THE IMPACT OF A CONDITIONAL CASH TRANSFER PROGRAMME ON CONSUMPTION IN COLOMBIA

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IFS, UCL, NBER and CEPR

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1. Introduction

In this report, we assess the short-term impact of the Colombian welfare programme *Familias en Acción* on consumption and its components. *Familias en Acción* (FA) is a conditional cash programme run by the Colombian government with the purpose of fostering the accumulation of human capital. The programme, inspired by the Mexican PROGRESA, has three main components: health, nutrition and education, and is targeted at the poorest 20% of households in selected areas in rural Colombia. The implementation of the programme began in 2002, and it was fully in operation in all targeted (treatment) communities, 627 in total, by 2003. In 2004, around 340,000 households were benefiting from the programme. The programme was financed with a loan from the World Bank and the Inter-American Development Bank.

The programme provides different grants conditional on specific forms of behaviour. In particular beneficiary households receive a 'nutritional' worth around 15US$ per month if they have children aged 0 to 5 and if the mothers participate into the health component of the programme (which consists of growth and development checkups for children, a vaccination program and some 'classes' on hygiene, diet, contraception for participating mothers). Moreover, mothers receive a payment of around 5.5US$ for each child enrolled and attending regularly primary school and of 11US$ for each child in secondary school.

The results summarized here are part of a large evaluation of the effects of *Familias en Acción* whose detailed results can be found in Attanasio et al (2005). The methodology used in this evaluation is based on the comparison of the outcome of interest, in this case consumption, between households living in towns where the programme was operating and households living in towns where the programme was not operating. As the allocation of the programme across towns was not random, the comparison is not straightforward. In what follows, we combine the use of difference in difference methods with the approach of controlling for observable differences at the individual and town level that could explain differential effects. We discuss the methodological issues in Section 2.

The sample used in the evaluation and in this report is made of approximately 11,500 households living in 122 towns, 57 of which were targeted by the program and 65 were

not. Households in the sample were interviewed twice: once in 2002 before the start of the programme and once in 2003, after the programme had operated for a year. The sample is described in Section 3. The same Section also contains some descriptive statistics on consumption patterns before the start of the programme.

In general, one would expect the program to have a positive effect on total consumption expenditure, as it presumably increases household disposable income. However, the effect might be more subtle than one would immediately think. First, disposable income does not necessarily increase by the same amount of the grant received if the conditionalities imposed by the programme reduce some income components (such as, for instance, child labour). Second, it is conceivable that not all the grant is consumed and part of it is saved, used to reduce current debt, or invested in productive activities.

In addition to the effect on total consumption expenditure we are also interested in its effect on the composition of consumption. As food constitute by a long margin the largest component of consumption for beneficiary households, the analysis of its components (and of the total) is particularly important. This aspect is of crucial importance as one of the stated aims of the programme is to improve the nutritional status of beneficiary households and, in particular, of the children living in these households. Evidence on expenditure on various food items would constitute the first necessary step to check whether this aim is achieved. Our results based on consumption expenditure can also be compared, if nothing else to check their consistency, with the results of the nutrition component that look both at objective outcomes (such as the weight and height of children) and at the food intakes of children in the week preceding the interview.

Checking what components of consumption increase and by how much also allows us to check indirectly who (and to what extent) in the family appropriates the benefits of the programme and if the programme reaches its stated aims of improving the nutritional status of beneficiaries. Some goods (such as alcohol and tobacco) are clearly consumed by adults while others (such as education or related expenses such as cloth and footwear for children) are consumed by children. Assessing the impact of the program on these specific goods will allow us to establish whether adults capture some of the benefits of the programme.

Section 4 presents the results. For total consumption and food consumption, we present our estimates of the effect of the programme using different approaches. As the results are not significantly different, when reporting the results on other components of consumption, we focus only on one set of results. Section 5 concludes.

