribution of INPRES funds is described in detailed government instruction through the appendices of *Surat Keputusan Bersama Menteri Dalam Negeri, Menteri Pendidikan & Kebudayaan, Menteri Agama, dan Menteri Negera Ekuin/Ketua Bappenas.* These appendixes were very precise on the allocation of schools between districts. We use this planned number in the paper rather than the actual number of schools constructed. Therefore, our design can be interpreted as the intention- to-treat design.

The Sekolah Dasar INPRES program accounted for almost 12 percent of the regional develop- ment budget in 1973 and 28 percent of the regional development budget in 1979. During the oil-boom era, Indonesia spent almost 15 of their total budgets for regional development. Comparing to the other human capital investment, the health budget in 1973 accounted for 3.4 percent of the total budget.

Summary of the program is presented in Table B.1, panel C. With its huge fund, the INPRES program managed to build one school per 500 children in all districts in Indonesia. Between 1973-1974 and 1978-1979, more than 60,000 new schools were built. The program intended to boost fraction of people attending school, which in 1971 it recorded only 18 percent of Indonesian ever attend school. In high intensity district, almost three schools managed to be built for every 1,000 children and only half of those build in districts where more children attend primary school in 1973.

4. Data

The primary data source for this paper is the 1995 Intercensal - Census Survey of Indonesia (SUPAS), which contains more than 210,000 households. Of these, more than 60% of them live in rural area. The SUPAS recorded that more than 15,000 of rural households in 1995 participate in agriculture sector of have household members who belong to the productive age, between 15 and 51. We then focus our analysis on farming household (Jolliffe (2004)) and only 18 percent of the households who participate in any kind of agriculture activity state they have a valid earning data. For consistency, we estimate the effects of the program on education, earning equation and labor supply decision using this truncated sample.

The geographic units employed in this study are place of birth rather than current residence. Matching individuals with current district of residence where they end up as adults would be difficult to interpret because of selective migration after they attain their education. Matching with district of birth is not endogenous to the program. Selective migration where some households might move from the origin where a child was born to high program intensity district might confound our estimates.[6]

We use two proxies for labor supply decisions that are available from the SUPAS data: fraction of workers in non-agriculture and share of working hours in non-agriculture. We expect for both variables to measure household labor supply preference between agriculture and non-agriculture sectors. While for earning, we extract directly from SUPAS as they collect data on last month's wage for people who are working for pay. From this, we calculate household earning by adding all the household member earnings.

Summary statistics on the study sample are shown in Table B.1. Mean number of household size is 3.53 with more than half of it aged over 15 years. Average year of education of all household members is 5.30 years. Of the 15,681 selected households, 40.4 percent of them have members aged 15 to 27 years old and the rest of the households have cohorts above 15 to 27 years old bracket. This last cohort act as the treatment group in our study while the first represent the treatment households. On average, the mean household earning is 127,981 IDR with total hours of work in last week are more than 55 hours. The mean age of household head is 32 years and average school attainment for sample with valid wage is 5.62 years. Only 15 to 18 percent members participate in non-agriculture sector, both in terms of reported job sector and working hours.

5. Identification Strategy

5.1 Identification: effect of the program on education

The date and district of birth of an individual i and their household composition

[6]If we had data on districts where individuals earn his/her education then we could use district of birth as an instrument for region of education as our first best solution to selective migration problem.

simultaneously determine household's exposure to the program. In Indonesian children normally enroll primary school between the age of 7 and 12. A child whose age was 12 or older in 1974 would get a minimal exposure to the program since they have left primary school before the first INPRES school were opened. Thus, the effect of the program should be negligible for children age 12 or older and positive for children younger than 12 in 1974. The district of birth is second dimension of variation in the exposure to the program. Young children born in a district where its enrollment rates in 1972 were low was very likely to attend INPRES school in that region, and thus their exposure to the program is supposed to be high. Using this observation, Duflo (2001) proposed to use the interaction between an individual's cohort and the number of schools built in his district of birth to evaluate the impact of the program.

In this study, we use the same set of strategy to estimate the effect of the program on household average educational attainment, household labor allocation, both in terms of worker and hours of work, and household earning. To identify it, we use the interplay between household structure and the level of the program in each household member's region of birth. If a household, by the of survey in 1995, has members age 15 to 27, the younger this household, the more likely that this household to have been exposed to the program. To illustrate, we present simple two-by- two table. Table (B.2) shows means of average education and total household earning for different household structures and program intensity levels. In panel A, we compare average years of education and earning of majority of household members were fully exposed the INPRES program when they were in primary school (their age in 1974 must not be older than 6 years) to those of household members who little or no exposure to the INPRES program, in both high and low intensity regions. In both household structures, the average years of education in districts that received *higher* schools are *lower* than in the districts that received *less* schools. In both type of regions, the years of educational attainment are increasing over time, but it increased *slightly* less in regions that received *less* schools. The difference in differences can be accounted to the INPRES program, under the assumption that, in absence of the INPRES program, the increasing educational attainment would not have been systematically different in the two regions. Interpreting result in panel A, a household with young members where their district of birth enrollment rates was low, thus get constructed more schools, received on average 0.81 more years of education and the logarithm of household earning was 0.16 higher. The simple estimator suggests that one school per 1,000 children contributed to an increase in education by 0.62 years (0.81/1.3) and earning by 0.12 for households whose majority of their members were exposed to the INPPRES program.

The proposed identification strategy should be not taken as it is: the pattern of increase in average education could vary systematically across regions. There could be mean reversion on the educational achievement across age-household structure. In Table (B.2) panel B, we checked this assumption by calculating the same difference in differences between households whose members were not exposed to the INPRES program. The increase in average education between two household structures shows no discernible difference between households in two regions. The estimated difference in differences are statistically not different than zero.

