Do school facilities matter? The case of the Peruvian Social Fund (FONCODES)

Christina Paxson[*] and Norbert R. Schady[**1]

Abstract

Since its creation in 1991, the Peruvian Social Fund (FONCODES) has spent about US $ 570 million funding micro-projects throughout the country. Many of these projects have involved the construction and renovation of school facilities. In this paper, we use data from FONCODES, the 1993 Peru population census, the 1994 and 1997 Peru Living Standards Measurement Surveys (LSMS), and a 1996 household survey conducted by the Peruvian Statistical Institute (INEI) to analyze the targeting and impact of FONCODES investments in the education sector. We present results based on a number of descriptive and econometric techniques, including non-parametric regressions, differences-in-differences, and instrumental variables estimators. We show that FONCODES projects in the education sector have reached poor districts and, to the extent that they live in these districts, poor households. FONCODES has had a positive effects on school attendance rates for young children, but not on the likelihood that children will be at an appropriate school level for their age. We conclude with some general recommendations and suggestions for further research.

[*] Woodrow Wilson School of Public and International Affairs, Princeton University, Princeton, NJ 08544.
[**] Poverty Reduction and Economic Management, The World Bank, Washington, D.C. 20433.
[1] This research was conducted as a background paper for the Social Funds 2000 ESW, and was partly funded by the Thematic Group on Impact Evaluation in the Poverty Group of the World Bank. We would like to thank Angus Deaton, Esther Duflo, Olivier Deschenes, Rutheanne Deutsch, John Gallup, Doug Miller, Martin Ravallion, Julia van Domelen, Juliana Weissman, and participants at a seminar at the World Bank and at the NEUDC Conference held at Harvard University on October 8th and 9th 1999 for helpful comments and suggestions. We also wish to thank staff at FONCODES and INFES and Livia Benavides for help providing us with the data. Please address correspondence to the authors at cpaxson@.princeton.edu and nschady@worldbank.org, respectively. The views and interpretations expressed in this paper are those of the authors and do not necessarily represent the views and interpretations of the World Bank, its Executive Directors, or the countries they represent.

1. Introduction

Since the creation of the Emergency Social Fund (ESF) in Bolivia in late 1986, social funds have been established in dozens of countries, often with support from multilateral organizations and international donors. Social funds like the ESF were originally put in place to mitigate the social costs of structural adjustment programs (for example, Newman, Jorgensen and Pradhan, 1991; Jorgensen, Grosh, and Schachter, 1992). Since then, they have been proposed, amongst other things, as a safety net for the poorest poor; a means of generating employment and transferring income; an efficient mechanism to construct small-scale infrastructure, especially in outlying, traditionally neglected areas; and a way of building on (or generating) local social capital by involving communities in project choice, preparation, and operation and maintenance (for example, World Bank, 1997; IDB, 1997).

This paper analyzes the targeting and impact of investments made by one social fund—the Peruvian Social Fund (FONCODES) —between 1992 and 1998. Specifically, the paper looks at the investments FONCODES made on education. In so doing, the paper aims to answer two questions. First, who received FONCODES transfers? This is, essentially, a question of targeting. FONCODES aims to transfer resources, including investments in education, to poor areas and to poor households within those areas. The paper evaluates the extent to which it has been successful in doing so. Second, did FONCODES transfers improve educational outcomes? This is, essentially, a question about the impact of investments in school facilities on measures such as school attendance rates, the likelihood of being at an appropriate grade level for a given age, and the average amount of time it takes children to get to school.

There are at least two reasons to focus on expenditures made by FONCODES on education, rather than on total FONCODES expenditures. First, education is one of the two biggest sectors in the FONCODES portfolio, accounting for about one-quarter of total FONCODES expenditures made between 1992 and 1998. Moreover, as Table 1.1 shows, the bulk of these expenditures on education was made in the first few years of FONCODES operations, so that measurable changes in outcomes such as school attendance have had some time to take place (although the full benefits of investments in education may not be realized for years). An analysis of FONCODES investments in education therefore deserves attention in its own right. Second, it is possible to use available data sources to construct credible outcome measures in education; this is not easily done in other sectors, such as agriculture, transportation, or even health.[2]

[2] Initial inspection of the data on health in the 1994 and 1997 Peru LSMS, for example, suggests that infant mortality went *up* during the period. This seems quite implausible given other changes which took place in Peru between 1994 and 1997, and is likely driven by the very small number of reported deaths in both

While this paper describes and evaluates a specific program—FONCODES—it adds to an ongoing debate about the relationship between educational inputs and outcomes (for a summary see Hanushek, 1995, and the response by Kremer, 1995). There is a growing body of literature which suggests that there are high rates of return to expenditures on school facilities in many developing countries (for example, Harbison and Hanushek, 1992; Velez, Schiefelbein and Valenzuela, 1994; Glewwe and Jacoby, 1994; Hanushek, 1995; Glewwe, Grosh, Jacoby, and Lockheed, 1995; Duflo, 1998). Our results suggest that expenditures on educational infrastructure in Peru had an impact on some educational outcomes—such as the attendance rate of young children—but not on others—such as the likelihood that these children would be at an appropriate grade for their age. Because expenditures made by FONCODES on education were well targeted towards poor districts and (less clearly) poor households, improvements in attendance rates were concentrated amongst the most needy.

We believe that this paper also makes a contribution to the evaluation literature. We use the geographic variation in FONCODES expenditures to estimate the impact of FONCODES investments in school facilities on educational outcomes. Identification strategies based on geographic differences are often used to estimate the impact of social programs and policies in the United States (for example, Card, 1990, 1992, and 1993; Card and Krueger, 1994 and 1998) and, more recently, in a developing country context (for example, Pitt, Rosenzweig and Gibbons, 1993; Angrist and Lavy, 1997; Duflo; Case and Deaton, 1999). Such estimation strategies are themselves not uncontroversial, however (for example, Besley and Case, 1994; Heckman, Farrar and Todd, 1996). In this paper we contrast results based on a number of estimation strategies, including "naïve" regressions, "differences-in-differences", and instrumental variables to test the robustness of our findings, and the assumptions on which these and similar results are based.

The rest of the paper proceeds as follows. In Section 2, we provide a brief overview of the Peruvian context, and FONCODES. Section 3 describes the data set. Section 4 briefly discusses previous evaluations of FONCODES. Sections 5 and 6 present our results on the targeting and impact of FONCODES investments in education. Section 7 concludes.

---

surveys. Chronic malnutrition could be a more promising health outcome, since it affected 30% of children under five in 1994, and 23.8% in 1997 (World Bank, 1999, p. 10). However, there appears to be no clear relationship between changes in chronic malnutrition and FONCODES expenditures on health, water and sanitation.

2. The setting

Peru has made substantial economic progress since the election of President Alberto Fujimori in 1990. After a brief recession that followed the adoption of stringent stabilization and adjustment measures in 1990, growth has been strong, inflation low, and poverty reduction sustained (World Bank, 1995 and 1999). Investments in the social sectors have increased dramatically. The Government of Peru has attempted to target these social investments to the poor—albeit, with only partial success (World Bank, 1999).

FONCODES was created in 1991. The stated objectives of FONCODES were to generate employment, help alleviate poverty, and improve access to social services (World Bank, 1998). Between 1992 and 1998, FONCODES funded almost 32,000 community-based projects, for an aggregate outlay of about 760 million soles.[3] As Table 1.1 shows, most community based projects in the education sector have entailed the construction and renovation of classrooms. Before 1995, however, FONCODES also had education projects to construct and renovate sports facilities, and provide textbooks and other educational material to students. In addition, FONCODES executed a series of centrally-designed "special" projects. Special projects in the education sector have included a school breakfast program, and the distribution of uniforms for schoolchildren. Between 1992 and 1996, FONCODES spent about 160 million soles on all special projects, including those in the education sector.[4]

FONCODES has much in common with other social funds in the region (Glaessner et. al. 1994). Two features of FONCODES that are particularly important for this study are the demand-driven and targeted nature of FONCODES projects. FONCODES is demand-driven in that communities themselves choose a project from a menu and prepare a proposal for funding. FONCODES then functions as a financial intermediary: rather than execute projects itself, it approves proposals and releases funds to the *nucleo ejecutor*--a group of community members elected specifically for this purpose. FONCODES also targets its investments—first, by using an index of "unmet basic needs" to assign resources to small geographic areas, and then conducting an informal on-site assessment of the "poverty" of the community requesting a project. FONCODES makes no attempt to target individual households within a given community.

[3] All reported expenditures are in 1992 soles, unless otherwise noted. The December 1992 exchange rate was 1.63 soles to the US dollar.

[4] The World Bank and the Inter-American Development Bank have both supported FONCODES since early 1994 with two subsequent loans, for US $ 100 and 150 million each, for a total of US $ 500 million. The community-based projects are eligible for funding under the World Bank and IDB loans, whereas the special projects are not.

3. The data set

This study evaluates the targeting and impact of FONCODES investments using available sources of data; no new data were collected. The data for the analysis come from two main sources: (i) information on the geographic distribution of FONCODES allocations and expenditures, kept by FONCODES itself; and (ii) information from the 1994 and 1997 Peru Living Standards Measurement Survey (LSMS), and a household survey conducted by the Peruvian Statistical Institute (INEI) in 1996.

Since 1992, FONCODES has used an "allocation rule" to direct resources to small geographic jurisdictions—provinces, for 1992 through 1995, and districts since 1996.[5] Specifically, the population of each province or district is weighted by an index of unmet basic needs. This index is an ad hoc composite of various measures—including access to schooling, electricity, water, sanitation, adequate housing, and measures of chronic malnutrition and illiteracy (Schady, 1998). Provinces or districts with higher values of the FONCODES index have more unmet basic needs. The index used by FONCODES has evolved over the years: until 1993, it was based on information from the 1981 population census, but it has been updated with information from the 1993 population census since 1994. The variables and weights in the index also changed somewhat between 1992 and 1995, but have remained constant since then. Information on the FONCODES index, the allocation rule, and the corresponding allocations is available for 1992 through 1998.

FONCODES also keeps monthly records on the number of projects and aggregate amounts spent in each district. Three points are worth noting about these data. First, because expenditures are recorded in the month in which a project is *approved*, there is a lag of about two weeks before *disbursement* of the first installment of funds, and of several months before the second (and final) disbursement. Second, only expenditures on projects are included, and not, for example, administrative costs or expenses for general overhead. Third, district-level information on the allocations and expenditures is only available for the community-based projects, and not for the special projects. Finally, we also have district-level data on the expenditures made by some other programs in the education sector.

