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# Targeting poverty under complementarities: Evidence from Indonesia's unified targeting system[☆]

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ABSTRACT

Developing countries are increasingly moving to unified targeted systems to better identify the poor and improve their outcomes. While social programs are nearly always delivered alongside one another however, the evaluations of these programs typically occur in isolation. Combining nationally representative administrative and survey data, we evaluate Indonesia's three largest social programs in unison. The setting for our evaluation is the launch of Indonesia's Unified Targeting system, an innovation developed to unify program eligibility, reduce targeting errors and increase program complementarities. Introducing a new method of evaluation under the condition of complementary programs, we show that the probability of targeted households receiving all three programs increased by 117 percent. Our analysis shows that households receiving all three complementary programs have at least 30 percentage points higher per capita expenditure than those receiving none. Our results highlight the need to account for program complementarities and provide support for unified program eligibility.

*"I can live for two months on a good compliment"*

Mark Twain

Targeted poverty programs represent important interventions to reduce poverty in developing countries. Recent years have witnessed a proliferation of unified poverty targeting systems, based on single consolidated registries. 92 countries are currently implementing or preparing to roll out unified targeting systems, which cover almost two billion people (Honorati et al., 2015, Bah et al 2018). Whereas poverty programs are nearly always delivered alongside one another however (Grosh et al., 2008), the evaluations of these programs typically occur in isolation.[1] If the benefits of poverty programs are complementary, in the sense that the marginal benefits of individual programs in the presence of complimentary programs are positive, then there is a case to be made for unified program eligibility *and* for the concurrent evaluation of complimentary programs on efficiency and accuracy grounds. Indeed multifaceted programs have been shown to have a significantly positive and persistent impact on the chances of 'ultra-poor' households escaping poverty (Banerjee et al., 2015). Since the vast majority of unified targeting programs are still being developed however, it is timely to evaluate their efficacy both in terms of their targeting performance and their impact on household welfare.

Traditionally, unified targeting systems have been implemented in developed rather than developing countries, which lack complete information on household welfare (Grosh et al., 2008). Recently there has been a proliferation in developing country uptake and program development however, due to relaxed budget constraints and the timely collection of better information on household welfare. The underlying philosophy of unified targeting is to consolidate beneficiary lists so as to standardise the target population (World Bank 2012a, 2012b). If differing and potentially complementary social programs adopt inconsistent beneficiary lists, then there will likely exist households that receive program A and not program B, and others that receive program B and not program A. Efforts, such as unified targeting, which therefore seek to unify uptake but do not change the overall incidence of uptake, will therefore necessarily yield better outcomes along the extensive

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[1] Examples include: Cornia and Stewart (1995), Jayne et al. (2002), Schultz (2004), Galasso and Ravallion (2005), Ravallion (2008, 2009), Angelucci and De Giorgi (2009), De Janvry et al. (2012), Niehaus et al. (2013) and Brown et al. (2018).

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margin. Evaluating complementary programs in isolation relying on outcome variables that may be affected by more than one program, may therefore lead to upward biases, since a household's outcomes might otherwise be driven by omitted programs.

In this paper, we evaluate the benefits of unified program eligibility in a developing country context, conducting the first judicious evaluation of multiple concurrent programs in unison for the first time.[2] Specifically, we examine how the benefits of social programs aimed at reducing poverty, complement one another, in the context of the introduction of Indonesia's Unified Targeting System (UDB), the primary aim of which was to standardise program eligibility.

Indonesia's targeting system has been the subject of evaluations using field experiments (Alatas et al., 2012; Alatas et al., 2016) restricted to fairly small samples, raising fears of external validitiy. Others have used nationally representative data to focus on single programs, for example the *Askeskin* and *Jamkesmas* programs (Sparrow, 2008; Sparrow et al., 2013) or the *Raskin* program (Sumarto et al., 2003; Olken, 2005). Bah et al (2018) represents an exception since those authors evaluate both the Unconditional Cash Transfers (*Bantuan Langsung Tunai* or *BLT*) and the Health insurance for the poor (*Jamkesmas)* programs, but they do so independently of each another, and focus on the process of targeting as opposed to targeting outcomes using a restricted sub-sample of the overall population.

Our focus is instead on Indonesia's three largest social welfare programs operating in unison. Together these welfare programs account for 87% of Indonesia's social expenditure. We exploit rich nationally representative administrative and survey data, which includes privileged access to the Proxy Means Test (PMT) coefficients and cut-offs for all 471 Indonesian municipalities as well as individual household PMT scores.[3] Introducing a new method of evaluating poverty targeting performance under the condition of multiple concurrent programs, which *a priori* are expected to complement one another, we first document the improvements in targeting performance, between 2005 and 2014; since we are able to observe over time, whether eligible and ineligible households took receipt of any of the three programs. In other words, relative to the existing literature, which used covariates to estimate whether a household was eligible or not,[4] we are able to observe, across the entire nationally representative sample, whether households were eligible for programs, and then among eligible households, which households actually received those programs. We show that the introduction of the UDB significantly increased the targeting performance of social programs in Indonesia. The probability of targeted households receiving all three programs increased by 117 percent compared to previous targeting efforts.

We continue by evaluating the impact on household welfare of moving to a unified targeting system. Our matching approach, allows us to estimate the difference in household outcomes, between households that received every combination of social program. We exploit the design of the anti-poverty programs, which has been argued to be first best when analysing social programs that target poverty (Ravallion, 2007). Specifically, we are able to match individual households using their PMT scores, using the optimal bounds procedure suggested by Crump et al. (2009). We subsequently show that this procedure yields far superior results than using covariates alone, by providing a much larger and better distributed common support from which to match treated and untreated units.

We compare the difference in the benefits various households receive, in terms of per capita expenditures, between the introduction of the UDB in 2011, (when the baseline data were collected) and 2014, a suitable period to follow-up in since all households were eligible for all three programs in the intervening period including when the Government of Indonesia provided Unconditional Cash Transfers to households in 2013, following their decision to reduce the nationwide oil price subsidy. In our preferred empirical specification, we use a control function with the PMT score entering the first stage regression. We therefore generate estimates of the marginal benefits of receiving Indonesia's three flagship social programs conditional on the receipt of either zero, one or else combinations of two other programs.[5] Households receiving all three programs, experienced an increase in household expenditure of at least 30 percentage points compared to those that received no programs, and household expenditure increased between 16 and 19 percentage points for households receiving all three programs compared to households that received only one or two programs. Our results highlight the tangible benefits of the introduction of the UDB, in other words of unified program eligibility.

## 1. Background

### 1.1. History of Indonesian poverty programs

The majority of Indonesians hover around the national poverty threshold (World Bank, 2012a) with approximately half the population living below IDR15,000 per day (around PPP USD 2.25 a day). Marginal shocks therefore have profound effects on household welfare in Indonesia (Pritchett et al., 2000; Suryahadi et al., 2003). This has made poverty and vulnerability central policy issues for successive governments.

Indonesia has a long history of targeted social programs, and since 2005, the Indonesian government has experimented with several methods to identify and access vulnerable groups, while implementing several complimentary social programs. Alatas et al. (2012) and Cameron and Shah (2014) however, show that a significant proportion of poor households in Indonesia do not benefit from targeted poverty programs. To address these concerns, the Government of Indonesia (GoI) developed a Unified Targeting System, called the *Basis Data Terpadu* or Unified Database (UDB), through the establishment of TNP2K[6] under the auspices of the Office of the Vice-President of Indonesia and the Indonesian Central Bureau of Statistics (BPS), which was introduced in 2011.

The primary objectives of the UDB are: (i) to provide detailed socioeconomic information on the poor and most vulnerable households by name, and by address; (ii) to improve the targeting of social welfare programs; and (iii) to ensure that the social protection programs better complement one another. To achieve these objectives, efforts were made to unify program eligibility through the development of Proxy Means Test coefficients, *such that the poorest 25% of the population should be eligible for all three of Indonesia's flagship social welfare programs.* In so doing, the GoI aimed to reduce targeting errors and ensure that poor households received the benefits from multiple complimentary programs (TNP2K, 2015). Indonesia's three flagship programs are: Health Insurance for the Poor (*Asuransi Kesehatan untuk Keluarga Miskin*, or *Askeskin*, later renamed *Jamkesmas*), Rice for the Poor (*Beras Miskin*, or *Raskin*) and

[2] While Brière and Lindert (2005) and Castaneda and Fernandez (2005) pertain to unified eligibility in Brazil and Colombia respectively, both are descriptive in nature and neither provides a comparison of the effectiveness of targeting performance of unified targeting programs relative to single programs and neither examine the welfare benefits of unified targeting systems.

[3] The only other paper we are aware of that implements the administrative PMT scores is Bah et al (2018), who rely on the PMT coefficients from only six municipalities.

[4] See for example: Jalan and Ravallion (2003), Godtland et al. (2004), Hoddinott and Skoufias (2004) Galasso and Ravallion (2005), Pradhan et al. (2007) van de Walle and Mu (2007) and Bazzi, S. et al. (2015).

[5] Household welfare is measured using per capita expenditure. The administrative data also prove vital for estimating household per capita expenditure in 2011, which is calculated by applying observable 2014 data to the 2011 PMT coefficients, in order to produce a measure of the change in household welfare between 2011 and 2014.

[6] *Tim Nasional Percepatan Penanggulangan Kemiskinan* or the National Team for Accelerating Poverty Reduction.

Unconditional Cash Transfers (*Bantuan Langsung Tunai*, or BLT, later renamed BLSM).[7] Of the GoI's overall allocation of around 11.5 percent of total expenditure in 2016 on social programs, 87% was allocated to these three flagship programs.

The GoI introduced social security programs for the first time following the Asian Financial Crisis (Grosh et al., 2008; Sumarto and Bazzi, 2011).[8] These, the first generation of Indonesia's social protection programs, called *Jaring Pengaman Sosial* (JPS), were implemented under President Habibie's Administration in 1999/2000 (Widjaja, 2009). The JPS sought to protect chronically poor households from falling further into poverty while eliminating vulnerable households' exposure to risk (Sumarto et al., 2002).[9] The JPS was tasked with ensuring, among other duties, the availability of affordable food through the OPK (*Operasi Pasar Khusus* or special market operation program). In 2002, the GoI changed the OPK to become one of the largest social protection programs named *Raskin* (*Beras untuk Keluarga Miskin* or Rice for the Poor), which aimed to reduce household spending on food, especially on rice.

The second generation of social protection programs were implemented between 2005 and 2008 to alleviate the financial burden on households from rising oil prices. To mitigate the negative effects, especially on poor and near-poor households, the GoI launched the Fuel Subsidy Reduction Compensation Program, namely *Program Kompensasi Pengurangan Subsidi Bahan Bakar Minyak* (World Bank, 2006; Yusuf and Resosudarmo, 2008; Rosfadhila et al., 2011). Under this scheme, an Unconditional Cash Transfer program was introduced to complement the BLT (for *Bantuan Likuiditas Tunai* or Direct Cash Assistance). This program was subsequently renamed BLSM (for *Bantuan Langsung Sementara Masyarakat* or Temporary Unconditional Cash Transfer program) in 2013.[10] From July to September 2005, the GoI through Statistics Indonesia (*Badan Pusat Statistik* or BPS) conducted a census of poor households for the first time, with the aim of effectively implementing the BLT program. The database was also known as PSE05 (*Pendataan Sosial Ekonomi Penduduk*, 2005, or Socio-economic Data Collection of the Population).[11]

Due to concerns about the poor performance of the PSE05,[12] in 2008–2009 the GoI once again restructured the nationwide programs, by updating the list of program beneficiaries. At this time the government's three main flagship program were the BLT, *Raskin* and *Jamkesmas*. From the perspective of budget disbursement, BLT spending constitutes 40 percent of total social assistance expenditure, *Raskin* accounts for 34 percent and Jamkesmas for 13 percent (Jellema and Noura, 2012). This updated version, was known as the PPLS08 (*Pendataan Program Lindungan Sosial*, 2008, or Data Collection for Targeting Social Protection Programs). As with PSE05, this database was primarily used to identify eligible households for unconditional cash transfers. Due to time constraints however, the problems associated with PPLS08 were similar or worse than those of the PSE05 and errors in targeting continued (Rosfadhila et al., 2011). Some argue that targeting errors catalysed social unrest (Widjaja, 2009; Cameron and Shah, 2014).[12]

### 1.2. Introduction of the UDB

The UDB, first introduced in 2011,[13] was developed to harmonise social program eligibility, by standardising the list of intended beneficiaries, such that the bottom 25% of the households, were eligible for all three social programs. The introduction of the UDB therefore represents an ideal testing ground to evaluate the benefits of unified program eligibility in developing countries and for examining the role of complimentary social program benefits.[14] The poorest 40 percent of the population was first identified for inclusion in social assistance programs through proxy means testing.[15] Although the bottom 40% of the population are eligible for *Jamkesmas*, only the bottom 25% of households are eligible for *Raskin* and the *BLT* (see Fig. 1). Harmonising social program eligibility through the introduction of the UDB was expected to improve targeting outcomes and improve welfare through lowering targeting errors and by increasing complementarities between social assistance programs, which failed to occur under the previous targeting regime (TNP2K, 2015).

