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TWO ESSAYS ON TAIWAN'S HEALTH CARE REFORM IN THE 1990s

By

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ABSTRACT

TWO ESSAYS ON TAIWAN'S HEALTH CARE REFORM IN THE 1990s

By

Kang-Hung Chang

This dissertation consists of two essays examining major health care reforms in Taiwan in the 1990s and their effects on the elderly. Both reforms are analyzed as natural experiments, comparing the experience of an affected group with a control group that was not affected.

The first essay investigates the National Health Insurance (NHI) program that provided universal coverage to Taiwan's 21 million citizens, including 8 million previously uninsured. The essay finds that NHI largely increased utilization for the previously uninsured elderly relative to their continuously insured counterparts. Corresponding to the increase in utilization, the mortality hazard ratio of the previously uninsured to the continuously insured elderly significantly dropped from 1.3 in the pre-NHI period to close to 1 in the post-NHI period, suggesting that NHI improved the health of the previously uninsured elderly through medical care.

The second essay inspects a special medicine culture in Taiwan—both physicians and pharmacists prescribed and dispensed drugs—which was profoundly changed in the 1990s by NHI and a separation policy (SP) that forbade physicians from dispensing and pharmacists from prescribing. The essay finds that NHI made the previously uninsured elderly more likely to visit physicians, including some who previously only visited pharmacists. Following this, the SP made all elderly patients more likely to only visit physicians and buy drugs from on-site pharmacists who were hired by physicians.

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Writing a dissertation is like cooking a dish, it takes a recipe and many ingredients. Without a doubt, the people and institutions mentioned above have provided me all I need. I hope I have cooked a good dissertation in return.

PROLOGUE

Financial incentives and institutional constraints are both important factors in determining people's health care utilization behavior and thus can further affect their consequent health outcomes. This dissertation consists of two chapters studying effects of two major health care reforms in Taiwan in the 1990s that profoundly changed financial incentives faced by and institutional constraints imposed upon the elderly.

In 1994, about 60% of Taiwan's 21 million citizens were insured by three major social insurance programs—Government Employees Insurance, Labor Insurance and Farmer's Health Insurance—which all provided comprehensive medical benefits. As suggested by their names, these programs were employment-based and had covered most of the healthy working population at the time. Besides, comprehensive private health insurance did not exist then. The rest of the population—mostly the elderly, women and children—were thus left as uninsured and had to pay their medical care expenditure completely out-of-pocket.

On March 1, 1995, the government integrated the three major social insurance programs into a single-payer third-party universal program entitled as National Health Insurance (NHI). This new program was not only universal but also compulsory—it required the previously uninsured to enroll regardless of their health, while others who were previously insured by the three major social insurance programs were automatically transferred to NHI. The implementation of NHI was a rare large-scale *natural experiment* on health insurance in recent decades. It greatly reduced the price of medical care faced by the previously uninsured elderly when ill. More importantly, its universal and

compulsory feature generated arguably exogenous variation in insurance status among the previously uninsured.

Exploiting the arguably exogenous variation in insurance status, the first chapter examines the NHI effects on health care utilization and mortality of the previously uninsured elderly. It adopts a difference-in-differences (DD) strategy and uses a longitudinal survey of the elderly in Taiwan. In particular, the previously uninsured elderly are treated as the treatment group and the continuously insured elderly as the control group. The first chapter finds that NHI largely increased utilization of health care of the previously uninsured elderly relative to their continuously insured counterparts. Corresponding to the increase in utilization, the mortality hazard ratio of the previously uninsured to the continuously insured elderly dropped from 1.3 in the pre-NHI period to close to 1 in the post-NHI period, suggesting that NHI improved the health of the previously uninsured elderly through health care.

The second chapter is about a special medicine culture in Taiwan—physicians and pharmacists both prescribed and dispensed drugs. This culture had a long history dating back to its Japanese colonial period (1895-1945) and remained unchanged until the mid-1990s. In the eyes of many elderly who were accustomed to this culture, physicians and pharmacists were to some extent synonymous, i.e. close substitutes, at least in the context of minor illness, because they both prescribed and dispensed drugs.

However, physician and pharmacist services differed in two major aspects. In general, physician services were of higher quality because physicians were well trained to diagnose and treat medical conditions, while pharmacists were not. On the other hand, physician services were also more expensive because physicians typically charged not

only for dispensing drugs but also for diagnosing, prescribing and other related fees, while pharmacists charged only for dispensing drugs. The price difference could be substantial depending on medical conditions. For example, to treat a common cold, an uninsured patient would have to pay NT$ 300 (US$ 10) if visiting a physician but only NT$ 100 (US$ 3.3) if visiting a pharmacist. It is thus not surprising that the uninsured elderly were more likely to visit pharmacists at that time.

The special medicine culture started to change because of two health care reforms in the mid-1990s—NHI in 1995 and a separation policy (SP) in 1997 called *yi yiao feng ye* (in Chinese), meaning separation of drug prescribing and dispensing. In particular, NHI provided an economic incentive for the previously uninsured elderly to visit physicians by paying for their visits and drugs. The SP basically forbad physicians from dispensing and pharmacists from prescribing. Patients were forced to visit a doctor in order to get a prescription, which was required when they purchased drugs from a pharmacist.

The second chapter investigates the effects of NHI and SP on elderly patients' choice between physician and pharmacist services. It also adopts the DD method to study both policies. For NHI, again, the previously uninsured elderly were treated as the treatment group and the continuously insured elderly as the control group. For SP, because the policy was gradually phased in throughout the country, the elderly who lived in counties that had adopted SP were treated as the treatment group, and others who lived in counties that had not adopted as the control group.

This chapter finds that by providing an economic incentive to the previously uninsured elderly, NHI raised the probability that they would visit physicians, relative to

their continuously insured counterparts. In particular, some previously uninsured elderly who once only visited pharmacists were more likely to also visit physicians after NHI was implemented. Following this, the SP made it more likely that all elderly patients would only visit physicians and buy drugs from on-site pharmacists hired by physicians. The SP effect was influenced by two critical concessions that the government made to physicians—physicians were allowed to hire their own on-site pharmacists in their clinics and to dispense drugs to elderly aged 65 and older if they could not go to a community pharmacy to purchase drugs by themselves. These two concessions saved the elderly patients a separate trip to a community pharmacy by allowing them to buy drugs from the physician or the on-site pharmacist in the clinic.

The lessons learned from Taiwan's experience in the 1990s provide an insight for other countries—financial incentives and institutional constraints matter when it comes to designing health care policies.

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Coverage Matters: Impacts of National Health Insurance on Health Care Utilization and Mortality of the Previously Uninsured Elderly in Taiwan

1.1. Introduction

The effects of health insurance have long been an interest among researchers and policy makers. In particular, how health insurance affects people's utilization behavior and health outcomes is an important question when a government considers providing insurance coverage to the public. Since the famous RAND Health Insurance Experiment in the 1970s (Manning et al. 1987), a large body of studies investigating insurance effects has accumulated in the literature. Researchers, however, have not yet reached a unanimous conclusion of the insurance effects on health outcomes despite their agreement that insurance affects utilization.

There are numerous reasons why there is a lack of consensus among researchers on the insurance effects on health outcomes (Levy and Meltzer 2004). One of the major reasons is that causation between insurance status and health may run in both directions. For example, while insurance provides people access to medical care that might improve health, it is also true that—at least in the United States—insurance companies often screen the health of their applicants and relatively healthy applicants are more likely to be approved. This could create a positive association between insurance and health that does not reflect a causal effect of insurance.

To tackle this problem, more and more studies have exploited exogenous variation in insurance status generated by so-called "natural experiments", which usually refer to a policy or an institutional change that is essentially uncorrelated with the health

of its targeted subjects (Card et al. 2004; Chen et al. 2007; Cheng and Chiang 1997; Currie and Gruber 1996a, 1996b, 1997; Haas et al. 1993a, 1993b; Hanratty 1996; Lichtenberg 2002; Polsky et al. 2006; Wen et al. 2008). Typically, a natural experiment involves two groups: one group is subject to the policy and called the treatment group and another group is not subject to the policy and called the control group. If the policy-generated variation in insurance status among the treatment group is uncorrelated with the subjects' health, it is feasible to identify the insurance effect on health by comparing the difference in differences in outcomes between the two groups before and after the experiment under some regular assumptions. Following this line of research, this paper examines the effects of a rare large-scale "natural experiment" on health insurance in Taiwan in the mid-1990s.

On March 1, 1995, Taiwan initiated its National Health Insurance (NHI) program that mandated insurance coverage to its 21 million citizens, including 8 million who had been previously uninsured. The previously uninsured were required to enroll in NHI regardless of their health.[1] In addition, the strict mandate eliminated the possibility for the previously uninsured to opt out. These two facts together generated arguably exogenous variation in insurance status. On the other hand, others who had been previously insured by various social insurance programs were automatically transferred to NHI and enjoyed a similar range of medical benefits as before. Exploiting these features, this paper adopts a difference-in-differences (DD) method to examine the NHI effects on utilization behavior and mortality. Due to data availability, this paper focuses on the elderly.

[1] In fact, no matter when the previously uninsured actually signed up for NHI, their coverage automatically took effect on March 1, 1995.

Several studies such as Cheng and Chiang (1997), Chen et al. (2007), Wen et al. (2008), Zimmer et al. (2002) and Zimmer et al. (2005) have also investigated the effects of Taiwan's NHI on utilization and health. The majority consensus among these studies is that NHI increased utilization for the previously uninsured. Nevertheless, while some of them claim that NHI also improved the health of the previously uninsured, others disagree.

In particular, Chen et al. (2007) used the same survey of the elderly that is also used in this chapter.[2] They find that NHI largely increased utilization for the previously uninsured elderly but did not improve their one-year mortality relative to their continuously insured counterparts.[3] They attribute the lack of evidence on mortality to several possible reasons. For example, they argue that one-year mortality may not be a sensitive measure of health and that their sample period—from 1989 to 1999, which includes only covers 4 years after 1995—may not be long enough to detect any NHI effect. Other explanations they provide are related with effectiveness and quality of health care in producing health.

In our view, however, there are two other possibilities. First, they did not fully utilize the mortality information. They only used two pre-NHI (1989 and 1993) and two post-NHI yearly mortality rates (1996 and 1999), while mortality information from other years was simply ignored. In this study, we are able to use more recent and detailed monthly mortality data up to the end of 2003. Our study period thus covers 8 years after NHI was implemented. Moreover, instead of looking at limited discrete points, we utilize

[2] Chen et al. (2007) was published after this paper had begun in 2006.

[3] They also examined the NHI effect on self-reported health but did not find evidence of an effect on it, either.

every piece of monthly mortality information throughout our entire analysis period from 1993 to 2003.

Second, the demarcation between the previously uninsured and continuously insured elderly in Chen et al. (2007) was problematic. They imputed insurance status for the elderly in the pre-NHI period and assumed their status remained unchanged until 1995 when NHI was initiated.[4] However, we examine the pre-NHI insurance history of the elderly using both administrative data and a telephone follow-up in 1991 of the same elderly survey, which they did not use; we find that many of the elderly changed their insurance status in the pre-NHI period. In particular, of the elderly in the survey whose insurance status was available in both 1989 and 1993, about 26% of them changed their insurance status, either from uninsured to insured or vice versa.[5] This implies that their imputed insurance status very likely has led to wrong demarcation between the treatment and control group, which is essential in their difference-in-differences analysis. (See Section 2 and Appendix 2 for more details.)

Therefore, to get a more accurate demarcation, we use insurance status in 1993, which is the available insurance information closest to 1995, to reduce the possibility of changing insurance status before the implementation of NHI.

The major findings of this chapter show that NHI largely increased the utilization of the previously uninsured elderly relative to their continuously insured counterparts. Correspondingly, the mortality hazard ratio of the previously uninsured elderly to their continuously insured counterparts significantly dropped from 1.3 in the pre-NHI period to

[4] They imputed insurance status because no insurance information was available in the initial survey in 1989. Another wave of the survey in 1993 does have insurance information. However, only a subset of the initial sample appeared in 1993.

[5] There were 2,887 elderly whose insurance status in 1989 and 1993 were both available. Among them, 672 changed from uninsured to insured, 75 from insured to uninsured and the rest did not change.

close to one in the post-NHI period, suggesting that NHI did improve the health of the previously uninsured elderly through health care.

The rest of this chapter is organized as follows. The second section introduces the institutional background. The third section illustrates the empirical method. The fourth section describes the data. The fifth section reports the estimation results. The major findings are then concluded in the last section. Appendix 1 demonstrates how the insured and uninsured elderly in the pre-NHI period are defined. A detailed comparison between this paper and Chen et al. (2007) is provided in Appendix 2.

1.2. Institutional Background

Health Insurance Before 1995

At the end of 1994, about 2.3 million people in Taiwan (11% of the entire population) were aged 60 and older. Among them, about 75% were insured by four social insurance programs: Farmers Health Insurance (FHI), Government Employees Insurance (GEI), Labor Insurance (LI) and Veterans Insurance (VI).[6] As suggested by their names, FHI covered farmers; GEI covered government employees and school teachers; LI covered private sector workers; VI covered veterans. In 1994, FHI, GEI, LI and VI respectively accounted for 45%, 20%, 18% and 17% of the insured elderly.[7] Besides social insurance, there was virtually no comprehensive private insurance at the time.[8]

These programs share six common features. First, they were employment-based and generally covered only active workers except for GEI and VI, which also provided

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[6] Several other social insurance programs also existed at the time but only accounted for a very small portion of the elderly population.

[7] All these numbers are my calculations using administrative statistics from Bureau of Central Trust, Bureau of Labor Insurance, Bureau of National Health Insurance, Veteran Affairs Commission and Ministry of the Interior.

[8] More precisely, private insurance provided only very limited coverage and generally did not cover any outpatient care (Liu and Chen 2002; Cheng 2003).

coverage to retirees and dependents.[9] Second, they were mandatory and all eligible individuals were required to enroll in one of these programs. Third, they provided comprehensive medical benefits including outpatient, inpatient, emergency care and prescription drugs.[10] Fourth, they all adopted a single-payer fee-for-service (FFS) system.[11] Fifth, there was virtually no copayment for any service.[12] Sixth, the insurance premium was generally shared among government, employers (if applicable) and insurees.

Given the nature of the pre-NHI insurance market, the remaining 25% of the elderly was left uninsured. Many of them were women and not working. In general, the uninsured elderly had to pay out-of-pocket for their health care expenditures and utilized less health care than their insured counterparts (Cheng and Chiang 1997).

