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Essays on Policy Evaluation

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Resumen en Castellano

En el trabajo de investigación realizado para la obtención del grado de Doctor en Economía, estudio dos preguntas empíricas utilizando diversas técnicas de la Microeconometría. Dichas preguntas son: 1. Como se puede incrementar la participación escolar de niños pertenecientes a familias pobres, que viven en países en desarrollo y, adicionalmente, han abandonado sus estudios? 2. Pueden los niños afectar comportamientos de salud preventiva de sus padres? Los dos primeros capítulos de la presente Tesis están abocados a responder la primer pregunta, mientras que en el tercer capítulo, en un trabajo conjunto con M. Lucila Berniell y M. Dolores de la Mata, respondemos a la segunda pregunta.

Capítulo 1: Did PROGRESA send dropouts back to school?

Objetivo: En este trabajo cuantifico el efecto causal que las becas escolares entregadas a través de PROGRESA han tenido sobre la participación escolar de niños que han abandonado la escuela en el año anterior a la implementación de dicho programa. Estos niños se enfrentan a la decisión de regresar a la escuela, decisión que puede implicar costos diferentes a los que deben afrontar aquellos niños que actualmente concurren a la escuela. Por esto, podemos esperar que existan efectos diferenciales de las becas escolares PROGRESA sobre la participación escolar de niños que concurren regularmente a la escuela y de aquellos que han abandonado sus estudios.

Metodología: Inicialmente PROGRESA fué implementado como un experimento natural, esto es, se eligió de forma aleatoria (con una loteria) las localidades que recibirían la ayuda de este programa en la primer fase. Todas las familias residentes en dichas localidades (localidades tratamiento) y designadas como pobres (designación realizada utilizando un índice de pobreza) comenzaron a recibir la ayuda en Octubre de 1998. En el resto de localidades (localidades control) se restrasó la implementación del programa hasta Septiembre del año 2000. Esta forma de implementación experimental introduce una fuente de variabilidad exógena en el monto de la beca que permite estimar la relación causal entre el monto de la beca y la probabilidad de asistir a la escuela utilizando estimadores de diferencias, o diferencias-en-diferencias.

Para estimar el efecto que las becas PROGRESA han tenido sobre la participación escolar de niños que se encuentran fuera del sistema educativo, utilizo un contexto de regresión que me permite controlar por las diferencias existentes en características observables entre niños que viven en localidades tratamiento y en localidades control. Con el objeto de evitar realizar supuestos sobre diferencias en la distribución de características inobservadas entre tratados y controles, en la estimación de los parámetros causales de interés, utilizo un modelo Correlated Random Effects Probit Model.

Conclusiones: Encuentro evidencia de la existencia de un efecto diferencial de las becas PROGRESA sobre la participación en la educación secundaria de niños que han abandonado la escuela y niños que concurren habitualmente a clase. En particular, existe un efecto mayor para varones que han abandonado sus estudios que el efecto sobre aquellos varones que se encuentran actualmente en la escuela. Por el contrario, las becas PROGRESA no afectan la decisión de regresar a la escuela para mujeres que han completado los estudios primarios, mientras que el efecto en mujeres que se encuentran dentro del sistema educativo es positivo.

Capítulo 2: The school reentry decision of poor girls. Structural estimation and policy analysis using PROGRESA database.

Objetivo: Como una continuación natural del análisis realizado en el primer capítulo, en este trabajo evalúo la efectividad de diversas políticas públicas diseñadas para incrementar la participación escolar de niñas pertenecientes a familias pobres.

Metodología: Estimo los parámetros estructurales de un modelo dinámico de decisiones educativas utilizando técnicas de Máxima Verosimilitud. El modelo dinámico de comportamiento que utilizo sigue en gran parte el modelo propuesto por Attanasio, Meghir, and Santiago (2005). En su trabajo, estos autores permiten a los individuos elegir entre dos alternativas mutuamente excluyentes: ir a la escuela o trabajar. En mi modelo, los individuos pueden elegir entre tres alternativas: ir a la escuela, trabajar o quedarse en casa. Esta tercer alternativa resulta crucial para las niñas, ya que el 88% de niñas que han abandonado sus estudios no trabajan por un salario, sino que se encuentran en casa, probablemente ayudando a sus madres en la atención de los niños más pequeños de la familia, y en tareas domésticas.
Luego de obtener los parámetros estructurales del modelo, los utilizo para simular tasas de participación escolar de mujeres, bajo diferentes escenarios de políticas: recibir las becas PROGRESA, recibir becas que implican el doble del monto provisto por las becas PROGRESA, disponer de una escuela secundaria en la localidad de residencia, y disponer de acceso gratuito a centros de día para los niños más pequeños de la familia.

Conclusiones: Las becas PROGRESA no aumentan la participación escolar de niñas que han abandonado sus estudios. Tampoco son efectivas otros esquemas de becas que provean a estas niñas con un monto mayor de dinero. La disponibilidad gratuita de un centro de día para los niños más pequeños de la familia incrementa solo marginalmente la participación escolar de estas niñas. Entre las políticas analizadas, la más efectiva en incentivar a niñas que han abandonado los estudios a regresar a la escuela es disponer de una escuela secundaria en la localidad de residencia. Esto genera una reducción no solo en los costos monetarios de transporte, sino también en los costos de tiempo de traslado entre la localidad de residencia y el lugar donde se encuentra la escuela.

Capítulo 3: Spillovers of Health Education at school on parental health lifestyles.

<ins>Objetivo:</ins> La enseñanza de Educación para la Salud se ha ido generalizando en los últimos años debido principalmente a su reconocimiento como un arma eficiente en la prevención de enfermedades que en la actualidad son categorizadas como epidémicas, como la obesidad y las afecciones cardiovasculares. Este particular tipo de educación tiene como objetivo mejorar el estado de salud de los estudiantes. Sin embargo, el estado de salud y los comportamientos de salud preventiva de otros miembros de la familia del estudiante pueden ser afectados. En este trabajo cuantificamos el efecto causal sobre la frecuencia de actividad física de padres de niños que están recibiendo clases de Educación para la Salud.

<ins>Metodología:</ins> Entre los años 2000 y 2006 se realizaron importantes reformas en la enseñanza de Educación para la Salud en Estados Unidos. Estos cambios no han sido uniformes entre estados, diferenciándose en el momento de la reforma, la cantidad de cursos introducidos y el nivel de rigurosidad con que estos cursos son impartidos. La heterogeneidad en las reformas entre estados nos provee de un quasi-experimento al introducir una fuente de variación exógena que nos permite estimar la relación causal entre la frecuencia de actividad física de un padre y el hecho de que su hijo reciba Educación para la Salud en la escuela. Para estimar el efecto de interés, utilizamos diferencias-en diferencias-en diferencias, o Triple Diferencias.

<ins>Conclusiones:</ins> Encontramos evidencia de la existencia de un efecto positivo del hecho de que un niño reciba Educación para la Salud en la escuela sobre la frecuencia de actividad física de su padre, mientras que su madre no se ve afectada. En particular, la implemetación de Educación para la Salud en las escuelas primarias incrementa la probabilidad de que los padres de los niños que concurren a estas escuelas realicen actividad física 7 veces por semana en 18.6 puntos porcentuales.
Consideramos que hay al menos dos canales que explican estos resultados. Primero, el hecho de compartir información en la familia. El nuevo flujo de información que ingresa a la familia a través del niño que recibe Educación para la Salud, es más probable que afecte a aquellos individuos que tienen un stock de información más bajo. Siguiendo esta hipótesis, aquellos padres con un nivel educativo menor serán más afectados, hipótesis que hemos podido sustentar con nuestros resultados. Si, adicionalmente, aceptamos que los padres tienen menos información que las madres en cuanto a los beneficios para la salud de realizar actividad física, nuestros resultados también son compatibles con la hipótesis antes

planteada. Segundo, la Educación para la Salud puede afectar directamente la frecuencia con la que los niños realizan actividad física, y, a través de los cambios en el comportamiento del niño, también afectar el comportamiento de sus padres. Como lo muestran las encuestas de uso de tiempo realizadas en Estados Unidos, las madres pasan el doble del tiempo cuidando de sus hijos, que los padres. Pero son estos últimos los que pasan más tiempo realizando activdades recreacionales con sus niños. Esto incluye también el tiempo en el que realizan actividad física. Esto hace a los padres, más que a las madres, susceptibles de ser influenciados por el cambio en el comportamiento de sus hijos.

Contents

Chapter 1

Did PROGRESA send dropouts back to school?

1.1 Introduction

The key role of education as an anti-poverty and pro-growth policy via the effect of education on the accumulation of human capital is widely recognized by both policy makers and the economic literature. Not surprisingly, policies aimed at increasing education have received a great deal of attention over the last 50 years.
Government policies can influence educational choices by affecting the three main factors that determine school enrollment and completion: demand, supply, and educational policy. On the demand side, policy makers have decreased direct costs by reducing or eliminating tuition or transport costs. Thet have also implemented cash transfers programs aimed at decreasing opportunity costs of wage income and/or home production forgone. Young people may also leave school for supply side reasons such as the availability or quality of education. Increasing the availability of primary and secondary schools and improving the quality of the education by reducing class size and strengthen the qualifications of teachers, have long been included in educational policy portfolios. On the institutional side, governments have been increasing the number of years of compulsory education. In most countries children have to attend school until the age of 16 years old, and many countries are imposing compulsory education until the secondary school level.
Over the last two decades there has been a widespread use of programs aimed at fostering the accumulation of human capital in the developing world. The increasing use of anti-poverty programs has been accompanied by comprehensive evaluations about the actual effectiveness of these programs. This has occurred not only because of the intrinsic interest in these programs but also because anti-poverty programs represent an important financial effort, both by governments and international institutions which often provide additional funds.

This paper focuses on a Mexican anti-poverty program for rural communities, called PROGRESA (Education, Health and Nutrition program), first implemented in 1997 by the Mexican Federal Government. The program comprise three major areas one of which, the subject of this paper, is education. In particular, program beneficiaries are given financial aid conditional on school attendance. This paper analyzes the effects of such grants on school enrollment for two different groups of beneficiaries, dropouts and non-dropouts. The identification of these effects relies on the randomized assignment of the program benefits. The evaluation of the program was conducted by the Mexican Federal Government and by external local and international evaluators such as the National Institute of Public Health (INSP, Mexico), Research and Advanced Studies Center in Social Anthropology (CIESAS, Mexico), International Food Policy Research Institute (IFPRI), and Research and Educational Documentation Center (CIDE, Spain)[1]. The evaluation efforts have resulted in an extensive literature by authors like Orazio Attanasio, Jere R. Behrman, David Coady, Costas Meghir, T. Paul Schultz, Emmanuel Skoufias, Petra Todd and Kenneth Wolpin, among others. In this literature authors estimate average treatment effects (ATE) exploiting the randomized assignment of the program. Their results prove the success of PROGRESA in increasing enrollment rates for those children who received the grants, and they agree in that this positive effect is higher on girls and on children who attend secondary school.

Although the ATE can be a good general characterization of the overall (average) effect, it is obvious that any program will have a different impact on different individuals. Some of them will benefit a lot whereas others will not. Therefore, it is also important to take into account individual heterogeneity.

The main contribution of this paper is to track the differential effect of the program on individuals that dropped out of school before the program started. These children are facing a re-enrollment decision that may imply higher direct and/or indirect costs of schooling than the costs faced by the average child. Moreover, dropouts are different from the average child in some observable characteristics related to the schooling decision. Thus, we can expect a different effect of the program on them. The methodologies applied are difference estimation and maximum likelihood estimation of a reduced form equation of education choice. For both cases the randomized design of PROGRESA is exploited. The outcome is the causal effect of the education component of this program.

The estimated marginal effects provide evidence on the existence of differences between the impact of PROGRESA grants on the overall target population and their impact on the children who face a re-enrollment decision. The rise in boys school attendance caused by PROGRESA grants is higher for dropout boys in both levels of education, primary and secondary school. However, for girls in secondary school who dropped out in 1997 or before,

[1] Corresponding web pages: Federal Government: www.oportunidades.gob.mx; INSP, www.insp.mx; CIESAS, www.ciesas.edu.mx; IFPRI, www.ifpri.org; CIDE, www.mec.es/cide/

the grant has a negligible effect on their decision to reentry in school. Among dropouts in secondary school the impact of PROGRESA grants is lower for girls even though they receive more money than boys.

The structure of the paper is as follow. Section 2 presents the main features of the program and a brief review of the literature evaluating PROGRESA. Section 3 discuses factors that influence the enrollment decision and presents differences to the re-enrollment decision made by dropouts. Section 4 describes characteristics of the PROGRESA data base. It provides some main statistics that focus on the differences between dropouts and non-dropouts. In Section 5 results for the difference estimation of the effects of the program are presented for both groups and are analyzed separately. Section 6 introduces a reduced form equation for the schooling decision including PROGRESA education grants variables. Section 7 presents maximum likelihood estimates of a probit model for schooling decision, comparing results for non-dropouts and dropouts. Finally, Section 8 concludes the paper with its main results and some suggestions for future research.

1.2 The PROGRESA program and its education component

The Education, Health and Nutrition program, PROGRESA, was implemented by the Federal Government of Mexico in 1997, with the aim of helping the poorest families in rural communities. A fundamental characteristic of the program is that aid is conditioned on a specific behavior of the beneficiary. This conditionality tries to guarantee that the program does not lead to undesired outcomes, such as distortions in work decisions, and that it successfully accomplishes its initial objectives.

The program comprises actions in three major areas: education, health and nutrition. The education component includes monthly grants for children of a family qualified as beneficiary. They need to be less than 18 years old, enrolled in school between the 3^{rd} year of primary school and the 3^{rd} year of junior secondary school, and to fulfill a minimum attendance requirement. The grants are not based on academic achievement. A child who does not pass a grade is still eligible for the grant in the following year. But if the child fails the same grade twice, she/he losses eligibility. The grant increases by years of schooling. In the junior secondary level the grant is slightly higher for girls, since there exist evidence that in poor families girls are more likely to dropout of school and that they also tend to dropout earlier than boys. Additionally, beneficiaries receive an annual grant for school supplies. The health component of the PROGRESA program consists of a basic package of free health services, nutritional supplements, and informative talks on health, nutrition, fertility, and hygiene. Special attention is paid to pregnant women and children younger than five years. Finally, the nutrition component of the program supplies beneficiary families with a monthly monetary payment intended to improve amount and diversity of food consumption and thus increase the nutritional status, in particular of children. This aid is independent of residence, and size, and composition of the family. All aid is given to the mother of the family as there exist evidence that mothers are better than fathers at allocating family resources.

A family is qualified as being poor and thus eligible for the program according to a single index. This index contains information on family income and housing like presence of running water, etc.[2]

Some numbers can provide a better understanding of the extent and significance of PROGRESA as an anti-poverty policy. In 1997 the program reached 6,357 communities, giving aid to 300,705 families. This implied transfers of 34 million USD (approx. 340 million Mexican pesos). After two years of being implemented the program included nearly 2.6 million families in 72,345 communities in all 31 Mexican states. It reached around 40% of all rural families and nearly 12% of all families in Mexico. Total annual transfers of the program in 1999 were around 710 million USD, equivalent to 0.15% of Mexican GDP. 40% were educational transfers, 42% food transfers and 18% was spent on health transfers. Among the total annual cash transfers of 578 million USD, food transfer accounted for 49%. The remaining 51% went to education. In 1999 the program distributed 273 million USD in education grants[3].

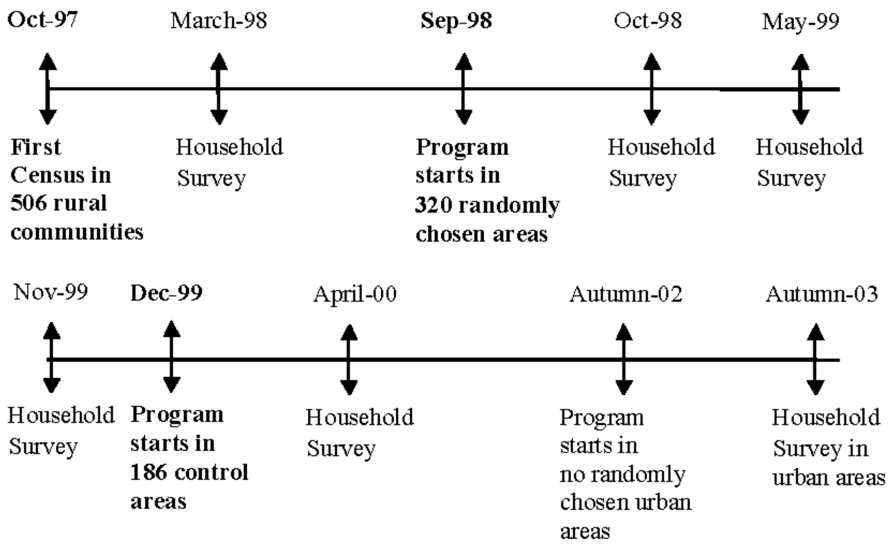
Given the financial importance of PROGRESA, Mexican authorities have intended to evaluate the program since its beginning, not only to measure results and impacts but also to provide information that allow for a redesign of policies. Accordingly, in 1997 and 1998 a high quality data set was collected in 506 communities where the program was to be implemented, and several surveys were carried out afterwards. In October 1998, the program was implemented in 320 randomly selected communities (treated communities) while in the remaining 186 communities (control communities) the implementation was postponed until December 1999[4]. In Figure 2.1 below, I present the timing of the program.

[2]For a complete analysis of the targeting see Skoufias, Davis, and Behrman (1999a) and Skoufias, Davis, and Behrman (1999b).

[3]For more details on PROGRESA costs see Coady (2000).

[4]The quality of the randomization has been extensively documented in Behrman and Todd (1999), who conclude that, at least at community level, the implementation of the random assignment was performed successfully.

Figure 1.1: Timing of the PROGRESA program



The evaluation of the program was conducted by the Mexican Federal Government and by external local and international evaluators such as the National Institute of Public Health (INSP, Mexico), Research and Advanced Studies Center in Social Anthropology (CIESAS, Mexico), International Food Policy Research Institute (IFPRI), and Research and Educational Documentation Center (CIDE, Spain). The evaluation efforts have resulted in an extensive literature by authors like Orazio Attanasio, Jere R. Behrman, David Coady, Costas Meghir, T. Paul Schultz, Emmanuel Skoufias, Petra Todd and Kenneth Wolpin, among others.

This paper is closely related to Schultz (2004). In his paper Schultz presents an extensive evaluation of the education component of PROGRESA. The author performs pre-program comparisons to check the randomization of the design, and he calculates difference and difference-in-difference estimators by gender and grade which allow him to quantify the program's causal effect. To validate difference estimations he shows results of maximum likelihood estimation of a reduced form equation of the school enrollment decision. He concludes that the program has effectively reached its goal since he finds positive and large post-program differences in enrollment rates of comparably poor children in treatment and control communities.

Other related relevant papers are Attanasio, Meghir, and Santiago (2005) and Todd and Wolpin (2006), who follow a structural approach to evaluate PROGRESA. They can thus simulate the effects of counterfactual programs and they can identify alternative subsidy schemes with a greater impact on schooling decisions.

Attanasio, Meghir, and Santiago (2005) estimate the structural parameters of a standard model of education choices that considers schooling as an individual decision. Similarly to

Schultz (2004), they find that PROGRESA has a positive effect on the school enrollment of children, especially after the completion of primary school. They also show that a revenue neutral change in the program that increased the grants for junior secondary school children while eliminating the ones for primary school children, would have a substantially larger effect on enrollment of secondary school children, while having only minor effects on the enrollment of primary school children.

Todd and Wolpin (2006) estimate a dynamic behavioral model of parental decisions about fertility and children schooling. Their paper differs from Attanasio, et al in two main aspects. Todd and Wolpin (2006) model schooling as a family decision. They use data from the control group prior to the experiment in the validation and estimation of the model, i.e. they use only pre-program information in the estimation of the parameters of interest. They then apply the model to analyze the effectiveness of alternative policies to increase enrollment rates.

1.3 Enrollment vs. re-enrollment decision

1.3.1 Influential factors for the enrollment decision

From an economic point of view, the school enrollment decision is taken based on the private price of schooling. The total price of schooling includes tuition fees, direct costs of attending school, such as clothing, books, materials and transportation costs but also the opportunity cost if attending school. Since in Mexican rural communities public schools are mostly tuition-free, the main component of the price of schooling is the opportunity cost of time. A student could devote her/his time spent at school to other activities, such as paid work, farming, or any other productive activity at home. PROGRESA directly reduces the price of schooling through grants and aid for school supplies. From this reduction in the price of schooling, we would expect a positive effect of the program on enrollment rates.

The main component of the opportunity cost of schooling is the rural wage a child can earn as farming or home production activities are difficult to measure in monetary terms. Unfortunately only a small fraction of communities report such information. As a proxy we consider the agricultural wage for adult male.

In communities with high salaries we expect that children are less likely to go to school, because they face a higher opportunity cost. Additionally, medium and large cities have more developed labor markets that usually offer higher wages. So we expect a child residing near a metropolitan area or near the main city of her/his municipality to be more likely to dropout of school and to work instead.

Transportation cost are an important direct cost of schooling for children attending junior secondary school. Only in 25% of all communities under study have a proper junior secondary school. A reasonable proxy for this cost is the distance from the community

where the child resides to the nearest one that has a secondary school. In all the communities studied there exist at least one primary school, so we can set transportation costs for primary school children equal to zero.
Given that a child's schooling is a family decision, it is necessary to analyze family characteristics that may influence this decision. There is a general agreement that more educated parents are more likely to send their children to school. If the father lives at home and works we expect his children to be more likely to go to school, as the financial situation of the family is more stable. Health and work status of the head of the household are also relevant for a child's schooling decision. If the head of the household was unemployed or ill for some weeks before the children should have been enrolled at school it is likely that the children are sent to work.

1.3.2 The re-enrollment decision

The focus of this paper is not the overall population but those children that are making a re-enrollment choice, i.e. dropouts. Drop-outs are those who have made the decision of not attending school at some point in time and were not enrolled in October 1997, before the implementation of PROGRESA. These children were not receiving enough incentives to go to school, to improve their educational level, and to contribute finally to human capital accumulation and the development of their communities. Regarding the aim of PROGRESA, dropouts are thus an important target of the program.
Is there any reason to think that PROGRESA education grants could have a different impact on enrollment rates of dropouts than on those of non-dropouts? A hypothetical answer to this question can be made based on observable differences between both groups. In particular, we can look at information provided by the pre-program census (October 1997) and interpret it referring to the conclusions from the previous subsection. Some numbers are given in Table 1.1 below.
As I expect values of some variables to be different for different levels of schooling and gender, data is presented separately for primary and secondary school children and for girls and boys. The primary school sample includes all children aged 6 to 18 who have completed 0-5 years of schooling and are thus qualified to enroll in primary school grades 1 to 6. In the secondary school sample I considered all children aged 11 to 18 who have completed at least 6 years of schooling and are thus qualified to enroll in junior or senior secondary school.

Table 1.1: Difference in averages and proportions of selected variables between Drop-outs and Non-dropouts (pre-program census)

Variable name	Primary		Secondary	
	Female	Male	Female	Male
Percentage of children belonging to a poor family	4.2***	3.7***	8.0***	3.7*
Mother's schooling (years)	-1.5***	-1.3***	-1.0***	-1.0***
Percentage of children with father living at home	-3.4**	-5.8***	0.9	-0.1
Number of siblings enrolled at school	-1.7***	-1.6***	-1.2***	-1.3***
Distance to secondary school (km)	0.92***	0.64***	1.11***	0.74***
Distance to metropolitan area (km)	-17.6***	-19.3***	-22.8***	-31.4***

* Statistical significance = 10%. ** Statistical significance = 5%. *** Statistical significance = 1%.
The statistical significance of the differences is tested using tests for equality of means and proportions.

Having a more educated mother increases the probability of enrollment for non-dropouts. The positive effect of parents' education on the education of their children is well known. Additionally, the proportion of children of families with the father living at home is higher, making them more likely to attend school (at least in primary school). Non-dropouts face lower direct cost of schooling since they live closer to a secondary school.

On the other hand, dropouts reside closer to metropolitan areas and the main cities of their municipalities. Drop-outs reside in communities with higher wages. These facts imply a higher opportunity cost of schooling leading to a lower probability of re-enrollment for dropouts. Moreover, a higher proportion of them belong to a poor family, making them more likely to work and not to attend school.

Additional information is given in the surveys that were carried out after PROGRESA started. In particular, these surveys ask why the child was not enrolled in school. For dropouts the answers are: "There was not enough money" (47%), "She/he did not like going to school" (26%), "The school was very far away" (9%), and "Her/his help was needed at work or at home" (4%). Clearly the main reason for not attending school are financial restrictions at home making the alternative of working even more attractive.

Summing up, there exists enough evidence to conclude that dropouts have more incentives to work rather than to attend school compered to non-dropouts. Given the higher opportunity cost of schooling that dropouts face and since the grants are a monetary incentive[5], I expect the effect of the program on dropouts to be larger than on the average child.

[5] The monthly agricultural wage is around $ 500, but a child actually earns less than this amount. A secondary school child's grant is approximately $ 250. These numbers show how important PROGRESA grants are as an additional source of family income.

Thus, the proposed hypothesis based on observable characteristics is that the program has a stronger effect on the schooling decision of dropouts. However, we do not know in which direction any unobservable characteristic of the child, like ability or ambition, could affect the schooling decision and if it could affect the schooling decision of dropouts and non-dropouts in a different way.

1.4 Data base and descriptive statistics

Since in this paper I analyze the grants' impact conditional on the schooling decision children have made before the implementation of the program, only post-program information can be used. From the education component of the PROGRESA post-program surveys (October 1998, May 1999 and November 1999) a matched panel sample for children aged 6 to 18 can be obtained. This panel includes 74,427 observations, 45,666 (61%) in the treatment group (individuals residing in a community where PROGRESA grants were implemented in September 1998) and 28,761 (39%) in the control group.

Before going into detail on the description of the data base, three comments should be made. First, there exists a maximum amount of aid a household can receive by means of the education component of the program. Those maximum amounts are updated every six month (as it happens with grants). When the maximum is reached each child receives only a percentage of the grant. Unfortunately, the exact amount each child receives is not reported in the data base. For this reason what can be measured is only the effect on school enrollment of the "potential grant". Using this measure for the effect of the program we may overestimate the "actual grant" effect. If the child's family is not receiving the maximum amount the potential grant coincides with the actual grant. In the PROGRESA data base the average number of children in a family is 4. This makes it very likely for a family to attain the maximum amount of aid.

The second comment is about the treatment group. Around 5% of those children fulfilling the requirements to obtain the grant are not receiving it[6]. The reason for this is not available in the data base. The grant amount for them is set to zero.

Finally, the variable reflecting the stock of education (years of schooling completed) presents some inconsistencies along the waves of the surveys. 29% of the observations show some of these inconsistences. For this reason I perform a hand-correction of the variable "stock of education" using the information available in five waves, that cover information on four academic years. For all observations to be corrected I have at least two consistent combinations of enrollment-age-schooling, and I correct the remaining points making it compatible with that sequence. In all cases I preserve the information related with enrollment to school and the age. Given the exogenous variation generated by the random assignment of the

[6] The exact numbers are 5.62% for non-dropouts and 5.14% for dropouts.

grant, there is no reason to think that the measurement error in the stock of education will bias the estimation of the grant effects. Nevertheless, it may induce some bias in the estimation of average treatment effects by level of education.

In terms of the data base, dropouts are those individuals aged 8 to 18 in the post-program surveys who were not enrolled at school in the first census (October 1997). The re-enrollment or dropouts panel includes 6,948 observations, 4,155 (60%) in the treatment communities and 2,793 (40%) in the control communities.

Table 1.2 presents a set of descriptive statistics that characterize the population in the dropout panel.

Table 1.2: Descriptive statistics for Drop-outs (post-program surveys)

Variable	Primary		Secondary	
	Female	Male	Female	Male
Sample size	1,310	1,490	2,431	1,717
Enrollment rate	0.526	0.528	0.258	0.259
Percentage of treatment communities	57.5	59.9	59.2	62.3
Percentage of children belonging to a poor family	92.9	92.9	84.2	83.1
Percentage of children eligible for receiving a grant	26.9	31.8	44.5	46.7
Grant (for grant different from zero) (pesos)	118.7	122.7	250.7	237.6
	(31.4)	(30.5)	(22.9)	(19.2)
Mother's schooling (years)	1.5	1.6	2.1	2.0
	(2.1)	(2.0)	(1.9)	(2.1)
Percentage of children with head of household ill	6.3	6.5	6.8	6.9
Percentage of children with head of household employed	89.2	89.5	90.2	87.5
Percentage of children with father not living at home	13.3	16.2	9.6	10.9
Number of girls from 5 to 16	2.0	1.0	1.9	1.0
	(1.2)	(1.0)	(1.2)	(1.0)
Number of boys from 5 to 16	1.0	1.9	1.0	1.9
	(1.0)	(1.3)	(1.0)	(1.2)
Number of children under 5	0.9	0.9	0.6	0.5
	(1.0)	(1.0)	(0.9)	(0.9)
Number of adult women	1.7	1.6	2.0	1.8
	(0.9)	(0.8)	(1.0)	(1.0)
Number of adult men	1.6	1.8	1.8	2.2
	(1.0)	(1.0)	(1.4)	(1.1)
Number of siblings enrolled at school	2.1	2.3	2.3	2.0
	(1.5)	(1.4)	(1.4)	(1.5)
Distance to secondary school (km)	3.2	3.0	2.8	2.6
	(3.3)	(3.0)	(2.0)	(1.9)
Percentage of children that have a secondary school in their community	18.0	21.9	15.1	18.5
Distance to nearest metropolitan area (km)	131.8	131.0	129.2	125.2
	(64.8)	(59.7)	(73.2)	(66.4)
Distance to the main city of her/his municipality (km)	11.2	11.2	12.0	10.7
	(7.2)	(7.2)	(9.4)	(7.4)
Community daily agricultural wage (pesos)	29.4	30.2	33.4	31.8
	(11.4)	(11.2)	(11.9)	(11.6)

Standard deviations are in parenthesis *(continued in Appendix as Table 1-continued)*

Table 1.6 of the Appendix reports similar statistics for non-dropouts.
In both tables variables are somewhat different for girls and boys, as expected. Also, we see different variable values for children in primary and in secondary school. Hence, I want to use an estimation strategy that will allow for differences in the program's effects by gender and by level of education.

Comparing Table 1.2 and Table 1.6 we observe differences between dropouts and non-dropouts. Below, in Table 1.3 there is a list of variables for which means and proportions in both panels are statistically different.

Table 1.3: Difference in variable means and proportions between Drop-outs and Non-dropouts (post-program surveys)

Variable name	Primary		Secondary	
	Female	Male	Female	Male
Enrollment rates	-0.450***	-0.443***	-0.508***	-0.531***
Percentage of children belonging to a poor family	3.5***	3.1***	3.7***	1.2
Mother's schooling (years)	-1.6***	-1.4***	-0.8***	-0.9***
Percentage of children with head of household employed	-2.6***	-2.1***	0.1	-1.9**
Percentage of children with father living at home	-3.5***	-6.1***	0.7	-0.2
Number of siblings enrolled at school	-0.7***	-0.6***	-0.6***	-0.6***
Community daily agricultural wage (pesos)	-1.2***	-0.4	1.7***	0.7**
Distance to secondary school (km)	0.9***	0.6***	0.9***	0.6***
Distance to metropolitan area (km)	-16.9***	-19.4***	-23.1***	-28.8***
Distance to the main city of her/his municipality (km)	-0.6**	-0.4**	-0.2	-0.8***

* Statistical significance = 10%. ** Statistical significance = 5%. *** Statistical significance = 1%.
The statistical significance of the differences is tested using tests for equality of means or proportions.

An important fact pointed out by Tables 1.2 and 1.3, is the low enrollment rate of dropouts. Only almost 60% of primary school children are actually attending class. Still worse is the situation for secondary children. Only 25% of them go to school. Compared with non-dropouts, enrollment rates after the implementation of the program are 45% lower for primary school dropouts, and more than 50% lower for secondary school dropouts. Some questions naturally arise from these figures. Why are these differences so large? Why is a child that decided not attend school once unlikely to re-enroll? Can we infer from these numbers that PROGRESA is not working all that well for dropouts contrary to what we expected?

To answer the first two questions take a look at Table 1.3. Again, as in Section 1.3.2, we conclude that, not considering grants, dropouts have more incentives to work than to attend school. Moreover, a higher proportion of dropouts come from poor families and with unemployed heads of households. Also, they have a higher direct cost of attending secondary school reflected in higher distances to secondary schools. Another explanation for the differences in enrollment rates could be that some unobserved characteristics as ability or personal ambition affect a child's schooling decision.

The remaining question is if the PROGRESA program is convincing those children who dropped out of school before the implementation of the program to go back to school and finish their education. If the answer is yes this implies that without the program enrollment rates would be much lower. On the other hand, if the program is not working for dropouts pre and post program enrollment rates should be equal. In both situations it is necessary to study alternatives schemes for the grant design that could send more dropouts back to school.

The answer cannot be obtained by just looking at descriptive statistics but needs to make use of the randomized assignment of the program. Comparison of results between treatment and control communities allows us to estimate the causal relationship between enrollment decision and PROGRESA grants.

1.5 Estimation of PROGRESA grants impact

1.5.1 Difference estimation

The random assignment of PROGRESA at community level has a crucial advantage. Randomization balances all observed and unobserved variables other than enrollment decision and treatment status across the two groups (treatment and control). Hence, this makes it possible to quantify the effect of the program on enrollment rates by simply comparing enrollment in treatment vs. enrollment in control communities, i.e., difference estimation can simply measure the program's effects.

To analyze if there exist differences in the effect of the program on non-dropout and dropouts we can estimate separately difference estimators for both groups, and compare the results. In the context of this paper difference estimators are defined in the following form:

$$\hat{\mathbb{E}}_n(S_{it} \mid grant_{it} > 0, P_i = 1) - \hat{\mathbb{E}}_n(S_{it} \mid grant_{it} = 0, P_i = 1), \qquad (1.1)$$

$$i = 1, ..., N \qquad\qquad t = 2, 3, 4$$

where S_{it}: dummy equal 1 if the child is enrolled in school at time t. $\hat{\mathbb{E}}_n$: post-program period averages. $grant_{it}$: the potential grant amount, that takes a value different from zero only if the child belongs to a poor family, resides in a PROGRESA community, and is attending a grade between 3^{rd} year of primary school and 3^{rd} year of junior secondary school. $grant_{it} > 0$ defines the treatment group while $grant_{it} = 0$ defines the control group[7]. P_i: is a dummy variable that takes on a value of 1 if the child belongs to a poor family. $t = 1, 2, 3, 4$ identify the October 1997 census and the October 1998, May 1999 and November 1999 surveys, respectively.

[7] For children who fulfill the requirement to obtain the grant but are not receiving it I set $grant_{it} = 0$. In the calculus of difference estimates they belong to the control group.

