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ABSTRACT

This study estimates the causal effect of Rwanda's unconditional cash transfer program (VUP-Direct Support) on the incidence of poverty, the poverty gap, and household food and non-food expenditure for direct support recipients. Our empirical analysis applies four matching methods to data from the 2013/14 household survey in order to estimate the program impact on the treated. The findings show that participation in the program has positive and statistically significant effects on measured headcount poverty and poverty gap. The program results in a small increase in both total and food consumption, with a reduction in consumption of food from home production, and no change in non-food consumption. The estimated treatment effects are relatively robust to violations of the conditional independence assumption, and to the choice of subsample.

The fact that average annual cash transfers are equivalent to a third of total consumption, for recipients, plays an essential role in the observed results. Households respond to VUP cash transfers by working less on their own farms. This is a qualitatively different response than the reaction to remittances, which are associated with more consumption of all types. While VUP cash transfers do not raise consumption by as much as would be expected, thus having a modest effect on *measured* poverty rates, the transfers in effect allow a significant number of older subsistence households to at least partly retire.

1. Introduction

In 2008, just fourteen years after the genocide against the Tutsis which left the country devastated, Rwanda had recovered enough to introduce an ambitious "flagship" anti-poverty package, the Vision 2020 *Umurenge* Program (VUP). The program rests on three pillars: a public works scheme, which began to operate that year; a system of direct support grants for those unable to fend for themselves, which was rolled out over a period of seven years, beginning in 2009; and a system of loans for small and medium businesses. The purpose of the VUP program is not just to alleviate poverty, but to help provide a more permanent pathway to sustainable livelihoods (MINALOC, 2011; MINECOFIN, 2017; NISR, 2012). The original, and unrealistic, goal was "to eliminate poverty by 2020", but the government of Rwanda sees value in the program and has continued to support it. The government and its development partners invested USD 50 million in the VUP program during its first decade of operation (Gatsinzi, 2019).

In this paper we assess the impact of the direct support component of the VUP, wherein the government provides unconditional cash transfers (UCTs) to households that are both extremely poor and lack able-bodied adult members. More specifically, we ask to what extent households that receive VUP direct support consume more overall, consume more food (including from their own production), and are pulled out of poverty, and this is the first contribution of our paper. Perhaps surprisingly, an empirical analysis of the impact of the direct support program on household living standards and welfare has not yet been published, although there have been some studies of the effects of the VUP overall (Gahamanyi & Kettlewell, 2015; Gatsinzi et al., 2019; Ndikubwimana & Dusingize, 2016) of the public works scheme (Murphy-McGreevey et al., 2017; Hartwig, 2014), and of whether the effects remain once a

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household has "graduated" out of the program (Gahamanyi & Kettlewell, 2015; Sabates-Wheeler et al., 2015).

There is a strong interest in UCTs in the least-developed countries, because conditional cash transfers, or unconditional in-kind transfers, or many other policies geared to the poor, such as microcredit, educational expansion, and infrastructure, either have their own limitations, or work too slowly. Rwanda is still a poor country, and while its economy is growing rapidly, its experience with UCTs is likely to be highly relevant elsewhere, especially in Africa; based on the World Bank's *World Development Indicators* database, an estimated 420 million people live in African countries whose GDP per capita lies between that of Rwanda in 1995 ($746 in 2017 international dollars in purchasing power parity terms) and today ($2,226 in 2019).

A recent World Bank study on *The State of Social Safety Nets 2018* (Ivaschenko et al., 2018) reviews the nature and extent of social support, including unconditional cash transfers, and includes some basic information on Rwanda. Compared to its peers, Rwanda's social safety nets – mainly unconditional cash transfers, public works, and pensions – have relatively low coverage, reaching an estimated 23 percent of the poorest quintile (Fig. 3.4), although for recipients, the benefits are comparatively large, especially for cash transfers (Fig. 3.21). An important drawback of the World Bank report is that it takes an accounting ("naïve") approach to measuring the impact of social transfers, which likely overstates their economic impact. For instance, a household spending USD 2.50 per person per day and receiving a transfer of USD 1 per person per day would be assumed to spend just USD 1.50 per person per day in the absence of the transfer; in this example, the transfer lifted the person out of poverty. But this ignores the potential behavioral responses to the transfer. Thus, the second contribution of our paper is that it measures the impact of unconditional cash transfers after allowing for changes in household behavior, and permits us to compare the observed results with those generated by the accounting approach.

We draw on cross-sectional data from a high-quality living-standards survey undertaken by the National Institute of Statistics of Rwanda (NISR) in 2014, and use matching methods to compare recipients of UCTs with those who would otherwise be eligible for them but did not receive them. This is possible because the direct support program only covered the whole country by 2016, so in 2014 there were still enough uncovered households to allow for a valid comparison and a plausible identification strategy.

We begin by presenting a discussion of what is known about UCTs in developing countries. This is followed by an overview of the Rwandan direct support program and the context in which it operates. We then present the data, the methodological approach, and our estimates, followed by some interesting conclusions.

2. Unconditional cash transfers and poverty alleviation

The World Bank estimates that about 800 million people in 150 countries benefit from a cash transfer program (Honorati et al., 2015). Such programs have been important in protecting and raising the living standards of vulnerable populations (Daidone et al., 2015; Margitic & Ravallion, 2019). Worldwide, an estimated 1.5 percent of GDP is spent on social safety nets, which include cash as well as in-kind transfers; the mean in Sub-Saharan Africa is also 1.5 percent of GDP. The most recent World Bank numbers show that Rwanda too spent 1.5 percent of its GDP on social safety net programs, with the bulk of it (1.21 percent of GDP) taking the form of unconditional cash transfers (Ivaschenko et al., 2018, Table D1).

There appears to be growing interest both in social safety net interventions, and in unconditional cash transfers more specifically worldwide. For instance, in Latin America and the Caribbean, social safety net spending rose from 0.43 percent of GDP in 2003 to 1.26 percent by 2015 (Ivaschenko et al., 2018). The number of countries in Sub-Saharan Africa with cash transfer programs doubled from 20 to 41 between 2010 and 2015, and now benefit 50 million people (Garcia & Moore, 2012; Honorati et al., 2015). In a few African countries, including Tanzania, Senegal, and Rwanda, "flagship" cash transfer programs have expanded particularly rapidly over the past decade. In Rwanda, the share of spending on UCTs rose from 0.02 percent of GDP in 2009 to 0.22 percent by 2019 (calculated based on Gatsinzi, 2019; NISR, 2019).

The appeal of unconditional cash transfer programs is that they represent a potentially straightforward mechanism for alleviating poverty. They are easier to manage than in-kind transfers, and leave households with the most possible flexibility in how to adjust their spending. Not only are the transaction costs low, but there is no subsequent need to monitor how the resources are used.

Nevertheless, UCTs have some disadvantages from a policy point of view: they might be spent on "non-essential" goods, and thereby compromise welfare in the long-term; they could lower labor supply due to their income effect (Baird et al., 2018); and their allocation could lead to conflict within the household or community (Bobonis et al., 2015; Hidrobo et al., 2016).

A recent World Bank study that used household survey data from 79 countries, rich and poor, found that unconditional cash transfers lowered the headcount poverty rate for those in the lowest quintile by 8 percent, and the poverty gap by 14 percent (Ivaschenko et al., 2018). The study found that the comparable figures for Rwanda were 4 and 8 percent respectively, and in general the observed effects were relatively small in low-income countries. That study, which used an accounting approach, also found modest reductions in inequality due to social safety net programs.

