YOKOHAMA NATIONAL UNIVERSITY

DOCTORAL THESIS

Four Essays in Labor Economics and Microeconometrics: Effects of Educational, Marital, and Fertility Decisions on Labor Market Outcomes

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"Why in almost all societies have married women specialized in bearing and rearing children and in certain agricultural activities, whereas married men have done most of the fighting and market work?"

Gary Becker

Abstract

This PhD dissertation discusses four important topics in labor economics and microeconometrics. In particular, I am concerned with four issues: (1) marital decision and female labor supply (2) fertility and female labor supply (3) effects of compulsory schooling law on educational outcomes (4) effects of education on labor market outcomes in the context of compulsory schooling reform.

The first chapter exploits exogenous variations in marriage caused by the Chinese Zodiac to estimate the causal effect of marital status on female labor force participation. Although the causality between marital status and labor supply is critical for a number of theoretical and practical reasons, the answer is not yet clear-cut because of the endogeneity of marriage. On the one hand, marriage could give a determinant effect on the female labor force participation. On the other hand, unobservable characteristics, such as women's preferences for marriage and their expectations of careers, could also induce a non-marriage status. Despite the rapid accumulation of literature on fertility and female labor supply, few has taken account for marriage effect on labor market outcomes. In this chapter, I use a Chinese superstition as a natural experiment to estimate the causal effect of marriage on female labor supply. In China, females born in Goat years (in lunar calendar) are always labeled as "widowed when young", and they are unpopular in the marriage market. Using this exogenous variation in marital status, I show that marriage has large effects for both rural and urban females, decreases the probability of female labor force participation by 46.4 percentage points and 30.7 percentage points, respectively.

The second chapter focuses on an important issue in aging societies that whether fertility affect female labor supply, and whether the fertility effect changes over time. Although the causal link between fertility and labor supply among married women is important, the main difficulty in determining the causality is that fertility decisions and labor market participation are simultaneously determined. Two potential endogeneity issues in this context are as follows. First, unobserved factors such as preferences regarding the number of children and career expectations are heterogeneous across individuals. Second, the amount of time passed since the last childbirth is important for the subsequent fertility decision, and the question of whether to have an additional child

also affects the labor market activities of married women. Following Angrist and Evans (1998), several studies have attempted to correct for such bias using mixed sibling-sex composition as an instrumental variable (IV). Using micro-data from the Taiwan Population and Housing Census, I exploit exogenous variations in the number of children caused by twin births to estimate the effect of fertility on female labor supply, conditioned on the time elapsed since the last childbirth. The instrumental variable estimates indicate that an additional child reduces female employment by 10.5 percentage points for those who have at least one delivery, and the effects gradually decline for females who have two or more deliveries, with the effects vanishing when females have three or more deliveries. This suggests that the effect of fertility on female labor supply is not monotonically decreasing in the number of births, which has rich policy implication in fertility promotion in aging societies.

The third chapter executes a policy evaluation of the compulsory schooling reform in Taiwan in 1968, which would be used as a natural experiment to investigate its effect on labor market outcomes later in the fourth chapter. In this chapter, to understand the policy effect of compulsory schooling law, I use a sharp regression discontinuity design to estimating the effect of raising mandatory years of schooling from 6 to 9 on students' educational outcomes. As the policy was implemented on September 1st in 1968, students who were still in their sixth grade of primary school had to go to junior high schools, while those who had graduated from primary schools were not required to do so. In other words, the policy compels students born on or after September 1st in 1955 to attend school longer than students born before the eligibility cutoff. The dataset used in this study (Population and Housing Census of Taiwan) records each student's exact date of birth, along with the huge sample of universal Taiwanese population, allows me to perform a very precise regression discontinuity estimates. Furthermore, I estimate the model by ethnic and gender groups to examine the heterogeneity. As a result, I have found an overall effect of 9-year compulsory schooling by 0.22 years for the first eligible cohort, and the effects are larger for males and local Taiwanese people, compared to females and Mainlanders. Compared to previous studies in UK and France(Oreopoulos 2006; Grenet 2013), the estimated policy effects of compulsory schooling in Taiwan are very small.

The fourth chapter uses the compulsory schooling reform, which has been discussed in the last chapter, to instrument years of schooling and then estimates the economic return to education. Education has a crucial implication in human society, as it has long been of interest to policymakers, economists, and individuals themselves. Since Angrist and Krueger (1991), economists have devoted a great deal of attention to correcting the omitted variable (ability) bias by instrumental variable (IV) method or fuzzy regression discontinuity design. Card (2001) offers a survey of IV literature on returns to education, the IV estimates of returns to education are typically larger than the corresponding OLS estimates. Using a Taiwanese survey data, I estimate the causal effect of schooling on earnings by a fuzzy regression discontinuity design. After controlling for demographic characteristics and tenure, the return to education is estimated to be 4.7% from IV, smaller than the corresponding OLS estimate. While the OLS estimate is comparable to the literature, the IV estimate only suggests very moderate returns to education. In Taiwan, education system is very exam-oriented, and parents would sacrifice own expenditures to support children's schooling altruistically due to traditional values of family. Marginal returns to education among those who acquired additional schooling as a result of the compulsory schooling law are not very high, the low ability, rather than the marginal costs of schooling may limit their return to education.

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List of Abbreviations

LFP	**L**abor **F**orce **P**articipation
TFR	**T**otal **F**ertility **R**ate
OLS	**O**rdinary **L**east **S**quares
IV	**I**nstrumental **V**ariable
RDD	**R**egression **D**iscontinuity **D**esign

Chapter 1

Identifying the Causal Effect of Marriage on Women's Labor Force Participation in the Presence of Chinese Superstition

1.1 Introduction

An understanding of the relationship between marital status and labor force participation is critical for a number of theoretical and practical reasons. First, economists and demographers (Mincer 1962; Becker 1965; Grossbard-Shechtman 1993) have established a variety of models to link marriage and the labor market. They predict a high negative correlation between the two because married women can pay more attention to childcare and home production than men. Second, the link between marital status and labor force participation might partially explain the work-family conflict (Greenhaus and Beutell 1985; Barnett and Hyde 2001). Apparently, both men and women make complex decisions about how to spend their time within and without marriage. These decisions about time use also affect whether to marry or, once married, whether to stay married. Otherwise, marriage is regarded as one type of investment in human capital.

Although the question is important, the answer is not yet clear-cut because the causality between marital status and female labor force participation is complicated by endogenous marriage decisions. Becker (1973) argues that the observed correlations do not necessarily reflect a causal effect. On the one hand, marriage could give a determinant effect on the female labor force participation. On the other hand, unobservable characteristics, such as women's preferences for marriage and their expectations of careers, could also induce a non-marriage status. Due to the endogeneity, identifying the causal effect of marriage on labor force participation is a challenging task.

Many previous studies have investigated the female labor supply through fertility without accounting for marriage. Angrist and Evans (1998) shows that children lead to a reduction in the female labor supply by using sibling sex composition and twinning births to instrument fertility. Chun and Oh (2002) estimate the effect of fertility on labor force participation in Korea. They use the first child's gender to instrument fertility, and they find that having an additional child reduces the labor supply of Korean women by 27.5 percentage points. Both Angrist and Evans (1998) and Chun and Oh (2002) include only married women in their samples, and thus, the effect of marital status on the labor supply has not been accounted for.

To our knowledge, Van der Klaauw (1996) is the pioneering paper that jointly estimates the marriage and labor supply decision in the United States. Based on a structural model, he finds that ignoring the endogeneity of marriage could overestimate the adverse effect of marriage. However, the structural method must impose parametric assumptions on marriage and labor supply decisions, and then, the dynamic programming problems can be solved. Nevertheless, Lee (2005) shows some reduced-form evidence that marriage has a significant negative effect on female labor force participation by approximately 90 percentage points. Using South Korean data, he exploits the Asian zodiac as an instrumental variable to remove the endogeneity of marital status.

In China, the causal effect of marriage on female labor force participation remains an open question. Few papers have focused on the causality between marriage and labor market outcomes. Maurer-Fazio et al. (2011) employs the Chinese Population Census to estimate married, urban women's labor force participation decisions. They find that the presence of preschool children significantly decreases the women's labor force participation. In their earlier working paper, Maurer-Fazio et al. (2005) analyzes the changing pattern of labor force participation in the Chinese labor market. They report a positive effect of marriage by 5.9 percentage points for rural women aged 15 and older and a negative effect by 1.2 percentage points for urban women. Both of these studies use Probit models to observe the determinants of the labor force participation.

Using micro-data from the Chinese Population Census, we exploit the Chinese superstition as an instrumental variable (IV) to address the endogeneity of marriage. The "Shēngxiào", which is known in English as the Chinese zodiac, derives a similar concept in western astrology and means "circle of animals". This system follows a 12-year mathematical cycle and relates each year to an animal. The animals are Rat, Ox, Tiger, Rabbit, Dragon, Snake, Horse, Goat, Monkey, Rooster, Dog, and Pig. Each zodiac sign has some specific characteristics, e.g., intelligence for the Rat, diligence for the Ox, and so forth. In particular, the Goat year is considered to be related to marriage. There is a widespread folk belief in China that literally "ten goats and nine incomplete". Namely, nine tenths of the people who were born in the Goat year are more likely to suffer from a miserable fate. Especially for women, they are always labeled as "widowed when young" because they are considered to bring misfortune to their husbands/partners. In brief, the Goat year women are unpopular in the marriage market. Therefore, the superstition that induces women into an exogenous selection of marriage might allow us to estimate the causality of marital status on labor force participation.

The remainder of this paper is organized as follows. Section 1.2 is a description of the data sets and briefly shows the difference in the demographic characteristics between the rural and urban samples. The following section presents the identification strategy that includes the empirical models and necessary assumptions to identify the causal effect. In Section 1.4, we first discuss the validity of the instrument and the first stage results, and then, we compare the estimates between OLS and IV. Section 1.5 provides a summary.

1.2 Data

Our primary data source is the 0.95‰ sample of the 2000 Population Census that has been conducted by the National Bureau of Statistics of the People's Republic of China. These data comprises the fifth of the series and follows the previous four censuses, which were collected in 1953, 1964, 1982, and 1990. The sample covers 1,180,111 individuals from 327,890 households. The dataset provides information on a variety of household characteristics and includes variables that describe the location, size, type, and composition of the household. Furthermore, information on each individual residing in the household is reported. The individual level information contains demographic characteristics, educational attainment, occupation, ethnicity, marital status, and fertility.

To sharpen the focus of this paper, first, we limit our sample to those women who have never been married or are in their first marriage. Second, we restrict these women to 17 - 30 years old, which is when the majority of women are first married. Approximately 90% of the women are married if they are over 30. The final sample contains 100,822 never married and first married women, only 27.4% of them have household registration records (hukou[1]) in urban areas. The average age is 23.7, and approximately 90% of the rural residents are not greater than middle school education while only 28% are middle school levels or less for urban residents.

We assign individuals' Chinese zodiac signs according to the birth information reported in the census. Unfortunately for privacy concerns, only the year of birth and month of birth information are provided, which are reported by the solar calendar. However, the Chinese zodiac signs follow the lunar calendar. We have to transform the solar calendar to the lunar calendar, which makes it difficult to calculate the Chinese zodiac signs[2]. We cannot confirm a person's Chinese zodiac sign if he was born in January or February when we do not know his date of birth. As an alternative method, we delete them from the sample and only keep those who were born during March to December. Thus, the reported year of birth by the solar calendar is identical to the year in the lunar calendar.

We define the labor force participation according to the information about the job status in the census. Only those who are employed or unemployed but looking for a job are defined as participating in the labor force. Students, homemakers, retired persons, and disabled persons are not counted as labor force. In this sub-sample, for women of age 17-30, the labor force participation rate is approximately 81.1%[3], which is slightly higher than the country aggregate and developed countries.

[1] A hukou is a record in the system of household registration that is required by law in the People's Republic of China (mainland China). A household registration record officially identifies a person as a resident of an area and includes identifying information such as name, parents, spouse, and date of birth.

[2] The lunar new year differs each year in the solar calendar, and it is the boundary of the two adjacent Chinese zodiac signs. For example, people who were born in 1979 are assigned Goat zodiac sign if their birthdays are after Jan. 28th, but Horse zodiac signs if not.

[3] According to the World Bank data, the labor force participation in China was 77% in 2000, 66% in the United States, 62% in Japan, 62% in the United Kingdom. In the 0.95‰ sample of the 2000 Population Census, the labor force participation was 76.9% in China.

In Table 1.1, we report the descriptive statistics by Chinese zodiac signs and locations. Columns 1 and 2 display statistics for the rural sample by the Goat sign and other signs, which contain 4951 persons born in the Goat years and 68287 persons born in other years. Columns 3 and 4 display statistics for the urban sample by the Goat sign and other signs, which contain 1922 persons born in the Goat years and 25662 persons born in other years. Those of Goat and other zodiacs are different in age, marital status, years of schooling and labor force participation. Especially Goats have higher educational attainments compared with other zodiac years, which implies that they are not popular in the marriage market, and thus, that they choose further studies. The average age of Goats is approximately 21.3, which is younger than other zodiacs in this sample. This finding also explains partially why Goats are less likely to be married. The proportion of the Goat zodiac does not differ between races.

1.3 Identification Strategy

Marital status could be correlated with female labor force participation due to unobserved factors such as attitude toward marriage, ability and so forth. If so, the estimated coefficients by Ordinary Least Squares (OLS) will be biased. Alternatively, we employ Chinese superstition as instrumental variables (IV) to address this endogenous problem. Following the Two-Stage Least Squares (2SLS) method, the model can be written as follows:

$$LFP_i = \beta_0 + \beta_1 Married_i + \boldsymbol{X}'\beta_2 + u_i \tag{1.1}$$

where LFP_i represents the labor force participation of the female individuals, which equals 1 if the individual participates and 0 otherwise. The variable $Married_i$ is the marital status, which equals 1 if the individual is first married and 0 for never married. To simplify the analysis, we drop those women who are divorced and widowed to observe the effect of marriage. X is a vector of individual characteristics, which includes the age, age squared, education level, ethnic group, and province where the woman resides. We also estimate rural and urban subsamples separately to determine the difference between areas that are in entirely different economic conditions.

Our parameter of interest is β_1, which represents the coefficient of the marital status. Due to the endogenous problem of marital status, we use the Goat sign as the instrumental variable for the marital status to identify the causality. The first stage of the two-stage least squares (2SLS) estimation equation is given by

$$Married_i = \alpha_0 + \alpha_1 Goat_i + \boldsymbol{X}' \alpha_2 + v_i \tag{1.2}$$

In Equation (2), $Goat_i$ is a dummy variable that equals 1 if born in the Goat year and 0 otherwise. X is the same vector of control variables, which includes age, age squared, education level, ethnic group, and province in Equation (1). We exploit the Goat sign as an unusual instrument to solve the endogeneity of marriage. To check the validity of this instrumental variable, two necessary conditions should be satisfied, namely, that

$$Cov(Goat, Married) \neq 0 \tag{1.3}$$

$$Cov(Goat, u) = 0 \tag{1.4}$$

where Condition (3) means that the Goat sign should have an effect on a woman's marriage. If not, then the Goat sign will not be valid as an instrument. Condition (4) is the exclusion restriction, which implies that the Goat sign is uncorrelated with any other determinants of the labor force participation.

We can check Condition (3) by the estimated coefficients in Table 1.2. We can see that the Goat sign instrument affects marriage significantly. In other words, Condition (3) is satisfied. However, Condition (4) cannot be tested empirically, and we consider that the Chinese zodiac sign could not directly affect the error term u_i of the labor force participation in Equation (1). The reason is that those women who were born in the Goat years are only discriminated against in the marriage market, and no evidence shows that they are discriminated against in the labor force market.

To allow the marriage probability to differ with age, we also estimate an over-identified model by three instruments, which are Goat and its interaction terms with age and age squared. The continuous age variable allows for a few variations among Goat in the cross-sectional census.

1.4 Results

In this section, we discuss the regression estimates of OLS and IV, which were designed to test whether marriage has a negative effect on female labor force participation in China. We first discuss several issues regarding the validity of using the Goat sign as the IV for marriage. Then, we use the Goat sign to instrument marital status (first married or never married), and we perform estimations as specified by Equations (1) and (2). The results are shown in Table 1.2. In the following table we show the over-identified model results. It is worthwhile to emphasize that we regressed the urban and rural samples respectively. This approach allows us to examine whether the effect of marriage is different in rural versus urban areas. For all of the regressions, we control for a full set of personal characteristics that comprise age, age squared, an indicator of being a minority, educational attainments, and provincial dummies. Due to space constraints, the estimates for the provincial dummies are not reported. When estimating the OLS and IV coefficients, linear probability models are used because they are more robust than Probit-IV, and we report White-Huber standard errors due to the heteroskedasticity.