National Health Insurance

After a decade of planning, the government decided to consolidate all social insurance programs into a universal and compulsory National Health Insurance (NHI). The National Health Insurance Act (NHIA) was passed in the legislature on July 19, 1994 and took effect on March 1, 1995. Meanwhile, a new government agency called the Bureau of National Health Insurance was established to administer NHI.

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[9] Since FHI and LI were supposed to cover only active farmers and private sector workers, it seems puzzling at first glance that many elderly were insured by the two programs in 1994. This was actually a result of loose law enforcement. According to the law, farmers and workers could sign up FHI and LI by becoming members of some farmers' association or association of some occupation or union. By law, these associations were supposed to screen and track their members' work status. In practice, however, screening and tracking work status is difficult. Therefore, many elderly retained their insurance through this loophole even after they had retired.

[10] In addition to medical care, they also provide maternity, injury, disability, death, and retirement benefits.

[11] Each program had its own contracted provider network. According to Cheng and Chiang (1997), roughly 85% of hospitals and 70% of clinics in Taiwan were contracted with these social insurance programs in 1994. Care providers within each network were reimbursed solely by a government agency based on a fixed fee schedule. Moreover, insurees had the right to choose any provider within the network without any referral.

[12] Patients typically only paid a fixed "registration fee" NT$ 50 (US$ 1.7) for each outpatient visit.

NHIA required all of the previously uninsured to enroll in NHI and insurees of the various social insurance programs to be automatically transferred to NHI.[13] Due to its mandatory and universal nature, NHI extended its coverage quickly. By the end of 1996, about 93% of the population had enrolled in NHI. Among the elderly aged 60 and older, the coverage rate was near 100%.

In general, NHI provides a similar wide range of medical benefits as previous social insurance programs did.[14] The insurance premium is also shared among government, employers (if applicable) and insurees. For low-income people and veterans, their premium is entirely paid by the government.

One feature that distinguishes NHI from previous programs is its copayment scheme. Typically, one pays an amount of NT$ 100 to NT$ 200 (roughly US$ 3.3 to US$ 6.7) copayment for each outpatient visit. For a hospital stay, the coinsurance rate varies from 5% to 30% depending on the length of stay and type of room, and the total copayment is subject to an upper limit. Copayments are waived for low-income people, veterans, and people with some major illnesses such as cancer, end-stage renal disease, etc.

NHI also has its own contracted provider network. According to the Department of Health, about 98% of hospitals and 91% of clinics contracted with NHI in 1996. In its early stage, NHI adopted a FFS system to reimburse its contracted care providers. Unfortunately, this system was unable to contain medical care expenditures, and NHI faced its first budget deficit two years after its inception.

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[13] Active military personnel were not covered by NHI until 2001. These social insurance programs still exist today and continue to provide non-medical benefits.

[14] Except that NHI has more preventive care and home care benefits.

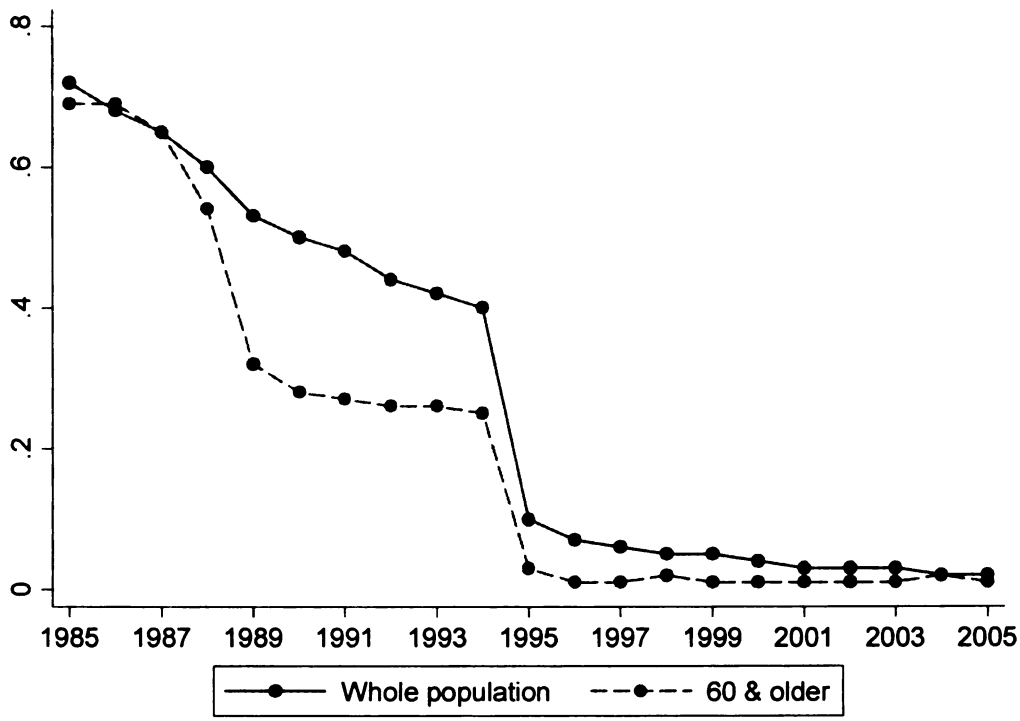
To tackle its growing budget woe, NHI adopted a series of measures. For example, it imposed a new copayment on prescription drugs in 1999 and raised the insurance premium by 7 percent in 2002. It also raised copayments for patients who have "excessive" doctor visits.[15] In addition, a Global Budget System (GBS) that uses a predetermined budget to control expenditures has been applied to all medical services since 1998 (Hsueh, Lee and Huang 2002; Lee and Jones 2004). Most recently, a Diagnosis-Related Group (DRG) system that reimburses hospital service based on classifications of cases that use similar hospital resources was launched in 2008.

Uninsurance Trend: 1985-2005

While it is well-known that uninsurance in Taiwan was mostly eliminated by NHI, it is relatively less noticed that the uninsurance rate actually had been declining fast as early as a decade before. Figure 1.1 shows the uninsurance rate in Taiwan from 1985 to 2005.[16] The solid line illustrates the uninsurance rate of the whole population and the dashed line is that of the elderly 60 and older. Both lines show a downward trend in the pre-NHI era (1985-1994). The downtrend in the pre-NHI era was mainly resulted from a series of expansions in the major social insurance programs. For example, GEI extended its coverage to private school staff in 1980, to dependents of government employees in 1982, to retired private school staff and their spouses in 1985 and to dependents of private school staff in 1990; FHI was initiated in 1985 and became a mandate for farmers

[15] For example, the copayment is raised by NT$ 50 starting on one's 49th visit within a year and by NT$100 starting on one's 157th visit (Liu and Chen 2002).

[16] Numbers are my own calculations using statistics from the following sources. Population statistics are from *Statistical Yearbook of Interior* published by the Ministry of Interior, which is available at http://www.moi.gov.tw/stat/english/year.asp. Numbers of GEI insurees are from *GEI Statistics* published by Bank of Taiwan (formerly by Bureau of Central Trust), which is available at http://www.bot.com.tw/GESSI/Statics/default.htm. Numbers of FHI and LI insurees are from *Annual Report* published by Bureau of Labor Insurance, which is available at http://www.bli.gov.tw/en/. Numbers of VI insurees are kindly provided by Veteran Affairs Commission (VAC). They are not available on-line but can be requested from the VAC.



Notes:the uninsured before 1995 refers to people who were not covered by GEI, LI FHI and VI; the uninsured starting in 1995 refers to people who were not enrolled in NHI.
Sources: Bureau of Central Trust; Bureau of Labor Insurance; Bureau of National Health Insurance; Veteran Affairs Commission; Ministry of Interior. See note 16 for more details.

Figure 1.1. Uninsurance Rate: 1985-2005

in 1989 (Peabody *et al.* 1997; Chiang 1997). The decline was particularly sharp for the elderly between 1987 and 1989 when FHI was quickly expanded. FHI had a particularly large impact on the elderly because farmers constituted a major component of the elderly population at the time. The downtrend of uninsurance in the pre-NHI period has an important implication—many elderly people changed their insurance status in the pre-NHI period. This suggests that we should use the insurance status right before the initiation of NHI in 1995 to demarcate between the previously uninsured and the continuously insured elderly.

1.3. Empirical Framework

Health Care Utilization

Our main identification strategy relies on the fact that all previously uninsured elderly were required to enroll in NHI regardless of their health. In practice, no matter when they actually enrolled, their coverage automatically took effect on March 1, 1995. This mandatory feature of NHI eliminated the possibility for them to opt out and generated exogenous variation in their insurance status before and after 1995.

Suppose we observe the average utilization outcomes of the previously uninsured elderly before and after NHI was implemented, we could have used the observed difference in outcomes to measure the NHI effect on utilization. However, this difference is likely to be confounded, especially when the two outcomes are far apart in time, by other factors such as aging, environmental change, medical technology improvement etc.[17] The challenge is thus how to filter out the confoundedness.

One natural choice is to use the difference in outcomes experienced by their continuously insured counterparts and adopt a difference-in-differences (DD) strategy under two standard assumptions. The first assumption is that the continuously insured elderly were not affected in terms of utilization outcomes by transferring from previous social insurance programs to NHI.[18] The second assumption is that on average both the previously uninsured and the continuously insured elderly would have experienced the

[17] Another concern is that the previously uninsured elderly may have anticipated the coming of NHI and delayed their pre-NHI utilization until 1995. This anticipatory effect could possibly lead to an overestimate of the NHI effect on utilization. However, the earliest time that the public in Taiwan learned that NHI would be implemented in March 1995 was in July 1994 after the *National Health Insurance Act* was passed in the legislature. In our later analysis, we compare utilization of the elderly in 1993, 1996, 1999 and 2003. Back in 1993, nobody knew if and when NHI will come. In addition, if the previously uninsured elderly really repressed their utilization in 1994, this pent-up demand was more likely to be satisfied in 1995 right after NHI was implemented than 1996 or later. Therefore, the anticipatory effect should be of less concern in our analysis.

[18] Or, loosely speaking, the NHI effect on the continuously insured elderly was so minimal that changes in their outcomes mainly reflect non-NHI factors.

same confoundedness in the absence of NHI.[19] By these DD assumptions, we aim to estimate the NHI effect on the previously uninsured elderly, or the treatment effect on the treated.[20] We discuss the validity of the first assumption in more detail below.

The continuously insured elderly could possibly be affected by NHI in two aspects. First, while under NHI they still enjoyed a similar wide range of medical benefits as before, they now had to pay copayments, which were virtually absent under the previous programs. Cheng and Chiang (1997) compare the utilizations of the continuously insured adults before and after 1995 and find that the new copayment scheme seemed to have only insignificant effect on curbing utilization. They suggest that the effect of copayment on utilization was likely offset by more contracted medical care providers under NHI and relatively inexpensive cost sharing.

Second, the inclusion of the previously uninsured into NHI may have caused an adverse effect on the continuously insured, if there was not sufficient medical care supply to serve the sudden rise in demand. Nevertheless, medical care supply actually outgrew the population in Taiwan from 1986 to 1995. Specifically, while the population increased by 10% during this period, the numbers of medical personnel, hospitals and clinics grew by 70%, 34% and 37% respectively.[21]

Under these assumptions, we use the following two DD models to estimate the NHI effect on utilization.

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[19] This assumption is generally not testable unless there was a group not subject to NHI. However, if such a group really existed, we would have used it as a control group to measure the common confoundedness.

[20] If we want to estimate the general treatment effect (the NHI effect) on the entire population using this DD approach, we would have to additionally assume that the treatment (NHI) would have the same effect on the control group (the continuously insured elderly), should they become treated.

[21] Numbers are calculated based on official statistics released by the Department of Health and available on its website http://www.doh.gov.tw/EN2006/index_EN.aspx. Part of this growth in supply was resulted from the government's planning in anticipation of NHI, and another part was from the response of the private medical care providers who foresaw the potential profits brought by NHI.

(1) $Y_{it} = \beta_0 + \beta_1 UI_i + \beta_2 YR_t + \beta_3 UI_i \times YR_t + X_i B + \varepsilon_{it}$,

where i indexes individuals; t indexes time; Y is a dummy variable indicating one's utilization of certain type of care in a given time period; UI is a dummy variable indicating the previously uninsured elderly; YR is a year dummy indicating the year in the post-NHI period; X is a row vector of pre-NHI controls including age, sex, education, ethnicity, marriage, residence region, living arrangement, employment history, income, self-reported health and health behavior; [22] B is a column vector of coefficients corresponding to the variables in X; ε is an unobserved error term. Equation (1) is a linear probability model and estimated by pooled OLS. [23]

Conditional on pre-NHI characteristics, β_1 measures the outcome difference between the previously uninsured and the continuously insured elderly in the pre-NHI period; β_2 picks up the common confoundedness; β_3 assesses the difference in outcome differences between the elderly groups before and after 1995, that is, the NHI effect.

To reduce the possible bias caused by non-random attrition, an issue will be discussed in more detail in the data section, we estimate equation (1) using three balanced two-year pooled panels with one year in the pre-NHI period and another in the post-NHI period—1993 versus 1996, 1993 versus 1999 and 1993 versus 2003. Every elderly subject is observed in both years of each pair. We pool in pairs and run separate

[22] Living arrangement is to capture the possible non-monetary resources provided by their adult children with whom they live; employment history is highly correlated with their pre-NHI insurance status as well as their lifestyle which is further correlated with their health and utilization. For example, both farmers and government employees tend to be insured in the pre-NHI period but they also tend to have different lifestyles. In particular, farmers are more likely to involve in strenuous work, while government employees tend to live a sedentary lifestyle.

[23] We also estimated a probit model. However, the estimated NHI effects are similar to the linear probability model.

regressions in order to retain as many observations as possible.[24] In addition, we use cluster-robust standard errors for statistical inferences because of the pooling.

In equation (1), the sample compositions of the three separate regressions are partially different. In other words, the NHI effects across years are estimated based on partially different samples. Alternatively, we estimate the following pooling model with the sample confined to the same group of elderly.

$$(2)\quad Y_{it} = \beta_0 + \beta_1 UI_i + \beta_2 YR96_t + \beta_3 YR99_t + \beta_4 YR03_t + \beta_5 UI_i \times YR96_t + \beta_6 UI_i \times YR99_t + \beta_7 UI_i \times YR03_t + X_i B + \varepsilon_{it},$$

where YR96, YR99 and YR03 are year dummies indicating 1996, 1999 and 2003 respectively; other notations are the same as before.

