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

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This version: June 2000

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 0.6 additional years of education for every school constructed per 1000 children between the ages of 12 to 14. 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 not significantly different from the OLS estimates.

* We thank Anne Case, Angus Deaton, Jeff Kling, Jonathan Parker, Christina Paxson, and Jeff Perloff for helpful comments. Christina Paxson generously provided the data from the *Survey of Personal Income Distribution* and Taiwan's DGBAS provided the data from the *Manpower Utilization Survey* and the *1990 Population Census.* Diana E. Clark thanks the National Science Foundation's graduate fellowship program for financial support.

1. Introduction

This paper uses the largest school building program in Taiwan's modern history to examine whether investment in schooling resources increases human capital and contributes to higher wages. Despite the commonsensical notion that educational resources are important determinants of schooling outcomes, the main problem in assessing whether schooling resources *cause* human capital accumulation is that these resources are not allocated randomly across communities. For example, if families who care more about education choose to live in regions with good schools and also engage in activities to improve the quality of local schools, a positive relationship between school resources and outcomes may be due to unobserved differences in the demand for education. Similarly, despite the enormous amount of evidence from many countries that individuals with more education receive higher wages, it is not clear whether this relationship is causal or whether it reflects unobserved differences between individuals that affect *both* their levels of education and their earnings. For example, it is commonly believed that due to ability bias, OLS estimates of the returns to education are upwardly biased estimates of the true causal effect of education on income. However, it is unclear whether this positive ability bias is larger than the negative bias due to measurement error and to the possibility that many children from disadvantaged backgrounds have low levels of human capital despite their high returns to education.[1]

Despite these difficulties, a number of recent studies have made significant progress in answering these questions by using exogenous sources of variation in educational inputs and in educational attainment. To measure the impact of class size on test scores, Krueger (1999) uses data from randomized class size experiments in Tennessee, Angrist and Lavy (1999) and Urquiola (1999) use Maimonides' rule governing maximum class sizes in Israel and rural Bolivia, respectively, and Case and Deaton (1999) use exogenous differences in class sizes in South Africa under the apartheid regime. Turning to studies that seek to assess the causal impact of education on wages, recent studies have used quarter of birth (Angrist and Krueger, 1991), college proximity (Kane and Rouse, 1993; Card, 1995a), and birth cohort (Card and Lemieux, 1998; Harmon and Walker, 1995;

[1] See Card (1995b) for the latter argument.

Ichino and Ebmer-Winter, 1999) as instruments for education. Finally, a notable recent study by Duflo (1999) addresses both questions by examining the effect of a primary school building program in Indonesia on educational attainment and then uses the exogenous differences in education due to this program to estimate the economic returns to education.

This paper exploits the largest one-time school building program in Taiwan's history due to the extension of basic education from six to nine years in 1968 to provide additional evidence on the impact of schooling resources on human capital and wages. More than 140 new junior high schools were opened in 1968 under this program, increasing the number of junior high schools for every thousand primary school graduate from 0.8 in 1967 to 1.4 in 1968 (Figure 1). To identify the effect of the school building 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.

There are two attractive features of analyzing the impact of the 1968 schooling building program in Taiwan. First, with the exception of Duflo's (1999) study of the primary school building program in Indonesia, there is little work from developing countries that exploit exogenous sources of variation in schooling resources to determine the importance of schooling inputs and the returns to education. This is surprising, particularly since the potential biases in conventional estimates of the importance of schooling resources and of the returns to education are 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, in most of the recent studies of the returns to education previously cited, the instruments used 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.[2] In contrast, as we will show in this paper, the 1968 school-building program had a significant impact on the educational outcomes of a large number of individuals.

We can therefore 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. We argue that this result is due to a "relative supply" effect caused by the higher relative supply of educated workers among the group that was exposed to the school program. In support of this interpretation, we find that IV estimates that are identified by regional differences in inter-cohort patterns are not significantly different from the corresponding OLS estimates. In other words, after we account for the relative supply effect, the returns from the additional schooling induced by the school building program are no different than that indicated by the OLS estimates.

The paper proceeds as follows. Section 2 describes the 1968 school program. Section 3 describes the data and Section 4 turns to an analysis of the impact of the program on educational outcomes. Section 5 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 6 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

[2] See Bound, Jaeger, and Baker (1996).

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). To get some sense of the magnitude of this school building program, it is useful to keep in mind that junior high schools in Taiwan are rather large, with an average of 1,500 students per school (see Figure 2).

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) and operational expenditures on junior high schools (also per primary school graduate) increased by 68 percent (in real terms) from 1967 to 1968 (Figure 4).[3] The immediate effect of the additional resources dedicated to junior high school education and the elimination of the junior high school entrance examination was an immediate increase in junior high school enrollment. The fraction of primary school graduates continuing their education in a junior high school, which increased from 60 percent in 1967 to 77 percent in 1968 (Figure 5). Despite the substantial increase in the number of junior high school students, the increase in the number of teachers was large enough such that the student-teacher ratio remained unchanged at roughly 33 pupils per teacher (Figure 6).

There was also substantial variation in the intensity of the program across regions in Taiwan. Table 1 presents the number of new junior high schools per thousand children between the ages of 12 and 14 in each county. As can be seen, there were regional

[3] An additional 17,191 junior high school teachers were hired from 1968 to 1970 to staff the new schools. Despite the enrollment increase in teacher colleges, this was still insufficient to meet the demand for additional junior high school teachers. Therefore, graduates from junior colleges were also recruited to work as teachers, accounting for roughly 45 percent of the new hires in 1968-1970. However, junior college graduates had to first participate in a training program consisting of 256 hours of classes before

differences in the impact of the 1968 school building program. For example, 0.55 new junior high schools were built for every thousand children between the ages of 12 to 14 in Taitung but only 0.06 new schools were built for every thousand children aged 12-14 in Taipei.

According to the Taiwanese authorities, more schools were to be allocated in regions where initial enrollment in junior high schools was low. The specific rule announced by the 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 were not supposed to benefit from the program.[4] Despite the announced intentions of the government, however, there is no evidence that the government built more schools where the need was highest. A scatterplot of our measure of program intensity with the junior high school enrollment rate in each county indicates virtually no relationship between the number of schools built and the junior high school enrollment rate in the county (Figure 7). For example, enrollment rates in Changhwa, Ilan, and Chiayi were significantly below the average in Taiwan, yet the intensity of the school building program in these three counties were much lower than elsewhere in Taiwan. On the other hand, the junior high school enrollment rate in Tainan was higher than elsewhere in Taiwan, yet it benefited more from the school building program than other counties. An alternative measure of a county's schooling needs is the importance of the agricultural sector in the county. However, there also appears to be no relationship between the agricultural share of employment in the county and the intensity of the program (Figure 8). In sum, there was significant variation in the intensity of the program across regions in Taiwan, but this variation does not appear to be related to the region's needs in terms of junior high school education.

Because of the timing and the regional variation in the intensity of the school building program, we can determine a person's exposure to the program by their age and region of residence in 1968. Children who graduated from primary school after 1968 were exposed to the new junior 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

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they were allowed to teach. See Bureau of Education, Taiwan Provincial Government (1973) for additional details.

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

largest exposure to this program. Children between the ages of 12 and 15 in 1968 who had not continued their 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. In turn, individuals who were older than 15 in 1968 were too old to benefit from the school expansion. 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.

3. Datasets

We base our analysis on three datasets: the Manpower Utilization Survey (MPU), the Survey of Personal Income Distribution (SPID), and the 1990 Population Census. 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 merge the 1994, 1995, 1996, and 1997 samples of this survey by year of birth and base our analysis 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 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 third and fourth columns in Table 2.[5]

Our second dataset is the merged (by year of birth) 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 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 two columns in Table 2. Due to 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.