2. Methodological Issues

2.1 Generalities

In order to estimate the impact of the programme on household consumption, or any other variable, one would like to observe average consumption outcomes in treatment areas both with and without the programme. The difference between the two would be entirely attributable to the programme, and the parameter of interest, Δ, would be estimated as

$$\Delta = E(Y_{1,A} - Y_{0,A} \mid T = 1) \tag{0.1}$$

where $Y_{j,k}$ is the outcome of interest and the two indexes denote whether a certain individual undergoes treatment (*j=1*) or not (*j=0*) and whether the observation is observed before the start of the programme (*k=B*) or after (*k=A*). *T=0(1)* denotes control (treatment) areas and *E* denotes the expected value. The standard problem in evaluation is that one does not observe the outcome of interest without treatment in treatment areas and therefore it is not possible to compute the second term on the right-hand side of (0.1). The approach to this problem used in much of the literature in such a situation is to use control areas to estimate this counterfactual. One could thus begin by comparing *post-programme* consumption outcomes across treatment and control areas, conditional on a range of observed characteristics that are likely to affect consumption outcomes, X.

$$\Delta = E(Y_{1,A} \mid T = 1, X) - E(Y_{0,A} \mid T = 0, X) \tag{0.2}$$

(0.2) yields an unbiased estimate of the programme impact under the assumption that conditional on observed characteristics *X*, there are no unobserved factors, *u* differentially affecting consumption in treatment and control areas.

$$E(Y_{1,A} - Y_{0,A} \mid T, X, u) = E(Y_{1,A} - Y_{0,A} \mid T, X) \tag{0.3}$$

$$\text{i.e. } u \perp T \mid X$$

This implies that the difference estimated in (0.2) is entirely attributable to the programme. (0.3) is of course a potentially strong assumption. No matter how similar treatment and control areas are on the basis of observed characteristics, it is always of concern that there are non-programme related unobserved differences across areas that may also affect consumption outcomes. Thus one risks confounding programme effects with non-programme related unobserved factors that affect household consumption, in which case a comparison of post-programme consumption outcomes only can be very misleading.

If data on the outcomes of interest are observed prior to the start of the programme in both treatment and control areas, one can estimate the impact of the programme under less stringent assumptions. The first of these is that, in the absence of the programme, there are common time effects across treatment and control areas, i.e.

$$E(Y_{0,A} - Y_{0,B} \mid T = 1) = E(Y_{0,A} - Y_{0,B} \mid T = 0) \tag{0.4}$$

This allows us to estimate the effect of the programme on consumption as

$$\begin{aligned} \Delta &= E(Y_{1,A} \mid T = 1, X) - E(Y_{0,B} \mid T = 1, X) + E(Y_{0,B} \mid T = 1, X) - E(Y_{0,A} \mid T = 1, X) \\ &= \underbrace{\left[E(Y_{1,A} \mid T = 1, X) - E(Y_{0,A} \mid T = 0, X) \right]}_{A'} - \underbrace{\left[E(Y_{0,B} \mid T = 1, X) - E(Y_{0,B} \mid T = 0, X) \right]}_{B'} \end{aligned} \tag{0.5}$$

The second assumption underlying this estimator is that any difference in consumption across treatment and control areas due to unobserved factors is fixed over time. One

can think of this difference as being represented by B' in (0.5).[1] By netting them out from A', one obtains the effect of the programme on household consumption.

It is necessary for both assumptions (0.4) and (0.5) to hold in order for the difference in difference estimation of the programme impact to be unbiased. Moreover, obviously, to implement the difference in difference estimator in equation (0.5), it is necessary to observe data before and after the programme was started in control and treatment areas. With an important caveat discussed below, this is what happens in our case.

2.2 Some caveats and peculiarities

Assumption (0.4) is likely to be violated if individuals living in treatment areas change their consumption decisions *in anticipation of* the programme. This would mean that $E(Y_{0,B}|T=1,X)$ is a biased estimate of consumption outcomes in treatment areas in the absence of the programme. Even if the effect of unobserved factors on consumption is fixed over time (i.e. the second assumption holds), B' in (0.5) confounds both the effects of unobserved variables and programme (anticipation) effects. (0.5) would thus estimate the change in consumption outcomes after the programme is implemented (the parameter of interest), net of the change that was induced by anticipation of the programme.

