# What Would You Do?
# An Investigation of Stated-Response Data

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## 1. Introduction

When analysing choices or policy impacts, economists generally rely on what people actually do, rather than what they *say they would do*. The effectiveness of a particular randomised policy, for example, is routinely estimated as the difference between outcomes amongst the group of programme participants (treatment) and non-participants (control). An alternative approach is to elicit information from respondents about what they *would* do, if some particular aspect of their environment changed. This is the 'stated-response' approach. This is often treated with scepticism by economists, for a variety of reasons, such as concern that respondents may not think as carefully about a hypothetical choice as about an actual decision that will affect them; that they may respond in a way they think the interviewer would deem most socially acceptable; or may answer strategically if they believe their response may affect policy.

However, the 'stated-response' approach is often the more practicable one. For instance, suppose policymakers are interested in the effects of abolishing a welfare programme that is considered to be effective. Testing this on a subset of recipients is likely to meet with significant political opposition (and is often criticised on grounds of horizontal equity), so probing individuals using hypothetical questions may be a more feasible alternative. The cost of inclusion of such questions in surveys is relatively low and uncontroversial, and in this note we seek to understand the information that such questions can provide and the extent of their usefulness. We investigate responses to two hypothetical questions that we piloted in a survey in an environment in which a welfare programme providing cash transfers conditional on school attendance, is in operation – *Familias en Acción* in Colombia. The questions, piloted in the most recent survey and described in more detail in section 3, probed recipients about withdrawal from school if the cash transfers were stopped, or were made unconditional. To our knowledge, such questions have not previously been used in surveys and environments such as these (and with such a large sample).

It should be stressed from the outset that we do *not* expect an individual's stated-response to "withdrawal of a programme" to proxy what the individual's state would have been, had the programme never been in place (which is proxied by controls). The outcome (school attendance) in this hypothetical world is still affected by past exposure to the programme, unlike controls. For example, the subsidy may have altered the intra-household allocation of schooling, and scrapping it may have different implications for siblings; the programme may have allowed households to achieve higher levels of consumption, and in its absence this may be maintained at the expense of schooling; or the programme may have altered preferences for education. In saying this, it is worth

noting that the Colombian government is actively considering removing subsidies for younger children from the programme that we consider. We would argue that even though the programme has had little or no impact on this group, removing subsidies whilst keeping all other aspects of the programme unchanged may have detrimental effects on them. Indeed, taking the stated-responses that we elicit at face value, younger children would in fact be the group *most* at risk were the programme abolished. Note however, that the Government is planning to redirect the savings to older children, so this is likely to mitigate at least some of the unintended perverse consequences, though we should stress that we of course cannot simulate the effects of any policy changes in this framework. What we do here, is to evaluate the reliability of stated-responses and to explore their potential in applied microeconomic research.

The paper proceeds as follows. Section 2 describes the key features of the *Familias en Acción* programme, and section 3 describes the related literature and the stated-response questions that we investigate. Section 4 and 5 take a more detailed look at the responses to each of the two questions. Section 6 concludes.

## 2. Familias en Acción

*Familias en Acción* is a welfare programme that was set up by the Colombian government in 2001 with the help of a World Bank and Inter-American Development Bank loan. It aims at fostering the accumulation of human capital amongst the poorest Colombians, through the provision of conditional subsidies for investments into education, nutrition and health. Initially only available in rural areas, it has now been extended to urban districts and as of 30th June 2006 was being received by 518,000 families.[1]

Analysis of the programme has primarily involved the evaluation of the impact in a sample of the 622 rural areas where it was first rolled out.[2] The areas were elected on the basis of the following criteria: access to basic education and health infrastructure; presence of at least one bank; less than 100,000 inhabitants and not the regional capital; the registration for the programme by the local authority and the provision of key documents. Within these municipalities, a family is eligible if it has children under 18 and is formally classified as being in the lowest quintile of the official socio-economic classification (SISBEN 1) as of December 1999.

[1] It is estimated that a further 388,000 families are eligible but currently not in receipt, of whom 134,000 have registered and are awaiting first payment.

[2] See Attanasio et al, 2005; 2006.