Our last dataset is the entire 1990 Population Census of Taiwan. This dataset also provides demographic and education information. In addition to its size, the main advantage of this dataset is that it also provides information on the individual's county of origin, which is defined as the county where the person's father was born. In contrast, our other two datasets only provide information on the county where the person was residing at the time of the survey. The main limitation of the Population Census is it does not provide any income information. Therefore, we can use the Census to check our identification assumptions, but not to estimate the returns to education. The summary statistics from the 1990 Population Census are presented in columns 5 and 6 in Table 2. According to the Census, 86 to 90 percent of the men listed a county in Taiwan as their county of origin and that 60 to 70 percent of these men were living in the same county in which their father was born. Therefore, despite the rapid industrialization and massive rural-urban migration in Taiwan over the last few decades, this migration has primarily been from rural to urban areas within the same county.

5 Additional details are provided in the data appendix.

3. Measuring the "1968 Effect"

A. Cohort Difference Approach:

The simplest way to measure the impact of the 1968 school program is to look at the educational attainment of different birth cohorts. More precisely, we can estimate 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:

$$(1) \quad E_i = \alpha_1 T_i + z_i{}' \alpha_2 ,$$

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) when using the MPU and SPID, 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 described by Thistelthwaite and Campbell (1960) and employed in recent studies by Angrist and Lavy (1999) and Urquiola (1999) on the effect of class size on academic achievement. This identification strategy is valid provided that the independent effect of age on education due to pre-existing trends is sufficiently "smooth."[6]

Panel A in Table 3 presents the estimates of α_1 from equation (1) for our central experiment (ages 6-11 and 15-20 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 after controlling for

[6] This specification implicitly assumes that the discontinuity due to the school program is additive after controlling for pre-existing trends. One way to test this assumption of additivity is by interacting the treatment cohort dummy with a quadratic in age. The F-statistic for the test that the two coefficients are zero never exceed two, which suggest that our assumption of an additive discontinuity due to the school program is not an unreasonable one.

pre-existing trends by a quadratic in age-cohort, 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 to 0.74 years. The last four columns present the estimates from the 1990 Population Census, which suggest a similar effect of the program; they indicate that males in the "treated" age-cohort received 0.43 to 0.58 additional years of education due to the school building program.

These estimates of the impact of the 1968 school building program depend critically on the assumption that a smooth quadratic in age in 1968 captures the effect of pre-existing trends. One way 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 the difficulty of capturing the effect of pre-existing trends by a smooth function of age-cohort is to present similar estimates comparing the educational attainment of the age 1-5 cohort with that of the age 6-11 cohort (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 very small and except for the estimates from the Census, are statistically insignificant. This provides some assurance that the regression discontinuity method is capturing some of the impact of the 1968 school expansion.

B. Difference in Difference Approach:

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". This is the basic approach taken by Card and Lemieux's (1998) study on the effect of the Canadian GI Bill and

Duflo's (1999) work on the impact of the primary school building program in Indonesia in the 1970s.

We estimate the following model for individuals between the ages of 6 and 11 and between 15 and 20 in 1968:

$$(2) \quad E_{ij} = \beta_1 (T_i \cdot P_j) + z_i{}' \beta_2 ,$$

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 children), 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 main difficulty in estimating this model is that the three datasets we use in this paper do not have information on the county in which a person received his or her education. We can not use the county of residence as the county in which the person was educated if there has been migration between counties in Taiwan over the last few decades as people moved from rural to urban areas. However, it is well known that industrialization in Taiwan was spread out in an even manner across the island and that migration has consequently been largely from rural to urban areas *within the same county*. According to 1990 Population Census, which has information on the county where the individual's father was born, 60 to 70 percent of the people whose father did not come from the Chinese mainland live in the same county where their father was born (see Table 2). Since a person could have been educated in the same county where she currently resides even if her father migrated between counties, 30 to 40 percent is an upper-bound estimate of the fraction of people who migrated between different counties in Taiwan. Nonetheless, measurement error in the county of education due to random inter-county migration will induce a downward bias in the estimated impact of the program, in the same manner as pure measurement error in the county of education. If migration was endogenous, the bias could be either negative or positive, depending on whether relatively more educated individuals from the treated cohort migrated from counties of high program intensity to counties of low program intensity, or whether the migration was dominated by less-educated people from the treated cohort.

An additional potential problem with our identification strategy is that there could be other factors correlated with the allocation of schools that changed the expected cost and benefits of education. For example, since Taiwan was undergoing rapid industrialization during this period, regional differences in the rate of industrialization may result in regional differences in educational gains. Our identification assumptions will therefore be violated if the intensity of the program is correlated with the rate of industrialization in the region. In the estimates presented below, we deal with this by introducing controls for the interactions between the cohort dummy and the change in the non-agricultural share of total employment in the county between 1961 and 1971.

Similarly, if reversion to the mean causes increases in education across cohorts to be negatively correlated with the initial enrollment rate, then our identification assumption will be violated if the intensity of the program was related to the enrollment rate. Although we have shown earlier that this was not the case (again, see Figure 7), the identification assumption might be more likely to be satisfied after we control for mean reversion by including interactions between a cohort dummy and the initial enrollment rate in junior high school (in 1966).

With this discussion in mind, we turn to the estimates that use the county of residence as the proxy for the county where the person was educated. The first four columns in Panel A in Table 4 present estimates of β_1 from the 1994-97 MPU with unrestricted age-dummies, first for all men and then for men with positive wage income. The estimates without any controls for the initial enrollment rate and the change in the non-agricultural share of employment indicate that an additional junior high school (per thousand children aged 12-14) increases education of all the men in the sample by 0.56 to 0.64 years. The estimated impact of the program is slightly lower (0.45 to 0.48 additional years of education for every school per thousand children aged 12-14) once these controls are introduced. Columns 5 through 8 present similar estimates, but with a quadratic in age in 1968 instead of unrestricted age in 1968 dummies. The estimated coefficients are positive and are typically statistically significant. They are slightly larger (0.65 to 1.02

additional years of education for every school per child) than the estimates with unrestricted age dummies.

Panel A of Table 5 turns to the 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 indicate that an additional junior high school per thousand children aged 12-14 results in 0.89 to 1.66 additional years of education and as before, are slightly lower (0.84 to 0.95 additional years of education) once controls for initial enrollment and industrialization are introduced. Finally, the first two columns in Panel A of Table 6 present the estimates from the 1990 Population Census. They indicate that every junior high school per thousand children aged 12-14 increases educational attainment by 1.08 to 1.19 years.

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 or inter-county migration flows that are correlated with the intensity of the 1968 school expansion, we examine whether we see the same regional differences between cohorts that should not have been affected differently by the 1968 school building program. Specifically, since all children younger than 12 in 1968 were affected by the program, we can test our identification assumptions by looking for regional differences in the educational attainment of children who were between the ages of 1 and 5 relative to that of children who were between the ages of 6 and 11 in 1968. These estimates are presented in Panel B of Tables 4, 5, and 6. The estimated coefficients are typically small and statistically insignificant (except for some of the estimates from the Census), 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.

