
MEMORANDUM**TO:** Rebecca Tunstall and Orlando Martínez**FROM:** Larissa Campuzano and Randall Blair**DATE:** 5/14/2010
ESVED-222**SUBJECT:** Final Impact Evaluation Design for the Scholarship Activity

Executive Summary

This memorandum presents the final impact evaluation design for FOMILENIO's scholarship activity.¹ The goal of the scholarship activity is to increase enrollment, persistence, and completion of secondary and postsecondary education, and ultimately to improve labor market outcomes of youth living in the Northern Zone. The final evaluation design agreed upon Mathematica, MCC, FOMILENIO, and MINED is **random assignment of eligible applicants to receive scholarships (intervention group) or not receive scholarships (control group)**. This method assigns scholarships in a fair and efficient way, as well as serves MCC's goal of conducting a rigorous impact evaluation. The randomization of scholarships occurred on Friday December 11, 2009. Because it was important to honor the distribution of scholarships that FOMILENIO and MINED allocated to each school and educational program, random assignment was done only within schools and programs in which the demand for scholarships exceeded available scholarships. A total of 636 students were randomly assigned to receive scholarships (intervention group), and 449 students were randomly assigned to no scholarship (control group). After adjustments made by Mathematica in January 2010 in response to potential contamination and eligibility issues, the final sample size for the study was 751 students: 515 students randomly assigned to receive scholarships (intervention group), and 236 students assigned to no scholarship (control group).

A. DESCRIPTION OF THE INTERVENTION

The scholarship activity is aimed at young people in El Salvador's Northern Zone who need financial assistance to pursue their middle school studies. The goal of the scholarship activity is to increase enrollment, persistence, and completion of secondary and postsecondary education,

¹ It builds on memoranda submitted on April 28, 2008 (ESVED-054), and July 16, 2008 (ESVED-094), that described potential evaluation designs for this activity.

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and ultimately to improve labor market outcomes. FOMILENIO has contracted with the Fundación Empresarial para el Desarrollo Educativo (FEPADE) to conduct outreach, process applications, and administer scholarships. For the 2010 school year, FOMILENIO plans to award 1,000 scholarships across 17 selected middle schools in 16 different municipalities and seven departments. Each scholarship recipient will receive a monthly payment of \$30 (totaling \$400 per year) to spend on transportation, food, schoolbooks, materials and uniforms. Students will meet regularly with FEPADE staff to discuss payments and their progress in school, and to resolve any difficulties related to school or their scholarships. If a scholarship recipient fails to make a minimum passing grade of 6.5 in all their classes during a grading period, FEPADE staff will ask the student to sign a letter committing to improved scholastic performance. If a student fails a class for the year, they could lose their scholarship.²

Scholarships will be awarded in the first year of middle school education (that is, 10th grade) and will typically be renewed for the subsequent two years of technical middle school, or the subsequent year of general middle school. However, it is likely that not all scholarship recipients in grade 10 will receive a scholarship in grade 11 (or grade 12 in the case of technical middle schools). It is critical that FOMILENIO and MINED develop clear criteria to determine students' ongoing eligibility for scholarships. Furthermore, it needs to be determined whether scholarships previously assigned to students that are not eligible to renew in 11th grade will be reassigned to other 11th graders or if they will not be replaced. This will have major implications for the study design; therefore we will maintain communication with FOMILENIO and MINED in the next few months to inform them which criteria will be more appropriate for the study design.

B. KEY RESEARCH QUESTION

The purpose of the impact evaluation is to determine whether or not FOMILENIO's scholarship recipients are better off than they would have been without receiving the scholarship. Specifically, the evaluation should answer the question:

- What is the impact of FOMILENIO's scholarships on recipients' education and labor market outcomes?

² During our next mission to El Salvador, we will verify the circumstances in which scholarships will be revoked.

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C. IMPACT EVALUATION DESIGN

The most rigorous impact evaluation design available for determining the effectiveness of the scholarship activity is random assignment among the pool of applicants that have met the program selection criteria (that is, *eligible* applicants). Random assignment is logistically feasible and ethical in cases of *oversubscription*—that is, when the number of eligible applicants exceeds the number of scholarships available. As we learned in December 2009, there were more applicants to the scholarship activity than scholarships available for some schools and educational programs (see Table A.1). This oversubscription of scholarships allowed us to proceed with random assignment of scholarships among eligible applicants within each school and educational program oversubscribed.

