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Do Cash Transfer Programs Protect from Poverty in the Case of Aggregate Shocks?

A Study on Typhoon Yolanda in the Philippines

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Abstract

Cash transfer programs are regarded as providing effective protection against poverty and household-specific negative income shocks. Little research has been done on their performance in situations of aggregate negative shocks. This paper assesses the performance of the Philippines' Conditional Cash Transfer Program in the aftermath of typhoon Yolanda in 2013. Using triple difference techniques, it finds that the program effectively protected households affected by the storm from falling into extreme poverty. It had the largest effect on nonfood consumption.

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Do Cash-Transfer Programs Protect from Poverty in the Case of Aggregate Shocks? A Study on Typhoon Yolanda in the Philippines. [*]

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1 Introduction

One of the biggest promises of social protection programs is to help beneficiary households cope better with adverse life events. Cash transfer programs, by providing an income floor, are at least partially designed to prevent a negative transitory shock from throwing a household into permanent destitution. Such poverty traps would ensue if, for example, a household was forced to sell off productive assets or assume debt in order to meet immediate financial needs. The poverty trap could extend to the next generation if the household also had to drastically reduce food consumption or withdraw children from school to cut costs or add income earners. These arguments are often invoked in the discussion on protection against household-specific shocks, such as a catastrophic health expenditure or a crop failure. Before the Covid-19 pandemic, the role of social protection policies in mitigating the effects of aggregate shocks was not widely debated.

The present paper partially fills this knowledge gap by analyzing the role of the Filipino *Pantawid Pamilyang Pilipino Program* (4P) in protecting households from adverse aggregate consumption shocks and the risk of falling into poverty. It does so by looking at the aftermath of typhoon Yolanda (known outside the Philippines as Haiyan). Using triple difference techniques, it shows that being a program beneficiary significantly increased food and non-food consumption and reduced the risk of falling into poverty - particularly at the World Bank's international extreme poverty line of US$1.90. These results are important as they show the potential of cash-transfer programs being effective policy response in times of aggregate adverse events such as the Covid-19 pandemic, natural disasters, or economic crises.

There appears to be a broad agreement that cash-transfer programs can offer effective protection against household-specific negative income shocks. But there has been little systematic research into whether or not this translates into effective protection in the case of large-scale events. There are several reason why this may not be the case, most of which can be thought of as general equilibrium effects. For example, if an entire neighborhood is destroyed and a critical mass of residents decides to leave, a mass exodus may ensue as economic life breaks down and/or public services may be scaled back or cut off completely. Instead of rebuilding their homes with the help of cash transfers, households would lose all their assets. Another example considers the effect on prices: If products of primary necessity become scarce, cash-transfers may further increase their prices while only having a small or negligible effect on consumption levels. This, in turn, may force households to engage in exactly the short-term responses thought to be the root-cause of many poverty traps, such as taking children out of school or liquidating productive assets.

Typhoon Yolanda made land fall in Eastern Samar in the early morning of November 8, 2013. Over the course of the day, it moved westward, making landfall five more times on different islands of the archipelago. According to the National Disaster Risk Reduction and Management Council (NDRRMC 2014), it affected a population of more than 16,000,000, killing 7,362 people (including 1,062 missing), and injuring a further 28,688. It destroyed 489,613 houses, and damaged a further 595,149, leading to total losses of more than US$1.8 billion. Since the precise storm path can be considered quasi-random, this paper combines the degree of exposure to the storm (either as a discrete or continuous treatment) with binary 4P beneficiary status and a before-after comparison. The identifying assumption is that the average double differences in outcomes between the before and after survey rounds and beneficiaries and non-beneficiaries would have been the same for households affected and not-affected by the storm had the storm not happened. The results are then submitted to a number of robustness checks that include placebo treatments (different hypothetical storm paths), different samples, and a direct test for selection into the program. None of these casts any doubts on the study's findings.

This paper consists of six parts: The next one will discuss the existing literature around social protection programs and negative aggregate shocks. This is followed by a brief description of the 4P program. Part four discusses the data and the empirical strategy and part five presents the results. Conclusions are presented in the last section.

2 Existing literature

The deep Covid-19 induced global recession is putting at risk the gains in poverty reduction made over the past three decades. Understanding the ability of already existing social protection programs to prevent a permanent increase in the levels of poverty is thus of obvious importance. Yet, academic research on their performance during and after large aggregate shocks is largely missing. The existing literature focuses mostly on the ability of cash-transfer programs to mitigate household-level shocks. An early study (de Janvry, Finan, Sadoulet & Vakis 2006) employs data from the randomized pilot of Mexico's flagship conditional cash-transfer (CCT) program Progresa which also contains information on a variety of self-reported shocks at the household and village levels. This allows for testing the interaction of the randomly assigned cash-transfer with the prevalence of a shock. It finds that in the control group an unemployment or illness shock for the household head, or a natural disaster, has a large and significant negative effect on schooling. These effects are either much smaller or non-detectable in treatment localities. Interestingly, this effect is not the result of higher labor force participation by school-aged children: While some of these shocks increase the probability of a child working, there is no mitigating effect due to Progresa. A related and more recent study (Adhvaryu, Nyshadham, Molina & Tamayo 2018) looks specifically at the longer term consequences of income shocks. Using the same data source, the authors interact the randomized beneficiary status with the prevalence of a negative rainfall shock at birth. They show that while such shocks have a long-term negative effect on children's school attendance, each year under Progresa mitigated this effect by 0.1 year of schooling. Rainfall shocks, in the form of absolute negative deviations from the mean, have also been used in a recent study on Zambia's Child Grant unconditional cash-transfer program (Asfaw, Carraro, Davis, Handa & Seidenfeld 2017). They find that every millimeter in average negative monthly deviation in rainfall reduces household expenditures on food and non-food items by around 4 percent and calorie consumption by close to 5 percent. The cash transfer offsets these effects by 70-80 percent. It is also found to significantly increase the household dietary diversity score.

