Cash Transfers and Migration: Experimental Evidence from Comoros*

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Abstract

Using a multi-level randomized control trial in Comoros, we find that cash transfers targeted to poor households increased migration to Mayotte – the neighbouring and richer French Island. Between 2016 and 2018, treated households received up to US$320 in cash and as a result were 3 percentage points more likely to have a household member migrating to Mayotte (a statistically significant 38 percent increase relative to the control group). The effect increases over time, with some signals of positive effects during the second quarter of 2017, and then regular increases until the second quarter of 2018. The results seem to be driven by individuals with similar characteristics as control migrants, but residing in households with a medium propensity to have a migrant. Together, these findings are consistent with the existence of binding financial constrains to migration.

JEL Classification: J61, O12, O15, F22

Keywords: Migration, Cash Transfers, Financial Constraints, Comoros

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1 Introduction

The link between income and international migration is complex (McKenzie and Rapoport, 2007; Clemens et al., 2014; Dustmann and Okatenko, 2014). On one hand, migrants need to finance their journey to the destination country. This upfront cost can be very high (Adhikari and Gentilini, 2018), especially for illegal migrants, who often need to pay smugglers and face important risks (Chiswick, 1988; Hanson, 2006). Aspiring migrants facing budget and credit constraints are often unable to afford this cost (Bazzi, 2017), despite the very high expected returns to migration (Yang, 2008; McKenzie et al., 2010; Clemens, 2011; Gibson and McKenzie, 2012; Bryan et al., 2014). On the other hand, the opportunity cost of migration increases with income at home (Sjaastad, 1962), which itself depends on human capital. Human capital, in turn, not only affect the expected returns to emigration but also increases as a result of migration (Gibson and McKenzie, 2012). The sum of these opposite effects is theoretically ambiguous, and has been the subject of empirical investigation since the seminal work of Zelinsky (1971).

Researchers using macro-level data have identified a clear inverted U-shaped relationship between income and migration rates (Clemens et al., 2014; Dao et al., 2018). A few recent micro-level studies explored the relationship between income and migration by exploiting exogenous variation in cash transfer programmes (Adhikari and Gentilini, 2018). These micro-level studies offer mixed results, suggesting that different mechanisms are operating in different contexts. For example, the effect of Mexico's Opportunitades programme on migration to the U.S. seems to depend on which type of migration is considered: while Stecklov et al. (2005) find that the programme reduced overall migration to the U.S., Angelucci (2015) shows that the programme increased labor-induced migration to the U.S. by relieving the credit constraints of eligible households. In India, the NREGA cash-for-work programme reduced short-term migration by increasing the opportunity cost of migrating (Imbert and Papp, 2019).

Given the widespread promotion of cash transfers to foster development, understanding how income shocks affect migration is crucial, both for academics but also for

policy-makers who have preferences over migration outcomes. In this paper, we use a multi-level randomized control trial in Comoros to assess the effects of a nation-wide social protection program on international migration. The Comoros Social Safety Net (SSN) program was initiated in 2015 by the Government of Comoros and the World Bank. The main component of the SSN program provided temporary cash-for-work (CFW) opportunities to selected poor households.[1] Between the baseline and follow-up surveys, beneficiary households received up to the equivalent of US$320 in cash conditional on their participation to public work activities.

Migration patterns are salient in Comoros, especially towards Mayotte – the neighboring French Island. A mix of geo-cultural proximity and economic disparities causes many Comorians to migrate to Mayotte. While Mayotte is located about 70 kilometers to the South-East of Comoros, the GDP per capita in Mayotte is more than 10 times that of Comoros, and Mayotte has much better public infrastructures. Comorian migrants typically use small fishing boats called *kwassa-kwassa* to reach Mayotte. The journey is both risky and costly, especially since 1995, after France established visa requirements for Comorians traveling to Mayotte, shifting potential migrants towards smugglers and illegal sea routes. The cost of a trip is currently about US$500. An average of 1,000 Comorians are estimated to die trying to reach Mayotte each year (Senat, 2008).

Consistent with the existence of binding liquidity and credit constrains, we find that cash windfalls had a sizeable and positive impact on migration to Mayotte. The migration rate of beneficiary households increased by about 38 percent (from 7.8% to 10.8%). This effect increases over time, with some signals of positive effects during the second quarter of 2017, and then regular increases until the second quarter of 2018. We rule out alternative causes for the observed migration increase, including selective attrition and negative indirect treatment effects on control households.

[1] Cash-for-work programs are widespread in low-middle-income countries and have been carried out in a variety of settings, including Argentina, Ethiopia, India and South Africa, among others.

2 Background

2.1 Study context

The Comoro archipelago consists of four islands located in the Mozambique Channel, between Mozambique and Madagascar (see Figure 1). Three islands belong to the Union of Comoros (Comoros henceforth), a poor country with a population of 760,000 people. The remaining island, Mayotte, is French. Mayotte is situated about 70 kilometers to the South-East of Comoros. Mayotte has a population of 240,000 people. The GDP per capita in Mayotte is more than 10 times that of Comoros.

Strong ties unite the four islands. During the French colonisation, the islands were unified under a single administration and placed under the authority of the French colonial governor of Madagascar. People share a similar language, *Shikomori*, and are predominantly Muslim.[2] They also have similar social structures such as a matrilineal system shaped by the informal institution of the *Grand mariage* – a determinant of social status whose completion greatly increases one's standing in society.

However, during the 1974 independence referendum, Mayotte voted to remain politically a part of France while other islands voted for independence and formed the Comoros nation.[3] Since then, Mayotte has been continuously administered by France and even became a French overseas department in 2011.[4] Socioeconomic conditions have steadily improved in Mayotte while stagnating in neighbouring Comorian islands.

