Wesleyan Economics Working Papers

http://repec.wesleyan.edu/
N°: 2006-025

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October 2006

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Freed from Illiteracy?

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

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October 2006

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.

* We thank the Venezuelan National Institute of Statistics for providing access to the Households survey. This paper has benefited from lengthy discussions with Chang-Tai Hsieh. María Eugenia Boza, Ricardo Hausmann, Manolis Kaparakis, Michael Penfold, José Pineda, Sanjay Reddy, and Alberto Unanue also provided valuable comments and suggestions. Corresponding author: Francisco Rodriguez, frrodriguez@wesleyan.edu, Wesleyan University, 238 Church Street, Middletown, CT 06459. The usual disclaimer applies.

I. Introduction

On October 28, 2005, Venezuelan President Hugo Chávez declared Venezuela "Illiteracy-Free Territory" ("*Territorio Libre de Analfabetismo"* in Spanish) in a nationally televised event held in the capital's Teresa Carreño Theater.[1] The announcement claimed what appeared to be a crowning success of the national literacy program *Misión Robinson*, launched on July 1, 2003. According to official claims, between the start of the program and the announcement made a little over two years later, the Cuban designed *Yo Sí Puedo* program had helped teach 1,482,543 persons how to read and write (Gobierno Bolivariano de Venezuela, 2005e, p.5).

The achievement received considerable international recognition. Among others UNESCO General Director Koichiro Matsuura sent the following message of congratulations and praise:

> The achievements reached by Misión Robinson would not have been possible were it not for the political will and support at the highest levels and for that, President Hugo Chávez Frías merits warm congratulations ... This is an example of a national compromise that I hope will serve as inspiration to others to accelerate their actions and free their countries, and the world in general, of the burden of illiteracy. (cited by Gobierno Bolivariano de Venezuela, 2005d).

The Venezuelan government's claim of illiteracy eradication 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 and December 2003. The source cited for this information is a presentation made at the UNESCO meetings by the Cuban Communist Party's organization *Juventud Rebelde*. Venezuelan sociologist Luis Pedro España, director

[1] Gobierno Bolivariano de Venezuela (2005a), p. 19.

of the Andrés Bello Catholic University's Institute for Economic and Social Research and a staunch critic of government policies, has accepted as a fact the dramatic reduction in illiteracy, and its proximity to zero, while pointing out that this may be the reflection of long-run trends.[2]

But what exactly is the evidence of this dramatic decline in illiteracy? Although there are plenty of statements from government officials giving estimates of illiteracy reductions, it is not altogether clear how these estimates have been constructed or whether they can be trusted. There is no official statistical series tracking illiteracy before and after the program, no published report explaining the methodology used to construct government estimates, and, to the best of our knowledge, no independent published evaluations of the program's effectiveness.

The availability of the official government Household Surveys, which includes self-reports on literacy, allows us to evaluate official claims of having wiped out illiteracy. We find a small decline in the number of illiterate persons in Venezuela between the start and the end of the program. This decline appears to be indistinguishable from what one would expect based on the long-run evolution of illiteracy rates. 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

[2] España, Luis Pedro, "Mentiras," *El Nacional*, September 30, 2006, p. A7.

in many specifications *Robinson* program impacts are statistically indistinguishable from zero.

The relevance of this analysis goes beyond ascertaining whether the current Venezuelan administration has been successful in using the country's oil wealth to benefit the neediest members of Venezuelan society. 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 the almost complete halt in World Bank financing of adult literacy programs since 1990 (Chowdury, 2005). 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.

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.[3]

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-

[3] "Misión Robinson: un híbrido cubano en Venezuela" *El Impulso*, May 13, 2006.

