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Causal Effect of the State Welfare Card on Poverty in Thailand

*Weerawat Phattarasukkumjorn**
Faculty of Economics, Thammasat University, Bangkok, Thailand.
Petnaree Suwanmanee
Graduate Study Program, Faculty of Economics, Thammasat University, Bangkok, Thailand.

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Abstract

This study aims to explore whether there is evidence supporting that the State Welfare Card causes the reduction of the poverty rate in Thailand. With data limitations, we rely on the 2019 household socio-economic survey to compare the consumption expenditure of those who received the card (treatment) and those who did not (control) using matching methods with propensity score and coarsened exact matching. The results show that the consumption expenditure of the treatment group is less than that of the control group, which aligns with previous research. We then perform matching among the poor and the non-poor separately and find that among the poor, differences in consumption expenditure between the treatment and control are tiny and mostly not statistically significant. We conclude that there is no sufficient evidence to support the claim that this program causally reduces the number of the poor in Thailand, probably due to the size of the provided benefits that is too small.

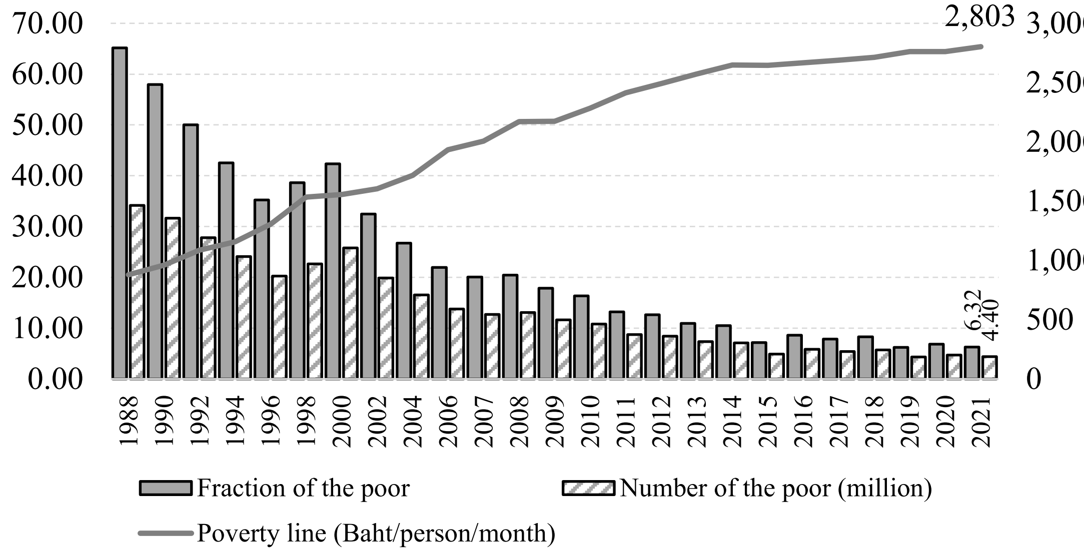
Keywords: State Welfare Card, Poverty, Poverty Targeting, Government Policy
JEL Classifications: I32, I38

* **Corresponding author**: Email: weerawat@econ.tu.ac.th

1. Introduction

For the past few decades, the number of Thai people under the poverty line has reduced substantially (see Figure 1 below). Official economic reports, especially those released from the Office of the National Economic and Social Development Council (NESDC) and Fiscal Policy Office (FPO) underline the importance and efficacy of poverty targeting, possibly because the rate of poverty reduction has started to slow down. “State Welfare Card” (or in Thai, “Bhat Sawaddikan Hang Rat”) can be said to be the first large-scale poverty-targeting welfare policy that began back in April 2017. Corresponding to the development of the national e-payment program in 2016, with cooperation between the Bank of Thailand and the Ministry of Finance, beneficiaries receive electronic money to spend on consumption goods, transportation, and utilities, which is directly transferred from the government into the card. The FPO, which is an initiating organization, expected this program to further reduce the number of the poor efficiently and mitigate income inequality. (Tulyasatien et al.,2017)

Figure 1: Poverty Statistics in Thailand.



Source: NESDC (2023)

After 3 years of implementation, a vice spokesperson, Traisulee Traisaranakul, reported that from 2018 to 2019, the number of the poor reduced significantly “due to economic expansion and poverty targeting policies such as the State Welfare Card” (Bangkokbiznews, 2020). Without citing any study or academic finding, this bold claim ignores confoundedness and may overestimate the effectiveness of the program even though the number of the poor was lower since 2017. Therefore, our question is very simple and straightforward. We want to figure out if the State Welfare Card causally reduces the number of the poor or not. However, with data limitations, which will be discussed in section 3, this question cannot be concluded directly, so we would like to explore whether there is any evidence supporting the claim or not.