Unfortunately, difference estimation applied to the original data set partitioned in our two groups, dropouts and non-dropouts, may not be reliable. Randomization in the assignment of the program assures that any kind of analysis of the complete panel that implies disaggregation based on observable characteristics, other than the dependent variable and the treatment definition variable, is valid. Also, a sufficiently high number of observations is needed for a law of large numbers to hold in both groups defined by the treatment status. However, the variable that defines the groups under analysis is the dependent variable, school enrollment, in October 1997. Moreover, the dropouts panel fails to contain enough observations when we split the data between treatment and controls, by school level and by gender[8].

Only if randomization still holds when considering non-dropouts and dropouts observations separately, difference estimation is valid. But this is not the case here. I carried out an analysis of the randomization in both sub-panels following the methodology of Behrman and Todd (1999). Comparing means and distributions of observable characteristics between treatment and control observations I found some differences. Hence, the main conclusion is that the assignment of the program is not completely random when considering the groups as presented. Results for a set of relevant variables can be consulted in Table 1.9 and Table 1.10 of the Appendix. Therefore, difference estimation cannot provide accurate results. Nevertheless, I present the difference estimates of PROGRESA grants effect on Table 1.4 below and I compare these estimates with those obtained using a regression framework in Section 1.5.3.

1.5.2 Regression framework

Following the discussion presented in Section 1.3.2 and including variables that reflect the impact of the PROGRESA program, a reduced form equation in latent variable form for the probability of being enrolled in school at time t, S^*_{it}, is[9]:

$$S^*_{it} = \eta_i + \alpha_{0t} + \alpha_1 P_i + \alpha_2 T_i + \sum_{k=2}^{8} \alpha_{3k} grant_{kit} + \sum_{k=1}^{K} \gamma_k C_{kit} + \sum_{j=1}^{J} \beta_j X_{jit} + e_{it} \tag{1.2}$$

$$i = 1, 2..., n \qquad t = 2, 3, 4 \qquad \text{and} \qquad e_{it} \sim F$$

What we observe, in fact is:

$$S_i = \mathbf{1}[S^*_i > 0], \qquad i = 1, 2..., n \tag{1.3}$$

[8]Size for each group are reported in Table 1.8 of the Appendix.

[9]This reduced form equation is similar to the one proposed in Schultz (2004). The main differences are the introduction of an additional term to allow for time-constant unobserved effects and the introduction of a set of variables that allow for identification of differential effects of the program for dropouts.

η_i is an unobserved factor, individual specific and time-constant. It may reflect ability, personal ambition, etc. α_{0t} is a time variant unobserved effect. P_i is a dummy variable that takes on a value of 1 if the child belongs to a poor family. T_i is a dummy variable that takes on value 1 if the child lives in a community where the program started in September 1998, i.e., in a treatment community. $grant_{it}$ as defined in Section 1.5.1[10]. C_{kit} is equal to 1 if the child has successfully completed k years of school, $k = 1$ or less, $2, ..., 8$ and 9 or more, which qualifies the child for enrollment in $(k+1)^{th}$ grade. X_{jit} are a set of J individual, family, and community characteristics that includes the age of the child and the square of the age, mother's schooling, a dummy equal to one if the head of household was ill, a dummy equal to one if the head of household was employed in the week before the survey was conducted, a dummy set equal to 1 if the father lives at home, the number of girls younger than 16 years in the family, and the number of boys younger than 16 years in the family, the number of children younger than 5 years in the family, the number of adults women and men in the family, number of siblings enrolled in school, daily mean agricultural wage for men, distance to nearest junior secondary school, distance to nearest metropolitan area, and distance to the main city of her/his municipality. F is a distribution function.

The expected values for the coefficients are the following. α_1 should be negative reflecting the common hypothesis that credit constraints limit the investment of the poor in their children's education. The effect of residing in a treatment community, or α_2, should be close to zero, since the assignment of the program is random, or slightly positive capturing some "spillover effects" of the treated communities on the control communities. α_{3k} captures the program effects, so it is greater than zero if the program successfully reaches its goal.

For the $\beta' s$, we expect a negative effect of age, since for a given grade being older implies higher costs of schooling (higher opportunity costs for being more likely to get a job and to obtain a higher salary, psychological cost of disappointment if she/he failed, etc), a positive effect if the mother is more educated, a negative effect if the head of the household was ill and a positive effect if she/he had a job, a positive effect if the father lives at home, also a positive effect if the proportion of siblings attending school is higher, a negative effect from the opportunity cost of schooling (captured by wages), a negative effect from the direct cost of attending a junior secondary school (i.e. the non-existence of a school in the community), and finally a positive effect of the distance to the nearest metropolitan area and of the distance to the main city of her/his municipality.

To answer the question "Did PROGRESA send dropouts back to school?" it is necessary to model the probability of being enrolled for individual i at time t conditional on the schooling decision she/he made before the program started:

[10] For children who fulfill the requirement to obtain the grant but are not receiving it I set $grant_{it} = 0$ and $T_i = 1$.

$$\mathbb{P}(S_{it} \mid S_{i1}), \qquad t = 2, 3, 4 \tag{1.4}$$

and then compare these probabilities between ex-ante dropouts ($S_{i1} = 0$) and children who were at school before the program started ($S_{i1} = 1$).

In order to capture the differences in the program's effects on non-dropouts compared to dropouts, the equation for the enrollment decision is modified as follows:

$$S^*_{it} = \eta_i + \alpha_{0t} + \alpha_1 P_i + \alpha_2 T_i + \alpha_3 D_i + \alpha_4 P_i * D_i + \alpha_5 T_i * D_i + \tag{1.5}$$
$$\sum_{k=2}^{8} \alpha_{6k} grant_{kit} + \sum_{k=2}^{8} \alpha_{7k} grant_{kit} * D_i + \sum_{k=1}^{K} \gamma_k C_{kit} + \sum_{j=1}^{J} \beta_j X_{jit} + e_{it},$$
$$i = 1, 2..., n \qquad \text{and} \qquad t = 2, 3, 4$$

where D_i is a dummy variable, that takes a value of 1 if the child dropped out of school before the program started. The impact of the program for non-dropouts is captured by the variable "grant", i.e., by the coefficient α_{6k}. The impact for dropouts is given by $\alpha_{6k} + \alpha_{7k}$. Hence, the difference in the program's impacts on non-dropouts and dropouts, is equal to α_{7k}.

In order to estimate the parameters of equation 1.5 we have to take into account its two main characteristics . First, it is a probability model, and second, there is an unobserved fixed effect.

A fixed effects conditional logit model would allow us to consistently estimate the parameters using a non-linear model and without any assumptions on how η_i is correlated with the exogenous variables. This approach is desirable for at least three reasons. The estimated probabilities are between 0 and 1, marginal effects are individual specific and it allows for the most flexible specification of the unobserved heterogeneity. However, such a model cannot be applied to the equation above since there is not enough variation in the data. A fixed effects non-linear estimation strategy can only considers observations for which the dependent variable has time variation. Applying this restriction to the PROGRESA panel we are left with 9,036 observations. Of those observations only 1,632 refer to dropouts. This is not enough data to identify the effect of interest.

Alternatively, we can use a fixed effects linear probability model (FELPM). It also has the most flexible specification for the fixed effect, so consistency is not an issue. Nevertheless, it has problems associated with the estimation of a probability using a linear model. One concern is about the marginal effects that in this model are assumed to be constant among individuals. In the context of schooling decision this assumption is not realistic. This feature may be overcome by allowing for non-linearities in the effect of the grant in the latent variable model, but only for time varying characteristics. Two important time-invariant factors affecting the schooling decision are the stock of education of the children's mother and the distance to the nearest school available, and by using a linear model the estimated marginal effects will be assumed to be the same for all children, regardless their mothers level of education and the transport cost they have to pay to attend school. We still can introduce interactions between children's characteristics relevant for the decision of attending school like age, schooling, and family composition variables, and most importantly by decomposing the effect of the grant by children's family characteristics. While this solution seems to be appealing for the estimation of the effect of the grant on non-dropout children, to decompose the effect of the grant on dropouts will rise the problem that some of these parameters will be identified by a small number of observations, so the accuracy of the resulting estimates may not be reliable. Another concern with the linear model is the number of observations for which the model estimates probabilities outside the interval $[0, 1]$. Using parameters values estimated with a FELPM, the proportion of observations for which the estimated probability lies outside the unit interval is 39%. This result makes difficult to asses how convincing is the fit of the model, that is usually tested comparing the distribution of actual and estimated probabilities.

Hence, I decided to estimate the effect of PROGRESA grants using the model proposed by Chamberlain (1980) andMundlak (1978), known as Correlated Random Effects Probit model (CREP). It is a non-linear probability model that impose some assumptions in the specification of the unobserved factor. In this sense, it is more restrictive than a fixed effects approach but also more flexible and its assumptions more likely to be fulfilled than a random effects model. The CREP model explicitly allows the individual specific unobservable term η_i to be correlated with time variant regressors assuming a conditional normal distribution with linear expectation and constant variance. The specification assumed for η_i is:

$$\eta_i = \psi + \xi \bar{x}_i + a_i, \tag{1.6}$$

where $\bar{x}_i$ is a vector including the average of: i)daily mean agricultural wage for men, ii)head of households' health and work status and iiii)grant amount interacted with the dropout dummy[11]

The complete set of assumptions for the identification of the parameters in the enrollment decision equation are the following:

1. $e_{it}|\eta_i, P_i, T_i, D_i, grant_{kit}, C_{kit}, X_{jit} \sim \Phi \qquad \forall i, \forall t, \forall k, \forall j$
where Φ stands for the standard normal distribution

2. $S_{i1}...S_{iT}$ are independent conditional on $\eta_i, P_i, T_i, D_i, grant_{kit}, C_{kit}, X_{jit} \qquad \forall i, \forall t, \forall k, \forall j$

[11] This correlation is only allowed between time variant variables and the fixed effect. A potential source of bias in this model is the existence of correlation between the unobserved term and some time constant variables. In the model presented so far the dummy D_i may be correlated with η_i, since some unobserved factor could have determined the decision of dropping out of school. I considered this fact in the model including the grant amount multiplied by the dropout dummy in the vector $\bar{x}_i$.

3. $\eta_i | P_i, T_i, D_i, grant_{kit}, C_{kit}, X_{jit} \sim N(\psi + \bar{x}_i \xi, \sigma_a^2)$

To compute the average values of estimated marginal effects across treated individuals with the parameter's estimates obtained with the CREP model I proceed as follows.
Let Z_{kit} be the vector of explanatory variables:

$\overline{Z}_{kit} \equiv \left(1, \bar{x}_i, \overline{grant}_k, \overline{grant}_k * D_i, X_{1it}...X_{Jit}\right)'$

for each $k = 2...8$.
Let Z^0_{kit} be the same vector with the only difference that $grant_{ki} = 0$, for all individuals in all time periods:

$Z^0_{kit} \equiv (1, \bar{x}_i, 0, 0, X_{1it}...X_{Jit})'$

Let $\hat{\pi}_k$ be the vector of estimated parameters.
The average values of estimated marginal effects across treated individuals are calculated using the following expression:

$$\sum_{i:\ grant_i > 0} \sum_{t=2}^{4} [\Phi(\hat{\pi}_k * \overline{Z}_{kit}) - \Phi(\hat{\pi}_k * Z^0_{kit})], \tag{1.7}$$

where Φ stands for the normal distribution function. I compute the average of the change in enrollment probabilities due to the implementation of the grant for children in conditions of receiving a grant (treated individuals). These averages are obtained for non-dropouts (i with $D_i = 0$) and dropouts (i with $D_i = 1$) separately.

1.5.3 Results

Table 1.4 presents the estimates of the effect of PROGRESA grants on school attendance for non-dropout and dropout children, by gender and education level, obtained with several estimation strategies. Notice that the results are reported grouping individuals according to the years of schooling completed as follows: from 2 to 5 years, corresponds to children in condition of attending primary school; a child that completed 6 years of schooling has graduated from primary school, and then is allowed to enter in secondary school; 7 and more years of schooling completed corresponds to children in condition of attending junior or secondary school. The last column of the table, column (11), shows the mean amount of the grant received by beneficiaries in the second semester of 1998 when they received the aid for the first time. These means are an approximation of the exact value used in the estimations, since the amount of the grant varies with the grade the child is attending and it was updated every semester to account for the increase in the level of general prices.

1.5.3.1 CREP model estimates: Main results

Columns (8) and (9) in Table 1.4 show average treatment effects on the treated computed using CREP model estimates of the parameters in the enrollment equation (equation 1.5). A complete report of these parameter's estimates can be found in Table 1.12 in the Appendix. Consider non-dropout girls that have completed 6 years of schooling and are receiving the grant. The average probability of enrollment for a girl in this group is 5.3% higher when she is receiving the mean grant compared to when she is not receiving it. The probability of being enrolled is 5.3% higher due to the grant. With this kind of interpretation in mind, we can derive several conclusions.

In general the grant effect is positive. In four cases the effect is negative but insignificant due to huge standard errors, so the effect of interest in those cases is not clearly identified (as it happens with girls in primary school). The impact of the program is higher in secondary school than in primary school. This is an expected result because grants in secondary school are more than twice the amount of grants in primary school. Additionally, since enrollment rates are lower in the secondary level of education the program has more scope to work at this level.

The average effect of the grant for girls is higher than for boys in the non-dropout group. This is due to the fact that girls in secondary school are receiving higher grants than boys. Surprisingly, the same is not true in the group of dropouts. Drop-out boys react more strongly to the grant, even though they receive less money than girls.

The effects are different when we compare dropouts with non-dropouts. Since the standard errors of the estimated effects for dropouts in primary school are quite high, I do not made conclusions on these groups and, in what follows, all comments refers to secondary school children. For those children that have to enter in secondary school (6 years of schooling completed) the results are conclusive enough. There is no effect of grants in the re-enrolment decision of dropouts girls while for non-dropouts grants increase their enrolment probability by more than 5%. Drop-out boys react more to the incentive given by grants than non-dropouts. After receiving the grant the enrolment probability of both groups increase, but for dropouts this increase is almost 10% higher than for non-dropouts.

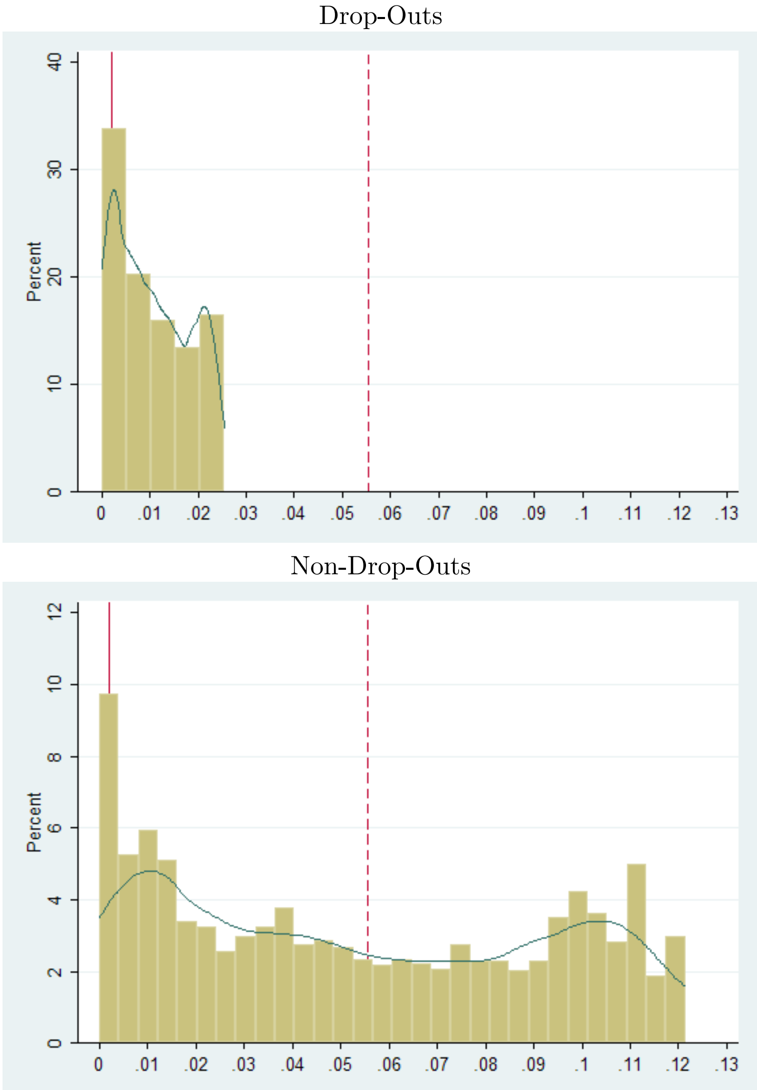
Table 1.4: Comparison of estimated marginal effects (percentage points)

Years of schooling completed			Difference Estimation			Regression Models with covariates							All Models
						Without unobserved heterogeneity Probit		With unobserved heterogeneity: Fixed effects Linear Probability Model		With unobserved heterogeneity: Correlated random effects Probit Model			Mean Grant in July 98' (mexican $)
			Marginal Effect	Standard Error	Sample Size	Marginal Effect	Standard Error	Marginal Effect	Standard Error	Marginal Effect	Standard Error	Sample Size	
			(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
from 2 to 5	Female	Non-dropout	0.022***	(0.003)	14,435	0.009***	(0.003)	0.001	(0.010)	0.009***	(0.003)	8,118	108.8
		Dropout	0.022	(0.036)	724	-0.114***	(0.028)	-0.271**	(0.115)	-0.052	(0.115)	352	
		Difference	0.000	(0.015)		0.123***	(0.028)	0.272**	(0.115)	0.061	(0.115)		
	Male	Non-dropout	0.020***	(0.003)	15,725	0.002	(0.002)	0.015	(0.010)	0.002	(0.002)	8,893	108.8
		Dropout	0.152***	(0.032)	867	-0.021	(0.027)	0.046	(0.095)	0.107	(0.081)	474	
		Difference	-0.132***	(0.014)		0.023	(0.027)	-0.030	(0.096)	-0.106	(0.081)		
6	Female	Non-dropout	0.122***	(0.016)	3,567	0.055***	(0.014)	0.024	(0.020)	0.053***	(0.014)	1,889	209.2
		Dropout	0.126***	(0.020)	1,770	0.039	(0.025)	-0.286**	(0.140)	0.010	(0.112)	956	
		Difference	-0.004	(0.026)		0.015	(0.028)	0.310**	(0.141)	0.043	(0.112)		
	Male	Non-dropout	0.077***	(0.015)	3,760	0.019	(0.012)	0.015	(0.020)	0.019	(0.011)	2,050	199.2
		Dropout	0.046*	(0.024)	1,164	-0.039	(0.029)	0.055	(0.108)	0.114**	(0.058)	688	
		Difference	0.031	(0.030)		0.058*	(0.031)	-0.040	(0.109)	-0.095	(0.059)		
7 or more	Female	Non-dropout	0.024**	(0.008)	2,701	0.015**	(0.006)	0.011	(0.023)	0.016***	(0.006)	1,455	244.2
		Dropout	0.214***	(0.074)	188	-0.023	(0.048)	-0.331**	(0.145)	-0.196***	(0.068)	127	
		Difference	-0.190***	(0.036)		0.038	(0.047)	0.343**	(0.144)	0.212***	(0.067)		
	Male	Non-dropout	0.014	(0.009)	3,347	-0.002	(0.007)	0.010	(0.024)	-0.001	(0.007)	1,868	214.2
		Dropout	-0.017	(0.077)	166	0.011	(0.043)	0.026	(0.105)	-0.022	(0.115)	113	
		Difference	0.031	(0.043)		-0.013	(0.043)	-0.016	(0.106)	0.021	(0.115)		

Standard errors for difference estimates obtained by performing test of difference of proportions. Standard errors for regression models computed by bootstrap with 1000 replications. Cluster set at family level. Significance levels: $***: 1\%$ $**: 5\%$ $*: 10\%$.
The set of covariates used in the regression models include child's information (age and education level), family characteristics (head of houselhold's health and working status, mother's education level, family composition and proportion of children attending school), and village characteristics (mean wage salary, distance to the main city at municipality level and to the nearest metropolitan area, and distance to the nearest village with a secondary school available). A detailed report of covariates can be seen in Table 1.11 in the Appendix.

Even though the equality of average marginal effects between dropouts and non-dropouts cannot be rejected at standard levels of significance the distribution of marginal effects between both groups show notable differences. Moreover, p-values from kolmogorov-smirnov tests of equality of distributions of marginal effects between dropouts and non-dropouts are always bellow 0.001, so the null hypothesis of equality is in all cases rejected even at 1% of significance. Figures 1.2 and 1.3 below depict marginal effects distributions stressing this conclusion.

Figure 1.2: Marginal effects distribution: female primary school completed (grade 6 completed)



- - - (dashed line) Average marginal effect for non-dropouts
—— (continuous line) Average marginal effect for dropouts

For girls that are about to enter in secondary school, the average impact is 5% for non-dropouts while the corresponding figure for dropouts is only 1% and statistically zero. Main characteristics of the distributions are a smaller range for dropouts, from 0.0 to 0.025, and for non-dropouts a high dispersion with almost all frequencies below 5%. Thus, for girls who have to decide whether to enter in secondary school grants are more convincing for those who were at school before the implementation of the program. PROGRESA education grants are not a strong enough incentive to persuade dropout girls to start secondary school. Notice that for this group the conclusion does not support the initial hypothesis that was made simply considering observable characteristics.

Turning to boys that have completed 6^{th} grade, there is an important and statistically significant difference in average marginal effects, more than 9% higher for dropouts (2% for non-dropouts, 11% for dropouts). For non-dropouts the distribution has a small range, from 0 to 0.05 with the highest frequency, more than 30%, between 0 and 0.002. The distribution for dropouts, on the other hand, is highly dispersed between 0 and 0.37 with frequencies in general below 5%. 25, 50 and 75 percentiles are all higher for dropouts. The message is clear. The program has a stronger effect on boys about to start secondary school who dropped-out in 1997 or before than on those who stayed on school.

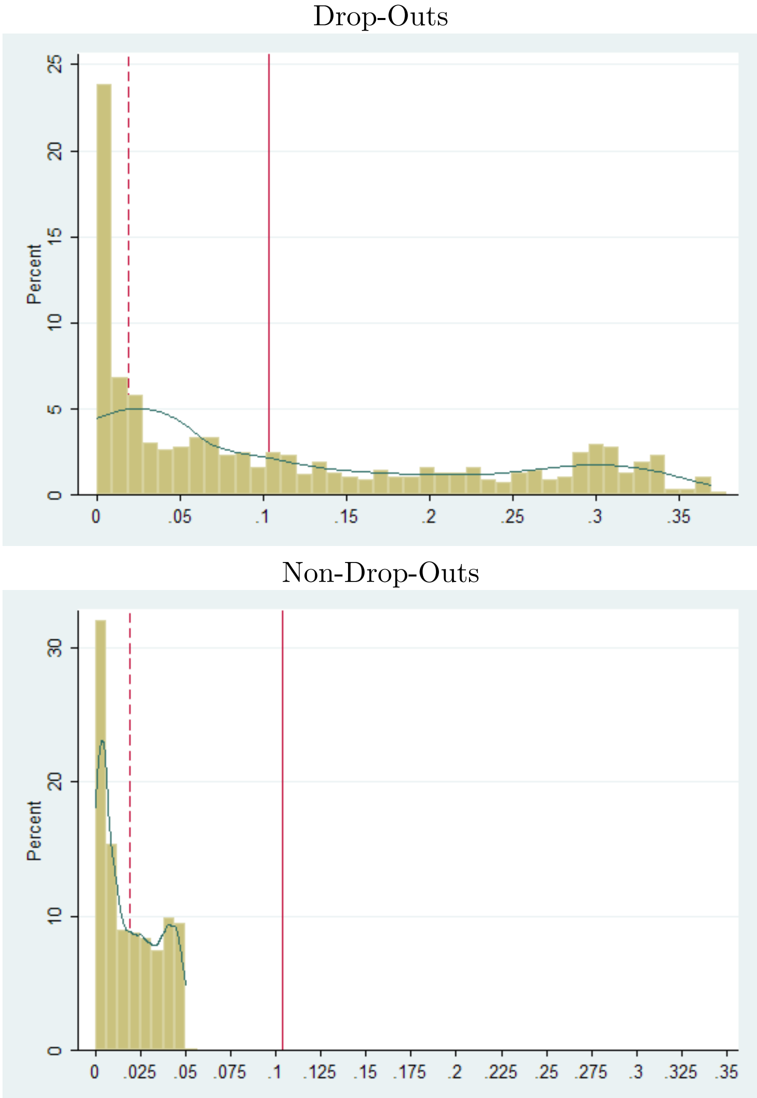
Summing up, the estimated results presented above allow us to conclude that there exists a differential effect of the grant between children that dropped out of school before the program started and those who did not. The direction of differential effects in secondary school is not uniform for girls and boys. Drop-outs boys react more strongly to the program's incentives than non-dropouts. Non-dropouts girls have stronger grants' effects than dropouts. Therefore, I found the expected result of a higher effect of the grants on dropouts in secondary school only for boys. In general the program is effective for children that dropped out of school before they started receiving any grants. But when they have to decide to enter secondary school PROGRESA grants provide a better incentive for boys than for girls.

1.5.3.2 Estimates with alternative models

First, I report results of difference estimation. Columns (1), (2), and (3) in Table 1.4 show the average increase in school attendance due by PROGRESA grants, its estimated standard error and the number of observations used to compute the averages, respectively. The main difference with the results of the CREP model appears for children that have finished 6 years of education. The difference estimates show no differential effect of the grant between non-dropout and dropout girls, and a higher effect of the grant for non-dropout boys than for dropout boys. As I discuss in Section 1.5.1 it is highly likely that these results are biased estimates of the true effect, since they do not control for differences in observable and unobservable characteristics between treated and control individuals.

Second, in columns (4) and (5) I present results of a probit model that controls for observed differences between treated and non-treated individuals, but still assumes that there

Figure 1.3: Marginal effects distribution: male primary school completed (grade 6 completed)



- - - (dashed line) Average marginal effect for non-dropouts

—— (continuous line) Average marginal effect for dropouts are no differences in time invariant unobservables. With this model we would conclude that PROGRESA grants do not affect the probability of attending secondary school for dropout boys. However, it is difficult to accept estimates of the parameters in an equation reflecting school attendance without controlling for the existence of differences in ability. At a minimum, it is necessary to test if the estimated parameters do not change with the introduction of unobserved heterogeneity in the model.

Third, columns (6) and (7) report the average treatment effects on the treated and their corresponding standard errors estimated using a FELPM. The results are qualitatively consistent with those obtained with the CREP model. This provide evidence that the assumptions related with the unobserved heterogeneity in the CREP model are fulfilled, so the consistency of the estimates is validated. A report of the parameter estimates obtained with this model can be seen in Table in the Appendix.

1.5.3.3 Exploring the reasons behind the differential effect of PROGRESA grants

Ideally, one would want to estimate the effect of the grant for non-dropout and dropout children interacted with several observed characteristics to disentangle the reasons for the lack of effectiveness of PROGRESA grants in sending dropout girls back to secondary school. Unfortunately, the limited number of dropout observations in the sample forbids us to perform such exercise.

Still, I can exploit the nonlinear feature of the estimated marginal effects by computing the grant effect at specific values of observable characteristics. These results are reported in Table 1.13.

I will concentrate the analysis on the differential effect of the grant between non-dropouts and dropout children in condition of starting secondary school. Non-dropout children that have a numerous family (high number of siblings regardless of their gender and age) are more responsive the the grant, while this family characteristic reduces the effect of the grant for dropout children. In the case of girls, this finding may be rationalized in terms of their outside option. Only 12% of dropout girls who do not attend school report to be working. It is natural to thing that they may be staying at home helping in housework. Having a more numerous family makes them more necessary at home so they are less likely to go back to school.

The characteristics analyzed so far does not seem to help in explaining the differential effect of the grant between dropout girls and dropout boys.

Table 1.5: Average treatment effects evaluated at specific values of observable characteristics: children with 6 years of schooling completed

		Father living at home (yes minus no)		Mother's schooling (6 minus 2 years)		Distance to secondary school (3 km. minus 0 km.)	
		Effect	S.E.	Effect	S.E.	Effect	S.E.
Female	Non-dropout	-0.009***	(0.003)	-0.003**	(0.001)	0.003***	(0.001)
	Dropout	0.002	(0.021)	-0.001	(0.008)	-0.001	(0.006)
Male	Non-dropout	-0.005*	(0.003)	0.000	(0.000)	0.001	(0.001)
	Dropout	0.040*	(0.022)	-0.013	(0.009)	-0.010*	(0.006)

		Number of girls aged 6 or more (4 minus 0)		Number of boys aged 6 or more (4 minus 0)		Number of children aged 5 or less (4 minus 0)		Number of children attending school (5 minus 2)	
		Effect	S.E.	Effect	S.E.	Effect	S.E.	Effect	S.E.
Female	Non-dropout	0.032***	(0.008)	0.017***	(0.005)	0.005**	(0.002)	-0.044***	(0.011)
	Dropout	-0.007	(0.065)	-0.008	(0.079)	-0.001	(0.010)	0.000	(0.051)
Male	Non-dropout	0.007*	(0.004)	0.014*	(0.009)	0.002	(0.001)	-0.018*	(0.011)
	Dropout	-0.140*	(0.077)	-0.117	(0.076)	-0.016	(0.010)	0.055	(0.066)

Standard errors computed by bootstrap with 1000 replications. Cluster set at family level.
Significance levels: $***: 1\%$ $**: 5\%$ $*: 10\%$.

1.6 Conclusions

I found evidence of the existence of a differential effect of PROGRESA grants on the school attendance decision of the overall target population and on the reentry decision of children who dropped out from school. These difference are observed in all groups analyzed. But the direction of the difference varies across groups. The expected result of a higher program effect on dropouts was found for boys in conditions of attending primary or secondary school. For girls in secondary school who dropped out in 1997 or before, the grant is not as good incentive to enroll in school as it is for non-dropout girls. Among dropouts in secondary school the impact of the education grants is lower for girls even though they receive more money than boys. The different responses of girls and boys to the grant in secondary school should be studied in more detail.

The last finding motivates the design of a particular model of schooling decision for girls. Individual variables such as marital state, pregnancy and number of children should be considered. Moreover, it can be argued that girls face a third option other than schooling or working. They may stay at home and take care of the children in the family. A model of schooling decisions should reflect this third option for girls. Additionally, there exist some perception that in the poorest rural communities girls are discriminated, so a monetary incentive like the PROGRESA grants may not be effective.

At a methodological level, the estimation of PROGRESA effects and the differential impact over dropouts can be improved by constructing a structural model of schooling. With the design of such a model, we might obtained a more conclusive answer to the question "Did PROGRESA send dropouts back to school?". Moreover, the estimation of a structural model will allow for the identification of a more effective and efficient policy that can send dropouts back to school.

1.7 Appendix

Table 1.2 *(continued)*

Variable	Primary		Secondary	
	Female	Male	Female	Male
Years of schooling completed				
0	0.000	0.003		
1	0.170	0.150		
2	0.114	0.117		
3	0.439	0.401		
4	0.113	0.154		
5	0.164	0.176		
6			0.863	0.816
7			0.071	0.072
8			0.019	0.039
9 or more			0.047	0.073
Age of child				
8	0.070	0.070		
9	0.115	0.090		
10	0.091	0.086		
11	0.092	0.089	0.001	0.000
12	0.106	0.097	0.015	0.015
13	0.125	0.097	0.096	0.087
14	0.127	0.133	0.253	0.209
15	0.134	0.149	0.310	0.306
16	0.111	0.144	0.247	0.288
17	0.029	0.045	0.075	0.093
18	0.002	0.001	0.004	0.002

Table 1.6: Descriptive statistics for Non-Drop-outs (post-program surveys)

Variable	Primary		Secondary	
	Female	Male	Female	Male
Sample size	25,564	23,521	8,659	9,735
Enrollment rate	0.976	0.971	0.767	0.790
Percentage of treatment communities	60.79	62.20	60.85	62.10
Percentage of children belonging to a poor family	89.44	89.85	80.51	81.87
Percentage of children eligible for receiving a grant	31.8	30.8	38.6	40.2
Grant (for grant different from zero) (pesos)	118.2	118.2	265.2	245.8
	(29.7)	(29.8)	(28.1)	(20.4)
Mother's schooling (years)	3.0	3.0	2.9	2.9
	(2.6)	(2.6)	(2.5)	(2.5)
Percentage of children with head of household ill	5.96	6.26	7.10	7.14
Percentage of children with head of household employed	91.80	91.60	90.03	89.51
Percentage of children with father not living at home	9.83	10.11	10.50	11.07
Number of girls from 5 to 16	2.0	1.0	1.9	0.9
	(1.2)	(1.0)	(1.1)	(1.0)
Number of boys from 5 to 16	1.0	1.9	1.0	1.9
	(1.0)	(1.1)	(1.0)	(1.1)
Number of children under 5	0.9	0.9	0.5	0.5
	(1.0)	(1.0)	(0.8)	(0.8)
Number of adult women	1.5	1.5	1.8	1.6
	(0.8)	(0.9)	(1.0)	(0.9)
Number of adult men	1.5	1.5	1.7	1.8
	(0.9)	(0.9)	(1.0)	(1.0)
Number of siblings enrolled at school	2.9	2.7	2.8	2.7
	(1.3)	(1.4)	(1.3)	(1.4)
Distance to secondary school (km)	2.3	2.3	1.9	2.0
	(2.1)	(2.1)	(1.8)	(1.8)
Percentage of children that have a secondary school in their community	23.2	26.5	32.6	28.3
Distance to nearest metropolitan area (km)	148.7	150.4	152.3	154.1
	(76.8)	(77.0)	(77.4)	(77.5)
Distance to the main city of her/his municipality (km)	11.7	11.6	11.8	11.5
	(8.1)	(7.9)	(8.2)	(8.0)
Community daily agricultural wage (pesos)	30.6	30.5	31.8	31.2
	(10.6)	(10.5)	(10.8)	(10.4)
Years of schooling completed				
0	0.000	0.000		
1	0.200	0.207		
2	0.181	0.184		
3	0.279	0.271		
4	0.177	0.172		
5	0.164	0.166		
6			0.489	0.455
7			0.231	0.245
8			0.175	0.190
9 or more			0.105	0.110
Age of child				
6	0.073	0.069		
7	0.146	0.136		
8	0.155	0.151		
9	0.155	0.152		
10	0.151	0.148		
11	0.151	0.142	0.023	0.021
12	0.092	0.098	0.181	0.139
13	0.042	0.056	0.255	0.224
14	0.022	0.028	0.232	0.247
15	0.008	0.013	0.182	0.210
16	0.003	0.005	0.103	0.127
17	0.000	0.001	0.022	0.030
18	0.000	0.000	0.002	0.001

Standard deviations are in parenthesis

Table 1.7: Comparison with Schultz (2004) confidence intervals for post-program difference estimation

Years of schooling completed		This paper 95% Conf. Interval		Schultz (2004)[a] 95% Conf. Interval	
2	Female	0.0144	0.0352	-0.0111	0.0471
	Male	0.0054	0.0217	0.0209	0.0210
3	Female	-0.0026	0.0155	-0.0122	0.0382
	Male	0.0061	0.0274	0.0489	0.0490
4	Female	0.0035	0.0267	0.0279	0.0481
	Male	0.0108	0.0332	0.0439	0.0440
5	Female	0.0287	0.0582	0.0457	0.0643
	Male	0.0101	0.0407	0.0409	0.0410
6	Female	0.0910	0.1521	0.1479	0.1480
	Male	0.0481	0.1054	0.0531	0.0769

[a]The results are taken from Table 3 in Schultz (2004).

