Do Direct Cash Transfers to Farming Households Alleviate Financial Distress and Improve Farm Productivity? Evidence from Eastern India

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(Version 14th September 2022)

Abstract

This paper studies the impact of direct income transfers, using a natural experiment arising from the "Krushak Assistance for Livelihood and Income Augmentation" program, a public program designed to deliver unconditional cash transfers for income support to small and marginal farmers in the Indian state of Odisha. We study the effect of these transfers at the household level by making use of a nationally representative household survey. This paper finds that while the transfer does not have a significant impact on overall indebtedness or post-program borrowing levels, it does have a substantial impact on the sources through which credit is procured. The transfers are also shown to cause a decline in the area cultivated by households in the crop season, following the commencement of the program but no significant impacts are found on investment into farm inputs or on crop yield.

Key Words: Cash Transfer, KALIA, Odisha, Impact evaluation, DID
JEL Code: C23, D13, H53

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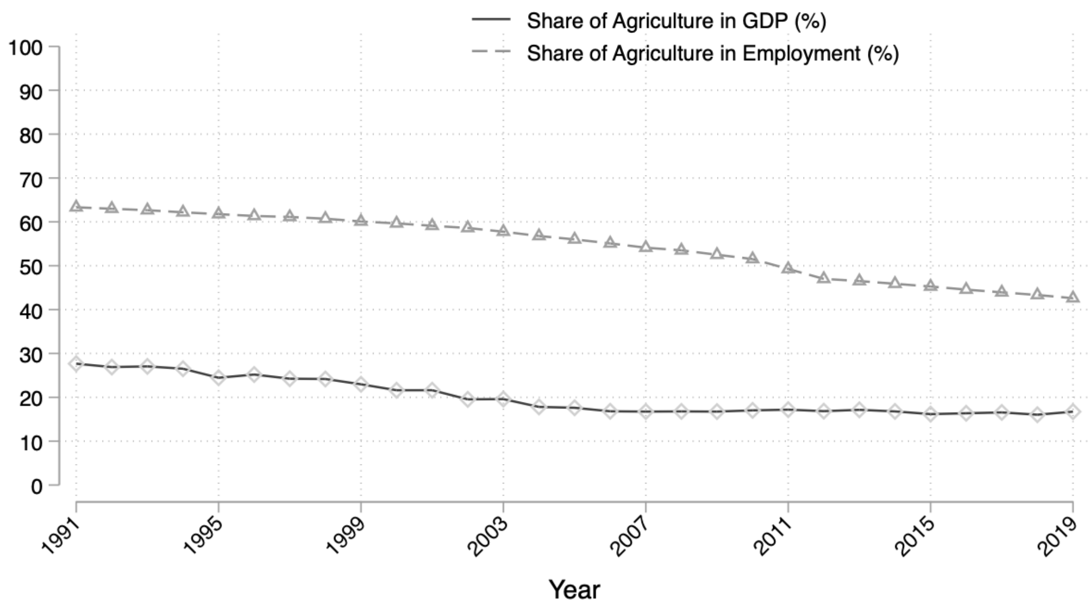
I. Introduction

Agriculture in India may have achieved self-sufficiency[3], but it is far from lucrative for the average Indian famer. Recent estimates of the annual income of the average Indian agricultural household[4] stand at around Rs. 1,21,536 (~$1500) (NSO, 2021). Although, farm income has risen over the last two decades, but so has the sectoral disparity in incomes with the ratio of income per non-agriculture worker to income per cultivator being 3.12. The sectoral disparity in income can be attributed to the structural transformation of the Indian economy, which lowered the share of agriculture in GDP, and employment, but the former declined significantly more than the latter (Figure 1), thereby reducing productivity per agricultural worker (15th Finance Commission). Likewise, there is significant disparity between earnings of households with small landholdings and those with large landholdings (Figure 2(a)). This variation in income by land size is largely due to the difference in yield and productivity by land size. Large farms have had greater success in leveraging modern farming equipment, advanced irrigation technology, and expensive high-yielding hybrid seeds to enhance productivity. These differences are especially concerning as more than 85% of agricultural households in India own less than 2 hectare of land and are classified as "small and marginal" farmers (Figure 2(b)).

Figure 1. Structural Transformation of the Indian Economy



Source: World Bank national accounts and International Labour Organization

[3] India is a net exporter of food grains and ranks second in global food production (ITA 2021; FAO 2022).
[4] Agricultural households are defined as households where at least one member is self -employed in agriculture and whose annual value of produce from such activity exceeds Rs. 4,000 (NSO).

Figure 2(a). Agricultural Income per Household by Land Size

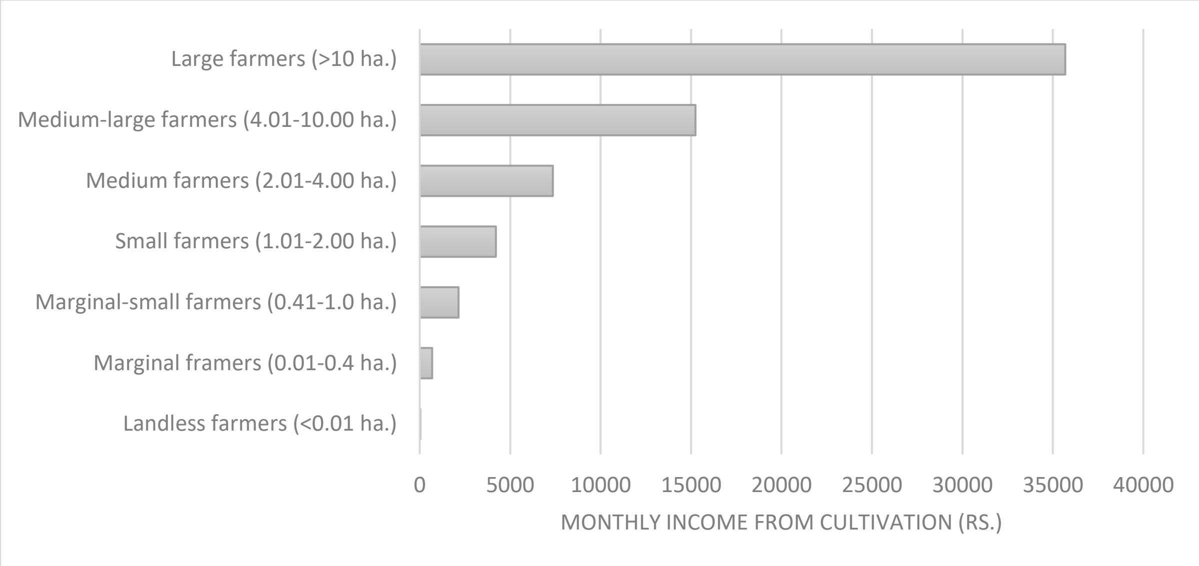
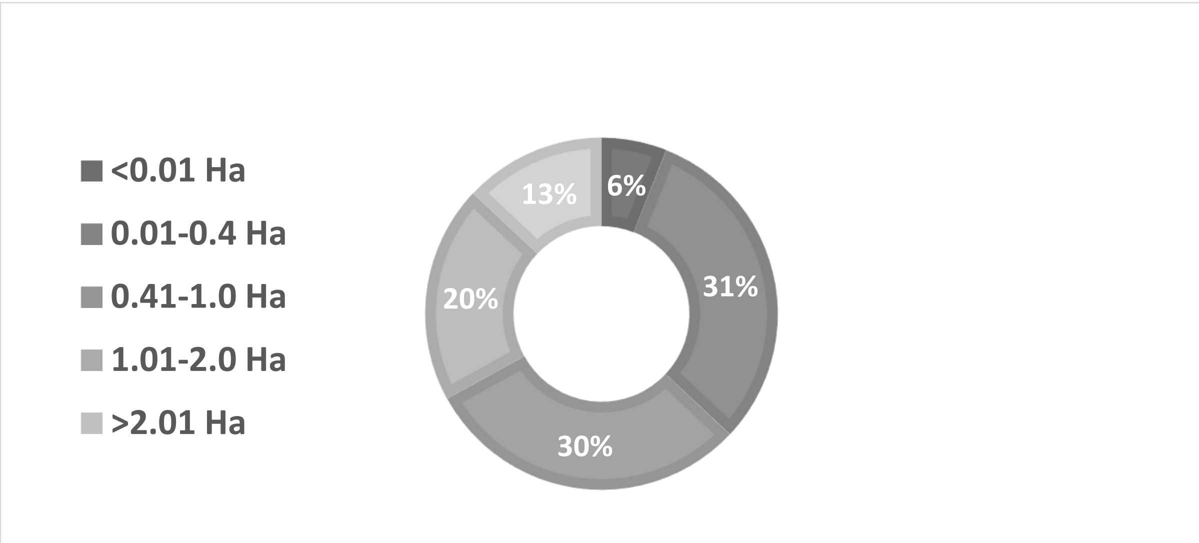


Figure 2 (b) Distribution of Agricultural Households by Land Size



Source 2(a): NSO 2014; Source 2(b): NAFIS 2018

Low and unpredictable levels of income have resulted in high levels of rural debt obtained primarily, to finance personal consumption needs, and at high interest rates from informal lenders. High indebtedness pushes households into a debt trap by imposing further constraints on liquidity, wherein additional debt is secured to finance repayment of existing debt. Additionally, it can also limit savings and productive investments, as predicted by the standard model of “debt overhang” (Myers1977; Krugman 1988).

Recent years have seen a stronger focus of Indian agricultural policy on raising income of agriculture households and alleviating their financial distress, particularly the debt crisis.