In comparison to the previous targeting system, a number of improvements were introduced, including: (1) an increase in the number of indicators used to measure household welfare (26 as opposed to 14) from the 2011 poverty census, namely PPLS11;[14] (2) greater coverage of households in PPLS11, reaching 40 percent of the population surveyed or approximately 24 million households; (3) the implementation of a two-stage targeting process in the data collection of PPLS11;[15] and (4) a PMT model to measure targeting thresholds based on 471 district-specific models, as opposed to using a single national threshold (TNP2K, 2015). Fig. 2 details the development of UDB and the use of the database for selecting poor beneficiaries of the poverty programs.

### 1.3. Introduction of the KPS

Following improvements in targeting, in the third quarter of 2013, the GoI introduced the Social Security Card (*Kartu Perlindungan Social - KPS)*. This card aimed to cover the bottom 25 percent of households or 15.5 million poor and near-poor households. The names of these households, derived from the UDB, entitled households to *Raskin*, temporary unconditional cash transfers (BLSM) and financial assistance for students of those family members (TNP2K, 2015). According to an ad-hoc committee established to diseminate information with regards to oil price subsidy reduction (Tim Sosialisasi Penyesuaian Subsidi Bahan Bakar Minyak, 2013), this card could also be used to access the Jamkesmas program. This is reasonable since, as shown in Fig. 1, the coverage of Jamkesmas is far higher than the coverage of the KPS. To ensure that every eligible household received the card without disruption, the GoI employed the postal mail service and cards were delivered

[7] We are unable to evaluate Indonesia's smaller social programs such as scholarship for the poor (Bantuan Siswa Miskin, or BSM), the Conditional Cash Transfer program (Program Keluarga Harapan, or PKH) and community block grants for education and development (Widianto, 2013), since their coverage is not nationwide and since their targeting is not based on the UDB but is rather based on the old targeting regime based on the PPLS08 else based on nominations by teachers, or school-based targeting.

[8] Please refer to in the Appendix, which summarizes the evolution of social safety net in Indonesia from 1997 to 2008.

[9] The GoI disbursed IDR3.9 trillion directly to JPS programs out of a total development budget of IDR14.2 trillion, with financial support from international donors including the World Bank and the Asian Development Bank (Sumarto and Bazzi, 2011).

[10] Under this program, the targeted household received cash transfers delivered via post office (Bazzi et al., 2015). The BLT cash benefit was IDR100,000 (roughly US$10) per month to each targeted recipient household and it was increased to IDR150,000 under the BLSM scheme.

[11] The data collection involved community-based nominations combined with other data to identify prospective beneficiary households based on fourteen selected indicators that represented the well-being of poor households, see Hastuti et al. (2006) for further details.

[12] Previous studies by Hastuti et al. (2006), Widjaja (2009) and World Bank (2012a), assert that the PSE05 and PPLS08 programs suffered from serious problems. They argue that since households who were nominated by sub-village heads were surveyed with the PMT questionnaire, many poor households were excluded.

[13] We take 2011 as our starting point, given that the baseline poverty census was conducted in that year.

[14] Pendataan Program Lindungan Sosial, 2011.

[15] The two-stage data collection involved (i) compiling lists of households using data from PPLS08 and the 2010 Population Census through poverty mapping; and (ii) complementing those data with the results of consultations with low-income groups and through impromptu discussions and general observations (Bah et al. 2018).

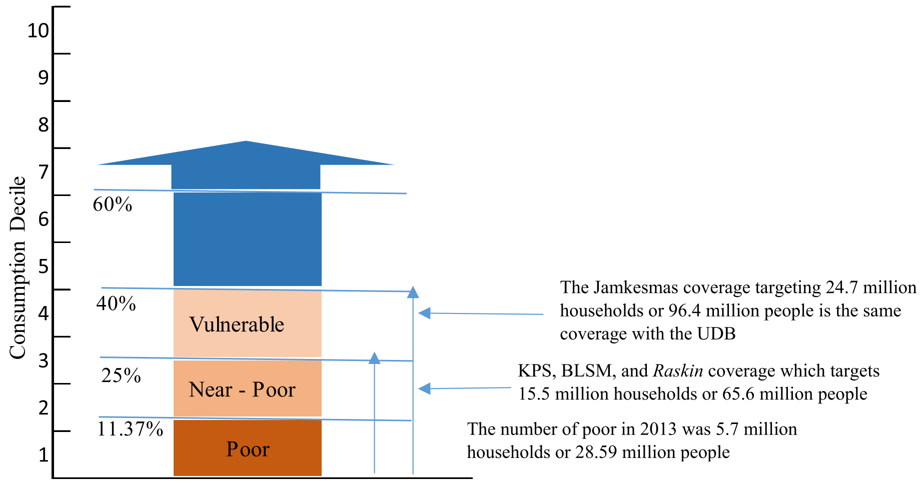


**Fig. 1.** The coverage of the UDB and Indonesia's three largest poverty programs.

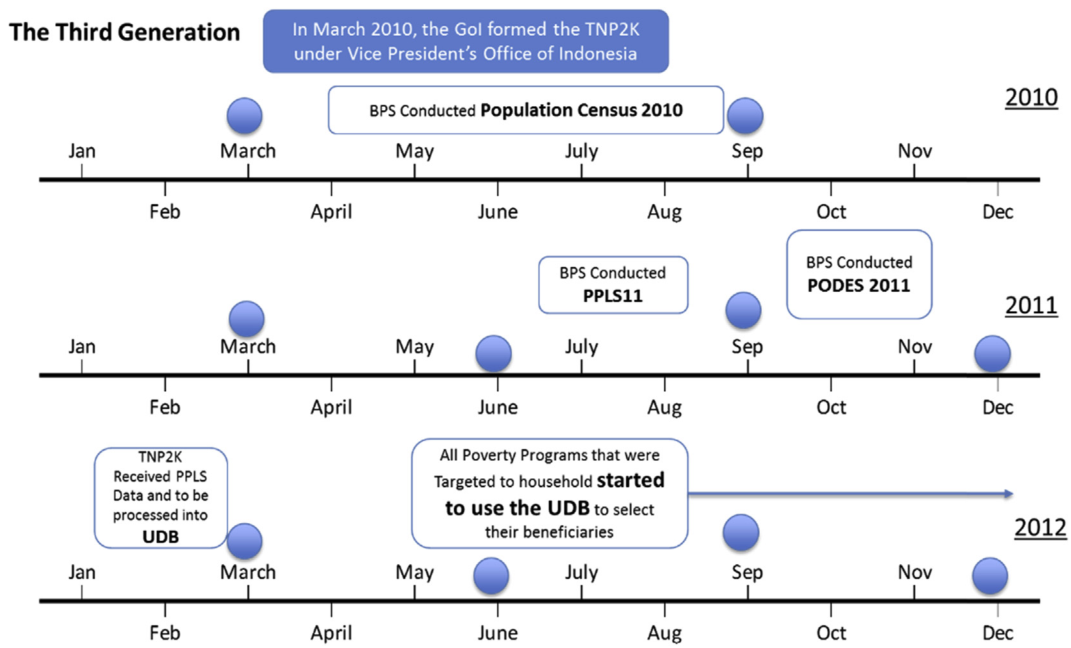


**Fig. 2.** Third generation of social protection programs and development of the UDB

directly to households where possible.[16] The overarching aim of the KPS, was to reinforce beneficiaries' rights to program entitlement, through receipt of the physical card as well as through the provision of an information pack that was delivered alongside the KPS. Given that the beneficiary list dervied from the UDB however, it is still possible that the Type I and Type II errors that remained features of the UDB were carried over to the disbursement of the KPS. These are described in further detail below.

### 1.4. Targeting under complementarities

The most popular indicators to measure targeting performance are: Type I errors (*undercoverage*) and Type II errors (*leakage*), although these have been developed along a number of dimensions (see: Coady et al., 2004, Galasso and Ravallion, 2005 and World Bank, 2012b). An important feature of all these performance measures is that they only evaluate the performance of single poverty programs. To evaluate the targeting performance of multiple programs simultaneously, we rather need to adopt an alternative approach, as shown in Table 1, wherein errors of inclusion and exclusion can be redefined under conditions of complementarities.

Following the notation and logic of Ravallion (1990), the three programs, BLT, *Raskin*, and *Jamkesmas* are abbreviated using (B), (R), and (J) respectively. Therefore, $B_B$ or $B_R$ or $B_J$ (the total number of beneficiaries in each program) is equal to P (the total number of individuals deemed poor). Similarly, the total number of non-beneficiaries under each program is denoted by NP. The term $ee_T$ refers to the share of poor households that do not receive any programs relative to the total number of poor households (i.e. errors of exclusion), and can formally be written as:

$$ee_T = \frac{E2_B}{P} + \frac{E2_R}{P} + \frac{E2_J}{P} + = \frac{E2_B + E2_R + E2_J}{P} \quad (1)$$

[16] According to the TNP2K (2015), between June and November 2013, the GoI sent the KPS card to 15, 530, 897 beneficiaries. At the end of the period, however, PT Pos reported that only 402,861 (i.e. 2.6% of the total) had been returned.

**Table 1**
Targeting matrix of the complementary multiple programs.

| | | Poverty Status | | Total |
|---|---|---|---|---|
| | | Poor | Non-poor | |
| Beneficiaries status of program BLT | Beneficiary | Correct inclusion ($C1_B$) | Error of Inclusion ($E1_B$) | $B_B$ |
| | Non- beneficiary | Error of Exclusion ($E2_B$) | Correct Exclusion ($C2_B$) | $NB_B$ |
| Beneficiaries status of program *Raskin* | Beneficiary | Correct inclusion ($C1_R$) | Error Inclusion ($E1_R$) | $B_R$ |
| | Non- beneficiary | Error of Exclusion ($E2_R$) | Correct Exclusion ($C2_R$) | $NB_R$ |
| Beneficiaries status of program *Jamkesmas* | Beneficiary | Correct inclusion ($C1_J$) | Error of Inclusion ($E1_J$) | $B_J$ |
| | Non- beneficiary | Error of Exclusion ($E2_J$) | Correct Exclusion ($C2_J$) | $NB_J$ |
| | | P | NP | T |

This table represent an extension of the standard matrix used in evaluation the performance of poverty targeting. The information about the standard matrix can be found in studies by Coady et al. (2004).

**Table 2**
Joint and marginal probabilities of poor households receiving poverty programs.

| | Joint probability | Marginal (BLT) | Marginal (*Raskin*) | Marginal (*Jamkesmas*) | Total |
|---|---|---|---|---|---|
| (1) | (2) | (3) | (4) | (5) | |
| BLT only | $(C_{B=1,R=0,\ J=0}\ /P)$ | $(C_{B=1,R=0,\ J=0}\ /P)$ | – | – | |
| *Raskin* only | $(C_{B=0,R=1,\ J=0}\ /P)$ | – | $(C_{B=0,R=1,\ J=0}\ /P)$ | – | |
| *Jamkesmas* only | $(C_{B=0,R=0,\ J=1}\ /P)$ | – | – | $(C_{B=0,R=0,\ J=1}\ /P)$ | |
| BLT and *Raskin* only | $(C_{B=1,R=1,\ J=0}\ /P)$ | $(C_{B=1,R=1,\ J=0}\ /P)$ | $(C_{B=1,R=1,\ J=0}\ /P)$ | | |
| BLT and *Jamkesmas* only | $(C_{B=1,J=1,\ R=0}\ /P)$ | $(C_{B=1,J=1,\ R=0}\ /P)$ | – | $(C_{B=1,J=1,\ R=0}\ /P)$ | |
| *Raskin* and *Jamkesmas* only | $(C_{R=1,\ J=1,\ B=0}\ /P)$ | – | $(C_{R=1,\ J=1,\ B=0}\ /P)$ | $(C_{R=1,\ J=1,\ B=0}\ /P)$ | |
| BLT, *Raskin* and *Jamkesmas* | $(C_{B=1,R=1,J=1}\ /P)$ | $(C_{B=1,R=1,J=1}\ /P)$ | $(C_{B=1,R=1,J=1}\ /P)$ | $(C_{B=1,R=1,J=1}\ /P)$ | |
| None | $(C_{B=0,R=0,\ J=0}\ /P)$ | – | – | – | $ee_T$ |
| Total | 100 | $C1_B\ /P$ | $C1_R\ /P$ | $C1_J\ /P$ | |

This table is constructed using information from Table 1 to measure the degree of complementarity of each program with respect to the others. For example, under perfect complementarities, the joint probability of poor households receiving three programs will be equal to the total marginal probability for all programs. This implies that under this condition, the joint probability for poor households receiving either one or two programs will be zero.

