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Impact Evaluation of Social Funds

An Impact Evaluation of Education, Health, and Water Supply Investments by the Bolivian Social Investment Fund

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This article reviews the results of an impact evaluation of small-scale rural infrastructure projects in health, water, and education financed by the Bolivian Social Investment Fund. The impact evaluation used panel data on project beneficiaries and control or comparison groups and applied several evaluation methodologies. An experimental design based on randomization of the offer to participate in a social fund project was successful in estimating impact when combined with bounds estimates to address noncompliance issues. Propensity score matching was applied to baseline data to reduce observable preprogram differences between treatment and comparison groups. Results for education projects suggest that although they improved school infrastructure, they had little impact on education outcomes. In contrast, interventions in health clinics, perhaps because they went beyond simply improving infrastructure, raised utilization rates and were associated with substantial declines in under-age-five mortality. Investments in small community water systems had no major impact on water quality until combined with community-level training, though they did increase the access to and the quantity of water. This increase in quantity appears to have been sufficient to generate declines in under-age-five mortality similar in size to those associated with the health interventions.

This article provides an overview of the results of an impact evaluation study of the Bolivian Social Investment Fund (SIF) and the methodological choices and

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constraints in designing and implementing the evaluation. The study used each of the main evaluation designs generally applied to estimate the impact of projects.[1] These include an experimental design applied to assess the impact of education projects in Chaco, a poor rural region of Bolivia, where eligibility for a project financed by the social fund was randomly assigned to communities.[2] Through the results from the randomization of eligibility in this case and those from statistical matching procedures using propensity scores in others, this article contributes to the body of empirical evidence on the effectiveness of improving infrastructure quality in education (Hanushek 1995, Kremer 1995), health (Alderman and Lavy 1996, Lavy and others 1996, Mwabu and others 1993), and drinking water (Brockerhoff and Derose 1996, Lee and others 1997).

The main conclusions of the study are as follows. Although the social fund improved the quality of school infrastructure (measured some three years after the intervention), it had little effect on education outcomes. In contrast, the social fund's interventions in health clinics, perhaps because they went beyond simply improving the physical infrastructure, raised utilization rates and were associated with substantial declines in under-age-five mortality. Its investments in small community water systems had no major effect on the quality of the water but did increase the access to and the quantity of water. This increase in quantity appears to have been sufficient to generate declines in under-age-five mortality similar in size to those associated with the health interventions. How the study came to these conclusions is the subject of this article.

I. The Bolivian SIF

Bolivia introduced the first social investment fund when it established the Emergency Social Fund in 1986. Program staff and international donors soon recognized the potential of the social fund as a channel for social investments in rural areas of Bolivia and as an international model for community-led development. In 1991 a permanent institution, the SIF, was created to replace the Emergency Social Fund, and the social fund began concentrating on delivering social infrastructure to historically underserved areas, moving away from emergency-driven employment-generation projects.

The Bolivian social fund proved that social funds could operate to scale, bringing small infrastructure investments to vast areas of rural Bolivia that line ministries had been unable to reach because of their weak capacity to execute projects.

1. Impact evaluations of World Bank–financed projects continue to be rare even where knowledge about development outcomes is at a premium, such as in new initiatives about which little is known or in projects with large sums of money at stake. A recent study by Subbarao and others (1999) found that only 5.4 percent of all World Bank projects in fiscal year 1998 included elements necessary for a solid impact evaluation: outcome indicators, baseline data, and a comparison group.

2. In the evaluation literature the random assignment of potential beneficiaries to treatment and control groups is widely considered to be the most robust evaluation design because the assignment process itself ensures comparability (Grossman 1994, Holland 1986, Newman and others 1994).

Providing financing to communities rather than implementing projects itself, the social fund introduced a new way of doing business that rapidly absorbed a large share of public investment. Between 1994 and 1998 (roughly the period between the baseline and the follow-up of the impact evaluation study) the SIF disbursed more than US$160 million, primarily for projects in education ($82 million), health ($23 million), and water and sanitation ($47 million).

The World Bank project that helped finance the SIF built in an impact evaluation at the outset. The design for the evaluation was developed in 1992; baseline data were collected in 1993. The Bolivian social fund is the only one for which there are both baseline and follow-up data and an experimental evaluation design, adding robustness to the results not found in other impact evaluations.[3]

II. Evaluation Design

Impact evaluations seek to establish whether a particular intervention (in this case a SIF investment) changes outcomes in the beneficiary population. The central issue for all impact evaluations is establishing what would have happened to the beneficiaries had they not received the intervention. Because this counterfactual state is never actually observed, comparison or control groups are used as a proxy for the state of the beneficiaries in the absence of the intervention. Several evaluation designs and statistical procedures have been developed to obtain the counterfactual, most of which were used in this evaluation. The average difference between the observed outcome for the beneficiary population and the counterfactual outcome is called the average treatment effect for the treated. This effect is the focus of this evaluation study and most others.

The evaluation used different methodologies for different types of projects (education, health, and water) in two regions, the Chaco region and the Resto Rural—an amalgamation of rural areas (table 1). The design of the SIF projects motivated the original choice of evaluation designs applied when setting up the treatment and control or comparison groups during the sample design and baseline data-collection phase. Similarly, changes in the way projects were implemented affected the choice of evaluation methodologies applied in the impact assessment stage.

Education: Random Assignment of Eligibility and Matched Comparison

The education case shows how two different evaluation designs were applied in the two regions: random assignment of eligibility in the Chaco region and matched comparison in the Resto Rural. The choice of evaluation design in each region was conditioned by resource constraints and the timing of the evaluation relative to the SIF investment decisions.

3. The impact evaluation cost about $880,000, equal to 1.4 percent of the World Bank credit to help finance the SIF and 0.5 percent of the amount disbursed by the SIF between 1994 and 1998.

TABLE 1. Evaluation Designs by Type of Project and Region

	Education		Health	Water
	Chaco	Chaco and Resto Rural combined	Chaco and Resto Rural combined	Chaco and Resto Rural combined
Original evaluation design	Random assignment of eligibility	Matched comparison	Reflexive comparison	Matched comparison
Final evaluation design	Random assignment of eligibility	Matched comparison	Matched comparison	Matched comparison
Final control or comparison group	Nonbeneficiaries randomized out of eligibility for receiving project promotion	Nonbeneficiaries matched on observable 1992 characteristics before the baseline; further statistical matching on baseline characteristics	Nonbeneficiaries statistically matched on baseline characteristics, after determining which clinics did not receive intervention	Nonbeneficiaries from health subsample
Impact analysis methodology[a]	Bounds on treatment effect derived from randomly assigned eligibility	Difference in differences on matched comparisons	Difference in differences on matched comparisons	Difference in differences on matched comparisons

[a]Estimations are of the average effects of the SIF interventions on community means, often assessed by aggregating household data.

Random Assignment of Eligibility. In 1991 the German Institute for Reconstruction and Development earmarked funding for education interventions in Chaco. But the process for promoting SIF interventions in selected communities had not been initiated, and funding was insufficient to reach all schools in the region. This situation provided an opportunity to assess schools' needs and use a random selection process to determine which of a group of communities with equally eligible schools would receive active promotion of a SIF intervention.

To determine which communities would be eligible for active promotion, the SIF used a school quality index.[4] Only schools with an index below a particular value were considered for SIF interventions, and the worst off were automatically designated for active promotion of SIF education investments.[5] A total of 200 schools were included in the randomization, of which 86 were randomly assigned to be eligible for the intervention. Although not all eligible communities selected for active promotion ended up receiving a SIF education project, and though a few schools originally classified as ineligible did receive a SIF intervention, the randomization of eligibility was sufficient to measure all the impact indicators of interest.

Matched Comparison. In the Resto Rural schools had already been selected for SIF interventions, precluding randomization. Nonetheless, it was possible to collect baseline data from both the treatment group and a similar comparison group constructed in 1993 during the evaluation design and sample selection stage.

In the original evaluation design applied to education projects in the Resto Rural, treatment schools were randomly sampled from the list of all schools designated for SIF interventions. A comparison group of non-SIF schools was then constructed using a two-step matching process based on observable characteristics of communities (from a recent census) and schools (from administrative data). First, using the 1992 census, the study matched the cantons in which the treatment schools were located to cantons that were similar in population (size, age distribution, and gender composition), education level, infant mortality rate, language, and literacy rate. Second, it selected comparison schools from those cantons to match the treatment schools using the same school quality index applied in the Chaco region.

Once follow-up data were collected and the impact analysis conducted, the study refined the matching, using observed characteristics from the baseline preintervention data. It matched treatment group observations to comparison

4. This index for the Chaco region assigned each school a score from 0 to 9 based on the sum of five indicators of school infrastructure and equipment: electric lights (1 if present, 0 if not), sewage system (2 if present, 0 if not), a water source (4 if present, 0 if not), at least one desk per student (1 if so, 0 if not), and at least 1.05 m^2 of space per student (1 if so, 0 if not). Schools were ranked according to this index, with a higher value reflecting more resources.

