Income Shocks and Suicides: Causal Evidence From Indonesia*

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Abstract

We examine how income shocks affect the suicide rate in Indonesia. We use both a randomized conditional cash transfer experiment, and a difference-in-differences approach exploiting the cash transfer's nation-wide roll-out. We find that the cash transfer reduced yearly suicides by 0.36 per 100,000 people, corresponding to an 18 percent decrease. Agricultural productivity shocks also causally affect suicide rates. Moreover, the cash transfer program reduces the causal impact of the agricultural productivity shocks, suggesting an important role for policy interventions. Finally, we provide evidence for a psychological mechanism by showing that agricultural productivity shocks affect depression.

Keywords: Economic Shocks, Mental Health, Suicides, Cash Transfer.
JEL classification: D12, C21, I38.

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1 Introduction

Suicide is a pressing public health concern, causing 800,000 deaths per year globally (WHO, 2014). Suicidal behavior is one of the leading causes of death for people with severe mental illness (Hawton et al., 2013). While the effects of economic conditions on mental health and depression are well-established (Baird et al., 2011; Haushofer and Shapiro, 2016), little is known about the causal effect of economic well-being on suicidal behavior. More specifically, no causal evidence exists that examines whether positive income shocks, such as poverty alleviation programs, can decrease suicides.

It is difficult to credibly quantify the impact of improved economic well-being and, in particular, poverty alleviation programs on suicides. First, it is challenging to find suicide data at sufficiently low levels of geographic disaggregation for statistical power. Second, the timing and geographic placement of large government programs is usually endogenous, and even if small-scale programs demonstrate causality, external validity concerns predominate (Allcott, 2015; Deaton, 2010).

Our data and empirical setting address these difficulties. We focus on Indonesia, the world's fourth most populous country. We leverage unique Indonesian village-level census data from 2000 to 2014, and use several identification strategies to examine the causal effect of income shocks: First, we use a conditional cash transfer program, Program Keluarga Harapan (PKH), which provides households with yearly cash transfers worth about 10% of their pre-treatment annual consumption over six years. Using a difference-in-differences specification, we estimate the effect of the program's nationwide roll-out. The program targeted subdistricts with high poverty, sufficient supply-side institutions (health and educational institutions), and was launched in 2007 when it covered 13% of Indonesian subdistricts in seven provinces and was strongly expanded, reaching 57% of subdistricts in all 33 provinces in 2013. We also analyze a randomized controlled trial of the same program. Second, we exploit plausibly exogenous agricultural productivity shocks using rainfall variation and we examine whether the effects of the cash transfer vary systematically with these agricultural productivity shocks.

We establish several novel facts on the relationship between economic well-being and suicides. Our first finding is that both the cash transfer's roll-out and the randomized cash transfer cause a large reduction in suicides. Our preferred specification suggests that an average per-capita transfer of 22.45 USD targeted at the poorest 10 percent of households causes a decrease by approximately 0.36 suicides per 100,000 population per year. This corresponds to a reduction of the suicide rate by approximately 18 percent relative to the mean suicide rate (in 2011 and 2014) of 2 suicides per 100,000 in control subdistricts. Second, we show that agricultural productivity shocks, proxied by rainfall, significantly affect the incidence of suicide. A one-standard deviation increase in rainfall increases yearly per-capita consumption by 21.6 USD and decreases the number of suicides per 100,000 inhabitants by approximately 0.08, corresponding to a reduction by 6 percent relative to the average suicide rate in Indonesia between 2000 and 2014. For the subsamples affected by the income shocks, our calculations suggest that per-dollar effects on suicides identified using rainfall are significantly smaller than those from cash transfers. Third, we establish that cash transfers lower suicides most strongly in subdistricts experiencing negative agricultural productivity shocks. This is consistent with social welfare programs mitigating the adverse effects of negative economic shocks.

Finally, we provide evidence that economic hardship may ignite suicidal behavior by affecting people's mental health. The medical literature suggests that stress (Mann et al., 1999) and mental illness, such as depression, are major causes for suicides (Boldrini and Mann, 2015). We use panel data from the Indonesian Family Life Survey to show that agricultural productivity shocks causally affect farmers' mental health. A one-standard deviation increase in subdistrict level rainfall increases consumption by approximately seven percent, and reduces depression by 0.12 of a standard deviation. Moreover, the relationship between rainfall and depression is absent for non-farmers, indicating that the effects of rainfall operate through an economic channel.

We contribute to the literature on how economic and social circumstances affect suicides (Becker and Woessmann, 2018; Campaniello et al., 2017; Cutler et al., 2001; Daly et al., 2013; Ludwig et al., 2009; Stevenson and Wolfers, 2006). Concurrent work by

Carleton (2017) shows that, across Indian states, for temperatures above 20°C, a 1°C rise causes roughly 70 suicides per day, particularly during the agricultural growing season.[1] Our evidence supports the view that cash transfer programs' positive effects on recipients' mental health (Haushofer and Shapiro, 2016) outweigh negative spillovers of such programs (Baird et al., 2013).

Moreover, our paper contributes to the literature on poverty, income shocks, and mental health, the latter of which is usually measured through self-reported scales (Adhvaryu et al., forthcoming; Apouey and Clark, 2015; Baicker et al., 2013; Cesarini et al., 2015; Das et al., 2007; Devoto et al., 2012; Friedman and Thomas, 2009; Gardner and Oswald, 2007; Kling et al., 2007; Kuhn et al., 2011; Persson and Rossin-Slater, 2018; Stillman et al., 2009). In particular, previous papers examine the effects of randomized cash transfers on mental health through survey questions (Baird et al., 2013; Paxson and Schady, 2007). An exception is Haushofer and Shapiro (2016), who show that large, unconditional cash transfers can reduce cortisol levels, consistent with self-reported reductions in stress.

Our study advances the literature on poverty and mental health in three ways. First, we provide the first causal evidence on whether positive income shocks, and in particular government poverty alleviation programs, reduce suicides. Second, unlike previous evidence on income shocks and mental health which mostly relies on small-scale experiments, we use both a large-scale nation-wide roll-out of a conditional cash transfer program and a randomized evaluation of the same program. Finally, we provide the first evidence on the interaction between social welfare programs and agricultural productivity shocks on mental health, underlining the potential of government programs to alleviate negative consequences of adverse economic shocks.

We proceed as follows. In Section 2, we outline a conceptual framework, describe the data, and the cash transfer program. In Section 3, we present our identification strategies, and describe our results using both the cash transfer and agricultural productivity shocks. In Section 4, we examine mechanisms underlying our estimated treatment effects by

[1] There is also correlational evidence highlighting that recessions increase suicides (Barr et al., 2012; Chang et al., 2013; Reeves et al., 2012).

employing microdata on depression. In Section 5, we discuss effect sizes and we conclude in Section 6.

2 Conceptual Framework, Context, and Data

In this Section we describe the conceptual framework, the main features of the conditional cash transfer program, the suicide data, and the construction of the subdistrict panel. Finally, we report some basic descriptive statistics about Indonesian suicide rates.

2.1 Conceptual Framework

Related literature What are the theoretical links between economic well-being and suicides? Previous psychiatric disorders are the most important factor in explaining death by suicide; 90% of those who commit suicides had such a disorder (Cavanagh et al., 2003). Hawton et al. (2013) claim that specific mental health problems, like depression, are mainly responsible for the decision to commit suicides. Poverty and negative economic shocks are, in turn, associated with mental health disorders and depression in particular (Haushofer and Fehr, 2014; McInerney et al., 2013; Schilbach et al., 2016). Negative life experiences, such as loss of income and job loss, have also been identified as risk factors for suicide.[2]

Stress is a likely mechanism at play as suicide risk is correlated with abnormal cortisol concentrations, and a maladaptive cortisol response to stress (O'Connor and Nock, 2014). This is line with psychological models of suicide that emphasize that pre-existing medical conditions lead to suicidal tendencies when compounded by stress (Mann et al., 1999).[3]

[2] In addition, there is a literature emphasizing that suicides can be contagious, i.e. that the social environment plays an important role (Hedström et al., 2008). There are also literatures that examine how genes (Roy, 1992), social isolation (Appleby et al., 1999), and personality traits (Blüml et al., 2013) affect people's tendency to commit suicides.

[3] Joiner (2005) puts forward an interpersonal theory of suicide, which posits that the coexistence of high feelings of burdensomeness, low levels of belongingness, and the belief that these conditions are

The main object of interest in this paper is to understand how economic shocks affect the suicide rate. Positive economic shocks could affect the suicide rate directly by mitigating the consequences of negative live experiences or through improvements in mental health. In Section 4, we provide suggestive evidence in support of mental health as a channel by analyzing the relationship between economic shocks and depression.

Functional Form Our data and setting allow us to provide evidence on the functional form of the relationship between income and suicides. Models of reference-dependent preferences predict declining effects of permanent income shocks over time as individuals' reference points adapt to the income level.[4] Indeed, previous evidence suggests that the effects of permanent improvements in economic circumstances on self-reported mental well-being decline over time as individuals adapt to their new economic situation (e.g. Frederick and Loewenstein, 1999; Galiani et al., forthcoming). We test the predictions of models of reference-dependence by analyzing the dynamic treatment effects over the program duration of six years.

Are the marginal returns of income on mental health and suicide decreasing in income? To address this question, we shed light on the functional form between the suicide rate and income. If the relationship between the suicide rate and income is concave (convex) the impact of a positive income shock should be larger (smaller) if the recipients of the shock are poorer to begin with. If the relationship is linear there should be no heterogeneity by initial poverty levels. We test between different functional forms by analyzing treatment heterogeneity of the cash transfer program by the extent of initial poverty as proxied by agricultural productivity shocks and pre-treatment per-capita household consumption.

Types of Income Shocks Different types of income shocks may have different quantitative impacts on the suicide rate. The effect of an income shock likely depends on

hopeless to change, lead to the development of suicidal desires. Finally, social isolation and lack of social support consistently predict suicide risk (Appleby et al., 1999).

[4] Models in which the reference point fully adapts after one period predict that there is a fall in suicides only in the first year of a permanent income shock and no effects thereafter.

its effect on expected lifetime income and the uncertainty about lifetime income, that is its current consumption value. Therefore, predictable and regular cash transfers should have larger per-dollar impacts on the suicide rate than unpredictable agricultural income shocks, a prediction that we test in this paper.

2.2 The Conditional Cash Transfer Program

We use the Program Keluarga Harapan (PKH) conditional cash transfer program to analyze the impact on suicide rates (Banerjee et al., 2017; Cahyadi et al., 2018; World Bank, 2011). A pilot version of the PKH was introduced in 2007 for 600,000 households and the program was then expanded to cover 5.2 million households in 2014 with a target of 10 million recipients in 2018 (World Bank, 2011). The introduction of the PKH program was part of a wider effort to reform the Indonesian social safety system. In 2005 the Indonesian government removed universal fuel subsidies. To alleviate the immediate inflationary shock for poor and near-poor households, the Indonesian government introduced an unconditional cash transfer program covering 19 million households from 2005 to 2006. Furthermore, with the end of fuel subsidies already existing large-scale social assistance programs such as rice subsidies for poor households (Beras untuk Rakyat Miskin, or Raskin) and subsidized health insurance for the poor (Asuransi Kesehatan Miskin, Askeskin) were expanded. The PKH program was introduced in 2007 after the unconditional cash transfer ended to provide more targeted assistance to the poorest households (World Bank, 2012).[5]

PKH was designed to improve poor households' health and education through a cash transfer, conditional on their participation in health and education services (World Bank, 2011). The intervention's size is substantial: households received between 39 and 220 US dollars per year. The average received amount constituted about 10% of pre-PKH yearly household expenditure (80.82 US dollars at 2005 prices) between 2007 and 2014 (World

[5] PKH's coverage is significantly below of that of the other social benefit programs, but due to its targeted nature the World Bank considers it one of the most effective Indonesian social assistance programs (World Bank, 2017).

Bank, 2011, 2017). Households are part of the PKH program for up to six years.

The total cost of the PKH program from 2007 to 2014 was around 716 million USD at 2005 prices. In total there were 7.6 million household-years of cash transfer such that the average expenditure per household-year was 94 USD (World Bank, 2017). According to the 2011 village census, the average household size in Indonesia in 2011 was 3.6 individuals which implies an expenditure of 26.11 USD per treated individual and year. Subtracting administrative overhead households received, on average, 22.45 USD per capita.

Geographic Roll-Out of Program In Section 3.1 we exploit the roll-out of the PKH program for identification. The program was first implemented as a pilot program in 2007 in seven provinces: West Java, East Java, North Sulawesi, Gorontalo, East Nusa Tenggara (NTT), West Sumatra and DKI Jakarta. These provinces are quite diverse in terms of their poverty levels, and other economic and geographic characteristics. Because the program's focus is on poverty alleviation, upper income quintile districts were initially excluded from PKH eligibility, based on an index considering poverty rates, malnutrition and schooling records.[6] The 2007 roll-out of the program was randomized among selected eligible districts.

From 2008 to 2010, the program maintained its pilot status but was further rolled out in the following provinces: Nanggroe Aceh Darussalam, North Sumatra, Banten, South Kalimanten, West Nusa Tenggara, and the Yogyakarta Special Region. From 2010 onward, the Secretariat of the National Team for the Acceleration of Poverty Reduction (TNP2K), at the Office of the Vice-President, has been promoting the nation-wide expansion of PKH leading to all Indonesian provinces being covered by 2012. Of the subdistricts included in our analysis, the PKH program covered about 13% of all Indonesian subdistricts when it started in 2007. By 2013, 57% of all Indonesian subdistricts in our sample were in receipt of the program.

[6] Districts receiving the rural community-driven development project were eligible for the community cash transfer program (PNPM Generasi), and therefore were ineligible for the PKH program during the pilot operation.

At the macro level, targets for overall recipient numbers and total expenditure were set at the national level which determined the overall speed of the roll-out. At the micro level, target subdistricts were determined in cooperation with provincial and district governments who made recommendations. The final decisions took into account three main factors: subdistrict poverty levels, existence of the necessary supply-side institutions (e.g. educational institutions and health centers), and the willingness of local partners to cooperate.

Randomized Experiment In Section 3.2 we use a subsample of subdistricts in which the treatment status was randomly assigned and which formed the basis for the World Bank's evaluation of the program (World Bank, 2011). A total of 736 subdistricts were included in the sample, with 438 subdistricts randomized to the treatment group (Cahyadi et al., 2018). Out of these, we observe treatment assignment for 360 subdistricts that were randomly chosen for data collection by the World Bank (180 treatment, 180 control). Political pressures and a consequent unexpected program expansion in East Java resulted in deviations of the realized allocation from the intended one.[7] To deal with this contamination, we use the original treatment assignment to measure the conditional cash transfer program's impact on suicide.

Beneficiary Selection At the subdistrict 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 initially used to construct the list of eligible households per village. Approximately 30-40 percent of beneficiaries from the unconditional cash transfer program

[7] In particular, 37 out of the 360 subdistricts that were supposed to be part of the control group received PKH funds before 2011. 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 subdistricts also affected household responses. The contamination increased further leading to 30 percent of control subdistricts receiving the program by 2014.

were not included in the list of eligible households.[8] Based on this list of households, demographic data was used to identify eligible households that fulfilled one of the following program criteria: (i) households with pregnant and/or lactating women; (ii) households with children aged 0-15 years; (iii) households with children aged 16-18 years who have not yet completed 9 years of basic education. However, only the subset of eligible households with the lowest predicted consumption were included in the program. In the end, approximately 10% of households received the program. The classification was based on proxy-means tests of all households on the list of eligibles to identify program beneficiaries. The proxy-means tests consisted of 29 variables, including housing characteristics, education levels, sources of fuel, employment information, and access to health and education services.

2.3 Data

We use the censuses of all Indonesian villages (PODES) from 2000, 2003, 2005, 2011, and 2014 to examine the PKH program's effect on suicide rates. The PODES data covers all 80,000 villages in Indonesia. In the village census, village heads report village characteristics, such as population size, the presence of health and educational institutions, and data on the percentage of farmers.

Outcome Definitions The census contains data on suicides at the village level. In 2000, 2003 and 2005 the village head was asked whether any suicide occurred in their village in the previous year. In 2011 village heads were asked about the number of suicides committed in the last year. Finally, in 2014 the PODES survey asked for the number of suicides and suicide attempts in the village last year. Village level population data is available from 2000 to 2011, but not for 2014. To obtain population data for 2014, we extrapolate population size using a liner trend from the years 2005 and 2011 to 2014 at the subdistrict level.

[8] Statistics Indonesia also conducted additional interviews in targeted subdistricts to identify newly poor households, in an attempt to minimize exclusion errors.

We use the PODES census data to construct our main outcome measure of interest: the number of suicides per 100,000 inhabitants at the subdistrict level. For the years 2000, 2003 and 2005 (all prior to the cash transfer program), we use the number of villages with at least one suicide per 100,000 inhabitants as a proxy for the actual suicide rate. For the PODES 2011 data we directly use the number of suicides per 100,000 inhabitants at the subdistrict level. Lastly, we define the suicide rate in 2014 as the number of suicides and suicide attempts per 100,000 inhabitants.

We also construct a measure of the suicide rate whose definition does not vary across years (except for 2014 when the question included suicide attempts). Specifically, we extrapolate the expected suicide rate from the subdistrict mean of village level occurrences of at least one suicide. We rely on two assumptions for this exercise: First, we assume that suicides are Poisson-distributed. Second, we assume that suicides in all villages of the same subdistrict are independent and have the same Poisson parameter λ. Under these assumptions we can use the mean incidence of at least one suicide ($\bar{s}$) to calculate the expected number of suicides $E_v(s)$ in a given village as $E_v(s) = -\ln(1 - \bar{s})$.[9] To calculate the expected number of suicides at the subdistrict level we multiply $E_v(s)$ by the number of villages in a subdistrict. We validate this measure with data on the actual number of suicides in the years 2011 and 2014 and find a correlation of $\rho = 0.95$. Moreover, in Section 3.4 we show that our results are robust to using different outcome measures.

