PRECAUTIONARY SAVINGS AND SELF-SELECTION: EVIDENCE FROM THE GERMAN REUNIFICATION "EXPERIMENT"*

NICOLA FUCHS-SCHÜNDELN AND MATTHIAS SCHÜNDELN

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 systematic underestimation of the importance of precautionary savings. 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.

I. 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 prudence[1] 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

1. We use prudence and risk aversion as synonyms. 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 II).

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 bias results in a systematic underestimation of the importance of precautionary savings.

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], and Guiso, Jappelli, and Terlizzese [1992]) to substantial evidence (e.g., Carroll and Samwick [1998] and Gourinchas and Parker [2002]).[2]

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.[3]

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

2. Browning and Lusardi [1996] give an excellent overview of the existing empirical literature.
3. In the only study so far that explicitly addresses this self-selection bias, Lusardi [1997] employs years of experience and regional unemployment rates as instruments for subjective income risk. Estimated precautionary savings increase from 2 percent of overall savings in the OLS-regressions to 20–24 percent in IV regressions. It is unclear, though, how to decompose this difference into self-selection versus measurement error.

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. A few recent studies indeed find evidence that risk aversion, captured by a lottery question, influences occupational choice [Cramer, Hartog, Jonker, and Van Praag 2002; Guiso and Paiella 2001; Guiso, Jappelli, and Pistaferri 2002]. 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.[4]

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: using our point estimates, the precautionary wealth that we infer from a sample of former GDR households that chose their occupation before reunification amounts to 22 percent of total wealth, while in the sample of West German households it

---

4. Some 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.

accounts for 13 percent 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 percent 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. Next, we describe the German institutional background. Section IV provides information about the data and the construction of our sample. In the following section we present the empirical results. Section VI deals with possible alternative interpretations of the results. The last section concludes.

II. 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 who faces a risky labor income path and maximizes the discounted value of future utility from consumption up to period T:

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

subject to the intertemporal budget constraint,

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

as well as a borrowing constraint,

$$(3) \qquad X_t - C_t \geq 0 \quad \forall\, t,$$

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 $P_t = GP_{t-1}\eta_t$ 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,

$$(4) \qquad u(C_t) = C_t^{1-\gamma}/(1 - \gamma),$$

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 percent, 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.[5]

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. We set the variances of the low-risk job $\sigma_\epsilon^{2,l} = \sigma_\eta^{2,l} = 0.0095$, slightly lower than in the calibration by Carroll and Samwick [1997]. 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, and we abstract from a retirement period. After having solved the model, we simulate 2000 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.

The simulations in Figure I show average wealth over permanent income if both types of individuals are allocated to equal

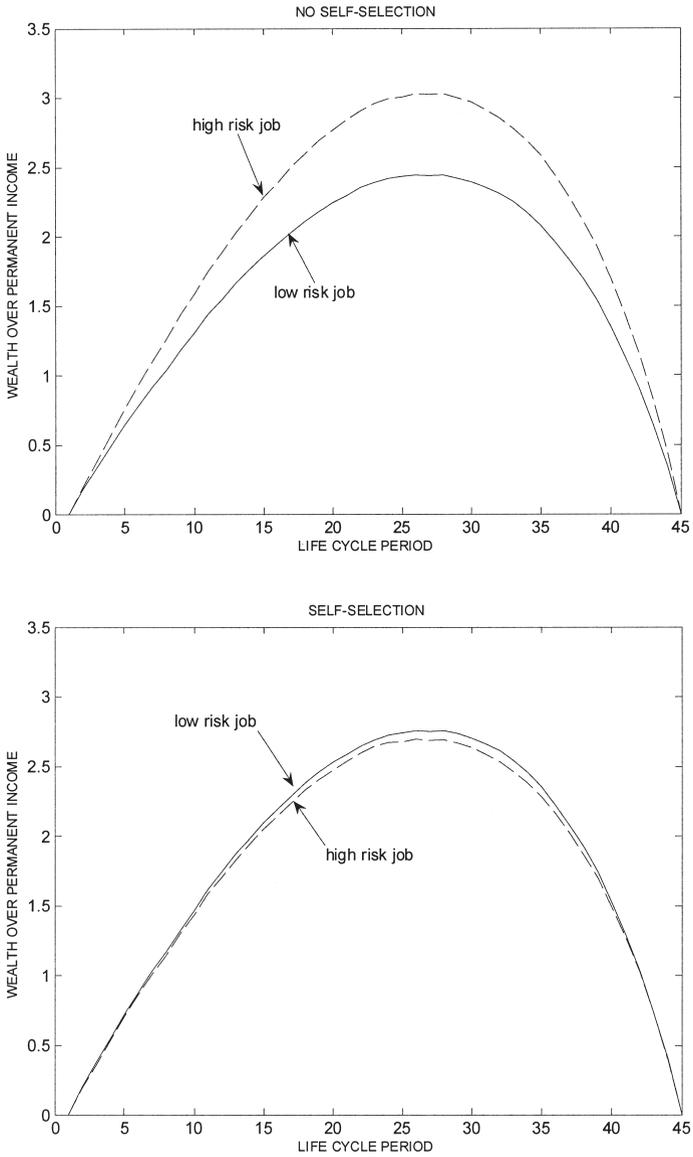5. The results remain qualitatively unchanged if we set $G > 1$.

