UNIVERSITY OF OKLAHOMA

GRADUATE COLLEGE

THREE APPLIED ESSAYS ON CHINESE HOUSEHOLDS

A DISSERTATION

SUBMITTED TO THE GRADUATE FACULTY

in partial fulfillment of the requirements for the

Degree of

DOCTOR OF PHILOSOPHY

By

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Norman, Oklahoma
2020

THREE APPLIED ESSAYS ON CHINESE HOUSEHOLDS

A DISSERTATION APPROVED FOR THE

DEPARTMENT OF ECONOMICS

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Acknowledgments

I would like to express my sincere gratitude to my committee members: Dr. Gregory Burge, Dr. Pallab Ghosh, Dr. Joan Hicks, Dr. Cynthia Rogers, and Dr. Stephanie Burge. This dissertation would not have been accomplished without their helpful comments and suggestions. Special and my deepest thanks to my dissertation advisor, Dr. Gregory Burge, who is always patient, professional, and enthusiastic about my research. I could not make it without his insights and help. Also, I would like to thank my father Weihong Liu, and my mother Qin Wang. They have always been supportive through the whole process. Finally, I want to thank my future husband, who didn't showed up in the past five years, so I did not have to waste my time on him and could focus more on research.

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Abstract

My dissertation chapters focus on Chinese households decision making under specific government policies. The first chapter is a collaborative work with my advisor Dr. Gregory Burge. It focuses on a public healthcare program in rural areas of China. In 2003, China launched the New Cooperative Medical Scheme (NCMS), a public healthcare system intended to cover households in rural areas. While the existing literature focuses on how the NCMS program affected health outcomes and service utilization rates, this study examines the impact of the NCMS on labor supply in China's rural areas. The analysis focuses on average work hours of households, but also extends to related variables including household savings and time spent on household chores. We estimate difference-in-differences models, along with instrumental variables (2SLS) models, to mitigate selection issues and identify the effects of NCMS coverage on various work outcomes and related variables. Our main result is that the expanded coverage offered by the NCMS reduced aggregate labor supply (measured by hours worked) by roughly 8%. Interestingly, the effect is strongest for higher income households.

Chapter 2 is also a joint paper with Dr. Gregory Burge. It studies the parental decisions over the timing of primary school entry in China. Under the Chinese Compulsory Education Law, children reaching age 6 by August 31st are eligible for primary school entry, whereas

those born later must wait another year. This creates a discontinuity in the distribution of school starting ages (SSA) as it relates to biological age. While many studies have investigated the impact of SSA on various student outcomes, few have focused directly on parental decisions over the timing of school entry. This paper provides robust evidence that Chinese parents "redshirt" children who are relatively shorter, even controlling for the biological age of the child. Shorter children born in the summer and early fall started primary school five weeks later on average, when compared to their taller counterparts. Of note is the presence of the effect for children born after the cutoff, when presumably the policy should be prohibiting entry. Intuitively, we find no significant impact of height on SSA for children born in winter and spring that lie further away from the threshold. We also relate child size to test scores, finding that taller children perform better, even after controlling for their biological age, SSA, and other characteristics.

The third chapter also focuses on the primary school starting age, and studies its short run and longer run impacts. Since the August/September threshold creates a natural experiment and it allows me to apply a regression discontinuity model to study the impact of school entry age on different outcomes of interest. The short run outcomes includes test performances in Chinese language/grammar and Math, while the long run impacts contains years of schooling completed, average monthly wage, and age at first marriage. Using data from the China Family Panel Studies, I find delaying school entry could significantly improve the test performances on Chinese language/grammar, especially for girls. However, less evidence shows that starting school late could neither improve performances on Math, nor benefit individuals in the longer run by enhancing education attainment, average monthly wages and age at the first marriage.

Chapter 1

The Effects of the New Cooperative Medical Scheme on Labor Supply in Rural China

1.1 Introduction

Even as China has rapidly urbanized over recent decades, it still contains massive rural populations. According to the National Bureau of Statistics, as recently as 1989 the rural population was above 830 million, or nearly 74% of China's total population. Although rural residents have continued to migrate into urban areas over recent years, even as recently as 2011, half of the population still lives in rural areas. Figure 1 shows how the trend of rural and urban populations transitioned between 1989 and 2011. Unlike their urban counterparts who are more likely to work in industrial jobs, most rural residents are self-employed, engaging primarily in farming, fishing, hunting, and other activities that are best described as home production. Hence, self-employment income plays a significant role in households' finance, as many rural residents obtain money by selling food and other simple products.

While the Chinese economy has grown rapidly since 1978, when many fundamental economic reforms were implemented, improvements regarding the healthcare system in China have lagged behind other sectors. Before the Chinese market reformed from central-planned to more market-oriented, there were three types of public medical insurance that covered the nation: the Cooperative Medical System (CMS) dominated in rural areas, while the Government Insurance Scheme (GIS) and Labor Insurance Scheme (LIS) covered residents of urban areas. The GIS aimed at covering government employees, retirees, disabled veterans, university teachers, and students. The GIS was financed directly through government budgets, whereas the LIS was based on the use of enterprise welfare funds, covering private sector employees and their dependents (Yip and Hsiao, 2008). In rural areas, local governments administered the old CMS under a commune-based approach, essentially engaging in collective farming and cost sharing. Each local government was responsible for setting up health centers, hiring doctors and nurses for the village, and for providing medical facilities. The original CMS provided partial reimbursement to patients after they paid their initial out-of-pocket expenditures at the township or county level health facilities.

However, after the open Economic reform of 1978, China introduced the Household Responsibility System, which was essentially a system with no public insurance option that left households to interact with private insurance possibilities only. The old CMS system rapidly dissolved due to the collapse of the communes and insufficient funding. Over this period, approximately 90% of all rural residents became uninsured and had to pay 100% of their inpatient and outpatient expenditures by themselves when they were sick or injured. Additionally, the GIS and LIS healthcare scheme in urban areas were gradually replaced by the urban employee-based insurance (UE-BMI) and urban residents-basic medical insurance (UR-BMI) over the same period.

Table 1 documents several key characteristics associated with these three main insurance programs using information from the 2010 World Health Report. The UE-BMI covers urban employees only, more specifically through a system where it is mandatory for employers to pay the costs of coverage for every full-time worker who has an urban "Hukou" registration. For urban residents who are not employed, for example children, the elderly, and disabled persons, the only option is to voluntarily enroll in the UR-BMI scheme and cover their own premiums.

While these urban healthcare schemes are interesting and worth attention in other pursuits, this paper focuses on how enrollment in the New Cooperative Medical Scheme (NCMS) impacted residents in rural China. Specifically, we use panel data from the China Health and Nutrition Survey (CHNS) data to estimate difference-in-differences and Two-Stage Least Squares (Instrumental Variables) models to investigate the causal effect of the NCMS enrollment on labor supply. In order to mitigate expected issues related to selection bias and the non-random assignment of enrollment in the NCMS, we use county level program eligibility as our instrument variable in the 2SLS models, after verifying the required tests for a valid instrument are satisfied.

Our results suggest that the implementation of NCMS significantly reduced labor supply for households in China's rural areas. The baseline reaction in our preferred models registers NCMA coverage to reduce the number of hours worked by roughly 8%. Given previous findings in the literature that the NCMS did in fact improve health outcomes, it is possible at least a portion of this reduction in hours worked (i.e., the quantity of labor supply) is offset by an increases in labor productivity (i.e., the quality of labor supply) in this setting. Moreover, we further explored the potential mechanism driving this reduction in hours worked, finding that enrolling in NCMS significantly reduces saving levels among rural households. We also find heterogeneities exists across households from different income levels. We conclude that enrolling in the NCMS reduces both hours worked and savings levels, with the effects registering as the largest and most significant among households in the top income quartile.

1.2 Public Health Care in China and the NCMS

After the collapse of the commune system, there was no comprehensive public healthcare scheme covering rural residents in China. The overwhelming majority of rural peasants simply could not afford the massive expenditure associated with catastrophic illness by themselves. Cheng et al. (2015) document that according to the Chinese Ministry of Health, nearly 80 percent of rural residents were left uncovered without any private or public healthcare in 2003, forming a population of those uncovered at about 640 million individuals. In an effort to ensure rural peasants had increased access to basic healthcare services, and to help make catastrophic illness more affordable for residents in rural areas, the Chinese government implemented the New Cooperative Medical Scheme (NCMS) in 2003. It replaced the previous cooperative medical scheme that had collapsed and was designed with the intention of covering all of China's rural areas by 2010.

There were several meaningful differences between the New Cooperative Medical Scheme and the original commune based Cooperative Medical Scheme. Table 2 provides a brief summary of these differences. The NCMS was designed to be a completely voluntary, but also very heavily subsidized, public healthcare insurance scheme possessing three levels of cost/copayment coverage. Since rural residents are voluntarily able to buy into the NCMS healthcare coverage, selection issues immediately jump out as a potential problem plaguing empirical research investigating its effects. Specifically, individuals who are less healthy have a greater set of incentives to enroll a public healthcare, whereas the healthy may prefer to leave themselves exposed without enrolling. Put another way, the NCMS coverage is expected to greatly suffer from adverse selection problems. Moreover, even beyond the typical adverse selection concerns, healthcare coverage (and health care services in general) is generally recognized to behave as a normal economic good – meaning higher income individuals are more likely to purchase NCMS coverage than lower income individuals, even after controlling for levels of health and other observable traits. Fortunately, the NCMS coverage could only be purchased at the household level, rather than at the individual level, and we are also able to rely on a clean instrument that is exogenous to household level factors (i.e., phased in rollouts of county level NCMS eligibility). Still, this marks an unprecedented environment for healthcare in rural China, as the old CMS was universally funded by mandatory collections from villages/farmers. This calls attention to the importance of better understanding the effects of the new system.

The NCMS program was rolled out through a staggered process starting with several pilot counties in 2003. By September 2006, our data verifies that over half of China's rural counties had become eligible, and roughly 30.9% of China's total rural population were enrolled in NCMS (Dib, Pan and Zhang (2008)). By 2009, nearly all counties had become eligible for this healthcare program, with 90% of rural residents enrolled (Barber and Yao (2010)). Figure 2 provides a visual timeline illustrating how the various rural Chinese public healthcare systems developed over the transition over the past four decades.

When beginning the NCMS program in 2003, the Chinese Central government mandated that all pilot counties would be selected by the various provincial level governments. The selections were to follow four main criteria. First, interested county level governments should actively participate in advancing the implementation of NCMS, and should proactively participate in making a qualifying application and subsequently promoting the program. Moreover, the county applying should have relatively stable financial conditions, and residents should have relatively higher levels of income, so that they could afford the basic premiums – presumably to boost levels of participation early on. Furthermore, pilot counties should be equipped with relatively capable health administrative departments and adequate health care facilities/institutions. Finally, residents in those pilot counties should be highly motivated to participate, and counties should have sound grass-roots rural organizations to help introduce and promote the NCMS program to their residents. As a result, those pilot counties that first became eligible to participate in the NCMS program were certainly not randomly selected.

Even though counties that first became eligible for NCMS were not randomly selected, according to the state council in 2002, the NCMS was intended to cover 100 percent of the nation level rural population by the year with 2010. Figure 3 shows the number of actual enrollees along with the enrollment rate starting in the year 2004. The total number of NCMS enrollees was 80 million in 2004, constituting 75.2% of the national enrollment rate. Figure 3 also shows that by 2011, 97.5% of the rural counties became eligible for the NCMS and over 830 million individuals had joined.

The central government also implemented several guidelines for local governments to follow as they implemented the NCMS program. First, households should voluntarily decide whether to enroll in this healthcare program – no household should be forced or coerced into paying for coverage. Second, the local county government should directly administrate this program. Third, copayments should come from the central government, the county government, and individuals, and the insurance policy benefits should focus mainly on coverage of catastrophic injuries or illnesses, as opposed to carrying high fractional co-payment rates for more routine costs. Fourth, since the adverse selection principle dictates that less healthy individuals have a greater incentive to enroll, the NCMS program required full household level participation. For example, in a potentially multi-generational rural household, as is very common, either all of the household members were enrolled, or none of the members were enrolled in NCMS.

Figure 3 also shows that the average rural household expenditure on health care and medical services was only 3.25% of per capita consumption in 1990, before the implementation of NCMS, but then doubled to over 6.5% by 2005. There is an increasing trend for rural household expenditures going to health care and medical services, as well as the number of visits in township health centers both before and after the implementation of NCMS.

For the original source of financing NCMS, both central and local governments, as well as individuals, contributed equal portions of the cost of the insurance premium. In 2003, the annual premium was 30 RMB per household member, with 10 RMB from enrollees, and 10 RMB each from the central and local government. However, as the NCMS coverage grew rapidly, the government increased its subsidies in order to cover expanded services and attract greater enrollment. By 2010, the annual premium had increased to 120 RMB per person: 50 RMB each from the central and local government as well as 20 RMB from individuals.

Figure 3 also shows the average rural household expenditure on health care and medical services was only 3.25% of per capita consumption in 1990, before the implementation of NCMS, but then doubled to 6.58% by 2005. There is an increasing trend for rural household expenditures going to health care and medical services, as well as the number of visits in township health centers both before and after the implementation of NCMS. For the source of financing NCMS, both central and local governments, as well as individuals, contribute portions of the insurance premium. In 2003, the annual premium was 30 RMB per person, with 10 RMB from enrollees, and 10 RMB each from the central and local government. However, as the NCMS coverage grew rapidly, the government increased its subsidies in order to cover expanded services and attract greater enrollment. By 2010, the annual premium had increased to 120 RMB per person: 50 RMB each from the central and local government as well as 20 RMB from individuals. Since the NCMS program was implemented at the county level, counties were free to pick from any of the five benefit models for giving basic medical reimbursement. The first was inpatient reimbursement along with medical savings accounts (MSA). [i.e., a portion of household inpatient expenditures can be reimbursed by local and central governments, and participants of NCMS could use their medical saving account to pay outpatient expenditures and preventive care services.] According to an internal report from the Ministry of Health, this plan was used in 47% of counties, and was particularly popular in the central and western regions. A second option was again providing both inpatient and outpatient reimbursement, but in this case with no MSA's offered to households. Under this plan, both inpatient and outpatient expenditures, as well as preventative care fees could be reimbursed at a pre-agreed upon rate, but with deductibles. Also, quite popular, this approach was selected by another 41% of counties. The remaining three options were selected by counties very infrequently. These were inpatient and outpatient reimbursement plans that focused almost exclusively on funds for treating catastrophic illnesses, inpatient reimbursement plus reimbursement of catastrophic illnesses but not for other outpatient expenditures, and finally, inpatient service reimbursement only.

1.3 The NCMS and Labor Outcomes

The potential linkages between the introduction of the NCMS and labor outcomes are nuanced. On the one hand, health is an important part of "human capital", meaning it could affect individual's ability to work or their capabilities while working. The current literature places a strong emphasis on the linkage between better health and better labor market outcomes including wages, labor force participation, hours worked, and even retirement decisions. Researchers generally agree that healthcare strongly impacts decisions over retirement through the mechanism that employers usually have to provide healthcare to their employees in most countries (e.g., Gruber and Madrian (2002), Blau and Gilleskie (2001)). Some papers have also considered healthcare impacts on the labor supply decisions of lower-income single mothers, finding little evidence of effects on increasing low income single mothers' labor force participation (Coulter and Ham (2000), Hamuryudan et al. (1997)).

Several studies have verified a common result that bad health status results in lower wages and less labor supply (e.g., Buchmueller and Valletta (1999), Gruber and Madrian (2002), Suhrcke et al. (2005)). This occurs because employers have reduced incentive to hire those workers, thus dragging the overall wage level lower within poor health groups. Furthermore, poor health leads to lower worker productivity, so that competitive equilibrium wages may in turn decrease. Most of the studies in this literature focus on the employment-based healthcare programs in developed countries, and thus also tend to find the impacts of "job lock" among employees to be strong. However, less work considers cases where rural residents healthcare coverage is not tied to their labor outcomes directly. Our investigation tries to shed light on a public healthcare scheme which only aims to cover those self-employed peasants in rural areas of China, importantly in a setting where there are no direct "job lock" impacts of the healthcare coverage itself, since the NCMS is not tied to a particular job, or even to employment in general.

On the other hand, even in light of better health and its connections to expanded labor outcomes, it is also possible that expanding healthcare coverage might also have other distinctly negative impacts on labor supply. According to the neoclassical model of labor-leisure choices for an individual, it is standard to assume individuals gain utility from both consumption goods (i.e., income) and from their leisure time (Borjas and Van Ours (2010)). Hence, there is a classic tradeoff between spending one's time on labor or on leisure. Individuals making decisions regarding the combination of labor and leisure could face situations where good health and leisure time are compliments – for example traveling or undertaking other physical activities – leading to a situation where better health status could increase or decrease the incentive to supply labor. Therefore, the overall effect of improved health is ambiguous.

When it comes to the overall role of health insurance, the theoretical prediction becomes even more complicated. On one hand, purchasing heavily subsidized insurance today will, on average at least, reduce health care related expenditures in the future by more than the cost of the (subsidized) insurance premium. Hence, individuals receive a form of a positive income boost – although it is fair to think of this as an in-kind transfer since there is no way for the household to turn this benefit into up from cash on hand. Based on this income effect, self-employed rural residents may decide to work less, also possibly saving less for future spending on medical care, which they now know will be mostly covered by the insurance. On the other hand, if better health care leads to improved health, individuals may simply be more capable of taking on higher levels of work hours, which makes the labor supply decision even more theoretically ambiguous.

This study focuses on the effects of the implementation of NCMS on annual work hours for rural residents in China, to investigate if this expanded health coverage significantly impacts labor supply and other potentially related outcomes. We find consistent evidence suggesting a meaningful income effect is present. Put another way, we show that NCMS coverage leads to a reduction in work hours of roughly 8%, suggesting household's reduced risks of paying massive out-of-pocket costs for procedures and hospitalizations leads to a reduced incentive to work (the income effect), which dominates any positive health capability effects that may otherwise tend to push healthier workers into more intense labor supply outcomes. Importantly, we stress that we investigate only the short run impacts of this program – and acknowledge that the long terms impact on labor supply could in fact play out to be different, including the possibility of a reversal of direction.

The rest of this paper is organized as follows: Section 4 reviews the literature, focusing on the effects of healthcare coverage on labor supply as well as studies considering the effects of NCMS implementation on other outcomes of interest. Section 5 documents the CHNS data and outlines our empirical strategy. Section 6 provides our primary estimation results, followed by several robustness checks in Section 7. Finally, we conclude with Section 8.

1.4 Findings in the Literature

A large body of literature has investigated the effects of enrolling in various types/examples of healthcare programs on individual's health outcomes. While fully reviewing this vast number of studies is not a present goal, we have selected several influential studies as a set of representative examples. Suhrcke et al. (2005) focus on coverage in the European Union, finding evidence to support the idea that better health care services (and coverage), as well as better health status outcomes, plays a large role in the promotion of macro level economic growth. Turning to the larger portion of the literature that focuses on household (micro) level outcomes, Gruber and Madrian (2002) summarize the empirical literature on the effect of healthcare coverage on labor supply, job mobility, and work decisions. Given the close proximity of these questions to those considered in this study, we point readers interested in a more detailed documentation of these findings to their paper. In conducting their review of over 50 papers, they find that health insurance in U.S. does in fact influence overall labor supply levels and timing of retirement decisions among elderly workers, whereas they do not find strong causal effects on labor supply and welfare exit decisions for younger workers, including low income mothers. Admittedly, the literature is far more nuanced than this general representation of their findings, and of course it has also expanded in several meaningful over the decades following the influential Gruber and Madrian review, but the main themes of this body of work have stood the test of time.

Royalty and Abraham (2006) examine the joint labor supply decision-making of husbands and wives, determining that household level access to health insurance through a spouse's work-provided health insurance carries a significant negative effect on own full-time work decisions. Borjas (2003) focuses on the enactment of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA). The program curtailed certain guaranteed rights for the immigrants participating in Medicaid, as well as some other welfare programs. By using Current Population Survey data from 1995 to 2001, the study finds that the welfare cutbacks leading to increasing numbers of uninsured immigrants spurs labor supply outcomes within the immigrant population contained in the CPS sample. Our paper complements the Borjas study by also focusing on lower income households and their labor supply reactions to changing health care conditions, namely an expansion of a nationwide public healthcare program (NCMS) that was implemented in the rural areas of China. We find that rural residents of China reduce their work hours after the implementation of NCMS. Hence, our paper provides support and complementary for the influential Borjas finding, in the sense that we show the same underlying effect holds even when the change in health care coverage moves in the opposite direction (i.e., an expansion rather than a contraction) and for another environment.

Similar to our present study, Liu and Tsegai (2011) also focus on rural China and the expansion of the NCMS program. They use propensity score matching, as well as a bounding approach, both reaching similar conclusions. They show clearly that there is a positive casual effect of NCMS program on outpatient and inpatient utilization of healthcare, as well as levels and consistency of patient preventive care. Their paper also shows that enrolling in the NCMS program has no significant impact on reducing medical burden, but somewhat surprisingly, increased the incidence of catastrophic expenditures. Their propensity matching models found that households with higher income, better sanitation environment, and better health status are less likely to enroll in this program. The paper also concludes there exits heterogeneity through eastern, middle, and western regions of rural China in the application of the program. Viewed in this light, the current paper should be viewed as investigating the mean impact of the NCMS program across China's rural regions.

Lipow (2010) adds an NCMS multiplied by distance from a medical facility interaction term, in addition to the previously studied NCMS indicator variable. The study essentially investigates whether or not the NCMS has a heterogenous set of impacts on health outcomes, health care utilization, and the burden of health care expenditures, based on one's distance from a pre-existing medical facility at the time of the NCMS implementation. The analysis suggests that people who enrolled in the NCMS, and live further away from a medical facility, actually display a greater incentive to utilize healthcare coverage and services. In a related study, Lei and Lin (2009) found clearly increasing rates of utilization of preventive care for NCMS participates. However, there was not a significant impact on access to formal medical care or improving individuals' health status. Additionally, the NCMS was not found to relive households' financial burden, measured using out-of-pocket expenditures on healthcare.

Qiu et al. (2011) focus specific attention on Chinese rural-to-urban migrants, considering their utilization rates of the NCMS. Importantly, since the NCMS requires enrollment as household units, this creates a somewhat challenging set of incentives. For example, rural-to-urban migrants who are enrolled in their local (rural community) NCMS are not eligible to receive reimbursement if they seek medical services/treatments in a health care providing facilities in their new urban destination city. They examined the associations between migration and household economic status, enrollment in the NCMS, and the use of its benefits. They find that household economic status was positively associated with enrollment rate in the NCMS, but that the enrollment rate was slightly lower in 2006 for households where more than half of the family members worked as migrants.

To the best of our knowledge, only one previous study had focused on the potential connections between the NCMS and labor supply related outcomes. Shen et al. (2017) find that enrollment in NCMS leads to increased levels of individual labor supply, a finding contrary to the present set of results. Although they also use data from the CHNS, and similarly focus on outcomes including hours worked just ad we have, there are a number of important differences between the two studies that help explain the reasons different conclusions are drawn. The most meaningful difference is that our work carries the benefit of the additional passage of time from the original implementation of the program. Shen et al. (2017) are only able to use data from the immediate wave after the first round of counties were selected – a serious issue given the program's stated selection criteria for the earliest county adopters. Thus, even though that study did in fact follow several standard approaches to mitigating the severe bias associated with the intense selection bias they were facing, our position is that conventional attempts to mitigate the bias were understandably inadequate in the face of such little data, and the fact that it did not appropriately cover the greater rural population at large. So for example, even though we do in fact use some of the same types of identification strategies – both difference-in-differences and instrumental variables approaches for example – these identification strategies are strengthened by the depth and breadth of the data expansion. By adding several additional panel waves following the early NCMS initial adoptions, we not only gain the straightforward benefit of quadrupling the number of data points observed after the program's start (i.e., since we have four post-implementation panel waves compared to their one), but also we are able to test the population wide impact on residents from all areas – including those rural areas that *were not* ideally set up to meet the high selection criteria originally laid out for the early adopters to meet. This modification is quite important, as we find that using a much longer panel and more cleanly identified models, the result of interest reverses in direction. This reversal of the direction of the effect is clearly possible given the relevant theory on the competing health impacts effects and their magnitude compared to the size of the income/endowment type effect we discussed above.

1.5 Data and Model

Our empirical models use data from the China Health and Nutrition Survey (CHNS), an ongoing panel data collection effort maintained through a collaboration between the University of North Carolina – Chapple Hill and the National Institute for Nutrition and Health at the Chinese Center for Disease Control and Prevention. We make use of nine survey waves in total, with five waves (1989, 1991, 1993, 1997, 2000) occurring before the initial implementation of NCMS and the other four waves (2004, 2006, 2009, and 2011) occurring after the NCMS was implemented.

Figure 4 shows the nine different provinces of China that participated in the CHNS data collection effort. A multistage, random cluster process was used to draw the samples surveyed in each of the provinces. Counties in each of the nine provinces were stratified by income (low, middle, and high), and a weighted sampling scheme was used to randomly select four counties in each of the nine provinces. All told, the CHNS generated a sample of 8,638 households containing data on over 35,000 individuals. The survey covers several aspects of households' daily lives including health status, biomarkers, demographic and economic factors, and outcomes related to labor supply and financial conditions. Additional details about this survey are available through the CHNS website maintained by the University of North Carolina. Table 3 provides the initial observation counts, before any filters are applied for the empirical analysis, for our CHNS data spanning the nine panel waves from 1989 through 2011. Table 3 also presents our initial observation count split by urban/rural designations, male/female status, and major age categories, as each of those designations also plays a further role in the later modeling. Table 4 shows the implementation pattern and participation rate for the NCMS across our nine survey waves. For example, prior to 2004, none of the individuals in our data participated in the NCMS program. Since 620 observations of the initial reported as participating in old CMS program in the dataset, our analysis drops those observations, following Liu and Tsegai (2011). 12 counties in the CHNS survey became eligible for the year of 2004, with 432 households and 890 individuals participating in NCMS. However, starting with 2006, large increases in county and individual participation are seen with the program. By 2011, all counties represented in the dataset had gained eligibility for NCMS.

Table 5 provides the complete summary statistics for the set of variables that we use in our empirical analysis. Panel 1 contains the six different variables we use as dependent variables in our analysis, including the four main labor supply measures. From the questionnaire, adult respondents are asked to provide their work status, primary occupation, specific employment position, and how many months they worked during the last year. They additionally report how many days they work in a typical week, and how many weeks they work in a typical month. Finally, they provide how many hours are contained with an average workday. Hence, we follow a standard approach in the labor literature and construct measures reflecting annual work hours by multiplying these figured with one another. Since the NCMS program aims to provide public healthcare only in rural areas in China, in the present analyses we do not use observations that report urban/city or urban/suburb. We also dropped any individual who never reported positive levels of working throughout the entire panel.

Due to concerns of possible data entry errors, we also dropped a very small number of observations where negative numbers of working hours were reported. Finally, we recoded any respondent that reporting working more than 13 hours in an average workday, as instead working 13 hours a day. This is useful given the relatively small number of individuals reporting daily work hours this high, as well as concerns that the respondent is conceptualizing 'work' in ways that may differ from traditional norms. We also later consider models that use household savings as one of our dependent variables, so we dropped both the top and bottom 2 percent of outliners in the distribution of household savings. Alternatively, we could have used that particular filter in only those regressions, leaving these individuals in the labor supply regressions, but we eventually decided it was best to have a consistent sample across applications and that the extreme 'tails' of the savings/wealth distribution in rural areas may also face work related incentives and work related opportunities that are very atypical when compared to the majority of workers. With all those requirements, we finally have 23,997 observations in our baseline regressions.

Specifically, we model:

$$Y_{ihct} = \beta_0 + \beta_1 NCMS_{hct} + \beta_2 X_{ihct} + \tau T + \phi C + \epsilon_{ihct} \tag{1.1}$$

Where Y indicates our various labor outcomes of interest, including annual work hours and log of annual work hours in our baseline models. The subscripts denote an individual i, who is a member of household h, living in county c, that reported in survey wave w. *NCMS* is an indicator variable that equals 1 if the household participated in the program. Note it is not indexed by i since participation in the program occurs at the household level. X is a vector containing a wide range of social-economic control variables including age, marital status, gender, level of education, self-reported health status, as well as number of children. Finally, wave fixed effects and county fixed effects are included in order to control for the potential impacts of unobservable variables that remain constant across time and counties.

