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Is the Allocation of Resources within the Household Efficient? New Evidence from a Randomized Experiment

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I study whether households make Pareto-efficient intrahousehold resource allocation decisions. Combining randomized variation in women's income generated by the evaluation of the Mexican PROGRESA program with variation attributable to localized rainfall shocks as distribution factors in the collective model, I find evidence favoring Pareto optimality. More specifically, female-specific income changes have positive effects on children's goods expenditures, whereas changes due to rainfall shocks have a smaller influence on household public goods expenditures. The evidence is consistent with female partners having greater sensitivity to own-income changes and norms that oblige women to devote their earnings to meet collective consumption needs.

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I. Introduction

It is well known that households are not perfectly harmonious entities in which individual preferences are subordinated to common goals and in which resources go to a common pool and are then channeled toward the best uses of the family. In particular, a substantial body of research indicates that familial allocation decisions are affected by the resources that individual decision makers bring to the table (e.g., Thomas 1990; Schultz 1990; Duflo 2003; see Duflo [2005] for a recent survey). This evidence has given support to theories of intrahousehold decision making that highlight the role of individuals' decision-making power in influencing these decisions. However, there is no clear consensus as to the nature of the interactions governing the intrahousehold resource distribution process. On one hand, a general characterization of intrahousehold interactions—the collective rationality model—is based on the assumption that households achieve a Pareto-efficient allocation of resources, irrespective of which bargaining mechanisms determine household members' decisions (Chiappori 1992). In contrast, a growing theoretical literature argues that decision makers' intrahousehold resource allocation decisions may be Pareto inefficient as a result of the imperfect enforceability of marital contracts (Lundberg and Pollak 1993, 2003; Ligon 2002; Basu 2006) or because of information asymmetries among partners within households (Bloch and Rao 2002; Ashraf, forthcoming).

It is not obvious that the allocation of resources within households must be Pareto efficient; noncooperative interactions in the provision of household public goods may undermine this, for example. Yet evidence from a number of studies assessing households' consumption patterns in developed countries suggests that Pareto efficiency is attained (Browning et al. 1994; Browning and Chiappori 1998; Chiappori, Fortin, and Lacroix 2002). Against this, empirical tests of the model among West African households provide evidence consistent with Pareto inefficiency.[1] This diverging evidence across developed and developing country contexts may arise from a variety of reasons: because partners in West African households tend to have their own individual budgets, control resources, and make consumption decisions fairly indepen-

[1] Udry (1996) finds a degree of inefficiency in households' agricultural production decisions. In addition, Duflo and Udry (2004) show that household members in Côte d'Ivoire seem unable to provide full insurance against shocks to their individual incomes—consistent with imperfect enforceability of risk-sharing contracts among partners. Other studies counter some of these results (see Rangel and Thomas 2005; Akresh 2008).

dently; or because researchers emphasize different tests of predictions of the collective familial decision-making model.[2]

In this paper, I propose and implement a novel quasi-experimental research design to assess the validity of the collective model of intrahousehold allocation decisions. According to the collective approach, allocation decisions are partly determined by individual decision makers' power within the household; this "power" function or sharing rule may be a function of partner-specific incomes, marriage market forces (i.e., sex ratios), and legislation influencing the division of marital goods upon divorce (Chiappori 1992; Browning et al. 1994; Browning and Chiappori 1998; Chiappori et al. 2002). Essentially, the sharing rule determines the distribution of income between partners, which can then be allocated by each decision maker to maximize his or her own utility. Since the theory assumes that these distribution factors affect allocation decisions strictly through their influence on the sharing rule, the ratio of household consumption good responses to distribution factor changes—in this case, changes in partner-specific incomes—must be equal to the ratio of distribution factors' influence on the resource sharing function. As a consequence, the ratio of partial derivatives of consumption goods demand with respect to each type of partner-specific income, conditional on the level of household resources, should be equal across all goods (Browning and Chiappori 1998; Bourguignon, Browning, and Chiappori 2009).

The empirical content of the theory relies on this proportionality property and thus imposes demanding requirements on empirical tests of the model. The ideal experiment would require the random assignment of income transfers to different decision makers in the household and compare households' resulting consumption choices. In practice, however, partner-specific income effects are often difficult to identify, because individual incomes may result from variation in prices or other possibly unobserved factors that independently affect household resource constraints or preferences.[3]

Following the ideal research design, I exploit exogenous variation in two factors that separately manipulate women's and other household income. I use data from the experimental evaluation of the PROGRESA program, a conditional cash transfer program in Mexico that provides income transfers to women in low-income households (contingent on

[2] Tests of Pareto efficiency in developed country contexts mostly consider tests of static models in household consumption decisions, whereas those among West African households have concentrated on tests of intrafamilial risk-sharing arrangements and household members' individual production decisions.

[3] In addition, all distribution factors, by changing partners' options outside current marriages, tend to increase divorce rates in the short run, leading to a potentially high degree of selection of households that maintain marital relations. See Behrman (1997) for a survey of the literature.

certain requirements in terms of children's school attendance and family members' use of various health services) and in which communities participating in an evaluation of the program were randomly assigned to early phase-in (treatment) and late phase-in (comparison) groups. Combining the randomized variation generated by the PROGRESA program evaluation with income variation attributable to localized rainfall shocks, which manipulates general household income, I assess whether household members' resource allocation decisions are Pareto efficient. More specifically, using these variables as the two distribution factors in a system of household consumption goods demand functions, I test these restrictions of the collective model. My tests provide no evidence against the main testable prediction of Pareto efficiency—that the ratio of distribution factor effects is equal across all public, collective, and individual private goods. The evidence thus favors the collective rationality approach to intrahousehold resource allocation: although different sources of income are allocated to different uses depending on the identity of the income earner, this does not prevent the household from achieving an efficient allocation of family resources.

The study takes an additional step toward improving our understanding of households' allocation patterns in rural Mexico, which seem to result from the differential claims that decision makers have on various forms of income. In particular, anthropological and sociological research has shown qualitative evidence that among most low- and moderate-income households in Mexico, male partners tend to control their own earned income, while contributing to a household common fund used to cover basic household expenditures. However, most female partners' incomes go entirely into the household's common fund; social norms may oblige them to devote their earnings to meet collective rather than individual consumption needs (Whitehead 1981; Roldán 1987).

The evidence from my empirical exercise is consistent with this "separate accounts" interpretation of the household's resource distribution process. I find that increases in income specific to female partners have substantial positive effects on expenditure shares in children's clothing as well as adult female clothing expenditures—clearly identifiable measures of child expenditures and female-specific private goods, respectively. In contrast, income changes due to rainfall shocks influence expenditures on household public goods to a much lesser extent. This is consistent with the idea mentioned above that social norms among poor Mexican households may oblige women to devote their earnings to meet collective rather than individual consumption needs or with women's marginal willingness to pay for children's goods being more sensitive to changes in their decision-making power than that of other decision makers within the household (Blundell, Chiappori, and Meghir 2005).

Nonetheless, one can conclude that these allocation patterns do not prevent a Pareto-efficient allocation of family resources.

Finally, I also examine the robustness of the estimates to various threats to validity. First, I construct and implement an alternative test of collective rationality by adopting an empirical model that allows a more powerful test of the proportionality condition—the *z*-conditional demand system approach.[4] Under the additional assumption that an observable distribution factor has a strictly monotone influence on one consumption good, one can construct a system of conditional demand equations as a function of total expenditures, preference factors, the consumption good assumed to be monotonically influenced by the distribution factor, and all additional distribution factors except the one identified above. The intuition behind this modeling strategy is that the level of the conditioning good provides sufficient information as to how the household equilibrium moves along the efficiency frontier when the balance of power is modified, and thus the other distribution factors provide no additional information about movements along the household's efficiency frontier, under the collective model assumptions (Donni and Moreau 2007; Bourguignon et al. 2009). I implement this approach and show that the model's predictions are maintained under this more robust empirical strategy. I also show that my results are robust to potential threats to validity due to the program's information and social marketing campaigns and potential effects of the rainfall shocks on prices and wages.

The paper is organized as follows: Section II gives an overview of the context—a brief survey of the ethnographic literature on marital resource allocation norms and patterns of landownership in rural Mexico. Section III presents a general version of the collective model, followed by its main testable implications. Section IV provides a description of the PROGRESA conditional cash transfer program as well as of the data used in the analysis, followed in Section V by the empirical implementation of the model, the study's research design, and the main identifying assumptions. The central empirical results of the paper and robustness evidence from the tests in favor of the Pareto efficiency assumption are presented in Section VI. The paper concludes in Section VII with a discussion aimed at a reconciliation of the existing evidence.

[4] Multiple relatively recent studies have exploited this alternative modeling approach; see Dauphin and Fortin (2001), Dauphin, Fortin, and Lacroix (2006), Donni (2006), and Donni and Moreau (2007).

II. Gender, Individual Incomes, and Household Budget Allocation Patterns in Rural Mexico

Gender inequality in familial relations is widespread in rural Mexico. It pervades multiple realms of everyday life in the Mexican countryside—from landholding patterns, to allocation decisions within the household, to various other areas of economic and social interactions (e.g., Wolf 1959; Elmendorf 1972; Chiñas 1992). Of particular interest to my work, anthropologists have documented that partners have differential claims on the various forms of income earned by household members. Among most low-income Mexican households, male partners tend to control their own earned income, while contributing to a household common fund used to cover basic household expenditures (e.g., Benería and Roldán 1987). This body of literature argues that men, who in many cases make significantly greater cash incomes than their female counterparts, may do this because they try to keep information about their income levels private and thus hold back significant amounts for personal consumption. This strategy may allow male partners to control how much they contribute to household expenses and to what specific ends.

In contrast, most female partners' incomes go entirely into the household's common fund; social norms, or an ideology of "maternal altruism," may oblige them to devote their earnings to meet collective rather than individual consumption needs (Whitehead 1981; Roldán 1987).[5] Husbands can also raid the pool for additional personal spending on items such as alcohol while vetoing other household expenditures such as basic household needs (Benería and Roldán 1987). It is considered by many female partners that disagreement over the amount of a husband's personal income is one of the leading causes of domestic violence between spouses (Castro 2004).

The relatively large degree of gender inequality within households may be a response to women's limited asset-holding opportunities. Although during the nineteenth and early twentieth centuries women had rights to own and manage privately inherited land, with the post-revolution communal land (*ejido*) reforms, women effectively lost sole prop-

[5] That said, in her observations in a Mixtec village in southern Puebla, Mindek (2003*b*) finds that women are very active in negotiations over household decisions, and many control a substantial amount of partner and children's earned incomes.

Ethnographers also document a complex relationship between men and women's contribution to the household income and the degree of conflict and control over partners' contributions to household expenditures or household public goods. Among lower-income families, where the male partner's labor income is barely sufficient to satisfy the households' basic economic needs, families tend to follow an allocation pattern that puts a greater emphasis on contributions to the common fund, whereas households in which the male partner's income is substantially higher tend to organize resource allocation decisions on the basis of housekeeping allowances provided to the female partner.

erty rights over *ejido* land, since only heads of households—male partners in most cases—were members of the *ejido* (Deere and León 2001*a*, 2005).[6] Moreover, the *ejido* reforms of the 1990s, which have provided land titles to individuals in the sector, have further eroded rural women's land rights. Familial landholdings are becoming the individual private property of the *ejidatario*, thus further transferring property rights in the majority of cases to male partners (Deere and León 2001*b*). The implications of these reforms are evident in my data: as of October 1998, only 3 percent of women in landowning households had property rights over any land, and only 3 percent of total household land was owned by females. Nonetheless, this is a context in which males have relatively strong usufruct or full ownership rights over land (in contrast with rural West African households, in which women own substantial amounts of land but their individual property rights over land are relatively weak; Goldstein and Udry 2008). These relatively secure land property rights may operate in ways that promote a Pareto-efficient allocation of resources in productive activities.

These intrahousehold allocation patterns are consistent with the motivations for a growing theoretical literature that argues that decision makers' intrahousehold resource allocation decisions may be Pareto inefficient as a result of the imperfect enforceability of marital contracts (Ligon 2002; Lundberg and Pollak 2003; Basu 2006), such as conflict over partners' contributions to household public goods (Lundberg and Pollak 1993), or because of information asymmetries among partners within households (Bloch and Rao 2002). Also, these claims on individual-specific incomes may induce significantly greater expenditures on household public goods from female partners' incomes (relative to their male partner's or collective incomes). As will be discussed below, these higher marginal expenditures on children from women's income—a pattern common in both developing and developed country households—were exploited in the design of the PROGRESA program to encourage the targeted funds to be allocated toward child investments and expenditures (Skoufias 2001).

