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THE IMPACT OF NATIONAL HEALTH INSURANCE ON BIRTH OUTCOMES: A NATURAL EXPERIMENT IN TAIWAN

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ABSTRACT

We estimate the impacts of the introduction of National Health Insurance (NHI) in Taiwan in March 1995 on the health of infants. Prior to NHI, government workers (the control group) possessed health insurance policies with comprehensive coverage for births and infant medical care services. Private sector industrial workers and farmers (the treatment groups) lacked this coverage. All households received coverage for the services just mentioned as of March 1995. Since stringent requirements for reporting births introduced in 1994 produced artificial upward trends in early infant deaths, we focus on postneonatal mortality (deaths from the 28th through the 364th day of life per thousand survivors of the first 27 days of life). We find that the introduction of NHI led to reductions in this rate for infants born in farm households but not for infants born in private sector households. For the former group, the rate fell by 0.5 deaths per thousand survivors or by 13 percent relative to the mean in the pre-NHI period of 4 deaths per thousand survivors. An especially large decline of 6 deaths per thousand survivors occurred for pre-term infants-- a 36 percent drop relative to the pre-NHI mean of 17 deaths per thousand survivors.

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I. Introduction

Improvements in infant and child health are widely accepted public health and public policy goals in most developing and developed countries. One obvious way to accomplish these goals is to provide health insurance to pregnant women, infants, and children. From the point of view of mothers and families, this policy lowers the prices of such medical care services as prenatal care, delivery, neonatal care, vaccinations, immunizations, and well-baby and child care. These price reductions should increase the quantities of services demanded. From the point of view of providers, health insurance guarantees the receipt of payment for services rendered, so that increases in supply should accompany the increased demand.

While appealing in theory, government financed or subsidized health insurance competes with a variety of other mechanisms to improve health including direct cash subsidies to families or providers, publicly supplied care, and health education campaigns to discourage such behaviors as cigarette smoking and inappropriate alcohol use. Hence, estimates of the impacts of the effects of insurance on infant and child health are key ingredients in the policy debate concerning the most efficient ways to improve health.

The extensive literature dealing with the effects of health insurance on infant and child health has reached few definitive conclusions. Given the serious challenges involved in this undertaking, these mixed results are not surprising. As emphasized by Brown, Bindman, and Lurie (1998), Kaestner (1999), and Levy and Meltzer (2008), observational or cross-sectional correlation studies throw little light on causality. On the one hand, women who anticipate a poor birth outcome or who have sick infants or children may be more likely to obtain health insurance. On the other hand, women with a propensity to avoid risk or to engage in a variety of healthy behaviors may be more likely to obtain insurance. In the first case the effect of insurance on health is understated, and in the second case it is overstated.

Studies employing experimental or quasi-experimental data have the potential to reach more definitive conclusions. Examples are the RAND Health Insurance Experiment (HIE), the introduction of National Health Insurance (NHI) in Canada and Medicaid in the United States, and the Medicaid income eligibility expansions in the late 1980s and early 1990s. Even here there are problems. Subjects recruited for the HIE might already have benefited from the medical care covered by their existing policies. The widespread adoption of oral and intrauterine contraceptive devices and the reform of restrictive abortion laws coincided with the introduction of Medicaid and NHI, making it difficult to sort out the impacts of each development. Many women made eligible for Medicaid by the expansions chose not to enroll in the program (Gruber 1997), and some women switched from private insurance to Medicaid (Cutler and Gruber 1996). These factors and year-to-year changes in income in the years just before and just after the expansions make it difficult to create the appropriate treatment and control groups required to evaluate their effects.

The objective of this paper is to estimate the impacts of the introduction of National Health Insurance (NHI) in Taiwan in March 1995 on the health of infants. There is enormous interest in the impacts of NHI on health outcomes, but the very nature of this intervention, whereby entire nations are covered universally, makes it difficult to estimate the health impacts of the change. The experience of Taiwan, however, provides a potential laboratory for overcoming these limitations. Prior to NHI, government workers possessed health insurance policies that covered prenatal medical care, newborn deliveries, neonatal care, and medical care services received by their children beyond the first month of life. Private sector industrial workers and farmers lacked this coverage. All households received coverage for the services just mentioned as of March 1995. Therefore, the introduction of NHI constitutes a natural experiment with treatment and control groups that form the basis of our empirical design. The former group consists of non-government employed households, while the latter group consists of government-employed households. We expect that increases in infant health after the introduction of NHI in the treatment group will exceed corresponding increases in the control group.

To sort out the effects of NHI from unobserved trends, we employ a difference-in-differences estimation methodology. This compares the experiences of the treatment and control groups before and after implementation of NHI under the assumption that other temporally coincident changes are the same for the two groups. We also employ a regression framework. This nets out the effects of observed factors that may change at different rates for the two groups. More importantly, it allows for interactions between health input availability and NHI.

II. Background

A. Taiwan Experience

Legislation authorizing National Health Insurance in Taiwan was enacted on July 19, 1994, and NHI went into effect on March 1, 1995. By the end of 1995, the percentage of the population with health insurance rose from approximately 54 percent in the month prior to implementation to approximately 92 percent. (Executive Yuan, Department of Health 1993; Peabody et al. 1995; Bureau of National Health Insurance 1997; Chiang 1997; Chou and Staiger 2001; Chou, Liu, and Hammitt 2003; personal communication with Jack Ho of the Taiwan Council of Labor Affairs. Unless otherwise noted, the material in this section is drawn from these seven sources.) Prior to NHI, dependent spouses and children and persons over the age of 60 accounted for almost all of the uninsured population, with dependent spouses and children amounting to almost 55 percent of the uninsured.

The large number of uninsured women and children prior to 1995 can be traced to the nature of the health insurance system before that year. Insurance was obtained through one of four government-sponsored health plans, three of which were tied to a person's place of employment in the government sector, the private industrial sector, or the agricultural sector. With the exception of supplementary coverage for a few selected conditions such as cancer and accidents, no private health insurance was available. The employment-based plans were Labor Insurance (LI), which covered 38 percent of the population in 1992; Government Employee Insurance (GI), which covered 8 percent of the population; and Farmers' Health Insurance (FI), which also covered 8 percent of the population. The fourth plan, Low-Income Households' Health Insurance (LII), was provided directly by the government and covered only 0.5 percent of the population.

LI covered individuals who were employed in the private industrial sector (hereafter termed the private sector). GI covered households in which at least one member was employed in the government sector. FI covered households in which at least one member was a farmer and a member of a local farmer's union. Coverage was mandatory for eligible persons in the three employment-based plans. Premium costs were shared by the employer, the employee, and the government in the case of LI by the employee and the government in the case of GI, and by the farmer and the government in the case of FI. Self-employed private sector workers could obtain LI only if they were members of an occupational union.

LI, the largest of these plans, did not provide coverage for non-working spouses (almost entirely wives) and children. FI did not provide coverage for children under the age of 15. GI provided coverage for spouses. Effective July 1, 1992 it also provided coverage for dependent children.

In addition to these differences in coverage, GI provided much more generous benefits in the case of medical care services received by pregnant women and their very young infants. Under each of the three plans, a pregnant woman received a cash benefit for childbirth equal to two months of salary if either she or her husband was covered.[1] Typically this benefit covered the cost of delivery. A woman who underwent a difficult delivery (for example, a cesarean section) could file a claim for its costs if she was willing to forego the cash benefit under LI or FI. Women covered under GI did not have to forego this benefit. More importantly, LI and FI did not cover prenatal care services, and they also did not cover extended hospital stays of low-weight infants during the neonatal period (the first 27 days of life), including the cost of neonatal intensive care. Finally, medical care services delivered to infants of LI and FI women during the postneonatal period (the period between the 28th and 364 days of life) were not covered. On the other hand, GI provided coverage, subject to a coinsurance rate of between 10 and 15 percent, for ten prenatal care visits: a first visit with a detailed physical examination and nine subsequent visits for routine checkups, an ultrasound exam around the 20th week of pregnancy, and screening tests for hepatitis B, rubella, and syphilis around the 32nd week of pregnancy. Moreover, neonatal care and postneonatal care received by the infant were covered regardless of where they were received.

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[1] Prior to NHI, women in the low-income plan received a cash benefit, but it obviously was very modest. The government did provide maternal and child care for poor women at public hospitals and clinics, but less than 1 percent of households were classified as poor. After NHI became effective, the administration of medical care benefits was transferred to the Bureau of National Health Insurance. The LI, FI, and GI plans continue to administer other types of insurance such as disability, unemployment, and old-age insurance.

After the introduction of NHI, non-government employed households enjoyed benefits similar to those received by government employed households prior to March 1995, with the exception that the cash benefit for childbirth received by private sector households was reduced to one month of salary. Premium costs continue to be shared in the manner described above by employers, employees, and the government, with the government continuing to pay the entire premium for low-income families. Thus, the introduction of NHI constitutes a "natural experiment" in which a large number of previously uninsured pregnant women, infants, and children in a country with a population of over 21 million persons received coverage for prenatal care, delivery, neonatal care, vaccinations, immunizations, and well-baby and child care for the first time. The behavior of this "treatment group" in the pre- and post-NHI periods can be compared to the behavior of the "control group" of government employed households whose health insurance coverage did not change.

B. Literature Review

Studies dealing with the effects of health insurance on infant health that employ experimental or quasi-experimental data are most relevant to our study. These studies deal with the introduction of National Health Insurance in Canada, the introduction of Medicaid in the U.S., and the Medicaid income eligibility expansions in the late 1980s and early 1990s. Detailed reviews of this literature are contained in Gruber (1997, 2000), Kaestner (1999), Currie (2000), Dubay et al. (2001), and Levy and Meltzer (2008). Our goal in this section is to highlight the diversity and lack of consensus of findings in it.