Anticipation effects are particularly relevant in our case because the programme had already registered beneficiaries. They were therefore definitely aware of the programme at the time of the baseline survey in 2003. Moreover, and more importantly, while the programme was supposed to start payments in treatment areas *after* the baseline survey, in about half the treatment areas, the treatment was actually started between the end of 2001 and the beginning of 2002 so that at the time of the baseline collection in the summer of 2002, households in those towns were already receiving payments. In what follows we will label TCP the treatment towns where the programme started before baseline and TSP those where the program started after the baseline.

Unfortunately, unlike with other outcomes (such as school enrolment) where we have retrospective information that refers to pre-baseline years, for consumption we do not

[1] One can think of B' as representing pre-existing and persistent differences in consumption outcomes across treatment and control areas.

have any information on pre-baseline years. Therefore, we cannot estimate (or test for the presence of) anticipation effects. However, unlike for other outcomes, we do not expect them to be particularly important. For consumption to react to the anticipation of future transfers, households should be able to borrow against such future transfers and/or reduce their savings. It is not clear that the very poor households in our sample are able to do so.

The presence of treatment communities where the programme starts early, does not necessarily constitute a problem for the use of the difference in difference strategy outlined above. Indeed, it can constitute an advantage if, while not completely random, the division of municipalities between TCP and TSP is likely to have yielded two groups that are more similar than the treatment and control groups. In the first instance, therefore, we can have preliminary estimates of the effects of the program on consumption comparing TSP and TCP towns at baseline. Moreover, if one does not think that the effect of the program on consumption cumulates (that is that what matter is the flow of current payments rather than the stock of payments received up to a point in time), one can combine observations from TSP, TCP, control towns in baseline and follow up to have a more efficient estimate of the effect.[2]

2.3 Estimation methods

Whether comparing treatment and control towns after the start of the program (equation (0.3) or when using a difference in difference approach (equation 0.5) we always control for observable variables both at the individual and at the town level. Obviously, there are different ways of doing so. One can impose a parametric dependence of the outcome of interest on the control variables or use non parametric methods. Parametric assumptions can be problematic and restrictive, particularly when invoked to extrapolate beyond the region of 'common support'. However, using non-parametric methods such as matching methods can reduce substantially the precision of the estimates.

We have experimented with both approaches, but here we report only the results obtained via parametric regressions. First, common support problems are practically non-existent within each of our samples, and thus we are safeguarded against misleading inferences that may arise from extrapolation beyond the common support. Second, the

[2] Diff in diff can in principle be computed using only TSP and TCP as the former switch from non receiving the program to receiving it while the former receive it throughout. Common unobserved factors are therefore differenced out using the follow up rather than the baseline data.

imposition of linearity seems to provide a very reasonable representation of the effects – the effects that are estimated using matching are very similar. Finally, the parametric specification leads to a large gain in efficiency compared to matching.

In practice there are several ways in which we can uncover the parameters of interest by a parametric diff in diff approach, depending on the control group we choose to utilize. In particular, one could rely on a comparison of the TSP town (for which a true baseline is available) with the control town, or a comparison of TSP and TCP towns or combine the two. The assumption behind the last two methods is, as we mentioned, that the size of the effects of the program does not depend on the cumulate of payments received in the past.