Probably due to data availability or lack thereof, so far there is only one study that attempted to tackle the same question, which is Durongkaveroj’s (2022). As one of the eligibility criteria of having an annual earned income less than 100,000 Baht (approximately 2,812 USD today) in 2016, he employs a sharp regression discontinuity design with earned income as a running variable. The study concludes that there is no significant difference in monthly total expenditure, consumption expenditure, or tobacco and alcoholic beverages around the cutoff. The only significant difference is found in monthly food expenditure, but surprisingly and contrary to the hypothesis, observations in the treatment group spend less than those in the control group. In other words, card owners spend less than those who are not.

In this study, we use matching methods which imitate random assignment to compare the consumption expenditure of the treatment and control groups using propensity score matching (PSM) and coarsened exact matching (CEM). We find similar results to Durongkaveroj (2022). In other words, those who have the card spend significantly less on consumption expenditure on average than those who do not with every matching method that we employ.

We then investigate further by matching within each subgroup, the poor and the non-poor, and find that the significant results above are driven by the non-poor group. Among the poor, differences in consumption expenditure are very small and mostly not statistically significant. The subgroup analysis reveals that the effect of having the card on the consumption expenditure of the poor is negligible. From our results, we do not find supporting evidence that the program causally reduces the number of the poor.

We believe that our study provides several contributions. First, we add this causal study into the pool of literature that is very limited. Second, our data cleaning and model selection process sheds some light on inclusion and exclusion error as a by-product, which contributes to policy recommendations later. Third, our findings are similar to the previous study, though employed methods are different, which can be a confirmation and suggestion that this program needs a rework.

This article proceeds as follows. In the next section, we start by discussing the history and details of this program, including the screening mechanism and benefits received. Then, in Section 3, we discuss data limitations that shape our question and model selection, which is also discussed in the Appendix. For Section 4, we detail matching methods that we employ to uncover average treatment on the treated. Section 5 deals with the data cleaning process, descriptive statistics, and covariate selection. Then, the results are shown in Section 6 with a balancing test after matching. Lastly, Section 7 concludes our findings and discusses policy recommendations.

2. A Brief History of the State Welfare Card

Tracing back the idea of poverty targeting in Thailand from the last decade, the first evidence is found from Ananapibut et al. (2013), who studied the possibility of a negative income tax policy if it is to be implemented in Thailand. The report was more or less impactful since it was released by the FPO, a thinktank organization under the Ministry of Finance.

Three years afterwards, a trial cash transfer program was initiated in 2016. The Ministry of Finance proposed to the cabinet, and the self-report registration was opened to Thai people who were more than 18 years old and earned less than 100,000 Baht (2,812 USD) in 2015. 8.4 million people signed up for the program, but the number was trimmed down to 7.5 million who passed the set criteria. A one-time cash transfer was handed over to the beneficiaries, 3,000 Baht (85 USD) for those who had earned less than 30,000 Baht (850 USD) and 1,500 Baht (42.5 USD) for those who had earned more than that, in January of 2017. 30,000 Baht is approximately close to the poverty line, and 100,000 Baht is slightly above the daily minimum wage, both annually totalled.

Then, three months later, in April to May, the State Welfare Card program was officially launched with another self-application. Targeted eligible beneficiaries must meet these criteria.

- Be a Thai citizen aged 18 or more.
- Earn less than or equal to 100,000 Baht (2,812 USD) in 2016.
- Possess less than or equal to 100,000 Baht (2,812 USD) worth of any financial asset.
- Own a house that is smaller than 100 square meters or an apartment smaller than 35 square meters. If one owns a piece of land for both accommodating and agricultural purposes, it must be smaller than 400 square meters (a "rai" in Thai unit of land measurement), but if it is only for agricultural purposes, it must be smaller than 4,000 square meters (10 rais).

There were 14.2 million submitted applications, but after screening, 11.4 million was the number of total beneficiaries, and they received the card in October 2017. The first batch of beneficiaries received all the benefits for five years onward without being monitored and having to update their income and other status. The regular benefits were (1) 300 or 200 (8.44 or 5.62 USD) Baht of value per month to spend on consumption goods in shops registered with the Ministry of Commerce ("Thong Fah" stores). This amount of money cannot be withdrawn and used as cash. Note that those who claim their income is less than 30,000 Baht (844 USD) get 300 Baht while those exceeding the level of income receive 200 Baht. (2) Discount on cooking gas for 45 Baht (1.27 USD) per 3 months. (3) Public transportation (sky train, train, buses) fee for 500 Baht (14 USD) per month. During the first wave of the State Welfare Card, there are some accompanying policies such as a job training program, monetary support for the elders, a few cash transfers that can be cashed out during the new year season, and further support for pipe water and electricity bills (for further detail, see Pitidol & Phattarasukkumjorn, 2019). The rationale behind these subsidiary policies is that targeting the poor is more efficient compared to universal transfer. The poor are believed, at least theoretically, to have a higher marginal propensity to consume compared to the rest of society. Thus, these policies were expected to also act as a kind of economic stimulus.

Though there had been news about changing the criteria to improve targeting due to criticism of inclusion and exclusion error, the second registration was delayed to July 2022 due to COVID-19 that hit Thailand since March 2020. This time, apart from the original criteria, there are additional criteria to become eligible listed below.

