Comments

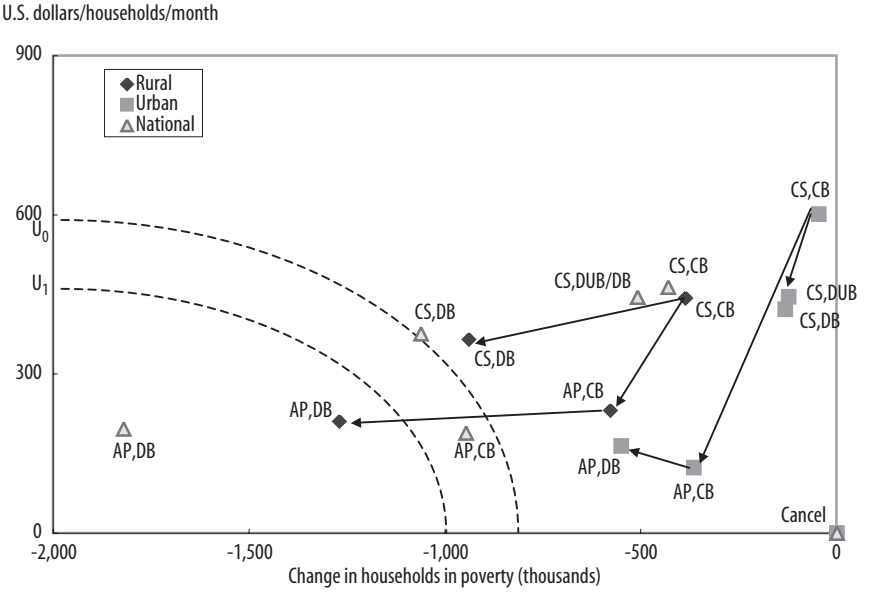
Carlos Medina: This study contributes to the better understanding of conditional cash transfer programs in several ways. It offers an empirical quantification of the effects on poverty of Oportunidades, an important program for Mexico and for other Latin American countries that have implemented similar programs; it provides an interesting benchmark for discussing the policy implications of how to implement this kind of programs and the political economy around it; and it provides an empirical example of the advantages and limitations of a specific methodology for ex ante program evaluations.

I start the discussion with the authors' quantification of the effects of Oportunidades on poverty. The paper assesses what would have happened to poverty under several scenarios: if the program had been cancelled, if its benefits were doubled under current targeting, if urban beneficiaries were doubled under current selection, if all poor were perfectly targeted by the program under current benefits, and if all poor were perfectly targeted by the program under doubled benefits. The authors estimate the economic costs of the program under each of these scenarios. The questions they ask are relevant to other Latin American and the Caribbean countries, many of which already have conditional transfer programs or might be considering them. To address these questions, the authors use the 2002 ENIGH survey to estimate an accounting exercise, which does not consider potential behavioral responses, and then estimate a behavioral model of those potential responses. Some of the key results of the accounting exercise are summarized in figure 4.

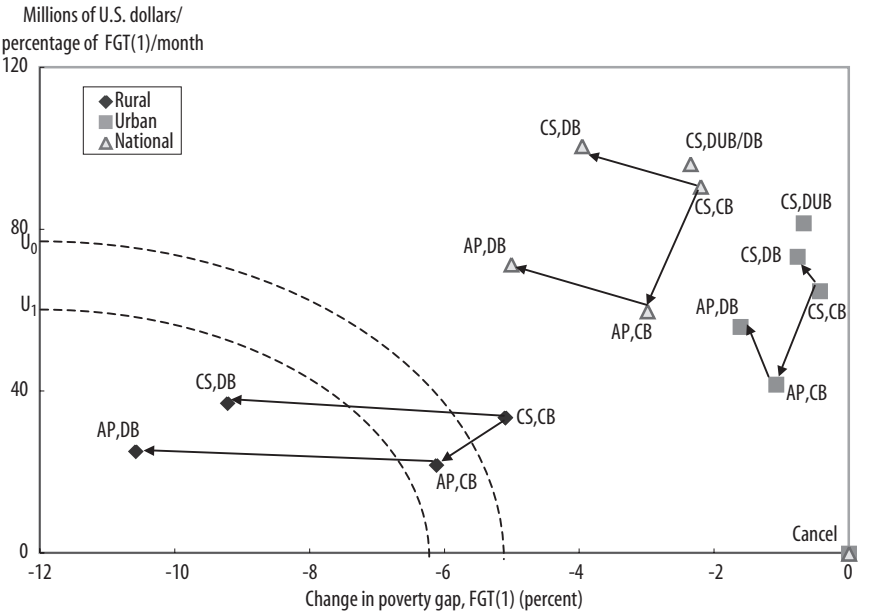
The figure illustrates the changes in the number of households below the poverty line and poverty gap, by policy or program design and by region. Panel A illustrates the status quo, in which the program is performing under the current mechanism of beneficiaries selection (*CS*) and current amount of benefits (*CB*): the *x* axis identifies how many poor households leave poverty, and the *y* axis shows how many dollars are required per month per household that leaves poverty, under different program designs. For example, the