Table 1.8: Groups size

Years of schooling completed in previous year	Non-dropouts				Drop-outs			
	Female		Male		Female		Male	
	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
From 2 to 5	8,118	6,317	8,893	6,832	352	372	474	393
6	1,889	1,678	2,050	1,710	956	814	688	476
7 or more	1,455	1,246	1,868	1,479	127	61	113	53

Table 1.9: p-values for test of randomization - Drop-out Observations

Variable	p-value (2,025 obs)	p-value based on community mean (440 obs)
Community population distribution	–	0.101 (3)
Distribution of communities over states	–	0.897 (3)
Age distribution of children under 16	0.892 (3)	0.925 (4)
Child's stock of education	0.107 (3)	0.340 (4)
Number of girls between 5 and 16 in the family	0.440 (3)	0.756 (4)
Number of boys between 5 and 16 in the family	0.426 (3)	0.336 (4)
Number of children under 5 in the family	0.099 (3)	0.856 (4)
Number of adult women in the family	0.002 (3)	0.456 (4)
Number of adult men in the family	0.180 (3)	0.720 (4)
Mother's schooling	0.000 (3)	0.955 (4)
Percentage of children with father not living at home	0.008 (2)	0.120 (4)
Percentage of children with head of household employed	0.019 (2)	0.321 (4)
Number of siblings enrolled at school	0.765 (1)	0.474 (4)
Community daily agricultural wage	0.000 (1)	0.914 (4)
Distance to metropolitan area	0.000 (1)	0.964 (4)
Distance to the main city of her/his municipality	0.001 (1)	0.742 (4)
Distance to secondary school	0.001 (1)	0.799 (4)

(1)Kolmogorov-Smirnov statistic for test of equality between two distribution functions. Ho: the distribution of the variable analyzed is equal in both groups
(2)T-test for equality of proportions. Ho: the variable analyzed has the same proportion of ones in both groups.
(3)Pearson's chi-squared statistic for the hypothesis that the frequencies in a two-way tabular are independent. Ho: the frequencies of the variable analyzed are independent.
(4)T-test for equality of means. Ho: the variable analyzed has the same mean in both groups.

Conclusion: At individual level several variables are different when comparing treatment and controls. At community level there exist statistical differences in a couple of variables. Hence, the random assignment of the program is lost when considering the group of dropouts separately.

Table 1.10: p-values for test of randomization - Non-dropout Observations

Variable	p-value (19,649 obs)	p-value based on community mean (492 obs)
Community population distribution	–	0.119 (3)
Distribution of communities over states	–	0.781 (3)
Age distribution of children under 16	0.230 (3)	0.925 (4)
Child's stock of education	0.009 (3)	0.369 (4)
Number of girls between 5 and 16 in the family	0.001 (3)	0.789 (4)
Number of boys between 5 and 16 in the family	0.000 (3)	0.415 (4)
Number of children under 5 in the family	0.000 (3)	0.295 (4)
Number of adult women in the family	0.000 (3)	0.701 (4)
Number of adult men in the family	0.007 (3)	0.525 (4)
Mother's schooling	0.002 (3)	0.930 (4)
Percentage of children with father not living at home	0.266 (2)	0.794 (4)
Percentage of children with head of household employed	0.016 (2)	0.324 (4)
Number of siblings enrolled at school	0.000 (1)	0.957 (4)
Community daily agricultural wage	0.000 (1)	0.997 (4)
Distance to metropolitan area	0.000 (1)	0.989 (4)
Distance to the main city of her/his municipality	0.000 (1)	0.414 (4)
Distance to secondary school	0.000 (1)	0.615 (4)

(1), (2), (3) and (4) idem Table 1.9.

Conclusion: There exist even more relevant differences at the individual level than those presented in Table 1.9. The evidence of lack of randomization is stronger when considering the group of non-dropouts separately.

Table 1.11: Description of variables in Tables 1.12 and 1.13

Variable name	Description
P	One if the child belongs to a poor family
T	One if the child resides in a community where PROGRESA grants were implemented in October 1998
D	One if the child was not enrolled in school in the October 1997 census
DP	Interaction between D (dropout) and P (poor family)
DT	Interaction between D (dropout) and T (treatment community)
grant	Grant amount (pesos)
grantd	Grant amount interacted with D (dropout child) (pesos)
wrepeon	Community daily agricultural wage for men (pesos)
health	One if the head of household was ill in the four weeks previous to the survey
work	One if the head of household has a job in the week previous to the survey
fhogar	One if the child's father is living at home with his family
distsec	Distance from the community where the child resides to the nearest community with a secondary school (km)
distmetro	Distance from the community where the child resides to the nearest metropolitan area. For communities in Hidalgo(state), these are Queretaro, Puebla, Tampico, or Mexico City; in Michoacan(state) it is Morelia; in Puebla it is Puebla; in Queretaro it is Queretaro; in San Luis de Potosi it is San Luis de Potosi; in Veracruz it is Veracruz and in Guerrero it is Acapulco (km)
distcab	Distance from the community where the child resides to the main city of her/his municipality (km)
schoolm	Years of schooling completed by the child's mother
girl	Number of girls from 5 to 16 years old in the child's family
boy	Number of boys from 5 to 16 years old in the child's family
baby	Number of children aged less than 5 years old in the child's family
women	Number of adult women (aged more than 16) in the child's family
men	Number of adult men (aged more than 16) in the child's family
w3	One for observations in the first post-program survey collected in October 1998
w4	One for observations in the second post-program survey collected in May 1999
age	Age of the child
age2	Square of the age of the child
ck	One if the child has completed k years of education (k = 1 or less, 2,..., 9 or more)
ageck	Age interacted with the stock of education of the child
asistest	Number of child's siblings enrolled in school
distsecd	Distance to secondary school interacted with dropout dummy D
distcabd	Distance to main city at municipality level interacted with dropout dummy D
workd	Variable work (head of household working status) interacted with dropout dummy D
schoolmd	Mother's stock of education interacted with dropout dummy D
workm	Time average for variable work (3 post-program observations)
healthm	Time average for variable health (3 post-program observations)
wrepeonm	Time average for variable wrepeon (3 post-program observations)
grantdm	Time average for variable grantd (3 post-program observations)
X_{fp}	Variable X interacted with a dummy variable equal 1 for girls in primary school
X_{fs}	Variable X interacted with a dummy variable equal 1 for girls in secondary school
X_{mp}	Variable X interacted with a dummy variable equal 1 for boys in primary school
X_{ms}	Variable X interacted with a dummy variable equal 1 for boys in secondary school
	X stands for the complete set of variables described above

Table 1.12: Probit estimates of enrollment probabilities[a]

			Number of obs =		74,427	
			Wald $\chi^2(166)$ =		7,636.54	
Log-pseudolikelihood =	-9565.1793		Prob > χ^2 =		0.0000	
			Pseudo R^2 =		0.6688	
Percentage correctly predicted = 95.34%						

enrolled	Coefficient	Standard Error	z	$P > \|z\|$	95% Conf.	Interval
P_{fp}	-0.06012	0.12335	-0.49	0.626	-0.30188	0.18164
T_{fp}	0.036389	0.096795	0.38	0.707	-0.15333	0.226104
D_{fp}	-1.78776	0.439293	-4.07	0.000	-2.64876	-0.92676
DP_{fp}	0.539533	0.345394	1.56	0.118	-0.13743	1.216493
DT_{fp}	0.233085	0.198013	1.18	0.239	-0.15501	0.621183
$grant_{fp}$	0.002198	0.000881	2.5	0.013	0.000471	0.003925
$grantd_{fp}$	-0.00452	0.003856	-1.17	0.242	-0.01207	0.003042
age_{fp}	0.329558	0.160167	2.06	0.04	0.015638	0.643479
$age2_{fp}$	-0.02601	0.007755	-3.35	0.001	-0.04121	-0.01081
$c2_{fp}$	2.228879	0.497638	4.48	0.000	1.253527	3.204232
$c3_{fp}$	1.676924	0.655976	2.56	0.011	0.391236	2.962613
$c4_{fp}$	1.73503	0.744903	2.33	0.02	0.275047	3.195013
$c5_{fp}$	1.829776	0.769984	2.38	0.017	0.320634	3.338917
$agec2_{fp}$	-0.22695	0.049548	-4.58	0.000	-0.32407	-0.12984
$agec3_{fp}$	-0.16305	0.057249	-2.85	0.004	-0.27525	-0.05084
$agec4_{fp}$	-0.14192	0.063374	-2.24	0.025	-0.26613	-0.01771
$agec5_{fp}$	-0.14879	0.06407	-2.32	0.02	-0.27436	-0.02322
$wrepeon_{fp}$	-0.00322	0.003184	-1.01	0.312	-0.00946	0.003019
$distsec_{fp}$	0.028863	0.016966	1.7	0.089	-0.00439	0.062116
$distcab_{fp}$	0.001253	0.00405	0.31	0.757	-0.00668	0.00919
$distmetro_{fp}$	0.001298	0.000486	2.67	0.008	0.000346	0.00225
$schoolm_{fp}$	-0.00521	0.014298	-0.36	0.715	-0.03324	0.02281
$health_{fp}$	-0.21307	0.083881	-2.54	0.011	-0.37747	-0.04867
$work_{fp}$	-0.09014	0.113698	-0.79	0.428	-0.31298	0.132706
$fhogar_{fp}$	0.40628	0.137486	2.96	0.003	0.136812	0.675747
$girl_{fp}$	-0.44548	0.048201	-9.24	0.000	-0.53995	-0.35101
boy_{fp}	-0.53084	0.04877	-10.88	0.000	-0.62642	-0.43525
$baby_{fp}$	-0.00321	0.034287	-0.09	0.925	-0.07041	0.063991
$women_{fp}$	0.005986	0.047786	0.13	0.9	-0.08767	0.099645
men_{fp}	0.024404	0.038311	0.64	0.524	-0.05068	0.099491
$asistest_{fp}$	1.505614	0.059769	25.19	0.000	1.38847	1.622758
$distsecd_{fp}$	-0.05231	0.03061	-1.71	0.087	-0.1123	0.007689
$distcabd_{fp}$	0.019577	0.009987	1.96	0.05	2.31E-06	0.039151
$workd_{fp}$	0.535823	0.254472	2.11	0.035	0.037067	1.034579
$grantdm_{fp}$	-0.00262	0.004232	-0.62	0.536	-0.01091	0.005676
$wrepeonm_{fp}$	0.006664	0.004662	1.43	0.153	-0.00247	0.015802
$workm_{fp}$	0.28682	0.195492	1.47	0.142	-0.09634	0.669977
$healthm_{fp}$	0.521563	0.215596	2.42	0.016	0.099003	0.944122
$workdm_{fp}$	-0.35971	0.353035	-1.02	0.308	-1.05165	0.332224
$asistemst_{fp}$	-0.66512	0.053696	-12.39	0.000	-0.77037	-0.55988
$w3_{fp}$	0.093972	0.049281	1.91	0.057	-0.00262	0.19056
$w4_{fp}$	-0.05902	0.044302	-1.33	0.183	-0.14585	0.027812
$cons$	0.17941	0.851053	0.21	0.833	-1.48862	1.847444

(continued on next page)

[a] Variable's definition are explained in Table 1.11.

Table 1.12*(continued)*

enrolled	Coefficient	Standard Error	z	$P > \|z\|$	95% Conf.	Interval
P_{fs}	-0.33882	0.095044	-3.56	0.000	-0.5251	-0.15254
T_{fs}	0.120591	0.088996	1.36	0.175	-0.05384	0.29502
D_{fs}	8.186298	5.750446	1.42	0.155	-3.08437	19.45696
DP_{fs}	0.075007	0.202451	0.37	0.711	-0.32179	0.471803
DT_{fs}	-0.0103	0.197861	-0.05	0.958	-0.3981	0.377501
$grantc6_{fs}$	0.001144	0.000435	2.63	0.009	0.000292	0.001997
$grantdc6_{fs}$	-0.0009	0.002183	-0.41	0.679	-0.00518	0.003375
$grantc7_{fs}$	0.001022	0.000473	2.16	0.031	9.41E-05	0.00195
$grantdc7_{fs}$	-0.00545	0.001853	-2.94	0.003	-0.00908	-0.00182
age_{fs}	-0.71461	0.461384	-1.55	0.121	-1.61891	0.189682
$age2_{fs}$	0.016453	0.014813	1.11	0.267	-0.01258	0.045486
$c6_{fs}$	1.629472	1.315147	1.24	0.215	-0.94817	4.207113
$c7_{fs}$	5.006646	1.351812	3.7	0.000	2.357142	7.656149
$agec6_{fs}$	-0.11693	0.086412	-1.35	0.176	-0.28629	0.052434
$agec7_{fs}$	-0.24183	0.088076	-2.75	0.006	-0.41445	-0.0692
$wrepeon_{fs}$	0.004344	0.00267	1.63	0.104	-0.00089	0.009577
$distsec_{fs}$	-0.05119	0.013887	-3.69	0.000	-0.07841	-0.02397
$distmetro_{fs}$	0.002178	0.000368	5.91	0.000	0.001456	0.0029
$distcab_{fs}$	0.000799	0.003078	0.26	0.795	-0.00523	0.006831
$schoolm_{fs}$	0.032229	0.013253	2.43	0.015	0.006254	0.058204
$health_{fs}$	-0.15281	0.065867	-2.32	0.02	-0.28191	-0.02372
$work_{fs}$	0.008876	0.079652	0.11	0.911	-0.14724	0.164991
$fhogar_{fs}$	0.512864	0.123557	4.15	0.000	0.270697	0.755031
$girl_{fs}$	-0.64605	0.05665	-11.4	0.000	-0.75708	-0.53501
boy_{fs}	-0.69022	0.057308	-12.04	0.000	-0.80254	-0.5779
$baby_{fs}$	-0.06231	0.028582	-2.18	0.029	-0.11833	-0.0063
$women_{fs}$	-0.00992	0.027727	-0.36	0.72	-0.06427	0.04442
men_{fs}	-0.0307	0.024896	-1.23	0.217	-0.0795	0.018092
$asistest_{fs}$	1.651837	0.065212	25.33	0.000	1.524023	1.779651
$aged_{fs}$	-1.2938	0.793451	-1.63	0.103	-2.84893	0.261335
$age2d_{fs}$	0.046772	0.027289	1.71	0.087	-0.00671	0.100257
$schoolmd_{fs}$	-0.07881	0.031583	-2.5	0.013	-0.14071	-0.01691
$grantdc6m_{fs}$	0.000432	0.002275	0.19	0.849	-0.00403	0.004891
$grantdc7m_{fs}$	0.008237	0.002846	2.89	0.004	0.002658	0.013815
$wrepeonm_{fs}$	-0.00456	0.003939	-1.16	0.247	-0.01228	0.00316
$workm_{fs}$	0.286502	0.175365	1.63	0.102	-0.05721	0.630211
$healthm_{fs}$	0.244266	0.215016	1.14	0.256	-0.17716	0.66569
$asistemst_{fs}$	-0.67138	0.047703	-14.07	0.000	-0.76488	-0.57789
$w3_{fs}$	0.378933	0.049531	7.65	0.000	0.281854	0.476012
$w4_{fs}$	0.35171	0.042589	8.26	0.000	0.268237	0.435182
fs	5.345003	3.769465	1.42	0.156	-2.04301	12.73302

(continued on next page)

Table 1.12*(continued)*

enrolled	Coefficient	Standard Error	z	$P > \|z\|$	95% Conf.	Interval
P_{mp}	0.0019842	0.1115879	0.02	0.986	-0.216724	0.2206924
T_{mp}	0.1787204	0.0759595	2.35	0.019	0.029843	0.3275982
D_{mp}	3.9781890	2.4427500	1.63	0.103	-0.809512	8.7658900
DP_{mp}	-0.2495031	0.2543473	-0.98	0.327	-0.748015	0.2490085
DT_{mp}	0.2368955	0.1807437	1.31	0.190	-0.117356	0.5911466
$grant_{mp}$	0.0003683	0.0007122	0.52	0.605	-0.001028	0.0017642
$grantd_{mp}$	0.0045421	0.0035240	1.29	0.197	-0.002365	0.0114490
age_{mp}	0.4517245	0.1438856	3.14	0.002	0.169714	0.7337351
$age2_{mp}$	-0.0293674	0.0068852	-4.27	0.000	-0.042862	-0.0158727
$c2_{mp}$	2.0547600	0.4078813	5.04	0.000	1.255328	2.8541930
$c3_{mp}$	2.7626040	0.5830192	4.74	0.000	1.619907	3.9053000
$c4_{mp}$	2.6819920	0.6220860	4.31	0.000	1.462726	3.9012580
$c5_{mp}$	2.1837420	0.7014966	3.11	0.002	0.808834	3.5586500
$agec2_{mp}$	-0.1899496	0.0392167	-4.84	0.000	-0.266813	-0.1130863
$agec3_{mp}$	-0.2544400	0.0485934	-5.24	0.000	-0.349681	-0.1591988
$agec4_{mp}$	-0.2101065	0.0499493	-4.21	0.000	-0.308005	-0.1122077
$agec5_{mp}$	-0.1773750	0.0550845	-3.22	0.001	-0.285339	-0.0694115
$wrepeon_{mp}$	-0.0038323	0.0033377	-1.15	0.251	-0.010374	0.0027095
$distsec_{mp}$	-0.0003181	0.0134879	-0.02	0.981	-0.026754	0.0261176
$distmetro_{mp}$	0.0025447	0.0004920	5.17	0.000	0.001580	0.0035091
$distcab_{mp}$	0.0006364	0.0038155	0.17	0.868	-0.006842	0.0081146
$schoolm_{mp}$	0.0178968	0.0132084	1.35	0.175	-0.007991	0.0437848
$health_{mp}$	0.0215211	0.1050984	0.20	0.838	-0.184468	0.2275102
$work_{mp}$	0.0301827	0.0868495	0.35	0.728	-0.140039	0.2004046
$fhogar_{mp}$	0.4758867	0.1065015	4.47	0.000	0.267148	0.6846258
$girl_{mp}$	-0.5780486	0.0397477	-14.54	0.000	-0.655953	-0.5001446
boy_{mp}	-0.4682509	0.0404441	-11.58	0.000	-0.547520	-0.3889820
$baby_{mp}$	0.0172073	0.0294409	0.58	0.559	-0.040496	0.0749103
$women_{mp}$	0.0573411	0.0347276	1.65	0.099	-0.010724	0.1254059
men_{mp}	0.0066312	0.0296029	0.22	0.823	-0.051390	0.0646518
$asistest_{mp}$	1.6161750	0.0665607	24.28	0.000	1.485718	1.7466310
$aged_{mp}$	-0.7478791	0.3750563	-1.99	0.046	-1.482976	-0.0127824
$age2d_{mp}$	0.0271769	0.0145600	1.87	0.062	-0.001360	0.0557140
$wrepeond_{mp}$	0.0129225	0.0072253	1.79	0.074	-0.001239	0.0270837
$schoolmd_{mp}$	-0.1354224	0.0340484	-3.98	0.000	-0.202156	-0.0686886
$grantdm_{mp}$	-0.0056409	0.0036899	-1.53	0.126	-0.012873	0.0015912
$wrepeonm_{mp}$	0.0077109	0.0044644	1.73	0.084	-0.001039	0.0164610
$workm_{mp}$	-0.2277633	0.1669524	-1.36	0.172	-0.554984	0.0994573
$healthm_{mp}$	0.1251161	0.2103195	0.59	0.552	-0.287103	0.5373348
$wrepeondm_{mp}$	-0.0077973	0.0093024	-0.84	0.402	-0.026030	0.0104351
$asistem_{s}t_{mp}$	-0.6956268	0.0626527	-11.10	0.000	-0.818424	-0.5728298
$w3_{mp}$	0.1979129	0.0510996	3.87	0.000	0.097760	0.2980662
$w4_{mp}$	0.0023492	0.0438664	0.05	0.957	-0.083627	0.0883257
mp	-1.0594210	1.1248610	-0.94	0.346	-3.264109	1.1452670

(continued on next page)

Table 1.12*(continued)*

enrolled	Coefficient	Standard Error	z	$P > \|z\|$	95% Conf.	Interval
P_{ms}	-0.35897	0.079264	-4.53	0.000	-0.51433	-0.20362
T_{ms}	0.119921	0.077159	1.55	0.12	-0.03131	0.27115
D_{ms}	-1.11906	0.215512	-5.19	0.000	-1.54146	-0.69667
DP_{ms}	0.354124	0.207761	1.7	0.088	-0.05308	0.761328
DT_{ms}	0.263892	0.204407	1.29	0.197	-0.13674	0.664523
$grantc6_{ms}$	0.000491	0.000407	1.21	0.227	-0.00031	0.001289
$grantdc6_{ms}$	0.003254	0.00192	1.69	0.09	-0.00051	0.007018
$grantc7_{ms}$	-5.1E-05	0.000481	-0.11	0.916	-0.00099	0.000893
$grantdc7_{ms}$	-0.00036	0.001714	-0.21	0.833	-0.00372	0.002997
age_{ms}	-0.80319	0.406263	-1.98	0.048	-1.59945	-0.00693
$age2_{ms}$	0.019289	0.012876	1.5	0.134	-0.00595	0.044525
$c6_{ms}$	2.651963	1.256475	2.11	0.035	0.189318	5.114608
$c7_{ms}$	3.685036	1.233131	2.99	0.003	1.268144	6.101929
$agec6_{ms}$	-0.17643	0.081999	-2.15	0.031	-0.33714	-0.01571
$agec7_{ms}$	-0.16052	0.079794	-2.01	0.044	-0.31691	-0.00412
$wrepeon_{ms}$	-0.00493	0.002574	-1.91	0.056	-0.00997	0.00012
$distsec_{ms}$	-0.04974	0.01343	-3.7	0.000	-0.07606	-0.02341
$distmetro_{ms}$	0.002609	0.000382	6.82	0.000	0.001859	0.003358
$distcab_{ms}$	0.000743	0.003112	0.24	0.811	-0.00536	0.006842
$schoolm_{ms}$	0.014608	0.011231	1.3	0.193	-0.0074	0.036619
$health_{ms}$	0.021708	0.08097	0.27	0.789	-0.13699	0.180406
$work_{ms}$	0.106334	0.07132	1.49	0.136	-0.03345	0.246118
$fhogar_{ms}$	0.682057	0.11782	5.79	0.000	0.451135	0.912979
$girl_{ms}$	-0.8378	0.052616	-15.92	0.000	-0.94092	-0.73467
boy_{ms}	-0.72726	0.050609	-14.37	0.000	-0.82645	-0.62807
$baby_{ms}$	-0.06182	0.027068	-2.28	0.022	-0.11487	-0.00876
$women_{ms}$	0.000452	0.029253	0.02	0.988	-0.05688	0.057786
men_{ms}	-0.05032	0.026325	-1.91	0.056	-0.10191	0.001278
$asistest_{ms}$	1.83169	0.062904	29.12	0.000	1.708399	1.95498
$schoolmd_{ms}$	-0.06732	0.032089	-2.1	0.036	-0.13021	-0.00442
$grantdc6m_{ms}$	-0.00508	0.00197	-2.58	0.01	-0.00894	-0.00121
$grantdc7m_{ms}$	0.002296	0.002065	1.11	0.266	-0.00175	0.006343
$wrepeonm_{ms}$	0.000636	0.003678	0.17	0.863	-0.00657	0.007845
$workm_{ms}$	0.056352	0.155714	0.36	0.717	-0.24884	0.361547
$healthm_{ms}$	-0.28532	0.187619	-1.52	0.128	-0.65305	0.082409
$asistemst_{ms}$	-0.73616	0.052679	-13.97	0.000	-0.83941	-0.63291
$w3_{ms}$	0.267694	0.047753	5.61	0.000	0.174099	0.361288
$w4_{ms}$	0.223197	0.040794	5.47	0.000	0.143243	0.303151
ms	6.435053	3.395908	1.89	0.058	-0.22081	13.09091

Table 1.13: Fixed-effects (within) regression coefficients[a]

				Number of obs =		74,427
				Number of groups =		24,809
R-sq:	within =	0.3887		Obs per group:		min = 3
	between =	0.3643				avg = 3
	overall =	0.3580				max = 3
$F(86, 49{,}532) =$	366.15					
corr(u_i,Xb) =	-0.4034				Prob > F =	0.0000
enrolled	Coefficient	Standard Error	t	$P > \|t\|$	95% Conf. Interval	
$grant_{fp}$	0.0000	0.0001	0.0900	0.9250	-0.0001	0.0002
$grantd_{fp}$	-0.0023	0.0005	-4.9700	0.0000	-0.0032	-0.0014
$grantc6_{fs}$	0.0001	0.0001	1.6900	0.0900	0.0000	0.0002
$grantd6_{fs}$	-0.0013	0.0002	-5.4300	0.0000	-0.0017	-0.0008
$grantc7_{fs}$	0.00003	0.0001	0.7600	0.4480	-0.0001	0.0001
$grantd7_{fs}$	-0.0012	0.0002	-6.0500	0.0000	-0.0016	-0.0008
$grant_{mp}$	0.0001	0.0001	1.6600	0.0970	0.0000	0.0003
$grantd_{mp}$	0.0002	0.0004	0.5700	0.5700	-0.0006	0.0011
$grantc6_{ms}$	0.0001	0.0001	1.1200	0.2640	0.0000	0.0002
$grantd6_{ms}$	0.0002	0.0002	0.7300	0.4660	-0.0003	0.0006
$grantc7_{ms}$	0.00003	0.0001	0.7300	0.4660	-0.0001	0.0001
$grantd7_{ms}$	0.0001	0.0002	0.2900	0.7700	-0.0004	0.0005
σ_u	0.2598					
σ_e	0.1574					
ρ	0.7315	(fraction of variance due to u_i)				
F test that all $u_i = 0$: $F(24{,}808,\ 49{,}532) = 4.37$ Prob > $F = 0.0000$						

Standard errors are clustered at family level. [a] Variable's definition are explained in Table 1.11.

Chapter 2

The school reentry decision of poor girls. Structural estimation and policy analysis using PROGRESA database.

2.1 Introduction

In this paper I evaluate the effectiveness of alternative policies to persuade dropout girls in poor families to go back to school and continue with their education. I discuss the differential effect that several policies have on reentry decisions and on enrollment decisions. I quantify the effect of demand-side policies such as conditional cash transfers and availability of daycare centers for young children. I also present results of the effect on school attendance of supply-side policies such as reduction in class size and increase in the number of communities where a secondary school is available. The analysis is based on a dynamic behavioral model of school choices for girls. The structural parameters of the model are estimated using the Mexican PROGRESA database.

The motivation behind this study is threefold. First, in the paper I address a relevant policy concern: how to increase educational participation in developing countries. Despite the efforts made by policy makers in increasing enrollment rates, educational participation is far from targets proposed by several international institutions[1]. UNESCO (2007) reports that between 2000 and 2006, the total number of out of school children in low-income countries decreased by 41%. Yet, in 2006 almost one in five of children of primary school age were not in school. Secondary net enrolment rates have been gradually increasing by around 2 to 3 percentage points per year in most regions. Still, in 2005 three in five children of secondary school age in low-income countries were not in school. To increase school attendance rates among poor children in developing countries, policy makers have

[1]For example, universal primary education is Goal 2 of both Education for All movement and the Millennium Development Goals adopted by UN Member States in 2000

implemented conditional cash transfers programs[2]. While transfers have been successful in keeping boys and girls at school, there exist evidence that they do not increase girls' reentry rates.

Second, in this paper I contribute with the evaluation of a well-known anti-poverty program, PROGRESA. I quantify the effect of PROGRESA grants on reentry decision for girls using a dynamic behavioral model of school choices. The methodology applied allows to perform a counterfactual analysis. The analysis of the effect of PROGRESA grants on reentry decision has been seldom discussed. Behrman, Sengupta, and Todd (2001), using difference-in-difference estimation techniques, conclude that PROGRESA grants increase reentry rates and this effect is lower for girls than for boys. Valdes (2007) addresses the analysis of the effect of PROGRESA grants on reentry rates by estimating a reduced form equation for schooling enrollment and finds that grants increase reentry rates among boys but do not affect girls' reentry rates.

Third, this study contributes to a growing literature that addresses empirical questions using discrete choice dynamic programming models of individual behavior. These models are attractive because structural parameters have a clear interpretation within the theoretical model and they are useful tools for the evaluation of counterfactual policies (Aguirregabiria and Mira (2007)). Miller (1984) and Keane and Wolpin (1997) propose and estimate dynamic models of occupational choices. Attanasio, Meghir, and Santiago (2005) and Todd and Wolpin (2006) use dynamic behavioral models to evaluate the PROGRESA program. In this paper schooling choices for girls in poor families are modelled following the individual decision approach as in Attanasio, Meghir, and Santiago (2005), where boys decide whether to attend school or to work. For families with many children the value of retaining a girl at home becomes more relevant since they are a good help in housework. As girls may dropout from school to stay at home I depart from Attanasio, Meghir, and Santiago (2005) by allowing girls to choose among three alternatives: attend school, stay at home and work. Under this framework a girl schooling decision can be assumed to be made by her parents in an altruistic fashion. That is, they choose the alternative that maximize their daughter inter-temporal welfare independently of the decision they make for their other children. I relax the assumption allowing the value of each alternative to be affected by family composition in two ways. Unobserved individual heterogeneity and the utility a girl derives from staying at home are affected by family characteristics.

The estimated model fits girls' schooling choices reasonable well. It replicates patterns observed in the actual distribution of schooling choices by ages: for each particular age reentry rates are lower than enrollment rates; reentry and enrollment rates decrease as age increases, and reentry rates decrease quicker than enrollment rates. It also replicates

[2]Examples of cash transfer programs are PROGRESA in Mexico, PRAF in Honduras, Red de Protección Social in Nicaragua and Familias en Acción in Colombia.

main features of the distribution of schooling choices by stock of education: reentry and enrollment rates decreases as the stock of education increases and, in the last grade of primary school and in the last grade of junior secondary school reentry and enrollment rates go down remarkably. It is observed in the data that most girls that were attending school in the previous academic year (non-dropout girls) are still in school in the current year while only 40% of girls who were out of school (dropout girls) come back. The estimated model is able to match these differences in the distribution of schooling choices between non-dropout and dropout girls. It rationalizes these differences by showing that persistence is relevant in the decision of attending school. The model also contributes to understand the reasons that make a girl dropout from school. A girl's decision to drop out of school is related to her age, the composition of her family and her mother's labor participation, but unrelated to unobserved characteristics of the girl, such as her unobserved ability at school. As her value at home increases with the number of members in her family and with her age, she leaves school not to work but to stay at home helping in housework. Additionally, results suggest that alternative policies to cash transfers, such as free access to community nurseries and kindergartens, availability of secondary schools, and reductions in class size, effectively increase school reentry rates for poor girls.

The paper is organized as follows. In Section 2 I present the theoretical model. Section 3 presents the main features of the PROGRESA program. Section 4 describes characteristics of the PROGRESA database. It provides some main statistics that focus on the differences between dropouts and non-dropouts. In Section 5 I discuss the empirical implementation of the model. In Section 6 I present results of the estimation of the structural parameters and of the counterfactual analysis. Finally, Section 7 concludes the paper with its main results.

2.2 The Data

2.2.1 Description of PROGRESA

The Education, Health and Nutrition program, PROGRESA, was first implemented by the Federal Government of Mexico in 1997, with the aim of helping the poorest families in rural communities. A fundamental characteristic of the program is that aid is conditioned on a specific behavior of the beneficiary. This conditionality aims to guarantee that the program does not lead to undesired outcomes, such as distortions in work decisions, and that it successfully accomplishes its initial objectives.

The program comprises actions in three major areas: education, health and nutrition. The expected outcomes were higher literacy rates, enrollment rates and completion rates; lower child mortality rates and higher vaccination rates; and lower rates of undernourishment. The program is targeted at family level. A family is qualified as being poor and thus eligible for the program according to a single index. This index contains information on family income and housing characteristics like presence of running water, electricity, pipes, etc.[3] Eligibility is independent of residence and family size and composition. All aid is given to the mother as there exist evidence that mothers are better than fathers at allocating family resources[4].

The education component includes monthly grants for children of a family qualified as beneficiary. To be given a grant, children need to be less than 18 years old, enrolled in school between the 3^{rd} year of primary school and the 3^{rd} year of junior secondary school, and to fulfill a minimum attendance requirement. The grants are not assigned based on academic achievement. A child who does not pass a grade is still eligible for the grant in the following year. But if the child fails the same grade twice, she/he losses eligibility. The grant increases with the years of schooling completed. In the junior secondary level the grant is slightly higher for girls, since there exist evidence that in poor families girls are more likely to dropout of school and that they dropout earlier than boys. Additionally, beneficiaries receive an annual grant for school supplies. In Table 2.1 there is a description of grants amounts. An eligible family was entitled to receive at most 420 pesos per month by means of scholarships in the second half of 1998. This amount represents 40% of the mean monthly family income and 67% of the mean monthly family expenditure in consumption. Thus, scholarships are potentially an important source of household's resources.

2.2.2 Evaluation of PROGRESA

Mexican authorities have intended to evaluate the program since its beginning, not only to measure results and impacts but also to provide information that allow for a redesign of policies. Accordingly, in 1997 and 1998 a high quality data set was collected in 506 communities where the program was to be implemented, and several surveys were carried out afterwards. In October 1998, the program was implemented in 320 randomly selected communities (treated communities) while in the remaining 186 communities (control communities) the implementation was postponed until December 1999[5]. In Figure 2.1 below, I present the timing of the program.

[3] For a complete analysis of the targeting see Skoufias, Davis, and Behrman (1999a) and Skoufias, Davis, and Behrman (1999b).

[4] See Rubalcava and Thomas (2000) for a discussion.

[5] The quality of the randomization has been extensively documented in Behrman and Todd (1999), who conclude that, at least at community level, the implementation of the random assignment was performed successfully.

