Freed from Illiteracy?

A Closer Look at Venezuela's *Robinson* Literacy Campaign[*]

Daniel Ortega

Instituto de Estudios Superiores de Administración

Francisco Rodríguez

Wesleyan University

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Abstract

We evaluate the success of the Venezuelan government's latest nation-wide literacy program, *Misión Robinson*, using official Venezuelan government survey data. Controlling for existing trends in literacy rates by age groups over the period 1975 to 2005, we find at most a small positive effect of *Robinson* on literacy rates, and in many specifications the program impact is statistically indistinguishable from zero. This main result is robust to time series analysis by birth cohort, and to state-level difference-in-differences estimation. The results appear to be inconsistent with recent official claims of the complete eradication of illiteracy in Venezuela, but resonate with existing research on other adult literacy programs, which have usually been expensive failures.

I. Introduction

On October 28, 2005, the Venezuelan government announced that the country had been declared "Illiteracy-Free Territory"[1], marking the success of the two-year old national literacy campaign *Misión Robinson.* According to the statement, between the start of the program and the announcement, the Cuban designed *Yo Sí Puedo* program had helped teach 1,482,543 persons how to read and write (Gobierno Bolivariano de Venezuela, 2005b, p.5). The achievement received considerable international recognition, and is generally taken at face value by specialists as well as by casual observers. A recent article in the *San Francisco Chronicle*, for example, reports that "illiteracy, formerly at 10 percent of the population, has been completely eliminated." UNESCO's latest *Education for All Global Monitoring Repor*t reports that 1 million people learned to read and write in Venezuela between July 2003 and December 2005[2].

If true, the success of the Venezuelan program would have significant implications for the design of literacy programs in developing countries. The literature on literacy programs in the developing world has generally been skeptical of large-scale adult literacy programmes, which tend to be plagued by low initial enrolments, high dropout rates, and rapid loss of acquired skills (Romain and Armstrong, 1987). Abadzi (1994) found that the percentage of students passing exams in large scale literacy programs ranged between a low of 8 percent and high of 47 percent. This general scepticism has been a main cause for a substantial reduction of World Bank financing of adult literacy programs since 1990 (Chowdury, 1995). If *Misión Robinson* has indeed achieved the results claimed by the Venezuelan government, it would demonstrate that adequately designed large-scale national programmes can be successful at reducing illiteracy, with possible implications for many other countries.

A key ingredient of the debate between the proponents and opponents of literacy programs concerns the relevance of basic literacy for development. While program supporters consider literacy an essential capability for the empowerment of poor individuals and communities, many critics point to the demonstrated ineffectiveness of large scale programs and to the capacity that non-literate individuals have to participate in the development process (Rogers, 2002). In particular, some experts have espoused an approach termed "literacy second", where participants learn work-related skills and progress to literacy training if and when they decide that they need it (Oxenham and Aoki, 2003).

The conventional wisdom in the literacy literature is that large-scale government administered programs are rarely successful. A recent survey of the evidence estimated that the large-scale programs implemented during the sixties and seventies had efficiency rates of about 12.5 percent, with few participants acquiring stable literacy skills (Abadzi, 2003a, p. 2)[3]. Recent programs have focused on shifting responsibility for instructional delivery to NGOs, giving them support and textbooks to teach literacy as they know best, focusing on women and out-of-school adolescents, and linking literacy with micro-credit initiatives. Although these programs have had better results than the previous ones, they have still largely disappointed expectations (Abadzi, 2003a, pp. 5-7).

The Venezuelan program, in contrast, is premised on a vision of literacy training as a vital entry point to a network of educational programs that can strengthen the capacity of the poor to participate in society. Those who complete the *Robinson* program become eligible for several fast-track educational programs that are meant to substitute for higher educational levels and work-related training. The program also shares the operational design of many of

the large scale literacy programs of the sixties and seventies: management and instructional delivery are government-administered, the courses are brief (lasting seven weeks), there is no role for NGO participation, and there is no systematic effort to evaluate reading speed. Evidence of success of the *Robinson* program would shed doubt on the recent conventional wisdom on the design of successful interventions.

This paper will assess the evidence of the success of the *Robinson* program using evidence from the Venezuelan Household Surveys, which includes self-reports on literacy, to evaluate official claims of having wiped out illiteracy and to assess the effectiveness of the program. We examine country-level impacts using a battery of alternative time-series methods in an attempt to estimate whether the implementation of *Robinson* coincides with significant reductions in overall Venezuelan illiteracy. We also adopt a more disaggregated state-level approach, combining official data on the number of *Misión Robinson* literacy trainers enrolled by state with the household data, in order to evaluate whether states that experienced higher program intensity also witnessed larger reductions in illiteracy over time. In both cases we find evidence for, at most, small positive literacy gains as a result of the program, though in many specifications *Robinson* program impacts are statistically indistinguishable from zero.

II. The Venezuelan Literacy Program *Misión Robinson*

Misión Robinson, also known as the Simón Rodríguez Extraordinary Literacy Program, was launched by the Venezuelan government in a nationally televised program on July 1, 2003. The program uses the "Yo Sí Puedo" ("Yes I Can") method designed by Cuban

educator Leonela Relys, which consists of sixty-five 45-minute video classes and practical exercises supervised by trained instructors[4].

The "Yo Sí Puedo" method builds on the fact that non-literate individuals are often familiar with numbers by asking students to identify unknown letters with known numbers. In *Misión Robinson*, each class is supervised by a government-appointed trainer who assists students in carrying out exercises and in evaluating their progress. Trainers were paid a monthly stipend of 160.000 Bs. (US$100). The intensive course lasts seven weeks, by the end of which students are expected to be able to write a letter to demonstrate their acquisition of skills. Graduates have immediate access to *Misión Robinson 2*, a follow-up program designed to provide the equivalent of a primary school education[5].

According to official announcements, the program was successful in almost completely eradicating illiteracy in Venezuela. Although there is some variation in the figures presented by different government spokespersons, estimates generally range between 1.4 and 1.5 million. However, the precise source of these claims remains unclear. Although the program was overseen by a Presidential Commission (*Comisión Nacional de Alfabetización*), it has not published any official reports describing the methodology used to arrive at the estimates of the reduction in illiteracy. It is likely that these estimates were arrived at on the basis of the collected field reports of trainers and program supervisors.

One puzzling fact about the government's claim is that, according to official statistics, the number of illiterate Venezuelans before the start of *Misión Robinson* was already well below 1.5 million persons. Table 1 presents the evolution of Venezuelan illiteracy as reported by the national censuses from 1936 to 2001[6]. The pre-*Robinson* 2001 census reports only

1.08 million illiterate Venezuelans of age 15 and greater – the standard UNESCO threshold - in 2001. Indeed, according to the census data, Venezuela appears to never have had as many as 1.5 million illiterate adults during the past seventy years.

This inconsistency was recognized by Education Minister Aristóbulo Istúriz, who has claimed that the Census figure of 1.2 million illiterate individuals underestimated illiteracy rates and that estimates carried out by the Ministry of Education in 2003 had put the number of illiterates at the higher 1.5 million[7]. These estimates referred to by the minister do not appear in any official publication and are therefore difficult to evaluate[8]. If we take this figure to be correct, the government's claim of having taught how to read and write to 1.4-1.5 million persons would imply a reduction of illiteracy to less than 0.1% of the country's adult population. According to UNDP (2005), no country outside the original OECD and Eastern Europe has an adult illiteracy rate lower than 0.1%[9]. The highest literacy rate in Latin America is that of Uruguay, at 97.7%. Cuba's literacy rate is 96.9%[10].

III. Empirical Analysis

1. Data

The analysis in our paper will be based on the National Statistical Institute's (Instituto Nacional de Estadística, INE) Household Survey. This survey, which is available through the second half of 2005, has included a question on self-reported literacy since 1975. In the survey, interviewers ask respondents the following question: "Does this family member know how to read and write?" ("¿Sabe leer y escribir?"). The question is asked to the person or persons present at the moment of the interview about all household members.

The focus of our analysis will be a set of pre and post-*Misión Robinson* estimates of literacy rates based on answers to this question. Unfortunately, no attempt is made by the interviewer to directly assess the respondent's real reading skills. One concern with a self-report of this kind is that some newly literate or semi-literate people may exaggerate their reading skills, thus overstating the effect of a literacy program. This possible response bias arguably allows us to place upper bounds on estimated program impacts, as discussed below.

We will start by analyzing whether the implementation of *Robinson* is associated with a change from the long-run trend evolution of literacy in the national data. We test whether this discontinuity or break in trend occurs for different age subgroups as well as by national cohorts and different econometric specifications. This analysis is obviously limited by the fact that *Robinson* may have coincided with other changes in economic and social trends that also affected illiteracy. Our second set of estimates will thus rely on the analysis of the evolution of state and state-cohort literacy rates and their relation with two measured of program intensity derived from official statistics on program trainers by state.