5.1.1 First Stage Results: Effect on Education

While the two-by-two-lead identification reveals some interesting results on the effect of the INPRES program on education but the variation in program exposure across districts and age groups is not fully exploited thus limits its potential impact. To exploit the variation in the planned number of schools constructed across districts and years, we can generalize the above identification to a regression framework. We hypothesize that additional schools constructed will lead to an increase in years of education of those who are exposed. Thus, the difference between the average education of household with majority of its member exposed to INPRES program and those with (older) members not exposed to the program will be positively related to the number of schools constructed by the INPRES program. This suggests running the following regression:

$$\overline{S_h} = c_1 + \sum_{i=15}^{51} a_{ih}\alpha_i + \sum_{m=1}^{282} b_{mh}\beta_m + (\overline{P_h} \cdot T_h)\gamma_1 + \sum_{i=15}^{51} (\boldsymbol{C_h} a_{ih})\delta_i + \varepsilon_h \ldots\ldots\ldots (1)$$

where $\overline{S_h}$ is the average education of household h, c_1 is a constant, a_{ih} is number of household members who aged i in 1995, b_{mh} is number of household members who were born in region m, $\overline{P_h}$ denotes the average number of schools constructed per 1,000 children from member of household h, T_h is a dummy indicating whether the household belongs to the 'young' household–those that its majority of its member exposed to the program—in the subsample, and $\boldsymbol{C_h}$ is a vector of control variables where we use average number of children and enrollment rate.

The average terms in (1) are defined as follows: where 9 denotes the number of household members aged 15 years or above.

$$\overline{S_h} = \frac{1}{H}\sum_{i=1}^{H} S_{imh} \qquad\qquad \overline{P_h} = \frac{1}{H}\sum_{i=1}^{H} P_{imh}$$

Table B.3 (columns 1-2) presents estimates of specification (1) for two rural household subsamples. In panel A, we contrast households whose member aged 15 to 27 in 1995 with households whose their member aged 33 to 38 in 1995. In column 1, the estimable results only control for the number of household members per cohort, number of household members per region of birth, and interaction between number of household members per cohort and the number of children in the region of birth (in 1971). The suggested effect is that for every school built per 1,000 children increased human capital stock, in form of years of education, of households whose members aged 15 to 27 in 1995 and live in the rural areas by 0.22 years for the whole sample and by 0.71 for the sample of household members who have valid wage data. This reading from the result relies on the assumption that there are no omitted time-varying and region-specific effects correlated with the program. How much schools each district get from the INPRES program fund depended on the number of enrollment rate in the district in 1972. Therefore, the estimate could confound the effect of the program with mean reversion that would have taken place, even in the absence of the program. To minimize this, we control for enrollment rate in 1971, the effect for school does not change as much as its expected, it gives some kind of mild indication that the estimates are not confounded by mean reversion or omitted programs related to INPRES program.

Panel B of Table B.3 presents the results of model (1) for the control subsamples (comparing households whose members aged 33 to 38 to households whose members is over aged 38 in 1995.). We know for sure that that household members in this age group could not possibly benefit from the INPRES program. Or in other words, for member of these cohorts we expect that educational attainment would not be systematically different between households who live in districts with more schools and households who live in districts with less schools. As it can be seen from the results, the effect of the program is very small and statistically never significant. While it cannot rule out the possibility that the educational attainment due to some unobserved factor may start to converge after the program started, these evidences are reassuring at some extent.

To dig further through the regression analysis, we follow Duflo's (2001) strategy utilizing interaction term analysis. The relationship between between the average years of education S_h of household h, where each of its adult member born in region j, ages k in 1995, and their exposure to the program could be described as follows:

$$\overline{S_h} = c_1 + \sum_{i=15}^{51} a_{ih}\alpha_i + \sum_{m=1}^{282} b_{mh}\beta_m + \sum_{i=16}^{33} (P_m \cdot A_{ih})\gamma_i + \sum_{i=15}^{51} (\boldsymbol{C_h} a_{ih})\delta_i + \varepsilon_h \quad \ldots\ldots\ldots (2)$$

where A_{ih} denotes a dummy that indicates household head of household h aged i in 1995 and P_m is program intensity that is received by household head who was born in region m. In this estimate, we impose restriction that γ_i equal to 0 for i > 34. The control group for this specification is comprised of household members aged greater than 33 in 1995. Employing this model, we hope to get a more efficient and precise estimates of the effect of the program.

Column 1 and 2 in Table B.4 show the coefficient of interactions between household head's age in 1995 and the intensity of the program in region of birth in two different specifications. In the two columns, almost all estimated effects are positive, as expected. Most of the coefficients are significantly greater zero and all the interactions statistically different from zero (the F— statistics for the null hypothesis that all the interaction are not different from zero are presented at the bottom of the table). Since the allocation of INPRES schools are not random but subject to enrollment rate prior to the program started, thus it is possible that there exists some region-specific effects. To control this, we add the estimated effects that already control for enrollment rate in 1971. The coefficients are slightly different after the 1971 enrollment rate control is added. The coefficients reach its peaks at age 21 to 23 and decline after and before that age. The estimates in column (2), suggest that additional school built per 1,000 children increases the education of adult household member with household head's age 22 in 1995 by 0.96 years. On average at high program district, 2.88 schools were built. It implies that at its mean value at high program district, the program caused an increase in education of 2.76 years for these children.

5.2 Identification: effect of the program on HH Labor Supply

We will mimic the above strategy to construct instrument for education in the equations that determine household decisions to whether and how much allocating their labor supply to agriculture or non-agriculture sectors. We use two variables at the household levels to represent those decisions: fraction of workers in non-agriculture and share of working hours in non- agriculture. The use of instrument for education is justifiable under the assumption that, in absence of the INPRES program, the pattern of household labor supply decisions across cohorts would not have been systematically different in district with low enrollment rate in 1971 than in districts that has high enrollment rate in 1971. We thus can compare the change of the household labor supply decisions over time and between districts and evaluate whether the INPRES program change these patterns.

