

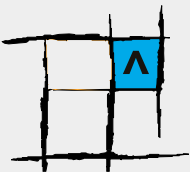


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**The Effect of Macroeconomics Crises on
Education Outcomes: Evidence from Perú**



The Effect of Macroeconomic Crises on Education Outcomes: Evidence from Peru

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Abstract

The impact of macroeconomic crises on the investments made by parents in the human capital of their children is a widely contested topic. We discuss the implications of the standard human capital investment model, and use a variety of regressions and propensity score matching methods to analyze the effects of the profound 1987-1992 macroeconomic crisis in Peru on the schooling and employment decisions taken by school-aged children. Our basic findings are three. First, there is no effect of the crisis on the attendance rates of school-aged children. Second, the proportion of children who have completed a certain number of years of schooling for their age went *up* significantly during the crisis. Third, the fraction of children employed fell dramatically during the crisis. We hypothesize that these last two findings are related: Children who are not employed may have more time available, and may therefore put more effort into school. We conclude with some discussion of the implications of these findings for the design of appropriate social protection policies.

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1. Introduction

Economic volatility, resulting from factors such as fiscal weakness, weak financial systems, changes in prices of primary commodities, and natural disasters is a reality in many Less Developed Countries (LDCs), and will continue to be a concern in the future. Countries in East Asia faced a massive economic contraction in 1998; Latin American countries generally had low or negative growth rates during the “lost decade” of the 1980s, and many countries in the region, including Argentina, Brazil, Ecuador, Mexico, and Peru faced sharp, precipitous drops in GDP; very high economic volatility is also an unfortunate fact of life for many African countries.

There has been a recent surge in academic and policy interest in the effect of macroeconomic crises on household behavior. Of particular concern are household investments in human capital. An oft-made argument is that households who are unable to smooth consumption perfectly may cut back the expenditures they make on the education, health, and nutrition of their children during a crisis. The World Bank’s flagship publication, World Development Report 2000/01: Attacking Poverty, strikes a particularly dire note: “When a crisis strikes and households cannot borrow or when adult unemployment is high or wages low, children are pulled out of school and sent to work. The lost schooling leads to a lifelong loss in earning ability for these children. Failures in the credit or labor markets thus transmit poverty and vulnerability across generations” (World Bank 2001, p. 146; see also Lustig 2000).

Despite these concerns, the empirical evidence on the effect of systemic shocks to household income on schooling outcomes is mixed: Neri and Thomas (2000) suggest that enrollment decisions are unaffected by macroeconomic conditions in Brazil, although grade attainment appears to suffer during a crisis. Cunningham and Maloney (2000) find that girls (but not boys) are more likely to drop out of school during a crisis in Argentina, while both boys and girls are *less* likely to drop out during crises in Mexico.² Using cross-country regressions, Flug, Spilimbergo and Wachtenstein (1998) report that macroeconomic shocks have negative effects on enrollment, and Behrman, Duryea, and Szekely (2000) suggest that the poor macroeconomic prospects of the 1980’s in Latin America set back the rate of growth of schooling attainment in the region. In Indonesia, Beegle, Frankenberg and Thomas (1999), Filmer et. al. (2001) and Pradhan and Sparrow (2000) all report some impact of the crisis on enrollment, although the effects tend to be small.³

² A summary of the evidence for Latin America can also be found in de Ferranti et. al. (2000, pp. 73-87).

³ Note that it is important to distinguish between the effect of *systemic* crises, such as macroeconomic contractions, and *idiosyncratic* crises, such as loss of employment or illness of a household breadwinner on schooling decisions. Systemic crises are generally associated with worse employment prospects for children, whereas idiosyncratic crises need not be. As a result, household responses may differ.

The fact that most empirical studies have not found clear, large effects of systemic crises on educational attainment may not be surprising. Households may be able to smooth consumption or, at least, to protect critical items in their budgets. Moreover, a severe economic crisis may so depress job prospects and wages for school-aged children that outside employment becomes a much less attractive option.

This paper analyzes the effect of the massive, prolonged economic crisis in Peru between 1987 and 1992 on school attendance, the probability of having completed a certain number of years of schooling for age, and the probability of employment for school-aged children. The Peruvian case is appealing from an analytical point of view for a number of reasons. First, the crisis in Peru was particularly deep and prolonged: National accounts data suggest that GDP per capita fell by almost 30 percentage points between 1987 and 1992. The depth and duration of the crisis could severely stretch the ability of households to protect investments in schooling. Second, reliable household data are available for Peru. Three Living Standards Measurement Surveys (LSMS) cover the period before the crisis (1985/86), during the crisis (1991), and after the crisis (1997). These LSMSs include detailed information on household characteristics, including demographic structure, income, consumption, access to credit, and parental education, and child outcomes, including educational attainment and employment. The length of the questionnaires and therefore the potential list of controls is much richer than that found in the labor force surveys which have typically been used for analyses of crisis impact (for example, in Brazil and Mexico), although the sample sizes are also smaller.

The rest of the paper proceeds as follows. In section 2, we present a theoretical framework to understand the impact of macroeconomic shocks on educational attainment. Section 3 briefly discusses the Peruvian setting during this period. Section 4 describes the data used for the analysis. In section 5 we describe our econometric specification, and present results based on a variety of regression and propensity score matching methods. Section 6 concludes.

2. Theoretical framework

In the standard neoclassical model of human capital investment, individuals acquire schooling until the (expected) marginal benefit to an additional year of education equals the marginal cost. Implicit in this model are the assumptions that markets are complete, the only cost of schooling is forgone earnings, and students earn nothing while in school, (Rosen 1977; Willis 1986).

In the context of LDCs like Peru, the standard model is potentially missing two important features of the schooling decision problem. First, part-time earnings of students can play a

significant role in the decisions of individuals, especially in the presence of temporary economic shocks. Second, the schooling decision may be affected by borrowing constraints. Low-income households, in particular, are more likely to both be unable to borrow and to lack “internal finance” for education purposes. It has long been recognized (Becker (1964)) that the presence of borrowing constraints increases the marginal cost of attending school, and leads to under-investment in human capital.

To assess the effects of aggregate economic fluctuations on schooling choices in the context of LDCs we first consider the model used by Card and Lemieux (2000), which supplements the standard neoclassical model with part-time earnings and other direct costs. This allows us to illustrate how aggregate fluctuations affect marginal costs and benefits from schooling. We then discuss the effect of borrowing constraints on the schooling decision.

Assume that individuals choose schooling to maximize the present discounted value of future earnings net of education costs. Let $y(S,t)$ denote the real earnings associated with S years of schooling in period or age t ($t \geq S \geq \text{Age}-6$, so that age is measured as $\text{Age}-6$). Note that for a student with normal school progression, S and t are the same index during the period of school enrollment.⁴ At age t , a student enrolled in schooling level S pays tuition and other direct cost $T(S)$ and can earn $p(S,t)$ in part-time earnings. The marginal cost of an additional year of schooling is then given by:

$$MC(S) = y(S,S) - p(S,S) + T(S) \tag{1}$$

The marginal cost includes two components: The direct cost $T(S)$ associated with tuition, transportation, books and other educational expenses, and the forgone earnings, net of part-time student earnings $y(S,S) - p(S,S)$. The marginal cost will be increasing in years of schooling (or age) if net forgone earnings or tuition increase with schooling. Net forgone earnings will rise with schooling (or age) provided the gap between $y(S,S)$ and $p(S,S)$ is increasing in S .⁵

The marginal benefit is given by the increase in the discounted expected stream of earnings resulting from one more year of schooling. In principle, this benefit should depend on the actual schooling level achieved, the ability of the student, and the discount rate. Individuals will invest in one more year of schooling as long as the marginal benefit from doing so is greater than the marginal cost.

⁴ In a more general model allowing for interruptions in schooling investments, t and S will be different.

⁵ Direct costs may also be rising in schooling because of higher costs of tuition, transportation or other direct expenses at higher education levels.

What is the effect of an adverse macroeconomic shock on the schooling decision? If a crisis depresses current employment and wage prospects (both from full-time and part-time employment), the opportunity cost of attending school represented by $y(S,S) - p(S,S)$ will fall. Holding everything else equal, this should lead to an increase in human capital investment.

When macroeconomic shocks are persistent, they may also depress expected lifetime earnings, and have an effect on the marginal benefit from schooling. If the lifetime earnings of all individuals are reduced by the same percentage, regardless of their schooling, then the marginal benefit associated with an additional year of schooling will be lower (by this same percentage). But crises need not have such a uniform across-the-board effect on expected earnings. For example, if the expected lifetime earnings for individuals with less education are disproportionately affected by the crisis, the marginal benefit to schooling could rise. The effect of a macroeconomic shock on the marginal benefit is therefore indeterminate, and could lead households to invest in more or less schooling.

The schooling decision problem can be illustrated more precisely if we assume that individuals do not interrupt their school progress. The relevant objective function in the case of a once and forever schooling decision is given by:

$$V(S) = \int_0^S [p(t,t) - T(t)]e^{-rt} dt + \int_S^{\infty} y(S,t)e^{-rt} dt \quad (2)$$

The marginal benefit – derived from (2) – associated with an additional year spent in school is:

$$MB(S) = \int_S^{\infty} \frac{d}{dS} y(S,t) e^{-r(t-S)} dt \quad (3)$$

Assuming that the marginal costs of an additional year of schooling rise faster than the marginal benefits, the optimal schooling choice is determined by the condition $MC(S)=MB(S)$. Under the assumption that log earnings are additively separable in years of education and years of post-schooling experience (Mincer 1974), log earnings are determined by integrating the marginal benefit schedule:

$$\log y(S,t) = a_i + b_i(S)S_i - 0.5kS_i^2 + g(t) \quad (4)$$

where $g(t)$ is concave function of labor market experience, a_i is the absolute advantage of individuals (or unobserved ability), which generates higher earnings at all schooling levels, and

the heterogenous slope of the earnings function b_i represents the comparative advantage of individuals, which confers higher returns to schooling. Equating marginal benefit and cost and solving for schooling yields the optimal schooling choice:⁶

$$S_i = \frac{b_i(S) - r_i(S)[1 - \theta(S) + \omega(S)]}{k} \quad (5)$$

where $\theta(S)=p(S,S)/y(S,S)$ is the ratio of part-time student earnings to full-time earnings for a person with S years of schooling and age $S+6$, and $\omega(S)=T(S)/y(S,S)$ is the ratio of tuition costs to full-time earnings associated with schooling level S .⁷ In this model, schooling decisions are determined by individual specific variables (like the expected benefit to an additional year of schooling and the discount rate) *and* aggregate variables (like tuition costs and net forgone earnings). Note that economic shocks, depending on their nature, can affect both individual and aggregate variables.

