Using a Social Experiment to Validate a Dynamic Behavioral Model of Child Schooling and Fertility: Assessing the Impact of a School Subsidy Program in Mexico*

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I. Introduction:

This paper has two goals. The first is to assess the validity of a dynamic behavioral model of parental decision-making about fertility and children's schooling by exploiting household data from a controlled social experiment. The model we develop and estimate is an extension of the static quality-quantity fertility model of Willis(1973) and Becker and Lewis (1973) to a dynamic setting under uncertainty that combines features of dynamic models of fertility as in Wolpin (1984) and Hotz and Miller (1993) and models of intra-household allocation of resources to children as in Becker and Tomes (1976) and Behrman, Pollak and Taubman (1986).[1] The social experiment is designed to augment completed schooling levels of children in rural Mexico by providing subsidies to parents conditional on school attendance. The validity of the model is assessed according to how well structural estimates of the model, based on data from the randomized-out control group and from the treatment group prior to the intervention, predict the experimental impact of the program.

The second goal of this paper is to use the structural estimates of the behavioral model to perform an evaluation of policy interventions that are not part of the original experimental design, such as variations in the subsidy, and to assess the longer- term impact of the program on behaviors related to child schooling decisions that extend beyond the life of the program, such as completed family size and completed schooling of all children ever born.

It is well known that the structural estimation of dynamic behavioral models requires auxiliary assumptions about the functional forms of structural relationships, i.e., preferences, technology and other constraints, and the distributions of unobservable random elements. Assessing the validity of such models by relying on tests of model fit to sample elements of the data used in estimation provides useful, but usually not compelling, evidence on the validity of the model. Such models are often subjected to a form of “pre-test” estimation in that the final formulation of the model is based on the fit of prior formulations to specific summary statistics of the data. This practice reduces the value of within-sample fit tests as a method of model

[1]For recent surveys if these literatures see Behrman (1997) and Hotz, Klerman and Willis (1997).

validation.

To mitigate the effect of pre-test estimation, there have been a number of attempts to assess model validity through out-of-sample forecasts. However, such applications are sparse and have been limited by the nature of the data. For example, Keane and Wolpin (1997) used the estimates of their model of occupational choice, based on a cohort of young men from the NLSY79 between the ages of 16 and 26 (over the years 1978 to1988), to forecast occupational choices for the same and nearby cohorts between the ages of 27 and 44 over the years 1989-1995 (using march CPS data). Although informative, because the data are highly age-trended and the model builds in such trends, tests based on this kind of out-of-sample data may not be able to discriminate finely among alternative models.[2]

Another type of model validation test makes use of regime shifts. For example, Lumsdaine, Stock and Wise (1992) compared the ability of structurally and non-structurally estimated models to forecast the impact of a pension "window plan" on the departure rates of workers from a single firm. The workers were subject to a defined benefit plan that provided a significant incentive to remain with the firm until age 55 but to leave before 65. In 1982, there was a major change in the plan and vested workers over the age of 55 were offered a bonus to retire. Lumsdaine, Stock and Wise (1992) compare forecasts of the models' predictions about the impact of the bonus on retirements, based on pre-1982 data, to actual retirements. The forecast is of a large change in the pension rules, and thus provides an arguably more convincing test of the validity of the model than do within-sample tests.[3]

In this paper, we similarly use out-of-sample forecasts to assess the validity of a structurally estimated model, but the comparison is to a completely new program rather than a change to an existing program. We

[2] Keane and Wolpin found that the structurally estimated model forecasts white-collar employment better than a non-structurally estimated model, but that the situation is reversed in forecasting blue-collar employment. They also found that a model in which individuals behave myopically provided incredible out-of-sample forecasts of occupational choices and wages.

[3] Lumsdaine, Stock and Wise (1992) found that the structural dynamic programming model forecasts the impact of the window plan better than a non-structural probit specification.

study the Mexican school subsidy program PROGRESA. This program was implemented as a social experiment beginning in 1997.[4] We obtain structural estimates of a model of household fertility and child schooling decisions using data on the randomly selected control group and on the treatment group prior to the experiment, for whom there are longitudinal data over three survey years. We assess the performance of the model by comparing the impact of the program predicted by the model to the impact obtained under the experiment. By design, the control and treatment groups are randomly drawn from the same population so that the behavioral model relevant to the control group should be the same as the model relevant to the treatment group. This experiment therefore represents a unique opportunity to assess the validity of a structurally estimated model.

Previous studies in the program evaluation literature have also made use of social experiments to study the performance of methods for estimating program effects using observational data. For example, Lalonde (1986) compared estimates of the impact of a job training program based on a variety of nonexperimental estimators to an experimental benchmark. More recently, Heckman, Ichimura and Todd (1997) studied the performance of a class of matching estimators in a similar context. The methods studied in that literature typically require data on program participants and are therefore not suitable for evaluating the effect of programs that have not been implemented. In contrast, the method adopted in this paper only requires data on nonparticipants. We show that the existence of an active child labor market and variation in child wages, a component of the opportunity cost of school attendance, can be used to identify the parameters of our behavioral model, which enables us to forecast the effect of a school subsidy program without any variation in the data in the direct cost of schooling.

The model developed in this paper assumes that a married couple makes sequential decisions about the timing and spacing of births and about the time allocation of children through age 15, including their school attendance and labor market participation. Parents receive utility contemporaneously from the stock of children

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[4] PROGRESA stands for Programa de Educacion, Salud, y Alimentacion (Program of Education, Health and Nutrition). The name of the program was recently changed to Oportunidades, but its essential features remain the same.

and their current ages, their children's current schooling levels and attendance and from their leisure time (home production). Household consumption, which also yields contemporaneous utility, is enhanced by their children's earnings. The decision to bear a child (for a woman to become pregnant) is made over a finite horizon beginning at the woman's age at marriage and ending when the woman is no longer fecund (assumed to be age 43); decisions about the time allocation of children are made through age 59. Parent's income is an exogenous function of the husband's age and the distance of the village of residence to the nearest large city. Parental preferences and income and children's earnings are subject to time-varying stochastic shocks. Preferences, parental income and child wages differ permanently among households according to their type, which is unobservable to us.

Attanasio, Meghir and Santiago (2001) develop and structurally estimate a quite different model of schooling decisions that they also use to evaluate the impact of the PROGRESA program and of variations to the subsidy schedule. The key differences are: (i) their model assumes that schooling is chosen to maximize each individual child's lifetime income, rather than being the outcome of an intra-household allocation decision that maximizes parental utility, and their model does not incorporate a fertility decision; (ii) they use data from post-program treatment households in estimating their model; (iii) they allow the income generated by working children to have a different effect on schooling decisions than income generated by the school subsidy, which we cannot allow for because we do not use post-program data in the estimation; and (iv) they estimate treatment impacts allowing for the possibility that the control group may have anticipated plans to bring them into the program at a future date. The empirical evidence presented about the importance of anticipatory effects is mixed, with some schooling patterns better fit by a model that assumes no anticipation, but other patterns better fit by a model with anticipation.

Our strategy of using the treatment group to validate the model assumes that the control group did not expect to be brought into the program. We base this assumption mainly on accounts from PROGRESA administrative personnel that special care was taken in administering the survey so as not to inform the control group families about the existence of the program or about future plans to incorporate them. If there were anticipatory effects, however, we would expect them to be present in 1998 but not in 1997, because the baseline data were gathered at a time before the initiation of the program. Thus, we would expect our model, estimated under the assumption of no anticipation, to fit the 1997 schooling patterns better than the 1998 patterns. As described later in the paper, the model fits similarly well for both years, which leads us to conclude that there is no strong evidence of anticipatory effects.

A structurally estimated model that is a valid representation of behavior can be used to evaluate the impact of counterfactual policies.[5] In contrast, social experiments provide information only about the impact of the program as it was implemented. They cannot be used to evaluate variations in the program or longer run effects that extend beyond the life of the experiment, and they cannot be used to evaluate radically different programs. Using the model's estimates, we determine the impact of the program for alternative subsidy schedules, including halving the subsidy level, doubling the subsidy level and subsidizing attendance only in higher grades. In addition, we evaluate two radically different programs, one that provides a bonus for graduating from junior secondary school but no other payments and one that provides an income subsidy without the school attendance requirement.

The model is also used to forecast long run impacts of the program, which might differ substantially from the short-run impacts that are measured by the experiment. Even if the program was viewed by the treatment group as permanent, the impact of the program measured by the experiment is conditioned on the circumstances of the families at the time the program was initiated, e.g., the number of children they have and their grade completion levels. The longer-run impact of the program may be to affect those circumstances. For example, one long-run effect of a schooling subsidy may be to alter the number of children families have. Another long-run impact may be to decrease the extent of discontinuous school attendance, leading to larger

[5] Wolpin (1996) reviews a number of examples in the context of discrete choice dynamic programming models.

long-run effects if returning to school after a period of non-attendance is viewed as costly.[6] Evaluating the long-run effects of the program using an experimental approach would require that the experiment be continued long enough to observe these changes, which is costly and often politically infeasible. We use the model to estimate what the completed family size and completed schooling would have been for the treatment families had the program been in existence for their entire lifetimes.

In the next section, we provide relevant details of the PROGRESA program, followed in section III by a description of the data used in the estimation. Section IV presents the model and estimation method and section V the results of the estimation, including an assessment of the model's validity and of counterfactual experiments. The latter exercise is clearly dependent on the success of the model at forecasting the impact of the subsidy program. The model does indeed perform quite well.

II. The PROGRESA Program:

We begin with a description of the PROGRESA program and the evaluation research that has already been performed. PROGRESA is a large-scale anti-poverty and human resource program begun in Mexico in 1997 that now provides aid to about 10 million poor families.[7] The program was begun in rural areas and is currently being expanded into semi-urban and urban areas. The major goal of the program is to stimulate investments in children's human capital. The program attempts to align household incentives with program goals by providing transfer payments that are contingent on children's regular attendance at school.[8] Programs with features very similar to those of PROGRESA have been initiated in many other Latin American and Asian

[6] For an alternative non-structural approach to analyzing the long-run impact of the PROGRESA program, see Behrman, Sengupta and Todd (2000a).

[7] These households account for 40 percent of all rural households and 10 percent of all households in Mexico (See Gomez de Leon and Parker (2000)).

[8] Children are required to attend at least 85% of days as verified by principals and teachers.

countries.[9]

In recognition of the fact that older children are more likely to engage in family or outside work, the transfer amount provided under the PROGRESA program varies with the child's grade level. As seen in Table 1, it is greatest for children in junior secondary school (grades 7 through 9) and is also slightly higher for female children, who traditionally have lower secondary school enrollment levels.[10] In addition to the educational subsidies, the program also provides some monetary aid and nutritional supplements for infants and small children that are not contingent on schooling.[11] In total, the benefit levels that families receive are substantial relative to their income levels. The monthly average total cash transfer is US $55 (more than 75% is due to the educational subsidy), which represents about one-fourth of average family income (Gomez de Leon and Parker, 2000).

For purposes of evaluation, the second phase of the PROGRESA program was implemented as a randomized social experiment, in which 506 rural villages (in 7 states) were randomly assigned to either participate in the program or serve as controls.[12] Randomization, under ideal conditions, allows mean program impacts to be assessed in a simple way through comparisons of outcomes for treatments and controls. Behrman and Todd (2000b) provide evidence that is consistent with the randomization having been carefully implemented. They document that the treatment and control groups are highly comparable prior to the initiation

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[9] For example, such programs exist in Bangladesh, Pakistan, Chile, Colombia, Brazil, Guatemala, and Nicaragua.

[10] Prior to 1992, Mexico had compulsory schooling that required that children complete at least 6 years of schooling. In 1992, the law was changed to require the completion of 9 years of schooling. However, as our data show, the law is not strictly enforced. Although a large proportion of children complete 6 years of schooling, the vast majority complete less than 9.

[11] Some of this aid is contingent on visiting a health clinic.

[12] The 506 localities were selected in a stratified random sampling procedure from localities identified by PROGRESA to be eligible to participate in the program, because of a "high degree of marginality" (determined mainly on the basis of analysis of data in the 1990 and 1995 population censuses (1990 Censo, 1995 Conteo)). There are 31 states in Mexico.

of the program. Over the three year time period covered by our data, the households living in the control villages did not receive program benefits.[13]

The data gathered as part of the PROGRESA experiment provide rich information at the individual and household levels, including information on school attendance and achievement, employment and wages of children and the income of the household. Data are available for all households located in 320 villages randomly assigned to the treatment group and for all households located in 186 villages assigned to the control group. The data that we analyze were gathered through two baseline surveys administered in October, 1997 and March, 1998 and through three follow-up surveys administered October, 1998, May, 1999, and November, 1999. Households residing in treatment localities began receiving subsidy checks in the fall of 1998. In addition to the household survey data sets, supplemental data gathered at the village level and at the school level are also available, most importantly for our purpose, the travel distance to the nearest secondary school and to the nearest city.

Within treatment localities, only households that satisfy program eligibility criteria receive the school subsidies, where eligibility is determined on the basis of a marginality index designed to identify the poorest families within each community.[14] Because program benefits are generous relative to families' incomes, most families deemed eligible for the program decide to participate in it, although not all families are induced by the transfers to send their children to school.[15] Data collection was exhaustive within each village and included children from ineligible families. There are 9,221 separate households in the control villages and 14,856 in the

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[13] However, the program has recently been expanded to include many of the control localities, so that it is possible that the behavior of the controls groups over the time period we observe them could have been influenced by their expectation of eventually receiving benefits. We present evidence about the existence of anticipatory effects below.

[14] Program eligibility is based in part on discriminant analysis applied to the October 1997 household survey data. The discriminant analysis uses information on household composition, household assets (such as whether the house had a dirt floor) and some other factors in determining program eligibility.

[15] A family could participate in the health component of the program, but not in the school subsidy component.

treatment villages.

Most of the existing research on the PROGRESA social experiment focuses on estimating the experimental impacts through mean comparisons of various outcome measures for treatment and control children. Gomez de Leon and Parker (2000) and Parker and Skoufias (2000) examine how children's time use, e.g., time spent working for pay, differs for children participating in the program. Shultz (2000) and Behrman, Sengupta, and Todd (2000a) analyze the effect of the program on school enrollment and attendance rates. Figure 1, adapted from Behrman, Sengupta and Todd, shows the impact of the subsidy on school enrollment rates by age and sex. Treatment impacts on enrollment rates are mainly confined to older ages, children between the ages of 12 and 15, and are of similar magnitudes for girls and boys.

III. Variable Definitions and Descriptive Statistics:

Variable Definitions

Our estimation sample consists of landless households in which there was a woman under the age of 50 reported to be the spouse of the household head.[16] This restriction reduced the sample to 1,531 households located in the control villages in 1997 and 2,162 households located in the treatment villages in 1997. Additional exclusions based on missing or otherwise inconsistent data reduced the sample to 1,362 households in the control villages (of which 1,355 are also observed in 1998) and 1,949 households in the treatment villages. As of 1997, there were 4,501children born to the control households and 6,219 to the treatment households, on average a little over 3 children per household. Of these, 2,096 children in the control village and 2,845 in the treatment villages are between the ages of 6 and 15 as of the October, 1997 survey. In contrast to the entire sample, landless households tend to be poorer and, therefore, have a higher proportion of eligible

[16] A landless household is defined as a household that reported producing no agricultural goods for market sale. This restriction was adopted both to make the sample more homogeneous and smaller (to reduce the computational burden) and to avoid having to model agricultural production, which would be necessary if family child labor is not a perfect substitute for hired labor. We also restricted the sample to nuclear households, which are the vast majority.

households. As of the 1997 survey, about 52 percent of the all households were eligible to participate in the program, while 66 percent of the landless households were eligible to participate.

