Tests of Income Pooling in Household Decisions

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Abstract. Using the Progresa data from Mexico, we investigate intra-household decision making using a variety of outcomes. We exploit both the experimental nature and the (short) panel dimension of the data to measure the impact of exogenous changes in the intra-household distribution of resources on household decisions. We test for global pooling of resources within households, which would correspond to the unitary model of household decision making. We also exploit a set of questions about power and the decision making process in the household to investigate aspects of strategic interactions between household members. Our findings confirm previous rejections of income pooling. We also cannot reject that the wife's relative income share is a significant determinant of the wife's decision making power in the household, with a higher share of income associated with more decision making power.

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1. Introduction

In the general inquiry into the modus operandi of households, the question of whether resources are pooled holds a particularly important status and consequently has received much attention. The answer to this question marks a line between very different representations of intra-household decision making, with implications for the shaping of tools used for policy and welfare evaluation.

The pooling of monetary resources is a necessary condition of the unitary model of household behaviour (but not of more general models). In the unitary model, household decisions are analysed under the hypothesis that the household is a single and monolitic decision unit that somehow maximizes the welfare of its members. This hypothesis is of great analytical convenience and vastly simplifies the empirical analysis, especially when data on individual members' consumptions are not measured or even hard to define. Such a hypothesis, however, is very strong, and it has been the subject of close scrutiny. If it is found that household members pool their resources, and excluding non benevolent behaviour from one of the household's members, assuming that welfare is equally or optimally distributed within the household may not be a shocking assumption, especially if the welfare considered is that of the adults of the household. However, if resources are not pooled, the unitary model of household behaviour, which precludes the analysis of the intra-household distribution of resources and welfare, becomes a very unattractive tool for policy analysis.

Although several authors using different data sets and considering different outcomes have rejected the pooling of monetary resources by adult household members (Thomas (1990), Schultz (1990), Bourguignon, Browning, Chiappori and Lechene (1993), Browning, Bourguignon, Chiappori and Lechene (1994), Browning (1995), Phipps and Burton (1998)), there exists some latitude for scepticism about tests which rely on differences in relative incomes between households in cross section data. It is always possible to argue that differences in relative incomes are not exogenous, and to rationalize rejections of income pooling as arising from some form of mis-specification of the model. Bourguignon et al. (1993), Browning et al. (1994), Phipps and Burton (1998) and Browning (1995) investigate the influence of individual earned incomes on demand or savings outcomes, whilst Thomas (1990) and Schultz (1990) test for the differential influences of individual unearned incomes on various outcomes. In both sets of tests, one can argue that incomes

reflect choices which are not independent from the outcome being investigated.

What is needed in order to test for the pooling of resources by household members is an exogenous source of variation in elements which might affect choices indirectly, but not through preferences or by shifting the household budget constraint. A government transfer policy, which redistributes resources within households is the example of such exogenous variation. To the best of our knowledge, the literature contains two tests of income pooling using such an exogenous variation in income. Both Lundberg, Pollak and Wales (1997) and Ward-Batts (2000) use the 1979 reform of child benefit in the UK to test for income pooling. Whilst this reform does constitute an exogenous change in the intra-household distribution of resources, Lundberg et al. (1997) only have access to data aggregated by income and number of children to investigate the effect of this change on the allocation of resources. Ward-Batts (2000) uses the individual household data describing the same reform. In both cases, the authors find that income pooling is rejected on demand outcomes, thereby providing the most convincing rejections of the unitary model so far.

In this paper we exploit a unique large scale data set from PROGRESA, a welfare program started in 1998 in rural Mexico, to investigate intra-household decision making using a variety of outcomes. Progresa and its evaluation sample are unique in several dimensions. While the main objective of the program is to foster the process of human capital accumulation in rural Mexico improving education, nutrition and health practices, another of its stated objectives is to improve the condition of women in rural households. For such a purpose, all the grants beneficiary households are entitled to are given to the mother (or the most senior woman) in the family. Moreover, in a number of randomly chosen villages in the evaluation sample, the program was delayed for one and a half year while data on several outcomes were collected. We exploit the experimental nature of the data to test for income pooling, and to measure the impact of changes in the intra-household distribution of resources on household decisions.

The evaluation sample collects information on a variety of outcomes that range from detailed information on income and consumption to information on who is usually in charge of various decisions within the household. In Section 2, we start by describing the program and our data. In section 3, we move on to the analysis of the data on decision making and attitudes. By exploiting the experimental nature and the panel dimension

of the data, we can check whether the program has any direct effect on the decision making process, at least as perceived by the women responding to the questions about such issues. First, they allow one to test whether perceived power is linked to the control over monetary resources, by comparing the answers about decision making of recipient and non recipient women. Second, they allow one to test whether perceived power is linked to actual or potential control over resources, by comparing the answers about decision making of recipient and future recipients in control villages. However, while interesting such analysis suffers from some important limitations. First, the questions about who makes some decisions are very simple and their interpretation might be different for different individuals. Second, it is difficult to frame the answers to these questions within a structural model and therefore to give an interpretation to any result one gets. For example, the fact that women answer that they decide together with their husbands how much to spend on say children's clothing can be rationalised either in a unitary model with income pooling or in a bargaining model without income pooling. Conversely, the fact that the husband (or the wife) decides alone, is not any more indicative: it could arise from pooling households as well as from bargaining or non cooperative households. For these reasons we also investigate the effect of the program on more traditional outcomes, namely expenditure shares.

Before presenting our tests, in Section 4 we review the unitary model and the income pooling property. Our main contribution is to exploit the exogenous variation implied by the randomization to test for income pooling. While the presence of treatment and control samples guarantees the exogeneity of the observed variation in part of the income share, we cannot ignore the fact that a large fraction of the grant is received conditional on enrolling children into school. As schooling decisions might be endogenous to expenditure shares, in Section 4 we discuss how we tackle this problem. We also need to pay attention to the fact that although participation to the program is randomised, the program leads to an increase in households' income rather than a re-distribution of resources between household members as would ideally be the case. To test for income pooling therefore, we are essentially comparing households who, by choice, were at different levels of income before the intervention, and have different levels of the income share of the wife, rather than comparing households that have a level of income which does not change between before and after the intervention, but a level of the income share of the wife which changes

because of the intervention. We discuss the impact of this on the interpretation of our tests also in Section 4.

In section 5, we report the results of our empirical tests, while Section 6 concludes with some thoughts for future research. Our results indicate a substantial rejection of the unitary model. In our opinion, our results are interesting for two different reasons. On the one hand, they give one of the first rejections of the unitary model that rests on truly exogenous variation. On the other hand, the nature of the violations of the unitary model we find makes sense intuitively.

