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German Reunification “Experiment”

by

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Precautionary Savings and Self-Selection - Evidence from the German Reunification “Experiment”*

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April 19, 2005

Abstract

We combine particular features of the German civil service with the unique event of German reunification to test the theory of precautionary savings and to quantify the importance of self-selection into occupations due to differences in risk aversion. In the presence of self-selection, failing to control for risk aversion in empirical tests of precautionary savings results in a bias that could lead to a false rejection of the theory. We exploit the fact that for individuals from the former German Democratic Republic (GDR) German reunification in 1990 caused an exogenous reassignment of income risks. Our findings suggest that self-selection of risk averse individuals into low-risk occupations is economically important and decreases aggregate precautionary wealth holdings significantly. (JEL classification: D91, E21, J24)

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1 Introduction

Three difficulties beset empirical studies of precautionary savings (Browning and Lusardi, 1996). The first and most important is the possible bias in precautionary savings regressions due to unobserved prudence¹ in the presence of self-selection. In the absence of complete insurance, expected future income shocks lead prudent individuals to build up precautionary savings in order to avoid wide fluctuations in the consumption path. The reaction of savings to expected future shocks is stronger the more risk averse an individual is. Risk aversion influences not only the savings behavior but it can also affect occupational choice in the first place. In particular, risk averse individuals might choose occupations that are associated with less risky income paths, while less risk averse individuals might prefer occupations with higher income risk. Assessing the importance of self-selection of individuals into occupations according to their risk aversion is difficult, since risk aversion is usually unobserved. Yet, failing to control for unobserved risk aversion in regressions of wealth on measures of labor income risk leads to a bias if self-selection takes place, since measures of labor income risk are negatively correlated with the error term, which captures the unobserved risk aversion. This could lead to a rejection of the theory of precautionary savings even if individuals act according to the theory.

The second problem of precautionary savings tests is the difficulty of finding a measure of labor income risk that corresponds to the perceived risk of a household. Third, labor income risk must vary enough in the observed sample to identify precautionary savings. The results of existing tests for precautionary savings range from little or no evidence (e.g., Skinner, 1988, Dynan, 1993, Guiso et al., 1992) to substantial evidence (e.g., Carroll and Samwick, 1998, Gourinchas and Parker, 2002).²

¹We use prudence and risk aversion as synonyms, as is often done in the literature. Strictly speaking, it is prudence that matters for precautionary savings. Prudence and risk aversion can be used as synonyms only in the case of a constant relative risk aversion utility function (see section 2).

²Browning and Lusardi (1996) give an excellent overview of the existing empirical literature.

In this paper, we shed light on the empirical relevance of self-selection of risk averse individuals into low-risk occupations. Our approach addresses the three problems of empirical studies of precautionary savings outlined above. We use data from the German Socio-Economic Panel (GSOEP) and take advantage of specific institutional features in Germany to identify an occupation with observably low income risk that differs significantly from risk in other occupations. In our main contribution to the literature, we exploit the natural experiment of German reunification to demonstrate and quantify the effects of self-selection.

We test for the existence of precautionary savings by testing whether civil servants, who face low labor income risk, have significantly lower wealth holdings than individuals in other occupations. The advantage of this approach is that we are able to address the second and third challenges listed above. The coincidence of objective and subjective labor income risk is more likely if the risk is determined by the legal situation, as in the case of civil servants, than if the risk is identified by an econometric analysis of the ex post labor income process. Further, the status of civil servant is associated with many occupations (e.g., teachers, people working in state or federal bureaucracy, judges and prosecution lawyers, the police corps), so that the subsample of civil servants is large enough to allow for empirical tests. To make our results comparable to earlier studies, we also show estimates using a more conventional risk measure (as in Carroll and Samwick, 1998).

We are still confronted with the possibility that the choice of occupation might be endogenous. To address this problem, we exploit the event of German reunification. As shown below, in the former German Democratic Republic (GDR), labor income risk was almost completely absent in any occupation. Moreover, occupational choice was often restricted by political considerations. Finally, German reunification in 1990 was not anticipated by anybody until shortly before the event. Hence, we assume that there was no self-selection of risk averse individuals in the GDR into occupations that would typically have the status of civil servant in the Federal Republic of Germany

(FRG). Yet, after reunification, many individuals in the corresponding occupations were granted the status of civil servant. We conclude that for individuals who chose their occupation in the former GDR before 1990, labor income risk is independent of risk aversion. By comparing wealth holdings of the civil servants among these individuals with wealth holdings of individuals in other occupations after the reunification, we are able to identify the amount of precautionary savings in the absence of self-selection.³

We have two main findings. First, even without controlling for unobserved risk aversion, precautionary wealth accounts for a significant part of total wealth. Second, self-selection seems to matter: The precautionary wealth that we infer from a sample of former GDR households that chose their occupation before reunification amounts to 22% of total wealth, while in the sample of West German households it accounts for 13% of total wealth. This suggests that risk aversion plays an important role in occupational choice. Self-selection of risk averse individuals into low-risk occupations is an economically important phenomenon, reducing the observed amount of precautionary wealth in our sample of German households by 42% in our most conservative estimates.

The next section provides a theoretical framework to demonstrate the empirical implications of the theory of precautionary savings in the presence of self-selection. We review the existing empirical evidence on self-selection in section 3. Next, we describe the German institutional background. Section 5 provides information about the data and the construction of our sample. In the following section we present the empirical results. Section 7 deals with possible alternative interpretations of the results, while section 8 offers sensitivity analyses. The last section concludes.

³Some individuals might have changed their occupation after reunification, thereby introducing some self-selection into the East sample. However, this will make it more difficult for us to find differences between the East and the West samples. If we still find a difference between both samples, this will be a lower bound to the actual effect of self-selection. In section 5.2 we discuss the consequences in some more detail.

2 A theoretical framework

To establish testable implications for our empirical analysis, we derive the consequences of self-selection in a model of precautionary savings. Following Carroll (1992, 1997), we consider an individual that faces a risky labor income path and maximizes the discounted value of future utility from consumption up to period T

$$\max_{\{C_t\}_{t=0}^T} \sum_{t=0}^T \beta^t E_0 \{u(C_t)\} \quad (1)$$

subject to the intertemporal budget constraint

$$X_{t+1} = R(X_t - C_t) + Y_{t+1} \quad (2)$$

as well as a borrowing constraint

$$X_t \geq 0 \quad \forall t \quad (3)$$

where C_t is consumption, X_t is cash on hand at the beginning of the period, such that $X_t - C_t$ is wealth at the end of the period, Y_t is labor income, β is the subjective discount factor, and R is the constant gross interest rate.

Labor income follows the stochastic path $Y_t = P_t \epsilon_t$ with $P_t = G P_{t-1} \eta_t$ and

$$\log \epsilon_t \sim N\left(-\frac{\sigma_\epsilon^2}{2}, \sigma_\epsilon^2\right) \quad \text{and} \quad \log \eta_t \sim N\left(-\frac{\sigma_\eta^2}{2}, \sigma_\eta^2\right)$$

where P_t is the permanent income component, G is the constant gross growth rate of permanent income, ϵ_t is a transitory shock to income, and η_t is a permanent shock.

The one period felicity function is of the constant relative risk aversion form

$$u(C_t) = \frac{C_t^{1-\gamma}}{1-\gamma} \quad (4)$$

where γ is the coefficient of relative risk aversion. Under this specific functional form, the degree of prudence is equal to $1 + \gamma$.

A closed-form solution of this problem does not exist; hence, we solve it numerically. In a Bellman equation formulation, the problem becomes

$$V_t(X_t, P_t) = \max_{C_t} \left\{ \frac{C_t^{1-\gamma}}{1-\gamma} + \beta E_t [V_{t+1}(X_{t+1}, P_{t+1})] \right\}$$

subject to (2) and (3).

To solve the model, we let a model period represent one year and set the interest rate equal to 4%, the discount factor equal to $\beta = R^{-1} = 0.9615$, and the gross growth rate of permanent income equal to $G = 1$, such that under certainty equivalence and without facing a borrowing constraint the individual would opt for a flat consumption path, and average life cycle wealth holdings would be zero.⁴

The model is solved for two different types of individuals, one with a higher risk aversion of $\gamma^h = 3$ and the second with a lower risk aversion of $\gamma^l = 2$. Moreover, there are two types of jobs. One job is associated with relatively low risk. We set the variance of this job $\sigma_\epsilon^{2,l} = \sigma_\eta^{2,l} = 0.0095$, slightly lower than the calibration by Carroll and Samwick (1997). The other job is associated with higher risk. For the high risk job, we set $\sigma_\epsilon^{2,h} = \sigma_\eta^{2,h} = 0.013$. For both jobs, expected income in every period equals the starting value of permanent income P_1 . Given free occupational choice, no individual would choose the high risk job unless there were some compensating variation in income; hence, we set $P_1^l = 1$ and $P_1^h = 1.05$.

A life consists of 45 periods, symbolizing the working life from age 20 to age 65, and we abstract from a retirement period. After having solved the model, we simulate 2,000 life cycle paths, assuming that all agents start life with zero wealth and permanent income equal to P_1^l or P_1^h respectively. Figure 1 shows average wealth paths over the life cycle, depicting wealth divided by permanent income. The left plot contrasts the wealth paths for individuals with a high degree of relative risk aversion γ^h in the two different jobs, while the right plot contrasts the wealth paths of both types of individuals in the job associated with lower risk and lower income.

⁴The results remain qualitatively unchanged if we set $G > 1$.

The main features of the wealth paths are the same in both plots. Agents accumulate wealth during the first 28 periods of the life cycle and then start to decrease their wealth holdings. In the last period, wealth holdings reach zero. As the left panel of figure 1 shows, the target level of wealth is increasing in income uncertainty. A higher variance of the income shock increases average wealth to permanent income in every period of the life cycle. The right plot, on the other hand, shows that, confronted with the same income processes, individuals with a higher coefficient of relative risk aversion accumulate more wealth than their counterparts with a lower coefficient of relative risk aversion.

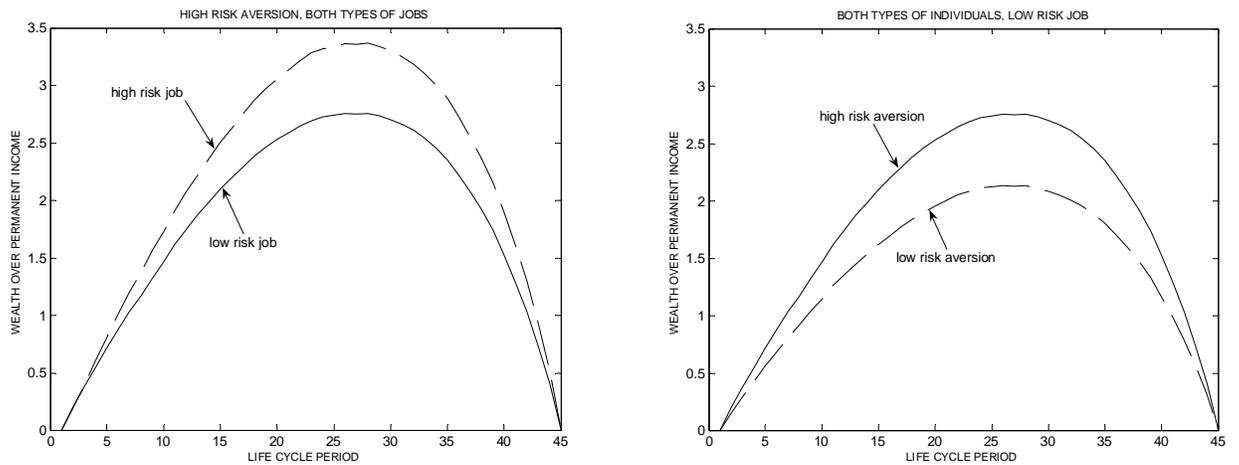


Figure 1: Averages of wealth to permanent income ratios of 2,000 simulations over the life cycle

Now suppose that there is one economy with both types of individuals and both types of jobs, and individuals are allowed to choose a job at the beginning of the life cycle. Under the current calibration, the expected utility for the individual with the lower degree of risk aversion is higher in the job with higher risk, while the expected utility for the individual with the higher degree of risk aversion is higher in the job with lower risk. Hence, individuals choose to self-select into different occupations based on their risk preferences.

