Schooling and Labor Market Impact of the 1968 Nine-Year Education Program in Taiwan*

Diana E. Clark
Department of Agricultural and Resource Economics
University of California at Berkeley
Berkeley, CA 94720
dclarkr@are.berkeley.edu

Chang-Tai Hsieh
Department of Economics and Woodrow Wilson School
Princeton University
Princeton, NJ 08544
chsieh@princeton.edu

Preliminary; comments welcome

Abstract:

The extension of basic schooling from six to nine years in 1968 was the largest expansion of education in Taiwan's modern history. More than 140 new junior high schools were opened in 1968 under this program, increasing the number of junior high schools by 70 percent from 1967 to 1968. We evaluate the effect of this program on education and wages by analyzing cohort differences in educational attainment induced by the timing of the program and by combining these cohort differences with differences across counties in the number of schools built. These estimates suggest that children who were between the ages of 6 and 11 in 1968 received 0.3 to 0.5 additional years of education for every school constructed per 1000 graduates from primary school. We use the exogenous variation in schooling due to this program to construct instrumental variable (IV) estimates of the returns to education. We find that IV estimates based on cohort differences in education are *lower* than the corresponding OLS estimates, but IV estimates based on regional differences in inter-cohort patterns are typically *higher* than the OLS estimates.

* We thank Christina Paxson for generously providing the data from the Survey of Personal Income Distribution and Taiwan's DGBAS for providing the microdata from the Manpower Utilization Survey.

1. Introduction

Despite the enormous amount of evidence from many countries that individuals with more education receive higher wages, many economists are still reluctant to interpret this relationship as causal. The central question is whether the higher wages of individuals with more education is *caused* by their education, or whether they reflect unobserved differences between individuals that affect *both* their levels of education and their earnings. A number of recent studies have attempted to estimate the causal effect of schooling by using exogenous sources of variation in education.[1] There are two main limitations of these studies. First, most of these instrumental variable (IV) estimates are for the US or Europe and not for developing countries.[2] This is surprising, particularly since the potential bias in conventional estimates of returns to education is probably large in developing countries due to the importance of liquidity constraints and social background in determining educational attainment and wages in these countries. Second, the instruments in these studies typically explain a small fraction of the variation in education which can result in finite-sample biases in the IV estimates even with large samples.[3]

These considerations highlight the importance of estimating the causal effect of schooling on earnings in developing countries using exogenous variation in education and the potential payoff from using quasi-natural experiments that have had a significant impact on the educational choices of a broad group of individuals. With these two purposes in mind, this paper analyzes the impact of the expansion of basic education from six to nine years in 1968 in Taiwan. This increase of basic schooling represented the largest one-time expansion of education in Taiwan's modern history. More than 140 new junior high schools were opened in 1968, increasing the number of junior high schools for every thousand primary school graduate from 0.8 in 1967 to 1.4 in 1968

[1] These studies have used quarter of birth (Angrist and Krueger, 1991), college proximity (Kane and Rouse, 1993; Card, 1995a), and birth cohort (Card and Lemiux, 1998; Harmon and Walker, 1995; Ichino and Ebmer-Winter, 1999) as instruments for education.
[2] Duflo's (1999) paper on the effect of a primary school building program in Indonesia is a notable exception.
[3] See Bound, Jaeger, and Baker (1996).

(Figure 1). The fraction of primary school graduates continuing their education in junior high school increased from 60 percent in 1967 to 77 percent in 1968 (Figure 2).

To identify the effects of nine-year school program, we use the fact that exposure to the program varied by date of birth and region. First, children under the age of 12 in 1968 were exposed to the program, while those who had already graduated from primary school in 1968 did not benefit as much. Second, there was also substantial variation in the intensity of the program across regions due to the government's effort to allocate more schools in regions where initial enrollment in junior high schools was low. Therefore, while the individuals who were young enough to benefit from the program should have more education than the older groups, this difference should also be larger in regions that received more schools relative to regions that received less. Thus, by comparing the cohort difference in educational attainment between counties in which more schools were built to those where fewer schools were built, we control for any systematic variation of education both across regions and across age groups.

We then use the exogenous variation in education induced by this program to obtain IV estimates of the returns to education. We find that IV estimates based on inter-cohort differences are *lower* than the corresponding OLS estimates. This may be evidence of a positive ability bias in the OLS estimates, but we argue that it is most likely a "relative supply" effect due to the higher relative supply of educated workers among the group that was exposed to the school expansion. In support of this interpretation, we find that IV estimates that are identified by regional differences in inter-cohort patterns are *higher* than the corresponding OLS estimates. Therefore, after we account for the relative supply effect, the returns from the additional schooling induced by this policy are actually higher than that implied by the OLS estimates. This evidence supports Card's (1995) argument that children from low-income families and with high returns to education are the ones that benefit the most from an increase in school access.

The paper proceeds as follows. Section 2 describes the 1968 school expansion program. Section 3 turns to an analysis of the impact of the program on educational outcomes. Section 4 uses the exogenous variation in education induced by the program to compare IV estimates of the returns to education with the corresponding OLS estimates. Section 5 concludes.

2. The 1968 School Program

The extension of basic schooling from six to nine years in 1968 was the largest one-time expansion of education in Taiwan's modern history. Primary school education in Taiwan was nearly universal by the mid 1960s, but roughly one-half of the primary school graduates did not continue their education since enrollment in junior high schools was restricted by a competitive national examination and by the limited number of junior high schools, primarily in the rural areas of the country. The 1968 school reforms abolished the junior high school entrance examinations and made it possible, at least in principle, for every primary school graduate to continue their education at a junior high school. Children who had previously terminated their education after primary school were also allowed to enroll in junior high school under the new program as long as they were still under the age of 15 in 1968. To meet the anticipated higher enrollment in junior high schools, the government opened 140 new junior high schools in 1968, increasing the number of junior high schools from 0.8 schools for every thousand primary school graduate in 1967 to 1.4 schools per thousand primary school graduates in 1968 (Figure 1).

