Can Cash Transfers Prevent Suicides? Experimental Evidence From Indonesia*

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Abstract

We ask how income shocks affect suicides, by using novel Indonesian village-level census data, and two identification strategies involving an Indonesian cash transfer scheme. First, we exploit the population-wide roll-out of the cash transfer program, and second, we use a randomized experiment of the same program. We find evidence from both strategies that the cash transfer significantly decreases the probability of suicide within a sub-district. Our evidence supports the view that economic circumstances are an important motive for suicides, and that social welfare programs can effectively improve mental health among the ultra-poor in developing countries.

Keywords: Conditional Cash Transfers, Mental Health, Well-Being, Suicides, Experiment.

JEL classification: D12, C21, I38.

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1 Introduction

Suicide is a pressing public health concern, causing 800,000 deaths per year, and is the second leading cause of death among 15-29 year olds worldwide (WHO, 2014). A rich social science literature, dating back to at least Émile Durkheim (1897), attempts to explain causes behind suicide. More recent economic theory and evidence suggests a role for intertemporal utility maximization in such self-inflicted harm (Becker and Posner, 2004; Campanello et al., 2015; Hamermesh and Soss, 1974). Understanding whether economic motives play a role in people's decision to commit suicides is therefore important. However, little causal evidence exists on suicide, especially in developing countries, where 75% of suicides occur (Inagaki, 2010).

Suicides are linked to the broader problem of mental illness, which has important economic consequences. Mental illness might create a poverty trap (Haushofer, 2011; Patel and Kleinman, 2003) by reducing a person's educational attainment (Currie and Stabile, 2006; Fletcher and Wolfe, 2008; Kessler et al., 1995; Zivin et al., 2009). Hawton et al. (2013) claim that specific mental health problems, like depression, are mainly responsible for the decision to commit suicides. It is therefore worth determining whether suicides have economic causes, or can be reduced through policy.

However, it is difficult to assess the impact of government policies on suicides for at least three reasons. First, and most importantly, it is challenging to find rich data that allow for sufficient statistical power[2] – indeed, little microdata on suicides actually exist, particularly in developing countries.[3] Second, the timing and geographic placement of large scale government programs is usually endogenous. Finally, even if a small-scale program demonstrates causality, external validity concerns predominate (Allcott, 2015; Deaton, 2010).

Our dataset and empirical setting surmount these difficulties. We focus on Indonesia, a developing nation and the world's fourth most populous country. In particular, we leverage unique Indonesian village-level census data and use two identification strategies. First, we exploit the roll-out of a conditional cash transfer, Program Keluarga Harapan (PKH), across all of Indonesia, using a difference-in-differences strategy. The transfer provides households with a 12% income increase over six years. The program's introduction reduces suicide probability by 7.2 - 9.4% within a subdistrict. These effects are driven by subdistricts receiving the program for a longer time period, pointing to the importance of long term effects.

[2] In addition, it is difficult to find such data at low levels of geographic identification.

[3] In our quasi-experimental analysis using all Indonesian subdistricts, we have a power of .8 to detect effect sizes of approximately .12 of a standard deviation.

Second, we study a subsample of this population that was allocated the cash transfer at random. The program's randomized introduction decreases suicide probability by about 13.1 - 16.8% within a subdistrict. We also conduct several robustness checks, showing that the common trend assumption is not violated and that our results hold for different definitions of suicide (number of villages with a suicide, and a proxy for the suicide rate).

Turning to mechanisms, we find no evidence that the program's effects on suicides operate through increases in social capital and local public goods, or decreases in crime and violence.[4] We find no significant heterogeneous treatment effects that would support explanations, such as initial levels of poverty or initial availability of health institutions. Our results are consistent with previous work on the relationship between exogenous poverty reductions and improved mental health - like Kling et al. (2007), who emphasize that stress reduction is an important method of improving mental health.

The cash transfer is tied to health and education services. To further rule out the possibility that these shocks are operating through public services, and to provide additional causal evidence on the role of economic circumstances, we exploit agricultural productivity shocks, as proxied by rainfall. We show that decreases in rainfall increase the incidence of suicide in Indonesian subdistricts. This provides further credibility to our claim that a substantial number of the developing world's suicides are motivated by economic hardship.

The seminal Hamermesh and Soss (1974) framework gives credence to our results. This framework views suicides as a rational utility maximization. It posits that "given the materialistic basis of modern society, it is reasonable to expect that variations in the suicide rate will be related to economic variables in ways predictable by economic theory" (p.16).[5] In this paper, we confirm this proposition, by showing that economic shocks affect people's decision to commit suicides.

Our paper also contributes to the literature on poverty, income shocks, and mental health, the latter of which is usually measured by self-reported scales (Adhvaryu et al., 2014; Apouey and Clark, 2015; Blattman et al., 2015b; Cesarini et al., 2015; Colantone et al., 2015; Das et al., 2007; Dorsett and Oswald, 2014; Farré et al., 2015; Friedman and Thomas, 2009; Gardner and Oswald, 2007; Haushofer and Fehr, 2014; Kahneman and Deaton, 2010; Kuhn et al., 2011; Persson and

[4]We cannot directly test whether poverty directly affects mental health through higher stress levels (Chemin et al., 2013; Haushofer and Fehr, 2014) and social isolation (Lund et al., 2011).

[5]More recently, De Quidt and Haushofer (2016) suggest a theoretical framework in which depression can be explained by negative shocks affecting people's beliefs about the future returns to effort. Such a framework could also explain the patterns in our data. If the positive economic shock alters people's beliefs about their future returns that may lower the probability of people commiting suicide.

Rossin-Slater, 2016; Stevenson and Wolfers, 2008; Stillman et al., 2009). In particular, there is a series of randomized cash transfers examining mental health (Baird et al., 2013; Haushofer and Shapiro, 2016; Paxson and Schady, 2007)

Our paper is more closely related to Baird et al. (2013) who randomly assign both unconditional and conditional cash transfers to show large mental health increases among recipients, using self-reported well-being measures. They find large and significant short-and medium term effects of their interventions.

Haushofer and Shapiro (2016) show that providing individuals with large unconditional cash transfers results in short-term increases of their self-reported well-being. The authors also collect data on a noisily measured bio-marker of stress, cortisol, in order to circumvent self-reporting problems. They do not find any main effects of the unconditional cash transfers on the biomarker cortisol.

Our paper is also related to a small but growing literature on how economic and social circumstances affect suicides (Becker and Posner, 2004; Becker and Woessmann, 2015; Campanello et al., 2015; Chen et al., 2012; Cutler et al., 2001; Daly et al., 2011, 2013; Hamermesh and Soss, 1974; Ludwig et al., 2009; Reeves et al., 2012; Stevenson and Wolfers, 2006). For example, Hebous and Klonner (2014) examine how rainfall shocks affect farmer suicides in India. They analyze rainfall shocks in two states and sixty-two districts and find that in one of the two states, a lack of rainfall increases suicides among male farmers, but decreases female suicides. They find no similar effects in the other state's district.

We believe that our study advances the literature on the effect of social welfare programs and mental health in three unique ways. First, we make use of a behavioral measure of mental health that does not suffer from any reporting biases, such as social desirability bias (Rosenthal, 1966) or experimenter demand effects (Zizzo, 2010). Second, we use both a randomized experiment, as well as an additional identification strategy using the entire population of Indonesian subdistricts. This in turn allows us to address any external validity concerns (Deaton, 2010) and enables us to assess the cash transfer's aggregate effects on eligibles and ineligibles. Finally, we also show that our results generalize to a different kind of economic shock: a negative agricultural productivity shock.

Our evidence supports the view that cash transfer programs' positive effects on recipients' mental health (Haushofer and Shapiro, 2016) outweigh by far possible negative spillovers of such programs (Baird et al., 2013). Our results also emphasize the importance of absolute rather than relative poverty as a determinant of suicides, which stands in contrast to previous evidence from the US (Daly et al., 2013).

We proceed as follows. In section 2, we describe the data and the experiment. In section 3, we present our identification strategy, describe our results and examine heterogeneous treatment effects. In Section 4, we present results using agricultural productivity shocks, as proxied by rainfall. Finally, we conclude in section 5.