Hanratty (1996) capitalizes on the staggered adoption of NHI by the ten provinces of Canada in the period from 1962 to 1971 to assess its impacts on infant mortality and low birthweight (percentage of infants who weigh less than 2,500 grams) using data for the years 1960-1975. She finds negative effects of NHI on each outcome, with a larger effect for mortality. Decker (1996) capitalizes on the similar staggered adoption of Medicaid by states of the U.S. in the period from 1966 through the early 1970s and finds negative effects for low birthweight in data for the period from 1964 through 1967. One problem with both studies is that they do not control for abortion reform and for the diffusion of oral and intrauterine contraceptive devices. These developments occurred between the mid 1960s and 1970s at rates that differed among states and provinces. Hence, they are not adequately reflected by the national time indicators employed in both studies.[2]

Many studies have explored the impacts of Medicaid reforms that took place between 1984 and 1990. These reforms extended coverage to poor and near-poor pregnant women and children. Most of these studies use a pre- and post-design with data for one or two states and low birthweight as the outcome. Treatment and control groups are defined by identifying women and children whose Medicaid status is most or least likely to be affected by the reforms. Most of them find no effect of the expansions (for example, Piper, Ray, and Griffin 1990; Haas et al. 1993; Piper, Mitchel, and Ray 1994; Ray, Mitchel, and Piper 1997; Epstein and Newhouse 1998). Dubay et al. (2001) argue that these findings may be due to the small incremental changes in Medicaid considered in these studies or because the studies included few women affected by the expansions. Dubay et al. (2001) also point to two fundamental difficulties in conducting a national evaluation of the Medicaid expansions based on well-defined treatment and control groups. In the first place, nationally representative data with information on family income, insurance status, and birth outcomes

[2] Grossman and Jacobowitz (1981) and Corman and Grossman (1985) report that abortion reform and family planning are important predictors of neonatal mortality, although they are not able to control for unmeasured area effects because they employ cross-sectional data.

are lacking. In the second place, the expansions were accompanied by improvements in the eligibility determination process, higher fees, and reimbursement for enhanced prenatal care. These developments were very likely to have affected one potential control group: women already covered by Medicaid.

Dubay et al. (2001) conduct a national before- and after-study by forming race-specific treatment and control groups based on mother's education and marital status for the period from 1980-1986 (before the enactment of broad expansions) and 1986-1990 (after the enactment of these expansions). They find that the expansions caused the rate of low birthweight to decline for some white women of low socioeconomic status (those with less than 12 years of formal schooling). They do not observe similar declines for other white women of low socioeconomic status (those who are unmarried and have less than 16 years of formal schooling) or for any black women of low socioeconomic status. They acknowledge that they are not able to identify treatment and control groups precisely. Moreover, they do not consider the targeted expansions occurring between 1981 and 1984 that Currie and Gruber (1996b) find to be important (see below).

Currie and Gruber (1996b) explore the effects of the expansions in a national study using a time series of states of the U.S. for the years 1979-1992. They do not consider separate treatment and control groups. Instead, they compute the fraction of women ages15-44 eligible for Medicaid by state and year. This is their key regressor in equations explaining low birthweight or infant mortality rates of all babies. They realize that the enrollment measure is endogenous and employ as an instrument for it the eligibility fraction based solely on state rules in effect in a given year. They find that predicted eligibility has negative effects on low birthweight and infant mortality. The mortality reductions are larger in absolute value than the birthweight impacts. They also find much bigger effects for the targeted legislation of the early 1980s aimed at increasing the eligibility of a relatively small pool of women relative to the broader legislation of the late 1980s which was aimed at a much larger pool. Indeed, the broad eligibility effects are not statistically significant. Their explanation is that the broad expansions had much lower take-up rates than the narrow expansions. Kaestner (1999) and Dubay et al. (2001) question these findings because they do not fully account for direction of the Medicaid program at poor and near-poor women. Thus, the expansions should have larger effects the larger is the fraction of poor and near-poor women in a given state.

Currie and Gruber (1996a) report that the expansions lowered the mortality rates of children between the ages of 1 and 14 from internal and external causes considered separately. Currie and Gruber (1997 and summarized in Currie and Gruber 2001) also report that teenage mothers or mothers who dropped out of high school and who live near a hospital with a neonatal intensive care unit experience significant reductions in neonatal mortality relative to older or more educated mothers due to the expansions. The former result is subject to the same criticism made above; it does not fully account for the larger anticipated effect of the expansions on mortality rates of poor and near-poor children. The latter finding is of considerable interest because it employs a treatment-group control-group methodology and includes the fraction of light births as a regressor. It would, however, be more conclusive if the neonatal mortality rate of low-weight infants were the outcome. Neonatal intensive care units are aimed at these infants and should have larger impacts on their mortality rate than on the rate of normal-weight infants.

By employing the Taiwan NHI experiment to evaluate the impacts of health insurance on infant, we circumvent some of the problems encountered in prior research. The treatment and control groups are sharply defined by employment in the private or agricultural sector on the one hand and by employment in the government sector on the other hand. As discussed in Section V, it is difficult to switch sectors. The introduction of NHI was not accompanied by changes in birth control techniques--changes that did occur when Canada adopted NHI and when the U.S. introduced Medicaid. Abortion has been legal in Taiwan since 1984. Households simply had to register with the Bureau of National Health Insurance to obtain coverage, and take-up rates were nearly 100 percent. One problem that we face is that the treatment and control groups have very different characteristics. We outline approaches to deal with this issue in Section V.

III. Conceptual Framework

From the point of view of mothers and families, the introduction of NHI reduces the price of medical care services delivered to pregnant women, newborns, older infants, children, and adolescents who had no or less generous benefits prior to its enactment. If the quantity of these services demanded is inversely related to price, the quantity of services demanded by these groups should rise. If more medical care leads to better health, the groups affected should exhibit improved child health outcomes.

If medical care services are supplied competitively, the nominal price of medical care (the price received by suppliers as opposed to the price paid by consumers) will not rise if the supply function of medical care services is infinitely elastic or if the supply function shifts downward (the quantity supplied at a given price rises) by an amount that is exactly equal to the upward shift in the demand function. Supply shifts are more likely if the government allocated additional resources to the health care sector in the period prior to the enactment of NHI. Even if the government did not pursue this policy, price may not rise because health insurance guarantees the receipt of payment for services rendered. Stability of the nominal price is required for the enactment of NHI to have no impact on the quantity of services demanded by the previously insured. Implications for our estimates of departures from competitive supply or stable nominal prices are discussed in Section V.

The preceding predictions come from a simple model in which parents maximize a utility function that depends on their own consumption and the health of their children. Complications arise, however, if the parents have a more complex utility function. For example, they may care about the number of children in the family as well as the health of each child. NHI lowers both the price of the former and the price of the latter. Leibowitz (1990) finds that women randomly assigned to receive free medical care for three to five years in the RAND Health Insurance Experiment had almost 30 percent more births than women assigned to insurance plans requiring cost-sharing for health services. Joyce, Kaestner, and Kwan (1998) report that the Medicaid income eligibility expansions discussed in Section II raised births and lowered abortions for white unmarried women aged 19-27 who had no more than a high school education. Given interactions between the quantity and quality (measured by health) of children emphasized by Becker and Lewis (1973), one possible outcome is that the optimal number of children rises while the optimal health of each child falls. The amount of medical care received by each child still could rise, but the favorable impact of this development on health could be more than offset by reductions in other health inputs.

A factor that goes in the opposite direction is that the health of infants and young children depends on the time that parents, especially mothers, allocate to pregnancies and to the care of infants and young children. Women in non-government employed households no longer had to work in order to obtain health insurance for themselves as a result of the introduction of NHI in Taiwan. Chou and Staiger (2001) report that women married to non-government employees were less likely to work in the post-NHI period. The end to the link between employment and insurance coverage allows women who face difficult pregnancies to withdraw from work. In addition, women who do not work when their children are very young have more time to allocate to them.

We adopt the hypothesis that NHI improves the health of the children whose medical care costs are affected by its introduction. Offsetting factors associated with increases in the number of children are most appropriately viewed as “second-order effects.” This is a reasonable position because the studies by Leibowitz (1990) and by Joyce, Kaestner, and Kwan (1998) do not distinguish between the timing of births and completed fertility. That is, women may have decided to give birth during the period that they received free care in the RAND experiment or were eligible for Medicaid but may not have altered their completed fertility decisions. Moreover, our preliminary studies find that the introduction of NHI had no effect on fertility in Taiwan. This is not surprising because after NHI became effective, the cash benefit for childbirth was reduced from two months of salary to one month for private sector households, the largest of the two treatment groups. Hence, the price of a birth for that treatment group did not fall by as much as it would have if the subsidy policy did not change.

IV. Data and Sample

Our two major data sources are annual birth and infant death certificates for the years 1990 through 2001. There were more than 300,000 births a year in Taiwan during this period. We link birth and death certificates through national identification numbers received by each person in Taiwan. Health outcomes studied in this paper include the probabilities of neonatal and postneonatal deaths. Neonatal deaths pertain to deaths within the first 27 days of life, while postneonatal deaths pertain to deaths between the ages of 28 and 364 days. We distinguish between neonatal and postneonatal mortality because their causes are very different. Most neonatal deaths are caused by congenital anomalies, prematurity, and complications of delivery, while most postneonatal deaths are caused by infectious diseases and accidents. Infants who die within the first 27 days of life are excluded when the probability of postneonatal death is the outcome.[3]

In addition to birthweight and gestational age, birth certificates contain the following information: place of birth (hospital, clinic, other); gender; parity; mother's town of residence; mother's marital status; mother's age; mother's schooling; father's age; and father's schooling. Mother's and father's occupation and industry also are reported, but there are many missing values.[4] Instead, we create treatment and control groups by matching birth and infant death certificates to annual files that we have obtained from the Central Trust of China and from the Bureau of Labor Insurance for the years 1990-2001. Prior to the March 1995 date on which NHI became effective, the Central Trust of China administered health insurance under Government Employee Insurance (GI), and the Bureau of Labor Insurance administered health insurance under Labor Insurance (LI) and Farmer's Insurance (FI). These two organizations maintained files containing the national identification numbers of all persons with coverage. They continued to maintain these files after March 1995 because they still are responsible for other types of insurance (for example, disability insurance, unemployment insurance, and old-age insurance).[5]

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[3] Birthweight or the probability of a low-birthweight birth is not considered as an outcome for reasons indicated below. Unlike US birth certificates, Taiwanese certificates do not contain such prenatal health inputs and behaviors as the month in which prenatal care began, the number of prenatal visits, and maternal cigarette smoking and alcohol use during pregnancy.

[4] Taiwanese Department of Health officials have informed us that this is because very detailed information is requested.

[5] In cases where no matches are made with the administrative files, we assume that the household would have been

As explained in Section II, infants of parents who did not work in the government sector had much more limited health insurance than those of parents who worked in that sector the period prior to NHI. Hence, our control group consists of births to households in which at least one parent is employed in the government sector. Treatment group I consists of birth to households in which the father works in the private sector and the mother also works in that sector or does not work. Treatment group II consists of birth to households in which the father is a farmer and the mother also is a farmer or does not work. We construct two treatment groups because farmers have lower levels of education and income than private sector workers. These factors may result in different responses to the introduction of NHI by the two groups.

