Can Unconditional In-kind Transfers Keep Children Out of Work and in School? Evidence from Indonesia*

Danusha Jayawardana[1]
Centre for the Business and Economics of Health, University of Queensland, Australia

Nadezhda V. Baryshnikova
School of Economics, University of Adelaide, Australia

Ngoc Thien Anh Pham
School of Architecture and Built Environment, University of Adelaide, Australia

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[1] *Corresponding Author (Present Address):* Danusha Jayawardana, Centre for the Business and Economics of Health, University of Queensland, Brisbane, QLD 4072, Australia. E-mail: d.jayawardana@uq.edu.au .Tel: +61 7 334 61955

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Abstract

Child labour is a global issue which creates a need for evidence-based interventions such as cash and in-kind transfers. However, there is limited evidence about the effect of in-kind transfers on child labour, impeding policy development. We address this gap by examining the impacts of an unconditional in-kind transfer, a nation-wide subsidised rice program, on child labour and schooling using longitudinal household survey data from Indonesia. To identify the causal effect, we employ coarsened exact matching with difference-in-differences estimator. The results indicate that the program is effective in decreasing the probability of working for boys though it does not have a significant impact on the probability of schooling. However, as an unconditional in-kind transfer, its ability to decrease child work for boys, especially of those who are both working and attending school, provides an important policy implication on how a food subsidy program can indirectly influence child wellbeing.

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Keywords: Child labour; Schooling; Food subsidy; Raskin; Indonesia; Coarsened exact matching

1 Introduction

The International Labour Organisation estimates that one in ten children aged 5-17 years are in child labour, accounting for a total of 152 million children worldwide.[1] Nearly half of these children (73 million) are engaged in hazardous work leading to adverse consequences on their wellbeing. Child labour also constitutes the violation of children's right to education, as 32 per cent of those in child labour are out of school and are completely deprived of education (ILO 2017). These figures reveal that child labour is an ongoing concern, and thus calls for evidence on the impact of policy interventions relevant to child labour and schooling (ILO 2017). As household vulnerabilities connected with poverty are considered to be the root cause of child labour (Basu & Van 1998, Edmonds 2007, Jafarey & Lahiri 2005), social protection programs are deemed as a potential mechanism in addressing it (ILO 2017). There are various social

[1] Child labour is defined as: "(i) any economically active child under the age of 12, (ii) children in the 12–14 age category engaged in productive activities that do not fall under permissible light work, (iii) children aged 17 and younger engaged in activities that are designated as 'hazardous' (affecting the child's safety, physical and mental development) or the 'worst forms of child labour (e.g. children in bondage or forced labour, commercial sexual exploitation, illicit activities and armed conflict, among others)" (Dammert et al. 2018, p. 106). The term 'economically active' usually refers to both paid and unpaid market work. (See Dammert et al. (2018) for further details.) Following prior empirical literature, in this study, we adopt a general definition of child labour as 'children aged 5 to 14 years who are economically active' (as in De Silva & Sumarto (2015), Gee (2010).

protection tools that ensure income security and welfare of poor households. From a child labour perspective, instruments such as cash and in-kind transfers, social health protection and public employment programs are stated to be most relevant (ILO 2013), even though the explicit objective of implementing those is not to reduce child labour.

This paper examines the impact of an 'unconditional' in-kind transfer, a food subsidy, on the labour supply and schooling of children. To this end, we consider one of the largest subsidised food programs known as 'Raskin' (or rice for the poor) that was in operation in Indonesia till 2017.[2] By relying on a rich data source - Indonesia Family Life Survey (IFLS), we seek to answer two specific questions: (1) Does the food subsidy program provide a sufficient incentive for households to reduce the supply of child labour? (2) Does it induce an increase in schooling of children?

This study contributes to the growing literature on policy interventions in improving the welfare of children. First, it adds to the evidence on the effectiveness of social protection instruments on child labour and schooling, with reference to a subsidised food program in a less developed country. There is a plethora of studies that have examined the impact of cash transfers - both conditional and unconditional.[3] [4] Nevertheless, there is little empirical evidence on the effects of other social protection tools on child labour, impeding policy development (de Hoop & Rosati 2013, Edmonds 2007). Particularly, when considering in-kind transfers, a small number of studies have only looked at the impact of 'conditional' in-kind transfers such as food for education programs and school vouchers on child labour and schooling.[5] However, the limited evidence on such interventions is also inconclusive (ILO 2013). According to the theoretical literature, unconditional in-kind transfers could be a potential resource for eradicating child labour due to two reasons: (1) food-based social assistance programs have a significant influence on households by easing their budget constraints (Adelman et al. 2008, Alderman et al. 2018), and thereby reducing the need to send children to work, (2) food programs can lead to a stronger effect on

[2]Since 2017, Raskin has been integrated with a food electronic voucher program known as *Bantuan Pangan Non Tunai* or BPNT (OECD 2019).

[3]Both conditional and unconditional cash transfers provide households with an income transfer to address issues and vulnerabilities associated with poverty. However, in contrast to unconditional cash transfers, conditional cash transfers are given on a certain condition that the individuals receiving the transfer should fulfil specific requirements. For instance, maintaining regular attendance in school or ensuring regular health checkups (De Hoop & Rosati 2014*a*) - on child labour and schooling in various country contexts.

[4]See Covarrubias et al. (2012), De Janvry et al. (2006), Edmonds & Schady (2012), Gee (2010), Levy et al. (2007) among many others. For a systematic review of cash transfers on child labour, see De Hoop & Rosati (2014*a*)

[5]See Angrist et al. (2002), De Hoop & Rosati (2014*b*), Kazianga et al. (2009), Ravallion & Wodon (2000). Cheung & Berlin (2015) and Meng & Ryan (2010) study the impact of food for education programs on schooling outcomes only.

child labour supply and education as they improve the nutrition status of children (Dammert et al. 2018).[6] Therefore, to the best of our knowledge, this is the first study that seeks to examine the effect of an unconditional food subsidy on the labour supply and schooling of children.

Second, though the Raskin program was introduced in 1998, the impact on child labour does not appear to have been studied (Gupta & Bihong 2018). Raskin was initially implemented as an emergency food security program, and one of the largest social protection programs in Indonesia. Given the magnitude of the program, it is interesting to examine to what extent such a well-established program could address vulnerabilities associated with poverty. To the best of our knowledge, this is the first evaluation of the Raskin program at the microeconomic level, which particularly looks at child wellbeing with regard to child labour and schooling.

Our study also differs from the existing impact evaluation studies in terms of methodology. The main identification issue arises from selection bias due to non-random distribution of the subsidy and unobserved heterogeneity. To address this, we implement the relatively new method of coarsened exact matching (CEM) with the difference-in-differences (DD) estimator. Compared to other commonly-used matching techniques, CEM has several desirable properties (as discussed in Section 4). Furthermore, combining CEM with DD provides an unbiased estimator which is robust to inherent unobservables (Gertler et al. 2011).

The results reveal that the subsidised rice program in Indonesia is effective in decreasing the probability of working for boys though there is no impact on the outcomes of girls. Specifically, we find that the Raskin program significantly decreases the likelihood of working for boys who engage in both working and schooling, by 0.9 percentage points (or approximately 11 percent of the mean). Additionally, we provide indicative evidence that children at pre and primary school age (5-12 years) and children residing in rural areas are more likely to reduce the probability of working due to the receipt of Raskin. From a policy perspective, these findings provide important implications on how a food subsidy program can indirectly influence child wellbeing.

The remainder of the paper proceeds as follows. Section 2 provides a brief background on the estimates of child labour and Indonesia's Raskin program. Section 3 describes the data source and the variables used in the study. Section 4 outlines the methodology, while Sections 5 and 6 present the main results and a discussion, respectively. The concluding remarks are given in Section 7.

[6] Currie & Gahvari (2008) provide a review on the rationale behind this.