5. Because the worst-off and best-off schools were excluded from the randomization and the sample, the study's findings on the impacts of the SIF cannot be generalized to all schools.

group observations on the basis of a constructed propensity score that estimates the probability of receiving an intervention.[6] Following the approach set forth in Dehejia and Wahba (1999), the study matched the observations with replacement, meaning that one comparison group observation can be matched to more than one treatment group observation. This matching was based on variables measured in the treatment and comparison groups before the intervention. Preintervention outcome variables as well as other variables that affect outcomes in the propensity score were included.

In effect, the matching produced a reweighting of the original comparison group so as to more closely match the distribution of the treatment group before the intervention. These weights were then applied to the postintervention data to provide an estimate of the counterfactual—what the value in the treatment schools would have been in the absence of the intervention. The ability to match on preintervention values is one of the main advantages of having baseline data. This analysis combined Chaco and Resto Rural data to yield a larger sample.

Finally, the results were presented using a difference-in-difference estimator, which assumes that any remaining preintervention differences between the treatment schools and the (reweighted) comparison group schools would have remained constant over time if the SIF had not intervened. Thus the selection effect was corrected for in three rounds: first by constructing a match in the design stage, then by using propensity score matching, and finally by using a difference-in-difference estimator.

Health: Reflexive Comparison and Matched Comparison

The health case demonstrates how an evaluation design can evolve between the baseline and follow-up stages when interventions are not implemented as planned. It also underscores the value of flexibility and relatively large samples in impact evaluations.

A reflexive comparison evaluation design based solely on before and after measures was originally developed for assessing SIF-financed health projects. This type of evaluation design involves comparing values for a population at an earlier period with values observed for the same population in a later period. It is considered one of the least methodologically rigorous evaluation methods because isolating the impact of an intervention from the impact of other influences on observed outcomes is difficult without a comparison or control group that does not receive the intervention (Grossman 1994). The original evaluation design was chosen in the expectation that the SIF would invest in all the rural health clinics in the Chaco and Resto Rural.

At the time of the follow-up survey German financing had enabled the SIF to carry out most of its planned health investments in the Chaco region, but financial constraints had prevented it from investing in all the health centers in the

6. See Baker (2000) for a description of propensity score matching.

Resto Rural. This change in implementation allowed the application of a new evaluation design—matched comparison. The question remained, however, whether the SIF interventions had been assigned to health centers on the basis of observed variables and time-constant unobserved variables or on the basis of unobservable variables that changed between the baseline and follow-up surveys. In discussions with SIF management in 1999 it proved impossible to identify the criteria used to select which health centers that would receive the interventions.

An examination of the baseline data revealed significant differences in characteristics between health centers that received the interventions and those that did not. To adjust for these differences, a propensity-matching procedure similar to that used with the education data in the Resto Rural was carried out. The difference between the distribution of the propensity scores in the treatment and comparison groups before and after the matching narrowed considerably, pointing to the effectiveness of the propensity-score-matching method in eliminating observable differences between the treatment and comparison groups.

Once the propensity score matching was applied to the baseline data, a difference-in-difference estimation was performed to assess the impact of the SIF-financed health center investments in rural areas. As will be discussed in the section on results, a series of additional tests were also applied to confirm the robustness of the results on infant mortality.

Water Supply: Matched Comparison

The water case illustrates how impact evaluation estimates for a particular type of intervention can be generated by taking advantage of data from a larger evaluation. At the time of the baseline survey, 18 water projects were planned for the Chaco and Resto Rural. These projects consisted of water supply investments designed to benefit all households within each intervention area. Project sites were selected on the basis of two criteria: whether a water source was available and whether the beneficiary population would be concentrated enough to allow economies of scale.

No specific comparison group was constructed ex ante. Instead, it was expected that the comparison group could be constructed from the health subsample using a matched comparison technique to identify similar nonbeneficiaries. At the follow-up data collection and analysis stage it was determined that all 18 projects had been carried out as planned and that there were sufficient data from which to construct a comparison group using the health sample, as originally expected. Thus the water case is the only one of the three in which the evaluation design did not change between the baseline and follow-up stages of the evaluation.

III. Results in Education

SIF-financed education projects either repaired existing schools or constructed new ones and usually also provided new desks, blackboards, and playgrounds. In many cases new schools were constructed in the same location as the old

schools, which were then used for storage or in some cases adapted to provide housing for teachers.

Schools that received a SIF intervention benefited from significant improvements in infrastructure (the condition of classrooms and an increase in classroom space per student) and in the availability of bathrooms compared with schools that did not receive a SIF intervention. They also had an increase in textbooks per student and a reduction in the student-teacher ratio.[7] But the improvements had little effect on enrollment, attendance, or academic achievement. Among student-level outcomes, only the dropout rate reflects any significant impact from the education investments.

Estimates Based on Randomization of Eligibility

The evaluation for the Chaco region was able to take advantage of the randomization of active promotion across eligible communities to arrive at reliable estimates of the average impact of the intervention (table 2). Because of the demand-driven nature of the SIF, not all communities selected for active promotion applied for and received a SIF-financed education project. This does not represent a departure from the original evaluation design, and randomization of eligibility (rather than the intervention) is sufficient to estimate all the impacts of interest (see appendix A).

But the fact that some communities not selected for active promotion nevertheless applied for and received a SIF-financed education project does represent a departure from the original evaluation design. This noncompliance in the control group (as it is known in the evaluation literature) can be handled by calculating lower and upper bounds for the estimated effects.[8] Thus the cost of the noncompliance is a loss of precision in the impact estimate as compared with a case in which there is full compliance. In the case considered here, the differences between the lower and upper bounds of the estimates are typically small and the results are still useful for policy purposes (see table 2 for these bounds estimates and appendix A for an explanation).

Estimates Based on Matched Comparison

In the Resto Rural schools had already been selected for the SIF interventions and no randomization of eligibility took place, making it impossible to apply an

7. For all education and health results the Wilcoxon-Mann-Whitney nonparametric test was used to detect departures from the null hypothesis that the treatment and comparison cases came from the same distribution. The alternative hypothesis is that one distribution is shifted relative to the other by an unknown shift parameter. The *p*-values are exact and are derived by permuting the observed data to obtain the true distribution of the test statistic and then comparing what was actually observed with what might have been observed. In contrast, asymptotic *p*-values are obtained by evaluating the tail area of the limiting distribution. The software used for the exact nonparametric inference is StatXact 4 (http://www.cytel.com). Although the exact tests take account of potentially small sample bias, in practice there were no major differences between the exact and asymptotic *p*-values.

8. This approach of working with bounds follows in the spirit of Manski (1995).

TABLE 2. Average Impact of SIF Education Investments in Chaco, with Estimation Based on Randomization of Eligibility

Indicator[a]	Mean for all schools, 1993	Impact of intervention, 1997			
		Lower bound	*p*-value	Upper bound	*p*-value
School-level outcomes					
Blackboards	0.35	1.46	0.17	1.79	0.08**
Blackboards per classroom	0.08	0.40	0.03*	0.43	0.02*
Desks	33.32	9.20	0.70	29.44	0.11
Desks per student	0.52	0.57	0.15	0.65	0.10**
Classrooms in good condition	0.37	1.01	0.42	1.98	0.06**
Fraction of classrooms in good condition	0.11	0.34	0.07**	0.41	0.02*
Teachers' tables	0.42	1.12	0.31	1.67	0.11
Teachers' tables per classroom	0.18	0.54	0.00*	0.59	0.00*
Fraction of schools with sanitation facilities	0.39	0.47	0.02*	0.58	0.00*
Fraction of schools with electricity	0.06	–0.05	0.75	–0.07	0.69
Fraction of teachers with professional degrees	0.46	–0.09	0.65	–0.10	0.63
Textbooks	17.47	–25.72	0.64	1.79	0.97
Textbooks per student	0.32	0.41	0.87	0.05	0.98
Students per classroom	22.93	2.12	0.68	0.47	0.93
Students' education outcomes					
Repetition rate (percent)	12.65	–1.75	0.61	–5.45	0.17
Dropout rate based on household data (percent)	9.49	–3.90	0.26	–6.00	0.08**
Dropout rate based on administrative data (percent)	10.73	3.01	0.53	3.17	0.50*
Enrollment ratio (ages 5–12)	0.83	0.15	0.14	0.05	0.63
Fraction of days of school attended in past week	0.93	–0.02	0.38	–0.07	0.11

*Significant at the 5 percent level.

**Significant at the 10 percent level.

[a]In 1997 (but not in 1993) achievement tests in language and mathematics were administered to the treatment and control schools. No significant differences were found.