If one uses the comparison between TSP and control the effect can be estimated from the following simple regression that pools baseline and follow-up data but does not use data from TCP towns:

$$Y = \alpha_1 + \alpha_2 d^{TSP*followup} + \alpha_3 d^{TSP} + \alpha_4 d^{followup} + \alpha_5 X + \varepsilon \qquad (0.6)$$

where Y is consumption, X is a set of control variables (both individual and town level) $d^{followup}$ is a dummy which equals 1 if the observation is a follow-up one, d^{TSP} a dummy that equals 1 for the TSP observations, while $d^{TSP*followup}$ equals 1 if the observations is a TSP *and* is observed in follow-up (that is receives payments). Clearly, the diff. in diff. estimate of the effect of the program is given by α_2. If one compares all treatment with all control one estimates the following equation estimated on all data:

$$Y = \alpha_1 + \alpha_2 d^{treatment} + \alpha_3 d^{TSP/TCP} + \alpha_4 d^{followup} + \alpha_5 X + \varepsilon \qquad (0.7)$$

where now $d^{TSP/TCP}$ equals one either in TSP or TCP towns and $d^{treatment}$ equals one for all TCP observations and for TSP observations in follow-up.

Equation (0.7) can be generalized to allow for systematic difference between TSP and TCP towns by splitting the $d^{TSP/TCP}$ dummy into two. More interestingly, equation (0.7) can be generalized by allowing intensity effects, that is by allowing the effect to be a function of the amount of time the programme has been operative. This can be done in a simple fashion by allowing the coefficient α_2 to be different in TSP and TCP towns (as the latter have exposed for longer). Notice that the difference between the 'short run'

and 'medium run' effects is identified by the comparison between TSP and TCP. Alternatively, one can allow the coefficient α_2 to be a function (typically a polynomial) of the number of payments received in a given town. This approach has the additional advantage of exploiting variability in the number of payments received across treatment towns due to administrative delays.

Such an approach is implemented by fitting the following equation :

$$Y = \alpha_1 + \alpha_{20} d^{treatment} + \alpha_{21} \#p + \alpha_{22} (\#p)^2 + \alpha_3 d^{TSP/TCP} + \alpha_4 d^{followup} + \alpha_5 X + \varepsilon \qquad (0.8)$$

where *#p* is the number of payment since the start of the programme in the town of residence.

3. The data

3.1 The Familias en Acción Survey

As mentioned in the introduction, the data base used in this evaluation contains information on approximately 11,500 households living in 122 towns. The households are all poor enough to qualify for the programme, which, however, is only implemented in 57 of the 122 towns in the sample. The dataset is a longitudinal one, made of two waves, the first collected in 2002 and the second in 2003. Attrition between the two waves is around 6%. The data set contains information on very many variables. A very detailed description of the baseline survey is contained in Attanasio et al. (2003), while the follow up survey is discussed in Attanasio et al. (2005).

Consumption information is collected through retrospective questions referring to consumption in the previous week for food and other frequently consumed items and on longer horizons (one month, three months or six months) for other items (such as utilities, clothing etc.). The information is comprehensive and in the case of food, includes information on 'consumption in kind', where by that we mean commodities consumed but not purchased (produced, received as pay, or as gift). Consumption in kind is expressed in pesos using town level prices observed for households buying similar commodities..

3.2 Some descriptive evidence.

The households that make the *Familias en Acción* data base are very poor and the survey is unique as a description of this type of population. A detailed description of

the data can be found in Attanasio et al. (2004). Here we provide some simple statistics on the level and structure of consumption.

In Table 1, we report the means, at baseline, of total consumption, food consumption and the share of food consumption. All consumption components are converted into monthly flows. The exchange rate between the US dollar and the Colombian peso is about 2,600. All these figures include, for food consumption, the imputed value of consumption in kind. Given the nature of the sample, consumption in kind is very important. 85% of the sampled households report some consumption in kind and, for this sub-sample, on average accounts for 25% of food consumption.

We divide the households in the survey between those living rural and urban areas where, by the latter, as elsewhere in this report, we mean households living in the 'cabecera municipal', the urban centre of the municipality.

The level of monthly consumption reflects the extreme poverty of hour households. The average monthly consumption corresponds to roughly 163 US dollars, which are used by families including, on average seven members. As to be expected, households living in rural areas are poorer than those living in urban areas.