To increase school enrolment, grants are paid to mothers conditional upon their children being enrolled in school and attending at least 80% of classes. Grants are per child, and there are two levels, with the grant for secondary school being twice the level for primary school.[3] To improve nutrition and health, a flat-rate monetary supplement of 46,500 pesos per month is given to all beneficiary families with children under 7 years of age that comply with a programme of vaccinations and growth and development checks for children, and maternal attendance to courses on nutrition, hygiene and contraception. In this note, we focus on the education component of the programme.

Previous evaluation of *Familias en Acción* has made use of specialised survey data collected in 122 municipalities, firstly in 2002, and twice since, in 2003 and 2005/06. The two later surveys were conducted whilst all eligible municipalities were in receipt of payments, of which about half had also been receiving payments in the 2002 baseline survey. Description of the survey sample, the construction of a suitable group of control municipalities, key municipality descriptive statistics, and our quantitative methodology can be found in previous publications.[4] Quantitative analyses of the impact of the programme (using a control-treatment difference-in-differences methodology) show sizeable impacts on school enrolment amongst older children, in the region of 5 to 7 percentage points (see Attanasio et al, 2006).

## 3. Stated-Response: A reliable measure?

Whilst the impact of implementation of such programmes has been studied extensively, to our knowledge no work has been conducted that looks at the impact of *withdrawal* of a programme, i.e. the termination of payments to families. This empirical lacuna is in part due to the political and ethical considerations in evaluating this, as in the absence of a structural model, it would generally involve an experimental set-up in which a successful programme was randomly withdrawn from some areas. It is however an important question for policymakers, who may face difficult choices if resources for welfare programmes are reduced.

In the absence of a structural framework, one potential approach is to use stated-responses, or in other words to ask beneficiaries directly how they *would* respond to the withdrawal of the programme. This approach is controversial, particularly amongst economists. Whilst many sociological surveys contain substantial sets of subjective

[3] The level of the grant in 2002 was 14,000 pesos per month for primary school children and 28,000 pesos per month for secondary school children (2002 prices). In practise the payments are up-rated annually with inflation.

[4] For instance, see "Child Education and Work Choices in the Presence of a Conditional Cash Transfer Programme in Rural Colombia" , IFS 2006, pages 8 – 18; available at http://www.ifs.org.uk/wps/wp0613.pdf

questions, doubts remain as to whether these questions provide meaningful and reliable answers that can be properly integrated into an economic or econometric framework. A growing literature, summarised in Bertrand and Mullainathan (2001), suggests the following key issues:

- Responses seem to be influenced by question order or the way in which a question is asked, particularly if questions elicit an emotional response – so-called 'framing effects';
- Responses (for both attitudes and predicted behaviour) are often unstable over time, and;
- People seem poor at forecasting future behaviour in hypothetical situations (particularly those of which they have little relevant experience), yet feel pressured to appear decisive.

Additionally, and of potentially significant importance for our paper, are two more 'conscious' forms of misleading response: Beneficiaries may respond in the way they that seems most socially acceptable (not to withdraw their child(ren) from school), or on the other hand they may respond strategically if they believe their response will impact policy (to withdraw their child(ren) from school to discourage the government from abolishing the programme). However, given that such questions can be included at relatively low-cost in existing survey questionnaires, it is worth investigating the value contained in such information, a fact that increasing numbers of economists have recognised.

The branch of economics in which such questions are most used is environmental economics, where "contingent valuation methods" are often employed to obtain values for non-market goods and services. Such methods have not been without criticism however, with many studies finding that hypothetical responses exceed actual values (see for example List et al (2006), and references therein). Despite this, List et al (2006) have found that choice experiments combined with cheap talk provide hypothetical values that are close to real ones. Additionally, Ameriks et al. (2007) use a strategic survey to identify the relative importance of precautionary savings and bequest motive in explaining expending during retirement. A very large of literature has used expectations data either as explanatory or dependent variables (Griliches, 1980; Hamermesh, 1985; Shaw and Shapiro, 1987; Bernheim and Levin, 1989; Van der Klauw 2003; Manski 2004; Hurd and McGarry (1995 and 2002)). A number of papers highlight the fact that endogeneity is likely to be of concern with the direction of causality difficult to ascertain without work on the formation of expectations and attitudes, which remains a largely undeveloped field.