The error of inclusion $ei_T$, the ratio of non-poor beneficiaries to the total number of beneficiaries in each program is:

$$ei_T = \frac{E1_B}{B_R} + \frac{E1_R}{B_B} + \frac{E1_J}{B_J} = \frac{E1_B + E1_R + E1_J}{P} \quad (2)$$

To evaluate poverty targeting under program complementarities, we propose evaluation methods using probabilities that measure the likelihood of a poor household receiving either one, two, or all three programs simultaneously, else no program at all. Table 2 (column 2) shows the joint probabilities of poor households participating in one, two, all three programs, or no program at any given time. The marginal probabilities of poor households receiving each program are presented in Columns 3–5 of Table 2. Under conditions of perfect complementarities, the joint probability of poor households receiving all three programs will be equal to the sum of the constituent marginal probabilities.[17]

Using the information in Table 2, we further measure the degree of complementarity of each program with respect to the others. For example, the complementarity between BLT and *Raskin* is measured by:

$$P(BLT = 1|Raskin = 1) = \frac{(C_{B=1,R=1,\ J=0}/P) + (C_{B=1,R=1,J=1}/P)}{C1_B/P} \quad (3)$$

Where $P(BLT = 1|Raskin = 1)$ denotes the conditional probability of poor households receiving the BLT, given that they also receive benefits from the *Raskin* program. The term $(C_{B=1,R=1,\ J=0}\ /P)$ represents the joint probability of receiving both BLT and *Raskin* programs and the expression $(C_{B=1,R=1,J=1}\ /P)$ is the joint probability of receiving all three programs. The denominator$C1_B\ /P$, refers to marginal probability of receiving the BLT program.

The complementarity of the BLT program with respect to the two other programs can be assessed using:

$$P(BLT = 1|Raskin = 1,\ Jamkesmas = 1) = \frac{(C_{B=1,R=1,J=1}/P)}{((C_{B=1,R=1,J=1}/P) + (C_{R=1,\ J=1,\ B=0}/P))}\ X\ 100 \quad (4)$$

Where $P(BLT = 1|Raskin = 1,\ Jamkesmas = 1)$ measures the likelihood of a poor household receiving BLT given that they also participate in the other two programs. The term $(C_{B=1,R=1,J=1}\ /P)$ represents the joint probability of poor households participating in those three programs, while $(C_{R=1,\ J=1,\ B=0}\ /P)$ is the joint probability of the poor households receiving both *Raskin* and *Jamkesmas* programs.

## 2. Data

To evaluate the performance of poverty targeting under the UDB, the analysis draws on data from the National Socioeconomic Survey (SUSENAS), the Social Protection Survey (SPS) and the Village Potential Census (PODES) described in detail below. Fig. 3 provides a time-line of the various data collection activities.

### 2.1. SUSENAS surveys

In this paper, we utilize data from the 2005, 2009 and 2014 waves of the SUSENAS survey to: (1) measure the benefit incidence from poverty programs and their targeting performance relative to previous efforts; (2) predict the poverty level of each household; and (3) estimate the relationship between poverty, social protection eligibility and household characteristics, particularly using the 2014 SUSENAS survey.[18]

[17] Under this condition, the joint probability for poor households receiving either one or two programs will therefore be zero.

[18] The National Socioeconomic Survey (SUSENAS) is an annual cross-sectional, nationally representative dataset, initiated in 1963–1964 and fielded once every year or two since then. In 2011, however, the BPS changed the survey frequency to quarterly. This covers some 300,000 individuals and 75,000 households quarterly.

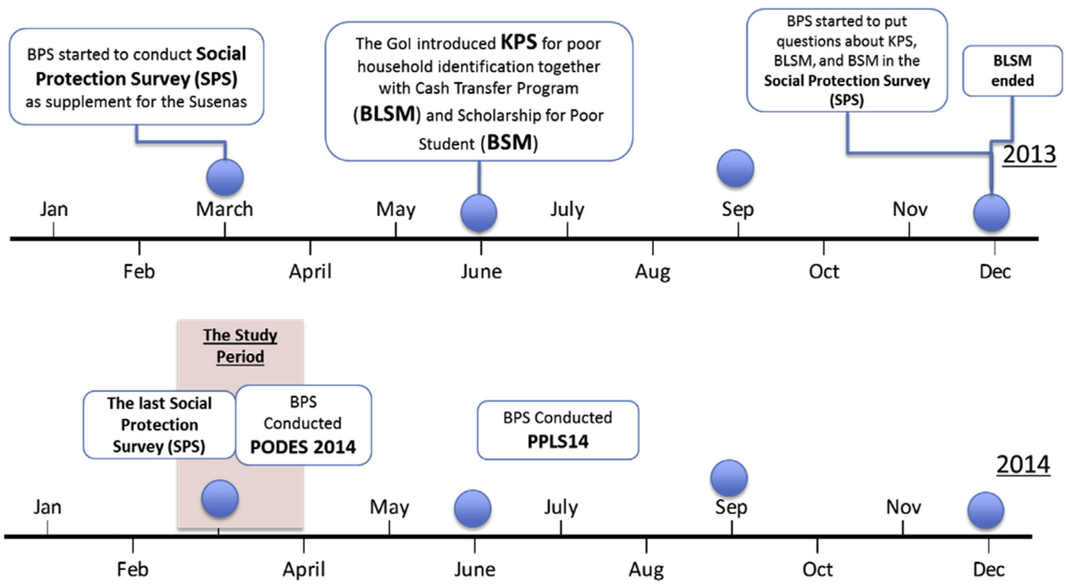


**Fig. 3.** Time horizon of data collection in the periods between 2013 and 2014.

**Table 3**
Observed joint and marginal probabilities of the poor household receiving the poverty programs.

| Targeting Methods → | 2005[a] | | | | 2009[a] | | | | 2014[b] | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Probabilities → | Joint | Marginal Probabilities | | | Joint | Marginal Probabilities | | | Joint | Marginal Probabilities | | |
| Programs ↓ | | BLT | *Raskin* | *Jamkesmas* | | BLT | *Raskin* | *Jamkesmas* | | BLT | *Raskin* | *Jamkesmas* |
| BLT only | 7.89 | 7.89 | | | 5.30 | 5.30 | | | 3.90 | 3.90 | | |
| *Raskin* only | 18.28 | | 18.28 | | 23.22 | | 23.22 | | 19.26 | | 19.26 | |
| *Jamkesmas* only | 0.93 | | | 0.93 | 1.75 | | | 1.75 | 4.42 | | | 4.42 |
| BLT and *Raskin* only | 30.96 | 30.96 | 30.96 | | 24.78 | 24.78 | 24.78 | | 9.60 | 9.60 | 9.60 | |
| BLT and *Jamkesmas* only | 1.73 | 1.73 | | 1.73 | 1.66 | 1.66 | | 1.66 | 8.36 | 8.36 | | 8.36 |
| *Raskin* and *Jamkesmas* only | 2.93 | | 2.93 | 2.93 | 3.29 | | 3.29 | 3.29 | 9.22 | | 9.22 | 9.22 |
| BLT, *Raskin* and *Jamkesmas* | 15.66 | 15.66 | 15.66 | 15.66 | 12.49 | 12.49 | 12.49 | 12.49 | 27.34 | 27.34 | 27.34 | 27.34 |
| None | 21.62 | | | | 27.51 | | | | 17.91 | | | |
| Total | 100.00 | 56.24 | 67.83 | 21.23 | 100.00 | 44.23 | 63.78 | 19.20 | 100.00 | 49.19 | 65.41 | 49.33 |

This table presents the joint and marginal probability of poor households receiving either one, two or all three programs, measured using the formula presented in Table 2. Source: Authors' calculation. Note: a) measured using SUSENAS 2006 and 2009; b) measured using SUSENAS and Social Protection Survey (SPS) 2014.

### 2.2. Social Protection Survey (SPS)

The second dataset used in the analysis is the 2014 Social Protection Survey (SPS). This survey was implemented from the first quarter of 2013 to the first quarter of 2014 and was specifically aimed at examining the performance of poverty targeting under the implementation of the UDB. A question pertaining to the KPS was only asked in the last two rounds. Therefore, we use data from the first quarter of 2014 since it was the period just after the implementation of the KPS. We use this survey to obtain information about the implementation of KPS relating to the benefits received by poor households from the poverty targeting.

### 2.3. Village census (PODES)

The last source of data is from the 2014 PODES, which provides information on all villages/*desa* in Indonesia. This village census covers a sample of around 80,000 villages and is fielded around periodic censuses. It includes useful information on village characteristics, including the main sources of income, population and labor force characteristics, socio-culture, type of village administration and other relevant village-level information.

### 2.4. Merging the datasets

Since 2011 the BPS has not published the village and subdistrict codes for the SUSENAS dataset, making the process for merging these datasets challenging. In meeting this challenge, we construct our data as follows:

i) We merge the Quarter 1 2014 SPS with Quarter 1 2014 SUSENAS using the household ID that is available in both datasets. In all, we merge 70,336 households of the SPS sample to the total 71,051 sample of the SUSENAS.
ii) We merge those two datasets with the 2014 pooled SUSENAS data to obtain village and sub-district IDs using a 'bridging code' shared privately with us.[19]
iii) Finally, we merge the resulting dataset with the PODES data using the village ID to obtain village level variables. After merging with the PODES data, we are able to identify 67,118 households as well as details of their expenditure, social protection and village

[19] We are grateful to a staff member of the TNP2K targeting team who provided us with this bridging code.

information that can be combined with the official PMT coefficients in order to obtain individual household PMT scores (see below).

In addition to the merging, the administrative data were relied upon to estimate household per capita expenditure in 2011. This was calculated first by applying the 2011 UDB district-specific coefficients to the 2014 SUSENAS household data comprising all variables used to calculate the PMT score (please refer to Table A5) to generate an estimate of per capita household expenditure in 2011. This was then adjusted for CPI inflation during the first quarter of 2011 so as to make per capita household expenditure as close as possible to the pre-treatment conditions in the second and the third quarter of 2011. To validate our estimates of real per capita expenditure at the district level, we compared them to the real per capita expenditure data from SUSENAS, 2011. No significant differences were found. This is unsurprising since the PMT coefficients used in the UDB were developed using data from SUSENAS, 2011. Nevertheless, our recovery of the 2011 household per capita expenditures raises the spectre of measurement error in our dependent variable, although ceteris paribus this should not affect our coefficient estimates but rather widen our confidence intervals. All the variables used in this study are presented in Tables A1 and A2 in the Appendix.

## 3. The implementation of the UDB, targeting errors and complementarities

In this section, using our nationally representative data, we document the evolution of targeting outcomes in Indonesia under the condition of complementarities between 2005 and 2014.[20] The poor performance of poverty targeting based on the PSE05 is confirmed in the 2005 panel (Table 3), which presents the joint and marginal probabilities of receiving different program combinations in that year. The probability of a poor household receiving *Raskin* was 67.83 percent, significantly higher than the 56.24 percent for BLT and 21.25 percent for *Jamkesmas*, respectively. Another striking feature is with regards to program complementarities. For example, as shown in the first column of Table 3, only 15.66 percent of eligible poor households received all three programs, while 21.62 percent of eligible households received none.[21]

The results of targeting based on the PPLS08 for the 2009 panels are presented in Tables 3 and 4 respectively. The joint probability of poor households receiving all three programs based on the PPLS08 targeting method is slightly lower than targeting based on PSE05 (12.49 percent as opposed to 15.66 percent). Table 4 shows that in 2009 the complementarities of the three programs were almost identical, albeit a little worse, to the previous targeting regime. For example, among poor households that were *Raskin* recipients, only 58.43 percent received BLT transfers and 24.75 percent received benefits from the *Jamkesmas* program, respectively.

The performance of poverty targeting after the introduction of the UDB in 2011, under unified program eligibility, is significantly better than the targeting based on the PSE05 and PPLS08, as illustrated in the 2014 panels of Tables 3 and 4 There was no significant improvement in the marginal probabilities of receiving *Raskin* or BLT compared to previous targeting efforts, although *Jamkesmas* participation more than doubled. Importantly, from the perspective of program complimentarities, the *joint* probability of participation in all three programs more than doubled between 2009 and 2014, from 12.49 percent to 27.34 percent; while the proportion of poor households that did not receive any program decreased from 27.51 percent to 17.91 percent over the same period. In Table A3, we further show that all these improvements are statistically significant.