In estimating the behavioral model, we use data on both program eligible and ineligible households. Because eligibility depends on the number of children in the family, which is a choice variable in our model, restricting the estimation sample to eligible families would create a choice-based sampling problem of the kind that often arises in program evaluation settings. We avoid the choice-based sampling problem by using data on both eligible and ineligible families in estimating the model.[17]

Unfortunately, the PROGRESA data provide information concerning school attendance and work essentially only at the survey dates. Therefore, allocating children to the school-work-at home categories that pertain to an entire school year requires additional assumptions. In defining school attendance, we use the data on school enrollment in the week prior to the survey and data on highest grade completed at the time of the survey. Specifically, we used the following rule in determining school attendance during each of the two school years, 1997-98 and 1998-99, covered by the surveys: (1) A child was considered as having attended school for the entire year if a child that was reported as enrolled in at least one of the two surveys during each school year and was reported as completing at least one grade level. (2) A child was considered as having not attended if the child was reported as not enrolled in both surveys during each school year and did not complete a grade level. (3) Essentially, all other problematic cases were hand-edited to provide a consistent sequence of attendance and grade completion. A child who was determined to have attended school, but did not complete a grade level, was assumed to have failed that school year. School attendance information was obtained for children between the ages of 6 and 15. Highest grade completed was obtained for all children born to the woman.

[17] Given the specification of the model, which is described in detail in the next section, solving the choice-based sampling problem would require knowledge of the distribution of unobserved family types among both eligible and ineligible families. Using ineligible households also has the advantage of increasing sample variability in parental income and initial conditions.

A child was defined as working during the school year if the child did not attend school using the criteria above and had been reported as working for salary (for 1997, in the October 1997 survey and for 1998, in the October 1998 survey). The weekly wage was provided in the surveys for those who were reported working in the week previous to the survey. A child was defined as being at home if the child was neither attending school nor working. Parents' weekly income was obtained from the October surveys and includes market earnings of both parents as well as their self-employment income.[18] Both the children's weekly wage and the parents' weekly income were multiplied by 52 to obtain an annual equivalent.[19]

Descriptive Statistics

Table 2 presents basic sample statistics. The mean age of the wives in the sample as of 1997 is 30.5, and that of their husbands 34.4. The mean age of marriage of the women is 18.1. On average, the families had 3 children as of 1997 and added another .2 children by 1998.The mean highest grade completed of children age 7 to 11 is 2.4 years of schooling, while children who were between the ages of 12 and 15 had completed 5.8 years, and those age 16 and over, 6.6 years. As also shown, there is almost no difference in the completed schooling of this latter group by sex. Parent's income over the two survey years was, on average, about 12,000 pesos (approximately 1,100 U.S. dollars). Approximately 8 percent of children between the ages of 12 and 15 were working over the two years. Among those that worked, their average income ranged from about 6,000 pesos for those 12 and 13 years of age to about 9,000 pesos for those who were 15 years of age.[20]

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[18] It is extremely rare, as reported in the survey, for children to have contributed to the self-employment income of the household.

[19] Weeks worked during the year were not reported in the data.

[20] Although mean earnings of children would appear to be large relative to parents' income, it should be recognized that the figures for children represent the means of accepted wages, that is, the mean offered wages for that relatively small fraction of children that work. Our estimates, as described below, of the mean of the offered wage distribution for children is about a third of the mean of the accepted wage distribution. Because parents' income is mostly composed of the income of working fathers and almost all fathers work, the mean accepted and mean offered wages will be very close.

Data were also collected about the households' villages. Two "distance" variables are of particular relevance: the distance from the village to a junior secondary school and the distance from the village to the nearest city. As seen in Table 2, about one-quarter of the villages have a junior secondary school located in the village, and among those villages that do not, the average distance to a village with a secondary school is approximately three kilometers. The villages are also generally quite distant from major cities. The average distance of the households from a city is 135 kilometers.[21]

Table 3 provides more detail concerning the time allocation of children. The first two columns contrast the reported school attendance rates (in percent) of children by sex from ages 6 through 15 based on the raw data, whether or not the child was enrolled as of the October 1997 interview date (in column one), and the revised rates based on the rules described above (in column 2). The third column shows the percentage of children working for pay and the last column the percentage at home (100 - (2) - (3)).

As is apparent from comparing the first two columns, the revised attendance rates are slightly higher than the raw attendance rates. Based on the revised rates, school attendance is almost universal from ages 7 to 11 for both boys and girls. Attendance at age 6 is lower, particularly for boys, although over 90 percent. Attendance rates fall to 89 percent for males and to 90 percent for females at age 12, an age by which many children have completed primary school (grade 6). After age 12, attendance rates continue to decline for both girls and boys, but more rapidly for girls. By age 15, attendance rates are only 48 percent for boys and 40 percent for girls. The percentage of children working for pay at age 12 is only 2.5 for boys and 1.1 for girls. By age 15, 28 percent of boys but only 16 percent of girls are working for pay.[22]

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[21] We thank T. Paul Schultz for making this data available to us.

[22] Child labor laws prohibit children under the age of 14 from working and also limit the kinds of employment and the length of the work day. Our model assumes that these restrictions are not binding, which is consistent with the fact that we observe children under the age of 14 who are working. A very small number of children were working before the age of 12. We assumed that, in fact, they were at home in order to avoid having to fit the model to those few observations.

Girls progress through the early grades somewhat faster than boys, but ultimately complete about the same amount of schooling. As of October 1997, girls who are 12 years of age have completed about .3 more years of schooling on average than have boys of the same age. At age 16, that difference has completely disappeared, with both sexes having completed, on average, 6.6 years of schooling. Girls are more likely to complete sixth grade, but are also more likely drop out of school after completing it. As seen in Table 4, among the children in our sample who are age 15 or 16 in 1997, 22 percent of the boys and 17 percent of the girls have less than 6 years of schooling, 32 percent of the boys and 39 percent of the girls have exactly 6 years of schooling and 46 percent of the boys and 44 percent of the girls have more than 6 years of schooling. Failure rates are slightly higher for boys than for girls, 15.2 percent for boys and 14.5 percent for girls over all grades, but considerably higher at the primary grades, 15.7 percent vs. 13.9 percent.[23]

Table 5 provides information about fertility patterns. In particular, it shows the duration distribution from the date of marriage to the birth of each of the first three children. Fertility occurs rapidly after marriage. A little more than 50 percent of the women had their first birth within a year of marriage.[24] First births occurred within two years for seventy percent of the women. As the second column shows, of the women who had at least two children, only 11 percent of the women had two births in two years, but 35 percent had their second birth within 3 years of marriage and two-thirds within 5 years. About 10 percent did not have their second birth until after 10 years of marriage. Over 20 percent of the women who have at least three children had their third

[23] National examinations are given at each primary grade level and adequate performance determines grade progression, although compliance is left to teachers. Certificates are awarded after the completion of primary school and junior secondary school.

[24] The duration to the first birth is calculated as the age of the woman in 1997 minus the age of the child in 1997 minus the age of the woman at marriage. Ten percent of first births were reported to have occurred at an age prior to the woman's age at marriage and 14 percent coincident with the woman's age at marriage. For the cases where the birth occurred at or before the age at marriage, the marriage was assumed to have occurred one year prior to the birth of the first child. An additional 26 percent of first births occurred at an age one year post-marriage. The sum of these is about equal to the 52 percent of first births occurring in the first year of marriage reported in the table.

birth after 10 years of marriage. Thus, most women have their births quickly after marriage, although some delay for a significant period.

Once children leave school, they rarely return. As seen in Table 6, only 13 percent of boys age 13 to 15 who worked in one year attended school in the next year. Similarly of those who were home in one year, only 15 percent attended school in the next year. Comparable figures for girls are 20 percent (although the sample size is only 5) and zero percent. The school-to-school transition over these ages exhibits substantial permanence for both boys and girls, with 86.2 percent of the boys and 76.9 percent of the girls who attended school in one year also attending in the next year. The home-to- home transition for girls and the work-to-work transition for boys also exhibit such permanence. Among girls in this age group 92.5 percent of those who were home in one year were also home in the next year, and among boys 62.5 percent of those who worked in one year were also working the next year.

IV. The Model:

An Illustrative Model and Identification of Subsidy Effects

Given that there is no direct cost of schooling through junior secondary school, and thus no variation from which to directly estimate the impact of a subsidy to attendance, it is useful to consider an illustrative model to demonstrate what information in the data would enable one to forecast the impact of the subsidy program. Consider, then, a household with one child making a single period (myopic) decision about whether to send the child to school or to work, the only two alternatives. Let utility of the household be separable in consumption (C) and school attendance (s), namely u = C + $(\alpha+\epsilon)$s, where s=1 if the child attends school and 0 otherwise and ϵ is a preference shock. Assume that the preference shock is normally distributed with mean zero and variance σ^2. The family's income is y + w(1-s), where y is the parent's income and w is the child's earnings if working. Under utility maximization, the family chooses to have the child attend school if and only if $\epsilon \geq w - \alpha$. The unknown parameters of the model are thus α and σ. In this simple model, the probability that family i's child attends school is $1-F((w_i-\alpha)/\sigma)$. Clearly, it is both necessary and sufficient to obtain estimates of α and σ that child wages vary among families and that we observe those wages.[25]

Now, suppose the government is contemplating a program to increase school attendance of children through the introduction of a subsidy to parents of amount b if they send their child to school. Under such a program, the probability that a child attends school will increase by $F((w-\alpha-b)/\sigma)-F((w-\alpha)/\sigma)$. As this expression indicates, knowledge of α and σ is sufficient to forecast the impact of the program.[26] Moreover, it is also sufficient to enable forecasts of the effect of varying the amount of the subsidy on school attendance. Variation in the opportunity cost of attending school, the child market wage, thus serves as a substitute for direct variation in the monetary (tuition) cost of schooling.

Model Description

In each discrete time period, a married couple makes fertility and child time allocation decisions. Specifically, a decision is made about whether or not to have the woman become pregnant, and have a child in the next period, and, for each child between the ages of 6 and 15, whether or not to send the child to school, have the child work in the labor market (after reaching age 12) or have the child remain at home.[27] At ages older than 15, children are assumed to become independent, making their own schooling and work decisions. A woman can become pregnant beginning with marriage (at age t = t_m) and ending at some exogenous age (at t = T-1) when she becomes infecund. The contribution of the husband and wife to household income is exogenous

[25] More precisely, in order to use the probability statement above to estimate the parameters, we need to observe child wage offers. If we observe only accepted wages, that is, the wages of children who work, then we need to able also to identify the parameters of the offered wage distribution together with α and σ. Standard arguments for selection models hold for the identification of the wage offer parameters, namely functional form and distributional assumptions. Identification of α and σ requires an exclusion restriction, a variable that affects the offered wage but not the family's preference for child schooling. Below, we discuss the specific identifying assumptions in the richer model that we estimate.

[26] See Wolpin (1999) for a similar analysis of the informational content of probabilistic subjective expectations.

[27] Although some information on contraceptive use is available, it is not detailed enough to allow modeling contraceptive decisions.

(there are no parental labor supply decisions) and stochastic, and the household cannot save or borrow.[28] The contributions to household income from working children (under the age of 16) are pooled with parental income in determining household consumption.

Children who neither attend school nor work for pay are assumed to contribute to household production and thus to parental utility. Therefore, the cost of sending a child to school consists of the opportunity loss in either home production or household income, each of which may differ by the child's age and sex. Parents are also assumed to derive utility in each period from the current average level of schooling that their children have completed, from the current number of children who have graduated from elementary school (grade 6) and from the current number who have graduated from junior secondary school (grade 9). Schooling is publicly provided and therefore parents bear no direct tuition costs. All of the villages have their own primary schools (grades 1 through 6), but not all villages have junior secondary schools (grades 7 through 9). We allow for a psychic cost of attending a junior secondary school that varies with the distance to the nearest village with a secondary school, for a potential utility loss from interrupted schooling, that is, sending children to school who are behind for their age and for an additional loss if a child attends grade 10, which often involves living away from home.[29]

More formally, let p(t) = 1 if a woman becomes pregnant at age t (and 0 otherwise) in which case a child is born at t+1, n(t+1)=1. Further let b(t+1) = 1 if the child that is born is male (and 0 otherwise) and g(t+1) = 1 if the child is female. Also, let **p(t)** be the vector of pregnancies up to age t, and **n(t+1)** be the corresponding vector of births that occur up to age t+1, **b(t+1)** the corresponding vector of male births and **g(t+1)** the vector of

[28] Labor force participation rates of married women in this sample are quite low, between 10-20% at most ages.

[29] It is more straightforward to treat these costs as utility losses rather than monetary costs. Given that monetary costs associated with school attendance are not observed in the data, consumption and psychic costs are indistinguishable.

female births.[30] The stock of children through t (the sum of pregnancies through t -1) is denoted by N(t) = N(t-1) + n(t), and analogously the stock of boys by B(t) and that of girls by G(t). A child born at the woman's age τ is zero years old at τ and, thus, t - τ years old at t. A child of birth order n is born at the woman's age τ_n.

Let s(t, τ) = 1 if a child of age t - τ, between the ages of 6, the minimum age of school eligibility, and 15, the last age at which parents are assumed to make a schooling decision, attends school at t and zero otherwise. The corresponding vector of school attendance decisions for school age children at t is **s(t)**, where an element is zero when there does not exist a child of a given age. Cumulative schooling at t for a child born at τ is given by S(t, τ) = S(t-1, τ) + c(t-1, τ) s(t-1, τ), where c(t-1, τ) =1 if a year of schooling is successfully completed and zero otherwise. The completion of a grade level conditional on attendance is probabilistic. The probability of completion is given by $\pi^c(t-1, \tau, S(t-1) | s(t-1) = 1, \mu_c)$, where μ_c is a permanent family-specific component of the success probability. The completion probability also may differ by the child's sex. The vector of cumulative schooling at t over all children is $\mathbf{S(t)}$ and the mean schooling level of those children at t, $\bar{S}(t)$. Sex-specific schooling variables are similarly defined and denoted with b or g subscripts.

Finally, let h(t, τ) = 1 if a child born at τ works at t, and zero otherwise, with **h(t)** the corresponding vector over all children at t. Children must be at least twelve years old to be eligible for work, i.e., h(τ+k, τ)=0 for k<12. A child who is neither in school nor at work is by definition at home, which we denote as l(t, τ) =1 - h(t, τ) - s(t, τ). Sex-specific variables, as before, carry b and g subscripts.