According to the WHO Health Data repository the age-standardized suicide rate in Indonesia stood at around 3 per 100,000 in 2015. This seems relatively well-aligned with our data where we find mean raw suicide rates of approximately 2 per 100,000 at the subdistrict level in 2011 and 2014, respectively.[10]

[9] This is derived from the cumulative distribution function of the Poisson Distribution: $CDF(k) = e^{-\lambda} \sum_{i=0}^{\lfloor k \rfloor} \frac{\lambda^i}{i!}$. We observe the fraction of villages with zero reported suicides, which gives a subdistrict specific estimate of $CDF(0)$. Expressed as fraction of villages with at least one suicide we get: $CDF(0) = 1 - \bar{s}$. Using the functional form of the Poisson distribution we obtain $E_v(s) = \lambda = -\ln(1 - \bar{s})$. There is one subdistrict in 2014 where all villages have at least one suicide or suicide attempt. For this observation we use the average subdistrict λ over the preceding four years.

[10] The slight discrepancy between those numbers can be explained by the fact that the WHO estimates

Indonesia has low suicide rates from an international perspective. In a WHO worldwide ranking of all nations by suicide rates, Indonesia ranked 173 out of 183 nations. This low baseline suicide rate could imply stronger social norms against suicides in Indonesia. These norms might not only affect the level of the suicide rate, but also the elasticity of suicides with respect to economic shocks. However, it is theoretically unclear how these norms would affect the relationship between the suicide rate and economic circumstances. On the one hand, decisions to commit suicide might be more marginal than in other countries, that is a small improvement in economic circumstances could prevent more suicides. On the other hand, it is possible that because of the strong stigma non-economic factors play a more important role for suicides in Indonesia.

Subdistrict Panel Construction Since our key identifying variation is at the subdistrict level, we aggregate our village panel at the subdistrict level, and collapse our observations at the subdistrict boundaries from 2000. We use 2000 subdistrict boundaries as 2000 is the first year for which suicide data is available.[11] For practical reasons we construct our panel at the subdistrict level as the number of administrative units in Indonesia substantially increased over time.[12] In the experimental sample there are 314 subdistricts according to 2000 boundary definitions. In our analysis we only make use of 310 subdistricts for which we can construct a cross-walk between 2000 and 2014. For the roll-out of the program and the analysis of agricultural productivity shocks (presented in

are model-based and only partially take into account micro-data on suicides in Indonesia.

[11] We use 2000 borders to provide a consistent interpretation of coefficients for all our subdistrict level specifications. Furthermore, using subdistrict border definitions from after 2000 would complicate both the presentation of pre-trends and the analysis of interactions between agricultural productivity shocks and cash transfers (presented in Section 3.1 and 3.6). We show that our results are robust to using the 2006 subdistrict borders (Tables A.8 and A.14).

[12] Decentralization reforms beginning from 1998 significantly increased the proliferation of administrative units. For example, the number of districts increased from 302 in 1999 to more than 500 in 2014 (Bazzi and Gudgeon, 2018). The number of subdistricts increased from about 3000 in the late 1990s to approximately 7000 in 2014. 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.

Section 3.4) we employ the universe of Indonesian subdistricts for which we could construct a consistent panel between 2000 and 2014. We were able to construct such a panel for 3138 out of all 3928 subdistricts according to 2000 boundary definitions. The panel's construction was based on a subdistrict-level crosswalk for the time period of 2000 to 2014. Owing to the subdistrict-splits, there are 1485 cases in which a subdistrict split from 2000 to 2014. If only a part of the 2000 subdistrict received the cash transfer in a given year, we define the treatment indicator as the fraction of new subdistricts receiving the PKH program. When we drop the observations with partially treated origin subdistricts our estimated treatment effects barely change (see Tables A.6 and A.12).

Descriptives and Correlates of Suicide Rates Table 1 displays descriptive statistics at the subdistrict level from the PODES 2005 data, before the cash transfer was implemented. On average, a subdistrict consists of 17.6 villages and has a population of approximately 56,000. Most subdistricts are relatively rural, with on average 74% of villages classified as rural and 56% of the population working as farmers. Thus, agricultural productivity shocks are likely to affect large parts of the population. The RCT sample is more rural than the average, but otherwise is similar to non-RCT subdistricts.

[**Insert Table 1**]

Second, we exploit baseline data from the 2005 census to characterize the correlates of suicides. We find a strong economic gradient in suicide rates. Subdistricts in districts with a 10% larger share of the households below the poverty line have, on average, a 0.142 higher suicide rate per 100,000 people (see Column 1 of Table A.1 in the Online Appendix). The same pattern is apparent when we consider per capita expenditure at the district level. Moreover, the share of farmers at the subdistrict level is strongly positively correlated with suicide rates. The share of farmers at the subdistrict level remains significantly correlated with suicide rates after controlling for local crime rates, and health, education, and social institutions.[13] Table A.1 also reveals that crime per

[13] Since the share of farmer variable is available at the subdistrict level, this may explain why it remains statistically significant, while per capita expenditure and fraction poor which are measured at the

capita is weakly positively related to suicide incidence, but that social organizations per capita and educational institutions per capita are not correlated with suicide rates.[14]

Finally, we observe an increase in the occurrence of any suicide in subdistricts over time. Specifically, the incidence of at least one suicide at the subdistrict level increased from 21 percent in 2000 to 45 percent in 2005 and 52 percent in 2011 and 2014.[15,16]

3 Main Results

In this Section, we first present evidence from the difference-in-differences approach using the population-wide roll-out of the cash transfer program. Then we show results of a randomized controlled trial of the same program. Thereafter, we examine the dynamics of treatment effects, and assess the robustness of our findings. Finally, we study how agricultural productivity shocks affect suicide rates, and examine how they interact with the roll-out of the cash transfer program.

3.1 Nation-Wide Program Roll-Out

We provide evidence that the PKH conditional cash transfer program substantially decreased the suicide rate using a difference-in-differences approach exploiting the nation-wide roll-out of the program. For our main specification we use suicide data from the census of villages from 2005, 2011, and 2014, but we also employ data from 2000 and 2003 to assess robustness and pre-trends. Our dependent variable is the number of suicides

district level, become statistically insignificant.

[14] We find that health institutions per capita are positively correlated with suicide rates, consistent with the government targeting health care provision to more needy subdistricts.

[15] The increase in reported suicides between 2000 and 2005 could be the result of shifting norms around suicides, potentially affecting the willingness to report suicides by the village chiefs. However, such a shift of norms can only explain our results if it occurred differentially in subdistricts with and without the cash transfer program and in subdistricts with and without agricultural productivity shocks.

[16] Comparing other measures of suicides over time is complicated by changing survey questions and changes in the number of villages per subdistrict.

per 100,000 individuals (y_{st}) in subdistrict s and at time t.

To estimate treatment effects, we include subdistrict fixed effects (α_s), time fixed effects (ϕ_t), and a treatment indicator, Treat_{st}, taking value one when a subdistrict started receiving the program.[17] 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. We estimate all of our main specifications with OLS and employ population weights from 2005.[18] Our specification of interest is:

$$y_{st} = \delta_1 \text{Treat}_{st} + \alpha_s + \phi_t + \varepsilon_{st} \tag{1}$$

Our main coefficient of interest is δ_1, which provides us with the treatment effect for the subdistricts that had started receiving the program at the time of the data collection. Column 1 of Table 2 shows that receiving the cash transfer program of on average 22.45 USD per year reduces the number of suicides per 100,000 inhabitants by 0.36. This corresponds to a reduction by approximately 18 percent relative to the control mean in 2011 and 2014. Our estimates remain economically and statistically significant when (i) clustering errors at the subdistrict level (Column 2), (ii) excluding the sample of subdistricts with randomized treatment assignment employed in Section 3.2 (Column 3), including all pre-treatment periods (Column 4) and including subdistrict specific time trends (Column 5). Column 6 displays the treatment effects that give equal weight to the subdistricts regardless of their population size in 2005. This reveals an estimate of -0.591 suicides per 100,000 people, suggesting that the suicide reductions are larger in subdistricts with smaller population sizes.[19]

[17] For subdistricts that split up over time, the treatment variable indicates the fraction of subdistricts (based on the 2000 boundary definitions) that receive the treatment.

[18] We use population weights since the welfare relevant metric of interest are changes in the overall Indonesian suicide rate and not the average subdistrict suicide rate. Furthermore, the suicide rate is measured with less error in larger subdistricts which increases the precision of our estimates.

[19] Our results are also robust to using district fixed effects instead of subdistrict fixed effects, controlling for district trends, including controls in a specification without subdistrict fixed effects, and allowing for differential trends by baseline covariates (Table A.9).

[**Insert Table 2**]

A key assumption underlying the difference-in-differences approach is that treatment and control subdistricts are on parallel trends. Figure 1 provides evidence supportive of the common trend assumption.[20] It displays pre-trends relative to the timing of the introduction of the conditional cash transfer program by treatment wave. Differences in the first period before the treatment are normalized to zero. The displayed coefficients are the difference-in-differences treatment effect estimates compared to subdistricts that had not received the PKH program until 2013. Moreover, as shown above, the effects remain both economically and statistically significant after controlling for subdistrict-specific trends. All in all we find consistently large negative and significant effects across a series of specifications. While we have provided evidence in support of the parallel-trend assumption, there is no formal test of the validity of this identification assumption.

[**Insert Figure 1**]

3.2 Randomized Controlled Trial

In this subsection, we use a subset of subdistricts in which the treatment was randomly assigned to test whether the experimental treatment effect estimates are in line with the non-experimental roll-out analysis from the previous section.

Balance As a first step, we test whether the treatment and control group are balanced in terms of observables. Let T_s denote the PKH program's original allocation, where $T_s = 1$ if the subdistrict was randomly assigned to receive the program, and $T_s = 0$ otherwise. We consider whether baseline balance holds for the original treatment assignment by comparing means and clustering standard errors at the subdistrict level. In Table A.2,

[20] We also provide further analysis consistent with parallel trends. Figure A.1 displays the evolution of mean suicide rates over time. Table A.3 shows that pre-trends are uncorrelated with timing of entry (Column 1) and that the level of the suicide rate in the period before a subdistrict received the program does not drive the treatment effects (Column 2).

we provide evidence of baseline balance on a set of observables. We cannot reject the null hypothesis of global balance ($p = 0.57$).

Results We estimate treatment effects using the randomized assignment of the cash transfer program, clustering standard errors at the subdistrict level and weighting observations by population size in 2005.[21] Despite the randomization of cash transfers at the subdistrict level, we find some evidence that treated subdistricts were on an upward trend compared to control subdistricts (Figure A.4) and some gap in the suicide rate before the program was launched (Table A.2). As a result of the common trend violation and the slight baseline imbalance, difference-in-differences estimators might be upward biased and a more conservative way of evaluating treatment effects is to employ an ANCOVA and a post-estimator.

The ANCOVA specification in Column 1 of Table 3 shows that subdistricts randomly assigned to receive the same conditional cash transfer program as in Section 3.1 have, on average, a 0.337 lower suicide rate (about 19% of the control mean in 2011). Comparing mean suicide rates between treatment and control subdistricts in 2011 also yields an insignificant average decrease of 0.258 suicides per 100,000 (Column 2). While the size of the effect is economically meaningful and of very similar magnitude to the roll-out estimates, it is statistically insignificant. We attribute this lack of significance to low statistical power.[22]

We also estimate the effect of the RCT using difference-in-differences specifications and find mostly significant and stronger negative treatment effects (Columns 3 to 6 of

[21] We do not use the 2014 census in this section. While contamination of the randomization was quite low in 2011 (with 10 percent of subdistricts having an actual treatment status differing from the randomly assigned one), the contamination of the program strongly increased over time. By 2014, 30 percent of control subdistricts received the program. Moreover, the cash transfer ended in 2012 and 2013 so that only a subset of subdistricts was still receiving the program during the relevant period from PODES 2014. We discuss the long-run effects of the PKH program in Section 3.3.

[22] Ex-post power calculations show that we had 80% power to detect a 0.770 effect size at the five percent level.

Table 3) confirming that the cash transfer program reduced suicides. However, given the pre-trend violation and slight baseline imbalance we think that the effect sizes of the ANCOVA and post-estimator are more credible. All in all, the results of the RCT are in line with the roll-out results. The key difference between the two strategies is that the results from the roll-out are much more precisely estimated as they are based on a 10 times larger sample size than the estimates from the RCT.

[**Insert Table 3**]

3.3 Dynamics of Treatment Effects

The cash transfer program had persistent treatment effects throughout the six-year duration of the program. Figure 2 plots the evolution of the estimated treatment effect of the PKH program on suicide rates over time. To estimate the plotted coefficients we exploit the fact that the census data-collection happened at different points in time relative to the beginning of the treatment for different subdistricts. This means that each point-estimate is obtained comparing a different sample of treatment and control subdistricts.[23] Treated subdistricts in a given period t are defined as having received the cash transfer program exactly t before (or after) the census. For $t \geq 0$ control subdistricts are defined as subdistricts that had not received the treatment at the time of the census. For $t < 0$ control subdistricts are defined as not having received the treatment t years after the census. This also leads to differences in sample size and precision in the estimation of treatment effects as apparent by variation in the width of confidence intervals.

A clear temporal pattern emerges from Figure 2. Similar to the roll-out analysis there are no detectable difference between treatment and control subdistricts in the years prior to receiving the cash transfer program. However, starting in the year subdistricts first receive the treatment cash transfer program the suicide rate declines by about -0.3 in line

[23] We do observe some treated subdistricts at two different points in time which allows us to directly compare treatment effects over time (Table A.16). The treatment effects for the subsample of treated subdistricts we observe twice are in line with the results from the overall sample.

with the aggregate analysis. The difference in the suicide rate persists throughout the six-year duration of the cash transfer program without any obvious changes in the effect size.[24] The treatment effect also persists into year seven after the treatment potentially suggesting a persistence beyond the receipt of the program. However, the periods are defined in calendar years so that we cannot rule out that persistence is driven by subdistricts who started receiving the program in late 2007 still receiving the program at the time of the census data collection in early 2014.

These findings also imply that the duration of exposure to the treatment does not affect the estimated effects of the program. To more formally test this we run a regression in which we include a treatment indicator as well as a variable on the number of years of treatment received by the subdistrict. Duration of receipt is not significantly related to suicide rates, and barely affects the coefficient on whether a given subdistrict received the program (Table A.18). All in all, we find comparable effect sizes for short-run and medium-run effects of the program. This finding is inconsistent with a model of reference dependence over past income levels which predicts declining effects on suicides as soon as people's reference point adjusts to the new income level.

[Insert Figure 2]

3.4 Robustness: Outcome Definitions

One concern with our analysis could be that the definitions of the suicide rate change over time. We provide three pieces of evidence that demonstrate the robustness of our results to using less rich but time-invariant measures of subdistrict suicides.

First, we employ a measure of the suicide rate using the Poisson extrapolation described in Section 2.3. Using this extrapolated suicide rate as outcome for all years we see the same treatment effect patterns with similar effect sizes (Columns 1 to 5 of Table A.4 and Figures A.6 and A.7). Second, we use a version of the suicide rate based on the

[24] This finding is in line with the evidence of constant and persistent effects of the PKH program on child health, education, and the prevalence of child labor (Cahyadi et al., 2018).

number of villages in a given subdistrict that report at least one suicide. This is effectively a truncated version of our main outcome variable. Our treatment effect estimates with this outcome definition remain largely unchanged (Columns 6 to 10 of Table A.4). Third, we employ a binary variable indicating whether any suicide occurred in a given subdistrict-year. The treatment effects are qualitatively similar to our main specification (Columns 11 to 15 of Table A.4). Our preferred specification implies that receiving the cash transfer reduces the likelihood of at least one suicide by 6 percentage points or 12 percent. Therefore, changes in the survey structure over time do not seem to affect our results.

Another concern could be that the cash transfer program affects migration patterns, and thereby shapes our treatment effect estimates. To test whether these potential changes in migration affect our results, we estimate treatment effects on two further outcomes not subject to this bias. First, we construct the suicide rate per 100,000 inhabitants using 2005 population for all years. This definition of the suicide rate is unaffected by changes in migration induced by the cash transfer. We find that with this measure treatment effects on the suicide rate are still highly significant (Columns 1 to 5 of Table A.5). Second, we use the number of suicides as the outcome variable.[25] Again, the treatment effect patterns remain largely unchanged. The point estimate of our preferred specification indicates that the cash transfer program decreased the number of suicides per subdistrict by 0.2 (Column 11 of Table A.5). This set of results suggests that changes in migration do not drive our main results.[26]

[25] For this analysis we stick with the definition used for our main outcome variable and use the actual number of suicides when available. The results remain very similar when we use the number of villages with at least one suicide as outcome (results available upon request).

[26] The results for the RCT are also robust to changing outcome definitions, though the results are more noisily measured (Tables A.10 and A.11).

3.5 Agricultural Productivity Shocks and Suicides

Our evidence from previous sections shows that a positive economic shock, the receipt of a conditional cash transfer, can lower suicide rates. In this section, we examine whether agricultural productivity shocks, as measured by rainfall, also affect suicide rates.

Rainfall Analysis The rainfall analysis has at least two advantages compared to the analysis of the cash transfer program: First, it enables us examine whether positive and negative income shocks have symmetric effects on the incidence of suicide. Second, it allows us to retrieve estimates with no concerns regarding differential social desirability bias between the treatment and the control group.[27]

Data Our empirical strategy relies on the following two facts: First, the agricultural sector in Indonesia is to a large extent governed by seasonal monsoon rainfall. Second, Indonesian rainfall exhibits substantial variability within a given year across subdistricts as well as within subdistricts over time. To examine the causal effect of agricultural productivity shocks, we leverage the ERA-Interim Reanalysis dataset which provides precipitation data from 1979 until 2016 on a 0.25×0.25 degree resolution (roughly a 27.5×27.5 kilometer grid at the equator). We define rainfall at the subdistrict level as weighted average rainfall at the five grid points closest to the geometric center of the subdistrict.[28] Each grid point is weighted with the inverse of the squared distances to the subdistrict center. Reanalysis data is based on a mix of real weather observations (station and satellite data) and an atmospheric climate model. 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. The rainfall data is matched to the 2000 subdistrict boundaries. We use suicide data from the 2000, 2003, 2005, 2011

[27] One may be concerned that village heads whose villages are in receipt of the PKH program report more favorable outcomes, but this critique does not apply to the rainfall analysis.

[28] We lack coordinates for 14 subdistricts. We use average rainfall and coordinates of other subdistricts in the same district for these subdistricts. For one district all subdistricts have missing coordinates. For those we use province level average rainfall and coordinates.

and 2014 waves of the Indonesian village census. As before, our main outcome variable of interest, y_{st}, is the suicide rate in a given subdistrict, s at time t.