- Do not have a credit card.
- Average household income is less than or equal to 100,000 Baht (2,812 USD).
- Do not have any loan or less than a million Baht (28,345 USD) for a car or less than 1.5 million Baht (42,517 USD) for a house mortgage.
- Priest of any religion, prisoner, civil servant, retired civil servant, politician, and people who live in governmental foster home cannot apply.

The number of applicants skyrocketed to 22.29 million this time, and the final number of beneficiaries is 15.04 million. Benefits were added on top of the first wave as well, which are; (1) a monthly subsidy for electricity and pipe water bills of 315 Baht (8.94 USD) and 100 Baht (2.84 USD), respectively, per household ; (2) the cooking gas discount is increased to 100 Baht (2.84 USD); (3) the disabled get 200 Baht (5.67 USD) and the elders get 50-100 Baht (1.42-2.84 USD) monthly top-up; and (4) small business owner can get an additional 100 Baht (1.42-2.84 USD) discount on cooking gas. Table 1 below summarizes key aspects of the program.

Table 1: Summary of State Welfare Card program

Wave / year implemented	Applicants/ beneficiaries (millions)	Screening criteria and regular benefits
1 / 2017	14.2 / 11.4	**Criteria** - Thai, aged more than 18. - As of 2016, earn and possess less than or equal to 2,812 USD of any financial asset. - Property ownership criterion. **Regular benefits** - Consumption expenditure: 8.44 or 5.62 USD per month. - Discount on cooking gas, 1.27 USD per 3 months. - Public transportation, 14 USD per month.
2 / 2022	22.29 / 15.04	**Criteria** - Same as wave 1 and, - Do not have a credit card. - Average household income is less than or equal 2,812 USD. - Do not have any loan or less than 28,345 USD for a car, less than 42,517 USD for a house mortgage. - Priest of any religion, prisoner, civil servant, retired civil servant, politician, people who live in governmental foster homes **Regular benefits** - Same as wave 1 and, - Monthly 8.94 USD subsidy for electricity and pipe water. - 5.67 USD monthly top-up for the disabled and 1.42-2.84 USD for the elders. - Discount for cooking gas now becomes 2.84 USD per 3 months and an additional 1.42-2.84 USD top-up for small businesses.

Source: Authors' compilation.

3. Data Limitations

To answer our question, there are some complications regarding data availability. First, a complete dataset of beneficiaries is not publicly available, so we must turn to a national survey instead. Fortunately, there is a question asked in a household Socio-Economic Survey (SES) whether the respondent "receives any support from the government," and an option of receiving benefits from the State Welfare Card was added since 2018.

Second, though this dataset is surveyed every year on the expenditure side, the income side is surveyed every other two years, and we need income data to determine those who are eligible and use it as a control variable.

Third, the 2017 dataset, when the program initiated, does not contain the option of receiving the card, and if there is, we cannot use the dataset since the card was distributed in October 2017.

In conclusion, the earliest dataset that we can fully utilize is 2019. Furthermore, we are concerned that expenditure in the 2021 dataset may be abnormal from the spread of COVID-19, which leaves us with the only option of using only the 2019 dataset, a year and three months after the card was distributed during the first wave of the program.

Hence, the question of interest that the reduction in the number of the poor from 2018 to 2019 was caused by the State Welfare Card cannot be addressed directly using only a year of cross-sectional data. Still, we attempt to find whether there is a difference in consumption expenditure between those who have the card and those who do not. Especially when it comes to the poor, if the beneficiaries spend more on consumption expenditure than those who are not, the claim can be partially justified.

4. Research Design

This program does not randomly assign people into treatment and control groups. The application was conducted from self-reported information and screened after the officials received the report. The coefficient estimated from linear regression will be biased due to self-selection bias, and we must exploit other methods instead.

In short, we end up using matching with propensity score (PSM) and coarsened exact matching (CEM). The discussion of why we disregard other models can be found in the Appendix. Matching simulates random assignment of treatment and control groups. The first step is to generate a propensity score of having the State Welfare Card as follows.

$$P(X) = P(D_i = 1|X) = E(D_i|X) \tag{1}$$

$P(X)$ is the predicted value generated by either a probit or logit model. It is a function of a set of control variables X, and D_i is either 0 or 1, indicating treatment status. A key assumption of this method is called the Conditional Independence Assumption (CIA). In other words, being in either the treatment or control group must be "independent conditional on the propensity score." (Caliendo & Kopeinig, 2008) Unfortunately, CIA cannot be proved mathematically since it involves omitted variables.

Apart from the main assumption, there are two more conditions that should be fulfilled for the model to perform effectively. The first one is that the selected covariates should not be significantly different between the treatment and control groups after matching. In other words, two groups should be balanced. According to Rosenbaum & Rubin (1983), similar characteristics between treatment and control groups can also partially justify the CIA.

The second condition is called common support: there should be sufficient observations that can be matched between two groups. There are several algorithms to complete the matching, which are nearest neighbor, radius, stratification, weighting, and kernel matching. For most of the methods, a caliper can be manually chosen to increase successful matching but at the same time introduce more bias.