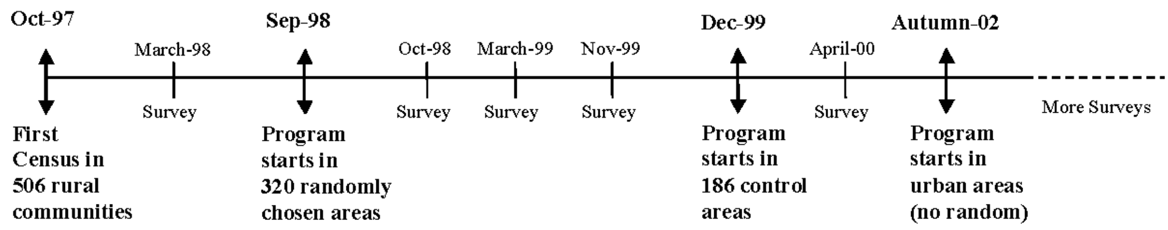
Table 2.1: Grant amount and household income and consumption (in Mexican pesos)

Monthly grant		July - Dec, 98	Jan - June,99
Primary School			
	3	70	75
	4	80	90
	5	105	115
	6	135	150
Secondary School			
1	girl	195	210
	boy	185	200
2	girl	220	235
	boy	195	210
3	girl	240	255
	boy	205	225
Monthly maximum support by means of grants per family		420	465
Annual aid for school supplies			Academic year 98/99
Primary School			135
Secondary School			170

Monthly Household Income and Consumption	Nov 98
Income	1071
Consumption	630

Source: Data on grants from Histórico de apoyos monetarios. SEDESOL 2005. Data on income and consumption from Albarran and Attanasio (2002)

Figure 2.1: Timing of the PROGRESA program



There exist a large literature on the evaluation of the average effect of PROGRESA schooling grants. Authors agree in their main conclusions: the program has increased enrollment rates for those children who received the grants, and this positive effect is higher on girls and on children who attend secondary school. We can distinguish two approaches in this literature according to the methodology applied. Researchers exploited the random assignment of the program at a village level and calculated difference and difference-in-difference estimators. Schultz (2004) is one of the main references. Then, researchers turned to analyze how to improve the effectiveness of the program estimating structural dynamic models of discrete choice[6] to simulate schooling decisions under alternative policies. Attanasio, Meghir, and Santiago (2005) models schooling as an individual decision and Todd and Wolpin (2006) which uses a model of parental decisions about fertility and child schooling.

2.2.3 Summary statistics

The sample used for the estimation of the model includes observations for females from 8 to 17 years old from the October 1998 survey that was conducted one year after the implementation of the program[7]. This includes 9,174 girls belonging to 6,303 families. To identify dropout girls I use information from the October 1997 survey. In particular, I use the following question: "Is she attending school now?" A girl is considered a "dropout girl" if the answer is "no", and a "non-dropout girl" if the answer is "yes". The sample consist of 7,884 (86%) non-dropout and 1,290 (14%) dropout observations.

By the time of the October 1998 survey, 85% of girls were enrolled in school, 2.2% were working for a salary and 12.8% were neither in school not working, so I assume they were at home helping in housework. The distribution of choices is not the same for non-dropout

[6] Eckstein and Wolpin (1989), Rust (1994) and Aguirregabiria and Mira (2007) are exceptional surveys on the estimation of structural dynamic models of discrete choice.

[7] I do not include 6 and 7 years old girls because PROGRESA grants are given to those children that have completed at least 2^{nd} grade in primary school. So a children aged 7 or less is not entitled to receive a grant. Additionally, even thought the entrance in primary school is delayed one or two years, enrollment rates in 1^{st} and 2^{nd} grade in primary school were above 96% in the 1998 survey.

Table 2.2: Distribution of choices for Non-dropout and Dropout Girls

Choice	Non-dropout	Dropout	Total
school	7,276	516	7,792
	(92.3)	(40.0)	(84.9)
work	110	95	205
	(1.4)	(7.4)	(2.2)
home	498	679	1,177
	(6.3)	(52.6)	(12.8)
Total	7,884	1,290	9,174

Percentages in parenthesis.

and dropout girls. As it can be seen in Table 2.2 most non-dropout girls were still at school in 1998 while more than 60% of dropout girls didn't go back to school and were mainly at home. For both groups the alternative of working for a salary is negligible.

Differences in the distribution of choices between non-dropout and dropout girls are even more important when they are analyzed by age and by stock of education, as it is shown in Figures 2.2 and 2.3 below[8]. In both graphs it is evident that girls leave school to stay at home, and not to work for a salary. Additionally, enrollment rates for non-dropout are always higher than for dropout girls. Looking at the distribution of choices by ages, enrollment rates decrease with age and the rate at which they decrease is higher for dropout girls.

There are two grades in which enrollment rates for non-dropout girls go down remarkably. Grade 6, when girls finish primary school, and grade 9, when girls finish secondary school. A similar situation occurs with reentry rates: they are at their minimum levels in grades 6 and 9.

[8]A complete report of distribution of actual choices can be found in the Appendix in Table 2.7 and in Table 2.8.

Figure 2.2: Distribution of choices by age

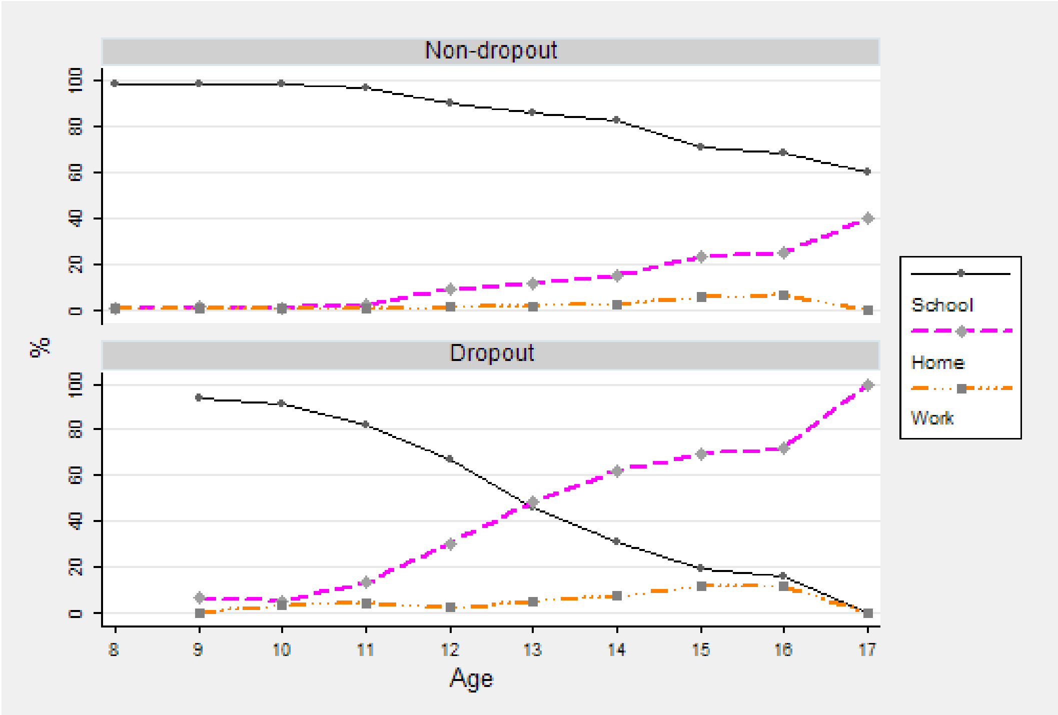
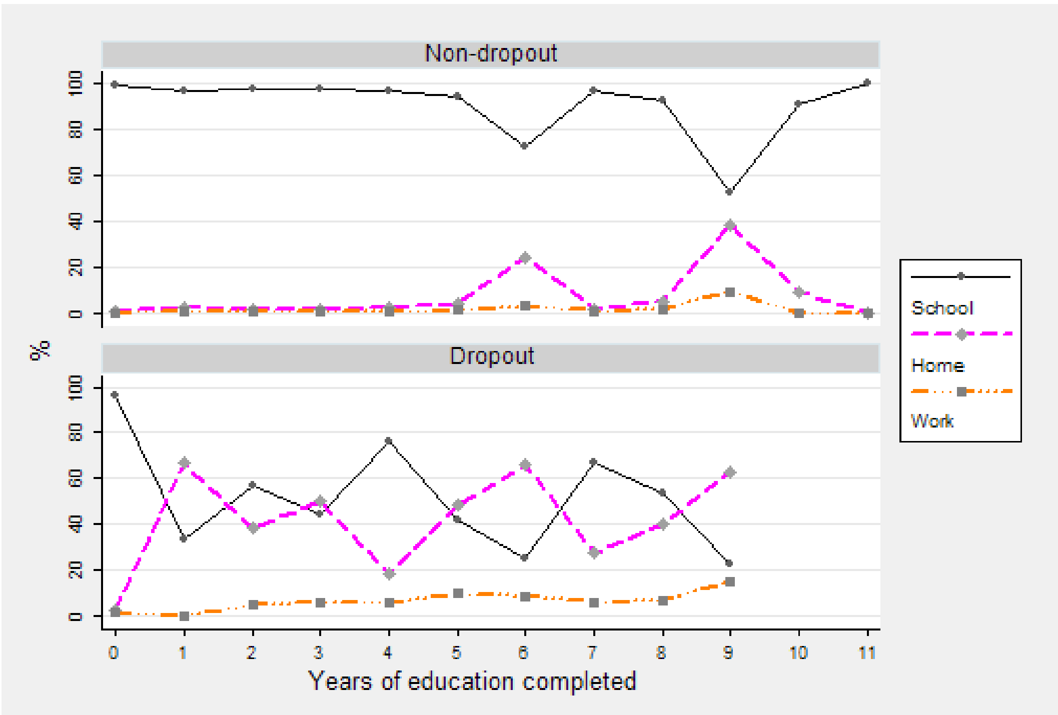


Figure 2.3: Distribution of choices by stock of education



The information contained in the PROGRESA surveys refers to individual characteristics, family composition, parents activities and background, and community characteristics. Descriptive statistics for selected variables for non-dropout and dropout girls are presented in Tables 2.3 and 2.4.

Table 2.3: Summary statistics for Non-dropout Girls

Variable	Mean	Std. Dev.	Min.	Max.
Age	11.04	(2.19)	8	17
Years of education	4.19	(2.1)	0	11
Potential monthly wage	374.32	(203.63)	11.82	1928.53
Percentage of girls belonging to a poor family	0.87	(0.34)	0	1
Percentage of girls whose father is present at home	0.93	(0.26)	0	1
Number of sisters	1.28	(1.12)	0	6
Number of brothers	1.27	(1.09)	0	6
Number of siblings aged 5 or less	1.16	(1.19)	0	11
Percentage of girls whose mother works	0.09	(0.28)	0	1
Mother's years of education	2.88	(2.54)	0	18
Percentage of girls that reside in a community with secondary school	0.35	(0.48)	0	1
Class size in primary school	25.27	(4.35)	16.66	38.5
Class size in secondary school	22.27	(4.38)	10.11	45

Table 2.4: Summary statistics for Dropout Girls

Variable	Mean	Std. Dev.	Min.	Max.
Age	13.59	(1.86)	9	17
Years of education	4.93	(2.01)	0	9
Potential monthly wage	491.15	(166.77)	102.66	1385.65
Percentage of girls belonging to a poor family	0.87	(0.34)	0	1
Percentage of girls whose father is present at home	0.93	(0.26)	0	1
Number of sisters	1.39	(1.18)	-1	6
Number of brothers	1.29	(1.07)	0	5
Number of siblings aged 5 or less	1.14	(1.17)	0	7
Percentage of girls whose mother works	0.09	(0.28)	0	1
Mother's years of education	1.92	(2.09)	0	16
Percentage of girls that reside in a community with secondary school	0.22	(0.41)	0	1
Class size in primary school	26.32	(4.42)	16.66	38.5
Class size in secondary school	22.33	(4.94)	10.11	45

The mean non-dropout girl is 11 years old and has 4 years of education completed. Her mother has completed 3 years of education. She has two sisters and a brother older than six years, and one sibling aged less than 5. If she decides to work she can earn 375 pesos, an amount of money higher than the amount of the scholarship. In her municipality the mean class size is 25 students per class in primary school and 22 in secondary school. 87% of non-dropout girls belong to a poor family, only 9% of them has a working mother and 7% do not live with her father. 35% of non-dropout girls have a secondary school in their community of residence.

The mean dropout girl is 13 years old and has 5 years of education completed. Her mother has completed 2 years of education. She has two sisters and a brother older than six years, and one sibling aged less than 5. If she decides to work she can earn 490 pesos. In her municipality the mean class size is 26 students per class in primary school and 22 in secondary school. 87% of dropout girls belong to a poor family, 9% of them has a working mother and 7% do not live with her father. 22% of dropout girls have a secondary school in their community of residence.

Comparing both groups we can conclude that dropout girls have less educated mothers, a higher proportion of them have to travel to other community to attend secondary school and if they work they receive a higher salary than non-dropout girls.

2.3 Model and Empirical implementation

2.3.1 The general model

In this section, I present a dynamical behavioral model of schooling decision for girls aged 6 (the official age to enter school) to 17 (the stopping period)[9]. At each age t, a girl chooses one of three mutually exclusive actions: go to school ($a_{it} = 1$), work for a salary ($a_{it} = 2$) or stay at home to help in housework ($a_{it} = 3$). This is consistent with assuming that parents make decisions in the best interest of each of their children, so there are no interactions between the decisions of children that belong to the same family. Let Ω_{it} denote the state vector which contains all variables known by girl i at age t which have an impact on her current and future choices. Among other components, it also includes the girl's stock of education and she faces uncertainty about the evolution of her future stock of education. Denote by π^s_{tg} the probability of passing the grade at age t for grade g, that is the transition probability for the girl's stock of education. At age 18 girls either work and earn wages in accordance to their levels of education or stay at home.

Period t alternatives are chosen to maximize the intertemporal utility function

$$\mathbb{E}[\sum_{j=0}^{T-t} \beta^j u(a_{i,t+j}, \Omega_{i,t+j})|a_{it}, \Omega_{it}] + \beta^{T-t+1}\mathbb{E}[V^{T+1}(\Omega_{T+1})|a_{it}, \Omega_{it}] \tag{2.1}$$

subject to the evolution of future values of the state variables, particulary to the probability of passing a grade π^s_{tg}. β is the intertemporal discount factor, $V^{T+1}()$ is the terminal value function, $\mathbb{E}_t$ is the expectation operator conditional on the state and $u(a_{it}, \Omega_{it})$ is the instantaneous utility function at age t for individual i that is specific for each choice a. By Bellman's principle of optimality, the choice specific value functions can be obtained using the recursive expression:

$$v(a, \Omega_{it}) \equiv u(a, \Omega_{it}) + \beta\mathbb{E}[\max_{a\in A} v(a, \Omega_{i,t+1})|a_{it}, \Omega_{it}] \tag{2.2}$$

for $a = 1, 2, 3$ and $t \leq T - 1$, and $v(a, \Omega_{it}) = u(a, \Omega_{it}) + \beta\mathbb{E}[V^{T+1}(\Omega_{T+1})|a_{it}, \Omega_{it}]$ for

[9] The reason for choosing 17 as the stopping age is that all women aged 18 or more in the database report not to be enrolled in formal education.

$a = 1, 2, 3$ and $t = T$. The optimal decision rule is then:

$$\alpha(\Omega_{it}) = \arg\max_{a \in A} v(a, \Omega_{it}) \tag{2.3}$$

In the database there is information on the individual's action a_{it} and a set of individuals characteristics X_{it}. From an econometric point of view, the state vector includes two subset of state variables: $\Omega_{it} = (X_{it}, \epsilon_{it})$. X_{it} are observed variables and ϵ_{it} are unobserved variables.

2.3.2 Utilities

Let $a_{it} = 1 \equiv w$, $a_{it} = 2 \equiv e$, $a_{it} = 3 \equiv h$ identify the alternatives of working, attending school, and staying at home respectively.
The per-period utility function of working is:

$$u(w, \Omega_{it}) = \eta w_{it} + \epsilon^{w}_{it} \tag{2.4}$$

where w_{it} is the potential wage a girl can earn[10].
The per-period utility function of attending school is:

$$u(e, \Omega_{it}) = \mu_i + \alpha_1 \eta G_{it} + \alpha_2 D_i + \alpha_3 AS_{i,98} + \alpha_4 CS_{i,98} + \alpha_5 S_{it} + \alpha'_6 x_{it} + \epsilon^{e}_{it} \tag{2.5}$$

μ_i is the unobserved type, individual specific and time-constant. G_{it} is the potential grant amount, that takes a positive value if the child belongs to a poor family, resides in a treated community, and is attending a grade between 3^{rd} year of primary school and 3^{rd} year of junior secondary school. D_i is a dummy variable, which equals 1 if the child dropped out of school in the previous academic year[11]. $AS_{i,98}$ is a dummy equal 1 if there is a junior secondary school in the community where the girl resides (is a measure of the direct cost of attending secondary school). $CS_{i,98}$ is a municipality measure of class size. S_{it} is the girl's stock of education. x_{it} is a set of individual and family characteristics that includes the age of the child, an indicator of the socioeconomic situation of the family and mother's schooling.
The per-period utility function of staying at home is:

$$u(h, \Omega_{it}) = \delta_0 + \delta_1 D_i + \delta_2 MW_i + \delta_3 C5_{it} + \delta_4 SI_{it} + \delta_5 B_{it} + \delta_6 S_{it} + \delta'_7 x_{it} + \epsilon^{h}_{it} \tag{2.6}$$

[10] Since in the survey it is reported only in a small percentage of the cases it is estimated by OLS. For more details see the Appendix.

[11] The dropout dummy is constructed using information on school attendance from the September 1997 survey. It is based on the same question used to construct the alternative chosen by the girl in 1998.

MW_i is a dummy variable equal to 1 if the mother works for a salary. $C5_{it}$ is the number of siblings aged less than 5 years old. SI_{it} is the number of sisters aged from 12 to 16, and B_{it} is the number of brothers from 6 to 18 years old.

2.3.3 Assumptions

On random shocks ϵ_{it}^a for $a = w, e, h$ is a random variable which affects the utility of action a in period t for individual i. It is observed by the individual but not by the econometrician. The ϵ_{it}^a's satisfy the conditional independence assumption, i.e., they are independent across choices, individuals and periods with distribution $F_\epsilon(.)$.
On utilities $u(a_{it}, \Omega_{it})$, the utility functions, are additively separable in observables and unobservables:

$$u(a_{it}, \Omega_{it}) = \tilde{u}(a, X_{it}) + \epsilon_{it}^a \tag{2.7}$$

Thus, the optimal decision rule becomes

$$\alpha(X_{it}, \epsilon_{it}) = \arg\max_{a \in A} v(a, X_{it}) + \epsilon_{it}^a \tag{2.8}$$

And, for any $(a, X) \in A \times \mathbb{X}$, the conditional choice probability is:

$$Pr(a|X) = \int \mathbf{1}[v(a, X_{it}) + \epsilon_{it}^a > v(a^{'}, X_{it}) + \epsilon_{it}^{a^{'}} \forall a^{'}] dF_\epsilon(\epsilon_{it}) \tag{2.9}$$

On unobserved heterogeneity Following Heckman and Singer (1984) there are M types of individuals, for M a finite set of types. μ_m is the parameter related to type m and π_m is the proportion of the population of that type[12]. Girls are heterogeneous in their ability at school. Each girl knows her own type but it is not observed by the econometrician.
On transition probabilities π_{tg}^s, the transition probability of the stock of education, is exogenous and does not depend on effort or on the willingness to continue schooling. It varies with the grade and the age of the individual[13] and it is known to the individual. Age of the girl, amount of the grant and salaries evolve deterministically[14]. Availability of secondary schools and class sizes remain constant since 1998. Girls' mothers stock of education is constant. To control for the socioeconomic situation of the family I use the poor family indicator reported in PROGRESA. This indicator do not vary across time. Girls expect that the composition of her family will not change after 1998. Work status for the girl's

[12]Types probabilities are estimated using a logit model. Types probabilities depend on family composition variables

[13]It is also different between those girls that receive PROGRESA grants and those who do not receive the aid, since the grant could be an incentive to perform better at school.

[14]The evolution of the amount of the grant from 1998 to 2007 is observed and reported in Oportunidades (2008). The evolution of salaries in the period 1998-2007 is constructed using observed salaries in 1998 and updating them with the annual increase in the minimum wage for Mexico reported in CONASAMI (2008).

mother is assumed time-invariant and identified with her work status reported in 1998. If her father does not live with his family I assume he is not present at home in all periods. The number of sisters, brothers and sibling aged 5 years old or less evolve with the age of the siblings and I assume there are not newborn children through all periods.

On individual decision approach I assume that each girl is a single decision unit. The model presented so far is valid if it is the girl or her altruistic parents who decide girl's actions that maximize her lifetime welfare. In particular, interrelationship of schooling decisions across siblings are not directly considered. The individual decision assumption is relaxed in two ways. I allow girl's utility of staying at home to vary with several family composition variables. Additionally, the unobserved type, that enters the utility of attending school, is affected by the number of adults and children in the family and by the girl's birth order. It can be argued that parents make schooling decision for all their children simultaneously. Particularly, the decision of whether or not to send a daughter to school is affected by the number, ages, gender and action chosen for the other children in the family. In the model, to choose the action for a girl, parents take into account the number of children they have, their ages and their genders; parents also consider whether the mother is working outside the household and the total number of adults in the family. The assumption that the decision of one girl in a family do not depend on the decision of other girls in the family may be strong, since, as I show in Tables 2.16 and 2.17 in the Appendix, there exist a positive correlation between a girl's decision of attending school and the proportion of sisters that do attend school even after controlling for family composition variables. For girls attending secondary school it only matters the proportion of sisters attending the same level of education, while for girls in primary school there is a positive correlation with the proportion of sisters attending both levels of education. Assuming that a girl's school participation do not depend on her brothers choices seems more plausible since there is no correlation between girls's school participation and the proportion of brothers attending school.

2.3.4 Identification discussion

There are two concerns about the identification of the parameters in the proposed model: state dependence in the utility of attending school and identification of the effect of PROGRESA grants.

State dependence I introduce state dependence in the model in to ways. First, by allowing the utility of attending school to depend on the dropout indicator D_i. Second, state dependence is also present throught the stock of education S_{it} that is determined by past decisions of school attendance, and that also affects the utility of attending school in the current period. Both variables D_i and S_{it} are correlated with the unobserved type μ_i. I assume that state dependence is fully controlled by $Pr(S_{it})$, that is, I assume that

$Pr(S_{it}, D_i|\mu_i) = Pr(S_{it}|\mu_i, D_i)$. To introduce in the model the equation for the probability of having completed $S_{it} = s$ years of schooling, $Pr(S_{it} = s)$, I follow Attanasio, Meghir, and Santiago (2005). I model this probability as an interval regression probit model with grade specific (predetermined) cut-off points. The identification of the parameters of $Pr(S_{it} = s)$ relays on the availability of variables that affect this probability but do not affect the current utility of attending school. Those variables are one period lags of availability of secondary school and class size measures. The identification of the unobserved heterogeneity using a single cross section of data in a model with state dependence is very tenuous and relies in functional forms.

$$Pr(S_{it} = s|Z_{it}, \mu_i) = \Phi(s - (\zeta' Z_{it} + \xi\mu_i)) - \Phi((s-1) - (\zeta' Z_{it} + \xi\mu_i)), \tag{2.10}$$

where Z_{it} is a set of individual, family, and community characteristics that includes the age of the child, the dropout indicator D_i, the mother's schooling level, the socioeconomic indicator of the family, availability of junior secondary school in 1997 and the municipality measure of class size at primary and junior secondary school in 1997. The load factor ξ governs the covariance between the probability of having a stock of education s and the utility of attending school.

Grant effect The effect of the grant in the utility of attending school is modelled as a proportion of the impact of the wage in the utility of working. Then, the model can reflect a different effect on the decision of attending school given by one peso received as a grant or one peso received as a salary. For the identification of both effects it is necessary to have two different sources of exogenous variation. Wages vary with girls' age and stock of education and a set of labor market variables at village level. The amount of the grant also varies with girls' age and stock of education, and, most importantly, it has an exogenous (random) variation between girls who reside in treatment and control communities.

2.3.5 Likelihood

Define $\theta^a = \{\eta, \alpha_1, ..., \alpha_6, \delta_0, ..., \delta_6\}$ as the set of parameters in utilities, and $\theta^s = \{\zeta, \xi\}$ as the set of parameters in the initial condition equation. Let's denote $\rho = \{\theta^a, \theta^s, \{\mu_m\}_{m=1}^M, \pi_m, \beta, \pi_{tg}^s\}$, the set that includes all the parameters to be estimated in the model and the transition probability of the stock of education. Suppose $\tilde{u}(a, X_{it}), V^{T+1}()$ and $F_\epsilon()$ are known up to ρ. A girl contribution to the likelihood is:

$$l_i(\rho) = \sum_a \mathbf{1}(a_{it} = a) \sum_m^M Pr(a|X_{it}, S_{it} = s, \mu_m, \theta^a, \pi_{tg}^s, \beta,) \times Pr(S_{it} = s|\mu_m, \theta^s) \times \pi_m \tag{2.11}$$

and the sample log-likelihood is then $L(\rho) = \sum_i \ln l_i(\rho)$.

In order to evaluate the l_i for a particular value of ρ it is necessary to know the optimal decision rules $\alpha(X_{it}, \epsilon_{it}, \rho)$. Therefore, for each trial value of ρ the value functions $v(a, \Omega_{it})$ have to be calculated. The expression for the value functions at subsequent ages are computed recursively starting from age 18 and working backwards until the current age t. Under the assumption that the unobserved state variables ϵ^a_{it} are drawn from an extreme value distribution, conditional choice probabilities and recursive value functions in equation 2.2 have convenient (logistic) closed forms[15]. I estimate the model by a combination of maximum likelihood for $\theta^a, \theta^s, \{\mu_m\}^M_{m=1}, \pi_m$ and a grid search for the discount factor.

2.4 Results

2.4.1 Parameter estimates

Maximum likelihood estimates of the model's structural parameters are reported in Tables 2.10, 2.11 and 2.12 in the Appendix. I report probabilities of passing grade s at age t used in the estimation of the model in Tables 2.14 and 2.15 in the Appendix.

The estimated parameters in the three instantaneous utilities and in the stock of education equation have the expected signs. The utility of attending school is higher for younger girls, more educated, with more educated mothers, living in communities where there exists a secondary school and in municipalities where the mean class size is lower. Salaries have a positive effect on the utility of working. The utility of staying at home is higher for older girls, who are more educated, with less educated mothers, belonging to a family with at least one children aged 5 years old or less. This utility is higher if the girl's mother works outside the household. The stock of education of a girl is higher when she has a more educated mother, was enrolled in school in the previous year (non-dropout) and there is a secondary school in her village. On the other hand, girls belonging to poor families, who dropped out of school before 1997 and attending school in a municipality with higher class size have less years of education completed.

The model identifies two type of individuals. The high type individuals have a higher utility of attending school with an estimated unobserved effect equal to 9.4. The corresponding estimated value for low types is 5.5. The probability of being of high type is higher for the older girl in the family, whose mother do not work, and belongs to a family with a lower number of adults and children. Those girls who choose to attend school or to stay at home are of high type with probability above 95%. The probability of being high type is lower than 86% for those who choose to work. So unobserved heterogeneity partly explains why girls choose to work instead of attend school or stay at home. But it does not help to explain why a girl, once she decides not to work, decides to stay at home or to attend

[15] See the Appendix for the explicit functional form of value functions, conditional choice probabilities and $\mathbb{E}max$ function.

school. As it can be seen in Table 2.13 in the Appendix, unobserved heterogeneity does not contribute to explain differences in choices between non-dropout and dropout girls. In fact, new dropout girls, that is girls who were at school last year but are not attending school the current year, have the same probability of been high type than former non-dropout girls that choose to attend school.

2.4.2 Model Validity

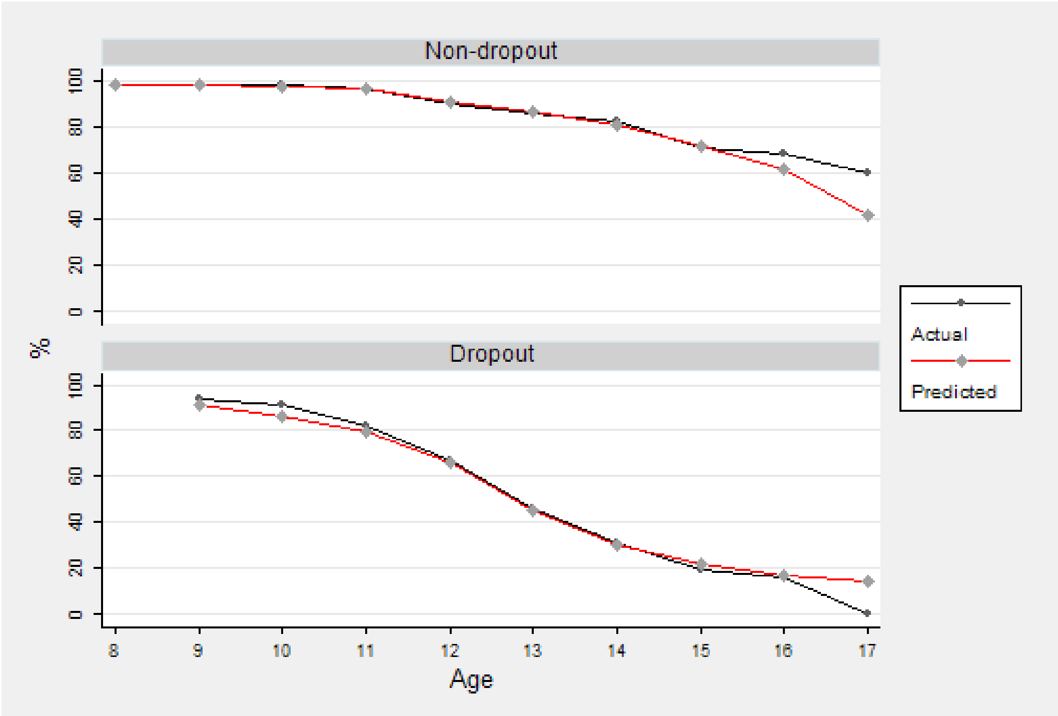
The validity of the estimates for the structural parameters relies strongly in the functional form assumptions made on utilities and on the initial condition equation. Thus, it is crucial to test the validity of the estimated model. In what follows I present several evidence on the validity of the estimated parameters.

First I compare the distribution of predicted choice probabilities obtained with the estimated parameters with the actual choices the individuals in the sample have made[16]. A complete report of distribution of actual and predicted choices can be found in the Appendix in Table 2.7 and in Table 2.8. The model does quite well in predicting distribution of choices by ages for non-dropout girls and dropout girls. As we can see in Figure 2.4 bellow, actual and predicted schooling enrollment rates are close for all ages except 17 years old[17].

[16]Predicted conditional choice probabilities are computed following Carro and Mira (2006). The procedure is explained in the Appendix.

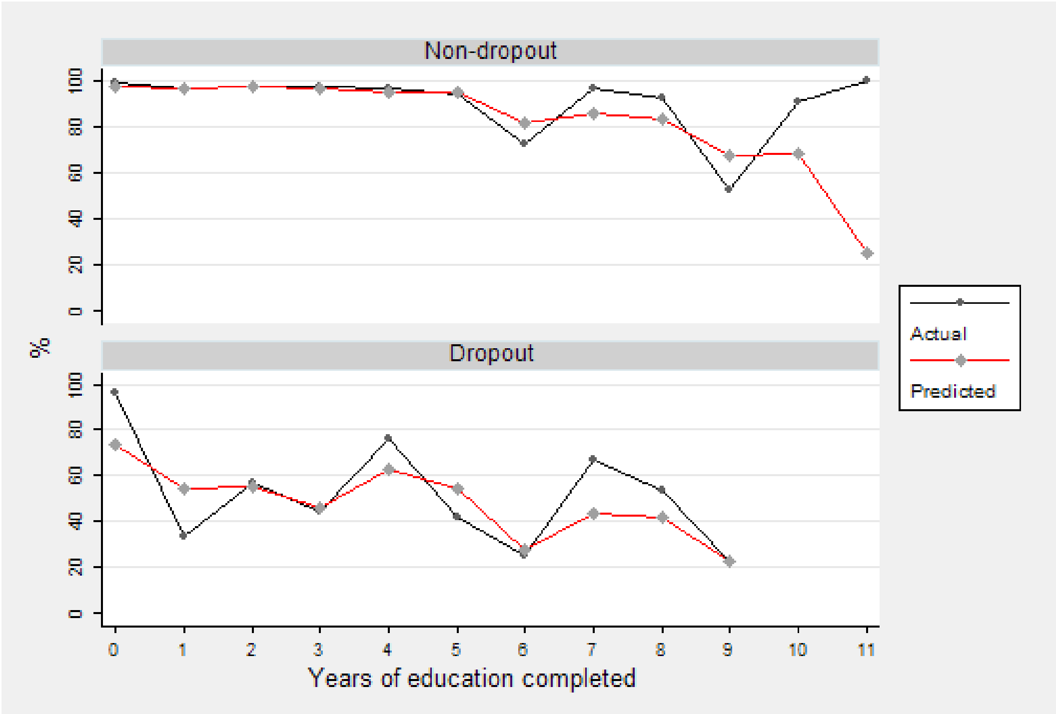
[17]The reason is the tiny number of girls aged less than 17 years old in the sample. It is 5 non-dropout and 3 drop-out

Figure 2.4: Actual and predicted enrollment rates by age (%)



Predicted choice probabilities by years of schooling completed reflect the main patterns in the actual distribution of choices: enrollment rates for non-dropout girls are always higher than for dropout girls; enrollment rates for both groups decrease as the stock of education increases; the lowest enrollment rate in primary school appears in the last grade, that is grade 6; and the lowest enrollment rate considering all grades come in the last year of junior secondary school.

Figure 2.5: Actual and predicted enrollment rates by stock of education (%)



Second, I compare the estimated grant effects with the estimates reported in Schultz (2004) and Valdes (2007). The effect of PROGRESA grants in the present model is computed by comparing the choices predicted when girls are receiving the grant with the choices predicted when the grant amount is set to zero for all girls. The results obtained in this paper agree with those reported elsewhere, suggesting that the model does well in fitting the effects of PROGRESA grants for non-dropout and dropout girls. A summary of results are presented in Table 2.9 in the Appendix.

2.4.3 Counterfactual analysis

Although PROGRESA grants do not increase school reentry rates among girls, perhaps other policies do. In what follows, I analyze the effectiveness of several policies by means of counterfactual exercises. Results are presented in Table 2.5 and details on the policies follows bellow. It is worth to notice that counterfactuals exercises obtained by using the parameters related with PROGRESA grants are more credible than other exercises. The reason is that the identification of PROGRESA grants parameters is obtained with the exogenous variation provided by the random assignment of the scholarships, while there is not a natural experiment behind the identification of the other parameters in the model.

Table 2.5: Increase in school attendance due to different policies (in %)

Policy	Non-dropout Primary	Non-dropout Secondary	Drop-out Primary	Drop-out Secondary
Enrollment rate	97	80	59	28
PROGRESA grant	0.7	5.1	0.8	0.8
Duplicate PROGRESA grant in secondary	0.7	5.2	0.7	1.0
Free access to daycare center	0.0	1.0	1.2	1.7
Availability of secondary school in almost all villages	0.6	2.6	4.6	5.3
Reduction of class size to 25 children	0.2	0.5	1.5	1.1

Duplicate the amount of PROGRESA grants in secondary school: Enrollment rates in primary school is near 90% while the figure in secondary school is 67%. A policy intended to increase school enrollment could at most increase in 10% enrollment in primary school but more than 30% in secondary school. This fact makes it attractive the implementation of scholarships that give a higher amount of money in secondary school. The results show that for non-dropout the actual amount of the grant is already optimal since the response to the extra money is almost negligible. For dropout girls the effect is only 1%, confirming the initial hypothesis that cash transfers, no matter how much money they receive, do not substantially change their utility of attending school.