Student enrollment in teacher colleges was increased in the mid-1960s to meet the anticipated higher demand for junior high school teachers. When the new junior high schools were opened in 1968, the number of junior high school teachers (per primary school graduate) increased by 30 percent (Figure 3). Operational expenditures on junior high schools (also per primary school graduate) increased by 68 percent (in real terms) from 1967 to 1968 (Figure 4). The immediate effect of these additional resources devoted to junior high school education was an increase in the fraction of primary school graduates who continued their education in a junior high school, from 60 percent in 1967 to 77 percent in 1968 (Figure 2).

There was substantial variation in the intensity of the program across regions in Taiwan due to the government's effort to allocate more schools in regions where initial enrollment in junior high schools was low. The rule followed by the Taiwanese authorities was that a new junior high school was to be built in every school district that did not already have a junior high school. On the other hand, school districts that already had a junior high school did not benefit from the program.[4] Table 1 presents the number of new junior high schools per thousand primary school graduates in different counties. As can be seen, there were regional differences in the impact of the 1968 school expansion. For example, 1.9 new junior high schools were built for every thousand primary school graduate in Taitung County but only 0.14 new schools were built for every thousand primary school graduate in Taipei County and City.

The expansion of junior high school education was part of a comprehensive plan by the government to increase the supply of skilled technical workers for Taiwan's rapidly growing manufacturing industries. The other component of this strategy was a shift in emphasis from academic senior high schools to vocational senior high schools; the number of first year students in senior vocational school (per primary school graduate three years earlier) almost doubled from 1968 to 1972 (Figure 5). These new senior vocational schools were built in the same regions in which new junior high schools were opened in 1968. This also served the purpose of accommodating the demand for further education from the junior high school graduates who were affected by the 1968 junior high school expansion.

3. Impact of the 1968 School Expansion on Educational Attainment

A. Data

With this background, we now turn to an analysis of the effects of the 1968 program on educational attainment. Exposure to the school expansion was determined by age and region of residence in 1968. Children who graduated from primary school after 1968 were exposed to the new junior and senior vocational high schools. Since most children in Taiwan graduate from primary school at the age of 12, students who were 12 or younger in 1968 had the largest exposure to this program. On the other hand, individuals who were older than 15 in 1968 were too old to benefit from the school expansion. Children between the ages of 12 and 15 in 1968 benefited from the expansion of senior vocational schools, but were less exposed to the wave of new junior high schools. As previously mentioned, children under the age of 15 who had not continued their

[4] There were 429 school districts in Taiwan in 1968, each with approximately 40,000 people.

education after graduating from primary school were allowed to enroll in junior high school, but it is probably more difficult for them to do so after being out of school for several years. We therefore use the cohort between the ages of 6 and 11 in 1968 as our “treatment” group and the cohort between the ages of 15 to 20 as the “control” group.

We base our analysis on two datasets: the Manpower Utilization Survey (MPU) and the Survey of Personal Income Distribution (SPID). The MPU is a household survey conducted every year by Taiwan's Directorate-General of Budget, Accounting, and Statistics (DGBAS) since 1976. It provides basic demographic and labor force information for a representative sample of roughly 60,000 individuals over the age of 15. We base our analysis on the merged 1994, 1995, 1996, and 1997 samples of this survey and on men born between 1948 and 1967. The people potentially affected by the program (those younger than 12 in 1968) were in their late thirties and early forties at the time of the survey, and therefore on the "flat" portion of their lifecycle age-earnings profiles. This will allow us to look at the effect of education on permanent income, and make the estimates less sensitive to how we account for the independent effect of the age gap between different cohorts on their income.

Our main sample from the MPU consists of 16,057 men who were between the ages of 6 and 11 in 1968 (the "treatment" group) and 12,436 men who were between the ages of 15 and 20 in 1968 (the "control" group). We will also use an additional group (13,698 men between the ages of 1 and 5) to test our identification assumptions. The MPU provides data on monthly income from the individual's main job (including income from self-employment), but not income from secondary jobs. It also provides data on the hours worked in the week prior to the survey, from which we estimate average hourly wages. Summary statistics for this sample are presented in the second column in Table 2.[5]

Our second dataset is the merged 1994-97 sample of the Survey of Personal Income Distribution (SPID). The SPID is an annual household income and expenditure survey conducted by the DGBAS since 1976. It provides demographic and detailed income and expenditure information for approximately 17,000 households in Taiwan. For our purposes, the main differences of this dataset from the MPU are that the SPID provides

[5] Additional details are provided in the data appendix.

more comprehensive income data than the MPU and that the measure of income is annual income rather than monthly income. We define income as wages (including overtime and income from secondary employment) and self-employment income. The summary statistics for the three age groups from the merged 1994-97 sample of the SPID are presented in the first column in Table 2. Due the broader definition of income in the SPID, the average income in the SPID is higher than in the MPU. In addition, the fraction of men who report positive earnings is also higher in the SPID.

B. Measuring the "1968" Effect:

The simplest way is see the impact of the 1968 school program is to look at the educational attainment of different birth cohorts. Figure 6 presents the fraction of each birth cohort with a junior high school education from our two datasets. This figure clearly shows the steady increase in educational attainment in Taiwan over the last few decades. It also shows some evidence of an acceleration of this upward trend for the people that were between the ages of 11 to 15 in 1968, which is some evidence of the "1968" effect.

We can obtain more precise estimates of the discontinuity due to the "1968" effect by computing differences in years of completed education between the age 6-11 group and the age 15-20 group after controlling for the independent effect of age-cohort on education. Specifically, we estimate the following model:

$$(1) \quad E_i = a_1 T_i + z_i' a_2 ,$$

where i indexes individuals, E_i measures individual i's years of education, T_i is an indicator variable which is equal to one if individual i belongs to the "treated" age group (between the ages of 6 and 11 in 1968), and z_i is a vector of control variables including regional dummies (17 regions), dummies for the year of the survey (1994, 1995, 1996, or 1997), and a quadratic in age in 1968 to capture pre-existing trends in educational attainment. This estimation strategy is analogous to the "regression discontinuity" method employed in a recent study by Angrist and Lavy (1999) on the effect of class size on academic achievement, and is valid provided that the independent effect of age on education due to pre-existing trends is sufficiently "smooth."