Program complementarities dramatically improved following the introduction of the UDB, as shown in Table 4. Among poor households who benefited from the BLT for example, 75.09 percent were also *Raskin* recipients, while 72.56 percent also received *Jamkesmas* benefits. These figures are significantly higher than the unconditional probabilities of receiving *Raskin* (65.41 percent) and *Jamkesmas* (49.33 percent). In 2014, 72.35 percent of *Jamkesmas* beneficiaries from poor households also received BLT and 74.10 percent also received *Raskin*. Overall, in terms of the complementarities with other programs, we observe that access to *Jamkesmas* improved significantly. Prior to the implementation of the UDB, for example, only 33.58 percent and 33.52 percent of poor households in 2005 and 2009 respectively that received *Raskin* and *BLT* had access to *Jamkesmas*. Following the implementation of the UDB however, this percentage increased to 74.01. This improvement can be explained by the fact that prior to the implementation of the UDB, *Jamkesmas* was delivered based on self-targeting through the use of a poverty statement (*Surat Keterangan Miskin*) issued by local leaders (World Bank, 2012a, 2012b).

### 3.1. Did the KPS improve poverty targeting and poverty programs complementarities?

While our previous analysis highlights the improvements made in poverty targeting and program complementarities following the introduction of the UDB, in this section we further examine the impact of the introduction of the KPS on these outcomes.

Table 5 compares the joint and marginal probabilities of participating in the poverty programs comparing poor households that received the KPS (KPS holders) to those that did not (Non-KPS holders). From columns (5) and (9) we observe that the joint probability of participating in all three programs for KPS holders is significantly higher than for non-KPS holders (56.64 percent as opposed to 3.78 percent). Conversely, the joint probability of not receiving any of the three programs for a KPS holder is significantly lower than for a non-KPS holder (0.45 percent compared to 30.83 percent). The marginal probabilities are also much higher for KPS holders. For example, the probability of receiving BLT is 96.25 percent for KPS holders, while it is only 11.45 percent for non-KPS holders. We document these improvements in Table A4 in Appendix, which shows that the differences in joint probabilities over time are statistically significant.

Table 6 demonstrates that the introduction of the KPS also improved the complementarities between poverty programs. Among KPS holders, for example, the likelihood of receiving BLT for those who also received *Raskin* and *Jamkesmas* is much higher than for Non-KPS holders, (97.57 percent as compared with 19.8 percent). Similarly, the probability of KPS holders to receive *Jamkesmas*, while also being BLT and *Raskin* beneficiaries is 77.24 percent, while it is only 48.91 percent for non-KPS holders.

This evidence complements the findings of Banerjee et. al (2018) in the context of the *Raskin* program, since those authors find that eligibility status and information provision significantly increased subsidies received by beneficiaries. While the present study focuses on the extensive margin, we further show that receiving the KPS increases the probability of poor households receiving additional programs.

[20] The sample of households we use for this analysis is the bottom 11.37% of the distribution of households i.e. 'poor' households. This is because, as outlined in World Bank (2012a) the definition of the 'near-poor' proved inconsistent in the time periods before and after the introduction of the Unified Database. In the first part of our analysis therefore, we only document the receipt of programs for households that are eligible for all three programs over the entire period.

[21] The conditional probabilities of participating households in the 2005 panel are provided in Table 4, which can also be used to measure the complementarities between social assistance programs. For example, the probability of a poor household receiving *Raskin*, given that they are a recipient of both BLT and *Jamkesmas* is higher than 90 percent, while the probability of a poor household participating in both BLT and *Raskin* to also receive the *Jamksemas* program is 33.6 percent.

**Table 4**
Observed conditional and unconditional probabilities of poor households receiving poverty programs based on different targeting methods.

| Targeting Methods → | 2005[a] | | | 2009[a] | | | 2014[b] | | |
|---|---|---|---|---|---|---|---|---|---|
| Probabilities → | BLT | *Raskin* | *Jamkesmas* | BLT | *Raskin* | *Jamkesmas* | BLT | *Raskin* | *Jamkesmas* |
| Programs ↓ | | | | | | | | | |
| P (.) | 56.24 | 67.83 | 21.23 | 44.23 | 63.78 | 19.20 | 49.19 | 65.41 | 49.33 |
| P (. \|BLT = 1) | 100.00 | 82.89 | 30.91 | 100.00 | 84.26 | 32.00 | 100.00 | 75.09 | 72.56 |
| P (. \|*Raskin* = 1) | 68.73 | 100.00 | 27.40 | 58.43 | 100.00 | 24.75 | 56.47 | 100.00 | 55.88 |
| P (. \|*Jamkesmas* = 1) | 81.85 | 87.51 | 100.00 | 73.74 | 82.23 | 100.00 | 72.35 | 74.10 | 100.00 |
| P (. \|*Raskin* = 1, *Jamkesmas* = 1) | 84.25 | 100.00 | 100.00 | 79.14 | 100.00 | 100.00 | 74.79 | 100.00 | 100.00 |
| P (. \|BLT = 1, *Jamkesmas* = 1) | 100.00 | 90.07 | 100.00 | 100.00 | 88.25 | 100.00 | 100.00 | 76.59 | 100.00 |
| P (. \|BLT = 1, *Raskin* = 1) | 100.00 | 100.00 | 33.58 | 100.00 | 100.00 | 33.52 | 100.00 | 100.00 | 74.01 |

This table presents conditional and unconditional probabilities measured based on information in Table 3. The number on each cell of the table is derived using formula presented in either Equations (1)–(3), or 4 depending its condition. Note: a) are measured using SUSENAS 2006 and 2009; b) is measured using SUSENAS and Social Protection Survey (SPS) 2014.

**Table 5**
Observed joint and marginal probabilities of the poor household receiving the poverty programs in 2014 (with or without KPS).

| Classification → | Poor Households - All Sample | | | | Poor Households - with KPS | | | | Poor Households - without KPS | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Probabilities → | Joint | Marginal Probabilities | | | Joint | Marginal Probabilities | | | Joint | Marginal Probabilities | | |
| Programs ↓ | | BLT | *Raskin* | *Jamkesmas* | | BLT | *Raskin* | *Jamkesmas* | | BLT | *Raskin* | *Jamkesmas* |
| BLT only | 3.90 | 3.90 | | | 6.37 | 6.37 | | | 1.95 | 1.95 | | |
| *Raskin* only | 19.26 | | 19.26 | | 0.62 | | 0.62 | | 34.87 | | 34.87 | |
| *Jamkesmas* only | 4.42 | | | 4.42 | 1.27 | | | 1.27 | 7.08 | | | 7.08 |
| BLT and *Raskin* only | 9.60 | 9.60 | 9.60 | | 16.69 | 16.69 | 16.69 | | 3.95 | 3.95 | 3.95 | |
| BLT and *Jamkesmas* only | 8.36 | 8.36 | | 8.36 | 16.55 | 16.55 | | 16.55 | 1.78 | 1.78 | | 1.78 |
| *Raskin* and *Jamkesmas* only | 9.22 | | 9.22 | 9.22 | 1.41 | | 1.41 | 1.41 | 15.77 | | 15.77 | 15.77 |
| BLT, *Raskin* and *Jamkesmas* | 27.34 | 27.34 | 27.34 | 27.34 | 56.64 | 56.64 | 56.64 | 56.64 | 3.78 | 3.78 | 3.78 | 3.78 |
| None | 17.91 | | | | 0.45 | | | | 30.83 | | | |
| Total | 100.00 | 49.19 | 65.41 | 49.33 | 100.00 | 96.25 | 75.36 | 75.88 | 100.00 | 11.45 | 58.36 | 28.41 |

This Table presents probabilities measured as in Table 4 by dividing the sample whether the households received KPS or did not. Note: These probabilities are measured using SUSENAS and Social Protection Survey (SPS) 2014.

**Table 6**
Observed conditional and unconditional probabilities of the poor household receiving the poverty programs in 2014 (with or without KPS).

| Classification → | Poor Households - All Sample | | | Poor Households - with KPS | | | Poor Households - without KPS | | |
|---|---|---|---|---|---|---|---|---|---|
| Probabilities → | BLT | *Raskin* | *Jamkesmas* | BLT | *Raskin* | *Jamkesmas* | BLT | *Raskin* | *Jamkesmas* |
| Programs ↓ | | | | | | | | | |
| P (.) | 49.19 | 65.41 | 49.33 | 96.25 | 75.36 | 75.88 | 11.45 | 58.36 | 28.41 |
| P (. \|BLT = 1) | 100.00 | 75.09 | 72.56 | 100.00 | 76.19 | 76.05 | 100.00 | 67.49 | 48.52 |
| P (. \|*Raskin* = 1) | 56.47 | 100.00 | 55.88 | 97.31 | 100.00 | 77.03 | 13.24 | 100.00 | 33.49 |
| P (. \|*Jamkesmas* = 1) | 72.35 | 74.10 | 100.00 | 96.46 | 76.51 | 100.00 | 19.56 | 68.82 | 100.00 |
| P (. \|*Raskin* = 1, *Jamkesmas* = 1) | 74.79 | 100.00 | 100.00 | 97.57 | 100.00 | 100.00 | 19.34 | 100.00 | 100.00 |
| P (. \|BLT = 1, *Jamkesmas* = 1) | 100.00 | 76.59 | 100.00 | 100.00 | 77.39 | 100.00 | 100.00 | 68.02 | 100.00 |
| P (. \|BLT = 1, *Raskin* = 1) | 100.00 | 100.00 | 74.01 | 100.00 | 100.00 | 77.24 | 100.00 | 100.00 | 48.91 |

This table present probabilities measured as in Table 5 with dividing the sample becomes either the households received KPS or did not. These probabilities are measured using SUSENAS and Social Protection Survey (SPS) 2014.

## 4. Empirical estimation

While the introduction of the UDB and the KPS significantly improved poverty targeting and program complimentarities, in this section, we assess the impact of these improvements on household welfare.[22] To assess program complimentarities, an outcome measure that is affected by all three program is required, for which we use household per capita expenditures and a P1 measure of poverty.

For each household $h$, $h = 1,\ 2,\ \ldots N$ in the sample, the triplet $(Y, R, X)$ is observed. $Y$ is the potential outcome, $R$ is a multilevel treatment variable, which takes an integer value between 0 and $P$. $X$ represents the vector of pre-treatment covariates, while $D_h^r(R_h)$ is the indicator of receiving treatment $r$ for household $h$:\

$$D_h^r(R_h) = \begin{cases} 1, & \text{if } R = r \\ 0, & \text{otherwise.} \end{cases}$$

For each household, there is a set of potential outcomes $(Y_h^0, \ \ldots, Y_h^P)$. $Y_h^r$ represents the potential outcome for each household $h$, for which $R = r$ where $r \in \mathbb{N}_0 = \{0, \ldots\ldots, P\}$ In this study, we are primarily interested in the average treatment effect on the treated (ATT), $\theta$, of participating in one or more of several poverty programs $R$, relative to the counterfactual of not receiving one or more of the programs, such that:

[22] Note that in this section we include the bottom 40% of households in our sample, in other words, all ‘poor’, ‘near poor’ and vulnerable households. The fact that ‘vulnerable’ households are only eligible for Jamkesmas is irrelevant for our analysis, since the prevalence of Type I and Type II errors in each strata of the propensity score provides sufficient counterfactual observations. Also note that due to our stratification of the propensity score, our matching process will likely never match and thus compare ‘poor’ and ‘vulnerable’ households since they will differ in their PMT score.

$$\theta_{\widetilde{rc}} \equiv \theta_r - \theta_c \equiv E\left[Y_h^r(\theta_r) - Y_h^c(\theta_c)\ |R = r\right] \quad (5)$$

for potential outcomes of household $h$. The different treatment level received by each household given pre-treatment variables is represented either by $r$ or $c$. Our goal is to identify the parameter vector $\delta \equiv \theta_{\widetilde{rc}}$. We therefore denote the difference in per capita expenditure (PCE) as:

$$(PCE_{h+1}|R = r)e(PEC_{h,h+1}|R = c) = \theta_{rc+\varepsilon h,h+1} \quad (6)$$

where $\theta_{\widetilde{rc}} = \theta_r - \theta_c$ is the average-on-the-treated effect and $\varepsilon_{h,\ t+1}$ is the error term. Since our study relies on observational data, our aim is to ensure that $\varepsilon_{h,\ t+1}$ is as close as posssible to zero, such that our results equate as closely as possible to a quasi-experimental scenario.[23]

Taking into consideration the advantages of efficiency and practicality, following Hirano et al. (2003), Abadie (2005) and Bazzi et al. (2015), we implement a semiparametric reweighting estimator.[24,25]

### 4.1. Estimation of the propensity score

Propensity score estimation, which can be used to adjust for differences in pre-treatment variables, is a crucial step when matching is implemented as an evaluation strategy (Rosenbaum and Rubin, 1983, 1984). The underlying principle is that the pre-intervention variables that are not influenced by participation in the program should be included in the regression (Jalan and Ravallion, 2003).

Non-experimental estimators can benefit from exploiting the program design for identification.[26] The first-best solution is to estimate the propensity scores using both the PMT score generated from official coefficients used by the GoI as well as the underlying variables selected for the construction of the PMT score.[27,28] The PMT score for the poorest 40 percent in the UDB was measured using the district-specific models for the 471 Indonesian districts.[29] We apply these official district-specific coefficients, using data from the first quarter of 2014 to generate $\widehat{p}_h$, the probability that a household received the poverty program i.e. the PMT scores. We implement the procedure from Crump et al (2009) to calculate the optimal bounds. For the sake of comparison we use the same covariates that were used in the PMT model, as detailed in Table A5 of the Appendix.