2 Data

2.1 The Conditional Cash Transfer Program

We use the Program Keluarga Harapan (PKH) conditional cash transfer program (Alatas, 2011; Banerjee et al., 2015; Roth, 2015). PKH's aim was to improve poor households' health and education through a cash transfer, conditional on households' participation in health and education services (Alatas, 2011). In particular, PKH delivers cash transfers to poor households only if a certain set of conditions is satisfied: these include basic health provision for pregnant women, women with newborns, or families with school-aged children. The intervention's size is substantial: households received between 60 and 220 US dollars per year. Average yearly payments to households are 130 US dollars, or about 12% of pre-PKH yearly household expenditure. The households are intended to be part of the PKH program for up to six years. More than 70% of PKH subdistricts are urban or semi-urban.

In 2007, PKH was launched in five provinces: West Java, East Java, North Sulawesi, Gorontalo and East Nusa Tenggara (NTT). The PKH program was then implemented in an additional two regions: West Sumatra and DKI Jakarta. These seven provinces, which were part of the initial pilot of the PKH program, were selected based on their representativeness of Indonesia's diversity and their willingness to participate in the study (Alatas, 2011).

We use a subsample of subdistricts in which the treatment status was randomly assigned. These subdistricts fulfilled certain selection criteria, notably supply-side and poverty criteria. Because the program's focus is on poverty alleviation, upper income quintile districts were excluded from PKH eligibility, based on an index considering poverty rates, malnutrition and schooling records. Of eligible regions, only those with sufficient health and education service institutions were chosen. Then, out of the list of 360 eligible sub-districts, 180 were randomly selected for treatment and 180 were chosen to be the control group.

Political pressures and a consequent unexpected program expansion in East Java resulted in deviations of the realized allocation from the intended one. In particular, 37 out of the 360 sub-districts that were supposed to be part of the control group received PKH funds. Moreover, for a very few subdistricts, the program started in 2008 or 2009 rather than in 2007. Bias might result from this contamination, since it is possible that unobserved factors within the contaminated sub-districts also affected household responses. To deal with this contamination, we use the original treatment assignment to measure the conditional cash transfer program's impact on suicide.

At the sub-district level, the cash transfer program was offered to a list of eligible households that satisfied both certain demographic as well as certain economic requirements. A 2005 census from a national unconditional cash transfer program was used to construct the list of eligible households per village. Statistics Indonesia classified those targeted as "very poor." The classification was based on proxy-means tests of all poor households to identify program beneficiaries. About 10% of households in any given subdistrict were eligible to receive the program, i.e. the program was targeted at the ultra-poor.

2.2 The village censuses data

We use the censuses of all Indonesian villages (PODES) from 2000, 2003, 2005 and 2011 to examine the PKH program's effect on suicide incidence. These data have several advantages: (i) they comprise all villages in Indonesia and thus provide high statistical power, (ii) they allow us to examine the impact up to four years after the treatment and (iii) they overcome reporting biases at the household level.

In the village census, village heads report verifiable[6] village characteristics, such as the presence of asphalted roads, irrigation channels, population size, natural disaster prevalence, the presence of social organizations and data on the economic environment. The village census also contains unique data on suicides. In 2000, 2003, 2005 and 2011 the village head was asked whether any suicide occurred in their village in the previous year.[7] We construct a variable taking the value one if a suicide happened in a given subdistrict, and the value zero otherwise.

To the best of our knowledge this is the only suicide data available at low geographical

[6]It is important that these characteristics are verifiable, as it decreases the probability that village heads are lying. Moreover, there is a large literature emphasizing the importance of lying costs (Abeler et al., 2014) which addresses some concerns regarding reporting biases.

[7]It is conceiveable that the reports from the village are down-ward biased as a result of the social stigma attached to suicides in Indonesia.

levels in Indonesia[8]. Indeed, the Indonesian government does not systematically collect data on suicides. According to estimates from the WHO the suicide rate in Indonesia stood at 1.6 to 1.8 per 100,000 or 500 suicides per year. However, this is likely an under-estimate as suicides are still stigmatized in Indonesian society.

We link our village-level data to both the randomized conditional cash transfer program, and administrative data on the timing of the PKH roll-out for every Indonesian subdistrict. Since our key identifying variation is at the subdistrict level, we aggregate our village panel to the subdistrict level, and collapse our observations at the subdistrict boundaries from 2000 and 2006 respectively. For the main specification we show results using the 2000 boundaries, which is the first year for which suicide data is available.[9]

For practical reasons the construction of a panel over time is only possible at the subdistrict level (or a higher geographical level) owing to an increase in the number of administrative units in Indonesia over time. Decentralization reforms beginning from 1998 significantly increased the proliferation of administrative units. For example, the number of districts increased from 302 in 1999 to 514 in 2011 (Bazzi and Gudgeon, 2015; Pierskalla, 2013). The number of subdistricts increased from about 3000 in the early 2000s to over 6000 in 2011. Given the large number of district, subdistrict and village splits, it is challenging to construct panels of geographically homogeneous units over time (Bazzi and Gudgeon, 2015).[10]

Indeed, as a result of the large numbers of village, subdistrict and district splits we are only able to recover the cross-walk for 280 subdistricts with the 2000 boundary definitions.[11] The panel's construction was based on a unique subdistrict-level crosswalk for the time period of 2000 to 2011.

As a result of the subdistrict-splits, we have a few cases in which a subdistrict split into two parts in 2006, the year before the treatment was assigned. One part was randomly assigned to receive the treatment, while the other one was randomly assigned not to receive the treatment. In such a case the treatment indicator takes value 0.5. We have made sure that our results are

[8]To the best of our knowledge there is no other suicide dataset in the world with such high geographical disaggregation.

[9]We demonstrate the robustness of our main results using the 2006 boundaries in Tables A4 and A5 in Appendix A for for the extensive and intensive margin.

[10]Constructing a cross-walk at the village level is particularly challenging and would necessarily result in a large number of incorrect matches over time. This in turn would substantially increase measurement error of outcomes.

[11]308 subdistricts with the 2006 boundary definitions. We do not use border definitions from 2007 or later as there are some concerns about endogeneity. Specifically, if the cash transfer program affected the proliferation of administrative units, then using subdistrict definitions from 2007 or later could introduce endogeneity concerns.

robust to dropping cases in which the original subdistrict split into two. Indeed, when we drop the few observations with partially treated origin subdistricts (less than three % of our sample), our results are slightly stronger.

2.3 Balance

As a first step, we test the experiment's subdistrict-level characteristics for balance. The subscript i denotes the household, s denotes the sub-district and t denotes the time period. Let T_s denote the PKH program's original allocation, where $T_s = 1$ if the sub-district was originally assigned to receive the program and $T_s = 0$ if it was not assigned to do so. Let Y_{is0} and X_{is0} be the baseline values of our outcome variables and covariates respectively.

We consider whether baseline balance holds for the original treatment assignment by comparing means and clustering standard errors at the district level.[12] In Table A3 in the appendix, we provide evidence of baseline balance on a set of observables.[13] We find barely any evidence of baseline imbalances.[14]

We now turn to some of the sample's descriptive statistics. In Table 1, we summarize the entire population's subdistrict level characteristics (Panel A), and the sample of subdistricts used for the randomized controlled trial (Panel B). The probability of any suicide at the subdistrict-year level is about 44% for the whole population and about 40% for the experimental subsample. The average number of villages with at least one suicide at the subdistrict-year level is about .68 for both the population data and the experimental subsample. The characteristics of our experimental subsample and the population data are similar.

[Insert Table 1]

Two patterns in the data are important. First, suicides in Indonesia become more common over time.[15] Second, there is a negative correlation between a district's average per capita expenditure and the frequency of suicides at the district level. In other words, suicides are more common in poorer districts.

[12]We cluster at the district level, since that is our level of identifying variation.

[13]Whether the subdistrict is rural or urban, the number of hospitals, the number of maternity hospitals, the total number of health facilities, the presence of asphalted roads and lighting, the average population size, the average number of families in the subdistrict, the percentage of households with electricity, the number of primary school and the total number of educational institutions

[14]We find that the number of lighting is statistically different between treatment and control subdistricts at the 10% level. For all other outcomes we tested there are no imbalances.