Source: SIF Evaluation Surveys

experimental design and calculate impact in the same way as in the Chaco region. Instead, a matching procedure based on propensity scores was used, as described in the section on evaluation design. This analysis combined the Chaco and Resto Rural samples. The first-stage probit estimations used to calculate the propensity scores employed only values for 1993, before the intervention, to ensure preintervention comparability between the treatment and comparison groups.

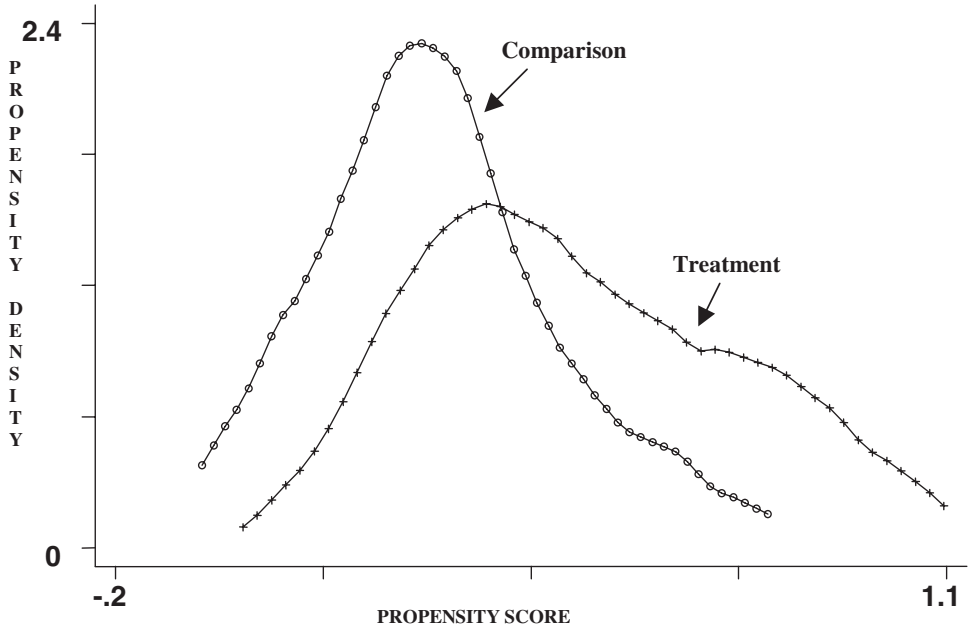
The kernel density estimates of the propensity scores for the treatment and comparison groups before propensity score matching indicate that differences

remained between the groups before the intervention took place (figure 1). The kernel density estimates of the propensity scores after matching, however, show that propensity matching does a relatively good job of eliminating preprogram differences between SIF and non-SIF schools (figure 2).

Even so, there is a range where the propensity scores do not overlap. In this range observations in the treatment group have propensity scores exceeding the highest values in the comparison group. For this group of treatment observations no comparable comparison group is available. The group consists of only five observations, however, and can be taken into account by setting bounds on the possible counterfactual values for these five. In practice, for each treatment school that cannot be matched to a comparison school, a comparison is constructed by matching the school with itself. That is, the comparison is an exact replica but with the intervention dummy variable set to 0. This is equivalent to assuming that for these schools the intervention has no effect. (For a discussion of the upper bound, see appendix A.)

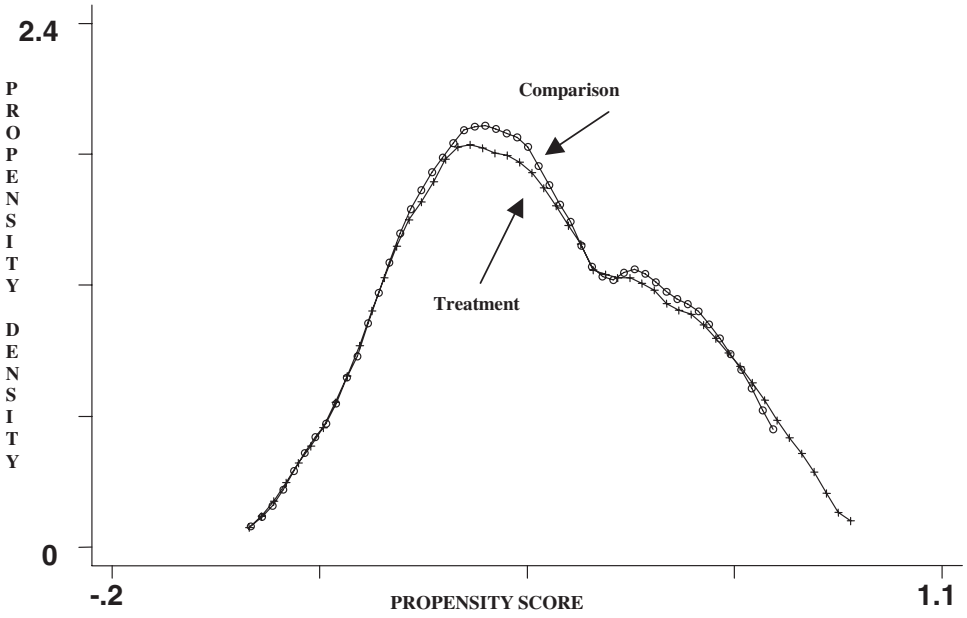
The results of a difference-in-difference estimation (intertemporal change in the treatment group minus intertemporal change in the comparison group) before and after the propensity score matching are not dramatically different from those based on randomization of eligibility (table 3). This indicates that the matching in the evaluation design stage, before the statistical propensity score matching, was rela-

FIGURE 1. Kernel Density Estimates of Treatment and Comparison Schools' Propensity Scores Before Matching



Source: Authors' calculations.

FIGURE 2. Kernel Density Estimates of Propensity Scores for Treatment and (Reweighted) Comparison Schools After Matching



Source: Authors' calculations.

tively effective. Only for a couple of variables were there preprogram differences, and these were eliminated with the propensity score matching.

The ability to eliminate the preintervention differences in means between treatment and comparison groups after matching increases confidence in the evaluation results, although it is by no means a guarantee that the estimates are unbiased. But the matching procedure did remove observable differences between treatment and comparison groups, and the difference-in-difference estimation also removed the time-constant unobservable differences. In presenting the impact estimates, one has to assume that the matching has also eliminated the preintervention differences in time-varying unobservable variables that affect outcomes.

Although initial differences in unobservable characteristics cannot be examined, baseline data make it possible to check whether differences in observable characteristics between the treatment and comparison groups have been addressed. Baseline data also make it possible to use difference-in-difference estimates to eliminate the effect of time-constant unobservables in estimating program impact. Most evaluations that have only postintervention data on beneficiaries and nonbeneficiaries rely on some type of statistical matching procedure to try to generate appropriate comparison groups for those receiving the intervention (Rosenbaum and Rubin 1983, Heckman and others 1998, Angrist and Krueger 1999).

TABLE 3. Difference-in-Difference Estimates of Average Impact of SIF Education Investments in Chaco and Resto Rural
(intertemporal change in the treatment group minus intertemporal change in the comparison group)

	Before matching differences			After matching differences		
Indicator	Treatment group	Comparison group	*p*-value	Treatment group	Comparison group	*p*-value
School-level outcomes						
Fraction of schools with electricity	0.152	0.127	0.70	0.152	0.159	0.93
Fraction of schools with sanitation facilities	0.347	0.082	0.032*	0.341	–0.048	0.016*
Textbooks per student	3.78	3.05	0.219	3.78	1.97	0.027*
Square meters per student	1.87	0.47	0.004*	1.87	0.448	0.002*
Students per classroom	–7.53	1.22	0.006*	–7.53	3.01	0.002*
Fraction of classrooms in good condition	0.365	0.064	0.005*	0.365	0.019	0.015*
Students per desk	–1.30	–0.72	0.97	–1.30	0.30	0.74
Students per teacher	–5.05	–1.17	0.176	–5.05	–0.136	0.048*
Students' education outcomes						
Dropout rate	–0.028	0.006	0.010*	–0.028	–0.003	0.045*
Number of registered students per school	6.4	18.27	0.68	6.4	42.6	0.038*
Number of students attending classes regularly per school	8.76	17.2	0.68	8.76	3.84	0.042*
Number of students repeating classes	–2.36	1.09	0.417	–2.39	38.8	0.40

*Significant at the 5 percent level.
**Significant at the 10 percent level.
Source: Authors' calculations.

IV. RESULTS IN HEALTH

SIF-financed health projects repaired existing health centers and constructed new ones. The SIF worked with prototype designs that included a waiting room, a room for outpatient consultations, a room with several beds for inpatients, a space for a pharmacy, bathrooms, and a meeting room for presentations on health topics. The SIF also provided health centers with medicines, furniture, and medical equipment; a motorcycle to allow health personnel to conduct more home visits; and a radio to call for ambulances and to keep in contact with other health centers. Where centers lacked electricity, the SIF provided solar panels to power lights, a radio, and a refrigerator for storing medicines and vaccines. Finally, it made drinking water available and typically installed showers.