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$83). The intensive course lasts seven weeks, and graduates have immediate access to *Misión Robinson 2*, a follow-up program designed to provide the equivalent of a primary school education. In total 150,000 scholarships with a stipend equal to that of trainers were given to needy participants, commonly when they reached the *Misión Robinson 2* stage. A special Solidarity Fund was set up, with 10% of program contributions going to fund program participants with special needs (such as the purchase of wheelchairs, crutches, or medical care). [4]

Some of the official information on the number of trainers that participated in *Misión Robinson* is seemingly contradictory. On October 28, 2005, Education Minister Istúriz claimed that 128,967 trainers had been involved in the program (Gobierno Bolivariano de Venezuela, 2005e, p. 4). The 2004 Annual Report of the Education Ministry, however, had claimed that by the end of 2004 a considerably higher figure, 210,353 trainers, had been involved (Ministerio de Educación, Cultura y Deportes, 2005, p. 913). The Ministry of Planning and Development, in contrast, reports a total of 110,703 trainers involved in *Misión Robinson* through June of 2005 (Escuela de Gerencia Social, 2006). The difference between the Ministry of Education's earlier numbers and the Ministry of Planning data may be due to the high turnover rates of trainers, which some informal field reports have put at around 40%.[5] Both series are available at the state-level and will be used in our analysis below. Despite these discrepancies, according to these government data, a massive 1 to 2% of the total Venezuelan labor force of 11.74 Million was employed in the government's adult literacy drive during 2003-2005.

[4] Gobierno Bolivariano de Venezuela (2004), p. 11.
[5] *Diario El Carabobeño*, "40% de facilitadores ha desertado de Misión Robinson en Carabobo," 8/31/2005.

There is somewhat greater agreement among official sources regarding claims of the program's success in almost completely eradicating illiteracy. The government's official declaration of Venezuela as "Illiteracy-Free Territory" (*Territorio Libre de Analfabetismo*), made by President Chávez on October 28 of 2005, claimed that 1,482,543 persons had been successfully taught to read and write (Gobierno Bolivariano de Venezuela, 2005e, p.5). Several estimates have been announced by official spokespersons, all of them between 1.4 and 1.5 million. The first formal announcement of the achievement was made by Education Minister Aristóbulo Istúriz during the July 3, 2005 edition of President Chávez's weekly program *Aló Presidente*, where he gave the figure of 1,436,637 persons. On October 19, María Luz Monte, member of the Misión Robinson Presidential Commission, had announced that by August 10, 1,406,858 persons had been taught how to read and write (thirty thousand less than Minister Istúriz's announcement made one month earlier). On the same day of the October 28 event, during the opening ceremony of the Ministerial Dialogue on the Social Charter of the Americas, Minister Istúriz claimed that the 1.5 million threshold had already been surpassed.[6] Any of these estimates would imply the reduction of Venezuelan illiteracy to a negligible fraction of the population.

However, the precise sources 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.

[6] Gobierno Bolivariano de Venezuela (2005a, 2005b).

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. 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 in the July 3 *Aló Presidente* program, where he 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]

The National Statistical Institute's (Instituto Nacional de Estadística, INE) Household Survey, however, allows us to independently verify the government's

[7] Gobierno Bolivariano de Venezuela (2005a), p. 17.
[8] During the October 29th event, 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, 2005e, 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.

claim. 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.

In the next sections, we present 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.

III. 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.

Thus, relying on official Household Survey data, Venezuela's literacy gains, while significant, have not erradicated 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.[11] 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

[11] 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, 2005e, p. 31).

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). We also display the effect of a cubic time trend, which visually appears to provide 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:

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

where $ROBINSON_t$ is an indicator variable that takes the value 1 after the start of the literacy program, *1(Post-1994)* is an indicator that takes on a value 1 after 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 just a linear trend (Table 2, column 1), the estimated *Robinson* program effect is actually large, negative and statistically significant, at a drop of 2.2 percentage points in the literacy rate. We view this result as a consequence of the downward bias imparted by imposing an inappropriately linear time trend on inherently nonlinear data. With a more reasonable cubic time trend (column 3) the estimated *Robinson* effect becomes slightly negative but now is no longer statistically significant, with an estimated drop in literacy of only 0.2 percentage points. Thus using a cubic time trend, which is visually quite successful at capturing pre-program literacy trends, the estimated effect of the program is basically a precisely estimated zero (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 decrease in the literacy rate of 3.0 percentage points in the linear time control specification (column 4), although again we argue that the linear time control is likely to be unreliable. The program is associated with a small and statistically

insignificant decrease in literacy of 0.09 percentage points with a cubic time control (column 5).