In this note, we analyse responses to two questions, designed to elicit school choices from recipients if *Famias en Acción* were withdrawn or made unconditional, and that were piloted by the authors in the second follow-up survey in 2006. The first question asks directly whether a family would withdraw each child from school if the programme was abolished. The respondent (typically the mother) of the survey module on 'participation in the programme' was asked the following:

*Suppose the government withdraws the subsidy. Would you withdraw your child(ren) from school? [Question 1]*

If the response is yes, they are then asked whether they would withdraw all of their children or just some of them. If the response is "some", they are requested to identify which child(ren) they would withdraw.[5] The survey respondents are also asked the following question, with responses recorded in the same manner as above.

*Suppose now the government makes the subsidy unconditional so that it does not depend upon sending your child to school. Would you withdraw your child(ren) from school?*
*[Question 2]*

By probing respondents about the importance of conditionality, we obtain indirect evidence on the importance of credit market failures: if conditionality were unimportant, this would be consistent with the presence of credit market failures affecting education choices. We return to this in section 5, after first discussing the responses to Question 1 in some detail.

## 4. "Withdrawal" of *Familias en Acción*

This section analyses the responses to Question 1. We first show that the responses are of reasonable magnitudes and display patterns suggesting that they have been carefully thought out by respondents (section 4.1). We go on to show how withdrawal varies by age for broad groups, using probit models to investigate the factors affecting whether children are withdrawn, and whether these are consistent with priors (section 4.2). This is followed by a closer look at variation, in particular focussing on potential roles for dynamic programme effects and intra-household allocation (section 4.3). Section 4.4 rounds up our discussion on the question about withdrawal.

[5] Question 2403, page 3, Questionnaire 2, 2006. Translation from Spanish.

### 4.1 Response Patterns

Table 1 shows the percentage of children attending school in the sample in 2006, and the "counterfactual attendance", i.e. the attendance rate if the programme was abolished. The difference between the two columns shows the percentage that would be withdrawn under this hypothetical scenario. The table also shows the effects of the programme on enrolment, estimated using treatment-control data and in pre- and post-programme periods.

**Table 1: Actual and counterfactual school attendance if the programme is withdrawn**

| Group | Actual Attendance | Counterfactual Attendance | Withdrawal Rate (1) | Estimated Effect (2) | Difference (2)-(1) |
|---|---|---|---|---|---|
| Rural 8 – 11 | 97.50 | 89.30 | 8.21 | ***2.65*** | -5.56 (1.58)** |
| Urban 8 – 11 | 97.82 | 94.21 | 3.64 | ***1.45*** | -2.19 (1.15)* |
| Rural 12 – 17 | 76.15 | 66.76 | 9.49 | ***6.50*** | -2.98 (2.59) |
| Urban 12 - 17 | 85.68 | 80.93 | 4.95 | ***3.85*** | -1.11 (1.73) |

Notes: Figures in column (2) estimated using parametric differences-in-differences from data on enrolment in treated and control areas in all four waves. Figures may not sum due to rounding. Bootstrapped standard errors in parentheses (500 replications). ** denotes statistical significance at 5% level; * denotes statistical significance at 10% level.

The figures in the Table indicate, in our opinion, that the responses to Question 1 are reasonable. The percentage withdrawn does not seem large enough to indicate significant strategic behaviour; moreover withdrawal rates are higher in rural areas than in urban areas, and it is difficult to think why strategic behaviour would differ between the two. Note also that the magnitudes of the effects of the programme that would be suggested by these counterfactual rates, are similar to those obtained in the evaluation for older children. Whilst we do not necessarily expect the response to withdrawal to equal the estimated programme, we see this comparison as useful in placing the responses to the question in context. However, for young children, particularly in rural areas, the responses indicate a significantly lower rate of attendance if the programme is withdrawn than is suggested by prior evaluation. This shall be investigated further in later sections.

As indicated earlier, the question is such that respondents are first asked whether they would withdraw all, some or none of their children from school. The option of stating

that they would withdraw all may lead to concerns that the respondent may not consider his/her response fully and use this 'easy option' instead of deciding carefully about the education of each child. At the same time, responding that they would withdraw all children, may be genuine and reflect, for example high fixed costs some families face in sending their children to school, or aversion to inequality amongst children. The patterns of response are shown in table 2: amongst the 326 families that both state they would withdraw *and* that have more than one child, approximately 56% respond that they would withdraw all of their children, and the remaining 44% report that they would withdraw some. As discussed above, for this 56% of households, it is difficult to know to what extent this may reflect "lazy" responses versus other alternatives such as fixed costs in sending children to school. Further analysis is needed in order to determine the reliability of responses to the question, which we now turn to.