Community nurseries/kindergartens: Suppose all the children aged less than five in the family are sent to a (free) daycare center[18]. Girls will be no longer needed at home to look after them and may go back to school. In the model the utility of staying at home is (positively) related with the number of children aged less than five in the family. The effect of this policy on girls' school enrollment can be approximated by simulating girls' choices after setting the number of children aged less than five equal zero. Notice that making zero the number of children less than five in the family may overestimate the actual effect of having free access to a daycare center. Not having children below five at home means the girl will not have to take care of them during all day, while having access to a child care facility will reduce the need of help in taking care of the youngest children in the family during at most 8 hours. In the case that school ours fully coincide with the time the children can be left in the daycare center, the upward bias in the estimation will be reduced. The approximate effect of availability of nurseries on non-dropout girls is lower than the effect of PROGRESA grants while it is higher for dropout girls. A combination of both policies has the desired effect, increasing enrollment in secondary school for non-dropout in 6% and

[18]I do not discuss how the daycare center would be financed.

2% for dropout girls.

Availability of secondary school in almost all villages As it is shown in Table 2.3 and Table 2.4 above, only in 34% of the villages where non-dropout girls reside and in 22% of the villages where dropout girls reside there exists a secondary school. No availability of a secondary school in a village implies transportation and time costs. Both costs decreases the utility of attending school. If the government establishes a secondary school in at least all villages where the demand is high enough, a positive effect on school enrollment and reentry rates could be expected. I simulate girls' choices by setting equal one the indicator variable of availability of secondary school for girls who reside in villages where the potential number of secondary school students is higher than 25. The result is promising for all groups, non-dropout and dropout girls attending primary and secondary school. In primary school enrollment rate increases 1% and reentry rate increases 5% while the figures in secondary school are 3% increment in enrollment rate and 5% increment in reentry rate.

Reduction in the class size The quality of the education process is an important determinant of the utility of attending school. In classrooms where the number of students is high teachers cannot pay enough attention to all of them and the acquisition of knowledge is likely damaged. Average class size in the villages analyzed is around 25, not a huge value. But in some villages classes have 45 students in secondary school and 39 in primary school. An improvement in school enrollment and reentry rates could be expected from a reduction in class size. The proposed policy is to reduce to 25 the mean number of students per class wherever it is necessary. Simulations of girls choices imposing this reduction in class size show that class size matter only for dropouts, and this policy is more effective in primary school.

2.5 Alternative model

In this section I discuss an alternative model specification that implement a different solution for the initial conditions problem. Instead of modeling the correlation existent between the stock of education S_{it}, the dropout indicator dummy D_i and the unobserved heterogeneity μ_i introducing an equation for the probability of having completed s years of schooling, I make the type probability a function of both S_{it} and D_i. Additionally, I estimate the new model using a panel data that has information in two academic years 98/99 and 99/2000 with the aim of improving the identification of the unobserved heterogeneity. In the new model, the individual likelihood is:

$$l_i(\rho) = \sum_a \mathbf{1}(a_{it} = a) \sum_m^M \prod_t Pr(a_t = a|X_{it}, S_{it}, D_i, \mu_m, \theta^a, \pi^s_{tg}, \beta) \times Pr(\mu_m = \mu|S_{it}, D_i) \tag{2.12}$$

This new approach to solve the initial conditions problem implies changes in the computation of the predicted conditional choice probabilities that are shown in the Appendix under the title “Predicted probabilities”.

I also make several changes in the specification of the utilities to avoid having to relay in functional forms for the identification of the parameters related with the variables girl's stock of education, age, mother's stock of education, and poor indicator dummy.

The per-period utility function of attending school is now:

$$u(e,\Omega_{it}) = \mu_i + \alpha_1 \eta G_{it} + \alpha_2 D_i + \alpha_3 AS_{it} + \alpha_4' x_{it}^e + \epsilon_{it}^e, \tag{2.13}$$

where x_{it}^e is a set of individual and family characteristics that includes a dummy variable equal one if the child is behind in school, a dummy variable equal one if the child has graduated from primary school and a dummy variable to reflect the effect of graduation from secondary school, and mother's schooling. I remove the class size from the utility of schooling since its effect is more likely to appear in the probability of passing a grade. For this reason I re-estimate those probabilities using a municipality measure of class size for each academic year, 98/99 and 99/2000.

The per-period utility function of staying at home in the new model is:

$$u(h,\Omega_{it}) = \delta_0 + \delta_1 C5_{it} + \delta_2 SI_{it} + \delta_3 MW_i + \delta_4 C5_{it} MW_i + \delta_5 C5_{it} SI_{it} + \delta_6 B_{it} + \delta_7' x_{it}^h + \epsilon_{it}^h, \tag{2.14}$$

where x_{it}^h is a set of individual and family characteristics that includes the age of the child, a dummy equal one if the girl's father lives with his family, and indicator of the socioeconomic situation of the family.

I introduce a small change in the Terminal Value function that is discussed in the Appendix.

2.5.1 Results

2.5.1.1 Parameter estimates

Maximum likelihood estimates of the model's structural parameters are reported in Tables 2.20 and 2.21 in the Appendix. These results are a first approximation to the definite estimates for the parameters in the alternative model, in the sense that they may correspond to a local optimum of the likelihood function. More work in terms of checking the validity of the estimated coefficients is currently been done.

The estimated parameters in the three instantaneous utilities have the expected signs and the most relevant parameters for the policy analysis pursued in this paper are statistically significant.

The estimation of the model has been done so far with two type of individuals, that are well identified. The high type individuals have a higher utility of attending school with an estimated unobserved effect equal to 5.0. The corresponding estimated value for low types is 1.8. The probability of being of high type is higher for younger girls in the family, a girl that do not drop out from school in 1997, whose father lives with her family, and belongs to a family with a lower number of adults and children. The estimated coefficient of D_i is highly significant, that is, the model relates the types with the dropout status. The probability of being high type is 76% for non-dropouts and 35% for dropouts. The approach followed in the new model to specify unobserved heterogeneity seems to improve the identification of the distribution of random effects. As it can be seen in Table 2.6 below, unobserved heterogeneity does contribute to explain differences in the decision of attending school between non-dropout and dropout girls. The probability of attending school for a high type non-dropout girl is 78.2% while the probability of attending school for a low type dropout girl is only 52.5%. Among non-dropout girls, those of them who are low type have a higher relative probability of staying at home rather than attending school.

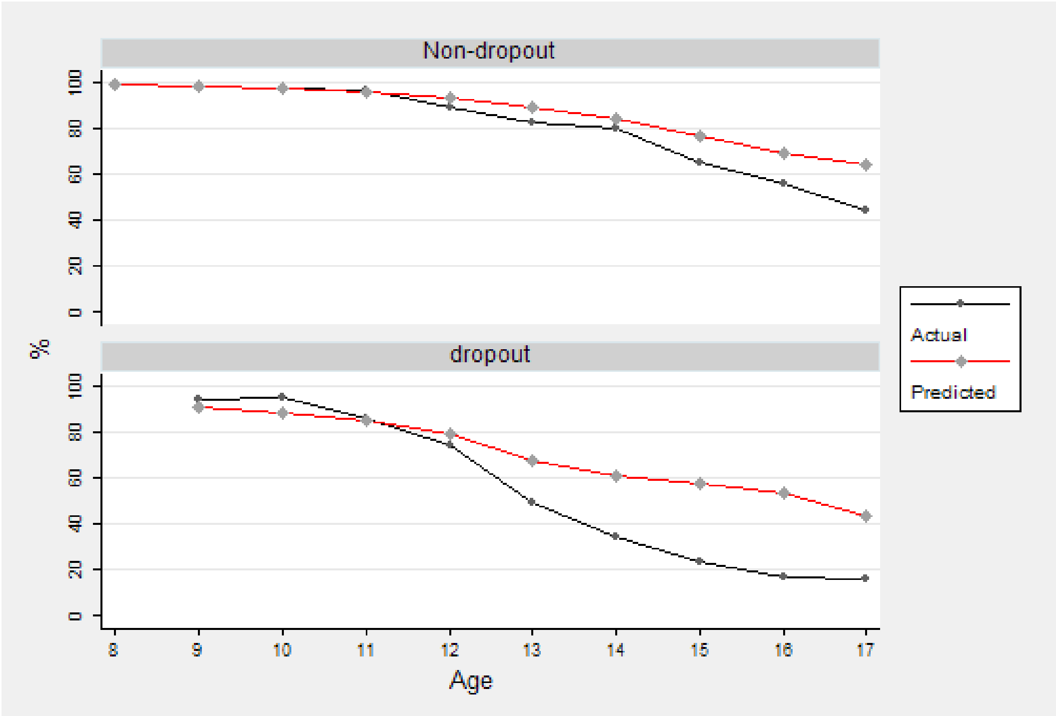
Table 2.6: Types distribution in the Alternative Model: Non-dropout and Dropout (%)

	Type 1: High Type		Type 2: Low Type	
Choice	Non-Dropout	Dropout	Non-Dropout	Dropout
School	78.2	47.4	21.2	52.5
Work	60.7	25.3	39.3	74.7
Home	55.2	27.3	44.8	72.7

2.5.1.2 Model Validity

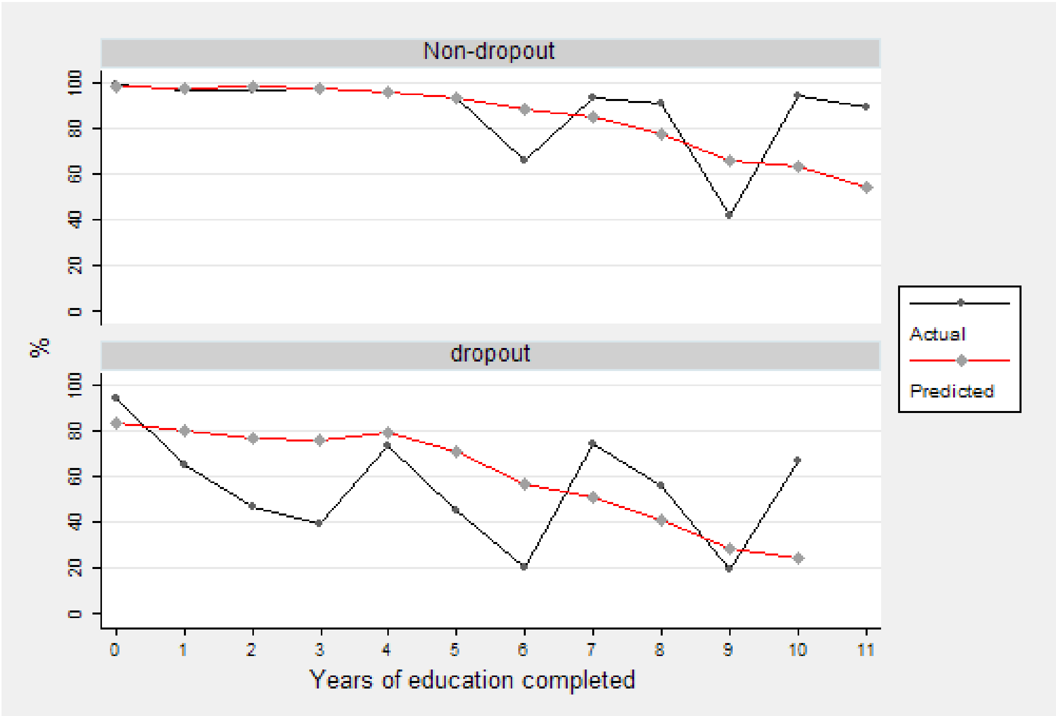
Looking at Figures 2.6 and 2.7 we can compare the distribution of predicted choice probabilities obtained with the estimated parameters with the actual choices the individuals in the sample have made. A complete report of distribution of actual and predicted choices can be found in the Appendix in Table 2.18 and in Table 2.19. The model does quite well in predicting distribution of choices by ages for non-dropout girls and dropout girls until the age of 12. Above that point, the model is not so accurate and for dropouts observations the differences are more relevant.

Figure 2.6: Alternative Model: Actual and predicted enrollment rates by age (%)



Predicted choice probabilities by years of schooling completed reflect two important pattern in the actual distribution of choices: enrollment rates for non-dropout girls are always higher than for dropout girls, and enrollment rates for both groups decrease as the stock of education increases. But the current estimation of the model is not able to pick the characteristic fall in enrollment rates in grades 6 and 9. Instead, the estimated model show a smooth decrease in enrollment probabilities as the stock of education increases.

Figure 2.7: Alternative Model: Actual and predicted enrollment rates by stock of education (%)



In general, the current estimated model overestimate enrollment probabilities for both, non-dropouts and dropouts observations. This in turn makes the fit of the effect of the grant quite inaccurate. In particular it underestimate the effect of PROGRESA grants on non-dropouts. For this reason I do not report results of counterfactual policy exercises.

2.6 Conclusions

In this paper I present a dynamic behavioral model of school choices for girls in poor families and estimate its structural parameters using the Mexican PROGRESA database. The estimated structural model fits girl's schooling choices reasonable well. It is able to replicate patterns observed in the actual distribution of schooling choices, and it also matches differences in the distribution of schooling choices between non-dropout and dropout girls. The model explains these differences highlighting the relevance of persistence in the decision of attending school and the importance of the girl's family composition. Results also suggest that unobserved heterogeneity in schooling decision does not explain differences between reentry and enrollment decisions.

The evaluation of PROGRESA grants resulting from the estimated model is consistent with previous literature. Grants are a good incentive to keep girls at school but the ones that are out of school do not come back. Simulations suggest that cash transfers do not increase school reentry rates even when the amount of the scholarship is duplicated. Since dropout girls are mainly at home helping to take care of the youngest children in the family, the availability of daycare centers implemented simultaneously with PROGRESA grants is efficient in increasing both school enrollment and reentry rates. Both targets are also efficiently achieved by reducing transportation and time costs in secondary school. Reduction in class size increases reentry rates but it does not change enrollment rates.

The relevance of family characteristics in school choices for girls suggested by the present model, invites for future research. Probably one of the most natural extensions is the study of school reentry decisions in the context of a family decision model. The estimation of a model of family child schooling and fertility decisions, like the model presented in Todd and Wolpin (2006), allows relaxing the assumption that there is no newborn children in girls' families.

As a further step, it would be interesting to estimate a collective decision model in which parents make labor and consumption decisions along with schooling decisions for their children. Such a model would allow analyzing interrelations between parents' labor participation decisions and girls schooling choices in poor families. Results in the present study show that mothers' working status affects girls' utility of staying at home. It can be expected that a girl whose mother works in the labor market would be more valuable at home, replacing her mothers' housework. However, worker mothers in the sample have less children than mothers who stay at home. This family characteristic is coherent with the result suggested by the present model, namely that a girl whose mother works outside the household has a lower utility of staying at home. A collective decision model in which parents simultaneously decide their labor status and their children schooling would shed light in the relation between both decisions. In the framework proposed, it would be possible to analyze the effect of policies intended to increase children school participation on parents' labor participation and girls schooling.

2.7 Appendix

Value functions

The value function for choosing to attend school is:

$$\begin{aligned} v(e, X_{it}) = \tilde{u}(e, X_{it}) \\ &+ \beta \pi^s_{tg} \mathbb{E}_\epsilon[\max_{a\in A}\{v(a, X_{i,t+1}) + \epsilon^a_{it}\}|X_{it}, S_{i,t+1} = S_{it} + 1, a_{it} = e] \\ &+ \beta(1 - \pi^s_{tg})\mathbb{E}_\epsilon[\max_{a\in A}\{v(a, X_{i,t+1}) + \epsilon^a_{it}\}|X_{it}, S_{i,t+1} = S_{it}, a_{it} = e] \end{aligned}$$

for $a = e, w, h$ and $t \leq T - 1$. At age $t = T \equiv 17$ it is:

$$\begin{aligned} v(e, X_T) = \tilde{u}(s, X_T) \\ &+ \beta \pi^s_{tg} V^{T+1}(X_{T+1}, S_{i,T+1} = S_{iT} + 1) \\ &+ \beta(1 - \pi^s_{tg}) V^{T+1}(X_{T+1}, S_{i,T+1} = S_{iT}) \end{aligned}$$

The value function for working (or staying at home) is:

$$\begin{aligned} v(w, X_{it}) = \tilde{u}(w, X_{it}) \\ &+ \beta \mathbb{E}_\epsilon[\max_{a\in A}\{v(a, X_{i,t+1}) + \epsilon^a_{it}\}|X_{it}, S_{i,t+1} = S_{it}, a_{it} = w] \end{aligned}$$

for $a = e, w, h$ and $t \leq T - 1$. At age $t = T \equiv 17$ it is:

$$v(w, X_T) = \tilde{u}(w, X_T) + \beta V^{T+1}(X_{T+1}, S_{i,T+1} = S_{iT})$$

I assume that girls do not attend school beyond 18 years old, so when they are 18 they have to decide wether to work or stay at home with her birth family or with her new family if she gets married[19]. The value of working is given by the salary an 18 years old girl can earn and her stock of education. The value of staying at home depends on the composition of her family and also on her stock of education. Unfortunately I do not have information on family composition at the age of 18 for girls in the sample. For this reason I cannot estimate separately parameters in both, the terminal value of staying at home and the terminal value of working. I estimate the parameters that affects the difference in the terminal value between both alternatives. This difference depends on the stock of education and on the salary. The terminal value function is:

[19]Most of the girls that get married in this villages stay in her new home taking care of her new family.

$$V^{T+1} = \delta_5 S_{i,18} - \eta r w_{i,18}$$

where rw_i is the real wage for adult workers in the community where the girl resides.
In the second new model I consider a slightly different specification for the terminal value function, in which I introduce a parameter for the effect of schooling in the terminal value and this parameter is not present in the current utility of attending school:

$$V^{T+1} = \gamma S_{i,18} - \eta r w_{i,18}$$

In all cases below, $\mathbb{E}max$ function are as follows:

$$\mathbb{E}_\epsilon[\max_{a \in A}\{v(a, X_{i,t+1}) + \epsilon_{it}^a\}|X_{it}, S_{i,t+1}, a_{it}] = ln(\sum_{a=1}^{3} exp(v(a, X_{t+1}))) + E$$

where E is the Euler constant (0.577215665). This expression is given by the extreme value distribution and by the conditional independence assumptions on ϵ_{it}^a.

Conditional choice probabilities

Assuming the ϵ_{it}^a are drown from an extreme value distribution and are conditional independent, the probability of choosing action a at time t is:

$$Pr(a_{it} = a'|X_{it}) = \frac{\exp v(a', X_{it})}{\sum_{a \in A} \exp v(a, X_{it})}$$

Predicted probabilities

Following Carro and Mira (2006), predicted conditional choice probabilities for each girl are computed as the weighted average of conditional choice probabilities for each unobserved type, with weights given by the ex post probability that the girl is of each type conditional on her stock of education and choice in Oct98.

$$\mathbb{P}_{ia} = \sum_{m}^{M} \mathbb{P}_{iam} \mathbb{P}(\mu_i|a_i, S_i)$$

$$\mathbb{P}(\mu_i|a_i, S_i) = \frac{\mathbb{P}(\mu_m, a_i|S_i)}{\mathbb{P}(a_i|S_i)}$$

$$\mathbb{P}(\mu_m, a_i|S_i) = \mathbb{P}(a_i|S_i, \mu_m)\mathbb{P}(\mu_m|S_i)$$

$$\mathbb{P}(a_i|S_i, \mu_m) = \mathbb{P}_{iam}$$

$$\mathbb{P}(\mu_m|S_i) = \frac{\mathbb{P}(S_i|\mu_m)\pi_m}{\sum_m^M \mathbb{P}(S_i|\mu_m)\pi_m}$$

$$\mathbb{P}(a_i|S_i) = \sum_m^M \mathbb{P}(a_i|S_i,\mu_m)\mathbb{P}(\mu_m|S_i)$$

where $\mathbb{P}_{iam}$ is the probability that girl i chooses action a if she is of unobserved type m, conditional on the state variables. $\mathbb{P}_{iam}$, $\mathbb{P}(S_i|\mu_m)$ and π_m are obtained from the model given parameter estimates.

Using the second approach proposed to deal with the initial conditions problem generates a change in the computation of predicted conditional choice probabilities. In the new model, these probabilities are estimated as follows:

$$\mathbb{P}_{ia} = \sum_m^M \mathbb{P}_{iam}\mathbb{P}(\mu_i|a_i,S_i,D_i)$$

$$\mathbb{P}(\mu_i|a_i,S_i,D_i) = \frac{\mathbb{P}(\mu_m,a_i|S_i,D_i)}{\mathbb{P}(a_i|S_i,D_i)}$$

$$\mathbb{P}(\mu_m,a_i|S_i,D_i) = \mathbb{P}(a_i|S_i,D_i,\mu_m)\mathbb{P}(\mu_m|S_i,D_i)$$

$$\mathbb{P}(a_i|S_i,D_i,\mu_m) = \mathbb{P}_{iam}$$

$$\mathbb{P}(a_i|S_i,D_i) = \sum_m^M \mathbb{P}(a_i|S_i,D_i,\mu_m)\mathbb{P}(\mu_m|S_i,D_i)$$

Also in this specification $\mathbb{P}_{iam}$ and $\mathbb{P}(\mu_m|S_i,D_i)$ are obtained from the model given parameter estimates.

Estimation of salaries

The salary for a girl i residing in village l that chooses to work is computed using the OLS parameters of the following equation:

$$ln(w_{il}) = \gamma_0 + \gamma_1 ln(w_l) + \gamma_2 S_i + \gamma_3 age_i + \gamma_4 distmetro_l + \gamma_5 distcab_l + \omega_{il},$$

where w_l is the agricultural wage in community l, $distmetro_l$ is the distance (km) from the community where the girl resides to the nearest metropolitan area and $distcab_l$ is the distance (km) from the community where the girl resides to the main city at her municipality.

A sample selection problem arise in the estimation of the previous equation. The resulting

estimated salaries may be not a good approximation of the potential salaries for girls who do not work and for girls who do work but do not report their salaries. This problem is solved adding the assumption that the transitory shocks to potential earnings ω_{il} are not observed before the girl make her choice. Moreover, the variance of gilrs's salaries may be low since they are expected to work in low skilled homogenous agricultural activities.
In the second model proposed I also introduce as a regressor a municipality measure of class size.

Fit of the model: Main model

Table 2.7: Actual an predicted choices: Non-dropout observations (%)

Years of schooling	School		Work		Home	
completed	Actual	Predicted	Actual	Predicted	Actual	Predicted
0	99.2	97.7	0	1.2	.8	1
1	97	97.2	.8	1	2.3	1.7
2	97.4	98.1	1.1	.6	1.5	1.3
3	97.8	96.8	.7	.8	1.4	2.5
4	97.2	95.2	.6	1.1	2.1	3.7
5	94.8	95.1	1.2	1	3.9	3.9
6	72.6	81.8	2.9	3	24.4	15.2
7	97.4	85.9	.8	1.7	1.9	12.4
8	93.2	83.5	1.8	2	5.1	14.5
9	52.2	67.4	9.3	4.1	38.5	28.5
10	90.9	68.5	0	4.2	9.1	27.3
11	100	25.3	0	1.2	0	73.5

Table 2.8: Actual an predicted choices: Dropout observations (%)

Years of schooling	School		Work		Home	
completed	Actual	Predicted	Actual	Predicted	Actual	Predicted
0	96.3	73.4	1.2	6.9	2.4	19.7
1	33.3	54.8	0	8.2	66.7	37
2	56.9	55.3	4.6	8.5	38.5	36.2
3	44.2	45.7	5.8	10.2	50	44.1
4	75.9	63.1	5.7	4.6	18.4	32.3
5	42.1	54.6	9.6	7.3	48.2	38.1
6	25.5	27.7	8.5	7.1	66	65.2
7	66.7	43.9	6.1	6.7	27.3	49.5
8	53.3	42.1	6.7	4.5	40	53.4
9	22.2	22.3	14.8	5	63	72.7

Table 2.9: Comparison of results with related literature

	Non-dropout		Drop-out	
Author	Primary	Secondary	Primary	Secondary
Schultz (2004)	0.02	0.065	—	—
Valdes (2007)	0.01	0.061	-0.05	0.01
This paper	0.01	0.051	0.01	0.01

Estimation results: Main Model

Table 2.10: Estimates of structural parameters: Instantaneous Utilities

Variable	**Estimate**	**Standard Error**
Schooling utility		
age	-9.79	0.94
stock of education	2.72	0.39
dropout indicator dummy	-1.51	0.19
PROGRESA grant effect	3.52	1.61
mother stock of education	0.35	0.65
poor indicator dummy	0.16	0.23
availability of secondary school	0.39	0.10
class size	-2.43	0.47
Working utility		
wage	1.71	1.42
Staying at home utility		
age	-3.57	1.02
stock of education	2.72	0.40
number of babies at home	1.82	0.41
number of sisters between 12 and 16	-0.51	0.72
number of brothers	1.12	0.98
mother stock of education	-1.27	0.69
worker mother indicator dummy	-2.51	0.22
poor indicator dummy	-0.01	0.23
dropout indicator dummy	0.53	0.19
father at home indicator dummy	-0.43	0.18
constant	3.62	0.74

Log-likelihood = -31968.63, Discount Factor = 0.95

Table 2.11: Estimates of structural parameters: Stock of Education equation

Variable	**Estimate**	**Standard Error**
mother stock of education	0.27	0.58
poor indicator dummy	-0.02	-0.11
dropout indicator dummy	-0.46	-0.75
dropout indicator*age	0.44	0.42
availability of secondary school in 1997	-0.14	-0.25
availability of secondary school*age	0.30	0.30
class size in 1997	-1.79	-2.22
class size*age	3.17	2.73
unobserved heterogeneity load factor	0.04	1.07

Table 2.12: Estimates of structural parameters: Types and Types probabilities

	Estimate	**Standard Error**
Unobserved type effect		
Type 1	9.49	0.81
Type 2	5.50	0.69
Variables in types probabilities		
birth order	0.85	0.61
number of adults in the family	-1.09	-1.23
number of children in the family	-0.31	-0.47
father at home indicator dummy	-0.55	-1.60
worker mother indicator dummy	-3.83	-8.26
constant	5.05	7.98

Reference category is Type 2

Table 2.13: Types distribution: Non-dropout and Dropout (%)

	Type 1: High Type		Type 2: Low Type	
Choice	Non-Dropout	Dropout	Non-Dropout	Dropout
School	95.8	95.3	4.2	4.7
Work	76.8	86.4	23.2	13.6
Home	95.7	96.5	4.3	3.5

Table 2.14: Probability of passing grade s at age t for girls who fulfill the conditions required to receive the grant

	Grade (s)														
Age (t)	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
6	0.81	0.48	1.00	1.00	1.00	1.00	0.70	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
7	0.93	0.84	0.87	1.00	1.00	1.00	0.65	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
8	0.89	0.82	0.91	0.88	0.80	1.00	0.61	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
9	1.00	0.74	0.87	0.91	0.87	1.00	0.56	1.00	1.00	0.90	1.00	1.00	1.00	1.00	1.00
10	0.93	0.78	0.91	0.88	0.94	0.91	0.00	0.99	0.95	0.79	1.00	1.00	1.00	1.00	1.00
11	0.91	0.73	0.70	0.82	0.89	0.89	0.67	0.95	0.87	0.68	1.00	1.00	1.00	1.00	1.00
12	0.83	0.40	0.85	0.72	0.83	0.88	0.71	0.95	0.78	0.57	1.00	1.00	1.00	1.00	1.00
13	1.00	0.33	0.50	0.65	0.88	0.84	0.56	0.90	0.50	0.46	1.00	1.00	1.00	1.00	1.00
14	1.00	0.25	0.33	0.27	0.73	0.77	0.37	0.80	0.64	0.38	1.00	1.00	1.00	1.00	1.00
15	0.99	0.29	0.00	0.18	0.20	0.61	0.22	0.80	0.71	0.17	1.00	1.00	1.00	1.00	1.00
16	1.00	0.25	0.00	0.00	0.25	0.44	0.19	0.83	0.79	0.19	0.00	1.00	0.00	0.00	0.00
17	1.00	0.20	0.03	0.03	0.32	0.47	0.00	0.76	0.00	0.00	0.00	1.00	0.00	0.00	0.00

Table 2.15: Probability of passing grade s at age t for girls who do not fulfill the conditions required to receive the grant

	Grade (s)														
Age (t)	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
6	0.84	0.51	0.33	1.00	1.00	1.00	1.00	1.00	0.00	0.66	0.00	1.00	0.00	0.00	0.00
7	0.89	0.85	0.87	0.67	1.00	1.00	1.00	1.00	0.00	0.64	0.00	1.00	0.00	0.00	0.00
8	0.93	0.83	0.92	0.88	0.75	1.00	1.00	1.00	0.00	0.63	0.00	1.00	0.00	0.00	0.00
9	0.88	0.77	0.83	0.91	0.82	1.00	0.90	1.00	0.00	0.61	0.00	1.00	0.00	0.00	0.00
10	1.00	0.77	0.77	0.85	0.95	0.91	0.78	0.99	0.00	0.59	0.00	1.00	0.00	0.00	0.00
11	1.00	0.63	0.79	0.74	0.90	0.92	0.67	0.95	0.12	0.58	0.00	1.00	0.00	0.00	0.00
12	0.83	0.60	0.55	0.61	0.86	0.95	0.60	0.90	0.00	0.56	0.00	1.00	0.00	0.00	0.00
13	1.00	0.50	0.45	0.38	0.82	0.82	0.46	0.91	0.65	1.00	0.00	1.00	0.00	0.00	0.00
14	0.50	1.00	0.00	0.36	0.53	0.67	0.14	0.76	0.75	0.24	0.00	1.00	0.00	0.00	0.00
15	0.51	0.33	0.00	0.05	0.53	0.42	0.15	0.68	0.77	0.25	0.40	1.00	0.40	0.40	0.40
16	0.00	1.00	0.00	0.00	0.00	0.17	0.07	0.79	0.77	0.07	1.00	1.00	1.00	1.00	1.00
17	0.38	0.72	0.02	0.00	0.28	0.19	0.00	0.67	1.00	1.00	1.00	1.00	1.00	1.00	1.00

Correlation in school attendance among siblings

Tables 2.16 and 2.17 bellow show the OLS estimates of the coefficients of the following simple linear model:

$$y_{it} = \beta_0 + \beta_1^{'} X_{it},$$

where y_i is equal one if the girl is attending school, and X includes the age of the girl (*age*), the proportion of sisters aged below 12 attending school (*asgypr*), the proportion of sisters aged 12 or more years old attending school (*asgopr*), similar variables for the proportion of brothers attending school (*asbypr* and *asbopr*), a dummy equal one if the girl's father is living with her family (*f_hogar*), number of brothers between 6 and 16 years old (*boy*), number of sisters between 6 and 11 years old (*girl*11), number of sisters between 12 and 16 years old (*girl*16), number of sisters between 17 and 18 years old (*girl*16), number of children aged less than 5 years old (*baby*), a dummy equal one if the girl's mother works for a salary (*work_m*), and an indicator of the socioeconomic status of the girl's family (*poor*).

Table 2.16: Estimation results : Girls in primary school

Variable	**Coefficient**	**(Std. Err.)**
age	-0.043***	(0.002)
asgypr	0.701***	(0.041)
asgopr	0.137***	(0.010)
asbypr	0.046	(0.039)
asbopr	-0.005	(0.010)
f_hogar	-0.021	(0.023)
boy	-0.002	(0.006)
girl11	0.017**	(0.007)
girl16	-0.012	(0.008)
girl18	0.017*	(0.010)
baby	-0.003	(0.004)
work_m	-0.008	(0.019)
poor	0.010	(0.018)
Intercept	0.575***	(0.061)

Significance levels : $*$: 10% $**$: 5% $***$: 1%

Table 2.17: Estimation results : Girls in secondary school

Variable	**Coefficient**	**(Std. Err.)**
age	-0.041***	(0.005)
asgypr	0.007	(0.052)
asgopr	0.959***	(0.017)
asbypr	0.014	(0.055)
asbopr	0.038**	(0.016)
f_hogar	-0.009	(0.037)
boy	-0.010	(0.009)
girl11	0.000	(0.013)
girl16	-0.004	(0.011)
girl18	0.068***	(0.015)
baby	-0.007	(0.006)
work_m	0.030	(0.029)
poor	0.021	(0.023)
Intercept	0.576***	(0.110)

Significance levels : $*$: 10% $**$: 5% $***$: 1%

Fit of the model: Alternative model

Table 2.18: Actual an predicted choices: Non-dropout observations (%)

Years of schooling	School		Work		Home	
completed	Actual	Predicted	Actual	Predicted	Actual	Predicted
0	98.6	97.7	.7	13.6	.7	19.6
1	96.1	97.4	.8	12.7	3.1	20.9
2	96.7	97.9	1.1	11.5	2.2	19.4
3	97.1	97.3	1.1	9.8	1.8	20.9
4	95.9	95.6	1.4	8.9	2.7	24.5
5	93.3	93.2	1.8	7.7	4.8	26.1
6	65.7	88.2	3.9	6.9	30.4	30
7	93.4	84.7	2.1	5.9	4.6	29.3
8	90.5	77.1	2.8	5.6	6.8	34.4
9	41.5	66.1	10.5	6.2	48	46.6
10	93.8	63.2	4.2	5.1	2.1	44.9
11	88.9	53.7	0	4.7	11.1	54.6

Table 2.19: Actual an predicted choices: Dropout observations (%)

Years of schooling	School		Work		Home	
completed	Actual	Predicted	Actual	Predicted	Actual	Predicted
0	94.3	83.4	1.1	13.2	4.6	31.4
1	65.2	80.1	0	11.4	34.8	37.3
2	47.2	76.7	8.5	13.8	44.3	52.8
3	39.8	75.6	6.2	14.6	54	57.8
4	73.4	79	5.7	16.9	20.9	60.5
5	45.5	71.1	6.9	14.5	47.6	65.6
6	20.4	56.7	10.8	11.5	68.8	72.6
7	74.3	51.1	4.7	9.7	21.1	64.5
8	55.7	40.8	3.3	9	41	66.3
9	19.4	28.5	11.9	8.3	68.7	78.6
10	66.7	24.9	0	7.6	33.3	83.4

Estimation results: Alternative Model

Table 2.20: Estimates of structural parameters: Instantaneous Utilities and terminal value function

Variable	**Estimate**	**Standard Error**
Schooling utility		
drop	-1.35	0.15
existence of secondary	0.34	0.07
mother stock of education	2.02	0.27
graduation from primary	0.84	0.17
graduation from secondary	-0.43	0.15
repeater	-0.51	0.05
grant	4.09	1.04
Working utility		
wage	1.42	0.65
Staying at home utility		
age	5.51	0.58
number of children aged less than 5	0.36	0.40
number of girls aged 12 to 16	-0.81	0.62
number of boys aged 6 to 16	-0.06	0.48
poor	-0.01	0.09
worker mother	-0.37	0.12
father present at home	0.18	0.13
interaction babies*number of girls	-1.29	4.15
interaction babies*worker mother	-0.82	0.90
constant	-2.00	0.38
Terminal value function		
school	-2.20	7.25

Log-likelihood = -7293.73, Discount Factor = 0.95

Table 2.21: Estimates of structural parameters: Types and Types probabilities

	Estimate	**Standard Error**
Unobserved type effect		
Type 1	5.06	0.50
Type 2	1.77	0.49
Variables in types probabilities		
school	-6.12	0.41
drop	-1.23	0.17
birth order	2.23	0.60
number of adults	-1.14	0.35
number of children	-1.03	0.31
working mother	0.13	0.13
father present at home	0.96	0.23
constant	2.85	0.31

Reference category is Type 2

Chapter 3

Spillovers of Health Education at School on Parental Health Lifestyles

(joint work with M. Lucila Berniell and M. Dolores de la Mata)

3.1 Introduction

Although the prevalence of serious communicable diseases is very low in developed countries, conditions such as heart disease and cancer are the major causes of death. As a result, prevention increasingly involves lifestyles changes that reduce risk factors for these conditions (Kenkel, 2000). Interactions inside the family may crucially affect the "production" of such healthy lifestyles. As Kenkel (2000) points out, the family is often identified as being the unit of production of prevention practices, and this may be due to the fact that there is not an identifiable industry that produces prevention viewed broadly.