Food accounts for a large fraction of total consumption, which is particularly high in rural areas, reaching 74% of total consumption. Interestingly, the *level* of food consumption is not very different between urban and rural areas, which, given the fact that urban total consumption is about 5% higher than rural consumption, explains the larger share of food in rural areas.

Table 1
Total and food consumption at baseline

	Total cons.	Food cons.	Share of food
Total	424,028.1	301,385.4	0.719
Rural	415,026.3	303,975.3	0.739
Urban	433,024.9	298,796.9	0.698
Treatment (TSP)	415,338.6	301,111.6	0.735
Treatment (TCP)	445,586.8	317,339.1	0.715
Control	413,559	289,527.1	0.710
Source: Familias en Accion baseline data			

We also report the means in the table for the three groups of towns included in our sample: the 65 control towns and the treatment towns, divided among those where the programme started early and those where the programme started after the baseline. Encouragingly, there are no large differences in average consumption between control and TSP towns. The higher level of consumption in TCP town that are already receiving the programme, is a first indication of the effect of *Familias en Acción.*

Relative to the analysis contained in the baseline report (see Attanasio et al., 2004), we have made some minor adjustment to the classification of commodities that form the various components of consumption. In Table 2 we report our classification and, for each of the groups of commodities, the monthly expenditure,[3] the share in total consumption and the number of zeros at baseline. The commodities considered account for 90% of consumption expenditure.

Table 2
Consumption components and their shares
TSP and controls at baseline

	Mean	Share in total consumption	% of zeros
Food in	296083.1	0.711	0.000
Food out	7581.9	0.012	0.853
Housing services	28635.4	0.069	0.011
Alcohol & Tobacco	5919.0	0.013	0.728
Clothes and footwear	10288.6	0.020	0.578
Men cloth & footwear	2217.5	0.004	0.833
Women cloth & footwear	2649.7	0.005	0.849
Children cloth & footwear	5034.9	0.011	0.684
Entertainment	1292.9	0.002	0.905
Health	11269.5	0.025	0.383
Education	16739.4	0.042	0.192
Durables	2580.4	0.005	0.766

Source:Familias en Acción baseline survey.

[3] The questions for different commodities refer at different horizons. For instance, the questions about food consumption refer to the previous week, the ones about utilities to the previous month and the questions about durables and clothing to the previous six months. The reported quantities are then converted into monthly amounts for consistency.

Given the importance that food has for the households in our sample and the importance given by the Familias program to nutrition, we also report, in Table 3, the most important components of food consumption. It should be remembered that these include food in kind and is not confined to expenditure. In terms of value, the largest share of food expenditure is taken by what we call 'proteins'. These include meat, chicken, fish, milk, eggs. Virtually all households report consuming some of these commodities in the week preceding the interview. The second largest share is constituted by cereals, among which the most common is rice, which account for nearly 20% of the food budget. These are followed by potatos and other roots and fruits and vegetables.

Table 3
Composition of food: TSP and control in baseline

	Value of consumption	Share in total food	% of zeros
Proteins (meat, chicken, milk)	111201.3	0.377	0.011
Potatoes, yucca and other tuberc.	26401.1	0.095	0.065
Cereals	52716.6	0.185	0.031
Fruits and vegetables	32203.2	0.109	0.034
Pulses	9324.0	0.032	0.280
Fats and oils	12810.2	0.046	0.115
Sugar and sweets	23093.3	0.083	0.063

Source: Familias en Acción baseline survey

4. Programme Impacts

4.1 Impact on total consumption and food consumption

We start the analysis of the effect of the program on consumption considering several methodologies. In particular, we estimate the effect of the program considering both the simple difference between treatment and controls at the first follow up and differences in differences. As we discussed in the methodology section, the simple comparisons at follow up is a valid if, conditional on observables, there are no systematic differences between treatment and control municipalities and if the effect of the program does not depend on the number of payments received by beneficiaries households.

In all our exercises, we control for a large number of observable characteristics, both at the individual and at the municipality level. The list of the control variables is given in the Appendix.