Since the NCMS program followed a staggered rollout process to cover all rural areas of China, the NCMS treatment is varied over surveyed waves in our empirical model. Following Beck, Levine and Levkov (2010) we tested the parallel trends assumption for Diff-in-Diff model with multiple treatment periods. Specifically, the NCMS program followed a staggered rollout process to cover all rural counties of China that began in 2003. Hence, our surveyed counties became eligible to NCMS at different waves, meaning our treatment of interest is spread out over a multi-period introduction. To standardize the test for each county, respecting their actual year of implementation, we apply the technique used by Beck et al. (2010), showing the parallel trends test results in Figure 5. Figure 5 plots the relationship between time periods before/after NCMS enrollment for the county and work hours aggregated at the county level. We have four survey waves prior the NCMS adoption and three waves after it. We use the following regression and report the estimated coefficients as the plotted points with 5% confidence intervals around them shown as well:

$$log(WorkHours)_{c,t} = \beta_0 + \beta_1 * D_{c,t} + \phi_C + \tau_T + \epsilon_{c,t} \tag{1.2}$$

Where the vector D variables are equal to zero, excluding D^{-j} which equals one for county C in the J^{th} year prior the NCMS adoption for the county, and D^{J} which equals one for county C in the J^{th} year after the adoption. ϕ_C and τ_T are county fixed effect and wave fixed effects respectively. Figure 5 indicates that all of the coefficients for the NCMS county dummy variables predating implementation are all insignificantly different from zero. On the other hand, they sharply drop and become (dramatically) lower than 0 at conventional levels of certainty for each of the three surveying periods after the NCMS adoption. These are strong results suggesting that, at least at the aggregate level, the effect of the NCMS program on labor supply is a true causal relationship, as no preexisting trends in labor supply were present prior to implementation.

However, since households volunteer to participate in the NCMS program and pay their portion of its costs, as opposed to universal enrollment for free or even with forced co-pays, the potential for adverse selection into the program is another empirical issue we are worried about. Households may be more likely to purchase the coverage if they have poor health, particularly expensive to treat chronic health conditions, and could also be more likely to purchase coverage if they have greater levels of income/wealth – as participation was not free. We make several simple comparisons to see if these concerns could lead to bias in estimated models that do not directly deal with these kinds of endogeneity issues. Specifically, we examine the NCMS participation rate according to various individual characteristics in Figures 6, 7, and 8.

To begin, we separated the NCMS participation rate by self-reported health status in Figure 6. Exactly as one would expect given concerns about adverse selection, we find individuals self-reporting "poor" health are more than 40% more likely to report having the NCMS coverage than are individuals in the "excellent" or "good" health categories. Hence, even though enrollment occurs at the household level, which one would expect to mitigate adverse selection, we still see strong evidence that adverse selection occurs. It is possible that this way of framing the adverse selection problem is related to the overall correlation between health and age. In Figure 7, we separated NCMS participation rates by 5-year age ranges. Again, we see that even though enrollment occurs for entire households, clearly the household age composition is highly nonrandom with respect to enrollment. For example, an individual age 60 or older (i.e., the last three columns) is about twice as likely to report having NCMS coverage compared to individuals still in their 30's and 40's. Finally, we want to see if there exists heterogeneity across different income levels, suspecting higher income families may simply be more capable of signing up for coverage and paying the up-front and on-going costs of coverage. We divided our sample into the four income quartiles shown in Figure 8. Not surprisingly, the NCMS participation rate increases monotonically as one moves through the income quartiles, with the highest income quartile members over 65% more likely to have coverage that those in the lowest income quartile. The strength of this effect is somewhat dampened by shifting from household income to per capita household income, but significant increases in NCMS coverage rates are still observed at the higher per capita income levels. Based on all these analyses, one must conclude that significant endogeneity issues related to selection effects into NCMS coverage would be present if we only use a simple OLS model. For that reason, we estimate two-stage least squares (IV) models that correct for this shortcoming, instrumenting for household level selection into the program (the endogenous variable) with county level eligibility for the program (a variable clearly exogenous to the households' individual traits).

To be a valid instrument, the county level NCMS eligibility must satisfy two main requirements. First, the instrumental variable should have a direct and strong relationship with the endogenous independent variable of interest. In our case, since individuals could only enroll if the county gained program participation status, and since participation levels were instantly high, the two variables in question are correlated at exceedingly high rates, right around 0.75. Table 6 shows this another way, by providing the first stage result of the 2SLS regression showing individual NCMS enrollments is positively corelated to NCMS county level eligibility, also displaying an F-statistic of over 65. This is well higher than 10, the level typically sought after in applied IV approaches, so we are fortunate to move forward with a strong instrument.

The second requirement is that the instrument should not have its own independent direct influence on the dependent variable, save the channels operating through the original endogenous variable, a requirement commonly referred to as the exclusion restriction. In our case, the exclusion restriction is satisfied, as county level eligibility is plausibly unrelated to our individual level dependent variables, once the correlation between eligibility and take-up of NCMS is accounted for, since the county level eligibility for the NCMS occurs at a community wide levels and is therefore exogenous to any given individual worker's labor supply decision. Whether the county is eligible for the NCMS program (and when) is decided by both the central and county government. An individual household can only influence their own take-up decision following that outcome.

Since the county level data for NCMS eligibility is strictly confidential (an obstacle we tried unsuccessfully to overcome), we generated our own county level eligibility measure using the CHNS data and the following procedure: if one or more households report participation/enrollment in the NCMS within a given county at a particular wave, we treat that county as having eligibility for the program. We then code county level eligibility equal to 1 at that given wave. If none of the at that point in time. So for example, to be coded "0" for the NCMS eligibility variable, none of the households in the county at a particular wave can report NCMS enrollment. This procedure produces an incredibly intuitive set of results – namely that when we aggregate household level participation in NCMS at the county/wave level, we nearly always get 0 or a very large number. In a very rare number of cases we get a number that is positive, but exceedingly small (2 or fewer). For this reason, we later test for potential sensitivity of our results to the inclusion of the few counties that were "close calls" on this designation (i.e., the cases where a very small positive number was reported) in the robustness check session.

1.6 Results

Table 7 shows our baseline results for both the OLS and 2SLS models. All estimations include wave fixed effects as well as county fixed effects in an effort to control for the effects of unobservable variables correlated with the presence of the NCMS program. Wave fixed effects can be viewed as controlling for factors that vary over time, but are common to all locations in rural China, whereas the county fixed effects control for factors that are geographically influenced, but remain constant over time. The first two columns show the NCMS participation effect on annual work hours, first using the baseline OLS regression and then applying the 2SLS procedure that corrects for the endogeneity associated with household take-up of the NCMS coverage.

The OLS results considering annual work hours suggest that after enrolling in the NCMS, individuals reduced their labor supply by just over 4 hours per week. In the OLS regression using log of annual work hours instead, the effect registers at greater than a 20% reduction. However, there are a number of reasons to believe these magnitudes are biased – and in our case specifically – biased towards a more strongly negative finding. For example, we know the adverse selection problem brings older and less healthy individuals to the NCMS coverage, and those are exactly the types of workers with baseline levels of working hours that are lower to begin with.

When shifting to the 2SLS regressions that correct for our endogeneity issue, the result stays consistent in terms of direction of effect and significance, but now we see a somewhat smaller magnitude. The annual work hours regression registers the effect as a reduction of roughly 2.7 hours. Again, we also take the natural log of annual work hours to explore the percentage change associated with participating in NCMS. The 2SLS regression, our preferred approach, suggests an effect of approximately 8% of in work hours. Given the mean value for annual work hours in the sample is approximately 1,695, the two estimates fall nearly identically in line with one another.

While they are not our main focus, several of the results concerning the control variables are also noteworthy. For example, we see strongly declining rates of labor supply as individuals become older. This begins immediately, progresses monotonically, and is full of meaningful gaps/jumps. A typical respondent in their 50's would work more than 200 fewer hours per year – or roughly 4 hours per week – less than an otherwise similar respondent in their 30's. The coefficient on female is negative, significant, and of nearly identical size across the OLS and 2SLS models. This suggests males work about 2 hours more per week than females, a result that may be consistent with the survey question leading individuals to focus more on formal/market activities and less on home production activities. Those with higher levels of education are found to work more, as well as those who have excellent current health. Somewhat surprisingly though, the gap between individuals reporting "excellent" health and those reporting "poor" health is only about 3 hours per week – an effect right around the same magnitude of the NCMS coverage. As we expected the health related effects to be larger, this may provide some evidence that for lower income rural households, there is very little ability to avoid work and still earn enough income to support one's self/family.

To further explore potential nuances of the effects of NCMS coverage, we also estimated several complementary regressions. Table 8 explores the role of gender as it pertains to NCMS coverage. If the OLS models are accurate, which we worry is not the case given selection bias into the NCMS coverage, the results would suggest male and female labor supply were impacted in nearly equal magnitudes. However, our 2SLS results indicate that, compared with females, males have much smaller elasticities for their labor supply with respect to NCMS eligibility. Compared to the baseline 2SLS estimate of an 8% reduction in hours worked, the magnitude of effect essentially doubles, whereas the point estimate is cut in half for males, with a loss in statistical significance. This finding likely relates to marital status, as the same 2SLS regressions show our "married" status variable increases labor supply by about 7% for males, but at the same time reduces it by more than 15% for females. We also separate our sample into workers who report being the household head, those who report being a non-household head, and the sample aggregated into the household level (i.e., all work hours of any household member aggregated into a single total). We provide results in Table 9 showing that the effect on household head is smaller than the initial average effect. We then expanded our sample by adding back the non self-employed workers within the household. This exercise produces a consistent sign when compared with models using only self-employed peasants. These results are shown in Table 10.

Given that we find consistent results showing that enrolling in NCMS has a negative impact on rural residents' work hours, we are also interested in trying to better understand the various causal mechanisms driving this. For example, is the reaction driven by the substitution effect or due to the income effect of the NCMS plan. In this particular context, we define the substitution effect as spending more time on leisure (or, by definition, less time devoted to labor) when one's health status is improved. The income effect is defined as the expected gain in household purchasing power that is experienced when the household's (heavily subsidized) insurance premium is less than the expected costs for reimbursement of their inpatient and outpatient expenditures.

In order to better understand the substitution effect, we use household chores as a proxy for individuals' productive time use other than their reported working hours. In computing our household chores variable, we summed up average number of minutes per day spent buying food, preparing food, washing clothes, and cleaning house. Table 11 shows the OLS results exploring whether enrolling in the NCMS has an impact on time spent doing household chores. And Table 12 provides the 2SLS results of the NCMS impact on household chores. We find that all groups displayed a negative and significant impact on the minutes/day of household chores. Since both working hours and household chores are reduced after joining NCMS, one plausible interpretation would be assuming an increase in individuals' leisure time is significant, suggesting the substitution effect is a meaningful contributor to the overall impact on labor supply.

To investigate the income effect, we explored household savings levels to see if enrolling in NCMS has any impact on levels of household savings. To calculate annual household savings, we used reported household net income minus reported household expenses in each wave. As we outlined earlier, prior to the NCMS individuals were responsible for covering their health care costs, sometimes by having to dip into their savings. Since NCMS participants can get reimbursement if they get sick or injured, their savings for future or unexpected healthcare usage might be reduced, as it no longer has to cover this purpose. We use household savings as our dependent variable to see if the NCMS has an impact on this possible precautionary behavior. We present the OLS results in Table 13 and the 2SLS results in Table 14. This includes the overall sample, as well as separate estimations by income quartile, as levels of household savings are highly skewed (i.e., the top quartile is where the vast majority of all savings is occurring). Our findings indicate that the full sample and 4th quartile income group always display a significant decline in annual household savings after participating in NCMS, with the effect essentially being driven by the highest income quartile. These findings are consistent with Cheung and Padieu (2015) who show that NCMS coverage reduced household savings for high and middle income households, but had no significant impact for the poorest households.

Since we found the NCMS enrollment affects household saving levels heterogeneously across different income quartiles, we divided our sample by income quartile, and then ran the same basic OLS regression model in Table 15, and 2SLS models in Table 16. From the OLS results we see decreased labor supply occurs in all four quartiles, with the largest impact registering in the third quartile. On the other hand, the preferred IV models show no significant impact for the lower two quartiles, and larger significant drops in the two highest income quartiles.

1.7 Robustness Checks

Undoubtedly, one of the biggest challenges associated with understanding the impacts of NCMS coverage on labor supply is the worry that respondents age plays a significant role in determining both their labor supply and their level of interest in purchasing the NCMS coverage. Fortunately, we have that NCMS sign-up was extremely common (likely due to the large subsidy of the cost) and that it occurred at the household level. Still, we clearly demonstrated before that individuals who were older were more likely to be covered. Therefore, even though all of our models control for the respondent's age, in Table 17 we use the combination of age and age squared to replace the age-range categories, again finding results that are still consistent with the baseline model, with the point estimate on NCMS coverage moving only slightly. We also recursively dropped one wave at a time for each of the pre-NCMS waves (shown in Table 18) and each of the post-NCMS waves (shown in Table 19). We did this to ensure no particular wave drives our effects of interest. These explorations show there is no one wave in particular that is particularly driving the results.

Since we use a data-generated NCMS county level eligibility variable as our instrument variable, we also wanted to explore whether or not that choice impacted our results in any meaningful ways. Recall that we had coded the county level indicator as one (or 'on' for the NCMS) if one or more households enrolled in the NCMS program within that county at the particular wave in question. A natural question to wonder about is whether or not there exist possible errors when generating this variable. Therefore, we calculated the number of NCMS household enrollment for each surveyed county through different waves in Table 20. From this, we can see that most counties contain large amounts of households' enrollment, except CountyID 2321, 4222, 4323 in wave 2004, and CountyID 4323 in wave 2006 each with only two household reported their NCMS enrollment. We then use two different methods to check the robustness of our original coding method. One procedure involved recoding those four county eligibilities as 0 instead of 1. These results are presented Table 21. The other path simply dropped those four counties, then running the same models. The results of this second approach are shown in Table 22. Reassuringly, each of these exercises produces consistent results to our original findings.

To further convince ourselves that enrolling in the NCMS is leading to a causal reduction in work hours among Chinese rural households, we also use propensity score matching as an alternative approach to mitigating potential selection bias between treated and untreated observations. After constructing a control group based on observables using a propensity to enroll score, we estimated that enrolling in NCMS reduces labor supply by 488 hours annually using one-on-one matching, a considerably larger estimate than our favored original results, but of course in the same negative direction. This alternative approach also leads to a statistically significant decrease in household chores after enrolling in NCMS. These results are provided in Table 23, as a final robustness check, while Table 24 provides the conventional covariates T-test between treated and control group, pre-matching versus post-matching. From these statistics, one can see there exists significant statistical differences between the NCMS and non-NCMS group pre-matching, but also that these differences go away after we carry out the propensity score matching procedure.

1.8 Conclusion

Using nine waves of data from the China Health and Nutrition Survey, we consider the relationship between the rollout of a new rural healthcare insurance system in a developing county – China – and labor supply. We consistently find the implementation of the NCMS reduced annual work hours among rural residents, across a number of different models and identification strategies. Importantly, this is complemented with evidence coming from regressions considering household chores and savings levels that suggest both the substitution effect and income effect play a role in shaping this response.

We also consider several potential forms of heterogeneity that may influence the nature of this relationship. In exploring the role of gender, we see that females supply labor for more elastically with regard to NCMS coverage than males, and that a similar pattern holds when comparing head-of-household respondents with non-household heading individuals (i.e., the household heads supply labor less elastically, with much smaller effects of the NCMS coverage). We also estimate several models where we further segment our sample by income level, as well as different age groups and health status categories, we also find the direction of impact is consistent and strong.

We then examine the income effect of this public healthcare program by testing its effect on household savings, and find heterogeneous results across different income groups. Our results show that enrolling in NCMS significantly reduces the saving levels among higher income households, while there is no significant impact on either lower income quartile. Based on those findings, we separate our sample into income quartiles and run regression on individuals' labor supply within in different income groups. We find that enrolling in NCMS did not lower labor supply among the lower income group, but did lead to a decrease in work hours within higher income level households. In many ways, this aspect of our results gives support to Borjas' (2002) main findings.

Importantly, the income effect of the NCMS coverage is found to be significant in China's poor rural areas. This should allow governments and economic forecasters to better predict labor supply reactions to expanded healthcare coverage in other similar developing economies.

1.9 Acknowledgments

We thank the National Institute of Nutrition and Food Safety, China Center for Disease Control and Prevention, Carolina Population Center, the University of North Carolina at Chapel Hill, the NIH (R01-HD30880, DK056350, and R01-HD38700) and the Fogarty International Center, NIH for financial support for the CHNS data collection and analysis files from 1989 to 2006 and both parties plus the China-Japan Friendship Hospital, Ministry of Health for support for CHNS 2009 and future surveys.

Chapter 2

Parental Decisions over the Timing of Primary School Entry in China: The Role of Child Size

2.1 Introduction

Broad literatures within the Social Sciences note that parents hold a variety of different opinions concerning when it is most appropriate for their children to start primary public schooling. Perhaps due to an awareness that early success in school may help track children for subsequent higher achievements in school and the labor market, parents in western countries tend to prioritize the "readiness" and "maturity" of their children when deciding whether or not to sending them to school. In fact, studies consistently show that enhanced maturity relative to classmates does lead to better school performance, advanced cognitive skills, better mental health, increased school leadership, and reduced levels of disciplinary actions (Dhuey et al. (2019); Bassok and Reardon (2013); Cook and Kang (2016); Lubotsky and Kaestner (2016); Depew and Eren (2016)).

However, as cultural beliefs play a critical role in this process, it is worth noting that many parents in China believe that it may be beneficial for children to start schooling earlier than their similar-aged counterparts. In fact, there is an old saying in China that is now well known across the globe: "Zao qi de niao er you chong chi", which means "the early bird catches the worm". The idea is that since start schooling earlier means graduating earlier, and then in turn entering the labor market earlier than other peers, Chinese parents seem to believe that entering primary school at a younger age could help reduce the opportunity cost associated with late entry into the labor market. For example, a recent published paper by Huang, Zhang and Zhao (2020) shows that parents in some Chinese provinces go as far as manipulating the timing of birth/delivery of their children around the August/September eligibility cut-off, in order to allow their children to start school earlier rather than later.

While it is not clear what portion of this effect is driven by actual birth timing manipulation, relative to the contribution of strategic misreporting/manipulation of the truth in reporting dates of actual birth, what is clear is the underlying intent to gain "eligibility" to send the child to public school at an earlier point in the youth's life, as opposed to waiting until the child is older and more mature/prepared. Moreover, the birthday manipulation effect was strongest for higher socioeconomic status families, which potentially runs counter to the consensus findings from the US. In particular, if family SES background and individual human capital are complements in the labor market, one might easily expect wealthier families might tend to favor the later birth dates that facilitated school entry at higher levels of maturity.

For this reason, it is important to study the behavior of Chinese households independently, rather than simply assuming they behave the same way American or European parents would. Also, the main innovation of this study is the development of the idea that parents' preferences over the initial timing of school entry may significantly interact with other observable traits possessed by the child. Intuitively, parents' beliefs regarding their child's emotional maturity, attention span, underlying intellectual and problem solving capabilities, and even the ability to effectively communicate with adults and other children may all influence the timing of school entry decisions. Unfortunately, many of these concepts are difficult to measure using a straightforward econometric approach. For that reason, we seek to make a small but meaningful initial contribution to this line of research that focuses directly on one easily measurable trait: child size (as measured by height). In our application, our height variable is truly exogenous to SSA, our outcome of interest. We define our variable dichotomizing children size based on their height-within-age category. Since SSA is not capable of changing a child's height or their date of birth, we have no need to worry about reverse causality in our relationship of interest – allowing us to see the true underlying effect of child size on the timing of school entry. Our preferred measure of child size is based on having above-average height for a given biological age, but our findings are robust to alternative definitions of the size variables.

Our hope is that the robust finding that taller than average children enter school earlier than shorter children in China, even after controlling for their biological age, establishes that not all parental preferences over the timing of primary school entry are uniform with respect to age or other traits that may influence the outcome of interest – an implicit assumption that supports many of the studies using the regression discontinuity approach to measuring the impacts of SSA. Instead, the age of a given child may interact significantly with a number of other child level characteristics to produce eventual school enrollment decisions. To the extent these decisions are non-random, and the trait in question also correlates directly to the outcome of interest to the researcher, acknowledging and mitigating this issue would become an important part of investigating the effects of SSA.

Importantly, our study provides a novel piece of evidence that parents in China make decisions based upon desires to avoid having smaller/shorter children enter school at younger ages. An additionally fascinating nuance of our findings is that gender plays a role, as parents of girls are found to react more strongly to child size than parents of boys. While we have no hard evidence for this conjecture, our findings are consistent with parents having underlying concerns that smaller children may be more likely to suffer from bullying and/or social exclusion than other larger children. Furthermore, that concern on the part of parents would be consistent with evidence from the education literature.

2.2 Primary School Entry in China

The Chinese government requires that a child must turn 6 years old by August 31st of the school year in question to gain eligibility for enrollment into primary public school. Effectively then, the law in China strongly differentiates between children born just before

August 31st and children who are born just after the cutoff date.[1] Based on this aspect of the legal environment concerning school entry in China, many recent studies (e.g., Zhang, Zhong and Zhang (2017), Liu and Li (2016)) investigate the impacts of this "natural experiment" that relates to school starting age (SSA) in China, focusing on outcomes including children's cognitive skills and test performances. Unsurprisingly, many of the findings of this newly emerging literature considering SSA effect in China support and complement other similar findings that investigated the effects of SSA in the United States, Europe, and other previously investigated settings. However, exceedingly few studies have directly focused on the role of parental control/decisions over the timing of school entry for their children. That is to say, most empirical investigations in the large SSA literature assume the eligibility threshold is followed strictly, or at least implicitly assume that any occurrence of "sorting" or "bunching" on either side of the threshold is random with respect to the selected outcome of interest to the researchers. Importantly, to our knowledge we are able to offer the first systematic empirical investigation of parental decisions on school entry in China – an important emerging economy with more school aged children than any other country.

Using individual level panel data from China Family Panel Survey (CFPS), we investigate parental decisions concerning when to first send their children to primary school, with specific attention given to the role of child size (as measured by height). We find robust evidence that Chinese parents tend to "redshirt" their children when they are relatively smaller (shorter) than average, whereas larger (taller) children of the same biological age are

[1]Beyond China, of course many other nations use similar policies governing eligibility for entry into local public schools. Nearly every public schooling environment that we are aware has age play at least some sort of a role in determining eligibility.

more likely to be sent to school. We explore the robustness of this finding through the use of several different types of empirical models, always uncovering this same basic underlying result.

Additionally, we posit that parents' preferences over sending relatively smaller (larger) sons into primary school may not mirror their preferences for sending their smaller (larger) daughters under otherwise similar conditions. Hence, we explore potential interactions between gender and child size, finding strong evidence that parents in China are less (more) sensitive to the size of boys (girls) when it comes to the initial decision to start primary school. We further investigate the independent size effect on test performances, finding that taller children perform better score on examinations, even after controlling for biological age, SSA, and other observable traits.

2.3 Literature Review

A large literature concerning the various effects of SSA on outcomes of interest has developed over the past two decades. Most of the studies in this literature follow the seemingly sound assumption that the cutoff entry date for primary school is a sharp/solid one, that is to say assuming that the eligibility date is strictly followed by parents, or at least implicitly assuming that for children born arbitrarily close to the cutoff there is not significant strategic manipulation over the decision to enter school young or not that is correlated to the outcome of interest in the study (e.g., student achievement measured by test scores or some eventual labor outcome). Given that birth dates are assumed to be randomly distributed, the hard cutoff for eligibility creates a clean “natural experiment”, as children born just before the threshold are at least eligible to start school one year younger than children who are born just after the cutoff. And while the Huang et al. 2019 study does suggest a small amount of birth date manipulation does in fact occur, the more significant assumption embedded with this approach is that parents are not making a decision based jointly/simultaneous on the interaction between the threshold birthdate outcome and a different independent outcome (e.g., size/height) that also exerts its own independent effect on the outcome of interest.

Based on the eligibility threshold natural experiment assumption, a large number of important and influential papers follow a regression discontinuity design (RDD) approach to test the effect on school starting age (SSA) on outcomes including cognitive skills and test scores while still acquiring human capital in school (e.g., Fredriksson and Ockert (2005), Puhani and Weber (2007), McEwan and Shapiro (2008), Lubotsky and Kaestner (2016), Zhang, Zhong and Zhang (2017), and Dhuey et al. (2019)), eventual levels of mental health/wellbeing and early pregnancy (e.g., Dee and Sievertsen (2018), Elder (2010), Black, Devereux and Salvanes (2011)), engagement in criminal activities (e.g., Cook and Kang (2016)), educational attainment (Fertig and Kluve (2005), Barua and Lang (2009)), labor market outcomes (Dobkin and Ferreira (2010), Fredriksson and Ockert (2005)) and many other outcomes beyond these few.

In general, SSA is found to have meaningful impacts on these outcomes. In one influential paper for example, Black et al. show that starting school at later ages carries nuanced effects. Those students do far better in school, as age itself is a predictor of success on exams, but the effect of SSA itself is actually negative. Starting school later is also found to improve mental health and reduce later changes of entering into teenage pregnancy. In fact, many of the papers in this literature take up multiple outcomes and paint a nuanced picture of the effects of SSA. Simply put, since it would be difficult to fully review this large literature, it makes more sense for the current effort to focus more narrowly on portions of the literature investigating the role of parental preferences over school entry decisions as well as papers investigating the effects of SSA using data from China. Importantly, a comparison of the studies using data from developing countries with others using data from developing countries like China reveals that in the developing country context, where dropping out early from school is far more common, entering school at a younger age has proven to be far more beneficial for a number of interesting outcomes.

Focusing next on the relatively few studies that explore the various non-random elements of outcomes/behaviors around the cutoffs, some studies have in fact taken up the topic of strategic manipulation around the eligibility threshold. So for example, Huang et. al (2020) carefully examine the number of births reported just before and after the school eligibility cutoff point in China. They concluded that Chinese parents, and particularly among the highest-SES families, seem to be displaying the tendency to have births themselves occur in a non-randomly strong rate prior to August 31st, which is the hard national cutoff date determined under Chinese Compulsory Education Law. While one wonders the extent to which this is entirely driven by the actual timing of births, relative to strategic reporting manipulations, their study did confirm that at least some mothers choose to undergo the cesarean section surgery procedure in an effort to bring forward deliveries when their due date was already very close to the threshold.

Again, possibly due to differences in cultural preferences across developed and developing economics, Shigeoka (2015) uses data from Japan to show that rather than hastening their deliveries near the manipulation cutoff, parents in Japan tended to postpone their birth deliveries until after the school cut-off date if possible, in order to allow their children to start school a year later. Presumably this means that in the Japanese context, there is a perception that advantages go to children who are older and more mature when they begin school, and those benefits dominate any costs associated with subsequently entering the labor market at an older age. Interestingly, they also find the phenomena (albeit in the opposite direction) to be stronger among socioeconomically advanced families. They posit this is due to the fact that they are more capable of affording an extra year of childcare costs. One also wonders if the effect is simply strongest where the parents are most capable of actually acting on their preferences, perhaps in environments where they have more money to pay for better health care or access to planned caesarean section births. Finally, there are a handful of studies examining the potential for timing of birth manipulation for other countries such as the United States (Dickert-Conlin and Elder (2010)) and Chile (McEwan and Shapiro (2008)), but these papers find no evidence for an obvious discontinuity around the cutoff.

Beyond strategic timing of birth dates, a few studies have considered various aspects of strategic sorting behaviors around the school entry decision. For example, the literature shows that in United States and other developed countries, parents tend to place a higher emphasis on the academic readiness and level of emotional maturity when their children approach the time to kindergarten and/or first grade. Thus, parents may decide to withhold their children from starting school, a term known as 'academic redshirting' in the literature, perhaps believing their children will benefit from being able to perform at a higher level once they begin classes. Graue and DiPerna (2000) show that parents in the United States strategically postponed their child from entering kindergarten, and that the effect was particularly strong for boys who were born before the entrance cutoff. They discuss studies in the child development literature that explore the various ways in which boys tend to be "later-maturing" compared with girls.

For example, one parent might perceive their son to be highly academically capable, and may want to increase the probability he scores highly enough on standardized testing to attain National Merit status – opening doors for academic scholarships to Universities. Another set of parents may have a daughter who possesses strong athletic ability, and may want to increase the chances she excels in high school basketball to a point where she earns a scholarship to play College Basketball at a major University. In either case, the children would have performed to perfectly acceptable levels if they started school at a younger age, but the parents are seeking to enhance the probability of creating an exceptionally positive outcome by delaying. They also show that the magnitude of the redshirting effect strengthens as the biological birthdate is closer to the school eligibility cutoff, and that the effect is strongest for white male children.

Our main contribution to the large literature investigating SSA effects and timing of school entry is to shed light on the connection to child size using data from a large developing country. While we already know that parents in both developed and developing countries act to strategically impact SSA for their children born close to the eligibility threshold, and we know a handful of findings about how these practices differ across cultures. However, there is not yet an understanding of how a factor like child size could interact with this environment. From other literatures, it is already well established that child size positively impacts cognitive student performances (e.g., Villimez, Eisenberg and Carroll (1986), Figlio et al. (2014), Haile et al. (2016)), thus it seems like child size/height is a reasonable trait to add to this discussion. And in fact we do find strong evidence to support the notion that parents wish to avoid sending their smaller children to school at young ages, particularly for girls. We complement these important findings with additional models that validate the underlying connection between child size and academic performance does also hold in the context of China – essentially showing this well established result holds in yet another setting.