The 1994 and 1997 LSMS and the 1996 INEI survey contain a wealth of information on the expenditures or income of households, education levels, and other household characteristics. The INEI survey has a large sample size—more than 18,000 households, in 403 districts. The

[5] Provinces and districts correspond to the two levels of local government in Peru. In 1997, there were 194 provinces and 1812 districts in Peru (Webb and Fernandez Baca, 1997, p. 112). The median population of a district is about 4,000 people, but there is considerable variation in the number of people per district: some (rural) districts have less than 500 people, and other (urban) districts can have more than 100,000 people.

LSMS surveys are both smaller—about 3,500 households each, in 199 districts (in 1994) and 228 districts (in 1997)—but they have the advantage that a very similar questionnaire was applied in both years. The LSMS includes a panel covering just over a quarter of the households in the two samples.

The LSMS and INEI surveys are useful for the analysis of targeting and impact in two ways. All of the surveys include district-level identifiers for every household. Household-level information from the surveys can therefore be linked to geographic information on allocations and expenditures kept by FONCODES. In addition, the 1996 INEI survey includes questions about benefits from various social programs. Specifically, households who had at least one member attending a public school were asked about recent improvements in school facilities, and whether these had been carried out by (separately) FONCODES, the educational infrastructure program INFES, or the local parents' committee. Respondents in the 1996 INEI survey were also asked whether any household member currently benefited from a feeding or nutrition program carried out by one of 14 organizations, including FONCODES' school breakfast program.

We use the household surveys to construct various education outcome measures, including school attendance rates, measures of the number of years of schooling passed for a given age, and the average amount of time it takes children in a household to get to school, as well as household-level covariates, such as the age, gender, ethnicity, and education of other household members. Because these surveys were not designed specifically for an evaluation of FONCODES, however, they also have shortcomings for our analysis. Three limitations are worth noting. First, none of the surveys collected information on the quality of education, including measures such as the amount of time spent in school, pupil-teacher ratios, and scholastic achievement. Second, there appears to be a large amount of measurement error in the FONCODES "treatment" variable in the 1996 INEI survey. Third, questions about benefits from FONCODES programs in the 1996 INEI survey were asked only of families with children in school. They cannot therefore be used to determine whether children *not* in school had access to a FONCODES-improved school. We further discuss the way in which each of these limitations affects our analysis in sections 5 and 6 below.

4. Previous evaluations of FONCODES

Since 1994, Apoyo, an external public research NGO in Peru, has conducted five ex-post evaluations of FONCODES community-based projects. These evaluations have included interviews with FONCODES beneficiaries, members of the *nucleo ejecutor*, project operators, and officials in FONCODES' regional and central offices. Beneficiaries and others in the sample generally rated the impact of FONCODES investments to be positive (for a summary, see World

Bank, 1998). In addition to the Apoyo evaluations, Moncada (1996) considers various aspects of FONCODES performance; Schady compares the FONCODES index of unmet basic needs with other district-level mesures of welfare available in Peru, and analyzes political influences on the timing and distribution of FONCODES investments (Schady, 1998 and 1999, respectively).

The analysis in this paper complements the work done by Apoyo and others. The Apoyo evaluations are useful to assess the opinions that FONCODES beneficiaries have of FONCODES, but they have only limited value to measure the targeting and impact of FONCODES investments. The evaluations did not create a rigorous control group necessary for a comparison of FONCODES beneficiaries with others; they were not based on a sampling framework which would make it possible to apply the results from the sample to a larger population—even if this population were limited to FONCODES beneficiaries; and information was not gathered on a number of important variables, including actual figures on school attendance or grade repetition.

5. The targeting of FONCODES investments in education

Social expenditures in Peru have traditionally favored vocal, urban middle-class constituencies. FONCODES was set up, in part, to redress this balance, and funding projects which benefited poor areas and poor households has been an important program objective from the outset.

Targeting education resources is important in Peru because there are large differences in measures of educational attainment across regions and income groups. We use data from the 1996 INEI survey to illustrate this point. In Figure 5.1 we graph the average number of years of schooling attained by children of different ages who live in the poorest 25% of districts, up to those who live in the richest 25% of districts, when districts are ordered by mean per capita income.[6] Figure 5.2 presents a comparable graph for household income quartiles. Figures 5.1 and 5.2 clearly show that children in poor districts and poor households lag behind in the years of schooling they attain for any given age. The differences across quartiles increase with age, so that by age sixteen there is almost a two-year difference between children in the poorest and richest districts, and almost a one-year difference between those in the richest and poorest income quartiles.

A host of factors probably contribute to differences in the educational attainment of children, including differences in the education of other household members, income,

[6] INEI combined information from the 1993 population census and a household survey conducted in 1995 to estimate mean district per capita income. This is done by first, using the household survey to regress income per capita on a set of variables common to the census and the survey, and second, predicting mean income per capita for each district by applying the coefficients from the regression to the census (see INEI, 1996, and Hentschel et. al. for a discussion of the methodology).

employment opportunities, and ethnicity. Many of these factors cannot be changed with public policies—at least not in the short run. But educational attainment is also likely to be a function of the number and quality of teachers, learning material, and classrooms which are available in a community. Poor districts and poor households may therefore need additional resources, including resources spent on school facilities, to catch up with their better-off counterparts. In this section, we use nonparametric regressions to assess the extent to which FONCODES has reached poor districts and poor households.[7]

(i) Geographic targeting: Has FONCODES effectively reached poor districts? We consider three aspects of the geographic targeting of FONCODES investments in education: changes in targeting over time, the sectoral composition of FONCODES expenditures, and a comparison of district-level expenditures made by a number of programs in education, including FONCODES. In each case, the analysis is based on actual, rather than allocated expenditures.[8] Breaking down FONCODES expenditures on education by year shows how FONCODES' geographic targeting has evolved over time. Similarly, breaking down FONCODES expenditures by sector (education, health, water and sanitation, etc.) allows us to describe the relationship between average district welfare and project choice. Finally, comparing district-level expenditures made by FONCODES, INFES, and the feeding program PRONAA (which is responsible for a large school breakfast program) places FONCODES' geographic targeting in the context of other government programs in education.

Figure 5.3 presents a nonparametric univariate density estimate of the population living in districts with different mean per capita incomes. Figure 5.3 shows that most Peruvians live in districts with a mean per capita income of between 150 and 500 soles, although there are some districts which are considerably better off.[9]

The main results of the analysis on geographic targeting are summarized in Figures 5.4 through 5.6. Figure 5.4 presents nonparametric regressions of the predicted per capita FONCODES expenditures in a district given its mean per capita income in four "typical" years: 1992, 1994, 1996, and 1998. The regression lines show that FONCODES expenditures on education were highest in 1994, and that poorer districts generally received more in per capita

[7] All nonparametric regressions are Fan regressions, with a quartic kernel (see Fan, 1992, and Deaton, 1997).

[8] There are (at least) two reasons why allocated and actual expenditures are likely to differ in practice. First, allocations are an exact figure, while FONCODES projects are "lumpy" (for example, one classroom), so that it would be almost impossible for actual expenditures to correspond to allocations in any given district. Second, because of the demand-driven nature of a program like FONCODES, some districts may be proactive and prepare a large number of fundable projects, while others may not prepare enough projects of an acceptable technical quality to meet their allocations.

[9] These figures are taken directly from a INEI publication (INEI, 1996), and are therefore in 1995 soles.

terms than their better-off counterparts, especially after 1992. Moreover, geographic targeting has clearly improved over time. By 1996, households living in the wealthiest quartile of districts were receiving virtually no investments in school facilities from FONCODES at all.

Figure 5.5 presents comparable figures for expenditures made by FONCODES in 1995 in three sectors: education, electrification, and water and sanitation. The corresponding curves for FONCODES projects in other sectors— community centers, health, agriculture, and transportation—are not included to avoid cluttering the pictures, but the results are very similar for other years and other sectors. Figure 5.5 suggests that education projects may be somewhat better targeted to the poorest districts than some of the alternatives in the FONCODES portfolio, although these differences do not appear to be very large.[10]

Figure 5.6, finally, compares district-level expenditures made by FONCODES, the educational infrastructure program INFES, and the feeding program PRONAA in 1995. The results show that the geographic distribution of expenditures made by FONCODES was clearly more pro-poor than that of INFES or PRONAA. In sum, the geographic targeting of FONCODES projects in the education sector is progressive, has improved over time, and is noticeably more pro-poor than that of two other large public sector programs in the sector.

(ii) Household-level targeting: In Peru, there is considerable heterogeneity in the intra-district distribution of welfare: for example, a simple decomposition of the variance in per capita income into inter- and intra-district components suggests that only 24% of the variance is explained by differences across districts. Reaching poor districts is therefore only a weak proxy for reaching poor households. This section looks at the targeting record of FONCODES investments in education at the household level.

The basic approach we use calculates the household-level incidence of FONCODES benefits with information from the 1996 INEI survey on per capita income, access to educational infrastructure, and access to school breakfast programs financed by FONCODES. Two (possible) concerns with this approach are measurement error, and the impact of participation in FONCODES programs on household income.

In rural areas in Peru, households are likely to have little choice of primary school. In the absence of measurement error, one would therefore expect a high degree of consistency in the answers given by households within a given rural community about the presence of FONCODES-

[10] Note that it is important to break the analysis down by year, because of changes over time in both targeting performance and the composition of expenditures. For example, most FONCODES projects in education were funded in the earlier years, whereas most FONCODES projects in water and sanitation were funded in the later years. Graphs of the distribution of expenditures by sector for the entire 1992-1998 period would therefore (misleadingly) suggest that it was easier to target water and sanitation than education projects.

funded education projects. Unfortunately, this is not always the case. Consider, for example, households in rural areas who only have children attending primary school. In 4 rural communities in the sample, all such households reported that FONCODES had financed improvements to the local school, and in a further 107 all reported that FONCODES had not financed improvements. In 46 rural communities, however, there was no agreement, with different households providing different responses. These results, and similar ones for the FONCODES school breakfast program, suggest that households in the 1996 INEI survey do not always report program benefits accurately. Measurement error of this sort will bias the estimates of program incidence if it is correlated with income, so that richer (or poorer) households are more (or less) likely to report that they benefited from FONCODES.

A second problem arises if program participation has an impact on per capita income. Ideally, to estimate program incidence, one would rank households by the per capita income they would have had in the absence of the program. But this income is, by definition, unobservable. We believe that in the case at hand the impact of program participation on household income is not a major source of concern because the monetary value to a household of FONCODES investments in education should only be a small fraction of total household income. Throughout the analysis, we simply rank households by their observed per capita income.[11] As before, we present nonparametric regressions for educational infrastructure and school breakfast programs.

Figure 5.7 presents a nonparametric univariate density estimate of the distribution of the log of per capita income. A very small fraction of the population in Peru lives on a (log of) per capita income of less than 6 soles or more than 10 soles per year. The graphs for the nonparametric regressions trim these households at the top and bottom 1% of the income distribution.