Consider the simulations in figure 2, which show average wealth over permanent income if both types of individuals are allocated to equal parts in both types of jobs (left plot), and if the individuals self-select into jobs according to their risk aversion (right plot). In the left plot the distribution of risk aversion is the same for both jobs, while in the right plot all individuals with higher risk aversion are in the low risk job, and vice versa. Implicitly, empirical analyses of precautionary savings that cannot control for risk aversion rely on the underlying assumption that either risk aversion is homogeneous or the distribution of risk aversion is the same in all jobs. This is the case in the left plot, and as expected the wealth holdings are increasing in the riskiness of the job. However, this does not have to be the case if self-selection takes place. Under the current calibration, the average wealth to permanent income ratios over the life cycle of individuals in the high risk job are almost indistinguishable from those in low risk jobs (see right plot of figure 2). While it is a result of the calibration that this exact outcome arises, it generally holds true that self-selection induces a bias against finding significant wealth differences according to income risk differences, if the econometrician cannot control for risk aversion.

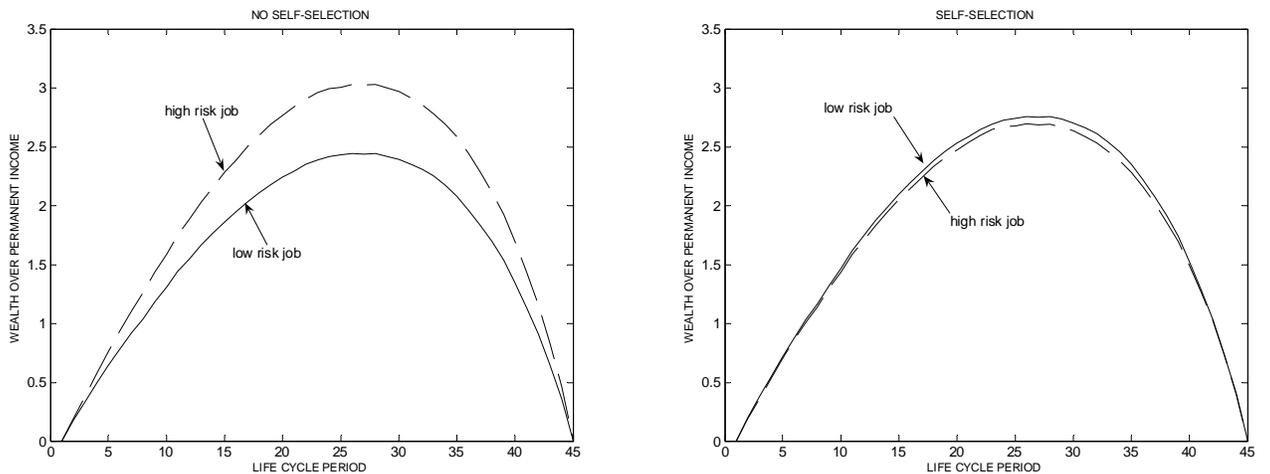


Figure 2: Averages of wealth to permanent income ratios of 2,000 simulations over the life cycle in the absence and presence of self-selection

3 Existing empirical evidence for self-selection

In the only study that explicitly addresses the self-selection bias while testing for precautionary savings, Lusardi (1997) employs years of experience and regional unemployment rates as instruments for subjective income risk. While a simple OLS-regression suggests that precautionary savings account for only 2% of overall savings, instrumental variable regressions point toward 20-24%. Lusardi concludes that, although part of this difference might be attributed to measurement error, part is probably due to the self-selection bias. Dynan (1993) estimates a very small degree of prudence in a consumption growth estimation, instrumenting consumption risk with occupation and other variables. She tests the overidentifying restrictions to assess whether self-selection drives this result, but she cannot reject the restrictions, concluding that self-selection does not play an important role. On the other hand, Carroll et al. (2003) find that occupation is not a valid instrument for unemployment risk in a precautionary savings regression, a finding that they attribute to potential self-selection.

Only a few recent studies directly test whether risk aversion influences occupational choice. Cramer et al. (2002) determine the degree of risk aversion in a sample of Dutch individuals based on a lottery question. They analyze whether this measure of risk aversion helps to explain the occupational status of individuals, namely, being self-employed or not. They find that risk aversion significantly influences occupational choice. Guiso and Paiella (2001) also find in a sample of Italian households that risk aversion, again deduced from a lottery question, significantly influences the choice to become self-employed. Moreover, they find that risk averse individuals are more likely than risk loving or risk neutral individuals to become public sector employees and that among risk averse individuals an increase in risk aversion leads to a higher probability of becoming a public sector employee. Guiso et al. (2002) regress different measures of labor income risk on risk aversion and find that risk aversion,

captured by a dummy, negatively influences the variance of earnings. Moreover, risk averse individuals choose occupations where large negative income events occur with a relatively low probability. They conclude that “the self-selection effect cannot be easily dismissed in empirical studies [of precautionary savings].”

4 Institutional background

Our analysis relies crucially on two identifying assumptions. First, we argue that civil servants face significantly lower labor income risk than non-civil servants. Second, we reason that self-selection into occupations according to risk aversion can be excluded to a large extent for individuals who chose their jobs in the former GDR. In this section, we describe the features of the institutional environments on which we base our claims.

4.1 Civil service

The privileges and duties of German civil servants are regulated by federal and state laws on civil servants (*Beamten-gesetze*), while categories for salaries are defined in another law (*Besoldungsgesetze*). The actual salaries are determined by the states or the federal government, respectively. Salaries depend on the classification of the position that a civil servant occupies and on her age and family situation. Federal law states that a civil servant can only be transferred to a new position if her wage does not decline due to the transfer. A civil servant can only be dismissed if she is sentenced to at least one year in prison for any criminal charge or if she is sentenced to six months in prison for charges associated with treason. Unemployment has become a fairly severe problem in Germany since the beginning of the 1990s. From 1992 to 2001, unemployment rates averaged 9.8%. The average duration of unemployment spells was around 11 months. One third of the unemployed were unemployed for more than a year during this period. From 1997 to 2001, of those unemployed for more than a year, 50% were 50 years or older (Bundesanstalt für Arbeit). Although

unemployment insurance payments are more generous in amount and duration in Germany than, for example, in the United States, long unemployment spells still lead to significant income reductions.⁵ For the older unemployed, it is especially hard to become employed again after long spells of unemployment, and as a result they often end up entering early retirement. Unemployment in these cases reduces lifetime income not only by the difference between labor income and unemployment insurance payments, but also by the lost retirement income due to a much shortened employment spell. The monthly unemployment incidence averaged 0.45% from 1984 to 1995 in West Germany (Gangl, 2001).⁶ One can conclude that unemployment risk constitutes a significant labor income risk in Germany. Consequently, due to life tenure, the labor income risk of a civil servant is much smaller than the labor income risk in other professions.

The nominal salary of a civil servant is completely sticky downward. Moreover, the variations of the base salaries of civil servants (disregarding variations based on the family situation) are relatively narrow. All positions belong to one of four categories, and crossing into a higher category is difficult, often requiring further formal education (e.g., obtaining a university degree). Within the categories, there are subcategories for which the upper and lower salary boundaries are defined by decree. Because an individual can assess from the beginning of her civil servant career which categories she can hope to achieve in her life given a certain educational level, and because she can check the associated current salaries in public tables, a civil servant can predict

⁵An unemployed fulfilling certain requirements receives *Arbeitslosengeld* (“unemployment money”), currently amounting to 60% of the last net income (67% with children). *Arbeitslosengeld* is paid for a certain amount of time from at least 180 days up to 960 days, depending on age and contribution history. After this time, an unemployed person can receive *Arbeitslosenhilfe* (“unemployment support”), which pays lower amounts (currently 53% of the last net income or 57% with children) and requires a means test, incorporating wealth and incomes of other household members. In September 2001, 44% of the unemployed received *Arbeitslosengeld*, and 40% *Arbeitslosenhilfe*.

⁶This number compares to an average monthly unemployment incidence of 0.86% in the same period in the United States. Gangl (2001) computes incidence from GSOEP and SIPP data, respectively. Unemployment incidence was falling in the United States over this period, such that by the mid-1990s both incidence rates for Germany and the United States lay between 0.5% and 0.6%.

her future income path better than individuals in other professions. Nominal salaries of civil servants rise on average at a rate similar to nominal salaries of non-civil servants. Therefore, low nominal wage risk translates into low real wage risk.⁷ In addition to being able to predict their future incomes better than non-civil servants due to institutional reasons, civil servants also face lower income variations than non-civil servants (see section 7.2).

Our conclusion that civil servants face lower income risk than individuals in other occupations is only valid if civil servants do not opt out of the civil service frequently. While they cannot be dismissed, civil servants can still choose to end their contract and enter another occupation. Since they do so voluntarily, this decision might be anticipated, and hence the individual income risk that a civil servant who contemplates leaving civil service faces is not necessarily lower than the individual income risk of a person in another occupation. In our data set, we estimate that 6.1% of civil servants leave civil service during the years 1992 to 2000.⁸ This number is lower than for any other occupation. During that same period, 13.6% of white collar workers, 18.7% of blue collar workers, and 12.1% of self-employed change occupation. We conclude that only a small percentage of civil servants opt out of civil service, and, hence, our identifying assumption that civil servants face lower individual lifetime income risk is valid.^{9,10}

⁷In our sample, the average nominal salaries of non-civil servants rose by 26% from 1992 to 2000, while the average nominal salaries of civil servants rose by 27%.

⁸We concentrate on individuals older than 30 years, as in our sample restrictions used later.

⁹In appendix B, we talk about differences in pension regulations for civil servants and non-civil servants and argue that these differences do not drive the self-selection results.

¹⁰There is no evidence that civil servants experience larger income growth than individuals in other professions, which might have given a separate incentive to save less and could have led to self-selection according to time preferences. In a fixed effects regression of the change in the logarithm of income on age, tenure, hours worked (and higher-order terms of these three variables), change in hours worked, family composition, marital status, education, year dummies, a residence dummy, and a civil servant dummy, the civil servant dummy is close to zero and insignificant (p-value 0.874).

4.2 Occupational choice and occupational characteristics in the former GDR

Occupational choice in the former GDR was to a certain extent restricted. In practice, the most obvious intervention in occupational choice occurred in university admission. Only a certain quota of students was allowed to complete the last two years of high school, which were necessary to attend university. Additional criteria besides qualification were membership in the official GDR youth organization (*FDJ*), political opinion in accordance with official government positions, and family background. Children from working class families were given priority in attending university (Hille, 1991). The income inequality in the GDR was very small compared to the Federal Republic of Germany (FRG). In 1988, the average net income of individuals with a university degree was only 15% higher than that of blue collar workers, compared to 70% in the FRG. Also, intersectoral differences in net incomes were minimal, on average amounting to only 150 Mark per month with an average monthly income of around 1,100 Mark in 1988 (Stephan and Wiedemann, 1990, Schäffgen, 1998). Wage inequality as measured by the Gini coefficient was very low but rose rapidly after reunification from a Gini of 0.17 in 1990 to 0.19 for male and 0.2 for female workers in 1994 (Steiner and Puhani, 1997). From 1960 on, official labor market statistics of the GDR do not show any unemployment. It appears that the government quickly found a new job for anybody who might have been displaced in order to achieve the goal of full employment spelled out in the constitution (Rytlewski and Opp de Hipt, 1982). We draw three conclusions that corroborate our hypothesis that self-selection due to risk aversion was absent in the GDR and that we can therefore treat occupation as an exogenous variable.¹¹ First, the income path was very predictable and income shocks were small in all occupations. Second, job security was constitutionally guaranteed; and, third, occupational choice in the GDR was limited.

