# Do Supply-Side-Oriented and Demand-Side-Oriented Education Programs Generate Synergies? Evidence from Rural Mexico

Paul Gertler

Harry Patrinos

Marta Rubio-Codina[†]

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***Abstract***

*Mexico's Compensatory education programs provide extra resources to primary schools that enroll students in highly disadvantaged rural communities, thus increasing the supply of education. By reducing the price of schooling through school stipends conditional on school attendance and performance, Oportunidades is increasing the demand for schooling amongst its eligible beneficiary households. This study exploits the different phasing-in over time and space across these interventions to test their degree of complementarity (or substitutability). We focus on the effects on intermediate school quality indicators (failure, repetition and dropout) of teacher training, provision of supplies, and empowerment and financing of parent associations -on the supply side; and conditional on attendance cash transfers -on the demand side. Difference-in-difference estimates prove reducing the opportunity cost of schooling and decentralizing school management at the lower level as effective measures in improving educational outcomes. No robust evidence of synergies between the two interventions is found.*

[†]Contact information: Paul Gertler, Haas School of Business, University of California at Berkeley; pgertler@worldbank.org, Harry Patrinos, The World Bank; hpatrinos@worldbank.org, Marta Rubio-Codina, University College of London; m.rubio-codina@ucl.ac.uk.

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## 1 Introduction

The focus placed on education in the Millennium Development Goals shows the existence of a large interest from policymakers and international organizations in the quantity of education (educational attainment).[1] This is particularly so in developing countries with very low school attainment rates. Nonetheless, the quality of education (learning attainment) is attracting greater attention in developing and developed countries, for several reasons. First, returns to education are high in most countries and in developing countries in particular (Psacharopoulos and Patrinos 2004; OECD 2001; Duflo 2001). Several investigations find substantial effects of higher achievements on standardized tests on earnings (Blackburn and Neumark 1993, 1995; Murnane et al 1995). Second, performance in standardized examinations is also shown to have a dramatic impact –larger than the impact of years of schooling– on productivity and national growth rates (Hanushek and Woessmann 2007; Hanushek and Kimko 2000). Third, better performing students stay longer in school and are more likely to continue to higher education (Hanushek 1996; Berhman et al 1998). As a consequence, improving quality of schooling can help equalize income levels between racial and social groups (Hanushek 2003; Hall and Patrinos 2006).

The early literature on academic performance pointed at family background and socioeconomic status as the main determinants of student academic performance. However, since Hanushek's (1986) work on education production functions, the number of studies emphasizing the differential effects of school characteristics and institutional factors in reducing learning inequalities and increasing learning outcomes has grown extensively (Hanushek and Luque 2003). Even so, the quality of schooling continues to be very low in middle- and low-income countries: many schools lack the basic equipment and supplies; teacher absenteeism runs high; and many children learn much less than the learning objectives set in the official curriculum, often inadequate and ignoring the needs of particular population groups.

Aware of such problems, the Mexican Government is currently running two key educational programs –the *Compensatory Education Programs* and *Oportunidades* scholarships– as part of a larger educational reform that began in 1992. They are both aimed at increasing quantity and quality of schooling amongst Mexico's most disadvantaged population. The Secretariat of Public Education's (*Secretaría de Educación Pública*, SEP) Compensatory Program –implemented by

[1] Two of the eight Millennium Development Goals (MDGs) adopted at the United Nations Millennium Summit in September 2000 focus on education: first, for all children to complete primary school by 2015; and second, to achieve gender equality at all levels of education by 2015.

the National Council of Education Promotion (*Consejo Nacional de Fomento Educativo*, CONAFE)– is a supply-side intervention that channels extra resources to the worst performing schools in highly disadvantaged areas. *Oportunidades* (formerly known as *Progresa*) is a demand-side intervention that provides cash transfers to poor families contingent on children attending school and family members obtaining preventive medical care. This added income may lead families to curtail their children's labor activities in favor of going to school, thus increasing enrollment and improving accumulated learning overall. However, these new students might be unprepared for school and hence unable to fully take advantage of the new educational opportunities. They may also be entering poorly performing schools. Through improving school functioning and quality, the Compensatory Program might solve –to some extent– schooling deficiencies and satisfy the needs of an increasing demand for school. Therefore, it is likely that the marginal effects of each program are larger in the schools where they coexist.

This paper uses existing data at the national level on the Compensatory Program and *Oportunidades* to test the existence of synergies between supply- and demand-side interventions in this unique situation. In other words, we address the question of whether school quality interventions enhance the effect of conditional school subsidies. We take advantage of the considerable overlap over the population both programs attend and estimate impacts on school-aggregate failure, repetition and dropout rates in rural primary schools. Both interventions were gradually phased-in which allows identification of the difference-in-difference (triple differences) estimates of the individual (joint) impacts of either (both) programs. Because the programs targeted schools and students in poor rural areas with low educational performances first, the methodological challenge relies in finding a set of comparable comparison schools. After testing their validity, we use schools incorporated in either intervention at later stages as comparisons. We estimate a school fixed effects model and additionally control for other co-existing educational programs aimed at altering either supply- or demand-related school factors. In further specifications, we disentangle the effect of the Compensatory Program into some of its different components; namely, teacher training, provision of school supplies, and monetary support to parent and teacher associations to be spent on school.

Results show effects of demand-oriented interventions on repetition and drop out. A possible explanation might rely in that the *Oportunidades* scholarship is conditional on both school enrollment *and* on not repeating a grade more than twice. Another mechanism through which *Oportunidades* may impact learning outcomes are the improved nutrition and better health

practices the program enforces. The Compensatory Program's positive impact on failure disappears once we control for the intensity of *Oportunidades*. Interestingly enough, if we break up the Compensatory Program into its different components, the effect of empowering parents' associations on failure and repetition persists even after controlling for *Oportunidades*. So does the impact of school supplies, although to a lesser extent. This gives suggestive evidence that supply-oriented interventions should be redirected towards decentralizing school management and decision-making to the local level once basic input needs have been met. Finally, we find no evidence of joint effects. We attribute the non-result to an inability of the econometric specification to capture super-additive effects rather than as evidence of no synergies between the two interventions.

The remainder of the paper is organized as follows. Section 2 reviews previous research on international education policies. Section 3 describes the Mexican educational context and provides institutional background on the Compensatory Program and the *Oportunidades* intervention. Section 4 discusses the data and develops the identification strategy used. Results and a discussion of potential biases are provided in section 5. Section 6 concludes.

## 2 International Evidence on Education Interventions in Developing Countries

Despite the positive correlation between economic growth, poverty reduction and education, little consensus exists upon which policy initiatives can most effectively enhance the quantity and quality of schooling. Hanushek (1995) summarizes the econometric evidence on education production functions from 96 studies in developing countries. The author shows that in 27 percent of the studies the pupil-teacher ratio has positive significant effects on student performance; teacher's education does so in 55 percent; teacher's experience in 35 percent; teacher's salary in 31 percent of the studies; and facilities in 65 percent. Glewwe and Kremer (2006) suggest that the mixed evidence brought up by Hanushek (1995) should not be taken as conclusive of no systematic relationship between schooling inputs and student performance, but rather as data and/or methodological limitations. The authors review the empirical evidence on the impacts of several policy interventions on both school quality and quantity from the mid-1990s in developing countries. They classify this evidence by type of intervention:

(i) policies directed to increasing the amount of educational inputs available in the classroom such as textbooks (Glewwe et al 1998), flipcharts (Glewwe et al 2004) and

other physical supplies; repairing and/or building new schools (Drèze and Kingdon 2001; Duflo 2001); or lowering student-teacher ratios (Case and Deaton 1999);

(ii) incentives to attend school, such as reductions in the cost of education (Schultz 2004) or the provision of subsidized meals (Drèze and Kingdon 2001; Vermeersch and Kremer 2004);

(iii) school-based health programs (Miguel and Kremer 2004; Bobonis et al 2006);

(iv) more fundamental institutional arrangements and reforms, such as teacher incentives (Lavy 2002; Glewwe et al 2003); school-based management programs (Jimenez and Sawada 1999); and vouchers and school-choice programs (Angrist et al 2002).

The studies reviewed provide inconclusive evidence on the extent to which student participation in school and academic achievement respond to school quality. Glewwe et al (1998) find no effects of randomly providing textbooks on school participation and only limited effects on test scores; similarly, Glewwe et al (2004) find no effects of the random provision of flip-charts with instructional material on learning. On the other hand, a retrospective study in Ghana by Glewwe et al (1994) reports increases in reading and math test scores of around 2 standard deviations after repairing leaking classrooms. The introduction of blackboards had effects of similar magnitudes, while adding libraries had smaller effects. Drèze and Kingdon (2001) and Schultz (2004) suggest that school participation is fairly responsive to reductions in the cost of schooling in India and Mexico, respectively. School health programs also appear as a cost-effective way of increasing the quantity of schooling.

The limited evidence existing on performance-based teacher incentives is compatible with the belief that these induce teachers to teach more to the test (Lavy 2002; Glewwe et al 2003). Finally, evidence from Central America suggests correlations between local decentralization reforms and improved school access in poor rural areas (Di Gropello 2006); and reduced student absenteeism, drop out, failure and repetition (Jimenez and Sawada 1999, 2003; Skoufias and Shapiro 2006; Murnane et al 2006; Gertler et al 2006). Additional studies find weak positive impacts on student achievement (Di Gropello and Marshall 2005; Parker 2005; Lopez-Calva and Espinosa 2006).

## 3 The Mexican Context and the Interventions

Mexico's education indicators are relatively poor. The average educational attainment of the Mexican population aged 15 and over is a disappointing 7.2 years, as compared with 7.6 in

Chile, Uruguay and Peru; 8.8 in Argentina; and 10 to 12 years for other, more advanced OECD countries. Despite the high enrollment rates for primary education, net enrollment in secondary education is only 58 percent (75 percent in Chile and 79 percent in Argentina).

Not surprisingly, Mexico has undertaken a large educational reform that began in 1992 with the decentralization of educational services from the federal to the state level, the "National Agreement for the Modernization of Basic Education." The numerous initiatives implemented at the central and state levels included: (i) a curricular reform; (ii) the provision of teaching and learning materials at the primary school level (free textbooks, textbooks in native languages for indigenous students); (iii) the introduction of communication technology in both primary and secondary schools (satellite systems and computers); (iv) the establishment in 1993 of *Carrera Magisterial*, a voluntary pay per performance scheme targeted to all educators; (v) legally supported advancement of parental participation in schools; and (vi) the development of innovative demand- and supply-side interventions to promote education. Amongst others, these initiatives included: the *Oportunidades* program started in 1997; and the creation of the Quality Schools Program *(Programa Escuelas de Calidad*, PEC) in 2001, a school-based management initiative. In 2004, 44 percent of social development expenditures were devoted to education. This represented a 7.1 percent of GNP compared to a 6.2 percent in 2000.

As a result of these efforts, Mexico has made substantial progress in expanding access to primary and secondary education, especially in rural areas and for the poor. Between 1994 and 2002, net enrollment in lower secondary school increased from 25 to 48 percent in rural areas, and completion rates rose from 55 to 67 percent. Primary education completion rates were practically universal and 95 percent of primary school graduates continued to lower secondary school in 2003. However, investments in secondary education continue to be low and more worryingly, primary education is not imparting functional literacy to its graduates (Schmelkes 1997). The consequences of low quality schooling include low learning achievement, failure and repetition. The unsatisfactory performance of Mexican students in international achievement tests such as PISA 2000 and 2003 confirm this (World Bank 2005).

A body of research has evaluated some of the interventions the Mexican government undertook as part of the educational reform. McEwan and Santibañez (2004) exploit state level variation in the probability of principal promotion in the *Carrera Magisterial* scheme. They find no robust evidence that stronger incentives lead to higher test scores. Several other studies –

which we will review in the next sections– have also evaluated the effects of *Oportunidades* and the Compensatory Program from different perspectives. To our knowledge no study has so far studied them jointly.

### 3.1 The Compensatory Education Program

In 1991 CONAFE started to implement the Compensatory Program on behalf of SEP. The intervention channels extra monetary and in-kind resources to state governments to improve the supply and quality of education in schools with the lowest educational performance levels in highly disadvantaged communities. It currently serves about five million students in initial, preschool and primary education, and about 300,000 students in *telesecundaria* education (lower secondary school imparted by satellite and television. These represent 31 percent of all primary school students. Compensatory education costs just over $50 per student per year on average, an extremely low cost compared to a typical cost of $527 per *telesecundaria* student and $477 per general lower secondary school student (Shapiro and Trevino 2004).[2]

#### 3.1.1 Targeting and Phasing In

Since its start, the Compensatory Program has substantially expanded its coverage both to new geographical areas and to new school levels. From 1991 to 1996, the program operated exclusively in all indigenous and general primary schools in rural localities in the four states with the highest incidence of poverty: Oaxaca, Guerrero, Chiapas and Hidalgo. In 1993, the program included all general and indigenous primary schools in the poorest and educationally worst performing municipalities in the next ten poorest states. A project to support initial education was also initiated.

In 1995, the program extended coverage to all indigenous primary schools and to general primary schools with first year repetition rates above the state average, in the next nine poorest states. In 1998, the eight remaining Mexican states were incorporated. Worse performing schools were selected according to a targeting index constructed by CONAFE on the basis of: (i) Mexico's community disadvantage index; (ii) teacher-student ratios; (iii) the number of students per school; and (iv) educational outcomes. All general primary schools falling in the third and fourth quartiles of the targeting index were selected as beneficiary schools.[3] As in previous stages, all indigenous primary schools were automatically enrolled. The program also

[2] Costs are expressed in 2002 US dollars, using an exchange rate of 9.74MXP = $1 US dollar.
[3] See Section 4.2 for further details.

incorporated pre-schools, lower secondary schools and *telesecundarias.* Finally, it extended coverage to disadvantaged semi-urban and urban areas.

### 3.1.2 Intervention Components

The supports given have varied across school types and along the different program phases. Moreover, the state government has discretion on which resources to allocate on the basis of school needs and resources availability. This generates substantial variation in the type, number and timing of the supports attended schools receive.

In 1996, the number of interventions was reduced to the following: (i) improvement of existing and/or building of new school infrastructure and facilities (classrooms, labs, latrines, etc.); (ii) provision of updated audiovisual technology (computers, TVs, etc.) and equipment (desks, bookcases, etc.); (iii) provision of didactic materials for each student (notebooks, pens, etc.); (iv) administrative and pedagogical training to all educational staff; (v) performance based monetary incentives to teachers (monitored by parents) and principals; (vi) monetary support to school supervisors and improvement of monitoring methods; (vii) institutional strengthening, updating of the informational systems and evaluation planning; and (viii) support to school management management (*Apoyo a la Gestión Escolar,* AGEs, or School Management Support) in the form of grants to parent associations to be spent on their choice of civil works and infrastructure improvements; and training to guide them on their spending (*Capacitación para el Apoyo a la Gestión Escolar*, CAPAGEs). In indigenous schools, the Compensatory Program additionally supports the development of curricula and intercultural education; and textbooks for bilingual education. For *telesecundaria* education, the intervention is supposed to provide audiovisual materials and infrastructure improvements to all schools. In practice, benefits have been limited to one or two computers per intervened *telesecundaria* school.