Figure 5.8 compares the household-level incidence of FONCODES investments in school infrastructure with those made by INFES, and the Parents' Committees (akin to the PTA's in the United States). Two things are worth noting about the graph. First, it shows that the incidence of investments in educational infrastructure is more pro-poor when these are made by FONCODES than when they are made by the Parents' Committees or INFES. The comparison between FONCODES and INFES is particularly clear. To some extent, no doubt, this reflects the fact that

[11] In theory, participation in an education program could increase or decrease household income in the short run. Benefiting from a free school breakfast program will likely represent an increase in income, but putting children in school may lead to a short-term decrease in income if these children would otherwise have been gainfully employed. Note that a similar problem with an unobservable counter-factual may also arise with our estimates of the incidence of FONCODES-level geographic targeting for 1992 and 1993. This is because the analysis ranks districts by mean district per capita income, but this income could itself have been affected by FONCODES investments made prior to 1993.

INFES has (mainly) built or renovated secondary schools in urban areas, while FONCODES has (mainly) built or renovated primary schools in rural areas. In Peru, as in many other countries, the poor are less likely to send their children to secondary school and more likely to live in rural areas. Second, the nonparametric regressions show that the shape of the FONCODES distribution slopes *upwards* at very low levels of (log) per capita income: the poorest 7% of households are less likely to benefit from FONCODES investments in education infrastructure than their (slightly) better-off counterparts.

Figure 5.9 compares the household-level incidence of FONCODES' school breakfast program with similar programs executed through PRONAA, the Ministry of Education, and municipal governments. The results suggest that the FONCODES programs tend to be more pro-poor than those executed by other public sector agencies. Indeed, the probability of receiving a school breakfast provided by FONCODES decreases (almost) monotonically with income, and is highest for children in the lowest quartile of the distribution. Unlike FONCODES projects to improve educational infrastructure, the school breakfast program is one of the centrally-administered "special" projects. It is therefore much less dependent on the ability of communities to organize, prepare proposals, liaise with FONCODES officials, and administer funds than the community-based projects in education. It may be harder to target community-based projects than special projects if poor households in poor communities have less institutional capacity.

FONCODES has placed a great deal of importance on geographic targeting, and less on other forms of targeting, such as means testing (World Bank, 1996).[12] A comparison of the results on geographic- and household-level targeting suggests that FONCODES has been more able to reach poor districts than poor households in these districts. To explore this issue further, we graph the estimated probability of benefiting from investments made by INFES, PRONAA, and FONCODES on the number of standard deviations that the income of household i in district j is above or below the mean income in district j when both household and district incomes are calculated from the 1996 INEI survey. Figure 5.10 shows that the nonparametric regression lines for FONCODES school infrastructure and school breakfast programs are "humped", peaking at about one standard deviation above mean district income. Within a given district, households that are somewhat better-off than their counterparts are more likely to benefit from FONCODES investments. This suggests that there was essentially no (positive) intra-district targeting of FONCODES resources in 1996.

[12] The amount of targeting may be limited by the nature of FONCODES projects, especially those which involve the construction of infrastructure. It may be possible, however, to distinguish between communities of different characteristics within a district—in particular in the larger districts, which also tend to be less homogeneous. Means testing may also be possible for some of the "special" projects.

6. The impact of FONCODES investments in education

Investments in education have long-term effects: higher levels of school attainment are associated with greater future earnings, lower fertility, better health outcomes both for those who receive more schooling as well as for their children, and higher levels of educational attainment for the children of those with more schooling. FONCODES is much too new for any of these long-term effects to have appeared. The program is old enough, however, to have had an impact on rates of school attendance and grade completion of children in Peru. If FONCODES investments do not result in more children attending school, staying in school longer, and learning more while they are there, then the long-term benefits will not materialize. In this section, we examine the impact of FONCODES investments on school attendance, the probability of being at an appropriate grade level for a given age, and the average time it takes children to get to school.

(i) Methods: The simplest way to estimate the impact of FONCODES investments in education on educational outcomes would be to regress measures of these outcomes on the level of past program expenditures and a set of controls. This "naïve" regression strategy is likely to produce biased and inconsistent estimates of the impact if program placement is not random (for example, Rosenzweig and Wolpin, 1986; Pitt, Rosenzweig, and Gibbons; Besley and Case; Strauss and Thomas, 1995; Friedlander and Robbins, 1995). A major reason for bias is the presence of omitted and often unobserved variables that affect both outcomes and the level of program benefits.

There are a variety of methods for dealing with the problem of unobserved heterogeneity, including approaches based on regression discontinuities, different variants of fixed effects estimators, and instrumental variables (IV) estimators (for a summary, see Meyer, 1995). We use a number of these techniques to estimate the impact of FONCODES investments.

All of our work is based on a comparison of outcomes of children in districts that received varying levels of FONCODES funding. A useful starting point is to specify an equation for an outcome measure (for example, school attendance) before and after the program has started. Let Y_{hd0} be an educational outcome for household (or child) h in district d in the "pre-treatment" year 0, and Y_{hd1} be the same outcome measured in year 1, after the program has begun. Assume that the outcome is a function of a set of household and district characteristics $\boldsymbol{X}_{hdt}$, a time dependent intercept α_t and, for the later time period, the amount of per capita FONCODES expenditure received by the district between year 0 and year 1, F_d. The equations for outcomes in the two years are:

$$(1)\quad Y_{hd0} = \alpha_0 + \boldsymbol{X}_{hd0}\beta + e_{hd0}$$

and

(2) $Y_{hd1} = a_1 + \boldsymbol{X}_{hd1}ß + dF_d + e_{hd1}$,

where d is the (marginal) impact of program expenditure on the outcome. These two equations can be more compactly written as follows:

(3) $Y_{hdt} = a_t + \boldsymbol{X}_{hdt}ß + dF_d\, I(t=1) + e_{hdt}$,

where $I(t=1)$ is a indicator function equal to 1 in year 1, and 0 otherwise.

The "naïve" approach is to estimate equation (2) by ordinary least squares, using only data from time period 1. This will produce a consistent estimate of d only under the assumption that the expectation of e_{hdt} conditional on $\boldsymbol{X}_{hdt}$ is zero. This assumption is suspect, however, both because FONCODES is targeted to poor areas, and because it is demand-driven. If poorer districts, with worse educational outcomes, receive larger levels of F_d, and if poverty is not adequately controlled for in $\boldsymbol{X}_{hdt}$, then it is likely that the estimate of d will be biased down. The estimate could be negative even if the program actually improves outcomes (see, for example, the results in Pitt, Rosenzweig and Gibbons for Indonesia). To some extent, this problem can be countered by including district-level measures in $\boldsymbol{X}_{hdt}$, in particular the composite index of unmet basic needs which FONCODES uses to make allocations. It is still possible, however, that decisions to fund projects are based on more accurate information about local conditions, information that we do not possess. It is also possible that households in districts that care more about education, or have greater local institutional capacity, are those that organize to apply for FONCODES funding. If institutional capacity and "tastes" for education are unobserved and positively correlated with better educational outcomes, the naive estimate of d may be biased up.

If two cross-sections of data are available, one from time 0 and one from time 1, more estimation strategies are possible. If the same districts and households are represented in both cross-sections, then a fixed effects estimator is a good alternative. However, this has an important disadvantage for our analysis because fixed effects models can be estimated on only a subset of the data. The panel of households interviewed in both 1994 and 1997 consists of only 25% of all households interviewed, while the corresponding fraction of households living in districts which were included in both the 1994 and 1997 LSMS accounts for 71% of the sample. Although the use of district (household) panel data would enable us to handle district (household) level heterogeneity, it comes at the cost of fewer observations and less precision.

An attractive alternative is to use a "difference-in-difference" estimator. Assume that the error term in (3) takes the special form $e_{hdt} = ?F_d + e_{hdt}$. This is equivalent to a specification with district-level fixed effects, with the added restriction that the fixed effects are proportional to the level of FONCODES funding received. We then estimate:

(4) $Y_{hdt} = a_t + \boldsymbol{X}_{hdt}ß + ?F_d + dF_d\, I(t=1) + e_{hdt}$,

using the full sample of panel and non-panel households. To see how the parameters in (4) should be interpreted, assume for the moment that funding is either received or not received, so that F_d takes on one of two values, 0 or 1. In this case, ? measures the difference in the outcome variable across households in funded and unfunded districts (given $\boldsymbol{X}_{hdt}$) in time period 0, prior to the expenditure. We can therefore refer to it as the "targeting" effect. The sum of ? and d measures the difference across funded and unfunded districts in period 1, after the program is conducted. The parameter d is the "difference-in-difference", which measures the impact of FONCODES funding between period 0 and 1. When F_d is a continuous variable, these parameters represent marginal rather than level differences (see, for example, Angrist and Lavy, 1997, or Duflo for applications of similar methods to the evaluation of education programs).

The difference-in-difference estimate will be biased if there are unobserved factors (such as poverty, or tastes for education, or local capacity) which have components that vary over time and are correlated with F_d. For example, the demand for education could change, due to (unobserved) changes in local labor markets, prompting households in a village to apply for funding and to keep their children in school longer, with or without the help of FONCODES. A solution to this problem is to estimate the model using instrumental variables (IV). In the case of FONCODES, the *allocation* of funds to districts is a good instrument for actual expenditures. The instrument must meet two well-known conditions: it must be correlated with expenditures, and uncorrelated with the error term. The first condition can be tested in practice, while the second seems plausible since allocations during the years in question were developed from formulae based on the 1981 and 1993 censuses—both of which took place before the period covered in the analysis. It is therefore unlikely that allocations are correlated with unobserved changes in "tastes" for education between 1994 and 1997.[13]

Several potential problems with the strategies discussed above should be kept in mind when assessing our results. The 1994 LSMS is not a true baseline survey: it was conducted after FONCODES had made expenditures in 1992 and 1993. In what follows, our measure of FONCODES expenditure F_d consists of (per capita) expenditures made in 1994, 1995 and 1996. We include 1994, since lags between project approval, disbursement of funds, and actual school

[13] However, it is still possible that the allocation of funds is correlated with other (unobserved) factors that affect school attendance. For example, it could be that other programs spend more or less money in districts that have been allocated more by FONCODES, or that there are macroeconomic shocks that are correlated with allocations and happen to affect attendance. As we discuss in more detail below, including controls for the amount spent by other programs on school infrastructure does not alter our results. Still, without more years of data, and more information on other programs, we cannot rule out all problems of this type.

improvements make it unlikely that 1994 expenditures affected 1994 education outcomes. Similarly, we exclude 1997 expenditures because these expenditures should not have affected educational outcomes in 1997. A potential problem is that expenditures made in 1992 and 1993 affected outcomes in both 1994 and 1997, possibly in different ways. If this is the case, and if expenditures in 1992 and 1993 are correlated with F_d, our results could be biased. We do not think this is a large problem. We experimented with defining F_d as expenditures from 1992 to 1996, and it made little difference to the results that follow. Separately, we also used the 1991 Peru LSMS to estimate the effect of FONCODES expenditures in 1992 and 1993 on changes in educational outcomes between 1991 and 1994. The results (not reported) are quite similar to those for the 1994-1997 period, although they tend to be more noisy because of the smaller sample size of the 1991 LSMS.

