More Schooling Is Not Always Better: Evidence from an Instrumental Variables Approach to Educational Reform in Vietnam

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Abstract

It is universally accepted that more education afforded to the people is tied to increased future earnings, as shown in many influential studies during the last four decades. However, evidence from Vietnam's educational reforms during the 1980s, in which an additional school year - the previously missing 9th grade - was mandated for people born in and after 1976, may suggest otherwise. Examining four recent surveys of earnings of people born during the transition from the communist K-10 to the Western K-12 education system, with educational attainments no greater than a high-school diploma, I find strong evidence of nonlinear returns on education and a low or even negative return for some grades. Marginal returns are strong for primary school and upper secondary school but are small for lower secondary school. Using the reform and the administrative demarcation line that separated the country into two halves prior to the unification in 1975 as instruments for educational attainments, I show that adding 9th grade to lower secondary school has no impact on future earnings. Ineffective schooling, inappropriate time allotments between taking exams and building skills, and a "sheepskin effect" may be responsible for the zero return on 9th grade in Vietnam.

Keywords: educational reform, return on education, sheepskin effect, instrumental variables (IV), local average treatment effects (LATE)
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1 Introduction

During the past hundred years, Vietnamese education has gradually transformed from a Chinese mandarin writing system, which previously lasted thousands of years, to the Latin

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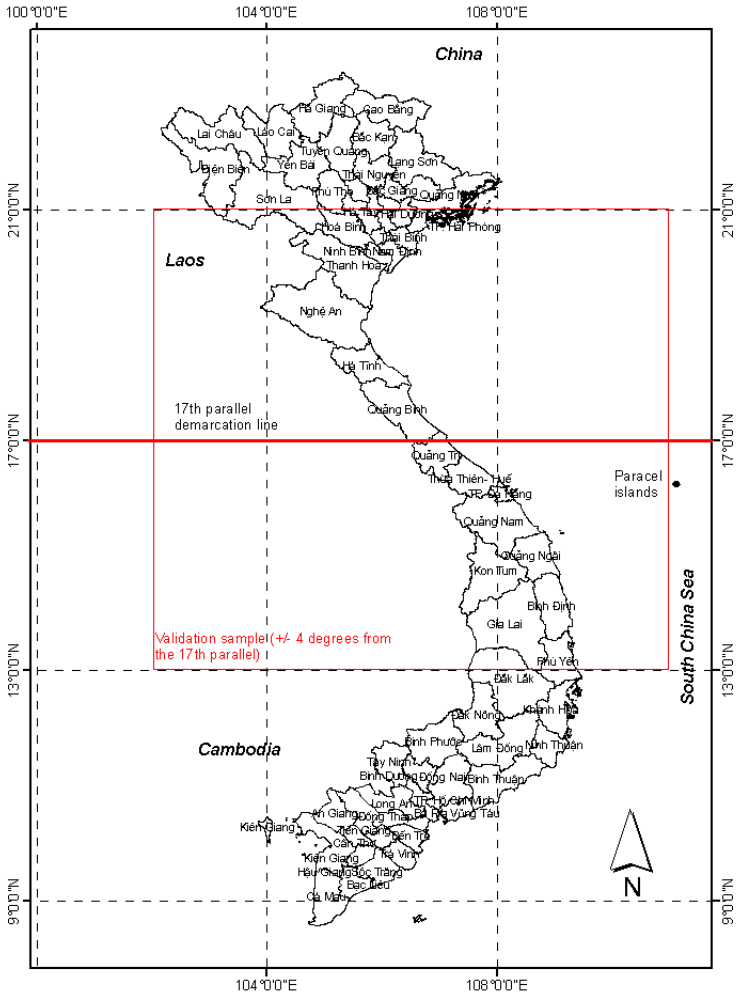
alphabet letter system implemented during the French colonization of the Indochina peninsula in the late 19th and mid-20th century. In the 20th century alone, three educational reforms took place after each major political upheaval in the country. The first two consecutive education reforms in 1950 and 1956 aimed at removing the residual colonial education system and replacing it with a communist-oriented K-10 education system, as in the Eastern bloc in North Vietnam. During that time, primary, lower secondary, and upper secondary school took four, three, and three years to complete, respectively. Prior to the unification of the country in 1975, two educational systems existed, running concurrently in the North and the South, separated at Quang Tri province by the 17th parallel demilitarized zone (Figure 1). South Vietnam adopted the Western education system, like that in North America, which requires 12 years of education in total, with six years in primary schools, three years in lower secondary school, and three years in upper secondary school. The third reform, which took place in the late 1980s, unified these two systems into a single 12-year general education system throughout the country. In the new system, primary, lower, and upper secondary schools require five, four and three years to complete, respectively. Together with the changes in the number of schooling years, there were also gradual changes in textbooks, and new subjects were introduced. The third education reform was completed in 1996.

During a gradual transformation from the communist education structure to the current system, additional classes were supplemented to increase the number of schooling years from ten, typical in the ex-Eastern bloc, to twelve, as in many Western countries. The last wave of reform was in the late 1980s, which added the 9th grade, which was skipped prior to 1990 in North Vietnam. This addition completed the transition from a K-10 to a K-12 standard. Students born prior to 1976 benefited from getting a high school diploma in less than 12 years in the ex-communist education system, whereas those born in and after 1976 were required to take 12 years in the new system. In the communist education system in the North, there was a preschool program that allowed children to enroll at the age of five and attend for one year prior to entering primary school. Due to the canceling of the preschool program in 1981, students born during the gap years between the two systems received less education than people born prior to or after this period (Figure 2).

Unlike *Doi Moi*, the most dramatic economic reform initiated in 1986, which saw the departure of a self-reliance doctrine forcibly adopted in the communist country during the Cold War period, the third education reform included a series of small educational renovations rather than a rapid adoption of the new education system (Le, 2009). Primary education remained free as it is aimed for universalization, and tuition was required for secondary schools. Increasing private contributions and the opening of semi-public and private schools were first permitted. During this time, wage reforms together with more economic freedom, the elimination of (most) state subsidies, and price controls of most staple goods had encouraged more investment in education, and a higher return on education as well.

However, reforms on curriculum and textbooks moved much more cautiously (Le, 2009).

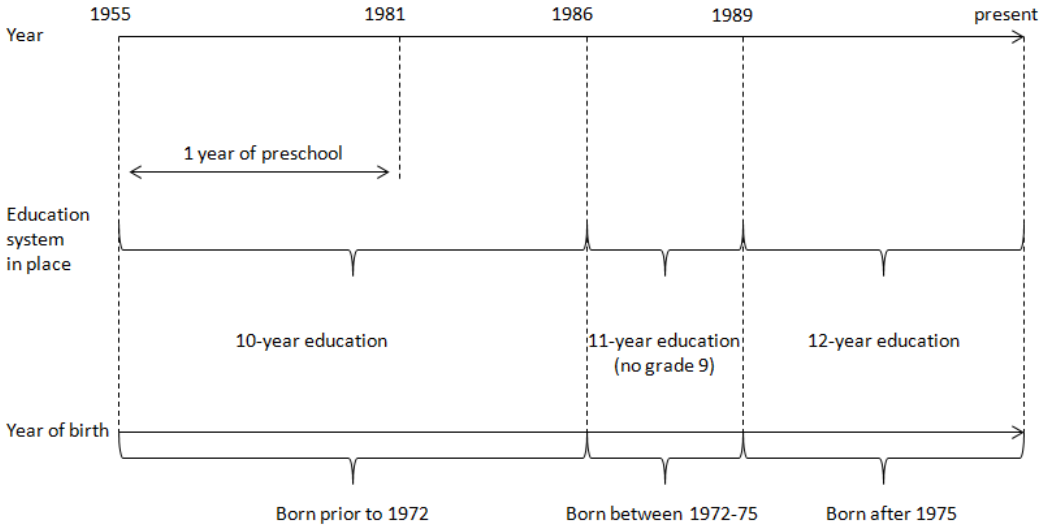
Figure 1
The Demarcation Line between North and South Vietnam prior to 1975



For lower secondary schools, a 33-week curriculum delivered in half-day schooling was shorter than that in other comparable countries. The teaching method was teacher centered, which encouraged rote learning. The curriculum was overloaded and fragmented with 13 subjects at a low level of integration. Furthermore, irregular cost and supplementary teaching remains widespread, making access to education a challenge for the poor and disadvantaged. A more worrying trend recently is the decline in net enrollment rate, which is lower than in the Philippines and about at the same level as China and Indonesia (Nguyen, 2013).

This study is the first to quantify the impact of the missing 9th grade in Vietnam. The dramatic transition from a communist economy to a more liberated market economy, coupled with a large population boom, in post-war Vietnam has resulted in a generation with virtually no knowledge of the forgotten year. Using a rich set of household income surveys, I estimate the impact of the education reform in the 1980s on education attainments and on earnings two decades later. I find that adding an extra year of schooling through 9th grade helped raise the average schooling of the affected students by about a third of a year. Due to an overly

Figure 2
Official Schedule of the 3rd Education Reform in North Vietnam
(Based on the VHLSS Instruction)



cautious approach as well as financial constraints, the reform did not enter in full effect until several years later. Results from a conventional Mincerian approach to education earnings are consistent with existing literature, which suggests that the rate of returns on education in developing countries in recent years appears to be increasing, reaching 6-7%, and this rate higher for women. This may be evidence of effective wage reforms by encouraging private markets to decide worker compensation instead of using the fixed wage grids imposed by the government under the communist economy.

