Do Conditional Cash Transfers Impact Child Health?

Evidence from the Philippines

Abstract: Conditional cash transfer (CCT) programs remain as one of the main policy strategies in addressing global poverty. Yet, their impacts may still vary depending on institutional contexts. The goal of this paper is to provide new causal estimates of the impacts of a CCT program in the Philippines, the *Pantawid Pamilyang Pilipino* Program (4Ps), on child health outcomes. Recent evaluation of 4Ps reveals that, despite program exposure, negative nutritional outcomes persist among child beneficiaries. Leveraging birth-level data from the Demographic and Health Surveys (DHS) and exploiting the staggered rollout of 4Ps, I utilize a fixed effects counterfactual estimator (FEct) to investigate the impacts of 4Ps on infant and child mortality. I find an overall reduction in infant mortality, but not for child mortality among cohorts who received 4Ps. However, these declines were largely driven by reductions in infant mortality among those birth cohorts born during the time when 4Ps changed. I do not find significant declines in child mortality, despite programmatic reform.

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In Partial Fulfillment of the Requirements for Departmental Honors for the Bachelor of Arts in Public Affairs

Acknowledgements

To Professor Darin Christensen, I would like to thank you for the patience, effort, and all the advice as I write this thesis. Words cannot express my gratitude enough; it has been an honor to learn both the art and science of the empirical process from you. Thank you for all the lessons that taught me what an empiricist should be: a rigorous yet a cautious thinker.

To my mom, thank you for listening to me as I ramble this thesis to you. I know that you may not fully understand what it is all about, but I appreciate all your questions and rebuttals that made me think about my thesis more deeply. To the rest of my family and loved ones, especially the ones back in the Philippines, thank you for grounding and inspiring me to pursue this work and be able to give something back to our homeland.

To my *ate* in college, Jessa, thank you for all the home-cooked meals, the laughs, and the conversations. To my college friends, Abigail, Mariana, Aaron, Isabel, and Shay, thank you for cheering me up. Thank you for checking in on me. I don't know what college would be like without all of you.

To my other advisors in college, Chhan and Professor Paul Ong, thank you for giving me the opportunity to dive into the scholarship of neighborhoods. My work at the center has truly motivated me to also think about how I can bring this scholarship into understanding disadvantaged communities back in the Philippines.

Lastly, to all the folks who I have had meaningful conversations with throughout my undergraduate years, thank you for sharing all of your experiences with me. I cannot say each and everyone of y'alls names, but thank you for shaping how I currently see and understand the world.

Padayon! (English Translation: Carry On!)

Introduction

Improving children's health can break the intergenerational transmission of poverty. A large body of evidence shows that investment in the early stages of life provides long-term net gains as individuals grow into adulthood. This is intuitive: healthier children are more likely to have better cognitive abilities and skill development, allowing them to perform well in school and earn higher earnings later in life (Aizer, 2017). To invest in human capital among disadvantaged families, governments across the Global South started conditional cash transfer (CCT) programs.

CCT programs provide cash transfers to qualifying households as long as they meet specific conditions. These conditions often include prenatal care for pregnant women or regular health check-ups and school attendance for children. The rationale behind CCT programs is that they aim to provide income to decrease poverty today (Levy, 2006 as cited in Morley & Coady, 2003). The conditions encourage investments in children's health and education to develop human capital among poor families, preventing poverty among the next generation.

One such program exists in the Philippines: the *Pantawid Pamilyang Pilipino* Program (4Ps). It is the flagship poverty-reduction program in the Philippines overseen by the Philippines Department of Social Welfare and Development (DSWD). About four million households are active beneficiaries of 4Ps, covering about 40,000 *barangays* or villages, 1,485 municipalities across 82 provinces, and 148 urban areas (DSWD, 2024). It was piloted in 2007 across six cities and municipalities, then formally launched and scaled up in 2008 (Fernandez & Olfindo, 2011).

During the early years of the program, 4Ps provided a monthly education grant of 300 PHP (i.e., $6.75[1]), allowing up to three children ages 0-14 to participate as long as the registered children attend their school 85% of the time (Orbeta et al., 2023).[2] Additionally, there was a monthly lump-sum health grant of 500 PHP ($11.24) as long as all pregnancies and children meet particular requirements. Pregnant women, for example, must utilize prenatal care and postnatal care after giving birth, which is monitored once every two months. Monitored monthly, children whose ages are 0-2 years old, must be immunized. For the 2-5 years old age group, they must receive nutrition counseling and weight monitoring once a month.

Additionally, Family Development Sessions (FDS) were established as one of the program requirements. FDS are seminar-style activities conducted in local neighborhoods that require adult household members receiving 4Ps payments to attend regularly (Dizon et al., 2017; Orbeta et al., 2021). This learning opportunity helps these household members learn how to improve their familial relationships, bolster their social awareness, and encourage social and political participation within their community. Most of the modules, however, that are presented, are tailored to the issues the neighborhoods are facing and are dependent on the needs local stakeholders identified (Dizon et al., 2017).

Recent evaluation of 4Ps showed that despite program exposure, negative health outcomes persist among children (Orbeta et al., 2023). This paper offers new causal estimates of the impacts of 4Ps on child health outcomes, focusing specifically on infants and children aged one through five. To explore this, I exploit the phased-in distribution of 4Ps across households in the Philippines. Like other CCT programs, 4Ps is rolled out incrementally among poor families

[1] This is equivalent to the average annual exchange rate in 2008. From the *Bangko Sentral ng Pilipinas.* All of the basis of the converted amounts relies on this exchange rate metric.
[2] Raitzer et al. (2022) discussed that for the majority of the program history, beneficiary children of 6-14 years old, then 3-5 years old, until the maximum number of grantees have been reached, were prioritized.

in certain geographies (Schady et al., 2009). Utilizing birth histories of mothers from the United States Agency for International Development's (USAID) Demographic and Health Surveys (DHS), I explore the impacts of 4Ps using a fixed effects counterfactual estimator (FEct) on infant mortality rate (i.e., death rate of children before the age of 1 or under-1 mortality rate) and child mortality rate (i.e., death rate of children after the age of 1 and before the age of 5 or 1-to-under-5 mortality rate). Aggregating these outcomes at the cluster-cohort level and specifying whether those cluster-cohorts received 4Ps, I extract estimates of the average treatment effect on the treated (ATT). My results indicate an average reduction in infant mortality by about 34%. I do not, however, find a statistically significant decline in child mortality.

Furthermore, it is crucial to note that the design of 4Ps considerably changed during 2014 (Orbeta et al., 2021; Raitzer et al., 2022). During this year, the program extended the age coverage of eligible beneficiaries from 0-14 to 0-18 years old, increased its educational cash grant, and lifted its five-year exit policy. In 2015, they initiated an "open selection" program, permitting households to select the children that they would like to be monitored. Around 2017, they added new grants, like a lump-sum rice subsidy. By 2019, 4Ps was institutionalized as a law. More recently, fringe benefits like automatic coverage in PhilHealth, the country's public health insurance program, and priority in DSWD's sustainable livelihood programs have also been added.

Therefore, another question I hope to address is: how did these changes impact infant and child mortality? I hypothesize that because of these programmatic changes, treated cohorts born during and after 2014 should have experienced larger reductions in early-life mortality outcomes than the earlier cohorts. My evidence confirms this: I observe significant declines in infant mortality by about 54% among cohorts born during 2014 and onwards. For births prior to 2014, I

observe no significant reduction in infant mortality in clusters covered by 4Ps. I also notice an average decrease in child mortality following 4Ps' redesign, but this decline is not statistically significant.

My findings suggest that after adjustments were made in the program, 4Ps drove significant improvements in child health outcomes. To the best of my knowledge, this is one of the first few studies that exploits the program rollout of CCT programs like 4Ps, while explicitly examining the impacts of programmatic overhaul on infant and child mortality outcomes. This type of analysis, as a systematic review from Millan et al. (2019) has called for, is crucial in assessing whether the trajectory of CCT programs' short-term impacts is on track to meeting their long-term goals.

These results also offer important and practical policy implications for the implementation of 4Ps. Unlike other CCT programs, 4Ps was introduced much later than its Latin American counterparts.[3] As a consequence, it is challenging to estimate its intergenerational impacts as the initial cohorts of beneficiaries have yet to approach late adulthood. While practitioners, policymakers, and researchers await these long-term outcomes, my findings highlight the value of inclusive programmatic changes to strengthen positive short-term health outcomes among current beneficiaries. These outcomes are essential as they likely translate to positive long-term outcomes later in life.

The paper is organized as follows. First, I review previous and current literature on the impacts of CCT programs on infant and child mortality. Second, I describe my data and empirical strategy to estimate my outcomes of interest. Third, I present and discuss my key results. Lastly, I conclude with limitations and implications of my analysis.

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[3] The first CCT program started in Brazil in 1995. The first wave of CCT programs were implemented in Mexico, Honduras, and Costa Rica. Stampini & Tornarolli (2012) offers a brief history of these programs.