[15]This could be driven by actual increases in suicide over time or by differences in reporting due to a decrease of the extent to which suicides are stigmatized.

3 Results

In this section, we first present difference-in-differences results for the population roll-out. Then, we show that the results also hold for the randomized trial sample. Finally, we reveal heterogeneous treatment effects and mechanisms.

3.1 Identification using the population data

Our identification strategy exploits the PKH program's roll-out, which covered about 10% of all Indonesian subdistricts when it started in 2007. By 2013, 47% of all Indonesian subdistricts were in receipt of the program. We use all subdistricts in Indonesia who did not receive the program in 2011 or earlier as counterfactuals.

We employ a difference-in-differences strategy which relies on the common trend assumption, i.e. that in the absence of the treatment, treated and control subdistricts would have followed the same trend. For our main specification we use data from the census of villages from 2005 and 2011, since suicide data is only available for those years.[16]. Our dependent variable is y_{st} which takes the value one if at least one suicide was reported in subdistrict s and at time t, and takes value zero otherwise.

We focus on the extensive margin of any suicide rather than the number of villages with at least one suicide for three reasons: first, the variation occurs primarily along the extensive margin. Conditional on any incident occurring, the most common outcome is the report of one incident at the subdistrict level.[17] Second, analyzing at the extensive margin also simplifies estimation, since the incidental parameter problem hinders the inclusion of fixed effects in count models (Lancaster, 2000). Third, by focusing on the extensive margin, we do not have to worry about outliers that might affect our results. Fourth, we cannot construct reliable measures of suicide rates as we do not observe the actual number of suicides in any village for most of our sampling period. In addition, measurement error in the local population size[18] would add additional noise to the outcome variable of interest.

Our empirical specification also includes subdistrict fixed effects (α_s), time fixed effects (ϕ_t), and two treatment indicators: $PKH0708_{st}$, taking value one when a subdistrict started receiving PKH in 2007 or 2008 and $PKH1011_{st}$ which takes value one for subdistricts which started

[16] (and not in the 2008 census)

[17] In section 3.3 we show robustness to looking instead at the intensive margin, i.e. using the number of incidents as outcome variable.

[18] Our estimates of population size are based on the self-report of village heads.

receiving PKH in 2010 or 2011. In this specification, we cluster the standard errors at the district-level to allow for arbitrary covariance of the error term at the district level and to account for serial correlation of errors over time (Angrist and Pischke, 2008). Even though the identifying variation is at the subdistrict level, we cluster standard errors at the district level, as the roll-out of the program was correlated at the district level.[19] We estimate all of our main specifications with OLS using a linear probability model.[20]

Our specification of interest is:

$$y_{st} = \delta_1 PKH0708_{st} + \delta_2 PKH1011_{st} + \alpha_s + \phi_t + \varepsilon_{st} \tag{1}$$

Our main coefficients of interest are δ_1 and δ_2, which provide the treatment effect for the subdistricts that started to receive the program in 2007/2008 and 2010/2011 respectively. Turning to results, we find significant decreases in the probability of a suicide happening in a given subdistrict. As column 1 of table 2 shows, the probability of a suicide happening at the subdistrict level decreases by 9.4 percentage points, or about 18 % for subdistricts who received the cash transfer program either in 2007 or 2008. We find no significant effect of receiving the cash transfer program in 2010 or 2011 on the probability of suicide. In column 2 of table 2 we pool the subdistricts that received the treatment at different points in time and show that the probability of a suicide occurring was decreased by more than seven percentage points for the whole sample.

This analysis highlights two features: first, to evaluate the implications of social welfare programs on people's lives, obtaining medium and long-term evidence is important. Second, we find no significant decreases in suicides for subdistricts receiving the program in 2010 or 2011, i.e. it takes time for cash transfers to change people's lives.

[Insert Table 2]

In Table 3 we show the results of a variety of robustness checks. In column 1 and 2 we display the results of Table 1. In column 3 we assess the potential of differential trends to explain the patterns in the data by controlling for district trends. In column 4, we interact a set of baseline covariates[21] with a Post indicator, taking value one in 2011.

[19]We have made sure that our results are robust to clustering standard errors at the province level as well as the subdistrict level. These results are omitted for brevity's sake, but are available upon request.

[20]We have made sure that our results are robust to a non-linear specification, such as logit.

[21]We control for several baseline covariates: the percentage of farmers at the subdistrict, whether the subdistrict is rural or urban, the number of hospitals, the number of maternity hospitals, the total number of health facilities, the presence of asphalted roads and lighting, the average population size, the average number of families in the subdistrict, the percentage of households with electricity, the number of primary school and the total number of educational institutions

In columns 5-7 we show that our results are robust to removing subdistrict fixed effects, and replacing them with a set of baseline covariates and district fixed effects. For robustness, we also use a more conservative definition of what constitutes a good counterfactual, as displayed in column 8. Specifically, we only use subdistricts that received the PKH in 2012 or 2013 as counterfactuals. We hypothesize that subdistricts receiving the program in 2012 or 2013 are more similar to subdistricts from our treatment group receiving the program before or during 2011. The results from this different set of counterfactuals are very similar to our main results, which further corroborates the validity of our identifying assumption. Finally, we have also checked that our results are robust to dropping the subdistrict from our sample that received the PKH program in 2010 or 2011.[22]

[Insert Table 3]

To test for the common trend assumption's validity, we focus on village census data from before PKH program treatment started: specifically, from the years 2000, 2003 and 2005. We create a pseudo-treatment indicator for 2005, $Pseudotreatment2005_{st}$ and 2003, $Pseudotreatment2003_{st}$ taking value one for treatment subdistricts in 2005 and 2003 respectively. In other words, we test whether prior to the receipt of the program in 2007 (at the earliest) treatment subdistricts were on a differential trend. We estimate the following specification controlling for subdistrict fixed effects, α_s as well as time fixed effects, ϕ_t:

$$y_{st} = \pi_1 Pseudotreatment2005_{st} + \pi_2 Pseudotreatment2003_{st} + \alpha_s + \phi_t + \varepsilon_{st} \tag{2}$$

If the coefficients π_1 and π_2 are statistically significantly different from zero, then this would be indicative of a violation of the common trend assumption. As can be seen in Table 4 below, we find no evidence for the violation of the common trend assumption for the probability of a suicide happening. The estimated coefficient is quite noisily measured and points to an insigificant increase in suicides between 2005 and 2003 and an insignificant decrease between 2000 and 2003. Moreover, the two coefficients are also jointly insignificant. Taken together, this suggests that the common trend assumption is satisfied.

[Insert Table 4]

[22]In particular, this is relevant as it it unclear whether the subdistrict receiving the program in 2011 completed the census before the program actually started.

To provide further evidence on the robustness of our result, we show in Table 5 that our results are robust to using both 2003 and 2005 as baseline period in our estimations. As expected we see that the coefficient estimate decreases once we also include observations from 2003, but it still remains both statistically and economically significant. The diminished effect size reflects the positive and insignificant positive pre-trend in our treatment municipalities displayed in Table 4. To rule out that our results are driven by pre-trends we re-estimate our main specification, but also include subdistrict trends, $\alpha_s \times t$.

$$y_{st} = \delta_1 PKH0708_{st} + \delta_2 PKH1011_{st} + \alpha_s + \phi_t + \alpha_s \times t + \varepsilon_{st} \tag{3}$$

We find clear evidence that – if anything – the slight upward pre-trend downward biases our coefficient of interest. Indeed, as can be seen in column (3) of Table 5, we find that our estimated coefficient becomes much larger once we account for differential trends at the subdistrict level. Finally, in column (4) of Table 5 we also show that our results are robust to using an ANCOVA estimator. We find that the effect size lies in between the effect size estimated in columns (1) and (2) in Table 5.