**Table 2: Patterns of Response to Question 1**

| **Response** | |
|---|---|
| Withdraw=1 *and* household has more than 1 child | 326 households |
| % that states withdraw=all | *56%* |
| % that states withdraw=some | *44%* |
| Withdraw=1 *and* household has 1 child | 90 households |

Figures 4.1 and 4.2 show the percentage withdrawn at each age, from 7 through 17, as well as actual attendance rates, for rural and urban areas respectively. In both areas, reported withdrawal is highest for those aged 11 through 14, compared to older and younger children, although withdrawal is highest relative to prior estimates of programme effects for those less than 11 years old.

**Figure 4.1: Rural**

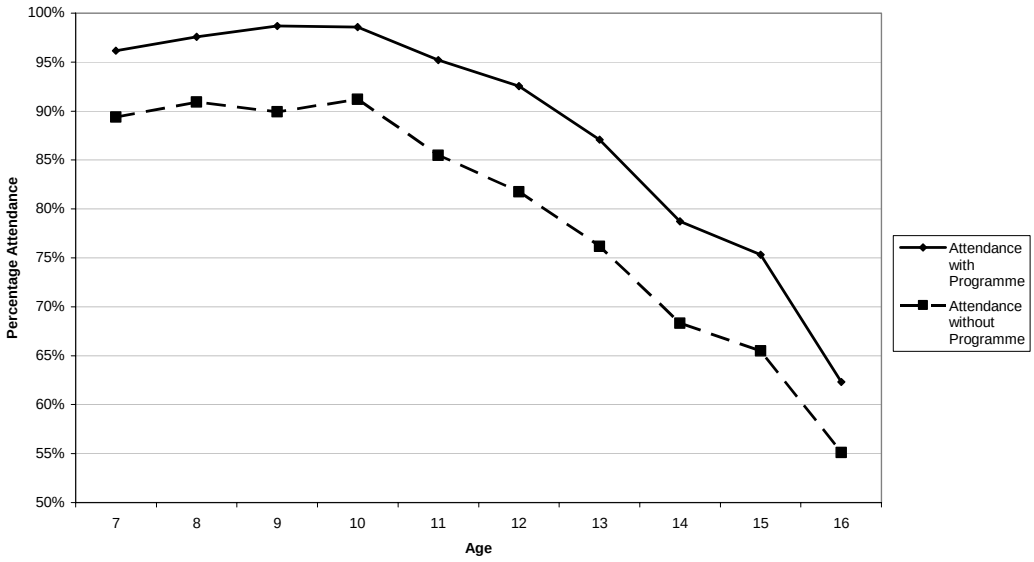



**Figure 4.2 Urban**

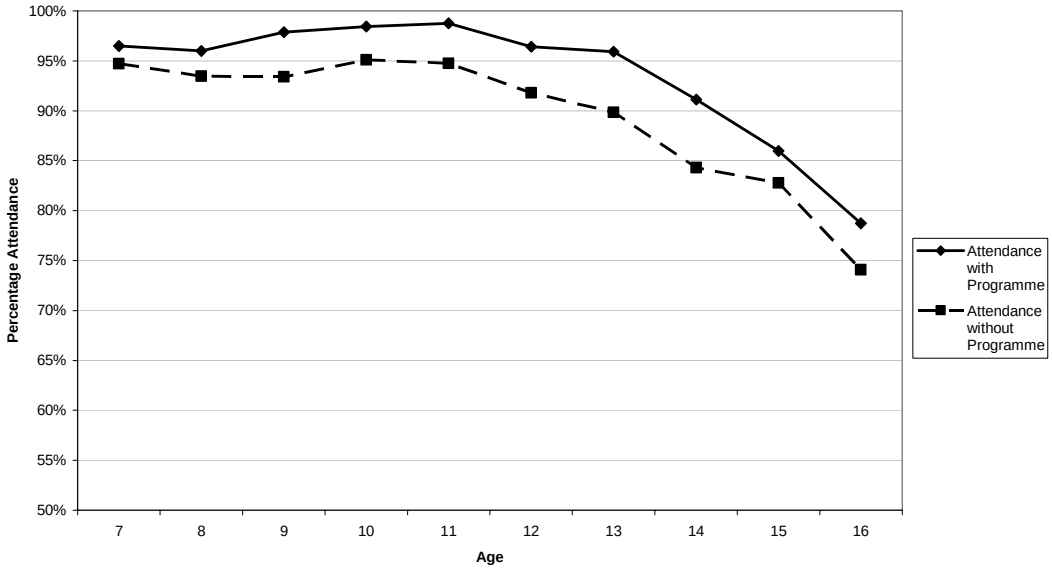


**Figure 4.3 Boys**

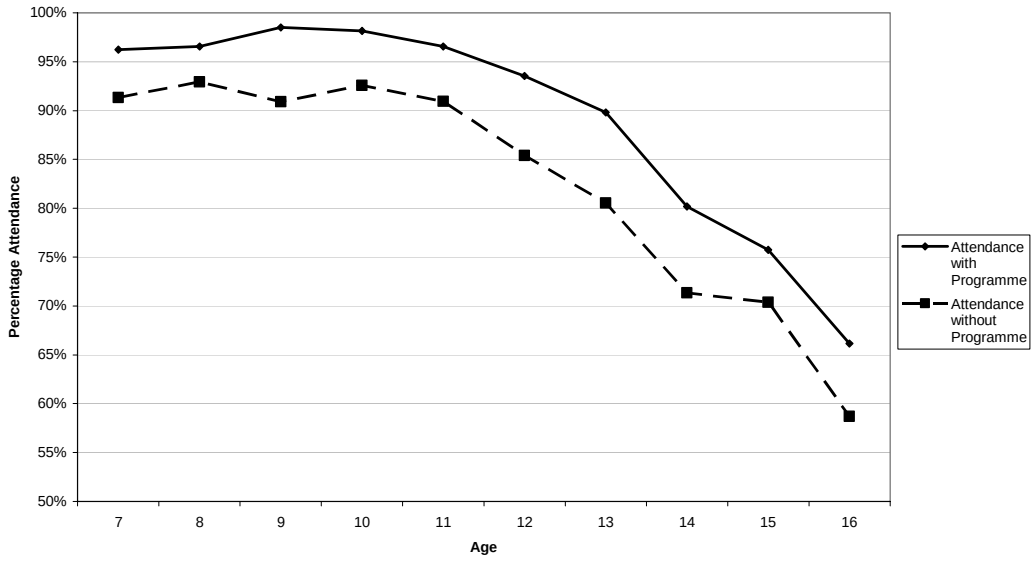
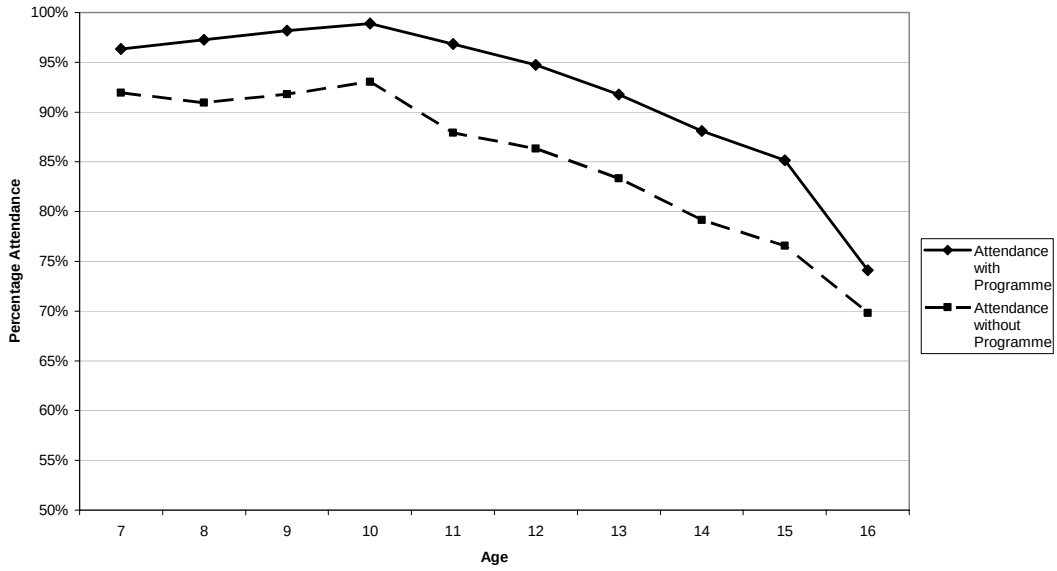



**Figure 4.4 Girls**




When broken down by gender, we see that withdrawal rates are considerably higher for females than for males, particularly amongst young children. Reasons for these differences by gender and age will be investigated in section 4.3.