2 Background

2.1 Child Labour and Education in Indonesia

With more than 270 million people, Indonesia is the fourth most populous nation in the world. As one of the largest economies in Southeast Asia, Indonesia has experienced a decline in economic growth since 2012, owing to the end of the export boom. In 2019, the country was ranked 135th among all the countries in the world in terms of its GDP per capita (in purchasing power parity), which was $11,812.[7] Indonesia struggles with many problems such as poverty, unemployment, inequality and corruption. However, as a country with a predominantly young population,[8] these problems affect the children most. According to UNICEF (2013), Indonesia has a high incidence of child labour, child marriage, sexual exploitation and lack of birth registration which inevitably lead to an adverse impact on child wellbeing.

Education is compulsory for Indonesian children aged seven to eighteen years. As a result, the country has made significant progress in ensuring more than 95 per cent of children aged between 7 to 12 years are attending primary or junior secondary school. Despite high enrolment rates, almost 15 percent of the children do not complete lower-secondary education (UNICEF 2020). Low transitions from primary to secondary school is mainly seen among children from low-income families and rural areas. On average, 1.8 million children were out of secondary school in 2015 (BAPPENAS & UNICEF 2017). One of the main reasons for high drop-out rates is poverty, forcing the children to engage in some form of child labour while depriving them of their right to education.

Child labour is a widely observed practice in Indonesia. Across Indonesia, 6.9 per cent of children were in child labour in 2009, and alarmingly, close to half of these child workers are engaged in hazardous work (BAPPENAS & UNICEF 2017). Though most of the working children attend school, it certainly limits the time available for education hindering their ability to reach the potential. Based on the 2015 Programme for International Student Assessment (PISA), less than half of the students aged 15 years achieve a minimum proficiency in reading and mathematics (BAPPENAS & UNICEF 2017). Therefore, as a developing country, eliminating child labour while increasing educational attainment is crucial for the country's sustainable economic growth and development.

[7] As cited in Central Intelligence Agency (16 June 2021) Retrieved from https://www.cia.gov/the-world-factbook/countries/indonesia/economy

[8] Almost one-third of the Indonesian population (84 million) accounts for children under the age of 18 years.

2.2 The Raskin Program

In order to address the problem of poverty as well as issues arising out of it, several social protection programs are implemented by the government of Indonesia. 'Raskin' (or rice for the poor) is one of the cross-sectoral national programs intended to alleviate poverty and provide social protection which is funded by the central government. Raskin was first introduced in 1998 as an emergency food security program in the form of subsidised rice assistance prioritised to poor and vulnerable households.[9] However, at present, it has become a permanent nation-wide social protection program targeted at the poorest 40 per cent of the households in Indonesia with the largest government budget allocation (Banerjee et al. 2016, Trimmer et al. 2018, The World Bank 2012). The targeted households are selected using a proxy-means test. In addition to the income of the household, factors such as the number of toddlers and school-age children in the household, whether the household head is a female and the physical condition of the house are also considered in determining the eligibility for the program.[10] However, there is no specific selection criterion for the program as it has changed several times based on the data sources used (Trimmer et al. 2018). In general, there is little control by the central government in monitoring and determining the eligibility, since the local officials have substantial authority in implementing the program at the local level (Banerjee et al. 2016, The World Bank 2012). As a result, Raskin has been criticised for considerable 'leakages' where eligible households obtain less than 35 per cent of the intended subsidy (see Banerjee et al. 2016, Trimmer et al. 2018).

The rationale of the program is to reduce the burden of household expenditure on food. In poor households, the food expenditure constitutes the largest share of its total expenditure which can range from 45 to 77 per cent (Banerjee & Duflo 2011). As rice is considered to be the staple food in Indonesia, an increase in the price of rice can adversely affect the purchasing power of the poor. This is because rice accounts for almost a quarter of the average monthly expenditure in poor households (Sumarto & Wenefrida 2008, Trimmer et al. 2018). Hence, by providing a certain quantity of rice at a subsidised price could lead to ease the budget constraints of poor households vulnerable to child labour (ILO 2013).

This program allows the beneficiary households to purchase up to a maximum of 15 kilograms of medium quality rice per month at a subsidised rate of one-fifth of the market price (Banerjee

[9] Initially, this program was named as Operasi Pasar Khusus (OPK) meaning Special Market Operation. The government changed its name to 'Raskin' (rice for poor families) in 2002. In 2016, it was again renamed as Rastra (literally prosperous rice).

[10] As cited in Rastra - Rice for Family Welfare (25 July 2018) Retrieved from http://raskin.bangda.kemendagri.go.id /tentang-raskin/tujuan-raskin.html.

et al. 2016). To put these numbers into perspective, the intended subsidy value of the allocation of 15 kilograms of rice accounts for about five per cent of the monthly consumption expenditure of those households who are below the poverty line. In terms of the effectiveness of the program, a 2005 study reports that the subsidy increased household consumption by 4.4 percent, while reducing the likelihood of falling below the overall poverty line by 3.8 percent (Sumarto et al. 2005). Raskin also benefits the children of poor households. Specifically, almost half of the children live in a household that receives Raskin rice (BAPPENAS & UNICEF 2017). As a food subsidy, Raskin improves the nutrition status of children. This, in turn, could lead to important implications on reducing child labour and increasing schooling.

3 Data and Measures

We use data from the Indonesia Family Life Survey (IFLS). The IFLS is an ongoing longitudinal survey which is administered by the RAND organisation. Currently, there are five waves covering years 1993 (IFLS 1), 1997/98 (IFLS 2 and IFLS2+), 2000 (IFLS 3), 2007 (IFLS 4) and 2014 (IFLS 5). The IFLS consists of several unique features. First, it is one of the few large-scale population-based surveys in operation for more than 20 years, especially in the context of a developing country. Second, in terms of representation, IFLS is a sample drawn from 13 of the country's 26 provinces which consists of 83 per cent of the Indonesian population (Strauss et al. 2009). In IFLS1, data were collected from over 22,000 individuals in 7,224 households. By 2014, the numbers had increased to 50,000 individuals from 17,000 households. Third, there is over 85 per cent re-contact rate in each wave leading to high quality of data with relatively low attrition (Strauss et al. 2016). Finally, IFLS is a multipurpose survey which collects information at the individual, household and community level. Therefore, it includes data on a range of topics such as demographics, household consumption patterns, labour market outcomes, health outcomes, schooling, migration and receipt of social transfers which facilitate the conduct of extensive research with regard to various aspects.

3.1 Sample and Variable Definitions

For this study, we use data from 1997, 2000, 2007 and 2014 waves of the IFLS.[11] Our sample is restricted to children between the age of 5 to 14 years old, as child labour is generally defined as children aged 5 to 14 years who are economically active. The term 'economically active' refers to the participation in the production of economic goods and services, meaning it can be either for wages or as unpaid work performed as part of family business (Edmonds 2007). Therefore, the supply of labour for household activities and chores are not considered as child labour.[12]

In our study, there are two main outcome variables of interest - child labour and schooling. The data in relation to these is extracted from the child module of the IFLS, which is administered to children below 15 years old.[13] Constructed as binary variables, child labour takes on a value of 1 if the child has ever worked and 0 otherwise.[14] Similarly, schooling takes on a value of 1 if the child is currently in school and 0 otherwise. The treatment variable used in this study is a dummy variable which is assigned a value of 1 if the household has ever bought subsidised rice from Raskin program during the past year and 0 otherwise.

We control for an extensive set of socio-demographic covariates that are well established in the literature. Specifically, we include child's age, religion, parental characteristics such as parent's age, marital status, occupation and educational attainment as control variables. Furthermore, we also include variables on the household's demographics, such as household size, dependency ratio, the gender of the household head and ownership of assets. Standard indicators such as access to electricity, water, proper sanitation and source of fuel are included as housing characteristics. The monthly per capita expenditure, which is constructed by adding both food and non-food expenditure, is used to proxy for household income. Moreover, we also consider regional heterogeneity by including a dummy variable for urban area as well as provincial dummy variables in our estimation. A complete list of variables used in this study is presented in Table S1 in the supplementary materials.

As the Raskin program began in 1998, there is one pre-exposure period: 1997 and three

[11] We do not use data from the first wave IFLS1 (1993) due to the differences in the format of the questions and the considerable number of missing observations on parental information.

[12] The data on child' s participation in household chores are provided only in 2007(IFLS 4) and 2014 (IFLS 5) waves.

