MONEY FOR NOTHING? THE EFFECT OF OPORTUNIDADES ON UPPER SECONDARY COMPLETION

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Abstract

Enrollment and completion rates at the Upper Secondary Education (USE) level are the lowest of any tier in the Mexican education system. One of the policies intended to address this issue is the conditional cash transfer program *Oportunidades*, which targets the country's most disadvantaged households and provides higher subsidies for girls than for boys. Impact evaluations of the program have proven its effectiveness in primary and lower secondary education. However, its effect on USE completion has not been analyzed and little is known about the gender heterogeneity effects at this level of schooling. Using a Difference-in-Difference analysis, I find that *Oportunidades* has a negative effect on boys' USE completion and no effect on girls. This evidence suggests that redesigning the program is necessary in order to improve its effectiveness on USE outcomes.

Key Words: *Oportunidades*, Mexico, Upper Secondary Education Completion, Gender, Conditional Cash Transfer Programs

Introduction

Enrollment and completion rates at the Upper Secondary Education (USE) level are the lowest of any tier in the Mexican education system. At present, two thirds of adults aged 25 to 64 have not completed USE, the USE dropout rate is 14.3%, and 30% of currently-enrolled USE students are behind in school (OECD 2015). One of the most important national policies intended to address these issues is the conditional cash transfer (CCT)[1] program *Oportunidades*, which, after showing positive results at the primary and lower secondary education levels, started providing monetary transfers for USE students in 2001.

As the main development policy for the last four political administrations in Mexico, researchers have continuously analyzed *Oportunidades'* impacts. On the one hand, *Oportunidades* proved to be effective in increasing enrollment, attendance, and schooling-years completed at the primary and lower secondary education levels (Parker 2003). On the other hand, evidence on USE completion is less conclusive. Although former impact evaluations suggest that Oportunidades' effect on USE students followed a similar trend, these studies focused on short periods and separately analyzed rural and urban contexts (Skoufias et al. 2001; Behrman, Parker, and Todd 2011; Behrman et al. 2012). Thus, this literature was not able to measure the effect of the program once authorities scaled it to urban and semi-urban communities. Furthermore, impacts on USE completion have not been analyzed, nor its different effects on girls and boys at this level.

Understanding these outcomes is critical because USE's enrollment rates only increased eight percentage points in the last ten years, and completion rates have remained low since 2008 (INEE 2015; World Bank 2016). Also, analyzing its gender heterogeneity effects is relevant

[1] CCTs provide monetary or non-monetary subsidies to families conditioned on certain behaviors. In the case of *Oportunidades*, economic transfers are given to disadvantaged families in exchange for health check-ups and children's school attendance.

because *Oportunidades* provides higher subsidies for girls at the secondary level, even though boys drop out more frequently than girls and former policies reversed gender gaps in school enrollment (Creighton and Park 2010; INEE 2015). Moreover, some studies acknowledge the importance of identifying the impact of *Oportunidades* in USE and propose to reallocate resources from primary to secondary education with the objectives of reducing the program's cost and improving its effectiveness on educational attainment (Todd and Wolpin 2006; Attanasio, Meghir, and Santiago 2011). Therefore, in order to efficiently redesign *Oportunidades*, Mexican decision makers require an updated estimation of the program's impact with a special focus on gender heterogeneity effects.

By utilizing a Difference-in-Difference methodology, I evaluate the effect of *Oportunidades* eligibility on girls' and boys' USE completion. [2] To perform this analysis, I use the three waves – 2002, 2005, 2009 – of the only longitudinal database available in Mexico (Mexican Family Life Survey), which provides information for cohorts that enrolled in USE before the subsidy was implemented and cohorts that enrolled in USE once the subsidy was available. This study aims to provide important information to help improve educational outcomes while allowing for the consideration of gender differences.

The paper will proceed as follows: first, I describe Mexico's USE. Second, I analyze the literature on CCTs in the international context as well as *Oportunidades*' structure and previous impact evaluations. Third, I detail the data, methodology, and findings of the research. In the last sections, I provide a discussion of the results and the conclusion of my analysis.

[2] In this study, a student is considered eligible for *Oportunidades* if at least one member of her family – including herself – reported receiving a transfer from the program in the three years before each wave of information was collected.

Mexico's Upper Secondary Education

There are three main stages in Mexico's compulsory education. The last stage is USE, and it includes three modalities (general, technological, and technical). USE's duration is from two to three years, with students entering at either 14 or 15. Despite its compulsory nature, enrollment rates have remained low for the last eight years (see Figure 1), and completion rates are almost 40 percentage points lower than the OECD's average (OECD 2015). Also, in 2013-2014, Mexico was the only OECD member where less than 60% of 15-19 year-olds were enrolled in USE, with just six out of ten enrolled students graduating (Avitabile and De Hoyos 2015).[3]

[Figure 1 about here]

In terms of gender disparities in school enrollment, at present, there are no significant differences at the lowest levels of education in Mexico (Creighton and Park 2010). Parker and Pederzini (2001) argue that gender gaps in education generally exist in the oldest Mexican cohorts because gender gaps in primary and lower secondary education were eliminated in 1976 and have been reversing since then (Creighton and Park 2010). This trend persists in USE, where girls have been enrolling more and attaining higher levels of education than boys. However, when policy makers first implemented *Oportunidades*, it was reported that girls in secondary education were facing higher risks than boys.[4] Currently, at the national level, 56.3% of girls enroll in USE, versus 53.3% of boys, and only three out of the 32 states have slightly higher enrollment rates for boys (INEE 2015).[5]

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[3] In terms of social context, in the most marginalized communities, the majority of schools are primary and lower secondary schools. In communities with less than 2,500 inhabitants, only 30% of schools are at the USE level (INEE 2015).

[4] *Oportunidades* continues to operate under the assumption that girls face higher educational risks than boys. However, they do not provide data on how its beneficiaries have been performing.

[5] Only one of these three states (Chiapas) is highly marginalized, and *Oportunidades* does not include a specific treatment for these students. Enrollment rates in Chiapas at USE are 55.5% for boys and 50.7% for girls. Enrollment

Regarding gender gaps in school completion, in 2013-2014, according to official data, the dropout rate for USE was 14.3% – with rates of 16.3% for boys and 12.3% for girls – and the "completion efficiency"[6] rate was 63% – with rates of 59.1% for boys and 67.1% for girls (INEE 2015). [7] Although the data show that, at the national level, girls face lower risk of not completing USE, *Oportunidades* is still providing higher subsidies for girls than for boys. These higher subsidies for girls might be due to the government's concern that girls' enrollment depends on post-graduation job expectations and could decrease again without the subsidy.[8] However, given historic trends, the elimination of gender gaps that took place over the last 40 years appears to be independent of *Oportunidades* and is more likely the result of previous mass schooling policies (Creighton and Park 2010).

Conditional Cash Transfers, *Oportunidades* and Impact Evaluations

Over the last two decades, many low- and middle-income countries began to implement CCT programs in order to increase investments in human capital and break cycles of poverty among the most disadvantaged families. After the 1997 Mexican experiment generated positive outcomes, many countries in Latin-America implemented similar CCT designs. In Central America – specifically Guatemala, Honduras and Nicaragua – CCTs proved successful in

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rates in Durango at USE are 62.2% for boys and 60.7% for girls. Enrollment rates in Nuevo Leon at USE are 51.2% for boys and 49.5% for girls.

[6] Estimated proportion of students that complete a level of education in a given year out of the newly enrolled students in each of the grades of the analyzed level.

[7] There is no official data on "completion efficiency" rates before 2008. In this year, rates varied from 42.4% to 62.2% depending on the USE modality. Rates for 2009 varied from 46.4% to 60.9%. Rates for 2010 varied from 50% to 64.5%. Rates for 2011 varied from 44.9% to 65.9%. Rates for 2012 varied from 47.6% to 63.9%. Rates for 2013 varied from 51.4% to 67.4%. In all the cases, completion rates were higher for girls.

[8] Parker and Pederzini (2001) affirm that female educational investments depend on female perceptions of work and on parents' investment decisions. Vella (1994) argues that girls' job expectations and factors of discrimination are determined before they come into contact with the education process. These factors shape their human capital attainment, labor supply, and rates of return to education. In the case of parents' investments, parents may invest differently in their sons' and daughters' educations based on who will be more likely to support them economically (King and Hill 1997), on whom parents feel more sympathy or altruism toward, or on the need to compensate for future disadvantages (Alderman and King 1998).

improving enrollment and attendance, and authorities have institutionalized them as national policies (Reimers, Da Silva, and Trevino 2006).

