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

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Abstract. We estimate the importance 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 the relative unemployment risks for SOE workers after the reform provide a clean identification of income uncertainty. We exploit the evolution of China's labor market reform and use information about when and how a worker obtained his job for identifying potential self-selection biases. We estimate that precautionary savings account for about 40 percent of SOE household wealth accumulation between 1995 and 2002. We also find evidence that demographic groups more vulnerable to unemployment risks accumulated more precautionary wealth in response to the reform.

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 cross-sectional variances of income as

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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).

Partly reflecting the difficulties in measuring income uncertainty and correcting self-selection biases, 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. 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 near-free health care, education, housing, and retirement benefits. In this sense, workers in both sectors held an "iron rice bowl" before the reform. Following the reform, 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. 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 urban households in two sectors (SOE and GOV) and two CHIP surveys (1995 and 2002). 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

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

relative saving behavior of workers across the two sectors capture the magnitude of precautionary savings caused by the SOE reform.

To mitigate the self-selection bias associated with occupational choices, we exploit the evolution of China's labor market policies and consider two identification strategies for self-selection. The first identification strategy is inspired by Fuchs-Schündeln and Schündeln (2005), who estimate the importance of self-selection biases using the event of German reunification. In particular, we follow their approach by restricting our sample to those households whose jobs were assigned by the government. 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. Since the final job outcome was determined by the local governments rather than individual workers, self-selection was unlikely. When we identify self-selection based on government job assignment, we obtain a significantly larger estimate of precautionary savings than that from the full sample without correcting for self-selection. This finding using Chinese data confirms that obtained by Fuchs-Schündeln and Schündeln (2005) from German data.

In practice, however, 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.

To further examine the importance of self-selection biases, we design a new identification strategy by using information on the timing of China's labor market reform. In 1986, the Chinese government began to implement a labor contract system for new hires in the state sector. Under the labor contract system, state-sector employers gained more flexibility in hiring and firing workers. However, the labor contract system was restricted to new hires only; existing workers who were hired prior to 1986 still had life-time job security and could not be easily fired. A similar set of labor contracts were applied to new hires in GOV. Thus, prior to the 1986 reform, jobs in SOE and GOV were essentially the same, providing no bases for job seekers' self selection.

This identification strategy is different from that using government job assignment: It is based on *when* a worker entered into the job market (e.g., before or after 1986), not *how* a worker obtained a job (e.g., through government assignment or not). Therefore, the sample with the pre-1986 worker cohort is different from that with government assigned jobs. Despite the difference between the two identification strategies for self selection, we obtain surprisingly similar estimates of precautionary savings, both implying a quantitatively important magnitude of self-selection biases.

By identifying changes in income uncertainty caused by the SOE reform and correcting the self-selection bias in occupational choices, we obtain estimates of precautionary savings that are significant both statistically and economically. We estimate that precautionary wealth accounts for about 40 percent of the total financial wealth accumulation for urban SOE workers during the period from 1995 to 2002. We also estimate that self selection results in a downward bias of the estimated precautionary savings of about 30%. Thus, both precautionary wealth and self-selection biases are quantitatively important for Chinese households.

Our identification and estimation rely on institutional features in China during a period with large structural transformations. In this sense, our approach is novel and contributes to the literature. The magnitudes of precautionary savings and self-selection biases that we have obtained from the Chinese data turn out to be very similar to what Fuchs-Schündeln and Schündeln (2005) found from the German data. Thus, our study lends further empirical support to the importance of precautionary savings and self-selection biases.

A second contribution of our paper is that, by exploiting the household-level details of the CHIP data, we find substantial heterogeneity of precautionary savings across different demographic groups. First, consistent with the life-cycle consumption theory, we find stronger evidence of precautionary savings for younger households (25-45 years) than for older households, confirming the finding of Gourinchas and Parker (2002) obtained from U.S. data. Second, we find that workers in local SOEs have stronger precautionary saving motives than workers in SOEs owned by the central government or provincial governments, consistent with the fact that layoffs were more likely in small and local SOEs (Hsieh and Song, 2015). Third, we find that the demographic groups more exposed to unemployment risks following the reforms, such as female, low-skilled, or less educated SOE workers [see Appleton et al. (2002)] accumulated more precautionary wealth in response to the SOE reform. A consistent message emerges from these exercises: the more vulnerable groups to the SOE reform tend to have a stronger precautionary saving motive.

A third contribution of our paper is that we examine explicitly the extent to which changes in income expectations could affect the estimation of PS. As we illustrate in a simple theoretical model in Section III.2 below, an increase in future unemployment risks not only raises savings through precautionary motives, but also through a permanent income hypothesis (PIH) channel since it reduces expected future income. In Section V.4, we control for the PIH effects in our empirical model by using information on both short-term income expectations and pension participation reported in the 2002 CHIP survey. We find that short-term income expectations do not seem to affect precautionary savings significantly, but pension participation is relatively more important.

The evidence of precautionary saving is robust when we control for potential sample-composition biases, a few alternative model specifications, and alternative variable measurements.

Our study also adds 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. 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. (2015) 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 China's Labor Market and SOE Reforms

Since we exploit some institutional features of China's labor market to help identify changes in income uncertainty and self-selection biases for estimating precautionary saving, it is useful to provide some background information, with a brief description of the history of reforms in China's labor market and the SOE sector.

II.1. **Labor market reforms.** 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. Job assignments were made typically through educational institutions (high schools or colleges) or local communal offices where potential workers registered their residency. The Ministry of Labour and Personnel assigned employment quotas to local governments, which then allocated the quotas to each school and local communal offices. Jobs were allocated to individuals who "need jobs," and individuals were not allowed to search for a job on their own. State-sector firms and government departments were not allowed to recruit workers either. Instead, each working unit was assigned an annual employment quota. Final decisions on quota assignment were made by local Bureaus of Labor and Personnel. Once assigned to a job, a worker could not quit or switch jobs and a firm could not dismiss workers unless a crime was convicted (Meng, 2000). For those workers who obtained jobs through government assignment, they could not choose their occupations freely, and thus self-selection was unlikely.

To support the goal of industrialization, workers under the central-planning regime were paid subsistence wages and, in exchange, they were guaranteed life-time employment along with near-free housing, education, health care, and retirement benefits (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 reforms, setting off China's transition to a free-market economy. In the early 1980s, some experimental labor market reforms to the state sector started, in order to relax the rigid life-time employment rules under the central planning regime. In 1986, a systematic labor contract system was introduced to the state sector.[2] Under the rules of the labor contract system, state-sector employers were allowed to use examinations and conduct interviews in recruiting new workers. Labor contracts could be terminated if a worker was deemed incompetent during probation, violated work rules, or committed crimes while employed. A similar set of rules were applied to government jobs.

The new labor contract system was implemented gradually, and it was applied only for new hires. The share of workers in the state sector covered by the labor contact system was 3.7% in 1985, which gradually grew to 13% in 1990 (Meng, 2000). The labor contract reform fundamentally changed the mechanism for labor allocations in China. As labor contracts were more widely implemented over time, the share of government job assignment gradually declined.

However, workers hired prior to the introduction of the labor contract system in 1986 still enjoyed the same life-time job guarantees and the associated benefits, regardless of the sectors where they work (SOE or government). Thus, for the pre-1986 worker cohort, jobs in SOEs and in government were essentially the same, providing no bases for self selection into occupations.

We argue that the nature of government assignment of jobs and the labor market reforms in 1986 both provide useful information for identifying self-selection in estimating precautionary saving. Indeed, they representative two distinct identification strategies, with one based on *when* a worker found a job (e.g., before or after 1986), and the other based on *how* a worker obtained a job (e.g., government assigned or not). As we show below, despite the difference between the two identification approaches for self selection, the estimated manitude of precautionary savings is surprisingly similar.