As explained, the SIF originally intended to make investments in all health clinics in the sample but was unable to do so mainly because of financial constraints. Thus by the time of the follow-up survey some clinics had received an interven-

tion and some had not. Thanks to the financing from the German bilateral aid agency, most clinics in the Chaco region received an intervention. Fewer did in the Resto Rural sample.

Kernel density estimates of the propensity scores for the treatment and comparison groups before matching reveal considerably greater differences than was the case for education (figure 3). This may reflect the inability to construct a comparison group before the intervention owing to the initial plans to reach all health clinics. Despite the initial differences, the matching procedure managed to eliminate virtually all the observable preprogram differences in the reported variables (figure 4).

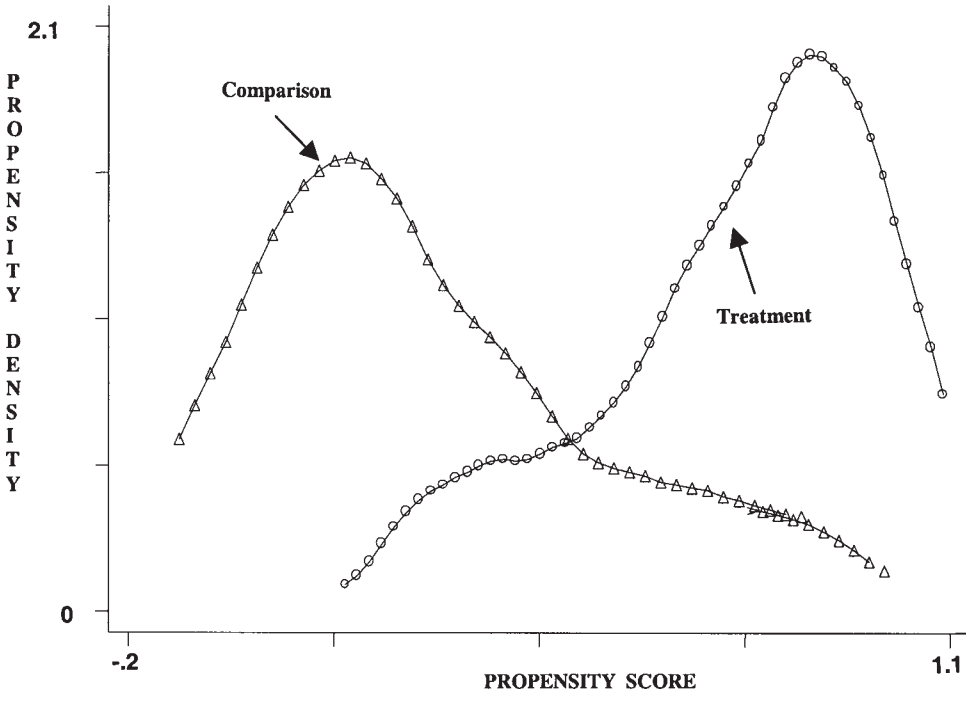
Infrastructure and Utilization Estimates

The SIF investments in health centers brought about significant improvements in their physical characteristics and in their utilization. Both the share of women's prenatal care and the share of births attended—two important factors affecting under-age-five mortality—increased significantly (table 4).

Under-Age-Five Mortality Estimates

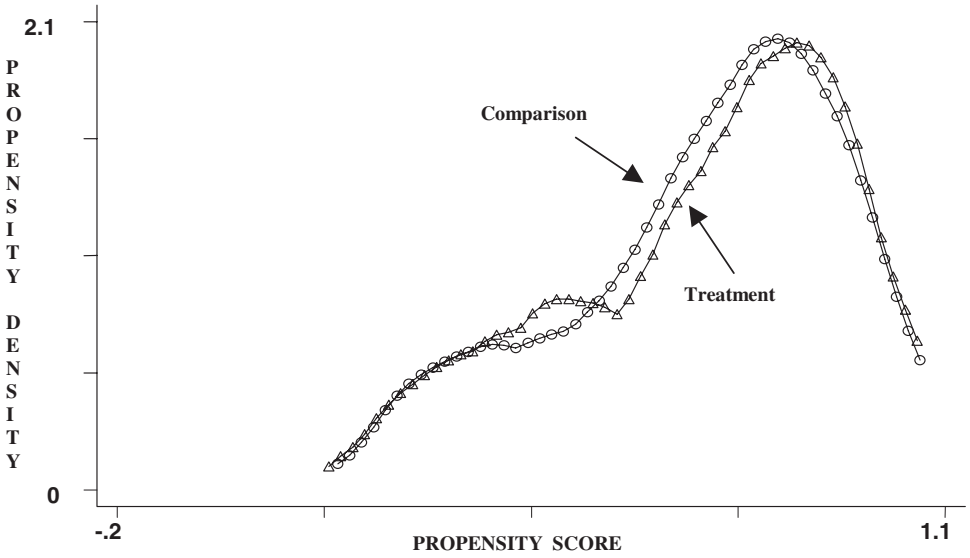
The impact evaluation drew on sufficiently large samples in the household surveys to allow assessment of the impact of SIF-financed investments in health

FIGURE 3. Kernel Density Estimates of Propensity Scores for Treatment and Comparison Health Clinics Before Matching



Source: Authors' calculations.

FIGURE 4. Kernel Density Estimates of Propensity Scores of Treatment and (Reweighted) Comparison Health Clinics After Matching



Source: Authors' calculations.

centers on under-age-five mortality. Using three different methods to assess this impact, the evaluation found consistent evidence of a significant reduction in under-age-five mortality in the areas served by health clinics receiving a SIF intervention.

The first method, using propensity score matching, uses recall data from the household surveys on deaths among children born 10 years before the survey. The results before propensity score matching show that the proportion of children dying was significantly higher in the treatment group than in the comparison group before the intervention, but significantly lower in the treatment group after the intervention (table 5). When matching, the study used the same procedure (and the same implicit weights) as it did when analyzing the effect of SIF investments on the infrastructure and utilization of health clinics. Just as with the variables for physical characteristics and utilization, the matching eliminates the preintervention differences. The postintervention differences remain, however: under-age-five mortality is lower in the treatment group.

The second method draws on life table estimates for the change in mortality using only the households for which survey data are available for both 1993 and 1997. For this reason the sample is smaller and no matching was done. The under-age-five mortality rates in this sample, covering the period 1988–93, are close to the rates reported in the 1994 National Demographic and Health Survey for the period 1989–94.

TABLE 4. Difference-in-Difference Estimates of Average Impact of SIF Health Investments in Chaco and Resto Rural
(intertemporal change in the treatment group minus intertemporal change in the comparison group)

	Before matching differences			After matching differences		
Indicator	Treatment group	Comparison group	*p*-value	Treatment group	Comparison group	*p*-value
Health clinic characteristics						
Number of beds	1.400	0.125	0.00*	1.39	0.71	0.003*
Fraction of clinics with electricity	0.077	0.050	0.81	0.078	0.098	0.89
Fraction of clinics with sanitation facilities	0.404	0.125	0.66	0.392	0.176	0.042*
Fraction of clinics with water	0.078	–0.025	0.58	0.08	0	0.64
Number of patient rooms	0.346	–0.205	0.07**	0.33	–0.54	0.00*
Index of availability of medical equipment in good condition***	0.252	0.109	0.24	0.25	0.22	0.40
Index of availability of medical supplies***	0.332	0.080	0.02*	0.33	0.07	0.00*
Intermediate health outcomes						
Use of public health service (unconditional)	0.002	–0.001	0.18	0.002	0.002	0.60
Use of public health service (conditional on illness)	0.011	–0.006	0.96	0.011	0.010	0.49
Fraction of women receiving any prenatal care	0.191	0.073	0.068**	0.207	0.007	0.001*
Fraction of births attended by trained personnel	0.068	0.020	0.60	0.063	0.050	0.58
Fraction of cases of diarrhea treated	0.006	0.069	0.92	0.006	–0.138	0.23
Fraction of cases of cough treated	0.030	0.053	0.18	0.031	0.133	0.08**
Health outcomes						
Incidence of diarrhea	–0.030	–0.079	0.17	–0.029	–0.013	0.84
Incidence of cough	–0.147	–0.089	0.64	–0.152	–0.178	0.34

*Significant at the 5 percent level.
**Significant at the 10 percent level.
***The index is calculated as the fraction of supplies that were found in a site inspection, relative to the norms for supplies specified by the Ministry of Health.
Source: Authors' calculations.