III. Theoretical Framework and Empirical Implications[7]

In this section, I discuss a general version of the collective model of intrahousehold allocation recently proposed by Bourguignon et al. (2009). This model incorporates the argument that individual partners may care differently about the allocation of private and household pub-

[6] Patterns of land inheritance include female participation throughout indigenous communities in Mesoamerica, as seen in a survey of ethnographic evidence (Robichaux 1994, 1997).

[7] This section draws heavily on Deaton (1997) and Bourguignon et al. (2009).

lic goods and that the different sources of income may influence the allocation of resources in a quite general form. Moreover, the model encompasses all cooperative bargaining models that take Pareto efficiency as an axiom, while providing general empirical predictions regarding the Pareto efficiency of allocations.

Consider a static version of the collective model for a two–decision maker ($i = A, B$) household. Assume that there are various types of commodities that individuals consume: q^A and q^B, two private and assignable goods; a vector of household public goods, K; and a vector of Hicksian composite goods, C, which may be consumed privately, publicly, or both ($C = C^A + C^B + C^H$). As first proposed by Weiss and Willis (1985), we can think of children as collective consumption goods from the point of view of the parents. Individual preferences are then represented by utility functions $u_A(q^A, q^B, C^A, C^B, C^H, K; \mu, \xi)$ and $u_B(q^B, q^A, C^B, C^A, C^H, K; \mu, \xi)$. The vectors μ and ξ represent, respectively, observed and unobserved heterogeneity in individual and household characteristics and preferences that influence individual utilities. Furthermore, assume that a set of z observable variables, named distribution factors, affect consumption choices directly and not through preferences or the budget constraint. These variables are important because their influence on behavior provides the testable restrictions for the collective model in the context of this study.

Following the Pareto efficiency assumption of intrahousehold allocation decisions, any efficient allocation of resources in the household can be characterized as the solution to the program

$$\max_{q^A, q^B, C^A, C^B, C^H, K} u_A(q^A, q^B, C^A, C^B, C^H, K; \mu, \xi) + \lambda u_B(q^B, q^A, C^B, C^A, C^H, K; \mu, \xi)$$

$$\text{subject to } p^A q^A + p^B q^B + p^C C + K \leq (\omega^A + \omega^B)T + y^A + y^B + y^O,$$

$$\lambda = \lambda(p^A, p^B, p^C, \omega^A, \omega^B, y, z; \mu, \xi), \tag{1}$$

where p^A, p^B, and p^C are price vectors of private and public consumption goods; ω^i and y^i are wages and nonlabor income of individual i ($i = A, B$); y^O is all income held jointly by household members; and T is the total time endowment of each individual.

In this model, the Pareto weight function $\lambda = \lambda(p^A, p^B, p^C, \omega^A, \omega^B, y, z; \mu, \xi)$ influences the sharing rule, which in turn determines the division of available resources between all household decision makers. The resource sharing rule (and, as a consequence, the utility of each household member) is related to the distribution factors that influence the "power" of decision making within the household. As empirically established in the literature (e.g., Schultz 1990; Thomas 1990), assume that partner-specific and jointly held incomes (y^A, y^B, and y^O) are distribution factors, variables that affect consumption choices through their impact on the decision process in addition to their influence through the aggregate resource constraint.

The solution to maximization problem (1) implies that households will have demand functions for private, composite, and public goods as functions of prices (including individual wages), total resources (i.e., expenditures) denoted by x, individual and household characteristics, and the Pareto weight function, which influences the power of each partner:

$$g = G[p,\ x,\ \lambda(p,\ x,\ y^A,\ y^O;\ \mu,\ \xi);\ \mu,\ \xi] \tag{2}$$

for all goods g, $g \in \{q^A,\ q^B,\ c,\ K\}$. Under the collective rationality model, changes in partner-specific and jointly held incomes may affect household demand (and therefore allocation decisions), since these may affect the household resource sharing rule.

The main testable prediction of the collective model based on variation in distribution factors follows from demand system (2): the ratio of partial derivatives of each good with respect to each distribution factor—in this case, partner-specific incomes—conditional on aggregate household resources is equal across all goods and equal to the ratio of distribution factor effects on the Pareto weight:

$$\frac{\partial q^A/\partial y^A}{\partial q^A/\partial y^O} = \frac{\partial q^B/\partial y^A}{\partial q^B/\partial y^O} = \frac{\partial c_l/\partial y^A}{\partial c_l/\partial y^O} = \frac{\partial K_m/\partial y^A}{\partial K_m/\partial y^O} = \frac{\partial \lambda/\partial y^A}{\partial \lambda/\partial y^O} \quad \forall l,\ m. \tag{3}$$

Bourguignon et al. (2009) have recently shown that these proportionality conditions are both necessary and sufficient for efficiency. The intuition for this result is as follows: the distribution factors' effects on the consumption of each good are equally proportional to the distribution factors' influence on the decision makers' Pareto weights, since the former affect consumption choices only through their effects on the decision makers' power. Since the proportionality condition holds for each consumption good, the ratio of partial derivatives should be equal across all private, composite, and public goods (Browning and Chiappori 1998; Bourguignon et al. 2009).

An alternative demand system consistent with the collective model assumptions, recently coined by Bourguignon et al. (2009) as the z-conditional demand system approach, helps resolve some challenges to empirical testing. Essentially, under the additional assumption that one of the observable distribution factors has a strictly monotone influence on one of the consumption goods (assume, without loss of generality, distribution factor y^A and composite good c_1), the demand function for good c_1 can be inverted on this factor:

$$y^A = \varsigma(p,\ x,\ y^O,\ c_1;\ \mu,\ \xi); \tag{4}$$

and substituting this into the demand for all other goods results in the system of *z*-conditional demand functions:

$$g_{-c_1} = \tilde{G}[p,\ x,\ y^O,\ c_1;\ \mu,\ \xi]. \tag{5}$$

This demand system is a function of total expenditures, preference factors, the consumption good assumed to be monotonically influenced by the distribution factor, and all additional distribution factors except the one identified above. Formally, Donni and Moreau (2007) show in the two-equation case, and Bourguignon et al. (2009) in the general case, that the proportionality condition is equivalent to the condition that $\partial g_{-c_1}/\partial y^O = 0$ for at least one good $g_{-c_1} \in \{q^A,\ q^B,\ c_{-1},\ K\}$ in this system of conditional demand functions. The intuition behind this result is simple: the level of the conditioning good provides sufficient information as to how the household equilibrium moves along the efficiency frontier when the balance of power is modified, and thus the other distribution factors provide no additional information about movements along the household's efficiency frontier. This alternative specification is useful for empirical testing since it relies on tests of joint significance of linear restrictions.

I will implement empirical versions of these models and perform tests of both the proportionality condition and the linear restrictions imposed by the *z*-conditional demand system. In order to explain the empirical strategies, in the following section I briefly describe the PROGRESA program, the data used in the analysis, and the sources of variation exploited to identify the source-specific income effects. This discussion will allow me to postulate the empirical models and validate their assumptions.

IV. The PROGRESA Program, Rainfall Variation, and Data

A. *Overview of the PROGRESA Program*

In 1997, the Mexican government initiated a conditional cash transfer program named PROGRESA, aimed at alleviating poverty and improving the human development of children in rural Mexico. The program targets the poor in marginal rural communities. It provides cash transfers to the mothers of over 2.6 million children conditional on school attendance, health checks, and participation in health clinics. The education component of PROGRESA consists of subsidies provided to mothers, contingent on their children's regular attendance at school.[8] These cash transfers are available for each child attending school in grades 3–9 and range from 70 to 255 pesos per month depending on the gender and grade level the child is attending (with a maximum of 625 pesos per month per family in 1998). The health and nutrition components consist of transfers of approximately 12 pesos per month and nutrition supplements targeted at children 4 months to 2 years old, pregnant and breast-feeding women, and children aged 2–5 years who exhibit signs of malnutrition (Gómez de León and Parker 2000). These benefits are contingent on attendance at a health clinic for preventive health checks. Overall, the program transfers are substantial, representing 8 percent of the average expenditures of beneficiary families in the sample.

A distinguishing characteristic of PROGRESA is that it included an evaluation component from its inception. The program was implemented following an experimental design in a subset of 506 communities located across seven states. Among these communities, 320 were randomly assigned into a treatment group, which started receiving benefits in March/April 1998; the remaining 186 communities served as a control group, thus providing an opportunity to apply experimental design methods to measure its impact on various outcomes. In addition, within these selected communities, a poverty proxy-means test was constructed using household income data collected from a baseline survey in both treatment and control communities in 1997 (see Skoufias, Davis, and de la Vega [2001] for a more detailed description of the targeting process).[9] While household eligibility was determined within all communities, only households classified as eligible and within the treatment villages became program beneficiaries during the evaluation period. That the eligibility classification exists for both treatment and control communities and that treatment was randomly assigned are critical design aspects for the identification of intrahousehold allocation effects.

B. Data and Measurement

Following a baseline census in October 1997, extensive interviews were conducted during October 1998, May/June 1999, and November 1999 on approximately 24,000 households of the 506 communities. Each survey is a community-wide census containing detailed information on household demographics, income, expenditures, and consumption, as

[8] Receipt of the education-specific benefits is contingent on children attending school at least 85 percent of the time, which is verified by school personnel.

[9] In addition to capturing the multidimensionality of poverty, another advantage of a welfare index is that it permits the classification of new households according to their socioeconomic characteristics other than income.

well as individual socioeconomic, health, and schooling-related behaviors and outcomes. More specifically, on the basis of the detailed (posttreatment) expenditures and consumption modules conducted in the October 1998, May/June 1999, and November 1999 rounds, I construct measures of total household expenditures and the share of the total expenditures budget on various household items. In particular, I construct measures for budget shares on adult female and adult male clothing, children's clothing, household educational expenditures, and an array of other types of expenditures that can be classified as household composite goods (e.g., food, transportation, hygiene, medical, and other household goods expenditures).[10] Following Browning et al. (1994), I assume that the first two measures are assignable goods to male and female partners (in single-family households), and I use these to estimate adult private good demand functions.[11] The child clothing and education measures arguably represent expenditures on child-specific goods and constitute an important component of (nonfood) total child expenditures. To the extent that partners have divergent preferences over child expenditures, these measures would allow us to infer that changes in partners' income shares that favor women would imply a shift in household expenditures toward these household public goods.

Since we are interested in identifying the effects of the (program-driven) female-specific income changes on intrahousehold resource allocation outcomes, using the complete sample of households may confound the income effect and the conditionality effects of the program (the fact that households received cash only if children were in school). Schultz (2004) and Behrman, Sengupta, and Todd (2005) report that school enrollment rates were close to 100 percent for primary school children among both PROGRESA and comparison village children and, thus, that the program had no impacts on primary school enrollment. On the basis of this evidence, I assume that schooling conditionality constraints are not likely to be binding for households with strictly primary school–aged children. Therefore, in order to minimize the confounding with the program conditionality effects, I restrict the sample used for the analysis to intact eligible households with children aged 9 years and younger at baseline, who would not be sufficiently old to attend secondary school throughout the study period. I further restrict

[10] There was a round of data collection in March 1998 just prior to the start of the intervention. Unfortunately, the consumption and expenditure module is not comparable between this round and subsequent rounds. Therefore, I do not employ these data in the analysis.

[11] Browning et al. (1994) provide estimates of sharing rules based on the assumption that clothing is assignable. This assumption implies that female (male) partners will care only about their partner's clothing strictly as it contributes to the welfare of their partner, an assumption that may be questionable. However, it is hard to think of any other private goods partners do not directly care about.

the sample to households with mothers between the ages of 16 and 55. These restrictions result in a sample of approximately 3,900 households.

I also exploit variation in rainfall shocks to identify changes in household income that are not exclusive of female partners. I use rainfall data from local weather stations collected by Mexico's National Meteorological Service and match it to the villages using Global Positioning System data. I construct two rainfall shock measures: the deviation in rainfall from its average level for the period 1991–2002, in the 6-month period preceding each survey round, and a variable indicating whether the magnitude of the deviation from average rainfall is greater than one standard deviation in the variation in rainfall in the region. These rainfall shocks are substantial, affecting 7 percent of households in each survey round, and lead to quite persistent reductions in total household expenditures of approximately 17 percent, on average (see subsec. C).

Table 1 presents the means of various baseline (October 1997) individual and household-level characteristics for eligible households and their means for both treatment and control villages. Individuals in this sample come from poor socioeconomic status households, since PROGRESA is targeted to poor individuals in marginalized rural communities. Among the overall eligible household sample, approximately half of the women have not completed primary school (panel A, col. 1). Most female partners do not earn cash income; only 6 percent either are wage laborers or are self-employed. A significant share of women and their partners are indigenous (37 and 38 percent, respectively), which is highly correlated with low socioeconomic status in Mexico. Male partners have age group and schooling attainment distributions similar to those of their female partners (panel B, col. 1). However, 76 percent of male partners work as wage laborers, and another 10 percent reported being self-employed.