2. THE PROGRESA PROGRAM AND THE EVALUATION SAMPLE

Progresa is a large scale welfare program implemented by the Mexican government since 1998 in its poorest rural areas. The program is unique in several respects. Firstly, it targets poverty in three dimensions thought to be complementary: education, health and nutrition. Secondly, transfers are given exclusively to women. Finally, an evaluation panel was started just before the implementation of the program in some of the villages in which the program is implemented. The evaluation contains an important randomization component we discuss below.

Progresa first targets communities on the basis of two general criteria: they have to be 'poor enough' and they have to have access to some basic infrastructure (schools, health centres) that would allow them an effective participation into the program. Within each of the eligible villages, Progresa then identifies a set of households that qualify as beneficiaries. Eligibility is determined by a set of criteria, which associates a monetary dimension of poverty to characteristics of the dwelling and the presence of children. Eligibility was first established on the basis of a Survey conducted in all Progresa localities at the end of 1997. In March 1998, before the start of the program, there was an additional survey that lead to the addition of a substantial number of households to the list of beneficiaries. These households are known as 'densificados'. Participation to the program is conditional on compliance with requirements on minimum school attendance by the children and number of visits to health care centres. The program combines monetary transfers and transfers in kind (health care and nutritional supplements).

All the beneficiary households receive a monetary sum of about ten dollars a month[1]. In addition, the households with school age children are entitled to monetary education

[1] This represents approximately 7% of monthly average household income.

grants, which constitute the largest component of the grant. These grants vary by grade and gender of the child, so as to reflect the true opportunity cost of sending a child of a given age and gender to school for a poor family in rural areas and are conditioned to school enrollment and attendance. In particular, they start at third grade and increase substantially upon completion of the sixth grade, which corresponds with graduation from primary school. This jump is justified by the fact that in rural Mexico, enrollment rates drop dramatically after sixth grade. Because girls tend to drop out of school more than boys, the grants allocated to high school girls are higher than those allocated to high school boys. These grants can constitute a substantial proportion of these households monetary income, which is often extremely low[2].

Households with infant children or pregnant women receive an additional nutritional supplement in kind. The health component of the program involves a vaccination program and check ups for children and pregnant women. Moreover, women recipients are also compelled to attend lectures on nutrition and health. Another part of the program consists in investment in health facilities and schools at the village level.

The program is now (at the end of 2001) very large. Nearly 50,000 localities and over 10 million people are now covered. The cost of the program is now close to 1 billion dollars or 0.2% of Mexican GDP. The Program has received large attention, both in Mexico and abroad, and is being copied in various Latin American countries. Within the villages, Progresa has clearly become an important fact of the village life. Anecdotal evidence seems to indicate that villagers have not been able to undo the algorithm that determines the beneficiaries. Participation into the program is far from stigmatized, but it seems that the program has created some tensions within the villages between beneficiaries and non-beneficiaries, with members of the latter group applying for participation.

To evaluate the program, the Progresa administration began the collection of an 'evaluation' sample in 506 localities in 7 states, in 1997 (just before the program was started). In each locality, all the households present were interviewed, for a total of about 25,000 households. The questionnaires administered provide detailed information on individual, household and community characteristics. Besides information on school attendance, health and nutrition, the survey contains information on labour force participation and

[2]The amounts of the monthly grants vary between 80 pesos (aproximately US$8.75) for a boy in primary school and 305 pesos (US$33.30) for a girl in secondary school. The average transfer amount is 375 pesos (US$41).

hours, incomes, transfers, expenditures, and a number of other, less traditional, outcomes regarding decision making, opinions, and women's freedom of movement and degree of socialization. At the time of writing, 5 waves of a panel covering all the households living in these villages had been collected (two waves in 1998, two waves in 1999 and one wave in 2000).[3]

Given the large size of the program, it was not possible for logistic and administrative reasons to start it simultaneously in all the 50,000 localities that qualified for it. Instead, the implementation was gradual and was completed by the end of 1999. The administration decided to put 186 localities randomly chosen among the 506 in the evaluation sample, 'at the end of the queue'. This means that in 186 villages in the evaluation panel, the program was not implemented until December 1999. This choice was done in order to have a control group equivalent to the 'treatment' group. It is possible, in the control villages, to identify beneficiary households, that is households that will qualify for the program once it is implemented. [4]

In this section we start with a description of the main features of the data. We then describe the sample we use in our tests.

2.1. The data. The data consists of 25,846 households, of which 61% reside in treatment villages, whether individually treated or not. In the control villages, we are able to identify households that will qualify when the program is implemented. Table 2.1 shows how the households are distributed between beneficiaries and non beneficiaries in control and treatment villages. Here and in the rest of the paper, we use the term "Poor" as a shortcut for poor in the sense of the program, i.e. as a potential beneficiary. Non poor households, both in control and treatment villages, are households that do not qualify for the program, either because of the level of their income, or because they do not have children of school age. Both in the control and in the treatment villages, the proportion of non poor households is about 22%. There is however, substantial variation in this proportion across villages. In some, as many as 90% of the households qualify for the program, while in others, the coverage is much lower.

[3] Two of these waves (the November 1998 and the March 1999) were contracted out to IFPRI. The others (March 1998 - which supplemented a first survey excuted in October 1997- November 1999 and April 2000) were done by Progresa. See IFPRI reports.

[4] It should be stressed, however, that households in the 'control' villages know that by December 1999 they will qualify for the program. This would be a concern for any dynamic model that would attempt to exploit the variation between treatment and control villages.

In the treatment villages, there is a significant number (3,412) of the 12,429 poor households that do not receive the program. Most of these households are "densificados", that is households who qualify for the program, but were not classified as such from the start, and experience delay in the receipt of the transfers. Even if we are not able to identify similar households in the control villages, these households should not pose a problem for the statistical analysis if the status of "densificados" is really random, as appears from the administrative branch of Progresa.

Behrman and Todd (2000) have studied the randomization of the evaluation sample comparing a large set of variables in the treatment and control villages. The conclusions that emerge from that study indicate that, by and large, the randomization worked effectively in that most village level variables (such as demographic structure, income, consumption and so on) are not significantly different between control and treatment villages. While not surprising, this evidence is comforting.[5]

Table 2.1: Proportions of Poor and Non Poor Households Progresa Data

	Poor	Non Poor	Total
Treatment Village	12429	3499	15928
	78.03%	21.97%	100%
Control Village	7708	2210	9918
	77.72%	22.28%	100%
Total	20137	5709	25846

Note: "Poor" refers to potential and actual program beneficiaries.

2.2. The sample. From this data, we select a sample of households containing at most 2 adults living maritally and any number of children. This selection considerably reduces the size of the sample used. Although nuclear families are not the norm in rural areas of Mexico, we proceed in this manner in order to be in line with previous tests of income pooling presented in the literature. After cleaning the data, and in particular dropping 732 households in which the wife's income is missing, the sample contains 7742 observations, distributed as described in the table 2.2. In our sample, the proportion of

[5] Unfortunately for the study of the effect of the program on scholl enrollment, one of the few variables for which there is a significant pre-program difference between treatment and control villages seems to be average enrollment in school. Of course, one would expect 5% false rejections of the null. We could not find any plausible explanation for such a result. Note that pre-program enrollment is higher in treatment villages than in control villages.