### 3.1.3 Existing Evidence on the Compensatory Program's Impact

Results from Government supported evaluations suggested correlations between the program and reduced school average repetition rates in rural primary schools (Benemérita Universidad Autónoma de Puebla 2004). López-Acevedo (2002) found larger increases in Spanish test scores for indigenous students in compensatory schools than in "comparable" schools in the state of Michoacán, where no schools had yet received benefits. A complementary evaluation by Paqueo and López-Acevedo (2003) compared the differential effects of the intervention on sixth graders' Spanish test scores between the poorest and the least poor children

in indigenous rural schools. The authors found that the poorest students benefited less from the intervention than the less poor students. These findings raise the question of whether the very poor are able to take advantage of school quality improvements or whether their ability is compromised by malnutrition and lack of brain stimulation at early life stages, amongst other reasons.

More recently, Shapiro and Moreno (2004) used propensity score matching to look at impacts on student Spanish and math test scores in primary and lower secondary schools. The authors found the Compensatory Program effective in improving primary school math learning and lower secondary school Spanish learning. The program also seemed to lower primary school repetition and failure rates. Lopez-Calva and Espinosa (2006) used matching techniques on cross-sectional data and found that the AGEs support had had a positive impact on test scores. Another evaluation of AGEs by Gertler et al (2006) further showed effects in reducing failure and repetition rates. The authors exploited pre-program data and the phased-in introduction of the program to construct a difference-in-difference estimator, controlling for school fixed effects.

### 3.2 Oportunidades

The Mexican Government initiated *Oportunidades* (originally called *Progresa*) in 1997. The program was designed to alleviate the immediate needs of poverty and break its intergenerational transmission by inducing parents to invest in the human capital of their children. Cash transfers are given to the female head every two months in two forms. The first is a transfer conditional on family members obtaining preventive medical care and is intended for families to spend on more and better nutrition. The second type comes in the form of educational scholarships and is given to each child less than 18 enrolled between the third grade of primary and the third grade of lower secondary school, conditional on the child attending a minimum number of school days and not repeating a grade more than twice. The educational stipend increases with the grade of the child and is higher for girls than boys during lower and upper secondary school. Beneficiary children also receive money for school supplies once a year. On average, the program pays students at the primary school level between $100 and $200 depending on their grade and gender.

While the program was first introduced in rural areas and specifically granted cash transfers to primary and lower secondary school students, it expanded to urban areas and covered upper secondary school students starting in 2001. For the reasons mentioned below, this study will

exclusively focus on rural primary schools. By 2004, *Oportunidades* distributed approximately $3 billion to some 5 million beneficiary households in both rural and urban areas.

### 3.2.1 Targeting and Program Phasing In

When *Oportunidades* was first rolled out, program eligibility was determined in two stages (Skoufias et al 2001). First, the program identified underserved communities using a specially constructed "marginality index" based on census data. Then, *Oportunidades* identified low-income households within those communities by means of a proxy means test constructed using data on household characteristics collected by the program. The original classification scheme designated 52 percent of households in eligible communities as eligible for treatment. All eligible households were offered *Oportunidades* and 90 percent enrolled. Once enrolled, households received benefits for a three-year period conditional on meeting the health care and schooling requirements. New households were not able to enroll until the next certification period in 2000. This prevented household migration into *Oportunidades* communities for benefits. Only 1 percent of households were denied benefits for noncompliance.

For logistical and financial reasons, the program could not cover all eligible households at once. Rather than purposely depriving households of program benefits, *Oportunidades* was phased-in over time starting with 6,344 rural localities (300,705 families) in 1997. In 1998, the program was greatly expanded reaching 40,711 rural localities (1,930,032 families) in all but one state. Beneficiary families in urban areas were incorporated starting in 2001. By 2002, 59 percent of the *Oportunidades* beneficiary students were enrolled in primary school, 31 percent in lower secondary school and only 10 percent in upper secondary. Moreover, 73 percent of beneficiary families were in rural areas compared to 14 percent in semi-urban and 13 percent in urban areas. Therefore, a majority of *Oportunidades* beneficiary students were enrolled in rural schools.

For the purpose of rigorous evaluation the Mexican Government randomized treatment across eligible households in 506 eligible localities. These households comprise the *Oportunidades* evaluation sample. Households in the control group (40 percent) only started receiving benefits a year and a half after households in the treatment group (60 percent). Most research on the program has used the baseline and follow-up data periodically collected on this sample. Because we could only identify a very small number of (non-) *Oportunidades* and (non-) Compensatory schools in the *Oportunidades* evaluation sample, we will use data on Compensatory and *Oportunidades* coverage at the *national level*.

### 3.2.2 Existing Evidence on *Oportunidades'* Impact on Education

Most of the existing impact evaluations on school enrollment and performance use panel data at the student level coming from the randomized sample. These studies consistently find significant increases in lower secondary school enrollment, especially for girls (Parker and Skoufias 2001; Schultz 2004). At the primary school level –where enrollment rates were 93 percent on average before the intervention– *Oportunidades* only increases enrollment marginally. Dubois et al (2004) report positive impacts on successful grade completion and reduced grade repetition for primary school students. Effects are however negative for lower secondary school students. The authors attribute this finding to the disincentive effect on learning effort introduced by the termination of the educational stipend at the end of lower secondary school.[4] Similarly, Behrman et al (2005) apply a Markov schooling transition model and find that the program reduces dropout and facilitates progression through grades, particularly during the transition from primary to lower secondary school. Simulation estimates show that if children were to participate in the program between the ages of 6 to 14, they would have an average of 0.6 extra years of schooling. There would also be a 19 percent increase in the proportion of children attending lower secondary school. Coady and Parker (2004) perform a cost-effectiveness comparison between building lower secondary schools and providing *Oportunidades* scholarships to lower secondary school students. They find that subsidies to demand are more cost-effective –the cost incurred in generating one extra year of schooling is lower– than increasing access through building schools. However, no study has found any gains on test scores so far, neither in the short (Berhman et al 2000) nor in the long run (Parker et al 2005). This lack of results on learning achievement brings forward the need to address quality issues while expanding access through scholarships.

To our knowledge, there is a single study that examines the effects of *Oportunidades* on education at the national level.[5] Parker (2003) uses *Oportunidades* coverage data and school census data on educational outcomes and school characteristics to obtain double difference estimates of the impact of *Oportunidades* on total school enrollment. In line with previous research, the author finds significant increases in enrollment to lower and upper secondary school but no effect at the primary school level. The study also shows preliminary evidence of positive effects (reductions) on dropout and failure rates for primary school girls.

[4] Note that since 2001, *Oportunidades* pays scholarships to eligible students all through upper secondary.
[5] Barham (2005) uses data on the *Oportunidades* national coverage to study the impact of the program on child mortality at the municipality level. The methodology applied therein inspired this work.

## 4 Estimation and Identification

Our objective is to estimate the impacts of increased school quality and capacity (Compensatory Program) and student targeted school subsidies (*Oportunidades*' scholarships) on intermediate indicators of student performance and school quality, namely failure, repetition and intra-year dropout.[6] We specifically focus on the joint impact of the Compensatory Program and *Oportunidades* between 1998 and 2001 in rural primary schools.

### 4.1 Econometric Specification

Let us assume that the probability that student *i* in school *s* at time *t* attains educational outcome $Y_{ist} = Y$ is a function of: (i) the presence of the Compensatory support in the school in the previous year, $C_{s,t-1}=\{0,1\}$; and (ii) whether she had benefited from the *Oportunidades* scholarship, $OP_{is,t-1} = \{0,1\}$; given her vector of *j* individual characteristics, $I_{isjt}$, such as family background, ability and skills; and the *k*-th vector of school characteristics, $X_{skt}$, that includes school quality. More formally,

$$pr(Y_{ist} = Y) = f(C_{s,t-1}, OP_{is,t-1}; I_{isjt}, X_{skt}) \quad (1)$$

We consider the following outcomes: the probability that the student fails an exam, repeats a grade or drops out of school. Since we do not have individual student performance, we are not able to estimate (1) directly. However, assuming that *f(.)* is a linear function, we can obtain the average rate of success/failure at the school level by adding up the student individual probabilities by school and normalizing them by the number of students in each school, $N_{st}$. Then, equation (1) becomes:

$$pr(\bar{Y}_{st}) = f(C_{s,t-1}, OPRatio_{s,t-1}; \bar{I}_{sjt}, X_{skt}) \quad (2)$$

where $\bar{Y}_{st} = \frac{1}{N_{st}} \sum_{i=1}^{N} Y_{ist}$ represents the school *s* average failure, repetition or drop out rate at time *t*; and $\bar{I}_{sjt} = \frac{1}{N_{st}} \sum_{i=1}^{N} I_{isjt}$ is the vector of the *j* school-averaged student characteristics. Let $B_{st} \leq N_{st}$ be the total number of *Oportunidades*' beneficiaries in the school at *t*. Then, $OPRatio_{s,t-1} = \frac{1}{N_{st}} \sum_{i=1}^{B} OP_{is,t-1}$ is the ratio of *Oportunidades*' beneficiaries to total students in the school, a measure of the

[6] Ideally, we would like to use test score data as a more direct measure of student performance. Unfortunately, because standardized assessments were collected for a representative sample of *all* Mexican schools –from all geographical and social strata– we had too little power to identify effects on test scores.

intensity of the *Oportunidades* treatment in the school. From (2), we estimate the following reduced form for all *t =1997-2001*:[7]

$$Y_{st} = \alpha_s + \eta_t + \xi_{lt} + \sum_t \pi_{1t} trend * CT_s + \sum_t \pi_{2t} trend * OPT_s + \sum_t \pi_{3t} trend * CT_s * OPT_s + \\ + \beta_1 C_{s,t-1} + \beta_2 OPRatio_{s,t-1} + \beta_3 C_{s,t-1} * OPRatio_{s,t-1} + \sum_{k=1}^{K} \phi_k X_{skt} + \bar{\varepsilon}_{st} \quad (3)$$

where $\alpha_s$ and $\eta_t$ are school and time fixed effects. $\xi_{lt}$ are state specific time dummies introduced to capture state specific aggregate time effects correlated with schooling outcomes (demographic trends or changes in government, for example). $CT_s$ and $OPT_s$ are dichotomous variables equal to 1 if the school *s* is a potential treatment school; this is to say, if *s* will receive the Compensatory support ($CT_s$ =1) or *Oportunidades* beneficiary students ($OPT_s$ =1) during some (or all) of the treatment years (*t* =1998-2001). Thus, the terms *trend*CT*$_s$, *trend*OPT*$_s$ and *trend*CT*$_s$**OPT*$_s$ are specific time trends for potential Compensatory-treatment only schools, potential *Oportunidades*-treatment only schools, and schools that will eventually receive both interventions. These terms are introduced to control for the different evolutions treatment and comparison schools might have experienced over time. $X_{skt}$ is the vector of time varying school characteristics. It includes the school student-to-teacher ratio, the average number of students per class and the proportion of teachers under *Carrera Magisterial*.[8] $\bar{\varepsilon}_{st} = \frac{1}{N_{st}} \sum_{i=1}^{N} \varepsilon_{ist}$ is the school averaged individual error terms that includes all the unobserved individual characteristics (learning ability, disutility from studying, etc.) that we assume uncorrelated with the explanatory variables for the time being.[9] We compute robust standard errors clustered at the school level to correct for heteroskedasticity and serial correlation. Because of the inclusion of school fixed effects, all time-invariant school observed and unobserved characteristics that could be correlated with both school outcomes and program placement are controlled for.

Depending on the specification, $C_{s,t-1}$ will either be a dummy equal to one if the school receives Compensatory supports, or a continuous variable reflecting the number of periods the school has received the supports continuously. Similarly, in certain specifications the ratio of *Oportunidades* beneficiary students in the school, $OPRatio_{s,t-1,}$ will be replaced by a dummy equal

[7] We take school year *t*=1997-98 as the baseline year. Evaluation years are from 1998-99 to 2001-02.

[8] We have replaced missing values for school regressors with the time specific municipality average (or the state average in its default). We have included indicator variables to account for the replacement.

[9] The interventions might alter the number of children enrolling in school and hence bias estimates if the distribution of student's skills is also altered. We will explore the existence of this bias in section 5.3.2. Because of the lack student data, we also include the characteristics of the average student in the school $\bar{I}_{sjt}$ in the error term.

to one if more than 25 percent of the students in the school are *Oportunidades* beneficiaries. $\hat{\beta}_1$ and $\hat{\beta}_2$ are the difference-in-difference estimates of the one period lagged effects of the presence of the Compensatory Program and the intensity of *Oportunidades* in the school. More specifically, they measure changes in school-averaged student performance trends between earlier intervened schools (treatment schools) and later intervened schools (comparison schools). The coefficient on the interaction, $\hat{\beta}_3$, captures the existence of super-additive effects resulting from both interventions. Notice that the specification assumes that both interventions require at least a full school year to be effective. Thus, we take educational outcomes at the end of the school year (at *t*) and run them as a function of the presence in the school of either one or both interventions for the entire school year; this is to say, starting at *t-1*.

As noted, the Compensatory intervention is composed of several interventions, which may have different impacts –if any– on educational outcomes. To the extent that there is heterogeneity on the impact of each intervention and variation in the number of schools that receive each support over time, treating the program as a package might be misleading. Therefore, in further specifications we will decompose the Compensatory treatment variable in (3), $C_{s,t-1}$, into the three interventions for which we have enough data points over time: monetary support to parents for school management (AGEs), provision of school and student supplies, and teacher training. In these cases we will control for the reception of other (sporadic) interventions, namely the provision of infrastructure, equipment and and performance-based incentives to teachers.

### 4.2 Data Sources and Sample Sizes

To identify beneficiary schools we use administrative data on the Compensatory Program coverage from 1991 to 2003 and on *Oportunidades* coverage from 1997 to 2003. We use data from the Mexican School Census (*Censo Escolar* 911) to measure failure, repetition and drop out. We also take advantage of Mexico's 1990 and 2000 Population Census and the 1995 *Conteo* to construct socioeconomic locality indicators that will help identify the evaluation sub-sample. All data sources are combined using unique school and locality identifier codes.[10]

[10] For a non-negligible number of localities, locality and municipality codes as registered in the Population Census have changed over time. This prevents following these localities through time. To construct locality level indicators, we take the 2000 Census as the reference year and keep only those localities whose identifying codes have remained the same.