Not all the empirical literature shows such unmitigated positive effects, however. One article (Gitter, Manley & Barham 2011) studies the interaction of a drop in coffee prices for households living in coffee producing localities, and the randomized receipt of a cash-transfer program, to find rather mixed effects. Concerned with early child development, it analyzes the effects of conditional cash-transfer programs in Mexico, Honduras, and Nicaragua on height-for-age z-scores. Only for Mexico is the CCT found to be mitigating the coffee price shock. No significant effect is found for Honduras, while for Nicaragua, the CCT program exacerbates the shock's negative effect. The authors speculate that this seemingly counter-intuitive result may be an unintended consequence of the program's conditionality: If households are forced to keep their children in school, they may be deprived of an additional bread winner. While this would be beneficial for the older, school-aged child, it may deprive younger siblings of resources. A somewhat different, yet interesting, intervention was tested in rural Tanzania (Gong, de Walque & Dow 2019). Participants of both sexes were randomly assigned to either a control group or to being offered cash payments of either US$10 or US$20 conditional on testing negative for sexually transmitted diseases (STD) each period before the payment was disbursed. The authors also collected self-declared information on having suffered a negative economic shock defined by a food scarcity outcome. They found that negative shocks increased the risk of contracting STDs for women but not for men. However, the treatment, at either level, did not lessen this effect.

This study will focus on the performance of an existing social protection policy in the context of an aggregate shock. This question must be conceptually separated from the performance of expansions, either vertically (increase in benefits to beneficiaries) or horizontally (inclusion of previously ineligible households), of existing programs. While also in need of more scholarly attention, some results exist on the latter question. The first such study comes from Argentina. In response to its severe economic crisis in 2002, the country's government implemented a cash-transfer/workfare program aimed at families with dependents whose breadwinner had become unemployed because of the crisis. For 80% of beneficiaries, this payment came with a work requirement. While not properly an expansion of a pre-existing program, it still brought adversely affected households under the cover of a social protection policy. The only thorough study of the program (Galasso & Ravallion 2004) shows somewhat mixed results: It failed to properly target the intended beneficiaries, reaching only about one-quarter of eligible families. Moreover, about one-third of benefit recipients were not in the labor force before the crisis. On the other hand, it succeeded in lowering the unemployment rate by 2.5 percentage points. A second study, and the only one to look at a vertical expansion of a pre-existing program, comes from the Fiji Islands after the country was hit by hurricane Winston in February 2016. The vertical expansion consisted of a top-up to three existing social assistance programs, worth around three months of the programs' regular payments. An impact evaluation of this intervention (Ivaschenko, Doyle, KIm, Sibley & Majoka 2020) implements a regression discontinuity design around the observable eligibility threshold for the cash-transfer program on a sample of households in the most affected regions. It finds that beneficiary households recovered significantly faster from the shock compared to non-beneficiary households around the eligibility threshold. The former were 26 percent more likely to have replaced a lost dwelling, and 13 percent more likely to have repaired damaged walls at the time of the survey. The study also finds that most of the funds were spent on necessities such as food and repairs, that female-headed households had a higher recovery rate compared to male-headed ones, and, importantly, that market-access was crucial to a household's recovery. While encouraging, these results show the joint effect of the basic program and top-ups, and not the isolated the effect of the latter.

3 The Pantawid Pamilyang Pilipino Program

The 4P conditional cash transfer program was first introduced in 2008 by the Department of Social Welfare and Development (DSWD). After a very limited pilot in 2007, the program started its geographic rollout in March 2008. Over the course of that year, 4P started operating in 160 municipalities in 33 provinces representing all 17 regions. Geographic eligibility was based on the poorest municipalities in the 20 poorest provinces, the poorest provinces in regions not covered by this criterion, and some pockets of poverty in urban areas. In the first half of 2009, the program was expanded to cover all municipalities with a poverty index of 61.23 percent or more. This added another 28 provinces and 140 municipalities to the area under coverage. In 2010 the threshold for the municipal poverty index was dropped to 57.13 percent and in 2011 to 36.99 percent. In 2011, the program also experienced its biggest expansion by adding over 1.2 million households as beneficiaries as it extended to cover almost all provinces. After a further expansion in 2012 it finally covered all municipalities in 2013. To determine household eligibility, since 2010 it employs the proxy means test based *Listahanan* registry. In addition to being considered poor according to *Listhanan*, eligible households must have a pregnant member or at least one child aged 0-14 or, after 2014, one aged 0-18. This last expansion added another 379,000 households to the roster of beneficiaries. Also, benefits were no longer limited to a maximum of five years. In 2013, the education grant for children attending high school was raised from ₱300 to ₱500.