## 4.2 How withdrawal rates correlate with observed characteristics

In this section, we take a look at how withdrawal rates correlate with various household characteristics by means of regression analysis. This provides some information as to the quality of the stated responses using standard regression analysis.

Table 3 shows the marginal effects from a probit regression in which the dependent variable is 1 if the child would be withdrawn, and 0 otherwise.[6] The results suggest that the stated responses are reasonable. For example, households that live further from school are more likely to respond that they would withdraw their child from school; mothers with secondary education are less likely to withdraw their children compared to mothers with no schooling; and, the higher the income of the head of household, the lower the probability of withdrawal.

**Table 3: Probability of Withdrawal**

| Variable | Coefficient | Standard Error |
|---|---|---|
| Age | 0.0260 | 0.0091*** |
| Age squared | -0.0009 | 0.0004** |
| Female | 0.0024 | 0.0050 |
| Distance to nearest school (mins) | 0.0010 | 0.0002*** |
| Mother education | | |
| Some primary | 0.0055 | 0.0094 |
| Complete primary | -0.0071 | 0.0117 |
| Some secondary or above | -0.0396 | 0.0141** |
| Urban area | -0.0458 | 0.0120*** |
| Coefficient of variation of head's historical labour income | 0.0062 | 0.0087 |
| Log of head's labour income | -0.0015 | 0.0008** |

Note: Robust standard errors clustered at village level.
** denotes significance at 5% level; *** denotes significance at 1% level.

This exercise suggests to us that the responses to the hypothetical questions contain genuine information about potential behaviour if *Familias en Acción* were scrapped. It alleviates concerns that responses are meaningless, or that strategic response is dominant. However, the relatively high rates of withdrawals of young children (particularly young girls and those in rural areas) remain puzzling, particularly since there is evidence (Attanasio et al, 2006) that the programme did little to boost the already high attendance of this group. It is to this issue that we now turn.

### 4.3 Comparison with actual programme effect

In this section we compare the effect with the actual quantitative effect of the programme (shown in Table 1). We have seen that effects for young children in rural

areas, and to a lesser extent, urban areas are statistically different from each other. Why might this be?

One possibility is intra-household effects. As the programme has been in place in treatment areas for up to four years, dynamic effects may make the impact of programme withdrawal different from its initial impact upon introduction (i.e. not equal and opposite). We know for example that the programme induced a significant number of older children to stay on in school. If credit constraints force households to choose between siblings, this may then induce a different allocation of schooling amongst children than if the programme had never existed. For example, older children who have been induced to stay in school due to the programme may now have greater attachment to school, and/or may have revealed otherwise hidden ability. Younger children, on the other hand, may not have yet formed strong attachments to school, can arguably rejoin school more easily later on, and their ability is still uncertain. They may therefore suffer in such trade-offs. This would be consistent with Thomas et al (2004), who find that poor households sought to protect investment in the education of older children after a large income shock, by withdrawing and/or reducing education-related expenditure of younger ones. They suggest that such behaviour is rational given the low returns to primary education with respect to secondary education in Indonesia, the uncertainty regarding whether the younger child actually progress to secondary level, and the greater opportunity for younger children to 'catch up'.

However, it does not seem that our findings are driven by intra-household trade-offs: 82% of 8-11 year olds withdrawn are from households that would withdraw all children, so they are not trading off schooling between children. Hence, whilst we do find a significant impact of having an older sibling upon the chance of being withdrawn, this is driven by those families where all children are withdrawn (and not by selective withdrawal of the younger siblings). Amongst households that would withdraw some, the number of 8 to 11 year olds is too low (35 in rural areas, 14 in urban areas) to allow for any robust investigation of intra-household effects.