We define the set of Compensatory and/or *Oportunidades* treatment schools as the set of schools that started receiving either (or both) intervention(s) between 1998 and 2001, and that received it continuously ever since.[11] The comparison group consists of those schools that started receiving the Compensatory support and had more than 25 percent of its students receiving *Oportunidades* beneficits in 2002 or later. Ideally, comparison schools would only differ from the group of treatment schools in their treatment status. However, given the Compensatory Program phasing-in criteria –indigenous schools and schools in poorer and higher marginalized areas were targeted first– this is unlikely to be the case; and less so if indigenous areas are systematically different from non-indigenous areas (Ramirez 2006). In order to achieve comparable samples, we restrict our study to the balanced panel of 4,132 rural non-indigenous primary schools that we observe continuously between 1995 and 2003, and that fall in quartiles three and four of the distribution of the targeting index CONAFE computed in 2000.[12] Out of these, 36 percent are Compensatory treatment schools, 66 percent are *Oportunidades* treatment, 28 percent receive both interventions, and 26 percent are pure comparison schools (see Table 1). Note however that only 8 percent of the schools receive the Compensatory Program and have less than 25 percent of their students receiving *Oportunidades* benefits. The small number of schools in this subgroup is likely to cause power issues in the empirical exercise.

The 2000 targeting index was constructed as a tool to target worse performing schools in less disadvantaged states. It used 2000 Census data on localities and School Census data for the school year 1999-00 on school characteristics and educational outcomes.[13] The targeting rule implied that (i) all rural schools in highly disadvantaged areas; and (ii) all schools falling in the third and fourth quartiles of the targeting index distribution in less disadvantaged areas would be selected as beneficiaries starting in 2001. As in previous stages of the program, all indigenous primary schools were automatically enrolled. We exploit the index as a way of testing for balance between the treatment and comparison groups we have constructed: schools with similar targeting indices are likely to have similar values of the variables that compose it. Hence, they are likely to be in similar environments and have a similar educational performance. Moreover, because

[11] Note that while the Compensatory support is given to the school, *Oportunidades*' scholarships are given to each individual student. Since we perform the analysis at the school level, we define an *Oportunidades* (treatment) school as a school where more than 25 percent of its students receive *Oportunidades* benefits.
[12] To allow comparison across outcomes, we restrict the analysis sample to those schools with non-missing observations for any of the dependent variables under study. Results are robust to the inclusion/exclusion of schools with missing outcome information. We have also drop schools with extremely high numbers of students or teachers (top 0.5 percent of each distribution).
[13] See CONAFE (2000) for more details on the construction of the targeting index.

comparison schools that fall in these two top quartiles comply with the targeting criteria and hence should receive Compensatory benefits at a later date, they are more likely to constitute a better comparison group. Figure 1 shows that the index distributions for treatment and comparison schools overlap over the entire support.[14]

Table 1 presents descriptive statistics for a few school observables and outcomes at baseline. Schools in the sample have, on average, 155 students, 7 classes and between 5 and 6 teachers. *Oportunidades* and Compensatory treatment schools have the lowest number of students and teachers on average (91 students and 3 or 4 teachers); whereas the schools that serve as pure comparison schools are clearly larger with about 255 students and 8 teachers on average. Comparison schools also present lower failure and grade repetition than the average school in the sample albeit but drop out rates. This might reflect a larger mobility in larger towns.

### 4.3 Sources of Variation and Balance in Pre-Intervention Trends

We rely on the phasing-in of schools into either intervention over space and time to generate sufficient variation in the treatment variables to achieve identification. Figure 2 plots the proportion of schools in each treatment group (no treatment, Compensatory treatment only, *Oportunidades* treatment only, or both treatments) by school year. Logically, as *Oportunidades* starts in 1998, the bulk of "both treatment" schools increases. An additional source of variation – which is not graphically depicted– comes from the increase in the intensity of treatment within a school; that is, with the number of *Oportunidades* beneficiary students in the school, which we assume increases as new localities are incorporated into the program.[15] Variation in the timing of first receiving the different Compensatory supports (AGEs, school supplies and teacher training) also allows independent identification of each intervention (see Figure 3).

As aforementioned, we define the set of comparison schools as the set of schools that start receiving Compensatory supports and/or have more than 25 percent of their students being *Oportunidades* beneficiaries from school year 2002-03 onwards. However, the existence of a

[14]At first, it might seem surprising the fact that the distribution of treatment schools (targeted earlier given their larger index values or lower efficiency levels) is more to the left than the distribution of comparison schools. Recall that the index was computed when most treatment schools had already been under treatment for a year or two, and therefore had had time to improve their educational outcomes with respect to comparison schools.

[15] The ratio of *Oportunidades* beneficiaries to total students could increase or decrease over time depending on the relative frequencies of potential beneficiaries to children enrolling in third grade for the first time ("new coming" beneficiaries) versus children graduating from primary ("exiting" beneficiaries). A school level fixed-effects regression assesses it increases over time.

comparison group does not imply its validity. Given the non-experimental nature of the data, it might be the case that schools with the strongest (weakest) potential for improvement have been incorporated at earlier stages. Then, our estimates would overestimate (underestimate) the true program effects. Unbiased identification of the difference-in-difference estimates in this setting heavily hinges on the fact that post-intervention trends between intervened and non-intervened schools would have been identical in the absence of the intervention:

$$E\left[Y_{1t} - Y_{1,t-1} \mid T = 0\right] = E\left[Y_{0t} - Y_{0,t-1} \mid T = 0\right] \quad (4)$$

Assumption (4) is impossible to test as the counterfactual is never observed. We can nonetheless test whether outcome pre-intervention trends were similar between the proposed treatment and comparison groups. If pre-intervention trends (at $t'<t$) were not significantly different between treatment and comparison schools, there is no reason to believe they would be significantly different in the post-intervention periods ($t$) *were* the interventions not in place. Thus the identifying assumption rewrites:

$$E\left[Y_{1t'} - Y_{1,t'-1} \mid T = 0\right] = E\left[Y_{0t'} - Y_{0,t'-1} \mid T = 0\right] \quad (4')$$

We test the validity of (4') in our data by running the following equation for all pre-intervention years:

$$Y_{st'} = \alpha_s + \xi_{lt} + \sum_{t'} \gamma_{t'} YR_{t'} + \sum_{t'} \delta_{1t'} CT_s * YR_{t'} + \sum_{t'} \delta_{2t'} OPT_s * YR_{t'} + \sum_{t'} \delta_{3t'} CT_s * OPT_s * YR_{t'} + u_{st'} \quad (5)$$

where $CT_s$ and $OPT_s$ are –as defined above– two dichotomous variables equal to 1 if the school $s$ is a potential treatment school. $YR_{t'}$ are year dummy variables for all pre-intervention school years $t'$ =1995-1997. As in equation (3), $\alpha_s$ are school fixed effects and $\xi_{lt}$ are state specific time dummies. Now, $u_{st'}$ denotes the heteroskedastic disturbance that allows for correlations across schools over time. If the $\delta_{t'}$'s are not significantly different from zero, then the pre-intervention trends for schools that will eventually become Compensatory treatment ($\delta_{1t'}$), *Oportunidades* treatment ($\delta_{2t'}$) and both ($\delta_{3t'}$) are not significantly different from those of schools in the comparison group at each time $t'$.

Table 2A reports results for school-averaged failure, repetition and intra-year drop out pre-intervention. Regressions labeled as "Model 2" disentangle the Compensatory treatment dummy ($CT_s$) into the three different Compensatory supports under study (AGEs, provision of supplies and teacher training) and thus assess the equality in pre-intervention trends across heterogeneous schools. Both sets of results present no significance differences and thus indicate

that educational outcomes in comparison schools and in schools receiving different treatments at different stages have followed rather similar patterns during the pre-intervention years.

To further justify the validity of the comparison group, we test the equality in pre-intervention trends between schools that entered the program in different years. In this case, the equation to estimate for all *t'* =1995-1997 is:

$$Y_{st'} = \alpha_s + \xi_{lt} + \sum_{t'} \gamma_{t'} YR_{t'} + \sum_{t'} \sum_{j} \lambda_{1t'} CIN_{sj} * YR_{t'} + \sum_{t'} \sum_{j} \lambda_{2t'} OPIN_{sj} * YR_{t'} + \\ + \sum_{t'} \sum_{j} \lambda_{3t'} CIN_{sj} * OPIN_{sj} * YR_{t'} + \upsilon_{st'} \quad (6)$$

where $CIN_{sj}$ and $OPIN_{sj}$ are two sets of dummies that take on the value 1 if the school *s* was phased into Compensatory education or *Oportunidades*, on year *j*=1998-2001. The coefficients on the interaction with the year dummies, the $\lambda_{.t'}$'s, capture differences in pre-intervention trends for schools entering either one or both programs in different years. Now, results show a few significant differences –many at the ten percent significance level– in failure, grade repetition and intra-year drop out pre-intervention trends between schools that started receiving each (or both) treatment(s) at different dates (see Table 2B). Many of these differences are for the very few schools that will potentially receive school supplies or teacher training in the early treatment years, and thus may be driven by a few outlier schools. Moreover, schools that will receive the AGEs support and will have more than 25 percent of its students receiving *Oportunidades* benefits starting in school year 2000-01 present significantly larger reductions in failure (column 2) and repetition rates (column 4) than comparison schools during the school year 1997-98. Contrarily, for schools that will receive treatments a year earlier (school year 1999-00) the coefficient on the interaction is positive and significant. These small differences suggest some –albeit little– scope for endogenous program placement bias –amongst others biases– to arise.

To address as many of these concerns as possible, identification also relies in the inclusion of school fixed effects in the estimation. These control for any differences in time-invariant characteristics across schools. In addition, state-time dummies are included to capture state-specific aggregate time effects that might be correlated with schooling outcomes: changes in demographic trends in the state that might affect enrollment; or changes in the state government characteristics, for example, shifts in tastes and priorities about education that might alter the allocation of resources (endogenous program placement). Treatment-specific time trends are included to capture the different evolutions that the various treatment and comparison schools might have experienced over time. In the same spirit, we also use as many school varying

characteristics we are able to construct as controls. Although there are not many, it seems plausible to assume that schools do not change substantially in the span of five years. Given these, the estimated treatment effects will be unbiased as long as there are no unobserved time-varying characteristics or trends correlated with the treatment variables. We further discuss and address potential biases in section 5.3.

## 5 Results on Failure, Repetition and Drop Out Rates

### 5.1 Graphical Evidence

Because of the multiple interventions that can be in place in one same school –although not necessarily at the same time– and the variation in the intensity of treatment over time, it is difficult to depict by intervention and joint treatment effects graphically. Even so, graphs can provide suggestive evidence. Figures 4, 5 and 6 present the evolution of failure, grade repetition and intra-year dropout rates over time. For each figure, the left hand side graph plots the mean of the dependent variable at the end of every school year for Compensatory treatment schools and Compensatory comparison schools. Similarly, the right hand side graph plots mean outcome values for *Oportunidades* treatment and comparison schools. The vertical line at 1997 marks the beginning of the intervention period.[16]

As shown in the descriptive statistics (Table 1), intra-year dropout rates are lower amongst treatment schools for almost all school years (Figure 6). Failure (Figure 4) and grade repetition rates (Figure 5), however, are larger for treated schools during the pre-intervention years. Nonetheless, during school year 2000-01 the higher average failure and repetition rates observed in treatment schools drop down to the levels of comparison schools, at around 10 percent. By the end of the year, average failure and repetition is already lower in treatment schools than in comparison schools. Although these differences in levels are unlikely significant, they show some trend towards minimizing the gap between compensated and non-compensated schools in terms of intermediate quality of school indicators.[17] In fact, this tendency towards convergence in failure and repetition rates is common in all graphs, either because of the larger drop in failure and repetition Compensatory treated schools experience, or because of the more moderate increase in failure and repetition in *Oportunidades* treated schools. Failure and grade repetition

[16] The intervention period starts in school year 1998-99. However, by construction, outcomes are measured at the end of the school year. For instance, average failure plotted at $t$ =1998 corresponds to average failure at the end of the school year 1998-99, once the program has already been in place for an entire school year had it started at its beginning. Hence, the vertical line separating pre- and post-intervention trends is drawn at $t\text{-}1$ =1997.

[17] Confidence intervals are not depicted for reading ease.

pre-intervention trends are more or less parallel which gives some extra –graphical and imprecise- evidence for the validity of our identification strategy.[18]

### 5.2 Individual and Joint Average Treatment Effects

Tables 3, 4 and 5 present estimates of the treatment effects in equation (3) on failure, grade repetition and intra-year dropout rates. In Panel A, the Compensatory intervention is treated as a package. The set of regressions in Panel B disentangle the Compensatory treatment dummy ($CT_s$) into the three Compensatory supports under study: AGEs, supplies provision and teacher training. In further specifications, we characterize the Compensatory intervention as a continuous variable reflecting the number of periods the school has received the supports continuously. Similarly, in other specifications the ratio of *Oportunidades* beneficiary students in the school, $OPRatio_{s,t-1,}$ is replaced by a dummy equal to one if more than 25 percent of the students in the school are *Oportunidades* beneficiaries. In all cases, we start by estimating the effect of each intervention alone. As noted earlier, we take the school year 1997-98 as the baseline year. All regressions include school and time fixed effects, state specific time trends, a treatment specific trend and all the time varying school characteristics listed above. Regressions in Panel B include additional controls for any other Compensatory supports the school receives.

Results show that a larger proportion of *Oportunidades* beneficiary students in the school significantly reduces failure, repetition and intra-year dropout, regardless of whether the school also receives the Compensatory Program. Given the mean levels for each dependent variable, the effect of having more than 25 percent of the students in the school receiving *Oportunidades* benefits implies a 3 percent almost significant reduction in average failure only significant at the 10 percent (Models 6 and 8 in Table 3); a 5.6 percent reduction in average repetition (Models 6 and 8 in Table 4); and a 25 to 12.5 percent reduction in the average intra-year drop out (Model 6 and 8 in Table 5). A possible explanation might rely in that the *Oportunidades* scholarship is conditional on both school enrollment *and* on not repeating a grade more than twice. This is compatible with the larger reductions in drop out observed.

[18] Note that the evidence depicted in these graphs is only partial. Treated schools are not necessarily Compensatory-treated-only or *Oportunidades*-treated-only. Although many receive both treatments simultaneously, the graphs here are "dissecting" each treatment independently, thus not accounting for potential synergies. These graphs do not take into consideration the differences in timing of phase-in across schools either.