Table A.1 presents the number of eligible applicants and the number of scholarships available in each educational program in each participating school. As illustrated, there is oversubscription in 15 educational programs in 12 of the 17 schools selected for the scholarships. As a result, randomization of scholarships was possible for these 15 programs.

1. Student Assignment Process

To promote scholarships for the 2010 academic school year, FEPADE staff visited all 162 primary schools that feed into the selected 17 middle schools. Scholarships applications were due between September 30 and October 7, 2009. FEPADE received 1,841 scholarship applications, which they reviewed in order to assess eligibility. According to FEPADE's review, 1,521 applications were deemed eligible to receive a scholarship. Table A.1 presents the breakdown of eligible applicants by school and program. As agreed with the stakeholders, random assignment was to be done only in schools and programs that were oversubscribed. A total of 15 schools and programs were oversubscribed, with a total number of 1,160 eligible applicants. In December 2009, FEPADE sent Mathematica a list of eligible applicants in each school and educational program that had more eligible applicants than available scholarships. Mathematica used this list to develop a computer program that randomized eligible applicants into three groups: the intervention group (scholarships), the control group (no scholarships), and the non-research group (students in a waiting list that could replace students in the intervention group if they drop out in the first few weeks of the school year).³

³ Random assignment was done by school and educational program. Within each school and educational program, the computer program assigned a random number to each student. The students with the highest numbers were assigned to the intervention group up to the point where scholarships were no longer available; the next five highest numbers were placed on the wait list; the rest of the students (those with the lower random numbers) were placed in the control group.

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On December 11, 2009, scholarships were randomly assigned to applicants in a public event sponsored by FOMILENIO and MCC. Out of a total of 1,160 eligible applicants, 636 scholarships were randomly awarded, 449 students were randomly assigned not to receive scholarships (control group), and 75 students were placed on a waiting list for scholarships (non-research group).

In late January 2010, Mathematica learned that scholarships were awarded to at least 36 students in the control group in one school, Dr. Francisco Martínez Suárez. To avoid biased estimates due to contamination of the control group, all intervention and control students from this school were excluded from the evaluation. All intervention and control students were also excluded from another school, Carolina, due to the large imbalance of intervention students (43) compared to control students (2) at the school. Another concern at this time was the relatively low acceptance rate (70 percent) among students in the intervention group.⁴ As a result, FEPADE had a substantial number of unclaimed scholarships for the 2010 school year, but a lack of viable scholarship recipients outside of the control group. To raise the number of claimed scholarships, Mathematica designated 100 students from the control group as eligible to receive scholarships for the 2010 school year. To respect the randomness of the process, these students were selected according to their random number from the original selection process.⁵ This transfer of students from the control group reduced the size of the study sample which reduced the study's statistical power.⁶ However, it met the more pressing need to award the majority of available scholarships for the academic year.

⁴ FEPADE informed Mathematica and MCC that there were several reasons for the low acceptance rate. In some cases, eligible applicants did not follow through with their intent of enrolling in 10th grade on time. By the time they tried to enroll, schools no longer had place for them. Others decided to enroll in schools that were not selected for scholarships. We have requested that FEPADE documents applicants' reasons for refusing the scholarship.

⁵ The original assignment process placed the students with the highest random numbers in the intervention group, the next five random numbers in the wait-list (non-research) group, and the rest of the students (those with the lowest random numbers) in the control group. The intervention group and the wait-list groups were not affected by the changes in January of 2010. However, in some schools or programs, the original control group changed. Among the original controls, those students with the highest random numbers were placed in a non-research group that was offered a scholarship at that time, and students with the lowest random numbers were kept in the control group and were not offered a scholarship. This decreased the sample size of the study, but respected the randomness of the process.

⁶ The reduction in statistical power due to these changes was the following: the minimum detectable difference went from 0.16 under the original sample size to 0.19 under the revised sample size. The section on statistical power provides more detail.

