Does Information Improve the Health Behavior of Adults Targeted by a Conditional Transfer Program?

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A B S T R A C T

We use data from the evaluation sample of Mexico's Food Assistance Program (PAL) to study whether including the attendance at health and nutrition classes among the requirements for receiving a transfer affects the health behavior of adults living in localities targeted by the program. The experimental trial has four different treatment types, randomly assigned to four groups of localities, one of which receives the in-kind transfer without the requirement to attend any health or nutrition sessions. Adult women living in localities where the in-kind transfer is conditional on class attendance display a significantly better health behavior than those living in localities where it is not. There is no significant evidence of changes in health outcomes among men.

I. Introduction

Drawing on the experience of *Oportunidades* (previously called PROGRESA), conditional cash transfer (CCT) programs have been introduced in many developing countries. Among other requirements, eligible households have to attend educational classes that cover health and nutrition issues. While there is well established evidence documenting the positive effect of CCT programs on health outcomes,[1] it is not clear whether the information received at health and nutrition-

1. Gertler and Boyce (2003) find that *Oportunidades* produced significant improvements in both child and adult health, measured by a reduction in the number of days of experiencing difficulty in conducting daily activities and the number of days of being confined to bed through illness. Gertler (2004) provides evidence of the effect of *Oportunidades* on child health including morbidity, height, and anemia. For a review of the effect of CCT programs on health outcomes in Latin America and Africa, see Lagarde, Haynes, and Palmer (2007).

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related discussions contributes to improve health behavior. In this paper we exploit the unique evaluation design of the Food Assistance Program, *"Programa de Apoyo Alimentario"* (PAL), implemented in rural Mexico, to study how the requirement to attend health and nutrition sessions as part of the conditionalities for receiving in-kind transfers, affects adults' propensity to have an abnormally large waist circumference, and drink and smoke heavily.

The evaluation designs implemented so far do not allow researchers to distinguish to what extent improvements in health-related indicators observed after the introduction of CCT programs are due to the increase in resource availability, and to what extent they are due to the behavioral requirements. Moreover, since most of these programs target women as the transfer recipients, and women's expenditure decisions are different from men's,[2] part of the combined effect of CCTs on health outcomes might be related to the increased bargaining power of women in their households.

Traditionally, malnutrition and infectious diseases are the main health-related burdens for developing countries. However, many of these countries are seeing dramatic increases in the incidence of obesity (see Popkin 2001) and related morbid and comorbid conditions. A distinctive feature of developing countries is that obesity rates are dramatically higher among women than men.[3] For Mexico, Fernald et al. (2004) using the 2000 National Health Survey find that the combined prevalence of obesity and being overweight is nearly 60 percent in women and more than 50 percent in men. More resources might not necessarily bring healthier lifestyles. On the one hand, an increase in the disposable income, driven by either a cash or an in-kind transfer, allows better access to health inputs (medical care and food). On the other hand, better-off individuals can buy more goods, including unhealthy food, cigarettes, and alcohol.[4] It is then crucial to differentiate the effect of the transfer per se from the effect of the behavioral requirements, such as course attendance and preventive checkups, on the propensity to engage in unhealthy behaviors.

PAL is a nutritional program that operates in poor rural localities in Mexico. According to the initial design, the evaluation sample comprises four different treatment types assigned randomly across localities, selected according to the following criteria: 50 localities as controls; 51 localities that receive transfers in-kind; 52 localities that receive transfers in-kind conditional on participation in nutrition and health education; 53 localities that receive cash benefits conditional on participation in nutrition and health education.[5] The nutrition and health education (or education

2. Attanasio and Lechene (2002) show that as the share of household income brought by the wife increases, expenditure on tobacco and alcohol falls and the expenditure on children's items increases. Rubalcava, Teruel and Thomas (2009), drawing on direct measures of inter-temporal preferences collected in the Mexican Family Life Survey (MxFLS), suggest that women have longer planning horizons.

3. Case and Menendez (2009) report that in 138 out of 194 countries for which World Health Organization (WHO) statistics on obesity are available, women are more than 50 percent more likely to be obese than men.

4. Ruhm (2000, 2005) finds that recessions improve adult health, arguing that individuals engage in healthier lifestyles during downturns as they take more exercise, and drink and smoke less.

5. Skoufias, Unar and Gonzàlez-Cossio (2008) find that, irrespective of whether the transfer is in cash or in-kind, the program generates a large increase in food and total consumption, and a significant reduction in poverty. The results in Cunha (2009) show that for food consumption the in-kind transfer is inframarginal for all households and, on average, in-kind or equivalent cash value transfers do not generate differential increases.

component) is delivered in sessions by local administrators who have received appropriate training.

In order to identify the effect of the requirement to attend health and nutrition sessions, we compare the propensity to engage in risky health behaviors of individuals who live in localities where the in-kind transfers are conditional on the attendance at health and nutrition sessions with those who live in localities where the in-kind transfers are unconditional. Our analysis does not consider those localities that received cash transfers as it is not possible to distinguish the effect of type of transfer from the effect of the education component.

We find evidence that the education requirement significantly improves the health behavior of women. Women who live in localities where food transfer (in-kind transfer) is conditional on health and nutrition education are significantly less likely to have a large waist circumference than women who live in localities where the food transfer is unconditional. The size of the effect corresponds approximately to 8 percent of the proportion of women with a large waist circumference in the group of control localities. Exploiting information on the topics covered during the sessions and the total number of courses attended, we provide evidence that both *exposure* to health [nutrition] information (as proxied by the probability of attending one or more health [nutrition]-related sessions) and *intensity* of information (as proxied by the total number of courses) reduce the probability of having an above-normal waist circumference among women. Unlike obesity, heavy drinking and smoking are not very common among women who receive PAL. Class attendance leads to a small and not statistically significant reduction in the female propensity to drink and smoke heavily. While in the great majority of cases courses are attended by women, our results show that living in a household exposed to health and nutrition information does not significantly affect the health behavior of men. Robustness checks rule out the possibility that our results are driven by differential changes in pregnancy-related outcomes, health supply, and prices. Additional tests do not support the hypothesis that women who live in localities where there is an education requirement are more reluctant to submit to having their waists measured.

This work contributes to two strands of the literature. First, our findings add to the literature on health information provision and behavior. While results for the United States do not provide conclusive evidence on the effect of information on health behavior (see, among others, Avery et al. 2007; Downs, Loewenstein, and Wisdom 2009), recently there has been increasing attention on the effect of information on health-related behavior in developing countries. Jalan and Somanathan (2008), using a randomized design in India, provide evidence that informing individuals that their drinking water is contaminated increases the probability that they will start purifying it. Dupas (2011) exploits a randomized research design that involves 328 primary schools in Kenya to show that teenagers' sex behavior changes as result of the provision of HIV risk-related information and it is more responsive to *risk reduction* than *risk avoidance* information. Finally, Lleras-Muney and Jensen (2010) use a randomized experiment in Jamaica, to show that providing information on the returns to schooling reduces the probability of smoking and heavy drinking among teenagers, but the effect is mainly explained by the fact that teenagers who spend more time in school belong to different peer networks and have less disposable income than those who work. Our work contributes to this strand of literature show-

ing that providing health and nutrition information can increase the effectiveness of in-kind transfers in improving adult health.

This study also contributes to the debate on whether transfer programs should be conditional or not.[6] Conditionalities can help to i) overcome information asymmetries on the benefits of preventive screening and health education, and ii) identify households that are less needy. However, it has been documented that imposing conditionalities contributes to significantly increased administration costs.[7] Moreover, some households may find the conditions too difficult to meet. We provide evidence that making in-kind transfers conditional on the attendance of health and nutrition courses can improve the health behavior of adults, who would not have attended in the absence of the mandatory requirement.

The paper is organized as follows. Section II provides details on PAL and the evaluation design. Section III discusses our empirical strategy. The main results are presented in Section IV. Section V presents the robustness checks and Section VI concludes.

II. Background

A. The PAL program

PAL, which began in 2004 and is still ongoing, is an intervention aimed at reducing poverty and improving the nutritional status of target households in rural localities of Mexico. PAL operates in small (population less than 2,500) localities, which are very marginalized (according to National Council for Population (CONAPO) criteria), do not receive other transfer programs, are accessible (not more than 2.5 km from a road) and close enough (not more than 2.5 km) to a DICONSA store. DICONSA is the public agency in charge of administering the program. PAL provides in-kind transfers (food baskets) to most of the 150,000 target households. An alternative cash transfer is offered to communities that DICONSA cannot reach regularly. Approximately 5 percent of PAL beneficiaries receive cash as opposed to in-kind goods. The cost to the Mexican government of both types of transfer is 150 Mexican pesos (about US$ 13) per month.[8] The food basket, which is not conditional on household size, contains powdered fortified milk, beans, rice, cornflour, soup pasta, vegetable oil, cookies, corn starch, powdered chocolate drink, ready-to-eat cereal, and sardines.[9] The contents were chosen by nutritionists and aim at providing a balanced nutritional intake of 1,750 calories per day, per household. It was originally

6. Paxson and Schady (2010) find that an unconditional cash transfer program implemented in Ecuador had a positive and beneficial effect on the physical, cognitive and socio-emotional development of children. De Brauw and Hoddinott (2011), exploiting the fact that some *Oportunidades* beneficiaries who received transfers did not receive the forms needed to monitor their children's school attendance, find that conditionality increased school enrolment and attendance.

7. Caldes, Coady and Maluccio (2006) show that monitoring conditionalities represented some 18 percent of the administrative costs related to *Oportunidades* and 2 percent of total program costs.

8. The mean share of transfer in preprogram consumption is 11.5 percent.

9. This basket of goods was distributed between June and October 2004. From November 2004 cereals were replaced by dried meat, and corn starch was replaced by lentils in order to improve variety of intake.

intended to make monthly food basket deliveries to beneficiary households; however, for logistical reasons delivery is two baskets every two months. Program rules specify that transfers should be made to women wherever possible. The program also includes a household eligibility means test criterion for the households in targeted villages.

Each village in the program is required to appoint a three member Committee of Beneficiaries. The food baskets are delivered to and stored in several warehouses, and then distributed by DICONSA to the rural communities. Eligible households collect their food baskets from the Committee of Beneficiaries. PAL beneficiaries have to attend regular courses (*platicas*) that include sessions on health-, nutrition-, and hygiene-related topics, as well as participation in program-related logistic activities. The members of the Committee of Beneficiaries, who are usually those in the communities with good levels of education, receive special training and are responsible for delivering the education sessions. Although, in principle, the courses are a requirement for the receipt of a transfer,[10] Skoufias, Unar, and Gonzàlez-Cossio (2008) report that since the start of PAL no household has been denied benefits on the grounds of not attending educational courses. The classes are meant to help empower individuals by allowing them to acquire knowledge, habits, attitudes, and practices that will encourage them to consume the right amount of food to avoid or prevent nutritional problems, such as malnutrition, anemia, vitamin A deficiency, diabetes, obesity, and hypertension.