In general, for developing countries, CCTs have been highly effective. A meta-analysis of impact evaluations of CCTs in 15 developing countries[9] by Saavedra and Garcia (2012) finds that CCTs are effective at improving educational outcomes, with the magnitudes of the impacts varying according to the level of education targeted, the amount of the subsidy, and the periodicity of the transfers. According to the authors, on average, CCTs' effect on enrollment, attendance, and dropout are usually greater for secondary than for primary education. Also, this meta-analysis finds that there is a positive relationship between the amount of the transfer and its effect on the outcome, indicating that higher transfers compensate more for the opportunity cost of sending children to school. Furthermore, the authors describe that CCTs are more effective when payments are less frequent (quarterly or bimonthly vs. monthly), and that adding conditions on students' academic achievement – most CCTs only condition transfers on enrollment and regular attendance – positively correlates with increases in enrollment and attendance in secondary education. The study concludes that CCT's effects are independent from how programs assign benefits, and that with limited economic resources, targeting CCTs' transfers to secondary education improves their cost-effectiveness.

Oportunidades

In order to address Mexico's extreme poverty and low investment in human capital, the Ministry of Social Development launched *Oportunidades* in 1997 as a randomized control trial of a CCT scheme. The program consists of providing monetary subsidies to the most disadvantaged families in exchange for health and nutrition check-ups and their children's

[9] Bangladesh, Brazil, Cambodia, Colombia, Costa Rica, Ecuador, Honduras, Indonesia, Jamaica, Malawi, Mexico, Nicaragua, Pakistan, Turkey, and Uruguay.

regular school attendance. For this purpose, subsidies are typically given to the mother of the household.

The 1997 pilot of the program involved 320 highly marginalized rural communities across the country. Officials based the selection of households on a poverty index that included social and economic indicators at the family level – family and household infrastructure characteristics – collected in the Survey of Household Socioeconomic Characteristics (ENCASEH), the baseline survey of the experiment. After initial successes increasing enrollment and attendance during the first three years, in 2000, program designers began to include other rural communities. In 2001, subsidies were scaled up to semi-urban and urban communities (Behrman, Parker, and Todd 2011). In this non-experimental phase, program designers selected families based on a marginalization index – using only socioeconomic indicators at the locality level derived from the 2000 national population census. As the program became a federal policy in 2000, they used the marginalization index for the identification of communities and selected households based on a poverty index similar to the one used in the experimental phase (Ranzani and Rosati 2014).[10]

In the 1997 pilot of the program, *Oportunidades* only provided education subsidies for students in primary and lower secondary education. The program introduced subsidies for USE when it became national policy in 2001. It gave 13% higher payments for girls than for boys (CNPPIS 2015). However, according to Behrman et al (2012), in that year, not all eligible families were aware of their eligibility status and did not apply to receive the benefits. Although benefits are the same for rural and urban communities, in 2001, the procedure to become a

[10] The variables considered in both indexes and their cutoff points are not publicly available. Behrman, Parker, and Todd (2011) and Behrman et al. (2012) estimate that the factors that determine the probability of participating in the program include variables like age, gender, schooling, missing grades and employment of the household head and his/her spouse; whether the household head and spouse speak an indigenous language; household income, and number, gender and age of siblings.

beneficiary differed between them. For budgetary reasons, program designers targeted rural families automatically, while inviting urban families to sign up if they considered themselves eligible (Behrman et al. 2012).

During the 2001-2002 school year, the monthly subsidy for girls in their third year of USE was 610 Mexican pesos (61USD); for boys it was 535 Mexican pesos (53 USD).[11] Figure 2 lists the amounts the subsidy paid to boys and girls in each grade of USE since 2001. At present, the program provides monetary transfers in all the rural, semi-urban, and urban communities of the country and benefits 1.1 million USE students (CNPPIS 2015).[12]

[Figure 2 about here]

Oportunidades' Educational Outcomes

Most of the literature on the impact of *Oportunidades* is based on official information from the program's pilot. This database includes a follow-up of the rural families that participated in the experimental phase, which only included subsidies for primary and lower secondary education in a small number of communities. As I state earlier, when the program expanded to urban communities in 2001, subsidies for USE were introduced and, for a sample of these communities, another three-year database was made available.

In one of the first evaluations of *Oportunidades* and based on official information, Skoufias et al. (2001) find that the program increased school attendance by 4.3 percentage points for boys and 7.8 percentage points for girls across primary and lower secondary education. Similarly, Parker (2003) – one of the most robust evaluations of the program – performs an analysis at the school level based on the estimated proportion of beneficiaries in each school. In

[11] These amounts are presented in Mexican pesos and US dollars according to their value 2001. The subsidy represents a third of average monthly income for a family in the first decile of income (INEGI 2015).
[12] The program benefits 6.1 million students in the three levels of compulsory education (2.2 million students in lower secondary, and 2.9 million students in primary education) (CNPPIS 2015).

general, her evaluation shows that *Oportunidades'* effects on secondary education (upper and lower) in rural communities are bigger than the ones in semi-urban and urban communities.[13] In USE, the study finds that the program, in its first year, increased average enrollment in rural localities by 37.8% (with a slightly higher effect for girls), and in semi-urban and urban localities by 6.4% (with almost double the effect for girls compared to boys: 8% versus 4.9%).[14]

In more recent evaluations, Behrman, Parker, and Todd (2011) analyze *Oportunidades*' long-term effects in rural communities and show that, after 18 months of exposure, boys and girls from 9-15 years old increased their total schooling by two months. According to the authors, this effect remained significant five years after the implementation of the program, with the largest impacts observed on students that had completed at least five years of schooling by 1997. Regarding urban communities, Behrman, et al. (2012) find that *Oportunidades* increased students' attainment by about two months, and that enrollment increased by two to three percentage points, with no significant differences between boys and girls in secondary education. Both studies analyzed the rural and urban official samples.

This literature shows that *Oportunidades* increases enrollment, attendance, and attainment for students in the three levels of education, and that girls usually benefit more than boys. Nonetheless, there are important gaps in the research. First, according to Attanasio, Meghir, and Santiago (2011), literature based on the experimental datasets answers limited questions and does not extrapolate to groups or parameters that differ from the initial experimental design. Second, researchers do not discuss relevant outcomes such as USE

[13] One of the strengths of Parker's methodology is that it does not use the experimental sample of the program. It analyzes non-public information regarding school characteristics from the school years 1995-1996 to 2001-2002. Moreover, it also includes information on all the students participating in the program, which allows the identification of beneficiaries in each school and the analysis of their educational trajectories.

[14] In the lower secondary level, Parker's study finds the following effects on school average enrollment: an increase of 23.2% (28.7% for girls and 15.7% for boys) in rural localities; an increase of 6.5% in semi-urban localities (with insignificant impact on boys), and no significant effects in urban localities.

completion. And third, there is not enough information to describe *Oportunidades*' gender heterogeneity effects in USE. In this research, I fill these gaps by analyzing longitudinal information for Mexican USE students, comparing those who did and did not receive subsidies through *Oportunidades*, and over a period before and after the implementation of the program at this level (2001-2009).

Strategy, Data and Method

Oportunidades' general objectives include alleviating poverty and increasing human capital. In the short run, the program intends to alleviate poverty through the provision of economic support conditioned on education, health, and nutrition. In the long run, the program aims to eliminate the intergenerational transmission of poverty through investments in human capital (Reimers, Da Silva, and Trevino 2006). Because human capital is usually measured as years of schooling, in this paper, I evaluate whether *Oportunidades* has achieved its objectives by measuring its impact on students' USE educational attainment. Specifically, I analyze whether students complete USE, focusing on gender differences, and how these might affect the design of the program. In order to achieve this objective, this paper answers the following questions:

Research Question 1: What is the effect of *Oportunidades* on completing USE?

Research Question 2: Does the program affect boys' and girls' USE completion differently?

Because *Oportunidades* started as a randomized control trial focused on rural communities but, at present, has expanded to urban and semi-urban communities with subsidies that are no longer assigned randomly, I adopt a Difference-in-Difference approach to compare the outcomes of a cohort of students that completed lower secondary education before the

subsidy was implemented and a cohort that completed lower secondary education once the subsidy was available.

Data

I use the Mexican Family Life Survey (MxFLS), which is a nationally representative survey that followed individuals and their families during the period between the school years 2001-2002 and 2008-2009. This database provides information at the individual, family, and community levels and serves the purpose of generating useful data for the evaluation of national public programs like *Oportunidades*. Moreover, it is the only publicly available longitudinal database in Mexico and, unlike other public sources of information, it not only contains information relevant to each of the analyzed waves, but also for the two previous years of each wave. For example, regarding my study, in the first wave (2001-2002), each individual reported if she was receiving the *Oportunidades* subsidy during the years 2002, 2001 and 2000. Because of this, I am able to identify which families were eligible to receive the USE subsidy before the students enrolled in USE.

