BREAKING THE "IRON RICE BOWL:" EVIDENCE OF PRECAUTIONARY SAVINGS FROM CHINESE STATE-OWNED ENTERPRISES REFORM

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ABSTRACT. We estimate the magnitude of precautionary saving by using China's large-scale reform of state-owned enterprises (SOEs) in the late 1990s as a natural experiment to identify changes in income uncertainty. Before the reform, SOE workers enjoyed similar job security as government employees. The reform caused massive layoffs in the SOEs, but government employees kept their "iron rice bowl." The changes in unemployment risks for SOE workers relative to that for government employees before and after the reform provide a clean identification of changes in income uncertainty for estimating precautionary saving. Our estimation also controls for a self-selection bias in occupational choices and disentangles the effects of uncertainty from pessimistic outlooks on saving behaviors. Our results suggest that precautionary saving is important and accounts for about 35 percent of the wealth accumulation for urban SOE workers between 1995 and 2002.

I. INTRODUCTION

Precautionary savings are potentially important for wealth accumulation, especially for an emerging market economy like China that has experienced large structural changes associated with policy reforms, which may have led to substantial increases in economic uncertainty. However, estimating the importance of precautionary saving has been a challenge in the empirical literature. One difficulty is to identify large and exogenous variations in income uncertainty (Lusardi, 1998; Carroll and Kimball, 2008). The literature typically uses the

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cross-sectional variances of income as a proxy for income uncertainty (Carroll and Samwick, 1998), and it is well known that such proxies suffer from measurement errors and potential endogeneity biases for estimating precautionary saving (Kennickell and Lusardi, 2005).

A second difficulty stems from a self-selection bias related to occupational choices. Precautionary saving depends not just on risk, but also on risk preferences (Caballero, 1990, 1991). Risk preferences affect not just saving behaviors, but also occupational choices. A more risk averse individual would save more for given income risks, but she is also likely to choose an occupation with lower income risks. The correlations between risk preferences and occupational choices imply a self-selection bias, and failing to control for this self-selection can lead to a significant downward bias in estimating precautionary saving (Fuchs-Schündeln and Schündeln, 2005).

A third difficulty is to disentangle the effects of income uncertainty from the effects of income expectations on saving behaviors. If an individual expects lower future income paths, she would choose to save more to smooth consumption. But this increase in saving reflects an optimal response to changes in permanent income (i.e., a PIH effect), instead of precautionary saving, which captures the response to increases in perceived income uncertainty (Gourinchas and Parker, 2002; Fuchs-Schündeln, 2008).

Partly reflecting the difficulties in measuring income uncertainty, correcting self-selection biases, and disentangling income uncertainty from income expectations, the existing literature has obtained mixed evidence of precautionary saving. Some studies report weak or no evidence of precautionary saving (Dynan, 1993; Guiso et al., 1992), while some other studies attribute a large fraction (50% or more) of household wealth accumulation to precautionary saving (Carroll and Samwick, 1998; Gourinchas and Parker, 2002).[1]

This paper presents new empirical evidence for precautionary saving using Chinese data. To address the first difficulty, we argue that China's large-scale reforms of state-owned enterprises (SOEs) in the late 1990s provides a natural experiment for identifying variations in income uncertainty. Prior to the reform, workers in the SOEs and the government sector (GOV) enjoyed similar job security, with guaranteed pensions and near-free health care and housing. In this sense, workers in both sectors held an "iron rice bowl" before the reform. Following the reform, however, over 27 million workers in the SOEs were laid off between 1997 and 2002. Those workers lost not just their jobs, but also the associated benefits. In contrast, workers in the government sector—where few layoffs occurred—were little affected by the reform; they were able to hold on to their iron rice bowl. The massive layoffs in the SOE sector significantly changed the perceived job security for the remaining SOE workers.

[1]See Carroll and Kimball (2008) for a survey.

The reform was largely unexpected to an individual worker and it created significant variations of unemployment risks for workers across the SOE and GOV sectors. Thus, the reform provides a clean identification of variations in perceived income uncertainty across time and across sectors.

To implement the idea that the SOE reform can be used as a natural experiment for estimating precautionary saving, we use the Chinese Household Income Project (CHIP) survey data and design a difference-in-differences (DID) approach, in which we focus on households in two sectors (SOE and GOV) and two CHIP surveys (1995 and 2002).[2] The large-scale SOE reform started to have significant impacts on SOE employment in 1997, with the effects tapering gradually through 2002. Thus, our sample covers both the pre- and post-reform periods. This data structure allows us to estimate the differences in household saving both across sectors (SOE vs GOV) and across time (before and after the reform). The time variations (between 1995 and 2002) of the relative saving behavior of workers across the two sectors capture the magnitude of precautionary savings caused by the SOE reform.

We address the remaining two difficulties for estimating precautionary saving by exploiting some unique features of the CHIP survey data. To mitigate the self-selection bias associated with occupational choices, we restrict our sample to the households whose jobs were assigned by the Chinese government. This approach is similar to Fuchs-Schündeln and Schündeln (2005), who estimate the importance of self-selection biases using the event of German reunification. As in the case of the former German Democratic Republic (GDR) studied by Fuchs-Schündeln and Schündeln (2005), job assignments by the Chinese government were often restricted by political considerations and job outcomes were often unrelated to individual preferences. Indeed, focusing on jobs assigned by the government in our sample turns out to substantially weaken the link between workers' occupational choices and their risk attitude.[3] Interestingly, the magnitude of self-selection biases in our estimates is remarkably similar to that obtained by Fuchs-Schündeln and Schündeln (2005).

To disentangle the effects of precautionary motives on saving from the PIH effects, we use the information on households' income expectations in the 2002 CHIP survey. By further restricting the sample to those households who did not expect their future income to decline, we are able to separate out the PIH effects on saving from our estimates of precautionary

[2]We also have the CHIP survey data for 1988 and 2007, although those surveys do not report wealth information and are thus less useful for studying precautionary savings.

[3]In practice, job assignments by the government were not completely independent of worker preferences because workers could signal their preferred job positions to the government before actual assignments took place. By focusing on the subsample with government assigned jobs, we are able to mitigate, but not completely eliminate the effects of self-selection. Nonetheless, we still obtain estimates of precautionary saving that are substantially greater than that obtained from the full sample.

saving. We find that, in our sample, the self-selection biases and the PIH effects are both quantitatively important.

By identifying changes in income uncertainty caused by the SOE reform, mitigating self-selection bias in occupational choices, and controlling for PIH effects, we obtain estimates of precautionary savings that are significant both statistically and economically. We estimate that precautionary savings accounted for about 35 percent of financial wealth accumulations for urban SOE workers during the period from 1995 to 2002. The evidence of precautionary saving is robust when we control for potential changes in the sample of SOE workers after the reform and when we take into account alternative wealth measures and differences in pension benefits between SOE workers and GOV workers.

Furthermore, consistent with the life-cycle consumption theory, we find stronger evidence of precautionary savings for younger households (25-45 years) than for older households, similar to what Gourinchas and Parker (2002) find using U.S. data. We also find that workers in local SOEs have much stronger precautionary saving motives than workers in SOEs owned by the central government or provincial governments, consistent with the fact that layoffs were concentrated in small and local SOEs (Hsieh and Song, 2015).

Our empirical approach to estimating precautionary saving is closely related to the seminal contribution by Fuchs-Schündeln and Schündeln (2005), who use the German reunification event to identify and quantify the self-selection bias for estimating precautionary savings. Following Fuchs-Schündeln and Schündeln (2005), we focus on the sample of workers with government-assigned jobs to mitigate self-selection biases associated with occupational choices. In addition, the CHIP data provide information about household income expectations, which help us disentangle the PIH effects on saving from precautionary saving. More importantly, since our data covers both the pre- and post-reform periods, we use the large-scale SOE reform event as a natural experiment to quantify the importance of changes in income uncertainty for precautionary saving.[4] Despite the differences in the data samples and the particular implementations of empirical methodologies, our estimate of precautionary savings using Chinese data is remarkably similar to that obtained by Fuchs-Schündeln and Schündeln (2005) using German data (35% vs. 22%).

Our study is also related to the literature on Chinese saving rate, although we do not intend to directly address the specific issue of what drives the rising Chinese saving rate. The recent studies by Chamon and Prasad (2010) and Chamon et al. (2013) show that the increased private burden of expenditures on housing, education, and healthcare combined with the lack of social safety net in China help explain the rising Chinese saving rate.

[4]The GSOEP sample used by Fuchs-Schündeln and Schündeln (2005) begins in 1990, after the reunification.

Some other studies examine the importance of life-cycle and other demographic factors for explaining China's high and rising saving rate (Kraay, 2000; Modigliani and Cao, 2004; Horioka and Wan, 2007). Wei and Zhang (2011) provide evidence that sex-ratio imbalances have led to a competitive savings motive: with a shortage of girls, parents with a son save more to increase the relative attractiveness of their son in a tighter marriage market. Curtis et al. (2011) present an overlapping generations model calibrated to Chinese data and show that demographic changes in China (such as changes in the dependency ratio caused by the one-child policy and population aging) account for a substantial fraction of the observed rise in China's saving rate. Our focus is instead on the general issue of identifying and quantifying precautionary savings. We provide empirical evidence that increases in income uncertainty associated with large structural changes in China have contributed to substantial precautionary wealth accumulation for urban Chinese households.