Literature Review

Across a variety of settings, experimental and non-experimental studies have found that CCT programs reduce infant mortality. For example, Barham (2011) found that *PROGRESA*, Mexico's largest CCT program, reduced infant mortality, but not neonatal mortality[4], across rural areas, using a robust econometric strategy. In Brazil, Rasella et al. (2013) found significant reductions in under-5 mortality from the *Bolsa Familia* program, using a fixed-effects negative binomial model on birth-level panel data. Furthermore, experimental evidence from Okeke and Abubakar (2020) illustrated that conditional payments[5] to households in Nigeria decreased child deaths in-utero.

The empirical literature has identified potential mechanisms behind these declines in infant mortality. Okeke and Abubakar (2020), for instance, highlighted the role of prenatal health investments, as program conditionalities require, in reducing child deaths in-utero. This aligns with theoretical expectations: pregnant women are incentivized to consume maternal healthcare. This, in turn, should translate to positive newborn and child health outcomes (Gaarder et al., 2010). Moreover, Brauw and Peterman (2020) emphasized that skilled birth delivery may also be an essential mechanism. Skilled birth attendance is crucial in decreasing newborn mortality because it professionally manages obstetric issues during deliveries (Bhutta et al., 2014; Oridanigo et al., 2022).

While findings from the literature have implied CCT's crucial role in improving infant mortality outcomes, an essential policy question remains: do these gains in early-life outcomes

[4] Moss et al. (2002) suggests that because neonatal mortality refers to the death on the first seven days of the baby, the conditionalities of most CCT programs may have not prevented it (as cited in Barham, 2011). Most of the conditionalities, such as prenatal care, may have only been relevant during a considerable amount of time a mother is pregnant.

[5] It may be important to note that this is not a typical CCT program. Their experiment relies on a "condition" that mimics what a CCT may have done.

translate into long-term, intergenerational impacts? Unfortunately, a review of the literature on the long-term impacts of CCT programs from Millan et al. (2019) revealed that due to the varying methodological approaches–both experimental and non-experimental–long-term findings are still inconsistent and limited. Cahyadi et al. (2020) reasoned that intergenerational impacts may be more noticeable in succeeding generations, rather than in the present generation who is receiving the cash transfers. Their experimental evidence in Indonesia displayed that while positive short-term effects are evident among households who were continuously receiving the transfers, no "transformational" gains were apparent.

Moreover, in another comprehensive review of the literature, Bastagli et al. (2016) noted that CCT programs have varying impacts within and across households. Across variation in household characteristics, Van de gaer et al. (2013) depicted that CCT programs induce more health gains among children from indigenous backgrounds, in comparison to non-indigenous children. In another analysis, Parker and Vogl (2023) found that childhood exposure to a CCT program led to positive educational and labor market outcomes for women; for men, these impacts were smaller. Within households, Raitzer et al. (2022) showed that differences in whose children are monitored in the program–to assure that they are meeting the program conditionalities–led to differences in their health outcomes.

Beyond variation within and across households, the literature has pointed out that the program's features can also influence the trajectory of outcomes. For example, the majority of CCT programs utilize a targeting strategy to determine eligible households (Schady et al., 2009). These targeting strategies naturally lead to timing differences when a household receives the transfers: some receive it early, while others receive it later. Empirical work by Clavijo (2020) argued that timing can have an influence on a household's welfare trajectory (or, economic

mobility), relative to their baseline assets and capital. Barham et al. (2024) provided further evidence of differential long-term impacts of the CCT program among men and women relative to when they received the cash transfer. Another example involves the amount of transfer itself. Experimental evidence from Bryan et al. (2023) revealed that larger transfers can induce distortionary effects, reducing the potential benefits the program may provide to its recipients.

What happens, then, to CCT programs' outcomes as its design changes over time? A few research studies have explored this question, in terms of child health outcomes. One of the earliest studies came from Attanasio et al. (2015) where they leveraged a program change, the family's registration date (FRD), of a CCT program in Colombia to assess whether conditionalities may matter. Under the redesign of the program, children born after the FRD were not subject to the condition of preventive healthcare visits. They found that children born before the FRD yielded better health outcomes than those born after it, underscoring the importance of conditionalities in CCT programs.

Program Context

Piloted in 2007 across six cities, 4Ps remains the largest social safety net program in the Philippines. After 2007, the program was gradually rolled out across administrative boundaries, prioritized based on their respective poverty incidence. 4Ps was then formally launched and scaled up in 2008 (Fernandez & Olfindo, 2011). The pilot set covered only 3 regions and 3 provinces in 2007. By the first wave, which was rolled out in 2008, all regions, 33 provinces, and 170 municipalities/cities were covered. By the end of 2013, the program was scaled up to 79 provinces, 143 cities, and 1,484 municipalities (DSWD, 2013). While larger administrative boundaries had already received 4Ps by around 2013, quarterly reports from 4Ps suggested that

not all *barangays*, or localities, received 4Ps, and the rollout occurred at the locality and household levels (Orbeta et al., 2021). At the end of 2013, about 41,008 localities were covered; this number increased as of the first quarter of 2014, where 41,358 were covered. By the end of 2014, 41,513 localities were covered, while approximately 1.2% of all localities nationwide still had not been covered (DSWD, 2014).

At the household level, potential beneficiaries were targeted by relying on a proxy means test (PMT) (Fernandez & Olfindo, 2011). The PMT utilized data from the National Household Targeting System for Poverty Reduction (NHTS-PR) or *Listahanan* to classify poor and/or near poor households, providing an objective representation of poverty status across households in the Philippines. It should be highlighted that the targeting system is nuanced and follows a multi-stage process. At the end of the process, selected households at the locality level are subject to community validation.

The gradual rollout of 4Ps has critical implications for outcomes among its recipients. In particular, Orbeta et al. (2021) found potential impacts of the program's staggered rollout on nutritional outcomes of 4Ps' child beneficiaries. Their analysis utilized birth cohorts from 2009 and 2013, from the localities that were originally a part of the treatment and control group. Those households in the treatment group received the benefits of the program during the critical early-life period (i.e., the first 1000 days of life), while the control group received them afterward. They found that, after several years, the nutritional outcomes, such as stunting and wasting, were better among children whose households received it during the first 1000 days of life, pointing to evidence that 4Ps had "lock-in" effects during the critical period.

While Orbeta et al. (2021) provided strong evidence in explaining the puzzling outcomes from the most recent evaluation, their analysis may have overestimated the "lock-in" effects of

4Ps using this cohort analysis. Specifically, the sample of the birth cohorts they used was born before 4Ps' major overhaul. If they observed the trajectory of the health outcomes of these birth cohorts over time, the programmatic changes may have also driven improvements in child health outcomes after these birth cohorts grew out of early childhood.

Therefore, my analysis extends and strengthens the findings of Orbeta et al. (2021). First, I analyze birth and child cohorts starting from 2008 to 2017, to isolate the causal impact of 4Ps, conditional on birth and child cohorts born during a specific year. This shows the natural evolution of the impacts of 4Ps on child health outcomes across cohorts after 4Ps was both implemented and changed nationwide. Additionally, because I focus on the immediate impacts of 4Ps on child health outcomes, such as infant and child mortality, I can also infer the potential mechanisms through which 4Ps affect children. Highlighting these impacts also provides clues as to which cohorts are more likely to have better health outcomes later in life.

Data

My main source of data is the Demographic and Health Surveys (DHS), which are nationally representative, cross-sectional surveys implemented in multiple waves across ninety countries. These surveys investigate the demographic and health characteristics of a country's population (Croft et al., 2023). The data is organized at different levels, as shown in **Appendix 1.A.1**.

I leverage records at the birth level from two DHS survey waves in the Philippines: 2017 and 2022. These records are available as each mother that was interviewed reported her birth history, including some birth-related information such as the child's date of birth.[6] My analysis

[6] While some of this information, like birth weight and duration of pregnancy, are available, there is missingness, incompleteness, and inconsistency with these variables.

focuses on infant and child mortality outcomes. Namely, I focus on the death of an infant before they turned 12 months old (i.e., under-1 mortality) and death of a child between 12 and 59 months of age (i.e., 1-to-under-5 mortality). For simplicity, I label the under-1 mortality as infant mortality and the 1-to-under-5 mortality as child mortality (Beckett et al., 2001 as cited in Godlonton & Okeke, 2016).[7] Each of these outcomes was initially coded as a binary variable at the birth level, where 1 corresponds to when an infant or child died and 0 otherwise.

I utilize the primary sampling units (PSUs), also known as the survey clusters, from a two-stage stratified sampling design as my spatial unit of analysis. In total, I have 2,064 clusters in my dataset from two of the survey waves. About 70% (N = 1443) are classified as rural areas, while 30% are classified as urban areas (N = 621). **Appendix 1.A.2** lays out the geographic distribution of some[8] of these clusters across the Philippines by survey wave. **Appendix 1.A.3** also maps the geographic distribution of these clusters by their urban classifications. Each of these clusters represents a *barangay* or village, a part of a large village, or at least two or more villages that are in proximity with one another. At each unique cluster from the survey waves, I aggregate the mortality rates by cluster and cohort (i.e., birth year), where I compute the rate of mortality in each cluster-cohort from 2000 to 2017 (Croft et al., 2023). I focus on cohorts from 2000 to 2017 to create equal pre-program and post-program periods.