II.2. **SOE reforms.** The labor market reforms implemented after 1986 also relaxed the tight controls over rural-to-urban migration flows. The influx of rural workers fueled expansions of private firms in urban areas. Furthermore, a wide-range of liberalization policies were adopted following Deng Xiaoping's "Tour of the South" in 1992. The boom in the private sector in urban areas intensified competition faced by SOE firms. 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. Although the labor contract rules gave SOE managers more flexibility in hiring new workers, they could not dismiss a worker on the ground of over-staffing (Meng, 2000). Indeed, very few SOE managers chose to fire workers unless their firms face serious financial stress

[2] In particular, China's State Council announced the "Interim Provisions for State-Sector Recruiting" on July 12, 1986, which introduced the basic framework of a new labor contract system for hiring new workers in the state sector. The labor contract system became effective on October 1, 1986.

or under the threat of closure. As competition from private firms intensified over time, 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 massive layoffs (*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 layoff waves 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.[3] 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.[4]

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, and low skilled workers 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).

Since 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), 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. Furthermore, our estimation below suggests that demographic groups more exposed to unemployment risks also had more precautionary savings in response to the SOE reform.

III. A Simple Model of Precautionary Saving

To illustrate how changes in income risks and job uncertainty could affect an individual's precautionary wealth, we consider a simple two-period endowment economy with a continuum of households.

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

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

An individual household has the expected utility function

$$U = u(c_1) + \beta \mathbb{E} u(c_2), \tag{1}$$

where $u(\cdot)$ is the period-utility function, $c_1 \geq 0$ and $c_2 \geq 0$ denote consumption in the two periods, $\beta \in (0,1)$ is a subjective discount factor, and $\mathbb{E}$ is an expectation operator. We assume that the period utility function is strictly increasing, strictly concave, and continuously differentiable, with a postive third derivative.

The household chooses consumption plans c_1 and c_2 and savings s to maximize the utility function in (1), subject to the budget constraints

$$c_1 + s = w_1, \tag{2}$$

$$c_2 = (1+r)s + w_2, \tag{3}$$

where $w_1 = \bar{w}$ is a constant and w_2 is a random variable, with a mean of $\bar{w}$ and a variance of σ^2. The variable r denotes the net real interest rate. For simplicity, we assume that $r > 0$ is a constant.[5]

The interior optimizing decisions for consumption and saving imply the intertemporal Euler equation

$$u'(\bar{w} - s) = \beta(1+r)\mathbb{E}u'((1+r)s + w_2), \tag{4}$$

where we have substituted out c_1 and c_2 using the budget equalities.

Given the exogenous endowments and the interest rate, Eq. (4) determines the equilibrium savings s. Define the functions $f(s)$ and $g(s)$

$$f(s) = u'(\bar{w} - s), \tag{5}$$

$$g(s) = \beta(1+r)\mathbb{E}u'((1+r)s + w_2). \tag{6}$$

The concavity of $u(\cdot)$ implies that $f(s)$ increases with s whereas $g(s)$ decreases with s. The equilibrium savings s^* satisfies $f(s^*) = g(s^*)$.

III.1. **The variance of income shocks and precautionary savings.** Consider the baseline case with no uncertainty in the second period (i.e., with $\sigma = \sigma_0 = 0$, so that $w_2 = \bar{w}$). The equilibrium saving is determined by the intersection of the $f(s)$ curve and the $g(s)$ curve, with the latter evaluated at $w_2 = \bar{w}$. This equilibrium corresponds to point E_0 in Figure 1, with the equilibrium savings given by s_0^*.

Now consider the case with an increase in income variance (i.e. $\sigma_1 > \sigma_0$). Since $u'' < 0$ and $u''' > 0$, the marginal utility of period-2 consumption is a decreasing and convex function of the level of income w_2. It follows from the Jensen's inequality that a mean-preserving spread in w_2 (i.e.,

[5]To ensure non-negative consumption in the second period, we impose a borrowing constraint $s \geq -b$ for some non-negative borrowing limit b. The non-negativity of consumption implies that $b \leq w_2/(1+r)$ for all realizations of w_2, which corresponds to the natural borrowing constraint of Aiyagari (1994).

raising σ while keeping the mean of w_2 at $\bar{w}$) raises the expected marginal utility $\mathbb{E}u'((1+r)s+w_2)$ and shifts the $g(s)$ function upward, leading to an increase in equilibrium savings to s_1^* from s_0^* shown in the top panel of Figure 1.

This example illustrates that precautionary saving increases with the variance of income shocks, provided that the third derivative of the utility function is positive. This is consistent with the textbook model of precautionary savings (Kimball, 1990; Carroll and Kimball, 2008). The variance of income σ in this model corresponds to the idiosyncratic income risks in our empirical model below.

III.2. **The probability of a bad income realization and precautionary savings.** Now consider a different type of income uncertainty, resulting from an increase in the probability of realizing a low-income state. For example, when the aggregate unemployment rate increases sharply, as experienced by Chinese SOE workers during the large-scale SOE reforms in the late 1990s, the perceived probability of unemployment by each individual SOE worker (who is still employed) would increase sharply. We now show that this type of uncertainty also raises precautionary savings.

Assume that the second-period endowment w_2 follows the binary process

$$w_2 = \begin{cases} w_h, & \text{with probability} \quad 1-p, \\ w_l, & \text{with probability} \quad p, \end{cases}$$

where $w_h > w_l$ and $p \in (0,1)$. The term w_l corresponds to the exogenous realization of the low income state.

All else equal, an increase in p reduces expected future income and raises the period-2 marginal utility. In response, the household reduces period-1 consumption and increases savings to smooth consumption. However, that increase in savings reflects the effects of both a reduction in permanent income and an increase in future income uncertainty associated with an increase in p. In order to isolate the precautionary saving effects from the permanent income effects on saving, we assume that the mean income is fixed at $\bar{w}$. This assumption implies that the income level at the good state is given by $w_h = \frac{\bar{w}-pw_l}{1-p}$.

The function $g(s)$ in equation (6) can be written as

$$g(s) = \beta(1+r)\left[(1-p)u'\left((1+r)s + \frac{\bar{w}-pw_l}{1-p}\right) + pu'((1+r)s+w_l)\right]. \tag{7}$$

Differentiating $g(s)$ with respect to p, we obtain

$$\frac{\partial g}{\partial p} = \beta(1+r)\left[-u'(c_{2h}) + u''(c_{2h})\frac{\bar{w}-w_l}{1-p} + u'(c_{2l})\right], \tag{8}$$

where $c_{2h} = (1+r)s + w_h$ and $c_{2l} = (1+r)s + w_l$ denote period-2 consumption in the good and bad states, respectively.

Since $u'(\cdot)$ is continuously differentiable, it follows from the Lagrangian Mean Value Theorem that there exists some $\tilde{c} \in (c_{2l}, c_{2h})$ such that

$$u''(\tilde{c}) = \frac{u'(c_{2h}) - u'(c_{2l})}{c_{2h} - c_{2l}} = \frac{u'(c_{2h}) - u'(c_{2l})}{w_h - w_l}.$$

Then, Eq. (8) implies that

$$\frac{\partial g}{\partial p} = \beta(1+r)\frac{\bar{w} - w_l}{1-p}\left[u''(c_{2h}) - u''(\tilde{c})\right] > 0, \tag{9}$$

where the last inequality follows from the assumption that $u'''(\cdot) > 0$ and $\bar{w} > w_l$.

Thus, an increase in p shifts the $g(s)$ curve upward, as shown in the bottom panel of Figure 1. As a consequence, equilibrium savings increase from s_0^* at point E_0 to s_2^* at point E_2. Since we are holding the expected income level constant at $\bar{w}$, the rise in saving following an increase in the probability of the bad-income state captures the precautionary saving effects rather than the response to changes in permanent income.

