

HEALTH SHOCKS, VILLAGE ELECTIONS, AND LONG-TERM INCOME:

EVIDENCE FROM RURAL CHINA *

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Abstract: Using a sample of 1,354 households in 48 Chinese villages for the period 1987-2002, we study the dynamic effects of major health shocks on household income and the role played by village elections in mitigating these effects. Our results show that in the first sixteen years after a shock, a shock-hit household on average falls short of its normal income trajectory by 12.3%. Various methods aiming at correcting possible composition biases in the reports of health shocks find larger effects. We also find that even by a conservative estimation, village elections reduce the negative effects of health shocks by 8.3 percentage points. In addition, we have found that elections make a village more likely to introduce a health care plan.

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HEALTH SHOCKS, VILLAGE ELECTIONS, AND LONG-TERM INCOME: EVIDENCE FROM RURAL CHINA

The traditional literature on risks in developing economies maintains that the costs of exposure to risks are small if households can effectively smooth their consumption (Morduch, 1995). However, for people living with very low income, consumption smoothing may come with very high costs because they may have to employ whatever limited ways they have, including selling productive assets, to maintain their level of consumption (Chetty and Loony, 2006). Consumption smoothing may therefore only be obtained at the cost of people's long-term income capabilities. Morduch (1995) thus directs researchers' attention to income smoothing.

Major health shocks are the most unpredictable risks for farm households in developing economies. They have both a direct effect and an indirect effect for uninsured or partially insured farm households in their long-term income. The direct effect is the income loss if the sick person is a major bread earner in the family. The indirect effect is related to the treatment of illness. The family has to spend a large amount of money in a short period of time, which often leads to heavy indebtedness. This forces the family to slow down its pace of asset accumulation including children's education. As a result, this family loses or partially loses its capability to generate income in the long run. Adding together, these two effects may cause a household struck by a major health shock to fall into persistent poverty.

While there have been an increasing number of studies looking at the negative effects of health shocks on income in developed countries using longitudinal data (e.g., Smith, 1999, 2003), much less has been done in developing countries, primarily because of the lack of properly designed longitudinal data (Foster, 1995; Strauss and Thomas, 1998). Existing studies on developing countries have focused on health shocks' short-term

impacts on income, labor supply, and consumption. Schultz and Tansel (1997) find that major illnesses reduce labor supply in Africa. Using a two-year panel dataset in Indonesia, Gertler and Gruber (2002) find that major illnesses (as measured by changes in the household head's physical abilities to perform activities of daily living) reduce household income by reducing labor supply. They also find that households cannot fully insure their consumption against health shocks. Lindelow and Wagstaff (2005) use four waves (1991, 1993, 1997, and 2000) of the China Health and Nutrient Survey (CHNS) to study how household income is associated with changes in household head's self-reported health between waves. They find that a large negative health shock reduces income by 12.4%, mainly through lost labor days. However, they do not explore the dynamic nature of health shocks' impacts. In addition, their estimates may be biased due to the endogeneity problem of self-reported health (Schultz and Tansel, 1997).

Taking advantage of a unique household-level longitudinal dataset of sixteen years from rural China, this paper provides an assessment of both the average and dynamic impacts of major health shocks on farm households' long-term income. The long coverage of our data allows us to examine some of the issues ignored by previous studies. For example, Gertler and Gruber (2002) are unable to account for health shocks' impacts on people's wage rates. With a long time span, we are able to capture the cumulative effects of health shocks, including their impacts on households' long-term income capabilities.

In addition, we will study the role played by village elections in helping households to mitigate the negative impacts of health shocks. During our sample period of 1987-2002, an important political development in rural China was that villages began to experiment democratic elections. The election law --- *The Organizational Law for the Village Committee* (OLVC) --- was first put into experiment in 1987 and formally launched in 1998. Despite many controversies, elections have spread to every corner of the country (O'Brien and Li, 2000).

Village elections may help farm households to deal with health shocks for several reasons. First, they enhance the accountability of the village government. Before elections, village cadres were appointed by the township government. It was more important for them to please higher-level officials through political loyalty and other patronage relations than to look after the demands of the villagers. Elections force them to face the pressures from within the village. Indeed, recent studies on India and China find significant evidence for local elections' role to make local governments more responsive to the needs of their constituencies. Chattopadhyay and Duflo (2004), for instance, find that the election of a woman village head in Indian villages has led to the introduction of pro-woman policies. Using a sample from China's Jiangsu province, Zhang, Fan, Zhang, and Huang (2004) find that village elections have shifted village government spending from items benefiting the village leaders (such as salaries and banquets) to those benefiting the general public (such as public facilities and income transfers), and Wang and Yao (2007) find similar results using a larger sample covering eight Chinese provinces. Both interpret their results as evidence for enhanced accountability of the village government. To the extent that public expenditures increase villagers' income capabilities and augment their consumption, village elections help farm households to deal with health shocks.

Second, elections can lead to pro-poor policies because the poor usually constitute the majority of the village. Since it is often more difficult for the poor to deal with economic shocks, elections improve the poor's capabilities, and for that matter, also improve the average capabilities within a village. Foster and Rosenzweig (2001) provide evidence for elections' role in creating pro-poor policies. They find that village elections in India have increased local governments' investment in road building and reduced their investment in irrigation facilities. They interpret this finding as evidence for a pro-poor policy because irrigation benefits landlords and building roads provides jobs to the landless.

Third, elections provide an institutionalized mechanism for villagers to take collective actions. For example, a health care plan benefits both the rich and the poor. Without a proper mechanism for collective decision making, however, such a plan is hard to set up. Elections and their associate institutional arrangements may provide such a mechanism.

The long time span of our data allows us to test the long-run effects of elections. In addition, we are also interested in studying whether elections make villages more likely to establish health care plans because such plans provide a direct means for households to deal with health shocks.

Our data come from two major sources. One is the National Fixed-point Survey (NFS) maintained by the Research Center of Rural Economy (RCRE), the Ministry of Agriculture, the People's Republic of China. It provides rich information at both the village level and the household level for the period 1987-2002. The other data source is a retrospective survey conducted by the authors in the spring of 2003 to obtain information on households' history of major illnesses and village governance in the period of 1987-2002. The two sources of data are matched to create a panel dataset that enables us to study the dynamic impacts of major health shocks on households' long-term income trajectories and elections' role in mitigating these impacts.

The rest of the paper is arranged as follows. Section 1 first provides a succinct introduction to health care and village elections in rural China. It then describes in detail the two data sources, and discusses some critical measurement issues. Section 2 presents both baseline econometric analyses for the average and dynamic impacts of health shocks on household income and several robustness tests for them. Section 3 studies the role of village elections and health care plans in mitigating the negative impacts of health shocks. It also investigates whether elections lead to the establishment of health care plans. Section 4 concludes the paper by discussing several implications of our results.

1. Background, data and measurement issues

1.1 Background

In the late period of the commune system, China had a cooperative medical system (CMS) in its countryside. Financed by the commune budget, this system provided basic medical services to farmers and was believed to have contributed to the improvement of rural health, as demonstrated by the fast decline of the mortality rate in rural China (Sidel, 1993). However, the system fell apart when the rural reform dismantled the commune system in the early 1980s. By 1993, only 10% of Chinese rural residents had some form of health insurance (Liu, 2004); and by 2003, the number increased only to 20% (Center for Health Statistics and Information, 2004). Starting in 2003, the Chinese government began to implement a heavily subsidized county-based insurance system, with an objective to cover all the Chinese counties by 2008. Our sample stops at 2002 and thus does not cover this new system. For our sample period the most popular health care system was still the old village-based CMS. Some richer regions might also have township-sponsored systems.¹ In addition to these two plans, some villages had participated in limited commercial health care plans.

During our sample period, the political system in rural China had experienced dramatic changes. As a response to the fall of the commune system, the 1982 Constitution (Article 111) defined the village, usually comprised of 2,000 to 3,000 people, as a self-governing entity outside the government hierarchy. Villagers govern by themselves through the village committee (VC). However, the VC members had remained appointed by the township government until in 1987 when the National People's Congress (NPC), China's legislative body, passed a tentative version of the OLVC which required that all VC members be elected by villagers. This law triggered the spread of village elections across the country. By 1994, half of the Chinese villages had

¹ A township is the lowest administrative unit in the government hierarchy. Above the township is county, and then province. Village is a self-governing body below the township.

begun elections; by 1997, 25 of the 31 mainland provinces had adopted a local version of the law, and 80% of the villages had begun elections (Ministry of Civil Affairs, 1998). In 1998, the NPC passed the formal version of the OLVC and the election has since been expanded quickly to all the villages in the country.

Depending on the size of the village, the VC is comprised of three to seven members. The core members are the chairman, the vice chairman, and the accountant. The term of the committee is three years but there is no term limit. Before 1998, candidates for the chairman were usually appointed by the township government although nominations from the villagers might also occur. The formal version of the OLVC requires that all candidates be nominated by villagers. There can be as many as a hundred nominated candidates for the chairman, so a common practice is to hold a primary election to reduce the number of candidates to two who then compete head-to-head in a runoff.

The authority of the elected VC, however, faces two challenges. Within the village, the communist party committee is supposed to supersede the VC in critical decisions. Usually appointed by the township party branch, the communist party committee may be more interested in pleasing upper-level governments instead of serving the villagers. Outside the village, the VC has to compromise its mandate to its constituency with the demands of higher-level governments ranging from family planning to the kinds of crops to plant. Both challenges may undermine the ability of the elected VC to work for the villagers. It is thus an empirical question as to whether elections would bring real changes to the villages.