FIGURE I

Averages of Wealth to Permanent Income Ratios of 2,000 Simulations over the
Life Cycle in the Absence and Presence of Self-Selection

parts in both types of jobs (upper plot) and if the individuals self-select into jobs according to their risk aversion (lower plot). In the upper plot the distribution of risk aversion is the same for both jobs, while in the lower 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 upper 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. Facing the same labor income risk, individuals with higher risk aversion hold strictly more wealth than individuals with lower risk aversion. This counteracts the positive correlation between risk and wealth. 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 lower plot of Figure I). 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.

III. 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.

III.A. Civil Service

The privileges, duties, and categories for salaries of German civil servants are regulated by federal and state laws. 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 wages do 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 percent. The average duration of unemployment spells was around eleven months, but longer for older workers [Bundesanstalt für Arbeit]. The monthly unemployment incidence in West Germany averaged 0.45 percent from 1984 to 1995 [Gangl 2001]. Although unemployment insurance payments are quite generous in amount and duration in Germany, long unemployment spells still lead to significant income reductions.[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 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

6. An unemployed person fulfilling certain requirements receives *Arbeitslosengeld* ("unemployment money"), during our sample period amounting to 60 percent of the last net income (67 percent with children). *Arbeitslosengeld* is paid for 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 (53 percent of the last net income or 57 percent with children) and requires a means test, incorporating wealth and incomes of other household members.

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

lower income variations than non-civil servants (see subsection VI.B).

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. In our data set, we estimate that 6.1 percent 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 percent of white-collar workers, 18.7 percent of blue-collar workers, and 12.1 percent 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]

III.B. 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 with that in the Federal Republic of Germany (FRG). In 1988 the average net income of individuals with a university degree was only 15 percent higher than that of blue-collar workers, compared with 70 percent in the FRG. Also, intersectoral differences in net incomes were minimal, on average amounting to only 150 Mark per month with an average monthly

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

9. In Appendix 2 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.

There 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, using data from 1992 to 2000, the civil servant dummy is close to zero and insignificant (*p*-value 0.606).

income of around 1100 Mark in 1988 [Stephan and Wiedemann 1990; Schäfgen 1998]. Wage inequality was very low with a Gini coefficient of 0.17 in 1990 [Steiner and Puhani 1997]. It appears that the government, in order to achieve the goal of full employment spelled out in the constitution, quickly found a new job for anybody who might have been displaced [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.[10] 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.

The German Unification Treaty of 1990 extended the FRG system of civil service to East Germany without major changes. 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].

IV. The Data

IV.A. *The German Socio-Economic Panel*

The data come from the German Socio-Economic Panel (GSOEP).[11] This annual panel survey was started in 1984 and

---

10. An underlying assumption is either that individuals did not self-select according to political risk, or that political risk is not correlated with income risk after reunification. Arguably, occupational advancement was 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; and, second, people who were closer to the party were more likely to work in jobs that correspond to civil service in the West. In Fuchs-Schündeln and Schündeln [2005] we discuss both hypotheses and provide evidence against them.

11. We use the 95 percent research sample of "GSOEP 1984–2000." A detailed description of the survey can be found in SOEP Group [2001].

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, 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.[12]

We restrict our sample as follows: we drop the subsamples that focus exclusively on foreigners and migrants. Further, we focus on labor force participants, i.e. we exclude retirees, but include households whose main income earner is unemployed. We eliminate households whose main income earner is self-employed, since we do not have good information about accumulation of business capital. Moreover, the self-employed are not required to contribute to the compulsory pension system and might choose to accumulate retirement savings in private funds. We report results including the self-employed in one of the sensitivity analyses in subsection V.D. 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.[13] We construct a measure of financial wealth from infor-

12. In 71 percent of all observations the main income earner is also recorded as the "head of the household," and in 95 percent 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.

13. The 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 house-

mation 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 1, Section A. Appendix 1, Section C 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.[14] We construct a measure of housing wealth (see Appendix 1, Section B), confirm its validity in Appendix 1, Section C, and repeat our analysis with a more comprehensive wealth measure in the robustness checks in subsection V.D. Nominal variables are inflated to year 2000 values.

IV.B. 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 twenty years old at the time of reunification, and we argue that they had already made their occupational choice at this time.[15] 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.[16]

---

hold income today? Please state the net monthly income, which means after deductions for taxes and social security. Please include regular income such as pensions, housing allowance, child allowance, grants for higher education, support payments etc."

    14. E.g., Kazarosian [1997] and Engen and Gruber [2001] focus exclusively on financial wealth, while Carroll and Samwick [1998] use financial wealth as well as total net worth as dependent variables.