Some researchers have examined the impact of UCT programs on productive assets, livestock accumulation, and health outcomes (see, for example, Covarrubias et al., 2012; FAO, 2014; Handa et al., 2016; Haushofer & Shapiro, 2016; Herrero et al., 2014; Lebihan and Mao Takongmo, 2019), or the effects on poverty-related indicators such as household expenditure, and food insecurity (see, for example, P. Gertler et al., 2006; P. J. Gertler et al., 2012; Haushofer & Shapiro, 2016; Hjelm et al., 2017; Sabates et al., 2019). Pfutze & Rodríguez-Castelán (2015) found that a small social pension in Colombia actually increased the labor force participation of younger males, by easing liquidity constraints on engagement in economic activity, although this was not true of other groups. Kassouf & de Oliveira, (2012) assessed the impact of a Brazilian non-contributory pension program on labor market outcomes, finding that it allowed older people to retire, and so reduced their labor force participation. Few studies have examined the effects of UCT programs on household consumption (see Banerjee et al., 2015; Garcia & Moore, 2012; Handa et al., 2018). But, none of these studies explicitly analyzed the impact of UCTs on poverty rates. However, Aguila et al. (2017) evaluated various targeting options for cash transfers in Mexico, and assessed their effects on the poverty gap for the poor older population in Mexico.

The World Bank confirms that cash transfer programs have been instrumental in lifting people out of poverty and reducing the poverty gap, especially in developing countries (Honorati et al., 2015). In the absence of empirical studies that directly measure the impact of unconditional cash transfers on the poverty rate, Giang & Nguyen, (2017) found a 3-percentage point poverty reduction as a positive and statistically significant causal effect of cash transfers programs on child welfare in Vietnam. Furthermore, Verhofstadt & Maertens, (2015) found a 10 percent poverty reduction effect of being a member of inclusive and effective agricultural cooperatives among farmers in Rwanda. The observed poverty reduction attributable to the program was associated with a relatively high amount of annual cash transfers received by the program beneficiaries (RWF 61,892 which is equivalent to USD[1] 89.2), which represented 38.8 percent of the national poverty line.

[1] USD 1= RWF 694 (Average exchange value of buying and selling values as of end-December 2014, from National Bank of Rwanda: www.bnr.rw)

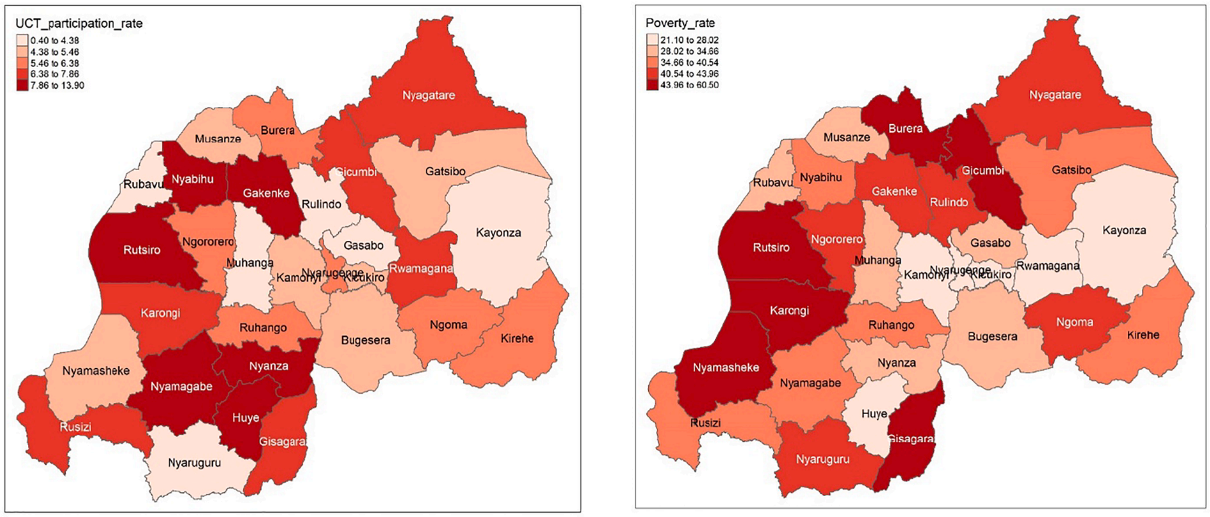

Fig. 1. Distribution of VUP-Direct Support beneficiaries (left) and headcount poverty (right). *Source:* Based on EICV 2014 data. Left panel shows VUP-DS beneficiaries as % of population.

Although mainly designed for poverty eradication, some cash transfer programs have greater ambitions. For example, the Malawi Social Cash Transfer Program, and Ghana's Livelihood Empowerment Against Poverty, both mention economic empowerment goals as additional program objectives (Covarrubias et al., 2012; Tsimpo & Wodon, 2012). Rwanda also views its VUP program as a way to help households emerge from poverty permanently, not just as a device for alleviating poverty in the short-term.

The UCT model is relatively simple, popular world-wide, and can reach a large number of recipients. However, what is less clear is the extent to which such programs reduce poverty – whether measured by a headcount measure or the poverty gap rate – or narrow inequality. Even less is known about the effects on food consumption, and on auto-consumption (i.e. the consumption of home-produced food and other items). We now address these issues in the context of Rwanda, both for their inherent interest, but also because the Rwandan experience has much wider applicability.

3. Direct support in Rwanda

Rwanda, a landlocked and mountainous country just south of the Equator with a population of 12.4 million in 2019, is the most-densely populated country in continental Africa. Recent economic growth has been rapid, with real GDP rising by an annual rate of 6.2 percent between 2001 and 2014 (NISR, 2019). Over the same period, the headcount poverty rate – based on a locally-developed poverty line that uses a cost-of-basic-needs approach – fell by nearly 20 percentage points from 58.9 to 39.1 percent (NISR, 2015a). During this time the inequality of expenditure per adult equivalent fell, with the Gini coefficient decreasing from 0.507 to 0.448 (NISR, 2015a). It is relevant to ask to what extent these changes are attributable to the unconditional cash transfers that constitute the main leg of the VUP program.

The Rwanda UCT program, called VUP-Direct Support, was introduced by the government in mid-2009. The main stated goal of the program is to provide regular and reliable income support to extremely poor, severely labor-constrained households, and ensure that these most vulnerable households can meet their most basic needs and be protected from destitution (LODA, 2016).

The program eligibility criteria are based on the *Ubudehe* targeting approach, which is a Participatory Poverty Assessment (PPA) that allows individual communities to articulate their definitions of poverty (Leurs, 1999). The process starts with a census of all households conducted, in principle, every three years by the Ministry of Local Government; however, the last one was in 2012/13, and is only now (in 2020) being updated. This census uses a simplified form to collect basic household characteristics, as well as information on house ownership, employment, the number of able-bodied adults, and the ability to afford food. Based on the data, the government assigned all households to *ubudehe* categories. These results were made available in the villages, which gave households an opportunity to appeal their classification, after which the lists were revised. The process was contentious, as many households wanted to be classified in a lower category, given the benefits (such a free health insurance, and educational scholarship) associated with a low classification.

Currently, all households in Rwanda are classified into the following four *Ubudehe* categories:

- *Ubudehe* category 1 (the very poor) are households who are "very poor and vulnerable and unable to feed themselves without assistance." They may have small amounts of land or livestock, but most of them are without land or livestock, or adequate shelter, clothes, or food. Some households have members who are physically capable of working on land owned by others, even if they have either no land or insufficient land to produce enough for survival needs.
- *Ubudehe* category 2 (the poor) is for households that have some land and housing and can live off their labor and production. They have no savings and their children do not always go to school.
- The non-poor (*Ubudehe* category 3) are households with some savings or assets, and children attend school regularly. This category includes people who have paid jobs, livestock, proper housing, may have a vehicle, and are food rich.
- The money-wealthy households are in *Ubudehe* category 4, and they have more substantial landholdings, many livestock, large private businesses, luxury housing, and they may own many vehicles.