Equation (2) is estimated using a balanced panel pooled across all 4 years: 1993, 1996, 1999 and 2003. The three interaction terms measure the NHI effect in 1996, 1999 and 2003 relative to 1993 using the identical sample. Again, cluster-robust standard errors are used for inferences. Because we restrict the pooled sample to be balanced across all four years, the sample is much smaller, compared with the three pooled pairs used in equation (1), and constitutes probably only the healthiest elderly who were able to survive to 2003. However, this pooling allows us to compare the NHI effect in the short-term (say, in 1996) to that in the long-term (say, in 1999 and 2003) at least for the subgroup who survived to the end of 2003.

Moreover, we devise the following more general DD model to provide suggestive evidence of the validity of the first assumption of our DD strategy. We divide the continuously insured elderly into five previous social insurance programs.

[24] The more years we include in one regression, the more observations we lose in order to keep a balanced pooled panel.

$$(3)\quad Y_{it} = \beta_0 + \beta_1 UI_i + \beta_2 GEI_i + \beta_3 LI_i + \beta_4 VI_i + \beta_5 OI_i + \beta_6 YR_t + \beta_7 UI_i \times YR_t + \beta_8 GEI_i \times YR_t + \beta_9 LI_i \times YR_t + \beta_{10} VI_i \times YR_t + \beta_{11} OI_i \times YR_t + X_i B + \varepsilon_{it},$$

where *GEI*, *LI*, *VI*, and *OI* are dummy variables indicating the elderly who are previously covered by GEI, LI, VI and OI (meaning other social insurance programs) respectively; all other notations are the same as in equation (1); note that the reference group here is FHI, the largest insurance group in the pre-NHI period. Again, equation (3) is estimated by OLS using three separate two-year pooled panels.

If we maintain the common time trend assumption but relax the first assumption, β_8 in equation (3) actually measures the "NHI effect" for those who transferred from GEI to NHI relative to the "NHI effect" for those who transferred from FHI to NHI. Likewise, β_9, β_{10} and β_{11} measure the "NHI effect" for those who shifted from LI, VI and OI to NHI respectively. If the continuously insured elderly groups were really affected by NHI, it is likely that the "NHI effects" for these five different groups were also different because these programs were different from each other.[25] This suggests a test of the following null hypothesis.

Hypothesis: $\beta_8 = \beta_9 = \beta_{10} = \beta_{11} = 0$.

This null hypothesis states that either the continuously insured elderly were not affected by transferring from former insurance programs to NHI or they were affected to the same extent. However, the latter possibility seems less probable.

Mortality Hazard Ratio

In general, we follow the same identification strategy and assumptions as in the utilization analysis. However, in contrast to the linear DD model of utilization, we

[25] Cheng and Chiang (1998) point out the three major programs, GEI, LI and FHI, differ in their medical availability.

incorporate the DD design into an exponential hazard function in order to fully exploit the mortality data. Let us define the hazard function as a conditional instantaneous rate of dying at any given time point as follows (throughout this chapter, "hazard" always refers to mortality hazard).

$$(4)\quad h(t)=\lim_{\Delta t\to 0}\frac{\Pr(t\le T<t+\Delta t \mid T\ge t)}{\Delta t},$$

where h is the hazard function; T is survival time that takes on only non-negative values, t; Pr $(\cdot)$ is the probability that an individual dies at some point between t and $t+\Delta t$ conditional on having survived for t units of time.

Further, let us model the hazard function as an exponential function with a DD design as follows.

$$(5)\quad h(t;UI,X)=\exp(\beta_0+\beta_1 UI+\beta_2 POST(t)+\beta_3 UI\times POST(t)+XB),$$

where t is the number of months of survival since the beginning of our analysis period from 1993 to end of 2003; *POST* is an indicator for the post-NHI period which is therefore a function of t; other notations are the same as in equation (1). In the set of baseline controls, X, we additionally add age-squared and age-cubic. Based on this hazard function, an individual's survival time in months, t, is assumed to be exponentially distributed. We estimate the parameters of the model by the maximum likelihood method.

Based on the hazard function, the log-likelihood function can be constructed accordingly as $\ln L=\sum_{i=1}^{n}\{d_i \ln f_i(t_i)+(1-d_i)\ln S_i(t_i)\}$, where d_i is an indicator that equals on if the individual has died by the end of our analysis period; $f_i(t_i)$ is the density function of t_i and equals $h_i(t_i)\exp\left\{-\int_0^{t_i} h_i(u)du\right\}$; $S_i(t_i)$ is the survivor function

and equals $\exp\left\{-\int_0^{t_i} h_i(u)du\right\}$. Note that if an individual was still alive at the end of the analysis period, her t_i equals the length of the analysis period and her contribution to the log likelihood is $\ln S_i(t_i)$; otherwise, her contribution is $\ln f_i(t_i)$.

In equation (5), $\exp(\beta_0 + \beta_2 POST(t))$ represents the baseline hazard common to both the previously uninsured and continuously insured elderly. Note that the baseline hazard can change across periods because of the time-varying variable, *POST*. Equation (5) can be viewed as a *piecewise constant hazard model* because it allows the baseline hazard to vary across the pre- and post-NHI periods but remain constant within each period. Assuming a constant hazard in each period may not be sensible in the context of elderly. However, for our DD strategy, we only need to estimate the change in baseline hazard as a result of NHI. The exact shape of the baseline hazard function is not our main concern. To allow additional flexibility in the way the hazard changes with time, we also estimate a version of the model with individual year effects that are common to all individuals.

In equation (5), conditional on the pre-NHI controls, $\exp(\beta_1)$ measures the ratio of the hazard of the previously uninsured elderly to the hazard of the continuously insured elderly in the pre-NHI period, while $\exp(\beta_1 + \beta_3)$ assesses that hazard ratio in the post-NHI period, and $\exp(\beta_2)$ is the pre- versus post-NHI within-group confoundedness which is assumed to be common for both groups.

Alternatively, we can interpret the βs in terms of log hazards. By taking natural logarithm of equation (5), we turn the hazard function into a log-hazard function and β_1 is the difference in log-hazards in the pre-NHI period, β_2 is the common

confoundedness, and β_3 is the difference in differences in log hazards. However, hazard ratios are more interpretable than log hazards.

1.4. Data

A Longitudinal Elderly Survey

We use a longitudinal survey of the elderly called *the Survey of Health and Living Status of the Middle-aged and the Elderly in Taiwan* (referred to as *the survey* hereafter) administered by *the Bureau of Health Promotion* (BOH) in Taiwan (formerly known as *Institute of Family Planning*).[26] The survey contains rich information regarding demographics, health care utilization, health outcomes, health insurance, income, employment history etc. The survey drew a nationally representative elderly sample of 4,049 who were aged 60 and older in 1989. The elderly were then followed in 1991, 1993, 1996, 1999 and 2003.[27]

In our analysis of utilization, we only use the 1993, 1996, 1999 and 2003 surveys for the following two reasons. First, the 1989 survey contains no health insurance information and its utilization questions are different from those in latter surveys (see Appendix 1 for more details). Second, the 1991 survey lacks health utilization information. In addition, we drop the privately insured elderly and those with unknown insurance status in 1993.[28] In the end, our final sample consists of 785 uninsured and 2,351 insured elderly in 1993.

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[26] Although the middle-aged are mentioned in the title of the survey, we do not use the middle-aged because they were added into the survey in 1996 and we do not know their pre-NHI insurance status.

[27] All waves were conducted in person except for the 1991 survey, which was done by telephone. Every wave was generally administered from March to June; the completion rate for each wave is over 90%. In 1996, the survey added in a middle-aged sample of 55 to 66 years old. The middle-aged sample is not used in this study because it has no pre-NHI insurance information.

[28] There are 16 privately insured and one with unknown insurance status. See appendix 1 for more details.

Table 1.1. Elderly Insurance Status in 1993		
	n	%
Government Employee Insurance Programs (GEI)	483	15.40
Labor Insurance (LI)	164	5.23
Farmer's Health Insurance (FHI)	1,227	39.13
Veteran Insurance Programs (VI)	391	12.47
Other Insurance Programs (OI)	86	2.74
Uninsured	785	25.03
Total	3,136	100.00

Notes: GEI includes Government Employee Insurance, Government Retiree Insurance, Government Employee's Dependents Insurance and Government Retiree's Dependents Insurance. VI includes Veteran Insurance and Veteran's Dependents Insurance. OI includes Councilor Insurance, Welfare Insurance, Fishermen Insurance, Military Insurance, Military Personnel's Dependents Insurance.

As shown in Table 1.1, FHI, GEI and VI amount to 39%, 15% and 12% of the sample respectively, while LI accounts for only 5% of the sample.[29] The insurance structure reflects the agriculture-oriented economy for this generation as well as a massive influx of troops from mainland China to Taiwan around 1949 due to a civil war.

The survey is supplemented with three sources of mortality data. The survey itself collects information about death month and year of each deceased elderly up to the end of 2003. To confirm with administrative data, the survey uses national ID numbers of the elderly to link them to mortality records at the Department of Health (DOH) and Ministry of the Interior (MOI). However, for various reasons, about 6% of the DOH death records are inconsistent, either in death month or year, with the survey's own collection, and only a rather small number of deaths are successfully linked to MOI data.[30] Instead of dropping the mismatched cases, we choose to use both the survey's collection and the

[29] In other words, LI only accounts for 7% of the insured elderly in our sample in 1993. Earlier on in the institutional background section, we mentioned that LI, based on administrative statistics, accounted for about 18% of the insured elderly aged 60 and older in 1994. However, in our sample, the elderly were aged at least 64 and older in 1993. Elderly in our sample were older and very few of them were still working in the private sector in 1993. And, recall that LI covered the private sector workers.

[30] According to the Bureau of Health Promotion, some national ID numbers provided by the elderly are wrong and some others' ID numbers are not even available. However, we are not told about how many cases have this problem.

DOH data to estimate the hazard model and present both sets of results. From the beginning of 1993 to the end of 2003, 1,367 and 1,389 out of the total 3,136 elderly had died according to the survey's collection and the DOH data, respectively.

Characteristics of the Elderly in the Pre-NHI Period

Table 1.2 summarizes the characteristics of the uninsured and the insured elderly in 1993. Compared with their insured counterparts, the uninsured elderly are more likely to be women, unmarried or widows, Minnan people, and live with their children in an urban area in the north region of Taiwan. They also tend to be less educated, less likely to be working and receive less monthly income. Meanwhile, they are more likely to live with their adult children.

As expected, their insurance status is correlated with their current or past occupation as the insured elderly are much more likely to have ever worked as farmers, government employees and military personnel than the uninsured elderly. In terms of health outcomes, the uninsured elderly are less healthy and more likely to die than the insured elderly. Surprisingly, the uninsured elderly also live slightly healthier in terms of smoking and betel nut chewing.[31]

Utilization Variables

To measure their utilization behavior, we use information about whether they have ever foregone doctor visits due to lack of money in a 3-month period, their outpatient visits in a month, hospitalizations and emergency room (ER) visits in a year. See Appendix 1 for original survey questions in Chinese and their English translations.

Table 1.3 summarizes these variables across years. Column (1) and (2) in the first panel show that the probability of forgoing doctor visits due to lack of money drops by 4

[31] Betel-nut chewing is found to be associated with mouth cancer by the medical profession.

Table 1.2. Characteristics of the Elderly in 1993		
	Insured	Uninsured
Age (mean)	71	72
Female (%)	39	57
Married with spouse present (%)	67	47
Ethnicity (%)		
Minnan	56	74
Hakka	17	10
Mainlander	24	14
Aboriginal	2	1
Region (%)		
East	9	6
North	23	38
Central	36	29
South	32	26
Urban area (%)	59	77
Live with adult children (%)	64	70
Years of education (%)		
0 Year	45	60
1-6 Years	33	31
7 Years and more	22	9
Currently employed/ self-employed (%)	23	11
Monthly income > NT$10,000 (%)	45	31
Ever worked as (%)		
Farmer	51	26
Government employee	15	5
Military personnel	21	6
Self-reported health (%)		
Very good/ good	41	36
Fair	33	31
Bad/ very bad	21	24
No response	5	9
Functional limitation (ADL) (mean)	0.38	0.54
Have one or more chronic condition(s) (%)	75	74
One Year Mortality (%)	3.3	4.8
Health behavior (%)		
Smoking	29	27
Drinking	13	13
Betel nut chewing	6	4
Total	2,351	785

Notes: ADL is an average score of 12 functional limitation items of daily activities. Each item ranges from 0 (no difficulty) to 3 (cannot do it at all). Chronic conditions include arthritis, asthma, cancer, cataract, diabetes, hypertension, heart problem, kidney problem, liver problem, stroke, and ulcer. Mortality is based on survey's collection.

Table 1.3. Summary of Utilization Variables

	Probability				Average Positive Visits			
	(1)		(2)		(3)		(4)	
	Insured		Uninsured		Insured		Uninsured	
Forego Visits								
1993	0.01	(2,351)	0.06	(785)				
1996	0.01	(1,928)	0.02	(584)				
1999	0.01	(1,649)	0.03	(503)				
2003	0.01	(1,249)	0.02	(362)				
Outpatient Visits								
1993	0.50	(2,347)	0.34	(784)	2.9	(1,153)	2.8	(261)
1996	0.63	(1,926)	0.60	(583)	2.6	(1,052)	2.6	(286)
1999	0.71	(1,648)	0.63	(503)	3.1	(1,154)	3.0	(312)
2003	0.74	(1,248)	0.70	(361)	2.4	(923)	2.3	(252)
Hospitalizations								
1993	0.20	(2,351)	0.10	(785)	1.4	(463)	1.2	(79)
1996	0.22	(1,928)	0.20	(584)	1.6	(419)	1.3	(114)
1999	0.24	(1,649)	0.23	(502)	1.6	(391)	1.6	(116)
2003	0.20	(1,249)	0.24	(362)	1.8	(254)	1.4	(86)
ER Visits								
1993	0.11	(2,351)	0.07	(783)	1.4	(240)	1.2	(52)
1996	0.13	(1,928)	0.11	(584)	1.9	(254)	1.9	(64)
1999	0.15	(1,637)	0.15	(502)	1.8	(245)	1.7	(76)
2003	0.18	(1,249)	0.17	(362)	1.7	(225)	1.7	(62)

Notes: observations in parentheses; majority of non-responses (over 70%) is due to death; percentages of non-responses due to death are roughly equal between insured and uninsured; about 9% of the insured and 11% of the uninsured respondents in 1996 fail to recall their exact number of outpatient visits; for other years and visits, percentages of failed recalls are less than 2%.

percentage points for the previously uninsured elderly from 1993 to 1996 and remains at roughly 2% afterwards, whereas it remains stable at 1% for the continuously insured elderly throughout this period. This suggests that NHI seems to have removed the financial barrier to visiting doctors for some of the previously uninsured elderly.