'non compliers' in the treatment villages is substantially lower than in the original sample: 18% (or 779) of the treatment villages poor instead of 27% in the whole sample.

Table 2.2: Proportions of Poor and Non Poor Households

Sample			
	Poor	Non Poor	Total
Treatment Village	4336	619	4955
	87.50%	12.50%	100.00%
Control Village	2388	399	2787
	85.68%	14.32%	100.00%
Total	6724	1018	7742

In addition to a number of demographic variables that we use in the empirical analysis, such as education, ethnicity, age, family composition and so on, our analysis focuses mainly on two sets of outcomes: expenditures on the one hand and life-style and decision making on the other hand. We discuss the latter at length in the next section. As for the former, we exploit the richness of the questionnaire that contains detailed data on expenditure and consumption of many commodities. These were recorded after the implementation of the program and therefore reflect its effect.

We consider eight types of non durable expenditures: food, vices (alcohol and tobacco), transportation, services, women's clothing, men's clothing, girls' clothing and boys' clothing. The questionnaire asks for consumption of food items and vices during the last week, for services and transport items over the last month and for clothing over the last six months. To compute shares, we convert all the figures to monthly expenditures. The survey records consumption of domestically produced goods, which is valued at local market prices, and added to the expenditure on the good concerned.

In Table 2.3, we report some summary statistics for the expenditure shares, including the mean, the standard deviation and the percentage of zeros. Notice the importance of food consumption in overall expenditure. With the exception of alcohol and tobacco, the goods considered are probably sufficiently broadly defined for zero expenditures to be due to infrequency of purchase rather than to tastes.

Table 2.3:
Budget shares of non durables

	Mean	Std Dev.	Min	Max	% of zeros
Food	75.73	.17	0	100	0.21
Alcohol	0.18	.02	0	47.00	97.58
Tobacco	0.30	.02	0	49.39	95.47
Transportation	4.55	.09	0	91.43	64.31
Services	15.09	.12	0	100	2.14
Women's clothing	0.77	.03	0	98.34	63.65
Men's clothing	0.88	.04	0	99.33	70.68
Girl's clothing	1.38	.05	0	98.41	45.97
Boy's clothing	1.13	.05	0	99.03	46.44

Note: The shares have been multiplied by 100.

3. AN INFORMAL INVESTIGATION INTO POWER AND DECISION MAKING

In this section we analyze the responses in the Progresa evaluation sample to a number of questions collected to gather information both on the status of women within Mexican rural families and on the process of household decision making. This information was collected as one of the intended goals of the program was the improvement of women's conditions within rural families. The purpose of this analysis therefore are two. On the one hand, we can document a number of facts about the condition of rural Mexican women that can be useful to interpret the evidence in the more formal investigation we present in the next sections. On the other, as we have data both before and after the program was started, we can check whether the program has had some direct effects on attitudes and the process of decision making, at least as perceived by the women answering the questionnaires. More precisely, we investigate whether receipt of monetary transfers is associated with more declared decision making power. We also look at whether future increases in income shares are associated with increased declared decision making power.

As we mentioned above, there is ample evidence, documented in the paper by Berhman and Todd (2000), that the randomization procedure was done properly. However, before we present evidence on the effect of the program on the decision making and life style questions, we check whether there are pre-program differences in these variables using chi square tests of independence[6]. This evidence provides a number of background facts and

[6] This tests whether the distribution of the responses is the same in treatment and control villages, ie whether the conditional and marginal distributions are the same. Under the null, $f(R|V = C) = f(R|V = T) = f(R)$.

ensures that there are no structural differences between treatment and control villages that might be wrongly attributed to the program.

3.1. Comparison between Treatment and Control before the Program. Life style and decision making questions fall into 3 categories: decision making, woman's freedom of movement and woman's freedom of opinion[7]. Respondents to all questions are the women. In the tables below, the numbers given in the first columns (of numbers) are percentages of household who answer the question presented in line by the answer presented in column. For each question, the percentages are out of the non missing respondents. The last two columns give the value of the chi square and the associated P-value[8].

Table 3.1:
Decision Making Questions before the Program

Treatment and Control Villages

Who decides:	The father /husband Cont./Treat.		The mother /wife Cont./Treat.		Together Cont./Treat.		$\chi^2(2)$	P under H_0
To send a sick child to the doctor	10.41	10.59	10.10	10.46	79.48	78.95	0.86	0.65
A child has to go to school when he/she does not want to	11.31	11.30	9.61	9.83	79.08	78.87	0.24	0.89
How to spend wife's extra money	5.72	5.87	18.65	18.42	75.64	75.71	0.32	0.85
About the household's important expend.	31.23	31.25	2.30	2.53	66.47	66.21	1.05	0.59
About expenditures for children clothing	25.13	24.34	5.67	6.30	69.19	69.15	4.14	0.13

Poor Households in Treatment and Control Villages

To send a sick child to the doctor	10.98	10.77	10.10	10.42	78.92	78.81	0.51	0.78
A child has to go to school when he/she does not want to	11.75	11.66	9.80	9.98	78.45	78.36	0.15	0.93
How to spend wife's extra money	6.11	6.11	18.13	17.99	75.76	75.90	0.05	0.98
About the household's important expend	31.23	31.38	2.39	2.42	66.38	66.20	0.06	0.97
About expenditures for children clothing	25.98	24.98	5.57	5.98	68.44	69.04	2.60	0.27

Note: All households: between 19200 and 19350 observations for each question Poor households: between 13000 and 15100.

Table 3.1 records the answers to questions about the decision making. The top part of the table shows the answers of all households in control and treatment villages and

[7] The survey also contains questions about the ownership of small livestock and garden. We do not use these questions as it is not clear what is meant by the answers.

[8] The P-value is the probability of observing a difference as large as those seen under the null hypothesis that responses are distributed identically in control and treatment villages.

the bottom part the answers of the poor households. We cannot reject that control and treatment villages are identical with respect to answers to the questions in table 3.1, both at the level of the villages and when focussing on the poor households. The results show that between 2/3 and 4/5 of households claim that decisions are made jointly by husband and wife. It is interesting to note that almost 6% of husbands are said to decide how to spend additional money received by the wife. Finally, in a third of cases, the husband is said to make the important expenditure decisions.