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As a result of these changes, the evaluation is now being conducted in 12 educational programs of 10 schools with 751 students, 515 of which were randomly assigned to receive scholarships and 236 of which remained in the control group (see Table A.2). The 100 students from the original control group that were designated as eligible for scholarships were excluded from the evaluation (non-research group) in a similar manner to the 75 students originally placed on the waiting list.

2. Advantages of the Proposed Design

We highlight below several features of the design as compared with alternative, less rigorous, designs.

Random assignment (or experimental) designs are preferred to quasi-experimental designs (such as matching of students or regression discontinuity designs) for the following reasons:

1. Random assignment is the best way to ensure that students who receive the scholarship will, on average, be similar in all characteristics to those who do not receive a scholarship.
2. Given oversubscription, random assignment is seen as a natural and fair way to allocate available resources.
3. Under random assignment, a simple comparison of average outcomes for students in the intervention and control groups would give an unbiased estimate of the impact on key outcomes.
4. Rich baseline data are essential for impact analysis under non-experimental designs, but are not crucial for analysis under random assignment designs.

D. OUTCOME INDICATORS AND DATA SOURCES

The outcome indicators for the impact evaluation hinge on the availability of data from administrative sources, scholarship applications and follow-up surveys. The impact evaluation will use student-level data to construct outcome indicators. Two types of outcome indicators are of interest to the stakeholders: (1) educational outcomes such as enrollment, grade completion, continuation in school, academic achievement, and middle school graduation, which will be collected from administrative records and scholarship applications; and (2) labor market outcomes such as employment, income, and continuation in post-secondary education, which would need to be collected from survey data.

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1. Educational Outcomes from Administrative Data (MINED)

As mentioned above, data for educational outcomes will come from the scholarship applications and administrative records. Assuming that the data in the application form can be merged with administrative data for middle schools, we would construct the enrollment and continuation outcomes from administrative data.

During each year of the evaluation, we will request student-level enrollment and graduation data from MINED for students in both the intervention and the control group. These data must include the student's name or some other unique identifier that allows us to match this data with the application form data. To facilitate the process, we could send MINED a finder file with students in the study and their identifying information (gleaned from the scholarship applications). MINED could use the finder file to extract the required enrollment and graduation information for each student in the evaluation.

Outcome indicators that we will build from administrative data are:

1. **Enrollment:** Starting in 2010, we can construct a student-level variable of enrollment for each grade level. A student will be considered enrolled if he or she is registered in grades 10, 11, or 12.
2. **Grade completion:** Starting in 2010, we will construct a student-level variable of grade completion for students that were enrolled in grade 10 in 2010. We will consider that a student completed grade 10, 11, or 12 if he or she was registered in that grade and completed it.
3. **Continuation in school:** Starting in 2011, we will construct a student-level variable of continuation to grades 11 and 12 for students that were enrolled in grades 10 and 11, respectively, in the previous school year. A student registered in grade 10 in 2010 will be considered continuing in school if the student is registered in grade 11 in 2011.⁷
4. **Academic achievement:** The PAES test scores will be available for students in intervention and control groups that take the test in 11th grade in 2011.

⁷ To define this outcome indicator we need data on registration for two consecutive years. For example, to define the continuation in school of 10th graders in 2010, we need data on the students registered in 10th grade in 2010, and data on students registered in 11th grade in 2011.

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5. **Middle school graduation:** By 2011, MINED data on graduation from middle school are likely to be available at the student level for middle school students in the intervention and control groups.⁸

2. Outcomes from Survey Data

As noted above, labor outcomes (wages and employment) for the intervention and control groups must be collected from an independent survey.⁹ For scholarships awarded to general middle school students enrolled in 10th grade in 2010, labor outcomes data for this cohort of students can be collected at the end of 2012, one year after they complete middle school. For scholarships awarded to technical middle school students enrolled in 10th grade in 2010, labor outcomes data for this cohort of students can be collected at the end of 2013, one year after they complete technical middle school (see Figure 1).