IV. Empirical Strategies

We now present our empirical model and estimation approach. In light of the theory illustrated by the simple model above, we examine empirically the effects of both an increase in idiosyncratic income risks and an increase in the probability of unemployment on precautionary savings.

IV.1. **The Empirical Model.** Following Lusardi (1998) and Carroll et al. (2003), we consider the empirical model

$$\frac{W_i}{P_i} = \beta_0 + \beta_1 SOE_i + \beta_2 RISK_i + \beta_3 \log(P_i) + \beta_4' Z_i + v_i. \tag{10}$$

In this model, the dependent variable is the ratio of financial wealth W_i to permanent income P_i for household i, as in Lusardi (1998). This ratio measures the household's cumulative savings relative to her permanent income. The explanatory variable SOE_i is a dummy variable that takes a value of one if the household head works for an SOE and zero if the household head works for a government or public institution (GOV).[6] It captures the relative job uncertainty faced by SOE workers after their iron rice bowl was broken following the SOE reform in the late 1990s. The explanatory variable $RISK_i$ measures idiosyncratic income risks. We also include the log-level of permanent income P_i in the regression to allow for the possibility of non-homothetic preferences. Finally, we include a number of demographic control variables summarized by the vector Z_i. The term v_i denotes regression errors.

We take a difference-in-differences approach to estimating precautionary saving. The CHIP data do not have a panel dimension and thus we cannot keep track of individual households over

[6]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.

time. We run two separate cross-sectional regressions to estimate the model in Eq. (10), one with the pre-treatment group in 1995 and the other with the post-treatment group in 2002.[7]

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 the probability of job losses), the variable $RISK_i$ reflects idiosyncratic income risks for all workers who are still employed. They capture different types of risks, as we discussed in the simple theoretical model in Section III. In our sample, the correlations between these two variables are very low, with a correlation coefficient of about -0.05 in 1995 and -0.17 in 2002, consistent with our view that they capture different aspects of 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.[8] Thus, we estimate an instrumental variable Tobit regression (IV-Tobit). In a robustness check, we also estimate the model in Eq. (10) by eliminating the zero-wealth observations from our sample and then applying the standard two-stage least squares (2SLS) method (see Section VII.3).

IV.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 10 provinces in China. The surveys contain detailed data on households' employment status, education, income, expenditures, and other demographic information. The CHIP data have been frequently used in the empirical literature.[9]

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. More

[7]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 VI.

[8]In our sample, 12.1% of households have zero wealth in 1995 and this share declined to 9.7% in 2002.

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

importantly, both surveys contain data on households' wealth and its compositions, allowing us to examine the quantitative importance of precautionary wealth accumulation caused by the SOE reform.[10]

We restrict our sample to include those households whose heads work in the SOE sector or the GOV sector. The SOEs in our sample include 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.[11] 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 for reasons more closely related to life-cycle factors than to income uncertainty (Carroll and Samwick, 1998; Gourinchas and Parker, 2002).[12]

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.

IV.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. We now describe the measurement of these variables. Table 1 shows the definitions of the variables used in our study. Tables 2 and 3 show the summary statistics of those variables, both in the full sample and for each sector (SOE or GOV).

For our purpose of studying precautionary saving, we focus on relatively liquid components of household wealth (Carroll and Samwick, 1998). In particular, we measure financial wealth (W) by the sum of checking accounts, savings accounts, stocks, bonds, contributions to employer funds, and loans to others. Table 4 shows some summary statistics of the household portfolio compositions in our CHIP sample. Our measure of financial wealth corresponds to asset categories 1-6 in the table.

Here, we use the stock of financial wealth instead of the flow of savings (or the saving rate) for two reasons. First, unlike saving flows, 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

[10] The CHIP surveys in 1988 and 2007 do not report wealth information, and thus they are less useful for studying precautionary saving.

[11] 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%.

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

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 savings 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. 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 dummies and interactions of education with age and age-squared as instruments in all the regressions.[13]

We measure idiosyncratic income risks ($RISK_i$) by the log of the variance of log annual household head income across time (in the current year and the recent past). Our measure here represents idiosyncratic income risks at the household level, which is different from the conventional measure of income risks based on cross-sectional variances of log income (Carroll and Samwick, 1998). To examine the robustness of our findings, we also estimate our model using the conventional risk measure.

In our regression, we control for household demographic characteristics, including the household head's occupation (professional, director or manager, skilled or office worker, or unskilled or other workers), education level (elementary school or below, middle school, high school, or some college or above), health care status (public health care, public health insurance, or own payments), home ownership status, age, age-squared, gender, marital status, occupation, the household size, the ages of children, the number of boys, the number of children at school, and the industry and the province where the household head worked.

The health care reform enacted in 1998 significantly changed the share of household expenditures on 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.

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

[13]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 IQRs are treated as potential outliers and excluded from the sample.

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 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 1.3). In 2002, the gaps in both wealth and income widened substantially across the two sectors, with GOV workers owning even more wealth and earning even higher 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 the 1995 sample, a large majority of 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, the share of government assigned jobs declined somewhat in both sectors (to about 76% in the GOV sector and 69% in the SOE sector), although they still constitute a majority of all jobs. 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.

In the 1995 sample, over 80% of SOE workers obtained their current jobs prior to the labor market reform in 1986. The pre-1986 worker cohort has a relatively smaller share in the GOV sector (at 65%). In the 2002 sample, the size of the pre-1986 cohort shrank in both sectors. They account for about 63% in the SOE sector and 40% in the GOV sector. As we have discussed in Section II, the labor contract system initiated in 1986 was applied to new hires only and did not apply to existing workers. Thus, for the pre-1986 cohort, the perceived unemployment risks for GOV and SOE jobs were equally low, until the iron rice bowl was broken for SOE workers. We use this unique feature of the Chinese labor market as an alternative identification of self-selection biases.

The SOE reform in the late 1990s also 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.[14] 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. In Section V.4.1 below, we examine the implications of income expectations for precautionary savings.

Accompanying the large scale SOE reform, the Chinese government also started to systematically reform the pension system in urban China in 1997. The goal was to establish a "three-pillar" pension system which combines a pay-as-you-go system and a mandatory individual retirement account (He et al., 2017). The reform was implemented more rapidly in SOE firms than in the government sector. In our 2002 CHIP sample, 82.4% of SOE workers were enrolled in the new pension system and 49% of GOV workers participated in the new system. Participation in the new pension system could affect individual workers' expectations of retirement income, and thereby influencing their saving decisions through a channel similar to the PIH effect. We discuss the implications of the pension participation for precautionary savings in Section V.4.2 below.

V. Empirical Results

We now present and discuss the main empirical results.

V.1. **Evidence of precautionary saving.** We estimate the empirical model in Eq. (10) using our CHIP data. 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.

Table 5 shows the estimation results for three different samples in 1995 and 2002: the full sample [Column (i)], the subsample with government assigned jobs [Column (ii)], and the subsample with the pre-1986 worker cohort [Column (iii)].

We begin with discussing the estimation results in the full sample. The estimated value of β_1 in 1995 is slightly negative (at -0.047) and statistically insignificant, indicating that the savings of SOE and GOV workers were statistically and economically similar in 1995 when demographic characteristics are controlled for. In 2002, however, SOE workers accumulated significantly more wealth than GOV employees (reflected by a much large estimate of $\beta_1 = 0.366$). The Chow test rejects the null hypothesis that β_1 is identical between 1995 and 2002, with a p-value of 0.055. The difference between the two estimated values of β_1 ($0.366-(-0.047) = 0.413$) is not just statistically significant, but also economically large; it suggests that, all else equal, the extra savings of SOE

[14]The 1995 survey does not include a question about income expectations.

workers relative to GOV workers after the reform were about 0.413 times of their annual permanent income, or about 5 months worth of permanent income.[15] Despite the potential downward bias caused by self-selection in the full sample, the evidence here suggests that increases in the relative income uncertainty for SOE workers after the reform has led to significant precautionary savings.