However, I find that the increased schooling associated with the reform did not appear to have any impact on future earnings. I show that returns on education exhibit strong nonlinear patterns, with the highest return for primary education, followed by upper and lower secondary education. Using the third educational reform status and the 17th parallel demarcation line as exogenous instruments for education attainments in Vietnam, I provide an estimate of schooling returns to those affected by the introduction of 9th grade. I find zero or even negative returns on the additional schooling among the affected people.

This finding adds another aspect to the current estimates of returns on education in Vietnam, with a brief summary in Table 1. Existing studies put the average returns on education in Vietnam between 5-10% per year since the late 1990s, consistent with effective market reforms and the removal of rigid communist wage grids. The return was less than 5% in the early 1990s (Moock et al., 2003; and Liu, 2005). The instrumental variables approach shows a higher return on education than the least squares estimate during the same period, from 5.1 to 7%, relative to 2.6% (Arcand et al., 2004). Returns as high as 8-10% were seen, and even higher in recent studies using more up-to-date data (Doan and Gibson, 2010; Oostendorp and Doan, 2013; and Phan and Coxhead, 2013). However, no existing study examines the impact of the educational

reform on either schooling years or potential earnings. Here, I present an exact estimate of the return on extra education obtained by people having to go through 9th grade, as required by the education reform of the 1980s. I arrived at the same conclusion of low or even zero returns to extra (compulsory) schooling as several studies in developed countries, such as Britain and Germany. Meanwhile, models relying on a conventional education-earnings approach produce comparable results with other studies in Vietnam. These two distinct estimates of returns on education are in fact the average return for the whole population, which is universally positive, and the local average return for a group of people affected by the policy, which might be above or below the population average.

The most likely explanation for a zero or even negative return on 9th grade in Vietnam is perhaps due to slow reform in the curriculum and textbooks, ineffective schooling, and excessive focus on passing exams rather than on building skills. As a result, the extra year of school did not really improve labor productivity and thus earnings. Moreover, the compensation scheme in developing countries is more dependent on experience and seniority rather than years of schooling. Finally, and admittedly not one of the most favorable conclusions, the discrete jumps in wages simply reflect a "sheepskin effect", the effect of having obtained an academic degree irrespective of the time spent in school, is particularly strong in Vietnam.

The paper proceeds as follows. The next section summarizes the literature on education return, followed by the description of the dataset in section three. In section four, I provide quantitative evidence of the effect of the last reform on educational attainments and present several empirical approaches to estimate the returns on education. Section five discusses the new findings. Section six concludes with policy implications.

2 Review of Literature on Returns on Education

A primary approach to schooling return, following Mincer (1974), describes the relation between school attainment and earnings by an empirical model:

$$\log \mathrm{Y_i} = \log \mathrm{Y_0} + \beta_1 \mathrm{S_i} + \beta_2 \mathrm{Exp_i} + \beta_3 \mathrm{Exp_i^2} + \mathrm{u_i}$$

in which $\mathrm{Y_i}$ is some measure of earnings, either the hourly wage rate, or annual earning; $\mathrm{S_i}$ is educational attainment, such as the number of years in school; and $\mathrm{Exp_i}$ is the years of experience. In the original Mincerian approach, $\mathrm{Exp_i} = (\mathrm{Age_i} - \mathrm{S_i} - \mathrm{b})$ where b is the age of compulsory schooling. u_i is the earning residuals, included to capture unobserved factors that influence earnings. Thus, $\mathrm{Y_0}$ is the average earnings, the intercept of the earning equation, which is the expected earnings of people with no experience or education. Mincer's earning approach assumes that the logarithm of personal earnings $\log \mathrm{Y_i}$ is linearly related to educational attainment, $\mathrm{S_i}$, thus implying a constant marginal rate of return. Experience affects earnings nonlinearly: there is a decreasing rate of return on experience, and an optimal level of experience for which earnings peak. Because the dependent variable is in logarithm, the coefficient of

Table 1: Some Recent Estimates of Returns on Education in Vietnam

	Rate of return (%/year)	Methodology	Data
Moock et al. (2003)	4.8 average, 3.4 (M), 6.8 (F)	Mincer-type regression	VHLSS 1992-93
Arcand et al. (2004)	2.6 (OLS); 5.1-7 (IV)	IV by parents' education, smoking habit, and college proximity	VHLSS 1992-98
Liu (2005)	5.9 (M), 4.2 (F) in 1992-93; 3.5 (M), 4.8 (F) in 1997-98	Katz-Murphy supply-demand	VHLSS 1992-98
Doan and Gibson (2010)	2.9-9.1 (OLS) ; 3.8-10 (Heckman-selection)	Modified Mincer-type regression	VHLSS 1998-2008
Oostendorp and Doan (2013)	1.9-2.7 (primary); 10-12.5 (secondary); 15.7-21.6 (college)	Modified Mincer-type regression with employment effects	VHLSS 1998-2006
Phan and Coxhead (2013)	1.3-8.7 for non-state, trading and state, non-trading sectors	Sectoral decompositions and Heckman-selection	VHLSS 1993-2008
This Study	4.9-6.4 (OLS); zero or negative (IV-LATE); sample including wage earners with the highest academic degree no greater than high-school diploma	Mincer-type regression, IV by reform status and the 17th parallel demarcation line	VHLSS 2004-10

Note: M and F denotes male and female. I exclude the 1992-93 estimates in Phan and Coxhead (2013) due to negative returns.

education is interpreted as the rate of change in earnings resulting from a one unit (year) increase in schooling.

Psacharopolous (1985) summarizes the returns on education in various countries and suggests that the highest returns are observed in primary school, followed by secondary school, and the lowest returns are for college and university graduates. High returns on education were also observed for women in countries with the lowest per capita income. The rate of returns varies, typically from 11-13% for each additional year of schooling in Asia and Africa to 8-9% in more advanced economies. The rate of returns appeared to have decreased slightly during the early 1990s in a 28-country cross-sectional study conducted by Trostel et al. (2002). The highest return was observed for working women in the Philippines, reaching 19.2%, in contrast to only 1.9% for working women in the Netherlands. They also reported some evidence of diminishing returns on schooling. That is, the return for each additional year of schooling decreases as the cumulative years of schooling increases. Early studies using the Mincer equation assume that there is a constant rate of return on education for the whole population. However, Heckman et al. (1995) show that returns on education are not linearly, or nearly linearly, related to the logarithm of earnings. Particularly, they show strong evidence of a "sheepskin effect" or

significant jumps in returns on education after the completion of a college degree.

Recent developments in econometric techniques seem to have acknowledged that least squares estimates envisioned in the original Mincer's study may not truly establish a causal relation between education and earnings. A seminar work by Griliches in 1977 shows that many problems may seriously bias the least squares estimates of the earnings equation. For an ordinary least squares method to produce an unbiased estimate of the return on education, β_1, several restrictive assumptions must hold. Among which, omitted variables bias and measurement errors are most evident with survey data or data collected from natural experiments. First, education and earnings are generally positively correlated with some unobserved innate abilities, thus a causal interpretation of the impact of education on earnings is misleading. Second, measurement errors in education tend to produce attenuation bias in least squares estimates. Many authors attempt to correct potential biases by including more controls such as industries, family background, and a proxy for personal abilities in addition to more flexible functional forms.

To make the arguments more tractable, consider a "true" Mincer-type earnings function:

$$\mathrm{y_i} = \mathrm{lnY_i} = \alpha + \beta \mathrm{S_i} + \gamma \mathrm{A_i} + \mathrm{X_i}\delta + \mathrm{u_i}$$

which is an extension of the original Mincer earning equation, augmented by an individual ability variable $\mathrm{A_i}$ and a vector of other controls $\mathrm{X_i}$. Because individual ability is often unobserved, the estimate of β is affected by the omitted variable - so-called omitted variables bias - as shown in Griliches (1977):

$$\beta_{\mathrm{OLS}} = \beta_{\mathrm{true}} + \gamma \frac{\mathrm{cov(A, S)}}{\mathrm{Var(S)}}$$

Because education and innate ability are generally positively correlated, the return on education based on an ordinary least squares method produces an upward biased estimate of the true return on schooling. That is, the estimated return is higher than the actual return because the coefficient of the return on education also picks up the influence of unobserved ability on earnings.