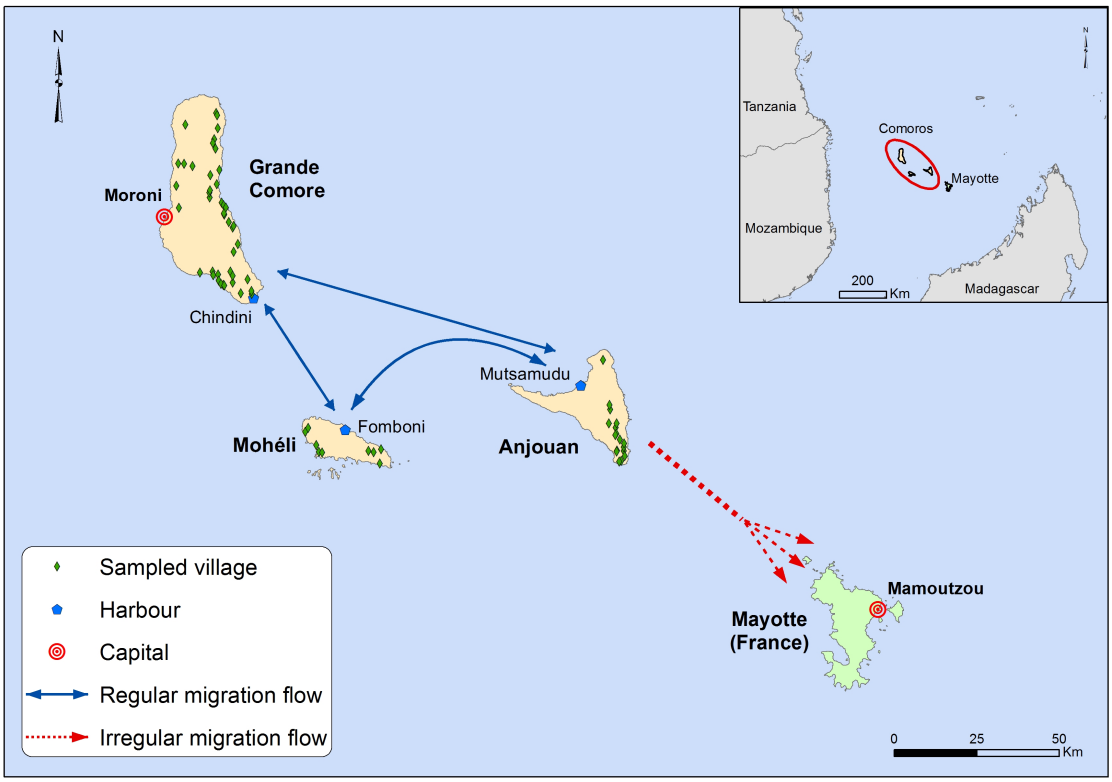
Since independence, Comoros has experienced recurring political crises and conflict between the islands. Comoros's low GNI per capita (US$770 in 2015) is stagnating because of relatively low GNI growth rates (between 2 and 3.5 percent) and high population growth (2.4 percent). Poverty is high with 48 percent of the population living with incomes below US$1.25 per day, and one-third of all children under five years

[2]Slightly different variants of *Shikomori* are found on each of the four islands (*Shingazidja*, *Shimwali*, *Shinzwani* and *Shimaore*) but people can easily communicate.

[3]See Blanchy (2002) for a discussion on why the people of Mayotte decided to remain French.

[4]The successive governments of Comoros all claimed to the island but France has vetoed several United Nations Security Council resolutions that would affirm Comorian sovereignty.

Figure 1: Migration route to Mayotte



Source: Author's elaboration

of age suffering from chronic malnutrition. Although Mayotte is the poorest French department, its US$10,600 GNI per capita in 2015 is extremely attractive relative to Comorian standards.

In 1995, in order to control migration of Comorians to Mayotte, France issued strict visa requirements. As a result, illegal sea routes and people smuggling emerged and the flow of Comorian migrants remains steady. The routes used by migrants are depicted in Figure 1. Migrants converge to the south east of Anjouan and then use *kwassa-kwassa* (small fishing boats) to reach Mayotte. Overall, the flow of Comorians has never stopped despite the continued efforts deployed by the French police to arrest illegal migrants.[5] It is estimated that 61 percent of Mayotte's population has a connection to Comoros with 42 percent born in Comoros and 19 percent having a Comorian mother (Marie et al., 2017).

[5]According to official statistics from the French administration, each year, about 20,000 illegal migrants (i.e. almost 10 percent of Mayotte's population) are deported to Comoros (Senat, 2008).

2.2 Intervention

The SSN program was initiated in 2015 by the Government of Comoros (Ministry of Health, Solidarity, Social Cohesion, and Gender Promotion and the Ministry of Labor) and the donors (World Bank, UNICEF). The main implementing agency was FADC (*Fonds d'Appui au Développement Communautaire*) – reframed as ANACEP (*Agence Nationale de Conception et d'Exécution de Projets*) in 2017. The objective was to improve poor communities' access to safety net and nutrition services, smooth consumption, and support the development of productive activities. Prior to running this SSN program, FADC had successfully implemented a variety of World Bank-supported projects, including similar cash-for-work programs.

The main component of the program provided cash-for-work (CFW) opportunities to poor households, i.e. cash transfers conditional on their participation in public works such as reforestation, water management, and terracing. From 2016 to 2018, households have been provided with an average 60 days of work per year at the daily wage rate of KMF 1,000 (approximately US$2.3).

According to the national distribution formula, Grande Comore should receive 45 percent of the program funds, while Anjouan should receive 42 percent and Mohéli 12 percent. Based on these percentages, FADC selected the poorest villages using the poverty map drawn up by the Comorian national institute of statistics (known as INSEED) in 2003/2004. A total of 69 rural villages were selected by FADC to receive the intervention.

3 Study design and data

3.1 Experimental design

The impact evaluation has been designed in collaboration with FADC based on the outcomes of an inception workshop organized in Moroni, in September 2015, and further discussions until February 2016. FADC and the authors agreed to evaluate the effects of the SSN program using a multi-level randomized control trial.

Within villages, the selection of beneficiaries relied on public lotteries organized by committees and FADC's staff. Note that eligible households were selected through a mix of self-selection and community targeting. Self-targeting was expected because of the labor requirement, the (non-monetary) front costs of applying, and the low wage rate for the public works. In addition, village committees, in collaboration with FADC's staff, applied specific selection criteria, such as the number of dependents in each household, the education level of the parents and the possession of land.[6] Based on these criteria, committee members pre-selected the poorest 60 percent of households.

At the cluster level, villages were randomly assigned to a low or high saturation version of the program in order to assess indirect effects. Specifically, in each village, 1/3 or 2/3 of the pre-selected households were randomly assigned to be beneficiaries. This means that overall 20 percent or 40 percent of eligible households were selected.[7]

3.2 Empirical strategy

The two levels of random assignment are core to the empirical strategy. Because of these random assignments, households and communities with different treatment conditions are similar (in expectation) in every respect except for their treatment. Any difference in outcome between treatment groups after the program can thus be attributed to the difference in treatment. Below we provide more details on how we estimate the direct, indirect and heterogeneous intention-to-treat (ITT) effects of the SSN program on migration.