There are some potential problems with these assumptions. First, the existence of specific time trends across districts that may be arise due other factors than the INPRES program itself. For illustration, the increase in labor supply to agriculture sector or non-agriculture sector may have been faster in the absence of the program if these districts in 1995 were in the early stage of the development, either agriculture and non-agriculture sector dependent. The level of development of agriculture or non-agriculture sector is likely to affect households smoothing decision over time, rather than only the households who were mostly affected by the program. We will thus check whether there are different time trends for households that were not exposed to the program. Later in the regression, we also add controls for enrollment rates in 1971 and interact it with household head's age dummies. In doing so, we hope to capture time-varying factors correlated with the enrollment rate prior to the program. Second is the potential problem of sample selection. Household average years of education can be calculated regardless of the job market sector status of each member. Each member participation in the agriculture or non-agriculture sector, however, is depended whether we could identify their labor market sector participation. In the sample, there are 15,681 households in rural area of Indonesia. Only 2,833 of them had participated in wage for work and thus have job sector status. Restricting the sample to households with complete information on their job market status means we are not including households who participate in the labor market as self employed and thus can introduce a selection bias in our estimates, if the probability participating in wage sector and sector selection potentially depend on education. Duflo (2001) shows that the probability of working for wage is indeed affected by education. While Duflo (2001) used only male sample in her estimation, but we think that the same pattern may be found when more complete sample is used, as it is in our case.

The first issue may affect the interpretation of the interaction between the program intensity and household labor supply decisions. Before moving to other part, it is useful to understand about the interpretation of that coefficient. As we have known, the instrumental variable method used in this study identifies the effect of giving one more year education to a random individual on household labor supply decisions. There were three possible household member status that individual can have in 1995: head of household, spouse to head household, and member of household. One that we most concern about is the first two. Head of household and their spouse are not randomly matched, they choose each other on the marriage market. Thus, administering education to these *random* individuals was most likely happen before their marriage. Marriage is coming after the education where future spouse can base his/her choice on the level of education of the proposed. The effect of education on the household labor supply decisions, thus, via head of household and its spouse will also likely reflect the effect of assortative matching.[7]

As with education, we estimate the effect of the INPRES program on household labor supply allocation to non-agriculture sector by running the following regression:

$$outcome_h = c_1 + \sum_{i=15}^{51} a_{ih}\alpha_i + \sum_{m=1}^{282} b_{mh}\beta_m + (\overline{P_h} \cdot T_h)\gamma_1 + \sum_{i=15}^{51} (\boldsymbol{C_h} a_{ih})\delta_i + \varepsilon_h \ldots\ldots\ldots (3)$$

where $outcome_h$ is the household h's outcome, where it is labor supply allocation to non-agriculture sector. We proxy the household labor supply allocation with fraction of total workers work in non-agriculture sector relative to total workers in a household and share of working hours worker in non-agriculture sector relative to total working hours of workers in a household.

Table B.3 (column 3-6) presents the results of specification (3). In panel A, we set Th equal to 1 for household whose member aged 15 to 27 in 1995 and use households whose members aged 33 to 38 in 1995 as the benchmark group. The suggested effect ranges from 2.27 to 2.51 percentage point for fraction total worker work in non-agriculture sector while for share of working hours of workers in non-agriculture sector the effect is from 3.17 to 3.39 percentage point. Like the effect to years of education, the estimates only slightly change when we control the interaction between number household members per cohort and enrollment rate in 1971. None of these estimates is statistically significant different from another. In panel B, we present the control experiment for the same specification. The suggested effect for this particular group is very small and not statistically significant different from zero in all specification.

We also estimate a specification that use interaction term between household head's age dummies and program exposure to household labor supply allocation to non-agriculture

[7]Breierova & Duflo (2004) argue that this could be the case when evaluating the effect of education on fertility and child mortality. They further put forth a case that the coefficient will incorporate the average unobserved quality of the men who choose to marry women with the education predicted by the instrument, over and above the direct impact of the husband's education.

sector. The specification that we estimate are as follow:

$$outcome_h = c_1 + \sum_{i=15}^{51} a_{ih}\alpha_i + \sum_{m=1}^{282} b_{mh}\beta_m + \sum_{i=16}^{33} (P_m \cdot A_{ih})\gamma_i + \sum_{i=15}^{51} (\boldsymbol{C_h} a_{ih})\delta_i + \varepsilon_h \ldots\ldots\ldots (4)$$

In columns (3)-(6) of Table (B.4), we present the estimates of (4). The effect of the INPRES program on household labor supply allocation to non-agriculture sector is smaller than as on education because the fraction of workers and share of working hours in household that work in non-agriculture sector are relatively small and we only estimate the effect for people who work for wage while for workers who work in non-agriculture sector as self-employed are not accounted for. The coefficients are slightly increase after the 1971 enrollment rate control is added. However, the coefficients of fraction of workers and share of working hours reach their peaks at different age, 31 and 22, respectively, and decline after and before that age. The estimates in column (5), without any additional controls, suggest that additional school built per 1,000 children increases the share of working hours in non-agriculture sector for household who had household head aged 22 in 1995 by 0.04. On average at high program district, 2.88 schools were built. It implies that at its mean value at high program district, the program caused an increase in the fraction of worker work in non-agriculture sector by 0.12.

5.3 Identification: effect on HH earning

We use the exact identification strategy to evaluate the impact of the INPRES program on household earning as outcome using (3). The results are presented in Table (B.3) columns 7 and 8. In panel A, we compare household that its majority of members aged 15 to 27 to household that its majority of members aged 33 to 38. We use the later as the comparison group. The effect of building additional school for every 1,000 children on rural household earning range from 12.9 to 13.3 percent. As in the case of two previous outcomes, educational attainment and household labor supply allocation to non-agriculture, the estimates increase slightly when we control for the interaction between number of household members per cohort and enrollment rate in 1971, although none of these estimates is significantly different from each other. In panel B, we compare two group of households, households whose member aged 33 to 38 and households whose member aged 39 to 51, that we call them as the control experiment cohort households. The interaction coefficient is small and never statistically significant different from zero in the two specifications.