The discussion so far assumes that there are no constraints to borrowing for human capital investment. In a more general model which includes preferences over consumption, and under the assumption that parents altruistically treat the consumption of their children as their own, Jacoby (1994) shows that borrowing constraints prevent perfect smoothing of consumption, increase the marginal cost of attending school, and lead to lower investment in human capital. Because poor households have less access to credit in Peru, they start withdrawing their children from school earlier.

In sum, macroeconomic crises can affect schooling decisions through three distinct channels: Increasing the opportunity cost of attending school, having an indeterminate effect on the marginal benefit of an extra year of schooling, and increasing the probability that borrowing constraints become binding. We therefore cannot determine unambiguously how schooling decisions are affected by a crisis—even if this crisis is perceived as temporary and has only a small effect on the marginal benefit from schooling: The lower opportunity cost of attending school and the increasingly binding credit constraint work in opposite directions.

How does the response of schooling decisions to crises depend on family income? In general, lower income households will be less able to smooth out the effect of the shock on their investments in human capital. A crisis may also affect some jobs more than others: For instance, if a crisis destroys relatively more of the jobs available to children from poor households, it

⁶ From the specification of equation (3), the marginal benefit is given by $1/r_i[b_i - kS_i]y(S,S)$.

should have a larger effect on the opportunity cost associated with attending school for poor households, and lead poor households to increase their investments in human capital by more than their better-off counterparts. We consider this question by comparing the responses of households in different income quintiles.

The model so far assumes that individuals make a single decision about when to leave school. Card and Lemieux (2000) suggest that in models with potential interruptions in schooling, current schooling decisions are more sensitive to variations in the current marginal cost of schooling. These variations in marginal costs could result from a number of reasons, including poor aggregate economic conditions. The intuition behind this is that dropping out today does not exclude the possibility of returning to school when the marginal cost is lower. The predictions of the theoretical framework described above might therefore understate the responsiveness of individual schooling decisions to temporary shocks, including macroeconomic shocks, although this has no implications for our empirical framework.

3. The Peruvian setting

Economic developments, 1985-1997: Peru followed an erratic economic course in the 1980's and 90's. Between 1985 and 1990, the government of Alan Garcia attempted to stimulate the economy with a "heterodox" stabilization program, which relied, inter alia, on reduced foreign debt payments, a price freeze, and economic reactivation via wage increases, job creation programs, and increased investments in education and health. These policies encouraged high growth rates in 1986 and 87, but the inflationary pressures and budget deficits quickly proved unsustainable (Glewwe and Hall 1994). As a result, the country slid into a deep recession and hyper-inflation in 1988. By the end of Garcia's government in 1990, the economy was in a state of near-collapse: GDP per capita had fallen by -10.5%, -13.4%, and -6.9%, and inflation had reached an incredible 667%, 3399%, and 7482%, in 1988, 89, and 90, respectively (see Figure 1). The economic crisis came hand in hand with a deterioration in public safety: By 1990, there was a state of virtual civil war between the armed forces and the insurgencies of Shining Path and Tupac Amaru Revolutionary Movement (MRTA) in some areas of the country, and the conflict was claiming at least 2,000 lives per year (INEI 1996, p. 58).⁸

⁷ If tuition is free and students earn nothing while in school, the equation simplifies to the familiar form $S_i = (b_i - r_i)/k$, so that differences in educational attainment arise solely because of variation in the return to schooling ($b_i - kS_i$) and in the discount rate (r_i).

⁸ These figures are based on incidents reported by the Ministry of Interior, but are likely to be underestimates. Other estimates place the number of deaths from terrorism in 1990 above 3,000 (see, for example, World Bank 1995).

The Fujimori government which took office in 1990 opted for more orthodox economic remedies. Economic reforms included the elimination of controls on prices, interest rates, and foreign exchange transactions, the reduction of tariffs, labor market de-regulation, and a far-reaching program of privatization. The new policies had an immediate effect on inflation, which fell to 74% in 1992, and to less than 12% by 1995. Growth was sluggish in 1991 and 1992, but picked up thereafter (Figure 1). Poverty decreased steadily between 1991 and 1994, and again between 1994 and 1997. Like the crisis, the recovery appears to have been far-reaching, affecting all regions and most households (World Bank 1995 and 1999a).⁹ Meanwhile, order was gradually re-established in the countryside, in particular after the capture of Abimael Guzman, Shining Path's leader, in 1992.

Education in Peru, 1985-1997: In comparison with many of its Latin American neighbors, and with other countries with similar income levels, Peru has managed to attain impressively high enrolment rates in both primary and secondary school: Analysis of the 1997 LSMS suggests that 97.5% of 6-11 year-olds, 86.3% of 12 to 17 year-olds, and 34.9% of 18 to 25 year-olds were attending some educational institution.¹⁰ These figures are particularly impressive because of the low levels of public spending on education: In 1997, total public spending on education in Peru was 3% of GDP (2.4%, net of spending on pensions), compared with the Latin American average of 4.5% (World Bank 1999b).¹¹

Private expenditures on education in Peru are about 2% of GDP, well above the OECD average of 1.3%. As in other countries, these expenditures vary across education levels and income quintiles. In 1997, households spent about 3 times as much to send an 18-25 year-old to school as they did to send a 6-11 year-old. (Most of the older individuals would be attending university or some other tertiary institution, while the younger children would be in primary school). Differences across income quintiles are starker: On average, households in the richest

⁹ Strict comparisons in poverty measures are not possible because of differences in the coverage of the surveys (see Section 4).

¹⁰ Authors' calculations, based on the 1997 LSMS tapes. Note that these numbers do not correspond to those in Table 1 for two reasons. First, these are numbers for all seven regions in Peru, including rural areas and the urban jungle, whereas the numbers in Tables 1a and 1b correspond to Lima and the urban areas of the coast and sierra only. Second, these are means weighted by the appropriate (individual) expansion factors, while the figures in Table 1 are unweighted means. Figures for school attendance for 6 to 14 year-olds based on the 1996 Peru Demographic and Health Survey (DHS) can also be found in Filmer (1999). These tend to be quite similar, except for 6 year-olds, which appear to have much lower enrollment rates in the DHS (61.3%), likely as a result of differences in the wording of the question.

¹¹ A recent review of the education sector in Peru concludes that there are four important reasons for these accomplishments: (i) relatively equitable distribution of public resources (so that these also benefited poorer households); (ii) the ability to control the growth of personnel expenditure; (iii) the high level of private expenditures on education; and (iv) the fact that virtually universal primary enrolment was achieved before improvements in quality and expansion of higher education levels (World Bank 1999b).

income quintile spent about 8 times as much for every child aged 6 to 11 attending school as did households in the poorest income quintile, 5 times as much for those aged 12 to 17, and 4 times as much for those aged 18 to 25. Still, by international standards, these differences in the expenditure patterns across income quintiles are modest.¹²

In Peru, education at the primary and secondary levels is dominated by the public sector: Ministry of Education figures suggest that 88.2% and 84.0% of students in primary and secondary school, respectively, attend a public school; these percentages have been virtually unchanged since 1990, when they were 88.3% and 85.6%. The private sector plays a much larger—and growing—role at the tertiary level: 43% of all tertiary education students (university and non-university) were enrolled in a private institution in 1997, up from 37.6% in 1990 (World Bank 1999b, Volume 2, p. 53).¹³

Public provision of education has traditionally been quite centralized in Peru. In 1991, however, the government initiated a policy of deconcentration to twelve regional governments. As a result, a growing fraction of resources has been transferred directly to the regions: In 1990, the Ministry of Education managed 71% of total public expenditures on education, and the regions 17%, but by 1997 these figures had been reversed, with the ministry of Education managing 25%, and the regions 56% (World Bank 1999b, Vol. 1, p. 16).¹⁴ The regional governments which manage education do not have capacity to raise own revenues, however, and are appointed by the central government rather than elected.¹⁵

4. The data set

The main sources of data used for this paper are the 1985/86, 1991, and 1997 Peru Living Standards Measurement Surveys (LSMS). These surveys contain reliable information on household characteristics, school attendance, grade attainment and labor force participation.¹⁶

¹² World Bank 1999b, Vol. 2, page 164, presents a table for Peru in which household expenditures per student are broken down by education level (primary, secondary, tertiary). The differences across quintiles reported in this table are larger than the ones we report (for example, households in the richest quintile spend 12 times as much per student in primary school as do households in the poorest income quintile). It is not clear, however, how this disaggregation by level was conducted given the order of the questions in the 1997 LSMS. As a comparison, in the Philippines households in the richest quintile spend 20 times as much per student in primary school as households in the poorest quintile, 11 times as much per student in secondary school, and 11 times as much per student at the tertiary level (Schady 2001, p. 34).

¹³ See the caveat given in Section 4 on the reliability of enrollment and repetition statistics provided by the Ministry of Education.

¹⁴ Autonomous universities and other government bodies manage the remaining 19%.