    15. People in the GDR graduated from high school between the ages of 16 and 18, depending on their final schooling level.

    16. It is, however, 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. If the emigrants already

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. Of main income earners in the West German sample who are employed in 1990 and 2000, 82.5 percent 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 VI in Appendix 3.

V. EMPIRICAL RESULTS

We start our estimation from the following specification, as in Carroll and Samwick [1998]:

$$(5) \qquad \log (W) = \beta_0 + \beta_1 risk + \beta_2 \log (P) + \delta'Z + \epsilon,$$

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 used as the measure of 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, Jappelli, and Terlizzese [1992], in a first approach households with zero or negative wealth holdings are eliminated from the sample.[19] This may bias the results if observations with zero or negative wealth are

---

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 who have a GDR education indeed lived in the GDR until reunification.

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

18. Estimates 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.

19. Carroll and Samwick [1998] and Guiso, Jappelli, and Terlizzese [1992] exclude households with negative wealth. Our data do not allow us to distinguish between zero and negative wealth holdings.

not just due to measurement error. Hence, in an additional analysis in subsection V.C, 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.[21] Therefore, we instrument permanent income, using education dummies and interaction terms of education with age and age squared as instruments, and estimate 2SLS models.[22]

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 age, the number of children, and the number of adults in the household, as well as marital status, in order to capture other wealth accumulation motives, especially savings for bequests and children's education.[23] We also employ a dummy variable for current residence that takes on the value of one if the household lives in western Germany in the observation year. The omitted marital status is single or widowed.

V.A. *Demonstrating the Self-Selection Bias: Using the Reunification "Experiment"*

Table I shows the regression results. Permanent income, number of adults, and number of children have the same signs

---

20. We calculate permanent income as follows: we detrend total noncapital 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.

21. Note that for households that were added in the refreshment sample 2000, permanent income equals income in 2000.

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

23. Note that 84 percent of retired West German households receive public pensions as the only source of retirement income, and the average contribution of private pension income to total retirement income amounts to only 3 percent [Börsch-Supan and Schmidt 2001]. Hence, private retirement savings play a very limited role.

TABLE I
2SLS WEALTH REGRESSIONS, WEST AND EAST SAMPLES

Dependent 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

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.

and are significant in all specifications in both subsamples. The dummy for residence in western Germany is positive and significant in the West sample, but negative and significant in the East sample. The inclusion of the home ownership dummy does not alter the other coefficients in a significant way.

The coefficient on the civil servant dummy in the East sample is negative and significant (at the 8 percent significance level). In the West sample, the coefficient is also negative and signifi-

cant, but only slightly more than half the size in absolute terms of the coefficient in the East sample. 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. However, according to the point estimates, 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. We 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.

V.B. *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.[24] 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.[25] In a next step, we assume that 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.

24. Specifically, 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]).

25. We use specifications (ii) and (iv) from Table I.

For the East sample, precautionary wealth accounts for 22.1 percent of all wealth, while for the West sample it accounts for only 12.9 percent.[26] 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.

V.C. Including Zero Wealth Observations

In the regression analysis so far, 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 nonrandom, and, hence, their elimination can lead to a bias in the estimation. Of the civil servants in the East, 6.9 percent have zero wealth holdings, while the corresponding number in the West is 3.8 percent. On the contrary, among the non-civil servants the percentage of zero wealth holders is slightly larger in the West (12.2 percent) than in the East (11.1 percent). 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. We now keep all observations with zero wealth and estimate instrumental variable Tobit models [Newey 1987].[27] We report bootstrap standard errors that are corrected for pooling.

We get even stronger evidence for self-selection than in the 2SLS estimations (see Table II). The coefficient on the civil servant dummy is positive and insignificant in the West sample, but negative and significant at the 1 percent 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.

Based on the Tobit estimates, the quantitative importance of the bias is even more striking. We redo the quantification exercise in subsection V.B with the results from these parameter esti-

---

26. These 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 with 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.

27. Since 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.

TABLE II
IV-Tobit Wealth Regressions

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

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.

mates.[28] In the West sample none of the wealth holdings are due to precautionary reasons, while in the East sample, precautionary savings account for 68 percent 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 percent. Hence, the difference between East and West is not only economically significant but also statistically significant.

---

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

V.D. 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. 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 regressions and the instrumental variable Tobit regressions.

Results for the coefficient on the civil servant dummy are reported in Table III. Generally, the coefficients on the civil servant dummy are similar to the ones in the baseline results, which are reported in the first panel for comparison. 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 GDR, 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.

V.E. 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 with 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. 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 fifteen different educational and occupational groups.[29] We then estimate 2SLS estimations

---

29. To 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 are not consistently available in GSOEP over the 1990s.

TABLE III
ROBUSTNESS CHECKS

| 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]	0.003
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]	0.001
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]	0.001
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]	0.004

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 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.