[13] This means the respondent is usually a child below 15 years old. Sometimes the questions are answered by an older sibling or another household member such as mother, aunt or grandmother who deemed the most knowledgeable source of information for the child.

[14] From the year 2000 onwards, the child module contains separate questions on the child's work status for the last month, week and ever as well as type of work. However, to ensure consistency of the child labour measure across different waves, we have used the ever worked participation.

potential post-exposure periods of 2000, 2007 and 2014. However, since there is a seven-year gap between the subsequent waves after year 2000, the use of panel data leads to a loss of significant number of observations. This is because children who are eight years or older in 2000 are excluded from the child modules in 2007 and 2014 waves as they would be above 15 years of age. Therefore, we use pooled cross-section data to maximise the number of observations.[15] To employ DD technique, we restrict our pooled sample to households that are observed in both pre-treatment and at least once in any of the post-treatment periods (as Raskin is provided to eligible households rather than individuals). Accordingly, our estimating sample consists of 4,370 children (Girls - 2,193 and Boys - 2,177) between the age of 5 to 14 years. Approximately 63 per cent of the children are from a household that has received Raskin at least once in a year.

3.2 Descriptive Statistics

Table 1 presents the summary statistics. The sample is balanced in terms of gender, and the average age is 9.5 years. Half of the children are from a rural household. Around 20 per cent of the children are in poverty as reflected by the household characteristics such as poor sanitation and use of nearby river, land or sea as the toilet. Approximately, eight per cent of children are engaged in work which corresponds to the actual percentage of child labour in Indonesia. On average, 83 per cent of children are currently attending school.

To understand the context in terms of those who receive Raskin and those who do not, we derive the descriptive statistics by the control and treatment assignment. Table 1 reports the mean values across groups and the results of t-tests on the difference of means. When considering the mean values of the outcomes variables of child labour and schooling, it is evident that there is a significant difference between the control and treatment groups. As anticipated, the households that receive Raskin have a higher proportion of children involved in child labour and a lower proportion of children in school. Furthermore, as expected, there are also significant differences across the control and treatment groups, especially in terms of household and parent characteristics. This is because as Raskin is targeted at the poorest households, the two groups are likely to differ in variables that capture the aspects of poverty. In general, the households that receive Raskin are poorer and less educated. Such significant differences in the two groups

[15] The use of panel data from waves 2 (1997) and 3 (2000) results in a small sample size as the majority of the households with working children in wave 3 have missing data on the receipt of Raskin.

may imply that there is non-random selection into treatment, and thus the control group may not act as a perfect counterfactual to the treatment group.

Table 1: Summary Statistics

Variables	Full Sample N = 4370		Control Group Raskin = 0 N = 1631		Treatment Group Raskin = 1 N = 2739		Mean Difference
	Mean	SD	Mean	SD	Mean	SD	
Child-working	0.08	0.26	0.06	0.24	0.08	0.28	0.02***
Child-still in school	0.83	0.38	0.84	0.37	0.82	0.38	-0.02***
Child Characteristics							
Child-gender	0.49	0.50	0.48	0.50	0.49	0.50	0.01
Child-age	9.52	2.88	9.44	2.88	9.56	2.88	0.13***
Child-religion-Islam	0.90	0.29	0.86	0.35	0.93	0.26	0.07***
Household Characteristics							
Urban	0.48	0.50	0.66	0.47	0.40	0.49	-0.26***
Household size	5.41	1.87	5.41	1.87	5.41	1.86	0.00
Dependency ratio	1.13	0.70	1.12	0.65	1.14	0.73	0.02**
HHH female	0.12	0.33	0.10	0.30	0.13	0.34	0.03***
Assets per capita (ln)	15.20	1.92	15.97	1.99	14.85	1.78	-1.12***
Per capita expenditure (ln)	12.49	1.20	12.98	1.26	12.27	1.10	-0.71***
Own business	0.43	0.49	0.46	0.50	0.41	0.49	-0.05***
Own farmland	0.32	0.47	0.28	0.45	0.34	0.47	0.07***
Electricity	0.92	0.27	0.95	0.21	0.91	0.29	-0.05***
Water	0.27	0.44	0.39	0.49	0.22	0.41	-0.17***
Cook - firewood	0.38	0.49	0.19	0.39	0.46	0.50	0.28***
Toilet - river/land/sea	0.22	0.41	0.09	0.28	0.28	0.45	0.20***
Poor sanitation	0.23	0.42	0.17	0.38	0.25	0.44	0.08***
Parent Characteristics							
Mother-age	36.62	6.81	36.81	6.17	36.54	7.09	-0.27**
Father - age	41.40	7.99	41.18	7.02	41.51	8.42	0.33**
Mother married	0.97	0.18	0.97	0.17	0.96	0.18	-0.01*
Mother - paid occupation	0.42	0.49	0.39	0.49	0.43	0.50	0.04***
Father - paid occupation	0.87	0.34	0.88	0.33	0.87	0.34	-0.01**
Mother - elementary	0.47	0.50	0.28	0.45	0.56	0.50	0.28***
Mother - junior	0.18	0.39	0.18	0.38	0.18	0.39	0.00
Mother-senior	0.20	0.40	0.35	0.48	0.13	0.33	-0.22***
Mother - tertiary	0.06	0.24	0.15	0.36	0.02	0.13	-0.13***
Father - elementary	0.44	0.50	0.24	0.42	0.53	0.50	0.30***
Father - junior	0.16	0.36	0.15	0.36	0.16	0.37	0.01
Father - senior	0.25	0.43	0.38	0.49	0.18	0.39	-0.20***
Father - tertiary	0.08	0.27	0.19	0.39	0.03	0.17	-0.16***
Mother - highest edu	0.61	0.49	0.74	0.44	0.55	0.50	-0.19***
Father - highest edu	0.61	0.49	0.73	0.44	0.55	0.50	-0.19***

Note: The significant values are obtained from the test of significance in the difference of means between the treatment and controls groups for each of the variables. *** p<0.01, ** p<0.05, * p<0.1.

4 Empirical Strategy

Our estimation strategy begins with establishing a causal relationship between receiving Raskin and the outcome variables of child labour and schooling. Given that this study is based on non-experimental data, identifying the treatment effect is not straightforward as the variables that determine the program participation would also affect the decision of child labour and schooling.

In other words, the households that are meant to receive Raskin are in fact the poor households with a high probability of child work and low schooling. This means that the treated and the control groups differ in terms of other covariates leading to selection bias (Caliendo & Kopeinig 2008, Heckman et al. 1998).

Previous studies on program evaluation have relied on techniques such as randomised controlled trial (RCT), difference-in-differences (DD), regression discontinuity design (RDD), matching or a combination of aforesaid methods to deal with sample selection bias arising from non-random assignment of the treatment and unobserved heterogeneity. In this study, we use a matching technique combined with difference-in-differences (matched DD) to estimate the treatment effect. We select matching as the identification strategy for two reasons. First, the Raskin program does not have any clear assignment rules such as an eligibility score (Trimmer et al. 2018) that explain why some households received the rice subsidy and others did not. Second, the availability of a rich data source that contains data on both households that received Raskin and that did not, enables us to estimate a control group that has as similar as possible characteristics as the treatment group (Gertler et al. 2011).

There are several types of matching methods that are widely applied in the empirical literature. These methods differ primarily on the technique that is used to find at least one control unit for each of the treated units that is similar on the covariates. However, the major limitation of using the common matching methods, such as propensity score and Mahalanobis matching, lies in the fact that they do not necessarily guarantee a reduction of imbalance (i.e. differences between the treated and control groups) in a given data set. For instance, the application of propensity score matching leads only to an improvement of balance on some covariates while decreasing the balance on other covariates (Iacus et al. 2012). Moreover, these methods depend on a set of unverifiable assumptions about the data generation process and despite such assumptions, its properties only hold on average across samples. Therefore, the use of these techniques can increase both model dependence and imbalance (Iacus et al. 2012); meaning that they are ad-hoc and inefficient (Blackwell et al. 2009). As a solution for these problems we employ coarsened exact matching (CEM) proposed by Iacus, King & Porro (2012) which is explained in detail below.