I, then, create an indicator variable to identify whether a particular birth cohort did or did not receive 4Ps. Unfortunately, I do not have the exact year that each cluster received 4Ps in the data. Therefore, I had to infer the year a cluster started receiving 4Ps by extracting the earliest

[7] Child mortality is often defined as the death under the age of 5, inclusive of the death rate of births before the age of 1. While I recognize that there are significant implications on the way this is defined in the paper, I reason that the criteria I used in this paper is to isolate the impact of 4Ps at birth and after birth.

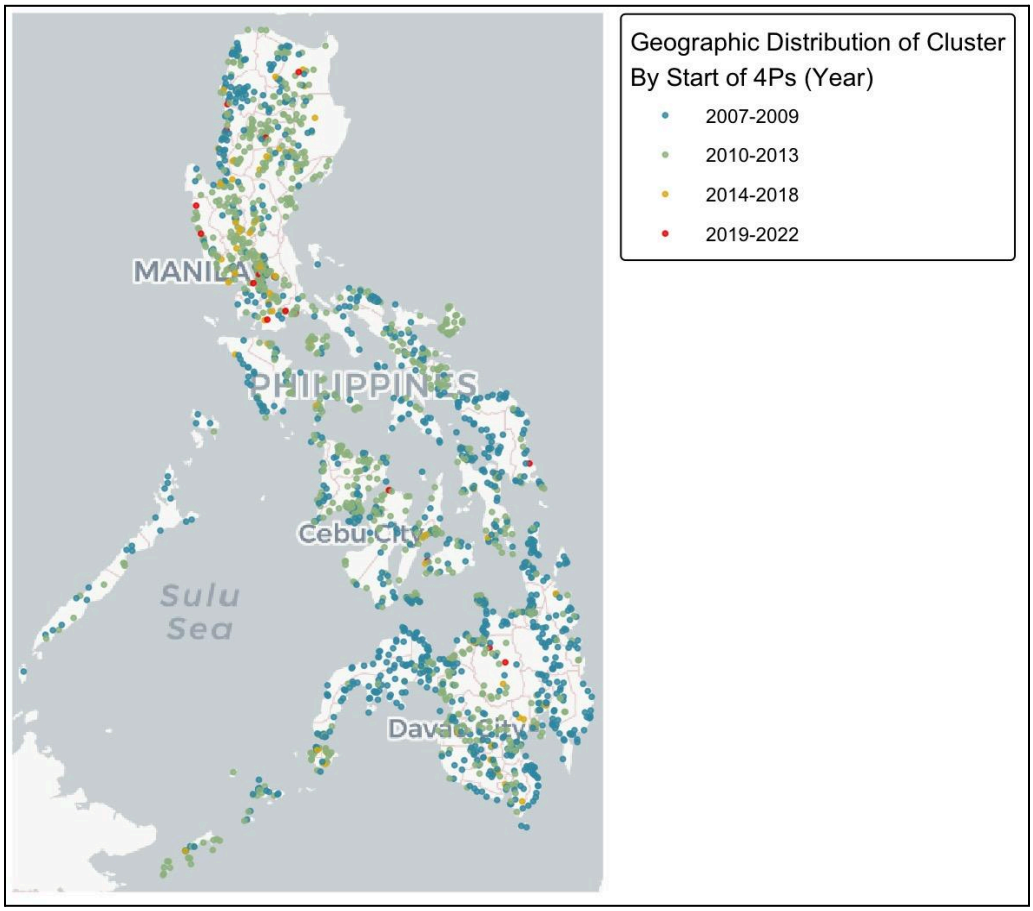
[8] DHS displaced the exact location of these clusters for both the survey waves. This is to maintain confidentiality. There are some clusters that have also been displaced to geometric coordinates of (0,0) within 0-2000 meters and 0-5000 meters for urban and rural points respectively. Though, for 1% of the rural points, this was displaced from 0-10,000 meters. I dropped these cluster units in the visualization, but not for my analysis. The geographic distribution visualized represents 2031 clusters, 98.4% of the total cluster units I have.

year any household reported receiving 4Ps. This approximation is possible because each household self-reported on whether they received 4Ps[9] and the year they received it. Because of self-reporting, some responses from households had measurement errors.[10] Consequently, I had to reasonably correct these errors using information from 4Ps.

For each unique cluster, **Figure 1** maps the rollout of 4Ps across the Philippines. **Appendix 1.A.4** also shows that the majority of clusters received 4Ps around 2009 and 2010. After these years, the number of new clusters who received 4Ps decreased. Then, for each cluster, I define the year they started receiving 4Ps as their start of treatment. For each cluster's birth or child cohort, I code their "treatment" and "control" status using an indicator variable. For cohorts born after a cluster's start of treatment, I code it as 1 to denote that they were treated. For cohorts born during or before a cluster's start of treatment, I code it as 0 to denote that they were in control. I argue that when a cluster started receiving 4Ps, the impact of 4Ps may be well-defined for those cohorts who were born a year later, rather than those who were conceived at the start of the cluster's treatment.

[9] This follows a conventional binary assignment of "treatment" where 1 was assigned if the household received 4Ps and 0 if otherwise.
[10] One example of such measurement errors includes indicating a year even before the 4Ps was rolled out.

Figure 1: Geographical Distribution of Starting Years Cluster Received 4Ps



One assumption, however, that I am making is that once a cluster receives 4Ps, the succeeding cohorts receive it as well. I justify that this is a reasonable assumption given my data. Quarterly reports suggest that within localities, not all households would receive it because it continuously targets households depending on their poverty status. Because I am also using a cluster-level analysis, there are no concerns about the delisting of households over time. While there would be households that would be removed, it is unrealistic to expect that the cluster would have no recipients for a given cohort, reversing the cluster-cohort's treatment status from treated to control.

In the end, I have a time-series, cross-sectional (TSCS) dataset. **Table 1** summarizes my data. Each survey wave represents different births, households, and clusters, which is the

cross-sectional component. In addition, in each of these survey waves, I observe mortality rates and indicate the treatment status of each cluster across each birth cohort (for parsimony, I will refer to this as cluster-cohorts throughout the rest of the paper), which is the time-series component.

Table 1: Summary Statistics of the TSCS Data

	Min	Median	Max	Mean	SD
Birth Cohort (Year)	2000.000	2009.000	2017.000	2008.658	5.073
Cluster Urban Indicator[1]	0.000	0.000	1.000	0.293	0.455
4Ps Indicator[2]	0.000	0.000	1.000	0.388	0.487
Infant Mortality	0.000	0.000	1.000	0.020	0.109
Child Mortality	0.000	0.000	1.000	0.006	0.061

[1] This indicates whether the cluster is an urban area. Urban corresponds to indicator value, 1; rural corresponds to indicator value, 0

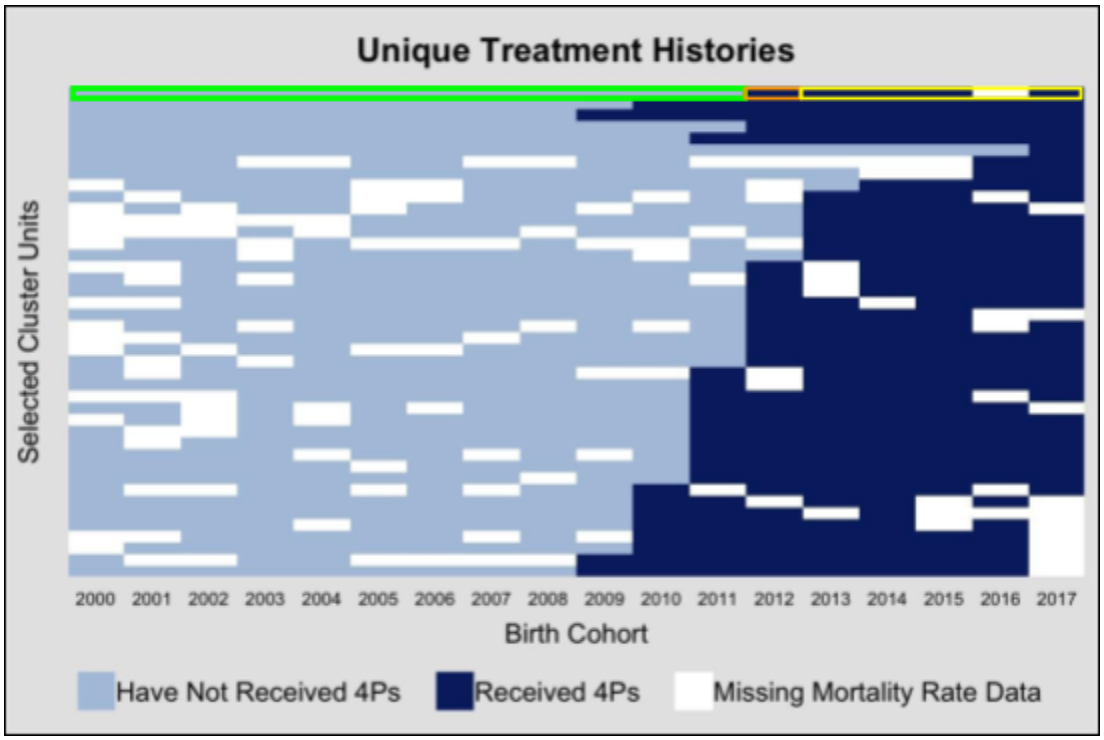
[2] This indicates the cluster's 4Ps status. Receiving 4Ps corresponds to indicator value, 1; not receiving corresponds to indicator value, 0

Empirical Strategy

Finally, I use my data to provide causal estimates of the impacts of 4Ps on infant and child mortality outcomes. I observe mortality rates for most cohorts, but some cluster-cohorts do not have any recorded births. **Figure 2** illustrates the structure of my data. To explain this further, consider the first cluster in **Figure 2.** The orange outline indicates that the first birth cohort that received 4Ps in the first cluster was in 2012. After 2012, the yellow outline denotes those cohorts as treated. Untreated cohorts, or those birth cohorts from 2000 to 2011, are represented by the green outline. Additionally, I clarify that in the 2016 cohort, there were no recorded births. I then estimate the average treatment effect on the treated (ATT) cluster-cohorts. To construct these ATT estimates, I approximate the counterfactual mortality rates for every treated cluster-cohort.