The estimated coefficient β_2 on $RISK$ suggests that idiosyncratic income risks had positive and significant effects on savings in both 1995 and 2002. Thus, all households—working in SOE or GOV— responded to increases in idiosyncratic income risks by raising savings, consistent with the implication of the theoretical model presented in Section III.1. 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 in magnitude and both are significant. In contrast, the value of β_1 was much larger in 2002 than in 1995 and turned from insignificant to significant. 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.

Our estimation also suggests that households with high permanent income tend to save more, consistent with the presence of non-homothetic preferences, although the coefficient on $\log(P)$ was insignificant in 1995 and became significant in 2002.

The occupation of the household head had mixed effects on savings. We partition the occupations into four groups: professionals, directors or managers, skilled workers, and unskilled workers and others. We use professionals as our reference group. The estimation suggests that directors and managers saved more than professionals in 1995, although the differences in saving behaviors across occupation groups become insignificant in the 2002 sample.

The coefficients of both health care dummy variables are small and insignificant in 1995 but become significantly negative in 2002 (the reference group here includes those households who self financed health care expenditures). This result is consistent with China's health care system and its reform. In 1995, most workers were covered under a near-free public health care system, so that the health care status did not have significant impact on households' savings. However, after the health 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.

[15]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 0.413 units implies an increase in W of an amount equivalent to $0.413 * 12 = 4.96$ months of permanent income.

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 leads 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).

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 insignificant for both years, possibly reflecting that the housing market in China was still under-developed through 2002.

Our empirical model also controls for other demographic variables such as age, age-squared, sex of the household head, marital status, and the household size. The full sample estimation suggests that households with female heads save significantly more than those with male household heads in 1995; and they save even more in 2002. Married households also saved more, although the effects of marital status on wealth accumulation were statistically significant only for 1995, not for 2002. The household size had little effects on savings in 1995, but larger households saved significantly more in 2002.

V.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. Without correcting self-selection biases, the estimation using the full sample may understate the true magnitude of precautionary savings.

We consider two alternative identification strategies for self-selection biases. First, we follow the approach in Fuchs-Schündeln and Schündeln (2005) and restrict our sample to workers whose jobs were assigned by the government. As we have discussed in Section II, jobs were assigned primarily based on quotas and "needs" of the local governments, rather than the preferences of individual workers. Thus, under the regime with government job assignments, individual workers were not able to self select occupations.

Second, we exploit the changes in China's labor market institution caused by the labor market reform in 1986. Under the pre-1986 regime, SOE and GOV workers all had guaranteed life-time employment and firing a worker was very difficult and rarely occurred in practice. Thus, for workers who obtained jobs prior to 1986 (i.e, the pre-1986 cohort), jobs in GOV and SOE were equally secure and there should be no bases for self selection.

The estimation results using the subsample with government assigned jobs are shown in Table 5 [Column (ii) under each year]. Our estimation shows that self selection indeed caused a significant downward bias in the estimated value of β_1 after the reform, but not before. In particular, the estimated value of β_1 in 1995 in the subsample with government assigned jobs is very similar to that in the full sample (-0.012 vs. -0.047), and both are statistically insignificant. In 2002, however, the estimate of β_1 for workers with assigned jobs is much greater and statistically more significant than that in the full sample (0.539 vs. 0.366). As in the full sample, the Chow test for the SOE dummy in this subsample estimation strongly rejects the null hypothesis that the estimated value of β_1 in 2002 is identical to that in 1995, with a p-value of 0.049.

Our estimation using the pre-1986 cohort confirms that the self-selection bias is important, as shown in Table 5 [Column (iii) under each year]. In the 1995 sample, the point estimate of β_1 based on the pre-1986 cohort is similar to that in the full sample (-0.059 vs. -0.047), both insignificant. In 2002, the estimate of β_1 from the pre-1986 cohort increases substantially to 0.526, much larger than that obtained in the full sample (0.366), although it is marginally significant (at the 12% level), partly reflecting the smaller sample size. Nonetheless, the Chow test rejects the null hypothesis that the estimated value of β_1 from the pre-1986 cohort is identical across the two years, with a p-value of 0.098.

Despite the differences between the two alternative identification strategies for self-selection biases, we obtained remarkably similar magnitude of precautionary savings. The 2002 estimate of β_1 is 0.539 (significant at the 5% level) from the sample with government assigned jobs, which is very close to that estimated from the pre-1986 cohort (0.526). The 1995 estimates of β_1 from the two samples are also similar, both are small and insignificant. Since the 2002 value of β_1 is more precisely estimated using the sample with government assigned jobs, we use that sample as our baseline for quantitative inferences on the importance of precautionary savings after controlling for self-selection biases.[16]

[16]Comparing the estimation results between the full sample and the subsample with government assigned jobs, we see that not only the coefficient on the SOE dummy changes, but some other coefficients, especially those on occupations changed significance. In particular, the coefficient on unskilled and other workers in 2002 turned from insignificant in the full sample to significant in the subsample. This difference partly reflects the fact that unskilled workers were less likely to obtain jobs through government assignments than other occupations. In our 2002 sample, the share of government assigned jobs for unskilled workers is about 52%, much lower than that for professionals

To summarize, we have obtained two important findings. First, even without controlling for self-selection biases, we find significant presence of precautionary savings caused by the large-scale SOE reform. Second, self-selection causes significant downward biases in estimating precautionary wealth accumulation. When we correct for self selection by focusing on government assigned jobs or the pre-1986 worker cohort, the magnitude of precautionary savings rises significantly relative to that estimated from the full sample.

V.3. **Quantitative importance of precautionary savings.** Using the SOE reform as a natural experiment, we have identified the presence of precautionary saving. But to what extent can precautionary savings 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.

To implement this idea, we go through the following steps. First, we calculate the model's predicted wealth held by SOE workers in 1995 and in 2002 (denoted by $\hat{W}^{soe}_{1995}$ and $\hat{W}^{soe}_{2002}$, respectively) using the baseline estimation results after correcting self-selection biases using the subsample with government assigned jobs [Column (ii) under each year in Table 5].

Second, 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 step, 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 third (and final) step, we compute the magnitude of precautionary wealth accumulation (denoted by W^{ps}) stemming from the large-scale SOE reforms according to the relation

$$W^{ps} = (\hat{W}^{soe}_{2002} - \hat{W}^{soe}_{1995}) - (\tilde{W}^{soe}_{2002} - \tilde{W}^{soe}_{1995}). \tag{11}$$

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 calculation suggests that, with self-selection biases corrected, precautionary savings account for 44.2% of financial wealth accumulation for SOE households between 1995 and 2002, and this magnitude is statistically significant with a standard error of 0.209. Thus, the SOE reforms in the late 1990s led to quantitatively important precautionary savings by SOE households.

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and directors or managers (about 81%) or that for skilled workers (about 70%), suggesting that self-selection biases are likely more pronounced for unskilled workers.

In comparison, without correcting self-selection biases, the contribution of precautionary savings to SOE household wealth accumulation would have been lower at 31% [calculated based on Column (i) for each year in Table 5], which is also significant with a standard error of 0.152. Therefore, self selection leads to a downward bias of the estimated precautionary savings of about 30% ($(0.442 - 0.31)/0.442 \approx 0.30$).