3.2.1 Direct effects

First we estimate a regression equation of the following form to derive direct effects of the program:

[6]The methodology has been used in previous projects and improved over time by FADC.

[7]The evaluation design also had a gender component in which the gender of the worker was supposed to be randomly drawn within households. However, its implementation in the field was plagued with a variety of problems and we have not been able to leverage this level of randomization until now.

$$y_{iv} = \beta_0 + \beta_1 CFW_{iv} + \beta_2 X_{iv} + \varepsilon_{iv} \quad (1)$$

Where y_{iv} is the outcome of interest for household i in village v; CFW_{iv} is a dummy indicating whether an individual from household i in village v was employed in public works project or not; X_{iv} is a vector of imbalanced covariates at baseline; and ε_{iv} is the disturbance term for the regression. The direct effects of the program on the outcomes of beneficiaries are given by the coefficient β_1.

3.2.2 Indirect effects

Indirect average treatment effects (ITEs) of the SSN program are ascertained by comparing the outcomes of households in high intensity villages with those of households in low intensity villages. Specifically, we estimate an equation of the following form:

$$y_{iv} = \beta_0 + \beta_1 CFW_{iv} + \beta_2 P_{40v} + \beta_3 CFW_{iv} * P_{40v} + \beta_4 X_{iv} + \varepsilon_{iv} \quad (2)$$

Where y_{iv} is the outcome of interest for household i in village v; CFW_{iv} is a dummy indicating whether an individual from household i in village v was employed in public works or not; P_{40v} is a dummy variable at the village level indicating an assignment rate of 40% in village v; $CFW_{iv} * P_{40v}$ is thus a dummy for being assigned to treatment in a village with a rate of 40% assignment; X_{iv} is a vector of imbalanced covariates at baseline; and ε_{iv} is the disturbance term.

Equation 2 provides an estimation of ITE both on beneficiary and non-beneficiary households. ITE among non-beneficiary households are estimated by the parameter β_2, that is the effect of being assigned to the control group in a village where 40% of the eligible population was assigned to treatment, compared to being assigned to the control group in a village where only 20% of the eligible population was assigned to treatment. As for ITE among beneficiary households, this is given by $\beta_2 + \beta_3$, that is the effect of being assigned to treatment in a village where 40% of the eligible population was assigned to treatment, compared to being assigned to treatment in a village where

only 20% of the eligible population was assigned to treatment.

3.2.3 Heterogeneous effects

Finally, we estimate heterogeneous effects with an equation of the following form:

$$\begin{aligned} y_{iv} = \beta_0 + \beta_1 CFW_{iv} + \beta_2 CHARACTERISTIC_{iv} + \\ \beta_3 CFW_{iv} * CHARACTERISTIC_{iv} + \beta_4 X_{iv} + \varepsilon_{iv} \end{aligned} \tag{3}$$

Where y_{iv} is the outcome of interest for household i in village v; CFW_{iv} is a dummy indicating whether household i in village v was employed in public works or not; $CHARACTERISTIC_{iv}$ corresponds to the dimension of heterogeneity studied for household i in village v; $CFW_{iv} * CHARACTERISTIC_{iv}$ is their interaction; X_{iv} is a vector of imbalanced covariates at baseline; and ε_{iv} is the disturbance term. This equation tests whether the effects of the program is conditional on pre-specified characteristics of beneficiaries.

3.3 Sample

The sample is composed of the villages benefiting from the SSN program, with each village considered as statistical domains. Villages with population below 30 households were excluded from the experimental design highlighted in Section 3.1 because FADC explained the number of beneficiaries would have been too small to conduct the public works. In these villages, 100% of the eligible households participated in the public works. We are left with 62 villages, including 37 villages from Grande Comore, 16 villages from Anjouan and 9 villages from Moheli.

We performed power calculation exercises to determine the optimal number of households to include in the sample in order to measure both the impacts of CFW activities and minimize survey budget. In each village, we sampled 25 beneficiary households and 15 pre-selected but non-beneficiary households.[8] Each households

[8]Because we were interested to know whether the effect of the program varies according to the gender of the recipient we sampled more beneficiaries than non-beneficiaries. We also sampled 5 house-

within a given village and category had the same probability of being sampled.

3.4 Data collection

A baseline survey was conducted after randomization at the household level and before the start of CFW activities. The baseline survey took place in two phases (first in one third of the villages and then in the remaining two thirds) to mirror program implementation details:[9] (i) from July to September 2016 and (ii) from December 2016 to May 2017. A follow-up survey was conducted between July and September 2018, while treated households received between 3 and 7 rounds of CFW activities (equivalent to between US$140-320). INSEED, the national institute of statistics, was responsible for data collection and worked under the supervision of the authors. Surveys were administered on tablets, and data was automatically uploaded to an online server.

A qualitative survey is ongoing. While the quantitative survey can provide rigorous evidence of impact, it is limited in its explanatory power to determine the mechanisms through which that impact occurred. In particular, the qualitative component will help shed light on the various channels through which CFW activities might affect socio-economic outcomes for the poor. These interviews are currently conducted with a broad range of actors, including (i) participants and non-participants in project activities, (ii) government officials and local community leaders, and (iii) NGOs and local firms in charge of the execution of CFW activities.

3.5 Balance and attrition

Table 1 summarizes key baseline variables and tests for balance between treatment and control groups. The first fours columns report subsample means and standard deviation, and the last two columns report the difference and associated pvalue.

Only one of the 19 variables tested has imbalance at the 10% significance level.

holds which enrolled to benefit from the CFW activities but were declared ineligible, and 5 households which did not enroll. These aspects will be examined in separate output(s).

[9]The sampling frame described above required the completion of the targeting process, which was implemented by FADC in two phases due to capacity constraints.

Household heads assigned to treatment are slightly less likely to have completed primary school only (19% vs. 22%). This difference, while significant, is not too worrying because it concerns only one variable and because is is relatively small in size.