The utility function is given by

$$U(t) = U(C(t),\ p(t),\ \mathbf{n(t)},\ \mathbf{l_b(t)},\ \mathbf{l_g(t)},\ \mathbf{s_b}(t),\ \mathbf{s_g}(t),\ \mathbf{S(t)};\ z_s,\ \epsilon_p(t),\ \epsilon_{lb}(t),\ \epsilon_{lg}(t);\ \mu_N,\ \mu_S,\ \mu_{lg},\ \mu_{lb}), \quad (1)$$

where C(t) is household consumption, z_s is the distance to a secondary school, the ϵ's are stochastic shocks to being pregnant and to the value attached to having children of each sex at home and the μ's reflect permanent

[30] Bold type is used to indicate a vector.

differences across households in their preferences for children, for schooling and for the home time of children by sex. The parental utility function (1) is written generally enough to include the possibility, for example, that the value of household production is greater for older girls when there are also very young children in the household. The exact representation of the utility function, which is shown in Appendix A, was determined in part using model fit criteria.[31]

Family consumption at t is equal to total family income. Family income is the sum of parental income (y_p) and the earnings of children (y_o) who work in the market.[32] Thus, the family's budget constraint is given by

$$(2) \quad C(t) = y_p(t) + \sum_n y_o(t, \tau_n) h(t, \tau_n).$$

Income generating functions differ for parents and children. Parental income at t, which includes both earnings and self-employment income, depends on the age of the male parent ($a_p(t)$), on the distance of the household's village from a city (z_c), a random shock at t ($\epsilon_{y_p}(t)$) and a permanent parent-specific unobservable component (μ_{y_p}).[33] Similarly, the earnings of a child depends on the child's age and sex, on the distance of the household's village from a city, on a time-varying (but not child-varying) shock ($\epsilon_{y_o}(t)$) and on a permanent unobservable component (μ_{y_o}) that is the same for all children (within the same household). The distance from a city affects wage offers due to differences in the skill price reflecting the extent of the labor market to which the household has access. Specifically,

$$(3) \quad \begin{aligned} y_p(t) &= y_p(a_p(t), z_c, \epsilon_{y_p}(t); \mu_{y_p}) \\ y_o(t, \tau_n) &= y_o(t-\tau_n, I(b(\tau_n) = 1), z_c, \epsilon_{y_o}(t); \mu_{y_o}). \end{aligned}$$

[31] It is the absence of economic theory about the form of the utility function and our inability to directly elicit preferences that makes necessary pre-testing of the model.

[32] Child rearing costs are essentially indistinguishable from the psychic value of children of different ages, which is included in parental utility rather than in the budget constraint.

[33] Parental education does not directly affect income, but instead enters the parent income function through its relationship to the unobservable parental type.

The five time-varying ϵ-shocks are assumed to be jointly serially uncorrelated. Their joint contemporaneous distribution is denoted by $f(\boldsymbol{\epsilon}(\mathbf{t}))$.[34] The permanent components of parental preferences and income, of child earnings and grade completion are also assumed to be jointly distributed according to $g(\boldsymbol{\mu})$. In the application, we assume g to be discrete with a fixed number of support points, which we denote as indicating family "type." These permanent components are known to parents from the beginning of the marriage.

At any t, the couple is assumed to maximize the present discounted value of remaining lifetime utility. In any period, the family will face K(t) mutually exclusive alternatives, where K varies over time with the number of children eligible to attend school and work and the woman's age. Define $d_k(t) = 1$ if the kth alternative is chosen at t, and = 0 otherwise. (The ordering of the K(t) alternatives is irrelevant.) Further, define $\Omega(t)$ to be the state space at t, namely all of the relevant factors that affect current or future utility or that affect the distributions of future shocks, that is, $\mathbf{b(t)}$, $\mathbf{g(t)}$, $\mathbf{S_b(t)}$, $\mathbf{S_g(t)}$, $a_p(t)$, $\boldsymbol{\epsilon}(\mathbf{t})$, $\boldsymbol{\mu}$, t_m, z_s, z_c .

The maximized present discounted value of lifetime utility at t, the value function, is given by

$$(4) \quad V(\Omega(t),t) = \max_{k \in K(t)} E\{\sum_{\tau=t}^{\overline{T}} \delta^{\tau-t} U(t) \mid \Omega(t)\}$$

where $\overline{T}$ is the end of the couple's life (woman's age 59) and the expectation is taken over the distribution of parental preference and income shocks, the children's earnings shock and the implicit shocks to grade completion for choices that involve school attendance.[35] The solution to the optimization problem is a set of decision rules that relate the optimal choice at any t, from among the feasible set of alternatives, to the elements of the state space at t. Recasting the problem in a dynamic programming framework, the value function can be

[34] The implicit time-varying shock to grade completion is assumed to be independent of all other shocks in the model.

[35] The integration is also performed over whether a birth outcome is a boy or a girl. We assume the probability of each gender outcome to be .5.

written as the maximum over alternative-specific value functions, $V^k(\Omega(t), t)$, i.e., the expected discounted value of alternative $k \in K(t)$, that satisfies the Bellman equation, namely

$$
\begin{aligned}
V(\Omega(t),\ t) &= \max_{k \in K(t)} [V^k(\Omega(t),\ t)] \\
(5) \quad V^k(\Omega(t),\ t) &= U^k(t,\ \Omega(t)) + \delta E(V(\Omega(t+1), t+1 \mid d_k(t)=1), \Omega(t)) \quad \text{for } t < \overline{T}, \\
&= U^k(\overline{T},\ \Omega(\overline{T})) \qquad\qquad \text{for } t = \overline{T}.
\end{aligned}
$$

Model Solution:

The solution of the optimization problem is in general not analytic. In solving the model numerically, its solution consists of the values of $E\,V(\Omega(t+1), t+1 \mid d_k(t)=1, \Omega(t))$ for all k and elements of $\Omega(t)$. We refer to this function as $Emax$ for convenience. As seen in (5), treating these functions as known scalars for each value of the state space transforms the dynamic optimization problem into the more familiar static multinomial choice structure. The solution method proceeds by backwards recursion beginning with the last decision period.

There are two complications in solving the model numerically. First, at any fecund period in which there are children of school and work age the choice set is of order $2 \cdot 3^{N_1(t)}$, where the first term represents the choice of whether or not to have a child and the second reflects the number of joint school attendance - work choices (of which there are 3) and $N_1(t)$ is the number of children age 12 to 15. For example, if there are three children between the ages of 12 and 15, there are 54 possible choices. One way to reduce the size of the choice set in a way that is for the most part consistent with the data is to assume that for each sex, a child may attend school only if all younger children attend school and, independent of sex, a child may work for pay only if all older children work for pay.[36] In the case of three children within the 12 to 15 age range, if they are of the same

[36] Violations of the assumption in the 1997 survey occur in about 5% of the households in the case of schooling and in about 1% of the households for working.

sex the number of alternatives is now reduced to 20. We do not impose these restrictions on 6 and 7 year old children to accommodate the fact that school entry is sometimes delayed.

Second, the size of the state space makes a full solution of the problem computationally intractable. The Emax functions must be calculated for all state values at each t. As long as the ages of children affect lifetime utility, as it must because of the age restrictions on children's eligibility for schooling and work, the state space will include the entire sequence of births by sex and not simply the stock of children. With 30 fecund periods, there are 3^{30} such sequences. In addition at any t, the schooling level of each child affects expected lifetime utility at t. To solve the dimensionality problem, we adopt an approximation method in which the Emax functions are expressed as a parametric function of the state variables or composites of the state variables, using methods developed in Keane and Wolpin (1994, 1997, 1999). In particular, the Emax functions are calculated at a subset of the state points and their values are used to fit a global polynomial approximation in the state variables.[37] To further limit the size of the state space, we also assume that women can have no more than eight children.[38] As in Keane and Wolpin, the multivariate integrations necessary to calculate the expected value of the maximum of the alternative-specific value functions at those state points are performed by Monte Carlo integration over the ϵ-shocks.[39]

Model Estimation:

The solution to the agents' maximization problem serves as input into estimating the parameters of the model. The numerical solution method described above provides (regression approximations to) the Emax

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[37] Stinebrickner (2001) develops a local approximation to the Emax function.

[38] Only about 3 percent of women in our sample report having more that eight children. In the empirical implementation, we assume that children of birth order greater than eight were not born.

[37]We used 2500 state points for the estimation of the Emax approximations and 50 draws for the numerical integrations. The Emax approximations did not appear to be sensitive to increases in these parameters, up to 10,000 state points and 300 draws. There were approximately 150 variables used in the Emax approximation, which includes interactions among the state variables. The R-squares were above .99 in all time periods.

functions that appear on the right hand side of (5). The alternative-specific value functions, $V^k(t)$ for k=1,..,K(t), are known up to the parental random preference and income shocks and the earnings shock of the children. Thus, conditional on the deterministic part of the state space, the probability that an agent is observed to choose option k takes the form of an integral over the region of a subset of the random shocks such that k is the preferred option.

Specifically, in the decision model presented above the observed outcomes at each period include (i) the choice (from the feasible set) made by the couple of whether or not to initiate a pregnancy, which children to send to school, which to work in the market and which to remain at home, (ii) the wages received by the children who work in the market, (iii) the success or failure of those children who attend school to complete a grade level and (iv) parental income. Let the outcome vector at t be denoted by $O(t) = \{d^k(t), \mathbf{y_o(t)}, \mathbf{c(t)}, y_p(t)\}$. Suppose we observe these outcomes for a sample of N households beginning at marriage, t = t_{mn}, and ending at some t= $\overline{t}_n$. Then, the likelihood for this sample is

$$(6) \quad \prod_{n=1}^{N} \Pr(O(\overline{t}_n),\ ......\ ,\ O(t_{m+1,n}),\ O(t_{mn})) \mid \overline{\Omega}(t_{mn}), \boldsymbol{\mu}),$$

where $\overline{\Omega}(t_{mn})$ is the observable components of the initial state space at the time of marriage, that is, the state space net of the family's type (the $\boldsymbol{\mu}$ vector) and stochastic shocks at $t = t_{mn}$. The observable part of the state space at marriage consists only of the age of the woman at t_{mn}, the age of the man at t_{mn} and distance from a secondary school and from a city. Because type is unobserved, it must be integrated out. Thus, the sample likelihood is

$$(7) \quad \prod_{n=1}^{N} \sum_{j=1}^{J} \Pr(O(\overline{t}_n),\ ......\ ,\ O(t_{m+1,n}),\ O(t_{mn})) \mid \overline{\Omega}(t_{mn}), \text{type}=j) \Pr(\text{type}=j \mid \overline{\Omega}(t_{mn}))\ ,$$

We assume that the initial conditions, the ages of marriage of both parents and the distances, are exogenous conditional on type.

There are two additional considerations in computing the likelihood. Because we assume that the child wage shock is family-specific, having an observation on the wage for two children in the same family working in the same period who have different wages (conditional on the relevant observable determinants of child earnings, child age and sex as in (3)) will lead to a degenerate likelihood. We therefore assume that the children's wages are measured with error, which seems like a reasonable assumption in any event.[40] Thus, assuming a multiplicative measurement error, observed child earnings is given by $y_o^{obs}(t) = y_o(t)\exp(\eta(t))$.

Another difficulty arises because, for most of the families, we do not observe decisions from the start of marriage. In particular, although we have a complete fertility history for all women, we do not have a complete school attendance and work history for children who are above the school or work eligibility ages at the first survey. For example, consider a family with 3 children whose ages are 10, 13 and 16 as of the October 1997 survey date and whose marriage occurred in 1980 when the woman was age 19 (t_m). For this family, we observe fertility outcomes at every t between 1980 and 1997, the woman's age 19 through 36. However, we are missing the complete history of school decisions for all children above the age of 6, and the work decisions for all children above the age of 12, as of 1997. Although it is conceptually straightforward to accommodate this feature of the data into the likelihood function (7), it is computationally infeasible to perform the integrations over all of the feasible unobserved choice paths as would be required to calculate the likelihood.

To avoid having to deal with missing data on the schooling and work decisions of children, one could restrict the sample to marriages that occurred between 1989 and 1997 for whom there are complete data. But for the earliest marriages in this range, the oldest age a child could be in 1997 is 6, the first age at which a

[38]We follow this strategy as opposed to allowing for child-specific wage shocks to avoid having to integrate over all of the child shocks in calculating the Emax functions. The problem of degeneracy exists more generally, namely that with family level shocks some choices may not be generated by the model. Restricting the choice set as above reduces the likelihood of this event, but does not eliminate it necessarily. Estimation is feasible when such events occur because our procedure smooths over zero likelihood events (see below). After estimating the model, we verified that simulations of the model could generate all of the outcomes that were observed in the data, so none of these outcomes has zero probability of occurrence.

schooling decision is made. It is obviously not possible to identify all of the parameters of the model solely from those observations, because children do not start work for pay until age 12.

For all families, we observe the complete set of outcomes in the two survey years, 1997 and 1998. The difficulty in using that data is that the state variables at the time of the surveys, including for instance the birth history and the schooling levels of all children, are not exogenous.[41] The assumption of serial independence in the shocks, however, implies that the state variables at any time t are exogenous with respect to decisions at t conditional on type. Thus, the likelihood for the observations in 1997 and 1998 can be written, analogous to (7),

$$\text{(8)} \quad \prod_{n=1}^{N} \sum_{j=1}^{J} \Pr(O(t_n^{98}),\ O(t_n^{97}) \mid \bar{\Omega}(t_n^{97}), \text{type}=j) \Pr(\text{type}=j \mid \bar{\Omega}(t_n^{97})) \ ,$$

where t_n^{97} and t_n^{98} are the ages of the woman in 1997 and 1998. A problem with (8) is that we must specify how the type distribution is related to the state variables. In actuality, the form of this conditional distribution function is given by the structure of the behavioral model together with the relationship between type and the initial state variables, i.e., the second term in (7). There is clearly a trade-off in how one specifies this conditional type distribution. The more flexible the functional form the better the approximation to its true functional form and the closer the exogeneity requirement is met. However, the more flexible the form, the more parameters there are to estimate. Furthermore, these parameters are themselves functions of the structural parameters; the estimation method is thus not efficient.

To summarize, in estimating the model we use (7) for the families with complete decision histories as described above (Sample A) and we use (8) for the families with incomplete decision histories (Sample B), ignoring the information about pregnancy decisions made prior to 1997 . Now, given the assumption of joint serial independence of the vector of shocks (conditional on type), both (7) and (8) can be written as the product

[41] This is exactly the initial conditions problem in discrete choice models as discussed in Heckman (1981).

of within-period outcome probabilities conditional on the corresponding state space and type. Each of these conditional probabilities are of dimension equal to the number of contemporaneous shocks in $\epsilon(t)$.

To illustrate the calculation of the likelihood, it is sufficient to consider a specific outcome at some period. Suppose that the kth alternative that is chosen at period t is to send at least some children to work. The children who work are observed to have wages given by $y_{oj}(t)^{obs}$, where j signifies the jth working child and the superscript "obs" distinguishes the observed wage from the true wage, $y_{oj}(t)$. Then the likelihood contribution for such an observation is (for a given type)

$$\Pr(d^k(t)=1, \tilde{y}_o(t)^{obs} \mid \Omega(t), type) = \int_{\tilde{y}_o(t)} \Pr(d_t^k=1 \mid \tilde{y}_o(t), \Omega(t), type) \cdot \Pr(\tilde{y}_o^{obs}(t), \tilde{y}_o(t) \mid \Omega(t), type), \tag{9}$$

where "~" signifies the vector of child wages over j and the integration is of the same order as the number of children who work.[42] Notice that it is necessary to integrate over the vector of true wages in (9) because the choice probability depends on true wages , which we observe only with error. Probability statements for other alternative choices are calculated similarly. We calculate the right hand side of (9) by a smoothed frequency simulator.[43]

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[42] For ease of exposition, we have ignored parents' income in the formulation of the likelihood function as well as whether the children that were sent to school failed to progress to the next grade level. The modifications of (9) to account for these additional observable variables are straightforward and in estimation we take them into account in evaluating the likelihood.

[43] The kernel smoothed frequency simulator we adopt was proposed in McFadden (1989). For each of K draws of the error vector, $\epsilon_p(t), \epsilon_{lb}(t), \epsilon_{lg}(t), \epsilon_{y_p}(t), \eta(t)$, noting that $\epsilon_{y_o}(t)$ is chosen to satisfy the observed wage for each child, that is, inclusive of the measurement error. The kernel of the integral is $\exp[\frac{V^k(t)-\max(\mathbf{V^j(t)})}{\tau}]/\sum_i \exp[\frac{V^i(t)-\max(\mathbf{V^j(t)})}{\tau}]$ times the joint density of the observed and true wage, where the j superscript denotes the vector of value functions over all alternatives. The first term in the kernel is the smoothed simulator of the probability that $d_k(t) = 1$, with τ, the smoothing parameter, set equal to 10, which provided sufficient smoothing given the magnitudes of the value functions. See Keane and Wolpin (1997) and Eckstein and Wolpin (1999) for further applications.