Households in the sample had on average 2.3 children in 1997, and approximately 72 percent of these children were in the 0–5 years age group (panel C, col. 1). Finally, note that approximately one-third (34 percent) of couples report cohabiting rather than being married. This type of marital relationship is common in rural Mexico since many are considered "trial marriages." Also, on the basis of a survey of the ethnographic literature, Mindek (2003*a*) remarks that most marital dissolutions are in the form of separations rather than official divorces.[12]

[12] The frequency of dissolution varies across indigenous groups: Otomíes, Triquis, and Tzotziles experience low dissolution rates, whereas Mixtecs, Zapotecs, Nahuas, and others experience high ones. According to the survey in Mindek (2003*a*), these patterns are partly due to the variation in the incidence of arranged marriages.

Norms of family support for women and their children in the event of dissolution are similar across ethnic groups in Mexico. Chiñas (1992) comments that upon marital dissolution, Zapotec women in the Isthmus of Tehuantepec (Guerrero) keep custody over children and are expected to go back to their parents' or siblings' household. Also, the

TABLE 1
Baseline Characteristics

	Overall		Program Groups Comparison		Rainfall Shock Comparison	
	Mean (1)	Standard Deviation (2)	Program Mean (3)	Control Mean (4)	Deviation from Average Rainfall (mm) (5)	Constant (6)
	A. Mothers' Characteristics					
Age 14–25 years	.59	(.49)	.59	.60	−.00005 (.00017)	.593
Age 26–35 years	.35	(.48)	.35	.34	−.00003 (.00016)	.351
Age 36–45 years	.05	(.21)	.05	.05	.00001 (.00007)	.046
Age 46–55 years	.01	(.11)	.01	.01	.00007* (.00004)	.010
Schooling < primary	.52	(.50)	.52	.52	−.00009 (.00033)	.523
Indigenous woman	.37	(.48)	.63	.63	.00299** (.00052)	.728
Wage laborer	.03	(.17)	.03	.03	.00005 (.00008)	.029
Self-employed	.03	(.16)	.03	.02	.00000 (.00011)	.027
	B. Partners' Characteristics					
Age 14–25 years	.32	(.46)	.31	.32	−.00011 (.00016)	.319
Age 26–35 years	.54	(.50)	.55	.52	.00001 (.00017)	.540
Age 36–45 years	.11	(.31)	.10	.12	.00003 (.00011)	.106
Age 46–55 years	.03	(.16)	.02	.03	.00005 (.00005)	.026
Age 56–65 years	.01	(.09)	.01	.01	.00003 (.00003)	.007
Schooling < primary	.52	(.50)	.51	.54	−.00058* (.00029)	.539
Indigenous partner	.38	(.48)	.38	.38	.00291** (.00052)	.713
Wage laborer	.76	(.43)	.75	.78	.00077** (.00020)	.736
Self-employed	.10	(.31)	.11	.10	−.00002 (.00015)	.105
	C. Household Characteristics					
Number of children	2.32	(1.12)	2.33	2.31	.00051 (.00037)	2.30
Number of boys aged:						
0–5	.83	(.77)	**.85**	**.79**	.00025 (.00025)	.819

TABLE 1
(*Continued*)

	Overall		Program Groups Comparison		Rainfall Shock Comparison	
	Mean (1)	Standard Deviation (2)	Program Mean (3)	Control Mean (4)	Deviation from Average Rainfall (mm) (5)	Constant (6)
6–7	.19	(.41)	.19	.19	−.00016 (.00012)	.195
8–9	.14	(.37)	**.15**	**.12**	.00011 (.00010)	.135
Number of girls aged:						
0–5	.83	(.79)	**.82**	**.86**	.00018 (.00024)	.828
6–7	.20	(.42)	.19	.20	−.00001 (.00011)	.196
8–9	.11	(.33)	.11	.12	.00007 (.00010)	.112
Cohabiting couple	.34	(.47)	**.32**	**.37**	.00106** (.00027)	.305
Dirt floor	.66	(.47)	.65	.68	.00196** (.00032)	.595
Own house	.90	(.30)	.90	.90	.00024** (.00011)	.890
Total agricultural land	1.30	(2.52)	1.27	1.34	.00210^{+} (.00135)	1.230
Landowning households (1/0)	.49	(.50)	.50	.47	.00156** (.00034)	.436
Women with landholdings among landowning households	.03	(.16)	.03	.03	.00004 (.00004)	.026
Share of household land owned by females	.03	(.15)	.02	.03	.00004 (.00004)	.024

Note.—Means of characteristics by groups and standard deviations (in parentheses) are presented, as well as coefficient estimates of regressions of each baseline characteristic on the deviation from mean average rainfall in October 1998, June 1999, and November 1999; robust standard errors are based on disturbance terms that are allowed to be correlated within villages. Figures in bold in cols. 3 and 4 represent statistically significant differences at least at 95 percent confidence levels. The sample is composed of approximately 3,900 eligible households with children 9 years old or younger at baseline. Baseline data are from the October 1997 household survey except landholding data, which are from the October 1998 survey, and rainfall shocks, which are from October 1998, June 1999, and November 1999.

$^{+}$ Significant at the 85 percent confidence level.

* Significant at the 90 percent confidence level.

** Significant at the 95 percent confidence level.

I also compare mean attributes at baseline across treatment and control villages to evaluate the balance of the samples (table 1, cols. 3 and 4). As one would hope from the random assignment, there are no statistically significant differences in the observed characteristics of these individuals in most dimensions.[13]

Table 2 reports descriptive statistics for aggregate household resources (income and expenditures), average household daily wages, and household consumption patterns for the October 1998, June 1999, and November 1999 survey rounds, in addition to the means for both treatment and control villages. Monthly household expenditures for this population are quite limited, 701.5 pesos on average (approximately US$70), and household incomes (excluding the PROGRESA transfers) are 960.5 pesos ($96), on average. Also, as expected, the program group-specific means suggest that PROGRESA led to a significant rise in household consumption—an average increase of 65.4 pesos (10 percent). Finally, note that mean household daily wages, which are on average 39.6 pesos, do not differ significantly across program and comparison villages. The next subsection describes in more detail the effects of the program and of the rainfall shocks on overall household resources.

C. *Effects of the Program and Rainfall Shocks on Household Resources*

The program led to an average increase in total household expenditures among couples who remained in union during the period (table 3, panel A). The pooled post-treatment periods estimates, conditioning on the baseline observable characteristics described above, imply an average increase of 56.6 pesos, or an 8.1 percent increase, in total expenditures (panel A, regression 1). Restricting the sample to the June 1999 and November 1999 survey rounds, respectively, 1 year and almost 18 months following the start of the intervention, provides program effect estimates of 78.6 pesos, an 11.1 percent increase in total household expenditures (panel A, regression 2). The estimated effects are very precisely estimated. These results are consistent with program impacts on household consumption for the overall sample of eligible households (Hoddinott, Skoufias, and Washburn 2000).

The impacts of the rainfall shocks on household expenditures are also very robust. The average impact of the rainfall shock during any

indigenous groups surveyed by Mindek (2003*a*) have the custom that parents of one gender retain custody over children of the opposite gender (i.e., mothers take care of sons and fathers take care of daughters), except young children, who always remain under the custody of the mother irrespective of their gender.

[13] Behrman and Todd (1999) conduct an exhaustive analysis of the degree of success of the random assignment of villages in the PROGRESA program and conclude that the randomization was successful.

post-treatment period implies a reduction in expenditures of 1.10 pesos for each millimeter of rainfall deviation from the average of the region (table 3, panel A, regression 1) or, given an average deviation of 32.4 millimeters from average rainfall in the region, an average effect of 35.6 pesos (5.1 percent). Using as an alternative measure the indicator variable for a severe rainfall shock, I find evidence of substantial negative impacts: households that experienced a rainfall shock in a given period suffered a decrease in household expenditures of 117.0 pesos, a 16.7 percent reduction in household expenditure levels, on average (panel B, regression 1). The graphical evidence confirms this relationship. A plot of mean village-level household expenditures against the absolute value of the deviation from mean monthly rainfall during each survey round shows a stark downward-sloping relationship (fig. 1*A*). The coefficient estimate from a linear prediction of this relationship is −1.72 (standard error 0.21; regression not shown). In summary, these results indicate that both the intervention and the rainfall shocks have a substantial effect on the level of household resources.[14]

As mentioned above, the different shocks affecting household incomes may have varying time spans. For instance, it is generally thought that rainfall shocks affect agricultural households' transitory incomes (e.g., Paxson 1992), whereas the transfer program is more likely to have effects on households' incomes throughout their period of eligibility in the program—3 years at the start of the evaluation. Therefore, these differences in time span may lead one to find heterogeneous responses in consumption patterns due to the different types of shocks. However, I find that the rainfall shocks have persistent effects on total household expenditures. Again, the graphical evidence confirms this relationship: the plot of mean (village-level) household expenditures against the 1-year lagged deviation from average rainfall in the region shows a stark downward-sloping relationship (fig. 1*B*). The coefficient estimate from a linear prediction of this relationship is −1.59 (standard error 0.25; regression not shown).

The household-level regressions also indicate a persistent effect. I estimate reduced-form models to ascertain whether 6-month and 1-year lagged rainfall shocks have effects on household expenditure levels. One set of specifications includes the current and 6-month lagged rainfall shock measures (using the June 1999 and November 1999 survey round data), and another includes the current and the 12-month lagged rainfall shock measure (using the November 1999 round data; table 3, regressions 2 and 3). Both coefficient estimates on the deviation measures

[14] Estimates using the natural log of total household expenditures as the dependent variable (the functional form assumption under the almost ideal demand system) are also very robust (not reported in the table).

TABLE 2
DESCRIPTIVE STATISTICS

	Overall		Program Groups Comparison		Rainfall Shock Comparison	
	Mean (1)	Standard Deviation (2)	Program Mean (3)	Control Mean (4)	Deviation from Average Rainfall (mm) (5)	Constant (6)
	A. Household Resources (Pesos)					
Household expenditures	701.5	(471.4)	**725.9**	**660.5**	−1.80**	759.9
					(.21)	
Household income (excluding program transfers)	960.5	(1,533.2)	851.2	1,142.3	−1.26**	1,051.2
					(.38)	
Agricultural profits (if nonmissing, October 1998)	203.3	(400.7)	207.5	197.4	−1.13	242.2
					(1.02)	
Labor earnings	612.9	(847.9)	602.4	630.5	−.97**	644.2
					(.35)	
Household mean daily wage (pesos)	39.6	(73.6)	38.9	40.7	−.10**	43.0
					(.02)	
	B. Consumption Goods Expenditure Shares (%)					
Adult female clothing	1.04	(1.88)	1.04	1.03	−.0024**	1.12
					(.0008)	
Adult male clothing	1.20	(2.32)	1.22	1.16	−.0010	1.23
					(.0010)	
Educational	1.31	(3.70)	**1.21**	**1.46**	.0009	1.28
					(.0014	
Child clothing	3.10	(3.70)	**3.34**	**2.69**	−.0056**	3.28
					(.0017)	
Fruits and vegetables	11.48	(6.95)	**11.92**	**10.74**	.0113**	11.11
					(.0033)	
Cereals and grains	27.87	(14.63)	**27.18**	**29.04**	.0245**	27.08
					(.0070)	
Meats	14.26	(10.88)	**14.67**	**13.58**	.0009	14.23
					(.0052)	
Other foods	16.40	(9.45)	16.31	16.56	.0414**	15.06
					(.0059)	
Transport	.04	(.08)	.04	.04	−.0001**	.04
					(.0000)	
Alcohol and tobacco	.28	(1.89)	.24	.34	.0002	.27
					(.0011)	
Hygiene	7.85	(5.78)	7.72	8.06	.0039*	7.71
					(.0022)	

TABLE 2
(*Continued*)

	Overall		Program Groups Comparison		Rainfall Shock Comparison	
	Mean (1)	Standard Deviation (2)	Program Mean (3)	Control Mean (4)	Deviation from Average Rainfall (mm) (5)	Constant (6)
Medicine	2.22	(7.36)	2.14	2.37	−.0093** (.0026)	2.53
Other household goods	5.74	(6.27)	**5.58**	**6.03**	−.0099** (.0029)	6.06

Note.—Means of characteristics by groups and standard deviations (in parentheses) are presented, as well as coefficient estimates of regressions of each baseline characteristic on the deviation from mean average rainfall in October 1998, June 1999, and November 1999; robust standard errors are based on disturbance terms that are allowed to be correlated within villages. Figures in bold in cols. 3 and 4 represent statistically significant differences at least at 95 percent confidence levels. The sample is composed of eligible households with children 9 years old or younger at baseline. Data are from the October 1998, June 1999, and November 1999 surveys.
* Significant at the 90 percent confidence level.
** Significant at the 95 percent confidence level.

are large, of similar magnitude as those of the current shock, and significantly different from zero. These imply an average impact of approximately 38.6 pesos (5.5 percent) and 41.8 pesos (5.9 percent) 6 months and 12 months following a rainfall shock, respectively. The estimates using the binary rainfall shock measure imply a greater current rainfall shock effect and a larger, but less precisely estimated, lagged rainfall shock effect (table 3, panel B, regressions 2 and 3). Although these results do not prove that these shocks have the same timing effects, they show suggestive evidence that the rainfall shocks can have a substantial effect on expenditures in subsequent periods and are thus comparable to the program-driven increases in household income.