Nevertheless, matching with propensity score was criticized by Iacus et al. (2012) for tending to overfit. For instance, two observations from the treatment and control groups may have totally different characteristics, but predicted scores are accidentally neighboring to each other. Comparing these two observations may not be optimal or sensible. They recommend exact matching instead, but common support may become an issue since exact matching can be very difficult, especially exact matching of continuous variables. Therefore, the middle-ground method they suggest is coarsened exact matching. Basically, matching remains exact for categorical variables that can be easily matched, such as broad location of residence, but for continuous variables, they can be "coarsened" or allow matching within a specific range. We will follow their guideline and apply this method as a comparison to propensity score matching.

As for the results, we expect to uncover the Average Treatment Effect on the Treated (ATT). Within the context of this study, let Y_i be monthly consumption goods expenditure; there are two estimating equations as follows.

$$D_i = \pi_0 + \pi_i Z_i + V_i \tag{2}$$

$$Y_i = \beta_0 + \beta_i D_i + \beta_Z Z_i + U_i \tag{3}$$

Treatment status D_i is given by Z_i, an indicator of whether one is eligible or not in equation (2). The coefficient of interest is β_i or how much different monthly consumption goods expenditure is between the treatment and control, which is natural to assume that the effect is heterogenous across individuals. To figure out what β_i is, the Wald estimand is a starting point of an analysis, defined as

$$\frac{\mathbb{E}(Y_i|Z_i = 1) - \mathbb{E}(Y_i|Z_i = 0)}{\mathbb{E}(D_i|Z_i = 1) - \mathbb{E}(D_i|Z_i = 0)} \tag{4}$$

Typically, the Wald estimand is $\mathbb{E}(\beta_i)$ or the *Average Treatment Effect* (ATE) but for this study, it is sensible to further assume (1) Exclusion restriction ($\beta_Z = 0$): being eligible or not has no direct effect on monthly consumption expenditure and (2) No "cross-overs" ($Z_i = 0$ and $D_i = 1$): there is no observation who is not eligible but receives the card. The latter holds automatically because we clean our dataset to cover only those who are eligible and separate the treatment and control by using the question indicating whether an observation has a card or not. Selecting eligible observations will be discussed in the next section. Therefore, fulfilling these two assumptions, the Wald estimand identifies the ATT that we expect to uncover as follows.

$$\frac{\mathbb{E}(Y_i|Z_i = 1) - \mathbb{E}(Y_i|Z_i = 0)}{\mathbb{E}(D_i|Z_i = 1) - \mathbb{E}(D_i|Z_i = 0)} = \mathbb{E}(\beta_i|D_i = 1) \tag{5}$$

5. Data Cleaning and Descriptive Statistics

As mentioned earlier in Section 3, we rely solely on a national cross-sectional 2019 Household Socio-economic Survey. Though the unit of observation in SES is household, there is a possibility to extract individual-level data from record 2. We prepare the dataset for our analyses following these steps.

(1) Instead of using average household income to assign earned income for each member equally, we extract individual earned income from record 13, wage earner; record 14, business owner; and record 15, agricultural worker, then merge them back into record 2 corresponding to each member within a household. Divide annual income by 12 to convert to monthly income.

(2) We then attempt to filter the eligible from the first wave from the survey using proxy variables: average financial asset data is taken from record 17 and shared within a household for members who aged over 18, and housing and land ownership information is taken from record 3. We use the number of bedrooms (not more than 3) as a proxy for the size of the house. Together with individual income in (1), we can distinguish those who are eligible and exclude the rest from our following analyses.

(3) The outcome variable of interest is consumption expenditure, which is officially used by the Office of the National Economic and Social Development Council (NESDC) to determine the poor. We then assign average household consumption expenditure for each observation. There is an issue regarding interpretation with this variable, though: when respondents are asked to report their consumption expenditure, it is unknown whether they include or exclude the value they get from the State Welfare Card. We will address this problem when we report our results.

Table 2 below displays the profile of our sample. The total number of observations is 39,022. Though the number of observations is dropped to roughly 36,000 in the following analyses due to some missing values, we expect that common support would not be an issue.

Table 2: Descriptive Statistics

Variables		**N**	**%**
Card owner	No	22,236	56.98
	Yes	16,786	43.02
Region	Bangkok	664	1.7
	Center	8,427	21.6
	North	11,335	29.05
	Northeast	12,545	32.15
	South	6,051	15.51
Area	In a municipal area	18,768	48.1
	Not in a municipal area	20,254	51.9
Sex	Male	17,501	44.85
	Female	21,521	55.15
Disability	No	36,684	94.01
	Yes	2,338	5.99
Work status	Employer	295	0.76
	Own-account worker	10,946	28.05
	Contributing family worker	4,289	10.99
	Government employee	1,025	2.63
	State enterprise employee	44	0.11
	Private company employee	9,447	24.21
	Member of producers’ cooperative	7	0.02
	Housewife	2,782	7.13
	Students	839	2.15
	Children, elderly person	7,158	18.34
	Ill, disabled person	1,471	3.77
	Looking for a job	131	0.34
	Not looking for work	194	0.5
	Others	394	1.01

Variables	Min	Mean	Median	Max	S.D.
Age	18	53.01	55	97	17.19
Number of household members	1	3.01	3	12	1.53
Years of education	3	6.18	6	22	3.77
Individual income	-157,643	3,175	3,000	8,333	3,294
Average household income	-51,037	5,314	4,477	142,500	3,785
Average household expenditure	500	4,629	3,960	55,375	2,759

Note: Business owners can report negative income. Income and expenditure are in per-month term.