The data on trainers is derived from two official sources: The 2004 Annual Report of the Education Ministry, (Ministerio de Educación, Cultura y Deportes, 2005, p. 913), and the electronic database of the Ministry of Planning and Development's School of Social Work. (Ministerio de Planificación y Desarrollo, 2006). The latter series reports a total of 110,703 trainers involved in *Misión Robinson*, while the former claims a considerably higher figure, 210,353 trainers. The difference between the series may be due to the high turnover rates of trainers, which some informal field reports have put at around 40%[11].

2. Analysis of National Time Series Trends

Figure 1 shows the evolution of illiteracy rates in Venezuela between the first semester of 1975 and the first semester of 2005, as calculated from the nationally representative Household Surveys. According to this data, in the second semester of 2005 – the first period after the government declaration of the eradication of illiteracy – there were still 1,014,441 illiterate Venezuelans over age 15, only slightly less than the estimate for the first semester of 2003 (before *Robinson* began) of 1,107,793 persons. Because of population growth, this small reduction in the absolute number of illiterate Venezuelans coincides with a moderate drop in the illiteracy rate from 6.5% to 5.6% among those over-15, and an even larger 8.2% to 6.9% drop in the over-25 illiteracy rate[12].

Thus, relying on official Household Survey data, Venezuela's literacy gains, while significant, have not eradicated illiteracy. We next examine the perhaps more important question of whether this moderate reduction in illiteracy rates between 2003 and 2005 can more conclusively be associated with the effect of the *Misión Robinson* literacy campaign, or whether it is driven by other factors or pre-existing trends. This question is the focus of the remainder of the paper.

Inspection of Figure 1 reveals that there has been a long-run reduction in Venezuelan illiteracy rates, which have been falling steadily since the start of the series in the mid-seventies. Viewed in the context of this long-run trend, it is not readily apparent that the reported post-2003 drop in illiteracy is in fact due to the effects of *Misión Robinson*.

Robinson is primarily an adult education program[13]. Therefore we should expect its impact on literacy to be most pronounced among older age groups, although this effect may be dampened if the program is less effective in teaching older participants, perhaps because it is generally harder for them to learn new skills. Figure 2 presents literacy rates broken down by age group and reveals a similar pattern to that found in Figure 1: although there is a pronounced decline in illiteracy among older age groups after the start of *Robinson*, the decline appears to be the continuation of a longstanding trend. For instance, although Robinson coincided with a yearly decline of 1.4 percentage points in the illiteracy rate for persons older than 55, this rate had already been declining at a rate of 1.1 percent yearly during the eight preceding years.

We use various econometric methods to evaluate whether the effect of *Misión Robinson* is distinguishable from long-run trends. We first test whether the period of program implementation is associated with changes in national literacy rates after controlling for time trends, as well as an indicator variable to capture any effects of a 1994 change in the survey question methodology. The 1994 change is associated with a discrete jump in the literacy rate (Figures 1 and 2). Observation of the time-series trend, however, suggests that the linear trend is probably not a good approximation to the evolution of literacy rates, as it obviously becomes more difficult to boost literacy as one approaches the maximum of 100% literacy. A linear trend does in fact over-predict literacy gains towards the end of the sample, imparting a downward bias to the estimated effect of *Robinson* (Figure 3). A quadratic trend also appears inadequate as it predicts a decline in literacy for the last years in the sample. A cubic time trend, in turn, appears to be the simplest specification that provides a reasonable fit to pre-existing trends. In analysis below we control for cubic as well as higher-order polynomial trends.

An indicator variable for the period of program implementation and afterwards – the second semester of 2003 onwards– captures the effect of *Misión Robinson*. An obvious concern is that that this indicator variable captures the effect of other changes or programs that occurred in the Venezuelan economy during the period, for instance the large number of other social assistance *Misiones* launched during the same period, all of which targeted similarly marginalized populations. If these other *Misiones*, as well as the rapid economic expansion (due to rising oil prices) since 2003, boosted literacy, this would likely lead our estimates to, if anything, overstate *Robinson* program impacts. Thus we should interpret any estimated post-2003 effect as an upper bound on the impact of *Robinson*. On the other hand, it is difficult to imagine any other national-level phenomenon that would have a greater effect on literacy than a campaign on the massive scale of *Misión Robinson*, and it remains the leading explanation for any shifts in literacy during this period.

Consider the following simple OLS estimation equation:

$$LITERACY_t = \alpha + \beta \cdot ROBINSON_t + \gamma_0 1(\text{Pr}e - 1994)_t + \gamma_1 t + ... + \gamma_n t^n + \varepsilon_t + \rho\varepsilon_{t-1} \qquad (1)$$

where $ROBINSON_t$ is an indicator variable that takes the value 1 after the start of the literacy program, *1(Pre-1994)* is an indicator that takes on a value 1 before the 1994 methodology change, and the remaining terms capture any pre-existing time trends. For ease of interpretation, we set *t=0* in 2003 semester 1 (although this is inessential for the results). The data cover all but two semesters between 1975 semester 1 and 2005 semester 2, giving us a total sample size of 60 national literacy observations.

Controlling for a cubic time trend (Table 2, column 1) the estimated *Robinson* effect is slightly negative but statistically insignificant, with an estimated drop in literacy of only 0.2 percentage points associated with the program (the 95% confidence interval ranges from -.008 to .004). Any gains in literacy in Venezuela between 2003 and 2005 appear likely to reflect long-standing trends in Venezuelan society rather than the impact of *Misión Robinson*. Focusing on those aged 25 and older, who were more likely to enroll in *Robinson* than adolescents, yields very similar results: *Robinson* is associated with a small and statistically insignificant decrease in literacy of 0.09 percentage points with a cubic time control (Table 2, column 3).

In order to verify that our results are not caused by misspecification of the time trend term, Table 2 shows an additional pair of specifications, in which the order of the Taylor approximation used to capture the time trend is selected by introducing additional polynomial time terms until the marginal time polynomial term lacks statistically significant predictive power. In our data, this method selects a sixth order polynomial trend. Controlling for this polynomial (Table 2, columns 2 and 4) yields a positive and statistically insignificant effect of *Robinson* on both the over-15 (.0029, s.e.=.0018) and the over-25 (.0033, s.e.=.0022) national literacy rates. The increase in the literacy rate associated with *Robinson* according to the point estimates of this specification are in both cases less than one-half of the increase in national literacy experienced between the first semester of 2003 and the first semester of 2005. They would imply that *Misión Robinson* coincided with an above-trend increase of 49,431 in the number of literate Venezuelans aged 15 or over, and 43,299 among those aged 25 or over, by 2005, although note that even these very modest gains are not robust to even seemingly minor changes in how we control pre-existing time trends. This gain is only a tiny fraction of the 957,268 people who were illiterate according to government statistics in 2001.

We next turn to estimating more disaggregated effects by age subgroups. Column (1) of Table 3 reports the coefficients obtained from estimation of equation (1) for each of five distinct age groups (15 to 24, 25 to 34, 35 to 44, 45 to 54 and 55 or older), with the system is then estimated by Seemingly Unrelated Regressions (SUR). For comparison purposes, we also report the OLS estimates for the whole adult population. Reported results include a cubic time trend control[14]. The results are again very similar: for none of the age groups do we find a statistically significant effect of *Robinson*, and a joint test of the significance of the age group effects is also insignificant (p=.469). Effects on literacy are statistically indistinguishable from zero, although the point estimates range from 0.01 percentage points for those aged 35-44 up to 0.55 percentage points for the over-55 group. This corresponds to a reduction in illiteracy in this age group of roughly 1/6 of the total reduction observed between the first semester of 2003 and the second semester of 2005.

Table 3 also shows a number of additional robustness tests which try slightly different specifications of the potential *Robinson* effect. A potential source of misspecification may come from the fact that the program's effects are observed with a lag. Since the program courses are designed to last seven weeks, it is probable that Household Survey respondents enrolled in *Robinson* will not report having learned to read and write until the semester after taking the course. In order to determine whether this affects the main results, we report in column (2) the result of running our same tests with the *ROBINSON* indicator variable term lagged by one semester, thus taking on a value of one after the first semester of 2004. This makes little difference in the estimated program impacts (column 2, Table 3). The SUR coefficient estimates on the older age groups now become marginally statistically significant. Somewhat surprisingly, so does the coefficient on the 15-24 age subgroup. Yet even in the

case of this age group, the largest estimated effect in the table, the magnitude remains moderate at a 1.18 percentage point literacy gain.