TABLE 5. Deaths among Children under Age Five among Children Born in Previous 10 Years in Chaco and Resto Rural, 1993 and 1997

Indicator	1993		1997	
	Treatment group	Comparison group	Treatment group	Comparison group
Before matching				
Percentage of children dying	10.6	8.4	6.1	9.8
	(292)	(122)	(134)	(120)
Percentage of children surviving	89.4	91.6	93.9	90.2
	(2,469)	(1,322)	(2,068)	(1,107)
Difference between comparison and treatment groups in percentage of children dying	–2.1 [0.076]**		3.7 [0.023]*	
After matching				
Percentage of children dying	10.3	10.2	6.0	10.7
	(237)	(182)	(110)	(149)
Percentage of children surviving	89.7	89.8	94.0	89.3
	(2,057)	(1,595)	(1,723)	(1,242)
Difference between comparison and treatment groups in percentage of children dying	–0.08 [0.96]**		4.7 [0.07]*	

*Significant at the 5 percent level.
**Significant at the 10 percent level.
Note: Figures in parentheses are number of deaths and survivors. Figures in square brackets are *p*-values. Results corrected for cluster sampling.
Source: Authors' calculations.

Again, the results show a significant reduction in mortality in the treatment group from 1993 to 1997 (table 6). In the comparison group mortality does not decline and, if anything, increases.

The third approach to measuring the change in mortality is based on estimations of a Cox proportional hazard function. The sample is first divided into a group of clinics that received a SIF intervention and a comparison group matched according to the propensity score, which takes into account characteristics of the health facility, the community and health outcomes, and characteristics of the households in the service area (see appendix C). Data on individual households residing in the service area of the two groups of clinics are used to estimate a hazard function and, based on the estimated hazard, an under-age-five mortality rate. The hazard function is written as

$$\lambda(\text{time}; X_j, i_j) = \lambda(\text{time})\exp(X_j\beta + \theta i_j) \quad (1)$$

where X is a vector of characteristics of child j and i denotes whether or not the clinic in the area received an intervention. The advantage of using a hazard model is that it allows one to easily deal with right censoring and thus to estimate an under-age-five mortality rate.

TABLE 6. Life Table Estimates of Infant and Under-Age-Five Mortality Rates in Chaco and Resto Rural, 1993 and 1997

	1993		1997	
	Treatment group	Comparison group	Treatment group	Comparison group
Infant mortality rate (per 1,000 live births)	61.5	59.8	30.8	67.2
Under-five mortality rate (per 1,000)	94.0	92.6	54.6	107.9
Number of observations	838	822	620	596
Cumulative failure at month				
0	0.029	0.027	0.016	0.032
1	0.038	0.038	0.020	0.044
3	0.050	0.050	0.025	0.053
6	0.062	0.061	0.031	0.067
12	0.072	0.074	0.040	0.081
24	0.091	0.090	0.055	0.107
60	0.091	0.090	0.055	0.107
Likelihood ratio test for homogeneity	0.007		10.04	
Chi2(1)	[0.932]		[0.002]*	

*Significant at the 5 percent level.
Note: Figures in square brackets are *p*-values.
Source: Authors' calculations.

The estimated coefficients of β and θ in table 7 represent results after matching, using the procedure described. Per capita consumption, age of mother at child's birth, and education of mother are expressed as deviations from the mean, with values of 2,600 (bolivianos), 27 (years), and 3 (years), respectively. The reported under-age-five mortality rates are derived from the estimated survival function evaluated at the mean values of *X*.

The results again show no significant differences in 1993 between the treatment and comparison groups (the intervention variable is not significant), but significantly lower under-age-five mortality in the treatment group after the intervention. The impact can be derived by using the differences in predicted under-age-five mortality rates with and without the intervention between the two years. Selection bias is addressed by using difference in differences.

Thus all three of the approaches show a similar pattern of declining under-age-five mortality in the treatment group receiving a SIF-financed health investment and no decline in the comparison group. The Cox proportional hazard estimates, the most accurate, show a decline in under-age-five mortality from 88.5 deaths per 1,000 to 65.8 among children living in the service area of a health center that received a SIF investment.

What are some possible explanations for the finding of lower mortality in the treatment group? One is that the treatment group might have received interventions not provided by the SIF that could have led to lower mortality, such as in water and sanitation.

TABLE 7. Cox Proportional Hazard Estimates of Under-Five Mortality in Chaco and Resto Rural, 1993 and 1997

	1993			1997		
Variable	Coefficient	Standard error	*p*-value	Coefficient	Standard error	*p*-value
Duration (year of birth–1992)	–.029	0.025	0.259	–.039	0.033	0.24
Intervention dummy variable (= 1 if living in area of influence of health clinic with intervention)	–.009	0.195	0.96	–.55	0.28	0.05*
Per capita household consumption	–.000012	0.00001	0.36	1.45e–07	4.40e–06	0.97
Age of mother at child's birth	.029	0.027	0.28	–.0007	0.01	0.95
Education of mother	.022	0.047	0.65	–.011	0.038	0.74
Number of observations	3,881			3,107		
Wald Chi2(5)	5.16			8.06		
Prob > Chi2	0.40			0.153		
Estimated under-age-five mortality rate (per 1,000)						
Treatment group	88.5			65.8		
Comparison group	89.3			111		

*Significant at the 5 percent level.
Source: Authors' calculations.

Between the baseline and follow-up surveys the comparison group received more non-SIF water interventions than the treatment group, though there was no significant difference in the non-SIF sanitation projects received (table 8). Although not reported here, regressions of the difference between 1997 and 1993 in availability of piped water, adequacy of water throughout the day and year, distance to water supply, and adequacy of sanitation facilities on the intervention dummy variable also revealed no significant differences between the treatment and comparison groups.

If the reduction in under-age-five mortality had something to do with the services provided in the clinics, greater reductions in mortality would be expected among those who used the clinics than among those who did not. Data show that under-age-five mortality among families in which the mother received at least one prenatal checkup before the last birth was significantly lower in the treatment group than in the comparison group in 1997 but not in 1993 (table 9). This result strongly suggests that something associated with the health clinic after the intervention accounts for the lower mortality observed.

V. Results in Water Supply

SIF water supply investments provided financing for small-scale potable water systems whose design varied depending on the geographic location. Initially, the investments in infrastructure were not accompanied by adequate training. But in later years greater effort was made to provide training through the World Bank–financed Rural Water and Sanitation Project (Prosabar).

Data from before and after the SIF water supply investments in Chaco and the Resto Rural show that the main changes were a reduction in the distance to

Table 8. Non-SIF Water and Sanitation Projects Benefiting Treatment and Comparison Groups in Chaco and Resto Rural, 1993–97

	Treatment group	Comparison group
Non- SIF water projects		
Percent of households who benefited from water projects not financed by the SIF	14.5 (656)	32.7 (457)
Percent of households who did not benefit	85.5 (3,863)	67.3 (941)
Design-based *F*	3.28 [0.073]	
Non-sif sanitation projects		
Percent of households who benefited from sanitation projects not financed by the SIF	8.5 (384)	6.2 (87)
Percent of households who did not benefit	91.5 (4,135)	93.8 (1,311)
Design-based *F*	0.144 [0.705]	

Note: Figures in parentheses are number of observations. Figures in square brackets are *p*-values. Results adjusted for cluster sampling.

Source: Authors' calculations.

TABLE 9. Deaths in Previous Five Years among Children under Age Five in Families with and without Prenatal Checkups in Chaco and Resto Rural, 1993 and 1997
(percent)

	1993		1997	
	Treatment group	Comparison group	Treatment group	Comparison group
At least one prenatal checkup before last birth				
Percentage of children dying	8.4	8.2	4.8	9.6
	(57)	(23)	(37)	(31)
Percentage of children surviving	91.6	91.8	95.2	90.4
	(620)	(258)	(728)	(293)
Design-based *F*	0.015		7.40	
	[0.90]		[0.01]*	
No prenatal checkup before last birth				
Percentage of children dying	7.8	7.7	9.6	8.4
	(62)	(39)	(31)	(31)
Percentage of children surviving	92.2	92.3	90.4	91.6
	(732)	(467)	(293)	(338)
Design-based *F*	0.003		0.267	
	[0.95]		[0.61]	

*Significant at the 5 percent level.

Note: Figures in parentheses are number of deaths and survivors. Figures in square brackets are *p*-values. Results corrected for cluster sampling.

Source: Authors' calculations.

the water source and, in the Resto Rural, a substantial improvement in sanitation facilities (table 10). Unfortunately, data on water consumption were collected only for 1997, making it impossible to measure the improvement in this important indicator.

A laboratory analysis of the quality of water from the old and new sources showed surprisingly little improvement in the 18 SIF water projects in the impact evaluation study.[9] Results indicated fecal contamination in the old system for 9 of the 15 projects where samples could be taken, and in the new system for 7 of the 14 projects where samples were taken. Samples from both the old and the new systems showed a complete absence of residual chloride, suggesting that no chlorination had taken place. Interviews with beneficiaries pointed to the following explanations for the lack of improvement in water quality:

- The personnel designated by each community to maintain the water systems lacked training in procedures for cleaning the water tanks, repairing the water tubes, chlorinating the water supply, and managing the proceeds from user fees.

9. The testing followed recommended parameters defined by the World Health Organization. For more details see Coa (1997) and Damiani (2000).