Specification and Results We follow the recommendation in Levine and Yang (2014) and Maccini and Yang (2009) and calculate rainfall in a particular year by focusing on rainfall in complete wet seasons (rather than in calendar years). As in Maccini and Yang (2009), we define rainfall, $zrain_{st}$, as the normalized deviation of rainfall from the long-term mean within a given subdistrict.[29] This measure of rainfall has been shown to significantly and strongly predict 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. As rainfall is heavily spatially correlated, we report Conley (1999) standard errors allowing for arbitrary spatial and temporal correlation of error terms in a 100km radius around the subdistrict center (we also report standard errors clustered at the district level). We estimate the following equation, using 2005 population to weight the subdistrict observations:[30]

$$y_{st} = \gamma_1 \text{zrain}_{st} + \alpha_s + \phi_t + \varepsilon_{st} \tag{2}$$

Table 4 provides evidence that higher rainfall significantly reduces suicides. In Column 1 we show that increases in subdistrict-rainfall by one standard deviation from the long-run subdistrict mean lowers the suicide rate by 0.08. In Column 3 we include subdistrict trends, to rule out that differential trends can explain our findings. We find that our results are virtually unchanged by the inclusion of trends, and if anything, become somewhat stronger.[31] Column 5 assesses the sensitivity of our estimates to also

[29] We use rainfall data from 1979 to 2016 to construct the subdistrict specific leave-one-out long-run means and leave-one-out long-run standard deviations. Our results are robust to constructing the rainfall variable in different ways. As Indonesia is located around the equator, temperature is relatively constant and therefore does not have effects on agricultural yields (Kleemans and Magruder, forthcoming).

[30] We use 2005 population weights to make the analysis comparable to the cash transfer estimates.

[31] We also estimate the impact of a detrended (at the subdistrict level) measure of rainfall on the suicide

controlling for the first, second, and third lag of rainfall. This leaves our estimated coefficients largely unaffected. Our estimated coefficients increase further both in economic and statistical significance once we give equal weight to subdistricts, i.e. once we do not weight by population size (see Table A.20). This most likely reflects that subdistricts with lower population size are more strongly economically affected by agricultural productivity shocks.

Table A.19 tests whether the relationship between positive and negative rainfall shocks and the suicide rate is approximately symmetric. To do so we augment the above equation by two dummy variables: posshock_{st}, taking value one for subdistricts experiencing a positive shock in rainfall (top one third of the standardized rainfall distribution in our sample) and negshock_{st}, taking value one for districts experiencing a negative shock in rainfall (one third percent of the standardized rainfall distribution in our sample). Then we estimate the following equation:

$$y_{st} = \beta_1 \text{zrain}_{st} + \beta_2 \text{posshock}_{st} + \beta_3 \text{negshock}_{st} + \alpha_s + \phi_t + \varepsilon_{st} \tag{3}$$

We find little evidence of asymmetric responses to shocks. While the absolute value of the point estimates for β_2 are slightly larger than the estimates of β_3, we cannot reject that they are of equal size. In Figure 3 we non-parametrically assess the relationship between rainfall and suicide. To do so, we partial out time fixed effects, subdistrict fixed effects, and subdistrict-trends from both suicide rates and the rainfall measure. Then we use the predicted residuals from these regressions to run local polynomial regressions between these residuals. Figure 3 highlights a strong negative relationship between rainfall and the suicide rate, confirms our previous result that the responses to positive and negative rainfall shocks are fairly symmetric, and that the overall relationship is approximately linear. However, at the top end of the rainfall distribution we observe a slight flattening of the relationship, potentially indicating a concave relationship between income and the

rate (Tables A.22 and A.23). The results are qualitatively in line with the results from the main specification.

suicide rate.

3.6 Cash Transfers and Agricultural Productivity Shocks

Do the cash transfers mitigate the adverse effects of agricultural productivity shocks on suicides? If economic hardship caused by negative economic shocks affects suicide rates, then we would expect cash transfers to more strongly reduce suicide rates in the face of negative economic shocks. We test whether there are significant interactions between receiving the cash transfer, Treat_{st}, and rainfall, zrain_{st}. We leverage the suicide data from 2000 to 2014, and estimate the following equation, reporting Conley standard errors and standard errors clustered at the district level:[32]

$$y_{st} = \gamma_1 \text{zrain}_{st} + \gamma_2 \text{Treat}_{st} + \gamma_3 \text{zrain}_{st} \times \text{Treat}_{st} + \alpha_s + \phi_t + \varepsilon_{st} \tag{4}$$

Our key coefficient of interest is γ_3. Column 2 of Table 4 reveals that there is a significant interaction effect, consistent with the idea that cash transfers are more (less) effective at lowering suicides in years with lower (higher) agricultural productivity. Our estimates imply that cash transfers lower suicides by 0.3 suicides per 100,000 inhabitants in a year with subdistrict rainfall one standard deviations below its long-run mean, but only lower suicides by 0.1 suicides per 100,000 inhabitants in a year with subdistrict rainfall one standard deviations above its long-run mean.

These effects become slightly smaller and statistically insignificant, but remain economically meaningful, once we control for subdistrict trends. This most likely reflects the limited power for studying interaction effects after controlling for subdistrict trends. The estimated effects of the interaction between the cash transfer and rainfall are much stronger in specifications that do not use population weighting (see Table A.20). This stems from the fact that rainfall more strongly impacts incomes in more rural subdistricts which have a lower population size. We also analyze whether the effect of the cash transfer separately for positive and negative rainfall shocks in Table A.21. While this

[32] As in all other main specifications we use population size from 2005 to weight the observations.

analysis is limited by lower statistical power, the estimates suggest that the cash transfer program reduces the effects of both positive and negative agricultural productivity shocks symmetrically.

The observed heterogeneity in treatment effects suggests that social welfare programs can dampen the effects of negative and positive economic shocks. Put differently, the cash transfer program breaks the relationship between agricultural productivity shocks and suicides.[33] The finding of a positive interaction effect between rainfall and the receipt of the cash transfer suggests that the relationship between the suicide rate and income is concave and not linear or convex.

Further Heterogeneity Do other covariates predict heterogeneous responses to the cash transfer program? There is no statistically significant heterogeneity by any predetermined characteristics (fraction of farmers, fraction poor, per capita expenditure, per capita crimes, per capita social institutions, and per capita health institutions; see Table A.27). However, our effective statistical power to detect statistical differences in treatment effects across groups is quite limited (see the minimum detectable effect sizes in Table A.27).

4 Mechanism

In the next section, we provide suggestive evidence in favor of depression as a channel through which economic circumstances could affect people's inclination to commit suicides. In particular, we show that economic shocks directly affect a measure of depression, in line with the framework in Section 2.1. Finally, we examine the importance of several potential subdistrict-level mediators.

[33] This interaction results suggest that the effects of the cash transfer are not driven by differential social desirability bias between the treatment and the control group.

Economic Shocks and Depression: Micro-Evidence To provide evidence that economic circumstances affect people's inclination to commit suicides through changes in mental health, we use unique individual-level data on depression from the Indonesian Family Life Survey (IFLS). The IFLS waves 4 and 5 (in 2007 and 2014) administer a ten question version of the CES-D depression scale (Radloff, 1997). The raw CES-D score ranges from 0 (not depressed) to 30 (severely depressed).[34] Moreover, we leverage rich data on household expenditure available for all five waves of the IFLS (1993, 1997, 2000, 2007, and 2014).

As in Section 3.5, we exploit agricultural productivity shocks to study the effects of economic circumstances. Specifically, we assess the effects of rainfall, zrain_{st}, on depression, depression_{ist}, as measured by the CES-D score and monthly per captita household expenditure, $\exp_{ist}$, in levels and logs. Our object of interest are households with at least one agricultural worker, the group of households whose income is most strongly dependent on rainfall.[35] We employ the same subdistrict-level rainfall measure as in Section 3.5, and also report both Conley standard errors and standard errors clustered at the district level. We also include individual level fixed effects, α_i, in our regression which allows us to control for time-invariant individual-specific unobservables. Specifically, we estimate the following equation:

$$\text{y}_{ist} = \delta_1 \text{zrain}_{st} + \alpha_i + \phi_t + \varepsilon_{ist} \tag{5}$$

Table 5 shows that a one-standard deviation increase in rainfall increases monthly per

[34] The literature estimates that 80 percent of suicides are committed by individuals with a CES-D score above 9 (Cheung et al., 2007). This suggests that depression could be a key mechanism through which economic shocks alter people's inclination to commit suicides. Individuals who received the PKH cash transfer in 2014, had higher depression scores in 2007 (0.14 standard deviation; see Table A.25).

[35] Households count as "working in agriculture" if any household member works in agriculture either in self-employment (without permanent employees), or as a casual or family worker in any of the five IFLS waves. There is no sector information for IFLS Wave 1. We therefore use working "in self-employment without permanent employees" or working as "temporary worker" as proxies. The sample is restricted to individuals observed in all 5 IFLS waves.

capita consumption of farmers by 40,000 Rupiah (Column 1), monthly log consumption by 6.9 percent (Column 2)[36], and decreases depression by 0.12 standard deviations (Column 3). Columns 4 to 6 of Table 5 also provide evidence that the rainfall shocks operate through an economic mechanism by showing that both depression and consumption of non-farmers do not respond to rainfall shocks. This suggests that our estimated effects on depression do not operate through direct effects of weather on mental health. Indeed, the coefficients on the effects of rainfall on log consumption and mental health are statistically different between farmers and non-farmers ($p < 0.06$).[37] We also use the individual level data to study heterogeneous effects of agricultural productivity shocks on depression. We find that depression scores of individuals with higher baseline expenditure are more strongly affected by rainfall shocks (Table A.24). This is consistent with the finding that cash transfers more strongly reduce suicides in the presence of negative agricultural productivity shocks and suggests a concave relationship between depression and income. The effects are also significantly larger for women and below median age individuals.

Mediation Analysis What other factors could account for the effects of the cash transfer program on suicide rates? The cash transfer increased recipients' welfare by increasing their consumption and improving their health outcomes (World Bank, 2011). Guided by this, we examine several potential subdistrict-level mediators, including local crime rates, health, and education institutions, and social organizations through which the cash transfer program could lower the incidence of suicides. Therefore, we include time-varying endogenous controls at the subdistrict level in our main specification of interest. These controls could have been affected by the cash transfer in systematic ways and therefore act as a channel through which our treatment effects operate. However, we find little evidence that any subdistrict level institutions mediate our results. Indeed, the treatment effect estimates hardly move when the potential mediators are included (Table A.26). This mediation analysis is limited by the fact that we have to rely on subdistrict

[36] A 6.9 percent increase over the median per-capita consumption across all years corresponds to ca. 18,000 Rupiahs or roughly 1.8 USD at 2005 prices.

[37] The difference in effects on consumption levels are marginally insignificant ($p = 0.162$).

level mediators, and points to the importance of individual level mediators.

5 Interpreting Effect Sizes

The impact of cash transfers on suicides of cash transfer recipients is quantitatively very large if we assume no spillovers in suicides. Spillovers similar to those identified in previous work are consistent with moderately large direct effects. Implied direct per-dollar effects of income on suicides identified using rainfall are significantly smaller than those from cash transfers.

Cash Transfer Program There are two main factors to consider when calculating the size of the cash transfer program on recipients: First, there is a strong economic gradient in the suicide rate. The correlation between the fraction of individuals classified as poor and the suicide rate is large and positive (see Section A.2). A 10% higher share of the poor population is, on average, associated with a 0.14 higher suicide rate in 2005 (Table A.1). A back-of-the-envelope calculation suggests that poor individuals are 2.24 times more likely to commit suicides than non-poor households.[38] Second, there are likely two types of spillover effects on non-treated individuals. First, there is evidence that suicides are highly contagious (Hedström et al., 2008). Furthermore, the conditional cash transfer might have positive economic spill-overs to households not receiving the PKH program (Angelucci and De Giorgi, 2009). Thus, we would expect that reducing suicides among PKH recipients may also decrease suicides among non-recipients. Assuming no spillovers, our preferred treatment effect estimate (0.36 suicides per 100,000 people) implies that the suicide rate among the poor decreased by 3.6 suicides per 100,000 or 89% of the implied mean suicide rate. This effect size would imply that the suicide rate among PKH recipients who received the transfer is lower than the rate among non-recipients post-treatment. Therefore, it may be more reasonable to calculate the implied suicide rate reduction among PKH recipients assuming that the cash transfer equalized suicide rates

[38] For details for this and the following back of the envelope calculations see Section A.1.

between poor and non-poor households. This yields an estimate of a direct treatment effect of 2.36 per 100,000 or 58.7% of the implied control group mean for PKH recipients and an indirect effect of 0.12 for non-poor individuals (4.9% of the direct treatment effect). Our estimated effect sizes of spillovers are modest compared to estimates from the literature on suicide contagion (Hedström et al., 2008). We estimate that the program prevented about 1065 suicides, i.e. one suicide for every 672,000 USD spent.

Agricultural Productivity Shocks To estimate the per-dollar effect size of agricultural productivity shocks we use micro data from the Indonesian Family Life Survey. We show that a one standard deviation change in rainfall induces a 6.9% change in monthly per capita consumption for individuals in agricultural households (Table 5). This translates into a change of about 18,000 Rupiahs or roughly 1.8 USD of monthly per capita consumption at the median consumption level. This is equivalent to a yearly change in per capita expenditures by 21.6 USD at 2005 prices per one standard deviation change, a similar magnitude as the annual cash transfer amount. We use this measure as our preferred dollar values to deal with outliers in the consumption data which might more strongly influence the level estimates. Assuming no spillovers to non-farmers our estimates suggest that an increase in annual per capita expenditure of 21.6 USD decreases the suicide rate by 0.14 per 100,000 farmers which is equivalent to 7.4% of the implied mean suicide rate for farmers across all periods.

Effect Size Comparison Next, we compare the per-dollar impact of the cash transfer program and of the agricultural productivity shocks. This comparison requires strong assumptions, such as the baseline suicide rates of different groups of people. The implied effect sizes in terms of percentage changes in the suicide rate for a 10 dollar change in income or consumption (assuming no spillovers) for the cash transfer program are roughly twelve times larger than of those of rainfall shocks (twenty-five times larger in absolute terms of the suicide rate). Extrapolating linearly, we find that an increase in the annual per capita income or consumption by 10 USD decreased the suicide rate of poor individuals and farmers by 1.61 suicides (39.64%) or 0.06 suicides (3.34%), respectively. We can reject the hypothesis of equality of the implied direct effect of the cash transfer and the rainfall shocks in dollar terms ($p<0.01$). This finding implies that the income shock targeted at initially poorer individuals was more effective, and is therefore consistent with a concave relationship between the suicide rate and income.

The larger per-dollar impact of the cash transfer program compared to agricultural productivity shocks of the same value is in line with the predictions outlined in Section 2.1. There are several reasons why the cash transfer could have larger effects. First, the six year duration of the program substantially decreases uncertainty about future income streams (alleviating a potentially important source of stress). Second, it is possible that program recipients expected that the cash transfer may continue after the initial six year period which would substantially increase the expected net present value of the program. Third, the conditionalities of the cash transfer may have an additional effect on mental health as they may induce more social interactions and may relieve stress related to the children. Fourth, individuals classified as poor have higher implied mean suicide rates than farmers. Thus, the cash transfer program targeted a high risk population and, therefore, likely had a larger impact. Fifth, we assume that all farmers are affected equally by rainfall, but there is a large degree of heterogeneity of how rainfall affects harvest, for example, depending on the type of crop. Sixth, it is possible that the cash transfers have a direct psychological effect. They may also act as a signal that the government may be willing to offer insurance from bad outcomes more generally, thereby shifting recipients' economic outlook.

6 Conclusion

We establish an important economic dimension in suicides. Using the nation-wide roll-out of a conditional cash transfer program and a randomized experiment of the same program, we show that the program decreased suicides by approximately 0.36 per 100,000 inhabitants. We also show that agricultural productivity shocks, proxied by rainfall, significantly affect the suicides. A one-standard deviation increase in rainfall lowers the number of suicides per 100,000 inhabitants by approximately 0.08. Moreover, we establish that cash transfers lower suicides most strongly in subdistricts experiencing negative agricultural productivity shocks. This supports the idea that social welfare programs can mitigate the adverse effects of negative economic shocks on mental health. Our evidence points to an important role of government policies in alleviating the consequences of poverty on mental health.

We also use micro-data from the Indonesian Family Life Panel showing that agricultural productivity shocks affect the mental health of farmers. This suggests that economic shocks may affect people's inclination to commit suicides through mental health. However, understanding the exact mechanisms through which economic shocks ignite suicidal behavior leaves ample scope for future research. We believe that there are several fruitful avenues for future research: first, we need better micro-data on how economic circumstances affect mental health, the formation of economic beliefs and preferences (e.g. time preferences). Second, more research should be carried out to understand which economic and psychological interventions are best-suited to increase mental health and prevent suicides. Third, we still lack an understanding of which populations to target to most cost-effectively increase mental health and lower suicides.

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7 Tables

Table 1: Summary statistics

	Mean	SD	Median	Min.	Max.	Obs.
Panel A: Population data						
Suicide rate	1.39	2.14	0.65	0.00	64.52	3138
Any suicide	0.53	0.50	1.00	0.00	1.00	3138
Educ. institutions per 100,000 pop.	152.03	49.66	146.20	11.84	781.56	3138
Health institutions per 100,000 pop.	181.98	59.06	174.19	17.88	788.13	3138
% villages with asphalted road	0.72	0.32	0.83	0.00	1.00	3138
% villages with lighting	0.78	0.32	1.00	0.00	1.00	3138
% rural villages	0.74	0.40	1.00	0.00	1.00	3138
Number of villages	17.22	12.75	14.00	2.00	137.00	3138
Population size	56021.47	40427.31	47483.50	2371.00	415394.00	3138
Number of families	14089.84	9850.95	12182.00	540.00	90682.00	3138
Percentage farmers	56.84	30.97	67.17	0.00	100.00	3138
Panel B: RCT data						
Suicide rate	1.41	1.98	0.63	0.00	14.50	310
Any suicide	0.52	0.50	1.00	0.00	1.00	310
Educ. institutions per 100,000 pop.	161.63	49.83	151.46	69.72	475.15	310
Health institutions per 100,000 pop.	181.80	43.36	176.57	82.88	504.15	310
% villages with asphalted road	0.70	0.29	0.79	0.00	1.00	310
% villages with lighting	0.78	0.32	0.98	0.00	1.00	310
% rural villages	0.90	0.27	1.00	0.00	1.00	310
Number of villages	16.74	6.85	16.00	3.00	46.00	310
Population size	59334.46	36211.28	50114.00	10116.00	260321.00	310
Number of families	15558.43	9121.26	13361.00	2345.00	68967.00	310
Percentage farmers	60.00	23.85	64.00	0.00	98.00	310

Table 1 displays summary statistics based on the Indonesia Village census 2005 . Observations are weighted using 2005 population except for number of villages, population size, and number of families. Panel A shows summary statistics for the population of subdistricts in Indonesia. Panel B shows summary statistics for the RCT sample.