The program has three components: The already mentioned educational grant of ₱300 per month for ten months for daycare/kindergarten and primary school and ₱500 for high school. These are paid for up to three children and conditional on school attendance. Furthermore, the program pays a health grant of ₱500 per month, based on compliance with health conditions and attendance at monthly Family Development Sessions (FDS). Lastly, a rice subsidy of ₱600 per month, introduced in 2017 and not relevant for the present analysis, is paid if family members comply either with the health or educational conditions. The highest possible subsidy a household can receive, which would correspond to one with three children in high school, amounts to ₱28,200 per year. Health conditions are divided into those applying to pregnant women (consisting of visits to pre-natal and post-natal care facilities and the use of professional delivery services) and those that apply to children (immunization, deworming, and visits to health centers). The educational conditionality consist of 85 percent attendance at the appropriate school level, including daycare/kindergarten, for children aged 3-18. Compliance is monitored jointly by DSWD, the Ministry of Health (for Health conditionalities) and local governments (for participation in the FDS).

In the aftermath of the storm, DSWD waived the program conditionalitites in the affected areas but did not alter the benefits. The agency did provide some in-kind assistance in the affected areas, but this was not linked to being a 4Ps beneficiary. However, the World Food Program delivered two payments of ₱1,300 to affected households between December 2013 and January 2014 using Listahanan and the 4P delivery system. This approach was later replicated by UNICEF for households living in the most affected region, Eastern Samar (Bowen 2015, Bowen 2016). For the purpose of this study, only the expansion in 2014 could potentially interfere with the estimation. The data used were collected in the second halves of 2012 and 2015. When Yolanda made landfall in November 2013, the expansion in that year had largely been implemented. Since the 2014 expansion was solely based on the number of pre-existing household members aged 15-18, it can be expected to be orthogonal to the impact of the typhoon. Nonetheless, at the end of this paper I will present a direct test showing that Yolanda had no direct effect on beneficiary status.

4 Data and empirical strategy

The paucity of research on the question of the performance of social protection systems in response to large aggregate shocks most likely stems from the high demands on the data necessary to produce a state-of-the-art study. Ideally, one would want to have longitudinal household and individual level data that captures information not only on all outcomes of interest, but also on beneficiary status at baseline. In order to have clear treatment and control groups, the aggregate shock must not affect the entire country in equal measure but have considerable variation. This variation, in turn, must not be correlated with unobservable household characteristics- as may be the case with economic crises. Natural disasters are thus a good candidate given that they usually show considerable variation in the degree to which households are exposed to them. Moreover, the precise geographical area of their impact can be considered quasi-random. This, unfortunately, raises the requirements on the data, which now need to have enough density to yield a sample with statistical power in a geographically limited area. This is a requirement that longitudinal surveys with their smaller sample sizes are unlikely to meet. In addition, the data need to be observable within a relatively short time window before and after the shock. Lastly, the data need to allow for the observation of either beneficiary status or eligibility to the program under study.

While this perfect data may not exist , there are several second-best options to address this question. Causal inference is still possible under the imposition of some plausible assumptions on the data generating process. The principal data sources used here are the 2012 and 2015 rounds of the Family Income and Expenditure Survey (FIES). The effects of the typhoon are thus evaluated roughly two years after the event. The FIES constitutes a representative stratified random sample of Filipino households (with the exception of those living in very inaccessible areas, which is the case for less than 0.4% of the population) and is conducted every three years by the Philippine Statistics Authority (PSA). The sample size in each round is about 50,000 households. It is the only survey data source that allows for the estimation of consumption level and poverty status. It has the downside of not capturing much information on individual household socio-economic variables other than income and consumption. The empirical specification discussed below is, therefore, very parsimonious. The Philippines consist of 17 regions, subdivided into 81 provinces which, in turn, are composed of 1,634 cities and municipalities. The lowest geographic level are barangays (villages), of which the country has a total of 42,046. All four levels can be identified in FIES. For the data used in this study 1,142 of the 1,145 municipalities present in the 2012 round were resampled in 2015 (plus an additional five not present in 2015). However, at the barangay level only 1,641 of the 2,397 present in the 2012 round were resampled in 2015 (and 885 new ones added). This has obvious implications for the choice of fixed effects.

Below, results will be presented for a variety of different samples, defined by varying distance to the typhoon path and the exclusion of higher income households. As explained above, in theory only households that have at least one child under age 18 and fall under their respective provincial rural/urban poverty line qualify for the program. But limiting the sample to poor households poses two problems: Firstly, they would be able to keep the benefit for a considerable amount of time even if their income increased significantly. Restricting the sample to observations below this poverty line would thus induce a selection on the outcome variable, leading to biased results. Secondly, targeting is far from perfect as in the years under study only 58%-59% of households below their applicable poverty line receive the benefit, as do 38%-41% of households in the second quintile, 19%-22% in the third, and even 6%-8% in the fourth quintile (Acosta, Avalos & Zapanta 2019). The average income of households with at least one child under the age of 18 is close to ₱50,000, falling into the fourth quintile. On the other hand, not restricting the sample at all runs the risk of including too many high-income households for whom the program would be completely irrelevant. The analysis will start with a main sample of households earning less than ₱50,000 and residing within 200km of the typhoon path (with a binary treatment group defined as living within 100km). Subsequently, results will be extended for different income thresholds and distances in both directions (₱20,000 to ₱80,000 and 20km to 400km).