Outcome indicators that must be collected from survey data are:

1. **Middle school graduation:** Graduation from middle school for students who were enrolled in their last grade of middle school at the beginning of the school year. An important consideration is that the last grade of middle school will be grade 11 for those students registered in the general specialization, and grade 12 for those students that were registered in the technical specialization.¹⁰
2. **Employment:** Employment one year after students attended the last year of middle school (grade 11 for students in the general specialization, and grade 12 for students in the technical specialization).
3. **Income:** Income one year after students attended the last year of middle school (grade 11 for students in the general specialization, and grade 12 for students in the technical specialization).

⁸ This has not been confirmed by MINED, but our preliminary conversations indicated that it was possible to include data for this outcome variable in the file they will provide to us.

⁹ This survey can employ the survey instrument developed for the Encuesta de Seguimiento de Estudiantes (ESE), which collects labor outcome data. However, this survey's sample is comprised of eligible scholarship applicants, and is thus distinct from the ESE's survey sample.

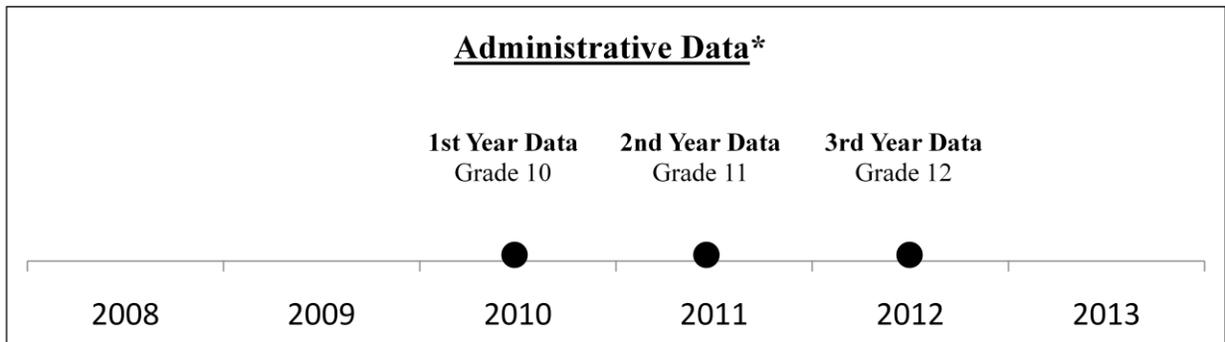
¹⁰ To ensure accuracy, evaluators should compare each student's reported graduation status with graduation status obtained from MINED's administrative data.

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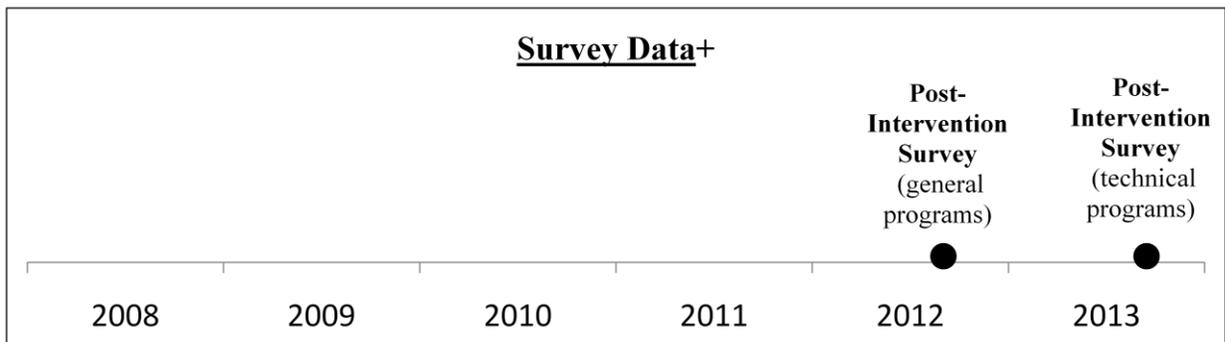
4. **Post-secondary education:** Post-secondary education one year after students attended the last year of middle school (grade 11 for students in the general specialization, and grade 12 for students in the technical specialization).

In order to be able to analyze the impact of scholarships on employment, income and post-secondary education outcomes (as well as graduation outcomes for technical middle school students), follow-up data on students’ outcomes after they leave middle school are needed. For this reason, independent surveys should be administered in late 2012 and late 2013 to track these outcomes.