Increasing the productivity of small and marginal farms is considered one of the means to achieving this end. Relaxing capital constraints through increased access to micro-credit to spur investment has delivered modest results (Stewart et al. 2010; Banerjee et al. 2015). At the same time, unconditional income transfers (basic income) have garnered attention in many developing economies as tools for poverty alleviation (Baird, De Hoop, and Özler 2013; Blattman, Fiala, and Martinez 2014; Bannerjee, 2016; Ghatak et.al. 2019). The relative merits and demerits of cash transfers with respect to in-kind transfers have been widely studied in different settings. Cash transfers function better at meeting the diverse needs of heterogenous households by allowing recipients the flexibility to choose the way they use the transfer. Delivery costs and monitoring effort are also reduced.

More specifically, cash transfers designed to provide income support to poor, agricultural households could be channelled into productive investments and potentially improve agricultural outcomes (Boone et al. 2013; Ambler et al. 2020). However, studies examining the impact of various types of transfers on agriculture have found varying levels of impacts, highlighting the importance of the specific context and the relative suitability of the design of the transfer program in the given context. For instance, small, regular transfers for sustained income support have been found to yield modest results for lumpy investments, but are relatively more effective at insulating households against income shocks than a large, one-time transfer (Beaman el al. 2015; Ambler et al. 2018). It is important to note that most of the evidence on the effectiveness of cash transfers in agriculture currently comes from Sub-Saharan Africa. This paper examines the effectiveness of income support "cash" transfer as a policy instrument in the context of agricultural households in India.

With improving financial inclusion in India, thanks to the rollout of initiatives designed to open bank accounts for the largely unbanked, rural population, income support transfers can now be readily disbursed to the target population with minimal leakages. Direct income transfer is now a viable policy instrument. Thus, the timing is ripe for governments to design and pilot schemes that involve transferring money to the target group. In this paper, an income support transfer program underway in the Indian state of Odisha is evaluated. The program, named Krushak Assistance for Livelihood and Income Augmentation (KALIA), was announced in December of 2018, and is designed to provide financial assistance of Rs. 25,000 (~$315) over five crop seasons between 2019 and 2022[5] to small and marginal agricultural households, averaging Rs 5,000 per crop season, and Rs 10,000 per annum. Further details about the program are provided in the subsequent section.

This paper attempts to estimate the causal impact of the KALIA program on a wide range of outcomes. The outcomes most widely evaluated in the literature in the context of transfers to agricultural households include crop production (in units, dollar value, or per acre of area cultivated), ownership of or investment into farm and non-farm equipment and assets, and investment in inputs such as seeds and fertilizers. In addition to the above-mentioned outcomes, this paper is also interested in assessing the impacts of this program on household indebtedness, sources of credit, and use of procured credit. These outcomes are highly relevant in the Indian context wherein rural debt levels have been historically high[6], with the average amount of outstanding loan being Rs 74,121, making it 61% of the average annual income of an agricultural household. Moreover, income transfer payments have been purported as superior alternatives to debt waiver schemes, where the government cancels the household's debt, as they do not distort incentives to repay debt, punish honest debtors, reduce future supply of credit for households, and impose a huge fiscal burden on the state. This makes it important to evaluate the impact of transfers on debt related outcomes to facilitate a qualitative comparison between the two policy instruments.

Impact evaluations of transfer programs in different settings are often conducted by running randomized controlled trials (RCTs), as they are widely seen as the best way to generate a valid control group (a group similar to the treatment group but not receiving the transfer). Researchers often partner with the government to pilot a cash transfer program or introduce beneficiaries in a randomized phase-in fashion into the programs. However, experimental designs can be difficult to implement due to political, ethical, and logistical reasons. It has also been noted that some of the programs that yield encouraging results in RCTs don't scale-up very well. As the scale of the program increases, bringing a wider audience within its fold, the population being targeted becomes more diverse and heterogenous, targeting becomes more challenging, and leakages increase (Glennerster and Takavarasha, 2013). All this is to say that

[5] A crop season refers to the months during which a crop is cultivated, starting with the sowing of seeds and ending with the harvesting of the crop. There are two primary crop seasons in India: Rabi (winter) and Kharif (summer).

[6] Across all land size classes, over 1 in two agricultural household is in debt (NSO, 2021).

studying the impacts of scaled-up transfer programs is imperative and can inform policy design. The KALIA program, the income transfer program being evaluated in this paper, encompasses the entirety of the state of Odisha, and aims to target about 3 million small and marginal farmers in the state. The task of correctly identifying the beneficiaries and disbursing the transfer amount in time is a monumental one in this case. Taken together with the large financial outlay of the program, these challenges underscore the importance of evaluating the impacts of this program on cultivators in the state.

This paper looks at the impacts of the program about six months after the launch of the program and after the receipt of the first two installments of the income transfer. The decision to focus on short-term impact is driven by the following reasons. Firstly, in about a year from the commencement of the program, the country found itself in the midst of a global pandemic. The pandemic was accompanied by huge income and livelihood losses for man households. It also stalled economic activity in most places. This shock to income, health, and labor supply can impact the way the transfers are used and may not necessarily reflect the impacts of the program under normal "business-as-usual" circumstances. Secondly, the national survey data used in this paper conducted its last data collection round in 2019. In-person data collection activities had to be suspended.

In summary, this paper makes contributions to the existing literature on the effects of transfer programs by evaluating a scaled-up transfer program designed to provide direct income support, targeting agricultural households in India. The rest of this paper is organized as follows. Section 2 provides a brief description of the setting of the program, its salient features, as well as an overview of the dataset used in this paper. Section 3 discusses the empirical strategy used for causal identification. Section 4 presents the main results as well as robustness checks and provides a discussion of potential mechanisms. Section 5 concludes.

II. Setting, Program, and Data Sources

II.I. *Program Location, and Sample Population*

The program being studied was designed by the state government of Odisha, making Odisha the default location of the study. Located in eastern India, Odisha is an agrarian sate with about 62 percent of the population of the state relying on agriculture for livelihood. However, agriculture only contributes 21.3 percent to the state's Gross Domestic Product (GDP), indicating low productivity and low per-capita income in the farm sector (Department of Agriculture, Odisha). Table 1.1 shows a quick comparison between the average levels for Odisha and the national averages for key variables. We can see that Odisha falls below the national average in income level, consumption level, land size and rises above the national average in reliance on agriculture for employment, and indebtedness. Overall, it appears that farmers in the state of Odisha are worse off and in greater financial peril than the average farmer in the country. We would expect an income support program to be of greater relevance and importance in Odisha than in many other states in the country.

At the same time Odisha is a suitable location to study a scaled-up program which is well-implemented and well-targeted. The government of Odisha has been presented with the Krishi Karman award several times for its successful implementation of agricultural policies. The land records in Odisha are digitized and publicly available, thus ensuring transparency. The KALIA program took off in a matter of two weeks and managed to disburse its first installment in January of 2019 (Financial Express, 2019). The government conducted robust verifications to identify the eligible beneficiaries.

Table 1.1. Comparison of Key Descriptive Statistics between Odisha and India

	Odisha	India
Share of agricultural households (%)	58	48
Average land owned by agricultural households (ha.)	0.52	1.00
Average monthly income of agricultural households (Rs.)	7731	8931
Average monthly consumption expenditure per household (Rs.)	5613	6646
Incidence of indebtedness among households (%)	47	35
Share of informal sources in outstanding debt (%)	53	34

Source: NAFIS 2018; NSO 2021; and author's calculations

II.II. *Salient Features of the KALIA Program*

The KALIA program is a multi-faceted intervention designed by the government of Odisha to provide income support to cultivators in the state. The program has multiple components, which include direct income transfers (DIT) of Rs 5000 every crop season up to five crop seasons (Rs 25000, cumulatively), financial assistance to landless agricultural households of Rs 12,500, life insurance for cultivators and agricultural households, among other things. For the purposes of this paper, we are interested in the DIT component of the program which targets small and marginal farmers who own less than 2 hectares of land (Government of Odisha).

The transfer is unconditional, such that the households receiving the transfer are free to dispose of this money in any manner. Having said that, the government has framed this transfer as "assistance for cultivation" and as money than can be used for purchase of various agricultural inputs. Moreover, the transfers are also timed to be disbursed at the start of the crop season. Additionally, the government has also presented this program as an alternative to a loan waiver scheme. Therefore, the income transfer may in some ways be considered a framed transfer, wherein no specific conditions are enforced instead recipients are encouraged to use the funds in a certain way. Benhassine et. al. (2015) found framing to be a simple yet effective way in which the use of transfer funds can be directed in the context of education. Likewise, Ambler et. al. (2019) also found evidence in support of framed transfers being effective in directing funds towards productive uses in agricultural households in Senegal.

II.III. *Data Sources*

This paper makes use of unit-level data collected in the 77th round of National Sample Survey (NSS) to evaluate the impact of income transfers as part of the KALIA program on the outcomes discussed earlier. The NSS is a nationally representative survey conducted by the Government of India. Each round of the NSS covers a specific subject and collects information pertaining to it. The 77th round covers the following two subjects — 'Land and Livestock Holdings of Households and Situation Assessment of Agricultural Households' (Schedule 33.1) and 'Debt and Investment' (Schedule 18.2). The two schedules are administered in different samples. Both the schedules collect information on size of land owned, and other common household characteristics. Schedule 33.1 collects detailed information about crops grown, area cultivated, quantity produced, expenditure incurred on the purchase of various inputs, investments into farm and non-farm assets among other things and is only canvassed in rural India. Schedule 18.2, on the other hand, gathers information about the loans taken by households, their sources, their purpose, interest rates, etc., and is conducted in both rural and urban regions. For this paper, we restrict the sample to rural households.