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 IV reports the results. 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

TABLE IV

REGRESSIONS WITH CONVENTIONAL RISK MEASURE

Dependent variable: log(financial wealth)	West sample	East sample
log(permanent income)	1.873	1.581
	(0.171)	(0.239)
log variance (log income)	0.395	0.778
	(0.130)	(0.305)
age	−0.059	−0.086
	(0.039)	(0.049)
age squared ($\cdot 10^2$)	0.072	0.109
	(0.046)	(0.057)
sex (1 = male)	0.059	0.206
	(0.052)	(0.057)
married	−0.301	−0.175
	(0.067)	(0.093)
divorced	−0.327	−0.047
	(0.077)	(0.094)
adults (age > 16)	−0.218	−0.204
	(0.032)	(0.042)
children	−0.101	−0.154
	(0.022)	(0.034)
current residence (1 = West)	0.418	−0.185
	(0.159)	(0.093)
home ownership	0.111	0.076
	(0.051)	(0.053)
constant	−4.078	−0.230
	(1.610)	(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

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.

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 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. Note that a Chow test does not reject equality of the coefficients on the risk measure in the East and West samples. However, the difference in the point estimates gives evidence for self-selection, which induces a downward bias on

the coefficient in the West sample.[30] Hence, this exercise confirms the earlier results.

VI. 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.[31]

VI.A. 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 analyze whether wealth differences according to labor income risk evolved over the 1990s in the East sample, instead of being present already in 1990. One constraint for this analysis is that the number of (future) civil servants becomes very small if the refreshment samples from 1998 and 2000 cannot be used. We therefore present this analysis using the conventional risk measure introduced in subsection V.E. Results from a similar analysis using the civil servant dummy are qualitatively similar and are reported in Fuchs-Schündeln and Schündeln [2005].

We reestimate the regression for the East and West samples as in Table IV for a rolling sample, always pooling three years and starting with the 1992–1994 sample. In Figure II 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

---

30. The 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.

31. In Fuchs-Schündeln and Schündeln [2005] we present some independent suggestive evidence, derived from smoking behavior, that civil servants are more risk averse than non-civil servants in the West sample but not in the East sample.
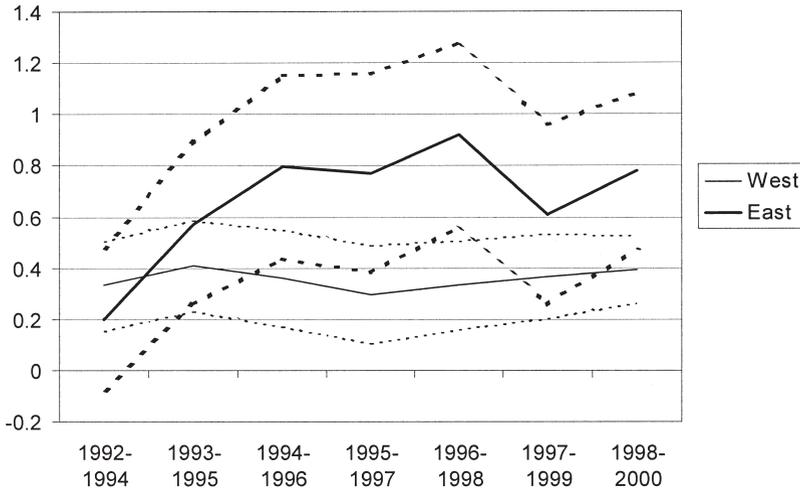
FIGURE II

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

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, Dynan, and Krane [2003], who for this reason suggest excluding households that have recently experienced an unemployment spell from the estimation).

VI.B. East-West Differences in Labor Income Risk

As discussed in subsection III.A, 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.[32] Second, German unemployment insurance payments are quite generous. Third, publicly financed work creation programs have been used to a large extent in the East [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.[33] More importantly, the wage distribution in the GDR was more compressed than in West Germany before reunification, and while income inequality in the East has been rising since reunification, it still has not reached the Western German level [OECD 2001; Biewen 2000].[34]

To analyze income risk, we construct both the average time series variance and the coefficient of variation of the unpredictable component of individual household income (including unemployment insurance payments) from 1992 to 2000, separately for the East and West, and for civil servants and non-civil servants.[35] 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, and the unpredictable component equals actual income minus the predictable component. Four clear features emerge. First, income risk is lower in the East than in the West. Second, both risk measures indicate that this also holds true among non-civil servants. Third, civil servants have lower risk than non-civil servants. Finally, and most importantly, the risk difference between civil servants and non-civil servants is smaller in the East than in the West. Full results are reported in Fuchs-Schündeln and Schündeln [2005].

32. It could of course still be that, due to different human capital, East and West Germans living in the same federal state face different risks.
33. We 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, provided by the Bundesanstalt für Arbeit. Results are available from the authors upon request.
34. The OECD [2001] uses net equivalent income from the Income and Expenditure Survey 1998 as the source of data. Biewen [2000] uses the same measure of disposable household income that is employed in this paper.
35. When 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.