In all of our results, we do not control for expenditures on other school-based inputs. We therefore estimate the "gross" effect of FONCODES expenditures on educational outcomes, inclusive of any "crowding in" or "crowding out" which may occur. For example, if the Ministry of Education were more likely to supply teachers to a school renovated by FONCODES, the effect of these additional teachers would be included in our measure of the impact of FONCODES investments. Conversely, if INFES were less likely to invest in school facilities in a district in which FONCODES were working, the "net" effect of FONCODES investments may be larger than the "gross" effect that we estimate. We believe that this "gross" effect is the appropriate parameter to estimate for policy purposes: presumably, if FONCODES were to be abolished, the resulting effect would be approximated by our results. Nonetheless, we do not think that there are very large differences between the "gross" and "net" estimates of FONCODES investments. The Ministry of Education does not have a policy whereby schools which receive FONCODES funding receive priority attention for other educational inputs as well. We also experimented with including district-level measures of INFES expenditures in our regressions. The results (not reported) are virtually identical to those which follow, suggesting that INFES and FONCODES investments are orthogonal to each other once appropriate district-level controls are included in the model.

Finally, we were unable to use the 1996 INEI survey to construct cluster-level measures of FONCODES "treatment" because of the low level of agreement amongst likely FONCODES "beneficiaries" in a given community. The inability to accurately determine which children in a community have and have not been treated, and the fact that most districts included in the INEI and LSMS surveys received some FONCODES education projects, precludes the use of statistical matching as an estimation strategy (see, for example, Heckman, Ichimura, and Todd, 1997; Jalan and Ravallion, 1998).

(ii) Results: We start by examining whether districts that received the most FONCODES expenditure for school improvements achieved the largest gains in school attendance.[14] Although the LSMS surveys can be used to construct measures of district-level attendance, district-level measures based on larger numbers of children from more districts can also be obtained from the 1993 census and the 1996 INEI survey. While we do not have household-level census data, we do have information on district-level attendance rates for children aged 6 to 11.[15]

Figure 6.1 shows the relationship between school attendance rates and a modified version of the FONCODES index. We modify the FONCODES index so that we exclude one variable—the fraction of children aged 6-11 who are not attending school.[16] Keeping this variable in the FONCODES index would have produced a purely mechanical negative relationship between the index and school attendance in 1993. Each line in Figure 6.1 corresponds to a nonparametric regression of the attendance rate on the index in each of the two years, 1993 and 1996. Figure 6.1 shows that the relationship between attendance rates and the FONCODES index has changed during the period. There is an obvious negative relationship between school attendance and high levels of unmet basic needs in 1993, but this is no longer apparent in 1996. In other words, worse-off districts had large gains in school attendance, but better-off districts did not. Figure 6.2 graphs the *change* in the school attendance rate and the per capita FONCODES expenditures on education—both as a function of the FONCODES index. This figure shows that the worse-off districts which experienced greater gains in school attendance also received more funding for school improvements. The degree of co-movement between attendance gains and school funding is striking. It is possible that the relative gains of children in poorer districts were due to some (unobserved) factor which is coincidentally associated with FONCODES expenditures. But the data are also consistent with a causal relationship between expenditures and attendance gains.[17]

---

[14] Throughout section 6, we consider only FONCODES expenditures on the construction and renovation of classrooms (see Table 1.1). Specifications based on total education expenditures do not fit the data quite as well, suggesting that the construction of sports facilities and the provision of educational material by FONCODES did not have a discernible impact on educational outcomes.

[15] The 1993 census and the 1996 INEI surveys both ask about whether children in the household were attending school ("asistiendo al colegio"), which is slightly different from the LSMS question of whether children were attending school "or studying something" ("asistiendo al colegio o estudiando algo"). One consequence of this difference in the questionnaires is that school attendance rates from the LSMS are somewhat higher than for the other data sources. Note also that attendance rates are unlikely to be equal to enrollment rates, as some children may be enrolled but not attending school.

[16] Our procedure is to construct, for each district, what the FONCODES index would have been had the district-level school attendance rate been equal to the average over all districts. Essentially, this means that the modified index is based only on the seven non-attendance components of the original.

[17] One puzzling feature of Figures 6.1 and 6.2 is that attendance rates appear to have *declined* for children in well-off districts between the earlier and later years. However, this decline may be at least partly a feature of the timing of the surveys. In Peru, the school year runs from April to December. The 1993 census data was collected in June, relatively early in the school year. The 1996 INEI was collected in November. Attrition of attendance over the course of the school year could account for the lower mean

We use simple linear regression to further investigate the relationship between changes in school attendance, the FONCODES index, and FONCODES expenditures. Table 6.1 shows results for four regressions. The change in the district-level school attendance rate is the dependent variable in all of the specifications. The independent variables are the FONCODES index only (row 1), per capita FONCODES expenditures on education only (row 2), both the index and expenditures (row 3), and per capita expenditures instrumented with the index (row 4). An identifying assumption of the instrumental variables specification is that initial conditions, as measured by the modified FONCODES index, affect *changes* in school attendance only through their effect on the amount of funding received by a district.

The results in Table 6.1 suggest that changes in school attendance are consistently related to the FONCODES index: when only the index is included in the regression, a one-standard deviation increase in the index is associated with a non-trivial increase in the attendance rate of 0.043 points. Changes in school attendance are also related to differences in per capita FONCODES expenditures: when only per capita expenditures are included in the regression, a one-standard deviation increase in expenditures is associated with an increase of 0.043 in the attendance rate.[18] When both the index and expenditures are included in the regression, the effect of school expenditure decreases substantially. This is perhaps not surprising, since the FONCODES index is the main determinant of FONCODES expenditures. Finally, when per capita expenditure is instrumented with the index, the effect of education expenditures is significant and very large indeed: a one-standard deviation in expenditure is associated with an increase in the school attendance rate of 0.141 points.

---

attendance rates in the later years. Moreover, if attrition is negatively correlated with income, so that children in poorer areas are more likely to drop out during the school year than their counterparts in richer areas, our estimates could *underestimate* the effect of FONCODES expenditures on attendance. The LSMS surveys provide some evidence that attrition does affect measures of district-level school attendance rates. The 1994 LSMS was conducted between June and August, and the 1997 LSMS was conducted between September and November. In the 1994 LSMS, there is a drop in attendance rates of primary-aged children of two percentage points, from 96.6% to 94.6%, between June and July. This decline is concentrated among children in districts with a higher value of the FONCODES index. For example, for children in districts with a value of the FONCODES index greater than 14 (roughly the median), the attendance rate declined from 96% to 92%. However, there is no systematic change in attendance rates across months after July in either the 1994 or 1997 LSMS. Since the 1993 census was conducted in June, a portion of the high rate of attendance in 1993 shown in Figure 6.1 may reflect the higher attendance during the earlier part of the school year. Furthermore, given that attrition after June is more likely for poorer children, the results may understate the gains made by children in poor districts relative to those in rich districts between 1993 and 1996. As a double-check on our results, we repeated the analysis of Figures 6.1 and 6.2 using the 1994 and 1997 LSMS surveys, including and excluding observations from June. Although the results are somewhat noisier, since there are fewer districts, we found that the results based on the LSMS surveys are similar to those using the census and INEI, and that excluding June has almost no effect on the relationship between the gain in attendance and the FONCODES index.

[18] The standard deviations of the FONCODES index and per capita expenditures are 7.6 points and 9.7 soles, respectively.

These district-level results do not account for changes in school attendance patterns that are the product of changes in the characteristics of children and their families. We now turn to multivariate analysis based on the 1994 and 1997 LSMS to see whether the positive effects of FONCODES expenditures are still apparent when we control for other factors.

We consider four measures of educational outcomes in the multivariate regressions: the school attendance rate for children aged 6-13, the school attendance rate for children aged 14-16, the probability that children aged 8-10 will be at least at the appropriate grade for their age, and the amount of time (in minutes) it takes children aged 6-13 to get to school.[19] In each case (except for the measure of school proximity) we run regressions at both the "household" and "individual" levels. The "individual" regressions take a given child as the unit of observation, while the "household" regressions take a given household which has at least one child in the relevant age group as the unit of observation: for example, the first set of individual regressions estimates the probability that a child aged 6-13 is attending school, while comparable household regressions estimate the probability that all of the children aged 6-13 in a household are attending school. All of the results we present for regressions with dichotomous dependent variables (that is, those for the probability of attending school or being on track for a given age) are based on the linear probability model. The results from probit regressions (not reported) are very similar throughout.

We start by showing results of "naïve" regressions in Table 6.2. These specifications correspond to equation (2) above, when the sample is limited to observations in the 1997 LSMS (similar regressions were estimated using the 1996 INEI, with similar results). The "naïve" estimates illustrate the problems with drawing inferences from bivariate relationships between treatments and outcomes based on a single cross-section of data. The coefficients on FONCODES expenditure when no controls are included (rows 1 and 3) tend to be small and imprecisely estimated. Adding a set of controls for household and district-level characteristics (rows 2 and 4) substantially alters the results. FONCODES funding becomes positively and significantly related to school attendance for both younger and older children, although there is no relationship between spending and the likelihood of being at the right grade level, or spending and the time it takes to travel to school.

The naïve estimates may be badly biased because the targeted and demand-driven nature of FONCODES projects induces a correlation between the parameter for FONCODES expenditures and the error term in the regression. We now turn to estimates of the effect of FONCODES

[19] When testing the fraction of children at appropriate grades for their age we limit the sample to the youngest children because of the cumulative effect of repetition.

spending on the change (rather than the level) of education outcomes. Table 6.3 presents results from "difference-in-difference" regressions, corresponding to equation (4) above.[20] We report estimates of ?, the coefficient on FONCODES expenditures, and d, the coefficient on FONCODES expenditure interacted with a dummy for 1997. The coefficient ?—the "targeting" effect—measures the relationship between the outcome and the amount of FONCODES funding in 1994. The coefficient d—the "impact" effect—measures the change in the outcome between 1994 and 1997 associated with a one-sol increase in FONCODES spending. Results are shown for models with no controls; with a set of controls for household and district characteristics; and instrumental variables estimates with a set of controls.[21] FONCODES allocations, as given by the allocation rule, are used as an instrument for expenditures. An identifying assumption of this specification is that the district-level index of unmet basic needs which forms the basis of FONCODES allocations does not have a direct effect on outcomes other than through its effect on FONCODES expenditures once we have controlled for other district characteristics such as population, the fraction of children aged 6-11 who were attending school in 1993, and income.