B. Evaluation Design and Data

The evaluation is designed as an experimental community trial. The localities for the evaluation sample were chosen from the universe of the localities in the center and the south of Mexico, 18 states in total. Initially, SEDESOL selected localities from eight states (Campeche, Chiapas, Guerrero, Oaxaca, Quintana Roo, Tabasco, Veracruz, and Yucatan) but Campeche and Yucatan were excluded for receiving *Oportunidades*. The final evaluation sample consists of 206 localities from six Mexican states. These localities were randomly assigned to a "control" group of 50 localities, and three treatment groups—assigned to receive "in-kind transfers without education" (52 localities referred to as *Ink*), "in-kind transfer and education" (51 localities referred to as *InkPl*), and "cash transfer and education" (53 localities referred to as *Cash*). The means test criterion was applied to treatment villages, but the data do not include household eligibility determined through means testing.

While attendance at sessions on the logistics and the organization of the program was compulsory for all three treatment groups, the sessions on health and nutrition topics were supposed to be only for localities *InkPl* and *Cash*. However, sessions on health and nutrition were also delivered in localities *Ink*, which resulted in the contamination of the experimental design. According to González-Cossio et al. (2006) and Skoufias, Unar and Gonzàlez-Cossio (2008), this was due to poor supervision of the local program administrators. We will discuss in greater detail the extent of the contamination in Section III.

10. Households are supposed to be excluded from the program if they miss more than two consecutive courses or four in one year.

In each locality, 33 households were randomly chosen for inclusion in a pre and postintervention survey based on interviews conducted by the National Institute of Health (INSP). Data were collected two years apart: baseline in October 2003 through April 2004, and followup in October through December 2005.[11] PAL began to phase-in the transfer delivery after the baseline surveys. Receipt of the program in localities in cash and in-kind localities is self-reported in the postintervention survey. Approximately 90 percent of households receive the transfer in each of the three treatment groups.

The surveys provide extensive information at household and individual level on household consumption of food (based on seven-day recall) and nonfoods, and individual nutritional intakes of all children younger than five, and their mothers (based on 24-hour recall). At the baseline, height, and weight were measured among all children younger than five and women in the age group 12–51. In the followup survey also waist circumference was measured among all nonpregnant women aged 12 or older and men 31 or older. We also have detailed information on number, content and timing of education courses.

C. Baseline Balance and Attrition

The initial sample of adults includes 14,260 individuals aged 20–65 in 206 localities. Table 1 reports means, and differences in means, by treatment group, for key individual, household and locality characteristics as collected in the baseline survey. For each characteristic, we test whether there are significant differences between each of the treatment groups and the control group. For each variable, we also report the *F*-statistic of an OLS regression on the three treatment dummies. Standard errors are clustered at village level. Results in Table 1 show that only in one case we find an imbalance that is statistically significant at 5 percent significance level: in localities where beneficiaries were required to attend health and nutrition courses in order to obtain in-kind there is a higher fraction of males with respect to control localities. Although statistically significant, the size of the difference is small. Overall, Table 1 provides evidence that, as result of the randomized assignment, the characteristics of the control and treatment groups are balanced at the baseline.

Given the aim of this work, we do not use the group of *Cash* localities for our estimates. Therefore, our estimation sample includes individuals aged 20–65 who were interviewed in the baseline and in the followup survey in three groups of localities, namely Control, *Ink*, and *InkPl*. Some of the individuals interviewed at the baseline were not resurveyed in the followup. The attrition rate for individuals aged 20–65 was significantly higher in control localities (23.1 percent) than in the the two groups that receive the in-kind transfer: 19.6 percent in *Ink* localities and 18.3 percent in *InkPl* ones. Reassuringly, attrition rates are not significantly different between *Ink* and *InkPl* localities.[12] Descriptive statistics for the baseline character-

11. Further details about the sampling procedure can be found in Skoufias, Unar and Gonzàlez-Cossio (2008).

12. Although we are primarily interested in individual outcomes, the survey was designed as household based. Attrition rates at household level are significantly lower: 15 percent in Control localities, 10.7 percent in *Ink* localities, 10.3 percent in *InkPl* ones. These attrition rates are in line with those found by Angelucci and De Giorgi (2009) for PROGRESA. Attrition might be due to household dissolution or death, to temporary or permanent migration, or to household members being unavailable for interviews.

istics of our estimation sample are presented in Table 2. Since our identification strategy exploits the differential requirements for receiving the transfer in localities *InkPl* and *Ink*, Column 4 of Table 2 reports differences in means between these two groups of localities.

For two characteristics out of 18, we find imbalances significant at 10 percent level and this pattern holds for other characteristics not reported for reasons of space. The average age of the individuals surveyed at the baseline was just older than 38 years. Around 77 percent of them were literate and 25 percent had been educated to secondary level or older. While Spanish is the official language, there is a small percentage of individuals who only speak the indigenous language. 47 percent of the individuals in the sample declared that they had worked the week before the interview. The monthly total consumption per adult equivalent is lower in treatment localities than in control ones: on average it is about 470 pesos per month.[13] On average, around 85 percent of the individuals in our sample own at least one house, while 75 percent own plots of land. Household respondents were asked about receipt of any additional welfare program and, in the case of affirmative answers, which one. On average, at the baseline 37 percent were receiving aid from at least one program in addition to PAL. When we consider specific welfare programs, we found that the average proportion of beneficiary households did not differ significantly across the three groups.[14] Reassuringly, for each demographic characteristic the difference between *InkPl* and *Ink* localities is very small and never statistically significant. In summary, Table 2 shows that, in spite of the attrition between the baseline and the followup survey, the baseline characteristics of the estimation sample are balanced across the control group and the two groups that receive the in-kind transfer.

The prevalence of health-risk behaviors differs substantially by gender. Therefore, we present results for health-risk factors separately for men and women included in our estimation sample. In the first wave, we collected information on body mass index (BMI) only for children younger than five, and women in the age group 12–51. At the baseline, 63.2 percent of women aged 20–51 living in control localities have a BMI equal to or greater than 25 and therefore are classified as overweight.[15] There are very small and not statistically significant differences between treatment and control groups (see top panel of Table 3).

Individuals were asked whether they drank alcohol, even occasionally, and the number of drinks they had consumed in the week before the interview. According to the WHO, a woman (man) should not exceed 1 (2) units of alcohol per day. We therefore classify as heavy drinkers those women (men) who consumed 7 (14) or more drinks the week before the interview. Baseline data show an extremely low percentage of heavy drinkers among women aged 20–65 (see Table 3). These results, while potentially biased by severe underreporting, are in line with those in the National Survey of Addictions (ENA) 2002, which reports that 0.27 percent of women

13. When we compare locality medians instead of means, the differences in total consumption per adult equivalent become statistically not significant.

14. Results are available upon request.

15. We tested the balancing properties of the demographic characteristics presented in Table 2 for women in the age group 20–51 and we found results perfectly in line with those presented.

Table 1
Pretreatment Balance of Adults' Characteristics by Treatment Group: Full Sample

	Contrast by treatment status					
	Control mean	*Ink* v. Control	*InkPl* v. Control	Cash v. Control	*F*−Stat	N
Male (Y/N)	0.466	0.014*	0.016**	0.010	1.807	14,260
		(0.007)	(0.007)	(0.007)		
Age	37.259	0.136	−0.350	0.618	1.510	14,260
		(0.444)	(0.422)	(0.450)		
Head of household (Y/N)	0.423	−0.013	−0.002	−0.015	1.133	14,260
		(0.011)	(0.010)	(0.010)		
Married (Y/N)	0.516	0.005	0.048*	0.036	1.647	14,260
		(0.027)	(0.028)	(0.030)		
Literate (Y/N)	0.783	0.001	−0.003	−0.007	0.033	14,259
		(0.030)	(0.029)	(0.029)		
No schooling (Y/N)	0.198	−0.009	−0.003	0.005	0.083	14,260
		(0.028)	(0.029)	(0.027)		
Primary education (Y/N)	0.528	−0.002	0.035	0.022	0.974	14,260
		(0.023)	(0.025)	(0.023)		
Secondary education (Y/N)	0.182	0.006	−0.031	−0.018	1.354	14,260
		(0.020)	(0.020)	(0.019)		
Tertiary education (Y/N)	0.082	0.009	0.001	−0.009	0.424	14,260
		(0.016)	(0.015)	(0.014)		

Only indiginous language (Y/N)	0.028	0.034	0.001	0.004	0.557	14,260
		(0.027)	(0.023)	(0.021)		
Worked last week (Y/N)	0.481	0.001	−0.008	−0.011	0.368	14,260
		(0.013)	(0.012)	(0.013)		
Total consumption AE	474.714	−69.703*	−59.472	−69.612*	1.655	13,875
		(37.982)	(37.103)	(35.669)		
Own house (Y/N)	0.838	0.011	−0.004	−0.005	0.235	14,219
		(0.022)	(0.023)	(0.022)		
Own land (Y/N)	0.736	0.027	−0.020	0.002	0.691	14,212
		(0.032)	(0.034)	(0.034)		
Other welfare program (Y/N)	0.365	0.011	0.010	0.006	0.014	14,260
		(0.061)	(0.059)	(0.058)		
Locality Characteristics						
Fraction head literacy	0.781	−0.015	−0.004	−0.015	0.149	14,260
		(0.027)	(0.028)	(0.028)		
Fraction own casa	0.830	0.007	0.002	−0.006	0.095	14,260
		(0.023)	(0.023)	(0.024)		
Fraction own land	0.731	0.020	−0.020	−0.005	0.479	14,260
		(0.032)	(0.034)	(0.034)		

Note: The sample includes all the individuals in the age group 20–65 interviewed in the baseline survey. *Ink* denotes the localities that according to the original design receive the transfer with no requirement to attend health and nutrition sessions; *InkPl* denotes the localities that receive the transfer in-kind subject to the educational requirement; *Cash* denotes the localities that receive the transfer in cash subject to the educational requirement. Columns 2 to 4 report the coefficients and the standard errors in parenthesis of an OLS regression of the variable on three treatment dummies. Column 5 reports the $F-$test from this regression. *** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Standard errors are adjusted for clustering at locality level.