Table 3.2:
Women's Freedom of Movement before the Program

Treatment and Control Villages

Do you:	Yes Cont./Treat.		No Cont./Treat.		$\chi^2(1)$	P under H_0
Go out to visit relatives, female friends?	61.67	62.80	38.23	37.20	2.50	0.11
Have to ask permission from your husband to go out?	88.79	88.23	11.21	11.77	0.87	0.35
Poor Households in Treatment and Control Villages						
Go out to visit relatives, female friends?	61.88	63.39	38.12	36.61	3.48	0.06
Have to ask permission from your husband to go out?	89.60	89.14	10.40	10.86	0.50	0.48

Note: Between 9000 and 15000 observations.

A second set of questions concern women's freedom of movement. Table 3.2 shows that we cannot reject that control and treatment villages are identical with respect to answers to these questions. Almost 90% of women answer that they have to ask for permission from their husbands to go out. In another set of questions (not shown here), women are asked who accompanies them when they go out. Of the women who can go out, only 18.75% can go out on their own, whereas the other have to be accompanied, be it by their children (60.12%), husband (19.43%) or someone else (1.72%). However, it is difficult to interpret the answers to this question, as being accompanied by children might or might not indicate a restriction in freedom of movement. Note that the answers to the questions on going out are missing for about half of the sample.

Table 3.3
Opinions before the Program
Treatment and Control Villages

Do you agree with the following	Agree		Disagree		No opinion		$\chi^2(2)$	P H_0
	Cont./Treat.		Cont./Treat.		Cont./Treat.			
A woman has to be at home	55.21	56.68	43.31	41.80	1.47	1.51	4.28	0.12
A woman has to obey men	76.88	77.80	21.76	20.77	1.35	1.43	2.83	0.24
A woman can hold a job outside the home	72.27	71.49	25.54	26.24	2.20	2.27	1.37	0.51
Women and men have the same rights	89.29	88.74	7.79	8.25	2.92	3.01	1.49	0.47
Women and men can have their own opinions	87.67	86.04	8.95	9.53	3.37	4.43	15.82	0.00
A woman can have opinions about problems of the community	77.86	77.24	19.26	19.30	2.88	3.46	4.98	0.08

Note: 19362 observations.

A last set of questions elicits women's opinions on women's role in the family and in society. These questions and the percentages of women in each category are given in table 3.3. The answers to these questions are somewhat puzzling, as they paint a contradictory picture of women who, in their majority claim that their place is at home, and that they should obey men but who nonetheless are entitled to their own opinions and can have a job outside the home. When we exclude non responses, we cannot reject that control and treatment villages are distributed identically across the different modalities, except for the response to the question on women having their own opinion. Women in treatment village tend to be more often without opinion as to whether they are entitled to their own opinion.

We conclude from this analysis that there are no structural differences between control and treatment villages. Indeed, in some cases, a purely statistical reading of the tests results might lead one to conclude to the existence of significant differences between control and treatment. Given the sample sizes (several thousands of observations in each case), very small differences can appear statistically significant, when in actual fact, no one would consider them as such in substance. Here for instance, in the case of the question on whether men and women can have their own opinion, we find that the probability of observing differences between answers in control and treatment villages as large as 1.63%, 0.58% and 1.06% under the null hypothesis that control and treatment villages are identical, is zero. However, it seems rather obvious that these differences are not substantive. Indeed, control and treatment villages are essentially identical when

respectively 87.67% and 86.04% of households agree that women and men can have their own opinions.

3.2. Effect of monetary transfers on reported decision making power. To document the effect of the program on the decision process as perceived by the women, we compare the distributions of answers provided by program beneficiaries in treatment villages and potential beneficiaries in control villages, after the start of the program. Notice that tables 3.1, 3.2 and 3.3 indicated no significant differences between these two groups before the program. After the implementation of the program, instead, we find substantial differences in the distributions of answers to the decision making questions between treated and control households. Again we use chi square tests, and we find that we always reject the null hypothesis that the distribution of answers to the questions is independent from being in a treated or a control village. If we consider the details, we see that most of the results are explained by the fact that in treatment villages, husbands make less decisions on their own and more respondents answer that the decisions are made jointly. There is not much increase in the number of respondents who answer that the mother makes decision on her own. Perhaps surprisingly, the smallest effects are found in the answer to the question about who decides how to spend any additional money a woman might get (and on the decision to buy furniture).

A possible and plausible interpretation of this table is that the program, by giving income directly to the women, enhances their role in the decision processes within the households. While the differences between the treatment and control villages are not dramatic, they are significant, especially because the measurement is taken only six months after the start of the program.

Table 3.4:
Decision Making Questions After the Program
Poor Households in Treatment and Control Villages

Who decides:	The father /husband Cont.	The father /husband Treat.	The mother /wife Cont.	The mother /wife Treat.	Together Cont.	Together Treat.	$\chi^2(2)$ (P value)
To send a sick child to the doctor	13.39	10.30	11.63	11.83	74.98	77.87	31.80 (0.00)
A child has to go to school	14.66	11.36	9.98	10.28	75.36	78.36	33.33 (0.00)
Expenditures for children's clothing	27.17	24.44	4.81	5.51	68.02	70.05	15.19 (0.00)
Expenditures for the house	35.77	34.22	3.02	3.45	61.21	62.33	4.98 (0.08)
How to spend wife's extra money	2.96	2.31	30.95	31.38	66.08	66.31	5.91 (0.00)

Note: Approximately 13000 observations.

To better understand the effects of the program, we also compare how the distributions of answers of actual beneficiaries before and after the program has changed, even though this comparison reflects, in addition to the effect of the program, the effect of any 'aggregate' trend that might have had an effect on the answers to these questions. For each of the 'decision questions' considered in Table 3.4, Table 3.5 reports a matrix containing the transitions among the three possible answers. The table reports the percentages of observations in each cell, as well the two marginals, referring, respectively to the before (column) and after (row). The cells to the right of the diagonal contain observations where 'decision power' has moved towards the wife, while for the observations to the left of the main diagonal it has moved towards the husband.

Table 3.5
Decision Making Questions Before and After the Program
Beneficiaries

Before\After	He decides	Together	She decides	Total	$\chi^2(4)$	Pvalue
Who decides to send a sick child to the doctor						
He decides	1.28	8.08	1.15	10.50		
Together	7.97	62.61	8.52	79.10		
She decides	0.92	7.40	2.08	10.40		
Total	10.17	78.08	11.75		56.46	0.00
Who decides a child has to go to school						
He decides	1.33	8.98	1.02	11.33		
Together	8.89	62.08	7.59	78.57		
She decides	1.04	7.50	1.56	10.10		
Total	11.26	78.57	10.17		23.97	0.00
How to spend the wife's extra money						
He decides	0.24	3.80	1.84	5.88		
Together	1.53	51.70	22.68	76.11		
She decides	0.49	11.47	6.04	18.00		
Total	2.26	66.98	30.76		16.19	0.00
Expenditures for the house						
He decides	11.17	19.94	0.87	31.98		
Together	22.33	41.28	2.10	65.72		
She decides	0.64	1.37	0.29	2.30		
Total	34.14	62.60	3.26		45.90	0.00
Expenditures for children's clothing						
He decides	7.14	17.13	0.93	25.20		
Together	16.18	49.31	3.68	69.17		
She decides	1.02	3.95	0.66	5.63		
Total	24.34	70.39	5.27		60.79	0.00

Note: Number of observations between 6900 and 7100.