FIGURE 1
 DATA COLLECTION SCHEDULE



*All administrative data will be cross-sectional, student-level data.



+Students enrolled in general programs will be interviewed in 2012; this is one year after grade 11, which is the last middle school year for general programs. Students enrolled in technical programs will be interviewed in 2013; this is one year after grade 12, which is the last middle school year for technical programs. Both post-intervention surveys are outside the scope of Mathematica’s contract.

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Table 1 provides descriptions and data sources of the outcome indicators discussed above.

TABLE 1
 DESCRIPTIONS AND DATA SOURCES OF OUTCOME INDICATORS

Outcome Indicator	Description	Data Source
Enrollment	Students' registration in grades 10, 11, or 12	MINED
Progression in school	Students' completion of grades 10, 11, or 12	MINED
Continuation in school	Students' registration in the subsequent grade (11 or 12) for students registered in grades 10 or 11	MINED
Academic achievement	Students' PAES test scores in grade 11	MINED
Middle school graduation	Students' graduation from middle school (grade 11 or 12)	MINED
Middle school graduation	Students' graduation from middle school (grade 11 or 12)	Independent Survey
Employment	Students' employment one year after they attended middle school	Independent Survey
Income	Students' income one year after they attended middle school	Independent Survey
Post-secondary education	Students' post-secondary education one year after they attended middle school	Independent Survey

E. ESTIMATING PROGRAM IMPACTS

The impact analysis will rely on a regression specification that compares outcomes of students receiving scholarships with outcomes of students not receiving scholarships, controlling for idiosyncratic differences in the two groups. The basic model can be expressed as follows:

$$(1) \quad y_{ipst} = \alpha + \beta' x_{ipst-1} + \gamma z_{st-1} + \lambda T_{ips} + \eta_{ps} + \varepsilon_{ipst}$$

where y_{ipst} is the outcome of interest for student i in educational program p in school s at time t ; x_{ipst-1} is a vector of baseline characteristics of student i in educational program p in school s ; z_{st-1} is the vector of baseline characteristics in school s ; T_{ips} is an indicator equal to one if student i in program p in school s is in the intervention group and zero if he or she is in the control group; η_{ps} is a program-school-specific indicator variable to account for the fact that randomization was done within programs and schools; and ε_{ipst} is a random error term for student i in program p in school s observed at time t . The parameter estimate for λ is the estimated impact of the program on the outcome of interest.

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We should note that the estimate is based on an intent-to-treat approach, so the estimates described above will be based on the sample that was randomized by the study. Students that drop out of school or of the scholarship program will still be treated as intervention or control, based on their randomization outcome. Students on the waiting list are not part of the research study, and thus referred to as the non-research group.

Statistical Power. Statistical power analysis is conducted to determine minimum detectable impacts (MDIs) for each relevant outcome. A MDI is the smallest program impact that a research design can measure with confidence. Our ability to detect statistically significant impacts will be influenced by factors such as: the total number of eligible students participating in the study; the rates of assignment to the intervention and control groups¹¹; the response rate attained by follow-up data collection (assumed at 80 percent); the correlation between the outcome and other available student and school characteristics (assumed at 0.40); and the variance of the outcome of interest. An equivalent measure is the minimum detectable effect size (MDE), which is the MDI measured in standard deviations of the outcome. The advantage of using MDE is that we have a common comparison for different outcomes.

Our power calculations indicate that the study is powered to detect effect sizes of around 0.20 standard deviations of the outcome, which are within the typical range of effect sizes encountered in educational studies. We assume that 751 students are randomly assigned, with 70 percent of them assigned to the intervention group and 30 percent assigned to the control group. The smallest effect on enrollment that the study is likely to detect is 0.19 standard deviations, which is equivalent to an increase of 8.4 percentage points in graduation rate assuming that the initial graduation rate is 75 percent.¹² Our experience in other educational interventions is that effect sizes near 0.20 standard deviations are common. We therefore believe that the scholarship evaluation is well powered to detect policy-relevant effects.¹³

¹¹ Although the original random assignment was done in 15 strata or clusters with different probabilities of assignment to facilitate power calculations, we use the average assignment rates within the strata as the overall assignment probabilities (70 percent assigned to intervention and 30 percent assigned to control).