Table 1: Household characteristics at baseline

	Control		Treatment			
	Mean	SD	Mean	SD	Diff	p-value
Household size	6.55	2.80	6.57	2.82	-0.01	0.91
Consumption (log PEA)	6.48	1.01	6.45	0.96	0.03	0.52
Has a bank account	0.28	0.45	0.27	0.45	0.01	0.64
Has an income generating activity (other than agriculture)	0.48	0.50	0.45	0.50	0.03	0.17
Owns fields	0.76	0.43	0.75	0.43	0.01	0.72
Livestock (tropical unit)	0.49	0.93	0.52	0.99	-0.03	0.48
Has electricity	0.59	0.49	0.60	0.49	-0.01	0.50
Has a private water access	0.63	0.48	0.63	0.48	0.01	0.74
Head is male	0.77	0.42	0.76	0.43	0.01	0.59
Head age	48.66	16.03	48.34	15.20	0.32	0.63
Head education						
Did not complete primary	0.56	0.50	0.58	0.49	-0.02	0.39
Primary	0.22	0.41	0.19	0.39	0.03	0.06
Secondary	0.18	0.38	0.19	0.39	-0.01	0.48
Tertiary	0.04	0.20	0.04	0.20	-0.00	0.83
Willingness to migrate to Mayotte	0.21	0.41	0.23	0.42	-0.02	0.31
Migrant network in Mayotte	0.13	0.34	0.14	0.35	-0.01	0.49
Island of residence						
Ngazidja	0.58	0.49	0.58	0.49	0.00	0.84
Ndzuani	0.27	0.44	0.28	0.45	-0.01	0.58
Mwali	0.15	0.36	0.15	0.35	0.01	0.67
Observations	900	900	1372	1372	2272	2272

Notes: This table reports subsample means with standard deviations. The last column reports the pvalue of a ttest of mean equality across subsamples. PEA denotes per adult equivalent.

Table 2: Differential attrition test

	Control		Treatment			
	Mean	SD	Mean	SD	Diff	p-value
Attrition rate	0.044	0.206	0.037	0.189	0.007	0.39
Observations	900	900	1372	1372	2272	2272

Notes: This table displays the difference in mean attrition between treatment and control groups.

Attrition from the follow-up survey was low (about 4 percent of the baseline sample) and balanced across treatment and control groups, as shown in Table 2.

4 Impacts of the SSN program

4.1 Program take-up

In Table 3, we check that households assigned to treatment are indeed more likely to perform CFW activities and whether they see an improvement of their levels of employment and income. On the one hand, access to CFW opportunities should directly increase employment and income of beneficiaries. On the other hand, substitution effects could undermine these direct effects (e.g. if beneficiaries give up on other profitable activities because of the labor requirement of the program).[10]

Table 3: Treatment effects on labor market outcomes

	Employment			Income		
	(1) CFW	(2) Total (excl. CFW)	(3) Total (incl. CFW)	(4) CFW	(5) Total (excl. CFW)	(6) Total (incl. CFW)
Panel A						
Treatment	4.984***	0.742	5.726***	1.285***	-0.216*	1.069***
	(0.317)	(1.621)	(1.667)	(0.074)	(0.114)	(0.137)
Extended controls	No	No	No	No	No	No
Island FE	No	No	No	No	No	No
Panel B						
Treatment	4.872***	0.515	5.388***	1.255***	-0.231**	1.024***
	(0.315)	(1.549)	(1.593)	(0.074)	(0.109)	(0.131)
Extended controls	Yes	Yes	Yes	Yes	Yes	Yes
Island FE	No	No	No	No	No	No
Panel C						
Treatment	4.862***	0.518	5.380***	1.253***	-0.233**	1.020***
	(0.313)	(1.518)	(1.564)	(0.073)	(0.107)	(0.129)
Extended controls	Yes	Yes	Yes	Yes	Yes	Yes
Island FE	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	1.881	51.924	53.805	0.489	3.098	3.587
Observations	2181	2181	2181	2181	2181	2181

Notes: Employment variables are expressed as number of days worked. Total employment includes farming, livestock rearing, fishing, and other activities (and CFW if specified in the column header). An inverse hyperbolic sine (IHS) transformation is applied to income variables. All estimates control for unbalanced covariates. Standard errors in parentheses are clustered at the household level. *** p<0.01, ** p<0.05, * p<0.1.

In columns 1 and 4, we see that the randomization is effective at increasing treated households participation in CFW activities. However, the control group seems to be

[10]Employment and incomes are measured for all individuals and then aggregated at the household level. A 30 days recall period has been used in order to limit the scope for measurement errors. Note however that CFW income (column 4) is derived from CFW employment (column 1) as no payments occurred in the 30 days preceding the interviews (it should therefore be seen as expected or deferred income).

contaminated since control households spend an average of 1.88 days in public works. When income derived from CFW activities are excluded, treated households earn a lower total income than their control counterparts (column 5), which is consistent with the existence of substitution effects. However, the total treatment effect on employment and income is substantial and positive (columns 3 and 6), so that overall the program can be considered as a positive income shock for treated households. The estimates are similar when extended controls and island fixed effects are included (panels B and C).

One partial explanation for the contamination of the control group is related to the formal and informal replacements of beneficiaries dropping out. It could also be due to measurement error, or to a different definition of households in the survey relative to the definition used during project targeting. Finally, there were some cases where two different household members registered the same household during the targeting (either inadvertently or in an attempt to increase the probability of the household to get selected). As mentioned in Section 3.2, we use ITT estimates in order to limit problems.

4.2 Impact on migration

The main results of the paper are presented in Table 4, where we report the ITT effect of the SSN program on migration to Mayotte. We were concerned about the sensitivity of the topic because migration of Comorians to Mayotte is almost completely illegal, especially for the study population which is poorer than the average Comorian and has a tiny probability of getting visas. In addition, many people have died in the last few decades trying to reach Mayotte and development agencies are increasingly concerned by the phenomenon. In terms of identification, experimenter demand effects and socially desirable answers could induce beneficiary households to be more reluctant to reveal they sent migrants to Mayotte, which would bias the treatment effects downward. In order to avoid respondents discomfort and biased responses, we collected information as indirectly as possible, by leveraging data on household composition collected at baseline. In particular, our main measure of migration relies on questions asking whether each baseline household member is still residing in the household at

follow-up, and if not, where he is currently residing with Mayotte as one of the choices. Because it does not make salient that the purpose of the questions is to assess migration to Mayotte, we believe that the risks of respondents unease and biased responses are limited.