The evidence reported in Table 3.5 clearly indicates that the distributions of answers to the decision making questions before and after the program are very different in the treatment villages. For the five questions which are asked both before and after the program, the null hypothesis of independence from the treatment is rejected. Apart from the question on expenditures on children clothing, women in beneficiary household answer more often that they are responsible for decision making. The most striking change is registered for the question on spending extra money received by the wife, where the proportion of women who answer they have control goes from 18% to 30%. As the effect of the program inferred from Table 3.4 seem, for this as for other variables, much smaller, we cannot necessary interpret this increase as being caused by the program

To further investigate this issue, we turn to the analysis of the distribution of answers

in the first and second wave of the panel among the potential beneficiaries in the control villages: table 3.6 is equivalent to Table 3.5 for this group of households. It is worth stressing that the program is not implemented in the control villages.

Table 3.6
Decision Making Questions Before and After the Program
Potential Beneficiaries in Control Villages

Before\After	He decides	Together	She decides	Total	$\chi^2(4)$	Pvalue
Who decides to send a sick child to the doctor						
He decides	2.27	7.69	0.94	10.90		
Together	9.90	60.37	8.78	79.05		
She decides	1.38	6.69	1.97	10.05		
Total	13.55	74.76	11.69		69.47	0.00
Who decides a child has to go to school						
He decides	2.62	8.23	0.89	11.74		
Together	10.94	59.98	7.56	78.48		
She decides	1.04	6.99	1.76	9.78		
Total	14.60	75.19	10.20		74.23	0.00
How to spend the wife's extra money						
He decides	0.16	4.00	1.96	6.12		
Together	2.05	50.87	22.85	75.77		
She decides	0.65	11.30	6.16	18.11		
Total	2.87	66.16	30.97		9.10	0.06
Expenditures for the house						
He decides	11.80	18.51	0.97	31.28		
Together	23.91	40.89	1.67	66.48		
She decides	0.75	1.19	0.29	2.24		
Total	36.46	60.60	2.94		48.75	0.00
Expenditures for children's clothing						
He decides	8.33	16.69	1.03	26.05		
Together	17.81	47.49	3.21	68.51		
She decides	1.35	3.48	0.61	5.44		
Total	27.49	67.66	4.85		45.97	0.00

Note: Number of observations between 5400 and 5500.

It appears by comparing the distributions of answers to the decision making questions of the potential beneficiaries in the control villages before and after the implementation of the program that, as in the treatment villages, the distribution of answers has changed significantly, although no one in these villages actually participates in the program in the sense of receiving monetary transfers. The changes in the distribution of answers are quite interesting. The percentage of households in which the wife answers that decisions are made jointly has decreased for all questions. Apart from the question on spending additional money received by the wife, women in control villages answer more frequently that their husbands are the ones to decide. However, women also answer more frequently

that they are the ones to decide. Finally, most strikingly, as for the beneficiaries, the percentage of households where the wife answers that she decides how to spend extra money has gone from 18% to 30%. This is rather surprising. Such an effect could be due to some entirely different cause, which we are not aware of, which would be affecting rural Mexico at the same time as Progresa.

This evidence can be summarized by saying that, while the distribution of answers is significantly different in the two waves of the panel among the potential beneficiaries in the control villages, except for the question on who decides how to spend the wife additional income, there is no strong evidence that the decrease in 'joint' decision goes in favour of the wife. For the wife additional income question, we find, as in the treatment villages, a massive increase in the women who decide on this variable. It is worth stressing that the wording of the question in the two surveys is exactly the same.

Several interpretations of these results are possible. First, there could be an effect arising from being a potential beneficiary in the sense that future additional money received by the wife gives her more power now. As we discuss above, the control villages, were excluded from the program only for the first 2 years. Moreover the program was announced in these villages. The possibility of these announcement effects that change the outcome of the control villages are obiously important. They invalidade the simple comparison performed in Table 3.4, where treatment and control villages outcome were compared and differences were interpreted as being the effect of the program.

Alternatively, it is possible that the changes reflect some 'aggregate' trends of shocks that changed the answers to these questions between the two panels. One thing that happened both in the treatment and the control villages is the administration of the survey: it is possible that being surveyed brings about different perceptions for these women, in some form of Hawthorne effect, independently from what is happening in the treated villages, although it is not clear why this should happen.[9]

Although the possibility of anticipation effects should grant great caution, it does appear that participation in the survey has lead to changes for households in control villages that are different from the changes that occur for actual beneficiaries. Given the

[9] It is also theoretically possible that there is some sort of spill-over between the treatment and control villages. It is possible that women in the control villages are affected by what happens to women in the treatment villages, independently of the amount of resources received. However, we do not have any evidence about the interactions across villages. We believe these effects to be small.

absence of pre-program differences, the divergence should be interpreted as an effect of the program, or at least of its *immediate* rather than *delayed* implementation.

To discriminate between an effect of future monetary transfers on the one hand and spill-over or Hawthorne-type effects on the other hand, we can examine the distribution of answers of non beneficiaries in treatment villages. If the distributions of answers change for them as well, then we cannot exclude that what we observe for the potential beneficiaries is at least partially due to spill over and/or Hawthorne effects, or some other cause. If the distribution of answers for them does not change, the hypothesis of some other cause can be ruled out, and to invoque spill-over or Hawthorne effects, one would have to argue that rich and poor react differently to being surveyed.

Table 3.7
Decision Making Questions Before and After the Program
Non Beneficiaries in Treatment Villages

Before\After	He decides	Together	She decides	Total	$\chi^2(4)$	Pvalue
Who decides to send a sick child to the doctor						
He decides	1.99	6.76	0.87	9.62		
Together	10.03	63.27	6.88	80.18		
She decides	1.24	7.09	1.87	10.20		
Total	13.27	77.11	9.62		36.21	0.00
Who decides a child has to go to school						
He decides	2.17	6.73	0.84	9.73		
Together	11.53	62.82	6.64	80.99		
She decides	1.34	6.10	1.84	9.28		
Total	15.04	75.65	9.31		43.25	0.00
How to spend the wife's extra money						
He decides	0.12	3.60	1.09	4.81		
Together	1.82	52.43	21.12	75.36		
She decides	0.36	12.10	7.36	19.82		
Total	2.31	68.12	29.57		18.49	0.00
Expenditures for the house						
He decides	12.38	16.80	1.20	30.38		
Together	21.83	42.30	2.56	66.69		
She decides	0.87	1.86	0.21	2.93		
Total	35.08	60.96	3.96		17.23	0.00
Expenditures for chilren's clothing						
He decides	6.80	14.02	1.00	21.82		
Together	16.67	50.19	4.02	70.88		
She decides	1.91	4.44	0.95	7.30		
Total	25.38	68.64	5.97		30.38	0.00

Note: Number of observations between 2300 and 2500.