[Insert Table 5]

3.2 Randomized Controlled Trial

We also use a subset of the above sample in which the treatment was randomly assigned. Specifically, out of a list of 360 eligible subdistricts, 180 were randomly assigned to receive the PKH conditional cash transfer program. Here, we make use of the original treatment assignment to deal with potential endogeneity issues arising from the actual treatment assignment. We estimate a very similar specification as above, and also cluster standard errors at the district level.

$$y_{st} = \delta_1 PKH_{st} + \alpha_s + \phi_t + \varepsilon_{st} \tag{4}$$

Here the coefficient δ_1 provides us with the intent-to-treat effect. In Table 6, we summarize the program's effects on suicides. We find that the PKH program significantly reduced the incidence of suicide. The estimated coefficients are stable across a wide range of specifications and are also similar to the previous program roll-out estimates. In column 1, we display the results from the main specification, outlined in the above equation. In column 2, we also include district specific trends to account for differential trends; the coefficient barely moves. In column 3, we interact a set of baseline covariates[23] with an indicator taking the value one for all observations in 2011 (the follow-up period). Finally, we also ensure the robustness of our results when we do not include subdistrict fixed effects and instead control for covariates and district fixed effects (columns 4 - 6 of Table 6).

[Insert Table 6]

In addition, we also examine the validity of the common trend assumption. In particular, we employ a similar strategy as for the population roll-out. We employ census data from 2000, 2003 and 2005 and test whether the treated subdistricts were on a different trend. As Table 7 shows, the common trend assumption is satisfied. This demonstrates that the randomization worked well and corroborates our previous finding that there was balance between treatment and control subdistricts, in terms of observables.

[Insert Table 7]

In Table 8 we show that our results are robust to using both 2003 and 2005 as baseline period in our estimations. As expected we see that the coefficient estimate decreases once we also include observations from 2003, but it still remains both statistically and economically significant. As in the previous section, we show that our estimated treatment effects once we include subdistrict fixed effects. Finally, we provide evidence that the estimate of the treatment effect remains both statistically and economically significant once we apply an ANCOVA estimator.

[Insert Table 8]

3.3 Intensive margin

In our main specifications above, we focus on suicide occurrence, since most of the subdistrict-level variation in suicides occurs along the extensive margin. This binary outcome measure also simplifies our estimations, compared to the estimation of count models. In what follows, we present results along the intensive margin, i.e. looking at the number of villages with at least one suicide as our outcome variable. To be more precise, by number of suicides we mean the

[23]Whether the subdistrict is rural or urban, the number of hospitals, the number of maternity hospitals, the total number of health facilities, the presence of asphalted roads and lighting, the average population size, the average number of families in the subdistrict, the percentage of households with electricity, the number of primary school and the total number of educational institutions

number of villages in a given subdistrict in which there was at least one suicide. We thus re-estimate our main empirical specifications and additionally control for a fourth order polynomial of population size (which is taken from the village census).[24] This substantially reduces the noise in our estimates, since the number of suicides (but not suicide probability) depends greatly on population size.[25]

Specifically, in column (1) of Table 9, we use the PKH program's roll-out to estimate our main specification, but with the number of suicides as the outcome. We estimate an OLS model with subdistrict and time fixed effects, and a polynomial of population size. The results on the intensive margin are in line with those on the extensive margin, i.e. the PKH's introduction reduced the number of suicides. The effect sizes are slightly smaller than before, given that our results are more noisily measured. Specifically, the number of suicides decreases by about 14 %, rather than 18 % when looking at the extensive margin.

We then examine whether our results are robust to taking into account the data's count nature. To do so, we estimate both a poisson and a negative binomial model (which takes account of the large number of zeros). These models do not allow for the inclusion of subdistrict fixed effects, so we present our results by simply controlling for initial treatment status, time fixed effects and a polynomial of population size.[26] As shown in columns 2 and 3 of Table 9, the results are robust to this.

Next, we construct a variable proxying for the suicide rate. Specifically, we divide the number of suicides (i.e. the number of villages with a least one suicide) by population size in the subdistrict.[27] As we do not observe the total number of suicides in any given subdistrict but just know in how many villages in a given subdistrict a suicide happened, this should be interpreted as the lower bound to the actual suicide rate. We estimate the main specification controlling for time and subdistrict fixed effects and clustering standard errors at the district level. As can be seen in column (4) of Table 9, we find that the results are robust. In particular, we find that the suicide rate decreases by about 14 %.

Similarly, we re-examine the evidence with the sample from the randomized controlled trial using the number of suicides as the outcome variables of interest. As is evidenced in columns

[24] Our results are similar when not controlling for the polynomial of population size.

[25] Our results are robust to not controlling for the polynomial of population size.

[26] We have checked that the results on count models are robust to the inclusion of district fixed effects and a set of control variables. Indeed, the inclusion of more control variables lowers standard errors and increases significance.

[27] We expect the estimate of population size to be noisily measured as it is based on the village head's self-report.

1-4 of Table 10, we find that the results are very similar to the results looking at the extensive margin. In particular, the effect sizes are quite comparable and even slightly larger in the analysis looking at the intensive margin. Finally, our results are not sensitive to taking into account the count nature of the outcome variable and to looking at the per capita number of suicides, i.e. the suicide rate.

[Insert Tables 9 and 10]

Our point estimates on the effect of cash transfer programs could be biased towards zero due to migration responses to the placement of social welfare programs (Imbert and Papp, 2015). Specifically, there is evidence that social welfare programs can decrease short-term migration. Given that our point estimates are not very sensitive to the inclusion of a fourth order polynomial of population size, we believe that migration responses are not a big cause of concern. Moreover, the fact that our results are robust to using different definitions of our outcome variable of interest (dummy taking value one if a suicide happened, number of villages in which a suicide happened, suicide rate) makes us confident that our results are not confounded by migration responses.

Overall, the two identification strategies demonstrate that the cash transfer reduced suicides. The effect sizes are moderate and the results suggest that there are important medium to long-run returns to be reaped from the conditional cash transfer program. We find no evidence of short-term effects of the program on suicides. It should be noted that our estimates of the program on suicides likely constitutes a lower bound to the actual treatment effect as we do not have data on the number of suicides in each villages, but only observe whether at least one suicide happened in a given village for most of our suicide data.

3.4 Interpreting the effect size

We find that the cash transfer program reduces suicides by about 10%.[28] This may seem like a substantial effect size as only 10% of the population was in receipt of the cash transfer program. Given that the poorest 10% of households are at much higher suicide risk than the rest of the population, let us assume that they make up about 20% of all suicides.[29] Then, our estimates

[28]This is based on the estimate using the whole sample from the roll-out and pooling the treatments from 2007 and 2010 together.

[29]There is a large amount of evidence showing the poorer individuals are more likely to commit suicides. We believe that it is plausible that this gradient between suicide and poverty is particularly high in a developing country context. However, little quantitative evidence exists from a developing country context as administrative data on suicides is lacking.

imply that suicide risk was halved among the poorest 10% of the population assuming that those not receiving the cash transfer program were not affected by the program.[30][31]

All in all, our treatment effect estimates imply that the baseline suicide rate incidence among the poorest 10% must have been very high and that the cash transfer program strongly lowered suicide rates among the poorest 10% of the population. Alternatively, our treatment effect estimates could also imply that positive economic spillovers of the cash transfer program could also have lowered the suicide rate among households not in receipt of the cash transfer program.

Our estimated effect sizes of the cash transfer program can also be interpreted relative to the effect size we find for the rainfall shocks. We find that a negative rainfall shock increases the probability of suicide by between 5 and 15 percentage points, or between 10 - 30 %. This, in turn, implies that the cash transfer program's suicide reduction is comparable with the effect sizes of a large negative agricultural shock, as measured by rainfall below 1.5 of a standard deviation. Thus, our evidence suggests that the incidence of suicide is quite elastic to both positive and negative economic shocks that only affect the most vulnerable subset of the population.

3.5 Heterogeneity

In what follows, we analyze heterogeneous treatment effects by interacting our treatment indicators with variables across several dimensions that try to capture levels of poverty, availability of public goods and health care as well as social capital. In particular, we look at the following variables: mean district level per capita expenditure, percentage of farmers, number of health institutions, number of social organisations, number of crimes, number of educational facilities, population size as well as the number of youth organizations. To have the highest possible power we focus on heterogeneous treatment effects in the sample using the roll-out of the PKH program.[32] In all of our specifications, we control for differential trends by interacting our variable of interest with an indicator taking value one for the follow-up period.