Previous literature on intra-household decision making and health-related decision has focused on the interactions between spouses.[1] On the other hand, the literature on intergenerational transmission of characteristics such as health, ability, education or income, has focused on the effects that parents's decisions may have on children's behaviors and outcomes about these same characteristics.[2] Nevertheless, little research has been done to evaluate the impact of children on parental health decisions.

The first goal of this paper is to assess the existence of spillover effects of Health Education (HED) received by children at school on their parents.[3] Health Education is likely to affect children's health behaviors.[4] However, it may be the case that parents are also affected

[1] For instance, see Clark and Etile (2006) on spousal correlation of smoking behavior.

[2] There is a large amount of studies quantifying the role of intergenerational transmission of parents characteristics and behaviors on children outcomes (Currie, 2009).

[3] According to the Centers for Disease Control and Prevention (CDC) "*Health Education is a planned, sequential, and developmentally appropriate instruction about Health Education designed to protect, promote, and enhance the health literacy, attitudes, skills, and well-being*" (Kann, Telljohann, and Wooley, 2007).

[4] As stated by WHO (1999), there are several reasons for promoting healthy behaviors through schools.

by the education about preventive health care that their kids acquire at school. Potential effects of HED on parental lifestyles are twofold. When the child receives new information from the HED school program he may also bring this new information to the household, which may affect parents's knowledge about particular healthy lifestyles. Also, a child who learns the benefits of healthy lifestyles at school and changes his behavior accordingly, may also affect his parents's behavior, for instance, by increasing the time they spend together doing healthy activities. In this work we focus in a particular type of healthy lifestyle, the regular practice of physical activity. A second goal of this paper is to discuss the plausible channels through which children receiving HED at schools may affect the frequency with which their parents do physical activity.

This work is related to two strands of literature. First, it is related to the literature on policy evaluation that focuses on measuring the spillover effects of policy interventions on non-targeted individuals, also called Indirect Treatment Effects (ITE). In our case, we focus on spillovers on parental behavior of a program targeted to children. There is little research assessing the existence of spillovers inside the household. One exception is Bhattacharya, Currie, and Haider (2006), who analyze the effects of the School Breakfast Program (SBP) in the US not only on targeted children but also on adult (not targeted) family members and find that the SBP improves diet quality even for family members who were not directly exposed to it. Jacoby (2002) and Shi (2008) also analyze the effects of policies directed to children on other -not eligible- members of the household but they do not find evidence of the existence of family spillover effects.[5] The explanations for the existence of family spillover effects in this literature operate to the extent that the particular program loosens the family budget constraint, so resources are freed up by the program and maybe redirected towards other household members. In contrast, in this paper we explore the existence of family spillovers occurring through non-budgetary channels. There is a related literature that quantifies ITE of some policies interventions arising at the community level and not at family level. For instance, Angelucci and Giorgi (2009) is a recent paper that evaluates the existence of spillover effects of an aid program (PROGRESA) on the entire local economies (villages) where the program was implemented. Also, Lalive and Cattaneo (2006) find that PROGRESA significantly increases school enrollment among non-eligible families in the villages and that this raise is driven by a peer effect. Miguel and Kremer (2004) using evidence from a randomized experiment show that a deworming program

Schools are an efficient way to reach school-age children and their families in an organized way and also the school is a place where students spend a great portion of their time, and where education and health programmes can reach them at influential stages in their lives.

[5]Jacoby (2002) analyzes the impact of a school feeding program in Philippines on caloric intake of targeted and non-targeted individuals inside the family, while Shi (2008) studies the existence of resources reallocation inside the household after a child receives a subsidy for covering the schooling fees in rural China. These two papers find evidence on the existence of intra-household flypaper effects, that is, there is no sizeable reallocation of resources after a child receives the subsidy.

substantially improved health and school participation among untreated children in both treatment schools and neighboring schools.

The second strand of literature related to our work consists of recent works evaluating the impact of particular aspects of health education at the school level on health outcomes and behaviors. Cawley, Meyerhoefer, and Newhouse (2007) find positive effects of physical education requirements on student physical exercise time but no effects on their BMI or the probability that the student is overweight. Also, McGeary (2009) assesses the effects of state-level nutrition-education program funding on individual-level Body Mass Index (BMI), the probability of obesity and the probability of above normal weight.[6] Her results suggest that this funding is associated with reductions in BMI and the probability of an individual having an above normal BMI.

In this work we exploit the quasi-experiment provided by the changes in the state-level HED requirements in elementary schools implemented between school-years 1999/2000 and 2005/2006 to see what the effects of these programs on parents's health lifestyles are.[7] Thus, we focus on a policy that does not imply any transfer of resources to children -the targeted individuals-, but it provides them with new information. Therefore, this paper contributes to the literature on ITE evaluation by providing evidence that HED affects the behavior of parents of children receiving this education at school and that these spillovers occur through non-market channels, such as information sharing.

To identify the spillover effects of HED policies we use a "differences-in-differences-in-differences" (DDD) strategy, that allows as to control for any systematic shocks to the parents's outcome behavior that are correlated with, but not due to, changes in HED policies. In the estimation procedure we exploit the time series and the cross sectional state variation, as well as the within state variation, since within each state there are groups exposed and not exposed to the treatment. The time dimension allow us to include year effects, to capture national trends in the frequency of physical activity. The variation across states allow us to control for systematic differences in the frequency of physical activity between people living in states that change their HED policies and people living in states that do not change their HED policies. And most importantly and key for our analysis, we can control for state-specific shocks over the period of analysis which are correlated with the decision of changing HED policies. By controlling for the existence of state-specific time trends we do not need to impose the usual "difference-in-difference" identifying assumption that the implementation of the policy in a given state at a particular point in time has to be exogenous, that is, not correlated with the outcome. Even if the reason to change the HED policy is related with the particular trend in the level of physical activity observed in

[6]Notice that this funding is allocated to public-school systems, public-health clinics, as well as public-service announcement and advertisements, so her analysis exceed the effects of education at school, thus she constructs her estimates for the entire population in each state.

[7]Further details on these policy changes on Section 3.2.

the state, we still are able to isolate the effect of HED because we have a key comparison group for treated individuals in a given state: non-treated individuals that reside in the same state during the same time period.

We use data from the Panel Study of Income Dynamics (PSID) for the period 1999-2005 merged with data on state HED reforms from the 2000 and 2006 School Health Policies and Programs Study (SHPPS). PSID is a nationally representative longitudinal survey of U.S. individuals (men, women, and children) and the family units in which they reside. PSID gathers health-related information such as health status, health behaviors, health insurance, and health care expenditures. It also provides detailed information about family income as well as information on family composition and demographic variables, including age of family members, race, marital status, employment status and education. SHPPS is a nationwide survey that was designed to gather information on the characteristics of each health education program at the state, district, school, and classroom levels and across elementary, middle, and high educational levels.

We find evidence of a positive effect of HED at elementary school on fathers's frequency of light physical activity. There are two ways of rationalizing our results. First, it can be argued that fathers have less information about the importance of preventive health behavior and healthy lifestyles for obtaining good health outcomes. Thus, the arrival of new information about healthy lifestyles coming from the HED imparted at school is likely to have a stronger effect on fathers, which are the individuals with the relative smaller stock of information on health preventive care. The second idea has to do with the role models that mothers and fathers play for their children. Parents usually spend more time with their children doing gendered activities. Thus, fathers are more likely to do stereotypically male activities with their children, among which we can include physical activity. Accordingly, the impact of HED reforms on physical activity is also expected to appear for fathers rather than for mothers. Our results suggest that fathers are those that are affected the most by the HED reforms, since they change their level of physical activities after these reforms are in place while mothers do not alter their behavior in this respect. We also explore the existence of the information sharing channel by analyzing the differential impact of HED reforms on individuals with low and high education levels. The idea here is that the less information parents have (less educated parents), the more knowledge will they gain with the introduction of HED at schools. Although we do find the expected signs in the coefficients of interest -individuals with low levels of education are those affected by the HED reforms-, the estimates are in general not statistically significant.

The existence of spillovers of HED on parental lifestyles indicates that the interaction between children and parents play a role in the formation of healthy lifestyles inside the household and that this fact must be taken into account to properly design policy interventions aiming to increase the acquisition of healthy lifestyles in a given community.

3.2 Health Education Policies

3.2.1 SHPPS

The Centers for Disease Control and Prevention (CDC) conducts the SHPPS every 6 years since 1994. This nationwide survey is designed to gather information on the characteristics of each school health program at the state, district, school, and classroom levels and across elementary, middle, and high schools. SHPPS analyzes eight components, including HED.[8]
One important data limitation in SHPPS is that it is not possible to know the exact date at which the HED reforms took place in each state. However, we do know the changes between the two survey years, 2000 and 2006. The data collection in SHPPS starts in January of the corresponding year, which implies that SHPPS 2000 gathers information on the school-year 1999/2000 and SHPPS 2006 information on the school-year 2005/2006.
We use the information of the HED component from the SHPPS state-level survey. The state-level questionnaires were completed by state education agency personnel in all 50 states plus the District of Columbia in both years.

3.2.2 Policies on HED: topics and enforcements

HED policies have several dimensions, which we collapse in two variables. The first variable refers to the number of specific health education topics that the state requires that elementary schools must teach. We select five health topics that consist of information that could be transmitted from children to parents and which could also contain new information for parents. We exclude from our final list of topics those that are related to sexual education, and HIV/violence/suicide/injury prevention.
The second category consists of the number of specific policies implemented in order to guarantee the effective implementation of HED education requirements. We broadly refer to each one of these requirements as enforcements. Table 3.1 describes the five topics and the specific state requirements enforcing HED which we consider in our analysis. The full list of topics and requirements can be consulted in Table 3.15 in the Appendix.
According to Kann, Brener, and Allensworth (2001), who analyze the changes in HED requirements using SHPPS, between years 1994 and 2000 health education policies and programs generally remained stable at the state level. For instance, the percentage of states requiring schools to teach alcohol and other drug use prevention, nutrition and dietary behavior, HIV prevention, accident or injury prevention, and physical activity and fitness remained fairly constant. However, positive changes were detected since 2000 at the state level.[9] Tables 3.12, 3.13, and 3.14 in the Appendix summarize the HED reforms in each

[8]The remaining seven components are Physical education and activity, Health services, Mental health and social services Nutrition services, Healthy and safe school environment, and Faculty and staff health promotion.
[9]According to Kann, Telljohann, and Wooley (2007), fewer increases in HED requirements were noted

Table 3.1: HED topics and enforcements

Topic Code	Description
1	Alcohol- or Other Drug-Use Prevention
2	Emotional and Mental Health
3	Nutrition and Dietary Behavior
4	Physical Activity and Fitness
5	Tobacco-Use Prevention
Enforcement code	Description
1	State requires districts or schools to follow national or state health education standards or guidelines
2	State requires students in elementary school to be tested on health topics
3	State requires each school to have a HED coordinator

of the two dimensions -topics and enforcements- in all states between 1999 and 2005. We do not include in our analysis those states that experimented a reduction in the number of topics and/or enforcements. For this reason we dropped the following states: California, Indiana, Iowa, Louisiana, Massachusetts , Minnesota, Mississippi, Missouri, Nevada, New Hampshire, New Jersey, New York, Ohio, Oklahoma, Oregon, and Wisconsin. There is no information in SHPPS explaining the reasons for the reverse in the application of the policy. It seems plausible that they have decided to reduce the number of topics and/or enforcements for budgetary reasons, parents's demands, and/or introduction of other topics not related with HED. It seems less likely that these states reversed the policy because of it does not achieve its goal, since there is no formal evaluation of the effects of HED on children and/or their families. In section 3.4.1.2 we discuss how these selection in the sample may potentially affect our results and we provide evidence showing that in fact there is no bias in our estimation of the spillover effects of HED on the other states. The results in the existence of HED spillover effects that we present in what follows have to be thought as spillover effects of only positive changes in HED.

3.3 Data and Identification Strategy

Our goal is to identify the spillover effects of elementary school HED policies implemented in certain states -the "experimental states"- on the behavior of parents of children of elementary-school-age -the treatment group. Identifying this effect requires, as stated in Gruber (1994), controlling for any systematic shocks to the parents's outcome behavior in

at the school and classroom levels. Nonetheless, it is possible that the increased state and district efforts to improve health education and professional preparation requirements may have provided the support schools needed to at least maintain if not improve their health education activities.

the experimental states that are correlated with, but not due to, changes in HED policies. To do so we use a "differences-in-differences-in-differences" (DDD) approach that allows us to exploit the variation of HED policies across time (time dimension), across states (geographical dimension) and across different groups of individuals residing in the same state (individual dimension). That is, we compare the treatment individuals in experimental states to a set of control individuals in those same states and measure the change in the treatments' relative outcome, relative to states that did not change HED policies. The identifying assumption requires that there is no contemporaneous shock affecting the relative outcome of the treatment group in the same state-years as the change in the HED policy.

We analyze the impact of HED policies on the behavior of adults who have children attending elementary school using data from the PSID. It is a nationally representative longitudinal survey of U.S. individuals (men, women, and children) and the family units in which they reside. Since 1999 PSID has expanded the set of health-related questions for family units' heads and wives, gathering information such as health status, health behaviors, health insurance, and health care expenditures. We concentrate on the indirect effect of HED policies on individuals' level of physical activity, that is one of the health behaviors reported in this survey. PSID also provides detailed information about family income as well as information on family composition and demographic variables, including age of family members, race, marital status, employment status and education. PSID covers all states.

We base our analysis on the PSID survey years 1999 and 2005, using 1999 as the pre-reform period. Given that the SHPPS does not provide the exact year in which HED reforms were introduced at the state level, we are not able to use the additional data available from PSID for periods 2001 and 2003. The DDD design we use to identify the effect of interest does not require the use of a panel, but the identification is improved by using longitudinal data. Even though we do not specify a model for panel data, in our final sample about 90% of the observations correspond to individuals in a panel.

Treated individuals, those exposed to HED policies, are adults who have children of elementary-school-age (6-10). PSID does not provide information on whether a child is attending elementary school. However, it provides information on the age of children, allowing us to determine if the individuals have children of school-age.[10]

The control group includes individuals who were unaffected by state HED requirements. We use as control group adults who have children of elementary-school-age (6-10) living in states that did not implement HED policies and those residing in states that even when having HED requirements did not introduce any reform on this policy. Furthermore, to control

[10]Notice that the drop-out rate in elementary school is very low in the US, contrary to the case of secondary education. Therefore, by knowing the age of the children we are able to know whether the child is or not attending elementary education.

Table 3.2: State groups, by policy implemented by 1999 or by 2005, and by policy reforms

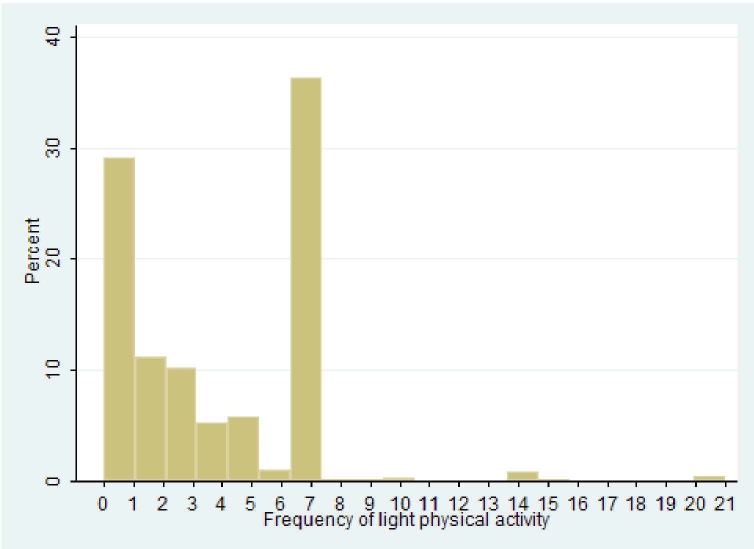
Group		Topics 1999	Topics 2005	Enforcements 1999	Enforcements 2005	Num. of states	Observations
Non-Experimental	S_0	no	no	no	no	3	350
	S_1	no	no	yes	yes	1	157
	S_2	yes	yes	yes	yes	9	2,250
Experimental	S_3	yes	yes	no	yes	1	347
	S_4	yes	yes	yes	yes (increased)	5	943
	S_5	no	yes	no	yes	3	681
	S_6	no	yes	yes	yes	2	685
	S_7	yes	yes (increased)	yes	yes	3	623
	S_8	yes	yes (increased)	yes	yes (increased)	2	460
Total						29	6,496

for possible correlation of state HED policies with unmeasured state trends in health and health behaviors, we use a sample of adults who have children aged 17 or younger but not of elementary-school-age as a comparison group. We group the non-treated individuals in three different control groups. We include in the Treatment-Non-Experimental group (Control 1) individuals with children of elementary-school-age residing in non-experimental states. The Control-Experimental group (Control 2) includes individuals with children above elementary-school-age residing in experimental states. Finally, in the Control-Non-Experimental group (Control 3) we include individuals with children above elementary-school-age residing in non-experimental states.

The HED reforms that took place between 1999 and 2005 were dissimilar across states since some of them have modified one, two, or none of the policy dimensions we are analyzing. According to the observed type of HED policy reform, we classify states in nine groups as shown in Table 3.2, in which states are sorted taking into account whether they have topics and enforcements in both years and whether they have increased or maintained the number of topics and enforcements between survey years. Table 3.2 also groups states in two broad sets: experimental and non-experimental states. The experimental states are those states that have introduced some HED reforms -by requiring for the first time topics and/or enforcements or by increasing the number of topics and/or enforcements on HED- between 1999 and 2005. There are six different types of treatments (policies) that define six types of states, that we name S_3 to S_8. On the contrary, non-experimental states are those that have not introduced any change in their HED requirements in this period, which we name S_0 to S_2.

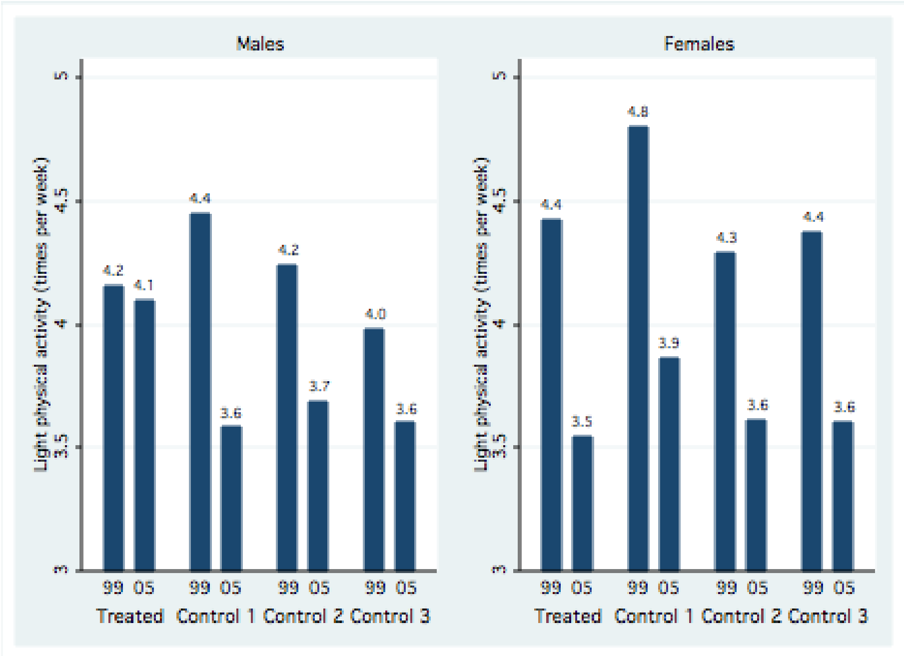
Our final sample consists parents of children under the age of 18 years old, women and men, that were part of the PSID in 1999 and/or in 2005. We have a database of 6496 observations distributed across 9 groups of states, as described in Table 3.2 and Table 3.16 in the Appendix. It is worth to notice that for most of the individuals we also have her/his couple in the sample. Given the way in which PSID is designed, for some of the individuals we also have another relative in the sample, for instance her/his siblings. This feature of our data makes it important to control for cluster at the family level in all the regressions. We use light physical activity as the outcome variable. PSID respondents are asked about their physical activity habits in two questions. They first answer how often they do light physical activity and then they report the time unit that allows to measure the frequency of these activities (daily, weekly, monthly or annually). There was a change in the question between 1999 and 2005 that we discuss in section 3.4.1.4 Based in these two questions we construct a variable that indicates the number of times per week individuals do light physical activity. It is an ordinal variable that assumes 44 different values, from 0 to 21. Its histogram is presented in Figure 3.1. There two well-differentiated mass points -at values 0 and 7- can be identified. Also, more than 10% of the total number of observations lies in the interval (0,2] and around 22% are included in the interval [2,7). This inspection of the outcome variable makes it clear that we cannot treat it as a continuous variable in the specification of our empirical model.

Figure 3.1: Outcome variable's histogram: frequency of light physical activity (times per week)



In Figure 3.2 we show the average weekly frequency of light physical activity by gender in 1999 and 2005 for treated and control individuals, pooling all groups of states. We observe a downward trend in all groups for both genders. In particular, for the groups of treated individuals the frequency of light physical activity goes down. This simple Before-After estimator is telling us that HED policies have a negative impact on the outcome of interest. However, this estimate is obviously biased given the fact that the average of the outcome variable in the Treatment-non-Experimental group (Control 1) has also a downward trend which is even higher. Exploring gender differences we can see that Females in the Treatment-Experimental group (Treatment) present a larger drop in the frequency of light physical activity than that one observed for males in the same group. This fact suggests the potential need for taking into account gender differences when estimating the effect of HED policies.

Figure 3.2: Average frequency of light physical activity (times per week) by treatment/control groups and by gender, in 1999 and 2005

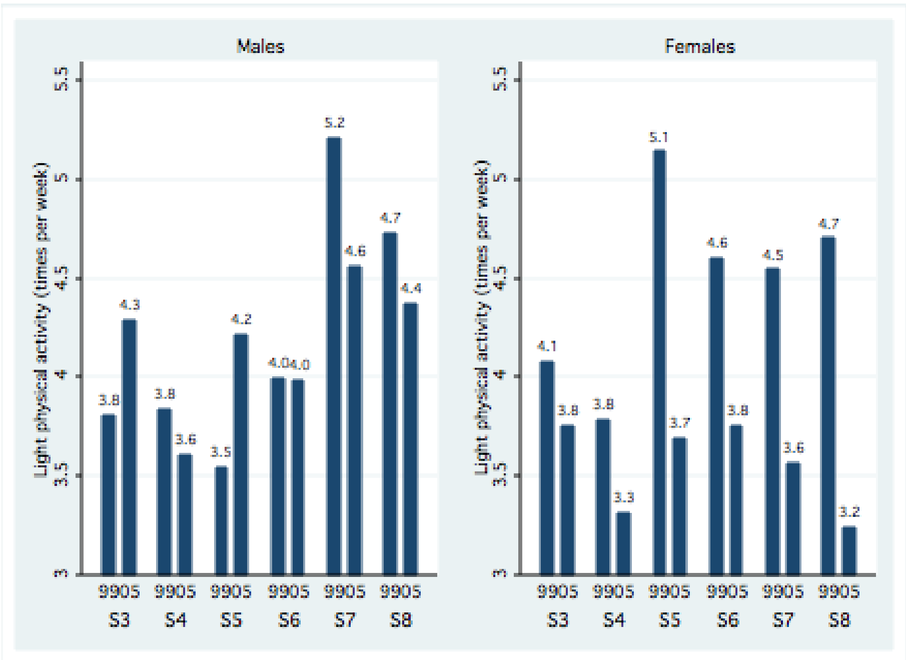


Treated: individuals with children of elementary school age in experimental states. Control1: individuals with children of elementary school age in non-experimental states. Control2: individuals without children of elementary school age in experimental states. Control3: individuals without children of elementary school age in non-experimental states.

As we discuss above, the implementation of HED policies between 1999 and 2005 was not homogenous across states. For this reason we may expect differences in the temporal evolution of the outcome of interest across the 8 groups of states previously defined. We show the average frequency of light physical activity by gender and by group of states in Figure 3.3. We see that for males residing in states belonging to groups S_3 and S_5 the frequency of light physical activity raises between 1999 and 2005. These upward trends suggest the existence of a positive effect of HED policies on the outcome variable of males in these two particular groups of states.

To assess the existence of differences in observable characteristics among individuals under treatment compared to those not exposed to HED policy reforms, in Table 3.3 we report descriptive statistics -in year 1999- of the outcome variable, and other demographic and socioeconomic characteristics. We present the descriptive statistics for the four mutually exclusive groups: Treatment-Experimental, Treatment-Non-Experimental, Control-Experimental, and Control-Non-Experimental.

Figure 3.3: Average frequency of light physical activity (times per week) by treatment groups and by gender, in 1999 and 2005



The type of policies corresponding to each group of states S are as follows. S3: topics unchanged and implementation of enforcements; S4: topics unchanged and increase the number of enforcements; S5: implementation of topics and enforcements; S6: implementation of topics and enforcements unchanged; S7: increase the number of topics and enforcements unchanged; S8: increase the number of topics and enforcements.

Table 3.3: Descriptive statistics treatment and control groups, 1999

	Full Sample	Treatments in Exp. states	Treatments in Non-Exp. states	Controls in Exp. states	Controls in Non-Exp. states
	(1)	(2)	(3)	(4)	(5)
Light phy. act.	4.331	4.309	4.618	4.281**	4.216**
(sd)	(3.062)	(3.012)	(3.310)	(2.925)	(3.105)
Heavy phy. act.	2.004	2.170	2.103	1.786	2.051
(sd)	(2.578)	(2.707)	(2.705)	(2.375)	(2.594)
Smoke	0.242	0.240	0.287***	0.228	0.230
(sd)	(0.428)	(0.428)	(0.453)	(0.420)	(0.421)
BMI	26.830	27.224	26.845	26.539	26.790
(sd)	(5.588)	(5.781)	(5.545)	(5.318)	(5.752)
Health Excellent	0.271	0.226	0.277***	0.271	0.315**
(sd)	(0.445)	(0.419)	(0.448)	(0.445)	(0.465)
Female	0.567	0.574	0.585	0.559	0.560
(sd)	(0.496)	(0.495)	(0.493)	(0.497)	(0.497)
Age	36.714	36.182	35.558	37.126***	37.563***
(sd)	(8.158)	(6.275)	(6.641)	(9.344)	(9.037)
Education	12.931	12.897	12.686***	12.989	13.067
(sd)	(2.366)	(2.335)	(2.265)	(2.294)	(2.553)
Num. of Children	2.341	2.608	2.690	2.084***	2.150***
(sd)	(1.232)	(1.319)	(1.248)	(1.064)	(1.223)
Children in Elementary	0.432	1.000	1.000	0.000	0.000
(sd)	(0.495)	(0.000)	(0.000)	(0.000)	(0.000)
White	0.534	0.519	0.473***	0.563*	0.554
(sd)	(0.499)	(0.500)	(0.500)	(0.496)	(0.497)
Married	0.754	0.764	0.697***	0.767	0.766
(sd)	(0.431)	(0.425)	(0.460)	(0.423)	(0.424)
Unemployed	0.035	0.050	0.037	0.026**	0.030
(sd)	(0.184)	(0.218)	(0.188)	(0.160)	(0.170)
Retired	0.004	0.001	0.000	0.005**	0.007***
(sd)	(0.063)	(0.037)	(0.000)	(0.074)	(0.086)
Disabled	0.019	0.014	0.018	0.021***	0.024***
(sd)	(0.137)	(0.117)	(0.134)	(0.142)	(0.152)
Labor income pc	14,488	13,120	12,161	14,792***	17,227***
(sd)	(14,946)	(13,981)	(10,410)	(14,877)	(18,083)
Total income pc	17,160	15,800	13,594	17,366***	20,925***
(sd)	(17,622)	(17,322)	(11,292)	(16,416)	(22,056)
N	2,804	720	491	918	675

Stars in columns (3) to (5) show statistical significance of differences in proportion or distribution of the referred variable, between the referred control group and the treatments in experimental states group (column (2)). We perform tests of difference in proportion for the dummy variables Smoke, Health Excellent, Children in Elementary, White, Married, Unemployment, Retired, and Disabled. We perform tests of differences in distribution for the categorical variables Light phy. act., Heavy phy. act., Age, Education, and Number of Children, and for the continuous variables BMI, Labor income pc, and Total income pc. Significance levels: * = 10%; ** = 5%; *** = 1%.

We find evidence of statistically significant difference in some observable characteristics between the Treatment-in-Experimental group and each of the three control groups. These differences are less important between Treatment-in-Experimental and Treatment-in-Non-Experimental states. That is, individuals with children of elementary-school-age are reasonable alike in some observable characteristics regardless their exposition to changes in HED policies. In terms of our DDD identification strategy this means that treated individuals have a close comparison group in the third dimension (individual dimension). To account for the observable differences between treatment and control individuals we use a regression framework in the estimation of the effect of interest.

3.3.1 DDD estimation in a simple linear model

Table 3.4 presents the DDD estimate of the effect of changes in the HED policy on fathers's behavior for a particular group of states, S_5, in which both topics and enforcements where implemented between 1999 and 2005 for the first time. In this section we treat the outcome variable, number of times per week individuals do light physical activity, as if it were a continuous variable. With this assumption we cannot make valid quantitative interpretations of the effect of the policy, but we can still make inference regarding the sign of the effect.

The top panel compares the change in the frequency of physical activity for fathers with children of elementary-school-age residing in states S_5 to the change for fathers with children of elementary-school-age in non-experimental states. Each cell contains the mean average frequency of light physical activity for the group labeled on the axes, along with the standard errors and the number of observations. The Before-After estimate (Δ_E^T) of the effect is presented in the third column. There was a non-significant increase in the frequency of light physical activity for fathers with children of elementary-school-age in experimental states, compared with a significant fall in the frequency of light physical activity for fathers with children of the same age in other states. Thus, the diff-in-diff estimator ($\Delta_E^T - \Delta_{NE}^T$), reported in the bottom part of the upper panel, is positive and significant; the relative frequency of light physical activity of fathers with children of elementary-school-age has risen.

If there were a different shock common to the experimental states that affected fathers's frequency of physical activity, the previous estimator does not identify the spillover effects of the implementation of HED policies. In the middle panel of Table 3.4 we perform the same exercise for the groups of fathers with children above elementary-school-age. For those groups we find a fall in the relative frequency of light physical activity in the experimental states, relative to the other states. Although not significant, this suggests that it may be important to control for state-specific shocks in estimating the impact of HED policies.

Taking the difference between the two panels of Table 3.4, there is a significant increase in the relative frequency of physical activity for fathers of children in elementary-school-

Table 3.4: DDD estimator for males in S_5

	Before HED change	**After HED change**	**Time difference**	
A. Treatment individuals: with children in elem				
Experimental states	3.588	4.228	0.640	Δ_E^T
	(0.458)	(0.555)	(0.721)	
	[46]	[47]	[93]	
Non-experimental states	4.379	3.632	-0.747**	Δ_{NE}^T
	(0.239)	(0.214)	(0.320)	
	[204]	[210]	[415]	
Difference in difference			**1.387***	
			(0.789)	
			[508]	
B. Control Individuals: without children in elem				
Experimental states	4.164	3.122	-1.042**	Δ_E^C
	(0.308)	(0.294)	(0.433)	
	[79]	[109]	[188]	
Non-experimental states	3.995	3.637	-0.358	Δ_{NE}^C
	(0.168)	(0.149)	(0.230)	
	[297]	[474]	[771]	
Difference in difference			**-0.684**	
			(0.491)	
			[959]	
$\mathbf{DDD} = (\boldsymbol{\Delta}_{\mathbf{E}}^{\mathbf{T}} - \boldsymbol{\Delta}_{\mathbf{NE}}^{\mathbf{T}}) - (\boldsymbol{\Delta}_{\mathbf{E}}^{\mathbf{C}} - \boldsymbol{\Delta}_{\mathbf{NE}}^{\mathbf{C}})$			**2.071****	
			(0.929)	
			[1,467]	

Significance levels: * = 10%; ** = 5%; *** = 1%. Cells contain mean frequency of light physical activity for the group identified. Standard errors are given in parentheses, sample sizes are given in square brackets. The non-experimental states are groups of states S0, S1 and S2.

age in the states that implemented HED requirements, compared to the change in relative frequency of physical activity in non-experimental states. This statistically significant DDD estimate provides some evidence on the existence of spillovers of HED on fathers's physical activity. However, its quantitative interpretation is problematic since the support of the outcome variable is not the real line. We discuss in the next Section how the DDD design can be expressed within a regression framework in which we can explicitly model the discrete support of the outcome variable as well as we can control for observed characteristics.

3.3.2 Empirical model

Our outcome variable, the number of times per week individuals do light physical activity, is an ordinal variable with clearly distinguished mass points as it was shown in Figure 3.1 above. For this reason, we redefine it as a variable with 5 categories and we estimate ordered Probit specifications. We perform several robustness checks over the definition of the dependent variable that are discussed in Section 3.4.1.1. The categories for the outcome variable are the following:

$$y_i = \begin{cases} 0 & \text{0 times} \\ 1 & \text{more than 0 and less than 3 times} \\ 2 & \text{from 3 times until less than 7 times} \\ 3 & \text{7 times} \\ 4 & \text{more than 7 times} \end{cases}$$

The latent variable version of the model with six types of treatment has the following form:

$$\begin{aligned} y^*_{itj} = \beta_0 + \beta_1\tau_t + \beta_2 elem_i + \sum_{k=1}^{8}\beta_{3,k}S_k + \\ \beta_4(elem_i \times \tau_t) + \sum_{k=1}^{8}\beta_{5,k}(S_k \times \tau_t) + \sum_{k=1}^{8}\beta_{6,k}(S_k \times elem_i) + \\ \sum_{k=3}^{8}\beta_{7,k}(S_k \times elem_i \times \tau_t) + \beta_8 X_{itj} + u_{itj}, \end{aligned} \tag{3.1}$$

where $i = 1...N$ indexes individuals, $t = 0, 1$ indexes time (0=before policy, 1999; 1=after policy, 2005), $j = 1...29$ indexes states and $k = 1, .., 8$ indexes state groups; τ_t is a dummy variable, equal one in 2005; S_k is a dummy equal one if the individual resides in the state j that belongs to group k; $elem_i$ is a dummy equal one if individual i has children of elementary school (children aged between 6 and 10 years old); and X_{itj} is a set of observable individual characteristics including age, race, gender, marital status, number of children, children of high school-age, education level, employment status, total family income level and state of residence.