We can also observe the relationship between the rainfall shocks and household resources in alternative models that use household income (excluding the PROGRESA transfer) as the measure of aggregate resources (table 3, col. 4). The partial correlation with the continuous rainfall measure suggests that deviations from average rainfall lead to an increase in income of 3.44 pesos for each millimeter of rainfall deviation from the average of the region (or, given an average deviation of 32.4 millimeters from average rainfall in the region, an average increase of 111.5 pesos or 11.6 percent; panel A, regression 4). However, this relationship is nonmonotonic. Using the indicator for a severe rainfall shock, I find evidence of a substantial negative impact: communities that experienced a rainfall shock in a given period suffered a decrease in household income of 334.8 pesos, a 34.9 percent reduction in household income levels (panel B, regression 4). These relationships are im-

TABLE 3
PROGRAM IMPACTS AND RAINFALL SHOCK EFFECTS ON TOTAL HOUSEHOLD EXPENDITURES AND SEPARATION

	ELIGIBLE COUPLES IN UNION SINCE BASELINE			ALL PERIODS	AT BASELINE
	All Periods	June 1999 and Nov. 1999	Nov. 1999		Nov. 1999
	Total Household Expenditures			Household Income (Excluding Transfer)	Currently Separated
	(1)	(2)	(3)	(4)	(5)
	A. Explanatory Variables: Deviation from Average Rainfall				
Treatment indicator	56.57**	77.60**	59.64**	−259.94	.0054**
	(15.14)	(17.10)	(23.20)	(223.34)	(.0024)
Deviation from average rainfall:					
Current	−1.10**	−1.05**	−.82**	3.44	−.000024
	(.19)	(.23)	(.26)	(3.48)	(.000035)
6-month lag		−1.19**			
		(.37)			
1-year lag			−1.27**		
			(.29)		
Time indicators	Yes	Yes	No	Yes	No
Controls	Yes	Yes	Yes	Yes	Yes
	B. Explanatory Variables: Deviation from Average Rainfall > 1 Standard Deviation				
Treatment indicator	56.66**	76.49**	60.40**	−254.89	.0054**
	(15.26)	(17.20)	(23.45)	(223.40)	(.0024)
Deviation from average rainfall > 1 SD:					
Current	−117.04**	−151.52**	−171.01**	−334.81	−.0045**
	(27.79)	(24.87)	(26.25)	(651.09)	(.0022)
6-month lag		−59.00			
		(59.54)			
1-year lag			−119.29*		
			(39.54)		
Time indicators	Yes	Yes	No	Yes	No
Controls	Yes	Yes	Yes	Yes	Yes
Observations	11,733	7,348	3,293	11,733	4,105
Mean of dependent variable	701.5	702.1	668.8	960.5	.0061

NOTE.—Coefficient estimates from OLS regressions are presented. Robust standard errors are in parentheses; disturbance terms are allowed to be correlated within villages. Controls include indicators for mother's and partner's age group (26–35 years, 36–45 years, and 46–55 years); indicator variables for none or less than primary schooling, indigenous language indicator for both women and their partners; wage laborer and self-employed indicators for women; wage laborer, self-employed, and agricultural worker indicators for partners; cohabitation status, total household agricultural land, having a dirt floor, and owning the residence; demographic controls include number of children by gender and age group categories (0–5 years, 6–7 years, and 8–9 years). Samples in the expenditures and income regressions are composed of all eligible couples in union since baseline with children 9 years old or younger at baseline. The sample in the separation regression is composed of all eligible households in union at baseline with children 9 years old or younger at baseline.

* Significance at the 90 percent confidence level.

** Significance at the 95 percent confidence level.

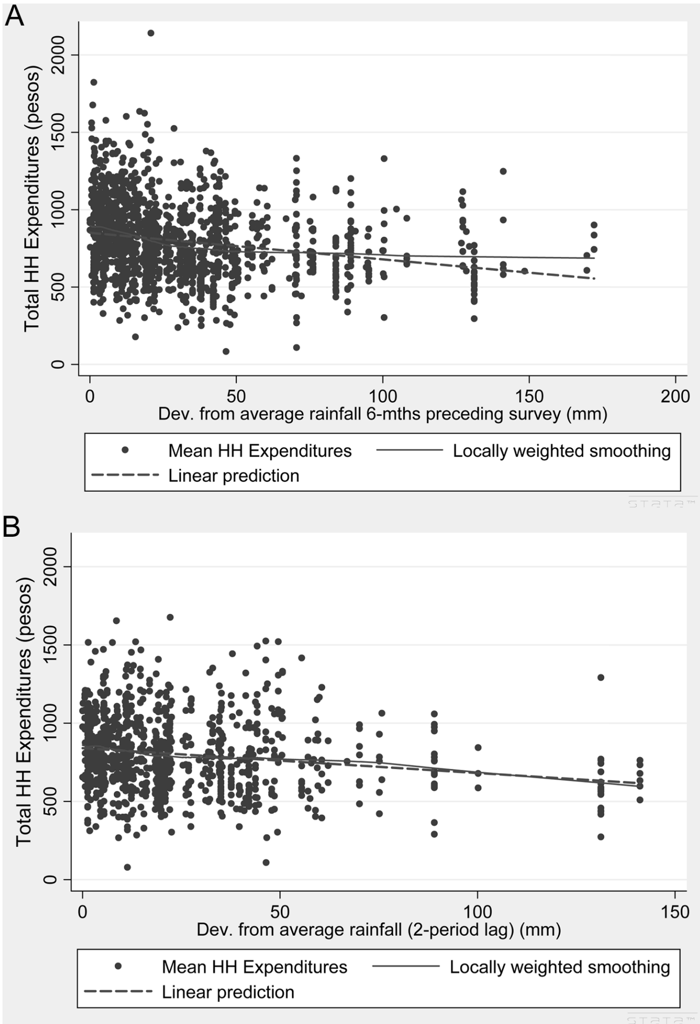

FIG. 1.—Deviation from average rainfall and household monthly expenditures. *A*, Current shocks on total household monthly expenditures in October 1998, June 1999, and November 1999. Linear regression coefficient estimate (standard error) is −1.715** (0.207). *B*, One-year lagged shocks on total household monthly expenditures in November 1999 and November 2000. Linear regression coefficient estimate (standard error) is −1.586** (0.249). The figures represent graphical relationships between deviations from average monthly rainfall (rainfall shocks) during the 6-month period preceding each survey round period and the village-level mean of total monthly household expenditures. The solid and dashed lines represent locally weighted smoothing (LWS) and linear predictions, respectively, of total monthly household expenditures by the rainfall shock in the village; bandwidth in LWS is 0.5.

precisely estimated, as perhaps expected because of the extent of measurement error in income among farm households (Deaton 1995).

The effects on aggregate household income are concentrated among households' farm profits (data available only for the October 1998 survey round) and in their labor earnings. A simple correlation of these components of household income with the deviation from average rainfall measure suggests that these are substantially reduced when abnormal rainfall levels occur (table 2, panel A). Moreover, extreme rainfall events lead to a decrease in agricultural profits of 216.5 pesos (standard error 130.8; significant at 90 percent confidence) and a decrease in labor earnings of 152.8 pesos, on average (standard error 70.3; significant at 95 percent confidence; estimates not shown in the table).

I also examine whether we can assume that rainfall shocks are exogenous with respect to household consumption patterns. Although the assignment to program treatment and control villages is uncorrelated with unobserved household characteristics because of random assignment, it is a priori possible that the rainfall shocks are correlated with unobserved determinants of intrahousehold allocation decisions. This could be the result of, for instance, geographic sorting of agricultural households into areas with differing rainfall variability or increased marital dissolution caused by these income shocks. Although the assumption of a lack of correlation with unobservable characteristics cannot be directly tested, I show evidence consistent with it. First, mean baseline observable characteristics of households in the restricted sample do not differ systematically among those who suffered a rainfall shock and those who did not (in any period), as measured by the correlation of the deviation in rainfall measure and the individual and households' baseline characteristics (table 1, col. 5). Second, I test whether there are differences in prerainfall shock trends in the dependent variables of interest among these groups. Estimates of preshock differences in all outcome variables used in the analysis are all insignificantly different from zero, and tests of the joint significance of the preshock differences also fail to reject the null hypothesis (not reported in the tables).[15] Although it is possible that other underlying unobserved factors correlated with the rainfall shocks affect the demand for private and house-

[15] I estimate the following model for the pooled post-treatment data:

$$D_{jict} = \theta_{O98} R_{ic,N99} + \theta_{J99} R_{ic,N99} + \theta_{N99} R_{ic,N99} + R_{ic,O98}\gamma_1 + R_{ic,J99}\gamma_2 + \alpha_t + \eta_{jict},$$

where D_{jict} is the outcome variable of interest in period t; $R_{ic,O98}$, $R_{ic,J99}$, and $R_{ic,N99}$ denote, respectively, the rainfall shocks preceding the October 1998, June 1999, and November 1999 survey rounds; α_t denotes period-specific effects common to all households; and η_{jict} is an error term. I allow the coefficients on the November 1999 shock to vary by survey round, which allows me to capture the preshock differences (θ_{O98}, θ_{J99}) and the reduced-form effect of the shock (θ_{N99}) on the outcome variables.

hold public goods, this evidence provides confidence that the results are not likely to be driven by unobserved heterogeneity.

V. Econometric Models and Research Design

In this section, I discuss the specific econometric models used to assess the validity of the collective approach, as well as the assumptions needed for identification. As discussed in Section III, households have demand functions for private, composite, and public goods (system of eqq. [2]) as functions of prices (including wages), aggregate household resources, the partner-specific incomes, and individual and household characteristics:

$$\ln(q_{jic}) = \alpha_j + \beta_{j1} y^A_{ic} + \beta_{j2} y^O_{ic} + f_j(y_{ic}) + \mu_{ic}\delta_j + \xi_{jic}. \quad (6)$$

Equation (6) for each good j is the parametric "reduced form" of household demand equations, where q_{jic} is the budget share on good j for household i in village c; y^A_{ic} and y^O_{ic} are female partner-specific and collective household incomes (as defined above); $f_j(y_{ic})$ is a function of total household resources; μ_{jic} is a set of baseline woman, partner, and household (including detailed demographic) controls; and ξ_{jic} are unobservable determinants of demand for each good.[16] Baseline (pretreatment) controls for household and individual characteristics include indicators for mother's and partner's age group (26–35 years, 36–45 years, and 46–55 years); indicator variables for less than primary schooling for each partner; indigenous language indicators for each partner; wage laborer and self-employed indicators for women; wage laborer, self-employed, and agricultural worker indicators for partners; total household agricultural land, having a dirt floor, and owning the residence; and demographic controls that include the partners' baseline cohabitation status, the household's size, and the number of children by gender and age group categories (0–5, 6–7, and 8–9 years). Price data for many household consumption goods are available only at the village level; I first estimate models in which prices enter equation (6) through the error term but relax this assumption to include village-level prices as a robustness test.

Under the unitary household model (Samuelson 1956; Becker 1991), partner-specific incomes are pooled; that is, the distribution of income within the household should not play a role in intrahousehold allocation decisions. This implies that, conditional on aggregate household income, the partner-specific and joint household income coefficients

[16] The log-linear functional form assumption, although dissimilar to Deaton and Muellbauer's (1980) almost ideal demand system, allows easy interpretation of the empirical model coefficient estimates. However, the empirical results are robust to the choice of functional form assumption (results available on request).

should equal zero ($\beta_{j1} = \beta_{j2} = 0$ for all j). Under the collective model alternative, a change in an individual partner's income (which may imply differences in the sharing rule) has an additional impact on the demand for private and public goods if the marginal willingness to pay of this member is more sensitive to changes in his or her share than that of the other member. This is essentially a test of the income pooling hypothesis, the main testable prediction of the unitary household model. Moreover, under the collective model alternative, we can estimate the female income-specific and household collective income distribution factor effects, which under standard identification assumptions allows us to construct a version of the proportionality test (condition [3])—that the ratio of distribution factor effects is equal across all goods:

$$\frac{\beta_{j1}}{\beta_{j2}} = \frac{\beta_{k1}}{\beta_{k2}} \quad \forall \text{ goods } j,\ k,\ j \neq k. \tag{7}$$

Note that, to the extent that individual incomes are a result of variation in prices and wages faced by households or other possibly unobserved factors affecting household resource allocation decisions, ordinary least squares (OLS) estimates of the system of equations (6) may suffer from omitted variables bias. The empirical content of the theory imposes demanding requirements on tests of the model because it requires exogenous variation in two distribution factors with which to get consistent estimates of their influences on consumption choices.