1.2 Data

As mentioned in the introduction, our data come from two sources, the NFS and a retrospective survey conducted by the authors in 2003. The NFS has not been widely used in academic research. Two papers that use the dataset extensively are Benjamin, Brandt, and Giles (2005) and Giles and Yoo (2007). Benjamin et al. (2005) provides a

detailed assessment of the quality of the NFS data in Appendix I of their paper. Here we provide a general description of NFS, paying more attention to issues related to our current study.

The NFS is a longitudinal survey started in 1986 and currently covers about 23,000 rural households in 350 villages in all the continental Chinese provinces. It was intended to track the same villages and households through time when it was first conceptualized. Except for 1990, 1992, and 1994, NFS has collected data in each year since 1986. When it was started, the NFS adopted a mixed sampling strategy featuring stratified representation and random sampling. In each province, three strata were identified. The first one, based on geographic topology, divided a province into three regions: plain, hilly, and mountainous. The second stratus consisted of counties that were divided into three groups by per-capita income: low, middle, and high. Several representative counties, i.e., counties with average characteristics of their income group, were chosen for the sample. The last stratus was village. Within each county, one representative village was chosen for sampling. Within this chosen village, households were randomly sampled. There are about 2,600 counties in China; the NFS sample covers about 14% of them. The number of households surveyed in each village ranged from 50 to more than 100. These numbers would add up to reflect the population share of each geographic region in a province.

The NFS assigned each household in the initial sample a unique identification code. However, attritions are inevitable for such a long panel survey. They happen for two major reasons. The first and more important reason has been the dropout of the whole village because it was annexed into an urban district as the nearby city expanded, or because the county has refused to continue the survey. The second reason is that the entire household moves to another location. When a village dropped out, another village with about the same characteristics was added in the survey. The same thing happened to household dropouts, i.e., dropout households were replaced by other households with similar characteristics. This method of replacement is likely to preserve the randomness

of the NFS sample.

The biggest problem of the replacement, however, is the mis-labeling of households. Normally, newly added households should be assigned new identification numbers. However, many of the replacements have used the numbers of those being replaced; therefore, two or more different households end up sharing the same identification number. This makes it difficult to identify the panel structure in the data.

The NFS has both a village and a household questionnaire. The village questionnaire contains information on village population, public facilities, agricultural production, enterprises, income, village government revenues and expenditures. The household questionnaire asks for detail information about household demography, land and asset holdings, agricultural production, agricultural product sales, family businesses, income and expenses, cash flows (including savings), and consumption. Data are gathered by the county survey teams every three months. The village questionnaire is administered to members of the VC, most likely the accountant. The household questionnaire is filled by the surveyor based on the diaries maintained by the sample households. RCRE only keeps annual data, however.

The 2003 survey was conducted by the authors in eight provinces. From south to north, they were Guangdong, Hunan, Zhejiang, Henan, Sichuan, Gansu, Shanxi, and Jilin. These provinces were chosen to represent geographic and economic diversities in the country. Guangdong and Zhejiang are two coastal provinces and the richest in the country. Gansu and Shanxi, one in the northwestern region and the other in the inland north region, are two of the poorest provinces in the country. The other four provinces represent the average province in the country. There were on average 4,975 households in 92 villages in these eight provinces that reported data in the NFS throughout the period of 1987-2002. The 2003 survey was conducted on 1,354 households in 48 villages. The distribution of those villages was: seven villages in Guangdong, Hunan, and Shanxi; nine villages in Zhejiang; three villages in Henan and Jilin; and five villages in Gansu.

The selection of the 1,354 households was guided by the objective to establish a strict panel out of the NFS data. The main task involved was to eliminate the observations that shared the same household codes but in fact belonged to different households. For that, three criteria were used for the elimination. They were the building area of the house, the amount of land, and the number of land parcels. The building area of the house was used to identify households that had been split. It is usually the case that rural households build new houses or expand old houses for their sons' marriages, which often leads to splits. NFS continues to survey one of the split households. However, the split households may not be regarded as the same as the old household because their characteristics change dramatically. The amount of land and the number of land parcels are also important criteria because the change of landholding should be small between two consecutive years although it could happen due to reallocation of land within the village.² Using these three criteria, we calculated the Euclidean distance, i.e., the square-root of the sum of the squared differences, between two observations of consecutive years with the same household identification number. Those with a distance larger than the median were dropped from the sample. The remaining observations were checked manually by the names of their household heads and other family characteristics. Household-year observations were kept in the sample if they shared the same household head or experienced minor changes in household characteristics. Other households were dropped. Villages with few matched households were also dropped. This process required intensive manual reading and subjective judgments by the authors. The matched households were checked again in the survey to make sure that they had stayed in the survey for the entire sample period.

The 2003 supplemental survey consisted of two questionnaires, one for the village, and the other for the household. The village questionnaire recorded information on village elections, the history of village health care facilities and services, and its current

² After the rural reform, Chinese land tenure has been characterized by a two-tier system with the village having the legal rights to land and households having the use rights. Villages have engaged in periodic land redistribution to maintain a roughly egalitarian distribution of land based on family population (Liu, Carter, and Yao, 1998).

health care arrangements. The household questionnaire recorded information on household demographics at the individual level, the history of major illnesses since 1987, health insurance of each household member since 1987, current health expenditures of each member, family social networks, and willingness to pay for health insurance.

The cost of obtaining a strict panel is that we have arrived at a much smaller sample size than the NFS has. There is also a possibility that the matching strategy would create a biased sample. The 48 sample villages were 14% richer than the 92 NFS sample villages in the eight selected provinces, but also had a 23% larger standard deviation.³ The 48 villages were therefore a relatively good representation of the 92 NFS sample villages.

[Figure 1 about here]

It is more important that no major difference between the 2003 sample and the NFS sample existed at the household level in the 48 sample villages. Because the impacts of health shocks and elections can be sensitive to household wealth, and for that matter also sensitive to household income, over-sampling of either richer or poorer households would lead to biased estimates. Figure 1 compares the two samples in terms of the average per-capita net income in each year. Households in the 2003 sample on average were slightly richer than households in the NFS sample. The overall average in the 2003 sample was 3,082 yuan (in 2002 prices; same for subsequent figures), and the overall average in the NFS sample was 2,975 yuan. The difference was not statistically significant, though. Overall, the distribution of income was more compact in the 2003 sample than in the NFS sample although the 2003 sample had a more dispersed distribution than the NFS sample in some years. Looking at the averages across years, one finds that the two samples overlap substantially in coverage. The one standard deviation interval is [-2103.7, 8054.1] for the NFS sample, and [-1232.6, 7408.7] for the 2003 sample. The differences between the two intervals are very small in terms of the number of observations they cover. The first interval captures 95.1% of the

³ The village-level mean per capita income in the 2003 sample was 2,855 yuan (in 2002 prices; same for subsequent figures), while the corresponding number in NFS' 92-village sample was 2,455 yuan. The corresponding standard deviations were 5,203 yuan and 4,218 yuan, respectively.

household-year observations in the NFS sample, and the second interval captures 94.2% of the same observations. Therefore, at least for the key variable of per-capita income, households in the 2003 sample do not seem to be very different from those in the NFS sample in the 48 villages.

1.3 Measurement issues

The 2003 survey asked households to recall major illnesses happening to their household members in the period of 1987-2002. A major illness was specifically defined as an illness (excluding births) or injury that required inpatient treatments or a total medical expenditure over 5,000 yuan (in current yuan).

We adopted this definition of a major health shock for two reasons. The first was practical. As discussed in Bound, Brown, and Mathiowetz (2001), retrospective survey on health-related information often results in substantial measurement errors. Overall, using US data, Cannell, Fisher and Bakkar (1965) show that 12% of doctor visits were not reported for one year recall period, and the under-reporting appeared to increase with the length of the recall period, but to decrease with the salience of the event. Since the time period covered by our survey is very long, we had to restrict our attention to major health events so as to ensure the accuracy of people's recounting of their families' health history. The second was that major illnesses requiring inpatient treatments or a large expenditure were more likely to be unexpected shocks: the treatment was likely to push the spending close to or above the limit of what the household budget allowed for, and thus was a result of households' passive reaction rather than their consumption maximization.⁴

Among the 676 reported cases of health shocks, 66.1% were inpatient treatments with expenditure less than 5,000 yuan, 28.0% were inpatient treatments with expenditure more than 5,000 yuan, and the remaining 5.9% were illnesses with expenditure more than

⁴ This is also emphasized by Lindelow and Wagstaff (2005).

5,000 yuan but nevertheless without inpatient treatment. Most health shocks thus fell into the category of inpatient treatments.

However, our definition may be susceptible to several issues involving biased reports. The first is related to household preferences. Some families might have stronger preferences for health than others, so they would be more likely to report major illnesses. People's preferences might also change over time. The society tended to provide more information about health over time so people might develop preferences for better health as they obtained more information. As a result, there could be more reports for more recent years that involved less serious illnesses than those for the earlier years.

The second issue is related to what we call the income effect. Poor households might have experienced a major health shock but nevertheless neither spent more than 5,000 yuan nor sent the patient to hospital unless the illness was very serious or life-threatening. As a result, shocks reported by poorer households would be more serious than shocks reported by richer households. Another possibility is that later shocks were less severe than the early shocks because increase in income induced families to treat less severe illnesses.

The third issue is related to what we call the price effect. In our sample period, prices of medical treatments had increased while the expenditure measure was asked in nominal terms. As a result, an illness of 5,000 yuan in 2002 would be almost surely less serious than an illness requiring the same amount of nominal expenditure in 1987.