¹⁵ Municipal governments at the province and district levels, by contrast, are elected.

¹⁶ The 1985/86 LSMS was carried out by the Peruvian Statistics Institute (Instituto Nacional de Estadística e Informática—INEI), while the 1991 and 1997 LSMS were carried out by the Peruvian thinktank Cuanto.

The coverage of the surveys is not comparable across years. While the 1985/86 and 1997 LSMS visited households in each one of Peru’s seven “natural regions”—Lima, and the urban and rural areas of the coast, sierra (or highlands) and selva (or jungle)—the 1991 LSMS sampled households only in Lima, the urban coast, and the urban and rural areas of the sierra. Unfortunately, there is also some reason to be concerned about the 1991 sample for the rural sierra: Mean income in the rural sierra appears to be *higher* in 1991 than in 1997, and the mean years of education of parents in the rural sierra approximately doubles between 1985/86 and 1991, and stays constant thereafter (see Table 1a below). Our suspicion is that lack of funding or the dangerous conditions caused by widespread terrorism in the rural sierra in the early 1990’s prevented enumerators from visiting outlying, poorer, less well-educated households in this region in 1991. We therefore limit the sample to Lima and the urban areas of the coast and sierra for all of our analysis.¹⁷ However, all of the results we report in this paper would be stronger had we included the rural sierra in our analysis.

The questionnaires in all three surveys are generally comparable.¹⁸ The Peruvian thinktank GRADE has calculated comparable consumption and income aggregates for the 1985/86, 1991, and 1997 LSMS, as well as for an LSMS conducted in 1994. Specifically, GRADE used 1997 price deflators to adjust for price differences across natural regions, and the national CPI to deflate consumption and income over time. We use GRADE’s income aggregates for the estimations in this paper. We do not use the 1994 LSMS for our analysis because the GRADE aggregates suggest that mean income in this year was lower than in 1991, while the national accounts show a clear improvement in income per capita during this period. It is therefore unclear whether 1994 should be treated as a “crisis” year.¹⁹

In addition to income, we take variables from the LSMS on household composition and size, age, and access to credit. We also have information on the years of completed education of both parents (separately) for a sub-sample of children—specifically, those who live in households in which both parents are present and whose relationship to the household head is son or daughter. Finally, we have identifiers with which to map households in the surveys to the districts in which they live. There are more than 1800 districts in Peru, and these are the smallest political and administrative division in the country (roughly comparable to counties in the United

¹⁷ In 1997, these three regions jointly accounted for approximately 58.1% of the population of the country. (Authors’ calculations based on the expansion factors included in the data files of the 1997 LSMS.)

¹⁸ There are some unfortunate exceptions. For example, there are differences across surveys in the recall periods and in the level of disaggregation that is asked in the questions concerning household expenditures on education.

¹⁹ See Deaton 2000 for an analysis and possible explanations of such divergences between the survey and national accounts figures for India in the 1990’s.

States).²⁰ We use these identifiers to construct a district-level panel to control for unobserved heterogeneity across districts.²¹

We made several attempts to supplement the data in the surveys with administrative data, with only limited success. We use national accounts data for a general description of national economic trends in the 1980's and 1990's. Up to 1995, regional branches of INEI also computed estimates of GDP for each one of Peru's 25 departments. These data would have been useful for a difference-in-difference analysis of the impact of the crisis. Unfortunately, combining these data with the survey data raises serious problems of measurement error. The department-level pattern of crisis and recovery which comes out of the national accounts data is strikingly varied—indeed, so varied as to seem less-than-credible given anecdotal evidence about the depth of the crisis throughout the country.²² There are also problems merging these data with the household-level data from the surveys. Only 15 departments were covered in both the 1985/86 and 1991 surveys, and the sample sizes vary wildly across departments—ranging from more than 2,000 individuals between ages 6 to 17 in Lima to less than 50 in some of the smaller departments. A further source of measurement error is introduced by the fact that the department-level GDP figures purport to cover both urban and rural areas, whereas the analysis conducted in this paper is based on the urban samples of Lima, the coast, and the sierra only. For these reasons, we do not provide difference-in-difference estimates of the impact of the crisis.

We use data provided by the Ministry of Education on total (public) recurrent and capital expenditures on education during the period to inspect whether changes in school outcomes could be attributed to changes in spending patterns, rather than to changes in the economic environment. These data are considered to be reasonably accurate and comparable over time. Finally, we attempted to use administrative data on enrollment and repetition levels, but discarded these as unreliable. Administrative data on enrollment in various grade levels is collected in Peru from the school headmasters themselves. This creates a serious incentive problem, as

²⁰ According to the 1993 population census, the average district population was about 12,600 inhabitants. Some, predominantly rural districts have less than 200 inhabitants, whereas a handful of urban districts in Lima have more than 100,000 inhabitants. See Schady 2000, p. 3.

²¹ Note that district fixed effects are *not* a satisfactory solution for the problem of differences in the sample in the rural sierra between, on the one hand, the 1985/86 and 1997 surveys and, on the other hand, the 1991 sample. The likely problem with the 1991 sample for the rural sierra is that enumerators may not have visited outlying areas *within* a given district, rather than not visiting outlying districts.

²² This large regional variation in the estimates occurs even though INEI “imputed” part of the department-level flows using data for the entire country: For example, in some economic sectors, national changes are attributed to different departments according to the fraction of the stock of total employment or total industry output located in each department. In general, one would expect that this procedure would tend to make estimated department-level growth rates more, not less, similar to each other.

headmasters tend to inflate enrollment figures to receive larger transfers.²³ Moreover, these (highly unreliable) figures are available for only three years—1993, 1998, and 1999. The administrative enrollment and repetition figures for other years in the 1980’s and 1990’s are based on imputations which statisticians at the Ministry of Education themselves do using the LSMS surveys.

5. Results

Construction of variables: The theoretical framework in section 2 relates macroeconomic shocks to the total amount of schooling chosen by individuals (or their parents). Unfortunately, it is too early to determine the effect of the 1987-92 crisis in Peru on “final” educational outcomes. We therefore evaluate the impact of the crisis on two “proximate” determinants of total schooling attained: Attendance rates, and the probability that children of a given age have completed an appropriate number of years of schooling for their age. We believe that these are reasonable intermediate variables because children will only complete less total schooling if they start school late, drop out of school early, or pass fewer grades during their time in school. A positive or zero estimated effect of the crisis on attendance and completion rates would therefore be consistent with the hypothesis that the crisis did not have a negative impact on total schooling. On the other hand, a negative estimated effect of the crisis on attendance or grade completion would not be conclusive because children could return to school at a later point, or stay in school longer to compensate for their slower progression.

We define attendance based on two questions in the LSMS: All household members aged 6 or older are first asked whether they are “currently attending school or studying something”, and those who answer “no” are then asked whether they “attended school or studied something in the last 12 months”.²⁴ Our variable for attendance takes the value of one for all individuals who answer “yes” to either one of these questions. Note that it is important to take into account *both* these questions, as a large fraction of households in the 1985/86 LSMS were surveyed during the summer vacation months of December through March, so that attendance rates based on only the

²³ The last time that the Ministry of Education figures could be checked, by comparing them with the figures from the 1993 population census, there were estimated to be almost one million “ghost” students in the statistics supplied by the headmasters themselves. (Author’s conversation with Cesar Guadalupe, head of the statistics unit in the Ministry of Education).

²⁴ This measure of “attendance” does not take into account the number of times children are actually going to school—a shortcoming of the data. Using the term “enrollment” would also be somewhat inaccurate, however, as children may be formally enrolled in school, but not attend. The Spanish word used in the questions in the surveys (“*asistir al colegio*”) is closer to attendance than it is to enrollment (the translation for enrollment would be “*estar inscrito en el colegio*”). See Paxson and Schady 1999 for a similar construction of variables.

first question in this survey would appear to be unreasonably low. We analyze attendance patterns separately for children aged 6 to 11, roughly corresponding to primary school, and children aged 12 to 17, roughly corresponding to secondary school.

The variable we construct for completion of a standard number of years of schooling for age, which we refer to as being “on-track”, is based on questions about age and last grade in school completed. Children in Peru are expected to start first grade at age 6 or 7. Therefore, unless they start school late or repeat a grade, they should have completed first grade by age 8, second grade by age 9, and so on. We consider children to be on-track if they have completed *at least* this number of school years for their age.

Simple comparisons of the likelihood of being on-track between 1997 and earlier years are difficult because of the cumulative nature of late entry and repetition. We can therefore only compare the 1997 results with those for 1985/86 and 1991 for children who were too young to be attending school during the crisis years of 1987-1992. Specifically, we assume that children born in 1986 or later, who would be 5 years old or younger in 1991, and 11 years old or younger in 1997, were unaffected by the crisis, while their older counterparts may not have been. Moreover, children aged 7 or younger will be on-track by definition, since they are only expected to have completed one school year by age 8. For both of these reasons, we limit most of our comparisons of the probability of being on-track to children aged 8 to 11.²⁵

Our measure of employment is based on separate questions asked of all household members aged 6 or older. These questions first ask respondents whether they worked “as an employee for a business, corporation, government, a boss, or another individual”, and second, whether they worked “for themselves” (“a cuenta propia”, or self employed), “as an unpaid family member, or on the (family) farm”. The reference period for both questions is the last week; follow-up questions with longer reference periods (12 months) are asked of those who did not work in the last week. For the purposes of this paper, we consider all those who answer affirmatively to *any* of the four questions to have been employed.²⁶ As with attendance, we conduct our analysis of employment separately for 6 to 11 year-olds, and 12 to 17 year-olds.

Sample means: We begin our analysis in Tables 1a and 1b by showing unconditional, unweighted means for school attendance, completion of a standard number of years of schooling

²⁵ We could also have made age-specific comparisons between children older than 11 years of age in 1997, who were exposed to the crisis, and children older than 11 years of age in 1985. We did not make these comparisons because of the cumulative effect of late entry and grade repetition.