That is, I estimate the outcome: had a specific treated cluster-cohort not received 4Ps, what would have been their infant and child mortality rates?

Figure 2: Staggered Rollout of 4Ps in Selected Clusters



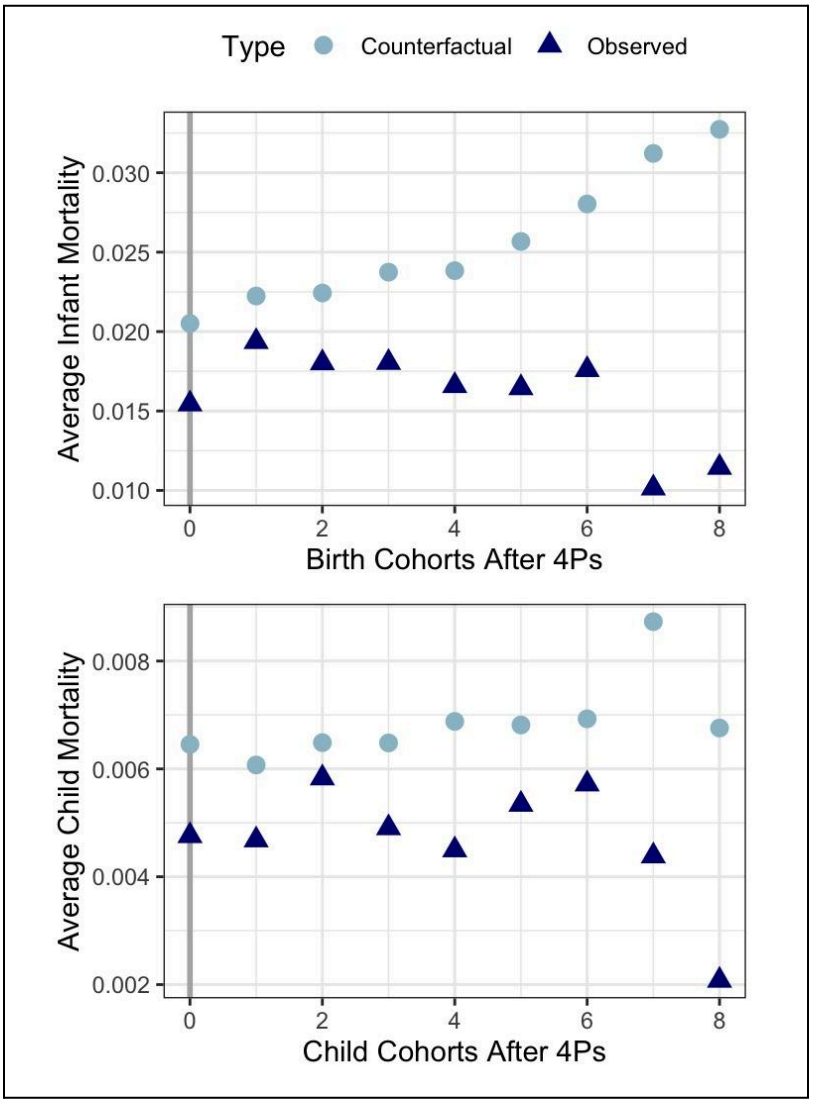
Leveraging the TSCS nature of my data, I use the fixed effects counterfactual estimator (FEct), proposed by Liu et al. (2021) to estimate these counterfactuals. Applying their estimation strategy, I use the untreated cluster-cohorts to estimate the following two-way fixed effects model:

$$Y_{cb}(0) = \alpha_c + \beta_b + \varepsilon_{cb}$$

where $Y_{cb}(0)$ is the counterfactual rate for a specific treated cluster-cohort, α_c is the cluster-fixed effects, β_b is the cohort fixed effects, and ε_{cb} is the idiosyncratic error term. The model assumes no time-varying confounding variables that influence why a cluster-cohort would receive 4Ps and their respective outcomes. The standard errors are cluster-robust.

Taking the difference between the average estimated counterfactual and the average of the observed outcome produces the ATT of 4Ps on mortality outcomes. I note that there are many ways to represent these ATTs. For example, in **Figure 3**, I take the average of all of the observed and counterfactual estimates of the mortality rates for cohorts who were the first ones to get treated in their cluster. I also do this for the other cohorts who got treated second, third, and so on in their respective clusters. Plotting these averages, the vertical gap between the observed and the counterfactuals is the ATT relative to the cohort's treatment timing.

Figure 3: Counterfactual and Observed for Infant and Child Mortality Outcomes



Furthermore, I utilize FEct as an estimator because of several reasons. I use a linear two-way fixed effects (TWFE) model to estimate my counterfactuals, where FEct effectively controls for the time-invariant and unobserved attributes of the clusters and the time-specific shocks in birth cohorts through the cluster-level fixed effects and birth-cohort fixed effects, respectively. These time-invariant and/or unobserved characteristics within a specific cluster, like culture, terrain, and geography, and time-specific shocks at the macro-level, can all influence an infant's health outcome, especially in developing countries like the Philippines. Consequently, this removes the need to use a measure of wealth to control for potential confounders, like the provided wealth-index factor from DHS data, which gives limited information as these variables are not observed at each cluster-cohort.

This strategy also addresses potential biases that may arise from using TWFE models as addressed in the current literature. One source of bias–also known as attenuation bias–comes from the potential "mismatch"[11] Imai and Kim (2021) cautioned about when using TWFE. Another potential concern arises when there are heterogeneous treatment effects across time and units that create negative weights on the coefficient of the treatment effects (de Chaisemartin and d'Haultfoeuille, 2020). However, as Liu et al. (2022) emphasized, because I never use the clusters who received the 4Ps early on as the control group, I prevent the "negative weighting" problem induced by cluster units who received 4Ps early on (Goodman-Bacon, 2021). Additionally, although the literature has proposed various work-arounds to this problem (Sun and Abraham, 2021; Callaway and Sant'Anna, 2021), they are sometimes statistically inefficient or

[11] This "mismatch" stems from the variation in the state of being in a "control" for a particular time and unit. However, my applied estimation strategy is not contaminated with these "mismatches." This is because my "treated" units are denoted as "missing." Therefore, when I estimated my TWFE, I only have the pure "control" units to estimate my counterfactuals from.

have restricted applicability. Borusyak et al. (2024) found that this estimator can be more efficient in comparison to these other proposed estimators, under homoskedasticity.

Results

To reiterate, the estimated impact of 4Ps on early-life mortality outcomes is represented as the difference between the averages of the counterfactual and observed mortality rates across all treated cluster-cohorts. **Table 2** shows that, on average, 4Ps significantly reduced infant mortality by about 34% compared to the average predicted counterfactual. For child mortality rates, there is a noticeable decline, though it is not statistically significant.

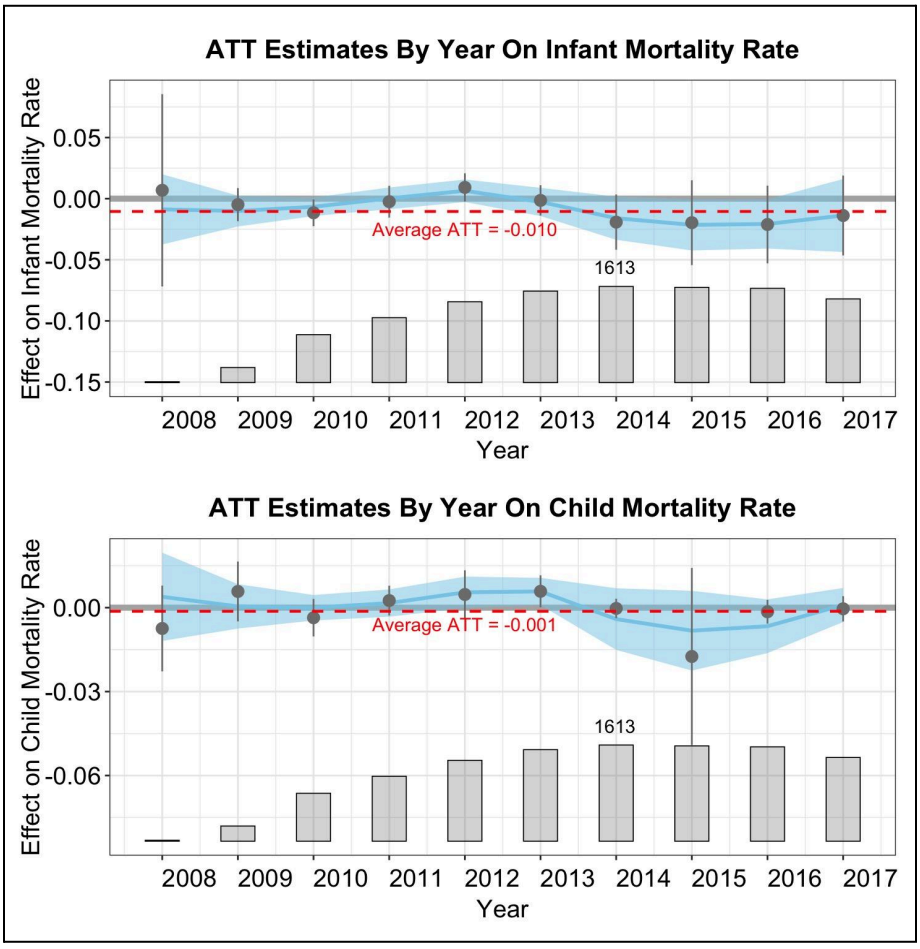
Table 2: Overall ATT Estimates of 4Ps on Infant and Child Mortality Rates