The entire set of model parameters enters the likelihood through the choice probabilities that are computed from the solution of the dynamic programming problem. Subsets of parameters enter through other structural relationships as well, e.g., child wage offer functions, the parents' income function and the school failure probability function. The estimation procedure, i.e., the maximization of the likelihood function, iterates between the solution of the dynamic program and the calculation of the likelihood.

Approximate Decision Rules

In this section, we present estimates of an approximate decision rule for schooling to investigate relationships among the variables in the state space of our model and the decisions that they are assumed to affect. Approximate decision rules can be motivated in the following way.

Let $V^{\neg k}(t, \Omega^{\neg k}(t))$ denote the maximum of the alternative-specific value functions excluding that of alternative k. Then, the decision rule for the optimal choice takes the form

$$(10) \qquad \begin{aligned} d^k(t) &= 1 \quad \text{iff} \quad V^k(t,\ \Omega^k(t)) - V^{\neg k}(t,\ \Omega^{\neg k}(t)) = F^k(t,\ \Omega(t)) > 0 \\ &= 0 \qquad \text{otherwise} \ . \end{aligned}$$

Note that although the alternative-specific value functions may depend on specific subsets of the state space, the decision rule depends on the entire state space. Given the state variables that enter the model, to estimate approximate decision rules, then, requires choosing functions F^k.

Table 7 presents logit regressions for the approximate decision rule regarding school attendance, that is, the relationship between the state variables and the probability of a child attending school. In the first specification, the only initial conditions included are the woman's age of marriage and the distance of the household from a secondary school; all of the other state variables evolve with time, either exogenously, e.g., the woman's age at the decision period, the parent's income, or as a function of past choices, e.g., the sequence of prior births as represented by the age distribution of children at the decision period. In the second specification, initial conditions are augmented to include the parents' education and the state of residence, incorporated in our structural estimation as unobserved permanent components of preferences and constraints. Although these additional initial conditions are jointly statistically significant, the results do not differ greatly. In the regression, we treat each child in the family as a separate observation, although in the model the decision is jointly made about all of the children.

As seen in table 7, the probability of attending school declines with the child's age. It also declines with the mother's age (given age at marriage and the other state variables). Attendance also declines with the existence of an additional child at most ages of that child, with the largest reductions occurring with the existence of a 5 or 6-year old and children age 12 and over. The higher is the child's own attained schooling at the decision period, the more likely is the child to attend school for an additional year (suggesting either some form of increasing returns or heterogeneity). Also, the higher is the average school attainment of 7-11 year old children the greater the probability that a given child will attend school. Living further from a secondary school is associated with a lower propensity to attend school and parent's income with a higher propensity, although imprecisely estimated. These results demonstrate systematic differences across families in their child schooling decisions that appear to be related to summary statistics reflecting past behavioral choices and to initial conditions, the kinds of state variables included in the model.

V. Results

Parameter Estimates

The precise functional forms of the model's structure are provided in appendix A.[44] Parameter estimates, and their standard errors, are provided in appendix table A.1. Most of the parameters are not of direct

[44] Identification in the model is achieved through a combination of functional form (for example, the CRRA utility function, normality of error distributions) and exclusion restrictions. As discussed in the illustrative example, identification of the variance in the preference shock to leisure (school attendance in the example) can be obtained if at least one variable that affects the child wage offer function does not affect the preference for leisure. The distance of the village to a city serves here as this exclusion restriction. However the model would also be identified in the absence of this restriction because child age is parameterized differently in the wage function than elsewhere, which essentially also serves as an exclusion restriction.

interest, although a few may be worth highlighting. In particular, the CRRA parameter (λ_{00}) is .87, implying that utility is close to linear in consumption. Consumption is a substitute in utility with fertility (λ_{01}<0), a complement with the average school attainment of children (λ_{02}>0) and a substitute with the leisure of children age 12 to 15 (λ_{03}<0).

The model was fit with three household types, where types differ with respect to their underlying preferences (for fertility, child schooling and child leisure), school failure rates, parental income potential and child earnings potential. The three types have distinctly different behaviors. As seen in table 8, type 1 households, comprising 36 percent of the sample, and type 3's, comprising 8 percent of the sample, value schooling less than type 2's. However, type 1's and type 3's also differ; the percent of the youngest children, age 6 to 11, from type 3 households who attend school is considerably lower than those from type 1 households. Moreover, in terms of schooling overall, type 1 households seem to favor boys and type 3 households girls, with type 2 households exhibiting little sex-bias. Children age 12 to 15 from type 2 households are least likely to work. And, although school attendance rates of children age 12 to 15 are similar for type 1 and type 3 households, those from type 3 households are considerably more likely to work, and concomitantly, less likely to be at home. Child offered wages, on the other hand, differ very little among the types and are only about one-third as large as mean accepted wages, while parental income is 20 percent higher for type 2's than for type 1's or 3's. Type 2 household's, in addition to sending their children to school at a higher rate, are much less likely to have an additional pregnancy during the year than either of the other types, about 2/3's less likely.

Within-Sample Fit

We next present evidence on the within-sample fit of our model along various dimensions of the data. Table 9 compares the model’s prediction of the distribution of child activity allocations (school, work or home) at individual ages by sex to the actual distribution and reports the relevant chi-square statistic for the null that they are the same. At younger ages, when school attendance is nearly universal, the model predicts an attendance rate nearly identical to the actual rate. Between ages 11 and 12, when attendance drops as children finish primary school, the model captures this drop for both boys and girls. It predicts a 11.8% drop for boys compared to an actual drop of 9.2% and a 9.4% drop for girls compared to an actual drop of 7.3%. The model also fits the choices between working for pay and staying at home. For example, it captures the pattern in the data that teenage girls are twice as likely as teenage boys to be at home at age 15, while teenage boys are more likely to work for pay. As seen in the table, the null that predicted and actual rates are the same is never rejected at the 5 percent level.[45]

Table 10 compares the actual and predicted school attendance rates for children whose schooling attainment differs from their maximum potential, defined as the level they could have achieved had they enrolled at age 6 and attended school continuously without repeating grades. The predicted rates for the subgroups that are not behind in school are about 5% too low (the null is rejected at the 5 percent level), but the attendance rates for the other subgroups are within 1-2% of the actual rates. Table 11 compares the observed wages of children who are working to the wages for working children predicted under the model. The model's predicted (accepted) wages tend to be too high relative to the observed (accepted) wages. Averaged over the ages of 12 through 15, the mean accepted wage is approximately 10 percent too high for boys and 28 percent too high for girls.[46]

The Test of Model Validity: Comparison of Impacts Predicted Under the Model to Experimental Impacts

Given the parameter estimates, it is straightforward to predict the impact of the school subsidy program on school attendance. A subsidy paid to the family for each child that attends school augments family income and affects the family's school attendance and fertility decisions by changing the family budget constraint (2). Resolving the optimization problem for each family in the presence of the subsidy will lead to a different

[45] These tests do not correct for the fact that the predicted distributions are based on estimated parameters.

[46] Tables B.2 - B.6 provide further evidence on the model's fit. As seen in these tables, there are specific quantitative features where the model fit is deficient, but the model clearly captures the qualitative features of the data quite well.

pattern of school attendance and fertility decisions. Comparing the decisions of the treatment group predicted under the model to their actual decisions (at the same stage in the life cycle and for the same state variables) provides a direct out-of-sample test of the model's validity.

We predict the subsidy effects in two different ways. A one-year ahead prediction uses information on the state variables in a base year (1997 or 1998) to forecast the effects of the program during the subsequent year. An N-year ahead prediction makes use only of information on initial conditions, i.e. the age of the wife and husband at marriage, parental education levels, and the distances to schools and to the nearest city. Using the initial conditions and the estimated model parameters, we simulate from the beginning of marriage the couples' fertility and school/work/home choices over their lifetime. This long-term prediction is used to evaluate the consequences of long-term participation in the PROGRESA subsidy program on fertility and schooling, as described below.

One-year ahead (short-term) predictions

Table 12 compares the actual and predicted school attendance rates for different categories of children, defined by age, gender and completed schooling, in the control and treatment groups. The only group that received the subsidy is the treatment group in 1998. This group was not used in fitting the model and, therefore, a comparison of the predictions shown in the last two columns of the table with the actual attendance rates represent an out-of-sample test of the model's validity. The table also presents within-sample comparisons for the control group in 1997 and 1998 and for the treatment group in 1997. As seen in the first two rows of the table, predicted attendance rates usually come within 1-2 percent of the actual attendance rate for all the groups in the 6-11 age category. In 1998, the model predicts an attendance rate for the treatment group equal to 97.1% for both boys and girls, compared to actual attendance rates of 98.5 percent and 98.7 percent. For children age 12 to 15, the predicted attendance rates tend to be a few percentage points lower than the actual rates for the 1997 treatment and control groups and the 1998 control group. However, the predictions for the 1998 treatment group are very close to the experimental impacts. The predicted attendance rate is within 1 percentage point of the rate observed under the experiment (74.9 vs. 74.4 percent for girls and 77.1 vs. 76.3 percent for boys).[47]

To further assess the validity of the model, we restrict the sample of children age 12 -15 to those who are behind in school. As would be expected, attendance rates are lower for those who are already behind in school. In that case as well, the predicted attendance rate with the subsidy is quite close to the actual (72.3 vs. 71.4 percent for girls and 72.9 vs. 71.6 percent for boys). Further restricting this last sample to those who have completed the 6th grade leads to considerably lower attendance rates and poorer prediction. The model overpredicts attendance rates by 7.2 percentage points for girls and by 8.4 percentage points for boys, although the experimental treatment effect is not precisely estimated for these subsamples.

Table 13 compares the model's predicted impacts of the subsidy on attendance to the experimental impact estimates. Three different ways of computing the experimental impacts are shown in the row labeled "Experimental Treatment Effect." The cross-section effect is the average attendance rate for the treatment group minus the average rate for the control group in the post-subsidy year, 1998. The longitudinal impact estimate is the difference in the post-subsidy and pre-subsidy attendance rates for the treatment group. Finally, the difference-in-difference estimate subtracts from the cross-sectional impact estimate the pre-subsidy (1997) difference between the groups' attendance rates. "*" denotes whether the impact estimates are statistically significant under the experiment at a 10% level.

Predicted subsidy effects are shown for three different categories of children (the last three in the previous table) for the control group in 1997 and 1998 and for the treatment group in 1997. For example, the model predicts an impact of 10.1% for treatment group girls age 12-15 in 1997. That is, given the state space for

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[47] We also looked at more restricted age ranges For12-13 year old girls (145 children), the actual and predicted attendance rates were 86.9 vs. 85.7 percent and for boys (141 children), 89.4 vs. 87.6 percent. For 14-15 year old girls (78 children), the actual and predicted rates were 57.3 vs. 51.3 and for boys (121 children) 61.2 vs. 65.6 percent. Although the differences are somewhat larger than in the combined 12-15 year old group, they are still quite close, especially considering that the predictions could range up to100 percent attendance.

households in the treatment group as of (October) 1997, this figure represents the difference between the attendance rate of girls during the 1997/1998 school year that is predicted by the model if the subsidy had been in force and the actual attendance rate. This predicted subsidy effect falls within the range of the experimental estimates (7.9%-10.3%). For the control groups, the estimates are close but slightly below the range. Similarly, the estimates of the subsidy effects for girls who are behind in school are also close to the actual treatment effects. However, predicted subsidy effects are less accurate for boys. The experimental impact estimates for boys are smaller and are not usually statistically significantly different from zero, while the model's predicted subsidy impacts are of a similar magnitude for girls and boys. The experimental impacts, especially for the boys that are behind and have completed the 6th grade, are considerably too high.[48]

As a further evaluation of the model's performance, Table 14 presents evidence on the model's ability to forecast the full school/work/home choice distribution, by sex, for the 1998 treatment group for all children age 12-15, for children of those ages not behind in school, those behind in school and those behind who have completed 6th grade. The model predicts well the rates of staying home or working for pay (usually within 2% of the actual rates) and it captures the differences in the work/home pattern between boys and girls, although as before not as well for the last category.

N-year ahead (long-term) predictions

Social experiments usually last only a few years, because they are typically very expensive and because it is often politically infeasible to deny those in the control group access to the treatment for a long time period. The short-term nature of experiments limits the usefulness of experimental data in evaluating the long-term consequences of programs like PROGRESA. However, PROGRESA's long-term effects are arguably of greater interest than its short-term effects, because the policy change being considered is that of making it a permanent feature of Mexico's social welfare system. As described earlier, short- and long-term consequences of

[48]Recall that we focus on the subsample of landless households. Interestingly, in the full sample, which includes also landed households, the experimental impacts for boys are larger and tend to be of similar magnitude to those of girls. See Figure 1.

PROGRESA may be quite different. For example, it may be difficult over the short-term to bring children who have dropped out back into school; but if the program were available from the beginning, it may prevent dropping-out. We, therefore, evaluate the performance of the model in making long-term forecasts by using it to predict school and fertility outcomes at the survey dates using only information on initial conditions.

Table 15 shows the actual and predicted school attendance rates obtained from that simulation exercise for the control sample in 1997 and 1998 and for the treatment sample in 1997 (at the baseline). The predictions shown in the table represent a within-sample fit test of the model's ability to make accurate long-term forecasts of attendance rates in the absence of the subsidy. The N-year ahead predictions of attendance rates are clearly not as accurate as the one-year ahead predictions and tend to underpredict attendance rates. Nevertheless, the predictions are still reasonably good. For example, for the 12-15 year old age category, the predicted attendance rates are between 6 and 10 percentage points below the actual rates. In comparison, the difference between them based on one-step ahead forecasts ranged from 0 to 5 percentage points. The last four rows of the table show the model's predictions of pregnancy rates for women of different age ranges. They are usually within 2-3 percentage points of the actual rates.

Given that the model's longer range forecasts of fertility and school attendance rates are reasonably accurate, we now use the model to predict the long-term impact of exposure to the PROGRESA subsidy program. That is, we predict the effect of the subsidy on family choices for the control and treatment groups at each survey date assuming that the program had been available to them from the time of marriage.[49] Table 16 compares the short-run (one-year ahead) and long-run (N-year ahead) predictions of the program on school

[49] Our long-term forecasts assume that the families will be eligible over their entire lifetimes. In reality, a family could become ineligible, for example, by accumulating certain assets, such as a car. Given that our model does not incorporate asset accumulation, we do not take into account that eligibility may change with changes in assets. However, our model does allow families to change fertility decisions to become eligible for program subsidies.

attendance rates for girls and boys age 12-15.[50] As expected, long-run impacts are larger than the short-run impacts; however, they only exceed the short-run effects by 0.5-1.5 percentage points. Therefore, the estimates suggest that much of the effect of the program on attendance is observed over the short-run.

Tables 17 and 18 report estimated long-term impacts on completed schooling and fertility. These estimates are obtained by simulating fertility and schooling outcomes from the mother's age at marriage through age 59, when all the children in the family would be at least 16 years of age. The model predicts that without the subsidy, girls will complete 6.29 years and boys 6.42 years of schooling. Had the program been in existence from marriage, given our estimates, children's mean years of completed education at age 16 would have increased by 0.54 years for both girls and boys. The model also predicts an increase in girls completing 6th grade by 6.4 percentage points and in boys by 4.5 percentage points. Increases in 9th grade completion rates are predicted by the model to be 6 percentage points for girls and 5.3 percentage points for boys.[51]

Although the impacts of the program on attendance rates and schooling attainment are substantial, table 18 provides little evidence for an effect of the program on fertility. Without the subsidy, the predicted long-run average number of children is 4.24 in comparison to 4.28 with the subsidy. There is also little change in the distribution of numbers of children across families.