The above issues imply a possible composition bias in our definition of health shocks,⁵ i.e., health shocks are not measured consistently across time and households. Because of the income effect, shocks reported by poorer households would be more serious than shocks reported by richer households; also because of the income effect, plus the price effect and preference changes over time, shocks in earlier years would be more serious than shocks in later years.

⁵ We owe this point to an anonymous referee and the editor.

To find out the seriousness of the composition bias, we present in Figure 2 the incidence of health shocks by income quarter and year. Among the 500 households reporting at least one shock in the period of 1987-2002, the majority (73.6%) had one shock, 19.8% had two, 4.6% three, and 2% four or more. In the figure, income is defined as per-labor net income in 2002 yuan, which is the primary dependent variable that we will analyze in subsequent empirical analysis. Household-year observations after a household received a shock are deleted. Households having received shocks are represented by their per-labor income of the year immediately before the shock. The figure shows that households in the third income quarter reported the highest incidence of health shocks on average, followed by the second quarter and the fourth quarter, with the first quarter being the lowest. However, the differences among the four quarters were statistically insignificant; households in the first income quarter even had the highest incidence in 1998 and 2001. The income effect was therefore unlikely a serious factor causing the composition bias.

[Figure 2 about here]

However, Figure 2 also shows that there was a significant upward trend over time. The average incidence before 1996 was between 2.7% and 3.8%; it then increased to the range of 5 to 6% between 1997 and 2000; and finally increased to more than 8% in 2001 and 2002. Although other explanations, such as memory loss and aging of the households, are possible, those figures suggest that the composition bias might indeed exist along the time dimension. It might be caused by preference changes, the price effect, or both. In the next section, we will adopt several methods to deal with the composition bias in general and the price effect in particular.

2. Negative Impacts of Health Shocks

2.1 Econometric models and variables

To study the negative effects of health shocks on income, we first estimate the following baseline panel model with household and year fixed effects:

$$(1) \ln y_{it} = X_{it}\beta + \alpha_s S_{it} + \alpha_i + \alpha_t + e_{it}.$$

In the equation, y_{it} is the per-labor income (in 2002 yuan) of household i in year t , X_{it} is a set of control variables, and S_{it} is a dummy variable of health shocks. The parameter α_i stands for the fixed effect for household i , and the parameter α_t stands for the fixed effect for year t . The error term e_{it} is assumed to be clustered at the household level.

We use per-labor income instead of per-capita income to avoid adding in regressions family control variables (such as dependent ratio) that may be endogenously determined in our long sample period. The health shock dummy variable S_{it} is assigned a value 1 since the year of a health shock, and 0 otherwise. For households having multiple shocks, S_{it} begins to be coded 1 since the year of their first shock.⁶ Defining S_{it} in this way allows us to assess the accumulated effects of a health shock over time; the coefficient α_s captures the average of the accumulated effects. The set of control variables X_{it} consists of per-capita landholding (in mu , which is equal to one fifteenth hectare), the age and age square of the household head. In rural China, land is still legally owned and allocated by the village to maintain roughly equal per-capita landholding (Liu, Carter, and Yao, 1998); that is, per-capita landholding is pre-determined. The age of the household head and its square are added to capture possible life-cycle effects in a household's income capability. Our fixed-effects model controls for time-invariant household heterogeneity including preferences for health and common time effects such as government policies and macroeconomic conditions.

After observations with missing data are deleted,⁷ we are left with 1,185 households with a total of 13,515 observations for the period of 1987-2002 (the data of 1990, 1992,

⁶ Shocks happening in 1990, 1992, and 1994 are shifted to 1991, 1993, and 1995, respectively because we do not have data for those three years.

⁷ These include observations with negative net income for the sake of taking logs. It is noteworthy that households having negative net income were mostly those with self-run businesses. The incidence of negative net income was not correlated with the incidence of health shocks.

and 1994 are missing). Basic statistics of the variables in 1987, 1995, and 2002, as well as in the whole sample are provided in Table 1.

[Table 1 about here]

2.2 Average effects of health shocks

[Table 2 about here]

Table 2 reports the results for the average effect of health shocks. Column (1) reports the baseline results from a regression of model (1) using the whole sample. It shows that a health shock reduces household per-labor income by an average of 12.3% in each year after the shock at the 1% significance level. Since a household has already sent the patient to hospital or spent at least 5,000 yuan for the shock, the negative welfare impact of a major health shock is striking.

As expected, per-capita landholding has a positive and statistically significant contribution to household income although the magnitude is only 4.6% for one more *mu* of land. The age of the household head has no impact on the income while the age square is significant and negative.

Our result regarding the impact of health shocks may be affected by the composition bias caused by temporal inconsistencies in our measurement of health shocks. If that were the case, it would be expected that shocks happening in earlier years would have larger effects than shocks happening in later years. To check this possibility, we first create a set of year dummies indicting the year of the first shock that a household experienced.⁸ Then we interact these dummies with the health shock dummy and replace the health shock dummy in equation (1) with these interaction dummies and run the regression again. The results are shown in Table 3.⁹ Shocks in and before 1996 are found

⁸ To match our early results (see footnote 6 for an explanation), we assign the shocks happening in 1990, 1992, and 1994 --- the three years without data --- to 1991, 1993, and 1995, respectively.

⁹ Notice that the dummy for 1987 is omitted because it is collinear with the fixed effects of households that got a shock in that year. In fact, the interaction dummy of that year will not contribute to the estimation of health shocks' impacts because its values do not vary within the household-year observations of a shock-hit household because of the construction of the shock dummy. The coefficients of the other interaction terms thus can still be interpreted as the effects of shocks happening in different years.

to have effects that are larger and more likely to be statistically significant than shocks after 1996. The average of the earlier years is 18.8% whereas the average of the later years is only 8.9%. Therefore, the composition bias might indeed exist. However, one potential problem of the results in Table 3 is that the constructed interaction dummies make shocks in early years have longer time period to accumulate their effects than shocks in later years. The interaction dummies estimate the average of the accumulative effects of health shocks in different years. Because early shocks have longer periods to accumulate their effects, their estimates must be larger than those of later shocks.

[Table 3 about here]

With this problem in mind, we divide the sample into two periods. The year 1996 is used as the cutoff year for two reasons: the earlier period contained more serious shocks than the later period (see Table 3), and the incidence of shocks was more uniform in the earlier period but increased quickly in the later period (see Figure 2). Estimating those two periods separately allows both earlier and later shocks to have time (although not equal) to accumulate their effects. The regression results from the divided samples are presented in Columns (2) and (3) of Table 2. Their specification is the same as that of Column (1). When running the regression using the post-1996 sample, we have deleted households who had health shocks before 1997. The estimates for health shocks are -0.152 and -0.131 for the early and later periods, respectively, which confirms again the existence of the composition effect.

Our next task then is to find an estimate for the effect of health shocks by explicitly taking care of the temporal composition bias. Since our early analysis showed that the income effect was not a serious problem and the household and year fixed effects have taken care of heterogeneous household preferences and their common changes, here we only account for the price effect of the composition bias. Because the price effect is caused by our definition using the nominal expenditure, a natural approach to correct it is to make all the shocks comparable on the basis of real expenditure. We use 1987 as the

base year for our comparison. Let $S_{i\tau}$ be the probability of shock of household i in year τ recorded in our data based on a nominal expenditure more than 5,000 yuan.

Correspondingly, let $S_{i\tau}^*$ be the probability of shock based on a real expenditure of more than 5,000 yuan. $S_{i\tau}^*$ should be used if we want to make all the shocks be comparable to shocks happening in 1987. However, it is apparently that $E(S_{i\tau}^*) \leq E(S_{i\tau})$ (see equation (A1) in Appendix A). Therefore, applying $S_{i\tau}$ instead of $S_{i\tau}^*$ in equation (1) would yield biased estimates of coefficients. To correct this bias, we obtain weights $\lambda_{i\tau}$ such that $E(S_{i\tau}^*) = E(S_{i\tau})\lambda_{i\tau}$. Equation (1) is then rewritten as:

$$(2) \ln y_{it} = X_{it}\beta + \alpha_s S_{it} \lambda_{i\tau} + \alpha_i + \alpha_t + e_{it}.$$

Notice that τ denotes the year of shock while t denotes the calendar year. The weight $\lambda_{i\tau}$ is subsequently referred to as the price weight. Notice that $\lambda_{i\tau} = \lambda_{j\tau}$, for any i and j , i.e., for shocks occurring in the same year, the price weights are the same. Also notice that $\lambda_{i\tau}$ decreases over time. Applying this weight to the shock dummy thus reduces the influence of more recent shocks in regressions. Estimates based on (2) are free of the composition bias caused by the price effect. The details of the construction of $\lambda_{i\tau}$ can be found in Appendix A.

We run two regressions on model (2) and present their results in Columns (4) and (5) of Table 2. The regression in Column (4) multiplies the weights to all the observations of the health shock dummy, assuming price-induced composition bias affects all health shocks. The regression in Column (5) multiplies the weights only to the observations corresponding to the shocks arising from expenditures larger than 5,000 yuan. As a result, we can regard the estimate of the first regression as providing the upper bound of the impacts of health shocks, and the second regression as providing the lower bound when the price effect is taken into account.

As being expected, the estimates for the shock dummy are larger in both regressions

than the estimate reported in Column (1). The estimate in Column (4) is -0.192, and the estimate in Column (5) is -0.131. The baseline result of -0.123 assuming no price-induced composition bias underestimates the impact of a health shock by 35.9% if compared with the upper bound, and by 6.1% if compared with the lower bound.