The survey was conducted in 2019 and collected data in two rounds, with the reference period for the first round being July-December 2018 and the reference period for the second round being January to June 2019. The second round was administered on the households sampled in the first round. Table 1.2 gives an overview of the surveys recorded in each schedule and in each visit.

Table 1.2. Description of Data Sources and Sample

	Schedule 33.1 (Visit 1)	Schedule 33.1 (Visit 2)
Total number of records	58040	56899
Total number of records for Odisha	2275	2246
	Schedule 18.2 (Visit 1)	Schedule 18.2 (Visit 2)
Total number of records (rural)	69455	68291
Total number of records for Odisha (rural)	3001	2995

III. Empirical Strategy

III.1. *Identification*

As discussed earlier, the direct income transfer under the KALIA program is not randomly assigned to households but is a form of targeted intervention aimed at small and marginal famers. This implies that a comparison of means between the eligible and ineligible cohorts will not provide us with estimates of the causal impact of the program on various outcomes. There are significant differences in both observed and unobserved characteristics between the two groups. Including household controls will eliminate the observed heterogeneity but the treatment variable (a binary variable indicating whether or not the household is eligible to receive the income transfer) may continue to be correlated with the error term due to unobserved heterogeneity. This gives rise to be to problem of endogenous regressor, which makes the estimator biased and inconsistent. At the same time, a pre-post comparison of outcomes for the eligible cohort will suffer from the presence of time-varying shocks, making it difficult to isolate the impact of the program from the impact of these transitory shocks.

In order to estimate the causal effect of income transfer, we exploit the fact that eligibility for the KALIA program depended on the amount of land owned by the farm household. As described earlier, only households who owned less than 2 hectares of land were deemed beneficiaries of the income transfer. It must be noted that we don't have information on compliance. Due to targeting issues, some eligible households may have not received the transfer and vice versa. Although the KALIA program has been considered to be a well-targeted program with robust checks and verifications, we should expect some, even if fewer than other public programs, instances of mis-targeting. Therefore, we are actually looking at the impact of "eligibility" for direct income transfer under the KALIA program on farm outcomes. This can be considered analogous to ITT (intention-to-treat) estimates commonly reported in randomized controlled trials (RCTs).

We have a sharp discontinuity in the probability of being deemed eligible and by construction, a sharp discontinuity in the probability of getting treated which gives rise to a sharp discontinuity in outcomes driven by the treatment. In other words, this generates quasi-random variation in treatment status. The assignment rule of the treatment is formally defined as follows, where I_i is a dummy variable that indicates whether the household is eligible to receive the income transfer under the KALIA scheme and L_i indicates the size of the land owned by the household, in hectares.

$$I_i = \begin{cases} 1 & if\ L_i < L_c = 2 \\ 0, & otherwise \end{cases} \tag{1}$$

This feature of the program is leveraged to use a regression discontinuity (RD) design (see Hahn, Todd, and Van der Klaauw 2001; Imbens and Lemieux 2008; Lee and Lemieux 2010) to achieve causal identification of the treatment effect. Under this setup, identification rests on the assumption that since treatment status is determined by a cut-off score, L_c, along the running (forcing) variable L_i, it is quasi-randomly assigned. This makes farm households just below, and hence "just eligible" for the program comparable to households just above, and hence "just ineligible" to receive the income transfer. For subsequent analysis, the running variable is redefined by rescaling land size to center eligibility cut-off at 0. The rescaled running variable, L_i^*, is defined as "hectares from cut-off". Under a set of assumptions discussed in the subsequent section, the local average treatment effect (LATE) of receiving income transfers, τ_{RD}, can be estimated as the difference between the regression functions at the discontinuity L_c,

$$\tau_{RD} = \lim_{L \to L_c^-} E[\,Y_i | L_i = L_c] - \lim_{L \to L_c^+} E[\,Y_i | L_i = L_c] \qquad (2)$$

The above equation can be intuitively understood as the difference in the observed ex-post outcomes between the units in the immediate neighborhood on either side of the cutoff. This difference can be attributed to the income transfer program if these households do not differ in their observed pre-program characteristics and cannot affect their treatment status once the program is announced. The limits on the right-hand side of equation (2) are estimated by regressing the outcome of interest on a polynomial of the running variable, using data within the vicinity of the cutoff on the left as well as on the right to obtain the limit of the outcome variable as the running variable as it approaches the cutoff from the left as well as from the right, respectively. The difference between the two limits, which is the regression discontinuity estimate, is estimated from the following OLS regression

$$Y_i = \alpha_0 + \beta_1 I_i + \beta_2 f(L_i^*) + \varepsilon_i \qquad (3)$$

where Y_i is the outcome of interest; I_i is the binary treatment indicator which equals 1 if the household is below the cut-off and 0 otherwise; $f(L_i^*)$ is a polynomial of the running variable along with the corresponding interaction terms with I_i; and ε_i is the stochastic error term. The coefficient β_1 is the estimate of the treatment effect. Additional covariates can be adjusted to equation 3.

III.II. *Validity of RD design and Diagnostics*

This section discusses the identification assumptions associated with the RD design and presents results of the related identification tests.

a). Imprecise control over the assignment variable

A key assumption for an RD design to be valid is for individuals to have imprecise control over the assignment (running) variable. In this case, if households could precisely manipulate their land size, and by extension their treatment status, we would no longer be able to consider access to the program as being quasi-random. This manipulative sorting has implications for causal identification as it could render the treatment (units on left of cut-off) and control (units on the right of cut-off) groups non-comparable, thus generating a self-selection bias in the RD estimator (Lee and Lemieux 2010; McCrary 2009).

To test for manipulation, Figure (3) plots the density of the running variable. We also perform the McCrary test to check for discontinuity in the density of the running variable around the cut-off (see Appendix A3). The figure displays some bunching on the right of the cut-off. This may give rise to the concern that households were able to manipulate the running variable. However, certain features of the program as well as the nature and direction of bunching could help alleviate this concern. Firstly, if households were strategically sorting in response to the KALIA program, they would do so to gain access to the treatment, which would be reflected as bunching on the left of the cut-off (which comprises of the eligible group) as opposed to the right of the cut-off, which we see in the sample.

Secondly, the variable determining access to the program is pre-program land size, which is collected in the pre-treatment round of the survey. It is unlikely that households were able to pre-empt the timing and eligibility criterion of the program ahead of the formal launch of the program. Moreover, the land records in Odisha are digitised and hence, expected to be more transparent. Households eligibility to the program was verified using multiple databases. The beneficiary lists were publicly posted on the KALIA portal. This system of checks and balances is likely to ensure that households would be unable to manipulate land records.

It is also unlikely that households would have strategically sold off parts of their landholding to meet the legibility criterion. The time elapsed between the announcement of the program and the publication of the draft beneficiary list was a mere 15 days, making it virtually impossible to find buyers for their land, and successfully update their official land records, thus plausibly ruling out any ex-post manipulation of land records or land size. Furthermore, we look for changes in the distribution of size of land owned over time (Figure 4) and don’t find any major differences in the overall distribution of land, which could have indicated ex-post manipulation.

Figure 3. Density of Running Variable

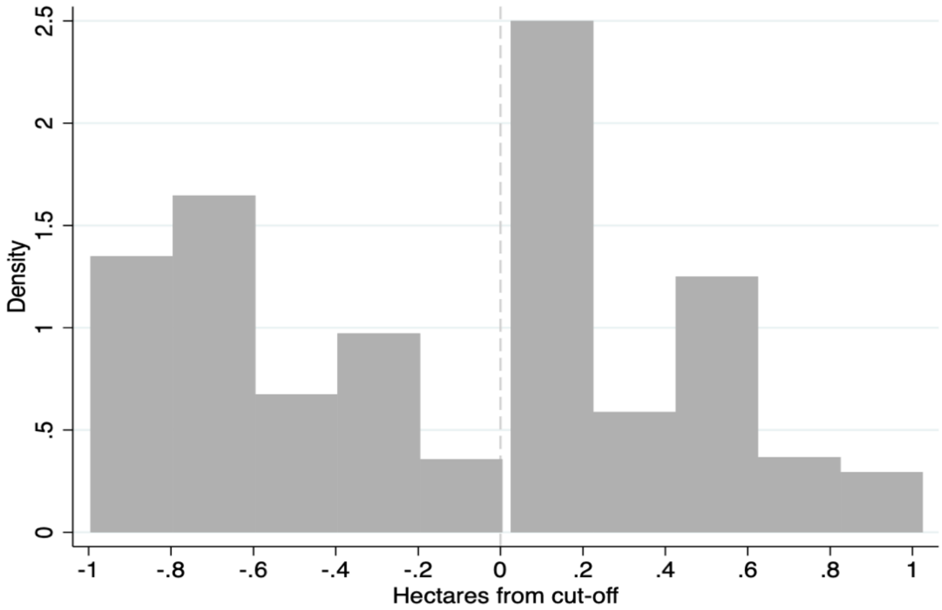
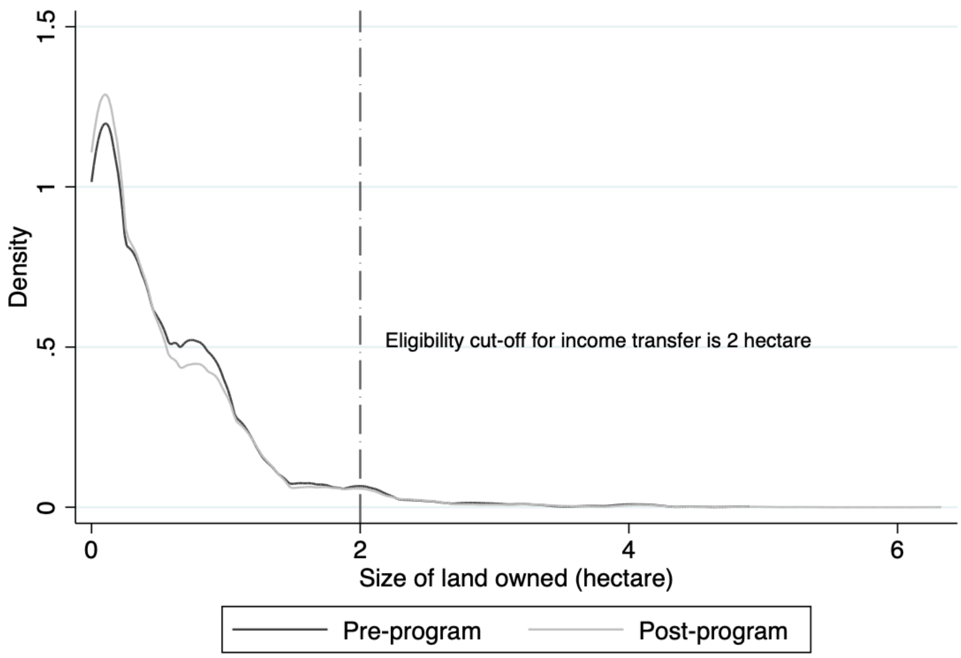