1.4.1 The First Stage

Before reporting the estimates, we first discuss the validity of using the Goat sign as IV in our paper. A good IV should be highly correlated with the marital status but should not affect female labor supply except through the marital status. In other words, a valid IV should not be correlated with unobserved characteristics that are captured by the error term u_i in Equation (1). The zodiac sign is an important source of exogenous variations in marriage for Asian countries, and Lee (2005) shows that the Horse zodiac has a negative effect on women's marriages in South Korea. The zodiac sign will be a good instrument for marriage because it is determined by the birth year, which is predetermined to an individual. Although the correlation between the Goat sign and unobserved characteristics cannot be tested by design, no previous studies have shown any evidence that the birth year is correlated with the family background.

The Goat sign affects people's marriages statistically. In other words, the IV is valid in the first stage. From columns (3) and (6) of Table 1.2, we can see that our instrument, the Goat sign, has significant negative effects on women's marital status for both the rural and urban samples. If a rural woman were born in the Goat year, she would be less likely to be married by 10.5 percentage points. However, if an urban woman were born in the Goat year, she would be less likely to be married by 13.7 percentage points. This finding implies that the Chinese male's preference in the potential wife's zodiac sign is powerful and decreases woman's probability of marriage. We show the over-identified model results in Table 1.3, the Goat effect keeps stable with the just-identified model and the over-identification test easily passed.

Age shows a different size coefficient, but it is in the same direction for both the rural and urban areas. Compared to rural women, urban women are not as anxious to get married when they turn a year older. From the first stage results from Table 1.2, we can see that turning 1 year older will increase the probability of being married by 29 percentage points in the rural sample, while it is only increased by 6.8 percentage points in the urban sample. In rural areas, getting married at an early age is very common because most of the people are engaged in agriculture. They rarely face the challenge of how to weigh the potential benefits of marriage against their careers.

In addition, education shows different effects for rural and urban women. In rural areas, women who are educated less than primary school will have higher probabilities to be married by 5.1 percentage points, which is significant. However, women who are educated less than primary school will have lower probabilities to be married in urban areas (by 1.2 percentage points, which is insignificant in terms of statistics). Going to high/technical schools and above shows a significant negative effect in both rural and urban areas, but the coefficients are larger in the rural sample. This finding implies that more education makes it difficult for women to get married when the men's educational attainments are not very high.

1.4.2 OLS and IV Estimations

Table 1.2 shows OLS estimates in columns (1) and (4) and IV estimates in columns (2) and (5) for the rural and urban samples, respectively, along with the first-stage relationship between marriage and the Goat sign. The over-identified model results in Table 1.3 keep stable with Table 1.2. In all of the regressions, provincial dummies are controlled. The 2000 Population Census covers 31 provinces except for Taiwan, Hongkong, and Macau. Due to space constraints, the estimates for provincial dummies are not reported. The OLS estimates show that marriage significantly decreases the probabilities of female labor force participation. From columns (1) and (4) in Table 2, married women have lower probabilities of participating in the labor market by 13.3 percentage points in rural areas and by 12.3 percentage points in urban areas. The difference in the marriage effects between the rural and urban areas is not very large, at the same level at least. However, the OLS estimates do not reflect the causal effect because the endogeneity of marriage is not accounted for.

Compared to the OLS estimates, the IV estimates show a larger effect of marriage. From columns (2) and (5) in Table 1.2, we can see from the IV estimation that marriage decreases the probability of participating in the labor force market by approximately 47.4 percentage points for the rural sample and 30.2 percentage points for the urban sample. The differences between the OLS estimates are 34.1 percentage points and 17.9 percentage points. The IV estimates change dramatically for the following reasons. First, in the OLS estimation, there are several potential omitted variables that will cause an OVB (Omitted Variable Bias), such as attitude toward marriage, ability, and other unobserved characteristics. The OLS estimates will be upward biased toward zero if unobserved factors have positive effects on marriage. Suppose that more capable people have higher probabilities to get married; then, the positive correlation between ability and marital status will bias the OLS results upward. Second, the IV method captures the effect of treatment on compliers. The average effect for this group is called a local average treatment effect (LATE). In our research, this effect is the effect of marriage on those of Goat, who married because they were born in the Goat year but would not get married otherwise.

In addition, the IV estimates display an enormous gap between the rural and urban areas. The IV estimate of being married in the rural sample is larger in size than in the urban sample by 17.2 percentage points. However, no such evidence has been found in the OLS results. In general, rural women have higher probabilities of performing housework after getting married, and they have more pressure to look after babies. In the urban areas, the one child policy is relatively intensive, and as a result, the number of children of ever-married women is potentially less for the rural women.

What also shocks us is that education has an entirely inverse effect on rural and urban residents. From the IV estimation, columns (2) and (5), we can see that being educated less than or equal to primary school decreases the probability of labor force participation by 14.7 percentage points for the urban sample; however, it increases the probability of labor force participation by 2.4 percentage points for the rural sample. A reasonable fact is that knowledge and skill are required while finding a job in the urban areas because the urban areas have experienced a high-speed growth in economics, which brings a large amount of technical progress. However, the rural residents do not face this situation at all and have no incentive to be highly educated.

1.4.3 Policy Implication

Women face a complicated problem of work-family conflict with respect to whether to choose a more successful career or a happier family life. On the one hand, decreasing the female labor force participation will have a negative impact on their incomes and the development of human capital. On the other hand, increasing the time spent with families will enhance their marriage-specific human capital. During 1990 to 2010, the labor force participation of women aged 15-64 in China decreased by approximately 10 percentage points. The rapid recession in the female labor supply could have adverse effects on the women and their children through the decline in human capital. We make the following proposals to increase the female labor supply in China. First, the government should improve the employment conditions for married women. A friendly environment for married women will decrease the opportunity costs of their employment in such a way that they can weigh the balance between work and family. Second, the governments should discourage women from getting married at an early age, and then, the decreasing female labor supply might be slowed down.

1.5 Concluding Remarks

In this paper, we analyze the effect of marital status on the labor force participation by utilizing a representative census dataset from China. To ind the causal link between marriage and female labor force participation, we then instrument marital status with the Chinese zodiac sign to run an IV estimation. Finally, we ind supportive evidence that marriage decreases the probability of participating in the labor market by 46.4 percentage points and 30.7 percentage points for rural and urban areas, respectively. The effects of marriage are not uniform between the rural and urban areas in terms of their size, which is not found in the OLS estimation.

TABLE 1.1: *Descriptive Statistics*

	Rural Sample		Urban Sample	
	(1) Others	(2) Goat	(3) Others	(4) Goat
LFP	0.861	0.910	0.672	0.672
	(0.346)	(0.286)	(0.470)	(0.470)
Married	0.593	0.281	0.487	0.089
	(0.491)	(0.449)	(0.500)	(0.286)
Age	23.936	21.283	23.869	21.272
	(3.972)	(0.234)	(3.966)	(0.236)
Age squared	588.704	453.000	585.481	452.540
	(186.697)	(9.973)	(186.299)	(10.038)
Minority(=1 if not *Han*)	0.105	0.104	0.070	0.075
	(0.306)	(0.306)	(0.256)	(0.264)
Education				
Primary school or less	0.334	0.260	0.024	0.018
	(0.472)	(0.439)	(0.154)	(0.134)
Middle school	0.577	0.621	0.269	0.183
	(0.494)	(0.485)	(0.444)	(0.386)
High/technical school	0.084	0.109	0.474	0.482
	(0.278)	(0.312)	(0.499)	(0.500)
Higher education	0.005	0.009	0.233	0.317
	(0.067)	(0.097)	(0.423)	(0.466)
Observations	68,287	4,951	25,662	1,922

Notes: Standard errors in parentheses. Columns (2) and (4) show descriptive statistics for females of Goat. Columns (1) and (3) show descriptive statistics among females of other 11 zodiacs.

TABLE 1.2: *Estimated Coefficients*

	Rural Sample			Urban Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	First Stage	OLS	IV	First Stage
Dependent Variables	LFP	LFP	Married	LFP	LFP	Married
Age	0.1553***	0.2450***	0.2895***	0.5243***	0.5320***	0.0678***
	(0.0047)	(0.0133)	(0.0042)	(0.0083)	(0.0091)	(0.0075)
Age squared	-0.0029***	-0.0042***	-0.0042***	-0.0098***	-0.0096***	0.0005***
	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0002)	(0.0002)
Married	-0.1326***	-0.4644***		-0.1225***	-0.3067***	
	(0.0037)	(0.0463)		(0.0065)	(0.0777)	
Goat			-0.1046***			-0.1371***
			(0.0064)			(0.0071)
Minority(=1 if not *Han*)	0.0168***	0.0189***	0.0060	-0.0171*	-0.0146	0.0140
	(0.0043)	(0.0047)	(0.0047)	(0.0103)	(0.0105)	(0.0087)
Education						
Primary school or less	0.0068**	0.0236***	0.0506***	-0.1447***	-0.1474***	-0.0117
	(0.0027)	(0.0038)	(0.0027)	(0.0212)	(0.0213)	(0.0143)
High/technical school	-0.2814***	-0.3086***	-0.0817***	0.0285***	0.0169**	-0.0623***
	(0.0060)	(0.0072)	(0.0042)	(0.0066)	(0.0082)	(0.0052)
Higher education	-0.3657***	-0.4565***	-0.2702***	-0.1111***	-0.1402***	-0.1551***
	(0.0258)	(0.0292)	(0.0179)	(0.0071)	(0.0137)	(0.0060)
Constant	-1.1598***	-2.3507***	-3.7886***	-6.0884***	-6.2942***	-1.3839***
	(0.0602)	(0.1764)	(0.0496)	(0.0978)	(0.1345)	(0.0854)
Provincial dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	73,238	73,238	73,238	27,584	27,584	27,584
R^2	0.1167	0.0257	0.6060	0.2869	0.2703	0.5706

Notes: Robust standard errors in parentheses. Provincial dummies are controlled in all regressions. The base group of educational attainment dummies is the middle school level. *** p<0.01, ** p<0.05, * p<0.1

TABLE 1.3: *Estimated Coefficients for Over-Identified Models*

	Rural Sample			Urban Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	First Stage	OLS	IV	First Stage
Dependent Variables	LFP	LFP	Married	LFP	LFP	Married
Age	0.1553***	0.2478***	0.2892***	0.5243***	0.5317***	0.0679***
	(0.0047)	(0.0132)	(0.0042)	(0.0083)	(0.0091)	(0.0075)
Age squared	-0.0029***	-0.0042***	-0.0042***	-0.0098***	-0.0096***	0.0005***
	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0002)	(0.0002)
Married	-0.1326***	-0.4749***		-0.1225***	-0.2999***	
	(0.0037)	(0.0456)		(0.0065)	(0.0773)	
Goat			-106.6989***			-54.2042***
			(21.3767)			(17.6958)
Goat*Age			9.9648***			5.1076***
			(2.0103)			(1.6661)
Goat*Age squared			-0.2329***			-0.1206***
			(0.0473)			(0.0392)
Minority(=1 if not *Han*)	0.0168***	0.0189***	0.0059	-0.0171*	-0.0147	0.0141
	(0.0043)	(0.0047)	(0.0047)	(0.0103)	(0.0105)	(0.0087)
Education						
Primary school or less	0.0068**	0.0242***	0.0505***	-0.1447***	-0.1473***	-0.0117
	(0.0027)	(0.0038)	(0.0027)	(0.0212)	(0.0213)	(0.0143)
High/technical school	-0.2814***	-0.3094***	-0.0817***	0.0285***	0.0173**	-0.0623***
	(0.0060)	(0.0071)	(0.0042)	(0.0066)	(0.0082)	(0.0052)
Higher education	-0.3657***	-0.4594***	-0.2697***	-0.1111***	-0.1391***	-0.1552***
	(0.0258)	(0.0292)	(0.0180)	(0.0071)	(0.0136)	(0.0060)
Constant	-1.1598***	-2.3882***	-3.7860***	-6.0884***	-6.2865***	-1.3858***
	(0.0602)	(0.1741)	(0.0496)	(0.0978)	(0.1342)	(0.0855)
Provincial dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	73,238	73,238	73,238	27,584	27,584	27,584
R^2	0.1167	0.0198	0.6061	0.2869	0.2715	0.5706
F of excluded instruments			158.30			83.29
Hansen J statistic			p=0.1790			p=0.3024

Notes: Robust standard errors in parentheses. Provincial dummies are controlled in all regressions. The base group of educational attainment dummies is the middle school level. *** p<0.01, ** p<0.05, * p<0.1

Chapter 2

A Dilemma of Fertility and Female Labor Supply: Identification Using Taiwanese Twins

2.1 Introduction

In the decades since family planning policy was introduced in the 1960s, the growth rate of the Taiwanese population has decreased. In 1951, each Taiwanese woman had approximately seven children over her lifetime. However, in the 21st century, Taiwan's fertility rate has become one of the lowest in the world, falling below one child per woman in 2010. The total fertility rate (TFR) of Taiwan was 1.12 in 2015, only slightly higher than Macau and Singapore[1], placing the country third from the bottom among 224 countries (Agency 2015). In addition to the development of women's human capital, infant mortality has decreased substantially (Chou et al. 2010), and fertility behavior is changing within Taiwanese society (Freedman et al. 1975; Freedman et al. 1977). The extremely low birthrates have become an unprecedentedly challenging social issue in Taiwan (Lin and Yang 2009; Chen 2012).

Corresponding to a sharp decline in the fertility rate, the labor force participation (LFP) of married women in Taiwan has experienced a high rate of growth. Figure 2.1 shows the long-term time series data of TFR and LFP for married women in Taiwan. On the one hand, the TFR had decreased to 1.165 per woman in 2014 from 2.455 per woman in 1981. On the other hand, the LFP of married women had grown to 49.76% in 2014 from

[1] Singapore 0.81, Macau 0.94, Japan 1.40, China 1.60, U.S. 1.87, UK 1.89

a rate of 33.23% in 1980. The strong negative correlation between the two leads to a policy dilemma regarding whether having more children reduces the likelihood of labor market participation among married women. For policy makers, simultaneously increasing the fertility rate and female labor supply is a difficult task, as fertility promotion policy may lead to an increase in the fertility rate but harms female labor supply (Rosenbluth et al. 2002; Dudel 2009).

Although the causality between fertility and labor supply among married women is important, the main difficulty in determining the causality is that fertility decisions and labor market participation are simultaneously determined. Two potential endogeneity issues in this context are as follows. First, unobserved factors such as preferences regarding the number of children and career expectations are heterogenous across individuals. For example, those who are ambitious at work may have a lower expected number of children while being more likely to participate in the labor market. Any estimates that fail to control for unobserved heterogeneity would be downward biased, as career ambition is negatively correlated with fertility but positively correlated with labor supply, which would overestimate the negative effect of fertility on labor supply. Following Angrist and Evans (1998), several studies have attempted to correct for such bias using mixed sibling-sex composition as an instrumental variable (IV), due to the well-known preference for sons in some Asian countries (Chun and Oh 2002; Ebenstein 2009; Azimi 2015). Following Rosenzweig and Wolpin (1980), economists also use twin births as a natural experiment for fertility to analyze its effect on labor supply (Bronars and Grogger 1994; He and Zhu 2016) or to test the quantity-quality trade-off for children (Black et al. 2005; Li et al. 2008; Angrist et al. 2010; Åslund and Grönqvist 2010; Black et al. 2010).

Second, the amount of time passed since the last childbirth is important for the subsequent fertility decision, and the question of whether to have an additional child also affects the labor market activities of married women. The more time that has passed since the last childbirth, the more easily a woman can return to work. Controlling for the youngest child's age as an explanatory variable would attenuate the above-mentioned bias, but the estimates are far from causal because the assumption of a time-invariant preference for the number of children is imposed implicitly. However, the preference for the number of children should be treated as time-variant in the real world when applying either mixed sibling-sex composition or twin births as an instrument. For example, mothers of twins and mothers of non-twins would have different paths (preferences) for having an additional delivery over time, as the burden of caring for children is doubled for mothers of twins. As an alternative approach, this study estimates the effect of fertility using two groups for which the interval since the last childbirth is no more than one year for the first group and no more than three years for the second. The choice of intervals is discussed specifically in the results section.