Employing the same identification strategy as the above, we also estimate the direct effect of INPRES program on household total earning as specified in (4). The effect of the INPRES program on household earning is relatively smaller than the effect of the program on educational attainment. The effect reaches its peaks at age 20 to 24 and declines after and before that age. It indicates that households who have household head aged 20 to 24 experience the biggest effect of the program on their earning.

5.4 Unrestricted Reduced Form Results

The above strategy can be generalized to an unrestricted interaction terms analysis. We expand the relationship between households metrics for years of education, outcomes which are labor supply decisions and earning of households h and their exposure to the INPRES progam, respectively:

$$\overline{S_h} = c_1 + \sum_{i=15}^{51} a_{ih}\alpha_i + \sum_{m=1}^{282} b_{mh}\beta_m + \sum_{i=16}^{50} (P_m \cdot A_{ih})\theta_i + \sum_{i=15}^{51} (\boldsymbol{C_h} a_{ih})\delta_i + \varepsilon_h \,\ldots\ldots\ldots (5)$$

$$outcome_h = c_1 + \sum_{i=15}^{51} a_{ih}\alpha_i + \sum_{m=1}^{282} b_{mh}\beta_m + \sum_{i=16}^{50} (P_m \cdot A_{ih})\theta_i + \sum_{i=15}^{51} (\boldsymbol{C_h} a_{ih})\delta_i + \varepsilon_h \,\ldots\ldots\ldots (6)$$

In these unrestricted estimates, we measure the time dimension of exposure to the program with 35 household head's age dummies. Head of household who aged 51 in 1995 form the control group, we omitted the dummy from our estimation. Each coefficient θ_i can be interpreted as estimates of the impact of the INPRES program on a given cohort member of household.

Figures (A.3) and (A.4) overlay plots the combination of θ_i from education and outcomes estimations. Each dot is the coefficient of the interaction between dummy for being a given household's age in 1995 and the number of schools constructed per 1,000 children in respective region of birth. The main idea presenting this plot is to show whether the change in the outcomes, education, labor supply allocation and earning is driven by the change in the program intensity. If this is the case then the plot of these coefficients should track one another. As expected, the average impact of the program on education is higher for the treatment group than the control group. The coefficient for the cohort group who get less exposure from the program fluctuates within -1 to 0 intervals while for the cohort group who get more exposure to the program it fluctuates within 0.3 to 0.7 intervals. Pattern of the impact of the program on household labor supply allocation, however, is not clearly visible for the two different cohorts. Interestingly, figure (A.4) show that change in the impact of program on household earning seems to follow change in the impact of program on education, even if for household head aged older than 36, the coefficients are oscillatory and some cases are higher than treatment. Almost the same pattern happens between change in education and the labor supply allocations. These two evidences seem to suggest that the program effect on household earning was caused by the changes in years of education and change in allocation of household labor supply.

6. Estimating Household Return to Education

6.1 Total Earning

The previous sections of this paper have laid our effort to show that the change household earning and educational attainment would not be different statistically between districts and between cohorts, before the INPRES program was implemented, are sufficient to estimate the causal impact of the INPRES program. In addition to that, if the assumption that INPRES program had no impact on household earning other than by increasing the average of years of education is justified then we could use it to form instrumental variables estimates of the impact of change in average education on household total earning. Thus, estimates of (1) and (2) relates significantly to the process of estimating household return to education as it provides the first- stage of a two-stage least square (2SLS) estimation of the impact of household average education on their total earning.

Main question of interest of this study is to estimate the following:

$$outcome_h = c_1 + \sum_{i=15}^{51} a_{ih}\alpha_i + \sum_{m=1}^{282} b_{mh}\beta_m + \overline{S_h}\theta + \sum_{i=15}^{51} (\boldsymbol{C_h} a_{ih})\delta_i + \eta_h \;\ldots\ldots\ldots (7)$$

Ordinary Least Square (OLS) estimates of (7) lead to biased estimates if the error term η_h correlates with $\overline{S_h}$. To address this, we utilize two sets of instruments for education: (i) the single instrument, interaction of household cohort in 1995 and the program intensity and (ii) the multiple instruments, interaction of household head's age in 1995 and the program intensity in district of birth.

The results are presented in panel A of Table B.5. The first line shows the OLS estimate. The estimated return to education is 6 percent and it is stable across different specifications. The second line shows 2SLS estimates of specification (7) with additional controls. In column (1), we add only interaction between number of household members per cohort and number of children in 1971. The point estimate of the causal impact of household education on household total earning is 19.2 percent, a relatively bigger than the OLS estimate. As stated earlier, schools built under the INPRES fund was not random but depend heavily on the enrollment rate before the program started. Take into account this, we add interaction between number of household members per cohort and enrollment rate in 1971 as additional control in column (2) and the coefficient becomes 20 percent. In the third line, we present the 2SLS estimates using multiple instruments and the results are relatively stable at 11 percent when we use the interaction be- tween age of household head in 1995 with program intensity. The results of multiple instruments are different comparing to single instrument, but more precise, since they use more variation. Although the instruments are valid, our concern in 2SLS estimates especially in multiple instruments is the problem of weak instrument where the F-statistics and effective F-statistics of first stage are less than 10. As suggested by Angrist & Pischke (2009), we compare 2SLS in Table B.5 and Limited Information Maximum Likelihood (LIML) in Table B.6. Using the same set of instruments, we find that LIML estimates show *slightly* different results where the impacts are 13.3 to 13.7 percent.