Table 1 shows summary statistics for the main sample (up to 200km distance from the typhoon path and less than ₱50,000 in household per-capita income). Six different outcomes are of interest. Unfortunately, FIES does not collect direct information on school attendance. To fill this gap, I created a a binary variable with value equal to one if a household reports any expenditure on education and zero otherwise. The analysis for this outcome is restricted to households with at least one school-aged child (5-17 years). Almost 95% of such households report a positive expenditure on education. All other outcomes are computed for households with at least one child under the age of 18. Average per-capita food consumption at ₱12,659 is higher than non-food consumption (₱9,292) and also has a lower standard deviation. While only 14% of households in this sample are extremely poor (at the US$1.90 poverty line), 40.59% are at their respective provincial poverty line and 49.35% at the poverty line for lower middle income countries (US$3.20).

To assess whether the 4P helped limit the damage suffered from Yolanda, results will be presented for a triple differenced specification. The three dimensions are: Before vs. after, being (more) affected by the typhoon vs. being less/not affected, and being a program beneficiary vs. being a non-beneficiary. The data contain self-reported beneficiary status in the 4Ps, which is received by 40.74% of households. Not shown in the table, but nonetheless of interest, this proportion is 52.83% for households with incomes lower than ₱20,000, 31.93% for those with incomes between ₱20,000 and ₱50,000, and still 8.22% for those between ₱50,000 and ₱80,000. Again, showing the imperfect targeting of the 4Ps. The other two dimensions used in the triple distance specification are both well-balanced. The average distance to the storm path is 94.53km and 55.36% of observations fall inside the 100km band. Moreover, 48.39% of the observations come from the 2015 round.

In formal terms, the principal regression specification is:

$$Y_{i,m,j,t} = \beta_0+\beta_1 B_{i,m,j}*T_{m,j}*A_t+\beta_2 B_{i,m,j}*T_{m,j}+\beta_3 B_{i,m,j}*A_t+\beta_4 T_{m,j}*A_t+\beta_5 B_{i,m,j}+\beta_6 T_{m,j}+\beta_7 A_t+\theta_{i,m}+\varepsilon_{i,m,j,t} \tag{1}$$

where $Y_{i,m,j,t}$ is the outcome of interest for observation (household or individual) i, living in municipality m and barangay j in time period t (before or after the event). $B_{i,m,j}$ is a binary variable capturing beneficiary status; $T_{m,j}$ (treatment) is either a binary variable equal to one if the district or municipality has been affected by the natural disaster (i.e. falls within the defined treatment band which is 100km for the main specification) or measures the continuous distance to the storm path. Next, A_t is a binary variable equal to one in the time period after the event. Lastly, $\theta_{i,m}$ denote a set of municipality and beneficiary status specific fixed effects. That is one set of municipality-specific fixed effects are included for beneficiaries and another one for non-beneficiaries. The municipal level (as opposed to the barangay level) is chosen because of the small number of observations in each barangay discussed above. The idiosyncratic error term $\varepsilon_{i,m,j,t}$ is always clustered at the Barangay-level.

The map in figure 1 gives a visual impression of the geographic extent of the analysis. The solid line in the center shows the actual typhoon path. The differently shaded areas correspond to the main specification with a 100km band treatment group and the control groups to the north and south. Note that the extent of all the shaded areas also corresponds to the sample used in the continuous treatment specification.

The parameter of interest, which captures the differential effect of the program, is β_1. The last three parameters (β_5-β_7) capture all time-invariant differences between beneficiaries and non-beneficiaries, all time-invariant differences between affected and less-affected geographical areas, and any common time trends between the two survey rounds. The double interaction terms allow beneficiaries and non-beneficiaries to have different time-invariant characteristics in i) affected and less-affected areas, and ii) to have different time trends. The last double interaction term also allows affected geographical areas to have different independent time-trends than less-affected ones. The identification assumption to give β_1 a causal interpretation as an average treatment effect on the treated (ATE) is that the difference in the outcome between beneficiaries and non-beneficiaries would have evolved on average in parallel in affected and less-affected barangays in the absence of the calamity, allowing for independent time trends for beneficiaries and non-beneficiaries in each municipality.

5 Results

Results will be presented for the discrete and continuous distance treatments, followed by a visual presentation of the results when the threshold value for the former is moved from 10km distance to the typhoon path to 200km in increments of 10km. With these results firmly established, robustness checks will look at placebo paths for the typhoon, at different income cutoffs for the sample used, and at whether exposure to the typhoon had a causal effect on beneficiary status.

5.1 Main results

Tables 2 and 3 show the study's principal results for households living within a 200km wide band on either side of the typhoon path. For the discrete distance treatment, presented in table 2, this implies that households within 100km from the path are considered treated, whereas those living at a 100-200km distance act as the control group. The 100km cutoff was chosen for its virtue of being a round value. As will become clear below, the largest effects are found at cutoffs between 100-150km, so the results presented here can be thought of as conservative estimates. Moreover, results are presented for all six outcomes of interest with and without the inclusion of municipal-beneficiary fixed effects. The aim is to show that their omission does not change the results in any qualitatively important manner. The upshot is that the results are not an artifact of unobserved characteristics at the municipal level or between beneficiaries and non-beneficiaries within municipalities. Also note that the continuous treatment in table 3 is measured as distance to the typhoon path. Therefore, estimates have the opposite sign compared to the discrete treatment in table 2

The results indicate at best a very tenuous effect on households with at least one school-aged child having positive educational expenditures. Only for the discrete treatment is the effect statistically significant at the 10-percent level. The implied effect of the 4Ps is estimated to increase this probability by 3.4 percentage points. The effect on food consumption is also only significant at the 10-percent level for both treatments and independent of the inclusion of fixed effects. The estimated effect of the program is to raise food consumption by ₱816 for households living within 100km of the storm's path, whereas for the continuous treatment it is estimated that for each kilometer of proximity to the path, the program raised it by over ₱6. The estimated effects on non-food consumption are decidedly more significant, in statistical as well as economic terms. For the discrete treatment non-food consumption is increased by ₱1,350, significant at the 1-percent level. For the continuous treatment each kilometer of proximity raises it by over ₱9, significant at the 5-percent level.