A. *Tests Based on Standard (Unconditional) Demand System Estimation*

To overcome these identification issues, I use exogenous variation in two factors that manipulate women's and other household members' incomes—the randomized variation generated by the PROGRESA program evaluation and income variation attributable to localized rainfall shocks—to test these restrictions. More formally, I use the PROGRESA treatment village indicator (T_c) variable as a measure of female-partner income and the village-level rainfall shock measure (R_c) as a proxy for joint household income and estimate the following set of equations:

$$\ln (q_{jic}) = \alpha_j + \beta_{j1} T_c + \beta_{j2} R_c + f_j(x_{ic}) + \mu_{ic}\delta_j + \xi_{jic} \tag{8}$$

for all j goods mentioned above, where $f_j(x_{ic})$ is assumed to be a polynomial function on household expenditures (x_{ic}). Under the standard assumptions that the explanatory variables are uncorrelated with the error term (ξ_{jic}), OLS estimation provides consistent estimates of the conditional distribution factor effects on household consumption choices. Alternatively, since total household expenditures may be endogenous in the model, we can exploit variation in total household income (y_{ic}) as an instrumental variable (IV) for the former.[17]

The proportionality tests require the joint estimation of the system of demand equations and tests of nonlinear cross-equation restrictions over the distribution factor parameter estimates. Therefore, in addition to allowing for heteroscedasticity-robust and clustered standard errors at the village level in each equation, I estimate the models as a system, allowing for correlation of the disturbance terms across equations. I conduct Wald test formulations of the nonlinear cross-equation restrictions. However, Wald tests face a number of statistical problems; in particular, (i) the tendency to overreject the null hypothesis in system OLS and seemingly unrelated regression (SUR) models and (ii) the noninvariance of nonlinear Wald test statistics to the mathematical reformulation of the null. To address the first issue, I estimate the appropriate p-value of the test statistic using the percentile F interval of the statistic based on its bootstrap distribution, which has been shown to significantly reduce the overrejection bias in this setting (Rilstone and Veall 1996).[18] To address the second issue, I assess the robustness of my inferences by constructing linear Wald tests based on the system of parametric versions of the z-conditional demand functions, which I discuss below.

B. *Tests Based on z-Conditional Demand System Estimation*

The formulation of the z-conditional demand system helps resolve the latter challenge to empirical testing based on the following implementation strategy. Under the additional assumption that the PROGRESA program intervention (one of the observable distribution factors) has a strictly monotone influence on one of the consumption goods—as will be seen below, expenditures on child clothing—the demand function for this good, denoted q_1, can be inverted on this factor:

$$T_c = \frac{1}{\beta_{11}} \ln (q_{1ic}) - \frac{\alpha_1}{\beta_{11}} - \frac{\beta_{12}}{\beta_{11}} R_c - \frac{1}{\beta_{11}} f_1(x_{ic}) - \mu_{ic} \frac{\delta_1}{\beta_{11}} - \frac{1}{\beta_{11}} \xi_{1ic}. \quad (9)$$

[17] The usual reason for considering total expenditure as endogenous in a demand system is that unusually high (low) expenditures on a consumption good will induce a correlation between the error term and total expenditures. As implemented in Browning and Chiappori (1998), among others, total household income may serve as an IV because it is correlated with aggregate household expenditures, but conditioning on it should have no influence on the distribution of expenditures.

[18] See Fiebig (2003) for a survey of the literature on SUR models and Horowitz (2001) for a discussion of bootstrap methods.

Substituting this equation for T_c in the demand equations for all other goods results in the system of z-conditional demand functions:

$$\ln(q_{jic}) = \left(\alpha_j - \frac{\alpha_1\beta_{j1}}{\beta_{11}}\right) + \frac{\beta_{j1}}{\beta_{11}}\ln(q_{1ic}) + \left(\beta_{j2} - \frac{\beta_{12}\beta_{j1}}{\beta_{11}}\right)R_c$$

$$+ \left[f_j(x_{ic}) - \frac{\beta_{j1}}{\beta_{11}}f_1(x_{ic})\right] + \mu_{ic}\left(\delta_j - \frac{\delta_1\beta_{j1}}{\beta_{11}}\right) + \xi_{jic} - \frac{\beta_{j1}}{\beta_{11}}\xi_{1ic} \quad (10)$$

for all goods $j \neq 1$. As can be seen from the composite parameter on the rainfall shock distribution factor (R_c) for each equation, a joint test of significance of these parameter estimates is equivalent to a test of the proportionality condition. Under this alternative modeling strategy, the child clothing expenditures share, q_{1ic}, is clearly correlated with ξ_{1ic}, but under standard IV assumptions, and since we have already assumed that the program treatment indicator is uncorrelated with the error term in each equation, this variable can be employed as an IV to address the additional endogeneity concern.

C. *Threats to Validity*

Satisfying the uncorrelatedness, IV robustness, and exclusion conditions is crucial for the consistent estimation of the parameters in the model and of the resulting hypothesis tests. The IV robustness conditions can be tested in the data, and results will be discussed in Section VI. The uncorrelatedness and exclusion restrictions are not testable and are maintained assumptions of the model; the random assignment of the program across villages and the quasi randomness of the rainfall shock are not sufficient to ensure that these conditions hold. There are thus various potential threats to validity of the research design, which I discuss in turn.

Interpreting the program and rainfall shock variation as income shocks relies on the following assumptions: that (i) the program did not affect eligible households' consumption decisions through other channels (e.g., health knowledge) and that (ii) the rainfall shocks are random and uncorrelated with unobservable determinants of household expenditure decisions. Since it is possible that the program could have affected eligible households' choices through other channels, I follow various strategies to provide evidence that this is not likely to be the case. First, I restrict the sample to households whose program conditionality conditions are not likely to be binding (see Sec. IV.B). Second, I examine whether ineligible households in treatment villages—to the extent that they are affected by village-level interventions (i.e., if program-based community meetings for women lead to changes in their information or awareness on issues that may influence households' decisions or behavior, or in the effectiveness of women support networks, which may have analogous consequences)—change their expenditure patterns, and I do not find any evidence of changes for this group. With regard to the rainfall variation, I do not find any evidence of preshock correlations with the dependent variables or differences in predetermined characteristics of households or any evidence of major effects on relative prices.

Labor incomes may be endogenous to the households' consumption goods allocations decisions. If individuals' preferences for leisure are not separable from those for the goods used in the analysis, then total household income, which includes labor income, may not satisfy the exclusion restriction. Therefore, I assume that conditioning on the baseline indicators for agricultural laborer, wage laborer, or self-employment status and the additional socioeconomic and demographic variables, preferences for leisure are separable from those for the goods in the analysis. I further relax this assumption in some specifications by conditioning on contemporaneous product prices and wages and show that the results still hold under this more flexible specification. I defer the discussion of these results to Section VI.C.

The exogenous variation in income shocks can also lead to differential separation rates, and couples who remain in union may have different preferences for child expenditures and/or for gender-specific adult consumption. Therefore, the distribution factors could potentially be correlated with unobservable characteristics of couples who choose to remain in a relationship. To the extent that unions that would experience the largest (smallest) changes in income share allocations are more likely to dissolve as a result of the distribution factor change, it would lead to a downward (upward) bias in the estimates of β_{j1} and β_{j2}. However, since divorce rates are very low (0.61 percentage points after 2 years) and nearly identical across groups in this context, we can confidently set aside a potential selection problem due to marital dissolution.[19]

Independent of selection into marriage, divorce rates may serve as a distribution factor affecting the sharing rule. In this case, the distribution factors may have indirect effects on the sharing rule through their effects on divorce rates in the marriage market, since remarriage market options may improve women's decision-making power within

[19] Divorce rates are 0.54 percentage points higher in the program treatment group relative to the control group and 0.54 percentage points lower in the one standard deviation rainfall shock group relative to the no-shock group (table 3, col. 5). We could take this potential source of bias into consideration by estimating Lee (forthcoming) treatment effect bounds, due to the quasi experimentally driven nonrandom selection. Given the minute difference in dissolution rates across groups, the nonparametric bounds on reduced-form estimates are extremely tight.

the household (Chiappori, Iyigun, and Weiss 2006). However, this mechanism does not invalidate the identification strategy; the tests take into account the possibility that the income shocks may influence the sharing rule indirectly through effects on other distribution factors.[20] Finally, Bobonis (2004) shows that divorce rate effects in this context were minimal and new cohabitation rates of separated/divorced mothers at baseline increased, thus reducing absolute divorce ratios in the communities. This implies that the tests are robust to any indirect effect of the distribution factors on the Pareto weights, including the small marriage market effects in this context.

Another concern not previously addressed in this literature involves the possibility that the distribution factors used to identify household consumption choices may affect different subgroups of the population and therefore that differences in estimates of the demand functions arise from heterogeneity in individuals' demand functions rather than as a result of differences in individuals' decision-making power (Heckman, Urzua, and Vytlacil 2006). As a consequence, tests of Pareto efficiency in consumption that rely on tests of condition (7) may consider significant differences as evidence against the predictions of the collective model, where in reality these differences may be driven by heterogeneity in households' demand functions. Therefore, I follow a diagnostic method proposed by Angrist, Graddy, and Imbens (2000) that permits one to show evidence that the set of income shocks would not lead to estimates of significantly different distribution factor effects as a result of nonlinearities or heterogeneity in individual or household demand elasticities. I present this robustness analysis in Appendix A.

A final issue in the empirical analysis is the extent of sample attrition. If being out of sample is correlated with the likelihood of receiving treatment, this could lead to bias in the coefficient estimates. Sample attrition rates through the four survey rounds are approximately 15 percent for the samples of women in union at baseline (App. table B1, regression 1). Although attrition rates are balanced across treatment groups, the likelihood of attrition is highly correlated with individuals' observable characteristics (regressions 2–4). Therefore, to reduce the

[20] To see this, start from any equation in system (2). Letting the distribution factor z (in this case some feature of the marriage market) vary with y^A and y^O and taking the derivative with respect to y^i ($i = A, O$), we get

$$\frac{\partial q_j}{\partial y^i} = \frac{\partial q_j}{\partial \lambda} \cdot \left(\frac{\partial \lambda}{\partial y^i} + \frac{\partial \lambda}{\partial z} \cdot \frac{\partial z}{\partial y^i}\right).$$

The proportionality condition is modified to include the indirect effect of the income source on the marriage market condition. However, since the ratio of the (direct and indirect) effects on the Pareto weight does not involve anything specific to good j, the distribution factors' effects on the consumption of each good are again equally proportional to the distribution factors' influence in the decision makers' Pareto weights. I thank a referee for this clarification.

extent of potential attrition bias, I control for baseline women, partner, and household's characteristics in all specifications.

In summary, on the basis of this series of analyses to assess the robustness, exogeneity, and the degree of heterogeneity of impacts of the various instruments, we can be more confident that the exogenous variation used actually satisfies the conditions necessary for identification.

VI. Results

A. *Tests of Income Pooling and Pareto Efficiency in the Collective Household Model*

Table 4 reports estimates of the system of demand equations based on equation (8). The specifications use the rainfall shock indicator as the rainfall shock measure, a linear control for the (endogenous) total household expenditures, instrumented with the monthly household income measure. The coefficient estimates indicate that, conditional on aggregate household expenditures, the PROGRESA program led to a 42.0 percent increase in the adult female clothing expenditure share, a 156.3 percent increase in the child clothing expenditure share, and a 10.1 percent decrease in the alcohol and tobacco expenditure share, among other changes in expenditure patterns (table 4, col. 1). In contrast, the distribution factor-specific effect of the rainfall shock led to reductions of 100 percent, 45.6 percent, and 59.8 percent in the expenditure shares on adult female clothing, adult male clothing, and child clothing, respectively (col. 2). In addition, it led to an average increase of 60.2 percent in the alcohol and tobacco expenditure share. These estimates are consistent with the existing empirical literature, which shows that an increase (decrease) in the share of women's income in the household leads to reallocations toward (away from) goods arguably preferred by female decision makers (e.g., Lundberg, Pollak, and Wales 1997). On the basis of the above discussion, I can formally indicate that the estimates are clearly inconsistent with the unitary model of intrahousehold allocation. Moreover, the joint test of significance of all distribution factor parameter estimates rejects that these are equal to zero ($\chi^2(26)$ statistic $= 297.3$; p-value < 0.001).