¹¹An underlying assumption is that individuals either did not self-select according to political risk, or that political risk is not correlated with income risk after reunification. Some evidence for this assumption is provided in appendix C.

The German Unification Treaty of 1990 extended the FRG system of civil service to East Germany without major changes. It was acknowledged that it would be infeasible to dismiss all former GDR public employees (i.e., individuals working in positions that were typically taken by civil servants in the FRG) and replace them with new personnel. On the other hand, the GDR public system was oversized, and it seemed necessary to replace some individuals due to lack of qualification or due to their past involvement in oppressive political acts. The treaty allowed for dismissal of a public employee if there was no longer a need for the employee's services. Moreover, it was possible to close entire administrative divisions and dismiss their personnel. Finally, an employee could be laid off if she had violated the principles of humanity or rule of law as defined under the provisions of the Universal Declaration of Human Rights or if she had been active on behalf of the Ministry of State Security (*Stasi*). Individuals who remained in their position underwent an individual review after a three-year probationary period before achieving the lifetime status of civil servant. If the review was positive, the person was granted the full status of civil servant, including all privileges (Quint, 1997).

5 The data

5.1 The German Socio-Economic Panel

The data comes from the German Socio-Economic Panel (GSOEP).¹² This annual panel survey was started in 1984 and initially included only West German households. From 1990 on, the survey also covers the territory of the former German Democratic Republic. We use the survey rounds from 1992 to 2000 to construct a measure of permanent income and the survey rounds from 1998 to 2000 for the main analysis. We start with 1998 for several reasons. First, in 1998 a refreshment sample was added that significantly increased the number of observations. Second, in the East sample,

¹²We use the 95% research sample of the GSOEP. A detailed description of the survey can be found in SOEP Group (2001).

wealth effects that differ between civil servants and other occupational groups can only be expected some time after reunification, since wealth is not a jump variable. Third, the review process before granting the civil servant status to an East German took several years.

The wealth data in the survey are recorded at the level of the household. For households where more than one person is earning an income, we determine who is the main income earner in the household (i.e., who has the highest gross monthly income) and associate her occupation and other personal characteristics with the household.¹³

We restrict our sample as follows: We drop the subsamples that focus exclusively on foreigners and migrants, because these groups might have different savings motives (e.g., due to return migration or transfers home). Further, we focus on labor force participants and exclude households whose main income earner is retired but include households whose main income earner is unemployed. We eliminate households whose main income earner is self-employed. The self-employed are not required to contribute to the compulsory pension system and might choose to accumulate retirement savings in private funds. The survey is also not clear on whether accumulation of business capital should be included in the wealth measures. To validate the results, we include the self-employed in one of the sensitivity analyses in section 8. Further, we also drop households whose main income earner is serving an apprenticeship. Finally, we focus on households whose main income earner is 55 years or younger at the time of the survey in order to avoid issues raised by self-selection into early retirement.

Household net monthly income is reported directly in the survey.¹⁴ We con-

¹³In 71% of all observations the main income earner is also recorded as the “head of the household” and in 95% she is either the “head of the household” or the “spouse of the head” (including the “life partner”). The average gross personal income of main income earners is 1.6 times higher than the average gross personal income of non main income earners, conditional on non main income earners receiving a positive income.

¹⁴The question regarding household income reads: “If you take a look at the total income from all members of the household: how high is the monthly household income today? Please state the net monthly income, which means after deductions for taxes and social security. Please include regular

struct a measure of financial wealth from information about interest and dividend income. The exact wording of the question, as well as the calculation of the financial wealth measure, are described in appendix A.1. Appendix A.3 confirms the validity of the data constructed from GSOEP by comparing it with data from the German Income and Expenditure Survey. The financial wealth measure is left-censored at zero. GSOEP does not provide measures of home equity but does provide information on home ownership. It can be argued that housing wealth serves purposes of retirement wealth more than purposes of precautionary wealth, since it cannot be liquidated easily. Many empirical tests of precautionary savings concentrate exclusively on financial wealth (e.g., Kazarosian, 1997, Engen and Gruber, 2001) or use financial wealth as well as total net worth as dependent variables (e.g., Carroll and Samwick, 1998). We construct a measure of housing wealth (see appendix A.2) and confirm the results with a more comprehensive wealth measure in the robustness checks in section 8.

Nominal variables are inflated to year 2000 values. Tables 8 and 9 in appendix D show summary statistics for the variables of interest.¹⁵

5.2 Construction of the “East” and “West” samples

For the self-selection test, we split the sample into an “East” sample and a “West” sample, where East and West refer to the place where households lived *before* the reunification and, in particular, where they made their occupational choices. To be included in the East sample, individuals have to fulfill one of two criteria. First, we include individuals from the “GDR-sample” that was added to the GSOEP in 1990, if they are more than 30 years old in 2000. These individuals were at least 20 years old at the time of reunification, and we argue that they had already made their occupational choice at this time. People in the GDR graduated from high school

income such as pensions, housing allowance, child allowance, grants for higher education, support payments etc.”

¹⁵Due to different approaches toward wealth observations of value zero (as will be explained in section 6) we work with different samples. We report summary statistics for the sample as it is used in the first set of wealth regressions (table 1).

between the ages of 16 and 18, depending on their final schooling level. Second, we include households from the refreshment samples taken in 1998 and 2000 if the main income earner has a GDR education and is older than 30 years in 2000. The refreshment samples do not report the place of residence before reunification, so we can only infer it indirectly via education. We impose the lower age limit for the same reasons as for the “GDR-sample”. We also need an upper age limit. It is possible that people finished their education in the GDR before the Berlin Wall was built in 1961 and still emigrated to the West. These individuals should not be included in the East sample. After 1961, emigration was almost impossible. If the emigrants already had a GDR school degree before 1961, they should now be older than 55 years. Hence, since we restrict our sample to main income earners of age 55 and below, we can assume that the individuals in our sample that have a GDR education indeed lived in the GDR until reunification.

Some of the individuals in the East sample might have switched their occupation after reunification. If individuals changed their occupation after reunification, this would induce self-selection in the East sample, which should work against finding a stronger effect of risk on wealth holdings in the East sample.¹⁶ 82.5 percent of main income earners in the West German sample who are employed in 1990 and 2000 have not changed occupation, while the corresponding number in the East sample is 81.0 percent.¹⁷ Hence, it does not seem that the transition in East Germany led to major occupational shifts.

To make results comparable, we also exclude individuals younger than 30 from the West sample. There are 713 civil servant observations in the remaining sample “West”, and 94 civil servant observations in the sample “East”. We report the summary statistics in table 10 in appendix D.

¹⁶Therefore, any difference we might find will rather underestimate the true effect of self-selection. Our results can hence be seen as a lower bound for the true effect of self-selection.

¹⁷Note that the survey in 1990 was conducted before reunification.

6 Empirical results

We start our estimation from the following specification, as in Carroll and Samwick (1998),

$$\log(W) = \beta_0 + \beta_1 risk + \beta_2 \log(P) + \delta'Z + \varepsilon \quad (5)$$

where W is wealth, P is permanent income, and Z is a vector of household characteristics and year dummies. A civil servant dummy, which is equal to one if the main income earner is a civil servant, is included to capture differences in risk. To increase the number of observations in which the main income earner is a civil servant, we pool the data for the three sample years 1998-2000.¹⁸ Following Carroll and Samwick (1998) and Guiso et al. (1992), in a first approach households with zero or negative wealth holdings are eliminated from the sample.¹⁹ While this procedure is commonly used in the empirical literature, it may bias the results if observations with zero or negative wealth are not just due to measurement error. Hence, in an additional analysis in section 6.3, we include all observations with zero wealth and estimate Tobit models left-censored at zero.

We calculate permanent income for every observation using net income data from 1992 to 2000.²⁰ Permanent income represents the component of income that the household would earn in the absence of idiosyncratic shocks. Splitting observed income into permanent and temporary income obviously introduces measurement error, especially for households that we only observe for a few years. Therefore, we instrument permanent income, using education dummies and interaction terms of education

¹⁸Estimates are very similar, although somewhat less precise due to the smaller sample size, if we use data for the year 2000 alone, which is the single year with the largest number of observations. Results are available from the authors upon request.

¹⁹Carroll and Samwick (1998) and Guiso et al. (1992) exclude households with negative wealth. Our data does not allow us to distinguish between zero and negative wealth holdings.

²⁰We calculate permanent income as follows: We detrend total non-capital family income by dividing it through the average income of all households in the corresponding survey year. Next, we calculate the average detrended household income for every household over all available observation years starting in 1992. Permanent income equals the product of this average detrended household income with the average income of all households within each survey year.

with age and age squared as instruments, and estimate 2SLS models.²¹

As argued above, self-selection in occupational choice should be absent for households from the former GDR. Hence, we do the analysis separately for the East and West samples. For both subsamples, we present two specifications. The second specification includes an indicator variable for home ownership. We control for other wealth accumulation motives, especially savings for retirement, bequests, and children’s education. We also employ a dummy variable for current residence that takes on the value of 1 if the household lives in western Germany in the observation year. The omitted marital status is single or widowed. Year dummies are included but not reported.

6.1 Demonstrating the self-selection bias: Using the reunification “experiment”

Table 1 shows the regression results separately for the East and West samples. Permanent income, number of adults, and number of children have the same signs and are significant in all specifications in both subsamples. Only the East-West differences between the divorced dummies and the residence dummies are statistically significant. The dummy for residence in western Germany is positive and significant in the West sample, but negative and significant in the East sample. The coefficient on the home ownership dummy always turns out positive and significant.²² The inclusion of the home ownership dummy does not alter the other coefficients in a significant way.

We turn to the results for the civil servant dummy. The coefficient on the civil servant dummy in the East sample is negative and significant (at the 8% significance level). In the West sample, the coefficient is also negative and significant, but only

²¹Included educational variables are college, vocational training, secondary schooling, and intermediate or technical degree with less than or equal to 10 years of schooling. Sargan tests of overidentifying restrictions confirm the validity of the instruments and are reported in the tables.

²²It could therefore be the case that financial wealth serves mainly precautionary purposes, while housing wealth serves as retirement savings or has mainly a consumption value (see Engen and Gruber, 2001).

slightly more than half the size in absolute terms of the coefficient in the East sample.²³ After controlling for other savings motives, civil servants in the West hold significantly less wealth than the rest of the population. This is evidence in favor of the precautionary savings motive.²⁴ However, the difference in wealth holdings between civil servants and the rest of the population is much larger in the East sample than in the West sample. This result suggests that a self-selection bias is present in the West sample. Endogeneity of occupational choice based on risk aversion in the West sample can lead to a higher average risk aversion among civil servants than among the rest of the population. This leads to an upward bias in the coefficient on the civil servant dummy.²⁵

²³We conduct a Chow test of equality of the coefficients on the civil servant dummy in the East and West samples, but it does not reject equality of the coefficients. Equality of the coefficients is rejected for the residence dummy and for the divorced dummy at the 5% significance level.

²⁴It may be objected that pension regulations differ in some aspects between civil servants and non-civil servants, potentially influencing their savings behavior. In appendix B, we describe differences in the pension systems and perform a robustness check. In any event, these differences between civil servants' pensions and other pensions are the same in eastern and western Germany and, hence, should not matter for our reunification "experiment".