The MxFLS-1 – first wave of the panel – covers the time period 2001-2002 and includes educational, economic, and sociodemographic variables for 8,440 households in 150 rural and urban communities of all the country. The next two waves, MxFLS-2 (2004-2005) and MxFLS-3 (2008-2009), survey 90% of individuals from the first wave. This allows me to follow students and estimate whether the cohorts that completed their lower secondary education in 2002 and 2005 completed USE in a period of at least three years.

Sample

My methodology draws on a Difference-in-Difference model that compares USE completion between eligible and non-eligible students before and after the policy was implemented.[15] The "before cohort" comprises students who reported completing the 9th grade – the last grade of lower secondary education – and were aged between 14 and 16 years old[16] when the first wave of the survey was collected. I assume that these students decided whether to enroll in the 10th grade (first year of USE) in the school year 2001-2002 before policy makers announced and implemented the subsidy in that same year.

This assumption is pertinent for two reasons. First, in 2002, the survey supposed that only students in primary and lower secondary received the subsidy and kept that structure in the following two waves – i.e., students in USE were not asked about *Oportunidades* in any of the three years the survey was collected. Second, according to Behrman et al. (2012), in 2001, one third of the households in urban areas were not aware of their eligibility to the program. Also, in 2001, out of the 3.1 million students enrolled at this level, only 266,965 received the USE subsidy, and, by 2005, the amount of USE beneficiaries doubled to 616,044 – out of 3.5 enrolled in USE. Although the number of beneficiaries in 2005 is not large, it is significant that the program grew that much over time, which could imply that it was not fully implemented in 2001. At present, there are more than four million students enrolled in USE and, as I state in previous sections, 1.1 million students receive the subsidy (CNPPIS, 2015).

[15] MxFLS does not provide information about whether the USE student received the *Oportunidades* subsidy. Therefore, I estimate the impact of the program by using a student's eligibility to receive the subsidy as a proxy for actual subsidy use. A similar approach is performed in Dynarski (2003).

[16] Although the official optimal age to enroll in USE is 14-15 years old, the majority of the observations are 16 years old. The high amount of 16 year-olds can be explained by day of birth (if the student was born after August, she might have been forced to wait a year to enroll in primary school); by the type of preschool (some private schools offer an extra year fully dedicated to English learning); or by the student dropping out or repeating one grade. Within this age period, I include students who finished lower secondary education on time or, for any reason, missed one year.

The "after cohort" comprises students who reported completing the 9th grade and were aged 14-16 years old when the second wave of the survey was collected (2004-2005 school year). Based on this information, my analysis includes four groups. Three groups belong to the control and one group belongs to the treatment. The control includes the eligible and non-eligible students of the "before cohort" and the non-eligible students of the "after cohort." The treatment comprises eligible students in the "after cohort."

The outcome of the "before cohort" is USE completion in 2005 (complete 12th grade), and the outcome of the "after cohort" is USE completion in 2009 (complete 12th grade). This means that the students from the "before cohort" had three years to complete USE while, because of the structure of the third wave, whose collection took three years (from 2009 to 2012), students from the "after cohort" had between 3 to 5 years to complete USE.[17]

Because my data include three waves and not all of the students were re-contacted after the first wave, some observations do not have an outcome. This required dropping 38 observations from the "before cohort" and 93 observations for the "after cohort". Tables 1 and 2 present the general summary statistics of the utilized samples.[18] The general sample provides information for all the students aged 14-16 who completed the 9th grade in their corresponding cohort. The analytical sample includes the students for whom I could measure their outcome. Nonetheless, as is shown in both tables, there are no significant differences between the means of the most relevant covariates in both samples. Therefore, the generalizability of my results is not compromised.[19]

[17] 63% of the students in the "after cohort" had 3 years to complete USE; 25% had 4 years; and the rest had 5 years. This challenge was solved by controlling for age of completion fixed-effects.
[18] Summary statistics were weighted by 2002 and 2005 sample weights. Descriptive and causal results are unchanged if weights are not considered.
[19] The survey does not provide specific information on whether the student lives in a rural community. Future studies might be able to estimate this variable by merging the MxFLS with census data. For the purpose of this study, the variable was not necessary and the heterogeneity analysis was only based on gender differences.

[Table 1 about here]

[Table 2 about here]

In table 3, I present the means for eligible and non-eligible students before and after the policy was introduced in USE. It shows that students in the eligible group belong to bigger families – in terms of the number of family members and siblings – and are more likely to be indigenous than those in the non-eligible group. This is not surprising because the subsidy targets families in extreme poverty who are usually located in rural and indigenous communities.

Regarding non-eligible students in both cohorts, they tend to belong to wealthier families where the head of the household and the spouse have higher levels of education. Also, in both cohorts, the head of the household is usually a male. In terms of the employment status[20] of the students and the head of the household, there are no significant differences between groups. However, in terms of the employment status of the spouse of the head of the household, which is usually a female, the non-eligible group has a higher mean. Differences are stable over time, which, according to Dynarski (2003), could imply that covariates are following a common trend, which is the main assumption needed for the Difference-in-Difference approach.

[Table 3 about here]

Method

My methodology is based on the following Difference-in-Difference model:

$$USE\ Completion_i = \alpha + \beta\ (Oportunidades_i * AfterCohort_i) + \gamma\ Oportunidades_i + \delta Aftercohort_i + \pi_i$$

USE Completion_i is a binary variable that measures if the student completed USE. $Oportunidades_i$ is a binary variable that measures if the student belonged to a family in which at least one of the members received aid from *Oportunidades* in the last three years. If the student,

[20] This variable refers to whether the observation declared on the survey to have worked in the previous 12 months.

or at least one of her siblings, received the subsidy during her primary or lower secondary education, the variable will have the value of 1. Also, if one of the members of the family reported receiving any income from the program, the student will have a value of 1.[21] Because *Oportunidades* eligibility is the same across educational levels, this variable indicates if the student is eligible to receive the subsidy in USE. $AfterCohort_i$ is a binary variable that indicates if the student belonged to a cohort that completed lower secondary education (9 years of education) and was on time to enroll in USE in 2005 (when the subsidy was nationally available for this level) – i.e. AfterCohort has a value of zero if students belong to the "before cohort" and a value of one if students belong to the "after cohort".

The interaction term captures the effect of being eligible for *Oportunidades* by controlling for changes over time in USE completion of those students eligible to receive the subsidy and those who were not eligible. This term is designed to estimate the causal effect of being eligible for *Oportunidades* on USE completion, rather than actually benefitting from the program. I assume that the only difference between the two cohorts is that one had access to *Oportunidades* and the other did not. This estimate captures the Intention to Treat (ITT) of the policy.

Findings

My research is focused on understanding the impact of being eligible for *Oportunidades* on completing USE. Because the program provides higher subsidies for girls than for boys, I also analyze gender heterogeneity effects. Using a Difference-in-Difference methodology that compares eligible and non-eligible students before and after the policy was implemented, I find

[21] The survey asks all the family members if they receive income from *Oportunidades*. In the case of children in primary and lower secondary education, it specifically asks if they receive the *Oportunidades* scholarship. By including the answers of all the family members, I was able to increase the size of the sample, because not all of the students answered the question.

no significant effect of being eligible for the program on USE completion. Before providing more detail on this finding, it is important to note the first row of table 3, which describes the differences in means of USE completion rates for eligible and non-eligible students in the analyzed cohorts.

This row shows that, regardless of *Oportunidades* eligibility, the majority of the students in Mexico tend to not complete USE. These rates are consistent with the previously described low completion rates and the low level of educational attainment achieved by Mexicans currently aged 25 to 64 (INEE 2015; OECD 2015). Also, the row shows that non-eligible students tend to complete USE more often than eligible students, which is expected, given that disadvantaged students generally attain fewer years of education. It is important to note that completion rates increased over time. In fact, both treatment and control groups increased their USE completion, but at different rates. However, if the growth of the control group (the non-eligible group) does not represent the counterfactual scenario of the eligible group in the absence of *Oportunidades*, then the assumption that the control group would difference out the time trend from the completion rate difference between cohorts will be wrong. I further discuss this issue in the next section.

Presented in table 4 is the Difference-in-Difference estimate for USE completion. I weighted all the estimates by each household sample weights – results are unchanged if I do not consider weights – and I corrected standard errors for heteroscedasticity due to the dichotomous nature of the dependent variable.[22] Because there is a small number of families with siblings aged 14-16 years old in the sample (9 families), I did not correct standard errors for within-household correlation. In the first column (Model 1), I report the Difference-in-Difference estimation with a full sample and give the coefficient of interest – *Oportunidades**AfterCohort –

[22] Based on Dynarski (2003), the analysis was performed with an OLS, rather than a Probit or Logit model.

in the first row. As shown in the table, the estimated effect of being eligible for *Oportunidades* once the subsidy was available is negative, but not significant. If I assume that *Oportunidades* did not have a negative effective in the eligible group – which is likely given the program's impact in lower levels of education – I can only conclude that *Oportunidades*' effect, if there is any, was not as big as the growth in the USE completion rates that non-eligible students had at that time.