Other than using exogenous variation in the number of children caused by twin births, this study also effectively compares the difference between mothers of twins and mothers of non-twins using three sub-samples to investigate the causal effect of fertility on female labor supply. Specifically, in each sub-sample, individuals have the same frequency of childbearing experience, and those who have had at least one additional child after twin births are excluded from the sub-samples because these individuals tend to have a higher expected number of children; thus, mothers of twins always have one more child than mothers of non-twins. Each sub-sample allows me to analyze the effect of an additional child caused by twin births on female labor supply, relative to those mothers of non-twins when the preference for the number of children is held constant. In the literature, when a two-stage least squares estimation is applied, preferences for number of children are assumed to be the same among mothers with at least n births. However, mothers with n births and mothers with $n+1$ births may have different preferences for the number of children. To my knowledge, this research is the first that properly holds constant the preference for number of children for mothers with n births, where $n \in \{1, 2, 3\}$.

The results show that an additional child (caused by twin births) reduces the likelihood of labor market participation by 10.6% for a first-time mother and by 4.7% for a second-time mother, with both estimates accounting for the preference for the number of children and being statistically significant. In contrast to the results for mothers with relatively fewer deliveries, the effect of fertility vanishes for third-time mothers who have a fourth child through twin birth, no longer being significant at the 10% level. This suggests that the effect of fertility on female labor supply is not monotonically decreasing in the number of births. Contrary to previous findings, our IV estimates and sub-sample analysis results are both larger in magnitude than OLS estimates when holding the time since the last childbirth constant, revealing the endogeneity problem caused by the time-variant preference for the number of children suggested in the previous paragraph.

The reminder of this paper is organized as follows. Section 2.2 describes the data sets. The following section presents the identification strategy. Section 2.4 compares the estimates among OLS, IV and sub-sample analyses. Section 2.5 checks the robustness of the estimates. Section 2.6 provides a summary.

2.2 Data

The data used in this research originate from the 2000 Population and Housing Census, which is a universal sample of all residents of Taiwan. It is the fifth in the series, following the previous four censuses conducted in 1956, 1966, 1980, and 1990. The data set covers 22,300,929 individuals across 6,495,751 households. Individual-level information on demographic characteristics, marital status, education, employment status, ethnicity, etc. is collected.

One advantage of this data set is the huge sample size, which captures the overall characteristics of the Taiwanese population. Such universal census data offer extremely high statistical power in individual-level analyses. Compared with previous studies, we can apply much more strict sample selection rules to obtain a less heterogeneous sample. Moreover, the huge sample size allows me to analyze fertility decisions by delivery order [2] to observe the heterogenous effect across families with different preferences for the number of children, while it is difficult to do so with survey data. Second, along with the *hukou* system-based population census of Taiwan, our sample has less measurement error caused by migration (because of job allotment, etc.). As the U.S. population census is conducted on the basis of the current residence, previous studies cannot track husbands and wives who are living in two places, as well as their minor children.

We match children to their parents through the relationship identifier within households. First, we identify individuals who are labeled "child" and calculate the fertility information (of their mothers), including the number of children and twin births at the nth delivery. Second, I supplement the fertility information with data on mothers who are

[2] It is up to three in this research. Only 2.51% women have more than 3 deliveries in the final sample.

labeled "household head" or "spouse" in each household. The mothers are the primary observations in this study. For each mother, I also construct the husband's information, including years of schooling and employment status.

For the analysis of the fertility and labor supply of married women, the sample is restricted as follows: (1) Following Angrist and Evans (1998), we only use children of the household head to construct the fertility information, as we cannot identify the relationship among other relatives within the households. Households with no children are also dropped. (2) I restrict the sample to mothers who are between 16 and 35 years of age and whose eldest child is no more than 20 years of age. In Taiwan, the legal marriage age is 16 for females, and children are not allowed to move out of a household before age 20. I impose such restrictions to ensure that no adult children have already left the household by the time of the survey. (3) Finally, I exclude single mother households because information on fathers can not be obtained. I also dropped households with fathers who are under 18 or over 50, the latter of which can result in problems of remarriage in a cross-sectional census.

The final sample contains 617,852 females, 9,561 of whom have given birth to twins. Because the census does not include an exact identifier for twins, we define twins as children who were born in the same year. Information on the birth month and birthdate is not offered in the census. To avoid measurement errors, we dropped households with more than 2 children who were born in the same year, which may caused by adoption. In 2000, there were 3987 children adopted children in Taiwan while the number of newborn babies was 305,312[3]. Moreover, approximately 26% of adoptive households abandoned adoptions for unknown reasons in each year during the period from 1993 to 2003[4]. Adoption would not be a severe problem in our data.

To investigate the difference between mothers who have and have not given birth to twins, descriptive statistics tabulated by delivery of twins are shown in Table 2.1. From Column 1 and Column 2, we find that the mothers of twins have a lower probability of being employed than the mothers of non-twins, and the mothers of twins have more children. Although no large differences are observed, the t-statistics shown in Column 4 do indicate significant differences in the covariates between the two samples. Specifically,

[3]Department of Household Registration, MOI.

[4]Author's calculation using the data from the *Statistical Year Book of Interior.*

mothers of twins and mothers of non-twins differ with respect to age, years of schooling, and husband's years of schooling, but they do not differ with respect to ethnic minority status, husband's employment status, and co-residence with an elderly parent. The birth of twins is a perfect instrument only when such an event is totally random, and the characteristics of mothers of twins should be as same as those of mothers of non-twins.

To address potential parental selection into twin births, I also construct a matched sample with extremely similar mothers of twins and mothers of non-twins. The t-statistics of the matched sample are displayed in Column 5 of Table 2.1, which are not significant in all covariates. This matched sample will be used in the section on robustness checks, in which the selection issue will be specifically discussed.

2.3 Identification Strategy

2.3.1 IV Estimation

To estimate the effect of the number of children on married women's labor force participation, the benchmark model is specified as follows:

$$LFP_i = \beta_0 + \beta_1 Children_i + \boldsymbol{X}_i^{'}\delta_1 + \boldsymbol{Z}_i^{'}\delta_2 + \epsilon_i \tag{2.1}$$

where the LFP_i is a binary outcome variable indicating married women's labor force participation, which equals 1 if working and 0 otherwise. β_1 is the coefficient of interest, capturing the effect of the number of children. $\boldsymbol{X}_i$ is a vector of individual characteristics including age, age squared, education, and ethnicity. $\boldsymbol{Z}_i$ is a vector of husband's characteristics and living arrangements, which includes husband's education and labor supply, and a binary variable indicating co-residence with an elder parent.

However, the OLS estimates are consistent only if number of children is not correlated with error term ϵ_i, which is obviously not the case. To address the endogeneity of fertility, I use twin birth as an instrumental variable for the number of children. The first stage of the IV estimation is as follows:

$$Children_i = \gamma_0 + \gamma_1 Twins_i + \boldsymbol{X}_i^{'}\rho_1 + \boldsymbol{Z}_i^{'}\rho_2 + \varepsilon_i \tag{2.2}$$

where $Children_i$ is the number of children of a married woman. $Twins_i$ is a binary instrumental variable that equals 1 if a woman has given birth to twins at the nth delivery and 0 otherwise. $\boldsymbol{X}_i$ and $\boldsymbol{Z}_i$ are the same vectors of control variables as in Equation (1).

2.3.2 Sub-sample OLS Estimation Using an Efficient Instrument

In this research, the second identification strategy employs OLS estimation with three sub-samples of our data. As noted by Rosenzweig and Wolpin (1980), the probability of twin birth increases in the number of deliveries. To control for the preference for the number of children, I follow Li et al. (2008) and restrict the sample to families with at least n births in IV estimations, which assumes that families with n births and $n+1$ births have same preference for number of children. In this subsection, I will discuss a model that compares mothers of twins and non-twins who have exactly the same frequency of deliveries, and the model is specified as

$$LFP_i = \beta_0 + \beta_1 Twin_i + \boldsymbol{X}_i'\delta_1 + \boldsymbol{Z}_i'\delta_2 + \epsilon_i \tag{2.3}$$

Table 2.2 provides a graphical descriptions of the sub-samples. Sub-sample A includes mothers of non-twins with a single child and mothers of twins at the first delivery, namely women who only have one delivery. Sub-sample B includes mothers of two non-twins and mothers of twins at the second delivery, namely mothers who have two deliveries. Sub-sample C includes mothers of three non-twins and mothers of twins at the third delivery, namely mothers who have three deliveries. In each sub-sample, mothers of twins have one more child than mothers of non-twins. I construct sub-samples for up to three deliveries because only 2.52% of women have more than 3 deliveries in the final sample, which is also shown in Figure 2.2.

In these sub-sample analyses, IV estimations are not necessary as the variation in the number of children is entirely determined by twin birth. In other words, the efficient instrumental variable, $Twin_i$, is equivalent to the number of children. This approach estimates the effect of an additional child (caused by a twin birth) on female labor supply, relative to mothers of non-twins.

2.4 Results

This section discuss the OLS and IV regression estimates, which were designed to test whether fertility has a negative effect on the labor supply of married women in Taiwan. It is worthwhile to emphasize that the we examine some cases that would deliver biased IV estimates, which will be discussed in the subsection on the sub-sample analyses. In all regressions, linear probabilities are used along with robust standard errors. Before presenting the results of the OLS and IV estimation, we discuss the validity of twin births as an instrument.

2.4.1 The First Stage

Unobserved Heterogeneity. A good IV in this case should be highly correlated with number of children but should not affect the labor supply of married women except through the number of children. In other words, a valid IV should not be correlated with unobserved characteristics that are captured by the error term, ϵ_i, in Equation (1).

One concern is that the occurrence of twin births may not be random and may be correlated with unobserved family characteristics, which by design, cannot be tested. As an alternative approach, I control for a full set of family characteristics including parents' education and living arrangement in regressions. Moreover, I examine the difference between families with twins and those without twins as shown in Column 4 of Table 2.1. The enormous sample size provides the power to show that mothers of twins exhibit significant difference in years of schooling, as do their husbands. To avoid parental selection into twin births, I construct a matched sample with extremely similar families with twins and without twins to check the robustness of the findings, which will be discussed in the section on robustness checks.

Time Since the Last Childbirth Another concern is that a twin birth may affect female labor supply through the time since the last childbirth. As in a cross-sectional census, women are assigned different durations since the last childbirth because the survey time is fixed. Figure 2.3 provides a graphical example. Suppose that mother 1 married at t_0 , had her first delivery at t_1 and had her second delivery at t_3; thus, she would have a duration of a from her second delivery until survey time T. Suppose that mother 2 married at t_1, had her first delivery at t_2, and experienced a twin birth at t_4, which is her second delivery. However, mother 2 would have a duration of b until survey time T, which is totally different from mother 1.

There are two possible channels through which twin births might affect female labor supply via the time elapsed since the last childbirth. In both cases, the IV and sub-sample OLS estimates of the effects of fertility could be biased. First, the time since the last childbirth is highly related to presence of a younger baby. For example, mothers of younger babies have to provide more childcare in terms of time and energy, which cannot be completely substituted by market services. Moreover, mothers of twins take on twice the burden of childcare relative to women who are not mothers of twins. Consequently, the former should be more likely to drop out of the labor market relative to mothers of non-twins.

Second, twin births may affect female labor force participation through a time-variant preference for the number of children, which is not observable. Fertility should be a sequential choice, which is not decided at a given time. That is, a woman would decide whether to have another birth after having given birth, and this choice may change over time. As shown in Figure 2.3, suppose that both mother 1 and mother 2 expect three children over their lifetime; then, mother 2 (the mother of twins) would likely stop fertility after her twin birth at the second delivery, and the longer the duration b is, the higher the probability that mother 2 will participate in the labor market. Because if duration b is long enough for a child to grow to be school age, mother 2 can devote relatively less attention to her child than when her child is younger. However, mother 1 would prepare to have her next pregnancy, which is censored at survey time T, and this would also affect her labor supply.

To address this possibility, in both the IV estimation and sub-sample analyses, I have regressed unconstrained samples and, separately, those with 3-year and 1-year intervals. To hold the time-variant preference and presence of younger baby constant, a short interval should be given, such as 6 months or 1 year. However, month information is not available in the data, and thus I assign a 1-year interval, along with a 3-year interval as a comparison. Although this approach can measure a precise decline in female labor supply immediately after they give birth, a huge sample size is required. To my knowledge, this paper is the first to analyze the effect of fertility on female labor supply while controlling for the time elapsed since the last childbirth in a cross-sectional census.

2.4.2 OLS and IV Estimations

Table 2.3 presents the OLS and IV estimates of the effect of the number of children on female labor supply, in addition to the first-stage relationship between the number of children and twins at the nth delivery. The results without controlling for the time since the last childbirth are reported in the first three columns, the results with the 3-year interval are reported in the middle three columns, and the results with the 1-year interval are reported in the last three columns. From top to bottom, three panels are listed for families with at least n births in ascending order of n from 1 to 3.

The OLS estimates in Column (1), Column (4), and Column (7) consistently show a significantly negative correlation between the number of children and the mother's labor force participation, regardless of the sample and duration assigned. For example, the OLS coefficient in Panel A (Column 1) suggests that, holding covariates controlled for in the regression constant, an additional child in the family reduces a mother's likelihood of labor force participation by approximately 5 percentage points, when time since the last childbirth is not restricted.

Using twin births as the instrument, the IV estimates in Column (2), Column (5), and Column (7) continue to suggest a negative effect of the number of children on the mother's outcome, except for Panel C of families with at least 3 births, and the coefficient sizes decrease in birth order. In particular, the IV coefficients on number of children are significant at the 1% level for families with at least one birth (Panel A), significant at the 5% level for those with two or more births (Panel B), and not significant, even at the 10% level, for families with three or more births (Panel C). Note that according to the previous discussion, the IV estimates may be subject to upward bias induced by not accounting for the time elapsed since the last childbirth.

As shown in Column (8), when a 1-year interval is assigned, the IV coefficients change dramatically from those in Column (2) except for Panel C, in which time passed since the last childbirth is not held constant. Specifically, for families with one or more births (Panel A), the estimates when the 1-year interval is assigned (Column 8) double to 10.5 percentage points, relative to those when the interval is not assigned (Column 2). Moreover, the estimates in Panel B change to 6.5 percentage points from 2.8 percentage points. However, the IV coefficients for families with 3 or more births (Panel C) remain highly stable even after the 1-year interval is assigned, which indicates no causal effects of fertility on female labor supply.

2.4.3 Sub-Sample Analyses

Table 2.4 presents the estimates of the sub-sample analyses of an additional child (caused by twin births) on female labor supply, where the preference for the number of children is held constant. The results without control variables are reported in odd columns, and the results with control variables are reported in even columns. Furthermore, the estimates are tabulated by duration since the last childbirth, where the first two columns are assigned no intervals, the middle two columns are assigned a 3-year interval, and the last two columns are assigned a 1-year interval. From top to bottom, three panels are listed for families with exactly n births in ascending order of n from 1 to 3.

The estimates of the sub-sample analyses are very similar to those from the IV analysis in Table 2.3, indicating negative effects of fertility on female labor supply except for families with more than 3 deliveries. For example, mothers of twins at the first delivery (Panel A) are 5.8 percentage points less likely to participate in the labor market (Column 2), and the results remain stable irrespective of the inclusion of control variables. However, estimates with and without control variables show a different pattern for higher-order births, which are marginally decreasing when controlling for demographic characteristics, husband's characteristics, and living arrangements.

As in the IV estimation, the results of the sub-sample OLS estimation also change dramatically when a 1-year interval is assigned. According to Column (6), an additional child (caused by a twin birth) reduces the labor force participation of a first-time mother by 10.6 percentage points (Panel A), which almost doubles the estimates in Column (2). A similar pattern can be found for a second-time mother (Panel B). However, an additional child no longer decreases female labor supply for a third-time mother (Panel C), and we even observe a positive but insignificant effect when a 1-year interval is assigned (Column 6).

2.5 Robustness Check

To address parental selection into twin births, I construct a matched sample with extremely similar family backgrounds for mothers of twins and those of non-twins. As shown in Column (5) of Table 2.1, mothers of twins differ only with respect to employment status and the number of children and no longer differ in age, years of schooling, husband's education and employment status, and living arrangements. The test for balanced covariates is displayed in Figure 2.4, indicating that the matched sample has much less standardized bias across covariates.

Table 2.5 presents the results of a matching estimation of twin births (treatment) on female labor supply, which is robust to IV estimation and sub-sample OLS estimation. Results from a sample without replacement are shown in odd columns and those from a sample with replacement are shown in even columns.