6.2 Labor Supply Allocation

Using similar instruments like the above, we employ model (7) for labor supply allocation as an outcome. The results are showed in panel B and C of Table B.5. The first line in panel B and C presents the OLS estimates of the impact of education on the share of household total worker in non-agriculture sector and the share of household total working hours spent in non-agriculture sector. The OLS estimates of two variables that we use as a proxy for household labor supply allocation indicate similar return to additional education. The estimates are 1.7 percent and 2.0 percent and these estimates are stable when we include additional controls. In the second and third lines, we employ 2SLS and IV estimation process to causally estimate the impact of change in household average educational attainment on their labor supply allocation between agriculture and non-agriculture sectors. Same with the total household earning, the 2SLS and IV estimates are relatively larger than the OLS estimates. Using multiple instruments, the estimates of the impact of additional years of education on fraction of household worker in non-agriculture sector are 3.8 to 3.9 percent. However, if we employ single instrument, the impact of additional years of education on this outcome is statistically insignificant. Estimates using the share of total working hours in non-agriculture sector as outcome suggest the larger impacts in both single and multiple instruments where are 3.94 to 4.25 percent and 4.71 to 4.86 percent, respectively. The LIML estimates give larger impact than 2SLS estimates where the impact of education on the fraction of household worker and the share of total working hours in non- agriculture sector are 4.57 to 4.61 percent and 5.45 to 5.53 percent, respectively.

The evidences so far have pointed to statistically impact of an increase of average years of education at the households on share of labor and working hours to non-agricultural sector. How meaningful are these impacts in real terms? To answer that, we compare the coefficients of education with unconditional average of share of work hours in non-agriculture. The coefficients are between 4.71 to 4.86 while the unconditional average of share of working hours in non-agriculture sector for sample with valid wage is about 18 percent. The real effect of an increase of education is about 27 percent (4.86/18), which is economically meaningful impact. While for the fraction of worker in non-agriculture equation, we found that the real effect is about 22 percent, also a meaningful impact.

7. Conclusion

This paper investigates the impact of a large investment in educational infrastructure in Indonesia, INPRES program, on the long-term household labor market outcomes: household labor supply allocation and household total earning. Combining the program intensity in district of birth of each household members with demographic structure, we try to infer the effect that are driven by changes in human capital accumulation within household. The results, consistent with Duflo (2001), show that the program increase years of schooling particularly to the group that was fully exposed to the program. This in turn facilitates household to allocate its labor supply more toward non-agriculture sector increased reflecting increased in allocative efficiency. Furthermore, we find that the positive effects of higher average of years of education on household earning. Quite simply, these findings indicate that huge investment in improving educational infrastructure significantly improved human capital investment that helped household in allocating better their labor supply to sector with higher yield and it leads to the improvement of household earnings. We believe these findings are quite robust and based on reasonably large samples.

More broadly, the evidence that investment in educational infrastructure have ramifications for subsequent household human capital stock and earning. The present study, demonstrating the effect of education on rural households, particularly whether education investment can shed some lights on the mechanics on how education affect earning. As in these and other papers, the present finding highlights that labor supply allocation toward more profitable sector may drive the increase on earning although it is not ruling out other mechanism.

Data limitation make these explanations more speculative. The results stem from this paper is consistent with other studies in the literature that education expands the human capital of households and individual likely to work in sector that could give them highest earning.

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FIGURES & TABLES

A. List of Figures

Figure A.1: Total of Schools In Indonesia

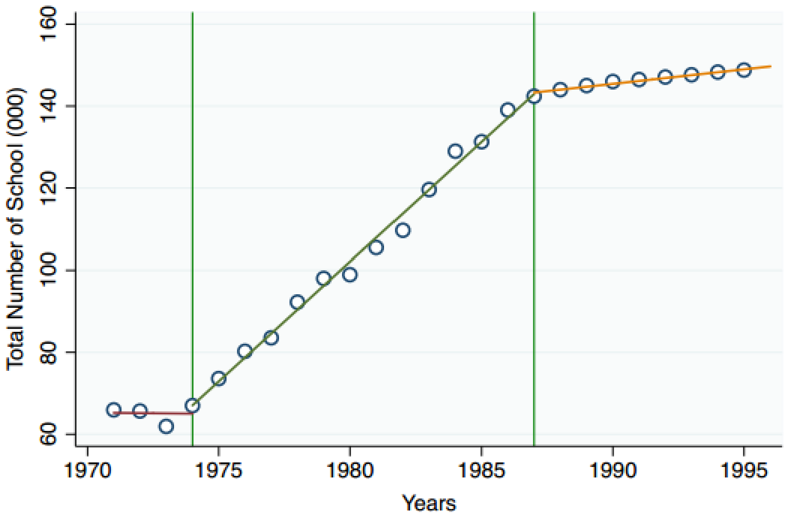


Figure A.2: Sectoral Mincer Returns to Schooling

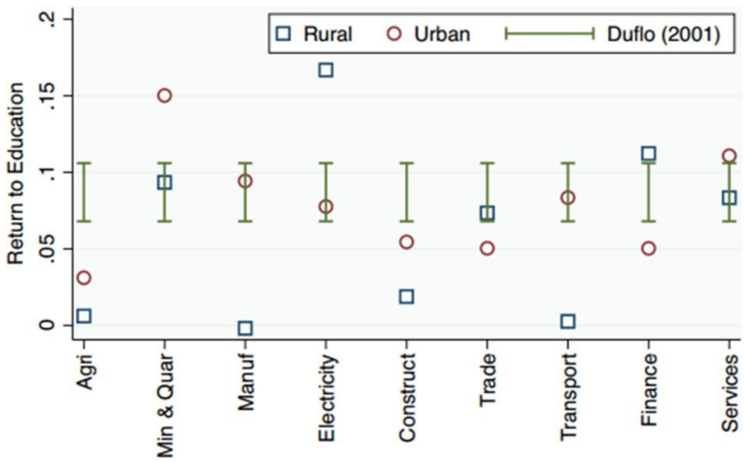


Figure A.3: Coefficient of The Interaction Age of HH Head*Program Intensity in The Region of Birth In Education And Frac of Worker Equations