In order to test the robustness of the analysis, Model 2 and Model 3 show the Difference-in-Difference estimates by controlling for the reported age of USE completion and region fixed-effects. Because not all of the students live in the same state and some students in the "after cohort" had more than three years to complete USE, controlling for these variations could have changed the result. However, as both model show, the negative and not significant estimate remains.[23]

Also, along the lines of Dynarski (2003), I estimated two models (Models 4 and 5) with a set of sociodemographic covariates and their interactions with *Oportunidades* eligibility and the indicator of whether the students belonged to the "after cohort". The new interaction terms are intended to absorb bias caused by the relationship between the covariates and the eligibility status. Similar to the fixed-effect models, the estimate of the impact of *Oportunidades* remains not significant. Although, for Models 4 and 5, it increases its magnitude and becomes positive when I include the interaction terms.[24]

[Table 4 about here]

[23] For the age of completion fixed-effect analysis, 34 observations were dropped. There were inconsistencies with the differences between the reported age in the first and second wave and the outcome wave. Specifically, differences were higher than expected (more than 6 years) or even negative.

[24] Models 4 and 5 were also calculated with age of completion and region fixed-effects with no significant changes on the variables of interest. Because of the nature of the survey – not all the individuals answered all the questions – half of the observations were lost. Samples were fixed through Imputation. Estimations are presented in Appendix A1.

Gender Heterogeneity Effects

I show the USE completion rates of boys and girls in table 5. As I mention before, regardless of gender, the majority of the students do not complete USE and there is an increase in completion rates for both genders over time. One important insight of this table is that, in the "before cohort", completion rates were higher for girls than for boys, and, in the "after cohort", gender differences in completion were not statistically significant.

[Table 5 about here]

As seen in table 6, boys increased their USE completion at higher rates than girls. However, in the case of girls, the eligible group grows at a faster rate than the non-eligible group, and, in the case of boys, the opposite happens. This is interesting because, in the "before cohort", both eligible and non-eligible boys had the same completion rates and, in the "after cohort", the non-eligible group highly increased its completion.

[Table 6 about here]

Regarding the Difference-in-Difference analysis, in table 7, I provide estimates of the gender heterogeneity effects. The first model uses the whole sample and identifies the impact of the program on girls by interacting all the variables with the Female indicator. This estimate is shown in the second row – *Oportunidades**AfterCohort*Female – and, similar to the general analysis, does not show a significant effect of the program. The second model only uses the female sample, and the third model is focused on males only. For theses analyses, I provide the causal estimate in the first row, an estimate consistent with the previously explained models. There are large gender differences favoring girls, although these are not significant. [25]

[Table 7 about here]

[25] Because of the nature of the survey – not all the individuals answered all the questions – half of the observations were lost. Samples were fixed through Imputation.

However, when I separately analyze females and males and include covariates and region fixed-effects in the models, I find that being eligible for *Oportunidades* has no significant effect on girls and a negative and significant effect for boys. Estimates for boys are shown in table 8 and the rest of the robustness checks for the gender heterogeneity analysis are presented in Appendix A2.

[Table 8 about here]

Discussion

I find no significant impact of the program on girls' USE completion and a negative and significant effect on boys. However, these findings do not necessarily suggest that the program is not achieving its objectives. Estimates might imply that the effect of *Oportunidades*, if there is any, was not as big as the growth in the USE completion rates of the comparison group, growth that was mainly driven by non-eligible boys.

In this sense, one should question whether the non-eligible students group is a good approximation of the eligible group in absence of *Oportunidades* (i.e., the counterfactual scenario). If this assumption is not met and the non-eligible group is growing at higher rates than expected, then the model might not be capturing the real impact of the program.

There are alternative explanations for the estimated effects. First, for the identification strategy, I assume that the control groups remained untreated in the analyzed periods. However, if students in the non-eligible group managed to receive the subsidy, they would be, again, violating the assumption that they represent the treated group in absence of the treatment. This is of particular concern because studies suggest that, in practice, CCTs do not necessarily target the poorest individuals, and they tend to benefit students that would have enrolled in school independently of the subsidy (Rawlings and Rubio 2005).

Second, USE subsidies were implemented in 2001, and I assume that students in the "before cohort" (2001-2002) decided to enroll in USE before officials announced the subsidies. However, it is possible that students were aware of the program and joined it during their first or subsequent years of USE, or that previously-benefitting students based their USE enrollment on the expectation of the implementation of the subsidy at this level. If this happened, the cohort that entered USE in 2002 would not be a good approximation of pre-program behavior. Moreover, even if my assumption is met, it is also possible that there was enough treatment contrast between groups during the first year of USE only.

Nevertheless, the gender heterogeneity analysis does provide relevant insights for answering my second research question. Although I cannot know if higher completion rates in 2005 were related to the program, results suggest three general conclusions: a) eligible girls benefited more from *Oportunidades* than eligible boys, b) eligible boys might have been negatively affected by the program, and c) USE completion rates gap between eligible and non-eligible boys increased over time. This might imply that eligible boys require more attention from the program.

Overall, my study challenges the assumption that *Oportunidades* is effective at all levels of education and it confirms that differences in USE completion between boys and girls were decreasing over time, which is consistent with the elimination of gender gaps. An important implication of the Difference-in-Difference analysis is that the subsidies are not enough to ensure that the most disadvantaged students complete USE at the same rates as less disadvantaged groups, particularly for boys.

Conclusion & Areas for Future Research

Oportunidades is one of the most important policies intended to improve investments in education in USE. After performing a Difference-in-Difference methodology, in which I compared eligible and non-eligible students before and after the implementation of USE subsidies, I find no significant impact of the program on girls and a negative and significant effect on boys. Although *Oportunidades'* effectiveness in previous education levels suggested potential positive effects in USE, I show that the majority of students do not complete USE regardless of their eligibility for the program. Moreover, my results suggest that increases in overall USE completion rates over time are mainly driven by non-disadvantaged boys and that disadvantaged girls seem to benefit more from the program than disadvantaged boys. These results question if the program requires an increase in subsidies for USE students and whether the policy should stills assign higher subsidies for girls than for boys.

These concerns suggest the need to redesign the program in order to improve its effectiveness. One alternative policy proposed by Attanasio, Meghir, and Santiago (2011) and Todd and Wolpin (2006) is to reduce subsidies at the primary level of education, where full enrollment was achieved a few decades ago, and to reallocate these resources into USE. Because this alternative is not politically feasible – children in extreme poverty who attend primary education still depend on the subsidy –, I propose the reduction of subsidies in urban areas, where students have more incentives to enroll in school. This proposal implies maintaining the focus of the program on the most marginalized families and reallocating resources to increase subsidies for boys in USE. This way, subsidies will benefit the type of families who were originally considered in the design of the policy, and impacts could be more pronounced.

Future studies should investigate the effect of *Oportunidades* on the completion of each grade of USE in order to identify at which moment *Oportunidades* loses its impact. This analysis would also allow for measuring in which grade dropping out of school becomes attractive. Furthermore, future analyses should also determine the long-term effects of the program on professional trajectories. At present, there is no information on the long-term effects of *Oportunidades* on the rural students who were treated when the program was first implemented in 1997 and on the urban students who first benefited in 2000.

Oportunidades is the only Mexican policy intended to reduce poverty while incentivizing investments in human capital. Policy makers and researchers should constantly evaluate its design and update its implementation based on its differing benefits for girls and boys.

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Tables & Visuals

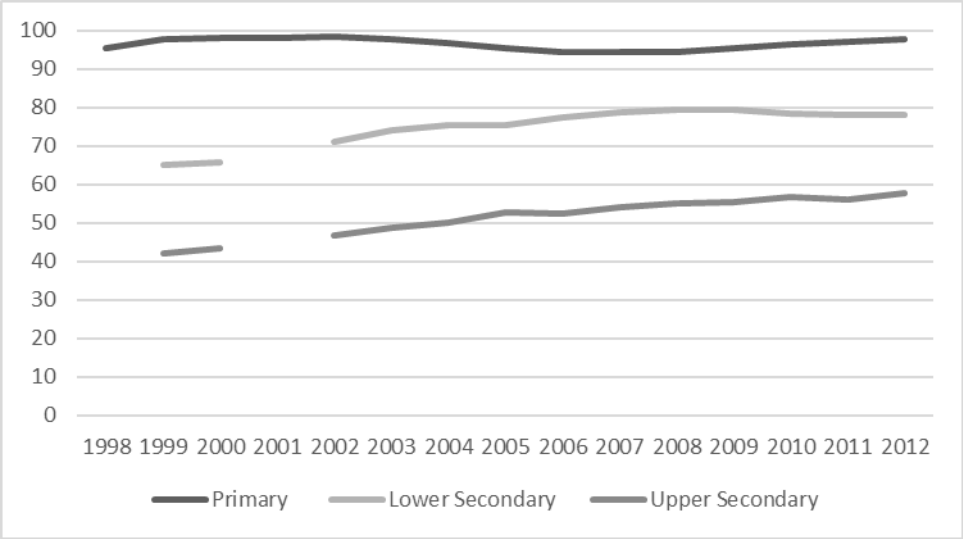

FIG. 1. — Adjusted net enrollment rates at primary and secondary education in Mexico, 1998-2012.
Source: Author's own elaboration with information from World Bank (2016).