Compared to IV estimation and sub-sample OLS estimation, the estimates of the matched sample reveal a slightly smaller fertility effect for first-time mothers and a slightly larger fertility effect for second-time mothers when the time elapsed since the last childbirth is held constant. According to Column (6), an additional child (caused by a twin birth) reduces the labor force participation of a first-time mother by 9 percentage points (Panel A), while the estimates from IV and sub-sample OLS are 10.5 and 10.6 percentage points, respectively. For a second-time mother (Panel B), an additional child reduces labor supply by 7 percentage points, while the estimates from IV and sub-sample OLS are 6.5 and 4.7 percentage points, respectively. The fertility effect for a third-time mother remains statistically insignificant across different identification strategies.

2.6 Concluding Remarks

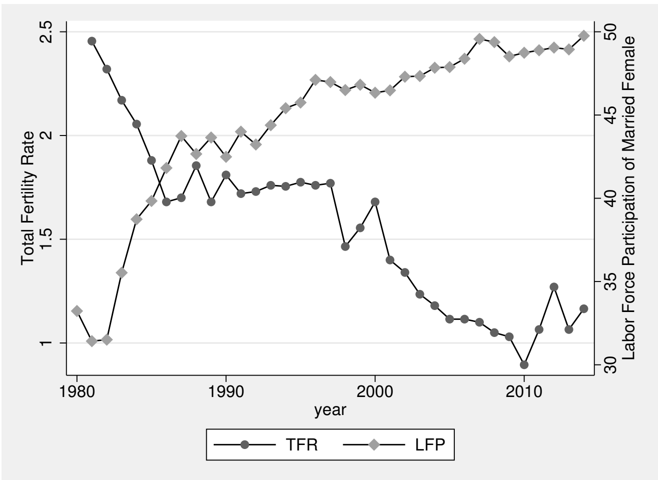
This paper estimates the causal effect of fertility on female labor supply. Using a universal sample of the Taiwanese population, which allows me to analyze the fertility decisions by each order to observe the heterogenous effect across families with different preferences for the number of children, I exploit the exogenous variation in the number of children caused by twin births to identify the causal effect of fertility on the labor supply of married women.

Three major findings are as follows. First, the effect of fertility on female labor supply is not monotonically decreasing in the number of children. The IV estimates show that fertility reduces the female labor supply by 10.5 percentage points for a first-time mother and 6.5 percentage points for a second-time mother. However, the fertility effect becomes positive and statistically insignificant for a third-time mother. This evidence offers a new policy implication that the government could encourage the fertility of mothers who have given multiple births to simultaneously increase the total fertility rate and female labor force participation rate. The differences in decision-making regarding labor supply are assumed to be associated with unobserved marriage-specific human capital.

Second, this paper shows that the effect of fertility varies substantially with the time elapsed since the last childbirth. I have constructed unconstrained samples and samples with 3-year and 1-year intervals to analyze the heterogenous effects with respect to the time elapsed since the last childbirth. For a first-time mother, the IV estimate with a 1-year interval assigned is approximately twice the IV estimate with no interval. However, the OLS estimates are stable across samples with different intervals. This evidence indicates that the IV estimation without controlling for birth intervals would depend on the distribution of the time elapsed since the last childbirth, which has consequences for differences in estimates across samples in the literature.

Finally, I use a new identification strategy to analyze the effect of one additional child on female labor supply with an efficient instrument, which captures a real sense of the preference for the number of children for mothers with n births. In the literature, when a two-stage least squares estimation is applied, preferences for the number of children are assumed to be the same among mothers who have at least n births. By constructing the three sub-samples described in Table 2.2, the exogenous variation in the number of children arises solely from twin births among mothers who have the same number of births. Thus, I present a sub-sample OLS estimation to compare with classical IV estimation. The results of the sub-sample OLS are similar to those of IV, which indicates that preferences for the number children may not change substantially between mothers with n and $n+1$ births.

FIGURE 2.1: *Time Trend of Fertility and Female Labor Supply*



Source: (1) Report on the Manpower Utilization Survey and (2) Vital Statistics of Taiwan.

TABLE 2.1: *Descriptive Statistics for Married Women*

		Mothers of		Difference	
VARIABLES	(1) Overall	(2) Twins	(3) Non-Twins	(4) Unmatched	(5) Matched
Employed	0.596	0.559	0.597	-0.038***	-0.023***
	(0.491)	(0.497)	(0.491)	(0.005)	(0.007)
Number of children	1.949	2.864	1.935	0.929***	0.916***
	(0.787)	(0.828)	(0.778)	(0.008)	(0.012)
Age	30.927	31.281	30.922	0.360***	-0.015
	(3.365)	(3.198)	(3.367)	(0.035)	(0.046)
Age squared/100	9.678	9.888	9.675	0.213***	-0.009
	(1.990)	(1.907)	(1.991)	(0.021)	(0.028)
Years of schooling	11.362	11.153	11.365	-0.212***	-0.002
	(2.627)	(2.614)	(2.627)	(0.021)	(0.038)
Minority	0.022	0.025	0.022	0.003	0.002
	(0.147)	(0.155)	(0.147)	(0.002)	(0.002)
Husband					
Years of schooling	11.627	11.442	11.630	-0.188***	-0.003
	(2.911)	(2.924)	(2.910)	(0.030)	(0.042)
Employed	0.964	0.965	0.964	0.001	-0.002
	(0.187)	(0.185)	(0.187)	(0.002)	(0.003)
Elderly					
Co-resident	0.088	0.093	0.088	0.005	0.003
	(0.284)	(0.291)	(0.284)	(0.003)	(0.004)
Observations	614,902	9,501	605,401	614,902	19,002

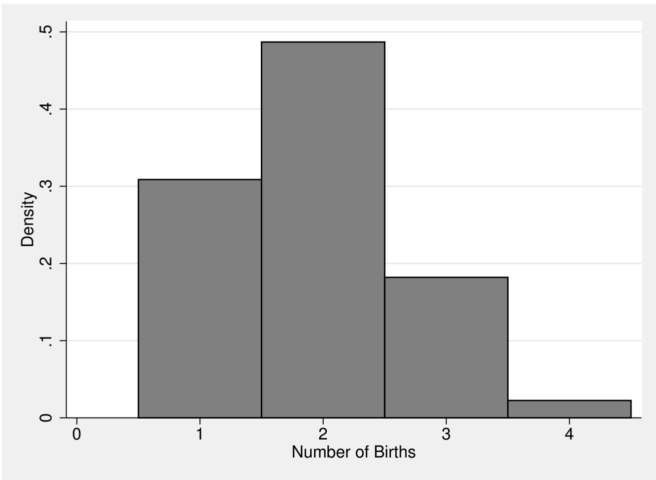
Notes: Standard errors in parentheses. Column 4 is the raw difference between mothers of twins and mothers of non-twins. Column 5 is the difference after one-to-one matching, which includes 9501 mothers of twins and 9501 mothers of non-twins. The results remain stable when I apply other matching methods such as *k*-nearest neighbors, radius, and kernel. The propensity score used for matching is calculated by logistic regression, which is available upon request. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE 2.2: *Descriptions of Sub-Samples*

	(1) Sub-Sample A	(2) Sub-Sample B	(3) Sub-Sample C
Mothers of non-twins	①	① ②	① ② ③
vs			
Mothers of twins	❶ ❶	① ❷ ❷	① ② ❸ ❸
Number of children	1 *vs* 2	2 *vs* 3	3 *vs* 4

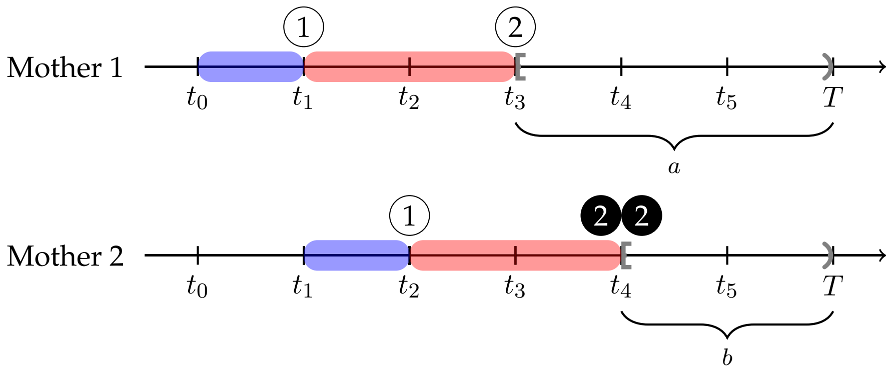
Notes: ◯ indicates non-twin, and ⬤ indicates twins. Numbers in circles show the birth order. Sum of circles shows the total number of children mothers have.

FIGURE 2.2: *The Distribution of Childbirth*



Notes: Author's calculation using Population and Housing Census 2000 of Taiwan. Mothers having more than 4 births are dropped, which covers 0.27% of the overall sample as described in Table 2.1.

FIGURE 2.3: *An Example of Potential Bias*



Notes: ◯ indicates non-twin, and ⬤ indicates twins. Numbers in circles show the birth order. Braces are durations from last childbirth to the survey time T, where $a = T - t_3$ for mother 1 and $b = T - t_4$ for mother 2.

TABLE 2.3: *Estimated Coefficients of OLS and IV Conditioned on Birth Order and Time since Last Childbirth*

	Since the last childbirth								
	Unconditioned			No more than 3 years			No more than 1 year		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	OLS	IV	First Stage	OLS	IV	First Stage	OLS	IV	First Stage
*Panel A: Mothers of twins at the first delivery **vs** Mothers of non-twins*									
Number of children	-0.049***	-0.051***		-0.052***	-0.097***		-0.057***	-0.105***	
	(0.001)	(0.008)		(0.001)	(0.010)		(0.002)	(0.014)	
Twins			0.723***			0.771***			0.796***
			(0.009)			(0.012)			(0.017)
Observations	612,504	612,504	612,504	342,877	342,877	342,877	194,748	194,748	194,748
*Panel B: Mothers of twins at the second delivery **vs** Mothers of non-twins with 2 or more births*									
Number of children	-0.047***	-0.028**		-0.039***	-0.041**		-0.038***	-0.065***	
	(0.001)	(0.012)		(0.002)	(0.017)		(0.003)	(0.022)	
Twins			0.884***			0.890***			0.919***
			(0.012)			(0.019)			(0.026)
Observations	421,605	421,605	421,605	213,680	213,680	213,680	114,168	114,168	114,168
*Panel C: Mothers of twins at the third delivery **vs** Mothers of non-twins with 3 or more births*									
Number of children	-0.026***	0.019		-0.017***	-0.011		-0.012**	0.018	
	(0.003)	(0.025)		(0.005)	(0.034)		(0.006)	(0.042)	
Twins			1.027***			1.024***			1.064***
			(0.026)			(0.040)			(0.061)
Observations	125,612	125,612	125,612	62,311	62,311	62,311	31,991	31,991	31,991

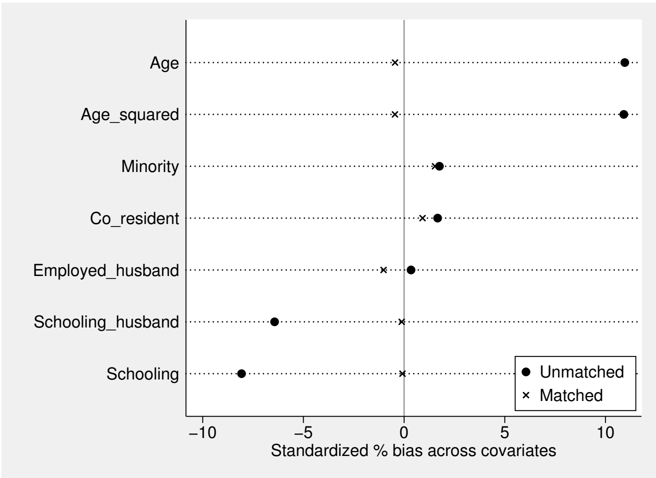
Notes: Robust standard errors in parentheses. All specifications control for age, age squared, years of schooling, ethnic minority status, husband's years of schooling, husband's employment status, co-residence with elder parents, and county dummies. In all panels, upper bounds on the number of children are not imposed. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 2.4: *Estimated Coefficients from OLS for Sub-Samples*

	Since the last childbirth					
	Unconditioned		No more than 3 years		No more than 1 year	
	(1)	(2)	(3)	(4)	(5)	(6)
	No controls	With controls	No controls	With controls	No controls	With controls
*Panel A: Mothers of twins at the first delivery **vs** Mothers of single child (2 **vs** 1)*						
Twins	-0.057***	-0.058***	-0.097***	-0.095***	-0.107***	-0.106***
	(0.008)	(0.008)	(0.011)	(0.011)	(0.015)	(0.015)
Observations	187,325	187,325	127,309	127,309	79,511	79,511
*Panel B: Mothers of twins at the second delivery **vs** Mothers of two non-twins children (3 **vs** 2)*						
Twins	-0.046***	-0.032**	-0.067***	-0.039**	-0.068***	-0.047**
	(0.013)	(0.012)	(0.018)	(0.017)	(0.025)	(0.023)
Observations	296,315	296,315	151,743	151,743	82,448	82,448
*Panel C: Mothers of twins at the third delivery **vs** Mothers of three non-twins children (4 **vs** 3)*						
Twins	-0.004	-0.001	-0.052	-0.033	-0.016	0.001
	(0.029)	(0.028)	(0.038)	(0.038)	(0.051)	(0.051)
Observations	111,495	111,495	54,528	54,528	27,847	27,847

Notes: Robust standard errors in parentheses. Specifications with controls include controls for age, age squared, years of schooling, ethnic minority status, husband's years of schooling, husband's employment status, co-residence with elder parents, and county dummies. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

FIGURE 2.4: *The Bias Reduction of Covariates after Matching*



Notes: Y-axis shows all covariates.

TABLE 2.5: *Matching Estimator of Twin Births on Female Employment*

	Since the last childbirth					
	Unconditioned		No more than 3 years		No more than 1 year	
	(1) Without repl.	(2) With repl.	(3) Without repl.	(4) With repl.	(5) Without repl.	(6) With repl.
*Panel A: Mothers of twins at the first delivery **vs** Mothers of single child (2 **vs** 1)*						
Twins	-0.042	-0.033***	-0.069***	-0.065***	-0.090***	-0.090***
	(0.012)	(0.012)	(0.016)	(0.016)	(0.021)	(0.021)
Observations	6,984	6,984	3,812	3,812	2,110	2,110
*Panel B: Mothers of twins at the second delivery **vs** Mothers of two non-twins children (3 **vs** 2)*						
Twins	-0.044**	-0.049***	-0.039	-0.061**	-0.060*	-0.070**
	(0.018)	(0.018)	(0.025)	(0.025)	(0.035)	(0.035)
Observations	3,150	3,150	1,576	1,576	834	834
*Panel C: Mothers of twins at the third delivery **vs** Mothers of three non-twins children (4 **vs** 3)*						
Twins	-0.010	0.007	-0.012	-0.012	0.086	0.086
	(0.040)	(0.040)	(0.055)	(0.055)	(0.072)	(0.072)
Observations	612	612	330	330	186	186

Notes: Robust standard errors in parentheses. Columns (1), (3), (5) are without replacement, and Columns (2), (4), (6) are with replacement. In all specifications, 1-to-1 caliper matching is applied. The propensity score used for matching is calculated by logistic regression, which is available upon request. *** p<0.01, ** p<0.05, * p<0.1.

Chapter 3

The Impact of 9-year Compulsory Education: Quasi-Experimental Evidence from Taiwan

3.1 Introduction

Compulsory education has been implemented in most countries in the world as it accumulates a nation's human capital in the long term and is supposed to reduce poverty by offering fair educational opportunities. The United Nations Organization for Education, Science and Culture (UNESCO) claims that education is a fundamental human right and an essential dimension of democracy, development and human dignity. Furthermore, additional education due to compulsory schooling law has been found to be causally related to more earnings(Angrist and Krueger 1991; Harmon and Walker 1995; Oreopoulos 2006), better health(Kemptner et al. 2011; Fischer et al. 2013; Crespo et al. 2014), and fewer crimes(Lochner and Moretti 2004). Although compulsory schooling has its particular implications for policy and research, causal evidence has rarely been shown in Asian countries, where they have common traditional values in education.