In Table 4, we report our estimates of the impact of the program on total and food consumption. We report both results for the rural and urban sub-samples separately. As mentioned above, the control variables are entered parametrically.

Table 4

Effects of Familias en Acción on total and food consumption

Variable	Follow up: Treatment vs control	Diff in diff TSP vs Control	Diff in diff Treatment vs Control
Total consumption: urban	44386*** (15358)	53189*** (19612)	52576*** (13551)
Total consumption: rural	87714*** (20479)	67587*** (18563)	53831.1*** (18888)
Food consumption: Urban	40274*** (8863)	40556*** (11296)	37018.1*** (9898)
Food consumption: rural	70171*** (11352)	60062*** (16129)	41956.6*** (16075)
Log tot. cons	0.104***	0.130 ***	0.147***
Urban	(0.03)	(0.044)	(0.034)
Log tot cons	0.166***	0.156***	0.145***
Rural	(0.033)	(0.045)	(0.051)
Log food cons	0.216***	0.186***	0.158***
Urban	(0.037)	(0.042)	(0.034)
Log food. cons.	0.25***	0.203 *** (0.063)	0.157***
Rural	(0.036)		(0.056)

Source: Familias en Acción baseline and follow up survey.
Standard errors are clustered at the municipality level.
* denotes statistical significance at the 10% level and ** denotes statistical significance at the 5% level *** denotes statistical significance at the 1 % level or less.

In the first column of Table 4, we report the results obtained by comparing treatment and control towns at follow up. In the various rows, we report the effect of the program

on total and food consumption both in levels and in logs. The results indicate relatively large effects of the program. These results, obtained from a parametric specification for the controls, seem to be larger in rural than in urban areas. Similar results are obtained non-parametrically.

The results in column 1, would be biased in the presence of systematic and unobservable (or uncontrolled for) differences between treatment and control towns. To check for this possibility, in the next two columns, we compute the impact of the program using diff in diff. In Column 2, which corresponds to estimates of equation (0.6), we use only the TSP municipalities. In Column 3, which are obtained estimating equation (0.7), we use all treatment municipalities. The points estimates are smaller than in the first column, especially in rural areas, but not significantly so. Indeed the impact is not very different between urban and rural areas. The main difference between Column 2 and 3 is the fact that, in the former, the rural impacts seem slightly larger again.

The effects we observe are relatively large, at about 15% of total consumption. They compare to an average monthly grant (conditional on being paid) of about 100,000 pesos. Part of the difference is probably explained by the fact that, especially in the first phase of the program not all municipalities received the program every month. Moreover, the conditionalities imposed by the program might cause a reduction in income (through child labour). Finally, part of the grant might be saved, possibly for investment purposes.

Perhaps surprisingly, the increase in consumption is, proportionally, as large as the increase in total consumption. This implies that the share of food consumption is not affected by the program. This result would not be inconsistent with a unit elasticity of food expenditure to total consumption or with a quadratic relationship so that part of the population would have an elasticity less than one and part greater than one. Attanasio, Battistin and Mesnard (2005) analyze the shape of Engel curves in this population.

As we mentioned above, we also computed the impact of the program using Propensity Score Matching techniques. The results are not reported for the sake of brevity, but can be easily summarized saying that they are not statistically different from those in Table 4. Standard errors, as to be expected, are much larger and sometimes cause the results not to be statistically significant. The point estimates are not too different from those in Table 4, especially when they are estimated with some precision. The only relatively large differences are observed (especially in urban areas) in cases in which the precision deteriorates badly.

Given these considerations, in what follows we decided to report the results obtained by diff-in-diff with parametric controls. While the full set of results is available upon request, the pattern that emerges is similar to that discussed. Whenever the PSM estimates have a reasonable amount of precision, they are not very different from the parametric ones. When the precision of these estimates is reduced, we obtain occasionally very different effects, especially in urban areas.