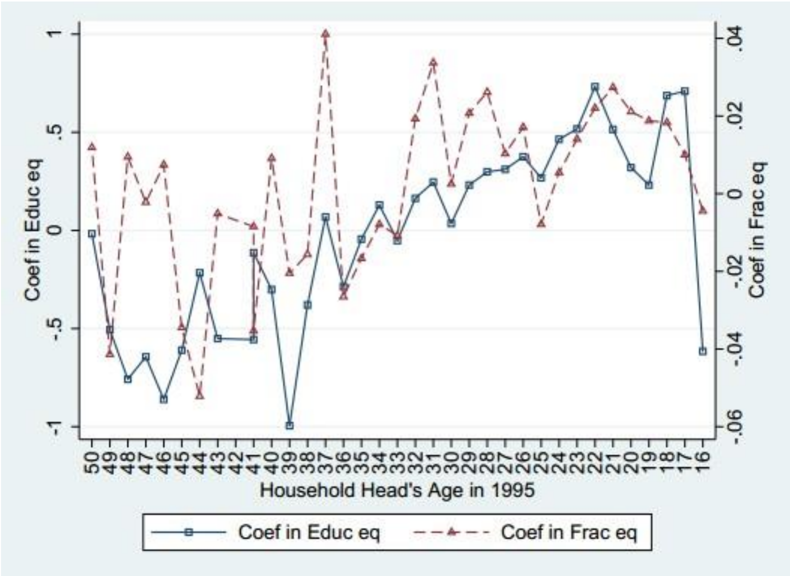
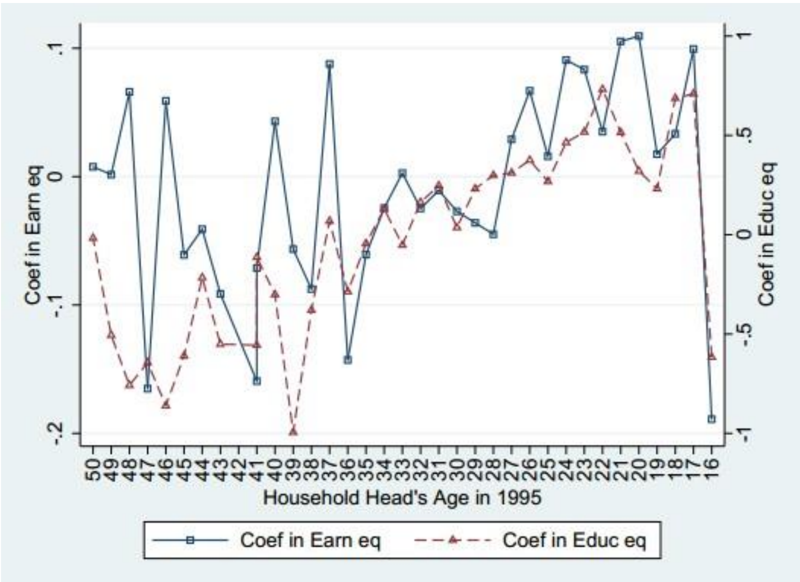


Figure A. 4: Coefficient of The Interaction Age of HH Head*Program Intensity in The Region of Birth In Education And Earning Equations



B. List of Tables

Table B.1. Descriptive Statistics of Selected Sample

Variable	Mean	Std. Dev	Min	Max
Panel A. Household level (N = 15,681)				
Household size	3.533	1.425	1	11
Avg. years of education (adult only)	5.295	2.883	0	16
# household adult member	1.894	0.371	1	6
HH members aged 15 — 27 years old	0.404	0.491	0	1
HH members aged 28 — 32 years old	0.191	0.393	0	1
HH members aged 33 — 38 years old	0.185	0.388	0	1
HH members aged 39 — 51 years old	0.220	0.414	0	1
Panel B. With Valid Wage (N = 2,833)				
HH earning (000 Rp./Month)	127.981	106.438	10	2,750
Total work hours	55.286	24.673	0	198
Age of HH head	32.238	8.382	16	51
Avg. years of education	5.616	3.103	0	16
HH members participate in labor market	1.524	0.526	1	5
Share of working hours in non-agriculture sector	0.186	0.296	0	1
Frac. worker in non-agriculture	0.154	0.231	0	0.667
Panel C. District level (N = 282)				
INPRES schools constructed per 1,000 child	2.287	1.176	0.491	9.554
Fraction of the pop. attending school 1971	0.180	0.080		
Total SD INPRES per 1,000 childern (high)	2.887	1.210	1.422	9.554
Total SD INPRES per 1,000 childern (low)	1.500	0.436	0.491	2.856

Source: SUPAS 1995, Surat Keputusan Bersama Menteri Dalam Negeri, Menteri Pendidikan & Kebudayaan, Menteri Agama, Menteri Keuangan dan Menko Ekuin/Ketua Bappenas various years

Table B.2. Means of Education And Log(HH Earning) by Cohort And Program Intensity

	Years of Education			Log(Household Earning)		
	Program intensity in region of birth			Program intensity in region of birth		
	High	Low	Difference	High	Low	Difference
Panel A: Cohort Experiment:						
HHMs aged 15-27	6.318	6.533	-0.215	11.538	11.437	0.101
	(0.092)	(0.154)	(0.180)	(0.024)	(0.040)	(0.047)
HHMs aged 33-38	4.997	6.025	-1.028	11.568	11.628	-0.061
	(0.179)	(0.235)	(0.295)	(0.045)	(0.059)	(0.074)
Difference	1.321	0.508	0.813	-0.03	-0.192	0.162
	(0.177)	(0.251)	(0.308)	(0.047)	(0.066)	(0.081)
Panel B: Cohort Control						
HHMs aged 33-38	4.997	6.025	-1.028	11.568	11.628	-0.061
	(0.179)	(0.235)	(0.295)	(0.045)	(0.059)	(0.074)
HHMs aged 39-51	3.684	4.639	-0.955	11.385	11.348	0.037
	(0.172)	(0.212)	(0.273)	(0.043)	(0.053)	(0.068)
Difference	1.313	1.385	-0.072	0.183	0.28	-0.097
	(0.248)	(0.316)	(0.308)	(0.062)	(0.079)	(0.100)