Table 4: Treatment effects on migration to Mayotte

	Migration (excl. returns)			Migration (incl. returns)		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.030**	0.027**	0.027**	0.036**	0.032**	0.032**
	(0.013)	(0.012)	(0.012)	(0.015)	(0.015)	(0.015)
Extended controls	No	Yes	Yes	No	Yes	Yes
Island FE	No	No	Yes	No	No	Yes
Control mean	0.078	0.078	0.078	0.128	0.128	0.128
Observations	2181	2181	2181	2181	2181	2181

Notes: This table reports LPM estimates of treatment effects. The dependent variable in columns 1 to 3 is a dummy equal to one if at least one household member migrated to Mayotte after the baseline survey and is still in Mayotte during the follow-up survey. In columns 4 to 6, the dependent variable also equals one if at least one household member migrated to Mayotte after the baseline survey but returned (voluntarily or not). All estimates control for unbalanced covariates. Extended controls include the following variables (measured at baseline): household willingness to migrate; network in Mayotte; household head's gender, age, and schooling; household size, consumption, and livestock; dummy variables equal to one if the household has a bank account, income-generation activities (other than agriculture), fields, electricity, and a private water access. Standard errors in parentheses are clustered at the household level. *** p<0.01, ** p<0.05, * p<0.1.

Because the French police expels a large number of illegal Comorians, migration is often short-term.[11] Therefore, we also collected information on return migrants, by inquiring whether any household member at follow-up took a *Kwassa* for Mayotte in the last 24 months. This measure is not without caveats and could bias the estimates, given that (i) it is more direct and thus exposed to the bias mentioned above, (ii) the 24 months recall period may include pre-program migrations because of program's progressive roll-out, and (iii) it does not inquire about household members who have died (some of which may have died en route to Mayotte), or household members who have left the household and are not currently in Mayotte, but could still have been in

[11]Each year, about 20,000 migrants are deported to Comoros (Senat, 2008). This corresponds to roughly 8 percent of Mayotte population or 2 percent of Comoros population.

Mayotte in between.[12]

We find that the program has a sizeable and positive impact on migration to Mayotte. Column 1 shows that the treatment effect is 3 percentage points (significant at the 5% level), which represents a 38 percent increase relative to the control group. Including returnees does not alter the results. The treatment effect is now 3.6 percentage points, equivalent to a 28 percent increase relative to the control group. Both coefficients are stable when extended controls and island fixed effects are included (columns 2, 3, 5 and 6). These results are consistent with the existence of high migration costs and financial constraints.

4.3 Timing of the impact

When respondents reported a migrant, we further inquired about the month and year of migration. This retrospective data allows us to explore temporal dynamics in the treatment effect. Figure 2 shows the evolution of the treatment effect over time. For each quarter between July 2016 and September 2018, we report the treatment effect and the migration rate in the control group.[13] Consistent with the presence of liquidity and credit constrains, the treatment effect increases over time. We see some signals of positive effect during the second quarter of 2017, and then regular increases until the second quarter of 2018. Figure 2 also represents the timing of cash transfers (measured using administrative data). The correlation with treatment effects seems rather strong: increases in treatment effect follow closely the disbursement of cash transfers.

In order to have a better understanding of these dynamics, Table 5 investigates in a more systematic way the timing of cash transfers and migration. We assemble a panel of individuals with detailed information on migration history and cash transfers received. We are particularly interested to check (i) whether migration decisions at time t are explained by the amount of cash received at time t vs. cash received at time

[12]Comorian migrants are always deported to Anjouan (Mayotte's closest neighbor), even though they are from Grande Comore or Moheli. Then, they either return to their island of origin, settle in a new location, or try to get back to Mayotte.

[13]Some respondents only recalled the year. In such cases, we generate a random month in order to avoid power loses. However, the dynamic of the treatment effect is the same if we exclude households with missing migration month data.

t-1 vs. total cash received pre-*t*, and (ii) whether the impact of the cash received at time *t* is conditional on the total amount of cash received beforehand.

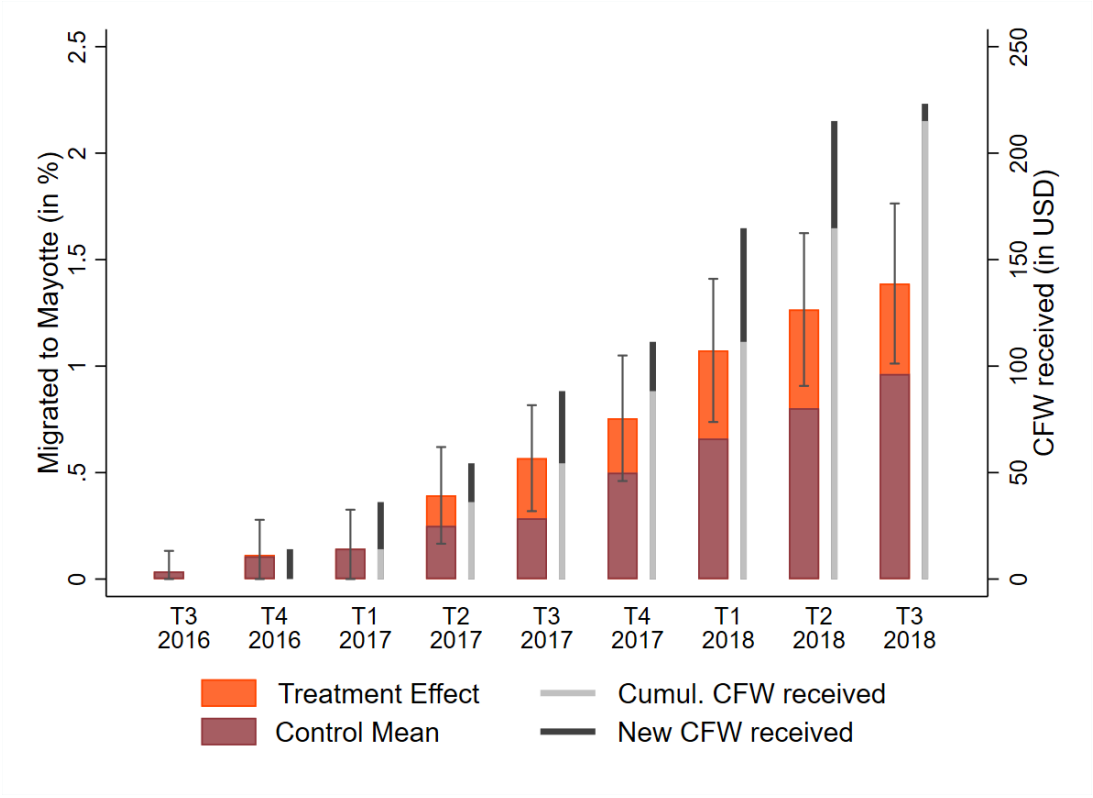
Table 5: Timing of cash transfers and migration