FIGURE 4. Changes in Poverty, by Policy and Region[a]

A. Headcount: FGT (0)



B. Poverty gap: FGT (1)



Source: Freije, Bando, and Arce (in this volume, table 2).

AP: all poor; CB: current benefits; CS: current selection; DB: double benefits; DUB: double urban beneficiaries.

a. The FGT indexes are from Foster, Greer, and Thorbecke (1984).

program, as it is currently being implemented in the rural area (*CS, CB* in the figure), allows 387,000 households to leave poverty, at an average monthly cost of US$443 per poor household that leaves poverty. Perfectly targeting the current benefits given to all poor (*AP, CB* in the figure) would increase the number of households raised out of poverty to 579,000, at a monthly cost of US$231 per poor household. Doubling benefits under the current beneficiary selection mechanism (*CS, DB* in the figure) would result in 939,000 households leaving poverty, at a monthly cost of US$365 per household. The implications are similar for panel B, which illustrates the monthly cost in millions of U.S. dollars per one percent decrease in the poverty gap in the *y* axis versus the change in the poverty gap in the *x* axis. In both panels, a comparison of the results obtained under (*AP, CB*) with those obtained under (*CS, DB*) clearly illustrates that under any well behaved preference function, it would be better in urban areas to improve targeting rather than increase benefits.[1] In rural areas, some preference functions might conceivably favor improving targeting over increasing benefits. However, given that reaching all poor households is basically unfeasible, increasing benefits provides more room to reduce both households under the poverty line and the poverty gap than does improving targeting.[2]

The accounting exercise, despite its simplicity, provides immediate directions in which the program should be reoriented. As the authors conclude, increasing the amount of transfers would be very cost effective in rural areas and worthless in urban communities, while improving the program's targeting would have a relatively much larger effect in the (much worse targeted) urban areas.

The second point mentioned above is that the paper provides an excellent benchmark for discussing cash transfer programs. It highlights deep differences in the implementation of a program in rural versus urban areas, which generated strikingly different results in terms of the program's effects on poverty. In particular, the program's implementation seems to have been better planned in rural areas: it expanded from the poorest municipalities to areas that were better off; it used proxy-means tests as a targeting mechanism within municipalities; and its evaluation strategy included the random selection of beneficiary municipalities from within a sample of eligible municipalities. In the urban areas, however, beneficiaries self-selected into the program, and no sort of randomization took place.

1. In the figure, U1 > U0.
2. Moreover, improving targeting once an acceptable targeting is achieved is very costly.

Why did these differences in targeting and evaluation between rural and urban areas exist? Even if the program's planners assumed that urban areas would experience the same positive impact as the rural areas, so that no more random evaluations were needed, the program should have been targeted to the poorest households in the urban areas. This raises the issue of whether a formerly excellent program is now being mishandled, or whether there were political pressures to execute the program as soon as possible, rather than when it could be correctly implemented.[3]

The promoters of Progresa (as Oportunidades was originally called) anticipated the need for a well-conducted program evaluation, not only to assess the program's impacts, but also to help the program survive through different governments.[4] Colombia, in contrast, implemented programs in education and health for more than forty years, with no program evaluation that would allow researchers to accurately assess their impact. When the Colombian government decided to implement *Familias en Acción* (a program similar to Progresa), the multilateral banks imposed a well-designed program evaluation as a condition to funding it. Progresa was initially fully funded by the Mexican government, without the participation of multilateral banks. It was very well planned, however, and it ultimately became an example for other countries and multilateral banks regarding the benefits of program evaluation for giving stability to good programs.[5] Nonetheless, evaluations have emphasized parameters that assess whether beneficiaries drop out less often, attain more years of schooling, work fewer hours, and so forth, rather than the cost effectiveness of the program. Papers by Coady and by Coady and Parker provide cost effectiveness studies comparing the

3. Once these programs are installed, every new government must face the challenge of not only maintaining, but increasing current coverage. In its 2002 evaluation of the National Development Plan, Mexico announced that it had achieved a 31 percent increase in the coverage of Oportunidades, which included primary and secondary education as well as the introduction of transfers to higher education in 2001 (see Presidencia de la República de México, 2003).