VI.C. 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 2300 DM in the East, while in the West it amounted to 2600 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 percent versus 21.6 percent [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.

Finally, civil servants and non-civil servants could also differ

along observable characteristics in a way that is different in the East than in the West. Since we control for these characteristics, this would only cause concern if some controls had nonlinear effects on wealth holdings that are not captured in the functional form of the regression analyses. Table VI in Appendix 3 shows the ratios of mean values among civil servants to mean values among non-civil servants for all variables used in the regressions, separately for the East and West samples. While differences in these ratios exist, there is no clear pattern in which direction they go.[36] The largest East-West differences in these ratios arise in two characteristics that apply to a relatively small proportion of the overall population, namely being divorced and having secondary schooling as the highest educational attainment.

VII. 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 with that of 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

---

36. For half the variables, the ratio is larger in the East, while for the other half it is larger in the West. More importantly, there is no clear correlation between the East-West difference of the ratios and the sign of the coefficient of the respective variable in the regressions.

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 percent 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 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.

Appendix 1: Data

A. *Construction of the Financial Wealth Measure*

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 percent in 1998, 2.8 percent in 1999, and 4 percent in 2000, calculated based on average wealth portfolios and average rates on savings accounts, bonds, and stocks in the respective years [Statistisches Bundesamt, various issues; 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 2,000 DM, 2,000 to 5,000 DM, 5,000 to 10,000 DM, more than 10,000 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.

B. 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 households 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 ten-year fixed mortgages during the period 1971 to 2001, namely 8.25 percent (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. If a household indicates that it purchased the house it was previously renting, we use instead the year of the purchase. Given this information, we can calculate the accumulated amount of amortization 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 percent 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 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 home owners, 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 down payment 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.

C. Validation of Wealth Measures

To check the validity of the constructed wealth data, we resort 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.[37] The EVS is conducted every five years without a panel component, with the last available survey round being 1998. We compare our data with detailed sample statistics provided by the Federal Statistical Office. The EVS reports current residence, but not residence before reunification;[38] hence, in the following comparisons between EVS and GSOEP, we use current residence for the sample split. 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 percent of the EVS sample in western Germany and 5 percent in eastern Germany.

Our wealth measure would be problematic if the composition of financial wealth differed significantly between civil servants and non-civil servants. EVS data show, however, that the portfolio compositions of civil servants and the rest of the population are very similar in East and West [Münnich 2001]. See Fuchs-Schündeln and Schündeln [2005] for detailed figures of the composition of financial wealth.

Next, we compare the amounts of average financial wealth holdings constructed from the GSOEP data in 1998 with the average amounts reported by EVS [Münnich 2001].[39] While the EVS reports average gross financial household wealth of 61,200 DM in western Germany and 31,900 DM in eastern Germany, our

37. In the 1998 round, EVS interviewed more than 62,000 households.
38. This is the main reason why we cannot use EVS data for our analysis.
39. To make the results from GSOEP representative, we use the full sample and cross-sectional weights.

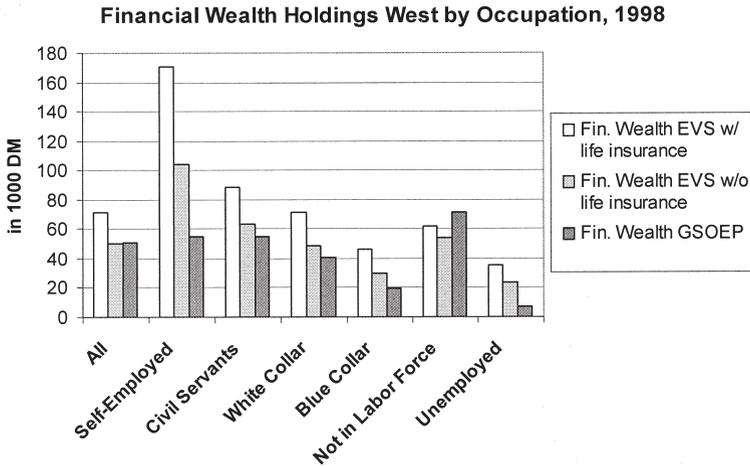**Financial Wealth Holdings West by Occupation, 1998**

Figure III

Financial Wealth of West German Households by Occupational Groups as
Reported by EVS and GSOEP

wealth measure has means of 50,500 DM in western Germany
and 19,800 DM in eastern Germany, i.e., 71 percent and 62
percent of the values from EVS. If wealth invested in life insur-
ance is omitted from the financial wealth holdings reported by
EVS, the GSOEP data correspond to 101 percent in the West and
75 percent in the East. We are probably missing this component
of financial wealth, since life insurance contracts do not pay
yearly interest. Life insurance contracts represent relatively il-
liquid 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 III and IV 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 by
GSOEP.[40] 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 re-
ports significantly higher wealth for the self-employed and lower

40. Note that some cell sizes are fairly small, and hence these numbers are
less reliable than a comparison of the overall wealth holdings.