The results provide evidence of a significant and sizeable impact of FONCODES expenditures on school attendance for *all* younger children in a household (top panel). Households in districts which received more FONCODES funding were initially less likely to send all their primary-aged children to school (the coefficient on ? is generally negative), but experienced greater increases in the likelihood that all children attended school than did households in districts which received less funding (the coefficient on d is positive and significant). The size of the estimated effect on school attendance is largest in the instrumental variables specifications: a one-standard deviation (5.2 sol) increase in per capita expenditures made by FONCODES on educational infrastructure is associated with an increase in the probability that all children in a household attend school of 0.09. Considering that median per capita consumption in Peru was about 564 soles in 1997, this is a very large effect indeed.[22] The results for individual school attendance, in the bottom panel, are smaller and less precisely

[20] We also calculated results based on specifications with district and household-level fixed effects. The estimates based on district-level fixed effects specifications are very similar to the results from the differences-in-differences specifications, although they tend to be more noisy. The results which include household-level fixed effects are all, essentially, equal to zero. This may not be surprising given the very small sample sizes: for example, there are only 376 households which were in the household panel and had primary-aged children in both 1994 and 1997.

[21] Note that the sum of the impact and targeting effects in the equations with no controls in Table 6.3 is (and must be) identical to the corresponding estimates of the impact effect from the "naïve" estimates in Table 6.2. This highlights that the naïve estimates do not properly account for the fact that districts which received more FONCODES funding had poorer initial educational outcomes.

[22] Here, too, both household consumption and FONCODES expenditures are reported in1992 soles.

estimated. The IV estimates indicate that a one-standard deviation increase in per capita expenditures increases the probability that a child attends school by 0.05.

There is less evidence of an impact of FONCODES on the school attendance of older children. Although the OLS estimates suggest that districts which received more FONCODES funding had greater gains in attendance for older children, this effect disappears when expenditures are instrumented with allocations. In other words, districts with higher *allocations* did not experience greater gains in school attendance of older children. A likely interpretation of this finding is that districts that *chose* to apply for school funds had characteristics, such as an increase in the return to education or a shift in preferences toward education, which also resulted in higher school attendance among older children.

FONCODES funding does not appear to have helped children stay at grade levels that are "appropriate" given their ages. This result may not be surprising: a program that increases school attendance may pull in older children who were not previously in school, or children who are less able to progress at the "expected" rate. Our measure of the "impact" of FONCODES expenditures mixes together the progress of children who would have been in school in the absence of FONCODES with the progress of children who would not have attended without FONCODES, and so does not provide a clean measure of (possible) increases in school quality.[23] There is also no strong evidence that FONCODES funding has reduced the time that children take to get to school. This may not be surprising, given that FONCODES expenditures were generally used to upgrade existing schools rather than to build new schools.

7. Conclusion

This paper analyzes the targeting and impact of FONCODES projects in the education sector. We use nonparametric regressions to evaluate the geographic and household incidence of FONCODES investments. Our findings show that FONCODES reached poor districts and, to the extent that they live in these districts, poor households. The targeting of FONCODES projects in education has improved over time, and compares favorably with the targeting of other public sector programmes.

In the second part of the paper, we use various estimation strategies to analyze the impact of FONCODES on school attendance rates, the probability that children are at the right school level for their age, and travelling time to school. We show that households in districts which received more FONCODES education expenditures had bigger gains in school attendance for

[23] For a similar argument about the impact of a de-worming program on educational outcomes in Kenya, see Miguel and Kremer (1999).

young children. There is no evidence that FONCODES affected the probability of being at the right school level, and only weak evidence that it decreased the average time it takes children to get to school.

There are large differences between the estimates of impact from "naïve" regressions based on a single cross-section of data and those which are based on repeated cross-sections. We argue that the "naïve" regressions are likely to be badly biased because of the targeted and demand-driven nature of a program like FONCODES. Estimates of impact which focus on *changes* in outcomes are likely to produce more accurate results, although it may still be necessary to use instrumental variables to correct for unobserved changes in district-level conditions.

The results in this paper suggest that investments in school facilities in Peru had positive effects on some educational outcomes. They thus add to a growing literature which finds evidence of a positive association between school-based inputs and measures of educational attainment (for example, Glewwe and Jacoby, Krueger, 1999; Angrist and Lavy, 1999, Case and Deaton). Nonetheless, the analysis in this paper was constrained by important limitations of the data. We would like to touch on three areas which deserve attention. First, at no point has FONCODES randomly assigned funds for education projects. As a result, various econometric strategies have to be employed in an attempt to mimic a quasi-experimental setting. Like the Bolivian Social Fund, FONCODES could consider some random assignment of education projects for a sub-sample of the population as a way of testing the robustness of the assumptions and results presented in this paper (see Newman, 1998). Second, given the absence of credible village-level measures of FONCODES funding, all of our estimates of the impact of education projects are based on district-level measures of FONCODES expenditures. Further disaggregation could be important, especially in urban districts, which can be very large. Village-level measures of "treatment" would also make it possible to use statistical matching as an estimation strategy. Finally, and perhaps most importantly, lack of disaggregated information on measures such as the time children spend in school, pupil-teacher ratios, and scholastic achievement, precluded any analysis of the impact of FONCODES education projects on school quality. Collecting such data, and understanding the mechanisms whereby improvements in school infrastructure in Peru interact with other school-level changes to result in more learning, should be an important priority for further research.

References

Angrist, Joshua D., and Victor Lavy. 1997. "The Effect of a Change in Language of Instruction on the Returns to Schooling in Morocco", Journal of Labor Economics, Vol. 15, No. 1, S48-S76.

----------, 1999. "Using Maimonides' Rule to Estimate the Effect of Class Size on Scholastic Achievement", Quarterly Journal of Economics, Vol. CXIV, Issue 2, 533-575.

Besley, Timothy, and Anne Case. 1994. "Unnatural Experiments? Estimating the Incidence of Endogenous Policies", National Bureau of Economic Research, Working Paper No. 4956.

Case, Anne, and Angus Deaton. 1999 ."School inputs and educational outcomes in South Africa", Quarterly Journal of Economics, 114, 1047-1084.

Card, David. 1990. "The Impact of the Mariel Boatlift on the Miami Labor Market", Industrial and Labor Relations Review, 43, 245-57.

----------, 1992. "Using Regional Variation in Wages to Measure the Effects of the Federal Minimum Wage", National Bureau of Economic Research, Working Paper No. 4058.

----------, 1993. "Using Geographic Variation in College Proximity to Estimate the Return to Schooling", National Bureau of Economic Research, Working Paper No. 4483.

----------, and Alan Krueger. 1994. "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania", American Economic Review, 84 (4), 772-84.

----------. 1998. "A Reanalysis of the Effect of the New Jersey Minimum Wage Increase on the Fast-Food Industry with Representative Payroll Data", National Bureau of Economic Research, Working Paper No. 6386.

Deaton, Angus. 1997. The Analysis of Household Surveys: A Microeconometric Approach to Development Policy, The Johns Hopkins University Press, Baltimore, Maryland.

Duflo, Esther. 1998. "Evaluating the Schooling and Labor Market Consequences of a School Construction Program: An Analysis of the Indonesian Experience", processed.

Fan, Jianqing. 1992. "Design-Adaptive Nonparametric Regression", Journal of the American Statistical Association, 87, 998-1004.

Friedlander, Daniel, and Philip K. Robins. 1995. "Evaluating Program Evaluations: New Evidence on Commonly Used Nonexperimental Methods", American Economic Review, Vol. 85, No. 4, 923-937.

Glaessner, Phillip J., Kye Woo Lee, Anna Maria Sant' Anna and Jean-Jacques de St. Antoine. 1994. "Poverty Alleviation and Social Investment Funds: The Latin American Experience", World Bank Discussion Paper 261.

Glewwe, Paul, and Hanan Jacoby. 1994. "Student Achievement and Schooling Choice in Low-Income Countries", The Journal of Human Resources, XXIX, No. 3, 843-64.

----------, Margaret Grosh, and Marlaine Lockheed. 1995. "An Eclectic Approach to Estimating the Determinants of Achievement in Jamaican Primary Education", The World Bank Economic Review, Vol. 9, No. 2, 231-58.

Hanushek, Eric A. 1995. "Interpreting Recent Research on Schooling in Developing Countries", World Bank Research Observer, Vol. 10, No. 2, 227-246.

----------, and Ralph Harbison. 1992. Educational Performance of the Poor: Lessons from Rural Northeast Brazil, New York, Oxford University Press.

Heckman, James, Anne Layne-Farrar, and Petra Todd. 1996. "Human Capital Equations with an Application to Estimating the Effect of Schooling Quality on Earnings", The Review of Economics and Statistics, Vol. 78, Number 4, 562-610.

Heckman, James J., Hidehiko Ichimura, and Petra E. Todd. 1997. "Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme", Review of Economic Studies, Vol. 64, 605-654.

Hentschel, Jesko, Jean Olson Lanjouw, Peter Lanjouw, and Javier Poggi. 1998. "Combining Census and Survey Data to Study Spatial Dimensions and Poverty : A Case Study of Ecuador", World Bank Policy Research Working Paper 1928.

Instituto Nacional de Estadistica e Informatica (INEI). 1996. Metodologia para Determinar el Ingreso y la Proporcion de Hogares Pobres (INEI, Lima).

Inter-American Development Bank. 1997. "Social Fund Investments in Latin America: Past Performance and Future Role", processed.

Jalan, Jyotsna, and Martin Ravallion. 1999. "Income Gains to the Poor from Workfare: Estimates for Argentina's Trabajar Program", World Bank Policy Research Working Paper 2149.

Jorgensen, Steen, Margaret Grosh and Mark Schachter. 1992. Easing the Poor through Economic Crisis and Adjustment: The Story of Bolivia's Emergency Social Fund, The World Bank, Washington, D.C.

Kremer, Michael R. 1995. "Research on Schooling: What We Know and What We Don't. A comment on Hanushek", World Bank Research Observer, Vol. 10, No. 2, 247-254.

Krueger, Alan B. 1999. ""Experimental Estimates of Education Production Functions", Quarterly Journal of Economics, Vol. CXIV, Issue 2, 497-532.

Meyer, Bruce. 1995. "Natural and Quasi-Experiments in Economics", Journal of Business and Economic Statistics, Vol. 13, No. 2, 151-161.

Miguel, Ted, and Michael Kremer. 1999. "The Educational Impact of De-Worming in Kenya", paper presented at the NEUDC Conference held at Harvard University on October 8th and 9th 1999.

Moncada, Gilberto. 1996. "Fondo Nacional de Compensación y Desarrollo Social (FONCODES)--Perú", Proyecto de Investigación Conjunta sobre los Fondos de Inversión Social en América Latina y el Caribe, Inter-American Development Bank, processed.