Table 2
Pretreatment Balance of Adults' Characteristics by Treatment Group: Estimation Sample

	Contrast by Treatment Status				
	Control Mean	*Ink* v. Control	*InkPl* v. Control	*InkPl-Ink*	N
Male (Y/N)	0.455	0.016*	0.015*	−0.001	8,401
		(0.009)	(0.008)	(0.008)	
Age	38.158	−0.121	−0.485	−0.364	8,401
		(0.495)	(0.493)	(0.498)	
Head of household (Y/N)	0.428	−0.004	−0.004	0.001	8,401
		(0.011)	(0.010)	(0.010)	
Married (Y/N)	0.550	0.001	0.034	0.034	8,401
		(0.028)	(0.028)	(0.024)	
Literate (Y/N)	0.774	0.002	−0.000	−0.002	8,401
		(0.032)	(0.030)	(0.032)	
No schooling (Y/N)	0.204	−0.006	−0.004	0.003	8,401
		(0.030)	(0.030)	(0.032)	
Primary education (Y/N)	0.540	−0.003	0.031	0.034	8,401
		(0.025)	(0.027)	(0.029)	
Secondary education (Y/N)	0.169	0.012	−0.021	−0.033	8,401
		(0.020)	(0.019)	(0.021)	
Tertiary education (Y/N)	0.081	0.001	−0.005	−0.006	8,401
		(0.016)	(0.015)	(0.016)	

Only Indiginous language (Y/N)	0.029	0.037	0.002	−0.035	8,401
		(0.029)	(0.024)	(0.031)	
Worked last week (Y/N)	0.470	0.004	−0.010	−0.014	8,401
		(0.014)	(0.013)	(0.014)	
Total consumption AE	473.346	−68.799*	−59.142	9.657	8,203
		(39.548)	(38.156)	(38.582)	
Own house (Y/N)	0.852	0.005	−0.010	−0.015	8,393
		(0.021)	(0.022)	(0.022)	
Own land (Y/N)	0.752	0.013	−0.038	−0.050	8,391
		(0.031)	(0.034)	(0.034)	
Other welfare program (Y/N)	0.374	0.008	0.003	−0.005	8,401
		(0.062)	(0.060)	(0.055)	
Locality Characteristics					
Fraction head literacy	0.782	−0.013	−0.003	0.009	8,401
		(0.027)	(0.028)	(0.029)	
Fraction own casa	0.833	0.006	0.000	−0.006	8,401
		(0.023)	(0.023)	(0.023)	
Fraction own land	0.739	0.015	−0.029	−0.044	8,401
		(0.032)	(0.034)	(0.034)	

Note: The estimation sample includes individuals in the age group 20–65 interviewed both in the baseline and the followup survey living in three groups of localities: Control, *Ink* and *InkPl* localities. Columns 2 to 3 report the coefficients and the standard errors in parenthesis of an OLS regression of the variable on the two treatment dummies. Column 4 reports the linear combination of the coefficients in Columns 2 and 3. *** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Standard errors are adjusted for clustering at locality level.

Table 3
Health Risk Factors at Baseline

	Contrast by treatment status				
	Control mean	*Ink* v. Control	*InkPl* v. Control	*InkPl-Ink*	N
Women					
BMI > 25	0.632	0.018	0.001	−0.017	3,247
		(0.033)	(0.035)	0.032	
Heavy drinking	0.003	−0.003*	−0.001	0.002	4,466
		(0.002)	(0.002)	(0.001)	
Smoking	0.010	0.001	−0.001	−0.002	4,472
		(0.004)	(0.004)	(0.004)	
Men					
Heavy drinking	0.044	0.003	−0.008	−0.011	3,888
		(0.009)	(0.009)	0.008	
Smoking	0.180	−0.047**	−0.033	0.014	3,892
		(0.021)	(0.023)	0.022	

Note: The sample includes individuals belonging to the estimation sample. Men and women with a BMI higher than 25 are considered to be overweight. Heavy drinking takes the value one if a woman (man) reports drinking at least 7 (14) units of alcohol in the week before the interview. Smoking takes the value one if the respondent smokes even occasionally. At baseline, BMI is collected only for women aged younger than 52. *** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Standard errors are adjusted for clustering at locality level.

aged 18–65 living in rural areas drink daily or almost daily. Drinking rates are higher among men (on average 4.4 percent in control localities). There are no significant differences in heavy drinking rates of *InkPl* and *Ink* localities, either for men or women.

All respondents aged 12 or over were asked whether they smoked, including occasionally. At the baseline, the smoking rate is extremely low for women in the age group 20–65—around 1 percent, and there are not significant differences across treatment and control groups (see Table 3). In control localities, 18 percent of men aged 20–65 admitted to smoking in the baseline survey and the proportion of male smokers is significantly lower in *Ink* localities. However, when we test differences in the smoking rates of men living in localities where the food basket is conditional on health and nutrition education and those where it is not, we do not find any significant difference.

III. Empirical Framework

In order to separate the effects of the educational component and increased resources on health-risk factors, we test whether adult behavior varied

Table 4
Program Take Up in In-Kind Localities

	At least One basket	Food Baskets	Number of courses	At Least One Organization Session	At Least One Nutrition Session	At Least One Health Session
Ink	0.93	13.049	4.179	0.437	0.593	0.371
	(0.256)	(5.049)	(4.131)	(0.496)	(0.491)	(0.483)
InkPl	0.913	13.527	4.978	0.399	0.766	0.52
	(0.282)	(5.117)	(3.903)	(0.490)	(0.423)	(0.500)
InkPl-Ink	−0.017	0.478	0.799	−0.038	0.174***	0.149***
	(0.033)	(0.498)	(0.550)	(0.049)	(0.051)	(0.053)

Note: The sample includes individuals in the age group 20–65 in *Ink* and *InkPl* localities. *** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Standard errors are adjusted for clustering at locality level.

significantly between those localities where the food basket was conditional on the attendance at health and nutrition sessions (*InkPl*) and those where it was not (*Ink*). We exclude individuals in localities where the transfer in cash was conditional on the attendance of health and nutrition sessions (*Cash*) for two reasons.

First, it would not be possible to separate the effect of educational sessions from the effect related to the type of transfer. Because food items are substitutable, there is no reason to believe that households that receive the cash transfer would buy the same items contained in the in-kind transfer even in the absence of classes that cover health and nutrition issues. Second, given the same nominal value, the value of the in-kind and cash transfer might differ in real terms. This might either be due to preprogram differences in prices across localities or to general equilibrium effects driven by the differential nature of the transfer. Cunha (2009) shows that, when valued at local prices, the value of the in-kind transfer is about 30 percent higher than the value of the cash transfer in PAL localities.

Table 4 compares the takeup of the PAL program in *Ink* and *InkPl* localities. The proportion of individuals aged 20–65 living in households that reported receiving at least one transfer is high (more than 90 percent) and is not significantly different for the two groups of localities. Since we do not have measures for eligibility, it is impossible to measure the takeup rate among those that were eligible. However, the extremely high takeup rates recorded among households eligible for other CCT programs implemented in rural Mexico[16] and the fact that eligible households had to show identification cards makes us believe that in our context the takeup should be

16. Angelucci and De Giorgi (2009) document that 97 percent of those eligible took part in the *Oportunidades* program, and the population is comparable to the PAL one.

highly correlated with the eligibility status. The average number of transfers is practically the same (around 13) for the two groups of localities.

In each household, the person who received the transfer (in 74 percent of cases it is a woman) has to answer questions on course attendance. Respondents were first asked if they ever attended courses (*platicas*) offered by the PAL. In affirmative case, they were asked: "In total, how many courses did you attend?" In the *InkPl* group respondents attended almost five courses on average, as opposed to an average of just over four in the *Ink* localities. The survey also asked: "Which topics did you cover during the educational courses?"[17] Beneficiaries in both *InkPl* and *Ink* localities were supposed to attend sessions that describe the organizational features of the program (type of benefit, timing and place of the delivery, requirements). Consistently, we did not detect any significant difference in the proportions of households that attended at least one session on the organization of the program. Health and nutrition topics were supposed to be covered only in *InkPl* localities. Due to the contamination of the evaluation sample, in group *Ink* 34 (55) percent of the respondents attended at least one session covering health (nutrition) topics. However, beneficiaries in group *InkPl* were significantly more likely to attend sessions that covered these themes: 47 (70) percent attended at least one health (nutrition) session. Despite contamination of the experimental design, households in *InkPl* localities were significantly more likely to be exposed to health and nutrition discussions than households in *Ink* localities.

As already mentioned, previous works suggest that contamination was due to poor oversight of the Committee of Beneficiaries, whose members are in charge of teaching the educational course. The potential concern is that in the group of *Ink* localities those individuals who did not comply with the initial design of the program, by attending the educational courses, were systematically different from those who complied. In order to provide evidence on this possibility, we study whether among individuals living in *Ink* localities there are systematic differences between those who did not comply with the original design and those who did. We denote as *class attendants* those individuals in *Ink* localities who reported the attendance of at least either a health or a nutrition session. These are the individuals who attended health and nutrition sessions even though their attendance was not formally required to receive the transfer. We instead classify as *class nonattendants* those individuals in *Ink* localities who were not exposed either to health or nutrition information, thus complying with the original design of the evaluation sample. The top panel of Table A1 reports means and differences in means, by attendance status, for baseline demographic characteristics in *Ink* localities. Differences are very small and never statistically significant. The middle panel in Table A1 reports differences in health-related outcomes as reported at the baseline, both for men and women. Reassuringly, class participants and class nonparticipants do not display statistically significant differences in the propensity to be overweight, to drink to excess, and to smoke.

The quality of the supervision provided by the members of the Committee of Beneficiaries might be potentially correlated with locality characteristics. The bottom

17. Respondents could select a maximum of four topics among the following: 1) organization and logistics of PAL; 2) nutrition; 3) health; 4) hygiene; 5) other subjects.

panel in Table A1 compares the locality characteristics of class attendants with those of nonattendants. For none of the characteristics displayed there is any significant difference. Similar patterns hold for other locality characteristics not reported for space reasons. In summary, the evidence presented in Table A1 does not support the hypothesis that there is systematic selection into class attendance among those beneficiaries living in localities that were supposed to receive the food basket without any educational requirement.

Our baseline specification relies on a cross sectional comparison of the effect of the program on the health behavior of all adults living in control localities with those living in *Ink* and *InkPl* localities. Formally, we estimate the following model:

$$(1) \quad Y_{ij} = \beta_0 + \beta_1 Ink_j + \beta_2 InkPl_j + \gamma' X_{ij} + u_{ij}$$

where Y_{ij} is the health-risk-related behavior of individual i in locality j recorded in the followup survey. Ink_j is a dummy variable for whether the locality j belongs to the group where the transfer in-kind is not conditional on attendance at educational sessions, and zero otherwise. $InkPl_j$ is a dummy variable that takes the value one if in the locality j receipt of the food basket is conditional on attendance at health and nutrition courses, and zero otherwise. In this specification the control localities act as the omitted category.

The parameters in Equation 1 measure Average Intention-to-Treat (AIT) effects.[18] β_1 represents the AIT effect of providing an in-kind transfer on the propensity to engage in unhealthy behaviors, while β_2 measures the AIT of providing an in-kind transfer and educational classes as a requirement. The main object of interest is the difference between β_2 and β_1 which identifies the AIT of adding health and nutrition courses as part of the requirements for receiving the transfer on the propensity to have an abnormal waist circumference, to drink to excess and to smoke. While the difference between the two parameters measures the differential effect of offering the two treatments, assessing their size separately is interesting per se. In fact, a priori it is not clear how providing food baskets with selected goods can affect the individual caloric intake. Beneficiary households might substitute the food nutrients previously consumed with those in the basket and/or increase the overall food intake. Two assumptions are needed for the identification of the AIT parameters (see Angrist, Imbens, and Rubin 1996): (1) the stable unit treatment value assumption (SUTVA); (2) the random assignment. Given the randomization at village level, a potential violation of the SUTVA would be only possible in the presence of contiguous villages belonging to different treatment groups. However, in accordance with the design of the experiment, the distances between localities included in our estimation sample make it extremely unlikely that individuals from a *Ink* locality would either share information with beneficiaries from *InkPl* locality or attend the courses in a *InkPl* locality.[19] With respect to the second assumption, the evidence provided in Tables 1 and 2 suggests that overall the randomization was successful.