As we have argued previously, individuals may exaggerate their literacy in self-reported assessments. Ideally, we could limit this bias by restricting our sample to the assessment by respondents of the literacy status of other family members. Regrettably, the National Institute stopped identifying the family member that served as informant in its database in 2000. Using surveys previous to that year, we have indeed found that informants' assessment of their own literacy tends to be consistently higher than their assessment of others' status, even after controlling for gender, education, age, and head of household status of the subject assessed[15]. We have also found that the literacy status for heads of households and their spouses is much more likely to be self-reported than that of other household members[16]. Column 3 of Table 3 thus uses literacy estimates derived from individuals other than the head of household or their spouse. The coefficient of *Robinson* is now positive, but very far from economically or statistically significant. Since the age composition of the restricted subsample is very different from that of the population as a whole, it may make more sense to concentrate on the estimates for age subgroups. Here we find that the point estimate actually turns negative for three of the five age subgroups, and is statistically insignificant for all the subgroups. Restricting the sample to answers that are less likely to be self-reported thus does not seem to strengthen the evidence in favour of program effectiveness.

Our baseline specification tests for a discontinuity in literacy rates associated with the implementation of the program. The fourth and fifth columns of Table 3 try two alternative specifications that introduce a continuous effect. In the first one the effect of *Robinson* is

assumed to increase uniformly in every period during which the program was in operation[17]. This specification attributes no significant effect to *Robinson* on the oldest age subgroup but does find a significant effect on the youngest age subgroup, of a still moderate 1.64 percentage points. The second specification evaluates instead whether the implementation of the program coincided with a change in the trend of literacy rates by testing for the significance of an interaction between the *Robinson* dummy and the terms that make up the cubic time trend. In none of the cases do we find statistical evidence that a break in trend can be associated with the program.

Table 4 presents the result of a specification very similar to equation 1, but in which the dependent variable is the birth cohort-specific literacy rate, controlling for a cohort fixed effect and cohort-specific time trends. We report specifications with both the contemporaneous and the lagged specification. In columns (1) and (2) we restrict all cohorts to have the same coefficient on *Robinson,* while in columns (3) and (4) we allow those coefficients to vary by groups of cohorts classified according to their age in the last semester of our sample.

A virtue of this specification is that the cohort-specific literacy rates will not be as affected by changes in composition as age-specific regressions are. Age-specific literacy rates will tend to increase over time as their composition changes from older, less literate cohorts, which either die or exit the group, to younger cohorts that enter the group. This effect is absent in the cohort-specific rates[18]. In the analysis we denote cohorts by their age in 2005 and keep only the cohorts for which we have at least three years of observations previous to the implementation of *Robinson* in mid-2003. We also drop all cohorts older than 90 years

(which accounted for 0.14% of the population at the start of the program) since we have too few observations to reliably calculate literacy rates for them.

The results of this specification are broadly similar to those of the previous exercise, but with some interesting distinctions. While the contemporaneous specification gives a positive insignificant coefficient, the lagged specification gives a borderline statistically significant but economically small coefficient estimate which implies an overall increase in literacy of 0.18 percentage points attributable to the program. When we break up the exercise by age groups, we find that only the 55 and over cohort sees a positive, borderline significant coefficient. Except for this cohort, all remaining estimated effects are far from statistical significance and some are negative. The point estimate on the over-55 age subgroup, (.51-.67 percentage points) is slightly lower than that which is estimated in the analogous specifications in Table 3, although the effects are not strictly comparable.

In sum, the analysis of time series trends fails to consistently find that *Misión Robinson* had a significant impact on aggregate literacy rates in Venezuela. If there is such an effect, according to our estimates, it is positive but quite small, a reduction of well under one percentage point in illiteracy in most regression specifications. The bulk of program impact estimates are positive but small and not statistically significant at traditional confidence levels, with some of them small and negative. Most of the estimates presented in this section are essentially precisely estimated zeroes, as the standard errors of the estimated coefficients tend to be very low (between .001 and .004 for most estimates).[19] The largest point estimate for the national data presented in this section (Table 3, column 2) associates the program with an increase in the literacy rate of 0.54 percentage points, representing 65,748 adults. A few of our age group specifications do appear to capture a significant effect of *Robinson* of the

self-reported literacy rates of the 55 and older group. Even this effect, which our estimates put well below 1 percentage point, is at best a minor contributor to the increase in the aggregate literacy rate, given that this group constitutes less than 15% of the Venezuelan adult population.

The use of deviations from pre-existing time trends to estimate the effect of *Misión Robinson* has obvious limitations. The period between the second semester of 2003 and the first semester of 2005 saw a number of other changes in the Venezuelan economy and in society, several of which may have also affected literacy rates. Recall that these estimates are likely to be upper bounds on literacy gains for at least two reasons – first, the exaggeration of literacy among recent program participants in survey self-reports, and second possible positive contemporaneous impacts of other government programs – so actual program impacts are plausibly smaller. However, the strong economic recovery during this period could have also raised the opportunity cost of participating in adult education programs, dampening program effects – unless new job opportunities generated by the booming economy led some people to become literate independently of *Robinson*, a bias that would go in the other direction. Between the first semester of 2003 and the first semester of 2005, the Venezuelan economy grew at an annual rate of 15.9%, in part as a result of the recovery from the national strike of December 2002 and the large increase of government spending linked to rising oil revenue.

In order to at least partially address some these concerns about national economic and social trends, which could conceivably bias estimates in either direction, in the next section we turn to state-level estimation of impacts using a difference-in-differences econometric approach.

3. State Panel Regressions

We utilize variation in the number of trainers involved in *Misión Robinson* at the state level to estimate the effect of the program on literacy. We have a continuous time series of literacy rates for 21 of the 24 Venezuelan states. Both the overall state literacy rate, and the birth cohort specific literacy rate by state, are used as dependent variables. The first specification estimates:

$$Literacy_{jt} = \alpha + \beta \cdot TRAINERS_{jt} + \gamma_{1j}t + \gamma_{2j}t^2 + \gamma_{3j}t^3 + \eta_j + \theta_t + \varepsilon_{it} \qquad (2)$$

where $Literacy_{jt}$ is the literacy rate of state j at time t, η_j and θ_t are state and semester fixed effects, respectively, and $\gamma_{1j}t + \gamma_{2j}t^2 + \gamma_{3j}t^3$ captures state-specific cubic trends. $TRAINERS_{jt}$ denotes the number of trainer-semesters per adult used in the program in state j up until semester t. It thus captures the fact that the intensity of the program depends positively both on the duration and the number of trainers used in each period. In this sense, it is analogous to the specification used in column (3) of Table 3. We report the results both under the 15 years and 25 year threshold to define adulthood. Equation (2) is estimated by Generalized Least Squares with correction for autocorrelation and a heteroskedastic error structure with cross-state correlation. Since this method requires a balanced panel, we drop the three states for which a complete time series is not available since 1975 (Amazonas, Delta Amacuro and Vargas). These three states accounted for 2.17% of Venezuela's population in 2003.

There are two sources of data on the number of trainers, which give somewhat contradictory figures, as was already discussed above. One series is provided by the Venezuelan School of Social Management, which is formally part of the national Ministry of Planning and Development (Ministerio de Planificación y Desarrollo, 2006). The second series is reported in the 2004 Annual Report of the Ministry of Education (Ministerio de Educación, Cultura y Deportes, 2005). Both series of state-level data are presented in Appendix Table A1.

The Ministry of Education *Robinson* series has considerably higher values for the number of trainers than the Ministry of Planning's series for all states except Amazonas, the only state for which they are exactly equal. According to the Ministry of Education data, 1.16% of all adult Venezuelans participated as trainers in these literacy campaigns, while according to the Ministry of Planning data, that figure was approximately half, at 0.61%. Both of these series report the total number of trainers involved during the duration of the program. Since the program did not exist before the second semester of 2003 and was declared finished at the end of the first semester of 2005, we set $TRAINERS_{it}$=0 for all periods before the start of the program. There is state-level literacy data for every semester between 1975 and 2005 (except for the first semester of 1994, coinciding with the change of survey methodology, as well as the second semester of 1985, both of which are missing).

Table 5 shows the results of estimating the state-level panel specification of equation (2). Both the Ministry of Education and the Ministry of Planning data give similar results. Coefficient estimates are far from statistical significance in all cases. Estimates for the 15 and older age threshold are negative, while those for the 25 and older age subgroup are positive. Even the positive point estimates imply very small effects of *Robinson*. To understand their

magnitude, note that the average number of semester-trainers per adult person by the end of the program was 0.033 for the Ministry of Planning Data and 0.067 for the Ministry of Education data. Thus a coefficient estimate like that found in column (4) of Table 5 (0.0302) implies an increase in literacy of 0.2 percentage points due to the program, with associated confidence interval bounds of -0.7 and 1.1 percentage points. The *largest* point estimate on the table (column 7), implies an average increase in literacy over the duration of the program of 0.40 percentage points, or 48,327 persons. As in the case of the time-series regressions, these estimates are inconsistent with a large literacy effect of *Robinson*.