Panel A in Table 3 presents the estimates of α_1 from equation (1) for our main sample (ages 6-11 and 11-15 in 1968). The first two columns present the estimates from the MPU. The coefficient in the first column estimates the difference in years of education between the “treated” group and the "control" group for all men, and the second column presents a similar estimate for men with positive earnings. The estimates of α_1 from these two samples are quite similar. They indicate that the "treated" cohort (ages 6 to 11 in 1968) received an additional 0.42 years of education relative to the "control" cohort (ages 15-20 in 1968). The third and fourth columns present the estimates from the SPID, first for all men and then for men with positive wage income. The point estimates from the SPID are higher than those from the MPU; they indicate that the 1968 school expansion increased relative educational attainment of men who were between the ages of 6 and 11 in 1968 by 0.66-0.74 years.

To check that the discontinuity in the upward trend in educational attainment between ages 6-11 and ages 15-20 is due to the "1968" effect and not due to pre-existing trends, we present similar estimates comparing the age 1-5 cohort with the age 6-11 cohort (in Panel B). As expected, since the 1968 school program should not result in any differences between these two groups, the estimates of α_1 are small and statistically insignificant. This provides some assurance that our regression discontinuity method is capturing some of the impact of the 1968 school expansion.

An alternative manner to measure the impact of the 1968 school program that does not rely on the assumption that the independent effect of age is “smooth” is to use the fact that exposure to the program differed by region as well as by age. We can use this fact to compare the cohort differences in years of education between regions of high program intensity and regions of low program intensity. This approach is valid as long as the *difference* in the effect of birth cohort on education in the high program intensity regions relative to the low program intensity regions is "smooth". We estimate the following model for individuals between the ages of 6 and 11 and between 15 and 20 in 1968:

$$(2) \quad E_{ij} = b_1 \left(T_i \cdot P_j\right) + z_i{}' b_2 ,$$

where i indexes individuals, j indexes regions, T_i is a dummy variable indicating whether the individual was between the ages of 6 and 11 in 1968, P_j measures the intensity of the program in region j (the number of new junior high schools in region j per thousand primary school graduates), and z_i is a vector of dummies for region, year of survey, and an unrestricted set of age in 1968 dummies. The coefficient β_1 measures the additional years of education of individuals who were between the ages of 6 and 11 in 1968 relative to those who were between the ages of 15 and 20 for a unit increase in P_j (intensity of the program).

The first two columns in Panel A in Table 4 presents estimates of β_1 from the 1994-97 MPU, first for all men and then for men with positive wage income. These estimates indicate that an additional junior high school (per thousand primary school graduates) increases education by 0.2 years for all the men in the sample. The next two columns presents similar estimates, but with a quadratic in age in 1968 instead of unrestricted age in 1968 dummies. The estimated coefficients are positive and statistically significant. They are slightly larger than the estimates with unrestricted age dummies. Part of the reason for this is that a quadratic in age does not completely control for the cohort differences in educational attainment due to the 1968 program (as seen by the estimates presented in Table 3).

Panel A of Table 5 presents similar estimates from the 1994-97 SPID. The estimated impact of the 1968 school reform on educational attainment from this dataset is larger than the estimates from the MPU. Nonetheless, the estimates are positive, generally significant, and follow the same pattern as the estimates from the MPU. The estimates from this dataset indicates that an additional junior high school per thousand students results in 0.34 to 0.56 additional years of education.

To check that our estimates are actually measuring the impact of the 1968 school expansion and are not due to differences in pre-existing regional trends that are correlated with the intensity of the 1968 school expansion, we examine whether we see the same regional differences between cohorts that should have been affected by the 1968 school program in the same way. Specifically, since all children younger than 12 in 1968 were affected by the program, we can test for the presence of pre-existing regional trends by looking for regional differences in the educational attainment of children who were between the ages of 1 and 5 relative to that of those who were between the ages of 6 and 11 in 1968. These estimates are presented in Panel B of Tables 4 and 5. The estimated coefficients are small and statistically insignificant, which provides some assurance that our comparison of the regional difference in educational attainment between the 6-11 and the 15-20 age cohorts is providing a reliable estimate of the impact of the 1968 school expansion.

Our final specification test is to examine the level of education at which the program was effective. The simplest way to do this is to estimate the following set of linear probability models for the probability of completing different levels of education (primary school or more, junior high school or more, senior high school or more, junior college or more, and university or more):

$$(3) \quad S_{ijk} = \gamma_{1k}\left(T_i \cdot P_j\right) + z_i^{\prime}\gamma_2,$$

where S_{ijk} is a dummy variable which indicates whether the individual i in region j completed the kth level of schooling. The estimates of γ_{1k} measure the impact of the program at each level of education for each unit of P_j. They are plotted in Figures 7 and 8, along with their respective 95% confidence intervals. These estimates indicate in a county where one school was built for every 1000 primary school graduates, 15 to 25 percent of the men were induced by the program to attend junior high school, and about 10-15 percent were induced to attend senior high school. There is, however, little evidence of a spillover effect of the program on post-secondary education. This is reassuring: the 1968 program did affect junior and senior high school education, but did not induce those affected to seek university education. As a final note, our two datasets show contradictory evidence on the impact of the program on the probability of attending primary school. This, however, should be interpreted with caution, due to the very small sample of people without a primary school education.[6]

One limitation with these estimates is that we use the region of residence which may not be the region in which an individual was educated. Unfortunately, this information is not available in the two datasets we use. If educated people migrated from the high program intensity regions to the low program intensity regions, then our approach which uses the current region of residence as the measure of the current region of education would lead to a downward biased estimate of the impact of the 1968 school expansion. We do not believe this is a big problem since migration in Taiwan has mostly been from rural to urban areas *within the same counties*, and the regions in our analysis are counties.

Nonetheless, we plan to assess the magnitude of this bias in the future by working with Census data, which does have information on the county in which a person originally lived.

4. IV and OLS Estimates of Returns to Education

We have shown that the school expansion had an effect on educational attainment. What about its impact on labor market outcomes and wages? We start by examining the effect on labor market participation. Figure 9 presents the fraction of men in each age cohort with positive wage income in 1994-1997 from our two datasets. This figure reveals no difference in labor force participation rates between different age cohorts, despite the higher levels of educational attainment of the younger age cohorts.