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, column 3) yields a positive and statistically insignificant effect of *Robinson* on both the over-15 (.0029, p=0.124) and the over-25 (.0033, p.=.133) 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 (comparing columns 2 and 3). 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. Reported results

include a cubic time trend control.[12] 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 two 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 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. The last column of Table 3 tries yet another specification, in which the effect of *Robinson* is assumed to increase uniformly in

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

every period during which the program was in operation.[13] 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.

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, beyond any compositional changes due to migration and mortality among members of the birth cohort. 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

[13] 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.

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. A few of our 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.

IV. 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:

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

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 (3) 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, 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 (3). 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. The point estimates imply very small effects of *Robinson*. To take one example, the *largest* point estimate on the table (column 7), implies an average increase in literacy over the duration of the program of 0.42 percentage points, or 51,136 persons.

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 (3) 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, 1985). 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 6.[14]

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

[14] 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.

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.

V. Concluding Remarks

This paper has tried to establish three things. The first is that Venezuela is currently very far from eradicating illiteracy. According to the government's own Household Surveys, 1 million Venezuelans claimed not to know how to read and write at the end of 2005, only slightly less than the 1.1 million at the start of 2003. 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.

The second is that the implementation of *Misión Robinson* coincided with at most a moderate reduction in Venezuelan illiteracy. Most program impact estimates represent quantitatively small and rarely statistically significant effects of *Robinson*, with 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 51,136 persons).

The third fact is that these very small program impacts appear to have been purchased at a very high cost for Venezuelan taxpayers. Even if we assume that all of the literacy gains observed between the first semester of 2003 and the second semester of 2005 were achieved as a result of *Misión Robinson*, and this is likely to seriously overestimate program effects given the strong pre-existing trends, each

trainer would have led only one additional person to become literate on average, hardly a rousing success. With more realistic program impact estimates, the ratio becomes much less favourable for the program.

Another way to analyze the issue of program return 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), all of it allocated in the 2003 budget.[15] This amount may well substantially understate *Robinson* expenditures, since it excludes a number of off-budget expenditures on the program. 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.[16]

The Ministry of Finance's budget estimates, thus, give us a lower bound for the cost of the program. Even if one again 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, and again this is probably a gross overestimate of actual program cost effectiveness. In

[15] Ministerio de Finanzas, 2006. There was no budget allocated to Robinson 1 for 2004 and 2005 according to the Ministry's official numbers. 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.

[16] Gobierno Bolivariano de Venezuela (2005e), p.2. The total cost to Venezuela (mainly in terms of subsidized petroleum exports) of the agreement with Cuba has been estimated at up to 2.6 billion dollars, though these calculations are controversial. See *El Universal, "El país pierde con Cuba,"* [Interview with José Toro Hardy], February 3, 2002, as well as the Cuban Embassy's reply (Embajada de Cuba en Venezuela, 2002). 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*.

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.[17] The costliest programme surveyed by UNESCO was Bolivia's *Ayuda en Acción* programme, which was estimated to cost $199 per successful pupil. *Robinson* costs nearly three times as much as the Bolivian program even under highly optimistic assumptions. Under more a more – yet still optimistic - estimate of program success, namely that the total number of people who become literate through the program was only 51,136, then the cost per newly literate person would be much higher, at US$977.

While this paper's program impact estimates are not inconsistent with *Misión Robinson* having a small to moderate positive effect on Venezuelan literacy, longer-term effects of the government's seemingly hasty declaration of the end of illiteracy in Venezuela could be damaging, by reducing political support for further educational investments. With the end of *Robinson 1*, Venezuela today lacks any public sector-sponsored adult literacy program to attend the more than 1 million Venezuelans who have not learned how to read and write.