4.1 Coarsened Exact Matching

Following Iacus et al. (2012), the basic idea of CEM can be explained in three steps. First, it temporarily coarsens each covariate to reduce the differences between the treated and control groups in terms of observables. Second, it applies exact matching to the coarsened data and generates weights to each matched unit. Finally, the original values of the matched units along with the CEM weights are used to estimate the average treatment effect on the treated (ATT).

As an exact matching technique, CEM belongs to the class of monotonic imbalance bounding (IMB) matching methods. Therefore, in contrast to other matching methods, CEM balances between the control and treatment groups chosen ex-ante, and adjusting the imbalance on one covariate does not affect the balance of any other (Blackwell et al. 2009). It is shown that "CEM dominates commonly used existing matching methods in its ability to reduce imbalance, model dependence, estimation error, bias, variance, mean square error, and other criteria" (Iacus et al. 2012, p.2). Therefore, contrary to the previous empirical literature on child labour, we use CEM to deal explicitly with the treatment selection bias owing to its desirable features.

4.2 Matched Difference-in-Differences

The existence of data in relation to before and after intervention allows us to combine the coarsened exact matching with the difference-in-differences (DD) technique. Though the use of panel data would have been ideal in this setting, it is not possible due to the seven-year gap between the subsequent waves after year 2000. Specifically, children who are eight years or older in 2000 are excluded from the child modules in 2007 and 2014 waves as they would be above 15 years of age, resulting in a loss of significant number of observations. However, panel data are not necessary for estimating DD (Ravallion 2007).

To employ the DD technique, we first restrict our sample to households that are observed in both pre-treatment and at least once in any of the post-treatment periods. Since there is no specific rule followed in determining which households are eligible for the receipt of Raskin in the pre-treatment period, we use post-treatment period data to identify the treated and control households in the pre-treatment period. Combining coarsened exact matching with DD, allows us to offset any limitations of matching as an identification strategy and thereby to increase the robustness of the estimated counterfactual (Gertler et al. 2011).

4.3 Empirical Model

We employ a bivariate probit model to estimate the effect of Raskin on the likelihood of child labour and schooling. Given that both outcomes are denoted as binary variables, a bivariate probit model allows us to model child labour and schooling jointly as both are interrelated decisions that compete for a child's time. In contrast to a univariate probit model, a bivariate probit model can capture any interrelation between work and schooling by identifying the correlation between the unobservables of the two outcome variables.

The bivariate DD model is derived as follows:

$$Work^*_{1it} = \alpha_1 + \gamma_1 Raskin_{1it} + \delta_1 Post_{1it} + \beta_1 Raskin * Post_{1it} + \eta_1 \mathbf{X}'_{1it} + \varphi_{1i} + u_{1it} \quad (1)$$

$$Schooling^*_{2it} = \alpha_2 + \gamma_2 Raskin_{2it} + \delta_2 Post_{21i} + \beta_2 Raskin * Post_{21i} + \eta_2 \mathbf{X}'_{2it} + \varphi_{2i} + u_{2it} \quad (2)$$

where the observed outcomes are:

$$Work_{1it} = \begin{cases} 1, & \text{if } Work^*_{1it} > 0 \\ 0, & otherwise \end{cases} \quad (3)$$

$$Schooling_{2it} = \begin{cases} 1, & \text{if } Schooling^*_{2it} > 0 \\ 0, & otherwise \end{cases} \quad (4)$$

where $Work_{1it}$=1 if the child has ever worked in year t and 0 otherwise; $Schooling_{2it} = 1$ if the child is currently in school in year t and 0 if not.[16] $Raskin_{it}$ is a dummy variable that equals to 1 if the child i lives in a household that receives Raskin in year t and 0 otherwise. $Post_{it}$ is an indicator variable for the period after Raskin was introduced. This takes on a value of 1 for the years 2000, 2007 and 2014 and 0 for the year 1997. Our variable of interest is $Raskin * Post_{it}$ which equals to 1 if the child i lives in a household that receives Raskin in post period and 0 otherwise. Vectors $\mathbf{X}_{1it}$ and $\mathbf{X}_{2it}$ represent individual, parent and household covariates that affect child labour ($Work_{1it}$) and schooling decision ($Schooling_{2it}$), respectively. φ_i denotes the household fixed effects. The error terms u_{1it} and u_{2it} come from a bivariate normal distribution with $Cov\,[u_{1it}, u_{2it}|X_{1it}, X_{2it}]=\rho$

In the event where $\rho = 0$, the model collapses into two separate probit models for $Work_{1it}$ and $Schooling_{2it}$. If ρ is significant we can conclude that there is a correlation between the unobserved factors affecting both working and schooling. In such a case, the results of the univariate probit model would be inefficient and biased.

[16] $Work^*_{1it}$ and $Schooling^*_{2it}$ represent the latent variables of desire to work and attend school respectively.

5 Empirical Results

5.1 Coarsened Exact Matching Results

We begin our empirical analysis with coarsened exact matching (CEM). The first step of CEM is to select the control variables to be included in matching. In view of Heckman et al. (1998), all variables that can affect both treatment assignment and outcome should be included in the matching process to satisfy the assumption of strong ignorability. In this study, we match the treated and the control households based on the observable household characteristics that act as proxies for household's poverty level and thus leads to treatment assignment. This is because, as a food subsidy Raskin is targeted at the poorest households, which is determined by the level of household's income and welfare. Therefore, based on a probit estimation, we identify the significant covariates that determine Raskin and hence mimic the rules of eligibility into the program (see Table S2 in the supplementary materials). Accordingly, the covariates that are used for the coarsening process are, place of residence (urban or rural), household size, dependency ratio, per capita expenditure, ownership of business, access to electricity, whether the household purchases water, uses firewood for cooking, uses the nearby river, land or sea as the toilet and poor sanitation.[17]

The quality of the matching outcomes on pre-treatment data (i.e. wave 2) is diagnosed by an assessment of covariate balance. Table S3 in supplementary materials reports the results for both pre- and post-matching of the sample. According to Table S3, the overall multivariate imbalance decreases from 0.91 to 0.68. There is also a significant reduction in the univariate imbalance for each of the covariates. Further, the post-match mean differences between treated and control groups are almost negligible. This suggests that CEM has produced a reasonable match.

It is important to note that with coarsening, there would be some imbalance remaining in the matched data. Such imbalance can be controlled via a statistical model (Blackwell et al. 2009). Therefore, we use a bivariate probit model with a double-difference approach on the matched data to estimate the causal effect of Raskin on child labour and schooling. The weights generated by the CEM process are also included in the model, to equalise the number of treated

[17] As CEM is an exact matching technique, it is important to limit the number of covariates used in the coarsening process to avoid the curse of dimensionality. Therefore we select only those variables that are significant at 1% level. Since assets per capita and per capita expenditure are continuous variables, the inclusion of both leads to poor matching outcomes. Hence, out of these two, we select per capita expenditure for matching based on the magnitude of the effect. The use of assets per capita instead of per capita expenditure provide qualitatively similar results (see Section 6.1.2).

and control units within each stratum (Iacus et al. 2012).

5.2 Bivariate Probit Estimates

Panel A of Table 2 reports the main regression results of the bivariate probit model.[18] As a benchmark, we also report the estimates without the corresponding matching weights (Columns 1 and 2). The correlation coefficient between the error terms - rho (ρ) is significantly different from zero for both estimations at 1% level. This confirms the importance of employing the bivariate probit model as the estimations derived from a univariate model would be inefficient. As expected, its sign suggests that there is a negative correlation between the unobserved factors affecting the probability of working and attending school. In Table 2, the estimated coefficient of Raskin*Post is the treatment effect of receiving Raskin on the probability of working as a child or attending school. The DD specification without matching weights suggests that Raskin decreases the probability of child work while increasing the probability of schooling for children. However, these estimates could be biased due to possible selection bias. Thus, our preferred estimation is a bivariate probit model with matched DD. Column 3 shows that, on average, Raskin decreases the likelihood of child labour which is significant at 5% level. The estimated coefficient on schooling is positive though it is not statistically significant from zero.