In saying this and noting the economic psychology literature discussed in section 3, one may be concerned that the phrasing of the question introduces potential biases into our findings. Whilst the response to Question 1 (section 3) appears to be a valid indicator of which households would withdraw children in the absence of the programme - table 3 shows that the decision to withdraw children varies in the expected manner with respect to household-level variables such as location, distance to school and income - the choice of which child(ren) to withdraw, in the follow-up to Question 1, may be less reliable. Households that respond in the affirmative to Question 1 are probed about which

children they would withdraw, and one option is to state "all". This response may be chosen for various reasons such as an inability to decide on the spot, an unwillingness to reveal any preferences over different children in front of the interviewer (and potentially the children themselves), or choosing the quick option. Unable or unwilling to discriminate between children, but wanting to withdraw at least one, they may reply "all", thereby biasing upwards the number of children that they say they would withdraw. This is a potential explanation for why we see high numbers of young children being withdrawn amongst the "all" group and within this group only where they have older siblings. Statistically-speaking, the worry is that, measurement error in withdrawal is positively correlated with the presence of older siblings and it is this correlation that we are picking up, rather than an underlying causal impact of older siblings[7].

It is of course difficult to know the extent to which this is going on, and we acknowledge it as a shortcoming in the piloting of the question, and one that we would change in future surveys. To provide an indication as to the bias it may have induced, we perform the following back of the envelope sensitivity analysis: we assume that children aged 8 to 11 from families that would withdraw all are, instead, withdrawn at the same rate as families withdrawing 'some' withdraw their young children. This means we assume only approximately 25% of these young children would actually be withdrawn. Table 4 shows that for these groups, the estimated rates of withdrawal are no longer statistically significant from the estimated programme impacts.

**Table 4: Sensitivity Analysis**

| Group | Actual Attendance | Counterfactual Attendance | Withdrawal Rate (1) | Estimated Effect (2) | Difference (2)-(1) |
|---|---|---|---|---|---|
| Rural 8 – 11 | 97.50 | 93.55 | 3.95 | ***2.65*** | -1.30 |
| Urban 8 – 11 | 97.82 | 96.23 | 1.49 | ***1.45*** | -0.04 |

Notes: Figures in column (2) estimated using parametric differences-in-differences from data on enrolment in treated and control areas in all four waves.
Bootstrapped standard errors in parentheses (500 replications). ** denotes statistical significance at 5% level; + denotes statistical significance at 10% level.

[7] Bertrand & Mullainathan (2001) highlight that errors in subjective question responses are very likely to be correlated with both observable and unobservable characteristics of respondents and that if correlated with an explanatory variable, any relationship found may be spurious.

If we take the responses at face value, however, it is important to note is that whilst the responses indicate that more young children are being withdrawn than entered school due to the programme, there are not fewer old children being withdrawn; it seems the effect of withdrawing the programme is larger than equal and opposite to the effect of its introduction. Whilst this may be explained by the problems of the "withdraw all" response pattern (i.e. excess children counted as withdrawn because of inaccurate response), other possibilities may exist. For instance, if families receiving *Familias en Acción* became used to higher consumption they may therefore contemplate sending their children to work instead of school to fund this when the programme is withdrawn. However, this was investigated and seems not to be the case.[8]

What did occur between the introduction of the programme and the fielding of this hypothetical question is this; a fall in the reported real labour incomes of surveyed families This is driven by both a rise in the recorded non-employment rate of prime-aged males in our sample, of around 7 percentage points, and a small fall in income for those working. However, even discounting potential measurement concerns, this could only explain a fall in enrolment of between 0.04% and 0.4% points, significantly less than that actually observed.[9]

### 4.4. Summary

In this section we have demonstrated that the responses to Question 1 at the household-level provide withdrawal decisions that are plausible and are correlated in the expected manner with key factors determining school enrolment (distance from school, mother's education and income). Withdrawal is highest for girls, children in rural areas and those aged between 12 and 14 years old. Whilst we would have preferred a more in-depth analysis of dynamic and intra-household effects our sample sizes and doubts about the reliability of intra-household responses restricted our investigation of these factors. Furthermore, the potential impact of the recorded falls in real income is negligible in relation to our results. We see our analysis as a first step in exploring and understanding the potential value in stated-response questions, and has pointed to the following observations (a) stated-response data are reasonable and informative about decisions at the household level; (b) they are less reasonable when respondents are asked for more refined breakdowns (e.g. discriminating amongst children); (c) choice of question and

[8] We regressed withdrawal upon the change in consumption – change in private incomes. If this had been positive it would indicate those that used the subsidy to fund higher consumption (instead of savings) were more likely to withdraw their children. Instead we found the opposite.
[9] The former is based upon the coefficient on log(headincome), whilst the latter is based on an alternate specification using a measure in levels.

design of response input is crucial, and; (d) they are best seen as a complement to rather than a substitute for control data in programme evaluation.