The results are shown in Fig. 4. The estimation of the propensity score based on the PMT scores alone are shown in the left panel (A), while the estimation using the underlying covariates is shown in the right panel (B). The estimation based on the PMT score is demonstrably better in terms of the considerable overlap in the propensity score of treated (T = 1) and control (T = 0) units. We therefore select the PMT score-based estimates as inverse probability weights to rebalance recipient and non-recipient households along observable dimensions.

### 4.2. Balancing groups

Next we reweight the sample to ensure that the non-treated group is as comparable as possible to the treated group (in terms of the propensity score). As described by Abadie (2005), Smith and Todd (2005) and Busso et al. (2014), all estimators adjusting for covariates can be understood as different methods to weight the observed outcomes using weight, $\widehat{\omega}$.

Under the case of binary treatment, we can rewrite the average treatment effect on the treated as:

$$\widehat{\theta} = {}^1/_{N_1} \sum_{h=1}^{N} \widehat{D_h^r(R_h)}\widehat{\omega}_h Y_h^r - {}^1/_{N_0} \sum_{h=1}^{N}\left(1 - D_h^r(R_h)\right)\widehat{\omega}_h Y_h^0 \quad (7)$$

$$N_1 = \sum_{h=1}^{N} \widehat{D_h^r(R_h)}, \qquad N_0 = N - N_1 \quad (8)$$

Where $N$ represents the sample size of an i.i.d sample, $N_1$ denotes the size of the treated subsample and $\widehat{D_h^r(R_h)}$ the sample's predicted probability of receiving any poverty programs.

Following Busso et al. (2014), we normalize the weights such that: ${}^1/_{N_0} \sum_{h=1}^{N}(1 - \widehat{D_h^r(R_h)})\widehat{\omega}_h Y_h^0 = 1$. The contribution of the non-recipient to the counterfactual, $\widehat{\omega}$, can then be directly computed as proportional to their estimated odds of treatment, $\widehat{\omega_h} = \widehat{D_h^r(R_h)} \,/\, (1 - \widehat{D_h^r(R_h)})$.

Fig. 5 shows the distribution of the baseline PMT across treatment levels. Reassuringly, households receiving all three programs are on average relatively poor, compared to other households. Conversely, households that do not receive any program benefits (i.e. our control group) are relatively rich when compared to other groups. After reweighting however, the distribution of the control group moves substantially to the left, therefore significantly improving the overlap with the treatment groups.

### 4.3. Alternative estimators of the average treatment effect

Studies by Imbens and Wooldridge (2009) and Busso et al. (2014) discuss different estimators (beyond OLS) that are suitable under the reweighting approach. Following these, we consider (1) reweighting estimators using the estimated odds of treatment, $\widehat{\omega}$; (2) double robust estimation controlling the IPW estimators using either their propensity scores, $(\widehat{P}_h)$ or the PMT score $(PMT_h)$, as suggested by Scharfstein et al. (1999) and Lunceford and Davidian (2004) [30] (3) Control function

[23] This means that estimation of $\theta_{\widetilde{rc}}$ should satisfy several assumptions including (1) weak unconfoundedness which Imbens and Guido (2000) formally states as follows: $Y_h^r \perp D_h^r(R_h)\ \big|\ X_h,\ \forall r \in \mathbb{N}_0$. where $\perp$ denotes orthogonality or independence. Under this assumption, it requires that all determinants of treatment level and the outcome variable are observed. (2) complete overlap that can be formally stated as follows: $0 < \Pr[R_h = r\ |X_h = h],\ \forall r \in \mathbb{N}_0$ and $\forall x$ in the support of.$X$.

[24] Reweighting estimators often have better finite sample properties than common matching procedures (Busso et al., 2014), and given that, multiple treatments are considered, it is computationally less complicated.

[25] Despite our privileged access to administrative data, our empirical setting and our emphasis on program complementarities does not naturally lend itself to an RDD as is the case for example in Tohari et al., (2018). This is because in theory, 15% more of the population were eligible for *Jamkesmas* as when compared to *Raskin* and *BLT*, which in turn would mean that in an RDD setting in the case of *Jamkesmas*, we could only compute the ATE for households who received *Jamkesmas* with those that received nothing and *Jameskas* beneficiaries with households that received all three programs.

[26] We also attempted to merge the SPS data with the UDB database so as to estimate the PMT score for each household. Using KPS codes to facilitate the merge, however, we only managed to match 5669 households from the SPS sample of 70,336 and the UDB sample of 25.5 million households. Ultimately, the matched households all belonged to the same consumption decile and did not vary sufficiently in terms of their PMT score, number of poverty programs received and household characteristics. These matched data fail to generate a sufficiently large region of common support, or so-called "failure of common support" (Ravallion, 2007).

[27] We are grateful to TNP2K for providing us with access both to the PPLS 2011 database and the 471 district-specific coefficients for generating the UDB database.

[28] Most covariates attributed to the non-poor condition of households have a negative relationship with the probability of receiving poverty programs, inlcuding (1) the likelihood of male headed households the government programs; (2) the education level of the household head (3) households that have more assets (e.g. gas ≥12 kg; refrigerator; motorcycle).

[29] There are 482 districts in Indonesia, of which we use 471 in our analysis since 11 districts are dropped when we merge our data.

[30] Under this treatment, the estimation produces consistent estimators, while also potentially reducing bias due to any misspecification of the propensity score.

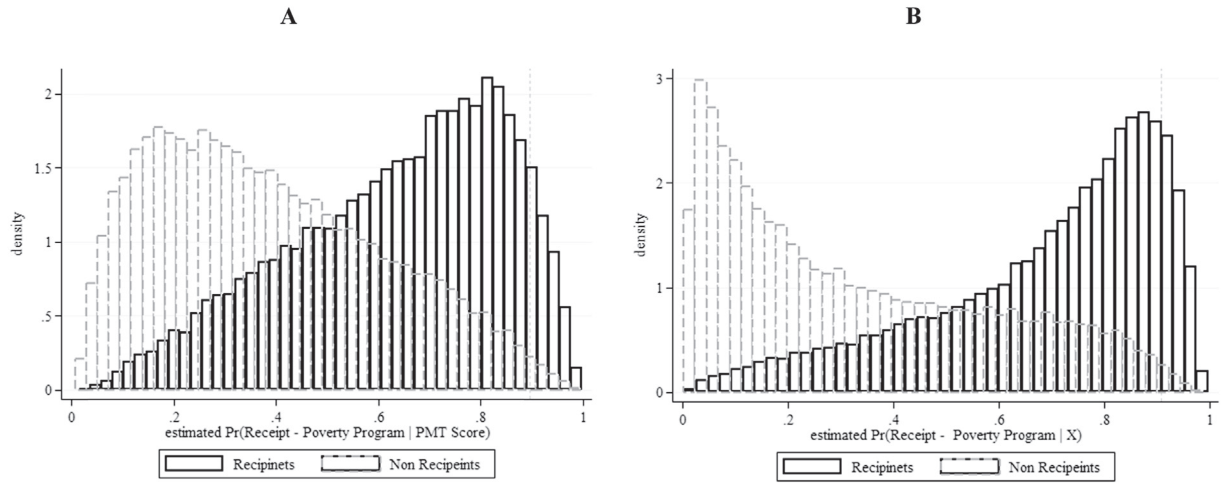


**Fig. 4.** Estimation of Propensity Score based on PMT Score and Underlying Covariates.

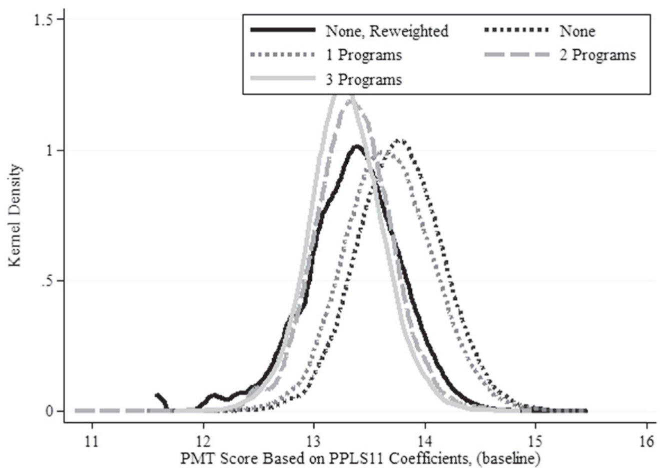


**Fig. 5.** Baseline of PMT score across treatment level.

estimation, following Rosenbaum and Rubin (1984), which stratifies the propensity score (which in turn is based on the PMT score) into five subclasses, while including the PMT score in the first stage regression. The average treatment effect on the treated is then measured within a specific stratum and is then weighted across strata.

## 5. Results

### 5.1. Household per capita expenditure

Table 7 presents the estimation of the ATT using as the dependent variable the difference, between 2011 and 2014 per capita expenditure (PCE) between households before and after treatment. This outcome variable is measured using the ratio of real per capita expenditure in 2014 and an estimate of real per capita expenditure, which is generated using 471 district-specific coefficients of the UDB.

Over the period of study, households that did not receive any poverty program experienced a decrease in their PCE of between 19 and 35 percentage points. Households that received all three programs experienced PCE growth of around 33 percentage points on average (Table 7). Similarly, poor households that received two poverty programs also experienced an increase in their PCE, though at a lower rate when compared to households that received all three. Relative to households that did not receive any program, households that received two programs experienced a rise in per capita expenditure of about 26 percentage points on average. Households that received only one program experienced negative growth in PCE of 13 percentage points on average. Comparing these households to the non-receiving group however, we observe that these households are still better off (by around 15 percentage points) relative to those households that did not receive any program.

Summing up, the implementation of multifaceted poverty programs are shown to significantly impact on per capita expenditures of poor households. A household that received only one program experienced a negative growth in PCE. This may be because during the study period, the GoI reduced the fuel price subsidy, which resulted in inflation in the basket of goods used by the poor (World Bank, 2006; Yusuf and Resosudarmo, 2008).

### 5.2. Robustness check: alternative outcome variable

As an alternative outcome variable, one which is affected by all three social programs that we are evaluating, we use a P1 measure of poverty, a normalised per capita poverty gap (Foster et al., 1984). These results confirm the monotonic increases of the impact of multifaceted programs, in other words households that received a greater number of programs experienced a larger decrease in the P1 measure of poverty. For example, households that receive all three programs (in the upper panel of Table 8) experience a decrease of around 0.9 percentage points on average. Concurrently, the poverty gap of the control group increases by almost 2.6 percentage points. Taken together, we observe that on average the poverty gap of those households that receive all three programs shrank by around 3.5 percentage points.

### 5.3. Robustness check: generalised propensity score

Taking into consideration the multilevel treatment and joint inference due to complementarities of the programs, next we discuss the robustness of our findings accounting for alternative approaches to deal with multiple treatments (see: Athey and Imbens, 2017). For example, Imbens and Guido (2000) and Hirano and Imbens (2004) propose the use of a Generalised Propensity Score (GPS) which is a generalization of the conventional binary-case matching estimation. Under the GPS, the conditional probability of receiving a specific level of treatment given pre-treatment variables is defined as:

$$g(r,x) \equiv \Pr[R_h = r \mid X_h = x] = E\left[D_h^r(R_h) \mid X_h = x\right]. \tag{9}$$

The average potential outcomes can also be identified, as in the binary treatment case, by weighting observed outcomes with the conditional probability of receiving treatment, as follows:

**Table 7**
Difference in per capita expenditures.

| Estimator | OLS | IPW | Double Robustness $(\hat{p}_h)$ | Double Robustness $(PMT_h)$ | Control Function |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *3 Programs vs. None* | | | | | |
| | 0.085 | 0.085 | 0.126 | 0.126 | 0.143 |
| | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) |
| | −0.248 | −0.284 | −0.274 | −0.274 | −0.190 |
| | (0.009) | (0.010) | (0.009) | (0.009) | (0.011) |
| | **0.333** | **0.369** | **0.400** | **0.400** | **0.332** |
| | (0.013) | (0.014) | (0.013) | (0.014) | (0.013) |
| Reweighted | No | Yes | Yes | Yes | Yes |
| Propensity Score Control | No | No | Yes | No | Yes |
| PMT score control | No | No | No | Yes | No |
| Number of Households | 63,681 | 66,972 | 66,972 | 66,972 | 66,972 |
| | 0.154 | 0.159 | 0.178 | 0.178 | 0.184 |
| *2 Programs vs. None* | | | | | |
| | 0.036 | 0.034 | 0.053 | 0.052 | 0.058 |
| | (0.010) | (0.010) | (0.011) | (0.011) | (0.011) |
| | −0.256 | −0.293 | −0.287 | −0.287 | −0.204 |
| | (0.009) | (0.009) | (0.010) | (0.009) | (0.012) |
| | **0.292** | **0.327** | **0.340** | **0.339** | **0.262** |
| | (0.011) | (0.012) | (0.013) | (0.013) | (0.013) |
| Reweighted | No | Yes | Yes | Yes | Yes |
| Propensity Score Control | No | No | Yes | No | Yes |
| PMT score control | No | No | No | Yes | No |
| Number of Households | 63,681 | 66,972 | 66,972 | 66,972 | 66,972 |
| | 0.153 | 0.158 | 0.177 | 0.176 | 0.182 |
| *1 Program vs. None* | | | | | |
| | −0.068 | −0.064 | −0.104 | −0.105 | −0.130 |
| | (0.010) | (0.010) | (0.011) | (0.010) | (0.010) |
| | −0.300 | −0.334 | −0.353 | −0.352 | −0.276 |
| | (0.010) | (0.011) | (0.012) | (0.012) | (0.012) |
| | **0.232** | **0.270** | **0.248** | **0.248** | **0.146** |
| | (0.009) | (0.010) | (0.010) | (0.010) | (0.012) |
| Reweighted | No | Yes | Yes | Yes | Yes |
| Propensity Score Control | No | No | Yes | No | Yes |
| PMT score control | No | No | No | Yes | No |
| Number of Households | 63,681 | 66,972 | 66,972 | 66,972 | 66,972 |
| | 0.154 | 0.159 | 0.179 | 0.178 | 0.185 |

Notes: the dependent variable is the difference log total per capita expenditure between before and after treatment. The first column denotes the pure OLS estimation, while the next column is the results of IPW estimator. Column 3 and 4 are the double robustness estimation which is proposed by Scharfstein et al. (1999), and column 5 is the five-subclass estimation following Rosenbaum and Rubin (1984). The standard errors (presented in parentheses) in column 2–5 are clustered by the village and computed over the entire two-step using a block bootstrap with 500 repetitions following Cameron et al, 2008.

**Table 8**
Differences in poverty gap indices (FGT).

| Estimator | OLS | IPW | Double Robustness $(\hat{p}_h)$ | Double Robustness $(PMT_h)$ | Control Function |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *3 Programs vs. None* | | | | | |
| | −0.017 | −0.017 | −0.010 | −0.009 | −0.009 |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| | 0.019 | 0.007 | 0.008 | 0.008 | 0.026 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.002) |
| | **−0.0358** | **−0.023** | **−0.018** | **−0.018** | **−0.035** |
| | (0.001) | (0.002) | (0.002) | (0.002) | (0.002) |
| Reweighted | No | Yes | Yes | Yes | Yes |
| Propensity Score Control | No | No | Yes | No | Yes |
| PMT score control | No | No | No | Yes | No |
| Number of Households | 63,681 | 66,972 | 66,972 | 66,972 | 66,972 |
| | 0.057 | 0.0561 | 0.103 | 0.107 | 0.112 |
| *2 Programs vs. None* | | | | | |
| | −0.010 | −0.008 | −0.005 | −0.005 | −0.006 |
| | (0.001) | (0.002) | (0.002) | (0.002) | (0.001) |
| | 0.020 | 0.008 | 0.009 | 0.009 | 0.027 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.002) |
| | **−0.0298** | **−0.016** | **−0.014** | **−0.014** | **−0.032** |
| | (0.001) | (0.002) | (0.002) | (0.002) | (0.002) |
| Reweighted | No | Yes | Yes | Yes | Yes |
| Propensity Score Control | No | No | Yes | No | Yes |
| PMT score control | No | No | No | Yes | No |
| Number of Households | 63,681 | 66,972 | 66,972 | 66,972 | 66,972 |
| | 0.054 | 0.0538 | 0.103 | 0.106 | 0.112 |
| *1 Programs vs. None* | | | | | |
| | 0.018 | 0.018 | 0.011 | 0.010 | 0.009 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| | 0.031 | 0.019 | 0.016 | 0.015 | 0.032 |
| | (0.001) | (0.002) | (0.002) | (0.002) | (0.002) |
| | **−0.0137** | **−0.001** | **−0.005** | **−0.005** | **−0.023** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.002) |
| Reweighted | No | Yes | Yes | Yes | Yes |
| Propensity Score Control | No | No | Yes | No | Yes |
| PMT score control | No | No | No | Yes | No |
| Number of Households | 63,681 | 66,972 | 66,972 | 66,972 | 66,972 |
| | 0.060 | 0.0589 | 0.104 | 0.108 | 0.113 |

Notes: the dependent variable is the difference of Poverty Gap Index (FGT1) between 2011 and March 2014. All conditions used in the estimation can be seen in Table 7.

$$E\left[\frac{Y_h D_h^r(R_h)}{g(r, X_h)}\right] = E\left[Y_h^r\right]. \tag{10}$$

In the implementation of the GPS, $g(r,x)$ is usually unknown, but can be estimated using discrete response models if the multivalued treatment does not have a logical ordering, or by ordered response models if a natural ordering exists (Imbens and Guido, 2000). All methods that assume uncounfoudedness however depend on the selection of covariates used in both measuring the GPS and estimating the outcomes.

Under the case of multivalued treatments in the GPS approach, the current literature has classified the estimation approaches into three groups. The first is based on regression adjustment, under which the conditional mean function of the potential outcomes for households who received treatment $r$ can be defined as:

$$\theta_r = E\left[Y_h^r \,|X_h\right] = E[Y_h \,|R = r,\ X_h] = \gamma_0^r + X_h^{'}\gamma_h^r\ , \forall r \in \mathbb{N}_0 \tag{11}$$

The average treatment effects from two levels of treatment of the regression adjustment (RA) is then:

$$\widehat{\theta}_{\widetilde{rc}}^{RA} = \frac{1}{N}\sum_{h=1}^{N}\left(\widehat{\gamma}_0^r + X_h^{'}\widehat{\gamma}_h^r\right) - \left(\widehat{\gamma}_0^c + X_h^{'}\widehat{\gamma}_h^c\right) \tag{12}$$

$$\theta_{\widetilde{rc}}^{IPTW} = \frac{1}{N}\sum_{h=1}^{N}\left(\widehat{\theta}_r\,(X_h) - \widehat{\theta}_c(X_h)\right)$$

Where $r$ and $c$ represent levels of treatment received by each household given pre-treatment variables. The implementation of regression adjustment should be conducted carefully however, since it can generate a biased treatment effect due to misspecifications of the functional form of the outcome model (Drake, 1993; Abadie and Imbens, 2011).

The second estimation method used under the GPS approach is based on weighting estimators; the most popular of which is to use the inverse probability of treatment weighting (IPW). Under this estimator, the average treatment effect sample counterpart of equation (10) is given as:

$$\widehat{\theta}_{\widetilde{rc}}^{IPW} = \frac{1}{N}\sum_{h=1}^{N}\frac{Y_hD_h^r(R_h)}{\widehat{g}(r,\ X_h)} - \frac{1}{N}\sum_{h=1}^{N}\frac{Y_hD_h^c(R_h)}{\widehat{g}(c,\ X_h)} = \widehat{\theta}_r - \widehat{\theta}_c \tag{13}$$

Where $\widehat{g}(r,\ X_h)$ is the estimated GPS. Following Busso et al. (2014), as in the binary treatment in equation (7), we normalize the weights such that:

$$\widehat{\theta}_{\widetilde{rc}}^{IPW} = \left[\sum_{h=1}^{N}\frac{Y_hD_h^r(R_h)}{\widehat{g}(r,\ X_h)} \Big/ \sum_{h=1}^{N}\frac{D_h^r(R_h)}{\widehat{g}(r,\ X_h)}\right] - \left[\sum_{h=1}^{N}\frac{Y_hD_h^c(R_h)}{\widehat{g}(c,\ X_h)} \Big/ \sum_{h=1}^{N}\frac{D_h^c(R_h)}{\widehat{g}(c,\ X_h)}\right] \tag{14}$$

Cattaneo (2010) shows that $\widehat{\theta}_{\widetilde{rc}}^{IPW}$ emerges from the generalised method of moments (GMM) representation of treatment effects. Despite its advantage that the degree of overlap in the distribution of covariates between treatment levels can be easily summerised in numeric forms, the IPW has limitations including (1) the treatment effect can become distorted when the overlap assumption is violated, and (2) poorly estimated coefficients can result when the weights for few variables are relatively large.

Another alternative to estimate causal effects under the condition of multiple treatments, is the Augmented IPTW (A-IPTW) which Cattaneo (2010) also terms the Efficient Influence Function (EIF). If the GPS is correctly specified, the unconditional mean can be estimated using (Cattaneo, 2010):

$$\widehat{\theta}_{\widetilde{rc}}^{EIF} = \frac{1}{N}\sum_{h=1}^{N}\left[\frac{Y_hD_h^r(R_h)}{\widehat{g}(r,\ X_h)} - \left\{\frac{D_h^r(R_h)}{\widehat{g}(r,\ X_h)} - 1\right\}\widehat{Y}_h(R_h)\right] \tag{15}$$

Where $\widehat{Y}_h$ is the predicted outcome that is obtained from regressing $Y_h$ on $X_h$ for those observations with $D_h^r(R_h) = 1$. GMM can then be utilized to estimate equation (15) and measure its standard errors (Cattaneo, 2010).

To simulataneously estimate the average treatment effect of targeted social programs under the condition of program complimentarities we therefore consider: (1) The Regression Adjustment estimator of the ATE i.e. equations (12) and (2) The IPW estimator i.e. equations (13), and (3) two double robust estimators including EIF (equation (15)) as well as the inverse probability weighted regression adjustment (IPW-RA) following Uysal (2015); the results of which are contained in Table 9.

**Table 9**
Difference on Per Capita Expenditure estimated by Multivalued Treatment Effects.

| Estimators | RA | IPW | Double Robustness | |
|---|---|---|---|---|
| | | | EIF | IPW-RA |
| | (1) | (2) | (3) | (4) |
| 1 Program vs None | 0.162 | 0.162 | 0.124 | 0.172 |
| | (0.007) | (0.010) | (0.008) | (0.008) |
| 2 Programs vs None | 0.259 | 0.236 | 0.197 | 0.264 |
| | (0.012) | (0.019) | (0.013) | (0.016) |
| 3 Programs vs None | 0.329 | 0.287 | 0.214 | 0.303 |
| | (0.015) | (0.024) | (0.016) | (0.021) |
| Covariates Control | Yes | Yes | Yes | Yes |
| PMT score control | Yes | Yes | Yes | Yes |
| Number of Households | 66,972 | 66,972 | 66,972 | 66,972 |

Notes: the dependent variable is the difference log total per capita expenditure between before and after treatment. All estimators are estimated under the multivalued approach using Generalised Propensity Score estimation. The estimation of average treatment effects use: the Regression Adjustment approach as in equation (12) in Column (1); the IPW estimator as in equation (13) in Column (2); and two double robust estimators including EIF as in equation (15) in Column (3) and inverse probability weighted regression adjustment (IPW-RA) following Uysal (2015) in Column (4). The standard errors are presented in parentheses.

As in the binary case, the multiple treatment approaches generate estimates of the average treatment effects that monotonically increase with the receipt of increased number of programs. The results in Table 9 are somewhat smaller in magnitude however, especially for the one vs. no program case.

### 5.4. Does the type of the program matter?

We also examine which type of program delivers the greatest impact on household per capita expenditure, thereby contributing to the debate on cash vs. in-kind transfers and the circumstances in which they apply (see for example: Lindert et al. (2007), Currie and Gahvari (2008), Khera (2014) and Hidrobo et al. (2014)). In Table 10, we compare the per capita expenditures of households in receipt of every combination of poverty program. The bottom rows of columns 9–11 show that the impact of receiving a single program is marginal and statistically insignificant. In the bottom row of column 10 for example, we can see that the impact on household expenditure of receiving either BLT or *Raskin* is statistically insignificant from one another. Even though the subsidy received from BLT (cash with amount IDR. 100.000 (or USD 10)) is slightly higher than from *Raskin* (IDR. 80.000 (or USD 8)). Interestingly, if we compare the benefit of receiving two programs, *Raskin* and *Jamkesmas*, there is no differential impact and the coefficients are statistically insignificant. We hypothesise that this may be because the benefits of either *Raskin* or BLT are marginal to the total household expenditure. Moreover, this evidence contradicts previous research by Hidrobo et al. (2014), who claim that cash is preferable if the objective of the transfers are to improve household welfare.