There are 3,548,321 births without missing values on parental education, ages of mothers and fathers, birth date of child, and parity from 1990 through 2001. We restrict our analysis file to births of infants whose mothers were between age 15 and 45 and fathers were between age 15 and 65, which results in 3,543,389 births. After deleting births that could not be matched to any insurance administrative files, we are left with 3,471,044 observations. Finally, we delete multiple births and births to unmarried women.[6] These restrictions result in a sample of 3,340,695 from 1990 through 2001 or 94.15 percent of all births in that period. Of this sample, 384,622 births (11.51 percent) were in government-employed households; 2,665,442 (79.79 percent) were in private sector households; and 290,631 (8.70 percent) were in farm households.

Figures 1 and 2 show trends in neonatal and postneonatal mortality rates from 1990

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eligible for Low-Income Households' Health Insurance or would have been eligible for government-provided maternal and child health care at public clinics. Only 0.5 percent of all persons had Low-Income Households' Health Insurance in 1992, and only 0.9 percent of the population were members of low-income households in 2000 (Chou, Liu, and Hammitt 2003). The type of care delivered to low income-infants may differ in important respects from that received by other children in either the treatment or control groups. Moreover, some infants and children may be incorrectly classified as members of low-income families because self-employed workers could only obtain labor insurance if they were members of an occupational union.

[6] Births to unmarried women are very rare in Taiwan.

through 2001 by treatment status. Before discussing these trends, we want to mention two developments in addition to NHI that occurred during our sample period. The most important of these is the requirement effective October 1994 that mandated child delivery institutions to report births directly to household registration offices (the source of our birth and death certificates) and to health authorities. Throughout our sample period, parents required a birth certificate completed by a physician or another hospital official to obtain national identification numbers for their infants. But prior to October 1994 some parents did not bother to report births to household registration offices if their infants died within the first year of life. Clearly, low-weight births resulting in neonatal deaths were most likely to go unreported. As of October 1994 delivery institutions were required to send birth certificates to household registration offices and to health authorities. Hence, the percentage of low-weight births and the neonatal mortality rate all rose after 1993 (Department of Health 1996 and personal communication with Pau-Chung Chen, M.D., of the National Taiwan University Medical College).

The second development is that effective July 1992 children of government workers received health insurance coverage. This did not affect coverage for neonatal care services because prior to that date mothers covered by GI filed claims for these services under their own national identification numbers. But after that date, benefits had to be claimed under infants' national identification numbers. Hence, the control group had more incentives to report low-weight births to household registration offices than the treatment group in the period between July 1992 and October 1994.

These two developments are clearly reflected in the trends in Figure 1. The neonatal mortality of the control group increased from 1.7 per thousand births in1991 to 2.2 per thousand births in 1992 and remained high in 1993. The neonatal mortality of the treatment groups increased drastically after 1994. For treatment group 1 (private sector households), the neonatal mortality increased from 1.4 per thousand births in 1993 to 1.8 in 1994 and 2.8 in 1995. For treatment group 2 (farm households), the neonatal mortality increased from 0.9 per thousand births in 1993 to 1.6 in 1994 and 3.1 in 1995.

According to Figure 2, the 1992 and 1994 developments did not affect the postneonatal mortality rate. Before the introduction of NHI, the control group had the lowest postneonatal mortality rate, followed by private sector households and then by farm households. After NHI, all groups experienced a downward trend in postneonatal morality. The lack of an impact in the changes in reporting on the postneonatal mortality rate is not surprising. That rate is defined as

$$P = D_{28\text{-}364}/(B - D_{1\text{-}27}), \qquad (1)$$

where $D_{28\text{-}364}$ is the number of postneonatal deaths, B is the number of births, and $D_{1\text{-}27}$ is the number of neonatal deaths. The more stringent reporting requirements raised both B and $D_{1\text{-}27}$, but should have had little or no effect on $D_{28\text{-}364}$. If B and $D_{1\text{-}27}$ increased by the same amount, the denominator of equation (1) would not change, leaving P unaffected.

Based on our preliminary analysis of the effect of NHI on neonatal mortality using the methodology outlined in Section V (available on request) and the trend in Figure 1, we have concluded that it is not possible to sort out the impact of NHI on neonatal mortality from the impact of the more stringent 1994 reporting requirement on this outcome. Therefore, we focus on the postneonatal mortality rate in the remainder of this paper. To minimize any impacts of the 1994 development on our estimates, we exclude births in 1994 and 1995. This exclusion also is desirable because NHI was introduced on March 1, 1995, making 1996 the first full year after its introduction. Thus, our before-NHI period is 1990-1993 and after-NHI period is 1996-2001. This restriction results in a sample of 2,753,860 births of infants who survived the first 27 days of life. Of these 313,606 (11.39 percent) were in government households; 2,201,663 (79.95 percent) were in private sector households; and 238,591 (8.66 percent) were in farm households.[7]

Table1 presents summary statistics for household characteristics by treatment group/control group status. The variables in the table are employed in the regression analyses in Section VI. Parents in the two treatment groups are somewhat younger than those in the control group, and there are more third and higher order births in the former two groups. The most significant difference in the groups, however, is in education levels. In the government households, 85.63 percent of mothers and 92.95 percent of fathers had 10 or more years of schooling. In private sector households, 57.78 percent of mothers and 59.75 percent of fathers obtained that much education. The corresponding figures in farm households are 45.97 percent and 45.58 percent, respectively. Since education is a major proximate determinant of age at birth and family size, these differences explain why the treatment group women are younger when they give birth and have more higher-order births. As discussed in more details below, we will run regressions by mother's education level so that treatment and control groups will be more comparable.

V. Empirical Strategy

A. Basic Strategy

To estimate the effects of NHI on infant health outcomes, we capitalize on its differential impact on the coverage and benefits of government workers and their dependents compared to other workers and their dependents. As explained in Section II, the former group had the same coverage and benefits before and after the adoption of NHI, while the latter group did not. With modifications discussed below, families with a government worker serve as the control group,

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[7] To control for the increased incentives of government workers to report births as of July 1992, we experimented with excluding births from that month through the end of 1993. That resulted in a very small control group sample in the period before the introduction of NHI and did not change the results reported in Section VI.

and families with no government employees serve as the treatment group in a difference-in-differences (DD) estimation strategy. Health outcomes in the control group should not be affected by the introduction of NHI, while health outcomes in the treatment group should be affected. Unobserved trends may, however, impact outcomes in both groups. Examples of unmeasured time effects include advances in neonatology, diffusion of these advances, and changes in the level of air pollution.

The DD methodology assumes that unobserved effects are the same for the two groups. Hence the change in a health outcome in the treatment group before and after the introduction of NHI minus the corresponding change for the control group provides an estimate of the impact of the legislation on this outcome. The simple DD estimator can be expressed as:

$$\Delta^{NHI} = (H^{After\ NHI}_{Treatment} - H^{Before\ NHI}_{Treatment}) - (H^{After\ NHI}_{Control} - H^{Before\ NHI}_{Control}), \qquad (2)$$

where Δ^{NHI} represents the effect of NHI, and H is a health outcome (the probability of dying in the postneonatal period) for the treatment and control groups before and after NHI, as indicated by the subscripts and superscripts, respectively. Within-group differences in H before and after the introduction of NHI [$(H^{After\ NHI}_{Treatment} - H^{Before\ NHI}_{Treatment})$, $(H^{After\ NHI}_{Control} - H^{Before\ NHI}_{Control})$] control for time-invariant effects that are unique to each group. The difference-in-differences given by equation (2) controls for time-varying effects that are common to both groups. The identifying assumption of the DD estimator is that time-varying factors affect both groups equally.

The difference-in-differences estimator can be expressed within a regression framework. This allows one to net out the effects of other factors that may affect health outcomes, may be correlated with membership in the treatment or control group, and may change over time at different rates for the two groups. In situations in which only the first two factors are relevant, one obtains more efficient DD estimates (estimates with smaller standard errors) in a multiple regression context than the estimates given by equation (2). Suppose that data on births occurring between January 1990 and December 1993 and between January 1996 and December 2001 in the treatment and control groups are pooled, the following regression is estimated:

$$\begin{aligned} H_{ijt} &= \alpha_0 + \alpha_1 NHI_t + \alpha_{21} PRI_{ijt} + \alpha_{22} FRM_{ijt} + \alpha_{31} NHI_t * PRI_{ijt} + \alpha_{32} NHI_t * FRM_{ijt} \\ &\quad + \alpha_4 Z_{ijt} + f_j + f_t + \varepsilon_{ijt}, \end{aligned} \quad (3)$$

where i indexes births in households, j denotes one of the 24 cities or counties in Taiwan, and t indexes years. The dependent variable (H_{ijt}) in this equation is the health outcome of an infant born to a woman in household i residing in city or county j in year t. The independent variables are an indicator variable for the period after implementation of National Health Insurance (NHI_t); indicator variables for private sector households (PRI_{ijt}) and farm households (FRM_{ijt}); a vector of observable household characteristics (Z_{ijt}); county and year fixed effects (f_j and f_t, respectively),[8] and a random error term (ε_{ijt}). The effect of NHI in equation (2) on treatment group k (k = 1 or 2) can be expressed as

$$\Delta^{NHI} = [(\alpha_1 + \alpha_{2k} + \alpha_{3k}) - \alpha_{2k}] - [\alpha_1 - 0] = \alpha_{3k}. \quad (4)$$

The coefficient α_{3k} on the interaction between NHI_t and the treatment group dummy measures the difference-in-differences defined in equation (1) for treatment group k.

B. NHI and the Medical Technology

To explore a plausible mechanism via which the legislation may have affected the infant survival, we modify the model to take account of the positive effects of NHI on the adoption and use of state-of-the-art medical technology reported by Chou, Liu, and Hammitt (2004). Our

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[8] Large cities are separate local entities. Hereafter we use the term county to refer to both cities and counties. Our regressions include 10 years: 1990-1993 and 1996-2001. Since we include an indicator for the period after the introduction of NHI (1996-2001), equation (3) contains 8 year indicators.

specific hypothesis, which is similar to the one proposed and explored by Currie and Gruber (1997), is that NHI will have a larger impact on the survival prospects of treatment infants if their mothers live near to a hospital with more advanced technology and that this effect is larger in the period after NHI became effective for the treatment group. Put differently, both financial considerations and distance rationed intensive care services received by infants in the treatment group prior to NHI, but only distance rationed these services in the period after NHI. We broaden this hypothesis to include the possibility that NHI effects are larger in areas with more medical care services in general.