The results show that for non poor households in treatment villages, the percentage

of households where the wife answers that she decides how to spend extra money has also increased from 20% to 30%. For all questions, we reject the null hypothesis of independence, and the changes are qualitatively similar to those occurring for the potential beneficiaries in the control villages.

While intriguing, this evidence is difficult to interpret in terms of the real process of decision making. This is so for several reasons. First, it is not clear that all women give the same interpretation to these questions. The fact that the mode for most decision answers is the one that 'both' decide might be indicative of this. Second, even if we observe a few women in the treatment villages shifting from answering 'the husband' to answering 'both' or even 'the wife', it is difficult to interpret the quantitive meaning of these questions. The fact that a change in the income share of the women (implied by the program) seems to be related to a change in the process of decision making is suggestive of a failure of the unitary model. However, the real test is to check whether these changes are then reflected in changes in expenditure patterns that are inconsistent with the unitary model.

4. The Unitary model of household behaviour and its implications

In this section we consider the empirical implications of the unitary model of household behaviour. Obviously, this model is not novel. However it is useful to establish notation and to lay down the main issues. Moreover we discuss the empirical specifications that constitute the basis for the tests presented in the next section and some of the issues involved with our particular application.

For notational simplicity, we assume that households are composed of 2 members, A and B, who decide on the allocation of a given total x between goods which can be privately consumed by either A or B, or jointly consumed by both. Let q^A and q^B be vectors of private consumptions and Q be a vector of household public goods. The choice set could be defined to include hours of work or any other economic choice of households. In particular, we can and will allow for the presence of children, as long as they are not considered as possible decision units.

4.1. The unitary model and the income pooling property. Within the framework of the unitary model, choices are represented as if resulting from the maximization

of a utility function representing a preference ordering on choice vectors of goods consumed by household members, subject to a household budget constraint. An obvious consequence of such a model, and one that has been the focus of most tests, is the so called income pooling property. Household choices will depend only on the total income earned by the household and not on the identity and the important of the earners: the solution of a constrained maximization problem cannot depend on arguments other than those appearing in either the function being maximized or the constraint.

To formalize, the household's optimal choice (q^A, q^B, Q) is the solution of:

$$(P1) \qquad \begin{cases} \text{Max}_{q^A,q^B,Q}\ U(q^A, q^B, Q \mid z, \varepsilon) \\ \text{s.t. } p'(q^A + q^B) + P'Q = x \end{cases}$$

where z represents observable heterogeneity, such as the age and demographic composition of the household, education and region of residence, and ε represent unobservable heterogeneity; p is the vector of prices of private goods, P is the vector of prices of the public goods[10], and x is total expenditure. The solution of program $(P1)$ is the demand system:

$$\begin{aligned} q^A &= f^A(p, P, x \mid z, \varepsilon_{q^A}) \\ q^B &= f^B(p, P, x \mid z, \varepsilon_{q^B}) \\ Q &= f(p, P, x \mid z, \varepsilon_Q) \end{aligned} \tag{1}$$

Demands for goods are described by a set of functions f^A, f^B, f defined on prices, total expenditure and heterogeneity, observable and unobservable. Demand is independent of any variable y which doesn't belong to the set $(p, P, x, z, \varepsilon)$. In particular, household income $(y_m + y_f)$, individual incomes (y_m and y_f), or individual income shares ($y_m/(y_m + y_f)$) should not appear in the demand equations once demand is conditioned on total expenditure. If leisure is part of the choice vector, either as a private or public good, then x represents full time income, individual wages are elements of one of the price vectors, and the above shows that choices should not depend on the allocation of unearned income but only on its total.

Notice that the program $(P1)$ is static. One can interpret this program as part of the solution of a dynamic problem where two stage budgeting applies. Under the assumption

[10] In a cross-section, if the choice set only includes demand outcomes, prices can be neglected.

that the only function of household income that affects the allocation between savings and expenditure is its total, as it would be natural in a dynamic extension of the unitary model, one has the property that the distribution of income within the household does not affect expenditure patterns. Such a result is known as the income pooling property and is a necessary condition of the unitary model.

4.2. Exclusion restrictions and identifying assumptions for testing income pooling. The idea behind the test of income pooling that has repetedly used in the literature is straightforward and is based on estimates of the system 1. If there is a single decision unit, any variable other than prices, total expenditure or preference heterogeneity should be excluded from the system.

Most tests of the unitary model have focused on the exclusion of income distribution variables. As we mentioned above, the big problem with these tests is that such variables are likely to be correlated with unobserved heterogeneity. This correlation is therefore likely to result in spurious rejections of the unitary model. That is, it might be that income distribution variables as well as many other variables, might appear as statistically significant not because the unitary model is violated, but because of their effect on preferences and/or budget constraints. In the case of the share of woman's income in total income one could argue that, if preferences are not separable between female leisure and expenditures, one would expect an effect of female leisure (and therefore female income) on the expenditure system. And even if one focuses on households with the same female labour supply (as in Browning et al. (1994)), it is possible that cross sectional differences in women wages are correlated with unobserved heterogeneity and therefore bias the results one obtains.

A possible strategy, in such a situation, is to use instrumental variables. It should be noticed, however, that the woman income share (or other variables that might be used in testing the unitary model), is not the only potentially endogenous variable in system 1. Total expenditure should be treated as endogenous and therefore be instrumented as well. Below we also discuss the issue of schooling which, because of the conditionality of the program, must be controlled for in the demand system, and is also potentially endogenous. The issue then becomes to find instruments that allow one to estimate the parameters of system 1 and those on the additional variables (income share and schooling,

for instance) consistently. That is we would like to identify (instrumental) variables that induce variation in the schooling, income share and total expenditure variables and yet can be excluded from the system 1, in that we can confidently say that they are uncorrelated with the unobserved heterogeneity terms that form part of the residuals of system 1.

In what follows we use truly exogenous exogenous variation in the share of women income. As we discussed in the previous section, this variation is provided to us by the randomization implemented in the evaluation sample of the Progresa program: corresponding to the households in the treatment villages whose women receive a substantial boost to their income, we have statistically identical households in the control villages that we can use for comparison. As long as the randomization is done properly, as it seems to be, there is no reason to doubt the exogeneity of such variation.