II. Some Background of the SOE Reform

From 1949 to 1978, China's economy was under a central-planning regime. The government maintained tight controls over production and factor allocations. Most jobs were assigned by the government. To support the goal of industrialization, workers were paid subsistence wages and, in exchange, they were guaranteed life-time employment along with near-free housing, education, health care, and pension (Cai et al., 2008). This cradle-to-grave regime is known as the "iron rice bowl," which has long been advocated as one advantage of China's socialist system.

In the late 1970s, the Chinese government under Deng Xiaoping's leadership initiated an "open door" economic policy and systematic economic reform, setting off China's transition to a free-market economy. In the mid-1980s, the government introduced a labor contract system, under which workers were permitted to search for jobs and employers gained some flexibility in hiring (Meng, 2000). These reform policies led to a large-scale urban migration and increased competition facing SOEs. Following Deng Xiaoping's tour of the south in 1992, more liberalization policies were adopted by the government, leading to a boom in urban economies, which further intensified competition for SOEs. At that time, with soft budget constraints and the requirement to implement the government's goal of full-employment, the SOE sector had substantial redundant labor and many SOE firms were making losses. In 1995 and 1996, around 50% of the SOEs (mostly small or medium sized) reported losses (Meng, 2003). The Asian financial crisis in 1997 exacerbated the situation.

The Chinese government was forced to take actions to improve efficiency of the SOEs and to stem losses. Specific actions were laid out at the Fifteenth Communist Party Congress held in September 1997. A central spirit of the restructuring policy was to "grasp the large and let go of the small." Large (and usually more profitable) SOEs in strategic sectors such as electricity, oil, raw materials, and telecommunications were corporatized and maintained under state controls, while smaller (and often loss-making) SOEs were either privatized or let go bankrupt (see Hsieh and Song (2015)). These policy changes led to a massive layoff (*xia gang* in Chinese) of SOE workers starting in 1997, the scale of which was unprecedented. By the end of 1997, a cumulative of about 6.92 million SOE workers were laid off. The wave of layoffs reached a peak in 1999, with about 6.2 million SOEs workers losing their jobs in that year. The massive wave of layoffs started to subside by 2002. During the 5-year period from 1997 to 2002, a remarkable total of over 27 million SOE workers had been laid off.[5] In contrast, government employees were little affected by the reform. According to the Chinese Household Income Project (CHIP) survey, which is the dataset that we use for estimating precautionary saving, 58% of the individuals who had layoff experience prior to 2002 worked in SOEs. In contrast, only 2.3% of those individuals worked for the government.[6]

There is evidence that the SOE layoffs were concentrated in small and loss-making firms and in some demographic groups. For example, female, less educated, less skilled, less healthy workers, and non-members of the communist party were more likely to be laid off than others. Workers in SOEs owned by local governments were also more likely to be laid off than those in SOEs owned by the central government (Appleton et al., 2002). However, the scale and the breadth of the layoffs were largely unexpected by individual workers (see Appendix A for a case study of the SOE layoff experience). Thus, for the SOEs workers who were fortunate to keep their jobs, the reform that broke the iron rice bowl had led to significant changes in their perceptions about future job security and substantially increased their perceived income uncertainty.

III. Empirical Strategies

III.1. **The Empirical Model.** To estimate precautionary saving, we follow Lusardi (1998) and Carroll et al. (2003) and consider the empirical model

$$W_i/P_i = \beta_0 + \beta_1 SOE_i + \beta_2 RISK_i + \beta_3 \log(P_i) + \beta_4' Z_i + v_i. \tag{1}$$

In this model, the dependent variable is the ratio of financial wealth W_i to permanent income P_i for household i. This ratio measures the household's cumulative savings relative to her permanent income. The explanatory variables include the dummy variable SOE_i, which takes a value of one if the household head works for an SOE and zero if the household head

[5] Data source: China Labor Statistical Yearbook, 2003.

[6] The remaining 39.7% worked in the private sector.

works for a government or public institution (GOV);[7] the variable $RISK_i$ that measures idiosyncratic income risks; the log-level of permanent income P_i that allows for the possibility of nonhomothetic preferences; and a vector of demographic characteristics summarized by the variable Z_i. The term v_i denotes regression errors.[8]

We take a difference-in-differences approach to estimating precautionary saving. Since we cannot keep track of individual households in the CHIP data, we run two separate cross-sectional regressions using equation (1), one with the pre-treatment group in 1995 and the other with the post-treatment group in 2002.[9] The key parameter of interest is β_1, the coefficient for the SOE dummy. The estimated β_1 from each regression (denoted by β_1^{1995} and β_1^{2002}, respectively) captures – all else equal – the average excess savings by SOE workers relative to GOV workers. The difference $\Delta\beta_1 = \beta_1^{2002} - \beta_1^{1995}$ then captures the magnitude of precautionary saving of the SOE workers caused by increases in their unemployment risks following the breaking of the iron rice bowl.

It is important to recognize that, while the SOE dummy captures income uncertainty specific to SOE workers derived from unemployment risks, the variable $RISK_i$ reflects idiosyncratic income risks when workers are employed. In our data, these two variables are uncorrelated, with a correlation coefficient of about -0.04 in each of the two sample years, consistent with our view that they capture different aspects of the risks for individual households.

In our estimation, we follow Fuchs-Schündeln and Schündeln (2005) and instrument the permanent income measure using education dummies and interactions of education with age and age-squared as instrumental variables. We also address the issue that arises with observations of zero wealth by treating it as a censored data problem.[10] Thus, we estimate our model using an instrumental variable Tobit regression (IV-Tobit). In a robustness check, we also estimate the model in equation (1) by eliminating the zero-wealth observations from

[7]For a single-earner family, the household head is the bread winner. For a multiple-earner family, the head is the person with the highest income.

[8]The model specification here is consistent with theories of life-cycle consumption and savings (Lusardi, 1998; Carroll and Samwick, 1998), which predict that the ratio of wealth to permanent income is a function of income uncertainty and household characteristics. We have also estimated an alternative model in which the dependent variable is the logarithm of financial wealth instead of the ratio of financial wealth to permanent income and obtained similar estimates of precautionary savings.

[9]The lack of panel data implies that the treatment group (the SOE workers) may not be stable over time. In particular, the post-treatment group includes only those who survived the SOE reform and those who chose not to quit from their SOE jobs. These issues may cause biases in our estimation. We address this sample stability issue in Section V.

[10]In our sample, 11.3% of households have zero wealth in 1995 and this share declined to 4.5% in 2002.

our sample and then applying the standard two-stage least squares (2SLS) method (see Section VI.4).

III.2. **The Data.** The data that we use are taken from the Chinese Household Income Project (CHIP) surveys. The surveys were conducted by the Chinese Academy of Social Science (CASS) and National Bureau of Statistics (NBS) through a series of questionnaire-based interviews done in rural and urban areas in China in four different years— 1988, 1995, 2002 and 2007. The households in each survey are randomly selected following a strict sampling process so that they are nationally representative. The surveys cover a sample of about 15,000 to 20,000 households in about 10 provinces in China. The surveys contain detailed data on households' employment status, education, income, expenditures, wealth, and other demographic information. The CHIP data have been frequently used in the empirical literature.[11]

In this paper, we focus on the sample of urban households in the CHIP surveys of 1995 and 2002, which span the period of China's large-scale SOE reforms that had led to massive layoffs of SOE workers. We restrict our sample to include only those households whose heads work in the SOE sector or the GOV sector. Before the reform, workers in these two sectors had similar job security. The reform has led to a large number of layoffs of SOE workers, while GOV workers were able to keep their iron rice bowl. The reform thus injected substantial income uncertainty to those SOE workers who survived the layoffs relative to GOV workers. The different impact of the reform on workers across the two sectors provides an ideal natural experiment for identifying precautionary saving due to a sudden and substantial increase in unemployment risks for SOE workers.

The SOE sector includes firms that are directly owned by the government (including central, provincial, and local governments), those in which the government holds a controlling share of stocks, and those under collective ownership. The GOV sector includes all levels of government and public institutions.[12] We further restrict our sample to include prime-age workers, whose ages are between 25 and 55 years. This choice is partly driven by concerns of measurement errors in wealth and permanent income for younger workers. It is also driven by concerns that the saving behaviors of workers close to retirement ages change dramatically

[11]The website `http://www.icpsr.umich.edu/icpsrweb/ICPSR/series/243` lists some recent studies that use the CHIP survey data.

[12]According to the *China Labor Statistics Year Book*, the SOE and the GOV sectors together employed about 94.1% of total urban workers in 1995. This share declined to 75.5% in 2002. During this period, however, the large-scale SOE reform has led to a substantial decline in the relative share of employment in the SOE sector from 70.5% to 42.4%.

for reasons more closely related to life-cycle factor than to income uncertainty (Carroll and Samwick, 1998; Gourinchas and Parker, 2002).[13]

With these sample restrictions, we end up with 4390 household-level observations in 1995, consisting of 2977 SOE workers and 1413 GOV employees; and in 2002, we have 3027 observations consisting of 1702 SOE workers and 1325 GOV employees.