Table 2: Main results: roll-out

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: Suicide rate						
Treatment	-0.358***	-0.358***	-0.381***	-0.366***	-0.267**	-0.591***
	(0.101)	(0.086)	(0.110)	(0.101)	(0.115)	(0.164)
Subdistrict FE	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y
Cluster subdistrict	N	Y	N	Y	N	N
Exclude RCT sample	N	N	Y	N	N	N
Include pre-treatment periods	N	N	N	Y	Y	N
Subdistrict trends	N	N	N	N	Y	N
Population weights	Y	Y	Y	Y	Y	N
Control mean (2011 & 2014)	2.016	2.016	2.033	2.016	2.016	2.721
N	9414	9414	8484	15690	15690	9414
Census waves	05-14	05-14	05-14	00-14	00-14	05-14

Table 2 displays the difference-in-differences estimate of the effect of the conditional cash transfer program on the suicide rate. All specifications include year and subdistrict fixed effects. Estimates are weighted using 2005 population size unless otherwise noted. Standard errors are clustered at district level unless otherwise noted. Column 2 reports standard errors clustered at subdistrict level. Column 3 excludes the RCT sample (which we employ in Table 3). Column 4 includes all pre-treatment periods. Column 5 further includes subdistrict time-trends. Column 6 does not use 2005 population weights to estimate treatment effects. The definition of the suicide rate per 100,000 people changes slightly over time. In 2014 it is defined as the number of suicides and suicide attempts per 100,000. In 2011 it is defined as the number of suicides per 100,000. From 2000 to 2005 the suicide rate is defined as the number of villages in a given subdistrict with at least one suicide per 100,000.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Main results: randomized experiment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable: Suicide rate							
Treatment	-0.337	-0.258	-0.665**	-0.665**	-0.466	-1.064*	-0.474
	(0.266)	(0.275)	(0.318)	(0.266)	(0.334)	(0.593)	(0.325)
Subdistrict FE	N	N	Y	Y	Y	Y	N
Time FE	N	N	Y	Y	Y	Y	N
Cluster district	N	N	N	Y	N	N	N
Include pre-treatment periods	N	N	N	N	Y	Y	N
Subdistrict trends	N	N	N	N	N	Y	N
Baseline suicide	Y	N	N	N	N	N	Y
Population weights	Y	Y	Y	Y	Y	Y	N
Control mean (2011)	1.774	1.774	1.774	1.774	1.774	1.774	2.058
N	310	310	620	620	1240	1240	310
Census waves	11	11	05-11	05-11	00-11	00-11	11

Table 3 displays the results of the RCT experiment. The suicide rate is defined as explained in Table 2. Column 1 reports an ANCOVA specification using the 2011 suicide rates as the outcomes and controlling for the 2005 suicide rate. Column 2 displays the treatment effect estimate of a post comparison of treated and control subdistricts. Columns 3 to 6 report difference-in-differences estimates of the treatment effect and control for both subdistrict and time fixed effects. Standard errors are clustered at the subdistrict level unless otherwise noted. Estimates are weighted using population size from 2005 unless otherwise noted. In Column 3 we report the baseline difference-in-differences specification using data from 2005 and 2011. Column 4 reports standard errors clustered at district level. Column 5 includes data from the 2003 and 2000 census waves. Column 6 further includes subdistrict specific time-trends on top of subdistrict and time fixed effects. Column 7 shows the ANCOVA specification from Column 1 without population weights.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Interactions between agricultural productivity shocks and the cash transfers

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: Suicide rate						
Rain (z-scored)	-0.082*	-0.112**	-0.081*	-0.106**	-0.094*	-0.129**
	(0.045)	(0.050)	(0.044)	(0.048)	(0.049)	(0.054)
	[0.039]	[0.043]	[0.038]	[0.042]	[0.039]	[0.046]
Treat		-0.216**		-0.255**		-0.193
		(0.109)		(0.101)		(0.117)
		[0.122]		[0.115]		[0.130]
Rain (z-scored)× Treat		0.118*		0.096		0.133*
		(0.072)		(0.068)		(0.079)
		[0.070]		[0.068]		[0.077]
Subdistrict FE	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y
Subdistrict trends	N	N	Y	Y	N	N
Include lagged rainfall	N	N	N	N	Y	Y
N	15690	15690	15690	15690	15690	15690
Census waves	00-14	00-14	00-14	00-14	00-14	00-14

Table 4 displays the impact of rainfall on the suicide rate. The suicide rate is defined as explained in Table 2. Odd columns report the impact of standardized rainfall on suicides. Even columns include the treatment variable from the conditional cash transfer roll-out and the interaction with rainfall. All specifications include subdistrict and year fixed effects. Columns 3 and 4 control for subdistrict time-trends. Columns 5 and 6 include the first, second and third lag of rainfall. Parentheses report Conley (1999) standard errors, allowing for arbitrary time correlation and two-dimensional spatial correlation within a 100 kilometre radius of the subdistrict centroid (used for stars). Standard errors clustered at the district level are in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Impact of rainfall on consumption and depression z-scores

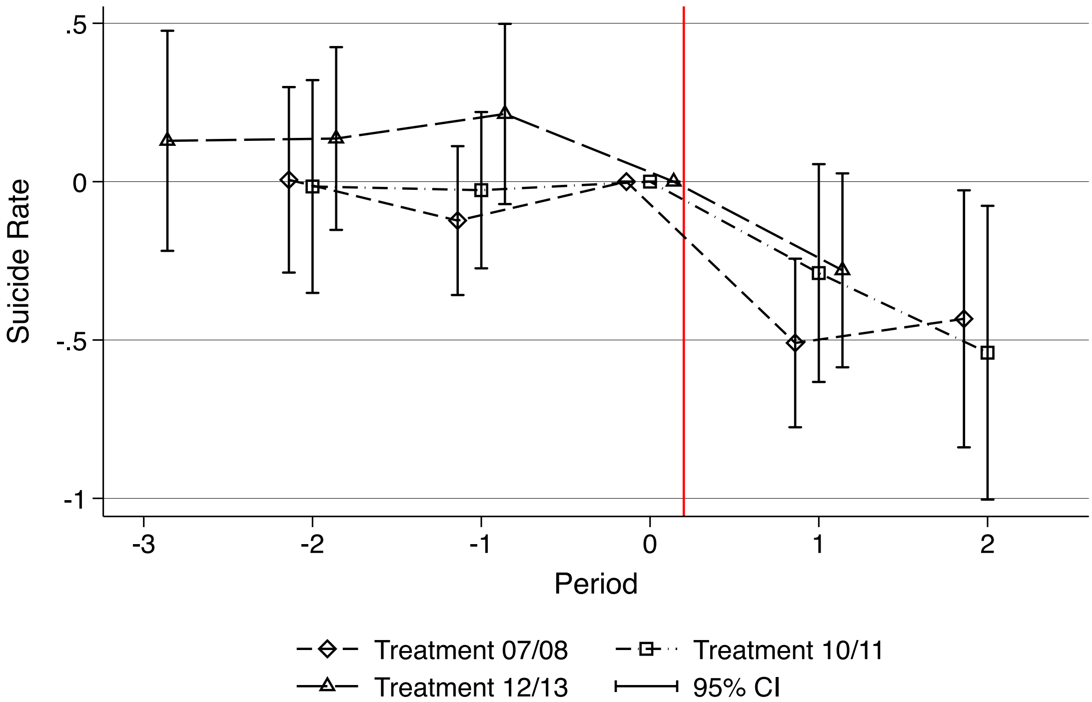
	Working in agriculture			Not working in agriculture		
	(1)	(2)	(3)	(4)	(5)	(6)
	Per capita cons.	Log per capita cons.	Depression (z)	Per capita cons.	Log per capita cons.	Depression (z)
Rain (z-scored)	39706.205***	0.069***	-0.124***	11687.256	0.008	-0.008
	(14587.567)	(0.022)	(0.042)	(15481.365)	(0.018)	(0.050)
	[9805.642]	[0.019]	[0.042]	[14631.741]	[0.017]	[0.053]
Individual Fixed Effect	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y
N	12790	12790	5192	8356	8356	3420
IFLS waves used	1-5	1-5	4-5	1-5	1-5	4-5

Table 5 displays the impact of rainfall on monthly per-capita household consumption and depression z-scores in the Indonesian Family Life Survey (IFLS). The sample is restricted to individuals tracked in all five waves of the IFLS. Columns 1 to 3 use a sample of individuals living in households with at least one member working in agriculture (self-employed without or with temporary workers, or as casual or family workers) in any of the five waves. IFLS wave 1 contains no coded sector information. Therefore, all individuals working as temporary workers or those who are self-employed without permanent workers are counted as working in agriculture. Columns 4 to 6 use individuals from households without any agricultural worker (as defined above). Per-capita household consumption is measured in Indonesian Rupiah. Consumption is deflated to 2005 levels. In 2005, 10,000 Rupiah were roughly equivalent to 1 USD. Depression scores are measured using the 10 item CES-D scale (Cheung et al., 2007; Radloff, 1997). All columns include individual and time fixed effects. Parentheses report Conley (1999) standard errors, allowing for arbitrary time correlation and two-dimensional spatial correlation within a 100 kilometre radius of the subdistrict centroid (used for stars). Standard errors clustered at the district level are in square brackets. Test for equality of coefficients for individuals from agricultural households and individuals from non-agricultural households have the following p-values: 0.162, 0.006, and 0.06 for monthly consumption, log monthly consumption, and depression scores, respectively.

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

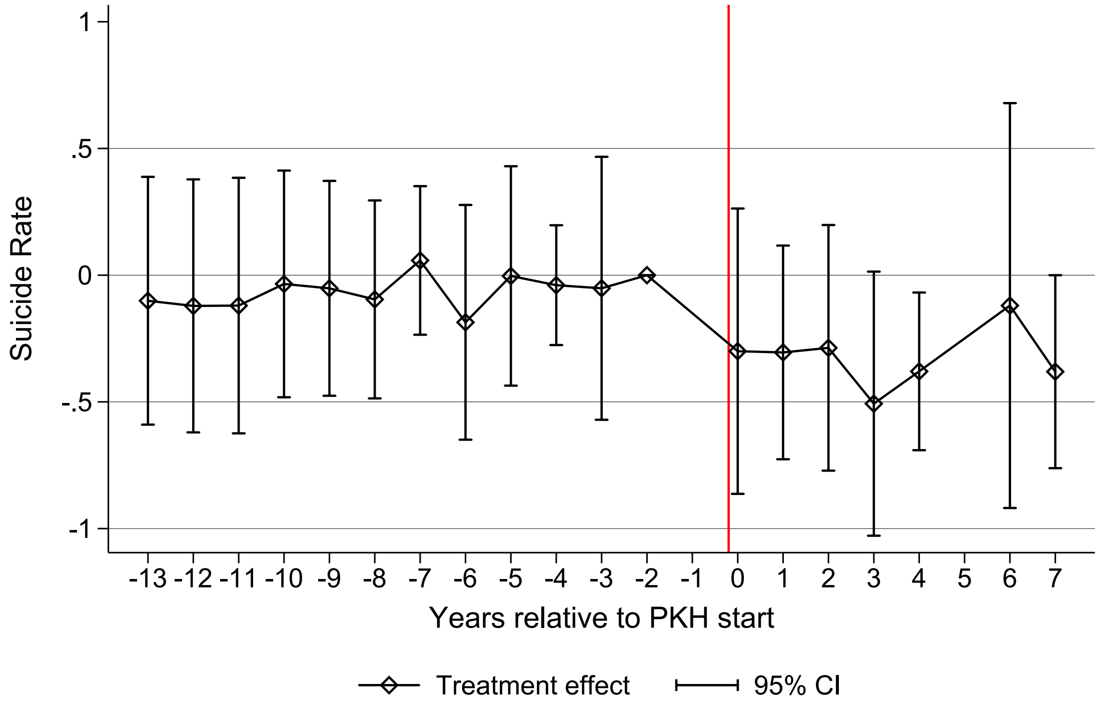
8 Figures

Figure 1: Event-study: roll-out of conditional cash transfer program



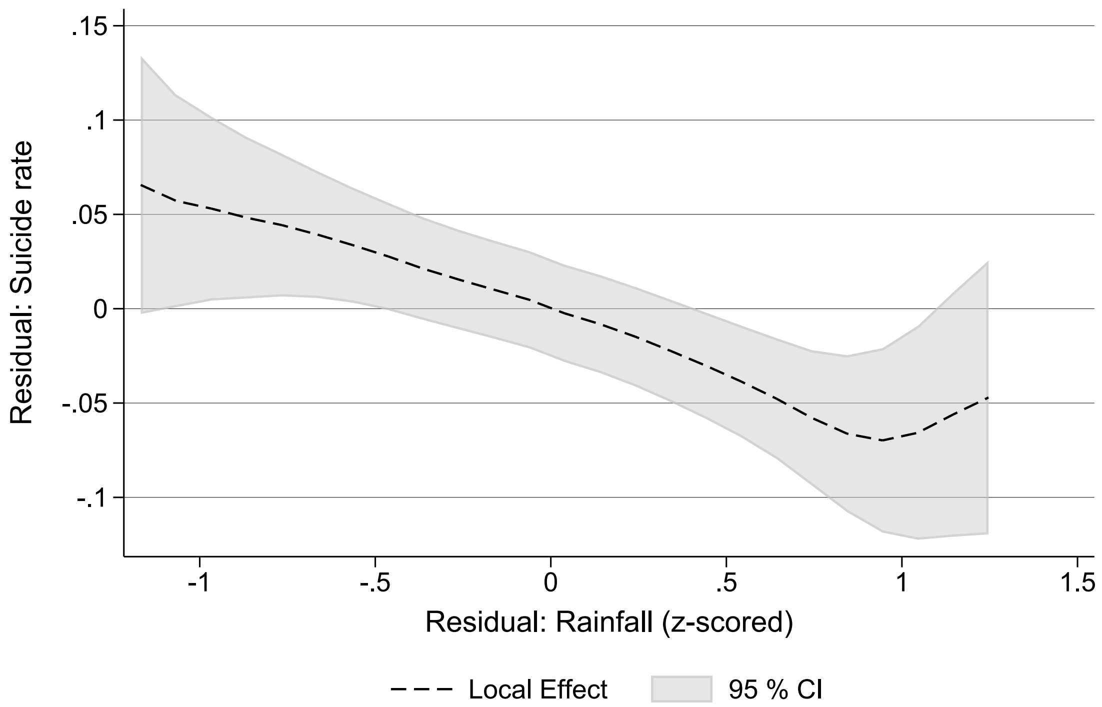
Notes: Figure 1 displays treatment effects relative to the timing of the introduction of the conditional cash transfer program by treatment wave. The difference in the first period before the treatment are normalized to zero. The displayed coefficients are the difference-in-differences treatment effect estimates relative to subdistricts that had not received the treatment until 2013. All standard errors are clustered at the district level. The vertical red line indicates the program start. The definition of the suicide rate per 100,000 people changes slightly over time. In 2014 it is defined as the number of suicides and suicide attempts per 100,000. In 2011 it is defined as the number of suicides per 100,000. From 2000 to 2005 the suicide rate is defined as the number of villages in a given subdistrict with at least one suicide per 100,000.

Figure 2: Dynamics of treatment effects



Notes: Figure 2 displays the treatment effects on the suicide rate relative to the year of introduction of the conditional cash transfer program. The suicide rate is defined as in Figure 1. To estimate the coefficient for each year we use different sub-samples of subdistricts. Treated subdistricts in a given period t are defined as having received the cash transfer program exactly t before (or after) the census (e.g. to estimate the effect in year 1, we use observations from the 2011 census subdistricts that received PKH in 2010 and observations of the 2014 census of subdistricts first receiving the PKH in 2013). For $t \geq 0$ control subdistricts are defined as subdistricts that had not received the treatment at the time of census. For $t < 0$ control subdistricts are defined as not having received the treatment t years after the census. Coefficients are obtained conditional on year and subdistrict fixed effects. Standard errors are clustered at the district level. The vertical red line indicates the program start. Differences two years before the treatment are normalized to zero.

Figure 3: Agricultural productivity shocks and suicides: Local Polynomial Regression



Notes: Figure 3 displays a local polynomial regression of the residuals of the suicide rate and the residual of standardized rainfall using PODES 2000, 2003, 2005, 2011 and 2014. Residuals are obtained from a regression with time and subdistrict fixed effects and subdistrict time-trends. We employ population weights from 2005 in our estimations. We employ an Epanechnikov kernel with a bandwidth of 0.44. Top and bottom percentile of the residual rainfall distribution are omitted, because of concerns about biased estimates close to the limits (Li and Racine, 2006). The suicide rate is defined as described in Figure 1.

Online Appendix: Income Shocks and Suicides: Causal Evidence From Indonesia

Cornelius Christian, Lukas Hensel, and Christopher Roth

Summary of the Online appendix

The Appendix starts with detailing the calculations used to obtain the direct effect sizes discussed in Section 5 of the paper.

In Section A.2 we present descriptive statistics. Table A.1 shows correlates of suicide rates. Table A.2 shows evidence in favor of the integrity of the randomization. Section A.3 examines the sensitivity of our results from the difference-in-differences strategy. Table A.3 provides further checks on the assumption that the timing of roll-out of the program was quasi-random. Tables A.4 and A.5 examine robustness of the roll-out results to using different definitions of the outcome variables. Table A.6 examine sensitivity of our results to dropping partially treated subdistricts. Table A.7 shows the main roll-out results without using population weights. Table A.8 shows the results using 2006 subdistrict definitions. Table A.9 shows further robustness checks.