	Migration t (excl. returns)			Migration t (incl. returns)		
	(1)	(2)	(3)	(4)	(5)	(6)
Cash t	0.0013***	0.0015***	0.0007	0.0018***	0.0018***	0.0010*
	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)
Cash Tot. t-1	0.0000		-0.0001***	0.0000		-0.0001
	(0.000)		(0.000)	(0.000)		(0.000)
Cash t-1		-0.0006			-0.0002	
		(0.001)			(0.001)	
Cash Tot. t-2		0.0001			0.0000	
		(0.000)			(0.000)	
Cash t **x** Cash Tot. t-1			0.0004**			0.0005**
			(0.000)			(0.000)
Migration t-1	0.997***	0.997***	0.997***	0.996***	0.995***	0.996***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Extended controls	Yes	Yes	Yes	Yes	Yes	Yes
Island FE	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.006	0.006	0.006	0.010	0.010	0.010
Observations	113752	99533	113752	113752	99533	113752

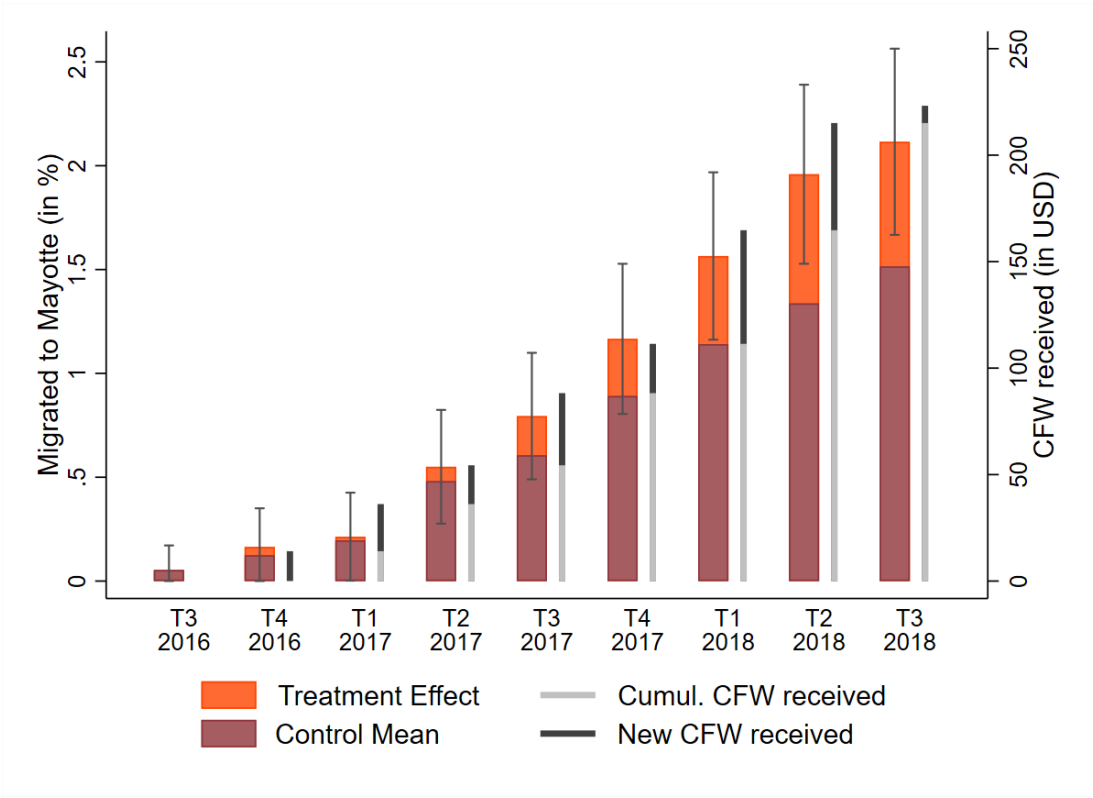
Notes: All estimates control for unbalanced covariates. Standard errors in parentheses are clustered at the household level. *** p<0.01, ** p<0.05, * p<0.1.

Most of the impact seems to come from cash received at time *t* (column 1), meaning that individuals react rather quickly to cash transfers. In contrast, cash transfers received at time *t*-1 do not seem to make much difference. However, it is interesting to see in column 3 that the impact of cash received at time *t* is actually conditional on the total amount received beforehand. Overall, it seems individuals migrate when they receive cash at time *t* conditional on having accumulated enough liquidity in the previous periods. The inclusion of returnees does not alter the results (columns 4-6). Again, this is very consistent with the presence of liquidity and credit constraints.

Figure 2: Treatment effect over time



(a) Excluding returnees



(b) Including returnees

4.4 Heterogeneous effects

We now examine heterogeneity in the effects by baseline characteristics. In Table 6, we analyze whether the effect vary with (i) the willingness to migrate, (ii) the number of rounds of CFW received, (iii) the number of working age adults in the household, (iv) the total consumption per adult equivalent, and (v) the schooling of the household head. Because of the financial constraints highlighted above, we expect the effect to increase with household willingness to migrate and the number of CFW received, and decrease with consumption. The mediating effect of the number of working age adults is more ambiguous. The more working age adults in the household, the less binding the labor requirement of CFW opportunities. However, the marginal effect of cash received may be smaller in larger households.

Table 6: Heteregeneous Effects

	(1)	(2)	(3)	(4)	(5)
	Mig	Mig	Mig	Mig	Mig
Treatment	0.020	-0.040	0.026	0.052	0.028*
	(0.013)	(0.042)	(0.024)	(0.069)	(0.015)
Treatment x Willing to migrate	0.029				
	(0.033)				
Treatment x CFW rounds (N)		0.014			
		(0.009)			
Treatment x Working age adults (N)			-0.001		
			(0.008)		
Treatment x Consumption				-0.002	
				(0.005)	
Treatment x Schooling					-0.003
					(0.013)
Extended controls	Yes	Yes	Yes	Yes	Yes
Island FE	Yes	Yes	Yes	Yes	Yes
Control mean	0.078	0.078	0.078	0.078	0.078
Observations	2181	2181	2181	2181	2181

Notes: All estimates control for unbalanced covariates. Standard errors in parentheses are clustered at the household level. *** p<0.01, ** p<0.05, * p<0.1.

While no interaction term is significant at conventional significance levels, it seems that the effect is stronger for households willing to migrate and receiving more CFW rounds. We investigate this pattern more comprehensively by implementing the endogenous stratification method, a three-step procedure which allows to assess how different groups are affected by the treatment. First, using control households, we regress the outcome variable (migration to Mayotte) on the baseline characteristics

highlighted in Table 1. We then use the fitted coefficients to predict migration in the absence of treatment for both the treatment and control groups. Finally, we split the households into different groups on the basis of their predicted migration values and estimate treatment effects across these groups.[14]

The results are presented in Table 7. Figure 3 shows migration rates by experimental conditions for each groups of households and individuals.