Following the *Ubudehe* classification, households from *Ubudehe* categories 1 or 2 that do not have any member of the household who is able-bodied enough to work are eligible for direct support (LODA, 2016).

The geographic coverage of the UCT program has evolved over time. The program was first rolled out in the 30 poorest of the country's 416 administrative "sectors" – i.e. the sectors with the highest number of households in *Ubudehe* categories 1 and 2 that also lacked any able-bodied adults. In subsequent years the coverage expanded, and by 2013/14 the program covered 44 percent of all sectors in Rwanda (Gatsinzi, 2019). As shown in the maps in Fig. 1, the districts with high poverty rates in 2014 (right panel) also had relatively high proportions

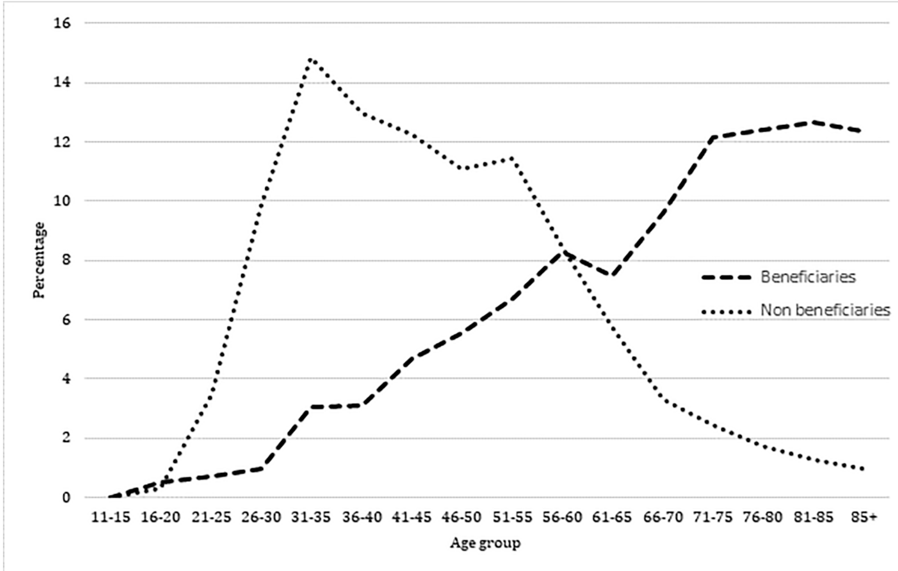

Fig. 2. Age distribution of household head for beneficiaries and non-beneficiaries of the VUP-Direct Support program Source: Authors' calculation based on EICV 2014 dataset.

Table 1
Comparison of monthly received transfers and official transfers set by the Government per household size.

Household size	Official transfers (in RWF)	Received transfers (in RWF)
1 member	7,500	9,477
2 members	12,000	12,384
3 members	15,000	13,118
4 members	18,000	14,025
5 + members	21,000	15,204

Source: LODA, 2016 & Calculation from EICV 2014 dataset.

of households who were benefiting from the UCT program (left panel).

In addition, the participation in VUP-DS program was generally ongoing, particularly during the survey period, in which no new targeting exercise was undertaken. Participants therefore normally joined when VUP-DS was rolled out to their sector, and then remained in the program until the time of the survey. In our evaluation sample, 28 percent joined the program between 2008 and 2010, another 28 percent between 2010 and 2012, and the remaining 44 percent between 2012 and 2014. Thus, more than half of those surveyed had been in the program for about three years or more at the time of the survey. For the VUP-DS program, the long duration of participation should in theory be associated with greater impacts, and indeed we would not expect to see much in terms of impacts for those who only joined the program in the year of the survey, as they would potentially only have had a few months of transfers. Nevertheless, the program did well in terms of targeting geographically, as shown by a comparison of the maps that show the districts of Rwanda in Fig. 1.

The program beneficiaries mainly live in households headed by older people, as Fig. 2 makes clear. In principle, they receive a regular, monthly unconditional cash transfer ranging from RWF 7500 (USD 10.8) for a household with one member to a maximum of RWF 21,000 (USD 30.3) for a household with five or more members (LODA, 2016). The monthly transfers are deposited to the beneficiary's bank account in the local community microfinance institution, the Sector Savings and Credit Cooperative (Umurenge SACCO), which is available in each of the 416 administrative sectors of Rwanda (Kamurase et al., 2012). However, the actual transfers received by the recipient household do not necessarily match the amount of official cash transfers set by the Government, for various reasons, including mismanagement of funds and corruption (Mbonyinshuti, 2016; Ntirenganya, 2017a, 2017b; Rwembeho, 2016), as well as erratic timing – just 15 percent of beneficiaries report getting their benefits on time. The survey data show that household beneficiaries with more than five members received 28 percent less than the official cash transfers they are supposed to receive, as Table 1 shows. In our analysis we use the actual reported payments received.

4. The data

The data we use come from the Integrated Household Living Conditions Survey of 2013/14, typically referred to as EICV 2014 (for Enquête Intégrale sur les Conditions de Vie des Ménages, fourth round). This nationally representative survey, conducted by the NISR, aims primarily to collect information on living standards with a view to measuring poverty (NISR, 2015a), but also gathers information on a wide range of socio-economic information related to the household and its members. The data were collected from October 2013 through September 2014 to allow for seasonal patterns in income and spending.

There are two main samples, which we have pooled for our study, both of which used the same questionnaire. The first surveyed 14,420 households who were selected nationally using a two-stage stratified-sampling strategy and selected systematically with probability proportional to size using the 2012 Rwanda Population and Housing Census as a frame (Nilsson et al., 2019; NISR, 2015c). The second sample, which included 2,460 households, was also nationally representative, selected from the beneficiaries of the VUP program. The sampling frame was a separate administrative database of all VUP recipients at the time of the survey (NISR, 2015b). In the summary results reported below, which are based on the pooled data, we use the appropriately adjusted sampling weights so that the data are nationally representative.

4.1. Outcome variables

We are interested in the impact of the direct support program on five main outcomes:

(i) The first is the headcount poverty rate, which is the percentage of people who are poor. We use the same methodology to measure poverty as employed by the NISR. Spending per adult equivalent is compared to a poverty line of RWF 159375 per person per year

Table 2
Descriptive statistics of outcomes for direct support program beneficiaries and non-beneficiaries, 2013/14.

Variable	Beneficiaries (B)	Non-Beneficiaries (NB)	All	Mean difference (B-NB)
Poverty measures	*percentages*			
Headcount poverty rate	45.6	38.6	38.7	7.0 **
Poverty gap rate	15.0	11.9	12.0	3.1 ***
Consumption and transfers	*'000 RWF per adult equivalent p.a. in January 2014 prices*			
Household consumption	193.2	308.3	306.2	−115.1 ***
Household food consumption of which	126.0	143.7	143.4	−17.8 ***
Own-food consumption	50.1	50.6	50.6	−0.5
Expenditure on food	75.8	93.1	92.8	−17.3 ***
Household annual nonfood expenditure	67.2	164.5	162.8	−97.3 **
Annual VUP-DS transfers received	61.9			
Estimated percentage of population	1.8	98.2	100.0	
Sample size	1,047	15,833	16,880	

Notes: *, **, *** represent statistical significance at 10%, 5%, and 1% level, based on a regression that includes sampling weights. The exchange rate in January 2014 was RWF631 per USD (National Bank of Rwanda). The poverty line was RWF 159,375 per adult equivalent per year. VUP-DS refers to direct transfers under the VUP program. *Source:* Calculation from EICV 2014 dataset

expressed in January 2014 prices (NISR, 2015a), equivalent to about USD 230.

Spending per adult equivalent is the measure of welfare that the NISR uses. It starts with household consumption from their own production ("autoconsumption") as well as items that they have purchased, and divides this by the number of adult equivalents. The NISR adult equivalence scale seeks to create a welfare measure that recognizes the differential costs of providing for family members with varying ages and gender; further details may be found in NISR, 2015a (Table B2).