In our main specification we include all interaction terms at once as this allows us to account for the inter-correlations of the different variables.[33] As can be seen in Table 11, we find no

[30] If, on the other hand, we assumed that the poorest 10% made up only 10% of all suicides then our estimates imply that suicide among the ultra-poor were reduced to zero.

[31] This estimate might be an overstatement of the effect of the cash transfer on the treated if there are positive economic spillovers of the cash transfers propagated through local social networks as has been documented in the context of other cash transfer programs (Angelucci and De Giorgi, 2009).

[32] We have verified that the results are similar for the experimental subsample.

[33] We have made sure that our results are similar if we conduct the interaction term analysis for each of the different variables separately. These results are displayed in Tables A6-A8.

evidence of heterogeneous treatment effects by initial poverty levels as proxied by the mean per capita district expenditures[34] and the mean share of farmers. We also find no heterogeneous treatment effects by measures of institutional quality such as the number of educational facilities. Moreover, we hypothesized that the treatment effects could differ by the population size of the subdistrict. We find not evidence for this.[35]

We test whether the baseline number of health institutions[36] are important for the effect of the social welfare program. In particular, one could hypothesize that the treatment effects of the program might be larger in the presence of worse health institutions. Therefore, we investigate whether baseline levels of health institutions are related to the size of the treatment effect. In Table 11, we show that there no significant interaction effect between initial health institutions and receiving the treatment for subdistricts who receive the program.

Finally, we hypothesize that subdistricts with initially higher levels of social capital might respond less strongly to the cash transfer program as informal safety networks for mental illness might exist. We use three different proxies for social capital: the total number of social organizations in a subdistrict[37], the number of youth organisations as well as the number of crimes at the subdistrict level. We find no heterogeneoues treatment effects for any of the different measures of social capital.

[Insert Table 11]

Overall, we find no significant evidence of heterogeneous treatment effects by initial poverty levels, health institutions, educational institutions, population size as well as social capital. This shows that the treatment effects we observe are quite homogeneous across the population of Indonesian subdistricts. Given that we have a large overall sample size, we have large statistical power to also detect small effect sizes. This implies that the lack of heterogeneous effects cannot be explained by lacking statistical power.

[34] In Panel A of table A6 (Appendix A) we use an alternative variable for poverty levels that captures the proportion of villages in a given district that are classified as poor by Statistics Indonesia in 2007. Our results remain unchanged when using this alternative measure.

[35] We have also checked for heterogeneous treatment effects by urban/rural status.

[36] By the number of health institutions we mean the number of hospitals, health clinics, supporting health clinics, private practices, midwives, health posts, maternity clinics, pharmacies and drug stores.

[37] The number of youth organisations, religious organisations, orphanages, organisations for the elderly, the handicapped, the number of NGOs as well as an organisation for funeral services.

3.6 Mechanisms

The cash transfer increased recipients' welfare by increasing their consumption (Alatas, 2011; Roth, 2015) and improving their health outcomes (Alatas, 2011). Guided by this, we examine several additional mechanisms through which the cash transfer program could lower the incidence of suicides. To do so, we include "endogenous controls" in our main specification of interest to examine whether the inclusion of these time-varying controls reduces our treatment effects. These controls could have been affected by the cash transfer in systematic ways and therefore act as a channel for our treatment effects.

In particular, we control for three sets of outcomes. First, we control for data on crime and violence[38] which has been shown to be an important channel for mental health improvements (Kling et al., 2007). Second, we control for several variables capturing the quality of public goods[39] which may be important as for example access to more health facilities may directly prevent the incidence of suicide. Third, we control for changes in social capital as captured by the total number of social organizations at the subdistrict level. These sets of endogenous controls are motivated by a large literature emphasizing the importance of social interactions for mental health (Cohen and Wills, 1985; Kawachi and Berkman, 2001) and suicides (Trout, 1980). Therefore, if the cash transfer program increased social capital, it could thereby lower the incidence of suicides

As can be seen in columns (2) to (4) in Table A1 (Appendix A), we do not find that our treatment effects are sensitive to the inclusion of any of the time-varying controls that capture capture crime, institutional quality, public goods as well as social capital. Indeed, the coefficient is fairly stable across specifications.

4 Agricultural Productivity Shocks and Suicides

4.1 Data and Specification

Our evidence from previous sections shows that a positive economic shock, the receipt of a conditional cash transfer, can lower the incidence of suicide. In this section, we examine whether

[38]We control for the following set of covariates: whether there was any crime in the subdistrict, the number of crimes, the numer of robberies, thefts, lootings, violence, rape, drug abuse, murder, child sale, drug trafficking and combustion.

[39]The presence of mutual work customs, the number of educational facilities, the proportion of villages with an asphalted main road, the number of hospitals, the number of total health institutions (as defined above) the proportion of villages with lighting and irrigation.

a different type of economic shock also affects suicides. Specifically, we examine the role of agricultural productivity shocks, as measured by rainfall.

We use the ERA-Interim Reanalysis dataset which provides monthly precipitation data from 1979 until 2011. We define rainfall at the district level[40] as rainfall at the grid point closest to the geometric center of the district. Reanalysis data is based on a mix of real weather observations (station and satellite data) and an atmospheric climate model.[41] The rainfall data is matched to the 2000 district boundaries.[42]

We use suicide data from the 2000, 2003, 2005 and 2011 waves of the Indonesian village census. As before, our main outcome variable of interest, y_{sdt}, is an indicator variable taking value one if a suicide happened in a given subdistrict, s and district, d at time t.

As in Maccini and Yang (2009), we define rainfall, $zrain_{dt}$, as the normalized deviation of rainfall from the long-term mean within a given district.[43] This measure of rainfall has been shown to significantly and strongly predict district-level rice output (Levine and Yang, 2014). In all of our specifications we control for subdistrict level fixed effects, α_s, as well as time fixed effects, ϕ_t and a subdistrict-specific error term, ε_{sdt}, which is clustered at the district level, i.e. the level of identifying variation. We estimate the following equation:

$$y_{sdt} = \gamma_1 zrain_{dt} + \alpha_s + \phi_t + \varepsilon_{sdt} \tag{5}$$

4.2 Results

In columns (1) to (4) of Panel A of Table A2 in Appendix A, we provide evidence that high rainfall significantly reduces suicides. Specifically, in columns (1) and (2) we present results for the probability of suicide, while in columns (3) and (4), we show results for the number of suicides. In column (1) we show that increases in district-rainfall by one standard deviation from the long-run district mean lowers the probability of suicide by 1.4 percentage points. In column (2) we include district trends, to rule out that districts that experience different levels of rainfall in our time periods are on different trends. We find that our results even get stronger once we control for district-specific trends. Specifically, a one-standard deviation increase in rainfall from

[40] This is the lowest possible available level of geographic disaggregation.

[41] The main advantage of reanalysis data is the homogeneous data quality across time and space, which alleviates the concern of endogenous placement of weather stations.

[42] Which is the earliest period for which we have suicide data.

[43] We use rainfall data from 1979 to 2011 to construct the district specific long-run means and long-run standard deviations. Our results are robust to constructing the rainfall variable in different ways.

the long-run mean decreases the probability of suicide by 3.2 percentage points. In columns (3) and (4) we present this set of results for the intensive margin, i.e. the number of suicides and we find similar patterns as in columns (1) and (2).

4.2.1 Asymmetric effects

In Panel B of Table 11, we differentiate between positive and negative shocks to agricultural productivity. To do so, we create a dummy variable, $posshock_{dt}$, taking value one for districts experiencing a positive shock in rainfall of at least 1.5 standard deviation above their long-run mean. In addition, we create a dummy variable, $negshock_{dt}$, taking value one for districts experiencing a negative shock in rainfall of at least 1.5 standard deviation below their long-run mean. Then we estimate the following equation:

$$y_{sdt} = \beta_1 posshock_{dt} + \beta_2 negshock_{dt} + \alpha_s + \phi_t + \varepsilon_{sdt} \tag{6}$$

In columns (1) and (2) of Panel B in Table 11, we present the results on the probability of suicide and in columns (3) and (4) we show results on the number of suicides. In column (1) we show that the probability of suicide significantly increases when there is a negative shock and is unchanged when there is a positive shock. In column (2) we also control for district-specific trends and show that negative shocks significantly increase the probability of suicide, while positive shocks do not significantly affect the probability of suicide. The magnitudes of the effect are very large for the negative shock, but close to zero for the positive shock. These results are very similar when considering the number of suicides as can be seen in columns (3) and (4). Overall, these results corroborate the view that bad harvests ignited by a drought can increase suicide rates in developing countries (Deshpande, 2002; Gruère and Sengupta, 2011; Mohanty, 2005).