Counterfactual Subsidy Experiments

Designing an optimal subsidy scheme to achieve some desired increase in schooling requires knowledge of the effects of many alternative subsidy schedules. As discussed earlier, a limitation of experimental data is that they are only informative about the effects of the particular experiment that was implemented and do not

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[50] N is the number of years between the time of marriage and 1997 and ranges from 8 to 38 years in the sample used for the simulations.

[51] The predictions of mean schooling and the percentage of children completing 6th grade or more are also quite accurate. In the sample of eligible households, girls age 16 to 20 in 1997 had completed 6.30 and boys 6.41 years of schooling (with completed schooling as in the model and table truncated at 10 years) and the percent completing at least 6th grade is 75.0 for girls and 73.4 for boys. However, the percentage completing 9th grade is significantly understated, actually being 30.6 percent for girls and 30.7 percent for girls.

provide a reliable way of extrapolating to learn about effects of counterfactual policies. Although a small change in the subsidy schedule might be well approximated by a simple extrapolation of the experimental treatment effect, any extrapolation to a more radical change in the subsidy schedule would be *ad hoc*. For example, one might be interested in evaluating an unconditional income grant to families that removes the school attendance requirement, or a bonus system that rewards the completion of certain grade levels.

Table 19 reports the results of a number of counterfactual experiments based on simulations to the mother's age 59 as in the previous two tables. The first column reports predictions of completed schooling and fertility at the baseline. To establish an upper bound for the effect of alternative subsidy schemes on school completion levels, the second column reports the effect of a perfectly enforced school attendance requirement for all children between the ages of 6 and 15. Although maximum completed schooling by age 16 is 10 years, because failure rates are significant, mean completed schooling with compulsory attendance is only 8.29 years for girls and 8.37 for boys, an increase over the baseline of about 2 years for both.[52] Fertility declines very slightly with compulsory school attendance.[53]

The next column again shows the effect of the original subsidy scheme as in the previous two tables, and the following two columns report experiments in which the subsidy amounts are first doubled and then halved. As seen in the table, completed fertility is essentially invariant to subsidy levels, increasing slightly as subsidy levels rise.[54] Mean completed schooling increases at a linear rate with increments in subsidy amounts

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[52] Failure rates differ significantly among the types. Type 2's complete almost 9 years of schooling under the compulsory school attendance requirement, while type 3's complete less than 6.5 years.

[53] This experiment is the flip side of the natural experiment that Rosenzweig and Wolpin (1980) exploit in which the impact on mean child schooling of an additional exogenous birth associated with a twin birth is used to test the quality-quantity fertility model. The above result, that fertility declines with an exogenous increase in mean schooling, arises because the increase in the per-child cost of schooling with distance from a junior secondary school is large enough to compensate for the estimated complementarity of mean schooling and fertility.

[54] In a static quality-quantity fertility model, Willis (1973) shows that a decrease in the per-child price of quality, e.g., a subsidy to school attendance, will increase fertility as long as quality and quantity are complements in utility. We find that average schooling and numbers of children are indeed complements

up to the original amount and then at a diminishing rate. For example, for girls the increases in mean schooling between the baseline and one-half of the original subsidy and between one-half and the full original subsidy amount are both .27, while the increase from doubling the subsidy amount is .47 years. However, whether there are diminishing returns to the program depends, in addition, on how the total cost of the program increases with the subsidy levels. The next to the last row of the table, which calculates the average cost of the subsidy program on a per family basis, shows that doubling the subsidy amounts more than doubles the per family cost. Based on these figures, the change in average schooling induced by a unit change in total costs is 41% higher at the one-half subsidy level than at the full subsidy level.

The next column restricts the subsidy to attendance in the 6th grade or higher, that is, the subsidy is zero for attending grades 3 through 5. Because the great majority of children complete at least the 5th grade, the subsidy to the earlier grades acts mainly as a direct income transfer program and might have, therefore, only a small effect on schooling although a large effect on the cost of the program. As seen, restricting the subsidy to higher grades reduces the per-family cost of the program considerably, from around 26,000 pesos to less than 16,000 pesos. Perhaps surprisingly, however, the fall in completed schooling is also not insignificant, with approximately 30 percent of the gain in mean schooling for girls and 33 percent for boys being lost.[55]

The reason that restricting the subsidy to attendance in higher grades only has a non-negligible impact on completed schooling levels is due to the interdependence of parental decisions among children within the family. If there are multiple children of school age in the household, providing a subsidy to attendance for children at lower grades, because it increases family income, increases the incentive for older children to attend

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in the per-period utility function (see appendix table A.1). We would expect the basic intuition of that model to also hold in the more complex model that we estimate.

[55] Moreover, relative to the original subsidy there is a substantially smaller gain in the fraction of children graduating from elementary school (6th grade), although offset by a slight gain in the fraction of children graduating from junior secondary school (9th grade). With the restricted subsidy, 77.6 percent of girls and 80.0 percent of boys graduate from elementary school, an increase of 1.6 percentage points for girls and 0.8 for boys as compared to increases of 6.5 and 4.5 percentage points with the original subsidy.

higher grades rather than to produce additional income through work. That this intra-household allocation mechanism is important can be seen from the fact that the reduction in the gain from the subsidy under the restriction falls with the number of children ever born. In one child families, mean completed schooling is 8.66 under both the original subsidy and under the restricted subsidy, a zero percent reduction in the gain, in two child families 8.54 and 8.51, a 12.5 percent reduction, in three child families 8.30 and 8.24, a 16.7 percent reduction, and in four child families 7.51 and 7.36, a 29 percent reduction. The reduction in the gain is about 35 percent in families with five or more children.

In light of this finding, the next column reports an experiment in which the subsidy is again restricted to attendance in grades 6 through 9, but the subsidy schedule is set at a level (1.43 times the subsidy amounts at each grade) at which the cost per family is the same as the original subsidy without the restriction (column 2). Relative to the original subsidy, the gain in mean completed schooling is predicted to be .14 years for girls and .11 years for boys, an increase of about 25 percent over the original gain. There is also a difference, as compared to the original subsidy, in the distribution of completed schooling, with a very slight increase in the proportion of children completing less than 6 years of schooling and a significant increase in the proportion completing 9 or more years. Ignoring distributional impacts, namely that families whose children do not attend the higher grade levels receive no income transfer under the restricted subsidy program, the restricted subsidy program would appear to be more efficient in producing higher completed schooling levels in this population.

An alternative subsidy scheme rewards completion rather than attendance. In the next column, we assess the impact of ninth grade graduation bonus in the form of a payment of 30,000 pesos to families when a child graduates from junior secondary school.[56] Clearly, the effect of such a bonus scheme requires, as the model assumes, that families be forward-looking.[57] The simulations show that the bonus increases the percentage of

[56] Keane and Wolpin (2000) assess such bonus-type schemes in the U.S. context.

[57] The effect of a bonus program would also be sensitive to assumptions about the ability of families to smooth consumption intertemporally. Increased opportunities to smooth consumption through saving/borrowing would presumably increase the value of large lump-sum payments. Recall that, in the

children completing junior secondary school significantly, by about ten percentage points for both girls and boys, but has a relatively small impact on average schooling. In fact, the increase in average schooling is not as large as the effect of the original subsidy even though the cost of the bonus program is about 50 percent higher. Interestingly, the proportion of children who complete at least 6th grade actually falls below the non-subsidy level, suggesting that families are substituting more schooling for some children and less for others. Thus, the effect of the bonus is largely to induce children who were already attending junior secondary school to complete ninth grade.

The additional interventions we consider all have rather small effects on schooling. In particular, enforcing a child labor law prohibiting children under the age of 16 from working and building a junior secondary school in each village where it is absent each would raise mean schooling by .1 years or less. We also simulate the impact of a pure income transfer program, one that pays 5000 pesos per year to families without any school attendance requirement. This amount is close to the maximum benefit that families may currently receive under the program in any year, and represents about a 50 percent increase in annual family income. As seen, mean schooling increases due to the subsidy as schooling is a normal good.[58] However, the increase in schooling is only about 20 percent as large as the original attendance-based subsidy. Moreover, its cost per family is an order of magnitude larger.

VI. Conclusions

In this paper, we have used a social experiment that subsidized school attendance of children in rural Mexico to evaluate the validity of a forward-looking model of family decision-making about fertility and child

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model, families cannot smooth consumption between periods at all. However, our estimates imply that utility is close to being linear in consumption so that the capability to smooth consumption is not of great relevance.

[58] As with all of the other subsidy schemes, fertility, though falling with the income subsidy, is essentially unchanged.

schooling. The model was estimated on control families who did not have access to the program and on treatment families prior to the existence of the program. The problem in identifying the impact of subsidies is that there are no direct costs of attending school that could serve as information from which the effects of the subsidy program could be extrapolated. However, given the existence of an active child labor market, we showed that variation in child wages, the opportunity cost of attending school, coupled with the assumptions necessary to implement the structural estimation of our behavioral model, could serve to identify the model's parameters and, from those estimates, simulate the impact of the program.

We found that the model provided an accurate forecast of the effect of the program, as given by the experiment, on school attendance rates of children, although the forecast was better for girls than for boys. Given this evidence on the model's performance, we simulated a number of counterfactual policy experiments that illustrate a menu of options that could be placed before policy-makers. We provided information on the long run benefit and cost trade-offs associated with doubling the subsidy at all grade levels, of halving it, of restricting it to only higher grade levels, of providing a bonus for graduation from junior secondary school and of a pure income transfer program.

An important caveat to our evaluation of the long-run impact of counterfactual experiments is that our analysis is partial equilibrium. As school attendance rates rise due to the program, and children withdraw from the child labor market, one would expect child wage rates to rise and the increase in school attendance rates due to the subsidy to be somewhat mitigated. Although our forecast of the subsidy effect does not incorporate such a labor market equilibrium response and is yet quite accurate, such adjustments in the child labor market may require time. To get some idea of the quantitative significance of the equilibrium effect, we also performed a counterfactual experiment which combines the original subsidy program with a concomitant increase in child wage rates of 25 percent.[59] The results of that experiment are shown in the last column of table 19. The degree of

[59] We assume that parental income would be unchanged, although there may be an effect on adult wages depending on the degree to which child and adult labor are substitutes.

mitigation of the increase in mean schooling differs by sex. For girls, the increase above the baseline accounting for the wage increase is 85 percent of the partial equilibrium effect of the original subsidy, while for boys it is only 69 percent. Of course this example is at best illustrative because we do not know how elastic is the demand for child labor. A complete analysis would require a general equilibrium model of the rural labor market, which we leave for future work..

One lesson that we draw from this validation exercise is that there is a potential synergy between the implementation of social experiments and the structural estimation of behavioral models. It arises because of, on the one hand, the inherent limitation of social experiments to exhaustively explore the impacts of competing policies and, on the other, the difficulty in providing credible evidence of the validity of counterfactual policy experiments that are based on structural estimates of behavioral models. Exploiting the strengths of both methodologies requires, as in PROGRESA, that data collected from social experiments go significantly beyond the more limited types of data necessary for the simple estimation of treatment effects. We view this paper as an example of one such effort to exploit this synergy.

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Table 1
Monthly transfers for school attendance
under the PROGRESA program

School Level	Grade	Monthly Payment in Pesos	
		Females	Males
Primary	3	70	70
	4	80	80
	5	105	105
	6	135	135
Secondary	1	210	200
	2	235	210
	3	255	225

(a) Source: Schultz (1999a, Table 1). Corresponds to first term of the 1998-99 school year.

Table 2
Means and standard deviations of selected variables

	Mean	Standard Deviations
Wife's age in 1997	30.5	8.1
Husband's age in 1997	34.4	9.5
Wife's age at marriage	18.1	3.4
Number of children ever born (1997)	3.01	1.92
Number of children ever born (1998)	3.06	1.84
Number of children ever born to women age 35-49 (1997)	4.05	2.14
Highest grade completed of children age 7-11	2.39	1.41
Highest grade completed of children aged 12-15	5.79	1.76
Highest grade completed of children age 16 or older		
All	6.60	2.81
Boys	6.64	2.82
Girls	6.56	2.81
Percentage with secondary school in Village	26.7	
Distance to secondary school if not in Village (km)	2.82	1.60
Distance to city (km)	136	74
Parent's income (pesos)	11,841	12,551
Percentage of children aged 12-15 who worked for pay	8.4	-
Market income of working children age:		
12	5605	3310
13	6707	2671
14	8037	7415
15	9073	9549

Table 3
Percent of children attending school, working and home
by age and sex [a]

Age	Attends school (Oct. 1997)		Attends school (revised)		Works		At home	
	boys	girls	boys	girls	boys	girls	boys	girls
6	91.0	94.9	92.9	95.7	-	-	7.1	4.3
7	97.8	97.4	98.9	97.8	-	-	1.1	2.2
8	97.5	97.3	98.6	99.2	-	-	1.4	0.8
9	99.6	98.4	99.6	99.2	-	-	0.4	0.8
10	97.2	97.9	97.6	98.7	-	-	2.4	1.2
11	97.7	95.9	98.6	96.9	-	-	1.4	3.1
12	89.2	89.3	88.7	90.0	2.5	1.1	8.8	8.9
13	78.1	67.5	78.1	70.9	8.6	4.0	13.4	25.2
14	66.9	58.8	67.3	60.4	16.1	10.1	16.7	29.5
15	48.7	38.5	47.7	40.2	27.5	15.6	24.8	44.3

a. Control and treatment groups in 1997

Table 4
Distribution of highest grade completed
at ages 15 and 16[a]

Years of schooling	Male Children	Female Children
0	2.9	2.3
1	1.0	0.8
2	2.3	1.6
3	3.6	1.2
4	4.5	3.1
5	7.8	8.2
6	32.0	38.5
7	10.7	5.8
8	12.3	12.1
9+	23.0	26.5

a. Control and treatment group in 1997

Table 5
Distribution of the durations from marriage
to first, second and third birth[a]

Duration from marriage (years)	To First Birth	To Second Birth	To Third Birth
1	52.4	-	-
2	18.5	10.6	-
3	9.7	24.5	1.7
4	4.6	18.6	8.9
5	3.4	11.9	16.0
6	2.5	8.3	15.4
7	1.5	6.4	12.4
8	1.4	4.2	10.1
9	1.3	3.3	8.0
10	1.1	2.2	6.6
11	0.8	1.8	4.5
12+	2.7	8.2	16.5

a. Control and treatment groups in 1997

Table 6
One period transition rates by sex: age(a) 13 to 15

	Boys		
	Home (a)	Work (a)	School (a)
Home (a-1)	44.4	40.7	14.8
Work (a-1)	25.0	62.5	12.5
School (a-1)	8.3	5.5	86.2

	Girls		
	Home (a)	Work (a)	School (a)
Home (a-1)	92.5	7.5	0.0
Work (a-1)	40.0	20.0	20.0
School (a-1)	21.5	1.5	76.9

Table 7
Approximate decision rule:
Logit regression of school attendance[a]