(ii) The second outcome is the poverty gap rate. This measures the extent to which expenditure per adult equivalent for the whole sample falls short of the poverty line, expressed as a proportion of the poverty line (Haughton & Khandker, 2009). It is possible, even likely, that the unconditional cash transfers may, for many households, reduce the poverty shortfall without being enough to lift them out of poverty entirely. Such an effect would be captured better by the poverty gap rate than the headcount poverty rate.
(iii) The third outcome measure is consumption per adult equivalent. This is the measure used, when set against a poverty line, to determine the poverty rate, and is widely used as the most suitable measure of welfare (i.e. wellbeing) in Rwanda.
(iv) The next item of interest is spending on food, including autoconsumption. For poor households, just over 70 percent of consumption is on food, and we are interested in the extent to which the direct support program relaxes the constraints on food consumption, as this is the most fundamental of basic needs. Recent studies indicate that households use transfers to improve the quality of their diet, mostly from livestock products, hence increasing food expenditures (Hidrobo et al., 2018).
(v) The final outcome that we examine is consumption of home-produced food. Over half of Rwandan households are

Table 3
Descriptive statistics of program beneficiaries and non-beneficiaries.

Variable	Beneficiaries (B)	Non-Beneficiaries (NB)	All	Mean difference (B-NB)
Household characteristics (%)				
Ubudehe category 1	19.0	2.0	2.4	17.0 ***
Ubudehe category 2	65.2	22.0	22.9	43.2 ***
Ubudehe category 3	14.9	66.2	65.1	−51.3 ***
Ubudehe category 4	0.9	9.8	9.6	−8.9 ***
Head of household being female	59.2	20.0	20.7	39.2 ***
Household size *(Number)*	4.2	5.6	5.6	−1.4 ***
Average age of household head *(Number)*	65.1	45.1	45.5	20.0 ***
Household with elderly household members (aged 65 +)	60.9	10.7	11.6	50.3 ***
Number of able-bodied adults in household	2.1	2.6	2.6	−0.4 ***
Work status of the household head (%)				
Farm wage-earner	0.3	0.3	0.3	0.0
Non-farm wage earner	0.9	4.8	4.8	−4.0 ***
Independent Farmer	74.8	49.8	50.3	24.9 ***
Independent Non-farm work	22.2	37.0	36.7	−14.8 ***
Unpaid non-farm and other work	1.9	8.0	7.9	−6.2 ***
Education of the household head (%)				
No education	59.6	22.4	23.1	37.2 ***
Primary	38.2	62.9	62.5	−24.7 ***
Vocational	0.8	3.7	3.7	−3.0 ***
Secondary and higher	1.4	10.9	10.8	−9.5 ***
Net Enrollment rate in primary school	91.3	93.6	93.6	−2.3
Net Enrollment rate in secondary school	23.5	31.6	31.5	−8.2 *
Literacy rate (15 + years)	65.2	90.8	90.3	−25.6 ***
Disability				
Household with at least one disabled adult	27.2	10.8	11.1	16.3 ***
Number of observations	1,047	15,833	16,880	

Notes: *, **, *** represent statistical significance at 10%, 5%, and 1% level. *Source:* Calculation from EICV 2014 dataset.

independent farmers, and for them, own-food consumption accounts for an average of 34 percent of their total consumption. It is possible that direct support would replace home-produced food, and this is the effect we would like to measure.

The essential descriptive statistics for these outcome variables (and a few other related variables) are shown in Table 2. Households who benefit from the VUP-Direct Support program are poorer and spend less on food (per adult equivalent), than non-beneficiaries, and these differences are highly statistically significant. For instance, the poverty rate is 45.6 percent for beneficiaries, and 38.6 percent for those who do not receive direct support. All of the differences are due to the gap in spending on food and non-food; both beneficiaries and non-beneficiaries derive the same amount of food consumption (per adult equivalent) from their own farms and gardens.

The value of consumption used here (and in the measurement of poverty) is comprehensive, and includes direct spending on food and non-food items, as well as autoconsumption, the imputed value of rent (for homeowners), and the use-value of durables. In this it follows the recommendations of Deaton and Zaidi (2002). Further details are shown in Table A1.

It is also worth noting that only an estimated 1.8 percent of the population benefits from the Direct Support program. It follows immediately that the potential of the program to reduce the *national* poverty rate is inherently limited.

The program recipients are statistically different from non-recipients on various socio-economic dimensions. It follows that any simple comparison of means (for the output variables) would not reflect a causal effect. Table 3 shows that those who receive direct support transfers are far more likely to be classified in *Ubudehe* categories 1 and 2, as one would expect, since being in these categories is supposed to be a requirement for receiving these funds. However, 16 percent of UCT recipients are in *Ubudehe* categories 3 and 4, which indicates a miss-classification of some households.

Households that receive UCTs are more likely to be headed by a woman (59 percent, compared to 20 percent for non-recipient households), to have a much-older head, to have fewer able-bodied adults, and to have less educated household head. Indeed, 60 percent of the heads of UCT-recipient households have had no education, compared to 22 percent for non-recipients. Furthermore, 27 percent of households receiving direct support have a disabled adult member, compared to 11 percent for non-recipients, numbers that are consistent with World Health Organization estimates (WHO, 2011). The numbers in Table 3 also show that UCT program beneficiaries are more likely than non-beneficiaries to cook with firewood (98 percent vs. 84 percent), and to use firewood, candles, or oil lamps for lighting (29 percent vs. 20 percent).

5. Methods

The aim of the study is to estimate the causal effect of the UCT program on the incidence of poverty, the poverty gap, and household food and non-food expenditure for direct support recipients. This requires establishing a counterfactual (Bagnoli, 2019; Shikuku et al., 2019; Vo and Van, 2019), by comparing the outcomes of those who receive direct-support transfers with those of a control or comparison group that is as similar as possible to those receiving the transfers.

The "gold standard" procedure of random assignment of treatment, even conditional on observed household characteristics, did not occur with the Rwandan program. The program was first rolled out in the poorest administrative sectors of the country, and selection into the program is also, obviously, not random, as the aim is to include only the poorest households. Thus, we are obliged to use a quasi-experimental design.

There are a number of approaches to measuring treatment effects, all based on the idea of matching the treated (who received UCTs) with observationally similar non-treated households (Abadie et al., 2004). We start with propensity score matching, but check for the robustness of our results by also estimating the treatment effects using three other approaches, discussed below. The methods yield relatively similar results, which helps allay the concerns of Gary King and others (King & Nielsen, 2016) that the availability of a multitude of models for measuring causal effects leaves too much discretion in the hands of the researcher.

The **Propensity Score Matching** (PSM) approach developed by Rubin and his collaborators (Rosenbaum and Rubin, 1983; Rubin, 1973) first estimates a logistic regression that models how households are assigned to treatment (the "assignment model"), and computes the estimated probabilities of treatment (the propensity scores). Then those who are treated – in our case, households receiving direct-support transfers – can be matched with those who are not, but who have similar propensity scores, and the relevant outcomes (food spending, poverty, and the like) compared.

More formally, the study measures the outcome of interest, Y_i, for both treated and control groups, where T_i denotes treatment status (1 for treated and 0 for control). The propensity score, denoted by $P(X_i) = Pr(T_i|X_i)$, measures the probability of participation in the program given the set of X_i observable characteristics. The average treatment effect on the treated (ATET), given by the expected impact (I), becomes:

$$I^{ATET} = E\left[\left(Y_i^T - Y_i^C\right)\middle|T_i = 1\right] \quad (1)$$

Here, $(Y_i^T|T_i = 1)$ is observed, but the counterfactual $E(Y_i^C|T_i = 1)$ has to be established, and this is done by using the propensity score to identify non-treated cases whose propensity scores are "close" to those of treated households.