4. See Easterly (2006); Levy (1991).

5. The World Bank cites Oportunidades as an example of good practice in education and acknowledges the World Bank's financial support of the program (see web.worldbank.org/WBSITE/EXTERNAL/BANCOMUNDIAL/NEWSSPANISH/0,,contentMDK:20549940~menuPK:1074643~pagePK:64257043~piPK:437376~theSitePK:1074568,00.html). The World Bank has funded similar programs in other countries of the region (see Duflo and Kremer, 2003), as has the Inter-American Development Bank (see the IDB's press release, "IDB Approves Its Largest-Ever Loan for Mexico: $1 Billion for Expansion of the PROGRESA Poverty-Reduction Program," January 2002, available at www.iadb.org/exr/PRENSA/2002/cp1002e.htm).

unitary costs of Progresa with the unitary costs of supply schemes (such as building additional schools and thus decreasing the average distance to the nearest school); they estimate that the subsidies are more than seven times cheaper than supply schemes.[6] The papers do not compare the unitary costs of the program with the costs of providing schooling in areas where the existing education infrastructure is insufficient to meet demand, or with providing scholarships for children admitted to selected private schools, to name just two other possibilities.[7]

In Colombia, the unitary costs of *Familias en Acción* are 5.7 and 2.5 times larger than the unitary costs of public schools for primary and secondary education, respectively. No cost effectiveness or cost-benefit study of the program has yet been undertaken, more than four years after initiation. Nonetheless, the number of families covered by the program grew from the initial 300,000 (US$120 million) in 2002, to nearly 600,000 in 2006, and the recently re-elected president aims to cover 1.5 million families by 2010.[8] The expansion plan mainly reflects political pressures in the presidential campaign, despite evidence that beneficiaries of cash transfers to secondary education were, at the most, 10 percent more likely to attend than youths in the comparison group.[9]

Finally, the behavioral model the authors use to complement the accounting exercise is limited by calibration and specification problems, resulting in inconsistency with earlier findings. Figure 1 illustrates the behavioral exercise, which differs from the accounting exercise mainly in modeling school attendance for all children and changes in labor supply and wages for chil-

6. Coady (2000); Coady and Parker (2002).

7. In the 1990s, Colombia implemented a program called PACES, which offered scholarships for secondary education to children who had recently graduated from primary school and who had been admitted to private schools. Experimental data indicate that the program is cost effective, and its impact is positive in the short and long run (see Angrist and others, 2001; Angrist, Bettinger, and Kremer, 2006).

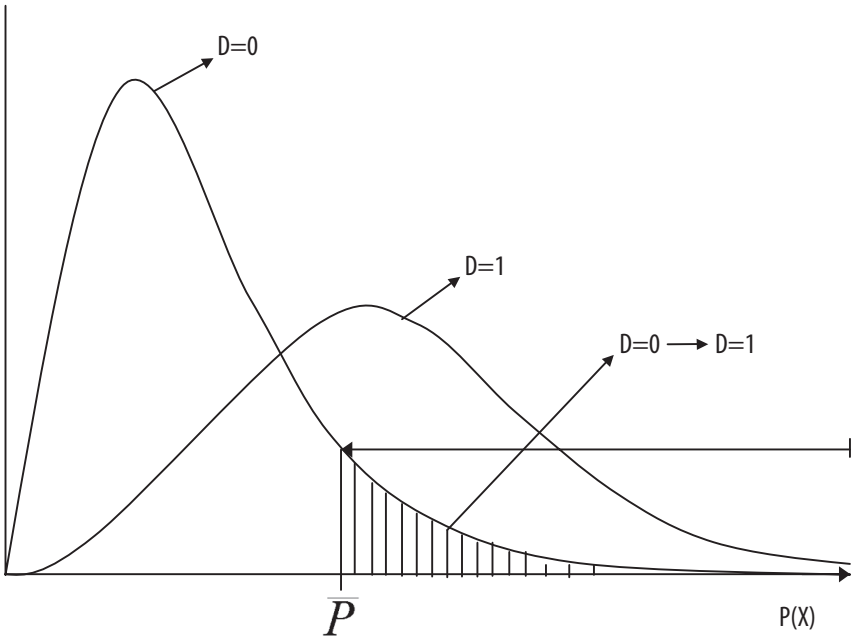
8. Figures on families covered are available at www.sigob.gov.co/pnd/indicador.aspx?m=230&i=774; see also the government plan for 2006—2010, section *Politica Social y Redistributiva* (available at www.terra.com.co/elecciones_2006/reportaje/26-05-2006/nota286033.html).