Section A.4 examines the sensitivity of the results from the randomized cash transfer program. Tables A.10 and A.11 examine robustness of the RCT results to using different definitions of the outcome variables. Table A.12 examines sensitivity of our results to dropping partially treated subdistricts. Table A.13 shows the RCT results without employing population weights. Table A.14 shows the results using 2006 subdistrict definitions. Finally, Table A.15 shows further robustness checks for the RCT results. Section A.5 presents further analysis of the dynamics of treatment effects. Table A.16 present the results on "dynamic treatment effects" which using population weights. Table A.17 present the results on "dynamic treatment effects" which does not apply population weights. Table A.18 examines whether treatment effects vary with treatment intensity.

Section A.6 provides further robustness checks for the rainfall analysis. Table A.19

examines whether rainfall and suicide rates are symmetrically related. Table A.20 presents the main specifications without using population weights. Table A.21 examines whether the interaction of rainfall and conditional cash transfers are non-linearly related. Table A.22 shows the main specification with rainfall detrended at the subdistrict level using population weights. Table A.23 shows the main specification with rainfall detrended at the subdistrict level without using population weights.

Section A.7 presents micro-evidence using the Indonesian Family Life Survey. Table A.24 analyzes heterogeneous effect of rainfall shocks on depression symptoms by gender, age and baseline expenditures. Table A.25 examines whether PKH recipients have higher depression scores before the receipt of the cash transfer program.

Section A.8 sheds further light on mechanisms. Table A.26 examines whether our results are mediated by changes in crime rates, the quality of local institutions or social capital. Table A.27 describes heterogeneous responses to the treatment.

Section B presents additional figures. Figure A.1 displays the evolution of mean suicide rates over time for different treatment groups. Figure A.2 displays mean suicide rates over time by treatment wave without applying population weights. Figure A.3 shows the event-study analysis without applying population weights. Figure A.4 presents an event-study analysis for our RCT sample with effects normalized relative to 2005. Figure A.4 presents an event-study analysis for our RCT sample with effects normalized relative to 2000. Figure A.4 presents an event-study analysis for our RCT sample. Figure A.6 displays mean suicide rates (based on a Poisson extrapolation) over time by treatment wave. Finally, Figure A.7 showcases the event-study graph using the Poisson extrapolated suicide rates.

A Online Appendix

A.1 Effect Size Calculations

This section explains how we calculate the implied direct effect on cash transfer recipients and farmers. This section is not meant to provide unambiguous estimates of direct effects, but rather to provide a back of the envelope calculation based on several simplifying assumptions. We conduct this analysis in the following steps for the cash transfer program and agricultural productivity shocks.

1. Calculate the economic gradient of suicide rates.
2. Calculate the implied direct treatment effect on treated individuals.
3. Account for potential spillover effects of changing suicide rates.

A.1.1 The Cash Transfer Program

First, we calculate the economic gradient of the suicide rate with respect to being classified as poor. We use this specification as a proxy for households eligible to receive PKH.[1] Column 1 of Table A.1 suggests that a 10% higher share of the eligible household is, on average, associated with a 0.142 higher suicide rate 2005 before the program was rolled out. Assuming that this linear relationship between the fraction of poor individuals and the suicide rate holds we can use the constant as the suicide rate for non-eligible individuals ($\bar{s}_{\text{non-elig}} = 1.15$). The relationship between the share of eligible individuals ($x_{\text{elig}} = 0.1$) and the suicide rate is:

$$1.15 + 1.42 \cdot x_{\text{elig}} = (1 - x_{\text{elig}}) \cdot 1.15 + x_{\text{elig}} \cdot \bar{s}_{\text{elig}}$$

[1] We abstract from the fact that 15% of the population is classified as poor and assuming that 10% of the population is poor and that only these 10% are eligible for treatment. If anything this would make us underestimate the economic gradient between PKH recipient and non-recipients.

From this we obtain the implied suicide rate for poor individuals $\bar{s}_{\text{elig}} = 2.57$. Thus, we conclude that eligible individuals are 2.24 times more likely to commit suicides than non-eligible individuals.

Second, we calculate the implied direct effect on eligible individuals assume that this ratio of suicide rates for eligible and non-eligible individuals relationship remains the same for 2011 and 2014 for non-treated subdistricts. As a first step, we calculate the implied suicide rates for eligible and non-eligible individuals in 2011 and 2014. The control mean ($\bar{s}_{\text{control}} = 2.02$) is equal to a weighted average of eligible and non-eligible individuals:

$$\bar{s}_{\text{control}} = 2.02 = 0.9 \cdot s_{\text{non-elig}} + 0.1 \cdot \underbrace{2.24 \cdot s_{\text{non-elig}}}_{s_{\text{elig}}} \tag{6}$$

From this we obtain $s_{\text{non-elig}} = 1.80$ and $s_{\text{elig}} = 4.03$. Next, we use the calculated suicide rate in treated subdistricts $\bar{s}_{\text{treat}} = 2.02 - 0.36 = 1.66$ to calculate the implied direct treatment effect on eligible individuals. Assuming no spillovers on untreated individuals we obtain:

$$0.9 \cdot 1.8 + 0.1 \cdot (4.03 + \Delta s_{\text{elig}}) = 1.66 \tag{7}$$

.

This yields a direct treatment effect on PKH recipients as $\Delta s_{\text{elig}} = -3.6$ or a reduction of about 89% of the implied control suicide rate for eligible individuals.

These numbers imply that the suicide rate of eligible individuals receiving the cash transfer would be reduced to levels below the suicide rate of non-eligible households. Therefore, we conduct a further calculation allowing for the cash transfer program to have positive spillovers on non-eligible individuals, in line with evidence on cash transfers and suicides (Angelucci and De Giorgi, 2009; Hedström et al., 2008). Our benchmark specification calibrates the strength of within subdistrict spill-overs such that the ex-post suicide rates of eligible and non-eligible individuals in treated subdistricts is equalized.

We model the total treatment effect as consisting of a multiplier effect and a direct treatment effect. This requires an assumption about the functional form of the multiplier.

We assume that the suicide rate of non-PKH recipients in treated subdistricts is a function of the direct treatment effect times a multiplier μ. The suicide rate in treated subdistricts can then be written as:

$$\bar{s}_{\text{treat}} = 0.9 \cdot (s_{\text{contr,non-elig}} + \underbrace{\mu(s_{\text{treat,elig}} - s_{\text{contr,elig}})}_{\text{Spill-overs}}) + 0.1 \cdot s_{\text{treat,elig}} \tag{8}$$

From this we can obtain the implied direct treatment effect on PKH recipients as:

$$\Delta s_{\text{elig}} = s_{\text{treat,elig}} - s_{\text{contr,elig}} = \frac{\bar{s}_{\text{treat}} - 0.9 \cdot s_{\text{contr, non-elig}} - 0.1 \cdot s_{\text{contr,elig}}}{0.9 \cdot \mu + 0.1} \tag{9}$$

Equation 9 confirms the intuition that the implied direct effect on poor individuals decreases with size of the multiplier μ. Setting ex-post suicide rates for poor and non-eligible individuals equal at the mean suicide rate in treated subdistricts of 1.66 suicides per 100,000, we obtain a multiplier of $\mu = 0.049$ and an implied direct effect on PKH recipients of 2.36 per 100,000 or 58.7% of the implied control group mean.

Effect on number of suicides Our preferred estimate for the number of suicides is Column 11 of Table A.5 which shows an average treatment effect of 0.2 suicides. In total, there we observe 5324 subdistrict-years for treated subdistricts over time. Assuming constant treatment effects over time, we calculate that the PKH program prevented 1065 suicides.

A.1.2 Rainfall

To calculate the effect size of agricultural productivity shocks, we conduct a similar calculation.

First, we obtain the economic gradient of the suicide rate with the share of farmers using the results from Column 2 of Table A.1. We observe that in 2005 an increase in the share of farmer by 10% is associated with an increase in the suicide rate of 1.19. The population weighted average share of farmers in 2005 is $x_{\text{farm}} = 0.56$.[2]

We again use the constant as the suicide rate for non-farmers ($\bar{s}_{\text{non-farm}} = 0.72$). The relationship between the share of farmers x_{farm} and the suicide rate is:

$$0.72 + 1.19 \cdot x_{\text{farm}} = (1 - x_{\text{farm}}) \cdot 0.71 + x_{\text{farm}} \cdot \bar{s}_{\text{farm}}$$

From this we obtain the implied suicide rate for farmers $\bar{s}_{\text{farm}} = 1.91$. Thus, farmers are 2.67 time more likely to commit suicides than non-farmers households. The calculations for the effect of rainfall are complicated by the fact that there is no clear control group. Therefore, we use the population weighted mean suicide rate across all years (1.39 suicides per 100,000). Assuming no spill-over effects, we then calculate the implied suicide rates for farmers and non-farmers using the following equation:

$$\bar{s} = 1.39 = 0.44 \cdot s_{\text{contr,non-farm}} + 0.56 \cdot \underbrace{2.67 \cdot s_{\text{contr, non-farm}}}_{=s_{\text{contr, farm}}}$$

We obtain mean suicide rates of $s_{\text{contr, farm}} = 1.91$ for farmers and $s_{\text{contr, non-farm}} = 0.72$ for non-farmers

Based on these implied means we can then calculate the implied direct effect size assuming no spill-overs. The average effect of a one-standard deviation in rainfall according to our preferred specification is -0.08. We obtain $\bar{s}_{\text{treat}} = 1.31$ by subtracting a one standard deviation treatment effect (0.08) from the population weighted mean suicide rate across all years (1.39). We then use an analogue equation to the cash transfer calculations to compute the implied direct effect:

$$\bar{s}_{\text{treat}} = 1.31 = 0.44 \cdot s_{\text{contr,non-farm}} + 0.56 \cdot (s_{\text{contr, farm}} + \Delta s_{\text{farm}})$$

With these assumptions, we obtain an implied direct effect size as $\Delta s_{\text{farm}} = -0.14$ which is equivalent to 7.4 percent of the implied long-term mean for farmers. Allowing

[2] We use the 2005 share to make the calculation comparable to the cash transfer calculations.

for spillover with a multiplier in line with the cash transfer (0.049), the implied direct treatment effect decreases to -0.047 suicides per 100,000 or 2.5 percent of the implied long-term mean for farmers.

A.1.3 Calculating per-dollar impacts for implied direct effects

This section outlines how we calculate comparable monetary values for implied direct effect of the cash transfer and agricultural productivity shocks. For this exercise, we need to assume that the impact is linear in dollar amounts. For the cash transfer, we estimate that 22.45 USD lower the suicide rate by 3.6 suicides per 100,000 or 89 percent, so that 10 USD would lower the suicide rate by 1.61 suicides per 100,000 or 39.64 percent. For agricultural productivity shocks, we estimate that a yearly per-capita consumption increase by 21.6 USD reduces the suicide rate by 0.14 suicides per 100,000 or 7.4 percent. This implies that 10 USD would reduce the suicide rate by 0.06 suicides per 100,000 or 3.34 percent.

Additional Tables

A.2 Descriptives

Table A.1: Correlates of the suicide rate

	(1)	(2)	(3)	(4)	(5)
Dependent variable: Suicide rate					
Fraction poor	1.422**			-0.488	-0.155
	(0.591)			(0.721)	(0.684)
Fraction farmers		1.194***		1.109***	0.692***
		(0.129)		(0.168)	(0.204)
Average HH expenditure (per capita)			-0.273***	-0.075	-0.063
			(0.051)	(0.067)	(0.066)
Education (per capita)					-0.000
					(0.001)
Health facilities (per capita)					0.004***
					(0.001)
Social organisations (per capita)					0.000
					(0.000)
Crime (per capita)					0.013***
					(0.003)
Constant	1.147***	0.715***	2.035***	1.024***	0.120
	(0.114)	(0.070)	(0.144)	(0.294)	(0.348)
R2	0.006	0.030	0.017	0.030	0.066
N	3138	3138	3138	3138	3138
Census wave	05	05	05	05	05

Table A.1 shows correlates of the suicide rate per 100,000 individuals in 2005 with district and subdistrict covariates. Our main outcome is the suicide rate per 100,000 individuals. Due to data constraints its definition varies slightly over time. In 2014 it is defined as the number of suicides and suicide attempts per 100,000. In 2011 it the number of suicides per 100,000. From 2000 to 2005 the suicide rate is defined as the number of villages in a given subdistrict with at least one suicide per 100,000 population. The fraction of individuals below the poverty line (poor) and the average per-capita household expenditure are measured at the district level. All other variables are measured at the village level and aggregated to the subdistrict level. Share of population classified as poor or farmers ranges from 0 to 1. Column 5 further includes per capita educational institutions, per capita health institutions, per capita social organizations, and crime rates. Regressions weighted by subdistrict population in 2005. Standard errors clustered at district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: Baseline balance: randomized experiment

	Treatment	Control	Δ	se(Δ)	p(Δ=0)
Suicide rate	1.719	1.337	0.386	(0.282)	0.173
Any suicide	0.468	0.427	0.040	(0.059)	0.504
Education institutions per capita	91.763	91.713	-0.085	(5.338)	0.987
Health institutions per capita	100.784	100.573	0.401	(5.998)	0.947
% villages with asphalted road	0.651	0.662	-0.013	(0.037)	0.731
% villages with lighting	0.786	0.732	0.051	(0.040)	0.207
% rural villages	0.950	0.925	0.026	(0.026)	0.323
Number of villages	16.144	15.979	0.219	(0.783)	0.779
Population size	55811.295	54974.091	760.818	(3669.322)	0.836
Number of families	14758.525	14510.671	231.873	(927.610)	0.803
Percentage of farmers	68.063	68.620	-0.498	(2.524)	0.844
N	167	143	310	310	310

Table A.2 compares population weighted baseline covariates of the RCT sample using the 2005 village census. The suicide rate is defined as described in Table A.1. P-values are based on standard errors clustered at the subdistrict level. Test for joint significance of differences between the treatment and control group is not rejected (p=0.57).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.3 Further sensitivity - Roll-Out of Cash Transfer Program

Table A.3: Roll-out identification: regression tests

	(1) Δ Suicide rate	(2) Suicide rate
Treatment wave 1: 07-11	-0.002	
	(0.063)	
Treatment wave 2: 12-13	0.028	
	(0.064)	
Treatment		-0.399***
		(0.121)
Include pre treatment dummy	N	Y
N	6276	15690
Census waves	00-05	00-14

Table A.3 displays two tests in support of the identification assumptions for the main roll-out specification. Column 1 presents a test for whether the timing of the entry into the program correlates with pre-trends. The dependent variable is changes in the suicide rate between rounds. The sample in Column 1 is restricted to pre-treatment periods. Column 2 displays a test for whether the treatment effect is driven by changes in the suicide rates right before the introduction of the treatment. It shows the treatment effect on the suicide rate controlling for an indicator variable for the period prior to treatment. The suicide rate is defined as described in Table A.1. All specifications include time and subdistrict fixed effects and are weighted by 2005 population. Standard errors are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Roll-out: additional outcome definitions 1

	Suicide rate (extrapolated - Poisson)					Suicide rate (truncated)					Any Suicide				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Treatment	-0.298***	-0.474***	-0.313***	-0.284***	-0.240**	-0.268***	-0.418***	-0.282***	-0.265***	-0.206**	-0.061***	-0.061***	-0.062***	-0.046**	-0.068**
	(0.096)	(0.154)	(0.103)	(0.087)	(0.113)	(0.081)	(0.132)	(0.088)	(0.076)	(0.097)	(0.022)	(0.022)	(0.023)	(0.020)	(0.028)
Subdistrict FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Exclude RCT sample	N	N	Y	N	N	N	N	Y	N	N	N	N	Y	N	N
Include pre-treatment periods	N	N	N	Y	Y	N	N	N	Y	Y	N	N	N	Y	Y
Subdistrict trends	N	N	N	N	Y	N	N	N	N	Y	N	N	N	N	Y
Population weight	Y	N	Y	Y	Y	Y	N	Y	Y	Y	N	Y	N	N	N
Control Mean (2011 & 2014)	1.750	1.750	1.767	1.750	1.750	1.615	1.615	1.629	1.615	1.615	0.547	0.547	0.546	0.547	0.547
N	9414	9414	8484	15690	15690	9414	9414	8484	15690	15690	9414	9414	8484	15690	15690
Census waves	05 - 14	05 - 14	05 - 14	00 - 14	00 - 14	05 - 14	05 - 14	05 - 14	00 - 14	00 - 14	05 - 14	05 - 14	05 - 14	00 - 14	00 - 14

Table A.4 displays the difference-in-differences estimate of the effect of the conditional cash transfer program on three alternative outcomes. Specifications use population weights unless otherwise noted. Columns 1 to 5 show treatment effects on the Poisson-extrapolated suicide rate (extrapolated from the subdistrict average of village-level incidences of at least one suicide in a given year). Columns 6 to 10 report the effects on a truncated version of the suicide rate based on the number of villages with at least one suicide in a subdistrict. Columns 11 to 15 report the treatment effect on a binary variable equal to one if there was at least one suicide in a subdistrict last year. All specifications include year and subdistrict fixed effects. Standard errors are clustered at district level. Column 2, 7, and 12 do not employ population weights. Columns 3, 8, and 13 exclude the RCT sample. Columns 4, 9, and 14 include all pre-treatment periods. Columns 5, 10, and 15 additionally include subdistrict time-trends. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Roll-out: additional outcome definitions 2

	Suicide rate (2005 population)					Number of suicides (extrapolated - Poisson)					Number of suicides				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Treatment	-0.452***	-0.833***	-0.484***	-0.475***	-0.319***	-0.193***	-0.193**	-0.203***	-0.124***	-0.202***	-0.198***	-0.219***	-0.210***	-0.140***	-0.183**
	(0.109)	(0.163)	(0.119)	(0.113)	(0.117)	(0.053)	(0.086)	(0.057)	(0.044)	(0.067)	(0.055)	(0.084)	(0.060)	(0.050)	(0.091)
Subdistrict FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Exclude RCT sample	N	N	Y	N	N	N	N	Y	N	N	N	N	Y	N	N
Include pre-treatment periods	N	N	N	Y	Y	N	N	N	Y	Y	N	N	N	Y	Y
Subdistrict trends	N	N	N	N	Y	N	N	N	N	Y	N	N	N	N	Y
Population weight	Y	N	Y	Y	Y	N	Y	N	N	N	N	Y	N	N	N
Conrol mean (2011 & 2014)	2.226	2.226	2.041	2.226	2.226	0.920	1.127	0.922	0.920	0.920	1.061	1.318	1.326	1.061	1.061
N	9414	9414	8484	15690	15690	9414	9414	8484	15690	15690	9414	9414	8484	15690	15690
Census waves	05 - 14	05 - 14	05 - 14	00 - 14	00 - 14	05 - 14	05 - 14	05 - 14	00 - 14	00 - 14	05 - 14	05 - 14	05 - 14	00 - 14	00 - 14