2.4 Data and Empirical Methodology

To examine the dynamics governing parental decisions to redshirt their children when entering primary school, we use data from the China Family Panel Studies (CFPS), which has been conducted and maintained by the Institute of Social Science Survey (ISSS) at Peking University. The CFPS is a large nationally representative annual survey of Chinese communities, families, and individuals. Launched in 2010, the CFPS data is designed to collect longitudinal data at each of these three levels in contemporary China. The CFPS collects data on a wide variety of financial as well as non-financial outcomes. For example, it contains variables indicating general well-being, health status, family and demographic characteristics, wealth/income/financial measures, labor outcomes including home time-use related outcomes, educational outcomes, and other measures related to housing and migration. The original survey design spanned 25 different Chinese provinces and included roughly 16,000 households. Importantly, the CFPS surveyed all the family numbers for each household. This paper specifically uses panel waves from 2010, 2012, 2014 and 2016. Based on the size and comprehensive nature of the CFPS, it stands as an ideal data source for the investigation of our main questions.

The CFPS database contains two main portions: the "adult" questionnaire and the "child" questionnaire, both containing unique household ID markers allowing the research to match individuals moving across the two. The children's data set contains detailed survey items asking about the month and year of birth, the child's gender, their current height and weight, the academic school year of entering primary school, measures tracking their academic performance at school, and a large host of other demographic characteristics. While the 'adult' questionnaire contains much of the information needed for our empirical analysis, they do not produce observations per se, since our focus in on children. However, various characteristics of the family/household that children live in are obtained from the adult response database. For the purpose of this study, we merge information from child and adult survey waves spanning 2010 to 2016, in order to obtain the maximum number of cases. Some measures, like test performances and the current height of the child, can differ across waves. At the same time, other important measures including birth date, primary school entry year, and most demographic characteristics remain stable across all waves. One advantage of merging the various waves together is that our statistical power increases to a point where some of the more nuanced aspects of the relationships we are interested surface more clearly. Still, it is worth noting that all of our main results can be easily obtained using any one of the individual waves.

Table 1 provides the summary statistics for the dataset. [Insert Table 1 about here.] Our initial number of total observations comes in at 12,660. For our baseline model, this marks the number of observations in the regression. All of our critical measures like birth year, birth month, School Starting Age, current biological age, gender, height, Ethnicity Status, as well as rural/urban status are observed perfectly for all 12,660 cases. Some of the other variables are occasionally missing from being non-reported, reducing the number of observations contained in some of our later regressions.

Regarding child size, we construct “Tall” as an indicator variable equals to 1 if a child’s current height is greater than average at their current age. This decision carries several advantages and a few disadvantages. First off, if this is a true causal relationship, our effect of interest should surface even when using a fairly blunted dichotomous variable. Put another way, we should not need to rely on identifying small groups of outliers that are extremely tall or very short for their ages – although if we do find general evidence of a pattern one could assume it would hold in a particularly strong way for children in both tails of the height distribution. For that reason, we decided to use a very simple method to operationalize larger children. We note that other breakdowns, for example grouping students into “short”, “average”, and “tall” categories – where the “average” category is either the middle third or middle half of the distribution, also lead to the same set of qualitative findings.

One disadvantage of the CFPS data is that while we have the accurate height of the student at the time they respond to the survey, we do not measure their precise height at the time their parents are making the decision to send then to primary school (i.e., that occurred earlier and in unobservable for any student not in the first year of primary school at the time of the survey). However, we view this problem to be relatively minor. Evidence from the biological sciences shows that children’s position within the distribution of height over the life course remains fairly stable. Put another way, if the CFPS surveys a taller than average 11 year-old, the same student would have carried an extremely high probability of being recorded as taller than average at age 6 or 7, when they likely entered primary school. Importantly, our variables placing students in the "Tall" or "Short" categories have absolutely nothing to do with their SSA realizations – all size related variables used for this analysis are set in motion purely based on child height and biological age. Put another way, these variables are not constructed after the fact by looking at whether or not they are taller/shorter relative to classmates, they use the same age peers as a comparison group.

The average recorded height in our sample is 129.29 centimeters – strikingly similar to the global average height for a child around 9 years old, which is in turn right around the average biological age recorded in our sample. We also observe child's weight at birth for a considerable fraction of the sample, but eventually decided to focus on height for at least 2 reasons. First, current height is always recorded, whereas birth weight is typically recorded. Second, as the survey is conducted when the child is an adolescent, such that the reported birth weight comes from memory/perception, and is therefore not expected to be as accurately reported as current height. Since we know the biological literature suggests the two variables should be very highly correlated, we want to use the variable expected to display a higher degree of accuracy. Having said that, we can also produce essentially the same types of results if birth weight were instead used to create our different size related groups rather than height.

While the birth year and birth month are always observed, unfortunately we do not observe the specific birth date. Hence, we are forced to work only with twelve distinct months of birth each year, rather than being able to dive into an analysis of days/weeks within the school eligibility threshold. Still, this limitation is minor, as students further away than a full month from the threshold are still found to demonstrate a significant relationship. Again, the more blunted monthly measure would only tend to make it harder to uncover a significant result. The average birth month in the sample is 6.65, fairly close to the 6.5 that would be expected under a uniform distribution of births. In many empirical models, a key variable is the binary variable indicating a child was born on September 1st or later. Our sample is split 53%/47% between young men and young women, is 88% of Han ethnicity, and is roughly 39% percent urban. All three of these outcomes are quite close to China's overall population averages during the time periods the survey was conducted.

Beyond the critical measures showing child size and biological age, the CFPS data also contain annual Math and Chinese test grades earned on the previous midterm or final. While an ideal measure would be extremely precise (continuous), our measure is categorical and coded as follows: 1 (Excellent), 2 (Good), 3 (Average) and 4 (Poor). In anything, a somewhat blunted measure like this should simply bias our models towards finding an insignificant result – but in fact we find a strong significant effect of height on performance, so we are confident the measure contains enough meaningful variation for our purposes.

Based on the reported biological age and school enrollment history provided in CFPS, we are able to compute each student's actual school starting age (SSA) by comparing the child's birth year and birth month to the primary school entry year. Since the school year across all districts begins on September 1st in China, we follow an approach used by Elder and Lubotsky (2009) and others, calculating SSA for each child as follows:

$$SSA_i = (YearEntry_i - BirthYear_i) + (9 - BirthMonth_i)/12 \tag{2.1}$$

where SSA_{i} is the actual age when beginning primary school for child i, $YearEntry_{\mathrm{i}}$ is the reported year when a child first enters primary school, and $BirthYear_{\mathrm{i}}$ and $BirthMonth_{\mathrm{i}}$ are the reported birth year and month, respectively. In order to make the SSA variable more reliable, we excluded any observations where the calculated SSA was greater than 8.0 or less than 5.0. These extreme outliers, which constitute only a few small portion of the date, are in many cases likely to be simple data entry errors, and in other rare cases where accurate – would still be quite questionable (i.e., a case where the actual SSA was more than a full year away from the typically encouraged normal range of 6.0 to 7.0).

Figure 1 depicts the full sample relationship between SSA and birth month, clearly revealing in visual form the gap/jump between August and September, and then again importantly, an even bigger jump between September and October September. [Insert Figure 1 about here.] Importantly then, regardless of what the Chinese Compulsory Law says on the books, the data reveal that children born in September – at least in the aggregate – display SSA values similar to children born during the summer months. Large upward movements in average SSA then occur at October and again at January. The mean SSA across the entire sample is 6.48 years old, exactly as would be expected. Importantly, our eventual results suggest that at least some children born after the eligibility cutoff are being "harvested forward" into school, whereas others born during the summer months who are eligible to enter school, are being held back. This simple phenomenon can be seen for example from the August bar – which is still taking a value around 6.25, as opposed to 6 (e.g., these children will either have a value at/around 6 years, or around 7 years, depending upon whether they enter young or are held back).

In terms of child size, we use the reported current height –specifically height within age group profile – as a proxy variable for size. We made this choice for a few reasons, most notably that height, rather than weight, tends to be more easily seen and is expected to correlate better with the perceived 'size' of the child. We divided children's size into two groups: "Tall" if the child's size is greater than the age adjusted average and "Short" if their height is less than the age adjusted average. An obvious shortcoming of this simple approach is that it 'grabs' the large middle of the distribution into these two groups, a choice we made for reasons explained earlier, but also recall that our findings are not sensitive to this choice.

Figure 2 provides the calculated SSA measures by birth month, now separated out for children falling into each of the two size categories. [Insert Figure 2 about here.] Importantly, the basic underlying pattern can be seen in the trend lines for both groups. Put another way, it seems that parents of both larger and smaller children play an important role here. Parents being willing to send larger children earlier is one important factor driving our results. Similarly, parents wishing to hold back their shorter children also plays an important role. The dashed line captures primary school entry age when children are in the "Tall" group, while the solid line measures the school entry age for children within "small" group. Bu/Bl are the upper bound and lower bounds, respectively, of primary school starting age for the "tall" group, at a 95% level of confidence. Similarly, Su/Sl shows the upper bound and lower bounds for the "short" group. As can be seen, Figure 2 provides some initial visual evidence that children with smaller size start school later, meaning at least some "redshirted" is occurring, as the solid line always lies above the dashed line (i.e., for every month). However, further regression analysis is needed before the significance and magnitude of this effect can be better understood. Importantly, the visual gaps between the two lines do in fact seem to be predictably larger in the summer and early fall than at other points in the year, but that is clearly not so uniform that other odd gaps jump up (e.g., the gap in March). All of our later regressions are specifically designed to model the actual decision to redshirt or not, focusing in particular on the role of proximity to the eligibility cutoff.

We also divide the data by gender, showing the month-by-month SSA patterns in Figure 3 for both boys and girls since we expect parental decision could interact with gender. [Insert Figure 3 about here.] While there is a great deal of volatility in these, and we suspect having even larger sample sizes might remove some of the noise around these mean values, we do see some initial visual evidence that gender matters, as the gaps in the female students' series seem to be larger for both August and September, and also seem to extend into October and even November with larger gaps. Still, there is obviously much to be learned from estimating some simple econometric models using our data, so following our primary research interest, we run an OLS regression relating child height to students' actual school starting age as follows:

$$SSA_{i,j,k} = \beta_0 + \beta_1 Tall_i + \gamma X_i + \beta_2 \phi_j + \beta_3 \omega_k + \epsilon \tag{2.2}$$

Where *SSA* is the actual school starting age for child i, living in Province j, reporting in CFPS wave k. Also, $Tall_{\mathrm{i}}$ is a dummy variable equal to one if a child's height is above average. X_{i} is a set of individual and/or household specific control variables including parents educational level, the child's biological age, and their weight at birth. To control for a host of unobserved cultural, regional, and dynamic trends in society, we follow a standard technique and include Province and Wave fixed effects, represented by the vectors P_{j} and ω_{k}. Finally, ϵ_{i} is an error term assumed to be randomly distributed.

Since we are also interested in whether or not parents of taller (shorter) girls and boys would act similarly, we further add the *Female*Tall* interaction term into the above equation to further isolate whether the redshirting behavior is associated with child's gender. And the new model comes at:

$$SSA_{i,j,k} = \beta_0 + \beta_1 Tall_i + \beta_2 Female * Tall + \gamma X_i + \beta_2 \phi_j + \beta_3 \omega_k + \epsilon \tag{2.3}$$

Where *Female*Tall* is an interaction term of interest that registers equal to 1 if the survey respondent is both tall and female. This model allows us to further isolate the effects gender may play in determining parents' concerns over school entry timing.

As an alternative approach, we also model the 'redshirt' decision directly using a probit model, where an outcome of 1 marks a 'redshirt' decision – the choice to hold an eligible age child back from entering primary school. While the underlying variation in the data is of course the same in both empirical environments, this could be viewed either as a robustness check or an attempt to frame the magnitude of the effect from a slightly different vantage point, focusing on the probability of passing on an eligible enrollment. Specifically, the baseline probit model is as follows:

$$P(Redshirt_{i,j,k} = 1|x) = \phi(\beta_0 + \beta_1 Tall_i + \gamma X_i + \beta_2 \phi_j + \beta_3 \omega_k + \epsilon) \tag{2.4}$$

And adding the *Female*Tall* interaction term into the model:

$$P(Redshirt_{i,j,k} = 1|x) = \phi(\beta_0 + \beta_1 Tall_i + \beta_2 Female * Tall + \gamma X_i + \beta_2 \phi_j + \beta_3 \omega_k + \epsilon) \tag{2.5}$$

While the main focus of our study is clearly modeling the impact of child size on SSA, we are additionally interested in trying to use our data to better understand the various reasons why parents may be motivated by child size in the first place. Hence, in an effort to see how height might correlate with readiness for school and/or the ability to achieve various academic performance levels once enrolled in school, we want to look for the effect of our height variable on academic performance net of any other impacts it may exert on school starting age. Hence, these models all include biological age, SSA, and other controls, as we explore the effects of child height on student test performances from both Chinese and Math scores:

$$\begin{aligned} Score_{i,j,k,g} &= \beta_0 + \beta_1 Tall_i + \beta_2 Above_i + \beta_3 Tall_i * Above_i \\ &+\beta_4 bmonth_i + \beta_5 SSA_i + \Theta_X + \beta_6 P_j + \beta_7 \omega_k + \beta_8 \Phi_g + \eta_j \end{aligned} \tag{2.6}$$

Where score comes from a grade in either Chinese or Math that falls into one of four different categories: Excellent, Good, Average and Poor. $Tall_i$ is the same indicator variable that captures height. $Above_i$ is a dummy variable equal to 1 for children who are born after August 31st (i.e., September-December births). $Above*Tall_i$ is the interaction term of interest, which eventually will show whether or not height/size independently raises or lowers students' ability to perform at school, net of any other impacts that size may have on an individual student's school entry timing. The variable *bmonth* is the child's reported birth month. *SSA* is the actual school starting age. X is a vector of control variables including parental education, child's gestational age, weight at birth, and whether the child suffers from a disability. Several fixed effects are included in the regression in order to capture the unobserved characteristics across different waves and provinces. Since there is no uniform standardized test that cuts across all provinces, levels of school, and grades within levels, we do include a set of categorical variables controlling for the stage of school (e.g., primary school, middle school, high school) the student is currently in. These pair with current grade fixed effects to mitigate any bias that might otherwise be associated with students across schools in different areas taking different exams. Since the dependent variable in this case is an ordered categorical variable, here we use ordered logistic regression to estimate the model.

2.5 Results

Table 2 presents the results obtained from estimating equations (2) and (3), which investigate the various determinants of SSA, including child size. [Insert Table 2 about here.] Columns (1) through (3) show the full pooled sample OLS results, beginning with a parsimonious specification that is included purely for comparative purposes. Column 1 demonstrates that larger children start primary school earlier than their smaller counterparts by roughly 0.08 years, almost exactly a one-month gap. Of course, this is just a starting point. Column 2 adds several important covariates, including the addition of biological age and parents' educational levels (which in this case, essentially proxies for socioeconomic status of the household), and we see the effect declines slightly in magnitude – now more like 20 days instead of 30, with some of the explanatory power shifting towards a reflection that more highly educated parents tend to send their children to school at significantly younger ages. Importantly though, to understand the potentially interactive role gender plays in governing this effect, Column (3) adds our *Female*Tall* interaction term, revealing that the effect is primarily seen for girls, as opposed to boys. The coefficient for taller males registers at -0.0272, which is about 10 days, and also falls just shy of statistical significance at the 90%

level of confidence. On the other hand, the estimated effect for taller females registers at -0.0884 (summing the two applicable coefficients), or just over 32 days. This provides an initial piece of strong evidence that parents carry markedly different sets of preferences for girls than they do for boys.

Still, we want to further understand how the birth month of the child, and specifically its proximity to the eligibility cutoff, influences the magnitude and significance of this relationship. Columns (4) and (5) show how all of these baseline effects become stronger/larger as we limit the sample to the 45% of respondents who are born in June, July, August, September, and October. Within this group, a child is at most 3 months "old enough" to start school and may be up to 2 months "too young". For the former group, parents can legally send the child into school, but are not required to do so. For the latter group, the child is ineligible under the strict letter of the law, but parents and school officials are obviously breaking away from this rule in at least some cases. So put another way, this group contains the cases where parents seem to have actual choices to make.

For our 'near cutoff' birthdays, the estimated effect registers at starting school 34.4 days earlier on average for taller children. In the Column 5 model exploring the role of gender, we very interestingly still see an insignificant effect for boys - although this would be a reasonable point to mention that it is still more statistically likely a true effect does exist as opposed to the conclusion that it does not, but there is no way to reject the null hypothesis of no effect at conventional levels of certainty. Still, the point estimate implies taller boys begin school about 14.6 days earlier. The larger effect is seen from the *Female*Tall* interaction term, where added onto the baseline coefficient we see a point estimate of -0.156 years, or nearly 57 days earlier (later) for taller (shorter) girls. While we are confident the effect is truly larger for girls than for boys, we are not as confident that we have an easy or clear explanation for why. Looking at the raw data on child height, we see the male and female distributions are nearly identical.

One conjecture is that smaller girls may be more targeted more frequently or more intensively by bullies, and are thus more likely to have adverse experiences at school than their male counterparts of the same smaller size. Another is that instructors are less likely to ignore/disadvantage smaller males than they are smaller females, such that the educational penalty of starting younger/smaller is harsher for girls than for boys. Unfortunately, our data do not allow explorations of these competing and potentially overlapping explanations. On a related note though, below we present findings that suggest the impact of height on students' performances in school, conditional on their age and SSA, is similar for boys and girls.

Unsurprisingly, the influence of child size on SSA dissipates into an insignificant relationship once we examine the 55% of observations born in November, December, January, February, March, April, and May. Columns 6 and 7 indicate no significant impact of child height – for either boys or girls, or the sample pooled together – showing that in these cases parents either are not able to, or do not desire to, manipulate the decision of when to have their child begin school. We expect it is likely that both explanations play a role. On the one hand, asking school officials to 'bend' the rule becomes less reasonable in these cases, as now more than 2 months stand between young children and the stated eligibility age. On the other, even smaller than average children for their age are likely starting to blend into their group of peers in terms of size, as children in this range are anywhere from 3 to 10 months older than a child born on August 31st (the eligibility date).

While we are of course confident in the findings from the OLS models exploring the determinants of SSA, another intuitive approach comes from modeling the parents' decision itself directly. Table 3 contains the results from estimating equations (4) and (5), both probit models where "redshirting" is a binary variable that serves as the outcome of interest. [Insert Table 3 about here.] In this context, redshirting is defined as the act of having a child eligible to start primary school in the current year based on their date of birth, but is held back to start the following year. Column 1 begins with a simple baseline model estimated for comparative purposes. All 12,656 observations are included since no covariates are in the model. From this we find that taller children are just over 21% less likely to be redshirted than shorter children. The magnitude of this effect diminishes a bit as we move to Column 2, which adds covariates including biological age, but the effect is still highly significant and about 16.4% in magnitude.

Column 3 explores the role of gender, in this case suggesting the same pattern displayed in the previous results, with the exception that a significant effect is retained even for boys in this case. For taller (shorter) boys, the probability of being redshirted decreases (increases) by about 12%. However, for taller (shorter) girls the likelihood falls (rises) by almost 22%. Column (1) to (3) tested the full sample. Column (2) proves that Tall children give their parents less incentive to redshirt, and Column (3) shows that parents are more possible to withhold daughters if they come smaller than average.

Column 4 and 5 contain the results of probit models that only use children who are born near the eligibility cutoff – using the same June-October horizon from above. Here we again see the effect strengthens as expected. Now the baseline decline in probability of being redshirted if tall drops by nearly 23%, and registers as 15.6% for boys and 31.9% for girls in the Column 5 results exploring the gender interaction term. Somewhat surprisingly, the effect does not seem to completely go away when we shift to the estimations presented in columns 6 and 7 where children are born further away from the eligibility cutoff. The gendered nature of the effect seems to fall away, but the underlying reduction (increase) in the probability of redshirting for taller (shorter) children remains, and stands around 12%. Importantly, this does not mean that parents are 'breaking' the Compulsory law, as children are forced to move forward into school even if they are eligible by several months or more. Hence, these models provide some evidence that child height may continue to influence parents' preferences – albeit to a lesser extent – even when their child's birth month is further from the eligibility cutoff.

As mentioned earlier, we want to complement our analysis of the timing of school entry decisions and how they relate to child height with another set of regressions that explore the effect of height on school performance/achievement in China. Table 4 shows the results of estimating several versions of equation (6) that consider Reading Scores. [Insert Table 4 about here.] Although it is somewhat confusing at first glance, recall that in this context, strong performance in school means achieving a lower number – much like scoring in golf! Students earning a score of 1 are in the highest performing category, while scores like 3 and 4, mark average and poor performances, respectively. So for example, one could note that our tall variable, as well as biological age and parents' levels of education will always register negative and significant results, which means better academic performance in our data's preexisting scheme.

While a number of different specifications explore the effect of including/excluding control variables that are known to correlate with our "tall" variable, the first and second columns present what we believe can be viewed as the lower and upper bound, respectively, on this particular effect of interest. On the one hand, we know endogeneity from reverse causality is impossible, since a student's performance in school could not possibly influence their height (that we are aware of). On the other hand, family characteristics like higher socioeconomic status (as measured by levels of parents' education) may lead to better nutrition and more physical growth, but then may also be correlated with other unobservable positive family characteristics that help children perform better at school.

Hence, column 1 shows the smallest impact of height – essentially a 'pure' or residual effect of height that remains after controlling for those other factors, whereas column 2 shows how one would adjust the predicted school performance if taller versus shorter child comparison was the only known information. Following that logic, the impact of shifting from a shorter than average child to a taller that average, child, controlling for no other factors, falls at roughly one-half of a performance "level". However, after controlling for biological age, SSA, birth weight, parental education levels, and disability status, only about one-third of that initial impact remains. This comes from column 1 where the impact of the Tall variable is reduced to about one-sixth of a one level movement in performance.

Table 5 shows the same sets of regressions, but not examining Math Scores as opposed to Reading Scores. [Inserts Table 5 about here.] These results can easily be summarized by pointing out all the same patterns hold, and the magnitudes grow ever so slightly. In fact, the two point estimates are so close, we could not reject a null hypothesis that they are the same. For that reason, our work has little to say about the relationship between child height and their acquisition of verbal/reading learning relative to quantitative/math learning, other than that our best guess is the two underlying processes are similar to one another.

Our last sets of results are presented in Table 6. [Insert Table 6 about here.] They show that when examining reading scores, the role of height seems nearly identical for girls and boys, but when shifting to performances in math, perhaps taller boys carry a slightly larger premium. The point estimate for tall boys shows an extra 15% of one performance level coming from being taller, but we would emphasize this result should be interpreted cautiously given the size of the estimated standard errors. Interestingly, girls born after the eligibility cutoff date also seem to perform significantly better in both reading and math, even controlling for biological age and SSA, but the same result is not found in the regressions for boys. Still, seeing as the coefficients on the "Birth Month" variable are somewhat volatile, there is clearly some underlying simultaneity influencing the overall system we are using to try to track both biological age and school starting age – a reasonable problem to run into given the underlying correlation between those variables.

2.6 Conclusion

This paper has taken two broad literatures, one investigating the effects of school starting age on child learning outcomes and another investigating the effects of child size on leaning outcomes, and has used insights from both to study the effect of child size on SSA. To accomplish this, we use CFPS household level data from China over the period 2010 through 2016, modeling both SSA as well as the actual decision to redshirt. We complement these with other regression that explore the effect of child height on academic performance in reading and math, controlling from SSA and biological age.

The overall conclusion of our paper is that child size influences many different aspects of the pathway followed by young people as they accumulate human capital. Initially, taller (shorter) children of otherwise equivalent ages are more (less) likely to begin school at younger ages. We present robust evidence that SSA outcomes are lower for taller children in China, even controlling for biological age, and that this effect is significantly stronger for girls than for boys. While not a true 'placebo' test, the models that use only children born further away from the eligibility threshold date of August 31st to show that parents of shorter children have a significantly compromised ability to engage in redshirting when their child was born in the winter or spring. The impact of height on SSA goes away when focusing on this group, and while a small effect still registers among this group in the redshirting outcome probit models, it is a considerably smaller effect. On the other hand, we show the role of height plays an enhanced role when children are born near the cutoff. For example, we find that taller girls born in the summer and early fall months surrounding the cutoff have nearly two full months shaved off their average SSA outcome and are 32% less likely to experience redshirting than shorter girls of the same biological age.

Why the relationship is so much stronger for girls than for boys is a question over which we offer a few of our own conjectures, but to be fair, have very little definitive evidence on. Hopefully this leads to opportunities for interesting future research on the topic of gender interactions with child size and SSA.

Chapter 3

Does the Early Bird Catch the Worm? School Starting Age in China: Short and Long Run Impacts

3.1 Introduction

Governments of all types spanning the globe typically create laws concerning the age requirement for kindergarten and/or primary school entry. However, opinions about the 'optimal' school starting age - and even over whether or not starting younger or older on average is a positive or negative influence on children's lives- are held in ways that seem to differ across developed countries versus developing countries. For example, in the United States, researchers consistently find that children who start school later are more mature, and thus better ‘prepared’ to be educated, when compared to their younger companions within a given class cohort. Hence, the elder children within a given cohort perform better on test scores, have reduced levels of criminal activity, and experience enhanced mental health (Cook and Kang (2016),Dee and Sievertsen (2018), Elder and Lubotsky (2009)).

In contrast, many developing countries have produced data sets suggests that starting school earlier leads to eventually higher levels of overall education – an intuitive result given that leaving school early/young is far more common in developing countries than it is in higher income countries. Importantly, children in China who delayed their school entry timing are also more likely to come from lower-SES families, since they might not be able to afford the tuition and fees for their children to begin school earlier. Moreover, many Chinese parents hold a cultural belief that “the early bird catches worm”. Put another way, this frames the idea that entering school earlier leads to graduating/leaving earlier, and that finishing school sooner in turn leads to working and earning income earlier. As a result, it is common in China for parents to want their children to start school as early as possible. Interestingly, whether one focuses on developed or developing environments, there have been a number of studies focusing on the various impacts of school starting age (SSA) on a host of different outcomes – most commonly with the focus being on shorter term academic performance.

This paper focuses on school starting ages in China, investigating whether or not it is actually beneficial on net for a child to enter primary school at a younger age. This is accomplished using data from the China Family Panel Studies; more specifically by observing children’s test performances while still in school, as well as adults’ eventually completed years of schooling, average monthly wages, and age at first marriage. The results of this exercise show very little longer term impacts of the SSA variable, although we do support the typical finding in the literature that SSA does positively impact academic achievement for young people while they are still in school.

According to the Chinese Compulsory Education law, which was first implemented and took affect forward from July 1st, 1986, children in China are required to reach the age of 6 years old by August 31st of the school year in question to become eligible to enroll in primary school. Effectively then, the law strongly differentiates children born just before August 31st, from those who are born in September or later, just after the cutoff date. Based on this policy and attached eligibility cut-off date, recent studies (Zhang, Zhong and Zhang (2017), Liu and Li (2016)) investigate the impacts of this "natural experiment" in China, focusing on outcomes including children's cognitive skills and test performances. My research also makes use of this natural experiment, applying a regression discontinuity approach as in common in the SSA literature, in an effort to move beyond short term cognitive outcomes (e.g., test scores or grades while still in school) to see if this policy and the parallel effects of SSA not only had impacts on academic tests/achievement during adolescent years, but also whether or not any initially significant effects are retained to a point where longer run outcomes including total education attainment, eventual wages/salaries, and age at first marriage surface as being meaningfully impacted by ones SSA. Using both the child and adult panels from China Family Panel Survey (CFPS), my empirical results show that children born just after the eligibility cut-off date perform better on average on Chinese language/grammar tests, but also that no similar significant results surface when considering their achievement in Math. The advantages in verbal test performances are also found to be somewhat stronger among older girls, as compared to otherwise similarly aged boys.

However, this early advantage in language proficiency does not seem to enhance individuals' later life outcomes in education, earnings, and personal decisions in marriage – for either women or men. Results of this nature are particularly important for both policy makers and academics to understand. Whereas an intuitive assumption would be that short term gains in academic achievement translate into other longer terms gains in social and economic pursuits, the present findings paint a picture of longer run convergence, and would tend to minimize the overall importance of the effects of SSA. In particular, no evidence is found to support the idea that aggregate welfare gains could be achieved through a strategic manipulation seeking to increase SSA among the population (i.e., delay school entry by requiring youth be older when they begin formal education outside the home).

The remaining portions of this essay are as follows. Section 2 presents a stripped down review of a very large literature that developed around the measurement of SSA, focusing most intently on the relatively few studies that have investigated the effects of SSA using recent data from China. Section 3 describes the CFPS data used to carry out the empirical work. Section 4 briefly outlines the standard RD approach to modeling the effects of SSA on various outcomes of interest, while Section 5 reports the results of our empirical investigation. Section 6 concludes with a brief summary of the findings paired with comments on how they might be useful to policy makers.