While the availability of this source of variation constitutes a big advantage relative to other data sets, there are still issues with the exercise we want to perform. The first, which is related to the nature of the randomization that was implemented in the Progresa evaluation, refers to the fact that the transfer is not a re-distribution of income among household members, but rather an increase in household income exclusively in the hands of women. That is, the randomization consisted in providing some women with some additional income while others were excluded from the same benefit. For our purposes, it would have been more useful if the randomization was over who, within the beneficiaries households, would receive the transfer: the husband or the wife. Unfortunately, this type of randomization was not available.

The treatment and control households, therefore, differ in two dimensions: the share of women income and the size of total income. In principle, one can control for the latter through a standard two stage budgeting argument: we condition on total expenditure in the equations for expenditure shares. It is therefore crucial that the total expenditure is dealt with properly. For this reason, in our application, we usea flexible functional form in that we enter a polynomial in log expenditure. After trying with higher powers, we settled for a quadratic specification, similar to the one used by Banks, Blundell and Lewbell (2000) on UK data.

As we mention above, in most studies, total expenditure is treated as endogenous. Most studies, however, use as an instrument total income. If labour supply is endogenous, in that the utility function is non-separable between leisure and the commodities

considered in the demand system, it is likely that total income will be correlated with the residuals of system 1. In what follows, in addition to using income as an instrument for expenditure (as is commonly done in the literature), we have also adopted an alternative and novel strategy. In particular, we have used village level agricultural wages (and its powers) to instrument (log) total expenditure (and its powers). The village level information, which is collected in a locality questionnaire, is unlikely to be correlated with household level unobserved characteristics, especially at the aggregation level we use to construct expenditure shares. In other words, it seems to us unlikely that unobserved heterogeneity in the taste for the commodities we consider is correlated with the variation in agricultural wages across villages, especially given that we control for state effects and other observable variables. Moreover, it turns out that the agricultural wage is a good predictor of individual total expenditure, regardless of whether one controls or not for individual income. Indeed, the first step R-squared for total expenditure one gets with agricultural wage as an explanatory variable, is slightly higher than the one gets with individual income. [11]

The second problem we have to deal with is the fact that the education grant (but not the nutrion one) is conditional on kids' school attendance As attendance is likely to be related to some expenditures, this can create some problems. It is to this that we now turn.

4.3. Conditionality of the grant on schooling. As we mentioned above a large fraction of the grant that beneficiary households receive is conditional on sending their kids to school. Investment in schooling is obviously related to expenditure shares both because it involves direct costs (such as clothing, school supplies etc.) and because it might have implications for income (if children labour supply is reduced by school participation). As schooling is then correlated to receiving the grant (as the whole program is designed to increase school enrollment), a failure to control properly for this problem can seriously bias our tests.

The only solution to this problem is the parametrization of the effect of schooling choices on expenditure shares. In particular, we specify the demand system we estimate

[11] We thank the referee for forcing us to go into this direction. We have also checked whether agricultural wages are somehow related to the implementation of the program. We have found no evidence that the program induced changes in agricultural wages.

conditional on schooling behaviour.[12] That is, the conditioning variables z we consider in the demand equations will include school enrollment of children of various ages. If one is willing to assume school enrollment is uncorrelated with the unobserved heterogeneity terms in equations 1, then simply considering these additional controls would solve our problem. If one is not willing to make such an assumption, one needs to find an additional instrument for school enrollment, that is a variable that would affect school enrollment and have no direct effect on demand. While the randomization into the program provides a natural instrument, if it is the only one, our identification strategy would be jeopardized as we use that instrument to take into account the endogeneity of women income. Fortunately, the nature of the grant can help us: in addition to the randomization dummy we can use the quantity of the grant that households are entitled to as an additional dummy. Notice that there is variation across households both because differences in age and gender of the kids and because of the presence of an unconditional component for the grant.

Finally, we also instrument current schooling with lagged schooling decisions. The reason for this choice is that current school decisions might be correlated with the program (as the program was designed to increase schooling). If the effect of schooling on some expenditure items is not completely captured by our specification this might invalidade the use of our program dummy as an instrument. Lagged schooling decisions, as they were taken before the program was implemented (and announced) are not influenced by the program.

Notice that if the randomization had an additional dimension, in that some randomly selected beneficiary households the husband rather than the wife would receive the grant, the whole problem would be much simpler and we would have needed a smaller number of assumptions to test our hypothesis. As it is, we need to assume that the income effects are fully controlled by total expenditure (with its quadratic specification), that the intrumenting for total expenditure is valid and that the effect of schooling (and its instrumenting) is correctly specified. This goes to show that even when a relatively good randomized sample is available, it is necessary to use structural economic models to study most interesting economic problems.

[12] Notice that comparing households in treatment and control villages with the same school enrollment is not an option if the program has an effect on school enrollment. The randomization is done to match pre-program features of the treatment and control villages.

5. Empirical implementation and results

We report our results on testing the unitary model in Table 5.1. In addition to our favourite specifcation, we also report a set of results that use different instrumenting strategies. The motivation to do so, is to show the importance that choosing the right strategy might have and to compare our results to others that have been presented in the literature. For all specifications, we only report the estimated coefficient on the share of woman's income. Under the null of the unitary model, this coefficient should be zero for every specification. All share equations include a full set of demographic controls, log expenditure and its square and some additional controls. To save space we do not report the coefficients on these variables but they are available upon request.

In column 1 of Table 5.1 we present the results we obtain without instrumenting for the share of resources of the wife (table 5.1, specification 1). This column effectively replicates previous tests of income pooling presented in the literature. The budget shares are conditioned on the share of income of the wife, (log) total expenditure and its square, household size, number of children in school, number of children in primary school and number of female children. As we discussed above, the controls for school enrollment are important given the fact that a large fraction of the Progresa grant is conditional on school enrollment. We also condition demand on an indicator of whether the household is poor in the program sense, i.e. whether the household is a potential beneficiary, regardless of whether the household lives in a control or treatment village, and in the latter case, regardless of whether the household actually receives the transfer. Finally, we allow (and find) demands to depend on education of the wife. As is customary in these models, total expenditure is instrumented with the husband's education, age of both adult members and functions of household income (log income, its square and a dummy for low income).

The results show that income pooling is rejected. The share of income of the wife affects the structure of the expenditure significantly, with the share of food decreasing in the share of the wife's income, and the shares of expenditure on children's clothing, both for boys and for girls, increasing in the share of income of the wife. According to these results, the effect on the food budget share is quite important: on average, an increase of one percentage point in the share of income of the wife is associated with a food budget lower by close to 2 percentage points. A budget share of boy's clothing1.3 percentage points higher than average is associated to a share of income of the wife 10 percentage

points higher, and finally for girl's clothing, the effect is higher than for boys, but of the same order of magnitude, with 2.3 percentage points in the share of clothing associated to 10 percentage points in the share of income. The budget share of food is decreasing in total expenditure, except for the 25% poorest households in the sample; it is increasing in household size, and decreasing with the level of education of the wife.