Our findings on the quantitative importance of both precautionary savings and self-selection biases are consistent with those obtained by Fuchs-Schündeln and Schündeln (2005) using German data.[17]

V.4. **The PIH effects.** 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. As we illustrate in the simple theoretical model in Section III, 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 PIH 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.

Parallel to the SOE reform, the Chinese government also rolled out a new pension system in 1997. The new pension system is a pay-as-you-go system that requires employee contributions. It was implemented gradually, but more quickly in SOEs than in GOV. By 2002, about 82% of SOE workers in our CHIP sample participated in the new system and made pension contributions, whereas about half of GOV workers participated.[18]

For SOE workers who were not enrolled into the new pension system, they were still with the old system but facing the risk that, once the iron rice bowl is broken, they would lose both their jobs and retirement benefits. Thus, implementations of the pension system could lead to different expectations of future retirement income for SOE workers, depending on whether or not they participated in the new system.

We now discuss our approach to control for PIH effects in estimating precautionary savings. We consider the effects of both short-term income expectations and pension participation.

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[17]Fuchs-Schündeln and Schündeln (2005) find that precautionary wealth estimated from a sample with former GDR households who chose their occupations before the German reunification amounts to 22% of total wealth, while that in the West German sample amounts to 13% of total wealth. They argue that, since the former GDR sample is not subject to self-selection biases while the West German sample is, self selection causes a downward bias in the estimation of precautionary savings of about 41%, which is slightly larger than our estimate of 30% using Chinese data.

[18]In 1995, the question about pension contributions was not available since all workers were on the old system with retirement benefits included as a part of the iron rice bowl.

V.4.1. *Short-term income expectations.* We measure short-term income expectations by using a unique question in the 2002 CHIP survey that asks households about their expectations of income changes for the next five years (increase, decrease, or no change). 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.[19]

To control for the effects from income expectations, we construct a dummy variable "income decline" that equals one if the household head expected income to decline in the next 5 years and zero otherwise. We add the "income decline" dummy and its interactions with the SOE dummy to the baseline model.

The estimation results are reported in Table 6 (Column (ii)). For comparison, we also include the estimation results from the baseline model (Column (i)). The estimated coefficients on both the dummy variable "income decline" and its interactions with SOE are small and insignificant.

The coefficient on the SOE dummy (β_1) under this model specification captures the extra savings by an SOE household relative to a comparable GOV household in the group that did not expect income to decline (i.e., with the "income decline" dummy set to zero). The estimated β_1 is very similar to that from the baseline model (0.542 vs. 0.539), and it remains significant at the 10% level. This finding suggests that changes in short-term income expectations did not have significant marginal impact on households' precautionary savings.

V.4.2. *Pension participation.* We now control for the effects of pension participation on our estimation of precautionary savings. We construct a dummy variable "no-pension" that takes a value of one if a household head in our 2002 sample *did not* participate in the new pension system and zero otherwise. We add the no-pension dummy and its interactions with the SOE dummy in the baseline empirical model and report the estimation results in Column (iii) of Table 6.

The estimated coefficient on the SOE dummy (β_1) is large (0.562) and significant at the 5% level. This coefficient measures the extra savings by an SOE household relative to a comparable GOV household, conditional on that they both participated in the new pension system. Since the new pension system provides a more reliable source of retirement income than the old system, this estimate reflects SOE households' precautionary savings in response to the SOE reform when we control for the effects of expectations about future retirement income. The magnitude of

[19]The fact that SOE workers are more likely to expect an income decline than GOV workers could be driven by the increased unemployment risk. In our 2002 sample, the correlation between the SOE dummy variable and the expected income decline dummy is small but positive (at 0.158 and significant at the 1% level). Since the 2002 CHIP survey does not provide information on the size of the expected income declines, it is hard to completely disentangle precautionary savings from income expectations conditional on staying employed. Still, the qualitative information about expected income changes provided in the CHIP survey helps to control for the PIH effects on household saving.

precautionary savings obtained under this specification is modestly greater than that from the baseline model (0.562 vs. 0.539).

The estimated coefficient on the no-pension dummy is positive (0.267) and significant at the 10% level. This positive coefficient implies that, all else equal, GOV workers who did not participate in the new pension system saved significantly more than those who did. This result indicates that, for GOV workers who stayed on the old pension system, uncertainty about future pension arrangment (e.g., whether and when they would be enrolled in the new pension system) led to more savings. The estimated coefficient on the interaction term is positive but insignificant.

Our findings here suggest that, although changes in short-term income expectations did not have significant impact on precautionary savings, pension participation was relatively more important.[20]

V.5. **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 energy, telecommunications, and heavy manufacturing) 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). Evidence suggests that those large SOEs that survived the reorganization gained even more government protections for their monopoly power, leading to higher profits than before the reform (Li et al., 2015).

Since the government policy explicitly favored large SOEs, workers in large SOEs faced smaller increases in unemployment risks than those in small SOEs (Appleton et al., 2002). Therefore, 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).[21] 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.[22] We modify the benchmark model in equation (10) 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

$$W_i/P_i = \beta_0 + \beta_1^{CSOE} CSOE_i + \beta_1^{LSOE} LSOE_i + \beta_2 RISK_i + \beta_3 \log(P_i) + \beta_4' Z_i + v_i \qquad (12)$$

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.

[20] Since our data on income expectations and pension participation are available only in the 2002 survey and not in the 1995 survey, we do not use the estimation results in Table 6 for quantitative inferences.

[21] LSOE also includes urban collective enterprises.

[22] 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.

Table 7 (Panel A) reports the regression results from the sample with government assigned jobs. From 1995 to 2002, β_1^{CSOE} increased from -0.157 to 0.343, although it is not significant in both years. In contrast, β_1^{LSOE} was estimated to be 0.075 and insignificant in 1995, but it rose sharply to 0.769 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 0.08. 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.6. **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 (10) for each age cohort using the sample with government assigned jobs.

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 age cohorts. Table 7 (Panel B) shows that, in 2002, the estimated value of β_1 for the young cohort is much greater than that for the full sample (0.942 vs. 0.539), and both are significant at the 5% level. The estimated value of β_1 for the old cohort is much smaller (0.292) 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.

V.7. **Other demographic factors.** During the periods of the SOE reform, there is evidence that specific demographic groups including female, less skilled, and less educated workers are more likely to be laid off (Appleton et al., 2002). We now examine the precautionary saving behaviors of these specific demographic groups.

Table 7 (Panel C) shows the estimation results using a few different subsamples of the data (again, focusing on government assigned jobs in both years). Evidently, if a household head is female, then the household has a stronger precautionary saving motive. The value of β_1 for this group in 1995 is insignificant, but becomes significant in 2002 and is indeed larger than that in the full sample (0.931 vs. 0.539). We obtain qualitatively similar results when we consider the samples with female or less skilled; female or less educated; or female, less skilled, or less educated. These results, putting together, suggest that SOE workers who faced higher unemployment risks accumulated more precautionary wealth in response to the reform.

VI. 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 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.

VI.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

$$\Pr(\text{layoff}_i = 1 \mid Z_i) = \Phi(Z_i \delta), \tag{13}$$

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 one if an individual had layoff experience, and zero otherwise.[23]

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

[23]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 add all jobless households to 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.

similar characteristics with the 2002 sample (who are ex post survivors of the layoffs), except that they face different levels of unemployment risks.

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 baseline results reported in Table 5 [Column (ii) under 1995]. Column (2) here shows that if we drop those SOE workers who had the top 10% layoff probability, the coefficient β_1 of the SOE dummy remains insignificant, although it increases slightly from -0.012 to 0.003. 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 0.055 and 0.087 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 ($0.452 = 0.539 - 0.087$ vs. $0.551 = 0.539 - (-0.012)$), 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 savings caused by the large-scale SOE reform remain evident.