## 6. Conclusion

We contribute to the poverty targeting literature along two dimensions. First, we provide the first judicious evaluation of unified program eligibility, in the context of Indonesia's Unified Targeting System, which was introduced to reduce targeting errors while increasing complementarities between programs. Secondly, we account for program

**Table 10**
Matrix Comparison – Combination between Treatments and Controls using Control Function Estimation.

| | None | 3 Programs | 2 Program | 1 Program | BLT and *Raskin* only | BLT and *Jamkesmas* only | *Raskin* and *Jamkesmas* only | BLT only | *Raskin* only | *Jamkesmas* only |
|---|---|---|---|---|---|---|---|---|---|---|
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| None | | 0.316 | 0.247 | 0.128 | 0.245 | 0.274 | 0.232 | 0.162 | 0.129 | 0.136 |
| | | (0.014) | (0.012) | (0.011) | (0.019) | (0.020) | (0.016) | (0.038) | (0.012) | (0.019) |
| 3 Programs | 0.316 | | 0.0731 | 0.184 | 0.071 | 0.0395 | 0.090 | 0.160 | 0.185 | 0.189 |
| | (0.014) | | (0.013) | (0.013) | (0.020) | (0.022) | (0.016) | (0.040) | (0.013) | (0.021) |
| 2 Programs | 0.247 | 0.073 | | 0.110 | | | | 0.0820 | 0.112 | 0.111 |
| | (0.012) | (0.013) | | (0.011) | | | | (0.039) | (0.012) | (0.020) |
| 1 Programs | 0.128 | 0.184 | 0.110 | | 0.112 | 0.145 | 0.092 | | | |
| | (0.011) | (0.013) | (0.011) | | (0.019) | (0.020) | (0.014) | | | |
| BLT and *Raskin* only | 0.245 | 0.071 | | 0.112 | | 0.052 | 0.019 | 0.084 | 0.114 | 0.114 |
| | (0.019) | (0.020) | | (0.019) | | (0.023) | (0.022) | (0.041) | (0.019) | (0.025) |
| BLT and *Jamkesmas* only | 0.274 | 0.040 | | 0.145 | 0.051 | | 0.033 | 0.117 | 0.148 | 0.143 |
| | (0.020) | (0.022) | | (0.020) | (0.023) | | (0.026) | (0.042) | (0.021) | (0.024) |
| *Raskin* and *Jamkesmas* only | 0.232 | 0.090 | | 0.0920 | 0.019 | 0.033 | | 0.068 | 0.093 | 0.095 |
| | (0.016) | (0.016) | | (0.014) | (0.022) | (0.026) | | (0.041) | (0.015) | (0.023) |
| BLT only | 0.162 | 0.160 | 0.082 | | 0.084 | 0.117 | 0.068 | | 0.029 | 0.028 |
| | (0.038) | (0.040) | (0.039) | | (0.041) | (0.042) | (0.041) | | (0.039) | (0.042) |
| *Raskin* only | 0.129 | 0.185 | 0.112 | | 0.114 | 0.148 | 0.093 | 0.029 | | 0.000 |
| | (0.012) | (0.013) | (0.012) | | (0.019) | (0.021) | (0.015) | (0.039) | | (0.020) |
| *Jamkesmas* only | 0.136 | 0.189 | 0.111 | | 0.114 | 0.143 | 0.095 | 0.028 | 0.000 | |
| | (0.019) | (0.021) | (0.020) | | (0.025) | (0.024) | (0.023) | (0.042) | (0.020) | |

Notes: the dependent variable is the difference log total per capita expenditure between before and after treatment. All estimations are conducted under five-subclass estimation following Rosenbaum and Rubin (1984).

complimentarities, both in terms of targeting outcomes and household welfare; since while social programs aimed at targeting poverty are typically rolled out as part of a broader program packages, they are almost exclusively evaluated in isolation.

Our results illustrate the tangible benefits realised as a result of the introduction of the Unified Targeting System. The proportion of households that benefited from all three social programs more than doubled, and furthermore those households in reiept of all three programs are at least 30 percentage points better off than those that receive none. As suggested by Bah et al (2018), the innovation also represented good value for money since the cost of PPLS11 was about 11 percent of the value of additional benefits received by households from the poorest three deciles previously omitted from the registry. Our results also serve as a cautionary tale however, to the results of any policy evaluation that omits complimentary programs since such results might otherwise be upward biased.

Our results also serve as a warning to those countries that are currently rolling out unified targeting systems. While the tangible benefits in the Indonesian context were large, these results are relative to the low base from which the targeting initiatve began. In 2014, 17.5% of households that were eligible for all three social programs in fact received none. Further work is therefore needed to understand exactly why such households are falling through the social net.

## Appendices.

**Table A1**
Household Characteristics by Receiving Poverty Program

| | Non-Receiving Program | | Receiving Program | | Difference | |
|---|---|---|---|---|---|---|
| PMT Score | 13.616 | (0.43) | 13.492 | (0.352) | −0.100 | [0.013] |
| Percapita Expenditure (‘000) | 913.84 | (1034) | 632.980 | (529.81) | −239,707 | [24,571] |
| | | | | | | |
| *Head of Household Characteristics* | | | | | | |
| Male | 0.858 | (0.349) | 0.842 | (0.364) | −0.012 | [0.007] |
| Married | 0.818 | (0.386) | 0.813 | (0.390) | −0.005 | [0.007] |
| Age | 47.676 | (13.568) | 48.811 | (13.737) | 0.789 | [0.179] |
| Education | | | | | | |
| Elementary | 0.202 | (0.401) | 0.248 | (0.432) | 0.042 | [0.008] |
| Junior High | 0.206 | (0.405) | 0.247 | (0.431) | 0.037 | [0.005] |
| Senior High & University | 0.415 | (0.493) | 0.299 | (0.458) | −0.106 | [0.012] |
| Working Status: Employed | 0.879 | (0.326) | 0.893 | (0.310) | 0.015 | [0.005] |
| Employment Sector: | | | | | | |
| Agriculture | 0.362 | (0.481) | 0.457 | (0.498) | 0.098 | [0.010] |
| Mining & Quarrying | 0.020 | (0.139) | 0.015 | (0.122) | −0.000 | [0.002] |
| Processing Industry | 0.063 | (0.242) | 0.068 | (0.251) | −0.001 | [0.003] |
| Trading | 0.128 | (0.334) | 0.112 | (0.315) | −0.016 | [0.004] |
| Construction/building | 0.067 | (0.249) | 0.078 | (0.268) | 0.010 | [0.004] |
| Hotel & Restaurant | 0.013 | (0.115) | 0.009 | (0.097) | −0.004 | [0.001] |
| Transportation & ICT | 0.050 | (0.218) | 0.047 | (0.212) | −0.002 | [0.003] |

(continued on next column)

**Table A1** (*continued*)

| | Non-Receiving Program | | Receiving Program | | Difference | |
|---|---|---|---|---|---|---|
| *Household Characteristics* | | | | | | |
| Size (Person) | 3.878 | (1.725) | 3.787 | (1.687) | −0.056 | [0.021] |
| Dependency ratio | 0.654 | (0.649) | 0.650 | (0.651) | −0.004 | [0.010] |
| Number of Household Member: 0–4 yrs | 0.336 | (0.568) | 0.334 | (0.554) | 0.003 | [0.005] |
| Number of Household Member at School Age | | | | | | |
| Elementary | 0.527 | (0.737) | 0.471 | (0.696) | −0.043 | [0.010] |
| Junior High | 0.218 | (0.457) | 0.201 | (0.440) | −0.013 | [0.004] |
| Senior High | 0.160 | (0.405) | 0.138 | (0.371) | −0.019 | [0.003] |
| University | 0.076 | (0.308) | 0.039 | (0.217) | −0.035 | [0.004] |
| Assets | | | | | | |
| Bicycle | 0.327 | (0.469) | 0.321 | (0.467) | −0.030 | [0.008] |
| Gas ≥ 3 kg | 0.148 | (0.355) | 0.043 | (0.203) | −0.091 | [0.010] |
| Refrigerator | 0.445 | (0.497) | 0.298 | (0.458) | −0.128 | [0.014] |
| Motorcycle | 0.671 | (0.470) | 0.627 | (0.484) | −0.041 | [0.015] |
| Water Access | | | | | | |
| Branded/Recycled Bottle Water | 0.290 | (0.454) | 0.163 | (0.369) | −0.106 | [0.011] |
| Pipe with Meter | 0.121 | (0.326) | 0.094 | (0.292) | −0.025 | [0.008] |
| Terrestrial well/pump | 0.119 | (0.323) | 0.143 | (0.350) | 0.012 | [0.006] |
| Protected/Covered well | 0.197 | (0.398) | 0.254 | (0.435) | 0.038 | [0.008] |
| Unprotected/Uncovered well | 0.274 | (0.446) | 0.346 | (0.476) | 0.082 | [0.011] |
| From buying from other parties | 0.444 | (0.497) | 0.312 | (0.463) | −0.115 | [0.009] |
| Housing | | | | | | |
| Own | 0.799 | (0.401) | 0.864 | (0.342) | 0.049 | [0.006] |
| rent | 0.036 | (0.186) | 0.015 | (0.123) | −0.017 | [0.002] |
| Lease | 0.046 | (0.210) | 0.018 | (0.131) | −0.021 | [0.003] |
| Company House | 0.024 | (0.152) | 0.006 | (0.078) | −0.015 | [0.003] |
| Others | 0.096 | (0.294) | 0.096 | (0.295) | 0.004 | [0.005] |
| | Non-Receiving Program | | Receiving Program | | Difference | |
| Lighting Sources | | | | | | |
| PLN Electricity 450 W | 0.784 | (0.412) | 0.759 | (0.427) | −0.030 | [0.013] |
| PLN Electricity without Meter | 0.104 | (0.306) | 0.120 | (0.325) | 0.008 | [0.007] |
| Non-PLN Electricity | 0.050 | (0.218) | 0.047 | (0.212) | 0.004 | [0.005] |
| Non-Electricity | 0.062 | (0.241) | 0.073 | (0.261) | 0.019 | [0.006] |
| Final disposal | | | | | | |
| Septic Tank | 0.624 | (0.484) | 0.554 | (0.497) | −0.067 | [0.014] |
| Pit hole | 0.118 | (0.322) | 0.147 | (0.354) | 0.026 | [0.008] |
| River/Lake/Sea | 0.176 | (0.380) | 0.195 | (0.396) | 0.018 | [0.007] |
| Beach/open field/farm | 0.064 | (0.245) | 0.078 | (0.269) | 0.019 | [0.006] |
| Defecation facility use | | | | | | |
| Personal | 0.715 | (0.451) | 0.657 | (0.475) | −0.053 | [0.016] |
| Mutual | 0.285 | (0.451) | 0.343 | (0.475) | 0.053 | [0.016] |
| House Characteristics | | | | | | |
| Wall material: Concrete | 0.624 | (0.484) | 0.597 | (0.491) | −0.057 | [0.017] |
| Wall material: Wood | 0.376 | (0.484) | 0.403 | (0.491) | 0.057 | [0.017] |
| Roof Materials: Concrete | 0.025 | (0.156) | 0.017 | (0.127) | −0.008 | [0.002] |
| Roof Materials: Roof Tile | 0.370 | (0.483) | 0.477 | (0.500) | 0.007 | [0.007] |
| Roof Materials: Iron Sheet/Asbeston | 0.546 | (0.498) | 0.445 | (0.497) | −0.009 | [0.008] |
| Roof Materials: Shingle/Fiber/Palm | 0.059 | (0.235) | 0.061 | (0.240) | 0.011 | [0.006] |
| Number of Households | 49,949 | 17,075 | 66,972 | | | |

This table present the mean tests of the characteristics of households who received the poverty program and did not. Number inside the parentheses is the standard deviation, while inside the square brackets denote the standard error.