The main limitation of these estimates is that we use the region of residence as a proxy for the region where the person attended school. Despite the fact that the extent of inter-county migration in Taiwan is low, inter-county migration could still lead to some bias in our estimates. To address this problem, we present alternative estimates from the

Population Census to gauge the bias due to this problem by restricting the sample to people who live in the same county where their father was born. We can be reasonably confident that these people were educated in the same county in which they were living at the time of the Census. In addition, since the families of these men have been living in the same county for at least two generations, it is reasonable to believe that the school building program would have a smaller effect on these men. Therefore, the estimated impact of the program on this restricted sample of men is a downward biased estimate of the effect of the school building program on the entire population in Taiwan. These estimates are shown in the last two columns (3 and 4) in Table 6, and indicate that every additional school per thousand children aged 12-14 increases educational attainment by 0.56 to 0.65 years. The estimates are slightly lower than those obtained when using the county of residence as the proxy for the county of origin, but are still large and statistically significant.

Finally, it is worth comparing these estimates of the impact of the junior high school building program in Taiwan with Duflo's (1999) estimates of the effect of the primary school building program in Indonesia. According to her estimates, children in the treated age-cohort received an additional 0.12 to 0.18 additional years of education for every primary school built for every thousand children between the ages of 6 and 12. Our central estimate is that children in the treated-age cohort received 0.6 additional years of schooling for every junior high school built per thousand children between the ages of 12 and 14. To make Duflo's coefficient estimates comparable to ours, we have to account for the fact that primary schools in Indonesia are 1/12th the size of junior high schools in Taiwan, that a junior high school provides three years of education while primary schools provide six, and that the denominator of our measure of program intensity (children between ages of 12 to 14) is 3/10th that of Duflo's (children between the ages of 5 to 14).[7] After multiplying Duflo's numbers by 1.875 to make them comparable to ours, we conclude that our estimate of the impact of the junior high school building program is

[7] The average primary school in Indonesia has 120 students while the average junior high school in Taiwan has 1,500 students.

about twice as large as that of the primary school building program in Indonesia (0.23 to 0.34 in our units).[8]

C. Effect of 1968 School Program on Different Levels of Education

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 (junior high school or more, senior high school or more, junior college or more, and university or more):

$$\text{(3)} \quad S_{ik} = \gamma_{1k} \cdot T_i + z_i{}' \gamma_2$$

where S_{ik} is a dummy variable which indicates whether individual i completed the kth level of schooling. The estimates of γ_{1k} measure the impact of the program at each level of education after controlling for a smooth upward trend in educational attainment. They are plotted in Figures 9-11, along with their respective 95% confidence intervals. These estimates indicate that relative to the control cohort, the treated cohort was 25 to 40 percent more likely to have attended junior high school, and 8 to 13 percent more likely to have attended high school. There is, however, no effect beyond the high school level.

As before, these estimates rely on the assumption of a smooth pre-existing trend. Another way to estimate the impact of the program at different levels of education is to estimate the following set of probability models for the same set of schooling levels:

$$\text{(4)} \quad S_{ijk} = \lambda_{1k}\left(T_i \cdot P_j\right) + z_i{}' \gamma_2,$$

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 λ_{1k} measure the impact of the program at each level of education for each unit of P_j. They are plotted in Figures 12-15, along with their respective 95% confidence intervals. These estimates indicate that in a county where one school was built for every 1000 children, 37 to 71 percent of the men were induced by the program to attend junior high school, and about 12 to 37 percent were induced to attend senior high school. There is, however, little evidence of a positive spillover effect of the program on post-secondary education. This is reassuring: the 1968

[8] 1.875=(1500/120) x (3/6) x (3/10)

program did affect junior and senior high school education, but did not induce those affected to seek tertiary-level education.

5. IV and OLS Estimates of Returns to Education

We have shown that the school expansion had an effect on educational attainment. The natural question is whether the higher levels of schooling induced by the program affected labor market outcomes. We start by examining the effect on labor market participation. Figure 16 presents the fraction of men in each age cohort in the labor force in 1994-1997 from our three 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 from the 1994-97 MPU.[9] The second row in Table 7 presents 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 four columns is log monthly wages, while the dependent variable in the next four columns is log hourly wages. Columns 1, 2, 5, and 6 present the estimates from the specification with unrestricted age in 1968 dummies while the estimates in columns 3, 4, 7, and 8 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 survey errors or mismeasurement of the "quality" of schooling) will bias OLS estimates downward. The standard solution to these problems is to employ IV methods. As long

[9] The other covariates in 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.

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.[10]

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 (1995b) has argued that access to additional schools induces children with high discount rates (e.g., children from disadvantaged backgrounds) but with high returns to education to obtain additional education. This explanation, along with the presence of measurement error, would explain why most existing 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 Table 7). The instrument is the product of the product of the age-cohort dummy (age 6-11 in 1968) and the measure of program intensity (new schools per thousand children aged 12-14). The estimates are therefore identified by comparing the difference

[10] Also, see Kling (1999) for a discussion of this.

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 smaller than the corresponding OLS estimates, although the difference is not statistically significant.

Table 8 presents IV estimates of the return to education from the merged 1994-97 SPID. The first two columns presents estimates with unrestricted age dummies, and the last two columns 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). As before, the IV estimates are smaller than the OLS estimates, but the difference is once again not statistically significant.

Table 9 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 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 the corresponding OLS estimates in two of the three estimates. Since the instrument is a birth-cohort dummy, the difference between the IV and OLS estimates is 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 7 and 8) are not significantly lower than the OLS estimates.

Finally, it is possible that changes in the quality of schools may drive some of these estimates. Figure 6, which presents the average pupil-teacher ratio in Taiwan, provides no evidence of a dilution in school quality due to the 1968 program. As an alternative manner to assess whether the 1968 program changed school quality, we can estimate whether the wages of people with a large exposure to the program, but whom nonetheless were not induced to acquire additional education by the program, are lower than similar individuals in regions with less exposure to the program. As previously shown in the probit estimates (in Figures 12-15), the people in the treated age-cohort that attended junior college or university did not acquire additional education due to the 1968 program. Thus, if their wages are lower, then this would be one piece of evidence that school quality worsened. These reduced form estimates of wages on the instrument (the product of a cohort dummy with program intensity) for men who attended junior colleges or universities provide no evidence that the program significantly affected school quality (Columns 1 and 3 in Table 10).

Similarly, the probit estimates presented in Figures 9 through 11 show that after controlling for a quadratic in age, there is no difference in the fraction of men in the treated age-cohort (age 6-11 in 1968) who obtained tertiary-level education relative to that in the control age-cohort (age 15-20 in 1968). To assess whether school quality changed, we can focus on the men who attended junior college or university and examine whether the wages of men who were exposed to the program are different from those who were not exposed to the program. As before, the reduced form estimates of wages on the instrument (this time, a cohort dummy) also provide no evidence that the 1968 school program affected the quality of junior high schools (Columns 2 and 4 in Table 10).

6. 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. 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 0.4 to 0.6 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 0.6 years for each new junior high school built per 1000 children between the ages of 12 to 14.

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 of 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 not significantly lower than the OLS estimates.

This paper thus adds to the growing body of evidence that school resources do matter for educational outcomes. Similarly, our estimates add to the evidence that while conventional OLS estimates may be upwardly biased due to ability bias, this bias is counteracted by measurement error and discount rate bias. Therefore, standard OLS estimates of the returns to education are relatively accurate measures of the true returns to schooling. Finally, we also show that large increases in the relative supply of educated workers have significant general-equilibrium effects on the returns to education.

Data Appendix

Geographic Regions: We divided Taiwan into 17 geographical regions: Taipei, Ilan, Taoyuan, Hsinchu, Miaoli, Taichung, Changhua, Yunlin, Chiayi, Tainan, Kaohsiung, Pintung, Taitong, Hualien, Penghu, and Keelung. Each region includes the county and the respective city (in Taiwan, most large cities are separate local administrative entities). For example, Taipei includes Taipei County and Taipei City and Hsinchu includes Hsinchu County and Hsinchu City.