The validity of the PSM matching results depends on meeting two conditions successfully, namely the assumptions of conditional independence, and common support. Conditional independence requires that there be a set X_i of covariates, observable to the researcher, such that after controlling for these covariates, the potential outcomes are independent of the treatment status (Caliendo & Kopeinig, 2008; Diagne & Demont, 2007; Heckman & Navarro-Lozano, 2004; Smith and Todd, 2005). Formally, it implies that:

$$\left(Y_i^T, Y_i^C\right) \perp T_i|X_i \quad (2)$$

In other words, it ensures that the comparison group identified by the matching process is sufficiently similar to the treatment group, ensuring balance in the groups that "recreate" the natural balance that would occur in a randomized selection process.

The common support assumption requires that for each value of the propensity score there is a nonnegative probability of being both treated and untreated (Caliendo & Kopeinig, 2008; Crump et al., 2006).

$$0 < Pr(T_i = 1|X_i)\langle 1 \quad (3)$$

Eq. (3) implies that the probability of receiving the UCT program, conditional on X_i covariates lies between 0 and 1. By the rules of probability, this means that the probability of not receiving the program support lies between the same values $Pr(T_i = 0|X_i) = 1 - Pr(T_i = 1|X_i)$. For every possible score, there must be both treated and untreated individuals, or there must be enough overlap in characteristics (or 'common support') between the two groups so that there are adequate matches.

In estimating the propensity score we use a logit model, and use covariates that are correlated with treatment status but are not themselves affected by the outcomes of treatment (Imbens, 2015; Sianesi, 2004). The details are presented below. PSM methods are not able to control for unobserved individual heterogeneity, but they mitigate or remove selection bias. However, King & Nielsen, (2016, p.2) show that propensity score matching may actually "degrade inferences" if the data are initially well balanced.

To address this concern, we also measure the effects of the direct-support transfer program using three other methods.

The **nearest neighbor ("covariate") matching** (NNM) method starts by normalizing the variables and constructing a measure of distance between every pair of observations – we employ the widely-used Mahalanobis distance. Then every treated case is matched with the closest non-treated case, and compared the outcomes (Abadie et al., 2004; Abadie & Imbens, 2011; Austin, 2010, 2014; Wager & Athey, 2018).

Inverse probability weighting (IPW) estimates a weighted regression of the form

$$Y_i = \alpha + \beta T_i \quad (4)$$

where the weights for measuring the ATET are given by

$$w_i = \begin{cases} 1 & \text{fortreatedcases} \\ \widehat{p}_i \Big/ \left(1 - \widehat{p}_i\right) & \text{fornon} - \text{treatedcases} \end{cases}$$

Here the $\widehat{p}_i$ are the estimated propensity scores. The estimator has

Table 4
Model Results for Program Participation ("Propensity Score Equation").

	Logit model			Linear probability model	
	Coefficient	Standard error	Marginal effect	Coefficient	Standard error
***Ubudehe* category (reference is groups 3 and 4)**					
1 (poorest)	3.173	0.138	0.122	0.267	0.009
2 (poor)	2.164	0.103	0.083	0.093	0.004
Gender of household head					
Male (reference category)					
Female	0.504	0.085	0.019	0.018	0.004
Age of household head (years)	0.030	0.004	0.001	0.001	0.000
Proportion aged 15 + who are literate	−0.247	0.097	−0.010	−0.022	0.005
Presence of able-bodied adult					
No able-bodied adult (reference category)					
At least one able-bodied adult	−0.367	0.116	−0.014	−0.129	0.008
At least one elderly (65 +) member	1.008	0.140	0.039	0.096	0.007
Household size	−0.126	0.025	−0.005	−0.005	0.001
Presence of member with disability					
No disabled member (reference category					
At least one disabled member	0.513	0.093	0.020	0.015	0.005
N	16,878			16,878	
Pseudo R2 / Adjusted R2	0.410			0.252	

Notes: Dependent variable is whether household receives VUP-Direct Support (yes = 1) or not. Dummy variables for the 30 districts were included in the regression but their coefficients are not shown here. All coefficients shown are statistically significant at the 1% level or better, except for Literacy in the logit model (significant at 5% level). "Marginal effect" measures the average effect on participation of a unit change in the relevant right-hand variable.

been found to work well provided that the propensity scores are neither very low nor high (Austin, 2016; Han & Kim, 2011; Hirano et al., 2003; Pirracchio et al., 2016; Wooldridge, 2007).

The **inverse probability-weighted regression adjustment** (IPWRA) method is "doubly robust", and combines the regression adjustment (RA) method with inverse probability weights. Under the RA method, and assuming linearity, the approach is to estimate one equation that predicts performance for the treated, and another for the non-treated, using the distance between the two lines, suitably aggregated, to measure the impact of treatment (Austin & Laupacis, 2011; Myers & Louis, 2010; Vansteelandt & Daniel, 2014). The IPWRA approach estimates the regression adjustment equations using the inverse probability weights. The method can yield good results even if some of the assumptions underlying RA or IPW are not met (Kreif et al., 2013; Qin et al., 2017; Rotnitzky & Robins, 2014; Tan, 2010).

Table 5
Exploring balance in the data.

	Raw difference	Propensity Score Matching	Nearest Neighbor Matching	Inverse Probability Weights
Ubudehe category 1 (poorest)	**0.656**	0.020	0.002	−0.035
Ubudehe category 2 (poor)	**0.873**	−0.027	0.051	0.018
Household head is female	**0.895**	−0.063	−0.014	−0.066
Age of head of household	**1.380**	−0.004	0.159	−0.070
Proportion aged 15 + literate	**−0.762**	−0.016	−0.069	0.023
At least one able-bodied adult	**−0.967**	−0.048	−0.088	0.033
At least one elderly (65 +)	**1.332**	−0.021	0.106	−0.062
Size of household	**−0.762**	−0.035	−0.063	0.013
At least one disabled member	**0.356**	−0.060	0.084	−0.074

Notes. Each row shows the difference in the standardized value of the variable for households receiving VUP direct support and those who do not: first the simple difference, and then the differences after different forms of matching (but with the same propensity score equation). Information on districts is not shown here.

6. Empirical results

For the three methods that require a propensity score, we proceeded as in the spirit of Rubin, (1973). First, we estimated an initial logit participation equation (the "propensity score equation"); then we examined whether the resulting matches were balanced, in the sense of there being no difference between the average value of variables between the treated cases and their matched non-treated cases. In the light of the estimates of balance, we revised the participation equation by dropping some of the variables that looked promising initially. After just a few iterations we achieved acceptable balance, as shown below (Table 5). We then used that version of the participation equation to generate our measures of impact. In what follows, we first present the results of the participation equation, then some information on balance, and finally the measures of impact.

The estimated coefficients of the logit participation equation, which is needed in order to compute the propensity scores, are shown in Table 4, along with the average marginal effects on participation of changing each regressor by one unit, and OLS estimates for comparison. As expected, households in the two lowest *ubudehe* categories are more likely to receive direct support, as are female-headed household, and households with older or disabled members. Conversely, households with more literate members, or that have able-bodied adults, are less likely to benefit from the program. The equation fits fairly well, with a Pseudo R^2 of 0.410. While the "linear probability model" gives, at best, an approximation of the marginal effects of the right-hand variables associated with participation, it is reassuring to see that it yields coefficients that are generally fairly close to the average marginal effects derived from the logit specification.

If the participation equation is well specified, it should yield balanced matches. The relevant data are shown in Table 5, where the "raw difference" measures the standardized difference between beneficiaries and non-beneficiaries in the pooled sample. The rest of the table shows the standardized difference between beneficiaries and the

Table 6
Effects of the VUP-Direct Support Program on Poverty of Beneficiaries.