We therefore turn to the impact of the 1968 program on wages for men. We begin by presenting OLS estimates of the returns to education.[7] The second row in Table 6 presents these estimates from the 1994-97 MPU. Panel A presents our main estimates, namely the estimates of returns to education for the "treated" cohort (age 6-11 in 1968) and the "control" cohort (age 15-20 in 1968). The dependent variable in the first two columns is log monthly wages, while the dependent variable in the next two columns is log hourly wages. The first and third columns present the estimates from the specification with unrestricted age in 1968 dummies and the specification in the second column and fourth columns has a quadratic in age in 1968. The OLS estimates with log hourly wages as the dependent variable are lower than the estimates using log monthly wages as the dependent variable, which is partly due to the higher average working hours among individuals with more education. Otherwise, the OLS estimates are remarkably consistent across the four specifications.

However, many people argue that OLS estimates exceed the true return to schooling because people who would earn higher wages at any level of schooling may choose to acquire more schooling. On the other hand, measurement error in education (due to

[6] Only 0.5 percent of the people in both samples did not attend primary school.
[7] The other covariates (not presented) in all the OLS (and IV) regressions presented in this paper are regional dummies, dummies for year of survey, a dummy for whether the individual is self-employed, and either unrestricted age dummies or a quadratic in age.

survey errors or mismeasurement of the “quality” of schooling) will bias OLS estimates downward. The standard solution to this problem is to employ IV methods. As long as the return to education is constant across individuals, IV estimates will be consistent estimates of the true return to education. However, if the returns to education differ across individuals, IV estimates may not be consistent estimates of the *average* return to education in the population. Angrist and Imbens (1995) show that as long as the instrument is dichotomous and has a uniformly positive effect on schooling, the IV estimates are consistent estimates of the average marginal return to education among the individuals affected by the instrument.

Is there any reason to expect the returns to schooling to differ across individuals? Specifically, do the returns to schooling for individuals affected by the availability of additional schools differ from those of the general population? Card (1995) has argued that access to additional schools induces individuals with high returns to education (e.g., children from disadvantaged families) to obtain additional education. This explanation, along with the presence of measurement error, would explain why most IV estimates of the returns to schooling exceed the OLS estimates.

However, OLS estimates may exceed the IV estimates due to a "relative supply" effect if the instrument significantly affected the educational choices of a large group of people. Specifically, if the people affected by the instrument are imperfect substitutes for individuals that are not affected by the policy, then the higher relative supply of educated workers of the affected group may lower their returns to schooling relative to that of the non-affected group. In our case, since the 1968 school program resulted in a large increase in the relative supply of educated young workers, the supply effect could lower the younger cohort’s return to education relative to that of older workers. Card and Lemieux (1999), for example, argue that a decline in the relative supply of young college-educated workers in the US explains the increase in their relative wage over the last two decades.

With this discussion in mind, we turn to our IV estimates (presented in the fourth row of Panel A of Table 6). The instrument is the product of the product of the age-cohort dummy (6-11 in 1968) and the measure of program intensity (T_iP_j). The estimates are therefore identified by comparing the difference between "treated" cohort and the "control" cohort in regions of high program intensity relative to regions of low program intensity. For reference, the third row replicates the coefficient estimates (already presented in Table 4) from the first stage regression of years of education on the instrument. The IV estimates are typically larger than the corresponding OLS estimates. This is consistent with the results from recent studies that compare IV and OLS estimates of the returns to schooling. The difference between the IV and OLS may be due to regional differences in the returns to education. To check this, we estimate a wage regression which includes a variable which is the product of years of education with the measure of program intensity. This yields a marginally significant estimate of 0.005. Since the IV and OLS estimates differ by an average of 0.015 (see Table 6), regional differences in returns to education can account for roughly a third of the difference between the IV and OLS estimates. Therefore, this suggests that the gap between the IV and OLS estimates may be because the individuals affected by the 1968 school expansion had high returns to education and were from disadvantaged backgrounds.

A final aspect of Table 6 is the comparison of the OLS and IV estimates for the other cohorts. Panel B presents the estimates for two cohorts that should not have been affected differentially by the 1968 school expansion. These two cohorts are children who were between the ages of 1 and 5 and between 6 and 11 in 1968. The instrument is the product of a dummy for whether an individual belongs to the 1-5 age cohort and the measure of program intensity in each region. The estimates are uniformly poor, with insignificant first stage estimates for the instrumental variable and large standard errors for the IV estimates.

Table 7 presents IV estimates of the return to education from the merged 1994-97 SPID. The first column presents estimates with unrestricted age dummies, and the second column presents estimates with a quadratic in age. The OLS estimates (in the second row) are higher than those from the MPU, but this is probably due to the different measure of income in the SPID (annual vs. monthly income and total wage and self-employment income vs. income from main job). The IV estimates with unrestricted age dummies are higher than the corresponding OLS estimates, but are slightly lower when we use a quadratic in age to control for the independent effect of age in income. The lower panel presents similar estimates for the 1-5 and the 6-11 group. Once again, the first-stage yields insignificant results, and the IV estimates are consequently insignificant.

Table 8 turns to IV estimates that rely only on inter-cohort comparisons rather than on inter-cohort comparisons between high-program intensity regions and low-program intensity regions. The instrument we use is an indicator variable for whether the individual was between the ages of 6 and 11 in 1968. The estimates are therefore identified by the discontinuity in the educational attainment of different birth cohorts due to the 1968 program. The first two columns present the estimates from the MPU, and the third column presents the estimate from the SPID. The three estimates control for the independent effect of age on income by a quadratic in age.

The OLS estimates are similar to those presented in the previous two tables, but the IV estimates are significantly smaller than OLS estimates. Since the instrument is a birth-cohort dummy, the difference between the IV and OLS estimates are dominated by the difference in the returns to schooling between the 6-11 age cohort and the 15-20 age cohort. Since the 1968 school program increased the relative supply of educated workers who were younger than 12 in 1968, this relative supply shift may have lowered the returns to schooling of these workers relative to that of older workers who were unaffected by the policy. This interpretation is supported by the fact that the IV estimates that abstract from cohort differences (presented in Tables 6 and 7) are higher than the OLS estimates.