Panel B of Table 2 presents the average marginal effects of the estimated coefficients from the matched DD model. Given that we use a bivariate probit model, there are four observed joint outcomes of work and school. Specifically, with regard to treatment effect, it is possible to identify the impact of receiving Raskin on the probability of: (1) working only, (2) schooling only, (3) both working and schooling (4) neither working nor schooling (idle). We find that receiving Raskin decreases the probability of engaging solely in child labour by 0.7 percentage points. Moreover, it decreases the probability of working and attending school simultaneously by 3.6 percentage points and increases the probability of only attending school by 6.2 percentage points. This implies that the decrease in child labour occurs among those children who are both working and schooling, resulting in a corresponding increase in the likelihood of schooling only.

As a goodness-of-fit measure, we report the comparison of the sample means of actual work-school outcomes versus the predicted probabilities after bivariate probit model. Table S4 in supplementary materials shows that the estimated model performs well such that the predicted

[18]The reported results are with robust standard errors clustered at both household level. Clustering at provincial, municipalities or subdistricts levels also provide quantitatively similar results.

probabilities are almost similar to that of actual sample means.

Table 2: Effect of Raskin on child labour and schooling

	DID without CEM weights		DID with CEM weights	
	(1)	(2)	(3)	(4)
Variables	Work	School	Work	School
Post treatment	1.089***	-0.640***	1.096***	-1.145***
	(0.128)	(0.083)	(0.309)	(0.227)
Raskin	0.250**	-0.142**	0.403**	-0.096
	(0.108)	(0.063)	(0.196)	(0.117)
Raskin*Post	-0.250**	0.217***	-0.505**	0.206
	(0.115)	(0.068)	(0.232)	(0.152)
Rho	-0.269***		-0.254***	
	(0.026)		(0.066)	

Panel B

Variables	(4) Work only (work=1 school=0)	(5) School only (work=0 school=1)	(6) Both (work=1 school=1)	(7) Idle (work=0 school=0)
Post treatment	0.025***	-0.204***	0.058***	0.121***
	(0.009)	(0.034)	(0.018)	(0.025)
Raskin	0.005**	-0.040**	0.028**	0.008
	(0.002)	(0.020)	(0.013)	(0.013)
Raskin*Post	-0.007**	0.062**	-0.036**	-0.019
	(0.003)	(0.026)	(0.018)	(0.017)
Observations	4,309	4,309	4,309	4,309

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parenthesis, clustered at household level. All estimations include the full set of control variables as given in Table S1, household, year and provincial fixed effects as well as the corresponding weights generated by CEM.

5.3 Gender Heterogeneity

Gender differences can play a significant role in determining participation in child work and schooling (Edmonds 2007). Therefore, we perform a heterogeneity analysis considering two separate subsamples of girls and boys to investigate whether the effect of Raskin varies based on the gender of the child. Panel A of Table 3 reports the bivariate probit estimation results. The coefficient of the treatment effect (Raskin*Post) for girls is statistically insignificant at conventional levels for both work and school. This means Raskin has no significant effect on the probability of working and schooling for girls. When considering boys, Raskin has a negative impact on child labour, while no significant impact on schooling is observed.

Table 3: Effect of Raskin by gender

Panel A	Girls		Boys	
	(1)	(2)	(3)	(4)
	Work	School	Work	School
Post treatment	0.846**	-1.289***	1.389***	-0.948***
	(0.401)	(0.308)	(0.431)	(0.331)
Raskin	0.251	0.081	0.556**	-0.288*
	(0.274)	(0.172)	(0.257)	(0.155)
Raskin*Post	-0.333	0.254	-0.738**	0.152
	(0.326)	(0.202)	(0.299)	(0.221)
Rho	-0.378***		-0.238**	
	(0.100)		(0.093)	
Number of observations	2,150		2,159	
Panel B - Girls	(5)	(6)	(7)	(8)
	Work only (work=1 school=0)	School only (work=0 school=1)	Both (work=1 school=1)	Idle (work=0 school=0)
Post treatment	0.018**	-0.194***	0.035**	0.141***
	(0.009)	(0.043)	(0.017)	(0.036)
Raskin	0.001	-0.006	0.016	-0.011
	(0.003)	(0.028)	(0.016)	(0.019)
Raskin*Post	-0.005	0.049	-0.019	-0.025
	(0.004)	(0.033)	(0.021)	(0.021)
Panel C - Boys	(9)	(10)	(11)	(12)
	Work only (work=1 school=0)	School only (work=0 school=1)	Both (work=1 school=1)	Idle (work=0 school=0)
Post treatment	0.028**	-0.196***	0.080***	0.088***
	(0.012)	(0.050)	(0.027)	(0.033)
Raskin	0.008**	-0.070***	0.035**	0.027
	(0.003)	(0.025)	(0.016)	(0.016)
Raskin*Post	-0.009*	0.070**	-0.052**	-0.010
	(0.004)	(0.034)	(0.022)	(0.024)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parenthesis, clustered at household level. All estimations include the full set of control variables as given in Table S1, household, year and provincial fixed effects as well as the corresponding weights generated by CEM.

Panels B and C of Table 3 present the average marginal effects (AME) of the estimated coefficients for girls and boys, respectively. In line with the bivariate probit model, Panel B shows that despite the expected direction of the effect, none of the probabilities of the four outcomes of work and school for girls are statistically significant at conventional levels. In contrast, for boys, Raskin significantly reduces the likelihood of engaging solely in work by 0.9 percentage points while increasing the probability of only attending school by seven percentage points. As a result, the probability of boys who are both working and schooling decreases by 5.2 percentage points. Taken together, this suggests that the effect of Raskin is heterogeneous.

Specifically, it reduces the likelihood of work for boys while there is no impact on the outcomes of girls.

5.4 Age Heterogeneity

In Indonesia, there is near-universal primary education but secondary enrolment is much lower (BAPPENAS & UNICEF 2017), implying that the effect of Raskin may differ according to primary and secondary school age. We investigate this possibility by using two subgroups classified as pre and primary school age (5-12 years) and junior secondary school age (13-15 years). Panel A of Table 4 presents the bivariate probit estimates. We find that Raskin has a significant negative impact on the probability of child work for pre and primary school age children while no effect on schooling for both young and older children.

Table 4: Effect of Raskin by age

Panel A	Pre and primary school age (5 - 12 years)		Junior secondary school age (13-15 years)	
	(1) Work	(2) School	(3) Work	(4) School
Post treatment	2.016***	-1.326***	0.192	-0.611
	(0.433)	(0.261)	(0.400)	(0.494)
Raskin	0.550*	-0.014	0.355	-0.278
	(0.322)	(0.142)	(0.253)	(0.217)
Raskin*Post	-0.771**	0.201	-0.349	-0.106
	(0.344)	(0.170)	(0.321)	(0.311)
Rho	0.220		-0.668***	
	(0.143)		(0.109)	
Number of observations	3,359		950	
Panel B - Pre and primary school age (5 - 12 years)	(5) Work only (work = 1 school =0)	(6) School only (work = 0 school =1)	(7) Both (work = 1 school =1)	(8) Idle (work =0 school =0)
Post treatment	0.021*	-0.247***	0.095***	0.131***
	(0.012)	(0.040)	(0.028)	(0.028)
Raskin	0.001	-0.027	0.025*	0.000
	(0.001)	(0.020)	(0.014)	(0.015)
Raskin*Post	-0.002	0.063**	-0.042**	-0.019
	(0.001)	(0.027)	(0.021)	(0.017)
Panel C - Junior secondary school age (13-15 years)	(9) Work only (work = 1 school =0)	(10) School only (work = 0 school =1)	(11) Both (work = 1 school =1)	(12) Idle (work =0 school =0)
Post treatment	0.025	-0.095	0.008	0.061
	(0.022)	(0.092)	(0.052)	(0.053)
Raskin	0.020*	-0.081*	0.042	0.018
	(0.012)	(0.048)	(0.033)	(0.020)
Raskin*Post	-0.007	0.039	-0.055	0.022
	(0.017)	(0.066)	(0.044)	(0.033)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parenthesis, clustered at household level. All estimations include the full set of control variables as given in Table S1, household, year and provincial fixed effects as well as the corresponding weights generated by CEM.