Notes: Standard errors are in parentheses

Table B.3. Means of Fraction of Worker in Non-agriculture and Share of Working Hours in Non-agriculture by Cohort and Program Intensity

	Fraction of worker in non-agriculture			Share of working hours in non-agriculture		
	Program intensity in region of birth			Program intensity in region of birth		
	High	Low	Difference	High	Low	Difference
Panel A: Cohort Experiment:						
HHMs aged 15-27	0.109	0.112	-0.003	0.134	0.134	0
	(0.007)	(0.012)	(0.014)	(0.010)	(0.016)	(0.019)
HHMs aged 33-38	0.194	0.234	-0.041	0.236	0.272	-0.036
	(0.013)	(0.018)	(0.022)	(0.017)	(0.023)	(0.029)
Difference	-0.085	-0.122	0.038	-0.102	-0.138	0.036
	(0.014)	(0.020)	(0.025)	(0.018)	(0.026)	(0.032)
Panel B: Cohort Control						
HHMs aged 33-38	0.194	0.234	-0.041	0.236	0.272	-0.036
	(0.013)	(0.018)	(0.022)	(0.017)	(0.023)	(0.029)
HHMs aged 39-51	0.176	0.175	0.001	0.217	0.21	0.008
	(0.013)	(0.016)	(0.020)	(0.017)	(0.021)	(0.026)
Difference	0.018	0.06	-0.042	0.018	0.062	-0.044
	(0.019)	(0.024)	(0.025)	(0.024)	(0.031)	(0.039)

Notes: Standard errors are in parentheses

Table B.4. Effect of the Program on Education, HH Labor Supply, & Earning: Coefficients of the Interaction between Interactions between Cohort Dummies and the Number of School Constructed per 1,000 Children in the Region of Birth

	Dependent variable:							
	Years of Education		Frac. Work in Non-Agri		Share of Work Hours in Non-Agri		Log(HH Earning)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Experiment of Interest: majority HHMs aged 15-27 or aged 33-38								
Whole Sample	0.215***	0.317***						
	(0.0485)	(0.0523)						
Observations	9234	8912						
Sample of valid wage	0.712***	0.606***	0.0251**	0.0227*	0.0339**	0.0317**	0.129***	0.133***
	(0.172)	(0.180)	(0.0115)	(0.0118)	(0.0146)	(0.0151)	(0.0443)	(0.0455)
Observations	1712	1698	1712	1698	1700	1686	1712	1698
Panel B: Experiment of Interest: majority HHMs aged 33-38 or aged 39-65								
Whole Sample	0.0300	0.0574						
	(0.0521)	(0.0581)						
Observations	6343	6141						
Sample of valid wage	0.350	0.241	-0.000417	-0.00172	0.00655	0.00581	-0.0280	-0.0482
	(0.235)	(0.236)	(0.0176)	(0.0181)	(0.0239)	(0.0247)	(0.0657)	(0.0688)
Observations	1107	1096	1107	1096	1096	1085	1107	1096
Control variables:								
(# HHM per cohort)*Enrollment rate	N	Y	N	Y	N	Y	N	Y

Notes: All specifications include number of HHMs per cohort, number of HHMs per region of birth, number of HHMs per cohort an d the number of children in the region of birth (in 1971). Robust standard errors are in parentheses.

Table B.5. Effect of the Program on Outcomes: Coefficients of the Interactions Between Dummies Indicating Age in 1995 And the Number of Schools Constructed per 1,000 Children in Region of Birth