Table A.5 displays the difference-in-differences estimate of the effect of the conditional cash transfer program on three alternative outcomes. Specifications use population weights unless otherwise noted. Columns 1 to 5 show treatment effects on the suicide rate based on 2005 population numbers. Columns 6 to 10 report the effects on the Poisson extrapolated number of suicides (extrapolated from the subdistrict average of village-level incidences of at least one suicide in a given year). Columns 11 to 15 report the treatment effect on the number of suicides used to construct our main suicide rate. All specifications include year and subdistrict fixed effects. Standard errors are clustered at district level. Column 2, 7, and 12 do not employ population weights. Columns 3, 8, and 13 exclude the RCT sample. Columns 4, 9, and 14 include all pre-treatment periods. Columns 5, 10, and 15 additionally include subdistrict time-trends. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Roll-out: drop partially treated

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: Suicide rate						
Treatment	-0.336***	-0.336***	-0.358***	-0.339***	-0.442**	-0.582***
	(0.103)	(0.087)	(0.112)	(0.103)	(0.180)	(0.169)
Subdistrict FE	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y
Cluster subdistrict	N	Y	N	Y	N	N
Exclude RCT sample	N	N	Y	N	N	N
Include pre-treatment periods	N	N	N	Y	N	N
Subdistrict Trends	N	N	N	N	Y	N
Population weights	Y	Y	Y	Y	Y	N
Control mean (2011 & 2014)	2.016	2.016	2.033	2.016	2.016	2.391
N	8454	8454	7686	14090	14090	8454
Census waves	05-14	05-14	05-14	00-14	00-14	05-14

Table A.6 displays the difference-in-differences estimate of the effect of the conditional cash transfer on the suicide rate excluding partially treated subdistricts. The suicide rate is defined as described in Table A.1. All specifications use population weights from 2005 unless otherwise noted. All specifications include year and subdistrict fixed effects. Standard errors are clustered at district level unless noted differently. Column 2 report standard errors clustered at subdistrict level. Column 3 excludes the RCT sample. Column 4 includes all pre-treatment periods. Column 5 further includes subdistrict time-trends. Column 6 does not use population weights. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Main results: roll-out - not population-weighted

	(1)	(2)	(3)	(4)	(5)
Dependent variable: Suicide rate					
Treatment	-0.591***	-0.591***	-0.633***	-0.638***	-0.428**
	(0.164)	(0.143)	(0.179)	(0.158)	(0.174)
Subdistrict FE	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y
Cluster subdistrict	N	Y	N	Y	N
Exclude RCT sample	N	N	Y	N	N
Include pre-treatment periods	N	N	N	Y	Y
Subdistrict trends	N	N	N	N	Y
Control mean (2011 & 2014)	2.950	2.950	2.983	2.950	2.950
N	9414	9414	8484	15690	15690
Census waves	05-14	05-14	05-14	00-14	00-14

Table A.7 displays the difference-in-differences estimates of the effect of the conditional cash transfer program on the suicide rate without using population weights. The suicide rate is defined as described in Table A.1. All specifications include year and subdistrict fixed effects. Standard errors are clustered at the district level unless noted differently. Column 2 reports standard errors clustered at the subdistrict level. Column 3 excludes the RCT sample. Column 4 includes all pre-treatment periods. Column 5 additionally includes subdistrict time-trends. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.8: Main results: roll-out - 2006 subdistrict boundaries

	(1)	(2)	(3)	(4)
Dependent variable: Suicide rate				
Treatment	-0.264***	-0.264***	-0.255**	-0.347*
	(0.102)	(0.088)	(0.110)	(0.180)
Subdistrict FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Cluster subdistrict	N	Y	N	N
Exclude RCT sample	N	N	Y	N
Population weights	Y	Y	Y	N
Control mean (2011 & 2014)	2.170	2.170	2.183	3.046
N	14522	14522	13469	14522
Census waves	05-14	05-14	05-14	05-14

Table A.8 displays the difference-in-differences estimates of the effect of the conditional cash transfer program on the suicide rate in subdistricts according to 2006 boundaries using population weights (unless otherwise noted). The suicide rate is defined as described in Table A.1. All specifications include year and subdistrict fixed effects. Standard errors are clustered at the district level. Column 2 reports standard errors clustered at the subdistrict level. Column 3 excludes the RCT sample. Column 4 does not employ population weights. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.9: Main results: roll-out - additional robustness checks

	(1)	(2)	(3)	(4)
Dependent variable: Suicide rate				
Treatment	-0.359***	-0.255**	-0.393***	-0.284***
	(0.093)	(0.103)	(0.121)	(0.106)
Subdistrict FE	N	Y	N	N
Time FE	Y	Y	Y	Y
District FE	Y	N	N	N
District trends	N	Y	N	N
Baseline controls	N	N	Y	Y
Baseline controls × Post	N	N	N	Y
Population weights	Y	Y	Y	Y
Control mean (2011 & 2014)	2.016	2.016	2.033	2.016
N	9414	9414	8484	9414
Census waves	05-14	05-14	05-14	05-14

Table A.9 displays estimates of the effect of the conditional cash transfer program on the suicide rate using 2005 population weights. The suicide rate is defined as described in Table A.1. Standard errors are clustered at the district level. Column 1 uses district instead of subdistrict fixed effects. Column 2 controls for district specific time trends. Column 3 includes baseline covariates instead of subdistrict fixed effects. Controls include per capita health, education, and social institutions as well as the fraction of farmers, fraction of individuals classified as poor, per-captita expenditure levels, and crime rates. Column 4 further includes interactions of these variable with time trends. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.4 Further sensitivity - Cash Transfer Program - RCT

Table A.10: Randomized experiment - alternative outcomes 1

	Suicide rate (extrapolated - Poisson)					Suicide rate (truncated)					Any Suicide				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Treatment	-0.236	-0.160	-0.633**	-0.244	-0.318	-0.225	-0.192	-0.562**	-0.224	-0.306	-0.096	-0.084	-0.142	-0.078	-0.086
	(0.227)	(0.234)	(0.305)	(0.224)	(0.294)	(0.206)	(0.253)	(0.272)	(0.203)	(0.266)	(0.066)	(0.067)	(0.086)	(0.066)	(0.059)
Subdistrict FE	N	N	Y	Y	N	N	N	Y	Y	N	N	N	Y	Y	N
Time FE	N	N	Y	Y	N	N	N	Y	Y	N	N	N	Y	Y	N
Include pre-treatment periods	N	N	N	Y	N	N	N	N	Y	N	N	N	N	Y	N
Baseline suicide	Y	N	N	N	Y	Y	N	N	N	Y	Y	N	N	N	Y
Population weights	Y	Y	Y	Y	N	Y	Y	Y	Y	N	Y	Y	Y	Y	N
Control mean (2011)	1.514	1.514	1.514	1.514	1.775	1.420	1.420	1.420	1.420	1.666	0.578	0.578	0.578	0.578	0.545
N	310	310	620	1240	310	310	310	620	1240	310	310	310	620	1240	310
Census waves	11	11	05 - 11	00 - 11	11	11	11	05 - 11	00 - 11	11	11	11	05 - 11	00 - 11	11

Table A.10 displays RCT treatment effects on three alternative outcomes. All specifications use population weights unless otherwise noted. Columns 1 to 5 show treatment effects on the Poisson-extrapolated suicide rate (extrapolated from the subdistrict average of village-level incidences of at least one suicide in a given year). Columns 6 to 10 report the effects on a truncated version of the suicide rate based on the number of villages with at least one suicide in a subdistrict. Columns 11 to 15 report the treatment effect on a binary variable equal to one if there was at least one suicide in a subdistrict last year. Columns 1, 6, and 11 report an ANCOVA specification. In Column 2, 7, and 12 we report treatment effect estimates of a post comparison of treated and control subdistricts. Columns 3, 8, and 13 report difference-in-differences estimates of the treatment effect. In Columns 4, 9, and 14 we report difference-in-differences results including all pre-treatment time periods. In Columns 5, 10, and 15 we report ANCOVA estimates without using baseline population weights. Standard errors are clustered at subdistrict level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.11: Randomized experiment - alternative outcomes 2

	Suicide rate (2005 population)					Number of suicides (extrapolated - Poisson)					Number of suicides				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Treatment	-0.344	-0.256	-0.663**	-0.313	-0.483	-0.099	-0.129	-0.345**	-0.118	-0.170	-0.149	-0.129	-0.365**	-0.167	-0.232
	(0.281)	(0.293)	(0.328)	(0.280)	(0.340)	(0.133)	(0.164)	(0.171)	(0.131)	(0.182)	(0.161)	(0.164)	(0.183)	(0.157)	(0.250)
Subdistrict FE	N	N	Y	Y	N	N	N	Y	Y	N	N	N	Y	Y	N
Time FE	N	N	Y	Y	N	N	N	Y	Y	N	N	N	Y	Y	N
Include pre-treatment periods	N	N	N	Y	N	N	N	N	Y	N	N	N	N	Y	N
Baseline suicide	Y	N	N	N	Y	Y	N	N	N	Y	Y	N	N	N	Y
Population weights	Y	Y	Y	Y	N	N	N	N	N	Y	N	N	N	N	Y
Control mean (2011)	1.883	1.883	1.883	1.883	2.167	0.878	0.878	0.878	0.878	1.014	1.035	1.035	1.035	1.035	1.228
N	310	310	620	1240	310	310	310	620	1240	310	310	310	620	1240	310
Census waves	11	11	05 - 11	00 - 11	11	11	11	05 - 11	00 - 11	11	11	11	05 - 11	00 - 11	11

Table A.11 displays RCT treatment effects on three alternative outcomes. Specifications use population weights unless otherwise noted. Columns 1 to 5 show treatment effects on the suicide rate based on 2005 population numbers. Columns 6 to 10 report the effects the Poisson extrapolated number of suicides (extrapolated from the subdistrict average of village-level incidences of at least one suicide in a given year). Columns 11 to 15 report the treatment effect on the number of suicides used to construct our main suicide rate. Columns 1, 6, and 11 report an ANCOVA specification. In Column 2, 7, and 12 we report treatment effect estimates of a post comparison of treated and control subdistricts. Columns 3, 8, and 13 report difference-in-differences estimates of the treatment effect. In Columns 4, 9, and 14 we report difference-in-differences results including all pre-treatment time periods. In Columns 5, 10, and 15 we report ANCOVA estimates without using baseline population weights. Standard errors are clustered at subdistrict level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.12: Randomized experiment - drop partially treated

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable: Suicide rate							
Treatment	-0.326	-0.238	-0.640**	-0.640**	-0.453	-1.041*	-0.467
	(0.267)	(0.276)	(0.318)	(0.261)	(0.334)	(0.593)	(0.325)
Subdistrict FE	N	N	Y	Y	Y	Y	N
Time FE	N	N	Y	Y	Y	Y	N
Cluster district	N	N	N	Y	N	N	N
Include pre-treatment periods	N	N	N	N	Y	Y	N
Subdistrict trends	N	N	N	N	N	Y	N
Baseline suicide	Y	N	N	N	N	N	Y
Population weight	Y	Y	Y	Y	Y	Y	N
Control mean (2011)	1.774	1.774	1.774	1.774	1.774	1.774	2.058
N	282	282	564	564	1128	1128	282
Census waves	11	11	05-11	05-11	00-11	00-11	11

Table A.12 reports the RCT effects on the suicide rate excluding partially treated subdistricts. All specifications use population weights from 2005 unless otherwise noted. The suicide rate is defined as described in Table A.1. Column 1 reports an ANCOVA specification controlling for the 2005 suicide rate. Column 2 displays the treatment effect estimate of a post comparison of treated and control subdistricts. Columns 3 to 6 report difference-in-differences estimates of the treatment effect. Standard errors are clustered at the subdistrict level unless otherwise noted. In Column 3 we control for time fixed effects as well as subdistrict fixed effects. Column 4 reports standard errors clustered at district level. Column 5 includes data from the 2003 and 2000 census waves. Column 6 further includes subdistrict specific time-trends on top of subdistrict and time fixed effects. Column 7 shows the ANCOVA specification without population weights.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.13: Main results: randomized experiment - without population weights

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: Suicide rate						
Treatment	-0.474	-0.258	-0.809**	-0.809**	-0.466	-1.302*
	(0.325)	(0.275)	(0.401)	(0.315)	(0.334)	(0.726)
Subdistrict FE	N	N	Y	N	Y	Y
Time FE	N	N	Y	Y	Y	Y
Cluster district	N	N	N	Y	N	N
Include pre-treatment periods	N	N	N	N	Y	Y
Subdistrict trends	N	N	N	N	N	Y
Baseline suicide	Y	N	N	N	N	N
Population weights	N	N	N	N	N	N
Control mean (2011)	2.058	2.058	2.058	2.058	2.058	2.058
N	310	310	620	620	1240	1240
Census waves	11	11	05-11	05-11	00-11	00-11

Table 3 displays the results of the RCT experiment without population weights. The suicide rate is defined as described in Table A.1. Column 1 reports an ANCOVA specification controlling for the 2005 suicide rate. Column 2 displays the treatment effect estimate of a post comparison of treated and control subdistricts. Columns 3 to 6 report difference-in-differences estimates of the treatment effect. Standard errors are clustered at the subdistrict level unless otherwise noted. Estimates are weighted using population size from 2005 unless otherwise noted. In Column 3 we control for time fixed effects as well as subdistrict fixed effects. Column 4 reports standard errors clustered at district level. Column 5 includes data from the 2003 and 2000 census waves. Column 6 further includes subdistrict specific time trends on top of subdistrict and time fixed effects. Standard errors clustered at the subdistrict level in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.14: Main results: randomized experiment - 2006 boundaries

	(1)	(2)	(3)	(4)	(5)
Dependent variable: Suicide rate					
Treatment	-0.248	-0.192	-0.544*	-0.544*	-0.537
	(0.256)	(0.263)	(0.309)	(0.294)	(0.383)
Subdistrict FE	N	N	Y	Y	N
Time FE	N	N	Y	Y	N
Cluster district	N	N	N	Y	N
Baseline suicide	Y	N	N	N	Y
Population weights	Y	Y	Y	Y	N
Control mean (2011)	1.720	1.720	1.720	1.720	2.205
N	351	351	702	702	351
Census waves	11	11	05-11	05-11	11

Table A.14 displays the results of the RCT experiment on the suicide rate according to 2006 subdistrict boundaries with 2005 population weights unless otherwise noted. The suicide rate is defined as described in Table A.1. Standard errors are clustered at subdistrict level unless noted otherwise. Column 1 reports an ANCOVA specification controlling for the 2005 suicide rate. Column 2 displays the treatment effect estimate of a post comparison of treated and control subdistricts. Columns 3 and 4 report difference-in-differences estimates of the treatment effect. In Column 3 we control for time fixed effects as well as subdistrict fixed effects. Column 4 reports standard errors clustered at district level. Column 5 reports the ANCOVA specification without population weights. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.15: Main results: randomized experiment - additional robustness checks

	(1)	(2)	(3)	(4)
Dependent variable: Suicide rate				
Treatment	-0.665**	-0.649**	-0.809**	-0.665**
	(0.330)	(0.272)	(0.405)	(0.321)
Subdistrict FE	N	Y	N	N
Time FE	Y	Y	Y	Y
District FE	Y	N	N	N
District trends	N	Y	N	N
Baseline controls	N	N	Y	Y
Baseline controls × Post	N	N	N	Y
Population weights	Y	Y	Y	Y
Control mean (2011)	1.774	1.774	1.774	1.774
N	620	620	620	620
Census waves	05-11	05-11	05-11	05-11

Table A.15 displays the results using the RCT experiment on the suicide rate with population weights. The suicide rate is defined as described in Table A.1. Columns 1 to 4 report difference-in-differences estimates of the treatment effect. Column 1 uses district instead of subdistrict fixed effects, while also controlling for treatment status. Column 2 controls for district specific time trends. Column 3 includes baseline covariates instead of subdistrict fixed effects. Controls include per capita health, education, and social institutions as well as the fraction of farmers, fraction of individuals classified as poor, per-captita expenditure levels, and crime rates. Column 4 further includes interactions of these variable with time trends. Standard errors clustered in subdistrict level in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.5 Dynamics of treatment effects

Table A.16: Dynamics of treatment effects

	(1) RCT	(2) Treat 07-08	(3) Treat 10-11	(4) Treat 12-13
Dependent variable: Suicide rate				
Panel A: First post treatment census wave				
Treatment	-0.665**	-0.509***	-0.289	-0.280*
	(0.318)	(0.136)	(0.175)	(0.156)
Years since launch of PKH	3-4	3-4	0-1	1-2
Census waves	05 & 11	05 & 11	05 & 11	11 & 14
Number of treated	167	621	233	1137
Number of counterfactuals	143	1186	1186	1186
Receiving Treatment	Y	Y	Partial	Y
Panel B: Second post treatment census wave				
Treatment	-0.273	-0.433**	-0.540**	
	(0.371)	(0.207)	(0.237)	
Years since launch of PKH	6-7	6-7	3-4	
Census waves	05 & 14	05 & 14	05 & 14	
Number of treated	167	621	233	
Number of counterfactuals	143	1186	1186	
Receiving treatment	Partial	Partial	Y	

Table A.16 displays treatment effects on the suicide rate for each treatment wave relative to never treated subdistricts. The suicide rate is defined as explained in Table A.1. Estimates are weighted using 2005 population size. Panel A shows treatment effects in the first PODES survey after subdistricts started receiving the conditional cash transfer program. Panel B shows treatment effects in the second PODES survey after subdistricts started receiving the program. Impacts in Columns 1 and 2 report treatment effect estimates past the six year duration of the program. Standard errors are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.17: Dynamics of treatment effects: not population-weighted