In summary, we do find evidence for the existence of the composition bias, but accounting for this bias would most likely increase the magnitude of the impact of a health shock. Health shocks happening in later years have a large presence in our baseline regression. To the extent that they are less severe than early shocks, they tend to result in underestimates of the impacts of health shocks. Our weighted regressions have provided two estimates for the extent of the underestimation. Our baseline result then can be regarded as providing the lower bound of the average impact of health shocks.

2.3 Dynamic effects of health shocks

The long coverage of our data allows us to have a fuller understanding of the negative impacts of health shocks over time. The basic model we are going to estimate is a variant of the baseline model (1) with the health shock dummy being replaced by a set of dummies indicating the order of year after the shock, i.e., the year when a shock happened, the first year after the shock, the second year after the shock, and so on. The time period of our data, which runs from 1987 to 2002, allows us to run up to the 15th year after the shock.

[Table 4 about here]

The key results of the baseline dynamic model are presented in Column (1) of Table 4. The estimates are significant from the shock year to the 13th year after the shock. Figure 3 plots these estimates along with their 5% confidence intervals. The polynomial trend is an interesting *U*-curve with its bottom at the eighth year after the shock. In that year, the shortfall of income is 31.2%. Even in the 13th year, the shortfall is still 14.7%. Therefore, health shocks lead to a prolonged poverty trap.

[Figure 3 about here]

To fully understand the *U*-shaped trajectory requires a serious dynamic model that is beyond the scope of this paper. Intuitively, when a household receives a shock, the first impact would be a loss of labor supply. This could have an immediate effect to reduce its income. Because of no coverage or only a limited coverage from health insurance, the household would have to pay all or most of the expenses for treatments. As a consequence, the household may have to draw from its savings to pay for the expenses. In many cases, it may be forced to borrow from relatives, neighbors, friends, or informal credit markets. Since these informal borrowings are not likely to meet all its needs, the household may have to reduce its consumption and/or investment in productive assets including children's education (Sun and Yao, 2006), which could lead to lower current and/or future productivity. Consequently, its income may continue to drop even if the sick member recovers. The decline in income would stop only when the household begins to accumulate productive assets at a rate higher than its steady-state rate.

The baseline results are checked with two more regressions. The first one addresses the concern that the declining trend before the 8th year after the shock might be driven by the concentration of more recent and less severe shocks in the early portion of that trend. Therefore, we restrict the sample to shocks happening in or before 1995; that is, we want to check the validity of the declining portion of Figure 3 by using only early shocks. The results are presented in Column (2) of Table 4. Although the estimates become more irregular than those presented in Column (1), the declining trend still exists albeit its bottom moves back to the fifth year. The irregular estimates may be caused by a smaller sample.

The second regression weighs the shock dummies by the price weights. Since our previous results show that correcting all the shocks may tend to overstate their impacts, here we only multiply the price weights to shocks defined by expenditures. The results are presented in Column (3) of Table 4. Not surprisingly, the estimates of this regression

are larger than those obtained in the baseline regression except in the third and fourth year after the shock. Their trajectory mimics the one formed on the baseline results; it would form a parallel but generally deeper *U*-curve if it were also plotted in Figure 3.

The lengthy time coverage of our panel data also allows us to test whether the estimated effects of health shocks could merely capture declines in income that were already happening before the shock. Declining income may lead to worsened health; then the estimated negative impacts of health shocks may just pick up the declining trend. Conversely, it is also possible that households had experienced a recent transitory increase of income so they could afford to buy medical services for sick family members. In this case, declines of income after health shocks may therefore simply be due to mean reversion.¹⁰

To check these two possibilities, we add to the baseline model of Table 4 fifteen dummies indicating the order of year before a family received a shock. The year of the shock is used as the reference and hence omitted in the regression. Instead of presenting the results in a table, we plot the estimates of the year dummies and their 5% confidence intervals in Figure 4. The line connecting the estimates after the shock is still a *U*-curve albeit deeper than in Figure 3. It seems that the income before the shock is indeed smaller than the income of the shock year as all their estimates are not larger than zero, the default value of the shock year.¹¹ However, all but two estimates (for the 11th and 10th year before the shock) are between 0 and -0.10, and only one (for the 10th year before the shock) is statistically significant from 0. Starting from the 9th year onward, the estimates bounce between 0 and -0.10 and do not show a trend.

[Figure 4 about here]

The weak upward trend of income before the shock is understandable because the pre-shock year dummies also capture some of the income growth over time. The key

¹⁰ We owe the editor for pointing out the above two possibilities.

¹¹ This is also why a deeper *U*-curve is found for years after the shock when pre-shock year dummies are added. Now we are comparing the post-shock years with the year of the shock whereas in the baseline model we were comparing them with the average of the pre-shock years, which is shown here to be smaller than the income of the shock year.

message here is that there is no abrupt increase of income before the shock so our finding of negative impacts of the shock cannot be attributed to mean reversion. Nor can they be attributed to merely picking up a declining trend of income that has already happened before the shock because no such trend is found.

3. The Role of Village Elections

The timing of the first election varied amongst our 48 sample villages, as shown in Figure 5. Twelve of the sample villages were among the first in the nation to introduce elections in 1987. By 1990, more than half of the sample villages had at least one election. Two features not shown in Figure 5 are relevant for our econometric estimations. One is that there was a clear regional pattern in the introduction of the first election. Villages in the same province tended to introduce the first election around the time when the province enacted an implementation version of the OLVC. The other is that the introduction of the first election was not related to the level of income. For example, villages in both Zhejiang (an affluent province) and Sichuan (a poorer province) began to have elections in 1987 whereas villages in Guangdong province (an affluent province) only began elections in 1998.

[Figure 5 about here]

As long as the election is introduced, a village is required to hold elections every three years. Except for a few delays, our sample villages had followed this rule. Therefore, our main focus is the introduction of the first election as it marks the start of grassroots democracy in a village.

A likely key channel for village elections to mitigate the negative impacts of health shocks is to encourage the establishment of health care plans in a village. To the extent that risk aversion prevails in the population, a health care plan benefits all. But due to the lack of initiative of the leadership or an institutionalized mechanism for collective decision making, such a plan may not come up in the absence of elections. On the other

hand, the literature reviewed in the introduction shows that elections increase public expenditures and reduce administrative spending in the village budget, so it may not necessarily cost families more to establish a health care plan. A task of this sub-section is therefore to study if elections help the villages establish health care plans. Moreover, we are interested in finding out whether health care plans helped mitigate the negative impacts of health shocks. Of course, previous studies have shown that elections have other ways to increase families' abilities to deal with health shocks. Our strategy then is to study the effects of health care plans and elections separately.

3.1 Elections, health care, and health shocks

We first study how elections and health care plans affect households' abilities to deal with health shocks. For that, we consider the following model:

$$(3) \ln y_{it} = X_{it}\beta + \alpha_s S_{it} + \alpha_{ps} S_{it} \times P_{it} + \alpha_p P_{it} + \alpha_i + \alpha_t + e_{it},$$

where P_{it} is the dummy variable standing for health care plans or elections, and the definitions of the other variables and parameters are the same as in model (1). The key parameter is α_{ps} , which should be positive if health care plans or elections play a positive role in mitigating the negative impacts of health shocks. The election dummy equals 1 for a village after it introduced the election and 0 otherwise. Like the health shock dummy, this definition allows for the accumulative effects of elections.

The health care dummy takes value 1 when a village had a health care plan in a specific year and value 0 when it did not have one. The difference between this dummy and the election dummy is that villages could stop a health care plan in one year and restart it later whereas elections continued as long as they were started.

We code a village as having a health care plan as long as it had any health care plan --- the CMS, township-sponsored systems, commercial plans, or any other plan --- regardless of the benefits they provided.¹² Among the 48 sample villages, twenty-eight

¹² The 2003 survey asked both the villages and sample households about health care plans. There were discrepancies

did not have any health care plan while four had a health care plan over the entire sample period. The rest of the villages had a health care plan in part of the sample period.

[Table 5 about here]

We first run a baseline regression on model (3) for health care plans and elections, respectively. Their results are shown in Columns (1) and (2) of Table 5. While health care plans are shown not to mitigate the negative impact of health shocks on income, elections are shown to reduce it by 10.1 percent points with a significance level of 1%. Elections themselves are found to increase income substantially, but this result may only capture the time trend because of the way we have defined the election dummy. This last point also reminds us that elections' positive role in reducing the negative effects of health shocks might also be related to time trends. There could be many forces, some observable, some not, that have contributed to enhancing households' abilities to deal with health shocks over time. The election dummy may just pick up the trend. On another count, the health shocks in more recent years were less severe than those in early years, so elections' positive role may only reflect the reduced severity of health shocks. On the other hand, existing health care plans might only insure more severe illnesses, so health care plans' insignificant effect may also be an artifact of reduced severity of health shocks. Next, we run two robustness checks for these two possible problems.

The first robustness check--to see if the effects of health care plan and elections are related to omitted time trends--is to replace the year fixed effects by village-specific time trends in the baseline regression. These trends are defined as the village dummies multiplied by a quadratic time trend. We use $(\text{year} - 1986)$ as the measurement of time. The quadratic time trend is used to allow for more flexibility in capturing the rate of income growth in different villages. In addition to controlling heterogeneous time trends,¹³ the village-specific time trends effectively deal with the potential issue of

between the reports of the villages and those of the sample households. We use the reports of the villages realizing that reports of households might contain more noises.