Denote a measure of intensive care availability or medical care availability as Q_{ijt}, measured in the county in which a given household resides. Then we estimate the following equation

$$\begin{aligned} H_{ijt} = {} & \beta_1 NHI_t + \beta_2 Treat_{ijt} + \beta_3 NHI_t * Treat_{ijt} + \beta_4 Q_{ijt} + \beta_5 NHI_t * Q_{ijt} + \beta_6 Treat_{ijt} * Q_{ijt} \\ & + \beta_7 NHI_t * Treat_{ijt} * Q_{ijt}, \end{aligned} \quad (5)$$

where j indexes counties, one treatment group, denoted by $Treat_{ijt}$, is assumed, and other determinants are suppressed.[9] Note that the model specified by equation (5) in which a measure of the availability of intensive care in the mother's area of residence interacts with the introduction of NHI incorporates an important correlate of the introduction and diffusion of new technology. Hence, not all of the impacts of technology are assumed to be captured by time effects.

The equation contains a natural measure of the impact of NHI:

$$\beta_3 + \beta_4[(Q_{ka}-Q_{kb}) - (Q_{ca}-Q_{cb})] + \beta_5(Q_{ka}-Q_{ca}) + \beta_6(Q_{ka}-Q_{kb}) + \beta_7 Q_{ka}, \quad (6)$$

[9] With two treatment groups, equation (5) includes indicators for each group, interactions between each indicator and the NHI indicator, interactions between each indicator and Q_{ijt}, and interactions between the NHI indicator, Q_{ijt}, and each of the two treatment group indicators.

where Q_{kb} and Q_{ka} are the mean values of Q for the treatment group (denoted by the subscript k) in the period before and after NHI, respectively, and Q_{cb} and Q_{ca} are the mean values of Q for the control group in the period before and after NHI, respectively. Some persons may object to the comprehensive nature of that measure and may prefer the more narrowly defined one given by $\beta_3 + \beta_7 Q_{ka}$. The latter estimate, however, assumes that trends in Q and its differential impacts on the treatment and control groups are unrelated to the introduction of NHI. Moreover, suppose that β_5, β_6, β_7, and $[(Q_{ka}–Q_{kb}) – (Q_{ca}–Q_{cb})]$ are zero. Does that rule out the adoption and use of state-of-the-art medical technology as a mechanism via which the introduction of NHI affected infant health? The answer is no because in the absence of this development, Q might have changed at a slower rate for the treatment group than for the control group. Indeed, there might have been no growth for the former group. Given these considerations and the high degree of collinearity among the last four variables in equation (5), we use the broad estimator of the impact of NHI given by equation (6)

We employ three alternative measures of Q at the county level divided by the county population: the number of hospitals with a pediatric department, the number of hospitals with an obstetrical department, and the total number of infant hospital beds. We aggregated individual hospital data to the county level from Hospital Registry Files and Hospital Service Files collected and maintained by the Department of Health. While none of these is a direct measure of neonatal intensive care availability or the adoption of state-of-the-art medical technology used to treat infants, each of the variables just defined is likely to be positively correlated with that measure. Moreover, they are likely to be highly correlated in a positive manner with the number of physicians who provide medical care services to infants during their first year of life.

C. NHI and the Medical Care Market

The introduction of NHI may affect the nominal price of medical care and other components of the full price. In turn, these developments may have different effects on the treatment and control groups. To be specific, the increased demand for medical services by the treatment group after the introduction of NHI will cause the nominal price of medical care services to rise if the supply function slopes upwards. This will cause the net price paid by the control group (the nominal price minus the amount paid by insurance) to rise at the same time as the net price paid by the treatment group falls. Under these circumstances, the DD estimates the sum of the increased utilization by the treatment group and the absolute value of the reduced utilization by the control group. Put differently, it may overestimate the increased utilization by the treatment group.

In Section III we pointed out conditions under which the nominal price would remain constant in the context of a competitive model of supply. In other situations the nominal price will rise, and the quantity of services demanded by the control group will fall. In situations in which the supply function is stable and upward sloping or in the short run perfectly inelastic, this will occur even if the government fixes the nominal price by fixing reimbursement rates. In that situation, demand will be rationed by non-price mechanisms. These could take the form of longer waiting times in providers' offices and longer appointment delays.

If one allows for departures from the assumptions of the competitive model, other outcomes are possible. For example, suppose physicians or hospitals have monopoly power and treat both insured and uninsured patients in the pre-NHI period. The reimbursement rate for the former group of patients is fixed by the insurer, but the provider's uninsured patients have a downward sloping demand function. Then the provider will charge a higher price to the uninsured patients (Feldstein 1993). With NHI, the reimbursement rate is the same for all patients; the provider loses his or her monopoly power; and the optimal number of patients treated remains the same. Of course, the increased demand by the previously uninsured patients still will generate upward pressure on the nominal price or reimbursement rate given an upward sloping market supply function. If the reimbursement rate does not adjust, then the indirect cost component of medical care generated by waiting and travel time and appointment delays will rise.

More complicated models contain different predictions. McGuire (2000) outlines a model of imperfect competition in which providers may have incentives to raise output in response to a reduction in the regulated price. The same effect could be observed in a model in which providers are in perfect competition but maximize utility instead of profits. Chiang (1997) argues that this actually occurred in Taiwan after the introduction of NHI: physicians could no longer under-report their income when they filed tax returns because almost all of it is derived from the government.

The real price of medical care in Taiwan (the medical care component of the Consumer Price Index divided by the CPI for all goods) rose at an average annual rate of 1.4 percent per year between 1994 and 2000, while there was no trend in this price in the six years before NHI was enacted (Executive Yuan 2002). The stability in the earlier period and modest increase in the latter period reflects in part policies pursued by the government to increase the supply of hospitals and physicians since the early 1980s (Chiang 1997),

In summary, our study pertains to a country that adopted NHI following growth in resources in the health care sector. The real price of medical care rose, although at a modest rate, in the post-NHI period. The assumption that the downward shift in the supply function offset the upward shift in the demand function may not be unreasonable. If it is or if time costs increased, the DD is an overestimate in one sense because utilization by the control group falls due to NHI. But in another sense the DD relative to the simple difference for the treatment group approximates the effect that would be observed if the full price of medical care remains the same. That is, the price reduction experienced by the treatment group is smaller than the one that they would experience if the market continues to clear with no change in the nominal money price or in the time price. While the DD may be larger than the simple difference, it is not necessarily larger than the DD that emerges when the full price paid by the previously insured remains the same.

To spell out the preceding point in more detail, first consider a simple competitive model with a rising supply function and no time costs. Suppose that the enactment of NHI reduces the net price of medical care for the treatment group from P_0 (the nominal price in the period before enactment) to cP_1, where c is the coinsurance rate ($0 < c < 1$) and where $P_1 > P_0$. At the same time, the net price rises from cP_0 to cP_1 for the control group. Given a linear demand function for medical care (M) with a net price coefficient of $\beta < 0$, changes in M for the treatment and control groups are

$$\Delta M_k = \beta(cP_1 - P_0) \quad (7)$$

$$\Delta M_c = \beta c(P_1 - P_0), \quad (8)$$

where time effects are suppressed. Hence the DD is

$$\Delta M_k - \Delta M_c = \beta P_0(c - 1). \quad (9)$$

Here the DD is the same as the one that would be observed in a model in which the nominal price remains the same ($P_1 = P_0$).

Now introduce a time cost of W per visit that differs by period and by group. Equation (9) becomes

$$\Delta M_k - \Delta M_c = \beta P_0(c - 1) + \beta(\Delta W_k - \Delta W_c). \quad (10)$$

Since the control group has higher income than the treatment group, a given change in waiting time causes a greater change in the time cost for the former. On the other hand, the treatment group is likely to reside in areas with fewer physicians, making a greater change in waiting time more likely for them. Equations (9) and (10) are the same if these two factors offset each other, so that time costs increase at the same rate for both groups. In other situations (9) and (10) are not the same, but it is not clear which is larger.

The above analysis can be modified to introduce a time cost per visit that differs by period and by group and to allow the net price parameter (β) to differ. Empirically, we will examine how the DD varies by education. By comparing responses of treatment and control group members with the same education levels, we can be more confident that the groups have similar net price parameters in the demand function for medical care. This strategy also reduces differences between the characteristics of the treatment and control groups.

D. NHI and the Labor Market

The research design that we have outlined may be compromised by potential changes in the composition of the control and treatment groups in response to the introduction of NHI. To be concrete, parents who anticipate a poor birth outcome or who have sick infants or children or parents with a propensity to avoid risk or engage in a variety of healthy behaviors are more likely to obtain health insurance. Prior to the enactment of NHI, that was possible only if they were in the government sector. After the enactment of NHI, the motive to enter the government sector for this reason no longer exists. Consider two equations: one determining membership in the treatment group (the private or agricultural sector) and one determining health outcomes of infants or children. The model that has just been sketched out suggests that there is a time-varying unobservable that partially determines both the health outcome and membership in the treatment group.

A key assumption of our methodology is that the composition of the treatment and control groups does not change during the sample period. Incentives for these changes do exist. For example, persons may have switched from the government sector to the industry sector after NHI because employment in the former sector no longer was required to obtain more generous health insurance. To cite another example, women who married men employed in the government sector to obtain health insurance before NHI might choose to marry men employed in the industry sector or not to marry at all after NHI. To cite a final example, government sector wives with industry sector husbands might quit their jobs after NHI became effective.

While the incentives just mentioned do exist, a variety of factors suggest that they will have small impacts on the composition of the treatment and control groups, especially the latter, in our sample period. It is difficult to transfer from the industry or agricultural sectors to the government sector because government workers who are civil servants must pass very demanding examinations. Government workers are better paid and enjoy more fringe benefits than industry workers and have little incentive to leave that sector.

Chou and Staiger (2001) report that turnover (both entry and exit) was very low in the government sector from 1980 through the 1997 and changed little during this period.[10] In addition, Chou and Staiger (2001) find little systematic changes in such characteristics of households as husband's and wife's education and family income between 1992 and 1997 in government-employed households compared to non-government-employed households. Chou and Staiger also find that women married to non-government employees were less likely to work

[10] In the more recent period of 1992 through 2000, the entry rate ranged between 2.2 and 3.8 percent, while the exit rate ranged between 1.2 and 2.2 percent (Central Trust of China 2002). Entry and exit rates were almost equal in the period before NHI, while the latter exceeded the former in the period after NHI.

in the post-NHI period, but this does not affect the composition of the treatment group. Finally, women who were eligible for Government Employee Insurance prior to NHI because they worked in the public sector remain in the administrative records of the Central Trust of China even if they quit their jobs after the enactment of NHI. This is because they are still eligible for old-age insurance through their previous employment. Births to these women in the post-NHI period are included in the control group.