TABLE 10. Impact of SIF Water Investments in Chaco and Resto Rural

	Chaco		Resto Rural	
Indicator	1993	1997	1993	1997
Incidence of diarrhea in past 24 hours among children less than 6 years old	0.11 (0.31)	0.09 (0.29)	0.09 (0.29)	0.09 (0.29)
Duration of diarrhea (days)	3.03 (2.26)	2.95 (2.71)	5.07 (5.79)	3.28 (2.76)
Fraction of diarrhea cases treated	0.34 (0.48)	0.37 (0.49)	0.53 (0.26)	0.36 (0.49)
Fraction of households with piped water	0.49 (0.50)	0.67 (0.47)	0.44 (0.50)	0.54 (0.50)
Fraction of households with sanitation facilities	0.58 (0.49)	0.61 (0.49)	0.27 (0.44)	0.71 (0.45)
Distance from house to principal water source (m)	211.47 (433.23)	57.95 (207.62)	92.48 (165.11)	41.11 (116.81)
Hours a day of water availability	21.95 (5.95)	19.38 (8.79)	18.49 (8.61)	21.15 (6.97)
Fraction of year with adequate water	0.79 (0.40)	0.89 (0.31)	0.87 (0.34)	0.91 (0.29)
Household water consumption (L/day)	—	23.73 (13.82)	—	20.51 (12.55)
Fraction of households boiling water before consumption	0.54 (0.50)	0.28 (0.45)	0.61 (0.49)	0.45 (0.50)
Fraction of households with knowledge of oral rehydration therapy	0.78 (0.41)	0.95 (0.21)	0.74 (0.44)	0.84 (0.36)
Fraction of households using oral rehydration therapy	0.52 (0.50)	0.55 (0.50)	0.33 (0.48)	0.44 (0.50)

— Not available.
Note: standard deviation in parentheses
Source: Authors' calculations.

- The systems lacked meters for measuring household water consumption, which would have made it easier to collect user fees adequate for providing the necessary maintenance of the system.
- In some cases inappropriate materials had been used (such as tubes designed for oil, not water) and the work was of poor quality (resulting in a rough finish for the water tanks, which made cleaning more difficult).

When the water quality results were presented to SIF representatives, they acknowledged that their initial water projects did have problems, mostly attributable to inadequate training. But they explained that this problem had been solved with the assistance of Prosabar. To test this explanation, a second water quality analysis was carried out using the same approach but covering more recent projects.

The second analysis found significant levels of fecal contamination in 10 of 18 old water sources but in only 2 of 15 new sources. In contrast with the first sample of projects, in which the beneficiaries received little training, in the second sample of projects all communities had received training through Prosabar.

A disturbing finding, however, was that no chlorination was taking place in any of the more recent projects. This could cause problems down the road if maintenance deteriorates or there is an external source of contamination.

In a review of several studies of the health impact of improvements in water supply and sanitation facilities, Esrey and others (1990) suggest that such improvements can be expected to reduce under-age-five mortality by about 55–60 percent. To maximize the health impacts of water projects, they indicate that the supply of water should be as close to the home as possible so as to increase the quantity available for hygiene. They conclude that safe excreta disposal and proper use of water for personal and domestic hygiene appear to be more important than the quality of drinking water in achieving broad health impacts.

The results from the SIF water and sanitation investments are consistent with these findings, showing a significant reduction in deaths among children under age five (table 11). Converting the results to under-age-five mortality rates by estimating a Cox proportional hazard model (as in the health case) shows a reduction from 105 deaths per 1,000 to 61, a decline of 42 percent.

VI. Contribution of the SIF to Declines in Dropout Rates and Under-Age-Five Mortality

The results from the impact evaluation study can be scaled up to suggest the impact that the SIF had in the country as a whole. In Bolivia, as elsewhere, one of the important features of the social fund model is its ability to operate to scale. Between 1994 and 1998 the SIF financed investments in 1,041 of the roughly 3,900 rural primary schools in the country, benefiting roughly 185,000 students. The study estimated that these investments led to a reduction in dropout rates ranging from 3 percentage points (from the propensity score matching) to 3.8 percentage points (the lower bound from the randomization of eligibility). On the basis of these results it can be estimated that the SIF investments led to an additional 5,550–7,030 students remaining in school over the four-year period of the study.[10] The average cost of the school interventions was about $60,650.

SIF health and water investments accounted for roughly 25 percent of the reduction in deaths among children under age five in rural areas between 1994 and 1998. This finding is based on a scaling up of the estimated mortality effects of the sample of SIF investments in the evaluation compared with the change in the total number of deaths in the under-age-five population, in the five-year period before the survey. Data on total deaths are from Demographic and Health Surveys carried out in 1994 and 1998.

The estimate of the number of deaths averted as a result of the SIF health interventions (1,150) was obtained by multiplying the difference in the proportion of children dying between the treatment and comparison groups (0.04) by the

10. Of course, this says nothing about whether the additional students remaining in school stayed to graduate. More time and larger samples would be needed to determine how long lasting the effect is.

TABLE 11. Deaths in Previous Five Years among Children under Age Five in Households Benefiting from SIF Water Investment in Chaco and Resto Rural, 1993 and 1997

	1993	1997
Percentage of children dying	9.74 (167)	5.73 (77)
Percentage of children surviving	90.26 (1,547)	94.27 (1,247)
Pearson design-based $F(1,28)$	14.715 [0.0007]*	

*Significant at the 5 percent level.

Note: Figures in parentheses are number of survivors or deaths. Figure in square brackets is the *p*-value.

Source: Authors' calculations.

estimated number of children under age five served by the 473 SIF-financed health centers (28,853).[11] The estimate of the number of deaths averted because of the SIF water interventions (2,640) was similarly obtained by multiplying the difference in the proportion of children dying between the treatment and comparison groups (again, 0.04) by the estimated number of children under age five served by the 639 SIF-financed water projects (65,945).

Mortality data from the 1994 and 1998 Demographic and Health Surveys (which cover a period roughly coinciding with that covered by the baseline and follow-up surveys of the SIF evaluation) and rural population estimates from the National Statistical Institute indicate a decline of some 13,870 deaths between 1994 and 1998.[12] If not for the SIF interventions, there would have been a decline of only 10,080 deaths.

It is possible to arrive at a rough estimate of the cost per death averted for both the health and the water interventions. The average health intervention cost $47,780, and the average water intervention $62,905. Thus the cost per death averted was roughly $20,000 for the health interventions and $15,200 for the water interventions. This estimate refers only to the initial four years of SIF investments. As long as the investments are maintained, they can be expected to avert more deaths in the coming years. Moreover, the investments lead to benefits beyond the effects on under-age-five mortality.

VII. CONCLUSIONS

The main finding of the evaluation is that SIF-financed investments in health centers and water supply systems appear to have resulted in a significant reduction

11. The mean number of individuals served by health centers was 380, of which 16 percent (61) were under age five. The mean number of individuals benefiting from water projects was 645.

12. The calculations are based on an estimated under-age-five mortality rate of 115.6 per 1,000 in 1994 and 91.7 per 1,000 in 1998 and an estimated population of children under age five in rural areas of 505,510 in 1994 (3,008,993 × 0.168 percent) and 485,984 in 1998 (3,018,535 × 0.161 percent). The estimated number of deaths among children under age five in rural areas was 58,436 in the five-year period before 1994 and 44,564 in the five-year period before 1998.

in under-age-five mortality. By contrast, investments in school infrastructure led to little improvement in education outcomes apart from a decline in dropout rates. But in all three sectors the investments resulted in a demonstrable improvement in the physical facilities.

Why did the SIF investments in health facilities have a greater effect than those in schools? Part of the reason may be that the health investments went beyond simply providing infrastructure. They also provided medicines and medical supplies—and radios and motorcycles supporting outreach to patients and communication with regional health centers and hospitals. Moreover, the results suggest a link between an increase in the utilization of health centers—particularly for prenatal care—and the reduction in under-age-five mortality.

The finding that the investments in school infrastructure are insufficient to achieve the desired impact on education outcomes has implications as much for the education sector as it does for the SIF. Motivated in part by this finding, shared with the government of Bolivia in 1999, the SIF and the Ministry of Education have devoted much effort to changing the projects financed through the SIF. They now give more attention to the "software" of education, and where the SIF finances physical infrastructure, it does so as part of an integrated intervention.

The improvements in under-age-five mortality arising from the investments in water supply were accompanied by significant reductions in the distance of water sources from households and, in the Resto Rural, a substantial improvement in the adequacy of sanitation facilities but not by improvements in the quality of water. Water quality did not improve substantially until after training in operations and maintenance was provided to the communities receiving water projects.

From a methodological standpoint, the three cases highlight the variety of approaches available to evaluators, the benefit of having baseline data, and the need for flexibility in the face of changes in the implementation of interventions. Projects often are not carried out as planned, particularly when they are demand-driven.