III.3. **The Measurement.** The variables that we use in the regressions include wealth (W), permanent income (P), the SOE dummy, a measure of idiosyncratic risks ($RISK$), and a set of household characteristics. Table 1 provides a brief description of our measurement of these variables.

Our measure of financial wealth (W) is the sum of checking accounts, savings accounts, stocks, bonds, loans to others, family business assets, and other business assets (Item 401 in the CHIP surveys). These assets are liquid and are thus useful to safeguard against income uncertainty (Carroll and Samwick, 1998). We use the stock of financial wealth instead of the flow of saving (or the saving rate) for two reasons. First, unlike the flow of saving, financial wealth is not influenced by high-frequency fluctuations in income and expenditures. Thus, it is better able to capture long-run (or average) saving behavior in which we are interested. Second, financial wealth is a direct measure of cumulative savings and is thus less subject to measurement errors than the flow of saving or the saving rate, which are indirectly calculated based on income and consumption expenditures.

We construct a measure of permanent income following the approach by Fuchs-Schündeln and Schündeln (2005). The CHIP surveys report earnings by the household heads in the current year and the recent past. In particular, the 1995 survey reports earnings in 1990 through 1995 and the 2002 survey reports earnings in 1998 through 2002. We construct permanent income in three steps. First, we calculate a household head's earnings relative to the average earnings of all households in each year with reported earnings. Second, we take the time-series average of the household relative earnings. Third, we multiply the household head's earnings in each of the survey years (1995 or 2002) by the average relative earnings to obtain an annual permanent income for the household in that year.[14] To mitigate potential measurement errors introduced in the process of constructing permanent income, we follow Fuchs-Schündeln and Schündeln (2005) by instrumenting permanent income using education

[13]The normal retirement age for female workers in China is between 50 and 55; for male workers, it is between 55 and 60.

[14]We use box plot to detect possible outliers in the data of wealth measures and permanent income. We first determine the first and third quartiles (denoted by Q_1 and Q_3, respectively) for the data set. Define the interquartile range $IQR = Q_3 - Q_1$, which is a measure of noise or scale for the data set. Observations that are more than three IQR's are treated as potential outliers and excluded from the sample.

dummies and interactions of education with age and age-squared as instruments in all the regressions.[15]

We measure idiosyncratic income risks ($RISK_i$) by the coefficient of variation (CV) of the logarithms of income, which is the ratio of the standard deviation to the mean of log income over the current year and the recent past as reported in the CHIP surveys. In our sample, average household income has grown substantially from 1995 to 2002 and different households have experienced different income growth. Thus, for our sample, using the unit-free measure CV is more appropriate for comparing the response of saving behaviors to changes in idiosyncratic income risks across time than using the standard cross-sectional variances of log income in the literature (Carroll and Samwick, 1998).

The demographic characteristics that we consider include the household head's age, age-squared, gender, marital status, occupation, the household size, the ages of children, the number of boys, and the number of children at school, health care status (public health care, public health insurance, or own payments), home ownership status, and the industry and the province where the household head works.

We divide the occupations of the household heads into four groups: (1) professional, (2) director or manager, (3) skilled or office workers, and (4) unskilled, service workers, or other workers. The last group is our reference group in the regressions.

The health care reform enacted in 1998 significantly changed the share of household expenditures on health care. We categorize the types of health care that the households receive into three groups: public health care (almost free), public health insurance, and self-financing of health care. As shown in Table 2, in 1995, 71.3% of households in our sample had access to free public health care. This share was halved to about 35.0% in 2002, reflecting the impact of the health care reform on household health expenditures.

To control for the effects of rising education expenditure on households' saving, we include in the regressions the mean age of children and the number of children at school. To control for effects of potential competitive savings motive emphasized in Wei and Zhang (2011), we add the number of boys among children as an independent variable.

Purchasing a house is argued to be one of the major motives of saving for Chinese households (Wei and Zhang, 2011). The housing reform that started in 1998 has led to extensively privatized housing market. As shown in Table 2, the homeownership rate in our sample doubled over the seven year period, from 42.0% in 1995 to 80.4% in 2002. We control for

[15]We categorize the education level of a household head into four groups: elementary school or below, middle school, high school, and some college or above. We take the first group as a reference group when constructing education dummies.

the potential effects of saving for home purchases by including a non-homeownership dummy that takes a value of one if the household is not a home owner and zero otherwise.

Since the SOE reform and the massive layoffs hit some industries and geographic areas more heavily than others, we include in our regression dummy variables that indicate the industries and provinces where the household head worked.

Table 3 compares some key characteristics between GOV and SOE workers. It shows that the reform impacted GOV workers and SOE workers differently. In 1995, before the reform took place, GOV employees had on average modestly more financial wealth and higher permanent income than SOE workers. The wealth-income ratios (W/P), however, were similar (at around 1.38). In 2002, the gaps in both wealth and income between workers in the two sectors widened substantially, with GOV workers owning even more wealth and earning even more income than SOE workers compared to the pre-reform year in 1995. However, the wealth-income ratios diverged. In particularly, the W/P ratio for the SOE workers increased much more than that for the GOV workers, suggesting that SOE workers on average raised savings more than GOV workers did in the post-reform years. Consistent with this suggestive evidence, our estimation below shows that SOE workers did increased their savings significantly relative to GOV workers in response to increased income uncertainty associated with the massive layoff waves.

Table 3 also shows that the homeownership rate for the two types of workers. In 1995, the home ownership rate for GOV workers was slightly higher than for the SOE workers (45% vs. 40%). In 2002, the home ownership rate rose for both groups (to 83% for GOV workers and 78% for SOE workers), although the difference in the average home ownership rates across the two groups remained unchanged.

In 1995, most jobs were assigned by the government in both sectors. In particular, nearly 90% of the GOV jobs and 80% of the SOE jobs were assigned by the government. In 2002, a large majority of jobs were still assigned by the government, although the percentage of assigned jobs declined somewhat in both sectors (to about 76% in the GOV sector and 69% in the SOE sector). When we estimate the importance of precautionary saving, we restrict our sample to government assigned jobs in both years to mitigate the self-selection bias related to occupational choices.

Furthermore, the SOE reform led to different income expectations between the two groups. In the 2002 survey, about 24% of the SOE workers expected to have lower income in the next five years, compared to 11% of GOV employees who expected income to decline.[16] As we

[16]The 1995 survey does not include a question about income expectations. Before the reform, since workers in both the sectors all hold an iron rice bowl, we argue that in general they would not expect their income to decline.

discuss below, pessimistic income outlooks can also raise saving, but such saving behavior represents a desire for intertemporal consumption smoothing (or PIH effects) rather than a motive for precautionary saving. To obtain a clean estimation of precautionary saving, we use the information about self-reported income expectations to disentangle the PIH effects from the precautionary motive.

IV. Empirical Results

We now report the main empirical results. First, we discuss the estimation results with self-selection corrected in Subsection IV.1 and then examine the quantitative importance of the self-selection bias and the PIH effects in Subsections IV.2 and IV.3, respectively. In Subsection IV.4, we conduct a counterfactual analysis to quantify the importance of precautionary saving for wealth accumulation. Finally, we investigate the lifecycle effects and the influence of SOE firm sizes on precautionary saving in Subsections IV.5 and IV.6.

IV.1. **Evidence of precautionary saving.** We now present evidence of precautionary saving when we correct the self-selection bias by focusing on the subsample with government assigned jobs. The estimation results for 1995 and 2002 are shown in Table 4 (columns (i) and (iii)).

The parameter of interest is the coefficient of the SOE dummy, β_1, which captures the difference in wealth accumulation between SOE and GOV workers when we control for the effects of all the demographic characteristics in the empirical model described by equation (1). The estimated value of $\beta_1 = 0.028$ is statistically insignificant in 1995 (column (i)), indicating that the saving behaviors of SOE and GOV workers were statistically (and economically) similar in 1995 when demographic characteristics are controlled for. In 2002, however, SOE workers saved significantly more than GOV employees (reflected by a much large estimate of $\beta_1 = 0.748$, shown in column (iii)). The Chow test strongly rejects the null hypothesis that β_1 is identical between 1995 and 2002, with a p-value of 0.020. The difference between the two estimated values of β_1 ($0.748 - 0.028 = 0.72$) is not just statistically significant, it is also economically large. These estimation results suggest that, all else equal, the extra savings of SOE workers relative to GOV workers after the reform were about 0.72 times of their annual permanent income, or a bit over 8 and a half months worth of permanent income.[17] This evidence suggests that increases in the relative income uncertainty following the reform has led to significant precautionary savings by SOE workers.

[17]The dependent variable in our model is the ratio of financial wealth to annual permanent income (W/P). Thus, an increase in W/P of 0.72 units implies an increase in W of an amount equivalent to $0.72 * 12 = 8.64$ months of permanent income.

We now discuss the interpretations of estimated coefficients for the control variables. In addition to the demographic controls such as the age, gender and occupation of the household head, we highlight here a few important control variables. These controls include an indicator of idiosyncratic income risks (CV), the permanent income (P) that captures non-homothetic preferences, and additional income or expenditure risks introduced by reforms between 1995 and 2002, such as health care reforms, education reforms, and housing reforms.