Our point estimates on the effect of rainfall shocks could be biased towards zero due to migration responses to rainfall shocks (Kleemans and Magruder, 2015). Specifically, there is evidence that negative rainfall shocks increase out-migration. Given that our point estimates are not very sensitive to the inclusion of a fourth order polynomial of population size, we believe that migration responses are not a big cause of concern.

5 Conclusion

We provide evidence that the introduction of a conditional cash transfer program, targeted at the poorest 10 % of the Indonesian population, significantly decreased suicides. Using both a randomized experiment, and the population-wide roll-out of the program, we find convergent evidence from both sources. This in turn ensures both a high internal and external validity of our study. Moreover, we show that positive agricultural productivity shocks significantly reduce the incidence of suicide. Overall, we provide strong evidence that economic circumstances matter for people's decision to commit suicides.

We show that the probability of any suicide in a treated subdistrict decreases by about 15 %. We find that the effects are driven by subdistricts that are in receipt of the program for longer, pointing to the importance of gathering more long-term evidence. We find no significant short-term evidence that the cash transfer program decreased suicides.

Our results complement a small but growing literature that uses suicides as an outcome measure (Anderson and Genicot, 2015; Hamermesh and Soss, 1974; Hebous and Klonner, 2014). We believe that the use of suicide data advances the literature on social welfare programs in two important ways: First, the use of suicide data circumvents problems of self-reports and reporting biases which plague the credibility of self-reported measures of well-being in particular in the context of cash transfer programs where reciprocity motives of recipients are important drivers of social desirability bias. Second, the suicide data in itself is an important measure of welfare in a developing country context (Hebous and Klonner, 2014).

These results are important, because they imply that an intervention that increases household income by only about 12 % over six years can still have substantial mental health benefits. This complements existing evidence on the large benefits of cash transfers on mental health and well-being (Baird et al., 2013; Haushofer and Shapiro, 2016).

The paper contributes to a growing literature emphasizing the large positive effects of giving individuals direct cash transfers (Baird et al., 2011; Blattman et al., 2015a, 2014; Fafchamps and Quinn, 2015; Fafchamps et al., 2014; McKenzie, 2015) and to the literature looking at the psychological effects of cash transfer programs (Baird et al., 2013; Green et al., 2015; Haushofer and Shapiro, 2016; Roth, 2015).

We believe that more work should be conducted on how exogenous economic shocks affect the prevalence of suicides: First, more work needs to be done to understand the mechanisms through which income shocks affect the incidence of suicides. Second, more causal evidence on the link between negative economics shocks, social welfare programs and suicides needs to be carried out.

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Tables

Table I: Summary statistics

Variable	Mean	Std. Dev.	N
Subdistrict-level characteristics			
Panel A: Population Data			
Number of suicides	0.68	1.012	3158
Probability of suicide	0.448	0.497	3158
% of farmers	66.49	27.423	3158
Population Size	47974	34903	3158
Number of health facilities	40.228	19.148	3158
Asphalted Road	0.645	0.341	3158
Lighting	0.712	0.359	3158
Number of families	12111	8494	3158
% of households with electricity	0.655	0.234	2959
Number of educational facilities	73.265	42.457	3158
Panel B: Experimental Subsample			
Number of suicides	0.682	1.019	308
Probability of suicide	0.409	0.492	308
Population Size	48713	25721	308
Number of health facilities	43.11	19.327	308
Asphalted Road	0.658	0.326	308
Lighting	0.763	0.348	308
Number of families	12924	6452	308
% of households with electricity	0.662	0.246	281
Number of educational facilities	80.042	40.124	308

Table II: Main Results

	(1)	(2)
	Probability of Suicide	
Treatment 2007/2008	-0.094***	
	(0.026)	
Treatment 2010/2011	-0.020	
	(0.038)	
Treatment		-0.072***
		(0.024)
Control Mean	.517	.517
Subdistrict Fixed Effects	Y	Y
Time Fixed Effects	Y	Y
Number of Subdistrict	3158	3158
N	6316	6316
R^2	0.012	0.011

Standard errors clustered at the district level in parentheses. In columns 1 and 2 we control for time fixed effects as well as subdistrict fixed effects* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table III: Robustness of main result: Roll-out

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					Probability of Suicide			
Treatment 2007/2008	-0.094***		-0.090**	-0.091***	-0.094***	-0.106***	-0.094***	-0.080**
	(0.026)		(0.043)	(0.029)	(0.026)	(0.028)	(0.026)	(0.032)
Treatment 2010/2011	-0.020		-0.091	-0.033	-0.020	-0.028	-0.020	-0.006
	(0.038)		(0.068)	(0.042)	(0.038)	(0.041)	(0.039)	(0.042)
Treatment		-0.072***						
		(0.024)						
Control Mean	.517	.517	.517	.517	.517	.517	.517	.517
Subdistrict Fixed Effects	Y	Y	Y	Y	N	N	N	Y
Time Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
District Trend	N	N	Y	N	N	N	N	N
Controls × Post	N	N	N	Y	N	N	N	N
Controls	N	N	N	N	N	Y	N	N
District Fixed Effects	N	N	N	N	N	N	Y	N
Restricted counterfactuals	N	N	N	N	N	N	N	Y
N	6316	6316	6316	5918	6316	5918	6316	3872

Standard errors clustered at the district level in parentheses. In columns 1 and 2 we control for time fixed effects as well as subdistrict fixed effects. In addition, in column 3 we also include district trends. In column 4, we interact a set of baseline covariates with a Post indicator. In column 5 relative to the main specification we do not include subdistrict fixed effects, but control for treatment status at baseline. In column six we do not include subdistrict fixed effects, but control for a large set of control variables. In column 7 we include district fixed effects, but no subdistrict fixed effects. In column 8, we only use subdistricts who received the program until 2013 as counterfactuals.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table IV: Common Trend: Rollout data

	Suicide
Pseudotreatment × Year = 2005	0.038
	(0.026)
Pseudotreatment × Year = 2003	-0.027
	(0.029)
Subdistrict Fixed Effects	Y
Time Fixed Effects	Y
N	9474
R^2	0.072

Standard errors clustered at the district level in parentheses. The specification includes both subdistrict as well as time fixed effects. Pseudotreatment takes value one for subjects receiving the cash transfer program before or in 2011 and zero otherwise. $Year = 2005$ takes value one for all observations in year 2005. $Year = 2003$ takes value one for all observations in year 2003.[*] $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table V: Main Results: Further sensitivity - Rolllout Sample

	(1)	(2)	(3)	(4)
		Probability of suicide		
Treatment 2007/2008	-0.094***	-0.054**	-.175***	-0.073***
	(0.026)	(0.023)	(.047)	(0.028)
Treatment 2010/2011	-0.020	-0.007	-.048	0.063
	(0.038)	(0.046)	(0.064)	(0.039)
Sample	05 & 11	03 & 05 & 11	03 & 05 & 11	05 & 11
Subdistrict Fixed Effects	Y	Y	Y	N
Time Fixed Effects	Y	Y	Y	Y
Subdistrict Trends	N	N	Y	N
Baseline Suicide Control	N	N	N	Y
Number of subdistricts	3158	3158	3158	3158
N	6316	9474	9474	3158

Standard errors clustered at the district level in parentheses. In column 1 we control for time fixed effects as well as subdistrict fixed effects. In addition, in column 2 we also include observations from 2003 as the base period. In column 3, we additionally include subdistrict trends. In column 4 we implement an ANCOVA estimator by examining only observations in the final period and controlling for baseline suicide controls.