9. Beneficiary children aged twelve to seventeen register the largest impact on school attendance, at 10 percent in rural areas. Given a monthly cost of US$11 per child, the total cost of one new child attending school for one year (ten months) because of the program would be US$1,100, which is similar to the unitary cost of providing public secondary education. Nonetheless, since the child in rural areas would attend a public school, the total cost would be nearly twice the unitary cost of public secondary education. A statistically positive impact, although negligible in magnitude, was found for cash transfers to primary education. See DNP (2004, 2006).

dren aged twelve to seventeen. Prior to undertaking the behavioral and accounting exercises, however, the authors calibrated the ENIGH survey so that the number of program beneficiaries would be the same as the official figures reported by SEDESOL in 2002. Since the number of beneficiaries in the ENIGH is about 75 percent of those reported by SEDESOL, the authors model the program's targeting mechanism by estimating probit models for the urban and rural areas separately. They then proceed to estimate propensity scores for all ENIGH households and impute as beneficiaries all households in the ENIGH survey that are above a specific estimated propensity score, so that the total number of imputed households (plus those households self-reporting as beneficiaries) equals the number of beneficiaries reported by SEDESOL in 2002.

Figure 5 illustrates what the authors do with the calibration. The figure shows the relative frequencies of the propensity scores, $P(X)$, for the beneficiary households ($D = 1$) and households in the comparison group ($D = 0$).

FIGURE 5. Procedure Used to Impute Beneficiaries in the ENIGH Survey



Source: Microsimulation exercise using ENIGH 2002.

The authors' predicament is that they want to have both official figures: the number of households under the poverty line and the number of program beneficiaries reported by SEDESOL in 2002. They therefore impute as beneficiaries those in the comparison group with the largest p score ($D = 0 \rightarrow D = 1$ in the figure).

Imputing treatment this way is unlikely to generate a sample that resembles the actual distribution of treatment and comparison groups in the population. Rather, it implies an ex ante simulation in which new individuals (a third of those already covered) are covered by the program with perfect target according to current beneficiary selection procedures. Moreover, while imputing treatment to untreated households in the 2002 ENIGH may help replicate the official poverty figures, it contaminates the behavioral estimates, since imputing treatment artificially does not affect households' behavior. In other words, all behavioral model estimates will be biased because they estimate coefficients from a model that defines a quarter of the untreated as treated, which is likely to affect the model's power to simulate the program's effects. Interpretations of the accounting exercise also need to take this calibration into account, although I expect that the main conclusions in that case would not change substantially.

Regarding the specification of the labor supply model, the authors treat transfers as exogenous, even though they recognize that the selection process is merely self-selection in urban areas and that both eligible and ineligible populations are included in the sample in rural areas (although selection is random for the eligible rural population). In addition, transfers are not included in per capita family income in the empirical model, but are treated as an additional covariate. This both requires a conceptual argument (an issue that Raquel Bernal addresses in her comments on the paper) and poses an empirical difficulty, in that each of the groups (including the full-time and overtime categories) must include families receiving transfers.[10] This restriction limits the model's identifying power, since these two groups will probably only include a handful of families in the regression for children aged twelve to seventeen.

These characteristics embedded in the behavioral model might explain, at least in part, the lack of consistency between the paper's results and previous empirical estimates. The results of the behavioral model seem at odds with the available evidence cited by the authors in table 11. Buddelmeyer and

10. The authors' approach contrasts with Bourguignon, Ferreira, and Leite (2002), who include transfers as part of family income in their labor supply model.