Planning ahead for an evaluation and responding creatively to budgetary or administrative constraints can provide opportunities for randomization. In education randomization of eligibility for active promotion of projects was sufficient to obtain all the indicators of interest. This finding is especially useful for evaluations of demand-driven programs because people's behavior can often result in changes to the original evaluation design, as it did for the SIF-financed education projects in the Chaco region. Noncompliance in the control group can be handled by working with bounds to estimate a range of impacts in cases where contamination is not too severe.

Where randomization was not possible, applying propensity score matching to baseline data was reasonably successful in eliminating preintervention differences between treatment and comparison groups and allowed difference-in-difference estimates to measure program impact. The baseline data collected from comparison and treatment groups were essential to this analysis. Preintervention

data can help form better statistical matches and also make it possible to check whether the statistical matching eliminates preintervention differences. If the statistical matching produces a treatment group and a comparison group that do not differ except for the effect of the intervention, there should be no differences in the average values of key characteristics before the intervention. Future impact evaluations should make a greater effort to collect preintervention data.

Appendix A. Using Randomization of Eligibility to Estimate the Average Treatment Effect on the Treated for School Investments in Chaco

This appendix explains how the impact evaluation study derived an average impact estimate for the communities that received a SIF education intervention (the treated population) by taking advantage of the information that some communities were randomly assigned to be eligible to receive such an intervention.

The evaluation design for school investments in the Chaco region included two types of schools: those that were eligible to receive the SIF intervention and those that were not. In the implementation stage, however, the demand-driven nature of the SIF, combined with common difficulties in maintaining a planned evaluation design throughout a project's implementation, gave rise to four groups:

1. Schools that were eligible to receive a SIF intervention and did receive an intervention (compliers in the treatment group)
2. Schools that were eligible to receive a SIF intervention and did not receive an intervention (noncompliers in the treatment group)
3. Schools that were not eligible to receive a SIF intervention and did not receive an intervention (compliers in the control group)
4. Schools that were not eligible to receive a SIF intervention but did receive an intervention (noncompliers in the control group)

Consider first the situation with full compliance in both the treatment and the control group. Using a potential outcome notation, let Y_i (1) denote the outcome for subject i under treatment and let Y_i (0) denote the outcome for subject i without treatment. The average treatment effect on the treated ($ATET$) can be written as

$$ATET = E[Y(1) - Y(0)|se = 1] = E[Y(1)|se = 1] - E[Y(0)|se = 1] \quad \text{(A-1)}$$

where $se = 1$ denotes that treatment was received.

The first expectation in the last expression in equation (A-1) is just the average outcome for the treated, $E(Y \mid se = 1)$.

$$E[Y(1)|se = 1] = E[Y(1)|e = 1, se = 1] \quad \text{(A-2)}$$

where $e = 1$ denotes that the subject was eligible for the intervention. This expectation can be estimated by observing the mean outcomes for group 1.

The second expectation in equation (A-1) is counterfactual and not observed. However, it is possible to derive an expression for this counterfactual from the observed outcomes for other groups when there is randomization of eligibility and full compliance in the control group.

Note that the expected outcome without treatment for the eligible group can be expressed as the weighted average of the expected outcome without treatment for the subgroup that received treatment and that for the subgroup that did not:

$$E[Y(0) \mid e = 1] = E[Y(0) \mid se = 1, e = 1]\Pr(se = 1 \mid e = 1) + E[Y(0) \mid se = 0, e = 1]\Pr(se = 0 \mid e = 1) \tag{A-3}$$

which, because of the randomization of eligibility, can be rewritten as

$$E[Y(0) \mid e = 0] = E[Y(0) \mid se = 1, e = 1]\Pr(se = 1 \mid e = 1) + E[Y(0) \mid se = 0, e = 1]\Pr(se = 0 \mid e = 1) \tag{A-4}$$

The left-hand side of equation (A-4) is the average outcome for the noneligible group, which can be estimated as the average outcome in group 3 (with full compliance in the control group, group 4 is empty). The two probabilities on the right-hand side of the equation can be estimated using the fraction of the eligible group that received treatment and the fraction that did not. The last expectation can be estimated using the mean outcomes for group 2. Thus equation (A-4) can be solved for $E[Y(0) \mid se = 1, e = 1]$. With full compliance in the control group, this is equal to $E[Y(0) \mid se = 1]$. Substituting this solution into equation (A-1) yields an expression of the average treatment effect in terms of the observable expectations and probabilities:

$$ATET = E(Y \mid se = 1) - \frac{E(Y \mid e = 0)}{\Pr(se = 1 \mid e = 1)} + \frac{\Pr(se = 0 \mid e = 1)E(Y \mid se = 0, e = 1)}{\Pr(se = 1 \mid e = 1)} \tag{A-5}$$

Standard errors can be computed using the delta method.

In the data there is noncompliance in the control group: three schools received SIF funding even though they were not in the eligible group. Therefore, $E[Y(0) \mid e = 1]$ cannot be estimated directly. It is possible, however, to derive bounds for this expected value. Note that the original control group of schools consists of a group that did not receive SIF funding (compliers) and a group that did (noncompliers).

$$E[Y(0) \mid e = 0] = \Pr(se = 1 \mid e = 0)E[Y(0) \mid se = 1, e = 0] + [1 - \Pr(se = 1 \mid e = 0)]E[Y(0) \mid se = 0, e = 0] \tag{A-6}$$

where $\Pr(se = 1 \mid e = 0)$ is the probability that a control group subject receives treatment. Note that $\Pr(se = 1 \mid e = 0) \neq \Pr(se = 1 \mid e = 1)$. That is, the probability of selecting for treatment depends on the assignment to the treatment or control group. It is usually easier to select for treatment if a subject is assigned to the eligible group. Thus

$$\Pr(se = 1 \mid e = 0) < \Pr(se = 1 \mid e = 1). \tag{A-7}$$

The last expectation on the right-hand side of equation (A-6) can be estimated directly using the mean observed outcomes for the subjects in the control group that did not receive an intervention (group 3). The probability can be estimated using the fraction of controls that received treatment. The first expectation on the right-hand side of the equation cannot be estimated directly. It is only possible to observe the expected outcome for the control group that received treatment. The following assumptions are used to derive an upper and lower bound for the unobserved expectation:

- Assumption 1: Treatment has no negative impact on outcomes.
- Assumption 2: The average outcome without treatment for control group members who received treatment is not less than the expected outcome before treatment for that same group plus 0.5 times the average change in outcome observed in the nontreated control group.

The first assumption needs little explanation. The second was chosen because education outcomes have generally improved in Bolivia, including for those groups not reached by the SIF. Stating that the expected improvement is more than half the trend observed for the nontreated population is therefore a mild assumption. The bound assumption can be written as

$$\begin{aligned}(\text{A-8}) \qquad & E[Y_{t=0} \mid se = 1, e = 0] + 0.5(E[Y_{t=1} \mid se = 0, e = 0] \\ & - E[Y_{t=0} \mid se = 0, e = 0]) \leq E[Y_{t=1}(0) \mid se = 1, e = 0] \\ & \leq E[Y_{t=1}(1) \mid se = 1, e = 0] = E[Y_{t=1} \mid se = 1, e = 0]\end{aligned}$$

where the subscript t has been introduced to denote the period before the intervention (baseline, $t = 0$) or that after the intervention (follow-up, $t = 1$).

Using these bounds, one can estimate the upper and lower bounds of the treatment effect as defined in equation (A-1). Because $\Pr(se = 1 \mid e = 0)$ is small, these bounds will be relatively close. The three noneligible schools that managed to receive an intervention have relatively little impact on the final estimate of the treatment effect. Standard errors of the bounds can be computed using the delta method.

Rather than assumption 2, an alternative assumption could have been that the lower bound is 0 or that it is equal to $E(Y_{t=0} \mid se = 1, e = 0)$. These weaker assumptions give wider bounds. The bounds are reasonable if the degree of noncompliance in the control group is small. If there is substantial noncompliance, the local average treatment effect (LATE) estimator of Imbens and Angrist (1994) can be used. With full compliance in the control group, this estimator is the same as that derived. With noncompliance in the control group, the LATE estimator is not an estimator of the average treatment effect on the treated.

Appendix B. The Data

Data for the impact evaluation were collected through a baseline survey in 1993 and a follow-up survey that extended over late 1997 and early 1998 (the data from the follow-up survey are referred to as 1997 data in the article). Both sur-

veys collected data from 5 provinces in the Chaco region and 17 provinces in selected rural areas, referred to as the Resto Rural.

The data reflect the three types of investments considered: health care, education, and water supply. Five types of data collection instruments were used: household surveys, facilities surveys, community surveys, water quality samples, and student achievement tests. Each of these instruments was used in collecting data from both treatment and comparison groups.