Another famous problem with least squares estimations is measurement errors in reported schooling, which causes attenuation biases in the least squares estimate:

$$\beta_{\mathrm{OLS}} = \beta_{\mathrm{true}} \frac{\mathrm{var(S)}}{\mathrm{var(S + errors)}}$$

The measurement errors in education then lead to a downward bias of the estimated return, which is dependent on the relative size of noises (errors) to signals (true schooling): the larger the errors, the larger the downward bias on estimated educational return. Then, it is difficult to determine the overall bias due to the presence of omitted variables, measurement errors, and perhaps simultaneity between education and earnings. A natural choice in such a situation is to attempt an instrumental variable that is correlated with schooling but not the residuals of earnings. Supposing that an exogenous variable Z is related to schooling but does not directly

influence earnings, a two-stage estimation strategy could be employed to provide a consistent estimate of the return on education coefficient:

$$S_i = z_0 + z_1 Z_i + X_i z_2 + v_i$$

$$\ln Y_i = \alpha + \beta S_i + \gamma A_i + X_i \delta + u_i$$

Assuming that the exclusion restriction is satisfied, u_i and v_i are uncorrelated, then a consistent estimate of returns on education can be computed from:

$$\beta = \frac{\text{cov}(Z, S)}{\text{cov}(Z, Y)}$$

Apparently, the OLS estimates could be either upward or downward biased, depending on the relative influence of measurement errors and the omitted variable bias. Griliches (1977) suggests that the OLS bias is insignificant, which was later corroborated by IV estimates in Angrist and Krueger (1991, 1992). For example, they argue that due to a compulsory schooling law in the US that forced students to enroll at a certain age, the quarters of birth are correlated with schooling attainment yet are not expected to influence earnings. Therefore, the quarters of births can be used as instruments for education attainments. They find little evidence of bias in least squares estimates, and IV estimates are consistently slightly larger than OLS estimates, normally within 10%. They conclude that measurement errors in education are the primary source of bias in OLS estimates, in agreement with Griliches (1977). Duflo (2001) also shows no significant difference between the OLS and IV estimates of the returns on education in Indonesia, ranging from 6.8 to 10.6%.

However, other authors seem to have a different view. Notably, Card (1994, 1999, 2001) suggests that IV estimates using institutional changes tend to exceed OLS estimates by approximately 20-40%. Similarly, Trostel et al. (2002) observe that estimates by instrumental variables are consistently approximately 20% higher than the OLS counterparts in a large sample of 28 countries. They use spouses' or parents' education levels as instruments for education attainment because marriages often occur between couples with matched educational backgrounds, and educated parents often afford good educations to their children. Meanwhile, the education levels of spouses or parents do not in any comprehensible way directly affect an individual's earnings, thus satisfying the exclusion restriction.

Another source of bias is heterogeneous returns on education. If the rates of return differ between individuals in the population rather than being uniform across the population, then the IV approach might produce a local average treatment effects (LATE) estimate corresponding to the affected subpopulation rather than the average rate of return for the whole population (Imbens and Angrist, 1994). Card concludes that this local effect explains why IV estimates might be significantly higher than OLS estimates if the subpopulation affected by the change also has the highest marginal return on schooling. For example, people who benefit from additional schooling are often the poor and most disadvantaged, whose returns to schooling are

often the highest. Then, instrumenting schooling attainments by policy changes presents an average return on additional schooling for the low-attainment-high-return group that will likely differ from the average return for the whole population. This interpretation of the IV-LATE estimator is particularly interesting because it is directly related to the effect of the policy on the group we are interested in. However, the external validity of the IV-LATE estimator is often limited.

For illustrative purposes, as in Card (2001), suppose that the instrument Z is dichotomous, e.g., an individual was either affected by a school reform or not. The effect of the school reform on earnings is approximated by:

$$\Delta \text{logY}_\text{i} = \beta_\text{i} * \Delta \text{S}_\text{i}$$

where β_i is the marginal return to schooling and ΔS_i is the additional schooling caused by the reform on individual i. At the probability limit of the instrumented coefficient:

$$\text{plim } \beta_\text{IV} = \frac{\text{cov(Z, S)}}{\text{cov(Z, Y)}} = \frac{\text{E[logY}_\text{i}|\text{Z}_\text{i} = 1] - \text{E[logY}_\text{i}|\text{Z}_\text{i} = 0]}{\text{E[S}_\text{i}|\text{Z}_\text{i} = 1] - \text{E[S}_\text{i}|\text{Z}_\text{i} = 0]}$$

Suppose identical distributions of earnings and schooling conditional on the observable and unobservable characteristics in the treated and control population; therefore:

$$\text{E[logY}_\text{i}|\text{Z}_\text{i} = 1] = \text{E[logY}_\text{i}|\text{Z}_\text{i} = 0] + \text{E}[\beta * \text{S}_\text{i}]$$

and

$$\text{E[S}_\text{i}|\text{Z}_\text{i} = 1] = \text{E[S}_\text{i}|\text{Z}_\text{i} = 0] + \text{E}[\Delta \text{S}_\text{i}]$$

The IV estimator then reduces to:

$$\text{plim } \beta_\text{IV} = \frac{\text{E}[\beta_\text{i} * \Delta \text{S}_\text{i}]}{\text{E}[\Delta \text{S}_\text{i}]}$$

which means that at the probability limit, the return on education is equal to a weighted average of individual marginal returns, with weights equal to the effect of the reform on affected individuals, such as how many years of schooling an individual gained due to the reform. This is the LATE estimator of the return on education for the subgroup whose treatment status changes with the policy assignment. This estimator will collapse to $\text{E}[\beta_\text{i}]$ if the reform induces an equal change in education for all. Otherwise, the LATE estimate of β can be higher, or lower than the average treatment effect (ATE), $\text{E}[\beta_\text{i}]$, if the subpopulation affected by the reform has a higher or lower marginal return. Several studies, including Card (1994,1999,2001), suggest that LATE estimates of schooling returns are higher than the average return because the individuals affected by educational reforms are also most likely to have the highest marginal return. For example, using the World War II cohort as the instrument for education attainment in the axis countries, Ichino and Winter-Ebmer (2004) find sizable earnings losses among people directly affected by the war through the adverse impacts of war on education. They estimate

a 2.5% earning loss among people born in 1930-39 in Austria, but no similar loss pattern was observed in other comparable countries, such as Sweden or Switzerland. Kling (2000) also finds significantly higher returns on education using college proximity as the instrumental variable. The IV estimates are in the range of 10 to 14%, as opposed to only 8% by the OLS method, apparently relating to high returns in the group of people with disadvantaged family backgrounds or those with lower levels of parental education.

A more interesting finding, however, was that some instrumental variables estimates of returns on compulsory schooling are very low or even zero in several countries in Europe. For example, Pischke and von Wachter's (2005) study in Germany after World War II shows that extending the number of compulsory schooling years from eight to nine does increase the average schooling attainment, though the increase is not very large, only 0.17 years. However, the return on the extra compulsory schooling is essentially zero. They suspect that academic skills relevant to labor productivity were perhaps learned prior to reaching 9th grade, which was later corroborated by Kamhofer and Schmitz's (2013) study that shows no cognitive skills gains from the additional schooling. Pischke (2007) examines the effect of an unusual change in schooling term without changing the curriculum, the highest grade, or academic degree received in Germany during 1966-67. He concludes that the loss of about two thirds of a schooling year adversely affected grade repetition and enrollments in higher secondary school tracks, but finds no negative impact on earnings or employment later. This could be explained by the signaling value of having an academic degree, also known as the "sheepskin effect", rather than actual human capital accumulation that is affected by the time spent in school.

This study shares the same setting as Pischke and von Wachter (2005) and Kamhofer and Schmitz (2013) in which an additional schooling year, the 9th grade, was added to lower secondary school. However, institutional differences between post-war Germany and post-war Vietnam spell out very different motivations and impacts on schooling achievement and on future earnings. In Germany, the additional schooling year was compulsory. In Vietnam, the additional schooling year was not. Compulsory education was limited to primary education, the first five grades, at the time of the reform in the 1980-90s. Universalization of lower secondary education was achieved only recently. In Germany, the policy had a limited impact on schooling length. In Vietnam, significant increases in schooling years were observed after the reform. However, the estimated return on extra schooling is low or zero in both instances. That is, students were forced to take the extra schooling year without any reward in terms of future earnings.

3 Data Description and Summary Statistics

I employ the four latest rounds of the Vietnam Household's Living Standard Surveys (VHLSS), which were conducted by Vietnam's General Statistical Office (GSO) under the auspices of

the World Bank. The VHLSS survey has a long history, dating back to the first survey in 1992-93. Subsequent surveys in 1997-98, 2002, 2004, 2006, 2008, and 2010 extended the sample and materials to cover additional modules including: agricultural and nonagricultural business, health and education, and governance. I skip surveys conducted before 2004 to avoid the effect of market reforms on wages. Using more recent data also ensures that people affected by the education reform would likely have had sufficient time to settle into a formal wage-earning job. Again, the 9th grade was first introduced in 1990 and applies to people born in 1976, who turned 15 in 1990 and 28 in 2004.

The derivation of the variables follows the Mincer-type regression, with some exceptions. The wage is annual wage in thousands of Vietnam Dong (VND). Education is the actual years of schooling not converted from completed academic degrees. The years of experience is the reported number of actual years of experience the person had at the time of survey. This is slightly different from the original Mincer study in which experience is the difference between age, education, and the compulsory schooling age, $\text{Exp}_\text{i} = \text{Age}_\text{i} - \text{S}_\text{i} - \text{b}$, where b is the compulsory primary school age. The reason is that experience, for what matters, is only relevant if it is related to the current wage-earning job. In Vietnam, job switching is prevalent and many may have obtained non-relevant experience prior to beginning a formal wage-earning job. As a result, the availability and use of actual experience data is a superior choice to the original Mincer study. Apparently, the actual year of experience is significantly lower than the one derived by the original Mincer (1974) approach.