Skoufias, for example, provide evidence on school attendance and labor supply in rural areas based on experimental impact estimates.[11] Although their estimates are not always significant under their regression discontinuity approach, their difference-in-differences experimental estimates are always statistically (or close to statistically) different from zero: positive for school attendance (0.05 for boys and around 0.09 for girls) and negative for labor supply (around –0.04 for both boys and girls).[12] Todd and others report quasi-experimental matching estimates for urban areas that are not always statistically significant for single ages (which is to be expected since there are only 131 to 423 observations per age); once they aggregate children aged 15–18, however, they find positive effects for school attendance (10.9 percent for boys) and negative effects for labor supply (–6.5 percent for boys and –5.14 percent for girls).[13]

Despite these shortcomings, the paper identifies key elements for improving the program. It also extends the discussion along several dimensions that will likely be of great help to policymakers in the region and in other developing areas, as well as to researchers interested in cash transfer programs.

Raquel Bernal: Samuel Freije, Rosangela Bando, and Fernanda Arce estimate the effects of Oportunidades, a cash transfer program in Mexico, on urban and rural poverty. They undertake both an accounting exercise and a behavioral exercise, in which they estimate a labor supply model and use it to simulate counterfactual changes in Oportunidades.

A key issue dealt with in the paper is the potential endogeneity of labor supply to changes in the cash transfer program. In a standard framework, one would expect individuals to adjust their labor supply choices to changes in household income. Thus, a crucial component of the evaluation of the impact of changes in the program on poverty measures should incorporate the possible effects on household labor supply choices. To do this, the authors estimate a labor supply model and use it to simulate several changes in Oportunidades, particularly in terms of subsidy amounts and the number of beneficiaries. The results indicate that plausible changes in the cash transfer amount are associated with very small changes in labor supply for males and females. In addition, both the accounting and behavioral exercises indicate that approximately one-third of the reduction in rural poverty since 2002 can

11. Buddelmeyer and Skoufias (2004).
12. Skoufias and Parker (2001) and Schultz (2004) find similar results.
13. Todd and others (2005).

be attributed to the implementation of Oportunidades, while urban poverty has barely changed.

This paper is a welcome addition to the literature on the effects of cash transfer programs on short-run outcomes such as poverty. Although the program is designed and implemented with the objective of increasing long-run human capital measures (such as school attainment), it is crucial to explore whether the program can actually have a short-run impact on participant households in terms of variables such as poverty and consumption. This contributes to the general understanding of household behavior and also helps policymakers improve the design of such programs. Given the widespread use of cash transfer programs in developing economies, all contributions to the evaluation of their effects are clearly relevant.

This paper also adds to the literature on estimating structural models for the purpose of assessing counterfactual policy experiments. It is very encouraging to see policymakers and researchers increasingly relying on this kind of economic tool to evaluate the effects of new policies or changes in existing ones. The use of experimental studies for the evaluation of treatment effects has been more popular than the estimation of structural models for various reasons. The latter has two clear advantages, however. First, it allows the evaluation of counterfactual changes in a given policy. In other words, one can assess the effects of different versions of a policy, without being restricted to the policy that was actually implemented during the experimental phase. Second, it allows the estimation of the long-run effects of a policy or changes in an existing policy, whereas experimental studies can usually only address short-run effects since the studies tend to be short lived as a result of high costs or political issues related to denying access to the policy to a subset of individuals.

In this particular case, however, one can exploit a potential synergy between the two approaches, given that Oportunidades (previously known as Progresa) was initially implemented as a random experiment. The experimental data provide an interesting source for validating the structural model proposed to assess counterfactual policy changes. Showing that a structural model is well-specified and hence has reasonable predictive power generally is not a straightforward proposition, but the availability of the experimental data provides a great opportunity to conduct an out-of-sample validation. This makes the results of the simulation much more reliable and robust. The authors provide some examples of this in table 11, in which they compare their results on the effects of Oportunidades on school attendance and child labor with the results from earlier experimental and

quasi-experimental studies. However, it is a bit worrying that the effects obtained from the microsimulation exercise on children's schooling are significantly different from some of the experimental results reported by various authors—in some cases by a factor of eight. Although part of this difference might be attributable to the use of different datasets, the gap is still significant, and this reduces the reliability of the model for counterfactual policy experiments.

A difference-in-differences approach using the experimental data would be useful to evaluate the effect of the cash transfers on poverty. Given that this is the paper's main objective, the authors could improve the reliability of their findings by taking advantage of the existence of the experimental data to compare their results with the experimental results; this would also provide an interesting avenue for understanding the mechanism through which the cash transfers have an effect on poverty (in this case, through changes in labor supply).