We continue to focus on the case with self-selection bias controlled for (columns (i) and (iii) in Table 4). The estimated coefficient β_2 of idiosyncratic income risks (CV) is positive and significant for both years. This result indicates that all households respond to increases in idiosyncratic income risks by increasing savings. We emphasize that this source of savings represents households' responses to variations in idiosyncratic income risks, and it is different from the responses of saving behaviors to job uncertainty specific to the SOE households captured by β_1. Furthermore, the estimated values of β_2 for 1995 and 2002 are similar and both are significant. In contrast, the value of β_1 was small and insignificant in 1995 but became much larger and significant in 2002. In other words, whereas β_2 stays roughly constant over time, β_1 has much larger time-variations that capture the effects of changes in job uncertainty for SOE workers caused by the reform.

The estimated coefficient β_3 of $\log(P)$ is positive in both years, although it is significant only in 2002, implying a significant income effect for that year. To control for the impact of health care spending on households' saving behavior, we include in the regression a dummy variable indicating public health care (mostly free) and another dummy indicating public health insurance. The coefficients of both dummy variables are small and insignificant in 1995 but become significantly negative in 2002. This result is intuitive. In 1995, most workers were covered under a near-free public health care system, so that the health care status did not impose any significant impact on households' saving behavior. However, after the urban employee basic medical insurance reform that started in 1998, a significant fraction of health care spending was shifted to private households (Huang and Gan, 2015). Thus, households not covered by public health care or public health insurance had a strong incentive to save. This finding is consistent with that obtained by Chamon and Prasad (2010), who report that declining public provisions of health care in the late 1990s in China created strong motives for precautionary saving against potential health expenditure shocks.

To control for the effects of education reforms on households' saving behavior and potential competitive saving motive in the marriage market emphasized by Wei and Zhang (2011), we include in our regression three additional variables: the mean age of children, the number of children enrolled in schools, and the number of boys in each household. Our estimation shows that the mean age of children does not explain wealth accumulation. The number of children enrolled in schools tends to reduce wealth accumulation in both years, although the effects were significant only in 2002. Having more children at school requires more expenditure on education after the education reforms in the late 1990s, which led to lower disposable income and reduced wealth accumulation. The number of boys contributes positively to savings in 1995, although the estimated coefficient is insignificant for that year. In 2002, however, having more boys in the household actually reduced savings and the effect is significant at the 10% confidence level. A possible explanation lies in the reforms of social security and the pension system, which substantially weakened the public safety net for retirees. In the Chinese culture, sons are supposed to take responsibility of taking care their elderly parents. Therefore, facing an uncertain future of safety net, having more boys means having better insurance for their parents. Parents thus do not need to save that much for their old-age consumption. In our 2002 sample, this self-insurance effect of having more boys dominates the potential competitive savings motive highlighted by Wei and Zhang (2011).

Finally, to control for the effects of housing reform on saving, we include in the regression a non-homeownership dummy. The coefficient for this variable is not significant for both years. A possible explanation is that, in 2002, the housing market was not fully developed and home purchases were still heavily subsidized. This result indicates that the saving motive for home purchases was weak in both 1995 and 2002.

IV.2. **The self-selection bias.** The literature shows that self-selection in occupational choices can lead to a substantial downward bias in the estimated magnitude of precautionary saving (Fuchs-Schündeln and Schündeln, 2005). An individual with high risk aversion has an incentive to choose a job with low income risk and, all else equal, she is also likely to save more. To correct the downward bias caused by self-selection, we restrict our sample to workers whose jobs were assigned by the government. To the extent that the government's job assignments are not systematically correlated with individual risk attitude, our sample restriction should mitigate the bias caused by self-selection in occupational choices.

Our estimation shows that self-selection did cause a significant downward bias in the estimated value of β_1 after the reform, but not before. As shown in Table 4 (column (ii)), in 1995, the estimated value of β_1 using the full sample (and thus without correcting for self-selection biases) is slightly smaller than that in the restricted sample with government assigned jobs (-0.020 vs. 0.028), and it remains statistically insignificant. In 2002, however, self-selection caused a large downward bias in the estimate of β_1. As shown in column (iv) of Table 4, the estimate of β_1 using the full sample is smaller and less significant both statistically and economically than that obtained in the restricted sample (0.387 vs 0.748).

The estimated magnitude of precautionary saving – captured by the difference between the estimated values of β_1 in the two sample periods – also declines substantially to 0.407

($= 0.387 - (-0.20)$) in the full sample from 0.720 obtained in the restricted sample. Thus, without correcting for the self-selection bias, the magnitude of precautionary saving would have been understated by 0.313 ($= 0.720 - 0.407$, in units of W/P), implying that precautionary wealth accumulation would have been under-estimated by an amount equivalent to a bit under 4 months of permanent income. This magnitude of self-selection biases is remarkably similar to that obtained by Fuchs-Schündeln and Schündeln (2005) using German data.

IV.3. **Disentangling PIH effects from precautionary saving.** The large-scale SOE reform not only led to significant changes in the relative job security between SOE and GOV employees, it also produced potentially large differences in future income expectations between the two groups. All else equal, a worker who expects declines in future income would like to increase saving, but such increases in saving reflects a desire for intertemporal consumption smoothing (i.e., a permanent income effect) rather than a motive of precautionary saving. To the extent that the difference in perceived job security and income expectations between the two groups of workers were both caused by the SOE reform, disentangling the PIH effect from precautionary saving is particularly important for the post-reform period in 2002.

To isolate the effects of precautionary motives on saving from the PIH effects, we use a unique question in the 2002 CHIP survey that asks households about their expectations of income paths for the next five years. As Table 3 shows, a significant fraction of SOE workers (23.8%) surveyed in 2002 expected future income declines, although a much smaller fraction of GOV workers (11.4%) expected income declines. Thus, the reform has caused different income expectations in addition to different unemployment risks across the two groups of workers.

To disentangle the PIH effects from the precautionary motive on saving, we separate the sample of SOE workers in the 2002 survey into two groups based on their reported expectations of future income. One group expected income to decline in the next five years, and the other group expected income to increase or stay the same. We estimate the empirical model in equation (1) for the two groups of SOE workers in 2002, respectively, using all GOV employees in that year as the control group.[18]

[18]Using all GOV employees as a control group allows us to compare the effects of different income expectations of SOE workers on their saving (i.e., the PIH effect on saving); it also allows us to compare the magnitude of estimated precautionary savings with or without controlling for the PIH effects. In an unreported exercise, we considered an alternative regression using the 2002 data without splitting the SOE sample based on income expectations. Instead, we added two variables to the baseline regression model (1): one is a dummy variable "decline" that takes a value of 1 if a household head expects future income to

Table 5 displays the estimation results. The first column shows the estimation results for the group of SOE workers who expected their income to decline. The second column shows the results for the group that did not expect their income to decline. In both cases, we restrict our sample to those workers whose jobs were assigned by the government to control for the self-selection bias.

For the group of SOE workers who expected their income to decline, the estimated value of β_1 (1.217) significantly exceeds the benchmark estimate reported in Table 4 (0.748). This finding is consistent with the PIH theory because this group of households increased their savings not just for precautionary reasons, but also for consumption smoothing. In contrast, the estimate of β_1 for those households who did not expect future income to decline is lower than the benchmark estimate (0.662 vs 0.748), although it remains statistically significant at the 5% level.

The PIH theory predicts that, all else equal, a household who does not expect future income to decline should save less and consume more. Thus, our estimation based on the 2002 subsample of SOE households who did not expect income to decline provides a lower bound of the estimation of precautionary savings.[19]

IV.4. **Importance of precautionary saving.** Using the SOE reform as a natural experiment, we have identified the presence of precautionary saving. But to what extent does precautionary saving account for the observed increases in financial wealth for SOE workers between 1995 and 2002? To answer this question, we follow the literature (Carroll and Samwick, 1998; Fuchs-Schündeln and Schündeln, 2005) to quantify the contributions of precautionary saving to wealth accumulation. The idea is to compare the difference between (1) the model's predicted change in financial wealth held by SOE workers from 1995 to 2002 and (2) the counterfactual change in financial wealth had SOE workers enjoyed the same job security as GOV workers before and after the reform.

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decline and 0 otherwise; and the other is an interaction term "$SOE \times decline$." In this alternative specification, the coefficient of the SOE dummy (β_1) captures precautionary saving of SOE workers relative to GOV employees when they do not expect future income to decline. The estimated value of β_1 is 0.746, with a standard error of 0.307. This estimated value is statistically significant at the 5% level and modestly larger than that obtained in our preferred specification with the SOE sample split based on income expectations. The values of coefficients of the "decline" dummy (-0.127) and the interaction term "SOE*decline" (0.147) are both insignificant.

[19]There are at least two reasons for the downward bias. First, we do not exclude workers who expected their future income to rise; whereas for this group, the PIH channel should induce them to save less. Second, workers who expected their future income to decline might be the group who also faced higher probability of being laid-off and higher future income uncertainty; those workers might have stronger motives for precautionary saving than the group who expected their income not to decline.