²⁵The results are very similar if in addition to our variable of interest - the civil servant dummy - we only include interactions with variables that are statistically significantly different between East and West - namely, the divorce dummy and the residence dummy.

Dep. variable: log(financial wealth)	West Sample		East Sample	
	(i)	(ii)	(iii)	(iv)
log(permanent income)	1.958	1.946	1.819	1.821
	(0.181)	(0.187)	(0.239)	(0.239)
civil servant	-0.128	-0.138	-0.248	-0.249
	(0.069)	(0.068)	(0.143)	(0.143)
age	-0.059	-0.061	-0.087	-0.090
	(0.039)	(0.039)	(0.049)	(0.049)
age squared ($\cdot 10^2$)	0.074	0.075	0.111	0.114
	(0.046)	(0.046)	(0.057)	(0.057)
sex (1=male)	0.040	0.035	0.172	0.171
	(0.051)	(0.051)	(0.054)	(0.054)
married	-0.306	-0.316	-0.199	-0.209
	(0.069)	(0.068)	(0.095)	(0.094)
divorced	-0.335	-0.324	-0.052	-0.051
	(0.076)	(0.077)	(0.095)	(0.095)
adults (age>16)	-0.220	-0.226	-0.233	-0.238
	(0.034)	(0.033)	(0.042)	(0.042)
children	-0.100	-0.104	-0.168	-0.170
	(0.022)	(0.022)	(0.035)	(0.035)
current residence (1=west)	0.413	0.407	-0.209	-0.201
	(0.158)	(0.157)	(0.095)	(0.096)
home ownership		0.096		0.059
		(0.052)		(0.054)
constant	-5.395	-5.262	-3.356	-3.310
	(1.653)	(1.704)	(2.120)	(2.121)
year dummies	yes	yes	yes	yes
observations	5532	5532	2510	2510
R ²	0.100	0.103	0.130	0.131
p-value of overidentification test	0.509	0.465	0.480	0.498
p-value of Chow test for civil servant			0.448	0.480

Notes: Results from 2SLS regressions. Observations are pooled for years 1998-2000. Instruments used for permanent income: education dummies and interaction terms of education with age and age squared. Standard errors are in parentheses and are corrected for pooling. Omitted category is single/widowed.

Table 1: Wealth regressions, West and East samples

6.2 Quantifying the size of precautionary wealth and the importance of self-selection

We go through a simulation to approximate the amount of precautionary wealth as a percentage of overall wealth holdings.²⁶ Based on the estimated parameters of the regressions above, we construct the mean predicted wealth of all non-civil servant households for the East and the West samples.²⁷ In a next step, we assume everyone faces the same labor income risk as a civil servant by setting the civil servant dummy equal to one for every household, keeping everything else unchanged. Again using the same estimated coefficients as before, we predict how much wealth non-civil servant households would have accumulated in this counterfactual economy. The difference between predicted wealth and counterfactual wealth divided by predicted wealth is our measure of precautionary wealth. If the main income earner of every non-civil servant household faced the same low income risk as civil servants do, *ceteris paribus*, overall wealth of non-civil servant households would be smaller by the percentage identified as precautionary wealth.

For the East sample, precautionary wealth accounts for 22.1% of all wealth, while for the West sample it accounts for only 12.9%.²⁸ We conclude that precautionary wealth is economically important. Moreover, these numbers show that the economic implications of self-selection are large. Without self-selection, we would observe almost twice the size of precautionary wealth in the West sample.

²⁶Specifically, we analyze by which amount wealth would decline if everyone faced the same risk as individuals in the occupation with the lowest labor income risk. This approach is commonly used in the literature instead of the alternative of setting labor income risk to zero, because estimates are obtained in a region of the data far from zero uncertainty (see e.g. Carroll and Samwick [1998]).

²⁷We use specifications (ii) and (iv) from table 1.

²⁸These numbers imply that precautionary wealth is 5.3 percent of yearly income at the median in the East sample, and 3.7 percent in the West sample. These ratios compare favorably to ratios calculated by Kennickell and Lusardi [2003] based on the Survey of Consumer Finances. They find median values of precautionary wealth to income of 10 percent. The larger value is to be expected since their analysis does not focus exclusively on income risk, but includes all sources of risk, e.g. health risk and longevity risk.

6.3 Including zero wealth observations

In the regression analysis above we followed the common approach in the literature and eliminated observations with zero wealth, which correspond to zero or negative actual wealth holdings. However, these observations are probably non-random, and, hence, their elimination can lead to a bias in the estimation. We now keep all observations with zero wealth and estimate instrumental variable Tobit models (Newey, 1987).²⁹ As above, we instrument permanent income by education dummies and interactions with age and age squared. We report bootstrap standard errors that are corrected for pooling.³⁰ Of the civil servants in the East, 6.9% have zero wealth holdings, while the corresponding number in the West is 3.8%. On the contrary, among the non-civil servants the percentage of zero wealth holders is slightly larger in the West (12.2%) than in the East (11.1%). This already points to a higher degree of risk aversion among civil servants in the West than in the East, inducing them to hold positive wealth.

²⁹Since our dependent variable is the logarithm of wealth, we add one Deutsche Mark to zero wealth observations and estimate Tobit models left-censored at zero. There are 1382 observations with zero wealth.

³⁰We use 250 repetitions to calculate the bootstrap standard errors.

Dep. variable: log(financial wealth)	West Sample		East Sample	
	(i)	(ii)	(iii)	(iv)
log(permanent income)	4.924 (0.415)	4.780 (0.422)	5.711 (0.992)	5.665 (0.671)
civil servant	0.049 (0.156)	0.028 (0.150)	-1.152 (0.375)	-1.147 (0.360)
age	-0.128 (0.091)	-0.134 (0.089)	-0.408 (0.156)	-0.430 (0.157)
age squared ($\cdot 10^2$)	0.120 (0.109)	0.124 (0.107)	0.459 (0.164)	0.484 (0.163)
sex (1=male)	0.090 (0.142)	0.078 (0.141)	0.399 (0.171)	0.384 (0.161)
married	-0.372 (0.166)	-0.393 (0.161)	-0.496 (0.436)	-0.557 (0.344)
divorced	-1.178 (0.197)	-1.139 (0.197)	-0.667 (0.354)	-0.649 (0.354)
adults (age>16)	-0.531 (0.090)	-0.541 (0.087)	-0.636 (0.195)	-0.666 (0.139)
children	-0.265 (0.056)	-0.276 (0.056)	-0.491 (0.109)	-0.500 (0.113)
current residence (1=west)	1.675 (0.510)	1.649 (0.504)	-0.779 (0.308)	-0.720 (0.298)
home ownership		0.345 (0.141)		0.434 (0.196)
constant	-29.875 (3.872)	-28.569 (3.923)	-28.138 (9.180)	-27.338 (6.534)
year dummies	yes	yes	yes	yes
observations	6230	6230	2820	2820
log likelihood	-5394.2	-5414.1	-3264.1	-3305.4
p-value of Chow test for civil servant			0.003	0.003

Notes: Results from instrumental variable Tobit regressions (total coefficients are shown, not marginal effects). Observations are pooled for years 1998-2000. Instruments used for permanent income: education dummies and interaction terms of education with age and age squared. Bootstrap standard errors are in parentheses (using 250 repetitions) and are corrected for pooling. Omitted category is single/widowed.

Table 2: IV-Tobit wealth regressions

We get even stronger evidence for self-selection than in the 2SLS estimations (see table 2). The coefficient on the civil servant dummy is positive but insignificant in the West sample, but negative and significant at the 1% significance level in the East sample. Self-selection seems to counterbalance the precautionary savings motive in the West sample. However, in the East sample, we can detect a strong precautionary savings motive.³¹

Based on the Tobit estimates, the quantitative importance of the bias is even more striking. We redo the quantification exercise in section 6.2 with the results from these parameter estimates.³² In the West sample none of the wealth holdings are due to precautionary reasons, while in the East sample, precautionary savings account for 68% of overall wealth holdings. A Chow test rejects the equality of the coefficients on the civil servant dummies in the East and West samples at a significance level of 1%. Hence, the difference between East and West is not only economically significant but also statistically significant.³³

6.4 Using a conventional risk measure

We see the use of the civil servant dummy to capture subjective income risk as one of the major advantages of our study. However, in order to compare our study to other empirical investigations of precautionary savings, one might be interested in results relying on a commonly used risk measure. Moreover, the number of civil servants in the East sample is relatively small, and hence the use of another risk measure could validate our results. In this section, we use the same risk measure used by Carroll and Samwick (1998), namely, the logarithm of the variance of the logarithm of income for 15 different educational and occupational groups. This is a cross-sectional risk

³¹Again, the results are very similar if we interact only the civil servant dummy and the residence dummy.

³²We predict the latent variable, not the observed variable, in this exercise, since we are interested in the differences in actual, not observed, wealth holdings.

³³Of the other variables, the Chow test only rejects equality of the coefficients for the residence dummy.

measure.³⁴ We then estimate 2SLS estimations excluding zero wealth observations, as they do. Carroll and Samwick (1998) show in simulations that the relationship between this risk measure and the target wealth to permanent income ratio is close to linear.

Table 3 reports the results from 2SLS estimations using this new risk measure instead of the civil servant dummy. Note that the theoretical predictions for the sign of the new risk measure are opposite from the ones for the civil servant dummy, because the civil servant dummy is equal to one if income risk is low. The theory of precautionary savings predicts a positive coefficient on the new risk measure, and self-selection introduces a downward bias in the coefficient. The positive and significant coefficient on the logarithm of the variance of the logarithm of income indicates that wealth holdings are increasing in the income risk a household faces. This positive effect is much stronger in the East sample than in the West sample, resulting in a coefficient of almost double the size in the East sample.³⁵ This gives evidence for self-selection, which induces a downward bias on the coefficient in the West sample.³⁶ Hence, this exercise confirms the earlier results.

³⁴To construct this risk measure, we use data from 1992 to 2000. We exclude the self-employed from our analysis. We use three occupations (civil servants, white collar workers, and blue collar workers), and five education levels (college, vocational training, intermediate/technical schooling, secondary schooling, secondary schooling not completed). One difference between our risk measure and the one used by Carroll and Samwick (1998) is that they additionally consider sector groups. However, data on sectors is not consistently available in GSOEP over the 1990s.

³⁵As in the 2SLS regressions using the civil servant dummy, a Chow test does not reject equality of the coefficients on the risk measure in the East and West sample.

³⁶The coefficient of 0.395 in the West sample is very close to the coefficient Carroll and Samwick (1998) find in their regression of financial wealth on this risk measure using data from the Panel Study of Income Dynamics, namely 0.368.

Dep. variable: log(financial wealth)	West Sample	East Sample
log(permanent income)	1.873 (0.171)	1.581 (0.239)
log variance (log income)	0.395 (0.130)	0.778 (0.305)
age	-0.059 (0.039)	-0.086 (0.049)
age squared ($\cdot 10^2$)	0.072 (0.046)	0.109 (0.057)
sex (1=male)	0.059 (0.052)	0.206 (0.057)
married	-0.301 (0.067)	-0.175 (0.093)
divorced	-0.327 (0.077)	-0.047 (0.094)
adults (age>16)	-0.218 (0.032)	-0.204 (0.042)
children	-0.101 (0.022)	-0.154 (0.034)
current residence (1=west)	0.418 (0.159)	-0.185 (0.093)
home ownership	0.111 (0.051)	0.076 (0.053)
constant	-4.078 (1.610)	-0.230 (2.288)
year dummies	yes	yes
observations	5472	2468
R ²	0.111	0.141
p-value of overidentification test	0.318	0.556
p-value of Chow test for risk measure		0.249

Notes: Results from 2SLS regressions. Instruments used for permanent income: education dummies and interaction terms of education with age and age squared. Observations are pooled for years 1998-2000. Standard errors are in parentheses and are corrected for pooling. Omitted category is single/widowed.