In the following three panels, we observe a similar pattern across outpatient visits, hospitalizations and ER visits. In 1993, the then-uninsured elderly have a much lower probability of utilization than the insured elderly. However, in 1996, the probability of utilization for the previously uninsured elderly rises dramatically. For example, the probability of outpatient visits increases by 26 percentage points, the probability of

hospitalizations by 10 percentage points and ER visits by 4 percentage points for the previously uninsured elderly.

On the other hand, the changes for the continuously insured elderly are not as dramatic as for the previously uninsured elderly. Furthermore, the increase in 1996 brings the previously uninsured elderly to par with their continuously insured counterparts afterwards.

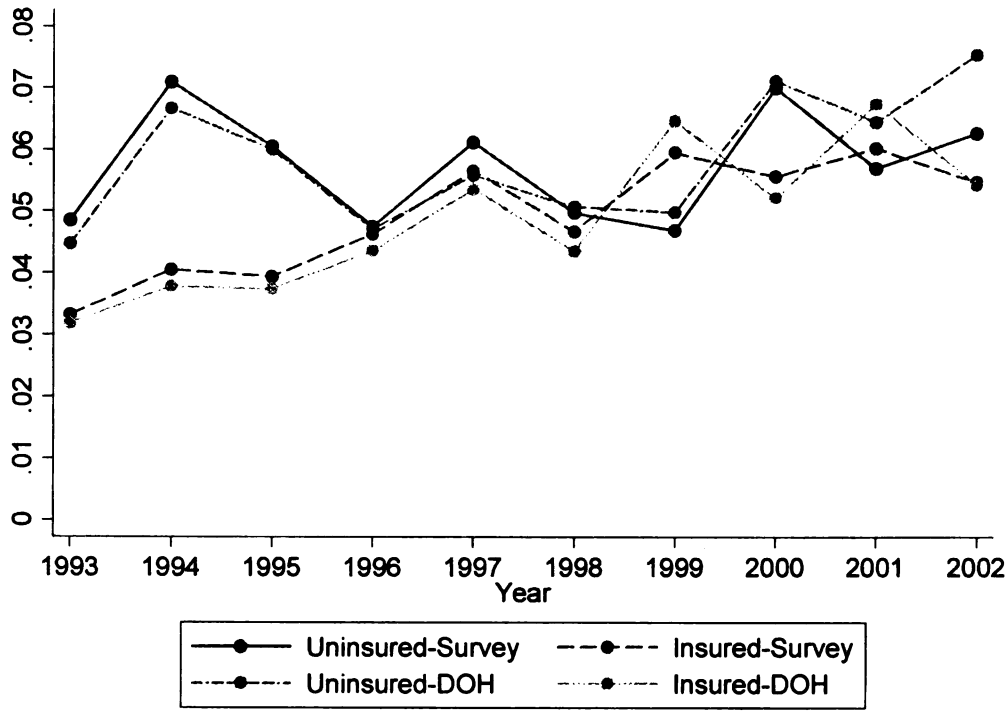
In contrast to probability of a visit, the two elderly groups have very similar numbers of positive visits conditional on having at least one visit across years (column (3) and (4)), suggesting that NHI affects the extensive margin but not the intensive margin of utilization.

Sample Attrition Problem

Table 1.3 obviously shows that our sample depletes across years. The sample attrition is mostly a result of death and could possibly bias our DD estimation of the NHI effect on utilization if the attrition is not random. For example, if the uninsured elderly tend to die faster than the insured and the deceased elderly are more likely to be high users than their living counterparts, then the DD method with an unbalanced panel tends to underestimate the NHI effect on utilization. We verify this by comparing utilization in 1993 of those who only appeared in the 1993 survey with that of those who appeared in 1993 and later surveys. We find that the former actually had higher utilization in 1993 than the latter. To reduce this possible attrition bias, we use balanced panels in all our regressions.

Yearly Mortality Rate

To get a general picture of the crude mortality, we use the yearly mortality rate defined as deaths occurred during a 12-month period divided by those alive in the beginning of that period. To correspond to the implementation date of NHI, each period begins in March and ends in February. Figure 1.2 plots the yearly mortality rates for the two elderly groups from March 1993 to February 2003 using the survey's collection of death records and the DOH mortality data.



Notes: every year period starts in March and ends in February.

Figure 1.2. Yearly Mortality Rate by Insurance Group

As shown, whereas the yearly mortality rate of the continuously insured elderly rises steadily from 3% in 1993 to 4.5% in 1996, that of the previously uninsured elderly first rises from 5% in 1993 to 7% in 1994 and then quickly declines to 6% in 1995 and to 4.7% in 1996. After 1996, the two groups basically merge and move together until 1999 when they start to oscillate up and down again. However, the mortality gaps between the

two groups after 1999 are never as big as in the pre-NHI period. At first glance, the pattern in yearly mortality rate seems to be associated with the pattern in utilization.

1.5. Estimation Results

NHI Effect on Utilization

Estimation results of NHI effects on probabilities are reported in Table 1.4.[32] Note that model 1 is the model stated in equation (1) using three separate panels pooled across only a pair of years and model 2 is the pooling model stated in equation (2) using a panel pooled across all four years. In general, the results show that NHI removes the financial barrier and largely increases utilization for the previously uninsured elderly relative to their continuously insured counterparts. More precisely, in panel A (1993 versus 1996), model 1 results show that NHI decreases the probability of foregoing doctor visits by 4 percentage points, increases the probability of outpatient visits by 13 percentage points and the probability of hospitalizations by 9 percentage points for the previously uninsured elderly relative to their continuously insured counterparts; and these results are statistically significant. In contrast, we only find an insignificant 1.7-percentage-point change in their probability of ER visits.

Model 1 results in panel B (1993 versus 1999) and C (1993 versus 2003) are basically similar to panel A except that there is a significant 5 percentage-point increase and an insignificant 3.4 percentage-point increase in the probability of ER visits in the panel B and C respectively. This similarity across waves suggests that the NHI effect on utilization was realized soon after the NHI was implemented.

[32] We also estimate NHI effect on numbers of visits conditional on having at least one visit. However, we do not find any significant evidence and the results are thus not reported.

Table 1.4. NHI Effect on Probability of Utilization

Forego Visits		*Outpatient Visits*		*Hospitalizations*		*ER Visits*	
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
			A: 1993 versus 1996				
-0.041	-0.043	0.131	0.143	0.093	0.042	0.017	0.000
(-3.66)	(-2.79)	(4.51)	(3.68)	(4.18)	(1.62)	(0.9)	(0.02)
[2,512]	[1,469]	[2,504]	[1,463]	[2,512]	[1,469]	[2,510]	[1,458]
			B: 1993 versus 1999				
-0.032	-0.038	0.09	0.09	0.095	0.062	0.049	0.036
(-2.74)	(-2.58)	(2.88)	(2.37)	(3.75)	(2.15)	(2.27)	(1.48)
[2,152]	[1,469]	[2,148]	[1,463]	[2,151]	[1,469]	[2,137]	[1,458]
			C: 1993 versus 2003				
-0.046	-0.043	0.117	0.11	0.126	0.139	0.034	0.045
(-3.32)	(-2.95)	(3.21)	(2.88)	(4.20)	(1.62)	(1.33)	(1.64)
[1,611]	[1,469]	[1,607]	[1,463]	[1,611]	[1,469]	[1,609]	[1,458]

Notes: dependent variables are dummy variables indicating each type of utilization; only the interaction terms are reported; all pooled panels are restricted to balanced panels; model 1 only pools a pair of years; model 2 pools all 4 years; cluster-robust *t* in parentheses; subjects in brackets; all regressions include pre-NHI demographic, economic and health controls.

Model 2 results are similar to model 1 results with the largest differences in the cases of hospitalizations and ER visits. In particular, NHI effects on hospitalizations in 1996 and 1999 estimated by model 2 are smaller than those estimated by model 1. Recall that model 2 uses only the elderly who appeared in all four years, while model 1 only requires the elderly to appear in two years. More specifically, some of those who appeared, for example, in the 1993 versus 1996 sample died soon after 1996, and these deceased elderly were supposedly the most fragile ones who most likely would have been hospitalized in 1999 were they still alive. However, these deceased elderly are excluded from the model samples. Therefore, this finding suggests that differences in NHI effects are likely resulted by attrition due to death.

Subgroup Analysis of Utilization

Since the previously uninsured elderly and their continuously insured counterparts were different in many aspects in their baseline characteristics, e.g. age, gender, ethnicity, health, etc., it is interesting to see if the NHI effects also differ among subgroups. Especially, one might expect that the sick elderly, e.g. elderly with chronic conditions, would have benefited more from NHI than healthy elderly.

Panel A in Table 1.5 shows that the relatively younger uninsured elderly (aged 64-73 in 1993) benefit from NHI more than the very old uninsured elderly (aged 74 and older in 1993) in terms of foregoing doctor visits and outpatient visits, but the very old uninsured benefit more in hospitalizations and ER visits. In panel B, uninsured women tend to benefit more than uninsured men except for outpatients visits. In panel C, uninsured non-Minnan people seem to benefit more than uninsured Minnan people.

As for chronic conditions, in our sample, diabetes (10%), hypertension (30%) and heart diseases (21%) are the three most prevalent chronic conditions among the elderly. These three conditions typically require constant medical attention and medications. We thus compare people with these conditions to those with no chronic conditions. The results suggest that (in panel D) uninsured elderly with these chronic conditions experienced larger increases in utilization than those without chronic conditions.

NHI Effect on Mortality Hazard

In Table 1.6, the top panel reports the original estimates of coefficients in equation (5), while the bottom panel reports pre- and post-NHI hazard ratios based on the estimated coefficients. Columns (1) to (3) use the survey's collection and column (4) to (6) use the DOH mortality data. Column (1) and (4) do not control for pre-NHI characteristics; column (2) and (5) control for pre-NHI demographic and economic

Table 1.5. NHI Effect on Probability of Utilization by Subgroups

Forego Visits		*Outpatient Visits*		*Hospitalizations*		*ER Visits*	
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			A: By Age in 1993				
64 to 73	74+	64 to 73	74+	64 to 73	74+	64 to 73	74+
-0.052	-0.018	0.159	0.074	0.066	0.152	-0.012	0.068
(-3.72)	(-0.94)	(4.54)	(1.39)	(2.54)	(3.45)	(-0.65)	(1.68)
[1,838]	[674]	[1,832]	[672]	[1,838]	[674]	[1,836]	[674]
			B: By Sex				
Female	Male	Female	Male	Female	Male	Female	Male
-0.057	-0.021	0.119	0.146	0.099	0.089	0.034	-0.000
(-3.43)	(-1.47)	(2.84)	(3.57)	(3.21)	(2.71)	(1.28)	(-0.02)
[1,109]	[1,403]	[1,106]	[1,398]	[1,109]	[1,403]	[1,107]	[1,403]
			C: By Ethnicity				
Minnan	Non-Minan	Minnan	Non-Minan	Minnan	Non-Minan	Minnan	Non-Minan
-0.041	-0.042	0.121	0.142	0.085	0.106	0.019	-0.002
(-2.95)	(-2.48)	(3.44)	(2.66)	(3.17)	(2.46)	(0.85)	(-0.05)
[1,540]	[972]	[1,534]	[970]	[1,540]	[972]	[1,539]	[971]
			D: By Chronic Conditions				
Diabetes	Heart Disease	Diabetes	Heart Disease	Diabetes	Heart Disease	Diabetes	Heart Disease
-0.023	-0.068	0.210	0.074	0.086	0.2	-0.043	0.11
(-0.85)	(-2.49)	(2.33)	(1.11)	(1.01)	(3.53)	(-0.69)	(2.26)
[236]	[503]	[235]	[502]	[236]	[503]	[235]	[520]
Hypertension	No	Hypertension	No	Hypertension	No	Hypertension	No
-0.060	-0.022	0.106	0.092	0.111	0.06	-0.013	-0.022
(-3.14)	(-1.36)	(1.93)	(1.7)	(2.28)	(1.68)	(-0.32)	(-0.83)
[754]	[338]	[751]	[337]	[754]	[338]	[753]	[338]

Notes: cluster-robust *t* statistics in parentheses; subjects in brackets; results are from model 1 using 1993 versus 1996 pooled panel; all regressions include pre-NHI controls except for the variable of interest in each subgroup comparison.

Table 1.6. NHI Effect on Mortality Hazard

Data Source	Survey			DOH		
	(1)	(2)	(3)	(4)	(5)	(6)
UI	0.449	0.311	0.246	0.447	0.321	0.273
	(3.48)	(2.34)	(1.84)	(3.37)	(2.36)	(1.99)
POST	0.307	0.438	0.55	0.381	0.492	0.576
	(3.71)	(5.26)	(6.56)	(4.49)	(5.79)	(6.75)
UI×POST	-0.334	-0.318	-0.268	-0.293	-0.282	-0.248
	(-2.29)	(-2.17)	(-1.83)	(-1.97)	(-1.89)	(-1.66)
Pre-NHI controls:						
Demographic & Economic	No	Yes	Yes	No	Yes	Yes
Health	No	No	Yes	No	No	Yes
Subjects	3,136	3,136	3,136	3,136	3,136	3,136
Deaths	1,367	1,367	1,367	1,389	1,389	1,389
Hazard Ratio:						
Pre-NHI	1.57	1.36	1.28	1.56	1.38	1.31
Post-NHI	1.12	0.99	0.98	1.17	1.04	1.03

Notes: *t* statistics are in parentheses; POST is an indicator for period after March 1995 when NHI was implemented; hazard ratio is calculated as exp(UI) in the pre-NHI period and exp(UI+UI×POST) in the post-NHI period.

variables; column (3) and (6) further control for pre-NHI health variables. The more controls we add in, the more conservative the estimates become.

Our main interest lies in the interaction term *UI×POST*, which represents the difference in differences in log hazards between the two groups across the pre and post-NHI periods.

The results show that the DD in log hazards range from -0.334 to -0.248, which implies the log hazard of the previously uninsured elderly decreased in the post-NHI period relative to their continuously insured counterparts. The more controls we add, the smaller the absolute values of the DD estimates. However, they are all significant at least at the 10% level.

To get more comprehensible interpretations, it is helpful to look at the hazard ratios at the bottom panel in Table 1.6. In general, pre-NHI hazard ratios are all well

above one and post-NHI hazard ratios are close to one. More precisely, the hazard ratio is about 1.3 in the pre-NHI period, indicating the risk of dying of the then-uninsured elderly is about 30% higher than that of the insured elderly. It then drops to one in the post-NHI period, implying the risk of dying for the two elderly groups merge together after NHI has been implemented.