	Specification	
	(1)	(2)
Child's age	-0.620 (0.070)	-0.604 (0.073)
Child's schooling	0.399 (0.052)	0.361 (0.053)
Indicator for child is a boy	0.313 (0.110)	0.309 (0.112)
Wife's age	-0.039 (0.016)	-0.019 (0.016)
Woman's age at marriage	0.028 (0.023)	0.010 (0.024)
Birth order	0.575 (0.189)	0.574 (0.195)
Has a newborn	-0.176 (0.204)	-0.182 (0.203)
Has a 1 year-old	0.040 (0.195)	0.015 (0.201)
Has a 2 year-old	-0.121 (0.172)	-0.148 (0.174)
Has a 3 year-old	-0.128 (0.162)	-0.116 (0.159)
Has a 4 year-old	-0.187 (0.145)	-0.154 (0.148)
Has a 5 year-old	-0.412 (0.140)	-0.337 (0.142)
Has a 6 year-old	-0.355 (0.147)	-0.303 (0.151)

Has a 7 year-old	0.023 (0.148)	0.110 (0.154)
Has a 8 year-old	0.235 (0.141)	0.305 (0.140)
Has a 9 year-old	0.073 (0.136)	0.162 (0.134)
Has a 10 year-old	-0.112 (0.164)	-0.014 (0.165)
Has a 11 year-old	-0.056 (0.174)	0.025 (0.176)
Has a 12 year-old	-0.331 (0.142)	-0.272 (0.145)
Has a 13 year-old	-0.614 (0.157)	-0.571 (0.164)
Has a 14 year-old	-0.650 (0.174)	-0.616 (0.179)
Has a 15 year-old	-0.747 (0.194)	-0.759 (0.197)
No. of children 16 +	-0.622 (0.193)	-0.597 (0.197)
Avg schooling of children 7-11	0.131 (0.059)	0.113 (0.059)
Average schooling of children 12-15	-0.002 (0.042)	0.003 (0.042)
Average schooling of children 16 +	0.025 (0.022)	0.020 (0.023)
Distance to a secondary school	-1.28E-4 (2.78E-5)	-9.64E-5 (2.99E-5)
Parents income	1.53E-4 (2.30E-4)	5.69E-5 (2.29E-4)

Year = 1998	-0.230 (0.131)	-0.167 (0.134)
1997 dummy × control group dummy	-0.158 (0.132)	-0.076 (0.135)
Wife's schooling	-	0.049 (0.028)
Husband's schooling	-	0.097 (0.029)
State dummies	no	yes
Constant	30.6 (12.8)	24.3 (13.1)
Pseudo-R^2	0.308	0.323

a. Robust standard errors in parentheses

Table 8
Predicted Selected Characteristics by Unobserved Type

	Type 1		Type 2		Type 3	
	Girls	Boys	Girls	Boys	Girls	Boys
Percent of Children Age 6-11 in school	98.5	99.4	97.6	99.9	78.7	64.2
Percent of Children Age 12-15 in school	37.3	50.2	84.6	86.9	44.5	36.8
Percent of Children Age 12-15 at home	55.9	31.0	11.3	7.0	33.5	30.9
Percent of Children Age 12-15 at work	6.8	18.8	4.1	6.1	21.9	32.3
Mean wage of children 12-15	2675	3599	2599	3499	2738	3665
Mean parental income	9916		11927		10124	
Percent becoming pregnant	15.3		5.7		15.0	
Percent of Sample	36.0		55.9		8.1	

Table 9
Actual and Predicted Choice Distribution by Child Age and Sex

	Actual			Predicted			
Age	School	Work	Home	School	Work	Home	χ^2
Boys							
6	0.934	-	0.067	0.923	-	0.077	0.58
7	0.982	-	0.019	0.980	-	0.020	0.02
8	0.987	-	0.013	0.980	-	0.020	0.99
9	0.994	-	0.006	0.980	-	0.020	3.49
10	0.982	-	0.018	0.974	-	0.026	0.86
11	0.977	-	0.023	0.964	-	0.036	1.45
12	0.885	0.021	0.094	0.846	0.039	0.115	3.99
13	0.780	0.084	0.136	0.736	0.078	0.186	4.51
14	0.677	0.157	0.166	0.619	0.191	0.190	3.41
15	0.490	0.276	0.235	0.521	0.251	0.229	0.88
Girls							
6	0.965	-	0.035	0.942	-	0.058	3.84
7	0.976	-	0.024	0.968	-	0.032	0.77
8	0.987	-	0.013	0.976	-	0.024	1.96
9	0.991	-	0.009	0.976	-	0.024	3.26
10	0.979	-	0.021	0.970	-	0.030	0.93
11	0.969	-	0.031	0.948	-	0.052	2.97
12	0.896	0.007	0.097	0.854	0.020	0.126	4.61
13	0.723	0.028	0.245	0.676	0.025	0.299	2.85
14	0.582	0.089	0.329	0.566	0.092	0.342	0.22
15	0.419	0.123	0.458	0.402	0.157	0.442	1.68

$\chi^2(.05,1)=3.84$, $\chi^2(.05,2)=5.99$

Table 10
Actual and Predicted School Attendance Rates by Number of Years Lagging Behind in School: Age 13-15

Age	Boys Actual	Boys Predicted	Boys χ^2	Girls Actual	Girls Predicted	Girls χ^2
Not behind	88.3	82.1	8.50	83.8	78.2	6.02
Behind one year	79.8	76.4	1.56	75.4	74.5	0.09
Behind two years	65.8	62.5	0.91	52.9	51.0	0.20
Behind three years or more	49.1	51.7	0.62	44.7	42.7	0.39

$\chi^2(.05,1)=3.84$

Table 11
Actual and Predicted Annual Wage if working by Child Age and Sex[a]
(number of observations in parentheses)

	Boys		Girls	
Age	Actual	Predicted	Actual	Predicted
12	6233 (6)	9298	3720 (2)	7301
13	7064 (21)	7618	5460 (6)	6907
14	7643 (34)	10218	8726 (19)	9306
15	10189 (53)	10313	6386 (22)	9848

a. in real 1997 pesos

Table 12
Actual and Predicted School Attendance Rates by Child
Age, Sex and School Attainment: Control and Treatment Groups by Year[a]

	Girls				Boys			
	Control Group		Treatment Group		Control Group		Treatment Group	
	1997	1998	1997	1998	1997	1998	1997	1998
Age 6-11								
Actual	96.9	96.5	97.6	98.5[b]	96.6	96.7	97.6	98.7[b]
Predicted	96.1	96.2	96.4	97.1	96.4	96.4	96.3	97.1
No. obs.	449	431	632	600	471	460	671	678
Age 12-15								
Actual	65.3	66.5	62.9	74.4[b,c,d]	68.8	72.5	69.5	76.3[c]
Predicted	61.6	61.8	61.8	74.9	68.8	68.8	68.0	77.1
No. obs.	190	176	205	223	189	182	279	262
Age 12-15 Behind in School								
Actual	58.3	58.7	56.9	71.4[b,c,d]	64.0	67.4	64.2	71.6[c]
Predicted	54.2	55.5	55.6	72.3	63.9	65.3	62.7	72.9
No. obs.	127	121	144	161	139	135	204	190
Age 13-15, HGC≥6 Behind in School								
Actual	40.9	44.4	30.3	51.5[c,d]	59.0	57.1	52.6	58.3
Predicted	40.2	45.3	37.3	58.7	55.0	53.0	51.7	66.7
No. obs.	66	72	66	66	61	56	95	96

a. based on 200 simulation draws per family
b. cross-section treatment effect (T98-C98) p-value ≤ .10
c. longitudinal treatment effect (T98-T97) p-value ≤ .10
d. difference-in-difference treatment effect ((T98-T97) – (C98-C97)) p-value ≤ .10

Table 13
Actual verses Predicted Subsidy Effects on Percent Attending School

Subsample	Girls, Age 12-15 (1) Actual Attendance Rate	(2) Pred. with Subsidy	(2)-(1)	Girls, Age 12-15, Behind in school (1) Actual Attendance Rate	(2) Pred. with Subsidy	(2)-(1)	Girls, Age 13-15, HGC≥6, Behind in school (1) Actual Attendance Rate	(2) Pred. with Subsidy	(2)-(1)
97 Control	65.3	72.7	7.4	58.3	67.0	8.7	40.9	58.6	17.7
98 Control	66.5	72.9	6.4	58.7	66.9	8.2	44.4	60.6	16.2
97 Treatment	62.9	73.0	10.1	56.9	67.6	10.7	30.3	56.2	25.9
Experimental Treatment Effect: X-Section, Longitudinal, Difference-in-Difference	7.9*, 11.5*, 10.3*			12.7*, 14.1*, 14.5*			7.1, 21.2*, 17.7*		

	Boys, Age 12-15 (1)	(2)	(2)-(1)	Boys, Age 12-15, Behind in school (1)	(2)	(2)-(1)	Boys, Age 13-15, HGC≥6, Behind in school (1)	(2)	(2)-(1)
97 Control	68.8	79.6	10.8	64.0	75.8	11.8	59.0	72.7	13.7
98 Control	72.5	80.2	7.7	67.4	78.0	10.6	57.1	72.8	15.7
97 Treatment	69.5	79.4	9.9	64.2	75.8	11.6	52.6	71.6	19.0
Experimental Treatment Effect: X-Section, Longitudinal, Difference-in-Difference	3.8, 6.8*, 3.1			4.2, 7.4*, 4.0			0.8, 5.7, 2.7		

* p-value of treatment effects ≤ .10

Table 14
Actual and Predicted Choice Distribution by Child
Age, Sex and School Attainment: Post-Subsidy Treatment

	Girls						Boys					
	In School		Home		Work		In School		Home		Work	
	Act[a]	Pred[b]	Act.	Pred.	Act.	Pred.	Act.	Pred.	Act.	Pred.	Act.	Pred.
Age 12-15[c,d]												
All	74.4	74.9	21.2	22.3	4.1	2.8	76.3	77.1	14.9	15.0	8.8	7.9
Not Behind	82.3	82.1	14.5	14.8	3.2	3.1	88.9	88.2	9.7	9.7	1.4	2.1
Behind	71.9	72.3	23.7	25.0	4.4	2.7	71.6	72.9	16.8	16.9	11.6	10.2
Behind and HGC ≥ 6	52.3	58.7	41.5	37.7	6.2	3.6	58.3	66.7	25.0	20.9	16.7	12.4

a. Based on observations in which neither the school nor work choice is missing.
b. Based in all observations including those missing school or work.
c. Numbers of observations for each of the four rows are 222, 62, 160 and 65 for girls, and 262, 72, 190 and 96 for boys.
d. Based on 200 simulation draws per family.

Table 15
Comparison of Actual and Predicted Attendance and Fertility
Based on N-Year Predictions Using Initial Conditions

	Controls, 1997		Controls, 1998		Treatments, 1997	
	Actual	Predicted	Actual	Predicted	Actual	Predicted
Percent Attending School						
Age 6-11						
Girls	96.9	95.3	96.5	95.4	97.6	95.3
Boys	96.6	93.3	96.7	93.5	97.6	93.2
Age 12-15						
Girls	65.3	58.2	66.5	58.5	62.9	56.6
Boys	68.8	62.5	72.5	62.7	69.5	61.2
Age 12-15, Behind in School						
Girls	58.3	52.4	58.7	52.6	56.9	51.1
Boys	64.0	56.4	67.4	56.8	64.2	55.2
Age 12-15, HGC≥6, Behind in School						
Girls	40.9	41.3	44.0	41.0	30.3	39.6
Boys	59.0	51.1	57.1	50.1	52.6	48.9
Percent Pregnant						
Age 20-24	17.9	21.2	17.0	19.6	17.3	20.8
Age 25-29	16.7	20.0	14.6	19.4	16.4	19.8
Age 30-34	13.1	10.8	9.3	10.8	12.8	11.0
Age 35+	5.2	7.8	6.7	8.1	6.3	7.7

Table 16
Short-run and Long-run Effects of the Subsidy on the Percent of 12-15 Year-olds Attending School

	Girls		Boys	
	Short-Run Effect[a]	Long-Run Effect[b]	Short-Run Effect	Long-Run Effect
Control Group				
1997	10.9	11.9	10.7	12.0
1998	11.2	12.3	11.4	12.7
Treatment Group				
1997	11.2	12.3	11.3	12.4
1998	11.7	12.7	12.1	12.4

a. predicted value with subsidy minus predicted value without subsidy, conditional on current state space
b. predicted value with subsidy minus predicted value without subsidy, based on initial conditions

Table 17
Predicted Effect of the Subsidy on Completed Schooling of Children by Age 16:
All Children Ever Born[a]

	Girls		Boys	
	No Subsidy	Subsidy	No Subsidy	Subsidy
Mean Schooling	6.29	6.83	6.42	6.96
Percent Completing Grade Six or More	75.8	82.2	78.8	83.3
Percent Completing Grade Nine or More	19.8	25.8	22.8	28.1

a. completed schooling truncated at grade 10

Table 18
Predicted Effect of Subsidy on Completed Fertility:
All Children Ever Born

	Without subsidy	With subsidy
Mean Number of Children Ever Born	4.24	4.28
Percent of Families with		
Zero Children	0.05	0.04
One Child	1.16	1.13
Two Children	9.23	8.74
Three Children	22.97	22.48
Four Children	24.43	24.60
Five Children	21.54	21.46
Six Children	14.78	15.30
Seven Children	5.05	5.36

Table 19
The Effectiveness and Cost of Alternative Programs

	Baseline[a]	Compulsory School Attendance through Age 15	Original Subsidy	2x Subsidy	.5x Subsidy	Restricted Subsidy[b]	1.43x Restricted Subsidy
Mean Completed Schooling							
Girls	6.29	8.37	6.83	7.30	6.56	6.67	6.97
Boys	6.42	8.29	6.96	7.44	6.68	6.79	7.07
Percent Completed Grade 6 or more							
Girls	75.8	95.1	82.3	86.9	79.3	77.4	82.0
Boys	78.8	93.7	83.3	86.7	81.1	79.6	82.8
Percent Completed Grade 9 or more							
Girls	19.2	55.5	25.8	31.6	23.1	26.2	29.3
Boys	22.8	54.7	28.0	34.6	25.5	29.2	31.8
Cost per Family	0	-	26,096	59,956	12,318	15,691	25,193
Mean Number of Children	4.24	4.21	4.28	4.32	4.27	4.25	4.27