Table 3: Regressions with conventional risk measure

7 Can alternative interpretations explain the results?

Our analysis relies on the implicit assumption that other factors potentially influencing differences in wealth holdings between civil servants and non-civil servants are equal between East and West Germans. In this section, we discuss and address the most important of these factors. First, wealth differences caused by reasons other than precautionary savings but correlated with a precautionary savings motive could have existed in the East already at the time of reunification. Second, the risk differences between civil servants and non-civil servants could differ between East and West. Third, even given the same risk differences, other factors than self-selection could cause larger wealth differences in the East than in the West. In the last subsection, we show some independent suggestive evidence for self-selection by analyzing smoking behavior of civil servants and non-civil servants in East and West.

7.1 Initial wealth differences in the East sample

If the wealth holdings of civil servants and non-civil servants did not differ significantly in the East in the early 1990s, it is more likely that the differences found in the late 1990s are indeed due to precautionary reasons. To confirm our interpretation of the results, we hence show estimates for the East sample from the early 1990s. For this analysis, we assign the civil service status over all years to any household that is a civil servant household in 2000. We then reestimate our baseline regressions in the East sample for the years 1992 to 1994, and 1998 to 2000.³⁷ We start in 1992 instead of immediately after reunification, since this is the first year for which the underlying assumptions in our construction of financial wealth are reasonably met. Of course, at this point in time, a small wealth difference between civil servants and non-civil servants due to precautionary reasons could already have been built up. In table 4, we

³⁷Note that, due to the new definition of a civil service household, the results change slightly from the previous results for the 1998 to 2000 sample.

show the resulting coefficients on the civil servant dummy from estimations including the usual control variables.

Dep. variable: log(financial wealth)		
	1992-1994	1998-2000
2SLS	-0.149	-0.242
(excluding zero wealth obs.)	(0.119)	(0.145)
IV-Tobit	-0.134	-1.025
(including zero wealth obs.)	(0.259)	(0.384)

Notes: Reported is the coefficient on the (future) civil servant dummy in the East sample (for details see the text); Standard errors are in parentheses and are corrected for pooling.

Table 4: Results from East sample, 1992-1994 vs. 1998-2000

In the 1992-1994 sample, (future) civil servant households hold less wealth than the rest of the population. However, this wealth difference is not significant, neither in the 2SLS estimates nor in the IV-Tobit estimates. In the 2SLS estimates, the coefficient on the civil servant dummy rises from -0.15 in 1992-1994 to -0.24 in 1998-2000. Moreover, it becomes statistically significant over time. A Chow test however does not reject equality of the coefficients. In the IV-Tobit estimates, the coefficient on the civil servant dummy rises almost tenfold from the beginning to the end of the 1990s, and again becomes significant over time. A Chow-test now rejects equality of the coefficients. We conclude that the significant wealth difference between civil servants and non-civil servants in the East sample arose over the course of the 1990s, and did not exist right after reunification.

One problem with this analysis is that the number of (future) civil servants becomes very small if the refreshment samples from 1998 and 2000 cannot be used.³⁸ We therefore repeat and expand this analysis using the conventional risk measure introduced in section 6.4.

We reestimate the regression for the East and West samples as in table 3 for a

³⁸For the 2SLS estimation using data from 1992 to 1994, we have 1,983 observations, including 66 civil servant households, while for the IV-Tobit estimation, the number of observations is 2,186 households including 67 civil servant households.

rolling sample, always pooling three years and starting with the 1992-1994 sample. In figure 3 we show the estimates for the coefficient on the logarithm of the variance of the logarithm of income over time. As expected, the coefficient in the West sample is very stable through the 1990s. However, in the East sample it rises quickly in the early 1990s from an insignificant coefficient of 0.2 in 1992 to 1994, to a significant coefficient of 0.92 in 1996 to 1998. This gives evidence that households in the East sample start building up precautionary savings after reunification. The coefficient falls in subsequent years, which may be due to the peak in unemployment rates in eastern Germany in 1997 and 1998, which may have forced some households to deplete their buffer stock of savings (see Carroll et al., 2003, who for this reason suggest excluding households that have recently experienced an unemployment spell from the estimation).

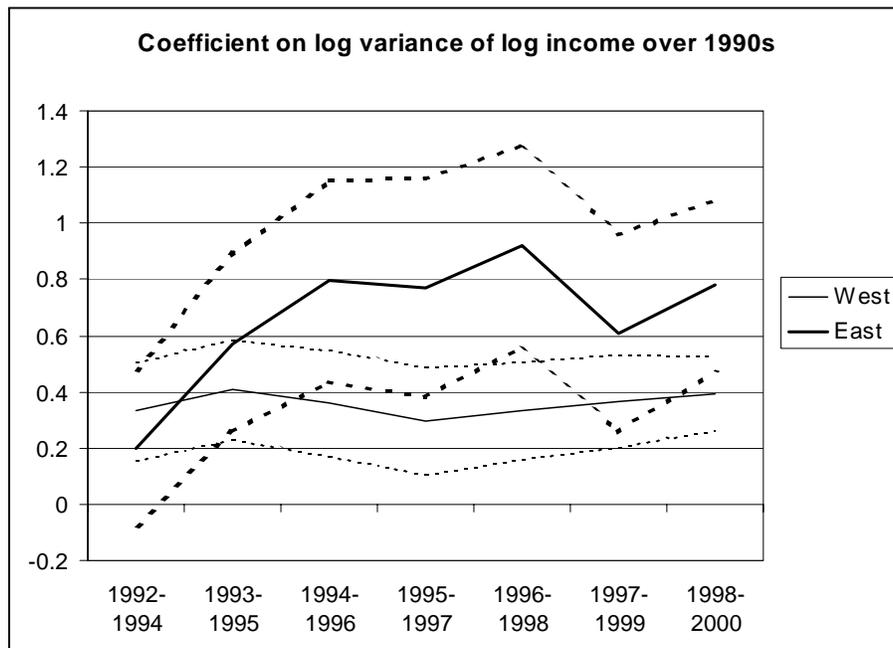


Figure 3: Coefficient on log variance of log income from estimations in East and West samples using a conventional risk measure, 1992-2000; dotted lines represent coefficient +/- one standard error

7.2 East-West differences in labor income risk

As discussed in section 4.1, the civil service system from the West was established without major changes in the East, and hence the income risk of civil servants in the East and in the West should be the same. However, the labor income risk for non-civil servants could differ systematically between East and West.

Although unemployment rates are much higher in the East than in the West, there are several reasons why this does not necessarily translate into risk differences. First, migration between eastern and western Germany is possible and occurs especially from the east to the west in large numbers, such that it is unclear how much the unemployment risk really differs for households from the former GDR and the former FRG.³⁹ Second, German unemployment insurance payments are quite generous. Third, publicly financed work creation programs have been used to a large extent in the East. In 1999, 18.9% of unemployed benefited from these programs (Bonin and Zimmermann, 2001). We did several sensitivity analyses including unemployment rates as controls, and the coefficients on the civil servant dummy are almost unchanged from our baseline regressions in both samples.⁴⁰

More importantly, the wage distribution in the GDR was more compressed than in West Germany before reunification, and while wage dispersion in the East has been rising since reunification, it still has not reached the Western German level (OECD 2001). Biewen (2000) uses the measure of disposable household income employed in this paper and finds that, despite a rise since reunification, inequality remains substantially lower in the East than in the West by 1996.

Table 5 shows the average time series variance and coefficient of variation, respectively, of the unpredictable component of individual household income from 1992 to

³⁹It could of course still be that, due to different human capital, East and West Germans living in the same federal state face different risks.

⁴⁰We included either yearly unemployment rates in the *Bundesland* (federal state) of current residence, or yearly age-specific unemployment rates for the East and the West. Results are available from the authors upon request.

2000, separately for the East and West, and for civil servants and non-civil servants.⁴¹ The predictable component was constructed by regressing the logarithm of income on a cubic function of age, occupation, education, marital status, family composition, interaction terms with age and age squared, and time dummies, separately for East and West Germans. Four clear features emerge. First, both risk measures point toward lower risk in the East than in the West. Second, both risk measures indicate that this holds also true among non-civil servants. Third, civil servants have lower risk than non-civil servants. Finally, the risk difference between civil servants and non-civil servants is smaller in the East than in the West.⁴²

We conclude that these estimates reinforce the assumption that civil servants face lower labor income risk than non-civil servants.⁴³ More importantly, income risk of non-civil servants seems to be *lower* in the East than in the West. Note that unemployment spells are included in the measure of income risk. Last, the income risk difference between non-civil servants and civil servants seems to be larger in the West than in the East. Hence, our estimation results cannot be attributed to larger risk differences between civil servants and non-civil servants in the East than in the West.

⁴¹When splitting the sample into civil servants and non-civil servants, occupation information from the year 2000 is used. The calculations include the self-employed in order to get more realistic estimates of income risk, since some households switched into and out of self-employment. The relative income risks remain unchanged if self-employed are excluded from the calculations (results are available from the authors upon request).

⁴²The first, second, and fourth results also hold if we analyze the variance and the coefficient of variation of income, instead of the unpredictable component of income. The third result then also holds in the West, while the risk of civil servants seems to be larger than that of non-civil servants in the East if we look at income rather than the unpredictable component of income. Results are available from the authors upon request.

⁴³Note, however, that we believe and claim that due to institutional characteristics labor income risk is lower for civil servants than for non-civil servants, even if econometric risk measures relying on an incomplete set of controls would not capture this.

	West	East
variance		
all	0.0795	0.0722
only civil servants	0.0437	0.0667
only non-civil servants	0.0838	0.0724
coefficient of variation		
all	0.0615	0.0585
only civil servants	0.0373	0.0501
only non-civil servants	0.0644	0.0587

Table 5: Time-series variances and coefficients of variation of unpredictable component of income

7.3 Other differences between East and West Germans

Two additional potential differences between East and West Germans could suggest themselves to explain the regression results. The first is that East Germans could on average be more prudent than West Germans. While it is hard to argue about the average degree of prudence, it seems implausible to us that differences between East and West Germans should be big enough to create the resulting wealth differentials. We start our analysis in 1998, by which the immediate reunification process was long over, East Germans had lived under the new regime for eight years, and a reversal to a socialist state seemed highly implausible.

A second possibility to explain bigger wealth differentials in the East for the same risk differences in East and West could be easier access to credit in the West. Against this, we argue that the banking system is the same in the East as in the West. Moreover, average household consumer credit is relatively small in Germany and does not vary much between the East and the West. In 1998, it amounted to 2,300 DM in the East, while in the West it amounted to 2,600 DM (Münnich 2001). Relative to household income, consumer credit is hence larger in the East than in the West. Moreover, a lower percentage of households had consumer credit debt in the West than in the East, namely 16.7% vs. 21.6% (Münnich 2001). While this is not

proof that credit accessibility is the same in the East and in the West, it gives some evidence that, relative to household income, East households have the same access to consumer credit as West households.

On the other hand, one factor suggests *smaller* wealth differences in the East for the same risk differences in East and West. Because households in the East started to differentiate their wealth levels due to their income risk only after 1990, the observed wealth differences should be smaller than those in the West, especially for older households that had a lot of time to accumulate wealth in the West.

Overall, we conclude that, although several factors might suggest larger or smaller wealth differences in the East for the same risk differences in East and West, these reasons are rather weak, and it is not clear in which direction any net effect would go.