5. Conclusion

The extension of basic education from six to nine years in Taiwan in 1968 was the largest expansion of education in Taiwan's history. Simple plots of data from our two datasets indicate that this program accelerated the upward trend in education in Taiwan for children who were younger than 12 in 1968. Exploiting the discontinuity created by the 1968 program to identify its effects on education, our estimates indicate the wave of new schools raised the education of children aged 6 to 11 in 1968 by 0.5 to 0.8 years. We also exploit the large regional differences in the number of schools that were built in each region to identify the impact of the program. Using this regional variation to identify the effects of the program, our estimates suggest that this program increased the education of children who were between the ages of 6 and 11 in 1968 by 0.3 to 0.5 years for each new junior high school built per 1000 primary school graduate.

We then use the variation in schooling generated by this policy to estimate the return to schooling. Using an indicator variable for the cohort of men who were between the ages of 6 to 11 in 1968 as an exogenous determinant of schooling, we obtain IV estimates that are significantly lower than the corresponding OLS estimates. We argue that this is a "cohort effect" due to the large increase in the relative supply of educated workers born in the mid-1950s. In support this explanation, when we use the product of an indicator variable for the cohort of men between the ages of 6 and 11 and the number of new schools built in 1968 in each region as an instrument, we obtain IV estimates that are typically higher than the OLS estimates. This suggests that while the 1968 school expansion lowered the returns to schooling for the affected cohort by increasing the relative supply of educated workers, it also induced individuals with high returns to schooling to acquire additional education.

References:

Angrist, Joshua, and Imbens, Guido (1995), "Two-Stage Least Squares Estimation of Average Causal Effects in Models with Variable Treatment Intensity," Journal of the American Statistical Association, 90(43): 431-42.

Angrist, Joshua, and Krueger, Alan (1991), "Does Compulsory Schooling Affect Schooling and Earnings," Quarterly Journal of Economics, 106: 979-1014.

Angrist, Joshua, and Lavy, Victor (1999), "Using Maimonides Law to Estimate the Effect of Class Size on Scholastic Achievement," Quarterly Journal of Economics, 114: 533-75.

Bound, John, Jaeger, David, and Baker, Regina (1995), "Problems with Instrumental Variables Estimation When the Correlation Between the Instruments and the Endogenous Explanatory Variable is Weak." Journal of the American Statistical Association 90(430): 443-450.

Card, David (1995a), "Using Geographic Variation in College Proximity to Estimate the Return to Schooling," in Aspects of Labour Market Behavior: Essays in the Honor of John VanderKamp, Christofides, Louis, Grannt, Kenneth, and Swidinsky, Robert, editors. Toronto, Ontario: University of Toronto Press: 201-22.

Card, David (1995b), "Earnings, Schooling, and Ability Revisited," in Research in Labor Economics, Solomon Polachek, editor. Greenwich, CT: JAI Press.

Card, David, and Lemiux, Thomas (1999), "Can Falling Supply Explain the Rising Return to College for Younger Men? A Cohort-Based Analysis," UC Berkeley working paper.

Card, David, and Lemieux, Thomas (1998), "Education, Earnings, and the G.I. Bill." NBER Working Paper 6718.

Duflo, Esther (1999), "Schooling and Labor Market Impact of a School Construction in Indonesia: Evidence from an Unusual Policy Experiment," MIT mimeo.

Harmon, Cole and Walker, Ian (1995), "Estimates of the Economic Returns to Schooling in the United Kingdom," American Economic Review, 85: 1278-1296.

Ichino, Andrea and Ebmer-Winter, Rudolf (1999), "The Long-Run Education Costs of World War II," European University Institute mimeo.

Kane, Thomas and Rouse, Cecilia (1993), "Labor Market Returns to Two- and Four-Year Colleges: Is a Credit a Credit and Do Degrees Matter?" NBER Working Paper 4268.

Smith, Douglas, editor. The Confucian Continuum: Educational Modernization in Taiwan. New York: Praeger, 1991.

Taiwan, Provincial Department of Education. 9-Year Universal Free Education in Taiwan Province, Republic of China. Taipei: Taiwan Provincial Department of Education, May 1969.

Table 1. Number of New Junior High Schools per Thousand Primary School Student by County

Taipei County and City	0.14
Ilan County	0.20
Taoyuan County	0.34
Hsinchu County	0.15
Miaoli County	0.67
Taichung County and City	0.54
Changhwa County	0.08
Nantou County	0.59
Yunlin County	0.43
Chiayi County	0.26
Tainan County and City	0.64
Kaohsiung County and City	0.10
Pingtung County	0.83
Taitung County	1.89
Hwalien County	1.25
Penghu County	2.46

Table 2. Summary Statistics

	Survey of Personal Income Distribution	*Manpower Utilization Survey*
All men ages 1 to 5 in 1968		
n	8,908	13,698
Mean age in 1968[1]	3.1	3.1
Mean years of education[1]	12.0	11.6
% with junior high or more	96	96
% with positive wage income[2]	92	86
Mean monthly income[1,3]	45,147	36,157
Percent Observations from:		
94	29	26
95	25	25
96	24	25
97	23	24
All men ages 6 to 11 in 1968		
n	11,801	16,057
Mean age in 1968[1]	8.5	8.5
Mean years of education[1]	11.5	11.0
% with junior high or more	91	88
% with positive wage income[2]	95	90
Mean monthly income[1,3]	52,656	40,078
Percent Observations from:		
94	29	26
95	25	25
96	23	25
97	23	24
All men ages 15 to 20 in 1968		
n	9,756	12,436
Mean age in 1968[1]	17.3	17.3
Mean years of education[1]	10.1	9.6
% with junior high or more	62	57
% with positive wage income[2]	95	91
Mean monthly income[1,3]	57,273	42,510
Percent Observations from:		
94	29	26
95	25	25
96	23	25
97	23	24

[1] Based on sample of men with positive income.
[2] Positive income defined as 8,000 1996 NT$ per month (roughly 300 US dollars).
[3] In 1996 NT dollars. MPU collects data on monthly income while the SPID measure of income is annual.