This specification controls for time trend in the dependent variable (β_1), for time-invariant characteristics of the treatment group (β_2), and for time-invariant characteristics of the different groups of states ($\{\beta_{3,k}\}_{k=1}^{8}$). The second-level interactions control for changes over time for the treatment group nationwide (β_4), changes over time in each group of experimental and non-experimental states ($\{\beta_{5,k}\}_{k=1}^{8}$), and time-invariant characteristics of the treatment group in each group of states ($\{\beta_{6,k}\}_{k=1}^{8}$). The third-level interactions ($\{\beta_{7,k}\}_{k=3}^{8}$) capture all variation in frequency of physical activity specific to the treatments (relative to controls) in the experimental states (relative to the non-experimental) in the year after the HED requirements changed. These are the DDD estimates. Given the existence of different time trends on the frequency of light physical activity between females and males observed in Figure 3.2 the model we estimate also interacts the policies with a dummy variable for gender.
The model with interactions by gender has the following form:

$$\begin{aligned} y^*_{itj} = {} & \beta_0 + \beta_1\tau_t + \beta_2 elem_i + \sum_{k=1}^{8}\beta_{3,k}S_k + \beta_4 gender_i + \beta_5(\tau_t \times gender_i) + \beta_6(elem_i \times gender_i) + \\ & \sum_{k=1}^{8}\beta_{7,k}(S_k \times gender_i) + \beta_8(elem_i \times \tau_t) + \beta_9(elem_i \times \tau_t \times gender_i) + \sum_{k=1}^{8}\beta_{10,k}(S_k \times \tau_t) + \\ & \sum_{k=1}^{8}\beta_{11,k}(S_k \times \tau_t \times gender_i) + \sum_{k=1}^{8}\beta_{12,k}(S_k \times elem_i) + \sum_{k=1}^{8}\beta_{13,k}(S_k \times elem_i \times gender_i) + \\ & \sum_{k=3}^{8}\beta_{14,k}(S_k \times elem_i \times \tau_t) + \sum_{k=3}^{8}\beta_{15,k}(S_k \times elem_i \times \tau_t \times gender_i) + \beta_{16}X_{itj} + u_{itj}. \end{aligned} \tag{3.2}$$

The DDD estimates in this model are $\beta_{14,k}$ for males and $\beta_{14,k} + \beta_{15,k}$ for females. If the coefficient $\beta_{15,k}$ is significantly different from zero, then there is evidence of the differential impact of HED policies among fathers and mothers. Morover, if $\beta_{14,k}$ is positive and significant and $\beta_{15,k}$ is negative and significant, then there is evidence that our hypothesis on the higher effect of HED for those individuals with lower stock of information holds. We estimate the parameters of interest by Maximum-Likelihood and we compute standard errors corrected for cluster at family level. A report of the estimated coefficients can be found in Table 3.18 in the Appendix.

3.4 IATE estimates

In this Section we report the estimates of the Indirect Average Treatment Effect (IATE). The IATE is computed as the average value of the indirect treatment effect across treated individuals.
Let $\hat{\pi}_k = (\beta_0, \beta_1, \beta_2, \{\beta_{3,k}\}_{k=1}^{8}, \beta_4, \{\beta_{5,k}\}_{k=1}^{8}, \{\beta_{6,k}\}_{k=1}^{8}, \beta_8)$ be the vector of estimated parameters without including the parameters that measure policy effects ($\{\beta_{7,k}\}_{k=3}^{8}$). Sim-

ilarly, let Z_{itj} be the vector of variables for the individual i at time $t = 2005$ residing in state j without including the third level interaction variable $S_k \times elem_i \times \tau_t$.

The IATE across treated individuals of the policy change applied in the group of states S_k corresponding to the category m of the outcome variable is computed using the following expression:

$$\sum_{i:\ elem=1} [\Phi(\alpha_m - \hat{\pi}_k Z_{kit} - \beta_{7,k}(S_k \times elem_i \times \tau_t)) - \Phi(\alpha_m - \hat{\pi}_k Z_{kit})]/N_{kt}, \tag{3.3}$$

where Φ stands for the normal distribution function, α_m is the cutoff point corresponding to the m category in the ordered Probit model of the dependent variable, and N is the number of treated individuals in the group of states S_k at time $t = 2005$.

We report in Table 3.5 the IATE for the six different types of treatment. A complete report of the estimated IATE including 95% confidence intervals can be found in Table 3.19 in the Appendix.

Table 3.5: IATE across treated individuals. Outcome variable: frequency of light physical activity (times per week)

		MALE						FEMALE					
GROUP	**POLICY**	**0**	**(0,3)**	**[3,7)**	**7**	**>7**	**# of obs.**	**0**	**(0,3)**	**[3,7)**	**7**	**>7**	**# of obs.**
S_3	Topics unchanged	-0.086	-0.049	0.005	0.117	0.012	25	-0.124	-0.033	0.028	0.122	0.007	39
	Implement Enforcements	(0.103)	(0.047)	(0.021)	(0.121)	(0.016)		(0.121)	(0.031)	(0.032)	(0.105)	(0.010)	
S_4	Topics unchanged	-0.035	-0.013	0.006	0.039	0.003	69	-0.025	-0.006	0.006	0.025	0.002	102
	Increase Enforcements	(0.078)	(0.027)	(0.016)	(0.083)	(0.009)		(0.067)	(0.017)	(0.016)	(0.064)	(0.005)	
S_5	Implement Topics	-0.172*	-0.059**	0.032	0.186**	0.014	47	0.069	0.042	-0.002	-0.097	-0.012	84
	Implement Enforcements	(0.099)	(0.029)	(0.027)	(0.089)	(0.011)		(0.050)	(0.034)	(0.010)	(0.074)	(0.013)	
S_6	Implement Topics	-0.097	-0.038	0.016	0.111	0.008	63	0.032	0.018	-0.002	-0.044	-0.005	69
	Enforcements unchanged	(0.089)	(0.029)	(0.021)	(0.090)	(0.011)		(0.064)	(0.037)	(0.009)	(0.087)	(0.011)	
S_7	Increase Topics	-0.037	-0.029	-0.004	0.060	0.010	48	-0.054	-0.021	0.008	0.061	0.005	56
	Enforcements unchanged	(0.070)	(0.046)	(0.013)	(0.101)	(0.018)		(0.077)	(0.027)	(0.016)	(0.080)	(0.009)	
S_8	Increase Topics	-0.018	-0.013	-0.001	0.028	0.004	29	0.046	0.016	-0.009	-0.049	-0.003	47
	Increase Enforcements	(0.080)	(0.052)	(0.013)	(0.117)	(0.018)		(0.090)	(0.032)	(0.019)	(0.097)	(0.009)	

Standard errors computed by bootstrap with 2000 replications. Cluster is set at family level.Significance levels: $* = 10\%; ** = 5\%; *** = 1\%$. The regression includes the following covariates: age, race, gender, marital status, number of children, children of high school-age, education level, employment status, total family income level and state of residence.

We found evidence of a positive effect of HED education at elementary school on parents's frequency of light physical activity. Requiring topics and enforcements for the first time (S_5 group of states) raises fathers's frequency of doing physical activity. In particular, the probability of not doing physical activity (category 0) for a father affected by this policy is 17 percentage points lower than a comparable father not affected by the policy. Moreover, the probability of doing physical activity 7 times per week (category 3) is 18 percentage points higher. The effect on mothers's frequency of physical activity is never statistically significant, but interestingly the signs are the opposite of those found for fathers.
From these probabilities it is possible to compute a rough measure of the plausible savings on health care expenditures caused by physical inactivity. The costs associated with inactivity and obesity accounted for some 9.4% of the national health expenditure in the US in 1995, and in Canada physical inactivity costs about 6% of total health care spending (?). For the case of the US, reducing the probability of fathers being physical inactive by 17% may imply cost savings of about 0.8% of national health expenditure.[11] This is of course a very rough assessment of plausible economic benefit of introducing HED at schools because of two main reasons. First, this measure comes from aggregate health care costs. Second, looking only at the costs of physical inactivity to the health care system grossly underestimates their total cost: production losses from work absenteeism and also from the costs of informal care contribute greatly to the overall financial burden.
We now comment on the direction of the effect obtained for the other types of treatment, but in all cases the estimated effects are not statistically significant. For males, the estimated effect has the same sign in all groups of states. HED policies on average seem to increase the frequency of light physical activity for fathers who have children of elementary school age. For females, the effect of the different types of policies is not homogeneous. The difference may be explained by the timing in the implementation of topics. The estimated IATEs suggest a negative effect of HED on mothers residing in groups of states S_5, S_6, and S_8. In most of the states belonging to these groups the number of topics in 1999 was 0, and only one state has 2 topics implemented by that time. We find a positive sign of the estimated HED effects for mothers living in states included in groups S_3, S_4, and S_7. In all of them, the number of topics in 1999 was at least 4. Thus, it seems that the positive effect of HED on mothers generated by the implementation of topics is reflected in the frequency of light physical activity after a longer period of time than for fathers.
We conclude that there are positive spillovers of HED reforms on fathers's probability of doing physical activity, while for mothers we do not find a statistically significant effect of these reforms.

[11] This simple measure is obtained computing the product of the lower probability of being a father physically inactive times the share of the health care costs associated with inactivity, divided by two (to take into account just the male population).

3.4.1 Robustness checks

We perform several types of robustness checks. First, we run the estimations with alternative categorizations of the dependent variable. Second, to address the sample selection concern related to not including in our estimation sample those groups of states where HED policies have been reversed, we estimate our model under several specifications including in the sample all groups of states. Third, we estimate different model specifications, redefining the types of treatment evaluated. So far, we have a model that is general enough, in the sense that we do not impose separable effects of the reforms on topics and on enforcements. In the specifications described below we assume separability in the effects of each one of the two dimensions of the HED policy we are analyzing. Fourth, we use probit estimates of the HED policies to provide evidence that the change that took place between 1999 and 2005 in the question about frequency of light physical activity does not affect our results. Last, we perform a falsification test to show that there is none contemporaneous shock to HED policies affecting the frequency of light physical activity of individuals of the same range of ages than those having children of elementary school age.

3.4.1.1 Alternative categorization of the outcome variable

We obtain the IATE presented in Table 3.5 using a redefinition of the dependent variable that has 5 categories. In Figure 3.1 we can clearly distinguish mass points in the distribution of the outcome variable for values 0, 1 and 7. There is also an important accumulation of observations in the interval (2,7) that can be treated as a unique mass point or as several categories. To analyze whether our results are sensitive to different redefinitions of the outcome variable, we estimate our model using alternative categorizations of it. In Table 3.6 we summarize the alternative specifications and in Table 3.7 we report the estimated IATE corresponding to the group of states S_5.

We start by reporting results of OLS estimates for two specifications, one of them includes covariates and the other does not. Both groups of estimates are in line with the results obtained with the ordered probit model: positive and significant effects of HED on fathers's frequency of physical activity and no effects on mothers. Comparing the results with and without covariates we can see that the effect of HED is overestimated when no covariates are included.

To propose a reliable specification of the probit model we need an objective criterion to construct a dummy variable that indicates whether the individual is doing enough physical activity or not. To obtain important health benefits, the CDS recommends to do 2 hours and 30 minutes (150 minutes) of moderate-intensity aerobic activity (i.e., brisk walking) every week and muscle-strengthening activities on 2 or more days a week. They recommend to spread the activity out during the week in modules of at least 10 minutes at a time. Following these guidelines, the BRFSS classifies a respondent as “meeting the objective” if she/he does 30 or more minutes per day of moderate physical activity and for five or more days per week of moderate physical activity. Unfortunately, in our data individuals only report how many times per week they do physical activity, and they do not report how many minutes they spend each time in this activity. This makes unfeasible for us to propose a unique and convincing probit model, so we present several alternative specifications and then we compare all the results.

Table 3.6: Alternative categorization of the outcome variable

Model		New outcome variable values	Original outcome variable values
Probit	A	0, 1	$\{0\}$, $(0, >0]$
	B	0, 1	$[0, 3)$, $[3, >3]$
	C	0, 1	$[0, 5)$, $[5, >5]$
	D	0, 1	$[0, 7)$, $[7, >7]$
Ordered	3	0, 1, 2	$\{0\}$, $(0, 7)$, $[7, >7]$
	4	0, 1, 2, 3	$\{0\}$, $(0, 2)$, $[2, 7)$, $[7, >7]$
	5	0, 1, 2, 3, 4	$\{0\}$, $(0, 3)$, $[3, 7)$, $\{7\}$, $[7, >7]$
	9	0, 1, 2, 3, 4, 5, 6, 7, 8	$\{0\}$, $(0, 1)$, $[1, 2)$, $[2, 3)$, $[3, 4)$, $[4, 5)$, $[5, 7)$, $\{7\}$, $[7, >7]$

We found statistically significant positive effect of HED on fathers's frequency of light physical activity, and non-statistically significant effect on mothers in all ordered probit specifications. For the probit models, the signs of the estimated effects are in all cases compatible with the ordered probit specifications but we obtain IATEs for fathers statistically significant only with the probit B model. Additionally, we obtain a statistically significant negative effect for mothers with model probit A.

Summing up, we are confident that our results presented in section 3.4 are not driven by the categorization used for the outcome variable, since the estimated IATE are robust to several changes on it.

3.4.1.2 Exclusion of states in which the policy was reversed

As we explain before, we do not include in our analysis those states that experimented a reduction in the number of topics and/or enforcements. The main reason why we decided to drop these states is because we do not know why the reverse application of the policy was decided in each state. Possible reasons are that topics and/or enforcements were eliminated due to budgetary constraints, parents's demands, and/or the introduction/substitution of other topics not related with HED. The elimination of some topics and/or enforcements may also be a consequence of some evidence related with the lack of effectiveness, but we do not give much credit to this explanation since there is no available formal evaluation of HED policies.

Regardless the reason driving the decision, the reverse application of the policy itself can be considered as another type of treatment. Let us call S_9 to an additional group of states that includes all states that reduced the policy. Assuming that there exists no correlation in the effect of HED topics between S_9 and S_3 to S_8, excluding S_9 from the model estimated so far should not produce a sample that favor our results, since individuals in dropped states are not part of the control group that is required for identification of the effect of policies in states S_3 to S_8. In terms of the model in equation 3.2, we are assuming that it is a restricted model that imposes the value zero to the parameter that measures the effect of HED in S_9. We can test the validity of both assumptions by estimating IATEs using the complete set of states. That is, we estimate a model that considers 7 types of treatment, 6 with positive changes in HED and 1 with negative changes in HED.

The results for the estimation of several alternative specifications of the model including all 7 types of treatment are reported in Table 3.7. The results indicate that IATE for male and female in groups S_3 to S_3 are robust to the inclusion of S_9.

Table 3.7: Testing the categorization of the outcome variable and the exclusion of states that reversed the policy. IATE estimates for type of treatment S5: Implementation of Topics and Enforcements

	MODEL	MALE									FEMALE								
		Original variable				**Categorical variable (5 categories)**					**Original variable**				**Categorical variable (5 categories)**				
	OLS	With covariates		Without covariates		With covariates		Without covariates			With covariates		Without covariates		With covariates		Without covariates		
		1.775*		2.043**		0.617*		0.773**			-0.804		-0.644		-0.292		-0.227		
		(0.925)		(0.918)		(0.317)		(0.307)			(0.670)		(0.676)		(0.239)		(0.241)		
		0	**(0,1)**	**[1,2)**	**[2,3)**	**[3,4)**	**[4,5)**	**[5,7)**	**7**	**>7**	**0**	**(0,1)**	**[1,2)**	**[2,3)**	**[3,4)**	**[4,5)**	**[5,7)**	**7**	**>7**
6 types of treatment	**PROBIT**																		
	A $\{0\}(0,>0]$		0.199									-0,165 **							
			(0.162)									(0.071)							
	B $[0,3)[3,>3]$					0,270**									-0.106				
						(0.127)									(0.098)				
	C $[0,5)[5,>5]$							0.172									-0.108		
								(0.121)									(0.107)		
	D $[0,7)[7,>7]$								0.158									-0.061	
									(0.126)									(0.107)	
	ORDERED																		
	3 categories	-0.147	-0.045						0.192*		0.076	0.044						-0.120	
		(0.100)	(0.038)						(0.107)		(0.055)	(0.045)						(0.095)	
	4 categories	-0.176*	-0.059*			0.033			0.201**		0.070	0.045			0.000			-0.115	
		(0.104)	(0.030)			(0.029)			(0.099)		(0.051)	(0.037)			(0.011)			(0.092)	
	5 categories	-0.172*	-0.059**			0.032			0.186*	0.014*	0.069	0.042			-0.002			-0.097	-0.012
		(0.100)	(0.029)			(0.027)			(0.089)	(0.011)	(0.050)	(0.034)			(0.010)			(0.074)	(0.013)
	9 categories	-0.162	-0.017**	-0.028**	-0.011	0.006	0.008	0.015	0.176*	0.013	0.068	0.010	0.019	0.013	0.004	-0.001	-0.004	-0.097	-0.012
		(0.099)	(0.008)	(0.014)	(0.010)	(0.011)	(0.007)	(0.010)	(0.091)	(0.011)	(0.050)	(0.008)	(0.015)	(0.012)	(0.006)	(0.002)	(0.003)	(0.074)	(0.014)
7 types of treatment	**PROBIT**																		
	A		0.202									-0.161**							
	$\{0\}(0,>0]$		(0.165)									(0.072)							
	B					0.275**									-0.102				
	$[0,3)[3,>3]$					(0.124)									(0.099)				
	C							0.178									-0.108		
	$[0,5)[5,>5]$							(0.117)									(0.110)		
	D								0.166									-0.062	
	$[0,7)[7,>7]$								(0.120)									(0.109)	
	ORDERED	-0.178*	-0.061**			0.036			0.189**	0.015	0.066	0.041			-0.002			-0.094	-0.012
	5 categories	(0.097)	(0.029)			(0.029)			(0.085)	(0.013)	(0.050)	(0.035)			(0.010)			(0.075)	(0.015)

Standard errors computed by bootstrap with 2000 replications. Cluster is set at family level.Significance levels: $* = 10\%; ** = 5\%; *** = 1\%$. The regression includes the following covariates: age, race, gender, marital status, number of children, children of high school-age, education level, employment status, total family income level and state of residence. IATE estimated with 47 observations for males and 84 for females.

3.4.1.3 Alternative model specifications

The IATE estimates presented so far are computed with a relatively small number of observations. To explore whether sample size is an issue we present two alternative specifications of our model in which states are grouped in a different fashion. In these new specifications we impose the assumption that the effect of changing topics and enforcements are separable. For instance, the effect of an increase on the number of topics in two states is the same regardless of what they change in their enforcement requirements. In this sense, these models are more restrictive than the model described in equation 3.1.
First, we propose an alternative specification with 4 types of treatment:

$$\begin{aligned} y^*_{itj} = \beta_0 + \beta_1\tau_t + \beta_2 elem_{it} + \sum_{k=1}^{8}\beta_{3k}S_k + \\ \beta_4(elem_{it}\times\tau_t) + \sum_{k=1}^{8}\beta_{5k}(S_k\times\tau_t) + \sum_{k=1}^{8}\beta_{6k}(S_k\times elem_{it}) + \\ \sum_{k\in\{5,6\}}\beta_{7.1}(S_k\times elem_{it}\times imptop_{tj}) + \sum_{k\in\{7,8\}}\beta_{7.2}(S_k\times elem_{it}\times inctop_{tj}) + \\ \sum_{k\in\{3,5\}}\beta_{8.1}(S_k\times elem_{it}\times impenf_{tj}) + \sum_{k\in\{4,8\}}\beta_{8.2}(S_k\times elem_{it}\times incenf_{tj}) + \\ \beta_9 X_{itj} + u_{itj}, \end{aligned} \tag{3.4}$$

where $imptop_{tj}$ is a dummy that takes the value 1 if state j that is in the group of states S_k has implemented at least one topic for the first time in 2005; $inctop_{tj}$ is a dummy taking the value 1 if the state j that is in the group of states S_k has increased in 2005 the number of topics already taught in 1999; $impenf_{tj}$ is a dummy that takes the value 1 if the state j that is in the group of states S_k has implemented at least one enforcement for the first time in 2005; $incenf_{tj}$ is a dummy taking value 1 if the state j that is in the group of states S_k has increased in 2005 the number of enforcements already in place in 1999.
In this specification the identification of the parameters of interest, $\beta_{7.1}, \beta_{7.2}, \beta_{8.1}, \beta_{8.2}$, is achieved using a non-mutually exclusive groups of states. Parameter $\beta_{7.1}$ measures the variation in the frequency of light physical activity in those states where topics were implemented for the first time in 2005, and it is identified by two groups of states S_5 and S_6. Parameter $\beta_{7.2}$ captures the effect on the frequency of doing light physical activity of increasing the number of topics, and it is identified by the groups of states S_7 and S_8. Parameter $\beta_{8.1}$ captures the effect of implementing enforcements, and it is identified by S_3 and S_5. Finally, $\beta_{8.2}$ measures the effect of increasing the number of enforcements, and it is identified by groups S_4 and S_8.
IATE estimates across treated individuals for this model are reported in Table 3.8 below. We compute the IATE across treated individuals corresponding to the implementation of

topics for the first time as follows:

$$\sum_{i:\ elem=1} \{ \sum_{i:\ S_k=5} [\Phi(\alpha_m - \hat{\pi}_k Z_{kit} - \beta_{7.1}(S_5 \times elem_i \times imptop_{tj}) - \beta_{8.1}(S_5 \times elem_i \times impenf_{tj}))$$
$$- \Phi(\alpha_m - \hat{\pi}_k Z_{kit} - \beta_{8.1}(S_5 \times elem_i \times impenf_{tj}))] +$$
$$\sum_{i:\ S_k=6} [\Phi(\alpha_m - \hat{\pi}_k Z_{kit} - \beta_{7.1}(S_6 \times elem_i \times imptop_{tj})) - \Phi(\alpha_m - \hat{\pi}_k Z_{kit})]\}/N_{kt}, \tag{3.5}$$

where N_{kt} is the total of number of treated individuals in the groups of states S_5 and S_6 at time $t = 2005$.

Table 3.8: Model with four types of treatment: IATE estimates for Implementation of Topics (G1) and Implementation of Enforcements (G3). Outcome variable: frequency of light physical activity (times per week)

MODEL	POLICY	MALE With covariates	MALE Without covariates	FEMALE With covariates	FEMALE Without covariates
OLS Original variable	G1	0.910	0.965	-1.089*	-1.073
		(0.709)	(0.705)	(0.598)	(0.598)
	G3	0.817	0.994	0.360	0.456
		(0.788)	(0.774)	(0.661)	(0.654)

MODEL	POLICY	MALE 0	(0,1)	[1,2)	[2,3)	[3,4)	[4,5)	[5,7)	7	>7	FEMALE 0	(0,1)	[1,2)	[2,3)	[3,4)	[4,5)	[5,7)	7	>7
PROBIT																			
A $\{0\}(0,>0]$	G1		0.108									-0.073							
			(0.072)									(0.049)							
	G3		0.073									-0.074							
			(0.103)									(0.096)							
B $[0,3)[3,>3]$	G1					0,139*									-0.038				
						(0.065)									(0.053)				
	G3					0.090									0.067				
						(0.120)									(0.106)				
C $[0,5)[5,>5]$	G1							0.095									-0.043		
								(0.061)									(0.052)		
	G3							0.076									0.088		
								(0.119)									(0.101)		
D $[0,7)[7,>7]$	G1								0.070									-0.020	
									(0.062)									(0.051)	
	G3								0.121									0.059	
									(0.113)									(0.092)	
ORDERED																			
3 categories	G1	-0.071	-0.027						0.098*		0.023	0.016						-0.040	
		(0.044)	(0.021)						(0.052)		(0.034)	(0.022)						(0.045)	
	G3	-0.080	-0.043						0.123		-0.016	-0.004						0.019	
		(0.080)	(0.034)						(0.104)		(0.075)	(0.023)						(0.089)	
4 categories	G1	-0.082*	-0.035**			0.012			0.106**		0.022	0.015			0.000			-0.037	
		(0.044)	(0.017)			(0.011)			(0.050)		(0.033)	(0.017)			(0.010)			(0.044)	
	G3	-0.076	-0.042			0.006			0.112		-0.028	-0.012			0.004			0.035	
		(0.079)	(0.033)			(0.015)			(0.099)		(0.071)	(0.028)			(0.013)			(0.087)	
5 categories	G1	-0,080*	-0,036**			0.011			0.098**	0.008	0.020	0.015			0.001			-0.032	-0.004
		(0.041)	(0.017)			(0.010)			(0.045)	(0.006)	(0.032)	(0.016)			(0.009)			(0.037)	(0.006)
	G3	-0.073	-0.039			0.005			0.097	0.010	-0.025	-0.010			0.004			0.029	0.002
		(-0.074)	(0.032)			(0.014)			(0.086)	(0.009)	(0.068)	(0.026)			(0.013)			(0.075)	(0.008)
9 categories	G1	-0.074*	-0.009**	-0.017**	-0.009	0.000	0.003	0.006	0.091**	0.008	0.019	0.003	0.007	0.005	0.002	0.000	-0.001	-0.031	-0.005
		(0.041)	(0.005)	(0.008)	(0.006)	(0.004)	(0.002)	(0.004)	(0.046)	(0.006)	(0.031)	(0.004)	(0.007)	(0.006)	(0.004)	(0.002)	(0.003)	(0.037)	(0.006)
	G3	-0.072	-0.010	-0.018	-0.011	-0.002	0.002	0.005	0.096	0.010	-0.027	-0.003	-0.006	-0.003	0.000	0.001	0.002	0.032	0.003
		(0.074)	(0.008)	(0.015)	(0.009)	(0.005)	(0.004)	(0.007)	(0.086)	(0.009)	(0.068)	(0.007)	(0.013)	(0.007)	(0.004)	(0.003)	(0.006)	(0.075)	(0.008)

Standard errors computed by bootstrap with 2000 replications. Cluster is set at family level.Significance levels: $* = 10\%; ** = 5\%; *** = 1\%$. All regressions, except OlS without covariates, include the following covariates: age, race, gender, marital status, number of children, children of high school-age, education level, employment status, total family income level and state of residence. IATE of type of treatment G1 estimated with 110 observations for males and 153 for females. IATE of type of treatment G3 estimated with 72 for males and 123 for females.

We found evidence of a positive effect of HED reforms on parents's frequency of doing light physical activity. The probability that fathers do light physical activity once a day is increased by 9.8 percentage points with the implementation of topics. The probability of not doing light physical activity is reduced in 8 percentage points. We could not estimate the effect of changes in the number of enforcements accurately but still we can see a positive effect of this policy on fathers's frequency of light physical activity. Additionally, we found that the effect of both policies, implementing topics and implementing enforcements, on mothers's frequency of light physical activity is negligible.

Second, we propose an alternative model specification that defines two types of treatment: changes in the number of topics or changes in the number of enforcements. The latent variable version of the model with two types of treatment is as follows:

$$
\begin{aligned}
y^*_{itj} = \beta_0 + \beta_1\tau_t + \beta_2 elem_{it} + \sum_{k=1}^{9}\beta_{3k}S_k + \\
\beta_4(elem_{it}\times\tau_t) + \sum_{k=1}^{8}\beta_{5k}(S_k\times\tau_t) + \sum_{k=1}^{8}\beta_{6k}(S_k\times elem_{it}) + \\
\sum_{k\in\{5,6,7,8\}}\beta_7(S_k\times elem_{it}\times dtop_{tj}) + \sum_{k\in\{3,4,5,8\}}\beta_8(S_k\times elem_{it}\times denf_{tj}) + \\
\beta_9 X_{itj} + u_{itj},
\end{aligned}
$$

where $dtop_{tj}$ is a dummy equal one if state j that is in group of states S_k has changed the number of topics in 2005; $denf_{tj}$ is a dummy equal one if state j that is in group of states S_k has changed the number of enforcements in 2005.

This model has two underlying assumptions. First, the effects of implementing topics (enforcements) for the first time has the same effects than increasing the number of topics (enforcements). Second, the effects of changing topics and enforcements are separable. These assumptions imply that this model is the most restrictive one.

We report IATE estimates in Table 3.9. With this third model specification we also find similar IATE estimates to those found with the first model specification.

With all results reported in this section we can conclude that the relative small number of observations we use to obtain IATEs is not imposing a bias to our results. Additionally, assuming separability in the effect of topics and enforcements leads to results qualitatively similar to our main model results but the estimates are less accurate.

Table 3.9: Model with two types of treatment: IATE estimates for Change in Topics (T) and Change in Enforcements (E). Outcome variable: frequency of light physical activity (times per week)

MODEL	POLICY	MALE					FEMALE				
		With covariates		Without covariates			With covariates		Without covariates		
OLS Original variable	T	0.472		0.601			-0.679		-0.565		
		(0.523)		(0.529)			(0.433)		(0.439)		
	E	0.574		0.729			0.068		0.146		
		(0.528)		(0.532)			(0.429)		(0.430)		
		0	**(0,3)**	**[3,7)**	**7**	**>7**	**0**	**(0,3)**	**[3,7)**	**7**	**>7**
ORDERED 5 categories	T	-0.049	-0.028*	0.002	0.067*	0.008	0.020	0.010	-0.002	-0.026	-0.003
		(0.032)	(0.016)	(0.005)	(0.039)	(0.006)	(0.025)	(0.013)	(0.004)	(0.032)	(0.004)
	E	-0.039	-0.020	0.004	0.050	0.005	0.002	0.001	0.000	-0.002	0.000
		(0.049)	(0.022)	(0.008)	(0.059)	(0.007)	(0.043)	(0.016)	(0.008)	(0.047)	(0.004)

Standard errors computed by bootstrap with 2000 replications. Cluster is set at family level. All regressions, except OLS without covariates, include the following covariates: age, race, gender, marital status, number of children, children of high school-age, education level, employment status, total family income level and state of residence. IATE of type of treatment T estimated with 187 observations for males and 256 for females. IATE of type of treatment E estimated with 170 for males and 272 for females.

3.4.1.4 Change in the question

There was a change in the questions we use to construct the outcome variable. In 1999 the questions were *"How often (do you/does he) participate in light physical activity- such as walking, dancing, gardening, golfing, bowling, etc.?"* and *"How often (do you/does he) participate in vigorous physical activity or sports -such as heavy housework, aerobics, running, swimming, or bicycling?"*. In 2005 the questions were *"How often do you do light or moderate activities for at least 10 minutes that cause only light sweating or slight to moderate increases in breathing or heart rate?"* and *"How often do you do vigorous activities for at least 10 minutes that cause heavy sweating or large increases in breathing or heart rate?"*. The change of the wording of the question may impede to separately identify the effects of the reform from the change in the question. However, this concern is based in a too demanding assumption: that the wording of the question not only has different effect on different individuals, but also that it affects systematically (positively or negatively) the group of parents with children in elementary school within each experimental state. We do not see any a priori reason to believe that the interpretation of the new question should create a bias across treatment and control groups in a specific state and in a systematic direction. However, we do not want to impose any assumption in this sense, so we proceed to test whether there is or not a systematic effect of the change of the question on different groups. A way to minimize the effect of a systematic overestimation or underestimation of the true level of physical activity is to group the dependent variable in broad categories. Thus, we reduced the dependent variable to a 0/1 dummy and estimated the respective probit model.

Now, let us suppose that the bias exists, that it is positive, and that the two categories for the dependent variable are $\{0\}$ and > 0. Then, if any given individual does physical activity in both years 1999 and 2005 and, due to the change in the question she/he over-reports her/his actual frequency in 2005, this will not affect the final value we attach to the dummy variable for this individual. Still, we may have problems with those whose actual frequency of physical activity is 0 but they report a positive number. To alleviate this problem we tried alternative cut-off points in the definition of the 2 categories for the outcome variable. If the bias exists, we will never reduce it to zero since we would always have problems with those individuals whose actual level of physical activity is close to the cut-off point defined. Despite this drawback, and given that no extra information is available for 2006, dichotomization of the dependent variable is the best solution we can provide to the problem of the change in the question.
According to the results for the different probit specifications in Table 7, we still find positive effects of the policy in S_5. So we conclude that if any bias exist, it is negligible.

3.4.1.5 Existence of other shock affecting the outcome variable

There may exist a concern that our results might be driven by an age-related shock that affects in a different manner the outcome variable of individuals in the control and in the treatment groups. The fact that the definition of control and treatment groups of individuals is based on the age of their children may imply that this definition resides also on the age of the individual himself. That is, a person could be affected by a shock that has to do with their age and not with the fact that they have children of primary school-age. To analyze whether this concern is indeed flawing our results, we construct a falsification test in which the control and treatment groups are formed by people without children. In the treatment group we include those individuals that, while they do not have any children, they are in the same range of ages of those with children of primary school age. Moreover, the control groups comprise those individuals that are younger or older than individuals whose ages overlap the range of ages of those with children that attend primary schools. To define the relevant range of ages we took the 5% percentile (25 years old) and the 95% percentile (47 years old) in the distribution of ages of individuals with children of primary school age as the cutoffs of this interval.
Table 3.10 presents the IATE estimates for the three alternative definitions of the treatments (S-groups, G-groups, and T and E groups). None of these estimates are statistically significant, which means that the concern about the existence of an age-related shock is not affecting our findings on the existence of family spillovers of HED received at school.