Table VI: Main Results: Randomized Experiment

	(1)	(2)	(3)	(4)	(5)	(6)
			Probability of suicide			
Treatment	-0.131**	-0.137**	-0.161**	-0.131**	-0.168**	-0.131**
	(0.060)	(0.061)	(0.072)	(0.060)	(0.063)	(0.062)
Control Mean	.476	.476	.476	.476	.476	.476
Subdistrict Fixed Effects	Y	Y	Y	N	N	N
Time Fixed Effects	Y	Y	Y	Y	Y	Y
District Trend	N	Y	N	N	N	N
Controls × Post	N	N	Y	N	N	N
Controls	N	N	N	N	Y	N
District Fixed Effects	N	N	N	Y	N	Y
Number of subdistricts	280	280	257	280	257	280
N	560	560	514	560	514	560

Standard errors clustered at the district level in parentheses. In column 1 we control for time fixed effects as well as subdistrict fixed effects. In addition, in column 2 we also include district trends. In column 3, we interact a set of baseline covariates with a Post indicator. In column 4 relative to the main specification we do not include subdistrict fixed effects, but control for treatment status at baseline. In column 5 we do not include subdistrict fixed effects, but control for a large set of control variables. In column 6 we include district fixed effects, but no subdistrict fixed effects.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table VII: Common Trend: Randomized Experiment

	Suicide
Pseudo-treatment $\times$ Year $=$ 2005	0.101
	(0.066)
Pseudo-treatment $\times$ Year $=$ 2003	0.018
	(0.060)
N	840
Subdistrict Fixed Effects	Y
Time Fixed Effects	Y
N	840

Standard errors clustered at the district level in parentheses. The specification includes both subdistrict as well as time fixed effects. Pseudotreatment takes value one for subjects receiving the cash transfer program before or in 2011 and zero otherwise. $Year = 2005$ takes value one for all observations in year 2005. $Year = 2003$ takes value one for all observations in year 2003.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table VIII: Main Results: Further sensitivity - RCT Sample

	(1)	(2)	(3)	(4)
		Probability of suicide		
Treatment	-0.131**	-0.089*	-0.215*	-0.077**
	(0.060)	(0.046)	(0.123)	(0.036)
Sample	05 & 11	03 & 05 & 11	03 & 05 & 11	05 & 11
Subdistrict Fixed Effects	Y	Y	Y	N
Time Fixed Effects	Y	Y	Y	Y
Subdistrict Trends	N	N	Y	N
Baseline Suicide Control	N	N	N	Y
Number of subdistricts	280	280	280	280
N	560	840	840	280

Standard errors clustered at the district level in parentheses. In column 1 we control for time fixed effects as well as subdistrict fixed effects. In addition, in column 2 we also include observations from 2003 as the base period. In column 3, we additionally include subdistrict trends. In column 4 we implement an ANCOVA estimator by examining only observations in the final period and controlling for baseline suicide controls.

Table IX: Intensive Margin: Roll-out sample

	(1)	(2)	(3)	(4)
		Number of suicides		
	OLS	**Poisson model**	**Negative binomial**	**Suicide Rate**
Treatment 2007/2008	-0.105**	-0.155**	-0.160**	-0.004***
	(0.052)	(0.074)	(0.073)	(0.001)
Treatment 2010/2011	-0.027	-0.051	-0.045	0.000
	(0.079)	(0.088)	(0.089)	(0.002)
N	6316	6316	6316	6316
Subdistrict Fixed Effects	Y	N	N	Y
Time Fixed Effects	Y	Y	Y	Y
Population Size Controls	Y	Y	Y	N
Control Mean	.754	.754	.754	.02
N	6316	6316	6316	6316

Standard errors clustered at the district level in parentheses. The specification in column (1) includes both subdistrict as well as time fixed effects and is estimates using OLS. The specification in column 2-3 does not include subdistrict fixed effects, but just controls for treatment status at baseline. The specification in column (2) is estimated using a poisson model. The specification in column (3) is estimated using a negative binomial model. In specification we define our outcome variable as suicide rate, which is given by the number of suicides divided by population size.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table X: Intensive Margin: RCT sample

	(1)	(2)	(3)	(4)
	Number of suicides			
	OLS	**Poisson model**	**Negative binomial**	**Suicide Rate**
Treatment	-0.300**	-0.407**	-0.408**	-0.005*
	(0.125)	(0.166)	(0.166)	(0.003)
Control Mean	.696	.696	.696	.012
Subdistrict Fixed Effects	Y	N	N	Y
Time Fixed Effects	Y	Y	Y	Y
Population Size Controls	Y	Y	Y	N
N	560	560	560	560

Standard errors clustered at the district level in parentheses. The specification in column (1) includes both subdistrict (2000 boundaries) as well as time fixed effects and is estimated using OLS. Moreover, we control for a polynomial of population size. The specification in column 2-3 does not include subdistrict fixed effects, but just controls for treatment status at baseline. The specification in column (2) is estimated using a poisson model. The specification in column (3) is estimated using a negative binomial model.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table XI: Heterogeneous Effects

	Suicide: Yes
Treatment 2007/2008	-0.180
	(0.210)
Treatment 2010/2011	-0.336
	(0.293)
Treatment 2007/2008 × Number of Youth Organisations	-0.120*
	(0.067)
Treatment 2007/2008 × Number of Crimes	0.004
	(0.004)
Treatment 2007/2008 × Number of Social Organisations	0.000
	(0.001)
Treatment 2007/2008 × Population Size	-0.000
	(0.000)
Treatment 2007/2008 × Number of Educational Facilities	0.001
	(0.002)
Treatment 2007/2008 × Percentage of Farmers	-0.000
	(0.002)
Treatment 2007/2008 × Per Capita Expenditure	0.000
	(0.000)
Treatment 2007/2008 × Number of Health institutions	-0.002
	(0.002)
Treatment 2010/2011 × Number of Youth Organisations	0.034
	(0.190)
Treatment 2010/2011 × Number of Crimes	0.006
	(0.005)
Treatment 2010/2011 × Number of Social Organisations	0.001
	(0.001)
Treatment 2010/2011 × Population Size	-0.000
	(0.000)
Treatment 2010/2011 × Number of Educational Facilities	-0.000
	(0.003)
Treatment 2010/2011 × Percentage of Farmers	0.003
	(0.002)
Treatment 2010/2011 × Per Capita Expenditure	0.000
	(0.000)
Treatment 2010/2011 × Number of Health institutions	-0.004
	(0.004)
N	6230
R^2	0.021
Subdistrict Fixed Effects	Y
Time Fixed Effects	Y
Interaction Variables × Post	Y

Standard errors clustered at the district level in parentheses. The specification includes both subdistrict as well as time fixed effects. In addition we also control for differential trends by the interaction term variables.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Table A1: Including Endogenous Controls

	Probability of suicide			
	(1)	(2)	(3)	(4)
Treatment 2007/2008	-0.094***	-0.105***	-0.091***	-0.085***
	(0.026)	(0.026)	(0.027)	(0.026)
Treatment 2010/2011	-0.020	-0.028	-0.020	-0.019
	(0.038)	(0.039)	(0.039)	(0.037)
Subdistrict Fixed Effects	Y	Y	Y	Y
Time Fixed Effects	Y	Y	Y	Y
Crime Controls	N	Y	N	N
Institutional Controls	N	N	Y	N
Social Capital Controls	N	N	N	Y
N	6316	6316	6316	6316
R^2	0.012	0.030	0.014	0.015

Standard errors clustered at the district level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Main Results: Rainfall

	(1)	(2)	(3)	(4)
	Probability of suicide		**Number of suicides**	
Panel A				
Rainfall (z-score)	-0.014**	-0.032***	-0.018	-0.062***
	(0.006)	(0.009)	(0.013)	(0.016)
Panel B				
Positive Shock	0.013	-0.002	0.025	-0.100
	(0.020)	(0.036)	(0.038)	(0.069)
Negative Shock	0.055*	0.145**	0.131*	0.298***
	(0.032)	(0.058)	(0.074)	(0.103)
Subdistrict Fixed Effects	Y	Y	Y	Y
Time Fixed Effects	Y	Y	Y	Y
District Trends	N	Y	N	Y
N	12632	12632	12632	12632