Contrarily, the Compensatory package does not seem to have much of an effect on outcomes. Estimates show a significant reduction in average failure for schools that have been under the Compensatory scheme for longer (Table 3, Models 2 in Panel A). Not surprisingly, this effect is less precisely estimated for schools receiving the Compensatory support only (Table 3, Models 7 and 8 in Panel A). However, results by type of Compensatory support received follow a different pattern. Panels B of Tables 3, 4 and 5 show that, were it significant alone, the AGEs effect is likely to persist even after the introduction of *Oportunidades*. Indeed the AGEs support significantly reduces average failure and average repetition by around 13 to 14 percent in schools only receiving Compensatory supports (Model 4 in Panel B –Tables 3 and 4, respectively). Reductions in failure are also observed for those schools that receive AGEs benefits for longer (Table 3, Models 7 and 8 in Panel B). The number of periods the school receives school supplies also implies reductions in average failure and repetition, which could be read as it takes time for quality inputs to take effect. Contrary to expectations, the larger the number of periods teachers in the school receive training, the larger is the increase in average failure and repetition in schools only receiving Compensatory supports (Models 7 an 8 in Panel B –Tables 3 and 4). Note that this finding might be driven by a few outlier schools as (i) there are very few schools receiving teacher training and no *Oportunidades* students; and (ii) for schools that have *Oportunidades* students and receive support to teacher training, the interaction is negative and significant.

Joint effects are generally not significant, which implies that we cannot accept the existence of complementarities between interventions. One remarkable exception is the –almost significant– increase in dropout rates in schools receiving both Compensatory supports –and more specifically the AGEs support– and *Oportunidades* students (Table 5, Models 5 to 8 in Panels A and B, respectively). A possible reading of this result is that *Oportunidades* helps reduce intra-year drop out but less so in those schools that also receive Compensatory supports. This might suggest the existence of a positive endogenous program placement bias in the intra-year drop out equation, which could possibly be also present in the failure and repetition equations. In turn, this would imply that complementarities between interventions –while potentially existent– might be underestimated.

### 5.3 Potential Biases

#### 5.3.1 Endogenous Program Placement Bias

Program placement biases might arise if the state authority decides to allocate programs non-randomly in response to political considerations. For instance, the state government could

assign benefits to more disadvantaged schools first given budget constraints. In this case, estimates would be downward biased. There is enough variation in the time schools first receive different supports in the data to raise such concerns. We argue that the inclusion of state-specific trends captures state specific aggregate time effects (shifts in tastes, changes in the allocation of resources, etc.) and minimizes the potential for this bias.

It could also be that program placement responds to some specific characteristics of the school that are correlated with school performance. Indeed, the test of equality of the pre-intervention trends showed that better performing schools in school year 1997-98 are receiving the Compensatory treatment earlier (before school year 2002-03). If we assume these characteristics time-invariant, then the inclusion of school fixed effects corrects for the bias. What if program placement responds to time-varying characteristics instead? If, for instance, these better performing schools have more motivated teachers and students living in families where education is perceived as a priority, and if these characteristics both attract government support and generate better educational outcomes, then regression estimates are likely to overestimate the true program impact. Moreover, if better schools are intervened first there is a concern that the identified effect on AGEs may disappear over time as worse performing or less motivated schools join the program. Probably in anticipation of such a pattern, CONAFE already started introducing –beginning in school year 2003-04– a new support called CAPAGEs (*Capacitación para el Apoyo a la Gestión Escolar*) aimed at providing guidance on the administration of the monetary resources provided by the AGEs support. If we suspect that time-varying unobservables are indeed correlated with outcomes, then we will have to rely on the identifying assumption in equation (4') to legitimate our estimates. As shown in Tables 2A and 2B, pre-intervention trends are rather well balanced. [19]

### 5.3.2 Changes in the Distribution of Students in the School

[19] A more direct way to minimize the potential for endogenous program placement bias would have been to control for the program rule in the estimation. Three reasons made this approach unattractive: first, the targeting rule is computed at one point in time. Although we could construct a time-varying targeting index by applying the formula to data from different years, we would first need to generate time variation to some variables by extrapolation which might add considerable measurement error. Second, the targeting rule for primary schools is not unique: schools that were to be phased in earlier and were subject to a specific criterion would continue to respond to that criterion, even if they started to receive benefits at later dates once a different criteria –the targeting rule, for example– were already in operation. Third, the targeting rule does not determine when schools receive a particular intervention (AGEs, school supplies, etc).

The error term $\bar{\varepsilon}_{st}$ in equation (3) includes unobserved student characteristics ($\theta_{ist}$ = skills, ability, motivation) that we have so far assumed uncorrelated with the observed treatment variables. However, treatment might affect the individual decision of enrolling in school. Assume, for instance, that through equipping schools, training teachers and empowering local decision making, Compensatory-supported schools attract higher skill students or that more motivated parents enroll more motivated students. On the other hand, a demand-side intervention like *Oportunidades* might be attracting lower skill or less motivated students with an opportunity cost of schooling large enough to not attend school without the subsidy. In either situation, if changes in total enrollment significantly alter the distribution of students' skills in the school, then treatment is correlated with unobserved ability and the estimated average treatment effect will likely be biased. In other words, if the interventions affect the individual probability of enrolling in school, $N_{st} = g(C_{s,t-1}, OP_{s,t-1})$, and if the skills of the marginal student attracted are different from the average pre-intervention distribution of skills in the school, $\theta_{ist} \neq \overline{\theta_{st}} = \frac{1}{N_{st}} \sum_{i=1}^{N} \theta_{ist}$; then $pr(\bar{Y}_{st}) = g(C_{s,t-1}, OPRatio_{s,t-1}, \overline{\theta_{st}}(N_{st} = g(C_{s,t-1}, OP_{s,t-1})); \bar{I}_{sjt}, X_{skt})$. Consequently, changes in the school aggregate rates can either come from (i) changes in existing students' individual performance (changes in the numerator) and (ii) changes in the distribution of students, which is affected by the total number of students (changes in the denominator). If better (worse) students are attracted, our estimates of the treatment effects on outcomes will be upward (downward) biased.

Although it is difficult to determine the direction of the bias, we can at least test for its existence by examining changes in enrollment in response to the interventions. We test whether this difference in the pace of reduction of total enrollment is significant by running equation (3) on total enrollment.[20] First, in Table 6, we test for the equality in total enrollment pre-intervention trends between treatment and comparison schools and find that they are well-balanced. Results in Table 7 show that neither the Compensatory package nor any of its supports significantly affect total enrollment in beneficiary schools. Contrarily, estimates show a marginal reduction of a little over half a student in total enrollment in schools with a concentration of *Oportunidades* students over 25 percent (Model 3 in Panels A and B, Table 7). This effect is larger –one student– in schools receiving both Compensatory supports and a large enough number of *Oportunidades* students. This could be read as a crowding out effect of *Oportunidades*. In any case, these effects

[20] We do not use the *Oportunidades* ratio variable in this specification as is dependent on total enrollment.

are very small in size and only significant at the 10 percent level, which suggest the potential for bias is almost negligible.

### 5.3.3 Spillover Effects

If there are positive spillovers between *Oportunidades* beneficiary and non-beneficiary students, then the estimated impacts might include both the treatment effect on the treated (beneficiary) students and the spillover effects on their peers and overestimate the average *Oportunidades* treatment effect. Spillover effects could also exist across schools in the same locality or geographical area. For example, improved Compensatory schools that provide free supplies might attract better students in neighboring communities. Although plausible, we do not consider this a major concern. First, because there is not much room left for school choice in the rural disadvantaged areas studied here where access to a school is not necessarily guaranteed. Second, as shown in the previous section, there are no changes in total enrollment between treated and non-treated schools as a result of Compensatory education.

## 6 Conclusions

Mexico addresses the issue of education for disadvantaged students primarily through supply-side-oriented Compensatory education and demand-side scholarships. This paper has benefited from the existing data on the coverage of both the Compensatory Program and *Oportunidades* to test for the existence of synergies between these interventions on school averaged failure, grade repetition and intra-year dropout rates between 1998 and 2001.

Results show mild effects of *Oportunidades* on repetition and a much larger effects on intra-year drop out. This is not surprising as the *Oportunidades* scholarship is conditional on both school enrollment *and* on not repeating a grade more than twice. The mild effects on failure might be related –at least partly– to the improved nutrition and better health practices the program enforces. This finding, while giving some extra evidence in support of the already established strong positive effects of health on school performance (Miguel and Kremer 2004; Glewwe et al 2004), is in line with the Oportunidades evaluation literature that finds no effects on student achievement (Berhman et al 2000, Parker et al 2005) nor effects on cognitive development (Gertler and Fernald).

The Compensatory program's positive impact on failure disappears once we control for the intensity of *Oportunidades* treatment in the estimation. However, when we break up the

Compensatory intervention into its different components, then the effect of the support to school management componenet (AGEs) and the provision of school supplies on failure and grade repetition persist even after controlling for *Oportunidades* –or in other words, even in schools with no or less than 25 percent of students receiving *Oportunidades*.

Finally, we find no robust evidence of joint effects. We are not able to show that Compensatory education –in the form of support, training and empowerment of local agents or the provision of inputs– enhances the effects of targeted scholarships, nor vice versa. We surmise that such finding is due to an inability of the econometric specification to capture super-additive effects, rather than as evidence of no synergies between the two interventions. There might be other omitted factors that jointly affect the effects of supply- and demand-side educational interventions on schooling outcomes that we are unable to control for given the available data. Alternatively, it might be that the potential synergy effects are not strong enough, or that there is too much variation in the effects across schools, for them to be identified given the sample sizes we have worked with.

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# TABLES

**Table 1: Descriptive Statistics for Covariates and Dependent Variables at Baseline (1997)**

| Sample | Variable | N | Percentage % | Mean | SD |
|---|---|---|---|---|---|
| ***All Schools*** | Number of Teachers | 4132 | 100 | 5.500 | (3.912) |
| | Proportion of Teachers in Carrera Magisterial | 4132 | 100 | 0.524 | (0.356) |
| | Number of Desks | 4132 | 100 | 89.105 | (98.425) |
| | Number of Classrooms | 4132 | 100 | 7.324 | (2.665) |
| | Total Enrollment | 4132 | 100 | 154.989 | (130.170) |
| | Failure Rate | 4132 | 100 | 0.105 | (0.056) |
| | Repetition Rate | 4132 | 100 | 0.097 | (0.053) |
| | Intra-Year Drop Out Rate | 4132 | 100 | 0.043 | (0.045) |
| ***Comparison Schools*** | Number of Teachers | 1074 | 26 | 8.419 | (4.321) |
| | Proportion of Teachers in Carrera Magisterial | 1074 | 26 | 0.623 | (0.311) |
| | Number of Desks | 1074 | 26 | 152.927 | (138.392) |
| | Number of Classrooms | 1074 | 26 | 9.183 | (3.466) |
| | Total Enrollment | 1074 | 26 | 255.150 | (155.732) |
| | Failure Rate | 1074 | 26 | 0.090 | (0.048) |
| | Repetition Rate | 1074 | 26 | 0.083 | (0.043) |
| | Intra-Year Drop Out Rate | 1074 | 26 | 0.056 | (0.042) |
| ***Compensatory Beneficiary Schools Only*** | Number of Teachers | 330 | 8 | 5.034 | (3.253) |
| | Proportion of Teachers in Carrera Magisterial | 330 | 8 | 0.610 | (0.337) |
| | Number of Desks | 330 | 8 | 92.177 | (80.737) |
| | Number of Classrooms | 330 | 8 | 6.749 | (2.172) |
| | Total Enrollment | 330 | 8 | 138.615 | (102.089) |
| | Failure Rate | 330 | 8 | 0.098 | (0.051) |
| | Repetition Rate | 330 | 8 | 0.092 | (0.051) |
| | Intra-Year Drop Out Rate | 330 | 8 | 0.062 | (0.054) |
| ***Oportunidades Beneficiary Schools Only*** | Number of Teachers | 1570 | 38 | 5.065 | (3.463) |
| | Proportion of Teachers in Carrera Magisterial | 1570 | 38 | 0.462 | (0.348) |
| | Number of Desks | 1570 | 38 | 67.900 | (71.943) |
| | Number of Classrooms | 1570 | 38 | 7.009 | (2.159) |
| | Total Enrollment | 1570 | 38 | 163.877 | (110.018) |
| | Failure Rate | 1570 | 38 | 0.113 | (0.055) |
| | Repetition Rate | 1570 | 38 | 0.103 | (0.051) |
| | Intra-Year Drop Out Rate | 1570 | 38 | 0.035 | (0.039) |
| ***Oportunidades and Compensatory Schools*** | Number of Teachers | 1158 | 28 | 3.514 | (2.424) |
| | Proportion of Teachers in Carrera Magisterial | 1158 | 28 | 0.483 | (0.382) |
| | Number of Desks | 1158 | 28 | 58.574 | (52.367) |
| | Number of Classrooms | 1158 | 28 | 6.189 | (1.349) |
| | Total Enrollment | 1158 | 28 | 91.317 | (68.827) |
| | Failure Rate | 1158 | 28 | 0.109 | (0.062) |
| | Repetition Rate | 1158 | 28 | 0.104 | (0.062) |
| | Intra-Year Drop Out Rate | 1158 | 28 | 0.038 | (0.047) |

Notes: *Compensatory* beneficiary schools are schools that receive at least one of the following CONAFE provided supports continuously starting in 1998 (or later) until 2001: School management support (AGEs), school supplies and teacher training. *Oportunidades* beneficiary schools are those that have more than 25 percent of students receiving *Oportunidades* beneficits continuously starting in 1998 (or later) until 2001.