The key role of the state-specific cubic trend that we have used in estimating equation (2) is to capture the effect of long-run trends that may have affected literacy in each state and that may also have been correlated with the variation in intensity of the Robinson program, thus possibly biasing the estimate of β. For example, if the national government targeted states that were experiencing deteriorations in literacy with higher resources, then a negative correlation would emerge between program intensity and changes in literacy. While the introduction of state-specific trends will pick up the longer-term tendencies, they will be inappropriate at capturing short-run variations that may still be associated with program intensity and literacy improvement. For example, states that were hardest hit by the 2002-03 recession may have as a result received more government resources through the program. If state income is correlated with literacy, this will produce a downward bias in β. One way to tackle this issue is to include control for these potentially omitted variables. Thus in the lower panel of Table 5 we augment equation (2) with controls for state-level years of education, unemployment, share of population over 65, and average real income. We find that all of these variables have significant effects. Interestingly, unemployment is positively associated with improvements of literacy. This might be a reflection of the fact that individuals take

advantage of unemployment spells to improve their level of education. The results on the *Robinson* variables are largely unaffected: in none of the specifications is there a significant effect of trainers on state-level literacy.

We can use the results from the previous exercise to understand how much of the decline in illiteracy can be attributed to changes in these demographic and socio-economic indicators. Using the coefficients from the bottom panel of Table 5, we can estimate the fraction of the change in literacy since 2003 that can be attributed to changes in education, employment, age composition, and real income. The surprising result is that the effect of these changes would lead us to expect an increase of 1.5 percentage points in the over-15 and 1.7 percentage points in the over-25 literacy rates. It thus appears that the totality (if not more) of the increase in literacy that occurred since the beginning of 2003 can be attributed to demographic and socio-economic trends and not to the implementation of the *Robinson* program.

In Table 6 we try an alternative solution to the endogeneity problem. Our idea comes from the simple observation that there is a strong correlation between the amount of resources given to states through the program and the political leaning of the state governors in office when the program was initiated. Regardless of whether one uses the Ministry of Education or the Ministry of Planning data, five of the six states receiving the lowest amount of resources were under control of opposition governors in 2003[20]. We thus propose using a simple instrument for program intensity:

I_{it}=1 if program is active and state governor is pro-Chávez

I_{it}=0 otherwise. (3)

In other words, our instrument is an interaction between having a state governor whose political orientation coincides with the national government and the implementation of the program. One obvious problem with this instrument comes from the possibility that shocks to literacy could affect the election of state governors. However, all governors in power in 2003 had been elected in the 2000 local elections, a full three years before the program started. Our key identifying assumption is that shocks to literacy in 2003 are uncorrelated with electoral results in 2000. Given that low frequency fluctuations in state literacy rates will be picked up by the state-specific cubic trends, this assumption appears reasonable[21]. Our exclusion restriction assumption also seems reasonable: it is hard to think of a reason why pro-Chávez state governors would become more efficient at reducing illiteracy after 2003 that is not associated to the implementation of the *Robinson* program[22].

An alternative potential problem with our instrument comes from the possibility that it may capture the effect of omitted variables that are correlated with both literacy and the allocation of trainers. Poorer states, for example, could be more likely to have pro-Chávez governors while also being the recipients of a higher program effort. The lower panel of Table 6 shows that our instrument retains its strength even after we include controls for schooling, unemployment, share of population over 65, and real income. Obviously, these control variables may only imperfectly capture the variations across time in state-level unobservables, so that our results must be interpreted with caution. Nevertheless, it is reassuring to note that introducing these controls does not have a significant effect on the explanatory power of our instrument.

Indeed, our crude instrument is a very good predictor of program effort, attaining significance at 1% in all eight of the first stage specifications. According to the estimates form the first-stage regressions, states with pro-opposition governors received on average

between 13 and 20 trainers (equivalent to 1.8-2.7 standard deviations) per thousand adults less than those with pro-Chávez governors. It is interesting to note that neither education nor average income appear to be significantly related to the variation in the number of trainers in these regressions. Instead, the data suggests that higher unemployment and a younger population are associated with higher program effort. These results are consistent with the hypothesis that *Robinson* may have been primarily viewed as a program to generate jobs as trainers for young and middle-aged unemployed workers. The results in Table 6 also show that the instrumental variables estimates do not strengthen the evidence in favour of a program effect. Indeed, the point estimates for the 15 and older literacy rate now turn negative, although none of the estimated program effects are significant.

Our next specification uses the state-specific birth cohort literacy rate as the dependent variable. In order to minimize measurement error while taking full advantage of the information in our data, we group state-level cohorts into groups of five-year intervals according to the age of the person in the last semester of our data. In other words, one cohort will correspond to those who reach ages 20-24 in the second semester of 2005, another one to those aged 25-29, etc. We also exclude from analysis those cohorts of individuals aged less than 20 years – for which there is no sufficient pre-*Robinson* information on their literacy attainment to evaluate the program - as well as those cohorts aged over 80 (for which there are very few state-level observations in the cohort group by state in any given semester). Similarly to the analysis in Table 4 above, we estimate one specification where the *Robinson* effect is constrained to be the same across all birth cohorts, and another one in which the effect can vary by age subgroup of the cohort members at the end of the sample. We estimate the equation:

$$Literacy_{ijt} = \alpha + \beta \cdot TRAINERS_{jt} + \gamma_{1j} t + \gamma_{2j} t^2 + \gamma_{3j} t^3 + \phi_{ij} + \theta_{jt} + \varepsilon_{it} \qquad (4)$$

The key distinction between this equation and equation (2) is that we introduce a cohort-state fixed effect ϕ_{ij} as well as cohort-time specific effects θ_{jt}. Our estimation method also varies. Generalized least squares estimation with cross-sectional correlation requires that number of time periods *T* be greater than the number of cross-sectional units *N* (see Beck and Katz, 1995). If *T*<*N*, the estimated covariance matrix will not be invertible. The logical alternative is to apply OLS with clustered standard errors, which will be consistent as $N \to \infty$ (in our data *N* is the number of state-cohort groups, which equals 312). Another alternative, which would account for autocorrelation in the state-cohort series, is to use the Bhargava et al. (1982) correction for autocorrelation in the fixed effects model. Both results are reported in Table 7[23].

The results are consistent with what we have found in our previous specifications. We find no significant positive effect of *Misión Robinson* for either the Ministry of Planning or the Ministry of Education data. Indeed, the point estimates on the OLS estimates for the Ministry of Education data are significantly negative. This is not the case in the Ministry of Planning data nor when the AR(1) correction is introduced, suggesting that that particular result is sensible to specification. However, most of the estimates of Table 6 coincide in associating the intensity of the *Robinson* program with a moderate deterioration of literacy in the 55-80 cohort groups, while one of the estimates associates performance in the younger age cohorts significantly with *Robinson*.

One possibility is that these results are due to the misspecification that comes from the fact that literacy rates are constrained to be on the 0-1 range while a linear model is by definition unconstrained in this respect. This problem is likely to worsen if the state-cohort observations are observed with greater error, since in that case the upper bound on literacy is likely to be hit more frequently, biasing downwards the estimate of any variable that has a positive effect. In our data, literacy rates hit a boundary condition on 1.58% of observations. In order to verify that this is not affecting our results, we reran the specifications of Table 7 using as our dependent variable the logit transform $x_{ijt} = \ln(LIT_{ijt} /(1 - LIT_{ijt}))$. The results – available upon request – are broadly similar to those of Table 6, with the 55-90 cohort associated with a significantly negative effect of Robinson in 5 out of 8 estimations. We have also attempted a number of alternative specifications – among them omitting state trends, defining the cohort groups more or less broadly, including older cohorts, using the Baltagi and Wu (1999) random effects estimate – all of which give substantively the same results.

The estimated pattern of coefficients is surprising. One possible explanation is that is that the high profile literacy drive may have reduced the stigma from reporting illiteracy among older age cohorts. An alternative interpretation is that a broad-based program such as *Robinson* could have been effective in raising literacy among younger cohorts, but that the dismantling of existing programs could have had more detrimental effects among those older cohorts that traditional programs were designed to benefit. While those interpretations are certainly plausible, we would caution against reading too much into the pattern of the estimates. Both state-cohort literacy rates and our measure of intensity of the program are measured with error, and some of those errors could be correlated in ways that may invalidate our estimates. Our reading is thus much more conservative. We take these results as evidence that, even if one takes the data to a very fine level of detail, it is extremely hard to find

significant positive effects of *Robinson* on Venezuelan literacy. The specification of Table 7 should be interpreted as the final in a battery of tests which have attempted to identify effects of the program on the literacy data by looking at increasingly finer levels of detail. The fact that at none of these levels have we been able to find decisive evidence in favour of a *Robinson* effect suggests that the effect either does not exist or, if it does, is very hard to detect and thereby probably not very large.