18. AIT estimates provide lower bounds of the estimates of the Average Treatment on the Treated (ATT) effects.

19. Both types of contamination would attenuate the effect of living in a locality where class attendance is compulsory.

For each health outcome, we present three specifications. In the first specification X_{ij} only includes state fixed effects. In the second we add a full set of individual and household characteristics as reported in the baseline survey: age (in single year dummies), a dummy for household head status, marital status, a dummy for whether the individual is literate or not, dummies for educational attainment, a dummy for whether the individual can only speak the indigenous language, a dummy for whether the individual was working the week prior the interview, the adult equivalent monthly total consumption, dummies for asset holding (house, land), and a dummy for whether the household receives additional welfare programs. Finally, in the third specification we also control for baseline values of outcome indicators. Although randomization should result in treatment and control localities being similar across all variables, in the particular sample we consider there can be baseline differences, and controlling for demographic variables and health outcomes at the baseline is meant to capture these differences. Standard errors are always clustered at village level to account for intra-village correlated shocks.

Since we are considering the impact of the treatment on a number of outcomes, in addition to presenting the results for each individual outcome, we present two other statistics. The first is the mean difference between β_2 and β_1 across the three outcomes, computed using the methodology described in Kling, Liebman, and Katz (2007). For each outcome variable we create a standardized outcome by subtracting the control group mean and dividing by the control group standard deviation. The average effect is computed as the unweighted average of the differences $\beta_2-\beta_1$ for the three standardized outcomes. We also provide *F*-tests of the null hypothesis that the differential effect between *InkPl* and *Ink* treatments is jointly zero for all outcomes.

The estimated difference between β_2 and β_1 in Equation 1 captures the effect of including attendance at health and nutrition sessions among the requirements to receive the food basket on the propensity to engage in unhealthy behaviors. However, this difference does not measure the causal effect of health and nutrition information on adult outcomes, since there are two sources of dilution. First, noncompliance with the requirement to attend health and nutrition sessions among those who live in the group of localities where the food basket was conditional on attendance, *InkPl*. Second, attendance at health and nutrition sessions by those living in the group of localities where the food basket is unconditional on the attendance at health and nutrition sessions, *Ink*. Therefore, in the last part of the empirical section, we estimate the causal effect of the type and the intensity of the information received on the probability of having an above-normal waist circumference, of drinking to excess, and smoking. Formally, we estimate the following model:

$$(2) \quad Y_{ij} = \delta_0 + \delta_1 Info_{ij} + \gamma' X_{ij} + u_{ij}$$

where δ_1 is the parameter of interest that measures the effect of (proxies for) the information received by individual i in locality j, $Info_{ij}$. In order to overcome the endogeneity problem related to the presence of unobserved factors that are correlated with both $Info_{ij}$ and the propensity to be unhealthy, we use an instrumental variable (IV) strategy that exploits the assignment dummy for living in a *InkPl* locality rather than in a *Ink* one. Because of the contamination, health and nutrition sessions are

provided in both *InkPl* and *Ink* localities. However, only in the former one they are compulsory for receiving the transfer. Therefore, the instrument is defined as a dummy variable that takes the value one if the individual lives in a locality *InkPl*, or zero for living in a locality *Ink*. This generates an estimate of the Local Average Treatment Effect (LATE) of health and nutrition information on adult health-related behavior. Three additional assumptions are needed for identification of the LATE: (i) potential outcomes and treatment variable are jointly independent of the instrument; (ii) the instrument moves the variable $Info_{ij}$ only in one direction (monotonicity); (iii) first stage differences in $Info_{ij}$ between *InkPl* and *Ink* localities are statistically significant. The first assumption requires that making the educational courses compulsory for receiving the food basket, rather than simply offering them, can only affect adult health behavior by changing individual information about health and nutrition issues. As far as we are concerned, the assumption might be violated if beneficiaries for whom attendance is a requirement, spend more time outside home and the different time usage has an independent effect on health behavior. According to the initial design, courses should take place at the time of basket collection. Therefore, the relatively low frequency of the courses makes it extremely unlikely that there is any significant change in daily activities. With respect to the second assumption, we expect that in those locality where attendance is compulsory, beneficiaries receive at least as much information as those in localities where attendance is not compulsory. The third assumption requires the relationship between the instrument and the treatment variable to be sufficiently strong. *F*-statistics on the first stage regressions are reported in order to provide evidence on the strength of the relationship between the instrument and the treatment variable.

IV. Results

A. Average Intention To Treat Estimates

In the followup survey we measured the waist circumferences (WC) of all nonpregnant women aged 12 or over, and all men aged 31 or over. Waist circumference is a convenient and simple measure which is unrelated to height and is an approximate index of intra-abdominal fat mass and total body fat. According to the WHO, women (men) with a waist circumference equal or above 88 (102) cm[20] have an increased risk of metabolic complications. Medical evidence suggests that body fat distribution is a more important determinant of disease risk than body mass.[21] Therefore, waist circumference is becoming accepted as a more sensitive measure of obesity-related

20. Or 35 (40) inches.

21. Individuals with a high proportion of abdominal fat are at greater risk of developing diabetes mellitus type 2, coronary artery, and cardiovascular diseases. Among others, Yusuf et al. (2004), using data from the cross country study INTERHEART, finds that the effect of the BMI on the risk of myocardial infarction becomes statistically not significant once abdominal obesity (waist/hip ratio) is included in the controls in the multivariate regression.

health risk,[22] especially among menopausal and postmenopausal women.[23] In our sample, 52.5 percent of the women living in the control localities have a waist circumference of 88 cm or more, while 15.2 percent of the men living in the control localities display a waist circumference of at least 102 cm.

We start by considering how the program affects the health behavior of adult women. Columns 1, 4, and 7 of Table 5 show how the differential requirements to receive the food basket affect the probability of having a waist circumference of 88 cm or above. Since in the pretreatment survey we do not have information on the waist circumference, we include the BMI score as regressor in the specification that controls for the baseline outcome. However, baseline information on BMI is only available for women younger than 52 and the number of observations drops by about 27 percent when we estimate the specification reported in Column 7.

Living in a locality where, according to the original design, the transfer is not conditional on health and nutrition education, leads to an increase in the probability of being obese. The effect is particularly large and statistically significant at 10 percent in the specification that controls for the baseline BMI score. Among women who live in localities where the transfer is conditional on the attendance of health and nutrition sessions, there is a not statistically significant reduction in the probability of having a large WC. The effect of living an in *InkPl* locality becomes null when adding the BMI score among the regressors. Overall, those who live in localities where attendance at health and nutrition discussions is a requirement are less likely to have a large waist circumference than those who live in localities where the food basket is unconditional, with a difference that varies between 3.9 and six percentage points according to the specification. The size of the differential effect varies in the range between 7.5 percent and 11.3 percent of the proportion of obese women in the control group localities.

In Columns 2, 5, and 8 of Table 5 we report the results for heavy drinking. Living in a *Ink* locality has a negative although very small effect on the probability of drinking to excess. We find a reduction in the probability of heavy drinking also among women who live in *InkPl* localities. While the size of the *InkPl* treatment is larger than the *Ink* one, the difference between the two is never statically significant. Results presented in Columns 3, 6, and 9 of Table 5 do not find any differential treatment effect on the probability of smoking for women living in *InkPl* and *Ink* localities.

As already mentioned, in most cases women are the beneficiaries of the transfer and are in charge of attending the courses. Our results so far show that the requirement to participate in health and nutrition sessions significantly reduces the probability among women of displaying an above-normal WC, but does not significantly affect the propensity to smoke and drink heavily. There are two potential explanations for these differences across health behaviors. First, smoking and heavy drinking—as opposed to obesity—are not common among women. Second, available

22. Klein et al. (2007) using the National Health and Nutrition Examination Survey III for the United States find that 14 percent of women as opposed to 1 percent of men had a large waist circumference but a normal BMI (below 25).

23. During the menopause there is an increase in abdominal adiposity that is countered by an accelerated loss of lean mass, such that body weight should not change significantly.

Table 5
Average Intention To Treat Estimates for Women

	Abnormal WC	Heavy Drinking	Smoking	Abnormal WC	Heavy Drinking	Smoking	Abnormal WC	Heavy Drinking	Smoking
Ink	0.010	−0.002	−0.000	0.031	−0.002	−0.001	0.042**	−0.001	−0.001
	(0.027)	(0.003)	(0.004)	(0.025)	(0.003)	(0.004)	(0.021)	(0.003)	(0.003)
InkPl	−0.029	−0.005	−0.002	−0.029	−0.005*	−0.000	0.003	−0.005	−0.000
	(0.027)	(0.003)	(0.004)	(0.027)	(0.003)	(0.004)	(0.021)	(0.003)	(0.004)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Baseline outcome	No	No	No	No	No	No	Yes	Yes	Yes
N	3,465	4,339	4,345	3,371	4,227	4,233	2,474	4,206	4,216
InkPl-Ink	−0.040*	−0.003	−0.002	−0.060***	−0.004	0.000	−0.039*	−0.003	0.000
	(0.024)	(0.003)	(0.004)	(0.021)	(0.003)	(0.004)	(0.020)	(0.003)	(0.004)
Average *InkPl-Ink*	−0.045*			−0.066**			−0.050**		
	(0.027)			(0.026)			(0.025)		
Joint $F-$test	4.22			9.81			6.15		
$P-$value	0.239			0.020			0.104		

Note: The sample includes women in the age group 20–65. Abnormal WC takes the value one if a woman has a waist circumference equal or above 88 cm. Baseline controls include the following variables as reported in the baseline survey: age (in single year dummies), a dummy for household head status, marital status, a dummy for whether the individual is literate or not, dummies for educational attainment, a dummy for whether the individual can only speak the indigenous language, a dummy for whether the individual was working the week prior the interview, the adult equivalent monthly total consumption, dummies for asset holding (house, land) and a dummy for whether the household receives additional welfare programs. Baseline outcomes include respectively BMI score, heavy drinking and smoking at the baseline. BMI data at the baseline are available only for women younger than 52. *** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Standard errors are adjusted for clustering at locality level.

information for the PAL, as well as for other CCT programs,[24] suggests that smoking and heavy drinking might receive less attention than nutritional issues during the sessions.

The findings so far are consistent with the hypothesis that, while adult women who live in households not subject to the education requirements respond to an increase in available resources by increasing the amount of food intake, those who are required to attend the courses substitute, at least partially, the food items being routinely consumed, with those included in the basket. Overall, we find that the requirement to attend health and nutrition sessions reduces the probability of engaging in unhealthy behaviors among women. Irrespective of which specification we use, the Kling-Liebman-Katz average differential effect is statistically significant at conventional levels. The results for the joint *F*-test vary according to the specification we perform. However, as stressed by Duflo, Glennerster and Kremer (2007), the mean standardized treatment effect is preferable to the joint *F*-test since it is unidirectional and thus has more power to detect whether all effects go in the same direction.