VI.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

$$\Pr(\text{quit}_i = 1 \mid Z_i) = \Phi(Z_i \delta), \tag{14}$$

where, similar to the Probit model for layoffs in equation (13), the term Z_i is a vector of individual characteristics and the dependent variable is a dummy variable that takes a value of one if the individual has quit experience and zero 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 SOE workers in 1995, the estimated value of β_1 is slightly larger than the full sample (0.027 vs. -0.012), but it remains statistically insignificant. When we further restrict the 1995 sample by excluding the top 4% or even the top 6% of most likely quitting SOE 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.

VII. Robustness

In this section, we examine the sensitivity of our estimation of precautionary saving. In particular, we consider the implications of spouse occupations, homeownership status, and some alternative sampling and measurement methods. Although these factors change 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 self-selection biases remains robust.

Table 9 shows the estimated values of β_1 for 1995 and 2002 under these alternative specifications, as well as the implied contributions of precautionary savings to overall wealth accumulations for SOE households (the last column). For comparison, we also display the baseline estimation (Panel A).[24]

VII.1. **Spouse effects.** The precautionary savings that we have estimated are based on the regression model in equation (10), 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 control for the effects of the working status of the spouse, we add a dummy variable SOE^{sp} that indicates whether or not the spouse works for an SOE in our regressions. 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. Thus, the coefficient for SOE^{sp} in the 2002 sample should be positive. This turns out to be true. In particular, for the 2002 sample, the estimated coefficient for SOE^{sp} is significantly positive at 0.237 (with a p-value less than 10%, not shown in the table).

With the spouse effects controlled, 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 0.464, which is smaller than the benchmark value of

[24]To conserve space, we report the detailed estimation results in the alternative models and with alternative measurements in a Supplemental Appendix available online at `http://www.frbsf.org/economic-research/files/wp2014-04_appendix.pdf`.

0.539, 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 41.7% for SOE households.

VII.2. **Housing effects.** To control for the effects of potential savings by SOE workers for home purchases rather than for precaution against future unemployment risks, we add a dummy variable indicating non-homeownership and its interaction with the SOE dummy in our regression model (10). 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 homeownership status controlled, the estimated coefficient for the SOE dummy is 0.068 (s.e. $=$ 0.113) in 1995 and 0.520 (s.e. $=$ 0.259) 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. But it still accounts for 37.9% (s.e. $=$ 0.208) of total wealth accumulation for those households.

VII.3. **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 (10) 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 3190 and 1977 observations for 1995 and 2002, respectively. Table 9 summarizes the estimation results (in Panel D).

The estimated value of β_1 is 0.034 (insignificant) in 1995 and 0.372 (significant at the 10% level) in 2002. Thus, excluding zero-wealth observations from the sample modestly reduces the estimated magnitude of precautionary saving. Nonetheless, precautionary savings still account for about 24.3% (s.e. $=$ 0.144) of total wealth accumulations for SOE workers.

VII.4. **Alternative risk measure.** In our benchmark model, we measure household idiosyncratic risks ($RISK$) by the log variance of log income over the current and recent past years. To examine the sensitivity of our results, we consider the 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.[25] Unlike our measure $RISK$, which reflects a household's income variations across time,

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

this alternative risk measure is computed based on cross-sectional variations of income in the current year. The estimation results are shown in panel E of Table 9.

Our main results are not sensitive to using the alternative risk measure. The estimated values of β_1 are similar to those obtained from the baseline model. In particular, β_1 increases from -0.022 (insignificant) in 1995 to 0.522 (significant at the 5% level) in 2002. These estimates imply that precautionary savings account for about 43.1% (s.e. = 0.208) of the increases in financial wealth for SOE workers from 1995 to 2002.

VII.5. **Alternative wealth measures.** Some alternative measures of wealth such as very liquid assets (VLA) 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 definition of these variables).

Panel F of Table 9 presents the results using very liquid assets as wealth measure to construct the dependent variable in equation (10). The estimated value of β_1 increases from 0.003 (insignificant) in 1995 to 0.475 (significant at the 10% level) in 2002. These estimates imply that precautionary savings contribute about 40.2% (s.e. = 0.213) 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 0.111 and insignificant in 1995 and it increases substantially to 0.851 (significant at the 5% level) in 2002. In this case, precautionary savings account for about 47.8% (s.e. = 0.201) of the increases in wealth accumulation for SOE workers from 1995 to 2002.

VIII. 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 savings in a rapidly growing transition economy. With self-selection in occupational choices corrected, 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 savings can account for about 40 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 71,000 workers in SOEs in the coal sector in Fushun, 35,000 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. In our sample, we include all four types of layoffs.

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, female, less educated, or low skilled. It is very hard for them to find a job given the 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.[26]

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[26]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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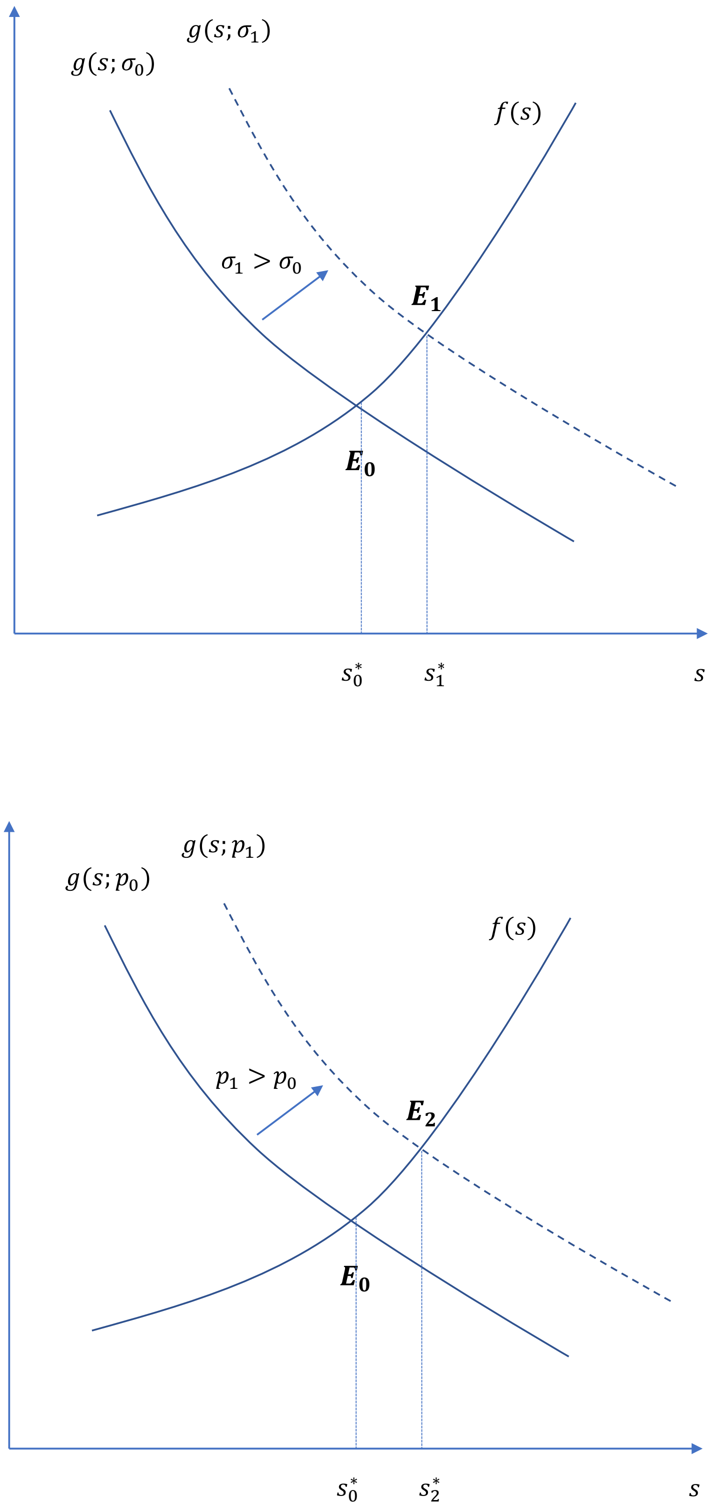

FIGURE 1. Precautionary savings illustrated: Effects of an increase in income variance (upper panel) and an increase in the probability of the low-income state (lower panel).