¹² MCC's Monitoring and Evaluation Plan for El Salvador assumes graduation rates of 72 and 78 percent.

¹³ The calculations were done for an intent-to-treat effect. However, the statistical power of any treatment-on-the-treated analysis will be lower as a result of the low scholarship acceptance rate. For example, if the acceptance rate were to be 70 percent then the effect sizes the evaluation would be able to detect are of 0.30 standard deviations, which translate into an effect of 12 percentage points.

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F. REPORTING PLANS

In October 2010, we will provide a short memorandum summarizing the baseline findings from MINED student-level data from 2010.¹⁴ In September 2011, we will submit another short memo summarizing the findings from the first follow-up data from MINED administrative records, which correspond to student level data from 2010.¹⁵ The main focus of this document will be to estimate the impact of one year of scholarships on students' enrollment in 10th grade and 10th grade completion. In summer 2012, we will provide a second impact analysis summarizing the findings of the second follow-up data from MINED administrative records, which correspond to the student level data from 2011.¹⁶ The main focus of this report will be to estimate the impact of two years of scholarships on students' continuation in 11th grade, 11th grade completion, and middle school graduation for students in general middle school track. A third impact analysis should be scheduled for summer 2013. The focus of this report should be to estimate the impact of three years of scholarships on students' continuation in 12th grade, 12th grade completion, and middle school graduation for students on the technical middle school track, as well as the impact on labor market outcomes of students on the general middle school track that should have graduated in 2011. In the summer of 2014, a report of the labor market outcomes for students on the technical middle track that should have graduated in 2012 should be released. Table 2 provides the tentative schedule of deliverables.

¹⁴ The submission of this memo may be dependent on our ability to obtain and use data from scholarship applications. If we receive this data, the baseline analysis would explore differences in achievement, household income and enrollment among scholarship recipients (treatment) and non-recipients (control).

¹⁵ This date assumes MINED will provide us with data by May 2011; this includes 2010 data as well as complete registration information for 2011, which is necessary to construct the continuation in school indicator.

¹⁶ This date assumes MINED will provide us with data by May 2012; this includes all 2011 data as well as complete registration information for 2012, which is necessary to construct the continuation in school indicator.

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TABLE 2
SCHEDULE OF DELIVERABLES

Deliverable	Main Focus	Tentative Due Date
Baseline Analysis	Brief statistical comparison of intervention and control students at baseline	October 2010
Impact Analysis (First Follow-up)	Educational outcomes after one year (10 th grade)	Summer 2011
2nd Impact Analysis (Second Follow-up)	Educational outcomes after two years (11 th grade)	Summer 2012
3rd Impact Analysis (Third Follow-up)	Educational outcomes after two years (12 th grade) and labor market outcomes for 11 th grade of general schools students	Summer 2013
Final Impact Analysis (Fourth Follow-up)	Labor market outcomes for 12 th grade students of technical schools	Summer 2014

cc: Sabinela Alfaro (FOMILENIO), Vince Ruddy (MCC-El Salvador), Van Crowder (MCC-DC), Lorenzo Moreno, File

TABLE A.1

DEMAND FOR SCHOLARSHIPS, BY EDUCATIONAL PROGRAM AND SCHOOL

School	Department	Educational Program Name	Specialty	Eligible Applicants	Scholarships Available	Random Assignment Possible	Eligible Applicants Without Scholarships
14 De Julio De 1875	Morazán	Opción Logística de Aduana	Bto. Técnico	56	45	Yes	11
		Comercial (Contador)	Bto. Técnico	88	20	Yes	68
		Comercial (Secretariado)	Bto. Técnico	33	20	Yes	13
		Opción Mecánica Automotriz	Bto. Técnico	76	76	No	0
School Total				253	161	Yes	92
Aguilares	San Salvador	Ingeniería Civil	Bto. Técnico	101	45	Yes	56
Anamoros	La Unión	Diplomado en Transformación de Leche	Bto. General	60	45	Yes	15
Benjamín Estrada Valiente	Santa Ana	Opción Ingeniería Civil	Bto. Técnico Civil	69	50	Yes	19
		Opción Mecánica General	Bto. Técnico	28	28	No	0
		Opción Electrotecnia	Bto. Técnico	27	27	No	0
School Total				124	105	Yes	19
Carolina	San Miguel	Diplomado en Cultivos Orgánicos e Hidropónicos	Bto. General	50	43	Yes	7
Chapeltique	San Miguel	Diplomado en Agroforestería	Bto. General	74	40	Yes	34
De La Reina	Chalatenango	Diplomado en Transformación de Leche	Bto. General	22	22	No	0
Dr. Francisco Martínez Suárez	Chalatenango	Agrícola	Bto. Técnico	9	9	No	0
		Comercial (Contador)	Bto. Técnico	146	39	Yes	107
		Comercial (Secretariado)	Bto. Técnico	53	39	Yes	14
School Total				208	87	Yes	121