These structural (or quasi-structural) models are particularly useful when they have good predictive power for the variables of interest. In other words, a crucial step in this type of exercise is to specify and estimate a model that can correctly predict individual choices of the main variables of interest. If this is not the case, then the results of the simulation are dubious or not reliable at all. It is thus extremely important to develop a model with these characteristics. As the authors note, the predictive power of the urban labor supply model (and the program participation model) is relatively weak, probably because of the short period between the implementation of Oportunidades in urban areas and the time of the data collection. This is an important issue given that assessing the effects of changes in the program in urban areas is one of the study's main objectives.

Showing the goodness of fit of the model is crucial for providing some evidence that the model is suitable to simulate policy experiments. As the authors explain in the text, they use the estimated parameters of their labor supply model and random draws for the stochastic terms in the utility function to simulate labor supply decisions. The authors restrict these random draws such that (1) they are drawn from the appropriate distribution (namely, a type I extreme value distribution) and (2) they guarantee that the maximum simulated utility corresponds exactly to the actual (observed) choice of the individual. In other words, the simulated model perfectly fits the data by construction. The authors then use these random draws to simulate choices under different policy scenarios. An important initial step, however, would be to compare actual choices with predicted choices in the

baseline scenario (that is, no change in policy implemented) to verify the fit of the model and, hence, the strength of its predictive power. This would require taking unrestricted random draws from the appropriate distribution of the stochastic terms and using these to obtain predicted choices. It would be pointless to show goodness of fit of the model using the restricted random draws (that is, random draws that comply with the two requisites mentioned above) because simulated choices are constructed to be identical to actual choices.

A key issue that is not fully dealt with in the paper is the potential endogeneity of participation in the program to changes in transfer amounts or eligibility criteria. The cash transfers were not randomly assigned in urban areas. Instead, eligible individuals opted to participate in the program. Participation rates range from 30 percent to 55 percent depending on the data source; this suggests that the nonparticipation of eligible individuals is definitely an issue. The authors estimate a program participation model with the following two objectives: to calibrate the ENIGH (the survey data used to estimate the behavioral models) to match the official figures on the total number of program beneficiaries, given that there seems to be a significant problem of underreporting in this dataset; and to include or exclude beneficiaries in simulation exercises pertaining to the expansion or contraction of the program in terms of number of individuals. For example, if the experiment amounts to assessing the effects of doubling the number of urban beneficiaries, then the authors rank individuals according to their p score (obtained from the participation model) and include in the program those individuals with values above a threshold corresponding to the level that would exactly double the number of participants. However, the paper does not address the fact that the participation decision might change in response to changes in the cash transfer amount or other program features. For example, in assessing the effects of doubling the cash transfer, the authors keep the number and identities of the beneficiaries in the baseline case identical, which assumes that the program participation decision does not change in response to variations in the program characteristics. This is a strong assumption that might have important implications for their findings. The endogeneity of the participation decision is generally the main issue in the literature on treatment effects. While this paper does a careful job incorporating the endogeneity of labor supply choices, it neglects to fully incorporate the participation decision into the behavioral exercises.

Similarly, the simulations do not deal with the fact that as household income changes in response to changing labor supply choices or variations in

the cash transfer amount, some households cease to be eligible because their increased income surpasses the income eligibility criterion. In other words, the simulations do not incorporate the income eligibility criterion as an additional constraint. In tables 8 and 9, for example, the number of beneficiaries remains fixed at 507,652 when the cash transfer is doubled, which implies that not a single individual lost his or her eligibility as a result of the increase in income associated with the new cash transfer and the corresponding response of labor supply changes to this new subsidy. This exercise would amount to including an additional constraint in the maximization problem faced by individuals, perhaps as follows: individual i from household j chooses labor supply (say, H) to maximize

$$U_{ij} = f\left(C_{ij}, L_{ij}\right),$$

such that $C_{ij} = \overline{Y}_{ij} + H_{ij}w_{ij}$ and

$$\overline{Y}_{ij} + H_{ij}w_{ij} \leq Y^*,$$

where C_{ij} is consumption, L_{ij} is leisure, H_{ij} is the number of hours worked, w_{ij} is the hourly wage, $\overline{Y}_{ij}$ is nonlabor income, and Y^* is the income eligibility ceiling for program participation. This exercise should be pretty straightforward and simply involves incorporating this additional constraint. Though one would not expect this addition to change their findings significantly, the authors should take these eligibility issues into consideration.

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