Standard errors clustered at the district level in parentheses. In columns 1 to 4 we control for time fixed effects as well as subdistrict fixed effects. In column 2 and 4 we also control for district-specific trends. Positive Shock takes value one for districts experiencing a positive shock in rainfall of at least 1.5 standard deviation above their long-run mean. Negative Shock takes value one for districts experiencing a negative shock in rainfall of at least 1.5 standard deviation below their long-run mean. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Baseline Balance: Randomized Experiment

	Treatment	**Control**	**P-value**
Probability of a suicide	0.45	0.39	0.278
Number of educational facilities	82.06	84.44	0.580
Number of health institutions	44.05	45.79	0.374
Asphalted road	0.65	0.66	0.717
Lighting	0.81	0.76	0.070*
Rural	0.95	0.93	0.173
Population Size	49687	49865	0.977
Number of families	13237	13268	0.999
% of households with electricity	0.67	0.66	0.875
N			280

Standard errors clustered at the district level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Robustness of main result: Roll-out - 2006 subdistrict boundaries

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					Probability of Suicide			
Treatment in 2007/2008	-0.060**		-0.052	-0.052*	-0.060**	-0.071***	-0.060**	-0.049*
	(0.024)		(0.039)	(0.027)	(0.024)	(0.027)	(0.025)	(0.029)
Treatment in 2010/2011	0.003		-0.078	-0.007	0.003	-0.003	0.003	0.014
	(0.035)		(0.067)	(0.038)	(0.035)	(0.036)	(0.036)	(0.039)
Treatment		-0.041*						
		(0.022)						
Control Mean	.517	.517	.517	.517	.517	.517	.517	.517
Subdistrict Fixed Effects	Y	Y	Y	Y	N	N	N	Y
Time Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
District Trend	N	N	Y	N	N	N	N	N
Controls × Post	N	N	N	Y	N	N	N	N
Controls	N	N	N	N	N	Y	N	N
District Fixed Effects	N	N	N	N	N	N	Y	N
Restricted counterfactuals	N	N	N	N	N	N	N	Y
N	7884	7884	7884	7270	7884	7270	7884	4502

Standard errors clustered at the district level in parentheses. In columns 1 and 2 we control for time fixed effects as well as subdistrict fixed effects. In addition, in column 3 we also include district trends. In column 4, we interact a set of baseline covariates with a Post indicator. In column 5 relative to the main specification we do not include subdistrict fixed effects, but control for treatment status at baseline. In column six we do not include subdistrict fixed effects, but control for a large set of control variables. In column 7 we include district fixed effects, but no subdistrict fixed effects. In column 8, we only use subdistricts who received the program until 2013 as counterfactuals.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Main Results: Randomized Experiment - 2006 subdistrict boundaries

	(1)	(2)	(3)	(4)	(5)	(6)
	Probability of suicide					
Treatment	-0.078*	-0.079*	-0.133**	-0.078*	-0.127**	-0.078*
	(0.043)	(0.044)	(0.065)	(0.043)	(0.057)	(0.044)
Control Mean	.476	.476	.476	.476	.476	.476
Subdistrict Fixed Effects	Y	Y	Y	N	N	N
Time Fixed Effects	Y	Y	Y	Y	Y	Y
District Trend	N	Y	N	N	N	N
Controls × Post	N	N	Y	N	N	N
Controls	N	N	N	N	Y	N
District Fixed Effects	N	N	N	Y	N	Y
Number of subdistricts	308	308	308	308	308	308
N	616	616	616	616	616	616

Standard errors clustered at the district level in parentheses. In column 1 we control for time fixed effects as well as subdistrict fixed effects. In addition, in column 2 we also include district trends. In column 3, we interact a set of baseline covariates with a Post indicator. In column 4 relative to the main specification we do not include subdistrict fixed effects, but control for treatment status at baseline. In column 5 we do not include subdistrict fixed effects, but control for a large set of control variables. In column 6 we include district fixed effects, but no subdistrict fixed effects.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Heterogeneous Effects

	Suicide
Panel A: Poverty Rates	
Treatment 2007/2008	-0.132***
	(0.040)
Treatment 2010/2011	-0.072
	(0.065)
Treatment 2007/2008 ×	0.072
High Poverty	(0.052)
Treatment 2010/2011 ×	0.086
High Poverty	(0.079)
High Poverty ×	-0.021
Post	(0.030)
Panel B: Per Capita Expenditure	
Treatment 2007/2008	-0.178**
	(0.071)
Treatment 2010/2011	-0.001
	(0.094)
Treatment 2007/2008 ×	0.000
Per capita Expenditure	(0.000)
Treatment 2010/2011 ×	-0.000
Per capita Expenditure	(0.000)
Per capita Expenditure×	-0.000***
Post	(0.000)
Panel C: Percentage of Farmers	
Treatment 2007/2008 ×	-0.001
Percentage of farmers	(0.001)
Treatment 2010/2011 ×	0.002
Percentage of farmers	(0.001)
Treatment 2007/2008 ×	-0.047
	(0.081)
Treatment 2010/2011 ×	-0.121
	(0.077)
Percentage of farmers ×	0.001*
Post	(0.001)
Subdistrict Fixed Effects	Y
Time Fixed Effects	Y

Standard errors clustered at the district level in parentheses. The specification includes both subdistrict (2000 boundaries) as well as time fixed effects. *HighPoverty* is an indicator variable taking value one for districts with above median poverty levels from 2007. Per capita Expenditure is the district-level mean per capita expenditure from 2007.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Heterogeneous Effects

	Suicide
Panel A: Health Institutions	
Treatment 2007/2008 × Health Institutions	-0.000
	(0.002)
Treatment 2010/2011 × Health Institutions	-0.003
	(0.003)
Treatment 2007/2008	-0.078
	(0.062)
Treatment 2010/2011	0.096
	(0.103)
Health Institutions × Post	-0.000
	(0.001)
Panel B: Education	
Treatment 2007/2008 × Educational Institutions	-0.000
	(0.001)
Treatment 2010/2011 × Educational Institutions	-0.001
	(0.001)
Treatment 2007/2008	-0.086
	(0.068)
Treatment 2010/2011	0.082
	(0.074)
Educational Institutions	-0.000
	(0.000)
Panel C: Population size	
Treatment 2007/2008 × Population Size	-0.000
	(0.000)
Treatment 2010/2011 × Population Size	-0.000
	(0.000)
Treatment 2007/2008	-0.085
	(0.051)
Treatment 2010/2011	0.056
	(0.062)
Population Size × Post	-0.000
	(0.000)
Subdistrict Fixed Effects	Y
Time Fixed Effects	Y
N	6316

Standard errors clustered at the district level in parentheses. The specification includes both subdistrict as well as time fixed effects. Health Institutions is a count variable indicating the number of health institutions at the subdistrict level. Similarly, educational facilities is a variables capturing the number of educational facilities in the subdistrict. Population size it the total number of individuals living in the subdistrict.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Heterogeneous Effects

	Suicide
Panel A: Crimes	
Treatment 2007/2008 × Number of crimes	0.003 (0.003)
Treatment 2010/2011 × Number of crimes	0.001 (0.004)
Treatment 2007/2008	-0.129*** (0.039)
Treatment 2010/2011	-0.036 (0.065)
Number of crimes × Post	0.002 (0.002)
Panel B: Number of Social Organisations	
Treatment 2007/2008 × Number of Social Organizations	0.000 (0.001)
Treatment 2010/2011 × Number of Social Organizations	0.001 (0.001)
Treatment 2007/2008	-0.096*** (0.034)
Treatment 2010/2011	-0.042 (0.045)
Number of Social Organizations × Post	-0.000 (0.000)
Panel C: Youth Organisations	
Treatment 2007/2008 × Number of Youth Organizations	-0.055 (0.071)
Treatment 2010/2011 × Number of Youth Organizations	-0.025 (0.172)
Treatment 2007/2008	-0.089*** (0.026)
Treatment 2010/2011	-0.020 (0.039)
Number of Youth Organizations× Post	-0.026 (0.030)
Subdistrict Fixed Effects	Y
Time Fixed Effects	Y
N	6316

Standard errors clustered at the district level in parentheses. Number of crimes is the number of crimes reported by all village heads in a given subdistrict. Similarly, youth and social organisations are all social and youth organisations reported by all village heads in a given subdistrict.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$