**Table 2A: Difference in Pre-Intervention Trends (1995 to 1997) of the Educational Outcomes between Intervened and Non-Intervened Schools**

| | FAILURE RATE | | REPETITION RATE | | INTRA-YEAR DROP OUT RATE | |
|---|---|---|---|---|---|---|
| | Model 1 | Model 2 | Model 1 | Model 2 | Model 1 | Model 2 |
| ***Comparison Schools*** | | | | | | |
| Mean Dependent Variable in 1995 | 0.107** | 0.107** | 0.097** | 0.097** | 0.043** | 0.043** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Difference in year 1996 | 0.031+ | 0.029+ | 0.027 | 0.026 | 0.021+ | 0.022+ |
| | (0.016) | (0.016) | (0.019) | (0.018) | (0.013) | (0.013) |
| Difference in year 1997 | 0.016 | 0.016 | 0.009 | 0.010 | -0.010 | -0.012 |
| | (0.011) | (0.011) | (0.012) | (0.012) | (0.011) | (0.011) |
| ***Compensatory Beneficiary Schools -Package*** | | | | | | |
| Difference in year 1996 | -0.000 | | -0.001 | | 0.003 | |
| | (0.004) | | (0.004) | | (0.004) | |
| Difference in year 1997 | -0.006 | | -0.004 | | 0.007 | |
| | (0.005) | | (0.005) | | (0.005) | |
| ***Compensatory Beneficiary Schools -by Intervention*** | | | | | | |
| AGEs -difference in year 1996 | | -0.003 | | -0.001 | | 0.005 |
| | | (0.006) | | (0.006) | | (0.006) |
| AGEs -difference in year 1997 | | -0.012 | | -0.007 | | 0.008 |
| | | (0.008) | | (0.008) | | (0.006) |
| Supplies -difference in year 1996 | | 0.002 | | 0.001 | | 0.004 |
| | | (0.006) | | (0.006) | | (0.006) |
| Supplies -difference in year 1997 | | 0.007 | | 0.004 | | 0.003 |
| | | (0.008) | | (0.008) | | (0.007) |
| Teacher Training -difference in year 1996 | | 0.009 | | -0.002 | | -0.012 |
| | | (0.009) | | (0.009) | | (0.011) |
| Teacher Training -difference in year 1997 | | 0.003 | | -0.005 | | -0.005 |
| | | (0.011) | | (0.012) | | (0.013) |
| ***Oportunidades (OP) Beneficiary Schools*** | | | | | | |
| Difference in year 1996 | -0.002 | -0.001 | -0.003 | -0.004 | -0.001 | -0.001 |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Difference in year 1997 | -0.002 | -0.002 | -0.001 | -0.002 | 0.001 | 0.000 |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| ***Compensatory and OP Beneficiary Schools -Package*** | | | | | | |
| Difference in year 1996 | 0.004 | | 0.006 | | -0.002 | |
| | (0.005) | | (0.005) | | (0.005) | |
| Difference in year 1997 | 0.006 | | 0.004 | | -0.007 | |
| | (0.006) | | (0.006) | | (0.005) | |
| ***Compensatory and OP Beneficiary Schools -by Intervention*** | | | | | | |
| AGEs & *Oportunidades* -difference in year 1996 | | 0.006 | | 0.008 | | -0.004 |
| | | (0.007) | | (0.007) | | (0.006) |
| AGEs & *Oportunidades* -difference in year 1997 | | 0.013 | | 0.009 | | -0.009 |
| | | (0.009) | | (0.009) | | (0.007) |
| Supplies & *Oportunidades* -difference in year 1996 | | -0.004 | | -0.003 | | -0.002 |
| | | (0.008) | | (0.008) | | (0.007) |
| Supplies & *Oportunidades* -difference in year 1997 | | -0.007 | | -0.004 | | -0.004 |
| | | (0.009) | | (0.009) | | (0.007) |
| Teacher Training & *Oportunidades* -difference in year 1996 | | -0.006 | | 0.007 | | 0.011 |
| | | (0.011) | | (0.012) | | (0.012) |
| Teacher Training & *Oportunidades* -difference in year 1997 | | -0.005 | | 0.006 | | 0.014 |
| | | (0.013) | | (0.013) | | (0.014) |
| School Fixed Effects & State Specific Time Trends | Y | Y | Y | Y | Y | Y |
| Number of Observations | 12396 | 12396 | 12396 | 12396 | 12396 | 12396 |
| Number of Schools | 4132 | 4132 | 4132 | 4132 | 4132 | 4132 |

Notes: +significant at the 10%, *significant at the 5%, **significant at the 1%. Robust SE clustered at the school level in parantheses. *Compensatory* beneficiary schools are schools that receive at least one of the following CONAFE provided supports continuously starting in 1998 (or later) until 2001: Apoyo a la Gestión Escolar (AGEs), school supplies and teacher training. *Oportunidades* beneficiary schools are those that have more than 25 percent of students receiving *Oportunidades* beneficits continuously starting in 1998 (or later) until 2001. Control schools are schools that started receiving the *Compensatory Program* and/or *Oportunidades* beneficiary students after 2002 or that have not yet received benefits. Extreme values for the dependent variables trimmed at the top 0.5% of the dependent variable distribution.

**Table 2B: Difference in Pre-Intervention Trends (1995 to 1997) in Educational Outcomes between Non-Intervened Schools and Schools Intervened in Subsequent Years**

| | FAILURE RATE | | REPETITION RATE | | INTRA-YEAR DROP OUT RATE | |
|---|---|---|---|---|---|---|
| | Model 1 | Model 2 | Model 1 | Model 2 | Model 1 | Model 2 |
| ***Comparison Schools*** | | | | | | |
| Mean Dependent Variable in 1995 | 0.107** | 0.107** | 0.097** | 0.097** | 0.043** | 0.043** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Year 1996 | 0.031+ | 0.030+ | 0.029 | 0.029 | 0.024+ | 0.024+ |
| | (0.016) | (0.016) | (0.018) | (0.018) | (0.013) | (0.013) |
| Year 1997 | 0.011 | 0.008 | 0.009 | 0.009 | -0.013 | -0.012 |
| | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) |
| ***Compensatory Beneficiary Schools*** | | | | | | |
| Year 1996 * *Compensatory* starting in 1998 | 0.000 | | -0.003 | | -0.002 | |
| | (0.008) | | (0.008) | | (0.006) | |
| Year 1996 * *Compensatory* starting in 1999 | -0.003 | | 0.001 | | -0.001 | |
| | (0.006) | | (0.006) | | (0.005) | |
| Year 1996 * *Compensatory* starting in 2000 | 0.001 | | 0.003 | | 0.004 | |
| | (0.005) | | (0.005) | | (0.005) | |
| Year 1996 * *Compensatory* starting in 2001 | 0.006 | | 0.006 | | 0.004 | |
| | (0.004) | | (0.004) | | (0.003) | |
| Year 1997 * *Compensatory* starting in 1998 | 0.004 | | -0.002 | | 0.010 | |
| | (0.008) | | (0.009) | | (0.008) | |
| Year 1997 * *Compensatory* starting in 1999 | -0.010 | | -0.004 | | -0.002 | |
| | (0.007) | | (0.007) | | (0.005) | |
| Year 1997 * *Compensatory* starting in 2000 | -0.001 | | -0.002 | | 0.005 | |
| | (0.006) | | (0.006) | | (0.004) | |
| Year 1997 * *Compensatory* starting in 2001 | 0.002 | | 0.002 | | 0.003 | |
| | (0.004) | | (0.005) | | (0.003) | |
| ***Compensatory Beneficiary Schools -by Intervention*** | | | | | | |
| Year 1996 * AGEs starting in 1998 | | 0.003 | | -0.002 | | 0.028 |
| | | (0.035) | | (0.031) | | (0.022) |
| Year 1996 * AGEs starting in 1999 | | 0.000 | | 0.003 | | -0.002 |
| | | (0.006) | | (0.006) | | (0.005) |
| Year 1996 * AGEs starting in 2000 | | -0.004 | | 0.003 | | 0.006 |
| | | (0.006) | | (0.006) | | (0.005) |
| Year 1996 * AGEs starting in 2001 | | 0.004 | | 0.008 | | 0.003 |
| | | (0.005) | | (0.005) | | (0.004) |
| Year 1997 * AGEs starting in 1998 | | -0.026 | | -0.020 | | -0.004 |
| | | (0.029) | | (0.025) | | (0.030) |
| Year 1997 * AGEs starting in 1999 | | -0.005 | | -0.001 | | -0.004 |
| | | (0.007) | | (0.007) | | (0.005) |
| Year 1997 * AGEs starting in 2000 | | -0.007 | | -0.004 | | 0.003 |
| | | (0.007) | | (0.007) | | (0.005) |
| Year 1997 * AGEs starting in 2001 | | 0.001 | | 0.002 | | 0.004 |
| | | (0.005) | | (0.005) | | (0.004) |
| Year 1996 * Supplies starting in 1998 | | -0.002 | | -0.007 | | -0.013 |
| | | (0.011) | | (0.012) | | (0.011) |
| Year 1996 * Supplies starting in 1999 | | -0.029+ | | -0.033* | | -0.023 |
| | | (0.018) | | (0.016) | | (0.015) |
| Year 1996 * Supplies starting in 2000 | | 0.000 | | -0.012 | | -0.004 |
| | | (0.009) | | (0.008) | | (0.008) |
| Year 1996 * Supplies starting in 2001 | | 0.005 | | 0.005 | | 0.004 |
| | | (0.005) | | (0.005) | | (0.005) |
| Year 1997 * Supplies starting in 1998 | | 0.017 | | 0.013 | | -0.005 |
| | | (0.012) | | (0.013) | | (0.012) |
| Year 1997 * Supplies starting in 1999 | | -0.039+ | | -0.033 | | -0.019 |
| | | (0.022) | | (0.023) | | (0.022) |
| Year 1997 * Supplies starting in 2000 | | 0.005 | | -0.004 | | 0.004 |
| | | (0.010) | | (0.009) | | (0.008) |
| Year 1997 * Supplies starting in 2001 | | 0.002 | | 0.005 | | -0.002 |
| | | (0.006) | | (0.006) | | (0.005) |
| Year 1996 * Teacher Training starting in 1998 | | 0.004 | | 0.000 | | 0.008 |
| | | (0.011) | | (0.012) | | (0.010) |
| Year 1996 * Teacher Training starting in 1999 | | 0.019+ | | 0.028* | | 0.001 |
| | | (0.011) | | (0.012) | | (0.011) |
| Year 1996 * Teacher Training starting in 2000 | | 0.014 | | 0.012 | | 0.002 |
| | | (0.014) | | (0.012) | | (0.010) |
| Year 1997 * Teacher Training starting in 1998 | | -0.001 | | -0.011 | | 0.019+ |
| | | (0.012) | | (0.014) | | (0.010) |
| Year 1997 * Teacher Training starting in 1999 | | 0.009 | | 0.013 | | 0.000 |
| | | (0.013) | | (0.012) | | (0.015) |
| Year 1997 * Teacher Training starting in 2000 | | 0.007 | | 0.020 | | 0.016 |
| | | (0.018) | | (0.015) | | (0.013) |

**(table A1 -cont.)**

| | | | | | | |
|---|---|---|---|---|---|---|
| ***Oportunidades (OP) Beneficiary Schools*** | | | | | | |
| Year 1996 * *Oportunidades* starting in 1998 | -0.001 | -0.001 | -0.002 | -0.002 | -0.000 | -0.001 |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Year 1996 * *Oportunidades* starting in 1999 | -0.002 | -0.001 | -0.004 | -0.004 | -0.005 | -0.005 |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.003) | (0.003) |
| Year 1996 * *Oportunidades* starting in 2000 | -0.002 | -0.002 | -0.004 | -0.003 | -0.003 | -0.003 |
| | (0.005) | (0.005) | (0.005) | (0.005) | (0.004) | (0.004) |
| Year 1996 * *Oportunidades* starting in 2001 | 0.001 | 0.001 | 0.001 | 0.002 | -0.003 | -0.003 |
| | (0.005) | (0.005) | (0.005) | (0.005) | (0.004) | (0.004) |
| Year 1997 * *Oportunidades* starting in 1998 | -0.000 | 0.000 | 0.002 | 0.002 | -0.001 | -0.001 |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Year 1997 * *Oportunidades* starting in 1999 | 0.001 | 0.001 | -0.001 | -0.002 | -0.004 | -0.004 |
| | (0.005) | (0.005) | (0.005) | (0.005) | (0.003) | (0.003) |
| Year 1997 * *Oportunidades* starting in 2000 | -0.002 | -0.002 | -0.004 | -0.003 | 0.003 | 0.002 |
| | (0.005) | (0.005) | (0.005) | (0.005) | (0.004) | (0.004) |
| Year 1997 * *Oportunidades* starting in 2001 | 0.002 | 0.002 | 0.002 | 0.002 | -0.002 | -0.001 |
| | (0.005) | (0.005) | (0.005) | (0.005) | (0.004) | (0.004) |
| ***Compensatory and Oportunidades Beneficiary Schools*** | | | | | | |
| Year 1996 * *Compensatory* in 1998 * Oportunidades in 1998 | 0.008 | | 0.013 | | -0.001 | |
| | (0.010) | | (0.010) | | (0.009) | |
| Year 1996 * *Compensatory* in 1999 * Oportunidades in 1999 | 0.022 | | 0.006 | | 0.010 | |
| | (0.014) | | (0.018) | | (0.012) | |
| Year 1996 * *Compensatory* in 2000 * Oportunidades in 2000 | 0.017 | | -0.005 | | 0.000 | |
| | (0.026) | | (0.024) | | (0.021) | |
| Year 1996 * *Compensatory* in 2001 * Oportunidades in 2001 | 0.000 | | 0.006 | | -0.012 | |
| | (0.013) | | (0.014) | | (0.011) | |
| Year 1997 * *Compensatory* in 1998 * Oportunidades in 1998 | -0.001 | | 0.002 | | -0.010 | |
| | (0.010) | | (0.010) | | (0.010) | |
| Year 1997 * *Compensatory* in 1999 * Oportunidades in 1999 | 0.017 | | 0.007 | | -0.001 | |
| | (0.016) | | (0.017) | | (0.010) | |
| Year 1997 * *Compensatory* in 2000 * Oportunidades in 2000 | -0.040* | | -0.032 | | -0.003 | |
| | (0.019) | | (0.029) | | (0.022) | |
| Year 1997 * *Compensatory* in 2001 * Oportunidades in 2001 | 0.007 | | 0.004 | | 0.003 | |
| | (0.017) | | (0.020) | | (0.013) | |
| ***Compensatory and OP Beneficiary Schools -by Intervention*** | | | | | | |
| Year 1996 * AGEs in 1998 * *Oportunidades* in 1998 | | 0.010 | | 0.019 | | -0.026 |
| | | (0.039) | | (0.039) | | (0.026) |
| Year 1996 * AGEs in 1999 * *Oportunidades* in 1999 | | 0.026+ | | 0.016 | | 0.005 |
| | | (0.014) | | (0.016) | | (0.012) |
| Year 1996 * AGEs in 2000 * *Oportunidades* in 2000 | | -0.009 | | -0.023 | | -0.000 |
| | | (0.022) | | (0.023) | | (0.016) |
| Year 1996 * AGEs in 2001 * *Oportunidades* in 2001 | | 0.003 | | 0.014 | | -0.013 |
| | | (0.016) | | (0.016) | | (0.014) |
| Year 1997 * AGEs in 1998 * *Oportunidades* in 1998 | | 0.023 | | 0.011 | | 0.015 |
| | | (0.040) | | (0.032) | | (0.036) |
| Year 1997 * AGEs in 1999 * *Oportunidades* in 1999 | | 0.015 | | 0.007 | | 0.004 |
| | | (0.014) | | (0.015) | | (0.013) |
| Year 1997 * AGEs in 2000 * *Oportunidades* in 2000 | | -0.034* | | -0.042+ | | 0.002 |
| | | (0.015) | | (0.023) | | (0.022) |
| Year 1997 * AGEs in 2001 * *Oportunidades* in 2001 | | 0.006 | | 0.015 | | -0.010 |
| | | (0.019) | | (0.024) | | (0.015) |
| Year 1996 * Supplies in 1998 * *Oportunidades* in 1998 | | -0.003 | | -0.005 | | 0.015 |
| | | (0.013) | | (0.014) | | (0.014) |
| Year 1996 * Supplies in 1999 * *Oportunidades* in 1999 | | -0.003 | | 0.013 | | 0.035 |
| | | (0.027) | | (0.034) | | (0.035) |
| Year 1996 * Supplies in 2000 * *Oportunidades* in 2000 | | 0.053+ | | 0.022 | | -0.036 |
| | | (0.032) | | (0.031) | | (0.033) |
| Year 1996 * Supplies in 2001 * *Oportunidades* in 2001 | | -0.002 | | -0.008 | | -0.006 |
| | | (0.015) | | (0.014) | | (0.013) |
| Year 1997 * Supplies in 1998 * *Oportunidades* in 1998 | | -0.016 | | -0.021 | | -0.002 |
| | | (0.015) | | (0.016) | | (0.016) |
| Year 1997 * Supplies in 1999 * *Oportunidades* in 1999 | | 0.047 | | 0.053 | | 0.002 |
| | | (0.033) | | (0.032) | | (0.031) |
| Year 1997 * Supplies in 2000 * *Oportunidades* in 2000 | | 0.013 | | 0.017 | | 0.012 |
| | | (0.038) | | (0.049) | | (0.025) |
| Year 1997 * Supplies in 2001 * *Oportunidades* in 2001 | | 0.006 | | -0.004 | | 0.004 |
| | | (0.017) | | (0.018) | | (0.015) |
| Year 1996 * Teacher Training in 1998 * *Oportunidades* in 1998 | | 0.008 | | 0.015 | | -0.019 |
| | | (0.016) | | (0.016) | | (0.015) |
| Year 1996 * Teacher Training in 1999 * *Oportunidades* in 1999 | | -0.037 | | -0.039+ | | -0.014 |
| | | (0.023) | | (0.023) | | (0.027) |
| Year 1996 * Teacher Training in 2000 * *Oportunidades* in 2000 | | -0.036 | | -0.002 | | 0.037 |
| | | (0.065) | | (0.062) | | (0.030) |
| Year 1997 * Teacher Training in 1998 * *Oportunidades* in 1998 | | 0.007 | | 0.020 | | -0.012 |
| | | (0.018) | | (0.019) | | (0.018) |
| Year 1997 * Teacher Training in 1999 * *Oportunidades* in 1999 | | -0.037 | | -0.032 | | 0.001 |
| | | (0.031) | | (0.029) | | (0.032) |
| Year 1997 * Teacher Training in 2000 * *Oportunidades* in 2000 | | -0.011 | | -0.012 | | 0.005 |
| | | (0.059) | | (0.056) | | (0.029) |
| School Fixed Effects & State Specific Time Trends | Y | Y | Y | Y | Y | Y |
| Number of Observations | 12396 | 12396 | 12396 | 12396 | 12396 | 12396 |
| Number of Schools | 4132 | 4132 | 4132 | 4132 | 4132 | 4132 |