Table 6 reports the results for men. Columns 1 and 4 show how the different treatments affect the probability of having a waist circumference of 102 cm or above. We cannot estimate the specification that controls for the BMI score at the baseline, since it was not collected among men. Living both in *InkPl* and *Ink* localities generates an increase in the probability of having a large waist circumference. The size of the coefficient on the dummy for receiving the *Ink* treatment is larger than the one on the *InkPl* treatment. However, the difference between the two is not statistically significant. We observe a very small increase in the probability of drinking to excess for adults in *Ink* localities with respect to those who live in control localities, and a reduction among those living in localities *InkPl* (see Columns 2, 5, and 8 in Table 6). Overall, those who live in *InkPl* localities are about one percentage point less likely to drink heavily than those who live in *Ink* localities but the effect is not statically significant. The requirement to attend health and nutrition sessions as a condition to receive the food basket reduced the probability of smoking: the differential treatment effect is about one percentage point but is not statistically significant. Overall, both the Kling-Liebman-Katz average differential effect and the joint *F*-test suggest that the requirement to attend health and nutrition sessions does not significantly affect the propensity to engage in unhealthy behaviors among men.

In order to shed further light on how the differential requirements to receive the food basket affected nutritional outcomes among adults, we study whether the program had impacts that varied at different points of the waist circumference distribution. While women (men) with a waist circumference equal to or above 88 (102) cm are considered at high risk of obesity-related diseases, medical guides advise women (men) to not gain further weight if the waist circumference is greater than or equal to 80 (95) cm. Figures 1 and 2 plot the waist circumference distribution for each of the treatment groups that received the food basket along with the dis-

24. Skoufias (2005) reports that the lectures that were a requirement of the *Oportunidades* transfer, cover some 25 themes. However, the focus is on topics relevant to mothers, including nutrition, hygiene, infectious diseases, immunization, family planning, etc.

Table 6
Average Intention To Treat Estimates for Men

	Abnormal WC	Heavy Drinking	Smoking	Abnormal WC	Heavy Drinking	Smoking	Abnormal WC	Heavy Drinking	Smoking
Ink	0.027	0.002	0.000	0.028	0.001	0.000		0.001	0.021
	(0.028)	(0.013)	(0.023)	(0.028)	(0.013)	(0.023)		(0.013)	(0.018)
InkPl	0.017	−0.006	−0.010	0.016	−0.008	−0.010		−0.007	0.011
	(0.027)	(0.015)	(0.024)	(0.025)	(0.014)	(0.024)		(0.014)	(0.019)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes		Yes	Yes
Baseline controls	No	No	No	Yes	Yes	Yes		Yes	Yes
Baseline outcome	No	No	No	No	No	No		Yes	Yes
N	2,039	3,849	3,846	1,982	3,760	3,757		3,745	3,746
InkPl-Ink	−0.010	−0.009	−0.010	−0.011	−0.009	−0.010		−0.007	−0.010
	(0.027)	(0.012)	(0.023)	(0.027)	(0.012)	(0.023)		(0.012)	(0.017)
Average *InkPl-Ink*	−0.042			−0.044					
	(0.040)			(0.040)					
Joint $F-$test	0.70			0.78			0.50		
$P-$value	0.874			0.854			0.778		

Note: The sample includes men in the age group 20–65. Abnormal WC takes the value one if a man has a waist circumference equal or above 102 cm. Baseline controls include the following variables as reported in the baseline survey: age (in single year dummies), a dummy for household head status, marital status, a dummy for whether the individual is literate or not, dummies for educational attainment, a dummy for whether the individual can only speak the indigenous language, a dummy for whether the individual was working the week prior the interview, the adult equivalent monthly total consumption, dummies for asset holding (house, land) and a dummy for whether the household receives additional welfare programs. Baseline outcomes include respectively heavy drinking and smoking at the baseline. BMI data at the baseline are not available for men. *** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Standard errors are adjusted for clustering at locality level.

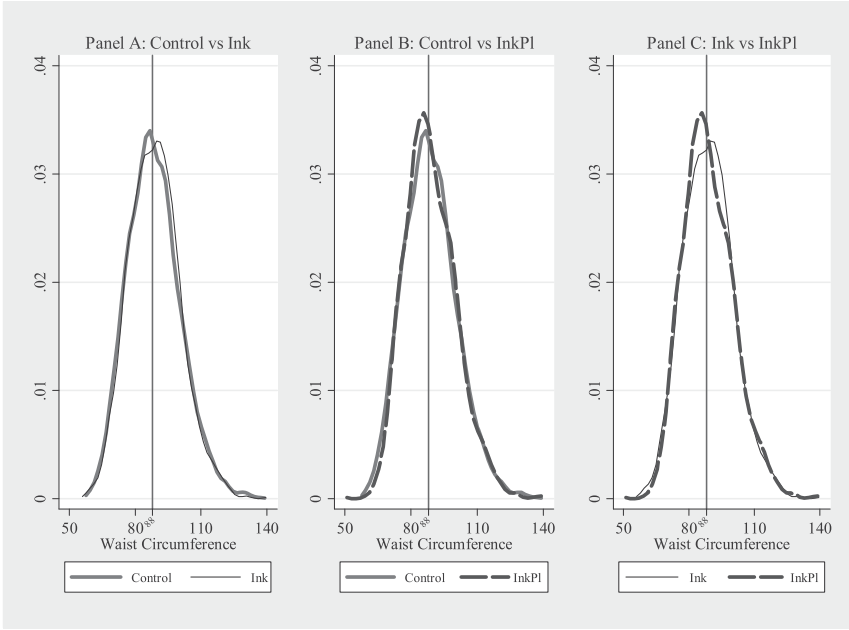

Figure 1
Women's Waist Circumference Distribution

tribution in the control group, separately for women and men. Among women, Panel A of Figure 1 shows that the waist circumference distribution in *Control* and *Ink* only differs around the threshold used to identify women who are more at risk: women in *Ink* localities are more likely to display a waist circumference above 88 cm. When we compare women in *Control* and *InkPl* we find that the latter are slightly less likely to display a waist circumference above 88 cm. However, neither the results of a Kolmogorov Smirnov test nor those of a nonparametric test on the equality of medians allow us to reject the hypothesis that the two samples were drawn from the same distribution. Finally, in Panel C we compare the waist circumference distribution in *Ink* and *InkPl* localities. The requirement to attend health and nutrition sessions induces a shift to left only in the areas of the distribution around the threshold that denotes high risk of obesity-related diseases: women in *InkPl* localities are more likely to display a waist circumference between 80 and 88 cm than those in *Ink* localities, but less likely to have a waist circumference slightly above 88 cm. In this case both a Kolmogorov Smirnov test and a test on the medians allow us to reject the hypothesis that the two samples were drawn from the same distribution. The fact that most of the effect of the educational requirement on female waist circumference distribution occurs around those values that are used as threshold to define the high risk of obesity-related health problems is consistent with the

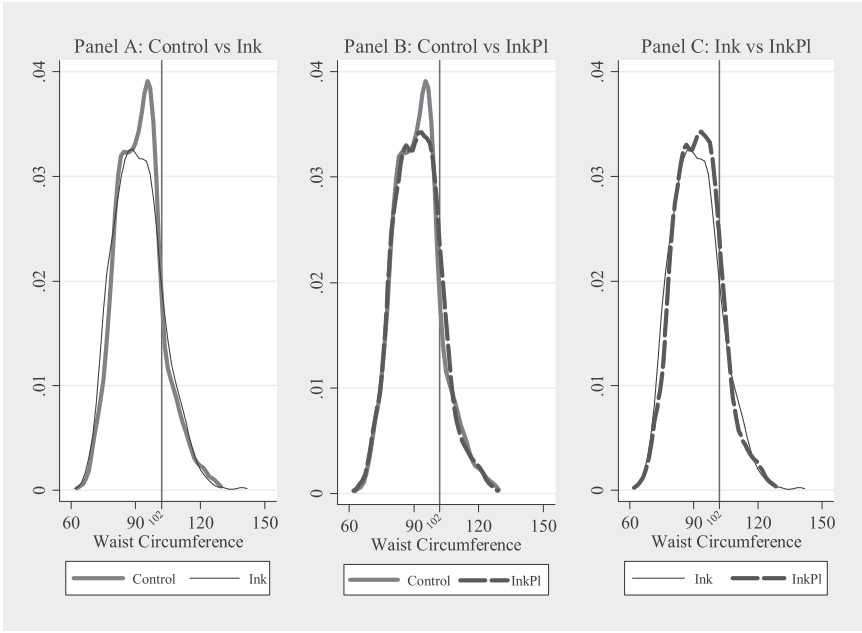

Figure 2
Men's Waist Circumference Distribution

hypothesis that women might learn about the increased risk during the health and nutrition courses.

Figure 2 reports the results for the subsample of men. The effect of living in a *InkPl* rather than an *Ink* locality increases the probability of having a waist circumference slightly below the obesity threshold, but reduces the probability of displaying extremely high values.

B. LATE Estimates

In the previous section we measured the average differential effect of living in a *InkPl* locality compared to a *Ink* one on the probability of engaging in unhealthy behaviors. However, as mentioned above, there are two reasons why this differential effect does not capture the average causal effect of health and nutrition information on adult health outcomes. First, not all beneficiaries in *InkPl* localities complied with the requirement to attend any health and nutrition sessions. Second, due to the contamination of the original design, individuals in *Ink* localities attended health and nutrition discussions.

We estimate the model in Equation 2 using two different sets of proxies for the information received. First, we exploit questions on the topics covered during the classes to construct two proxies for the type of information received. We define two

dummy variables: one for attendance at one or more health-related sessions and another for attendance at one or more nutrition-related sessions. In this way, we capture whether being exposed or not to health (nutrition) information has any effect on individual behavior. Second, in order to assess how adults' health behavior responds to the intensity of the information received, we use the total number of courses attended, as elicited during the followup interview, as a proxy for $Info_{ij}$. Courses not only include sessions that cover health and nutrition topics, but also those that discuss the organization and the logistics of the program, as well as hygiene-related topics. The last two types of sessions are unlikely to have any effect on the probability of having a large waist circumference, heavy drinking, and smoking. Therefore, the number of courses represents an imperfect measure for the intensity of the health and nutrition information.

The OLS estimation of Equation 2 may be biased by the presence of unobservable characteristics that are correlated with both the attendance at health and nutrition sessions and the propensity to engage in risky health behaviors. We treat $Info_{ij}$ as endogenous and use the randomized assignment to group *InkPl* rather than to group *Ink* as instrument. Therefore, the parameter δ_1 in Equation 2 is *just* identified. Although in the majority of cases, courses are attended by women, we can assess the existence of within household externalities by testing their effect on males' behavior. In particular, we assess separately for men and women how their health behavior responds to the information received by their household. In the regressions we control for state fixed effects and the full set of baseline individual and household characteristics.