IV. Concluding Remarks

This paper has shown that the implementation of *Misión Robinson* coincided with at most a moderate reduction in Venezuelan illiteracy. Most of our estimates of program impact represent quantitatively small and rarely statistically significant effects of *Robinson*, while some point estimates are actually negative. Even the most favourable estimates to *Misión Robinson* – such as, for example, the lagged specifications of the state panel regressions in Table 5 – imply quantitatively minor effects (in that case, an increase in literacy of 48,327 persons). The possibility that the *Robinson* program led some newly semi-literate individuals to claim they are literate in surveys means that even these very small gains might be overstated.

One way to evaluate the program's returns is by comparing our estimated program impacts with the program's official expenditures. According to the Ministry of Finance, *Misión Robinson* has received an investment of 80 billion Bolívares (US$50 million)[24]. This amount may well substantially understate *Robinson* expenditures, since it excludes a number of off-budget expenditures on the program[25]. Even if one attributes all of the reduction in illiteracy observed between the first semester of 2003 and the second semester of 2005 to

Robinson, the estimated cost would be $536 per pupil who learned to read. In contrast, a recent study by UNESCO of 29 international adult literacy programmes estimated the average cost per successful learner to be $47 in sub-Saharan Africa, $30 in Asia, and $61 in Latin America[26]. Under a more conservative – yet still optimistic - estimate of program success, namely that the total number of people who become literate through the program was only 48,327, then the cost per newly literate person would be much higher, at US$1035.

Why was *Robinson* unsuccessful? By any standards, *Robinson* was a large, well-funded, and high-profile effort. How then can we understand its failure to generate visible reductions in illiteracy? Does this failure reflect the idiosyncrasies of the Venezuelan experience with social policies during the Chávez era, or does it have broader implications for the design of adult literacy programmes in developing countries?

It appears probable that part of the failure of *Robinson* comes from the fact that the program was never as large as the government claimed. Official estimates of the number of program trainers imply a mobilization in this program of between 0.9 and 1.8% of the entire national labor force. Simply paying that number of trainers during two years would have entailed expenditures of $265-504 million, much higher than the $50 million allocated to the program. It would also have entailed an expansion in public employment by between 7.0% and 13.3%, which appears inconsistent with the evolution of public employment statistics[27].

Several characteristics of the program's design may also have contributed to its lack of success. The allocation of hundred-dollars-a-month scholarships to the program's needy participants – an amount equal to the legal minimum wage and significantly higher than informal sector wages - could have generated incentives for misrepresentation of literacy

status[28]. This effect may have been exacerbated by the perception of *Robinson* as an entry point to some of the government's other social programs, such as *Vuelvan Caras* (a training program for unemployed workers) that could ultimately lead to obtaining public sector employment. Anecdotal evidence also suggests that trainers devoted considerable class time to the political formation of program participants, a phenomenon that might have been exacerbated by the coincidence of the implementation period with the campaign for the 2004 recall referendum[29]. The program also appears to have been plagued by significant delays in payments to trainers, which in many cases led to high turnover rates[30].

Politicization, inadequate incentives, and budgetary problems are, however, common characteristics of large-scale literacy programs which do not appear to distinguish *Robinson* from many other cases of previous failures. Indeed what is remarkable about the record on literacy programs is that, despite a broad diversity in approaches, there are few cases of resounding success. Recent research has suggested that the problem may be in the cognitive model underlying the design of most literacy programs. As Abadzi (2003b) has argued, cognitive research has found that the process of learning to read in adult individuals may be systematically different from that in young children. The results of this literature suggest that significant changes must be made to the basic design of adult literacy programs in order for them to be successful.

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Tables and Figures

Table 1

Illiterate Population and Illiteracy Rates, National Censuses, 1936-2001

	1936	1941	1950	1961	1971	1981	1990	2001
Age Groups:								
10-14	267,413	278,155	272,656	235,541	254,340	145,639	100,080	71,528
15-19	203,195	212,094	211,387	171,622	153,432	108,785	81,640	59,723
20-24						104,430	81,055	65,494
25-34						192,095	161,211	128,629
35-44						216,068	165,234	157,618
45-54						245,518	184,992	168,226
55 and over						464,363	456,435	502,795
15 and over	1,187,376	1,302,511	1,433,852	1,499,250	1,373,561	1,331,259	1,130,567	1,082,485
Iliteracy Rate	59.26%	57.20%	49.04%	36.70%	23.29%	15.27%	9.95%	7.02%
25 and over	984,181	1,090,417	1,222,465	1,327,628	1,220,129	1,118,044	967,872	957,268
Iliteracy Rate	59.48%	58.17%	50.36%	38.99%	26.09%	15.81%	12.55%	8.74%

Source: INE (2006), Valecillos (1993), p. 174

Table 2

Time-Series Tests for Robinson Dummy

	15 and older		25 and older	
Dependent Variable: Literacy rate (%)	(1) Cubic Trend	(2) 6th Order Polynomial Trend	(3) Cubic Trend	(4) 6th Order Polynomial Trend
Robinson	-0.0020	0.0029	-0.0009	0.0033
	(0.0029)	(0.0018)	(0.0035)	(0.0022)
1(Pre-1994)	0.0049	0.0174	0.0067	0.0206
	(0.0028)*	(0.0018)***	(0.0033)**	(0.0021)***
Time	0.0015	0.0008	0.0018	0.0013
	(0.0003)***	(0.0002)***	(0.0004)***	(0.0003)***
Time2	3.6E-05	2.0E-04	2.5E-05	2.2E-04
	(0.0000)***	(0.0000)***	0.0000	(0.0000)***
Time3	9.7E-07	2.6E-05	1.0E-06	2.6E-05
	(0.0000)***	(0.0000)***	(0.0000)***	(0.0000)***
Time4		1.0E-06		9.3E-07
		(0.0000)***		(0.0000)***
Time5		1.7E-08		1.4E-08
		(0.0000)***		(0.0000)***
Time6		9.7E-11		7.5E-11
		(0.0000)***		(0.0000)***
Constant	0.9390	0.9340	0.9220	0.9170
	(0.0023)***	(0.0012)***	(0.0027)***	(0.0014)***
R-Squared	0.99433	0.99770	0.99515	0.99828
Number of Observations	60	60	60	60

Estimation sample starts in 1975-1 and ends in 2005-2. Newey-West standard errors adjusted for heteroskedasticity and autocorrelation of order 1 are in parentheses. Asterisks denote level of significance = *-10%, **-5%, ***-1%

Table 3

Alternative Specifications, time-series regressions

Dependent Variable: Literacy rate (%)	(1) Baseline	(2) Lagged	(3) Restricted	(4) Cumulative	(5) Break in Trend
	Whole Adult Population (Ordinary Least Squares)				
15 and over	-0.0020	-0.0003	0.0011	-0.0012	1.16
	(0.0029)	(0.0026)	(0.0027)	(0.0035)	(.34)
25 and over	-0.0009	0.0013	0.0054	0.0009	2.05
	(0.0035)	(0.0030)	(0.0056)	(0.0042)	(.12)
	By Age Subgroups (Seemingly Unrelated Residuals)				
55 and over	0.0055	0.0083	0.0114	0.0126	1.90
	(0.0042)	(0.0042)**	(0.0091)	(0.0078)	(.59)
45-54	0.0045	0.0067	-0.0023	0.0095	2.71
	(0.0037)	(0.0037)*	(0.0060)	(0.0066)	(.44)
35-44	0.0001	0.0036	-0.0016	0.0016	6.12
	(0.0025)	(0.0025)	(0.0033)	(0.0054)	(.11)
25-34	0.0008	0.0022	-0.0009	0.0032	2.20
	(0.0025)	(0.0024)	(0.0025)	(0.0058)	(.53)
15-24	0.0050	0.0118	0.0127	0.0164	2.61
	(0.0055)	(0.0054)**	(0.0099)	(0.0083)**	(.46)
Test for joint significance across age groups:					
Test Statistic	4.58 (.47)	8.31 (.14)	2.94 (,71)	9.8* (.08)	17.18 (.31)
Degrees of Freedom	5	5	5	5	15

All regressions include a pre-1994 indicator and a cubic trend. Standard errors in parenthesis. Column 5 corresponds to the test statistic for a Wald test that all coefficients on the terms of the Robinson*cubic trend interactions equal zero. Newey-West corrected standard errors are used for the OLS equation. SUR estimated via generalized least squares with correction for heteroskedastic error structure with cross-equation correlation and equation-specific AR(1) terms. Cumulative Robinson term increases uniformly during the application of the program and is normalized to equal one at the end of the program. Asterisks denote level of significance = *-10%, **-5%, ***-1%

Table 4

National level cohort estimates

Dependent variable: Cohort literacy rate (%)	(1)	(2)	(3)	(4)
Effect:	Contemporaneous	Lagged	Contemporaneous	Lagged
All groups	0.0004	0.0018		
	(0.0008)	(0.0009)*		
55 and over			0.0051	0.0067
			(0.0030)	(0.0030)*
45-54			-0.0013	0.0003
			(0.0026)	(0.0026)
35-44			0.0013	0.0025
			(0.0021)	(0.0021)
25-34			0.0002	0.0017
			(0.0014)	(0.0016)
21-24			-0.0018	-0.0016
			(0.0014)	(0.0014)
Chi-Squared Test of Ho: All Robinson coefficients=0			5.1100	8.9300
Number of observations	3619	3619	3619	3619
Number of cohorts	70	70	70	70

Method of estimation: Generalized Least Squares with adjustment for group-specific heteroskedasticity and autocorrelation. Dependent variable is national cohort literacy rate. All specifications include cohort dummies and cohort-specific cubic trends.