Figure 4. Distribution of Land Owned by Households Pre and Post Program



b). Continuity of other covariates at the threshold

It is important to check for any discontinuity in pre-existing characteristics for which data is available, and which are not known to have been affected by the treatment. Any pre-existing discontinuities are concerning as they suggest non-comparability of the treated and the not treated around the eligibility cut-off. Any discontinuity in outcomes due to discontinuities in covariates can be incorrectly interpreted as treatment effect. We check for continuity in covariates by using the local linear regression methods, described earlier, and don't find any evidence of pre-existing discontinuities.

Another cause for concern would be the use of land size and the 2 hectare cut-off as the qualifying threshold for other past or contemporary programs. This could result in a pre-treatment discontinuity in the outcomes if these policies also affected the outcomes targeted by this program. Additionally, these policies could also have led to pre-treatment sorting. However, that does not appear to the case. A past policy that employed landholding size as a determinant of program benefits was the Agricultural Debt Waiver and Debt Relief Scheme (ADWDRS), launched back in 2008. India has not seen a federal loan waiver policy in the past 5 years. The state government of Odisha has not announced any loan waiver schemes in the recent past. Additionally, we also don't find any evidence of discontinuities in pre-program covariates, which would have indicated ex-ante selection (Table 2).

Table 2. Covariate Continuity

	Ineligible mean	Eligible mean	Discontinuity at cut-off	Std error
Panel A: Schedule 33.1				
Household is Hindu	0.95	0.96	-0.08463	0.09216
Owns house	0.98	0.98	-0.0002	0.00766
Resides in pucca house	0.59	0.48	0.02616	0.15384
Belongs to a backward social group	0.74	0.81	0.20711*	0.12022
Usual monthly consumption expenditure	8497.76	5727.22	-1339.3	1154.4
Possesses Kisan Credit Card	0.33	0.18	-0.09937	0.11832
Panel B: Schedule 18.2				
Household is Hindu	0.96	0.97	-0.29993	0.1997
Belongs to a backward social group	0.82	0.81	-0.00302	0.11685
Practiced agriculture in last 365 days	0.91	0.76	0.01285	0.01477
Family size	4.07	3.91	0.71704	0.69647
Usual monthly consumption expenditure	6769.67	5869.68	1465.7*	852.66

Notes: Standard errors are robust and clustered at first-stage sampling unit. Optimal bandwidths for column 3 have been calculated using Calonico, Cattaneo, and Titiunik (2014a, b). Significance stars are as follows: * p<0.10 ** p<0.05 *** p<0.01

c). Falsification or placebo tests

A final test for validity of the RD design involves testing for discontinuities in the outcome variables at points other than the eligibility cut-off. If the eligibility criteria for the program is the only source of jumps in the outcome variable, there should not be any jump at a point other than the cut-off. Details of the test are reported in Appendix A3.

Finally, in order to understand and diagnose a possible cause for apparent sorting on the right side of the cut-off, we can refer to the survey's sampling methodology. The 77th NSS round uses a two-stage stratified sampling. Within each stratum (a district in the country), second stage strata are created as follows:

Composition	SSS No.	no. of sample households allocated
non-agricultural households	1	2
agricultural households with land possessed less than 0.250 hectare (0.618 acre)	2	2
agricultural households with land possessed equal to or more than 0.250 hectare but less than 1.000 hectare (2.471 acre)	3	2
agricultural households with land possessed equal to or more than 1.000 hectare but less than 2.000 hectares (4.942 acre)	4	2
agricultural households with land possessed equal to or more than 2.000 hectares	5	2
Total		**10**

Source: Note on Sample Design and Estimation Procedure of NSS 77th Round, NSO 2021.

Households just below the eligibility cut-off of 2 hectares and above the cut-off are in different strata. This could explain the difference in density of the bin 0.2 hectares left of the cut-off and 0.2 hectares right of the cut-off if more responses were collected from the bin on the immediate right than on the immediate left. This difference in response rate could be accidental and not correlated with any observed or unobserved characteristics as the incentive to take the survey appears to be the same for each stratum. Such a difference in density of the running variable at the cut-off was also found in Kanz (2016), which used survey data to estimate the effect of the ADWDRS using an RD design. Kanz (2016) also conducted extensive checks to test for manipulation, which seemed to be indicated by the density test but were unable to find any plausible mechanism which could suggest any precise ex-ante or ex-post manipulation of land records.

III.III. *Robustness checks*

As discussed in the previous sub-section, it appears to be unlikely that households were able to manipulate the running variable and strategically sort on either side of the cut-off in response to this program. However, despite not finding any discontinuities around the cut-off in the observed pre-treatment covariates, households close to the cut-off on the left might be different from households close to the cut-off on the right if selection occurred on unobserved characteristics. In order to circumvent this issue, we can exploit the panel nature of the collected data, which gives us a pre-treatment as well as a post-treatment value for each outcome. In this section, an alternative to standard RD estimation is discussed. RD designs have often been updated to account for compound treatments changing at the same threshold, or simultaneous changes in multiple laws and institutions on either side of the border in the case of geographical discontinuities (see Keele and Tituinik 2015; Grembi 2016; Eggers 2017).The existence of compound treatments can create a pre-existing discontinuity in the outcomes and can also result in pre-existing sorting that may have occurred in response to the confounding policy. In such cases, the pre-treatment data to compute the difference between the pre-treatment and post-treatment discontinuity around the cut-off to partial out the effect of the pre-existing confounding policy. The method, referred to as the "difference-in-discontinuities", is implemented by estimating the boundary points of regressions on either side of the cut-off, both in the pre-treatment and post-treatment period. This logic can also be extended to sorting, especially if such sorting can be reasonably assumed to be time-invariant between the pre-treatment and post-treatment period. If there are pre-existing unobserved differences between households on the left and the right of the cut-off, and such difference don't evolve with time, taking the difference between the pre and post outcomes of households around the cut-off could help to eliminate unobserved heterogeneity arising due to time invariant sorting.

In the context of this study, the two rounds of data collection are only a few months apart and households sampled in Visit 1 are also visited in Visit 2. It can be argued that if the discontinuity in the density of the running variable is a consequence of differential response rates of households in the immediate neighbourhood on either side of the cut-off, and if such differences are correlated with any unobserved characteristics, taking a first difference of the outcome variable will eliminate the household's unobserved heterogeneity. It is unlikely that these unobserved differences are time-varying, given that the pre-treatment and post-treatment outcomes are collected in a relatively short time frame, thus not allowing much time for things to change greatly. Moreover, we can also rule out any additional sorting between the two period due to the concerned policy or any past policy, for reasons discussed in the prior sub-section.

This paper presents both cross-sectional RD estimates as well as RD estimates with first differenced outcome variables. As a further robustness check, we also check for the sensitivity of cross-sectional estimates to bandwidth choice as well as the order of the polynomial (see Appendix A2).

IV. Results

This section presents the key empirical results. As discussed earlier, the direct income transfer component of the KALIA program seeks to provide monetary assistance to cultivators ahead of the crop season to enable them to purchase inputs, assets, as well as to provide livelihood support. We look at the impact of transfers under the KALIA program on the household's level of indebtedness, on the composition of debt, and on key agricultural outcomes like investment into agricultural inputs, area cultivated, and crop yield. Additional results on supporting outcomes are discussed as well. The estimating equation in all tables comes from the regression discontinuity specification described earlier, standard and covariate-adjusted. The optimal bandwidth is calculated using the method developed by Calonico, Cattaneo, and Titiunik (2014a). The point estimator is constructed for orders 1 (local linear regression) and 2 (local quadratic regression) of the local polynomial. Results for higher order polynomials are shown in the Appendix. Moreover, the RD estimates for first-differenced outcome are discussed here and shown in the Appendix A2.