²⁶ In the 1985/86 and 1991 surveys, working “on the farm” and working “for themselves, or as an unpaid family member” are asked separately, whereas in the 1997 survey both options are listed as part of the same question. Household chores are meant to be excluded from these categories, and are asked about in the following questions.

by age, employment, and a number of variables which are likely to have an effect on schooling decisions, by survey year.²⁷ Table 1a presents results for the “full” urban sample (not including the urban selva, which was not included in the 1991 survey), and for each strata separately. Table 1b breaks down the full urban sample into men and women, and compares the first (poorest) and fifth (richest) income quintiles.

Tables 1a, 1b, and Figure 2 show that attendance levels for school-aged children are very high throughout: For the younger age group, this average attendance rate is always above 98%, while for the older age group it is above 91. There is no discernible pattern in the attendance rates of either of these age groups across years. If anything, Figure 2 actually shows that attendance rates were *higher* in 1991 than in either one of the other two years. It would be difficult to reconcile Figure 2 and Tables 1a and 1b with a negative impact of the crisis on attendance.

The picture is clearer for the probability of being on-track. Conditional on attendance at school, the fraction of 8-11 year-olds who have completed the minimum appropriate number of school years for their age is noticeably higher in 1991 than in either 1985/86 or 1997. In the whole sample, 84% of 8-11 year-olds were on-track in 1991, but only 71% and 74% in 1985/86 and 1997, respectively. This difference across years holds for all regions, for both genders, and for the poorest and richest income quintiles. The difference between the crisis and non-crisis years is particularly pronounced for the poorest income quintile. Figure 3 confirms that children exposed to the crisis are more likely to be making satisfactory progress through school: The fraction of children aged 8 to 11 who are on-track is highest in 1991. Also, the fraction of students aged 12 or older who are on-track is higher for those exposed to the crisis (students above age 12, surveyed in 1991 or 1997), than those who were not (students above age 12 surveyed in 1985/86).²⁸ Tables 1a, 1b, and Figure 4 also show that the fraction of school-aged children employed was much lower in 1991 than in the other two years: In the sample as a whole, 3% of 6-11 year-olds and 15% of 12-17 year-olds were working in 1991. Comparable figures are 11 and 35% in 1985/86, and 6 and 26% in 1997. The simple means are therefore consistent with a crisis effect (employment levels are lowest in 1991) combined with a secular downward trend in

²⁷ We present unweighted means to make them more easily comparable to the coefficients we report for the unweighted regressions, but the picture for the means weighted by the appropriate expansion factors is very similar.

²⁸ Since we condition on attendance, these comparisons could suffer from selection bias—in particular, if “marginal” students who are less likely to make satisfactory progress in school are more likely to start school late or drop out early. We do not think that this is an important source of concern given that there are only very small variations in attendance rates across years.

employment (employment levels are lower in 1997 than in 1985/86). These differences hold for all regions, for girls as well as boys, and for the highest and lowest income quintiles.

Tables 1a and 1b suggest that the 1987-1992 crisis affected all regions, and all income quintiles (although the difference in mean income is much larger for the richest quintile). The crisis in Peru appears to have reduced access to credit: The fraction of households who report having had access to credit is lower in 1991 (16%) than in either 1985/86 (21%) or 1997 (40%). Finally, Tables 1a and 1b show predictable secular trends in parental education (rising), age (rising), household size (falling), and household composition.

Regression results: We next turn to regression analysis to estimate differences across years in attendance rates, the probability of being on-track, and employment rates. The basic approach we take is to run a series of probit regressions on pooled data from the three surveys. We run separate regressions for the full urban sample and for the poorest and richest income quintiles. In each specification, we include dummy variables for the two “non-crisis” years (1985/86 and 1997), as well as other controls.²⁹ The omitted category is therefore the “crisis” year of 1991. We interpret the coefficients on the 1985/86 and 1997 dummies as year effects conditional on changes over time in household characteristics. Pervasive, predictable differences across years could be consistent with a causal effect of the 1987-92 macroeconomic crisis on educational attainment and employment.

We report regression results only for the probabilities of being on-track and being employed. All of the coefficients on the year dummies in the attendance regressions were equal to zero—unsurprisingly, given the minimal amount of variation in attendance across years. Our simplest specification (i) includes only strata and gender dummies; specification (ii) also includes unrestricted age dummies; specification (iii) adds controls for the education of both parents (separately), unrestricted dummies for the age structure within the household, a main effect in household size, and variables for the age gap to the next oldest and youngest sibling. These child spacing variables are meant to capture the effect of the spacing of siblings on the timing of schooling (see Jacoby 1994 for a similar construction);³⁰ specification (iv) supplements specification (iii) with log income, a dummy variable for whether the household reports having access to credit, a series of interaction terms between father’s education, mother’s education, each strata dummy, the dummy variable for gender and all of the other variables in the regression (for a total of 161 interaction terms), as well as a Fourier series expansion of father’s education and

²⁹ Additional regressions which broke down the sample by strata and by gender revealed no important differences, so these are not reported, but are available from the authors upon request.

³⁰ If the child in question has no siblings, the age gaps are set to the sample mean.

mother's education.³¹ The fixed-effects specification in (v) limits the sample to households living in districts which were included in the sample in 1985/86, 1991, and 1997, and includes the variables in specification (iii), as well as a dummy variable for each district. These regressions therefore identify differences in the probabilities of being on-track and being employed from *within-district* variations across years.

Because of sample size considerations, we report results only for specifications (i) through (iii) when the sample is broken down by income quintiles. Finally, note that the estimations which include measures of parental education and those with district fixed effects are both limited to sub-samples of the data. In the three regions considered in the analysis in this paper, we only have data on the schooling of both parents for 73.9% of 6 to 11 year-olds, and only 77.8% of 6 to 11 year-olds live in districts which were included in the sample in 1985/86, 1991, and 1997. Better controls therefore come at the cost of smaller sample sizes and less precision.

Table 2 presents the regressions for the probability of being on-track of 8 to 11 year-olds, conditional on school attendance. The estimated coefficients on the two year dummies are negative, significant at the 5% level or better throughout, and surprisingly large—ranging from -0.058 to -0.127 for the 1985/86 dummy, and from -0.079 to -0.128 for the 1997 dummy. On average, the probability of making satisfactory progress through school is about 10 percentage points higher for those children who were exposed to the crisis than for those who were not. Moreover, because a decree was passed between the 1995 and 1996 school years whereby all children who completed first grade were automatically promoted to second grade, the coefficients on the 1997 dummy may actually be downward-biased estimates of changes between 1991 and 1997. When the sample is broken down by quintile, the estimated effects are larger for the poorest income quintile.

Tables 3a and 3b present regression estimates of the probability of being employed. Table 3a shows that the probability of being employed is consistently higher in 1985/86 and 1997 than in 1991. Again, the estimated differences are quite large: In non-crisis years, the probability of employment is 4 to 9 percentage points higher for children aged 6 to 11, and a remarkable 14 to 22 percentage points higher for children aged 12 to 17. All of these results are significant at the 5% level or better, and are robust to the inclusion of a large number of controls. As with the on-track results, Table 3b shows that the differences in the probabilities of employment are larger

³¹ As usual, this Fourier expansion includes terms for father's (mother's) education, its square, the sine of father's (mother's) education, the sine of twice the father's (mother's) education, the cosine of father's (mother's) education, and the cosine of twice the father's (mother's) education.

for the poorest income quintile. There is also some evidence of a downward trend in employment for both age groups, as the probability of employment is generally lower in 1997 than in 1985/86.

Propensity score matching results: The regression results we present may suffer from mis-specification bias if we have not adequately controlled for underlying differences in the distribution of the covariates between, on the one hand, the 1985/86 and 1997 sample and, on the other hand, the 1991 sample. We use propensity score matching to better adjust for possible differences in the distribution of the covariates (Rubin 1973; Rubin 1979). Specifically, we divide the sample into a “treated” group, corresponding to children in 1991, and a “control” group, corresponding to children in 1985/86 and 1997, where exposure to the 1987-92 crisis is the “treatment” being evaluated. We estimate the probability of being treated—the propensity score—on the basis of all of the covariates in our regression specification (iv). Finally, we match individual pairs in the treated and control groups by nearest available propensity score matching (for details, see Rubin and Rosenbaum 1985).

Figure 6 shows a boxplot for the distribution of propensity scores for the treated and untreated populations corresponding to the sample of 8 to 11 year-olds attending school who form the basis for our on-track estimates. Figure 6 suggests that there is considerable overlap in the distribution of these propensity scores, and therefore good potential for accurate matching. Figure 7 graphs the difference in propensity scores for each pair of matched observations. It shows that the differences in propensity score between the matched pairs are minimal: All of the differences are smaller than 0.005 (in absolute value), suggesting that the matching has eliminated the bulk of the differences in the distribution of covariates between the treated and control groups. Table 4, finally, presents differences in means in some of the covariates—specifically, the fraction of parental education and household size—between treated and control groups in the original sample, and in the new sample of matched pairs. Table 4 shows that the mean differences in the covariates are invariably smaller in the new sample of matched pairs, suggesting that the two samples are more similar to each other after matching.

We calculate two propensity score matching estimates of differences in the probability of being on-track in the two samples. The first reports the mean difference in the fraction on-track in the treated and control groups. This is 0.057, with a standard error of 0.027. Our second estimate controls for any remaining differences in the distribution of covariates in the two samples *after* propensity score matching with a pairwise, first-differenced regression (see Rubin 1979 for details).³² The estimated difference from this regression is 0.070, with a standard error

³² The regression we run on the stacked data set of matched observations is $Y_i = \alpha + \beta X_i + \delta T_i I(t=1) + \epsilon_i$, where Y_i is a dummy variable for whether the child is on-track; X_i is a vector of covariates, including all

of 0.024. Finally, we also report the estimated impact by quartile of the distribution of the propensity score in the control group (i.e. the mean difference of the probability of being on-track by quartile). Except for the second quartile, the estimated impacts are similar in magnitude to the overall ones, suggesting small difference in the response heterogeneity along the propensity score dimension.