Table 3.10: Testing the existence of an age-related shock: IATE estimates for alternative models. Outcome variable: frequency of light physical activity (times per week)

		MALE						FEMALE					
	POLICY	0	(0,3)	[3,7)	7	>7	# obs.	0	(0,3)	[3,7)	7	>7	# obs.
Models with 6 treatments													
Probit B	S5			0.347			44			-0.134			40
				(0.212)						(0.253)			
Ordered 5	S5	-0.171	-0.071	0.043	0.186	0.013	44	0.012	0.007	-0.002	-0.016	-0.001	40
categories		(0.186)	(0.070)	(0.072)	(0.141)	(0.082)		(0.136)	(0.066)	(0.043)	(0.143)	(0.064)	
Model with 4 treatments													
Ordered	G1	-0.062	-0.042	0.006	0.090	0.009	87	-0.043	0.013	0.030	0.005	-0.005	76
5		(0.067)	(0.040)	(0.030)	(0.078)	(0.044)		(0.075)	(0.047)	(0.035)	(0.066)	(0.030)	
categories	G3	0.003	0.002	0.000	-0.004	0.000	70	-0.147	-0.048	0.044	0.143	0.008	59
		(0.107)	(0.062)	(0.035)	(0.123)	(0.059)		(0.139)	(0.049)	(0.056)	(0.103)	(0.063)	
Model with 2 treatments													
Ordered	T	-0.071	-0.053	0.001	0.110	0.013	169	-0.001	-0.001	0.000	0.003	0.000	146
5		(0.054)	(0.035)	(0.025)	(0.068)	(0.043)		(0.047)	(0.026)	(0.014)	(0.054)	(0.023)	
categories	E	-0.056	-0.032	0.009	0.072	0.006	164	-0.039	-0.026	0.004	0.057	0.006	141
		(0.081)	(0.039)	(0.028)	(0.086)	(0.041)		(0.072)	(0.040)	(0.022)	(0.085)	(0.038)	

Standard errors computed by bootstrap with 2000 replications. Cluster is set at family level. All regressions include the following covariates: age, race, gender, marital status, number of children, children of high school-age, education level, employment status, total family income level and state of residence. S5 refers to the policy: implementation of topics and enforcements. G1 refers to the policy: implementation of topics; G3 refers to the policy: implementation of topics. T refers to the policy: change in the number of topics: E refers to the policy: change in the number of enforcements.

3.4.2 Plausible explanations for our results

The estimates from the model interacting the policies with a dummy variable for gender allow us to obtain some insights on the channels driving the potential existence of spillovers of HED on parental physical activity. There are two ways of rationalizing our results. First, the amount of information provided by HED programs is likely to have a stronger effect on those individuals who have a lower stock of information. This is for instance consistent with other works on Indirect Treatment Effects, such as Bandiera and Rasul (2006) who study agricultural technology adoption and find evidence on the fact that the adoption decisions of farmers who have better previous information about the innovation are less sensitive to the adoption choices of others. In our case, fathers are thought to own a pre-reform stock of information about healthy lifestyles that is smaller than that one of mothers. This idea is supported by the fact that mothers are those that take their children to the physicians and that in general are more aware of preventive care methods than fathers. Therefore, a specification that allows for differential impact of the policies by gender will provide indirect evidence on the existence of information sharing between the child and his parents as a plausible channel driving the potential spillovers of HED on parental health lifestyles. The second idea has to do with the role models that mothers and fathers play for their children. Parents usually spend more time with their children doing gendered activities. Figure 3.4 in the Appendix shows some evidence in this respect with data coming from the American Time Use Survey (ATUS). Women spend roughly twice as much time in childcare as do men, a pattern which holds true for all subgroups and for almost all types of childcare, except for "Recreational" childcare. This type of childcare activities includes playing games with children, playing outdoors with children, attending a child's sporting event or dance recital, going to the zoo with children, taking walks with children, etc. In the case of "Recreational" childcare, mothers allocate relatively less of their time with children when compared with the time allocation into types of childcare activities that fathers do. Thus, this is evidence on the fact that fathers are more likely to do stereotypically male activities with their children, among which we can include physical activity. Accordingly, the impact of HED reforms on physical activity is also expected to appear for fathers rather than for mother.

We also explore the existence of the information sharing channel by analyzing the differential impact of HED reforms on individuals with low and high education levels.[12] In the estimates of the model that interacts the policy variables with a dummy for level of education -low and high level- we do find the expected signs in the coefficients of interest. That is, individuals with low levels of education are those affected by the HED reforms. However the estimates are in general not statistically significant as it can be seen in Table 3.11. In the same Table, we present results by race, splitting the data into white and non-white groups of individuals. The results for this interaction are not convincing enough, since we found a zero effect of HED policies for both white and non-white individuals. The lack of accuracy in the estimation of HED effects by level of education and race may be explained by the fact that we are pooling together males and females in each education/race category. Ideally we would want to estimate by gender/education/race categories, but groups's sample sizes do not allow us to do it.

[12]The model with interactions by level of education is exactly the same as the one presented for the case of interactions with gender, but in this case the dummy variable is *he* and it takes the value 1 when the individual has a high level of education (at least 12 years of education), and the value 0 otherwise.

Table 3.11: Testing the channels behind our results: IATE estimates for alternative models. Outcome variable: frequency of light physical activity (times per week)

	POLICY	0	(0,3)	[3,7)	7	>7	# of obs.	0	(0,3)	[3,7)	7	>7	# of obs.
Models with 6 treatments													
Education				**LOW EDUCATION**						**HIGH EDUCATION**			
Ordered 5	S5	-0.214	-0.068	0.041	0.225*	0.016	8	0.017	0.009	-0.002	-0.023	-0.002	71
categories		(0.165)	(0.046)	(0.045)	(0.132)	(0.016)		(0.062)	(0.032)	(0.009)	(0.080)	(0.009)	
Race				**NON-WHITE**						**WHITE**			
Ordered 5	S5	0.003	0.001	-0.001	-0.003	0.000	27	-0.052	-0.041	-0.006	0.085	0.013	57
categories		(0.087)	(0.028)	(0.020)	(0.089)	(0.007)		(0.069)	(0.043)	(0.014)	(0.096)	(0.016)	
Model with 4 treatments													
Education				**LOW EDUCATION**						**HIGH EDUCATION**			
Ordered	G1	-0.074	-0.029	0.011	0.084	0.008	14	0.003	0.004	0.001	-0.006	-0.001	149
5		(0.069)	(0.043)	(0.027)	(0.081)	(0.024)		(0.030)	(0.015)	(0.007)	(0.037)	(0.004)	
categories	G3	0.016	0.009	0.000	-0.023	-0.003	10	-0.038	-0.018	0.005	0.047	0.004	119
		(0.120)	(0.061)	(0.025)	(0.147)	(0.037)		(0.061)	(0.025)	(0.011)	(0.069)	(0.006)	
Race				**NON-WHITE**						**WHITE**			
Ordered	G1	-0.010	-0.003	0.002	0.011	0.001	50	-0.009	-0.009	-0.002	0.017	0.003	113
5		(0.044)	(0.017)	(0.012)	(0.045)	(0.004)		(0.031)	(0.022)	(0.007)	(0.046)	(0.007)	
categories	G3	-0.015	(0.087)	0.003	0.015	0.001	34	-0.043	-0.032	-0.003	0.069	0.009	95
		-0.004	(0.027)	(0.020)	(0.087)	(0.007)		(0.058)	(0.035)	(0.010)	(0.081)	(0.011)	

Standard errors computed by bootstrap with 2000 replications. Cluster is set at family level. All regressions include the following covariates: age, race, gender, marital status, number of children, children of high school-age, education level, employment status, total family income level and state of residence. S5 refers to the policy: implementation of topics and enforcements. G1 refers to the policy: implementation of topics; G3 refers to the policy: implementation of topics.

3.5 Conclusion

We find evidence on the existence of positive spillovers of HED imparted in elementary schools on parents's frequency of light physical activity. However, our results suggest that fathers and not mothers are those affected by the HED reforms. We also analyze the differential impact of HED reforms on fathers and mothers as a way to explore the nature of the channels driving the spillovers.

We proposed two ways of explaining the differential impact by parents's gender. First, we argue that fathers have less information about preventive health behavior and thus are likely to be more affected by the arrival of new information on this subject. Since fathers are actually found to be more affected by HED received by children, there is an indirect evidence of information transmission from children to parents. We also explore the existence of the information sharing channel by analyzing the differential impact of HED reforms on individuals with low and high education levels, and even when we do find the expected signs in the coefficients of interest -individuals with low levels of education are those affected by the HED reforms-, the estimates are in general not statistically significant. The second idea has to do with the role models that mothers and fathers play for their children. Parents usually spend more time with their children doing gendered activities. Since physical activity can be included into the group of the typically male-activities, the effect of the promotion of the advantages of doing physical activity is more likely to appear for fathers rather than for mothers.

We perform several robustness checks, changing the model specification while regrouping states in different manners, and changing the definition of categories of the dependent variable. To summarize, the existence of a positive effect of the implementation of topics and enforcements on fathers's frequency of physical activity still holds true after performing all these checks.

Our results also highlight the importance of clearly distinguishing the existence of several dimensions in the implementation of a policy. In our case, considering the two dimensions in the HED reforms -changes in topics and enforcements- as well as the distinction between implementing requirements for the first time relative to reforms in already existing requirements is important for the policy evaluation. Our main result shows the existence of spillovers only in the case when both policy dimensions are simultaneously implemented for the first time.

The existence of spillovers of HED on parental lifestyles indicates that the interaction between children and parents play a role in the formation of healthy lifestyles inside the household. Therefore, taking into account these spillovers is important in the cost-benefit analysis of introducing health education at schools. In addition, the conclusion that implementing reforms in topics is not enough to obtain spillovers at the family level helps to properly design policy interventions aiming to increase the acquisition of healthy lifestyles in a given community.

3.6 Appendix

Table 3.12: States That Require Elementary Schools to Teach Health Topics, by Topic and Year

	2000					2006				
State	topic 1	topic 2	topic 3	topic 4	topic 5	topic 1	topic 2	topic 3	topic 4	topic 5
Alabama	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Alaska	no	no	no	no	no	no	no	no	no	no
Arizona	no	no	no	no	no	no	no	no	no	no
Arkansas	no	no	no	no	no	no	no	yes	yes	no
California	yes	yes	yes	yes	yes	yes	yes	yes	no	yes
Colorado	no	no	no	no	no	no	no	no	no	no
Connecticut	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Delaware	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
District of Columbia	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Florida	no	no	no	no	no	yes	yes	yes	no	no
Georgia	yes	yes	yes	no	yes	yes	yes	yes	yes	yes
Hawaii	no	no	no	yes	no	yes	yes	yes	yes	yes
Idaho	no	no	no	no	no	yes	yes	yes	yes	yes
Illinois	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Indiana	yes	yes	yes	yes	yes	yes	yes	yes	no	yes
Iowa	yes	yes	yes	yes	yes	yes	no	yes	no	yes
Kansas	no	no	no	no	no	no	no	no	no	no
Kentucky	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Louisiana	yes	yes	yes	yes	yes	yes	yes	yes	no	yes
Maine	yes	yes	yes	no	yes	yes	yes	yes	yes	yes
Maryland	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Massachusetts	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Michigan	yes	yes	yes	no	yes	yes	yes	yes	no	yes
Minnesota	yes	no	yes	yes	yes	no	no	no	no	no
Mississippi	yes	yes	yes	yes	yes	no	no	no	yes	no
Missouri	yes	yes	yes	yes	yes	yes	no	no	no	yes
Montana	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Nebraska	yes	no	no	no	yes	yes	no	yes	yes	yes
Nevada	yes	yes	yes	yes	yes	yes	no	yes	yes	yes
New Hampshire	yes	yes	yes	yes	no	yes	no	no	no	no
New Jersey	yes	yes	yes	yes	yes	yes	yes	yes	no	yes
New Mexico	no	no	no	no	no	yes	yes	yes	yes	yes
New York	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
North Carolina	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
North Dakota	yes	no	no	no	yes	yes	no	no	yes	yes
Ohio	yes	no	yes	yes	yes	no	no	no	no	no
Oklahoma	yes	yes	yes	yes	yes	no	no	no	no	no
Oregon	yes	yes	yes	yes	yes	yes	no	no	no	yes
Pennsylvania	yes	no	no	no	yes	yes	yes	yes	yes	yes
Rhode Island	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
South Carolina	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
South Dakota	no	no	no	no	no	no	no	no	no	no
Tennessee	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Texas	no	no	no	no	no	yes	yes	yes	yes	yes
Utah	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Vermont	yes	yes	yes	no	yes	yes	yes	yes	yes	yes
Virginia	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Washington	yes	no	yes	yes	yes	yes	yes	yes	yes	yes
West Virginia	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Wisconsin	yes	yes	no	no	yes	no	no	no	no	no
Wyoming	no	no	no	no	no	no	yes	no	no	no

Source: School Health Policies and Programs Study (SHPPS).
Topic 1:Alcohol or other drug-use prevention; **Topic 2**: Emotional and mental health;
Topic 3: Nutrition and dietary behavior; **Topic 4**: Physical activity and fitness; **Topic 5**: Tabacco-use prevention.

Table 3.13: States That Implemented Enforcements, by Enforcement and Year

	2000			2006		
State	Enf 1	Enf 2	Enf 3	Enf 1	Enf 2	Enf 3
Alabama	yes	no	no	yes	no	yes
Alaska	no	no	no	yes	no	no
Arizona	yes	no	no	yes	no	no
Arkansas	yes	no	no	yes	no	no
California	no	no	no	no	no	no
Colorado	no	no	no	no	no	no
Connecticut	no	no	no	no	no	no
Delaware	yes	no	yes	yes	no	yes
D. of Columbia	no	yes	yes	yes	no	yes
Florida	yes	no	no	yes	no	no
Georgia	yes	no	no	yes	no	no
Hawaii	yes	yes	no	yes	no	no
Idaho	no	no	no	yes	yes	yes
Illinois	yes	no	no	yes	no	no
Indiana	yes	no	no	yes	no	no
Iowa	no	no	no	no	no	no
Kansas	no	no	no	no	no	no
Kentucky	no	yes	no	yes	yes	no
Louisiana	yes	no	no	yes	no	no
Maine	yes	yes	no	yes	yes	no
Maryland	yes	no	no	yes	no	no
Massachusetts	yes	no	no	no	no	no
Michigan	yes	no	no	yes	no	no
Minnesota	yes	no	no	no	no	no
Mississippi	yes	no	no	yes	no	no
Missouri	yes	yes	no	yes	yes	no
Montana	yes	no	no	yes	no	no
Nebraska	no	no	no	no	no	no
Nevada	yes	no	no	yes	no	no
New Hampshire	no	no	no	no	no	no
New Jersey	yes	yes	no	yes	no	no
New Mexico	yes	yes	no	yes	no	no
New York	yes	no	yes	yes	no	no
North Carolina	yes	no	no	yes	no	no
North Dakota	no	no	no	no	no	no
Ohio	no	no	no	no	no	no
Oklahoma	no	no	no	yes	no	no
Oregon	no	no	no	yes	no	no
Pennsylvania	yes	no	no	yes	yes	yes
Rhode Island	yes	yes	no	yes	yes	yes
South Carolina	yes	no	no	yes	yes	no
South Dakota	no	no	no	no	no	no
Tennessee	yes	no	no	yes	no	no
Texas	no	no	no	yes	no	no
Utah	yes	no	no	yes	yes	no
Vermont	yes	no	no	yes	yes	no
Virginia	no	no	no	yes	no	no
Washington	yes	yes	no	yes	yes	no
West Virginia	yes	no	no	yes	no	no
Wisconsin	no	no	no	no	no	yes
Wyoming	no	no	no	yes	no	no

Source: School Health Policies and Programs Study (SHPPS).
Enforcement 1: State requires districts or schools to follow national or state health education standards or guidelines.
Enforcement 2: State requires students in elementary school to be tested on health topics.
Enforcement 3: State requires each school to have a HED coordinator

Table 3.14: Number of Topics and Enforcements, by State and Year

State	topics 2000	topics 2006	enforcements 2000	enforcements 2006
Alabama	5	5	1	2
Alaska	0	0	0	1
Arizona	0	0	1	1
Arkansas	0	2	1	1
California	5	4	0	0
Colorado	0	0	0	0
Connecticut	5	5	0	0
Delaware	5	5	2	2
District of Columbia	5	5	2	2
Florida	0	3	1	1
Georgia	4	5	1	1
Hawaii	1	5	2	1
Idaho	0	5	0	3
Illinois	5	5	1	1
Indiana	5	4	1	1
Iowa	5	3	0	0
Kansas	0	0	0	0
Kentucky	5	5	1	2
Louisiana	5	4	1	1
Maine	4	5	2	2
Maryland	5	5	1	1
Massachusetts	5	5	1	0
Michigan	4	4	1	1
Minnesota	4	0	1	0
Mississippi	5	1	1	1
Missouri	5	2	2	2
Montana	5	5	1	1
Nebraska	2	4	0	0
Nevada	5	4	1	1
New Hampshire	4	1	0	0
New Jersey	5	4	2	1
New Mexico	0	5	2	1
New York	5	5	2	1
North Carolina	5	5	1	1
North Dakota	2	3	0	0
Ohio	4	0	0	0
Oklahoma	5	0	0	1
Oregon	5	2	0	1
Pennsylvania	2	5	1	3
Rhode Island	5	5	2	3
South Carolina	5	5	1	2
South Dakota	0	0	0	0
Tennessee	5	5	1	1
Texas	0	5	0	1
Utah	5	5	1	2
Vermont	4	5	1	2
Virginia	5	5	0	1
Washington	4	5	2	2
West Virginia	5	5	1	1
Wisconsin	3	0	0	1
Wyoming	0	1	0	1

Source: School Health Policies and Programs Study (SHPPS).

Table 3.15: HED topics and enforcements - Full list

Topics Description
Alcohol- or Other Drug-Use Prevention
Emotional and Mental Health
Nutrition and Dietary Behavior
Physical Activity and Fitness
Tobacco-Use Prevention
Human immunodeficiency virus (HIV) prevention
Accident or injury prevention
Sexually transmitted disease (STD) prevention
Pregnancy prevention
Suicide prevention
Violence prevention, for example bullying, fighting, or homicide

Enforcements Description
State requires districts or schools to follow national or state health education standards or guidelines
State requires students in elementary school to be tested on health topics
State requires each school to have a HED coordinator
State uses staff development for health education teachers to improve compliance with health education standards or guidelines
State uses written reports from districts or schools to document compliance with health education standards or guidelines
State provides a list of one or more recommended elementary school health education curricula
State provides a chart describing the scope and sequence of instruction for elementary school health education
State provides lesson plans or learning activities for elementary school health education
State provides plans for how to assess or evaluate students in elementary school health education
State adopts a policy stating that newly hired staff who teach health education at the elementary school level will have undergraduate or graduate training in health education
State offers certification, licensure, or endorsement to teach health education
State adopts a policy stating that teachers will earn continuing education credits on health education topics to maintain state certification, licensure, or endorsement to teach health education

In Italic topic and enforcements considered for the analysis.

Table 3.16: States classified by groups S_k

State	Num. of observations
NON-EXPERIMENTAL	
S_0	
Colorado	224
Kansas	70
South Dakota	56
S_1	
Arizona	157
S_2	
Delaware	12
District of Columbia	63
Illinois	376
Maryland	431
Michigan	571
Montana	13
North Carolina	532
Tennessee	229
West Virginia	23
EXPERIMENTAL	
S_3	
Virginia	347
S_4	
Alabama	128
Kentucky	170
Rhode Island	9
South Carolina	543
Utah	93
S_5	
Idaho	25
Texas	640
Wyoming	16
S_6	
Arkansas	261
Florida	424
S_7	
Georgia	391
Maine	30
Washington	202
S_8	
Pennsylvania	453
Vermont	7

Table 3.17: DDD estimator for females in S_5

	Before HED change	**After HED change**	**Time difference**	
A. Treatment individuals: with children in elem				
Experimental states	5.137	3.675	-1.462***	Δ_E^T
	(0.363)	(0.318)	(0.481)	
	[70]	[84]	[154]	
Non-experimental states	4.787	3.777	-1.010***	Δ_{NE}^T
	(0.191)	(0.198)	(0.275)	
	[287]	[298]	[585]	
Difference in difference			**-0.451**	
			(0.554)	
			[739]	
B. Control Individuals: without children in elem				
Experimental states	4.390	3.594	-0.615	Δ_E^C
	(0.168)	(0.126)	(0.379)	
	[96]	[150]	[246]	
Non-experimental states	4.390	3.594	-0.797***	Δ_{NE}^C
	(0.168)	(0.126)	(0.207)	
	[378]	[608]	[986]	
Difference in difference			**0.182**	
			(0.432)	
			[1,232]	
DDD $= (\boldsymbol{\Delta}_{\mathbf{E}}^{\mathbf{T}} - \boldsymbol{\Delta}_{\mathbf{NE}}^{\mathbf{T}}) - (\boldsymbol{\Delta}_{\mathbf{E}}^{\mathbf{C}} - \boldsymbol{\Delta}_{\mathbf{NE}}^{\mathbf{C}})$			**-0.633**	
			(0.703)	
			[1,971]	

Significance levels: * = 10%; ** = 5%; *** = 1%. Cells contain mean frequency of light physical activity for the group identified. Standard errors are given in parentheses, sample sizes are given in square brackets.

Table 3.18: Ordered Probit Model: light/moderate physical activity

Number of obs=6496 | Wald chi2(113) = 501.67
Prob > chi2 = 0.0000
Log pseudolikelihood = -8892.6266 | Pseudo R2 = 0.0255
(Std. Err. adjusted for 1182 clusters at family level)

Variable	Coefficient	(Std. Err.)	Variable	Coefficient	(Std. Err.)
tau	-0.127	(0.161)	S3_elem_tau_w	0.057	(0.448)
elem	0.092	(0.174)	S4_elem_tau_w	-0.036	(0.319)
S1	-0.189	(0.325)	S5_elem_tau_w	-0.881***	(0.316)
S2	0.501	(0.379)	S6_elem_tau_w	-0.472	(0.377)
S3	-0.195	(0.286)	S7_elem_tau_w	0.006	(0.349)
S4	0.109	(0.257)	S8_elem_tau_w	-0.233	(0.377)
S5	-0.361	(0.430)	S1_jhs	-0.077	(0.259)
S6	0.143	(0.254)	S2_jhs	0.332**	(0.154)
S7	0.084	(0.245)	S3_jhs	0.247	(0.210)
S8	-0.177	(0.401)	S4_jhs	0.462***	(0.171)
elem_tau	-0.142	(0.132)	S5_jhs	0.296*	(0.173)
S1_tau	-0.190	(0.291)	S6_jhs	0.300	(0.187)
S2_tau	0.000	(0.168)	S7_jhs	0.551***	(0.179)
S3_tau	0.066	(0.289)	S8_jhs	0.238	(0.193)
S4_tau	-0.096	(0.213)	jhs	-0.191	(0.151)
S5_tau	-0.300	(0.225)	jhs_tau	-0.054	(0.070)
S6_tau	-0.139	(0.227)	gender	0.009	(0.201)
S7_tau	-0.103	(0.234)	age1	-0.029**	(0.013)
S8_tau	-0.047	(0.261)	age2	0.000	(0.000)
S1_elem	-0.004	(0.302)	white	0.261***	(0.038)
S2_elem	0.005	(0.178)	edu1	0.019***	(0.007)
S3_elem	-0.058	(0.307)	marital1	-0.066	(0.057)
S4_elem	-0.240	(0.235)	marital3	-0.235	(0.166)
S5_elem	-0.215	(0.259)	marital4	-0.029	(0.070)
S6_elem	-0.332	(0.258)	marital5	0.080	(0.089)
S7_elem	0.159	(0.259)	ch_nchildren	0.071**	(0.034)
S8_elem	0.153	(0.273)	nch2	-0.006	(0.004)
S3_elem_tau	0.346	(0.401)	rhwy_pclabinc	0.011	(0.008)
S4_elem_tau	0.118	(0.275)	empstatusd2	0.069	(0.149)
S5_elem_tau	0.589*	(0.303)	empstatusd3	0.014	(0.082)
S6_elem_tau	0.340	(0.305)	empstatusd4	-0.158	(0.158)
S7_elem_tau	0.186	(0.306)	empstatusd5	-0.504***	(0.104)
S8_elem_tau	0.076	(0.342)	empstatusd6	0.062	(0.050)
tau_w	-0.054	(0.225)	empstatusd7	0.177*	(0.094)
elem_w	0.306	(0.236)	stated4	0.050	(0.154)
S1_w	0.372	(0.368)	stated5	-0.485	(0.510)
S2_w	0.066	(0.207)	stated6	-0.495	(0.349)
S3_w	0.492	(0.305)	stated7	0.041	(0.110)
S4_w	-0.171	(0.234)	stated8	-0.146	(0.111)
S5_w	-0.166	(0.234)	stated9	0.856**	(0.425)
S6_w	-0.306	(0.249)	stated10	-0.515	(0.334)
S7_w	0.053	(0.240)	stated11	0.100	(0.218)
S8_w	0.220	(0.240)	stated12	-0.062	(0.153)
elem_tau_w	-0.016	(0.157)	stated14	-0.517	(0.332)
S1_tau_w	-0.023	(0.421)	stated15	-0.582*	(0.330)
S2_tau_w	-0.091	(0.223)	stated17	-0.557*	(0.333)
S3_tau_w	-0.279	(0.391)	stated18	0.141	(0.299)
S4_tau_w	0.090	(0.281)	stated20	-0.088	(0.122)
S5_tau_w	0.290	(0.285)	stated22	-0.552*	(0.333)
S6_tau_w	0.273	(0.303)	stated23	0.391	(0.351)
S7_tau_w	-0.078	(0.291)	stated24	0.141	(0.154)
S8_tau_w	-0.151	(0.316)	stated27	-0.055	(0.124)
S1_elem_w	-0.183	(0.432)	stated28	-0.342	(0.375)
S2_elem_w	-0.291	(0.243)	cut1	-1.318***	(0.335)
S3_elem_w	-0.785**	(0.368)	cut2	-0.551	(0.336)
S4_elem_w	-0.247	(0.298)	cut3	0.026	(0.335)
S5_elem_w	0.307	(0.307)	cut4	1.909***	(0.333)
S6_elem_w	0.195	(0.329)			
S7_elem_w	-0.610*	(0.325)			
S8_elem_w	-0.579*	(0.313)			

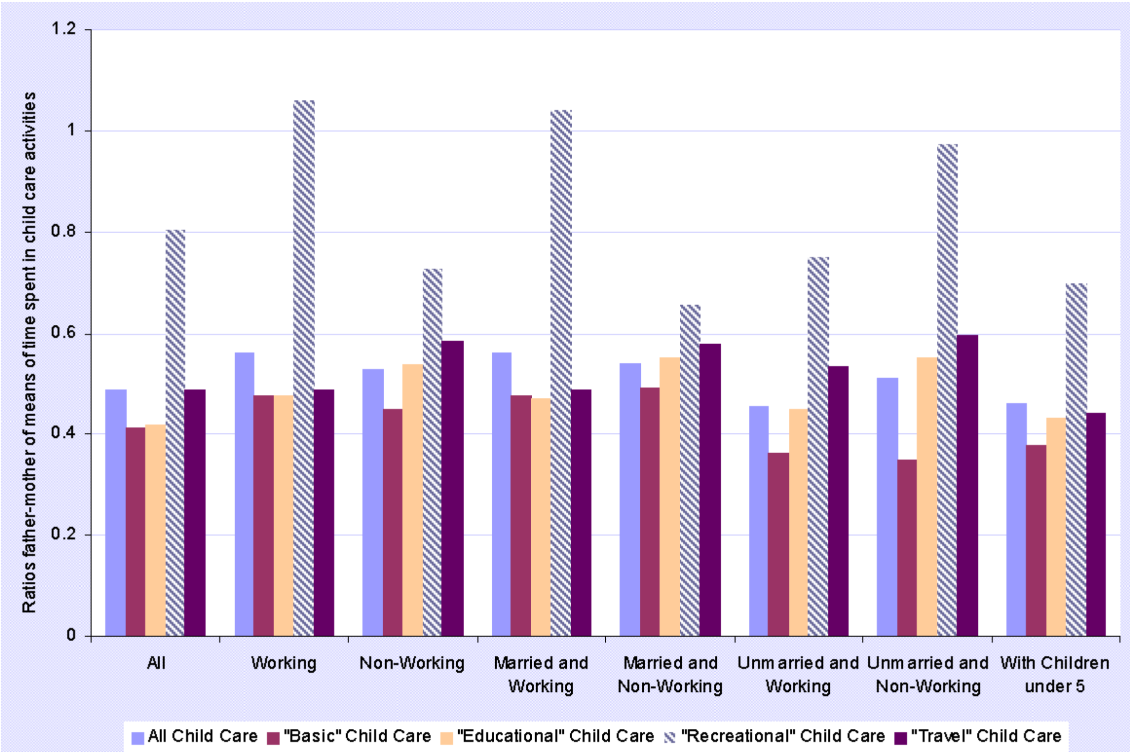
Significance levels: * = 10%; ** = 5%; *** = 1%.

Table 3.19: Indirect Average Treatment Effects across treated individuals

	Observed Coef.	**Bootstrap Std. Err.**	**z**	$P > \|z\|$	Normal-based [95% Conf. Interval]	
$me0_{3m}$	-.0844734	.1034015	-0.82	0.414	-.2871366	.1181898
$me1_{3m}$	-.048021	.0480886	-1.00	0.318	-.142273	.0462309
$me2_{3m}$	.0047773	.0211703	0.23	0.821	-.0367158	.0462705
$me3_{3m}$	.1156734	.1226193	0.94	0.345	-.1246561	.3560029
$me4_{3m}$	.0120437	.0152942	0.79	0.431	-.0179324	.0420198
$me0_{4m}$	-.0337388	.0766463	-0.44	0.660	-.1839627	.1164852
$me1_{4m}$	-.0121408	.0272559	-0.45	0.656	-.0655614	.0412797
$me2_{4m}$	.0058584	.0151691	0.39	0.699	-.0238726	.0355893
$me3_{4m}$	.0372102	.0822599	0.45	0.651	-.1240163	.1984366
$me4_{4m}$	.0028111	.0072691	0.39	0.699	-.011436	.0170582
$me0_{5m}$	-.1702191	.0965041	-1.76	0.078	-.3593637	.0189255
$me1_{5m}$	-.0585404	.0271658	-2.15	0.031	-.1117844	-.0052964
$me2_{5m}$	.0310267	.0268503	1.16	0.248	-.0215989	.0836524
$me3_{5m}$	.1841226	.0857774	2.15	0.032	.0160019	.3522432
$me4_{5m}$	.0136102	.0107841	1.26	0.207	-.0075263	.0347468
$me0_{6m}$	-.0956321	.0878547	-1.09	0.276	-.2678242	.0765601
$me1_{6m}$	-.037293	.0292409	-1.28	0.202	-.0946041	.0200181
$me2_{6m}$	.0158249	.0206585	0.77	0.444	-.024665	.0563148
$me3_{6m}$	.1088925	.0891369	1.22	0.222	-.0658127	.2835977
$me4_{6m}$	.0082077	.0084164	0.98	0.329	-.0082882	.0247036
$me0_{7m}$	-.0369869	.0674435	-0.55	0.583	-.1691738	.0951999
$me1_{7m}$	-.0291552	.0451443	-0.65	0.518	-.1176364	.0593259
$me2_{7m}$	-.0049656	.0133	-0.37	0.709	-.0310331	.0211019
$me3_{7m}$	.0609014	.0986592	0.62	0.537	-.1324672	.2542699
$me4_{7m}$	.0102064	.0178787	0.57	0.568	-.0248352	.045248
$me0_{8m}$	-.0164481	.0817089	-0.20	0.840	-.1765946	.1436985
$me1_{8m}$	-.0119896	.0519691	-0.23	0.818	-.1138472	.0898679
$me2_{8m}$	-.0009434	.0138114	-0.07	0.946	-.0280132	.0261264
$me3_{8m}$	.025987	.1170299	0.22	0.824	-.2033873	.2553613
$me4_{8m}$	.0033941	.0174641	0.19	0.846	-.0308349	.0376231
$me0_{3f}$	-.1239576	.1243801	-1.00	0.319	-.3677382	.1198229
$me1_{3f}$	-.0328852	.0297108	-1.11	0.268	-.0911173	.0253469
$me2_{3f}$	.027637	.0329414	0.84	0.401	-.0369269	.0922009
$me3_{3f}$	.1217515	.1058499	1.15	0.250	-.0857105	.3292135
$me4_{3f}$	.0074544	.0108136	0.69	0.491	-.0137399	.0286486
$me0_{4f}$	-.0253278	.068778	-0.37	0.713	-.1601302	.1094747
$me1_{4f}$	-.0063751	.0173171	-0.37	0.713	-.0403159	.0275657
$me2_{4f}$	.005731	.0164839	0.35	0.728	-.0265769	.0380389
$me3_{4f}$	.0244799	.0646005	0.38	0.705	-.1021347	.1510944
$me4_{4f}$	.0014919	.0043562	0.34	0.732	-.0070461	.0100299
$me0_{5f}$	.0687081	.0509396	1.35	0.177	-.0311316	.1685479
$me1_{5f}$	.0415787	.0356309	1.17	0.243	-.0282565	.1114139
$me2_{5f}$	-.0015858	.0104186	-0.15	0.879	-.0220058	.0188342
$me3_{5f}$	-.0971605	.0766509	-1.27	0.205	-.2473934	.0530725
$me4_{5f}$	-.0115405	.0138457	-0.83	0.405	-.0386775	.0155966
$me0_{6f}$	.0323267	.0642038	0.50	0.615	-.0935104	.1581638
$me1_{6f}$	.0180261	.0365282	0.49	0.622	-.0535678	.0896201
$me2_{6f}$	-.0018965	.0095666	-0.20	0.843	-.0206467	.0168537
$me3_{6f}$	-.0438458	.0866248	-0.51	0.613	-.2136272	.1259357
$me4_{6f}$	-.0046106	.0113587	-0.41	0.685	-.0268732	.0176521
$me0_{7f}$	-.0534697	.0752558	-0.71	0.477	-.2009685	.094029
$me1_{7f}$	-.0206909	.0264992	-0.78	0.435	-.0726284	.0312467
$me2_{7f}$	.0082421	.0159583	0.52	0.606	-.0230355	.0395197
$me3_{7f}$	.0608204	.0790659	0.77	0.442	-.0941459	.2157867
$me4_{7f}$	.0050981	.0073761	0.69	0.489	-.0093588	.019555
$me0_{8f}$	.0459586	.0887619	0.52	0.605	-.1280114	.2199287
$me1_{8f}$	.0157106	.0319778	0.49	0.623	-.0469647	.078386
$me2_{8f}$	-.0088347	.0182866	-0.48	0.629	-.0446758	.0270065
$me3_{8f}$	-.049489	.0956355	-0.52	0.605	-.236931	.1379531
$me4_{8f}$	-.0033456	.0088801	-0.38	0.706	-.0207502	.014059

Note: mei_{jg} is the IATE corresponding to the category i of the outcome variable obtained for state group S_j. $g = f$ is the effect on females and $g = m$ is the effect on males.

Figure 3.4: Ratios father-mother of means of time spent in childcare activities by different demographic subgroups (hours per week).



Source: Ratios computed using data in Table 1 in (**?**) based on the 2003-2006 waves of the American Time Use Survey (ATUS). Childcare activities are classified into: “Basic” childcare (breast feeding, rocking a child to sleep, general feeding, changing diapers, providing medical care to child, grooming child, etc.); “Educational” childcare (reading to children, teaching children, helping children with homework, attending meetings at a child’s school, etc.); “Recreational” childcare (playing games with children, playing outdoors with children, attending a child’s sporting event or dance recital, going to the zoo with children, taking walks with children, etc.); “Travel” childcare (any travel related to any of the three other categories of childcare). Samples include all individuals between the ages of 21 and 55 (inclusive) who had time diaries summing to a complete day and at least one child under the age of 18.

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