	Child Health Outcomes	
	Infant Mortality	Child Mortality
Overall Average Treatment Effect on the Treated (ATT) Estimates		
Obs. equally weighted	**-0.010***	-0.001
	(0.005)	(0.003)
Predicted Counterfactual	0.029	0.007
Treated Clusters	2009	2009
Never Treated Clusters	54	54
Time Periods	18	18
Observations	37,134	37,134
* $p < 0.05$		

Nonetheless, the effects may not all be the same among the treated cluster-cohorts. In particular, major programmatic reform may have led to differential impacts between treated cluster-cohorts born during and after 2014 and those born before these changes. To examine this further, **Figure 4** presents the

evolution of the ATT estimates, with a 95% confidence interval, after 4Ps was piloted.[12] I observe that the overall reductions in infant mortality rates are largely driven by treated cluster-birth cohorts born from 2014 onwards. For child mortality rates, I observe no significant effect across all treated cluster-child cohorts over time, though the largest effect seems to have taken place around the 2015 child cohort. These results suggest that programmatic changes in 4Ps may have significantly impacted early-life mortality.

Figure 4: ATT Estimates of 4Ps on Infant and Child Mortality Rates By Calendar Year



While it may be concerning that my ATT estimates are statistically insignificant for both outcomes, this is due to the limited sample of treated cluster-cohorts in a calendar year. To overcome this, I re-estimated my model and constructed new ATT estimates on the two mortality outcomes by defining two new groups. One group comprises treated cluster-cohorts in 2014 and onwards; the other group

[12] This is the difference between the averages of the counterfactual and observed mortality rates of the treated cluster-cohorts born in each specific calendar year after 4Ps was piloted.

includes treated cluster-cohorts before 2014. This disaggregation increases statistical precision of the ATT estimates. **Table 3** showcases these new ATTs. My results indicate that after the major program overhaul, 4Ps yielded a decrease in infant mortality by about 54% from the predicted counterfactual. Prior to these changes, I observe no significant effects. For child mortality, I observe a reduction by about 67% after the program changed, but it remains statistically insignificant. Still, this indicates a larger decline compared to the pre-2014 child cohorts, when child mortality increased by about 67%.

Table 3: ATT Estimates of 4Ps on Infant and Child Mortality Rate Pre- and Post- 2014

	Child Health Outcomes			
	Infant Mortality		Child Mortality	
	Pre-2014 Group	Post-2014 Group	Pre-2014 Group	Post-2014 Group
Obs. equally weighted	0.002	**-0.018***	**0.004***	-0.006
	(0.003)	(0.009)	(0.002)	(0.004)
Predicted Counterfactual	0.019	0.035	0.006	0.009
Treated Clusters	1803	2000	1803	2000
Never Treated Clusters	261	64	261	64
Time Periods	18	18	18	18
* $p < 0.05$				

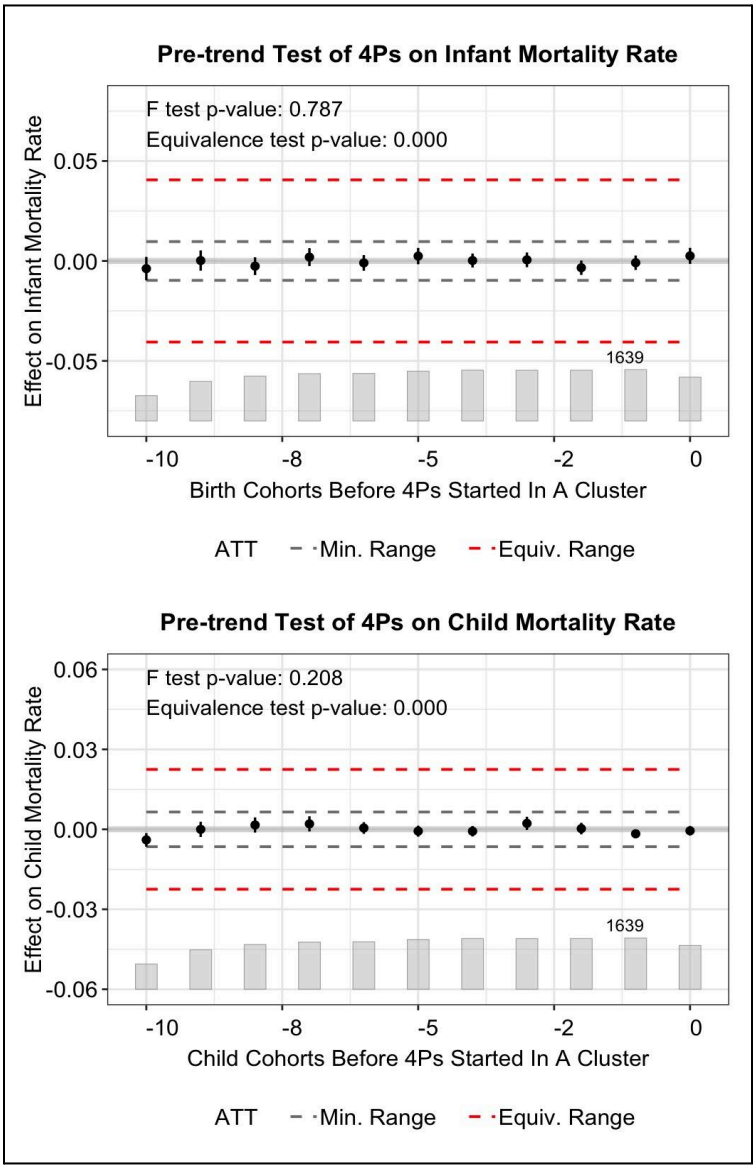
It should be noted that my estimation strategy yields estimates that are equivalent to conventional difference-in-differences (DiD) estimates. Like in any empirical setting that uses DiD as an estimation strategy, it is a vital concern if there are parallel trends between the observed and counterfactual mortality rates, among cohorts, before their clusters even received 4Ps. This means that in the absence of 4Ps, the observed and counterfactual mortality rates would have had the same trends or trajectories among cluster-cohorts. Though testing whether the parallel trends assumption holds is impossible, I test for

pre-trends before clusters even receive 4Ps. Pre-trends refer to the differential trends between the observed and the counterfactual outcomes; its existence is suggestive of potential violations of the parallel trends assumption and misspecification of my FEct model.

I apply an F-test of pre-trend fit and an equivalence test to provide evidence that my estimation strategy is robust from such concerns. The F-test is a hypothesis test for pre-trends; it has a null hypothesis of no pre-trends. Additionally, the equivalence test diagnoses whether the confidence intervals (CIs) of my ATT estimates, relative to the cohort's treatment timing, fall within an equivalence range: a "reasonable" range[13] through which the ATTs CIs can fall on before pre-trends are likely to hold. **Figure 5** reveals that I fail to reject the null hypothesis for pre-trends using the F-test. On average, my estimates also fall inside the equivalence range. My evidence suggests that pre-trends between my true and counterfactual estimates are unlikely and that my model is appropriate to estimate my counterfactuals.

[13] I used the pre-specified, default range from Liu et al. (2021). The specific range is 0.36σ in which σ is the standard deviation of the mortality rates after estimating the TWFE.