To implement this idea, we go through the following steps. First, we calculate the model's predicted wealth held by SOE workers in 1995 (denote this by $\hat{W}^{soe}_{1995}$) using the benchmark estimation results reported in column (i) of Table 4. Second, we calculate the predicted wealth held by SOE workers in 2002 (denote this by $\hat{W}^{soe}_{2002}$) using the estimation results reported in column (ii) of Table 5, where we have controlled for both the self-selection bias and the PIH effects. Third, we compute the counterfactual wealth holdings by SOE workers in each year of the surveys by assuming that those workers had the same job security as GOV employees, while keeping all the other characteristics unchanged. In particular, we use the same estimated coefficients as in the first two steps, except that we set the SOE dummy to zero. Denote by $\tilde{W}^{soe}_t$ the counterfactual wealth holdings of SOE workers in year $t \in \{1995, 2002\}$. In the fourth (and final) step, we compute the magnitude of wealth accumulation for precautionary reasons according to the relation

$$W^{ps} = (\hat{W}^{soe}_{2002} - \hat{W}^{soe}_{1995}) - (\tilde{W}^{soe}_{2002} - \tilde{W}^{soe}_{1995}), \tag{2}$$

where W^{ps} denotes the wealth accumulation from precautionary savings. The ratio $\frac{W^{ps}}{\hat{W}^{soe}_{2002} - \hat{W}^{soe}_{1995}}$ then measures the fraction of the changes in financial wealth held by the SOE workers that can be accounted for by precautionary savings.

Our estimation implies that precautionary savings account for 35.7% of financial wealth accumulation for SOE workers between 1995 and 2002, which is statistically significant with a standard error of 0.163. This result suggests that the SOE reform in the late 1990s have led to quantitatively important precautionary savings by SOE workers.

IV.5. **Lifecycle effects.** Households' consumption and saving behaviors vary significantly over the lifecycle. In an important contribution, Gourinchas and Parker (2002) estimate a structural lifecycle model using U.S. data and provide evidence that young households save for precautionary reasons whereas old households save mainly for retirement. To examine the lifecycle patterns of precautionary saving for Chinese households, we split our sample into two cohorts: a young cohort (aged 25-44) and an old cohort (aged 45-55). We estimate the benchmark model in equation (1) for each age cohort.

In 1995, the estimated coefficient β_1 for the SOE dummy variable is small and statistically insignificant for both age groups, as in the full sample. In contrast, in 2002, the estimated value of β_1 is very different for the two different age cohorts. Table 6 shows that, in 2002, the estimated value of β_1 for the young cohort is much greater than that for the full sample (1.081 vs. 0.662), and both are significant at the 5% level. The estimated value of β_1 for the old cohort is much smaller (0.222) and statistically insignificant. This evidence is consistent with the finding obtained by Gourinchas and Parker (2002) that young households behave as buffer-stock agents and old households behave more like certainty equivalent consumers.

IV.6. **SOE firm sizes.** The SOE reform in the late 1990s had very different impact on workers in large SOE firms than those in medium and small firms. The spirit of the reform was to "Grasp the Large and Let Go of the Small." Accordingly, large and profitable SOEs in strategically important sectors (such as oil, electricity, and telecommunications) were corporatized or consolidated into large state-owned conglomerates, while smaller and loss-making SOE firms were shut down or privatized (Hsieh and Song, 2015). Those large SOEs that survived the reorganization typically gained stronger government protections of their monopoly power, leading to higher profits than their privatized counterparts (Li et al., 2012). Since workers in large SOE firms typically face lower unemployment risks than those in small SOEs (Appleton et al., 2002), we should expect to see stronger precautionary saving motives for workers in smaller (and riskier) SOEs.

To examine this issue, we divide the SOE firms into two groups based on their size: central or provincial SOEs (CSOE) vs. local SOEs (LSOE).[20] Consistent with the evidence provided by Appleton et al. (2002), LSOE workers in the 2002 CHIP sample reported much more layoff experience than CSOE workers.[21] We modify the benchmark model in equation (1) by replacing the SOE dummy variable with the two dummy variables, indicating whether the household head works in a CSOE or an LSOE. The regression model is now

$$W_i/P_i = \beta_0 + \beta_1^{CSOE} CSOE_i + \beta_1^{LSOE} LSOE_i + \beta_2 RISK_i + \beta_3 \log(P_i) + \beta_4' Z_i + v_i \qquad (3)$$

where $CSOE_i$ and $LSOE_i$ are the two dummy variables indicating the type of the SOE firm in which the household head i works.

Table 7 reports the regression results. From 1995 to 2002, β_1^{CSOE} increased from -0.108 to 0.225, although it is not significant in both years. In contrast, β_1^{LSOE} was estimated to be 0.110 and insignificant in 1995, but it rose sharply to 1.059 in 2002 and became significant at the 5% level. The Chow test rejects the null hypothesis that β_1^{LSOE} has not changed between 1995 and 2002, with a p-value of 0.035. This finding is consistent with the view that workers in LSOEs had stronger precautionary saving motives than those in CSOEs because they faced significantly higher unemployment risks.

V. Sample selection issues

Since the CHIP surveys do not keep track of individual households over time, the post-treatment group observed in 2002 includes only those workers who survived the SOE reform and who chose not to quit from their SOE jobs. The difference of worker characteristics

[20]LSOE also includes urban collective enterprises.

[21]In particular, about 3.4% of workers in SOEs owned by the central or provincial governments reported prior layoff experience, whereas to 7.4% of local SOE workers and 16.4% of urban collective SOE workers had prior layoff experience.

before and after the reform can potentially cause biases in the estimation of precautionary saving. There is evidence that workers with lower educational attainment or lower skills were more likely to be laid off (Appleton et al., 2002). In addition, during the period from 1995 and 2002, some workers who were not laid off chose to quit from SOE firms for private-sector jobs. In this section, we focus on the implications of two types of sample selection issues – a survival bias and voluntary quits.

V.1. **The survival bias.** We first consider the survival bias. The observed SOE workers in the 2002 sample survived the massive layoffs. To the extent that those surviving workers have different characteristics than those in the 1995 sample (e.g., they have higher skills or higher incomes) and that such differences may affect saving behaviors, our estimates of precautionary saving may be subject to a survival bias.

To correct this bias, we adjust the 1995 sample to include only those workers who are likely to survive the massive layoffs. We estimate the layoff probability for an SOE worker in 1995 using information from the 2002 sample, expanded to include those who had experienced layoffs in 2002 or before. We use the expanded 2002 sample to estimate the Probit model

$$\Pr(\text{layoff}_i = 1 \mid Z_i) = \Phi(Z_i \delta), \tag{4}$$

where Z_i is a vector that summarizes individual i's characteristics, including age, gender, education levels, occupation, and industry and province dummies. The dependent variable in the Probit model is the dummy variable layoff_i, which takes a value of 1 if an individual had layoff experience, and zero otherwise.[22]

We fit the estimated Probit model to the 1995 sample to infer the probability of layoffs for SOE workers in that year. According to Giles et al. (2005), Chinese urban unemployment rate reached 11.1% in 2002. This implies that for SOE workers in 1995, at least 10% of them would not survive until 2002. Thus, we drop the SOE workers in the 1995 sample who, according to the estimated layoff probability, are the top 10% of the sample that are most likely to be laid off. In other words, we keep the 90% of SOE workers in 1995 sample who are most likely to survive the massive layoffs. We argue that the subsample of potential survivors of the layoffs in 1995 share similar characteristics with the 2002 sample (who are ex post survivors of the layoffs), except that they face different levels of unemployment risks.

[22]Ideally, the expanded 2002 sample should include only those workers who were laid off from SOEs, not from GOV or the private sector. Unfortunately, the 2002 CHIP survey does not provide information about the sector in which a jobless household had worked. As an approximation, we include all jobless households in our expanded 2002 sample. This may lead to a potential bias in estimating the layoff probability. But since most of the layoffs occurred in the SOE sector between 1995 and 2002, such a bias is unlikely to change our main results.

Table 8 (Panel A) shows the estimation results when the survival bias is corrected. Column (1) keeps all workers in 1995 sample and therefore simply replicates the results in column (i) of Table 4. Column (2) shows that if we drop those SOE workers who had the top 10% layoff probability in future years, the coefficient β_1 of the SOE dummy remains insignificant, although it increases slightly from 0.028 to 0.047. To further examine the importance of the survival bias, we drop the SOE workers with the top 20% and top 30% of layoff probabilities and reestimate the model. The results are reported in columns (3) and (4) in Table 8, respectively. The estimated value of β_1 increases further to 0.102 and 0.117 respectively, but it still remains insignificant. Accordingly, the difference in β_1 between 2002 and 1995 becomes somewhat smaller than that obtained in the benchmark model ($0.545 = 0.662 - 0.117$ vs. $0.634 = 0.662 - 0.028$), but the implied magnitude of precautionary saving remains significant both statistically and economically.

Thus, correcting the survival bias modestly reduces the quantitative magnitude of precautionary savings, but precautionary saving caused by the large-scale SOE reform remains evident.

V.2. **Voluntary quits.** Following the SOE reform in the late 1990s, some workers voluntarily quit from SOE firms for private-sector jobs.Our 2002 sample does not include those workers and is thus not completely comparable with the 1995 sample before the reform. If the workers who remained in the SOE firms in 2002 are more risk averse than the workers who quit, then the estimated precautionary saving would likely be biased upward.