I restrict the sample to people with a high school diploma as the highest educational level. There are several reasons for this limitation. First, those people with only high school diplomas were more likely affected by the required additional schooling than college or university graduates. Thus, the returns on education for this group may be more relevant for policy evaluation. Second, the datasets do not report how many years a person spent to obtain a college or university degree. Because college or university durations vary significantly between fields, with natural science and technology degrees normally taking one year longer than social sciences and humanities degrees, extrapolating from college degrees to years in school without knowing the exact field of study is prone to errors. Furthermore, if people with higher academic degrees were included, to avoid including fresh college graduates who may not have had sufficient time to settle into a stable job at the time of the survey in the analysis, only a very limited sample could be used. As a result, only those who were at least 20 years old or older, who were working for a wage at the time of the survey, who were not currently enrolled in any school, and whose education attainments were no greater than a high-school diploma, were included in the analysis. Sample statistics for the northern part of the country, where the reform and addition of 9th grade was applied, are presented in Table 2. Note that the four samples cannot be used in a panel data manner because only a few observations were repeatedly surveyed.

It is surprising that the average educational attainment is high for wage earners, almost 9.8 years of education. However, this high value should be interpreted with care. Prior to

the reform, there was a preschool program that provided an extra year of education before beginning primary school. Thus, taking that one year off would result in approximately 8.8 years of education on average. Although this is still a high number, it is not unreasonable, especially for those in wage-earning jobs as opposed to those who are self-employed or who own household businesses. Meanwhile, the average years of *relevant* experience is much lower than the one derived from the original Mincer approach, less than eight years in all samples. This reflects a very dynamic phase of market reform that comes with more frequent job switching in developing countries. Finally, the average wage growth was quiet impressive and is still rising, which might include two effects at the same time: rising returns on education and changing employment structures that offer more high-earning jobs in more recent years. Oostendorp and Doan (2013) show that once a changing employment effect was accounted for, the return on education is actually lower.

Table 2: Summary Statistics
Mean and standard deviation (in bracket).

Variable	**2004**	**2006**	**2008**	**2010**
Age	35.05	35.07	35.71	35.95
SD	(10.98)	(11.43)	(11.13)	(11.24)
Annual wage (thousand VND*)	7,840.39	10,230.59	14,512.24	22,366.23
SD	(6,173.53)	(7,742.54)	(11,392.26)	(14,105.42)
Years of schooling	9.82	9.93	9.96	9.76
SD	(2.07)	(2.02)	(2.05)	(2.42)
Years of experience	7.67	7.68	7.50	—
SD	(7.56)	(7.79)	(7.18)	
Number of observations	2,074	2,191	2,261	2,307

Note: The data are restricted to north of the 17th parallel line. The VHLSS 2010 survey does not report a separate question for the years of experience. Thus, in subsequent analyses, age is used in place of the year of experience whenever the year of experience is not available.
* The official exchange rate in 2004 is VND/USD 15,746, rising to VND/USD 18,613 in 2010. Source: the World Bank.

4 Empirical Strategy

4.1 Impact of the Reform on Educational Attainments

First, I investigate the impact of the reform on educational attainments. I regress the years of schooling S_i on a set of year dummies, D_k, and a constant. For this analysis I focus on the people most likely to be affected by the reform, those born 10 years prior to and those born after the cutoff year, 1975-76. The regression equation is as follows:

$$S_i = \sigma_0 + \sum_k \sigma_k D_k + \eta_i \tag{1}$$

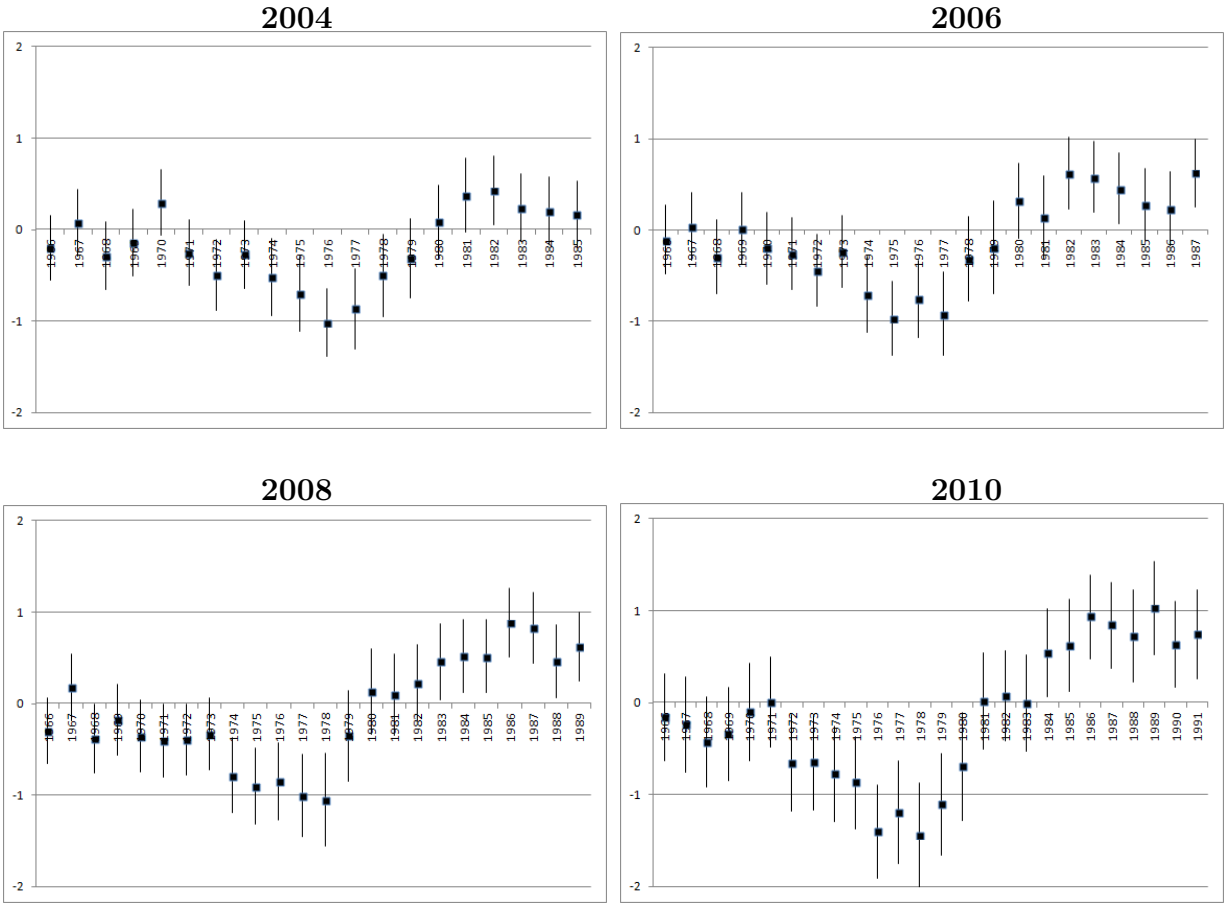
where k = 1966, 1967, etc. Year 1965 is the baseline. η_{i} is the education residual, assumed *iid* normally distributed.

Figure 3 shows the confidence interval estimates, $\widehat{\sigma_k}$, of the year dummies, which is the impact of the year of birth on the average education attainment, $\widehat{\sigma_0}$. All four surveys show a consistent pattern of the influence of the year of birth on educational attainment, with people born between 1972-78 most likely to be affected. Due to early dropouts, the average years of schooling was more than nine years for wage earners sampled in the four latest surveys. Younger cohorts appear to have more schooling by one-half to one year above the average, which is the result of a higher maximum number of schooling years, therefore allowing students to stay in school longer, and increasing investment in education by parents. Meanwhile, people born during the transition period appeared to have the lowest average schooling level among all cohorts. The reason is that older cohorts were subject to a preschool program, which started at the age of five and lasted for one year prior to enrollment in primary school at the age of six. Preschool was dropped in 1981, corresponding to the year of birth in 1976. The years of schooling is inclusive of preschool, primary school, and secondary school. The maximum number of years of schooling was 11 years for people born prior to 1972 (with one year in preschool included), 11 years for the 1972-75 cohort (without 9th grade), and 12 years for the post-1975 cohort.

There is evidence that the gap years affected more people than originally designed, i.e., the people born during 1972-1975, according to the reform schedule, would have entered 9th grade in 1986-89. There are several possible explanations for the unintended impact on other cohorts of birth. First, the administrative age of enrollment, which is six years old by calendar year, regardless of the month of birth, does not always match with the actual age of enrollment. Some parents might have had incentives to send their children to school prior to the required age of enrollment and were able to do so. Therefore, some started primary school at the age of five instead of six. Although this phenomenon was rare, it did happen. Second, grade repetitions may have delayed some people from taking advantage of the shorter education system. Third, the Vietnam War was entering the last phase around 1973-75, followed by two brief Cambodia-Vietnam and Sino-Vietnam military conflicts in 1978 and 1979, causing some children to enroll at an older age than six years old. Particularly, in rural Vietnam, where the use of child labor for family livelihood was universally required, the actual schooling age might be different, expectedly older than the legal age of six.

The evidence is clear that the reform and the transition between the two education systems negatively influenced the average schooling of the cohort born during the gap years, 1972-78. Because the Vietnam War ended in 1975 with the unification of North and South Vietnam, it is unlikely that educational attainments were affected by factors other than the reform and a general increasing education tendency. The educational reform, including the transition from the Eastern bloc's K-10 system to the Western K-12 system, and the abolition of preschool, caused a temporarily lower average years of schooling for those born between 1972-78.

Figure 3
Impact of Year of Birth on Education Attainment



Note: The dependent variable is the years of schooling. Each vertical bar represents the 95 percent confidence interval of the corresponding year dummy coefficient.