7.4 Independent suggestive evidence for self-selection

To corroborate our self-selection hypothesis, we analyze smoking behavior, which one could argue is a measure of risk-taking behavior. For this we can use information about whether an individual has ever been a smoker.⁴⁴ Simple cross-tabulations show that in the West sample civil servants are less likely to ever have smoked than the rest of the population (56.2% of civil servants say they smoke or used to smoke regularly in the past, while in the rest of the population 61.7% smoke or have smoked). The chi-square test rejects independence of these variables at the 10% level (the p-value is 0.063). On the other hand, in the East sample, there is no significant difference between civil servants and the rest of the population: 57.8% of civil servants and 58.5% of non-civil servants smoke or have smoked (the p-value of the chi-square test is 0.918). We also run a probit regression of a dummy variable, which is one if an individual has ever been a smoker, on a civil servant dummy and controls for age,

⁴⁴This information is only available for 1999. To expand the sample size we include here also non main income earners and persons older than 55 (except for East Germans in the refreshment sample) that are not yet retired, but still require individuals to be at least 30 years of age in 2000.

education, income, marital status, sex, family size, and age interactions, separately on the East and West samples. Results show a negative coefficient on the civil servant dummy in the West (coefficient of -0.134 with a standard error of 0.084), and a very imprecisely estimated negative coefficient of half that size in the East (coefficient of -0.064 with a standard error of 0.203). While we cannot reject equality of the coefficients, the results point towards higher risk aversion among civil servants than non civil servants in the West, but the same average risk aversion among both groups in the East.

8 Robustness checks

We report three sensitivity analyses. In the first one, we extend the wealth measure to incorporate housing wealth in addition to financial wealth. We construct a measure of housing wealth that combines information on mortgage payments and the length of the mortgage for households that bought a house but have not yet fully repaid their mortgage. For households that have no housing debt, we use information about imputed rent and house characteristics to impute housing wealth. The details of the procedure are described in appendix A.2, while appendix A.3 checks the validity of the constructed housing wealth measure. The second robustness check focuses on households whose main income earner is older than 35 years because younger individuals from the East were potentially more likely to change occupation after reunification than older individuals. Third, we confirm the results by including the self-employed in the sample. For all robustness checks, we reestimate the 2SLS wealth regressions and the instrumental variable Tobit wealth regressions.⁴⁵

⁴⁵When the dependent variable includes a measure of housing wealth, we omit the home ownership dummy as an explanatory variable. Otherwise, the usual controls are included.

Dependent variable	Sample	Estimator	coefficient on civil servant dummy		p-value of Chow test
			West Sample	East Sample	
log(financial wealth)	baseline	2SLS	-0.138 (0.068) [n=5532]	-0.249 (0.143) [n=2510]	0.480
		IV-Tobit	0.028 (0.150) [n=6230]	-1.147 (0.360) [n=2820]	
log(financial wealth + housing wealth)	baseline	2SLS	0.085 (0.078) [n=5299]	-0.225 (0.222) [n=2350]	0.189
		IV-Tobit	0.200 (0.133) [n=5973]	-1.276 (0.415) [n=2640]	
log(financial wealth)	age > 35	2SLS	-0.151 (0.079) [n=4085]	-0.339 (0.150) [n=1967]	0.266
		IV-Tobit	0.009 (0.177) [n=4604]	-1.361 (0.372) [n=2213]	
log(financial wealth)	including self-employed	2SLS	-0.184 (0.067) [n=6174]	-0.260 (0.142) [n=2738]	0.627
		IV-Tobit	0.014 (0.143) [n=6948]	-1.043 (0.352) [n=3079]	

Notes: Reported is the coefficient on the civil servant dummy from different wealth regressions (for details see the text). Zero wealth observations are excluded in the 2SLS estimations, but included in IV-Tobit estimations. Observations are pooled for years 1998-2000. Standard errors are in parentheses and are corrected for pooling. Numbers of observations are in squared brackets. Other controls included: log(permanent income), age, age squared, sex, married, divorced, number of adults, number of children, dummy for current residence, home ownership (except in the specification with total wealth as dependent variable), year dummies, and a constant. Omitted category is single/widowed. Instruments used for permanent income: education dummies and interaction terms of education with age and age squared.

Table 6: Robustness checks

Results for the coefficient on the civil servant dummy are reported in table 6. Generally, the coefficients on the civil servant dummy are similar to the ones in the baseline results. Standard errors often increase somewhat, because of a larger measurement error in the dependent variable if we add housing wealth, or because of smaller sample sizes if we focus on older households. A Chow test still rejects equality of coefficients between East and West in all Tobit estimations. Note two interesting results. If we focus on households older than 35 years, the East-West differences become larger, which one would expect if younger households in the East self-select into occupations. In contrast, the East-West differences become somewhat smaller if the self-employed are included. Since self-employment was very rare in the East, the self-employed in the East sample probably became self-employed after reunification. Hence, including the self-employed introduces some self-selection in the East sample.

9 Conclusion

We combine particular features of the German civil service system and the unique event of German reunification to present evidence for precautionary savings and to quantify the importance of self-selection into occupations due to differences in risk aversion. Our approach deals with the two principal problems of empirical studies of precautionary savings. First, an institutional feature of the German labor force allows us to identify an occupation group that experiences less income risk than other groups. As we show, German civil servants have an extremely secure job, and their incomes can be predicted fairly well even over a long time horizon. Further, the lower income risk associated with the position of a civil servant compared to other occupations is perceived as such by the German public and individual households because it is based on institutional features.

Second, in our main contribution to the literature, we are able to demonstrate the importance of self-selection of individuals into occupations according to their risk aversion by using the German reunification “experiment”. For East Germans

German reunification caused an exogenous reassignment of income risks to different occupational groups. In particular, we argue that labor income risk is not correlated with risk aversion for individuals who chose their occupation in the former GDR. Hence, we can avoid a self-selection bias for a subsample of our data. Comparing the estimates from this subsample with the estimates from the sample of households in which occupational choice cannot be assumed to be independent of risk aversion, we provide evidence that self-selection is present in the latter households, and we are able to quantify the effects of self-selection. If we consider just the subsample of households that chose their occupation in East Germany before German reunification, our estimates for precautionary wealth amount to 22% of total wealth. This amount is almost twice as large as it is in the West sample. We find that the wealth gap between civil servants and the rest of the population in the East sample is statistically different from this gap in the West sample in wealth regressions that include zero or negative wealth observations. The self-selection results are confirmed using a more conventional risk measure.

We draw the following conclusions from the results in this paper: First, risk-aversion influences the occupational choice of individuals. Second, individuals act according to the theory of precautionary savings. Third, self-selection, if not appropriately controlled for, can lead to a significant underestimation of the relevance of precautionary savings. The self-selection bias might help to explain the extreme differences in results of past empirical studies of precautionary savings.

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A Data appendix

A.1 Construction of the financial wealth measure

Direct wealth measures are not available in GSOEP. We construct a measure of accumulated financial wealth as follows. We use a question concerning income from interest and dividends to infer financial wealth, assuming that the average interest rate on financial assets was 3.3% in 1998, 2.8% in 1999, and 4% in 2000, calculated based on average wealth portfolios and average rates on savings accounts, bonds, and stocks in the respective years (Statistisches Bundesamt, Deutsche Bundesbank, various issues).

The survey question regarding interest and dividends reads: “How high was the income received from interest, dividends, and profits from these savings and securities in the last calendar year?” Some households give an exact amount, while others just indicate one of five given ranges (less than 500 DM, 500 to 2000 DM, 2000 to 5000 DM, 5000 to 10000 DM, more than 10000 DM). For those choosing to indicate a range, we use the mean income of households that actually give the exact amount within this range as a proxy.

A.2 Construction of the housing wealth measure

GSOEP contains information on home ownership but does not state the value of the house or the accumulated amortization payments on a mortgage. We calculate this information as follows.

For people who still repay a mortgage on their home, GSOEP reports the monthly payments of amortization plus interest. To calculate the amortization amount out of this annuity, we make the following assumptions. First, we assume that the overall duration of the mortgage is 30 years. Second, the interest rate on the mortgage is equal to the average interest rate on 10-year fixed mortgages during the period 1971 to 2001, namely 8.25% (according to Rheinische Hypo Bank Frankfurt). Third, we assume that the mortgage is paid back in constant annuities, and, finally, we assume that interest accrues yearly. We take the year the household moved into the current house as the year the mortgage was taken up. However, if a household indicates that it purchased the house it was previously renting, we take the year of the purchase as the year the mortgage was taken up. Given this information, we can calculate the accumulated amount of amortization payments, which corresponds to the wealth that the household accumulated through mortgage payments. Similarly, we can calculate the overall amount of the mortgage. From the value of the mortgage, we calculate the price of the house at purchase, based on the assumption that the down payment amounts to 20% of the purchase price. The housing wealth of the household, evaluated at housing prices at the year of purchase, is the sum of the down payment plus the accumulated amortization payments. We inflate this value to year 2000 values, based

on the year of purchase and the price index for residential buildings provided by the German Statistical Office. Implicitly, we assume that depreciation of the house is offset each year with equivalent investment in the house.

For households that own a house but do not have a mortgage, we impute the value of the house as follows. For all homeowners, we have information about various characteristics of the house and about the rent the household would consider appropriate for living in this house. For those who pay back a mortgage, we can calculate the approximate inflated value of the house as the value of the mortgage plus the downpayment as described above. For these households, we regress the value of the house on various house characteristics, the imputed rent, and interaction terms between characteristics and imputed rent. We get a predicted value of the house from this regression for households that do not pay back any mortgage.

We proceed similarly for non-owner-occupied dwellings. The only difference from owner-occupied housing is that we do not know the year of purchase. We therefore assume that the mortgage is in the median year of the mortgages on owner-occupied housing, corresponding to the eighth year.

Our real wealth measure consists of housing wealth from owner-occupied and non-owner-occupied housing.

A.3 Validation of wealth measures

To check the validity of the constructed wealth data, we recur to comparisons with the *Einkommens- und Verbrauchsstichprobe* (EVS - Income and Expenditure Survey), which is considered to be the best source for recorded household wealth in Germany. The EVS is conducted every five years without a panel component, with the last survey round being 1998. Due to the missing panel component, we cannot construct a measure of permanent income from EVS data. Unfortunately, these data are not available to researchers residing outside Germany. However, the Federal Statistical Office records detailed sample statistics, to which we compare our data. In the 1998 round, EVS interviewed more than 62,000 households, so its sample size is almost five times bigger than the GSOEP sample size. The EVS data is therefore well-suited to check the validity of our data. Note that the EVS reports current residence, but not residence before reunification; hence, in the following comparisons between EVS and GSOEP we use current residence for the sample split. This is another reason why we cannot do our analysis using EVS data but have to recur to GSOEP data. As in GSOEP, in the EVS the relative number of civil servant households in the East is around one third of the relative number in the West. Civil servants compose 14% of the EVS sample in western Germany and 5% in eastern Germany.

Our wealth measure would be especially problematic if the composition of financial wealth differed significantly between civil servants and non-civil servants. In this case, we might systematically bias the estimates of wealth holdings based on different returns earned on different forms of investment. Figures 4 and 5 show the composition

of financial wealth in 1998 for civil servants and the overall population from EVS, separately for western and eastern Germany (Münnich, 2001). Focusing on western Germany, the differences between the wealth composition of civil servants and the overall population are minimal. Civil servants tend to hold a larger share of their portfolio in savings at building societies and a smaller share in riskier securities like stocks, bonds, and other securities. Note that the last result gives a further indication that civil servants might be more risk averse than the rest of the population. Turning to eastern Germany, the differences between the wealth composition of civil servants and the overall population are somewhat bigger. Civil servants tend to hold more wealth in savings at building societies, stocks, and insurance, and less in savings accounts, other bank savings, and bonds.