[17] UNESCO, 2006, p. 235.

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Tables and Figures

Table 1: Illiterate Population and Illiteracy Rates, National Censuses, 1936-2001

Age Groups	1936 Illiterate	1941 Illiterate	1950 Illiterate	1961 Illiterate	1971 Illiterate	1981 Illiterate	1990 Illiterate	2001 Illiterate
10-14	267413	278155	272656	235541	254340	145,639	100,080	71,528
15-19	203195	212094	211387	171622	153432	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 y más						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

Figure 1: Literacy Rates in Venezuela, 1975-05

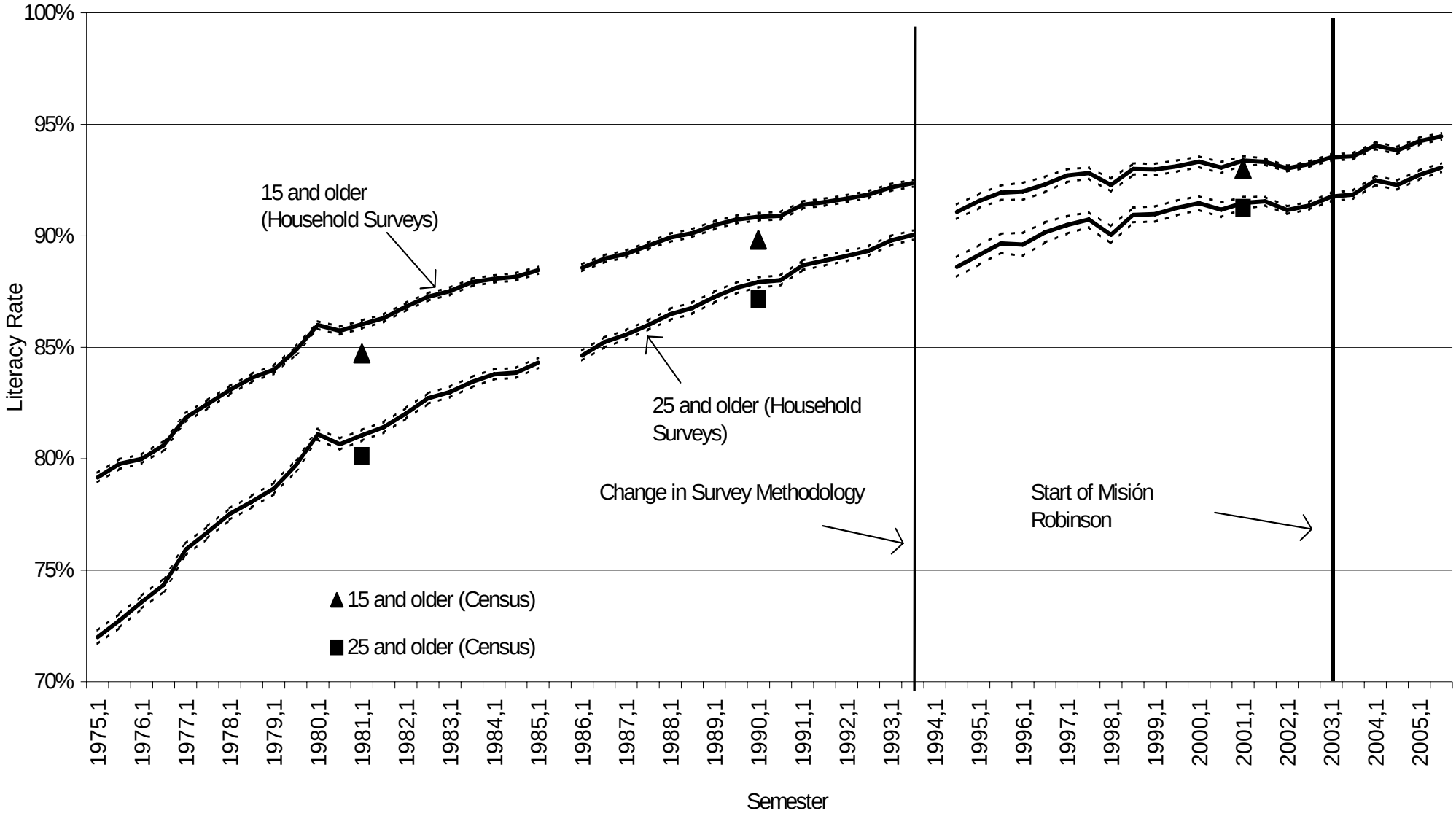

Figure 2: Literacy Rates by Age Groups, 1994-2004

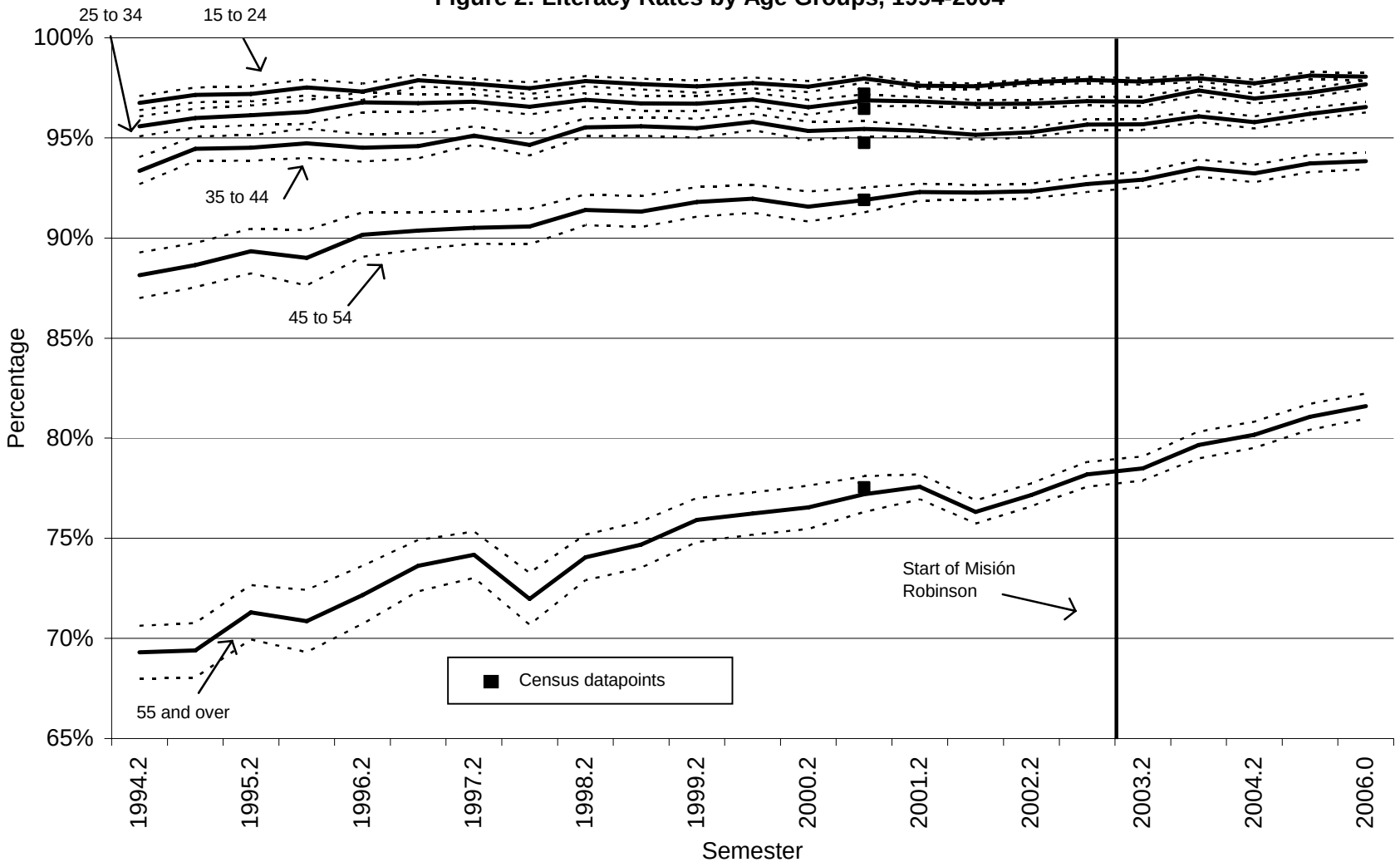

Figure 3: Historical and Fitted National Literacy Rate for Alternative Trends