Newman, John. 1998. “Impact Evaluation Study: Bolivian Social Investment Fund”, processed.

----------, Steen Jorgensen and Menno Pradhan. 1991. “How did Workers Benefit from Bolivia’s Emergency Social Fund?”, World Bank Economic Review, Vol. 5, No. 2, 367-393.

Pitt, Mark M., Mark R. Rosenzweig, and Donna M. Gibbons. 1993. “The Determinants and Consequences of the Placement of Government Programs in Indonesia”, World Bank Economic Review 7, 319-48.

Rosenzweig, Mark R., and Kenneth Wolpin. 1986. “Evaluating the Effects of Optimally Distributed Public Programs”, American Economic Review, 76(4), 470-82.

Schady, Norbert R. 1998. “Picking the Poor: Indicators for Geographic Targeting in Peru”, processed.

----------, 1999. “Seeking Votes: The Political Economy of Expenditures by the Peruvian Social Fund (FONCODES), 1991-1995”, World Bank Policy Research Working Paper 2166.

Strauss, John, and Duncan Thomas. 1995. “Human Resources: Empirical Modeling of Household and Family Decisions”, Chapter 33, pp. 1883-2023, in Handbook of Development Economics, J. Behrman and T.N. Srinivasan, eds., Elsevier, Amsterdam.

Velez, Eduardo, Ernesto Schiefelbein, and Jorge Valenzuela. 1993. “Factors Affecting Achievement in primary School: A Review of the Literature for Latin America and the Caribbean”, HRO Working Paper 2, World Bank, Human Resources Development and Operation Policy Department, Washington, D.C., processed.

Webb, Richard, and Gabriela Fernández Baca. 1997. Perú 1997 en Números: Anuario Estadístico, Lima, Cuánto.

World Bank. 1995. “Did the Poor Benefit from Peru’s Economic Recovery?”, processed.

----------, 1996. “Did the Ministry of the Presidency Reach the Poor in 1995?”, processed.

----------, 1997. “Portfolio Improvement Program Review of the Social Funds Portfolio”, processed.

----------, 1998. “Implementation Completion Report: Peru, Social Development and Compensation Fund Project (FONCODES)”, processed.

----------, 1999. Poverty and Social Developments in Peru, 1994-1997, The World Bank, Washington, D.C.

Table 1.1: Total FONCODES expenditures and FONCODES expenditures on education projects, by year

Year	Total expenditures		Construction and renovation of classrooms			Other education		
	number	amount	number	amount	%	number	amount	%
1992	2813	102.7	1185	26.2	25.5	386	6.9	6.7
1993	5238	144.9	2327	49.4	34.1	430	5.8	4.0
1994	4551	110.4	2380	48.7	44.1	100	1.3	1.2
1995	3056	79.3	1037	24.7	31.1	42	0.7	0.9
1996	4222	83.4	987	15.0	18.0	14	0.3	0.4
1997	5807	114.8	607	11.0	9.6	9	0.2	0.2
1998	6088	123.8	636	12.0	9.7	1	0.0	0.0
TOTAL	31775	759.2	9160	187.1	24.6	981	15.3	2.0

Note: Only expenditures on community-based subprojects are included.
Number refers to the number of projects, amount to the amount disbursed, in 1992 soles, and % to the percentage of education expenditures as a fraction of total expenditures.

Figure 5.1: Average years of school attained by age, for children in poorest 25% of districts to richest 25% of districts

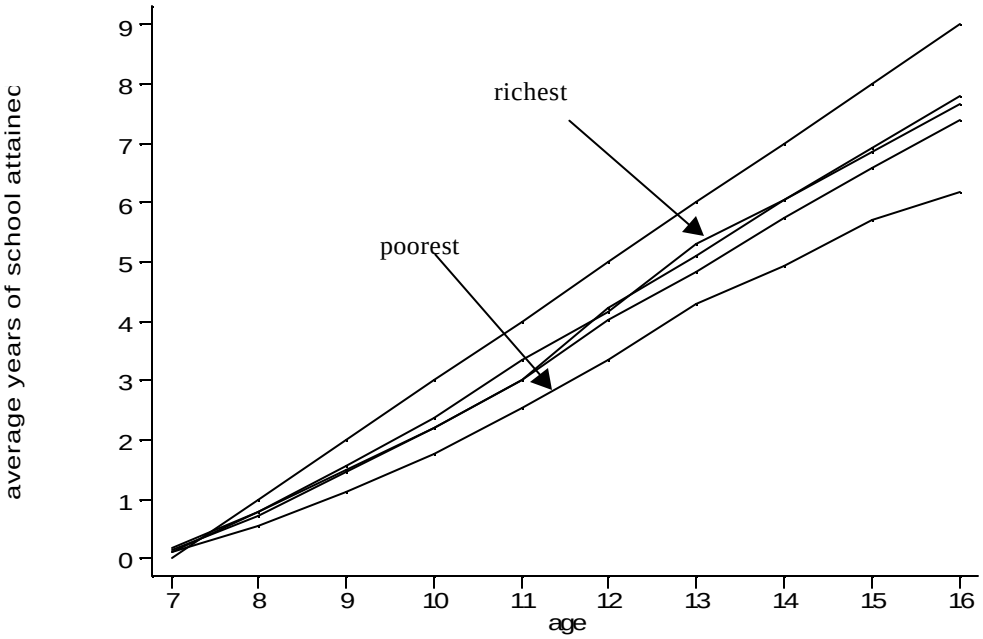

Figure 5.2: Average years of school attained by age, by income quartile

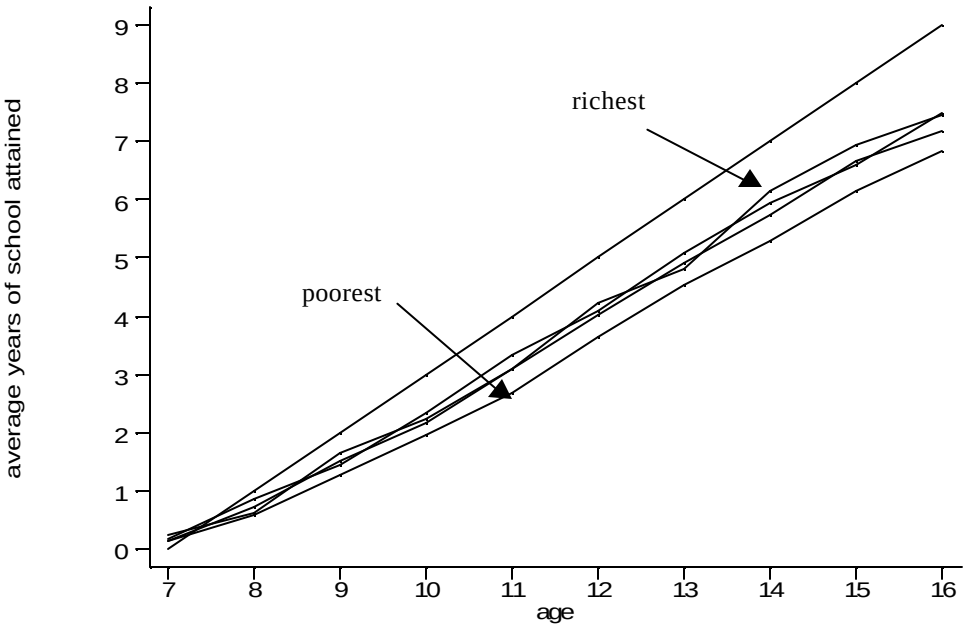

Figure 5.3: Estimated density function of the population living in districts with different mean district per capita income

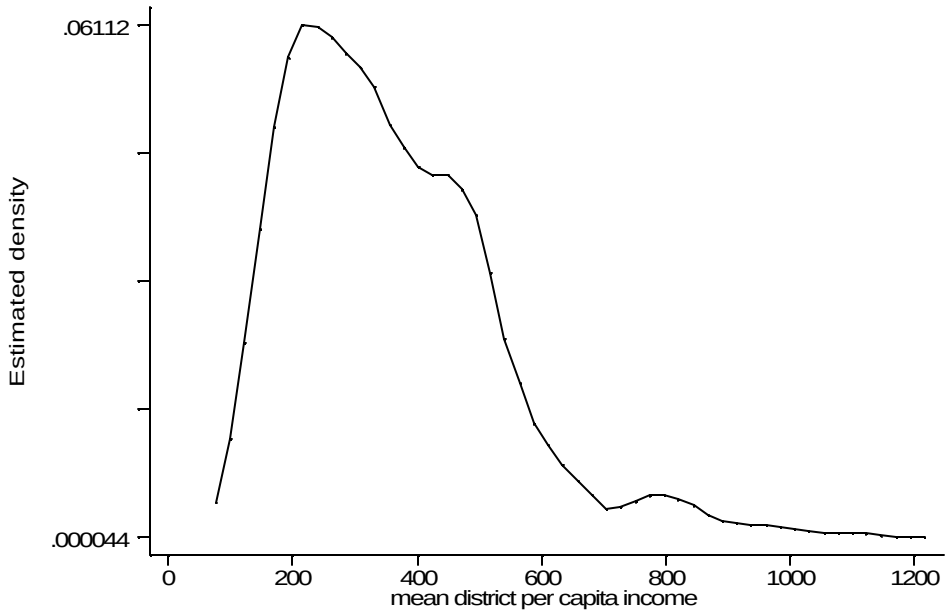

Figure 5.4: Geographic targeting of FONCODES education projects, various years

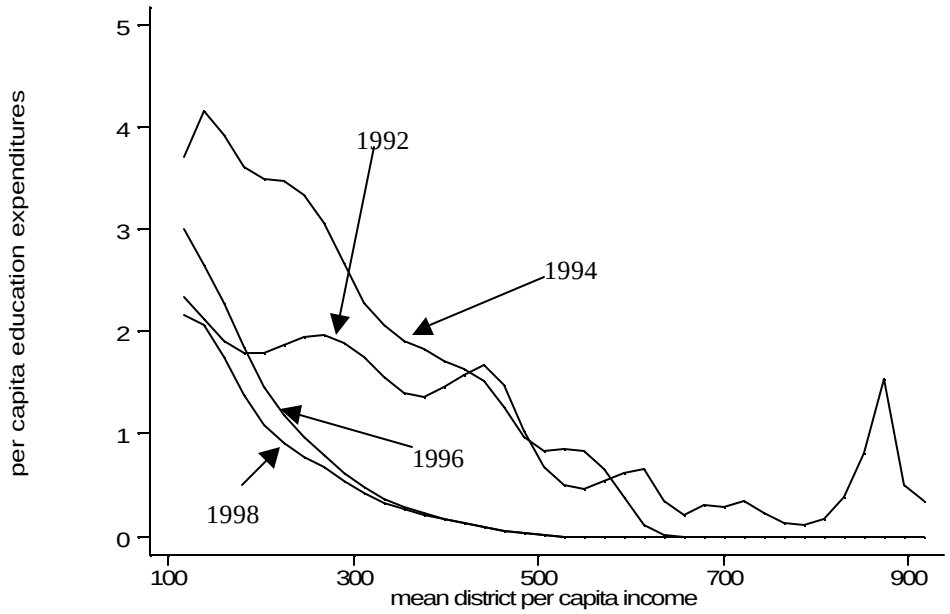

Figure 5.5: Geographic targeting of FONCODES projects in education, electrification, and sanitation, 1995