	(1) RCT	(2) Treat 07-08	(3) Treat 10-11	(4) Treat 12-13
Dependent variable: Suicide rate				
Panel A: First post treatment census wave				
Treatment	-0.809**	-0.612***	-0.162	-0.628**
	(0.401)	(0.173)	(0.302)	(0.259)
Years since launch of PKH	3-4	3-4	0-1	1-2
Census waves	05 & 11	05 & 11	05 & 11	11 & 14
Number of treated	167	621	233	1137
Number of counterfactuals	143	1186	1186	1186
Receiving Treatment	Y	Y	Partial	Y
Panel B: Second post treatment census wave				
Treatment	-0.017	-0.629**	-0.733*	
	(0.486)	(0.305)	(0.440)	
Years since launch of PKH	7-8	7-8	3-4	
Census waves	05 & 14	05 & 14	05 & 14	
Number of treated	167	621	233	
Number of counterfactuals	143	1186	1186	
Receiving treatment	Partial	Partial	Y	

Table A.17 displays treatment effects for each treatment wave relative to subdistricts who did not receive the treatment until 2013 without using population weights. The suicide rate is defined as described in Table A.1. Panel A shows treatment effects in the first PODES survey after subdistricts started receiving the conditional cash transfer program. Panel B shows treatment effects in second PODES survey after subdistricts started receiving the program. Impacts in Columns 1 and 2 report treatment effect estimates past the six year duration of the program. Standard errors are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.18: Roll-out effects: treatment intensity

	(1)	(2)
Dependent variable: Suicide rate		
Treatment	-0.358***	-0.336***
	(0.101)	(0.101)
Intensity		-0.012
		(0.024)
N	9414	9414
Census Waves	05-14	05-14

Table A.26 displays treatment effects on the suicide rate controlling for treatment duration in years since start of the program. The suicide rate is defined as described in Table A.1. All specifications include time and subdistrict fixed effects. Standard errors are clustered at the district level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.6 Rainfall

Table A.19: Rainfall: symmetry of effects

	(1)	(2)	(3)
Dependent variable: Suicide rate			
Rain (z-scored)	0.025	-0.004	0.025
	(0.062)	(0.059)	(0.061)
	[0.058]	[0.058]	[0.056]
Positive Shock	-0.201**	-0.149*	-0.224**
	(0.096)	(0.090)	(0.090)
	[0.065]	[0.063]	[0.067]
Negative Shock	0.124	0.077	0.140
	(0.105)	(0.101)	(0.102)
	[0.089]	[0.088]	[0.089]
p(|Positive Shock| = |Negative Shock|)	0.598	0.613	0.551
Subdistrict FE	Y	Y	Y
Time FE	Y	Y	Y
Subdistrict trends	N	Y	N
Include lagged rainfall	N	N	Y
N	15690	15690	15690
Census waves	00-14	00-14	00-14

Table A.19 displays the impact of non-linear measures of rainfall on the suicide rate. The suicide rate is defined as described in Table A.1. The Table reports the results of a regression of standardized rainfall and dummies indicating rainfall above the 66^{th} and below the 33^{th} percentile of z-scored rainfall in our sample. All specifications include subdistrict and year fixed effects. Column 2 also controls for subdistrict specific time trends. Column 3 includes the first, second and third lag of rainfall. Parentheses report Conley (1999) standard errors, allowing for arbitrary time correlation and two-dimensional spatial correlation within a 100 kilometre radius of the subdistrict centroid (used for stars). Standard errors clustered at the district level are displayed in square brackets.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.20: Interactions between agricultural productivity shocks and the cash transfers - not population weighted

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: Suicide rate						
Rain (z-scored)	-0.145***	-0.213***	-0.140***	-0.170***	-0.154***	-0.234***
	(0.054)	(0.061)	(0.051)	(0.053)	(0.055)	(0.064)
	[0.059]	[0.065]	[0.050]	[0.053]	[0.056]	[0.066]
Treat		-0.233		-0.223		-0.163
		(0.151)		(0.171)		(0.157)
		[0.161]		[0.185]		[0.171]
Rain (z-scored)× Treat		0.314***		0.200*		0.361***
		(0.107)		(0.109)		(0.117)
		[0.105]		[0.119]		[0.117]
Subdistrict FE	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y
Subdistrict trends	N	N	Y	Y	N	N
Include lagged rainfall	N	N	N	N	Y	Y
N	15690	15690	15690	15690	15690	15690
Census waves	00-14	00-14	00-14	00-14	00-14	00-14

Table A.20 displays the impact of rainfall on suicides. The suicide rate is defined as described in Table A.1. Odd columns report the impact of standardized rainfall on suicides. Even columns include the treatment variable from the conditional cash transfer roll-out and the interaction with rainfall. All specifications include subdistrict and year fixed effects. Columns 3 and 4 additionally control for subdistrict time trends. Columns 5 and 6 also include the first, second and third lag of rainfall. Parentheses report Conley (1999) standard errors, allowing for arbitrary time correlation and two-dimensional spatial correlation within a 100 kilometre radius of the subdistrict centroid (used for stars). Standard errors clustered at the district level are in square brackets.

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

Table A.21: Rainfall: non-linear interaction of rainfall and cash transfer

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: Suicide rate						
Positive shock	-0.181**	-0.178**	-0.152*	-0.151*	-0.201**	-0.202**
	(0.084)	(0.085)	(0.080)	(0.080)	(0.081)	(0.083)
	[0.054]	[0.054]	[0.053]	[0.053]	[0.059]	[0.059]
Positive shock × Treat		0.406		0.253		0.502
		(0.508)		(0.402)		(0.517)
		[0.529]		[0.468]		[0.551]
Negative shock	0.099	0.156	0.092	0.119	0.125	0.166*
	(0.096)	(0.100)	(0.090)	(0.094)	(0.093)	(0.099)
	[0.071]	[0.073]	[0.069]	[0.071]	[0.070]	[0.073]
Negative shock × Treat		-0.195		-0.115		-0.165
		(0.138)		(0.126)		(0.138)
		[0.073]		[0.071]		[0.073]
Treat		-0.223*		-0.295***		-0.244**
		(0.121)		(0.101)		(0.121)
		[0.121]		[0.113]		[0.120]
Subdistrict FE	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y
Subdistrict trends	N	N	Y	Y	N	N
Include lagged rainfall	N	N	N	N	Y	Y
N	15690	15690	15690	15690	15690	15690
Census waves	00-14	00-14	00-14	00-14	00-14	00-14

Table A.21 displays the impact of non-linear measures of rainfall on the suicide rate. The suicide rate is defined as described in Table A.1. The Table reports the results of a regression of dummies indicating rainfall above the 66^{th} and below the 33^{th} percentile of z-scored rainfall in our sample. Even columns also display the interaction with receiving the conditional cash transfer. All specifications include subdistrict and year fixed effects. Columns 3 and 4 also control for subdistrict specific time trends. Columns 5 and 6 includes the first, second and third lag of z-scored rainfall. Parentheses report Conley (1999) standard errors, allowing for arbitrary time correlation and two-dimensional spatial correlation within a 100 kilometre radius of the subdistrict centroid (used for stars). Standard errors clustered at the district level are displayed in square brackets.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.22: Interactions between agricultural productivity shocks and the cash transfers - detrended rainfall and population weighted

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: Suicide rate						
Rain (z-scored)	-0.097**	-0.112**	-0.091**	-0.102**	-0.104**	-0.125**
	(0.043)	(0.046)	(0.041)	(0.043)	(0.045)	(0.050)
	[0.033]	[0.036]	[0.033]	[0.036]	[0.035]	[0.040]
Treat		-0.311***		-0.338***		-0.291***
		(0.093)		(0.088)		(0.097)
		[0.105]		[0.099]		[0.108]
Rain (z-scored)× Treat		0.074		0.052		0.097
		(0.078)		(0.075)		(0.084)
		[0.075]		[0.073]		[0.081]
Subdistrict FE	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y
Subdistrict trends	N	N	Y	Y	N	N
Include lagged rainfall	N	N	N	N	Y	Y
N	15620	15620	15620	15620	15620	15620
Census waves	00-14	00-14	00-14	00-14	00-14	00-14

Table A.22 displays the impact of rainfall on suicides. The suicide rate is defined as described in Table A.1. Rainfall is net of subdistrict level trends and standardized to have mean zero and standard deviation one. Odd columns report the impact of rainfall on suicides. Even columns include the treatment variable from the conditional cash transfer roll-out and the interaction with rainfall. All specifications include subdistrict and year fixed effects. Columns 3 and 4 additionally control for subdistrict time trends. Columns 5 and 6 also include the first, second and third lag of rainfall. Observations are weighted by 2005 subdistrict population. Parentheses report Conley (1999) standard errors, allowing for arbitrary time correlation and two-dimensional spatial correlation within a 100 kilometre radius of the subdistrict centroid (used for stars). Standard errors clustered at the district level are in square brackets.

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

Table A.23: Interactions between agricultural productivity shocks and the cash transfers - detrended and not population weighted

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: Suicide rate						
Rain (z-scored)	-0.149***	-0.194***	-0.139**	-0.175***	-0.162***	-0.217***
	(0.057)	(0.060)	(0.056)	(0.058)	(0.059)	(0.064)
	[0.050]	[0.055]	[0.049]	[0.053]	[0.049]	[0.057]
Treat		-0.414***		-0.479***		-0.376***
		(0.128)		(0.127)		(0.130)
		[0.147]		[0.142]		[0.150]
Rain (z-scored)× Treat		0.256**		0.204*		0.301**
		(0.110)		(0.108)		(0.118)
		[0.107]		[0.106]		[0.117]
Subdistrict FE	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y
Subdistrict trends	N	N	Y	Y	N	N
Include lagged rainfall	N	N	N	N	Y	Y
N	15620	15620	15620	15620	15620	15620
Census waves	00-14	00-14	00-14	00-14	00-14	00-14

Table A.23 displays the impact of rainfall on suicides. The suicide rate is defined as described in Table A.1. Rainfall is net of subdistrict level trends and standardized to have mean zero and standard deviation one. Odd columns report the impact of rainfall on suicides. Even columns include the treatment variable from the conditional cash transfer roll-out and the interaction with rainfall. All specifications include subdistrict and year fixed effects. Columns 3 and 4 additionally control for subdistrict time trends. Columns 5 and 6 also include the first, second and third lag of rainfall. Parentheses report Conley (1999) standard errors, allowing for arbitrary time correlation and two-dimensional spatial correlation within a 100 kilometre radius of the subdistrict centroid (used for stars). Standard errors clustered at the district level are in square brackets.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.7 Microdata on Depression: IFLS

Table A.24: Microdata on depression: individual level heterogeneity

	Male	Aged < 50 (median)	Baseline expenditure (z-scored)
	(1)	(2)	(3)
Dependent variable: depression z-score			
Rain (z-scored)	-0.175***	-0.067	-0.128***
	(0.052)	(0.047)	(0.042)
	[0.050]	[0.049]	[0.042]
Rain (z-scored) × Heterogeneity	0.107*	-0.152**	0.047**
	(0.059)	(0.066)	(0.022)
	[0.057]	[0.068]	[0.023]
Individual FE	Y	Y	Y
Time FE	Y	Y	Y
N	5192	5192	5188
IFLS Waves used	4-5	4-5	4-5

Table A.24 displays heterogeneous effects of rainfall on depression z-scores by individual level characteristics using the Indonesian Family Life Survey (IFLS). Depression scores are measured in 2011 and 2014 using the 10 item CES-D scale (Cheung et al., 2007; Radloff, 1997). Age is based on the respondent's age in IFLS wave 4. Baseline expenditure uses pre-determined per-capita expenditure from IFLS wave 3. All specifications control for differential trends. The sample is restricted to individuals tracked in all five IFLS waves. The sample is further restricted to individuals living in households with a member working in agriculture (self-employed without or with temporary workers, or as casual or family workers) in any of the five waves. IFLS wave 1 contains no coded sector information. Therefore, all individuals working as temporary workers or those who are self-employed without permanent workers are counted as working in agriculture. This makes up 60% of IFLS individuals. The coefficients of the heterogeneity variable are omitted to save space. All specifications are conditional on individual and time fixed effects and allowing for different trends by the heterogeneity variable. Parentheses report Conley (1999) standard errors, allowing for arbitrary time correlation and two-dimensional spatial correlation within a 100 kilometre radius of the subdistrict centroid (used for stars). Standard errors clustered at the district level are in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.25: Correlates of depression scores

	(1)	(2)	(3)
Dependent variable: Baseline depression z-score			
Bottom decile of food expenditure	0.102***		0.097***
	(0.032)		(0.033)
CCT recipient in 2014		0.146***	0.133***
		(0.050)	(0.051)
N	19677	19637	19637
IFLS wave used	4	4	4

Table A.25 displays correlations with depression scores in the Indonesian Family Life Survey (IFLS) wave 4. IFLS 4 was collected before the roll-out of the conditional cash transfer program in 2007 and 2008. Depression scores are measured using the 10 item CES-D scale (Cheung et al., 2007; Radloff, 1997). Column 1 shows the correlation between being in the bottom decile of the IFLS wave 4 per-capita household expenditure distribution and depression z-scores. Column 2 shows the correlation between having received the program at some point between IFLS wave 4 and wave 5 (collected in IFLS wave 5 (2014)). Column three includes both variables simultaneously. The sample is restricted to individuals tracked in IFLS wave 4 and wave 5. Standard errors are clustered at the subdistrict level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.8 Mechanisms and heterogeneity

Table A.26: Including endogenous controls

	(1)	(2)	(3)	(4)	(5)
Dependent variable: Suicide rate					
Treatment	-0.358***	-0.327***	-0.366***	-0.325***	-0.271***
	(0.101)	(0.095)	(0.100)	(0.107)	(0.103)
Subdistrict FE	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y
Crime controls	N	Y	N	N	N
Institution controls	N	N	Y	N	N
Social capital controls	N	N	N	N	Y
Census waves	05-14	05-14	05-14	05-11	05-11
Control mean (2011 & 2014)	1.797	1.797	1.797	1.797	1.797
N	9414	9414	9414	6276	6276

Table A.26 displays treatment effects on the suicide rate controlling for potential mediators. The suicide rate is defined as described in Table A.1. All estimations are weighted using 2005 population size. All specifications include time and subdistrict fixed effects. In Columns (1) to (3) we use observations from the 2005, 2011 and 2014 census. In Columns (4) and (5) we use observations from the 2005 and 2011 village census, as data on social capital was not available in the 2014 census. Standard errors are clustered at the district level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

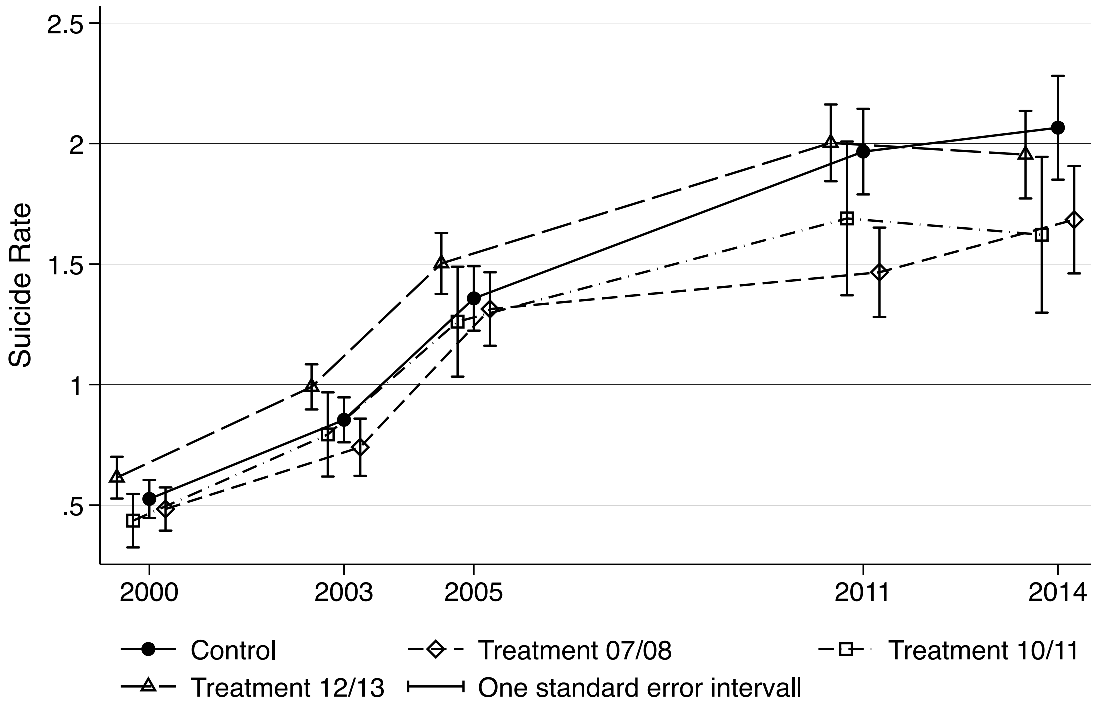
Table A.27: Subdistrict level heterogeneous treatment effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable: Suicide rate							
Treatment	-0.234	-0.357***	-0.356***	-0.380***	-0.346***	-0.283**	-0.274*
	(0.147)	(0.101)	(0.113)	(0.103)	(0.108)	(0.139)	(0.145)
Treatment × Fract poor (z)	0.218*	0.122					
	(0.122)	(0.095)					
Treatment × Perc. Farmers (z)	-0.105		-0.117				
	(0.102)		(0.072)				
Treatment × Per Capita Excp (z)	0.120			0.026			
	(0.086)			(0.059)			
Treatment × Per Capita Crimes (z)	0.145				0.077		
	(0.218)				(0.183)		
Treatment × Per Capita Social Institutions (z)	0.277					0.015	
	(0.198)					(0.191)	
Treatment × Per Capita Health Institutions (z)	-0.244						-0.127
	(0.256)						(0.223)
Minimum detectable effect size		0.266	0.202	0.165	0.512	0.535	0.624
N	9414	9414	9414	9414	9414	9414	9414
Census waves	05-14	05-14	05-14	05-14	05-14	05-14	05-14