The remaining six columns in each table show the implications for poverty. As explained above, not all beneficiaries fall necessarily underneath one of the different poverty lines. If the identified increases in consumption accrued mainly to the relatively better-off, there might be no effect on poverty rates. The risk of falling into extreme poverty is found to have been reduced by a statistically significant (at the 5 percent level) 8 percentage points for the discrete treatment. For the continuous treatment, the implied effect is a reduction of approximately 0.1 percentage point for each kilometer of proximity. The last effect is only statistically significant at the 10-percent level after fixed effects are included. The point estimates for the provincial poverty lines are of similar magnitude, 7.8 percentage points for the discrete and 0.1 per kilometer for the continuous treatment, but the levels of statistical significance are inverted. At the higher US$3.20 poverty line, which would correspond to the World Bank's general poverty line for lower middle income countries, no statistically significant effects can be found.

The 100km distance cutoff for the discrete treatment is of course somewhat arbitrary and was chosen mainly by virtue of being a round value. It is thus worthwhile to inspect how the effects change as the cutoff is varied. A visual impression of these regressions is given in figure 2. The distance defining the treatment group is increased from 10 to 200 kilometers in 10 kilometer increments. The corresponding control group always consists of households between the cutoff and twice that distance to the storm path. At very low cutoff points, one would not expect to find any significant effects as all households in close proximity are likely to be affected. On the other extreme, as the cutoff distances grow too far, many unaffected households will start forming part of the treatment group, drawing the estimated effect towards zero. The figure shows that this is indeed the case and that the 100km cutoff provides very conservative estimates, being the lower bound in the range of results that are statistically significant at the 5-percent level. Statistically significant effects are found, roughly, for a treatment group in a range of 100-150km from the typhoon path. The strongest results, in terms of significance and range, are for non-food consumption and poverty at the US$ 1.90 level. But for treatment group distances between 110-140km, the results are statistically significant for all poverty lines and also for food consumption. Only for positive educational expenditure outcome results are mostly statistically insignificant at the 5-percent level, except at 110km distance.

5.2 Robustness checks

The results just presented showed statistically significant results for consumption and poverty outcomes of the 4Ps for households living within 100km of the path of typhoon Yolanda. It was also shown that these results are conservative estimates and that at treatment group distances of 110-140km the effects would be larger. A number of concerns about the estimates presented remain: The first one being that the estimates may be spurious, and the results would be the same in the absence of Yolanda. The second one is that the restriction to household with incomes less than ₱50,000 biases the results in a favorable direction. Lastly, the estimates may be biased by selection into treatment. That is, that exposure to the storm itself may have increased the likelihood of becoming a 4P beneficiary.

The concern that the results may be spurious can be addressed by estimating a series of placebo models that use different hypothetical typhoon paths. The results in table 4 show the point estimates on the triple interaction term from the specification including fixed effects from tables 2 and 3 for the placebo path running parallel at 300km and 400km north and south to the actual path. Figure 1 shows why one cannot use placebo paths below 300km distance since the resulting samples include observations in a 200km band on either side. With an assumed cutoff of the treatment effect at 100km distance, any placebo path at less than 300km distance from the actual one would result in the inclusion of treated observations in the control group. For example, with a placebo path at only 200km distance approximately the control group on the side of the actual path would consist of observations in its 0-100km treatment group. Given that the strongest effects are detected at 110-140km cutoff, some contamination can still be expected to occur at 300km. The results around the placebo paths are almost all statistically insignificant. Only two point estimates (one for the discrete and one for the continuous treatment) are statistically significant at the 5 percent level. They can be safely considered to be random outcomes since the table shows a total of 48 different point estimates-i.e. two of them would be expected to be statistically significant. Moreover, the results on spending do not translate into effects for the poverty rates, nor are they consistent between the discrete and continuous treatments.

In table 5, the estimations stick to the original 100km cutoff around the actual path but the income thresholds for inclusion into the sample are varied. Included households range from those with incomes less than ₱20,000 to those with incomes less than ₱80,000. Since the binary outcome on educational expenditures is observed for smaller samples than those for the other outcomes, and since sample sizes vary with the changes in the income threshold, the table presents results for this outcome separately at the top of the table. Further down, results for the other outcomes are shown grouped by type of treatment. The results for the consumption outcomes are very stable with only small differences in the parameter estimates. The effects on poverty at the US$1.90 level are also very constant across different income cutoffs. For the provincial poverty lines and the international one at US$3.20 estimates a much lower at cutoffs below ₱50,000. This is the result of almost all households in these samples being considered poor at these poverty lines. For those below ₱20,000 in income, 90.82 percent fall below their respective provincial poverty line and 99.4 percent are poor using the international US$3.20 one. Only 33.3 percent fall under the US$1.90 line. For households with less than ₱30,000 in income, these numbers are still 55.72 percent, 67.75percent, and 19.27 percent, respectively.