Source: Authors’ calculation.

Next, the propensity score is predicted for each observation. This step can be arguably controversial since covariate selection differs across projects. As mentioned in the introduction, literature on this program is very limited, so we cannot follow previous research as a guideline. Therefore, we discuss briefly how other projects chose their covariates and attempt to justify our selection.

For instance, Sanguanwongse (2022) employed a logistic regression to study the correlation between borrowing from the National Village and Urban Community Fund and individual and household economic characteristics. He included sex, marital status, education level, number of household members, dependent members, income, expenditure, and number of cars owned by a household. Piyakarn & Sokatiyanurak (2020) evaluated the financial stability of households borrowing from the same fund above using the difference-in-differences of the propensity score. They control for an urban dummy, number of household members, earned members and dependent members, number of cars owned by a household, and characteristics of the head of household, which are occupation, sex, age, marital status, and education level. Sajjanand et al. (2018) studied the difference between landowners and tenants in terms of yield. They control for age, sex, dummy for debt, number of household members, agricultural product price, access to irrigation, number of reaping rounds in a year, and province dummies.

Examples above show that the authors attempt to include key covariates that are related to self-selection into an intervention, in line with the suggestion made by Harris & Horst (2019). Below are the sets of covariates we decided to include in our model to predict propensity score.

(1) Location of residence, which includes regions and area, of residence (urban and rural). These variables should represent how difficult (or easy) it is to access registration organization. Also, there can be differences across regions and areas in how much information of the program was disseminated or how strong people networks are.

(2) Age and disability should be determining factors since the benefits of the program might outweigh the cost of self-selection with older people and the disabled. Moreover, the Ministry of Finance organized an outreach once in 2018 for the disabled and bedridden patients who could not register themselves on site. It is more likely that these groups are treated. Age squared is also included to capture the curvature of less likelihood for very old-aged persons.

(3) Average household income, years of education, and employment status should represent current household income and the prospect of future income. We suspect that higher income and years of education represent a higher cost of registration since their current and prospective income can be higher, while different employment status can reflect either a benefit or cost of registration. This set of variables is expected to be the key covariates.

We left out sex and marital status out of the estimation since we do not believe that observations across these groups have the same tendency to join the program. The number of household members is also left out since it is already correlated with average household income.

Then, we put our data into both probit and logit models, but the probit model, displayed in Table 3, fits the data better in terms of pseudo R2 and classification performance. Balancing the test before matching results in 15.2% of mean bias (with a P-value of 0.000), which is quite large. However, the treatment and control groups become much more balanced after matching, and the comparison can be found in the following section.

Table 3: Covariates Selection and Probit Model Result

Variables	**Probit model**	
	Coefficient	**Robust s.e.**
Region (base = Bangkok)		
Center	0.323	0.068
North	0.841	0.068
Northeast	0.862	0.068
South	0.474	0.069
Area (base = municipal area)		
Not in a municipal area	0.084	0.014
Age	0.048	0.003
Age (square)[1]	-0.040	0.003
Disability (base = no)		
Yes	0.151	0.034
Average household income[1]	-0.005	0.000
Years of education	-0.031	0.003
Work status (base = employer)		
Own-account worker	0.070	0.078
Contributing family worker	0.034	0.080
Government employee	-0.083	0.089
State enterprise employee	-0.454	0.241
Private company employee	0.111	0.079
Member of producers' cooperative	-0.456	0.516
Housewife	0.031	0.081
Students	-0.430	0.102
Children, elderly person	-0.040	0.080
Ill, disabled person	0.061	0.088
Looking for a job	-0.317	0.154
Not looking for work	-0.304	0.135
Others	-1.104	0.150
Pseudo R2	0.086	
P > chi2	0.000	
Classification	63.68%	

Note: [1] coefficients and standard error are scaled up for two digits since the unit is too small.
Source: Authors' calculation.

6. Results

We then use propensity score to match observations from the treatment and control groups to compare their consumption expenditure. We employ nearest-neighbor with and without caliper, radius matching, and kernel matching for robustness, leaving stratification matching out to avoid strata selection since it can be subjective. According to Caliendo & Kopeinig (2008), a drawback of nearest-neighbor and kernel matching is bad matches, but kernel matching uses weighted averages of the control group to construct counterfactual outcomes. Radius matching uses all comparisons within the selected caliper to gain oversampling and avoid the risk of bad matches. Hence, we attempt matching by these algorithms with various levels of caliper.