Instead of dichotomously cutting time into the pre and post period, we modify equation (5) by estimating two alternative models. Model 1 includes a full set of year dummies but remains one interaction term of $UI \times POST$. Model 2 include a full set of year dummies and interaction terms of year dummies and UI.[33] In Table 1.7, the interaction terms of $UI \times POST$ in model 1 are similar to the results in Table 1.6. On the other hand, the interaction terms in model 2 suggest that the NHI effect on mortality gradually increased from 1996 and reached its peak in 1999 and then decreased towards the end.

Subgroup Analysis of Mortality Hazard

Our subgroup analysis results suggest that NHI effect on mortality hazard differs across subgroups. As shown in the top panel in Table 1.8, NHI seemed to have larger effect on the relatively younger uninsured elderly, women and non-Minnan people, compared to the very old uninsured elderly, men and Minnan people, respectively. In the bottom panel, NHI seemed to have a much larger effect on the uninsured elderly with diabetes or hypertension than those without chronic conditions. However, the NHI effect on elderly with heart diseases seemed minimal, although we previously find that NHI increased their utilization.

[33] All regressions still include a full set of pre-NHI controls.

Table 1.7. Alternative Mortality Hazard Models

	Survey		DOH	
	(1)	(2)	(3)	(4)
	Model 1	Model 2	Model 1	Model 2
UI	0.24	0.24	0.27	0.27
	(1.8)	(1.8)	(1.96)	(1.96)
UI×POST	-0.26		-0.24	
	(-1.77)		(-1.61)	
UI×1995		0.06		0.08
		(0.26)		(0.33)
UI×1996		-0.29		-0.25
		(-1.17)		(-1.00)
UI×1997		-0.33		-0.39
		(-1.46)		(-1.67)
UI×1998		-0.31		-0.27
		(-1.24)		(-1.06)
UI×1999		-0.58		-0.60
		(-2.33)		(-2.42)
UI×2000		-0.13		-0.12
		(-0.55)		(-0.50)
UI×2001		-0.42		-0.39
		(-1.64)		(-1.64)
UI×2002		-0.31		-0.21
		(-1.21)		(-0.83)
UI×2003		0.00		0.04
		(0.00)		(0.16)
Subjects	3,136	3,136	3,136	3,136
Deaths	1,367	1,367	1,389	1,389

Notes: all regressions include a full set of year dummies; model 1 includes one interaction term of UI and POST, which indicates the entire post-NHI period; model 2 includes a full set of interaction terms of UI and year dummies; a full set of pre-NHI controls are included; estimates of year dummies are not reported.

Table 1.8. NHI Effect on Mortality Hazard by Subgroups

	A: By Age in 1993		B: By Sex		C: By Ethnicity	
	64 to 73	74+	Female	Male	Minnan	Non-Minnan
UI	0.334	0.215	0.422	0.027	0.104	0.591
	(1.62)	(1.23)	(2.16)	(0.14)	(0.63)	(2.55)
POST	0.621	0.494	0.597	0.537	0.571	0.57
	(5.02)	(4.31)	(4.14)	(5.2)	(5.22)	(4.35)
UI×POST	-0.422	-0.169	-0.5	-0.008	-0.191	-0.587
	(-1.88)	(-0.87)	(-2.32)	(-0.04)	(-1.07)	(-2.21)
Hazard Ratio:						
Pre-NHI	1.4	1.24	1.52	1.03	1.11	1.81
Post-NHI	0.92	1.05	0.92	1.02	0.92	1
Subjects	2,183	953	1,365	1,771	1,901	1,235
Deaths	710	657	542	825	874	493

	D: By Chronic conditions			
	Diabetes	Hypertension	Heart Diseases	No chronic condition
UI	0.438	0.602	-0.18	0.418
	(1.43)	(2.51)	(-0.67)	(1.26)
POST	0.603	0.904	0.226	0.757
	(2.86)	(5.6)	(1.48)	(3.46)
UI×POST	-0.599	-0.734	0.165	-0.334
	(-1.7)	(-2.8)	(0.56)	(-0.95)
Hazard Ratio:				
Pre-NHI	1.55	1.83	0.836	1.52
Post-NHI	0.85	0.87	0.986	1.09
Subjects	328	946	669	790
Deaths	197	435	329	276

Notes: *t* statistics in parentheses; all regressions include pre-NHI controls except for the variable of interest in each subgroup comparison. Use survey data. Results using DOH data are similar but not reported.

Suggestive Evidence of the Validity of the First DD Assumption

Panel A in Table 1.9 reports *F* statistics and *p*-values to test the null hypothesis: $\beta_8 = \beta_9 = \beta_{10} = \beta_{11} = 0$ in equation (2). In general, the results do not reject the null hypothesis except for two cases.[34] This suggests that in general NHI had minimal impact on utilization for the continuously insured elderly.

[34] Outpatient visits in the case of 1993 versus 1999 and 1993 versus 2003.

Table 1.9. Validity Test of The First DD Assumption

A: Utilization			
Forego	*Outpatient*	*Hospitalization*	*ER*
(1)	(2)	(3)	(4)
1993 versus 1996			
1.16	1.2	0.96	1.7
(0.33)	(0.31)	(0.43)	(0.15)
1993 versus 1999			
1.4	2.51	1.01	0.52
(0.23)	(0.05)	(0.4)	(0.72)
1993 versus 2003			
1.77	3.13	1.06	0.63
(0.13)	(0.01)	(0.37)	(0.64)

B: Mortality Hazard	
Survey	DOH
1.16	0.88
(0.89)	(0.93)

Notes: *F* statistics and *p*-values (in parentheses) are reported; FHI is the reference group; all regressions include pre-NHI demographic, economic and health controls.

We also modify equation (4) by including 4 insurance dummy variables indicating GEI, LI, VI and OL and interaction terms of these dummies with the *POST* dummy. Then we test the same hypothesis. As show in panel B, we do not reject the null hypothesis, which implies that our first DD assumption is also valid for the mortality hazard model.

1.6. Conclusion

One common argument of providing universal insurance coverage to people is that insurance will improve people's health by giving them access to medical care and increasing their utilization. This paper finds that Taiwan's NHI removed the financial barrier for the previously uninsured elderly and largely increased their utilization relative to the continuously insured elderly. Corresponding to the increase in utilization, the mortality hazard ratio of the previously uninsured elderly to the continuously insured elderly dropped from 1.3 in the pre-NHI period to close to one in the post-NHI period.

Our findings suggest that NHI improved the health of the previously uninsured elderly through health care.

Our subgroup analyses provide a closer look at the NHI effects. We find that the NHI effects seem to differ across age, sex, ethnicity and health groups. While we find that the elderly with a larger increase in medical care utilization—due to NHI—generally also seemed to experience a larger decline in their mortality hazard, this is not always the case. For example, we find that NHI seemed to increase the utilization for the elderly with heart diseases but this increase in utilization did not seem to translate into a lower mortality hazard. This suggests that whether and how much insurance can improve people's health may still depend on the need and effectiveness of medical care.

While several previous studies had investigated Taiwan's NHI effects, the major contribution of this chapter is the finding of the NHI effect on mortality, which is different from the conclusion of Chen et al. (2007). As we mentioned in various places in the main text and Appendix 2, the difference between our findings can be attributed to several reasons. One of the main possible reasons is that Chen et al. did not fully utilize the mortality data. In addition, as detailed in Appendix 2, we find different mortality rates in 1999 than theirs.

Chapter 2

The Healer or the Druggist: Effects of Two Health Care Policies in Taiwan on Elderly Patients' Choice between Physician and Pharmacist Services

2.1. Introduction

Until the mid-1990s, both physicians and pharmacists prescribed and dispensed drugs in Taiwan, a nation with a special medicine culture that dates back to the Japanese colonial period of 1895–1945 (Unschuld 1976; Peabody et al. 1995; Hsieh 1999, 2003; Chou et al. 2003; Huang, Lee, and Huang 2004).[35] In the eyes of many elderly people accustomed to this culture, physicians and pharmacists were to some extent synonymous (i.e., close substitutes), generally in the context of minor medical conditions, in that they both prescribed and dispensed drugs.[36] This chapter studies two Taiwanese health care policies mandated in the 1990s that affected elderly people's choice between physicians and pharmacists: National Health Insurance (NHI), initiated in 1995, which provided insurance coverage for all citizens, and a separation policy (SP), gradually phased in since 1997, which forbad physicians from dispensing and pharmacists from prescribing drugs.

Prior to the mid-1990s, many elderly people in Taiwan, especially the uninsured, directly sought care at pharmacies because it was usually less expensive than seeing physicians. Pharmacists generally charged only for dispensing drugs, while physicians charged not only for dispensing drugs but also for diagnosing, prescribing, and other

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[35] Although not *legally* allowed to prescribe in the colonial period, pharmacists often provided free medical consulting and "recommended" drugs to patients (Unschuld 1976). The line between "prescribing" and "recommending" was usually vague. After the Chinese Nationalist Party took over Taiwan in 1945, pharmacists were legally allowed to prescribe drugs for numerous minor medical conditions based on two official prescription books.

[36] It is doubtful that pharmacists treated major illnesses such as cancer or broken limbs.

related services. The price difference could be substantial, depending on medical conditions. For example, to treat a common cold, an uninsured patient would have to pay NT$300 (US$10) out of pocket if visiting a physician but only NT$100 (US$3.3) out of pocket if visiting a pharmacist.[37]

Not only was their price different, but the quality of physician and pharmacist services was different. While physicians were well trained to diagnose and treat medical conditions, pharmacists were not. In the postwar period (after 1945), due to scarce medical resources and loose law enforcement, many pharmacists were neither well trained nor licensed (Hsieh 2003). Therefore, elderly patients who visited pharmacists could have had received better diagnoses and care from physicians.

In 1995 Taiwan initiated its NHI program, which provided compulsory coverage to the nation's 21 million citizens, including 8 million who were previously uninsured. The implementation of NHI lowered an economic barrier for the previously uninsured elderly to visit physicians because NHI paid for their physician visits and prescription drugs.

In 1997 a policy called *yi yiao fen ye*, meaning the separation of drug prescribing and dispensing, was launched in the two biggest cities and gradually phased into all other cities and counties over six years. The combination of drug prescribing and dispensing is believed to provide an economic incentive for care providers to prescribe drugs more than medically necessary and is associated with high drug expenditure. Meant to abate drug expenditures, the SP prohibits physicians from dispensing and pharmacists from prescribing drugs (Chou et al. 2003). In practice, pharmacists are not allowed to sell

[37] Estimates based on personal communications with several clinic physicians and pharmacists who have been practicing for at least 15 years.

drugs to patients without a physician's prescription. Under this legal constraint, patients are forced to first visit physicians in order to get a prescription.

However, the government made an exception in the case of physicians dispensing drugs to patients aged 65 and older. Also, physicians at clinics are allowed to hire their own on-site pharmacists.[38] These exceptions have had critical effects on patients' choice on where to purchase drugs after receiving a prescription from a doctor.

To avoid confusion, throughout this article the term *pharmacists* refers to those who practice at community pharmacies, while those who are hired by physicians and practice at clinics are specifically referred to as *on-site pharmacists.*

NHI and the SP are two major health care reforms that affected elderly people's choice between physicians and pharmacists in different ways. NHI eliminated the difference in out-of-pocket expenses between visiting physicians and pharmacists for the previously uninsured elderly. The SP forced all elderly patients to first visit physicians in order to get prescriptions. This paper empirically studies the effects of these two policies on elderly people's choice between physicians and pharmacists.

These two polices can be seen as natural experiments because their implementation was not contingent on the health or health care utilization of the elderly. Exploiting this feature, we adopt a difference-in-differences (DD) method to identify their effects on elderly outpatient and pharmacy visits. We also examine the policies' effects on self-reported health.

The first finding of this paper is that NHI induced more previously uninsured elderly patients to visit physicians, including those who were accustomed to only visiting

[38] According to the data presented by Chou et al. (2003), about 64 percent of clinics have hired on-site pharmacists in the post-SP period.

pharmacists. Moreover, NHI raised overall health care utilization for the previously uninsured elderly, which in turn is associated with an improvement in their self-reported health.

On the other hand, the SP did not increase overall utilization but only redirected elderly patients from pharmacists to physicians. More precisely, the probability of pharmacy visits was reduced by almost the same amount as the increase in the probability of only outpatient visits, which means that once elderly patients visited physicians, they did not come back to buy drugs at pharmacies. A probable reason for this result is that the government allowed clinic physicians to hire on-site pharmacists who dispensed drugs to the elderly.

Moreover, the SP's effect on self-reported health is trivial and not statistically significant, which suggests that before the SP was initiated, patients used to substitute pharmacist services for physician services only for minor medical conditions for which the difference in care quality did not matter much.

The rest of this chapter is organized as follows. The second section gives a detailed introduction to NHI and the SP and discusses their implications for the behavior of physicians, pharmacists, and elderly patients. The third section lays out our empirical framework. The fourth section summarizes the data. The fifth section reports estimation results. We discuss our findings and conclude in the last section.

2.2. Two Health Care Reforms in the 1990s

On March 1, 1995, Taiwan initiated its National Health Insurance (NHI) program, which provides compulsory insurance coverage to roughly 21 million people including 8 million previously uninsured. At the end of 1994, about 2.3 million people in Taiwan

(11% of the entire population) were aged 60 and older. Among them, about 75% was insured by four social insurance programs: Farmers Health Insurance (FHI), Government Employees Insurance (GEI), Labor Insurance (LI) and Veterans Insurance (VI).[39] As suggested by their names, FHI covered farmers; GEI covered government employees and school teachers; LI covered private sector workers; VI covered veterans. These programs all provided comprehensive medical benefits including inpatient, outpatient, dental, and emergency service as well as prescription drugs. In 1994, FHI, GEI, LI and VI respectively accounted for 45%, 20%, 18% and 17% of the insured elderly.[40] Besides social insurance, there was virtually no comprehensive private insurance at the time.[41]

In the pre-NHI period, if the uninsured elderly chose to visit physicians, they had to pay entirely out of pocket for all physician services and prescription drugs. In contrast, if they chose to visit pharmacists, they only needed to pay for drugs. The out-of-pocket expenditure difference between visiting physicians and pharmacists made the uninsured elderly more likely to choose the latter.

The implementation of NHI largely eliminated the out-of-pocket expenditure difference between visiting physicians and pharmacists for the previously uninsured elderly because NHI now paid for almost all physician services, including diagnosing, prescribing, and dispensing drugs. In fact, NHI provides a range of benefits similar to that of the former social insurance programs. See the previous chapter for more details about NHI.

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[39] Several other social insurance programs also existed at the time but only accounted for a very small portion of the elderly population.

[40] All these numbers are my own calculations using administrative statistics from Bureau of Central Trust, Bureau of Labor Insurance, Bureau of National Health Insurance, Veteran Affairs Commission and Ministry of the Interior.