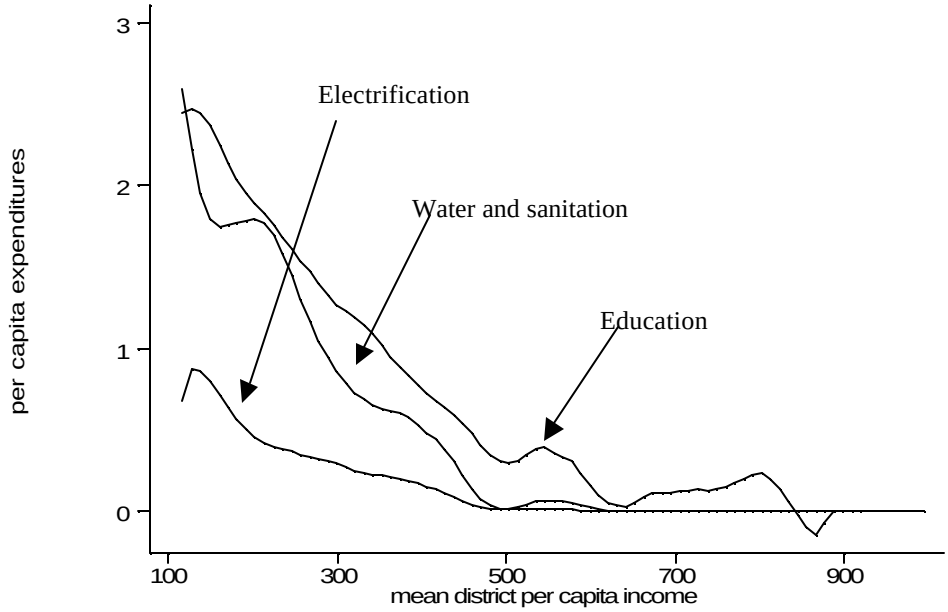

Figure 5.6: Geographic targeting of FONCODES, INFES, and PRONAA projects, 1995

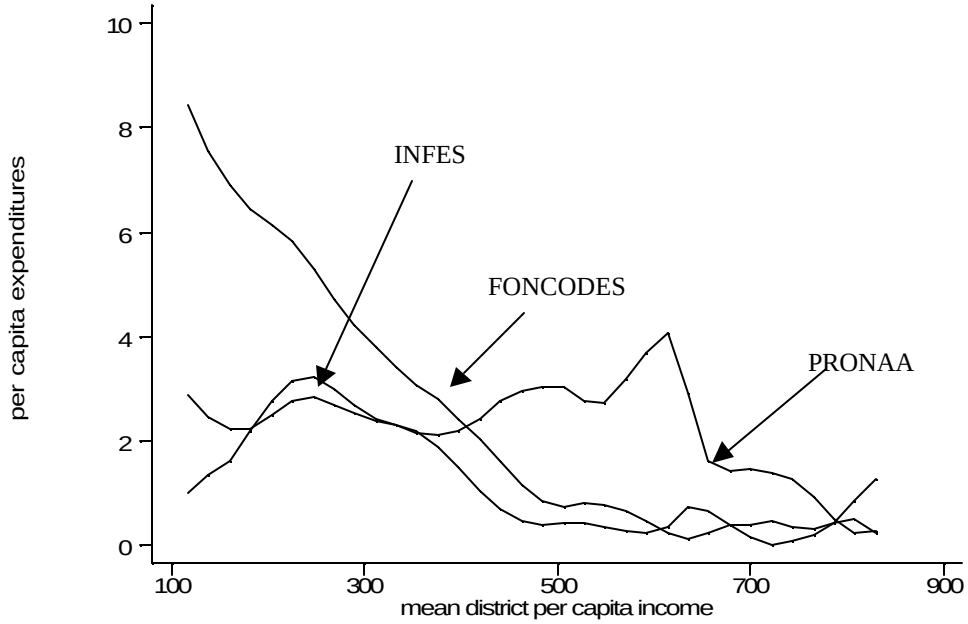

Figure 5.7: Estimated density function of the log of per capita income, Peru, 1996

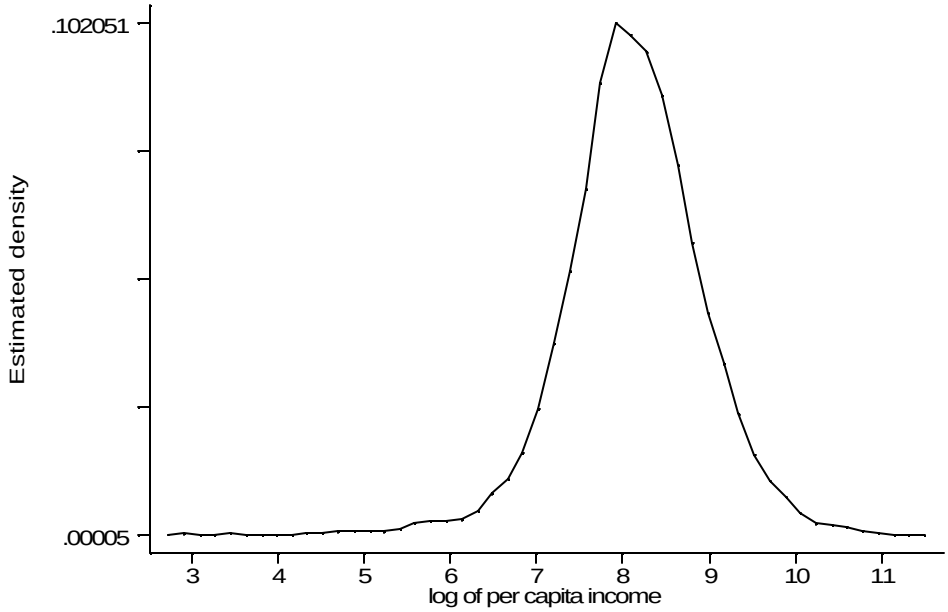

Figure 5.8: Household-level targeting of FONCODES, INFES, and the Parents' Committees

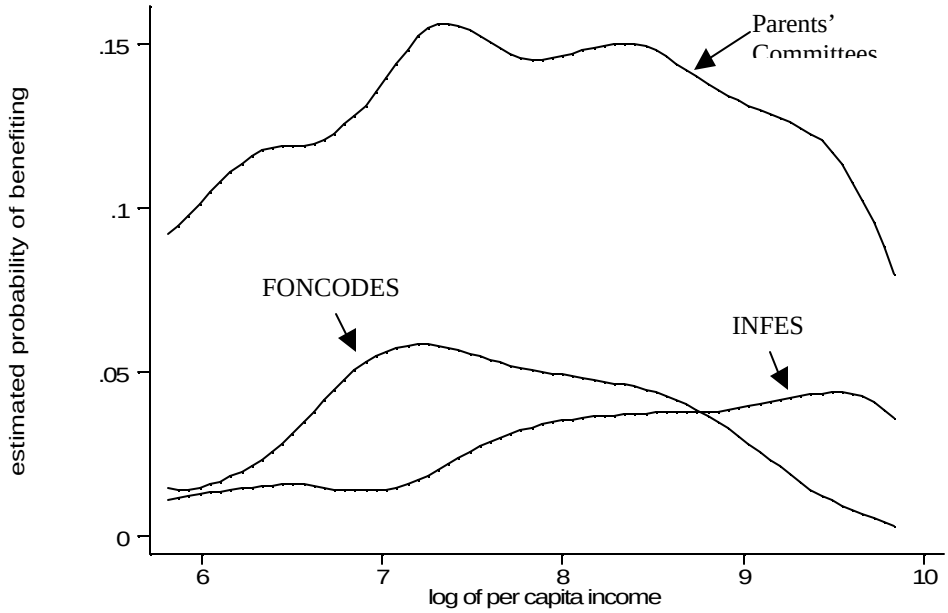

Figure 5.9: Household-level targeting of FONCODES, PRONAA, Ministry of Education and Municipal school breakfast programs

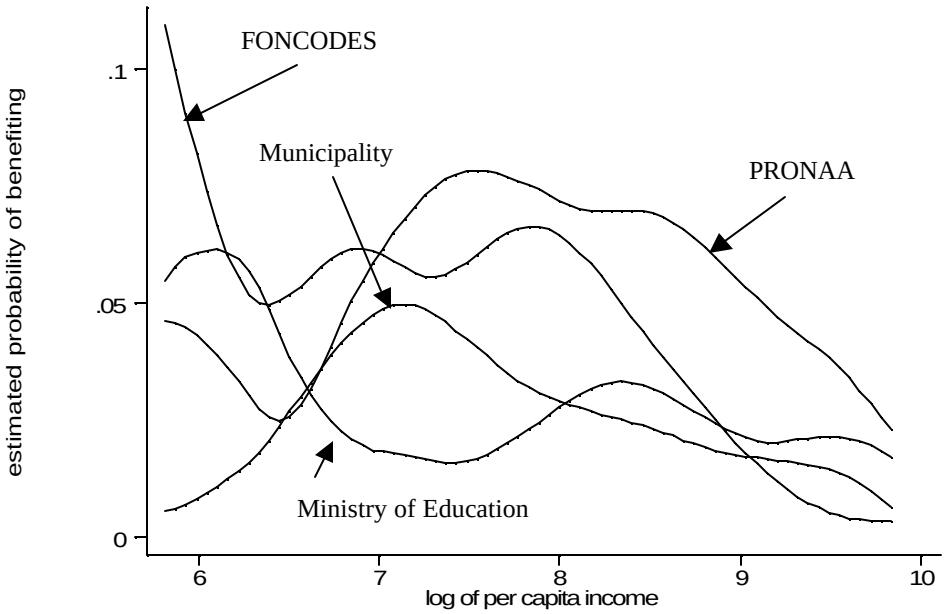

Figure 5.10: Probability of benefiting from social programs, by the number of standard deviations of household income above or below mean district income