Table 3. Cohort Difference in Educational Attainment
(dependent variable is years of education)

	Manpower Utilization Survey		*Survey of Personal Income Distribution*	
	All men	*Men with positive income*	*All men*	*Men with positive income*
Panel A: Experiment of Interest				
Treatment group: ages 6-11 **Control group: ages 15-20**				
n	28,493	25,614	21,557	20,453
Independent Variable: Dummy Variable for age 6-11 in 1968	0.4249 (0.1095)	0.4216 (0.1140)	0.6558 (0.1302)	0.7361 (0.1322)
Panel B: Control Experiment				
Treatment group: ages 1-5 **Control group: ages 6-11**				
n	29,755	26,138	20,709	19,418
Independent Variable: Dummy Variable for age 1-5 in 1968	–0.0343 (0.0682)	–0.0239 (0.0707)	0.0086 (0.0847)	0.0249 (0.0860)

Notes: Standard error in parentheses. Other covariates (not reported in Table) are regional dummies, year of survey, a quadratic in age in 1968, and a dummy variable for self-employed status.

Table 4.
Regional Difference in Inter-Cohort Patterns in Educational Attainment
Merged 1994-97 Manpower Utilization Survey
(dependent variable is years of education)

	Age Dummies		*Quadratic in age*	
	All men	*Men with positive income*	*All men*	*Men with positive income*
Panel A: Experiment of Interest				
Treatment group: ages 6-11 **Control group: ages 15-20**				
n	28,493	25,614	28,493	25,614
Independent Variable: Dummy Variable for age 6-11 in 1968 x Program Intensity	0.2000 (0.1100)	0.2241 (0.1166)	0.3145 (0.1037)	0.3376 (0.1101)
Panel B: Control Experiment				
Treatment group: ages 1-5 **control group: ages 6-11**				
n	29,755	26,138	29,755	26,138
Independent Variable: Dummy Variable for age 1-5 in 1968 x Program Intensity	0.1147 (0.0900)	0.0581 (0.0945)	0.0723 (0.0803)	0.0323 (0.0843)

Notes: Standard error in parentheses. Other covariates (not reported in Table) are regional dummies, year of survey, a quadratic in age in 1968 (or age in 1968 dummies), and a dummy variable for self-employed status.

Table 5.

Regional Difference in Inter-Cohort Patterns in Educational Attainment

Merged 1994-97 Survey of Personal Income Distribution
(dependent variable is years of education)

	Age Dummies		*Quadratic in age*	
	All men	*Men with positive income*	*All men*	*Men with positive income*
Panel A: Experiment of Interest				
Treatment group: ages 6-11 control group: ages 15-20				
n	21,557	20,453	21,557	20,453
Independent Variable: Dummy Variable for age 6-11 in 1968 x Program Intensity	0.3680 (0.1497)	0.3372 (0.1535)	0.5598 (0.1412)	0.5647 (0.1445)
Panel B: Control Experiment				
Treatment group: ages 1-5 control group: ages 6-11				
n	20,709	19,418	20,709	19,418
Independent Variable: Dummy Variable for age 1-5 in 1968 x Program Intensity	0.0214 (0.1329)	0.0645 (0.1356)	0.0228 (0.1181)	0.0665 (0.1206)

Notes: Standard error in parentheses. Other covariates (not reported in Table) are regional dummies, year of survey, a quadratic in age in 1968 (or age in 1968 dummies), and a dummy variable for self-employed status.

Table 6.
OLS and Difference in Difference IV Estimates of Returns to Education
Merged 1994-97 Manpower Utilization Survey.

	Dependent variable: log monthly inccome		*Dependent variable: log hourly wages*	
	(1)	*(2)*	*(3)*	*(4)*
Panel A: Experiment of Interest				
Treatment group: ages 6-11 **Control group: ages 15-20**				
n	25,613		25,388	
OLS	0.0408 (0.0008)	0.0407 (0.0008)	0.0387 (0.0008)	0.0387 (0.0008)
Reduced Form Education	0.2241 (0.1166)	0.3376 (0.1101)	0.2342 (0.1173)	0.3415 (0.1107)
IV	0.0551 (0.0223)	0.0532 (0.0210)	0.0355 (0.0232)	0.0441 (0.0220)
Age in 68 dummies	yes	no	yes	no
Quadratic in age in 68	no	yes	no	yes
Panel B: Control Experiment				
Treatment group: ages 1-5 **control group: ages 6-11**				
n	26,137		25,949	
Reduced Form Education	0.0581 (0.0945)	0.0323 (0.0843)	0.0610 (0.0949)	0.0375 (0.0847)
IV	0.3281 (0.2322)	0.2967 (0.1886)	0.2495 (0.1785)	0.2512 (0.1601)
Age in 68 dummies	yes	no	yes	no
Quadratic in age in 68	no	yes	no	yes

Notes: Standard error in parentheses. Instrument is product of indicator variable for age-cohort (6-11 or 1-5 in 1968) and program intensity in a county. Other covariates (not reported in Table) in the OLS or IV regression are regional dummies, year of survey, a quadratic in age in 1968 (or age in 1968 dummies), and a dummy variable for self-employed status. Reduced form education estimates are results from first-stage regression of years of education on the instrument.

Table 7. OLS and Difference in Difference Based IV Estimates of Returns to Education

Merged 1994-97 Survey of Personal Income Distribution.