Income: In the PSID, we define income as annual income (including bonuses and overtime pay) received from the person's main job or business (if the person is self-employed). In the MPU, income is the person's monthly income from her main job. To obtain a figure of average hourly income from the MPU, we estimated total hours worked per month by multiplying hours worked last week by 4, and divided the monthly wage by this figure. We converted the nominal income from each year into real 1996 NT dollars with the following deflators: 1994: 94.07; 1995: 101.01; 1995: 101.01; 1996: 100; and 1997: 99.54.

Years of Education: In our three datasets, we define years of education in the following manner: no school or self-taught=0; primary=6; junior high=9; senior high=12; vocational school =12; junior college=14; university=16; and graduate school=21.

Labor Force Participation: In the MPU and PSID, we define people who participate in the labor force as those with positive income (8000 1996 NT$ per month for the MPU and 96,000 1996 NT$ per year for the PSID). The Census asks whether the person has a job, which is what we use to define labor force participation for the Census. The three datasets also ask whether the individual is self-employed.

Program Intensity, Enrollment, and Regional Information: The data on the number of new junior high schools for every county is from the publication *9-Year Universal Free Education in Taiwan Province, Republic of China*. The publication *Taiwan Demographic Factbook, Republic of China* provides estimates of the number of children in each county between the ages of 10-14. We multiply this number by 3/5 to estimate the number of children between the ages of 12-14 for each county. The number of junior high school students in 1966 and the agricultural share of total employment by county are also from the *Taiwan Demographic Factbook, Republic of China*. The population in each county in 1968 is from the publication *Taiwan Statistical Abstract*. The aggregate data on enrollment, number of teachers, schools, and expenditures are from various issues of the publication *Educational Statistics of the Republic of China*.

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Table 1.
Intensity of the 1968 Program and Population by County

	Intensity of the 1968 program[1]	*Population in 1966*
Taipei County and City	0.06	2,454,362
Ilan County	0.06	384,420
Taoyuan County	0.14	609,979
Hsinchu County	0.04	534,877
Miaoli County	0.19	487,317
Taichung County and City	0.19	1,084,795
Changhwa County	0.02	991,538
Nantou County	0.17	475,315
Yunlin County	0.12	763,423
Chiayi County	0.07	805,811
Tainan County and City	0.19	1,300,826
Kaohsiung County and City	0.03	1,365,435
Pingtung County	0.24	760,101
Taitung County	0.55	267,336
Hwalien County	0.39	307,220
Penghu County	0.74	112,852
Keelung City	0.16	287,156
Average in Taiwan[2]	**0.13**	

Notes:
(1) Intensity is defined as number of new junior high schools per thousand children ages 12-14.
(2) The total number of Junior High Schools per thousand Primary school graduates in 1967 in Taiwan was 0.26.

Table 2
Summary Statistics of the Datasets

	Survey of Personal Income Distribution		*Manpower Utilization Survey*		*1990 Population Census*	
	All men	*Men with positive income*[1]	*All men*	*Men with positive income*[1]	*All men*	*Men with work*
All men ages 1 to 5 in 1968						
n	8,908	8,180	13,698	11,780	969,981	746,241
Mean age in 1968	3.1	3.14	3.04	3.05	3.08	3.19
Mean years of education	11.94	11.96	11.64	11.64	11.61	11.28
% with junior high or more	96.1	96.5	95.7	96.2	95.0	95.1
% with positive wage income[1, 2]	91.8		86		76.9	
Mean monthly income[3]		45,147		36,212		
% with father born in Taiwan					85.9	88.1
% living in same county where father was born					60.7	63.9
All men ages 6 to 11 in 1968						
n	11,801	11,238	16,057	14,409	1,192,984	1,063,297
Mean age in 1968	8.53	8.55	8.45	8.47	8.44	8.47
Mean years of education	11.48	11.55	10.1	11.01	11.17	11.13
% with junior high or more	90.6	91	87.9	88.5	88.4	88.6
% with positive wage income[1, 2]	95.2		89.7		89.1	
Mean monthly income[3]		52,656		40,133		
% with father born in Taiwan					87.3	88.7
% living in same county where father was born					56.9	58.2
All men ages 15 to 20 in 1968						
n	9,756	9,215	12,436	11,258	918,613	855,734
Mean age in 1968	17.3	17.3	17.27	17.27	17.3	17.3
Mean years of education	9.97	10.06	9.49	9.57	9.78	9.78
% with junior high or more	61.5	62.3	57.3	58.2	59.8	59.8
% with positive wage income[1, 2]	94.5		90.5		93.2	

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

	Manpower Utilization Survey		*Survey of Personal Income Distribution*		*1990 Population Census*			
					All men in Taiwan		*With county of residence same as county or origin*	
	All men	*Men with positive income*	*All men*	*Men with positive income*	*All men*	*Men with a job*	*All men*	*Men with a job*
Panel A: Experiment of Interest								
Treatment group: ages 6-11 in 1968 **Control group: ages 15-20 in 1968**								
N	28,438	25,613	21,556	20,452	2,111,596	1,919,030	1,180,351	1,092,018
Independent Variable: Dummy Variable for age 6-11 in 1968	0.4249 (0.1095)	0.4216 (0.114)	0.6558 (0.1302)	0.7361 (0.1322)	0.4291 (0.0132)	0.5022 (0.0135)	0.5243 (0.0165)	0.5816 (0.0168)
Panel B: Control Experiment								
Treatment group: ages 1-5 in 1968 **Control group: ages 6-11 in 1968**								
N	29,683	26,137	20,708	19,417	2,162,964	1,809,537	1,264,950	1,092,985
Independent Variable: Dummy Variable for age 1-5 in 1968	-0.0343 (0.068)	-0.0239 (0.0707)	0.0086 (0.0847)	0.0249 (0.0860)	0.0346 (0.0081)	0.0434 (0.0083)	0.042914 (0.00997)	0.03808 (0.01005)

Notes: Standard error in parentheses. Other covariates (not reported in Table) are regional dummies, year of survey (for the MPU and SPID), a quadratic in age in 1968, and a dummy variable for self-employed status.
Men with positive income in MPU refers to monthly income over NT$8000.

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	*All men*	*Men with positive income*	*Men with positive income*	*All men*	*All men*	*Men with positive income*	*Men with positive income*
Panel A: Experiment of Interest								
Treatment group: ages 6-11 in 1968 **Control group: ages 15-20 in 1968**								
N	28,438	28,438	25,613	25,613	28,438	28,438	25,613	25,613
Independent Variable: *Dummy Variable for age 6-11 in 1968* x *Program Intensity*	0.5632 (0.3450)	0.4517 (0.3675)	0.6389 (0.3659)	0.4784 (0.3903)	0.9454 (0.3222)	0.6454 (0.3387)	1.0179 (0.3421)	0.7310 (0.3592)
Controls for: *JHS enrollment rate* x *cohort* *? in non-agricultural share* x *cohort*	no	yes	no	yes	no	yes	no	yes
Panel B: Control Experiment								
Treatment group: ages 1-5 in 1968 **Control group: ages 6-11 in 1968**								
N	29,683	29,683	26,137	26,137	29,683	29,683	26,137	26,137
Independent Variable: *Dummy Variable for age 1-5 in 1968* x *Program Intensity*	0.3094 (0.2820)	0.1968 (0.3007)	0.1328 (0.2963)	-0.0026 (0.3168)	0.1780 (0.2480)	0.3272 (0.2746)	0.0571 0.2604	0.1306 (0.2884)
Controls for: *JHS enrollment rate* x *cohort* *? in non-agricultural share* x *cohort*	no	yes	no	yes	no	yes	no	yes

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.
Men with positive income refers to monthly income over NT$8000.