	Poverty rate		Poverty gap rate	
	Impact	p-value	Impact	p-value
Propensity score matching (5 matches)	−0.049	0.011	−0.021	0.018
Nearest neighbor matching (Mahalanobis)	−0.067	0.007	−0.019	0.072
Inverse probability weights	−0.047	0.024	−0.021	0.016
IPW with regression adjustment	−0.050	0.009	−0.021	0.009
Memo items				
Poverty rate, beneficiaries	0.456		0.150	
Poverty rate, non-beneficiaries	0.386		0.119	
Observations	16,878		16,878	

Notes: The first and third data columns show the average treatment effect on the treated (ATET). Data are from EICV 2014, undertaken in Rwanda in 2013/14. The propensity equation uses the variables listed in Table 5, district dummies, and indicators variables on whether the household has a TV or living room suite. The outcome equation has the same variables (except whether the household has a disabled member) and four additional variables related to the condition of the household's house and whether it has a radio or phone.

Table 7
Effects of the VUP-Direct Support Program on Household Consumption and use value of durable assets.

	Consumption		Food consumption		Own-food consumption		Durable assets	
	Impact	p-value	Impact	p-value	Impact	p-value	Impact	p-value
Propensity score matching (5 matches)	0.034	0.183	0.048	0.105	−0.113	0.027	0.014	0.870
Nearest neighbor matching (Mahalanobis)	0.052	0.076	0.049	0.136	−0.047	0.527	−0.196	0.066
Inverse probability weights	0.038	0.159	0.049	0.113	−0.138	0.009	−0.034	0.704
IPW with regression adjustment	0.044	0.068	0.054	0.065	−0.085	0.102	0.089	0.178
Memo items	*000 RWF per adult equivalent p.a. in January 2014 prices*							
Consumption, beneficiaries	193.2		126.0		50.1		0.8	
Consumption, non-beneficiaries	308.3		143.7		50.6		12.0	
Observations	16,868		16,866		15,017		12,846	

Notes. Impact is measured for the log values of the variables. All measures of consumption and use value of durable assets refer to consumption and value per adult equivalent per year in thousands of RWF. Based on EICV 2014, undertaken in Rwanda in 2013/14. The treatment effects from IPW with regression adjustment have been estimated using two different equations. One equation with variables that affect outcome but not the treatment. These variables include household dwelling material (cement floor, walls in mud bricks), assets ownership (radio, mobile phone, television set, living room suite). The second equation has variables that affect treatment but not the outcome including the household with disabled adult member.

matched non-beneficiaries in the pre-existing or exogenous variables. The idea is to create the equivalent of a randomized sample, where there should be little or no difference between the treated and the untreated. The differences depend on the matching procedure used, but it is clear from inspecting Table 5 that these differences have been reduced to very modest levels, for all of these variables, as a result of the matching process. We are therefore confident enough to proceed to the final step, which is examining the measures of impact of the direct support program.

The propensity scores for non-beneficiaries vary from 0.0002 to 0.889, and for beneficiaries they range from 0.002 to 0.967. There is thus a wide range of common support, with only 15 out of 1,047 beneficiaries having propensity scores higher than any of the 15,831 non-beneficiaries, which allows for good matching.

The estimates of the impact of the VUP-Direct Support program on the poverty rates of beneficiaries (ATET) are shown in Table 6. While the different methods yield somewhat different estimates, all show a statistically significant reduction in measured headcount poverty of between about five and eight percentage points. For instance, if the propensity score matching results are used, they imply that in the absence of the Direct Support program, the poverty rate for beneficiaries would have been about 50.5 percent instead of the observed 45.6 percent. The numbers for the poverty gap rate tell a similar story, with reductions of about two percentage points.

The estimated impact of the VUP-Direct Support on consumption, and purchases of durable goods,[2] is shown in Table 7, using a variety of measures and models. The impact was measured using the logs of consumption and of the value of durable assets, and so the upper panel in Table 7 shows proportionate changes. For instance, when propensity score matching is used, the consumption level of beneficiaries is raised by 3.4 percent; food consumption rises by 4.8 percent; and own-food consumption drops by 11.3 percent. A similar pattern is found using the other models, and about half of these effects are statistically significant at the 10 percent level or better. In all four models, the effects of VUP-DS on the use value of durable assets is not statistically significant at the five percent level.

In measuring the impact of direct transfers, it is essential to allow for behavioral responses, such as reducing autoconsumption, investing, or farming. Otherwise, the effect on consumption spending, and on *measured* poverty will be seriously overstated. This can be made clearer with the help of Table 8, which compares the impact of VUP-Direct Support using an accounting ("naïve") approach with that derived from our treatment effects estimation. The accounting approach implies a reduction of 24 percentage points in the measured headcount poverty rate, for beneficiaries, while the estimated effects are between five and seven percentage points.

If we simply added the direct support payments to household consumption, for recipients, their consumption would be 32 percent higher according to the accounting approach, while we found an increase of between three and five percent (depending on the estimation model used). This does not mean that the transfers are being wasted, only that they are being used for things other than boosting consumption.

There is some evidence that households respond to direct support differently from remittances. To examine this, we identified those households that receive remittances that are at least as large as the lowest one percent of direct support transfers, which is done in order to exclude remittances that are trivially small. We then estimate the effect of remittances using the same approaches as for direct support payments. Although, on average, these remittances are half the size of direct support payments (Mean value RWF 29100p.a. for remittances vs. RWF 61900 for DS payments), remittances have a larger impact in reducing measured poverty and boosting consumption, including (perhaps

[2] The main durable goods purchased by households in the year preceding the survey included: radios, mobile phones, TVs, satellite dishes, computers, furniture, bicycles, cookers, fans, sewing machines, fridges, electric generators, motorcycles, and cars.

Table 8
Treatment effects vs. Accounting impact for VUP-Direct Support, and Remittances.

		Estimated impact		
		VUP-Direct Support		Remittances
	Observed	Accounting	Treatment effects	Treatment effects
			Percentange point changes	
Poverty rate (%)	45.6	−24.4	**−4.9, −6.7, −4.7, −5.0**	**−12.4, −16.6, −13.7, −13.6**
Poverty gap rate (%)	15.0	−21.3	**−2.1, −1.9, −2.1, −2.1**	**−4.3, −5.5, −4.7, −4.3**
Consumption/ ae, RWF	193.2	61.9		
			Percentage point changes	
Consumption	100.0	32	3.4, **5.2**, 3.8, **4.4**	**23.5, 32.3, 25.4, 28.4**
Food consumption	65.2		4.8, 4.9, 4.9, **5.4**	**10.8, 16.5, 12.1, 13.5**
Own-food consumption	26.0		**−11.3**, −4.7, **−13.8**, −8.5	**11.4, 13.7, 11.1, 10.5**
Durable assets	0.4		1.4, **−19.6**, −3.4, 8.9	**27.2, 37.0, 31.4, 33.5**

Notes: Based on EICV 2014 undertaken in Rwanda in 2013/14. Treatment effects are from Tables 5 and 6 and Tables A2 and A3. The measures of the effects (ATET) come from the four estimation models (presented in order, as in Tables 6 and 7); values that are statistically significantly different from zero at the 10% level or better are shown in bold face. Remittances are only counted if they are at least as large as the lowest 1% of VUP-DS payments; on average they are half as large as direct support payments.

surprisingly) own-food consumption. The results for remittances are shown in Tables A2 and A3, and summarized in Table 8. A plausible story is that remittances respond to household needs that may not be observed in the EICV survey, while the direct support payments are more akin to dependable pensions. Whatever the reason, it does appear that households respond differently to these two sources of cash income. Whether this represents a change in preferences (when the money is in the form of direct support), or reflects a complex set of preferences, is not clear. But one result is that the measured impact on poverty (for those who receive them) is larger for remittances than for direct support, even though the welfare effects are presumably larger for direct support.