[41] More precisely speaking, private insurance only provided very limited coverage and generally did not cover any outpatient care (Liu and Chen 2002; Cheng 2003).

Unfortunately, NHI has been besieged by fast-growing budgetary woes since its inception.[42] Cheng (2003) points out that one of the biggest contributions to the budget crisis are growing drug expenditures, and many believed that physicians' ability to continue to both prescribe and dispense drugs was one of the major causes of these soaring costs. The government thus initiated the SP to split the two practices.[43] The SP was first initiated in 1997 in the two biggest cities, Taipei and Kaohsiung, and then gradually expanded to include the remaining 21 counties and cities over 6 years.

The law that requires drug prescribing and dispensing to be separated is called *yao shi fa* in Chinese, or the Pharmaceutical Affairs Act (PAA) in English, which was amended in 1993. Article 50 of the PAA forbids pharmaceutical sellers to sell prescription drugs to consumers without a physician's prescription, with some exceptions.[44] The exceptions include the sale of prescription drugs among pharmaceutical sellers and medical care and research institutions. In addition, drugs listed in two official collections of prescriptions—*zhong hua yiao dian* (Chinese medicine book) and *guo ming chu fang xuan ji* (National collection of prescriptions)—can be sold to patients without a physician's prescription.

In general, pharmacists will be fined between NT$3,000 and NT$15,000 (US$1,000–US$5,000) if they are caught selling drugs to patients without a physician's prescription. On the other hand, to encourage community pharmacists to fill prescriptions, NHI pays them NT$20 (US$0.7) for each prescription they fill (Chou et al. 2003).[45]

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[42] According to Cheng (2003), over the period 1995–2001, NHI revenues increased at an average rate of 4.26 percent, while expenditures increased at 6.26 percent.

[43] South Korea launched a similar policy in 2000 (Kwon 2003; Park et al. 2005).

[44] The law does not apply to over-the-counter (OTC) drugs.

[45] This prescription filling fee was changed several times afterwards.

For physicians, Article 102 of the PAA clearly states that physicians are not allowed to dispense drugs except in some special conditions.[46] For example, physicians can still dispense drugs to patients over age 65 and under age 3 who are unable to go to pharmacies by themselves. Meanwhile, to encourage physicians to release prescriptions to patients, NHI pays a NT$25 (US$0.8) "prescription release fee" for each prescription that they release to be filled by community pharmacies (Lee, Huang, and Huang 2007).[47]

The implementation date of the SP in each county was determined based on some criteria set up by the Department of Health (DOH). Two of the most important criteria are: (1) the ratio of community pharmacies to clinics contracted with NHI must be at least one to three within any given county, and (2) the outpatient department of a DOH-run county hospital has to release at least 3 percent of its prescriptions to patients. Table 2.1 lists the exact implementation dates in all 23 counties or cities in Taiwan.

2.3. Empirical Framework

To evaluate the impacts of NHI and the SP, it is helpful to first envision the triangular relationship of the physician, pharmacist, and patient. The analysis time involves three periods: (1) before 1995, (2) 1995–1997, and (3) after 1997. These periods are separated by the implementation of NHI and the SP, respectively.

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[46] Conditions include: (1) the patient is too young (under 3) or too old (over 65) to go to pharmacies by himself; (2) the patient has some emergency condition and need drugs immediately; or (3) the patient is in some officially announced remote area that has no licensed pharmacists. The latest definition of the third type of exception is that if there is no licensed pharmacist within a radius of 1.8 kilometers (1.1 miles). If so, the physician in the clinic is allowed to dispense drugs.

[47] This "prescription release fee" was canceled in 2006. The NHI reimbursement for medications was adjusted in 2002 so that the payment for a prescription filled by an on-site pharmacist is lower than by a community pharmacist. In response to this adjustment, some clinic physicians set up their own pharmacies, which may be registered under someone else's name, just in front of or next door to their clinics and refer patients there. This type of pharmacy is called a "gateway pharmacy" (Lee et al. 2007). To tackle this problem, NHI simply canceled the "prescription release fee" in 2006 and now strictly requires clinic physicians to release prescriptions.

Table 2.1. Timetable of the SP implementation	
Date	City/County
03.01.1997	Taipei City; Kaohsiung City
03.10.1998	Taichung City; Chiayi County; Chiayi City; Keelung City
04.20.1998	Changhua County; Miaoli County
06.06.1998	Kaohsiung County; Hsinchu County; Yunlin County
07.06.1998	Taichung County; Pingtung County
11.05.1998	Nantou County; Tainan County; Tainan City
12.09.1998	Taipei County
01.28.1999	Taoyuan County; Hsinchu City
06.21.1999	Yilan County
04.01.2001	Hualien County
05.01.2001	Taitung County
12.10.2002	Penghu County

Source: Department of Health, http://drug.doh.gov.tw/admin/new_file_download.php?Pact=FileDownLoad&Pval=297.

In the first period, both the physician and the pharmacist prescribed and dispensed drugs. In principle, the patient would have received better diagnosis and treatment from the physician than the pharmacist. But the cost of visiting the physician was also higher than visiting the pharmacist if one were uninsured and had to pay completely out of pocket. Therefore, the uninsured patient faced a trade-off between price and care quality.[48]

In the second period, everyone was insured by NHI. The physician and the pharmacist still both prescribed and dispensed drugs. However, the out-of-pocket expenditure difference between visiting the physician and the pharmacist was largely eliminated for the previously uninsured, because NHI paid for most physician services and prescription drugs. This provided an economic incentive for the previously uninsured patient to visit the physician, who would provide better care than the pharmacist. Moreover, the patient could still purchase drugs at the pharmacy without a prescription.

[48] It is also possible that the uninsured patient could choose to see the physician first and then buy drugs at the pharmacy. But, in practice, this was less likely to happen because he or she would have to pay the additional cost of traveling between the clinic and the pharmacy.

But he or she would have to pay out of pocket for drugs without a prescription, because NHI only paid for drugs with a prescription.

In the third period, the pharmacist was not allowed to dispense drugs to the patient without a physician's prescription. But the physician could still dispense drugs to the elderly patient. Besides, many physicians hired their own on-site pharmacists. Keep in mind that every patient was now covered by NHI. Because of the SP, the patient was now forced to see the physician in order to get a prescription. Then, the key question is where the patient chose to buy drugs. Most likely, the patient—whether elderly or not—would want to buy drugs from an on-site pharmacist so that he or she would not have to travel to a pharmacy outside the clinic. In other words, extraneous costs discouraged the patient, elderly or not, from visiting an outside pharmacy once the patient had visited the physician in the clinic.

Empirical Strategy

To assess the NHI effect, we adopt the same DD strategy used in the previous chapter. We consider the previously uninsured elderly as the treatment group and the elderly who were covered by the major social insurance programs as the control group. The validity of the DD method relies on the following two assumptions. First, shifting from social insurance to NHI had little effect on the control group. Second, the two groups would have experienced a similar time trend in the absence of NHI. We estimated the following DD model.

(1) $y_{it} = \beta_0 + \beta_1 UI_i + \beta_2 YR_t + \beta_3 UI_i \times YR_t + controls_i + \varepsilon_{it}$,

where i indexes observations; t indexes time; y is an outcome variable; UI is a dummy variable indicating the previously uninsured elderly based on their insurance status in

1993; *YR* is a dummy variable indicating the latter year of a pooled two-year panel; *controls* is a set of baseline control variables including sex, age, age-squared, education, ethnicity, marriage, employment, and income evaluated at the baseline year of 1993. To account for county level differences, we also include a full set of county dummies; ε is the error term. In equation (1), the two interaction terms measure the difference in differences in outcomes.

We also use the same longitudinal survey of the elderly in Taiwan used in the previous chapter. To estimate equation (1), we run three separate regressions with three separate pooled two-year panels—1993 versus 1996, 1996 versus 1999 and 1999 versus 2003. In each pooled panel, we retain only those who appear in both years. We do so because of a similar sample attrition issue as discussed in the previous chapter. The purpose is to reduce a possible non-random attrition bias as well as to retain as many observations in each pooled panel as possible.

To estimate the SP effect, we mainly exploit the gradual phasing-in of the SP. As mentioned above, the SP was first initiated in 1997 in the two biggest cities and then gradually applied to other counties and cities over six years. Because every wave of the survey was conducted between March and June in each year, we thus use June 1999 as a cutoff point.

By June 1999, 18 counties and cities on the west coast of Taiwan had implemented the SP, while the remaining three counties on the east coast—Yilan County, Hualien County, and Taitung County—had not (Table 2.1).[49] Therefore, we use the elderly people who lived on the west coast as the treatment group and those who lived on

[49] None of the elderly in our sample lived in Hsinchu County and Penghu County.

the east coast as the control group. Then we adopt the DD method to estimate the SP effect by assuming that the two groups would have experienced a similar time trend in the absence of the SP.

Moreover, the three counties on the east coast also adopted the SP during 1999 and 2001 (Table 2.1). Therefore, after 1999, we can further use the three counties as the new treatment group and the 18 counties on the west coast that had previously adopted the SP as the control group.

It is legitimate to question whether some of the elderly people from the west coast traveled the east coast, or vice versa, to purchase drugs without a prescription. If many elderly people did so, our identification strategy is problematic. But this chance is minimal because, as shown in Figure 2.1, the east coast counties are geographically separated from the west by the Central Mountain Range, which is 1,000–3,000 meters above sea level on average. This geographic segregation imposes a very high transaction cost for the elderly travel between the coasts.[50]

Specifically, to estimate the SP effect, we estimate the following DD model by OLS.

(2) $y_{it} = \beta_0 + \beta_1 SP_i + \beta_2 YR_t + \beta_3 SP_i \times YR_t + controls_i + \varepsilon_{it}$,

where *SP* is a dummy variable indicating the elderly who lived on the west coast if it is before June 1999 and the elderly who lived on the east coast if it is after June 1999; all other notations are the same as in equation (1). In equation (2), again, the interaction term measures the difference in differences in outcomes. To estimate equation (2), we also run three separate regressions with the same three pooled panels used for equation (1).

[50] For example, it would take 1.5 hours to travel from Taipei City to Yilan County by train, one of the most common ways to commute between the two places.

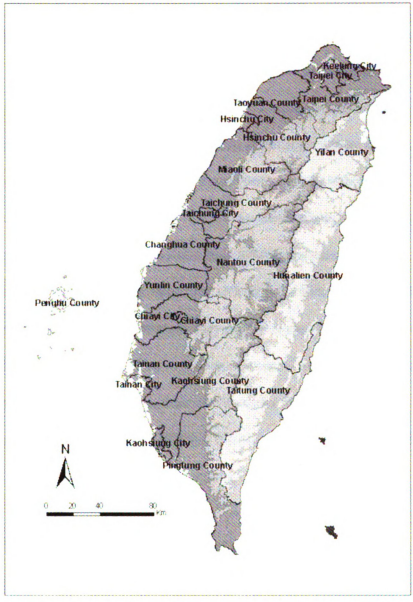

Figure 2.1. Map of Taiwan

It is worth noting that in the case of 1993 versus 1996 and 1996 versus 1999, the treatment group refers to those who lived on the west coast and the control group on the east coast. On the contrary, in the case of 1999 versus 2003, the treatment group refers to those who lived on the east coast and the control group on the west coast.

Under the DD assumptions mentioned above, we only aim to estimate the treatment effects, i.e. the NHI effects and the SP effects, on the treated, i.e. the previously uninsured elderly, the elderly who lived on the west coast before 1999 and the elderly who lived on the east coast after 1999. As mentioned in the previous chapter, estimating the treatment effects on the entire population require a stronger assumption—the control group would have experienced the same treatment effect if they were treated. However, we do not have a fully randomized treatment and control group in both the NHI and the SP case. Plus, we have observed that the two groups are different in many aspects in the baseline. Therefore, it is hard to believe that such a strong assumption would hold.

Furthermore, the previously uninsured elderly were more likely to seek care at pharmacies in the pre-NHI period because they paid less out-of-pocket than seeking care at clinics. It thus would be interesting to further examine if the SP had different effects on the previously uninsured elderly than their continuously insured counterparts. Therefore, after estimating the SP effects, we re-estimate equation (2) by insurance groups to see if the SP had different effects on the insurance groups.

Key outcome variables

In each wave of the elderly survey, respondents are asked to recall if they made any pharmacy and outpatient visits in the month prior to the interview.[51] Two dummy variables are constructed accordingly to the answers, *pharmacy* and *outpatient*.

[51] There are actually two questions asking about outpatient visits to practitioners of (1) Western medicine and (2) Chinese medicine. I define outpatient visits as visits to either a Western or Chinese outpatient service. In addition, outpatient visits can be visits to the outpatient department in a hospital or a clinic because the survey does not make a distinction between the two. The question regarding pharmacy visits is stated as follows: "In the past month, did you or your family member ever purchase drugs at *yao fang* for your use?" Here, *yao fang* is "pharmacy" in Chinese. In particular, it generally refers to pharmacies outside hospitals and clinics. One may worry about whether respondents may have interpreted *yao fang* to include so-called *gateway* pharmacies which are outside clinics but actually owned by physicians. But gateway pharmacies only started to emerge after 2000 (Lee et al. 2007), which is outside our analysis period.

To get a clearer picture of the substitution of physicians for pharmacists and vice versa, we construct four mutually exclusive variables: *outpatient only*, *pharmacy only*, *both,* and *neither*. *Outpatient only* is a dummy variable indicating that the interviewee made only outpatient visits but no pharmacy visits in the past month; *pharmacy only* is a dummy variable indicating only pharmacy visits but no outpatient visits in the past month; *both* is a dummy variable indicating both outpatient and pharmacy visits in the past month; *neither* is a dummy variable indicating neither outpatient nor pharmacy visits in the past month. These four variables together exhaust all possible combinations.

To measure health, we use a 5-point scale of self-reported health, with *very good* at the highest level and *very bad* at the lowest. Because of relatively small proportions at the two tails of the distribution, we combine *very good* and *good* into one group and *bad* and *very bad* into another.

2.4. Data and Descriptive Statistics

This chapter also uses *the Survey of Health and Living Status of the Middle-aged and the Elderly in Taiwan.* We mainly use the elderly sample in four waves of the survey: 1993, 1996, 1999 and 2003.[52] We do not use the 1989 survey because its utilization questions are different from other waves.[53] Note that, in the analysis of NHI, the treatment and control group are defined by the insurance status self-reported by the elderly in the 1993 survey. In the analysis of the SP, the treatment and control groups are defined by residence region.