Notes: +significant at the 10%, *significant at the 5%, **significant at the 1%. Robust SE clustered at the school level in parantheses. *Compensatory* beneficiary schools are schools that receive at least one of the following CONAFE provided supports continuously starting in 1998 (or later) until 2001: Apoyo a la Gestión Escolar (AGEs), school supplies and teacher training. *Oportunidades* beneficiary schools are those that have more than 25 percent of students receiving *Oportunidades* beneficits continuously starting in 1998 (or later) until 2001. Control schools are schools that started receiving the Compensatory Program and/or Oportunidades beneficiary students after 2002 or that have not yet received benefits. Extreme values for the dependent variables trimmed at the top 0.5% of the dependent variable distribution. Note that no school receives CONAFE's teacher training support in year 2001.

**Table 3: Effect on Failure Rates: *Compensatory Program* (Package and by Intervention), *Oportunidades* Intensity and Joint Effects from 1998 until 2000[1].**

| ***Dependent Variable: Failure Rate*** | **Model 1** | **Model 2** | **Model 3** | **Model 4** | **Model 5** | **Model 6** | **Model 7** | **Model 8** |
|---|---|---|---|---|---|---|---|---|
| ***PANEL A: Compensatory Program Package*** | | | | | | | | |
| ***Compensatory Program Package*** | | | | | | | | |
| *Compensatory* =1 | -0.003 | | | | -0.002 | -0.001 | | |
| | (0.003) | | | | (0.004) | (0.004) | | |
| Number Periods *Compensatory* | | -0.005* | | | | | -0.004+ | -0.004+ |
| | | (0.002) | | | | | (0.002) | (0.002) |
| ***Intensity of Oportunidades*** | | | | | | | | |
| *Oportunidades* Ratio | | | -0.007+ | | -0.007+ | | -0.007+ | |
| | | | (0.004) | | (0.004) | | (0.004) | |
| *Oportunidades* Ratio >0.25 =1 | | | | -0.004+ | | -0.003+ | | -0.003+ |
| | | | | (0.002) | | (0.002) | | (0.002) |
| ***Compensatory Program and Oportunidades*** | | | | | | | | |
| *Compensatory* = 1 * *Oportunidades* Ratio | | | | | -0.002 | | | |
| | | | | | (0.009) | | | |
| *Compensatory* = 1 * *Oportunidades* Ratio >0.25 =1 | | | | | | -0.003 | | |
| | | | | | | (0.004) | | |
| Number Periods *Compensatory* * *Oportunidades* Ratio | | | | | | | -0.001 | |
| | | | | | | | (0.005) | |
| Number Periods *Compensatory* * *Oportunidades* Ratio >0.25 =1 | | | | | | | | -0.002 |
| | | | | | | | | (0.002) |
| ***PANEL B: Compensatory Program by Intervention*** | | | | | | | | |
| ***Compensatory Program Supports*** | | | | | | | | |
| AGEs =1 | -0.009** | | | | -0.013** | -0.013** | | |
| | (0.003) | | | | (0.005) | (0.005) | | |
| Supplies =1 | 0.003 | | | | 0.002 | 0.001 | | |
| | (0.004) | | | | (0.006) | (0.006) | | |
| Training =1 | -0.001 | | | | 0.007 | 0.010 | | |
| | (0.005) | | | | (0.007) | (0.007) | | |
| Number Periods AGEs | | -0.005* | | | | | -0.007+ | -0.007+ |
| | | (0.002) | | | | | (0.004) | (0.004) |
| Number Periods Supplies | | -0.004 | | | | | -0.007+ | -0.007* |
| | | (0.003) | | | | | (0.004) | (0.004) |
| Number Periods Training | | 0.002 | | | | | 0.007+ | 0.008* |
| | | (0.003) | | | | | (0.004) | (0.004) |
| ***Intensity of Oportunidades*** | | | | | | | | |
| *Oportunidades* Ratio | | | -0.007+ | | -0.006 | | -0.007+ | |
| | | | (0.004) | | (0.004) | | (0.004) | |
| *Oportunidades* Ratio >0.25 =1 | | | | -0.004+ | | -0.003+ | | -0.003+ |
| | | | | (0.002) | | (0.002) | | (0.002) |
| ***Compensatory Program and Oportunidades*** | | | | | | | | |
| AGEs = 1 * *Oportunidades* Ratio | | | | | 0.014 | | | |
| | | | | | (0.011) | | | |
| Supplies = 1 * *Oportunidades* Ratio | | | | | 0.005 | | | |
| | | | | | (0.018) | | | |
| Training = 1 * *Oportunidades* Ratio | | | | | -0.031 | | | |
| | | | | | (0.020) | | | |
| AGEs = 1 * *Oportunidades* Ratio >0.25 =1 | | | | | | 0.007 | | |
| | | | | | | (0.005) | | |
| Supplies = 1 * *Oportunidades* Ratio >0.25 =1 | | | | | | 0.003 | | |
| | | | | | | (0.008) | | |
| Training = 1 * *Oportunidades* Ratio >0.25 =1 | | | | | | -0.019* | | |
| | | | | | | (0.009) | | |
| Number Periods AGEs * *Oportunidades* Ratio | | | | | | | 0.007 | |
| | | | | | | | (0.009) | |
| Number Periods Supplies * *Oportunidades* Ratio | | | | | | | 0.014 | |
| | | | | | | | (0.010) | |
| Number Periods Training * *Oportunidades* Ratio | | | | | | | -0.021+ | |
| | | | | | | | (0.011) | |
| Number Periods AGEs * *Oportunidades* Ratio >0.25 =1 | | | | | | | | 0.003 |
| | | | | | | | | (0.004) |
| Number Periods Supplies * *Oportunidades* Ratio >0.25 =1 | | | | | | | | 0.006 |
| | | | | | | | | (0.004) |
| Number Periods Training * *Oportunidades* Ratio >0.25 =1 | | | | | | | | -0.012* |
| | | | | | | | | (0.005) |
| School Fixed Effects & Time-Varying School Charact. (Panels A & B) | Y | Y | Y | Y | Y | Y | Y | Y |
| Controls for Other Interventions -Panel A | - | - | - | - | - | - | - | - |
| Controls for Other Interventions -Panel B | Y | Y | Y | Y | Y | Y | Y | Y |
| State Specific Time & Treatment Specific Trends (Panels A & B) | Y | Y | Y | Y | Y | Y | Y | Y |
| Number of Observations (Panels A & B) | 16528 | 16528 | 16528 | 16528 | 16528 | 16528 | 16528 | 16528 |
| Number of Schools (Panels A & B) | 4132 | 4132 | 4132 | 4132 | 4132 | 4132 | 4132 | 4132 |
| Mean Failure Rate (Panels A & B) | 0.10 | 0.10 | 0.10 | 0.10 | 0.10 | 0.10 | 0.10 | 0.10 |

Notes: +significant at the 10%, *significant at the 5%, **significant at the 1%. Robust SE clustered at the school level in parantheses. Extreme values for the dependent variables trimmed at the top 0.5% of the dependent variable distribution. Panel A: *Compensatory* =1 if the school receives any of the following *Compensatory* supports: AGEs, Supplies and/or Teacher Training. Panel B: AGEs =1 if the school receives school management support, Supplies =1 if the school receives student and school supplies, Training =1if at least one teacher in the school receives training from CONAFE staff. *Oportunidades* Ratio is the ratio of *Oportunidades* beneficiary students in the school over the total number of students in the school. *Oportunidades* Ratio >0.25 =1 if more than 25% of the students in the school are *Oportunidades* beneficiaries. All regressions include time varying school characteristics, state-time specific dummies and treatment specific time trends. Regressions in panel B additionally control for other *Compensatory* supports.

[1]School year 2001-02 has not been included given that no school receives teacher training support in this year.

**Table 4: Effect on Repetition Rates: *Compensatory Program* (Package and by Intervention), *Oportunidades* Intensity and Joint Effects from 1998 until 2000[1].**

| ***Dependent Variable: Repetition Rate*** | **Model 1** | **Model 2** | **Model 3** | **Model 4** | **Model 5** | **Model 6** | **Model 7** | **Model 8** |
|---|---|---|---|---|---|---|---|---|
| ***PANEL A: Compensatory Program Package*** | | | | | | | | |
| ***Compensatory Program Package*** | | | | | | | | |
| *Compensatory* =1 | -0.002 | | | | -0.002 | -0.001 | | |
| | (0.003) | | | | (0.004) | (0.004) | | |
| Number Periods *Compensatory* | | -0.003 | | | | | -0.002 | -0.002 |
| | | (0.002) | | | | | (0.002) | (0.002) |
| ***Intensity of Oportunidades*** | | | | | | | | |
| *Oportunidades* Ratio | | | -0.009* | | -0.008* | | -0.009* | |
| | | | (0.004) | | (0.004) | | (0.004) | |
| *Oportunidades* Ratio >0.25 =1 | | | | -0.005* | | -0.004* | | -0.005* |
| | | | | (0.002) | | (0.002) | | (0.002) |
| ***Compensatory Program and Oportunidades*** | | | | | | | | |
| *Compensatory* = 1 * *Oportunidades* Ratio | | | | | -0.002 | | | |
| | | | | | (0.009) | | | |
| *Compensatory* = 1 * *Oportunidades* Ratio >0.25 =1 | | | | | | -0.002 | | |
| | | | | | | (0.004) | | |
| Number Periods *Compensatory* * *Oportunidades* Ratio | | | | | | | -0.001 | |
| | | | | | | | (0.005) | |
| Number Periods *Compensatory* * *Oportunidades* Ratio >0.25 =1 | | | | | | | | -0.001 |
| | | | | | | | | (0.002) |
| ***PANEL B: Compensatory Program by Intervention*** | | | | | | | | |
| ***Compensatory Program Supports*** | | | | | | | | |
| AGEs =1 | -0.008** | | | | -0.013** | -0.013** | | |
| | (0.003) | | | | (0.005) | (0.005) | | |
| Supplies =1 | 0.002 | | | | 0.003 | 0.002 | | |
| | (0.004) | | | | (0.006) | (0.006) | | |
| Training =1 | 0.002 | | | | 0.009 | 0.011+ | | |
| | (0.005) | | | | (0.006) | (0.007) | | |
| Number Periods AGEs | | -0.004+ | | | | | -0.006 | -0.006 |
| | | (0.002) | | | | | (0.004) | (0.004) |
| Number Periods Supplies | | -0.005+ | | | | | -0.008* | -0.008* |
| | | (0.003) | | | | | (0.004) | (0.003) |
| Number Periods Training | | 0.004 | | | | | 0.009* | 0.010* |
| | | (0.003) | | | | | (0.004) | (0.004) |
| ***Intensity of Oportunidades*** | | | | | | | | |
| *Oportunidades* Ratio | | | -0.009* | | -0.008+ | | -0.009* | |
| | | | (0.004) | | (0.004) | | (0.004) | |
| *Oportunidades* Ratio >0.25 =1 | | | | -0.005* | | -0.004* | | -0.005* |
| | | | | (0.002) | | (0.002) | | (0.002) |
| ***Compensatory Program and Oportunidades*** | | | | | | | | |
| AGEs = 1 * *Oportunidades* Ratio | | | | | 0.016 | | | |
| | | | | | (0.011) | | | |
| Supplies = 1 * *Oportunidades* Ratio | | | | | -0.002 | | | |
| | | | | | (0.017) | | | |
| Training = 1 * *Oportunidades* Ratio | | | | | -0.028 | | | |
| | | | | | (0.019) | | | |
| AGEs = 1 * *Oportunidades* Ratio >0.25 =1 | | | | | | 0.007 | | |
| | | | | | | (0.005) | | |
| Supplies = 1 * *Oportunidades* Ratio >0.25 =1 | | | | | | 0.001 | | |
| | | | | | | (0.007) | | |
| Training = 1 * *Oportunidades* Ratio >0.25 =1 | | | | | | -0.016+ | | |
| | | | | | | (0.009) | | |
| Number Periods AGEs * *Oportunidades* Ratio | | | | | | | 0.005 | |
| | | | | | | | (0.009) | |
| Number Periods Supplies * *Oportunidades* Ratio | | | | | | | 0.013 | |
| | | | | | | | (0.010) | |
| Number Periods Training * *Oportunidades* Ratio | | | | | | | -0.019+ | |
| | | | | | | | (0.011) | |
| Number Periods AGEs * *Oportunidades* Ratio >0.25 =1 | | | | | | | | 0.002 |
| | | | | | | | | (0.004) |
| Number Periods Supplies * *Oportunidades* Ratio >0.25 =1 | | | | | | | | 0.006 |
| | | | | | | | | (0.004) |
| Number Periods Training * *Oportunidades* Ratio >0.25 =1 | | | | | | | | -0.010+ |
| | | | | | | | | (0.005) |
| School Fixed Effects & Time-Varying School Charact. (Panels A & B) | Y | Y | Y | Y | Y | Y | Y | Y |
| Controls for Other Interventions -Panel A | - | - | - | - | - | - | - | - |
| Controls for Other Interventions -Panel B | Y | Y | Y | Y | Y | Y | Y | Y |
| State Specific Time & Treatment Specific Trends (Panels A & B) | Y | Y | Y | Y | Y | Y | Y | Y |
| Number of Observations (Panels A & B) | 16528 | 16528 | 16528 | 16528 | 16528 | 16528 | 16528 | 16528 |
| Number of Schools (Panels A & B) | 4132 | 4132 | 4132 | 4132 | 4132 | 4132 | 4132 | 4132 |
| Mean Repetition Rate (Panels A & B) | 0.09 | 0.09 | 0.09 | 0.09 | 0.09 | 0.09 | 0.09 | 0.09 |