¹³ In fact, the village-specific time trends also place more control on temporal preference changes over health than the year dummies because such changes might be heterogeneous across villages.

endogeneity in our estimates of α_{ps} . Both health care plans and elections are village-level events. The village-specific time trends absorb unobserved village-specific and time-varying characteristics, so the impacts of health care plans and elections can be identified within the village. Given that household fixed-effects are included, the model is therefore identified by comparing the same households with or without shocks in the same village.

The results of the two regressions with village-specific quadratic time trends are presented in Columns (3) and (4) of Table 5. While Column (3) shows that health care plans still do not have a significant effect to reduce health shocks' negative impact, elections remain significant albeit with reduced significance level and magnitude. Its effect is to reduce health shocks' negative impacts by 8.3 percent points, smaller than what we have got from regression (2). This shows that ignoring village-specific time trends will indeed lead to an overestimation of the role of elections.

Our second robustness check--to see if the effects of health care plans and elections are linked to the reduced severity of shocks in later years--is to weigh the shock dummy by the price weights. Like when we studied the dynamic effects of health shocks, here we apply these weights only to health shocks solely defined by expenditures larger than 5,000 yuan. The regression results are presented in Columns (5) and (6) of Table 5. The coefficients of the health shock dummy increase slightly over the ones obtained in the baseline regressions in Columns (1) and (2). But again, the estimate of α_{ps} is not significant for health care plans, but is for elections. Its magnitude for elections is slightly smaller than the baseline result in Column (2). Because shocks of later years were less severe than shocks in early years, elections might have larger effects on them. Since the number of shocks of later years was larger than the number of shocks of early years, this will then lead to overestimating elections' role if the composition bias is not corrected.

To confirm this last point, we replace the $S_{it} \times P_{it}$ term in the baseline regression (3)

by its interaction terms with the shock year dummies that we used in Table 3. These new interaction terms decompose the effects of elections by year of shock; we expect that the effects on later years would be stronger than the effects on early years if the last point were correct. The regression results, presented in Table 6, indeed confirm this expectation. Still using 1996 as the cutoff year, the later period contains larger and more significant results than the early period. The average of the estimates for the later period is 0.230 whereas the average for the early period is only 0.111.¹⁴

[Table 6 about here]

The above results suggest that elections have other ways to help families smooth their consumption and wealth accumulation. These could include those increasing the mean income as well as other public policies targeting on the poor specifically. To the extent that it is more difficult for the poor to smooth their consumption and wealth accumulation, pro-poor policies can have stronger effects to stabilize family income, and for that matter, to facilitate families' handling of health shocks.

3.2 Elections and health care plans

The previous sub-section showed that health care plans do not play a significant role in reducing health shocks' negative impacts. One reason for this result might be that the health care plans in our sample villages might not provide sufficient coverage and benefits for major health shocks. Among the seventeen villages that had some kind of health care plan in 2002, the average coverage was only to cover 46.9% of the village population. In addition, the average deduction was 515 yuan, and the average annual reimbursement limit was 5,071 yuan. However, a health care plan, no matter how rudimentary it may be, has its own virtues that are worth promoting. At the minimum, it can serve as a seed for a better health care plan of the future when income increases. This

¹⁴ Another explanation is that elections became substantially more effective in recent years. This might be true as elections have become more competitive after the formal version of the OLVC was introduced in 1998. However, we would not have seen the effect of elections to decrease in the weighted regression of Column (6) in Table 6 if this had been the main reason because the election dummy is the same in this regression and the baseline regression of Column (2). So we conclude that increased competitiveness of elections is not a reasonable explanation.

sub-section therefore studies whether elections promote the establishment of health care plans. Among the twenty villages that had a health care plan for at least a period of time between 1987 and 2002, three had a plan before they started village elections and continued till 2002, while four started health care plans before elections but stopped (two of them) or had a period of interruption (two of them). The rest of the nine villages began to have a plan in or after the year when they held the first election. Therefore, it is likely that elections promote health care plans.

Following Fernández-Val (2007)'s suggestion for panel probit models, we run the following linear probability model (LPM) with village and year fixed effects:

$$(4) \quad I_{jt} = Z_{jt}\beta + \alpha_E E_{jt} + \alpha_j + \alpha_t + e_{jt},$$

where I_{jt} is the dummy variable indicating whether the j th village had a health care plan in year t ; Z_{jt} is a set of control variables at the village level; E_{jt} is the election dummy defined before; α_j is the village specific effect for the j th village; α_t is the year fixed effect for year t ; and e_{jt} is an error term clustered at the village level. The set Z_{jt} includes four variables, the logarithm of village average per-capita income (in 2002 yuan), the logarithm of village population, the Gini coefficient of per-capita household income, and the ratio of households having been hit by health shocks so far. The Gini coefficient is calculated using the original NFS household sample instead of the 2003 sample since the original NFS sample covers more households than the 2003 survey.

It is natural to expect that villages with higher levels of income would be more likely to have a health care plan. Since a larger population increases the difficulty for collective decision, and a higher Gini coefficient implies a more divided population, both could reduce the chances for a village to set up a health care plan. On the other hand, a village is more likely to establish a health care plan when more households have been attacked by health shocks, probably even if there are no elections happening. Descriptive statistics of the control variables as well as the health care and election dummies can be found in Table 1.

[Table 7 about here]

For comparison, we first run an LPM for the introduction of elections. Its results are presented in Column (1) of Table 7. In addition to the control variables used in model (4), the election regression also controls for a dummy variable indicating whether a province had adopted the OLVC. OLVC is an administrative law and requires provinces to set up parallel local laws to implement it. Although exceptions existed, many villages started elections after their province adopted the OLVC. The regression shows that per-capita income, population, and the incidence of health shocks are not significant in the regression. The result on income gives us confidence that no reverse causality exists in our early results concerning the role of village elections. Two variables are significant in the regression. One of them, not surprisingly, is the adoption of the OLVC, which is shown to increase a village's chances to start elections by 29.9%. The other significant variable is the Gini coefficient, which is shown to reduce a village's chances by 0.36% for an increase of 0.01 in its value. The first result is expected. The second result shows that it is more difficult for an economically divided village to start elections. In the political economy literature (e.g., Alesina and Rodrik, 1994; Benabou, 1996), it is believed that democracy leads to more redistributive policies because income distribution is usually skewed toward the lower end. It is then natural to expect that poorer people, who outnumber richer people, would be more motivated to hold elections in an economically more divided village. However, a more divided village may not start elections precisely because the rich and the poor have deeper conflicts of interests—and therefore the rich may object more strongly to elections. This last point is reminiscent of Acemoglu and Robinson (2006)'s argument that democratization is more difficult to build and consolidate when the privileged feel threatened by the redistributive policies brought by democracy.

To understand the effect of elections on establishing health care plans, we run three regressions on model (4) and present their results in the last three columns of Table 7.

Column (2) excludes the election dummy for comparing the determinants of health care plans and elections. Income and the incidence of health shocks have significant coefficients. An increase of 1,000 yuan for every person in a village raises its chances to set up a health care plan by 2.6%; a one-percent increase in the incidence of health shocks raises the chances by 0.42%. Column (3) adds the election dummy. While the results for variables other than this dummy remain qualitatively the same, the introduction of election is shown to increase a village's chances to set up a health care plan by 5.4% with a statistical significance of 10%. This is a reasonable estimate for the data. Since nine villages began to have health care plans after they started elections, and seven began before they started elections, but two of them stopped, the odds that elections induced health care plans were between two to four in forty-eight, or 4.2% to 8.4%.

The last regression reported in Column (4) replaces the common year fixed effects by the quadratic village-specific time trends we used before. Adding these trends may prove quite pertinent here because the coding of the health care and election dummies may cause spurious correlation between them. Allowing for heterogeneous time trends across villages, the regression places a good control of the time trends in the health care dummy in each village and will thus provide us a more reliable estimate for the election dummy. While the results of other variables do not change qualitatively, the coefficients of income and the incidence of health shocks become insignificant. Therefore, their significant estimates in the previous two regressions may well be picking up the time trends. The magnitude of the coefficient of the election dummy is not changed, but its statistical significance increases to 5%. Although the number of villages in our dataset is relatively small, the stringent control placed in the last regression raises our confidence that our result will survive qualitatively in future studies using larger samples.

4. Conclusions

Using a longitudinal sample of households in 48 Chinese villages for the period of 1987-2002, this paper finds that a major health shock has strong and persistent negative impacts on household income. In the first sixteen years after the shock, a shock-hit household drops below its normal income trajectory by an average of 12.3%. We have also found strong evidence that village elections help alleviate the negative impacts of health shocks. By our most conservative estimate, households living in villages with elections are found to be able to avoid the income reduction due to health shocks by 8.3 percentage points. In addition, villages are found to be more likely to set up a health care plan after it has started elections.

Several methods are applied to deal with the composition bias in the retrospective reports of health shocks. We find that the bias exists and tends to result in downward biases for estimates of health shocks' negative impacts and upward biases for estimates of elections' role in reducing these impacts. The reason is that shocks reported for later years tend to be less severe than and outnumber shocks for early years.

Our findings make a tangible contribution to the understanding of grassroots governance. Recent development literature emphasizes the role of informal social networks in helping families to pool risks in developing economies. To the extent that informal social networks have always existed in rural China, our finding that village elections increase families' capabilities to smooth their income shows that public interventions have certain advantages over private and informal networks.

There is of course a finance problem, that is, public interventions need higher taxes to support. However, better local governance can shift public spending from wasteful and redistributive purposes to more productive and long-term purposes. For example, Wang and Yao (2007) find that village elections have raised the share of public investment and reduced the share of administrative expenditures in village budget. They also find that

elections have not increased the level of taxation in the village. Zhang *et al.* (2004) find similar results using a different dataset.