Discussion: The results in this paper show that education outcomes for school-aged children in urban Peru were generally better in 1991 than in either 1985/86 or 1997: Attendance levels were unchanged, while the probability that children had completed a minimum number of years of school for their age was significantly higher in 1991. Moreover, we find that this effect was strongest among poorer households.

The theoretical discussion of section 2 can help shed some light on these results. Crises can affect schooling decisions in three ways: Making credit constraints more binding, and changing the marginal benefits and costs of education. The 1987-92 crisis in Peru appears to have reduced access to credit; if anything, this should have led to a reduction in human capital investment during the crisis. The effect of the crisis on the marginal benefits from education is unclear in theory. For this second channel to be part of the explanation of our finding the crisis would have had to be perceived both as persistent and as disproportionately affecting the earnings and employment prospects of low-skill workers. What evidence we have, however, suggests that incomes fell substantially for households with heads with different amounts of schooling.³³ This leaves us with the marginal cost channel, which in our view is the most likely explanation for our finding. The effect of the crisis in Peru on education outcomes appears to have been a substantial reduction in forgone income: The probability of employment for school-aged children was much

covariates used to estimate the propensity score and a dummy variable for each matched pair; $I(t=1)$ is an indicator function equal to 1 if the observation has been treated, and zero otherwise; and ε_i is the error term. The parameter δ is the fixed-effects estimate of the impact of the treatment—in this case, the estimated effect of the crisis on the probability of being on-track. Measurement error is a potential source of concern with this approach because of reduction in the signal-to-noise ratio in the first-differenced regression (for example, Deaton 1997, pp. 108-110). We do not believe that this is an important problem for our results because the treatment variable—the year in which an individual was surveyed—is unlikely to be measured with error.

³³ Glewwe and Hall (1998) report results based on a panel of households in Lima only included in the 1985/86 LSMS and in a follow-up survey conducted in 1990 which suggest that households in which the head had more schooling suffered smaller income losses. We find no clear pattern: A simple tabulation of income by the education of the household head shows that mean income fell by more in households with heads with some secondary education (-41%) than in households with primary education or less (-27%), or some tertiary education (-25%). The recovery in incomes between 1991 and 1997, by contrast, clearly favored households with more schooling: The incomes of households with heads with primary education or less increased by 2% between 1991 and 1997, by 19% for households with some secondary education, and by 76% for households with some tertiary education. Note that long-term trends in the marginal benefits to

lower in 1991 than in either 1985/86 or 1997. We hypothesize that this change in employment patterns helps explain the improvements in schooling outcomes: Children who are not employed are likely to have more time available, and may therefore expend more “effort” in school.

The stronger observed effect of the crisis on the schooling outcomes of poorer households is perhaps surprising, as these households are most likely to be liquidity constrained. We believe that there may be two possible explanations for this result. First, because the fractions employed are much larger for children in the poorest income quintile, there is also more room for downward adjustment—and therefore for larger, more clearly significant coefficients in the employment regressions. Second, the crisis may have disproportionately affected the jobs available to children from poor households. In either case, the larger employment shifts for children from poorer households freed up more time available for schoolwork.

Two concerns with interpretation of our results are possible changes in public expenditures on education and migration patterns, specifically migration from rural to urban areas. An increase in public expenditures on education during a crisis could reduce the marginal cost of education (for example, if the increase in expenditures takes the form of scholarships) or increase the marginal benefits (for example, if the increase in expenditures improves the quality of education). The Garcia government put no program in place to attempt to encourage schooling attainment during the crisis. Indeed, as Figure 5 shows, public expenditures on education followed roughly the same pattern as per capita GDP—rising sharply between 1985/86 and 1987, dropping equally sharply between 1987 and 1991, and steadily increasing thereafter. If anything, the constant attendance patterns and the improvements in age-specific grade completion took place *in spite of* the evolution of public expenditures on education. Moreover, it is unlikely that households compensated for changes in public expenditures by increasing private expenditures on education between 1985/86 and 1991, and decreasing them thereafter: At best, households may have been able to protect these expenditures somewhat in the context of dramatic cut-backs in overall household income.³⁴

Migration is a concern because the sample is limited to Lima, and the urban areas of the coast and sierra. In Peru, migration has traditionally been from rural to urban areas, although there appears to have been an important migration from urban to rural areas in the mid-1990’s. This reverse migration was a result of the increase in security in the countryside which followed

schooling are unlikely to account for our results because education outcomes *improved* between 1985/86 and 1991, and *worsened* between 1991 and 1997.

³⁴ Data from countries affected by the 1998 East Asian crisis, for example, show a varied picture: In Indonesia, private expenditures on education fell both in absolute terms and as a share of total household

the lower levels of activity of Shining Path and MRTA after 1992, as well as of a deliberate government policy designed to encourage resettlement of abandoned rural areas. Given lower mean incomes and lower adult education levels in rural areas, a reasonable assumption is that the underlying propensities to attend school and be on-track are lower—or at least no higher—among the migrants than the sedentary population. Another way to think about this is that the migrant populations are likely to have lower “ability” to attend school and make satisfactory grade progress. Rural-to-urban migration would tend to diminish ability in our sample in 1991 relative to 1985/86, while urban-to-rural migration could also diminish ability in 1991 relative to 1997. To the extent that this is the case, our estimates of the effect of the crisis would be downward-biased: In the absence of migration, the estimated coefficients on the year dummies in our on-track and employment regressions would be even larger (in absolute terms) than those which we report.

6. Conclusion

The call for a “socially responsible macroeconomic policy” which protects the poor during economic downturns has lately received a great deal of attention (this term, from Lustig 2000). There is no doubt that the poor often suffer disproportionately during crises—both in absolute terms, and because even a small reduction in their incomes can have dramatic effects on their welfare. The analysis in this paper suggests, however, that the poor, like other households, are extremely reluctant to make cutbacks in key human capital investments. Indeed, because macroeconomic crises normally depress employment prospects, households, including the poor, may rationally choose to invest more, not less, in education.

Programs of targeted cash transfers conditional on behavior, such as school attendance, have been implemented in a number of Latin American countries, including the PROGRESA program in Mexico, and the Bolsa Escola in Brazil. These programs effectively lower the price of schooling for poor households and, insofar as the demand for schooling is reasonably price-elastic, may increase the schooling attainment of children in poor households. More recently, policy makers have also begun to consider increasing the budgets of these programs during a crisis. The aim of these crisis-induced budget increases would be twofold: To transfer additional income to poor households, and prevent school desertion. Transferring income to poor credit-constrained households during a crisis is likely to substantially increase their welfare. The results

expenditures, whereas in Korea expenditures on education fell by less than overall household spending (World Bank 2000, p. 121).

in this paper suggest, however, that the probability of dramatic, crisis-induced reductions in household investments in human capital may be overstated.

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Figure 1: Real per capita GDP and prices in Peru, 1980-1999