To control for the effects of voluntary quits, we estimate the probability of quits using the 2002 sample, expanded to include those households who had quit from SOEs. The sample of quits includes those households who were not working in the SOEs in 2002 but who had prior experience of working in the SOEs and who had never experienced layoffs. We estimate the Probit model

$$\Pr(\text{quit}_i = 1 \mid Z_i) = \Phi(Z_i\delta), \tag{5}$$

where, similar to the Probit model for layoffs in equation (4), the term Z_i is a vector of individual characteristics and the dependent variable is a dummy variable that takes a value of 1 if the individual has quit experience and 0 otherwise.

We fit the estimated Probit model of quits to the 1995 sample to infer the probability of quits for SOE households observed in that year. We restrict the 1995 sample to non-quitting workers to make the samples comparable between 1995 and 2002. Since the quit rate in the 2002 sample is 1.88%, we exclude the top 2% of the most likely quitting SOE workers from the 1995 sample and focus on the remaining 98% of likely non-quitting workers. Table 8 (Panel B) shows that, for the 98% non-quitting workers in 1995, the estimated value of β_1 is slightly larger than the full sample (0.062 vs. 0.028), but it remains statistically insignificant.

When we further restrict the 1995 sample by excluding the top 4% or top 6% of most likely quitting workers, we continue to obtain small and insignificant estimates of β_1. In contrast, in 2002, β_1 is estimated to be large and significant. Thus, precautionary saving continues to be important when voluntary quits are taken into account.

VI. Robustness

In this section, we examine the sensitivity of our estimation of precautionary saving. In particular, we consider the implications of pension benefits, spouse occupations, homeownership status, and some alternative sampling and measurement methods. Although these factors changes the particular estimates of the contributions of precautionary savings to SOE household wealth accumulation, we show that the quantitative importance of precautionary saving that we have obtained in the benchmark model that controls for both self-selection biases the PIH effects remains robust.

VI.1. **Pension effects.** Pension benefits can also affect saving behaviors through a channel similar to that of income expectations: they both reflect PIH effects. Unfortunately, the CHIP surveys do not provide direct information on pension. However, we do have data (from the China Labor Statistical Yearbook) for the pension replacement ratios at the aggregate level for SOE workers and GOV employees in both 1995 and 2002. We use these aggregate observations to obtain a "back-of-the-envelope" estimate of the extent to which differences in pension benefits across the two sectors and the changes of those benefits over time would affect our estimates of precautionary saving.

According to the 2003 China Labor Statistical Yearbook, the average pension replacement ratio—defined as the ratio of pension income to annual salaries—was about 99.5% for GOV employees in 1995, and it declined slightly to 94.4% in 2002. The average pension replacement ratio for SOE workers was much lower at 74.2% in 1995, and it further declined to 64.3% in 2002. Thus, the pension replacement ratio for SOE workers was about 25.4% lower than GOV employees in 1995 ($(0.995 - 0.742)/0.995 = 0.254$), and this gap widened to 31.9% in 2002 ($(0.944 - 0.643)/0.944 = 0.319$).

To adjust for the pension effects in our estimation of precautionary saving, we assume that the positive estimated value of $\beta_1 = 0.028$ in 1995 reflects mainly the lower pension benefits for SOE workers than for GOV workers. This assumption seems reasonable because the two groups of workers had similar job security in 1995 and, in our regression, we have controlled for all other demographic characteristics for both groups except for differences in individual pension benefits (that are not directly observable). By 2002, the pension gap has widened by a factor of 1.26 ($0.319/0.254 = 1.26$). Thus, to a first-order approximation, the pension effects for the 2002 sample should be 1.26 times that for the 1995 sample (i.e., $1.26\beta_1^{1995} = 0.0353$).

With these pension effects taken into account, the estimated precautionary saving—which corresponds to the pension-adjusted differences between the estimated values of β_1 in 2002 and 1995, becomes slightly smaller than that in the benchmark estimation ($0.627 = 0.662 - 0.0353$ vs. $0.634 = 0.662 - 0.028$). Accordingly, the contribution of precautionary savings to the observed increases in financial wealth is also slightly less than that in the benchmark estimation (35.5% vs. 35.7%), as shown in Panel A in Table 9.

VI.2. **Spouse effects.** The precautionary saving that we have estimated are based on the regression model in equation (1), through a dummy variable indicating whether the head of the household works in an SOE. However, a large fraction of households in our sample are dual-income families. In particular, about 70% of SOE households and 76% of GOV households have dual income earners. The spouse working status affects the overall income uncertainty for a family in the post-reform period. For example, it is plausible that a household whose head works at the SOE sector but whose spouse works at the government sector is not as exposed to the reform as a household in which both the head and the spouse work for an SOE.

To examine the implications of the working status of the spouse, we run regressions which include two additional control variables: a dummy variable SOE^{sp} that indicates whether or not the spouse works for an SOE, and an interaction of SOE^{sp} with the SOE dummy. The interaction term takes the value of one if the head and the spouse both work for an SOE, and zero otherwise. All else equal, we should expect a family with the spouse working for an SOE to have more precautionary savings than an average household after the SOE reform. We should also expect a family with both the head and the spouse working for an SOE save even more for precautionary reasons after the reform. In other words, the coefficients for both SOE^{sp} and its interaction with the SOE dummy in the 2002 sample should be positive. This turns out to be true. In particular, the estimated coefficient for SOE^{sp} is 0.007, with a standard error of 0.216. The coefficient for $SOE^{sp}\times$ SOE is 0.274, with a standard error of 0.328.[23] Both coefficients are positive as expected, but statistically insignificant.

With these controls for the spouse working status, the coefficient β_1 for the SOE dummy captures the marginal impact for the household head to work in an SOE when the unemployment risks rose relative to GOV workers. The point estimate for β_1 is 0.538, which is smaller than the benchmark value of 0.662, but it remains statistically significant at the 10% level. As shown in Panel B of Table 9, the implied contribution of precautionary savings to the observed increase in total savings is about 30.6% for the family which only the head works for an SOE.

[23]To conserve space, we report the detailed estimation results with the two additional controls for spouse effects in a Supplemental Appendix available upon request.

VI.3. **Housing effects.** To further control for the effects of potential savings by SOE workers for home purchases rather than for precaution against future unemployment risks, we add an interaction term between the SOE dummy and non-homeownership dummy in our regression (1). In this specification, the coefficient β_1 of the SOE dummy should be interpreted as the difference in wealth accumulation between SOE and GOV workers given that they all are homeowners and have the same other demographic characteristics. The estimated coefficients for this interaction term and for the non-homeownership dummy are statistically insignificant in both 1995 and 2002. This finding is consistent with our argument that, during those periods, the Chinese housing market was not fully developed and thus the saving motives for home purchases were weak. With the interaction term added, the estimated coefficients for the SOE dummy are 0.090 (s.e. = 0.117) in 1995 and 0.572 (s.e. = 0.300) in 2002 (see Panel C of Table 9). Our results suggest that, among homeowners, the precautionary saving motive of SOE workers relative to GOV employees becomes slightly weaker than that in the benchmark specification ($0.482 = 0.572 - 0.090$ vs. $0.634 = 0.662 - 0.028$). But it still accounts for 29.1% (s.e. = 0.187) of financial wealth accumulation for those households.

VI.4. **Excluding zero wealth observations.** The empirical results that we have summarized above are obtained based on the sample that includes zero-wealth observations. To examine whether these results are driven by zero-wealth observations, we exclude those observations from the sample and re-estimate the benchmark model in Equation (1) using the standard IV (2SLS) approach (instead of the IV-Tobit approach used for estimating the benchmark model). With the zero-wealth observations excluded, the sample size reduces to 3221 and 1807 observations for 1995 and 2002, respectively. Table 9 summarizes the estimation results (in Panel D).

The estimated value of β_1 is 0.059 (insignificant) in 1995 and 0.514 (significant at the 5% level) in 2002. The difference between the estimated values of β_1 ($0.514 - 0.059 = 0.455$) is modestly smaller than that we have obtained in benchmark estimation. Thus, excluding zero-wealth observations from the sample modestly reduces the estimated magnitude of precautionary saving. Nonetheless, the estimated magnitude of precautionary saving remains significant, and precautionary savings still account for about 26.2% (s.e. = 0.131) of total wealth accumulations for SOE workers following the reform.

VI.5. **Conventional risk measure.** In our benchmark model, we have used CV of log income (i.e., the ratio of the standard deviation to the mean) in the current year and the recent past to measure idiosyncratic income risks. To examine the sensitivity of our results to alternative measures of idiosyncratic risks, we replace CV by the conventional risk measure used by Carroll and Samwick (1998), which is the logarithm of the variance of log income for 16 different educational and occupational groups.[24] The estimation results are shown in panel E of Table 9.

The estimated value of β_1 increases from 0.013 (insignificant) in 1995 to 0.800 (significant at the 5% level) in 2002. These estimates imply that precautionary savings contributed about 45.4% (s.e. = 0.201) of the increases in financial wealth for SOE workers from 1995 to 2002.