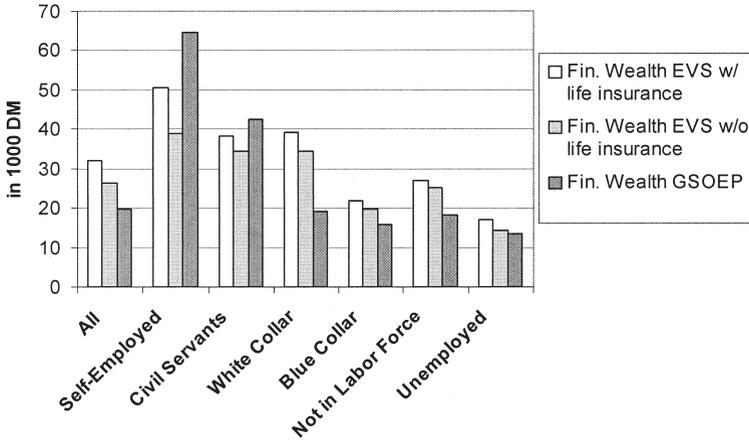**Financial Wealth Holdings East by Occupation, 1998**

FIGURE IV
Financial Wealth of East German Households by Occupational Groups as
Reported by EVS and GSOEP

wealth for white-collar workers than EVS, again excluding life insurance. GSOEP and EVS data differ most for the self-employed; we exclude the self-employed in our main analysis.

For civil servants the constructed GSOEP data match the EVS data without life insurance fairly well in the East and West. In the West the wealth of civil servants is slightly less underestimated than the wealth of white-collar workers, blue-collar workers, and the unemployed, while in the East civil servants are the only occupation group for which wealth is overestimated. If we believe that the true wealth holdings correspond to the wealth reported in EVS, we could rescale the GSOEP wealth measure by occupation group in order to match the averages from the EVS data without life insurance. Based on these rescaled data, our self-selection result and its quantitative importance would be even larger.[41]

We perform similar comparisons of our housing wealth data to EVS. For 1998, EVS reports higher home ownership rates than GSOEP, namely 47.8 percent and 33.3 percent for western and eastern Germany, respectively, in the EVS, and 42.1 percent and

41. Note 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.

28.4 percent, respectively, in GSOEP. The gross average total value of owner and non-owner-occupied housing (i.e., without deducting mortgage debt) in 1998 constructed from GSOEP amounts to 391,000 DM in western Germany, and 203,000 DM in eastern Germany.[42] These compare with 467,000 DM in western Germany and 231,000 DM in eastern Germany in the 1998 EVS [Münnich 1999], i.e., with 83.7 percent and 87.9 percent of the EVS data. The common belief that EVS overestimates home ownership rates, as well as average housing values conditional on possessing a home [Börsch-Supan et al. 1999; Münnich 1999] is hence consistent with our findings. Comparing across occupations, we find that EVS always reports higher housing values than GSOEP, except for eastern households not in the labor force. If we were to rescale the GSOEP data per group to match the average EVS data, again the quantitative implications of the self-selection bias would be even larger.[43]

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.

APPENDIX 2: 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. However, 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.

German pension regulations for civil servants and non-civil servants differ in many aspects, making general comparisons

---

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

43. See Fuchs-Schündeln and Schündeln [2005] for more details on the comparison of housing wealth in EVS and GSOEP.

TABLE V
ROBUSTNESS CHECK: PENSION DIFFERENCES

Dependent variable: log(financial wealth)	West sample	East sample
2SLS		
low rank civil servant (*unterer/mittlerer Dienst*)	−0.009	−0.318
	(0.091)	(0.205)
high rank civil servant (*gehobener/höherer Dienst*)	−0.235	−0.214
	(0.093)	(0.178)
IV-Tobit		
low rank civil servant (*unterer/mittlerer Dienst*)	0.571	−1.307
	(0.186)	(0.616)
high rank civil servant (*gehobener/höherer Dienst*)	−0.386	−1.058
	(0.186)	(0.448)

Reported are the coefficients on the civil servant dummies from different wealth regressions (for details see the text). Observations are pooled for years 1998–2000. Standard errors are in parentheses and are corrected for pooling. Other controls included: log(permanent income), age, age squared, sex, married, divorced, number of adults, number of children, dummy for current residence, home ownership, 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.

virtually impossible.[44] Comparing retirement behavior, Börsch-Supan et al. [2004] report that civil servants retire on average one year earlier than non-civil servants. The general perception is that civil servant pensions are more generous than non-civil servant pensions for individuals in the higher ranks of civil service. Hence 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. High rank civil servants may save less than non-civil servants not only because of lower income risk, 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 V.[45] 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, giving evidence that part of the lower wealth hold-

44. In Fuchs-Schündeln and Schündeln [2005] we lay out the differences between pension regulations of civil servants and non-civil servants in some detail. For a very thorough description see Börsch-Supan et al. [2004].