4.2 Impact on consumption components.

The fact that consumption increases as a consequence of a cash transfer is perhaps not very surprising. What is more interesting, both from a scientific and a policy point of view is to establish which components of consumption increase the most. Surely the evaluation of the program would be very different if it results in an increase in the expenditure on education than if it results in an increase in the expenditure on alcohol. While some of the effects that we will uncover are not completely surprising given the conditionalities of the program, it is nonetheless important to check whether some of the benefits of the program, which is intended to foster the human capital accumulation process of children, are captured by adults.

In Table 5, we report the effects estimated by diff in diff on several components in urban and rural areas. As many commodities have a relatively large number of zeros, we use a tobit specification where, in addition to the group, treatment and year dummies, we include the same list of controls we have used to estimate the effect of the program on total and food consumption. As before, in Table 2 we only report the estimate of the program impact. A full set of results is available upon request.

The results show that the effect of the program is very concentrated in very few commodities. In addition to food, which, as we have seen above absorbs a considerable fraction of the increase in consumption, we estimate positive effect on children clothing and footwear, and, in urban areas, on education. We do not find any effect on any other commodity. In particular, we do not find any effect on 'adult' commodities, such as adult clothing or alcohol and tobacco.

Table 5

Effect of Familias en Acción on consumption components
Diff in Diff- Treatment vs Control
Parametric controls

	Urban	Rural
Housing services	2296.2	1483.8
	(3480.3)	(3230.7)
Alcohol & Tobacco	2175.1	-1184.2
	(3578.7)	(2552.4)
Clothes and footwear	9792.6***	10296.8 ***
	(3153.1)	(3403.0)
Men cloth & footwear	-3952.4	-2090.4
	(2371.1)	(3269.8)
Women cloth & footwear	-1410.0	58.7
	(1845.2)	(2080.6)
Children cloth & footwear	12088.1***	11634.2***
	(2181.6)	(2267.3)
Entertainment	-4123.7	-2885.7
	(4735.2)	(3496.6)
Health	1898.7	3641.9
	(2381.8)	(3043.1)
Education	8005.5**	-610.7
	(3468.3)	(2691.4)
Misc	4090.8***	3477.0**
	(1565.1)	(1535.7)

Source: Familias en Acción baseline and follow up survey.
Standard errors are clustered at the municipality level.
* denotes statistical significance at the 10% level and ** denotes statistical significance at the 5% level *** denotes statistical significance at the 1 % level or less..

Particularly noticeable is the increase in children clothing, which is estimated in 12.088 pesos in urban areas and 11634 in rural ones. Such an effect induces a significant increase in the share of children clothing in total consumption.

Given the importance of the increase in food consumption and the emphasis the program places on nutrition, we look in more detail at the effect of the program on food consumption. In Table 6, we report the estimated effects on the components of food consumption we considered in Table 3. The increase in food consumption is concentrated in proteins, for which we find an increase of 21831 pesos in urban area and 21717 in rural area. As such an increase is proportionally greater than the increase in food consumption, it induces an increase in the share of proteins in food. We also observe some minor effects on cereals and fats, which increase significantly in all areas.

Table 6

Effect of Familias en Acción on food components

	Urban	Rural
Proteins (meat, chicken, milk)	21831.4***	21717.2***
	(4050.2)	(6581.7)
Potatoes, yucca and other tuberc.	2938.9 ***	4133.1
	(1564.1)	(3698.0)
Cereals	5008.8 ***	9094.6***
	(2258.2)	(3056.3)
Fruits and vegetables	1399.3*	4249.4
	(3750.5)	(4930.0)
Pulses	313.6	2008.4
	(705.9)	(1499.5)
Fats and oils	1887.8**	3139.4**
	(794.0)	(1295.4)
Sugar and sweets	1234.6	647.2
	(858.0)	(2206.7)
Miscellaneous food	6612.3 ***	4583.7***
	(1470.0)	(2566.9)

Source: Familias en Acción baseline and follow up survey.
Standard errors are clustered at the municipality level. * denotes statistical significance at the 10% level and ** denotes statistical significance at the 5% level *** denotes statistical significance at the 1 % level or less.