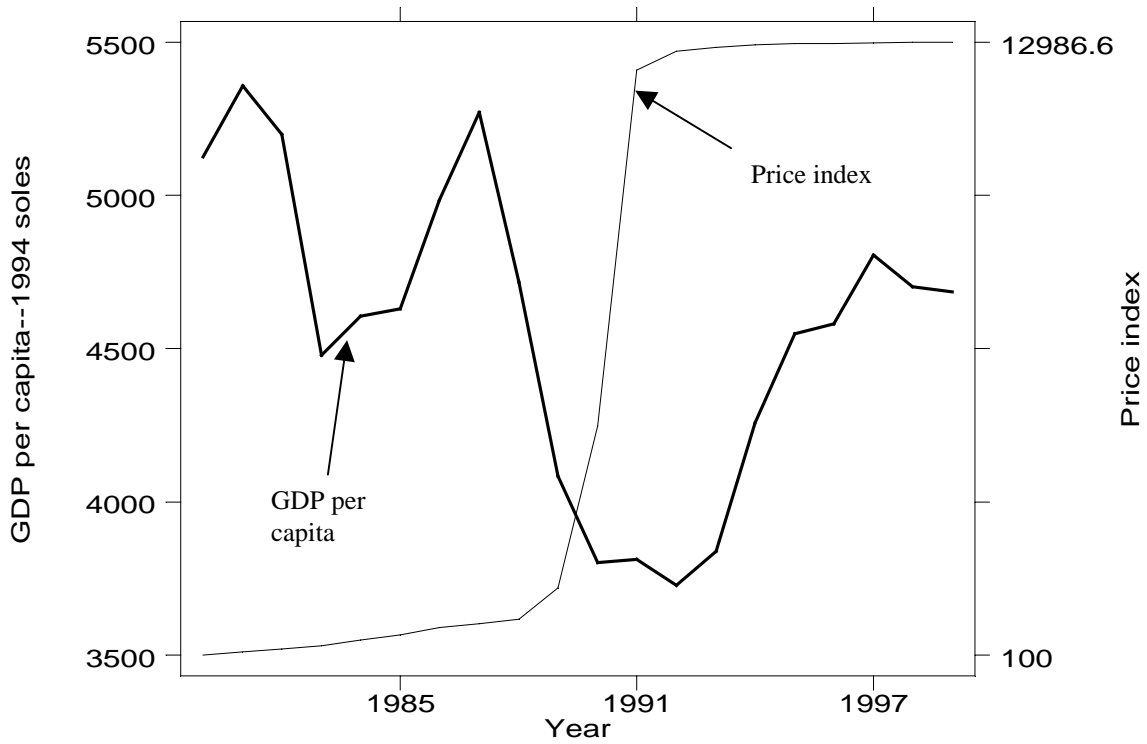


Figure 2: Age-specific school attendance rates, by survey year

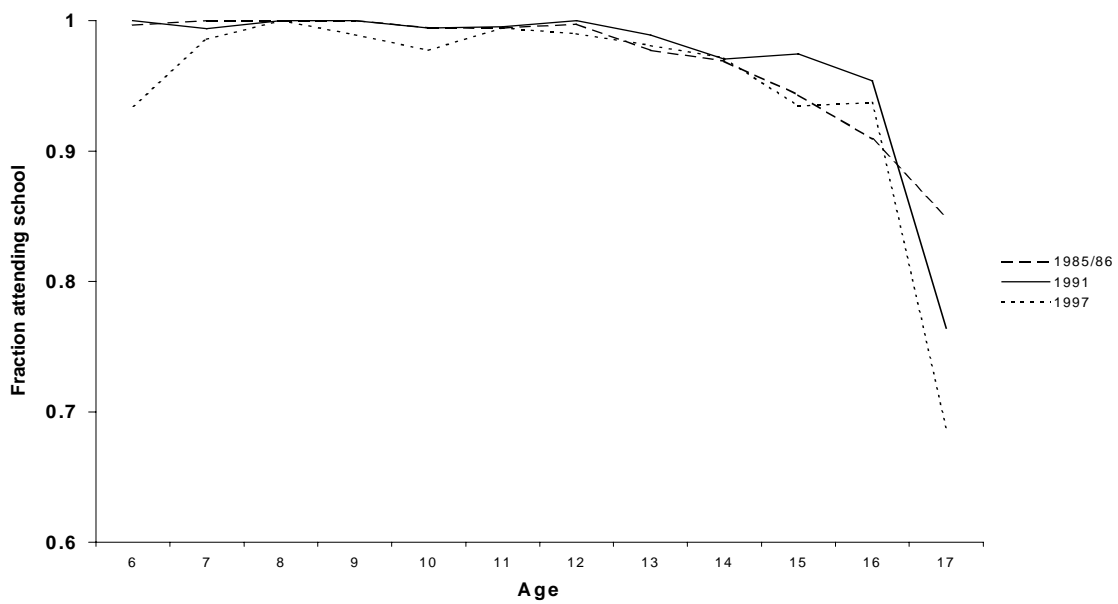


Figure 3: Age-specific fractions of students attending school who are on-track, by survey year

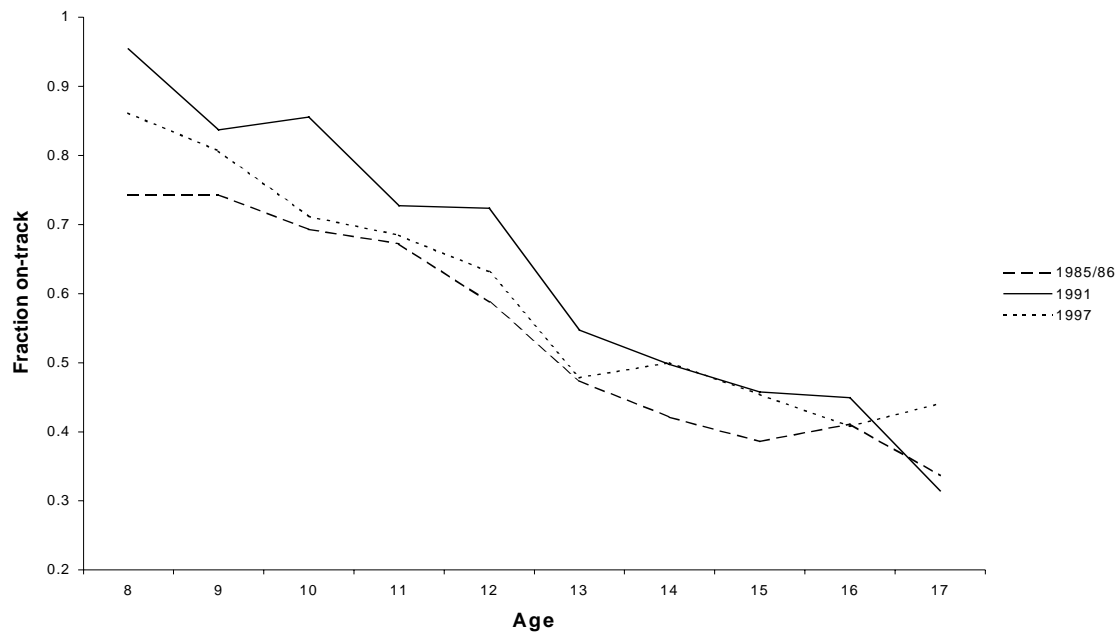


Figure 4: Age-specific employment rates, by survey year

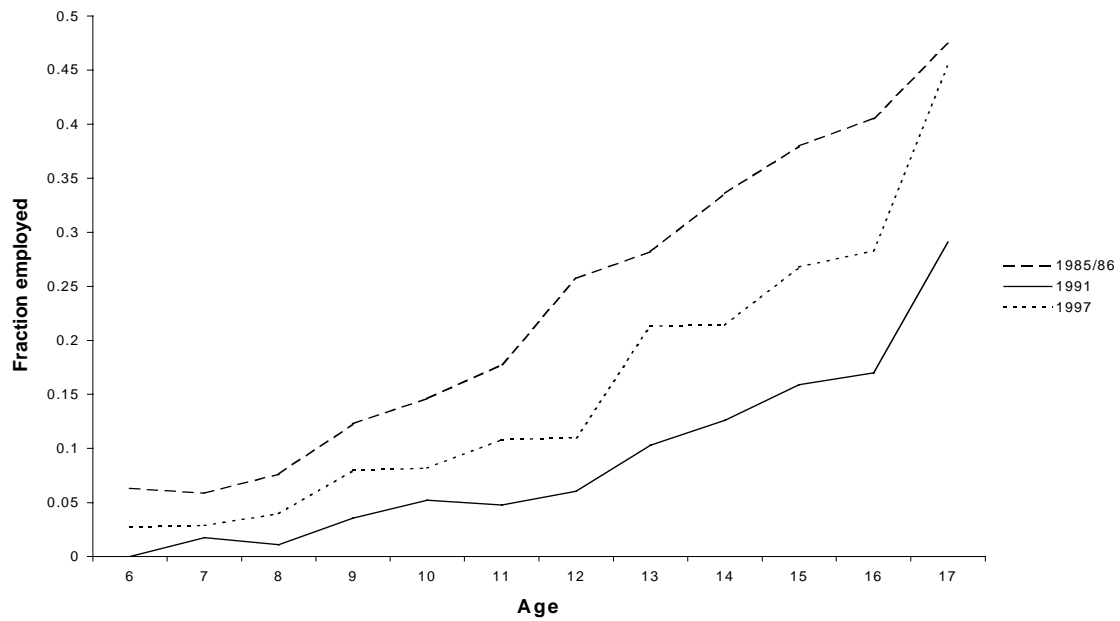


Figure 5: Recurrent and Capital Public Expenditures on Education, 1980-1997 (Constant 1997 soles)