Notes: +significant at the 10%, *significant at the 5%, **significant at the 1%. Robust SE clustered at the school level in parantheses. Extreme values for the dependent variables trimmed at the top 0.5% of the dependent variable distribution. Panel A: *Compensatory* =1 if the school receives any of the following *Compensatory* supports: AGEs, Supplies and/or Teacher Training. Panel B: AGEs =1 if the school receives school management support, Supplies =1 if the school receives student and school supplies, Training =1if at least one teacher in the school receives training from CONAFE staff. *Oportunidades* Ratio is the ratio of *Oportunidades* beneficiary students in the school over the total number of students in the school. *Oportunidades* Ratio >0.25 =1 if more than 25% of the students in the school are *Oportunidades* beneficiaries. All regressions include time varying school characteristics, state-time specific dummies and treatment specific time trends. Regressions in panel B additionally control for other *Compensatory* supports.

[1]School year 2001-02 has not been included given that no school receives teacher training support in this year.

**Table 5: Effect on Intra-Year Drop Out Rates: *Compensatory Program* (Package and by Intervention), *Oportunidades* Intensity and Joint Effects from 1998 until 2000[1].**

| ***Dependent Variable: Intra-Year Drop Out Rate*** | **Model 1** | **Model 2** | **Model 3** | **Model 4** | **Model 5** | **Model 6** | **Model 7** | **Model 8** |
|---|---|---|---|---|---|---|---|---|
| ***PANEL A: Compensatory Program Package*** | | | | | | | | |
| ***Compensatory Program Package*** | | | | | | | | |
| *Compensatory* =1 | -0.001 | | | | -0.005 | -0.006 | | |
| | (0.002) | | | | (0.003) | (0.003) | | |
| Number Periods *Compensatory* | | 0.001 | | | | | -0.002 | -0.002 |
| | | (0.002) | | | | | (0.002) | (0.002) |
| ***Intensity of Oportunidades*** | | | | | | | | |
| *Oportunidades* Ratio | | | -0.012** | | -0.014** | | -0.013** | |
| | | | (0.003) | | (0.003) | | (0.003) | |
| *Oportunidades* Ratio >0.25 =1 | | | | -0.004** | | -0.005** | | -0.005** |
| | | | | (0.001) | | (0.001) | | (0.001) |
| ***Compensatory Program and Oportunidades*** | | | | | | | | |
| *Compensatory* = 1 * *Oportunidades* Ratio | | | | | 0.013+ | | | |
| | | | | | (0.007) | | | |
| *Compensatory* = 1 * *Oportunidades* Ratio >0.25 =1 | | | | | | 0.006+ | | |
| | | | | | | (0.003) | | |
| Number Periods *Compensatory* * *Oportunidades* Ratio | | | | | | | 0.007+ | |
| | | | | | | | (0.004) | |
| Number Periods *Compensatory* * *Oportunidades* Ratio >0.25 =1 | | | | | | | | 0.004+ |
| | | | | | | | | (0.002) |
| ***PANEL B: Compensatory Program by Intervention*** | | | | | | | | |
| ***Compensatory Program Supports*** | | | | | | | | |
| AGEs =1 | -0.001 | | | | -0.005 | -0.005 | | -0.004 |
| | (0.002) | | | | (0.004) | (0.004) | | (0.003) |
| Supplies =1 | -0.001 | | | | -0.000 | 0.000 | | -0.001 |
| | (0.004) | | | | (0.005) | (0.005) | | (0.003) |
| Training =1 | -0.000 | | | | -0.002 | -0.004 | | 0.001 |
| | (0.004) | | | | (0.005) | (0.006) | | (0.003) |
| Number Periods AGEs | | -0.000 | | | | | -0.004 | |
| | | (0.002) | | | | | (0.003) | |
| Number Periods Supplies | | -0.001 | | | | | -0.001 | |
| | | (0.003) | | | | | (0.003) | |
| Number Periods Training | | 0.001 | | | | | 0.002 | |
| | | (0.002) | | | | | (0.003) | |
| ***Intensity of Oportunidades*** | | | | | | | | |
| *Oportunidades* Ratio | | | -0.011** | | -0.013** | | -0.013** | |
| | | | (0.003) | | (0.003) | | (0.003) | |
| *Oportunidades* Ratio >0.25 =1 | | | | -0.004** | | -0.005** | | -0.005** |
| | | | | (0.001) | | (0.001) | | (0.001) |
| ***Compensatory Program and Oportunidades*** | | | | | | | | |
| AGEs = 1 * *Oportunidades* Ratio | | | | | 0.011 | | | |
| | | | | | (0.008) | | | |
| Supplies = 1 * *Oportunidades* Ratio | | | | | -0.002 | | | |
| | | | | | (0.014) | | | |
| Training = 1 * *Oportunidades* Ratio | | | | | 0.009 | | | |
| | | | | | (0.015) | | | |
| AGEs = 1 * *Oportunidades* Ratio >0.25 =1 | | | | | | 0.005 | | |
| | | | | | | (0.004) | | |
| Supplies = 1 * *Oportunidades* Ratio >0.25 =1 | | | | | | -0.002 | | |
| | | | | | | (0.006) | | |
| Training = 1 * *Oportunidades* Ratio >0.25 =1 | | | | | | 0.007 | | |
| | | | | | | (0.007) | | |
| Number Periods AGEs * *Oportunidades* Ratio | | | | | | | 0.012+ | |
| | | | | | | | (0.006) | |
| Number Periods Supplies * *Oportunidades* Ratio | | | | | | | 0.000 | |
| | | | | | | | (0.008) | |
| Number Periods Training * *Oportunidades* Ratio | | | | | | | -0.001 | |
| | | | | | | | (0.009) | |
| Number Periods AGEs * *Oportunidades* Ratio >0.25 =1 | | | | | | | | 0.005+ |
| | | | | | | | | (0.003) |
| Number Periods Supplies * *Oportunidades* Ratio >0.25 =1 | | | | | | | | 0.000 |
| | | | | | | | | (0.004) |
| Number Periods Training * *Oportunidades* Ratio >0.25 =1 | | | | | | | | 0.000 |
| | | | | | | | | (0.004) |
| School Fixed Effects & Time-Varying School Charact. (Panels A & B) | Y | Y | Y | Y | Y | Y | Y | Y |
| Controls for Other Interventions -Panel A | - | - | - | - | - | - | - | - |
| Controls for Other Interventions -Panel B | Y | Y | Y | Y | Y | Y | Y | Y |
| State Specific Time & Treatment Specific Trends (Panels A & B) | Y | Y | Y | Y | Y | Y | Y | Y |
| Number of Observations (Panels A & B) | 16528 | 16528 | 16528 | 16528 | 16528 | 16528 | 16528 | 16528 |
| Number of Schools (Panels A & B) | 4132 | 4132 | 4132 | 4132 | 4132 | 4132 | 4132 | 4132 |
| Mean Intra-Year Drop Out Rate (Panels A & B) | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 | 0.04 |

Notes: +significant at the 10%, *significant at the 5%, **significant at the 1%. Robust SE clustered at the school level in parantheses. Extreme values for the dependent variables trimmed at the top 0.5% of the dependent variable distribution. Panel A: *Compensatory* =1 if the school receives any of the following *Compensatory* supports: AGEs, Supplies and/or Teacher Training. Panel B: AGEs =1 if the school receives school management support, Supplies =1 if the school receives student and school supplies, Training =1if at least one teacher in the school receives training from CONAFE staff. *Oportunidades* Ratio is the ratio of *Oportunidades* beneficiary students in the school over the total number of students in the school. *Oportunidades* Ratio >0.25 =1 if more than 25% of the students in the school are *Oportunidades* beneficiaries. All regressions include time varying school characteristics, state-time specific dummies and treatment specific time trends. Regressions in panel B additionally control for other *Compensatory* supports.

[1]School year 2001-02 has not been included given that no school receives teacher training support in this year.

**Table 6: Difference in Total School Enrollment Pre-Intervention Trends (1995 to 1997) between Intervened and Non-Intervened Schools.**

| | TOTAL ENROLLMENT | |
|---|---|---|
| | **Mod 1** | **Mod 2** |
| ***Comparison Schools*** | | |
| Mean Dep. Var. in 1995 | 155.565** | 155.565** |
| | (0.232) | (0.232) |
| Difference in year 1996 | 2.967 | 2.594 |
| | (3.144) | (3.292) |
| Difference in year 1997 | 7.653+ | 7.352 |
| | (4.372) | (4.588) |
| ***Compensatory Beneficiary Schools -Package*** | | |
| Difference in year 1996 | -1.430 | |
| | (1.395) | |
| Difference in year 1997 | -3.559 | |
| | (2.333) | |
| ***Compensatory Beneficiary Schools -by Intervention*** | | |
| AGEs -difference in year 1996 | | -2.428 |
| | | (1.653) |
| AGEs -difference in year 1997 | | -2.379 |
| | | (3.318) |
| Supplies -difference in year 1996 | | 2.058 |
| | | (2.220) |
| Supplies -difference in year 1997 | | 0.541 |
| | | (3.996) |
| Teacher Training -difference in year 1996 | | -2.207 |
| | | (2.462) |
| Teacher Training -difference in year 1997 | | -4.297 |
| | | (4.425) |
| ***Oportunidades Beneficiary Schools*** | | |
| Difference in year 1996 | -1.129 | -1.310 |
| | (0.947) | (0.937) |
| Difference in year 1997 | -1.143 | -1.220 |
| | (1.443) | (1.429) |
| ***Compensatory and Oportunidades Beneficiary Schools -Package*** | | |
| Difference in year 1996 | 0.682 | |
| | (1.417) | |
| Difference in year 1997 | 2.245 | |
| | (2.403) | |
| ***Compensatory and Oportunidades Beneficiary Schools -by Intervention*** | | |
| AGEs & *Oportunidades* -difference in year 1996 | | 0.079 |
| | | (1.712) |
| AGEs & *Oportunidades* -difference in year 1997 | | -1.234 |
| | | (3.371) |
| Supplies & *Oportunidades* -difference in year 1996 | | 0.118 |
| | | (2.049) |
| Supplies & *Oportunidades* -difference in year 1997 | | 2.953 |
| | | (3.698) |
| Teacher Training & *Oportunidades* -difference in year 1996 | | 3.513 |
| | | (2.557) |
| Teacher Training & *Oportunidades* -difference in year 1997 | | 4.990 |
| | | (4.479) |
| School Fixed Effects & State Specific Time Trends | Y | Y |
| Number of Observations | 12396 | 12396 |
| Number of Schools | 4132 | 4132 |

Notes: +significant at the 10%, *significant at the 5%, **significant at the 1%. Robust SE clustered at the school level in parantheses. *Compensatory* beneficiary schools are schools that receive at least one of the following CONAFE provided supports continuously starting in 1998 (or later) until 2001: school management support (AGEs), school supplies and teacher training. *Oportunidades* beneficiary schools are those that have more than 25 percent of students receiving *Oportunidades* beneficits continuously starting in 1998 (or later) until 2001. Control schools are schools that started receiving the *Compensatory* program and/or *Oportunidades* beneficiary students after 2002 or that have not yet received benefits. Extreme values for the dependent variables trimmed at the top 0.5% of the dependent variable distribution.