We start by considering whether adults' behavior responds to the exposure to one or more health (nutrition)-related sessions. The OLS and 2SLS estimates are reported in Table 7. The sample is restricted to adults aged 20–65 living either in *Ink* or *InkPl* localities. The top panel shows the results for waist circumference. Columns 1 to 4 show the OLS results for each type of educational course, separately for women and men. Attending at least one health (nutrition) session reduces the probability of displaying an above-normal waist circumference among women, but the effect is small and not statistically different from zero. Being exposed to health (nutrition) information has no effect on men's propensity to have a large waist circumference. Columns 5 to 8 show the 2SLS estimates of δ_1. The attendance at one or more health (nutrition) sessions significantly reduces the probability of having a large waist circumference among women: a ten percentage point increase in the probability of attending at least one class that covers health (nutrition)-related topics lowers the probability of having a large waist circumference by approximately 3.8 (3.3) percentage points. For each type of content the effect is statistically different from zero at the 5 percent significance level. The *F*-statistics on the first stage vary in the range between 4.9 and 10.8. When the number of instruments is greater than the number of endogenous variables, Stock and Yogo (2002) find that the first stage *F*-statistics should be at least 10 in order to obtain unbiased IV estimates. This is not the case when the number of instruments is the same as the number of endogenous regressors. In fact as suggested by Angrist and Pischke (2009), in the case of a *just identified* model, a first stage not sufficiently strong would only affect the precision of the 2SLS estimates, but would not make them biased.

Table 7
Type of Information and Health Behavior

	OLS				2SLS			
	Women	Men	Women	Men	Women	Men	Women	Men
Abnormal WC								
Health session	−0.028	0.006			−0.381**	−0.124		
	(0.020)	(0.024)			(0.189)	(0.237)		
Nutrition session			−0.039	−0.016			−0.332**	−0.127
			(0.024)	(0.027)			(0.146)	(0.244)
N	2,131	1,277	2,131	1,277	2,131	1,277	2,131	1,277
F first stage					7.810	4.868	10.816	5.286
Heavy Drinking								
Health session	0.001	−0.013			−0.024	−0.035		
	(0.003)	(0.013)			(0.023)	(0.093)		
Nutrition session			0.001	0.020			−0.023	−0.035
			(0.003)	(0.014)			(0.020)	(0.093)
N	2,633	2,368	2,633	2,368	2,633	2,368	2,633	2,368
F first stage					7.848	8.102	9.756	8.517
Smoking								
Health session	−0.006	0.001			−0.001	−0.053		
	(0.005)	(0.019)			(0.032)	(0.176)		
Nutrition session			−0.009	−0.004			−0.001	−0.053
			(0.006)	(0.022)			(0.029)	(0.178)
N	2,637	2,365	2,637	2,365	2,637	2,365	2,637	2,365
F first stage					7.900	8.234	9.879	8.425

Note: The sample includes women and men in the age group 20–65 living in localities *Ink* and *InkPl*. Health (Nutr.) Sess. takes the value one if the household respondent reports the attendance at at least one health (nutrition) session. All regressions control for state fixed effects and baseline controls. Baseline controls are defined as in Tables 5 and 6. *** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Standard errors are adjusted for clustering at locality level.

The size of the effect is much smaller for men: a ten percentage point increase in the probability of being exposed to health (nutrition) information reduces the probability of a large waist circumference by 1.2 percentage points. The effect of both types of information is not statistically different from zero for men. The 2SLS point estimates are much larger for women than for men. However, the large standard errors do not allow us to reject the hypothesis that the effect of the health and nutrition sessions does not vary by gender.

In the case presented above, the LATE captures the effect of exposure to health (nutrition) information only on those individuals whose attendance at one or more health (nutrition) sessions is affected, at the margin, by the fact that the sessions are a requirement to receive the food basket.[25]

The middle panel shows how drinking behavior responds to the attendance at at least one health (nutrition) session. The OLS estimates suggest that neither among women nor among men does exposure to health and nutrition-related topics have an effect on the drinking behavior. The 2SLS estimates show that a ten percentage point increase in the probability of receiving health (nutrition) information reduces the probability of drinking to excess by approximately 0.24 (0.23) percentage points among women, but the effect is not statistically significant. Similarly, among men the 2SLS estimates find a negative, although not significant effect, of the exposure to either health or nutrition information. Finally, we consider the effect of attending at least one health/nutrition session on the probability of smoking. Consistent with the results for drinking, the OLS estimates show no effect of the type of information received. When we consider the 2SLS estimates, we find that, irrespective of which type of session we consider, exposure has a negative but very small effect for the sample of women. The size of the effect is larger for men. However, also in this case, the effects are never statistically different from zero.

Since courses are offered in both *Ink* and *InkPl* localities, the number of courses attended will differ across the two groups of localities only to the extent that attendance decisions respond to its mandatory nature in *InkPl* localities. We then assess the range of variation in the number of courses induced by our instrument. As pointed out by Angrist and Pischke (2009), the IV estimates capture the *average causal response* to changes in the treatment variable only over the range in which the instrument induces a significant variation. In order to assess how the exclusion restriction affects the number of courses attended, we plot the estimated differences between *InkPl* and *Ink* localities in the probability that the number of courses attended is either the same as or exceeds each number reported on the X axis, that is, equivalent to one minus the cumulative distribution function.[26] Women (Men) living in *Ink* localities act as reference group in the top (bottom) panel of Figure 3. While

25. For the identification of the parameter it is irrelevant that the conditionality is not enforced *ex post* by the organizers just so long as the condition is perceived as such by the beneficiaries as existing.

26. For each value on the X axis we regress the probability that the number of courses attended is equal to or above that value on the dummy variable for living in a *InkPl* locality rather than an *Ink* one and the set of controls reported in Table 7. Note that since for each household only one member answers questions on class attendance, the number of courses associated to the adults living in each household is the same. Differences in the estimates between the sample of women and men are only driven by the differences in the individual specific covariates.

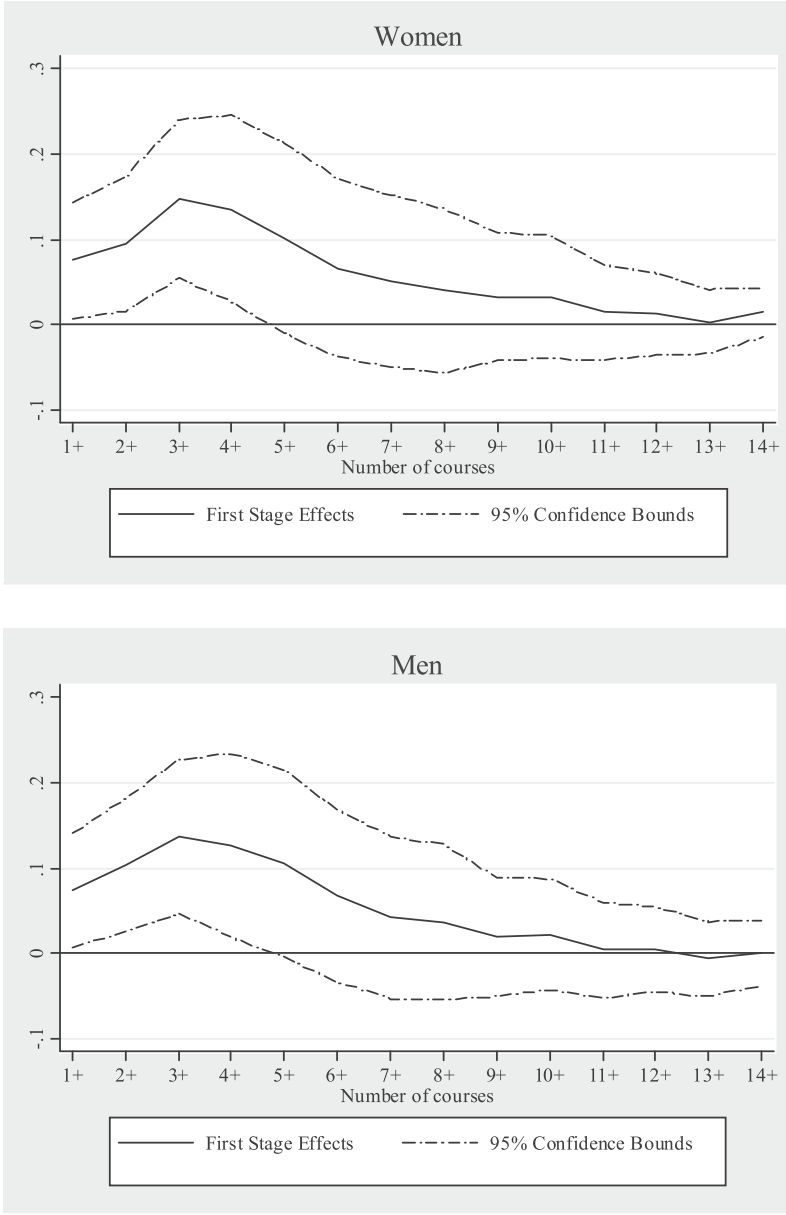

Figure 3

Heterogeneity in First Stage Effects

Note: These figures plot the estimated difference (with the relative confidence intervals) between *InkPl* and *Ink* localities in the probability of attending at least as many courses as those reported on the X axis. Details on the first stage specification are provided in the text.

the highest number of courses reported is 26, only 3 percent of the respondents report more than 14 courses attended and for expositional clarity the graphs report the difference in the attendance probability over the interval 1–14. Fig. 3 shows the positive and nonlinear effect of the instrument on the number of courses attended. Among women, those living in *InkPl* are about 15 percentage points more likely to attend three or more courses than those living in *Ink* localities and this represents the largest difference. The difference in attending six or more courses (this is the case for about 30 percent of the respondents) between the two groups of localities is no longer statistically significant. These results can be interpreted as evidence that there is a group of beneficiaries, those who attend a relatively small number of courses, for which making class attendance a requirement for receiving the transfer effectively increased their participation. On the other hand, those who attended a relatively higher number of courses would have done it irrespective of the requirement. Our results suggest that for the latter group of individuals, just offering the courses is sufficient to induce them to attend. In Table 8 we report the OLS and the 2SLS estimates of the effect of the number of courses on the three health outcomes. The OLS estimates consistently display lack of a relationship between the number of courses attended and the three health outcomes. When we control for the possible endogeneity of the number of classes, we find that an additional class reduces the probability of an abnormal waist circumference on average by 6.7 (3.1) percentage points among women (men), but the coefficients are very imprecisely estimated. The 2SLS estimates for heavy drinking and smoking display a negative but very small effect of the number of courses attended.