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[52] We are not able to use the middle-aged sample for two reasons. First, it was drawn in 1996 and the pre-NHI insurance status of the middle-aged was thus unknown. Second, the number of the middle-aged who lived in the area where SP was implemented after 2000 and appeared in both the 1996 and 1999 survey, was too small (only 28) to constitute a reliable control group in analyzing the SP effect.

[53] For example, the 1989 survey asks the elderly to recall their pharmacy visits in a year, while all other waves ask the elderly to recall their visits in one month.

Table 2.2. Baseline Characteristics of the Elderly

	By Insurance		By Region	
	Uninsured	Insured	West	East
	(1)	(2)	(3)	(4)
Age in 1993 (mean)	72	71	74	74
Female (%)	57	39	44	40
Married with spouse present (%)	47	67	62	60
Ethnicity (%)				
Minnan	74	56	62	52
Hakka	10	17	16	11
Mainlander	14	24	22	21
Aboriginal	1	2	0	16
Education (%)				
No education	60	45	49	44
1-6 years	31	33	32	39
7 years and more	9	22	19	17
Region (%)				
North	40	24	30	0
East	6	8	0	100
Middle	28	36	36	0
South	27	32	33	0
Employed/ self-employed (%)	11	23	13	9
Have enough money for living expenses (%)	75	85	83	79
Total	784	2,369	2,909	242

Notes: categorizations based on insurance status and residence region in 1993.

Table 2.2 summarizes the baseline characteristics of the elderly by insurance and residence region in 1993. In 1993, there were 784 uninsured and 2,369 insured elderly as well as 2,090 living on the west coast and 242 on the east coast. Compared to their insured counterparts, the previously uninsured were more likely to be women, unmarried or widows, Minnan, less educated, living in the North, not working and poorer.[54] On the other hand, compared to their east coast counterparts, the elderly lived on the west coast are more likely to be women, Mnnan, working and richer. It is worth noting that there

[54] There are four major ethnic groups in Taiwan: the Minnan, Hakka, Mainlanders, and aboriginals. The first three groups originated from China but migrated to Taiwan at different times. In particular, Mainlanders generally refer to those who migrated from mainland China in 1949.

were a relatively high proportion of the aboriginal people living on the east coast. This is because that the east coast is their traditional inhabitation area.

Table 2.3 summarizes their utilization and self-reported health. For probability of having outpatient visits, we find that, from 1993 to 1996, the probability for the uninsured elderly increased (about 26 percentage points) twice as large as the insured elderly (about 13 percentage points). However, from 1996 t 2003, the two groups experienced a similar increase of about 14 percentage points in probability. These findings suggest that NHI, initiated in 1995, increased the probability of outpatient visits for the previously uninsured elderly relative to their continuously insured counterparts.

Table 2.3. Summary of Outcome Variables

	By Insurance				By Region			
	(1) Uninsured		(2) Insured		(3) West		(4) East	
Outpatient	%	n	%	n	%	n	%	n
1993	33.59	783	50.02	2,365	46.04	2,904	45.04	242
1996	59.79	582	62.66	1,939	61.68	2,333	66.13	186
1999	63.15	502	70.64	1,659	69.02	2,001	67.09	158
2003	73.15	324	76.88	1,146	76.49	1,374	69.15	94
Pharmacy								
1993	28.83	784	21.41	2,368	23.18	2,908	23.97	242
1996	32.25	583	23.70	1,941	25.68	2,336	25.81	186
1999	16.17	501	14.79	1,656	14.47	1,997	23.42	158
2003	16.98	324	15.49	1,143	16.24	1,373	9.78	92
Very Good/ Good								
1993	35.59	784	41.07	2,369	40.53	2,909	29.75	242
1996	26.76	583	29.11	1,941	28.72	2,336	26.34	186
1999	24.10	502	26.69	1,660	26.37	2,002	22.78	158
2003	26.59	361	26.80	1,261	27.25	1,512	20.37	108
Bad / Very Bad								
1993	24.36	784	20.81	2,369	20.94	2,909	30.99	242
1996	29.50	583	31.12	1,941	30.31	2,336	36.02	186
1999	44.42	502	39.04	1,660	39.86	2,002	45.57	158
2003	45.98	361	42.51	1,261	42.92	1,512	47.22	108

Notes: categorizations based on insurance status and residence region in 1993.

In the long run, the two groups simply experienced a similar time trend, which strengthens the validity of our DD strategy.

By region, we find that from 1993 to 1999, the elderly living on both west and east coast experienced a similar increase in the probability of outpatient visits by about 23 percentage points. From 1999 to 2003, the west seemed to have a larger increase than the east, reason of which is not clear.

As for pharmacy visits, we find that the previously uninsured elderly were always more likely to visit pharmacies than their continuously insured counterparts. Both experienced a dramatic drop in probability of pharmacy visits from 1996 to 1999, which suggests the influence of the SP initiated in 1997. However, the drop for the previously uninsured elderly was much larger than that for the continuously insured elderly. Besides, their probabilities in 2003 remained similar to the 1999 level.

Comparing the west with the east, we find that both groups were very similar in the pre-SP period (before 1997), which also strengthens the validity of using DD strategy. From 1996 to 1999, we find that the west—already subject to SP—had a significant 11-point drop in probability of pharmacy visits, while the east—not subject—only had a small 2-point drop. From 1999 to 2003, on the contrary, the east—newly subject to SP—had a large 13.6-point drop in probability, while the west—still subject SP—had a small 1.8-point increase. These preliminary findings all suggest the SP affected the elderly in terms of their pharmacy visits.

Table 2.3 also summarizes the self-reported health of the elderly. Overall, the probability of reporting very good or good health decreased over time; the probability of reporting bad or very bad health increased over time. One interesting finding is that, from

1993 to 1996, while both previously uninsured and continuously insured elderly were having higher probabilities of reporting bad or very bad health, the increase for the previously uninsured elderly (5 percentage points) was only half of that for the continuously insured elderly (10 percentage points.)

For our DD strategy, one important assumption is that the treatment group and the control group share a common time trend. Comparing the time trends of both groups before the treatment, i.e. NHI or SP, could provide us some information. Such comparisons would take at least two points before the treatment. Unfortunately, for NHI which was initiated in 1995, we have only one point in 1993. However, for SP which was initiated in 1997, we have 1993 and 1996 and thus can look at the trend before 1997.

We calculate the trend in the probability of pharmacy visits from 1993 to 1996 for the west and the east by taking the difference between the two points. Further, since the uninsured elderly were affected by the 1995 NHI while the insured elderly were not, we separate the comparison by insurance groups. Then, we use *t* test to test if the difference across years is the same for the west and the east. Beside, in order to take difference, we only use those who appear in both 1993 and 1996. In Table 2.4, we find that among the uninsured elderly, the probability of pharmacy visits increased 3.4 percentage points for the west, while there was no increase for the east. However, the *t* statistic shows that the two differences for both groups are not statistically different. On the other hand, among the insured elderly, the west increased by 2.7 percentage points and the east by 1.3 points. Again, the test result shows that these two increases over time are not statistically different from each other. These findings suggest the common time trend is reasonable at least for the SP analysis.

Table 2.4. Comparison of Time Trends in Pharmacy Visits				
	Uninsured		Insured	
	(1)	(2)	(3)	(4)
	West	East	West	East
Pharmacy (%)				
1993	28.9	30	20.8	23.7
1996	32.3	30	23.5	25
Difference	3.4	0	2.7	1.3
n	551	30	1,777	156
t (*p*-value)	-0.31 (0.75)		-0.32 (0.75)	
Notes: the null hypothesis is that the difference in the west equals to the difference in the east.				

2.5. Estimation Results

NHI Effects on Outpatient and Pharmacy Visits

Estimated NHI effects are reported in Panel A in Table 2.5. Note that all results are from separate regressions and only the interaction terms are reported. In the case of 1993 versus 1999, we find a significant 13.2-point increase in probability of outpatient visits (column 1), which is resulted from a 6.5-point increase in probability of making only outpatient visits (column 3) and a 6.6-point increase in probability of making both outpatient and pharmacy visits (column 5). Meanwhile, there was almost no change in probability of pharmacy visits (column 2). However, we do find a significant 5.9-point drop in probability of making only pharmacy visits. These findings suggest that NHI made the previously uninsured elderly more likely to have outpatient visits, including those who previously only visited pharmacies. In addition, there was a 7.3-point decrease in probability of making neither outpatient nor pharmacy visits, suggesting NHI raised the overall utilization for the previously uninsured elderly.

Table 2.5. NHI and SP Effects on Utilization

	(1) *Outpatient*	(2) *Pharmacy*	(3) *Outpatient only*	(4) *Pharmacy only*	(5) *Both*	(6) *Neither*
Panel A: NHI effect						
1993 vs. 1996	0.132	0.007	0.065	-0.059	0.066	-0.073
(n=2,516)	(4.52)	(0.27)	(2.25)	(-2.78)	(2.94)	(-2.54)
1996 vs. 1999	-0.048	-0.07	0.036	0.013	-0.084	0.034
(n=2,044)	(-1.51)	(-2.49)	(1.05)	(0.74)	(-3.4)	(1.12)
1999 vs. 2003	0.024	0.004	-0.003	-0.024	0.027	0
(n=1,466)	(0.69)	(0.11)	(-0.09)	(-1.38)	(0.99)	(0)
Panel B: SP effect						
1993 vs. 1996	-0.044	0.018	-0.056	0.006	0.012	0.038
(n=2,514)	(-0.89)	(0.4)	(-1.07)	(0.17)	(0.35)	(0.82)
1996 vs. 1999	0.056	-0.092	0.12	-0.028	-0.064	-0.028
(n=2,042)	(1.03)	(-1.86)	(2.13)	(-0.82)	(-1.49)	(-0.57)
1999 vs. 2003	-0.031	-0.189	0.084	-0.075	-0.115	0.106
(n=1,464)	(-0.51)	(-3.49)	(1.27)	(-2.07)	(-2.39)	(1.72)

Notes: Cluster-robust *t* statistics are in parentheses. Results are from separate regressions. Only interaction terms are reported. All regressions include baseline controls such as sex, age, age-squared, education, ethnicity, marriage, employment, and income evaluated at the baseline year and a full set of county dummies. For the case of 1993 vs. 1996 and 1996 versus 1999 in panel B, the elderly who lived in the west coast counties are the treatment group and those who lived in the east coast counties the control group. For the case of 1999 versus 2003 in panel B, the treatment and control group are reversed.

For the case of 1996 versus 1999 and 1999 versus 2003, we find no statistically results except for a significant 7-point drop in probability of pharmacy visits from 1996 to 1999. This finding may be related to the implementation of the SP.

SP Effects on Outpatient and Pharmacy Visits

Estimated SP effects are reported in Panel B in Table 2.5. There was no statistically significant findings in the case of 1993 versus 1996, which is not surprising since the SP was absent in this period. However, we find a 9.2-point drop (when the SP was applied to the west) and 18.9-point drop (when the SP was applied to the east) in probability of pharmacy visits in the case of 1996 versus 1999 and 1999 versus 2003 respectively (column 2). Both drops result from decreases in probability of making only pharmacy visits and probability of making both outpatient and pharmacy visits. These findings suggest that the implementation of the SP made the elderly less likely to visit pharmacies and more likely to make only outpatient visits. In addition, it is interesting to note that the SP effect applied to the east was twice as large as that to the west. This suggests that the east was much more responsive to the SP than the west. However, it is not clear why this occurred, given our limited information about the two sides.

Effects on Self-reported Health

The NHI effect on self-reported health is reported in Panel A of Table 2.6. In the case of 1993 versus 1996, we find a significant 5-point drop in the probability of reporting bad or very bad health (column 2) for the previously uninsured elderly relative to their continuously insured counterparts. This suggests that although the health of both groups was deteriorating, the NHI made the previously uninsured elderly deteriorate at a slower rate than the continuously insured elderly, which was presumably resulted from

the increase in utilization.[55] However, other than this finding, we do not find any other significant result.

Estimated SP effects are reported in Panel B. We do not find any statistically significant results, which is somewhat surprising because one would expect the elderly should have received better care once they stopped visiting pharmacists and started to visit physicians.

Table 2.6. NHI and SP Effects on Self-reported Health

	(1) *Very Good / Good*	(2) *Bad / Very Bad*
Panel A: NHI effect		
1993 vs. 1996	0.024	-0.051
(n=2,516)	(0.92)	(-1.99)
1996 vs. 1999	-0.007	0.069
(n=2,044)	(-0.24)	(2.23)
1999vs. 2003	-0.022	0.003
(n=1,466)	(-0.61)	(0.07)
Panel B: SP effect		
1993 vs. 1996	-0.061	0.039
(n=2,514)	(-1.45)	(0.92)
1996 vs. 1999	-0.003	-0.028
(n=2,042)	(-0.06)	(-0.6)
1999 vs. 2003	-0.007	0.007
(n=1,464)	(-0.14)	(0.12)

Notes: See Table 2.5.

Differential SP Effects between the Insurance Groups

Table 2.7 reports the SP effects on utilization by insurance group. Panel A is the case of 1996 versus 1999 and Panel B is the case of 1999 versus 2003. Most of the results are not statistically significant. However, there are some interesting findings that suggest the SP seem to have differential effect on the previously uninsured and the continuously

[55] Of course, NHI also affected other types of care. For example, the previous chapter finds that NHI largely increased the probability of hospitalization for the previously uninsured elderly. Therefore, the improvement in self-reported health could have also come from increases in other types of care.

insured elderly. In the case of 1996 versus 1999, we find that the SP effect on pharmacy visits was dominated by the continuously insured (column 2), which is not surprising because the continuously insured elderly account for 77% of the sample. However, the SP did not seem to make the previously uninsured less likely to visit pharmacies. Instead, the SP seemed to make them much more likely to only make outpatient visits (20-point in column 3, although not statistically significant. Moreover, the SP also seemed to make the previously uninsured elderly more likely to make both outpatient and pharmacy visits in the case of 1996 versus 1999 (21-point in column 5). Consequently, the SP seemed to largely reduce their probability of making neither outpatient nor pharmacy visits.

In the case of 1999 versus 2003, we find that the SP effect on pharmacy visits seemed to be similar for the previously uninsured and continuously insured elderly (column 2).