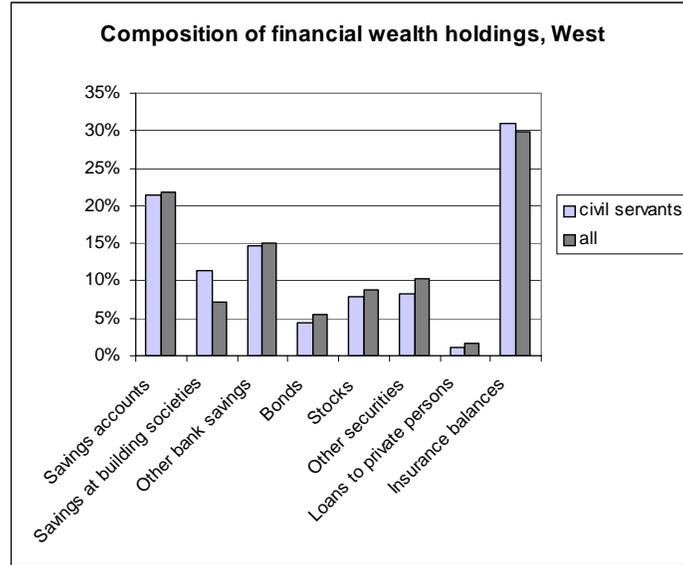


Figure 4: Composition of financial wealth held by civil servants and the whole population in western Germany, based on EVS 1998

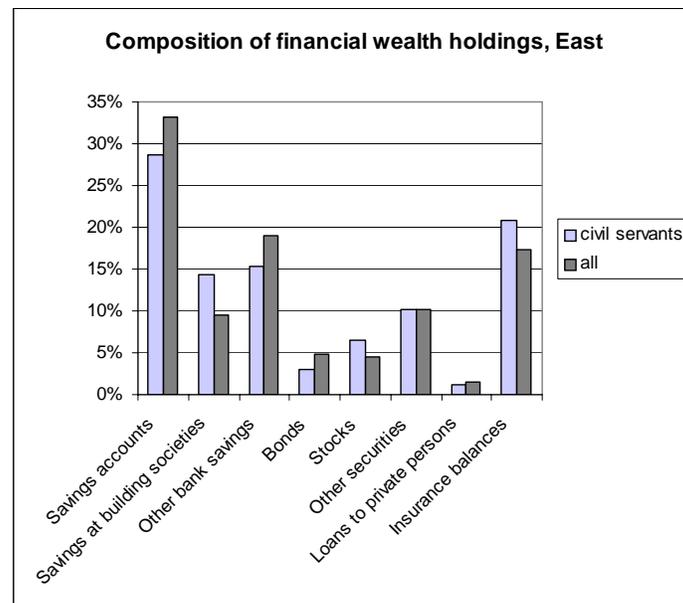


Figure 5: Composition of financial wealth held by civil servants and the whole population in eastern Germany, based on EVS 1998

Next, we compare the amounts of average financial wealth holdings constructed from the GSOEP data in 1998 to the average amounts reported by EVS (Münnich, 2001).⁴⁶ While the EVS reports average gross financial household wealth of 61,200 DM in western Germany and 31,900 DM in eastern Germany, our wealth measure reports averages of 50,500 DM in western Germany and 19,800 DM in eastern Germany, i.e., 71% and 62% of the values from EVS. Hence, it seems that we understate the true amount of financial household wealth by around one third. If we omit wealth invested in life insurances from the financial wealth holdings reported by EVS, the GSOEP data corresponds to 101% in the West and 75% in the East. We are probably missing this component of financial wealth with our wealth measure, since life insurance contracts do not pay yearly interest. Life insurance contracts represent relatively illiquid wealth, and, hence, their omission might be valid in a focus on liquid financial wealth. We are still underrepresenting the amount of wealth held by East Germans.

Figures 6 and 7 show the amount of financial wealth for different occupational groups in western and eastern Germany as reported by EVS, with and without life insurance, and GSOEP. Note that some cell sizes are fairly small, and hence these numbers are less reliable than a comparison of the overall wealth holdings. For western Germany, the constructed GSOEP data are fairly similar to the EVS data without life insurance except that GSOEP reports significantly less financial wealth for the self-employed and unemployed. For East Germany, GSOEP reports significantly higher wealth for the self-employed and lower wealth for white collar workers than EVS, again excluding life insurance. GSOEP and EVS data differ most for the self-employed. It is a well known problem that wealth of the self-employed is difficult to measure; we exclude the self-employed in our main analysis.

For civil servants, the constructed GSOEP data matches the EVS data without life insurance fairly well in East and West. In the West, the wealth of civil servants is slightly underestimated, as is the wealth of white collar workers, blue collar workers, and the unemployed. If we believe that the true wealth holdings correspond to the wealth reported in EVS, we could do the following. For every occupational group, we calculate the multiplication factor that has to be applied to data from GSOEP such that average wealth matches the one reported in the EVS, and we apply this factor to the wealth of each household belonging to the occupational group. The multiplication factor for civil servants would be slightly smaller than for the other groups in the West. Replicating our analysis with this new inflated wealth measure, we would hence find evidence for slightly higher precautionary savings in the west sample than in our analysis using GSOEP data. If we rescale wealth levels for the East similarly, the multiplication factor would be greater than 1 for all groups except civil servants, whose wealth is overestimated by GSOEP. Hence, redoing our analysis with these rescaled data would result in a more negative coefficient on the civil servant

⁴⁶To make the results from GSOEP representative, we use the full sample and cross-sectional weights.

dummy. In conclusion, our evidence for self-selection in the West and its quantitative importance would be even larger than in our main analysis.⁴⁷

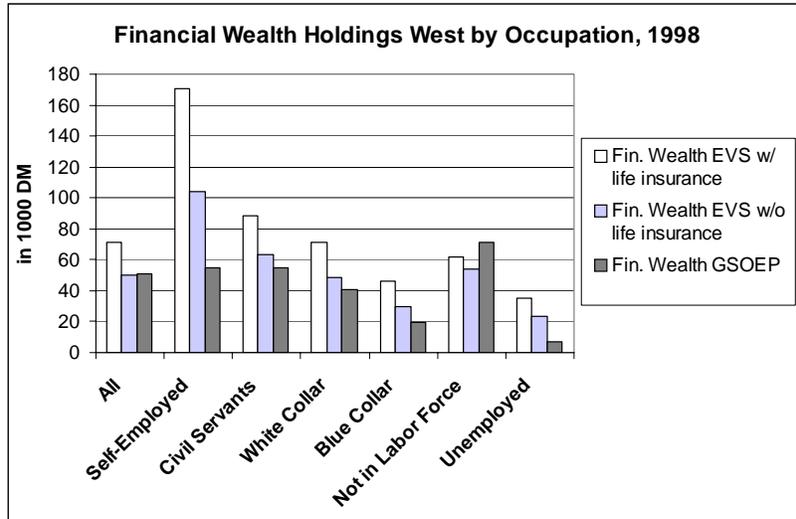


Figure 6: Financial wealth of west German households by occupational groups as reported by EVS and GSOEP

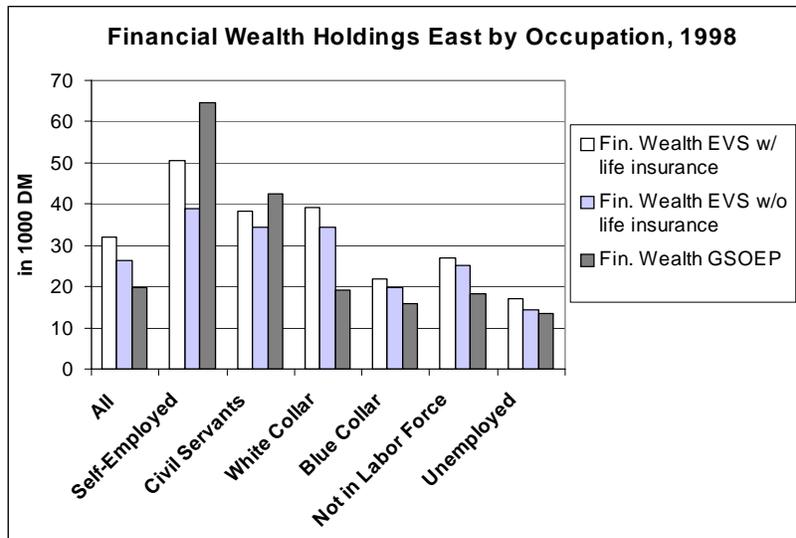


Figure 7: Financial wealth of east German households by occupational groups as reported by EVS and GSOEP

⁴⁷Note that this exercise relies on the assumption that there was no migration after reunification in the EVS sample, since we cannot deduce residence before reunification from the EVS.

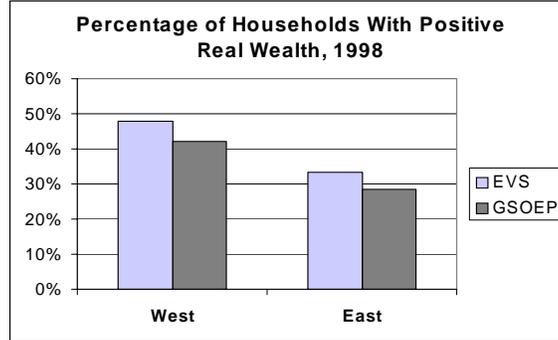


Figure 8: Percentage of households with positive real financial wealth from EVS and GSOEP, 1998, separately for western and eastern Germany

To compare the constructed real wealth measure with data from the EVS, we focus on the average gross value of owner-occupied plus non-owner-occupied housing (i.e. without deducting mortgage debt). As Börsch-Supan et al. (1999) state, the EVS overestimates home ownership. In 1993, EVS reports that 46.7% of the households own the house they are living in, while GSOEP reports the number to be 40%, which is close to the most trusted number of 40.9% that comes from the *Gebäude- und Wohnungsstichprobe* (Building and Apartment Survey). We similarly find that for the 1998 data, EVS reports higher home ownership rates than GSOEP (see figure 8).

To circumvent this inconsistency, we compare average gross house values conditional on possessing housing wealth. The gross average total value of owner and non-owner-occupied housing in 1998 constructed from GSOEP amounts to 391,000 DM in western Germany, and 203,000 DM in eastern Germany.⁴⁸ These compare to 467,000 DM in western Germany and 231,000 DM in eastern Germany in the 1998 EVS (Münnich 1999), i.e. to 83.7% and 87.9% of the EVS data. Münnich (1999) stresses that the amount provided by households in the EVS is supposed to reflect the sales price of the property in the current market, but that very often the impression arises that households are too optimistic in the estimation of their property. The true value of the property might actually lie between the values constructed from GSOEP and those provided by EVS.

Figures 9 and 10 show the average amount of gross real wealth by occupation of the household head, separately for western and eastern Germany.⁴⁹ For western Germany, GSOEP provides lower estimates of real wealth than EVS for every occupation, with the difference being largest for self-employed and civil servants. For eastern Germany, the estimates from GSOEP are higher than those from EVS for households whose head

⁴⁸For the construction of these data, we again use the entire sample and cross-sectional weights.

⁴⁹Münnich (1999) provides data for the self-employed split into farmers and others. We average this data by assuming that 10% of self-employed are farmers.

is not in the labor force. If we were to rescale the GSOEP data per group to match the average EVS data, in the West sample the multiplication factor would be biggest for civil servants, while in the East sample it would be smallest for civil servants. Hence, our evidence for precautionary savings would be weaker in the West sample and stronger in the East sample, corroborating our evidence for self-selection in the West.