	Dependent variable: log annual inccome	
	(1)	*(2)*
Panel A: Experiment of Interest		
Treatment group: ages 6-11 **Control group: ages 15-20**		
n	20,453	
OLS	0.0562	0.0561
	(0.0008)	(0.0008)
Reduced Form Education	0.3372	0.5647
	(0.1535)	(0.1445)
IV	0.0592	0.0523
	(0.0269)	(0.0252)
Age in 68 dummies	yes	no
Quadratic in age in 68	no	yes
Panel B: Control Experiment		
Treatment group: ages 1-5 **control group: ages 6-11**		
n	19,418	
Reduced Form Education	0.0645	0.0665
	(0.1356)	(0.1206)
IV	-0.3020	-0.2661
	(0.2861)	(0.2312)
Age in 68 dummies	yes	no
Quadratic in age in 68	no	yes

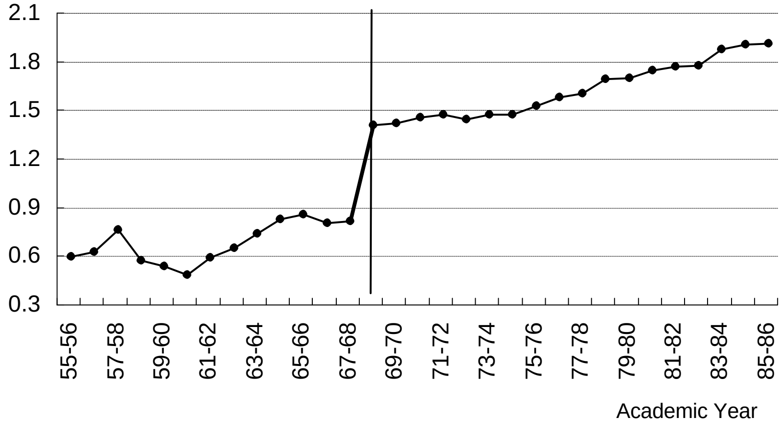
Notes: Standard error in parentheses. Instrument is product of indicator variable for age-cohort (6-11 or 1-5 in 1968) and program intensity in a county. Other covariates (not reported in Table) in the OLS or IV regression are regional dummies, year of survey, a quadratic in age in 1968 (or age in 1968 dummies), and a dummy variable for self-employed status. Reduced form education estimates are results from first-stage regression of years of education on the instrument.

Table 8. OLS and Cohort-Difference Based IV Estimates of Returns to Education

	Manpower Utilization Survey		*Survey of Personal Income Distribution*
	Dependent Variable: log monthly income	*Dependent Variable: log hourly income*	*Dependent Variable: log annual income*
Panel A: Experiment of Interest			
Treatment group: ages 6-11 **Control group: ages 15-20**			
n	25,613	25,388	20,453
OLS	0.0407 (0.0008)	0.0387 (0.0008)	0.0561 (0.0008)
Reduced Form Education	0.4216 (0.1140)	0.4034 (0.1145)	0.7361 (0.1322)
IV	0.0058 (0.0100)	0.0168 (0.0100)	0.0000 (0.0118)
Panel B:			
Treatment group: ages 1-5 **control group: ages 6-11**			
n	26,137	25,949	19,418
Reduced Form Education	-0.0239 (0.0707)	-0.0189 (0.0709)	0.0249 (0.0860)
IV	0.0152 (0.0149)	0.0229 (0.0153)	0.0176 (0.028)

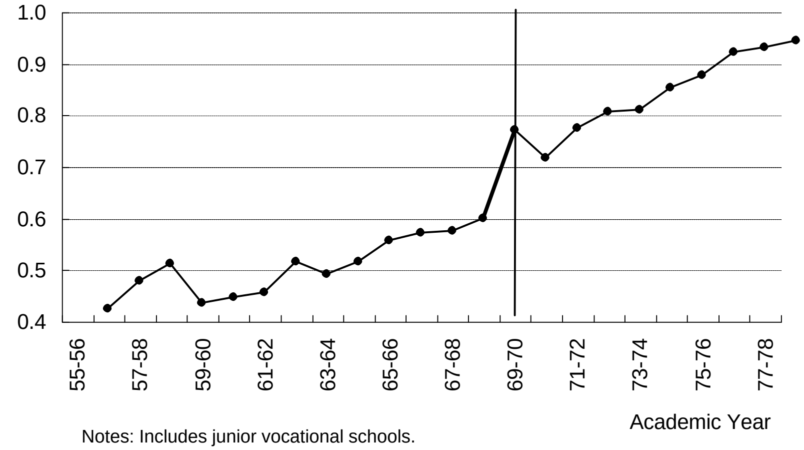
Notes: Standard error in parentheses. Instrument is an indicator variable for age-cohort (6-11 or 1-5 in 1968). Other covariates (not reported in Table) in the OLS or IV regression are regional dummies, year of survey, a quadratic in age in 1968 (or age in 1968 dummies), and a dummy variable for self-employed status. Reduced form education estimates are results from first-stage regression of years of education on the instrument.

Figure 1. Junior High Schools per Thousand Primary School Graduates



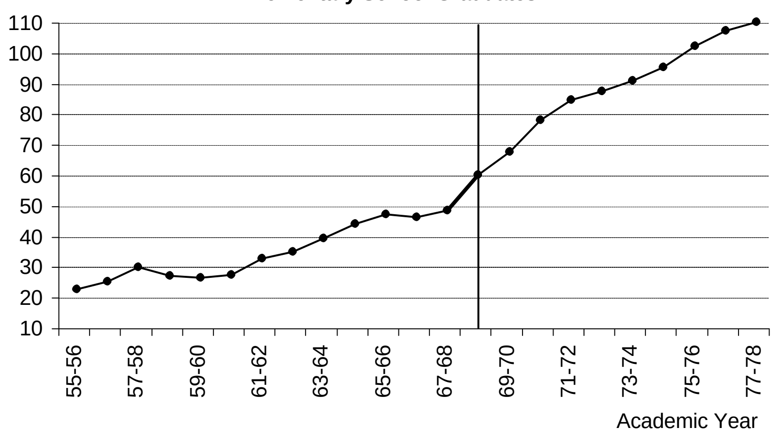
Notes: Includes junior vocational schools.
Vertical line drawn at 1968-69 school year.