Table 1. Definition of variables

Variable	**Description**
Financial wealth (W)	Balances in checking accounts, saving accounts, stocks, bonds, contributions to employer funds, and loans to others
Very liquid assets (VLA)	Financial wealth minus contributions to employer funds and loans to others
Non-housing non-business wealth (NHNBW)	Financial wealth plus estimated market value of durables and other assets, minus total debt
Income	Total annual income of the household head, including salaries and bonuses, subsidies, other labor income, property income, and transfer income
Income risk ($RISK$)	The log of the variance of log annual income over the past few years (see text)
SOE	Dummy variable that equals one if household head works for SOE and zero for Government
Permanent income (P)	Constructed based on earnings by household heads in the current year and the recent past (See text)
W/P	Ratio of wealth to permanent income
Age	Age of household head
Male	Dummy variable that equals one if household head is male and zero otherwise
Married	Dummy variable, equals one if household head is married and zero otherwise
Education	Dummy variable for household head's level of education: college, senior middle school, junior middle school, or elementary school or below
Occupation	Dummy variable for 4 occupations: (1) professional, (2) director or manager, (3) skilled or office workers, and (4) unskilled, service workers or other (see text)
Health care	Dummy variable for health care status: public health care, public health insurance, or self pay (see text)
Non-homeowner	Dummy variable for housing ownership, equals 1 if not a house owner and 0 otherwise
Child age	Mean age of children in household
Num. of boys	Number of boys in household
Children at school	Number of children at school
Job assigned by Gov.	Dummy variable that equals one if the household head obtained current job through government assignments and zero otherwise
Pre-1986 employment	Dummy variable that equals one if the household head obtained current job before the labor market reform in 1986 and zero otherwise
Expected income decline	Dummy variable that equals one if the household head expects income to decline in the next 5 years and zero otherwise
No-pension	Dummy variable that equals one if the household head did not participate in the new pension system and zero otherwise

TABLE 2. Summary statistics of the full sample

Variable	**1995**			**2002**		
	Obs.	**Mean/%**	**SD**	**Obs.**	**Mean/%**	**SD**
Financial wealth (W)	4390	9556	9892	3027	25669	27443
Annual permanent income (P)	4390	7520	3131	3027	12843	6018
log variance(HH log inc.)	4390	-3.41	1.28	3027	-3.76	1.92
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	19.9%		3027	23.1%	
Public health care	4390	71.3%		3027	35%	
Public health insurance	4390	8.8%		3027	41.9%	
Non-homeowner	4390	58.0%		3027	19.6%	
SOE	4390	67.8%		3027	56.2%	
Job assigned by Gov.	4375	82.9%		3018	71.9%	
Pre-1986 employment	4390	75.1%		3027	53.2%	
Expected income to decline	N.A	N.A		3020	18.4%	
No-pension	N.A	N.A		3027	32.2%	

Notes: Data are taken from CHIP surveys. Monetary values are in constant Chinese Yuan units, with 2002 as the base year.

Table 3. Comparison between employees in GOV vs. SOEs

Variable	**1995**			**2002**		
	Obs.	**Mean/%**	**SD**	**Obs.**	**Mean/%**	**SD**
GOV						
Financial wealth (W)	1413	10004	9940	1325	27041	27924
Annual permanent income (P)	1413	7905	3063	1325	13979	5853
W/P	1413	1.306	1.296	1325	1.981	2.117
Non-homeowner	1413	54.6%		1325	16.5%	
Job assigned by Gov.	1408	89.3%		1319	75.7%	
Pre-1986 employment	1408	64.7%		1319	40.2%	
Expected income to decline	N.A	N.A		1321	11.4%	
No-pension	N.A	N.A		1325	51.0%	
SOE						
Financial wealth (W)	2977	9343	9864	1702	24600	27023
Annual permanent income (P)	2977	7337	3146	1702	11958	5998
W/P	2977	1.305	1.383	1702	2.136	2.481
Non-homeowner	2977	59.7%		1702	22.0%	
Job assigned by Gov.	2967	79.8%		1699	68.9%	
Pre-1986 employment	2977	80.1%		1702	63.2%	
Expected income to decline	N.A	N.A		1699	23.8%	
No-pension	N.A	N.A		1702	17.6%	

Notes: Data are taken from CHIP surveys. Monetary values are in constant Chinese Yuan units, with 2002 as the base year.

TABLE 4. Wealth Composition

Items	**1995**			**2002**		
	Mean	**SD**	**% of W**	**Mean**	**SD**	**% of W**
(1) balances in checking accounts	6400	7844	67%	15406	20372	60%
(2) balances in saving accounts	1244	2406	13%	4666	7674	18%
(3) stocks	343	1705	4%	3277	10668	13%
(4) bonds	858	2484	9%	712	4427	3%
(5) contributions to employer funds	396	1904	4%	397	3097	2%
(6) loans to others	315	1456	3%	1211	5126	5%
Very liquid assets [VLA, items (1)-(4)]	8845	9459		24061	26264	
Financial wealth [W, items (1)-(6)]	9556	9892		25669	27443	
Non-housing, nonbusiness net worth (NHNBW)	19429	15876		39111	40337	
Sample size	4390			3027		

Notes: Data are taken from CHIP surveys. Monetary values are in constant Chinese Yuan units, with 2002 as the base year. Non-housing nonbusiness net worth (NHNBW) equals financial wealth plus estimated market value of durable goods and other assets, minus total debt.