Figure 5: Pre-trend Test of 4Ps on Infant and Child Mortality Rates

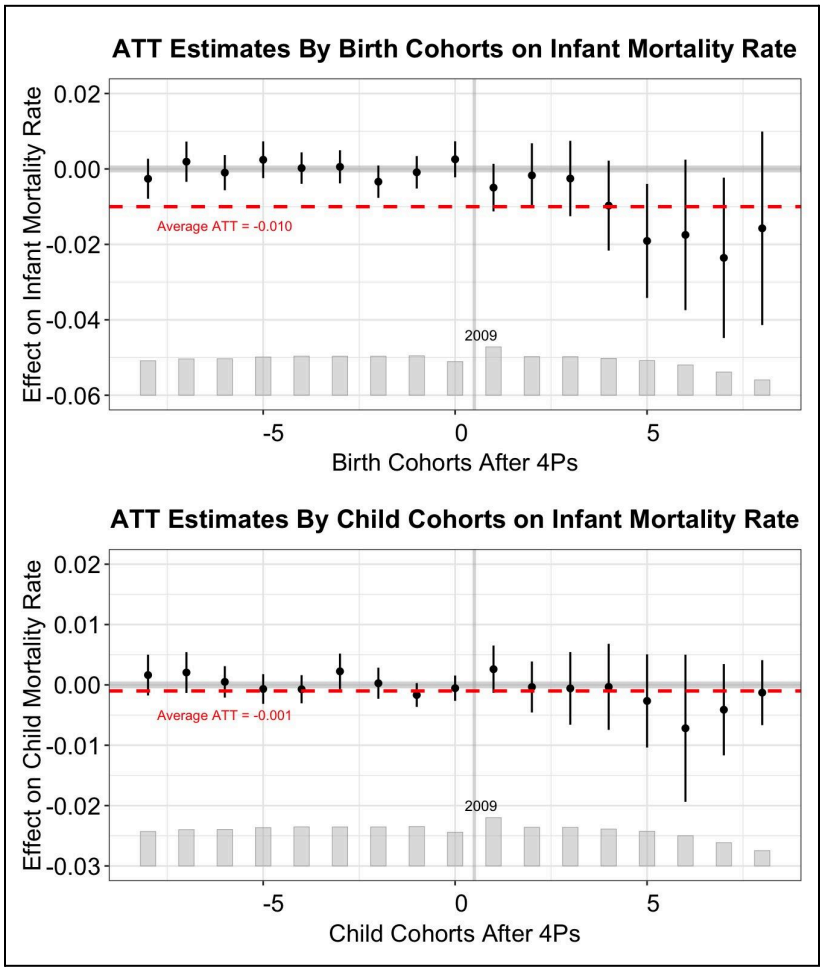


Discussion

Does 4Ps only have significant impacts on later birth or child cohorts? Recall earlier that the differences between the observed and counterfactual mortality rates in **Figure 3** are the ATT estimates based on the cohort's treatment timing. These differences are shown in **Figure 6** with a 95% confidence interval. I show that relative to a cohort's treatment timing, I see notable

declines in infant mortality rates four to seven birth cohorts after 4Ps started. Still, I do not observe striking reductions in child mortality rates across all treated child cohorts. While it is logical to infer that there is a delay in impacts as suggested by the glaring reduction in infant mortality rates on later cohorts, this may not be an appropriate interpretation. **Appendix 1.A.4** conveys that the majority of the treated clusters started during a major rollout in 2009 and 2010. After about four to seven cohorts within this majority of clusters, programmatic reform of 4Ps happened. This means that 4Ps may not necessarily have a delay in impacts, but its significant influence in child health outcomes may have coincided with the programmatic overhaul that started in 2014.

Figure 6: ATT Estimates of 4Ps on Infant and Child Mortality Rates By Time Since Treatment



Still, one striking outcome is the significant impact of 4Ps on infant mortality, but not for child mortality. Through what channels, then, do these changes in 4Ps' design impact infant and child mortality outcomes? One of the mechanisms that is essential to recognize is intra-household allocation (Raitzer et al., 2022). In theory, the allocation of benefits from 4Ps may have been prioritized among younger children (i.e., infants) than the older children. The first few months of life are the most vulnerable in a child's life (WHO, 2024); in turn, households may have prioritized infants to receive the cash transfer (Yi et al., 2015). This hints that the cash transfers may have had an impact on in-utero health investments more than health investments among older children, even when they are both subject to conditionalities.

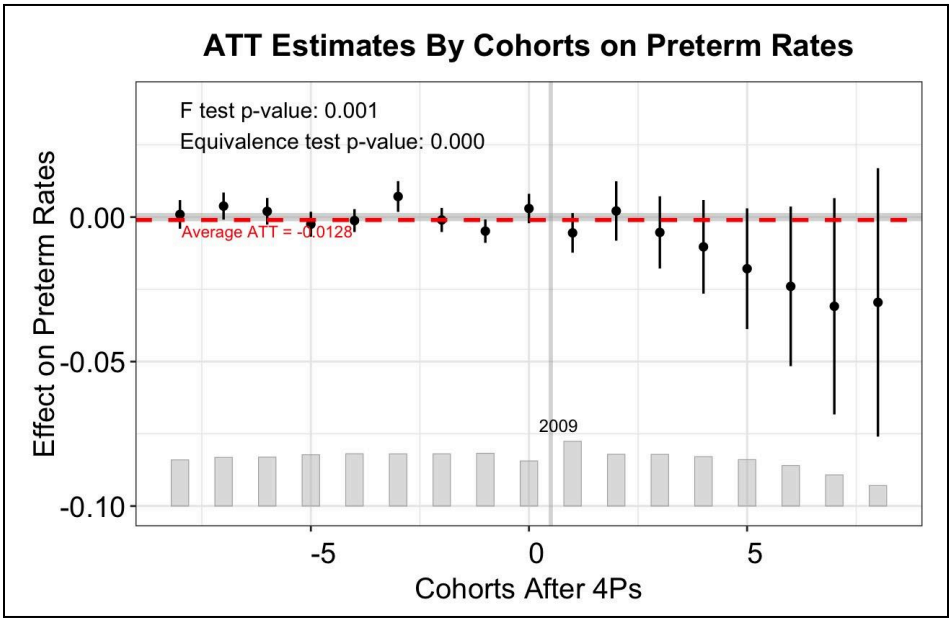
I, nonetheless, investigated this by looking at pre-term rate outcomes, or the rate of babies within a cluster who were born before a full-term pregnancy (i.e., the duration of the pregnancy is less than nine months). While I do not provide evidence for imbalanced intra-household allocation, using this outcome variable provides some indication as to whether 4Ps is effective in motivating pregnant women to seek and consume prenatal care.[14] That is, it is more reflective of the cumulative health investments, like prenatal care, that were made in-utero, in comparison to infant mortality that may already be influenced by postnatal care.[15] **Figure 7** demonstrates the ATT Estimates on pre-term rates by a cohort's treatment timing. Like for infant mortality outcomes, 4Ps has noticeable reductions in pre-term rates four to seven birth cohorts in; though, they may be statistically insignificant. Therefore, I infer, with caution, from my descriptive evidence that 4Ps may have only had a proper stimulation of in-utero health

[14] It is crucial to note that there are variables in the DHS data that are representative of what and how a mother consumed prenatal care. However, there is a lot of missingness at the birth level; from an empirical standpoint, the omission of missing data may decrease the statistical power, leading to insignificant estimates. Recall bias may also be another problem that may bias the estimates (Boerma & Sommerfelt, 1993).

[15] This is a consequence of the criteria I used for infant mortality. Because I defined pre-term rates within the first eight months of life, the next four months of life may have substantially improved, that can be a channel in reducing infant mortality. Isolating the pre-term rates allow some suggestive evidence that the channel that 4Ps may have affected my outcomes of interest may be through in-utero health investments.

investments around the time when the program was adjusted. This is not to say that pregnant women did not seek prenatal care before the program overhaul. A plausible explanation for this pattern is that the program features prior to these changes may not have provided a substantial income effect.

Figure 7: ATT Estimates of 4Ps on Preterm Rates By Time Since Treatment



This is not an overstatement. Qualitative literature regarding 4Ps has emphasized that the cash transfers are not considerably large, even with the recent changes in the amount of cash grants given. A policy note from Gudmalin et al. (2024) demonstrated that the grants have not adjusted with inflation since 4Ps' enactment, relative to the purchasing power of households at the bottom 30% of the income distribution. Alinsunurin (2021), and Nagasaka and Seki (2020) further supported these claims: qualitative findings showed that the amount of cash grants is not sufficient; most of them have to rely on other sources of support (e.g., other government programs and family overseas). Therefore, I speculate that the program adjustments on the cash grants may have resulted in a considerable income effect on household spending of an infant's

healthcare, but not for older children. This, as a consequence, may have translated into reductions in infant mortality, but not so much for child mortality.

It is also important to recognize that apart from the increase in transfer, expanding the eligibility criteria and lifting the five-year exit policy may have also contributed to these impacts. Qualitative and quantitative findings revealed that beneficiaries are often mistargeted (Ting, 2022) due to an outdated registry (Dadap-Cantal et al., 2021; Gudmalin et al., 2024; Raitzer et al., 2022; Velarde, 2018) and the highly political provision of public goods and services in the Philippines (Eadie, 2022; Eadie & Yacub, 2024; Hutchcroft, 2017). Expanding the eligibility criteria may have included more households, indirectly addressing the mistargetting issue by increasing the probability of poor households to be included in the program. Additionally, lifting the five-year exit policy may have also allowed more households to stay in the program longer, sustaining the cumulative impacts of 4Ps as a social safety net and diminishing the detrimental impacts of these exit policies on child health outcomes (Buser et al., 2014 as cited in Bastagli et al., 2016). In turn, all of these changes may have provided a general positive effect on the poor households' economic well-being, including improvements in infant health outcomes, and may have reduced some unintended social and economic costs (Cameron & Shah, 2014; Della Guardia et al., 2022).