A. Impact on the Level of Indebtedness

Tables 3-5 provide point estimates of the impact of income transfers on the level of household debt. Household indebtedness is captured using the following three variables— whether the household participates in the credit market (informal and formal) following the treatment; total amount borrowed by the household post-treatment, that is, in the first two quarters of 2019 (Table 3); amount outstanding as on the last day of the second quarter of 2019 (Table 5); and amount borrowed within six months after the launch of the program (Table 4). The former outcome reflects the borrowing needs of farm households after receiving income support and the latter outcome is indicative of the overall level of debt of the household.

The income transfer program aims to reduce household indebtedness by a) providing financial assistance to cultivators during the crop season, thereby reducing borrowing undertaken to finance the purchase of various inputs as well as for everyday household consumption in the pre-harvest phase of the crop cycle; b) facilitating repayment of past loans (including interest) with the transfer received under the program. If the program has been effective in achieving its objectives, we would ideally expect to see lower levels of borrowings in 2019 as well as lower levels of total outstanding loans in the treatment group, relative to the control group. Although unlikely, we may also expect an increase in borrowings if the increase in income and liquidity due to the program spurs investments in lumpy goods/durables and if households decide to finance these investments, in part by increasing borrowing. Additionally, there is also the possibility that the income transfer does not induce any changes in the levels of borrowing or in the pattern of repayment of outstanding loans.

The point estimates in Table 3 show a decline in the percentage of households participating in the credit market in the post-treatment phase for the first order polynomial but an increase for higher-order polynomials. However, all estimates are insignificant, thus indicating the lack of a clear effect or no effect. Similarly, the point estimates for amount borrowed post-treatment and the amount of outstanding loans are insignificant. Additional household controls were added to reduce omitted variable bias (if any) and increase precision. Columns 2 and 4 of all three tables show covariate-adjusted point estimates. The estimates remain insignificant.

Table 3. Effect of Income Transfer on Credit Market Participation, Cross-Sectional RD Estimates

	Post treatment borrowing=1 (%)			
	(1)	(2)	(3)	(4)
Treatment=1	-9.758	-6.997	13.19	25.64
	(17.02)	(18.04)	(28.49)	(30.92)
Household Controls	No	Yes	No	Yes
Bandwidth	0.65	0.56	0.66	0.55
Order polyn	1.00	1.00	2.00	2.00
Control mean	39	42	40	42
Observations	2538	2529	2538	2529

Notes: In this table, each column reports results from a separate regression with the dependent variable in the column header. The main explanatory variable is a binary variable indicating whether a household is eligible to receive income transfers under the KALIA program. The estimating equation is given by equation (3) and is estimated for orders 1 and 2 of the local polynomial. Columns 2 and 4 add household controls to equation (3) and report covariate-adjusted estimates. Household covariates include pre-program characteristics such as religion, social group (caste), type of house, family size, and whether the household practiced agriculture in the past 365 days. The total number of observations are included in the last row of the table. The effective number of observations vary with bandwidth and range between 120-150. Optimal bandwidths have been calculated using Calonico, Cattaneo, and Titiunik (2014a, b). Standards errors (in parentheses) are robust and clustered at the first-stage sampling unit.

Table 4. Effect of Income Transfer on Borrowing Needs, Cross-Sectional RD Estimates

	Amount borrowed post-treatment (Rs.)			
	(1)	(2)	(3)	(4)
Treatment=1	-2178.4	-5134.2	861.7	8253.7
	(11836.6)	(14328.9)	(22886.1)	(22667.1)
Household Controls	No	Yes	No	Yes
Bandwidth	1.43	0.77	0.91	0.82
Order polyn	1.00	1.00	2.00	2.00
Control mean	17994	18601	19213	18602
Observations	2538	2529	2538	2529

Notes: In this table, each column reports results from a separate regression with the dependent variable in the column header. The main explanatory variable is a binary variable indicating whether a household is eligible to receive income transfers under the KALIA program. The estimating equation is given by equation (3) and is estimated for orders 1 and 2 of the local polynomial. Columns 2 and 4 add household controls to equation (3) and report covariate-adjusted estimates. Household covariates include pre-program characteristics such as religion, social group (caste), type of house, family size, and whether the household practiced agriculture in the past 365 days. The total number of observations are included in the last row of the table. The effective number of observations vary with bandwidth and range between 250-300. Optimal bandwidths have been calculated using Calonico, Cattaneo, and Titiunik (2014a, b). Standards errors (in parentheses) are robust and clustered at the first-stage sampling unit.

Table 5. Effect of Income Transfer on Household Indebtedness, Cross-Sectional RD Estimates

	Amount of outstanding loans (Rs.)			
	(1)	(2)	(3)	(4)
Treatment=1	-51862.2	-51964.9	-66199.4	-66216.4
	(73373.3)	(78954.4)	(74898.2)	(81139.0)
Household Controls	No	Yes	No	Yes
Bandwidth	0.48	0.44	0.99	0.91
Order polyn	1.00	1.00	2.00	2.00
Control Mean.	77140	78138	69409	69409
Observations	2538	2529	2538	2529

Notes: In this table, each column reports results from a separate regression with the dependent variable in the column header. The main explanatory variable is a binary variable indicating whether a household is eligible to receive income transfers under the KALIA program. The estimating equation is given by equation (3) and is estimated for orders 1 and 2 of the local polynomial. Columns 2 and 4 add household controls to equation (3) and report covariate-adjusted estimates. Household covariates include pre-program characteristics such as religion, social group (caste), type of house, family size, and whether the household practiced agriculture in the past 365 days. The total number of observations are included in the last row of the table. The effective number of observations vary with bandwidth and range between 150-300. Optimal bandwidths have been calculated using Calonico, Cattaneo, and Titiunik (2014a, b). Standards errors are (in parentheses) robust and clustered at the first-stage sampling unit. The amount of outstanding loans are reported as on 30.06.3019.

B. Impact on the Composition of Debt

Table 6(a) and 6(b) reports estimates of the effect of income transfers on the composition of the debt portfolio of the household, by examining changes in the share of informal (non-institutional) versus formal (institutional) sources of credit in loans obtained by households, post-treatment. Although the KALIA program does not directly seek to usher any changes in the preferred source of credit for rural households, it does aim to reduce financial distress for farmers. As discussed earlier, farmers often turn to informal sources of credit during times of financial distress and cash crunch to finance their consumption needs. This is driven by informal credit being considerably more accessible to rural households, especially small and marginal farmers, relative to credit from institutional sources such as banks[7]. However, it has been noted that informal credit often comes at a high rate of interest, thus pushing many households down a debt spiral. Heavy reliance on informal credit has been linked to an increased prevalence of farmer suicides, thus indicating higher levels of distress (Merriot, 2016). Studying the impact of income transfer on relative reliance on informal sources of credit by rural households can give us an insight into the effectiveness of the program in providing a cushion against income shocks as well as in providing basic livelihood assistance.

The reported estimates reveal that there was a decline in the share of informal sources in credit obtained by households, following the launch of the KALIA program. For the first order polynomial with controls, we find a 65 percentage-point reduction in the share of informal credit in the debt portfolio of treatment households, relative to control households around the eligibility cut-off. The significant decline is also estimated for higher order polynomials.

We also test for changes in the amount borrowed from informal sources as well as from institutional sources. This sample includes both, households who participated in the credit market in 2019 and those who did not. The amount borrowed by the latter is taken to be zero. We find a decline in the amount borrowed from informal lender, although the estimate is not significant. We find a positive, significant effect on the amount borrowed from institutional sources after adjusting for covariates (Table 6b, col. 6).

[7] Informal credit does not require extensive paper work, proof of income, collaterals, etc., either due to the pre-existing interpersonal relationship between lenders and borrowers (in the case of borrowing from friends, relatives, and landlords) or due to the informality of the loan arrangement where the increased risk for the lender gets translated into very high interest rates (in the case of loan sharks, rural moneylenders).

Taken together with results discussed in the previous sub-section, these results indicate, that while income transfers may not have resulted in any changes in the overall need for loans, it has led to a decline in the need for high-interest loans, desperately obtained by households to weather periods of financial distress.

Table 6 (a). Effect of Income Transfer on Credit Sources, Cross-Sectional RD Estimates

	Share of informal sources in credit (%)			
	(1)	(2)	(3)	(4)
Treatment=1	-14.32	-64.99***	-85.37***	-73.00**
	(22.93)	(19.35)	(32.99)	(36.27)
Household Controls	No	Yes	No	Yes
Bandwidth	0.84	0.54	0.53	0.47
Order polyn	1.00	1.00	2.00	2.00
Control mean	51.2	53.4	53.4	51.3
Observations	783	783	783	783

Notes: In this table, each column reports results from a separate regression with the dependent variable in the column header. The main explanatory variable is a binary variable indicating whether a household is eligible to receive income transfers under the KALIA program. The estimating equation is given by equation (3) and is estimated for orders 1 and 2 of the local polynomial. Columns 2 and 4 add household controls to equation (3) and report covariate-adjusted estimates. Household covariates include pre-program characteristics such as religion, social group (caste), type of house, family size, and whether the household practiced agriculture in the past 365 days. The total number of observations are included in the last row of the table. The effective number of observations vary with bandwidth and range between 50-100. Optimal bandwidths have been calculated using Calonico, Cattaneo, and Titiunik (2014a, b). Standards errors are (in parentheses) robust and clustered at the first-stage sampling unit. The share of informal sources in credit can only be calculated for households who have non-zero borrowing post-treatment.