3.2 Literature Review

While reviewing the entire population of studies taking up issues related to SSA is a task far beyond the scope of the current exercise, the literature can be accurately characterized as containing a highly heterogeneous set of results. SSA has been linked to a diverse set of outcomes including student cognition/achievement (typically measured using either standardized test performances or grades), BMI index, problem solving skills, grade retention rates (i.e., being 'held back' upon finishing a grade), mental health outcomes during childhood, eventual levels of education attainment, various outcomes related to labor market interactions, marriage and fertility outcomes, and a handful of other items. Needless to say, conclusions varied across studies and across countries.

For example, several papers in the literature (e.g., Stipek (2002), Bedard and Dhuey (2006), Datar (2006), McEwan and Shapiro (2006)) found that older children (i.e., higher SSA) are more likely to score higher on standardized tests and less likely to repeat first grade. They attribute this to a general benefit of additional age for students, such that older students have more "readiness" and maturity upon entering school. Within this large literature, most of those conclusions are drawn in developed countries, such as United States, OECD countries, Chile, etc. However, in some developing countries, researchers reported children with lower values of SSA have better IQ scores, controlling for age, when compared in that testing environment to children of similar age, but who entered primary school later. Conducting a study using data from Norway, Black, Devereux and Salvanes (2011) showed that students who were younger when beginning school had better performances for various test performances.

Besides the test score, some paper focused on children's mental health as well as teenagers' crime rate. Dee and Sievertsen (2018) looked into displayed rates of inattention and hyper-activity for Danish children at age 7. They found that older school entrees more effectively self-regulated and experienced fewer behavioral problems at school. Landersø, Nielsen and Simonsen (2017) concluded that a one year delay to entering school, for students close to the eligibility threshold, significantly reduces the probability of commit crime later in life. To summarize many different papers in this large literature, a consensus that arises from use of data in higher income developed countries is essentially that delayed entry (higher SSA) is beneficial to students in their childhood and enhances cognitive development, particularly when measuring within-school-grade focused outcomes. The set of conclusions is more nu-anced when studies take the approach of controlling for biological age, as opposed to looking at within-grade outcomes.

Several research papers have also focused on longer-term effects of school starting age. Some of these studies found no significant impacts on eventual levels of education attain-ment and earnings/wages outcomes (e.g., Black, Devereux and Salvanes (2011), Fertig and Kluve (2005), and Fredriksson and Ockert (2005). McCrary and Royer (2011). Interestingly, McCrary and Royer (2011) focused on school starting age and connections to female educa-tion levels, fertility rates and infant (offspring) health, finding very modest impacts on the likelihood of becoming mothers, age giving birth to babies and the probability of birthing healthier infants.

Somewhat surprisingly given the size and importance of the Chinese economy, there have only been a few recent papers that have considered the effects of SSA in China. Like the present investigation, where these studies have taken up questions related to SSA in China, they also focused on the role of China's compulsory education law and how it related to primary school starting age. A recent paper by Zhang, Zhong and Zhang (2017) focused on junior high school students (i.e., early teenagers) and how their cognitive skills were affected by their primary school starting age. Unlike other studies using data from the US and Europe, they found that delayed school enrollment had a significantly negative impact on students' cognitive scores, and they found stronger effects for girls and for students living in rural areas, as compared to boys or students from urban areas.

Additionally Liu and Li (2016) implemented a self-conducted survey, generating their data from the City of Kunming. They also focused on SSA and its potential impacts on educational outcomes, showing that younger students in their data performed better in school (measured by test scores) than their higher SSA counterparts. While the literature using Chinese data has been interesting and potentially informative, there still remains much to be learned about the effects of SSA in China. For example, there have been very few studies conducted using large, nationally representative databases from China. Moreover, to our knowledge, this paper provides the first example of a study that uses a data source satisfying that criteria to simultaneously investigate the potential impacts of SSA on both shorter term (i.e., academic/school performance) and longer term (social/family/labor) outcomes.

3.3 Data

To get detailed information of the outcomes of interest, we make use of four consecutive survey waves from the China Family Panel Studies (CFPS) dataset. The CFPS is a large and nationally representative panel survey data set, collected and maintained by the Institute of Social Science Survey at Peking University in China. The survey started in 2010, then continued with follow up surveys in 2012, 2014, and 2016 that we also make use of. The dataset itself contains two distinct components: the children's dataset contains information from adolescents who are less than 16 years old, while the adult's dataset reports measures for all survey participants aged 16 or above. Importantly, unique id code variables allow continuity and merging of information between the two initially distinct sources.

While the CFPS data is not perfect, it displays several highly desirable traits that make it a good fit for the current exercise. First, it is a large nationally representative sources, protecting the results from selection bias and providing a large number of observations for later analysis. Additional, it reports the initial year of primary school entry for children as well as adults. While the former is often available in data sets focusing on children, the ability to pair school starting age with other longer term (i.e., adult age) outcomes is a nice advantage of the CFPS data.

By combining the school year entry information with each respondent's birth year and month, we are able to compute each respondent's actual primary school starting age (SSA).

Specifically, we compute the SSA variable as:

$$SSA_i = (YearEntry_i - BirthYear_i) + (9 - BirthMonth_i)/12^1 \tag{3.1}$$

where *SSA* is the actual primary school starting age for individual i, $YearEntry_i$ is the calendar year during which individual i entered primary school, $BirthYear_i$ and $BirthMonth_i$ are the surveyed birth year and month for individual i, respectively.

For a number of reasons, we trim a very small fraction of the data at it represents extreme outliers in regards to school starting age. Specifically, we require all observations to have a value for SSA that falls within the range of 5 years and 0 months, all the way up to 8 years and 11 months. Thus, a very small number of cases where SSA was reported to be 4 years 11 months or less, as well as a handful of cases where SSA was reported to be 9 years or older, are dropped. We expect this enhances the accuracy of our empirical work, as these must either be data entry errors or very extreme cases where some other (likely unobservable) factor plays a major role in determining SSA. Figure 1 depicts the relationship between the average school starting age and different birth cohorts. [Insert Figure 1 about here] From Figure 1, we can clearly see how passage of the Compulsory Education Law rapidly accelerated an already ongoing trend leading to earlier entry to primary school. Individuals born in the decade of the 1970's were essentially unaffected by the law, as its passage in 1986 meant they would have already aged into primary school under the old regime. Still, even within this group, SSA drops by roughly 2.5 months if we compare the cohort born in 1972 with

[1] We followed the Elder and Lubotsky (2009) Elderly & Lubotsky (2009) paper to compute the actual SSA.

the cohort born in 1980.

The dramatic policy-driven shift can be seen by comparing the 1980 and 1981 birth year cohorts, as the SSA value drops nearly 4 months over that single transition. Of course, if we instead "grouped" the birth year data using September-August 12-month range, as opposed to the standard January-December 12-month range, this drop would register as being even larger. Also of note, the pattern of lowering SSA values continues, presumably through a combination of enhanced levels of enforcement of the law over time, as well as ongoing changes in parental preferences over sending their children to school at younger ages. All told, the average SSA falls to just under 7 years for the 1991 cohort, which is roughly 9 months less than the initial 1972 value observed less than two decades earlier. Hence, it is fair to characterize this as a meaningful shift in the schooling environment, as children are attending school almost a full year earlier than the previous generation.

Recall that the present research uses both the children's and adult's data, merging information originally from survey waves 2010, 2012, 2014, and 2016, to obtain the maximum number of initial cases. Specifically, we build a repeated cross sectional dataset from surveyed children and adults, by appending the four distinct survey waves together. Of course, several variables in the data are time invariant; for example, an individual's birth date, primary school entry age, certain family level attributes, and essentially all of the demographic characteristics. For these sorts of time invariant measures, it matters very little whether we use only a single survey wave, as compared to using all four. Still, our number of observations increases as we use the additional waves. Of course, the data from even a single wave is easily large enough to uncover any significant effects of SSA that may be present. On the other hand, a few key outcomes – for example test performances or monthly wages –

these may actually differ for the same respondent across waves – such that new survey waves contain new meaningful information.

Table 1 provides the summary statistics for the dataset, which contains both the children and adult sub-groups. [Insert Table 1 about here.] The children's dataset contains 12,660 distinct observations containing the respondent's birth date, school performances, levels of parental education and other household traits, as well as several commonly examined demographic variables. The average primary school entry age of children is 6.44, with boys being just slightly higher than girls' school starting age, but not to a statistically significant difference in levels.

The adult data set contains 18,898 initial observations, each of which provide variables including year of birth, number of years of schooling, age at first school entry, average monthly income, marital and parental status, and a host of other traits. The summary statistics show that the average primary school starting age for the adult sample is 6.87 years, which is considerably higher than the children's data average, as was expected given the historical progression of SSA trends in China over the past several decades. Interestingly, while there is obviously some variation in levels of educational attainment in China, the average number of years of education in the adult sample is 11.13 years – somewhat close to the full 12 years that would complete the standard trajectory. The adult portion of the data contains only individuals who were born earlier that 1979, which is essentially the year of birth pinning down the first cohort affected by the later passage of the 1986 Chinese Compulsory Education Law. Hence, one was to characterize the split of our observed children's and adult's data is that, roughtly speaking, they respectively were (for the children's) and were not (for the adults) entering school under the Compulsory Education Law environment.

3.4 Model

To investigate the effects of SSA using a regression discontinuity approach, the first thing that needs to be established is whether or not parents are strategically timing the births of their children with respect to the school eligibility cutoff date (i.e., comparing levels of August versus September births). If there is strategic manipulation around the relevant RD cutoff point, that must then be accounted for. While, a McCrary-type density test is commonly used for establishing this, and in fact we do pass that test easily, one can also see from the simple visual evidence presented in Figure 2 that timing-of-birth issues are not present in our data – at least not for the SSA threshold separating late August births from early September births. There is minor but interesting seasonality in the Chinese birth data, as has been shown by others, where we see a small but meaningful spike in October births – as expected since October falls nine months after the high celebration period of the Chinese New Year in February. However, for the purposes of our exercise, the flat transition between August and September births is important. This provides confidence we are not experiencing manipulation of birth around the August 31st cut-off date.

Interestingly, in other research, we have verified that parents engage in "redshirting" behaviors when it comes to making decisions over when their children begin school, and that these decisions are influenced by the physical size (i.e., height) of the particular child in question. While this marks an interesting development and a likely avenue for future research endeavors, the present analysis of the effects of SSA on the various short and longer run outcomes we are interested in has been conducted using methodologies that would be considered standard in the literature (i.e., that ignore any significant interactions between child level traits and SSA values). Also, the typical RD results still provide meaningful information even in the presence of strategic parental choices regarding the timing of school entry.

Before applying the RD approach, we also need to investigate the underlying relationship between our school starting age (SSA) variable and individual's month of birth, to verify whether or not the Compulsory Education Law has the effect we initially expect it to display. Figure 3 provides this information in a simple visual manner. Note the considerable increase in SSA seen when moving from month 8 (August) to month 9 (September), as SSA jumps by almost exactly three months between the two, with another smaller jump upward moving from September to October. [Insert Figure 3 about here.]

We then follow standard procedures in the SSA literature and first build a linear model to directly investigate the first stage relationship between SSA and birth month. The expectation is that, given the nature of the policy, those born after the threshold, even after controlling for the effect of birth month in a linear fashion, will have significantly higher values of SSA (i.e., start school later.) The first stage regression model using the full (children's and adult's) data set is:

$$SSA_i = \beta_0 + \beta_1 After_i + \beta_2 BirthMonth_i + \gamma X_i + \epsilon \tag{3.2}$$

Where SSA_{i} is the actual school starting age for individual i, and $After_{\text{i}}$ is a dummy variable equal to one if the individual is born on September 1st or later (i.e., after the school eligibility cutoff). $BirthMonth_{\mathrm{i}}$ captures the individuals' birth month information, and X_{i} contains a set of control variables including parental education and province, birth year information. β_1 is the primary coefficient of interest, although to be "working" as expected the coefficient β_2 should be significant and negative, whereas β_1 should be significant and positive.

Table 2 provides the empirical results from estimating equation (2). Column (1) and (2) show the full pooled sample OLS results. [Insert Table 2 about here.] The coefficient in Column (1) registers a 0.44-unit delay of entering primary school if one is born after September 1st. Since we are measuring SSA in years, this translates roughly to a 160-day delay, or about five and one-third months. Column (2) suggests a smaller impact is registered after adding controls and fixed effects into the model. This reduction is intuitive, as the significance of the regional and cohort specific fixed effects suggests the relationship between biological age and SSA varies across both geography and time periods in China. Neither of these results is surprising, as regional norms and shifting cultures are nearly always expected to influence the decisions of parents and/or school administrators who collectively decide when children will actually enter classes. The consistently negative effect of mother's and father's level of education on SSA, even after controlling for biological age and the eligibility threshold, suggests that more highly educated parents – and also presumably higher income parents as the data supports that clear and strong correlation – prefer to have their children begin school at younger ages. This may reflect a desire within these households to obtain more formal educational for their children, a better ability to pay at the household level, a higher opportunity cost associated with a parent providing the childcare services, or some combination of all three factors. Column (3) provides the results obtained when restricting the sample by birth month, using only individuals born between June and November (i.e., the half of the year containing the three months prior to, and directly after, the August 31st cut-off). The same pattern is seen, while the magnitude of the coefficient on biological age dropping dramatically is expected – as the portion of the year where the cut-off makes little difference in terms of influencing SSA (leaving biological age to play the dominant role in its determination) – the same desired pattern associated with the threshold cut-off variable is still seen quite clearly. The final two columns show the full model split by gender, finding very few meaningful differences when examined in this manner. Importantly, across all these various first stage estimations, we see the needed result that SSA is dramatically affected by the individual's position relative to the August 31st eligibility cut-off. Since the first stage exercise does indeed show a consistent positive and significant relationship between the school starting age and birth month threshold, we ran several subsequent 2SLS models in order to estimate the causal impact of SSA on our various outcomes of interest:

$$S\hat{S}A_i = \beta_0 + \beta_1 After_i + \beta_2 BirthMonth_i + \beta_3 After * BirthMonth_i + \gamma X_i + \epsilon \tag{3.3}$$

$$Y_i = \gamma_0 + \gamma_1 S\hat{S}A_i + \gamma_2 After_i + \theta X_i + \xi \tag{3.4}$$

where Y_{i} represents various outcomes of interest, including test performances on Math and Chinese exams for respondents in the child's data that are used to investigate the short run impacts of SSA, as well as completed years of schooling, the natural log of average monthly wages, and age at first marriage, which were all used to measure the effects of SSA on longer run outcomes within the adult data set. Test performances come from a grade measuring student performance in either Chinese language or math. While an ideal measure would be a continuous measure, we observe a scaled outcome where 1 records “excellent”, 2 denotes “good”, 3 indicates “average”, and 4 registers the performance as “poor”. Additionally, $\hat{S}SA$ is of course the predicted value for the individuals SSA taken from the first stage estimation of equation (3). Our eventual coefficients of interest all come from the $\gamma 1$ term in equation (4).

3.5 Results

The results from all of our main RD estimations are presented in Tables 3, 4, and 5. [Inserts Tables 3, 4, and 5 about here.] Table 3 contains results of models where both females and males are included in the regressions, with Table 4 and 5 showing the results obtained by estimating the models for females and males, respectively. Each column presents results from a distinct outcome of interest. Columns (1) and (2) use the children's data to investigate short-run impacts on academic achievement, while columns (3), (4), and (5) use the adult's data to investigate the longer-run impacts on educational attainment, labor market outcomes, and entry into marriage.

The results from the first two columns provide convincing evidence that SSA does in fact exert a casual and positive impact on students' academic performances while they are still in school. Bear in mind our students could be in primary school (the first six years of formal schooling in China), or could have advanced into secondary school (the second six-year block, or what would essentially be middle and high school in the US). Hence, some of what we are defining as 'short-run' impacts are in fact trending towards what many would call medium run impacts in the education literature. The effects are strong and clear. Recall that our performance scale is such that 1 is "excellent", while a 4 is "poor". Hence, significant negative effects in the estimation results are actually gains in student performance levels.

A student with a one unit (one year) lower SSA value is likely to score 0.287 units lower (which is a better performance) on their Chinese language recorded performance. This is significant at even the highest levels of confidence. On the other hand, the enhancement in math performance is observed, but is smaller and also does not attain significance at traditional levels. The same one unit reduction in SSA would only lead to an estimated drop of 0.131 in the performance category – so just less than half the size of the effect for Chinese language performance – and the estimated standard error is just slightly smaller. In a sample with this many degrees of freedom, it does call into question whether or not SSA does in fact exert a causal impact on math performances, at least in the context of China over these recent decades. Of course, we are controlling for the effect of parental education levels in these models, so the best way to interpret this result would be that after controlling for those significant effects, pure SSA related effects are perhaps no longer present for mathematics, but are for reading/language.

Shifting the discussion to the potential longer term impacts of SSA, Columns (3) through (5) show the impact of SSA coming from the adult data set on years of schooling (educational attainment), log average monthly wages, and age at first marriage, respectively. Consistent with many studies that considered the effects of SSA in higher income countries, we do not find evidence for significant long term impacts of SSA on any of the three outcomes. The most likely possibility of a meaningful effect comes from age at first marriage, where the point estimate of a one-half year reduction in age at first marriage is just slightly larger than the estimated standard error. The other two estimations produce point estimates that suggest higher SSA leads to gains in both educational attainment and eventual wages, but neither comes close to achieving significance at conventional levels of certainty.

Recall that Table 4 and Table 5 provide results for the same five 2SLS regression models, but now segmented by the reported gender of the respondent. Table 4 shows the results for females, where we find positive and highly significant impact on Chinese language scores, with the magnitude of the point estimate increasing by just over 33 percent. Interestingly, the coefficient for math more than doubles – now registering at -0.312 – and attains statistical significance at the 90 percent level of confidence. Viewed in isolation, this would suggest the effects of SSA on academic achievement in math is much stronger for girls than for boys. While we suspect this is likely, another relevant point is that other work of our own has shown parents engage in strategic and highly systematic 'redshirting' of their children, and that redshirting has important connections to the gender of the child. Hence, the observed gendered effect should be interpreted with caution. Of course the other half of this gendered picture is that the male observations in the sample are clearly the reason the full sample RD estimation did not produce a significant effect in the math score estimation. In fact, we see the point estimate for boys actually moves in the opposite (unexpected) direction. This would be hard to believe, that is to say that older/higher SSA for boys was causing math performance to decline.

While this puzzling result is not statistically different from zero, it is somewhat suggestive that the dynamics at play are more nuanced than is commonly believed within this large literature. If we link this research to other currently ongoing work where we show taller (shorter) boys in China are sent to school at younger (older) ages, a plausible explanation is that parents are observing some aspect of their child's underlying talent/ability that the econometrician is not, and thus selection around the relevant SSA cutoff may be occurring. Recall that while we are able to verify that births are continuously distributed around our cutoff point (i.e., the McCrary density test passage), the behavior of parents in making potentially strategic (i.e., non-random) choices surrounding when to send their sons and daughters to primary school is not directly taken up within this paper.

Regarding the long run impacts of SSA and the role of gender, very little is obtained in the way of statistically significant results. In summary then, we are comfortable with the statement that SSA seems to have very little causal impact on any of these longer run outcomes. On the other hand, there are some interesting pieces of suggestive evidence. We find it interesting that the sign of the coefficient on labor wages/income for females' flips from the expected positive to now being negative (although clearly indistinguishable from zero). Starting school at a later age seems to raise the eventual level of educational attainment for girls, with the opposite direction of effect for boys – although neither attains significance.

3.6 Conclusion

This paper investigates the impact of primary school starting age on both short run and longer run outcomes, using a regression discontinuity design framework and data from a large nationally representative data set from China. We estimate a causal effect of 0.378

years (or roughly 138 days) in delayed school entry (higher SSA) for individuals born just after the cut-off date in the first stage, suggesting that the cut-off date set by the Compulsory Education Law did in fact strongly affect parents' decision making regarding the timing of school entry, but not through 100 percent (full) adherence to the policy.

SSA was shown to significantly influence student performance while still in school. We find children who begin school later (earlier), controlling for their biological age, are in fact able to perform better on Chinese language scores and math scores, although the overall effect seems stronger for language than for math. Moreover, these better performances on school exams do not seem to translate into greater educational attainment or higher wages in the longer run, nor do they seem to raise or lower the expected age at first entry into marriage – all findings that are consistent with at least the lion's share of the studies that have used data from higher income countries to investigate the effects of SSA. We do see some interesting evidence that suggests the role of SSA is stronger for girls that it is for boys, as well as some suggestive evidence that both girls and boys are being placed into school using somewhat different underlying behavioral models on the part of parents – as for example the sign of the estimated effect of math scores flips away from our initial expectations.

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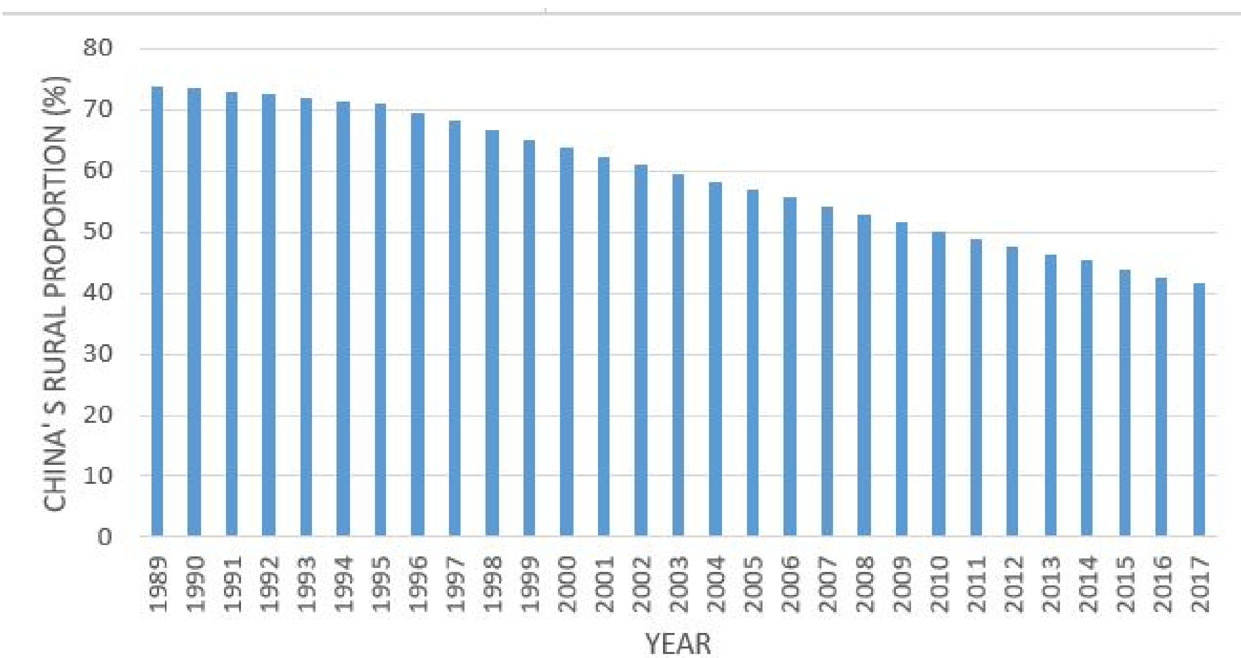
Figures

Chapter 1 Figures



Data Source: National Bureau of Statitics of China (Annual Data).

Figure 1.1: China Rural Proportion(1989-2017)

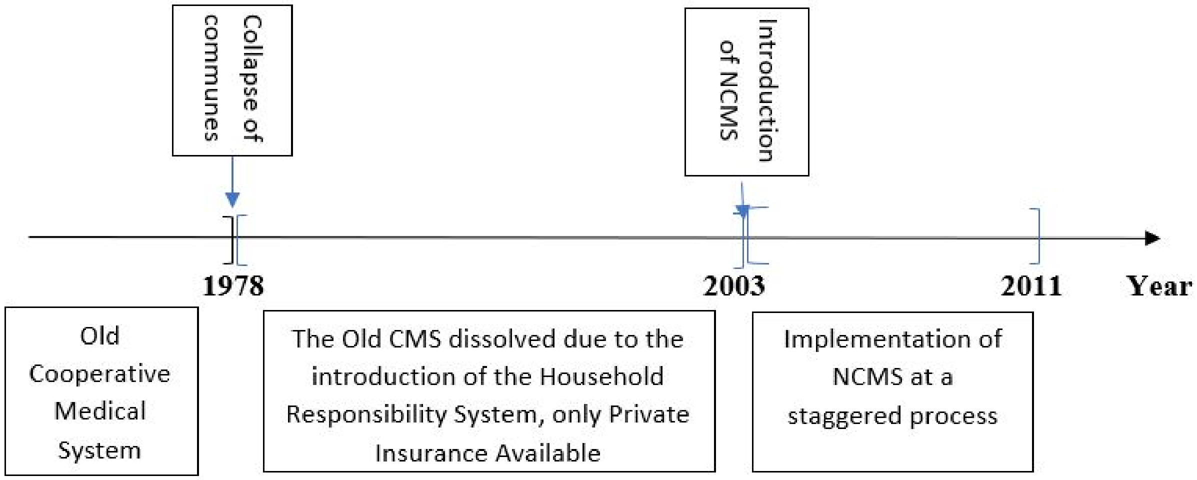


Figure 1.2: Timeline of Rural Area Public Healthcare

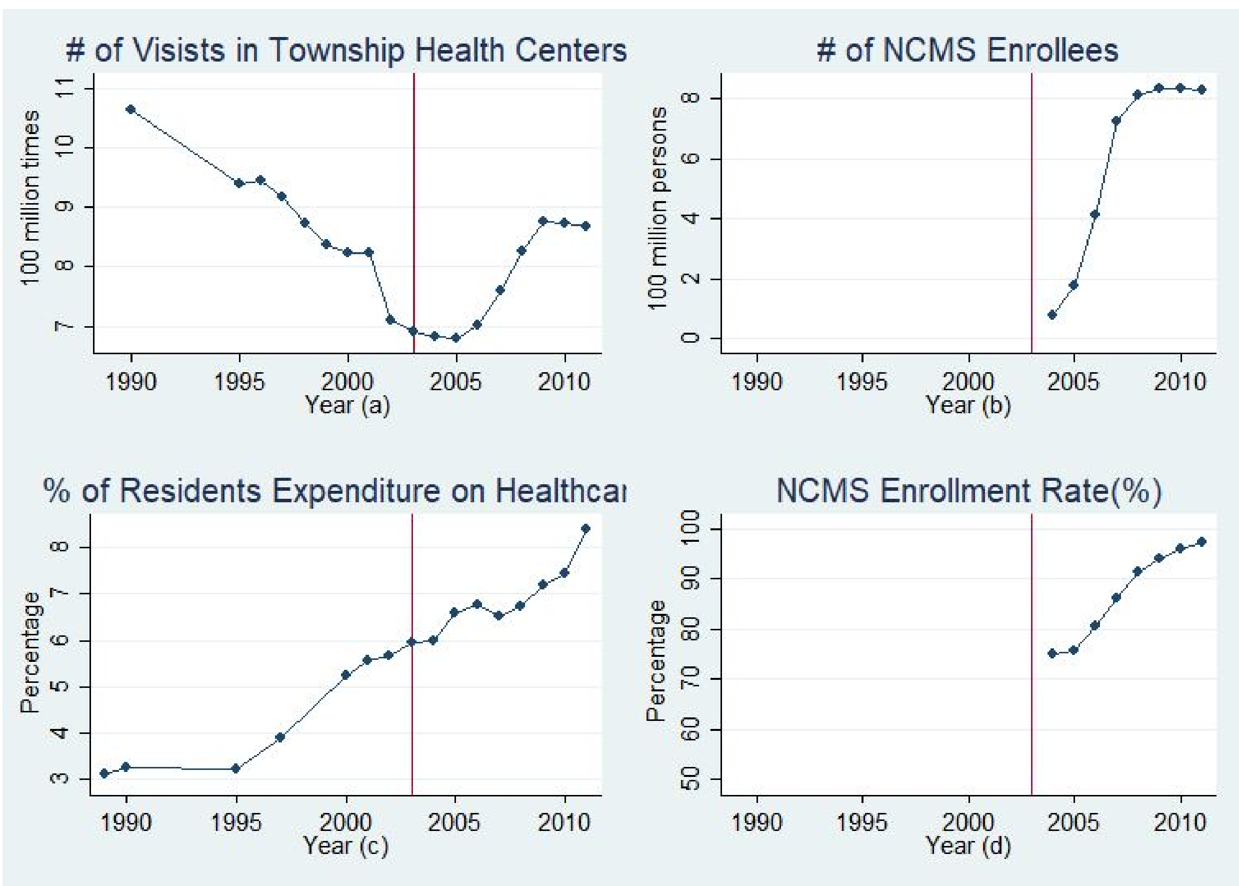


Figure 1.3: Timeline Trends for Relevant Variables

Sources: the CHNS website

Note: The darker shaded regions are Provinces where the CHNS was conducted

Figure 1.4: Map of CHNS Survey Regions

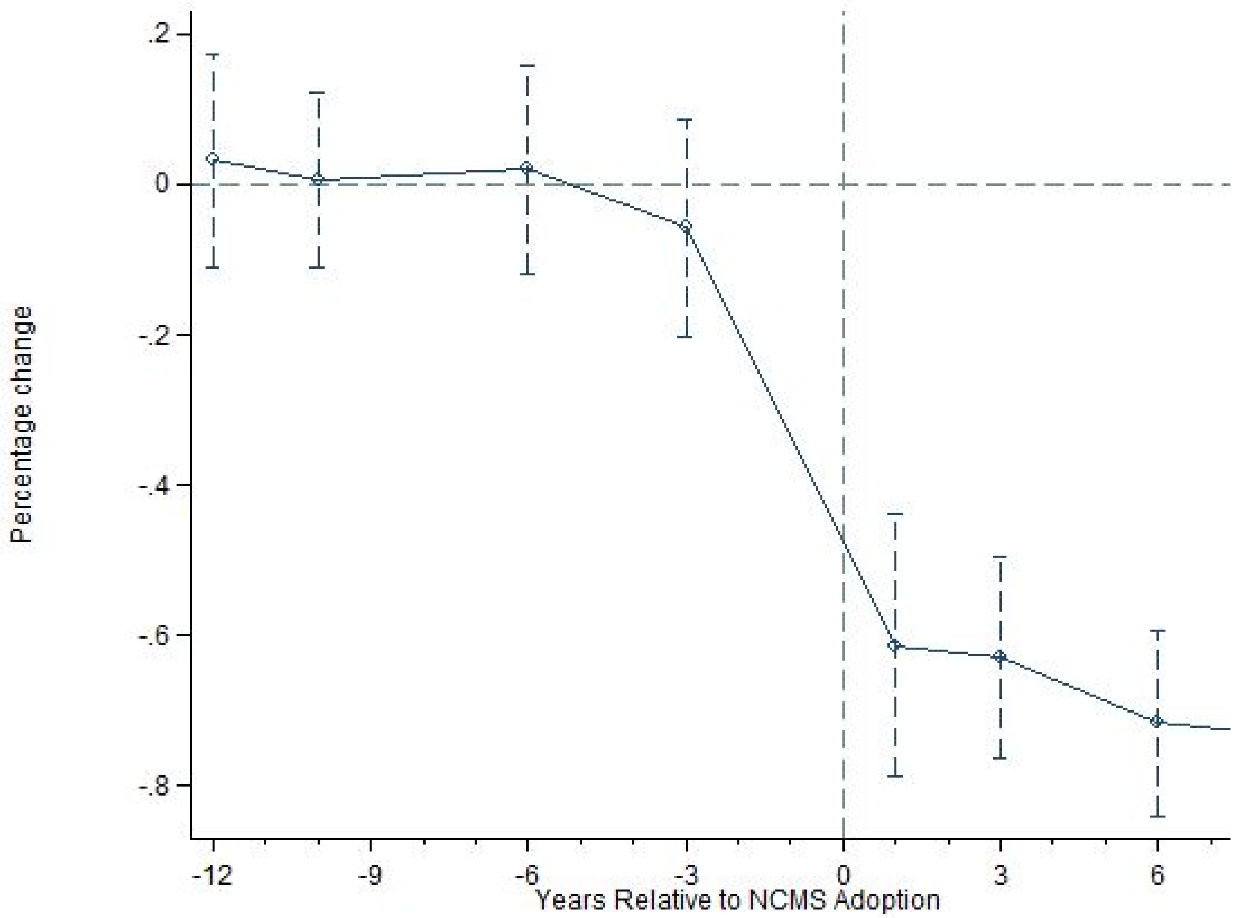


Figure 1.5: The Dynamic Impact of NCMS Enrollment on Annual Work Hours for Rural Residents in China