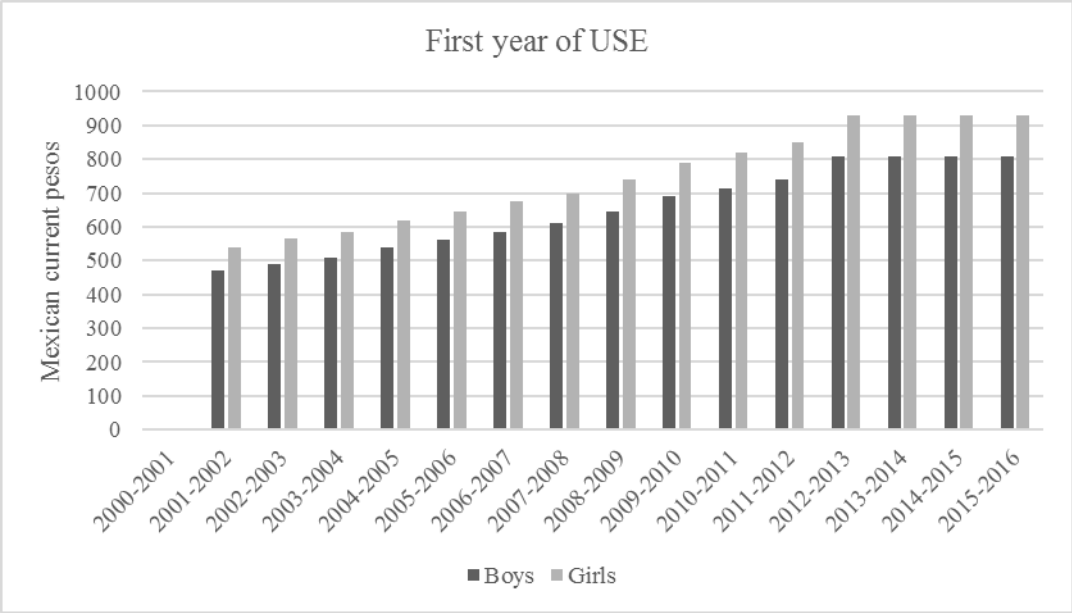

FIG. 2A. — *Oportunidades* monetary subsidies for girls and boys in the first year of Upper Secondary Education (10th), 2001-2014 (Mexican current pesos). Source: Author's own elaboration with information from CNPPIS (2015).

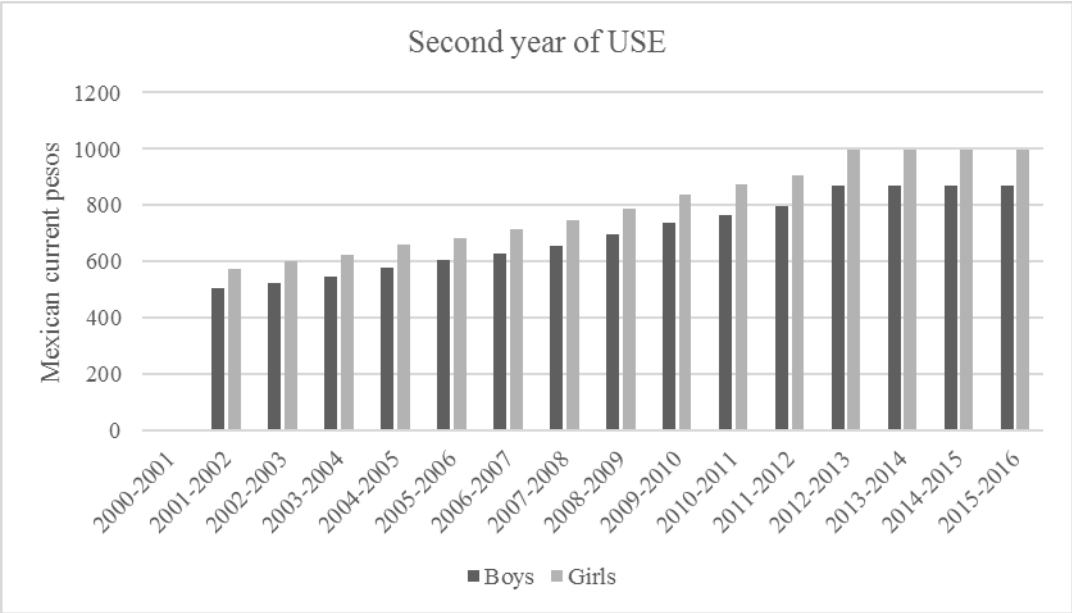

FIG. 2B. — *Oportunidades* monetary subsidies for girls and boys in the second year of Upper Secondary Education (11th grade), 2001-2014 (Mexican current pesos). Source: Author's own elaboration with information from CNPPIS (2015).

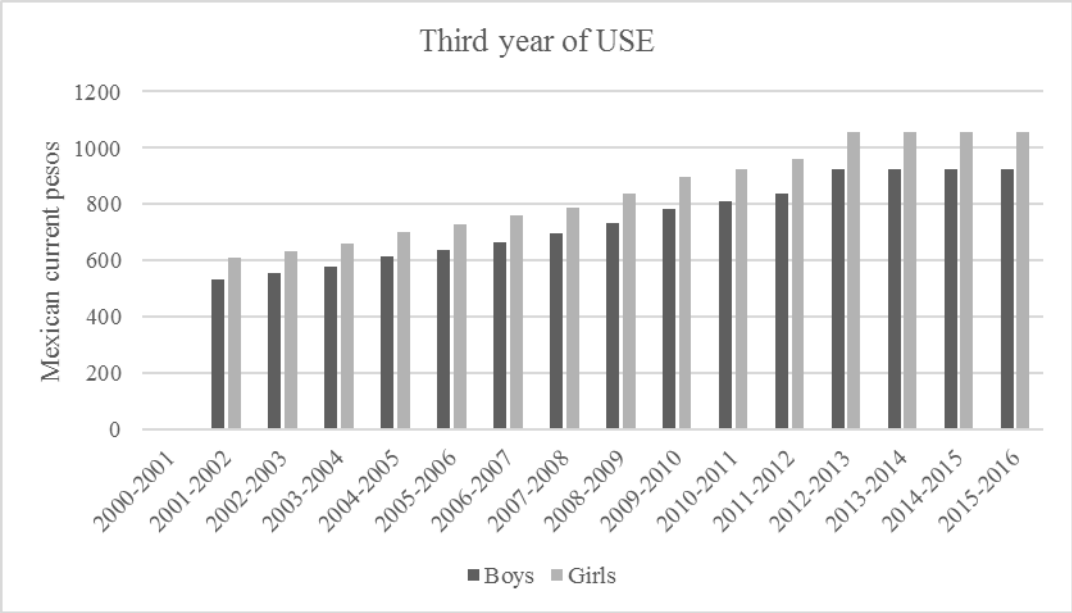

FIG. 2C. – *Oportunidades* monetary subsidies for girls and boys in the third year of Upper Secondary Education (12th grade), 2001-2014 (Mexican current pesos). Source: Author's own elaboration with information from CNPPIS (2015).