It is possible that some women married government employees prior to 1995 to get health insurance. Given, however, that these employees account for only about 10 percent of all paid employees, the likelihood of finding a mate was small. Moreover, Yelowitz (1998), Grogger and Bronars (2001), and Kaushal and Kaestner (2001) report very small effects of Medicaid expansions and welfare reform on marital status in the U.S.

Most of these factors pertain to the stability of the government sector. While turnover in the government sector was stable in the period covered by the application (1990-2001), private sector employment and the ratio of private sector to public sector rose over time. This is not a new phenomenon. It has been taking place since at least 1980, while NHI became effective in 1995 (Executive Yuan 2001). It does suggest, however, changes in unmeasured characteristics of the treatment group over time. For example, given the growth of industries requiring computer-intensive skills (high tech industries), skill levels in the private sector may have risen over time in ways that are not fully captured by years of formal schooling completed (an observed variable in our analysis). This unmeasured variable may be correlated with propensities to avoid risk or engage in healthy behaviors, or with unobserved health endowments of infants or children. We take three approaches to address this problem.

First, we obtain separate regressions for households with similar levels of education, as indicated in Section V.C. This procedure makes the treatment and control groups more comparable in terms of measured characteristics over time.

Second, we include the interactions between year and county of residence in the difference-in-differences regression. Those interactions capture shifts in the demand for labor that may affect the composition of the treatment group. If including those interaction terms does not affect our findings, it implies that changes of unmeasured characteristics of the treatment group over time may not be important.

Third, we obtain estimates of equation (3) including county-year interactions weighted by the propensity scores (Imbens 2004). The estimated probability (propensity score) of belonging to treatment group 1 (private sector households) or treatment group 2 (farm households) compared to the control group is estimated as a logistic function of observed characteristics including mother's and father's years of schooling and ages. We assign individual weights equal to the probability of belonging to the opposite insurance status group (as in McWilliams et al. 2003). For all observed characteristics, these propensity weights appear to balance the control and treatment groups extremely well. Since propensity scores are estimated separately for each treatment group, we obtain two weighted regressions. The first includes government households and private sector households, while the second includes government households and farm households.

VI. Empirical Results

Table 2 contains postneonatal mortality rates (deaths per thousand survivors of the neonatal period) before and after the introduction of NHI for the treatment and control groups. It also contains the simple difference-in differences estimator given by equation (2). Before NHI, government employees, the control group, had the lowest postneonatal mortality followed by private employees and then by farmers. The simple DD implies that the introduction of NHI decreased the postneonatal mortality rate by 0.02 deaths per thousand private sector infants who survived the neonatal period and by 0.65 deaths per thousand farm infants who survived the neonatal period. The former reduction is not statistically significant, but the latter is significant at the 5 percent level on a one-tailed test.[11]

For reasons outlined in Section V, we conduct separate analyses for infants born to mothers with less than 10 years of formal schooling and for infants born to mothers with 10 or more years of formal schooling. Moreover, it is plausible that the impacts of increases in medical care on infant survival are larger for infants who are in poor health at birth than for other infants. Therefore, we also examine mortality outcomes for pre-term infants (gestation age of less than 37 weeks), full-term infants (gestational age of 37 weeks or more), low-weight infants (birthweight less than 2,500 grams), and normal-weight infants (birthweight of 2,500 grams or more). Table 3 contains data for each of these six groups that are comparable to the data in Table 2. The largest absolute decreases in postneonatal mortality for the treatment groups after NHI are observed for pre-term and low-weight infants. The simple DD implies that NHI decreased the postneonatal mortality rate by 1.25 deaths per thousand private sector pre-term infants who survived the neonatal period and 6.82 deaths per thousand farm pre-term infants who survived the neonatal period. Only the latter is significant at the 5 percent level.

Table 4 contains regression estimates of equation (3). To make the DDs that emerge from the regressions comparable to those in Tables 2 and 3, we fit linear probability models and multiply all regression coefficients by 1,000. The t-ratios associated with regression coefficients

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[11] In the case of each regression coefficient on which we focus, the alternative hypothesis is that the coefficient is negative. Therefore, we perform one-tailed tests.

in the table are computed from standard errors that adjust for clustering by treatment group/control group status and year, which results in 30 clusters.

Six of the seven DDs are positive for private households, but none of them is statistically significant. The one negative effect--a reduction of 0.19 births per thousand survivors--is small and imprecisely estimated. The story is quite different for the postneonatal mortality rate of infants born in farm families. Here all seven DDs are negative, two are significant at the 5 percent level, and three are significant at the 10 percent level. Moreover, in assessing statistical significance, one should keep in mind that births in farm families constitute a small percentage of all births.

Some of the negative coefficients are quite substantial in magnitude, both in absolute and percentage terms. For the sample as a whole, the introduction of NHI lowered the postneonatal mortality rate of infants born to the wives of farmers by 0.48 deaths per thousand survivors. This is a reduction of 12.90 percent relative to the mean in the pre-NHI period of 3.72 deaths per thousand survivors. Larger absolute reductions are observed for infant born to mothers with less than 10 years of education. Especially large effects are observed for pre-term and low-weight infants. In the case of prematurity, the reduction amounts to 6.00 deaths per thousand survivors or to 35.90 percent relative to the pre-NHI mean of 16.71 deaths per thousand survivors.[12]

Table 5 contains estimates of a model that allows the effects of the introduction of NHI on postneonatal mortality to interact with the availability and diffusion of medical technology and medical care resources. We accomplish this by obtaining alternative estimates of equation (5) with each of the three medical care inputs (Q) described in Section V.B. Panels A, B, and C

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[12] NHI may have had no effects in private sector households because the childbirth subsidy received by these households was reduced from two months of salary to one month as explained in Section III. Other explanations are discussed in Section VII.

contain estimates in which Q is given by the per capita number of hospitals with an obstetrical department, the per capita number of hospitals with a pediatric department, and the per capita number of infant hospital beds, respectively. Since these inputs are measured at the county level and interact with the NHI indicator, t-ratios are computed from standard errors that adjust for clustering by county and before or after the introduction of NHI. This results in 48 clusters. As in Table 4, we fit regressions for the whole sample and for each of the six subgroups employed in that table. In addition to reporting the coefficients of key variables, we also report the impact of NHI on farm households that emerge from equation (6) because previous results and those in Table 5 suggest that NHI had no significant impact on private households.

Regardless of the measure of medical technology or resources employed, the estimates of equation (6) follow the same pattern as the estimates of the NHI effects in Table 4. All seven estimates are negative in each of the three panels in Table 5. In panel A, one of the estimates is significant at the 1 percent level; one is significant at the 5 percent level; and one is significant at the 10 percent level. In panels B and C, the pattern of statistical significance is the same, except that an additional coefficient is significant at 10 percent. Note that, in assessing statistical significant, one should keep in mind that they are obtained from coefficients of highly correlated variables.

The magnitudes of the NHI effects in Table 5 are very similar to those in Table 4. For the sample as a whole, the reduction of 0.48 deaths per thousand in each panel is exactly the same as the reduction in Table 4. As is the case in Table 4, compared to the sample as a whole and to the corresponding subgroups, the effects are bigger for infants of less educated mothers, for pre-term infants, and for low-weight infants. The largest effects occur for pre-term infants. They range from a reduction of 5.97 deaths per thousand survivors in panel B to 6.02 deaths per thousand in panel C. The former figure translates to a 35.73 percent decline relative to the pre-NHI mean of

16.71 deaths per thousand survivors, while the latter translates to 36.02 percent decline.

For the three measures of medical inputs, only two of the twenty-one coefficients of the triple interaction (NHI_t*FRM_{ijt}*Q_{ijt},) are not negative. Hence, the negative impact of NHI on the postneonatal mortality rate of infants in farm families rises in absolute value as medical inputs in the infant's county rise.

In Table 5, almost of all the coefficients of the interaction between the indicator for farm households and the indicator for the period after NHI are insignificant. This suggests that the impact of NHI on postneonatal mortality operates entirely through medical inputs. Therefore in Table 6, we exclude (NHI_t*$TREAT_{ijt}$) from equation (5) to reduce collinearity problems. All twenty-one coefficients of the triple interaction (NHI_t*FRM_{ijt}*Q_{ijt}) are negative in Table 6, while two are positive in Table 5. More importantly, eleven of these coefficients are significant in Table 6, compared to only two in Table 5. The coefficients are largest in absolute value and statistically significant at the 5 percent level for pre-term infants. The estimated effects of NHI given by equation (6) in the two tables are very similar.

In Table 7, we subject our findings to sensitivity analyses and specification checks. We first add county-year interactions to the regressions in Table 4 (see panel A) and then estimate propensity score weighted regressions (see panel B). The specification with county-year interactions is an attempt to examine the sensitivity of the results to changes in the composition of the treatment groups and control groups over time due in part to shifts in the demand for private sector workers at a differential rate among counties. The coefficients and t-ratios in Panel A are almost identical to the corresponding coefficients and t-ratios in Table 4. This suggests that our findings do not reflect changes in unobserved characteristics of the treatment and control groups over time.

The propensity score weighted regressions in panel B balance the control group (government households) and the treatment group (private sector or farm households) on observed characteristics. These regressions still suggest no effects of NHI for private sector households but negative and significant effects for farm households. Except for low-birth weight infants, the coefficients in panel B are larger in absolute value and more precisely estimated than those in Table 4. For example, the NHI effect in the whole sample amounts to a reduction of 0.66 deaths per thousand survivors in panel B of Table 7 compared to a reduction of 0.48 deaths per thousand survivors in Table 4. To cite another example, the effect for premature infants is a reduction of 8.05 deaths per thousand survivors in panel B, which exceeds the reduction in Table 4 by slightly more than 2 deaths per thousand survivors. If anything, the estimates in Table 4 are conservative lower-bound ones. But note that the same pattern emerges in the two tables. The effects are bigger for the infants of women with less than 10 years of schooling, for pre-term infants, and for low-weight infants.

VII. Discussion

Our results suggest that the introduction of National Health Insurance in Taiwan in 1995 led to reductions in the postneonatal mortality rate of infant born in farm households but not to infant born in private sector households. Due to more stringent requirements for reporting births introduced in 1994, we are not able to ascertain the impact of NHI on the neonatal mortality rate. That is because both the reported number of births and the reported number of neonatal deaths rose. Since births are much larger than neonatal deaths, the latter rose much more in percentage terms than the former, resulting in an increase in the neonatal death rates, especially in the two treatment groups affected by the introduction of NHI.