	Years of Education		Fraction of Worker in Non-Agriculture Sector		Share of Work Hours in Non-Agriculture Sector		Log(Household Earning Nominal)	
Age	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
33	0.0102	0.0554	0.00110	-0.00305	-0.0173	-0.0226	0.0649	0.0471
	(0.293)	(0.300)	(0.0182)	(0.0192)	(0.0260)	(0.0272)	(0.0636)	(0.0644)
32	0.598***	0.413*	0.0280*	0.0284*	0.0245	0.0250	0.0488	0.0132
	(0.223)	(0.237)	(0.0154)	(0.0169)	(0.0183)	(0.0241)	(0.0480)	(0.0522)
31	0.561**	0.485*	0.0346**	0.0428***	0.0280	0.0345	0.0469	0.0262
	(0.275)	(0.272)	(0.0141)	(0.0152)	(0.0204)	(0.0220)	(0.0563)	(0.0587)
30	0.338	0.257	0.00374	0.0117	-0.0128	-0.00796	0.0254	0.0101
	(0.207)	(0.212)	(0.0143)	(0.0148)	(0.0188)	(0.0197)	(0.0461)	(0.0491)
29	0.516**	0.472*	0.0263	0.0297*	0.00985	0.0127	0.0237	0.000848
	(0.246)	(0.252)	(0.0161)	(0.0167)	(0.0248)	(0.0257)	(0.0521)	(0.0543)
28	0.532*	0.518*	0.0323*	0.0360**	0.0140	0.0168	-0.00791	-0.00648
	(0.291)	(0.289)	(0.0172)	(0.0180)	(0.0172)	(0.0178)	(0.0566)	(0.0591)
27	0.613***	0.515***	0.0218*	0.0191	0.0330**	0.0302*	0.0726*	0.0653*
	(0.183)	(0.178)	(0.0122)	(0.0124)	(0.0156)	(0.0159)	(0.0386)	(0.0393)
26	0.641***	0.594***	0.0217*	0.0254*	0.0351**	0.0402**	0.0995**	0.104**
	(0.167)	(0.175)	(0.0125)	(0.0135)	(0.0174)	(0.0188)	(0.0413)	(0.0443)
25	0.507***	0.482***	-0.00394	0.000182	0.00356	0.00778	0.0486	0.0509
	(0.168)	(0.161)	(0.0111)	(0.0116)	(0.0140)	(0.0149)	(0.0361)	(0.0380)
24	0.625***	0.681***	0.0134	0.0139	0.0210	0.0205	0.134***	0.128**
	(0.160)	(0.164)	(0.0111)	(0.0120)	(0.0142)	(0.0154)	(0.0484)	(0.0524)
23	0.712***	0.738***	0.0184	0.0228	0.0273*	0.0298*	0.108**	0.121***
	(0.162)	(0.182)	(0.0128)	(0.0145)	(0.0157)	(0.0177)	(0.0437)	(0.0456)
22	0.822***	0.959***	0.0292**	0.0307**	0.0423***	0.0462**	0.0638	0.0724
	(0.198)	(0.178)	(0.0128)	(0.0144)	(0.0163)	(0.0182)	(0.0417)	(0.0468)
21	0.668***	0.748***	0.0291**	0.0360**	0.0369**	0.0431**	0.130***	0.142***
	(0.216)	(0.215)	(0.0139)	(0.0161)	(0.0182)	(0.0214)	(0.0411)	(0.0454)
20	0.501***	0.547***	0.0305**	0.0295*	0.0405**	0.0382*	0.131***	0.146***
	(0.160)	(0.195)	(0.0133)	(0.0164)	(0.0164)	(0.0205)	(0.0443)	(0.0480)
19	0.581**	0.448	0.0358	0.0257	0.0413	0.0298	0.0708	0.0496
	(0.284)	(0.290)	(0.0240)	(0.0264)	(0.0298)	(0.0335)	(0.0629)	(0.0658)
18	0.921***	0.910***	0.0190	0.0264	0.0125	0.0179	0.0431	0.0658
	(0.249)	(0.248)	(0.0215)	(0.0198)	(0.0265)	(0.0261)	(0.131)	(0.134)
17	1.383***	0.930**	0.0402*	0.0162	0.0527*	0.0206	0.166**	0.127
	(0.263)	(0.377)	(0.0231)	(0.0339)	(0.0302)	(0.0455)	(0.0746)	(0.0780)
16	0.841	-0.339	0.0726	0.0129	0.0614	-0.0420	-0.0978	-0.120
	(1.091)	(0.990)	(0.0527)	(0.0681)	(0.0690)	(0.0866)	(0.223)	(0.243)
N	2833	2812	2833	2812	2813	2792	2833	2812
R-sq	0.367	0.387	0.309	0.321	0.294	0.303	0.325	0.337
F interaction	3.007	2.666	1.353	1.173	1.260	1.056	1.356	1.255
Control variables: (# HHM per cohort)*Enrollment rate	N	Y	N	Y	N	Y	N	Y

Notes: All specifications include number of HHMs per cohort, number of HHMs per region of birth, and interaction between number of HHMs per cohort and the number of children in the region of birth (in 1971). Robust standard errors are in parentheses

Table B.6. Effect of Education on Outcomes: OLS & 2SLS Estimates

Method	Instrument		(1)	(2)
Panel A: Log(Household earning nominal)				
OLS			0.0612***	0.0599***
			(0.00503)	(0.00509)
		Observation	2833	2812
2SLS	(HHM aged 15-27)*program intensity		0.192***	0.200***
			(0.0611)	(0.0614)
		Observation	2833	2812
2SLS	Year of birth dummies*program intensity		0.109***	0.113***
			(0.0388)	(0.0399)
		Observation	2833	2812
Panel B: Fraction total worker in non-agriculture sector				
OLS			0.0172***	0.0176***
			(0.00147)	(0.00151)
		Observation	2833	2812
2SLS	(HHM aged 15-27)*program intensity		0.0173	0.0158
			(0.0141)	(0.0143)
		Observation	2833	2812
2SLS	Year of birth dummies*program intensity		0.0380***	0.0387***
			(0.0104)	(0.0111)
		Observation	2833	2812
Panel C: Share of Work Hours in Non-Agriculture Sector				
OLS			0.0198***	0.0200***
			(0.00187)	(0.00193)
		Observation	2813	2792
2SLS	(HHM aged 15-27)*program intensity		0.0425**	0.0394**
			(0.0170)	(0.0180)
		Observation	2813	2792
2SLS	Year of birth dummies*program intensity		0.0471***	0.0486***
			(0.0130)	(0.0138)
		Observation	2813	2792
Control variables:				
(# HHM per cohort)*Enrollment rate			N	Y

Notes: All specifications include number of HHMs per cohort, number of HHMs per region of birth, and interaction between number of HHMs per cohort and the number of children in the region of birth (in 1971). Robust standard errors are in parentheses.

Table B.7. Effect of Education on Outcomes: LIML Estimates

Method	Instrument		(1)	(2)
Panel A: Log(Household earning nominal)				
LIML	Year of birth dummies*program intensity		0.133**	0.137**
			(0.0593)	(0.0586)
		Observation	2833	2812
Panel B: Fraction total worker in non-agriculture sector				
LIML	Year of birth dummies*program intensity		0.0461***	0.0457***
			(0.0150)	(0.0152)
		Observation	2833	2812
Panel C: Share of Work Hours in Non-Agriculture Sector				
LIML	Year of birth dummies*program intensity		0.0545***	0.0553***
			(0.0170)	(0.0175)
		Observation	2813	2792
Control variables:				
(# HHM per cohort)*Enrollment rate			N	Y

Notes: All specifications include numb er of HHMs per cohort, number of HHMs per region of birth, and interaction between number of HHMs per cohort and the number of children in the region of birth (in 1971). Robust standard errors are in parentheses

THE NATIONAL TEAM FOR THE ACCELERATION OF POVERTY REDUCTION

Office of the Vice President's Secretariat
Jl. Kebon Sirih Raya No.14, Jakarta Pusat, 10110

Telephone : (021) 3912812
Facsmili : (021) 3912511
Email : info@tnp2k.go.id
Website : www.tnp2k.go.id