TABLE A.1 (continued)

School	Department	Educational Program Name	Specialty	Eligible Applicants	Scholarships Available	Random Assignment Possible	Eligible Applicants Without Scholarships
El Sauce	La Unión	Diplomado en Manejo de Desechos Orgánicos y Sólidos	Bto. Técnico Comercial	55	55	No	0
Gral. Juan Orlando Zepeda	Chalatenango	Diplomado en Promotor Comunitario	Bto. En Salud	50	50	No	0
Gral. Manuel José Arce	Morazán	Comercial	Bto. Técnico Comercial	28	28	No	0
Jutiapa	Cabañas	Diplomado en Contabilidad Financiera	Bto. Técnico Comercial	79	40	Yes	39
La Palma	Chalatenango	Diplomado en Cocina	Bto. General	17	17	No	0
		Opción en Gestión de Turismo Alternativo	Bto. Técnico	71	45	Yes	26
			School Total	88	62	Yes	26
Nueva Concepción	Chalatenango	Diplomados en Cultivos Orgánicos E Hidropónicos	Bto. General	52	52	No	0
Osicala	Morazán	Diplomado en Promotor Comunitario	Bto. Técnico Comercial	119	60	Yes	59
San Ignacio	Chalatenango	Opción en Gestión de Turismo Alternativo	Bto. Técnico	73	45	Yes	28
Sesori	San Miguel	Diplomado en Asesoría en Comercio Justo	Bto. Técnico Comercial	88	60	Yes	28
			Grand Total	1,524	1,000		524

TABLE A.2

FINAL DISTRIBUTION OF SCHOLARSHIPS, NO SCHOLARSHIPS, AND WAITING LIST IN SCHOOLS IN WHICH DEMAND OF SCHOLARSHIPS EXCEEDED THE NUMBER OF SCHOLARSHIPS AVAILABLE^a

School	Department	Educational Program Name	Number of Eligible Applicants	Number of Scholarships (Intervention group)	Number of students with no Scholarships (Control)	Number of students in the waiting list	Number of additional control students released
14 De Julio De 1875	Morazán	Comercial (Contador)	88	20	26	5	37
		Comercial (Secretariado)	33	20	8	5	0
		Opción Logística de Aduana	56	45	6	5	0
Aguilares	San Salvador	Ingeniería Civil	101	45	30	5	21
Anamoros	La Unión	Diplomado en Transformación de Leche	60	45	10	5	0
Benjamin Estrada Valiente	Santa Ana	Opción Ingeniería Civil	69	50	12	5	2
Chapeltique	San Miguel	Diplomado en Agroforestería	74	40	24	5	5
Jutiapa	Cabañas	Diplomado en Contabilidad Financiera	79	40	28	5	6
La Palma	Chalatenango	Opción en Gestión de Turismo Alternativo	71	45	18	5	3
Osicala	Morazán	Diplomado en Promotor Comunitario	119	60	33	5	21
San Ignacio	Chalatenango	Opción en Gestión de Turismo Alternativo	73	45	20	5	3
Sesori	San Miguel	Diplomado en Asesoría en Comercio Justo	88	60	21	5	2
Total			911	515	236	60	100

^a Dr. Francisco Martínez Suárez and Carolina do not appear in this table due to their exclusion from the study in February 2010. Therefore, the number of eligible applicants does not match with the number reported in Table 1A.