Table 6(b). Effect of Income Transfer on Credit Sources, Cross-Sectional RD Estimates

	Amount borrowed post-treatment (Rs.)					
	Informal lenders				Institutional sources	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment=1	-6389.1	-9511.0	-18404.4	-18716.3	20071.0	32483.4*
	(12576.0)	(11794.6)	(14355.4)	(14308.7)	(16743.0)	(18289.9)
Household Controls	No	Yes	No	Yes	No	Yes
Bandwidth	0.90	0.79	1.07	1.07	0.46	0.39
Order polyn.	1.00	1.00	2.00	2.00	1.00	1.00
Control mean	9625	9508	9659	9659	10222	11905
Observations	2538	2529	2538	2529	2538	2529

Notes: In this table, each column reports results from a separate regression with the dependent variable in the column header. The main explanatory variable is a binary variable indicating whether a household is eligible to receive income transfers under the KALIA program. The estimating equation is given by equation (3) and is estimated for orders 1 and 2 of the local polynomial. Columns 2 and 4 add household controls to equation (3) and report covariate-adjusted estimates. Household covariates include pre-program characteristics such as religion, social group (caste), type of house, family size, and whether the household practiced agriculture in the past 365 days. The total number of observations are included in the last row of the table. The effective number of observations vary with bandwidth and range between 300-400. Optimal bandwidths have been calculated using Calonico, Cattaneo, and Titiunik (2014a, b). Standards errors are (in parentheses) robust and clustered at the first-stage sampling unit.

C. Impact on agricultural investment and productivity

Investment into agricultural inputs can be broadly classified into two categories— investment into purchase of variable inputs such as seeds, fertilizers, pesticides, electricity, and fuel (diesel/petrol); and investment into productive assets and equipment such as tillers, harvesters, threshers, sickle, water pump, etc. The former are recurring expenses that a farm household needs to incur every crop season if it decides to cultivate land in that season. The adequacy and quality of these inputs have a bearing on total quantity produced, as well as on the yield (defined as, quantity per acre of cultivated land). Investment into acquisition of productive assets and farm equipment can enhance yield for current and future crop cycles by enhancing the efficiency with which farming is conducted. They can also create an additional income stream for the household by allowing the household to rent these assets to other cultivators.

The transfers under the KALIA program are designed to be delivered during the crop season, thereby relaxing liquidity constraints, and allowing households to invest in the purchase of necessary inputs. Additionally, we might expect the transfers to stimulate investment into costlier, productivity enhancing assets through two channels—by reducing the amount of accumulated debt which creates disincentives for investment, referred to as "debt-overhang"; by reducing livelihood uncertainty which often keeps households from investing into high-value assets and encourages precautionary savings, instead.

As shown in Table 7, the point estimate for investment per acre, which is defined as the expenditure incurred on the purchase of seeds, fertilizers, pesticides, and manure is negative and insignificant. Before moving to the evaluation of the transfer payment on productivity, we study its impact on the area cultivated by the agricultural households, post treatment. We find a decline of about 1.3-1.6 acres in the area cultivated by households (Table 8). The decline persists even after taking the first difference of the area cultivated to account for any pre-existing differences in the area cultivated between the treatment and control group.

We don't find conclusive evidence of any impact on crop yield (Table 9). The point estimates are positive but remain insignificant. However, upon differencing the pre-treatment crop yield from the post-treatment crop yield, the evidence in support of a positive impact of the program on change in crop yield becomes significant.

It is important to note that the above effects can only be estimated for agricultural households who choose to cultivate a non-zero value of land. We don't find any significant changes in household's decisions to practice agriculture on the extensive margin on either side of the cut-off.

Table 7. Effect of Income Transfer on Investment into Farm Inputs, Cross-Sectional RD Estimates

	Investment per acre (Rs)			
	(1)	(2)	(3)	(4)
Treatment=1	-2328.5	-1820.7	-2251.6	-1415.7
	(1463.2)	(1609.4)	(2093.9)	(2292.6)
Household Controls	No	Yes	No	Yes
Bandwidth	1.07	0.77	0.97	0.86
Order polyn	1.00	1.00	2.00	2.00
Control mean	5474	5295	5614	5437
Observations	809	809	809	809

Notes: In this table, each column reports results from a separate regression with the dependent variable in the column header. The main explanatory variable is a binary variable indicating whether a household is eligible to receive income transfers under the KALIA program. The estimating equation is given by equation (3) and is estimated for orders 1 and 2 of the local polynomial. Columns 2 and 4 add household controls to equation (3) and report covariate-adjusted estimates. Household covariates include pre-program characteristics such as religion, social group (caste), type of house, monthly consumption expenditure whether the household possesses a Kisan Credit Card, and whether the household practiced agriculture in the past 365 days. The total number of observations are included in the last row of the table. The effective number of observations vary with bandwidth and range between 150-250. Optimal bandwidths have been calculated using Calonico, Cattaneo, and Titiunik (2014a, b). Standards errors are (in parentheses) robust and clustered at the first-stage sampling unit.

Table 8. Effect of Income Transfer on Practice of Agriculture (intensive margin), Cross-Sectional RD Estimates

	Area cultivated (acre)			
	(1)	(2)	(3)	(4)
Treatment=1	-0.545	-0.554	-1.600**	-1.395**
	(0.632)	(0.447)	(0.762)	(0.633)
Household Controls	No	Yes	No	Yes
Bandwidth	0.69	0.81	0.69	0.70
Order polyn.	1.00	1.00	2.00	2.00
Control mean	2.3	2.3	2.3	2.2
Observations	818	818	818	818

Notes: In this table, each column reports results from a separate regression with the dependent variable in the column header. The main explanatory variable is a binary variable indicating whether a household is eligible to receive income transfers under the KALIA program. The estimating equation is given by equation (3) and is estimated for orders 1 and 2 of the local polynomial. Columns 2 and 4 add household controls to equation (3) and report covariate-adjusted estimates. Household covariates include pre-program characteristics such as religion, social group (caste), type of house, monthly consumption expenditure whether the household possesses a Kisan Credit Card, and whether the household practiced agriculture in the past 365 days. The total number of observations are included in the last row of the table. The effective number of observations vary with bandwidth and range between 120-180. Optimal bandwidths have been calculated using Calonico, Cattaneo, and Titiunik (2014a, b). Standards errors are (in parentheses) robust and clustered at the first-stage sampling unit.

Table 9. Effect of Income Transfer on Productivity, Cross-Sectional RD Estimates

	Crop Yield (kg per acre)			
	(1)	(2)	(3)	(4)
Treatment=1	-3.905	200.3	946.0	615.7
	(454.7)	(518.5)	(646.5)	(945.3)
Household Controls	No	Yes	No	Yes
Bandwidth	0.56	0.46	0.64	0.47
Order polyn.	1.00	1.00	2.00	2.00
Control mean	1227	1321	1221	1296

Observations	810	810	810	810

Notes: In this table, each column reports results from a separate regression with the dependent variable in the column header. The main explanatory variable is a binary variable indicating whether a household is eligible to receive income transfers under the KALIA program. The estimating equation is given by equation (3) and is estimated for orders 1 and 2 of the local polynomial. Columns 2 and 4 add household controls to equation (3) and report covariate-adjusted estimates. Household covariates include pre-program characteristics such as religion, social group (caste), type of house, monthly consumption expenditure whether the household possesses a Kisan Credit Card, and whether the household practiced agriculture in the past 365 days. The total number of observations are included in the last row of the table. The effective number of observations vary with bandwidth and range between 100-120. Optimal bandwidths have been calculated using Calonico, Cattaneo, and Titiunik (2014a, b). Standards errors are (in parentheses) robust and clustered at the first-stage sampling unit.

D. Discussion of Results and Mechanisms

The short-term impacts of the income support cash transfers offered to small, and marginal agricultural households are insignificant on overall indebtedness. This result can be contrasted against a clear decline in the level of household debt noted in the case of debt waiver (Kanz, 2016). This decline naturally follows from the design of loan waivers, which provide a complete or partial write-off of the household's debt. Cash transfers, on the other hand, do not explicitly dictate a decline in the level of accumulated debt, instead leaving the decision to repay existing debt on the household. We also don't find any changes in the likelihood that a household procures any new loans, or in the amount of loan obtained, post-treatment. This result echoes the finding in Kanz 2016, wherein the author does not find any significant impact of the debt relief scheme on new loan applications. The lack of an effect of the transfer on outstanding loans can occur if the quantum of the transfer was not large enough to bring about any detectable changes in accumulated debt. The transfer under the KALIA program is Rs. 10,000 for two crop seasons, which is about 11% of the average amount of outstanding debt of an agricultural household in our sample. Given that no significant changes are observed in post-treatment changes in overall debt levels can plausibly only occur through repayment of past loans. At most, we can expect an average reduction of 11%, which may be below the minimum detectable effect size, give the variation in the sample and the size of the sample.

Despite not finding any significant changes in current levels of accumulated debt, we do find evidence of a fall in the share of informal sources in credit procured, post-treatment. This result has implications for future levels of debt and financial distress for the household as households obtaining high-interest informal loans are much likelier to end up in debt traps. As discussed earlier, the persistence of informal credit in the debt portfolio of rural households has been a concern for policymakers for a long time (RBI, 2013). Despite growing access to institutional credit and supply of microfinance, the demand for informal credit has persisted. This can be attributed to the easy availability of informal credit for personal consumption use. Cash transfers smooth income and can therefore, eliminate the need for informal credit. This result is further supported by a decline in the share of loan obtained for personal, non-productive use, relative to the control group (See Appendix A1), which also gets reflected in the increase in the amount of institutional credit[8].