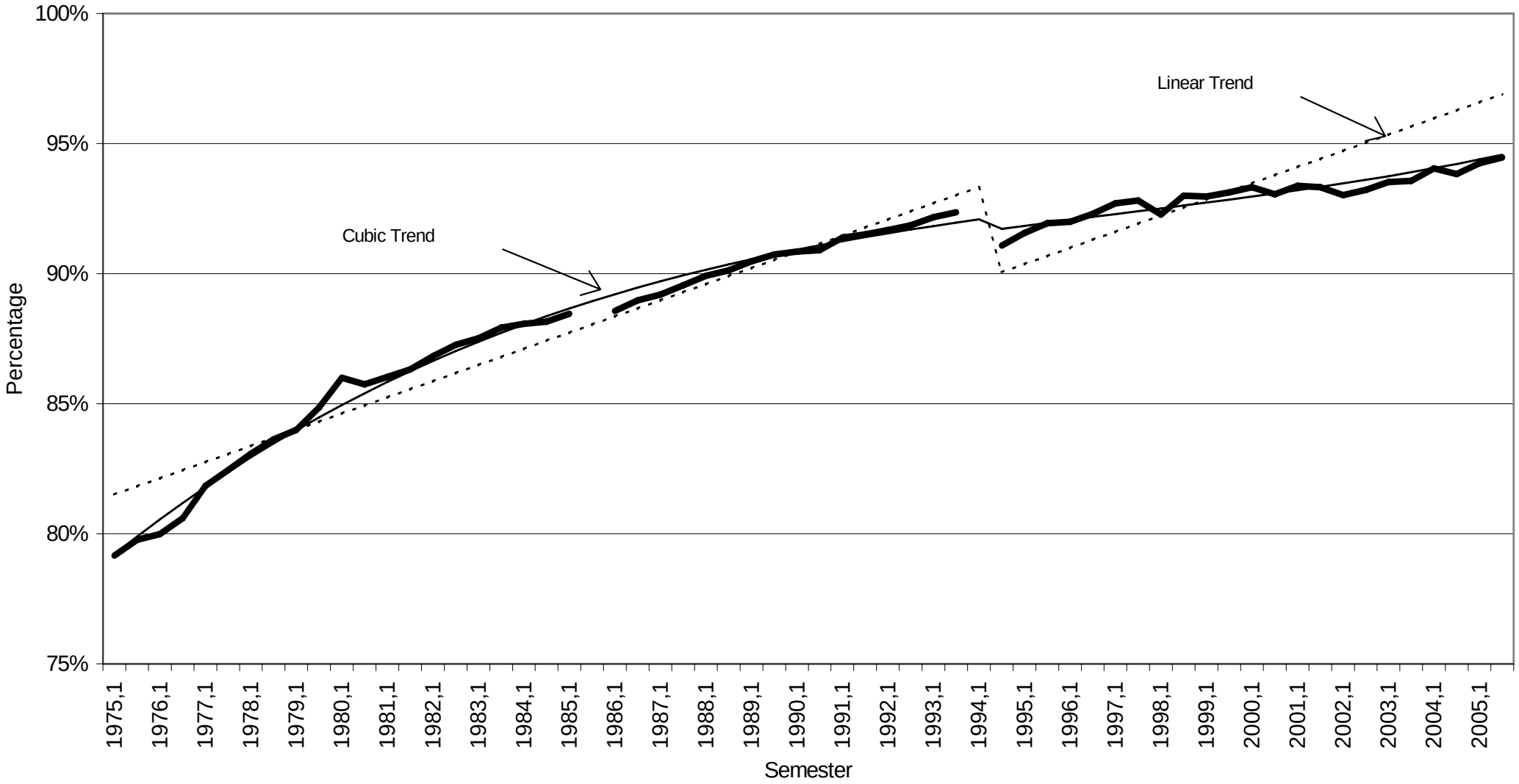

Table 2: Time-Series Tests for Robinson Dummy

	National Rate (15 and older)			Adult Rate (25 and older)		
	(1)	(2)	(3)	(4)	(5)	(6)
Robinson	-0.0224	-0.0020	0.0029	-0.0300	-0.0009	0.0033
	(-0.0047)***	(-0.0029)	(0.0018)	(0.0062)***	(0.0035)	(0.0022)
1(Post-1994)	0.0358	0.0049	0.0174	0.0451	0.0067	0.0206
	(0.0061)***	(0.0028)*	(0.0018)***	(0.0077)***	(0.0033)**	(0.0021)***
Time	0.0031	0.0015	0.0008	0.0042	0.0018	0.0013
	(0.00023)***	(0.0003)***	(0.0002)***	(0.0003)***	(0.0004)***	(0.0003)***
$Time^2$		3.6E-05	2.0E-04		2.5E-05	2.2E-04
		(0.0000)***	(0.0000)***		0.0000	(0.0000)***
$Time^3$		9.7E-07	2.6E-05		1.0E-06	2.6E-05
		(0.0000)***	(0.0000)***		(0.0000)***	(0.0000)***
$Time^4$			1.0E-06			9.3E-07
			(0.0000)***			(0.0000)***
$Time^5$			1.7E-08			1.4E-08