Table 5

Panel Regressions, State Literacy-Rates

Dependent variable: State-level literacy rate (%)\	Ministry of Education Data				Ministry of Planning Data			
	(1) 15 and older	(2) 15 and older	(3) 25 and older	(4) 25 and older	(5) 15 and older	(6) 15 and older	(7) 25 and older	(8) 25 and older
Trainers per capita	-0.0229		0.0312		-0.0312		0.1185	
	(0.0603)		(0.0730)		(0.1127)		(0.1495)	
Trainers per capita lagged		-0.0383		0.0302		-0.1062		0.0522
		(0.0639)		(0.0759)		(0.1197)		(0.1545)
Number of observations	1260	1239	1260	1239	1260	1239	1260	1239
Number of status	21	21	21	21	21	21	21	21
Specification with Additional Controls:								
Trainers per capita	-0.0368		-0.0368		-0.0156		0.0346	
	(0.0467)		(0.0573)		(0.1016)		(0.1419)	
Trainers per capita lagged		-0.038		-0.0392		-0.0527		-0.0323
		(0.0489)		(0.0618)		(0.107)		(0.1534)
Years of Schooling	0.0497	0.0497	0.0588	0.0591	0.0497	0.0496	0.0589	0.0592
	(0.0013)***	(0.0013)***	(0.0017)***	(0.0017)***	(0.0012)***	(0.0013)***	(0.0017)***	(0.0017)***
Unemployment	0.0514	0.0522	0.0648	0.0643	0.0506	0.0516	0.0628	0.0627
	(0.0083)***	(0.0084)***	(0.0116)***	(0.0115)***	(0.0083)***	(0.0084)***	(0.0116)***	(0.0116)***
Share of Population over 65	-0.7942	-0.7856	-1.0942	-1.1006	-0.7935	-0.7813	-1.0942	-1.0946
	(0.0676)***	(0.0683)***	(0.0912)***	(0.0915)***	(0.0675)***	(0.0684)***	(0.0915)***	(0.0919)***
Real Income	0.003	0.0023	0.0055	0.0041	0.003	0.0023	0.0054	0.0040
	(0.0012)***	(0.0012)**	(0.0016)***	(0.0016)**	(0.0012)**	(0.0012)**	(0.0016)***	(0.0017)**
Number of observations	1260	1239	1260	1239	1260	1239	1260	1239
Number of states	21	21	21	21	21	21	21	21

All regressions include state fixed effects, period dummies and state-specific cubic trends. Estimation is by Generalized Least Squares with adjustment for autocorrelation of order 1 and a heteroskedastoc error structure with cross-sectional correlation.. Period of estimation is from 1975-1 to 2005-2 and covers all states except Vargas, Amazonas and Delta Amacuro. Asterisks denote level of significance = *-10%, **-5%, ***-1%

Table 6

Instrumental Variables Estimates

Dependent variable: State-level literacy rate (%)\	Ministry of Education Data		Ministry of Planning Data	
	Baseline	Lagged	Baseline	Lagged
	Baseline Specification			
	First Stage Results			
Pro-Chávez governors*program active	.0201 (.0041)***	.0191 (.0039)***	.0134 (.0019)***	.0128 (.0017)***
	Second Stage Results			
15 and older	-0.1786	-0.2544	-0.267	-0.3802
	(0.1988)	(0.2067)	(0.2939)	(0.3027)
25 and older	0.0662	0.0084	0.099	0.0125
	(0.2386)	(0.2480)	(0.3566)	(0.3706)
Observations	1260	1239	1260	1239
Number of states	21	21	21	21
	Specification with Additional Controls			
	First Stage Results			
Pro-Chávez governors*program active	.0200 (.0041)***	.0192 (.0039)***	.0134 (.0019)***	.0129 (.0018)***
Years of Schooling	-.0002	.0002	-.0007	-.0007
	(.0011)	(.0010)	(.0003)	(.0003)
Unemployment	.018	.0132	.0120	.0105
	(.0065)***	(.0063)**	(.0034)***	(.0032)***
Proportion Over 65	-.1189	-.1159	-.0416	-.0393
	(.0410)***	(.0436)***	(.0169)**	(.0168)**
Average Real Income	.0002	-.0000	-.0001	-.0001
	(.0003)	(.0003)	(.0002)	(.0002)
	Second Stage Results			
15 and older	-0.1479	-0.2869	-0.2207	-0.4274
	(0.1728)	(0.1795)	(0.2605)	(0.2682)
25 and older	0.096	-0.0399	0.1433	-0.0595
	(0.2181)	(0.2220)	(0.3219)	(0.3317)
Observations	1260	1239	1260	1239
Number of states	21	21	21	21

Estimation by Panel Two Stage Least Squares. Standard Errors robust to heteroskedasticity and autocorrelation, calculated with a Bartlett kernel and bandwidth=1, in parentheses. All equations include state fixed effects, period dummies, and state-specific cubic trends. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 7

State-Level Cohort Panel Estimation. Dependent variable is literacy rate

Dependent variable: State-specific birth cohort literacy rate (%)	Ministry of Education Data		Ministry of Planning Data	
	Contemporaneous	Lagged	Contemporaneous	Lagged
All Groups - OLS	-0.2910	-0.2837	-0.5411	-0.4432
	(0.1383)**	(0.1348)**	(0.4669)	(0.4585)
55-90	-0.3911	-0.3788	-1.1693	-0.9888
	(0.1736)**	(0.1656)**	(0.3224)***	(0.302)***
45-54	-0.5112	-0.4987	-0.9069	-0.8256
	(0.1946)**	(0.1971)**	(0.3435)**	(0.3702)**
35-44	-0.2257	-0.1970	0.0529	0.1339
	(0.1708)	(0.1645)	(0.8999)	(0.9016)
25-34	-0.0844	0.0000	0.3203	0.6147
	(0.1448)	(0.1441)	(0.8373)	(0.8687)
21-24	0.1473	0.2319	0.7033	0.9542
	(0.118)	(0.1212)*	(0.7145)	(0.7261)
All Groups - AR1	-0.1018	-0.0825	-0.1451	0.0224
	(0.128)	(0.1188)	(0.2542)	(0.2392)
55-90	-0.2215	-0.1637	-0.9767	-0.6633
	(0.1567)	(0.1518)	(0.3073)***	(0.3021)**
45-54	-0.3376	-0.3513	-0.6588	-0.4294
	(0.2097)	(0.2154)	(0.4091)	(0.4243)
35-44	-0.1096	-0.0605	0.1392	0.3850
	(0.2122)	(0.223)	(0.4128)	(0.4359)
25-34	0.0039	0.0747	0.2490	0.5091
	(0.2196)	(0.2357)	(0.4252)	(0.4561)
21-24	0.1907	0.2410	0.5088	0.6734
	(0.2302)	(0.2455)	(0.4444)	(0.4735)

All regressions include state-cohort fixed effects, state-specific cubic trends, and cohort-semester dumies. Standard errors in parentheses. Standard error estimates of OLS regressions are clustered by state and robust. AR(1) estimates are the Bhargava et al. (1982) autocorrelation-corrected fixed effects estimators. Asterisks denote level of significance = *-10%, **-5%, ***-1%.