The possible weak instruments problem is not of concern. The first-stage F-statistic is 34.8, which clearly satisfies the Kleibergen and Paap (2006) critical value for strength of instruments under heteroscedasticity. The results are also robust to allowing for a more flexible relationship between the expenditure shares–aggregate household expenditures semi-elasticities (i.e., allowing for a higher-order polynomial), assuming exogeneity of household resources in the demand system, and including

TABLE 4
TESTS OF UNITARY AND COLLECTIVE RATIONALITY MODELS OF INTRAHOUSEHOLD RESOURCE ALLOCATION, SYSTEM OF UNCONDITIONAL DEMAND FUNCTIONS

Dependent Variable: ln(Expenditure Shares in . . .)	Treatment Indicator (1)	Deviation from Mean Rainfall > 1 SD Rainfall (2)	Total Household Expenditures (Tens of Pesos) (3)	Controls (4)
Female clothing	.351**	−.781**	.007	Yes
	(.140)	(.254)	(.006)	
Male clothing	.173	−.608**	.011**	Yes
	(.133)	(.204)	(.006)	
Child clothing	.941**	−.912**	−.002	Yes
	(.143)	(.256)	(.006)	
Schooling	.141	.280	.008⁺	Yes
	(.123)	(.241)	(.005)	
Fruits and vegetables	.252**	.293**	.006**	Yes
	(.077)	(.126)	(.003)	
Cereals and grains	−.073	−.033	.001	Yes
	(.055)	(.099)	(.003)	
Meats	.193*	−.243	.024**	Yes
	(.105)	(.221)	(.004)	
Other food	.037	.229**	−.002	Yes
	(.041)	(.062)	(.002)	
Transport	−.014	.378*	.014**	Yes
	(.095)	(.199)	(.004)	
Alcohol and tobacco	−.106**	.471**	.006**	Yes
	(.051)	(.165)	(.003)	
Hygiene	−.019	.208*	.001	Yes
	(.050)	(.108)	(.003)	
Medicine	−.159⁺	.342⁺	.007	Yes
	(.107)	(.232)	(.006)	
Other household goods	−.068	.900**	.019**	Yes
	(.151)	(.188)	(.005)	
Unitary rationality test		$\chi^2(26) = 279.3$ [p-value < .001]		
Proportionality test		$\chi^2(12) = 17.5$ [p-value = .26]		

NOTE.—IV coefficient estimates and standard errors are presented; disturbance terms are clustered at the village level. The excluded IV is the monthly household income in the 6-month period preceding the survey. For a list of controls, see the note to table 3. Nonlinear Wald tests of (i) the joint significance of the distribution factors' correlations (unitary rationality test) and of (ii) the joint significance of the ratio of distribution factors' correlations (proportionality test) are $\chi^2(26)$ and $\chi^2(12)$ statistics, and the respective p-value of each test is also reported. The sample is composed of all eligible households in union since baseline with children 9 years old or younger at baseline. $N = 11{,}733$.
⁺ Significant at the 85 percent confidence level.
* Significant at the 90 percent confidence level.
** Significant at the 95 percent confidence level.

village-level product price controls.[21] Finally, the benchmark distribution factor effects, as well as the unitary and collective rationality tests, are also robust to using the alternative rainfall shock measure.[22] Overall,

[21] Estimates including price controls are shown in App. table B2. Additional estimates are available on request.
[22] Estimates are available from the author on request. See, e.g., panel B of App. table B2.

these results give credence to the validity of the identifying assumptions and the robustness of the results.

The evidence presented in this empirical analysis is consistent with various interpretations, not necessarily mutually exclusive. First, as shown in Blundell et al. (2005), the heterogeneous source-specific income elasticities on children's clothing may be explained by a theoretical underpinning of the collective model: if the PROGRESA transfer shifts the Pareto weight in favor of the female partner, the demand for the household public good should increase if women's marginal willingness to pay for children's goods is more sensitive to changes in her decision-making power than that of other decision makers in the household. A related explanation for this heterogeneous use of household incomes could be obtained from existing social norms in rural Mexico regarding the uses of incomes from different sources in consumption choices within the household. Since most female partners' incomes may go entirely into a household common fund used to cover basic household expenditures, to the extent that the PROGRESA transfer income and the farm household profits from agriculture are considered, respectively, female- and male-specific or joint incomes, we should observe a greater use of the former in household public goods and more of the latter in adult males' private consumption. These predictions are supported by the empirical results.

Finally, the program conditionality restrictions may have imposed informal expectations on households to change expenditure patterns in a way consistent with the program objectives. Although I have used a sample of households that are unlikely to be directly affected by the program's conditionality constraints, I cannot completely rule out that this factor may have played an important role in explaining the differential allocation of resources across household consumption goods.

My results are consistent with the predictions of the collective model. The Wald test of the (nonlinear) proportionality condition fails to reject the null hypothesis of Pareto efficiency in intrahousehold allocations ($\chi^2(12)$ statistic $= 17.5$; p-value $= 0.26$), suggesting that (decision-making) household members make efficient allocations of household resources toward individual private, composite, and public goods. That said, the failure to reject the proportionality condition may reflect the fact that some of the coefficients are somewhat imprecisely estimated or that the nonlinear test may suffer from low power. In the next subsections, I present evidence regarding the robustness of the tests of Pareto efficiency to alternative formulations and to other potential threats to validity.

In sum, two conclusions arise from the evidence. First, the estimates indicate that we cannot reject Pareto efficiency in intrahousehold allocations. Second, the evidence suggests that program-based female income changes disproportionately increase the consumption of female adult and children's goods relative to those of adult male private goods and that rainfall-driven income shocks disproportionately decrease the consumption of adult female and child goods while also decreasing alcohol and tobacco expenditures.

B. *Tests of Pareto Efficiency Based on Estimates of z-Conditional Demand Functions*

Table 5 reports the estimates of the system of z-conditional demand functions (eqq. [10]). The main specification includes the shock indicator as the rainfall shock measure, a linear control for total household expenditures, and the natural logarithm of the child clothing expenditure share, both instrumented with the monthly household income measure and the program treatment indicator (table 5, panel A). Although the coefficient estimates on the rainfall shock indicator are significantly different from zero for specific goods (i.e., expenditure shares on female and male clothing, fruits and vegetables, other foods, alcohol and tobacco, hygiene goods, and other household goods), suggesting that the proportionality condition is not satisfied in some cases, we fail to reject the proportionality condition for each type of good, and the joint test of significance of the rainfall shock coefficients fails to reject the null hypothesis ($\chi^2(12)$ statistic = 73.6; p-value = 0.43). In addition, since the program's conditions may induce households to modify their consumption decisions in order to satisfy these constraints, the proportionality conditions may not hold for those goods likely to be affected by the program restrictions. However, we also fail to reject the proportionality test for the subset of goods unlikely to be affected by these constraints: adult male clothing, adult female clothing, alcohol and tobacco, transportation, and other household goods expenditures ($\chi^2(5)$ statistic = 31.2; p-value = 0.242; not shown in the table). Since Bourguignon et al. (2009) show that the proportionality condition is equivalent to the condition that the coefficient estimate on the rainfall shock distribution factor is equal to zero for at least one good, our individual and joint tests support the Pareto efficiency condition.

The tests from this alternative specification are also robust to using the alternative rainfall shock measure (table 5, panel B). In this case, the coefficient estimates on the rainfall shock measure are significantly different from zero for a different set of goods (i.e., expenditure shares on education, fruits and vegetables, other foods, hygiene goods, and other household goods), but we again fail to reject the proportionality condition for each type of good. The joint test of significance of the rainfall shock coefficients fails to reject the null hypothesis ($\chi^2(12)$ statistic = 62.0; p-value = 0.10).[23] In summary, I conclude that the alternative tests also fail to reject the idea that household partners make allocation decisions in a Pareto-efficient manner.

C. *Other Robustness Tests*

Interpreting the program and rainfall shock variation as valid income shocks relies on various assumptions, and violations of these could lead to wrong conclusions regarding the tests presented above. We need to assume that the program and rainfall shocks did not affect eligible households through other channels. Such a situation would invalidate our uncorrelatedness or exclusion assumptions, and we would be mistakenly attributing the effects of other mechanisms to distribution factor effects. In this subsection, I present a series of robustness checks to show that I am in fact providing consistent estimates of the distribution factor effects.

Information, Social Marketing, and Relative Price Effects

A first concern we face is the possibility that the program affected households' allocation decisions through information campaigns or women's meetings in the communities regarding child care and well-being, potentially affecting individual preferences (Adato, Coady, and Ruel 2000). I examine whether ineligible households in treatment villages—to the extent that they are affected by village-level information or women community group interventions—change their expenditure patterns, and whether conditioning on these potential differences we find program-driven income effects on eligible households. A second concern relates to the fact that, although the rainfall shocks can be considered random and uncorrelated with observable determinants of household expenditure decisions, they may still affect expenditure patterns due to relative product or labor price changes rather than due to changes in land rents. I address both concerns by using the sample of eligible and in-

[23] The possible weak instruments problem is not of concern in either specification. The first-stage $\chi^2(4)$ statistics of joint significance of the PROGRESA indicator and the household income in the expenditures and child clothing first-stage equations are 103.8 and 104.1, which clearly satisfy the robustness condition. The results are again robust to allowing for a more flexible relationship between the expenditure shares–aggregate resources semi-elasticity (e.g., allowing for a higher-order polynomial; see App. table B3) and assuming exogeneity of household resources in the demand system (estimates available on request).

TABLE 5
Tests of Collective Rationality Model of Intrahousehold Resource Allocation, z-Conditional Demand Functions

Dependent Variable: ln(Expenditure Shares in . . .)	Deviation from Average Rainfall > 1 SD Rainfall (1)	Total Household Expenditures (Tens of Pesos) (2)	ln(Child Clothing Budget Share) (3)	Controls (4)
	A. Explanatory Variable: Deviation from Average Rainfall > 1 Standard Deviation			
Female clothing	−.440*	.007*	Yes	Yes
	(.253)	(.004)		
Male clothing	−.440**	.011**	Yes	Yes
	(.184)	(.005)		
Schooling	.417^{+}	.008*	Yes	Yes
	(.262)	(.005)		
Fruits and vegetables	.537**	.007**	Yes	Yes
	(.150)	(.003)		
Cereals and grains	−.103	.001	Yes	Yes
	(.100)	(.003)		
Meats	−.056	.025**	Yes	Yes
	(.240)	(.004)		
Other food	.265**	−.001	Yes	Yes
	(.073)	(.002)		
Transport	.363*	.013**	Yes	Yes
	(.218)	(.003)		
Alcohol and tobacco	.368**	.006**	Yes	Yes
	(.172)	(.003)		
Hygiene	.191*	.001	Yes	Yes
	(.112)	(.003)		
Medicine	.187	.007	Yes	Yes
	(.240)	(.006)		
Other household goods	.834**	.019**	Yes	Yes
	(.213)	(.005)		
Distribution factor joint conditionality test	$\chi^2(12) = 73.6$ [p-value = .43]			

Dependent Variable: ln(Expenditure Shares in . . .)	Deviation from Average Rainfall (1)	Total Household Expenditures (Tens of Pesos) (2)	ln(Child Clothing Budget Share) (3)	Controls (4)
	B. Explanatory Variable: Deviation from Average Rainfall			
Female clothing	−.0018	.007*	Yes	Yes
	(.0014)	(.004)		
Male clothing	−.0006	.011**	Yes	Yes
	(.0014)	(.005)		
Schooling	.0058**	.009*	Yes	Yes
	(.0016)	(.005)		
Fruits and vegetables	.0026**	−.007**	Yes	Yes
	(.0009)	(.003)		
Cereals and grains	.0006	.001	Yes	Yes
	(.0006)	(.003)		

TABLE 5
(*Continued*)

Dependent Variable: ln(Expenditure Shares in . . .)	Deviation from Average Rainfall (1)	Total Household Expenditures (Tens of Pesos) (2)	ln(Child Clothing Budget Share) (3)	Controls (4)
Meats	.0016	.025**	Yes	Yes
	(.0016)	(.004)		
Other food	.0016**	−.001	Yes	Yes
	(.0005)	(.002)		
Transport	.0017+	.014**	Yes	Yes
	(.0011)	(.003)		
Alcohol and tobacco	.0014+	.006**	Yes	Yes
	(.0009)	(.003)		
Hygiene	.0015*	.001	Yes	Yes
	(.0008)	(.003)		
Medicine	−.0012	.007	Yes	Yes
	(.0014)	(.006)		
Other household goods	.0065**	.020**	Yes	Yes
	(.0014)	(.005)		
Distribution factor joint conditionality test	$\chi^2(12) = 62.0$ [*p*-value = .104]			

NOTE.—Coefficient estimates and standard errors from IV regressions are reported; disturbance terms are clustered at the village level. The excluded IVs are the PROGRESA treatment village indicator and the monthly household income in the 6-month period preceding the survey. For a list of controls, see the notes to table 3. Joint tests of collective rationality are based on a system of equations estimated by system OLS; disturbance terms are clustered at the village level and allowed to be correlated across equations. Wald tests of the joint significance of distribution factors' correlations in the *z*-conditional demand system (collective rationality test) are $\chi^2(12)$ statistics; the respective *p*-value of each test is also reported. The sample is composed of all eligible households in union since baseline with children 9 years old or younger at baseline. $N = 11{,}733$.