In summary, in this section we exploited the difference in the requirements between *InkPl* and *Ink* localities to identify the effect of exposure to health (nutrition) information—as defined by the probability of attending at least one health (nutrition) session—and of intensity of information—proxied by the number of courses attended—on adult health behavior. While, for both information exposure and its intensity, the LATE identifies the effect of the program on a subgroup of the relevant population, this parameter is of particular interest for the design of transfer programs. In contexts where monitoring compliance with conditionalities is particularly costly or where some households have a high opportunity cost of attending program-related activities, policymakers might decide to make the transfer unconditional and deliver health education on a voluntary basis. However, our results document a large effect of information on the nutritional outcomes of those beneficiaries who would have not attended in the absence of the conditionality.

Our results are consistent with the hypothesis that a high prevalence of female obesity can be explained, at least in part, by the fact that women are poorly informed about health and nutrition issues. It is important to stress that the results presented above do not isolate the effect of course attendance per se, but they capture the interaction between attendance and the in-kind nature of the transfer. Our strategy cannot distinguish between two pathways through which the information received during the classes can affect the level of health-related knowledge. It might generate a genuine increase in an individual's information set but might also add salience to a problem that is to an extent understood (see Della Vigna (2009) for a review). Since attention is a limited resource, people often use an "availability heuristic" to

Table 8
Number of Courses and Health Behavior

	Abnormal WC				Heavy Drinking				Smoking			
	OLS		2SLS		OLS		2SLS		OLS		2SLS	
	Women	Men	Women	Men	Women	Men	Women	Men	Women	Men	Women	Men
Num. Courses	−0.000	−0.003	−0.063	−0.031	0.000	0.000	−0.004	−0.009	−0.001	0.002	−0.002	−0.010
	(0.002)	(0.003)	(0.045)	(0.059)	(0.000)	(0.001)	(0.005)	(0.019)	(0.000)	(0.003)	(0.005)	(0.034)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1,993	1,191	1,993	1,191	2,469	2,211	2,469	2,211	2,473	2,208	2,473	2,208
F first stage			2.548	1.123			3.019	2.480			3.019	2.531

Note: The sample includes women and men in the age group 20–65 living in localities *Ink* and *InkPl*. Baseline controls are defined as in Tables 5 and 6. *** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Standard errors are adjusted for clustering at locality level.

weight personal experience more heavily, in decisions that involve a variety of self-protective behaviors.

C. Information and Calorie Intake

Above, we have shown that attending health and nutrition discussions may significantly reduce the propensity for a large waist circumference among adult women. A reduction in waist circumference may be due to a reduction in calorie intake or increased calorie expenditure. Cutler, Glaeser and Shapiro (2003) argue that the impressive rise in obesity observed in the United States is due primarily to increased calorie intake and that calories expended have not changed significantly. Here we disentangle the effects of the exposure to health (nutrition) information and the number of classes attended on the propensity to have an excessive calorie and fat intake. It should be stressed that this section is not meant to provide a comprehensive analysis of the effect of PAL on calorie consumption, but to assess whether the reduction in female waist circumference documented above is associated with changes in eating habits.

In the followup survey of PAL we collected individual information, based on a 24 hour recall method,[27] on the nutritional intake of children younger than five, and their mothers. We exploit the information on mothers' intake to test whether attendance at health and nutrition sessions can affect calorie consumption and fat intake. Although calorie requirements might change depending on metabolism and level of physical activity, nutritional guidelines on calorie intake provide recommendations that vary with age and gender. In Mexico the INSP advises women younger than 20 to not consume more than 2,300 kcal per day. Women between 21 and 34 should not exceed 2,000 kcal per day, women between 35 and 54 not more than 1,850, while women over 55 are advised not to consume more than 1,700 kcal per day.[28] Based on this information, we construct a binary variable for whether a woman consumes more than the recommended number of calories. Most medical guides recommend that no more than 30–35 percent of daily calories should come from fat. Therefore we exploit information on individual fat intake to construct a dummy variable that takes the value one if more than 35 percent of daily calorie intake comes from fat, 0 otherwise.

We test whether the content and the number of courses attended affect calorie consumption. In order to do that, we estimate Equation 2 using the dummies for the excessive calorie and fat intake as dependent variables.

The OLS estimates in Columns 1 to 3 of Table 9 display a small, positive, and not statistically significant effect associated with attendance at at least one health (nutrition) session and the total number of courses. When we instrument the proxies for exposure and intensity of information received, we find that both information dimensions reduce the probability of excessive calorie intake (see Columns 4–6 in

27. This relates to the type and the quantity of food consumed at home and outside the home, in the previous 24 hours.

28. Based on a 24 hour dietary recall system in a representative subsample of 2,630 Mexican women aged 12–49 from the National Nutrition Survey 1999, Barquera et al. (2003) find that the median energy consumption is 1,471 kcal.

Table 9
Information and Nutritional Intakes of Mothers

	Excessive Calorie Intake						Abnormal Fat Intake					
	OLS			2SLS			OLS			2SLS		
Health session	0.043			−0.267			0.005			−0.204		
	(0.034)			(0.285)			(0.026)			(0.284)		
Nutrition session		0.007			−0.221			−0.005			−0.169	
		(0.040)			(0.223)			(0.031)			(0.233)	
Num. courses			0.002			−0.050			0.000			−0.041
			(0.004)			(0.073)			(0.004)			(0.067)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	860	860	808	860	860	808	860	860	808	860	860	808
F first stage				5.105	8.269	1.171				5.105	8.269	1.171

Note: The sample includes mothers with at least one child aged five or younger, living in localities *Ink* and *InkPl*. Excessive Calorie (Abnormal Fat) Intake is the dummy for whether a woman has a higher than recommended calorie (fat) intake (see text for explanation). Health (Nutr.) Sess. takes the value one if the household respondent reports the attendance at at least one health (nutrition) session. Baseline controls are defined as in Tables 5 and 6. *** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Standard errors are adjusted for clustering at locality level.

Table 9). However, none of the effects is statistically different from zero. Arguably, the high standard errors of estimates in Table 9 are driven by the relatively small sample sizes. We find similar results when we look at the effect of information on the probability on an abnormal fat intake. The OLS estimates (Columns 7–9) display basically no effect of either exposure to health (nutrition) information or the total number of courses attended. Also in this case, the 2SLS estimates display negative, but not statistically significant effects of the proxies for the information received.

Attending health and nutrition sessions might also affect the propensity to burn calories. As stressed by Cutler, Glaeser, and Shapiro (2003), there are two components to calorie expenditure: voluntary exercise and involuntary expenditure associated with employment. Attendance at health-related sessions might result in an appreciation of the benefits of physical activity. Unfortunately, the survey does not collect information on time usage. With respect to involuntary calorie expenditure, it is unlikely that attendance at health and nutrition sessions affects the decision to work in more energy intensive jobs. Skoufias, Unar and Gonzàlez-Cossio (2008), studying the effect of PAL on labour outcomes, find no significant difference between localities *Ink* and *InkPl*.

In summary, these results suggest that the health and nutrition information received through the program led to a large, although not statistically significant, reduction in the probability of excessive calorie and fat consumption.

V. Econometric Concerns

A. Potential Confoundings

So far we have interpreted the differences in the health outcomes of women living in *InkPl* and *Ink* localities as a result of the increased health and nutrition information. Since our estimates suggest a large and statistically significant effect only on female waist circumference, our results might potentially reflect a differential effect of the program on pregnancy-related outcomes. During the classes women might learn about contraception and family planning and this might generate a larger reduction of pregnancy rates in those localities where attendance is a binding requirement for receiving the transfer. Alternatively, women might learn during the classes how to lose postpartum weight. In order to test whether different treatment status affected pregnancy differentially, we estimate the model in Equation 1 using as dependent variable the probability that a woman gave birth in the previous 12 months. Results reported in Columns 1 and 2 in Table 10 show that living in a *InkPl* locality rather than in *Ink* one has no effect on the probability of having a child aged 12 months or younger.[29] According to the information provided in the followup survey, beneficiary households have been exposed to the program for at most 23 months. We check whether our results are the artifact of a faster postpartum weight loss, by testing whether the effects of the program on waist circumference among women who did not have a child during the program phase in—the previous 23

29. When we restrict the sample to women who have recently completed their fertility spell—49 or below—the difference between *InkPl* and *Ink* is almost identical to the one displayed.

Table 10
Test for Changes in Pregnancy-related Outcomes

	Child 12 months or younger (Y/N)		Abnormal WC for women with no kids 12 months or younger		
Ink	−0.001	0.006	0.011	0.031	0.041*
	(0.009)	(0.008)	(0.027)	(0.025)	(0.021)
InkPl	0.000	0.005	−0.030	−0.030	0.002
	(0.009)	(0.008)	(0.027)	(0.027)	(0.021)
State fixed effects	Yes	Yes	Yes	Yes	Yes
Baseline controls	No	Yes	No	Yes	Yes
			No	No	Yes
N	4,780	4,495	3,427	3,335	2,445
InkPl-Ink	0.001	−0.001	−0.040*	−0.061***	−0.039*
	(0.009)	(0.008)	(0.024)	(0.022)	(0.020)

Note: In Columns 1 and 2, the sample includes women aged 20–65. In Columns 3–5 the sample is restricted to those women aged 20–65 who did not report having any child in the 23 months before the followup interview. Baseline controls are defined as in Tables 5. *** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Standard errors are adjusted for clustering at locality level.

months—are consistent with those we found for the entire female sample. Results on waist circumference for this restricted sample are reported in Columns 3 to 5 in Table 10 and are almost identical to those reported in Columns 1, 4, and 7 in Table 5. In summary, the evidence presented in Table 10 does not support the hypothesis that the effects of PAL on female waist circumference are driven by changes in pregnancy-related outcomes.

A second potential concern is that in localities where the transfer is subject to attendance at health and nutrition discussions, there might have been differential improvements in health supply or increased attention to nutrition-related diseases among health professionals. For instance, women who attend health centres in *InkPl* localities might be more likely to be reminded by doctors or nurses about the risks related to obesity. In order to rule out this confounding factor we test whether there are differential treatment effects on alternative health outcomes: the probability of being diagnosed as having hypertension, diabetes, and for each of these two conditions the probability of being advised a treatment after diagnosis. Since it is unlikely that in the short run the program can affect the risk of contracting diabetes and hypertension, differential effects on the prevalence of the two conditions would suggest that there are differences in the probability of their detection. All things being equal, medical guidelines for health professionals operating in *InkPl* localities might put more emphasis on the treatment of obesity-related diseases. The results in Table 11 show that there is no significant evidence of differential treatment effects for any of the health outcomes described above.

Table 11
Test for Changes in Health Supply

	Hypertension	Diabetes	Advised Hypertension Treatment	Advised Diabetes Treatment
Women				
Ink	0.006	−0.011	−0.041	−0.016
	(0.015)	(0.016)	(0.053)	(0.049)
InkPl	0.003	−0.003	0.010	−0.030
	(0.017)	(0.015)	(0.056)	(0.050)
State fixed effects	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes
N	4,206	2,637	5,86	279
InkPl-Ink	−0.003	0.008	0.051	−0.014
	(0.016)	(0.017)	(0.051)	(0.045)
Men				
Ink	0.001	0.009	0.214***	0.018
	(0.010)	(0.014)	(0.077)	(0.074)
InkPl	−0.000	−0.007	0.103	0.034
	(0.010)	(0.012)	(0.086)	(0.071)
State fixed effects	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes
N	3,738	1,931	258	144
InkPl-Ink	−0.001	−0.016	−0.111	0.016
	(0.009)	(0.013)	(0.087)	(0.090)

Note: The sample includes women and men in the age group 20–65. *Hypertension* (*Diabetes*) takes the value one if the individual has been diagnosed as hypertensive (diabetic). *Adv. Hyp. Treat.* (*Adv. Diab. Treat.*) takes the value one if the individual has been advised treatment for hypertension (diabetes), after diagnosis. Baseline controls are defined as in Tables 5 and 6. *** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Standard errors are adjusted for clustering at locality level.