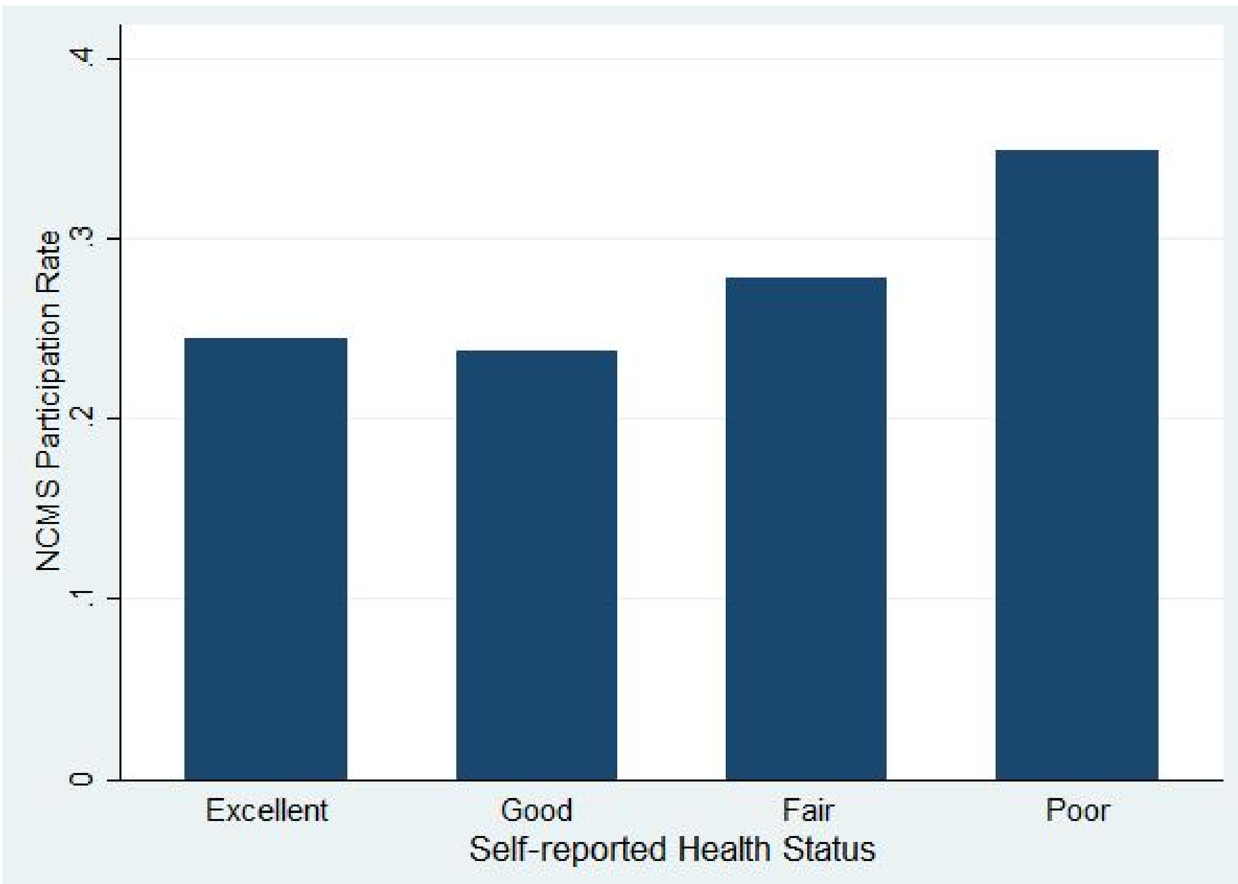


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Figure 1.6: NCMS Participation Rate by Health Status

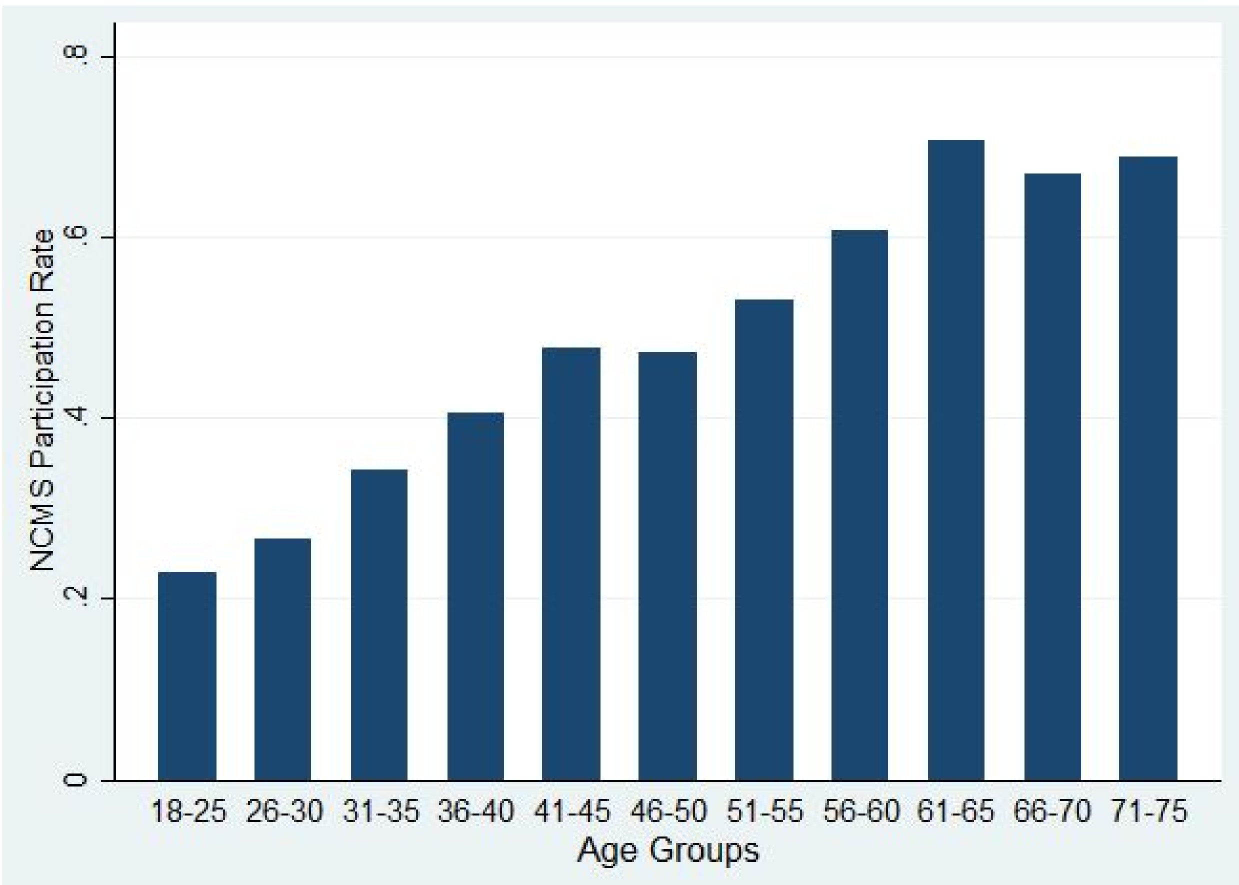


Figure 1.7: NCMS Participation Rate by Age Group

Figure 1.8: NCMS Participation Rate by Income Quartile

Chapter 2 Figures

Figure 2.1: Primary School Starting Age (SSA) and Child's Birth Month

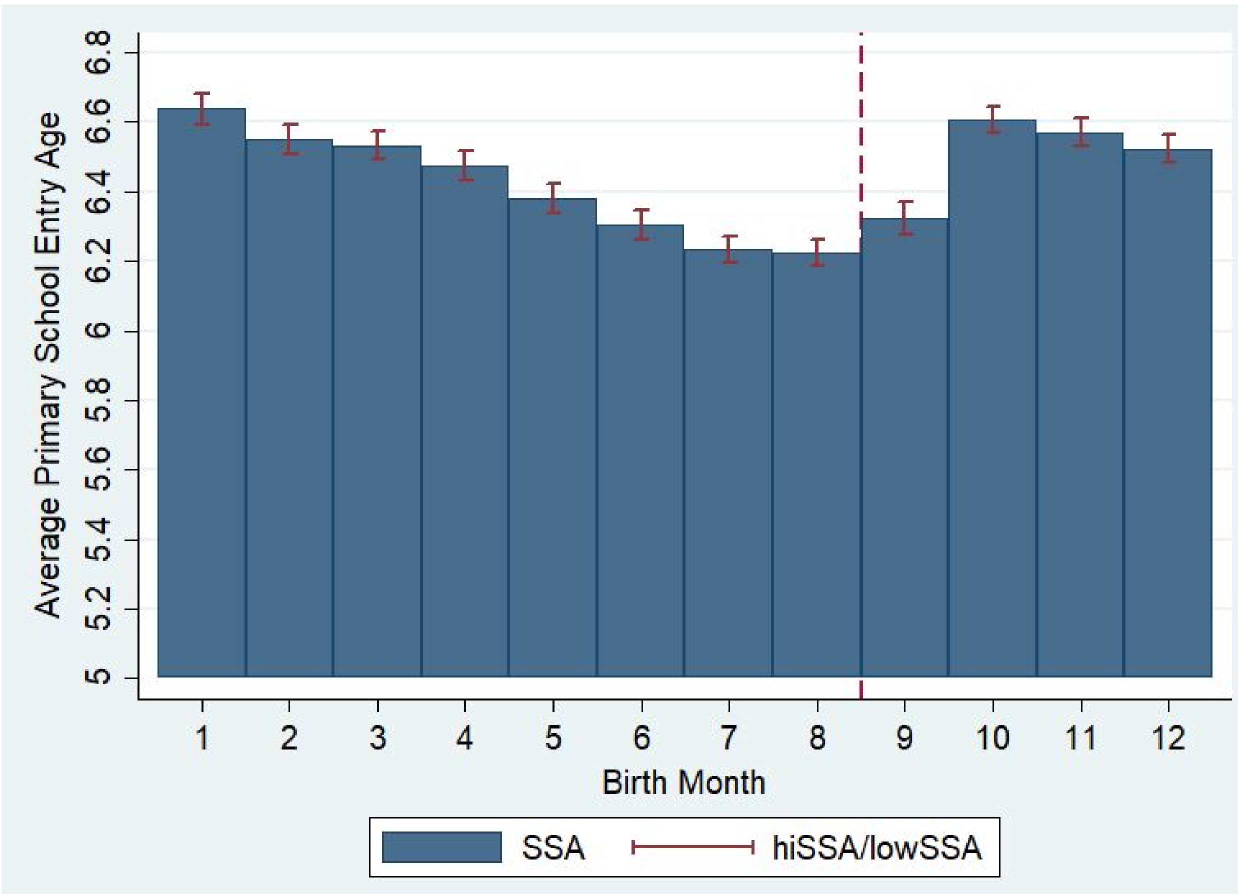


Figure 2.2: Children Size Impact on Primary School Entry Age

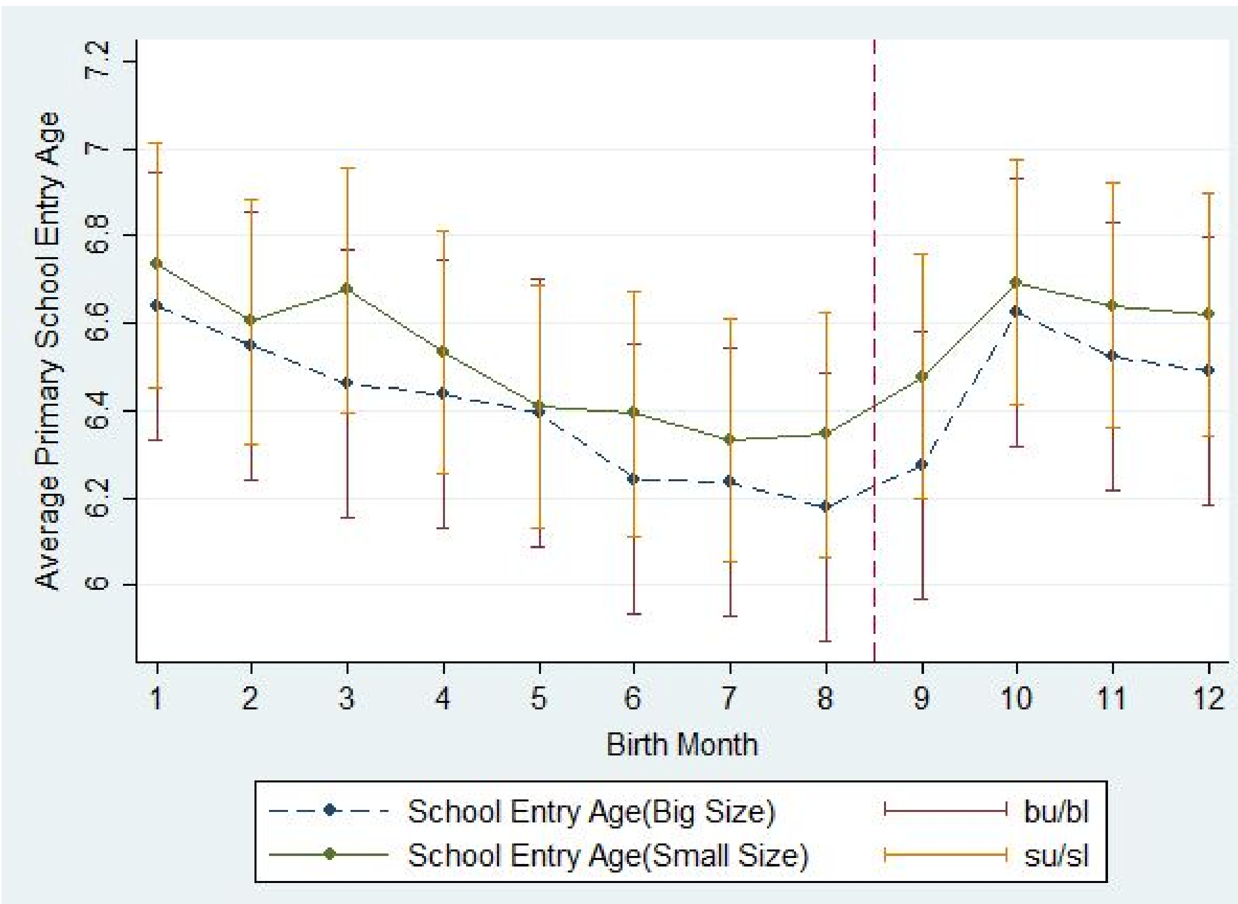
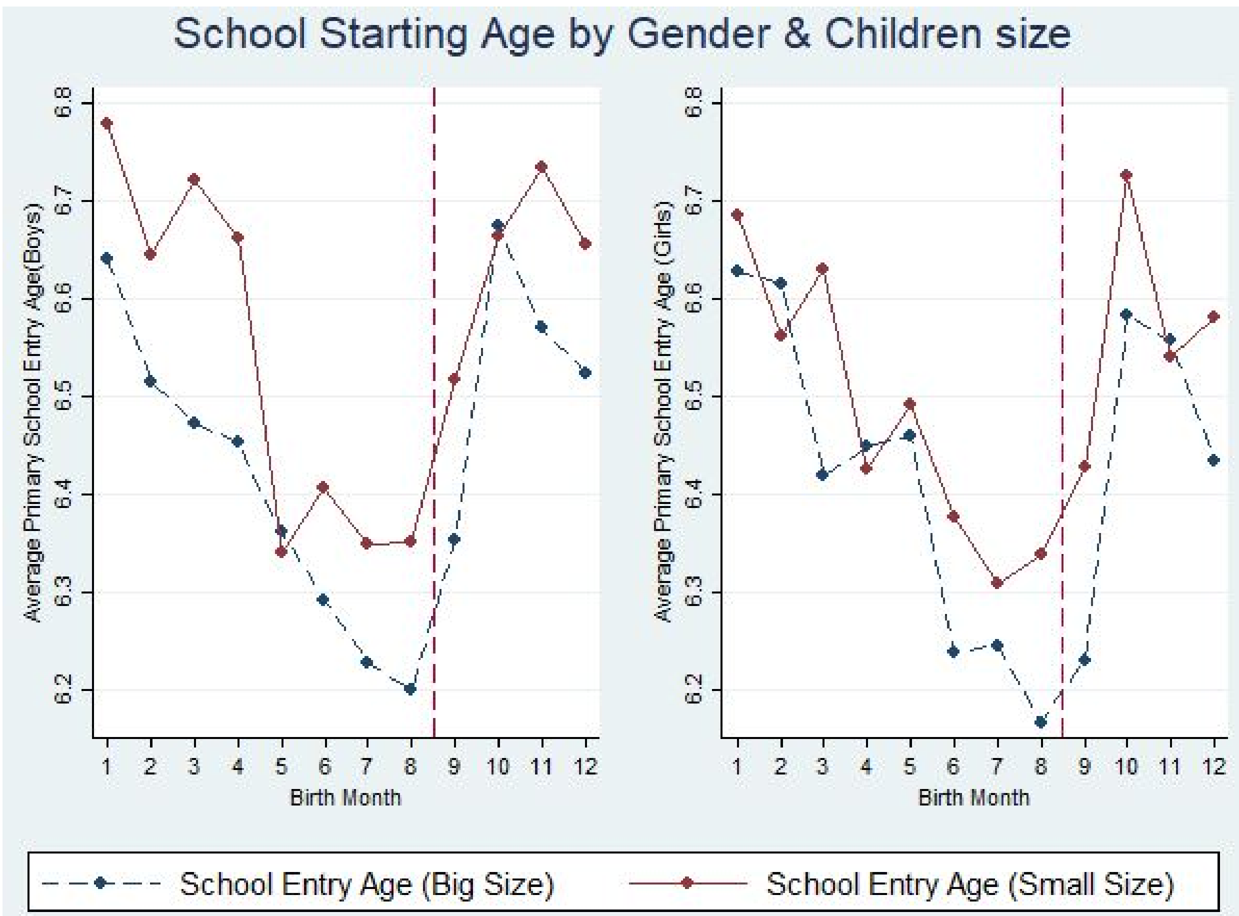


Figure 2.3: Primary School Entry Age by Children Size & Gender



Chapter 3 Figures

Figure 3.1: Trends in Primary School Starting Age in China

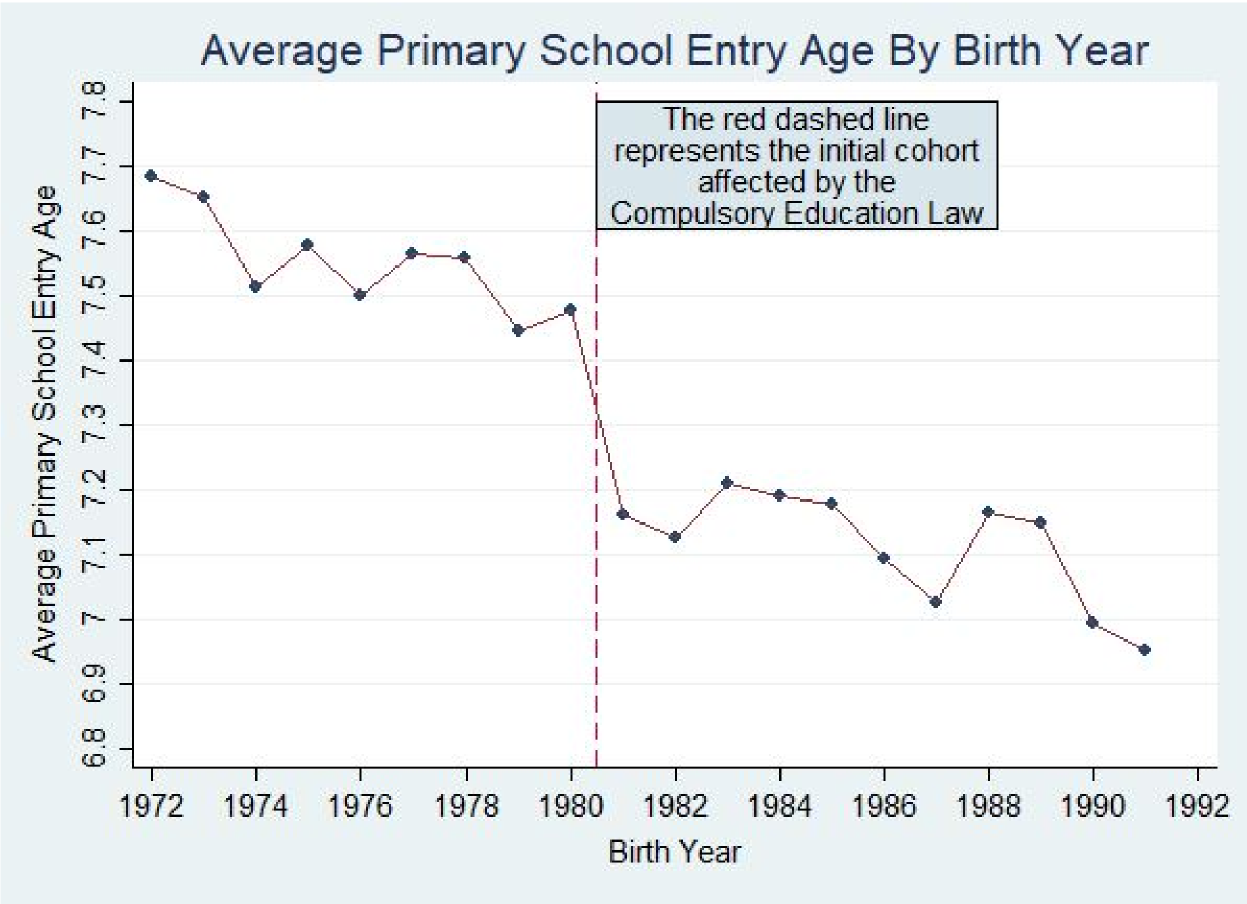


Figure 3.2: Regression Discontinuity Birthday Manipulation Test

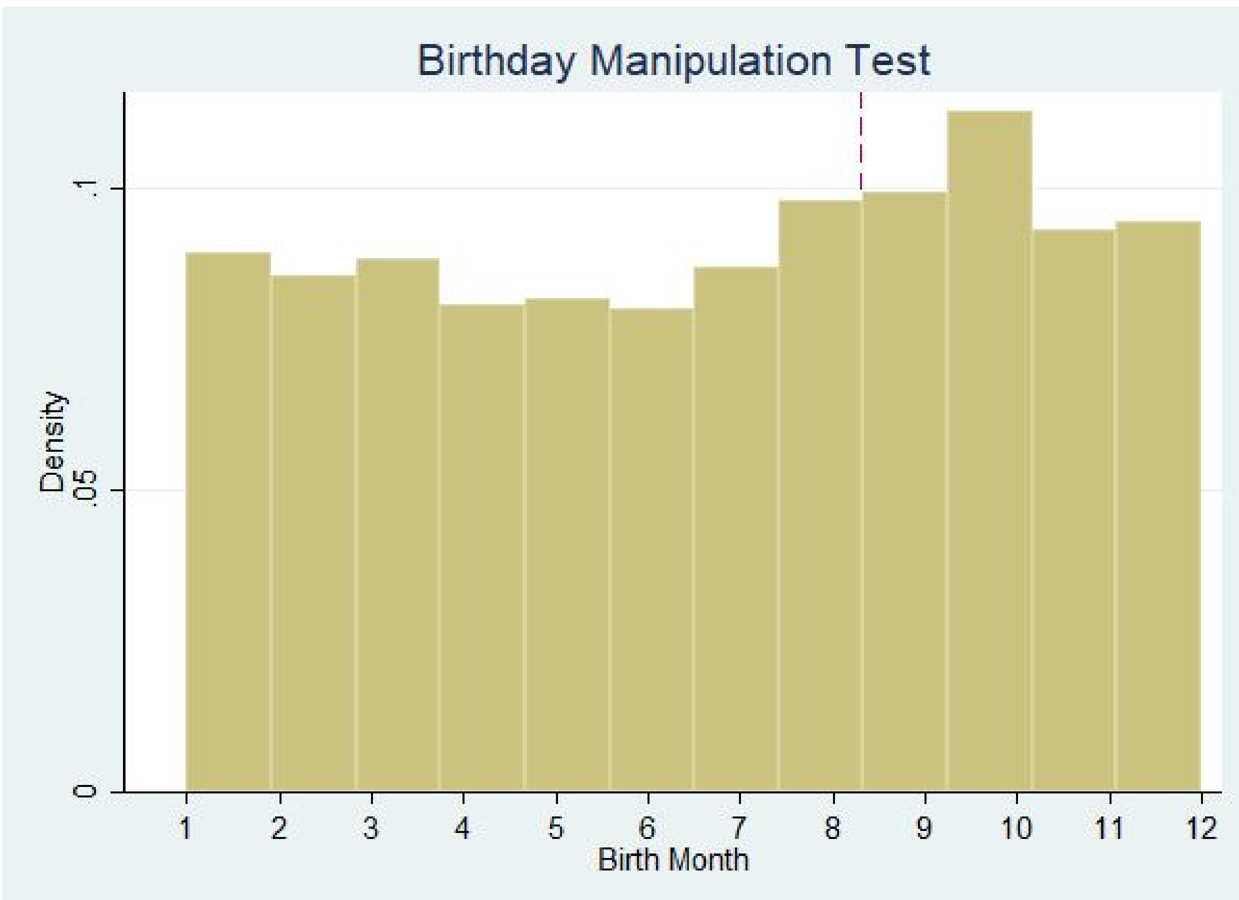
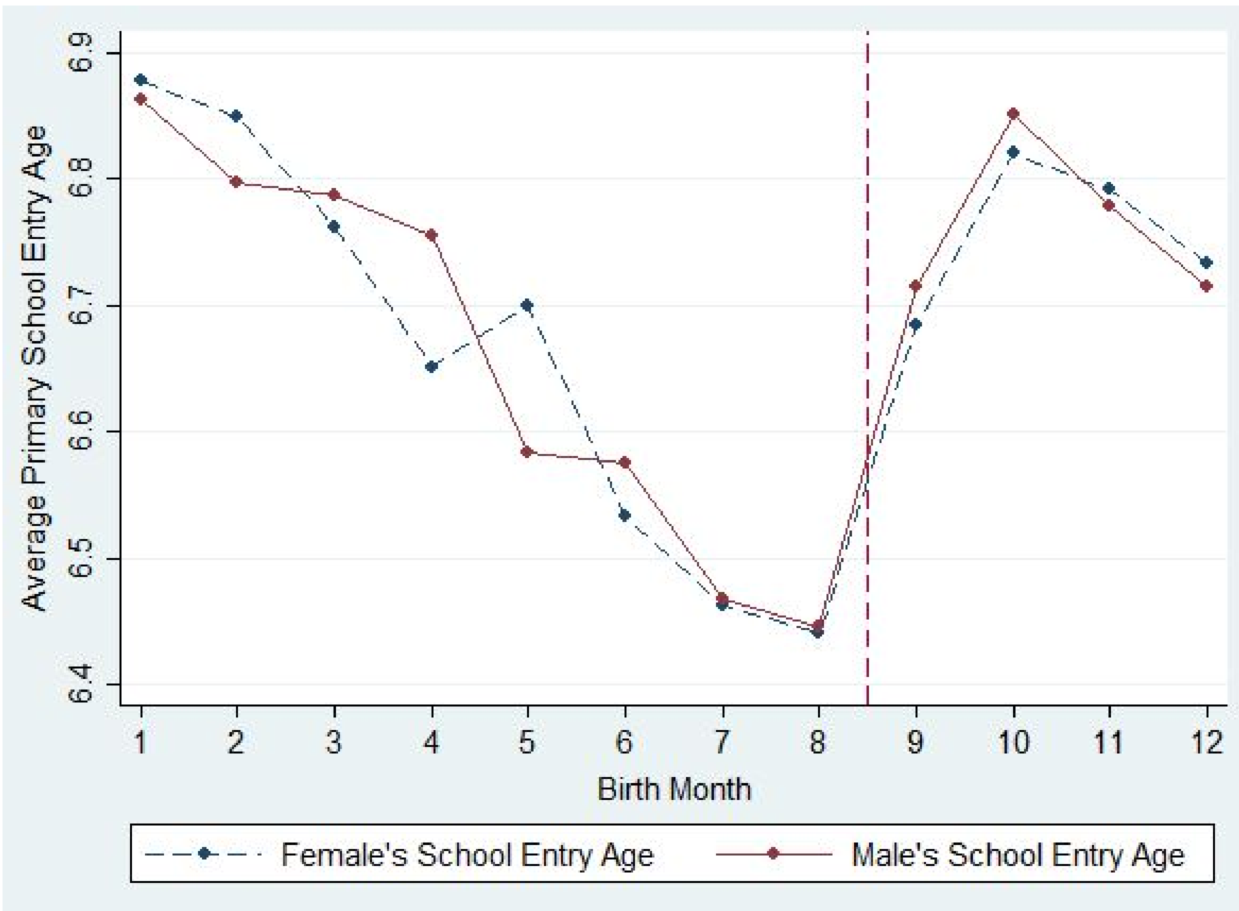