Table A1

Robinson trainers per state

	Ministry of Education		Ministry of Planning	
	Total	Per Adult Person	Total	Per Adult Person
Amazonas	1,293	0.0221	1,293	0.0221
Anzoátegui	15,326	0.0193	12,133	0.0153
Apure	8,922	0.0258	5,049	0.0146
Aragua	7,666	0.0069	4,137	0.0037
Barinas	12,434	0.0293	6,664	0.0157
Bolívar	6,781	0.0073	3,338	0.0036
Carabobo	3,971	0.0024	2,180	0.0013
Cojedes	5,695	0.0293	1,833	0.0094
Delta Amacuro	2,748	0.0278	1,137	0.0115
Distrito Capital	10,670	0.0069	2,528	0.0016
Falcón	9,613	0.0186	5,035	0.0098
Guárico	6,519	0.0140	4,018	0.0086
Lara	14,421	0.0122	12,962	0.0110
Mérida	4,887	0.0087	3,097	0.0055
Miranda	2,496	0.0012	977	0.0005
Monagas	12,558	0.0291	2,986	0.0069
Nueva Esparta	5,081	0.0171	1,421	0.0048
Portuguesa	9,979	0.0164	8,207	0.0135
Sucre	17,396	0.0315	6,796	0.0123
Táchira	11,556	0.0153	6,487	0.0086
Trujillo	17,949	0.0443	6,540	0.0161
Vargas	4,727	0.0211	705	0.0031
Yaracuy	5,265	0.0139	3,773	0.0100
Zulia	12,457	0.0051	7,407	0.0030
Total	210,410	0.0116	110,703	0.0061

Source: Ministerio de Educación (2005), p. 913, Ministerio de Planificación y Desarrollo (2006).

Figure Legends

Figure 1

Title: Literacy rates in Venezuela, 1975-05

Legend:

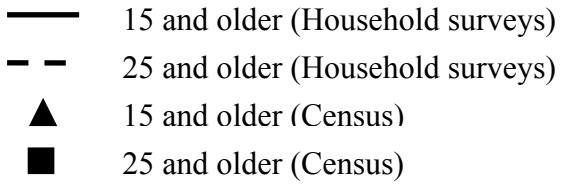


Figure 2

Title: Literacy rates by age groups, 1994-2004

Legend:

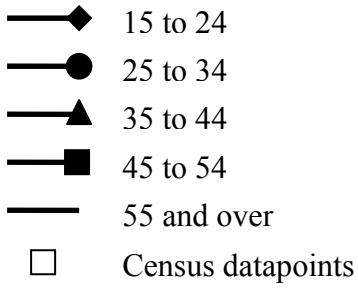


Figure 3

Title: Historical and fitted national literacy rate for alternative trends

Legend:

Historical
Cubic trend
Linear trend
Quadratic trend

Figure 1

Literacy rates in Venezuela, 1975-05

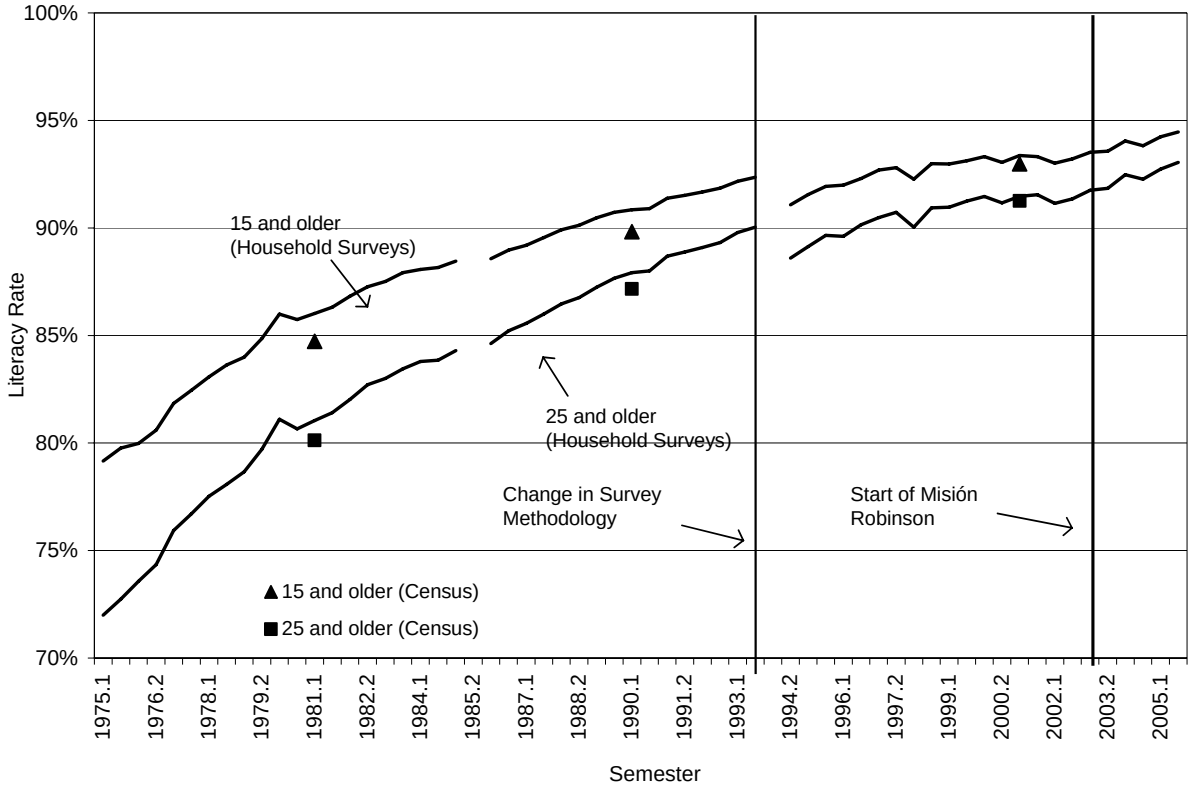


Figure 2

Literacy rates by age groups, 1994-2004

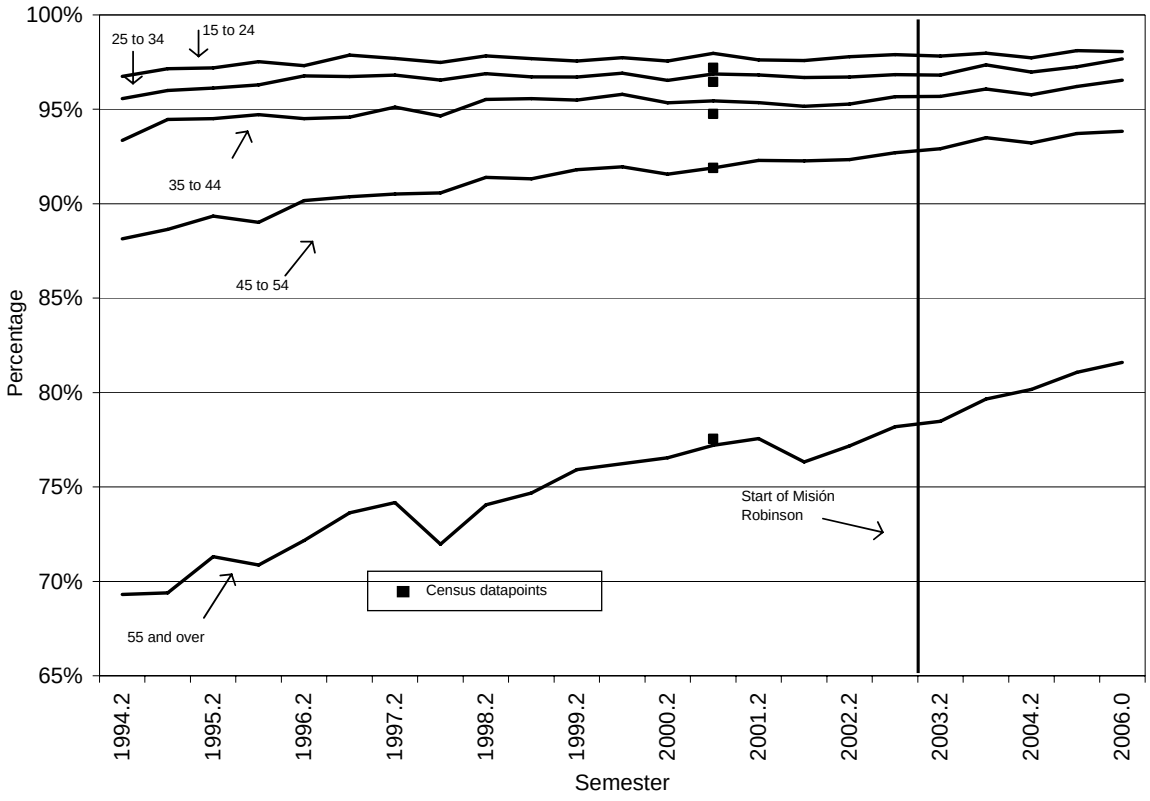
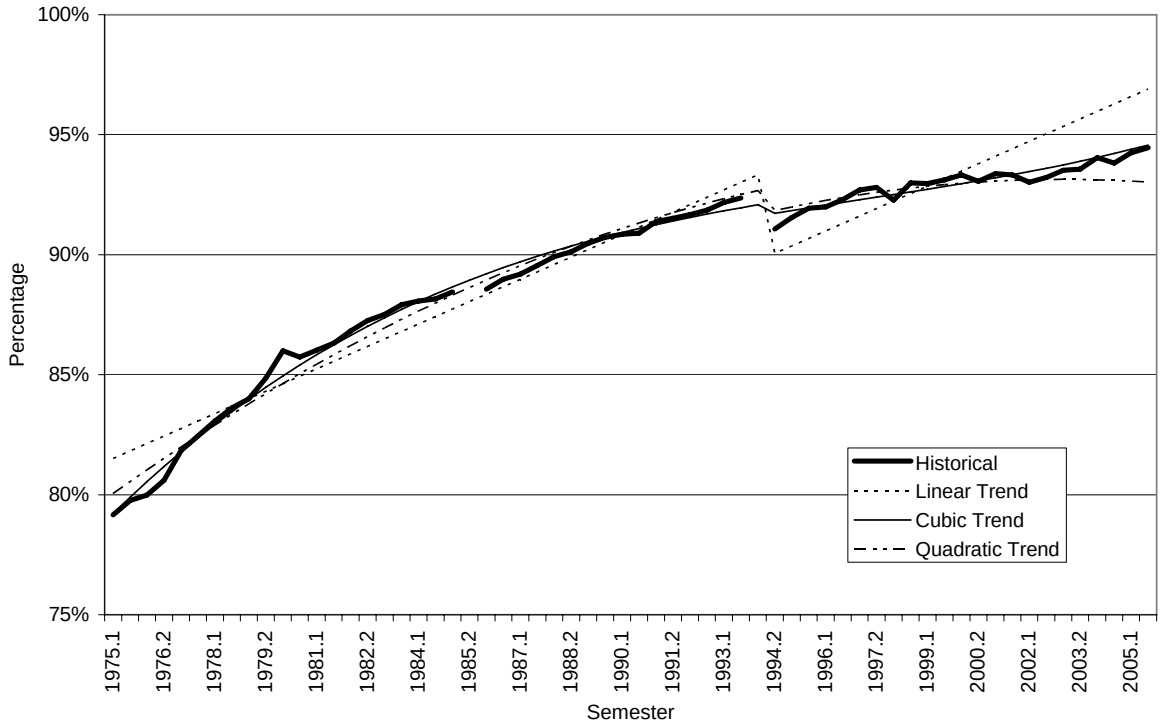