The lack of an effect on investment in agricultural inputs could be explained through the following mechanisms. Firstly, the transfer did not bring about any substantial reductions in the levels of accumulated debt, which could have stimulated investment as predicted by the theory of “debt-overhang”. Secondly, despite providing the liquidity needed for purchase of inputs, the sum transferred is not large enough to relax the binding constraint of fixed land. The transfer is not large enough to finance the purchase or leasing of additional land. As the size of available land does not increase, treatment households may opt out of investing in the procurement of additional inputs.

The decline in area cultivated by the households can be induced by any one of the following mechanisms or a mix of them. The first possibility is that the transfer payment allowed households to diversify their sources of livelihood by transitioning away from farm activities and towards non-farm activities. This can happen if households were able to invest in the acquisition of non-farm productive assets using the transfer. Despite finding conclusive evidence of reduced investment into productive, farm assets and equipment, we don’t find any evidence of a corresponding increase in ownership of or investment into non-farm assets. (see Appendix A1). We can conjecture that households may be saving to finance the purchase of non-farm assets, which usually require a large up-front investment. Unfortunately, we don’t have any direct evidence of this as the dataset used does not capture household savings. The rise in the amount of institutional loans obtained post-treatment, coupled with a fall in the share of credit obtained to satisfy personal consumption needs can lend some credence to this hypothesis.

[8] Institutional credit is credit procured from scheduled commercial banks, cooperative banks, NBFCs including micro-finance institutions, regional rural banks, provident funds, and other institutional agencies. Institutional lenders provide credit for productive uses and investments into productive assets.

The second possibility is that households move away from crop diversification and towards crop concentration after receiving the transfer. The reduction in uncertainty regarding income and livelihood due to the assured cash transfer can potentially result in farmers choosing to cultivate fewer crops than before as it eliminates the need to cultivate multiple crops to cope with risk through diversification. We, however, don't find any changes in the number of crops produced in the treatment group, relative to control group.

Lastly, there is the possibility that households reduce their labor supply to farm activities and substitute it for leisure. This can occur through two channels. Cash transfers have been found to reduce agricultural wage labor across different settings, as revealed in the meta-analysis of the impacts of cash transfers (Daidone, 2019). Agricultural labor is often considered as a "refuge" sector or activity of the last resort where poor households find work during times of financial distress. This is driven by the low returns to effort in the farm sector in less developed countries. A reduction in labor supplied to this sector is suggestive of improving economic conditions. Since the KALIA program also offers livelihood assistance to landless cultivators and tenant farmers, it is plausible that a reduced supply of agricultural wage labor from such households leads to a decline in area cultivated by households close to the cut-off as well. On the other hand, the reduction in cultivated area could be driven by the decision of landowning households to reduce the labor supplied to farm activities, especially on parts of the land that are not as productive and profitable. This decision can be also attributed to improving economic conditions. This could also explain the marginal increase in crop yield observed in the first difference RD estimate (see Appendix A2). Data on time use of households could help confirm one of these channels.

V. Conclusion

The results presented in this paper and the potential mechanisms discussed indicate that there is some positive evidence in favor of the transfer program and its role in relieving short-term liquidity constraints and reducing uncertainty pertaining to income and livelihood, as is evidenced by the declining share of informal credit and credit for personal consumption. However, the size of the transfer is not large enough to induce major productive investments, at least in the short run. Long run impacts of the program will reveal themselves in the due course of time and remain a topic for future research. In the context of Indian agriculture where plots are small and fragmented, posing a big constraint to any productivity enhancement, it may be only fair to expect these transfer payments to only act as cushions that households use to weather the storm, even in the long run.

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Appendix A1: Impact of income transfers on additional outcomes

A. Impact on use of credit

We look at the impact of income transfers on the purpose for which credit is procured. Broadly, credit can be procured to finance capital expenditure incurred for the purchase of productive assets, or to finance the personal consumption expenditure. The latter indicates a shortfall in income to finance basic consumption needs. Moreover, it is easier to obtain personal loans from informal "loan shark" type of lenders rather than scheduled commercial banks, rural banks, and other institutional sources.

Table A1.1. Effect of Income Transfer on Use of Credit, Cross-Sectional RD Estimates

	Share of credit procured for personal use (%)			
	(1)	(2)	(3)	(4)
Treatment=1	-52.68**	-62.88***	-83.49***	-76.74***
	(22.54)	(16.98)	(23.97)	(22.81)
Household Controls	No	Yes	No	Yes
Bandwidth	0.51	0.31	0.56	0.56
Order polyn	1.00	1.00	2.00	2.00
Mean of dep. var.	34	34	36	37
Observations	783	783	783	783

Notes: In this table, each column reports results from a separate regression with the dependent variable in the column header. The main explanatory variable is a binary variable indicating whether a household is eligible to receive income transfers under the KALIA program. The estimating equation is given by equation (3) and is estimated for orders 1 and 2 of the local polynomial. Columns 2 and 4 add household controls to equation (3) and report covariate-adjusted estimates. Household covariates include pre-program characteristics such as religion, social group (caste), type of house, monthly consumption expenditure whether the household possesses a Kisan Credit Card, and whether the household practiced agriculture in the past 365 days. The total number of observations are included in the last row of the table. The effective number of observations vary with bandwidth and range between 120-180. Optimal bandwidths have been calculated using Calonico, Cattaneo, and Titiunik (2014a, b). Standards errors are (in parentheses) robust and clustered at the first-stage sampling unit.

B. Impact on practice of agriculture (extensive margin)

We have previously looked at the impact of direct income transfers on area cultivated. We can also look at the impact of the program on the decision to practice agriculture and cultivate any amount of land in the post-treatment crop season. This allows us to check for changes at the extensive margin, whereas area cultivated allowed us to check for changes in the practice of agriculture at the intensive margin. A reduction in the proportion of households practicing agriculture post-treatment may indicate a shift from farm to non-farm sectors.

Table A1.2. Effect of Income Transfer on Practice of Agriculture (extensive margin), Cross-Sectional RD Estimates

	Practiced agriculture post-treatment (%)			
	(1)	(2)	(3)	(4)
Treatment=1	-1.011	3.354	-1.116	2.446
	(11.06)	(17.66)	(20.71)	(22.06)
Household Controls	No	Yes	No	Yes
Bandwidth	0.95	0.60	0.85	0.83
Order polyn	1.00	1.00	2.00	2.00
Control mean	45.2	44.3	43.9	43.5
Observations	2246	1839	2246	1839

Notes: In this table, each column reports results from a separate regression with the dependent variable in the column header. The main explanatory variable is a binary variable indicating whether a household is eligible to receive income transfers under the KALIA program. The estimating equation is given by equation (3) and is estimated for orders 1 and 2 of the local polynomial. Columns 2 and 4 add household controls to equation (3) and report covariate-adjusted estimates. Household covariates include pre-program characteristics such as religion, social group (caste), type of house, monthly consumption expenditure whether the household possesses a Kisan Credit Card, and whether the household practiced agriculture in the past 365 days. The total number of observations are included in the last row of the table. The effective number of observations vary with bandwidth and range between 120-180. Optimal bandwidths have been calculated using Calonico, Cattaneo, and Titiunik (2014a, b). Standards errors are (in parentheses) robust and clustered at the first-stage sampling unit.

C. Impact on investment into farm and non-farm assets

We also check for evidence of a change in the decision to invest in high-value, productive assets, both farm and non-farm. This allows us to test the hypothesis that the income transfers may stimulate investment into the acquisition of productivity enhancing assets. The household can decide to invest in assets or to use the funds for consumption expenditure. Secondly, if the household decides to direct the funds towards capital expenditure, it can choose to invest in either farm or non-farm equipment and assets.

Table A13. Effect of Income Transfer on Investment into Productive Assets, Cross-Sectional RD Estimates

	Invested in farm asset post-treatment (%)			
	(1)	(2)	(3)	(4)
Treatment=1	-15.40**	-14.21*	-22.28**	-22.00**
	(8.247)	(8.904)	(9.278)	(9.600)
Household Controls	No	Yes	No	Yes
Bandwidth	0.41	0.43	0.64	0.64
Order polyn	1.00	1.00	2.00	2.00
Control mean	12.7	14.3	16.5	16.4
Observations	2246	1839	2246	1839

	Invested in non-farm asset (%)			
	(1)	(2)	(3)	(4)
Treatment=1	-3.061	-1.508	-1.671	-2.128
	(1.625)	(1.542)	(2.159)	(2.209)
Household Controls	No	Yes	No	Yes
Bandwidth	0.53	0.78	0.77	0.78
Order polyn	1.00	1.00	2.00	2.00
Control mean	2.3	2.2	2.2	2.2
Observations	2246	1839	2246	1839

Notes: In this table, each column reports results from a separate regression with the dependent variable in the column header. The main explanatory variable is a binary variable indicating whether a household is eligible to receive income transfers under the KALIA program. The estimating equation is given by equation (3) and is estimated for orders 1 and 2 of the local polynomial. Columns 2 and 4 add household controls to equation (3) and report covariate-adjusted estimates. Household covariates include pre-program characteristics such as religion, social group (caste), type of house, monthly consumption expenditure whether the household possesses a Kisan Credit Card, and whether the household practiced agriculture in the past 365 days. The total number of observations are included in the last row of the table. The effective number of observations vary with bandwidth and range between 120-180. Optimal bandwidths have been calculated using Calonico, Cattaneo, and Titiunik (2014a, b). Standards errors are (in parentheses) robust and clustered at the first-stage sampling unit.