One caveat with the analysis is that it cannot be established whether or not a household would be program eligible in the absence of the typhoon. One can, however, directly test for whether or not the typhoon had any effect on beneficiary status with a standard difference-in-differences (DID) framework: A binary dependent variable that is coded equal to one if the household is a beneficiary and zero otherwise is regressed on the distance treatment (either binary or continuous), a binary variable for the 2015 survey round, and the interaction effect between the two. Under standard parallel trends assumptions, the parameter estimate on the interaction term could be interpreted as a causal estimate of the typhoon on the likelihood of being a beneficiary. Results for this exercise are shown in table 6 for the 100km distance cutoff, discrete and continuous treatments, and inclusion and exclusion of fixed effects. None of the four point estimates on the interaction term comes close to being statistically significant. There is hence no indication that the typhoon itself caused households to become beneficiaries. The significant point estimate on the distance variables vanishes once fixed effects are controlled for. The positive effect of the 2015 round merely shows the secular increase in coverage across the country.

6 Discussion and conclusions

Cash-transfer programs have been shown to be an effective way to reduce poverty and protect vulnerable households against idiosyncratic income shocks. For this reason, they are also often proposed as effective and efficient protection mechanisms in the case of large-scale adverse events such as natural disasters, pandemics, or economic crises. To this end, they became a favored policy tool during the Covid-19 pandemic. However, there has been very little research on the performance of cash-transfer programs (or other social protection schemes) in such settings. The results in this paper partially fill this gap by showing that in the aftermath of typhoon Yolanda in the Philippines the country's 4P program proved to an effective protection against extreme poverty. It was also shown that it raised in particular non-food consumption.

These results are important beyond the Philippine context. They show that even a moderate cash-transfer can significantly protect vulnerable populations when faced with a large aggregate shock. The upshot is that such programs are indeed an effective policy response in times of crisis. However, many questions remain to be answered. The results presented here should, of course, be corroborated in other settings. Moreover, it would be important to understand how cash-transfer programs can be temporarily expanded to increase their impact. Given that aggregate shocks are likely to affect many households that were previously not deemed vulnerable, the role of horizontal expansion (i.e., expanding the number of households covered) is of particular interest. The crucial question in this context is whether an additional dollar spent on either horizontal or vertical (i.e., increasing the amount of the benefit paid out) expansion has the largest effect on poverty reduction. Understanding how this trade-off depends on the nature of the shock (e.g., a pandemic vs. an economic depression caused by a financial crisis) is also of first-order importance.

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Table 1: Descriptive statistics

	Obs.	Mean	Std. Dev.	Min.	Max.
Outcome variables:					
Educational expenditure >0	8,763	0.9478	0.2223	0	1
Food consumption per capita	9,717	12659	5915	2086	206891
Non-food consumption per capita	9,717	9392	6397	791	64999
Poverty US$ 1.90	9,717	0.1404	0.3474	0	1
Poverty provincial	9,717	0.4059	0.4911	0	1
Poverty US$ 3.20	9,717	0.4935	0.5000	0	1
Treatment variables:					
Receives 4P	9,717	0.4074	0.4914	0	1
Distance 100km	9,717	0.5536	0.4971	0	1
Distance	9,717	94.53	58.16	0	200
After	9,717	0.4839	0.4998	0	1
Receives 4P*After	9,717	0.2291	0.4203	0	1
Distance 100km*Receives 4P	9,717	0.2014	0.4011	0	1
Distance 100km*After	9,717	0.2692	0.4436	0	1
Distance 100km*Receives 4P*After	9,717	0.1147	0.3187	0	1
Distance*Receives 4P	9,717	40.78	62.43	0	200
Distance*After	9,717	22.78	50.77	0	200
Distance*Receives 4P*After	9,717	45.55	62.20	0	200

Table 2: Results for discrete distance treatment.