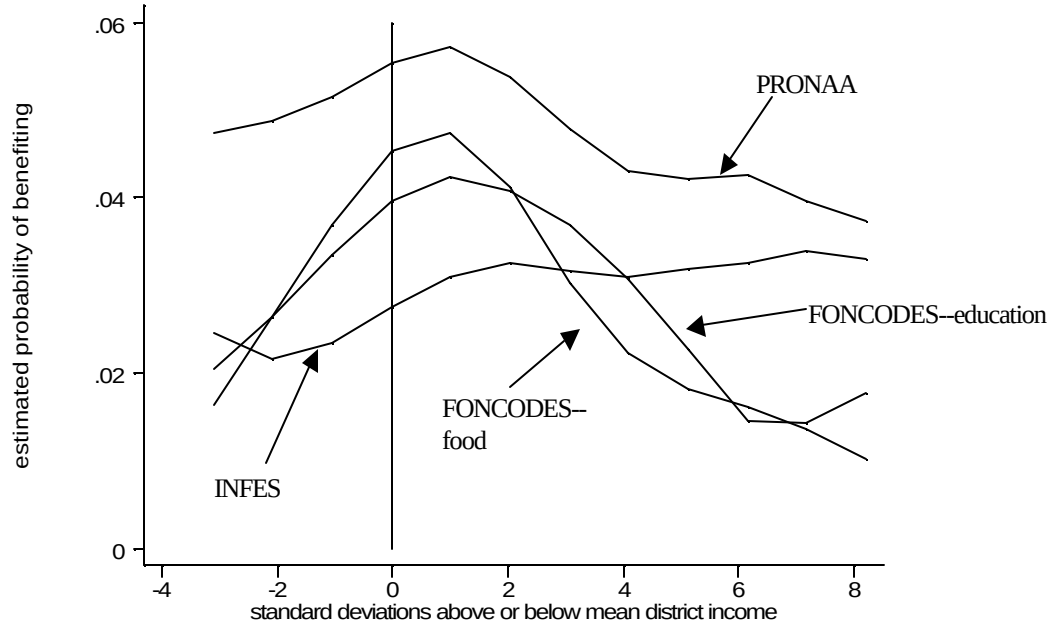

Figure 6.1: Nonparametric regressions of district school attendance rate on the (modified) FONCODES index

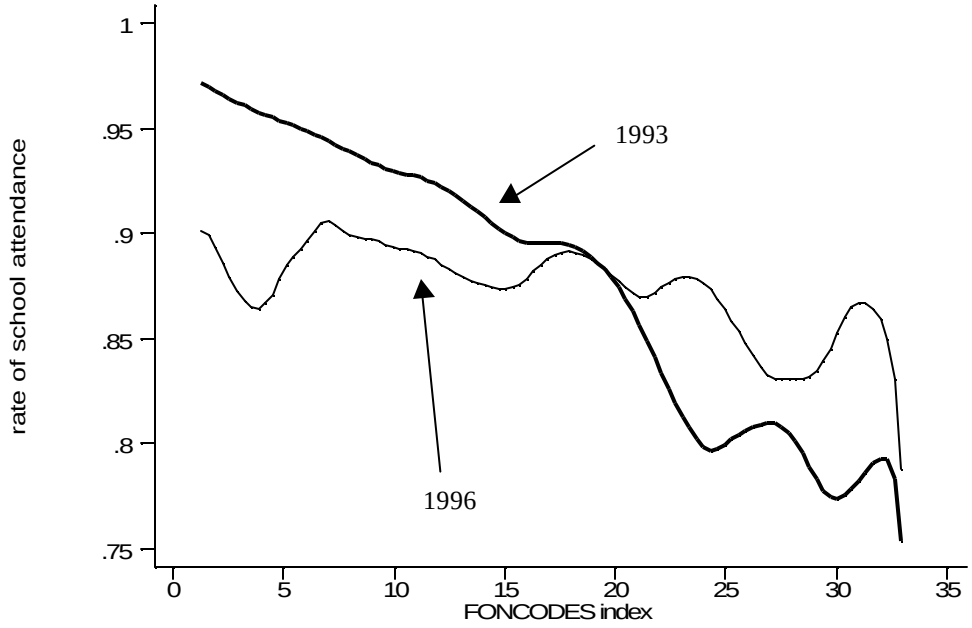

Figure 6.2: Non-parametric regressions of the change in district-level school attendance and FONCODES per capita education expenditure on the (modified) FONCODES index

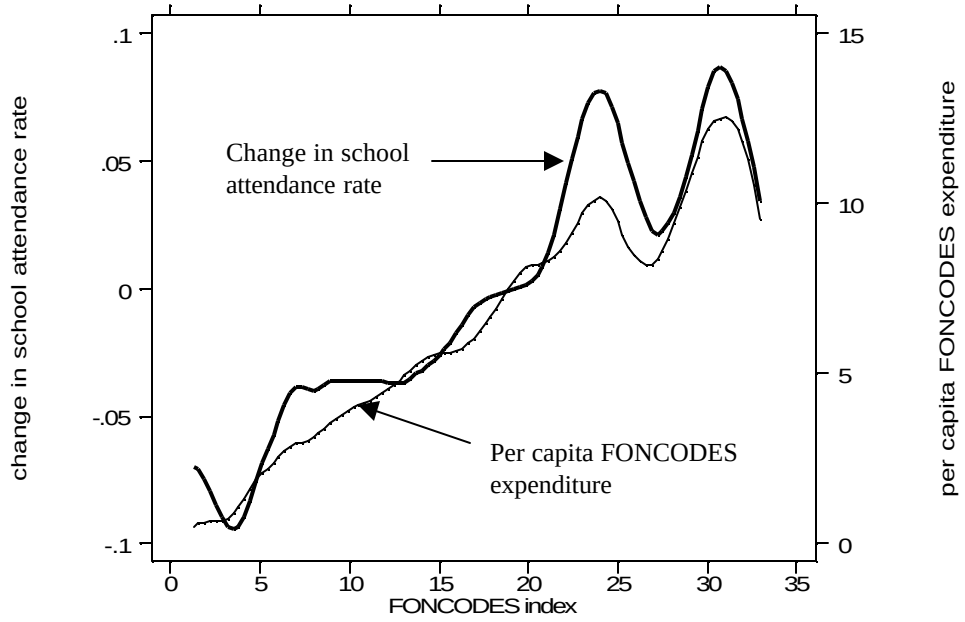

Table 6.1: Impact of FONCODES investments on school attendance rates, district-level data

	FONCODES index	Per capita education expenditure
	Dependent variable: change in school attendance rate: 1993 to 1996 (n=402)	
OLS	.0057 (7.38)	
OLS		.0044 (5.48)
OLS	.0049 (5.97)	.0018 (2.49)
IV		.0146 (6.29)

Note: The FONCODES index has been purged of the component that measures school attendance in 1993. This was done by calculating what the FONCODES index would have been for each district had it had a school attendance rate equal to the mean across districts. School infrastructure expenditure is per capita expenditure by FONCODES on school infrastructure between 1992 and 1995. Absolute t-statistics based on heteroskedasticity-consistent standard errors are reported in parentheses. In the instrumental variables regression per capita FONCODES expenditures on education is instrumented with the FONCODES index. The t-statistic on the instrument in the first stage of the IV equation was significant at better than the 1% level.

Table 6.2: "Naïve" regression estimates of the impact of FONCODES investments in education

	School attendance, children 6–13	School attendance, children 14–16	At right school year, children 8–10	Time to school, children 6–13
HOUSEHOLD				
OLS (no controls)	.0001	–.0002	–.0042	–.1755
	(0.19)	(0.10)	(1.37)	(2.21)
OLS (all controls)	.0012	.0039	-0006	–.0650
	(2.01)	(2.23)	(0.21)	(1.04)
Number	2126	1074	1211	2054
INDIVIDUAL				
OLS (no controls)	.0001	–.0003	–.0043	
	(0.28)	(0.14)	(1.56)	
OLS (all controls)	.0007	.0032	–.0012	
	(1.55)	(1.86)	(0.45)	
Number	3792	1262	1443	

Note: Absolute t-statistics are reported in parenthesis, and are based on heteroskedasticity–consistent standard errors corrected for clustering at the district level. The estimates with "no controls" include only one independent variable, FONCODES per capita expenditure on the construction or renovation of classrooms. The estimates with controls also include education of the household head, household size, the numbers of males and females in ten age categories, a dummy variable for whether the household lives in a rural area, a dummy variable for ethnicity of household head (indigenous or non-indigenous), twenty-four departmental dummy variables, a district–level measure of the FONCODES index of unmet basic needs, mean district per capita income, and district population. In addition, the individual–level regressions include dummy variables for the exact age of the child. The dependent variables for school attendance at the household level equal one if *all* children in the relevant age range are in school, and zero otherwise. These regressions include only households with children in the relevant age range. The household measure of whether children are at the right grade level for their age is defined similarly. The individual-level school attendance and grade-level variables equal one if the child is attending school (or at the right grade level), and zero otherwise.

Table 6.3: "Difference in difference" regression estimates of the targeting and impact effects of FONCODES investments in education

	School attendance, children 6–13		School attendance, children 14–16		At right school year, children 8–10		Time to school, children 6–13	
	?	**d**	**?**	**d**	**?**	**d**	**?**	**d**
HOUSEHOLD								
OLS (no controls)	–.0066	.0067	–.0156	.0154	–.0096	.0054	–.1038	–.0717
	(3.60)	(3.52)	(3.01)	(2.82)	(2.97)	(1.28)	(0.92)	(0.54)
OLS (all controls)	–.0026	.0040	–.0028	.0056	-.0001	-.0015	–.1363	–.0544
	(2.04)	(3.01)	(0.79)	(1.43)	(0.03)	(0.33)	(1.35)	(0.44)
IV (all controls)	–.0167	.0178	.0146	–.0041	.0200	-.0061	.9744	–1.181
	(1.32)	(2.43)	(0.49)	(0.29)	(0.79)	(0.44)	(1.03)	(1.92)
Number	4127		2144		2293		3987	
INDIVIDUAL								
OLS (no controls)	–.0026	.0027	–.0133	.0130	–.0085	.0042		
	(2.85)	(2.70)	(3.03)	(2.81)	(3.03)	(1.11)		
OLS (all controls)	–.0006	.0015	–.0022	.0042	.0002	-.0020		
	(0.68)	(1.42)	(0.71)	(1.25)	(0.06)	(0.54)		
IV (all controls)	–.0093	.0090	.0117	-.0074	.0264	–.0045		
	(1.09)	(1.95)	(0.45)	(0.60)	(0.99)	(0.33)		
Number	7380		2507		2746			

Notes: See notes to Table 6.2 for variable definitions and a list of controls. In the IV regressions, per capita FONCODES expenditures and its interaction with the year dummy are instrumented with per capita FONCODES allocations and its interaction with the year dummy. The FONCODES index of unmet basic needs is excluded from the IV regressions because it is highly correlated with FONCODES allocations, but other district-level measures, including mean district per capita income, the fraction of 6-11-year olds attending school, and district population are included as controls. The instruments in the first stage of the IV equations are jointly significant the 5% level or better in all of the samples.