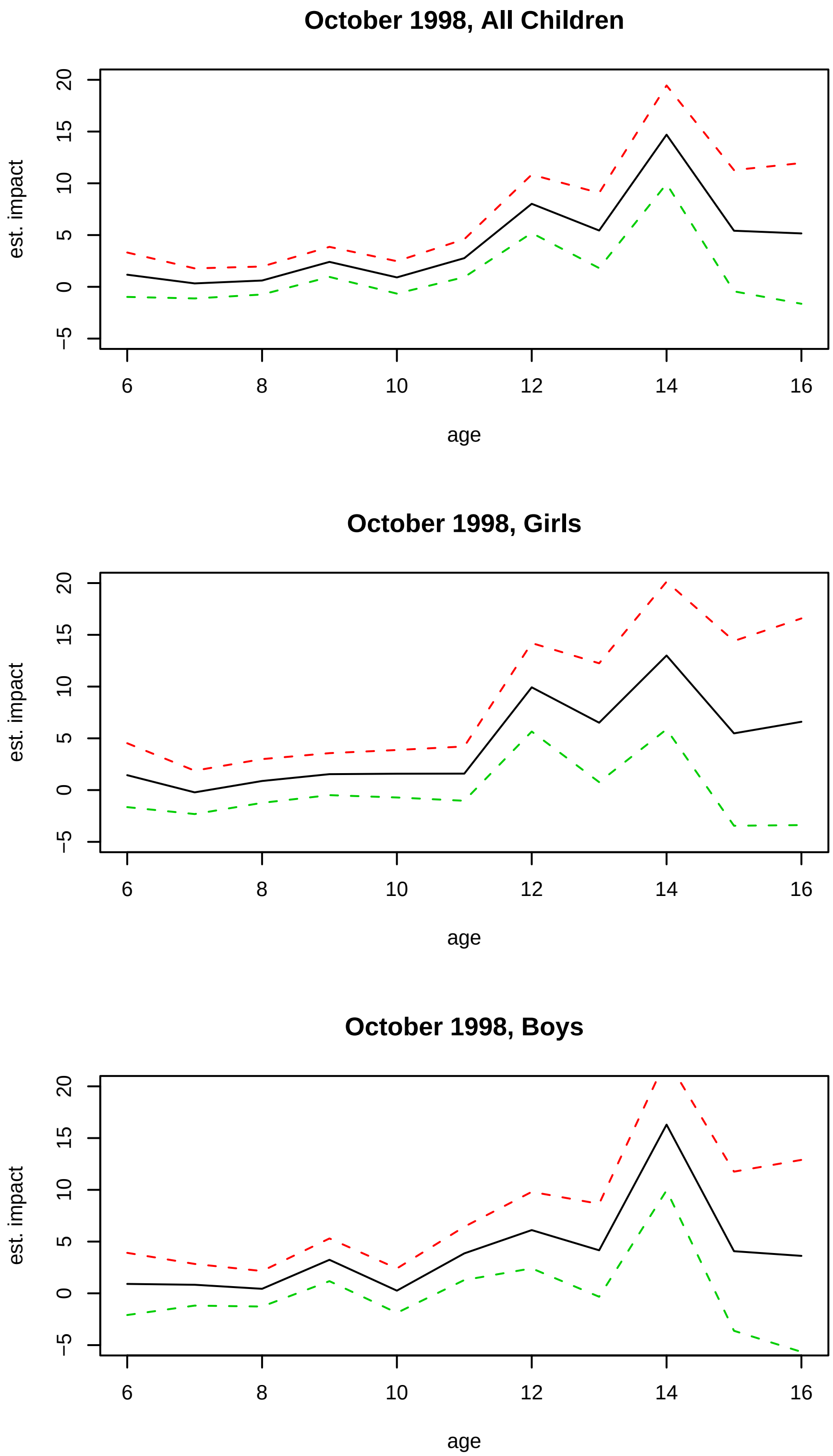
a. Predicted: control and treatment families.
b. Subsidy for attending school in grades 6-9 only.

Table 19 continued
The Effectiveness and Cost of Alternative Programs

	Bonus for Completing 9th Grade[c]	Junior Secondary School in Each Village	Unconditional Income Transfer 5,000 Pesos /Yr	No Child Labor through Age 15	Original Subsidy and 25% Wage Increase
Mean Completed Schooling					
Girls	6.50	6.39	6.41	6.30	6.75
Boys	6.58	6.55	6.53	6.52	6.79
Percent Completed Grade 6 or more					
Girls	74.9	76.0	77.6	76.1	81.5
Boys	76.9	79.0	80.0	79.9	81.8
Percent Completed Grade 9 or more					
Girls	28.8	21.2	20.8	19.7	25.3
Boys	32.7	24.1	23.6	23.5	26.5
Cost per Family	36,996	-	237,000	-	25,262
Mean Number of Children	4.20	4.25	4.23	4.25	4.29

c. The bonus is set at 30,000 pesos for girls and boys.

Figure 1: Average Treatment Impacts on Proportion Enrolled by Age



Appendix A

Utility function:

$$
\begin{aligned}
u_t = \frac{1}{\lambda_{00}} C(t)^{\lambda_{00}} [1 + \exp(\lambda_{01} N(t-1) + \lambda_{02}\bar{S}(t) + \lambda_{03} \sum_{j=12}^{15} l(t,t-j)n(t-j)] \\
+ \sum_{j=1}^{3} \lambda_{1,j} I(type=j)\, N(t) - \sum_{j=1}^{3} \lambda_{2,j} I(type=j)\, N(t)^2 + \sum_{j=1}^{3} \lambda_{3,j} I(type=j)\, \bar{S}(t) \\
+ \sum_{j=1}^{3} \lambda_{4,j} I(type=j)\, N(t)\bar{S}(t) + \lambda_5 \sum_{j=1}^{3} n(t-j) - \lambda_6 \sum_{j=1}^{3} n(t-j)^2 + \sum_{j=1}^{3} \lambda_{7,j} I(type=j) \sum_{n} I(S(t,\tau_n) \geq 6) \\
+ \sum_{j=1}^{3} \lambda_{8,j} I(type=j) \sum_{n} I(S(t,\tau_n) \geq 9) + \lambda_9 I(p(t_m) = 1) + \lambda_{10} I(\sum_{t=20}^{24} p(t)=1) + \lambda_{11} I(\sum_{t=25}^{29} p(t)=1) \\
+ \lambda_{12} I(\sum_{t=30}^{34} p(t)=1) + \lambda_{13} I(\sum_{t=35}^{39} p(t)=1) + \lambda_{14} I(\sum_{t=40}^{43} p(t)=1) + \lambda_{15} I(p(t-1)=1)\, I(p(t)=1) \\
+ \lambda_{16} \sum_{k=12}^{15} I(S_b(t,\tau_n) \geq 6, s_b(t,\tau_n)=1) \mid t-\tau_n=k)) + \lambda_{17} \sum_{k=12}^{15} I(S_g(t,\tau_n) \geq 6, s_g(t,\tau_n)=1) \mid t-\tau_n=k) \\
+ \lambda_{19,j} \sum_{j=1}^{3} I(type=j) \sum_{k=12}^{15} I(S(t,\tau_n) - (t-\tau_n-6) \geq 1, s(t,\tau_n)=1) \mid t-\tau_n=k)) \\
+ \lambda_{20} \sum_{k=12}^{15} I(S(t,\tau_n) - (t-\tau_n-6) \geq 2, s(t,\tau_n)=1) \mid t-\tau_n=k)) \\
+ \lambda_{21} \sum_{k=12}^{15} I(S(t,\tau_n) - (t-\tau_n-6) \geq 3, s(t,\tau_n)=1) \mid t-\tau_n=k)) \\
+ \lambda_{22} \sum_{k=12}^{15} I(S_b(t,\tau_n) - (t-\tau_n-6) = 0, s_b(t,\tau_n)=1) \mid t-\tau_n=k))
\end{aligned}
\tag{A.1}
$$

$$+\lambda_{23}\sum_{k=12}^{15} I(S_b(t,\tau_n)-(t-\tau_n-6)\geq 1, s_b(t,\tau_n)=1)|t-\tau_n=k))$$

$$+\lambda_{24}\sum_{k=12}^{15} I(S_b(t,\tau_n)-(t-\tau_n-6)\geq 2, s_b(t,\tau_n)=1)|t-\tau_n=k))$$

$$+\lambda_{25}\sum_{k=12}^{15} I(S_b(t,\tau_n)-(t-\tau_n-6)\geq 3, s_b(t,\tau_n)=1)|t-\tau_n=k))$$

$$+\lambda_{26}\sum_{k=12}^{15} I(S(t,\tau_n)-(t-\tau_n-6)\geq 1, l(t,\tau_n)=1)|t-\tau_n=k))$$

$$+\lambda_{27}\sum_{k=12}^{15} I(S(t,\tau_n)-(t-\tau_n-6)\geq 2, l(t,\tau_n)=1)|t-\tau_n=k))$$

$$+\lambda_{28}\sum_{k=12}^{15} I(S(t,\tau_n)-(t-\tau_n-6)\geq 3, l(t,\tau_n)=1)|t-\tau_n=k))$$

$$+\lambda_{29}\sum_{k=12}^{15} I(S_b(t,\tau_n)-(t-\tau_n-6)\geq 1, l_b(t,\tau_n)=1)|t-\tau_n=k))$$

$$+\lambda_{30}\sum_{k=12}^{15} I(S_b(t,\tau_n)-(t-\tau_n-6)\geq 2, l_b(t,\tau_n)=1)|t-\tau_n=k))$$

$$+\lambda_{31}\sum_{k=12}^{15} I(S_b(t,\tau_n)-(t-\tau_n-6)\geq 3, l_b(t,\tau_n)=1)|t-\tau_n=k))$$

$$+\lambda_{32} I(s(t,\tau_n)=1|t-\tau_n=12)\,n(t-12)+\sum_{j=1}^{3}\lambda_{33,j} I(\text{type}=j)\, I(l_b(t,\tau_n=1|t-\tau_n=6)$$

$$+\sum_{j=1}^{3}\lambda_{34,j} I(\text{type}=j)\, I(l_b(t,\tau_n=1|t-\tau_n=7)+\sum_{j=1}^{3}\lambda_{35,j} I(\text{type}=j)\, I(l_g(t,\tau_n=1|t-\tau_n=6)$$

$$+\sum_{j=1}^{3}\lambda_{36,j} I(\text{type}=j)\, I(l_g(t,\tau_n=1|t-\tau_n=7)+\sum_{j=1}^{3}\lambda_{37,j} I(\text{type}=j)\sum_{k=8}^{11} I(l_b(t,\tau_n=1|t-\tau_n=k)$$

$$+\sum_{j=1}^{3}\lambda_{38,j} I(\text{type}=j)\sum_{k=12}^{15} I(l_b(t,\tau_n=1|t-\tau_n=k)+\sum_{j=1}^{3}\lambda_{39,j} I(\text{type}=j)\sum_{k=8}^{11} I(l_g(t,\tau_n=1|t-\tau_n=k)$$

$$+\sum_{j=1}^{3}\lambda_{40,j} I(\text{type}=j)\sum_{k=12}^{15} I(l_g(t,\tau_n=1|t-\tau_n=k)+\lambda_{41}\sum_{j=0}^{5} n(t-j)\sum_{k=14}^{15} I(l_g(t,1\tau_n=1|t-\tau_n=k)$$

$$+\lambda_{42}\sum_{j=0}^{5} n(t-j)\sum_{k=12}^{15} I(l_g(t,\tau_n)=1|t-\tau_n=k)+\lambda_{43} z_s \sum_{n} I(S(t,\tau_n)\geq 6, s(t,\tau_n=1))$$

$$+\sum_{j=1}^{3}\lambda_{44,j}I(type=j)\sum_{n}I(S(t,\tau_n)=9, s(t,\tau_n)=1) + \lambda_{45}\sum_{n}I(S_b(t,\tau_n)=9, s_b(t,\tau_n)=1)$$

$$+\sum_{j=1}^{3}\lambda_{46,j}I(type=j)\epsilon_p(t)+\epsilon_{l_b}(t)[\sum_{k=12}^{15}l_b(t,\tau_n=1|t-\tau_n=k)$$

$$+\lambda_{47}\sum_{k=6}^{11}l_b(t,\tau_n=1|t-\tau_n=k)+\lambda_{48}\sum_{k=12}^{13}l_b(t,\tau_n=1|t-\tau_n=k)]$$

$$+\epsilon_{l_g}(t)[\sum_{k=12}^{15}l_g(t,\tau_n=1|t-\tau_n=k)+\lambda_{49}\sum_{k=6}^{11}l_g(t,\tau_n=1|t-\tau_n=k)+\lambda_{50}\sum_{k=12}^{13}l_g(t,\tau_n=1|t-\tau_n=k)]$$

Budget Constraint:

$$\text{(A.2)}\quad C(t) = y_p(t) + \sum_{n} y_o(t,\tau_n)h(t,\tau_n)$$

Parent Income Function:

$$\text{(A.3)}\quad y_p(t) = \sum_{j=1}^{3}\gamma_{0,j}^{p}I(type=j) + \gamma_1^p a_p(t) - \gamma_2^p a_p(t)^2 + \gamma_3^p z_c + \epsilon_{y_p}(t)$$

Child Wage Function:

$$\text{(A.4)}\quad y_0(t,\tau_n) = \sum_{j=1}^{3}\gamma_{0,j}^{o}I(type=j) + \gamma_1^o I(b(t,\tau_n)=1) + \gamma_2^o z_c + \gamma_3^o(t-\tau_n) + \gamma_4^o I(14\le t-\tau_n\le 15)$$

$$+\gamma_5^o b(\tau_n)I(14\le t-\tau_n\le 15) + \gamma_6^o I(t-\tau_n=15) + \gamma_7^o b(\tau_n)I(t-\tau_n=15) + \epsilon_{y_o}(t$$

School Failure Probability Function:

$$\pi^c(t,\tau_n | s(t,\tau_n = 1)) = \sum_{j=1}^{3} \pi_{0,j}\, I(\text{type}=1) + \pi_1\, S(t,\tau_n) + \pi_2\,(t-\tau_n) + \pi_3\, b(\tau_n)$$

(A.5)

$$+\pi_4\, I(8 \le t-\tau_n \le 15)\, I(S(t,\tau_n = 0) + \sum_{j=1}^{3})\, \pi_{5,j}\, I(\text{type}=j)\, I(S(t,\tau_n \ge 7)$$

$$+\pi_6\, b(\tau_n)\, I(S(t,\tau_n \ge 7)$$

Type Probability Function:

1. Couples married seven years or less at 1997 survey date.

(A.6)

$$\Pr(\text{type}=j) = \exp(X\beta_j)/1 + \sum_{k=1}^{2} \exp(X\beta_k),\ \text{ for } j=1,2, \text{ where}$$

$$X\beta_j = \beta^1_{0,j} + \beta^1_{1,j}\, t_m + \beta^1_{2,j}\, a_p(t_m) + \beta^1_{3,j}\, z_c + \beta^1_{4,j}\, z_s + \beta^1_{5,j} I(\max(S^m, S^f) \ge 9).$$

2. Couples married eight years of more at 1997 survey date.

(A.7)

$$\Pr(\text{type}=j) = \exp(X\beta_j)/1 + \sum_{k=1}^{2} \exp(X\beta_k),\ \text{ for } j=1,2, \text{ where}$$

$$X\beta_j = \beta^2_{0,j} + \beta^2_{1,j}\, t_m + \beta^2_{2,j}\, a_p(t_{1997}) + \beta^2_{3,j}\, z_c + \beta^2_{4,j}\, z_s + \beta^2_{5,j} I(\max(S^m, S^f) \ge 9)$$

$$+\beta^2_{6,j} \sum_n I(14 \le (t_{1997}-\tau_n) \le 15) + \beta^2_{7,j} \sum_n I(12 \le (t_{1997}-\tau_n) \le 15) + \beta^2_{8,j}\, N(t_{1997})$$

$$+\beta^2_{9,j}\, \bar{S}(t_{1997}) + \beta^2_{10,j} t_{1997} + \beta_{11,j} \sum_n I(S(t_{1997},\tau_n) \ge 7)$$

$$+\beta_{12,j}\, I(\sum_{j=0}^{5} n(t_{1997}-j) \ge 3 \vee \sum_{j=6}^{11} n(t_{1997}-j) \ge 3 \vee \sum_{j=12}^{15} n(t_{1997}-j) \ge 2)$$