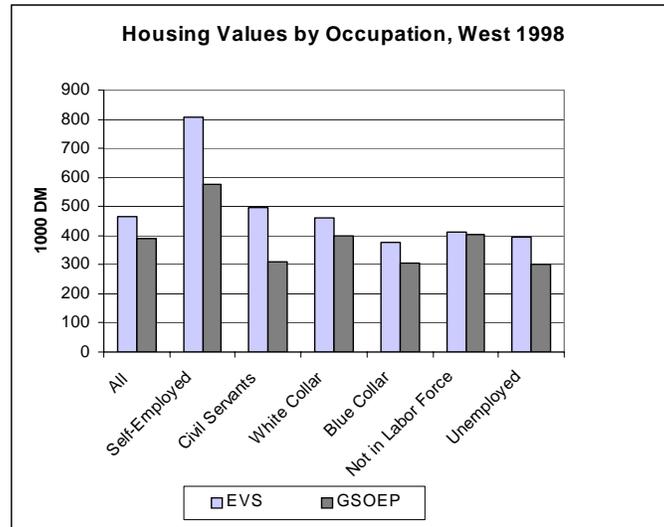


Figure 9: Average gross amounts of housing values conditional on owning a house by occupation in western Germany, taken from EVS and GSOEP 1998

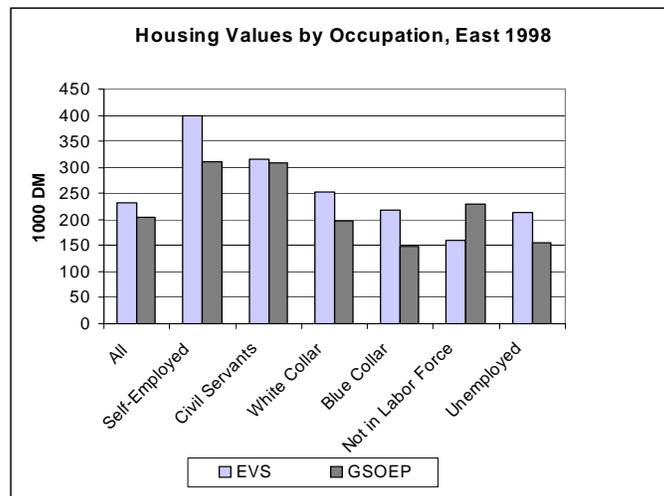


Figure 10: Average gross amounts of housing values conditional on owning a house by occupation in eastern Germany, taken from EVS and GSOEP 1998

Overall, we conclude that, based on a comparison to the best available wealth data from EVS, the wealth measures constructed from the GSOEP data seem to match the true wealth holdings reasonably well. While there are certain specific shortcomings, the unique episode of German reunification can be used to test for and quantify the importance of self-selection only based on GSOEP data. From a comparison to EVS, it seems that our qualitative results would not change, and our quantitative results might be even stronger, if we could use EVS data for our analysis.

B Pension differences

Pension differences between civil servants and non-civil servants could induce additional differences in the savings motives of both groups and could therefore complicate our analysis. In this section, we briefly describe the German pension system and perform an additional robustness check. Since any differences between pensions of civil servants and non-civil servants are the same in East and West, our main self-selection test is still valid even if part of the lower savings of civil servants in East and West might be caused by pension differences.

The German pension system offers relatively high replacement ratios. For an average worker with a 45 year contribution history the replacement ratio amounts to 70.5% (Börsch-Supan et al., 2004). Thus, additional private retirement savings play a less important role in Germany than, e.g., in the United States. Yet, pension regulations have been frequently reformed in the last decade and, given the unfavorable demographic development, the need for additional reforms is constantly stressed in the political debate. As a result, the amount of retirement income is perceived as uncertain, especially by younger generations. Retirement regulations from the West were adopted in East Germany immediately after reunification. Retirement incomes between eastern and western Germany differ only in as much as labor incomes still differ. The average household pension income in the East is even higher than in the West due to higher female labor market participation rates in the GDR (Sinn 2002).

German pension regulations for civil servants and non-civil servants differ in many aspects, making general comparisons virtually impossible.⁵⁰ There exist three especially important differences.⁵¹ First, civil servants do not pay explicit pension contributions during their working life. Instead, their pensions are paid by the government, and their gross income is lower than gross incomes of other public sector employees with comparable education. Second, civil servant pensions are taxable, while only

⁵⁰In the explanation of its ruling against the differential tax treatment of civil servant and non-civil servant pensions on March 6, 2002, the German Supreme Court stated that, “to compare both pension systems, one has to recur to a large number of case studies considering income paths and employment durations, family or labor market related disruptions of employment, marital status and number of children, in addition to the variety of possible entry paths into retirement.”

⁵¹The following descriptions just offer coarse summaries of the rules. For a detailed description of pension regulations for civil servants and non-civil servants, see Börsch-Supan et al. (2004).

a very small portion of non-civil servant pensions is taxable. Third, and probably most important, pensions of civil servants are calculated based on the last income, while pensions of non-civil servants are calculated based on the average income over the life cycle. Comparing retirement behavior, Börsch-Supan et al. (2004) report that civil servants retire on average one year earlier than non-civil servants. The German Supreme Court ruled the differential tax treatment of civil servant and non-civil servant pensions unconstitutional in March 2002 and asked the legislature to issue new rules by 2005. It acknowledged that the differential treatment was designed to counteract disadvantages faced by non-civil servants in other parts of the pension regulations, and implicitly demanded a thorough revision of all pension regulations.

The general perception is that civil servant pensions are more generous than non-civil servant pensions for individuals facing a steep income path over their life cycle. For individuals with comparatively flat income paths, it does not matter much whether pensions are calculated based on the average income or the last income, and the difference might be overcome by other regulations favoring non-civil servants. More highly educated individuals face steeper income paths, and these are found in the higher ranks of civil service. We redo our wealth regressions using two separate civil servant dummies for high (*gehobener* and *höherer Dienst*) and low (*unterer* and *mittlerer Dienst*) rank civil servants, instead of a common civil servant dummy for all civil servants. High rank civil servants may save less than non-civil servants with the same average income not only because of the lower income risk they face but also because of their more favorable pensions. The difference between low rank civil servants and non-civil servants is mainly in the formers' lower income risk.

The results of the estimation are shown in table 7. To economize on space we report only the coefficients and standard errors on the civil servant dummies. In the West sample, the coefficients on the high rank civil servant dummy are more negative than those on the low rank civil servant dummy. This gives evidence that part of the lower wealth holdings by civil servants might indeed be due to the pension provisions favoring high rank civil servants. However, except for the high rank civil servant dummy in the 2SLS regressions, it is still true that the coefficients on the high or low rank civil servant dummies are more negative in the East sample than the corresponding coefficients in the West sample. This difference should solely be based on risk differences.

Dep. variable: log(financial wealth)	West Sample	East Sample
2SLS (excluding zero wealth obs.)		
low rank civil servant (unterer/mittlerer Dienst)	-0.009 (0.091)	-0.318 (0.205)
high rank civil servant (gehobener/höherer Dienst)	-0.235 (0.093)	-0.214 (0.178)
IV-Tobit (including zero wealth obs.)		
low rank civil servant (unterer/mittlerer Dienst)	0.571 (0.186)	-1.307 (0.616)
high rank civil servant (gehobener/höherer Dienst)	-0.386 (0.186)	-1.058 (0.448)

Notes: Reported are the coefficients on the civil servant dummies from different wealth regressions (for details see the text). Standard errors are in parentheses and are corrected for pooling.

Table 7: Pension differences

C Political Risk and Occupational Choice in the GDR

Subsection 4.2 establishes that labor income risk was largely absent in the former GDR. Undeniably, however, there existed greater political risk than in the FRG. Arguably, occupational advancement and a life free of harassment were achieved more easily if a person was close to the party. This could influence the analysis if two statements were both true: first, more prudent individuals were closer to the party in order to avoid “trouble” of any kind; and, second, people who were closer to the party were more likely to work in jobs that correspond to civil service in the West.

With regard to the second statement, it is important to note that all major firms were state-owned, as was a large part of the agricultural sector. The “correct” political background was therefore not only necessary to advance in occupations that correspond to the civil service but also in occupations that correspond to the private sector in the West. Just as an indication, note that, in 1981, of the 213 members and candidates of the *Zentralkommittee*, which was the governing body of the ruling party SED, and hence the most powerful entity in the GDR, 75 worked full time for the party. Of the others, 53 worked in the “public sector,” while 27 worked in what corresponds to the “private sector” in the West, namely, agriculture and industry. 24 worked for unions, which do not belong to the civil service in the West, and 21 worked in science and culture, which is hard to classify (Rytlewski and Opp de Hipt, 1987). Of course, from the fact that closeness to the party was important for advancement in all occupations, one cannot conclude that it was not even more important in “civil service” occupations, in which case it could still be that civil servants were on average

more risk averse.⁵²

Moreover, the argument relies on the first statement that a prudent person would choose to be closer to the party, which relies on the assumptions that opportunism was wide-spread, or that it is positively correlated with prudence, both of which are very hard to test. We can, however, get some insights into the question of whether those that later became civil servants were more prudent or closer to the party from two survey questions. In the early summer of 1990, i.e., before the official reunification and even before the economic and currency union, GSOEP asked interviewees in the East whether they think it is desirable that a person is concerned/thoughtful about security/safety. A positive answer to this question comes closest to a measure of prudence available in the survey. The question could be answered on a scale of 0 (not desirable at all) to 10 (highly desirable). The average value to this question in 1990 of those individuals that are coded as civil servants in 1998 to 2000 is 8.27, while it is 8.62 for non-civil servants. This indicates that, if anything, those that later became civil servants on average consider prudence as less desirable than their counterparts.⁵³ If individuals act according to their social preferences, or build their social preferences based on their individual preferences, civil servants in the East should be less prudent than non-civil servants, if anything.

⁵²Unfortunately, we were not able to find data that allows us to calculate the percentage of party members in "civil service" occupations versus the percentage in other occupations.

⁵³Unfortunately, this question is never asked in the West.

D Summary statistics

Variable	full sample		
	Obs.	Mean / Percent	Std. Dev.
wealth	10007	35,223	103,364
monthly income	10007	4,617	2,065
age	10007	38.89	8.66
children	10007	0.85	0.96
adults (age>16)	10007	2.15	0.91
sex (male=1)	10007	69.2 %	
married	10007	60.5 %	
divorced	10007	10.7 %	
college	10007	23.2 %	
vocational training	10007	69.2 %	
secondary schooling	10007	4.1 %	
current residence (west=1)	10007	70.3 %	
own house	10007	45.3 %	
civil servant	10007	9.5 %	

Note: Monetary values are in DM, inflated to 2000 values (1 DM approx. 0.5 Dollar)

Table 8: Summary statistics for the full sample

	Variable	Obs.	Mean	Std. Dev.
civil servants	wealth	952	43,840	93,287
	monthly income	952	5,810	2,267
other occupations	wealth	9055	34,317	104,330
	monthly income	9055	4,492	2,002

Note: Monetary values are in DM, inflated to 2000 values (1 DM approx. 0.5 Dollar)

Table 9: Occupation group characteristics

Variable	West sample			East sample		
	Obs.	Mean / Percent	Std. Dev.	Obs.	Mean / Percent	Std. Dev.
wealth	5532	44,652	129,255	2510	23,006	46,179
monthly income	5532	5,119	2,210	2510	4,237	1,670
age	5532	41.51	7.05	2510	42.02	6.86
children	5532	0.97	1.03	2510	0.83	0.87
adults (age>16)	5532	2.13	0.89	2510	2.39	0.96
sex (male=1)	5532	76.0 %		2510	61.2 %	
married	5532	67.4 %		2510	70.3 %	
divorced	5532	11.8 %		2510	14.0 %	
college	5532	22.2 %		2510	34.0 %	
vocational training	5532	69.7 %		2510	64.0 %	
secondary schooling	5532	4.8 %		2510	1.1 %	
current residence (west=1)	5532	99.2 %		2510	8.6 %	
own house		54.2 %		2510	43.1 %	
civil servant	5532	12.9 %		2510	3.7 %	

Note: Monetary values are in DM, inflated to 2000 values (1 DM approx. 0.5 Dollar)

Table 10: Summary statistics, West sample and East sample