VI.6. **Alternative wealth measures.** Some alternative measures of wealth such as very liquid assets and non-housing non-business wealth (NHNBW) are also commonly used in the literature (Carroll and Samwick, 1998). We now examine the sensitivity of our empirical results to these alternative measures of wealth (see Table 1 for the construction of these variables in CHIP).[25]

Panel F of Table 9 presents the results using very liquid assets as wealth measure to construct the dependent variable in equation (1). The estimated value of β_1 increases from -0.00006 (insignificant) in 1995 to 0.446 (significant at the 10% level) in 2002. These estimates imply that precautionary savings contributed about 38.5% (s.e. = 0.210) to the observed increases in wealth accumulation following the reform.

Panel G of Table 9 shows that, when we use the non-housing non-business wealth to replace financial wealth, the estimated value of β_1 is 0.119 and insignificant in 1995 and it increases substantially to 0.698 (significant at the 5% level) in 2002, which are similar to the benchmark estimates. Precautionary savings contribute about 38.2% (s.e. = 0.204) of the increases in wealth accumulation for SOE workers from 1995 to 2002, also similar to that obtained in the benchmark estimation.

VII. Conclusion

Using China's large-scale reform of the state-owned enterprises (SOEs) in the late 1990s as a natural experiment, we identify and quantify the importance of precautionary saving in a rapidly growing transition economy. With self-selection in occupational choices corrected and

[24]The 16 groups correspond to the cross products of the 4 occupation categories and 4 education categories described in Section III.3 and Table 2.

[25]Another widely used measure of wealth is total net worth, which is NHNBW plus estimated market value of owner-occupied housing and fixed assets of farms and business. We have estimated the benchmark model using total net worth instead of financial wealth. We find that the estimate of β_1 continues to be small and insignificant for the 1995 sample. For the 2002 sample, β_1 becomes much larger (1.194, with a standard error of 0.866). Although statistically insignificant, the estimated value of β_1 in the 2002 sample is economically large, suggesting that precautionary saving is economically important, as we find in the benchmark estimation. The statistical insignificance likely reflects difficulties in measuring housing wealth since China had a highly under-developed housing market during our sample periods.

with expected income effects controlled for, we obtain significant evidence of precautionary saving stemming from sudden increases in unemployment risk for SOE workers relative to that for government employees. Our estimation suggests that precautionary saving can account for about 35 percent of the actual increase in wealth accumulation by urban SOE households in China for the period from 1995 to 2002. Thus, precautionary saving associated with large structural changes in the Chinese economy is quantitatively important.

Appendix A. A Case Study: Massive Lay-off in Fushun, Liaoning

Smyth et al. (2001) demonstrate a case study of massive lay-off happened in Fushun, Liaoning. Fushun is a medium sized city located 45 kilometers northeast of Shenyang, the capital city of Liaoning. It was well known as a state-owned heavy industrial base in the "rust belt" of China. In 2000, nearly 91% of workers in Fushun were employed by SOEs. And SOEs produced 88.5% of gross industrial output.

The wave of layoffs (*xia gang*) hit Fushun very severely. In 2000, laid-off workers from SOEs accounted for about 42% of total workers in SOEs in Fushun, which was the highest in Liaoning. The industries that had the largest number of layoffs were coal, textiles, light industry, electronics, machinery, and chemicals. For example, of the 71000 workers in SOEs in the coal sector in Fushun, 35000 or 49.7% of workers were classified as *xia gang*.

What differentiates *xia gang* from official unemployment (known as "registered unemployment") is that *xia gang* workers still retain ties with their former SOEs employers. In practice, there are four different types of layoffs from an SOE firm: 1) *fang jia*: a worker is put on a temporary leave; 2) *xia gang*: a worker is put on a long-term leave; 3) *tui yang*: a worker takes voluntary early retirement. 4) *mai duan*: a firm pays a lump-sum amount (usually not exceeding three years of salary) to buy out or terminate the labor contract with a worker.[26]

Allowances were paid to laid-off workers by their former employer, the local government, and the central government, each was supposed to contribute one-third. However, many SOE firms had financial difficulties in making the payments to the laid-off workers. For example, of the 35,000 laid-off workers from state-owned coal mines in Fushun, 33,000 did not receive basic living allowances from their former employers.

In Fushun, the main avenue for laid-off workers to find new jobs was through re-employment centers sponsored by the local government. The re-employment centers offered various training classes. However, there are several problems that hindered the effectiveness of government-sponsored re-employment institutions. A large proportion of laid-off workers were middle-aged, less educated, or female. It is very hard for them to find a job given the

[26]In our sample, we include all four types of layoffs.

discrimination against age and gender in Chinese labor market. And they were reluctant to take jobs in non-state-owned sector because they were concerned that seeking employment in non-state sectors would cut their ties with their former SOE employers. Among the laid-off workers who have registered at re-employment centers in Fushun, 50% were middle-aged. Among these middle-aged workers, only half of them were successfully re-employed.[27]

[27]This is consistent with the official number of national reemployment rate, see Lee (2000). However, a survey of 54,000 workers carried out by the Chinese Federation of Labor Unions in 1997 reports that only about 18% of the laid-off workers found new employment. See Lee (2000) for details.

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Table 1. Definition of variables

Variable	**Description**
Financial wealth (W)	Balances in checking accounts, saving accounts, CDs, stocks, bonds, loans to others, and family business assets (Item 401 in CHIP)
Very liquid assets (VLA)	Financial wealth minus business investment, housing fund, etc.
Nonhousing, nonbusiness wealth (NHNBW)	Financial wealth plus estimated market value of durable cons. goods and other assets, minus total debt
Annual income	Annual income of household head and revenues from business, farming, fishing, gardening, livestock, non-retirement wages, retirement income, subsidies, and other income
Income risk	Coefficient of variation (CV) of log annual income of past 5 or 6 years
SOE	Dummy variable for employers of HH, 1 for State Owned Enterprises (SOE), 0 for Government & Institutions
Permanent income (P)	Constructed based on earnings by household heads in the current year and the recent past (See text)
W/P	Wealth / permanent income ratio
Age	Age of household head
Male	Dummy variable for the gender of HH, 1 for male, 0 otherwise
Married	Dummy variable for the marital status of HH, 1 for married, 0 otherwise
Education	Four dummy variables for college, senior middle school, junior middle school, and elementary school or below (see text)
Occupation	Four dummy variables for professional, director or manager, skilled or office workers, unskilled or service workers or the others (see text)
Health care	Three dummy variables for public health care, public health insurance and own payment (see text)
No house owned	Dummy variable for housing ownership, 1 for no house owned, 0 otherwise
Age of children	Mean age of children in household
Num. of boys	Number of boys in household
Num. of students	Number of children at school in household

Table 2. Summary statistics of the full sample

Variable	1995			2002		
	Obs.	**Mean/%**	**SD**	**Obs.**	**Mean/%**	**SD**
Financial wealth (W)	4390	10042	10165	3027	32826	32140
Annual permanent income (P)	4390	7520	3131	3027	12843	6018
CV×100	4390	2.61	2.07	3027	2.9	7.67
Age	4390	40.91	7.37	3027	42.61	6.88
Age of children (mean)	4390	11.65	6.94	3027	12.5	7.58
Num. of boys	4390	0.57	0.58	3027	0.47	0.53
Num. of students	4390	0.65	0.48	3027	0.69	0.54
Household size	4390	3.18	0.68	3027	3.03	0.61
Male	4390	63.4%		3027	68.8%	
Married	4390	97.6%		3027	96.7%	
Education						
College	4390	24.6%		3027	37.2%	
Senior middle school	4390	39.5%		3027	38.8%	
Junior middle school	4390	30.8%		3027	21.5%	
≤Elemen. School	4390	5.1%		3027	2.4%	
Occupation						
Professional	4390	24.3%		3027	24.7%	
Director or manager	4390	14.3%		3027	15.3%	
Skilled worker	4390	44.7%		3027	44%	
Unskilled/other worker	4390	16.7%		3027	15.9%	
Health Care						
Own payment	4390	9.9%		3027	23.1%	
Public health care	4390	71.3%		3027	35%	
Public health insurance	4390	8.8%		3027	41.9%	
Own house	4390	42%		3027	80.4%	
SOE	4390	67.8%		3027	56.2%	
Job assigned by Gov.	4375	82.9%		3018	71.9%	

Notes: Monetary values are in constant RMB Yuan, base year = 2002.

Table 3. Comparison between employees in GOV vs. SOEs

		1995			2002		
	Variable	**Obs.**	**Mean**	**SD**	**Obs.**	**Mean**	**SD**
GOV	Financial wealth (W)	1413	10457	10209	1325	34677	32351
	Annual permanent income (P)	1413	7905	3063	1325	13979	5853
	W/P	1413	1.376	1.386	1325	2.559	2.36
	Non homeowners	1413	0.546	0.498	1325	0.165	0.372
	Job assigned by Gov.	1408	0.893	0.309	1319	0.757	0.429
	Expected income to decline	N.A	N.A	N.A	1321	0.114	0.318
SOE	Financial wealth (W)	2977	9845	10140	1702	31386	31910
	Annual permanent income (P)	2977	7337	3146	1702	11958	5998
	W/P	2977	1.382	1.448	1702	2.703	2.906
	Non homeowners	2977	0.597	.491	1702	0.220	.414
	Job assigned by Gov.	2967	0.798	0.401	1699	0.689	0.463
	Expected income to decline	N.A	N.A	N.A	1699	0.238	0.426

Notes: Data are taken from CHIP surveys. Monetary values of financial wealth and permanent income are in constant Chinese Yuan units, with 2002 as the base year.