45. To economize on space, we report only the coefficients and standard errors on the civil servant dummies.

ings by civil servants might indeed be due to 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 be based solely on risk differences.

APPENDIX 3: SUMMARY STATISTICS

TABLE VI
SUMMARY STATISTICS, WEST SAMPLE AND EAST SAMPLE

	West sample			East sample		
Variable	Obs.	Mean/ percent	Ratio CS/ non-CS	Obs.	Mean/ percent	Ratio CS/ non-CS
wealth	5532	44,652 (129,255)	1.15	2510	23,006 (46,179)	1.37
monthly income	5532	5,119 (2,210)	1.23	2510	4,237 (1,670)	1.48
age	5532	41.51 (7.05)	1.05	2510	42.02 (6.86)	1.05
children	5532	0.97 (1.03)	0.97	2510	0.83 (0.87)	0.71
adults (age > 16)	5532	2.13 (0.89)	1.08	2510	2.39 (0.96)	1.21
sex (male = 1)	5532	76.0%	1.10	2510	61.2%	1.26
married	5532	67.4%	1.10	2510	70.3%	1.22
divorced	5532	11.8%	1.15	2510	14.0%	0.30
college	5532	22.2%	2.43	2510	34.0%	2.29
vocational training	5532	69.7%	0.70	2510	64.0%	0.39
secondary schooling	5532	4.8%	0.35	2510	1.1%	0
current residence (West = 1)	5532	99.2%	1.00	2510	8.6%	1.13
own house	5532	54.2%	1.33	2510	43.1%	1.19
civil servant	5532	12.9%		2510	3.7%	

"Ratio CS/non-CS" of a variable corresponds to the ratio of the mean among civil servants to the mean among non-civil servants. Standard deviations are in parentheses. Monetary values are in DM, inflated to 2000 values (1 DM approximately 0.5 Dollar). Data are pooled for the years 1998–2000. Individual level information refers to the main income earner of the household. Only main income earners who were at least 30 years old in 2000 and less than 55 years old in the survey year are included in the sample. Self-employed and apprentices are excluded. Omitted categories are single/widowed, and incomplete secondary schooling as highest educational attainment.

HARVARD UNIVERSITY

References

Biewen, Martin, "Income Inequality in Germany during the 1980s and 1990s," *Review of Income and Wealth,* XLVI (2000), 1–19.

Bonin, Holger, and Klaus F. Zimmermann, "The Post-Unification German Labor Market," in Regina T. Riphahn, Dennis J. Snower, and Klaus F. Zimmermann, eds., *Employment Policy in Transition. The Lessons of German Integration for the Labor Market* (Berlin, Germany: Springer-Verlag, 2001).

Börsch-Supan, Axel, Anette Reil-Held, Ralf Rodepeter, Reinhold Schnabel, and Joachim Winter, "Ersparnisbildung in Deutschland: Meßkonzepte und Ergebnisse auf Basis der EVS," *Allgemeines Statistisches Archiv,* LXXXIII (1999), 385–415.

Börsch-Supan, Axel, and Peter Schmidt, "Early Retirement in East and West Germany," in Regina T. Riphahn, Dennis J. Snower, and Klaus F. Zimmermann, eds., *Employment Policy in Transition. The Lessons of German Integration for the Labor Market* (Berlin, Germany: Springer-Verlag, 2001).

Börsch-Supan, Axel, Reinhold Schnabel, Simone Kohnz, and Giovanni Mastrobuoni, "Micro Modeling of Retirement Decisions in Germany," in Jonathan Gruber and David A. Wise, eds., *Social Security Programs and Retirement around the World: Micro-Estimation* (Chicago, IL: The University of Chicago Press, 2004), pp. 285–343.

Browning, Martin, and Annamaria Lusardi, "Household Saving: Micro Theory and Micro Facts," *Journal of Economic Literature,* XXXIV (1996), 1797–1855.

Bundesanstalt für Arbeit, *Monatsbericht,* various issues.

Carroll, Christopher D., "The Buffer-Stock Theory of Saving: Some Macroeconomic Evidence," *Brookings Papers on Economic Activity,* 2 (1992), 61–135.

Carroll, Christopher D., "Buffer Stock Saving and the Life Cycle/Permanent Income Hypothesis," *Quarterly Journal of Economics,* CXII (1997), 1–56.

Carroll, Christopher D., Karen E. Dynan, and Spencer D. Krane, "Unemployment Risk and Precautionary Wealth: Evidence from Households' Balance Sheets," *Review of Economics and Statistics,* LXXXV (2003), 589–604.

Carroll, Christopher D., and Andrew A. Samwick, "The Nature of Precautionary Wealth," *Journal of Monetary Economics,* XL (1997), 41–71.

Carroll, Christopher D., and Andrew A. Samwick, "How Important Is Precautionary Saving?" *Review of Economics and Statistics,* LXXX (1998), 410–419.