## 5. An Unconditional Subsidy?

There are at least two justifications for education subsidies. The first is that parents are either myopic or under-invest in their children's education because they are unable to gain the full return; this interpretation emphasises the conditionality of *Familias en Acción* – the price of education needs to be reduced to encourage parents to educate their children to the socially optimal level. The second is that there are credit constraints: parents wish to educate their children to the optimum level but they may be both unable to fund this out of current income and to borrow the necessary money. This interpretation emphasises the income effect of the grant; it relaxes the income/credit constraint allowing the family to afford more education.

The responses to the second hypothetical question (see section 3) allow us to explore the relative importance of theses two justifications. It asks the parent to state what they would do if the education component of the programme were made unconditional and were paid for all school-aged children, regardless of attendance. If very few respond that they would respond by withdrawing their children from school, credit constraints seem most important, whereas if many withdraw, conditionality would be crucial suggesting myopia and/or a failure to internalise future benefits of education to their children. Table 4 shows the responses to this question for the four age and location groups.

**Table 6*:* Response to the Conditionality Question**

| Group | Actual Attendance | Counterfactual Attendance | Withdrawal Rate |
|---|---|---|---|
| **Rural 8 - 11** | 97.50 | 95.65 | 1.85 |
| **Urban 8 - 11** | 97.82 | 97.42 | 0.40 |
| **Rural 12 - 17** | 76.15 | 74.49 | 1.66 |
| **Urban 12 - 17** | 85.68 | 85.38 | 0.40 |

Compared to the responses to Question 1, analysed in section 4, the low numbers that state they would withdraw their children if the subsidies were made unconditional in

Table 6 suggests that the programme acts mainly through relaxing credit constraints. Unfortunately, we cannot analyze the reliability of these answers due to the small percentages reporting that they would withdraw children. However, even taken at face value, this does not make the case for making the subsidy unconditional. Firstly, it would increase quite considerably the cost of the programme, particularly in rural areas. Secondly, for rural areas, the importance of the conditionality (whilst a small fraction of the impact of withdrawing the programme) is not insignificant. Finally, one must consider the longer run behavioural implications; making it unconditional may remove the link between the programme and education in the minds of parents and others. It might be harder to sustain political support for the programme, and over time, the negative impact of unconditionality may become greater if the removal of the link reduces the perceived importance of education to parents.

Due to the small numbers reporting they would withdraw their child(ren) if the programme were made unconditional (250), more detailed analysis and break-downs by family background is likely to produce estimates with large margins of error. However, it is worth noting that urbanity and distance from school remain significant indicators of the withdrawal decision.

## 6. Conclusions

This note has, we hope, served two purposes. Firstly, it has demonstrated that direct questioning of survey respondents to hypothetical policy reforms seems to provide plausible answers that are correlated in the expected manner with key household characteristics. However, we have some doubts about the reliability of data at the intra-household level, and would suggest that in future, more attention is paid to the designing of the questions. In particular, the "withdraw all" option should be replaced by requiring the respondent to state whether they would withdraw each individual child.

Secondly, if we believe the potential problems engendered by the specific question asked are relatively minor, it has highlighted that the impact of the removal of a programme may not be the same as its impact on introduction. This can be for at least two reasons. For one, underlying conditions and constraints may have changed since the programme was introduced. Furthermore, a programme is likely to have dynamic effects; children who would otherwise have left school or attended less may now be 'attached'. In this context, families might withdraw their youngest children to free up resources to support those already attached. Whilst we are unable to find any robust evidence of this occurring in this instance, we believe it this must be a consideration when thinking about abolishing a programme as it implies that some children may actually be worse off

education-wise than if it had never been introduced in the first place. How does this work relate to the current policy context of Colombia considering the withdrawal of the subsidy for primary-aged children? Whilst this work is not directly comparable it does emphasise the danger in relying on reduced form policy impact estimates in making decisions in a different context; i.e. with the policy in place and children already going through 'the system'.

This paper is only a preliminary attempt at use of direct questioning, and we plan to refine the methodology in future surveys planned for Colombia. Despite some concerns, our results are encouraging and we hope that they encourage others to assess the reliability of 'stated intentions' as a complement to the more typical quantitative methodology.

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