In 1968, Taiwan carried out a 9-year compulsory schooling law, which was designed to extend the average educational attainment and relax the extremely intense competition in junior high school entry. Before 1968, a 6-year compulsory education system had been enforced for over 25 years since 1943, which was still during a colonial era of Taiwan under Japanese rule. After the new policy of compulsory schooling had been introduced on September 1st in 1968, Taiwan experienced a development regarding new school openings, quantity and quality of teachers, revision of curriculum guideline and so forth. As a consequence, Taiwan's junior high school enrollment rate increased to 99.95% in 2014 from only 31.78% in 1950[1]. Among these students graduated from junior secondary schools, 99.52% of them would continue to have senior secondary education in 2014, when only 51.15% would do so in 1950. Also, the contribution of compulsory schooling is well documented about literacy rate[2] and economic growth(Jennie Hay 1991; Liu and Armer 1993).

The policy impact of 1968 compulsory schooling law on students' educational outcomes itself is not well quantitatively evaluated yet due to limitations of data and statistical method. Other than the educational outcomes, several studies in Taiwan have investigated the impact of schooling on individual economic and health outcomes through institutional changes on the supply side of the education system in a narrow sense (e.g. increasing number of schools). Chou et al. (2010) analyzes the effect of parental schooling on child health by exploiting the variation in new junior high school openings across regions and cohorts. They find that parents' schooling causally improves infant health outcomes, which saves approximately one infant life in 1,000 live births. Their first stage results on schooling reflect partial effects of extension in the education system based on the increase in the number of schools, while the 1968 compulsory schooling law also changes the quality and quantity of teachers, curriculum guideline and so forth. Chuang and Lai (2010) estimates the return to university education using instrumental variable approach, and their estimated rate of the return to a university degree is approximately 15-19%. They take account of the endogeneity of education by a group of instruments, which are the implementation of compulsory education, residential area and sibling size. As first stage results are not reported in their paper, the impact of mandatory education on years of schooling remains an open question in Taiwan.

Using the Population and Housing Census, I exploit a regression discontinuity design (RDD) to evaluate the policy impact of the 9-year compulsory schooling law on students' educational outcomes. As the policy was implemented on September 1st in 1968, students who were still in their sixth grade of primary school had to go to junior high schools, while those who had graduated from primary schools were not required

[1]Source: Statistical Yearbook of the Republic of China (Taiwan) 2014
[2]98.6% in 2015, Source: Statistical Yearbook of Interior (Taiwan)

to do so. In other words, the policy compels students born on or after September 1st in 1955 to attend school longer than students born before the eligibility cutoff, which is a great natural experiment to evaluate its impact. Moreover, the dataset used in this study records each student's exact date of birth, along with the huge sample of universal Taiwanese population, allows me to perform a very precise regression discontinuity estimates. By comparing the educational outcomes of those who were born before and after the eligibility cutoff, this study shows that the 9-year compulsory schooling law has not only a causal impact on students' average years of schooling, but also the probability of drop out from junior high schools and senior high schools.

This paper contributes to the literature on compulsory education in following aspects. First, this study includes both a credible identification strategy and an unusual universal sample of Taiwanese population to evaluate the national compulsory schooling law. Since Angrist and Krueger (1991), massive quantities of evidence has been accumulated under the framework of instrumental variable (IV) approach. Depending on the IV used, the impact of compulsory schooling law might be a mixed effect with quarter-of-birth effect, or a partial effect driven by school infrastructure construction only. Recently, the RD design has been used to evaluate compulsory education reforms, as these policies usually require mandatory years of schooling or minimum age of leaving school, which creates a discontinuity in education attainment across cohorts. Due to data limitation, Oreopoulos (2006), Devereux and Hart (2010) and Grenet (2013) use the year of birth to measure the eligibility cutoff, which is with some measurement errors. Clark and Royer (2013) improves the identification using the month of births. Using the detailed information on the exact date of births, this study measures the bandwidths relative to eligibility cutoff by a 1-day interval, and shows a precise and reliable estimates of the compulsory schooling law.

Second, this paper is the first to study the causal impact of compulsory schooling law on students' educational outcomes in the context of Taiwan, which offers a new quasi-experimental evidence outside the U.S. and European countries. So far, the RDD literature on compulsory education is mostly limited to UK (Oreopoulos 2006; Devereux and Hart 2010; Grenet 2013; Clark and Royer 2013) and France (Grenet 2013). For instance, Clark and Royer (2013) finds that raising school-leaving-age from 14 to 15 increase the average years of schooling by 0.5, and raising school-leaving-age from 15 to 16 increases the average years of schooling by 0.25. In this study, the impact of raising mandatory schooling years from 6 to 9 is estimated to increase 0.22 years of schooling in Taiwan. Given the change in school-leaving-age or mandatory years of schooling, the impact of compulsory schooling law in Taiwan is smaller than previous evidence from European countries. A possible interpretation could be that Taiwan is a very education oriented country where has particular strong traditional values in education, the average years of schooling was already near or over nine years (depends on the bandwidths relative to the eligibility cutoff) before the compulsory schooling was implemented.

Third, this paper has investigated the heterogeneous effect of the compulsory schooling law across genders and provinces of origin. The main findings are as follows: (1) Regardless the educational outcomes used, the compulsory schooling law has a larger effect on male than female, which is in contrast to the literature. In all of the previous studies using a regression discontinuity design, which is mentioned in the last paragraphs, have found a larger effect for female, as those who have relatively weak socio-economic power would benefit more from a well enforced compulsory schooling law. (2) Similarly, the policy shows a larger effect on local Taiwanese people than Mainlanders (the second generation immigration). As families of Mainlanders in Taiwan pay extreme attention to children's education, average years of schooling of these children had already been approximately 12 years before the 9-year compulsory schooling law was conducted, the policy only affects them marginally.

The remainder of this paper is organized as follows. Section 3.2 describes the data and variables used in this study. The following section presents the identification strategy. Section 3.4 discusses the results of regression discontinuity design, and the heterogeneous effects. Section 3.5 provides a summary.

3.2 Data

The data used in this paper comes from the 1980 Population and Housing Census of Taiwan, which is a universal sample of all Taiwan residents. It is the third of the series, following the previous censuses that have been conducted in 1956 and 1966. The data set contains both information of individual and household members, including demographic characteristics, marital status, education, relationship to the household head and so forth. Furthermore, the data set includes an indicator of citizenship. I restrict the sample to domestic individuals to observe the policy impact, as resident aliens might be entirely different from native-born Taiwanese through unobserved factors like culture and family values.

It is worthwhile to emphasize two advantages of this data set. First, 1980 Taiwan census is the only available data set with variables of exact birth dates included. The censuses are not digitized prior to 1980, and only birth year is offered posterior to 1980 for personal information protection. The information on exact birth dates enables me to identify the discontinuity induced by the education reform within an extremely narrow bandwidth of time, which would have fewer measurement errors. Second, the data set covers 100% Taiwan population, which has a substantial statistical power. The large sample size assures that the regression discontinuity design could be applied to observe the causal impact of compulsory education reform in Taiwan.

To observe the policy impact, I use three outcomes for individuals who were born before and after the 9-year compulsory education reform. Specifically, the three dependent variables are as follows: (1) Years of schooling is obtained by recoding categorical educational attainment into equivalent years.[3] By comparing the difference in years of schooling between those who were born before and after the policy, we could tell how the policy extend the average education attainment. (2) Leaving school by age 15 (coded 1 if the person dropped out from school by age 15 and otherwise). Due to the 9-year compulsory schooling law, those who were born on or after September 1 in 1955 have to stay in school until they complete their junior high school education. By focusing on this variable, I can analyze the policy impact on children with less ability or education budget within the family, who have to drop out from school if the policy was not introduced. (3) Leaving school by age 18 (coded 1 if the person dropped out from school by age 18 and otherwise). I use the third outcome variable because the compulsory schooling law is supposed to have a long-term effect on individual's education, although only junior high school education is required.

Table 3.1 shows the descriptive statistics of the variables used in this study. The final

[3] Educational attainment is classified into nine categories: (1)Illiterate; (2)Self-taught; (3)Elementary school; (4)Junior high school; (5)Senior high school; (6)Vocational school; (7)Associate college; (8)College; (9)Graduate school.

sample is restricted to those who were born between 1949 and 1961, with a 6-year bandwidth relative to the eligibility cutoff of the compulsory schooling law. The average age is 24.5, and average years of schooling is 9.6. Approximately 53.5% individuals are eligible for the compulsory schooling law. Among all the individuals, 48.6% are females and 12.4% are Mainlanders whose province of origin is Mainland China. Generally, 37.5% individuals drop out from school by age 15, and 56.1% drop out by age 18.

3.3 Identification Strategy

In this study, I use a regression discontinuity design to analyze the causal impact of the 9-year compulsory education reform. This approach exploits the discontinuous change in educational outcomes in the year of birth, which is September 1 in 1955. The baseline model is specified as follows,

$$Y_i = \beta_0 + \beta_1 * Eligible_i + f(\tilde{X}_i) + \varepsilon_i \tag{3.1}$$

where the dependent variable Y_i represents educational outcomes for individual i (years of schooling, leaving school by age 15, leaving school by age 18). $Eligible_i$ is a dummy variable that is equal to 1 if individual i was born on or after September 1st 1955 and 0 if individual i was born on or before August 31st 1955. $\tilde{X}_i = X_i - X_0$ and $f(\tilde{X}_i)$ is an unknown smooth function of date of births relative to the eligibility cutoff for individual i. Linear, quadratic, cubic, quartic, and quintic forms of the date of births are used. ε_i is a random error term.

Following Imbens and Lemieux (2008), I also use local linear regression to approximate the unknown function of date of births. The local linear regression model is estimated by the following equation,

$$Y_i = \beta_0 + \beta_1 * Eligible_i + \beta_2 \tilde{X}_i + \beta_3 Eligible * \tilde{X}_i + \epsilon_i \tag{3.2}$$

which uses a similar definition of the variables in Equation (2). The coefficients β_2 and β_3 allow the slope to be different on either side of the eligibility cutoff. Although $\tilde{X}_i$ can be measured within a one-day interval of the exact birthdate relative to the cutoff, I adjust $\tilde{X}_i$ into decimal years to observe the cohort effect clearly, which is still a continuous variable. I use samples with bandwidths of 1-year, 2-year, 3-year and 4-year to perform the estimation. ϵ_i is a random error term.

The parameter of interest is β_1, which represents the effect of compulsory education reform on individual's educational attainment. Under the identifying assumption that other determinants of educational attainment are continuous at the eligibility cutoff, β_1 would be an unbiased estimate of the impact of the extension of compulsory schooling law.

3.4 Results

In this section, I discuss the estimates of regression discontinuity design, which were designed to analyze the policy impact of compulsory education reform on individual's educational attainment. I first discuss the overall effects of the 1968 policy, and then show the heterogeneous effects across genders and provinces of origin. Both RDD estimates of global polynomial and local linear regression are reported in Table 3.2, Table 3.3 and Table 3.4. Due to space constraints, from Table 3.3, I only show estimates of local linear regression with three different bandwidths, along with the estimates of quadratic form as a comparison. Among all of the specifications, the estimates of local linear regression with a 2-year bandwidth around the eligibility cutoff are my preferred results, and these estimates are the focus of the discussion in the following subsections.

3.4.1 Overall Effects

Before presenting the results of RD, Figure 3.1, Figure 3.2 and Figure 3.3 provide a graphical evidence of the policy impact on three educational outcomes, respectively. Each figure plots the relationship between exact birthdate relative to the eligibility cutoff for the 9-year compulsory education and each educational outcome, with fitted values of the quadratic form and confidence interval attached. In all figures, each data point shows the mean each of educational outcome variable (e.g. years of schooling in Figure 3.1) within a one-day interval of the exact birthdate relative to the cutoff, and the unit of each x-axis is adjusted into decimal years for concision. I use dots and square dots to indicate data points for those who were born before and after eligibility cutoff, respectively. The figures show clear discontinuous changes in the educational outcomes around the cut-off. Visually, schooling is approximately 0.2-0.3 years longer, and likelihoods of leaving school by age 15 or 18 are 2-5 percentage points less among the initially eligible cohorts.

Table 3.2 displays the RDD estimates of compulsory education reform on individual's educational outcomes. Column (1) to (4) show the global polynomial estimates of RD, which use the functional forms of quadratic, cubic, quartic and quintic. Column (5) to (8) show the local linear estimates of RDD with different bandwidths of 1-year, 2-year, 3-year and 4-year. The RDD estimates are robust across different functional forms and bandwidths. From top to bottom, I report the results for three educational outcomes, which are years of schooling, leaving school by age 15 and leaving school by age 18.

The effect of 9-year compulsory education on years of schooling is approximately 0.22 years (Column 6). That is to say, individuals born on or after September 1, 1995, who are eligible for the compulsory schooling law, are more likely to have longer years of schooling by 0.22. Similarly, these initially eligible cohorts are 4 percentage points and 2 percentage points less likely to drop out from school by age 15 and age 18, respectively. All of the estimates are significant in statistics at 1% level. Given the mean of the control group that is reported under each estimate of educational outcomes, the 9-year compulsory schooling law increases average years of schooling among initially eligible cohorts by 2.4%, reduces the likelihood of leaving school by 8.9% by age 15 and 3.3% by age 18[4]. Despite the estimates on years of schooling and leaving school by age 15, it is interesting that an adverse effect of the compulsory schooling law on dropout probability by age 18 is also observed, although senior high school education is not required by the compulsory schooling law. Those who are eligible for the 9-year compulsory schooling law not only benefit from the policy itself though attending junior high school, more years of schooling also open their sights in different kinds of knowledge, thus they are more likely to have the further education than those who are not eligible for the policy.

3.4.2 Heterogenous Effects, by Gender

The impact of the compulsory schooling law might be different between males and females for following reasons. With a limited budget, parents tend to support children

[4] $\frac{0.219}{9.254} * 100\% \approx 2.4\%$, $\frac{0.040}{0.449} * 100\% \approx 8.9\%$, $\frac{0.019}{0.579} * 100\% \approx 3.3\%$

who are expected to be more economically productive adults within a family (Rosenzweig and Schultz 1982). Before the policy was implemented, Taiwan's economy had been dominated by agriculture and female children were commonly used as labor forces. Given the circumstances, females with relatively lower socio-economic power would have less educational opportunities than males.

Figure 3.4, Figure 3.5 and Figure 3.6 show graphical differences in three educational outcomes between male and female. In each figure, Panel A and Panel B present the results for male and female, respectively. Regardless of the outcome variable used, discontinuous changes are observed around the eligibility cutoff for both male and female, although the compulsory schooling law shows a larger impact on initially eligible male than the corresponding female.

Table 3.3 displays heterogeneous effects of the compulsory schooling law by gender. The first four columns show the results for male, and the other columns show the results for female. Due to space constraints, only estimates of quadratic form and local linear regression with bandwidths of 2-4 years are reported. Still, the estimates keep stable across different specifications.

According to Column (2) and Column (6) of Table 3.3, the impact of compulsory education is estimated to increase the average years of schooling by 0.27 years for males and 0.18 years for females. Also, the policy reduces the likelihood of dropout from school by age 15 by 4.6 percentage points for male, and 3.4 percentage points for female. A similar gender difference is also observed in the likelihood of leaving school by age 18. Given the different means of educational outcomes between male and female, these gender gaps become even larger. For instance, the initially eligible males are 12.2% less likely to leave school by age 15, but only 6.5% corresponding females would do so.

Previous studies have found that females benefit more from a well enforced compulsory schooling law, as females have lower opportunities to enroll in school if compulsory education are not implemented. Devereux and Hart (2010) uses a fuzzy regression discontinuity design to estimate the return to education in the UK. Their first stage results show that raising the school-leaving-age from 14 to 15 increases schooling by 0.55 years for females, but only 0.47 years for males. Grenet (2013) compares two compulsory education reforms that raised the school-leaving-age to 16 in France and in England and Wales, and finds larger effects of compulsory education on female schooling for both samples. In contrast to the literature on compulsory education, this paper have found a smaller effect on females than on males. A possible interpretation for the different results in this study is as follows. The average years of schooling among females born before the eligibility cutoff have already been 8.57, which is very close to the mandatory years required. The compulsory schooling law only marginally compels female students to complete the junior high school education, but has very small effects on further education decisions for females. Furthermore, parents tend to invest more in boys than in girls because boys are supposed to have higher earrings as adults. Other than sending girls to have more education beyond junior high schools, parents would prefer daughters to participate in farm work at home or find a job.