D. Impact on number of crops produced

Lastly, we look for any changes in the number of crops that an agricultural household that decides to cultivate land in a given period chooses to produce. Households may sow and produce different varieties of crops in each period in an attempt to cope with the risk of crop failure, price fluctuations, etc. Reduced uncertainty of income can get reflected in cultivators opting for crop concentration (fewer crops) instead of crop diversification.

Table A1.4. Effect of Income Transfer on Investment into Number of Crops Produced, Cross-Sectional RD Estimates

	Number of crops grown			
	(1)	(2)	(3)	(4)
Treatment=1	0.0730	0.0548	0.225	0.275
	(0.328)	(0.359)	(0.433)	(0.382)
Household Controls	No	Yes	No	Yes
Bandwidth	0.73	0.60	0.95	1.05
Order polyn	1.00	1.00	2.00	2.00
Control mean	1.58	1.59	1.58	1.57
Observations	818	818	818	818

Notes: In this table, each column reports results from a separate regression with the dependent variable in the column header. The main explanatory variable is a binary variable indicating whether a household is eligible to receive income transfers under the KALIA program. The estimating equation is given by equation (3) and is estimated for orders 1 and 2 of the local polynomial. Columns 2 and 4 add household controls to equation (3) and report covariate-adjusted estimates. Household covariates include pre-program characteristics such as religion, social group (caste), type of house, monthly consumption expenditure whether the household possesses a Kisan Credit Card, and whether the household practiced agriculture in the past 365 days. The total number of observations are included in the last row of the table. The effective number of observations vary with bandwidth and range between 120-180. Optimal bandwidths have been calculated using Calonico, Cattaneo, and Titiunik (2014a, b). Standards errors are (in parentheses) robust and clustered at the first-stage sampling unit.

Appendix A2: Robustness checks

Table A2.1. RD Estimates on First-Differenced Outcomes

	Share of informal credit (%)		Amount borrowed (Rs)	
			Informal source	Formal source
	(1)	(2)	(3)	(4)
Treatment=1	-48.08	-56.23	-22620.4*	56094.1**
	(45.57)	(46.90)	(13665.9)	(19389.3)
Bandwidth	0.55	0.94	0.67	0.28
Order polyn	1.00	2.00	1.00	1.00
Control mean	14	15	4577	3220
Observations	347	347	2538	2538

	Area cultivated (acres)		Yield (kg/acre)	
	(1)	(2)	(3)	(4)
Treatment=1	-0.611	-1.107*	823.4	1023.2*
	(0.444)	(0.612)	(589.0)	(674.2)
Bandwidth	0.91	0.72	0.45	0.75
Order polyn.	1.00	2.00	1.00	2.00
Control mean	-2.2	-2.1	349.5	264.8
Observations	810	810	804	804

Table A2.2 Robustness of Bandwidth Choice and Order of Polynomial

	Share of informal sources in credit (%)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment=1	-54.88**	-44.00**	-35.17**	-25.24**	-16.10*	-13.42	-13.64
	(24.26)	(24.47)	(23.43)	(23.81)	(23.25)	(22.85)	(22.36)
Order polyn	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Bandwidth	0.4	0.5	0.6	0.7	0.8	0.9	1.0

	Share of informal sources in credit (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment=1	-14.32	-85.37***	-76.73*	-97.17**	-61.49	-57.82
	(22.93)	(32.99)	(38.40)	(42.46)	(60.43)	(69.50)
Bandwidth	0.84	0.53	0.69	1.09	1.07	1.38
Order polyn	1.00	2.00	3.00	4.00	5.00	6.00

	Area cultivated (acre)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment=1	-1.244***	-0.988***	-0.730**	-0.539**	-0.469	-0.478	-0.461
	(0.717)	(0.691)	(0.663)	(0.632)	(0.538)	(0.498)	(0.478)
Order polyn	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Bandwidth	0.4	0.5	0.6	0.7	0.8	0.9	1.0

	Area cultivated (acre)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment=1	-0.545	-1.600**	-2.358***	-2.466***	-2.586***	-2.372**
	(0.632)	(0.762)	(0.761)	(0.783)	(0.805)	(0.865)
Bandwidth	0.69	0.69	0.75	0.85	1.05	1.08
Order polyn	1.00	2.00	3.00	4.00	5.00	6.00

	Crop Yield (kg per acre)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment=1	388.8	190.3	-177.6	-401.5	-443.3	-464.8	-484.1
	(537.7)	(472.8)	(458.8)	(451.2)	(379.2)	(360.7)	(348.6)
Order polyn	1.00	1.00	1.00	1.00	1.00	1.00	1.00

Bandwidth	0.4	0.5	0.6	0.7	0.8	0.9	1.0

	Crop Yield (kg per acre)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment=1	-3.905	946.0	1149.6	1323.0	911.3	7.228
	(454.7)	(646.5)	(742.7)	(934.5)	(1128.4)	(1398.6)
Bandwidth	0.56	0.64	0.90	1.03	1.09	1.01
Order polyn	1.00	2.00	3.00	4.00	5.00	6.00

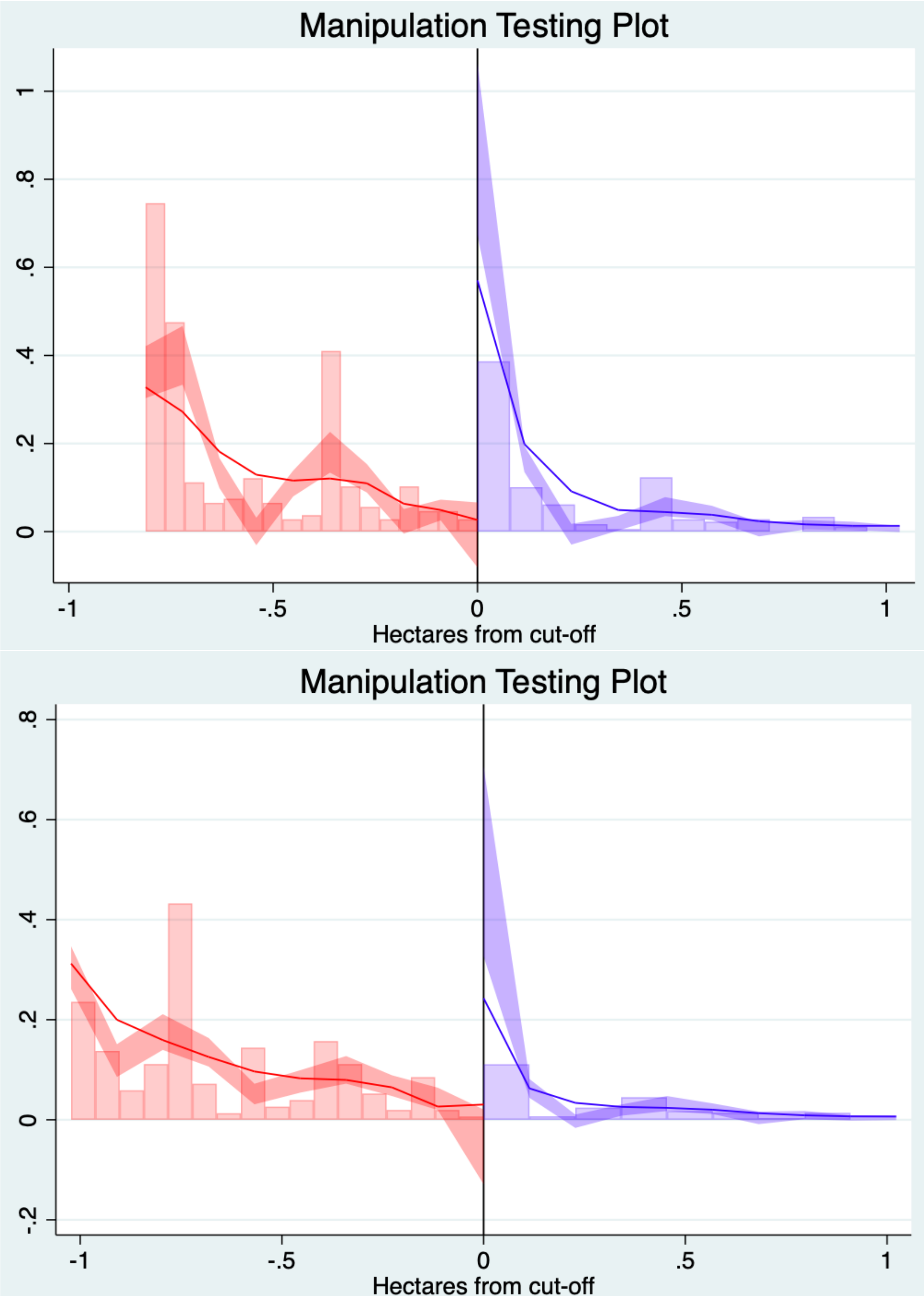
Appendix A3: Additional Validity Tests

(a) Falsification Test

The new cut-off for the falsification test is chosen as the median value of the running variable on the left side of the eligibility cut-off. We don’t find any significant evidence of discontinuities in outcomes at points other than the cut-off.

	Share of informal credit (%) (1)	Crop Yield (kg per acre) (2)	Area Cultivated (acre) (3)
Treatment=1	-1.383	2355.9	0.287
	(13.04)	(2032.9)	(0.358)
Bandwidth	0.2	0.14	0.4
Order polyn	1.00	1.00	1.00
Observations	783	810	818

(b) Manipulation Test



Notes: Top graph shows plot for sample collected under Schedule 33.1 and bottom graph shows plot for sample collected under Schedule 18.2. The test is conducted using the local polynomial density estimators in Cattaneo, Jansson and Ma (2020).