Our findings also have strong implications for the debate on the Chinese experiment of village elections. There are serious doubts both within and outside China on the election's effectiveness in achieving its goals. These are legitimate doubts because Chinese villages are democratic islands in the vast sea of a one-party state. Our results provide a strong piece of evidence on the positive role that the village election has played in the last twenty years. The election has delivered tangible economic benefits to villagers as well as empowered them politically.

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Table 1. Basic statistics of variables

	Whole sample		1987		1995		2002	
	Mean	St dev	Mean	St dev	Mean	St dev	Mean	St dev
Household variables								
(1,185 households for 1987-2002, 13,515 cases)								
Per-labor net income (2002 yuan)	5,717.6	10,624.0	3574.4	4291.9	5,568.9	7,900.9	9,206.7	21,988.5
Health shock dummy	0.22	0.42	0.05	0.21	0.18	0.38	0.45	0.50
Per-capita land (<i>mu</i>)	1.37	2.02	1.48	1.95	1.36	1.90	1.50	3.27
Household head age	47.0	12.8	39.0	11.6	47.2	11.9	53.6	11.8
Village variables (48 villages for 1987-2002, 759 cases)								
Election dummy	0.70	0.46	0.23	0.43	0.77	0.43	0.98	0.14
Health care plan dummy	0.27	0.45	0.06	0.25	0.17	0.38	0.23	0.42
Per-capita income (2002 yuan)	2,801.4	2,923.2	2,023.1	1,238.4	3,033.6	2,683.5	4,162.9	4,937.8
Population	1,464.2	1,066.6	1,375.1	994.7	1,478.4	1,089.0	1,530.1	1,158.4
Gini coefficient	0.28	0.08	0.26	0.08	0.28	0.08	0.30	0.08
Incidence of health shocks	0.18	0.16	0.04	0.06	0.15	0.10	0.40	0.21

Notes: Income is in 2002 yuan converted by the rural consumer price index published in NBS (2003). Data for household variables are from the NFS household survey and the 2003 survey. Among village variables, the election and health care plan dummies and incidence of health shocks are from the 2003 survey; per-capita income and population are from the NFS village survey; Gini coefficient is based on the per-capita income provided by the NFS household survey.

Table 2. Average effects of health shocks on income

Variables	Baseline model (1)	Earlier period (1987-1996) (2)	Later period (1997-2002) (3)	Weighted regression I (4)	Weighted regression II (5)
Constant	8.356*** (1.007)	11.123*** (0.243)	-10.458 (6.774)	8.369*** (1.007)	8.359*** (1.007)
Health shock dummy	-0.123*** (0.027)	-0.152*** (0.046)	-0.131** (0.055)	-0.192*** (0.041)	-0.131*** (0.029)
Per-capita land (<i>mu</i>)	0.046*** (0.006)	0.089*** (0.012)	0.041*** (0.009)	0.046*** (0.006)	0.046*** (0.006)
Age of household head	0.007 (0.022)	-0.076*** (0.030)	0.392*** (0.135)	0.007 (0.022)	0.007 (0.022)
Age of household head squared	-0.267E-3*** (0.571E-4)	0.163E-4 (0.103E-3)	-0.488E-3 (0.302E-3)	-0.268E-3*** (0.571E-4)	-0.268E-3*** (0.571E-4)
Adjusted R ²	0.386	0.472	0.382	0.386	0.386
Number of observations	13,515	7,290	5,027	13,515	13,515

Notes: All four models are estimated by the panel method with household and year fixed effects. Columns (1), (4) and (5) use the whole sample of 1,185 households of 48 villages in the period 1987-2002; columns (2) and (3) use data for the periods 1987-1996 and 1997-2002, respectively. Column (3) also deletes households who had health shocks before 1997. Column (4) weighs the entire health shock dummy by the price weights; Column (5) only weighs the observations of the shock dummy that correspond to shocks defined only by expenditures. Standard errors are clustered at the household level; their estimates are reported in the parentheses.

* Significant at the 10% significance level; ** Significant at the 5% significance level; *** Significant at the 1% significance level.

Table 3. The effects of health shocks by year of shock

Year	Y1988	Y1989	Y1991	Y1993	Y1995	Y1996
Estimate for health shock dummy	-0.069 (0.149)	-0.203** (0.106)	-0.343*** (0.131)	-0.236*** (0.081)	-0.078 (0.084)	-0.201** (0.081)
Year	Y1997	Y1998	Y1999	Y2000	Y2001	Y2002
Estimate for health shock dummy	0.074 (0.068)	-0.129* (0.073)	-0.164** (0.072)	-0.098 (0.073)	-0.116 (0.075)	-0.100 (0.107)

Notes: The regression uses the same specification and sample of Column (1) in Table 2 to estimate model (1) by replacing the health shock dummy with its interaction terms with the shock year dummies. The reported estimates are for the coefficients and their cluster-corrected standard errors of the resulting dummies. Household and year fixed effects are included in the estimation. The year 1987 is omitted to avoid collinearity with the fixed effects of households who had a shock in that year. The R^2 is 0.387. Results of other variables are not shown.

* Significant at the 10% significance level; ** Significant at the 5% significance level; *** Significant at the 1% significance level.

Table 4. Dynamic effects of health shocks

Year after shock	Column (1)	Column (2)	Column (3)
	Baseline regression	1987-95 sample	Weighted regression
0 th	-0.096 ^{***} (0.038)	-0.103 [*] (0.059)	-0.098 ^{**} (0.044)
1 st	-0.119 ^{***} (0.044)	-0.248 ^{***} (0.092)	-0.129 ^{***} (0.051)
2 nd	-0.136 ^{***} (0.046)	-0.247 ^{***} (0.092)	-0.145 ^{***} (0.054)
3 rd	-0.096 [*] (0.055)	-0.044 (0.149)	-0.092 (0.067)
4 th	-0.104 [*] (0.055)	-0.176 ^{**} (0.087)	-0.104 (0.066)
5 th	-0.201 ^{***} (0.067)	-0.380 ^{***} (0.150)	-0.239 ^{***} (0.083)
6 th	-0.145 ^{**} (0.066)	-0.362 ^{***} (0.095)	-0.184 ^{**} (0.082)
7 th	-0.274 ^{***} (0.076)	-0.193 (0.150)	-0.337 ^{***} (0.095)
8 th	-0.312 ^{***} (0.074)	-0.273 ^{**} (0.120)	-0.338 ^{***} (0.085)
9 th	-0.225 ^{***} (0.075)		-0.261 ^{***} (0.085)
10 th	-0.289 ^{***} (0.085)		-0.322 ^{***} (0.093)
11 th	-0.227 ^{***} (0.083)		-0.252 ^{***} (0.091)
12 th	-0.242 ^{***} (0.088)		-0.266 ^{***} (0.095)
13 th	-0.145 [*] (0.089)		-0.181 [*] (0.096)
14 th	-0.145 (0.103)		-0.168 (0.108)
15 th	-0.136 (0.124)		-0.155 (0.126)
Adjusted R ²	0.386	0.491	0.386

Notes: All three regressions include household and year fixed effects. Columns (1) and (3) use the full sample of 13,515 observations; Column (2) uses the sub-sample of 1987-1995 with 6,322 observations. The weighted regression weighs the shocks defined by expenditures by the price weights. Results of other variables are not shown. Standard errors are clustered at the household level; their estimates are reported in the parentheses. * Significant at the 10% significance level; ** Significant at the 5% significance level; *** Significant at the 1% significance level.

Table 5. The roles of health care plans and elections

Variables	Baseline regressions		Corrected for village-specific time trends		Weighted regressions	
	(1)	(2)	(3)	(4)	(5)	(6)
	Health care	Elections	Health care	Elections	Health care	Elections
Health shock dummy	-0.133 ^{***} (0.030)	-0.202 ^{***} (0.049)	-0.120 ^{***} (0.031)	-0.199 ^{***} (0.050)	-0.137 ^{***} (0.033)	-0.207 ^{***} (0.051)
Shock dummy × Health care dummy	0.034 (0.048)		-0.042 (0.053)		0.020 (0.051)	
Health care dummy	-0.013 (0.035)		0.288 ^{***} (0.057)		-0.009 (0.035)	
Shock dummy × Election dummy		0.101 ^{***} (0.049)		0.083 [*] (0.050)		0.097 ^{**} (0.050)
Election dummy		0.314 ^{**} (0.024)		0.400 ^{***} (0.036)		0.315 ^{***} (0.024)
Per-capita land (<i>mu</i>)	0.046 ^{***} (0.006)	0.047 ^{***} (0.006)	0.056 ^{***} (0.006)	0.055 ^{***} (0.006)	0.046 ^{***} (0.006)	0.047 ^{***} (0.006)
Age of household head	0.008 (0.022)	0.024 (0.022)	0.029 ^{***} (0.008)	0.023 ^{***} (0.008)	0.008 (0.022)	0.024 (0.022)
Age of household head squared	-0.269E-3 ^{***} (0.572E-4)	-0.297E-3 ^{***} (0.567E-4)	-0.261E-3 ^{***} (0.611E-4)	-0.252E-3 ^{***} (0.608E-4)	-0.269E-3 ^{***} (0.572E-4)	-0.297E-3 ^{***} (0.567E-4)
Village dummies × (year – 1986)	No	No	Yes	Yes	No	No
Village dummies × (year – 1986) ²	No	No	Yes	Yes	No	No
Adjusted R ²	0.386	0.388	0.403	0.409	0.386	0.396

Notes: Regressions (1), (2), (5), and (6) are estimated by the panel method with household and year fixed effects; regressions (3) and (4) are estimated with only household fixed effects. Regressions (5) and (6) use the price weights to weigh the observations of the health shock dummy defined only by expenditures larger than 5,000 yuan. The number of observations is 13,515 for 1,185 households of 48 villages in the period 1987-2002. Standard errors are clustered at the household level; their estimates are reported in the parentheses. Results for the constants are omitted.