Conclusion

While my findings provide crucial insights into the role of programmatic changes of CCT programs in infant mortality outcomes, I caution that they should not be overinterpreted. First, because multiple features of the program were adjusted, I cannot pinpoint the exact mechanism that drove these reductions in infant mortality. There may also be other factors, such as

improvements in healthcare supply, that may have driven these outcomes. Second, due to data limitations, my main unit of analysis is at the cluster level from the primary sampling units of the DHS data. These clusters may or may not truly represent the localities through which 4Ps was rolled out. Additionally, the cluster-level analysis may not fully explain why infant mortality decreased after these programmatic changes. As the literature has established, household-level factors influence child health outcomes; my empirical findings do not directly test this. Future research, therefore, should incorporate this type of analysis at the household level. They should also extend this analysis to other health outcomes; for instance, they may consider tracking the nutrition and anthropometric measures of post-2014 birth cohorts several years after. This assessment would allow for inferences about 4Ps' ability to sustain its effects well beyond the first year of life.

Despite potential limitations, my findings still have critical implications for the implementation of 4Ps in breaking the intergenerational transmission of poverty among the poorest households in the Philippines. While my findings may signify that 4Ps is delayed in delivering a significant impact on child health outcomes, this should not necessarily be the main takeaway. I highlight that dynamic and inclusive programmatic changes to social policies like 4Ps have an essential role in improving child health outcomes. As 4Ps remains the flagship social safety net program in the Philippines, policymakers and practitioners may need to focus their efforts and attention on constant reforms to the program if they aim to see sustained effects across all beneficiaries. It is only through these continuous adjustments that they may be able to see transformative changes in the lives of the vulnerable population in the Philippines.

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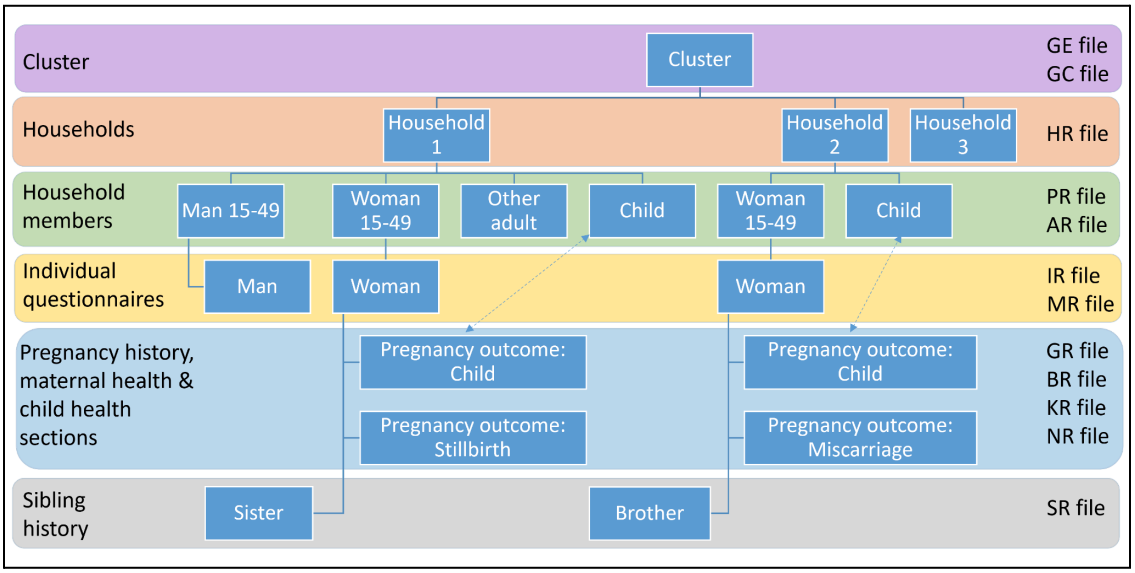
Appendix

Appendix 1.A

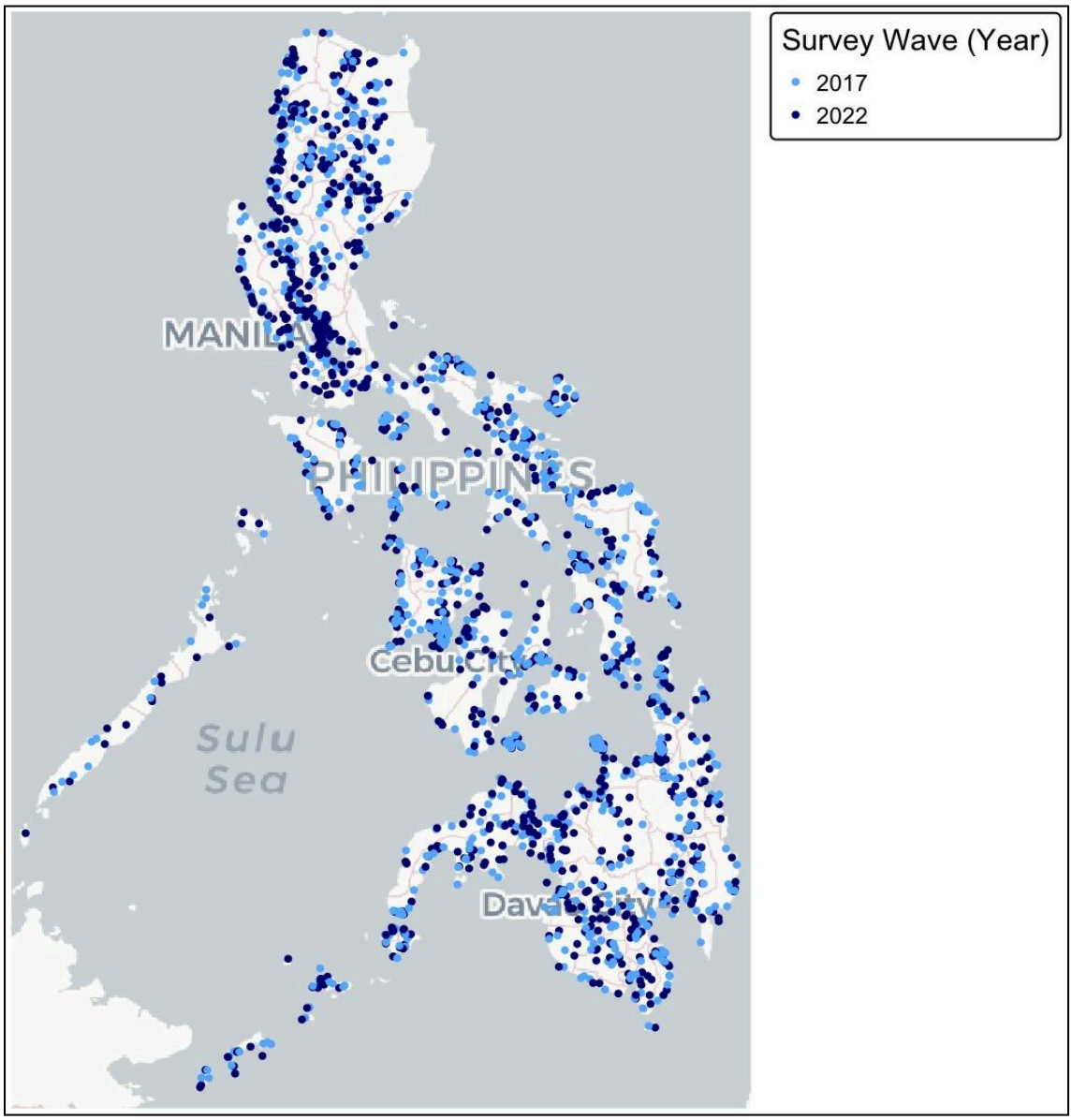
This appendix provides additional details on the structure of my data. It also shows some of the descriptive characteristics of my data. This, I hope, can help in interpreting the estimates I presented: what is the scope and what are the potential limitations from it.

Appendix 1.A.1.: Sample Structure of DHS Data from DHS-8 (2022 Survey Wave)