The indicator of whether the household is a potential beneficiary, that is an indicator that the household is 'poor' according to the Progresa definition and has children, regardless of whether it lives in a control or treatment village, is found to be associated with different budget structure, even when we condition on total expenditure, household demographic composition and education of the wife. Potential beneficiaries have, on average, significantly lower expenditure shares of services and womens' clothing, and higher expenditure shares of food and alcohol.

We now turn to results that exploit the experimental nature of the data. and the exogenous variation in the share of income of the wife. In the second column, we instrument the share of income of the wife with the transfer received through the program. We also instrument total expenditure and its square as previously. Here again, income pooling is rejected. The results indicate that a higher share of income for the wife is associated, on average, with significantly higher budget shares of expenditures on clothing for children of both genders and with significantly lower shares of alcohol. The effect of additional income to the wife on expenditures on clothing for boys is not significantly different from the effect on clothing for girls. When the share of income of the wife is instrumented, the effect on food vanishes.

In the third specification, we instrument the share of income of the wife with the transfer, total expenditure with income and schooling with lagged schooling decisions, for the reasons discussed above. The results we report in Column 3 are not very different from those in column 2, showing that this is not a very important issue. Obviously, we cannot test the hypothesis that lagged schooling decisions are correlated with the unobserved heterogeneity included in the residuals of system

Finally, in the fourth specification, we instrument the share of income of the wife with the transfer, and school with lagged school. Total expenditure and total expenditure squared are instrumented with the average of men's agricultural wage in the village, the square of this variable and the interaction of this variable with the number of children in

the household. Interestingly, the percentage of the variance of total expenditure explained by the average agricultural wage is higher than that explained by household income. The results show that for all specifications, income pooling is rejected. Moreover, while in Columns 2 and 3, we could only find an effect of the woman's income share on children clothing (positive- both for boys and girls) and on alcohol (negative), we now find also an effect on food. The sign of this coefficient is unchanged relative to the specifications in columns 2 and 3 (but has changed relative to the OLS specification), but its size increases dramatically, so that the coefficient is now strongly significant. It is interesting to note that when one does not use the proper instruments for total expenditure (as in column 3), one gets smaller estimates of the coefficient of the women income share on food. A similar story applies to the coefficient on the woman income share in the service equation: its sign does not change relative to columns 2 and 3, but its absolute value increases dramatically and is now significantly less than zero.

The results we report in Table 5.1 are remarkably robust. We have tried several sets of control variables ans several slightly different instrumentation strategies. However, the basic facts, that is the positive effect of the share of woman's income on children clothing and food and the negative on alcohol, are remarkably resilient[13].

These results are important for several reasons. First and foremost, they constitute an important rejection of the unitary model that would be hard to explain away using endogeneity arguments. We are exploiting variation that is by nature of the randomization, truly exogenous. Second, we show that instrumenting the woman's income share is important: results do change between the various specifications in Table 5.1. The effect that one obtains when using exogenous variation in the share of woman's income (columns 2 to 4) are very different from the results one gets when using OLS (as in Column 1). We also show that instrumenting for total expenditure using a variable different from total income, can be very important- probably for the same reasons why instrumenting for the woman income share. The results in Column 4 point to a stronger rejections of the unitary model than in the previous columns. The size of the coefficients in some of the equations change substantially once we use the agricultural wage rather than total income as an instrument.

[13] We have also experimented with the answer to the question about the wife's extra money. It appears that who decides after the program has no significant effect on behaviour, whereas who decides before the program does shape the structure of expenditures.

Table 5.1

Income pooling tests: Effect of the woman income share ρ

Good	Specification 1	Specification 2	Specification 3	Specification 4
Food	-1.61	0.91	0.94	6.95
	(0.87)	(1.48)	(1.43)	(2.21)
Alcohol	0.07	-0.19	-0.20	-.30
	(0.13)	(0.09)	(0.09)	(0.16)
Tobacco	-0.07	-0.19	-0.13	-0.43
	(0.07)	(0.13)	(0.13)	(0.27)
Transportation	0.02	-0.87	-1.30	-0.32
	(0.44)	(0.73)	(0.70)	(1.25)
Services	0.91	-0.61	-0.23	-8.06
	(0.68)	(1.13)	(1.05)	(1.71)
Women's clothing	0.13	0.14	0.13	0.06
	(0.09)	(0.14)	(0.14)	(0.19)
Men's clothing	0.15	-0.02	-0.04	0.30
	(0.09)	(0.16)	(0.15)	(0.26)
Girl's clothing	0.28	0.51	0.48	1.17
	(0.10)	(0.17)	(0.16)	(0.36)
Boy's clothing	0.13	0.31	0.31	0.63
	(0.08)	(0.14)	(0.13)	(0.26)

Note: Standard errors in parenthesis.
Coefficients and standard errors have been multiplied by 100
Specification 1: ρ not instrumented. Specification 2: ρ instrumented

6. CONCLUSION

In this paper we have used a unique data set from rural Mexico to analyze empirically the process of decision making. We first present evidence on direct questions on the decision making process. This evidence indicate that an (exogenous) shift of resources towards women tends to increase the weight that women have in the decision making process: more households whose women receive the Progresa grant answer that they are responsible for a number of important expenditure decisions. We then move on to more formal tests of the unitary model by using the information on expenditures contained in the data and exploiting the restrictions of the structural models for expenditure shares.

The evidence we present rejects the unitary model. We believe our results to be important because of the use of exogenous variation in the share of women's income. This is possible because of the presence of a genuine control group in our sample. While we have to tackle a number of problems related to the fact that a large part of the grant the

'treated' women receive through the program is conditional on school enrollment which could have an effect on expenditure shares through non-separabilities, we argue that our evidence is extremely difficult to reconcile with the unitary model.

Relative to the existing literature, we also use an alternative strategy to instrument for total expenditure. Instead of household income we use village level agricultural wages. We show that the appropriate choice of instruments for total expenditure is of practical importance, as the results change considerably across the two instrumenting strategies. This evidence is consistent with the fact that instrumenting for the share of woman's income is also of practical importance.

Our research should be extended in various directions. The two most immediate avenues we are likely to explore are the following. First, we would like to test with these data some versions of the collective model, that is a model that assumes only the (static) efficient allocation of resources. Moreover, from a policy perspective it is important to indentify the 'sharing rule' that determines the allocation of resources within the household. This is so both for assessing the effect of the program we have been studying (or of alternative programs) on the welfare of the women receiving it and to assess the effect on other outcomes such as children education and more generally children welfare. We could use the same source of exogenous variation in income shares that we use to test the unitary model to identify empirically the sharing rule.

Finally, it would be interesting to extend the collective model to consider more explicitely dynamic models and dynamic concepts of efficiency.

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