4.2 Impact of Educational Attainments on Earnings

For the basic result, I estimate a conventional Mincerian regression of the education-earnings function:

$$\log Y_i = \log Y_0 + \beta_1 S_i + \beta_2 Exp_i + \beta_3 Exp_i^2 + \beta_4 X_i + \varepsilon_i \quad (2)$$

in which $\log Y_i$ is the logarithm of annual wage, $\log Y_0$ is the average earnings (in log form), Exp_i is the number of years of experience, S_i is the years of schooling, and ε_i is the earning residual of individual i. X_i is a vector of other control variables. If the return on education is linear, β_1 is interpreted as the marginal return for an extra year of education. Meanwhile, a nonlinear return on work experience is assumed. The quadratic term of work experience is expected to be negative, implying a decreasing return on experience. Note that the measurement of schooling used in the Mincerian regression is the total number of years spent in school, regardless of the structure or content of education.

The result is presented in Table 3, with each column corresponding to a survey year, namely,

2004, 2006, 2008, and 2010. It is perhaps not surprising that all least-squares coefficients are well predicted and within reasonable ranges, as reported in other studies in Vietnam as well as in other developing countries (Psacharopoulos, 1985; Duflo, 2001, Phan and Coxhead, 2013). The return on an additional year of schooling is slightly higher than 6%, except for the year 2006, when only 4.9% was reported. A higher return on education is observed for women (results not reported). Work experience is as important as education for fresh graduates or people whose accumulated experience is still low. However, due to the diminishing marginal return on work experience, experience is less important for older people. The optimal years of experience is approximately 27 years, almost the same as the 26 years reported in the Czech Republic during communism and the transition (Munich et al., 2005). Including other control variables such as dummies for industries and public servants reduces the coefficient of education returns slightly, but not a lot.

Table 3: Mincerian Estimates of Returns on Education
(North of the 17th parallel line only)

Variable	**2004**	**2006**	**2008**	**2010**†
Years of schooling	.0639	.0491	.0611	.0576
SE	(.0089)	(.0104)	(.0083)	(.0097)
Years of experience	.0588	.0587	.0537	.1127
SE	(.0090)	(.0083)	(.0071)	(.0094)
Years of experience squared	-.0011	-.0010	-.0010	-.0015
SE	(.0003)	(.0003)	(.0003)	(.0001)
Constant	7.7611	8.1863	8.4396	7.2922
SE	(.0924)	(.1018)	(.0814)	(.2198)
Number of observations	2,074	2,191	2,261	2,307

Note: All estimated coefficients are statistically significant at the 99 percent confidence level. Robust provincial clustered standard errors are reported in bracket.
The dataset include working people born in and before 85, 87, 89 and 91, and less than 70 of age at the time of survey, corresponding to 2004, 06, 08, 10 survey year.
† Age variable is used instead of the years of experience as explained in section 3.

The result from the Mincerian education-earnings equation suggests two things. First, the return on education in Vietnam in the late 2000s is comparable to other developing countries. This may suggest effective market reforms, which encourage private markets to decide labor compensation instead of fixed-wage grids imposed by the government in a communist economy. Second, the importance of work experience becomes relatively more significant when a person does not possess much experience, which is the case for most fresh graduates and early dropouts.

However, the Mincerian education-earnings estimates in Table 3 simplify the earning profile to a linear function of education, i.e., an extra year of education received at a lower grade has the same impact on future earnings as one at a higher grade. This may not be the case. Most basic skills are learned during primary school; therefore, a higher than average return is expected for primary education. If educational return is indeed non-linear, specification bias

may result from aggregating schooling years across different school levels following the Mincer approach. Several authors have attempted to account for possible nonlinear returns by using a polynomial of a second or third degree of the years of schooling, such as Oostendorp and Doan (2013).

4.3 Evidence of Nonlinear Returns to Education Attainments

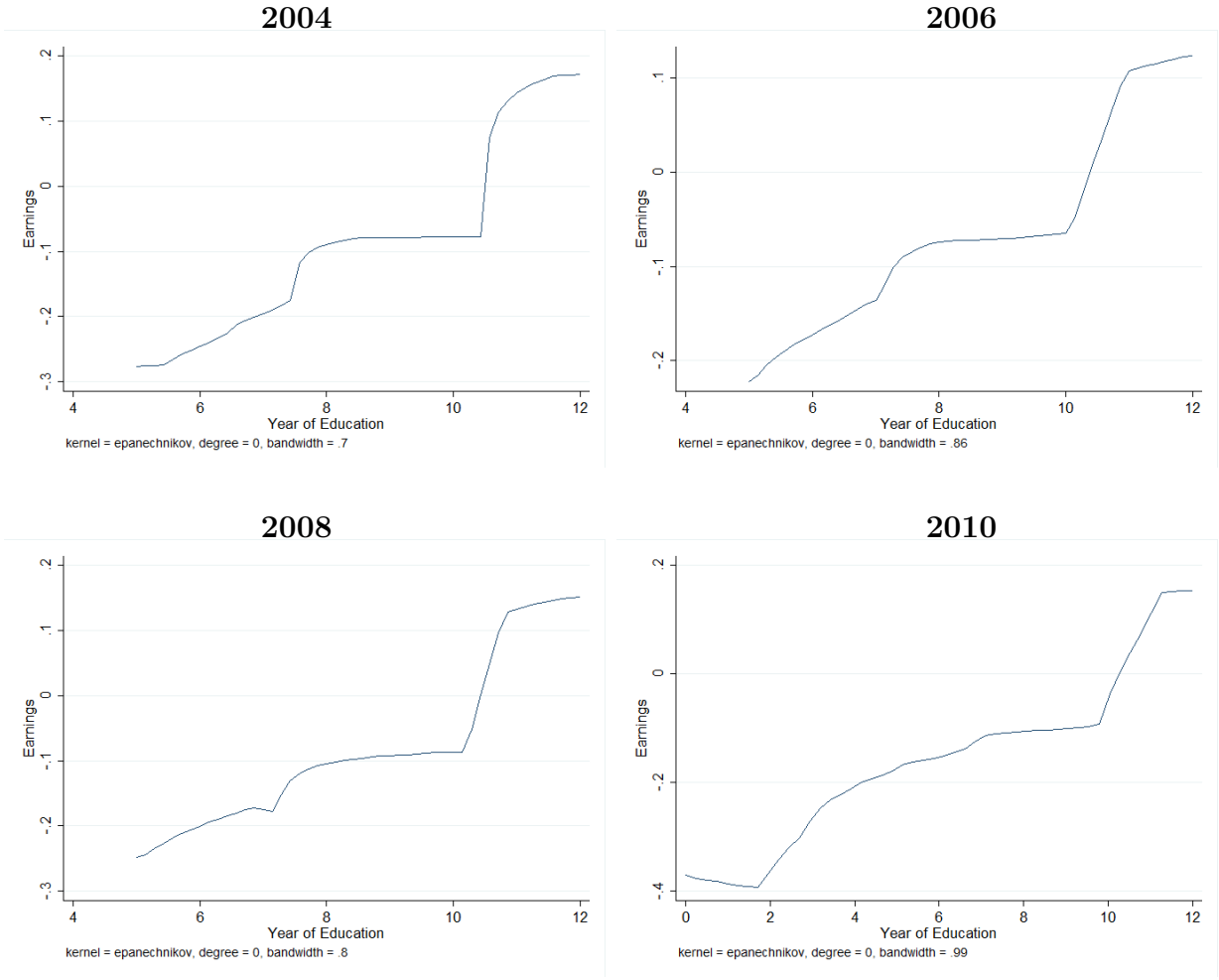
To allow for nonlinearity in education returns, without making any a priori assumption on functional forms, I implement a new two-stage strategy derived from Ichino and Winter-Ebmer (2004). First, I extract earnings residuals (in logarithm) from the regression of earnings on a third-order polynomial of age and a constant. The residuals are then assumed to contain only earnings variations due to omitted variables, most importantly, the years of education. In the second stage, I examine an education-earnings relation by a local polynomial. The result is shown in Figure 4.

These graphs show that earnings increase 40-50% or more for obtaining the full 12 years of education, equivalent to an annual rate of return of slightly below 5%, in strong agreement with the Mincerian estimates based on a linear return approach in Table 3. However, the earnings patterns associated with each level of education are very heterogeneous. A strong nonlinear education-earnings relation was consistently observed: returns on education rise faster for primary schools up to the 6th or 7th grade of lower secondary school, then flatten out until a person enters upper secondary school. The distinct jumps between lower and upper secondary schools in all four graphs are likely the "sheepskin effect" of obtaining a high-school diploma: earnings do not change much whether it takes either 11 or 12 years. The "sheepskin effect" is also observed, though a lot less obvious, for a transition from primary to lower secondary school, after which the person will receive a certificate of lower secondary school completion. There is no certificate issued for the completion of compulsory primary education.

These earnings patterns seem to agree with parametric estimates by Moock et al. (2003) using the first round of the VHLSS survey (1992-93). They reported the highest return from primary education, reaching 18.5% relative to no education, and a lower return for secondary school (in Table 6, Moock et al., 2003). In Figure 4, these observations correspond to a steeper earnings slope at a lower schooling level, followed by flatter segments at a higher level of educational attainment. However, their study does not separate lower secondary education from upper secondary education, therefore missing the discrete jumps that are possibly the "sheepskin effect" of the degrees received.