Table A.1
Parameterizations and Parameter Estimates

I. Utility Function					
Variable	Estimate (s.e.)	Variable	Estimate (s.e.)	Variable	Estimate (s.e.)
CRRA parameter: λ_{00}	0.8715 (0.0196)	Number of boys age 12-15, with 6 years of school and currently attending school: λ_{16}	-78.78 (242.13)	Number of boys age 8-11 at home Type 1: $\lambda_{37,1}$ Type 2: $\lambda_{37,2}$ Type 3: $\lambda_{37,3}$	582.93 (324.71) 807.91 (3792.75) 1043.24 (221.31)
Number of children Type 1: $\lambda_{1,1}$ Type 2: $\lambda_{1,2}$ Type 3: $\lambda_{1,3}$	2997.07 (136.34) 433.64 (209.52) 3817.54 (664.51)	Number of girls age 12-15, with 6 years of school and currently attending school: λ_{17}	63.97 (272.88)	Number of boys age 12-15 at home Type 1: $\lambda_{38,1}$ Type 2: $\lambda_{38,2}$ Type 3: $\lambda_{38,3}$	1595.62 (364.55) 1064.72 (520.19) 1085.42 (329.54)
Number of children squared Type 1: $\lambda_{2,1}$ Type 2: $\lambda_{2,2}$ Type 3: $\lambda_{2,3}$	1022.96 (19.98) 1110.74 (32.61) 1034.39 (80.43)	Number of children age 12-15 not behind in school and currently attending school Type 1: $\lambda_{18,1}$ Type 2: $\lambda_{18,1}$ Type 3: $\lambda_{18,3}$	-385.03 (280.14) 84.06 (185.26) 162.17 (320.56)	Number of girls age 8-11 at home Type 1: $\lambda_{39,1}$ Type 2: $\lambda_{39,2}$ Type 3: $\lambda_{39,3}$	332.95 (244.70) 1179.35 (446.82) 502.79 (208.61)
Consumption × Number of children net of current birth : λ_{01}	-0.001404 (0.01818)	Number of Children age 12-15, behind 1 year in school and currently attending school Type 1: $\lambda_{19,1}$ Type 2: $\lambda_{19,2}$ Type 3: $\lambda_{19,3}$	-222.35 (247.83) -266.29 (204.31) 15.99 (241.16)	Number of girls age 12-15 at home Type 1: $\lambda_{40,1}$ Type 2: $\lambda_{40,2}$ Type 3: $\lambda_{40,3}$	1815.27 (356.52) 969.30 (467.83) 629.22 (351.33)
Consumption ×Average Schooling: λ_{02}	0.0016905 (0.00895)	Number of Children age 12-15, behind 2 years in school and currently attending school: λ_{20}	-32.08 (171.88)	Number of girls age 14-15 at home × Number of children age 0-5: λ_{41}	287.46 (571.33)
Number of children age 12-15 at home × Consumption: λ_{03}	-0.110051 (0.02295)	Number of Children age 12-15, behind 3+ years in school and currently attending school: λ_{21}	-10.80 (183.88)	Number of girls age 12-15 at home × Number of children age 0-5: λ_{42}	3.27 (422.86)
Average schooling of all children Type 1: $\lambda_{3,1}$ Type 2: $\lambda_{3,2}$ Type 3: $\lambda_{3,3}$	-12.16 (90.74) 276.24 (86.41) 25.10 (79.21)	Number of Boys age 12-15 currently attending school and behind x years in school 0 years: λ_{22} 1 year: λ_{23} 2 years: λ_{24} 3+ years: λ_{25}	-156.03 (196.92) 32.16 (194.54) -47.15 (199.09) -138.69 (228.69)	Number of children attending a secondary school x Distance from a secondary school: λ_{43}	.12979 (0.01433)
Stock of Children ×Average Schooling of Children Age 6-15 Type 1: $\lambda_{4,1}$ Type 2: $\lambda_{4,2}$ Type 3: $\lambda_{4,3}$	121.38 (23.09) 134.64 (31.08) 113.81 (23.31)	Number of children age 12-15 currently at home and behind x years in school 1 year: λ_{26} 2 years: λ_{27} 3 years: λ_{28}	475.31 (247.83) 83.28 (204.31) 284.15 (241.16)	Number children attending grade 10 Type 1: $\lambda_{44,1}$ Type 2: $\lambda_{44,2}$ Type 3: $\lambda_{44,3}$	1929.04 (1149.34) 1060.54 (244.20) 443.54 (934.11)
Number of children age 0-2: λ_5	473.87 (1387.75)	Number of Boys currently at home and behind x years in school 1 year: λ_{29} 2 years: λ_{30} 3 years: λ_{31}	-641.05 (397.14) -86.90 (444.18) -264.70 (341.30)	Number of boys attending grade 10: λ_{45}	673.82 (311.36)

Table A.1, continued
Parameterizations and Parameter Estimates

I. Utility Function continued.					
Variable	Estimate (s.e.)	Variable	Estimate (s.e.)	Variable	Estimate (s.e.)
Number of children age 0-2 squared : λ_6	-811.75 (544.91)	Age 12 and in school: λ_{32}	186.85 (199.21)	Pregnancy shock Type 1: $\lambda_{46,1}$ Type 2: $\lambda_{46,2}$ Type 3: $\lambda_{46,3}$	1.0 0.90674 (0.03192) 0.97329 (0.12767)
Number of Children with 6 or more years of schooling Type 1: $\lambda_{7,1}$ Type 2: $\lambda_{7,2}$ Type 3: $\lambda_{7,3}$	184.63 (39.38) 11.50 (35.49) -54.21 (22.98)	Boy age 6 at home Type 1: $\lambda_{33,1}$ Type 2: $\lambda_{33,2}$ Type 3: $\lambda_{33,3}$	1205.91 (362.39) 1421.85 (1681.07) 1579.79 (323.36)	Shock to preferences for boys age 12 –15 at home x number of boys at home age 6-11: λ_{47} 12-13: λ_{48}	0.31036 (228.51) -0.11917 (0.1373)
Number of Children with 9 or more years of schooling Type 1: $\lambda_{8,1}$ Type 2: $\lambda_{8,2}$ Type 3: $\lambda_{8,3}$	1.43 (48.65) 147.43 (41.84) 6.06 (42.96)	Boy age 7 at home Type 1: $\lambda_{34,1}$ Type 2: $\lambda_{34,2}$ Type 3: $\lambda_{34,3}$	697.49 (2161.71) 644.18 (3.34E6) 935.43 (234.52)	Shock to preferences for girls age 12 –15 at home x number of girls at home age 6-11: λ_{49} 12-13: λ_{50}	.87932 (217.73) -.24124 (0.1510)
Pregnancy at: First yr. of marriage: λ_9 Age 20-24: λ_{19} 25-29: λ_{11} 30-34: λ_{12} 35-39: λ_{13} 40-43: λ_{14}	31834.43 (1312.82) -1126.39 (1030.11) -3071.81 (2030.60) -24413.72 (1355.16) -27203.99 (2030.60) -59672.14 (7098.44)	Girl age 6 at home Type 1: $\lambda_{35,1}$ Type 2: $\lambda_{35,2}$ Type 3: $\lambda_{35,3}$	582.94 (822.72) 1676.45 (387.47) 885.99 (272.70)		
Pregnancy× Pregnancy in previous period : λ_{15}	-37001.59 (1442.54)	Girl age 7 at home Type 1: $\lambda_{36,1}$ Type 2: $\lambda_{36,2}$ Type 3: $\lambda_{36,3}$	356.10 (462.73) 1072.03 (1231.52) 680.21 (382.96)		

(a) the discount rate δ is set equal to 0.95

Table A.1, continued
Parameterizations and Parameter Estimates

II. Parents Earnings Function, Child Earnings Function, and Failure Probability Function					
II. Parent Income Function		III. Child Income Function		IV. School Failure Probability Function	
Variable	Std. Error	Variable	Std. Error	Variable	Std. Error
Constant Type 1:$\gamma^p_{0,1}$ Type 2:$\gamma^p_{0,2}$ Type 3:$\gamma^p_{0,3}$	8.86943 (0.09523) 9.06828 (0.09424) 8.79538 (0.128124)	Constant Type 1:$\gamma o_{0,1}$ Type 2:$\gamma o_{0,2}$ Type 3:$\gamma o_{0,3}$	6.98440 (0.19946) 6.92017 (0.206614) 6.98412 (0.278429)	Constant Type 1:$\pi_{0,1}$ Type 2:$\pi_{0,2}$ Type 3:$\pi_{0,3}$	-2.21386 (0.27721) -2.77271 (0.278188) -1.25098 (0.35503)
Husband's age : :γ^p_1	0.02034 (0.00487622)	Child is a boy:γo_1	0.362 (0.15800)	Highest Grade Completed:π_1	-0.12641 (0.046662)
Husband's age squared :γ^p_2	0.0005055 (0.00012301)	Distance of village to nearest city:γo_2	-0.0001329 (0.0005342)	Child age:π_2	0.09557 (0.039131)
Distance of village to nearest city:γ^p_3	-0.0018124 (0.00013122)	Child's age: γo_3	0.02811 (0.0026638)	Child is a boy:π_3	0.11942 (0.08678)
		Child is aged 14-15: γo_4	0.52091 (0.175615)	Child is age 8-15 and has zero years of schooling:π_4	1.62278 (0.255226)
		Child is a boy× Child is aged 14-15: γo_5	-0.05345 (0.202948)	Child's grade >= 7 Type 1:$\pi_{5,1}$ Type 2:$\pi_{5,2}$ Type 3:$\pi_{5,3}$	-0.14309 (1.49843) 0.74041 (0.24014) -0.67016 (1.25187)
		Child is aged 15: γo_6	0.12469 (0.168712)	Child's grade >= 7 and child is a boy:π_6	-0.52010 (0.283648)
		Child is a boy× Child is aged 15: γo_7	-0.08053 (0.205024)		

Table A.1, continued
Parameterizations and Parameter Estimates

V. Type Probabilities: Couples married 8 years or more				VI. Type Probabilities: Couples married 7 years or less		VII. Variance-Covariance Matrix f(ε)=N(0,Ω)	
Variable	Std. Error	Variable	Std. Error	Variable	Std. Error	Variable	Std. Error
Constant Type 1:$\beta^2_{0,1}$ Type 2:$\beta^2_{0,2}$	2.5289 (1.7435) 1.7650 (1.6122)	Mother's age in 1997 Type 1:$\beta^2_{10,1}$ Type 2:$\beta^2_{10,2}$	-0.029033 (0.04564) 0.009445 (0.04145)	Constant Type 1:$\beta^1_{0,1}$ Type 2:$\beta^1_{0,2}$	2.0681 (10.0052) 1.1235 (9.07815)	Variance of pregnancy shock	3.274E9 (1.5114E8)
Mother's age at marriage Type 1:$\beta^2_{1,1}$ Type 2:$\beta^2_{1,2}$	-0.04049 (0.072084) 0.04235 (0.059344)	Number of kids with 7 or more years of education Type 1:$\beta^2_{11,1}$ Type 2:$\beta^2_{11,2}$	-0.80875 (0.41467) 1.27330 (0.33881)	Mother's age at marriage Type 1:$\beta^1_{1,1}$ Type 2:$\beta^1_{1,2}$	-0.0163 (0.36489) 0.0619 (0.3314)	Variance of boy's age 12-15 leisure shock	1.584E6 (5.3911E5)
Father's age in 1997 Type 1:$\beta^2_{2,1}$ Type 2:$\beta^2_{2,2}$	0.00695 (0.01850) -0.00864 (0.017967)	Family has either 3 or more children age 0-5, or 3 or more age 6-11, or two or more age 12-15 Type 1:$\beta^2_{12,1}$ Type 2:$\beta^2_{12,2}$	0.12185 (0.46245) -0.05946 (0.42595)	Father's age at marriage Type 1:$\beta^1_{2,1}$ Type 2:$\beta^1_{2,2}$	0.0142 (0.2344) 0.0016 (0.2156)	Variance of girl's age 12-15 leisure shock	1.015E5 (3.5834E5)
Distance of village to nearest city Type 1:$\beta^2_{3,1}$ Type 2:$\beta^2_{3,2}$	0.000387 (0.00276) 0.001824 (0.00269)	Average schooling of children as of 1997 Type 1:$\beta^2_{9,1}$ Type 2:$\beta^2_{9,2}$	-0.024591 (0.14238) 0.071278 (0.130876)	Distance to nearest city Type 1:$\beta^1_{3,1}$ Type 2:$\beta^1_{3,2}$	0.0020 (0.00844) -0.0014 (0.0082)	Variance of parent's income shock	0.3744 (7.1505E-3)
Distance to secondary school Type 1:$\beta^2_{4,1}$ Type 2:$\beta^2_{4,2}$	0.00001930 (0.0001093) -0.000057 (0.000099)			Distance to secondary school Type 1:$\beta^1_{4,1}$ Type 2:$\beta^1_{4,2}$	0.00016 (0.00109) -0.00011 (0.000995)	Variance of child's wage shock	0.5051 (.1345)
Maximum of mother's and father's education>=9 Type 1:$\beta^2_{5,1}$ Type 2:$\beta^2_{5,2}$	-0.13470 (1.21770) 2.00331 (0.84585)			Maximum of mother's and father's education>=9 Type 1:$\beta^1_{5,1}$ Type 2:$\beta^1_{5,2}$	-0.3498 (2.5492) 1.06700 (2.2920)	Child wage measurement error std. deviation	0.5402055 (0.05363)
Number of kids 14-15 Type 1:$\beta^2_{6,1}$ Type 2:$\beta^2_{6,2}$	-0.00410 (0.5312) -0.15981 (0.5262)						
Number of children 12-15 Type 1:$\beta^2_{7,1}$ Type 2:$\beta^2_{7,2}$	-0.13983 (0.3975) -0.31883 (0.3990)						
Number of children as of 1997 Type 1:$\beta^2_{8,1}$ Type 2:$\beta^2_{8,2}$	0.103014 (0.15726) -0.349685 (0.13455)						

Appendix B
Goodness of Fit Tables

Table B.1
Actual and Predicted Distribution of Number of Births[a]

Number	Actual (Percent)	Predicted (Percent)
0	6.6	15.1
1	36.1	35.6
2	38.5	28.4
3	16.0	14.3
4	2.8	5.2
5 or more	0.1	1.5
Mean Number of Births	1.73	1.63

a. Households married 7 years or less

Table B.2
Actual and Predicted Mean Number of Pregnancies by Age of Mother[a]

Age	Actual	Predicted
20-24	0.168	0.195
25-29	0.151	0.185
30-34	0.098	0.090
35-39	0.066	0.083
40-43	0.018	0.038

a. Households married 8 years or more

Table B.3
Actual and Predicted Fraction Becoming Pregnant
Conditional on Number of Prior Pregnancies[a]

Number of Prior Pregnancies	Actual	Predicted
0	0.568	0.527
1	0.254	0.241
2	0.179	0.182
3	0.115	0.123
4	0.059	0.055

a. Households married 7 years or less

Table B.4
Actual and Predicted One Period Transition Rates Between 1997 and 1998:

	Actual (1998)			Predicted (1998)		
1997[a]	School	Home	Work	School	Home	Work
Girls Age 13 to 15						
School (205)	81.5	17.6	1.0	74.9	20.6	4.5
Home (4)	0.0	92.7	7.3	23.4	63.0	13.6
Work (5)	40.0	40.0	20.0	22.7	54.9	22.4
Boys Age 13 to 15						
School(224)	88.0	8.0	4.0	77.3	14.3	8.5
Home (29)	13.8	48.3	37.9	35.7	36.8	27.6
Work (16)	12.5	25.0	62.5	31.4	35.0	33.6

a. Number of observations, actual

Table B.5
School Continuation Rates:
Children Age 7-15

Highest Grade Completed	Boys Actual	Boys Predicted	Girls Actual	Girls Predicted
0	0.775	0.879	0.692	0.818
1	0.988	0.970	0.993	0.967
2	0.965	0.965	0.975	0.964
3	0.956	0.939	0.961	0.938
4	0.946	0.918	0.972	0.927
5	0.906	0.860	0.940	0.879
6	0.568	0.567	0.472	0.508
7	0.963	0.811	0.957	0.816
8	0.871	0.854	0.927	0.867
9	0.625	0.657	0.467	0.458