A full table of results can be found in Table A2 in the Appendix. According to Harris and Horst (2019), there is no gold standard to select caliper size since the distribution of propensity scores is different for each prediction. Previous studies rely on a proportion of standard deviation, i.e., 10 or 20 percent, so we use even lower 1, 5 ,and 10 percent of standard deviation (0.16 in our study) of the propensity score to keep the bias as low as possible. Furthermore, all the matching is non-replacement to prevent oversampling.

We only show the main results in Table 4 of radius matching with a caliper width of 1 percent of standard deviation (0.0016), which is the matching algorithm with the lowest mean bias and the most balanced (highest P-value) after matching, as shown in Table 5. Unmatched and coarsened exact matching results are shown as a comparison.

Table 4: Matching Results and Balancing Test After Matching

Matching methods	ATT	s.e.	Off support		Balancing test	
			Treatment	Control	Mean bias	P > chi2
(1) Unmatched	-1,022.6$^{(1)}$	29.4	-	-	15.2	0.000
(2) Radius						
Caliper - 1% of S.D.	-234.3$^{(1)}$	29.4	8	-	0.5	0.988
(3) Coarsened	-229.9$^{(1)}$	18	11	631	-	-
N			20.574	15,317		

Note: $^{(1)}$ *refers to statistically different from zero at 0.01 level of significance.*
Source: Authors' calculation.

From row (2), those who have the card spend on consumption goods significantly less than those who do not have the card for 234.3 THB per month (6.68 USD), while coarsened exact matching gives a comparable result of 229.9 THB per month (6.55 USD) in the same direction. We use the same set of covariates for coarsened exact matching, which is quite satisfactory since expenditure is highly correlated with income, resulting in an adjusted R-square of 0.462. Coarsened variables and methods were selected automatically by a default algorithm. Though 11 observations and 631 observations from the treatment and the control group, respectively, cannot be matched, common support is still neglectable compared to the sample size. As a reference, Durongkaveroj (2022) found only a significant difference in food expenditure: those who have the card spend less on food compared to those who do not, but only 90 THB (2.56 USD) per month.

Table 5: Balancing Test, Before and After Radius Matching

Variables	Before matching		After matching		Bias reduction (%)
	Bias (%)	P > |t|	Bias (%)	P > |t|	
Region (base = Bangkok)					
Center	-29.0	0.000	0.6	0.550	97.9
North	19.4	0.000	-0.3	0.736	98.0
Northeast	22.9	0.000	0.5	0.654	97.6
South	-16.3	0.000	-1.0	0.302	93.4
Area (base = municipal area)					
Not in a municipal area	14.6	0.000	0.9	0.382	93.1
Age	29.0	0.000	-0.6	0.546	97.9
Age (square)[1]	23.4	0.000	-0.5	0.592	97.6
Disability (base = no)					
Yes	11.0	0.000	1.3	0.186	86.4
Average income[1]	-34.1	0.000	-0.0	0.983	99.9
Years of education	-40.4	0.000	-0.2	0.825	99.4
Work status (base = employer)					
Own-account worker	11.2	0.000	0.8	0.432	91.7
Contributing family worker	11.0	0.000	-0.0	0.993	99.9
Government employee	-8.2	0.000	-0.7	0.495	91.4
State enterprise employee	-3.5	0.001	0.2	0.871	96.4
Private company employee	-12.2	0.000	-0.1	0.952	99.4
Member of producers' cooperative	-0.0	0.993	0.1	0.933	82.0
Housewife	-1.1	0.271	0.2	0.823	77.5
Students	-20.9	0.000	1.3	0.212	96.5
Children, elderly person	5.8	0.000	-1.2	0.234	76.8

Variables	Before matching		After matching		Bias reduction (%)
	Bias (%)	P > |t|	Bias (%)	P > |t|	
Ill, disabled person	7.2	0.000	0.3	0.801	96.0
Looking for a job	-5.1	0.000	0.1	0.892	97.8
Not looking for work	-6.0	0.000	-0.2	0.830	96.9
Others	-17.2	0.000	0.2	0.818	99.5
P > chi2	0.000		0.988		-
Mean bias	15.2		0.5		-

Source: Authors' calculation.

So far, the results do not directly suggest that the card can or cannot boost consumption expenditure for the poor to remove themselves from poverty. Therefore, we match within each subgroup of the non-poor and the poor using an official number of 2,763 THB (78.77 USD) monthly poverty line as a separator using the same set of covariates.

Table 6: Subgroup Matching Results and Balancing Test After Matching

Matching methods	ATT	s.e.	Off support		Balancing test	
			Treatment	Control	Mean bias	P > chi2
Panel A: Non-poor						
(1) Unmatched	-1,018.8⁽¹⁾	33	-	-	15.2	0.000
(2) Radius Caliper - 1% of S.D.	-235.3⁽¹⁾	32.4	7	-	0.5	1.000
(3) Coarsened	-235.8⁽¹⁾	21.2	13	632	-	-
N			17,393	11,913		
Panel B: Poor						
(1) Unmatched	-2.4	9.4	-	-	15.2	0.000
(2) Radius Caliper - 1% of S.D.	-4.8	10.1	5	-	0.9	0.972
(3) Coarsened	-19.4⁽²⁾	8.8	140	67	-	-
N			3,177	3,397		

Note: ⁽¹⁾ and ⁽²⁾ refer to statistically different from zero at 0.01 and 0.05 level of significance respectively.
Source: Authors' calculation.