These findings are consistent with students learning the most basic skills relevant for future employment, such as writing and reading in primary education. Students start to become familiar with sciences, literature, and history in the lower secondary school, yet these subjects only add marginally to future earnings. Students in the last year of lower secondary school spend a disproportionate amount of time taking exams, which would then allow them to obtain

Figure 4
Nonlinear Returns to Education and Sheepskin Effects



Note: The dependent variable is the residuals of a regression of logarithm of yearly wage on a polynomial of third degree of age and a constant.

a certificate of completion and apply to upper secondary schools. The workloads in upper secondary schools are exceptionally heavy in Vietnam. In the last year of upper secondary school, students have to take a passing exam, which normally consists of three or four up to six subjects, in order to obtain a baccalauréat (or high-school diploma). The examined subjects are recycled from year to year in a predictable pattern. Except mathematics and literature, which are compulsory, the remaining subjects can be physics, chemistry, history, geography, biology, and foreign languages. For wage earners, having a lower secondary or upper secondary education degree is a decisive factor in employability and future earnings, especially for administrative jobs, including almost exclusively public servants.

Having established a nonlinear return pattern on education, in the following part, I examine the effect of the educational reform in the 1980s on future earnings. The reform added a year of schooling through 9th grade without much change in the substance of the academic program,

at least until many years later. The instrumental variables approach is a powerful tool for examining the impact of the reform on the affected cohorts.

4.4 Instrumental Variables Estimation of Education Returns

4.4.1. Choice of Instruments

A natural instrument when exogenous variations in education attainments were caused by institutional changes would be a dummy variable for the policy change, the educational reform status. Because the educational reform in Vietnam took several steps during the transition from the K-10 to K-12 system and the abolition of a preschool program, people born in the gap between these two systems are shown to have a lower average education attainment. Therefore, the first instrument is a dummy variable D_1, which takes the value of one for people born between 1972 and 1978, and zero otherwise:

$$D_1 = \begin{cases} 1 & \text{if} \quad 1972 \leq \text{year of birth} \leq 1978 \\ 0 & \text{otherwise} \end{cases}$$

which means, using causal-effect estimation language, the treatment is the cohort born in the gap between the two systems, who would have obtained a high-school diploma without 9th grade. The control is other cohorts, people born prior to 1972 or post 1978.

To improve the reliability of the estimation, I extend the controls to include data from another half of the country, the former South Vietnam, separated by the 17th parallel demarcation line. Thus, the second instrument for education attainment is a dummy for people born in the North during the gap years, 1972-78. The controls are all other cohorts, including all people born in the South. Let P be the indicator of the 17th parallel line, which separates the country into the reform-affected location in the North and the reform-free South:

$$P = \begin{cases} 1 & \text{for} \quad \text{Provinces north of the 17th parallel line} \\ 0 & \text{otherwise} \end{cases}$$

then the second instrument is constructed as:

$$D_2 = D_1 \times P$$

There are several reasons why this strategy might improve the sample significantly. First, in terms of school length, the education reform did not apply in the South. Second, all other characteristics that might influence earnings are expected to be similar in either region post war. Effectively, the estimation relies not only on a single population from the North and comparisons between different age cohorts but also on people from a different part of the country who might otherwise be the same, except not affected by the reform. This follows Ichino and Winter-Ebmer (2004), who use the interactions between country dummies and birth

cohort status to instrument for education losses caused by World War II in the axis countries. The 17th parallel line also has an interesting application in Miguel and Roland (2005), who find a negligible long-term impact of American bombings on economic growth in Vietnam. The distance to the demarcation line was used as an instrument for bombing intensity during the Vietnam War. They reason that the bombing intensity increased near the demarcation line, especially at Quang Tri province, where fighting was most heavy; meanwhile, the distance to the demarcation line was not expected to correlate with other economic variables in post-war Vietnam. In this study, the 17th parallel line divides the country into two halves with almost identical economic conditions, especially the provinces bordering the demarcation line, but are to two different education systems.

To interpret the IV estimate as the local average treatment effects of education attainment on earnings, I quickly go over several crucial assumptions described in Angrist et al. (1996), i.e.: (1) the stable unit treatment value assumption, (2) the random treatment assignment, (3) the exclusion restriction, (4) the nonzero average causal effect of assignment on treatment, and (5) the monotonicity assumption.

Some assumptions are satisfied trivially, but others required careful explanations. The first assumption, that one individual obtaining more education does not affect the potential earnings of others, would hold without considering general equilibrium effects. For people who have obtained academic degrees, the potential earnings might not be affected by how many years they spent to achieve it, as shown by a strong sheepskin effect. It might affect people who had not earned a degree, but perhaps not strongly. Thus, it is safe to assume that the first assumption holds. The second assumption is satisfied by default because years of birth are random, and the people were born before the reform was considered and administered. The third assumption, exclusion restriction, is also expected to hold because cohorts of birth or age should not affect earnings after controlling for the years of relevant experience. This assumption might be violated if job opportunities were limited during a certain period. For example, those born earlier graduated during the initial phase of market reforms, whereas younger cohorts entered the job market when reforms were more mature. It is impossible to tell which cohorts might benefit more: there might be more better paying jobs later, but more competition then would drive earnings down. People can also switch jobs when better opportunities are available; therefore, it is reasonable to assume that cohorts of birth do not affect earnings. The cohorts of birth may only affect earnings through possible impacts on education attainments, which is the fourth assumption. This has been shown extensively in the earlier sections. The fifth assumption implies that a person who went through the K-10 system would not get more education than he or she would have in the K-12 system, regardless of whether he or she completed all three educational levels (primary, lower, and upper secondary education) and received a high-school diploma or dropped out of school. This assumption should hold naturally.

Using complier - noncomplier terminology, the IV-LATE approach estimates the returns

on education of compliers - people who would get more schooling if required to take 9th grade. Non-compliers include only early dropouts (never takers), who never passed 9th grade. There are no always-takers because people born before the educational reform were in the K-10 education system without 9th grade. Defiers are ruled out by the monotonicity assumption. Because the average schooling was high, approximately 9.8 years (less one year in preschool for people born prior to 1981), lower for older cohorts, and higher for younger cohorts of birth, the educational reform may have likely involved a large number of compliers (Table 2 and Figure 3). Thus, the IV-LATE estimates of the returns on education may be close to the population's average return on the additional schooling year, which is 9th grade, in a population with slightly more than nine years of education on average. This means that the IV-LATE estimates likely carry strong external validity.

4.4.2. Two-Stage Least Squares Model with Instrumental Variables

In the first stage, I regress the endogenous variable, schooling attainment, on each of the two instruments separately and on other controls:

$$\text{Schooling}_\text{i} = \alpha_0 + \alpha_1 \text{D} + \alpha_2 \text{Exp}_\text{i} + \alpha_3 \text{Exp}_\text{i}^2 + \text{X}_\text{i}\alpha_4 + \eta_\text{i} \tag{3}$$

in which D is one of the instrumental variables, D_1 or D_2, described in part **4.4.1**. X_i is a vector of all other control variables, and η_i is the residual.

In the second stage, the predicted value of schooling attainment is used as a regressor in a Mincerian-type regression of the education-earnings function:

$$\log\text{Y}_\text{i} = \log\text{Y}_0 + \beta_1 \times \widehat{\text{Schooling}}_\text{i} + \beta_2 \text{Exp}_\text{i} + \beta_3 \text{Exp}_\text{i}^2 + \text{X}_\text{i}\beta_4 + \varepsilon_\text{i} \tag{4}$$

4.4.3. Empirical Estimations

The result of the first stage is shown in Table 4, and the second stage in Table 5, for each survey year separately. Column [1] corresponds to estimates of the North with D_1 as the instrument. Column [2] corresponds to the full country data with D_2 as the instrument.

Restricted to the north of the 17th parallel line, in three out of four surveys, 2006, 2008, and 2010, there is a strong impact of the education reform on schooling in the first-stage regression (Table 4, column [1]). The reform appeared to leave a lower level of schooling attainment by about .35 to .75 years for the affected cohort, people born in between 1972 and 1978, in strong agreement with the graphical evidence in section 4.1. The estimate of the 2004 survey still has a correct negative sign, but is not statistically significant.

When full country data are used, a fixed effect dummy was included to control for any North-South difference. In column [2], all estimated instruments are negative and statistically significant, except the 2004 survey. The location fixed effects indicates about a one-year difference in education attainments between the region north of the 17th parallel and the region south of it. Despite having a theoretically longer schooling system in the South, schooling was

and remains a low priority. Schooling attainment was about one year less in the South relative to the North, which reflects different attitudes toward education. Note that the instrument is the interaction between two dummies, being in the North and born during the gap years. Therefore, the estimated instrument is the average difference in schooling attainment between those born in the North during the gap years and the rest of the country.

The second-stage results are sharply different from the Mincerian estimates in Table 3. Using the first instrument with data from the North only, most schooling returns coefficients are negative, although only the estimates of 2006 and 2008 are significant (Table 5, column [1]). In line with the local average treatment effects interpretation of the IV coefficient, it seems that those who were forced to take the 9th grade might experience zero or even negative returns on the additional schooling. Expanding the dataset to incorporate more controls does not change this result. In column [2], the returns on education are either zero or negative.