**Table A2**
Village Characteristics by Receiving Poverty Program

| | Non-Receiving Program | | Receiving Program | | Difference | |
|---|---|---|---|---|---|---|
| *Village Characteristics* | | | | | | |
| Rural area | 0.650 | (0.477) | 0.819 | (0.385) | 0.142 | [0.011] |
| Distance to the nearest | | | | | | |
| Market (Km) | 6.968 | (16.655) | 7.587 | (17.647) | 1.259 | [0.583] |
| Health Facility (Km) | 5.098 | (11.853) | 6.168 | (13.250) | 1.318 | [0.311] |
| Sub-district office (Km) | 6.261 | (25.432) | 6.803 | (20.573) | 0.699 | [0.438] |
| District office (Km) | 29.843 | (53.452) | 34.582 | (47.342) | 6.132 | [1.708] |
| *Village has:* | | | | | | |
| Shophouse | 0.340 | (0.474) | 0.223 | (0.416) | −0.117 | [0.011] |
| Hotel | 0.152 | (0.359) | 0.074 | (0.262) | −0.065 | [0.007] |
| Cooperation | 0.525 | (0.499) | 0.472 | (0.499) | −0.062 | [0.009] |
| Credit Finance | 0.502 | (0.500) | 0.496 | (0.500) | −0.025 | [0.008] |
| Access to the Bank | 0.297 | (0.457) | 0.180 | (0.384) | −0.105 | [0.010] |
| School building | | | | | | |

(*continued on next column*)

**Table A2** (*continued*)

| | Non-Receiving Program | | Receiving Program | | Difference | |
|---|---|---|---|---|---|---|
| Elementary | 0.947 | (0.224) | 0.947 | (0.223) | −0.002 | [0.003] |
| Junior High School | 0.609 | (0.488) | 0.552 | (0.497) | −0.054 | [0.008] |
| Senior High School | 0.435 | (0.496) | 0.332 | (0.471) | −0.095 | [0.008] |
| Village Health Facility (*Polindes*) | 0.460 | (0.498) | 0.519 | (0.500) | 0.034 | [0.009] |
| Sub-Vil. Health Facility (*Posyandu*) | 0.978 | (0.147) | 0.977 | (0.151) | −0.003 | [0.003] |
| | Non-Receiving Program | | Receiving Program | | Difference | |
| Asphalt Road | 0.782 | (0.413) | 0.730 | (0.444) | −0.060 | [0.010] |
| Road can be accessed 4-wheel car | 0.929 | (0.257) | 0.913 | (0.282) | −0.026 | [0.008] |
| *Head of Village Characteristics* | | | | | | |
| Gender: Male | 0.910 | (0.286) | 0.933 | (0.250) | 0.019 | [0.004] |
| Age (years old) | 44.067 | (10.150) | 44.321 | (9.507) | 0.019 | [0.176] |
| Education background | | | | | | |
| No Education | 0.014 | (0.116) | 0.019 | (0.135) | 0.005 | [0.002] |
| Elementary | 0.017 | (0.128) | 0.018 | (0.133) | 0.002 | [0.002] |
| Junior High School | 0.101 | (0.302) | 0.137 | (0.344) | 0.032 | [0.007] |
| Senior High School | 0.456 | (0.498) | 0.512 | (0.500) | 0.051 | [0.010] |
| University | 0.040 | (0.196) | 0.044 | (0.204) | 0.001 | [0.003] |
| Number of Households | 49,949 | | 17,075 | | 66,972 | |

This table present the mean tests of the village characteristics where the households who received the poverty program and did not. Number inside the parentheses is the standard deviation, while inside the square brackets denote the standard error.

**Table A3**
Joint Probabilities of the Poor Households Receiving Poverty Programs Between Different of Targeting Regimes

| | Joint Probabilities | | | Difference | | |
|---|---|---|---|---|---|---|
| | 2006 | 2009 | 2014 | 2006 vs. 2009 | 2006 vs. 2014 | 2009 vs. 2014 |
| BLT only | 0.079 | 0.053 | 0.039 | −0.026 | −0.040 | −0.014 |
| | (0.270) | (0.224) | (0.194) | [0.002] | [0.003] | [0.003] |
| Raskin only | 0.183 | 0.232 | 0.193 | 0.049 | 0.010 | −0.040 |
| | (0.387) | (0.422) | (0.394) | [0.003] | [0.005] | [0.006] |
| Jamkesmas only | 0.009 | 0.017 | 0.044 | 0.008 | 0.035 | 0.027 |
| | (0.096) | (0.131) | (0.206) | [0.001] | [0.002] | [0.002] |
| BLT and Raskin only | 0.310 | 0.248 | 0.096 | −0.062 | −0.214 | −0.152 |
| | (0.462) | (0.432) | (0.295) | [0.003] | [0.006] | [0.006] |
| BLT and Jamkesmas only | 0.017 | 0.017 | 0.084 | −0.001 | 0.066 | 0.067 |
| | (0.130) | (0.128) | (0.277) | [0.001] | [0.002] | [0.002] |
| Raskin and Jamkesmas only | 0.029 | 0.033 | 0.092 | 0.004 | 0.063 | 0.059 |
| | (0.169) | (0.178) | (0.289) | [0.001] | [0.003] | [0.003] |
| BLT, Raskin and Jamkesmas | 0.157 | 0.125 | 0.273 | −0.032 | 0.117 | 0.148 |
| | (0.363) | (0.331) | (0.446) | [0.003] | [0.005] | [0.005] |
| None | 0.216 | 0.275 | 0.179 | 0.059 | −0.037 | −0.096 |
| | (0.412) | (0.447) | (0.383) | [0.003] | [0.005] | [0.006] |
| Number of HHDs | 40280 | 34,680 | 6511 | | | |

This table present the mean test of the Joint Probabilities as in Column (1), (5), and (6) of Table 3. The number inside of the parentheses is the standard deviation, while inside the square brackets denote the standard test.

**Table A4**
Joint Probabilities of the Poor Households Receiving Poverty Programs Between KPS and Non-KPS Holders

| | Joint Probabilities | | Difference |
|---|---|---|---|
| | Non KPS | KPS | |
| BLT only | 0.019 | 0.064 | 0.044 |
| | (0.138) | (0.244) | [0.005] |
| Raskin only | 0.349 | 0.006 | −0.342 |
| | (0.477) | (0.078) | [0.009] |
| Jamkesmas only | 0.071 | 0.013 | −0.058 |
| | (0.257) | (0.112) | [0.005] |
| BLT and Raskin only | 0.039 | 0.167 | 0.127 |
| | (0.195) | (0.373) | [0.007] |
| BLT and Jamkesmas only | 0.018 | 0.166 | 0.148 |
| | (0.132) | (0.373) | [0.007] |
| Raskin and Jamkesmas only | 0.158 | 0.014 | −0.144 |
| | (0.364) | (0.118) | [0.007] |
| BLT, Raskin and Jamkesmas | 0.038 | 0.566 | 0.529 |
| | (0.191) | (0.496) | [0.009] |
| None | 0.308 | 0.004 | −0.304 |
| | (0.462) | (0.067) | [0.009] |
| Number of HHDs | 3545 | 2906 | |

This table present the mean test of the Joint Probabilities as in Column (4) and (7) of Table 5. The number inside of the parentheses is the standard deviation, while inside the square brackets denote the standard test.

**Table A5**
Underlying Variables of PMT Score

| | dy/dx | (S.E) | | dy/dx | (S.E) |
|---|---|---|---|---|---|
| Head of HHD: Male | −0.099 | (0.014) | *Primary income source (reference = other)* | | |
| Married Status Head of HHD | 0.042 | (0.012) | Head of HHD working | −0.017 | (0.014) |
| h_hhsize | 0.058 | (0.004) | Agriculture | 0.111 | (0.016) |
| Age of Head of HHD | 0.000 | (0.001) | Mining and Quarrying | 0.092 | (0.034) |
| # HHD member 0–4 years | −0.021 | (0.009) | Processing Industry | 0.110 | (0.023) |
| Dependency Ratio | 0.008 | (0.007) | Trading | 0.060 | (0.015) |
| | | | Construction/building | 0.189 | (0.016) |
| *Household head education level (reference = No education)* | | | Hotel and Restaurant | 0.011 | (0.028) |
| Elementary | −0.073 | (0.010) | Transportation and warehousing | 0.115 | (0.016) |
| Junior High | −0.126 | (0.017) | Public service | 0.054 | (0.012) |
| High School - S3 | −0.291 | (0.022) | | | |
| | | | Self-Owned business | 0.014 | (0.009) |
| *Highest Education Background in the Household (reference: No education)* | | | Self-Owned business with non-permanent worker | −0.013 | (0.011) |
| Elementary | 0.056 | (0.011) | Self-Owned business with permanent worker | −0.124 | (0.020) |
| Junior High | 0.056 | (0.009) | | | |
| Senior High - S3 | −0.059 | (0.010) | *Home ownership Status (reference = Other)* | | |
| | | | Own | −0.019 | (0.014) |
| *Number of Household members who are studying at:* | | | rent | −0.207 | (0.038) |
| Elementary | 0.000 | (0.005) | Lease | −0.279 | (0.033) |
| Junior High | 0.002 | (0.007) | Company House | −0.332 | (0.073) |
| Senior High | −0.024 | (0.009) | | | |
| University | −0.063 | (0.014) | *Source of Ligthing (Reference = No Electricity)* | | |
| | | | Source of Lighting: | 0.059 | (0.040) |
| | | | PLN Electricity without Meter | 0.124 | (0.042) |
| | | | Non-PLN Electricity | 0.002 | (0.030) |

| | dy/dx | (S.E) | | dy/dx | (S.E) |
|---|---|---|---|---|---|
| *Household Assets:* | | | Final Disposal Location (Reference = Other) | | |
| Bicycle | 0.004 | (0.008) | Septic Tank | −0.060 | (0.026) |
| gas ≥ 12 kg | −0.262 | (0.019) | River/Lake/Sea | −0.015 | (0.028) |
| Refrigerator | −0.164 | (0.014) | Pit hole | −0.032 | (0.024) |
| Motorcycle | −0.092 | (0.011) | Beach/open field/farm | −0.038 | (0.026) |
| | | | River/Lake/Sea | −0.015 | (0.028) |
| *Source of Drinking Water (reference = Unprotected well)* | | | Pit hole | −0.032 | (0.024) |
| Branded/Recycled Bottle Water | −0.124 | (0.016) | Beach/open field/farm | −0.038 | (0.026) |
| Pipe with Meter | −0.065 | (0.024) | | | |
| Terrestrial well/pump | −0.027 | (0.018) | *Defecation facility use (Reference = mutual)* | | |
| Protected/Covered well | 0.005 | (0.014) | Personal | −0.083 | (0.013) |
| Buying | 0.007 | (0.011) | | | |
| | | | *Type of wall material (reference = wood)* | | |
| *Roof Materials (Reference = Shingle/Fiber/Palm)* | | | Type of wall material: Concrete | −0.099 | (0.010) |
| Concrete | −0.073 | (0.037) | | | |
| Roof Tile | −0.013 | (0.043) | Type of flooring material: Not Soil | −0.076 | (0.056) |
| Iron Sheet/Asbeston | −0.027 | (0.032) | | | |
| | | | | | |
| Pseudo R2 | 0.2953 | | | | |
| Households | 66,972 | | | | |

This table presents the marginal effect of Probit estimation. The dependent variable is 1 if the household receive any poverty programs, 0 otherwise. Standard errors in parentheses.

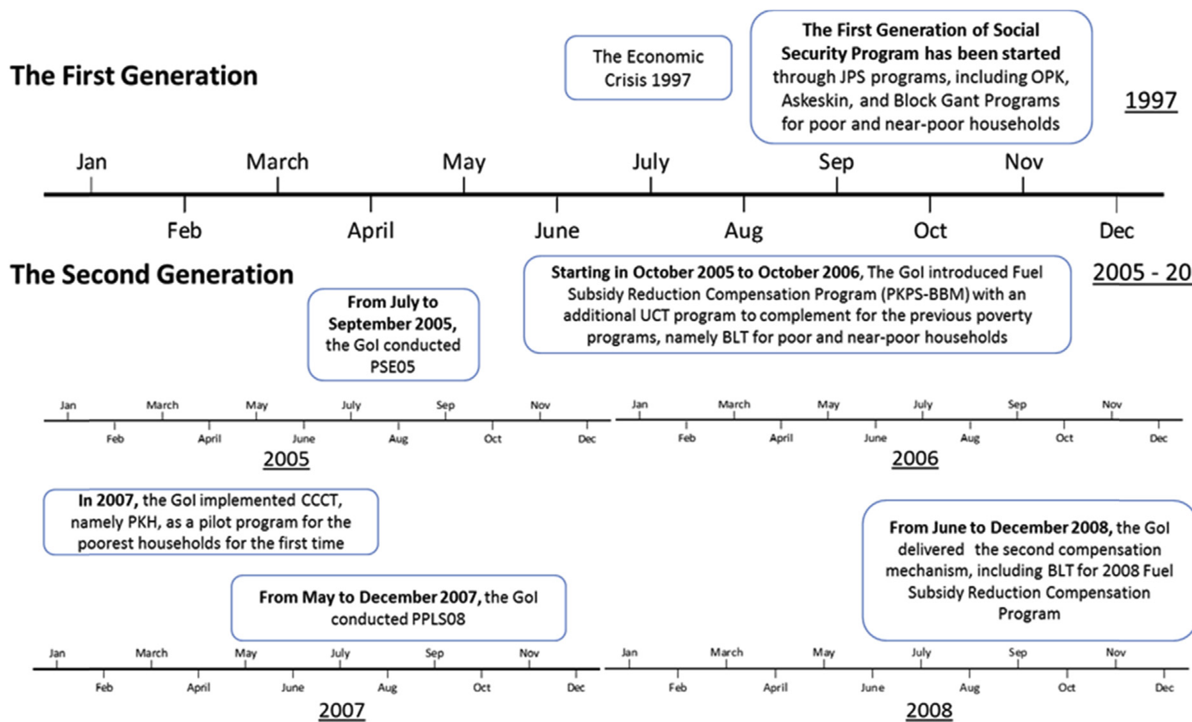


**Fig. A1.** The Evolution of the Social Protection Programs in Indonesia: The First and Second Generations

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