Panel A and B in Table 6 apparently show that the results in Table 4 are mainly driven by the difference in consumption expenditure of the non-poor. The ATTs of the poor are still consistently negative, suggesting that those who have the card spend less on consumption goods than those who do not, but the results using propensity scores are very small and not significant. The only statistically significant result can be found when matched with coarsened exact matching, but the effect is also tiny (19.4 THB or approximately 0.55 USD per month). Lastly, the balancing tests are still solid for each subgroup after matching.

As for the concern of how consumption expenditure is reported mentioned earlier in section 5, we do not know exactly whether respondents report this number including spending from the card or not. Let's bluntly consider two extreme cases for all samples as follows.

First, if the reported expenditure includes spending from the card, beneficiaries' out-of-pocket consumption expenditure is lower than the reported value. It implies that provided benefits are not strong enough to differentiate consumption expenditure between the treatment and control groups. This is even more obvious when only considering the poor in panel B.

Second, if the reported expenditure does not include spending from the card, beneficiaries' out-of-pocket consumption expenditure is as reported. In other words, total beneficiaries' consumption expenditure is higher than the reported value, which is beneficial for them in real life. However, this case is irrelevant to the reduction in poverty since it is measured by reported consumption expenditure from the survey.

Though our results cannot fully conclude whether this program causes the reduction of the poor since our samples are not matched over time, we do not find any concrete evidence to claim that it does, regardless of how consumption expenditure is reported.

7. Further discussion and conclusion

Our results imply that the size of provided benefits may not be large enough to statistically significantly boost consumption expenditure of the poor beneficiaries. The reduction in the number of the poor might be driven by other confounders. For instance, we notice that the number of the poor can be very sensitive to inflation or deflation since the poverty line is converted into monetary value based on the consumer price index. Consider Figure 1 once again; the number of the poor also dropped significantly in 2015 when the economy deflated by -0.9 percent year-on-year, lowering the poverty line. If we assume that the poor whose consumption expenditure was near the poverty line spend equally to the previous year or just a little more, they can become the non-poor without changing their core behavior.

In terms of policy recommendation, the size of provided benefits can be increased if targeting is more accurate without expanding fiscal constraints. When the program had been launched for a few months, there were a lot of news covers about complaints from those who were classified as non-eligible to reconsider their application. Meanwhile, some of the eligible displayed themselves online and boasted that though they are not poor, they passed the screening process and became one of the card owners.

The Ministry of Finance quickly responded to the complaints but addressed the excluded side more seriously. A country-wide campaign was launched in 2017-2018 to have those who were immobile and could not register themselves, including the elders, the disabled, and the bedridden, be included if they were eligible, which ended up having 3.1 million more beneficiaries. Meanwhile, the inclusion problem was only mentioned that if it is detected, those who were screened incorrectly will be removed from the program immediately, but no active measure was taken.

Though inclusion and exclusion errors are widely discussed, it seems that they have not been addressed earnestly throughout the first wave of the program. According to our discussion on other research designs in the Appendix, we also agree that inclusion and exclusion error are a source of inefficient targeting. Table 7 below shows the estimated rate of inclusion and exclusion error taken from two sources. While there are discrepancies due to different years of datasets used, 2017 for the former and 2018 for the latter, they are still somewhat inline. To be specific, 6.05 or 10.96 percent of those who have the card are not eligible, which is not as severe compared to the exclusion error; 79.4 or 57 percent of those who do not have the card are eligible.

Table 7: Estimated Proportion of Inclusion and Exclusion Errors

		Eligible	
		Yes	No
Card owner	Yes	20.6 / 43	**6.05 / 10.96**
	No	**79.4 / 57**	93.95 / 89.04

Source: Numbers on the left are taken from Durongkaveroj (2022) numbers on the right are from authors' calculation.

NESDC (2023) also reports similar issues but in a different fashion in Table 8 below. Though the program seems to have been reaching more to the poor over time (column iii), the non-poor have been prevalent among the card owners (column ii). These estimates suggest this program's targeting is far from being efficient and has room for improvement.