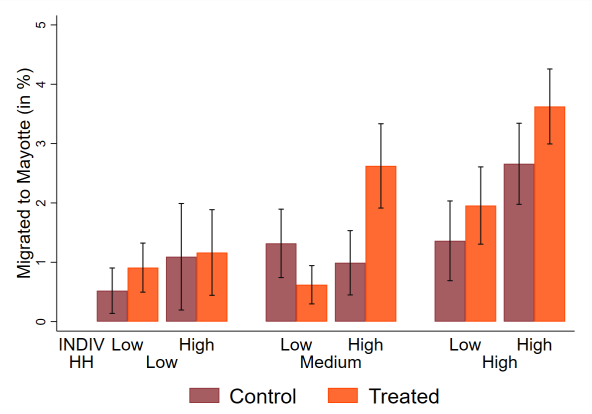
Table 7: Endogenous stratification

	Household		Individual			
	Migration (excl. returns)	Migration (incl. returns)	Migration (excl. returns)		Migration (incl. returns)	
	(1)	(2)	(3)	(4)	(5)	(6)
Low predicted migration						
Treatment	0.014	-0.002	0.000	0.005	0.003	0.006
SE	(0.017)	(0.024)	(0.002)	(0.003)	(0.003)	(0.004)
Control mean	0.024	0.076	0.011	0.007	0.015	0.009
Medium predicted migration						
Treatment	0.034	0.054		-0.002		0.001
SE	(0.021)	(0.029)		(0.005)		(0.004)
Control mean	0.061	0.080		0.014		0.022
High predicted migration						
Treatment	0.036	0.052	0.011	0.014	0.014	0.018
SE	(0.032)	(0.034)	(0.005)	(0.006)	(0.005)	(0.007)
Control mean	0.151	0.230	0.019	0.023	0.033	0.040
Number of groups	3	3	2	3	2	3
Predictors:						
Extended controls	Yes	Yes	No	No	No	No
Island FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	No	Yes	Yes	Yes	Yes
Observations	2181	2181	14288	14288	14288	14288

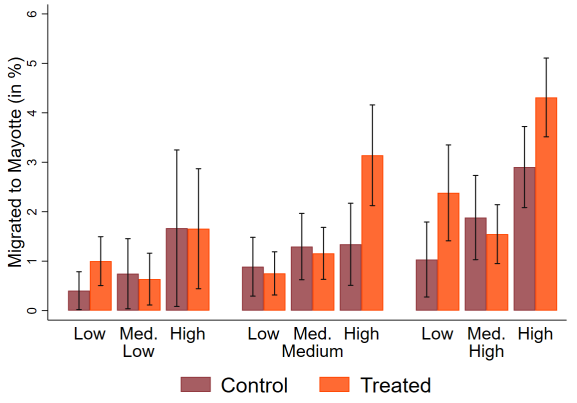
Notes: Using the leave-one-out estimation procedure. Standard errors in parentheses are bootstrapped (500 repetitions). *** p<0.01, ** p<0.05, * p<0.1.

[14]The fitted model is estimated excluding the observation itself to avoid bias (Abadie et al., 2018). We used the *estrat* Stata command with the leave-one-out option which automates the procedures.

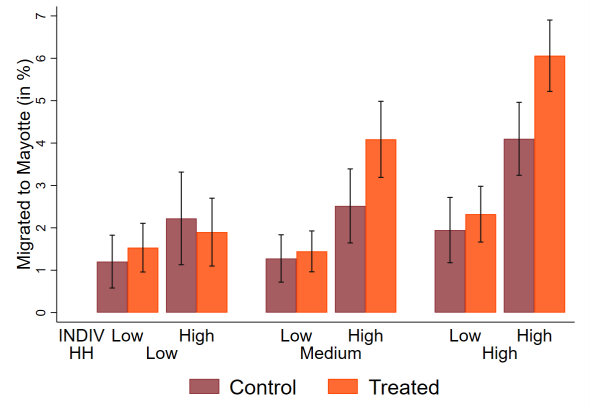
Figure 3: Endogenous stratification



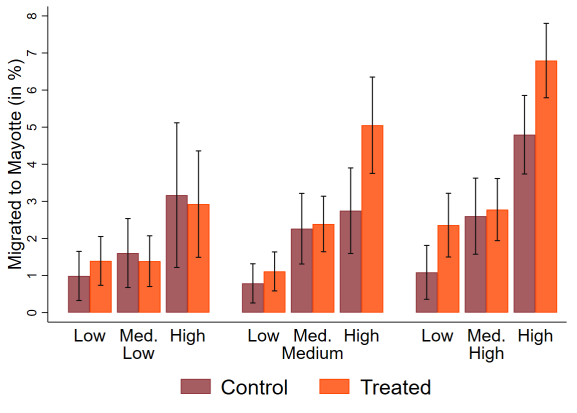
(a) 2 INDIV groups
(excl. returns)



(b) 3 INDIV groups
(excl. returns)



(c) 2 INDIV groups
(incl.returns)



(d) 3 INDIV groups
(incl.returns)

5 Threats to our interpretation

The estimates in Table 4 are consistent with the idea that cash transfers relax the financial constraints faced by beneficiary households, and therefore generates more migration to Mayotte. However, this interpretation is exposed to various threats that would produce a similar pattern in the data, in particular selective attrition and negative indirect effects on control households.

5.1 Selective attrition

It could be the case that attrition is explained by households migrating as a whole to Mayotte. This is especially worrying if control households are more affected than treated households by whole migration, as the estimated impact of the program on migration would be biased upward. A few observations helps to mitigate this concern. First, as we saw in Table 2, the attrition rate is very low and similar across experimental groups. Furthermore, (the first) qualitative interviews indicate that whole household migration is uncommon in Comoros. Households typically send one migrant to Mayotte, sometimes two (e.g. when an individual migrates with his child). However, respondents could not list examples of whole household migration, and argue that these are unlikely because of the high migration costs and harsh living conditions of Comorian migrants in Mayotte. Finally, even if we considered an unlikely scenario in which all attritors migrated to Mayotte, we would still observe the positive impact on migration.