TABLE 1
SUMMARY STATISTICS FOR THE BEFORE COHORT (STUDENTS ENTERING UPPER SECONDARY EDUCATION IN 2002)

	General sample				Analytical sample				
	mean	sd	min	max	mean	sd	min	max	difference
Schooling years	9	0.00	9	9	9	0	9	9	0
Indigenous	0.07	0.25	0	1	0.07	0.26	0	1	0.00
Female	0.59	0.49	0	1	0.58	0.49	0	1	0.04
Age in 2002/2005	15.35	0.69	14	16	15.39	0.69	14	16	-0.04
Number of family members	5.46	2.00	1	17	5.73	2.12	1	17	-0.27
Number of siblings	2.95	1.65	0	10	3.17	1.71	0	10	-0.22
Education HH	6.50	4.86	0	19	5.75	4.15	0	19	0.75
Education spouse of HH	5.69	3.82	0	17	5.19	3.26	0	17	0.5
Female (HH)	0.19	0.40	0	1	0.15	0.36	0	1	0.04
Income	4615.30	5023.10	0	46000	4096.787	4163.35	0	38020.83	518.52
Oportunidades	0.14	0.34	0	1	0.23	0.42	0	1	-0.09
Employed (student)	0.19	0.39	0	1	0.21	0.73	0	1	-0.02
Employed (HH)	0.87	0.33	0	1	0.85	0.35	0	1	0.01
Employed (Spouse HH)	0.31	0.46	0	1	0.36	0.74	0	1	-0.04
Observations	350				312				662

SOURCE. — Mexican Family Life Survey (MxFLS 2002).
NOTE. — Means are of MxFLS 2002 (wave 1) and weighted by 2002 sample weights. Results are unchanged if weights are not considered. Income and household composition are measured once the students completed the last grade of lower secondary education. Both samples only include students aged 14-16 years old. HH = Head of the Household. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 2
SUMMARY STATISTICS FOR THE AFTER COHORT (STUDENTS ENTERING UPPER SECONDARY EDUCATION IN 2005)

	General sample				Analytical sample				
	mean	sd	min	max	mean	sd	min	max	difference
Schooling years	9	0	9	9	9	0	9	9	0
Indigenous	0.13	0.33	0	1	0.13	0.34	0	1	0.00
Female	0.55	0.50	0	1	0.54	0.49	0	1	0.01
Age in 2002/2005	15.17	0.72	14	16	15.20	0.72	14	16	-0.03
Number of family members	6.16	2.44	2	18	6.17	2.34	2	18	-0.01
Number of siblings	3.36	1.75	0	11	3.26	1.70	0	11	0.09
Education HH	6.52	4.67	0	19	6.19	4.49	0	19	0.33
Education spouse of HH	6.14	4.16	0	17	6.04	4.07	0	17	0.09
Female (HH)	0.18	0.39	0	1	0.15	0.35	0	1	0.03
Income	5333.41	8660.54	10	166666.7	5423.42	9211.04	10	166666.7	-90.01
Oportunidades	0.24	0.43	0	1	0.26	0.44	0	1	-0.01
Employed (student)	0.13	0.33	0	1	0.16	0.32	0	1	-0.04
Employed (HH)	0.86	0.35	0	1	0.88	0.32	0	1	-0.01
Employed (Spouse HH)	0.33	0.47	0	1	0.30	0.49	0	1	0.03
Observations	662				569				1231

SOURCE. — Mexican Family Life Survey (MxFLS 2005).
NOTE. — Means are of MxFLS 2005 (wave 2) and weighted by 2005 sample weights. Results are unchanged if weights are not considered. Income and household composition are measured once the students completed the last grade of lower secondary education. Both samples only include students aged 14-16 years old. HH = Head of the Household. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 3
SUMMARY STATISTICS: TREATMENT AND CONTROL GROUPS

	Before Cohort			After Cohort			
	Oportunidades Eligible	*Oportunidades* Non-eligible		*Oportunidades* Eligible	*Oportunidades* Non-eligible		Difference-in-Difference
	mean	Mean	difference	mean	mean	difference	
USE completion	0.16	0.24	0.08	0.31	0.41	0.10**	-0.01
	(0.39)	(0.42)		(0.47)	(0.50)		(0.09)
Schooling years	9	9	0	9	9	0	0
	(0)	(0)		(0)	(0)		(0)
Indigenous	0.17	0.05	-0.09**	0.28	0.08	-0.15***	0.04
	(0.38)	(0.22)		(0.45)	(0.27)		(0.09)
Female	0.58	0.58	0.00	0.56	0.54	-0.02	-0.022
	(0.48)	(0.49)		(0.49)	(0.50)		(0.079)
Age in 2002/2005	15.55	15.35	-0.19**	15.26	15.18	-0.07	-0.12
	(0.62)	(0.72)		(0.73)	(0.72)		(0.107)
Number of family members	6.75	5.36	-1.4***	7.02	5.87	-1.13***	-0.26
	(2.44)	(1.87)		(2.16)	(2.34)		(0.366)
Number of siblings	4.08	2.89	-1.18***	4.01	3	-1.01***	-0.16
	(2.11)	(1.50)		(1.86)	(1.56)		(0.308)
Education HH	4.04	6.28	2.24***	4.73	6.70	1.97***	0.27
	(3.26)	(4.44)		(3.6)	(4.66)		(0.09)
Education spouse HH	3.32	5.79	2.47***	4.27	6.69	2.42***	-0.06
	(2.52)	(3.60)		(3.24)	(4.16)		(0.52)
Female (HH)	0.16	0.15	-0.00	0.13	0.15	0.00	-0.009
	(0.37)	(0.39)		(0.34)	(0.35)		(0.057)
Income	2636.22	4451.01	1814.8**	3410.408	6089.463	2679.1***	-864.31
	(2143.473)	(4629.37)		(2965.742)	(10406.73)		(765.24)
Employed (student)	0.26	0.19	-0.06	0.17	0.14	-0.03	-0.03
	(0.44)	(0.40)		(0.38)	(0.32)		(0.113)
Employed (HH)	0.79	0.88	0.07	0.88	0.85	0.03	0.04
	(0.41)	(0.33)		(0.33)	(0.36)		(0.059)
Employed (Spouse HH)	0.14	0.34	0.17***	0.27	0.33	0.07*	0.09
	(0.35)	(0.47)	(2.92)	(0.45)	(0.47)		(0.12)
Observations	74	238	312	149	420	569	

SOURCE. — Mexican Family Life Survey (MxFLS 2002 and MxFLS 2005).
NOTE. — Means are of MxFLS 2002 (wave 1) and MxFLS 2005 (wave 2) and weighted by 2002 and 2005 sample weights. Results are unchanged if weights are not considered. Income and household composition are measured once the students completed the last grade of lower secondary education. HH = Head of the Household. Standard errors in parentheses. Standard errors in the Difference-in-Difference column adjusted for heteroscedasticity. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 4
DIFFERENCE-IN-DIFFERENCE ESTIMATES OF THE EFFECT OF *OPORTUNIDADES* ELIGIBILITY ON COMPLETING UPPER SECONDARY EDUCATION

	(1)	(2)	(3)	(4)	(5)
	USE comp	USE comp	USE comp	USE comp	USE comp
*Oportunidades**AfterCohort	-0.017	-0.012	-0.027	-0.042	0.079
	(0.094)	(0.100)	(0.093)	(0.093)	(0.092)
Oportunidades	-0.082	-0.043	-0.091	0.060	-1.486
	(0.072)	(0.080)	(0.074)	(0.075)	(1.080)
AfterCohort	0.159***	0.250***	0.148***	0.111**	0.262
	(0.055)	(0.056)	(0.057)	(0.051)	(1.030)
Indigenous				-0.022	-0.050
				(0.072)	(0.083)
Female				-0.012	-0.019
				(0.041)	(0.072)
Age in 2002/2005				-0.172***	-0.154***
				(0.031)	(0.051)
Number of family members				0.005	-0.009
				(0.014)	(0.026)
Number of siblings				-0.048**	-0.034
				(0.021)	(0.040)
Education HH				0.010*	0.006
				(0.006)	(0.012)
Education spouse of HH				0.007	-0.010
				(0.008)	(0.015)
Female (HH)				0.196**	0.386***
				(0.099)	(0.136)
Income				-0.000	-0.000**
				(0.000)	(0.000)
Employed (student)				-0.128***	-0.188***
				(0.049)	(0.068)
Employed (HH)				0.161**	-0.052
				(0.067)	(0.124)
Employed (Spouse HH)				0.010	0.098
				(0.053)	(0.087)
Constant	0.269***	0.204***	0.280***	2.804***	2.976***
	(0.043)	(0.042)	(0.044)	(0.517)	(0.840)
Age of USE completion F.E.		Yes			
Region F.E.			Yes		
Covariates**Oportunidades*					Yes
Covariates*AfterCohort					Yes
Observations	881	847	881	881	881
R-squared	0.028	0.104	0.056	0.193	0.242

SOURCE. — Mexican Family Life Survey (MxFLS 2002, MxFLS 2005 and MxFLS 2009).
NOTE. — Regressions weighted by 2002 and 2005 sample weights (wave 1 and wave 2). Results are unchanged if weights are not considered. 34 observations were dropped for the age of completion fixed-effect analysis. Standard errors adjusted for heteroscedasticity in parentheses. HH=Head of the Household. *** p<0.01, ** p<0.05, * p<0.1. See table A1 for additional models.