Beneficiary households in localities belonging to groups *Ink* and *InkPl* receive the same food baskets. Therefore, we do not expect any differential change in prices between the two groups of localities. The followup survey included detailed questions about the prices of 67 items in the locality questionnaire. Table A2 shows that we did not detect any significant difference in the prices of unhealthy goods (chocolate, candies, biscuits) and healthy goods (fish). Results not displayed for other food items are in line with those presented.

The above results suggest that the differential effect of the program on female waist circumference cannot be explained by differential changes in either health supply or food prices.

Table 12
Nonresponse Rates by Health Outcomes

	Contrast by Treatment Status			
	Control Localities	*Ink* v. Control	*InkPl* v. Control	*InkPl-Ink*
Women				
Waist circumference	0.246	−0.023	−0.027	−0.004
		(0.030)	(0.028)	(0.029)
Heavy drinking	0.041	−0.004	−0.012	−0.008
		(0.013)	(0.011)	(0.011)
Smoking	0.039	−0.004	−0.012	−0.008
		(0.012)	(0.011)	(0.011)
Men				
Waist circumference	0.339	−0.047	−0.040	0.006
		(0.032)	(0.033)	(0.031)
Heavy drinking	0.015	−0.001	−0.000	0.001
		(0.006)	(0.006)	(0.006)
Smoking	0.015	0.000	0.001	0.000
		(0.006)	(0.006)	(0.006)

Note: Nonresponse rates are calculated among those individuals who have been reinterviewed in the followup survey. *** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent.

B. Attrition

In Section II we provided evidence that the proportion of individuals not resurveyed in the followup survey does not vary significantly across treatment and control groups. However, we might be selectively missing those men and women who do not report their health behaviors. Though at the baseline there should be no differences between treatment and control groups along this dimension, in the followup individuals might be more reluctant to reveal their drinking and smoking behavior and allow their waist circumference to be measured. In those localities where the transfer is conditional on attendance at health and nutrition sessions, there might be an increased social stigma toward unhealthy behaviors. If obese women are concerned about their waist circumference being disclosed during the educational courses, we might systematically miss women in *InkPl* localities with a large waist circumference.

The top panel of Table 12 shows that 24 percent of the women reinterviewed in the followup survey did not get their waist circumferences measured. The fraction of women for which we cannot observe the waist circumference is lower both in *Ink* and *InkPl* localities, respectively 22.3 percent and 21.9 percent, and differences are not statistically significant. In the baseline survey the BMI score was missing for 18.8 (18) percent of the women younger than 52 living in *Ink* (*InkPl*) localities.

These results do not support the hypothesis that in localities where attendance at health and nutrition sessions is compulsory there is a higher stigma toward female obesity. Much lower rates of nonresponse were observed for drinking and smoking. The differences in nonresponse rates between *Ink* and *InkPl* are very small and not statistically different from zero.

Waist circumference is missing for 33.9 percent of the men living in control localities. Consistent with the results for women, rates are lower in *Ink* and *InkPl* localities, but differences are not statistically significant (see bottom panel of Table 12). Rates of nonresponse are basically null for drinking and smoking.

In order to test whether individuals for which waist circumference is not reported differ in terms of unobservable characteristics that are correlated with the propensity to engage in unhealthy behaviors, we test whether they are more or less likely to report heavy drinking and smoking at the baseline. In Table A3 we report the results of OLS regressions of drinking and smoking behavior on the dummy for not reporting waist circumference. Women and men for which the waist circumference measure is not available were not more likely to report either drinking at excess or smoking in the pretreatment interview. Results do not change when we allow the correlation to vary according to the treatment status of the locality.

VI. Conclusions

It has been documented that CCT programs have strong positive effects on the well-being of beneficiary households, but little is known about how the individual components of these programs contribute to the combined result. This paper assessed the impact of an education requirement in a conditional transfer program implemented in rural Mexico on adult health behavior. We exploited the randomized evaluation design of the Food Assistance Program, PAL, to study how attendance at health and nutrition courses affects the propensity to smoke, drink heavily, and display a large waist circumference, in male and female adults.

Our findings provide evidence that the requirement to attend health and nutrition sessions contributes to an improvement of health behavior among women, mainly driven by a reduced risk of obesity-related illnesses. We do not find significant evidence of changes in the propensity to engage in unhealthy behaviors among men. In order to shed light on the mechanisms that drive the reduced probability of an abnormal waist circumference among women, we study whether attendance at nutrition-specific sessions reduces the probability of excessive calorie and fat intakes among mothers with at least one child younger than five. Overall, our results support the hypothesis that the requirement to attend content-specific classes, either by increasing the level of information or stressing the relevance of already known nutritional issues, can improve women's eating habits.

Our findings suggest that improved nutrition-related outcomes, especially among particular subgroups of the target population, can be achieved if the increased resources are accompanied by improved health/nutrition knowledge. Previous work has documented that, by targeting women as the transfer recipients, CCTs reduce household consumption of unhealthy goods and increase food and child-related expenditure. However, provision of specific information seems essential to achieve an

effective improvement in the nutritional outcomes of all household members. While women seem to take advantage of the information they acquire through the sessions, our results are suggestive that exposure to health and nutrition information does not significantly affect men's health behavior.

More generally, we show that lack of information can contribute to explaining the dramatically high prevalence of female obesity in developing countries. Policies addressed to improving health knowledge can have large and significant effects.

Table A1
Baseline Balance in Ink *Localities by Attendance Status*

	Class Attendants	Class Nonattendants	Diff	N
Male (Y/N)	0.467	0.472	−0.005	2,587
	(0.499)	(0.499)	(0.010)	
Age	38.227	37.844	0.383	2,587
	(12.161)	(12.662)	(0.644)	
Head of household (Y/N)	0.425	0.428	−0.003	2,587
	(0.494)	(0.495)	(0.014)	
Married (Y/N)	0.553	0.556	−0.003	2,587
	(0.497)	(0.497)	(0.029)	
Literate (Y/N)	0.779	0.769	0.010	2,587
	(0.415)	(0.421)	(0.041)	
No schooling (Y/N)	0.197	0.204	−0.007	2,587
	(0.397)	(0.403)	(0.041)	
Primary education (Y/N)	0.542	0.508	0.035	2,587
	(0.498)	(0.500)	(0.034)	
Secondary education (Y/N)	0.176	0.200	−0.024	2,587
	(0.381)	(0.401)	(0.027)	
Tertiary education (Y/N)	0.081	0.086	−0.005	2,587
	(0.273)	(0.281)	(0.017)	
Only indiginous language (Y/N)	0.067	0.074	−0.007	2,587
	(0.250)	(0.262)	(0.043)	
Worked last week (Y/N)	0.468	0.477	−0.010	2,587
	(0.499)	(0.500)	(0.018)	
Total consumption AE	402.938	404.255	−1.317	2,525
	(356.760)	(372.047)	(43.893)	
Own house (Y/N)	0.858	0.847	0.011	2,587
	(0.349)	(0.360)	(0.026)	
Own land (Y/N)	0.763	0.761	0.002	2,587
	(0.426)	(0.427)	(0.040)	
Other welfare program (Y/N)	0.398	0.318	0.081	2587
	(0.490)	(0.466)	(0.051)	

(continued)

Table A1 *(continued)*

	Class Attendants	Class Nonattendants	Diff	N
Health Behavior of Women				
BMI > 25	0.640	0.649	−0.009	987
	(0.480)	(0.478)	(0.038)	
Heavy drinking	0.000	0.002	−0.002	1,368
	(0.000)	(0.045)	(0.002)	
Smoking	0.007	0.016	−0.010	1,369
	(0.082)	(0.127)	(0.006)	
Health Behavior of Men				
Heavy drinking	0.050	0.046	0.005	1,212
	(0.219)	(0.209)	(0.014)	
Smoking	0.146	0.117	0.029	1,211
	(0.353)	(0.321)	(0.020)	
Locality Characteristics				
Frac. Head Literacy	0.766	0.769	−0.002	2,587
	(0.141)	(0.148)	(0.030)	
Frac. Own Casa	0.836	0.845	−0.009	2,587
	(0.118)	(0.113)	(0.019)	
Frac. Own Land	0.752	0.755	−0.003	2,587
	(0.157)	(0.160)	(0.032)	

Note: The sample includes individuals in the age group 20–65 in *Ink* localities. *Class Attendants* attended at least either a health or a nutrition session. *Class Nonattendants* did not attend any health or nutrition session. *** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Standard errors are adjusted for clustering at locality level.

Table A2
Effect on Prices

	Chocolate	Candies	Biscuits	Fish
Ink	0.430	−0.635	−3.763	3.178
	(7.005)	(5.383)	(6.796)	(5.424)
InkPl	−1.579	4.278	−9.327	−0.929
	(7.445)	(5.985)	(6.314)	(5.521)
N	149	149	149	149
InkPl-Ink	−2.008	4.913	−5.564	−4.107
	(7.130)	(5.657)	(6.155)	(5.104)

Note: The prices of these items are unavailable for 4 localities: 1 in the control group, 1 in the group *Ink,* and 2 in the group *InkPl*. *** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Prices are expressed in *pesos*.

Table A3
Missing Waist Circumference and Health Behavior at the Baseline

	Heavy Drinking		Smoking	
Women				
Missing WC	−0.000	−0.001	−0.001	−0.002
	(0.002)	(0.003)	(0.003)	(0.005)
Ink		−0.003		−0.000
		(0.002)		(0.004)
*Ink**Missing WC		−0.000		0.004
		(0.003)		(0.008)
InkPl		−0.001		−0.001
		(0.002)		(0.004)
*InkPl**Missing WC		0.001		−0.001
		(0.005)		(0.008)
N	4,468	4,468	4,474	4,474
Men				
Missing WC	0.006	0.010	0.013	0.014
	(0.007)	(0.014)	(0.012)	(0.023)
Ink		0.001		−0.040
		(0.013)		(0.025)
*Ink**Missing WC		0.004		−0.015
		(0.019)		(0.027)
InkPl		−0.000		−0.037
		(0.013)		(0.027)
*InkPl**Missing WC		−0.016		0.009
		(0.017)		(0.031)
N	3,889	3,889	3,893	3,893

Note: Missing WC takes the value one for those individuals reinterviewed in the followup survey for which the waist circumference is not available. *** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Standard errors are adjusted for clustering at locality level.

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