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	*All men*	*Men with positive income*	*Men with positive income*	*All men*	*All men*	*Men with positive income*	*Men with positive income*
Panel A: Experiment of Interest								
Treatment group: ages 6-11 in 1968 **Control group: ages 15-20 in 1968**								
N	21,556	21,556	20,452	20,452	21,556	21,556	20,452	20,452
Independent Variable: *Dummy Variable for age 6-11 in 1968* x *Program Intensity*	0.9954 (0.4690)	0.9489 (0.5105)	0.8882 (0.4809)	0.8420 (0.5234)	1.6527 (0.4368)	0.9228 (0.4614)	1.6694 (0.4472)	0.8429 (0.4729)
Controls for: *JHS enrollment rate* x *cohort* *? in non-agricultural share* x *cohort*	no	yes	no	yes	no	yes	no	yes
Panel B: Control Experiment								
Treatment group: ages 1-5 in 1968 **Control group: ages 6-11 in 1968**								
N	20,708	20,708	19,417	19,417	20,708	20,708	19,417	19,417
Independent Variable: *Dummy Variable for age 1-5 in 1968* x *Program Intensity*	0.1004 (0.4152)	-0.0941 (0.4495)	0.2545 (0.4238)	0.0210 (0.4594)	0.0944 (0.3609)	0.1084 (0.4050)	0.2429 (0.3684)	0.2082 (0.4135)
Controls for: *JHS enrollment rate* x *cohort* *? in non-agricultural share* x *cohort*	no	yes	no	yes	no	yes	no	yes

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
Regional Difference in Inter-Cohort Patterns in Educational Attainment
1990 Population Census
(dependent variable is years of education)

	All men in Taiwan. Identifucation based on region of residence				*Men who currently live in the same county as their county of origin*	
	All men	*All men*	*Men with positive income*	*Men with positive income*	*All men*	*Men with positive income*
Panel A: Experiment of Interest						
Treatment group: ages 6-11 in 1968						
Control group: ages 15-20 in 1968						
N	2,108,274		1,915,825		1,180,351	1,092,018
Independent Variable:	1.0845	0.5532	1.1850	0.5740	0.5798	0.6482
Dummy Variable for age 6-11 in 1968 x *Program Intensity*	(0.0456)	(0.0481)	(0.0471)	(0.0496)	(0.0515)	(0.0525)
Controls for:						
JHS enrollment rate x *cohort*	no	yes	no	yes	no	no
? in non-agricultural share x *cohort*						
Panel B: Control Experiment						
Treatment group: ages 1-5 in 1968						
Control group: ages 6-11 in 1968						
N	2,159,902		1,806,352		1,264,950	1,092,985
Independent Variable:	0.2095	0.1045	0.4023	0.3330	0.0198	0.0654
Dummy Variable for age 1-5 in 1968 x *Program Intensity*	(0.0354)	(0.0394)	(0.0365)	(0.0407)	(0.0400)	(0.0407)
Controls for:						
JHS enrollment rate x *cohort*	no	yes	no	yes	no	no
? in non-agricultural share x *cohort*						

Notes: Standard error in parentheses. Sample only includes people who reported a county in Taiwan as county of origin, where county of origin is defined as father's place of birth. Other covariates (not reported in Table) are regional dummies, a quadratic in age in 1968, and a dummy variable for self-employed status.

Table 7.
OLS and Difference in Difference IV Estimates of Returns to Education
Merged 1994-97 Manpower Utilization Survey.

	Dependent variable: log monthly income				*Dependent variable: log hourly wages*			
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Treatment group: ages 6-11 in 1968 Control group: ages 15-20 in 1968								
N	25,613				25,388			
OLS	0.0408 (0.0008)	0.0408 (0.0008)	0.0407 (0.0008)	0.0408 (0.0008)	0.0387 (0.0008)	0.0387 (0.0008)	0.0387 (0.0008)	0.0387 (0.0008)
Reduced Form Education	0.6389 (0.3659)	0.4784 (0.3903)	1.0179 (0.3421)	0.7310 (0.3592)	0.6702 (0.3679)	0.5439 (0.3922)	1.0284 (0.3439)	0.7365 (0.3611)
IV	0.0407 (0.0189)	0.0282 (0.0202)	0.0393 (0.0177)	0.0425 (0.0186)	0.0226 (0.0199)	0.0122 (0.0215)	0.0310 (0.0185)	0.0271 (0.0195)
Age in 68 dummies	yes	yes	no	no	yes	yes	no	no
Quadratic in age in 68	no	no	yes	yes	no	no	yes	yes
Controls for: JHS enrollment rate x *cohort ? in non-agricultural share* x *cohort*	no	yes	no	yes	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 8. OLS and Difference in Difference Based IV Estimates of Returns to Education

Merged 1994-97 Survey of Personal Income Distribution.

	Dependent variable: log annual income			
	(1)	*(2)*	*(3)*	*(4)*
Treatment group: ages 6-11 in 1968				
Control group: ages 15-20 in 1968				
N	20,452			
OLS	0.0562	0.0562	0.0561	0.0562
	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Reduced Form Education	0.8882	0.8420	1.6694	0.8429
	(0.4809)	(0.5234)	(0.4472)	(0.4729)
IV	0.0453	0.0260	0.0390	0.0378
	(0.0217)	(0.0241)	(0.0202)	(0.0214)
Age in 68 dummies	yes	yes	no	no
Quadratic in age in 68	no	no	yes	yes
Controls for: *JHS enrollment rate* x *cohort* *? in non-agricultural share* x *cohort*	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 9. OLS and Cohort-Difference Based IV Estimates of Returns to Education

	MPU		*SPID*
	Dependent Variable: log monthly income	*Dependent Variable: log hourly income*	*Dependent Variable: log annual income*
Treatment group: ages 6-11 in 1968 **Control group: ages 15-20 in 1968**			
N	25,613	25,388	20,452
OLS	0.0407 (0.0008)	0.0387 (0.0008)	0.0561 (0.0008)
Reduced Form Education	0.4249 (0.1095)	0.4034 (0.1145)	0.6558 (0.1302)
IV	0.0058 (0.0100)	0.0441 (0.0220)	0.00001 (0.0118)

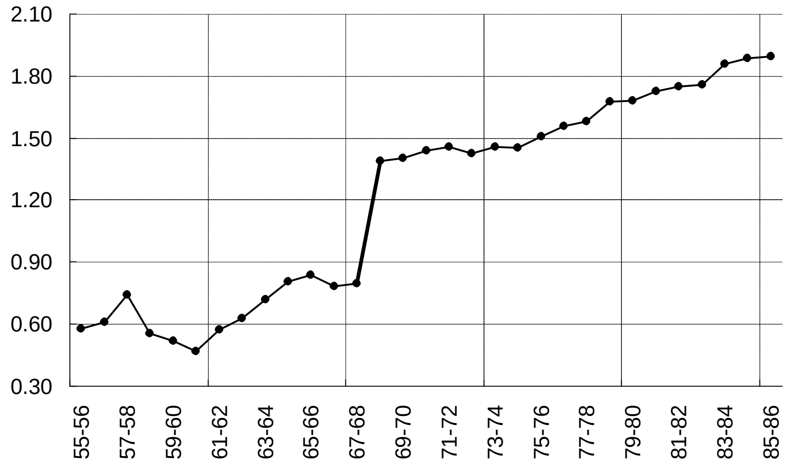
Notes: Standard error in parentheses. Instrument is an indicator variable for age-cohort (6-11 or 1-5 in 1968). Other covariates (not reported in Table) in the OLS or IV regression are regional dummies, year of survey, a quadratic in age in 1968, and a dummy variable for self-employed status. Reduced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

Table 10.
Regional Differences in Inter-Cohort
Wage Differences of Men with College Education
Dependent variable is log wages

	MPU		*SPID*	
	(1)	*(2)*	*(3)*	*(4)*
Treatment group: ages 6-11 in 1968 **Control group: ages 15-20 in 1968**				
N	5,282		5,404	
Dependent Variable: *Dummy Variable for age 6-11* *in 1968* x *Program Intensity*	0.0546 (0.1128)		0.0265 (0.1289)	
Dependent Variable: *Dummy Variable for age 6-11* *in 1968*		0.0215 (0.0300)		0.0414 (0.0302)
Age dummies	yes	no	yes	no
Quadratic in age in 1968	no	yes	no	yes

Notes: Standard error in parentheses. Reported coefficients are results from reduced from regression of log wages on the dependent variable. 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.