+ Significant at the 85 percent confidence level.

* Significant at the 90 percent confidence level.

** Significant at the 95 percent confidence level.

eligible households to estimate the following specification—an augmented version of equation (8):

$$\ln(q_{jic}) = \alpha_j + \beta_{j1} T_c E_{ic} + \beta_{j2} R_c + \beta_{j3} T_c + \beta_{j4} E_{ic} + p_{kic}\beta_{j5} + \beta_{j6} w_{ic} + f(x_{ic}) + \mu_{ic}\delta_j + \xi_{jic}, \tag{11}$$

where E_{ic} is a household-level program eligibility indicator, p_{kic} is a vector of agricultural goods prices, w_{ic} is the mean of household members' daily wage, and the other variables are defined as above. In this specification, the β_{j3} coefficient captures spillover effects of the program, including village-level information effects, on ineligible household expenditure decisions (under the assumption of no spillover effects in household income), and the β_{j1} coefficients on the $T_c E_{ic}$ interaction term can be interpreted as the program's income-driven distribution factor effect. This specification, in addition to controlling for common village-level confounding factors resulting from the intervention, includes village-level agricultural product prices and a measure of household daily

TABLE 6
Robustness Tests of Unitary and Collective Rationality Models of Intrahousehold Resource Allocation

Dependent Variable: ln(Expenditure Shares in . . .)	Treatment Indicator × Eligible (1)	Deviation from Mean Rainfall > 1 SD Rainfall (2)	Total Household Expenditures (Tens of Pesos) (3)	Household, Individual, and Price Controls (4)
Female clothing	−.150	.049	.022**	Yes
	(.256)	(.264)	(.005)	
Male clothing	−.156	.261	.025	Yes
	(.223)	(.199)	(.005)	
Child clothing	.547**	−.146	.005	Yes
	(.224)	(.291)	(.006)	
Schooling	−.098	−.020	.010**	Yes
	(.256)	(.258)	(.005)	
Fruits and vegetables	.223**	.200*	.001	Yes
	(.076)	(.118)	(.002)	
Cereals and grains	−.031	−.179	−.001	Yes
	(.078)	(.078)	(.002)	
Meats	.365**	−.169	.011**	Yes
	(.166)	(.242)	(.003)	
Other food	.005	−.018	−.002*	Yes
	(.067)	(.076)	(.001)	
Transport	.133	.458*	.007**	Yes
	(.155)	(.217)	(.003)	
Alcohol and tobacco	−.130	.368**	.005**	Yes
	(.119)	(.162)	(.002)	
Hygiene	.161+	.248**	.000	Yes
	(.106)	(.121)	(.002)	
Medicine	−.471**	.012	.002	Yes
	(.235)	(.233)	(.006)	
Other household goods	−.027	1.168**	.014**	Yes
	(.214)	(.229)	(.004)	
Unitary rationality test		$\chi^2(26) = 92.5$ [p-value < .001]		
Proportionality test		$\chi^2(12) = 12.3$ [p-value = .22]		

Note.—IV coefficient estimates and standard errors are presented; disturbance terms are clustered at the village level. The excluded IV is the monthly household income in the 6-month period preceding the survey. For a list of controls, see the note to table 3. Nonlinear Wald tests of (i) the joint significance of the distribution factors' correlations (unitary rationality test) and of (ii) the joint significance of the ratio of distribution factors' correlations (proportionality test) are $\chi^2(26)$ and $\chi^2(12)$ statistics, and the respective p-value of each test is also reported. The sample is composed of all eligible and ineligible households in union since baseline with children 9 years old or younger at baseline. $N =$ 11,133.
$^+$ Significant at the 85 percent confidence level.
* Significant at the 90 percent confidence level.
** Significant at the 95 percent confidence level.

wages, both of which may be confounded with the rainfall shocks and may affect household consumption decisions through price effects. The sample drops to 11,133 observations (including ineligible households) because we do not have household-level wage data for all households. Nonetheless, the estimates are robust to including mean village-level wages as an alternative control for wage effects (not shown).

Table 6 reports estimates of this alternative system of demand equations. The specifications use the rainfall shock indicator and a linear control for the (endogenous) total household expenditures, instrumented with the monthly household income measure. The coefficient estimates indicate that the program led to significant increases in household public and collective goods, in particular, child clothing, fruits and vegetables, meats, and medical expenditures (table 6, col. 1). In contrast, the distribution factor-specific effects of the rainfall shock are concentrated in alcohol and tobacco and transportation expenditures, among other goods (col. 2). The estimated distribution factor effects for adult female and male clothing expenditure shares are substantially smaller, and most are insignificantly different from zero, estimates that suggest that the collective model restrictions are validated by the data. Again, the unitary rationality model is rejected by the data ($\chi^2(26)$ statistic = 92.5; p-value < 0.001), and we fail to reject the proportionality conditions ($\chi^2(12)$ statistic = 12.3; p-value = 0.22).[24] In sum, I conclude that these specifications support the identifying assumptions of and results from the econometric model.

Conditionality and Constrained Pareto Efficiency

As mentioned above, since the PROGRESA program's conditions may induce households to modify their consumption decisions in order to satisfy the intervention's conditionality constraints, the proportionality condition may not hold for those goods likely to be affected by the program restrictions. Since this is likely to affect a set of composite and child public goods, I also estimate models that focus on individual adult goods. These empirical models are consistent with testing for constrained Pareto efficiency, a more flexible collective model in which individual adult goods allocation decisions—conditional on the allocation of resources toward household public goods—are Pareto efficient (Blundell et al. 2005). In this framework, the solution to the household problem can be thought of as a two-stage process. In the first stage, decision makers agree on the level of public expenditures, as well as on a particular distribution of the residual income between decision makers for their private consumption. In the second stage, each decision maker chooses private consumption to maximize his or her utility, conditional on the level of public expenditures and the individual residual income budget constraint. This alternative model predicts that individual private goods allocation decisions satisfy condition (3).

I carry out a test of constrained Pareto efficiency by estimating a z-

[24] All results are robust to allowing for a more flexible relationship between the expenditure shares–resource semi-elasticities (e.g. allowing for a higher-order polynomial) and assuming exogeneity of household resources in the demand system. Estimates are available from the author on request.

TABLE 7
Tests of Constrained Pareto Efficiency, z-Conditional Demand Functions Estimates
Dependent Variable: ln(Expenditure Share in Adult Male Clothing)

	(1)	(2)	(3)	(4)	(5)	(6)
Deviation from average rainfall > 1 SD	−.223	−.181	.102	.152	.163	.167
	(.201)	(.215)	(.643)	(.156)	(.157)	(.155)
Total household expenditures (in tens of pesos)	Yes	Yes	Yes	Yes	Yes	Yes
(Total household expenditures)^2		Yes	Yes		Yes	Yes
(Total household expenditures)^3			Yes			Yes
ln(adult female clothing budget share)	Yes	Yes	Yes	Yes	Yes	Yes
Household controls	Yes	Yes	Yes	Yes	Yes	Yes
Price controls	No	No	No	Yes	Yes	Yes
F-statistic: treatment indicator in adult female clothing budget share first-stage regression	8.20	8.29	8.17	10.46	10.62	10.51
p-value	.004	.004	.005	.001	.001	.001
First-stage χ^2 statistic (all excluded instruments)	101.2	285.0	304.3	97.2	258.0	301.4
p-value	<.0001	<.0001	<.0001	<.0001	<.0001	<.0001

Note.—Coefficient estimates and standard errors from IV regressions are reported; disturbance terms are clustered at the village level. The excluded IVs are the PROGRESA treatment village indicator and the monthly household income in the 6-month period preceding the survey, included in the same polynomial order as the household expenditure term(s). For a list of household and price controls, see the note to tables 3 and 6, respectively. The sample is composed of all eligible households in union since baseline with children 9 years old or younger at baseline.

conditional demand function for the adult male clothing expenditures share, in which the conditioning good is the adult female clothing expenditure share. The main specification includes the shock indicator as the rainfall shock measure, a linear control for total household expenditures and the (natural log of) adult female clothing expenditures, both instrumented by the monthly household income measure and the program treatment indicator, as well as the baseline household and individual characteristics controls (table 7, col. 1). Under the null hypothesis of constrained Pareto efficiency, the reduced-form coefficient estimate on the rainfall shock indicator is equivalent to the proportionality condition for these two goods.

Again, estimates of the z-conditional demand function for adult male clothing fail to reject this alternative model (table 7). The conclusions are robust to the inclusion of higher-order polynomials on the aggregate household resources measure (cols. 2 and 3), to the inclusion of price effects (cols. 4–6), and to the use of the alternative rainfall shock variable as the measure of the alternative distribution factor (results not shown).

These alternative tests confirm that households make (constrained) Pareto-efficient allocation decisions.

VII. Conclusion

This paper presents new quasi-experimental evidence that farm households are able to make Pareto-efficient allocations with respect to consumption decisions. I do so in a context in which gender-specific property rights over land are clearly established and marital dissolution is not a prominent phenomenon. Combining variation in women's income generated by the PROGRESA program evaluation with income variation attributable to localized rainfall shocks, I find no evidence against the main testable prediction of Pareto efficiency: that consumption good demand responses are proportional to changes in the distribution of decision makers' resources. This result implies that although different sources of income are allocated to different uses depending on the identity of the income earner, it does not prevent an efficient allocation of family resources. However, I find evidence consistent with previous work that female-specific income changes have a substantial effect on children's goods expenditure shares, whereas income changes due to rainfall shocks have a lesser influence on household public goods expenditures. This evidence is consistent with various interpretations: female partners' greater sensitivity of their marginal willingness to pay to own-income changes and social norms that may oblige women to devote their earnings to meet collective consumption needs.

Can we reconcile the evidence presented in this paper with the rejection of Pareto efficiency in West African contexts? Ethnographic studies of West African households consistently show that partners have their own individual budgets, control resources, and make consumption decisions fairly independently, and Duflo and Udry (2004) provide evidence consistent with a certain lack of cooperation in these expenditure decisions. Moreover, Udry (1996) also shows some degree of inefficiency in households' agricultural production decisions. Comparing the agricultural yields and the composition of inputs across plots under the managerial control of men and women within the same household, he finds that men—being wealthier and therefore less likely to be credit constrained—use more fertilizer on their own plots than in their partner's plots. As Duflo (2005) argues, a solution to increase household productivity would be for the female partner to rent or sell her land to the husband, but this transaction may not happen because of women's weak property rights over land in these contexts. However, as argued in Akresh (2008), these constraints may be quite specific to the West African setting studied in Udry (1996). We may thus not expect that the constraints or interactions governing the distribution of resources within the household extend to other contexts—such as the Mexican countryside—in which property rights institutions are stronger and more homogeneous across individuals.

Appendix A

Diagnostic Test of Unobserved Heterogeneity in Distribution Factor Effects

A concern that has not been previously addressed in the intrahousehold allocation literature involves the possibility that the distribution factors used to identify household consumption choices effects may affect different subgroups of the population and therefore that differences in estimates of the demand functions arise from heterogeneity in individuals' demand functions rather than as a result of differences in individuals' decision-making power (Heckman et al. 2006). Moreover, tests of Pareto efficiency in consumption rely on tests of condition (7) and may consider significant differences as evidence of violations of the predictions of the collective model, where in reality these differences may be driven by heterogeneity in households' demand functions.

I follow a diagnostic method proposed by Angrist et al. (2000) that permits me to show suggestive evidence that the set of distribution factors may not lead to estimates of significantly different income effects as a result of nonlinearities or heterogeneity in individual or household demand income elasticities. Under the usual monotonicity assumption in IV estimation with heterogeneous treatment effects—in this application, that a particular instrument leads to increases (or decreases) in household expenditures for all households—the IV estimator with one instrument is a weighted average of individuals' derivatives of the demand function with respect to income, where the weights for each subgroup are the proportion of observations under the range in which the instruments affect household income. As an example, in this application, it is possible that the PROGRESA program affected household income in lower ranges of the income distribution because the program was more likely to increase poorer households' incomes, whereas the flood shock affected households in the upper ranges of the income distribution because households with more land could have been more likely to be negatively affected by the shock. If this were the case, the two instruments could estimate income elasticities, respectively, for households in the lower and upper ranges of the villages' income distributions, and these could differ because of demand heterogeneity.