A similar issue is related to household dissolution and migration. There is a pervasive association between the migration of a household member and household dissolution (Bertoli and Murard, 2019). In particular, the migration of an individual increases the probability that his household of origin dissolves subsequently. Because the program was targeted at the household level, beneficiary households may have an incentive to preserve the living arrangement after the migration of a household member, thus being less likely to dissolve. Yet, this is not what is observed in the data. In Table

8, we check whether beneficiary households are less likely to dissolve by analyzing attrition reasons given by enumerators. While household dissolution is the most common reason for attrition, it affects similarly treated and control households. About two percent of households in both experimental groups could not be followed-up because they dissolved.

Table 8: Attrition reasons

	Control		Treatment			
	Mean	SD	Mean	SD	Diff	p-value
Attrition reason						
Duplicate household	0.002	0.047	0.002	0.047	0.000	0.99
Refusal	0.007	0.081	0.004	0.066	0.002	0.46
Absent	0.009	0.094	0.009	0.093	0.000	0.97
Dissolved household	0.020	0.140	0.019	0.136	0.001	0.86
Too sick	0.001	0.033	0.001	0.027	0.000	0.76
Other	0.006	0.074	0.002	0.047	0.003	0.19
Observations	900	900	1372	1372	2272	2272

Notes: This table displays difference in mean attrition rates between treatment and control groups by attrition reasons.

Two ingredients of the project implementation may explain why beneficiary households are not less likely to dissolve. First, payments were made to individuals performing the work rather than to household heads. Second, formal and informal arrangements to replace workers were possible both within and (at least to some extent) across households. Formally, the implementation manual of the project stated that replacement requests could be introduced to village committee members and then to FADC staffs. Drop-out workers should be replaced by another adult household member, but in practice, the exact initial household composition was unknown to FADC, meaning that the choice of the replacement could incorporate endogenous household changes. The (first) qualitative interviews with beneficiaries further reveal that informal replacements by extended family members or relatives were quite common. Taken together, these observations support the idea that incentives for beneficiaries to preserve the household structure were likely weak in practice.

Another way to test whether the observed impact is due to selective attrition is to check other migration patterns. If relaxed financial constraints are the main reason for the observed impact on migration to Mayotte, we should not detect similar effects on

migration to cheaper, previously unconstrained destinations. Reassuringly, in Table 9, we see no significant impact on domestic migration. We do not observe effects on migration to mainland France either, most likely because the binding constraint for this destination is administrative rather than financial for the most part.

Table 9: Other migration patterns

	(1) Domestic Mig. (intra-island)	(2) Domestic Mig. (inter-island)	(3) Migration France	(4) Migration Other
Panel A				
Treatment	-0.022	0.006	0.001	0.002
	(0.018)	(0.010)	(0.007)	(0.008)
Extended controls	No	No	No	No
Island FE	No	No	No	No
Panel B				
Treatment	-0.020	0.006	0.000	0.002
	(0.018)	(0.010)	(0.007)	(0.007)
Extended controls	Yes	Yes	Yes	Yes
Island FE	No	No	No	No
Panel C				
Treatment	-0.021	0.006	0.000	0.002
	(0.018)	(0.010)	(0.007)	(0.007)
Extended controls	Yes	Yes	Yes	Yes
Island FE	Yes	Yes	Yes	Yes
Control mean	0.236	0.057	0.029	0.030
Observations	2181	2181	2181	2181

Notes: All estimates control for unbalanced covariates. Standard errors in parentheses are clustered at the household level. *** p<0.01, ** p<0.05, * p<0.1.

5.2 Indirect effects

A number of recent studies, e.g. Beegle et al. (2017) or Angelucci and De Giorgi (2009), have highlighted the importance to estimate not just direct effects of anti-poverty programs, but also their indirect effects.

CFW programs such as the SSN program can influence outcomes in multiple indirect ways. First, infrastructures created through public works can affect the local economy as infrastructures are themselves productive assets, which can generate higher returns to households with productive assets, or community-wide spillover effects. Second, non-beneficiaries can be positively affected by the program through inter-household redistribution and informal insurance networks. Finally, the program can generate externalities on non-beneficiaries within communities through local markets

effects. The increased demand for goods generated by new income streams may affect prices, and therefore the real income of buyers and sellers on local markets. The CFW may also affect wages by diminishing labor supply.

Indirect effects could bias the coefficients estimated in Table 4 if they affect treated and control households in a different manner. For instance, control households could benefit indirectly from the program through solidarity or redistribution norms, or be able to lend money to beneficiaries to fund migration costs. In contrast, they could also be hurt by prices spikes or increased competition for scarce investment opportunities. Table 10 reports the sign and magnitude of indirect effects for both experimental groups. We see no evidence of significant indirect treatment effects. If anything, these affects are likely small and similar across treatment and control groups.

Table 10: Indirect treatment effects

	Migration (excl. returns)			Migration (incl. returns)		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.033	0.029	0.029*	0.032	0.027	0.027
	(0.022)	(0.021)	(0.017)	(0.029)	(0.028)	(0.021)
40% villages (β_2)	-0.001	-0.007	-0.009	-0.006	-0.013	-0.016
	(0.023)	(0.019)	(0.018)	(0.028)	(0.025)	(0.023)
Treatment x 40% villages (β_3)	-0.006	-0.003	-0.003	0.009	0.011	0.011
	(0.026)	(0.025)	(0.024)	(0.036)	(0.036)	(0.030)
$\beta_2 + \beta_3$	-0.007	-0.010	-0.012	0.002	-0.002	-0.005
	(0.024)	(0.019)	(0.016)	(0.031)	(0.022)	(0.020)
Extended controls	No	Yes	Yes	No	Yes	Yes
Island FE	No	No	Yes	No	No	Yes
Control mean (in 20% villages)	0.079	0.079	0.079	0.131	0.131	0.131
Observations	2181	2181	2181	2181	2181	2181

Notes: All estimates control for unbalanced covariates. Standard errors in parentheses are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

6 Conclusion

In this paper, we rely on a multi-level randomized control trial in Comoros to assess the effects of a nation-wide social protection program on international migration. Between the baseline and follow-up surveys, beneficiary households received up to the equivalent of US$320 in cash conditional on their participation to public work activi-

ties. Consistent with the existence of binding liquidity and credit constrains, we find that cash windfalls had a sizeable and positive impact on migration to Mayotte. The migration rate of beneficiary households increased by about 38 percent (from 7.8% to 10.8%). This effect increases over time, with some signals of positive effects during the second quarter of 2017, and then regular increases until the second quarter of 2018. We rule out alternative causes for the observed migration increase, including selective attrition and negative indirect treatment effects on control households.

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