Figure 3

Historical and fitted national literacy rate for alternative trends



Notes

[1] "Territorio Libre de Analfabetismo" in Spanish. Gobierno Bolivariano de Venezuela (2005a), p. 19.

[2] The source cited for this information is a presentation made at the UNESCO meetings by the Cuban Communist Party's organization Juventud Rebelde.

[3] The efficiency rate of a literacy program is the fraction of those enrolled who are able to pass a writing and reading comprehension exam and do not drop back into illiteracy later on.

[4] See Prato (2006).

[5] Gobierno Bolivariano de Venezuela (2004), p. 11.

[6] The census figures are based on respondents' answer to the question "Does this family member know how to read and write?" ("¿Sabe leer y escribir?") and is thus identical to the Household Survey Question we will use in the rest of the analysis. Both the Census and the Households Survey are administered by the National Statistical Institute.

[7] Gobierno Bolivariano de Venezuela (2005a), p. 17.

[8] On October 29th, Minister Istúriz noted that the 1.2 and 1.5 million estimates referred to the over-15 rate, citing a 2000 UNESCO study (instead of the 2001 Census) as the source for the 1.2 million figure (Gobierno Bolivariano de Venezuela, 2005b, p. 5).

[9] Traditionally developed economies generally do not collect adult illiteracy data and are assumed to have adult literacy rates above 99%. See UNDP (2005), p. 222, footnote e to Table 1.

[10] This refers to the UNESCO Institute of Statistics 2002 estimate. See UNDP (2005), p. 222, footnote k to Table 1.

[11] See Mendoza (2005).

[12] Figure 1 also shows that the literacy estimates derived from the household survey became quite close to those derived from the national census after the 1994 change in methodology, suggesting that the survey does not systematically under represent illiterate groups of the population. It is of course possible that both the census and the survey fail to pick up some illiterate groups, leading to an underestimate of the level of illiteracy in both series. If these groups become more likely to be picked up by the survey once they become literate, then our estimates of program effectiveness will be biased upwards. If, in contrast, these groups remain out of the survey coverage even if they become literate, the bias may operate in either direction. Even in that case, our estimate of absolute number of illiterate persons would still be a lower bound for the actual number.

[13] According to official estimates, 57.4% of program participants were older than 41, a much greater proportion than their share of the population, which is 34.2% (Gobierno Bolivariano de Venezuela, 2005b, p. 31).

[14] Estimation for higher order polynomial trends, available from the authors upon request, yielded substantially the same results.

[15] For the 1994-2000 sample, we find that literacy rates among individuals who self-report their status tend to be 1.15 percentage points higher than among those whose status is reported by others. The specification includes controls for gender, level of education, head of household status, age, and semester dummies. Details of the estimation are available from the authors.

[16] 39.6% of heads of households and 56.8% of spouses are informants, in contrast to 13.5% of other respondents. This is primarily a result of the interview protocol, which orders interviewers to select as their informant the head of household or, if s(he) is not present, their

spouse. If neither of these is present, the interviewer can choose another adult as the informant.

[17] The total effect is normalized to equal one in the last period during which the program was operative. Therefore, this variable takes the value 0 up to the first semester of 2003, after which it increases by ¼ every semester until the first semester of 2005, when it reaches 1 and remains there until the end of the sample.

[18] Cohort-specific literacy rates may still be affected by compositional changes due to migration and mortality among members of the birth cohort.

[19] We have carried out a number of simulations to estimate the power of our tests against the alternative of a moderately effective program and have found them to be reasonably high powered. For example, in the case of equation (1) in Table 1, our simulations indicate that under an AR(1) disturbance with autoregressive coefficient of .8 and standard error of the white noise component of half a percentage point, the power of the z-statistic would be .70 against an alternative in which *Robinson*'s effect was 1 percentage point and .98 against one in which it was 2 percentage points.

[20] See Penfold (2007) for evidence of the use of political criteria in the allocation of Misiones expenditure.

[21] After accounting for state-specific cubic trends, we found no evidence of serial correlation of order greater than one in the state-level series.

[22] Even if pro-Chávez governors became more efficient at reducing illiteracy after 2003 for reasons unrelated to the implementation of the program, this would actually bias the coefficient on trainers upwards and thus against our null. Note that the just-identified nature of our system impedes us from testing the exclusion restriction directly.

[23] We drop all state-age cohorts older than 80, fore which there are too few observations to estimate state-cohort literacy rates with any degree of precision. Our results are qualitatively similar if we include those age cohorts.

[24] Ministerio de Finanzas (2006). Interestingly, all of this allocation belongs to the 2003 budget, with no budget allocations for 2004 and 2005. However, Venezuelan law allows carrying over non-spent budget lines from one year to another, so that these resources may have been spread out over the whole period. According to its financial statements, the state-owned oil enterprise PDVSA (which directly finances many of the misiones) did not give any additional funding to Misión Robinson. It is, however, possible that other state-owned enterprises financed Robinson directly.

[25] For example, it excludes the value of donations made by Cuba within the context of the Cuban-Venezuelan Cooperation Agreement, through which Venezuela receives in-kind transfers in exchange for favourable conditions in oil sales. Cuban donations to the program included 1.9 million textbooks, 200,000 literacy trainer manuals, 80,000 television sets and VCR's for classroom use, 1 million literacy lesson videotapes, 2 million family libraries and 300,000 pairs of eyeglasses. Gobierno Bolivariano de Venezuela (2005b), p.2. Since the Agreement also covers donations given by Cuba to Venezuela for other purposes (among them the services of more than 10,000 Cuban doctors that participate in Misión Barrio Adentro), it is difficult to disentangle the cost to Venezuela of the donations given for Misión Robinson.

[26] UNESCO (2006), p. 235.

[27] Public employment did grow significantly during this period, but most of this growth appears to have come from other misiones – which did receive much higher levels of funding - and an expansion of public employment in areas unrelated to social policy such as the state-owned oil enterprise.

[28] Gobierno Bolivariano de Venezuela (2004)

[29] Trainers were required to devote time to "citizen formation", defined as "learning in subjects referred to the Constitution, the re-foundation of the Fatherland, the Boliviarian revolution, among others." (Ministerio de Educación, Cultura y Deportes 2005, p. 98) The program's final assignment required students to demonstrate their newly acquired skills by writing a letter to president Chávez. See Frente Internacional "Yo si puedo" (2007).

[30] See, for example Mendoza (2005), and Sánchez (2003)