7. Robustness

In this section we examine the robustness of our results to violations of the underlying assumptions, and to different filters.

Matching methods, such as propensity score matching, rely on the Conditional Independence Assumption (CIA), which requires the conditional independence of potential outcomes and treatment assignment, given the observed covariates. This would be violated if there is a confounder that simultaneously influences treatment and the outcome of that treatment. Ichino et al. (2008) propose a sensitivity analysis that

Table 11
Descriptive statistics of preferential use of unconditional cash transfers (UCT) for direct support program beneficiaries and non-beneficiaries, 2013/14.

Spending item	Percent reporting main use	Sample size
Durable assets or utensils	9.9	104
Education or health	6.0	63
Purchase of livestock	15.6	163
Farming or income generating activities	11.9	125
House renovation	13.6	142
Saving	1.0	10
Other purposes, including food	42.0	440
Total	100.0	1,047

Source: Based on EICV 2014 data.

Table 9
Results of Analysis of Sensitivity to Confounders Related to Binary Variables.

	Poverty rate		Poverty gap		Consumption		Food		Autoconsumption	
	ATT	s.e.	ATT	s.e.	ATT	s.e.	ATT	s.e.	ATT	s.e.
No confounder	−0.041	0.025	−0.017	0.011	0.062	0.032	0.042	0.035	−0.199	0.065
Neutral confounder	−0.049	0.031	−0.022	0.012	0.044	0.040	0.039	0.046	−0.169	0.078
Confounder like:										
Ubudehe category 1 (poorest)	−0.088	0.041	−0.034	0.018	0.078	0.048	0.061	0.064	−0.107	0.108
Ubudehe category 2 (poor)	−0.117	0.034	−0.049	0.016	0.118	0.044	0.092	0.052	−0.091	0.088
Household head is female	−0.064	0.037	−0.024	0.014	0.051	0.045	0.035	0.059	−0.187	0.059
Proportion aged 15 + literate	−0.080	0.036	−0.034	0.014	0.073	0.044	0.046	0.050	−0.153	0.091
At least one able-bodied adult	−0.030	0.047	−0.013	0.032	−0.002	0.065	−0.006	0.072	−0.298	0.119
At least one elderly (65 +)	−0.024	0.043	−0.004	0.021	0.015	0.062	−0.027	0.064	−0.282	0.109
At least one disabled member	−0.054	0.035	−0.023	0.014	0.035	0.043	0.011	0.051	−0.177	0.082

Source: Based on data from EICV 2014. Follows the method of Ichino et al. (2008), using the sensatt implementation in Stata. Simulations are based on 500 replications.

Table 10
Estimated VUP-DS treatment effects by ubudehe category and labor capacity.

	Full sample	*Ubudehe* category 1 and 2	Households with members in working age, but not able bodied to work	Households with members outside working age, but able bodied to work
Poverty rate (%)	**−4.9, −6.7, −4.7, −5.0**	**−5.4, −8.1, −5.8, −5.5**	−14.2, **−17.6**, **−19.9**, −10.5	−4.1, −4.9, −2.4, −5.1
Poverty gap rate (%)	**−2.1, −1.9, −2.1, −2.1**	**−2.3, −2.5, −2.4, −2.3**	**−4.1**, −3.5, **−9.2**, **−9.3**	−1.1, −1.2, −0.8, −1.6
Consumption (%)	3.4, **5.2**, 3.8, **4.4**	**7.3, 8.7, 7.2, 6.7**	4.4, 13.2, 13.3, 12.3	1.3, 1.7, −0.4, 3.7
Food consumption (%)	4.8, 4.9, 4.9, **5.4**	**7.2, 7.3, 7.0, 7.3**	−2.9, 7.6, 6.6, 12.9	0.2, 1.9, −0.3, 1.8
Own food consumption (%)	**−11.3**, −4.7, **−13.8**, −8.5	**−13.1, 3.1, −12.2**, −4.9	−13.7, 12.0, −13.7, 5.3	−9.7, −7.4, 9.3, −9.9
Sample size	16,868	5,056	211	1,542

Notes: Based on EICV 2014 undertaken in Rwanda in 2013/14. The working age is 16–65 according to Law No 66/2018 of 30/08/2018 regulating labor in Rwanda (https://www.mifotra.gov.rw/publications). The measures of the effects (ATET) come from the four estimation models (presented in order, as in Tables 6 and 7); values that are statistically significantly different from zero at the 10% level or better are shown in bold face.

Table A1
Descriptive statistics of components of household consumption and expenditure for direct support program beneficiaries and non-beneficiaries, 2013/14.

Variable	Beneficiaries (B)	Non-beneficiaries (NB)	All	Difference (B-NB) *'000 RWF*
Total consumption *(000 RWF per adult equivalent p.a. in January 2014 prices*	**193.2**	**308.3**	**306.2**	**−115.1*** **
Food	*Percentages*			
Food expenditure	39.3	30.2	30.3	−17.3***
Own food consumption	26.0	16.4	16.5	−0.5
Nonfood	*Percentages*			
Education	1.9	5.4	5.4	−13.0
Imputed rent	7.5	7.9	7.9	−9.9**
Actual rent	0.2	2.0	2.0	−6.0***
Imputed value of house provided	0.4	0.8	0.8	−1.7
Maintenance	1.2	1.6	1.6	−2.6
Water	0.5	0.6	0.6	−0.9***
Electricity	0.0	0.5	0.5	−1.4***
In-kind work-related payments	1.2	4.0	4.0	−9.9
Non-food expenditure	17.0	24.2	24.2	−42.0***
Use value of durable assets	0.4	3.9	3.9	−11.2
In-kind remittances received	4.1	2.4	2.4	0.7
Sample size	1,047	15,833	16,880	

Notes: *, **, *** represent statistical significance at 10%, 5%, and 1% level, based on a regression that includes sampling weights. *Source:* Calculation from EICV 2014 dataset.

Table A2
Treatment effects of in-kind received remittances on Poverty.

	Poverty rate		Poverty gap rate	
	Impact	p-value	Impact	p-value
Propensity score matching (5 matches)	−0.124	0.000	−0.043	0.000
Nearest neighbor matching (Mahalanobis)	−0.166	0.000	−0.055	0.000
Inverse probability weights	−0.137	0.000	−0.047	0.000
IPW with regression adjustment	−0.136	0.000	−0.043	0.000
Memo items	*(000 RWF per adult equivalent p.a. in January 2014 prices*			
Poverty rate, recipients	0.154		0.037	
Poverty rate, non-recipients	0.432		0.136	
Observations	16,880		16,880	

Notes: The first and third data columns show the average treatment effect on the treated (ATET). A dummy variable of received remittances is equal to one if received remittances are equal or more than the average VUP-DS payment of bottom 1% of VUP-DS beneficiaries. Based on EICV 2014, undertaken in Rwanda in 2013/14.

simulates the effects of allowing each of the major binary independent variables to have some confounding effect, and examines how robust the results are to these violations of the CIA.

The essential results are shown in Table 9, for propensity score matching with five matches; further details are available from the authors. Each pair of columns measures the impact of direct support and associated standard error, based on 500 simulations. As expected, the standard errors rise when the CIA is weakened, but the important findings remain intact: the direct support program reduces the measured poverty rate and poverty gap (modestly), has little effect on consumption or food consumption, and reduces autoconsumption (i.e., food consumed from one's own land). These simulations show that both the outcome and selection effects of a random confounder would have to be strong in order to represent a threat to the significance of the estimated ATT.