3.4.3 Heterogenous Effects, by Ethnicity

After World War II and the Chinese Civil War, approximately 1.3 million people from Chinese mainland migrated to Taiwan in 1949, the majority of which were either military men, government officials, or teachers (Barrett and Whyte 1982). In Taiwan, these people and their children are called Mainlanders (*Wai-sheng-ren*), while earlier immigrants who had settled down before 1949 are treated as local Taiwanese (*Ben-sheng-ren*). During 1949 to 1987, Taiwan had enforced a martial law, and Mainlanders had dominated advantages regarding fields of politics, military, media and so forth. Furthermore, children of Mainlanders had higher chances to enroll in schools not only because of higher education awareness of their parents, but also because of the severe unbalance in educational resources toward local Taiwanese people.

In Figure 3.7, Figure 3.8 and Figure 3.9, I graphically show the differences in three educational outcomes across ethnic group. In each figure, Panel A and Panel B present the results for local Taiwanese people and Mainlanders. Despite the outcome variables used, the impact of compulsory schooling law on local Taiwanese people is obviously larger than on Mainlanders. For instance, Figure 3.8 shows that the likelihood of leaving school by 15 are 4-5 percentage points less among the initially eligible cohorts of local Taiwanese people, while it is only 1-2 percentage points for Mainlanders.

Table 3.4 shows heterogeneous effects of the compulsory schooling law by the ethnicity. The first four columns show the results for local Taiwanese people, and the other columns show the results for Mainlanders. Still, the results are robust to different specifications within each group, buts show a great difference across groups.

According to Column (2) and Column (6) of Table 3.4, the impact of compulsory education is estimated to increase the average years of schooling by 0.23 years for local Taiwanese people and 0.11 years for Mainlanders. Moreover, the policy reduces the likelihood of leaving school by age 15 by 4.3 percentage points for local Taiwanese people, but only 1.1 percentage points for Mainlanders. Also similar difference can be observed in the likelihood of leaving school by age 18. However, if different means of educational outcomes between local Taiwanese and Mainland Chinese are taken into account, these gaps across provinces of origin are not as large as those of genders, only slight differences are observed. For instance, the initially eligible cohorts of local Taiwanese are 8.9% less likely to leave school by age 15, while it is 6.2% for Mainlanders.

3.5 Concluding Remarks

This paper estimates the causal impact of Taiwan's compulsory schooling law on students' educational outcomes. Using a unique census with detailed information on the exact birthdate, this paper exploits a regression discontinuity design to investigate the differences in years of schooling and dropout probabilities between before and after cohorts. The huge sample of the Taiwanese population allows me to perform a very precise estimate, which is robust to different specifications.

Major findings are as follows. First, the compulsory schooling law significantly affects students' educational outcomes. The average years of schooling are 0.22 years higher among the initially eligible cohorts, compared to those who are not eligible for the policy. Despite the extension in years of schooling, the policy also reduces 8.9% of the probability of dropout by age 15 and 3.3% of the probability of dropout by age 18. Although education after junior high school is not required by the compulsory schooling law, a long-term effect of the policy is observed. Other than compelling students to complete the mandatory years of education, the policy also affects people's further decisions of education by offering more educational opportunities and knowledge, the latter of which opens their sights in the long term.

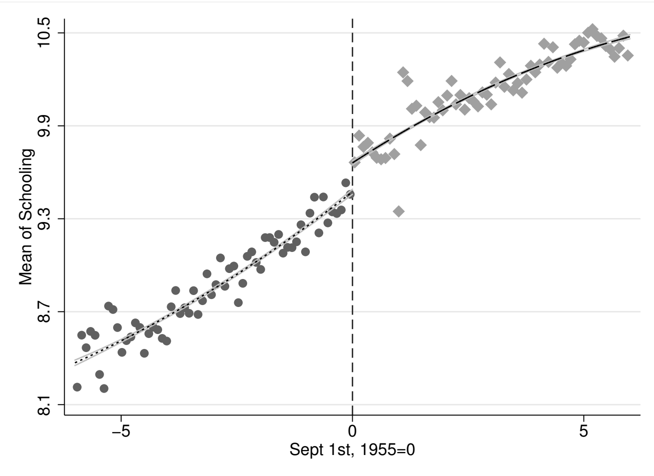
Second, a heterogeneous effect is found between males and females. The compulsory schooling law shows a larger effect on males, increases their years of schooling by 0.27 years, while female schooling is only extended by 0.18 years. Similarly, males are 12.2% less likely to drop out from school by age 15, the corresponding estimate on females is only 6.5%. However, most studies using a regression discontinuity design have found a larger effect for females regarding compulsory education. Finally, the impact of compulsory education shows a larger effect on local Taiwanese people than Mainlanders (the second generation immigration). The average years of schooling are 0.23 years longer among initially eligible local Taiwanese people, while the policy only increases schooling of the corresponding Mainlanders by 0.11 years.

TABLE 3.1: *Descriptive Statistics*

VARIABLES	(1) Mean	(2) Std. Dev.	(3) Min	(4) Max	(5) Obs
Birth year	1955.448	3.435	1949	1961	4,194,651
Age	24.544	3.434	19	31	4,194,651
Years of schooling	9.554	3.704	0	18	4,194,651
Female	0.486		0	1	4,194,651
Eligible(=1 if born after Sept.1 1955)	0.535		0	1	4,194,651
Leaving school by age 15	0.375		0	1	4,194,651
Leaving school by age 18	0.561		0	1	4,194,651
Mainlander	0.124		0	1	4,194,651

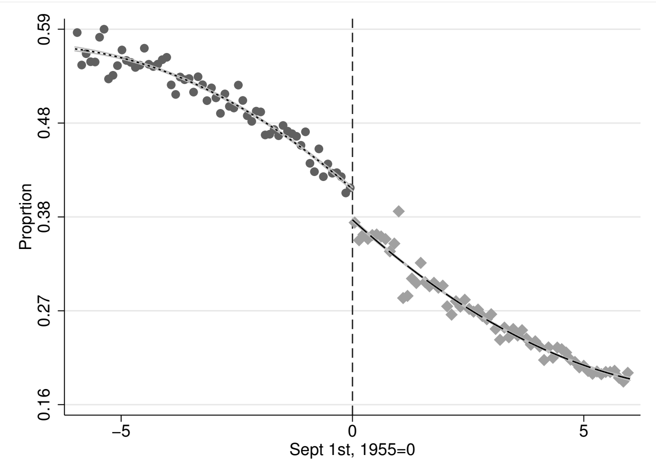
Note: The author's calculation by 1980 Population and Housing Census of Taiwan. The sample includes all individuals born on either side of the eligibility cutoff with a 6-year bandwidth.

FIGURE 3.1: *Schooling Years*



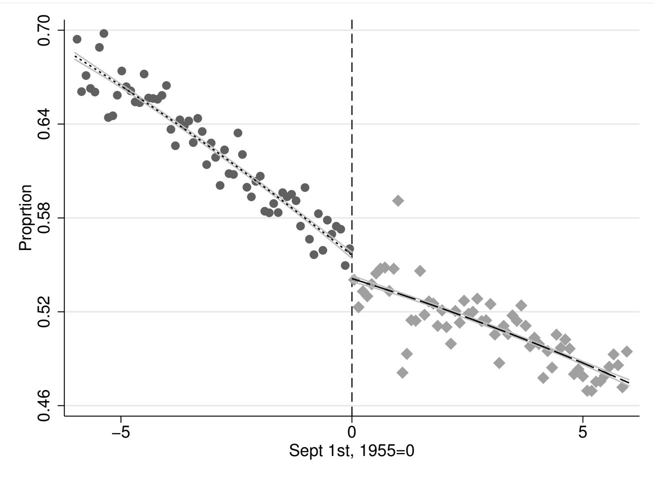
Notes: The X-axis indicates exact birthdates relative to the eligibility cutoff of the compulsory schooling law, which are adjusted into decimal years. September 1st in 1955 is treated as 0. Quadratic polynomial fit is used.

FIGURE 3.2: *Leaving School by Age 15*



Notes: The X-axis indicates exact birthdates relative to the eligibility cutoff of the compulsory schooling law, which are adjusted into decimal years. September 1st in 1955 is treated as 0. Quadratic polynomial fit is used.

FIGURE 3.3: *Leaving School by Age 18*



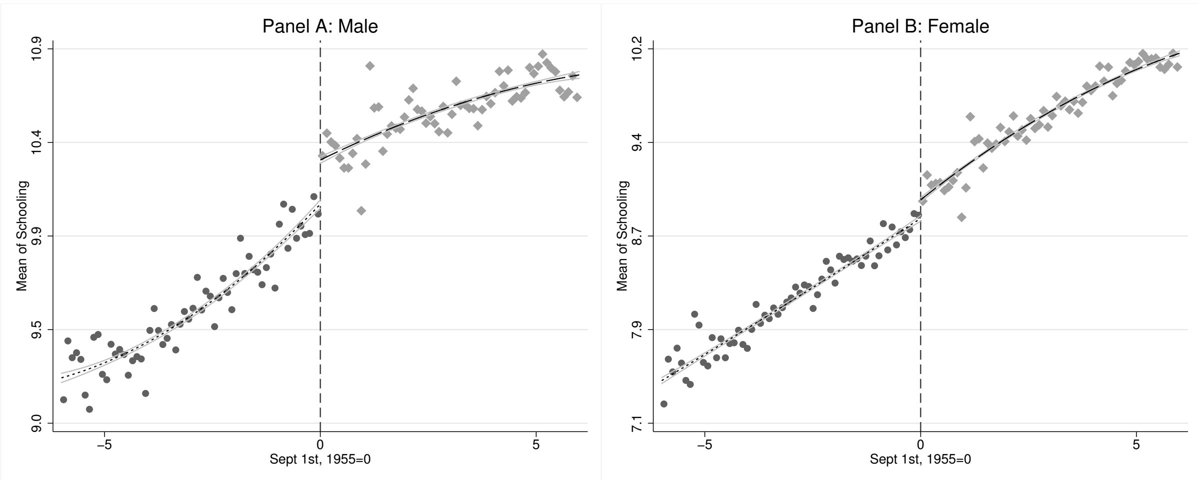
Notes: The X-axis indicates exact birthdates relative to the eligibility cutoff of the compulsory schooling law, which are adjusted into decimal years. September 1st in 1955 is treated as 0. Quadratic polynomial fit is used.

TABLE 3.2: *RDD Estimates of 1968 Compulsory Education on Educational Outcomes*

	Functional forms				Local linear windows			
	(1) Quadratic	(2) Cubic	(3) Quartic	(4) Quintic	(5) 1-year	(6) 2-year	(7) 3-year	(8) 4-year
Years of schooling	0.292***	0.206***	0.205***	0.239***	0.357***	0.219***	0.240***	0.257***
	(0.007)	(0.010)	(0.010)	(0.012)	(0.018)	(0.013)	(0.010)	(0.009)
Mean of control group	8.892	8.892	8.892	8.892	9.361	9.254	9.157	9.061
Left school by age 15	-0.072***	-0.043***	-0.044***	-0.038***	-0.053***	-0.040***	-0.045***	-0.054***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)
Mean of control group	0.504	0.504	0.504	0.504	0.428	0.449	0.467	0.481
Left school by age 18	-0.016***	-0.015***	-0.014***	-0.022***	-0.038***	-0.019***	-0.019***	-0.018***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Mean of control group	0.620	0.620	0.620	0.620	0.568	0.579	0.589	0.600
Observations	4,194,651	4,194,651	4,194,651	4,194,651	737,569	1,424,625	2,122,257	2,835,423

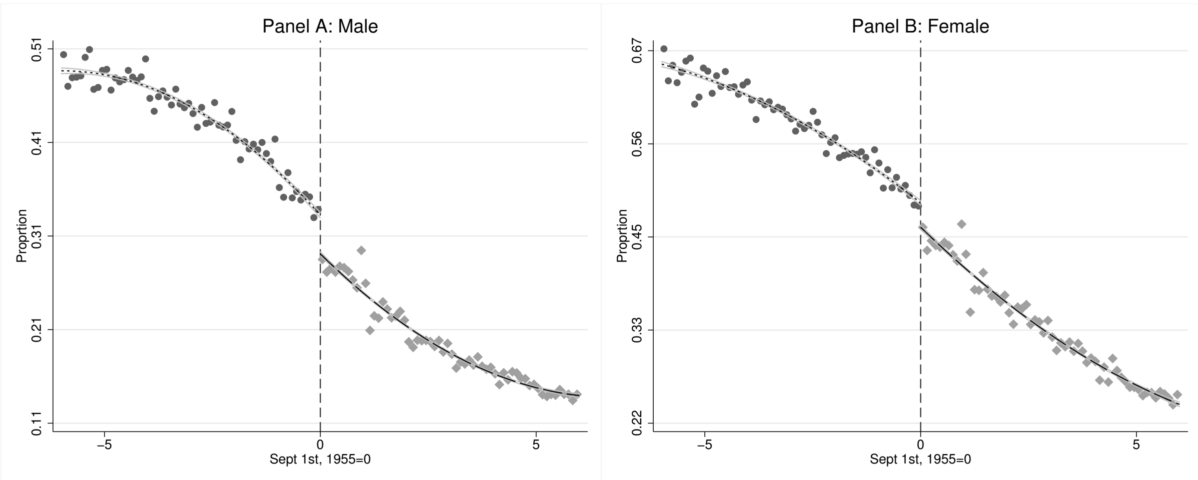
Notes: Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

FIGURE 3.4: *Schooling Years Graphed by Sex*



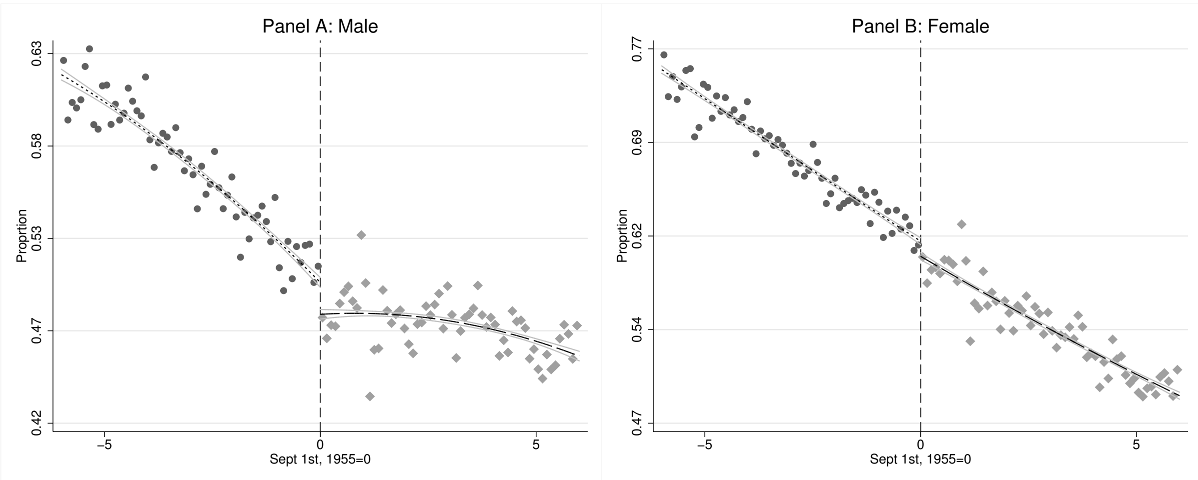
Notes: The X-axis indicates exact birthdates relative to the eligibility cutoff of the compulsory schooling law, which are adjusted into decimal years. September 1st in 1955 is treated as 0. Quadratic polynomial fit is used.

FIGURE 3.5: *Leaving School by Age 15 Graphed by Sex*



Notes: The X-axis indicates exact birthdates relative to the eligibility cutoff of the compulsory schooling law, which are adjusted into decimal years. September 1st in 1955 is treated as 0. Quadratic polynomial fit is used.