Figure 3.3: Average School Starting Age by Birth Month



Tables

Chapter 1 Tables

Table 1.1: Overview of Current Primary Health Insurance Programs in China

Characteristic	New Rural Cooperative Medical Scheme (NCMS)	Urban Employee-Basic Medical Insurance (UE-BMI)	Urban Residents-Basic Medical Insurance (UR-BMI)
Administration	County level	Municipal level	Municipal level
Local government authority	Counties determine the deductible, ceiling, reimbursement ratio, medical savings account	Wide variation across municipalities regarding eligibility, financing, benefits packages	Wide variations across municipalities regarding eligibility, financing, benefits packages
Date started	2003	1998	2007 (79 pilot cities) 2010 (all cities)
Participation	Voluntary at household level	Mandatory for individuals	Voluntary at household level
Populations/ Target	Rural residents/ Est. 840 million	Urban employed/ Est. 300 million	Children, students, elderly, disabled, other non-working urban residents/ Est. 200 million
Current coverage	94.2% (2009)	67% (200 million, end 2008)	60.4% (118 million, end 2008)
Revenues (billion RMB)	94.435 billion RMB (13.9 billion USD) (2009)	270.9 billion RMB (39.8 billion USD)	15.4 billion RMB (2.3 bill USD)
Expenditures (billion RMB)	92.292 billion RMB (13.6 billion USD) (2009)	201.6 billion RMB (29.6 billion USD)	6.7 billion RMB (985 mill USD)
Source of revenues	100 RMB/year (2009) For western areas, the initial contribution is 40 RMB each from local and central government, and 20 RMB from individuals. The central contribution to eastern provinces tends to be lower, compensated by higher provincial or municipal contributions.	8% of employee wages: "6+2": 6% payroll tax on employers (ranging from 4 to 1 % by municipality) and 2% employee contribution Medical savings accounts generally cover OP expenses, medicines (employer contribution + 30% of employee contribution)	Average 245 RMB for adults, 113 RMB for minors (pilots 2008). In 2008, the government contribution was at least 80 RMB /person, with a central level contribution to west and central areas of 40 RMB/ person. Provincial contributions vary. The poor and disabled receive an additional 60 RMB per year (50% from central).

Source: World Health Report (2010). Background Paper, No 37 (Figure 2).

Table 1.2: Comparison Between Old CMS and NCMS

Features	**New CMS**	**Old CMS**
Participation	Voluntary	Universal
Financial contribution	Premium paid by household and heavily subsidized by government, with financial obligations of all level governments	Funded by both village collectives and farmers, without financial obligations of all level governments
Risk pool level	County level	Township or Village level
Enrollment unit	Household	Individual
Guideline	Broad guidelines were issued by the central government, provincial and county governments retain considerable discretion over the details	No guidelines from central government
Government management responsibility	Managed at county-level and above by NCMS administrative office, and supervised by central and local governments	Managed at the village level by barefoot doctors and/or farmers
Covered services	Focus on inpatient services and catastrophic outpatient services	Focus on prevention and health care with outpatient services

Source:X. You, Y. Kobayashi / Health Policy 91 (2009) 1–9 Table 1 You and Kobayashi (2009)

Table 1.3: CHNS Preliminary Descriptive Statistics

	No. of Households	No. of Participants	No. of Observations
Total	8,638	35,703	157,286
Male	-	17,084	78,149
Female	-	18,394	80,730
Urban[city]	2,369	7,791	21,618
Urban[suburban]	1,370	5,725	25,466
Rural[town or county capital city]	1,627	6,458	25,019
Rural[rural village]	3,272	15,729	85,183
Age[0-17 years old]	-	11,476	34,338
Age[18-60 years old]	-	20,741	102,513
Age[61 years old and more]	-	3,462	20,353

Note: This is a description of the initial CHNS dataset, which contains all surveyed individuals. Our later analysis only focuses on rural residents reporting valid labor outcomes, which reduces our Number of observations to 23,997.

Table 1.4: Implementation of NCMS Across Survey Waves

	Individuals	Households	Counties
1989	0	0	0
1991	0	0	0
1993	0	0	0
1997	0	0	0
2000	0	0	0
2004	890	432	12
2006	2,892	1,352	26
2009	4,389	2,170	36
2011	5,031	2,558	40

Table 1.5: Summary Statistics

	ALL			Non-NCMS			NCMS		
	Obs	Mean	SD	Obs	Mean	SD	Obs	Mean	SD
Panel1: Dependent Variables									
Average # of Hrs/Day Worked Last Year	23997	7.03	2.54	13491	7.48	2.22	10506	6.46	2.79
Average # of Days/Week Worked Last Year	23997	5.41	1.54	13491	5.59	1.35	10506	5.19	1.73
# of Months Worked Last Year	23997	9.37	3.35	13491	10.21	2.89	10506	8.30	3.59
Annual Work Hours	23997	1694.58	1073.13	13491	1967.16	993.20	10506	1344.55	1070.46
Household Chores (hours)	6323	134.32	111.18	2643	121.32	147.16	3680	143.65	74.03
Household Savings (RMB)	23997	19855.31	19437.36	13491	15964.22	17509.43	10506	24851.97	20613.21
Panel 2: Independent Variables									
NCMS	23997	0.44	0.50	13491	0.00	0.00	10506	1.00	0.00
NCMS County Eligibility	23997	0.58	0.49	13491	0.24	0.43	10506	1.00	0.00
Panel 3: Covariates									
Age	23997	42.62	12.61	13491	39.66	12.18	10506	46.41	12.11
Female	23997	0.42	0.49	5453	0.40	0.49	4651	0.44	0.50
Never Married	23997	0.10	0.30	1966	0.15	0.35	482	0.05	0.21
Married	23997	0.85	0.35	10917	0.81	0.39	9536	0.91	0.29
Divorced	23997	0.01	0.09	155	0.01	0.11	62	0.01	0.08
Widowed	23997	0.02	0.15	255	0.02	0.14	330	0.03	0.17
Current Health Status (Self-Report)	10880	2.17	0.74	8125	2.15	0.73	2755	2.23	0.78
Current Health Status: Excellent	10880	0.08	0.26	1379	0.10	0.30	444	0.04	0.20
Current Health Status: Good	10880	0.24	0.43	4438	0.33	0.47	1383	0.13	0.34
Current Health Status: Fair	10880	0.12	0.32	2035	0.15	0.36	782	0.07	0.26
Current Health Status: Poor	10880	0.02	0.13	273	0.02	0.14	146	0.01	0.12
Completed Years of Formal Ed. in Regular School	23451	19.92	7.42	12952	21.14	7.09	10499	18.41	7.53
Primary School	23451	0.28	0.45	3137	0.23	0.42	3568	0.34	0.47
Middle School	23451	0.40	0.49	5143	0.38	0.49	4496	0.43	0.49
High School or More	23451	0.23	0.42	3986	0.30	0.46	1440	0.14	0.34
Number of Children	23997	1.00	0.99	13491	0.99	0.94	10506	1.02	1.04
Household Size	23994	4.06	1.57	13488	4.14	1.53	10506	3.96	1.61
Total Gross HH Income (RMB)	23997	26123.71	25500.04	13491	20313.49	21150.53	10506	33584.74	28493.00
Total HH Expenses (RMB)	23997	3121.53	7347.17	13491	2171.22	5637.87	10506	4341.85	8935.27

Table 1.6: First Stage Regression

First-Stage Regression for IV= NCMS County Level Eligibility

Dependent Variable: Household NCMS Enrollment	Coefficient	SD
IV: NCMS County Level Eligibility	0.7458***	0.3253
R^2	0.6485	
F-test Statistic	65.1181	

Note: ***denotes significance at the 1% level.

Table 1.7: Baseline Individual Level Regression: Effects of NCMS on Work Hours

	Annual Work Hours		Log of Hours	
	OLS	2SLS	OLS	2SLS
NCMS	-228.1***	-140.5***	-0.197***	-0.0787*
	(20.82)	(46.99)	(0.0203)	(0.0478)
26-30 Years Old	-55.62*	-57.11**	-0.0537*	-0.0557*
	(29.03)	(28.95)	(0.0288)	(0.0288)
31-35 Years Old	-64.08**	-66.60**	-0.0455	-0.0489*
	(29.34)	(29.26)	(0.0284)	(0.0284)
36-40 Years Old	-90.75***	-92.22***	-0.0947***	-0.0966***
	(29.17)	(29.08)	(0.0288)	(0.0287)
41-45 Years Old	-101.7***	-102.7***	-0.0809***	-0.0823***
	(29.57)	(29.47)	(0.0288)	(0.0287)
46-50 Years Old	-137.9***	-137.9***	-0.115***	-0.114***
	(30.19)	(30.09)	(0.0301)	(0.0300)
51-55 Years Old	-199.7***	-200.1***	-0.146***	-0.147***
	(31.47)	(31.35)	(0.0313)	(0.0312)
56-60 Years Old	-324.1***	-325.8***	-0.284***	-0.286***
	(33.78)	(33.66)	(0.0348)	(0.0347)
61-65 Years Old	-397.3***	-402.5***	-0.407***	-0.414***
	(39.64)	(39.61)	(0.0426)	(0.0426)
66-79 Years Old	-404.1***	-405.8***	-0.426***	-0.428***
	(48.65)	(48.44)	(0.0526)	(0.0525)
71-75 Years Old	-605.0***	-607.8***	-0.688***	-0.692***
	(68.70)	(68.42)	(0.0923)	(0.0920)
Female	-96.39***	-96.80***	-0.108***	-0.109***
	(12.37)	(12.32)	(0.0120)	(0.0120)
Married	2.722	0.209	-0.000474	-0.00388
	(23.65)	(23.61)	(0.0236)	(0.0236)
Widowed	-71.69	-70.97	-0.0793	-0.0783
	(44.79)	(44.53)	(0.0493)	(0.0491)
Primary School	-0.274	0.964	-0.0182	-0.0165
	(22.13)	(22.04)	(0.0228)	(0.0227)
Middle School	75.55***	77.11***	0.0380*	0.0401*
	(22.57)	(22.49)	(0.0227)	(0.0226)
High School or More	142.9***	148.4***	0.120***	0.127***
	(24.84)	(24.85)	(0.0237)	(0.0238)
Current Health Status: Good	-63.29***	-63.97***	-0.0343*	-0.0352*
	(23.45)	(23.35)	(0.0207)	(0.0206)
Current Health Status: Fair	-111.7***	-111.0***	-0.101***	-0.1000***
	(27.91)	(27.79)	(0.0258)	(0.0257)
Current Health Status: Poor	-168.1***	-169.4***	-0.123**	-0.125**
	(49.62)	(49.41)	(0.0512)	(0.0509)
Number of Children	-5.792	-6.024	-0.0101	-0.0104
	(6.572)	(6.540)	(0.00676)	(0.00673)
County FE	YES	YES	YES	YES
Wave FE	YES	YES	YES	YES
No. of Obs.	23997	23997	23997	23997
R-Squared	0.303	0.303	0.283	0.282

Note: Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.8: Gender Segmented Regression Results (Log of Hours)

	OLS		2SLS	
	Male	Female	Male	Female
NCMS	-0.174***	-0.221***	-0.0398	-0.164**
	(0.0267)	(0.0313)	(0.0637)	(0.0717)
26-30 Years Old	-0.0458	-0.0199	-0.0481	-0.0210
	(0.0370)	(0.0457)	(0.0369)	(0.0453)
31-35 Years Old	-0.0840**	0.0428	-0.0882**	0.0411
	(0.0365)	(0.0458)	(0.0363)	(0.0454)
36-40 Years Old	-0.126***	-0.0131	-0.129***	-0.0136
	(0.0372)	(0.0458)	(0.0371)	(0.0454)
41-45 Years Old	-0.112***	0.000752	-0.114***	-0.0000664
	(0.0369)	(0.0465)	(0.0367)	(0.0461)
46-50 Years Old	-0.115***	-0.0693	-0.114***	-0.0702
	(0.0382)	(0.0492)	(0.0380)	(0.0488)
51-55 Years Old	-0.116***	-0.159***	-0.117***	-0.160***
	(0.0386)	(0.0532)	(0.0383)	(0.0527)
56-60 Years Old	-0.263***	-0.304***	-0.265***	-0.306***
	(0.0437)	(0.0588)	(0.0435)	(0.0583)
61-65 Years Old	-0.361***	-0.470***	-0.368***	-0.475***
	(0.0534)	(0.0712)	(0.0532)	(0.0709)
66-70 Years Old	-0.373***	-0.481***	-0.381***	-0.479***
	(0.0644)	(0.0918)	(0.0642)	(0.0908)
71-75 Years Old	-0.687***	-0.760***	-0.688***	-0.763***
	(0.116)	(0.160)	(0.115)	(0.158)
Married	0.0670**	-0.137***	0.0649**	-0.140***
	(0.0302)	(0.0388)	(0.0300)	(0.0385)
Widowed	0.00173	-0.208***	0.00294	-0.208***
	(0.0694)	(0.0720)	(0.0686)	(0.0713)
Primary School	-0.0631*	-0.0448	-0.0605*	-0.0444
	(0.0362)	(0.0306)	(0.0360)	(0.0303)
Middle School	-0.00443	0.0256	-0.00143	0.0264
	(0.0348)	(0.0319)	(0.0346)	(0.0316)
High School or More	0.0712**	0.129***	0.0795**	0.133***
	(0.0356)	(0.0340)	(0.0356)	(0.0338)
Current Health Status: Good	-0.0416	-0.0252	-0.0433*	-0.0251
	(0.0262)	(0.0341)	(0.0260)	(0.0338)
Current Health Status: Fair	-0.102***	-0.0875**	-0.102***	-0.0860**
	(0.0335)	(0.0412)	(0.0333)	(0.0408)
Current Health Status: Poor	-0.162**	-0.0761	-0.165**	-0.0760
	(0.0756)	(0.0704)	(0.0748)	(0.0696)
Number of Children	-0.0168*	-0.00331	-0.0175*	-0.00322
	(0.00907)	(0.0102)	(0.00900)	(0.0101)
County FE	YES	YES	YES	YES
Wave FE	YES	YES	YES	YES
No. of Obs.	13853	10104	13853	10104
R-Squared	0.261	0.328	0.260	0.327

Note: Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.9: HH head, Non-HH head & Household Level Regression Results (Log of Hours)

	HH Head		Non-HH Head		HH Level	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
NCMS	-0.198***	-0.0800*	-0.122***	-0.0185	-0.105***	-0.282*
	(0.0203)	(0.0478)	(0.0256)	(0.0515)	(0.0350)	(0.147)
26-30 Years Old	-0.0554*	-0.0575**	-0.0286	-0.0267	-0.0288	-0.0209
	(0.0289)	(0.0289)	(0.0439)	(0.0436)	(0.0234)	(0.0238)
31-35 Years Old	-0.0475*	-0.0511*	-0.0212	-0.0220	-0.0324	-0.0257
	(0.0286)	(0.0285)	(0.0412)	(0.0409)	(0.0255)	(0.0254)
36-40 Years Old	-0.0970***	-0.0991***	-0.0985**	-0.0986**	-0.0302	-0.0268
	(0.0289)	(0.0289)	(0.0410)	(0.0408)	(0.0267)	(0.0264)
41-45 Years Old	-0.0828***	-0.0843***	-0.0699*	-0.0708*	-0.0640**	-0.0658**
	(0.0289)	(0.0289)	(0.0408)	(0.0406)	(0.0266)	(0.0264)
46-50 Years Old	-0.117***	-0.117***	-0.119***	-0.119***	-0.0553*	-0.0591*
	(0.0303)	(0.0302)	(0.0419)	(0.0417)	(0.0317)	(0.0318)
51-55 Years Old	-0.148***	-0.148***	-0.136***	-0.135***	-0.0758**	-0.0751**
	(0.0314)	(0.0313)	(0.0428)	(0.0426)	(0.0345)	(0.0340)
56-60 Years Old	-0.288***	-0.290***	-0.286***	-0.286***	-0.135***	-0.131***
	(0.0349)	(0.0349)	(0.0459)	(0.0457)	(0.0472)	(0.0465)
61-65 Years Old	-0.411***	-0.418***	-0.407***	-0.410***	0.0911	0.0968
	(0.0428)	(0.0428)	(0.0517)	(0.0515)	(0.0711)	(0.0702)
66-70 Years Old	-0.426***	-0.429***	-0.416***	-0.415***	0.169	0.176*
	(0.0531)	(0.0529)	(0.0605)	(0.0602)	(0.110)	(0.106)
71-75 Years Old	-0.704***	-0.708***	-0.698***	-0.697***	-0.0217	0.00898
	(0.0940)	(0.0937)	(0.0991)	(0.0986)	(0.164)	(0.165)
Female	-0.108***	-0.108***	-0.123***	-0.122***	-0.0219*	-0.0207*
	(0.0120)	(0.0120)	(0.0157)	(0.0156)	(0.0124)	(0.0122)
Married	0.00235	-0.000815	0.00950	0.00688	0.0101	0.0145
	(0.0240)	(0.0239)	(0.0345)	(0.0343)	(0.0204)	(0.0206)
Widowed	-0.0791	-0.0778	-0.0646	-0.0637	-0.0826	-0.0890
	(0.0499)	(0.0496)	(0.0578)	(0.0574)	(0.0919)	(0.0898)
Primary School	-0.0160	-0.0144	-0.0174	-0.0157	-0.0110	-0.00896
	(0.0228)	(0.0228)	(0.0274)	(0.0272)	(0.0306)	(0.0304)
Middle School	0.0397*	0.0417*	0.0173	0.0191	0.00523	0.00614
	(0.0227)	(0.0226)	(0.0290)	(0.0289)	(0.0224)	(0.0222)
High School or More	0.122***	0.129***	0.110***	0.114***	-0.00608	-0.0136
	(0.0237)	(0.0238)	(0.0333)	(0.0332)	(0.0216)	(0.0219)
Current Health Status: Good	-0.0331	-0.0341*	-0.0425	-0.0429	-0.0137	-0.0110
	(0.0207)	(0.0206)	(0.0278)	(0.0277)	(0.0229)	(0.0225)
Current Health Status: Fair	-0.0995***	-0.0986***	-0.110***	-0.109***	-0.0171	-0.0116
	(0.0259)	(0.0257)	(0.0327)	(0.0325)	(0.0304)	(0.0300)
Current Health Status: Poor	-0.121**	-0.122**	-0.100*	-0.103*	-0.168	-0.172
	(0.0511)	(0.0509)	(0.0572)	(0.0569)	(0.112)	(0.109)
Number of Children	-0.00984	-0.0101	-0.00799	-0.00789	-0.0110	-0.0102
	(0.00677)	(0.00674)	(0.00806)	(0.00801)	(0.0102)	(0.0100)
County FE	YES	YES	YES	YES	YES	YES
Wave FE	YES	YES	YES	YES	YES	YES
No. of Obs.	23957	23957	17308	17308	6649	6649
R-Squared	0.283	0.282	0.237	0.236	0.144	0.138

Note: Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.10: Self-employed VS. non Self-employed (Log of Hours)

	All		Self-employed		Employees	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
NCMS	-0.188***	-0.201***	-0.111***	-0.0987**	-0.107***	-0.196***
	(0.0196)	(0.0322)	(0.0249)	(0.0403)	(0.0331)	(0.0580)
26-30 Years Old	-0.0454	-0.0451	-0.0187	-0.0185	-0.0179	-0.0142
	(0.0283)	(0.0282)	(0.0427)	(0.0424)	(0.0230)	(0.0227)
31-35 Years Old	-0.0373	-0.0369	-0.00851	-0.00864	-0.0216	-0.0182
	(0.0281)	(0.0280)	(0.0402)	(0.0400)	(0.0250)	(0.0246)
36-40 Years Old	-0.0832***	-0.0829***	-0.0785*	-0.0786**	-0.0225	-0.0212
	(0.0285)	(0.0284)	(0.0401)	(0.0398)	(0.0264)	(0.0261)
41-45 Years Old	-0.0734***	-0.0733***	-0.0572	-0.0573	-0.0581**	-0.0594**
	(0.0285)	(0.0284)	(0.0398)	(0.0396)	(0.0262)	(0.0258)
46-50 Years Old	-0.104***	-0.104***	-0.102**	-0.102**	-0.0498	-0.0519*
	(0.0297)	(0.0296)	(0.0408)	(0.0406)	(0.0314)	(0.0312)
51-55 Years Old	-0.140***	-0.140***	-0.126***	-0.126***	-0.0719**	-0.0723**
	(0.0309)	(0.0307)	(0.0418)	(0.0415)	(0.0339)	(0.0334)
56-60 Years Old	-0.278***	-0.278***	-0.277***	-0.277***	-0.109**	-0.107**
	(0.0344)	(0.0342)	(0.0450)	(0.0447)	(0.0448)	(0.0442)
61-65 Years Old	-0.401***	-0.400***	-0.396***	-0.396***	0.109	0.112
	(0.0421)	(0.0420)	(0.0506)	(0.0503)	(0.0705)	(0.0695)
66-79 Years Old	-0.418***	-0.417***	-0.407***	-0.406***	0.148	0.151
	(0.0522)	(0.0520)	(0.0593)	(0.0590)	(0.101)	(0.0979)
71-75 Years Old	-0.636***	-0.636***	-0.630***	-0.630***	0.0107	0.0247
	(0.0906)	(0.0903)	(0.0957)	(0.0952)	(0.143)	(0.142)
Female	-0.106***	-0.106***	-0.121***	-0.121***	-0.0210*	-0.0205*
	(0.0118)	(0.0118)	(0.0154)	(0.0153)	(0.0123)	(0.0121)
Married	0.00257	0.00296	0.00607	0.00570	0.0128	0.0155
	(0.0231)	(0.0230)	(0.0331)	(0.0330)	(0.0194)	(0.0192)
Widowed	-0.0752	-0.0753	-0.0629	-0.0628	-0.0925	-0.0950
	(0.0484)	(0.0482)	(0.0559)	(0.0555)	(0.0908)	(0.0891)
Primary School	-0.0205	-0.0207	-0.0226	-0.0223	-0.0116	-0.0106
	(0.0227)	(0.0226)	(0.0272)	(0.0270)	(0.0303)	(0.0300)
Middle School	0.0395*	0.0393*	0.0179	0.0181	0.00376	0.00405
	(0.0226)	(0.0225)	(0.0288)	(0.0286)	(0.0223)	(0.0220)
High School or More	0.118***	0.117***	0.107***	0.107***	-0.0102	-0.0142
	(0.0236)	(0.0236)	(0.0329)	(0.0328)	(0.0214)	(0.0212)
Current Health Status: Good	-0.0347*	-0.0346*	-0.0444	-0.0445	-0.0106	-0.00928
	(0.0205)	(0.0204)	(0.0275)	(0.0273)	(0.0224)	(0.0221)
Current Health Status: Fair	-0.102***	-0.102***	-0.112***	-0.112***	-0.0123	-0.00916
	(0.0254)	(0.0253)	(0.0321)	(0.0320)	(0.0296)	(0.0292)
Current Health Status: Poor	-0.139***	-0.139***	-0.120**	-0.121**	-0.158	-0.160
	(0.0509)	(0.0507)	(0.0568)	(0.0565)	(0.111)	(0.109)
Number of Children	0.00198	0.00208	0.00125	0.00119	0.0105	0.0114
	(0.00648)	(0.00645)	(0.00778)	(0.00774)	(0.00803)	(0.00798)
County FE	YES	YES	YES	YES	YES	YES
Wave FE	YES	YES	YES	YES	YES	YES
No. of Obs.	25063	25063	18159	18159	6904	6904
R-Squared	0.278	0.278	0.234	0.234	0.142	0.140

Note: Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.11: Household Chores in Minutes/Day (OLS)