Table A.27 displays heterogeneous treatment effects on the suicide rate for the cash transfer roll-out by baseline district level characteristics. The suicide rate is defined as described in Table A.1. All specification include time and subdistrict fixed effects. All estimations are weighted using 2005 population size. All interaction term variables, interact$_{sd}$, are based on the 2005 census, and are z-scored using the mean and standard deviation of the sample. All specifications control for differential trends by interaction variables, interact$_{sd}$ × post$_t$, where post is defined as post-baseline years. "Minimum detectable effect size" provides us with the minimum detectable effect size at 0.8 power and significance levels of 0.05 for heterogeneous treatment effects for the different interactions variables. Estimates are based on years 2005 to 2014. Standard errors are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

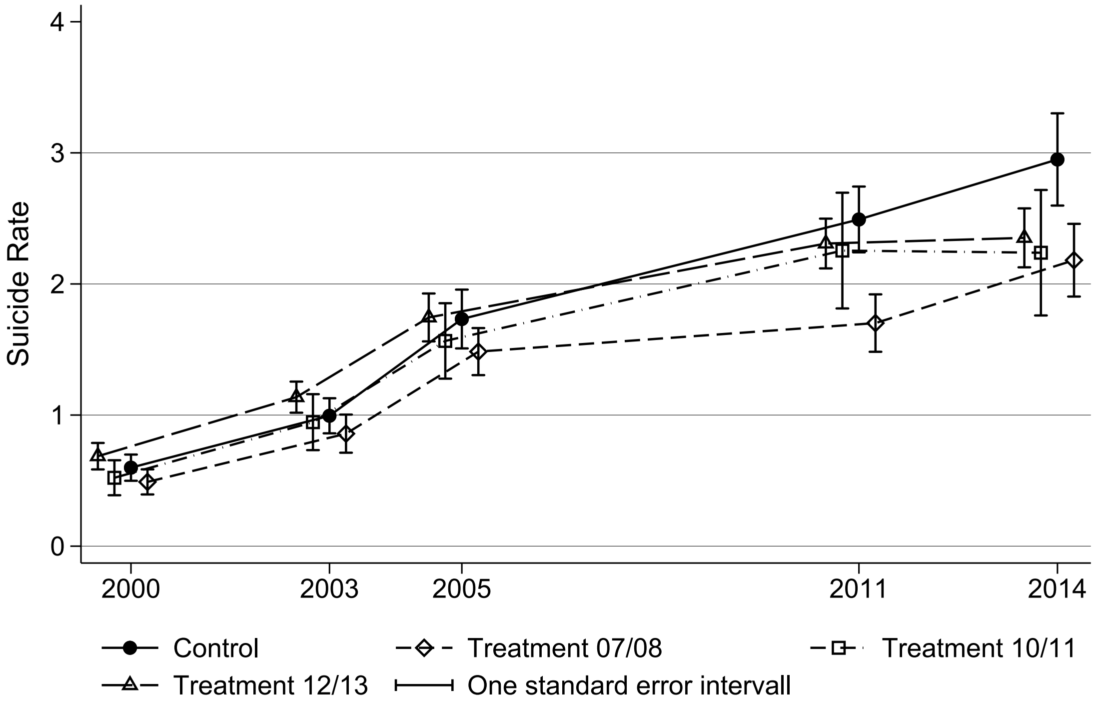
B Additional Figures

Figure A.1: Mean suicide rates over time



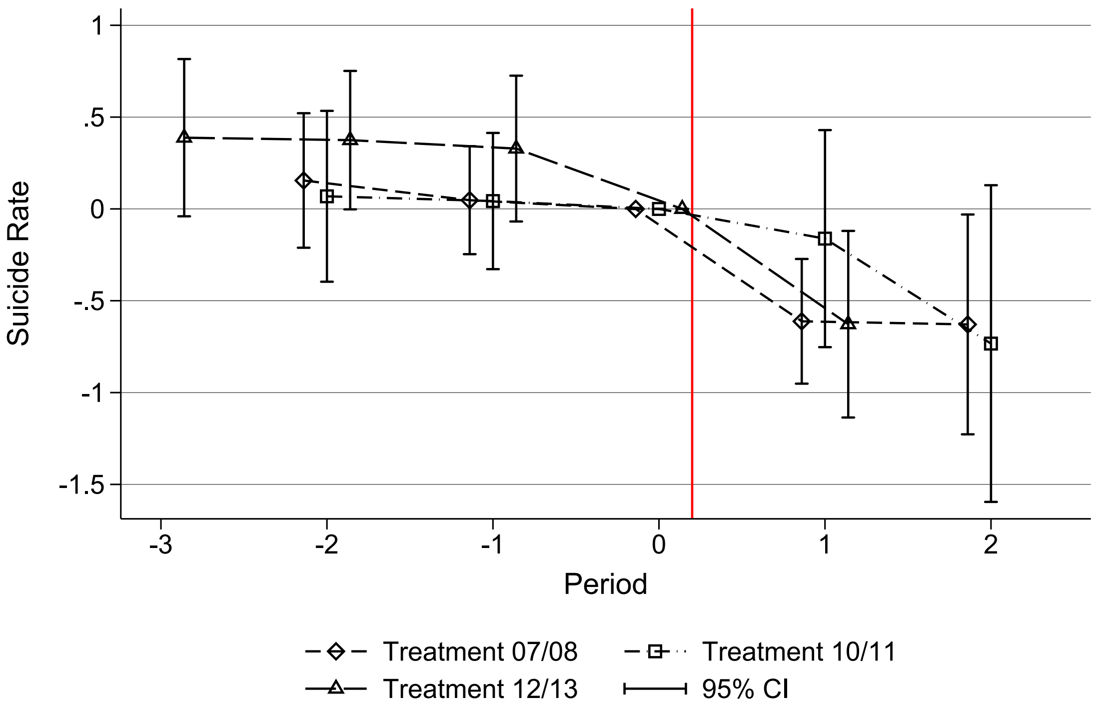
Notes: Figure A.1 displays mean suicide rates over time by treatment wave. The definition of the suicide rate per 100,000 people changes slightly over time. In 2014 it is defined as the number of suicides and suicide attempts per 100,000. In 2011 it is defined as the number of suicides per 100,000. From 2000 to 2005 the suicide rate is defined as the number of villages in a given subdistrict with at least one suicide per 100,000.

Figure A.2: Mean suicide rates by treatment group over time – not population-weighted



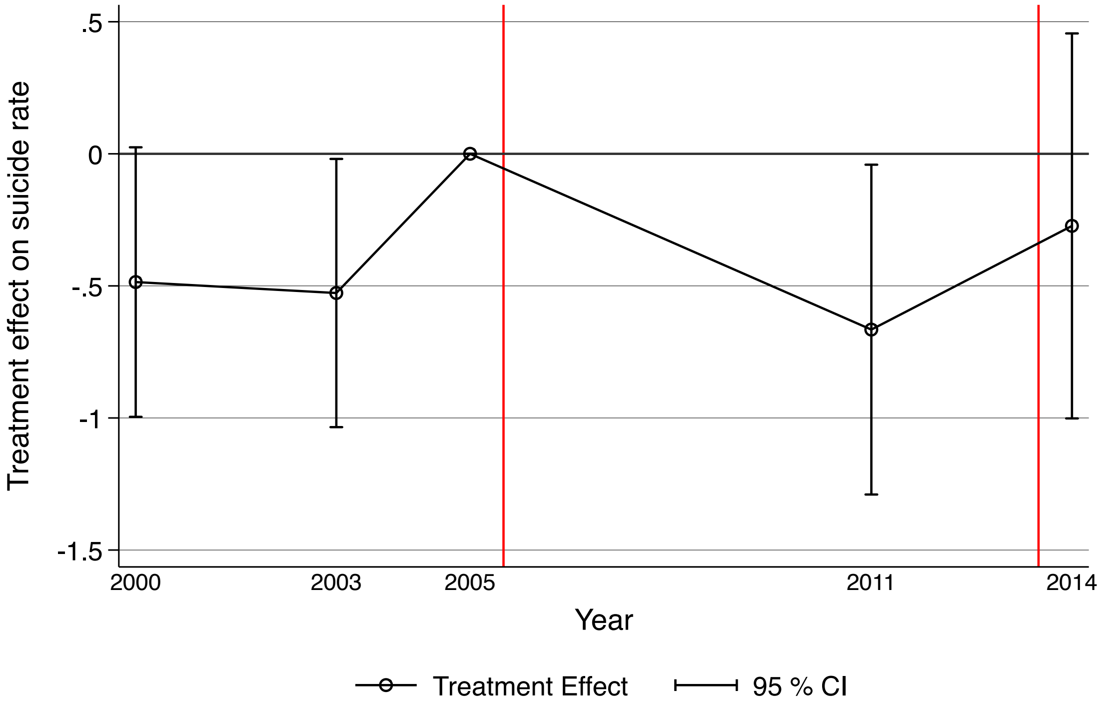
Notes: Figure A.2 displays mean suicide rates over time by treatment wave without population weights. The suicide rate is defined as in Figure A.1.

Figure A.3: Event-study: roll-out of conditional cash transfer program – not population-weighted



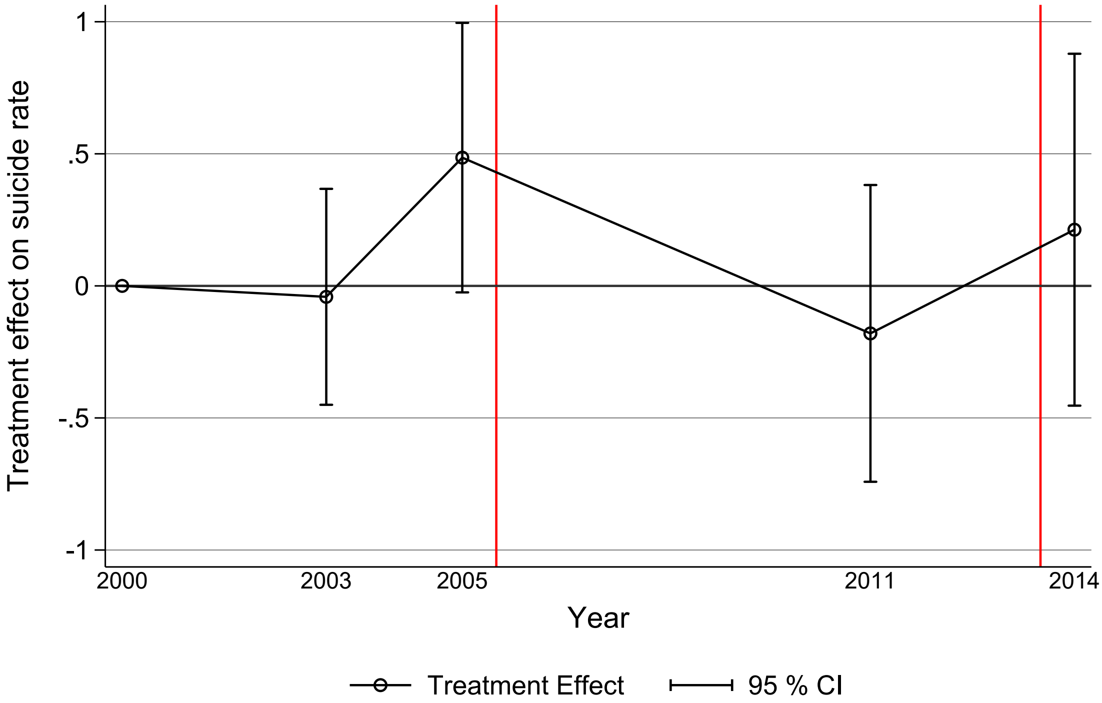
Notes: Figure 1 displays pre-trends relative to the timing of the introduction of the conditional cash transfer program by treatment wave without population weights. The suicide rate is defined as in Figure A.1. Differences in the first period before the treatment are normalized to zero. The displayed coefficients are the difference-in-differences treatment effect estimates relative to subdistricts that had not received the treatmet until 2014. All standard errors are clustered at the district level. The vertical red line indicates between which periods the program was first introduced.

Figure A.4: Event-study: randomized controlled trial



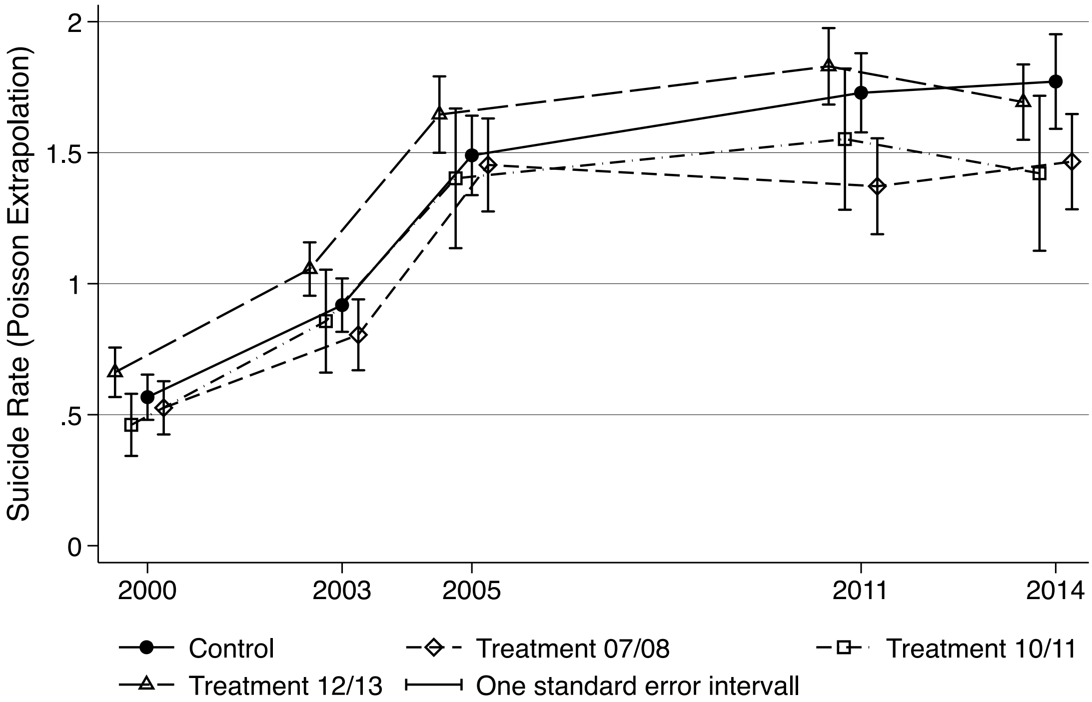
Notes: Figure A.4 displays the coefficients of an interaction between an indicator for whether a given subdistrict was randomly chosen to receive the PKH cash transfer program and year dummies, conditional on year and subdistrict fixed effects. Effects in 2005 are normalized to zero. Standard errors are clustered at subdistrict level. The suicide rate is defined as in Figure A.1.

Figure A.5: Event-study relative to 2000: randomized controlled trial



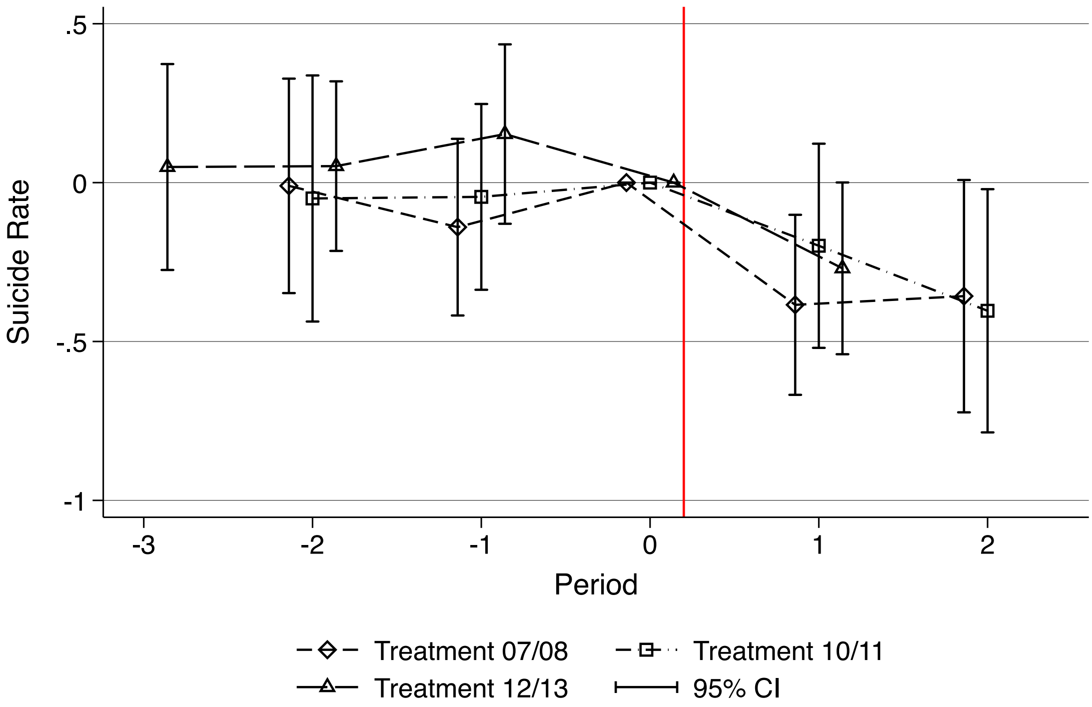
Notes: Figure A.5 displays the coefficients of an interaction between an indicator for whether a given subdistrict was randomly chosen to receive the PKH cash transfer program and year dummies, conditional on year and subdistrict fixed effects. Effects in 2000 are normalized to zero. Standard errors are clustered at subdistrict level. The suicide rate is defined as in Figure A.1.

Figure A.6: Mean suicide rates over time (Poisson extrapolation)



Notes: Figure A.6 displays mean Poisson-extrapolated suicide rates over time by treatment wave with 2005 population weights. Subdistrict number of suicides is extrapolated from mean incidence of at least one subdistrict assuming a homogeneous village level Poisson distribution within each subdistrict and year.

Figure A.7: Event-study: roll-out of conditional cash transfer program (Poisson extrapolation)



Notes: Figure A.7 displays pre-trends relative to the timing of the introduction of the conditional cash transfer program by treatment wave. The suicide rate is defined as in Figure A.6. All specifications use 2005 population weights. Differences in the first period before the treatment are normalized to zero. The displayed coefficients are the difference-in-differences treatment effect estimates relative to never treated subdistricts. All standard errors are clustered at the district level. The vertical red line indicates between which periods the program was first introduced.