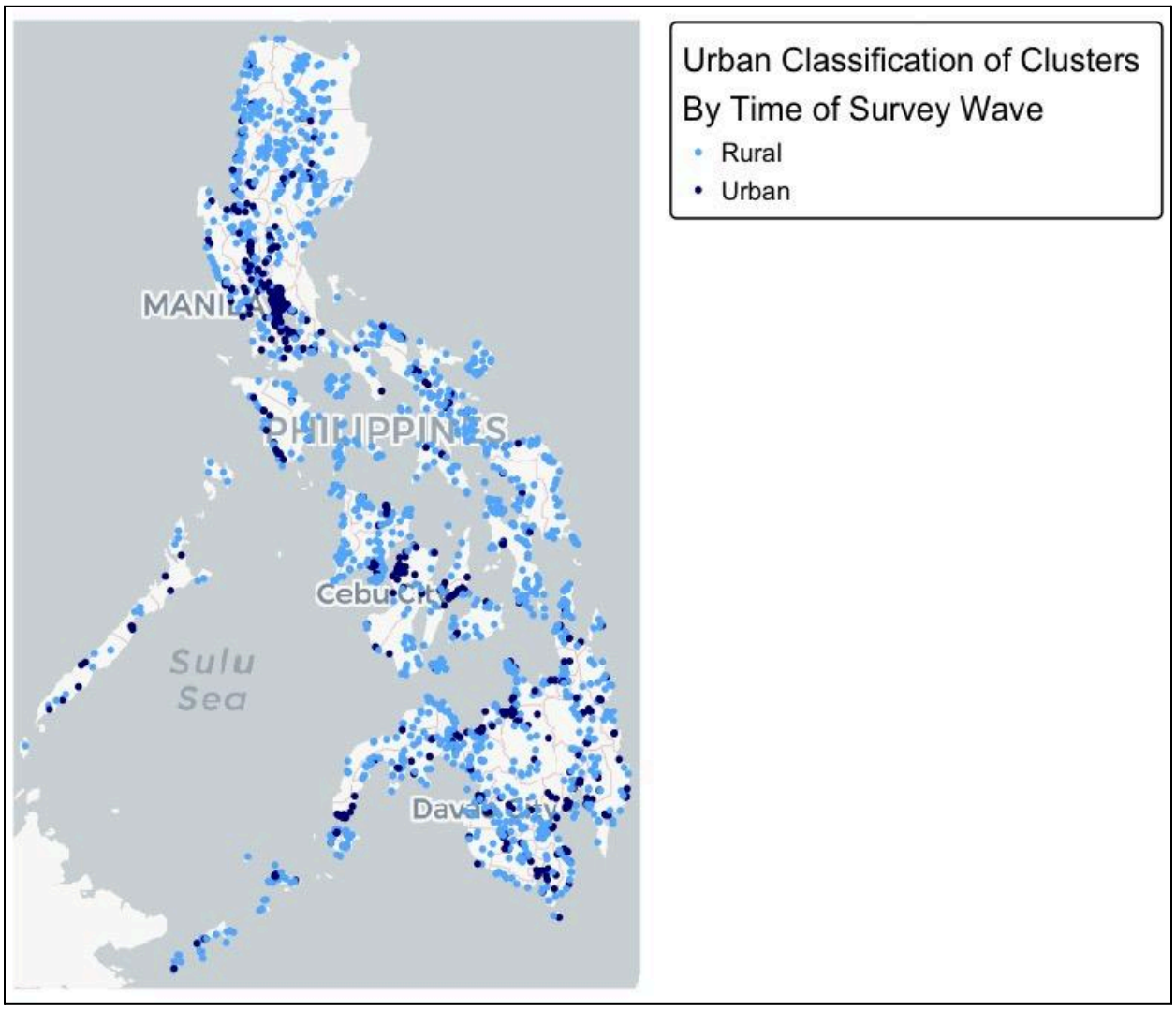


Source: Croft et al. (2023)

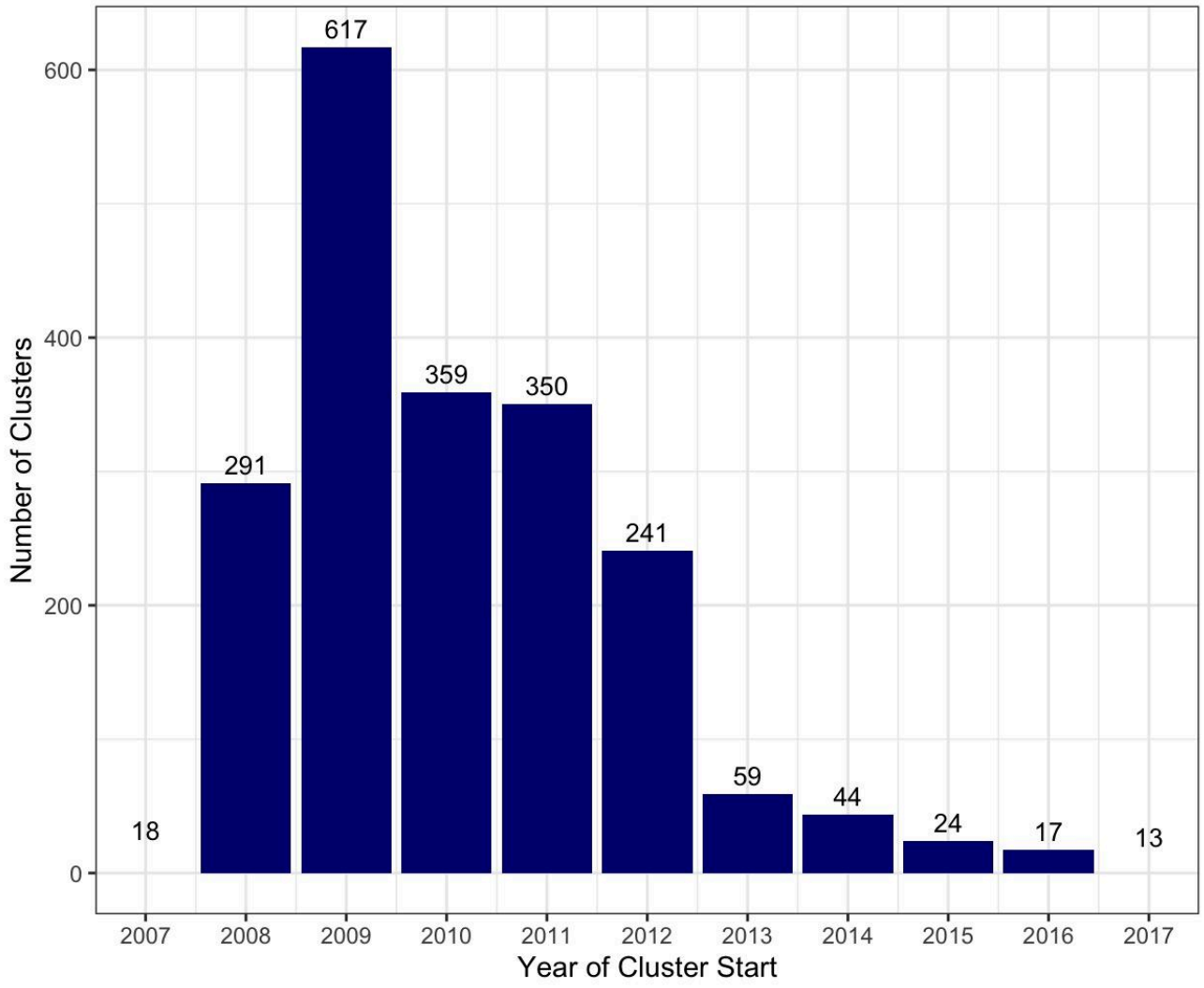
Appendix 1.A.2.: Geographic Distribution of Clusters By Survey Wave



Appendix 1.A.3.: Geographic Distribution of Clusters By Urban Classification



Appendix 1.A.4.: Frequency of Clusters By Starting Year of 4Ps



Appendix 2.B

This appendix shows ATT estimates of 4Ps on infant and child mortality, and preterm rates aggregated by groups like regions and urban classifications. While the ATT estimates may seem insignificant, again, this may be due to statistical power issues. In turn, the estimates presented are preliminary; please avoid overinterpreting them.

Appendix 2.B.1: ATT Estimates of 4Ps on Infant and Child Mortality Rates By Urban Classification

	Child Health Outcomes		
	Infant Mortality	Child Mortality	Preterm Rates
Obs. equally weighted	**-0.010***	-0.001	-0.013
	(0.005)	(0.003)	(0.008)
Rural	**-0.011***	-0.002	-0.014
Urban	-0.009	0.001	-0.010
Predicted Counterfactual	0.029	0.007	0.034
Treated Clusters	2009	2009	2009
Never Treated Clusters	54	54	54
Time Periods	18	18	18
* $p < 0.05$			

Appendix 2.B.2: ATT Estimates of 4Ps on Infant and Child Mortality Rates By Region

	Child Health Outcomes		
	Infant Mortality	Child Mortality	Preterm Rates
Obs. equally weighted	**-0.010***	-0.001	-0.013
	(0.005)	(0.003)	(0.008)
Autonomous Region in Muslim Mindanao	-0.011	-0.004	**-0.021***
Cordillera Administrative Region	-0.011	-0.002	-0.017
National Capital Region	-0.010	0.001	-0.015
Region I × Ilocos	-0.015	-0.001	-0.010
Region II × Cagayan Valley	**-0.024***	-0.004	-0.018
Region III × Central Luzon	-0.008	-0.005	-0.006
Region IVA × CALABARZON	-0.009	0.001	-0.011
Region IVB × MIMAROPA	-0.012	0.001	-0.007
Region IX × Zamboanga	-0.015	-0.005	-0.014
Region V × Bicol	-0.004	0.003	-0.006
Region VI × Western Visayas	-0.007	-0.005	-0.014
Region VII × Centeral Visayas	-0.012	-0.003	**-0.023***
Region VIII × Eastern Visayas	-0.010	-0.002	-0.006
Region X × Northern Mindanao	**-0.017***	0.002	**-0.019***
Region XI × Davao	-0.004	-0.000	-0.012
Region XII × SOCCSKSARGEN	-0.014	-0.000	-0.012
Region XIII × Caraga	-0.006	-0.000	-0.010
Predicted Counterfactual	0.029	0.007	0.034
Treated Clusters	2009	2009	2009
Never Treated Clusters	54	54	54
Time Periods	18	18	18

* $p < 0.05$

Note: There are currently 18 regions in the Philippines. One of the regions was formally established in 2015, but was dissolved during 2017. It was formally reestablished again in 2024. DHS do not have data on this region; the clusters associated with it, however, is under other regions.