* Significant at the 10% significance level; ** Significant at the 5% significance level; *** Significant at the 1% significance level.

Table 6. The effects of elections by year of shock

Year	Y1987	Y1988	Y1989	Y1991	Y1993	Y1995	Y1996
Health shock dummy × Election dummy	-0.139 (0.087)	0.239** (0.113)	0.189* (0.097)	0.027 (0.139)	0.044 (0.096)	0.261*** (0.101)	0.159 (0.098)
Year	Y1997	Y1998	Y1999	Y2000	Y2001	Y2002	
Health shock dummy × Election dummy	0.372*** (0.089)	0.146 (0.092)	0.140 (0.092)	0.243*** (0.093)	0.211** (0.093)	0.267** (0.120)	

Notes: The regression uses the same specification and sample of regression (2) in Table 6 replacing the interaction term of the health shock dummy and the election dummy by interacting it with the shock year dummies. The reported estimates are for the coefficients and their cluster-corrected standard errors of the resulting dummies. Household and year fixed effects are included in the estimation. The R^2 is 0.407. Results of other variables are not shown.

* Significant at the 10% significance level; ** Significant at the 5% significance level; *** Significant at the 1% significance level.

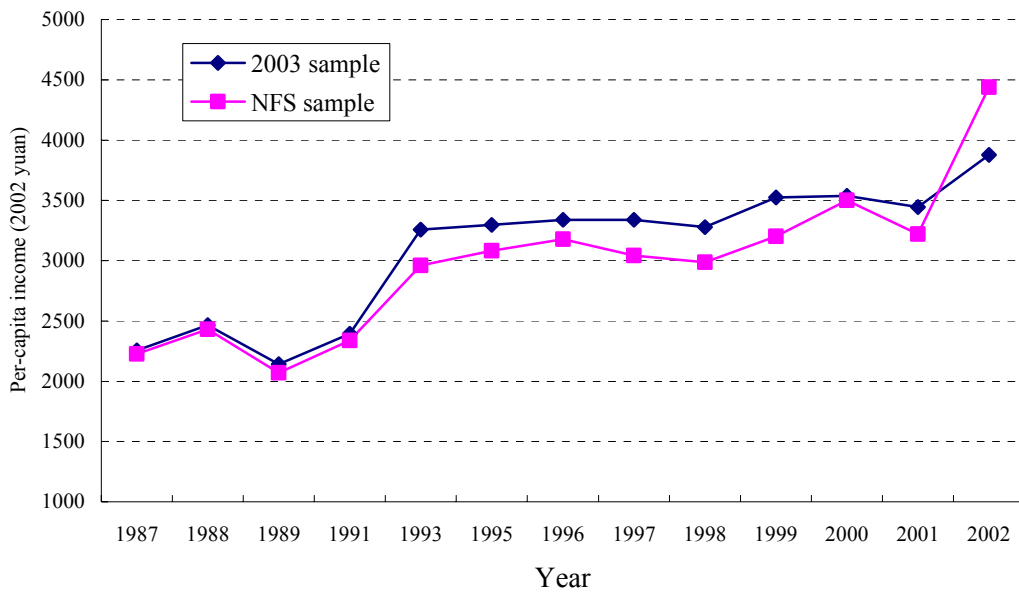
Table 7. Determination of elections and health care plans

	Elections	Health Care Plans	Health Care Plans	Health Care Plans
	(1)	(2)	(3)	(4)
Per-capita income (1,000 yuan)	0.007 (0.016)	0.026** (0.013)	0.025** (0.013)	0.002 (0.014)
log(population)	0.006 (0.128)	0.019 (0.106)	0.017 (0.106)	0.0087 (0.094)
Gini coefficient	-0.355* (0.217)	0.148 (0.177)	0.179 (0.178)	0.189 (0.138)
Ratio of shock-hit families	-0.114 (0.139)	0.417*** (0.114)	0.425*** (0.114)	-0.137 (0.110)
Adoption of OLVC	0.299*** (0.042)			
Introduction of elections			0.054* (0.030)	0.054** (0.025)
Adjusted R ²	0.637	0.741	0.743	0.919

Notes: All four regressions are estimated by the LPM. The sample size is 759 for 48 villages in the period of 1987-2002. Columns (2) and (3) have both village and year fixed effects; Column (4) replaces the year fixed effects by quadratic village-specific time trends. Village-level clustered standard errors are reported in the parentheses.

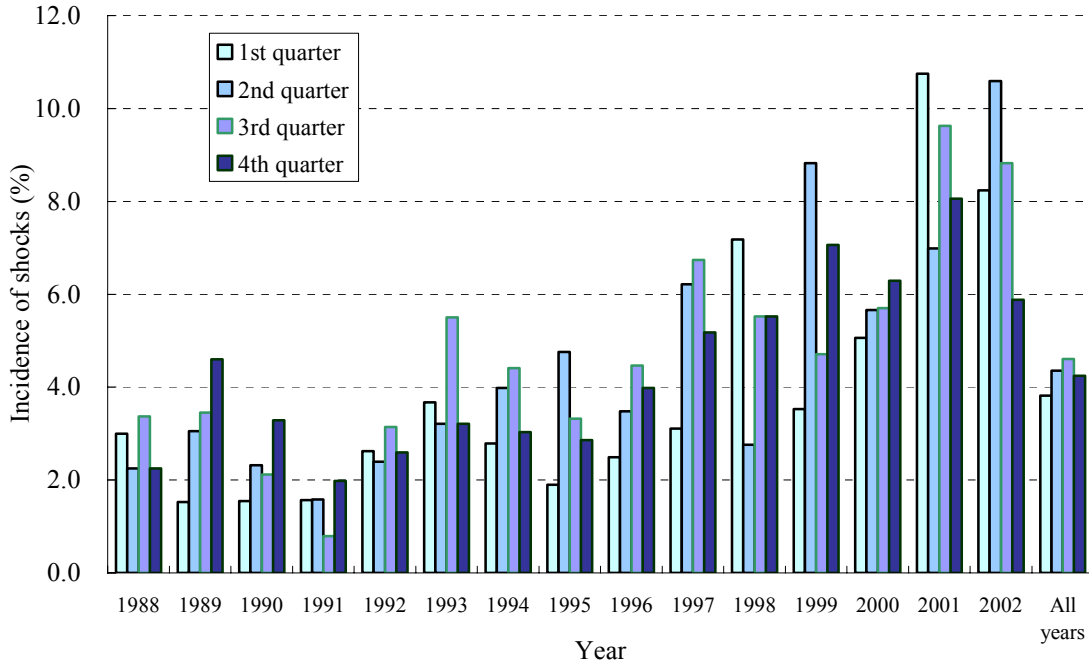
* Significant at the 10% significance level; ** Significant at the 5% significance level; *** Significant at the 1% significance level.

Figure 1. Per-capita net income in the 2003 and NFS samples



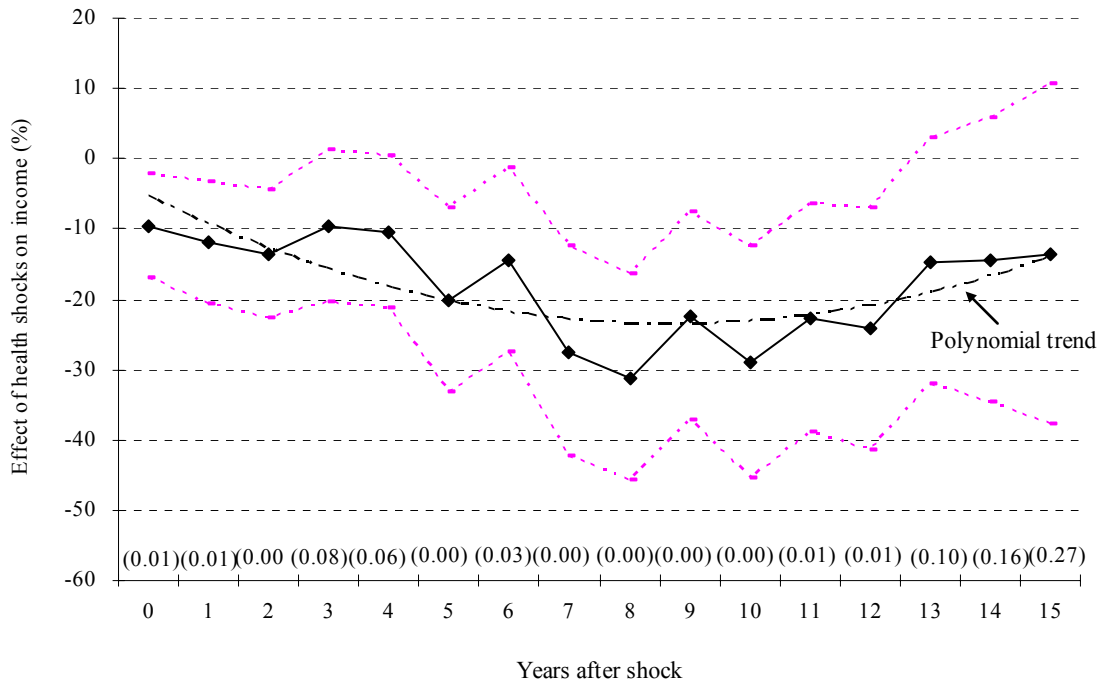
Notes: Income has been converted to 2002 RMB using the rural consumer price index published in NBS (2003). The NFS sample is for the 48 villages covered by the 2003 survey.