I estimate the (numerator of the) weight functions to assess whether the two variables affect different ranges of the household expenditure distribution, as suggested by Angrist et al. (2000). These can be estimated as the difference in the empirical distribution functions of x_{ic} evaluated at x for each instrument $T_c = 1$ versus $T_c = 0$, or $R_c = 1$ versus $R_c = 0$, where I use as the rainfall shock measure the indicator variable for a large rainfall shock in the current period (fig. A1).[25] As can be seen in the figure, the two variables affect household

[25] Unfortunately, this analysis can be performed only for variables that take discrete values. Therefore, I cannot perform the analysis for the mean rainfall intensity instrument.

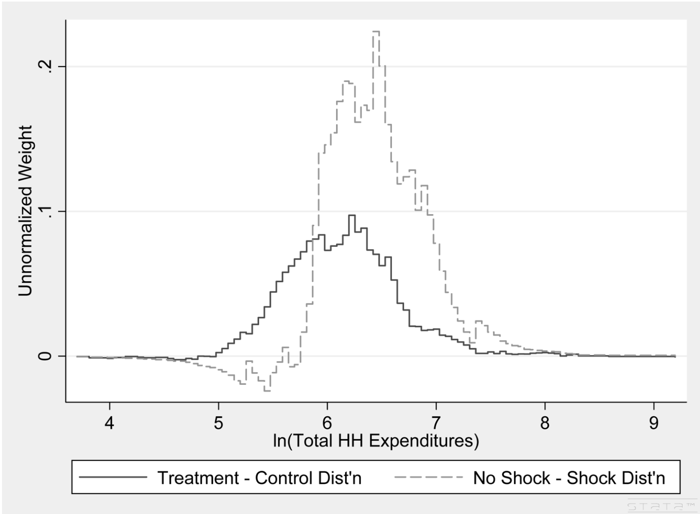

FIG. A1.—Differences in ln(total household expenditures) cumulative distribution functions by program groups and rainfall shock groups, October 1998–November 1999. The lines represent estimates of differences in ln(total household expenditures) distribution functions for households in program and comparison villages (solid line) and households that suffer (or do not suffer) a rainfall shock greater than one standard deviation of the average rainfall in the 6-month period preceding the survey round (dashed line).

expenditures in relatively similar, although not perfectly overlapping, ranges of the household expenditures distribution. The program led to an increase in household expenditures in the [150, 1,800] pesos range (a [5, 8] natural log of the total household expenditures range), and the rainfall shock led to reductions in expenditures in the [315, 1,800] pesos range (a [5.75, 8] natural log of the household expenditures range).[26] This analysis may minimize concerns that the differential effects may be explained by treatment effect heterogeneity across points of the household expenditure distribution.

[26] Note that the differences in the magnitude of the changes in the distribution are consistent with the higher estimated effects of the rainfall shock on household expenditures rather than the effects of the program. However, these differences in magnitude of the changes in expenditure levels do not affect my description of the group or income range of households whose expenditure levels are being affected by the two instruments.

Appendix B

TABLE B1
Relationship between Attrition and Characteristics of Individuals at Baseline
Dependent Variable: Attrition Indicator

	Treatment (1)	Correlates (2)	Main Effect of Correlates (3)	Interaction with Treatment (4)
Constant	.148**	.189**	.211**	. . .
	(.009)	(.032)	(.061)	
Treatment indicator	.005	. . .	−.034	. . .
	(.012)		(.072)	
Woman's age:				
26–35		−.003	−.019	.027
		(.010)	(.015)	(.019)
36–45		−.013	−.045*	.055
		(.019)	(.027)	(.038)
46–55		−.074*	−.097**	.031
		(.033)	(.046)	(.066)
Woman's school < primary		.011	.008	.004
		(.010)	(.015)	(.019)
Nonindigenous woman		.021	.024	−.006
		(.024)	(.044)	(.052)
Woman wage laborer		.028	.058	−.047
		(.029)	(.053)	(.062)
Woman self-employed		.025	.018	.015
		(.024)	(.048)	(.053)
Partner's age:				
26–35		−.030**	−.011	−.032
		(.011)	(.015)	(.021)
36–45		−.049**	−.024	−.044
		(.017)	(.028)	(.034)
46–55		.002	.046	−.071
		(.030)	(.046)	(.061)
56–65		−.134**	−.089**	−.078
		(.025)	(.039)	(.051)
66–96		−.124**	−.130**	.019
		(.030)	(.033)	(.061)
Partner's school < primary		.019**	.014	.009
		(.009)	(.014)	(.019)
Nonindigenous partner		−.012	.001	−.020
		(.024)	(.044	(.052)
Partner wage laborer		−.015	−.081*	.102*
		(.026)	(.047)	(.055)
Partner self-employed		−.021	−.090*	.106*
		(.028)	(.050)	(.060)
Partner agricultural worker		−.060**	−.093*	.054
		(.029)	(.055)	(.063)
Partner nonwage laborer		−.039	−.090	.182**
		(.049)	(.055)	(.080)
Cohabiting couple		−.018*	.039**	−.035
		(.010)	(.019)	(.022)
Home with dirt floor		−.013	−.050	−.013
		(.010)	(.017)	(.021)

TABLE B1
(*Continued*)

	Treatment (1)	Correlates (2)	Main Effect of Correlates (3)	Interaction with Treatment (4)
Own home		−.054**	−.061**	.014
		(.015)	(.023)	(.030)
Agricultural land		.002	.000	.004
		(.002)	(.003)	(.003)
Number of boys aged:				
0–5		−.012**	.002	−.023**
		(.006)	(.008)	(.011)
6–7		−.023**	−.001	−.035*
		(.009)	(.015)	(.019)
8–9		−.034**	−.018	−.023
		(.010)	(.015)	(.020)
Number of girls aged:				
0–5		−.006	−.002	−.008
		(.005)	(.009)	(.011)
6–7		−.031**	−.046**	.023
		(.008)	(.012)	(.016)
8–9		−.020*	.001	−.033
		(.011)	(.017)	(.023)
F (treatment interactions)			1.31	
p-value			.13	

Note.—Robust standard errors are in parentheses; disturbance terms are allowed to be correlated within villages, but not across villages. Columns 3 and 4 present results from one regression with main effects (col. 3) and all covariates interacted with treatment (col. 4). $N = 15{,}564$.
* Significant at the 90 percent confidence level.
** Significant at the 95 percent confidence level.

TABLE B2

Tests of Collective Rationality Model of Intrahousehold Resource Allocation, *z*-Conditional Demand Functions with Price Effects

	A. Explanatory Variable: Deviation from Average Rainfall > 1 Standard Deviation				B. Explanatory Variable: Deviation from Average Rainfall			
Dependent Variable: ln(Expenditure Shares in . . .)	Deviation from Average Rainfall > 1 SD Rainfall (1)	Total Household Expenditures (Tens of Pesos) (2)	ln(Child Clothing Budget Share) (3)	Household and Price Controls (4)	Deviation from Average Rainfall (1)	Total Household Expenditures (Tens of Pesos) (2)	ln(Child Clothing Budget Share) (3)	Household and Price Controls (4)
Female clothing	.019	.013**	Yes	Yes	.0005	.013**	Yes	Yes
	(.232)	(.005)			(.0013)	(.005)		
Male clothing	.159	.019**	Yes	Yes	.0021	.019**	Yes	Yes
	(.166)	(.005)			(.0014)	(.005)		
Schooling	.094	.009*	Yes	Yes	.0027+	.009*	Yes	Yes
	(.247)	(.005)			(.0017)	(.005)		
Fruits and vegetables	.225*	.004	Yes	Yes	.0005	.004	Yes	Yes
	(.127)	(.003)			(.0007)	(.003)		

Cereals and grains	−.115	.000	Yes	Yes	.0003	.000	Yes	Yes
	(.103)	(.003)			(.0005)	(.003)		
Meats	−.047	.024**	Yes	Yes	.0003	.024**	Yes	Yes
	(.232)	(.004)			(.0013)	(.004)		
Other food	.019	−.002	Yes	Yes	.0008+	−.002	Yes	Yes
	(.076)	(.002)			(.0005)	(.002)		
Transport	.398*	.004**	Yes	Yes	.0012	.014**	Yes	Yes
	(.210)	(.005)			(.0011)	(.004)		
Alcohol and tobacco	.364**	.007**	Yes	Yes	.0015*	.007**	Yes	Yes
	(.176)	(.003)			(.0009)	(.003)		
Hygiene	.282**	.003	Yes	Yes	.0011	.003	Yes	Yes
	(.120)	(.003)			(.0008)	(.003)		
Medicine	.182	.008	Yes	Yes	−.0007	.008	Yes	Yes
	(.248)	(.006)			(.0014)	(.007)		
Other household goods	1.356**	.021**	Yes	Yes	.0067**	.022**	Yes	Yes
	(.232)	(.005)			(.0016)	(.006)		
Distribution factor joint conditionality test	$\chi^2(12) = 54.0$ [p-value = .41]				$\chi^2(12) = 31.3$ [p-value = .24]			

Note.—Coefficient estimates and standard errors from system of equations estimated by system OLS are presented; disturbance terms are clustered at the village level. For a list of controls, see the note to table 3. Nonlinear Wald tests of (i) the joint significance of distribution factors' correlations (unitary rationality test) and of (ii) the joint significance of the ratio of distribution factors' correlations (proportionality test) are $\chi^2(26)$ and $\chi^2(12)$ statistics, and the respective p-value of each test is also reported. The sample is composed of all eligible households in union since baseline with children 9 years old or younger at baseline. $N = 11{,}733$.

+ Significant at the 85 percent confidence level.

* Significant at the 90 percent confidence level.

** Significant at the 95 percent confidence level.

TABLE B3

ROBUSTNESS CHECKS OF TESTS OF COLLECTIVE RATIONALITY, HIGH-ORDER POLYNOMIALS IN HOUSEHOLD EXPENDITURES AND PRICE EFFECTS

	A. Second-Order Polynomial on Household Expenditures			B. Third-Order Polynomial on Household Expenditures			C. Fourth-Order Polynomial on Household Expenditures		
Dependent Variable: ln(Expenditure Shares in . . .)	Deviation from Average Rainfall 1 SD (1)	ln(Child Clothing Budget Share) (2)	Household and Individual Controls (3)	Deviation from Average Rainfall 1 SD (1)	ln(Child Clothing Budget Share) (2)	Household and Individual Controls (3)	Deviation from Average Rainfall 1 SD (1)	ln(Child Clothing Budget Share) (2)	Household and Individual Controls (3)
Female clothing	−.032	Yes	Yes	−.030	Yes	Yes	−.036	Yes	Yes
	(.248)			(.272)			(.285)		
Male clothing	.170	Yes	Yes	.162	Yes	Yes	.227	Yes	Yes
	(.174)			(.208)			(.248)		
Schooling	.192	Yes	Yes	.223	Yes	Yes	.055	Yes	Yes
	(.252)			(.269)			(.314)		
Fruits and vegetables	.310**	Yes	Yes	.355**	Yes	Yes	.156	Yes	Yes
	(.132)			(.152)			(.183)		

Cereals and grains	.004	Yes	Yes	.071	Yes	Yes	−.240⁺	Yes	Yes
	(.110)			(.129)			(.155)		
Meats	.013	Yes	Yes	.022	Yes	Yes	.018	Yes	Yes
	(.243)			(.259)			(.240)		
Other food	.013	Yes	Yes	.003	Yes	Yes	.058	Yes	Yes
	(.075)			(.076)			(.082)		
Transport	.481*	Yes	Yes	.510*	Yes	Yes	.382⁺	Yes	Yes
	(.212)			(.224)			(.246)		
Alcohol and tobacco	.414**	Yes	Yes	.044**	Yes	Yes	.298⁺	Yes	Yes
	(.181)			(.187)			(.191)		
Hygiene	.282**	Yes	Yes	.231*	Yes	Yes	.495**	Yes	Yes
	(.116)			(.126)			(.144)		
Medicine	.044	Yes	Yes	−.021	Yes	Yes	.270	Yes	Yes
	(.265)			(.296)			(.325)		
Other household goods	1.230**	Yes	Yes	1.112**	Yes	Yes	1.707**	Yes	Yes
	(.225)			(.235)			(.289)		
Distribution factor joint conditionality test	$\chi^2(12)$ = 55.3 [p-value = .24]			$\chi^2(12)$ = 45.9 [p-value = .39]			$\chi^2(12)$ = 51.9 [p-value = .27]		

NOTE.—Coefficient estimates and standard errors from a system of equations estimated by system OLS are presented; disturbance terms are clustered at the village level. For a list of controls, see the note to table 3. Nonlinear Wald tests of (i) the joint significance of distribution factors' correlations (unitary rationality test) and of (ii) the joint significance of the ratio of distribution factors' correlations (proportionality test) are $\chi^2(26)$ and $\chi^2(12)$ statistics, and the respective p-value of each test is also reported. The sample is composed of all eligible households in union since baseline with children 9 years old or younger at baseline. N = 11,733.

⁺ Significant at the 85 percent confidence level.
* Significant at the 90 percent confidence level.
** Significant at the 95 percent confidence level.

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