Cramer, J. S., J. Hartog, N. Jonker, and C. M. Van Praag, "Low Risk Aversion Encourages the Choice for Entrepreneurship: An Empirical Test of Truism," *Journal of Economic Behavior & Organization,* XLVIII (2002), 29–36.

Deutsche Bundesbank, *Statistischer Anhang zu Monatsberichten,* various issues.

Dynan, Karen E., "How Prudent Are Consumers?" *Journal of Political Economy,* CI (1993), 1104–1113.

Engen, Eric M., and Jonathan Gruber, "Unemployment Insurance and Precautionary Saving," *Journal of Monetary Economics,* XLVII (2001), 545–579.

Fuchs-Schündeln, Nicola, and Matthias Schündeln, "Precautionary Savings and Self-Selection—Evidence from the German Reunification 'Experiment'," Harvard Institute of Economic Research Working Paper No. 2069, 2005.

Gangl, Markus, "Virtues of Employment Protection, Burdens from Economic Restructuring: Unemployment Incidence in the United States and West Germany over the 1980s and 1990s," Paper prepared for the ISA Research Committee on Social Stratification, 2001.

Gourinchas, Pierre-Olivier, and Jonathan A. Parker, "Consumption over the Life Cycle," *Econometrica,* LXX (2002), 47–89.

Guiso, Luigi, Tullio Jappelli, and Luigi Pistaferri, "An Empirical Analysis of Earnings and Employment Risk," *Journal of Business & Economic Statistics,* XX (2002), 241–253.

Guiso, Luigi, Tullio Jappelli, and Daniele Terlizzese, "Earnings Uncertainty and Precautionary Savings," *Journal of Monetary Economics,* XXX (1992), 307–337.

Guiso, Luigi, and Monica Paiella, "Risk Aversion, Wealth and Background Risk," CEPR Discussion Paper No. 2728, 2001.

Hille, Barbara, "Nicht nur Blauhemden: Die Situation der Jugendlichen in der

ehemaligen DDR," Deutschland-Report 13 der Konrad-Adenauer-Stiftung, 1991.

Kazarosian, Mark, "Precautionary Savings—A Panel Study," *Review of Economics and Statistics,* LXXIX (1997), 241–247.

Kennickell, Arthur, and Annamaria Lusardi, "Wealth Accumulation and the Importance of Precautionary Savings," Dartmouth College, 2003.

Lusardi, Annamaria, "Precautionary Saving and Subjective Earnings Variance," *Economics Letters,* LVII (1997), 319–326.

Münnich, Margot, "Haus- und Grundbesitz sowie Wohnverhältnisse Privater Haushalte in Deutschland. Ergebnisse der Einkommens- und Verbrauchs-stichprobe 1998," *Wirtschaft und Statistik,* 3 (1999), 210–220 and 183*–190*.

——, "Einkommens- und Geldvermögensverteilung privater Haushalte in Deutschland—Teil 2. Ergebnis der Einkommens- und Verbrauchsstichprobe 1998," *Wirtschaft und Statistik,* 2 (2001), 121–137.

Newey, Whitney, "Efficient Estimation of Limited Dependent Variable Models with Endogenous Explanatory Variables," *Journal of Econometrics,* XXXVI (1987), 231–250.

OECD, *Economic Surveys: Germany* (Paris, France: 2001).

Quint, Peter, *The Imperfect Union. Constitutional Structures of German Unification* (Princeton, NJ: Princeton University Press, 1997).

Rytlewski, Ralf, and Manfred Opp de Hipt, *Die Deutsche Demokratische Republik in Zahlen, 1945/49–1980* (München, Germany: Verlag C.H. Beck, 1982).

Schäfgen, Katrin, "Die Verdoppelung der Ungleichheit. Sozialstruktur und Geschlechterverhältnisse in der Bundesrepublik und in der DDR," Dissertation, Humboldt-Universität, 1998.

Skinner, Jonathan, "Risky Income, Life Cycle Consumption, and Precautionary Savings," *Journal of Monetary Economics,* XXII (1988), 237–255.

SOEP Group, "The German Socio-Economic Panel (GSOEP) after More Than 15 years—Overview," *Vierteljahrshefte zur Wirtschaftsforschung* LXX (2001), 7–14.

Statistisches Bundesamt, *Statistisches Jahrbuch,* various issues.

Steiner, Viktor, and Patrick A. Puhani, "Die Entwicklung der Lohnstruktur im Ostdeutschen Transformationsprozeß," in Karl Heinrich Oppenländer, ed., *Wiedervereinigung nach sechs Jahren: Erfolge, Defizite, Zukunftsperspektiven im Transformationsprozeß* (Berlin/München, Germany: Duncker & Humblot, 1997).

Stephan, Helga, and Eberhard Wiedemann, "Lohnstruktur und Lohndifferen-zierung in der DDR: Ergebnisse der Lohndatenerfassung vom September 1988," *Mitteilungen aus der Arbeitsmarkt- und Berufsforschung,* XXIII (1990), 550–562.