Table 2.7. SP Effect on Utilization by Insurance Group

	(1)	(2)	(3)	(4)	(5)	(6)
	Outpatient	*Pharmacy*	*Outpatient only*	*Pharmacy only*	*Both*	*Neither*
Panel A: SP effect—1996 vs. 1999						
Uninsured	0.221	0.022	0.2	0.001	0.021	-0.222
(n=471)	(1.6)	(0.21)	(1.33)	(0.01)	(0.22)	(-1.99)
Insured	0.03	-0.107	0.102	-0.035	-0.073	0.005
(n=1,571)	(0.5)	(-1.95)	(1.67)	(-0.92)	(-1.52)	(0.09)
Panel B: SP effect—1999 vs. 2003						
Uninsured	0.017	-0.193	0.111	-0.098	-0.095	0.081
(n=324)	(0.1)	(-1.94)	(0.62)	(-1.2)	(-1.47)	(0.54)
Insured	-0.041	-0.188	0.077	-0.07	-0.118	0.111
(n=1,140)	(-0.64)	(-2.99)	(1.1)	(-1.75)	(-2.06)	(1.64)

Notes: results are from separate regressions among the previously uninsured elderly and continuously insured elderly. See Table 2.5 for other notes.

Table 2.8. SP Effect on Self-reported Health by Insurance Group		
	(1)	(2)
	Very Good / Good	*Bad / Very Bad*
Panel A: SP effect—1996 vs. 1999		
Uninsured	0.177	-0.013
(n=471)	(1.39)	(-0.09)
Insured	-0.036	-0.038
(n=1,571)	(-0.77)	(-0.78)
Panel B: SP effect—1999 vs. 2003		
Uninsured	0.046	0.098
(n=324)	(0.36)	(0.74)
Insured	-0.02	-0.014
(n=1,140)	(-0.35)	(-0.23)

Notes: See Table 2.7.

SP effects on self-reported health by insurance group are reported in Table 2.8. Again, we do not find any statistically SP effects. However, the SP seemed to have a positive effect on the previously uninsured elderly in the probability of reporting very good or good health (column 1), especially for the case of 1996 versus 1999 (17.7 percentage points, although not statistically significant.)

2.6. Conclusion

This paper studies the effects of NHI and the SP on elderly people's choice between physicians and pharmacists and reaches the following conclusions. First, NHI induced more previously uninsured elderly patients—including those who previously only visited pharmacists—to visit physicians by providing them an economic incentive. Meanwhile, NHI also raised their overall utilization and there was some evidence suggesting the increase in utilization is associated with an improvement in self-reported health.

Second, the SP shifted the elderly patients from pharmacists to physicians. In particular, the SP made them more likely to make only outpatient visits, which suggests

that once the elderly patients had visited physicians, they did not come back to pharmacies to buy drugs. Instead, they bought drugs from on-site pharmacists who were hired by the clinic physicians.

Third, there is little evidence on the SP effect on self-reported health. This is somewhat surprising because one would expect that elderly patients would have received better care by visiting physicians instead of visiting pharmacists. A possible explanation for this finding is that in the past elderly patients only substituted pharmacist services for physician services for minor medical conditions for which quality did not matter much.

Fourth, there was some evidence suggesting that the SP had differential effects between the previously uninsured and continuously insured elderly. Specifically, while the SP made both insurance groups more likely to only make outpatient visits, the effect was much larger for the previously uninsured elderly than their continuously insured counterparts. In addition, in the case of the 1996 versus 1999, the SP seemed to largely raise the probability of reporting very good or good health for the previously uninsured elderly relative who lived on the west coast, although the effect was not statistically significant.

Last, it is worth noting that the two policies are actually intertwined. In particular, the SP would have had a much different impact were it not preceded by NHI. For example, the out-of-pocket expenditure difference would still have been substantial for the uninsured in the absence of NHI. It is thus possible that implementing the SP without NHI could have led some poor uninsured patients to simply forego the care they needed because they could not afford to visit a physician to get a prescription, which is needed in order to buy drugs from a pharmacist.

Appendix 1

INSURANCE STATUS IN 1993 AND SURVEY QUESTIONS

Insurance status in 1993

In the 1993 survey, the elderly respondents are first asked if they are insured. If yes, they are further asked what kind of insurance they have. In the second question, there are 15 insurance programs of which 14 are social insurance programs and 1 is private insurance. In the raw data, 787 report uninsured, 2,367 insured and 1 unknown. However, among the 787 uninsured, 4 report being insured by some insurance program in the following question. To check these conflicts, I use another question asking them who paid for most of their medical expenses. If they answer health insurance paid for most of their medical expenses, they are recoded as insured. In the end, 2 are recoded and thus 785 are uninsured in 1993.

For the insured, I drop the 16 privately insured because private insurance is not comprehensive and they are likely to be affected by NHI. On the other hand, they are not completely uninsured and should not be categorized as uninsured.

In addition, I drop one case that actually died before 1993 but surprisingly appeared in the 1993 survey. I also drop another 2 insured cases without knowing their pre-NHI insurance program. Finally, there are 2,351 insured elderly who are covered by various social insurance programs.

Survey Questions about Utilization

In each wave of survey, the elderly respondents were asked a series of utilization questions in Chinese. Below are the original questions in Chinese and their English

translations. We do not use the 1989 survey because its questions are generally different from other waves of the survey.

Hospitalization

In the 1993, 1996, 1999 and 2003 survey, the elderly respondents were asked the following question regarding hospitalization.

過去一年裏，你有沒有住院過? (*In the last year, have you ever been hospitalized?*)

In the 1989 survey, however, the question was stated as:

過去半年以來，你是否曾經住過院? (包括去急診、住院觀察一天或兩天等情形) (*In the past half year, have you ever been hospitalized? (Including visits to an emergency room, stays in a hospital for a day or two for observation.*))

ER

In 1993, 1996, 1999 and 2003 survey, the elderly respondents were asked the following question regarding emergency room visits.

在過去一年裏，你是否曾到醫院看急診? *(In the last year, have you ever visited the emergency room in a hospital?)*

There was no separate question about emergency room visits in the 1989 survey.

Western Outpatient Visits

In 1993, 1996, 1999 and 2003 survey, the elderly respondents were asked the following question regarding Western outpatient visits.

在過去一個月裏，你有沒有看過西醫?(不包括住院) (*In the last month, have you ever had western medicine visits?(Excluding hospitalizations.*))

This question refers to western medicine outpatient visits.

In the 1989 survey, there was no western outpatient visits question. Instead, there were questions stated as:

在過去一年裏，你是否曾經去過公立診療院所? (*In the last year, have you ever visited a public hospital or clinic?*)

在過去一年裏，你是否曾經去過私立診療院所? (*In the last year, have you ever visited a private hospital or clinic?*)

From these two questions, we do not know if the patients were hospitalized or just visited the outpatient department in a hospital or clinic.

Foregoing Doctor Visits

In 1993, 1996, 1999 and 2003 survey, the elderly respondents were asked the following questions.

最近三個月裏，你有沒有身體不舒服，想去看醫生，但卻沒去? (*In the past three months, have you ever felt ill and would like to see a doctor but eventually did not do it?*)

If one answered yes, he or she was further asked to choose from a list of reasons. One of the listed reasons was because 沒有錢 (*lack of money*). Respondents were allowed to have multiple choices.

We thus use these two questions to construct a dummy variable indicating one did not visit a doctor because lack of money. There were no such questions in the 1989 survey.

Appendix 2

Differences between Chapter 1 and Chen et al. (2007)

Approach on Estimating NHI Effect on Mortality

As we briefly discussed before, Chen et al. use a linear DD model to estimate the NHI effect on one-year mortality. More precisely, they compare the one-year mortality rates in 1996 and 1999 to the pre-NHI rate which is an average of the 1989 and 1993 rates. In this sense, they only look at 4 annual data points and ignore information outside those years. In their Table III, their DD estimates show 1.16 and 0.42 percentage points of NHI effect on one-year mortality in 1996 and 1999 respectively, although the effects are not statistically significant. We replicate their one-year mortality rates in 1996 and 1999 and find similar rates in 1996 but not in 1999: their 1999 rate is 1.87% for the continuously insured elderly and 3.08% for the previously uninsured elderly, which are much lower than what we find. We find them to be at least 5.95% and 4.69%.[56] In addition, their rates decrease from 1996 to 1999, while we find them to increase over time. See Table A2.1 for comparison of one year mortality rates.

In their paper, they argue that their analysis time (1989-1999) may not be long enough to detect any NHI effect on mortality. In contrast to their approach, we incorporate the DD design into a hazard model that allows us to utilize all mortality data without having to give up any piece of information we have. This approach makes a major difference even without extending the analysis beyond 1999. We can verify this by limiting the analysis time to 1993-1999 and re-estimating equation (4) in chapter 1. The results are reported in Table A2.2. From 1993 to 1999, there are 901 and 875 deaths

[56] It is not clear which data they use. Besides, we are not able to replicate their pre-NHI rates which should be averages of 1989 and 1993 rates, unless we know their imputation of insurance for those who appeared only in the 1989 survey.

Table A2.1. Comparison of One Year Mortality Rates (%)

	Previously Uninsured Elderly		Continuously Insured Elderly	
	1996	1999	1996	1999
Chen et al. (2007)	4.85	3.08	4.38	1.87
Survey	4.75	4.69	4.63	5.95
DOH	4.71	4.98	4.36	6.47

Notes: yearly mortality rates in the first row are adopted from Table III in Chen et al. (2007); in their table, the previously uninsured were called "No Pre-NHI Insurance Group" and the continuously insured "Pre-NHI insurance group."

Table A2.2. NHI Effect on Hazard Ratio
Analysis Time: 1993-1999

Data		Survey			DOH	
	(1)	(2)	(3)	(4)	(5)	(6)
UI	0.449	0.304	0.241	0.447	0.315	0.257
	(3.48)	(2.25)	(1.77)	(3.37)	(2.27)	(1.84)
POST	0.26	0.346	0.451	0.288	0.363	0.451
	(2.94)	(3.91)	(5.07)	(3.19)	(4.01)	(4.96)
UI×POST	-0.33	-0.326	-0.272	-0.315	-0.313	-0.269
	(-2.09)	(-2.07)	(-1.73)	(-1.96)	(-1.95)	(-1.67)
Pre-NHI controls:						
Demographic & Economic	No	Yes	Yes	No	Yes	Yes
Health	No	No	Yes	No	No	Yes
Subjects	3,136	3,136	3,136	3,136	3,136	3,136
Deaths	901	901	901	875	875	875
Hazard Ratio:						
Pre-NHI	1.57	1.35	1.27	1.56	1.37	1.29
Post-NHI	1.13	0.98	0.97	1.14	1	0.99

Notes: *t* statistics are in parentheses; POST is an indicator for period after March 1995 to end of 1999; hazard ratio is calculated as exp(UI) in the pre-NHI period and exp(UI+UI×POST) in the post-NHI period.

according to the survey's collection and the DOH data respectively. Even with this much shorter time period, we still find that the pre-NHI hazard ratio is about 1.3 and the post-NHI hazard ratio is about 1. And the pre-NHI ratio is statistically different than 1, while the post-NHI ratio is not.

Defining the Insured and Uninsured in the Pre-NHI Period

Chen et al. try to use insurance status of the elderly in 1989 and assume they remained unchanged throughout the pre-NHI period.[57] However, since there is no insurance information in the 1989 survey, they have to impute insurance status of the elderly using the following two rules: 1) for those who appeared in the 1993 survey, they assume their insurance status in 1989 is identical to 1993 status; 2) for those who appeared in the 1989 survey but did not appear in the 1993 survey, they assume those who were government employees, farmers and soldiers in 1989 were insured in 1989. In their final sample, they have 2,990 insured (called pre-NHI insurance group) and 909 uninsured (called no pre-NHI insurance group) in 1989.

However, their imputed insurance status is problematic for two main reasons. First, they assume the 1989 and 1993 insurance statuses are identical which is unlikely to be true. As we discussed in section 2, there was a series of expansions of GEI and FHI in the late 1980s and early 1990s. This implies that some elderly who were insured in 1993 could have been uninsured in 1989. We verify this by comparing the 1989 insurance information contained in the 1991 survey with the 1993 insurance status.[58] Of the 2,887 elderly whose insurance statuses in both 1989 and 1993 were available, 672 changed from uninsured to insured and 75 changed from insured to uninsured. In other words, about 26% of them changed their insurance status. This implies that the 1989 and 1993 insurance statuses of the elderly are not identical.

Second, they assume that government employees, farmers and soldiers were insured in 1989. This assumption obviously ignores the private sector workers who might

[57] In fact, they only use 3,899 of the initial 4,049 elderly. They are not able to assign pre-NHI insurance status to 150 elderly due to incomplete information about employment status.

[58] The 1991 survey is a telephone follow-up focusing on only health and life insurance. It has insurance history of the elderly including when they gained and stopped insurance.

be covered by LI. In addition, only 20% of those who appeared in the 1989 survey but did not appear in the 1993 survey reported that they were working in 1989. It is thus not clear how they imputed insurance status for the other 80%.

Our sample consists of only those who appeared in the 1993 survey and is thus smaller. However, we have a more accurate demarcation between the insured and the uninsured in the pre-NHI period by using the 1993 insurance status.[59] In the context of estimating NHI effects on the previously uninsured elderly, an accurate demarcation between the treatment and control group is essential and probably more important than having a larger sample of the elderly population.

Why Our Estimates of NHI Effects on Utilization Are Similar?

Although they use the initial 1989 sample and we use the 1993 sample, we happen to have very similar estimates of NHI effects on utilization. This happens simply because comparable information about utilization is not available in the 1989 survey and both of us use utilization information starting in 1993. As a result, those who appeared in the 1989 survey but did not appear in the 1993 survey do not enter their utilization analysis. In addition, since we both adopt DD method, our results on utilization are thus similar.

However, we still find one thing puzzling in their Table III. As indicated in note 3 in their Table III, they claim that "only the 1993, 1996 and 1999 surveys offered information on outpatient and inpatient care utilization." Therefore, the columns under "before NHI" in their Table III should contain only 1993 utilization information and the sample in that year. However, in the first two rows, probability of using outpatient and

[59] It is possible that the elderly may still change their insurance status between 1993 and 1995. However, the chance is much lower.

inpatient care, the sample size (indicated in parenthesis) of the 'No pre-NHI insurance group' (correspond to our previously uninsured) is 598 and the sample size of the 'Pre-NHI insurance group' (correspond to our continuously insured) is 2,557. These two numbers are not consistent with the data. As mentioned in Appendix 1, the original data show that there are 787 uninsured, 2,367 insured and 1 with unknown status in 1993.

Appendix 3

Availability of Data and Full Estimation Results

For brevity reasons, only key estimation results are reported in the previous chapters. For readers who are interested in learning more, the full estimation results are available upon request. Please contact me at changkanghung@gmail.com.

The availability of the elderly data requires permission from the Department of Health Promotion in Taiwan. Therefore, I do not have the right to distribute the data to readers who are interested in replicating the results or in doing further analysis. To acquire the data, please visit their website at http://www.bhp.doh.gov.tw/NHPnet/Portal/.

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