It is possible that our results are sensitive to the choice of sample. In principle, only households in *ubudehe* categories 1 and 2, who lack able-bodied workers, are eligible for direct support. In practice, some households outside these categories are receiving direct support, either because of poor initial targeting, or because they remain in the program even through their family situation may have changed. To address this issue, we applied our four techniques to subsamples of our data: only those in ubudehe categories 1 and 2; then only those lacking an able-bodied adult aged 16–65; and then only households with able-bodied members outside of working age.

The results are summarized in Table 10. The results based just on households in *ubudehe* categories 1 and 2 are very similar to those based on the whole sample. This is not surprising, given that matching methods are used, and only 16 percent of direct support recipients are in *ubudehe* category 3 or above. The measured impact of the VUP-DS program on households that lack able-bodied working-age members is more mixed, but the sample is small and almost none of the coefficients are statistically significant. And if the sample is confined to households that are older, but still have an able-bodied member, the effects of direct support are not statistically significant. Presumably the VUP-DS recipients in this subgroup are anomalous, because in principle they should not be receiving the support.

8. Discussion

The findings on the magnitude and trend pattern of headcount poverty reduction as an effect of the program are in line with other studies that have measured the poverty-reducing effects of different social protection or socio-economic development programs, including cash transfers, elsewhere (Giang & Nguyen, 2017; Honorati et al., 2015; Verhofstadt & Maertens, 2015). And the measured reductions in the poverty gap rate are not far from the findings of Becerril & Abdulai, (2010), who found a 5.1 percentage point reduction in the poverty gap as an effect of adopting improved varieties of maize in Chiapas (Mexico), although that program was more agriculturally-oriented than cash transfers. Aguila et al., (2017) found a much higher effect (16 percentage point reduction) of a simulated flat rate scheme of cash transfers on the poverty gap in Mexico, targeted at the elderly population.

The results on consumption are substantially more modest than those found by Hjelm et al., (2017), who found an increase of 20 percent of monthly per capita expenditure as a result of the multiple category unconditional cash transfer program (MCP) in Zambia. Becerril & Abdulai, (2010) also found a positive and statistically significant 27.2 percent increase in household consumption per capita as an effect of adopting improved maize varieties in Chiapas. Our results are in the same direction, but not the same magnitude, as those reported by Phiri (2013), who found a 15 percent positive and statistically significant effect of the cash transfer program in Zimbabwe.

The effects of direct support on consumption (and hence on measured poverty) are smaller than our a priori expectations, given the substantial size of the direct transfers that households report receiving, which are, on average, equivalent to 39 percent of the national poverty line. They are also in contrast with the impact of remittances received by households, whose poverty-reducing effect is twice as great even though the average payments are half as large.

Our results show that direct support payments have little effect on overall food consumption (which includes own-food consumption), and are not channeled into purchasing durable good. One other possibility is that the funds are invested. The EICV survey did ask recipients of direct support what they mainly spent the money on, but unfortunately did not collect any information on the amounts involved. The results are shown in Table 11, and show that just over a quarter of recipients say that the single biggest use of the funds was for farming, including livestock. By

Table A3
Treatment effects of received remittances on consumption and use value of durable assets.

	Consumption		Food consumption		Own-food consumption		Durable assets	
	Impact	p-value	Impact	p-value	Impact	p-value	Impact	p-value
Propensity score matching (5 matches)	0.235	0.000	0.108	0.000	0.114	0.000	0.272	0.000
Nearest neighbor matching (Mahalanobis)	0.323	0.000	0.165	0.000	0.137	0.000	0.370	0.000
Inverse probability weights	0.254	0.000	0.121	0.000	0.111	0.000	0.314	0.000
IPW with regression adjustment	0.284	0.000	0.135	0.000	0.105	0.000	0.335	0.000
Memo items	*000 RWF per adult equivalent p.a. in January 2014 prices*							
Consumption, recipients	505.6		198.1		56.9		23.9	
Consumption, non-recipients	267.3		132.8		49.4		9.5	
Observations	16,880		16,866		15,017		12,846	

Notes. Impact is measured for the log values of the variables. A dummy variable of received remittances is equal to one if received remittances are equal or more than the VUP-DS payment of the first percentile of VUP-DS beneficiaries. All measures of consumption and use value of durable assets refer to consumption or value per adult equivalent per year in thousands of RWF. Based on EICV 2014, undertaken in Rwanda in 2013/14.

far the most important stated use of the monies was for general consumption, “including food”. These findings are in line with what Aker, (2013) found in the Democratic Republic of Congo, where the UCT program was an efficient tool to improve outcomes of extremely vulnerable populations. In addition, Bazzi et al., (2012) found positive and statistically significant effects of a UCT program on household expenditure for smaller (but not bigger) households’ families in Indonesia.

In short, it appears that the VUP-DS program mainly allowed a significant fraction of older subsistence households to at least partly retire. The effect on the poverty rate – a traditional measure of the benefits of such programs – does not adequately capture the welfare effects.

9. Conclusions and policy implications

Unconditional cash transfers (UCTs) have been getting renewed attention as a tool for poverty reduction in developing countries, and governments have been increasing their investments in the same programs to alleviate poverty and improve the living conditions of vulnerable and poor populations. Given the relative simplicity of UCTs compared to other social safety net programs, their popularity worldwide, and their potential to reach a large number of recipients, an obvious question is how much a small, predictable sum of money paid monthly might lead to a reduction in poverty.

Rwanda, although devastated by the 1994 Genocide against the Tutsi, had recovered enough by 2009 to be able to introduce a system of UCTs, mainly through the “flagship” VUP-Direct Support program, which reaches an estimated 1.8 percent of the population. Our first contribution is to show that the program has indeed increased the consumption levels of beneficiaries (between three and five percent), and has reduced the proportion who are in poverty (between five and seven percentage points).

However, the measured effects are smaller than one might have expected given the size of the transfers. This is in part because many households respond by reducing food produced for home consumption, but in some cases because substantial numbers of beneficiaries take the opportunity to invest the proceeds (including in farming). Our second contribution is to show that the measured effects on consumption and poverty are well below those derived from an accounting approach.

Two broad implications are of interest. First, the accounting approach to measuring (or simulating) the effects of unconditional cash transfers, seriously overstates the effect on consumption and poverty. The benefits may be real, but are not reflected in reductions in observed poverty rates.

The second implication is that any assessment of the value of UCTs cannot be confined to an examination of overall spending or poverty. The transfers may have important long-run effects, raising future welfare via investment in livestock, farming, and other assets. Future surveys could usefully collect information that would allow researchers to quantify these effects more precisely. In partnership with development partners, efforts by the Government of Rwanda to collect detailed information on household spending on investment, and income-generating activities from received transfers, should allow researchers to shed more light on the effectiveness of UCT programs.

The program does reduce measured poverty for those it helps, but it only gets to a small fraction of the population, so there is unmet need still. To meet the goals of the VUP program, the Government and donors need to mobilize resources to cover all of the eligible population.

The program should be judged not just on its effects on poverty, but also on how it relieves old and infirm people of the need to work their fields so much. In addition, the program is based on the *ubudehe* classification, and a shortcoming is that the classification is not updated very frequently, and the targeting will become worse over time. Given that the *ubudehe* classification is an important tool for program targeting, it should be updated more frequently, for instance every three years. National poverty assessments that take place every three years would then allow a clear assessment of the targeting effectiveness, and impacts, of the UCT program. Last but not least, the experience of Rwanda from UCT program targeting mechanism and data collection to support impact evaluation could be beneficial to other countries with similar programs.

Contributions

DH and JH conceived, planned, and revised the paper; DH compiled and analyzed the data, including the development of STATA codes and wrote the paper content; JH edited the paper and validated the empirical results; JN and DMAH reviewed and provided feedback on the final paper.

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Data statement

The survey data used in this paper were collected by the National Institute of Statistics of Rwanda, and they are publicly available on its website (NISR, 2015), while the software programs used in the analysis are available on request.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A

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