Table 5. IV-Tobit regressions, 1995 and 2002 samples

Dep. variable:	**1995**			**2002**		
W/P	**(i)**	**(ii)**	**(iii)**	**(i)**	**(ii)**	**(iii)**
	Full sample	**Job assigned**	**Pre-1986**	**Full sample**	**Job assigned**	**Pre-1986**
SOE	-0.047	-0.012	-0.059	0.366*	0.539**	0.526
	(0.093)	(0.094)	(0.104)	(0.194)	(0.264)	(0.338)
RISK	0.197***	0.170***	0.169***	0.180***	0.145***	0.177***
	(0.048)	(0.052)	(0.046)	(0.043)	(0.049)	(0.054)
log(P)	1.253	0.846	1.164	2.473***	2.840**	5.187***
	(0.905)	(1.010)	(0.895)	(0.854)	(1.261)	(1.721)
Director/manager	0.164**	0.190**	0.114	-0.039	0.122	-0.065
	(0.078)	(0.080)	(0.088)	(0.157)	(0.177)	(0.269)
Skilled worker	-0.027	-0.070	-0.072	-0.009	0.148	0.384
	(0.095)	(0.104)	(0.097)	(0.158)	(0.188)	(0.299)
Unskilled/others	-0.003	-0.127	-0.055	0.362	0.798*	1.024*
	(0.164)	(0.180)	(0.166)	(0.319)	(0.485)	(0.561)
Public health care	0.042	0.024	0.048	-0.831**	-0.976**	-1.259**
	(0.168)	(0.189)	(0.173)	(0.340)	(0.454)	(0.536)
Public med insurance	0.090	-0.009	0.132	-0.594**	-0.654*	-0.943*
	(0.145)	(0.165)	(0.158)	(0.299)	(0.396)	(0.483)
Child age (mean)	0.005	0.006	0.008	-0.002	0.000	-0.007
	(0.005)	(0.006)	(0.006)	(0.009)	(0.011)	(0.013)
Num. of boys	0.022	0.045	0.027	-0.283***	-0.281**	-0.362**
	(0.044)	(0.047)	(0.049)	(0.102)	(0.120)	(0.166)
Children at school	-0.050	-0.097	-0.050	-0.274**	-0.221	-0.267
	(0.061)	(0.064)	(0.070)	(0.120)	(0.143)	(0.230)
Non-homeowner	0.083	0.018	0.067	-0.055	-0.095	-0.021
	(0.065)	(0.068)	(0.077)	(0.148)	(0.179)	(0.235)
Age	-0.000	0.033	0.047	0.156	0.038	0.234
	(0.048)	(0.050)	(0.054)	(0.106)	(0.125)	(0.259)
$Age^2 * 100$	-0.007	-0.046	-0.060	-0.166	-0.033	-0.242
	(0.056)	(0.057)	(0.063)	(0.125)	(0.145)	(0.290)
Male	-0.452***	-0.364***	-0.464***	-0.862***	-0.807***	-0.894***
	(0.091)	(0.098)	(0.093)	(0.131)	(0.169)	(0.213)
Married	0.461***	0.503***	0.469***	0.228	0.385	0.502
	(0.158)	(0.191)	(0.180)	(0.324)	(0.357)	(0.492)
HH size	-0.012	-0.039	-0.041	0.389***	0.270*	0.334*
	(0.047)	(0.050)	(0.053)	(0.125)	(0.152)	(0.175)
Log-Likelihood	-8711.71	-7045.37	-6475.21	-7855.88	-5519.87	-4084.53
p-value						
(Chow test for SOE)				0.055	0.049	0.098
Sample size	4390	3627	3298	3027	2170	1609

Notes: For each year, Column (i) shows the estimation results in the full sample, Column (ii) shows the results using the sample with government-assigned jobs, and Column (iii) shows the results using the pre-1986 worker cohort. All regressions include controls for fixed effects of locations (provinces of current residence) and industries. Robust standard errors are in parentheses. ***, **, and * indicate p-values of less than 1%, 5%, and 10%, respectively.

Table 6. Regressions with 2002 sample: controlling for PIH effects

Dep. variable:	**Model specifications**		
W/P	**(i)**	**(ii)**	**(iii)**
SOE	0.539**	0.542*	0.562**
	(0.264)	(0.278)	(0.269)
RISK	0.145***	0.150***	0.147***
	(0.049)	(0.050)	(0.049)
log(P)	2.840**	2.962**	2.851**
	(1.261)	(1.283)	(1.278)
Income decline		-0.080	
		(0.260)	
Income decline× SOE		0.115	
		(0.329)	
No-pension			0.267*
			(0.155)
No-pension × SOE			0.217
			(0.316)
Log-Likelihood	-5519.78	-5505.39	-5515.17
p-value of Chow test for SOE	0.049	0.059	0.044
Sample size	2170	2164	2170

Notes: IV-Tobit regression results based on the 2002 sample with government assigned jobs. Column (i) shows the baseline regression results. Column (ii) adds controls for expected income declines and its interaction with SOE. Column (iii) adds controls for pension participation and its interaction with SOE. In each regression, all other control variables shown in Table 5 are included. Robust standard errors are in parentheses. ***, **, and * indicate p-values of less than 1%, 5%, and 10%, respectively.

Table 7. Precautionary saving and demographic factors

Sample group	**Variable**	**Coefficient**	
		1995	**2002**
***Panel A**: Firm size effects*			
Baseline sample	CSOE	-0.157	0.343
		(0.128)	(0.237)
	LSOE	0.075	0.769**
		(0.150)	(0.367)
		[n=3627]	[n=2170]
***Panel B**: Lifecycle effects*			
Age 25-44	SOE	-0.015	0.942**
		(0.144)	(0.380)
		[n=2349]	[n=1123]
Age 45-55	SOE	0.042	0.292
		(0.145)	(0.594)
		[n=1278]	[n=1047]
***Panel C**: Other demographic factors*			
Female	SOE	-0.130	0.931*
		(0.193)	(0.526)
		[n=1305]	[n=585]
Female or Less skilled	SOE	-0.126	1.365*
		(0.160)	(0.777)
		[n=1572]	[n=756]
Female or Less educated	SOE	-0.043	0.871*
		(0.125)	(0.459)
		[n=2063]	[n=984]
Female, or Less educated, or Less skilled	SOE	-0.063	1.227*
		(0.126)	(0.697)
		[n=2157]	[n=1060]

Notes: Results are from the IV-Tobit regressions. All household heads in the samples had government assigned jobs. Each regression includes the same set of control variables shown in Table 5. Robust standard errors are in parentheses. ***, **, and * indicate p-values of less than 1%, 5%, and 10%, respectively.

Table 8. Correcting sample selection biases

***Panel A**: Controlling for survival biases*				
Dep. variable	1995 survival threshold			
W/P	100%	90%	80%	70%
SOE	-0.012	0.003	0.055	0.087
	(0.094)	(0.100)	(0.113)	(0.112)
RISK	0.170***	0.189***	0.247***	0.240***
	(0.052)	(0.071)	(0.077)	(0.063)
log(P)	0.846	1.271	2.420*	2.303*
	(1.010)	(1.367)	(1.465)	(1.179)
Sample size	3627	3415	3198	2971

***Panel B**: Controlling for voluntary quits*				
Dep. variable	1995 non-quit threshold			
W/P	100%	98%	96%	94%
SOE	-0.012	0.027	-0.016	-0.004
	(0.094)	(0.103)	(0.120)	(0.128)
RISK	0.170***	0.174***	0.180***	0.177***
	(0.052)	(0.050)	(0.058)	(0.061)
log(P)	0.846	0.930	0.945	0.901
	(1.010)	(0.966)	(1.109)	(1.199)
Sample size	3627	3582	3532	3485

Notes: Results are from the IV-Tobit regressions. All household heads in the samples had government assigned jobs. Each regression includes the same set of control variables shown in Table 5. Robust standard errors are in parentheses. ***, **, and * indicate p-values of less than 1%, 5%, and 10%, respectively.

TABLE 9. Robustness

Cases	**1995**	**2002**	**Contributions**
A. Benchmark	-0.012	0.539**	44.2%**
	(0.094)	(0.264)	(0.209)
B. Spouse effects	-0.006	0.464*	41.7%*
	(0.099)	(0.265)	(0.231)
C. Housing effects	0.068	0.520**	37.9%*
	(0.113)	(0.259)	(0.208)
D. Eliminating zero wealth	0.034	0.372*	24.3%*
	(0.086)	(0.216)	(0.144)
E. Alternative risk measure	-0.022	0.522**	42.5%**
	(0.094)	(0.260)	(0.204)
F. Very liquid asset	0.003	0.475*	40.2%*
	(0.091)	(0.251)	(0.213)
G. Non-housing Non-business wealth	0.111	0.851**	47.8%**
	(0.131)	(0.357)	(0.201)

Notes: We use the standard IV (2SLS) regression for the case that eliminates zero-wealth observations (Panel D) and the case with non-housing non-business wealth (Panel G). We use IV-Tobit regressions for the other cases. Panel A shows the estimation results from the baseline model. Panels B-G report the estimation results under alternative model specifications and alternative variable measurements. Each regression uses the sample with government assigned jobs and includes the same set of control variables shown in Table 5. The last column shows the contributions of precautionary savings to total wealth accumulation by SOE workers. Robust standard errors are in parentheses. ***, **, and * indicate p-values of less than 1%, 5%, and 10%, respectively.