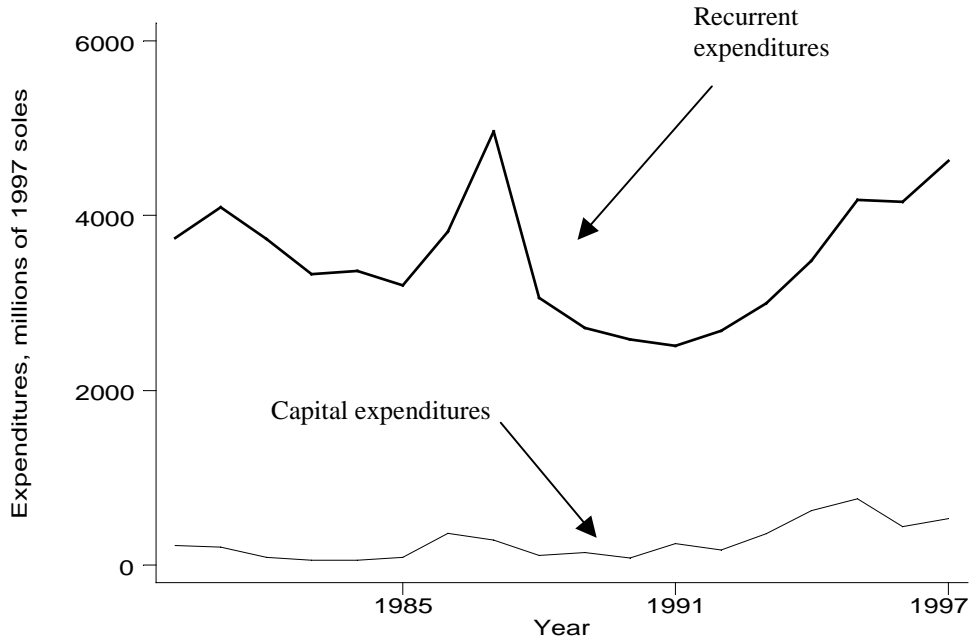


Figure 6: Boxplots of the Estimated Propensity of Being On-Track

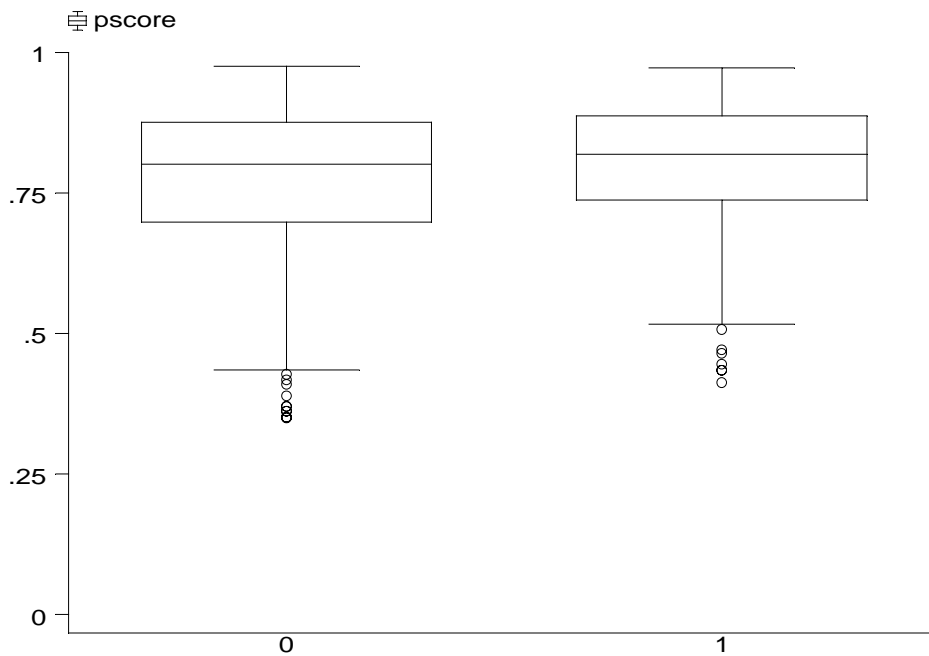


Figure 7: Scatterplot of Difference in Propensity Scores for Matched Pairs, On-track Sample

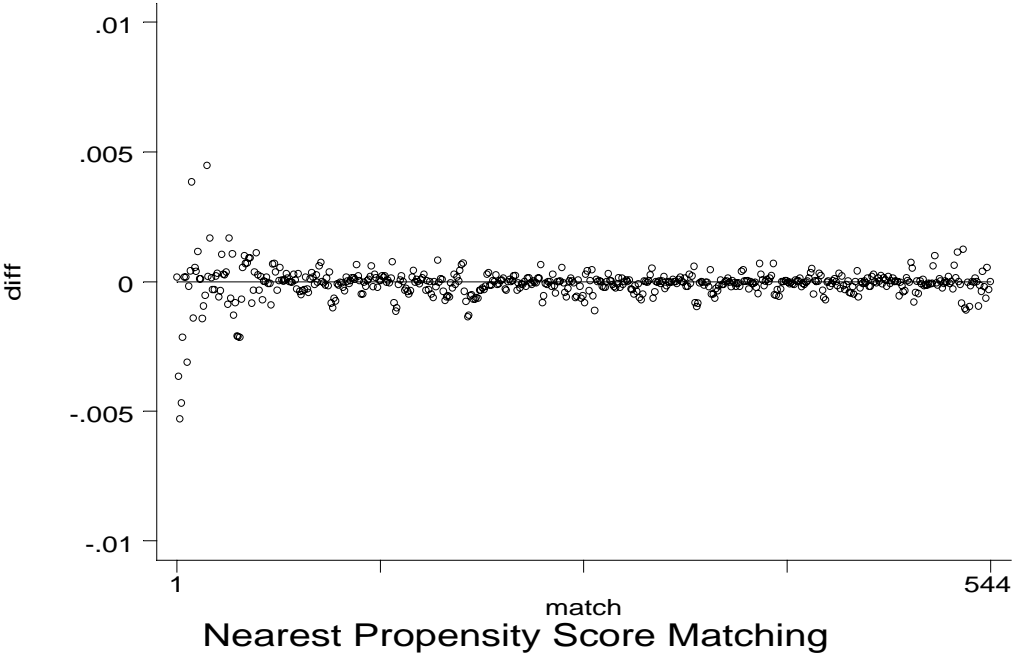


Table 1a: Sample means (unweighted), by region and year

YEAR	Urban sample (excluding urban selva)			Lima only			Urban Coast only			Urban sierra only			Rural sierra only		
	1985 /86	1991	1997	1985 /86	1991	1997	1985 /86	1991	1997	1985 /86	1991	1997	1985 /86	1991	1997
Proportion attending school (6-11)	.998	.998	.981	.997	.996	.976	.998	1.00	.985	.998	.997	.971	.983	.991	.971
Proportion attending school (12-17)	.943	.940	.918	.955	.940	.918	.926	.933	.895	.957	.944	.945	.759	.896	.783
Proportion on-track for age (8-11) *	.710	.835	.740	.689	.795	.770	.698	.859	.705	.767	.874	.813	.330	.649	.587
Proportion employed (6-11)	.108	.028	.061	.075	.016	.037	.088	.009	.051	.180	.058	.111	.574	.295	.615
Proportion employed (12-17)	.352	.154	.256	.308	.150	.224	.348	.084	.244	.424	.216	.333	.834	.620	.804
Log (per capita income)	7.61	7.34	7.64	7.82	7.44	7.77	7.42	7.27	7.45	7.48	7.22	7.58	7.22	6.92	6.63
Proportion with access to credit	0.21	0.16	0.40	0.23	0.15	0.39	0.18	0.15	0.40	0.23	0.21	0.40	0.10	0.11	0.19
Proportion rural	---	---	---	---	---	---	---	---	---	---	---	---	1.00	1.00	1.00
Mean age	25.30	26.71	28.92	25.98	27.27	29.20	24.35	26.91	29.10	24.83	25.60	28.08	24.42	25.60	25.20
Mean years of education of father	7.59	8.52	8.72	8.30	8.48	8.98	6.64	7.49	7.82	7.33	9.39	9.20	3.15	5.16	5.20
Mean years of education of mother	5.70	6.80	7.28	6.33	6.66	7.59	5.14	6.11	6.26	5.15	7.55	7.77	1.38	3.23	3.05
Mean household size	6.45	6.14	5.98	6.34	6.25	6.03	6.65	6.32	6.12	6.47	5.83	5.72	6.30	5.69	6.06
Number of hh members aged 0-2	0.26	0.19	0.18	0.22	0.15	0.17	0.27	0.20	0.20	0.30	0.24	0.20	0.43	0.31	0.43
Number of hh members aged 3-5	0.33	0.27	0.08	0.30	0.22	0.08	0.36	0.28	0.09	0.36	0.34	0.06	0.53	0.45	0.18
Number of hh members aged 6-8	0.41	0.30	0.27	0.39	0.26	0.23	0.44	0.27	0.29	0.42	0.38	0.31	0.55	0.52	0.54
Number of hh members aged 9-11	0.44	0.35	0.30	0.41	0.34	0.28	0.45	0.34	0.31	0.48	0.38	0.36	0.53	0.47	0.53
Number of hh members aged 12-14	0.46	0.37	0.34	0.42	0.36	0.33	0.50	0.39	0.34	0.50	0.36	0.33	0.51	0.46	0.45
Number of hh members aged 15-17	0.41	0.41	0.34	0.38	0.43	0.34	0.45	0.39	0.35	0.42	0.39	0.34	0.39	0.39	0.39
Age gap to next oldest sibling	7.89	8.69	9.30	8.07	8.92	9.70	7.65	8.55	8.63	7.79	8.42	9.22	7.97	8.06	7.88
Age gap to next youngest sibling	5.43	6.16	6.62	5.60	6.37	6.98	5.26	6.08	6.08	5.24	5.88	6.48	5.29	5.46	5.34

* Conditional on school attendance.

Table 1b: Sample means (unweighted), by year, gender, and income quintile (urban sample, excluding urban selva)

YEAR	Males			Females			Poorest income quintile			Richest income quintile		
	1985/86	1991	1997	1985/86	1991	1997	1985/86	1991	1997	1985/86	1991	1997
Proportion attending school (6-11)	.998	.998	.984	.997	.996	.976	.996	1.00	.964	.994	.994	.995
Proportion attending school (12-17)	.952	.941	.923	.934	.939	.913	.910	.947	.913	.960	.947	.930
Proportion on-track for age (8-11) *	.710	.835	.740	.697	.832	.792	.585	.810	.723	.774	.882	.853
Proportion employed (6-11)	.112	.023	.066	.104	.033	.054	.119	.039	.058	.088	.019	.046
Proportion employed (12-17)	.405	.154	.281	.297	.154	.232	.370	.148	.288	.249	.160	.180
Log (per capita income)	7.63	7.35	7.64	7.60	7.33	7.64	6.28	6.27	6.60	8.87	8.42	8.75
Proportion with access to credit	0.21	0.16	0.40	0.22	0.16	0.39	0.11	0.18	0.35	0.31	0.21	0.41
Proportion rural	---	---	---	---	---	---	---	---	---	---	---	---
Mean age	24.79	26.19	28.48	25.81	27.20	29.34	22.84	24.83	26.23	29.03	29.81	32.47
Mean years of education of father	7.56	8.42	8.70	7.63	8.62	8.73	5.55	7.13	7.07	9.47	10.35	11.20
Mean years of education of mother	5.64	6.79	7.26	5.75	6.80	7.30	3.78	5.66	5.60	7.68	8.93	9.97
Mean household size	6.45	6.17	6.02	6.44	6.11	5.94	7.26	6.55	6.59	5.28	5.10	4.84
Number of hh members aged 0-2	0.26	0.19	0.19	0.25	0.18	0.18	0.35	0.26	0.29	0.16	0.13	0.14
Number of hh members aged 3-5	0.33	0.28	0.08	0.33	0.26	0.07	0.47	0.43	0.10	0.19	0.16	0.06
Number of hh members aged 6-8	0.42	0.30	0.28	0.41	0.29	0.26	0.58	0.48	0.42	0.26	0.16	0.17
Number of hh members aged 9-11	0.44	0.36	0.31	0.44	0.35	0.30	0.59	0.44	0.49	0.30	0.24	0.18
Number of hh members aged 12-14	0.47	0.37	0.34	0.45	0.37	0.33	0.51	0.46	0.49	0.33	0.21	0.21
Number of hh members aged 15-17	0.41	0.42	0.35	0.40	0.40	0.33	0.31	0.30	0.21	0.31	0.30	0.21
Age gap to next oldest sibling	7.94	8.77	9.08	7.84	8.62	9.52	6.88	7.47	7.68	8.99	10.08	10.71
Age gap to next youngest sibling	5.50	6.12	6.43	5.36	6.20	6.82	4.54	4.92	5.20	6.51	7.57	8.06

* Conditional on school attendance.