4.3 Intensity effects

In section 4.1 and 4.2 we have assumed that the effect of the program on consumption does not depend on the 'intensity' of the program or on the amount of time for which the program has been operating. This is equivalent to assuming that the program has no cumulative effects on consumption. While such a hypothesis is not implausible, especially for non durable items, in this section we check whether it is borne out by the data. In particular, as we discussed in the methodology section, we let the effect of the program to be a function of the number of payments received by each households, as in equation (0.8). Our data are, at least theoretically, able to identify these effects because of the large variability observed in the number of payments. A large part of this variability is driven by the fact that the program started earlier in TCP towns. Once again, we focus on parametric specifications and simply add to the regressions whose results we reported in Section 4.1 and 4.2, the interaction of the treatment dummy with the number of payments and, in some specifications, its square.

While we do find some significant results on some of the components, by and large, there seem to be very little 'intensity' effects in consumption and its components. Those that are worth mentioning are on miscellaneous items in urban parts. Such evidence is not particularly surprising, as consumption is a flow concept that should react to (permanent) changes in the flow of resources and not to the cumulate amount of transfers.

5. Conclusion

In this report we have considered the impact of the conditional cash transfer programme in Colombia on total consumption of very poor households and its components. Our best estimates take into account pre-existing differences across treatment and control areas and controls for a wide range of observable characteristics that are likely to affect consumption. We find that the programme has been effective at increasing largely total consumption and its main component, food consumption, both in rural and urban areas. The estimated effect on total household consumption estimated around 53000 pesos represents a 15% increase as compared to the average consumption level at baseline. Food consumption that represents 72% of total consumption at baseline has increased by around 39000 pesos as a consequence of the programme, which does not represent a significant increase in the share of food consumption in total consumption.

The programme has also contributed to improve the quality of food taken. In particular, households have increased the consumption of items rich in proteins (milk, meat and eggs) by an average monthly value around 22.000 pesos, and of items rich in cereals by an average monthly value around 15000 pesos in urban area and around 9000 in rural area. Furthermore it has created redistributive effects in favour of children through education and related cloth expenditures in all areas, while it has not affected significantly consumption of adult goods like alcohol or tobacco and cloth. We also find that the magnitude of the programme effects on these consumption outcomes are different in rural, which are poorer at baseline, than in urban areas.

References

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Appendix

Control variables

Age of household head

Age of spouse of household head

Dummy variable for single head of household

Dummy variables for education of household head and spouse

Dummy variables indicating type of health insurance of the head of the household – subsidised, unsubsidised or none

Dummy variables indicating type of health insurance of the spouse of the head of the household – subsidised, unsubsidised or none

Dummy variable for whether live in a house

Dummy variable for whether own or rent house

Dummy variables for material of house walls (brick, wood, cardboard, no walls)

Dummy variable for existence of gas pipe

Dummy variable for existence of water pipe

Dummy variable for whether household has sewage system

Dummy variable for whether household has rubbish collection

Dummy variable for whether household has or has access to a telephone

Dummy variable for whether toilet is connected to sewage system / septic tank or not connected / no toilet

Dummy variable for whether at least one individual in the household has suffered from extreme violence in 2000, 2001 or 2002

Dummy variables for regions

Altitude

Number of urban public schools in the municipality

Number of rural public schools in the municipality

Number of students per teacher in the municipality

Classroom square metres per student in the municipality

Number of public hospitals in the municipality

Index of quality of life in the municipality in 1993

Population in the urban part of the municipality in 2002

Population in the rural part of the municipality in 2002

Number of public puestos (small healthcare providers) in the municipality

Number of pharmacies in the municipality

Proportion of households in the municipality with piped water

Proportion of households in the municipality with sewage system

Dummy variable for whether any IPS (healthcare provider) in the municipality suffered task-force desertion