	ALL	Male	Female	HH head	Non HH head	HH level
NCMS	-13.51***	-30.67***	-11.61***	-6.635	-13.23***	-12.74***
	(3.613)	(10.89)	(3.944)	(9.390)	(3.997)	(3.987)
26-30 Years Old	-18.52**	-9.700	-21.28**	-30.73	-18.64*	-32.14**
	(9.399)	(18.84)	(10.70)	(44.73)	(9.771)	(15.48)
31-35 Years Old	2.743	-13.99	2.228	13.14	1.692	-12.58
	(8.852)	(21.02)	(9.996)	(48.27)	(9.169)	(13.97)
36-40 Years Old	7.649	33.65*	0.908	54.55	1.721	-6.262
	(8.475)	(18.46)	(9.478)	(46.36)	(8.869)	(13.79)
41-45 Years Old	4.080	26.89	-1.342	43.35	0.0239	-10.88
	(8.769)	(24.67)	(9.731)	(46.54)	(9.156)	(14.46)
46-50 Years Old	-2.443	0.434	-6.524	21.48	-5.244	-19.46
	(8.166)	(16.68)	(9.335)	(44.16)	(8.693)	(14.00)
51-55 Years Old	0.640	8.634	-2.877	22.37	-2.489	-16.50
	(8.356)	(17.21)	(9.600)	(43.67)	(9.089)	(13.67)
56-60 Years Old	1.376	19.20	-5.247	22.43	0.267	-14.33
	(8.787)	(18.48)	(9.920)	(43.56)	(9.916)	(14.25)
61-65 Years Old	-0.573	-2.391	-1.447	12.43	2.268	-14.89
	(8.938)	(18.27)	(10.23)	(44.22)	(9.906)	(14.41)
66-70 Years Old	-3.411	5.201	-8.338	20.53	-3.579	-25.91*
	(9.728)	(19.93)	(11.65)	(44.04)	(11.82)	(14.92)
71-75 Years Old	-13.10	13.14	-21.32	23.22	-16.43	-29.58*
	(10.69)	(21.13)	(13.40)	(44.84)	(12.92)	(16.60)
Female	22.90***	0	0	38.67***	1.616	-1.128
	(3.853)	(.)	(.)	(11.77)	(11.75)	(5.397)
Married	30.31***	13.88	38.59***	20.06	42.49***	37.03***
	(5.949)	(16.46)	(7.823)	(15.13)	(8.221)	(7.738)
Widowed	19.41***	14.17	24.85***	8.769	34.32***	19.66**
	(6.636)	(17.17)	(8.888)	(14.72)	(9.689)	(8.679)
Primary School	-7.834	-20.11	-8.387*	-9.101	-6.733	-8.750
	(4.799)	(21.99)	(5.084)	(13.17)	(5.410)	(8.463)
Middle School	-4.522	-5.640	-6.928	9.454	-6.451	-8.275
	(5.928)	(23.52)	(6.370)	(17.74)	(6.646)	(9.735)
High School or More	-6.644	5.659	-11.63	-1.012	-6.867	-5.084
	(7.899)	(25.95)	(8.579)	(19.95)	(9.124)	(12.30)
Current Health Status: Good	4.340	-13.07	7.721	21.11	1.727	11.79
	(7.904)	(21.53)	(8.293)	(22.19)	(8.696)	(9.858)
Current Health Status: Fair	1.857	-6.222	3.459	14.61	-0.407	6.873
	(7.772)	(24.09)	(8.085)	(22.27)	(8.516)	(9.393)
Current Health Status: Poor	-0.701	-30.79	2.953	-6.698	-2.125	1.073
	(9.761)	(28.02)	(10.26)	(26.10)	(10.99)	(11.83)
Number of Children	1.909*	3.532	1.751	-0.823	1.891	3.321**
	(1.106)	(3.633)	(1.184)	(3.243)	(1.234)	(1.462)
County FE	YES	YES	YES	YES	YES	YES
Wave FE	YES	YES	YES	YES	YES	YES
No. of Obs.	6619	1007	5580	1241	5346	5603
R-Squared	0.231	0.324	0.243	0.295	0.246	0.224

Note: Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.12: Household Chores in Miniutes/Day (2SLS)

	ALL	Male	Female	HH head	Non HH head	HH level
NCMS	-18.26***	-26.15	-16.30**	-37.62*	-15.63**	-21.61***
	(5.963)	(30.80)	(6.451)	(19.63)	(6.220)	(6.552)
26-30 Years Old	-18.31**	-9.938	-21.24**	-28.41	-18.64*	-32.22**
	(9.296)	(24.02)	(10.50)	(42.61)	(9.583)	(15.19)
31-35 Years Old	2.975	-14.22	2.266	11.15	1.712	-12.45
	(8.782)	(22.58)	(9.810)	(45.66)	(8.995)	(13.72)
36-40 Years Old	7.739	33.60	0.801	51.32	1.667	-6.351
	(8.390)	(21.31)	(9.281)	(43.83)	(8.678)	(13.53)
41-45 Years Old	4.239	26.73	-1.359	39.95	0.0102	-10.89
	(8.720)	(21.65)	(9.546)	(43.92)	(8.976)	(14.19)
46-50 Years Old	-2.456	0.670	-6.596	16.25	-5.291	-19.73
	(8.113)	(21.32)	(9.153)	(41.74)	(8.518)	(13.73)
51-55 Years Old	0.471	8.775	-2.986	18.02	-2.556	-16.72
	(8.323)	(21.47)	(9.414)	(41.46)	(8.906)	(13.41)
56-60 Years Old	1.799	18.91	-5.306	20.53	0.216	-14.39
	(8.777)	(22.65)	(9.729)	(41.44)	(9.715)	(13.98)
61-65 Years Old	-0.115	-2.333	-1.232	10.18	2.340	-14.60
	(8.920)	(23.36)	(10.07)	(42.01)	(9.733)	(14.17)
66-70 Years Old	-3.159	5.032	-8.617	17.18	-3.741	-26.16*
	(9.774)	(24.88)	(11.41)	(41.85)	(11.56)	(14.64)
71-75 Years Old	-11.06	12.68	-21.16	23.87	-16.41	-29.28*
	(10.81)	(29.81)	(13.22)	(42.73)	(12.70)	(16.35)
Female	22.76***	0	0	37.29***	1.676	-1.356
	(3.813)	(.)	(.)	(10.66)	(11.54)	(5.315)
Married	30.00***	13.77	38.81***	21.52	42.56***	37.62***
	(6.249)	(10.70)	(7.670)	(13.97)	(8.046)	(7.571)
Widowed	18.86***	14.28	24.86***	8.238	34.47***	19.80**
	(7.004)	(14.54)	(8.733)	(13.78)	(9.466)	(8.528)
Primary School	-7.886*	-19.92	-8.472*	-10.03	-6.772	-8.974
	(4.746)	(17.14)	(4.993)	(12.12)	(5.308)	(8.321)
Middle School	-4.696	-5.403	-7.019	8.872	-6.495	-8.561
	(5.853)	(17.23)	(6.253)	(16.31)	(6.522)	(9.569)
High School or More	-7.233	6.443	-12.03	-4.899	-7.079	-6.087
	(7.843)	(18.48)	(8.478)	(18.30)	(9.010)	(12.18)
Current Health Status: Good	4.365	-12.68	7.633	17.99	1.708	11.77
	(7.816)	(13.53)	(8.144)	(19.87)	(8.532)	(9.675)
Current Health Status: Fair	1.611	-5.806	3.252	10.93	-0.459	6.646
	(7.722)	(14.63)	(7.958)	(20.24)	(8.366)	(9.227)
Current Health Status: Poor	-0.588	-31.08	2.919	-7.483	-2.116	0.958
	(9.663)	(26.86)	(10.10)	(24.13)	(10.79)	(11.67)
Number of Children	1.878*	3.431	1.719	-0.439	1.879	3.303**
	(1.093)	(3.574)	(1.163)	(3.025)	(1.210)	(1.435)
County FE	YES	YES	YES	YES	YES	YES
Wave FE	YES	YES	YES	YES	YES	YES
No. of Obs.	6587	1007	5580	1241	5346	5603
R-Squared	0.231	0.324	0.242	0.290	0.246	0.224

Note: Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.13: Household Savings in RMB (OLS)

		Income Quartiles			
	ALL	25%	50%	75%	100%
NCMS	-2605.3***	168.3	72.86	-354.6	-2838.3***
	(363.8)	(117.4)	(158.8)	(242.9)	(876.9)
26-30 Years Old	-956.8**	108.3	237.2	12.38	1172.8
	(475.8)	(117.9)	(184.1)	(334.1)	(1308.6)
31-35 Years Old	-1158.5**	-147.0	-185.5	-912.7**	811.0
	(495.2)	(121.8)	(183.2)	(377.1)	(1302.0)
36-40 Years Old	-1975.7***	-168.6	-235.5	-870.0**	1020.8
	(483.2)	(119.7)	(185.5)	(351.7)	(1273.5)
41-45 Years Old	-1489.8***	-205.5	-226.5	-129.5	-1295.4
	(492.1)	(126.7)	(183.5)	(356.0)	(1224.3)
46-50 Years Old	-1539.2***	74.58	-406.6**	-1080.7***	-700.3
	(503.8)	(128.2)	(185.5)	(357.2)	(1238.0)
51-55 Years Old	-2211.0***	239.9*	-24.90	-468.9	-393.7
	(522.2)	(139.5)	(195.9)	(356.1)	(1293.4)
56-60 Years Old	-3020.9***	57.62	-56.82	-1050.2***	-760.5
	(559.3)	(144.8)	(207.3)	(381.8)	(1413.6)
61-65 Years Old	-3874.5***	119.6	91.68	-758.8*	527.1
	(652.1)	(166.9)	(241.0)	(458.0)	(1670.6)
66-70 Years Old	-5762.0***	332.2*	-147.4	-1230.4**	-801.7
	(782.7)	(185.9)	(292.0)	(568.8)	(2017.3)
71-75 Years Old	-6465.0***	-224.7	-115.5	-270.1	2267.1
	(1082.3)	(276.7)	(408.8)	(1006.5)	(3380.8)
Primary School	-111.0	-110.9	56.48	547.7**	-47.91
	(339.7)	(90.75)	(137.6)	(267.5)	(1122.1)
Middle School	1265.2***	-130.6	-82.05	605.8**	1543.6
	(348.6)	(92.12)	(137.6)	(264.1)	(1129.6)
High School or More	5193.0***	-106.9	285.7*	1581.0***	3608.7***
	(384.3)	(103.6)	(156.6)	(289.5)	(1183.7)
Current Health Status: Good	-1209.1***	33.69	58.29	-164.7	73.50
	(338.1)	(117.9)	(134.1)	(237.4)	(1023.0)
Current Health Status: Fair	-1627.8***	-160.9	-89.24	-306.5	1186.9
	(389.1)	(128.0)	(157.1)	(294.8)	(1329.2)
Current Health Status: Poor	-3253.9***	-178.5	-414.4	-2147.1***	-407.9
	(589.4)	(191.7)	(283.8)	(609.8)	(4116.5)
Household Size	2268.0***	110.3***	-21.64	59.29	1720.9***
	(81.03)	(22.84)	(30.79)	(57.01)	(177.0)
County FE	YES	YES	YES	YES	YES
Wave FE	YES	YES	YES	YES	YES
No. of Obs.	23994	6000	5997	5999	5998
R-Squared	0.426	0.205	0.180	0.198	0.220

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.14: Household Savings in RMB (2SLS)

		Income Quartiles			
	ALL	25%	50%	75%	100%
NCMS	-820.1	8.835	269.6	1360.8**	-3749.7*
	(679.4)	(215.0)	(288.6)	(575.9)	(2243.7)
26-30 Years Old	-996.7**	109.6	233.0	-71.18	1138.0
	(474.3)	(117.2)	(181.2)	(334.5)	(1286.2)
31-35 Years Old	-1222.3**	-149.7	-192.4	-1047.4***	808.7
	(492.1)	(122.5)	(180.3)	(377.1)	(1280.6)
36-40 Years Old	-2019.2***	-168.1	-236.6	-1000.1***	993.8
	(480.7)	(119.6)	(182.4)	(352.6)	(1256.5)
41-45 Years Old	-1524.1***	-207.1*	-234.7	-234.3	-1333.6
	(489.6)	(124.8)	(180.7)	(356.1)	(1212.1)
46-50 Years Old	-1552.6***	75.72	-409.8**	-1124.0***	-728.6
	(501.2)	(128.8)	(182.4)	(355.0)	(1221.6)
51-55 Years Old	-2232.3***	240.7*	-25.42	-572.2	-437.5
	(519.6)	(134.4)	(192.7)	(355.5)	(1277.9)
56-60 Years Old	-3066.6***	60.05	-62.21	-1156.8***	-800.3
	(556.2)	(143.5)	(204.2)	(381.1)	(1399.2)
61-65 Years Old	-3989.8***	125.0	78.96	-955.0**	487.1
	(649.0)	(160.8)	(237.8)	(459.2)	(1651.9)
66-70 Years Old	-5804.2***	325.1*	-159.6	-1363.1**	-868.0
	(779.3)	(180.2)	(288.4)	(565.5)	(1994.2)
71-75 Years Old	-6524.7***	-233.5	-128.9	-445.0	2229.5
	(1075.8)	(264.7)	(403.4)	(979.8)	(3331.4)
Primary School	-85.60	-110.7	60.55	575.6**	-69.82
	(338.6)	(89.33)	(135.6)	(261.7)	(1103.7)
Middle School	1297.8***	-130.3	-74.92	621.3**	1516.8
	(347.5)	(91.52)	(135.9)	(258.6)	(1113.7)
High School or More	5307.2***	-106.8	294.1*	1659.5***	3535.7***
	(385.6)	(104.7)	(154.2)	(285.4)	(1180.8)
Current Health Status: Good	-1224.0***	40.14	53.25	-185.4	111.0
	(336.4)	(113.4)	(132.2)	(235.2)	(1013.4)
Current Health Status: Fair	-1614.8***	-155.2	-94.44	-303.3	1186.3
	(386.7)	(124.5)	(154.6)	(290.8)	(1309.8)
Current Health Status: Poor	-3284.1***	-160.6	-411.7	-2314.2***	-408.1
	(587.5)	(186.4)	(279.3)	(607.6)	(4068.1)
Household Size	2262.5***	110.3***	-21.07	55.34	1727.9***
	(80.61)	(21.10)	(30.34)	(56.12)	(174.7)
County FE	YES	YES	YES	YES	YES
Wave FE	YES	YES	YES	YES	YES
No. of Obs.	23994	6000	5997	5999	5998
R-Squared	0.425	0.205	0.179	0.191	0.220

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.15: Quartile Log of Work Hours (OLS)

	By Income Quartile				
	ALL	25%	50%	75%	100%
NCMS	-0.197***	-0.116**	-0.144***	-0.264***	-0.136***
	(0.0203)	(0.0537)	(0.0480)	(0.0405)	(0.0405)
26-30 Years Old	-0.0537*	-0.0606	-0.0587	0.0786	-0.0399
	(0.0288)	(0.0549)	(0.0535)	(0.0599)	(0.0603)
31-35 Years Old	-0.0455	0.0349	-0.00556	-0.0138	-0.0871
	(0.0284)	(0.0512)	(0.0542)	(0.0619)	(0.0621)
36-40 Years Old	-0.0947***	-0.0402	-0.0142	-0.0647	-0.136**
	(0.0288)	(0.0529)	(0.0556)	(0.0598)	(0.0637)
41-45 Years Old	-0.0809***	0.0514	-0.00919	-0.0687	-0.176***
	(0.0288)	(0.0543)	(0.0543)	(0.0608)	(0.0623)
46-50 Years Old	-0.115***	-0.00598	-0.0425	-0.0486	-0.242***
	(0.0301)	(0.0606)	(0.0579)	(0.0592)	(0.0645)
51-55 Years Old	-0.146***	-0.0139	-0.0633	-0.0981	-0.270***
	(0.0313)	(0.0603)	(0.0609)	(0.0625)	(0.0683)
56-60 Years Old	-0.284***	-0.163**	-0.229***	-0.176**	-0.399***
	(0.0348)	(0.0648)	(0.0670)	(0.0705)	(0.0760)
61-65 Years Old	-0.407***	-0.235***	-0.271***	-0.296***	-0.589***
	(0.0426)	(0.0822)	(0.0816)	(0.0842)	(0.0972)
66-70 Years Old	-0.426***	-0.256***	-0.304***	-0.301***	-0.517***
	(0.0526)	(0.0948)	(0.107)	(0.106)	(0.116)
71-75 Years Old	-0.688***	-0.362**	-0.755***	-0.645***	-0.770***
	(0.0923)	(0.152)	(0.179)	(0.206)	(0.238)
Female	-0.108***	-0.0833***	-0.0948***	-0.107***	-0.132***
	(0.0120)	(0.0241)	(0.0239)	(0.0242)	(0.0241)
Married	-0.000474	0.00701	-0.0764*	-0.0623	0.0710
	(0.0236)	(0.0465)	(0.0430)	(0.0464)	(0.0525)
Primary School	-0.0182	-0.0340	0.0190	-0.0606	-0.0211
	(0.0228)	(0.0424)	(0.0426)	(0.0432)	(0.0601)
Middle School	0.0380*	-0.00939	0.0920**	-0.00939	0.0373
	(0.0227)	(0.0422)	(0.0424)	(0.0431)	(0.0606)
High School or More	0.120***	0.0146	0.153***	0.0786*	0.0434
	(0.0237)	(0.0452)	(0.0450)	(0.0448)	(0.0622)
Current Health Status: Good	-0.0343*	-0.0448	-0.0257	-0.0291	-0.0508
	(0.0207)	(0.0514)	(0.0360)	(0.0362)	(0.0528)
Current Health Status: Fair	-0.101***	-0.162***	-0.0981**	-0.0348	-0.0827
	(0.0258)	(0.0588)	(0.0451)	(0.0471)	(0.0716)
Current Health Status: Poor	-0.123**	-0.161*	-0.0932	-0.0834	0.0861
	(0.0512)	(0.0974)	(0.0862)	(0.106)	(0.194)
County FE	YES	YES	YES	YES	YES
Wave FE	YES	YES	YES	YES	YES
No. of Obs.	23997	6000	6000	5999	5998
R-Squared	0.283	0.348	0.328	0.292	0.306

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.16: Quartile Log of Work Hours (2SLS)

		By Income Quartile			
	ALL	25%	50%	75%	100%
NCMS	-0.0787*	0.107	0.00907	-0.215**	-0.214*
	(0.0478)	(0.111)	(0.0877)	(0.0963)	(0.115)
26-30 Years Old	-0.0557*	-0.0617	-0.0602	0.0775	-0.0422
	(0.0288)	(0.0541)	(0.0528)	(0.0590)	(0.0592)
31-35 Years Old	-0.0489*	0.0395	-0.00912	-0.0160	-0.0866
	(0.0284)	(0.0504)	(0.0534)	(0.0608)	(0.0611)
36-40 Years Old	-0.0966***	-0.0400	-0.0132	-0.0667	-0.138**
	(0.0287)	(0.0521)	(0.0547)	(0.0590)	(0.0625)
41-45 Years Old	-0.0823***	0.0544	-0.0138	-0.0700	-0.179***
	(0.0287)	(0.0534)	(0.0535)	(0.0597)	(0.0611)
46-50 Years Old	-0.114***	-0.00696	-0.0434	-0.0482	-0.244***
	(0.0300)	(0.0597)	(0.0572)	(0.0582)	(0.0634)
51-55 Years Old	-0.147***	-0.0144	-0.0621	-0.0994	-0.274***
	(0.0312)	(0.0594)	(0.0600)	(0.0615)	(0.0670)
56-60 Years Old	-0.286***	-0.166***	-0.232***	-0.178**	-0.402***
	(0.0347)	(0.0639)	(0.0659)	(0.0694)	(0.0746)
61-65 Years Old	-0.414***	-0.243***	-0.280***	-0.300***	-0.593***
	(0.0426)	(0.0814)	(0.0805)	(0.0830)	(0.0954)
66-70 Years Old	-0.428***	-0.247***	-0.312***	-0.303***	-0.522***
	(0.0525)	(0.0931)	(0.107)	(0.105)	(0.114)
71-75 Years Old	-0.692***	-0.352**	-0.765***	-0.648***	-0.774***
	(0.0920)	(0.150)	(0.176)	(0.203)	(0.234)
Female	-0.109***	-0.0833***	-0.0950***	-0.107***	-0.133***
	(0.0120)	(0.0238)	(0.0235)	(0.0238)	(0.0237)
Married	-0.00388	0.00503	-0.0797*	-0.0652	0.0734
	(0.0236)	(0.0458)	(0.0426)	(0.0458)	(0.0514)
Primary School	-0.0165	-0.0339	0.0221	-0.0598	-0.0233
	(0.0227)	(0.0417)	(0.0419)	(0.0425)	(0.0590)
Middle School	0.0401*	-0.00942	0.0975**	-0.00890	0.0345
	(0.0226)	(0.0416)	(0.0418)	(0.0424)	(0.0595)
High School or More	0.127***	0.0149	0.160***	0.0808*	0.0366
	(0.0238)	(0.0446)	(0.0444)	(0.0442)	(0.0616)
Current Health Status: Good	-0.0352*	-0.0539	-0.0295	-0.0296	-0.0477
	(0.0206)	(0.0508)	(0.0354)	(0.0356)	(0.0515)
Current Health Status: Fair	-0.1000***	-0.170***	-0.102**	-0.0347	-0.0826
	(0.0257)	(0.0581)	(0.0444)	(0.0464)	(0.0704)
Current Health Status: Poor	-0.125**	-0.186*	-0.0910	-0.0881	0.0859
	(0.0509)	(0.0971)	(0.0847)	(0.104)	(0.191)
County FE	YES	YES	YES	YES	YES
Wave FE	YES	YES	YES	YES	YES
No. of Obs.	23997	6000	6000	5999	5998
R-Squared	0.282	0.346	0.326	0.292	0.306

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.17: Robustness Check: Extensions Modifying How Age Enters the Model (2SLS)

	All	Male	Female	HH Head	Non-HH Head	HH Level
NCMS	-0.0752	-0.0333	-0.164**	0.0179	-0.167***	-0.0294
	(0.0480)	(0.0640)	(0.0716)	(0.0720)	(0.0642)	(0.0579)
Age	0.0171***	0.0122***	0.0324***	0.0413***	0.0207***	0.0207***
	(0.00361)	(0.00454)	(0.00603)	(0.00710)	(0.00502)	(0.00460)
Age Squared	-0.000289***	-0.000215***	-0.000492***	-0.000495***	-0.000360***	-0.000339***
	(0.0000409)	(0.0000514)	(0.0000689)	(0.0000744)	(0.0000594)	(0.0000520)
Female	-0.109***	0	0	-0.112***	-0.0947***	-0.145***
	(0.0120)	(.)	(.)	(0.0411)	(0.0177)	(0.0217)
Married	-0.0380*	0.0251	-0.161***	0.0266	-0.0287	-0.0635**
	(0.0223)	(0.0284)	(0.0365)	(0.0451)	(0.0273)	(0.0269)
Widowed	-0.116**	-0.0398	-0.224***	-0.0569	-0.114	-0.133**
	(0.0483)	(0.0681)	(0.0695)	(0.0650)	(0.111)	(0.0560)
Primary School	-0.0166	-0.0492	-0.0518*	-0.0753*	-0.0178	-0.0462
	(0.0227)	(0.0359)	(0.0304)	(0.0393)	(0.0286)	(0.0317)
Middle School	0.0397*	0.0108	0.0184	-0.0000310	0.0347	0.0402
	(0.0226)	(0.0346)	(0.0316)	(0.0399)	(0.0282)	(0.0306)
High School or More	0.127***	0.0917**	0.124***	0.103**	0.114***	0.151***
	(0.0238)	(0.0357)	(0.0339)	(0.0413)	(0.0298)	(0.0316)
Current Health Status: Good	-0.0341*	-0.0423	-0.209***	-0.0138	-0.0467*	-0.0436*
	(0.0207)	(0.0261)	(0.0575)	(0.0310)	(0.0279)	(0.0252)
Current Health Status: Fair	-0.0984***	-0.101***	-0.270***	-0.0838**	-0.103***	-0.127***
	(0.0258)	(0.0334)	(0.0612)	(0.0379)	(0.0353)	(0.0325)
Current Health Status: Poor	-0.120**	-0.161**	-0.259***	-0.0451	-0.170**	-0.140**
	(0.0510)	(0.0748)	(0.0841)	(0.0726)	(0.0700)	(0.0672)
Number of Children	-0.00912	-0.0157*	-0.00300	-0.0134	-0.0140	-0.00143
	(0.00672)	(0.00901)	(0.0101)	(0.0102)	(0.00912)	(0.00900)
County FE	YES	YES	YES	YES	YES	YES
Wave FE	YES	YES	YES	YES	YES	YES
No. of Obs.	23997	13853	10104	10478	13519	12559
R-Squared	0.281	0.258	0.328	0.277	0.297	0.378

Note: Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.18: Robustness Check: Wave Specification Regression_Pre NCMS

	Without 1989		Without 1991		Without 1993		Without 1997		Without 2000	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
NCMS	-0.115***	-0.0907**	-0.116***	-0.0919**	-0.115***	-0.0900**	-0.0976***	-0.0727*	-0.0967***	-0.0676*
	(0.0248)	(0.0388)	(0.0249)	(0.0390)	(0.0249)	(0.0391)	(0.0253)	(0.0397)	(0.0253)	(0.0399)
Control Variables	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Wave FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
No. of Obs.	18051	18051	17717	17717	17729	17729	17469	17469	17095	17095
R-Squared	0.235	0.235	0.238	0.238	0.236	0.236	0.229	0.229	0.230	0.230

Note: Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.19: Robustness Check: Wave Specification Regression_Post NCMS

	Without 2004		Without 2006		Without 2008		Without 2011	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
NCMS	-0.0714***	0.0633	-0.327***	-0.485***	-0.105***	-0.0950**	-0.0745***	-0.0412
	(0.0275)	(0.0469)	(0.0342)	(0.0548)	(0.0271)	(0.0418)	(0.0280)	(0.0421)
Control Variables	YES	YES	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	YES	YES	YES	YES	YES
Wave FE	YES	YES	YES	YES	YES	YES	YES	YES
No. of Obs.	14473	14473	14150	14150	14077	14077	13647	13647
R-Squared	0.256	0.255	0.244	0.243	0.242	0.242	0.237	0.237

Note: Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.20: NCMS County Eligibility Table

	NO. of NCMS Household Enrollment				
County ID\Survey Waves	2000	2004	2006	2009	2011
1121	0	0	0	0	74
1122	0	0	0	0	81
2121	0	0	74	94	84
2122	0	0	28	78	83
2123	0	0	88	121	106
2124	0	0	86	101	116
2321	0	2	101	115	103
2322	0	36	0	93	101
2323	0	0	58	78	79
2324	0	0	66	108	128
3121	0	0	0	0	72
3122	0	0	0	0	24
3221	0	126	152	133	121
3222	0	125	142	124	114
3223	0	27	80	90	122
3224	0	168	176	133	129
3721	0	0	118	92	112
3722	0	0	6	199	124
3723	0	51	89	84	96
3724	0	26	73	103	90
4121	0	0	0	85	85
4122	0	0	0	69	55
4123	0	0	132	176	140
4124	0	0	0	142	202
4221	0	0	0	77	77
4222	0	2	54	87	115
4223	0	0	76	125	74
4224	0	0	145	132	106
4321	0	0	0	103	59
4322	0	52	51	48	47
4323	0	2	2	116	102
4324	0	1	0	80	70
4521	0	0	112	146	164
4522	0	0	75	152	154
4523	0	0	71	116	133
4524	0	0	0	59	128
5221	0	0	95	87	97
5222	0	0	0	86	82
5223	0	0	0	188	125
5224	0	0	82	102	69
5521	0	0	0	0	70
5522	0	0	0	0	64
5523	0	0	0	0	124

Table 1.21: Robustness Check:Recoding within County Household Enrollment Less than 2 as Non-eligible Counties

	All	Male	Female	HH Head	Non-HH Head	HH Level
NCMS Enrollment	-0.0831*	-0.0651	-0.137**	-0.0277	-0.140**	-0.0657
	(0.0437)	(0.0577)	(0.0663)	(0.0643)	(0.0596)	(0.0509)
26-30 Years Old	-0.0557*	-0.0477	-0.0172	-0.0458	-0.0409	-0.115***
	(0.0288)	(0.0368)	(0.0452)	(0.113)	(0.0315)	(0.0373)
31-35 Years Old	-0.0488*	-0.0874**	0.0471	-0.0331	-0.0164	-0.0962***
	(0.0284)	(0.0363)	(0.0453)	(0.110)	(0.0327)	(0.0357)
36-40 Years Old	-0.0966***	-0.129***	-0.00753	-0.0162	-0.0900***	-0.138***
	(0.0287)	(0.0370)	(0.0454)	(0.110)	(0.0346)	(0.0362)
41-45 Years Old	-0.0822***	-0.114***	0.00597	-0.0205	-0.0532	-0.128***
	(0.0287)	(0.0366)	(0.0461)	(0.110)	(0.0349)	(0.0367)
46-50 Years Old	-0.114***	-0.115***	-0.0660	0.00390	-0.140***	-0.144***
	(0.0300)	(0.0380)	(0.0487)	(0.110)	(0.0384)	(0.0389)
51-55 Years Old	-0.147***	-0.116***	-0.156***	-0.0104	-0.203***	-0.164***
	(0.0312)	(0.0383)	(0.0527)	(0.110)	(0.0424)	(0.0401)
56-60 Years Old	-0.286***	-0.265***	-0.303***	-0.168	-0.316***	-0.360***
	(0.0346)	(0.0434)	(0.0582)	(0.113)	(0.0484)	(0.0459)
61-65 Years Old	-0.414***	-0.367***	-0.472***	-0.264**	-0.498***	-0.474***
	(0.0426)	(0.0532)	(0.0707)	(0.116)	(0.0653)	(0.0577)
66-70 Years Old	-0.428***	-0.380***	-0.474***	-0.300**	-0.478***	-0.566***
	(0.0525)	(0.0641)	(0.0908)	(0.120)	(0.0905)	(0.0667)
71-75 Years Old	-0.692***	-0.688***	-0.760***	-0.490***	-0.921***	-0.857***
	(0.0919)	(0.115)	(0.158)	(0.147)	(0.167)	(0.116)
Female	-0.109***	0	0	-0.111***	-0.0997***	-0.146***
	(0.0120)	(.)	(.)	(0.0409)	(0.0177)	(0.0217)
Married	-0.00375	0.0653**	-0.134***	0.0328	-0.00499	-0.00325
	(0.0236)	(0.0300)	(0.0386)	(0.0449)	(0.0281)	(0.0283)
Widowed	-0.0783	0.00271	-0.201***	-0.0537	-0.0777	-0.0613
	(0.0491)	(0.0686)	(0.0712)	(0.0648)	(0.112)	(0.0572)
Primary School	-0.0166	-0.0609*	-0.0456	-0.0756*	-0.0148	-0.0541*
	(0.0227)	(0.0360)	(0.0303)	(0.0394)	(0.0286)	(0.0317)
Middle School	0.0400*	-0.00200	0.0241	-0.000574	0.0396	0.0318
	(0.0226)	(0.0346)	(0.0316)	(0.0399)	(0.0282)	(0.0305)
High School or More	0.127***	0.0779**	0.133***	0.0983**	0.122***	0.139***
	(0.0237)	(0.0355)	(0.0338)	(0.0412)	(0.0297)	(0.0313)
Current Health Status: Good	-0.0352*	-0.0430*	-0.210***	-0.0167	-0.0470*	-0.0421*
	(0.0206)	(0.0260)	(0.0576)	(0.0309)	(0.0279)	(0.0250)
Current Health Status: Fair	-0.1000***	-0.102***	-0.271***	-0.0876**	-0.103***	-0.125***
	(0.0257)	(0.0333)	(0.0612)	(0.0377)	(0.0353)	(0.0324)
Current Health Status: Poor	-0.125**	-0.165**	-0.262***	-0.0555	-0.171**	-0.145**
	(0.0509)	(0.0748)	(0.0838)	(0.0722)	(0.0696)	(0.0668)
Number of Kids	-0.0104	-0.0173*	-0.00334	-0.0133	-0.0143	-0.00549
	(0.00673)	(0.00900)	(0.0101)	(0.0102)	(0.00912)	(0.00899)
No. of Obs.	23997	13853	10104	10478	13519	12559
R-Squared	0.282	0.260	0.328	0.279	0.298	0.382