TABLE 5
UPPER SECONDARY EDUCATION COMPLETION RATES FOR GIRLS AND BOYS IN THE BEFORE AND AFTER COHORTS

	Before Cohort			After Cohort		
	Girls	Boys		Girls	Boys	
	mean	mean	difference	mean	mean	difference
USE completion	0.28	0.22	-0.06*	0.39	0.42	-0.02
	(0.45)	(0.41)		(0.49)	(0.49)	

SOURCE. — Mexican Family Life Survey (MxFLS 2002 and MxFLS 2005).
NOTE. — Means are of MxFLS 2002 (wave 1) and MxFLS 2005 (wave 2) and weighted by 2002 and 2005 sample weights. Results are unchanged if weights are not considered. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 6
UPPER SECONDARY EDUCATION COMPLETION RATES FOR GIRLS AND BOYS IN THE TREATMENT AND CONTROL GROUPS

	Before Cohort			After Cohort		
	Oportunidades Eligible	*Oportunidades* Non-eligible		*Oportunidades* Eligible	*Oportunidades* Non-eligible	
	mean	mean	difference	mean	mean	difference
Girls: USE completion	0.16	0.30	0.13	0.35	0.40	0.05
	(0.38)	(0.46)		(0.48)	(0.49)	
Boys: USE completion	0.22	0.22	0.01	0.30	0.46	0.15**
	(0.42)	(0.41)		(0.46)	(0.50)	

SOURCE. — Mexican Family Life Survey (MxFLS 2002 and MxFLS 2005).
NOTE. — Means are of MxFLS 2002 (wave 1) and MxFLS 2005 (wave 2) and weighted by 2002 and 2005 sample weights. Results are unchanged if weights are not considered. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 7
DIFFERENCE-IN-DIFFERENCE ESTIMATES OF THE EFFECT OF *OPORTUNIDADES* ELIGIBILITY ON COMPLETING UPPER SECONDARY EDUCATION: GENDER HETEROGENEITY EFFECTS

	(1)	(2)	(3)
	DiD*Female	DiD just Female	DiD just Male
	USE comp	USE comp	USE comp
*Oportunidades**AfterCohort	-0.166	0.092	-0.166
	(0.147)	(0.123)	(0.147)
*Oportunidades**AfterCohort*Female	0.259		
	(0.192)		
*Oportunidades**Female	-0.146		
	(0.150)		
AfterCohort*Female	-0.146		
	(0.109)		
Oportunidades	0.007	-0.138	0.007
	(0.120)	(0.090)	(0.120)
AfterCohort	0.242***	0.096	0.242***
	(0.078)	(0.075)	(0.078)
Female	0.089		
	(0.085)		
Constant	0.216***	0.305***	0.216***
	(0.061)	(0.059)	(0.061)
Observations	881	482	399
R-squared	0.033	0.017	0.052

SOURCE. — Mexican Family Life Survey (MxFLS 2002 and MxFLS 2005).
NOTE- — Regressions weighted by 2002 and 2005 sample weights (wave 1 and wave 2). Results are unchanged if weights are not considered. Standard errors adjusted for heteroscedasticity in parentheses. HH=Head of the Household. Additional specifications reported in tables A2, A3 and A4. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 8
DIFFERENCE-IN-DIFFERENCE AND GENDER HETEROGENEITY ESTIMATES OF THE EFFECT OF *OPORTUNIDADES* ELIGIBILITY ON COMPLETING UPPER SECONDARY EDUCATION: AGE OF COMPLETION AND REGION FIXED-EFFECTS (JUST MALE SAMPLE)

	(2)	(3)	(4)	(5)	(6)
	USE comp	USE comp	USE comp	USE comp	USE comp
*Oportunidades**AfterCohort	-0.154	-0.189	-0.243*	-0.249	-0.256*
	(0.153)	(0.153)	(0.143)	(0.152)	(0.143)
Oportunidades	0.035	0.001	0.193*	0.210*	0.186
	(0.123)	(0.131)	(0.116)	(0.126)	(0.119)
AfterCohort	0.325***	0.235***	0.169**	0.231***	0.166**
	(0.081)	(0.082)	(0.077)	(0.086)	(0.078)
Indigenous			-0.000	-0.084	-0.009
			(0.114)	(0.121)	(0.119)
Age in 2002/2005			-0.159***	-0.092	-0.171***
			(0.045)	(0.064)	(0.045)
Number of family members			-0.024	-0.019	-0.014
			(0.020)	(0.021)	(0.021)
Number of siblings			0.006	0.006	-0.006
			(0.031)	(0.032)	(0.031)
Education HH			0.007	0.008	0.009
			(0.008)	(0.008)	(0.008)
Education spouse of HH			0.011	0.010	0.011
			(0.011)	(0.011)	(0.011)
Female (HH)			0.025	0.047	0.029
			(0.142)	(0.140)	(0.147)
Income			0.000	-0.000	0.000
			(0.000)	(0.000)	(0.000)
Employed (student)			-0.110	-0.108	-0.117
			(0.073)	(0.078)	(0.075)
Employed (HH)			0.241**	0.196**	0.219**
			(0.097)	(0.099)	(0.095)
Employed (Spouse HH)			0.030	0.025	-0.005
			(0.078)	(0.082)	(0.078)
Constant	0.154**	0.226***	2.435***	1.397	2.614***
	(0.063)	(0.063)	(0.754)	(1.029)	(0.747)
Age of USE completion F.E.	Yes			Yes	
Region F.E.		Yes			Yes
Observations	386	399	399	386	399
R-squared	0.129	0.082	0.237	0.246	0.259

SOURCE. — Mexican Family Life Survey (MxFLS 2002, MxFLS 2005 and MxFLS 2009).
NOTE. — Regressions weighted by 2002 and 2005 sample weights (wave 1 and wave 2). Results are unchanged if weights are not considered. 13 observations were dropped for the age of completion fixed-effect analysis – differences with the reported age in the first and second wave were higher than expected or negative. Standard errors adjusted for heteroscedasticity. HH=Head of the Household. *** p<0.01, ** p<0.05, * p<0.1.

Appendix

TABLE A1
DIFFERENCE-IN-DIFFERENCE ESTIMATES OF THE EFFECT OF *OPORTUNIDADES* ELIGIBILITY ON COMPLETING UPPER SECONDARY EDUCATION: AGE OF COMPLETION AND REGION FIXED-EFFECTS

	(1)	(2)	(3)	(4)	(5)	(6)
	USE comp	USE comp	USE comp	USE comp	USE comp	USE comp
*Oportunidades**AfterCohort	-0.042	-0.043	-0.055	0.079	0.071	0.049
	(0.093)	(0.096)	(0.094)	(0.092)	(0.093)	(0.092)
Oportunidades	0.060	0.084	0.050	-1.486	-1.162	-1.674
	(0.075)	(0.078)	(0.077)	(1.080)	(1.147)	(1.089)
AfterCohort	0.111**	0.161***	0.099*	0.262	0.860	0.448
	(0.051)	(0.058)	(0.052)	(1.030)	(1.135)	(1.024)
Indigenous	-0.022	-0.041	-0.047	-0.050	-0.036	-0.067
	(0.072)	(0.075)	(0.074)	(0.083)	(0.086)	(0.082)
Female	-0.012	-0.014	-0.009	-0.019	-0.023	-0.016
	(0.041)	(0.043)	(0.042)	(0.072)	(0.073)	(0.074)
Age in 2002/2005	-0.172***	-0.108**	-0.171***	-0.154***	-0.050	-0.153***
	(0.031)	(0.042)	(0.031)	(0.051)	(0.066)	(0.050)
Number of family members	0.005	0.003	0.008	-0.009	-0.023	-0.003
	(0.014)	(0.015)	(0.014)	(0.026)	(0.026)	(0.026)
Number of siblings	-0.048**	-0.037*	-0.049**	-0.034	-0.008	-0.042
	(0.021)	(0.022)	(0.021)	(0.040)	(0.038)	(0.040)
Education HH	0.010*	0.010	0.011*	0.006	0.009	0.007
	(0.006)	(0.006)	(0.006)	(0.012)	(0.012)	(0.012)
Education spouse of HH	0.007	0.012	0.010	-0.010	-0.004	-0.005
	(0.008)	(0.008)	(0.008)	(0.015)	(0.015)	(0.015)
Female (HH)	0.196**	0.241**	0.187*	0.386***	0.453***	0.374***
	(0.099)	(0.107)	(0.097)	(0.136)	(0.138)	(0.133)
Income	-0.000	0.000	0.000	-0.000**	-0.000**	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Employed (student)	-0.128***	-0.121**	-0.130**	-0.188***	-0.151**	-0.189***
	(0.049)	(0.052)	(0.051)	(0.068)	(0.067)	(0.068)
Employed (HH)	0.161**	0.162**	0.166**	-0.052	-0.033	-0.027
	(0.067)	(0.070)	(0.069)	(0.124)	(0.127)	(0.133)
Employed (Spouse HH)	0.010	-0.001	-0.005	0.098	0.102	0.093
	(0.053)	(0.054)	(0.052)	(0.087)	(0.088)	(0.088)
*Oportunidades**Indigenous				0.041	0.032	0.046
				(0.051)	(0.052)	(0.051)
AfterCohort*Indigenous				0.028	0.008	0.033
				(0.048)	(0.052)	(0.048)
*Oportunidades**Female				0.071	0.086	0.046
				(0.085)	(0.089)	(0.085)
AfterCohort*Female				-0.014	-0.014	0.002
				(0.086)	(0.087)	(0.086)
*Oportunidades**Age in 2002/2005				0.086	0.068	0.097
				(0.068)	(0.071)	(0.068)
AfterCohort* Age in 2002/2005				-0.045	-0.073	-0.054