The average marginal effects reported in Panels B and C of Table 4 show that for pre and primary school age children, Raskin significantly decreases the probability of both working and schooling by 4.2 percentage points and as a result, the probability of only attending school increases by 6.3 percentage points. Consistent with bivariate probit estimates, none of the four probabilities for junior secondary school age children are statistically significant. This suggests that Raskin is effective in reducing child work for young children allowing them to solely attend school.[19]

5.5 Urban/Rural Heterogeneity

The place of residence is another important factor that determines the participation of child labour and schooling. For instance, children in rural areas are more likely to engage in child work compared to children in urban areas (Edmonds 2007). Therefore, we examine whether the effect of Raskin is heterogeneous based on urban-rural location. The bivariate probit estimates reported in Table 5 show that Raskin has a negative effect on child work for children in rural areas but not for urban children. The average marginal effects suggest that Raskin decreases the probability of both working and schooling for rural children by 4.5 percent. These findings provide indicative evidence that rural children are more likely to benefit from Raskin than urban children.

6 Discussion

6.1 Robustness Checks

6.1.1 Testing Model Robustness

One common approach used in examining the model robustness is to check the sensitivity of the estimated treatment effects to the inclusion of observed controls (Oster 2019). Therefore, we re-estimate the bivariate probit models by progressively including the control variables. Table 6 presents the results. The effect of Raskin on child labour remains consistently negative and significant in all specifications. Specification 1 reports the estimation results with only fixed effects. Controlling for child and household characteristics makes the effect even stronger in magnitude (specifications 2 and 3). Specification 4 provides the estimates from the original

[19] In Indonesia, the official age of primary school entry is seven. The regression results considering only primary school aged children, that is, 7-12 years, are qualitatively similar to that of 5-12 years old. The results are available upon request.

Table 5: Effect of Raskin by residence

Panel A	Rural		Urban	
	(1)	(2)	(3)	(4)
	Work	School	Work	School
Post treatment	1.560***	-1.334***	0.484	-0.948***
	(0.425)	(0.334)	(0.366)	(0.323)
Raskin	0.506**	-0.192	0.176	0.098
	(0.249)	(0.147)	(0.296)	(0.177)
Raskin*Post	-0.579*	0.034	-0.435	0.134
	(0.306)	(0.215)	(0.338)	(0.228)
Rho	-0.357***		-0.158	
	(0.084)		(0.108)	
Number of observations	2,189		2,120	
Panel B - Rural	(5)	(6)	(7)	(8)
	Work only	School only	Both	Idle
	(work = 1 school =0)	(work = 0 school =1)	(work = 1 school =1)	(work =0 school =0)
Post treatment	0.035***	-0.275***	0.105***	0.136***
	(0.009)	(0.057)	(0.031)	(0.037)
Raskin	0.008**	-0.062**	0.037*	0.016
	(0.004)	(0.028)	(0.019)	(0.017)
Raskin*Post	-0.007	0.049	-0.045*	0.003
	(0.005)	(0.037)	(0.023)	(0.024)
Panel C - Urban	(9)	(10)	(11)	(12)
	Work only	School only	Both	Idle
	(work = 1 school =0)	(work = 0 school =1)	(work = 1 school =1)	(work =0 school =0)
Post treatment	0.006*	-0.124***	0.022	0.096***
	(0.003)	(0.038)	(0.018)	(0.032)
Raskin	0.000	0.000	0.011	-0.011
	(0.002)	(0.027)	(0.016)	(0.019)
Raskin*Post	-0.003	0.040	-0.025	-0.012
	(0.002)	(0.033)	(0.021)	(0.024)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parenthesis, clustered at household level. All estimations include the full set of control variables as given in Table S1, household, year and provincial fixed effects as well as the corresponding weights generated by CEM.

model with all covariates, as reported in Table 2. The stability of the treatment effect suggests that our results are robust to the choice of control variables.

In addition to Raskin, there are several other social protection programs such as cash transfers, scholarships for poor students and social health assistance implemented in Indonesia. These programs use more or less the same eligibility criteria or database to target poor and vulnerable households. This suggests that certain households may receive assistance through various programs not just Raskin, and therefore, the estimated effect may reflect the cumulative effects of other programs.

To alleviate this concern, we estimate our model by including controls for several major programs as a robustness check. In Table 7 Columns (1) and (2) we control for three main types of cash transfers, conditional cash transfer (*Program Keluarga Harapan* or PKH), unconditional cash transfer (*Bantuan Langsung Sementara Masyarakat* or BLSM) and cash assistance for

Table 6: Model Robustness

	Specification 1		Specification 2		Specification 3		Specification 4	
	Work	School	Work	School	Work	School	Work	School
Post treatment	1.056***	-0.021	1.090***	0.028	1.106***	-1.174***	1.096***	-1.145***
	(0.200)	(0.091)	(0.226)	(0.111)	(0.315)	(0.222)	(0.309)	(0.227)
Raskin	0.429**	-0.112	0.492**	-0.272**	0.386*	-0.161	0.403**	-0.096
	(0.171)	(0.085)	(0.197)	(0.112)	(0.197)	(0.115)	(0.196)	(0.117)
Raskin*Post	-0.451**	0.056	-0.466**	0.159	-0.491**	0.282*	-0.505**	0.206
	(0.203)	(0.113)	(0.230)	(0.139)	(0.233)	(0.148)	(0.232)	(0.152)
Household FE	Yes		Yes		Yes		Yes	
Individual FE	Yes		Yes		Yes		Yes	
Province FE	Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes	
Child controls	No		Yes		Yes		Yes	
Household controls	No		No		Yes		Yes	
Parent controls	No		No		No		Yes	
Rho	-0.022		-0.225***		-0.231***		-0.254***	
	(0.059)		(0.065)		(0.069)		(0.066)	
Observations	4,370		4,370		4,309		4,309	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parenthesis, clustered at household level. All estimations include the corresponding weights generated by CEM.

poor students (*Bantuan Siswa Miskin* or BSM). Columns (3) and (4) report the estimates after controlling for social health assistance (*Jamkesmas*), whereas the estimates with all programs are shown in Columns (5) and (6). The effect of Raskin on child labour remains negative and statistically significant in all three specifications. Interestingly, we observe that the effect is larger than that in our original specification of Columns 7 and 8. This implies that our estimates may be underestimated in the presence of any other unobservables of this nature.

Table 7: Model Robustness - Controlling for other social protection programs

	Cash transfers		Health assistance		Transfers + Assistance		Original Model	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Work	School	Work	School	Work	School	Work	School
Post treatment	1.121***	-1.158***	1.068***	-1.149***	1.122***	-1.160***	1.096***	-1.145***
	(0.320)	(0.236)	(0.307)	(0.236)	(0.319)	(0.237)	(0.309)	(0.227)
Raskin	0.403**	-0.096	0.403**	-0.096	0.403**	-0.096	0.403**	-0.096
	(0.195)	(0.117)	(0.196)	(0.117)	(0.195)	(0.117)	(0.196)	(0.117)
Raskin*Post	-0.522**	0.198	-0.525**	0.203	-0.521**	0.198	-0.505**	0.206
	(0.239)	(0.155)	(0.240)	(0.153)	(0.240)	(0.155)	(0.232)	(0.152)
Conditional cash transfer (PKH)	Yes		No		Yes		No	
Unconditional cash transfer (BLSM)	Yes		No		Yes		No	
Scholarship for poor students (BSM)	Yes		No		Yes		No	
Health assistance	No		Yes		Yes		No	
Rho	-0.254***		-0.254***		-0.253***		-0.254***	
	(0.066)		(0.066)		(0.066)		(0.066)	
Observations	4,309		4,309		4,309		4,309	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parenthesis, clustered at household level.All estimations include the full set of control variables as given in Table S1, household, year and provincial fixed effects as well as the corresponding weights generated by CEM.