Household Survey Data

The household survey consisted of three subsamples. The first was a random sample of all households in the Chaco and Resto Rural, which also served as the sample for the health component. The second was a sample of households living near the schools in the treatment or control group for the education component. The third consisted of households in the area of influence of water and sanitation projects.

The household survey gathered information on a variety of aspects, including household composition, household consumption (to generate a measure of poverty for an assessment of targeting), household involvement with the SIF project, individual household members' access to social services, and health and education outcome measures.

In 1997 an effort was made to interview the same households as in 1993, and 65 percent of the 1997 sample consisted of households that were also in the 1993 sample. A household that could not be traced was usually replaced by one nearby. The survey sample sizes are shown in table B-1.

Facilities Survey Data

SCHOOLS. The school survey used two questionnaires, one for the director and one for each teacher. It gathered information on infrastructure, equipment, teaching methods, and student dropout and repetition rates

In the Chaco region the sample of schools surveyed in 1993 was drawn from the group of primary and secondary schools that had been randomly selected as eligible (the treatment group) or not eligible (the control group) for active promotion of a SIF intervention (table B-2). In 1997 it appeared that half the eligible schools surveyed had succeeded in obtaining a SIF intervention.

TABLE B-1. Household Survey Samples
(number of households)

	Chaco		Resto Rural	
Type of investment	1993	1997	1993	1997
Health	2,029	1,941	2,138	1,901
Education	995	1,109	902	856
Water supply	666	594	569	540
Total	3,690	3,644	3,609	3,297

TABLE B-2. School Survey Sample in Chaco
(number of schools)

	1993	1997 intervention	1997 no intervention	Not surveyed in 1997[a]
Eligible	36	17	18	1
Not eligible	35	3	31	1
Augmented sample (added in 1997)	n.a.	15	0	0
Total	71	35	49	2

n.a. = Not applicable.
[a]Schools were not surveyed if key informants were absent at the time of the follow-up survey.

Because only a small number of schools from the original sample received an intervention, it was decided to augment the sample of treatment schools by selecting schools from the universe of those that had participated in the random assignment and had been selected for active promotion of a SIF intervention but had not been surveyed in 1993. The additional schools were randomly drawn from the set of schools that had obtained a SIF intervention by 1997. Because they had been subject to the original randomization process, it was assumed that their average characteristics would not differ significantly from those of the other schools included in the random assignment. The original design was contaminated by three schools that had been classified as noneligible for active promotion but had nevertheless obtained an intervention.

In the Resto Rural the sample consisted of a random sample of treatment schools along with a group of schools that had similar characteristics but did not receive investments (table B-3). Unlike in Chaco, schools were selected for the sample after decisions on interventions were made. Thus the sample included no schools that were eligible but did not receive an intervention.

WATER SUPPLY PROJECTS. At the time of the baseline survey the SIF was just beginning to invest in rural water projects. The 18 projects in the survey constitute the universe of projects considered for funding in 1992. (The SIF has greatly expanded its work in water and sanitation since then.) For these 18 projects, baseline and follow-up surveys were conducted to gather data on the character-

TABLE B-3. School Survey Sample in Resto Rural
(number of schools)

	1993	1997 intervention	1997 no intervention	Not surveyed in 1997[a]
Treatment group	33	31	n.a.	2
Control group	35	n.a.	33	2
Total	68	31	33	4

n.a. = Not applicable.
[a]Schools were not surveyed if key informants were absent at the time of the follow-up survey.

istics of households. A community survey was also conducted in the areas that received water projects, but there was no facility questionnaire like those for health and education.

In addition to these surveys, water quality tests were carried out, for both the old drinking water sources and the new, social fund–financed sources. These tests were carried out in situ using portable equipment and in one of the main water-quality testing laboratories in Bolivia.

HEALTH CENTERS. The health facility survey gathered information on staffing, visits to the center, and the quality of the infrastructure. Because the SIF had planned to invest in all health centers in Chaco and the Resto Rural, all were included in the survey (table B-4).

The survey distinguished between health centers at the sector, area, and district levels. Sector health centers are typically very small, providing basic health care. Area-level health centers provide more sophisticated care and serve a larger geographic region. District-level health centers are hospitals, the largest type of facility. The larger the health center, the more detailed the questionnaire administered. The questionnaires were nevertheless comparable and collected similar types of information.

Other Data

Mathematics and language achievement tests were applied in the follow-up survey in schools in the Chaco and Resto Rural. They could not be applied at the time of the baseline survey because they were developed and introduced as part of the Education Reform Program launched in 1994. However, the equivalency between the treatment and comparison groups established during the evaluation design stage in 1993, particularly through the application of the experimental design in the Chaco region, supports the assumption that the treatment and control groups would not have registered significantly different test scores in 1993.

A community survey collected data from community leaders on topics ranging from the quality of the infrastructure and distance to facilities to the presence of local organizations in both 1993 and 1997.

APPENDIX C. FIRST-STAGE ESTIMATES FOR PROPENSITY MATCHING

Education

Table C-1 presents estimates from the probit equation used to calculate the propensity score for the impact analysis of education investments. All variables are measured in 1993, before the intervention. Each variable was defined as the actual value if observed and 0 if missing. A dummy variable equal to 1 if any of the variables was missing for that observation was added to the probit equation. The number of observations was 119, and the pseudo-R^2 for the probit equation was 0.185.

TABLE B-4. Health Center Survey Sample
(number of health centers)

Type of health center	Chaco				Resto Rural			
	1993	1997 intervention	1997 no intervention	Not surveyed in 1997[a]	1993	1997 intervention	1997 no intervention	Not surveyed in 1997[a]
District	5	4	1	0	4	4	0	0
Area	16	9	6	1	22	4	17	1
Sector	62	47	5	10	84	22	58	4

[a]Health centers were not surveyed if key informants were absent at the time of the follow-up survey.

TABLE C-1. Results of First-Stage Probit for Impact Analysis of Education Investments

Variable	Coefficient	Standard error	*t*-statistic
School characteristics			
Number of classrooms	–.043	0.093	–0.457
Number of classrooms in adequate condition	.035	0.164	0.214
Have sanitation facilities	.090	0.289	0.312
Textbooks per student	–.204	0.412	–0.495
Cost of matriculation per student	–.218	0.09	–2.301
Number of students registered	.002	0.003	0.520
Repetition rate	–.002	0.019	–0.097
Community characteristics			
Number of nongovernmental organizations in community	.518	0.174	2.98
Population of community	.0003	0.0004	0.635
Functioning parents school association	–.007	0.357	–0.019
Whether respondents had heard of SIF	–.004	0.366	–0.012
Household characteristics (average for community)			
Father's education	–.15	0.10	–1.48
Mother's education	.22	0.16	1.32
Per capita household consumption	–.0002	0.0001	–1.08
Distance from household to school	.00004	0.00007	0.59
Dummy variable for missing observation	–.79	0.51	–1.57

Source: Authors' calculations.

Health

Table C-2 presents estimates from the probit equation used to calculate the propensity score for the impact analysis of health investments. All variables are measured in 1993, before the intervention. Each variable was defined as the actual value if observed and 0 if missing. A dummy variable equal to 1 if any of the variables was missing for that observation was added to the probit equation. The number of observations was 92, and the pseudo-R^2 for the probit equation was 0.403.

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TABLE C-2. Results of First-Stage Probit for Impact Analysis of Health Investments

Variable	Coefficient	Standard error	*t*-statistic
Health facility characteristics			
Have electricity	–0.922	0.550	–1.676
Have sanitation facilities	–0.007	0.449	–0.017
Have water	0.412	0.478	0.863
Number of patient rooms	–0.576	0.411	–1.405
Index of availability of medical equipment in good condition	–.017	0.016	–1.123
Index of availability of medical inputs	0.005	0.019	0.237
Number of beds	–0.328	0.237	–1.381
Health outcomes before intervention			
Fraction of women receiving any prenatal care	0.862	1.10	0.782
Fraction of women receiving at least one prenatal checkup who received at least four	2.513	0.975	2.577
Fraction of births attended by trained personnel	0.810	2.19	0.369
Incidence of cough	0.475	1.18	0.402
Fraction with cough receiving treatment	–0.767	0.887	–0.864
Incidence of diarrhea	–4.887	2.787	–1.753
Fraction with diarrhea receiving treatment	–0.448	0.757	–0.591
Fraction of children dying before age five	4.23	3.02	1.40
Fraction of entire community population using health center	10.85	6.325	1.716
Community characteristics			
Distance to nearest main road	0.003	0.012	0.251
Knowledge of SIF	0.558	0.451	1.239
Population of community	–0.001	0.0007	–0.810
Number of nongovernmental organizations in community	0.497	0.270	1.839
Household characteristics (average for community)			
Sum of father's and mother's education	–0.15	0.10	–1.48
Per capita household consumption	0.0002	0.0002	0.682
Dummy variable for missing observation	–1.45	0.51	–2.82
Constant	1.48	1.08	1.364

Source: Authors' calculations.

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