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.22: Robustness Check: Dropping Counties with Household NCMS Enrollment Less than 2

	All	Male	Female	HH Head	Non-HH Head	HH Level
NCMS	-0.116***	-0.0866	-0.187***	-0.0373	-0.192***	-0.0848
	(0.0446)	(0.0595)	(0.0667)	(0.0667)	(0.0600)	(0.0526)
26-30 Years Old	-0.0672**	-0.0537	-0.0381	-0.0484	-0.0549*	-0.119***
	(0.0285)	(0.0367)	(0.0443)	(0.115)	(0.0311)	(0.0374)
31-35 Years Old	-0.0597**	-0.0907**	0.0221	-0.0334	-0.0315	-0.0998***
	(0.0280)	(0.0362)	(0.0441)	(0.112)	(0.0322)	(0.0358)
36-40 Years Old	-0.111***	-0.133***	-0.0373	-0.0177	-0.109***	-0.145***
	(0.0284)	(0.0369)	(0.0442)	(0.111)	(0.0342)	(0.0363)
41-45 Years Old	-0.0998***	-0.121***	-0.0291	-0.0268	-0.0749**	-0.139***
	(0.0283)	(0.0365)	(0.0448)	(0.111)	(0.0344)	(0.0367)
46-50 Years Old	-0.128***	-0.119***	-0.0932*	0.00220	-0.157***	-0.151***
	(0.0297)	(0.0379)	(0.0476)	(0.112)	(0.0380)	(0.0391)
51-55 Years Old	-0.162***	-0.121***	-0.189***	-0.0150	-0.224***	-0.169***
	(0.0309)	(0.0382)	(0.0516)	(0.112)	(0.0420)	(0.0402)
56-60 Years Old	-0.298***	-0.270***	-0.325***	-0.173	-0.327***	-0.367***
	(0.0344)	(0.0433)	(0.0573)	(0.114)	(0.0481)	(0.0461)
61-65 Years Old	-0.431***	-0.373***	-0.499***	-0.272**	-0.514***	-0.484***
	(0.0426)	(0.0535)	(0.0703)	(0.117)	(0.0654)	(0.0582)
66-70 Years Old	-0.446***	-0.398***	-0.495***	-0.318***	-0.484***	-0.574***
	(0.0524)	(0.0646)	(0.0896)	(0.122)	(0.0897)	(0.0675)
71-75 Years Old	-0.716***	-0.708***	-0.793***	-0.507***	-0.950***	-0.873***
	(0.0935)	(0.117)	(0.160)	(0.150)	(0.169)	(0.119)
Female	-0.108***	0	0	-0.110***	-0.0996***	-0.148***
	(0.0121)	(.)	(.)	(0.0411)	(0.0178)	(0.0219)
Married	0.00756	0.0671**	-0.108***	0.0334	0.0105	0.00321
	(0.0235)	(0.0301)	(0.0379)	(0.0454)	(0.0278)	(0.0284)
Widowed	-0.0675	0.00199	-0.176**	-0.0498	-0.0588	-0.0497
	(0.0496)	(0.0696)	(0.0717)	(0.0657)	(0.114)	(0.0581)
Primary School	-0.0170	-0.0625*	-0.0431	-0.0769*	-0.0138	-0.0547*
	(0.0229)	(0.0362)	(0.0304)	(0.0397)	(0.0287)	(0.0319)
Middle School	0.0378*	-0.00605	0.0249	-0.00561	0.0394	0.0315
	(0.0227)	(0.0347)	(0.0316)	(0.0402)	(0.0282)	(0.0307)
High School or More	0.126***	0.0785**	0.129***	0.0984**	0.120***	0.139***
	(0.0238)	(0.0357)	(0.0338)	(0.0415)	(0.0298)	(0.0316)
Current Health Status: Good	-0.0374*	-0.0478*	-0.198***	-0.0192	-0.0486*	-0.0426*
	(0.0209)	(0.0263)	(0.0574)	(0.0317)	(0.0280)	(0.0255)
Current Health Status: Fair	-0.0982***	-0.103***	-0.255***	-0.0843**	-0.101***	-0.119***
	(0.0261)	(0.0338)	(0.0614)	(0.0385)	(0.0357)	(0.0329)
Current Health Status: Poor	-0.125**	-0.184**	-0.237***	-0.0809	-0.153**	-0.143**
	(0.0522)	(0.0774)	(0.0846)	(0.0746)	(0.0711)	(0.0683)
Number of Kids	-0.00807	-0.0168*	0.00144	-0.0113	-0.0120	-0.00219
	(0.00678)	(0.00911)	(0.0101)	(0.0104)	(0.00915)	(0.00908)
No. of Obs.	23544	13600	9907	10257	13287	12305
R-Squared	0.286	0.263	0.333	0.282	0.302	0.386

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.23: Robustness Check: Propensity Score Matching

	Annual Work Hours	Household Chores	Household Savings
One-on-one Matching	-488.68***	-11.57*	3089.18
	[159.537]	[8.883]	[2877.594]
No. of Obs	25,022	6,587	6,305

Note: Annual work hours are in unit of hours/year, while the unit of household chores is miniutes/day. Househould savings is in units of RMB.

Table 1.24: Robustness Check: Matching Balancing Properties between Treated and Control

	Pre-matching				Post-matching				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Treated	Control	t-stat	p>|t|	Treated	Control	t-stat	p>|t|	Bias
26-30 Years Old	0.113	0.054	17.10	0.000	0.054	0.055	-0.10	0.923	-0.1
31-35 Years Old	0.129	0.087	10.78	0.000	0.087	0.087	0.17	0.867	0.2
36-40 Years Old	0.150	0.132	4.05	0.000	0.133	0.131	-0.45	0.651	0.6
41-45 Years Old	0.137	0.159	-4.75	0.000	0.159	0.159	0.03	0.974	0.0
46-50 Years Old	0.123	0.141	-4.28	0.000	0.142	0.144	-0.47	0.641	-0.6
51-55 Years Old	0.093	0.132	-9.80	0.000	0.133	0.134	-0.27	0.789	-0.4
56-60 Years Old	0.056	0.111	-15.30	0.000	0.111	0.110	0.42	0.676	0.6
61-65 Years Old	0.024	0.074	-17.90	0.000	0.074	0.075	-0.29	0.769	-0.5
66-70 Years Old	0.014	0.036	-10.71	0.000	0.036	0.038	-0.70	0.486	-1.1
71-75 Years Old	0.004	0.012	-6.79	0.000	0.011	0.008	1.97	0.049	2.9
Gender	1.406	1.442	-5.71	0.000	1.441	1.448	-1.05	0.293	-1.4
Never Married	0.146	0.046	27.86	0.000	0.045	0.041	1.52	0.128	1.4
Married	0.810	0.909	-23.01	0.000	0.910	0.930	-5.59	0.000	-6.0
Divorced	0.011	0.006	4.53	0.000	0.006	0.003	2.95	0.003	3.0
Widowed	0.019	0.031	-5.83	0.000	0.030	0.020	5.06	0.000	6.9
Primary School	0.232	0.335	-17.95	0.000	0.333	0.324	1.49	0.137	2.1
Middle School	0.380	0.430	-7.90	0.000	0.430	0.439	-1.28	0.202	-1.7
High School or More	0.300	0.143	30.60	0.000	0.143	0.142	0.17	0.863	0.2
Current Health Status: Excellent	0.102	0.041	19.21	0.000	0.041	0.042	-0.47	0.637	-0.5
Current Health Status: Good	0.328	0.129	39.12	0.000	0.129	0.129	-0.06	0.952	-0.1
Current Health Status: Fair	0.151	0.073	19.88	0.000	0.074	0.077	-0.94	0.348	-1.1
Current Health Status: Poor	0.020	0.014	3.90	0.000	0.014	0.014	-0.13	0.897	-0.2
Number of Kids	0.982	1.012	-2.41	0.016	1.014	0.707	25.07	0.000	31.4
Household Size	4.143	3.971	8.56	0.000	3.976	3.398	30.19	0.000	36.8
Observations	13,990	11,073			11,059	13,963			

Chapter 2 Tables

Table 2.1: Summary Statistics

	Full			Female			Male		
	Obs	Mean	SD	Obs	Mean	SD	Obs	Mean	SD
Panel1: Dependent Variables									
Child's grade in Chinese language/grammar last semester	10523	2.18	0.96	4904	2.04	0.94	5619	2.31	0.96
Child's grade in math last semester	10524	2.17	1.00	4903	2.16	0.99	5621	2.18	1.01
Primary School Entry Age	12660	6.44	0.70	5895	6.42	0.70	6765	6.47	0.70
Redshirt	12660	0.20	0.40	5895	0.19	0.39	6765	0.21	0.41
Panel 2: Independent Variables									
1 if Tall	12660	0.56	0.50	5895	0.55	0.50	6765	0.57	0.50
Female*Tall	12660	0.26	0.44	5895	0.55	0.50	6765	0.00	0.00
Panel 3: Covariates									
Birth year	12660	2003.16	2.79	5895	2003.20	2.81	6765	2003.13	2.77
Birth month	12660	6.65	3.42	5895	6.64	3.41	6765	6.67	3.43
Indicator for birthday September 1st or later	12660	0.35	0.48	5895	0.35	0.48	6765	0.35	0.48
Child's current age in years	12660	9.43	2.73	5895	9.40	2.73	6765	9.47	2.73
Female=0; Male=1	12660	0.53	0.50	5895	0.00	0.00	6765	1.00	0.00
Child's current height	12660	129.29	22.55	5895	128.74	22.35	6765	129.78	22.71
Child's current weight (kg)	12358	29.92	11.38	5753	29.10	10.82	6605	30.64	11.81
Child's gestational age	12169	9.30	0.60	5671	9.31	0.61	6498	9.30	0.58
Child's weight at birth (kg)	10427	3.20	0.56	4861	3.12	0.55	5566	3.28	0.56
Child suffered from disability because of serious injury or disease	9929	0.01	0.08	4628	0.01	0.07	5301	0.01	0.09
Current stage of school	8142	2.07	0.38	3816	2.07	0.39	4326	2.07	0.36
Grade of child	10574	3.42	1.69	4933	3.42	1.69	5641	3.41	1.68
Class rank in last exam(mid-term/final)	3782	2.32	1.24	1739	2.18	1.23	2043	2.45	1.25
No. of students in class this/last semester	5724	44.77	17.75	2666	44.43	17.56	3058	45.07	17.91
Father's education	12489	2.80	1.20	5818	2.80	1.21	6671	2.80	1.19
Mother's education	12459	2.54	1.34	5781	2.56	1.32	6678	2.53	1.36
1 If Han Ethnic Group	12660	0.88	0.32	5895	0.88	0.32	6765	0.88	0.33
Rural=0; Urban=1	12660	0.39	0.49	5895	0.39	0.49	6765	0.38	0.49

Table 2.2: OLS regression_Children size impact on SSA

	Pooled			Birth Month between Jun. to Oct.		Birth Month Excluding Jun. to Oct.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1 if big	-0.0813***	-0.0556***	-0.0272	-0.0942***	-0.0401	-0.0299	-0.0224
	(0.0132)	(0.0149)	(0.0168)	(0.0226)	(0.0255)	(0.0195)	(0.0219)
Female*Big			-0.0612***		-0.116***		-0.0163
			(0.0166)		(0.0256)		(0.0211)
Child's gestational age		0.0401***	0.0414***	0.0633***	0.0671***	0.0216	0.0218
		(0.0116)	(0.0116)	(0.0176)	(0.0175)	(0.0147)	(0.0147)
Child's weight at birth (kg)		-0.0623***	-0.0677***	-0.0366*	-0.0472**	-0.0634***	-0.0647***
		(0.0131)	(0.0132)	(0.0193)	(0.0194)	(0.0174)	(0.0176)
Father's education		-0.0195***	-0.0196***	0.00339	0.00285	-0.0390***	-0.0390***
		(0.00713)	(0.00713)	(0.0109)	(0.0109)	(0.00911)	(0.00911)
Mother's education		-0.0308***	-0.0307***	-0.0393***	-0.0387***	-0.0179**	-0.0180**
		(0.00713)	(0.00712)	(0.0110)	(0.0109)	(0.00884)	(0.00883)
Province FE	YES	YES	YES	YES	YES	YES	YES
Wave FE	YES	YES	YES	YES	YES	YES	YES
No. of Obs.	12660	10144	10144	4593	4593	5551	5551
R-Squared	0.0420	0.0602	0.0614	0.0723	0.0762	0.0641	0.0642

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.3: Probit Model: Probability of Redshirting

	Pooled			Birth Month between Jun. to Oct.		Birth Month Excluding Jun. to Oct.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1 if big	-0.194***	-0.152***	-0.116***	-0.205***	-0.145***	-0.116***	-0.0994*
	(0.0276)	(0.0327)	(0.0374)	(0.0487)	(0.0557)	(0.0443)	(0.0509)
Female*Big			-0.0810**		-0.132**		-0.0366
			(0.0410)		(0.0617)		(0.0554)
Child's gestational age		-0.00933	-0.00805	-0.0172	-0.0130	-0.00621	-0.00612
		(0.0255)	(0.0255)	(0.0368)	(0.0370)	(0.0360)	(0.0360)
Child's weight at birth (kg)		-0.0997***	-0.106***	-0.0754*	-0.0863**	-0.108***	-0.111***
		(0.0293)	(0.0295)	(0.0427)	(0.0429)	(0.0408)	(0.0411)
Father's education		-0.0842***	-0.0843***	-0.0148	-0.0153	-0.153***	-0.152***
		(0.0180)	(0.0180)	(0.0260)	(0.0261)	(0.0250)	(0.0250)
Mother's education		-0.0827***	-0.0826***	-0.0761***	-0.0754***	-0.0835***	-0.0836***
		(0.0175)	(0.0174)	(0.0255)	(0.0254)	(0.0243)	(0.0243)
Province FE	YES	YES	YES	YES	YES	YES	YES
Wave FE	YES	YES	YES	YES	YES	YES	YES
No. of Obs.	12656	10139	10139	4591	4591	5548	5548
R-Squared							

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.4: Children Size & School Entry Age Effect on Grade in Chinese Language

	OLS		Ordered Logit Model		
	(1)	(2)	(3)	(4)	(5)
1 if big	-0.171***	-0.565***	-0.283***	-0.340***	-0.338***
	(0.0382)	(0.0456)	(0.0613)	(0.0757)	(0.0761)
Indicator for birthday September 1st or later	-0.168***	-0.0578	-0.243**	-0.330***	-0.328***
	(0.0596)	(0.0555)	(0.100)	(0.119)	(0.120)
1 if Big*1 if birth month greater than 8	0.0457	-0.0230	-0.0140	0.107	0.0991
	(0.0566)	(0.0747)	(0.0965)	(0.114)	(0.114)
Birth month	0.0137**		0.0248**	0.0254*	0.0255*
	(0.00664)		(0.0114)	(0.0136)	(0.0136)
Primary School Entry Age	-0.0130		-0.0549	-0.0414	-0.0405
	(0.0208)		(0.0348)	(0.0415)	(0.0420)
Child's gestational age	-0.0101		-0.0961**	-0.0251	-0.0209
	(0.0232)		(0.0385)	(0.0471)	(0.0471)
Child's weight at birth (kg)	-0.0121		-0.0242	-0.0202	-0.0228
	(0.0257)		(0.0422)	(0.0514)	(0.0514)
Child suffered from disability because of serious injury or disease	0.535***		1.410***	1.141***	1.159***
	(0.190)		(0.336)	(0.379)	(0.379)
Father's education	-0.114***		-0.202***	-0.234***	-0.237***
	(0.0139)		(0.0236)	(0.0279)	(0.0280)
Mother's education	-0.0734***		-0.206***	-0.165***	-0.160***
	(0.0133)		(0.0223)	(0.0256)	(0.0258)
Child's Current Stage* Current Grade FE	YES	NO	NO	YES	YES
Province FE	YES	NO	NO	YES	NO
Wave FE	YES	NO	NO	NO	YES
No. of Obs.	4916	10523	6703	4916	4916
R-Squared	0.112				

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.5: Children Size & School Entry Age Effect on Grade in Math

	OLS		Ordered Logit Model		
	(1)	(2)	(3)	(4)	(5)
1 if big	-0.229***	-0.603***	-0.315***	-0.445***	-0.434***
	(0.0399)	(0.0453)	(0.0611)	(0.0753)	(0.0757)
Indicator for birthday September 1st or later	-0.116*	-0.0224	-0.144	-0.249**	-0.242**
	(0.0619)	(0.0550)	(0.0997)	(0.118)	(0.118)
1 if Big*1 if birth month greater than 8	0.0760	0.0212	0.0532	0.170	0.154
	(0.0588)	(0.0745)	(0.0958)	(0.113)	(0.113)
Birth month	0.00857		0.0170	0.0178	0.0177
	(0.00694)		(0.0114)	(0.0136)	(0.0136)
Primary School Entry Age	0.00371		-0.0120	0.000164	0.0000701
	(0.0219)		(0.0346)	(0.0413)	(0.0419)
Child's gestational age	-0.0495**		-0.0959**	-0.0961**	-0.0943**
	(0.0248)		(0.0384)	(0.0471)	(0.0471)
Child's weight at birth (kg)	-0.0606**		-0.143***	-0.114**	-0.115**
	(0.0266)		(0.0420)	(0.0509)	(0.0509)
Child suffered from disability because of serious injury or disease	0.610***		1.417***	1.242***	1.261***
	(0.198)		(0.327)	(0.368)	(0.368)
Father's education	-0.0901***		-0.151***	-0.171***	-0.174***
	(0.0139)		(0.0234)	(0.0274)	(0.0275)
Mother's education	-0.0951***		-0.237***	-0.204***	-0.202***
	(0.0137)		(0.0223)	(0.0256)	(0.0257)
Child's Current Stage* Current Grade FE	YES	NO	NO	YES	YES
Province FE	YES	NO	NO	YES	NO
Wave FE	YES	NO	NO	NO	YES
No. of Obs.	4915	10524	6704	4915	4915
R-Squared	0.121				

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.6: Gender Deviation on Test Grades (Ordered Logit Model)

	Male		Female	
	Chinese Grade	Math Grade	Chinese Grade	Math Grade
1 if big	-0.339***	-0.492***	-0.363***	-0.351***
	(0.106)	(0.106)	(0.112)	(0.111)
Indicator for birthday September 1st or later	-0.130	-0.167	-0.517***	-0.291*
	(0.166)	(0.165)	(0.178)	(0.174)
1 if Big*1 if birth month greater than 8	0.0985	0.322**	0.0476	-0.0923
	(0.158)	(0.158)	(0.169)	(0.165)
Birth month	0.00177	-0.0106	0.0504**	0.0476**
	(0.0187)	(0.0187)	(0.0203)	(0.0201)
Primary School Entry Age	-0.0921	-0.0383	-0.0318	0.0498
	(0.0585)	(0.0589)	(0.0614)	(0.0605)
Child's gestational age	-0.0488	-0.165***	0.0800	0.00373
	(0.0632)	(0.0636)	(0.0728)	(0.0717)
Child's weight at birth (kg)	-0.133*	-0.162**	-0.0973	-0.0634
	(0.0702)	(0.0696)	(0.0790)	(0.0777)
Child suffered from disability because of serious injury or disease	1.029**	1.227***	1.422**	1.352**
	(0.483)	(0.465)	(0.623)	(0.610)
Father's education	-0.233***	-0.182***	-0.259***	-0.158***
	(0.0393)	(0.0396)	(0.0413)	(0.0388)
Mother's education	-0.192***	-0.228***	-0.133***	-0.179***
	(0.0348)	(0.0355)	(0.0394)	(0.0380)
Child's Current Stage FE	YES	YES	YES	YES
Child's Current Grade FE	YES	YES	YES	YES
Province FE	YES	YES	YES	YES
Wave FE	YES	YES	YES	YES
No. of Obs.	2622	2621	2294	2294
R-Squared	0.0539	0.0559	0.0486	0.0502

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Chapter 3 Tables

Table 3.1: Summary Statistics

	All		*Female*		*Male*	
	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
Children						
Birth Year	2003.16	2.79	2003.20	2.81	2003.13	2.77
Birth Month	6.65	3.42	6.64	3.41	6.67	3.43
Indicator for birthday September 1st or later	0.35	0.48	0.35	0.48	0.35	0.48
Primary school entry age	6.44	0.70	6.42	0.70	6.47	0.70
Child's current age in years	9.43	2.73	9.40	2.73	9.47	2.73
1 If Female	0.47	0.50	1.00	0.00	0.00	0.00
Child's current height	129.29	22.55	128.74	22.35	129.78	22.71
Child's current weight (kg)	29.92	11.38	29.10	10.82	30.64	11.81
Child suffered from disability because of serious injury or disease	0.01	0.08	0.01	0.07	0.01	0.09
Child's grade in Chinese language/grammar last semester	2.18	0.96	2.04	0.94	2.31	0.96
Child's grade in math last semester	2.17	1.00	2.16	0.99	2.18	1.01
Current stage of school	2.07	0.38	2.07	0.39	2.07	0.36
Grade of child	3.42	1.69	3.42	1.69	3.41	1.68
Father's Highest Level of Education Attained	2.80	1.20	2.80	1.21	2.80	1.19
Mother's Highest Level of Education Attained	2.54	1.34	2.56	1.32	2.53	1.36
1 If Han Ethnic Group	0.88	0.32	0.88	0.32	0.88	0.33
1 If Urban	0.39	0.49	0.39	0.49	0.38	0.49
No. of Observations	**12660**		**5895**		**6765**	
Adults						
Birth Year	1988.28	4.72	1988.32	4.71	1988.25	4.73
Birth Month	6.92	3.49	6.90	3.46	6.94	3.51
Indicator for birthday September 1st or later	0.41	0.49	0.41	0.49	0.41	0.49
Primary School Entry Age	6.87	0.70	6.87	0.70	6.86	0.69
Years of Schooling Completed	11.13	3.18	11.20	3.21	11.06	3.16
Average Monthly Income (RMB)	1355.79	1761.74	1027.94	1515.66	1675.29	1919.16
1 If Currently Employed	0.69	0.46	0.61	0.49	0.77	0.42
Current Marital Status - Categorical	1.52	0.57	1.56	0.56	1.47	0.57
Age at First Marriage	23.09	2.23	22.64	2.07	23.73	2.30
Father's Years of Schooling	7.71	3.95	7.78	3.85	7.64	4.04
Mother's Years of Schooling	5.85	4.48	6.06	4.36	5.64	4.58
1 If Han Ethnic Group	0.94	0.24	0.94	0.24	0.94	0.24
1 If Urban	0.54	0.50	0.56	0.50	0.52	0.50
No. of Observations	**18898**		**9342**		**9556**	

Table 3.2: First Stage: Impacts of the After Cut-off Point Born on Primary School Entry Age

	(1)	(2)	(3)	(4)	(5)
	All	All	Jun-Nov Born	Female	Male
Indicator for birthday September 1st or later	0.440***	0.378***	0.259***	0.366***	0.387***
	(0.0348)	(0.0342)	(0.0400)	(0.0368)	(0.0431)
Birth Month	-0.0569***	-0.0555***	-0.0116	-0.0556***	-0.0548***
	(0.00411)	(0.00325)	(0.00781)	(0.00455)	(0.00508)
Father's Highest Level of Education Attained		-0.0224**	-0.0123	-0.0249***	-0.0214*
		(0.00821)	(0.00832)	(0.00861)	(0.0119)
Mother's Highest Level of Education Attained		-0.0232***	-0.0301***	-0.0230**	-0.0239***
		(0.00667)	(0.00782)	(0.00919)	(0.00812)
Province Fixed Effect	NO	YES	YES	YES	YES
Birth Year Fixed Effect	NO	YES	YES	YES	YES
No. of Obs.	31555	30717	16622	14751	15966
R-Squared	0.0287	0.191	0.202	0.208	0.180

Standard error in parentheses is robust and clustered at the province level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3.3: SSA Impacts on Short Run vs. Longer Run Outcomes (Pooled)

	(1) Child's grade in Chinese	(2) Child's grade in math	(3) Years of Schooling Completed	(4) Log Average Monthly Income	(5) Age at First Marriage
Predicted SSA	-0.287***	-0.131	0.117	0.100	-0.494
	(0.0958)	(0.108)	(0.240)	(0.249)	(0.439)
Child suffered from disability because of serious injury or disease	0.564***	0.632***			
	(0.138)	(0.202)			
Father's Highest Level of Education Attained	-0.118***	-0.102***	0.491***	0.0923***	0.247***
	(0.0169)	(0.0146)	(0.0357)	(0.0226)	(0.0469)
Mother's Highest Level of Education Attained	-0.111***	-0.107***	0.610***	0.0645*	0.108
	(0.0145)	(0.0152)	(0.0537)	(0.0321)	(0.0714)
No. of Obs.	8023	8026	18402	15469	4947
R-Squared	0.100	0.100	0.199	0.149	0.270
Province FE	YES	YES	YES	YES	YES
Birth Year FE	YES	YES	YES	YES	YES

Standard error in parentheses is robust and clustered at the province level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3.4: SSA Impacts on Short Run vs. Longer Run Outcomes (Female)

	(1) Child's grade in Chinese	(2) Child's grade in math	(3) Years of Schooling Completed	(4) Log Average Monthly Income	(5) Age at First Marriage
Predicted SSA	-0.382***	-0.312*	0.492	-0.180	-0.658
	(0.124)	(0.169)	(0.492)	(0.484)	(0.528)
Child suffered from disability because of serious injury or disease	0.605**	0.500			
	(0.225)	(0.344)			
Father's Highest Level of Education Attained	-0.126***	-0.100***	0.487***	0.182***	0.251***
	(0.0322)	(0.0276)	(0.0547)	(0.0334)	(0.0591)
Mother's Highest Level of Education Attained	-0.110***	-0.102***	0.711***	0.106*	0.106
	(0.0185)	(0.0165)	(0.102)	(0.0541)	(0.0889)
No. of Obs.	3715	3716	9038	7502	2854
R-Squared	0.112	0.100	0.209	0.143	0.271
Province FE	YES	YES	YES	YES	YES
Birth Year FE	YES	YES	YES	YES	YES

Standard error in parentheses is robust and clustered at the province level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3.5: SSA Impacts on Short Run vs. Longer Run Outcomes (Male)

	(1) Child's grade in Chinese	(2) Child's grade in math	(3) Years of Schooling Completed	(4) Log Average Monthly Income	(5) Age at First Marriage
Predicted SSA	-0.181	0.0195	-0.261	0.162	-0.666
	(0.107)	(0.112)	(0.331)	(0.278)	(0.738)
Child suffered from disability because of serious injury or disease	0.553***	0.734***			
	(0.166)	(0.169)			
Father's Highest Level of Education Attained	-0.111***	-0.102***	0.487***	0.00103	0.226***
	(0.0182)	(0.0169)	(0.0510)	(0.0242)	(0.0784)
Mother's Highest Level of Education Attained	-0.110***	-0.110***	0.522***	0.0687	0.201**
	(0.0171)	(0.0184)	(0.0660)	(0.0550)	(0.0852)
No. of Obs.	4308	4310	9364	7967	2093
R-Squared	0.103	0.115	0.200	0.211	0.314
Province FE	YES	YES	YES	YES	YES
Birth Year FE	YES	YES	YES	YES	YES

Standard error in parentheses is robust and clustered at the province level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$