Figure 1. Junior high schools per thousand primary school graduates



Note: Includes junior high school and junior vocational school

Figure 2. Junior High Students per School

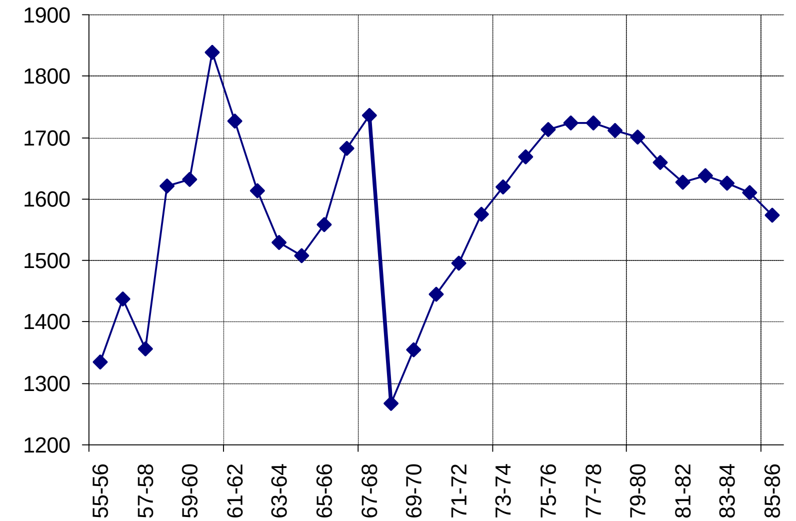
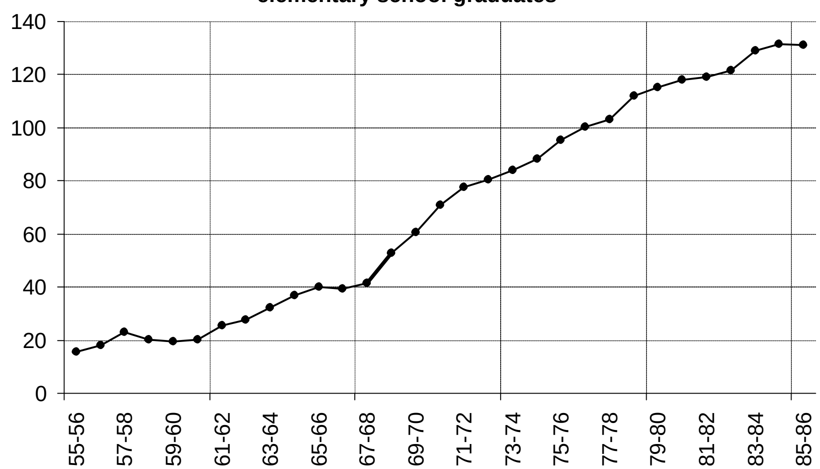
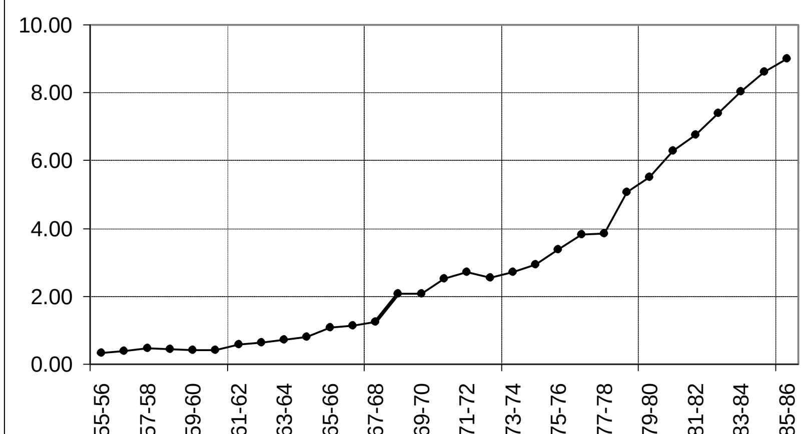


Figure 3. Junior high teachers per thousand elementary school graduates



Note: Does not include junior vocational school.

Figure 4. Real expenditures on Junior High School per primary school graduate



Note: Does not include Junior vocational school

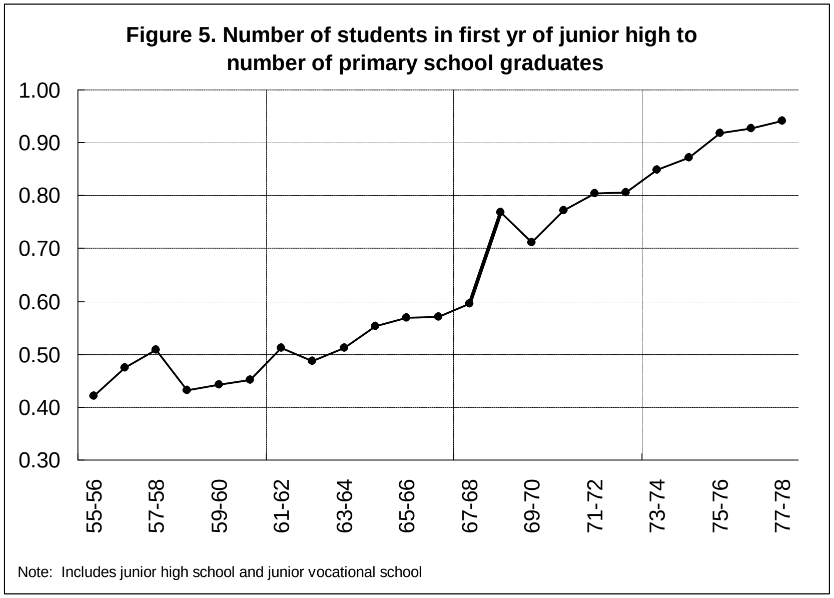


Figure 5. Number of students in first yr of junior high to number of primary school graduates
1.00
0.90
0.80
0.70
0.60
0.50
0.40
0.30
55-56
57-58
59-60
61-62
63-64
65-66
67-68
69-70
71-72
73-74
75-76
77-78
Note: Includes junior high school and junior vocational school