In the pre-NHI period, the postneonatal mortality rate of farm infants was approximately 23 percent higher than the corresponding rate of private sector infants. Hence, our findings are consistent with the notion that the provision of health insurance to previously uninsured infants has larger effects on those born in poor health than on others. For farm infants, we also observe bigger effects when mothers have less than 10 years of education and when infants are premature or low-weight at birth. Our result that the effects of NHI rise in absolute value as the availability of medical care inputs in the infant's county of residence rises is evidence that increases in medical care services received by infants made eligible for insurance coverage by NHI account for the improvements in health outcomes that we observe. Farm families have lower levels of education and income than private sector families and premature and low-weight infants are in worse health than other infants. Thus, taken as a set, our findings suggest that health insurance improves infant health outcomes of population subgroups characterized by low levels of education, income, and health.

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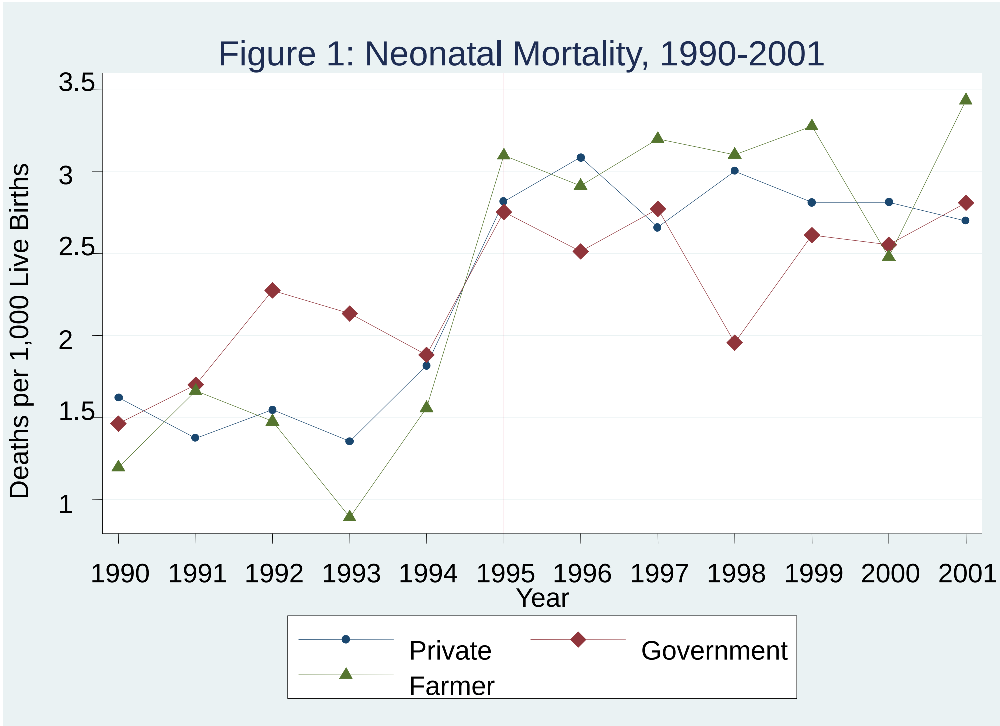

Figure 1: Neonatal Mortality, 1990-2001

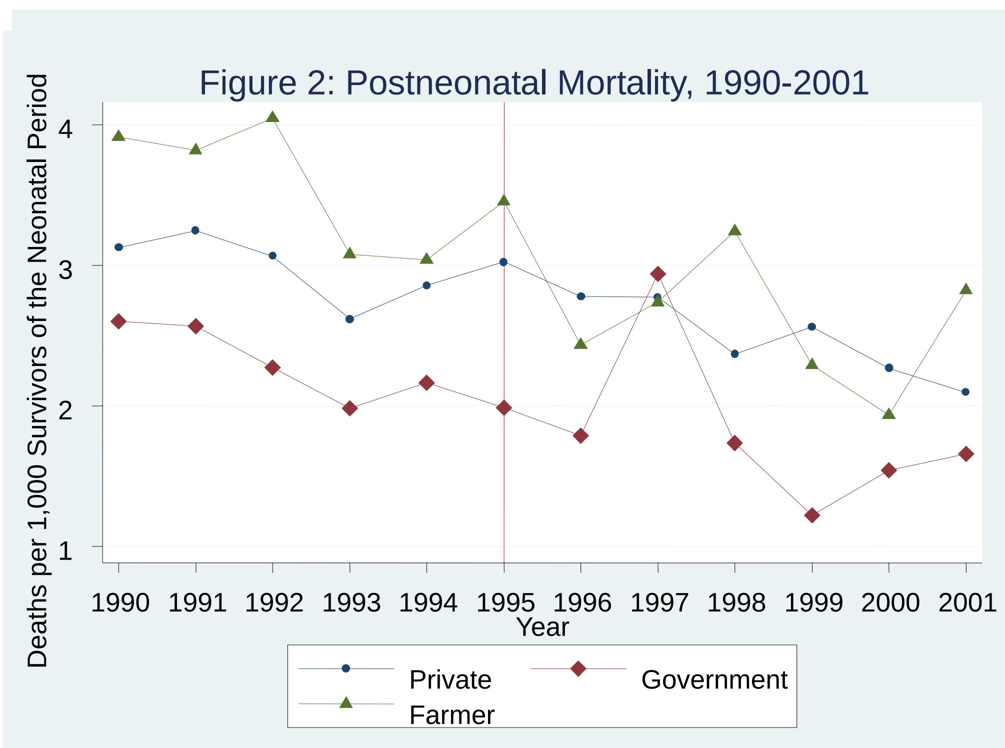

Figure 2: Postneonatal Mortality, 1990-2001

Table 1: Means and Standard Deviations of Independent Variables by Group

	Whole Sample		Control Group Government Employees		Treatment Group I Private Employees		Treatment Group II Farmers	
	Mean	(Std. Dev.)	Mean	(Std. Dev.)	Mean	(Std. Dev.)	Mean	(Std. Dev.)
Mother's Age Dummies								
21-25	26.56%	(44.17%)	11.56%	(31.97%)	27.84%	(44.82%)	34.52%	(47.54%)
26-30	41.86%	(49.33%)	45.33%	(49.78%)	41.64%	(49.30%)	39.34%	(48.85%)
31-35	21.24%	(40.90%)	33.97%	(47.36%)	20.07%	(40.05%)	15.38%	(36.08%)
36-40	4.59%	(20.93%)	7.68%	(26.62%)	4.30%	(20.30%)	3.17%	(17.53%)
41-45	0.43%	(6.54%)	0.69%	(8.27%)	0.40%	(6.32%)	0.35%	(5.89%)
Father's Age Dummies								
21-25	10.49%	(30.65%)	4.01%	(19.61%)	11.09%	(31.40%)	13.52%	(34.19%)
26-30	37.08%	(48.30%)	30.03%	(45.84%)	37.58%	(48.43%)	41.75%	(49.32%)
31-35	34.69%	(47.60%)	43.41%	(49.56%)	33.94%	(47.35%)	30.17%	(45.90%)
36-40	12.97%	(33.60%)	18.06%	(38.47%)	12.54%	(33.12%)	10.25%	(30.33%)
41-45	2.76%	(16.38%)	3.68%	(18.83%)	2.68%	(16.14%)	2.30%	(15.00%)
46+	0.70%	(8.37%)	0.71%	(8.41%)	0.69%	(8.30%)	0.80%	(8.89%)
Mother's Education Dummies								
Junior High School	31.41%	(46.42%)	12.06%	(32.56%)	32.97%	(47.01%)	42.43%	(49.42%)
Senior High School	42.44%	(49.42%)	34.28%	(47.46%)	43.83%	(49.62%)	40.32%	(49.05%)
Junior College	6.34%	(24.36%)	16.18%	(36.83%)	5.37%	(22.54%)	2.35%	(15.14%)
College	10.56%	(30.74%)	31.86%	(46.59%)	8.32%	(27.62%)	3.27%	(17.79%)
Graduate School	0.59%	(7.66%)	3.31%	(17.90%)	0.26%	(5.12%)	0.03%	(1.83%)
Father's Education Dummies								
Junior High School	31.11%	(46.30%)	6.05%	(23.84%)	32.93%	(47.00%)	47.26%	(49.92%)
Senior High School	39.63%	(48.91%)	30.29%	(45.95%)	40.90%	(49.17%)	40.20%	(49.03%)
Junior College	8.92%	(28.50%)	18.44%	(38.78%)	8.20%	(27.43%)	3.03%	(17.14%)
College	12.02%	(32.52%)	35.45%	(47.84%)	9.73%	(29.64%)	2.30%	(14.98%)
Graduate School	1.74%	(13.08%)	8.77%	(28.29%)	0.92%	(9.56%)	0.05%	(2.14%)
Parity								
1st Child	43.93%	(49.63%)	45.67%	(49.81%)	44.35%	(49.68%)	37.73%	(48.47%)
2nd Child	37.07%	(48.30%)	40.66%	(49.12%)	36.76%	(48.21%)	35.19%	(47.76%)
3rd+ Child	15.40%	(36.10%)	12.16%	(32.69%)	15.29%	(35.99%)	20.68%	(40.50%)
Other Characteristics								
Mother with ≥ 10 Years of Schooling	59.93%	(49.00%)	85.63%	(35.08%)	57.78%	(49.39%)	45.97%	(49.84%)
Premature Infant	4.93%	(21.65%)	4.70%	(21.17%)	4.95%	(21.69%)	5.02%	(21.85%)
Low-weight Infant	4.44%	(20.60%)	3.59%	(18.61%)	4.53%	(20.80%)	4.69%	(21.14%)
Sample Size	2,753,860		313,606		2,201,663		238,591	

Table 2: Mean of Postneonatal Rate by Group and Time Period[a]

	Government Employees	Private Employees	Farmers
Before NHI (1990-1993)	2.359	3.017	3.720
After NHI (1996-2001)	1.859	2.495	2.574
Simple DD (Treatment Status*NHI)[b,c]		-0.022	-0.646**
		[-0.07]	[-1.70]
Number of Observations (1990-1993)	133,930	938,659	107,253
Number of Observations (1996-2001)	179,676	1,263,004	131,338

[a] Deaths from the 28th through the 364th day of life per thousand survivors of the first 27 days of life. Total sample size is 2,753,860.

[b] Estimates of α_4 and α_5 from the regression $H=\alpha_0+\alpha_1 PRI+\alpha_2 FRM+\alpha_3 NHI+\alpha_4 PRI*NHI+\alpha_5 FRM*NHI$.

[c] T-ratios, reported in brackets, are computed from standard errors that adjust for clustering by group and year.