			(0.0000)***			(0.0000)***
$Time^6$			9.7E-11			7.5E-11
			(0.0000)***			(0.0000)***
Constant	0.9565	0.9390	0.9340	0.9463	0.9220	0.9170
	(0.0043)***	(0.0023)***	(0.0012)***	(0.0056)***	(0.0027)***	(0.0014)***
R-Squared	0.94917	0.99433	0.99770	0.95802	0.99515	0.99828
Number of Observations	60	60	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

		Baseline	Lagged	Cumulative
15 and over	OLS	-0.0020	-0.0003	-0.0012
		(0.0029)	(0.0026)	(0.0035)
25 and over		-0.0009	0.0013	0.0009
		(0.0035)	(0.0030)	(0.0042)
55 and over	SUR	0.0055	0.0083	0.0126
		(0.0042)	(0.0042)**	(0.0078)
45-54		0.0045	0.0067	0.0095
		(0.0037)	(0.0037)*	(0.0066)
35-44		0.0001	0.0036	0.0016
		(0.0025)	(0.0025)	(0.0054)
25-34		0.0008	0.0022	0.0032
		(0.0025)	(0.0024)	(0.0058)
15-24		0.0050	0.0118	0.0164
		(0.0055)	(0.0054)**	(0.0083)**
Test for joint significance across age groups		4.5768	8.3114	9.8042*

All regressions include a pre-1994 indicator and a cubic trend. Standard erros in parenthesis. 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

	(1)	(3)	(5)	(7)
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

	Ministry of Education Data				Ministry of Planning Data			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	15 and older	**15 and older**	**25 and older**	**25 and older**	**15 and older**	**15 and older**	**25 and older**	**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	1260	1260	1260	1260	1260	1260	1260
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. State-Level Cohort Panel Estimation. Dependent variable is 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).