Table 4: Impact of Educational Reform on Years of Schooling (First-Stage)
The dependent variable is the years of schooling.

Survey →		**2004**		**2006**		**2008**		**2010**†
Variables ↓	**[1]**	**[2]**	**[1]**	**[2]**	**[1]**	**[2]**	**[1]**	**[2]**
INSTRUMENTS								
Cohort 1972-78	-.2085		-.3511***		-.5836***		-.7550***	
SE	(.1847)		(.1305)		(.1908)		(.1916)	
Cohort 1972-78 interacts North of the 17th Parallel		-.1566		-.3085**		-.5393***		-.7288***
SE		(.1845)		(.1279)		(.1904)		(.2041)
CONTROLS								
North of the 17th Parallel		.9577***		1.0415***		1.1074***		1.9869***
SE		(.1372)		(.1482)		(.1366)		(.1950)
Experience	.0177	-.0117	-.0145	-.0397**	-.0434**	-.0708***	-.0883**	-.1135***
SE	(.0206)	(.0182)	(.0249)	(.0195)	(.0170)	(.0141)	(.0403)	(.0315)
Experience-squared	.0002	.0018***	.0008	.0019***	.0017**	.0029***	.0008	.0010***
SE	(.0007)	(.0007)	(.0008)	(.0007)	(.0007)	(.0006)	(.0005)	(.0004)
Constant	9.6951***	8.7773***	10.0045***	9.0157***	10.2035***	9.1555***	11.9343***	10.4926***
SE	(.1095)	(.1397)	(.1180)	(.1500)	(.0877)	(.1149)	(.6926)	(.5465)
Observations	2,074	4,532	2,191	4,777	2,261	4,838	2,307	5,671

Note:
[1] The sample includes only provinces north of the 17th parallel line. The instrument is D_1, cohort 1972-78.
[2] The sample contains full country data. The instrument is D_2, cohort 1972-78 interacts with being born in the North of the 17th parallel line.
*, *, and *** indicates coefficients statistically significant at the 90, 95, and 99 percent confidence level. Robust provincial clustered standard errors are reported in bracket.
The dataset include working people born in and before 85, 87, 89 and 91, and less than 70 of age at the time of survey, corresponding to 2004, 06, 08, 10 survey year.
† Age variable is used in place of years of experience for the 2010 analysis.

Table 5: IV-LATE Estimates of Returns on Schooling (Second-Stage)
The dependent variable is the logarithm of yearly wage.

Survey →		**2004**		**2006**		**2008**		**2010**†
Variables	[1]	[2]	[1]	[2]	[1]	[2]	[1]‡	[2]
↓								
Schooling	-.7060	-.8664	-.5099**	-.5531**	-.2148**	-.2344**	.0577	.0107
SE	(.6909)	(1.1006)	(.2198)	(.2638)	(.0939)	(.1060)	(.0540)	(.0435)
Experience	.0723***	.0425**	.0476**	.0299	.0384***	.0354***	.1127***	.0932***
SE	(.0250)	(.0209)	(.0201)	(.0203)	(.0132)	(.0114)	(.0152)	(.0106)
Experience-squared	-.0008	.0007	-.0004	.0002	-.0004	-.0001	-.0015***	-.0012***
SE	(.0006)	(.0020)	(.0007)	(.0008)	(.0005)	(.0005)	(.0002)	(.0001)
North of the 17th Parallel		.6093		.3851		.1120		.0139
SE		(1.0847)		(.2958)		(.1627)		(.0973)
Constant	15.1916**	16.2143*	13.7528***	13.8572***	11.2378***	11.3151***	7.2913***	8.0566***
SE	(6.6863)	(9.6455)	(2.2251)	(2.4028)	(.9695)	(.9739)	(.7874)	(.5406)
Observations	2,074	4,532	2,191	4,777	2,261	4,838	2,307	5,671

Note:
[**1**] The sample includes only provinces north of the 17th parallel line; [**2**] The sample contains full country data.
*, *, and *** indicates coefficients statistically significant at the 90, 95, and 99 percent confidence level. Robust provincial clustered standard errors are reported in bracket.
The dataset include working people born in and before 85, 87, 89 and 91, and less than 70 of age at the time of survey.
† Age variable is used in place of years of experience for the 2010 analysis.
‡ The point estimates are essentially the same as the corresponding result in Table 3, but with larger standard errors. This is due to the IV, based on the cohort of birth, is correlated to one of the explanatory variables - the years of experience for the 2010 sample.

4.5 Robust Checks

To test the robustness of the IV approach against possible self-selection issues, I restrict the sample to 4 degrees north and south of the 17th parallel line, which covers almost entirely the center region of the country, excluding the two main economic engines - Hanoi, the capital, and Ho Chi Minh City and neighboring provinces (Figure 1). The immediate neighborhood around the demilitarized zone is expected to be similar in all characteristics, but were exposed to different education systems. The robust check estimates are presented in Tables 6 and 7. The returns on the additional schooling are all zero, in perfect agreement with the analysis using the full sample in Tables 4 and 5. This result eliminates a concern that the two main economic hubs - Hanoi and Ho Chi Minh City - may somehow influence earnings and the supply and demand of education.

Another potential problem raised by Heckman et al. (1995) is that selective migration might affect earnings, i.e., being born and educated in one region but moving to work in another region due to higher earnings. This concern is likely minor and unlikely to affect the overall result. The separation between the North and the South of Vietnam is deep in cultural roots, which constrains the ability to migrate freely. Migration is essentially unidirectional - from the North to the South, where the economic climate is more open. Therefore, attenuation bias due to

selective migration is limited and unlikely affects the analysis of the Northern region.

I pay particular attention to the weak instrument problem. A potentially serious problem arises if the instrument, the cohorts of birth, is correlated with the endogenous variable, schooling attainments, but only weakly so, despite the graphical and statistical evidence of the first stage in Tables 4 and 6 is strong at the 99 or 95% confidence level. It is well known that using a weak instrument may result in a wrong point estimate, wrong estimated confidence interval, and biases in the same direction as suffered by the least squares method, even with a large sample (Bound et al., 1995; Stock et al., 2002). If the instruments explain only small variations in the endogenous variables, then the second-stage standard errors might be "blown up", resulting in a false conclusion that the effect of schooling is insignificant. If this were the case, the zero returns on education could have been an artifact of the weak instrument problem.

The first test is a weak instrument test, an F-test on the instruments described in Stock and Yogo (2002), which is based on the minimum eigenvalue of the Cragg-Donald G_T statistics. I report two F-statistics corresponding to *iid* standard errors and clustered robust standard errors, which is the Cragg-Donald Wald F-statistic for the case of a single instrument and the Kleibergen-Kaap Wald rk F-statistic, respectively. The critical values are taken from Stock and Yogo (2002) for the maximal IV size of 10, 15, 20, and 25%. The null hypotheses of a weak instrument were comfortably rejected at the 10-15% in the *iid* cases, and less so for the test using clustered robust standard errors as expected, in three out of the four surveys, 2006, 2008, 2010. For those preferring "the rule of thumb" suggested by Staiger and Stock (1997), an F-statistic of almost 10 still gives reasonable confidence that the instrument does not suffer from a weak identification issue. The only exception is the 2004 data, with a clear weak instrument problem. A second test is the under-identification test, which is a Kleibergen-Kaap rk LM test of the matrix rank having a full rank. In addition, I also provide an Angrist-Pischke statistic for the under-identification test. The null hypothesis of the under-identification test is that the endogenous variable is under-identified, implying that the excluded instrument is uncorrelated with the endogenous variable. The nulls were also rejected with high confidence in all samples except the 2004 survey. All test statistics are provided in Table 8.

5 Why Zero Returns to 9th Grade in Vietnam?

The instrumental variables estimates in Tables 4 to 7 are strikingly different from the least squares estimates in Table 3 and results from other studies in Vietnam. At the same time, the traditional Mincerian estimates in Table 3 are consistent with most other studies with the estimated rates of returns between 5 and 10%. The IV estimates are also in contrast with many studies using the same approach in other countries. In Card (1999), the IV estimates are often higher than their OLS counterparts by approximately 20-40%. Only in a few cases are the IV estimates reported to be low or very low, such as Pischke and von Wachter (2005) and

Devereux and Hart (2008). So why have people with 9th grade education not earned more in Vietnam?

A quantitative reasoning follows Card (1999), that the instrumental variables estimates are the local average treatment effects of the education reform on returns for the affected subpopulation. In this study, the IV-LATE estimate of the rate of return is significantly lower, or even sub-zero, than the population average rate. Unlike most other education policies carried out in Europe or the US that targeted the poor, supposedly low school achievers, the subpopulation affected by the reform in Vietnam had already achieved a relatively high education level, a minimum of 8th grade, prior to the introduction of the 9th grade in the new education system. Therefore, their marginal return on the extra schooling time is significantly small, or even nonexistent. This additional schooling time lies in the flat segment of the education-earnings curves in Figure 4, which starts from grades 6-7 and finishes at grades 10-11.