	Education		Consumption				Poverty					
	Expenditure >0		Food		Non-Food		US$ 1.90		Provincial		US$ 3.20	
Distance 100km*Receives 4P*After	0.034*	0.035*	872.824*	816.301*	1,457.048***	1,350.009***	-0.072**	-0.080**	-0.074	-0.078*	-0.063	-0.063
	0.019	0.019	500.106	446.696	516.524	494.825	0.035	0.031	0.046	0.043	0.044	0.041
Receives 4P*After	-0.024	-0.021	-391.723	-419.146	-426.462	-368.136	-0.036	-0.028	-0.038	-0.030	-0.064**	-0.065**
	0.015	0.014	393.970	349.473	372.456	356.007	0.026	0.023	0.034	0.032	0.033	0.031
Distance 100km*Receives 4P	-0.019	0.034	-839.201***	-723.772	-919.170**	530.104	0.056*	0.041	0.057*	0.034	0.054*	-0.008
	0.013	0.046	324.230	1,105.283	359.658	1,183.008	0.029	0.068	0.034	0.101	0.032	0.092
Distance 100km*After	-0.038**	-0.035**	-186.655	-195.512	-425.534	-534.815	-0.007	-0.002	-0.039	-0.028	-0.043	-0.044
	0.016	0.016	444.716	377.418	455.400	426.525	0.020	0.017	0.030	0.028	0.031	0.029
Distance 100km	0.029***	-0.060	355.762	70.866	824.213**	-308.870	-0.025	0.032	-0.031	-0.013	-0.032	0.038
	0.011	0.038	268.404	857.020	330.375	939.835	0.016	0.036	0.022	0.068	0.024	0.074
Receives 4P	0.059***	0.033	-1,606.252***	-1,119.346	-3,220.897***	-4,196.777***	0.107***	0.002	0.236***	-0.186*	0.260***	0.408
	0.011	0.024	226.492	813.166	257.855	750.945	0.020	0.085	0.025	0.099	0.024	0.444
After	0.027**	0.023*	1,848.523***	1,900.669***	2,203.237***	2,228.174***	-0.039**	-0.037***	-0.023	-0.024	-0.060**	-0.057**
	0.012	0.012	368.709	307.456	341.594	321.480	0.016	0.014	0.024	0.022	0.024	0.023
Municipal FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	8,763	8,763	9,717	9,717	9,717	9,717	9,717	9,717	9,717	9,717	9,717	9,717
R-squared	0.012	0.093	0.045	0.222	0.094	0.226	0.060	0.174	0.032	0.208	0.070	0.212

Notes: Results show bias-corrected estimates for discontinuity using local linear regression; ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Robust p-values in parentheses

Table 3: Results for continuous distance treatment.

	Education		Consumption				Poverty					
	Expenditure >0		Food		Non-Food		US$ 1.90		Provincial		US$ 3.20	
Distance*Receives 4P*After	-0.000	-0.000	-7.733*	-6.570*	-10.063**	-9.153**	0.001**	0.001*	0.001*	0.001**	0.000	0.000
	0.000	0.000	4.018	3.629	4.277	4.138	0.000	0.000	0.000	0.000	0.000	0.000
Receives 4P*After	0.003	0.006	813.416*	666.160*	1,313.079***	1,249.187***	-0.130***	-0.134***	-0.140***	-0.130***	-0.147***	-0.146***
	0.017	0.016	421.769	378.839	486.549	474.486	0.031	0.039	0.042	0.029	0.042	0.039
Distance*Receives 4P	0.000	-0.000	2.381	-12.384	0.867	-40.065	-0.000	-0.001	-0.000	-0.002	-0.000	-0.001
	0.000	0.001	2.765	40.471	2.884	39.644	0.000	0.004	0.000	0.002	0.000	0.003
Distance*After	0.000	0.000	0.924	0.002	1.912	2.126	0.000	0.001**	0.001**	0.000	0.001**	0.001***
	0.000	0.000	3.550	3.055	3.927	3.637	0.000	0.000	0.000	0.000	0.000	0.000
Distance	-0.000*	0.000	0.417	-11.191	-1.202	4.716	0.000	0.002	-0.000	0.001	-0.000	0.003
	0.000	0.001	2.221	37.933	2.712	45.366	0.000	0.003	0.000	0.002	0.000	0.003
Receives 4P	0.043***	0.030	-2,293.689***	-1,753.119	-3,825.830***	251.438	0.161***	0.133	0.285***	0.410	0.304***	0.794
	0.011	0.254	318.075	6,943.880	325.803	7,975.467	0.026	0.577	0.032	0.312	0.030	0.638
After	-0.012	-0.012	1,661.175***	1,784.089***	1,788.804***	1,715.561***	-0.052***	-0.092***	-0.100***	-0.044***	-0.148***	-0.150***
	0.014	0.013	335.222	286.646	419.577	386.377	0.016	0.024	0.027	0.014	0.030	0.027
Municipal FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	8,763	8,763	9,717	9,717	9,717	9,717	9,717	9,717	9,717	9,717	9,717	9,717
R-squared	0.011	0.092	0.045	0.222	0.092	0.226	0.061	0.209	0.032	0.174	0.072	0.213

Notes: Results show bias-corrected estimates for discontinuity using local linear regression; ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Robust p-values in parentheses

Table 4: Results for Placebo Paths 300km and 400km North and South of Actual Path.

	Education	Consumption			Poverty	
	Expenditure >0	Food	Non-Food	US$ 1.90	Provincial	US$ 3.20
Discrete Treatment:						
300km North	0.017	595.029	72.149	-0.016	0.014	0.020
	0.019	394.106	447.638	0.031	0.044	0.043
300km South	-0.016	14.705	523.584	0.021	-0.042	-0.005
	0.021	431.253	402.960	0.030	0.036	0.034
400km North	0.000	657.859	501.219	0.001	-0.044	-0.009
	0.024	437.244	515.333	0.029	0.044	0.048
400km South	0.003	-41.711	915.876**	0.048	0.001	-0.015
	0.022	414.883	416.021	0.034	0.038	0.035
Continuous Treatment:						
300km North	-0.000	-1.214	-1.603	-0.000	-0.000	-0.000
	0.000	2.930	2.875	0.000	0.000	0.000
300km South	0.000	7.656**	-1.598	0.000	0.000*	-0.000
	0.000	3.551	2.463	0.000	0.000	0.000
400km North	0.000	-1.284	1.401	-0.000	-0.000	-0.000
	0.000	3.848	3.914	0.000	0.000	0.000
400km South	-0.000*	2.582	3.313	0.000	0.000	0.000
	0.000	2.948	2.709	0.000	0.000	0.000

Notes: Results show bias-corrected estimates for discontinuity using local linear regression; ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Robust p-values in parentheses

Table 5: Results for income cutoff ₱20,000-80,000.