6.1.2 Alternative Matching

In section 5.1, we matched the treated and control households on several household characteristics determined by a probit estimation. The covariates that were used for matching are place of residence, household size, dependency ratio, per capita expenditure, ownership of business, access to electricity, whether the household purchases water, uses firewood for cooking, uses the nearby river, land or sea as the toilet and poor sanitation. This selection was based on the variables that are statistically significant at 1% level. Despite the statistical significance of assets per capita, it was not used as a matching variable. This is because as continuous variables, inclusion of both assets per capita and per capita expenditure leads to poor matching outcomes. Therefore, as a robustness check, we consider assets per capita instead of per capita expenditure to examine whether the results are sensitive to the matched variables. The coarsened exact matching summary presented in Table S5 in supplementary materials show that CEM has produced a reasonable match where both the overall multivariate and univariate imbalances are reduced substantially in the post-match. Table 8 reports the results with new CEM weights. The results are qualitatively similar to those reported in Tables 2 and 3, indicating the effect of Raskin on child labour and schooling is robust to the choice of matched variables.

Table 8: Results with alternative matching

	Full Sample		Girls		Boys	
	(1)	(2)	(3)	(4)	(5)	(6)
	Work	School	Work	School	Work	School
Post treatment	1.204***	-0.854***	1.327***	-0.698*	1.389***	-0.899***
	(0.314)	(0.256)	(0.479)	(0.358)	(0.466)	(0.332)
Raskin	0.404*	-0.187	0.208	-0.115	0.666**	-0.285*
	(0.227)	(0.116)	(0.370)	(0.159)	(0.295)	(0.168)
Raskin*Post	-0.486*	0.061	-0.354	0.173	-0.760**	-0.216
	(0.258)	(0.163)	(0.405)	(0.225)	(0.346)	(0.236)
Rho	-0.403***		-0.368**		-0.582***	
	(0.083)		(0.144)		(0.132)	
Observations	3,532		1,805		1,727	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parenthesis, clustered at household level. All estimations include the full set of control variables as given in Table S1, household, year and provincial fixed effects as well as the corresponding weights generated by CEM.

6.2 Interpretation and Comparison with Related Literature

We find that Raskin is effective in decreasing child work for boys. However, its minimum or no effect on reducing child labour and increasing schooling, particularly of girls may be counter-intuitive at first. As an unconditional in-kind transfer, it is expected that the monetary benefit derived from it would generate an income effect leading to a reduction in the supply of child labour as well as an increase in schooling.[20] Nevertheless, given limited empirical evidence on similar social protection tools that are unconditional by nature, our results are not contradictory. For instance, Guarcello et al. (2010) show that providing health insurance is effective only in reducing child work while having no impact in increasing children's participation in school in Guatemala. Therefore, as an unconditional in-kind transfer, the ability of Raskin to decrease the probability of work for boys specifically of those who are working and attending school simultaneously, by approximately 0.9 percentage points provides a useful policy insight on how food subsidies can indirectly influence the wellbeing of children.

Considering conditional in-kind transfers, studies such as De Hoop & Rosati (2014*b*), Kazianga et al. (2009), Ravallion & Wodon (2000) all show that in-kind transfers such as food for education programs that are conditioned on school attendance are effective only in increasing schooling while having a minimum or no effect in reducing children's overall engagement in child labour. Interestingly, we find that the opposite is true in the context of an unconditional in-kind transfer such as a food subsidy which merits further discussion. Education is compulsory for Indonesian children aged seven to eighteen years. This means irrespective of the household's economic status parents are compelled to send their children to school. Therefore, receiving transfers, especially an unconditional one, may not provide the required incentive for the households to alter the decision of sending their children to school significantly. Compared to girls, boys generally have higher participation in market work, meaning a lower likelihood of attending school (Edmonds 2007). This justifies as to why Raskin only leads to a reduction in child work for boys, as boys are more likely to involve in wage work which is often considered to be a worse form of child labour.

The limited effect of the Raskin program on the supply of child labour and schooling may be due to several reasons. First, the benefit of the subsidy, which accounts for about five per cent of the monthly consumption expenditure of poor households, may not be sufficient to keep children out of the labour market. Particularly, to achieve greater impacts on child labour,

[20]To examine whether Raskin has a significant income effect we estimate our model with and without per capita expenditure (the proxy used for household income). If it is due to income effect, then controlling for per capita expenditure should result in a significant change in the estimated coefficients. However, Table S6 shows that the effect of Raskin on child labour is almost identical in both models, implying that the found effect may not be solely due to income effect, but through other mechanisms such as improvement in nutrition status of children as suggested in Dammert et al. (2018). Additionally, Table S7 presents the results from a Fixed Effects regression of Raskin on per capita expenditure. Though there is a positive association between Raskin and per capita expenditure, it is only significant at 10% and is very small in magnitude. This further provides indicative evidence that the income effect is not the only mechanism.

income transfers should be of sufficiently sizeable value (Datt & Uhe 2018). However, in view of Banerjee & Duflo (2011), the poor usually do not do what is in their best interest even if they could afford to do so. This means rather than utilising the income effect that they receive from the rice subsidy to forgo the income earned from child labour, they may spend it on less important things such as festivals and family events. Second, the limited effect of the Raskin program can also be attributed to the behavioural constraints of the poor such as small inconveniences (Banerjee & Duflo 2011) that restrict them to gain the full benefit of the subsidy. Around two to three per cent of those who are eligible to receive Raskin have refused the receipt of the subsidy at least once in a given year, due to reasons such as inability to go on the allocated day, lack of time or long distance to the distribution centre. Third, there may be issues of accurate targeting of the program leading to both inclusion and exclusion errors. It is stated that redistribution programs in less developed countries often leak due to various reasons such as targeting method used, take up problems, corruption and bribes (Banerjee et al. 2016, Currie & Gahvari 2008, Trimmer et al. 2018). Though the government of Indonesia spends over US$ 1.5 billion a year on the Raskin program, less than half of the rice was actually reaching the intended households (Banerjee et al. 2016). Therefore, this study also underscores the importance of accurate targeting of government social protection programs so as to achieve the ultimate goal of poverty reduction.

However, it is important to highlight some data limitations of our analysis. First, given that Raskin program does not have any clear assignment rules, we match the treated and the control households based on the observable household characteristics that act as proxies for household's poverty level. In other words, our results are based on the assumption that poverty is equal to Raskin eligibility, which may be plausible as Raskin is targeted to the poorest households. Second, the child labour variable only captures whether the child has worked or not and hence does not provide data on the number of hours worked or the nature of work activity. Considering the effect of unconditional in-kind transfers on both the intensity and type of work may have different policy implications, and therefore is a potential avenue for future research. Another methodological limitation is that the balance between the control and treatment group is chosen ex-ante. Further, almost 30 percent of the households are not exact matched, meaning these households are excluded from the estimating sample. However, restricting the sample to common support is important in any matching technique. We further check the robustness of our CEM estimates to other matching techniques such as propensity score matching and Malahanobis matching and find that the effects are qualitatively similar.[21]

7 Conclusion

Child labour continues to be a problem of the developing world, where nine out of every ten children in child labour are in the regions of Africa, Asia and Pacific. Therefore, there is a compelling need for evidence-based interventions on child labour to inform policy responses. Since child labour is strongly related with and determined by poverty, social protection programs are a potential source of mitigation. Though there is ample evidence on the impact of cash transfers on child labour, evidence on the effect of other social protection tools, particularly of in-kind transfers is limited. This paper addresses this empirical gap by examining the impacts of an unconditional in-kind transfer - a subsidised food program on child work as well as schooling. To this end, we consider the Raskin program, which is the largest subsidised rice program in Indonesia.

We find that in general, a food subsidy is not effective in reducing the labour supply of children and schooling for girls. However, the program has a strong effect in inducing boys who are both working and attending school to decrease child labour. Specifically, a subsidy on a staple food like rice can lead to a decrease in the probability of work for boys by 0.9 percentage points. Additionally, we find that children at pre and primary school age (5-12 years) and children residing in rural areas are more likely to reduce the probability of working due to the receipt of Raskin.

In line with previous studies on conditional in-kind transfers and child labour, our results are not contradictory. In fact, as an unconditional in-kind transfer, the ability of a food subsidy to decrease child labour of young boys in a developing country provides an important policy implication on how social protection tools can indirectly influence the wellbeing of children.