On the substantive issue, why is the marginal return on 9th grade low or zero? Comparable results obtained in Germany point to many possible reasons, including a rigid wage structure and the presence of the German apprenticeship training system. However, a primary reason preferred by Pischke and von Wachter (2005) is that German students learn most relevant skills prior to reaching 9th grade. Kamhofer and Schmitz (2013) confirm the negligible impacts of 9th grade on cognitive ability and therefore labor productivity or future earnings. There might be some similarities between Vietnam in transition and other developed countries in Europe during the 1960s, which means that the cause of low or zero returns on 9th grade in Vietnam may be related to learned skills. In Vietnam, political rulers were averse to changes out of fear of social disruption and the loss of influence. As a result, the reform took many years from inception in 1979 to completion in 1996. Consequently, the affected cohorts did not experience any substantive changes in the curriculum, except the time spent in school. Furthermore, a lack of a transparent scheme to evaluate learned skills puts the value of schooling squarely on the degree, the "sheepskin effect" of obtaining an academic degree, independent of the time spent in school.

Due to a high average schooling level of approximately 9 years in all surveys, it is likely that the education reform affected a large population rather than a small group of high achievers. This means that if the return on 9th grade is indeed zero or negative, then the education reform might have been counterproductive and imposed large financial losses on the population. To be clear, this does not necessarily imply that the reform was a total failure. More schooling might have had other non-monetary benefits, such as reduced crime and improved health and longevity. Furthermore, leaving school one year later might reduce labor supply, which then raises wages. A full general equilibrium effect of the reform is outside the scope of this study.

In addition, there are also other important reasons specific to the situation in Vietnam, though these remain as qualitative judgments at this stage. First, the temptation to obtain an academic degree to preempt the job market has become endemic. Some might be able to obtain

some sort of certificates by means of buying fake degrees on the black market or ghost studying. Although this phenomenon is widely known, it remains difficult, if not impossible, to quantify for an obvious reason - there would be no reliable data, or data at all for that matter. The second issue, the disconnect between skills built in schools and future labor productivity is also widely known. Many employers have to devote significant resources to train new employees, especially fresh graduates. Third, the presence of wage grids, while relaxed somewhat with more economic reforms recently, still remains in some sectors, including most state-owned enterprises. The prevalence of seniority-based and political affiliation-based, rather than performance-based, compensation schemes may discourage actual learning. These issues may likely complicate any possible education-earnings linkage.

6 Conclusions and Policy Implications

This study presents a new finding about the return on schooling in Vietnam. I find that returns on education exhibit a strong nonlinear pattern: high returns were observed for primary education, supposedly tied to real skills imperative to labor productivity in the future, followed by upper and lower secondary education. Most significantly, the "sheepskin effect", the value of having an academic degree irrespective of the number of years spent in school, is particularly strong. To complement many existing studies suggesting that the annual rate of return on schooling is in the 5-10% range in Vietnam, and higher with more recent data, I show that these estimates are the average return, consistent with a "regression to the mean" using a traditional least squares method, without accounting for nonlinearity in marginal returns associated with each school level.

Using the education reform in the 1980s, which introduced the 9th grade to complete the transition from the communist K-10 to Western K-12 education system, as the instrument for educational attainments, I find a zero or even negative returns on the extra schooling imposed by the reform. This result is consistent with the local average treatment effects (LATE) estimate of the returns on education of a subpopulation affected by the reform, who had already obtained a relatively high schooling level at the time of the reform. Among the leading causes, a gradualism approach preferred by Vietnam authority failed to bring materialistic changes to the new education system, and the lack of an effective mechanism to signal actual skills and labor productivity in the market may have promoted ineffective schooling and abuses of academic degrees.

The estimated zero or negative returns to 9th grade is a reason to call for further education policy adjustments. As the Vietnamese government is contemplating another round of education reform, the first in the 21st century, the workload could be slated for cutbacks and could be achieved by more integration between subjects even with a shorter schooling time. The current 9th grade is overloaded and consumes too many resources. Students spend almost all available

time preparing for exams - an exam for every course taken in 9th grade, a completion exam at the end of the academic year to receive a lower secondary school certificate (this was already abolished in 2006), and an entrance exam to upper secondary school - instead of learning more market-relevant skills. The elimination of the completion exam of upper secondary school may be a next logical step. More economic reforms are also needed to promote learning actual skills and removing the downside of the "sheepskin effect".

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Table 6: Validation Sample
Impact of Educational Reform on Years of Schooling (First-Stage)
The dependent variable is the years of schooling.

Survey →		**2004**		**2006**		**2008**		**2010**†
Variables ↓	**[1]**	**[2]**	**[1]**	**[2]**	**[1]**	**[2]**	**[1]**	**[2]**
INSTRUMENTS								
Cohort 1972-78	-.7898*		-.5661		-.8061**		-.4992*	
SE	(.4039)		(.3695)		(.3404)		(.2592)	
Cohort 1972-78 interacts		-.7648**		-.4733		-.6121*		-.6670*
North of the 17th Parallel								
SE		(.3796)		(.3446)		(.3337)		(.3768)
CONTROLS								
North of the 17th Parallel		.6831***		.7629***		.7990***		1.1367***
SE		(.2261)		(.1855)		(.1156)		(.2255)
Experience	-.0016	-.0489	.0224	-.1012***	-.0224	-.1174***	-.1236	-.0905
SE	(.0285)	(.0369)	(.0756)	(.0370)	(.0318)	(.0412)	(.1027)	(.0556)
Experience-squared	.0012	.0031**	-.0000	.0044***	.0004	.0044**	.0015	.0007
SE	(.0012)	(.0014)	(.0027)	(.0012)	(.0012)	(.0018)	(.0014)	(.0007)
Constant	9.8624***	9.2981***	9.8035***	9.4620***	10.2338***	9.6816***	12.1069***	10.8764***
SE	(.1572)	(.1885)	(.2638)	(.2242)	(.1617)	(.1426)	(1.6425)	(.8838)
Observations	265	887	293	954	249	899	337	1,105

Note:
The validation sample contains only provinces in the mid-central Vietnam, including Thanh Hoa, Nghe An, Ha Tinh, Quang Binh to the north, and Quang Tri, Thue Thien-Hue, Da Nang, Quang Nam, Quang Ngai, Binh Dinh, Phu Yen, and Khanh Hoa to the south of the 17th parallel. Highland provinces Kon Tum and Gia Lai are not included.
[**1**] The sample includes only northern provinces, with instrument D_1; [**2**] The sample includes both northern and southern provinces, with instrument D_2.
*, *, and *** indicates coefficients statistically significant at the 90, 95, and 99 percent confidence level. Robust provincial clustered standard errors are reported in bracket.
†Age variable is used in place of years of experience for the 2010 analysis.

Table 7: Validation Sample
IV-LATE Estimates of Returns on Schooling (Second-Stage)
The dependent variable is the logarithm of yearly wage.

Survey →		**2004**		**2006**		**2008**		**2010**†
Variables	[1]	[2]	[1]	[2]	[1]	[2]	[1]	[2]
↓								
Schooling	-.0407	-.0539	-.3803	-.4303	-.1002	-.2184	.0462	.0272
SE	(.0781)	(.0861)	(.2324)	(.3164)	(.1245)	(.1754)	(.2365)	(.1519)
Experience	.0538*	.0524***	.0743***	.0105	.0932***	.0424	.0986*	.0980***
SE	(.0297)	(.0125)	(.0179)	(.0439)	(.0231)	(.0368)	(.0542)	(.0244)
Experience-squared	-.0008	-.0007	-.0010	.0010	-.0023***	-.0003	-.0012*	-.0012***
SE	(.0010)	(.0005)	(.0009)	(.0018)	(.0007)	(.0014)	(.0007)	(.0002)
North of the 17th Parallel		-.0994		.0477		-.0803		-.1774
SE		(.0834)		(.2569)		(.1590)		(.1970)
Constant	8.7574***	8.9785***	12.1467***	12.8452***	9.6930***	11.1167***	7.3612**	7.6800***
SE	(.7573)	(.8009)	(2.3323)	(3.0321)	(1.3565)	(1.7353)	(3.3199)	(1.8073)
Observations	265	887	293	954	249	899	337	1,105

Note:
[**1**] The sample includes only selected northern provinces; [**2**] The sample includes all validation data.
*, *, and *** indicates coefficients statistically significant at the 90, 95, and 99 percent confidence level. Robust provincial clustered standard errors are reported in bracket.
†Age variable is used in place of years of experience for the 2010 analysis.

Table 8: Weak Instruments and Under-identification Tests

Survey →		**2004**		**2006**		**2008**		**2010**
	[1]	[2]	[1]	[2]	[1]	[2]	[1]	[2]
Weak Instrument								
Cragg-Donald Wald F statistic	2.99	1.36	8.88	5.25	24.61	16.64	27.20	18.77
Kleibergen-Paap Wald rk F statistic	1.23	.71	7.00	5.73	9.05	7.90	15.00	12.56
Under-Identification								
Kleibergen-Paap rk LM statistic χ^2(1)	1.22	.70	5.61	4.55	6.22	5.47	11.74	10.62
p-value	.2698	.4033	.0179	.0329	.0126	.0193	.0006	.0011
Angrist-Pischke								
Under-identification χ^2(1)	1.28	0.72	7.25	5.83	9.37	8.03	15.56	12.77
p-value	.2585	.3956	.0071	.0158	.0022	.0046	.0001	.0004

Note:
[**1**] The sample includes all provinces north of the 17th parallel line; [**2**] The sample includes full country data.
Stock and Yogo (2005) critical values of the maximal IV size corresponding to 10, 15, 20, and 25% size for models of one endogenous variable and one instrument are 16.38, 8.96, 6.66, and 5.53, respectively.