FIGURE 3.6: *Leaving School by Age 18 Graphed by Sex*



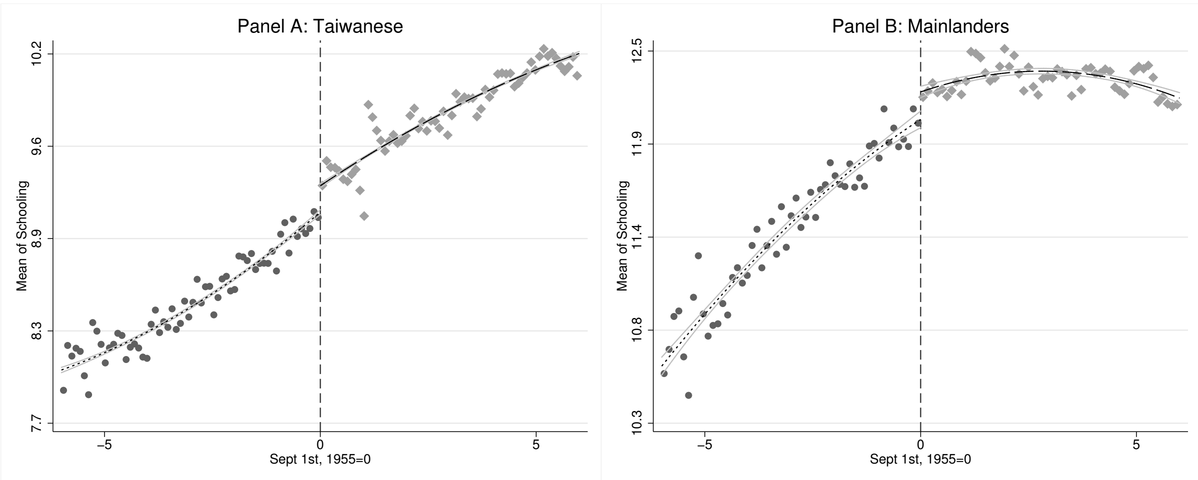
Notes: The X-axis indicates exact birthdates relative to the eligibility cutoff of the compulsory schooling law, which are adjusted into decimal years. September 1st in 1955 is treated as 0. Quadratic polynomial fit is used.

TABLE 3.3: *RDD Estimates of 1968 Compulsory Education on Educational Outcomes, by Sex*

	Male				Female			
	Polynomial	Local Linear Windows			Polynomial	Local Linear Windows		
	(1) Quadratic	(2) 2-year	(3) 3-year	(4) 4-year	(5) Quadratic	(6) 2-year	(7) 3-year	(8) 4-year
Years of schooling	0.340***	0.265***	0.283***	0.304***	0.242***	0.177***	0.200***	0.211***
	(0.010)	(0.017)	(0.014)	(0.012)	(0.011)	(0.018)	(0.015)	(0.013)
Mean of control group	9.603	9.906	9.817	9.736	8.141	8.565	8.458	8.348
Left school by age 15	-0.086***	-0.046***	-0.052***	-0.064***	-0.057***	-0.034***	-0.039***	-0.044***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Mean of control group	0.432	0.378	0.396	0.411	0.580	0.525	0.541	0.556
Left school by age 18	-0.018***	-0.023***	-0.022***	-0.021***	-0.013***	-0.015***	-0.016***	-0.015***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Mean of control group	0.562	0.522	0.533	0.544	0.682	0.638	0.649	0.660
Observations	2,156,225	731,349	1,089,938	1,456,773	2,038,426	693,276	1,032,319	1,378,650

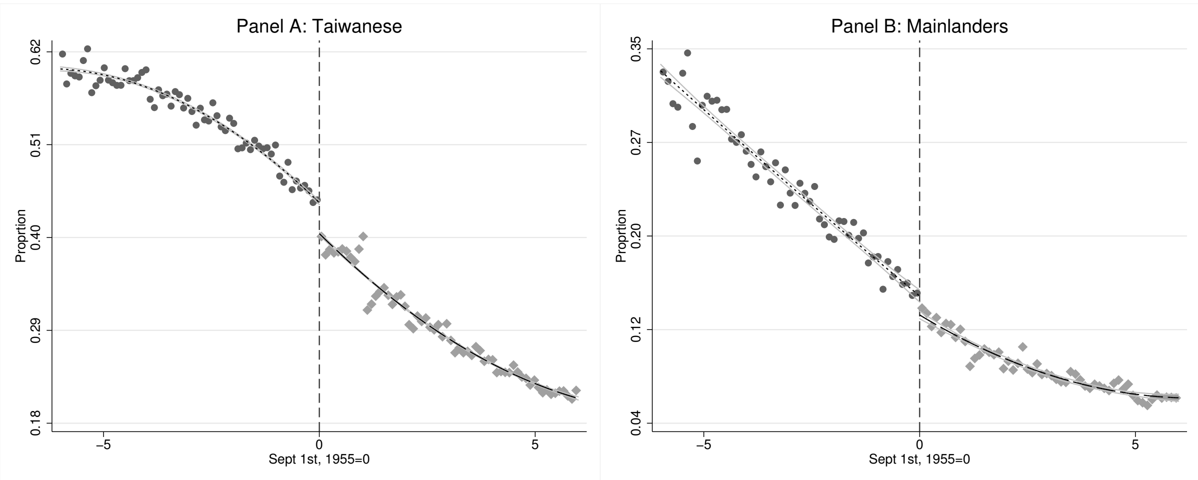
Notes: Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

FIGURE 3.7: *Schooling Years Graphed by Ethnicity*



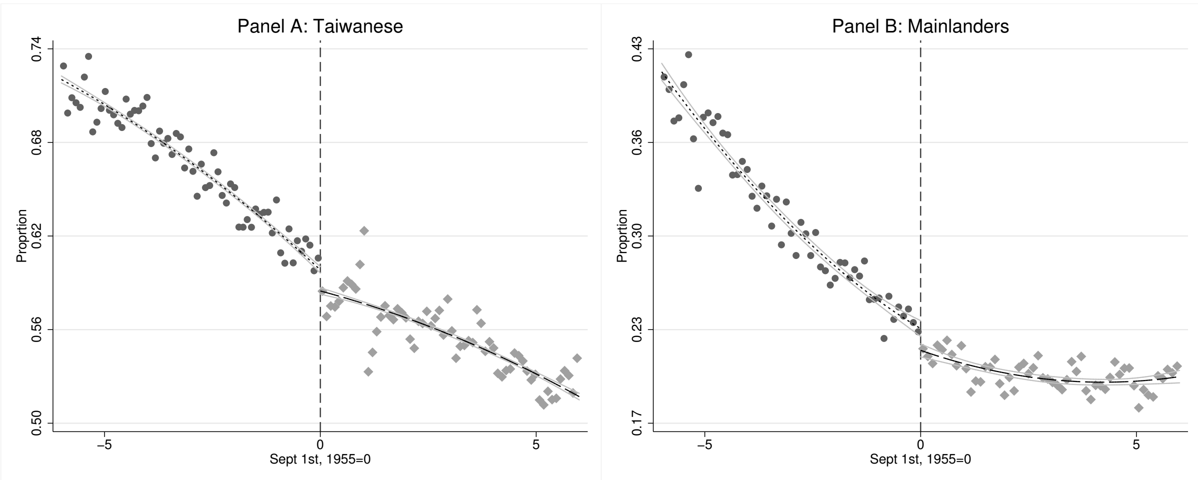
Notes: The X-axis indicates exact birthdates relative to the eligibility cutoff of the compulsory schooling law, which are adjusted into decimal years. September 1st in 1955 is treated as 0. Quadratic polynomial fit is used.

FIGURE 3.8: *Leaving School by Age 15 Graphed by Ethnicity*



Notes: The X-axis indicates exact birthdates relative to the eligibility cutoff of the compulsory schooling law, which are adjusted into decimal years. September 1st in 1955 is treated as 0. Quadratic polynomial fit is used.

FIGURE 3.9: *Leaving School by Age 18 Graphed by Ethnicity*



Notes: The X-axis indicates exact birthdates relative to the eligibility cutoff of the compulsory schooling law, which are adjusted into decimal years. September 1st in 1955 is treated as 0. Quadratic polynomial fit is used.

TABLE 3.4: *RDD Estimates of 1968 Compulsory Education on Educational Outcomes, by Ethnicity*

	Taiwanese				Mainlanders			
	Polynomial	Local Linear Windows			Polynomial	Local Linear Windows		
	(1) Quadratic	(2) 2-year	(3) 3-year	(4) 4-year	(5) Quadratic	(6) 2-year	(7) 3-year	(8) 4-year
Years of schooling	0.294***	0.232***	0.243***	0.251***	0.178***	0.108***	0.182***	0.202***
	(0.007)	(0.013)	(0.011)	(0.009)	(0.020)	(0.035)	(0.028)	(0.024)
Mean of control group	8.542	8.901	8.800	8.702	11.468	11.903	11.808	11.705
Left school by age 15	-0.077***	-0.043***	-0.048***	-0.057***	-0.024***	-0.011***	-0.017***	-0.022***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)	(0.002)
Mean of control group	0.541	0.485	0.504	0.519	0.234	0.177	0.191	0.205
Left school by age 18	-0.015***	-0.020***	-0.019***	-0.017***	-0.011***	-0.009**	-0.016***	-0.016***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.004)	(0.003)	(0.003)
Mean of control group	0.662	0.621	0.632	0.644	0.310	0.258	0.268	0.281
Observations	3,672,493	1,253,251	1,863,109	2,486,451	522,158	171,374	259,148	348,972

Notes: Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Chapter 4

Estimates of the Returns to Schooling in Taiwan: Evidence from a Regression Discontinuity Design

4.1 Introduction

Education has a crucial implication in human society, as it has long been of interest to policymakers, economists, and individuals themselves. On the national perspective, policymakers often assert that public expenditure on education may lead to poverty reduction, and promote economic growth. Economists have proposed that the growth can be explained by private returns to greater human capital, as more education enhances the skills and capacities of the labor force. On the individual perspective, students and their families also make educational choices with financial resources that they are able or willing to afford. With a limited budget, parents tend to invest more in the children who are expected to have higher earnings as adults.

Taiwan is an interesting case in understanding the relationship between education and economic outcomes. Since 9-year compulsory education was introduced in 1968, Taiwan has experienced a rapid economic growth with a low inflation rate for nearly half a century. As shown in Figure 4.1, its GDP per capita has increased to $22,294 in 2015 from $154 in 1951. This impressive economic growth is thought to be boosted by the national compulsory education reform(Jennie Hay 1991; Liu and Armer 1993; Lin 2003), which accumulates the human capital on the long-term. Following Mincer (1974), Wu (2003) estimates the Mincer wage equation and have found the return to education by

5.0% - 7.8% in Taiwan during 1978 to 2001. Liu et al. (2000) finds that family background is a substantial input of the wage function, and the return to schooling is 8.04% for married Taiwanese men in 1990, after controlling parents' and wife's education. Chuang and Chao (2001) finds that returns to schooling are heterogeneous across samples with different final educational attainments[1], and females have higher returns to schooling than males in Taiwan.

Despite the accumulation of literature on returns to schooling in Taiwan, it is challenging to establish the causality between education and wages. Specifically, unobserved factors such as ability and family backgrounds are heterogeneous across individuals. Those who are well educated may have higher wages as a result of higher ability and better family backgrounds. As ability (or family background) is positively correlated with both education and wages, estimates of returns to education by ordinary least squares (OLS) should be upward biased. Due to the endogeneity of educational choice arising from unobserved ability and family backgrounds, the true return to education in Taiwan remains an open question.

Since Angrist and Krueger (1991), economists have devoted a great deal of attention to correcting the omitted variable bias by instrumental variable (IV) method (Harmon and Walker 1995; Patrinos and Sakellariou 2005; Pischke and Wachter 2008; Oreopoulos 2007) or fuzzy regression discontinuity design (Devereux and Hart 2010). Card (2001) offers a survey of IV literature on returns to education, the IV estimates of returns to education are typically larger than the corresponding OLS estimates. Using German data, Pischke and Wachter (2008) exceptionally shows a zero return to education by exploiting the exogenous variation in years of schooling caused by the compulsory schooling law.

Using a large Taiwanese data set, we estimate the causal effect of schooling on earnings by a fuzzy regression discontinuity design. Specifically, Taiwan carried out a compulsory education reform in 1968, the mandatory years schooling were extended to 9 years from 6 years. Due to the compulsory schooling law, those who were born before and after 1955 have different probabilities to leave schools, thus have different years of schooling. Figure 2 shows there is a clear cutoff of schooling in 1955, which is the first cohort affected by the reform. Exploiting this natural experiment, we estimate the effect of

[1]2.30% for senior high school, 3.98% for vocational school, 4.58% for junior college, and 12.20% for university.

eligibility for the 9-year compulsory schooling law on years of schooling by a regression discontinuity design (RDD) at the first stage, and then estimate the return to education. As a result, the fuzzy RDD estimates shows a very moderate return to education by approximately 4.7%, while previous IV studies show returns to education by 10% - 15%.

The remainder of this paper is organized as follows. In Section 4.2, we present the identification strategy used to estimate returns to education. Section 4.3 describes the data and descriptive statistics. Section 4.4 discuss the results of OLS and IV. Section 4.5 provides robustness checks for our results. Section 4.6 concludes.

4.2 Identification Strategy

To estimate the effect of years of schooling on wages, I firstly use an ordinary least squares (OLS) to fit the following benchmark model to the pooled cross-sectional data:

$$w_{it} = \beta_0 + \beta_1 S_{it} + \boldsymbol{Z}^{'}_{it}\beta_2 + \varepsilon_{it} \tag{4.1}$$

where w_{it} is log monthly wage for individual i in survey year t, S_{it} is defined as years of schooling. β_1 is the coefficient that captures the effect of years of schooling on wages. $\boldsymbol{Z}_{it}$ is a vector of control variables including gender, marital status, year dummies, tenure, and tenure squared. ε_{it} is the error term.

The coefficient β_1 that measures the returns to education could be biased from omitted variables in Equation (1), which are related to both schooling and wages. For instance, individuals that are well educated may have higher wages as a consequence of higher ability, better family backgrounds and so forth. Without holding these usually unobserved inputs of wages constant, returns to education estimated by OLS suffers from an upward bias.

To address the endogeneity of educational choice, we use the change in compulsory schooling law to identify the causal effect of schooling on wages by a fuzzy regression discontinuity design. The new policy that raised mandatory years of schooling from 6 to 9 in 1968 generates a discontinuous change in individuals' educational outcomes across cohorts born before and after 1955. Using this exogenous variation in schooling, the estimate of interest, δ_1, can be defined in terms of limits under the local average treatment effect (LATE) framework as follows:

$$\delta_1 = \lim_{\Delta \to 0} \frac{E[w_{it}|X_0 < X_{it} < X_0 + \Delta, \boldsymbol{Z}_{it}] - E[w_{it}|X_0 - \Delta < X_{it} < X_0, \boldsymbol{Z}_{it}]}{E[S_{it}|X_0 < X_{it} < X_0 + \Delta, \boldsymbol{Z}_{it}] - E[S_{it}|X_0 - \Delta < X_{it} < X_0, \boldsymbol{Z}_{it}]} \tag{4.2}$$

where w_{it} and S_{it} represent log monthly wage and schooling, respectively. X_{it} is the year of birth, and X_0 is 1955 that the first cohort will be affected by the new compulsory schooling law. $\boldsymbol{Z}_{it}$ is the same vector of control variables as in Equation (1). Equation (2) captures the causal effect on compliers, defined as individuals whose eligibility for the new policy changes as we just move the value of X_{it} from $X_{it} - \Delta$ to $X_{it} + \Delta$, where Δ is very close to X_0.

As Imbens and Lemieux (2008) suggest that using a high-order polynomial function could lead to misleading results in RD design, we use the local linear regression approach to estimate δ_1 alternatively. At the first stage, we use the eligibility for 9-year compulsory schooling law to instrument S_{it} variable, and then use the predicted values of S_{it} to draw the causality between schooling and wages. In practice, a two-stage least squares (2SLS) system of equations is used to perform the fuzzy regression discontinuity design as follows:

$$w_{it} = \delta_0 + \delta_1 S_{it} + \boldsymbol{Z}_{it}^{'}\delta_2 + u_{it} \tag{4.3}$$

$$S_{it} = \gamma_0 + \gamma_1 E_{it} + \gamma_2 \tilde{X}_{it} + \gamma_3 E * \tilde{X}_{it} + \boldsymbol{Z}_{it}^{'}\gamma_4 + \epsilon_{it} \tag{4.4}$$

where Equation (3) is similar to Equation (1), and Equation (4) is the first stage of 2SLS. E_{it} is a binary instrumental variable that equals to 1 if individual i was born after 1955 in time t and 0 otherwise, which indicates the eligibility for education reform implemented in 1968. $\tilde{X}_{it} = X_{it} - X_0$, and X_0 is 1955. $\boldsymbol{Z}_{it}$ is the same vector of control variables as in Equation (1). u_{it} and ϵ_{it} are the error terms. In these local linear regressions, we use bandwidths of Δ-neighborhood with 3-year, 4-year, and 5-year. Under the identifying assumption that other determinants of wages change smoothly around the eligibility cutoff, δ_1 would be an unbiased estimates of returns to education.