Table 8: Inclusivity and Exclusivity of The Poor

Year	**Proportion of the poor and non-poor to card owner**		**Proportion of card owner to the poor and non-poor**	
	(i) Poor	**(ii) Non-poor**	**(iii) Poor**	**(iv) Non-poor**
2018	14.85	85.15	35.16	16.46
2019	11.08	88.92	46.17	21.97
2020	12.61	87.39	51.24	22.93
2021	10.45	89.55	49.72	24.94
2022	10.45	89.55	51.51	22.26

Source: NESDC (2023)

Although the application process was rebooted in July of 2022, we recommend that there should be systematic tracking and monitoring on both those who have the card and those who do not at least annually to mitigate targeting inefficiency. There are several recommendations that can be found, such as incorporating a tracking and monitoring process with an annual individual tax report as suggested by Ananapibut et al. (2013) when they proposed negative income tax. The Ministry of Finance can also study lessons learned in other developing countries, such as Listahanan in the Philippines. This program employs a proxy-means test, and targeting has been seriously studied and developed over the course of implementation. (Velarde, 2018)

Lastly, this study is flawed due to the lack of a time dimension to uncover the causal change in the number of the poor. Without a panel dataset, future research can better answer the question of interest when more appropriate datasets are released, i.e., employing a method such as difference-in-differences with propensity score matching to study the effect over time.

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Appendix

(i) Discussion on other research designs

According to data limitations discussed in Section 3, they frame model selection to uncover causal effects using only a year of cross-sectional data. The difference-in-differences method is disregarded immediately since it is clearly impossible to conduct results with only a year of observation.

We also prepared our dataset for fuzzy regression discontinuity instead of sharp regression discontinuity since we believe that inclusion and exclusion error, or crossing-overs in econometric terms, can be present for this program. Basically, regression discontinuity only considers the difference between an outcome variable of the treatment and control group at the cutoff, using a continuous variable as a running variable. Under the context of this program, the running variable is income since those who earned more than 2,812 USD are considered ineligible. A key assumption of this model for an unbiased estimation is continuity or continuous unobservability at the cutoff. In plain terms, the distribution at the cutoff should not jump dramatically, which is statistically testable by manipulation tests.

We do not discuss the technicality of this model here due to several reasons. First, we cannot link respondents' income between 2016 (the application year) and 2019 since they are cross-sectional. Observations before and after the cutoff cannot be divided as treatment and control groups straightforwardly. Second, we replicate Durongkaveroj (2022) using 2019 data, but our manipulation test results shown in **Table A1** fail most of the time. Third, we also argue that regression discontinuity may not be suitable to evaluate this policy with a single running variable since there are other criteria in place. Thus, those who are not eligible should be filtered out before aligning observations with the running variable. Though we already did, manipulation tests still failed.

In this Table, there are only two models that pass the manipulation test (the dotted ones), and the polynomial degree is high. Therefore, it is very likely that the coefficient of interest, if estimated, will be biased since the continuity assumption or continuous unobservable is violated.

Table A1: Manipulation Test Results

Polynomial	**1**		**2**		**3**		**4**	
	T	**P>|T|**	**T**	**P>|T|**	**T**	**P>|T|**	**T**	**P>|T|**
Triangular kernel								
Lowest bandwidth	5.779	0.000	3.240	0.001	5.510	0.000	0.044	0.965
Highest bandwidth	-22.073	0.000	-4.331	0.000	-30.887	0.000	-2.917	0.004
Uniform kernel								
Lowest bandwidth	4.365	0.000	11.561	0.000	2.551	0.011	4.246	0.000
Highest bandwidth	-23.842	0.000	-4.689	0.000	0.561	0.575	26.770	0.000

Note: Bandwidth selection was calculated by default algorithm using mean-squared error (MSE).
Source: Authors' calculation.

(ii) Full table of matching results

Table A2: All Matching Results Using Propensity Score Matching.

Matching methods	**ATT**	**s.e.**	**Off support**		**Balancing test**	
			Treatment	Control	Mean bias	P>chi 2
Unmatched	-1,022.6	29.4	-	-	15.2	0.000
Nearest Neighbor						
With replacement	-216.4	35.4	-	-	0.9	0.126
Without caliper	-390.4	25.4	-	-	4.6	0.000
1% of S.D.	-256.9	27.9	2,351	-	0.7	0.953
5% of S.D.	-241.2	28.0	2,342	-	0.7	0.969
10% of S.D.	-216.0	28.1	2,341	-	1.1	0.304
Radius						
1% of S.D.	-234.3	29.4	8	-	0.5	0.988
5% of S.D.	-233.4	31.1	1	-	0.5	0.985
10% of S.D.	-234.9	31.2	1	-	0.5	0.977
Kernel						
Epanechnikov						
1% of S.D.	-234.7	29.4	8	-	0.5	0.985
5% of S.D.	-233.9	31.1	1	-	0.5	0.985
10% of S.D.	-234.2	31.2	1	-	0.5	0.982
Biweight						
1% of S.D.	-235.0	29.5	8	-	0.5	0.981
5% of S.D.	-234.3	31.1	1	-	0.5	0.986
10% of S.D.	-234.0	31.2	1	-	0.5	0.984
Normal						
1% of S.D.	-235.6	31.3	-	-	0.5	0.987
5% of S.D.	-235.6	31.2	-	-	0.5	0.980
10% of S.D.	-239.3	31.2	-	-	0.6	0.913

Note: (1) Apart from "With replacement" with the nearest neighbor method, other rows are matching without replacement. (2) Standard deviation (S.D.) of propensity score is 0.162. (3) All ATTs are statistically different from zero at 0.01 level of significance.

Source: Authors' calculations.