				(0.063)	(0.071)	(0.063)
*Oportunidades**Family				-0.027	-0.023	-0.025
				(0.030)	(0.034)	(0.030)
AfterCohort*Family				0.025	0.040	0.022
				(0.029)	(0.029)	(0.028)
*Oportunidades**Siblings				0.055	0.043	0.059
				(0.038)	(0.041)	(0.038)
AfterCohort*Siblings				-0.033	-0.051	-0.027
				(0.042)	(0.042)	(0.042)
*Oportunidades**EducHH				0.015	0.011	0.011
				(0.014)	(0.014)	(0.013)
AfterSchool¨*EducHH				-0.000	-0.004	-0.000
				(0.013)	(0.014)	(0.013)
*Oportunidades**EducSHH				0.004	0.002	0.011
				(0.018)	(0.019)	(0.018)
AfterCohort*EducSHH				0.027	0.025	0.021
				(0.017)	(0.018)	(0.017)
Oportundiades*FHH				-0.245	-0.248	-0.358
				(0.233)	(0.266)	(0.250)
AfterCohort*FHH				-0.208	-0.281	-0.162
				(0.188)	(0.193)	(0.190)
*Oportunidades**Income				-0.000	0.000	-0.000
				(0.000)	(0.000)	(0.000)
AfterCohort*Income				0.000***	0.000**	0.000***
				(0.000)	(0.000)	(0.000)
*Oportunidades**Employment				0.016	0.010	0.015
				(0.074)	(0.081)	(0.073)
AfterCohort*Employment				0.064	0.017	0.067
				(0.067)	(0.066)	(0.066)
*Oportunidades**EmploymentHH				-0.077	-0.082	-0.079
				(0.159)	(0.187)	(0.164)
AfterCohort*EmploymetHH				0.285**	0.253*	0.260*
				(0.144)	(0.150)	(0.153)
*Oportunidades**EmploymentSHH				0.136	0.106	0.146
				(0.116)	(0.126)	(0.117)
AfterCohort*EmploymentSHH				-0.162	-0.173	-0.172
				(0.105)	(0.106)	(0.107)
Constant	2.804***	1.751***	2.774***	2.976***	1.238	2.916***
	(0.517)	(0.677)	(0.518)	(0.840)	(1.052)	(0.831)
Age of USE completion F.E.		Yes			Yes	
Region F.E.			Yes			Yes
Observations	881	847	881	881	847	881
R-squared	0.193	0.218	0.212	0.242	0.264	0.259

SOURCE. — Mexican Family Life Survey (MxFLS 2002, MxFLS 2005 and MxFLS 2009).
NOTE. — Regressions weighted by 2002 and 2005 sample weights (wave 1 and wave 2). Results are unchanged if weights are not considered. 34 observations were dropped for the age of completion fixed-effect analysis – differences with the reported age in the first and second wave were higher than expected or negative. Standard errors adjusted for heteroscedasticity. HH=Head of the Household. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A2
DIFFERENCE-IN-DIFFERENCE AND GENDER HETEROGENEITY ESTIMATES OF THE EFFECT OF *OPORTUNIDADES* ELIGIBILITY ON COMPLETING UPPER SECONDARY EDUCATION: AGE OF COMPLETION AND REGION FIXED-EFFECTS

	(1)	(2)	(3)
	DiD*Female	DiD*Female	DiD*Female
	USE comp	USE comp	USE comp
*Oportunidades**AfterCohort	-0.166	-0.152	-0.186
	(0.147)	(0.152)	(0.156)
*Oportunidades**AfterCohort*Female	0.259	0.244	0.273
	(0.192)	(0.202)	(0.196)
*Oportunidades**Female	-0.146	-0.134	-0.169
	(0.150)	(0.161)	(0.160)
AfterCohort*Female	-0.146	-0.115	-0.118
	(0.109)	(0.107)	(0.107)
Oportunidades	0.007	0.038	0.013
	(0.120)	(0.122)	(0.135)
AfterCohort	0.242***	0.313***	0.215***
	(0.078)	(0.080)	(0.083)
Female	0.089	0.062	0.083
	(0.085)	(0.084)	(0.084)
Constant	0.216***	0.169***	0.231***
	(0.061)	(0.063)	(0.067)
Age of USE completion F.E.		Yes	
Region F.E.			Yes
Observations	881	847	881
R-squared	0.033	0.107	0.060

SOURCE. — Mexican Family Life Survey (MxFLS 2002, MxFLS 2005 and MxFLS 2009).
NOTE. — Regressions weighted by 2002 and 2005 sample weights (wave 1 and wave 2). Results are unchanged if weights are not considered. 34 observations were dropped for the age of completion fixed-effect analysis – differences with the reported age in the first and second wave were higher than expected or negative. Standard errors adjusted for heteroscedasticity. HH=Head of the Household. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A3
DIFFERENCE-IN-DIFFERENCE AND GENDER HETEROGENEITY ESTIMATES OF THE EFFECT OF *OPORTUNIDADES* ELIGIBILITY ON COMPLETING UPPER SECONDARY EDUCATION: AGE OF COMPLETION AND REGION FIXED-EFFECTS (JUST FEMALE SAMPLE)

	(1)	(2)	(3)	(4)	(5)	(6)
	USE comp	USE comp	USE comp	USE comp	USE comp	USE comp
*Oportunidades**AfterCohort	0.092	0.100	0.105	0.094	0.131	0.147
	(0.123)	(0.133)	(0.115)	(0.116)	(0.123)	(0.116)
Oportunidades	-0.138	-0.098	-0.185**	-0.000	0.011	-0.067
	(0.090)	(0.104)	(0.084)	(0.089)	(0.096)	(0.088)
AfterCohort	0.096	0.186**	0.105	0.054	0.081	0.042
	(0.075)	(0.078)	(0.073)	(0.064)	(0.074)	(0.065)
Indigenous				-0.073	-0.036	-0.147
				(0.097)	(0.099)	(0.094)
Age in 2002/2005				-0.192***	-0.134**	-0.203***
				(0.042)	(0.054)	(0.042)
Number of family members				0.018	0.015	0.029
				(0.019)	(0.021)	(0.019)
Number of siblings				-0.075**	-0.064**	-0.091***
				(0.029)	(0.030)	(0.030)
Education HH				0.016**	0.016*	0.016**
				(0.008)	(0.008)	(0.008)
Education spouse of HH				0.004	0.009	0.005
				(0.009)	(0.010)	(0.009)
Female (HH)				0.283***	0.307**	0.282***
				(0.108)	(0.119)	(0.108)
Income				0.000	0.000	0.000
				(0.000)	(0.000)	(0.000)
Employed (student)				-0.207***	-0.215***	-0.227***
				(0.057)	(0.065)	(0.062)
Employed (HH)				0.061	0.095	0.066
				(0.091)	(0.103)	(0.091)
Employed (Spouse HH)				0.022	0.001	-0.018
				(0.069)	(0.072)	(0.069)
Constant	0.305***	0.244***	0.307***	3.275***	2.271***	3.474***
	(0.059)	(0.058)	(0.056)	(0.655)	(0.852)	(0.660)
Age of USE completion F.E.		Yes			Yes	
Region F.E.			Yes			Yes
Observations	482	461	482	482	461	482
R-squared	0.017	0.099	0.080	0.238	0.274	0.299

SOURCE. — Mexican Family Life Survey (MxFLS 2002, MxFLS 2005 and MxFLS 2009).
NOTE. — Regressions weighted by 2002 and 2005 sample weights (wave 1 and wave 2). Results are unchanged if weights are not considered. 21 observations were dropped for the age of completion fixed-effect analysis – differences with the reported age in the first and second wave were higher than expected or negative. Standard errors adjusted for heteroscedasticity. HH=Head of the Household. *** p<0.01, ** p<0.05, * p<0.1.