** Significant at the 5 percent level (one-tailed test).

Table 3: Mean of Postneonatal Mortality Rate by Subgroup and Time Period[a]

	Before NHI		After NHI		Before NHI		After NHI	
	Years of Schooling <10 (N=1,103,485)				Years of Schooling ≥10 (N=1,650,375)			
Government Employees	2.309	(19,055)	1.731	(26,003)	2.368	(114,875)	1.881	(153,673)
Private Employees	3.455	(474,683)	3.043	(454,829)	2.569	(463,976)	2.186	(808,175)
Farmers	4.258	(67,166)	3.077	(61,749)	2.819	(40,087)	2.127	(69,589)
Total	3.512	(560,904)	2.984	(542,581)	2.548	(618,938)	2.137	(1,031,437)
Simple DD (Private*NHI)[b, c]		0.167	[0.47]			0.104	[0.31]	
Simple DD (Farmer*NHI)[b, c]		-0.603*	[-1.41]			-0.205	[-0.43]	
	Pre-term (N=135,200)				Full-term (N=2,607,568)			
Government Employees	8.000	(5,250)	8.258	(9,445)	2.132	(128,048)	1.503	(169,683)
Private Employees	11.448	(35,903)	10.457	(72,682)	2.677	(898,071)	2.004	(1,186,449)
Farmers	16.707	(4,130)	10.141	(7,790)	3.156	(102,350)	2.082	(122,967)
Total	11.528	(45,283)	10.198	(89,917)	2.658	(1,128,469)	1.953	(1,479,099)
Simple DD (Private*NHI)[b, c]		-1.249	[-0.46]			-0.043	[-0.16]	
Simple DD (Farmer*NHI)[b, c]		-6.824**	[-2.13]			-0.445*	[-1.35]	
	Normal BW (N=2,624,120)				Low BW (N=121,907)			
Government Employees	1.969	(128,983)	1.432	(172,511)	13.892	(4,391)	12.558	(6,848)
Private Employees	2.527	(896,326)	1.890	(1,199,774)	14.260	(38,429)	14.288	(61,100)
Farmers	2.951	(101,991)	1.983	(124,535)	19.557	(4,653)	13.876	(6,486)
Total	2.502	(1,127,300)	1.845	(1,496,820)	14.745	(47,473)	14.093	(74,434)
Simple DD (Private*NHI)[b, c]		-0.100	[-0.42]			1.362	[0.36]	
Simple DD (Farmer*NHI)[b, c]		-0.430*	[-1.37]			-4.348	[-1.01]	

[a] Sample sizes are in parentheses.

[b] Estimates of α_4 and α_5 from the regression $H=\alpha_0+\alpha_1 PRI+\alpha_2 FRM+\alpha_3 NHI+\alpha_4 PRI*NHI+\alpha_5 FRM*NHI$.

[c] T-ratios, reported in brackets, are computed from standard errors that adjust for clustering by group and year.

** Significant at the 5 percent level (one-tailed test). * Significant at the 10 percent level (one-tailed test).

Table 4: Difference-in-differences Estimation[a]

	Whole Sample	YRs of Schooling <10	YRs of Schooling ≥10	Pre-term	Full-term	Normal BW	Low BW
NHI*Private	0.202	0.163	0.154	-0.193	0.121	0.059	2.184
	[1.01]	[0.58]	[0.67]	[-0.11]	[0.81]	[0.42]	[0.82]
NHI*Farmer	-0.479**	-0.702*	-0.208	-5.992**	-0.330*	-0.313*	-4.175
	[-1.68]	[-1.63]	[-0.64]	[-2.10]	[-1.54]	[-1.44]	[-1.15]
Sample Size	2,753,860	1,103,485	1,650,375	135,200	2,607,568	2,624,120	121,907

[a] All regressions include 5 mother's age dummies, 6 father's age dummies, 5 mother's education dummies, 5 father's education dummies, 3 parity dummies, year fixed effects and county/city fixed effects. T-ratios, reported in brackets, are computed from standard errors that adjust for clustering by group and year.

** Significant at the 5 percent level (one-tailed test).

* Significant at the 10 percent level (one-tailed test).

Table 5: NHI and Medical Technology[a]

Panel A	Whole Sample	YRs of Schooling <10	YRs of Schooling ≥10	Pre-term	Full-term	Normal BW	Low BW
Q= Number of Hospitals with an Obstetrical Department per 10,000 Population							
NHI*Private*Q	-3.143	-4.63	-3.527	-32.633	-1.833	-1.333	-45.704
	[-0.98]	[-0.61]	[-0.96]	[-0.98]	[-0.58]	[-0.41]	[-0.99]
NHI*Farmer*Q	-6.187*	-7.561	-9.104**	-25.125	-4.861	-3.398	-71.099
	[-1.39]	[-1.05]	[-1.77]	[-0.50]	[-1.13]	[-0.83]	[-0.95]
Private*Q	0.37	1.04	0.378	15.667	-0.2	-0.146	7.02
	[0.14]	[0.16]	[0.13]	[0.92]	[-0.07]	[-0.05]	[0.25]
Farmer*Q	-1.403	-5.142	4.549	-25.229	-0.306	-1.693	14.426
	[-0.39]	[-1.16]	[1.07]	[-0.56]	[-0.09]	[-0.53]	[0.22]
NHI*Q	2.092	4.857	1.379	32.528*	0.597	0.268	52.13
	[0.84]	[0.69]	[0.47]	[1.31]	[0.22]	[0.10]	[1.27]
NHI*PRI	0.654	0.845	0.657	4.645	0.377	0.244	8.85
	[1.24]	[0.65]	[1.15]	[0.93]	[0.74]	[0.45]	[1.28]
NHI*Farmer	0.382	0.417	1.004*	-2.271	0.335	0.154	5.637
	[0.57]	[0.40]	[1.33]	[-0.33]	[0.50]	[0.23]	[0.55]
Q	0.801	1.497	-0.163	10.246	0.3	0.845	5.432
	[0.33]	[0.23]	[-0.07]	[0.64]	[0.12]	[0.32]	[0.19]
Effects of NHI on Farm Households							
$\beta_3 + \beta_4[(Q_{ka}–Q_{kb}) – (Q_{ca}–Q_{cb})] + \beta_5(Q_{ka}–Q_{ca}) + \beta_6(Q_{ka}–Q_{kb}) + \beta_7 Q_{ka}$	-0.478**	-0.701*	-0.205	-6.012***	-0.329	-0.312	-4.193
	[-1.776]	[-1.361]	[-0.551]	[-2.231]	[-1.254]	[-1.181]	[-1.18]

Table 5: NHI and Medical Technology (continued)

Panel B	Whole Sample	YRs of Schooling <10	YRs of Schooling ≥10	Pre-term	Full-term	Normal BW	Low BW
Q=Number of Hospitals with a Pediatric Department per 1,000 Population							
NHI*Private*Q	-0.401	-7.547	-0.392	-14.559	0.062	1.453	-48.064
	[-0.13]	[-1.01]	[-0.10]	[-0.47]	[0.02]	[0.42]	[-1.18]
NHI*Farmer*Q	-1.228	-8.464	-1.492	46.726	-4.009	-1.053	-11.315
	[-0.23]	[-1.27]	[-0.22]	[1.12]	[-0.80]	[-0.22]	[-0.18]
Private*Q	-0.667	4.512	-0.837	-0.047	-0.769	-1.619	15.281
	[-0.26]	[0.74]	[-0.28]	[-0.00]	[-0.28]	[-0.55]	[0.55]
Farmer*Q	-2.856	1.412	-2.396	-61.086*	0.085	-2.5	-11.344
	[-0.64]	[0.29]	[-0.38]	[-1.62]	[0.02]	[-0.64]	[-0.21]
NHI*Q	-0.532	7.835	-1.732	18.593	-1.288	-1.838	37.465
	[-0.21]	[1.14]	[-0.55]	[0.76]	[-0.45]	[-0.60]	[1.00]
NHI*PRI	0.238	0.995	0.182	1.431	0.104	-0.108	7.464*
	[0.62]	[0.97]	[0.39]	[0.38]	[0.26]	[-0.25]	[1.53]
NHI*Farmer	-0.325	0.277	-0.061	-9.878**	0.059	-0.199	-2.429
	[-0.56]	[0.31]	[-0.09]	[-1.96]	[0.11]	[-0.35]	[-0.33]
Q	0.671	-4.555	1.414	14.271	0.163	0.881	-2.102
	[0.29]	[-0.79]	[0.61]	[0.85]	[0.06]	[0.33]	[-0.07]
Effects of NHI on Farm Households							
$\beta_3 + \beta_4[(Q_{ka}-Q_{kb}) - (Q_{ca}-Q_{cb})] + \beta_5(Q_{ka}-Q_{ca}) + \beta_6(Q_{ka}-Q_{kb}) + \beta_7 Q_{ka}$	-0.479**	-0.701*	-0.207	-5.971***	-0.331*	-0.314	-4.16
	[-1.816]	[-1.381]	[-0.564]	[-2.201]	[-1.283]	[-1.226]	[-1.163]

Table 5: NHI and Medical Technology (concluded)

Panel C	Whole Sample	YRs of Schooling <10	YRs of Schooling ≥10	Pre-term	Full-term	Normal BW	Low BW
Q=Number of Infant Hospital Beds per 1,000 Population							
NHI*Private*Q	0.086	0.642	-0.313	-10.39	0.722	0.653	-15.241
	[0.07]	[0.21]	[-0.24]	[-1.19]	[0.61]	[0.53]	[-1.22]
NHI*Farmer*Q	-0.27	1.515	-2.429	-2.795	-0.07	-0.163	-9.162
	[-0.13]	[0.47]	[-1.05]	[-0.15]	[-0.04]	[-0.09]	[-0.33]
Private*Q	-1.162	-0.222	-0.802	-1.791	-1.213*	-1.214	3.601
	[-1.30]	[-0.08]	[-0.87]	[-0.23]	[-1.35]	[-1.25]	[0.34]
Farmer*Q	-2.845*	-3.751*	-0.244	-8.832	-2.645*	-2.420*	-1.288
	[-1.56]	[-1.56]	[-0.12]	[-0.53]	[-1.59]	[-1.56]	[-0.05]
NHI*Q	-0.089	-1.031	0.4	11.261*	-0.566	-0.767	21.251**
	[-0.07]	[-0.36]	[0.29]	[1.54]	[-0.50]	[-0.67]	[1.81]
NHI*PRI	0.253	-0.019	0.311	3.425	-0.032	-0.073	7.077*
	[0.68]	[-0.02]	[0.74]	[1.09]	[-0.08]	[-0.18]	[1.58]
NHI*Farmer	-0.203	-0.859	0.467	-3.76	-0.156	-0.148	-0.062
	[-0.39]	[-0.92]	[0.81]	[-0.80]	[-0.31]	[-0.30]	[-0.01]
Q	0.281	0.992	-1.12	5.317	0.012	0.883	-17.749*
	[0.25]	[0.34]	[-0.86]	[0.56]	[0.01]	[0.84]	[-1.58]
Effects of NHI on Farm Households							
$\beta_3 + \beta_4[(Q_{ka}-Q_{kb}) - (Q_{ca}-Q_{cb})] + \beta_5(Q_{ka}-Q_{ca}) + \beta_6(Q_{ka}-Q_{kb}) + \beta_7 Q_{ka}$	-0.483**	-0.702*	-0.214	-6.019***	-0.334*	-0.315	-4.205
	[-1.931]	[-1.363]	[-0.582]	[-2.234]	[-1.336]	[-1.24]	[-1.21]
Sample Size	2,753,860	1,103,485	1,650,375	135,200	2,607,568	2,624,120	121,907

[a] All regressions include 5 mother's age dummies, 6 father's age dummies, 5 mother's education dummies, 5 father's education dummies, 3 parity dummies, year fixed effects and county/city fixed effects. T-ratios, reported in brackets, are computed from standard errors that adjust for clustering by county and NHI.