Figure 2. Number of Students in First Year of Junior High to Number of Primary School Graduates



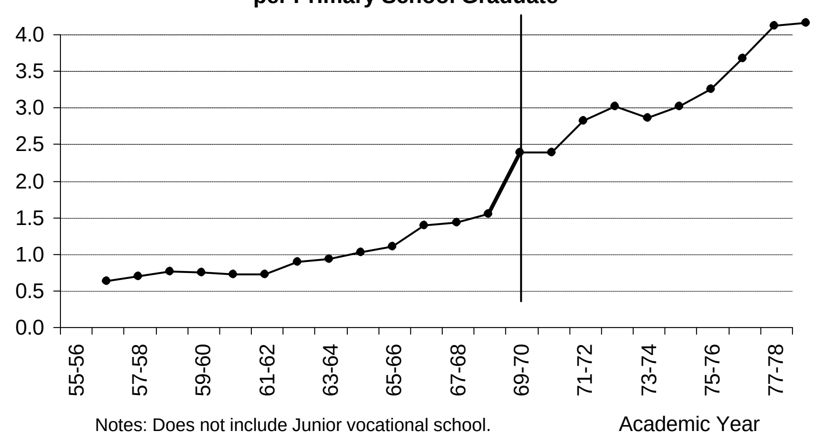
Notes: Includes junior vocational schools.
Vertical line drawn at 1968-69 school year.

Figure 3. Junior high Teachers per Thousand Elementary School Graduates



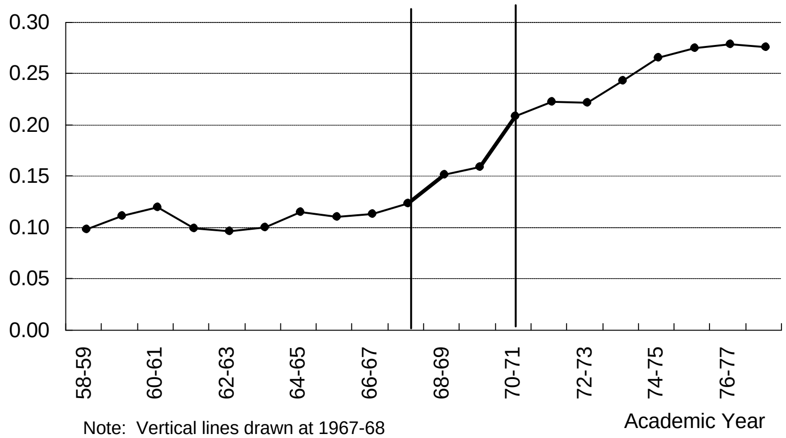
Notes: Does not include junior vocational school.
Vertical line drawn at 1968-69 school year.

Figure 4. Junior High School Operating Expenditures per Primary School Graduate



Notes: Does not include Junior vocational school.
Expenditures are in 1955 NT dollars.
Vertical line drawn at 1968-69 school year.

Figure 5. Number of First Year Senior Vocational School Students per Number of Primary School Graduates 3 Years Earlier



Note: Vertical lines drawn at 1967-68 and 1970-71 school years

Figure 6. Percent of Men with at Least Junior High School Education

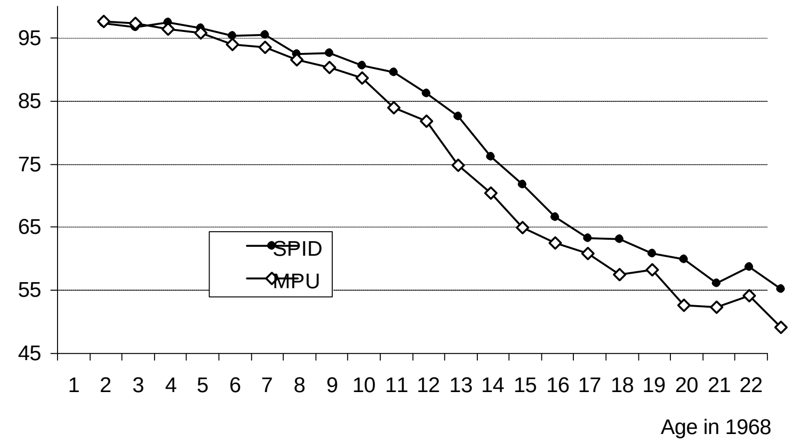


Figure 7. Difference in Difference Estimate of Impact of 1968 School Program by level of education (merged 1994-97 SPID)

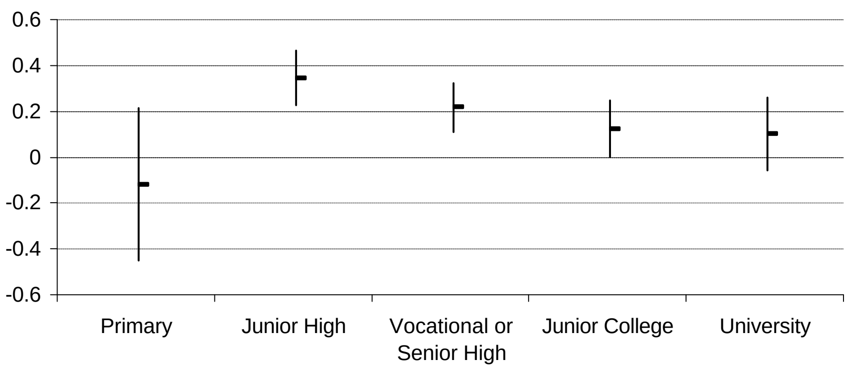


Figure 8. Difference in Difference Estimate of Impact of 1968 School Program by level of education (merged 1994-97 MPU)

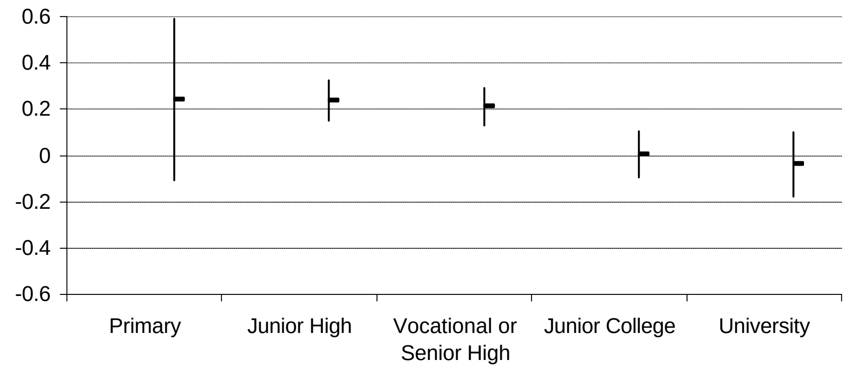


Figure 9. Percent of Men with work income