**Table 7: Effect on Total Enrollment: *Compensatory Program* (Package and by Intervention), *Oportunidades* Intensity and Joint Effects from 1998 until 2000[1].**

| ***Dependent Variable: Total Enrollment*** | **Model 1** | **Model 2** | **Model 3** | **Model 4** | **Model 5** |
|---|---|---|---|---|---|
| ***PANEL A: Compensatory Program Package*** | | | | | |
| ***Compensatory Program Package*** | | | | | |
| *Compensatory* =1 | 0.089 | | | 0.482 | |
| | (0.771) | | | (1.024) | |
| Number Periods *Compensatory* | | -0.130 | | | 0.560 |
| | | (0.676) | | | (0.814) |
| ***Intensity of Oportunidades*** | | | | | |
| *Oportunidades* Ratio >0.25 =1 | | | -0.654+ | -0.563 | -0.435 |
| | | | (0.356) | (0.392) | (0.389) |
| ***Compensatory Program and Oportunidades*** | | | | | |
| *Compensatory* = 1 * *Oportunidades* Ratio >0.25 =1 | | | | -0.665 | |
| | | | | (0.914) | |
| Number Periods *Compensatory* * *Oportunidades* Ratio >0.25 =1 | | | | | -1.080+ |
| | | | | | (0.596) |
| ***PANEL B: Compensatory Program by Intervention*** | | | | | |
| ***Compensatory Program Supports*** | | | | | |
| AGEs =1 | -0.280 | | | -0.242 | |
| | (0.706) | | | (1.000) | |
| Supplies =1 | 1.082 | | | 1.644 | |
| | (1.369) | | | (1.679) | |
| Training =1 | -0.027 | | | 0.729 | |
| | (1.184) | | | (1.620) | |
| Number Periods AGEs | | 0.086 | | | -0.157 |
| | | (0.586) | | | (0.836) |
| Number Periods Supplies | | -0.711 | | | 0.386 |
| | | (1.145) | | | (1.455) |
| Number Periods Training | | 0.733 | | | 0.834 |
| | | (0.847) | | | (1.193) |
| ***Intensity of Oportunidades*** | | | | | |
| *Oportunidades* Ratio >0.25 =1 | | | -0.649+ | -0.467 | -0.408 |
| | | | (0.356) | (0.391) | (0.381) |
| ***Compensatory Program and Oportunidades*** | | | | | |
| AGEs = 1 * *Oportunidades* Ratio >0.25 =1 | | | | -0.092 | |
| | | | | (0.947) | |
| Supplies = 1 * *Oportunidades* Ratio >0.25 =1 | | | | -1.016 | |
| | | | | (1.535) | |
| Training = 1 * *Oportunidades* Ratio >0.25 =1 | | | | -1.337 | |
| | | | | (1.735) | |
| Number Periods AGEs * *Oportunidades* Ratio >0.25 =1 | | | | | 0.302 |
| | | | | | (0.761) |
| Number Periods Supplies * *Oportunidades* Ratio >0.25 =1 | | | | | -1.893 |
| | | | | | (1.347) |
| Number Periods Training * *Oportunidades* Ratio >0.25 =1 | | | | | -0.053 |
| | | | | | (1.266) |
| School Fixed Effects & Time-Varying School Charact. (Panels A & B) | Y | Y | Y | Y | Y |
| Controls for Other Interventions -Panel A | - | - | - | - | - |
| Controls for Other Interventions -Panel B | Y | Y | Y | Y | Y |
| State Specific Time & Treatment Specific Trends (Panels A & B) | Y | Y | Y | Y | Y |
| Number of Observations (Panels A & B) | 16528 | 16528 | 16528 | 16528 | 16528 |
| Number of Schools (Panels A & B) | 4132 | 4132 | 4132 | 4132 | 4132 |
| Mean Total Enrollment (Panels A & B) | 154.90 | 154.90 | 154.90 | 154.90 | 154.90 |

Notes: +significant at the 10%, *significant at the 5%, **significant at the 1%. Robust SE clustered at the school level in parantheses. Extreme values for the dependent variables trimmed at the top 0.5% of the dependent variable distribution. Panel A: *Compensatory* =1 if the school receives any of the following *Compensatory* supports: AGEs, Supplies and/or Teacher Training. Panel B: AGEs =1 if the school receives school management support, Supplies =1 if the school receives student and school supplies, Training =1if at least one teacher in the school receives training from CONAFE staff. *Oportunidades* Ratio is the ratio of *Oportunidades* beneficiary students in the school over the total number of students in the school. *Oportunidades* Ratio >0.25 =1 if more than 25% of the students in the school are *Oportunidades* beneficiaries. All regressions include time varying school characteristics, state-time specific dummies and treatment specific time trends. Regressions in panel B additionally control for other *Compensatory* supports.

[1]School year 2001-02 has not been included given that no school receives teacher training support in this year.

## APPENDIX 2: GRAPHS

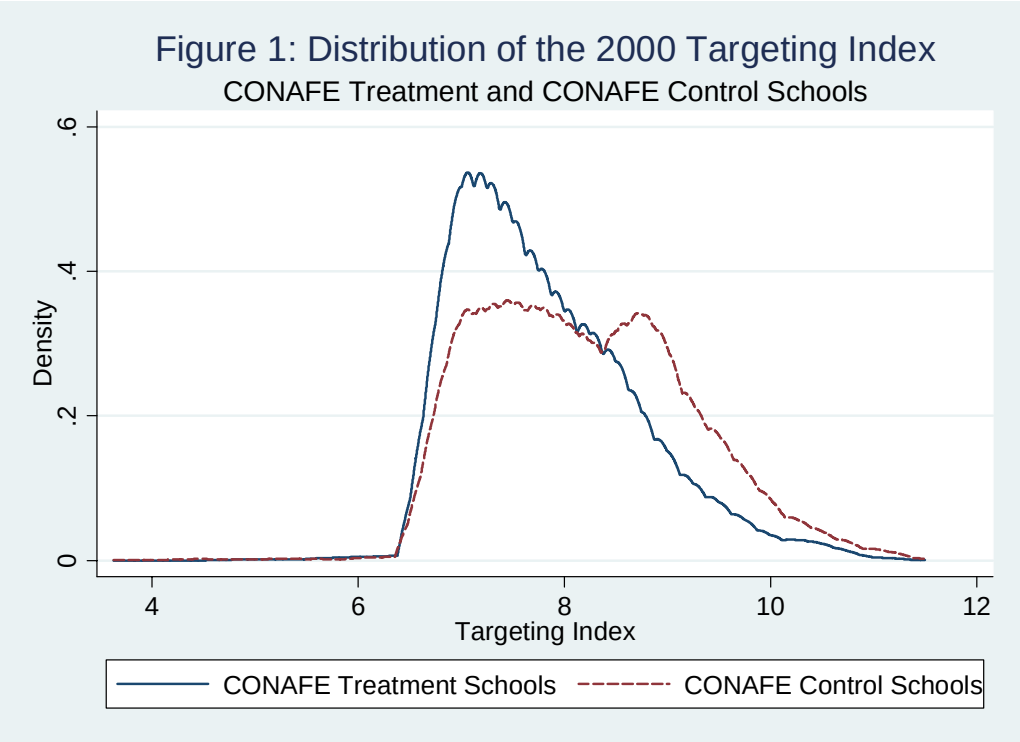



**Figure 2: Percentage of Schools By Treatment Status Over Time**

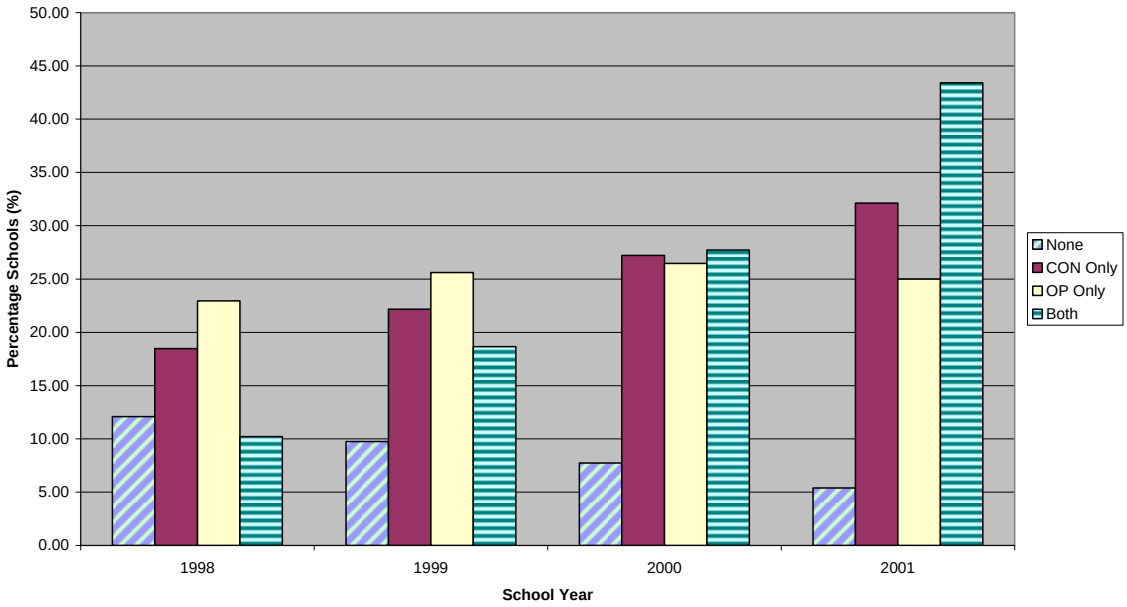
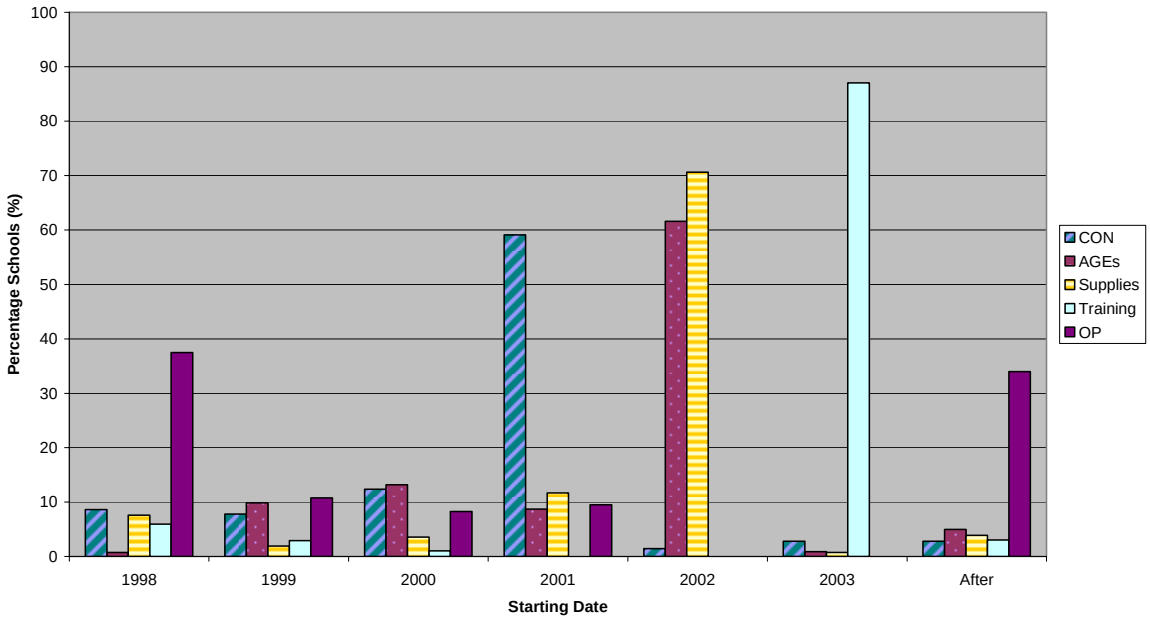
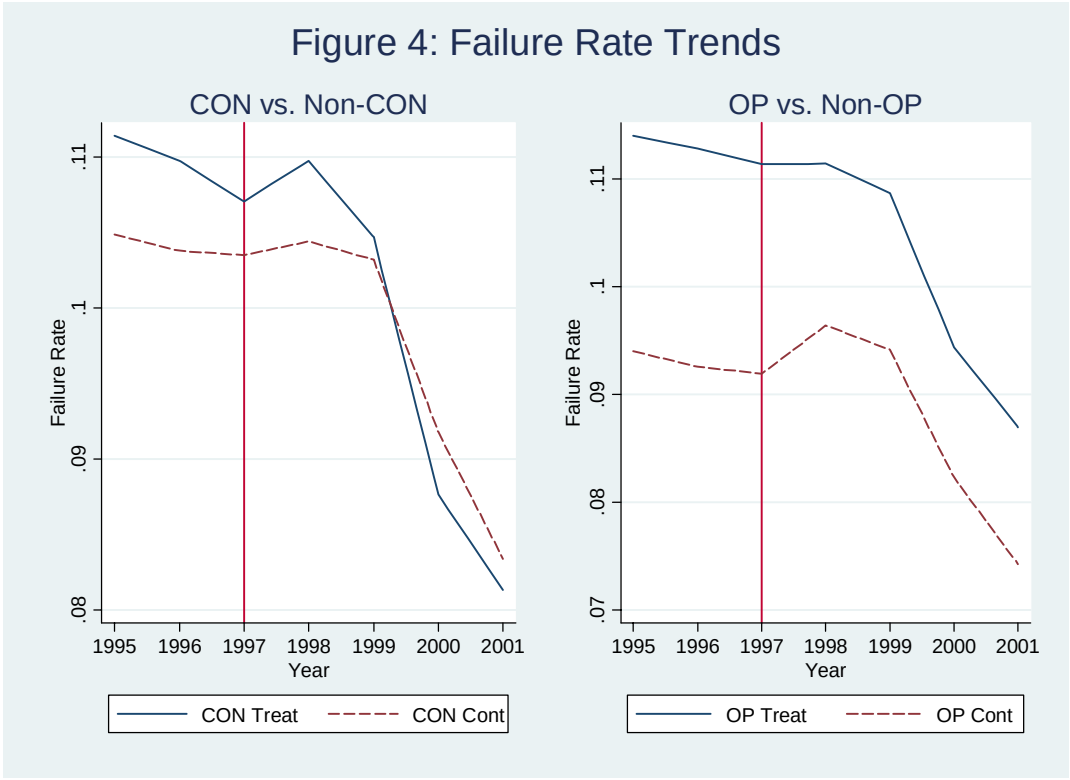
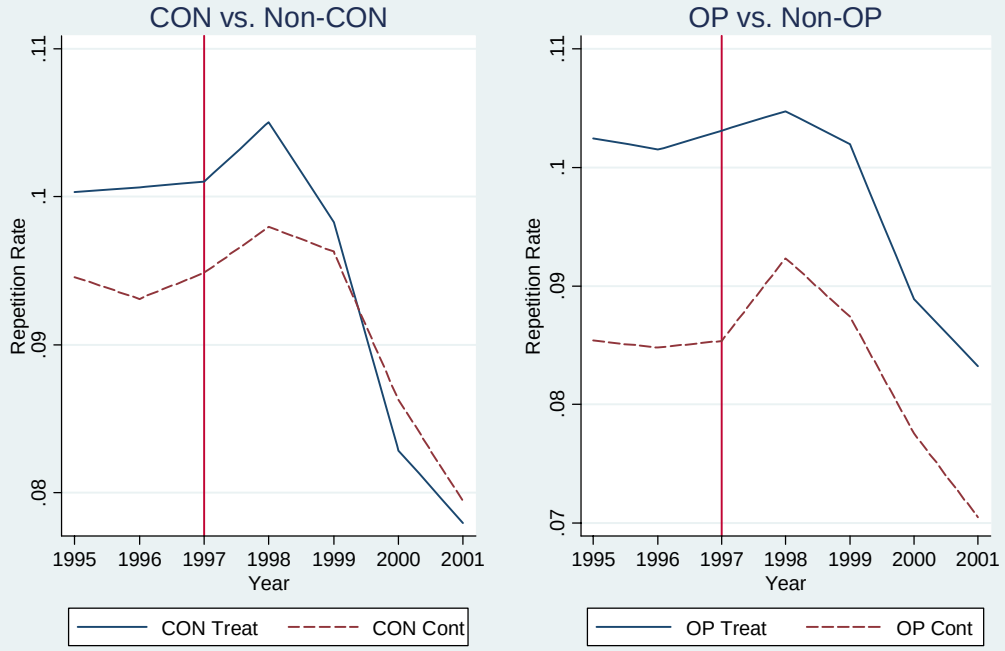


**Figure 3: Percentage of Schools by Date of First Treatment**



## Figure 5: Repetition Rate Trends




## Figure 6: Intra-Year Drop Out Rate Trends