*** Significant at the 1 percent level (one-tailed test). ** Significant at the 5 percent level (one-tailed test). * Significant at the 10 percent level (one-tailed test).

Table 6: NHI and Medical Technology, Assuming NHI Effect is Entirely Through Technology[a]

Panel A	Whole Sample	YRs of Schooling <10	YRs of Schooling ≥10	Pre-term	Full-term	Normal BW	Low BW
Q= Number of Hospitals with an Obstetrical Department per 10,000 Population							
NHI*Private*Q	0.808	0.481	0.436	-4.534	0.443	0.141	8.092
	[0.73]	[0.17]	[0.33]	[-0.41]	[0.39]	[0.12]	[0.55]
NHI*Farmer*Q	-4.036**	-5.311*	-2.766	-43.328**	-2.853**	-2.517*	-38.680*
	[-2.30]	[-1.47]	[-1.05]	[-2.10]	[-1.72]	[-1.51]	[-1.42]
Private*Q	-1.322	-1.115	-1.349	1.166	-1.168	-0.776	-18.558
	[-0.67]	[-0.26]	[-0.58]	[0.07]	[-0.64]	[-0.41]	[-0.84]
Farmer*Q	-2.445	-6.452*	1.872	-19.022	-1.165	-2.108	-2.57
	[-0.83]	[-1.57]	[0.55]	[-0.60]	[-0.44]	[-0.83]	[-0.06]
NHI*Q	-1.197	0.299	-1.987	11.963	-1.362	-0.964	5.614
	[-0.85]	[0.11]	[-1.26]	[0.76]	[-0.95]	[-0.70]	[0.30]
Q	2.222	3.43	1.339	20.718*	1.146	1.377	27.642*
	[1.21]	[0.76]	[0.67]	[1.39]	[0.61]	[0.74]	[1.39]
Effect of NHI on Farm Households							
$\beta_4[(Q_{ka}-Q_{kb})-(Q_{ca}-Q_{cb})]+\beta_5(Q_{ka}-Q_{ca})+\beta_6(Q_{ka}-Q_{kb})+\beta_7Q_{ka}$	-0.519***	-0.751*	-0.313	-5.751***	-0.365*	-0.328*	-4.778*
	[-2.154]	[-1.489]	[-0.907]	[-2.168]	[-1.552]	[-1.404]	[-1.366]

Table 6: NHI and Medical Technology, Assuming NHI Effect is Entirely Through Technology (continued)

Panel B	Whole Sample	YRs of Schooling <10	YRs of Schooling ≥10	Pre-term	Full-term	Normal BW	Low BW
Q=Number of Hospitals with a Pediatric Department per 1,000 Population							
NHI*Private*Q	1.369	-0.373	0.954	-3.662	0.826	0.672	7.363
	[0.94]	[-0.11]	[0.56]	[-0.28]	[0.56]	[0.45]	[0.42]
NHI*Farmer*Q	-4.250**	-7.299**	-2.225	-38.310*	-3.626*	-2.629	-39.587
	[-1.79]	[-1.79]	[-0.59]	[-1.57]	[-1.55]	[-1.15]	[-1.28]
Private*Q	-1.503	1.024	-1.49	-6.266	-1.13	-1.247	-14.072
	[-0.84]	[0.25]	[-0.67]	[-0.43]	[-0.61]	[-0.67]	[-0.75]
Farmer*Q	-1.292	0.664	-1.901	-8.657	-0.091	-1.722	5.358
	[-0.39]	[0.15]	[-0.43]	[-0.33]	[-0.03]	[-0.55]	[0.15]
NHI*Q	-1.676	1.664	-2.692*	17.412	-1.926	-1.086	-5.11
	[-1.07]	[0.53]	[-1.52]	[1.03]	[-1.18]	[-0.69]	[-0.24]
Q	1.23	-1.463	1.879	15.098	0.467	0.524	20.828
	[0.71]	[-0.37]	[1.05]	[1.08]	[0.26]	[0.29]	[0.99]
Effect of NHI on Farm Households							
$\beta_4[(Q_{ka}-Q_{kb})-(Q_{ca}-Q_{cb})]+\beta_5(Q_{ka}-Q_{ca})+$	-0.414**	-0.768**	-0.194	-3.955**	-0.342*	-0.275	-3.556
$\beta_6(Q_{ka}-Q_{kb})+\beta_7Q_{ka}$	[-1.785]	[-1.86]	[-0.577]	[-1.70]	[-1.531]	[-1.265]	[-1.169]

Table 6: NHI and Medical Technology, Assuming NHI Effect is Entirely Through Technology (concluded)

Panel C	Whole Sample	YRs of Schooling <10	YRs of Schooling ≥10	Pre-term	Full-term	Normal BW	Low BW
Q=Number of Infant Hospital Beds per 1,000 Population							
NHI*Private*Q	0.800*	0.594	0.547	-0.578	0.635*	0.451	4.799
	[1.65]	[0.48]	[0.94]	[-0.13]	[1.38]	[0.95]	[0.77]
NHI*Farmer*Q	-1.253	-1.696	-0.936	-20.355**	-0.627	-0.656	-16.206
	[-1.25]	[-0.87]	[-0.59]	[-1.74]	[-0.66]	[-0.69]	[-1.20]
Private*Q	-1.605***	-0.172	-1.342**	-8.584*	-1.159**	-1.089**	-9.607
	[-2.48]	[-0.10]	[-1.74]	[-1.57]	[-2.01]	[-1.86]	[-1.23]
Farmer*Q	-2.337**	-2.15	-1.176	2.192	-2.332**	-2.134**	1.711
	[-1.70]	[-0.93]	[-0.72]	[0.18]	[-1.85]	[-1.75]	[0.11]
NHI*Q	-0.607	-0.715	-0.336	4.392	-0.461	-0.572	5.009
	[-0.86]	[-0.59]	[-0.38]	[0.82]	[-0.69]	[-0.92]	[0.70]
Q	0.581	0.755	-0.659	9.798	-0.059	0.759	-7.236
	[0.67]	[0.41]	[-0.58]	[1.18]	[-0.08]	[1.00]	[-0.78]
Effect of NHI on Farm Households							
$\beta_4[(Q_{ka}-Q_{kb})-(Q_{ca}-Q_{cb})]+\beta_5(Q_{ka}-Q_{ca})+$	-0.450***	-0.544	-0.31	-5.368***	-0.303*	-0.285*	-4.369*
$\beta_6(Q_{ka}-Q_{kb})+\beta_7 Q_{ka}$	[-2.027]	[-1.279]	[-0.887]	[-2.252]	[-1.409]	[-1.386]	[-1.512]
Sample Size	2,753,860	1,103,485	1,650,375	135,200	2,607,568	2,624,120	121,907

[a] All regressions include 5 mother's age dummies, 6 father's age dummies, 5 mother's education dummies, 5 father's education dummies, 3 parity dummies, year fixed effects and county/city fixed effects. T-ratios, reported in brackets, are computed from standard errors that adjust for clustering by county and NHI.

*** Significant at the 1 percent level (one-tailed test). ** Significant at the 5 percent level (one-tailed test). * Significant at the 10 percent level (one-tailed test).

Table 7: Robustness Checks[a]

	Whole Sample	YRs of Schooling <10	YRs of Schooling ≥10	Pre-term	Full-term	Normal BW	Low BW
Panel A. County-Year Interactions Included							
NHI*Private	0.230	0.198	0.175	-0.062	0.148	0.075	2.856
	[1.15]	[0.70]	[0.77]	[-0.03]	[1.01]	[0.53]	[1.07]
NHI*Farmer	-0.497*	-0.723*	-0.247	-6.002**	-0.332*	-0.315*	-4.028
	[-1.65]	[-1.55]	[-0.81]	[-1.95]	[-1.51]	[-1.40]	[-1.11]
Sample Size	2,753,860	1,103,485	1,650,375	135,200	2,607,568	2,624,120	121,907
Panel B. Propensity Score Weighted Regression[b]							
NHI*Private	0.100	0.089	0.068	-1.408	0.116	0.037	1.379
	[0.55]	[0.35]	[0.34]	[-0.77]	[0.95]	[0.29]	[0.65]
NHI*Farmer	-0.664***	-0.866*	-0.611**	-8.049**	-0.463***	-0.500***	-3.865
	[-3.70]	[-1.89]	[-2.81]	[-2.73]	[-3.12]	[-3.52]	[-1.09]
Sample Size for Treatment I (Private) & Control	2,515,269	974,570	1,540,699	123,280	2,382,251	2,397,594	110,768
Sample Size for Treatment II (Farmer) & Control	552,197	173,973	378,224	26,615	523,048	528,020	22,378

[a] All regressions include 5 mother's age dummies, 6 father's age dummies, 5 mother's education dummies, 5 father's education dummies, 3 parity dummies, year fixed effects and county/city fixed effects. T-ratios, reported in brackets, are computed from standard errors that adjust for clustering by county and year.

*** Significant at the 1 percent level (one-tailed test). ** Significant at the 5 percent level (one-tailed test). * Significant at the 10 percent level (one-tailed test).

[b] The propensity scores of belonging to private households and farmers' households are estimated separately. Variables used to predict the propensity scores include mother's and father's years of schooling and ages. County and year interactions are also included.