4.3 Data

The data used in this research is Manpower Utilization Survey 1995-2015 of Taiwan. The survey has been collected annually since 1979, and each wave is a cross-sectional data. The survey contains basic information on individual characteristics and labor market activities. This research uses the pooled cross-sectional data of 20 years between 1995 and 2015 for following reasons. First, as educational attainment over university level can not be distinguished before 1995, we use data posterior to 1995 to avoid measurement errors in recoding categorical educational attainment into equivalent years of schooling. Second, we have to ensure the observed educational attainment is not censored at a survey year. After 1995, the initially eligible cohorts of the 9-year compulsory schooling law would be at least 35 years old, and have few probabilities to attend school.

Each wave of Manpower Utilization Survey contains approximately 50,000-60,000 individuals. The huge sample size enables us to get sufficient observations around the cutoff to perform a fuzzy regression discontinuity design. For the analysis of returns to education, we restrict the sample as follows: (1) Monthly wages are used to measure earnings, observations with missing wages are dropped. To get a relatively homogenous sample, top 1.25 percentile and bottom 1.25 percentile are also dropped. (2) We only use observations whose information on year-of-birth, gender, marital status, educational attainment is not missing. Year-of-birth is the forcing variable that exogenously determines schooling across cohorts. (3) As tenure could be an important input of wages, we only use individuals whose tenures are not missing. The data offer exact tenure variable with the accuracy of months. We convert the tenure variable into decimal years.

The descriptive statistics of the final samples used to estimate the OLS and fuzzy RD are shown in Table 4.1. Full sample is used to estimate the OLS and global polynomial of RD, discontinuity samples with 3-year, 4-year and 5-year bandwidths are used to estimate the local linear RD. The discontinuity samples are very similar across different bandwidths. From Column (3), the average age of the discontinuity sample is approximately 10.2, 50% of them benefit from the 9-year compulsory schooling law, and the average tenure is 12.2 years . As the discontinuity samples contain those born just before and after 1955, approximately 84% of them are married in the survey years. About 66%

are males in the discontinuity samples because males have higher labor force participation rate than females in Taiwan. Naturally, observations in the full sample are averagely younger than those in the discontinuity samples, and they have longer schooling and shorter periods of tenure slightly.

4.4 Results

In this section, I discuss the estimates of the fuzzy regression discontinuity design, which were designed to analyze returns to education in Taiwan. I first discuss the first stage results, and then compare the OLS and IV estimates. To understand the difference between our estimates and those in previous studies, I suggest two possible channels that may result in the different magnitude in IV estimates, while our OLS estimates are comparable to those found in U.S. and Europe. Table 2 displays the fuzzy RD estimates of the return to education, along with the first stage results. Both fuzzy RD estimates of global polynomial and local linear regression are reported. we use a quadratic form for global polynomial, and 3-year, 4-year and 5-year bandwidths for local linear regression. OLS estimates are also reported in Column (1) for comparison. From top to bottom, three panels are listed for specifications with different control variables.

4.4.1 First Stage

Figure 4.2 provides a graphical evidence of the first stage, which shows the effect of the compulsory schooling law on years of schooling. The relationship between birth years and years of schooling is plotted, with linear fit and 95% confidence interval attached. We use circles and crosses to indicate average years of schooling for those who were born before and after 1955, respectively. The figure shows a clear discontinuous change in average schooling around the eligibility cutoff. Optically, schooling is 0.3-0.4 years longer among initially eligible cohorts. While we can only measure the eligibility at the year-of-birth level, we also use 1980 Population and Housing Census of Taiwan to examine the jump at the cutoff point. The first stage result calculated from the census is shown in Table 4.3, which measures the eligibility at the date-of-birth level. Out first stage results from Manpower Utilization Survey (Figure 4.2) are consistent with the census (Figure 4.3), show a discontinuity at 1955. Note that in the Manpower Utilization Survey, wages are only observable for individuals that are working, observed average years of schooling are longer among these workers than the average population from the census.

Columns (3), (5), (7) and (9) of Table 4.2 present the first stage results of fuzzy RDD estimates, the effect of compulsory schooling law on individuals' years of schooling. Estimates from local linear regression are our main results, indicating longer schooling among initially eligible cohorts by 0.17-0.33 years. Column (7) with a 4-year window is our preferred bandwidth, and the results are also robust to specifications with different control variables. For instance, from Panel C of Column (7), the eligibility of 9-year compulsory education increases the average years of schooling by approximately 0.26. This is consistent with the pattern observed in Figure 1, where there is a discontinuous increase in years of schooling at 1955 birth cohort. The first stage from global polynomial approach is also listed in Column (3) for comparison, which tends to exaggerate the effect of compulsory schooling law.

4.4.2 IV and OLS estimates

From Column (2), (4), (6) and (8) of Table 4.2, we find a significant positive education return for IV estimates, regardless of functional forms or different local linear bandwidths used. From Panel C in Column (6), education is estimated to raise the wage by approximately 4.7%, when tenure variables (tenure and tenure squared) are controlled in regression. However, Panel A and Panel B in the same column show that an additional year of schooling is estimated to increase the wage by only approximately 3.3% when tenure variables are not controlled. Similar patterns can also be observed in other specifications of fuzzy regression discontinuity design in Column (2), (4) and (8). Our results indicate that omitting the tenure and tenure squared variables may cause a huge bias of estimated returns to education, even a natural experiment is used to address the endogeneity of educational choice. Due to some data limitation, few IV studies of the returns to education has controlled for tenure.

Before we discuss the differences between OLS and IV estimates, I would offer some literature review in returns to education. For instance, Angrist and Krueger (1991) finds a 7.8% return to education for 1940-1949 male cohorts in the United States by a birth-of-quarter instrument interacted with year-of-birth, while the corresponding OLS is only 5.2%. Using changes in minimum school leaving age, Harmon and Walker (1995) shows an IV coefficient of 15.3% and an OLS coefficient of 6.1% for British men. Similar evidences are also confirmed in Ireland (Callan and Harmon 1999), Netherlands (Levin and Plug 1999), Canada (Lemieux and Card 2001), Austria and Germany (Ichino and Winter-Ebmer 2004). Furthermore, outside the North America and Europe, Duflo (2001) shows an IV estimate of 10.6% and an OLS estimate of 7.7% in Indonesia, which uses the interaction of birth cohorts and school construction intensity as the instrument. Pischke and Wachter (2008) is among the very few that shows a zero return to education, which uses the changes in compulsory schooling law in Germany. Briefly, in the IV literature of return to education, previous studies often find the magnitude of IV estimates are approximately 8-15%, larger than the OLS estimates that are 5-8%.

However, why are the IV estimates of returns to education larger than the corresponding OLS estimates? As unobserved ability bias is predicted to be positively related to both education and wages, which will result in upward bias in OLS estimates, the IV estimates should be smaller than the OLS estimates. Card (2001) suggests that IV estimates using institutional features of the supply side of the education system usually represent local average treatment effects among families with relatively strict constraints, and tend to be different from the OLS that estimates the return to education on average. Returns to education in these families will be mostly limited to the marginal costs of schooling if supply sides of the education system (e.g. compulsory schooling laws, new school openings) are not expanded.

While our OLS estimates are comparable to the literature, our IV estimates only indicate very moderate returns to education. This magnitude of our IV estimates relative to the OLS is different with most previous studies. Specifically, our OLS estimates consistently suggest return to education by 5.7-5.9% across different specifications in Column (1), very close to the 5.2% returns to education of 1940-49 cohort for U.S. males (Angrist and Krueger 1991) and the 6.1% returns to education for UK males. (Harmon and Walker 1995). On the contrary, our IV estimates are only 4.7% from Panel C in Column (6), smaller than those in U.S. (7.8% for 1940-1949 cohort) and in UK (15.3%).

We consider two possible interpretations for this difference with the literature. First, Taiwan's education system is very exam-oriented, and parents would sacrifice own expenditures to support children's schooling altruistically due to traditional values of family. Marginal returns to education among those who acquired additional schooling as a result of the compulsory schooling law are not very high, the low ability, rather than the marginal costs of schooling may limit their return to education.

Second, our instrument may include much less unspecified time trend or structural changes than those instruments used in the literature. As we uses the 1968 compulsory education reform of Taiwan to estimate the return to education, our sample contains the first eligible cohorts born just before and after 1955, when the World War II had already been ended. Conversely, for instance, the UK had raised the minimum school-leaving-age in 1947 and 1973, the former of which was only two years after the World War II. When applying the 1947 reform (raising the minimum school-leaving-age from 14 to 15) instrument in the context of UK, control groups of those who were over 15 years old in 1947 (born after 1932) had higher chances to be affected by the war during early 1940s, potentially received fewer education and wage loss in their later lives. Ichino and Winter-Ebmer (2004) confirms that who were about 10 years old during the World War II, or were directly involved through their parents, experienced a long-term loss in schooling and wages. Furthermore, the earnings of individuals born in 1930s might have been affected by the Great Depression, if this event would have long-term affects through the loss of early childhood human capital.

4.5 Robustness

4.5.1 Placebo Effect

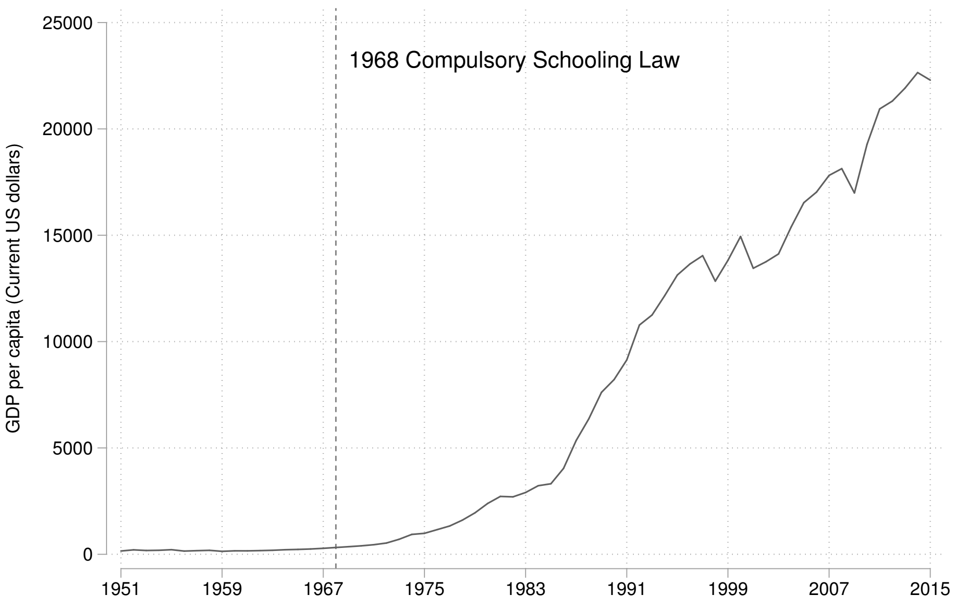
In this section, we construct placebo reforms to examine the robustness of the fuzzy RD estimates. These placebo treatments are three years prior, one year prior, one year posterior, three years posterior to the actual compulsory education reform. These placebo reforms should not have any effect on wages. If we find an impact of a placebo reform, the actual reform based results might be driven be other unobserved factors instead of the compulsory schooling law.

We estimates the reduced form regression to test the placebo effects, as the placebo treatments will have no effect on years of schooling by definition. Table 4.3 reports the reduced form estimates for wages, with a 3-year, 4-year and 5-year bandwidth, respectively. All of the estimates are not significant at 10% level. The place reforms of 1952, 1954, 1956 and 1958 show no effects on the wages across different bandwidths.

4.6 Concluding Remarks

In this study, we exploit the discontinuity in years of schooling induced by an extension of compulsory schooling law to estimate the returns to education. We find that the eligibility of 9-year compulsory education increases the average schooling by 0.26, among those who acquired additional schooling as a result of the reform. Subsequently, we show that this discontinuity in years of schooling leads to a significant increase in wages. An additional schooling is estimated to raise the wage by approximately 4.7%. Contrast to previous IV studies, the fuzzy RDD estimates (essentially an IV method) in this research are smaller than the corresponding OLS estimates. Our results are robust to different specifications and placebo tests.

FIGURE 4.1: *Trend in GDP per capita*



Source: National Statistics of Taiwan

FIGURE 4.2: *Average Years of Schooling by Year-of-Birth*

Notes: Author's calculation by Manpower Utilization Survey 1995-2015 of Taiwan

FIGURE 4.3: *First Stage Evidence from Census*

Notes: Author's calculation by 1980 Population and Housing Census of Taiwan.

TABLE 4.1: *Descriptive Statistics*

VARIABLES	(1) Full Sample	(2) ±3 Sample	(3) ±4 Sample	(4) ±5 Sample
Year of birth	1965.31	1955.19	1955.35	1955.57
	(12.60)	(1.99)	(2.55)	(3.08)
Years of schooling	11.55	10.18	10.21	10.26
	(3.55)	(3.58)	(3.56)	(3.54)
log wage	10.30	10.37	10.37	10.36
	(0.47)	(0.51)	(0.50)	(0.50)
Eligible (=1 if born after 1955)	0.78	0.47	0.50	0.53
	(0.41)	(0.50)	(0.50)	(0.50)
Age	39.46	48.34	48.20	47.98
	(11.82)	(6.09)	(6.25)	(6.42)
Male	0.62	0.67	0.66	0.66
	(0.49)	(0.47)	(0.47)	(0.47)
Married	0.62	0.85	0.84	0.84
	(0.49)	(0.36)	(0.36)	(0.37)
Tenure	8.64	12.27	12.20	12.11
	(8.91)	(8.97)	(8.97)	(8.96)
Observations	585,103	90,464	116,017	140,381

Data source: Manpower Utilization Survey 1995-2015 of Taiwan

TABLE 4.2: *RDD Estimates of Education Return*

		Polynomial		Local Linear					
		Quadratic		Discontinuity ± 3		Discontinuity ± 4		Discontinuity ± 5	
	OLS	IV	1st Stage	IV	1st Stage	IV	1st Stage	IV	1st Stage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	log wage	log wage	Schooling	log wage	Schooling	log wage	Schooling	log wage	Schooling
Panel A: Basic controls									
Years of schooling	0.057***	0.044***		0.032***		0.033***		0.033***	
	(0.000)	(0.000)		(0.003)		(0.002)		(0.002)	
Eligible			2.034***		0.168***		0.263***		0.320***
			(0.017)		(0.051)		(0.043)		(0.038)
Panel B: Basic controls+Year FE									
Years of schooling	0.057***	0.041***		0.032***		0.033***		0.033***	
	(0.000)	(0.000)		(0.003)		(0.002)		(0.002)	
Eligible			0.630***		0.168***		0.263***		0.319***
Panel C: Basic controls+Year FE+Tenure									
Years of schooling	0.059***	0.053***		0.046***		0.047***		0.048***	
	(0.000)	(0.000)		(0.003)		(0.002)		(0.002)	
Eligible			0.643***		0.167***		0.260***		0.317***
			(0.018)		(0.051)		(0.043)		(0.038)
Observations	585,103	585,103	585,103	90,464	90,464	116,017	116,017	140,381	140,381

Notes: Standard errors in parentheses. In Panel A, basic demographic variable of gender and marital status are controlled. In Panel B, basic controls and year fixed effect are included. In Panel C, tenure and tenure squared variables are additionally controlled. *** p<0.01, ** p<0.05, * p<0.1

TABLE 4.3: *Reduced Form Estimate of Placebo Treatment on Wage*

VARIABLES	±3 Sample (1) log wage	±4 Sample (2) log wage	±5 Sample (3) log wage
Panel A: -3 Years (1952)			
Placebo Treatment	0.009	0.004	0.004
	(0.008)	(0.007)	(0.006)
Observations	73,718	93,718	112,963
Panel B: -1 Years (1954)			
Placebo Treatment	-0.003	0.002	0.002
	(0.007)	(0.006)	(0.005)
Observations	85,729	108,503	131,984
Panel C: +1 Years (1956)			
Placebo Treatment	0.003	0.002	-0.003
	(0.007)	(0.005)	(0.005)
Observations	94,869	122,342	148,440
Panel D: +3 Years (1958)			
Placebo Treatment	-0.004	-0.000	-0.005
	(0.006)	(0.005)	(0.004)
Observations	103,961	132,874	161,474

Note: Robust standard errors in parentheses. Gender, marital status, year fixed effect, tenure, tenure squared are included in all regressions. Each coefficient denotes a separate regression. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

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