Figure 2. Incidence of shocks by income quarter and year



Notes: Income quarters are based on per-labor net income. Years after a household received a shock are deleted. Income quarters are lagged for one year (in the case of 1991, 1993, and 1995, two year lags are used). Income has been converted to 2002 yuan using the rural consumer price index published in NBS (2003).

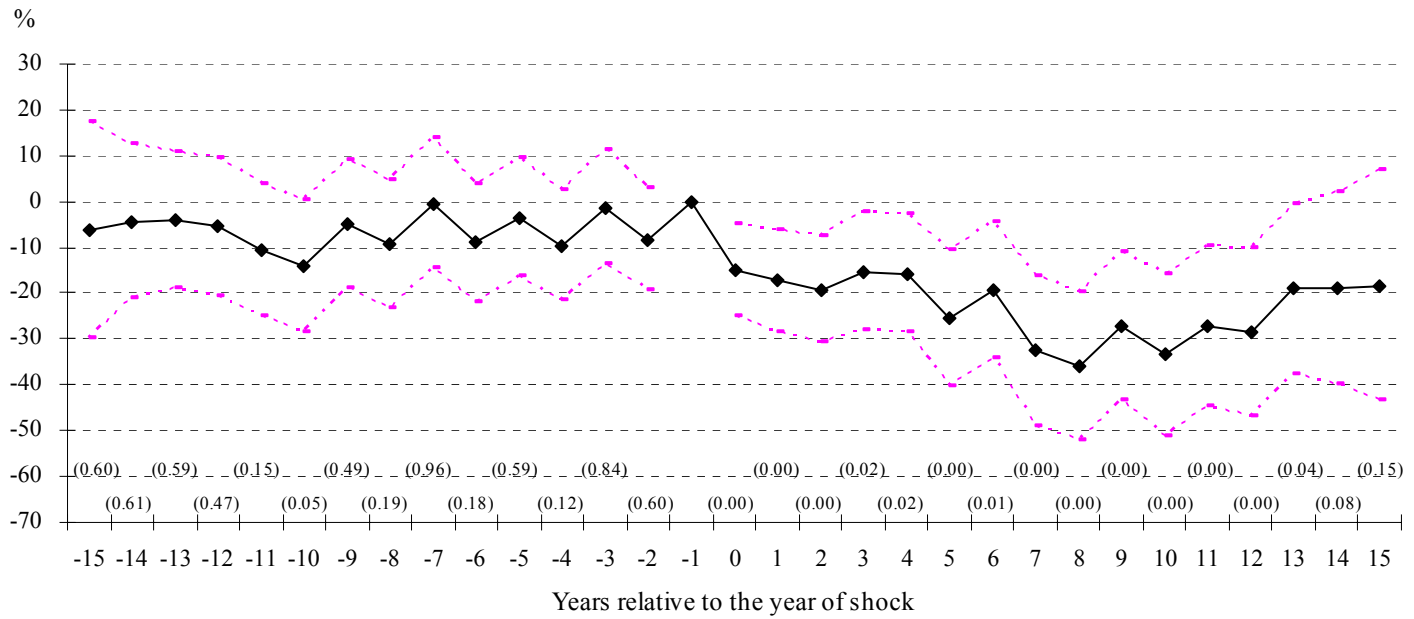
Figure 3. Dynamic effects of health shocks on income



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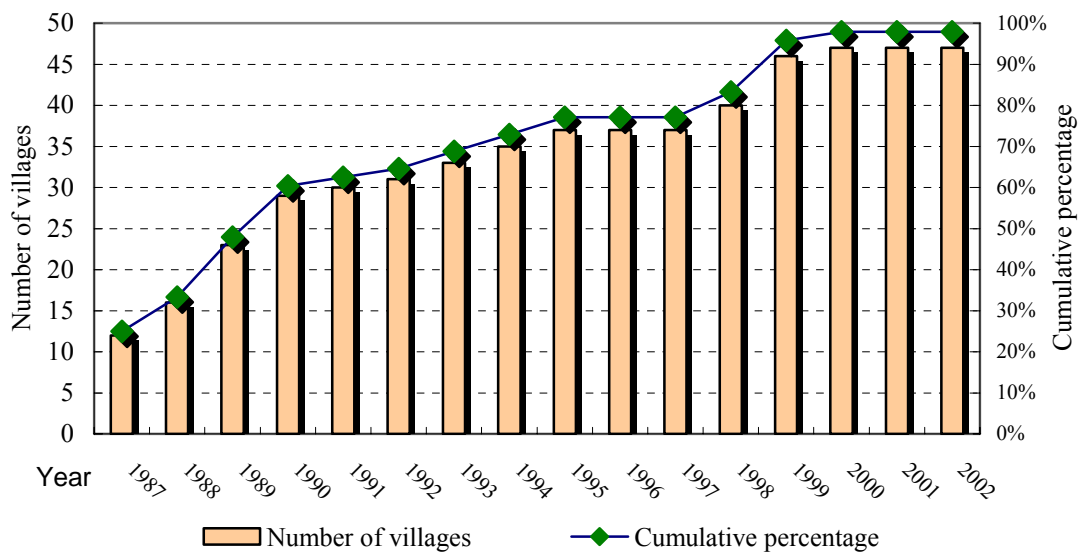
Notes: The solid line connects the estimates for the effects of health shocks on household per-labor income based on the baseline model reported in Column (1) of Table 4; the band between the two dashed lines contains the 5% confidence intervals of the estimates. Figures in the parentheses are the significance levels of the respective estimates.

Figure 4. Dynamic effects of health shocks: controlling pre-shock income growth



Notes: The solid line connects the estimates for the effects of shocks on household per-labor income; the band between the two dashed lines contains the 5% confidence intervals of the estimates. Figures in the parentheses are the significance levels of the respective estimates.

Figure 5. Introduction of elections in sample villages



Appendix A: Derivation of the price weights

Consider a representative household. Let D_τ^* be its medical spending in year τ in 1987 prices, while D_τ be the corresponding value in prices of year τ . The nominal threshold, denoted as \bar{d}_0 , is 5,000 yuan in year τ 's current prices. The observed incidence of health shocks defined by current expenditure in year τ , i.e., S_τ is given by

$$S_\tau = 1(D_\tau > \bar{d}_0).$$

To ensure that health shocks are comparable across years, the true incidence of health shocks should be comparing the threshold in real terms, which is 5,000 yuan in 1987 yuan and denoted here as \bar{d}_{87} , to the medical expenditure measured in 1987 yuan, D_τ^* , as in

$$S_\tau^* = 1(D_\tau^* > \bar{d}_{87}).$$

Let the total inflation in year τ over 1987 be z_τ . We have

$$D_\tau = (1 + z_\tau)D_\tau^*.$$

Therefore,

$$(A1) \quad E(S_\tau) = \Pr(D_\tau > \bar{d}_0) = \Pr\left(D_\tau^* > \frac{\bar{d}_0}{1 + z_\tau}\right) \geq E(S_\tau^*) = \Pr(D_\tau^* > \bar{d}_{87}),$$

where the equality holds for $\tau = 1987$. As a consequence, estimates using S_τ would be biased when the true model requires S_τ^* to make shocks comparable across years. To correct the bias, we further assume that D_τ^* has a log-normal distribution,

i.e., $\ln(D_\tau^*) \sim N(\mu, \sigma^2)$. According to (A1), we have

$$\begin{aligned} E(S_\tau) &= \Pr\left(D_\tau^* > \frac{\bar{d}_0}{1 + z_\tau}\right) = \Pr\left(\ln(D_\tau^*) > \ln\left(\frac{\bar{d}_0}{1 + z_\tau}\right)\right) \\ &= 1 - \Phi\left(\frac{\ln(\bar{d}_0 / (1 + z_\tau)) - \mu}{\sigma}\right). \end{aligned}$$

Rearrange this equation, we have

$$(A2) \quad \ln(\bar{d}_0 / (1 + z_\tau)) = \mu + \sigma \Phi^{-1}(1 - E(S_\tau)),$$

where Φ^{-1} is the inverse of the standard normal *cdf*. Since the threshold \bar{d}_0 , inflation rate z_τ , and the incidence of health shocks $E(S_\tau)$ are all observable, a least square estimation of (A2) would yield estimates of μ and σ . The weight we want for year τ is the ratio

$$(A3) \quad \lambda_\tau = \frac{E(S_\tau^*)}{E(S_\tau)} = \frac{1 - \Phi[\ln(\bar{d}_0 - \hat{\mu})/\hat{\sigma}]}{1 - \Phi\{\ln[\bar{d}_0/(1 + z_\tau) - \hat{\mu}]/\hat{\sigma}\}},$$

where $\hat{\mu}$ and $\hat{\sigma}$ are the least square estimates based on (A2). To correct the bias, we simply multiply S_τ by λ_τ in regressions wherever S_τ enters linearly in regressions as a regressor.

The most appropriate price index that should be used here is the price index for rural medical services. However, this index is not available. Instead, we use rural consumer price index. We estimate (A2) using the 16 year data of the incidence of shocks in the period of 1987-2002 and yield -2.59 and 5.29 for $\hat{\mu}$ and $\hat{\sigma}$, respectively. The estimated parameters are then used to construct the set of weights λ_τ .