Table 2: Probabilities of being on-track, conditional on attendance, aged 8-11 only

	(i)	(ii)	(iii)	(iv)	(v)
[A] All incomes					
1985/86	-0.121* [0.03]	-0.127* [0.03]	-0.061* [0.03]	-0.058* [0.03]	-0.078* [0.03]
1991	--	--	--	--	--
1997	-0.079* [0.03]	-0.084* [0.03]	-0.107* [0.03]	-0.108* [0.03]	-0.128* [0.04]
Observed P	.761	.761	.783	.783	.783
Predicted P	.766	.770	.806	.815	.815
N	2,901	2,901	2,130	2,111	1,660
[B] Lowest quintile					
1985/86	-0.192* [0.05]	-0.202* [0.05]	-0.043 [0.06]		
1991	--	--	--		
1997	-0.074 [0.06]	-0.082 [0.06]	-0.051 [0.07]		
Observed P	.699	.699	.711		
Predicted P	.709	.713	.737		
N	735	735	523		
[C] Highest quintile					
1985/86	-0.093 [0.05]	-0.101 [0.05]	-0.057 [0.05]		
1991	--	--	--		
1997	-0.042 [0.06]	-0.055 [0.06]	-0.045 [0.07]		
Observed P	.830	.830	.852		
Predicted P	.836	.840	.892		
N	464	464	339		

Note: Being “on-track” is defined as $\text{diff} \leq 0$, where diff is given by (age-7-years of completed schooling). For example, an 8-year old is considered to be on-track if (s)he has completed at least one year of schooling, a 9-year old if (s)he has completed at least 2 years of schooling, and so on.

* Significant at the 5% level or better.

Standard errors in square brackets are corrected for heteroskedasticity and clustering.

Specification (i) includes only strata and gender controls; specification (ii) includes a vector of age dummies; specification (iii) supplements specification (ii) with variables for the education of both parents (separately), variables for the number of household members under 3, between 3 and 5, between 6 and 8, between 9 and 11, between 12 and 14, and between 15 and 17, as well as a variable for the size of household; specification (iv) supplements specification (iii) with log income, a dummy variable for whether the household reports having access to credit, a series of interaction terms between father’s education, mother’s education, each strata dummy, the dummy variable for gender and all of the other variables in the regression (for a total of 161 interaction terms), as well as with a Fourier series expansion of father’s education and mother’s education, which includes terms for father’s (mother’s) education, its square, the sine of father’s (mother’s) education, the sine of twice the father’s (mother’s) education, the cosine of father’s (mother’s) education, and the cosine of twice the father’s (mother’s) education; the fixed-effects specification in (v) limits the sample to households living in districts which were included in the sample in 1985/86, 1991, and 1997, and includes the variables in specification (iii), as well as a dummy variable for each district.

Table 3a: Probabilities of being employed, aged 6-11 only (regression estimates)

	(i)	(ii)	(iii)	(iv)	(v)
[A] All incomes					
1985/86	.092*	.089*	.081*	.070*	.052*
	[.02]	[.02]	[.02]	[.02]	[.01]
1991	---	---	---	---	---
1997	.057*	.056*	.078*	.066*	.041*
	[.02]	[.02]	[.03]	[.02]	[.02]
Observed P	.071	.071	.075	.075	.068
Predicted P	.060	.053	.050	.038	.039
N	4,322	4,322	3,160	3,136	2,234
[B] Lowest quintile					
1985/86	.085*	.084*	.070*		
	[.03]	[.03]	[.03]		
1991	---	---	---		
1997	.039	.041	.065*		
	[.03]	[.03]	[.03]		
Observed P	.075	.075	.082		
Predicted P	.065	.053	.045		
N	1,103	1,103	789		
[C] Highest quintile					
1985/86	.067*	.062*	.032		
	[.03]	[.03]	[.02]		
1991	---	---	---		
1997	.043	.045	.036		
	[.04]	[.04]	[.03]		
Observed P	.053	.053	.051		
Predicted P	.047	.037	.025		
N	678	678	493		

Note: Individuals are considered to be employed if they responded affirmatively to any one of the following four questions in the surveys: (i) “In the last 7 days, did you work as an employee for a business, corporation, government, a boss, or another individual?”; (ii) “and in the last 12 months?”; (iii) In the last 7 days, did you work for yourself, (“a cuenta propia”, or self employed), or as an unpaid family member, or on the farm?”; (iv) and in the last 12 months?”.

* Significant at the 5% level or better.

Standard errors in square brackets are corrected for heteroskedasticity and clustering.

Specification (i) includes only strata and gender controls; specification (ii) includes a vector of age dummies; specification (iii) supplements specification (ii) with variables for the education of both parents (separately), variables for the number of household members under 3, between 3 and 5, between 6 and 8, between 9 and 11, between 12 and 14, and between 15 and 17, as well as a variable for the size of household; specification (iv) supplements specification (iii) with log income, a dummy variable for whether the household reports having access to credit, a series of interaction terms between father’s education, mother’s education, each strata dummy, the dummy variable for gender and all of the other variables in the regression (for a total of 161 interaction terms), as well as with a Fourier series expansion of father’s education and mother’s education, which includes terms for father’s (mother’s) education, its square, the sine of father’s (mother’s) education, the sine of twice the father’s (mother’s) education, the cosine of father’s (mother’s) education, and the cosine of twice the father’s (mother’s) education; the fixed-effects specification in (v) limits the sample to households living in districts which were included in the sample in 1985/86, 1991, and 1997, and includes the variables in specification (iii), as well as a dummy variable for each district.

Table 3b: Probabilities of being employed, aged 12-17 only (regression estimates)

	(i)	(ii)	(iii)	(iv)	(v)
[A] All incomes					
1985/86	.211*	.223*	.162*	.156*	.169*
	[.03]	[.03]	[.03]	[.03]	[.03]
1991	---	---	---	---	---
1997	.129*	.138*	.158*	.139*	.146*
	[.03]	[.03]	[.03]	[.03]	[.04]
Observed P	.269	.269	.254	.255	.258
Predicted P	.259	.251	.227	.216	.221
N	4,405	4,405	3,100	3,077	2,253
[B] Lowest quintile					
1985/86	.272*	.281*	.243*		
	[.05]	[.05]	[.06]		
1991	---	---	---		
1997	.202*	.203*	.260*		
	[.05]	[.06]	[.07]		
Observed P	.285	.285	.272		
Predicted P	.270	.265	.229		
N	996	996	688		
[C] Highest quintile					
1985/86	.075*	.092*	.029		
	[.05]	[.05]	[.05]		
1991	---	---	---		
1997	.031	.045	.072		
	[.05]	[.05]	[.06]		
Observed P	.199	.199	.197		
Predicted P	.196	.187	.164		
N	722	722	519		

Note: Individuals are considered to be employed if they responded affirmatively to any one of the following four questions in the surveys: (i) “In the last 7 days, did you work as an employee for a business, corporation, government, a boss, or another individual?”; (ii) “and in the last 12 months?”; (iii) In the last 7 days, did you work for yourself, (“a cuenta propia”, or self employed), or as an unpaid family member, or on the farm?”; (iv) and in the last 12 months?”.
* Significant at the 5% level or better.

Standard errors in square brackets are corrected for heteroskedasticity and clustering.

Specification (i) includes only strata and gender controls; specification (ii) includes a vector of age dummies; specification (iii) supplements specification (ii) with variables for the education of both parents (separately), variables for the number of household members under 3, between 3 and 5, between 6 and 8, between 9 and 11, between 12 and 14, and between 15 and 17, as well as a variable for the size of household; specification (iv) supplements specification (iii) with log income, a dummy variable for whether the household reports having access to credit, a series of interaction terms between father’s education, mother’s education, each strata dummy, the dummy variable for gender and all of the other variables in the regression (for a total of 161 interaction terms), as well as with a Fourier series expansion of father’s education and mother’s education, which includes terms for father’s (mother’s) education, its square, the sine of father’s (mother’s) education, the sine of twice the father’s (mother’s) education, the cosine of father’s (mother’s) education, and the cosine of twice the father’s (mother’s) education; the fixed-effects specification in (v) limits the sample to households living in districts which were included in the sample in 1985/86, 1991, and 1997, and includes the variables in specification (iii), as well as a dummy variable for each district.

Table 4: Covariate Imbalance in the Baseline and Matched Samples

	Baseline	Nearest Propensity Score Matched Sample
Treated Observations	554	554
Control Observations	1586	554
Father's Education	0.443 [0.20]	0.015 [0.03]
Mother's Education	0.745 [0.20]	0.224 [0.18]
Household Size	-0.096 [0.11]	0.067 [0.12]
Propensity Score	0.023 [0.01]	-0.000 [0.00]

Standard errors in brackets

Table 5. Estimated Impacts in the Baseline and Matched Samples

	Baseline Sample	Nearest Propensity Score Matched Sample	Regression Adjusted Propensity Score Estimated Impact
Treated Observations	554	554	554
Control Observations	1586	554	554
Percentile of the Propensity Score in the Control Group			
0-24	---	0.059	---
25-49	---	0.007	---
50-74	---	0.088	---
75-100	---	0.074	---
Overall Impact	0.068* [0.02]	0.057* [0.02]	0.070* [0.02]

Standard errors in brackets
* Significant at the 5% level