Table 4. IV-Tobit regressions, 1995 and 2002 samples

Dep. variable:	**1995**		**2002**	
W/P	**(i)**	**(ii)**	**(iii)**	**(iv)**
SOE	0.028	-0.020	0.748**	0.387*
	(0.096)	(0.098)	(0.293)	(0.219)
CV×100	0.111***	0.135***	0.105**	0.089***
	(0.039)	(0.037)	(0.049)	(0.028)
log(permanent income)	0.738	1.186	4.239***	3.511***
	(0.998)	(0.885)	(1.417)	(0.987)
Age	0.021	-0.018	0.044	0.242*
	(0.051)	(0.050)	(0.148)	(0.125)
Age squared(*100)	-0.031	0.018	-0.057	-0.277*
	(0.059)	(0.059)	(0.172)	(0.147)
Male	-0.360***	-0.459***	-1.137***	-1.168***
	(0.099)	(0.092)	(0.196)	(0.148)
Professional	0.131	0.006	-1.076**	-0.545
	(0.173)	(0.158)	(0.538)	(0.360)
Director	0.320*	0.158	-1.073*	-0.730*
	(0.173)	(0.168)	(0.559)	(0.386)
Skilled worker	0.068	-0.026	-0.889**	-0.577**
	(0.109)	(0.101)	(0.420)	(0.278)
Public health care	0.049	0.043	-1.182**	-0.961***
	(0.187)	(0.164)	(0.483)	(0.360)
Public med insurance	0.031	0.105	-0.888**	-0.741**
	(0.164)	(0.149)	(0.421)	(0.315)
Married	0.523***	0.493***	0.625	0.408
	(0.191)	(0.160)	(0.419)	(0.361)
Age of children (mean)	0.008	0.005	0.005	0.000
	(0.006)	(0.006)	(0.013)	(0.010)
Num. of boys	0.045	0.024	-0.255*	-0.200*
	(0.048)	(0.045)	(0.144)	(0.118)
Num. of children at school	-0.084	-0.034	-0.310*	-0.364***
	(0.065)	(0.063)	(0.173)	(0.140)
Household size	-0.038	-0.011	0.261	0.356***
	(0.050)	(0.048)	(0.169)	(0.135)
No house owned	0.006	0.065	-0.045	-0.040
	(0.068)	(0.064)	(0.207)	(0.167)
Industry & Province dummies	yes	yes	yes	yes
Log-Likelihood	-7168.63	-8876.75	-5815.62	-8242.60
p-value of Chow test for SOE			0.020	0.089
Sample size	3627	4390	2170	3027

Notes: Standard errors are in parentheses and are corrected for heteroskedasticity. ***, **, and * indicate p-values of less than 1%, 5%, and 10%, respectively. Columns (i) and (iii) show the estimation results using the sample with government-assigned jobs. Columns (ii) and (iv) show the results in the full sample.

Table 5. Regressions with 2002 sample: controlling for PIH effects

Dep. variable:	**expected future income**	
W/P	**decline**	**non-decline**
SOE	1.217**	0.662**
	(0.523)	(0.304)
CV×100	0.119**	0.102**
	(0.060)	(0.049)
log(permanent income)	5.243**	4.387***
	(2.162)	(1.563)
Age	0.097	0.091
	(0.170)	(0.164)
Age squared(*100)	-0.142	-0.114
	(0.198)	(0.191)
Male	-0.950***	-1.111***
	(0.221)	(0.217)
Professional	-1.528*	-1.155**
	(0.860)	(0.559)
Director	-1.436	-1.105*
	(0.898)	(0.569)
Skilled worker	-0.869	-1.013**
	(0.673)	(0.445)
Public health care	-1.185*	-1.316**
	(0.628)	(0.545)
Public med insurance	-0.591	-1.015**
	(0.460)	(0.486)
Married	0.411	0.563
	(0.520)	(0.431)
Age of children (mean)	0.011	-0.001
	(0.015)	(0.013)
Num. of boys	0.048	-0.259
	(0.189)	(0.158)
Num. of children at school	-0.246	-0.280
	(0.229)	(0.190)
Household size	0.158	0.306*
	(0.182)	(0.179)
No house owned	-0.447*	0.070
	(0.260)	(0.225)
Industry & Province dummies	yes	yes
Log-Likelihood	-3184.06	-4939.36
p-value of Chow test for SOE	0.025	0.047
Sample size	1284	1876

Notes: Results are from the IV-Tobit regressions. Standard errors (in parentheses) are corrected for heteroskedasticity. In the 2002 sample, 2164 out of 2170 households whose jobs were assigned by the government reported their expectations for future income, including 1168 SOE employees and 996 GOV employees. Among the SOE employees, there are 288 who expected their future income to decline and 880 who did not. The sample of the first column consists of 288 SOE employees who expected future income to decline and all 996 GOV employees as the control group (N=1284=288+996). The sample of the second column consists of the rest SOE employees and all GOV employees(N=1876=880+996).

TABLE 6. Precautionary saving motives of young vs. old households

	1995		**2002**	
Dep variable: W/P	**Age 25-44**	**Age 45-55**	**Age 25-44**	**Age 45-55**
SOE	-0.024	0.162	1.081**	0.222
	(0.146)	(0.156)	(0.453)	(0.483)
CV×100	0.122**	0.166**	0.215**	0.079
	(0.054)	(0.066)	(0.085)	(0.060)
log(permanent income)	1.049	2.117	5.341***	4.155
	(1.499)	(1.533)	(1.954)	(3.292)
Controls	yes	yes	yes	yes
Sample size	2349	1278	997	879

Notes: Results are from the IV-Tobit regressions. Standard errors are in parentheses and are corrected for heteroskedasticity.

Table 7. Precautionary saving by workers in central vs. local SOEs

Dep. variable: W/P	**1995**	**2002**
CSOE	-0.108	0.225
	(0.127)	(0.279)
LSOE	0.110	1.059**
	(0.151)	(0.424)
CV×100	0.115***	0.107**
	(0.043)	(0.050)
log(permanent income)	0.862	4.671***
	(1.137)	(1.658)
Controls	yes	yes
p-value of Chow test for CSOE		0.278
p-value of Chow test for LSOE		0.035
Sample size	3627	1876

Notes: Results are from the IV-Tobit regressions. Standard errors are in parentheses and are corrected for heteroskedasticity. "CSOE" denotes SOEs owned by the central and provincial governments and "LSOE" denotes those owned by local governments.

Table 8. Correcting sample selection biases

A. Controlling for survival biases				
Dep. variable	1995 survival threshold			
W/P	100%	90%	80%	70%
SOE	0.028	0.047	0.102	0.117
	(0.096)	(0.101)	(0.112)	(0.114)
CV×100	0.111***	0.124**	0.165***	0.175***
	(0.039)	(0.050)	(0.053)	(0.048)
log(permanent income)	0.738	1.145	2.123	2.252**
	(0.998)	(1.314)	(1.359)	(1.131)
Controls	yes	yes	yes	yes
Sample size	3627	3415	3198	2971

B. Controlling for voluntary quits				
Dep. variable	1995 non-quit threshold			
W/P	100%	98%	96%	94%
SOE	0.028	0.062	0.017	0.035
	(0.096)	(0.106)	(0.123)	(0.132)
CV×100	0.111***	0.114***	0.115***	0.114**
	(0.039)	(0.037)	(0.043)	(0.045)
log(permanent income)	0.738	0.850	0.836	0.771
	(0.998)	(0.970)	(1.122)	(1.213)
Controls	yes	yes	yes	yes
Sample size	3627	3582	3532	3435

Notes: Results are from the IV-Tobit regressions. Standard errors are in parentheses and are corrected for heteroskedasticity.

Table 9. Robustness

Cases	**1995**	**2002**	**Contributions**
Benchmark	0.028	0.662**	35.7%
	(0.096)	(0.304)	(0.163)
A. Pension effects	0.028	0.655**	35.5%
	(0.096)	(0.305)	(0.165)
B. Spouse effects	0.062	0.538*	30.6%
	(0.121)	(0.317)	(0.186)
C. Housing effects	0.090	0.572*	29.1%
	(0.117)	(0.300)	(0.187)
D. Eliminating zero wealth	0.059	0.514**	26.2%
	(0.088)	(0.269)	(0.131)
E. Conventional risk measure	0.013	0.800**	45.4%
	(0.095)	(0.360)	(0.201)
F. Very liquid asset	-0.00006	0.446*	38.5%
	(0.094)	(0.246)	(0.210)
G. Non-housing Non-business wealth	0.119	0.698**	38.2%
	(0.131)	(0.356)	(0.204)

Notes: The first panel displays the estimation results from the benchmark model which controls for self-selection biases and the PIH effects. Panels A-G report the estimation results under alternative specifications of the model or alternative measures of wealth and idiosyncratic risks. The control variables are the same as those in Table 4. We use the standard IV (2SLS) regression for the case with zero-wealth observations excluded (Panel D) and the IV-Tobit regression for the other cases. The last column (under "Contributions") shows the contributions of precautionary saving to total wealth accumulation in each case. Standard errors (in the parentheses) are corrected for heteroskedasticity.