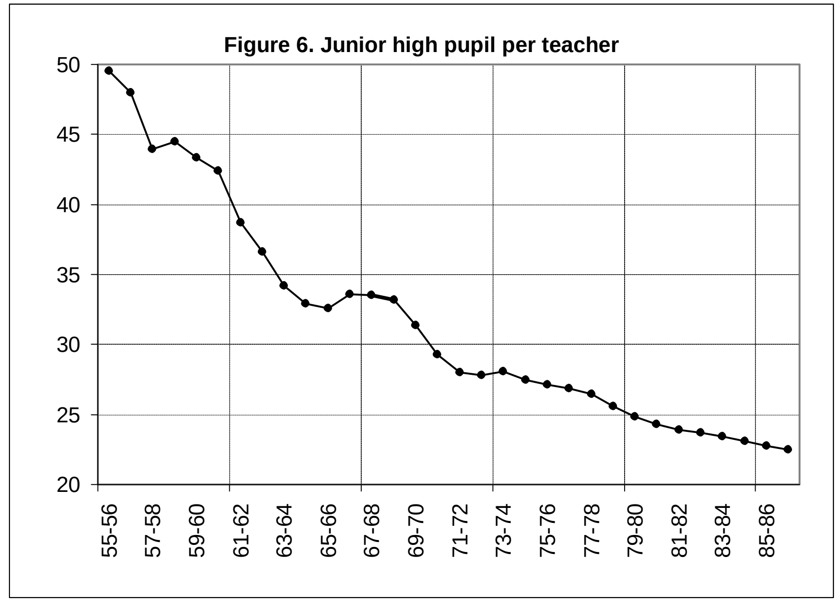


Figure 6. Junior high pupil per teacher
50
45
40
35
30
25
20
55-56
57-58
59-60
61-62
63-64
65-66
67-68
69-70
71-72
73-74
75-76
77-78
79-80
81-82
83-84
85-86

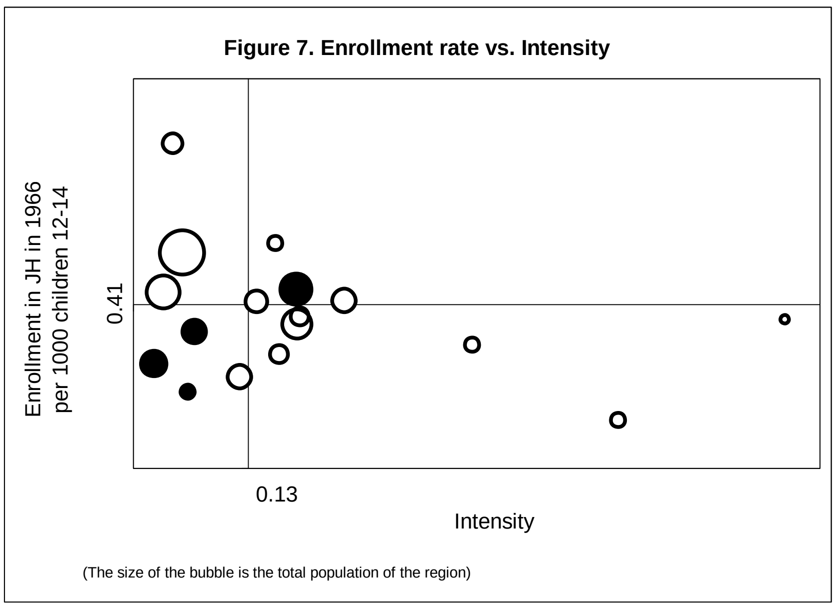


Figure 7. Enrollment rate vs. Intensity
Enrollment in JH in 1966 per 1000 children 12-14
0.41
0.13
Intensity
(The size of the bubble is the total population of the region)

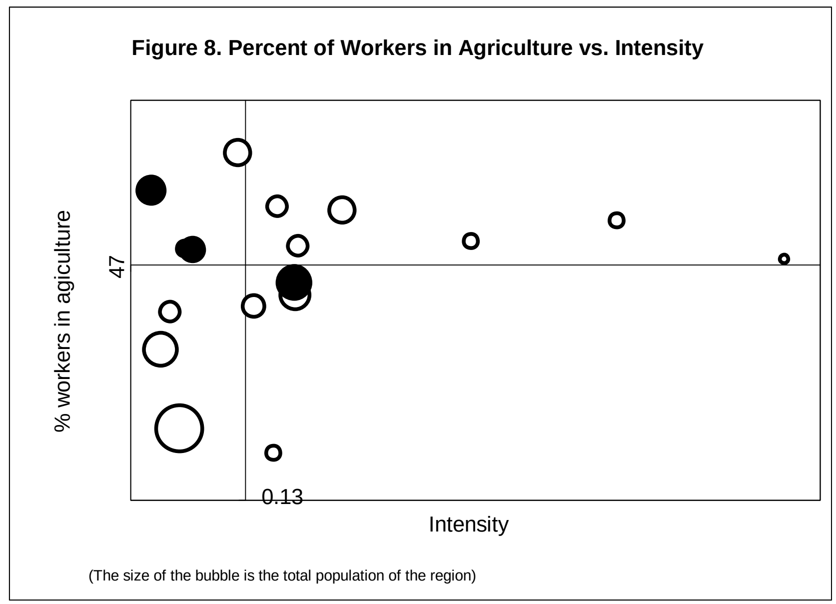


Figure 8. Percent of Workers in Agriculture vs. Intensity
% workers in agiculture
47
0.13
Intensity
(The size of the bubble is the total population of the region)

Figure 9. Cohort Difference Estimate of 1968 School Program by level of education (merged 1994-97 MPU)

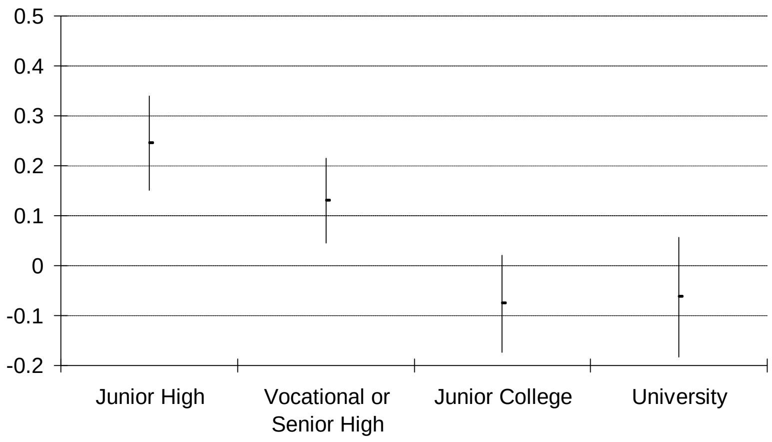


Figure 10. Cohort Difference Estimate of 1968 School Program by level of education (merged 1994-97 FAM)

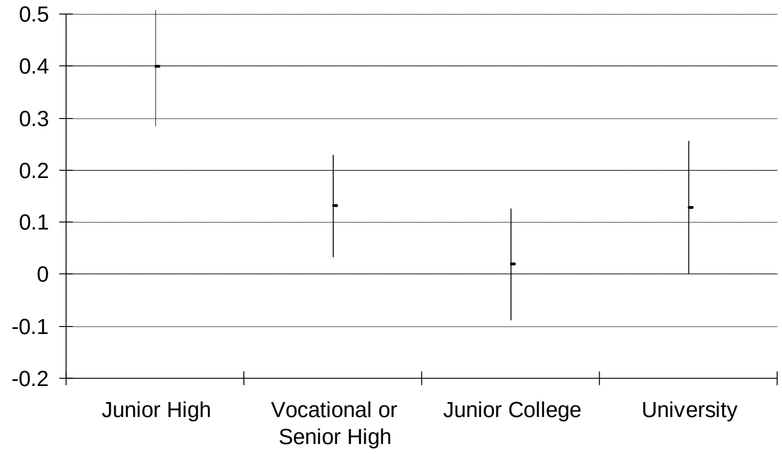


Figure 11. Cohort Difference, Everybody in Taiwan (1990 Population Census)