	Income thesholds in ₱						
	20,000	30,000	40,000	50,000	60,000	70,000	80,000
Education Expensditure >0:							
Discrete	0.016	0.010	0.026	0.035*	0.034*	0.030*	0.030*
	0.036	0.023	0.020	0.019	0.018	0.017	0.017
Continuous	0.000	0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	3,789	6,468	7,941	8,763	9,266	9,592	9,848
Discrete:							
Food Condsumption	763.867*	505.868	787.923*	816.301*	710.086	753.350	651.472
	441.979	492.196	447.895	446.696	463.997	476.021	474.358
Non-food Consumption	967.184***	413.246	810.711**	1,350.009***	1,221.364**	1,211.933**	936.832
	365.031	351.325	404.625	494.825	532.979	589.675	640.179
Poverty \$1.90	-0.130**	-0.074**	-0.074**	-0.080**	-0.082***	-0.082***	-0.080***
	0.065	0.038	0.033	0.031	0.030	0.030	0.030
Poverty Provincial	-0.008	-0.048	-0.063	-0.078*	-0.083**	-0.086**	-0.082**
	0.037	0.050	0.045	0.043	0.041	0.041	0.040
Poverty \$3.20	-0.000	-0.018	-0.047	-0.063	-0.070*	-0.073*	-0.070*
	0.010	0.047	0.043	0.041	0.040	0.039	0.039
Continuous:							
Food Condsumption	-6.737*	-6.064	-7.668**	-6.570*	-6.004	-6.441*	-6.371*
	3.753	4.380	3.767	3.629	3.687	3.779	3.777
Non-food Consumption	-6.525**	-3.280	-6.606*	-9.153**	-8.189*	-8.197	-6.492
	3.013	3.019	3.403	4.138	4.449	4.989	5.493
Poverty \$1.90	0.001*	0.001**	0.001**	0.001**	0.001**	0.001***	0.001**
	0.001	0.000	0.000	0.000	0.000	0.000	0.000
Poverty Provincial	-0.000	0.000	0.001	0.001*	0.001*	0.001**	0.001**
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Poverty \$3.20	-0.000	0.000	0.000	0.000	0.001	0.001	0.001
	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Observations	4,096	7,078	8,786	9,717	10,298	10,681	10,982

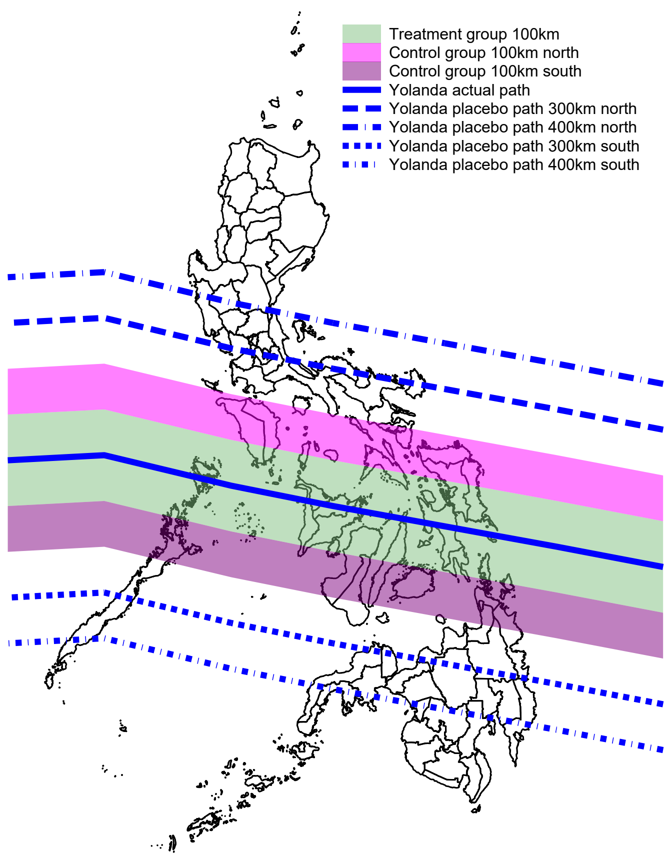
Notes: Results show bias-corrected estimates for discontinuity using local linear regression; ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Robust p-values in parentheses

Table 6: Selection into Treatment.

	Discrete		Continuous	
Distance*After	-0.015	-0.006	0.000	0.000
	0.020	0.019	0.000	0.000
Distance	-0.091***	-0.009	0.001***	0.002
	0.014	0.051	0.000	0.002
After	0.137***	0.128***	0.119***	0.118***
	0.015	0.014	0.019	0.018
Municipal FE	No	Yes	No	Yes

Notes: Results show bias-corrected estimates for discontinuity using local linear regression; ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Robust p-values in parentheses

Figure 1: Map of Yolanda path, treatment and control groups, and placebo paths.



Notes:The map gives a visual impression of the areas covered by the treatment and control groups for the discrete analysis shown in table 2. For the continuous treatment in table 3, the sample consists of all the shaded areas combined. The four placebo paths are analyzed in table 4

Figure 2: Treatment effects for expanding affected area from 10 to 200km.