The minimum effect of the subsidy on child labour and schooling may be due to several reasons. Among them, the size of the subsidy, as well as targeting issues leading to considerable leakages are prominent. Therefore, the findings of our study indicate that to reap the maximum benefits of pro-poor programs such as subsidised food programs, it is vital to design such programs in a manner that maximises its reach and intensity. This would inevitably have a considerable impact on the welfare of poor households and thereby ensure child wellbeing.

[21] The results are available upon request.

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Supplementary Materials

Table S1: Variable Description

Variable	Description
Child-working	=1 if the child has ever worked
Child-schooling	=1 if the child is still in school
Raskin	=1 if the household has ever bought rice from Raskin (during the past year)
Child Characteristics	
Child gender	=1 if the child is a female
Child-age	Age of the child
Child-age2	Age of the child squared
Child-religion-Islam	=1 if the child's religion is Islam
Household Characteristics	
Household size	The number of members in the household
Dependency ratio	The ratio of the number of household members aged below 14 and above 65 years to the number of working members aged 15 - 64 years
HHH-female	=1 if the household head is a female
Urban	=1 if the household is in an urban area
Own business	=1 if the household has its own farm business
Own farm land	=1 if the household has its own farm land
Per capita expenditure (PCE) (ln)	Logarithm of monthly per capita expenditure
Assets per capita (ln)	Logarithm of household assets per capita
Electricity	=1 if the household has access to electricity
Water	=1 if the household purchases water
Toilet-river/land/sea	=1 if the household does not have proper toilet facilities
Cook-firewood	=1 if the household uses firewood as the main source of energy for cooking
Poor sanitation	=1 if the household has poor sanitation
Parent Characteristics	
Mother-age	Age of the mother
Father-age	Age of the father
Mother-married	=1 if the mother is married
Mother-paid occupation	=1 if the mother is occupied in a paid occupation
Father-paid occupation	=1 if the father is occupied in a paid occupation
Mother - elementary	=1 if the mother has completed elementary school
Mother - junior	=1 if the mother has completed junior school
Mother -senior	=1 if the mother has completed senior school
Mother - tertiary	=1 if the mother has completed tertiary education
Father - elementary	=1 if the father has completed elementary school
Father - junior	=1 if the father has completed junior school
Father - senior	=1 if the father has completed senior school
Father - tertiary	=1 if the father has completed tertiary education
Mother - highest edu	=1 if the mother has completed the highest level of education
Father - highest edu	=1 if the father has completed the highest level of education
Provincial Dummies	Seperate indicator variables for each of the following provinces: North Sumarta, West Sumarta, South Sumarta, Lampung, Jakarta, West Java, Central Java, Yogyakarta, East Java, Bali, West Nusa Tenggara, South Sulawesi and South Kalimantan

Table S2: Probit Estimation for Matching

Variables	Coefficient
Urban	-0.309***
	(0.045)
Household size	-0.077***
	(0.009)
Dependency ratio	-0.073***
	(0.025)
HHH-female	0.003
	(0.064)
Assets per capita (ln)	-0.120***
	(0.014)
Per capita expenditure (ln)	-0.238***
	(0.033)
Own business	0.104***
	(0.039)
Own farm land	-0.086*
	(0.046)
Electricity	0.202***
	(0.056)
Water	-0.366***
	(0.044)
Cook-firewood	0.141***
	(0.047)
Toilet-river/land/sea	0.355***
	(0.041)
Poor sanitation	0.172***
	(0.045)
Constant	5.107***
	(0.370)

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors in parentheses, clustered at household level. Estimation is based on wave 2 data.

Table S3: Covariate Balance

Pre-match multivariate L1 distance: 0.9091

	Pre-match univariate imbalance		Sample mean	
	L1	Mean difference	Control (Raskin=0)	Treatment (Raskin = 1)
Urban	0.238	-0.238	0.657	0.396
Household size	0.091	-0.347	5.414	5.412
Dependency ratio	0.094	0.030	1.120	1.140
Per capita expenditure (ln)	0.251	-0.394	12.978	12.270
Own business	0.025	-0.025	0.460	0.414
Electricity	0.080	-0.080	0.954	0.906
Water	0.143	-0.143	0.387	0.219
Cook firewood	0.231	0.231	0.190	0.465
Toilet-river/land/sea	0.228	0.228	0.086	0.281
Poor sanitation	0.094	0.094	0.175	0.255

Post-match multivariate L1 distance: 0.6821

	Post-match univariate imbalance		Sample Mean	
	L1	Mean Difference	Control (Raskin=0)	Treatment (Raskin = 1)
Urban	0.000	0.000	0.378	0.378
Household size	0.012	0.010	5.156	5.166
Dependency ratio	0.007	0.002	1.085	1.087
Per capita expenditure (ln)	0.125	-0.021	11.195	11.174
Own business	0.000	0.000	0.323	0.323
Electricity	0.000	0.000	0.859	0.859
Water	0.000	0.000	0.102	0.102
Cook firewood	0.000	0.000	0.506	0.506
Toilet-river/land/sea	0.000	0.000	0.256	0.256
Poor sanitation	0.000	0.000	0.120	0.120

Table S4: Predicted and actual probabilities

	Work only (work = 1 school = 0)	School only (work = 0 school = 1)	Both work & school (work = 1 school = 1)	Idle (work = 0 school = 0)
Sample Mean	0.008	0.799	0.049	0.143
Predicted Probability	0.007	0.802	0.046	0.145

Note: Predicted probability represents the predictive margins of a given outcome

Table S5: Covariate Balance - Alternative matching

Pre-match multivariate L1 distance: 0.8999

	Pre-match univariate imbalance		Sample mean	
	L1	Mean difference	Control (Raskin=0)	Treatment (Raskin=1)
Urban	0.237	-0.237	0.657	0.396
Household size	0.088	-0.347	5.414	5.412
Dependency ratio	0.092	0.029	1.120	1.140
Assets per capita (ln)	0.235	-0.787	15.974	14.85
Own business	0.023	-0.023	0.460	0.414
Electricity	0.080	-0.080	0.954	0.906
Water	0.140	-0.140	0.387	0.219
Cook firewood	0.230	0.230	0.190	0.465
Toilet-river/land/sea	0.225	0.225	0.086	0.281
Poor sanitation	0.092	0.092	0.175	0.255

Post-match univariate imbalance: 0.6106

	Post-match univariate imbalance		Sample mean	
	L1	Mean difference	Control (Raskin=0)	Treatment (Raskin=1)
Urban	0.000	0.000	0.377	0.377
Household size	0.007	0.001	5.099	5.101
Dependency ratio	0.004	0.000	1.117	1.117
Assets per capita (ln)	0.131	-0.025	14.276	14.251
Own business	0.000	0.000	0.323	0.323
Electricity	0.000	0.000	0.851	0.851
Water	0.000	0.000	0.113	0.113
Cook firewood	0.000	0.000	0.473	0.473
Toilet-river/land/sea	0.000	0.000	0.257	0.257
Poor sanitation	0.000	0.000	0.081	0.081

Table S6: Effect of Raskin with and without per capita expenditure (PCE)

	Without PCE		With PCE	
	(1)	(2)	(3)	(4)
	Work	School	Work	School
Post treatment	1.465***	-0.473***	1.096***	-1.145***
	(0.246)	(0.146)	(0.309)	(0.227)
Raskin	0.411**	-0.087	0.403**	-0.096
	(0.197)	(0.115)	(0.196)	(0.117)
Raskin*Post	-0.520**	0.151	-0.505**	0.206
	(0.231)	(0.149)	(0.232)	(0.152)
Rho	-0.240***		-0.254***	
	(0.066)		(0.066)	
Number of observations	4,331	4,331	4,309	4,309

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parenthesis, clustered at household level. Both models include the full set of control variables as given in Table S1 household, year and provincial fixed effects as well as the corresponding weights generated by CEM.

Table S7: Effect of Raskin on per capita expenditure (PCE)

	PCE
Raskin	0.056*
	(0.033)
Household FE	Yes
Individual FE	Yes
Province FE	Yes
Year FE	Yes
Household controls	Yes
Observations	4,309

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parenthesis, clustered at household level.