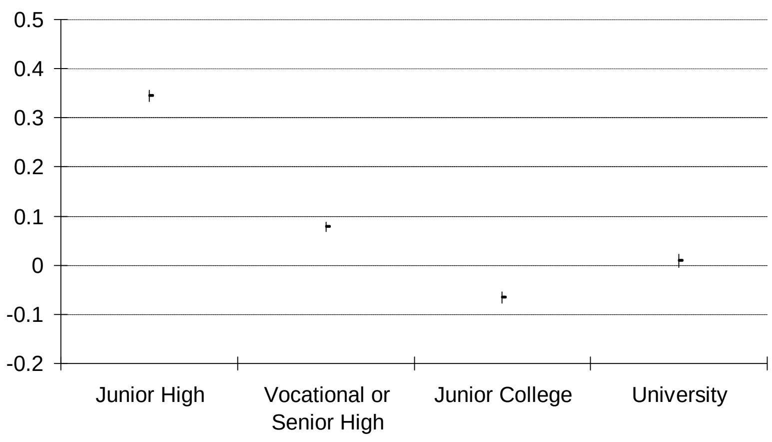


Figure 12. Difference in Difference Estimate of 1968 School Program by level of education (merged 1994-97 FIES)

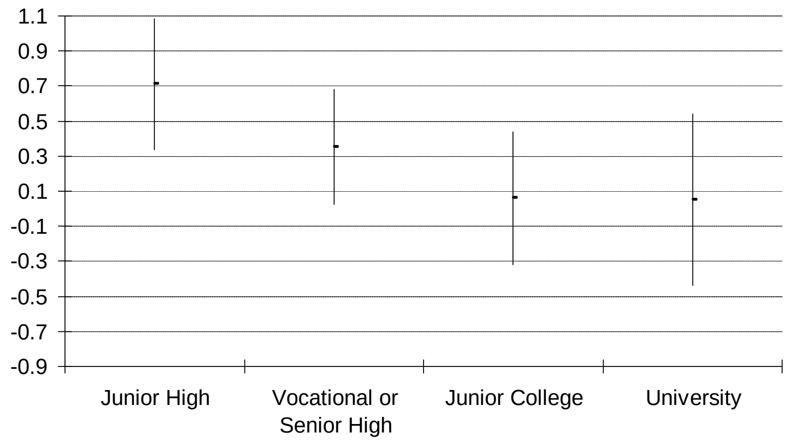


Figure 13. Difference in Difference Estimate of 1968 School Program by level of education (merged 1994-97 MPU)

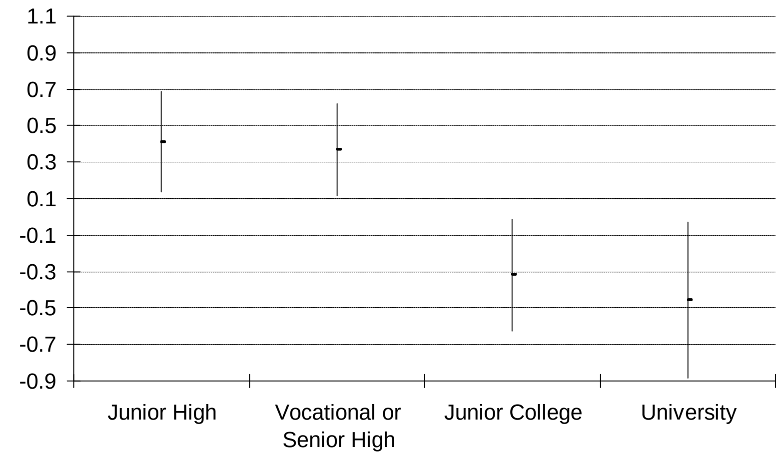


Figure 14. Difference in Difference Estimate of 1968 School Program by level of education for All Men in Taiwan (1990 Census)

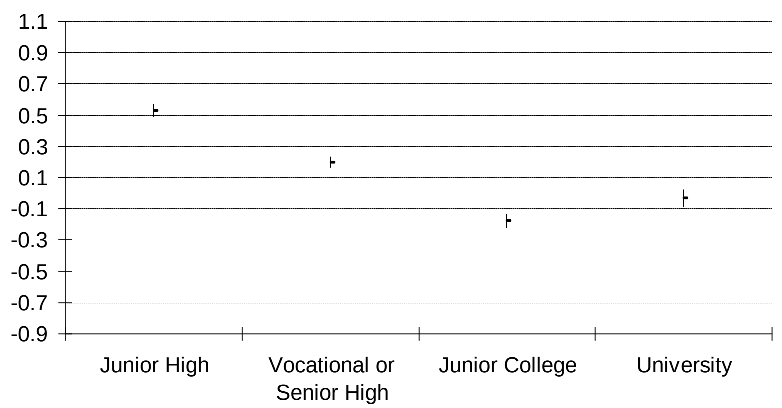


Figure 15. Difference in Difference Estimate of 1968 School Program by level of education for Men Living in Same County as Father's County of Birth (1990 Census)

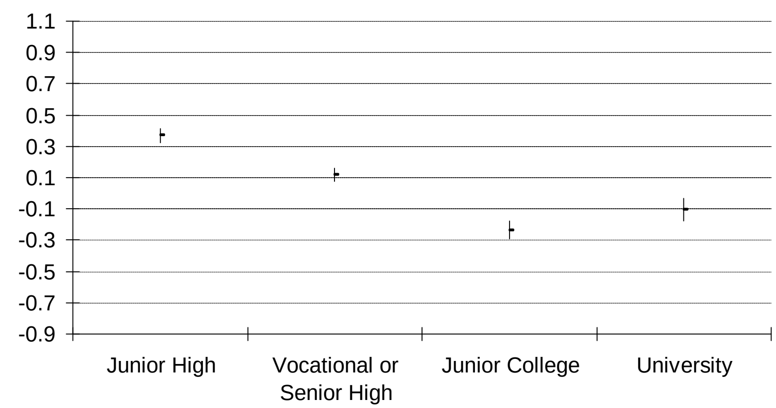
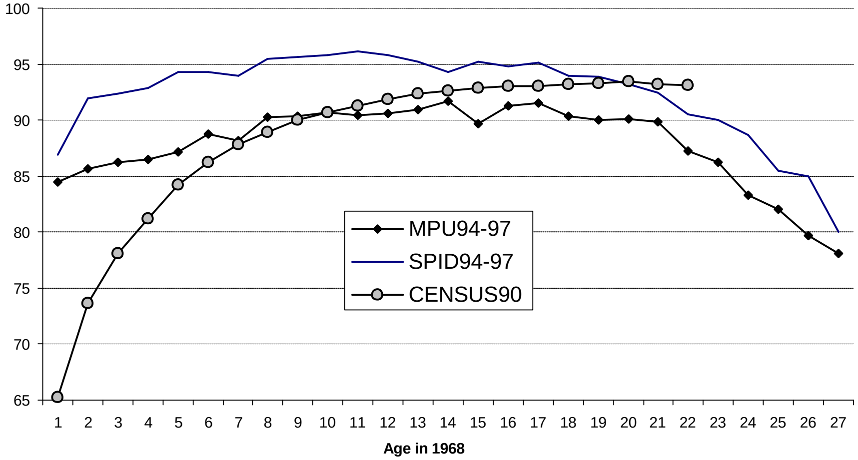


Figure 16. Percent of Men in the Labor Force



Note: In the MPU and PSID labor force participation is defined as men with income over NT$8,000 per month